The Effect of GSE Mortgage Purchases on Lenders' Screening Incentives*

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Abstract

This study investigates the effect of mortgage purchases by the government-sponsored enterprises (GSEs) on mortgage originators' screening incentives. We find a discontinuous change in the acceptance rates at the conforming/jumbo cutoff, which indicates a higher acceptance rate for mortgages eligible to be purchased by the GSEs. This discontinuous change in the acceptance rates largely diminished after the GSEs increased their efforts in reviewing the purchased loans in year 2009. Our findings suggest that GSE mortgage purchases have unintended negative effects on mortgage originators' screening incentives before the subprime mortgage crisis.

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1. Introduction

Standard theory of financial intermediation postulates that, in order to induce lenders to screen and monitor, lenders must be provided with appropriate incentives. For example, one way to ensure that lenders have enough “skin in the game” is to keep illiquid loans on lenders’ balance sheet. Not surprisingly, when the originator of a loan is separated from the bearer of the default risk of the loan, a distortion in lenders’ screening incentives can be created: lenders may exert less effort in screening a borrower if the loan is going to be taken off the balance sheet (Pennacchi, 1988; Gorton and Pennacchi, 1995). Such moral hazard issues in the subprime real estate market have received increasing attention since the 2008 housing meltdown and are examined in a number of studies.[1] In particular, Keys et al. (2010) show that securitization, a practice that transforms illiquid loans to securities and thus separates mortgage originations from mortgage holding, has adversely affected the screening incentives of subprime lenders. In this study, we investigate the incentive problems induced by a different channel, the mortgage purchases by the government-sponsored enterprises (GSEs), and examine whether moral hazard, which is shown to be severe in the subprime market, are manifested in another segment of the mortgage market that also experienced high delinquency rates during the housing crisis – the prime market.[2]

The GSEs, Fannie Mae and Freddie Mac, are created by the Congress as one important mechanism to encourage housing consumption and to promote home ownership. They purchase residential mortgages from the original lenders and hold them in their own portfolios or package them and sell the resultant mortgage-backed securities to investors. On the one hand, the GSE mortgage purchases from mortgage originators creates a separation of mortgage originating

[1] See Mian and Sufi (2009), Keys et al. (2010), Demyanyk and Van Hemert (2011), and Dell’Ariccia, et al. (2012).
[2] See Demyanyk and Van Hemert (2011).

from mortgage holding, which can lead to loosened screening standards by mortgage originators. In contrast to the private securitization practice where mortgage originators usually hold junior tranches as a mechanism to prevent moral hazard, the loans are completely off mortgage originators' balance sheets when they are sold to GSEs.[3] Furthermore, the implicit government backing associated with the GSE's quasi-government status could lessen the GSEs' incentives to adequately screen the acquired loans, leading to a lack of market discipline on the moral hazard problems of mortgage originators.[4] On the other hand, the GSEs have set specific underwriting guidelines regarding the mortgages that are eligible for their purchases, which suggests that the moral hazard problem documented in the subprime market may not exist in the prime market. Thus, how the GSE activities affect lenders' screening incentives remains an empirical question.

Using a large sample of loan applications for year 2006 to 2011 from the Home Mortgage Disclosure Act (HMDA) dataset, we examine whether the mortgage purchases by the GSEs reduce mortgage originators' screening incentives.[5] Our primary measure of screening incentives is the acceptance/rejection decision of a mortgage application by the lender, which has been used by Loutskina and Strahan (2009, 2011). In addition, we supplement our loan-level acceptance/rejection analysis with a zip code level loan performance analysis. In comparison with the loan quality (e.g., mortgage delinquency rate) as a measure for screening incentive, the measure using acceptance/rejection decision offers several advantages. First, mortgage delinquency rate is only available for the loans that are accepted. Thus analysis based

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[3] See Brunnermeier (2008). Although the GSEs also have mechanisms to prevent the issue of moral hazard such as representations and warranties, these mechanisms, as we discuss later in the paper, did not function effectively.

[4] The moral hazard problem of financial institutions that stems from "too-big-to-fail" policy has been widely studied. See French et al (2010) for example.

[5] Compared with other datasets (i.e. LPS), HMDA has the drawback of lacking some information on borrower characteristics and loan contracts. However, because our analysis requires information on both *denied* and *accepted* loan applications, employing LPS can't solve our problem.

on mortgage delinquency rate utilizes only a subset of all loan applications. In contrast, the measure of acceptance/rejection decision utilizes all loan applications including the ones that are denied. Second, using mortgage performance as a proxy for screening is affected by strategic default which has nothing to do with lenders' screening incentives (Guiso et al. 2009, 2013). Lastly, mortgage performance reflects jointly the effect of screening incentives and changes in borrowers' economic conditions which is difficult to disentangle.

We identify the causal effect of the GSE activities on lenders' screening incentives by incorporating a regression discontinuity design (RDD) into a difference-in-differences framework. Specifically, we employ a regression discontinuity design (RDD) by exploiting a specific eligibility rule that restricts GSEs to purchasing mortgages with loan sizes below a threshold, known as the conforming loan limit (CLL). For loans above this limit, called jumbo mortgages, lenders either hold them in their own portfolio or securitize them privately but only with sufficiently high cost (Loutskina and Strahan, 2009). This arbitrary rule creates a discontinuous change in the eligibility of a loan for GSE purchase at the cutoff and allows the opportunity to identify the causal effect of GSE purchases on lenders' screening incentives. The identification assumption is that the function that relates the likelihood of a loan application to be accepted by a mortgage originator and the loan size should not have precisely the same 'jump' at the conforming/jumbo cutoff as the function that relates the eligibility of a loan for GSE purchase and loan size.

One critical assumption of a RDD is that the treatment assignment should be random at the cutoff. In other words, mortgage applicants should not be able to manipulate their treatment status – the eligibility of their loans to be purchased by the GSEs. If this assumption holds, those who just barely received treatment are comparable to those who barely did not receive treatment. However, if this assumption is violated, (i.e., if borrowers change the loan amount they initially decided to borrow), the observed 'jump' in acceptance rates at the cutoff can be caused by a 'jump' in borrower or loan characteristics. We include borrower and loan characteristics as control variables to rule out the possibility of selection on observables. Because there is no borrowers' credit score data in HMDA, we follow (LaCour-Little et al., 2011, 2014) and use Equifax zip code credit data as a proxy. To specifically address the issue that some wealthy borrowers may shift to the conforming loan pool to take advantage of lower interest rates by using a larger down payment, we examined a subsample of piggyback loans, because the piggyback loan borrowers, who take out a junior lien loan to finance more than 80 percent of the house value without paying private mortgage insurance, are usually not wealthy individuals.

To further address the concern that some unobserved borrower or loan characteristics associated with treatment manipulation can also affect acceptance rates, we incorporate a difference-in-differences approach that uses a quasi-natural experiment in 2009 when the GSEs largely increased their efforts in reviewing their acquired loans and took disciplinary measures against mortgage originators (i.e., the issuance of large scale mortgage buyback requests to the mortgage originators). Fannie and Freddie describe these actions as "an important step to restore discipline in the underwriting process".[6] This shock is apparently a consequence of the housing meltdown and the large volume of underperforming loans and is not "exogenous" at a first glance. However, this quasi-natural experiment is exogenous for our specific identification task. The shock directly affects the explanatory variable but does not directly affect the dependent variable other than through the channel of the explanatory variable of our interest. Section 4.2.2

[6] Timiraos, Nick. "Burdened by Old Mortgages, Banks Are Slow to Lend Now." The Wall Street Journal, October 3, 2012.

provides the detailed discussion on the validity of the identification strategy. In essence, we combine the difference-in-differences approach with the regression discontinuity design to address potential endogenous issues.

We first combine the difference-in-differences approach with a parametric method of a regression discontinuity design where we include multi-polynomial terms of the assignment variable – loan size. The dependent variable, acceptance rate, is the estimated acceptance rate for each small bin – equally spaced small increments (bandwidth) of the loan amount. All observable loan and borrower characteristics available in the HMDA data, including loan-to-income ratio, whether the applicant is female, minority, or Hispanic, whether the loan is for refinance or purchase, whether the property is owner occupied, and average zip code credit score as control variables. We find that, prior to year 2009, there is a discontinuous change in loan acceptance rate at the conforming/jumbo loan cutoff. Specifically, loan applications barely below the conforming limit are on average 8 to 9 percentage points more likely to be approved than those with similar risk profiles but are barely above the limit. The discontinuous change in approval rates at the conforming/jumbo cutoff is largely diminished after the 2009 shock. This finding suggests that the observed 'jump' in acceptance rates at the conforming/jumbo cutoff prior to the shock of 2009 indicates a negative effect of GSE purchases on lenders' screening incentives. When GSEs dramatically increased their efforts in reviewing acquired loans and issued a large volume of mortgage buyback requests to the mortgage originators in year 2009, the loosened screening by mortgage originators is largely diminished. The results hold when we use a dummy for whether a loan is approved as the dependent variable and include census tract fixed effects to capture the riskiness of owning a property in a specific neighborhood. The results are robust to different orders of polynomial terms of loan size included in the model (i.e. fifth-, and sixth-order), 1 or 2-year shock windows, and alternative bin size.

In addition, we combine a difference-in-differences approach with a non-parametric approach of a Regression Discontinuity Design where we narrow the sample to a small neighborhood around the jumbo/conforming loan cutoff. The advantage of this non-parametric approach is that since it does not require the assumption of a functional form between acceptance decision and loan size, the estimates on the discontinuous change in acceptance rate at the cutoff are not subject to model misspecification. Consistent with the estimates using the parametric approaches, we find that prior to year 2009, there is a 'jump' in acceptance rate at the cutoff and this discontinuous change in approval rates at the conforming/jumbo cutoff is largely diminished after the shock of 2009. The results are robust to using different widths of the neighborhood such as 90%-110%, and 95%-105% of the cutoff.

