Did Securitization Lead to Lax Screening? Evidence From Subprime Loans*

Benjamin J. Keys†
Tanmoy Mukherjee‡
Amit Seru§
Vikrant Vig¶

First Version: November 2007
This Version: December 2008

*Acknowledgments: We thank Viral Acharya, Effi Benmelech, Patrick Bolton, Daniel Bergstresser, Charles Calomiris, Douglas Diamond, John DiNardo, Charles Goodhart, Edward Glaeser, Dwight Jaffee, Anil Kashyap, Jose Liberti, Gregor Matvos, Chris Mayer, Donald Morgan, Adair Morse, Daniel Paravisini, Karen Pence, Guillaume Plantin, Manju Puri, Mitch Petersen, Raghuram Rajan, Uday Rajan, Adriano Rampini, Joshua Rauh, Chester Spatt, Steve Schaefer, Henri Servaes, Morten Sorensen, Jeremy Stein, Annette Vissing-Jorgensen, Paul Willen, three anonymous referees, and seminar participants at Boston College, Columbia Law, Duke, Federal Reserve Bank of Philadelphia, Federal Reserve Board of Governors, London Business School, London School of Economics, Michigan State, NYU Law, Northwestern, Oxford, Princeton, Standard and Poor's, University of Chicago Applied Economics Lunch and University of Chicago Finance Lunch for useful discussions. We also thank conference participants at CAF Summer Research Conference, China International Conference, European Summer Symposium in Financial Markets at Gerzensee, Homer Hoyt Institute, Mitsui Symposium at Michigan, Moodys/NYU Credit Risk Conference, NBER Corporate, NBER PERE, SITE Workshop and Summer Real Estate Symposium. Seru thanks the Initiative on Global Markets at the University of Chicago for financial support. The opinions expressed in the paper are those of the authors and do not reflect the views of Sorin Capital Management. All remaining errors are our responsibility.

†University of Michigan, e-mail: benkeys@umich.edu
‡Sorin Capital Management, e-mail: tmukherjee@sorincapital.com
§University of Chicago, GSB, e-mail: amit.seru@chicagogsb.edu
¶London Business School, e-mail: vvig@london.edu

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Abstract

A central question surrounding the current subprime crisis is whether the securitization process reduced the incentives of financial intermediaries to carefully screen borrowers. We empirically examine this issue using a unique dataset on securitized subprime mortgage loan contracts in the United States. We exploit a specific *rule of thumb* in the lending market to generate exogenous variation in the ease of securitization and compare the composition and performance of lenders' portfolios around the ad-hoc threshold. Conditional on being securitized, the portfolio that is more likely to be securitized defaults by around 10-25% more than a similar risk profile group with a lower probability of securitization. We conduct additional analyses to rule out selection on the part of borrowers, lenders, or investors as alternative explanations. The results are confined to loans where intermediaries' screening effort may be relevant and soft information about borrowers determines their creditworthiness. Our findings suggest that existing securitization practices *did* adversely affect the screening incentives of lenders.

I Introduction

Securitization, converting illiquid assets into liquid securities, has grown tremendously in recent years, with the securitized universe of mortgage loans reaching $3.6 trillion in 2006. The option to sell loans to investors has transformed the traditional role of financial intermediaries in the mortgage market from "buying and holding" to "buying and selling." The perceived benefits of this financial innovation, such as improving risk sharing and reducing banks' cost of capital, are widely cited (e.g. Pennacchi 1988). However, delinquencies in the heavily securitized subprime housing market increased by 50% from 2005 to 2007, forcing many mortgage lenders out of business and setting off a wave of financial crises which spread worldwide. In light of the central role of the subprime mortgage market in the current crisis, critiques of the securitization process have gained increased prominence (Blinder 2007; Stiglitz 2007).

The rationale for concern over the "originate-to-distribute" model during the crisis derives from theories of financial intermediation. Delegating monitoring to a single lender avoids the problems of duplication, coordination failure, and free-rider problems associated with multiple lenders (Diamond 1984). However, in order for a lender to screen and monitor, it must be given appropriate incentives (Holmstrom and Tirole 1997) and this is provided by the illiquid loans on their balance sheet (Diamond and Rajan 2003). By creating distance between a loan's originator and the bearer of the loan's default risk, securitization may have potentially reduced lenders' incentives to carefully screen and monitor borrowers (Petersen and Rajan 2002). On the other hand, proponents of securitization argue reputation concerns, regulatory oversight, or sufficient balance sheet risk may have prevented moral hazard on the part of lenders. What the effects of existing securitization practices on screening were, thus, remains an empirical question.

This paper investigates the relationship between securitization and screening standards in the context of subprime mortgage loans. The challenge in making a causal claim is the difficulty in isolating differences in loan outcomes independent of contract and borrower characteristics. First, in any cross-section of loans, those which are securitized may differ on observable and unobservable risk characteristics from loans which are kept on the balance sheet (not securitized). Second, in a time-series framework, simply documenting a correlation between securitization rates and defaults may be insufficient. This inference relies on establishing the optimal level of defaults at any given point in time. Moreover, this approach ignores macroeconomic factors and policy initiatives which may be independent of lax screening and yet may induce compositional differences in mortgage borrowers over time. For instance, house price appreciation and the changing role of Government-Sponsored Enterprises (GSEs) in the subprime market may also have accelerated the trend toward originating mortgages to riskier borrowers in exchange for higher payments.

We overcome these challenges by exploiting a specific *rule of thumb* in the lending market which induces exogenous variation in the ease of securitization of a loan compared to a loan with similar characteristics. This *rule of thumb* is based on the summary measure of borrower credit quality known as the FICO score. Since the mid-1990s, the FICO score has become the credit indicator most widely used by lenders, rating agencies, and investors. Underwriting guidelines established by the GSEs, Fannie Mae and Freddie Mac, standardized purchases of lenders' mortgage loans. These guidelines cautioned against lending to risky borrowers, the most prominent rule of thumb being not lending to borrowers with FICO scores below 620 (Avery et al. 1996; Loesch 1996; Calomiris and Mason 1999; Capone 2002; Freddie Mac 2001, 2007).[1] While the GSEs actively securitized loans when the nascent subprime market was relatively small, since 2000 this role has shifted entirely to investment banks and hedge funds (the non-agency sector). We argue that persistent adherence to this ad-hoc cutoff by investors who purchase securitized pools from non-agencies generates a differential increase in the ease of securitization for loans. That is, loans made to borrowers which fall just above the 620 credit cutoff have a higher unconditional likelihood of being securitized and are therefore more liquid relative to loans below this cutoff.

To evaluate the effect of securitization on screening decisions, we examine the performance of loans originated by lenders around this threshold. As an example of our design, consider two borrowers, one with a FICO score of 621 (620^+) while the other has a FICO score of 619 (620^-), who approach the lender for a loan. In order to evaluate the quality of the loan applicant, screening involves collecting both "hard" information, such as the credit score, and "soft" information, such as a measure of future income stability of the borrower. Hard information by definition is something that is easy to contract upon (and transmit), while the lender has to exert an unobservable effort to collect soft information (Stein 2002). We argue that the lender has a weaker incentive to base origination decisions on both hard and soft information, less carefully screening the borrower, at 620^+ where there is a higher likelihood that this loan will be eventually securitized. In other words, because investors purchase securitized loans based on hard information, the cost of collecting soft information is internalized by lenders to a lesser extent when screening borrowers at 620^+ than at 620^-. Therefore, by comparing the portfolio of loans on either side of the credit score threshold, we can assess whether differential access to securitization led to changes in the behavior of lenders who offered these loans to consumers with nearly identical risk profiles.

Using a sample of more than one million home purchase loans during the period 2001-2006, we empirically confirm that the number of loans securitized varies systematically around the 620 FICO cutoff. For loans with a potential for significant soft information – *low documentation* loans

[1] We discuss the 620 rule of thumb in more detail in Section III and in reference to other cutoffs in the lending market in Section IV.G.

– we find that there are more than twice as many loans securitized above the credit threshold at 620^{+} vs. below the threshold at 620^{-}. Since the FICO score distribution in the population is smooth (constructed from a logistic function; see Figure 1), the underlying creditworthiness and demand for mortgage loans (at a given price) is the same for prospective buyers with a credit score of either 620^{-} or 620^{+}. Therefore, these differences in the number of loans confirm that the unconditional probability of securitization is higher above the FICO threshold, i.e., it is easier to securitize 620^{+} loans.

Strikingly, we find that while 620^{+} loans should be of slightly better credit quality than those at 620^{-}, low documentation loans that are originated above the credit threshold tend to default within two years of origination at a rate 10-25% higher than the mean default rate of 5% (which amounts to roughly a 0.5-1% increase in delinquencies). As this result is conditional on observable loan and borrower characteristics, the only remaining difference between the loans around the threshold is the increased ease of securitization. Therefore, the greater default probability of loans above the credit threshold must be due to a reduction in screening by lenders.

Since our results are conditional on securitization, we conduct additional analyses to address selection on the part of borrowers, lenders, or investors as explanations for the differences in the performance of loans around the credit threshold. First, we rule out borrower selection on observables, as the loan terms and borrower characteristics are smooth through the FICO score threshold. Next, selection of loans by investors is mitigated because the decisions of investors (Special Purpose Vehicles, SPVs) are based on the same (smooth through the threshold) loan and borrower variables as in our data (Kornfeld 2007).

Finally, strategic adverse selection on the part of lenders may also be a concern. However, lenders offer the entire pool of loans to investors, and, conditional on observables, SPVs largely follow a randomized selection rule to create bundles of loans out of these pools, suggesting securitized loans would look similar to those that remain on the balance sheet (Gorton and Souleles 2005; *Comptroller's Handbook* 1997). Furthermore, if at all present, this selection will tend to be more severe below the threshold, thereby biasing the results against us finding any screening effect. We also constrain our analysis to a subset of lenders who are not susceptible to strategic securitization of loans. The results for these lenders are qualitatively similar to the findings using the full sample, highlighting that screening is the driving force behind our results.

Could the 620 threshold be set by lenders as an optimal cutoff for screening that is unrelated to differential securitization? We investigate further using a natural experiment in the passage and subsequent repeal of anti-predatory laws in New Jersey (2002) and Georgia (2003) that varied the ease of securitization around the threshold. If lenders use 620 as an optimal cutoff for screening unrelated to securitization, we expect the passage of these laws to have no effect on the differential screening standards around the threshold. However, if these laws affected the differential ease of securitization around the threshold, our hypothesis would predict an impact on the screening standards. Our results confirm that the discontinuity in the number of loans around the threshold diminished during a period of strict enforcement of anti-predatory lending laws. In addition, there was a rapid return of a discontinuity after the law was revoked. Importantly, our performance results follow the same pattern, i.e., screening differentials attenuated only during the period of enforcement. Taken together, this evidence suggests that our results are indeed related to differential securitization at the credit threshold and that lenders did not follow the rule of thumb in all instances. Importantly, the natural experiment also suggests that prime-influenced selection is not at play.

Once we have confirmed that lenders are screening more rigorously at 620^- than 620^+, we assess whether borrowers were aware of the differential screening around the threshold. Although there is no difference in contract terms around the cutoff, borrowers may have an incentive to manipulate their credit scores in order to take advantage of differential screening around the threshold (consistent with our central claim). Aside from outright fraud, it is difficult to strategically manipulate one's FICO score in a targeted manner and any actions to improve one's score take relatively long periods of time, on the order of three to six months (Fair Isaac). Nonetheless, we investigate further using the same natural experiment evaluating the performance effects over a relatively short time horizon. The results reveal a rapid return of a discontinuity in loan performance around the 620 threshold which suggests that rather than manipulation, our results are largely driven by differential screening on the part of lenders.

As a test of the role of soft information on screening incentives of lenders, we investigate the *full documentation* loan lending market. These loans have potentially significant hard information because complete background information about the borrower's ability to repay is provided. In this market, we identify another credit cutoff, a FICO score of 600, based on the advice of the three credit repositories. We find that twice as many full documentation loans are securitized above the credit threshold at 600^+ vs. below the threshold at 600^-. Interestingly, however, we find no significant difference in default rates of full documentation loans originated around this credit threshold. This result suggests that despite a difference in ease of securitization around the threshold, differences in the returns to screening are attenuated due to the presence of more hard information. Our findings for full documentation loans suggest that the role of soft information is crucial to understanding what worked and what did not in the existing securitized subprime loan market. We discuss this issue in more detail in Section VI.

This paper connects several strands of literature. Our evidence sheds new light on the subprime housing crisis, as discussed in the contemporaneous work of Benmelech and Dlugosz (2008), Doms, Furlong, and Krainer (2007), Dell'Ariccia, Igan and Laeven (2008), Demyanyk and Van Hemert (2008), Gerardi, Shapiro and Willen (2007), Mayer, Piskorski, and Tchistyi (2008), Mian and Sufi (2008) and Rajan, Seru and Vig (2008).[2] This paper also speaks to the literature which discusses the benefits (Kashyap and Stein 2000 and Loutskina and Strahan 2007), and the costs (Parlour and Plantin 2007 and Morrison 2005) of securitization. In a related line of research, Drucker and Mayer (2008) document how underwriters exploit inside information to their advantage in secondary mortgage markets, while Gorton and Pennacchi (1995), Drucker and Puri (2007) and Sufi (2006) investigate how contract terms are structured to mitigate some of these agency conflicts.[3]

The rest of the paper is organized as follows. Section II provides a brief overview of lending in the subprime market and describes the data and sample construction. Section III discusses the framework and empirical methodology used in the paper, while Sections IV and V present the empirical results in the paper. Section VI concludes.

II Lending in Subprime Market

II.A Background

Approximately 60% of outstanding U.S. mortgage debt is traded in mortgage-backed securities (MBS), making the U.S. secondary mortgage market the largest fixed-income market in the world (Chomsisengphet and Pennington-Cross 2006). The bulk of this securitized universe ($3.6 trillion outstanding as of January 2006) is comprised of agency pass-through pools – those issued by Freddie Mac, Fannie Mae and Ginnie Mae. The remainder, approximately, $2.1 trillion as of January 2006 has been securitized in non-agency securities. While the non-agency MBS market is relatively small as a percentage of all U.S. mortgage debt, it is nevertheless large on an absolute dollar basis. The two markets are separated based on the eligibility criteria of loans that the GSEs have established. Broadly, agency eligibility is established on the basis of loan size, credit score, and underwriting standards.

Unlike the agency market, the non-agency (referred to as "subprime" in the paper) market was not always this size. This market gained momentum in the mid- to late-1990s. Inside B&C Lending – a publication which covers subprime mortgage lending extensively – reports that total subprime lending (B&C originations) has grown from $65 billion in 1995 to $500 billion in 2005. Growth in mortgage-backed securities led to an increase in securitization rates (the ratio of the dollar-value of loans securitized divided by the dollar-value of loans originated) from less than

[2] For thorough summaries of the subprime mortgage crisis and the research which has sought to explain it, see Mayer and Pence (2008) and Mayer, Pence, and Sherlund (2008).

[3] Our paper also sheds light on the classic liquidity/incentives trade-off that is at the core of the financial contracting literature (see Coffee 1991, Diamond and Rajan 2003, Aghion et al. 2004, DeMarzo and Urosevic 2006).

30 percent in 1995 to over 80 percent in 2006.

From the borrower's perspective, the primary distinguishing feature between prime and subprime loans is that the up-front and continuing costs are higher for subprime loans.[4] The subprime mortgage market actively prices loans based on the risk associated with the borrower. Specifically, the interest rate on the loan depends on credit scores, debt-to-income ratios and the documentation level of the borrower. In addition, the exact pricing may depend on loan-to-value ratios (the amount of equity of the borrower), the length of the loan, the flexibility of the interest rate (adjustable, fixed, or hybrid), the lien position, the property type and whether stipulations are made for any prepayment penalties.[5]

For investors who hold the eventual mortgage-backed security, credit risk in the agency sector is mitigated by an implicit or explicit government guarantee, but subprime securities have no such guarantee. Instead, credit enhancement for non-agency deals is in most cases provided internally by means of a deal structure which bundles loans into "tranches," or segments of the overall portfolio (Lucas, Goodman and Fabozzi 2006).

II.B Data

Our primary data contain individual loan data leased from LoanPerformance. The database is the only source which provides a detailed perspective on the non-agency securities market. The data includes information on issuers, broker dealers/deal underwriters, servicers, master servicers, bond and trust administrators, trustees, and other third parties. As of December 2006, more than 8,000 home equity and nonprime loan pools (over 7,000 active) that include 16.5 million loans (more than seven million active) with over $1.6 trillion in outstanding balances were included. LoanPerformance estimates that as of 2006, the data covers over 90% of the subprime loans that are securitized.[6] The dataset includes all standard loan application variables such as the loan amount, term, LTV ratio, credit score, and interest rate type – *all* data elements that are disclosed and form the basis of contracts in non-agency securitized mortgage pools. We now

[4]Up-front costs include application fees, appraisal fees, and other fees associated with originating a mortgage. The continuing costs include mortgage insurance payments, principle and interest payments, late fees for delinquent payments, and fees levied by a locality (such as property taxes and special assessments).

[5]For example, the rate and underwriting matrix of Countrywide Home Loans Inc., a leading lender of prime and subprime loans, shows how the credit score of the borrower and the loan-to-value ratio are used to determine the rate at which different documentation-level loans are made (www.countrywide.com).

[6]Note that only loans that are securitized are reported in the LoanPerformance database. Communication with the database provider suggests that the roughly 10% of loans that are not reported are for privacy concerns from lenders. Importantly for our purpose, the exclusion is not based on any selection criteria that the vendor follows (e.g., loan characteristics or borrower characteristics). Moreover, based on estimates provided by LoanPerformance, the total number of non-agency loans securitized relative to all loans originated has increased from about 65% in early 2000 to over 92% since 2004.

describe some of these variables in more detail.

For our purpose, the most important piece of information about a particular loan is the creditworthiness of the borrower. The borrower's credit quality is captured by a summary measure called the FICO score. FICO scores are calculated using various measures of credit history, such as types of credit in use and amount of outstanding debt, but do *not* include any information about a borrower's income or assets (Fishelson-Holstein 2005). The software used to generate the score from individual credit reports is licensed by the Fair Isaac Corporation to the three major credit repositories – TransUnion, Experian, and Equifax. These repositories, in turn, sell FICO scores and credit reports to lenders and consumers. FICO scores provide a ranking of potential borrowers by the probability of having some negative credit event in the next *two years*. Probabilities are rescaled into a range of 400-900, though nearly all scores are between 500 and 800, with a higher score implying a lower probability of a negative event. The negative credit events foreshadowed by the FICO score can be as small as one missed payment or as large as bankruptcy. Borrowers with lower scores are proportionally more likely to have all types of negative credit events than are borrowers with higher scores.

FICO scores have been found to be accurate even for low-income and minority populations (see Fair Isaac website www.myfico.com; also see Chomsisengphet and Pennington-Cross 2006). More importantly, the applicability of scores available at loan origination extends reliably up to two years. By design, FICO measures the probability of a negative credit event over a two-year horizon. Mortgage lenders, on the other hand, are interested in credit risk over a much longer period of time. The continued acceptance of FICO scores in automated underwriting systems indicates that there is a level of comfort with their value in determining lifetime default probability differences.[7] Keeping this as a backdrop, most of our tests of borrower default will examine the default rates up to 24 months from the time the loan is originated.

