Risk Transfer and Foreclosure Law: Evidence from the Securitization Market

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Abstract

We evaluate whether foreclosure law causes lenders to securitize mortgage loans. To establish causality we exploit exogenous variation in foreclosure law along US state borders using a regression discontinuity design. Within a narrow neighborhood of the border lenders are 3.27% more likely to securitize GSE-eligible mortgage loans when subject to borrower-friendly foreclosure law. The effects are present before and after the financial crisis, and imply that borrower-friendly foreclosure law increases taxpayers' holdings of mortgage debt by $140bn per annum. This highlights how lenders use securitization to exploit the GSEs' guarantees and transfer credit default risk.

JEL-Codes: G21, G28, K11.
Keywords: securitization, foreclosure law, risk transfer, mortgage default, GSEs

1 Introduction

The securitization market has grown rapidly over the past two decades and transformed the traditional role of mortgage originators from “buy to hold” to “buy to sell”. Understanding the determinants of securitization typically focuses on the cost of deposit funds (Loutskina and Strahan, 2009), agency costs (Gorton and Pennacchi, 1995), corporate tax rates (Han et al., 2015), and the transformation of illiquid assets into liquid securities (Loutskina, 2011). However, a frequently overlooked benefit of securitization to the lending industry is that it reduces exposure to credit default risk that threatens lenders’ stability (Pennacchi, 1988). In particular, little attention has been paid to how securitization enables lenders to avoid the risk and costs of mortgage default.

Several studies find that mortgage default imposes substantial costs upon lenders in the United States (US): over $50,000 per foreclosed property (Hatcher, 2006; Pence, 2006). When a lender holds a loan on its balance sheet it is liable for these costs. However, securitization allows the lender to transfer the credit default risk to a third party who bears the losses in the event of mortgage default (Greenbaum and Thakor, 1987). Lenders may therefore use securitization as a risk transferring device, and factors that influence the expected costs of mortgage default potentially provoke securitization.

In this paper, we evaluate whether lenders’ securitization decisions respond to the law that regulates foreclosure. Foreclosure laws provide an ideal laboratory for testing this conjecture because they influence the expected costs of default through two channels. First, prior research shows that Judicial Review (JR) law, which prioritizes borrowers’ rights during the foreclosure process, triggers substantially higher rates of mortgage default compared to lender-friendly Power of Sale (PS) law. For example, Demiroglu et al. (2014) estimate the probability of default to be 40% higher in JR relative to PS states. Second, JR law exacerbates the costs of mortgage default by imposing substantially higher legal and administrative expenses upon lenders when repossessing a delinquent property (Gerardi et al., 2013; Demiroglu et al., 2014). It is therefore plausible that lenders’ securitization choices are a function of the law governing the foreclosure process.[1]

[1] JR law mandates that each stage of the foreclosure process is overseen by a judge. The judicial requirement

Using a regression discontinuity design that exploits exogenous differences in foreclosure law along US state borders, we find evidence that such incentives are operative and economically important. Our tests revolve around loan-level securitization data within a 10 mile distance of the border between states that use JR and PS law. Within this narrow neighborhood economic conditions, housing market fundamentals, access to credit, demand for credit, and broader socioeconomic factors are observationally equivalent either side of the threshold (border) but the law regulating foreclosure differs sharply.

We find robust evidence that JR law impacts lenders' securitization decisions. Estimates of the local average treatment effect (LATE) indicate that JR law causes a 3.27% increase in the probability that a mortgage loan is securitized, relative to the counterfactual. The effects are statistically significant at conventional levels and comparable in magnitude across various specifications. The LATE we estimate is present before, and after, the financial crisis. In fact, the economic magnitude is larger for the post-crisis sample suggesting that the Government Sponsored Enterprises' (GSEs) entry into conservatorship neither limited their risk taking nor reduced lenders' propensity to exploit their guarantees.

Further tests using subsamples of the data reinforce our findings. For example, one would anticipate lenders' reaction to JR law to be more pronounced among riskier loans where default is more likely and expected default costs are higher. Indeed, this is the pattern we observe in the data. The probability of securitization is greater among loans originated to low-income borrowers, sole applicants, for loans with high loan-to-income (LTI) ratios, and in areas with above average unemployment and poverty rates.

We also examine which margin of JR law is more important in determining lenders' securitization decisions. Estimates show that JR law triggers securitization by raising lenders' costs of foreclosing a loan and by prolonging the duration of the foreclosure process which creates moral hazard among borrowers by increasing their returns to default. However, the latter effect is considerably more important.

results in lengthy foreclosure process. Lenders incur substantial costs throughout, and must also pay attorney and court fees. In contrast, PS law places a low burden upon lenders. Following default lenders may immediately transfer a property to a trustee for liquidation. The foreclosure process is therefore considerably shorter and lenders bear lower costs.

A series of robustness tests confirm that our findings are not driven by confounding factors. For example, placebo tests show that securitization only increases at the threshold where the laws governing foreclosure actually change. Meanwhile sensitivity checks demonstrate that our inferences are robust to other features of the legal environment, lenders' characteristics, borrowers' credit scores, lenders' pricing decisions, and many other plausible confounds. Diagnostic checks show no discontinuities in other covariates at the threshold. In essence, our findings are not contaminated by omitted variables. This is consistent with evidence reported by Gerardi et al. (2013), Ghent (2014), and Mian et al. (2015) that foreclosure law is exogenous with respect to contemporary financial market conditions as the laws originate from historical accidents during the pre-Civil War period. Further tests rule out that methodological considerations surrounding our research design drive the results.

We evaluate our hypothesis in the agency segment of the securitization market where the GSEs, Fannie Mae and Freddie Mac, pledge to provide liquidity by purchasing GSE-eligible loans. There are three advantages of this setting. First, it provides the cleanest test of our hypothesis. In this one sided market there are no strategic considerations among purchasers: providing a loan is GSE eligible, a GSE must buy any loan a lender sells. Lenders therefore do not face constraints upon risk transfer. Second, the servicer fee GSEs pay lenders for the loans they sell is uniform across US states meaning there are no discontinuities in the monetary incentive to securitize loans either side of the threshold. Third, GSEs are the largest buyers of mortgage loans in the secondary market. Understanding whether lenders exploit the GSEs' guarantees to transfer credit default risk is important as this exposes taxpayers to housing market costs and risks.

In addition, we analyze lenders' securitization behavior in the non-agency market where the GSEs do not operate. Here we find that JR law impacts lenders' securitization decisions quite differently. Specifically, foreclosure law has no effect on the probability that a lender securitizes a loan. This finding is consistent with investors demanding a risk premium to compensate for the additional risk associated with loans from JR states which erodes the risk transferring benefits of securitization.

Our paper bridges two distinct strands of literature. One area of research has documented the effects of foreclosure law on credit supply. Key references include Pence (2006) who finds that JR law causes a reduction in mortgage loan amounts. Dahger and Sun (2016) extend Pence's work by examining whether foreclosure law influences the probability of being granted a mortgage. Our paper complements these studies by illustrating that the effects of foreclosure law on lender behavior extend beyond credit supply responses. Specifically, conditional on origination, a mortgage loan is more likely to be securitized in JR relative to PS states. This suggests that screening alone does not mitigate the expected costs of default and that lenders use risk transfer as a complementary tool. In contrast to these articles, our findings illustrate the large extent to which foreclosure law induces lenders to exploit the GSEs' guarantees and expose taxpayers to volatility and losses in the housing market. So far, these issues have remained neglected.

A separate literature relates securitization to the type of loans that lenders originate. Broadly, these studies focus on whether the originate-to-distribute (OTD) model weakens lenders' incentives to screen borrowers, resulting in more risky lending. For example, Keys et al. (2010, 2012) find that securitization reduced financial intermediaries' incentives to carefully screen borrowers in the years before the subprime crisis. Purnanandam (2010) presents evidence that lenders that were most heavily involved in the OTD market during the pre-crisis period originated disproportionately low-quality mortgages. Our paper differs from these articles by studying what drives securitization behavior. We show that securitization is not simply due to moral hazard within lenders. Rather lenders securitize loans to mitigate credit default risk created by elements of the legal environment.

Our paper also contributes to three lively policy debates. First, US taxpayers' fortunes are now closely intertwined with the housing market (Economist, 2018). Our estimates imply that JR law adds \$140bn, or approximately 1% of GDP, to the GSEs' mortgage debt holdings each year. As the Treasury has run down the GSEs' capital buffers through time, only a small loss is required to render them technically insolvent. Ultimately, changes in house price sentiment could impose enormous losses upon taxpayers who bear the GSEs' losses. By exacerbating taxpayers' exposure to the housing market, JR law amplifies taxpayers' potential losses. Understanding the distortions foreclosure law creates and its fiscal ramifications is important for the design of prudential regulation and initiatives to reform the GSEs.[2]

Second, following the Great Recession and the US Foreclosure Crisis of 2010, which featured widespread improper foreclosures by lenders, policymakers have sought to provide greater guarantees for borrowers. For example, the National Consumer Law Center has called for implementation of JR law across all states (Rao and Walsh, 2009). The Consumer Financial Protection Bureau has also sought to enact legislative changes which protect homeowners during the foreclosure process. The evidence in this paper illustrates some of the potential unintended consequences of prioritizing borrowers' rights upon the financial sector and the GSEs.

Finally, the securitization model has been criticized due to its prominent role in the recent financial crisis. Whereas the potential risks of separating the mortgage originator from the bearer of credit risk are well understood, few studies ask whether securitization is an optimal response to distortions created by the policy environment. This paper provides novel insights on this issue.

The paper proceeds as follows. The following section provides an overview of our data set. We present institutional details in Section 3. We outline the identification strategy in Section 4 and report econometric results in Section 5. Section 6 deals with robustness tests. Finally, we draw conclusions in Section 7.

2 Data

Our data set contains loan-level information drawn from the 2000 to 2016 vintages of the Home Mortgage Disclosure Act (HMDA) database. The HMDA data contain 95% of all mortgages originations in the US. Each observation corresponds to a unique mortgage loan and provides information on the characteristics of the loan, borrower, and lender at the point of origination. For each loan the data report the loan amount, the type of loan (pur-

[2]The estimates do not imply that taxpayers incur annual costs of $140bn due to Judicial Review law. Rather this is how much more mortgage debt the GSEs hold because of JR law.

chase, refinance, home improvement), borrower characteristics (race, gender, applicant's income, whether there is a co-applicant), the originating financial institution, the rate spread of the loan, the lender's decision on the application (acceptance or rejection), and whether the loan is subsequently securitized.[3] Information on the census tract in which the property is located and the type of home (single- or multi-family) is also available.

2.1 Dependent Variable

Our main variable of interest is the securitization indicator. An advantage of the HMDA data is that it reports information on whether a loan is securitized and the identify of the purchaser (either a GSE or private investor). This allows us to classify whether a loan is securitized in the agency (GSE) or non-agency (private investor) market. Accordingly, we construct an Agency Securitization dummy variable equal to 1 if a loan is securitized by a GSE, 0 otherwise, and a Non-Agency Securitization dummy variable that equals 1 for non-agency securitization, 0 otherwise. While the former contains GSE-eligible mortgages, the later mainly consists of subprime and non-GSE-eligible mortgages.[4] Table 1 shows that across all residential mortgages, 94% are GSE-eligible. Approximately 52% of GSE-eligible mortgages are securitized versus 21% of non-GSE-eligible mortgages.

[Insert Table 1]

2.2 Explanatory Variables

The key explanatory variable is a dummy variable that captures the type of foreclosure law used in the state where the property is located. Using information provided by Realty Trac we construct a JR dummy variable that equals 1 if the property is in a JR state, 0 for PS states.[5]

Our empirical strategy relies on a regression discontinuity design (RDD). We therefore construct the assignment variable using the distance between the midpoint of the prop-

[3]The rate spread allows us to identify subprime mortgage applications.

[4]A GSE-eligible mortgage is one with a debt-to-income ratio of 43% or less and a loan term of 30 years or less. Mortgages with negative amortization, interest-only or jumbo loans are not GSE eligible.

[5]Mian et al. (2015) also use this source to define each state's foreclosure law.

erty's census tract and the nearest JR-PS border coordinate.[6] Following convention in the literature the assignment variable takes a negative value for observations in the control group (PS states) and positive values for observations in the treatment group (JR states).

We merge the loan-level data with several additional variables from other sources. For example, we generate dummy variables for whether a state has a right of redemption law and permits deficiency judgments (Ghent and Kudlyak, 2011). We measure access to credit using the number of lender branches per 100 population in each census tract. To capture credit demand we use the number of mortgage applications per 100 population in each census tract. We approximate competition in the local mortgage market using a Herfindahl-Hirschman Index (HHI).[7] In addition, we merge in data on the unemployment rate (Bureau of Economic Analysis), the share of the population living in poverty (US Census), the delinquency rate on automobile and credit card loans (NY Fed), various measures of criminal activity (US Census), the share of the population with a college degree (US Census), an urbanization dummy variable which equals 1 if an observation is from a metropolitan statistical area, 0 otherwise (HMDA), the state corporate tax rate (Tax Foundation), a mortgage brokering restrictiveness index (Pahl, 2007), and homestead and non-homestead bankruptcy exemptions (Corradin et al., 2016). We merge in county-level data on the original loan-to-value (LTV) ratio, and the mean FICO score of borrowers at the point of origination from the Single Family Loan Database (SFLD). The SFLD also provides information on the renegotiation rate in the agency market. That is, the percentage of mortgages that default and successfully renegotiate mortgage terms with the securitizer. We also incorporate census tract-level house prices, mortgage interest rates, arrangement fees, and loan maturity at the point of origination from the Federal Housing Finance Authority (FHFA).

Data on the mean cost to lenders of foreclosure in each state-year is provided by the

[6] As census tracts are geographically small, the census tract midpoint is an accurate approximation of the property's location. We then calculate the great circle distance to the nearest border point.