Some have argued that the Affordable Housing Goals (AHGs) have induced the GSEs to take excessive risk by expanding aggressively to the subprime and Alt-A mortgage market (Pinto, 2012). The Federal Housing Enterprise Financial Safety and Soundness Act of 1992 established three housing goals and mandates that a certain percentage of the mortgages purchased by the GSEs must be devoted to borrowers or neighborhoods with certain characteristics. For example, the Underserved Areas Goal (UAG) requires a certain share of the mortgages purchased by the GSEs in a given year to be originated for borrowers living in low-income census tracts or in high-minority tracts.[7] These goals may affect the activities of the GSEs in the secondary market, which may in turn affect lenders' behavior. To address the concern that the results are driven by the Affordable Housing Goals, we removed observations

[7] The other two housing goals are Low and Moderate-Income Goal (LMG) and Special Affordable Goal (SAG).

(roughly about 8% of the sample) that could be counted towards those goals and the results are virtually unchanged.

To our knowledge, this is the first study that examines how the GSEs' activities affect lenders' screening incentives in the prime market.[8] Complementing Keys et al. (2010) that documents the adverse effect of securitization practices on the screening incentives of subprime lenders, our study suggests an incentive distortion induced by the activities of two quasi-government entities in the prime market, a segment of the market that also experienced high delinquency rates. Among other related papers that investigate the moral hazard issues in the real estate market, Agarwal, Chang, Yavas (2012) document that in the prime market, banks generally sell low-default-risk mortgages to the secondary market while retaining high risk mortgage in their own portfolio prior to the crisis. This pattern reversed in 2007 as the crisis set in. Purnanandam (2010) shows banks with high involvement in the "originate-to-distribute" market during the pre-crisis period originated excessively poor quality mortgages. Jiang, Nelson, and Vytlacil (2011) find that loans originated by brokers have higher delinquency rates than those originated by banks, suggesting a misalignment of incentives for brokers who issue loans on a bank's behalf for commissions but do not bear the long-term consequences of low-quality loans.

The paper is organized as follows: Section 2 provides institutional background on the GSEs. Section 3 describes the data and the summary statistics. The econometric framework and the empirical results are described in Section 4 and 5, respectively. Section 6 conducts robustness checks. Section 7 concludes.

2. Institutional Background and the Research Question

[8] Several studies find that GSE mortgage purchases reduce mortgage yield (Kaufman, 2014; Ambrose, LaCour-Little, and Sanders, 2004; Naranjo and Toevs, 2002; Passmore, Sherlund, and Burgess, 2005).

The Federal National Mortgage Association (Fannie Mae) and the Federal Home Loan Mortgage Corporation (Freddie Mac) were government entities created by the Congress in the 1930s and 1960s, respectively.[9] The federal sponsorship of these two companies was one of the indirect ways through which the government provides housing finance and promotes home ownership.[10] Neither Fannie Mae nor Freddie Mac is permitted to originate mortgages themselves; instead, they purchase residential mortgages from the original lenders and hold them in their own portfolios, or package them, and sell the resultant mortgage-backed securities to investors. They also insure these securities against default by charging a guarantee fee. Over the years, Fannie Mae and Freddie Mac have become dominant players in fostering the development of the secondary mortgage market. We hypothesize that GSE mortgage purchases create a separation between mortgage origination and mortgage holding which leads to loosened screening standards by mortgage originators. Different from the private securitization practice where mortgage originators usually hold junior tranches as a mechanism to prevent moral hazard, the loans are completely off mortgage originator balance sheets when they are sold to the GSEs.

One may argue that since GSEs are sophisticated financial institutions, they should have set mechanisms and quality controls to prevent lenders from adverse selection. Why should moral hazard exist in equilibrium? We argue that moral hazard can exist for the following reasons. First, the mechanisms set by the GSEs may not be effective. The specific requirements set by the GSEs regarding what mortgages are eligible for purchase are based on only hard information such as FICO score, loan to value ratio (LTV), borrowers' income. Lenders, however, also base their lending decisions on soft information, i.e. borrowers' total

[9] See Frame and Lawrence (2005) for a general discussion of the GSEs.
[10] The arguments for the government support of housing include redistribution of wealth to lower-income households and positive externalities from owning a home (Aaronson 2000).

liability, employment history, and property occupancy (Stein, 2002 and Petersen, 2004). While hard information is easy to verify, soft information is not. Lenders thus have great leeway in deciding how much effort to put in collecting soft information. As an extreme case, lenders may not put any effort in collecting soft information if a loan is to be purchased by the GSEs.[11] Another disciplinary mechanism set by the GSEs is the framework of representations and warranties, which gives the GSEs the contractual right to return any mortgages if signs of negligence or misrepresentation are detected. However, GSEs rarely reviewed the loans they purchased upfront and instead relied on the automated-underwriting systems to decide on whether a loan is qualified.[12] This creates a time lag between loan origination/sale and any disciplinary actions enforced by the GSEs, which makes it possible for lenders to loosen screening at the time of loan origination.

Second, studies have shown that default rates are negatively associated with housing prices (i.e. Case et al. 1995). When housing prices continued to appreciate prior to the crisis, the perceived risks associated with those mortgages were low (Roberts, 2010; Demyanyk and Van Hemert, 2011). This may lower the GSEs' incentives to carefully screen mortgages since borrowers can always refinance a loan with an increased value of the house.[13] This loosened screening incentive is further exacerbated by the fact that the GSEs are backed by the government. Before the GSEs were placed into conservatorship in 2008, they were quasi-

[11] Berg et al. (2012) suggest that even hard information can be manipulated. A study by Fannie Mae shows that the most common areas of lender's misrepresentation for mortgages sold to GSEs are borrowers hiding liabilities, overstating income, and misclassifying property occupancy.

[12] Prior to 1982, Fannie Mae and Freddie Mac hired a staff of underwriters to re-underwrite every single loan they purchased; this situation changed later with the GSEs reviewing a sample of loans after funding the mortgages and lenders assuming the chief responsibility for mortgage risk evaluation. See Straka (2000).

[13] Even if the GSEs correctly anticipated a downturn in the housing market, some outside forces may still prevents them from taking the correct actions, a term coined irrational exuberance. In an interview with Financial Times in 2007, for example, Chuck Prince, the CEO of Citigroup described this situation. "When the music stops, in terms of liquidity, things will be complicated. But as long as the music is playing, you've got to get up and dance. We're still dancing". See http://www.ft.com/cms/s/0/80e2987a-2e50-11dc-821c-0000779fd2ac.html#axzz298uv1UB0

government entities: they were private companies on one hand but enjoyed an exclusive federal charter that gave them implicit government backing on the other.[14] This implicit government backing could lessen the GSEs' incentives to adequately screen the acquired loans, leading to a lack of market discipline on the moral hazard problems of mortgage originators. Investors of mortgage-backed securities (MBS), although the ultimate holders of these risky-assets, would also care less about the quality of mortgages they invested in since the mortgages are guaranteed by the GSEs.

Lastly, GSEs themselves also benefit greatly from the securitization business. When lenders who rely on the GSEs to replenish their loanable funds are also valuable clients of the GSEs, it creates a distortion in GSE incentives. Anecdotal evidence indicates that GSEs competed with the Wall Street firms for the securitization business. As competition became fierce, it is not surprising that GSEs would loosen their underwriting standards to gain business and lenders managed to take advantage of this loophole.

3. Data and Summary Statistics

3.1. Sample Selection

The main source of our data is the Home Mortgage Disclosure Act (HMDA) database for loan applications and originations and the sample period is 2006-2011.[15] The Federal Reserve collects these data for the purpose of detecting discriminatory lending practices. All regulated financial institutions (e.g., commercial banks, savings institutions, credit unions, and mortgage companies) with assets above $30 million must report.

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[14] The implicit (now it is explicit) government backing gives GSEs a special funding advantage that allows them to borrow at a cost only slightly higher than what is paid by treasury (Frame and White 2005), and part of this subsidy is passed on to homebuyers in the form of slightly favorable interest rates in the conforming market relative to the jumbo market.

[15] We use a symmetric 6-year window with 3 years prior and 3 years after the 2009 shock. This shock is discussed in Section 4.2.2.

The HMDA database is an annual database that covers individual mortgage applications starting from 1990 with information on loan, borrower, and property characteristics. For example, it provides information on loan size, loan type (i.e., whether it is conventional or Federal Housing Administration insured), loan purpose (i.e., home purchase, home improvement or refinancing), whether or not the application is approved, denied, or withdrawn, borrower annual income, borrower gender, ethnicity, race, property type (i.e., whether it is one to four-family or multifamily), and location (census tract), etc.

Our sample is limited to conventional mortgages, excluding the Federal Housing Administration (FHA), the Veterans Administration (VA), and other government program loans because the underwriting standards for these special programs are different from those of conventional loans. Loans in the category of conventional mortgages make up the majority of the observations in the database. We also restrict our sample to home purchase and refinance loans for one-to-four-family properties. We keep mortgages originated by financial institutions reporting to the FDIC, FR, and OCC (mostly commercial banks). Since our focus is on the prime market, we drop subprime mortgages originations identified as spread-reportable loans in HMDA.[16] We also drop observations from four statutorily designated high cost areas: Alaska, Hawaii, Guam, and the U.S. Virgin Islands. These areas carry a much higher conforming loan limit. We drop observations with missing characteristics such as loan size. We restrict our sample to loan applications that are approved, approved but not accepted by applicants, or denied.[17]

[16] Before 2004, subprime mortgages are typically identified using the U.S. Department of Housing and Urban Development (HUD) subprime lender list. Starting in 2004, lenders are required to report loans in which the annual percentage rate (APR) exceeds the rate on the Treasury securities of comparable maturity by three percent for first lien and five percent for junior lien. The spread-reportable loans are widely used as a proxy for subprime loans and are found to coincide closely with subprime loans identified through the HUD list (Dell'Ariccia et al. 2012).