Borrower quality can also be gauged by the level of documentation collected by the lender when taking the loan. The documents collected provide historical and current information about the income and assets of the borrower. Documentation in the market (and reported in the database) is categorized as full, limited or no documentation. Borrowers with full documentation provide verification of income as well as assets. Borrowers with limited documentation provide no information about their income but do provide some information about their assets. "No-documentation" borrowers provide no information about income or assets, which is a very rare degree of screening lenience on the part of lenders. In our analysis, we combine limited and no-documentation borrowers and call them low documentation borrowers. Our results are

[7] An econometric study by Freddie Mac researchers showed that the predictive power of FICO scores drops by about 25 percent once one moves to a three-to-five year performance window (Holloway, MacDonald and Straka 1993). FICO scores are still predictive, but do not contribute as much to the default rate probability equation after the first two years.

unchanged if we remove the very small portion of loans which are no documentation.

Finally, there is also information about the property being financed by the borrower, and the purpose of the loan. Specifically, we have information on the type of mortgage loan (fixed rate, adjustable rate, balloon or hybrid), and the loan-to-value (LTV) ratio of the loan, which measures the amount of the loan expressed as a percentage of the value of the home. Typically loans are classified as either for purchase or refinance, though for convenience we focus exclusively on loans for home purchases.[8] Information about the geography where the dwelling is located (zipcode) is also available in the database.

Most of the loans in our sample are for the owner-occupied single-family residences, townhouses, or condominiums (single unit loans account for more than 90% of the loans in our sample). Therefore, to ensure reasonable comparisons we restrict the loans in our sample to these groups. We also drop non-conventional properties, such as those that are FHA or VA insured or pledged properties, and also exclude buy down mortgages. We also exclude Alt-A loans, since the coverage for these loans in the database is limited. Only those loans with valid FICO scores are used in our sample. We conduct our analysis for the period January 2001 to December 2006, since the securitization market in the subprime market grew to a meaningful size post-2000 (Gramlich 2007).

III Framework and Methodology

When a borrower approaches a lender for a mortgage loan, the lender asks the borrower to fill out a credit application. In addition, the lender obtains the borrower's credit report from the three credit bureaus. Part of the background information on the application and report could be considered "hard" information (e.g., the FICO score of the borrower), while the rest is "soft" (e.g., a measure of future income stability of the borrower, how many years of documentation were provided by the borrower, joint income status) in the sense that it is less easy to summarize on a legal contract. The lender expends effort to process the soft and hard information about the borrower and, based on this assessment, offers a menu of contracts to the borrower. Subsequently, borrowers decide to accept or decline the loan contract offered by the lender.

Once a loan contract has been accepted, the loan can be sold as part of a securitized pool to investors. Notably, only the hard information about the borrower (FICO score) and the contractual terms (e.g., LTV ratio, interest rate) are used by investors when buying these loans as a part of securitized pool.[9] In fact, the variables about the borrowers and the loan terms in the LoanPerformance database are identical to those used by investors and rating agencies

[8] We find similar rules of thumb and default outcomes in the refinance market.

[9] See Testimony of Warren Kornfeld, Managing Director of Moodys Investors Service before the subcommittee on Financial Institutions and Consumer Credit U.S. House of Representatives May 8, 2007.

to rate tranches of the securitized pool. Therefore, while lenders are compensated for the hard information about the borrower, the incentive for lenders to process soft information critically depends on whether they have to bear the risk of loans they originate (Gorton and Pennacchi 1995; Parlour and Plantin 2007; Rajan et al. 2008). The central claim in this paper is that lenders are less likely to expend effort to process soft information as the ease of securitization increases.

We exploit a specific *rule of thumb* at the FICO score of 620 which makes securitization of loans more likely if a certain FICO score threshold is attained. Historically, this score was established as a minimum threshold in the mid-1990's by Fannie Mae and Freddie Mac in their guidelines on loan eligibility (Avery et al. 1996 and Capone 2002). Guidelines by Freddie Mac suggest that FICO scores below 620 are placed in the *Cautious Review Category*, and Freddie Mac considers a score below 620 "as a strong indication that the borrower's credit reputation is not acceptable." (Freddie Mac 2001, 2007).[10] This is also reflected in Fair Isaac's statement, "...those agencies [Fannie Mae and Freddie Mac], which buy mortgages from banks and resell them to investors, have indicated to lenders that any consumer with a FICO score above 620 is good, while consumers below 620 should result in further inquiry from the lender....". While the GSEs actively securitized loans when the nascent subprime market was relatively small, this role shifted entirely to investment banks and hedge funds (the non-agency sector) in recent times (Gramlich, 2007).

We argue that adherence to this cutoff by subprime MBS investors, following the advice of GSEs, generates an increase in demand for securitized loans which are just above the credit cutoff relative to loans below this cutoff. There is widespread evidence that is consistent with 620 being a rule of thumb in the securitized subprime lending market. For instance, rating agencies (Fitch and Standard and Poor's) used this cutoff to determine default probabilities of loans when rating mortgage backed securities with subprime collateral (Loesch 1996; Temkin, Johnson and Levy 2002). Similarly, Calomiris and Mason (1999) survey the high risk mortgage loan market and find 620 as a rule of thumb for subprime loans. We also confirmed this view by conducting a survey of origination matrices used by several of the top 50 originators in the subprime market (a list obtained from *Inside B&C Lending*; these lenders amount to about 70% of loan volume). The credit threshold of 620 was used by nearly all the lenders.

Since investors purchase securitized loans based on hard information, our assertion is that the cost of collecting soft information are internalized by lenders to a greater extent when screening borrowers at 620^- than at 620^+. There is widespread anecdotal evidence that lenders

[10]These guidelines appeared atleast as far back as 1995 in a letter by the Executive Vice President of Freddie Mac (Michael K. Stamper) to the CEOs and Credit Officers of all Freddie Mac Sellers and Servicers (see internet appendix Exhibit 1).

in the subprime market review both soft and hard information more carefully for borrowers with credit scores below 620. For instance, the website of Advantage Mortgage, a subprime securitized loan originator, claims that "...all loans with credit scores below 620 require a second level review....There are no exceptions, regardless of the strengths of the collateral or capacity components of the loan."[11] By focusing on the lender as a unit of observation we attempt to learn about the differential impact ease of securitization had on behavior of lenders around the cutoff.

To begin with, our tests empirically identify a statistical discontinuity in the distribution of loans securitized around the credit threshold of 620. In order to do so, we show that the number of loans securitized dramatically increases when we move along the FICO distribution from 620^- to 620^+. We argue that this is equivalent to showing that the unconditional probability of securitization increases as one moves from 620^- to 620^+. To see this, denote $N_s^{620^+}$ and $N_s^{620^-}$ as the number of loans securitized at 620^+ and 620^- respectively. Showing that $N_s^{620^+} > N_s^{620^-}$ is equivalent to showing $\frac{N_s^{620^+}}{N_p} > \frac{N_s^{620^-}}{N_p}$, where N_p is the number of prospective borrowers at 620^+ or 620^-. If we assume that the number of prospective borrowers at 620^+ or 620^- are similar, i.e., $N_p^{620^+} \approx N_p^{620^+} = N_p$ (a reasonable assumption as discussed below), then the unconditional probability of securitization is higher at 620^+. We refer to the difference in these unconditional probabilities as the differential *ease of securitization* around the threshold. Notably, our assertion of differential screening by lenders does not rely on knowledge of the proportion of prospective borrowers that applied, were rejected, or were held on the lenders' balance sheet. We simply require that lenders' are aware that a prospective borrower at 620^+ has a higher likelihood of eventual securitization.

We measure the extent of the jump by using techniques which are commonly used in the literature on regression discontinuity (e.g., see DiNardo and Lee 2004; Card et al. 2007). Specifically, we collapse the data on each FICO score (500-800) i, and estimate equations of the form:

$$Y_i = \alpha + \beta T_i + \theta f(FICO(i)) + \delta T_i * f(FICO(i)) + \epsilon_i \tag{1}$$

where Y_i is the number of loans at FICO score i, T_i is an indicator which takes a value of 1 at FICO $\geq$ 620 and a value of 0 if FICO $<$ 620 and ϵ_i is a mean-zero error term. $f(FICO)$ and $T * f(FICO)$ are flexible seventh-order polynomials, with the goal of these functions being to fit the smoothed curves on either side of the cutoff as closely to the data presented in the figures as possible.[12] $f(FICO)$ is estimated from 620^- to the left, and $T * f(FICO)$ is estimated

[11] This position for loans below 620 is reflected in lending guidelines of numerous other subprime lenders.

[12] We have also estimated these functions of the FICO score using 3rd order and 5th order polynomials in FICO, as well as relaxing parametric assumptions and estimating using local linear regression. The estimates throughout are not sensitive to the specification of these functions. In Section IV, we also examine the size and power of the test using the seventh-order polynomial specification following the approach of Card et al. (2007).

from 620^+ to the right. The magnitude of the discontinuity, β, is estimated by the difference in these two smoothed functions evaluated at the cutoff. The data are *re-centered* such that $FICO = 620$ corresponds to "0," thus at the cutoff the polynomials are evaluated at 0 and drop out of the calculation, which allows β to be interpreted as the magnitude of the discontinuity at the FICO threshold. This coefficient should be interpreted locally in the immediate vicinity of the credit score threshold.

After documenting a large jump at the ad-hoc credit thresholds, we focus on the performance of the loans around these thresholds. We evaluate the performance of the loans by examining the default probability of loans – i.e., whether or not the loan defaulted t months after it was originated. If lenders screen similarly for the loan of credit quality 620^+ and the loan of 620^- credit quality, there should not be any discernible differences in default rates of these loans. Our maintained claim is that any differences in default rates on either side of the cutoff, after controlling for hard information, should be only due to the impact that securitization has on lenders' screening standards.

This claim relies on several identification assumptions. First, as we approach the cutoff from either side, any differences in the characteristics of prospective borrowers are assumed to be random. This implies that the underlying creditworthiness and the demand for mortgage loans (at a given price) is the same for prospective buyers with a credit score of 620^- or 620^+. This seems reasonable as it amounts to saying that the calculation Fair Isaac performs (using a logistic function) to generate credit scores has a random error component around any specific score. Figure 1 shows the FICO distribution in the U.S. population in 2004. This data is from an anonymous credit which assures us that the data exhibits similar patterns during the other years of our sample. Note that the FICO distribution across the population is smooth, so the number of prospective borrowers around a given credit score is similar (in the example above, $N_p^{620^+} \approx N_p^{620^+} = N_p$).

Second, we assume that screening is costly for the lender. The collection of information – hard systematic data (e.g., FICO score) as well as soft information (e.g., joint income status) about the creditworthiness of the borrower – requires time and effort by loan officers. If lenders did not have to expend resources to collect information, it would be difficult to argue that the differences in performance we estimate are a result of ease of securitization around the credit threshold affecting banks incentives to screen and monitor. Again, this seems to be a reasonable assumption (see Gorton and Pennacchi 1995).

Note that our discussion thus far has assumed that there is no explicit manipulation of FICO scores by the lenders or borrowers. However, the borrower may have incentives to do so if loan contracts or screening differ around the threshold. Our analysis in Section IV.F focuses on a natural experiment and shows that the effects of securitization on performance are not being driven by strategic manipulation.

IV Main Empirical Results

IV.A Descriptive Statistics

As noted earlier, the non-agency market differs from the agency market on three dimensions: FICO scores, loan-to-value ratios and the amount of documentation asked of the borrower. We next look at the descriptive statistics of our sample with special emphasis on these dimensions. Our analysis uses more than one million loans across the period 2001 to 2006. As mentioned earlier, the non-agency securitization market has grown dramatically since 2000, which is apparent in Panel A of Table I, which shows the number of subprime loans securitized across years. These patterns are similar to those described in Demyanyk and Van Hemert (2007) and Gramlich (2007). The market has witnessed an increase in the number of loans with reduced hard information in the form of limited or no documentation. Note that while limited documentation provides no information about income but does provide some information about assets, a no-documentation loan provides information about neither income nor assets. In our analysis we combine both types of limited-documentation loans and denote them as *low* documentation loans. The full documentation market grew by 445% from 2001 to 2005, while the number of low documentation loans grew by 972%.

We find similar trends for loan-to-value ratios and FICO scores in the two documentation groups. LTV ratios have gone up over time, as borrowers have put in less and less equity into their homes when financing loans. This increase is consistent with a better appetite of market participants to absorb risk. In fact, this is often considered the bright side of securitization – borrowers are able to borrow at better credit terms since risk is being borne by investors who can bear more risk than individual banks. Panel A also shows that average FICO scores of individuals who access the subprime market has been increasing over time. The mean FICO score among low documentation borrowers increased from 630 in 2001 to 655 in 2006. This increase in average FICO scores is consistent with the rule of thumb leading to a larger expansion of the market above the 620 threshold. Average LTV ratios are lower and FICO scores higher for low documentation as compared to the full documentation sample. This possibly reflects the additional uncertainty lenders have about the quality of low documentation borrowers.

Panel B compares the low and full documentation segments of the subprime market on a number of the explanatory variables used in the analysis. Low documentation loans are on average larger and given to borrowers with higher credit scores than loans where full information on income and assets are provided. However, the two groups of loans have similar contract terms such as interest rate, loan-to-value, prepayment penalties, and whether the interest rate is adjustable or not. Our analysis below focuses first on the low documentation segment of the market, and we explore the full documentation market in Section V.

IV.B Establishing the Rule of Thumb

We first present results that show that large differences exist in the number of low documentation loans that are securitized around the credit threshold we described earlier. We then examine whether this jump in securitization has any consequences on the subsequent performance of the loans above and below this credit threshold.

As mentioned in Section III, the rule of thumb in the lending market impacts the ease of securitization around the credit score of 620. We therefore expect to see a substantial increase in the number of loans just above this credit threshold as compared to number of loans just below this threshold. In order to examine this, we start by plotting the number of loans at each FICO score in the two documentation categories around the credit cutoff of 620 across years starting with 2001 and ending in 2006. As can be seen from Figure 2, there is a marked increase in number of low documentation loans around the credit score of 620 – that is, at 620^{+} relative to number of loans at 620^{-}. We do not find any such jump for full documentation loans at FICO of 620.[13] Given this evidence, we focus on the 620 credit threshold for low documentation loans.

From Figure 2, it is clear that the number of loans see roughly a 100% jump in 2004 for low documentation loans around the credit score of 620 – i.e., there are twice as many loans securitized at 620^{+} as compared to loans securitized at 620^{-}. Clearly, this is consistent with the hypothesis that the ease of securitization is higher at 620^{+} than at scores just below this credit cutoff.

To estimate the jumps in the number of loans, we use the methods described above in Section III using the specification provided in equation (1). As reported in Table II, we find that low documentation loans see a dramatic increase above the credit threshold of 620. In particular, the coefficient estimate (β) is significant at the 1% level and is on average around 110% (from 73 to 193%) higher for 620^{+} as compared to 620^{-} for loans during the sample period. For instance, in 2001, the estimated discontinuity in Panel A is 85. The mean average number of low documentation loans at a FICO score for 2001 is 117. The ratio is around 73%. These jumps are plainly visible from the yearly graphs in Figure 1.

In addition, we conduct permutation tests (or "randomization" tests), where we varied the location of the discontinuity (T_i) across the range of all possible FICO scores and re-estimated equation (1). The test treats every value of the FICO distribution as a potential discontinuity, and estimates the magnitude of the observed discontinuity at each point, forming a counter-factual distribution of discontinuity estimates. This is equivalent to a bootstrapping procedure

[13] We will elaborate more on full documentation loans in Section V.

which varies the cutoff but does not re-sample the order of the points in the distribution (Johnston and DiNardo 1996). We then compare the value of the estimated discontinuity at 620 to the counterfactual distribution and construct a test statistic based on the asymptotic normality of the counterfactual distribution and report the p-value from this test. The null hypothesis is that the estimated discontinuity at a FICO score of 620 is that of the mean of the 300 possible discontinuities.[14]

The precision of the permutation test is limited by the number of observations used at each FICO score. As a result, regressions which pool across years provide the greatest power for statistical testing. While constructing the counterfactuals, we therefore use pooled specifications with year fixed effects removed to account for differences in vintage. The result of this test is shown in Table II and shows that the estimate at 620 for low documentation loans is a strong outlier relative to the estimated jumps at other locations in the distribution. The estimated discontinuity when the years are pooled together is 780 loans with a permutation test p-value of 0.003. In summary, if the underlying creditworthiness and the demand for mortgage loans is the same for potential buyers with a credit score of 620^- or 620^+, this result confirms that it is easier to securitize loans above the FICO threshold.

IV.C Contract Terms and Borrower Demographics

Before examining the subsequent performance of loans around the credit threshold, we first assess if there are any differences in hard information – either in contract terms or other borrower characteristics – around this threshold. The endogeneity of contractual terms based on the riskiness of borrowers may lead to different contracts and hence, different types of borrowers obtaining loans around the threshold in a systematic way. Though we control for the possible contract differences when we evaluate the performance of loans, it is insightful to examine whether borrower and contract terms also systematically differ around the credit threshold.

We start by examining the contract terms – LTV ratio and interest rates – around the credit threshold. Figures 3 and 4 show the distribution of interest rates and LTV ratios offered on low documentation loans across the FICO spectrum. As is apparent, we find these loan terms to be very similar – i.e., we find no differences in contract terms for low documentation loans above and below the 620 credit score. We test this formally using an approach equivalent to equation (1), replacing the dependent variable Y_i in the regression framework with contract terms (loan-to-value ratios and interest rates) and present the results in the appendix (Table

[14] In unreported tests, we also conduct a falsification simulation exercise following Card et al. (2007). In particular, we apply our specification to data generated by a continuous process. We reject the null hypothesis of no effect (using a 2-sided 5% test) in 6.0% of the simulations indicating that the size of our test is a reasonable. A similar test with data generated by a discontinuous process suggests that the power of our test is also reasonable. We reject the null of no effect about 92% of the times (in a 2-sided 5% test) in this case.

A.I). Our results suggest that there is no difference in loan terms around the credit threshold. For instance, for low-documentation loans originated in 2006, the average loan-to-value ratio across the collapsed FICO spectrum is 85%, whereas our estimated discontinuity is only -1.05%, a 1.2% difference. Similarly for the interest rate, for low-documentation loans originated in 2005, the average interest rate is 8.2%, and the difference on either side of the credit score cutoff is only about -0.091%, a 1% difference. Permutation tests reported in Table A.IV confirm that these differences are not outliers relative to the estimated jumps at other locations in the distribution.

Additional contract terms, such as the presence of a prepayment penalty, or whether the not the loan is ARM, FRM or interest only/balloon are also similar around the 620 threshold (results not shown). In addition, if loans have second liens, then a combined LTV (CLTV) ratio is calculated. We find no difference in the CLTV ratios around the threshold for those borrowers with more than one lien on the home. Finally, low documentation loans often do not require that borrowers provide information about their income, so there is only a subset of our sample which provides a debt-to-income (DTI) ratio for the borrowers. Among this subsample, there is no difference in DTI around the 620 threshold in low documentation loans. For brevity, we report only the permutation tests for these contract terms in Table A.IV.