[7] We define the local market as the county in which a census tract is located. We calculate the HHI index using lenders' market shares where market share is the ratio of the total value of new mortgages originated in a given year by lender l relative to the total value of new mortgages originated by all institutions in the county. The HHI then is calculated as the sum of the squares of the market shares of all financial institutions in each county-year.

SFLD. This variable contains legal costs, property insurance costs, losses due to depreciation in the value of the property, and miscellaneous costs. The US Foreclosure Network database reports information on the mean timeline (the duration of the foreclosure process) in each state-year.

Finally, we merge in bank-level data from Call Reports. For each loan the HMDA data provide an identifier for the originating institution. This identifier is also present in Call Reports provided by the Federal Deposit Insurance Corporation (FDIC).[8] This allows us to incorporate information on the bank's net interest income ratio, capital ratio, bank size (total assets), the cost of deposit funds (measured as the ratio of deposit interest expenses to total deposits), and Z-score.[9] We define whether a bank operates an OTD business model using a dummy variable which equals 1 if it securitizes more than 50% of the loans it originates, 0 otherwise.[10]

2.3 Sampling

To sharpen identification we restrict the sample to observations within a 10 mile distance of the border between states that use different foreclosure laws. As we show below, within this small area economic conditions are highly similar between the treatment and control groups, thereby eliminating omitted variables. To ensure a homogenous sample we include only observations of single-family home purchases. As securitization is only possible following acceptance of a loan, our sample does not contain any rejected loan applications. This results in a sample containing 512,754 observations of which 469,761 observations are GSE-eligible loans. Table 1 provides a summary of the variables in the data set, and data source.

[8] Not all financial institutions that appear in the HMDA data are present in the Call Reports. We therefore only have bank-level variables for 258,084 observations.

[9] The Z-score is calculated at an annual frequency using the equation: $Z_{bt} = (ROA_{bt} + ETA_{bt})/ROASD_{bt}$ where ROA_{bt}, ETA_{bt}, and $ROASD_b$ are return on assets, the ratio of equity to total assets, and the standard deviation of returns on assets over the sample period for bank b, respectively.

[10] We choose this value based on the fact that the average rate of securitization in the sample is approximately 50%.

3 Institutional Details

3.1 Judicial Review, Default and Foreclosure Costs

Foreclosure law governs the process through which creditors attempt to recover the outstanding balance on a loan following mortgage default. Typically, this entails repossessing the delinquent property. 21 US states regulate this process using JR law whereas the remaining 29 states use PS law (see Figure 1). JR law proceeds under the supervision of a court and mandates that lenders present evidence of default and the value of the outstanding debt. A judge then issues a ruling detailing what notices must be provided and oversees the procedure. In contrast, upon default lenders in PS states can immediately begin liquidation of the property by issuing a power-of-sale handled by a trustee (Ghent, 2014).

[Insert Figure 1] [Insert Figure 2]

JR law imposes a higher financial burden upon lenders compared to PS law. Each step of the process requires judicial approval meaning that the foreclosure process is more protracted. For example, Figure 2 shows that for the median state the timeline is between 80-90 days longer in JR states, although the duration can be substantially longer. The greater legal burden means that lenders in JR states incur substantially higher legal expenses through attorney and court fees. Moreover, during the foreclosure process the lender bears property taxes, hazard insurance, other indirect costs, and receives no mortgage payments (Clauretie and Herzog, 1990; Schill, 1991; Gerardi et al., 2013). Delinquent borrowers typically do not make investments to maintain the property because they do not expect to capture the returns to those investments, resulting in lower re-sale values (Melzer, 2017). These costs are increasing in the foreclosure timeline.

[Insert Figure 3]

Owing to these myriad factors, foreclosure law exacerbates the costs of mortgage default borne by lenders.[11] The extent of this increase is substantially larger in JR states

[11] The total costs of mortgage default to lenders comprise two components: 1) generic default costs which are similar across states (Hatcher, 2006; Pence, 2006), and 2) costs arising from foreclosure law.

due to the longer timeline. Figure 3 shows that the median cost incurred by a lender of foreclosing a property is approximately \$7,800 in JR states versus \$5,500 in PS states. However, in many JR states lenders' foreclosure costs exceed \$10,000 per property.

The expected costs of mortgage default also differ across states because foreclosure laws shape borrowers' incentives to default. As delinquent borrowers cease making mortgage payments, they effectively live in their house rent free during the foreclosure period. The returns to default therefore depend on the foreclosure timeline such that borrowers have greater incentives to default in JR states (Gerardi et al., 2013). Indeed, evidence shows that the probability of mortgage default is 40% higher in JR states compared to PS states (Demiroglu et al., 2014). Consistent with this finding, the data in Appendix Figure A1 show a higher rate of mortgage default in JR relative to PS states throughout our sample period.

To formally inspect whether JR law increases lenders' expected costs of default by increasing the probability and cost of mortgage default, we use loan-level information provided by the SFLD database to estimate the equation

$$y_{ist} = \alpha + \beta JR_{st} + \gamma X_{ist} + \varepsilon_{ist}, \tag{1}$$

where y_{ist} is either the foreclosure cost incurred by lenders on mortgage loan i in state s at time t, or mortgage default (measured as a binary dummy variable); JR_{st} is a dummy equal to 1 if state s uses JR law, 0 otherwise; X_{ist} is a vector of controls; ε_{ist} is the error term. To estimate equation (1) we use data from the SFLD.[12]

[Insert Table 2]

Table 2 presents the estimates. In column 1 we report results using a model that excludes the control variables. JR law imposes 48% higher costs on lenders, relative to PS law. Column 2 shows that this result remains economically and statistically significant when we include control variables in the model. Next, we test whether the rate of mortgage default is related to foreclosure law. Consistent with previous evidence (Gerardi et al.,

[12]See Appendix A for a description of the SFLD.

2013; Demiroglu et al., 2014; Mian et al., 2015), columns 3 and 4 of Table 2 show that the probability of default is significantly higher in JR states. Economically, the size of this effect is substantial. Column 4 shows the probability of default is 46% higher in JR relative to PS states.

3.2 The Securitization Market

In a traditional residential mortgage market, financial institutions originate fixed-rate mortgages and hold them on their balance sheet. During the life of a mortgage loan, the same financial institution collects instalments for principal and interest payment from the borrower and deals with delinquency in the event of default. This behavior was common before the 1980s when securitization was in its infancy (Frame and White, 2005).

Today, with the growth of the secondary market, mortgage originators can share part or all of the risks associated with fixed-rate residential mortgage loans with third parties. Lenders frequently securitize their GSE-eligible mortgage loans via agency pass-through pools provided by either Fannie Mae or Freddie Mac. These GSEs dominate the secondary market and account for between 60% to 75% of all mortgage debt. In this agency market segment, Fannie Mae and Freddie Mac pledge to purchase GSE-eligible loans to ensure liquidity.[13]

In contrast, jumbo loans and mortgages that do not meet certain GSE underwriting criteria may either be held in the originator's porfolio or securitized by non-agency securitizers such as financial institutions. The resulting mortgage-backed securities (MBS) carry a guarantee of the timely payment of principal and interest for the originator, and the originator can decide to either keep the MBS on its balance sheet or sell it. Post securitization, the originator collects payments from borrowers and transfers the collected monies to the securitizer after keeping a servicing fee.

The agency and non-agency segments of the securitization market differ in an important respect. Whereas the GSEs provide guarantees to purchase GSE-eligible loans, non-agency purchasers do not. Non-agency securitization entails contracting frictions be-

[13] Another GSE, Ginnie Mae, mainly purchases mortgage loans that are insured by either the Federal Housing Association or Veterans Association. These loans account for a small share of originations.

tween originators and purchasers as purchasers must evaluate credit default risk which they face in the event of default. Hence, in this market segment purchasers also have incentives to avoid credit default risk as they do not benefit from implicit federal guarantees for financial obligations like the GSEs.

4 Identification Strategy and Diagnostic Tests

Our econometric strategy utilizes a parametric RDD (Hahn et al., 2001; Lee, 2008; Imbens and Lemieux, 2008). We estimate

$$s_{ilrst} = \beta JR_{ilrst} + \gamma f(X_{ilrst}) + \varphi W_{ilrst} + \delta_{lt} + \delta_{rt} + \varepsilon_{ilrst}, \tag{2}$$

where s_{ilrst} is a dummy variable equal to 1 if loan i originated by lender l in region r of state s is securitized at time t in the agency market, 0 otherwise; JR_{ilrst} defines treatment status and is equal to 1 if an observation comes from a JR state, 0 for PS states; $f(X_{ilrst})$ contains first-order polynomial expressions of the assignment variable and an interaction between JR_{ilrst} and the assignment variable; W_{ilrst} is a vector of control variables; ε_{ilrst} is the error term.

Equation (2) includes lender-year fixed effects (δ_{lt}). These capture all time-varying, lender specific factors such as changes to lenders' risk preferences, managerial quality, or business models that may change over time and impact securitization decisions. Lender-year fixed effects also purge time-varying shocks to regulation across different types of lenders. For example, non-deposit taking institutions are regulated at the state level whereas domestic banks with national charters and foreign banks are regulated by the OCC and state chartered banks are supervised by the state regulator in conjunction with the FDIC or Federal Reserve. Including lender-year fixed effects therefore greatly limits potential confounds.

[Insert Figure 4]

In addition, we include region-year fixed effects (δ_{rt}) in equation (2). We define a region as an area 20 miles long by 10 miles wide that overlaps the threshold. As an example, Figure 4 illustrates the regions along a section of the Arkansas-Louisiana border. The region-year fixed effects eliminate time-varying local and aggregate unobserved heterogeneity and also sharpen identification. Specifically, the LATE is computed by comparing securitization behavior to the left and right of the threshold within the same region in the same year. As the source of identification is confined to such small, economically homogeneous areas at the same point in time, it is unlikely omitted variables drive our inferences.

4.1 Exogeneity

Critical to our identification strategy is the exogeneity of foreclosure law. Ghent (2014) reports that foreclosure law is exogenous with respect to contemporary economic conditions and lenders' behavior because most most states' foreclosure law was determined by idiosyncratic factors during the pre-Civil War period. For example, the original 13 states inherited JR law from England. PS law developed during the early eighteenth century in response to British lenders asking courts to agree to a sale-in-lieu of foreclosure (Ghent, 2014). As the laws governing foreclosure were determined in case law they have largely endured to the present day. This is because once there is precedent, the rules regarding the procedure a lender must follow rarely change substantially. Indeed, Ghent (2014) is explicit in her assessment, stating,

"Given the extremely early date at which I find that foreclosure procedures were established, it is safe to treat differences in some state mortgage laws, at least at present, as exogenous, which may provide economists with a useful instrument for studying the effect of differences in creditor rights."

Other recent papers that treat foreclosure law as exogenous with respect to lender behavior and contemporary economic matters include Pence (2006), Gerardi et al. (2013), and Mian et al. (2015).[14]

[14]There are no changes to state foreclosure laws during the sample period.

4.2 Diagnostic Checks

While treatment status is exogenous with respect to securitization in equation (2), the validity of our econometric strategy rests upon two identifying assumptions. First, all other factors that affect securitization must be continuous functions across the threshold. That is, economic conditions within the treatment and control groups must not systematically differ. If this assumption is violated, estimates of β will capture both the effect of JR law as well as the discontinuous factor leading to biased estimates.

Following convention in the literature, Table 3 presents t-tests that inspect whether the balanced covariates assumption holds in our data. In no instance are there significant differences between the treatment and control groups. For example, the income, gender, and ethnic background of applicants are not significantly different either side of the threshold. The original LTV ratio, interest rate, debt-to-income ratio, and FICO score are also strongly similar. In addition, there are no significant differences in macroeconomic conditions (unemployment rate, poverty), house prices, access to credit (lenders per 100 population), urbanization, criminal activity, educational attainment, competition between lenders (measured using the HHI), the probability of renegotiating a delinquent loan, and the ethnic composition of the population. We find no significant differences in the type of lenders either side of the threshold. Specifically, non-banks account for a similar share of mortgage originations. Finally, there is no significant difference in banks' Z-score, capital ratio, or the incidence of out-of-state lending between JR and PS states. These results indicate that lenders' risk preferences are similar across the threshold. The assumption of balanced covariates therefore holds.

[Insert Table 3] [Insert Table 4]

The second assumption is that neither borrowers nor lenders manipulate treatment status. For example, if risky borrowers strategically decide to purchase properties in JR states our estimates will be biased due to the composition of borrowers rather than the impact of foreclosure law. We therefore follow best practice using McCrary (2008)'s test for strategic manipulation by estimating whether the density of mortgage applications and lender branches per 100 population are continuous functions of the threshold. Manipulation by borrowers (lenders) would be consistent with a higher application density within JR (PS) states. We estimate the equation

$$y_{cst} = \alpha + \varphi JR_{cst} + \phi X_{cst} + \gamma_t + \varepsilon_{cst}, \tag{3}$$

where y_{cst} is either the number of mortgage applications or bank branches per capita within census tract c in state s at time t; JR_{cst} is equal to 1 if an observation is from a JR state, 0 otherwise; X_{cst} is a vector of control variables; γ_t are year fixed effects; ε_{cst} is the error term.[15]

The results of this test are presented in Table 4. We find no evidence of strategic manipulation by either borrowers or lenders. Specifically, estimates of φ are statistically insignificant throughout all columns of Table 4 irrespective of whether we include control variables, or estimate equation (3) parametrically or non-parametrically.

To provide further tests of whether borrowers manipulate treatment status we examine net migration flows between US counties. Manipulation would be consistent with significant inflows into JR counties. We therefore use data on bilateral net migration between US counties from 2005 to 2015 provided by the US Census Bureau. We estimate

$$m_{cjt} = \alpha + \beta JR_{ct} + \varepsilon_{cjt} \tag{4}$$

where m_{cjt} is the net flow of migrants per 1,000 population into county c from county j during year t; JR_c is a dummy variable equal to 1 if county c is in a JR state, 0 otherwise; ε_{cjt} is the error term. In column 7 of Table 4 we find no significant differences in net migration rates between JR and PS counties. Hence, individuals do not systematically migrate to areas with JR to manipulate treatment status.

