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Borrower Protection and the Supply of Credit: Evidence from Foreclosure Laws

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Abstract

Laws governing the foreclosure process can have direct consequences on the costs of foreclosure and could therefore affect lending decisions. We exploit the heterogeneity in the judicial requirements across U.S. states to examine their impact on banks' lending decisions in a sample of urban areas straddling state borders. A key feature of our study is the way it exploits an exogenous cutoff in loan eligibility to GSE guarantees which shift the burden of foreclosure costs onto the GSEs. We find that judicial requirements reduce the supply of credit only for jumbo loans that are ineligible for GSE guarantees. These laws do not affect, however, the relative demand of jumbo loans. Our findings, which also hold using novel nonbinary measures of judicial requirements, illustrate the consequences of foreclosure laws on the supply of mortgage credit. They also shed light on a significant indirect cross-subsidy by the GSEs to borrower-friendly states that has been overlooked thus far.

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I Introduction

The foreclosure crisis and the ensuing regulatory reforms of the mortgage market have revived an old debate surrounding the trade-off between borrower protection and credit access.[1] The importance of this question also lies in the fact that regulations are persistent and thus their cumulative costs could be considerable. Unfortunately, however, the impact of borrower protection laws on credit supply remains relatively understudied in the empirical literature.

In this paper we investigate the impact of foreclosure laws on lending decisions by banks. Foreclosure laws govern the process through which creditors can take repossession of real estate following a default by the borrower. These laws have important implications to the foreclosure process, its duration, and the associated costs and risks to borrowers and creditors. Since borrower-friendly foreclosure laws typically impose additional costs to creditors in the event of default, and could potentially increase borrowers' incentive to default, one wonders whether and to what extent they also reduce the supply of credit. Our main focus is on judicial foreclosure laws, i.e., whether states require that the foreclosure process be handled by the court. Judicial procedures are more costly than power-of-sale alternatives, since they are more time consuming and require the use of legal professionals (see, e.g. Clauretie and Herzog, 1990, Schill, 1991).[2] The foreclosure crisis has highlighted the stark differences in the foreclosure timelines between so-called judicial and non-judicial states and their implication to foreclosure levels, house prices and the economy, as recently shown in Mian, Sufi and Trebbi (2011).

In order to provide a convincing answer to our main question, one has to be able to trace the impact of the laws on the *supply* of credit, controlling for potentially confounding factors. We therefore design an empirical strategy that takes advantage of a quasi-experimental setting. Specifically, we exploit *two* sources of exogenous variation. The first is the variation in foreclosure laws across state borders. The second variation is the discontinuity in loans' eligibility to guarantees from Government Sponsored Enterprises (GSEs), guarantees that shift the financial burden of foreclosure costs onto the GSEs. The role of the GSEs cannot be ignored on the mortgage market. The GSEs guarantee a large share of mortgages in exchange for guarantee fees that are *uniform* across states. Whether a loan is conforming, i.e., eligible for GSE guarantees, is determined by an exogenous regulatory loan-limit cutoff set by their regulator. Ignoring the role of GSEs might lead one to underestimate or misinterpret the

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[1]See, e.g., Evans and Wright (2009), U.S. House of Representatives (2012), Bipartisan Policy Center (2013), Kupiec (2014).

[2]These losses typically include foregone interest, attorney fees, court costs, property taxes, repairs, hazard insurance and other indirect costs.

effects of foreclosure laws. The use of this exogenous cutoff also allows us to considerably sharpen our empirical strategy to further address concerns related to unobservable factors.

While the costs associated with judicial foreclosure are directly related to the longer foreclosure timelines, the extant literature has so far relied on a binary variable to capture this difference. One of the contributions of this paper is that it brings in more direct and continuous measures of differences in foreclosure time frames. Our favorite variable is based on data collected by the U.S. Foreclosure Network (USFN), who through their legal expertise, provide an estimate of the number of days it takes to foreclose on a property *solely* based on state laws.

Using a comprehensive dataset on mortgage lending in the U.S., we therefore study the impact of foreclosure laws on banks' probability to reject a loan application using a sample of loans from urban areas straddling state borders while exploring the variation between conforming and non-conforming (jumbo) loans. One of the advantages of a loan-level analysis is that it allows us to study the decision by banks, a variable that is more directly linked to the supply side of the market (see, e.g. Loutskina and Strahan, 2009), and to control for bank fixed effects. We show that the banking sector is quite heterogeneous across borders even within an urban area, an issue that has not been sufficiently highlighted in the literature.