Next, we examine whether the characteristics of borrowers differ systematically around the credit threshold. In order to evaluate this, we look at the distribution of the population of borrowers across the FICO spectrum for low documentation loans. The data on borrower demographics comes from Census 2000 and is at the zip code level. As can be seen from Figure 5, median household income of the zip codes of borrowers around the credit thresholds look very similar for low documentation loans. We plotted similar distributions for average percent minorities residing in the zip code, and average house value in the zip code across the FICO spectrum (unreported) and again find no differences around the credit threshold.[15]

We use the same specification as equation (1), this time with the borrower demographic characteristics as dependent variables and present the results formally in the appendix (Table A.II). Consistent with the patterns in the figures, permutation tests (unreported) reveal no differences in borrower demographic characteristics around the credit score threshold. Overall, our results indicate that observable characteristics of loans and borrowers are not different around the credit threshold.

[15]Of course, since the census data is at the zip code level, we are to some extent smoothing our distributions. We note, however, that when we conduct our analysis on differences in number of loans (from Section IV.B), aggregated at the zip code level, we still find jumps around the credit threshold within each individual zip code.

IV.D Performance of Loans

We now focus on the performance of the loans that are originated close to the credit score threshold. Note that our analysis in Section IV.C suggests that there is no difference in terms of observable hard information about contract terms or about borrower demographic characteristics around the credit score thresholds. Nevertheless, we will control for these differences when evaluating the subsequent performance of loans in our logit regressions. If there is any remaining difference in the performance of the loans above and below the credit threshold, it can be attributed to differences in unobservable soft information about the loans.

We estimate the differences in default rates on either side of the cutoff using the same framework as equation (1), using the dollar-weighted fraction of loans defaulted within 10-15 months of origination as the dependent variable, Y_i. This fraction is calculated as the dollar amount of unpaid loans in default divided by the total dollar amount originated in the same cohort. We classify a loan as under default if any of the conditions is true: (a) payments on the loan are 60+ days late as defined by Office of Thrift Supervision; (b) the loan is in foreclosure; or (c) the loan is real estate owned (REO), i.e. the bank has re-taken possession of the home.[16]

We collapse the data into one-point FICO bins and estimate seventh-order polynomials on either side of the threshold for each year. By estimating the magnitude of β in each year separately, we ensure that no one cohort (or vintage) of loans is driving our results. As shown in Figures 6A to 6F, the low documentation loans exhibit discontinuities in default rates at the FICO score of 620. A year by year estimate is presented in Panel A of Table III. Contrary to what one might expect, around the credit threshold, we find that loans of higher credit scores actually default *more often* than lower credit loans in the post-2000 period. In particular for loans originated in 2005, the estimate of β is .023 (t-stat=2.10), and the mean delinquency rate is .078, suggesting a 29% increase in defaults to the right of the credit score cutoff. Similarly, in 2006, the estimated size of the jump is .044 (t-stat=2.68), the mean delinquency rate for all FICO bins is .155, which is again a 29% increase in defaults around the FICO score threshold.

Panel B presents results of permutation tests, estimated on the residuals obtained after pooling delinquency rates across years and removing year effects. Besides the 60+ late delinquency definition used in Panel A, we also classify a loan in default if it is 90+ late in payments and if it is in foreclosure or REO. Our results yield similar, if not stronger, results. Compared to 620^- loans, 620^+ loans are on average 2.8% more likely to be in arrears of 90+ days, and 2.5% more likely to be in foreclosure or REO. Permutation tests p-values confirm that the jump in

[16] While there are two different definitions of delinquency used in the industry (Mortgage Banker's Association (MBA) definition and Office of Thrift Supervision (OTS) definition), we have followed the more stringent OTS definition. While MBA starts counting days a loan has been delinquent from the time a payment is missed, OTS counts days a loan is delinquent one month *after* the first payment is missed.

defaults at 620 using all the definitions of default are extreme outliers to the rest of the delinquency distribution. For instance, with default defined as foreclosure/REO, the p value for the discontinuity at 620 is 0.004. That we find similar results using different default definitions is consistent with high levels of rollover, whereby loans which are delinquent continue to reach deeper levels of delinquency. As shown in internet appendix Table 1, more than 80% of loans which are 60 days delinquent reach 90+ days delinquent within a year, and 66% of loans which are 90 days delinquent reach foreclosure twelve months after in the low documentation market.

While previous default definitions were dollar-weighted, we also use the raw number of loans in default to estimate the magnitude of the discontinuity in loan performance around the FICO threshold. The unweighted results with 60+ delinquency are also presented Panel B, and continue to exhibit a pattern of higher credit scores leading to higher default rates around the 620 threshold. In fact, the results are statistically stronger than the 60+ weighted results, with a permutation test p-value based on the pooled estimates of 0.004 and the discontinuity estimate being significant in all the years (unreported; see internet appendix Figure 4).

To show how delinquency rates evolve over the age of the loan, in Figure 7 we plot the delinquency rates of 620^{+} and 620^{-} for low documentation loans (dollar weighted) by loan age. As discussed earlier, we restrict our analysis to about two years after the loan has been originated. As can be seen from the figure, the differences in the delinquency rates are stark. The differences begin around four months after the loans have been originated and persist up to two years. Differences in default rates also seem quite large in terms of magnitudes. Those with a credit score of 620^{-} are about 20% less likely to default after a year as compared to loans of credit score 620^{+}.[17]

An alternative methodology is to measure the performance of each unweighted loan by tracking whether or not it became delinquent and estimate logit regressions of the following form:

$$Y_{ikt} = \Phi\left(\alpha + \beta T_{it} + \gamma_1 X_{ikt} + +\delta_1 T_{it} * X_{ikt} + \mu_t + \epsilon_{ikt} \right). \qquad (2)$$

This logistic approach complements the regression discontinuity framework, as we restrict the sample to the 10 FICO points in the immediate vicinity of 620 in order to maintain the same local interpretation of the RD results. Moreover, we are also able to directly control for the possibly endogenous loan terms around the threshold. The dependent variable is an indicator

[17]Note that Figure 7 does not plot cumulative delinquencies. As loans are paid out, say after a foreclosure, the unpaid balance for these loans falls relative to the time when they entered into a 60+ state. This explains the dip in delinquencies in the figure after about 20 months. Our results are similar if we plot cumulative delinquencies, or delinquencies which are calculated using the unweighted number of loans. Also note that the fact that we find no delinquencies early on in the duration of the loan is not surprising, given that originators are required to take back loans on their books if the loans default within three months.

variable (*Delinquency*) for loan i originated in year t that takes a value of 1 if the loan is classified as under default in month k after origination as defined above. We drop the loan from the regression once it is paid out after reaching the REO state. T takes the value 1 if FICO is between 620 and 624, and 0 if it is between 615 and 619 for low documentation loans, thus restricting the analysis to the immediate vicinity of the cutoffs. Controls include FICO scores, the interest rate on the loan, loan-to-value ratio, borrower demographic variables, as well as interaction of these variables with T. We also include a dummy variable for the type of loan (adjustable or fixed rate mortgage). We control for the possible nonlinear effect of age of the loan on defaults by including three dummy variables – that take a value of 1 if the month since origination is between 0-10, 11-20 and more than 20 months respectively. Year of origination fixed effects are included in the estimation and standard errors are clustered at the loan level to account for multiple loan delinquency observations in the data.

As can be seen from the logit coefficients in Panel C of Table III, results from this regression are qualitatively similar to those reported in the figures. In particular, we find that β is positive when we estimate the regressions for low documentation loans. The economic magnitudes are similar to those in the figures as well. For instance, keeping all other variables at their mean level, low documentation loans with credit score of 620^- are about 10-25% less likely to default after a year as compared to low documentation loans of credit score 620^+. These are large magnitudes – for instance, note that the mean delinquency rate for low documentation loans is around 4.45%; the economic magnitude of the effects in Column (2) suggest that the difference in the absolute delinquency rate between loans around the credit threshold is around 0.5-1% for low documentation loans.[18]

To account for the possibility that lax screening might be correlated across different loans within the same vintage, we cluster the loans for each vintage and report the results in Columns (3) and (4). Note that the RD regressions (Panel A) estimated separately by year also alleviates concerns about correlated errors across different loans with the same vintage.

In the mortgage market, the other way for loans to leave the pool is to be repaid in full through refinancing or outright purchase, known as prepayment. This prepayment risk decreases the return to investing in mortgage-backed securities in a similar manner to default risk (see, e.g. Gerardi, Shapiro, and Willen 2007 and Mayer, Piskorski, and Tchistyi 2008). To assess whether there are any differences in actual prepayments around the 620 threshold, we plot the prepayment seasoning curve for all years 2001-2006 in Figure 8. As can be observed, prepayments of 620^+ and 620^- borrowers in the low documentation market are similar (also see permutation

[18] Our logistic specification is equivalent to a hazard model if we drop loans as soon as they hit the first indicator of delinquency (60 days in default) and include a full set of duration dummies. Doing so does not change the nature of our results.

test in Table A.IV). Nevertheless, to formally account for prepayment rates, we also estimate a competing risk model using both prepayment and default as means for exiting the sample. We use the Cox-proportional hazard model based on the econometric specification following Deng, Quigley and Van Order (2000). In unreported tests (internet appendix Table 6), we find results that are similar to our logistic specification.

Finally, the reported specification uses five-point bins of FICO scores around the threshold, but the results are similar (though less precise) if we restrict the bins to fewer FICO scores on either side of 620 (internet appendix Table 2). This issue is also fully addressed by the regression discontinuity results reported in Panels A and B, which use individual FICO score bins as the units of observation. In sum, we find that even after controlling for all observable characteristics of the loan contracts or borrowers, loans made to borrowers with *higher* FICO scores perform *worse* around the credit threshold.

IV.E Selection Concerns

Since our results are conditional on securitization, we conduct additional analyses to address selection explanations on account of borrowers, investors and lenders for the differences in the performance of loans around the credit threshold. First, contract terms offered to borrowers above the credit threshold might differ from those below the threshold and attract a riskier pool of borrowers. If this were the case, it would not be surprising if the loans above the credit threshold perform worse than those below it. As shown in Section IV.C, loan terms are smooth through the FICO score threshold. We also investigate the loan terms in more detail than in Section IV.C by examining the distribution of interest rates and loan-to-value ratios of contracts offered around 620 for low documentation loans.

Figure 9A depicts the Epanechnikov kernel density of the interest rate on low documentation loans in the year 2004 for two FICO groups – 620^{-} (615-619) and 620^{+} (620-624). The distribution of interest rates observed in the two groups lie directly on top of one another. A Kolmogorov-Smirnov test for equality of distribution functions cannot be rejected at the 1% level. Similarly, Figure 9B depicts density of LTV ratios on low documentation loans in the year 2004 for 620^{-} and 620^{+} groups. Again, a Kolmogorov-Smirnov test for equality of distribution functions cannot be rejected at the 1% level. The fact that we find that the borrowers characteristics are similar around the threshold (Section IV.C) also confirms that selection based on observables is unlikely to explain our results.[19]

[19] The equality of interest rate distributions also rules out differences in the expected cost of capital around the threshold as an alternative explanation. For instance, lenders could originate riskier loans above the threshold only because the expected cost of capital is lower due to easier securitization. However, in a competitive market, the interest rates charged for these loans should reflect the riskiness of the borrowers. In that case, as mean interest

Second, there might be concerns about selection of loans by investors. In particular, our results could be explained if investors could potentially cherry pick better loans below the threshold. The loan and borrower variables in our data are identical to the data upon which investors base their decisions (Kornfeld 2007). Furthermore, as shown in Section IV.C, these variables are smooth through the threshold, mitigating any concerns on selection by investors.[20]

Finally, strategic adverse selection on the part of lenders may also be a concern. Lenders could for instance keep loans of better quality on their balance sheet and offer only loans of worse quality to the investors. This concern is mitigated for several reasons. First, the securitization guidelines suggest that lenders offer the entire pool of loans to investors and that conditional on observables, SPVs largely follow a randomized selection rule to create bundles of loans. This suggests that securitized loans would look similar to those that remain on the balance sheet (Gorton and Souleles 2005; *Comptroller's Handbook* 1997).[21] In addition, this selection if at all present will tend to be more severe below the credit threshold, thereby biasing us against finding any effect of screening on performance.

We conduct an additional test which also suggests that our results are not driven by selection on the part of lenders. While banks may screen and then strategically hold loans on their balance sheets, independent lenders do not keep a portfolio of loans on their books. These lenders finance their operations entirely out of short-term warehouse lines of credit, have limited equity capital, and no deposit base to absorb losses on loans that they originate (Gramlich 2007). Consequently, they have limited motive for strategically choosing which loans to sell to investors. However, because loans below the threshold are more difficult to securitize and thus are less liquid, these independent lenders still have strong incentives to differentially screen these loans to avoid losses. We focus on these lenders to isolate the effects of screening in our results on defaults (Section IV.D).

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rates above and below the threshold are the same (Section IV.C), lenders must have added riskier borrowers above the threshold – resulting in a more dispersed interest rate distribution above the threshold. Our analysis in Figure 9A shows that this is not the case.

[20] An argument might also be made that banks screen similarly around the credit threshold but are able to sell portfolio of loans above and below the threshold to investors with different risk tolerance. If this were the case, it could potentially explain our results in Section IV.D. This does not seem likely. Since all the loans in our sample are securitized, our results on performance on loans around the credit threshold are *conditional* on securitization. Moreover, securitized loans are sold to investors in pools which contains a mix of loans from the entire credit score spectrum. As a result, it is difficult to argue that loans of 620^{-} are purchased by different investors as compared to loans of 620^{+}.

[21] We confirmed this fact by examining a subset of loans held on the lenders' balance sheets. The alternative dataset covers the top 10 servicers in the subprime market (more than 60% of the market) with details on performance and loan terms of loans that are securitized or held on the lenders' balance sheet. We find no differences in the performance of loans that are securitized relative to those kept by lenders, around the 620 threshold. Results of this analysis are available upon request.

To test this, we classify the lenders into two categories – banks (banks, subsidiaries, thrifts) and independents – and conduct the performance results only for sample of loans originated by independent lenders. It is difficult to identify all the lenders in the database since many of the lender names are abbreviated. In order to ensure that we are able to cover a majority of our sample, we classify the top 50 lenders (by origination volume) across the years in our sample period, based on a list from the publication 'Inside B&C mortgage'. In unreported results, we confirm that independent lenders also follow the rule of thumb for low documentation loans. Moreover, low documentation loans securitized by independents with credit score of 620^{-} are about 15% less likely to default after a year as compared to low documentation loans securitized by them with credit score 620^{+}.[22] Note that the results in the sample of loans originated by lenders without a strategic selling motive are similar in magnitude to those in the overall sample (which includes other lenders that screen and then may strategically sell). This finding highlights that screening is the driving force behind our results.

IV.F Additional Variation From a Natural Experiment

IV.F.1 Unrelated Optimal Rule Of Thumb

So far we have worked under the assumption that the 620 threshold is related to securitization. One could plausibly argue, in the spirit of Baumol and Quandt (1964), that this rule of thumb could have been set by lenders as an optimal cutoff for screening that is unrelated to differential securitization. Ruling this alternative out requires an examination of the effects of the threshold when the ease of securitization varies, everything else equal. To achieve this, we exploit a natural experiment that involves the passage of anti-predatory lending laws in two states which reduced securitization in the subprime market drastically. Subsequent to protests by market participants, the laws were substantially amended and the securitization market reverted to pre-law levels. We use these laws to examine how the main effects vary with the time series variation in the ease of securitization likelihood around the threshold in the two states.

In October 2002, the Georgia Fair Lending Act (GFLA) went into effect, imposing anti-predatory lending restrictions which at the time were considered the toughest in the United States. The law allowed for unlimited punitive damages when lenders did not comply with the provisions and that liability extended to holders in due course. Once GFLA was enacted, the market response was swift. Fitch, Moodys, and S&P were reluctant to rate securitized pools that included Georgia loans. In effect, the demand for the securitization of mortgage loans from Georgia fell drastically during the same period. In response to these actions, the Georgia Legislature amended GLFA in early 2003. The amendments removed many of the

[22] More specifically, in a specification similar to Column (2) in Panel C of Table III, we find that the coefficient on the indicator T(FICO$\geq$ 620) is 0.67 (t=3.21).

GFLAs ambiguities and eliminated covered loans. Subsequent to April 2003, the market revived in Georgia. Similarly, New Jersey enacted its law, the New Jersey Homeownership Security Act of 2002, with many provisions similar to those of the Georgia law. As in Georgia, lenders and ratings agencies expressed concerns when the New Jersey law was passed and decided to substantially reduce the number of loans that were securitized in these markets. The Act was later amended in June 2004 in a way that relaxed requirements and eased lenders' concerns.

If lenders use 620 as an optimal cutoff for screening unrelated to securitization, we expect the passage of these laws to have no effect on the differential screening standards around the threshold. However, if these laws affect the differential ease of securitization around the threshold, our hypothesis would predict an impact on the screening standards. As 620^{+} loans become relatively more difficult to securitize, lenders would internalize the cost of collecting soft information for these loans to a greater degree. Consequently, the screening differentials we observed earlier should attenuate during the period of enforcement. Moreover, we expect the results described in Section IV.D to appear only during the periods when the differential ease of securitization around the threshold is high, i.e., before the law was passed as well as in the period after the law was amended.

Our experimental design examines the ease of securitization and performance of loans above and below the credit threshold in both Georgia and New Jersey during the period when the securitization market was affected and compares it with the period before the law was passed and the period after the law was amended. To do so, we estimate equations (1) and (2) with an additional dummy variable that captures whether or not the law is in effect (*NoLaw*). We also include time fixed effects to control for any macroeconomic factors independent of the laws.

The results are striking. Panel A of Table IV confirms that the discontinuity in the number of loans around the threshold diminishes during a period of strict enforcement of anti-predatory lending laws. In particular, the difference in number of loans securitized around the credit thresholds fell by around 95% during the period when the law was passed in Georgia and New Jersey. This effectively nullified any meaningful difference in the ease of securitization above the FICO threshold. Another intuitive way to see this is to compare these jumps in the number of loans with jumps in states which had similar housing profiles as Georgia and New Jersey before the law was passed (e.g., Texas in 2001). For instance, relative to the discontinuity in Texas, the jump during the period when the law was passed is about 5%, whereas the jumps are of comparable size both before the law is passed and after the law was amended. In addition, the results also indicate a rapid return of a discontinuity after the law is revoked. It is notable that this time horizon is too brief for any meaningful change in the housing stock (Glaeser and Gyourko 2005), or in the underlying demand for home ownership.

Importantly, our performance results follow the same pattern as well. Columns (1) and (2) of Panel B show that the default rates for 620^+ loans are below that of 620^- loans in both Georgia and New Jersey *only* when the law was in effect. In addition, when the law was either not passed or was amended, we find that default rates for loans above the credit threshold is similar to loans below the credit threshold. This upward shift in the default curve above the 620 threshold is consistent with the results reported in Section IV.D. Taken together, these results suggest that our findings are indeed related to differential securitization at the credit threshold and that lenders were not blindly following the rule of thumb in all instances.