An alternative means of inferring whether borrowers manipulate treatment status is to examine population growth rates. One would expect faster growth rates in JR regions if

[15] We conduct these tests at the census tract level because we require information on the rate of applications or the density of branches. Estimates of equation (3) do not include census tract fixed effects as these are perfectly collinear with the JR dummy variable.

manipulation is present. Using annual county-level population growth rates between 2000 and 2015 we estimate

$$p_{ct} = \alpha + \beta JR_c + \varepsilon_{ct} \tag{5}$$

where p_{ct} is the annual population growth rate in county c during year t; JR_c is a dummy variable equal to 1 if county c is in a JR state, 0 otherwise; ε_{ct} is the error term. We again find no evidence of strategic manipulation. The JR coefficient in column 8 of Table 4 is statistically insignificant.

5 Empirical Analysis

We begin by examining securitization patterns in the raw data at the JR-PS threshold using non-parametric methods. We group the loan-level data into 0.4 mile wide bins and fit local regression functions to the data on the left and right of the threshold.[16] Figure 5 provides clear evidence of a discontinuity in Agency securitization at the threshold within the agency market. Consistent with our hypothesis, we find that JR law causes a jump in the share of mortgages that are securitized relative to the counterfactual. Figures A2 and A3 in the Appendix show this pattern holds both before and after the financial crisis.

[Insert Figure 5] [Insert Table 5]

5.1 Securitization in the Agency Market

To pin down precise estimates of the LATE we turn to regression analysis. Table 5 reports estimates of equation (2). Column 1 presents the results of an unconditional specification that includes only the JR indicator and the assignment variable. The LATE is estimated to be 0.0228 and is statistically significant at the 1% level. Appending the model with region-year and lender-year fixed effects yields similar results. In column 2 of Table 5 the JR coefficient is estimated to be 0.0167 and is highly significant. Economically, this implies that JR law causes a 3.27% increase in the probability that a mortgage loan is

[16] We calculate optimal bin width following Lee and Lemieux (2010). The local regressions use a rectangular kernel. The results are similar when we fit the local polynomial regressions using half and twice the optimal bandwidth.

securitized, relative to the counterfactual. The evidence is consistent with JR law leading lenders to transfer risk to the GSEs.[17]

RDD estimates can be sensitive to the choice of the assignment variable's functional form. In column 2 the assignment variable coefficient is estimated to be statistically insignificant, implying that the local regression function is flat. This corroborates the patterns in Figure 5. In column 3 of Table 5 we allow the slope of the regression function to vary either side of the threshold by interacting the JR dummy and assignment variable. This has little effect on the magnitude or statistical significance of the LATE.[18]

Roberts and Whited (2013) highlight that if a treatment is exogenous, the magnitude of the LATE should be invariant to the inclusion of important control variables in the empirical model. We therefore append equation 2 with a vector of controls and present the estimates in column 4 of Table 5. The JR coefficient remains highly statistically significant and is equal to 0.0168. Given how similar the estimates of the LATE are between column 2 and 5 of the table, this adds further credibility to the view that foreclosure laws are exogenous with respect to securitization: they do not systematically correlate with borrower characteristics or elements of the external operating environment.

Among the control variables we find that applicant income is negatively related to securitization. We estimate that loans to male borrowers are approximately 1.63% more likely to be securitized. Securitization is positively associated with borrowers' ethnic background: loans made to borrowers from an ethnic minority are 3.99% more likely to be securitized. Within our sample 10 miles either side of the border, we find that securitization is unrelated to the original LTV ratio, house prices, and the number of lenders per capita.

In columns 5 and 6 of Table 5 we explore whether the effect of JR law on securitization

[17] To calculate the treatment effect relative to the counterfactual we compare the LATE to the mean rate of securitization within the control group which is 51%. Hence, (0.0167/0.51)*100 = 3.27%.

[18] So far we have fitted linear regression functions to the data. We therefore present estimates using second, third and fourth order polynomial expressions of the assignment variable and interactions between these variables and the JR dummy in Appendix Table A1. The results are very similar to the baseline estimates. Another approach to this issue is to estimate equation (2) non-parametrically, thereby removing discretion about the nature of the functional form. The results of this test are provided in column 4 of Appendix Table A1. Again, we obtain very similar inferences to before. The JR coefficient is similar in economic and statistical significance compared to the parametric results.

differs before and after the financial crisis. The GSEs were placed into conservatorship in 2008 which may affect their risk preferences. However, this does not appear to be the case. We find similar effects of JR law on the probability of securitization within both the pre- and post-crisis samples. This is unsurprising given the GSE guarantees did not change following conservatorship.

Regression discontinuity estimates invariably involve a trade-off between precision and efficiency. We therefore check whether our findings are sensitive to the choice of bandwidth. We restrict the sample to observations within 5 miles of the threshold. An advantage of this approach is that within this smaller area socioeconomic conditions are arguably more likely to be observationally equivalent either side of the threshold. The JR coefficient reported in column 7 of Table 5 remains positive and statistically significant at the 1% level.[19]

5.2 Expected Costs of Default Mechanism

Underpinning our tests is the idea that lenders use securitization to transfer the risk and expected costs of mortgage default to the GSEs. We therefore conduct a series of subsample tests to validate this hypothesis. Intuitively, the effects of JR law on securitization should be more pronounced within samples comprising riskier borrowers where JR law has the largest effect on the incentive to default.

[Insert Table 6]

We begin by splitting the sample based on borrowers' income levels and report the results in columns 1 and 2 of Table 6. Consistent with our hypothesis, we find that the probability of securitization is almost 30% larger for applications by borrowers with income below the mean compared to those with income greater than or equal to the mean.

[19] In Appendix A2 we test the sensitivity of the findings to alternative estimators. Column 1 of Table A2 reports estimates of equation (2) using a logit model. We continue to find JR law causes a significant increase in the probability of securitization. Column 2 of Table A2 presents the results of a multinomial logit model. That is, we allow for the possibility that a GSE-eligible loan is not securitized, or sold to either a GSE or non-GSE. We continue to find that JR law significantly increases the probability that a loan is securitized by sale to a GSE.

Next, we split the sample according to whether a mortgage application has a co-applicant. Loans to borrowers with co-applicants are potentially less prone to default because two income streams help smooth negative economic shocks. This is indeed what we find. In column 3 of Table 6 we estimate the LATE to be 0.0171 (t-statistic = 3.55) for the sample comprising applications with a co-applicant. In contrast, the LATE is 0.0178 (t-statistic = 4.01) in column 4 for the sole applicant subsample.

The probability of default increases with the LTI ratio. At high values borrowers are more susceptible to shocks that compromise their ability to meet mortgage payments. Consistent with this hypothesis, the probability that a loan is securitized is larger for loans with above compared to below the mean LTI ratio.

We split the sample based on socioeconomic conditions in the area where the applicant's house is located. In columns 7 and 8 of Table 6 we find that the probability of securitization in response to JR law is substantially larger for loans originated to borrowers who live in high relative to low unemployment areas. We obtain similar results in columns 9 and 10 of the table when we split the sample based on the poverty rate.[20]

Our last test follows the approach used by Agarwal et al. (2012) to calculate the predicted probability of default for each loan. We then split the sample according to whether the probability of default lies above or below the mean. The results in Appendix Table A3 show that the JR coefficient is positive and statistically significant in both subsamples. However, the LATE is 21% larger for loans with default probabilities above the mean.

5.3 Which Channel Matters Most?

So far, the empirical findings demonstrate that JR law affects securitization because it increases lenders' expected costs of default. Our next set of tests establish whether this effect is due to JR law increasing lenders' legal costs during the foreclosure process or because JR law increases borrowers' default incentives. Resolving these questions is essential

[20]Throughout Table 6 we use Chow tests to examine whether there are significant differences between the magnitude of the LATE in the sample splits. In all instances we reject the null of equality, indicating that the JR coefficient is larger in the riskier samples.

for the design of policy.

[Insert Table 7]

The identifying assumption in these tests is that legal costs and foreclosure timelines vary exogenously. This seems plausible as they are functions of exogenous foreclosure law. To enable comparability of economic magnitudes we use standardized legal cost and timeline variables. Column 1 in Table 7 shows a standard deviation increase in lenders' legal costs during foreclosure leads to a statistically significant 1.15% increase in the probability that a mortgage loan is securitized. In column 2 of the table we find a significant positive relationship between the foreclosure timeline and securitization. A standard deviation increase in the timeline increases the probability a loan is securitized by 0.93%.

To ascertain which channel dominates, we include both the legal cost and timeline variables simultaneously in the model and report the estimates in column 3 of Table 7. The legal cost coefficient equates to a 0.65% (t-statistic = 2.21) increase in the probability of securitization whereas the timeline coefficient is 0.76% (t-statistic = 4.69). Hence, while JR law affects securitization activity through both channels, its effect is primarily transmitted through creating incentives for borrowers to default. This suggests that efforts to tackle lenders' exploitation of GSE guarantees to avoid the expected costs of default created by JR law should focus on initiatives that speed up court procedures and shorten the foreclosure process.

5.4 Securitization in the Non-Agency Market

Lenders may also sell loans in the secondary market to private investors. Unlike the GSEs, private investors are not supported by government guarantees, may demand risk premiums, and potentially use screening technologies to identify riskier loans. Lenders' securitization decisions may therefore respond differently to foreclosure law in the non-agency market segment.

[Insert Table 8]

We begin by repeating our analysis by estimating equation (2) with the sole modification that the dependent variable is the Non-Agency Securitization dummy variable. In addition, we restrict the sample to Jumbo loans. In contrast to before, the LATE in column 1 of Table 8 is close to zero and statistically insignificant. This is also the case in column 2 and 3 when we split the sample into the pre- and post-crisis periods. We find very similar results in columns 4 to 6 of Table 8 when the sample comprises only subprime loans.

The insignificant relationship between JR law and securitization in the non-agency market is perhaps unsurprising. Evidence shows that institutional investors are aware of the state in which a property is located and incorporate this information into their pricing decisions. Hence, because purchasers are aware of the higher default rate on loans from JR regions, they demand a risk premium in compensation. As the higher cost of default is priced into non-agency securitization contracts, this erodes the risk transferring benefits of securitization to lenders such that they are indifferent between holding the loans in their portfolio and securitizing them.

In Appendix Table A4 we investigate how the probability of non-agency securitization is related to JR law in the agency market. That is, sales of GSE-eligible loans to private investors. In contrast to before, we find a significant negative relationship and the LATE is also close to zero throughout the table. The most obvious way to rationalize the negative relationship is that the GSEs' guarantee to purchase GSE-eligible loans crowds out private investors. When faced with a choice of securitizing a GSE-eligible loan lenders prefer to sell to GSEs rather than investors who demand a risk premium for holding loans from JR states that are more liable to default. Hence, lenders can transfer risk by exploiting the GSEs' guarantees whereas this is not possible if the purchaser is an investor.[21]

[21] Alternatively, one could argue that private investors believe that the recovery rate on delinquent securitized mortgage loans is low because servicers have weak renegotiation incentives. Private investors' expected returns are therefore lower on JR loans because they default at a higher frequency compared to PS loans. This reduces private investors' demand and the rate of securitization among loans from JR regions (Milonas, 2017). If this is the case, controlling for the renegotiation rate on mortgages in default should render the JR coefficient statistically insignificant. The results in column 3 of Appendix Table A4 show this is not the case.

5.5 Alternative Explanations: Loan Pricing and Borrower Quality

A natural question is whether lenders use price differentiation in response to JR law. Lenders could set higher interest rates to mitigate the expected costs of default. However, this may accentuate the risk of default by making loans less affordable, or lead to adverse selection (Stiglitz and Weiss, 1981).

Table 9 presents estimates of equation (2) with loan pricing controls. The JR coefficient in column 1 and 2 of the table remains positive and highly statistically significant when we control for the interest rate and effective interest rate, respectively. Alternatively, lenders may seek to offset the expected cost of default by charging borrowers higher arrangement fees or shortening maturities. The evidence in columns 3 and 4 of Table 9 show these factors do not confound our inferences.

[Insert Table 9]

Another way to examine the pricing hypothesis is to inspect how lenders' securitization decisions respond to JR law across competitive environments. In competitive markets the high elasticity of substitution between lenders prevents them from pricing loans differently across the threshold. JR law should therefore have a larger effect on securitization in more competitive markets where lenders are less able to offset expected default costs by raising interest rates. Consistent with this intuition, column 5 of Table 9 shows that increasing the HHI index (that is, as competition decreases), reduces the probability that lenders securitize. However, the JR effect is robust.

Alternatively, the effect of JR law on securitization may derive from the quality of borrowers. In the remainder of Table 9 we report estimates of equation (2) that control for borrowers' FICO score, debt-to-income ratio, and the mortgage insurance percentage. Across columns 6 to 8 the JR coefficient estimate is similar to the baseline results. Consistent with Agarwal et al. (2012) we find that lenders are significantly less likely to securitize loans to borrowers with high credit scores.

6 Robustness Checks

In this section, we conduct a host of robustness tests to ensure our findings are not contaminated by omitted variables.

6.1 Placebo Tests

A concern is that the relationship between securitization and foreclosure is fundamentally discontinuous at the threshold due to jumps in other factors. Placebo tests provide another means of inferring whether JR law drives the securitization behavior we observe in the data. Specifically, in samples where foreclosure law is continuous across the threshold, we should not observe discontinuities in securitization. We therefore estimate the equation

$$s_{ilrst} = \alpha_{rt} + \beta Placebo_{ilrst} + \gamma f(X_{ilrst}) + \varphi W_{ilrst} + \delta_t + \varepsilon_{ilrst}, \tag{6}$$

where all variables are the same as in equation (2) except $Placebo_{ilrst}$ which is a dummy variable equal to 1 on the right of the placebo threshold, 0 on the left of the placebo threshold; and X_{ilrst} is the distance to the placebo threshold.