Our findings point to a significant, robust, and economically meaningful impact of judicial foreclosure laws on the supply of credit. Specifically, judicial foreclosure laws are associated with a significant increase in the relative rejection rate on jumbo loans. Their impact on the rejection rate of conforming loans is, on the other hand, weak and overall not statistically significant. These results are in line with our hypothesis, since the foreclosure costs on GSE-securitized conforming loans are borne by the GSEs. While the supply of credit is unevenly affected by foreclosure laws around the jumbo cutoff, we show that the demand for loans does not exhibit such variation. Specifically, the relative number, volume, and characteristics of jumbo loans in comparison to conforming loans do not correlate with judicial foreclosure laws across borders. All these results hold when using either the standard binary measure used in the literature or the foreclosure time frames we obtain from authoritative sources on state foreclosure requirements.[3] We subject our findings to a battery of robustness and falsification tests (including in an Online Appendix). We find that they help strengthen our results and support our interpretation.

We also examine whether bank characteristics could affect the sensitivity of their credit supply to foreclosure costs. In the sample of jumbo loans we find evidence that lending

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[3]Note that all our regressions control for two other weaker variations in foreclosure laws (discussed in Section II) related to deficiency judgments and right of redemption, but we do not highlight them due to lack of sufficient variation in the cross-border sample.

by banks that rely more on private securitization, as opposed to balance sheet lending, is significantly most responsive to the variation in foreclosure timelines. We also find some evidence that credit supply by banks that are more geographically diversified is also more negatively affected by longer timelines. Based on the extant literature on securitization (see, e.g. Keys, Mukherjee, Seru and Vig, 2010) and geographical diversification (see, e.g. Loutskina and Strahan, 2011) we argue that this could be due to the relative over-reliance of these banks on observable information such as those related to foreclosure laws.

While the literature on foreclosure laws is extensive, the impact of foreclosure laws on mortgage lending has received limited attention. An important exception is Pence (2006), which offers a rigorous treatment of the subject using HMDA data focusing on urban areas that straddle state borders. Pence (2006) studies specifically the impact of these laws on the size of the loan and finds that loan sizes are 3 to 7 percent smaller in defaulter-friendly states. Our paper differs along several important aspects. Key to our empirical analysis is the focus on isolating supply side factors and the distinction in loans' eligibility to GSE guarantees based on the exogenous jumbo cutoff. Our analysis also differs in that we focus on the decision by banks to reject a loan and in that we control for the heterogeneity in the banking landscape across borders. We also make use of new data that provide a more direct measure of the costs associated with judicial foreclosures.

A related strand of literature has examined the impact of foreclosure laws on bank losses, borrower behavior, and foreclosure rates. For example, Clauretie and Herzog (1990) finds that judicial foreclosure and the right of redemption increase the cost of foreclosure. Ghent and Kudlyak (2011) finds that recourse laws lower the sensitivity of default to negative equity. The literature also points to evidence that longer foreclosure duration, associated with judicial requirements, increases the incentive to default (see, e.g. Zhu and Pace, 2011, Gerardi, Lambie-Hanson and Willen, 2013).[4] These findings provide an additional channel through which judicial foreclosure can affect the supply of credit, as we discussed earlier. Mian *et al.* (2011) study the impact of judicial foreclosure on the incidence of foreclosure and use this as an instrumental variable to find that foreclosures lead to a large decline in house prices. A related strand of literature also studies the impact of bankruptcy law on credit supply (see, e.g. Gropp, Scholz and White, 1997, Berkowitz and White, 2004, Goodman and Levitin, 2014). Our paper is also related to the broader literature that studies the impact of state laws on credit markets (see, e.g. Huang, 2008, Favara and Imbs, 2013).

The paper is organized as follows. Section II briefly reviews state foreclosure laws in the

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[4]Calem, Jagtiani and Lang (2014) shows the liquidity benefit to borrowers from longer foreclosure timelines. The free rent during the foreclosure process helps households to cure their delinquent non-mortgage debts and improve their balance sheets in the short run.

United States. Section III presents the empirical strategy, data, and results. Section IV concludes.