[17] Other statuses include applications withdrawn by applicants, file closed for incompleteness, loan purchased instead of originated by the institution, preapproval request denied by financial institutions, and preapproval

One common criticism about HMDA data is its lacking of borrower credit risk controls, such as credit scores. To mitigate this shortcoming, we matched HMDA loan-level application data with the Equifax zip code credit data, which is aggregated from the Equifax National Consumer Database and provides the distribution of consumer credit scores in each zip code. Following LaCour-Little et al. (2011, 2014), we match HMDA census tract numbers, MSA and county numbers with zip codes using a database from Missouri Census Data Center.[18]

3.2. Measure of Screening Incentive

3.2.1. Loan Acceptance Rate

Following Loutskina and Strahan (2011), we use the acceptance/rejection rates of mortgage applications by the lender as a measure of the lender's screening incentive.[19] In comparison with the commonly used loan quality (e.g., mortgage delinquency rate) as a measure for screening incentive, our measure offers several advantages. First, mortgage delinquency rate is only available for the loans that are accepted. Thus analysis using mortgage delinquency rate utilizes only a subset of all loan applications-the loans that are accepted. In contrast, our measure utilizes all loan applications including the ones that are denied. Second, other factors such as strategic default can affect mortgage performance but has nothing to do with lenders' screening incentives. A study by Guiso et al. (2009) finds that 26% of the existing defaults are strategic. Lastly, loan performance reflects jointly the effect of screening incentives and changes in borrowers' economic conditions. Loan performance is usually measured by whether a loan becomes delinquent within certain periods (usually 10, 15, or 24 months) after origination. Lenders, however, conduct the due diligence at the time of mortgage origination.

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request approved but not accepted. The results remain virtually unchanged when we include the preapproval requests denied by financial institutions.

[18] http://mcdc2.missouri.edu/websas/geocorr2k.html

[19] This measure is also used by Dell'Ariccia, et al. (2012) who find that loan denial rate is negatively related to credit demand.

This creates a time lag between a lender's screening and the observed mortgage outcome. Therefore, a delinquency may not indicate the quality of the underwriting but is driven by changes in the borrower' economic situation such as a layoff. For example, when the economy is experiencing a downturn and people employed in one industry may be hit harder than another. The delinquency of a loan thus can be caused by the fact that the borrower's repayment capacity is negatively affected by the economy downturn, and may not indicate the quality of the underwriting. Given that our sample period overlaps the crisis period, the analysis based on mortgage performance would be less reliable. Acceptance rate instead, overcomes these shortcomings by using information exactly at the time of mortgage origination and thus is more appropriate in the context of our study.

Although this measure offers some advantages, it also has some drawbacks. For example, acceptance rate is affected by the funding conditions of the banks. We address this concern by controlling for banks' capital and liquidity in the robustness check section of the empirical tests.

3.2.2. Loan Performance

We also conduct a zip code loan performance analysis by examining the zip code delinquency and default rates in relation to lenders' screening incentive and its changes after 2009. The delinquency rates are measured as the proportion of loans in a zip code being in a delinquent state (at least thirty or sixty days behind payment). The default rates are the proportion of loans that are at least ninety days behind payment. The loan performance data is from Equifax. The HMDA loan level borrower and loan characteristics are first aggregated to the census tract level by taking the average, and then matched to the zip code loan performance data using a database from Missouri Census Data Center. Because a given census tract can correspond to more than one zip codes, we create a weight variable based on the share of housing units in each census tract that lie within a given zip codes, similar to LaCour-Little, et al (2011, 2014).

3.3. Summary Statistics

Table I provides the summary statistics for the mortgage applications with loan sizes between 85% and 120% of the conforming loan limit.[20] This is the sample that our empirical tests are based on and our results also hold when we use the entire sample. The total number of mortgage applications is about 3,861,797 in the 2006-08 period and decreases to about 2,947,083 in the 2009-11 period. The average mortgage acceptance rates for conforming and jumbo loans are 79% and 71%, respectively, during 2006-08. During 2009-11, the average mortgage acceptance rates for conforming and jumbo loans are slightly higher, being 84% and 82%, respectively. The observed decrease in the number of mortgage applicants and increase in acceptance rates for both conforming and jumbo mortgages may reflect the fact that with the widely tightened screening in the lending industry after the housing meltdown the poorer quality applicants have been deterred from applying a loan. As a result, the applicant pool is of better quality after year 2008. As for borrower characteristics, both the average income and the average loan-to-income ratio are greater for jumbo than conforming mortgage applications. The proportion of minority borrowers are higher for jumbo than conforming mortgage applicants, while the proportions of female borrowers are similar in the jumbo and the conforming mortgage applicant pool.

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[20] The national conforming loan limit for mortgages that finance single-family one-unit properties has been climbing steadily overtime. Starting from $33,000 in the early 1970s, it has increased steadily to $417,000 *for one-family housing for 2006-2016*, with limits 50 percent higher for four statutorily-designated high cost areas: Alaska, Hawaii, Guam, and the U.S. Virgin Islands. The determination of the limit is based on the October-to-October change in the average house price in the Monthly Interest Rate Survey (MIRS) of the Federal Housing Finance Board (FHFB) so the steady increase in the limits reflects the country-wide appreciation in real estate market during the past several decades. In an effort to boost the battled housing market during the crisis, the Congress passed the Housing and Economic Recovery Act (HERA) in 2008, which designated certain areas as high-cost and increased the conforming loan limits for these areas, thereby making the limits for these areas higher than the general areas. We adjust these increases in conforming limits in our empirical analysis.

Figure 1 provides a graphic presentation of how the acceptance rate changes discontinuously around the conforming/jumbo loan cutoff. We first normalize each loan amount by its corresponding conforming loan limit with one indicating the normalized cutoff. We then group the normalized loan amount into a number of small non-overlapped bins. The approval rate which is the fraction of loans that are approved in each bin is graphed against the mid-point of the bin. Also shown in the figure is the fifth-order polynomial regression generated fits of the acceptance rates created separately on either side of the cutoff. The figure reveals a sharp discontinuous increase in the approval rates for the loans just below the cutoff relative to the loans just above. The magnitude of the discontinuity appears to be much larger before 2009 than that after 2009. The discontinuity in the acceptance rate at the cutoff and the large decrease in the magnitude of the 'jump' in the acceptance rates in year 2009 are consistent with the argument of a negative effect of GSE purchases on lenders' screening incentives prior to the 2008 mortgage meltdown.

4. Empirical Test Design

4.1. Regression Discontinuity Design

As mentioned earlier, the GSEs operate in a regulatory regime that restricts them to purchasing mortgages conforming to certain underwriting guidelines (conforming mortgages). One specific rule requires that origination balances be below a specific amount. The GSEs buy most but not all conforming mortgages, but they may not buy any of the jumbo mortgages, the mortgages with loan amounts above the threshold.[21] This arbitrary rule creates a discontinuous

[21] For conforming mortgages, while some lenders may hold some loans in their portfolios, the bulk of the production (especially in fixed-rate products) is securitized by the GSEs and sold into the capital markets. In contrast, most jumbo mortgages are held by the original lenders or securitized as private-label mortgage backed securities. The costs associated with securitizing jumbo mortgages are relatively high compared to those associated with conforming mortgages (Fabozzi, handbook of mortgage-backed securities, 6th ed., p20; Loustina and Strahan, 2009).

change in the eligibility of a loan for GSE purchase at the cutoff and allows the opportunity to identify the causal effect of GSE purchases on lenders' screening incentives.[22] The identification assumption is that the function that relates the likelihood of a loan application to be accepted and loan size should not have precisely the same 'jump' at the conforming/jumbo cutoff as the function that relates the eligibility of a loan for GSE purchases and loan size.

Intuitively, how GSE purchases induce lax screening by lenders can be understood through an example. Consider two mortgage applications, A and B, which have identical hard information (i.e., credit score, LTV, income, etc.). A's loan amount exactly equals the conforming loan limit, S_0, and B's is slightly larger, $S_0+\varepsilon$ ($\varepsilon > 0$). For A's application, as long as the hard information satisfies the GSE requirements, the lender accepts it and need not invest in the soft information. In contrast, since B's mortgage cannot be sold to GSEs, the lender exerts more effort in collecting soft information. In other words, lenders will base their approval decision for B's application on a broader set of information. The consequence is that B, although having identical hard information as A, may not be approved if the soft information revealed is unfavorable.

To formalize this idea, let S be the loan size and S_0 be the conforming loan limit. Let the treatment status D be a binary variable, with D = 1 for conforming mortgages and D = 0 otherwise. It follows that $D = 1(S <= S_0)$. Using the experimental terminology, let Y_{1i} and Y_{0i} be the two potential outcomes for unit *i* for the treated and the control groups, respectively. In our context, it refers to the acceptance decision corresponding to the applications for the conforming and jumbo mortgages, respectively. The average treatment effect of GSE purchase

[22] Here we loosely refer to loans below the conforming loan limits as conforming mortgages, although this rule is not the only one that determines a mortgage's conforming status. Nevertheless, it does create an exogenous variation in a mortgage's eligibility status: loans below the threshold have a higher unconditional probability of being eligible than loans just above the threshold.

on acceptance rate is then defined as the difference between these outcomes, $\rho \equiv E\ (Y_{1i}\text{-}Y_{0i}|S_i)$. This average treatment effect, however, is not practically identifiable, because there is no value of S_i at which we get to observe both the treatment and control observations; nevertheless, it can be shown that the local average causal effect can be retrieved for those mortgages around the conforming/jumbo cutoff by considering the following equation[23]:

$$\theta^{sharp} = \lim_{s \uparrow s_0} E\left[Y_i \middle| S_i = s\right] - \lim_{s \downarrow s_0} E\left[Y_i \middle| S_i = s\right] \tag{1}$$

Notably, this model identifies the difference in acceptance rate for mortgages marginally above and marginally below the cutoff.

