SECREITIZATION AND MORAL HAZARD: EVIDENCE FROM A LENDER CUTOFF RULE

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ABSTRACT. Recent research has investigated the moral hazard problem posed by securitization by supposing that securitizers are exogenously more willing to purchase loans to borrowers with credit scores above some cutoff and concluding that the jump in defaults at a credit score cutoff demonstrates that securitization reduced lenders' incentives to screen. However, the conclusions of such research depend crucially on the assumption that lenders' use of a credit score cutoff rule is entirely driven by the use of such a cutoff rule by securitizers. We offer an alternative theory in which cutoff rules are a rational response of lenders to per-applicant fixed costs in screening. We then demonstrate that this theory fits the evidence better than the exogenous securitizer cutoff rule theory. Discontinuous jumps in the mortgage default rate at a credit score cutoff are apparent, implying a change in lender screening behavior at the threshold, but in our main sample there is no corresponding jump in the securitization rate at this score. We conclude that the credit score cutoff rule provides no evidence that mortgage securitization led to moral hazard in screening.

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1. INTRODUCTION

It has now become conventional wisdom that securitization contributed to the sharp rise in mortgage defaults that precipitated the recent financial crisis. The logic of the moral hazard problem posed by securitization is straightforward: lenders that sell loans they originate to dispersed investors may have less incentive to screen borrowers. However, this potential problem was well-understood by market participants, who used a range of practices to mitigate the problem. Hence, the extent to which securitization actually reduced lenders’ screening is an empirical question.

One promising strategy for answering this research question is to use variation in the behavior of lenders induced by credit score cutoff rules. Credit scores are used by lenders as a summary measure of default risk, with higher credit scores indicating lower default risk. Examination of histograms of mortgage borrower credit scores, such as Figure I, reveals that they are step-wise functions. It appears that borrowers with credit scores just above certain thresholds are treated differently than borrowers just below, even though potential borrowers on either side of the threshold are very similar. These histograms suggest using a regression discontinuity design to learn about the effects of the change in behavior of market participants at these thresholds. But how and why does lender behavior change at these thresholds?

In this paper we distinguish between two explanations for credit score cutoff rules, each with divergent implications for what these cutoff rules can tell us about the relationship between securitization and lender screening. We refer to the explanation currently most accepted in the literature as the exogenous securitizer cutoff rule theory. First offered by Keys, Mukherjee, Seru, and Vig (2010) (hereafter, KMSV), it posits that secondary-market mortgage purchasers employ rules of thumb whereby they are exogenously more willing to purchase loans made to borrowers with credit scores just above some cutoff. KMSV exploit the resulting discontinuity in ease of securitization in a regression discontinuity design to investigate the effect of securitization on lenders’ incentives to screen. Examining a dataset of only securitized loans, they interpret discontinuities in loan performance at the securitizers’ purchasing cutoff as estimates of the causal effect of securitization on

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1A report from the Department of the Treasury summarizes the view succinctly: “Securitizers failed to set high standards for the loans they were willing to buy, encouraging underwriting standards to decline” (Department of the Treasury, 2009, p. 6). The report recommends that “Federal banking agencies ... promulgate regulations that require originators or sponsors to retain an economic interest in a material portion of the credit risk of securitized credit exposures” (Department of the Treasury, 2009, p. 44), a recommendation that was ultimately adopted in the 2010 Dodd-Frank Act.
lender screening behavior. Crucial to the validity of this research design is KMSV’s assumption that lenders’ discontinuous change in screening at the credit score cutoff is entirely driven by the use of a cutoff rule by securitizers in their purchasing decisions.

We offer an alternative rational theory for credit score cutoff rules, which we refer to as the lender-driven cutoff rule theory. When lenders face a fixed per-applicant cost to acquire additional information about each prospective borrower, cutoff rules in screening arise endogenously. Under the natural assumption that the benefit to lenders of collecting additional information is greater for higher default risk applicants, lenders will only collect additional information about applicants whose credit scores are below some cutoff (and hence the benefit of investigating outweighs the fixed cost). This additional information allows lenders to screen out more high-risk loan applicants. This theory—or any other theory in which lenders use cutoff rules for reasons other than a discontinuity in ease of securitization—implies that the change in loan performance at a credit score cutoff is not an unbiased estimate of the causal effect of securitization on lender screening.

We find that institutional evidence and quantitative evidence from a loan-level dataset are inconsistent with the exogenous securitizer cutoff rule theory but consistent with the lender-driven cutoff rule theory. The institutional evidence shows that lenders were directed by the government sponsored enterprises (GSEs) in the 1990s to adopt a credit score cutoff rule in their underwriting practices, and such cutoffs spread widely through originators’ use of underwriting software that incorporates those cutoffs. The discontinuous changes in both the guidance of the GSEs and the output of originator underwriting software confound any change in ease of securitization at the cutoffs, invalidating a regression discontinuity design based on the cutoffs.

A loan-level dataset that includes both securitized and portfolio loans reveals jumps in both mortgage volume and default rate at the FICO score of 620, confirming the presence of a lender screening cutoff at that score. But contrary to the predictions of the exogenous securitizer cutoff rule theory, we show that lenders with low rates of loan securitization use this cutoff rule at least as much as those with high rates. We also show that for several key subsamples, including the subsample used by KMSV, there is a lender screening cutoff at 620 but no discontinuity in the rate of securitization at the cutoff. The existence of lender cutoffs in the absence of securitization corroborates the institutional evidence that the jump in default rates at 620 first documented by KMSV is not exclusively caused by securitization. This rejection of the exogenous securitizer

2The credit scoring model developed by Fair Isaac and Company (FICO) is the industry standard.
cutoff rule theory implies that the credit score cutoff rule provides no evidence that mortgage securitization led to moral hazard in screening.

We also examine the implications of the lender-driven cutoff rule theory for securitization. The discontinuous change in information gathering at the screening threshold results in a discontinuity in the amount of private information lenders have about loans. As we know from a large literature in information economics, private information can inhibit trade (Akerlof [1970]), and trade in financial claims like mortgages is no exception. Consequently, the discontinuity in the amount of private information at the cutoff can result in a discontinuity in the volume of trade—the securitization rate—at the cutoff. However, if the securitizer is able to contract on lender screening behavior—either directly or via a repeated game in which screening behavior is ultimately revealed and lax screening punished—the discontinuity in lender screening at the credit score cutoff need not result in a discontinuity in securitization.

We look to the data and find it is consistent with these predictions. For markets dominated by the Federal National Mortgage Association (Fannie Mae) and the Federal Home Loan Mortgage Corporation (Freddie Mac), both of which are large securitizers that can credibly punish lenders by refusing to do business with them, securitization rates are continuous at the lender cutoff. For markets in which Fannie and Freddie do not operate, and which are populated by smaller “private-label” securitizers that lack a threat of serious long-run punishment, securitization rates are indeed lower below the lender cutoff than above.

KMSV investigate our thesis that credit score cutoff rules are used by lenders for reasons unrelated to the probability of securitization and reject it based on evidence from the passage of anti-predatory lending laws. We re-examine this evidence and conclude that it is consistent with our thesis.

Our paper contributes to a growing literature analyzing the causes of the subprime mortgage crisis. Mayer, Pence, and Sherlund (2009) document many of the basic facts of the subprime crisis and conclude that a combination of a decline in underwriting standards and a fall in house prices led to the sharp increase in defaults from 2005 to 2008. Further evidence on the central role of the fall in housing prices in the mortgage crisis is provided by Gerardi, Shapiro, and Willen (2007). Demyanyk and Van Hemert (2009) provide evidence that the increased future default rates of high LTV loans were to some extent priced into the mortgage rate well before the onset of the
crisis, suggesting that securitizers who influence those rates were aware of the coming increase in defaults. The connection between securitization and the increase in defaults is investigated by Jiang, Nelson, and Vytlačil (2009), Mian and Sufi (2009), and Rajan, Seru, and Vig (2008). Downing, Jaffee, and Wallace (2009) explore whether the market for mortgage backed securities is a lemons market. Adelino, Gerardi, and Willen (2009) and Piskorski, Seru, and Vig (2008) investigate whether securitization inhibited modifications of loans for distressed borrowers.

Our work also relates to the literature on loan sales more generally. Gorton and Pennacchi (1995), Pennacchi (1988), and Sufi (2007) consider institutional mechanisms to mitigate the moral hazard problem in screening and monitoring posed by loan sales, including the use of portfolio loans as an incentive instrument. Drucker and Puri (2008) document the use of loan covenants to address agency problems in loan sales.

The paper proceeds as follows. Section 2 presents the competing theories of cutoff rules and their implications. Section 3 presents institutional evidence on the origin of lenders’ credit score cutoff rules. Section 4 tests predictions of the competing cutoff rule theories with evidence from a loan-level dataset. Section 5 examines evidence from the passage of anti-predatory lending laws. Section 6 concludes.

2. Theory

Why might lenders adopt credit score cutoff rules? We posit that discrete costs to lenders of information gathering about loan applicants result in the observed cutoff rules in screening. We refer to this theory as the lender-driven cutoff rule theory. To formalize the theory, we analyze a baseline model of a portfolio lender (that is, a lender that retains the loans it originates) and then consider the case in which a securitizer purchases loans from the originator. After presenting the competing exogenous securitizer cutoff rule theory, we then discuss the implications of the theories for the use of cutoff rules in a regression discontinuity design to investigate the effect of securitization on lender screening.

2.1. A rational model of lender cutoff rules.

2.1.1. Baseline model. There is a continuum of prospective borrowers of unit mass. Each borrower has a type $x$ that represents hard information about the borrower that is relevant to predicting the performance of a loan to the borrower. Given our purposes here, think of $x$ as the borrower’s
credit score. To economize on notation, let \( x \in [0, 1] \) represent both the type of hard information about the borrower and his probability of repayment on a mortgage. Borrowers’ types are independently and identically distributed according to the strictly positive, continuous probability density function \( f(x) \). Borrowers would like to take out a mortgage for 1 unit of the numeraire good at time 0 to be repaid with interest at time 1, but they have an outside option such that they will refuse a loan offer with a gross interest rate above \( \bar{R} > 1 \). There is a single risk-neutral lender with discount factor normalized to 1. At time 0 each borrower applies to the lender for a mortgage. The lender observes each applicant’s \( x \).

The lender then chooses whether to further investigate each borrower’s creditworthiness. To do so, the lender must bear a fixed cost \( c > 0 \) per applicant. The fixed cost arises from discreteness in the information production function available to the firm managers who set underwriting policy. For example, requiring loan officers to meet with loan applicants in person, or to perform manual underwriting in addition to the commonly used computer-aided automated underwriting process, entails a fixed cost per applicant. Moreover, it would be difficult for managers to specify continuous investigation intensities for continuous distributions of borrowers, given difficulty in monitoring their agents’ screening behavior (Ellison and Holden, 2008). Consequently, firm managers face a discrete choice set of investigation intensities.

If the lender investigates and the borrower is a defaulter, the lender learns this with probability \( s \in (0, 1) \), and otherwise the lender observes nothing. The lender’s investigation thus reveals this “defaulter signal” about a borrower of type \( x \) with probability \((1 - x)s\). We assume that \( c < \frac{(\bar{R} - 1)s}{\bar{R}} \) so that investigation is cheap enough that it will pay for the lender to investigate some applicants.