We first estimate equation (6) using observations within 10 miles of a placebo threshold located 10 miles to the left of the actual threshold. At this point PS law governs the foreclosure process on both sides. The result reported in column 1 of Table 10 show that the placebo coefficient is economically close to zero and statistically insignificant. In column 2 of the table we repeat the procedure using observations within 10 miles of a placebo threshold 10 miles to the right of the actual threshold. That is, JR law regulates the foreclosure process either side of the placebo threshold. Again the placebo coefficient is statistically insignificant.

[Insert Table 10]

Another way to implement this placebo test is to estimate equation (6) using samples drawn around the border between states that use the same foreclosure laws. Column 3 of Table 10 presents the results of a placebo test using a sample 10 miles either side of the border where JR law is used on both sides. Again, the placebo coefficient is statistically insignificant. Repeating this test using borders where PS law governs foreclosure either side of the threshold again yields statistically insignificant placebo coefficients (column 4). These tests affirm that our baseline estimates do not simply capture border effects, other aspects of the legal environment, or political economy considerations.

If an omitted variable drives our previous findings, the placebo LATEs should be similar in magnitude and statistical significance as in the baseline estimates. Throughout Table 10 this is not the case. That securitization rates only jump at the actual threshold where there exist discontinuities in the laws governing foreclosure reinforces our argument that the effects we observe are not driven by observable or unobservable omitted variables.

6.2 The Legal Environment

The next set of tests focus on whether other aspects of the legal environment confound our inferences. For example, some states have right of redemption (ROR) law which grant borrowers the right to redeem their property for 12 months after foreclosure, potentially imposing further costs on lenders. Column 1 of Table 11 presents estimates of equation (2) with an additional control for whether a state has a ROR law. Our key finding remains robust.

[Insert Table 11]

Some states permit deficiency judgments. That is, if a mortgage foreclosure sale does not produce sufficient funds to cover the unpaid debt, lenders may obtain the outstanding balance from borrowers' future income. Deficiency judgments therefore potentially reduce the costs of default. Consistent with this view, the evidence in column 2 of Table 11 shows that despite controlling for deficiency judgments, the effect of JR law on securitization is preserved.

Prior research has documented that mortgage default is related to bankruptcy exemptions which allow individuals to discharge their debts but maintain wealth up to the exemption limit. Homestead exemptions are the most important bankruptcy exemption and evidence shows that mortgage default is more likely the more generous are homestead exemptions (Lin, 2001). Non-homestead exemptions allow individuals to maintain wealth in other asset categories but tend to be set at low levels.[22] We therefore append equation (2) with controls for the level of homestead and non-homestead exemptions in each state and present the results in column 3 of Table 11. The effect of JR law on securitization is insensitive to this change. However, we find that securitization is significantly increasing in the homestead exemption level. This is consistent with the higher likelihood of default in states with generous homestead exemptions. The non-homestead exemption coefficient is statistically insignificant.

Recent evidence suggests that deregulation of mortgage brokering reduced screening of loan applications leading to riskier lending (Shi and Zhang, 2018). For example, mortgage brokers are not exposed to the cost of default but their profits are increasing in the number of loan applications they process. The lifting of restrictions on broker services creates moral hazard and exposes lenders to riskier borrowers. We therefore add the state-level broker restrictiveness index as a further control to the estimating equation and report the estimates in column 4 of Table 11. The key finding remains intact.

Next, we estimate equation (2) using a sample that excludes Delaware and Pennsylvania. Both states offer a creditor-friendly form of JR law known as *scire facias*. This differs from other forms of JR law in that the onus is on the borrower to provide a reason why the lender should not be able to foreclose (Ghent, 2014). JR law in these states is therefore somewhat different compared to that used by other JR states in our sample. The results in column 5 of Table 11 are robust to the change in sample.

The Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA) made it more difficult for individuals to declare bankruptcy. This potentially reduced the incentive to default upon mortgage payments. It seems implausible that the BAPCPA drives our inferences as it is a federal law that applies across the threshold and is captured by the region-year fixed effects. Nevertheless, we report estimates using a sample that

[22]The mean homestead exemption across US states is \$122,754. In contrast, the average automobile, other property (clothing, jewellery and tools), and wildcard (miscellaneous possessions) exemptions are \$4,252, \$12,074, and \$3,359, respectively.

excludes the years 2005 to 2016 from the sample in column 6 of Table 11. The effect of JR law on securitization is very similar to before.

The Dodd-Frank Act of 2010 implemented a number of changes that strengthened regulation of financial intermediaries. As before, this was a federal law and therefore applies to both sides of the threshold. Nevertheless, the results in column 7 of Table 11 demonstrate that excluding observations from the years 2010 to 2016 from the sample when the law was in force has no bearing on our inferences.

Appendix Table A5 presents further legal robustness tests. We test the sensitivity of our findings to 1) excluding Louisiana from the sample on the grounds that it is the only Civil Law state, 2) excluding Texas as it is the only state that limits the loan-to-value ratio of mortgages to 80%, and 3) excluding Massachusetts which undertakes reforms to speed up foreclosure timelines during the sample period (Gerardi et al., 2013). Across columns 1 to 3 of Table A5, the JR law coefficient remains positive and statistically significant despite these changes.

6.3 Lending Industry Conditions

Approximately half of the observations in our sample are loans originated by depository institutions (banks) with the remainder originated by non-depository institutions (non-banks). Non-banks typically rely upon short-term wholesale market funding and are therefore more likely to securitize loans to ensure repayment. To avoid that our findings simply reflect a higher concentration of different lender types either side of the threshold, we split the sample and estimate equation (2) using non-banks and banks separately. The results in columns 1 and 2 of Table 12 show that JR law has a positive and highly statistically significant effect on the probability that a loan is securitized within both sub-samples.

[Insert Table 12]

To further sharpen the identification strategy we restrict the sample to loans originated by banks lend on either side of the threshold within the same region. This ensures organizational factors are held constant as we exploit variation in a lenders' response to JR law across the threshold within the same region-year dimension of the data. The results of this test are reported in column 3 of Table 12. The findings remain very similar.

So far, we have used lender-year fixed effects to eliminate omitted variables. In column 4 of Table 12 we use lender-level control variables to capture unobserved heterogeneity instead.[23] JR law continues to exhibit a strong positive effect on the probability of securitization. The control variables are intuitively signed. For example, large banks, those with higher Z-scores (that is, further from default), and well capitalized banks are more likely to securitize loans. The net interest income ratio and cost of deposit funds are inversely related to securitization.

For banks, cross-border lending is permitted. A state regulator may be more lenient on out-of-state activities compared to lending at home (Ongena et al., 2013). This may pose an econometric problem if the PS state is more often the home state and the regulator dislikes the OTD model at home. The estimates in column 5 of Table 12 allay these concerns. Specifically, our inferences are robust to controlling for whether a loan is originated outside a bank's home state.

Previously, we highlighted that banks with different charters are subject to different regulators. To ensure regulatory differences do not confound our results, we split the sample and focus on state chartered and national chartered banks separately. Column 6 and 7 of Table 12 report the estimates for the sample using state chartered and national chartered banks, respectively. The JR law coefficient is positive and highly statistically significant in each column.

Geographic diversification may affect banks' ability to attract deposit funds and influence their securitization decisions. We therefore constrain the sample to loans originated by banks that operate in only one state and report the estimates in column 8 of Table 12. Our key finding is preserved. Similarly, when we focus exclusively on loans originated by multi-state banks in column 9 JR laws remains an important determinant of securitization.

Next, we check whether the nature of banks' business models drives our results. A concern is that banks operating OTD models are highly dependent on selling loans. If such

[23] We do not have information on non-bank lenders' characteristics so focus exclusively on banks.

institutions are disproportionately clustered on the JR side of the threshold, our estimates will conflate banks' business models with the effect of JR law. To address this concern we focus exclusively on banks that do not operate an OTD model, defined as banks that securitize less than 50% of the mortgage loans they originate. The results in column 10 of Table 12 are very similar to before.

6.4 Miscellaneous Sensitivity Checks

A danger is that there exist discontinuities in the incentive to default on other types of debt at the threshold such that the estimates simply capture the riskiness of the population that live in border areas. Although this appears implausible, we append the empirical model with controls for the delinquency rate on automobile and credit card debt. The effect of JR law in column 1 of Table A6 remains robust.

Next, we consider whether our findings are driven by differences in the volume of loans originated. If lenders simply originate more mortgage loans in JR jurisdictions it seems plausible that the probability of securitization is also higher owing to deposit funding constraints. We find that controlling for the tract loan volume does not invalidate our inferences in column 2 of Table A6.

Our estimates are based on comparisons of loans either side of the threshold within small 20 x 10 mile regions. To ensure our findings are not a product of urbanization patterns within regions, we split the sample according to whether an observation is from an urban area. The results in columns 3 and 4 of Table A6 are very similar to before.

The next test focuses on the issue of renegotiation. The propensity to securitize a loan may be higher in JR states simply because the likelihood of successfully renegotiating with borrowers that default is lower (Piskorski et al., 2010; Agarwal et al., 2011) . The estimates in column 5 of Table A6 show that renegotiation does not drive our inferences. Interestingly, the renegotiation rate is negatively related to securitization. This is consistent with successful renegotiation reducing the expected costs of mortgage default.

Finally, Han et al. (2015) show that corporate tax rates create securitization incentives. The results in column 6 of Table A6 demonstrate that the LATE is robust to controlling for state-level corporate tax rates.

7 Conclusion

We show that, in markets where JR law governs the foreclosure process, lenders exhibit an excessive propensity to securitize mortgage loans to shift the risk and costs of mortgage default to the GSEs. We provide evidence supporting this mechanism using data from the US. Baseline estimates show that JR law causes a 3.27% increase in the probability a lender securitizes a loan. The magnitude of the JR effect size is considerably larger among samples of riskier borrowers where default is more likely to occur.

These findings have important policy implications. JR law is designed to protect borrowers from unscrupulous lenders. Our findings suggest that these measures have the unintended consequence of raising US taxpayers' exposure to the housing market. In fact, JR law causes a $140bn increase in Fannie Mae and Freddie Mac's mortgage debt holdings, each year. By expanding the GSEs' mortgage debt holdings, JR law amplifies the losses taxpayers incur due to negative shocks that limit borrowers' ability to repay.

Tackling this issue may involve reforming the GSEs' practices, guarantees, and introducing private capitalization. However, our evidence demonstrates that addressing elements of the legal environment also warrant attention. As JR law primarily affects securitization by increasing the foreclosure timeline, policy interventions aiming to improve the speed of judicial procedures may help limit the extent to which lenders exploit the GSEs' guarantees.

During the US Foreclosure Crisis of 2010, 4 million homes were improperly foreclosed. Recent policy initiatives seek to address this issue by extending greater protections to borrowers. An important insight of our research is the trade-offs this involves. Protecting borrowers' rights leads to higher expected costs of default, and these costs are largely borne by taxpayers.

The mechanism highlighted in this paper has bearings beyond the context of the housing market. In particular, it has implications for risk shifting behavior in any secondary market. There may be other laws that exacerbate the expected costs of default and affect securitization behavior to a larger degree. Exploring other areas in which lenders share the risk of default with third parties is an exciting avenue for future research.

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Tables

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max	Observations	Source
Agency Securitization (Dummy)	0.5203	0.4996	0.0000	1.0000	512,754	HMDA
Judicial review (Dummy)	0.4544	0.4979	0.0000	1.0000	512,754	Realtytrac
Distance to Border (Miles)	-0.2954	4.8563	-9.9978	10.0000	512,754	HMDA and Census Geography
GSE-eligible (Dummy)	0.9379	0.2413	0.0000	1.0000	512,754	HMDA
Jumbo (Dummy)	0.0621	0.2413	0.0000	1.0000	512,754	HMDA
Rate spread (%)	0.3810	1.4155	0.0000	26.0400	512,754	HMDA
Loan Amount (Ln)	11.3442	1.2385	6.9077	15.9903	512,754	HMDA
Non-agency securitization (Dummy)	0.2083	0.4061	0.0000	1.0000	512,754	HMDA
Right of redemption (Dummy)	0.6071	0.4884	0.0000	1.0000	512,754	Ghent and Kudlyak (2011)
Deficiency judgement (Dummy)	0.9201	0.2711	0.0000	1.0000	512,754	Ghent and Kudlyak (2011)
Broker Restrictiveness Index	6.0631	3.6721	0.0000	12.0000	512,754	Pahl (2007)
Homestead Exemption (Ln)	10.3305	0.9167	4.1352	13.1224	512,754	Gropp et al. (1997)
Nonhomestead Exemption (Ln)	8.8384	0.5923	5.7038	11.2450	512,754	Gropp et al. (1997)
Foreclosure legal cost (USD thousands)	4.8065	2.3287	2.2150	14.8100	5,107	SFLD
Timeline (Days)	109.1560	73.4516	27.0000	445.0000	5,107	USFN
Applicant Income (Ln)	11.1993	0.6834	6.9078	16.118	512,754	HMDA
Male Applicant (Dummy)	0.6744	0.4686	0.0000	1.0000	512,754	HMDA
Minority (Dummy)	0.2183	0.4131	0.0000	1.0000	512,754	HMDA
Lenders per capita	0.7026	1.2784	0.0092	7.6331	512,754	HMDA
Subprime loan (Dummy)	0.0811	0.2731	0.0000	1.0000	512,754	HMDA
House price Index	11.9351	1.4866	7.7883	14.7678	512,754	HUD
Renter-occupied housing units	0.1094	0.1124	0.0000	1.0000	512,754	HMDA
Interest rate (%)	7.7901	2.8537	3.4500	17.1100	512,754	FHFA
Effective interest rate (%)	8.0070	2.9769	3.5600	17.9100	512,754	FHFA
Arrangement Fee (%)	1.2856	0.7595	0.0100	4.3800	512,754	FHFA
Term to maturity (years)	27.0319	2.2917	8.9000	58.9000	512,754	FHFA