To estimate model (1), we specify the following polynomial equation that is typically used in the literature on regression discontinuity design (RDD).

$$rate_i = \alpha + \rho D_i + \sum_{p=1}^{m} \beta_p s_i^p + \sum_{p=1}^{m} \theta_p D_i s_i^p + \eta_i \tag{2}$$

Here $rate_i$ is an estimated acceptance rate for small bins equally spaced around the jumbo loan cutoff, D_i is an indicator variable that takes the value of 1 for conforming mortgages and 0 otherwise, p is the order of the polynomial terms, and s_i is our assignment variable, the normalized loan amount. This variable is re-centered in the model such that s = 1(the normalized cutoff) corresponds to "0". [24] Thus, ρ, our coefficient of interest, provides a direct estimate of the effect of GSE purchases on mortgage lender's screening incentives. As mentioned earlier, the interpretation of this coefficient should be made locally in an arbitrarily narrow window of loan amounts around the jumbo/conforming loan cutoff. The argument is that, within this interval, the unobserved factors related to loan and personal characteristics are

[23] See Lee (2008) for a formal treatment of how the discontinuity induces treatment status that is "as good as random" in the neighborhood of the cutoff.
[24] Re-center is necessary in this specification. It ensures that the coefficient on the treatment status dummy D, in a regression model with interaction terms, represents the treatment effect at the conforming/jumbo cutoff.

likely to be similar so that observations just above the cutoff provide a comparison group for observations just below. β_p and θ_p represent the coefficients on the flexible polynomial terms and the interaction of the polynomial terms and the conforming loan indicator, respectively. The interaction allows the shape of the underlying conditional expectations to be different above and below the threshold.

Table II presents the RDD estimates of the acceptance rate differential (ρ) based on Equation (2) for each year from 2006 to 2011. This allows us to exclude the possibility that the results are driven by some specific vintage of mortgages. In all these regressions, we include both borrower and loan characteristics such as the average zip code credit score, female, minority, Hispanic, Loan-to-Income ratio, refinance, owner occupancy dummies as control variables. As we can see from the upper half of Panel A in the Table, where the dependent variable is the mortgage application acceptance rate estimated in each small bin with bin size of 0.005, the acceptance rates for mortgages just below the conforming loan limits are significantly higher than mortgages just above the limits for years 2006 to 2008. In year 2008, for example, the acceptance rates for mortgages just below the limits are 6.4 percentage points higher than mortgages just above the limits. These results provide an initial evidence of the moral hazard in the mortgage generating process in the conforming loan markets (we deal with the selection issue in subsequent sections). Another noteworthy point from the Table is that there is an evident time trend in the RDD estimates: the acceptance rate differentials peaked in 2007, before they finally dropped to a level that are statistically non-distinguishable from zeros in 2009 and 2010. This striking difference in acceptance rates between conforming and jumbo mortgages around the cutoff is consistent with Figure 1. One possible explanation for this trend, which is to be elaborated in the following section, is that as the GSEs started to increase their efforts in reviewing the acquired loans in 2009, lenders are forced to screen the conforming loan borrowers more diligently, compared with the previous years.

Although our analysis focuses on Equation (2), model (1) can also be estimated using other approaches. For example, ρ can be estimated by slightly modifying Equation (2) using a dummy for whether a loan is approved as the independent variable. It can also be estimated non-parametrically by comparing the mean of Y across the cutoff using observations with loan amounts within a small distance from the cutoff. Table II also present results based on these alternative approaches in the lower half of Panel A (*approved* dummy) and in Panel B (non-parametric), and the results are largely consistent with the previous findings.

4.2. The Difference-in-Differences Approach

4.2.1. Sorting Issues (self-selection)

One critical assumption of a RDD is that the treatment assignment should be random at the threshold. In other words, mortgage applicants should not be able to manipulate their treatment status – the eligibility of their loans to be purchased by the GSEs. If this assumption holds, those who just barely received treatment are comparable to those who barely did not receive treatment. However, if this assumption is violated, the observed 'jump' in acceptance rates at the cutoff can be caused by a 'jump' in borrower or loan characteristics.

There is a possibility that some mortgage applicants may have sorted to the conforming loan applicant pool. If the attributes of those applicants who self-sorted into the treatment group are uncorrelated with the outcomes of interest, the estimator based on Equation (2) is still unbiased. However, if the applicants who sorted from the jumbo to the conforming mortgage pool are the ones with better or worse qualifications, the estimator based on Equation (2) is biased. Applicants can sort to the conforming loan pool for various reasons. Previously, we have included borrower and loan characteristics such as the borrowers' income as control variables to alleviate the problem of selection on observables. Borrowers, however, can sort to the conforming loan pool on variables that are not available in HMDA and Equifax. As one example, if a borrower, originally planning to get a jumbo mortgage, is convinced by the lender that he/she could get a conforming loan with a slightly lower interest rate by making a slightly larger down payment, he/she, if wealthy enough, will have strong incentive to do so. If sufficiently wealthy borrowers make the switch, this could cause a discontinuously 'jump' in the wealth level at the conforming loan limits. Thus, the estimated acceptance rates differentials in Table II may be caused by these 'jumps' in wealth level, rather than evidence of loosening screening by lenders.

In order to examine this possibility, we examine whether the jumps exist for piggyback loans. Piggyback loans, technically referred to as simultaneous close seconds, are junior lien mortgage loans taken out concurrently with the first mortgage to finance the home purchase. These are generally used by homebuyers to finance more than 80 percent of the house value without paying private mortgage insurance. Borrowers who took out a junior lien loan are typically not wealthy individuals. To identify piggyback loans in HMDA, we follow the method proposed by Avery (2007).[25] If the results hold for the piggyback subsample, the discontinuous jump in the mortgage acceptance rate is unlikely to be driven by the borrower wealth effect. Results on piggyback subsample, which are largely consistent with those in Table II, are reported in Table III.

[25] To identify piggyback loans in HMDA, we follow the method proposed by Avery (2007). We separate the home purchase loan data into two samples. The first sample includes all junior-lien purchase loans and the second sample includes all first-lien purchase loans. We then match the second sample to the first sample by census tract, lender ID, owner occupancy status, borrower income, race and ethnicity, and sex. If there is a match, then the matched junior-lien loan is identified as piggyback loan for the corresponding first lien.

4.2.2. The Exogeneity of the Quasi-natural Experiment

To further address the concern that some unobserved borrower or loan characteristics that are associated with treatment manipulation can also affect acceptance rates, we incorporate a difference-in-differences approach that utilizes a shock in 2009 when the GSEs considerably increased their efforts in reviewing the acquired loans and took disciplinary measures against the mortgage originators.

The mortgage meltdown highlighted the weaknesses of the automated underwriting systems that Fannie and Freddie had relied on to screen the acquired loans since the mid-1990s. The automated systems allowed lenders to enter standardized items, such as FICO score, income, and loan amount, to receive faster approvals or denials. Fannie and Freddie rarely reviewed the loans they purchased upfront.[26] When the default rates on mortgages reached their historically high levels in 2007 and 2008, Fannie Mae and Freddie Mac started to conduct detailed review of loan files and to take disciplinary measures to minimize losses. In 2009, through a framework called representations and warranties (reps & warranties) which gives the GSEs the contractual right to return a mortgage if violations are found in the loan applications, the GSEs started to request mortgage originators to buy back a large number of mortgages originated during the housing boom. In an October 3, 2012 Wall Street Journal article, Fannie and Freddie describe these actions as “an important step to restore discipline in the underwriting process”:

Fannie's chief executive, Timothy Mayopoulos, compared the loan assembly line to an auto maker's in an August interview. Car manufacturers have developed ‘very exacting standards’ so that a repeatable process produces ‘the same thing day--in and day--out,’ he says. The

[26] Passmore and Sparks (2000) posit that the automated system creates adverse selection from mortgage originators.

lending process still isn't exacting, he says, which is why lenders have been forced to tighten lending standards.

To expedite the mortgage review process, the GSEs hired auditors to check for signs of violations such as undisclosed debt, inaccurate appraisals, or fake income or employment data of delinquent mortgages made between 2005 and 2008. By 2012, Fannie and Freddie had asked banks to repurchase $66 billion in mortgages made between 2005 and 2008. Because most of these returned loans were non-performing, lenders suffered great losses when they bought them back. According to Inside Mortgage Finance, the estimated total mortgage expense for repurchased non-performing loan reached over 8 billion dollars in 2009. The types of lenders that had received repurchase demands were diverse. Among the national banks that were affected were Bank of America, JPMorgan Chase & Co., Citigroup Inc., US Bancorp, Wells Fargo & Co., and Fifth Third Bank. The loan applications in these banks account for the majority of the data in HMDA.

The event that the GSEs largely increased the efforts to review purchased loans and took disciplinary measures against mortgage originators in 2009 is not "exogenous" at a first glance. The event is apparently a consequence of the housing meltdown and the large volume of underperforming loans. However, this quasi-natural experiment is exogenous for our specific identification task. Our hypothesis is that the GSE mortgage purchases *combined* with a lack of reviewing efforts and disciplinary mechanisms by the GSEs lead to loosened screening by banks. Note that our explanatory variable is not just GSE mortgage purchases and we do not hypothesize that GSE mortgage purchases alone lead to loosened screening incentives by banks. Since this 2009 event (i.e. GSEs increased its reviewing efforts) directly affects the explanatory variable, the condition- "lack of reviewing efforts and disciplinary mechanism by the GSEs" is no longer satisfied after the event. In other words, this 2009 event constitutes a shock to the value of the explanatory variable. In order to be exogenous for our identification purpose, the second condition is that the shock should not directly affect the dependent variable other than through the explanatory variable of our interest. This condition is clearly satisfied as well: the increases in the reviewing efforts by the GSEs should not *directly* affect banks' screening incentives. It can affect banks' screening incentives only through our proposed channel-because the GSEs increased its reviewing efforts and took disciplinary efforts, banks can no longer sell poor quality loans to the GSEs without being held accountable.