The lender then chooses whether to lend to each applicant and, if so, makes a take-it-or-leave-it interest rate offer \( R(x) \). Those offered loans then decide whether to accept the offer. In period 1, borrowers learn whether they are defaulters, and the non-defaulters pay the lender \( R(x) \).

Obviously the lender never chooses to lend to applicants for which its investigation revealed the defaulter signal. Furthermore, because we have given the lender all of the bargaining power, it should be obvious that, if the lender lends, it is a dominant strategy to offer \( \bar{R} \), and for all borrowers

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3 Though for simplicity we model a binary investigation choice, the model could be extended to accommodate multiple levels of discrete investigation intensity, each with its own cost: \( c_1 < c_2 < c_3 \ldots \) and so on. Each discrete level of investigation would induce a separate threshold, a prediction consistent with the observation of multiple thresholds in the data (see Figure 1). However, a binary choice captures the essence of the theory.
offered a loan to accept\footnote{It is possible to complicate the model by making $R$ a decreasing function of $x$, but it does not yield new insights. Under reasonable assumptions the single crossing property still holds and lenders still employ a cutoff rule.}. Hence, the equilibria of the game are characterized by an investigation strategy (which borrower types the lender investigates) and a lending strategy (to which types the lender offers loans). We now have our main result:

**Proposition 1.** In the unique equilibrium, the lender uses cutoff rules based on a lending threshold $\bar{x} = \frac{1 - \frac{s + c}{R - z}}{R - z}$ and a screening threshold $\bar{x} = 1 - \frac{c}{s} > \bar{x}$:

1. The lender rejects borrowers with $x < \bar{x}$
2. The lender investigates borrowers with $\bar{x} \leq x < \bar{x}$ and offers loans to those for which its investigation does not reveal the defaulter signal.
3. The lender offers loans to borrowers with $x \geq \bar{x}$ without investigation.

All proofs are in the appendix.

With the equilibrium characterized, its implications for equilibrium loans are immediate. This screening behavior by lenders results in a discontinuous jump in the density of loans, denoted $h(x)$, at the $\bar{x}$ screening threshold proportional to $(1 - \bar{x})s$:

**Corollary 1.** The density of loans made in equilibrium, $h(x)$, is proportional to the following function:

$$h(x) \propto \begin{cases} 0 & \text{if } x < \bar{x} \\ (1 - (1 - x)s)f(x) & \text{if } \bar{x} \leq x < \bar{x} \\ f(x) & \text{if } x \geq \bar{x} \end{cases}$$

Figure 2 depicts the discontinuities in $h(x)$ at $\bar{x}$ and $\bar{x}$. The density of loans jumps at $\bar{x}$ because the lender only screens out the sure defaulters just below $\bar{x}$.

We have a similar result for equilibrium default rates:

**Corollary 2.** The default rate of equilibrium loans with hard information $x$ is given by the following function, $d(x)$:

$$d(x) = \begin{cases} \frac{(1-x)(1-s)}{1-(1-x)s} & \text{if } \bar{x} \leq x < \bar{x} \\ 1 - x & \text{if } x \geq \bar{x} \end{cases}$$

Figure 3 depicts $d(x)$. The default rate jumps discontinuously up when crossing the screening threshold $\bar{x}$ from below (one can easily show that $\frac{(1-x)(1-s)}{1-(1-x)s} < 1 - x$). The reason it jumps at $\bar{x}$
is because the lender only investigates applicants below \( \bar{x} \), which results in a lower default rate. Elsewhere, the equilibrium default rate is decreasing in \( x \).

Our simple model demonstrates how cutoff rules in screening emerge endogenously when there are fixed costs to generating information and the benefit to the lender of additional information varies smoothly with the lender’s initial estimate of the borrower’s default probability. Like the hard information \((x)\) in the model, there is a monotonic relationship between FICO score and default risk. Not surprisingly, lenders use a FICO score cutoff to determine which loan applications warrant increased scrutiny. Mapped into our model, a FICO score such as 620 corresponds to the screening threshold \( \bar{x} \). The intuition for how these discrete costs result in discontinuities in default rates is straightforward: if lenders gave stricter scrutiny to loan applicants just above the FICO threshold it would reduce the default rate, but this reduction would not justify bearing the fixed cost \((c)\) per applicant to collect the information. In contrast, for loan applicants just below the FICO threshold the benefit of additional information outweighs the fixed cost.

While the above analysis considered a single lender with a single \( c \), multiple lenders with modest differences in screening technologies may coordinate on a single cutoff, resulting in a similar aggregate discontinuity in screening. Uncertainty may be one reason for coordination. If a lender is uncertain about its optimal cutoff rule, and it is costly to learn about it, it may be rational for that lender to follow the group cutoff rule as a first approximation to its own.\(^5\) As will be discussed in more detail in Section 3, the widespread use of automated underwriting systems (AUSs) such as Loan Prospector and Desktop Underwriter acts as an important coordination mechanism among lenders. These underwriting software systems are discontinuously more likely to produce a “refer” outcome for loans to potential borrowers with FICO scores below particular cutoffs. Referred loans are often subsequently “manually underwritten”—a costly process similar to the investigation decision in our formal model. Many lenders use the same AUSs and as a consequence employ the same screening thresholds.

\(^5\)Coordination can also be an equilibrium in the absence of uncertainty about the optimal rule. Suppose, for instance, that each lender has its own idiosyncratic \( c^i \). If a mass of lenders has already coordinated on a particular cutoff, it will not be advantageous for an individual lender to deviate to a lower cutoff, even if that lender in isolation would have chosen the lower cutoff. Intensive screening below the group cutoff lowers the average quality of applicants who have not been given loans, because those rejected are more likely to be defaulters. This induced discontinuity in applicant quality makes small deviations from the group cutoff unappealing to lenders. Large deviations may still be advantageous, however. Lenders with \( c^i \) sufficiently distant from the \( c \) corresponding to the group cutoff may coordinate on their own cutoff. This is another possible explanation for the pattern of multiple well-spaced cutoff rules seen in Figure [1].
2.1.2. Securitization. Consider now the implications of lender cutoff rules for securitization. The lender’s greater investigation of applicants below the screening threshold results in the lender having greater private information about those loans than the loans above the cutoff. This increase in information asymmetry can inhibit trade in mortgages. If a loan purchaser, which we will refer to somewhat imprecisely as a securitizer, bargains with the lender over the purchase of loans after they are originated, it will face an adverse selection problem. The lender would like to originate and sell to the securitizer loans that it knows are lower credit quality, keeping for itself the high credit quality loans. As in Akerlof (1970), asymmetry of information on borrower quality can inhibit trade. And because the amount of private information changes discontinuously at the screening threshold, this adverse selection problem can result in the volume of trade—that is, the securitization rate—to change discontinuously at the same threshold.

If instead the lender and securitizer can bargain over a loan purchase contract prior to the lender making investigation and lending decisions, a moral hazard problem arises. If the contract specifies that a loan to a particular potential borrower will be sold, then the lender will have no incentive to investigate the borrower and make efficient lending decisions. A key incentive instrument that the securitizer can use to mitigate this problem is portfolio loans. The contract can specify that only a fraction of loans of each type $x$ will be sold, with the particular loans chosen at random and the remaining loans remaining on the books of the lender to maintain its incentives to screen.

We analyze the moral hazard case formally in an extension of the model provided in the Appendix. Our analysis yields two key results. First, somewhat obviously, if the securitizer and the lender can contract on the lender’s screening behavior, they will implement the first-best screening behavior characterized in Proposition 1 and the securitizer will buy all of the lender’s loans.

Second, if the securitizer and lender cannot contract on the lender’s screening behavior, the fraction of loans securitized will discontinuously jump at the screening threshold $\bar{x}$. When it is efficient for the lender to extend a loan without investigation (that is, $x \geq \bar{x}$), there is no moral hazard problem, and the securitizer purchases all of the loans. When it is efficient for the lender to investigate (that is, $\bar{x} \leq x < \bar{x}$), the securitizer purchases a fraction of loans for each value of $x$ such that the remaining portfolio loans provide sufficient incentive for the lender to investigate.
The lender-driven cutoff rule theory thus predicts that, when information is asymmetric, lender credit score cutoff rules can result in securitizers adopting corresponding cutoff rules in their purchasing decisions and a discontinuity in the aggregate securitization rate at the credit score cutoff used by lenders. But such discontinuities will shrink, and in the limit disappear, as the screening behavior of lenders becomes more contractible and the overall asymmetry of information is reduced.

2.2. **Exogenous securitizer cutoff rule theory.** KMSV offer a very different explanation of credit score cutoff rules—they posit that securitizers follow a rule-of-thumb such that they are more willing to buy mortgage loans made to borrowers with credit scores just above certain thresholds than just below, and that the incentives created by these rules induce lenders to employ screening cutoff rules via a moral hazard effect. We refer to this as the *exogenous securitizer cutoff rule theory*.

The motivation for securitizers’ use of cutoff rules is not explicitly modeled by KMSV. One possibility is that securitizers are acting in a naive or boundedly rational way, refusing to purchase loans below some credit score threshold because they are “too risky,” even though the optimal mortgage purchase behavior does not exhibit discontinuities. In principle there might be a rational model that would predict optimal securitizer cutoff rules. The defining feature of the exogenous securitizer cutoff rule theory is that it posits exogenous variation in ease of securitization at a credit score threshold such that the discontinuous change in default rates at the securitizers’ threshold is a reduced form estimate of the causal effect of the change in ease of securitization at that threshold on lenders’ screening behavior.

As a simple formalization of this theory, suppose that we add a rule-of-thumb securitizer to the baseline model considered above. In particular, suppose that, for all loans with $x$ strictly less than some $\hat{x} \in [\underline{x}, \bar{x}]$, the securitizer buys a fraction $\varpi$ of loans, while for loans with $x \geq \hat{x}$, the securitizer buys a fraction $\bar{\varpi} > \varpi$ of loans. It is straightforward to see that, for certain parameter values, this will cause the screening threshold to move from $\bar{x}$ to $\hat{x}$. Figure 4 depicts the resulting default rate of loans as a function of $x$, with the dashed line showing the default rate function in the absence of securitization.

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6Because securitizers do not generally analyze individual loans, per-loan fixed cost arguments similar to those made for lenders in our model would have difficulty explaining the independent use of cutoff rules by securitizers.
2.3. **Implications of competing cutoff rule theories for estimating the effect of securitization on lender screening.** We now turn to the potential use of credit score cutoff rules to investigate the effect of securitization on lender screening. First, suppose the exogenous securitizer theory is true. Figure 4 illustrates the causal effect of securitization on lender screening. The insight of KMSV is that, under the exogenous securitizer cutoff rule theory, one can estimate the size of this causal effect at the cutoff used by the securitizer in its purchasing decisions using standard regression discontinuity estimation techniques. This is so because, since default risk varies smoothly with FICO for fixed $\sigma$, the counterfactual default rate of loans at $\hat{x}$ had they been securitized at the lower rate $\sigma$ is equal to $\lim_{x \uparrow \hat{x}} d(x)$.

An important limitation of this approach is that it enables estimation only of the effect of the change in ease of securitization at the securitizers’ cutoff. This may not be very informative about the overall effect of securitization on lender screening or on the effects of alternative policy instruments that might be used to mitigate the moral hazard problem, like an originator credit risk retention requirement. The shaded area in Figure 4 defines the range of credit scores where securitization affects the lender’s screening behavior. For loan applicants with credit scores above $\bar{x}$, securitization has no effect on lender screening behavior, and similarly for credit scores below $\hat{x}$.