II   State Foreclosure Laws

When a borrower defaults in the payment of the debt she has agreed to repay according to the terms of the mortgage, the loan documents usually contain an acceleration clause which causes the entire indebtedness to become due and payable. The foreclosure process is the procedure through which a mortgaged property is repossessed by the creditor following default by the borrower and a failure to repay the outstanding debt. In the United States, the foreclosure process is mainly regulated at the state level. It provides protections to borrowers against the harsh consequences of default and eviction, although with varying degrees across states.

A key characteristic of state foreclosure laws relates to whether the foreclosure process is handled by the court. In states with judicial foreclosure, the lender initiates the foreclosure process with the court by bringing evidence of default on the mortgage. The court then issues a decree, stipulates what notices should be provided, and oversees the various steps of the procedure. In non-judicial states instead, the liquidation of the property takes place relatively quickly through a power-of-sale and is handled by a trustee. While the judicial process protects borrowers against potentially unfair practices, it imposes costs on the lender or their insurer due to the delays associated with the process and the required legal resources.

We follow recent work by Mian *et al.* (2011) and classify states as judicial or non-judicial based on publicly available data from RealtyTrac, a leading foreclosure data provider.[5] Figure 1 shows a map of the U.S. mainland in which the 20 states that require a judicial process are shaded in gray. The figure shows that judicial foreclosure states are concentrated in the eastern part of the United States.

While the extant literature relies solely on a binary measure of judicial foreclosure laws, this has some well known disadvantages. First, the categorization of states between judicial and non-judicial is not straightforward in all cases, which explains some of the inconsistencies observed across papers on the topic. Second, as foreclosure experts and attorneys are well aware, there is a substantial variation in the processing timelines of foreclosures even *within* the groups of judicial and non judicial states. That is, some states impose lengthier procedures by law.

To the best of our knowledge, our paper is the first to address these issues by bringing in data that captures this heterogeneity more accurately. Specifically, we exploit data collected

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[5]http://www.realtytrac.com/foreclosure-laws/foreclosure-laws-comparison.asp

by the U.S. Foreclosure Network (USFN), who through their legal expertise, provide an estimate of the number of days it takes to foreclose on a property *solely* based on regulations (abstracting from additional operational delays). An index of the timelines (scaled by the longest timelines) is shown in Figure 2, where the red bars are states that are categorized as being judicial.[6] We clearly see a strong correlation between judicial foreclosure and USFN timelines but also substantial variation within groups, indicating that one can improve on the dummy judicial variable by using these timelines. In addition, we supplement the USFN measure with a measure of processing timelines produced by Freddie Mac (which has a correlation of 0.8 with the USFN) as we discuss in the data section.

Two other significant variations in foreclosure laws are usually considered in the literature. The first relates to the ability of the lender to pursue the borrowers' other assets when the borrower's debt exceeds the proceeds from the foreclosure sale. These are typically referred to as recourse laws. In the U.S., 8 states allow the lender to pursue a deficiency judgment against the borrower's other assets (i.e., allow recourse). While such action is rarely pursued by the lender due to inherent costs and the availability of insurance on most mortgages, it gives the lender more power in the negotiation process, allowing her to obtain concessions from the borrower. The second variation relates to the borrower's ability to regain the property after the foreclosure sale for the amount of the delinquent payments and the costs of foreclosure. In the 9 states that give borrowers a statutory right of redemption, borrowers can legally regain the property within a period that typically ranges between three months and a year. Such borrower-friendly statute also imposes uncertainty and costs on the lender.

A recent in-depth historical analysis by Ghent (2013) supports the exogeneity of these foreclosure laws to current economic conditions. Ghent finds that with the exception of anti-deficiency statutes "most differences in state mortgage law date from the nineteenth century and in many cases from before the US civil War." Ghent shows that whether states require judicial foreclosure or make available non-judicial options can be traced to idiosyncratic factors and individual decisions by judges rather than economic considerations. Recourse or deficiency judgment laws can however be partly traced to states' experience with farm foreclosure in the early 1930s.

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[6]The timelines in days as reported in RealtyTrac are shown in Table A2 of the Online Appendix

III   Empirical Strategy, Data, and Results

A   Empirical Strategy

Consumer friendly foreclosure laws impose additional costs on banks in case of mortgage default. By providing additional protections to borrowers they could also increase the incentive to default. We therefore ask whether and to what extent do these laws affect bank lending decisions at the time of origination.