4.2.3. Combining the Difference-in-Differences Approach with the Regression Discontinuity Design

We combine the difference-in-differences approach with the regression discontinuity design to address potential endogenous issues. Specifically, we employ a modified model based on equation (2):

$$rate_i = \alpha + \rho D_i + \delta after_i + \gamma D_i * after_i + \sum_{p=1}^{m} \beta_p s_i^p * after_i + \sum_{p=1}^{m} \theta_p D_i s_i^p d * after_i + \eta_i \qquad (3)$$

where $after_i$ is a dummy for years after 2008. Our parameter of interest is γ. It captures the difference in lenders' differential screening incentives of conforming vs. jumbo mortgages between the year 2006-08 and 2009-11.[27] In essence, 2006-08 serves as our testing period and 2009-11 as the benchmark period. For reasons discussed earlier, we expect the sign of this parameter to be negative. Note that in the model we fully interact the $after_i$ dummy with polynomial terms to allow for polynomial functions to have different shapes for different periods.

[27] We also used 2007 as the testing year and the results are qualitatively unchanged.

5. Empirical Results

Table IV, Panel A presents a univariate analysis of whether the difference in lenders' screening incentives between conforming and jumbo mortgages changed from 2006-08 to 2009-11, using observations with loan amounts around 90% - 110% of the conforming loan limits. During 2006-2008, the conforming mortgage is 8.1 percentage points more likely to be approved than the jumbo mortgages at the cutoff. Consistent with our conjecture that lenders' loosened screening on loans eligible for GSE purchases should be largely reduced after the GSEs increased efforts in reviewing loans, this difference in acceptance rates at the cutoff is reduced to 4.2 percentage points in 2009-11. The difference between these two conforming-jumbo mortgage acceptance rate spreads provides an estimate of the GSE purchase effect of 0.039, which is statistically significant at the 1% level. As shown in Panel B, the results are similar when we use a smaller width of neighborhood around the cutoff, 95% to 105% of the conforming limit. These estimates, however, are the baseline results since the tests do not control borrower and loan characteristics.

Panel A of Table V reports the estimates of the method that combines a regression discontinuity design and a difference-in-differences approach. The sample period is from 2006 to 2011. The sample includes mortgage applications in both general and high-cost areas except the four statutorily designated states. Models (1) to (2) use parametric regression discontinuity models, as described in Equation (3), with fifth-, and sixth- order polynomial terms of loan size, respectively. The dependent variable is the mortgage application acceptance rate estimated in each small bin with bin size of 0.005. 'Conform' is a dummy variable that is equal to 1 if the loan amount is less than the conforming loan limit, and is equal to 0 otherwise. 'After' is a dummy variable that is equal to 1 if a mortgage application is originated after 2008, and is equal to 0 otherwise. All observable loan and borrower characteristics, such as loan-to-income ratio, whether the applicant is female, minority, or Hispanic, whether the loan is for refinance or purchase, whether the property is owner occupied, and the average zip code credit score, are included as control variables. The samples include applications with loan sizes within 85%-120% of the conforming loan limits. The results also hold when we include the entire sample.

The coefficients on the conforming dummy are positive and statistically significant for models (1) and (2). For example, the coefficient on the conforming dummy is 0.090 in model (2), which suggest that prior to year 2009, there is a discontinuous 'jump' in loan acceptance rates at the conforming/jumbo loan cutoff. Specifically, loan applications barely below the conforming limit are on average 9 percentage points more likely to be approved than those with similar risk profiles but are barely above the limit. Furthermore, the coefficients on the interaction term between the conforming dummy and the after dummy are negative and statistically significant for models (1) and (2). For example, the coefficient is -0.041 ($p<0.01$) in model (2), which suggests that the difference in the acceptance rates at the conforming/jumbo cutoff is largely diminished after the shock in the GSEs' screening efforts. The results are robust to using an alternative bin size (unreported). Since if applicants have incentives to sort to the conforming mortgage pool in 2006-08, they should be doing so in 2009-11 as well, these results confirm that prior to 2009, the difference in the acceptance rates at the cutoff can't be entirely driven by potential sorting by the applicants around the cutoff. Instead, it indicates a negative effect of GSE purchases on lenders' screening incentives. These estimation results are consistent with our observations in Figure 1: there is a significant 'jump' in acceptance rate at the conforming/jumbo cutoff in years 2006-08 and the magnitude of the 'jump' is largely diminished after the shock in the GSEs' screening efforts.

Models (3) and (4) differ from models (1) and (2) in that the dependent variable for models (3) and (4) is a dummy variable indicating whether a loan is approved and each loan application is one observation in the regressions. The models are estimated using the Linear Probability Model.[28] We also include census tract fixed effects to capture the riskiness of owning property in the neighborhood. Avery et al. (1996) suggest that lenders prefer to lend to areas with solid housing prices to reduce the cost when borrowers default. The results from this set of regressions are qualitatively similar to those of models (1) and (2).

Models (5) and (6) utilize non-parametric regression discontinuity models where samples are narrowed to within a small neighborhood around the conforming/jumbo loan cutoff. Specifically, the samples for models (5) and (6) are observations with loan sizes of 90%-110%, and 95%-105% of the conforming loan limits, respectively. The advantage of this non-parametric approach is that since it does not require an assumption of the functional form between acceptance decision and loan size, the estimates on the discontinuous change in acceptance rates at the cutoff are not susceptible to model misspecification. Consistent with the estimates using the parametric approaches, we find a 'jump' in acceptance rates at the cutoff in year 2008 and the magnitude of the 'jump' is largely reduced after the shock in the GSEs' screening efforts.

In Panel B, we exclude observations that could be counted towards the Affordable Housing Goals (AHGs) to investigate whether the observed 'jump' in the acceptance rate at the cutoff is driven by the fact that GSEs need to meet the Affordable Housing Goals. The Federal Housing Enterprise Financial Safety and Soundness Act of 1992 established three housing goals and it mandates that a certain percentage of mortgages that GSEs purchase must be devoted to

[28] They can also be estimated by logit or probit model. Given the large number of observations and variables, however, it is more practical to apply the linear probability model which provides qualitatively similar and consistent results.

borrowers or neighborhoods with certain characteristics. These housing goals include the Low- and Moderate-Income Goal which targets borrowers earning no more than the area median income, the Underserved Areas Goal which targets borrowers residing in lower income areas or higher minority areas, and the Special Affordable Goal which targets borrowers earning no more than 60 percent of the area median income or residing in low-income census tracts and earning no more than 80 percent of area median income. We drop observations that satisfy the criteria set in these three goals. There is about 8% of the sample that are removed as a result. As shown in Panel B of Table V, the results are qualitatively similar to the results in Panel A. This suggests that the higher acceptance rate for conforming loans at the cutoff prior to the shock that the GSEs increased screening efforts is not driven by the fact that GSEs need to meet the Affordable Housing Goals.[29]

To alleviate the concern that the discontinuously jump is driven by the wealth effect of the borrower, we test equation (3) using the piggyback subsample in Panel C of Table V. The results still hold. Detailed discussions of piggyback loans are provided in Section 4.2.1.

We further supplement our acceptance/rejection analysis by conducting zip code level loan performance regression in Table VI. The dependent variables are the zip code 30-day, 60-day first mortgage delinquency rates, and default rates. The independent variable *Conform (%)* is the proportion of conforming mortgage in a zip code. The control variables are zip code average loan-to-income ratio, percentage of female, minority, or Hispanic borrowers, percentage of refinance loans, percentage of owner-occupied properties and, and average zip code credit score. We match Equifax zip code loan performance data to the HMDA aggregated zip code

[29] One may also argue that the higher acceptance rates for the conforming loans are evidence that the GSEs provide liquidity to the market. In other words, the borrowers, who would not otherwise get approved due to banks' lack of liquidity, are able to get loans because of the GSEs' functions. However, this liquidity explanation, even if it plays a role, can't explain the results that the difference in acceptance rates at the cutoff is largely diminished after year 2008.

loan application data with one year lead so that loan performance in year *t* corresponds to loan application in year *t-1*. The procedure of aggregating the HMDA data was detailed in Section 3.2.2. The results show that, the coefficient on variable *Conforming (%)* is positive and significant at 1% level, indicating that, prior to 2009, zip codes with more conforming loan applications are associated with worse future loan performance. Again, consistent with the previous results when acceptance rates are used as the dependent variable, we found that the zip code delinquency and default rates are diminished after 2009, suggesting a better loan quality associated with improved GSE screening efforts after 2009.

Last, we conduct permutation tests where we vary the location of the discontinuity at randomly selected loan amounts. If discontinuities exist at points other than the cutoff, our interpretations of the discontinuity identified at the conforming cutoff would be questionable. In unreported tables, we find that there are no discontinuities at the randomly selected points prior to the shock that the GSEs increased screening efforts. The results from this exercise confirm our argument that the 'jump' in acceptance rates at the cutoff is due to the effect of GSEs.

6. Alternative Explanations and Robustness Check

6.1. Controlling for Banks' Financial Conditions

The mortgage acceptance rates also reflect lenders' willingness to supply credit which can be affected by lenders' financial conditions such as capital adequacy, availability of liquid assets held and cost of borrowing (Loutskina & Strahan, 2009). Thus, the finding that jumbo mortgages carry a lower acceptance rate could be caused by scarcity of liquid assets or high borrowing costs associated with originating banks. In addition, banks that don't have the capacity to fund a jumbo loan may convince the borrower to get a non-jumbo mortgage which banks will immediately sell and collect the origination fee. Loutskina and Strahan (2009) show that financially constrained banks see more non-jumbo loan applications. Since only the approved borrowers (potentially) are more likely to be persuaded to switch from jumbo to conforming market, the acceptance rates are higher for the conforming loans. If banks have more spare funding capacity after 2008, banks' incentives to push borrowers to the non-jumbo loans can be less strong, which leads to lower jumbo/conforming loan acceptance rate spread.[30] To rule out this alternative explanation, we include banks' financial conditions as additional control variables in this section.