A crucial identifying assumption of this regression discontinuity research design is that there is no reason for lenders’ behavior to discontinuously change at the threshold other than in response to a change in the ease of securitization. In standard regression discontinuity designs, this assumption is formulated as a continuity assumption on the outcome variable’s conditional regression functions. We can express it in terms of the formal theoretical model presented above by introducing a little additional notation. Let $d(x|\sigma)$ denote the equilibrium default rate of loans to borrowers of type $x$ given that an exogenous fraction $\sigma$ of the loans are securitized. Then the key identifying assumption is that this function is continuous in $x$ for fixed $\sigma$. If instead there is a reason for the default rate to discontinuously change at the securitizer’s credit score cutoff $\hat{x}$ even if the securitizers’ behavior did not change at that cutoff, then one cannot estimate the causal effect of any change in ease of securitization at $\hat{x}$ by estimating the change in default rates at $\hat{x}$. Imbens and Lemieux (2008) provide a more general formulation of this identifying assumption using a potential outcomes framework.
Now suppose instead that the lender-driven theory introduced above is true. Then it should be obvious that the change in default rates at the lender’s screening threshold is not an unbiased estimate of the causal effect of securitization on lenders’ incentives to screen. Such a discontinuous change in default rate would incur in the absence of any securitization. Or in the more formal terms of the identifying assumption introduced above, \( d(x|\sigma) \) is not continuous at \( \bar{x} \) for fixed \( \sigma \).

Moreover, as explained above, the lender-driven cutoff rule theory implies that causality can run in the reverse direction as supposed by the exogenous securitizer cutoff rule theory. In particular, by introducing discontinuities in the amount of private information that lenders have about loan applicants, the cutoff rule used by lenders can result in discontinuities in the securitization rate at the cutoff.

3. **Institutional Evidence**

We now turn to evidence to investigate the two competing theories for the origin of credit score cutoff rules. We begin with institutional evidence from the history of the use of credit scores in mortgage underwriting. It shows that credit score cutoff rules were adopted by lenders in response to guidance from the GSEs from the very beginning of the use of credit scores by the mortgage industry. Moreover, the rationale for the use of credit scores fits the lender-driven cutoff rule theory. Finally, the evidence shows that there are reasons for lender behavior to change at credit score cutoffs other than any change in ease of securitization, implying that lenders’ cutoff rules are not a useful laboratory for studying the effect of securitization on screening.

3.1. **The adoption of credit score cutoff rules by mortgage originators.** Mortgage lenders began to incorporate FICO scores into their underwriting procedures in the mid-1990s \cite{Straka2000}. Lenders employed cutoff rules that required increased scrutiny of loan applicants below some threshold FICO score, and 620 became a widely adopted threshold long before private-label mortgage securitization grew to any significant scale. \cite{Avery,Bostic,Calem,Canner} describe the use of cutoff rules in mortgage lending thus:

> To operate a scoring system for credit underwriting, a lender must select a cutoff score (such as 620) that can be used to distinguish acceptable from unacceptable risks. Regardless of the cutoff score selected, some customers with bad scores will be offered credit because of offsetting factors, and some customers with good scores will be denied credit, also because of offsetting factors.
An important catalyst of the mortgage industry’s adoption of FICO scores, and the 620 cutoff in particular, was guidance from Fannie Mae and Freddie Mac (the GSEs). The GSEs had conducted research into the relationship between FICO scores and mortgage performance showing that “despite the fact that those borrowers who had FICO scores in the lower range (620 or less) represented only a very small percentage of the total universe, they (as a group) accounted for approximately 50% of the eventual defaults...” (Fannie Mae, 1995, p. 4). They recommended that lenders apply increased scrutiny to borrowers with low FICO scores “to determine whether any extenuating circumstances contributed to the lower credit score” (Fannie Mae, 1995, p. 5).

In 1997, Fannie Mae released a letter giving further guidance to lenders by establishing three tiers of FICO scores: for borrowers with FICO scores above 720, default risk is “very low,” and “the underwriter should focus on ascertaining that all significant credit information is included in the credit file”; for those with scores between 660 and 719, default risk is “low,” and the lender similarly need only verify that the credit history is complete; those with scores between 620 and 659 “represent a high degree of default risk,” and “the underwriter must perform a complete assessment of all aspects of the applicant’s credit history”; and those with scores below 620 represent a “very high” risk of default, and “the underwriter must apply good judgment when he or she considers the unique circumstances of each application” and “if there are sufficient compensating factors or extenuating circumstances that offset the higher risk of default associated with credit scores in this range, the underwriter may approve the financing” (Fannie Mae, 1997, pp. 8-9). Freddie Mac (1996) established similar guidelines.

Lenders widely adopted the GSEs’ guidance on the use of FICO scores, including the use of the FICO score thresholds they recommended for gathering additional information about borrowers’ creditworthiness. The GSEs were essentially providing a public good by analyzing their data on the relationship between FICO scores and mortgage performance to determine the optimal cutoff rule. The GSEs were uniquely well-situated to provide this public good, given that they had much more data on mortgage performance than any single lender and stood to gain from the industry-wide improvement in underwriting that such research could bring about.

Why did lenders adopt cutoff rules at all, rather than a continuous schedule of investigation intensities? In general, lenders face non-divisible, discrete screening decisions: whether to conduct a face-to-face interview, whether to verify claims about unusual circumstances such as medical
emergencies, and so on. The discreteness of such decisions naturally leads lenders to employ
cutoff rules.

3.2. **Automated underwriting systems encode the cutoff rules.** The most important indivisible
screening decision that lenders make is probably the choice between relying on an automated
underwriting system alone, or conducting an additional manual underwriting process. Automated
underwriting systems (AUSs) became widely adopted in the mid-1990s (Hutto and Lederman, 2003). Most mortgage originators use either the Desktop Underwriter (DU) program, created by Fannie Mae, or the Loan Prospector (LP) program, created by Freddie Mac. These programs take as inputs information such as FICO score, loan-to-value ratio, and debt-to-income ratio, and quickly compute a recommendation. Fannie Mae’s website advertises that DU allows lenders to process mortgage loan applications “in 15 minutes or less.”

When lenders get an “approve” or “accept” recommendation from their AUS, that is usually the
end of the process and they approve the loan. When they receive a “refer” or “caution” recom-
mendation, they may then begin the process of manual underwriting (Hutto and Lederman, 2003). Manual underwriting is similar to underwriting as it was done before the advent of AUSs. The lender collects additional information, such as information about non-standard sources of income, cash reserves, and the applicant’s explanation of recent income or payment shocks. The lender may also conduct a face-to-face interview in order to gauge “character risk.” The lender then makes a holistic judgment to determine whether to extend credit. Hutto and Lederman (2003, p. 201) write:

> Mortgage bankers often describe underwriting as more of an art than a science. However, with the advent of the statistical systems used by AUSs, the “accept” and “approved” loans are now more science than art. However, those loans ranked “re-
fer” or “caution” do still require the use of the underwriting art since the evaluation
of compensating factors is involved... Automated underwriting has allowed underwriters to focus on those loans where mortgage bankers most need their special expertise—that is, in the refer/caution area where underwriting judgment is critical. These loans require manual review of credit and manual evaluation of compensat-
ing factors.

Manual underwriting is more costly and time-consuming than automated underwriting. Instead
of 15 minutes, manual underwriting may occupy days of a loan officer’s time. The decision to

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7One notable exception is Countrywide, which uses the Countrywide Loan Underwriting Expert System (CLUES). This proprietary software is similar to DU and LP.
undertake manual underwriting is discrete and a clear example of a fixed cost in information gathering.

Because DU and LP are designed and distributed by the GSEs, which advocate that lenders use 620 as a cutoff, these cutoffs are coded directly into the AUS decision rules. Though AUSs calculate default risk using smooth functions of FICO score, they also employ a layer of “overwrites” which are triggered when borrowers fall into certain categories—for instance, borrowers with FICO scores below 620\(^8\) The effect is that a loan to a borrower with a FICO of 620 is discontinuously more likely to receive an “approve” recommendation from DU or LP than a similar borrower with a FICO of 619. As a result, lenders following AUS recommendations are discontinuously more likely to initiate manual underwriting for a borrower with 619 FICO than for a borrower with 620 FICO. Reliance on AUSs is yet another reason why, even though the fixed cost \(c\) may theoretically vary between lenders, lenders coordinate on a few key FICO thresholds.

Loans that are “referred” are still eligible for purchase by the GSEs (and private securitizers) so long as the lender judges them to be acceptable through its manual underwriting process\(^9\). Notably, “reject” is not one of the recommendations given by AUSs: they merely “refer” the lender to a more thorough underwriting protocol (Fannie Mae, 2007). Securitizers commonly buy loans that are initially referred and later approved through the manual underwriting process.

3.3. Discussion. This institutional evidence is inconsistent with the exogenous securitizer cutoff rule theory but consistent with the lender-driven cutoff rule theory. It shows that lenders did not adopt credit score cutoff rules in response to exogenous discontinuous changes in the probability of securitization at certain thresholds. Rather, they were adopted by lenders in response to guidance from the GSEs and their incorporation into widely used underwriting software. The rationale given by market participants for the use of these cutoff rules is consistent with the fixed cost-based rational model presented above. This also implies that the change in defaults at these credit score thresholds is not evidence that securitization reduced lenders’ incentives to screen. Even if securitizers followed exogenous purchasing rules-of-thumb at those same thresholds, any resulting

\(^8\)Personal communication with Freddie Mac executives, October 9th, 2009.
\(^9\) Certain exceptions apply—for instance, GSEs will not buy loans over the conforming loan size threshold no matter what the lender determines. In addition to the approve/refer recommendation, DU presents a separate eligible/ineligible output that tells the lender whether the loan violates one of Fannie Mae’s eligibility guidelines. Until 2008, there was no minimum FICO score that would make a loan ineligible. The fact that AUSs can be used to evaluate loans ineligible for purchase by the GSEs, such as jumbo loans, demonstrates that AUSs are not merely meant to aid in securitization.
incentive effect would be confounded by lenders’ independent use of cutoff rules in screening and the effect of the discontinuous change in GSE guidance and AUS output at those same thresholds.

4. Quantitative Evidence

We analyze loan-level data to further distinguish between the lender-driven cutoff rule theory and the exogenous securitizer cutoff rule theory. Cutoff rules, we find, are used at least as much by lenders that seldom securitize as they are by lenders that often securitize, a result that would not be expected if the use of cutoff rules were driven by securitizers. We also find that, for several key subsamples, the probability a loan is securitized is constant across screening thresholds in which the number of loans and their default rate jump. This is direct evidence that differences in the probability of securitization are not the cause of lender cutoff rules.

Having demonstrated that lenders independently use screening cutoffs we next analyze the securitization evidence in light of our endogenous lender cutoff rule model. We find that in markets where the threat of punishment is large—those dominated by Fannie and Freddie—there is no evidence of a securitization discontinuity around the lender cutoff. In markets where securitizers are smaller and less permanent we do find a discontinuity. This is evidence that the discontinuous change in originator information at the screening cutoff is inhibiting the securitization of mortgages.