IV.F.2 Manipulation Of Credit Scores

Having confirmed that lenders are screening more at 620^- than 620^+, we assess whether borrowers were aware of the differential screening around the threshold. Even though there is no difference in contract terms around the cutoff, screening is weaker above the 620 score than below it, and this may create an incentive for borrowers to manipulate their credit score. If FICO scores could be manipulated, lower quality borrowers might artificially appear at higher credit scores. This behavior would be consistent with our central claim of differential screening around the threshold. Note that as per the rating agency (Fair Isaac), it is difficult to strategically manipulate one's FICO score in a targeted manner. Nevertheless, to examine the response of borrowers more closely, we exploit the variation generated from the same natural experiment.

If FICO scores tend to be quite sticky and it takes relatively long periods of time (more than 3 to 6 months) to improve credit scores, as Fair Isaac claims, we should observe that the difference in performance around the threshold should take time to appear after the laws are reversed. Restricting our analysis to loans originated within six months after the laws were reversed, Columns (3) and (4) of Panel B (Table IV) show that the reversal of anti-predatory lending laws has immediate effects on the performance of loans that are securitized. This result suggests that borrowers might not have been aware of the differential screening around the threshold or were unable to quickly manipulate their FICO scores. Overall the evidence in this section is consistent with Mayer and Pence (2008), who find no evidence of manipulation of FICO scores in their survey of the subprime market.[23]

[23] As a further check, we obtained another dataset of subprime loans that continues to track the FICO scores of borrowers after loan origination. Borrowers who manipulate their FICO scores before loan issuance should experience a decline in FICO score shortly after receiving a loan (because a permanent change in the credit score cannot be considered manipulation). Consistent with evidence for no manipulation around the threshold, we find that both 620^+ and 620^- borrowers are as likely to experience such a reduction within a quarter of obtaining a loan. Results of this analysis are available upon request.

IV.G Additional Confirmatory Tests

GSE Selection

Although the subprime market is dominated by the non-agency sector, one might worry that the GSEs may differentially influence the selection of borrowers into the subprime market through their actions in the prime market. For instance, the very best borrowers above the 620 threshold might select out of the subprime market in search of better terms in the prime market. We establish several facts to confirm that this is not the case.

First, the natural experiment we discuss in Section IV.F suggests that prime-influenced selection is not at play. The anti-predatory laws were targeted primarily towards the subprime part of the market (Bostic et al. 2007), while leaving the prime part of the market relatively unaffected. To confirm the behavior of the prime market during the enforcement of anti-predatory laws, we rely on another dataset of mortgages in the US which also covers the agency loan market. The data is collected from the top 10 US servicers and covers the period 2001 to 2006. As reported in Panel A of Table V, during the natural experiment, it was no more difficult to obtain an agency loan than before or after the law was in effect. Similarly, in unreported tests we find that contractual terms (such as LTV ratios and interest rates) around 620 see no change across time periods. Furthermore, in the prime market, there were no differences in defaults around the 620 threshold across the time periods (Table V, Panel B). Since borrower quality in the prime market did not change around 620 threshold across the two time periods, if there was indeed selection, the very best 620^+ subprime borrowers should have selected out into the prime market even while the laws were in place. As a result, we should have found that 620^+ borrowers in subprime market continue to default more than 620^- borrowers even when the law is in place. As we showed earlier in Table IV, this is not the case.

Second, the dataset confirms that Freddie Mac and Fannie Mae primarily do not buy loans with credit scores around FICO of 620 (especially low documentation loans). This is consistent with anecdotal evidence that the role of active subprime securitization in recent years had shifted to non-agency sector (Gramlich, 2007). In unreported permutation tests (see internet appendix Table 4, Panel A), we also find that the number of loans in the agency market is smooth around the 620 threshold. In addition, the loan terms and default rates are also smooth. Together these results suggest that, in general, there seems to be no differential selection in terms of number of loans or quality of loans around the 620 cutoff.

Third, if our results in the low documentation market around 620 threshold are driven by differential GSE selection, we should observe no differences in defaults when we combine the loans from agency with low documentation subprime loans around the 620 threshold. If it were purely selection, lower performance above the threshold in the low documentation subprime loans would be offset by differentially higher quality loans selected into the agency market. Unreported results (internet appendix Table 5) show that there are still differences in default rates around the 620 threshold when we examine the agency loans and low documentation subprime loans together.

Finally, we examine the set of borrowers in the subprime market (around 620) who are offered similar contractual terms as those offered in the prime market. If there is indeed selection into the prime market, it is likely based on contractual terms offered to borrowers. By examining borrowers who are offered similar contractual terms in the subprime market, we are able to isolate our analysis to borrowers of similar quality as those who are possibly attracted by GSEs (i.e., the good quality borrowers). For this subset of subprime borrowers, we are able to show that 620^{+} loans still default more than 620^{-} loans (internet appendix Table 4, Panel B). This evidence further suggests that selection by GSEs is unlikely to explain our results.

Other Thresholds

In the data, we also observe smaller jumps in other parts of the securitized loan FICO distribution as other ad-hoc cutoffs have appeared in the market in the past three years (e.g., 600 for low documentation in 2005 and 2006). We remain agnostic as to why or how these other cutoffs have appeared; either due to greater willingness to lend to riskier borrowers, or changing use of automated underwriting which generally included a matrix of qualifications and loan terms including FICO buckets. Several comments about why we focus on the 620 threshold are therefore in order.

First, the 620 cutoff is the only threshold that is actively discussed by the GSEs in their lending guidelines, where the ease of securitization is higher on the right side of the threshold (see internet appendix exhibit 1). This feature is essential for us to disentangle the effect of lax screening on defaults from what a change in FICO score might predict. As increasing FICO scores predict decreasing default rates, performing our analysis with any cut-off where ease of securitization is lower on the right side of threshold would not allow us to use this identification. For instance, consider the cutoff of 660 that is also discussed in the GSE guidelines and where we observe a jump in securitization. The ease of securitization is lower on the right hand side of this cutoff, i.e., the unconditional probability of securitization is lower at 660^{+} relative to 660^{-}, suggesting that 660^{+} loans would be more intensively screened and would default less frequently than 660^{-}. However, it would be impossible to disentangle this effect from just a mechanical effect of 660^{+} FICO loans being more creditworthy and thus defaulting less often than 660^{-} loans (by construction). This subtle advantage of the 620 cutoff is crucial to our identification strategy and rules out the use of several other ad-hoc thresholds.

Moreover, to identify the effects of securitization on screening by lenders, the liquidity differential for the loan portfolios around the threshold has to be large enough. Since 620 is the largest jump we observe in the loan distribution, it is a natural choice. This is confirmed in the permutation tests, which show that $FICO = 620$ has the smallest p-value (and is thus largest outlier) among *all* the visible discontinuities for *each year* in our sample. While other cutoffs may also induce slight differences in screening effort in some years, these differences may be small to make any meaningful inferences. In results not shown, we analyzed some of these other thresholds and find results for delinquencies that are consistent with those reported for the predominant cutoff (620), but are indeed quite small in magnitude.

Other Tests

We also conduct several falsification tests, repeating our analysis at other credit scores where there is no jump in securitization. In sharp contrast to the results reported in Section IV.D, the higher credit score bucket defaults *less* than the lower credit score bucket. This is consistent with the results of the permutation tests reported above, which estimate *every* false discontinuity and compare it to the discontinuity at 620. Moreover, as we will show in Section V, full documentation loans do not see any jumps at this threshold. We plot the delinquency rates of 620^+ and 620^- for full documentation loans (2001-2006) in Figure 10 and find loans made at lower credit scores are more likely to default.[24]

As further tests of our hypothesis, we also conducted our tests in the refinance market, and find a similar rule of thumb and similar default outcomes around the 620 threshold in this market. Finally, we re-estimated our specifications with state, lender and pool fixed effects to account for multiple levels of potential variation in the housing market and find qualitatively similar results.

V Did Hard Information Matter?

The results presented above are for low documentation loans, which necessarily have an unobserved component of borrowers' creditworthiness. In the full documentation loan market, on the other hand, there is no omission of hard information on the borrower's ability to repay. In this market, we identify a credit threshold at the FICO score of 600, the score that Fair Isaac (and the three credit repositories) advises lenders as a bottom cutoff for low risk borrowers. They note "...anything below 600 is considered someone who probably has credit problems that need to be addressed..."(see www.myfico.com). Similarly Fannie Mae in its guidelines notes "...a borrower with credit score of 600 or less has a high primary risk..." (see www.allregs.com/efnma/doc/).

[24]This test can also provide insight into the issue of GSE selection discussed earlier. Since 620^+ full documentation loans do not default more than 620^- loans, differential selection into the agency market must account for this fact as well. One possibility is selection on the basis of debt-to-income ratios. To examine this, we compare DTI ratios in the full and low documentation markets. Unreported tests (internet appendix Table 3) show that the DTI ratios are similar around the threshold and thus cannot entirely explain results across the two types of loans.

The Consumer Federation of America along with Fair Isaac (survey report in March 2005) suggests that "...FICO credit scores range from 300-850, and a score above 700 indicates relatively low credit risk, while scores below 600 indicate relatively high risk which could make it harder to get credit or lead to higher loan rates." Einav, Jenkins and Levin (2007) make a similar observation when they note that "...a FICO score above 600 [is] a typical cut-off for obtaining a standard bank loan."

Figure 11 reveals that there is a substantial increase in the number of full documentation loans above the credit threshold of 600. This pattern is consistent with the notion that lenders are more willing to securitize at a lower credit threshold (600 vs. 620) for full documentation loans since there is less uncertainty about these borrowers relative to those who provide less documentation. The magnitudes are again large – around 100% higher at 600^+ than at 600^- in 2004 – for full documentation loans. In Panel A of Table VI, we estimate regressions similar to equation (1) and find the coefficient estimate is also significant at 1% and is on average around 100% (from 80 to 141%) higher for 600^+ as compared to 600^- for post-2000 loans. Again, if the underlying creditworthiness and the demand for mortgage loans (at a given price) is the same for potential buyers with a credit score of 600^- or 600^+, as the credit bureaus claim, this result confirms that it is easier to securitize full documentation loans above the 600 FICO threshold. We repeated a similar analysis for loan characteristics (LTV and interest rates) and borrower demographics and find no differences for full documentation loans above and below the credit score of 600. Table A.III in appendix presents the estimates from the regressions (Table A.IV provides permutation test estimates corresponding to these loan terms).

Interestingly, we find that for full documentation loans, those with credit scores of 600^- (FICO between 595 and 599) are about as likely to default after a year as compared to loans of credit score 600^+ (FICO between 601 and 605) for the post-2000 period. Both Figures 12 and 13 and results in Panels B, C and D of Table VI support this conjecture. Following the methodology used in Figures 6 and 7, we show the default rates annually across the FICO distribution (Figure 12) and across the age of the loans (Figure 13). The estimated effects of the ad-hoc rule on defaults are negligible in all specifications.

The absence of differences in default rates around the credit threshold, while maintaining the same magnitude of the jump in the number of loans, is consistent with the notion that the pattern of delinquencies around the low-documentation threshold are primarily due to the soft information of the borrower. With so much information collected by the lender for full documentation loans, there is less value to collecting soft information. Consequently, for full documentation loans there is no difference in how the loans perform subsequently after hard information has been controlled for. Put another way, differences in returns to screening are attenuated due to the presence of more hard information.

VI Discussion

In the wake of the subprime mortgage crisis, a central question confronting market participants and policymakers is whether securitization had an adverse effect on the ex-ante screening effort of loan originators. Comparing characteristics of the loan market above and below the ad-hoc credit threshold, we show that a doubling of securitization volume is on average associated with about a 10-25% increase in defaults. Notably, our empirical strategy delivers only inferences on differences in the performance of loans around this threshold. While we cannot infer what the optimal level of screening at each credit score ought to be, we conclude from our empirical analysis that there was a causal link between ease of securitization and screening. That we find any effect on default behavior in one portfolio compared to another with virtually identical risk profiles, demographic characteristics, and loan terms suggests that the ease of securitization may have a direct impact on incentives elsewhere in the subprime housing market, as well as in other securitized markets.

The results of this paper, in particular from the anti-predatory lending laws' natural experiment, confirm that lender behavior in the subprime market did change based on the ease of securitization. This suggests that existing securitization practices did not ensure that a decline in screening standards would be counteracted by requiring originators to hold more of the loans' default risk. If lenders were in fact holding on to optimal risk where it was easier to securitize, there should have been no differences in defaults around the threshold. This finding resonates well with concerns surrounding the subprime crisis that, in an environment with limited disclosure on who holds what in the originate-to-distribute chain, there may have been insufficient 'skin in the game' for some lenders (Blinder 2007; Stiglitz 2007). At the same time, the results further suggest that the breakdown in the process only occurred for loans where soft information was particulary important. With enough hard information, as in the full documentation market, there may be less value in requiring market participants to hold additional risk to counteract the potential moral hazard of reduced screening standards.

In a market as competitive as the market for mortgage-backed securities, our results on interest rates are puzzling. Lenders' compensation on either side of the threshold should reflect differences in default rates, and yet we find that the interest rates to borrowers are similar on either side of 620. The difference in defaults, despite similar compensation around the threshold, suggests that there may have been some efficiency losses. Of course, it is possible that from the lenders' perspective, a higher propensity to default above the threshold could have exactly offset the benefits of additional liquidity – resulting in identical interest rates around the threshold.

Our analysis remains agnostic about whether investors accurately priced the moral hazard aspects of securitization. It may have been the case that moral hazard existed in this market though investors appropriately priced persistent differences in performance around the threshold (see Rajan et al. 2008). On the other hand, developing an arbitrage strategy to exploit this opportunity may have been prohibitively difficult given that loans are pooled across the FICO spectrum before they are traded. In addition, these fine differences in performance around the FICO threshold could have been obscured by the performance of other complex loan products in the pool. Understanding these aspects of investor behavior warrants additional investigation.

It is important to note that we refrain from making any welfare claims. Our conclusions should be directed at securitization practices as they were during the subprime boom rather than at the optimally designed originate-to-distribute model. We believe securitization is an important innovation and has several merits. It is often asserted that securitization improves the efficiency of credit markets. The underlying assumption behind this assertion is that there is no information loss in transmission, even though securitization increases the distance between borrowers and investors. The benefits of securitization are limited by information loss, and in particular the costs we document in the paper. More generally, what types of credit products should be securitized? We conjecture that the answer depends crucially on the information structure: loans with more hard information are likely to benefit from securitization as compared to loans that involve soft information. A careful investigation of this question is a promising area for future research.

More broadly, our findings caution against policy that emphasizes excessive reliance on default models. Our research suggests that by relying entirely on hard information variables like FICO scores, these models ignore essential elements of strategic behavior on the part of lenders which are likely to be important. The formation of a rule of thumb, even if optimal (Baumol and Quandt 1964), has an undesirable effect on the incentives of lenders to collect and process soft information. As in Lucas (1976), this strategic behavior can alter the relationship between observable borrower characteristics and default likelihood, rather than moving along the previous predicted relationship. Incorporating these strategic elements into default models, although challenging, is another important direction for future research.

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Table I

Summary Statistics

Information on subprime home purchase loans comes from LoanPerformance. Sample period is 2001-2006. See text for sample selection.

Panel A: Summary Statistics By Year

	Low Documentation			Full Documentation		
	Number of Loans	Mean Loan-To-Value	Mean FICO	Number of Loans	Mean Loan-To-Value	Mean FICO
2001	35,427	81.4	630	101,056	85.7	604
2002	53,275	83.9	646	109,226	86.4	613
2003	124,039	85.2	657	194,827	88.1	624
2004	249,298	86.0	658	361,455	87.0	626
2005	344,308	85.5	659	449,417	86.9	623
2006	270,751	86.3	655	344,069	87.5	621

Panel B: Summary Statistics Of Key Variables

	Low Documentation		Full Documentation	
	Mean	Std. Dev.	Mean	Std. Dev.
Average loan size ($000)	189.4	132.8	148.5	116.9
FICO score	656.0	50.0	621.5	51.9
Loan-to-Value ratio	85.6	9.8	87.1	9.9
Initial Interest Rate	8.3	1.8	8.2	1.9
ARM (%)	48.5	50.0	52.7	49.9
Prepayment penalty (%)	72.1	44.8	74.7	43.4

Table II

Discontinuity in Number of Low Documentation Loans

This table reports estimates from a regression which uses the number of low documentation loans at each FICO score as the dependent variable. In order to estimate the discontinuity (FICO$\geq$ 620) for each year, we collapse the number of loans at each FICO score and estimate flexible seventh-order polynomials on either side of the 620 cutoff, allowing for a discontinuity at 620. We report t-statistics in parentheses. Permutation tests, which allow for a discontinuity at every point in the FICO distribution, confirm that jumps for each year are significantly larger than those found elsewhere in the distribution (see Section IV.B for more details). For brevity, we report a permutation test estimate from pooled regressions with time fixed effects removed to account for vintage effects. $FICO = 620$ has the smallest permutation test p-value (and is thus largest outlier) among *all* the visible discontinuities in our sample.

Number of Low Documentation Loans

Year	FICO$\geq$ 620 (β)	t-stat	Observations	R^2	Mean
2001	36.83	(2.10)	299	0.96	117
2002	124.41	(6.31)	299	0.98	177
2003	354.75	(8.61)	299	0.98	413
2004	737.01	(7.30)	299	0.98	831
2005	1,721.64	(11.78)	299	0.99	1,148
2006	1,716.49	(6.69)	299	0.97	903
Pooled Estimate (t-stat) [Permutation Test p-value]			781.87 (4.14) [0.003]		

Table III

Delinquencies in Low Documentation Loans around the Credit Threshold

In Panel A, we estimate the differences in default rates using a flexible seventh-order polynomial on either side of the 620 cutoff, allowing for a discontinuity at 620. 60+ dollar-weighted fraction of loans defaulted within $10-15$ months is the dependent variable. In Panel B, we present estimates from permutation tests from pooled regressions with time fixed effects removed to account for vintage effects using specification similar to Panel A. Permutation tests confirm that the discontinuity at 620 has the smallest p-value (and is thus largest outlier) in our sample. We use alternative definitions of defaults as the dependent variable. In Panel C, we estimate differences in default rates on either side of the 620 FICO cut off using a logit regression. The dependent variable is the delinquency status of a loan in a given month that takes a value 1 if the loan is classified as under default, as defined in the text. Controls include borrower and loan terms discussed in Section IV. We report t-statistics are reported in parenthesis (marginal effects are reported in square brackets).