In order to answer this question one has to take on the traditional identification challenge of separating between demand and supply effects using the available cross-sectional variation in these laws. A key objective of the empirical strategy is to ensure that a comparison of credit supply is not tainted by regional economic factors. Further, one has to address the possibility that foreclosure laws could affect consumers' incentive to borrow and default. One has to also control for bank-specific factors, especially since the banking landscape can vary across state borders, as we will show. These potentially confounding factors dictate the use of a granular approach since aggregate variables, such as the total volume of credit, are more easily tainted by demand factors.

Our use of individual loan-level data has several advantages in that regard. First, we are able to study the impact of foreclosure laws on banks' decision to reject a loan controlling for borrower characteristics and regional fixed effects. By having our dependent variable reflect the bank's decision conditional on a loan application we severely reduce the potential for variations in demand to affect our results. Second, we restrict our sample to a homogeneous category of loans originated in urban areas straddling state borders. That is, we compare lending within neighboring urban areas to minimize the variation in unobservable characteristics that could affect the demand for credit. Third, the loan-level approach allows us to control for bank fixed effects. Fourth, and crucially, we exploit an exogenous increase in banks' exposure to foreclosure laws above a certain loan amount, as we explain below, which helps us considerably sharpen our identification strategy. This approach will allow us to further ascertain the role of foreclosure laws as well as to address concerns related to impact of foreclosure laws on demand. We are also able to test the identifying assumption that underpins this methodology since the data we use allow us to observe the quantity and the quality of loan applications.

The baseline regression takes the following form:

$$R_{i,k,u,s,t} = \alpha_k + \beta_u + \rho_t + \theta X_{i,k,u,s,t} + \gamma Reg_s + \epsilon_{i,k,u,s,t} \tag{1}$$

where $R_{i,k,u,s,t}$ is a dummy variable that takes the value one if a loan application i, at bank k, on a property in the urban area u, in state s, at time t, was rejected by the bank. We control for bank, urban area, and year fixed effects and for the characteristics of the applications available from HMDA (X), including income, gender, race, income of the census tract (area size typically smaller than zip code) and the ratio of loan to income. The regulatory variables of interest are included in the vector Reg_s. These are a dummy for each: (i) judicial foreclosure requirement, (ii) statutory right of redemption, and (iii) prohibition of deficiency judgment. We also replace the judicial dummy with continuous foreclosure timelines.

We estimate (1) with a linear probability model (LPM) to fit a binary dependent variable. In our setting, the LPM has an important advantage over Probit and Logit models when $N \to \infty$ and T is fixed, since the estimates are generally inconsistent in Probit or Logit, but are $\sqrt{N}$ consistent using LPM (see, e.g. Wooldridge, 2002).

Exploiting the segmentation of the mortgage market

One cannot study the U.S. mortgage market without taking into consideration the role of the government-sponsored enterprises (GSEs, i.e., Fannie Mae and Freddie Mac). The GSEs are at the heart of a securitization market that is a key feature of the deep mortgage markets in the United States. When a bank originates a mortgage with certain standard features, it has the option to sell the mortgage to the GSEs or to purchase credit guarantees from them. In both cases the credit risk on the loan is transferred to the GSEs.

Importantly, and in case of default, the protection offered by the GSEs compensates not only the creditors (banks or MBS investors) but also the servicers for the associated foreclosure costs. Aware of the differences in foreclosure time frames across states, the GSEs issue guidelines to servicers which take into account these differences. Importantly, the guarantee fees charged by the GSEs do *not* vary across states. Further, the explicit and transparent eligibility criteria to GSE guarantees also do not discriminate across states. This is no doubt due to their public charter and the mandate by Congress, prior to the crisis, for the GSEs to support homeownership. The fact that the GSEs did not price risk like a private insurer would have is a well acknowledged fact.[7]

The market share of loans purchased or securitized by the GSEs has increased steadily since the early 1990s. By 2003, around 45 percent of *all* outstanding mortgages had GSE guarantees.[8] Since the GSE eligible market is roughly around 75% of the overall market, the

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[7]When the Acting Director of the Federal Housing Financing Agency (FHFA) announced a relative increase in guarantee fees on states with longer foreclosures delays in 2013 he acknowledged that: "The new pricing continues the gradual progression toward more market-based prices, closer to the pricing one might expect to see if mortgage credit risk was borne solely by private capital."

[8]See, e.g. Acharya, Richardson, Van Nieuwerburgh and White (2011).

share of the GSEs in their market was closer to 60 percent. This share, however, declined to some extent during 2004-06 when the private label market boomed.