4.1. Data. Our data come from Lender Processing Services Applied Analytics, Inc. (LPS). These are loan-level data collected through the cooperation of 18 large mortgage servicers, including 9 of the top 10 servicers in the United States. Foote, Gerardi, Goette, and Willen (2009) provide a detailed discussion of the dataset, on which we draw. As of December 2008, the data covered about 60 percent of outstanding mortgages in the United States and contained about 29 million active loans. Key variables in the dataset include borrower FICO scores, detailed loan terms, securitization status, and monthly loan performance data. Originators commonly contract with outside servicers who manage the day-to-day collection of mortgage payments. These servicers are employed to collect payments and pursue accounts that are delinquent; they are the main agents with whom borrowers interact after a loan has been originated. All of the loans in LPS

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10These data are sometimes referred to by the name McDash. Lender Processing Services acquired McDash Analytics in November 2008.

11Servicers generally have some form of performance incentive built into their compensation contract. An originator’s decision to sell servicing rights is distinct from its decision to sell rights to the stream of payments from the loan itself.
were either originated by one of the 18 servicers or had their servicing rights sold to one of these 18 servicers. LPS contains privately securitized loans, GSE-purchased loans, and portfolio loans (loans for which the originator retains rights to the payment stream).

We select from LPS first-lien, non-Federal Housing Administration insured, non-Veterans Administration insured, non-buydown, home purchase loans originated between 2003 and 2007 for owner-occupied, single-family residences. We also eliminate Ginnie Mae buyout loans, as well as loans bought by the Federal Home Loan Bank or local housing authorities (together these constitute less than 1 percent of the original sample). Borrowers must have FICO scores of between 500 and 800 to be included in the sample.

The GSEs’ mortgage purchases and mortgage-backed securities issuance accounted for 55 percent of all mortgage loans by dollar amount originated in the United States in 2007 (Inside Mortgage Finance, 2008). Because of the large influence of the GSEs, we split the sample into a “conforming” sample of loans for amounts below the conforming loan limits which the GSEs are bound to observe, and a jumbo sample of loans that exceed those limits. The GSEs buy only loans that are for amounts below these limits and that meet additional eligibility criteria, such as limits on debt-to-income ratios. Although “non-jumbo” would technically be a more accurate term, for simplicity we use the term “conforming” for all loans that are for amounts below the conforming loan limits, including loans that fail to meet these other eligibility criteria. In the conforming market during our sample period the GSEs account for 76 percent of all loan purchases. In contrast, virtually all loan purchases in the jumbo market are done by private securitizers. Analyzing the jumbo market separately provides an opportunity to see whether the rules used in screening mortgage borrowers, and their effect on securitization, are different in the absence of the GSEs.

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12 Some originators are also servicers. For instance, Bank of America has a servicing division that ranks as one of the country’s largest. Servicing divisions of originating banks operate fairly autonomously, and can buy servicing rights to loans not originated by the parent bank.

13 A fraction of loans bought by GSEs are not used in the issuance of securities but are instead kept in the GSEs’ own portfolios. We cannot distinguish these from GSE-securitized loans in our data, though we know the majority of loans purchased by the GSEs—83 percent in 2007 according to Inside Mortgage Finance (2008)—are in fact securitized. Luckily, the distinction between loans that are purchased in the secondary market and subsequently securitized, and those that are purchased but never securitized, is irrelevant for our model. What matters is whether or not the loan is purchased at all. For simplicity we use the term “securitized” to refer to all loans purchased on the secondary-market.

14 We chose the 2003-to-2007 period because LPS sample sizes are relatively low before 2003.

15 For the continental United States, the conforming loan limits for single-family homes were $322,700 in 2003, $333,700 in 2004, $359,600 in 2005, and $417,000 in 2006 and 2007.
In addition to the conforming and jumbo samples, we examine a sample of low documentation loans in order to provide additional comparability with KMSV’s results. One feature of the recent mortgage boom was the proliferation of so-called low documentation or “low doc” loans, which unlike standard loans (“full doc” loans) required limited or no documentation of borrowers’ income and assets. In their exposition of the moral hazard story, KMSV restrict their main analysis to low documentation loans because, they argue, soft information plays a bigger role in screening these loans due to their lack of hard information. However, the amount of documentation required by lenders is an important aspect of lender screening, which is the outcome variable of interest. Indeed, documentation status is a more direct measure of lender screening than is loan performance. Figure 6 plots the percentage of loans in our conforming sample that are classified as low doc loans. Consistent with other evidence that lender screening changes at 620 (presented below), there is a dramatic fall in the fraction of low doc loans below 620. Accordingly, selecting the sample based on the outcome results in biased estimates of the effect of the change in market participants’ behavior at 620 on loan performance—a problem referred to by statisticians as posttreatment selection bias (Frangakis and Rubin, 2002). Nonetheless, we include analysis of the low-doc sample to provide better comparability with KMSV’s results.

We define loan default as a binary variable equal to 1 if payment was delinquent by 61 days or more at any time in the first 18 months after origination. We define a loan’s securitization status using its status at six months after origination. Many loans spend their first few months in portfolio before being sold, but the vast majority of loan sales occur within the first 6 months. From six months onward, the proportion securitized is stable, as can be seen in Figure 7. Loans with missing securitization status at six months are dropped from the sample.

Tables 1, 2, and 3 provide sample sizes and summary statistics for our data. Although the conforming and jumbo samples are mutually exclusive, all loans in the low doc sample appear also in either the conforming or the jumbo sample. Among conforming loans, 90 percent of the sample is securitized through either the GSEs or private securitizers. In the jumbo sample only 72 percent

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16Our definition of “low documentation” includes so-called “no documentation” loans.
17Results are similar if we use the default definition employed by KMSV, which is a binary variable equal to 1 if payment was delinquent by 61 days or more at any time between the 10th and 15th month after origination, and if we restrict our sample to the 2001-06 origination window used by KMSV.
are securitized; of these, nearly all are privately securitized.\footnote{We use a flag provided in the LPS dataset to identify which loans are jumbo loans. In theory the GSEs should not buy any jumbo loans; the 1.9 percent of our jumbo sample that was purchased by the GSEs are miscoded or the GSEs do not comply perfectly with the conforming loan limits.} Approximately 5 percent of loans in all samples default within the first 18 months, though the fraction is higher for borrowers in the neighborhood of 620.

4.2. \textbf{Econometric specifications.} Formally estimating the size of discontinuities in frequency is a more difficult problem than estimating discontinuities in a dependent variable that is defined for every observation. Our approach follows McCrary (2008), which develops a formal test of the continuity of the density function of the running variable in RD analyses that allows for proper inference. The method entails first estimating a histogram of the data and then estimating the regression function on either side of the cutoff using a weighted local linear regression of the (normalized) counts in the bins on the mid-points of the bins. This method has the advantage of a standard error estimator that is consistent under reasonable assumptions.\footnote{An alternate approach is to collapse the data so that there is one observation per FICO score and frequency is the dependent variable. Then we can apply standard regression discontinuity (RD) techniques to the collapsed data. This approach is straightforward, but the OLS standard errors are incorrect and are likely overestimates resulting from the application of OLS on collapsed data. Though we do not use this specification, all of our results are robust to using it.}

To examine discontinuities in dependent variables such as the default rate and the securitization rate we perform a more standard RD analysis. We estimate 6th-order polynomials on either side of the cutoff using the full sample:

\begin{equation}
Y_i = \beta_0 + \beta_1 \mathbb{1}_{FICO_i \geq 620} + f(FICO_i) + \mathbb{1}_{FICO_i \geq 620} * g(FICO_i) + \lambda_y + \epsilon_i
\end{equation}

where $i$ indexes individual loans, $Y_i$ indicates whether loan $i$ defaulted (or was securitized), $\lambda_y$ are year fixed effects, and both $f(FICO_i)$ and $g(FICO_i)$ are 6th-order polynomials in $FICO$.

For robustness we sometimes include a local linear regression as a second specification. We restrict the sample to a 10 FICO score point band on either side of the threshold and fit a line on either side.\footnote{Results are not sensitive to using alternative bandwidths.} This method is equivalent to the above specification where $f(\cdot)$ and $g(\cdot)$ are both first-order polynomials, performed on a sample restricted to the neighborhood [610,629].

4.3. \textbf{Evidence for FICO 620 as a lender screening cutoff.} The data show that credit score cutoff rules are common in mortgage origination. Figure\footnote{Results are not sensitive to using alternative bandwidths.} presents a histogram of loan originations by

18\textsuperscript{We use a flag provided in the LPS dataset to identify which loans are jumbo loans. In theory the GSEs should not buy any jumbo loans; the 1.9 percent of our jumbo sample that was purchased by the GSEs are miscoded or the GSEs do not comply perfectly with the conforming loan limits.}

19\textsuperscript{An alternate approach is to collapse the data so that there is one observation per FICO score and frequency is the dependent variable. Then we can apply standard regression discontinuity (RD) techniques to the collapsed data. This approach is straightforward, but the OLS standard errors are incorrect and are likely overestimates resulting from the application of OLS on collapsed data. Though we do not use this specification, all of our results are robust to using it.}

20\textsuperscript{Results are not sensitive to using alternative bandwidths.}
FICO score from 2003 to 2007. The graph is a step-wise function, with sharp, sizable increases in loan frequency at the FICO scores of 600, 620, 660, 680, and 700.\textsuperscript{21}

Panel A of Table 4 estimates the size of the discontinuity at each cutoff point. FICO 620 is associated with a discontinuity of 45 log points, nearly three times the size of the next largest discontinuity. Panel B presents estimates of the default rate discontinuities at each cutoff. The default discontinuities roughly scale with the frequency discontinuities—the 2.1 percentage point discontinuity at 620 is nearly twice the size of the next largest. Given the size of the 620 discontinuity, its use in KMSV, and its prominence in industry documents, we focus our analysis on the 620 cutoff for the remainder of the paper.

The evidence in Table 4 is consistent with both the exogenous securitizer and lender-driven cutoff rule theories. We now turn to evidence that can differentiate between the two theories.

4.3.1. \textit{Use of 620 by lenders unlikely to securitize.} If lender cutoff rules are driven by exogenous securitization cutoff rules, we expect to see them used more by lenders that are more likely to securitize. Under the exogenous securitizer cutoff rule theory, a portfolio lender that keeps the vast majority of its loans on its book has no reason to adopt a cutoff rule in screening, whereas a mortgage originator that sells many of its loans will be induced to use a cutoff rule. In contrast, the endogenous lender cutoff theory makes no such prediction.

To test this prediction, we identify individual lenders by merging our LPS data with Home Mortgage Disclosure Act (HMDA) filings for 2006 and 2007, merging exactly on loan origination date, origination amount, and zip code/census tract.\textsuperscript{22} We are able to match approximately 37\% of our sample to HMDA. Using lender identifiers we divide our sample into four quartiles: those lenders that are least likely to securitize their loans are in the first quartile, those that are slightly more likely are in the second, and so on.\textsuperscript{23} If lender cutoff rules were driven by securitization we would expect the size of the discontinuity to be larger for the higher quartiles, where the likelihood of securitization is greater.

\textsuperscript{21}Because the underlying distribution of FICO score is continuous in the population of potential borrowers (KMSV, p. 3), these discontinuities in the distribution of loans demonstrate that the lending rate itself (i.e. the probability a potential borrower gets a loan) jumps, rather than simply the number of potential borrowers jumping.