Panel A: Dollar Weighted Fraction Of Loans Defaulted (60+ Delinquent)

Year	FICO$\geq$ 620 (β)	t-stat	Observations	R^2	Mean
2001	0.005	(0.44)	254	0.58	0.053
2002	0.010	(2.24)	254	0.75	0.051
2003	0.022	(3.47)	254	0.83	0.043
2004	0.013	(1.86)	254	0.79	0.049
2005	0.023	(2.10)	254	0.81	0.078
2006	0.044	(2.68)	253	0.57	0.155

Panel B: Permutation Tests For Alternative Default Definitions
(Pooled 2001-2006 with Time Fixed Effects)

Dependent Variable (Default Definition)	FICO$\geq$ 620 (β)	t-stat	Permutation Test p-value	Observations	R^2	Mean
60+ (dollar weighted)	0.019	(3.32)	0.020	1523	0.66	0.072
90+ (dollar weighted)	0.028	(4.67)	0.006	1525	0.70	0.065
Foreclosure+ (dollar weighted)	0.025	(6.25)	0.004	1525	0.71	0.048
60+ (unweighted)	0.025	(5.00)	0.004	1525	0.65	0.073

Panel C: Delinquency Status Of Loans

	Pr(Delinquency)=1			
	(1)	(2)	(3)	(4)
FICO$\geq$620	0.12	0.48	0.12	0.48
	[0.004]	[0.011]	[0.004]	[0.011]
	(3.42)	(2.46)	(2.10)	(2.48)
Observations	1,393,655	1,393,655	1,393,655	1,393,655
Pseudo R^2	0.088	0.116	0.088	0.116
Other Controls	Yes	Yes	Yes	Yes
FICO$\geq$620*Other Controls	No	Yes	No	Yes
Time Fixed Effects	No	Yes	No	Yes
Clustering Unit	Loan id	Loan id	Vintage	Vintage
Mean Delinquency (%)	4.45			

Table IV

Number of Loans and Delinquencies in Low Documentation Loans around the Credit Threshold: Evidence From A Natural Experiment

This table reports the estimates of the regressions on differences in number of loans and performance of loans around the credit thresholds. We use specifications similar to Table II in Panel A to estimate the number of loans regressions and Table III (Panel C) in Panel B to estimate delinquency regressions. We restrict our analysis to loans made in Georgia and New Jersey. *NoLaw* is a dummy that takes a value 1 if the anti-predatory law was not passed in a given year or was amended and a value 0 during the time period when then the law was passed. Permutation tests confirm that the discontinuity in number of loans at 620 when the law is not passed has the smallest p-value (and is thus largest outlier) in the Georgia and New Jersey sample. We report t-statistics in parentheses (marginal effects are reported in square brackets).

Panel A: Number of Low Documentation Loans

Year	FICO$\geq$ 620 (β)	t-stat	Observations	R^2	Mean
During Law	10.71	(2.30)	294	0.90	16
Pre & Post Law	211.50	(5.29)	299	0.96	150

Panel B: Delinquency Status Of Low Documentation Loans

	Pr(Delinquency)=1			
	Entire Period 2001-2006		During Law and Six months After	
	(1)	(2)	(3)	(4)
FICO$\geq$620	-0.91	-0.91	-1.02	-1.02
	[0.043]	[0.043]	[0.030]	[0.030]
	(1.78)	(2.00)	(1.69)	(2.12)
FICO$\geq$620*NoLaw	.88	.88	1.13	1.13
	[0.040]	[0.040]	[0.034]	[0.034]
	(1.90)	(1.94)	(1.79)	(1.93)
Observations	109,536	109,536	14,883	14,883
Other Controls	Yes	Yes	Yes	Yes
FICO$\geq$620* Other Controls	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
Pseudo R^2	0.06	0.06	0.05	0.05
Clustering Unit	Vintage	Loan id	Vintage	Loan id
Mean Delinquency (%)	6.1		4.2	

Table V

Number of Loans and Delinquencies in Agency (GSE/Prime) Loans around the Credit Threshold: Evidence From A Natural Experiment

This table reports the estimates of the regressions on differences in number of loans and performance of loans around the credit thresholds. The analysis is restricted to prime loans made in Georgia and New Jersey. *NoLaw* is a dummy that takes a value 1 if the anti-predatory law was not passed in a given year or was amended and a value 0 during the time period when then the law was passed. Permutation tests confirm that the discontinuity in number of loans at 620 when the law is not passed or passed is no different from estimated jumps at other locations in the distribution in the Georgia and New Jersey sample. We report t-statistics in parentheses (marginal effects are reported in square brackets).

Panel A: Number of Prime Loans

Year	FICO$\geq$ 620 (β)	t-stat	Observations	R^2	Mean
During Law	4.80	(2.70)	249	0.88	20.30
Pre & Post Law	2.33	(1.02)	268	0.92	22.80

Panel B: Delinquency Status Of Prime Loans

	Pr(Delinquency)=1	
	60+ Delinquent	90+ Delinquent
	2001-2006	2001-2006
	(1)	(2)
FICO$\geq$620	-0.026	-0.029
	[0.001]	[0.001]
	(0.19)	(0.10)
FICO$\geq$620*NoLaw	-0.004	-0.003
	[0.0004]	[0.0004]
	(0.03)	(0.05)
Observations	56,300	56,300
Other Controls	Yes	Yes
FICO$\geq$620* Other Controls	Yes	Yes
Time Fixed Effects	Yes	Yes
Clustering Unit	Vintage	Vintage
Pseudo R^2	0.01	0.02
Mean Delinquency (%)	5.2	3.1

Table VI

Number of Loans and Delinquencies around the Credit Threshold for Full Documentation Loans

This table reports the estimates of the regressions on differences in number of loans and performance of loans around the credit threshold of 600 for full documentation loans. We use specifications similar to Table II in Panel A to estimate the number of loans regressions and Table III (Panels A, B and C) in Panels B, C and D to estimate delinquency regressions. Permutation tests confirm that $FICO = 600$ has the smallest permutation test p-value (and is thus largest outlier) among *all* the visible discontinuities in the full documentation loan sample. We report t-statistics in parentheses (marginal effects are reported in square brackets).

Panel A: Number of Full Documentation Loans

Year	FICO≥ 600 (β)	t-stat	Observations	R^2	Mean
2001	306.85	(5.70)	299	0.99	330
2002	378.49	(9.33)	299	0.99	360
2003	780.72	(11.73)	299	0.99	648
2004	1,629.82	(8.91)	299	0.99	1205
2005	1,956.69	(4.72)	299	0.98	1499
2006	2,399.48	(6.97)	299	0.98	1148
Pooled Estimate (t-stat) [Permutation Test p-value]			1241.75 (3.23) [0.000]		

Panel B: Dollar Weighted Fraction Of Loans Defaulted

Year	FICO≥ 600 (β)	t-stat	Observations	R^2	Mean
2001	0.005	(0.63)	250	0.87	0.052
2002	0.018	(1.74)	250	0.87	0.041
2003	0.013	(1.93)	250	0.94	0.039
2004	0.006	(1.01)	254	0.94	0.040
2005	0.008	(1.82)	254	0.96	0.059
2006	0.010	(0.89)	254	0.86	0.116

Panel C: Permutation Tests For Alternative Default Definitions
(Pooled 2001-2006 with Time Fixed Effects)

Dependent Variable (Default Definition)	FICO≥ 600 (β)	t-stat	Permutation Test p-value	Observations	R^2	Mean
60+ (dollar weighted)	0.010	(1.66)	0.240	1512	0.84	0.058
90+ (dollar weighted)	0.006	(1.00)	0.314	1525	0.75	0.046
Foreclosure+ (dollar weighted)	0.005	(1.25)	0.265	1525	0.77	0.032
60+ (unweighted)	0.011	(1.50)	0.150	1525	0.70	0.056

Panel D: Delinquency Status Of Loans

	Pr(Delinquency)=1			
	(1)	(2)	(3)	(4)
FICO≥600	-.06	-.02	-.06	-.02
	[0.002]	[0.0006]	[0.002]	[0.0006]
	(2.30)	(0.15)	(1.21)	(0.18)
Observations	3,125,818	3,125,818	3,125,818	3,125,818
Pseudo R^2	0.073	0.084	0.073	0.084
Other Controls	Yes	Yes	Yes	Yes
FICO≥600*Other Controls	No	Yes	No	Yes
Time Fixed Effects	No	Yes	No	Yes
Clustering Unit	Loan id	Loan id	Vintage	Vintage
Mean Delinquency (%)		4.54		

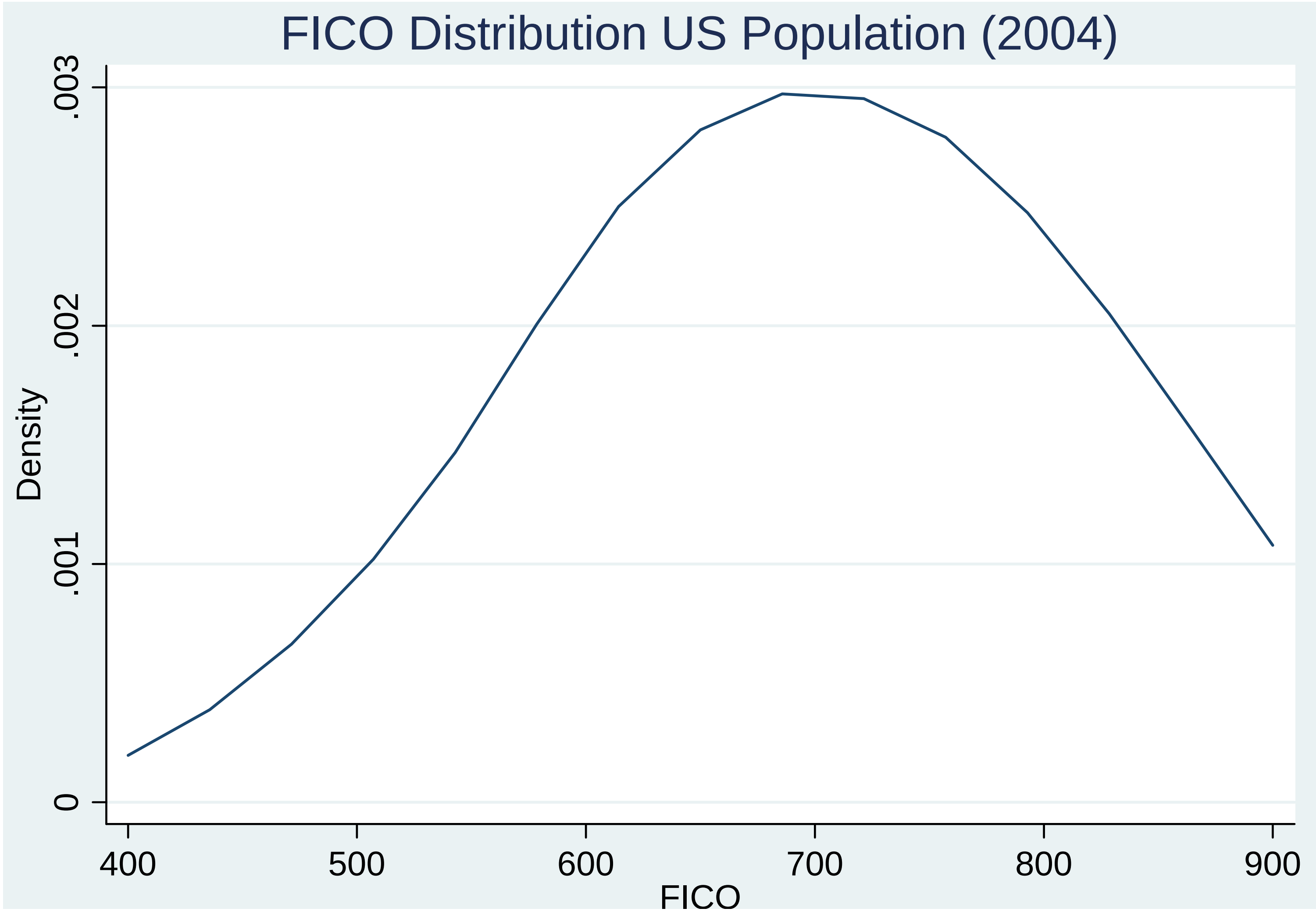

Figure 1: FICO Distribution (US Population)

Figure 1 presents the FICO distribution in the U.S. population for 2004. This data is from an anonymous credit bureau which assures us that the data exhibits similar patterns during the other years of our sample. The FICO distribution across the population is smooth, so the number of prospective borrowers in the local vicinity of a given credit score is similar.

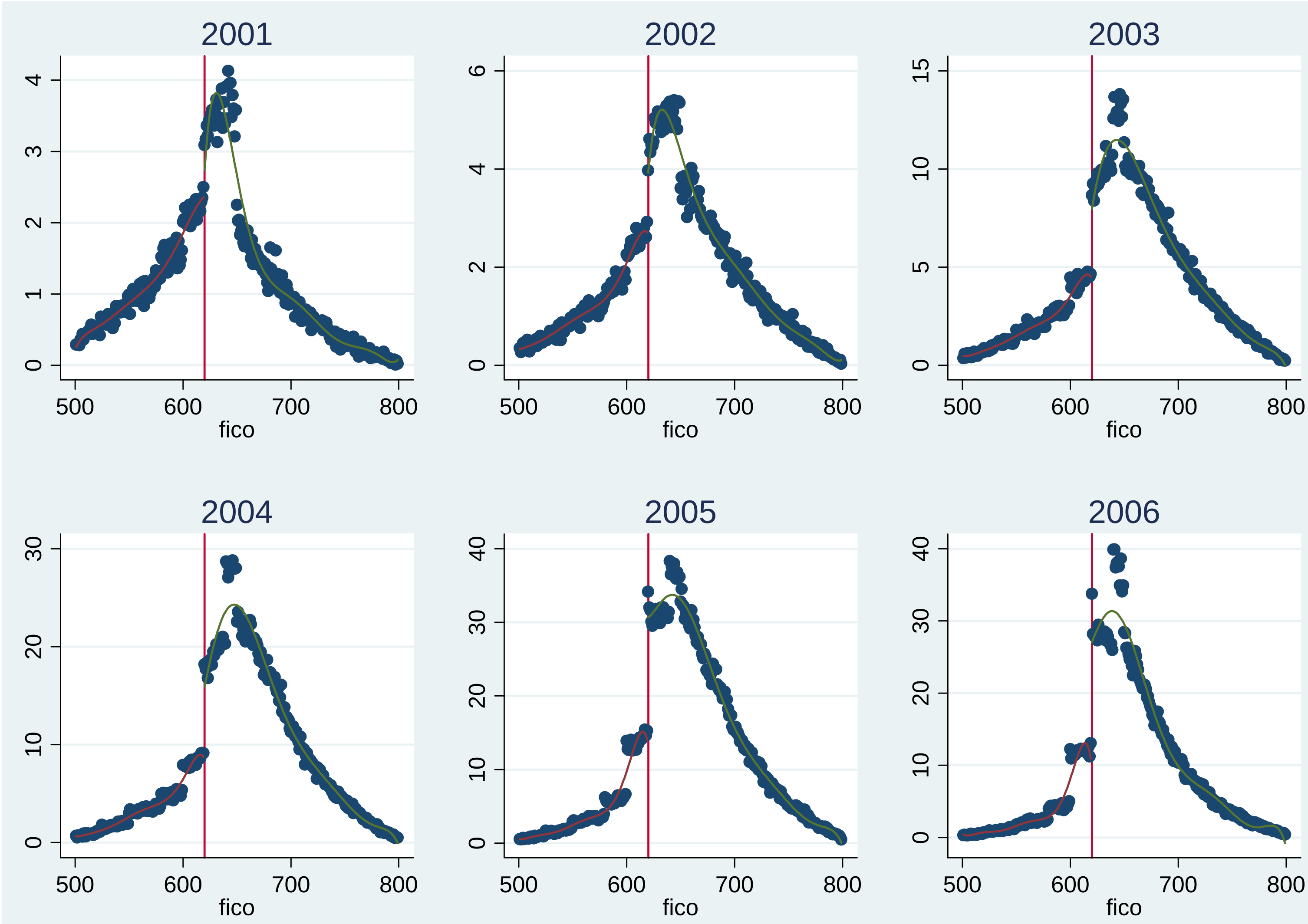

Figure 2: Number of Loans (Low Documentation)

Figure 2 presents the data for number of low documentation loans (in '00s). We plot the average number of loans at each FICO score between 500 and 800. As can be seen from the graphs, there is a large increase in the number of loans around the 620 credit threshold (i.e., more loans at 620^+ as compared to 620^-) from 2001 onwards. Data is for the period 2001 to 2006.

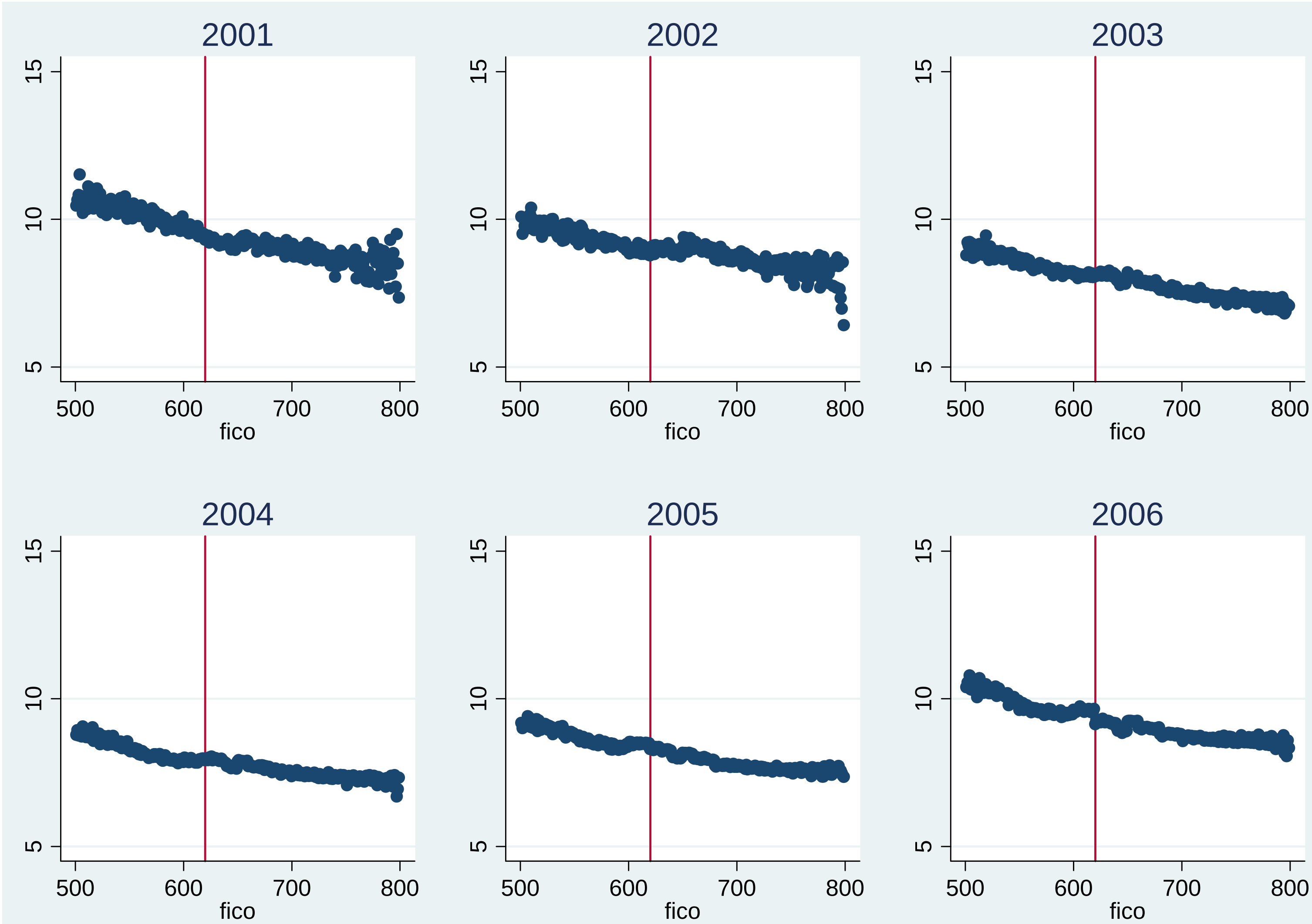

Figure 3: Interest Rates (Low Documentation)

Figure 3 presents the data for interest rate (in %) on low documentation loans. We plot average interest rates on loans at each FICO score between 500 and 800. As can be seen from the graphs, there is no change in interest rates around the 620 credit threshold (i.e., more loans at 620^+ as compared to 620^-) from 2001 onwards. Data is for the period 2001 to 2006.