Table 1 Cont'd: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max	Observations	Source
Mortgage Insurance (%)	24.2431	1.1563	12.0000	30.0000	512,754	SFLD
Debt-to-income ratio (%)	34.9544	2.9187	15.0000	47.2512	512,754	SFLD
Original LTV ratio (%)	76.4889	4.7678	53.4821	95.0000	512,754	SFLD
FICO	718.108	11.1381	576.0000	781.0000	512,754	SFLD
Renegotiation Rate (%)	0.03312	0.05645	0.0000	1.4710	512,754	SFLD
State corporate tax (%)	7.1040	1.6989	0.0000	9.9891	512,754	Tax Foundation
Mortgage delinquency rate (%)	1.3471	0.9893	0.0000	19.5500	512,754	NY Fed
Auto delinquency rate	2.3484	1.2540	0.0006	14.4601	512,754	NY Fed
Credit Card Delinquency Rate (%)	8.0501	2.6121	0.7504	41.5200	512,754	NY Fed
Unemployment Rate (%)	4.9812	1.0675	1.6070	11.4000	512,754	Bureau of Economic Analysis
Per capita Income (Ln)	17.4123	0.6915	4.6006	20.4625	512,754	Internal Revenue Service
Urbanization (Dummy)	0.7648	0.2012	0.0000	1.0000	512,754	US Bureau of the Census
Poverty Rate (%)	0.1142	0.0320	0.0310	0.3494	512,754	US Bureau of the Census
Black (Dummy)	0.1021	0.0812	0.0000	0.6691	512,754	HMDA
Hispanic (Dummy)	0.0942	0.0670	0.0045	0.7824	512,754	HMDA
Murder (%)	0.0001	0.0004	0.0000	0.0011	512,754	US Bureau of the Census
Rape (%)	0.0002	0.0003	0.0000	0.0012	512,754	US Bureau of the Census
Violent crimes (%)	0.0029	0.0041	0.0000	0.0271	512,754	US Bureau of the Census
Aggravated Assault (%)	0.0032	0.0028	0.0000	0.0147	512,754	US Bureau of the Census
Degree	25.6204	7.8920	7.4005	60.2006	512,046	US Bureau of the Census
HHI (ln)	10.2619	1.0179	6.9626	12.3121	512,754	HMDA
Bank size (ln)	16.5445	2.8014	8.6800	20.8941	258,084	FDIC
Z-score (ln)	3.6901	0.7315	2.1333	5.1824	258,084	FDIC
Capital ratio (%)	10.2944	3.4703	1.8078	20.1513	258,084	FDIC
NII (%)	3.4071	0.9432	0.0114	28.2652	258,084	FDIC
Cost of deposits (%)	0.6678	0.9437	0.0115	28.2649	258,084	FDIC
Out-of-state loan (Dummy)	0.1482	0.3553	0.0000	1.0000	512,754	HMDA and FDIC

Notes: This table provides descriptive statistics for the variables used in the empirical analysis. Since we study the securitization rate, all observations included are accepted mortgages. The sample for GSE-eligible loans includes 469,761 observations, the sample for jumbo loans includes 32,256 observations and the sample for subprime loans includes 42,126 observations. Assignment is the distance to the threshold in miles. Foreclosure cost is measured in thousands of 2007 US \$. Bank size is total bank assets. Capital ratio is the ratio of equity to total assets. Lenders per capita denotes the number of bank branches per 100 population. Foreclosure costs is measured in thousands of US\$ (defalted into 2016 prices). 'Ln' denotes that a variable is measured in natural logarithms. 'Source' denotes the data provider.

Table 2: Probability of Default and Foreclosure Cost

	1	2	3	4
Dependent variable	Cost	Cost	Default	Default
JR	0.4766***	0.2585***	0.1210***	0.4557***
	(27.87)	(11.37)	(6.61)	(23.10)
Control variables	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	5,107	5,107	168,201	168,201

Notes: This table presents estimates of the equation $y_{ilrst} = \alpha + \beta JR_{ilrst} + X_{ilrst} + \gamma_t + \varepsilon_{ilrst}$ where y_{ilrst} is either the cost of forcelosure in natural logarithms or a default dummy variable that equals 1 if loan i defaults, 0 otherwise. JR_{ilrst} is a dummy variable equal to 1 if state s uses JR law, 0 otherwise, X_{ilrst} is a vector of control variables, γ_t are year fixed effects, and ε_{ilrst} is the error term. We estimate the equation using a random 1% sample of observations from the Single Family Loan Database (SFLD) data between 2000 and 2016. An overview of the SFLD is provided in Appendix A. Control variables include per capita income, the debt-to-income ratio, the house price index, and loan age. Cost includes legal costs, associated taxes, property maintenance cost after foreclosing and miscellaneous costs. The sample in columns 1 and 2 use only observations where default has occurred. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

Table 3: Balanced Covariates Tests

Variable	JR	PS	Difference	t-statistic
Applicant Income (ln)	11.0104	11.0204	-0.0100	-0.23
Male Applicant (Dummy)	0.5901	0.5684	0.0224	1.51
Minority Applicant (Dummy)	0.5150	0.5153	-0.0003	0.01
Lenders per capita	0.0606	0.0576	0.0030	1.44
House price index	12.7068	12.4675	0.2393	1.25
Renter-occupied housing units (%)	10.7356	11.2401	0.5193	1.25
Interest rate (%)	7.8080	7.7797	0.0283	0.22
Effective interest rate (%)	8.0248	8.0001	0.0247	0.21
Arrangement Fee (%)	1.2749	1.2990	-0.0241	-0.71
Term to maturity (years)	26.9880	27.0878	-0.0998	-1.15
Mortgage Insurance (%)	24.2209	24.2173	0.0037	0.02
Debt-to-income ratio (%)	35.0530	34.9910	0.0610	0.05
Original LTV ratio (%)	76.5633	76.1377	0.4254	0.39
FICO	718.8501	717.9552	0.8949	0.79
State corporate tax (%)	6.5833	6.6123	-0.0290	-0.04
Renegotiation rate (%)	0.0585	0.0562	0.0324	0.91
Auto delinquency rate (%)	2.8012	2.7965	0.0047	0.04
Credit card rate (%)	4.8004	4.7802	0.0202	0.12
Unemployment rate (%)	4.8811	4.7612	0.1199	1.11
Per capita Income (Ln)	17.0113	17.7624	-0.7511	-1.02
Urbanization (Dummy)	0.7730	0.7271	0.0459	1.05
Poverty rate (%)	0.1136	0.1229	-0.0093	-0.88
Black population (%)	0.0757	0.0567	0.0190	0.75
Hispanic population (%)	0.0714	0.0778	0.0064	0.25
Murders	0.0002	0.0001	0.0001	1.04
Rape	0.0002	0.0003	-0.0001	1.02
Aggravated assault	0.0022	0.0020	0.0002	0.66
Violent crimes	0.0031	0.0027	0.0005	0.75
Degree (%)	26.8000	25.2100	0.0159	0.25
HHI (Ln)	9.9927	10.0652	-0.0725	1.07
Rate spread (%)	0.4553	0.5387	-0.0833	1.22
Non-bank (Dummy)	0.4593	0.4853	-0.0259	1.19
Z-score (ln)	3.6701	3.7107	-0.0406	1.02
Capital ratio (%)	10.2813	10.1976	0.0837	0.65
Out-of-state loan (Dummy)	0.1577	0.1489	0.0088	1.30

Notes: This table reports the results of t-tests for differences in the average level of various covariates between the JR and PS regions either side of the threshold. JR and PS denote the mean of each variable on the JR and PS side of the threshold, respectively. Difference is the difference between JR and PS. The null hypothesis is that JR = PS. t-statistic is the t-statistic from a two-tailed test of the null hypothesis that Difference is equal to zero.

Table 4: Tests for Manipulation of Treatment Status

Variable	Applications			Branches			Net Migration	Population
Estimator	PAR	PAR	NPAR	PAR	PAR	NPAR	Par	Par
JR	0.0247	-0.0166	-0.0081	0.0049	-0.0110	-0.0146	-0.0026	-0.0012
	(1.13)	(-0.80)	(-0.15)	(0.56)	(-1.35)	(-0.40)	(-0.39)	(-0.65)
Control Variables	No	Yes	No	No	Yes	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,249	6,249	6,249	6,249	6,249	6,249	430,862	49,783
R^2	0.08	0.09	-	0.04	0.06	-	0.70	0.04

Notes: This table reports estimates of the equation $y_{cst} = \alpha + \beta JR_{cst} + \delta X_{cst} + \gamma_t + \varepsilon_{cst}$, where y_{cst} represents the number of GSE-eligible mortgage applications per 100 population or the number of lender branches per 100 population in census tract c in state s during year t, the net migration rate per 1,000 population to county c in state s from county j, or the population growth rate in county c in state s during year t. JR_{cst} is a dummy variable equal to 1 if JR law is used in a census tract, 0 otherwise; X_{cst} is a vector of control variables (per capita income,the debt-to-income ratio, and the house price index); γ_t are year fixed effects; and ε_{cst} is the error term. 'PAR' indicates that parametric estimation is used to estimate the equation. 'NPAR' indicates that non-parametric estimation is used to estimate the equation. The net migration test uses a bilateral county-to-county data set. The population test uses annual county-level data. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

Table 5: Foreclosure Law and Securitization

	1	2	3	4	5	6	7
	All	All	All	All	2000-2007	2008-2016	5 miles
JR	0.0228***	0.0167***	0.0166***	0.0168***	0.0159***	0.0186***	0.0236***
	(8.91)	(5.26)	(5.22)	(5.28)	(3.78)	(3.86)	(5.76)
Assignment	-0.0001	0.0001	-0.0002	-0.0003	-0.0003	-0.0004	-0.0016*
	(-0.12)	(0.30)	(-0.55)	(-0.83)	(-0.49)	(-0.67)	(-1.87)
JR*Assignment			0.0006	0.0007	0.0003	0.0012	0.0006
			(1.07)	(1.23)	(0.42)	(1.33)	(0.45)
Applicant income				-0.0094***	-0.0147***	-0.0028	-0.0109***
				(-7.87)	(-9.14)	(-1.58)	(-7.12)
Male				0.0163***	0.0154***	0.0176***	0.0174***
				(11.11)	(7.86)	(7.95)	(9.38)
Minority				0.0399***	0.0344***	0.0487***	0.0348***
				(23.10)	(15.52)	(17.70)	(16.14)
Original LTV ratio				0.0002	-0.0098	0.3565**	-0.0045
				(0.01)	(-0.30)	(1.98)	(-0.11)
House price				-0.0013	-0.0030***	0.0032	-0.0025*
				(-1.34)	(-2.60)	(1.62)	(-1.95)
Lenders per capita				-0.0017	0.0108	-0.0033***	-0.0020
				(-1.63)	(1.23)	(-2.91)	(-1.43)
Region * Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Lender * Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	469,761	469,761	469,761	469,761	269,157	200,604	298,737
R^2	0.03	0.32	0.32	0.32	0.30	0.36	0.33
F-stat (Chow test)					5.52		
p-value (Chow test)					0.00		

Notes: This table present estimates of equation (2). The dependent variable is the Agency Securitization dummy variable. The sample includes only GSE-eligible loans. In columns 1-6 the sample includes all loans within 10 miles of the threshold. Column 7 uses observations within 5 miles of the threshold. In column 5 and 6 we restrict the observations between 2000-2007 and 2008-2016, respectively. The F-stat (p-value) is the F-statistic (p-value) from a Chow test testing for equality in the JR coefficient between column 5 and 6. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

Table 6: Sample Splits

	1	2	3	4	5	6	7	8	9	10
Sample	$\geq$ mean income	< mean income	Co-applicant	Sole applicant	< mean LTI	$\geq$ mean loan LTI	< mean unemp	$\geq$ mean unemp	< mean poverty	$\geq$ mean poverty
JR	0.0151***	0.0193***	0.0171***	0.0178***	0.0128***	0.0242***	0.0137***	0.0242***	0.0118***	0.0303***
	(3.35)	(3.85)	(3.55)	(4.01)	(3.25)	(4.36)	(3.14)	(4.67)	(3.11)	(3.90)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	251,650	218,111	248,515	221,246	302,334	167,427	263,157	206,604	345,533	124,228
R^2	0.39	0.36	0.29	0.48	0.35	0.39	0.35	0.37	0.34	0.33
F-stat (Chow test)	5.62		2.06		4.48		5.25		4.07	
p-value (Chow test)	0.00		0.00		0.00		0.00		0.00	

Notes: This table presents parametric estimates of equation (2). The dependent variable is the Agency Securitization dummy variable. The sample includes only GSE-eligible loans. The control variables are the same as those reported in column 4 of Table 5. The F-stat (p-value) is the F-statistic (p-value) from a Chow test testing for equality in the JR coefficient between the sample split regressions. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

Table 7: Foreclosure cost and Securitization

	1	2	3
Foreclosure legal cost	0.0115***		0.0065**
	(4.43)		(2.21)
Timeline		0.0093***	0.0076***
		(6.34)	(4.69)
Applicant Income	-0.0097***	-0.0089***	-0.0092***
	(-8.10)	(-7.42)	(-7.59)
Male	0.0167***	0.0163***	0.0166***
	(11.15)	(11.01)	(11.03)
Minority	0.0399***	0.0405***	0.0403***
	(22.76)	(23.26)	(22.87)
Original LTV	0.0071	-0.0015	0.0012
	(0.22)	(-0.05)	(0.03)
House price	-0.0010	-0.0009	-0.0008
	(-1.07)	(-0.92)	(-0.78)
Lenders per capita	-0.0014	-0.0014	-0.0014
	(-1.35)	(-1.27)	(-1.28)
Region * Year FE	Yes	Yes	Yes
Lender * Year FE	Yes	Yes	Yes
Observations	469,761	469,761	469,761
R^2	0.32	0.32	0.32