\textsuperscript{22}LPS contains zip code and HMDA contains census tract. Because zip codes and census tracts are not always congruent we consider it a match if there is any overlap in area between the zip code and the census tract.

\textsuperscript{23}We retain only those lenders for which we have at least 30 observations. We do this to ensure we have valid estimates of each lender’s probability of securitization.
Table 5 shows that, if anything, the pattern is the opposite. Lenders in the first quartile (those that securitize the least) display a larger discontinuity in frequency (55 log points) than those in any of the other quartiles (28, 26, and 28 log points). This evidence suggests that lenders’ use of credit score cutoffs is not driven by securitization.

4.3.2. Screening cutoffs without securitization cutoffs. Another way to test between the two cutoff rule theories is to examine how the securitization rate behaves around the cutoff. The securitizer-driven model is predicated on the assumption that the securitization rate changes at the cutoff. Finding a screening threshold without a corresponding jump in securitization is therefore evidence against the securitizer-driven model of cutoff rules. In contrast, the lender-driven cutoff rule theory predicts that screening cutoffs need not be associated with any change in the securitization rate—indeed, they would exist without any securitization.

We first clarify the relevant probability of securitization, as a conceptual matter. An unusual aspect of KMSV’s implementation of their empirical strategy is that they use a regression discontinuity design, where probability of securitization is the treatment, using a dataset with only securitized loans. Using this dataset KMSV cannot estimate a first stage to confirm that there is a discontinuity in the probability that loans are securitized at the 620 threshold. Instead, KMSV show that the number of loans in their dataset of securitized loans jumps at 620. Because the FICO distribution of potential borrowers is continuous at 620, they argue that this shows that the “unconditional probability” of securitization (that is, the probability that a potential borrower is given a loan which is later securitized, rather than either not being given a loan at all or being given a loan that is kept in portfolio) jumps at 620.

However, the probability relevant for testing the hypothesis that securitization diluted the screening incentive of lenders is the probability that a loan is securitized, not the probability a potential borrower is given a securitized loan. If a lender has a very high probability of selling a loan, say to a naive securitizer, then the lender’s incentives to screen borrowers might be attenuated. If instead there is a large chance that the lender will keep the loan, then the moral hazard problem is less severe. This probability a loan is kept is what is usually meant by “skin in the game.” The unconditional probability in which KMSV demonstrate a jump conflates two different probabilities: (1) the probability that potential borrowers are given a loan, which we will refer to as the lending rate; and (2) the probability that loans are securitized, which we call the securitization rate. More
formally, let $L_i \in \{0, 1\}$ denote whether potential borrower $i$ is given a loan and let $S_i \in \{0, 1, \emptyset\}$ denote whether borrower $i$’s loan is securitized (with $S_i = \emptyset$ if borrower $i$ is not given a loan). KMSV’s unconditional probability is then:

$$\Pr(S_i = 1) = \Pr(L_i = 1) \times \Pr(S_i = 1 | L_i = 1)$$

The first factor on the right-hand side of this equation is the lending rate; the second factor is the securitization rate. KMSV show that this product, measured as the number of securitized loans, jumps at 620, but they cannot tell whether this is because the lending rate jumps or because the securitization rate jumps. Our dataset contains both securitized and portfolio loans, enabling us to decompose the jump in the unconditional probability into jumps in the lending rate and securitization rate.

Table 6 presents estimates of the discontinuities at 620 in the lending rate, default rate, and securitization rate for the conforming, jumbo, and low documentation subsamples. Column 1 shows that there are large and significant jumps in the lending rate at 620 for all three subsamples. Figures 8, 9, and 10 plot the FICO histograms for the conforming, jumbo, and low doc samples, respectively. Discontinuities in the density functions at 620 are visually apparent.

Columns 2 and 3 of Table 6 report default rate results for the samples. We estimate a significant discontinuity in the default rate of the conforming sample of 2.1 percentage points using the polynomial regression and 1.4 percentage points using the local linear regression on a base level default frequency of about 14 percent. Results for the jumbo sample are similar or larger in magnitude, but the smaller sample size renders them insignificant. We estimate a discontinuity of 2.8 percentage points using the polynomial regression (p-value of 0.12) and 1.4 percentage points using the local linear regression (p-value of 0.39), on a base default rate of approximately 19 percent. Discontinuities for the low doc sample are largest of all, with an estimate of 5.9 percentage points for the polynomial regression on a base rate of 13.5. Figures 11, 12, and 13 plot default rates by FICO score for the conforming, jumbo, and low doc samples, respectively. The jumps in default rates at 620 are visually apparent.

Columns 4 and 5 of the same table report analogous specifications with securitization as the dependent variable. We estimate significant jumps of 4.7 and 5.8 percentage points for the jumbo sample, but much smaller jumps of 0.4 and 0.6 percentage points for the conforming sample, the
latter of which is marginally significant. For the low doc sample the point estimates are actually negative: -1.4 and -0.7 percentage points, the former of which is marginally significant. Figures [14], [15], and [16] reveal a visually apparent discontinuity for the jumbo sample, but not for the conforming nor low doc samples. We thus find evidence for a discontinuity in the securitization rate at 620 for the jumbo sample, but not for the conforming sample nor the low doc sample.

There is robust evidence that 620 is used as a screening threshold: we find lending and default discontinuities at 620 in all three of our samples. However, only the jumbo sample displays a discontinuity in the securitization rate at 620; the conforming and low doc samples have a smooth securitization rate across the threshold.

The existence of discontinuous changes in default rates in the absence of any jumps in the securitization rate is a strong rejection of the exogenous securitizer cutoff rule theory. It implies that there are reasons other than a change in ease of securitization for the use of cutoff rules by lenders. More formally, it is direct evidence of a failure of the identifying assumption that $d(x|\sigma)$ is continuous in $x$ for fixed $\sigma$. The institutional evidence makes clear why this is so—lenders have been following cutoff rules for reasons that have nothing to do with changes in ease of securitization. And the lender-driven theory provides a plausible explanation for these facts. This evidence also makes the regression discontinuity identifying assumptions implausible for other samples that do exhibit discontinuities in both screening and in securitization. Even if the discontinuity in securitization were exogenous, it is not possible to separate the part of the change in screening attributable to securitization vs. the other reasons for the change in screening at the threshold. Moreover, the lender-driven theory shows that the discontinuous changes in securitization may be an effect, rather than a cause, of the change in lender screening.

4.4. Discussion of securitizer evidence. As we have seen, in the jumbo mortgage market without the GSEs securitizers left a greater fraction of loans on lenders’ books when those loans were below the lender screening threshold. In contrast, in the conforming market in which the GSEs buy the majority of all loans, there is no jump in securitization rates at 620.

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24Figure [15] reveals that the securitization rate right at 620 in the conforming sample is an outlier. Furthermore, the FICO histograms in Figures [8], [9], and [10] reveal that bunching occurs at 620. The cause of this phenomenon is unclear, and our polynomial specifications limit its influence on our discontinuity estimates. Because of this outlier, the local linear estimate of the discontinuity for the conforming sample is sensitive to bandwidth—for a bandwidth of 1, it is a significant (but still modest) 2 percentage point jump. With data at 620 dropped from the sample, the local linear estimate using a bandwidth of 10 is an insignificant -0.3 percentage point change.
Given the differences between GSEs and private securitizers in their access to instruments to police lender moral hazard, these results are consistent with the predictions of the rational model. The main advantage GSEs have over private securitizers is the threat of terminating a relationship. The GSEs can terminate their relationship with a lender if they observe any abnormal increase in default rates of the originator’s loans or evidence of failure to comply with the GSEs’ underwriting guidelines.\footnote{Freddie Mac (2001), Chapter 5, “Disqualification or Suspension of a Seller/Servicer” details the process by which Freddie Mac can terminate its relationship with an originator.} As a result of both the GSEs’ huge market share and their permanence in the market, a lender that shirks on screening loans that it sells to the GSEs faces the loss of a huge source of lending capital were the GSEs to cease purchasing its loans. This is not just a theoretical possibility: many originators have been terminated by the GSEs. For instance, New Century Financial Corp., a subprime lender, was terminated by Fannie Mae in March, 2007\footnote{See “New Century says cut off by Fannie Mae,” Reuters, March 20, 2007.}. Similarly, Taylor, Bean & Whitaker Mortgage Corp. was recently suspended by Freddie Mac.\footnote{See James R. Hagerty and Nico Timirasos, “Taylor Bean Ceases Lending,” Wall Street Journal, Aug. 6, 2009, at C12.} And terminations are not just a recent phenomenon: Donohue (2008) discusses how Fannie Mae discovered problems with First Beneficial Mortgage Corporation in the late 1990s and terminated its relationship with it. In contrast, the threat of termination by a smaller private secondary market purchaser is far less significant to an originator. The GSEs’ size and permanence provide them with much better enforcement of reputational mechanisms for mitigating moral hazard than are available to private securitizers.

In addition to this main structural difference, there is evidence that GSEs use other means to maintain loan quality which are not used, or are used less often, by private securitizers. Prior to 1982, Fannie Mae and Freddie Mac each “re-underwrote” every loan they purchased by employing staff underwriters to review every single loan file (Straka, 2000, p. 209)—a procedure that, to our knowledge, has never been used by private secondary market purchasers. Since 1982, they each rely on random sampling of loans for “postfunding review” of the loan file. Moreover, the GSEs sample a larger fraction of loans below 620 than above, and this more intensive monitoring is a substitute for the use of portfolio loans as an incentive instrument.\footnote{Personal communication with Freddie Mac representative, September 11th, 2009.} GSEs also make heavy use of “buyback” clauses which force lenders to repurchase loans if they default quickly or if any irregularities are found in the file.
Our results suggest that the GSEs’ ability to punish resolves the lender agency problem without the need to limit loan purchases below the lender cutoff. In contrast, private securitizers with less ability to punish do limit purchases in order to mitigate lender moral hazard.

5. USING VARIATION FROM ANTI-PREDATORY LENDING LAWS

KMSV (pp. 21-23) explicitly consider our central hypothesis—that the 620 FICO score threshold was used by lenders for reasons unrelated to securitization—and attempt to reject it using variation induced by the passage of state anti-predatory lending laws in Georgia and New Jersey in 2002 and 2003, respectively. They argue that the laws made it harder for lenders to securitize mortgages but kept “everything else equal” (p. 21). They further argue that if 620 represents a threshold used by lenders independent of securitization then the passage of these laws should have no effect on the discontinuities at 620. They then show that the discontinuity in the number of loans at 620 gets smaller, and that similarly the jump in default rates at 620 disappears, in Georgia and New Jersey during the period in which these laws were in effect.

There are two problems with this analysis, one theoretical and one empirical. The theoretical concern is that these laws did not change only the ease of securitization. The goal of the New Jersey Home Ownership Security Act of 2002 (NJHOSA), for example, was to prevent abusive lending practices. In addition to enabling borrowers to assert any claims against the purchaser of their mortgage that they could have asserted against the originating lender (that is, creating “assignee liability”), it restricted a range of lending practices for all loans, including certain kinds of lender-financed insurance, loan “flipping,” and late payment fees. Furthermore, for a class of “high-cost” loans, the Act limited the rate at which scheduled payments could increase on adjustable rate mortgages, negative amortization, interest rate increases upon default, and the financing of points and fees. The Georgia Fair Lending Act (GFLA) contained similar provisions targeting a range of abusive lending practices. One of the express purposes of these provisions was to reduce default.