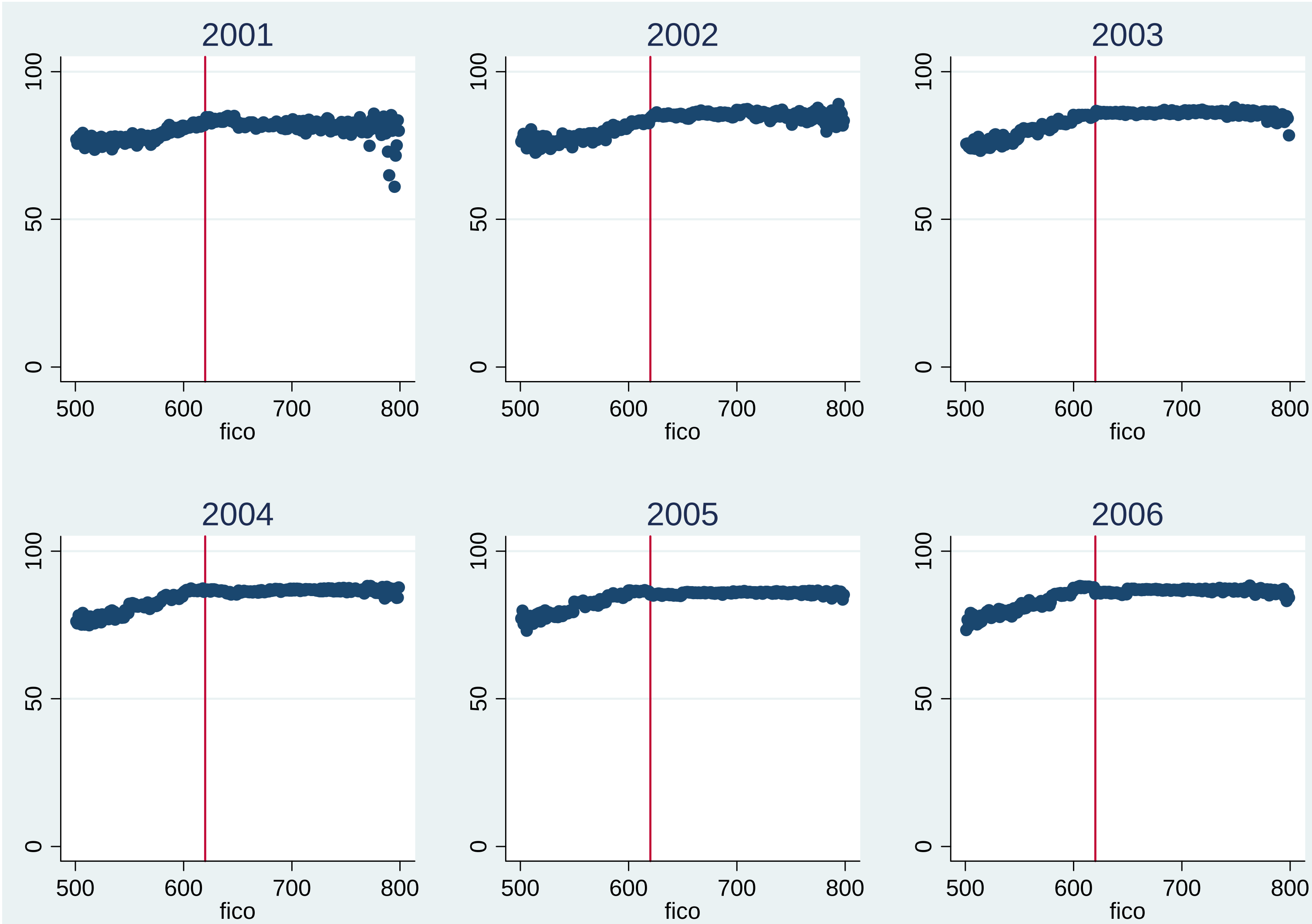

Figure 4: Loan-to-Value (Low Documentation)

Figure 4 presents the data for loan-to-value ratio (in %) on low documentation loans. We plot average loan-to-value ratios on loans at each FICO score between 500 and 800. As can be seen from the graphs, there is no change in loan-to-value around the 620 credit threshold (i.e., more loans at 620^+ as compared to 620^-) from 2001 onwards. Data is for the period 2001 to 2006.

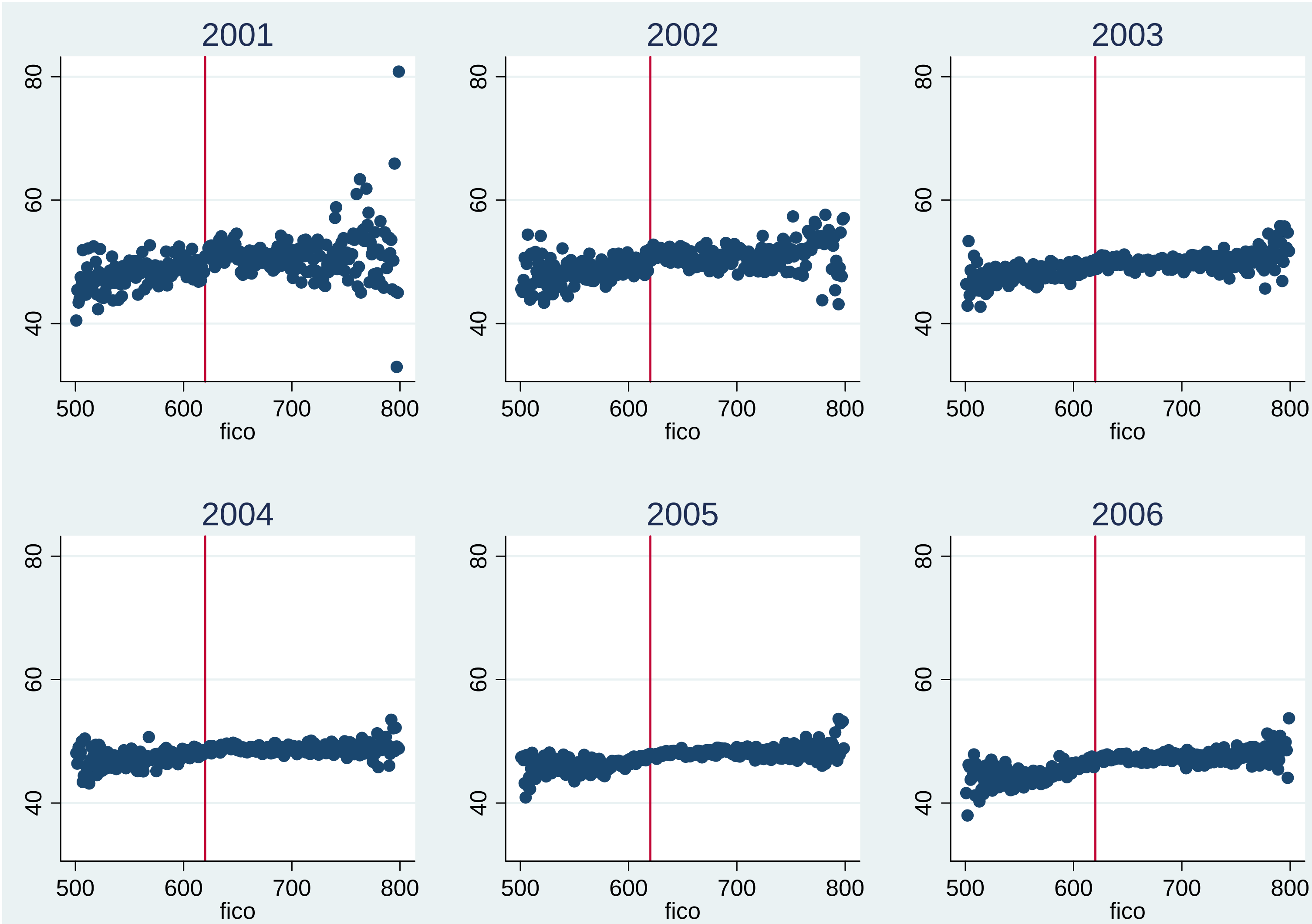

Figure 5: Median Household Income (Low Documentation)

Figure 5 presents median household income (in '000s) of zip codes in which loans are made at each FICO score between 500 and 800. As can be seen from the graphs, there is no change in median household income around the 620 credit threshold (i.e., more loans at 620^+ as compared to 620^-) from 2001 onwards. We plotted similar distributions for average percent minorities taking loans, and average house size and find no differences around the credit thresholds. Data is for the period 2001 to 2006.

Figure 6A: Annual Delinquencies for Low Documentation Loans in 2001

Figure 6A presents the percent of low documentation loans that became delinquent in 2001. We plot the dollar weighted fraction of the pool that becomes delinquent for one-point FICO bins between score of 500 and 750. The vertical line denotes the 620 cutoff, and a seventh order polynomial is fit to the data on either side of the threshold. Delinquencies are reported between 10-15 months for loans originated in the year.

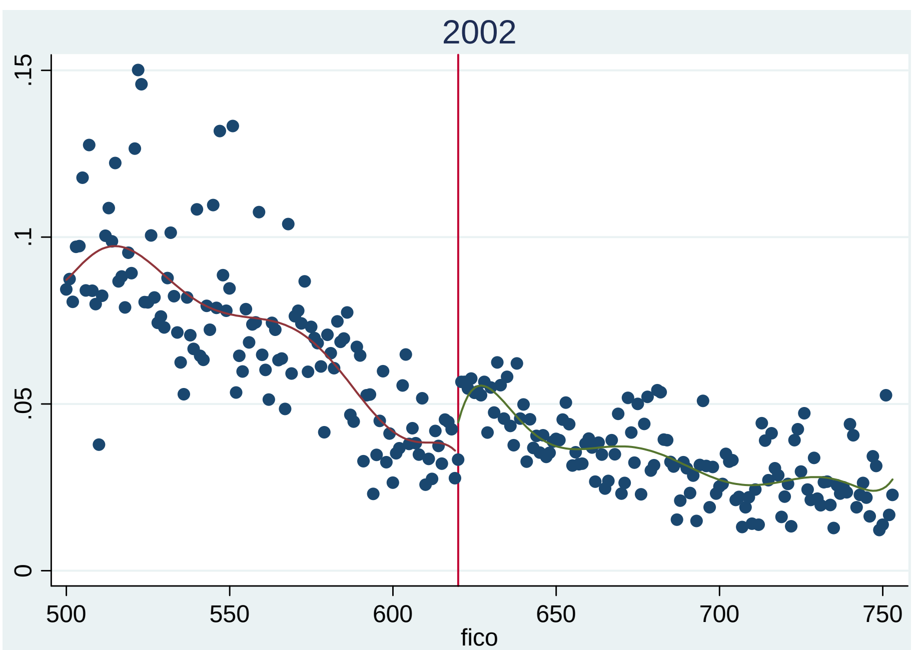

Figure 6B: Annual Delinquencies for Low Documentation Loans in 2002

Figure 6B presents the percent of low documentation loans that became delinquent in 2002. We plot the dollar weighted fraction of the pool that becomes delinquent for one-point FICO bins between score of 500 and 750. The vertical line denotes the 620 cutoff, and a seventh order polynomial is fit to the data on either side of the threshold. Delinquencies are reported between 10-15 months for loans originated in the year.

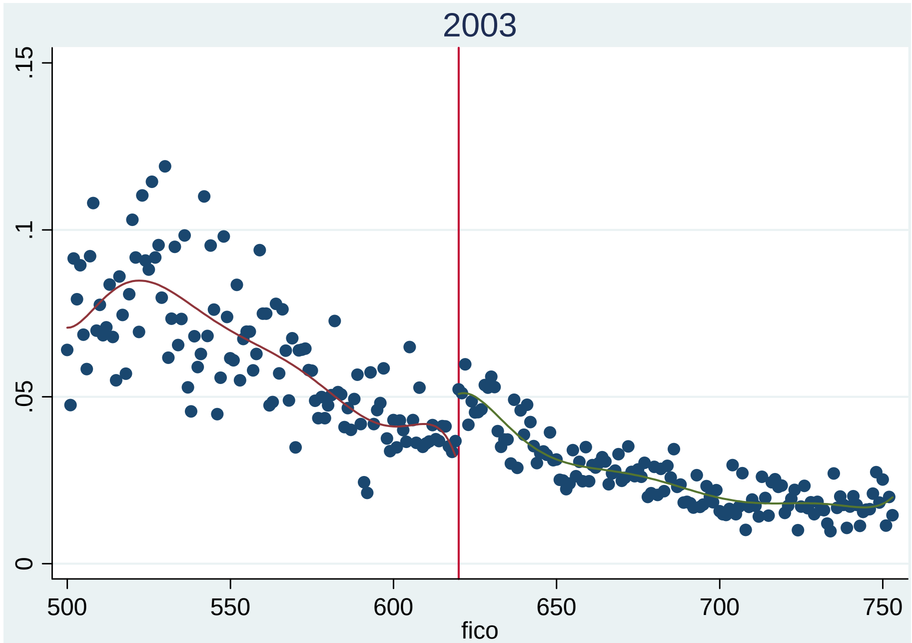

Figure 6C: Annual Delinquencies for Low Documentation Loans in 2003

Figure 6C presents the percent of low documentation loans that became delinquent in 2003. We plot the dollar weighted fraction of the pool that becomes delinquent for one-point FICO bins between score of 500 and 750. The vertical line denotes the 620 cutoff, and a seventh order polynomial is fit to the data on either side of the threshold. Delinquencies are reported between 10-15 months for loans originated in the year.

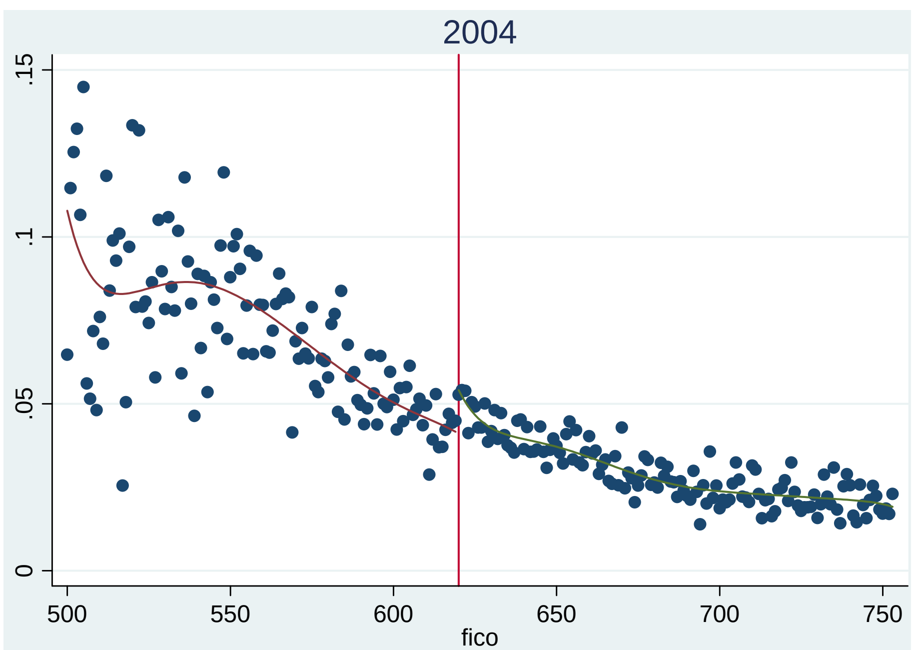

Figure 6D: Annual Delinquencies for Low Documentation Loans in 2004

Figure 6D presents the percent of low documentation loans that became delinquent in 2002. We plot the dollar weighted fraction of the pool that becomes delinquent for one-point FICO bins between score of 500 and 750. The vertical line denotes the 620 cutoff, and a seventh order polynomial is fit to the data on either side of the threshold. Delinquencies are reported between 10-15 months for loans originated in the year.

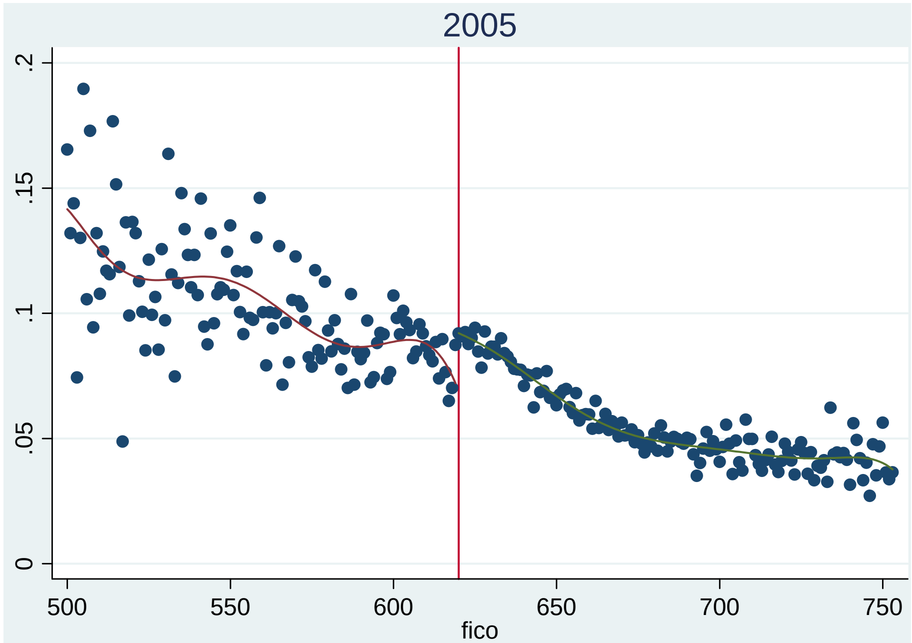

Figure 6E: Annual Delinquencies for Low Documentation Loans in 2005

Figure 6E presents the percent of low documentation loans that became delinquent in 2005. We plot the dollar weighted fraction of the pool that becomes delinquent for one-point FICO bins between score of 500 and 750. The vertical line denotes the 620 cutoff, and a seventh order polynomial is fit to the data on either side of the threshold. Delinquencies are reported between 10-15 months for loans originated in the year.