Notes: This table reports parametric estimates of the equation $s_{ilrst} = \beta C_{ilrst} + \varphi W_{ilrst} + \delta_{lt} + \delta_{rt} + \varepsilon_{ilrst}$ where all variables are defined as in equation (2) except C_{ilrst} which is either the legal costs of foreclosing a mortgage to lenders or the foreclosure timeline. The dependent variable is the Agency Securitization dummy variable. The sample includes only GSE-eligible loans. In columns 1 and 3 we use the mean value of foreclosure costs in state s during year t taken from the SFLD. In columns 2 and 3 we approximate $Timeline_{rst}$ using the mean foreclosure timeline in state s during year t taken from the US Foreclosure Network database. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

Table 8: Securitization in the Non-Agency Market

	1	2	3	4	5	6
Sample		Jumbos			Subprime	
Period	All	2000-2007	2008-2016	All	2000-2007	2008-2016
JR	-0.0046	-0.0184	0.0088	-0.0052	-0.0058	-0.0018
	(-0.42)	(-0.94)	(0.72)	(-0.47)	(-0.45)	(-0.14)
Assignment	0.0022	0.0027	0.0016	-0.0016	-0.0018	-0.0006
	(1.32)	(0.91)	(0.89)	(-1.20)	(-1.14)	(-0.46)
JR*Assignment	-0.0009	-0.0030	0.0013	0.0031	0.0030	0.0029
	(-0.39)	(-0.75)	(0.50)	(1.54)	(1.25)	(1.41)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Region * Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender * Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,256	15,101	17,155	42,126	25,257	16,869
R^2	0.70	0.61	0.76	0.77	0.70	0.87

Notes: This table reports parametric estimates of equation (2). The dependent variable is the Non-Agency Securitization dummy variable. In columns 1 to 3, the sample includes Jumbo loans. In columns 4 to 6, the sample includes subprime loans. The sample in columns 1 and 4 use data for the entire sample period, 2000 to 2016. The sample in columns 2 and 5 use data for the period 2000 to 2007. The sample in columns 3 and 6 use data for the period 2008 to 2016. The control variables are the same as those in column 4 of Table 5. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

Table 9: Loan Pricing and Quality

	1	2	3	4	5	6	7	8
JR	0.0164***	0.0166***	0.0166***	0.0158***	0.0169***	0.0166***	0.0167***	0.0168***
	(4.76)	(4.79)	(4.73)	(4.48)	(5.32)	(5.25)	(5.28)	(5.31)
Assignment	-0.0005	-0.0005	-0.0005	-0.0005	-0.0003	-0.0003	-0.0003	-0.0003
	(-1.15)	(-1.16)	(-1.15)	(-1.14)	(-0.85)	(-0.80)	(-0.82)	(-0.82)
JR*Assignment	0.0009	0.0009	0.0009	0.0009	0.0007	0.0007	0.0007	0.0007
	(1.41)	(1.41)	(1.41)	(1.41)	(1.25)	(1.16)	(1.22)	(1.22)
Interest rate	0.0151							
	(0.80)							
Effective interest rate		0.0161						
		(0.87)						
Arrangement fee			0.0045					
			(0.47)					
Term to maturity				0.0482				
				(0.64)				
HHI					-0.0019*			
					(-1.83)			
FICO						-0.1782**		
						(-2.30)		
Debt-to-income ratio							0.0006	
							(0.96)	
Insurance percentage								0.0012
								(1.11)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	469,761	469,761	469,761	469,761	469,761	469,761	469,761	469,761
R^2	0.32	0.32	0.32	0.32	0.32	0.32	0.32	0.32

Notes: This table present estimates of equation (2). The dependent variable is the Agency Securitization dummy variable. The sample includes only GSE-eligible loans. In columns 1-5 and 8-10 the sample includes all loans within 10 miles of the threshold. The control variables are the same as those in column 4 of Table 5. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

Table 10: Falsification Tests

Regression	1	2	3	4
Sample	- 10 miles	+ 10 miles	JR-JR	PS-PS
Placebo	-0.0035	-0.0036	0.0014	-0.0120
	(-1.10)	(-1.22)	(0.47)	(-1.53)
Assignment	0.0034***	0.0001	0.0006	0.0012*
	(4.68)	(0.18)	(0.81)	(1.71)
Placebo*Assignment	-0.0044***	0.0004	-0.0034***	-0.0017*
	(-4.15)	(0.41)	(-3.29)	(-1.85)
Control variables	Yes	Yes	Yes	Yes
Region * Year FE	Yes	Yes	Yes	Yes
Lender * Year FE	Yes	Yes	Yes	Yes
Observations	501,503	446,189	486,464	454,976
R^2	0.03	0.04	0.04	0.05

Notes: This table reports parametric estimates of equation (6). The dependent variable is the Agency Securitization dummy variable. The sample includes only GSE-eligible loans. *Placebo* is a dummy variable equal to 1 for observations to the right of the placebo threshold, 0 for observations to the left of the placebo threshold. "-10 miles" indicates that the placebo threshold is located 10 miles to the left of the actual threshold. That is, PS law is used on both sides of the placebo threshold. In column 1 the sample contains observations 10 miles either side of the -10 miles placebo threshold. "+10 miles" indicates that the placebo threshold is located 10 miles to the right of the actual threshold. That is, JR law is used on both sides of the placebo threshold. In column 2 the sample contains observations 10 miles either side of the -10 miles placebo threshold. JR-JR (PS-PS) indicate that the sample contains observations within 10 miles of the border between two states that use JR (PS) law. The control variables are the same as those in column 4 of Table 5. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

Table 11: Legal Environment Robustness Tests

	1	2	3	4	5	6	7
Sample:	All	All	All	All	Excludes DE & PA	Excludes 2005-2016	Excludes 2010-2016
JR	0.0165***	0.0170***	0.0166***	0.0191***	0.0165***	0.0152***	0.0159***
	(5.19)	(5.34)	(5.13)	(4.01)	(5.16)	(2.97)	(3.98)
Assignment	-0.0003	-0.0003	-0.0004	-0.0006	-0.0003	-0.0002	-0.0002
	(-0.82)	(-0.83)	(-0.89)	(-1.43)	(-0.88)	(-0.38)	(-0.50)
JR*Assignment	0.0007	0.0007	0.0008	0.0011*	0.0007	0.0007	0.0005
	(1.24)	(1.21)	(1.41)	(1.75)	(1.15)	(0.80)	(0.68)
Right of redemption	0.0032						
	(0.91)						
Deficiency judgment		0.0086					
		(1.41)					
Homestead exemption			0.0050***				
			(4.38)				
Nonhomestead exemption			-0.0030				
			(-1.53)				
Broker restrictiveness index				-0.0011			
				(-1.32)			
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	469,761	469,761	458,720	372,583	458,013	175,891	305,871
R^2	0.32	0.32	0.32	0.33	0.32	0.28	0.31

Notes: This table presents parametric estimates of equation (2). The dependent variable is the Agency Securitization dummy variable. The sample includes only GSE-eligible loans. Columns 1-4 use observations from the full sample. Column 5 excludes observations from Delaware (DE) and Pennsylvania (PA). Column 6 excludes observations from 2005 onward. Column 7 excludes observations from 2010 onward. Nonhomestead exemptions are the sum of automobile, other property, and wildcard exemptions. The control variables are the same as those reported in column 4 of Table 5. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

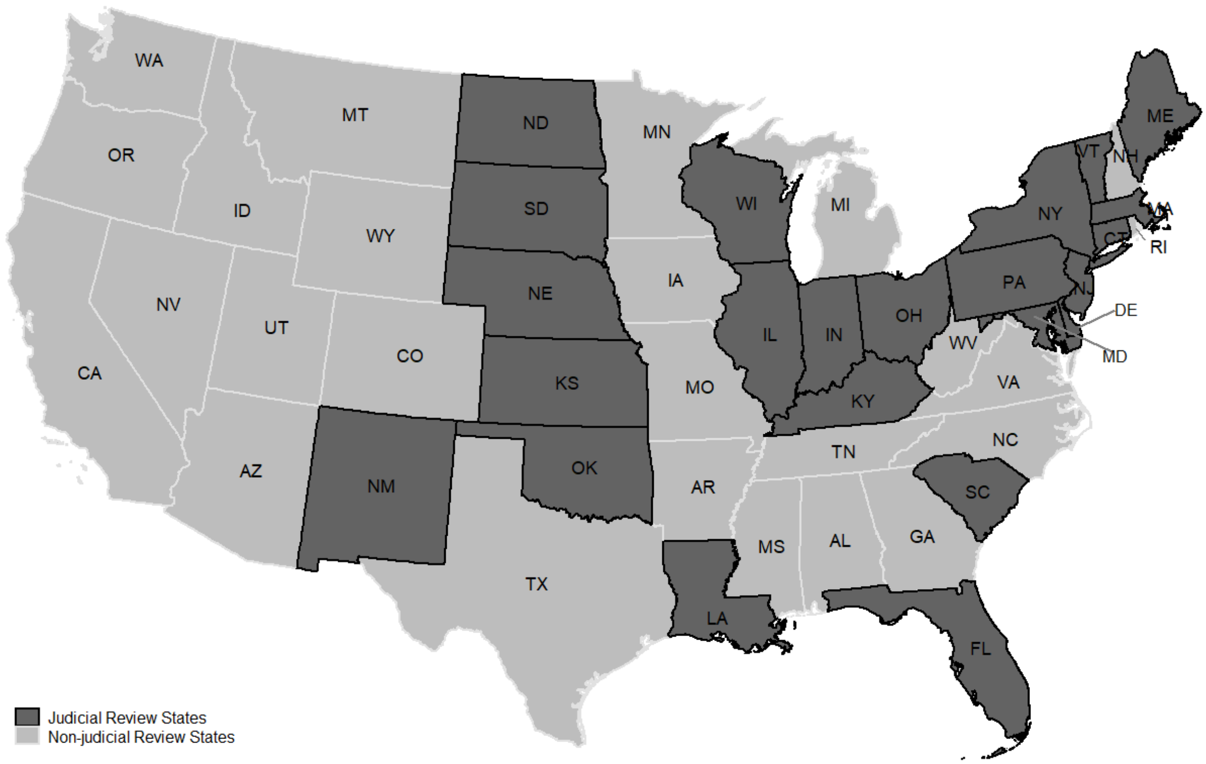
Table 12: Lending Industry Robustness Tests

	1	2	3	4	5	6	7	8	9	10
Sample	Non-banks	Banks	Banks in same region	Banks	Banks	State chartered	National chartered	Single state	Multi state	Low OTD
JR	0.0121**	0.0217***	0.0227***	0.0416***	0.0217***	0.0209***	0.0211***	0.0187***	0.0295***	0.0215***
	(2.54)	(4.88)	(4.88)	(9.01)	(4.88)	(2.69)	(3.64)	(3.32)	(3.66)	(4.64)
Assignment	-0.0008	-0.0001	-0.0004	-0.0001	-0.0001	-0.0001	-0.0004	0.0002	-0.0018*	0.0000
	(-1.35)	(-0.06)	(-0.64)	(-0.12)	(-0.07)	(-0.13)	(-0.52)	(0.32)	(-1.81)	(0.01)
JR*Assignment	0.0010	0.0004	0.0006	0.0017*	0.0004	0.0011	0.0002	-0.0001	0.0024	0.0005
	(1.14)	(0.48)	(0.66)	(1.95)	(0.50)	(0.80)	(0.21)	(-0.13)	(1.59)	(0.60)
Bank size				0.0227***						
				(47.83)						
Z-score (Ln)				0.0234***						
				(16.70)						
Capital Ratio				-0.0002						
				(-0.57)						
Cost of deposit				-0.0001**						
				(-2.02)						
Out of state					0.0032					
					(0.76)					
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender * Year FE	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes
Observations	211,677	258,084	204,127	258,084	258,084	116,724	141,360	178,960	79,124	244,998
R^2	0.36	0.33	0.33	0.12	0.33	0.34	0.37	0.36	0.33	0.31

Notes: This table presents parametric estimates of equation (2). The dependent variable is the Agency Securitization dummy variable. The sample includes only GSE-eligible loans. The sample in column 1 contains all non-banks whereas the sample in column 2 and 4-10 includes banks. Cost of deposits is the ratio of deposit interest expenses to total deposits. Column 3 uses observations of loans originated by banks that lend in either side of the threshold within the same region. Column 6 and 7 uses observations of loans originated by state chartered banks and national chartered banks, respectively. Column 8 uses observations of loans originated by single state banks, and column 9 includes banks that operate in more than 1 state. Column 10 uses observations of loans originated by banks that do not operate OTD business models, defined as banks that securitize less than 50% of mortgage loans. The control variables are the same as those reported in column 4 of Table 5. Heteroskedasticity robus t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