These restrictions may have changed the lending rate and default rate discontinuities at 620 through channels other than securitization. The laws were designed to lower default levels, and it need not be the case that their impact on default was the same just above the 620 threshold (where defaults rates are higher and the provisions of the law may bind more) as it was below. Given the

30 O.C.G.A. § 7-6A-1, et seq.
content of the laws, it is difficult to use them as a sharp test of whether credit score cutoffs should be ascribed to securitization.

Empirically, we check whether the laws in fact lowered the rate of securitization—a test that KMSV could not perform, as their main dataset contained only securitized loans. KMSV’s analysis of these laws implies that they reduced securitization. However, we find that they did not.

Shortly after they were passed, both laws were amended to weaken their restrictions. The amendment to the GFLA limited the relief that could be granted against an assignee, and the amendment to the NJHOSA provided that borrowers could seek relief under the act only in their individual capacity and not as part of a class action. We define the period when each law was in effect as the interval between the date when it initially took effect and the date its amendment took effect. These are from the start of October 2002 to the end of February 2003 for the GFLA, and between the start of December 2003 and the end of May 2004 for the NJHOSA.

We use a difference-in-differences (DD) strategy to estimate the effect of each law on securitization. In order to make the requisite parallel trends assumptions more plausible, we use as comparison groups for each state the states that border them and restrict the dataset to the period from six months before each law was passed to six months after it was amended. To maximize sample size, we pool conforming and jumbo loans. For Georgia, with the sample restricted to contain loans originated in Georgia and its comparison group during the appropriate time window, we estimate:

\[
Y_i = \delta_0 + \delta_1 GA_i + \delta_2 LawPeriod_i + \delta_3 Law_i + \epsilon_i
\]

where \(Y_i\) is a securitization dummy, \(GA_i\) is an indicator of whether loan \(i\) was originated in Georgia, \(LawPeriod_i\) is an indicator of whether the loan was originated during the period when the GFLA was in effect unamended, and \(Law_i\) is the interaction of \(GA_i\) and \(LawPeriod_i\). We thus pool the pre-law and post-amendment periods together as the control period. We estimate the analogous specification for New Jersey separately.

Table 7 shows results for the two law changes. For Georgia, the DD estimate of the effect of the law is a significant 2.7 percentage point increase in securitization. For New Jersey, the effect is

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31 Specifically, the bordering states are DE, NY, and PA for NJ; and AL, FL, NC, SC, and TN for GA.
32 Unfortunately, LPS sample sizes are relatively small in the year 2003 and before, and the coverage is not as nationally representative as in later years.
close to zero and insignificant. Our data thus show that the laws did not have a negative effect on the securitization rate. Because the New Jersey and Georgia laws may have affected default rates directly, and because the laws do not appear to have lowered securitization rates, analysis of these laws cannot be used as evidence against our thesis that lenders employed credit score cutoff rules for reasons unrelated to the probability of securitization.

6. Conclusion

In this paper we develop an equilibrium model of mortgage markets in which credit score cutoff rules emerge endogenously. We contrast that theory with the alternative extant explanation for cutoff rules, which posits that they are the result of an exogenous rule-of-thumb used by securitizers.

Institutional evidence suggests that, as predicted by our rational theory, lenders make discrete choices about screening intensity at the FICO score of 620 for reasons unrelated to the ease of securitization. Evidence from a loan-level dataset shows that in the conforming mortgage market, as well as in a low documentation sample, there are screening cutoffs at 620 but no securitization discontinuity—a pattern of evidence consistent with the lender-driven cutoff rule theory but not the exogenous securitizer cutoff rule theory. Interpreting the cutoff rule evidence in light of our theory, it suggests that private mortgage securitizers adjusted their loan purchases around the lender screening threshold to maintain lender incentives to screen, while the GSEs maintained lender screening incentives by other means.

Moreover, our analysis shows that credit score cutoff rules unfortunately do not provide a useful laboratory for estimating the effect of securitization on lender screening. The evidence is inconsistent with the theory required for the change in defaults at credit score cutoffs to be attributable to securitization—a theory we call the exogenous securitizer cutoff rule theory. The extent to which securitization contributed to the subprime mortgage crisis remains an open and pressing research question.

References


33 Analogous DD regressions using default as the dependent variable estimate no effect for either state (not reported). It appears likely that these laws had little impact on mortgage lending in either state.


APPENDIX A

Model extension: securitization. Suppose a securitizer exists with a cost of funds slightly less than the lender’s cost of funds, so that its discount factor is \( \delta = 1 + \varepsilon \) for arbitrarily small \( \varepsilon \). We call this purchaser a “securitizer,” but all of our arguments apply to any secondary market purchaser of mortgages, not just those that package purchased loans and issue securities against them.

The securitizer and lender bargain over a contract characterized by two functions and an up-front payment: \( \sigma(x) \) denotes the fraction of loans of type \( x \) that the securitizer will purchase, \( T(x) \) represents the price that it will pay, and \( T \) represents an up-front payment that determines the ultimate division of surplus between the securitizer and lender. The game then proceeds as in the baseline model but, after loans are made, the lender sells a fraction \( \sigma(x) \) of loans of each type \( x \) to the securitizer for a payment \( T(x) \) per loan, with the securitizer choosing the particular loans that it purchases randomly at each \( x \).

Contractible screening. Suppose first that lender screening behavior is contractible. We derive the following proposition:

**Proposition 2.** In the equilibrium of the model with contractible screening, the lender’s behavior is the same as in the model without securitization, given in Proposition[7] and the fraction of loans securitized is \( \sigma(x) = 1 \) for all \( x > \bar{x} \).

Because screening is contractible, the securitizer and lender contract on the surplus-maximizing screening behavior, which is the same as in the baseline model. And because the securitizer has a lower cost of funds, all loans will be traded. The model predicts discontinuities in the lending rate and default rates, but not in the securitization rate.

Noncontractible screening. We now assume that the purchaser does not observe any signal generated by investigations by the lender, or whether the lender investigated, as this information is assumed to be “soft.” Thus, the contract cannot condition on whether the lender investigated or on

---

34We assume \( \varepsilon \) to be arbitrarily small to simplify the analysis of the model. With a discrete \( \varepsilon \), securitization would have real effects, but our key prediction of a discontinuity in the securitization rate at the lender’s screening threshold would remain.
whether a defaulter signal was revealed\(^{35}\). The securitizer is aware of the potential moral hazard problem but has only limited tools to combat it. In particular, it can adjust the proportion of loans it purchases around the cutoff in order to maintain the lender’s incentives to screen.

We characterize the equilibrium in the following manner:

**Proposition 3.** In the equilibrium of the model with noncontractible screening, the lender’s behavior is the same as in the model without securitization, given in Proposition 1 and the fraction of loans securitized for each \(x\) is given by:

\[
\sigma^*(x) = \begin{cases} 
\frac{R_x(1-x)c - Rc}{R_x(1-x)x} & \text{if } x \leq x < \bar{x} \\
1 & \text{if } x \geq \bar{x}
\end{cases}
\]

Figure 5 provides a notional diagram of equilibrium securitization rates. An important feature of the securitization rate is that it jumps discontinuously as you cross the screening threshold \(\bar{x}\) from below. Above the screening threshold securitizers need not worry about diluting the lender’s investigation incentives and can purchase all loans. Below the threshold the lender must retain some loans to maintain incentives to investigate.

**Proof of Proposition 1** For each loan applicant type \(x\), the lender does one of three things: denies the applications, accepts the applications without investigation, or investigates each applicant and, if no defaulter signal is observed, accepts the application. Denote this choice as \(a \in \{D, A, I\}\). The per-applicant payoff to the lender of each of these actions for each value of \(x\) is given by:

\[
V(x|a) = \begin{cases} 
0 & \text{if } a = D \\
\bar{R}x - 1 & \text{if } a = A \\
1 - (1 - x)s(1 - x)s - c & \text{if } a = I
\end{cases}
\]

The lender’s optimization problem is thus to choose an action \(a(x)\) for each value of \(x\) that solves:

\[
\max_{a \in \{D, A, I\}} \left\{ V(x|a) \right\}
\]

Accepting is preferred to investigating if and only if \(\bar{R}x - 1 \geq \bar{R}x - (1 - (1 - x)s) - c \Leftrightarrow x \geq 1 - \frac{c}{s} = \bar{x}\). Accepting is preferred to rejecting if and only if \(\bar{R}x - 1 \geq 0 \Leftrightarrow x \geq \frac{1}{\bar{R}}\). Investigating is preferred to rejecting if and only if \(\bar{R}x - (1 - (1 - x)s) - c \geq 0 \Leftrightarrow x \geq \frac{1-s+c}{\bar{R}-s} = x^\dagger\). Hence, the proposition holds if and only if the following are true:

1. \(\bar{x} > x^\dagger\), or \(1 - \frac{c}{s} > \frac{1-s+c}{\bar{R}-s}\). Rearranging this inequality yields \(c < \frac{(\bar{R}-1)s}{\bar{R}}\), which we assumed was true.
2. \(\bar{x} < 1\), or \(1 - \frac{c}{s} < 1\), which is true since \(c > 0\) and \(s > 0\).
3. \(\bar{x} > 0\), or \(\frac{1-s+c}{\bar{R}-s} > 0\), which is true since \(\bar{R} - s > 0\) and \(s - c < 1\).

□

**Proof of Proposition 2** We set up the securitizer’s problem using the standard contract-theoretic approach: for each \(x\), the securitizer maximizes the total surplus in the contract. The per-applicant

\(^{35}\)For simplicity, we assume that there is uncertainty about consumer demand, which is given by \(f(x)\), so that the securitizer does not update on whether the lender screened out the sure defaulters based on the number of loans made. Also, because lenders could restrict originations in order to give the appearance of having screened, inference based on loan frequency is unreliable.
surplus for each \( x \), for fixed \( \sigma(x) \) and \( a(x) \), is given by

\[
S(x, \sigma(x), a(x)) = \begin{cases} 
0 & \text{if } a(x) = D \\
(\sigma(x)\delta + 1 - \sigma(x))\bar{R}x - 1 & \text{if } a(x) = A \\
1 - (1 - x)s\left((\sigma(x)\delta + 1 - \sigma(x))\frac{x}{1-(1-x)\delta}\bar{R} - 1\right) - c & \text{if } a(x) = I 
\end{cases}
\]

(6)

Because \( a(x) \) is contractible, the securitizer need not worry about satisfying an incentive compatibility constraint for the lender. The securitizer’s problem is to find functions \( \sigma(x) \) and \( a(x) \) that solve, for each \( x \):

\[
\max_{\sigma(x)\in[0,1],a(x)} \left\{ S(x, \sigma(x), a(x)) \right\}
\]

(7)

Notice that the only difference between the surplus function \( S(x, \sigma(x), a(x)) \), given by (6), and the payoff function of the lender in the baseline model \( V(x|a) \), given by (4), is that the surplus contains the weighted average of the securitizer’s and the lender’s discount factor. By substituting in \( 1 - \varepsilon \) for \( \delta \), we can rewrite the surplus in terms of the baseline payoff function and an additional \( \varepsilon\sigma(x)\bar{R}x \) term:

\[
S(x, \sigma(x), a(x)) = \begin{cases} 
V(x|a(x)) & \text{if } a(x) = D \\
V(x|a(x)) + \varepsilon\sigma(x)\bar{R}x & \text{if } a(x) \in \{A, I\}
\end{cases}
\]

(8)

Note that \( S(x, \sigma(x), a(x)) \) is additively separable in \( \sigma(x) \) and \( a(x) \). This implies it can be maximized by first choosing \( a(x) \) to maximize \( V(x|a(x)) \), then choosing \( \sigma(x) \) to maximize \( \varepsilon\sigma(x)\bar{R}x \). The \( a(x) \) that solved the lender’s problem in the case without securitization now maximizes \( V(x|a(x)) \) in the present case, and \( \varepsilon\sigma(x)\bar{R}x \) is maximized by \( \sigma(x) = 1 \). Lastly, \( T(x) \) and \( T \) simply allocate the surplus between lender and securitizer.