These considerations imply that, in order to estimate the impact of borrower-friendly laws on banks' lending decision, one needs to take into account the role of the GSEs. This is because the availability of a credit protection on GSE eligible loans, which also compensates for the uncertain foreclosure costs in exchange for a uniform guarantee fee across states, shields lenders from borrower-friendly laws.

It is possible to overcome this identification problem by acknowledging the fact that the GSEs operated under a special charter limiting the size of mortgages that they may purchase or securitize. Loans above the legal cutoff are typically called jumbo loans. As discussed in Loutskina and Strahan (2009), jumbo loans are less liquid and are often held by banks or sold to private securitization pools. The existence of an exogenous discontinuity in the GSE subsidization of borrower friendly states suggests that the impact of judicial foreclosure on the supply of credit, if it is significant, should be more conspicuous in the jumbo segment of the market. By comparing jumbo to conforming loans we can also difference out unobservable but potentially confounding demand-side factors.

Therefore, the following regression is key to our empirical strategy:

$$R_{i,k,u,s,t} = \alpha_k + \beta_u + \rho_t + \theta X_{i,k,u,s,t} + \gamma Reg_s + \delta J + \phi Reg_s J + \epsilon_{i,k,u,s,t} \qquad (2)$$

where J is a dummy variable that takes 1 if the loan is jumbo (above the designated cutoff).

We estimate this regression in both the full sample and a subsample that includes applications that lie within a small interval of the jumbo cutoff in order to compare loans of similar size. If indeed these borrower-friendly laws discouraged lending by banks we would expect to observe a positive and significant coefficient on ϕ. Note that potentially confounding factors, such as regional variations in demand that are non-orthogonal to foreclosure laws, would not affect the coefficient θ in equation (2) as long as they affect demand evenly around the jumbo cutoff. We also test this identifying assumption by examining the relation between foreclosure laws and the relative demand, both in quantity and quality, of jumbo to non-jumbo loans.

B   Data and Summary Statistics

Data

We use a comprehensive sample of individual mortgage applications and origination collected by the Federal Reserve under the provision of the Home Mortgage Disclosure Act

(HMDA). Under this provision, the vast majority of mortgage lenders are required to report their house-related lending activity. HMDA data offers the best coverage of mortgage loans originated in the U.S., with coverage nearing 95 percent in urban areas.

HMDA data provides information on the characteristics of applicant including income, ethnicity, race, sex, and the median income of the census tract where the property is located. The data also provides the year of the application, the lender, the characteristics of the loan, and whether the loan was approved. We restrict our sample to either approved or denied loans, and the loan type to be conventional (excluding loans guaranteed by the Federal Housing Agency, Veterans Administration, Farm Service Agency or Rural Housing Service), the property types to be single family, the loan purpose to be home purchase (excluding refinancing or home improvement), and the occupancy status to be owner-occupied as principal dwelling.

Our empirical strategy dictates that we focus on metropolitan counties lying on state borders. We follow Pence (2006) and define an urban area as the collection of contiguous counties that belong to a metropolitan statistical area (MSA). Our sample is therefore made of 52 urban areas straddling state borders.[9] The geographic sample is shown in Figure 3. We choose to restrict our sample period to 2001-2006. We exclude years before 2001 due to lack of data on foreclosure processing days. We do not extend our sample beyond 2006 to avoid tainting our results with factors that are specific to the crisis.[10] In order to minimize noise and outliers, from each year and county we only include banks that received more than 30 applications. All these restrictions leave us with around 1.3 million loan applications made at 699 banks, in 198 counties and 42 states. Our analysis exploits the changes in regulations across 49 state borders.

Data on foreclosure laws are taken from various sources. We follow Mian *et al.* (2011) and rely on RealtyTrac for the classification of states between judicial and non-judicial. Our paper in addition relies on more direct measure of foreclosure timelines from the U.S. Foreclosure Network (USFN), which are also available online from RealtyTrac and are reproduced in the Online Appendix. The USFN measure is our favorite since, as mentioned earlier, it captures the wide variation in time-lines across states. The fact that these time-lines are constructed purely based on state regulations, i.e., not incorporating additional delays that could be endogenous to market conditions, is ideal for our empirical strategy. We also supplement our measure of judicial requirements with numbers from Freddie Mac.

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[9]See Table A1 in online appendix for a list of these urban areas.