To obtain data on these bank level variables, we merge the HMDA loan application data with the *Reports of Income and Condition* for commercial banks (also known as the *call report*).[31] Following Purnanandam(2010), we measure liquidity as the sum of cash, federal funds sold, and government securities (U.S. treasuries and government agency debt) held by the banks divided by total asset. The cost of borrowing is measured as the ratio of interests on deposits to total deposits. We also include capital ratio, defined as equity capital divided by total assets and log total asset as additional controls. All bank level variables are measured at the end of previous year. The results based on these additional bank level controls are reported on Table VII. As shown in the table, the results are similar to those reported in Table V.

6.2. Controlling for Bank Fixed Effects

While the above analysis controls for observable lender characteristics, lenders, however, are different in unobservable ways (i.e., management team, culture, lending practice, etc.). For example, some lenders may be less strict in approving loans than the others due to lender

[30] Despite the conventional wisdom, the lending opportunity set for banks has shrunk dramatically after 2008 and banks were sitting on capital in search of new projects to fund.
[31] All FDIC insured commercial banks are required to file the call reports with their regulators on a quarterly basis. See Purnanandam (2010) for detailed description of this data and the data merging process.

specific reasons. Thus if we compare the approval rates between conforming and jumbo mortgages without controlling the differences across lenders, a significant result could be driven by the deficiency in empirical design rather than the existence of moral hazard among lenders.[32]

Ideally, we could address this issue by simply including lender fixed effects in our specification in addition to observable financial conditions. However, because there are over 4000 different lenders in our sample, it is technically infeasible to run a regression with a few thousands independent variables. For this reason, we only focus on five big national banks including Bank of America, US Bank, Chase Manhattan Bank, Wells Fargo Bank, and CITI Bank when controlling for bank fixed effects. The number of observations from these banks is over 1.6 million and accounts for more than one third of the total sample. The results based on the five banks are presented in Table VIII. Again, we obtain similar results.

7. Conclusion

Fannie Mae and Freddie Mac are two important government sponsored enterprises in the secondary residential mortgage market. The rescue of these two companies in the 2008 financial crisis was arguably the largest and costliest government bailout ever in the U.S history: so far it has cost taxpayers several hundred billion dollars to keep the GSEs solvent and operating[33]. Regarding the causes of their failure, existing literature focuses on their poor investment decisions (i.e., holding of subprime and Alt-A mortgage backed securities). While obviously important, this is not a complete story regarding the role the GSEs played in the crisis. Taking a different perspective, this research studies how the GSE purchases of mortgages in the secondary market affect mortgage lenders' screening incentive in the prime market.

[32] For example, if lender A and B have an approval rate of 0.8 and 0.7, respectively, for both conforming and jumbo mortgages, then comparing the approval rates across lender A and B could yield a conforming-jumbo rate spread of 0.1 even when there is no moral hazard in the lenders.
[33] Source: http://stimulus.org

Exploiting a special rule that mortgages with loan amounts above a certain threshold are not eligible for purchases by the GSEs and an unprecedented large scale of mortgage buyback requests from the GSEs in 2009, we document a causal link between GSE mortgage purchases and lenders' screening incentive. Employing a method that combines a regression discontinuity design (RDD) and a difference-in-differences approach, we find that, prior to year 2009, loan applications barely below the conforming/jumbo cutoff are on average 9 percentage points more likely to be approved than those with similar risk profiles but are barely above the cutoff. This 'jump' in the approval rates at the conforming/jumbo cutoff is largely diminished after the 2009 shock. It is important to point out that given the features of our identification strategy, this causal interpretation should be made locally. In other words, the results only apply to mortgages close to the threshold. In addition, while our research indicates a negative effect of the GSEs purchase, our results should be directed at a special period with a continuous increase in housing price.

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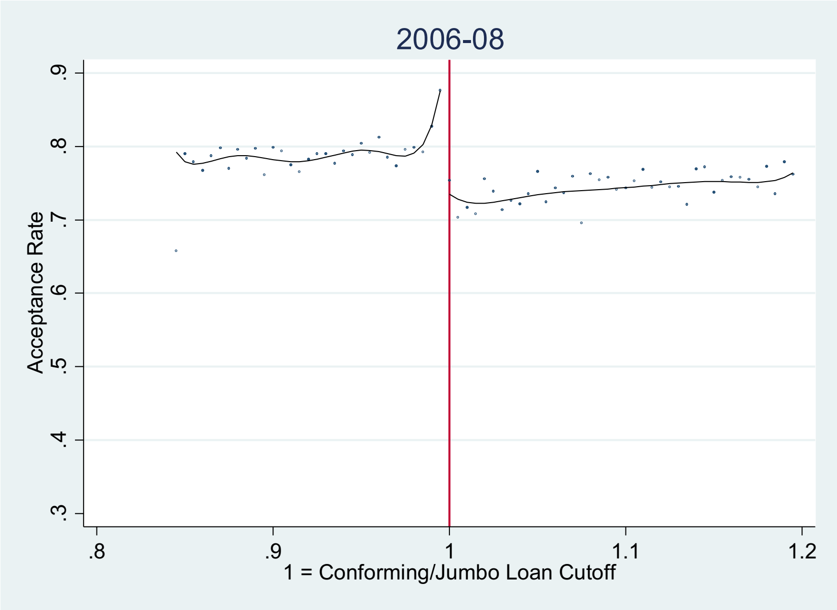
Figure 1. The discontinuity in acceptance rates of mortgage applications at the conforming/jumbo loan cutoff

This figure presents the acceptance rates for mortgage applications with loan amounts within each bin (0.005) – the small equally-spaced increments (bandwidth) of loan amount, and the fifth-order polynomial regression generated fits of the acceptance rates, created separately on either side of the cutoff. Loan amounts are normalized by their corresponding conforming loan limits.



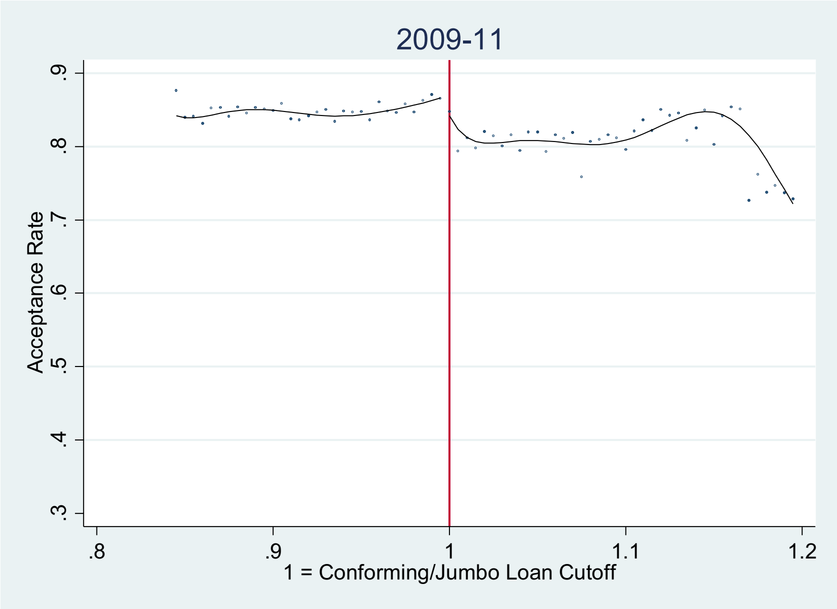


Table I. Summary Statistics.
This table provides the summary statistics for the loan applications with loan sizes between 85% and 120% of the conforming/jumbo mortgage cutoff in periods 2006- 2008 and 2009-2011, respectively.

Panel A: Year 2006-2008

	Conforming Loan Applications			Jumbo Loan Applications			All Applications		
variable	mean	s.d	N	mean	s.d	N	mean	s.d	N
Approved	0.79	0.41	2,855,166	0.71	0.46	1,006,631	0.77	0.42	3,861,797
Loan Amount	438.29	96.95	2,855,166	555.80	136.42	1,006,631	468.92	120.25	3,861,797
Income	188.55	191.33	2,855,166	230.87	244.72	1,006,631	199.58	207.41	3,861,797
Loan-to-Income	3.05	1.42	2,855,166	3.17	1.46	1,006,631	3.08	1.43	3,861,797
Female	0.22	0.41	2,855,166	0.24	0.43	1,006,631	0.23	0.42	3,861,797
Minority	0.14	0.35	2,855,166	0.18	0.38	1,006,631	0.15	0.36	3,861,797
Hispanic	0.09	0.28	2,855,166	0.11	0.31	1,006,631	0.09	0.29	3,861,797
Refinance	0.55	0.50	2,855,166	0.56	0.50	1,006,631	0.55	0.50	3,861,797
OwnerOccupy	0.11	0.31	2,855,166	0.11	0.32	1,006,631	0.11	0.31	3,861,797
CreditScore	712.65	32.05	2,855,166	714.46	32.72	1,006,631	713.12	32.24	3,861,797

Panel B: Year: 2009-2011

	Conforming Loan Applications			Jumbo Loan Applications			All Applications		
	mean	s.d	N	mean	s.d	N	mean	s.d	N
Approved	0.84	0.36	2,500,202	0.82	0.40	446,881	0.84	0.37	2,947,083
Loan Amount	423.53	69.03	2,500,202	614.78	108.09	446,881	452.53	102.56	2,947,083
Income	221.10	231.38	2,500,202	280.47	314.15	446,881	230.10	246.65	2,947,083
Loan-to-Income	2.66	1.37	2,500,202	3.03	1.46	446,881	2.72	1.39	2,947,083
Female	0.14	0.35	2,500,202	0.15	0.36	446,881	0.14	0.35	2,947,083
Minority	0.10	0.30	2,500,202	0.18	0.39	446,881	0.11	0.31	2,947,083
Hispanic	0.02	0.15	2,500,202	0.03	0.16	446,881	0.02	0.15	2,947,083
Refinance	0.76	0.43	2,500,202	0.70	0.46	446,881	0.75	0.43	2,947,083
OwnerOccupy	0.09	0.29	2,500,202	0.08	0.26	446,881	0.09	0.28	2,947,083
CreditScore	724.89	29.85	2,500,202	737.79	27.95	446,881	726.85	29.93	2,947,083

Table II. Regression Discontinuity Estimates of the Acceptance Rates by Year.