**Proof of Proposition 3** The securitizer’s problem is similar to the one in Proposition 2 with the important difference that the choice of \( a(x) \) is now subject to the incentive compatibility constraint of the lender. For each \( x \), the securitizer maximizes the total surplus in the contract. The per-applicant surplus for each \( x \), for fixed \( \sigma(x) \) and action by the lender \( a(x) \), is given by

\[
S(x, \sigma(x)|a(x)) = \begin{cases} 
0 & \text{if } a(x) = D \\
(\sigma(x)\delta + 1 - \sigma(x))\bar{R}x - 1 & \text{if } a(x) = A \\
1 - (1 - x)s\left((\sigma(x)\delta + 1 - \sigma(x))\frac{x}{1-(1-x)\delta}\bar{R} - 1\right) - c & \text{if } a(x) = I 
\end{cases}
\]

(9)

For fixed \( \sigma(x) \) and \( T(x) \), the lender receives the following per-applicant payoff for each \( x \) as a function of its choice \( a \):

\[
V(x, \sigma(x), T(x)|a) = \begin{cases} 
0 & \text{if } a = D \\
\sigma(x)T(x) + (1 - \sigma(x))\bar{R}x - 1 & \text{if } a = A \\
1 - (1 - x)s\left(\sigma(x)T(x) + (1 - \sigma(x))\frac{x}{1-(1-x)\delta}\bar{R} - 1\right) - c & \text{if } a = I 
\end{cases}
\]

(10)

Faced with a \( \sigma(x) \) and \( T(x) \), the lender will choose \( a(x) \), which we assume is non-contractible, to maximize \( V(x, \sigma(x), T(x)|a) \) for each \( x \).

The securitizer’s problem is thus to find functions \( \sigma(x) \), \( T(x) \), and \( a(x) \) that solve, for each \( x \):

\[
\max_{\sigma(x)\in[0,1],T(x)|a(x)} \left\{ S(x, \sigma(x)|a(x)) \right\}
\]

(11)
subject to the incentive compatibility constraints,

\[ \forall x, a(x) \in \text{argmax}_a V(x, \sigma(x), T(x)|a) \]  

As before, the only difference between the surplus function \( S(x, \sigma(x)|a(x)) \), given by (6), and the payoff function of the lender in the baseline model, \( V(x|a) \) given by (4), is that the surplus contains the weighted average of the securitizer’s and the lender’s discount factor. By substituting in \( 1 - \varepsilon \) for \( \delta \), we rewrite the surplus in terms of the baseline payoff function and an additional \( \varepsilon \sigma(x)\bar{R}x \) term:

\[ S(x, \sigma(x)|a(x)) = \begin{cases} V(x|a(x)) & \text{if } a(x) = D \\ V(x|a(x)) + \varepsilon \sigma(x)\bar{R}x & \text{if } a(x) \in \{A, I\} \end{cases} \]

We assumed that the difference \( \delta - 1 = \varepsilon \) is arbitrarily small. This implies that the securitizer’s preferences are lexicographic, and we can find the solution to (11) in two steps: first, find the max

\[ \sigma(x) \]

compatibility constraints, and second, among that set of contracts, choose the one with the largest \( \sigma(x) \) for each \( x \) (since \( \varepsilon \bar{R}x > 0 \), i.e., there are (small) gains to trade between the lender and securitizer).

Rewriting the problem for the first step, we have:

\[ \max_{\sigma(x), T(x), a(x)} \left\{ V(x|a(x)) \right\} \]

subject to the incentive compatibility constraints, (12).

The maximand in (14) is the same as the maximand in the lender’s unconstrained maximization problem in (5). We now show that the same unconstrained maximum can be achieved in the securitizer’s constrained problem. Recall the lender’s solution to (5), \( a^*(x) \):

\[ a^*(x) = \begin{cases} D & \text{if } x < \bar{x} \\ I & \text{if } \bar{x} \leq x < \bar{x} \\ A & \text{if } \bar{x} \geq \bar{x} \end{cases} \]

For each \( x \), we look for the largest \( \sigma(x) \) for which there exists a \( T(x) \) such that \( a^*(x) \) satisfies the lender’s incentive compatibility constraints under \( \sigma(x) \) and \( T(x) \).

For \( x \geq \bar{x} \), we will show by specific example of \( T(x) \) that \( \sigma^*(x) = 1 \) and \( a^*(x) = A \) can be implemented. Let \( T(x) = \bar{R}x \) (the expected value of the loan) and \( \sigma^*(x) = 1 \). The lender prefers \( a = A \) at these values of \( x \) if and only if \( \bar{R}x - 1 \geq 0 \) and \( \bar{R}x - 1 \geq (\bar{R}x - 1)(1 - (1 - x)s) - c \). The former condition is just the condition that the lender prefers \( a = A \) to \( a = I \) in the no-securitization case. The latter condition is true since we showed in the proof of Proposition 1 that the lender prefers \( a = A \) to \( a = I \) even when he gets a larger expected payment per loan under \( a = I \).

For \( \bar{x} \leq x < \bar{x} \), we will derive an upper bound on \( \sigma(x) \) such that \( a^*(x) = I \) can be implemented. For the lender to prefer \( a = I \) to \( a = D \), we must have \( V(x, \sigma(x), T(x)|I) \geq V(x, \sigma(x), T(x)|D) \), which is true if and only if \( (1 - (1 - x)s)\left(\sigma(x)T(x) + (1 - \sigma(x))\frac{\bar{R}}{1 - (1 - x)s} - 1 \right) - c \geq 0 \), or equivalently,

\[ T(x) \geq \frac{1 - (1 - x)s + c - (1 - \sigma(x))\bar{R}}{\sigma(x)(1 - (1 - x)s)} \equiv \overline{T}(x) \]

There is a lower bound on \( T(x) \) because if the securitizer does not pay enough for the loans it buys, the lender will not be willing to make the loans.
For the lender to prefer \( a = I \) to \( a = A \), we must have \( V(x, \sigma(x), T(x)|I) \geq V(x, \sigma(x), T(x)|A) \), which is true if and only if 
\[
(1 - (1 - x)s) \left( \sigma(x)T(x) + (1 - \sigma(x)) \frac{x}{1-(1-x)s} \bar{R} - 1 \right) - c \geq \sigma(x)T(x) + (1 - \sigma(x))\bar{R}x - 1,
\]
or equivalently,
\[
T(x) \leq \frac{(1 - x)s - c}{\sigma(x)(1 - x)s} \equiv \bar{T}(x).
\]
There is an upper bound on \( T(x) \) because if the securitizer pays too much for the loans it buys, the lender would prefer not to investigate and screen out borrowers and instead would prefer to lend to all of them.

A function \( T(x) \) can implement \( a^*(x) \) and \( \sigma(x) \) if and only if \( T(x) \leq T(x) \leq \bar{T}(x) \). Therefore, for each \( x \), we will maximize \( \sigma(x) \) subject to \( T(x) \leq \bar{T}(x) \). Rearranging \( T(x) \leq \bar{T}(x) \) gives the upper bound \( \sigma(x) \leq \frac{\bar{R} s(1-x) - c}{\bar{R} s(1-x)x} \), so the optimal \( \sigma(x) \) is given by:
\[
\sigma^*(x) = \frac{\bar{R} s(1-x) - c}{\bar{R} s(1-x)x}.
\]
One can check that \( 0 \leq \frac{\bar{R} s(1-x) - c}{\bar{R} s(1-x)x} < 1 \) for \( x \in [\bar{x}, \bar{x}] \).

To find the payment function that supports this equilibrium, we substitute \( \sigma^*(x) \) into \( (16) \) and \( (17) \), which then reduce to \( T(x) = \bar{T}(x) = \frac{\bar{R} s(1-x) - c}{\bar{R} s(1-x)x} \). Hence, in this region of \( x \), the equilibrium payment function is unique.

Finally, for \( x < \bar{x} \), we must have that the lender prefers \( a = D \) to \( a \in \{A, I\} \). For these values of \( x \), no loans are made, so the securitization rate has no effect on the surplus. We can thus set \( \sigma^*(x) = 0 \) and \( T^*(x) = 0 \). Since the lender denies the applicants, it follows immediately that the lender’s incentive compatibility constraints are satisfied with \( \sigma^*(x) = 0 \) and \( T^*(x) = 0 \). □
APPENDIX B

FIGURE 1. Discontinuities in the density of mortgages by credit score

FIGURE 2. Discontinuity in the density of loans
Figure 3. Discontinuity in the default rate of loans

Figure 4. Effect of rule-of-thumb securitizer on screening threshold
FIGURE 5. Discontinuity in the securitization rate of loans

FIGURE 6. Proportion low documentation by FICO. Fitted curves from 6th-order polynomial regression on FICO interval [500,800] without year fixed effects.

FIGURE 8. FICO histogram for conforming loan sample. Fitted curves from 6th-order polynomial regression on FICO interval [500,800] without year fixed effects. Vertical line is at 620 FICO.
Figure 9. FICO histogram for jumbo loan sample. Fitted curves from 6th-order polynomial regression on FICO interval [500,800] without year fixed effects. Vertical line is at 620 FICO.

Figure 10. FICO histogram for low documentation loans 2001-2006. Fitted curves from 6th-order polynomial regression on FICO interval [500,800] without year fixed effects.
Figure 11. Default by FICO for conforming loan sample. Fitted curves from 6th-order polynomial regression on FICO interval [500,800] without year fixed effects.

Figure 12. Default by FICO for jumbo loan sample. Fitted curves from 6th-order polynomial regression on FICO interval [500,800] without year fixed effects.
**Figure 13.** Default by FICO for low documentation loans 2001 - 2006. Fitted curves from 6th-order polynomial regression on FICO interval [500,800] without year fixed effects.

**Figure 14.** Securitization by FICO for conforming sample. Fitted curves from 6th-order polynomial regression on FICO interval [500,800] without year fixed effects.
Figure 15. Securitization by FICO for jumbo sample. Fitted curves from 6th-order polynomial regression on FICO interval [500,800] without year fixed effects.