[10]Due to the unprecedented number of foreclosures during the crisis, foreclosure laws had a real impact on the local economy (Mian *et al.* (2011)). In normal times, however, with foreclosure rates typically below 1 percent, differences in the way foreclosures are processed are unlikely to have meaningful general equilibrium effects. We will further address this issue below.

Freddie Mac provides servicers with accepted processing timelines based on observed delays. While Freddie Mac's measure can capture factors beyond regulatory differences, concerns about endogeneity are greatly limited by the fact that we study a period of low foreclosures. While Freddie Mac updated their guidelines yearly, we find that the permissible processing delays remains constant between 2001 (first available year) and 2006.[11] The USFN measure available from RealtyTrac corresponds to year 2004.[12]

Summary statistics

Table 1 presents summary statistics from each year in our sample. The first three columns show the number of urban areas, state borders, and counties in the sample. The fourth column shows the number of banks. The decreasing number of banks is in line with the consolidation in the banking industry during that period. Columns 5 and 6 show the average applicant income and loan amount respectively. A key variable from the perspective of the lender is the ratio of loan-to-income (LTI), which is shown in column 7. We see that LTI has increased during the boom period in line with the expansion in credit supply. Columns 8 shows the percentage minority borrowers. Column 9 shows that the jumbo market makes up around 10% of total loans, but that this share was higher at the peak of the boom. The final column shows the average rejection rate in our sample, which is decreasing over most of the boom period.

Next, we motivate our cross-border analysis by examining differences in economic characteristics between counties within the selected urban areas lying on different sides of the state borders. Panel A in Table 2 compares county characteristics related to income, employment, minority, construction and mortgage delinquency between the judicial and non-judicial state sub-samples. Based on the mean t-test we find no significant differences in these characteristics. In Panel B we run a regression of these characteristics on urban area fixed effects and the judicial dummy (in the first pair of columns), the USFN measure (the second pair of columns), and the Freddie Mac measure (the third pair of columns). We continue to find no significant relation between foreclosure duration and county characteristics. These results are in line with recent findings in Mian *et al.* (2011).

We next motivate the use of bank fixed effects (and thus loan-level data analysis) by comparing the banking landscape within urban areas but across state borders. We are specifically interested in understanding the extent to which lenders are homogeneous across state borders. Since urban areas are typically asymmetric in terms of number of counties on

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[11]We obtain these measures from the National Mortgage Servicers Reference Directory, see Online Appendix.

[12]We verify with the USFN that this was their earliest publication and that it was not until the crisis that states have made any meaningful changes in their regulatory timelines. This is also supported by the constancy of the Freddie Mac measure over 2001-2006.

each side of the border, we focus on county-pairs straddling state borders within these urban areas. We look at banks' operations across state borders in 2005. For the purpose of this exercise we consider all banks, ignoring our minimum limit on the number of originations. There are 801 banks operating in 157 county pairs in our sample. Among all bank and county-pair combinations for which the bank is operating in at least one of the counties, only about 40 percent of cases do we see the bank operating in both counties. For those banks which operate in both sides, market shares vary widely across borders. While it is not surprising that many state banks operate on only one side of the border, our results suggest that the difference in the banking landscape even in terms of overall volume is significant. This finding is best summarized in Figure 4. The figure plots a scatter of the market shares of bank k in counties A and B which are part of a county-pair, for all banks and county-pairs in the sample. We find that the market shares of banks are very heterogeneous within county pairs. Numerous examples of banks with market shares above 40 percent in one county but only with modest presence in the neighboring county suggest that the banking landscape can vary significantly at the state border. This heterogeneity is not related to or affected by differences in foreclosure laws, as it also holds for state borders with similar laws. Therefore, by using bank fixed effects, our empirical methodology aims at studying the impact of state foreclosure laws on lending within the average bank.

C   Results

C.1   Benchmark results

The results from the benchmark regressions are shown in Table 3. All regressions include urban area, bank, and time fixed effects. Standards errors are clustered at both the urban area and bank levels.