This table reports the regression discontinuity estimates of the acceptance rate differential around the conforming/Jumbo mortgage cutoff, based on Model (1), for each year from 2006-2011. Panel A use parametric regression discontinuity models with fifth- and sixth- order polynomial loan size, respectively. In the upper half of this Panel, the dependent variable is the mortgage application acceptance rate estimated in each small bin with bin size of 0.005, while in the lower half, the dependent variable is a dummy variable indicating whether a loan is approved. The observations are the applications with loan sizes within 85%-120% of the conforming loan limits. Panel B uses non-parametric regression discontinuity models where samples are narrowed to a small neighborhood around the conforming/jumbo loan cutoff. Specifically, the samples include observations with loan sizes of 90%-110% and 95%-105%, respectively, of the conforming loan limits. All regressions include Loan-to-Income ratio, female, minority, Hispanic, refinance, owner occupancy dummies, and average zip code credit score as control variables. For brevity, all these controls are omitted. All samples include mortgage applications in both general areas and high-cost areas except four statutorily designated states. Robust standard errors are in parentheses *, **, and *** denote statistical significance at 10, 5, and 1 percent level, respectively.

Panel A: Parametric

	Dep: acceptance rate					
Year	2006	2007	2008	2009	2010	2011
5th-order	0.119***	0.073**	0.064***	0.015	0.006	0.027
	(0.032)	(0.031)	(0.026)	(0.024)	(0.028)	(0.020)
6th-order	0.193***	0.139***	0.062**	0.023	0.022	0.034
	(0.042)	(0.039)	(0.033)	(0.029)	(0.033)	(0.023)
	Dep: approved					
5th-order	0.151***	0.163***	0.081***	-0.006	0.038***	0.018***
	(0.005)	(0.009)	(0.008)	(0.005)	(0.009)	(0.006)
6th-order	0.158***	0.165***	0.077***	-0.015***	0.032***	0.012*
	(0.007)	(0.010)	(0.007)	(0.005)	(0.010)	(0.007)

Panel B: Non-Parametric

	Dep: approved					
0.9-1.1	0.069***	0.095***	0.075***	0.032***	0.044***	0.030***
	(0.001)	(0.002)	(0.005)	(0.005)	(0.003)	(0.003)
0.95-1.05	0.079***	0.113***	0.082***	0.031***	0.039***	0.023***
	(0.002)	(0.004)	(0.006)	(0.006)	(0.004)	(0.004)

Table III. Year-by-Year Results on Piggyback Subsample.

This table reports the regression discontinuity estimates of the acceptance rate differential around the conforming/Jumbo mortgage cutoff, based on Model (1), for each year from 2006-2011 on a sample of piggyback loans. The procedure for identifying piggyback loans is detailed in footnote 25. Panel A use parametric regression discontinuity models with fifth- and sixth- order polynomial loan size, respectively. In the upper half of this Panel, the dependent variable is the mortgage application acceptance rate estimated in each small bin with bin size of 0.005, while in the lower half, the dependent variable is a dummy variable indicating whether a loan is approved. The observations are the applications with loan sizes within 85%-120% of the conforming loan limits. Panel B uses non-parametric regression discontinuity models where samples are narrowed to a small neighborhood around the conforming/jumbo loan cutoff. Specifically, the samples include observations with loan sizes of 90%-110% and 95%-105%, respectively, of the conforming loan limits. All regressions include Loan-to-Income ratio, female, minority, Hispanic, refinance, owner occupancy dummies, and average zip code credit score as control variables. For brevity, all these controls are omitted. All samples include mortgage applications in both general areas and high-cost areas except four statutorily designated states. Robust standard errors are in parentheses. *, **, and *** denote statistical significance at 10, 5, and 1 percent level, respectively.

Panel A: Parametric

Dep: acceptance rate						
Year	2006	2007	2008	2009	2010	2011
5th-order	0.194***	0.165***	0.101***	-0.021*	0.120	-0.176*
	(0.072)	(0.058)	(0.028)	(0.011)	(0.144)	(0.092)
6th-order	0.321***	0.154***	0.095***	-0.038***	0.054	-0.235**
	(0.079)	(0.024)	(0.024)	(0.005)	(0.072)	(0.114)

Dep: approved						
5th-order	0.147***	0.158***	0.101***	-0.008	-0.001	0.052*
	(0.006)	(0.009)	(0.020)	(0.012)	(0.024)	(0.017)
6th-order	0.164***	0.149***	0.095***	-0.028**	-0.011	0.036*
	(0.007)	(0.011)	(0.020)	(0.012)	(0.026)	(0.018)

Panel B: Non-Parametric

Dep: approved						
0.9-1.1	0.062***	0.101***	0.186***	0.106***	0.023***	0.055***
	(0.001)	(0.002)	(0.011)	(0.012)	(0.007)	(0.008)
0.95-1.05	0.075***	0.118***	0.173***	0.073***	0.023**	0.034***
	(0.002)	(0.003)	(0.014)	(0.014)	(0.010)	(0.010)

Table IV. Univariate Analysis.
This table reports the results of a difference-in-differences estimation of the effect of GSE purchases on mortgage application acceptance rates. In Panel A, the samples are narrowed to a small neighborhood, 90%-110% of the conforming limit. The average acceptance rates, standard errors (in parenthesis), and the number of observations are reported. Panel B uses a narrower width of neighborhood, 95%-105% of the conforming loan limits. *, **, and *** denote statistical significance at 10, 5, and 1 percent level, respectively.

Panel A: 90% - 110% around the conforming loan limits

	Conforming	Jumbo	Difference
2006-2008	0.814***	0.733***	0.081***
	(0.002)	(0.004)	(0.006)
	n=2,714,602	n=831,841	
2009-2011	0.855***	0.813***	0.042***
	(0.002)	(0.007)	(0.007)
	n=2,446,778	n=284,730	
Relative Difference			0.039***
			(0.009)

Panel B: 95% - 105% around the conforming loan limits

	Conforming	Jumbo	Difference
2006-2008	0.831***	0.727***	0.104***
	(0.003)	(0.007)	(0.007)
	n=1,686,456	n=423,543	
2009-2011	0.859***	0.820***	0.039***
	(0.003)	(0.009)	(0.009)
	n= 1,729,788	n=160,356	
Relative Difference			0.065***
			(0.009)

Table V. The Effect of GSE Purchases on Lenders' Screening Incentives.

This table reports the results from the combination of a regression discontinuity design and a difference-in-differences approach based on Equation (3). Models (1) and (2) use parametric regression discontinuity models with fifth-, and sixth- order polynomial loan size, respectively. The dependent variable is the mortgage application acceptance rate estimated in each small bin with bin size of 0.005. 'Conform' is a dummy variable that is equal to 1 if the loan amount is less than (or equal to) the conforming loan limit, and is equal to 0 otherwise. 'After' is a dummy variable that is equal to 1 if a mortgage application is after 2008 and equal to 0 otherwise. The observations are the applications with loan sizes within 85%-120% of the conforming loan limits. Models (3) and (4) also use parametric regression discontinuity models but the dependent variable is a dummy variable indicating whether a loan is approved. Model (5) and model (6) uses non-parametric regression discontinuity models where samples are narrowed to a small neighborhood around the conforming/jumbo loan cutoff. Specifically, the samples for models (5) and (6) are observations with loan sizes of 90%-110%, and 95%-105%, respectively, of the conforming loan limits. All regressions include Loan-to-Income ratio, female, minority, Hispanic, refinance, owner occupancy dummies, and average zip code credit score as control variables. The sample period is from 2006 to 2011. The samples include mortgage applications in both general areas and high-cost areas except four statutorily designated states. Panel B differs from Panel A in that it excludes observations that could be counted towards the Affordable Housing Goals. Panel C differs from Panel A in that it uses the piggyback loan application subsample. Robust standard errors are in parentheses.*, **, and *** denote statistical significance at 10, 5, and 1 percent level, respectively.

Panel A: All Data

	Parametric				Non-parametric	
	Acceptance Rate		Approved		Approved	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	5th-order	6th-order	5th-order	6th-order	0.9-1.1	0.95-1.05
Conform	0.080***	0.090***	0.083***	0.078***	0.080***	0.089***
	(0.020)	(0.021)	(0.003)	(0.003)	(0.002)	(0.002)
After	0.053***	0.053***	0.038***	0.037***	0.048***	0.058***
	(0.013)	(0.013)	(0.002)	(0.002)	(0.003)	(0.003)
Conform*After	-0.042***	-0.041***	-0.020***	-0.020***	-0.036***	-0.058***
	(0.011)	(0.011)	(0.002)	(0.002)	(0.003)	(0.003)
Constant	0.247	0.171	0.464***	0.469***	0.489***	0.531***
	(0.536)	(0.505)	(0.016)	(0.016)	(0.017)	(0.018)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Tract Fixed Effect	-	-	Yes	Yes	Yes	Yes
Observations	6,808,880	6,808,880	6,808,880	6,808,880	4,532,572	2,929,068
R-squared	0.800	0.800	0.100	0.100	0.110	0.110

Panel B: Excluding the observations that could be counted towards the Affordable Housing Goals.