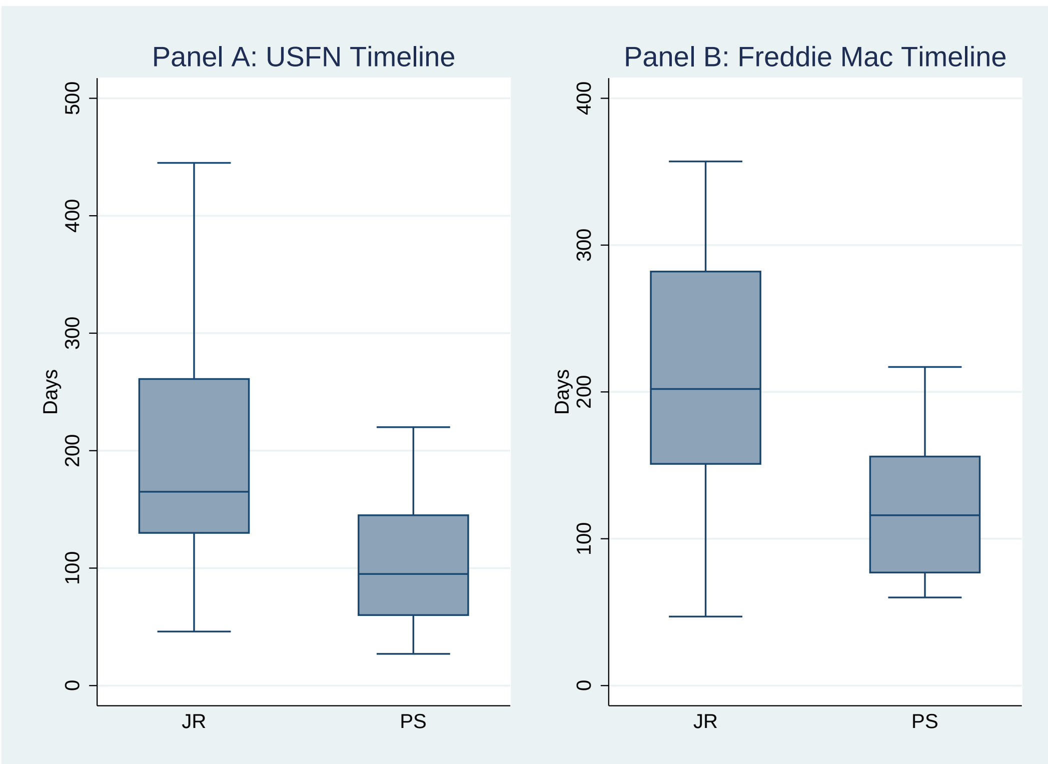
Figures

Figure 1: Foreclosure Laws in each State



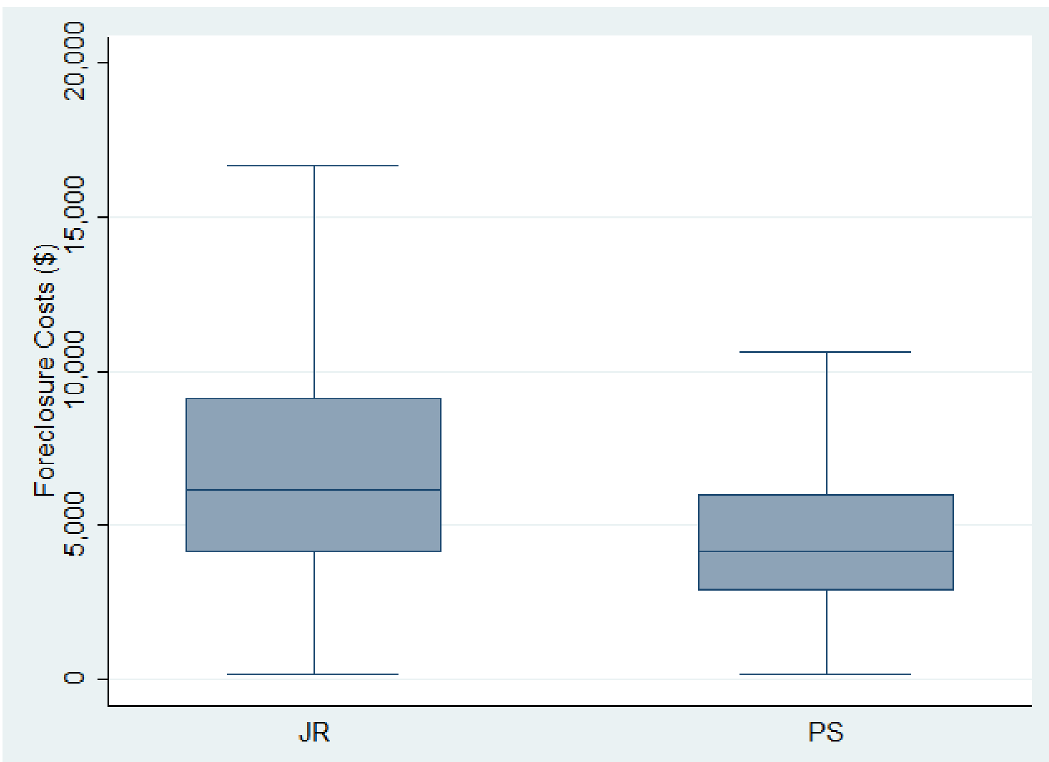
Notes: This figure reports the type of foreclosure law used in each US state. Information on type of foreclosure process a state uses is taken from Realtytrac.com. Although Alaska and Hawaii are not shown, both states use PS law.

Figure 2: Foreclosure Timelines



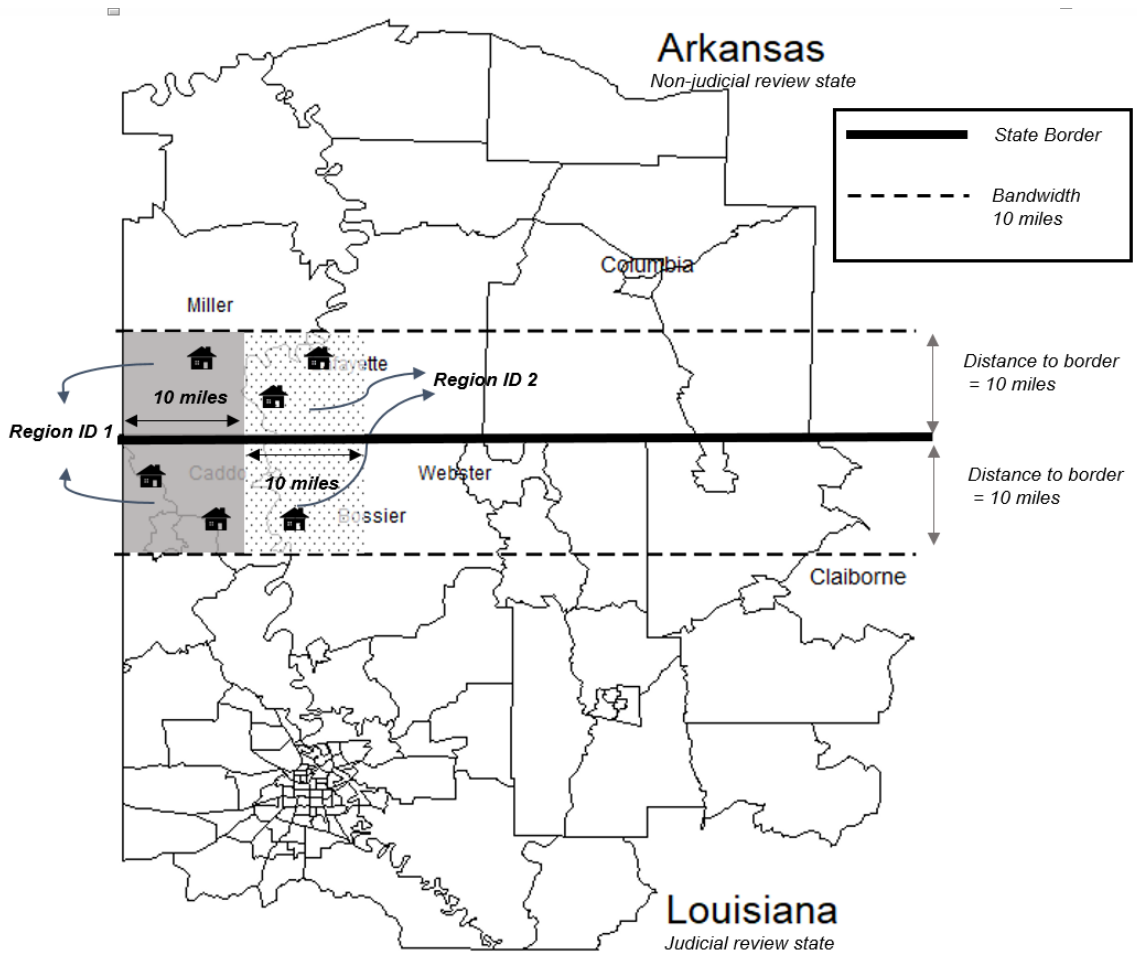
Notes: This figure presents the mean and range of the number of days required to process a foreclosure across US states. Panel A uses data from the US Foreclosure Network which provides an estimate of the number of days it takes to foreclose a property based on state regulations. That is, they do not include process delays. Panel B uses data provided by Freddie Mac through the National Mortgage Servicers' Reference Dictionary. The data in both panels covers the years 2001 to 2006.

Figure 3: Foreclosure Laws and Foreclosure Costs



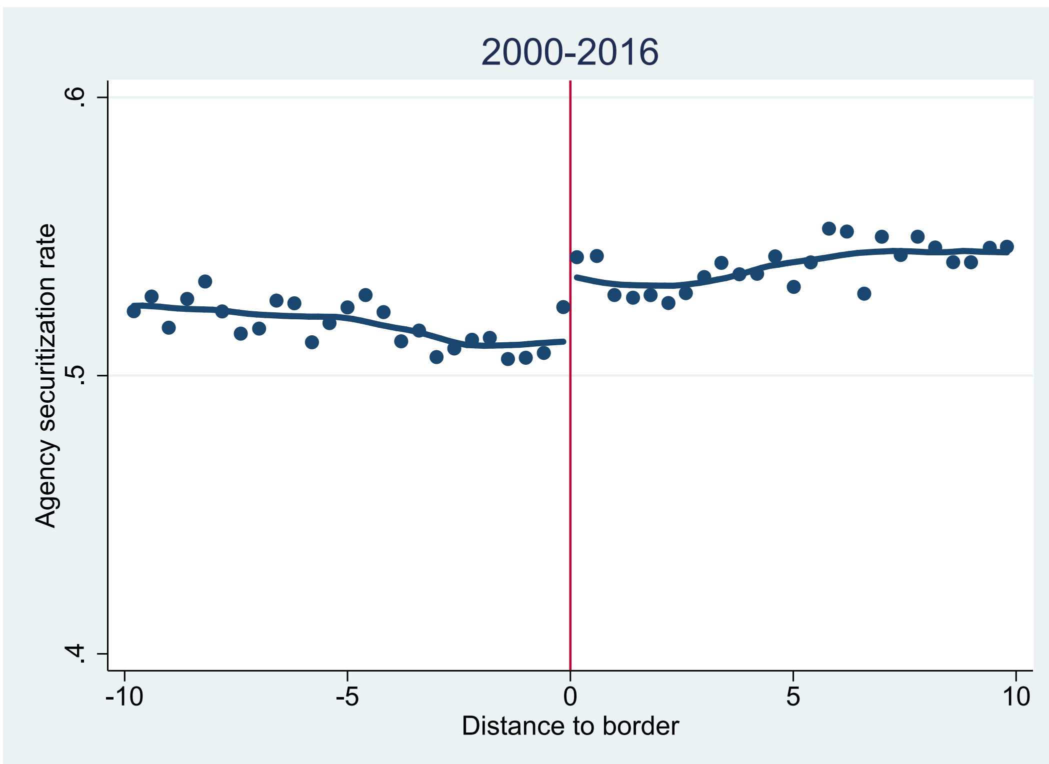
Notes: This figure presents the mean and range of the foreclosure costs in 2015 US$ incurred by lenders in JR and PS states. Information on foreclosure costs is taken from the SFLD database.

Figure 4: Region Fixed Effects



Notes: This figure provides an overview of region-year fixed effects we use in equation (2). The map plots census tracts along a section of the Arkansas-Louisiana border. The sample includes only loans made to buy houses that lie within 10 miles of the border (threshold). We define regions as geographical areas that span the border and measure 10 miles wide by 20 miles long. Each region is assigned an identifier (e.g. Region ID 1 and Region ID 2).

Figure 5: Securitization Discontinuity at the Threshold



Notes: This figure shows non-parametric RDD estimates for how Agency Securitization responds to foreclosure law during the full sample period 2000 to 2016. Distance to border = 0 defines the border (threshold) between JR and PS states. Observations to the left (right) of the threshold are from the PS (JR) side of the border. We calculate the optimal bin width to be 0.4 miles following Lee and Lemieux (2010). We then calculate $\bar{s}_j$, the mean rate of securitization within bin j using all mortgage applications within that bin. Next, we plot $\bar{s}_j$ against its midpoint. Distance to border is the great circle distance between the mean midpoint of census tracts within each bin and the nearest JR-PS border coordinate. Distance to border is negative for the control group (PS) and positive for the treatment group (JR). We fit local regression functions either side of the threshold using a rectangular kernel.

Appendix

A: Single Family Loan Database

Fannie Mae provide access to the Single Family Loan database (SFLD). This source contains loan-level information on the characteristics of all loans at the point of origination purchased by Fannie Mae and Freddie Mac. To comply with the Federal Housing Enterprises Financial Safety and Soundness Act of 1992, Fannie Mae and Freddie Mac are required to submit loan level data on loan performance and acquisitions to the Department of Housing and Urban Development (HUD). Although these data sources do not cover all mortgages in the US, they represent the majority.

The data provide monthly information on the performance of each loan during its lifetime. Importantly, the data report whether a mortgage is in default. We construct a default dummy that equals 1 if a loan defaults, 0 otherwise. Data are also available on the lender's cost of foreclosing each loan that defaults. Owing to the vast size of the SFLD we take a random 1% sample of mortgage loans. This provides a sample of 168,201 observations.

We also merge some variables from the SFLD into the HMDA data set. Unfortunately, the data sets do not contain a common identifier through which we can match the loan-level information. For that reason we aggregate the SFLD variables to the county-year level and merge these variables into the HMDA data.