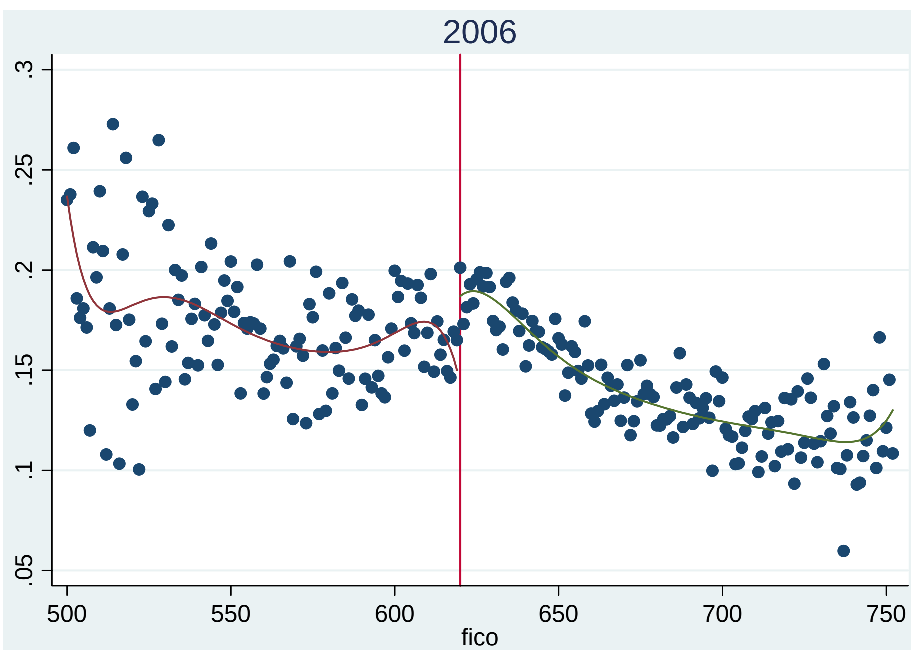

Figure 6F: Annual Delinquencies for Low Documentation Loans in 2006

Figure 6F presents the percent of low documentation loans that became delinquent in 2006. We plot the dollar weighted fraction of the pool that becomes delinquent for one-point FICO bins between score of 500 and 750. The vertical line denotes the 620 cutoff, and a seventh order polynomial is fit to the data on either side of the threshold. Delinquencies are reported between 10-15 months for loans originated in the year.

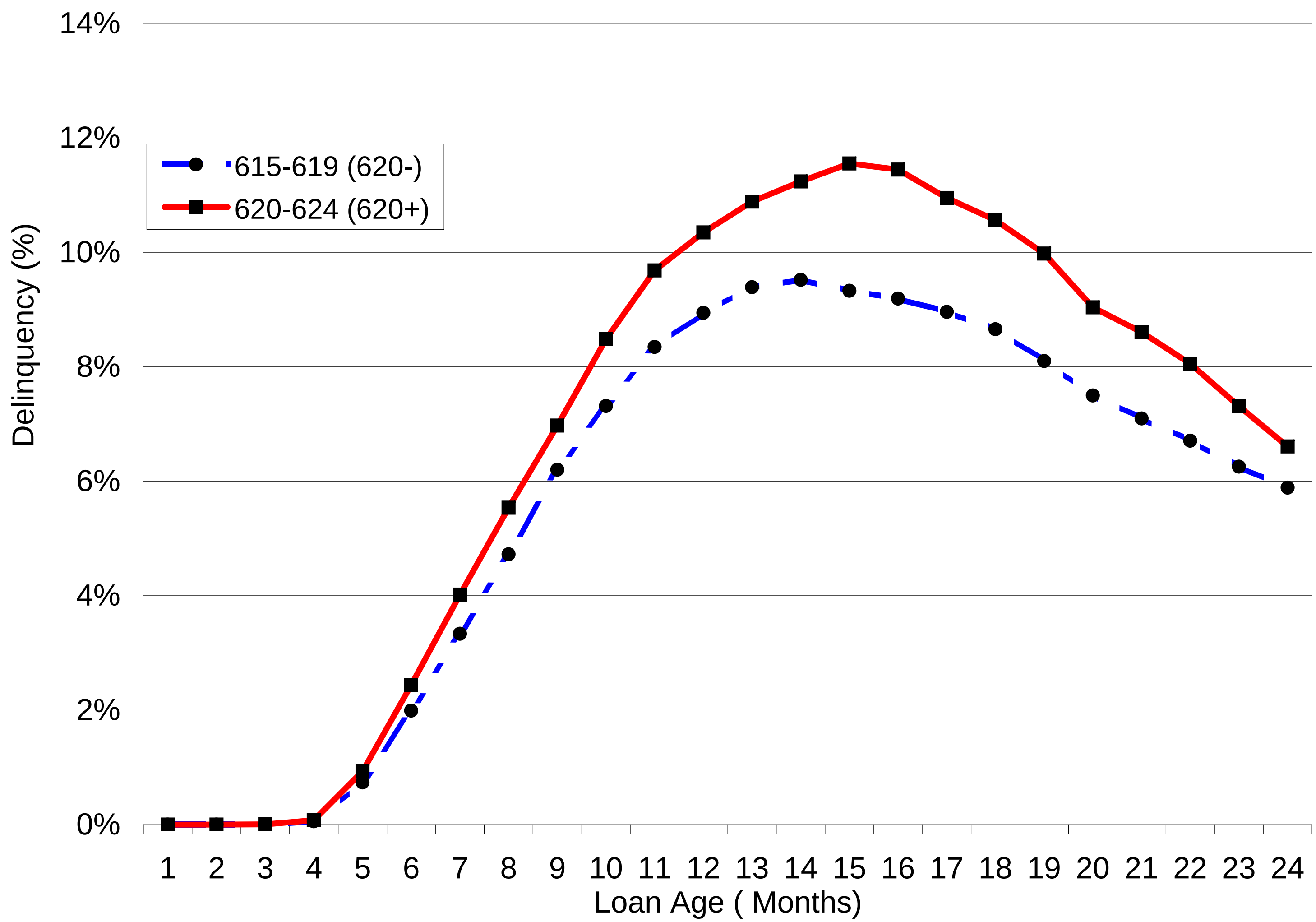

Figure 7: Delinquencies for Low Documentation Loans (2001-2006)

Figure 7 presents the percent of low documentation loans (dollar weighted) that became delinquent for 2001 to 2006. We track loans in two FICO buckets – 615-619 (620^{-}) in dotted blue and 620-624 (620^{+}) in red – from their origination date and plot the average loans that become delinquent each month after the origination date. As can be seen, the higher credit score bucket defaults *more* than the lower credit score bucket for post 2000 period. For brevity, we do not report plots separately for each year. The effects shown here in the pooled 2001-2006 plot are apparent in every year.

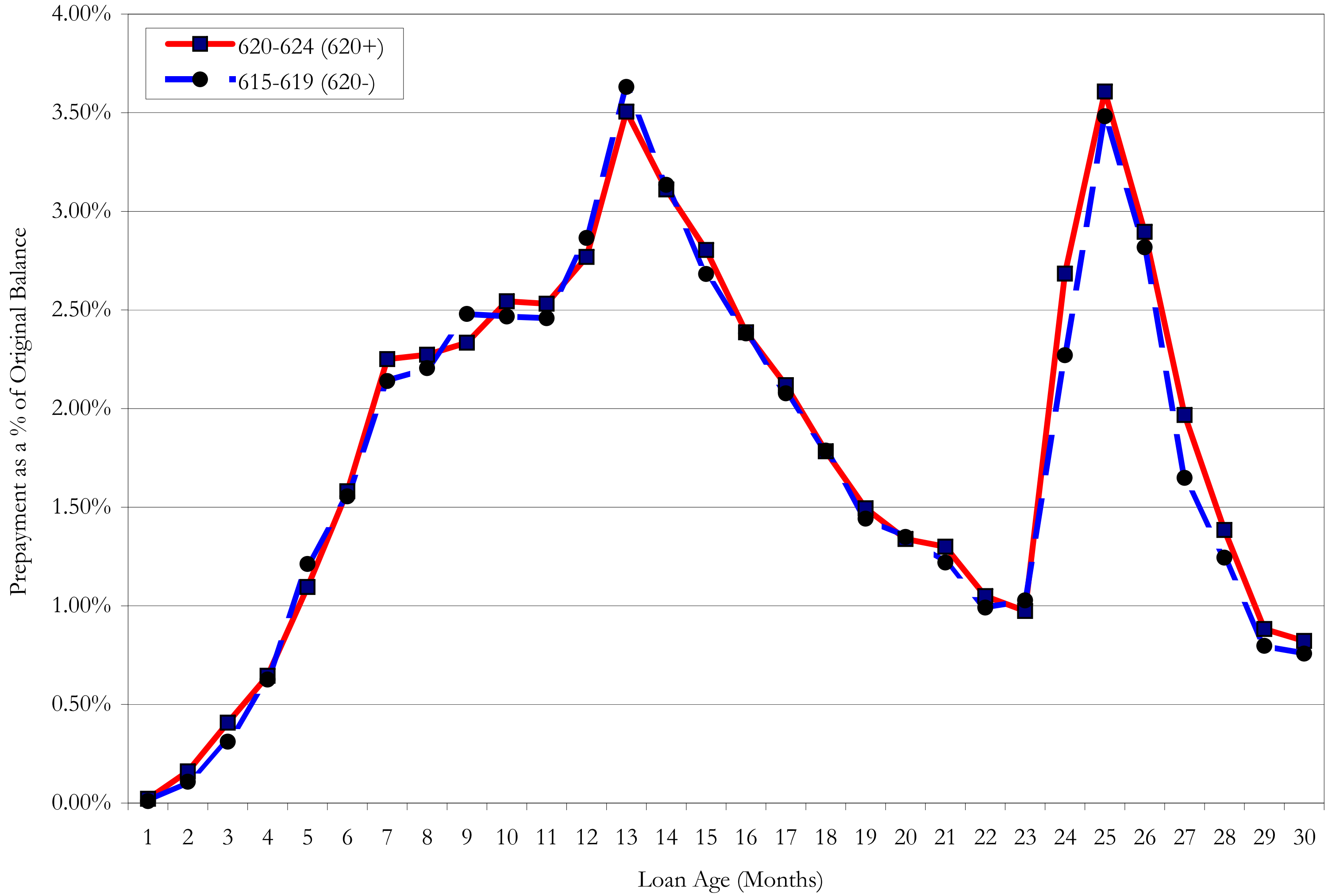

Figure 8: Actual Prepayments for Low Documentation Loans (2001-2006)

Figure 8 presents the percent of low documentation loans (dollar weighted) that were prepaid. We track loans in two FICO buckets – 615-619 (620^{-}) in dotted blue and 620-624 (620^{+}) in red – from their origination date and plot the average loans that prepaid each month after the origination date. As can be seen, there are no differences in prepayments between the higher and lower credit score bucket. For brevity, we do not report plots separately for each year. The effects shown here in the pooled 2001-2006 plot are apparent in every year.

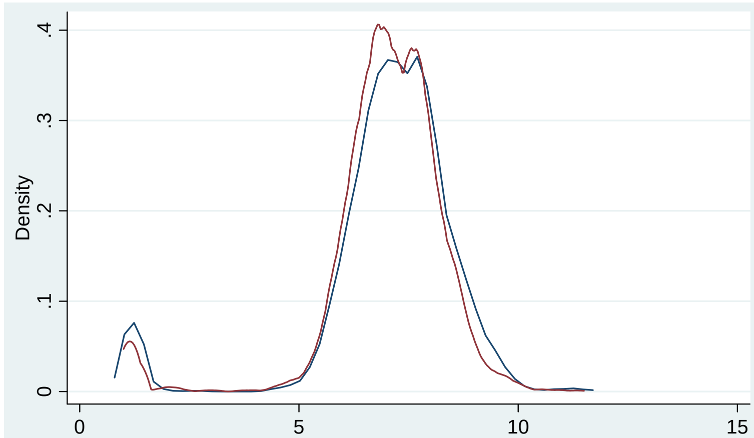

Figure 9A: Dispersion of Interest Rates (Low Documentation)

Figure 9A depicts the Epanechnikov kernel density of interest rate for two FICO groups for low documentation loans – 620^{-} (615-619) in blue and 620^{+} (620-624) in red. The bandwidth for the density estimation is selected using the plug-in formula of Sheather and Jones (1991). The figure shows that the density of interest rates on loans are similar for both the groups. A Kolmogorov-Smirnov test for equality of distribution functions cannot be rejected at the 1% level. Data for 2004 is reported here. We find similar patterns for 2001-2006. We do not report those graphs for brevity.

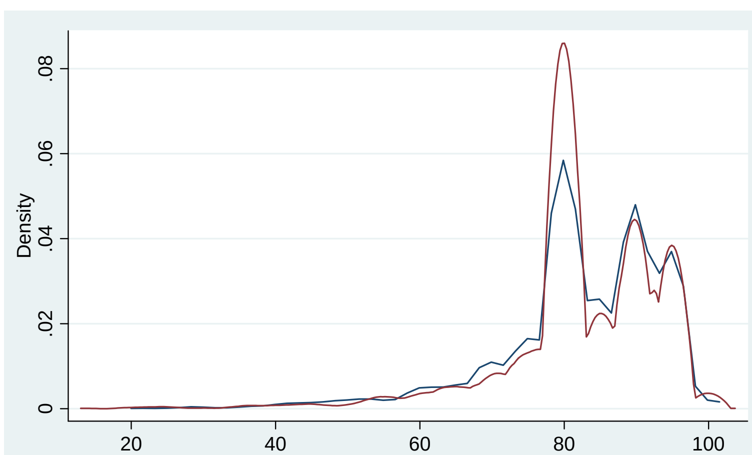

Figure 9B: Dispersion of Loan-to-Value (Low Documentation)

Figure 9B depicts the Epanechnikov kernel density of loan-to-value ratio for two FICO groups for low documentation loans – 620^{-} (615-619) in blue and 620^{+} (620-624) in red. The bandwidth for the density estimation is selected using the plug-in formula of Sheather and Jones (1991). The figure shows that the density of interest rates on loans are similar for both the groups. A Kolmogorov-Smirnov test for equality of distribution functions cannot be rejected at the 1% level. Data for 2004 is reported here. We find similar patterns for 2001-2006. We do not report those graphs for brevity.

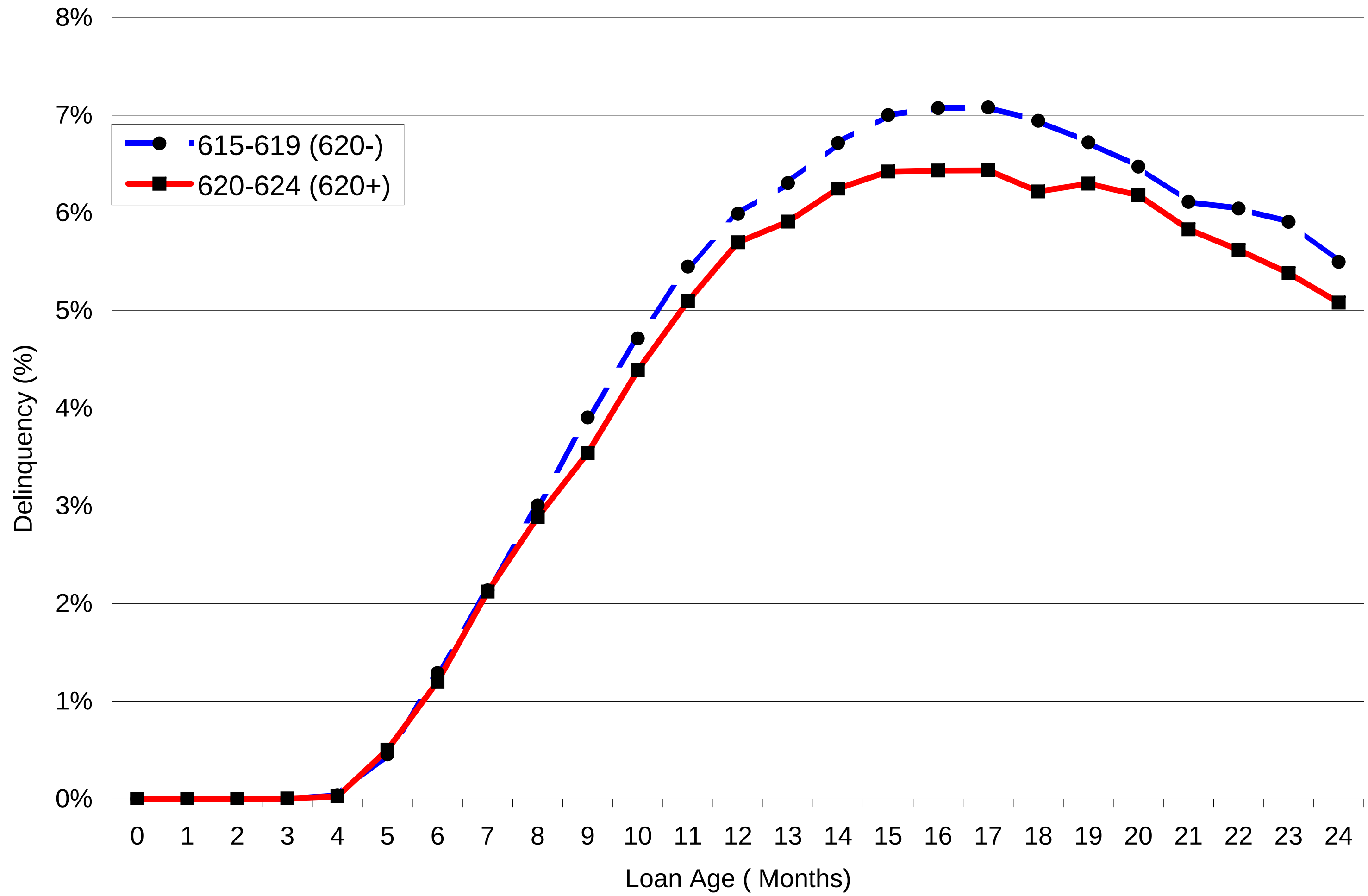

Figure 10: Falsification Test - Delinquencies for Full Documentation Loans Around FICO of 620

Figure 10 presents the falsification test by examining the percent of full documentation loans (dollar weighted) that became delinquent for 2001 to 2006. We track loans in two FICO buckets – 615-619 (620^-) in dotted blue and 620-624 (620^+) in red – from their origination date and plot the average loans that become delinquent each month after the origination date. As can be seen, the higher credit score bucket defaults *less* than the lower credit score bucket for post 2000 period. For brevity, we do not report plots separately for each year. The effects shown here in the pooled 2001-2006 plot show up for every year.