	Parametric				Non-parametric	
	Acceptance Rate		Approved		Approved	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	5th-order	6th-order	5th-order	6th-order	0.9-1.1	0.95-1.05
Conform	0.112***	0.126***	0.083***	0.078***	0.079***	0.087***
	(0.022)	(0.022)	(0.003)	(0.003)	(0.002)	(0.002)
After	0.060***	0.059***	0.042***	0.041***	0.051***	0.061***
	(0.013)	(0.013)	(0.002)	(0.002)	(0.003)	(0.003)
Conform*After	-0.021**	-0.019*	-0.020***	-0.020***	-0.034***	-0.054***
	(0.010)	(0.010)	(0.002)	(0.002)	(0.003)	(0.003)
Constant	0.067	-0.062	0.439***	0.444***	0.462***	0.505***
	(0.548)	(0.497)	(0.016)	(0.016)	(0.017)	(0.018)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Tract Fixed Effect	-	-	Yes	Yes	Yes	Yes
Observations	6,247,539	6,247,539	6,247,539	6,247,539	4,196,703	2,738,209
R-squared	0.802	0.806	0.082	0.084	0.090	0.090

Panel C: Piggyback Loan Subsample

	Parametric				Non-parametric	
	Acceptance Rate		Approved		Approved	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	5th-order	6th-order	5th-order	6th-order	0.9-1.1	0.95-1.05
Conform	0.198***	0.233***	0.121***	0.121***	0.071***	0.086***
	(0.022)	(0.021)	(0.004)	(0.005)	(0.001)	(0.002)
After	0.017	0.021	0.013***	0.013***	0.020***	0.034***
	(0.026)	(0.026)	(0.004)	(0.004)	(0.005)	(0.007)
Conform*After	-0.062***	-0.065***	-0.007*	-0.007*	-0.014**	-0.038***
	(0.012)	(0.012)	(0.004)	(0.004)	(0.005)	(0.007)
Constant	-2.079***	-2.193***	0.252***	0.254***	0.280***	0.315***
	(0.640)	(0.628)	(0.010)	(0.010)	(0.011)	(0.014)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Tract Fixed Effect	-	-	Yes	Yes	Yes	Yes
Observations	1,138,891	1,138,891	1,138,891	1,138,891	727,568	438,276
R-squared	0.815	0.823	0.056	0.055	0.056	0.060

Table VI. Zip Code Level Delinquency and Default Regressions

This table reports the results from the zip code level loan delinquency and default regression. The dependent variable is the zip code percentage of first mortgage delinquency or default rates. Column (1) uses one-lead lag of the zip code 30-day first mortgage delinquency rate as the dependent variable. Column (2) uses one-year lead of the zip code 60-day first mortgage delinquency rate as the dependent variable. Column (3) uses one-year lead of the zip code first mortgage default rate (90+ day delinquency) as the dependent variable. 'Conform (%)' is the percentage of conforming loans in the zip code. 'After' is a dummy variable that is equal to 1 if a mortgage application is after 2008 and 0 otherwise. The observations are aggregated at the zip code level using loan applications with loan sizes within 85%-120% of the conforming loan limits. All regressions include zip code level variables such as zip code average Loan-to-Income ratio, female, minority, Hispanic, refinance, owner occupancy dummies, and zip code credit score as control variables. The sample period is from 2006 to 2011. The samples include mortgage applications in both general areas and high-cost areas except four statutorily designated states. Robust standard errors are in parentheses.*, **, and *** denote statistical significance at 10, 5, and 1 percent level, respectively.

	30-day Delinquency $Rate_{t+1}$	60-day Delinquency $Rate_{t+1}$	Default $Rate_{t+1}$
VARIABLES	(1)	(2)	(3)
Conform (%)	0.014***	0.013***	0.011***
	(0.001)	(0.001)	(0.001)
After	-0.013***	-0.011***	-0.009***
	(0.002)	(0.001)	(0.001)
Conform (%)*After	-0.012***	-0.010***	-0.009***
	(0.002)	(0.002)	(0.001)
Constant	0.379***	0.215***	0.150***
	(0.005)	(0.004)	(0.003)
Controls	Yes	Yes	Yes
Observations	112,488	112,488	112,488
R-squared	0.100	0.090	0.079

Table VII. Regressions with bank level controls.

This table reports the results from the combination of a regression discontinuity design and a difference-in-differences approach based on Equation (3). Models (1) and (2) use parametric regression discontinuity models with fifth-, and sixth- order polynomial loan size, respectively. The dependent variable is the mortgage application acceptance rate estimated in each small bin with bin size of 0.005. 'Conform' is a dummy variable that is equal to 1 if the loan amount is less than (or equal to) the conforming loan limit, and 0 otherwise. 'After' is a dummy variable that is equal to 1 if a mortgage application is after 2008 and 0 otherwise. The observations are the applications with loan sizes within 85%-120% of the conforming loan limits. Models (3) and (4) also use parametric regression discontinuity models but the dependent variable is a dummy variable indicating whether a loan is approved. Model (5) and model (6) uses non-parametric regression discontinuity models where samples are narrowed to a small neighborhood around the conforming/jumbo loan cutoff. Specifically, the samples for models (5) and (6) are observations with loan sizes of 90%-110%, and 95%-105%, respectively, of the conforming loan limits. All regressions include Loan-to-Income ratio, female, minority, Hispanic, refinance, owner occupancy dummies, and average zip code credit score as control variables. Bank level variables are measured at the end of previous year. The sample period is from 2006 to 2011. The samples include mortgage applications in both general areas and high-cost areas except four statutorily designated states. Robust standard errors are in parentheses.*, **, and *** denote statistical significance at 10, 5, and 1 percent level, respectively.

Effect of GSE Purchase on Lenders' Screening Incentives

	Parametric				Non-parametric	
	Acceptance Rate		Approved		Approved	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	5th-order	6th-order	5th-order	6th-order	0.9-1.1	0.95-1.05
Conform	0.039*	0.035	0.089***	0.069***	0.056***	0.059***
	(0.023)	(0.026)	(0.019)	(0.022)	(0.006)	(0.006)
After	-0.022	-0.019	0.008	-0.004	-0.014**	-0.010
	(0.033)	(0.031)	(0.019)	(0.023)	(0.006)	(0.007)
Conform*After	-0.061*	-0.065*	-0.073***	-0.062***	-0.029***	-0.037***
	(0.034)	(0.033)	(0.020)	(0.023)	(0.006)	(0.007)
Constant	1.055***	1.055***	0.538***	0.559***	0.663***	0.699***
	(0.031)	(0.031)	(0.023)	(0.026)	(0.018)	(0.019)
Tract Fixed Effect	-	-	Yes	Yes	Yes	Yes
Observations	5,439,703	5,439,703	5,439,703	5,439,703	3,661,338	2,388,901
R-squared	0.120	0.120	0.060	0.060	0.070	0.070

Table VIII. Clinical Study on 5 National Banks.

This table reports the results from the combination of a regression discontinuity design and a difference-in-differences approach based on Equation (3). Observations are restricted to 5 national banks including Bank of America, US Bank, Chase Manhattan Bank (JP Morgan Chase after 2009), Wells Fargo Bank, and CITI Bank. Lender fixed effects are included. Models (1) and (2) use parametric regression discontinuity models with fifth-, and sixth- order polynomial loan size, respectively. The dependent variable is the mortgage application acceptance rate estimated in each small bin with bin size of 0.005. 'Conform' is a dummy variable that is equal to 1 if the loan amount is less than (or equal to) the conforming loan limit, and 0 otherwise. 'After' is a dummy variable that is equal to 1 if a mortgage application is after 2008 and 0 otherwise. The observations are the applications with loan sizes within 85%-120% of the conforming loan limits. Models (3) to (4) also use parametric regression discontinuity models but the dependent variable is a dummy variable indicating whether a loan is approved. Model (5) and model (6) uses non-parametric regression discontinuity models where samples are narrowed to a small neighborhood around the conforming/jumbo loan cutoff. Specifically, the samples for models (5) and (6) are observations with loan sizes of 90%-110%, and 95%-105%, respectively, of the conforming loan limits. All regressions include loan level variables such as Loan-to-Income ratio, log of income, and female, minority, and Hispanic dummies, and bank level variables such as liquidity, cost of borrowing, capital asset ratio and log of assets. Bank level variables are measured at the end of previous year. The sample period is from 2006 to 2011. The samples include mortgage applications in both general areas and high-cost areas except four statutorily designated states. Robust standard errors are in parentheses. *, **, and *** denote statistical significance at 10, 5, and 1 percent level, respectively.

Effect of GSE Purchase on Lenders' Screening Incentives

	Parametric				Non-parametric	
	Acceptance Rate		Approved		Approved	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	5th-order	6th-order	5th-order	6th-order	0.9-1.1	0.95-1.05
Conform	0.094***	0.113***	0.067***	0.067***	0.047***	0.043***
	(0.014)	(0.016)	(0.005)	(0.005)	(0.003)	(0.004)
After	0.063***	0.064***	0.068***	0.068***	0.061***	0.052***
	(0.014)	(0.014)	(0.004)	(0.004)	(0.005)	(0.006)
Conform*After	-0.050***	-0.050***	-0.036***	-0.036***	-0.027***	-0.019***
	(0.011)	(0.011)	(0.003)	(0.003)	(0.004)	(0.005)
Constant	0.326	-0.388	0.991***	0.991***	1.052***	1.219***
	(0.457)	(0.435)	(0.060)	(0.060)	(0.070)	(0.084)
Tract Fixed Effect	-	-	Yes	Yes	Yes	Yes
Lender Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,605,046	1,605,046	1,605,046	1,605,046	1,076,065	715,782
R-squared	0.772	0.773	0.125	0.125	0.128	0.135

Appendix A. Using an alternative bin size (0.01) in the Regression Discontinuity Design

This table provides the estimation results based on Equation (3). Data restrictions and variable definitions are the same as those used in the first set of two regressions in Panel A of Table V except that this test uses a large bin size and excludes both observations for piggyback loans and observations counted towards affordable housing goals. *, **, and *** denote statistical significance at 10, 5, and 1 percent level, respectively.

VARIABLES	(1) 5th-order	(2) 6th-order
Conform	0.090***	0.069*
	(0.025)	(0.036)
After	0.063***	0.064***
	(0.013)	(0.014)
Conform*After	-0.079***	-0.080***
	(0.014)	(0.015)
Constant	1.309	1.436
	(1.198)	(1.271)
Controls	Yes	Yes
Observations	5,338,211	5,338,211
R-squared	0.895	0.892