In the first two columns we show the coefficient estimate when we use the judicial foreclosure dummy. The first five rows in column (1) show the coefficients on the X variables which are the characteristics of the loan or borrower. The results are in line with expectations and the literature. We find that loan applications have a higher probability of rejection when they are made by applicants in lower income census tracts, with lower income, and higher loan to income ratios. The coefficient on the jumbo dummy is also positive as expected. We find that the coefficient on judicial dummy is small and far from significant. The coefficients on the other foreclosure laws are also not significant, suggesting that, on the average loan, foreclosure laws do not have a significant impact on banks' decision. As we discussed in section III(C), the absence of a significant effect of state laws could be simply a result of the GSE guarantees. We therefore differentiate between conforming and jumbo loans, and

estimate regression (2) which interacts the jumbo dummy with state laws. The results are shown in the second column. We find a positive and strongly significant coefficient on the interaction between the jumbo dummy and judicial foreclosure. The results suggest that, on average, the rejection rate of jumbo loans in judicial states is higher by 2.3 percentage points. This difference is statistically significant at the 1% level. The coefficient on the interaction between jumbo and deficiency prohibition dummy is also positive and weakly significant, suggesting an additional increase in the rejection rate on jumbo loans by around 3 percentage points in states with no recourse.

As discussed earlier, the judicial foreclosure dummy is not a sufficient statistic of the differences in foreclosure time-lines associated with the heterogeneity in the judicial proceedings across states. We therefore re-estimate the regression replacing the judicial dummy variable with the continuous measure taken from the USFN. We also try the measure from Freddie Mac of actual timelines. Note that for the purpose of comparison, we re-scale both measures in order for them to lie in the $[0, 1]$ interval. The results are shown in columns (3)-(4) for the USFN measure. The results are broadly similar. Regulatory processing timelines do not have a significant effect on the rejection rate in the full sample. Once we interact with the jumbo dummy in column (4), however, we find that their effect is significant on jumbo loans. The findings from this continuous measure suggests that the relative increase in the rejection rate on jumbo loans varies by around 3.4 percentage points between a state with zero delays (Texas scores 0.06 on the USFN rescaled measure) and a state like New York with the longest processing time (the USFN rescaled measure for NY is equal to one). Columns (5)-(6) shows similar results from the regressions in which we use the Freddie Mac timelines. These results show that whether or not states required judicial foreclosure was almost irrelevant to the supply of conforming loans, but not for jumbo loans. We find that the increase in the rejection rate above the jumbo cutoff (by around 3 percentage points) was almost twice as large in states with long judicial foreclosure timelines, suggesting a meaningful impact on credit supply.

C.2 Band samples and falsification

The estimation of the impact of foreclosure laws on the relative rejection rate of jumbo loans could be further sharpened by focusing on a subset of loans that are near the jumbo cutoff. This would zoom in the comparison on loans that are more similar in size (and thus to borrowers with more similar incomes) and will particularly help address the concern that the jumbo cutoff is simply capturing the difference in loan amounts. We therefore repeat regression (2) on subsamples of loans that are within 20%, 10%, and 5% of the jumbo cutoff, respectively. Since our focus is on the interaction term, we are also able to control for county

fixed effects rather than the urban area dummy.[13] Before presenting the results from the regressions we use a simple figure to illustrate the idea. Figure 5 shows a box-plot comparison of the distribution of those rejection rates for both conforming loans and jumbo loans in the 20% band sample, in non-judicial (left panel) and judicial states (right panel). In states not requiring judicial process, the rejection rates for conforming loans and jumbo loans are very close. However, the difference of rejection rate between jumbo and conforming loans is larger in judicial states by 2.5 percentage points for the median county (the mean rejection rate is also higher by around 1 percentage point).

The results from the band regressions are shown in Table 4. Note that we control, as stated in the table, for the characteristics of the application (X variables) but do not show them due to space constraints. The first three columns show the results when we use the judicial requirement dummy, while the next six columns replace the dummy with the USFN and Freddie Mac measures, respectively. We find a consistently positive and significant coefficient on the interaction between the jumbo dummy and the judicial requirement. The magnitude of the coefficient is consistent with the results in Table 3. While the coefficients are smaller than the benchmark results in the 20% and 10% band sample, they are bigger in the narrower 5% band sample. The coefficients on the interactions between jumbo dummy and redemption or prohibit deficiency dummies are on the other hand not significant.

In order to further confirm that our results are due to the discontinuation of the GSE guarantees at the jumbo cutoff we conduct a falsification test. Specifically, we assume an imaginary jumbo cutoff that is at 80% or 120% of the actual cutoff, respectively. We then create a new jumbo dummy based on the respective imaginary cutoff. We then repeat the band estimation using these false cutoffs in Table 5. Panel A and B show the low and high false cutoffs respectively. We find the coefficients on the interaction terms to be far from significant in all sub-samples for both cutoffs. The results strongly support our hypothesis that the impact of judicial requirement is more pronounced for jumbo loans due to the legal constraint on GSEs rather than other confounding factors such as loan size.