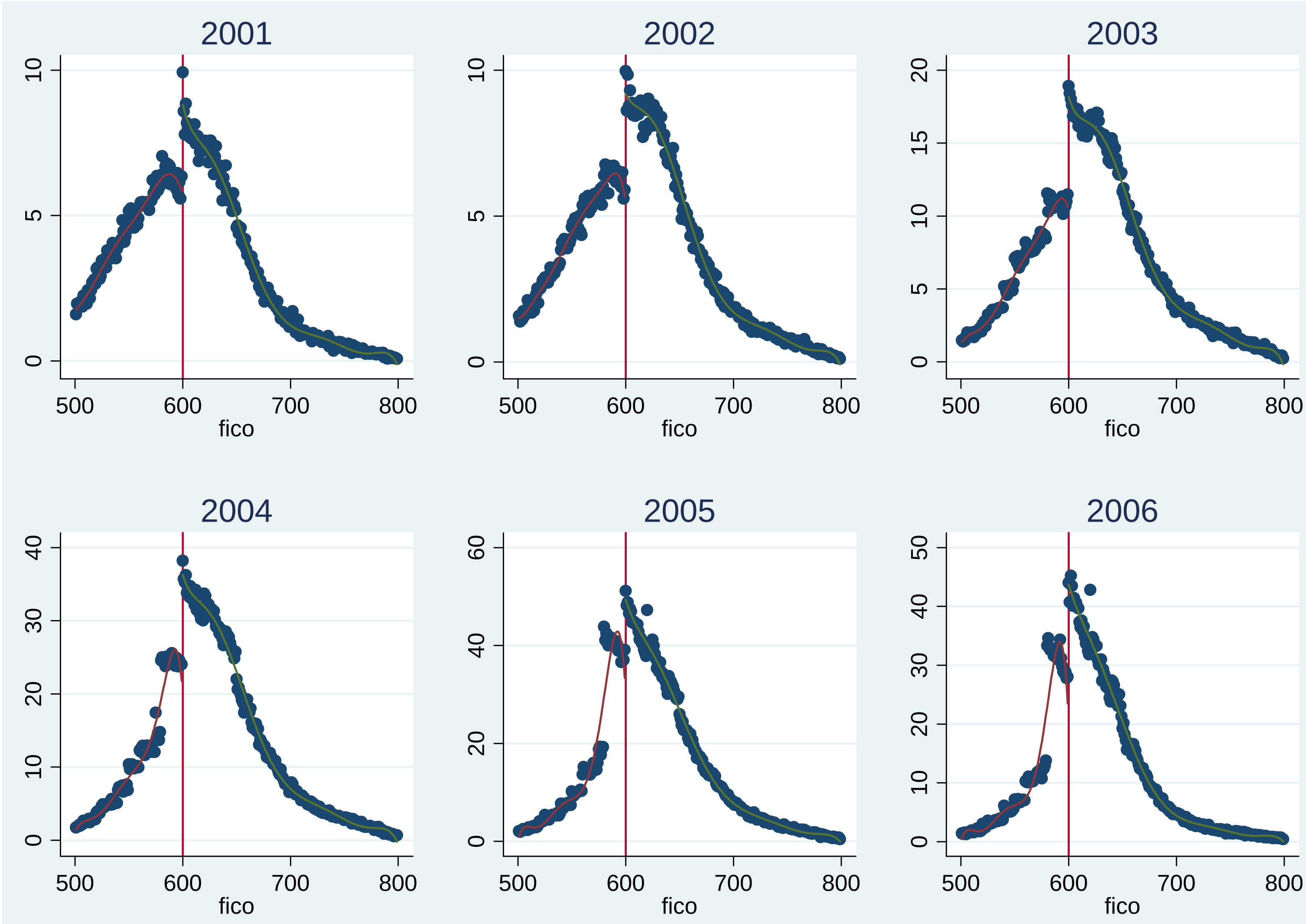

Figure 11: Number of Loans (Full Documentation)

Figure 11 presents the data for the number of full documentation loans (in '00s). We plot the average number of loans at each FICO score between 500 and 800. As can be seen from the graphs, there is a large increase in number of loans around the 600 credit threshold (i.e., more loans at 600^+ as compared to 600^-) from 2001 onwards. Data is for the period 2001 to 2006.

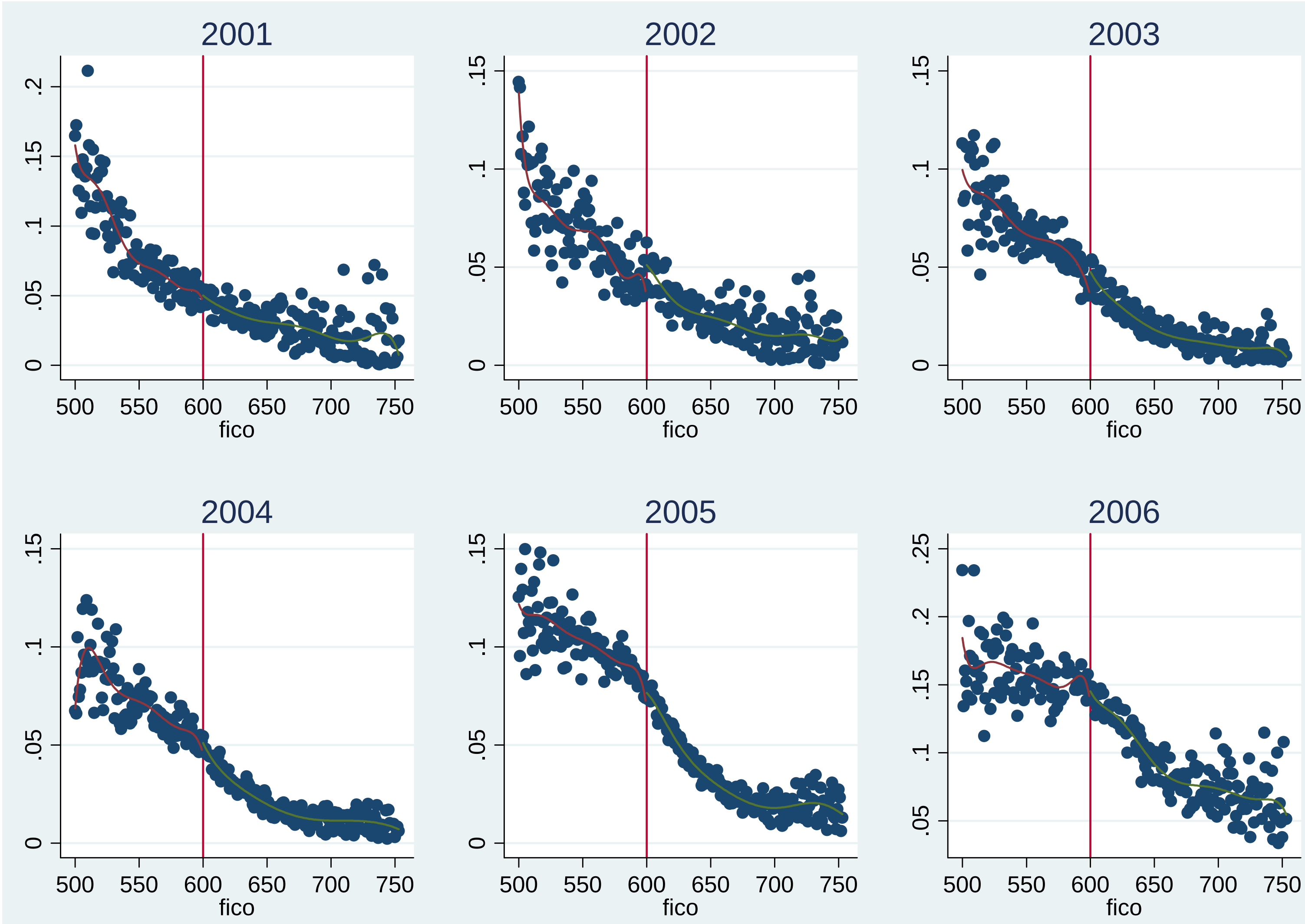

Figure 12: Annual Delinquencies for Full Documentation Loans

Figure 12 presents the percent of full documentation loans that became delinquent for 2001 to 2006. We plot the dollar weighted fraction of the pool that becomes delinquent for one-point FICO bins between score of 500 and 750. The vertical line denotes the 600 cutoff, and a seventh order polynomial is fit to the data on either side of the threshold. Delinquencies are reported between 10-15 months for loans originated in all years.

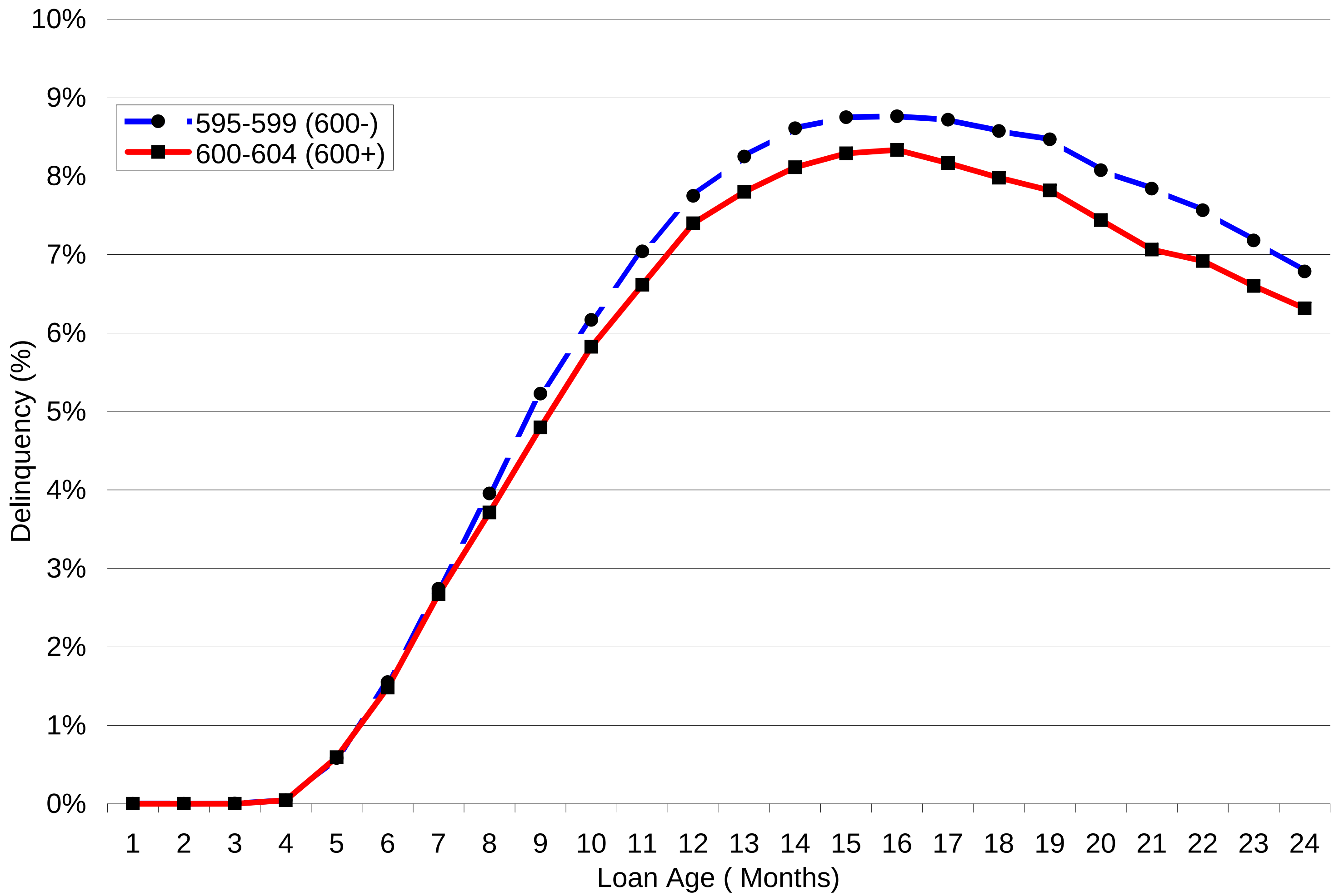

Figure 13: Delinquencies for Full Documentation Loans (2001-2006)

Figure 13 presents the percent of full documentation loans (dollar weighted) that became delinquent for 2001 to 2006. We track loans in two FICO buckets – 595-599 (600^-) in dotted blue and 600-604 (600^+) in red – from their origination date and plot the average loans that become delinquent each month after the origination date. As can be seen, the higher credit score bucket defaults *more* than the lower credit score bucket for post 2000 period. For brevity, we do not report plots separately for each year. The effects shown here in the pooled 2001-2006 plot show up for every year.

Appendix

Table A.I

Loans Characteristics around Discontinuity in Low Documentation Loans

This table reports estimates from a regression which uses the mean interest rate and LTV ratio of low documentation loans at each FICO score as the dependent variable. In order to estimate the discontinuity (FICO$\geq$ 620) for each year, we collapse the interest rate and LTV ratio at each FICO score and estimate flexible seventh-order polynomials on either side of the 620 cutoff, allowing for a discontinuity at 620. Because the measures of the interest rate and LTV are estimated means, we weight each observation by the inverse of the variance of the estimate. We report t-statistics in parentheses. Permutation tests, which allow for a discontinuity at every point in the FICO distribution, confirm that these jumps are not significantly larger than those found elsewhere in the distribution. For brevity, we report permutation test estimates from pooled regressions (with time fixed effects removed to account for vintage effects) and report them in Table A.IV.

Low Documentation Loans

	Loan To Value					Interest Rate				
Year	FICO$\geq$ 620 (β)	t-stat	Obs.	R^2	Mean (%)	FICO$\geq$ 620 (β)	t-stat	Obs.	R^2	Mean (%)
2001	0.67	(0.93)	296	0.76	80.3	0.06	(0.59)	298	0.92	9.4
2002	1.53	(2.37)	299	0.91	82.6	0.15	(1.05)	299	0.89	8.9
2003	2.44	(4.27)	299	0.96	83.4	0.10	(1.50)	299	0.97	7.9
2004	0.30	(0.62)	299	0.96	84.5	0.03	(0.39)	299	0.97	7.8
2005	-0.33	(0.96)	299	0.95	84.1	-0.09	(1.74)	299	0.98	8.2
2006	-1.06	(2.53)	299	0.96	84.8	-0.21	(2.35)	299	0.98	9.2

Appendix

Table A.II

Borrower Demographics around Discontinuity in Low Documentation Loans

This table reports estimates from a regression which uses the mean demographic characteristics of borrowers of low documentation borrowers at each FICO score as the dependent variable. In order to estimate the discontinuity (FICO$\geq$ 620) for each year, we collapse the demographic variables at each FICO score and estimate flexible seventh-order polynomials on either side of the 620 cutoff, allowing for a discontinuity at 620. Because the demographic variables are estimated means, we weight each observation by the inverse of the variance of the estimate. We obtain the demographic variables from Census 2000, matched using the zip code of each loan. Permutation tests, which allow for a discontinuity at every point in the FICO distribution, confirm that these jumps are not significantly larger than those found elsewhere in the distribution. We report t-statistics in parentheses.

Panel A: Percent Black in Zip Code

Year	FICO$\geq$ 620 (β)	t-stat	Observations	R^2	Mean (%)
2001	1.54	(1.16)	297	0.79	11.2
2002	0.32	(0.28)	299	0.63	10.6
2003	1.70	(2.54)	299	0.70	11.1
2004	0.42	(0.53)	299	0.72	12.2
2005	-0.50	(0.75)	299	0.69	13.1
2006	0.25	(0.26)	299	0.59	14.7

Panel B: Median Income in Zip Code

Year	FICO$\geq$ 620 (β)	t-stat	Observations	R^2	Mean (%)
2001	1,963.23	(2.04)	297	0.33	49,873
2002	-197.21	(0.13)	299	0.35	50,109
2003	154.93	(0.23)	299	0.50	49,242
2004	699.90	(1.51)	299	0.46	48,221
2005	662.71	(1.08)	299	0.64	47,390
2006	-303.54	(0.34)	299	0.68	46,396

Panel C: Median House Value in Zip Code

Year	FICO$\geq$ 620 (β)	t-stat	Observations	R^2	Mean (%)
2001	3,943.30	(0.44)	297	0.66	163,151
2002	-599.72	(0.11)	299	0.79	165,049
2003	-1,594.51	(0.36)	299	0.89	160,592
2004	-2,420.01	(1.03)	299	0.91	150,679
2005	-342.04	(0.14)	299	0.93	143,499
2006	-3,446.06	(1.26)	299	0.92	138,556

Appendix

Table A.III

Loan Characteristics and Borrower Demographics around Discontinuity in Full Documentation Loans

This table reports the estimates of the regressions on loan characteristics and borrower demographics around the credit threshold of 600 for full documentation loans. We use specifications similar to Tables A.I and A.II for estimation. We report t-statistics in parentheses. Permutation tests, which allow for a discontinuity at every point in the FICO distribution, confirm that these jumps are not significantly larger than those found elsewhere in the distribution. For brevity, we report permutation test estimates from pooled regressions (with time fixed effects removed to account for vintage effects) and report them in Table A.IV.

Panel A: Loan Characteristics

	Loan To Value					Interest Rate				
Year	FICO$\geq$ 600 (β)	t-stat	Obs.	R^2	Mean (%)	FICO$\geq$ 600 (β)	t-stat	Obs.	R^2	Mean (%)
2001	0.820	(2.09)	299	0.73	85.1	-0.097	(0.87)	299	0.97	9.5
2002	-0.203	(0.65)	299	0.86	85.8	-0.279	(3.96)	299	0.97	8.6
2003	1.012	(3.45)	299	0.95	86.9	-0.189	(3.42)	299	0.99	7.7
2004	0.755	(2.00)	299	0.96	86	-0.244	(6.44)	299	0.99	7.3
2005	0.354	(1.82)	299	0.93	86.2	-0.308	(5.72)	299	0.99	7.7
2006	-0.454	(1.96)	299	0.94	86.7	-0.437	(9.93)	299	0.99	8.6

Panel B: Percent Black in Zip Code

Year	FICO$\geq$ 600 (β)	t-stat	Observations	R^2	Mean (%)
2001	2.32	(2.03)	299	0.86	13.6
2002	-0.79	(1.00)	299	0.82	12.5
2003	0.40	(0.48)	299	0.87	12.5
2004	0.54	(0.96)	299	0.92	12.9
2005	-0.38	(0.85)	299	0.86	13.4
2006	-0.86	(1.40)	299	0.81	14.3

Appendix

Table A.IV

Permutation Test Results For Loan Characteristics in Low and Full Documentation Loans

This table reports the estimates of the regressions on loan characteristics around the credit threshold of 620 for low documentation loans and credit threshold of 600 for full documentation loans. We pool the loans across all years and remove year effects to account for vintage effects. We use specifications similar to Table A.I for estimation. Permutation tests, which allow for a discontinuity at every point in the FICO distribution, confirm that these jumps are not significantly larger than those found elsewhere in the distribution. We report p-values from these tests in the table.

Panel A: Low Documentation Loan Characteristics

	Interest Rate	Loan To Value Ratio	Debt To Income Ratio	Prepayment Penalty	Actual Prepayments	CLTV Ratio
Pooled FICO$\geq$ 620 (β)	0.02	0.54	0.42	-0.016	-0.0004	0.05
t-stat	0.32	1.40	1.25	1.23	0.44	0.73
Permutation Test p-value	0.90	0.46	0.32	0.55	0.84	0.96

Panel B: Full Documentation Loan Characteristics

	Interest Rate	Loan To Value Ratio	Debt To Income Ratio	Prepayment Penalty	Actual Prepayments	CLTV Ratio
Pooled FICO$\geq$ 620 (β)	0.39	-0.26	0.68	0.008	-0.0009	0.17
t-stat	1.63	1.91	1.83	0.73	1.51	0.39
Permutation Test p-value	0.20	0.07	0.11	0.45	0.35	0.62