Figure 16. Securitization by FICO for low documentation loans 2001 - 2006. Fitted curves from 6th-order polynomial regression on FICO interval [500,800] without year fixed effects.
### Table 1. Sample Sizes

<table>
<thead>
<tr>
<th></th>
<th>Total</th>
<th>2003</th>
<th>2004</th>
<th>2005</th>
<th>2006</th>
<th>2007</th>
</tr>
</thead>
<tbody>
<tr>
<td>Conforming</td>
<td>3,843,810</td>
<td>150,965</td>
<td>576,478</td>
<td>1,091,678</td>
<td>1,097,665</td>
<td>927,024</td>
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<tr>
<td>Jumbo</td>
<td>589,352</td>
<td>17,846</td>
<td>111,093</td>
<td>217,406</td>
<td>139,053</td>
<td>103,154</td>
</tr>
<tr>
<td>Low Doc</td>
<td>851,683</td>
<td>50,093</td>
<td>180,245</td>
<td>242,966</td>
<td>219,214</td>
<td>159,165</td>
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### Table 2. Summary Statistics: Conforming and Jumbo Samples

<table>
<thead>
<tr>
<th></th>
<th>Conforming</th>
<th>Jumbo</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>GSE Securitized</strong></td>
<td>.684</td>
<td>.019</td>
</tr>
<tr>
<td><strong>Private Securitized</strong></td>
<td>.216</td>
<td>.700</td>
</tr>
<tr>
<td><strong>Portfolio</strong></td>
<td>.101</td>
<td>.282</td>
</tr>
<tr>
<td><strong>Low Doc</strong></td>
<td>.309</td>
<td>.441</td>
</tr>
<tr>
<td><strong>Adjustable</strong></td>
<td>.272</td>
<td>.687</td>
</tr>
<tr>
<td><strong>Borrower FICO</strong></td>
<td>711.1</td>
<td>728.0</td>
</tr>
<tr>
<td><strong>Loan Amount ($)</strong></td>
<td>194,826</td>
<td>384,217</td>
</tr>
<tr>
<td><strong>Loan-to-Value</strong></td>
<td>79.0</td>
<td>76.0</td>
</tr>
<tr>
<td><strong>Defaulted</strong></td>
<td>.050</td>
<td>.054</td>
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</table>

**Notes:** Low Doc includes both “low” and “no” documentation loans. Loan Amount in 2007 dollars. Defaulted equal to 1 if loan became 61 days or more overdue within 18 months of origination.

### Table 3. Summary Statistics: Low Documentation Sample

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>S.D.</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>GSE Securitized</strong></td>
<td>.584</td>
<td>.493</td>
<td>851,683</td>
</tr>
<tr>
<td><strong>Private Securitized</strong></td>
<td>.263</td>
<td>.440</td>
<td>851,683</td>
</tr>
<tr>
<td><strong>Portfolio</strong></td>
<td>.153</td>
<td>.360</td>
<td>851,683</td>
</tr>
<tr>
<td><strong>Jumbo</strong></td>
<td>.160</td>
<td>.366</td>
<td>851,683</td>
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<td><strong>Adjustable</strong></td>
<td>.411</td>
<td>.492</td>
<td>850,180</td>
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<td><strong>Borrower FICO</strong></td>
<td>709.2</td>
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<tr>
<td><strong>Loan Amount ($)</strong></td>
<td>274,182</td>
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<td>851,683</td>
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<td><strong>Loan-to-Value</strong></td>
<td>78.2</td>
<td>13.6</td>
<td>851,234</td>
</tr>
<tr>
<td><strong>Defaulted</strong></td>
<td>.058</td>
<td>.233</td>
<td>851,683</td>
</tr>
</tbody>
</table>

**Notes:** Low Doc includes both “low” and “no” documentation loans. Loan Amount in 2007 dollars. Defaulted equal to 1 if loan became 61 days or more overdue within 18 months of origination.
### Table 4. Lending and Default Rate Discontinuities at Various FICO Cutoffs

<table>
<thead>
<tr>
<th>FICO Threshold</th>
<th>600</th>
<th>620</th>
<th>660</th>
<th>680</th>
<th>700</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>PANEL A: LOG(FREQUENCY)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Discontinuity</td>
<td>.157***</td>
<td>.448***</td>
<td>.149***</td>
<td>.159***</td>
<td>.113***</td>
</tr>
<tr>
<td>s.e.</td>
<td>(.008)</td>
<td>(.006)</td>
<td>(.005)</td>
<td>(.004)</td>
<td>(.003)</td>
</tr>
<tr>
<td>N</td>
<td>4,433,126</td>
<td>4,433,126</td>
<td>4,433,126</td>
<td>4,433,126</td>
<td>4,433,126</td>
</tr>
<tr>
<td><strong>PANEL B: DEFAULT RATE</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Discontinuity</td>
<td>.013***</td>
<td>.021***</td>
<td>.004***</td>
<td>.006***</td>
<td>.002</td>
</tr>
<tr>
<td>s.e.</td>
<td>(.004)</td>
<td>(.004)</td>
<td>(.002)</td>
<td>(.001)</td>
<td>(.001)</td>
</tr>
<tr>
<td>N</td>
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<td>4,433,126</td>
<td>4,433,126</td>
<td>4,433,126</td>
<td>4,433,126</td>
</tr>
</tbody>
</table>

*Notes:* Data is pooled sample of conforming and jumbo loans. Panel A uses a local linear regression, as outlined in McCrary (2008). Panel B uses a 6th-order polynomial in FICO on either side of the 620 cutoff, with year fixed effects. Heteroskedasticity-robust standard errors in parentheses. (***), (**), (*) significant at 1%, 5%, 10%, respectively.

### Table 5. Discontinuities in Lending Frequency at FICO 620 by Lenders’ Rate of Loan Securitization

<table>
<thead>
<tr>
<th>Quartile Number</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percent Loans Securitized (Range)</td>
<td>.536***</td>
<td>.276***</td>
<td>.255***</td>
<td>.280***</td>
</tr>
<tr>
<td>s.e.</td>
<td>(.026)</td>
<td>(.025)</td>
<td>(.023)</td>
<td>(.015)</td>
</tr>
<tr>
<td>N</td>
<td>233,466</td>
<td>179,525</td>
<td>262,954</td>
<td>294,500</td>
</tr>
</tbody>
</table>

*Notes:* LPS pooled sample merged with Home Mortgage Disclosure Act (HMDA) filings for 2006 and 2007, merging exactly on closing date, origination amount, and zip code/census tract. Lending banks are divided into quartiles according to the percentage of loans they keep in portfolio. Quartiles are not exactly equal in size due to the presence of several large indivisible lenders. All columns use local linear regression, with log frequency as the dependent variable, as outlined in McCrary (2008). Heteroskedasticity-robust standard errors in parentheses. (***), (**), (*) significant at 1%, 5%, 10%, respectively.
<table>
<thead>
<tr>
<th></th>
<th>log(Frequency)</th>
<th>Default</th>
<th>Securitization</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>McCrary</td>
<td>Polynomial</td>
<td>Local Linear</td>
<td>Polynomial</td>
</tr>
<tr>
<td>Discontinuity at 620</td>
<td>.434***</td>
<td>.021***</td>
<td>.014***</td>
</tr>
<tr>
<td>s.e.</td>
<td>(.006)</td>
<td>(.004)</td>
<td>(.004)</td>
</tr>
<tr>
<td>Predicted at 619</td>
<td>-</td>
<td>.142</td>
<td>.146</td>
</tr>
<tr>
<td>N</td>
<td>3,843,810</td>
<td>3,843,810</td>
<td>174,275</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>PANEL B: JUMBO LOANS</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Discontinuity at 620</td>
<td>.681***</td>
<td>.028</td>
<td>.014</td>
</tr>
<tr>
<td>s.e.</td>
<td>(.026)</td>
<td>(.018)</td>
<td>(.016)</td>
</tr>
<tr>
<td>Predicted at 619</td>
<td>-</td>
<td>.190</td>
<td>.193</td>
</tr>
<tr>
<td>N</td>
<td>589,352</td>
<td>589,352</td>
<td>11,061</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>PANEL C: LOW DOC LOANS</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Discontinuity at 620</td>
<td>.628***</td>
<td>.059***</td>
<td>.043***</td>
</tr>
<tr>
<td>s.e.</td>
<td>(.014)</td>
<td>(.009)</td>
<td>(.008)</td>
</tr>
<tr>
<td>Predicted at 619</td>
<td>-</td>
<td>.135</td>
<td>.142</td>
</tr>
<tr>
<td>N</td>
<td>851,683</td>
<td>851,683</td>
<td>38,990</td>
</tr>
</tbody>
</table>

Notes: Column 1 uses a local linear regression, as outlined in McCrary (2008). Columns 2 and 4 use a 6th-order polynomial in FICO on either side of the 620 cutoff. Columns 3 and 5 restrict the data to a local neighborhood [610,629] and fit a line on either side of 620. Columns 2 through 5 contain year fixed effects. Heteroskedasticity-robust standard errors in parentheses. (***), (**), and (*) significant at 1%, 5%, and 10%, respectively.
<table>
<thead>
<tr>
<th>Panel A: Georgia</th>
<th>Law Period</th>
<th>Non-Law Period</th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td>Georgia</td>
<td>.963</td>
<td>.862</td>
<td>.101***</td>
</tr>
<tr>
<td>s.e.</td>
<td>(.005)</td>
<td>(.005)</td>
<td>(.007)</td>
</tr>
<tr>
<td>N</td>
<td>1,276</td>
<td>5,041</td>
<td></td>
</tr>
<tr>
<td>Neighboring states (AL, NC, SC, TN, FL)</td>
<td>.946</td>
<td>.872</td>
<td>.074***</td>
</tr>
<tr>
<td>s.e.</td>
<td>(.004)</td>
<td>(.003)</td>
<td>(.005)</td>
</tr>
<tr>
<td>N</td>
<td>3,074</td>
<td>15,009</td>
<td></td>
</tr>
<tr>
<td>Difference</td>
<td>.017**</td>
<td>-.010*</td>
<td>.027***</td>
</tr>
<tr>
<td>s.e.</td>
<td>(.007)</td>
<td>(.006)</td>
<td>(.009)</td>
</tr>
<tr>
<td>Panel B: New Jersey</td>
<td>Law Period</td>
<td>Non-Law Period</td>
<td>Difference</td>
</tr>
<tr>
<td>New Jersey</td>
<td>.828</td>
<td>.862</td>
<td>-.034***</td>
</tr>
<tr>
<td>s.e.</td>
<td>(.004)</td>
<td>(.002)</td>
<td>(.005)</td>
</tr>
<tr>
<td>N</td>
<td>8,127</td>
<td>22,394</td>
<td></td>
</tr>
<tr>
<td>Neighboring states (NY, PA, DE)</td>
<td>.803</td>
<td>.839</td>
<td>-.036***</td>
</tr>
<tr>
<td>s.e.</td>
<td>(.002)</td>
<td>(.002)</td>
<td>(.003)</td>
</tr>
<tr>
<td>N</td>
<td>18,639</td>
<td>56,913</td>
<td></td>
</tr>
<tr>
<td>Difference</td>
<td>.025***</td>
<td>.023***</td>
<td>.002</td>
</tr>
<tr>
<td>s.e.</td>
<td>(.005)</td>
<td>(.003)</td>
<td>(.006)</td>
</tr>
</tbody>
</table>

Notes: For Georgia, Law Period is equal to 1 if the loan was originated between the start of October 2002 and the end of February 2003. The sample period is six months longer than the Law Period on either end: from April 2002 to August 2003. For New Jersey, Law Period is equal to 1 if the loan was originated between the start of December 2003 and the end of May 2004. The sample period is six months longer than the Law Period on either end: from June 2003 to November 2004. Heteroskedasticity-robust standard errors in parentheses. (***), significant at 1%, (**), significant at 5%, (*), significant at 10%.