B: Methodological Robustness Checks

Table A1: Non-parametric RDD and Higher Order Polynomial Regressions

	1	2	3	4
	PAR	PAR	PAR	NPAR
	Quadratic	Cubic	Quartic	
JR	0.0182***	0.0199***	0.0210*	0.0171***
	(4.38)	(2.87)	(1.75)	(3.36)
Assignment	-0.0016**	-0.0044***	-0.0066**	
	(-2.31)	(-2.98)	(-2.49)	
JR*Assignment	0.0046***	0.0127***	0.0266***	
	(4.70)	(6.05)	(7.03)	
Assignment^2	0.0001**	0.0005**	0.0011*	
	(2.01)	(2.48)	(1.74)	
JR*Assignment^2	-0.0002***	-0.0014***	-0.0051***	
	(-4.42)	(-5.13)	(-5.80)	
Assignment^3		-0.0001**	-0.0001	
		(-2.13)	(-1.28)	
JR*Assignment^3		0.0001***	0.0004***	
		(4.37)	(4.95)	
Assignment^4			0.0001	
			(0.99)	
JR*Assignment^4			-0.0001***	
			(-4.39)	
Region * Year FE	Yes	Yes	Yes	Yes
Lender * Year	Yes	Yes	Yes	Yes
Observations	469,761	469,761	469,761	469,761
R^2	0.29	0.32	0.30	

Notes: This table presents non-parametric results and estimates of equation (2) with higher order polynomial expressions of the assignment variable and interactions between these polynomials and the JR dummy variable. PAR and NPAR indicate that a parametric and non-parametric estimator is used, respectively. The non-parametric estimator uses a rectangular kernel. The dependent variable is the Agency Securitization dummy variable. The sample includes only GSE-eligible loans. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

Table A2: Logit Model Results

	1	2
Estimator	Logit	Multinomial logit
JR	0.0108***	0.0303**
	(9.74)	(2.54)
Assignment	-0.0045***	-0.0042**
	(-3.16)	(-2.49)
JR* Assignment	0.0082***	0.0076***
	(3.99)	(3.16)
Control variables	Yes	Yes
Region * Year FE	Yes	Yes
Lender * Year FE	Yes	Yes
Observations	469,761	469,761
R^2	0.02	0.03

Notes: This table reports parametric estimates of equation (2) using logit and multinomial logit models. The dependent variable is the Agency Securitization dummy variable. The sample includes only GSE-eligible loans. The control variables are the same as those in column 4 of Table 5. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

C: Probability of Mortgage Default

Table A3: Spliting sample by Probability of Default

	1	2
Sample	$<$ mean PD	$\geq$ mean PD
JR	0.0188***	0.0228***
	(3.52)	(5.27)
Assignment	0.0007	-0.0007
	(1.00)	(-1.38)
JR*Assignment	0.0004	0.0012
	(0.38)	(1.53)
Control variables	Yes	Yes
Lender*Year FE	Yes	Yes
Region*Year FE	Yes	Yes
Observations	206,671	263,090
R^2	0.28	0.26

Notes: This table presents estimates of equation (2). The dependent variable is the Agency Securitization dummy variable. The sample includes only GSE-eligible loans. The control variables are the same as those in column 4 of Table 5. PD denotes probability of default estimated using the approach outlined by Agarwal et al. (2012). Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

D: Private Securitization among Agency Loans

Table A4: Private Securitization in the Prime Market

	1	2	3	4	5
Sample	All	All	All	2000-2007	2008-2016
JR	-0.0001***	-0.0001***	-0.0002***	-0.0002***	-0.0001***
	(-3.15)	(-4.01)	(-9.15)	(-11.39)	(-2.97)
Assignment	-0.0000***	-0.0000***	-0.0000***	-0.0000***	-0.0000**
	(-21.50)	(-20.97)	(-14.19)	(-7.12)	(-2.41)
JR*Assignment	0.0000***	0.0000***	0.0000***	0.0000***	-0.0000***
	(24.46)	(23.55)	(12.91)	(15.85)	(-9.13)
Renegotiation rate			-0.0001***		
			(-15.93)		
Control variables	No	Yes	Yes	Yes	Yes
Region * Year FE	Yes	Yes	Yes	Yes	Yes
Lender * Year FE	Yes	Yes	Yes	Yes	Yes
Observations	469,761	469,761	469,761	269,157	200,604
R^2	0.35	0.39	0.38	0.32	0.34

Notes: This table reports parametric estimates of equation (2). The dependent variable is the Non-Agency Securitization dummy variable. The sample includes only GSE-eligible loans. Columns 1 to 3 report estimates based on observations from the entire sample period whereas column 4 and 5 use observations from 2000-2007 and 2008-2016, respectively. The control variables are the same as those in column 4 of Table 5. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

E: Supplementary Robustness Tests

Table A5: Legal Factors

	1	2	3
Sample	Excludes LA	Excludes TX	Excludes MA
JR	0.0157***	0.0169***	0.0182***
	(4.88)	(5.31)	(5.50)
Assignment	-0.0004	-0.0001	-0.0003
	(-0.93)	(-0.36)	(-0.77)
JR*Assignment	0.0007	0.0005	0.0006
	(1.20)	(0.88)	(1.00)
Control variables	Yes	Yes	Yes
Region * Year FE	Yes	Yes	Yes
Lender * Year FE	Yes	Yes	Yes
Observations	454,144	460,938	446,734
R^2	0.32	0.32	0.32

Notes: This table presents estimates of equation (2). LA, TX, and MA denote Louisiana, Texas, and Massachusetts, respectively. The dependent variable is the Agency Securitization dummy variable. The sample includes only GSE-eligible loans. The control variables are the same as those in column 4 of Table 5. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

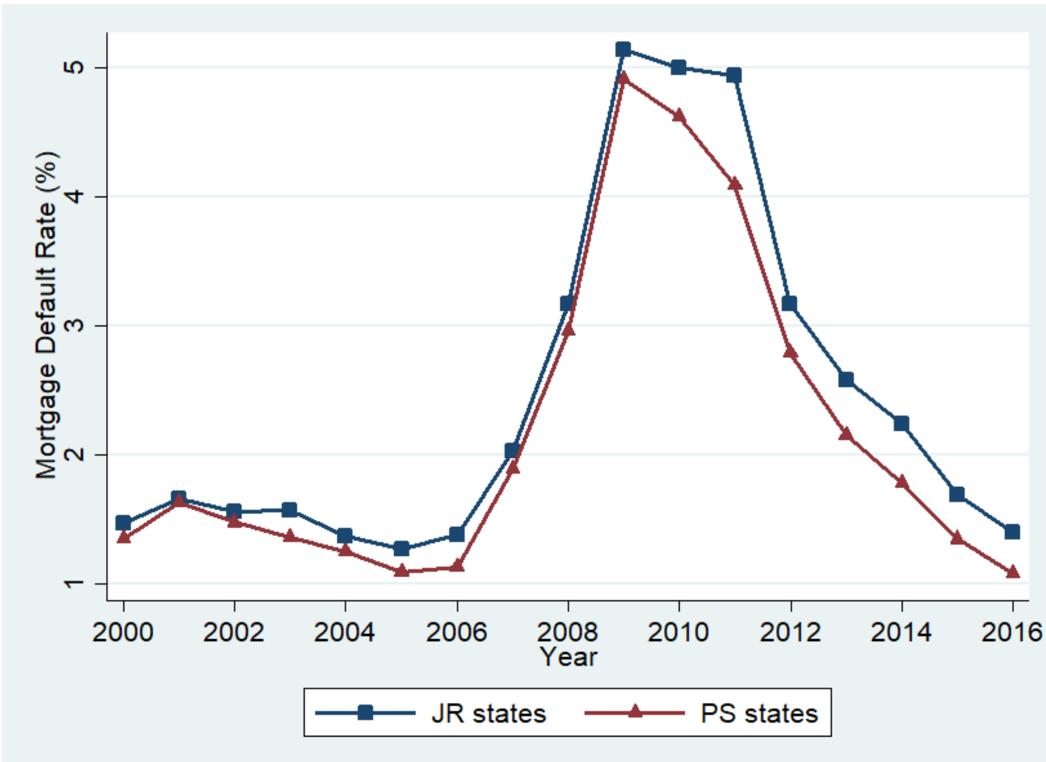
Table A6: Miscellaneous Sensitivity Checks

	1	2	3	4	5	6
Sample	All	All	Urban	Rural	All	All
JR	0.0158***	0.0178***	0.0109***	0.0308***	0.0178***	0.0165***
	(4.96)	(5.59)	(2.80)	(4.90)	(5.42)	(4.42)
Assignment	-0.0002	-0.0003	-0.0002	-0.0007	-0.0003	0.0000
	(-0.49)	(-0.77)	(-0.32)	(-0.97)	(-0.84)	(0.10)
JR*Assignment	0.0006	0.0008	0.0008	-0.0002	0.0008	0.0003
	(0.96)	(1.31)	(1.17)	(-0.16)	(1.29)	(0.47)
Credit card delinquency rate	0.0004					
	(0.84)					
Automobile delinquency rate	-0.0045***					
	(-4.99)					
Tract loan volume		0.0042***				
		(6.51)				
Renegotiation rate					-0.1015***	
					(-2.64)	
State corporate tax rate						0.0002
						(0.14)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Region * Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender * Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	469,761	469,761	337,229	129,598	465,390	414,514
R^2	0.32	0.32	0.32	0.35	0.32	0.32

Notes: This table presents parametric estimates of equation (2). The dependent variable is the Agency Securitization dummy variable. The sample includes only GSE-eligible loans. An urban area is defined as a metropolitan statistical area. The control variables are the same as those reported in column 4 of Table 5. In column 3 (4) the sample includes observations from urban (rural) areas. Heteroskedasticity robust t-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively.

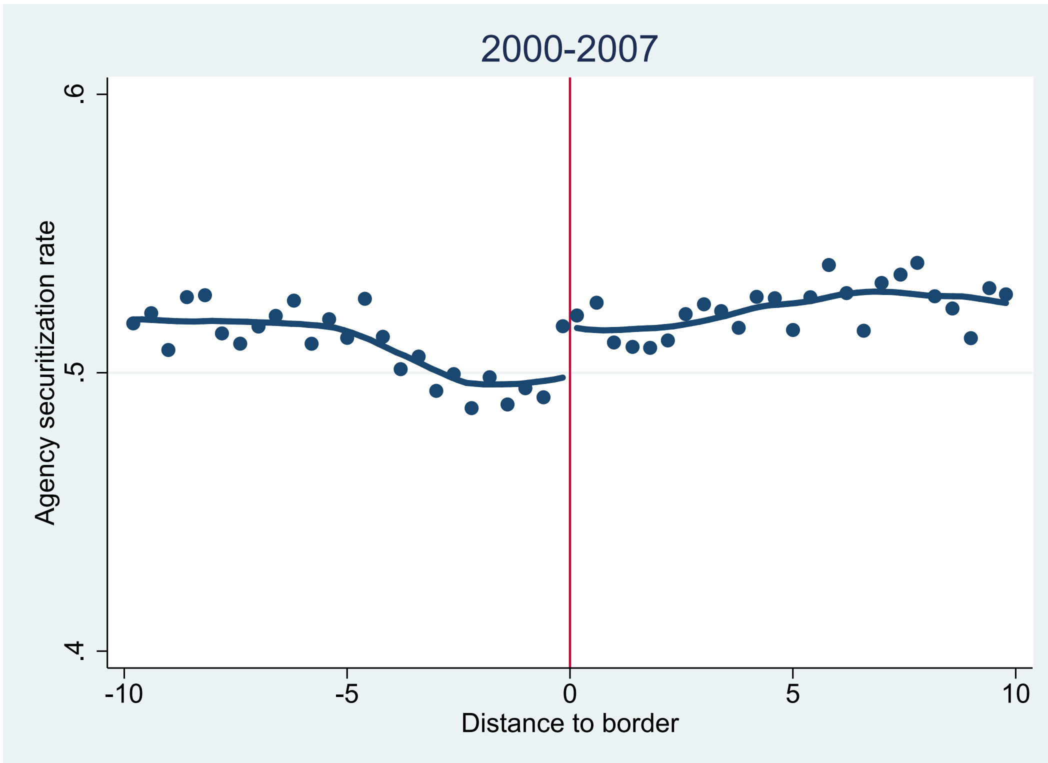
F: Appendix Figures

Figure A1: Mortgage Default Rates



Notes: This figure shows the mean rate of mortgage default (measured in %), defined as the share of mortgages that are at least 90 days late, in JR and PS states between 2000 and 2016. Data from 2000 to 2011 are taken from the NY Fed. Data from 2012 to 2016 are taken from the Consumer Finance Protection Bureau.

Figure A2: Pre-Crisis Securitization Discontinuity



Notes: This figure shows non-parametric RDD estimates for how Agency Securitization responds to foreclosure law during the period 2000 to 2007. Distance to border = 0 defines the border (threshold) between JR and PS states. Observations to the left (right) of the threshold are from the PS (JR) side of the border. We first calculate the optimal bin width to be 0.4 miles following Lee and Lemieux (2010). We then calculate $\bar{s}_j$, the mean rate of securitization within bin j using all mortgage applications within that bin. Next, we plot $\bar{s}_j$ against its midpoint. Distance to border is the great circle distance between the mean midpoint of census tracts within each bin-year and the nearest JR-PS border coordinate. Distance to border is negative for the control group (PS) and positive for the treatment group (JR). We fit local regression functions either side of the threshold using a rectangular kernel.

Figure A3: Post-Crisis Securitization Discontinuity

Notes: This figure shows non-parametric RDD estimates for how Agency Securitization responds to foreclosure law during the period 2008 to 2016. Distance to border = 0 defines the border (threshold) between JR and PS states. Observations to the left (right) of the threshold are from the PS (JR) side of the border. We first calculate the optimal bin width to be 0.4 miles following Lee and Lemieux (2010). We then calculate $\bar{s}_j$, the mean rate of securitization within bin j using all mortgage applications within that bin. Next, we plot $\bar{s}_j$ against its midpoint. Distance to border is the great circle distance between the mean midpoint of census tracts within each bin-year and the nearest JR-PS border coordinate. Distance to border is negative for the control group (PS) and positive for the treatment group (JR). We fit local regression functions either side of the threshold using a rectangular kernel.