C.3 Demand around the jumbo cutoff

Our empirical strategy has thus far focused on addressing potentially confounding demand factors in several ways. First, we have chosen the dependent variable to be the bank's decision to reject a loan (rather than the volume of loans or their average size), a variable that is more directly linked to the supply side of the market. Second, we control for loan

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[13]We continue using the border sample since one might argue that unobservable factors could interact with the jumbo dummy, hence it is best if we minimize imbalances in the sample by focusing on these border urban areas.

and borrower characteristics, as well as county fixed effect in our regressions. Third and crucially, our results from the jumbo cutoff analysis further minimize such concerns since for these results to be demand-driven there has to be (a) a significant shift in demand around the jumbo cutoff *and* (b) the extent of these changes in demand must correlate with foreclosure laws.

HMDA data allow us to investigate our identifying assumption, since we can observe the total number of applications, i.e., the demand for loans. Thus, to address concerns that foreclosure laws, by lowering the costs of foreclosure to households, can somehow tilt the relative demand of jumbo loans and subsequently impact the rejection rate, we can examine whether one can observed a correlation between foreclosure laws and relative changes in demand.[14]

We therefore proceed to compare demand characteristics within a band around the jumbo cutoff in line with our earlier exercise. We focus on the 20% band sample, but similar results are obtained using the smaller cutoffs. We compute the ratio of the number and dollar volume of jumbo to conforming applications. We also compute the ratio of average applicant income and the ratio of the average of census tract income (HMDA reports the median census tract income). We estimate the following regression:

$$\frac{J}{NJ}_{c,u,s,t} = \alpha_u + \rho_t + \gamma Reg_s + \epsilon_{c,u,s,t} \tag{3}$$

where $\frac{J}{NJ}_{c,u,s,t}$ is the ratio of jumbo to non-jumbo loans (or other demand measures) in county c, urban area u, state s, and time t. We control for both urban and time fixed effects and control for state regulations (Reg_s). The results are shown in Table 6. We first look at the results from the specification using the judicial foreclosure dummy as a control (columns 1-4). The first column shows the results from the regression of the relative number of jumbo to non-jumbo loans on foreclosure laws, controlling for urban area and year fixed effects, and clustering standard errors at the urban area level. In the second column we replace the number of loans by the dollar volume of loans. In column (3) the left hand side variable is the ratio of the average applicant income of jumbo to non-jumbo loans. This is replaced by the ratio of the average median tract income. All coefficients on foreclosure laws, including the judicial foreclosure dummy, are far from significant levels. We repeat these same four regressions replacing the judicial foreclosure dummy with the USFN timelines (columns 5-8) and the Freddie Mac timelines (columns 9-12). The results across all these specifications indicate no significant correlation between foreclosure requirements

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[14]Note that even in the presence of relative changes in demand, absent capacity constraints, it is not clear how higher demand should lead to a higher rejection rate, controlling for borrower characteristics.

and quantitative or qualitative differences in demand around the jumbo cutoff. Table 6 brings direct evidence in support of our identifying assumption, hence also supporting our interpretation of the earlier results as being evidence of a response in the supply of credit to foreclosure costs.

C.4 Economic magnitude

Our findings naturally raise the question of whether the overall effect of judicial fore-closure on credit supply is economically meaningful. While it is impossible to estimate the general equilibrium effects in the context of our empirical model, one can nevertheless illustrate the magnitude of the effect. We choose the simplest and most tractable approach to do so. To recall, our earlier results suggest that judicial foreclosure is associated with around a two percentage point relative increase in the rejection rate on jumbo loans. Similarly, we find that processing delays increase the rejection rate by around 3 percentage points going from a state with minimum delays like Texas to a state with very long delays like New York. We can therefore illustrate the overall effect on credit, using a back-of-the-envelope calculation. We will focus on the results from the dummy variable (judicial vs. non-judicial) for simplicity.

As we have shown, the uniform guarantees by the GSEs have shielded the conforming market from the adverse effect of foreclosure laws on credit supply. We therefore limit our attention to the jumbo market. Since we are interested in shedding light on the potential aggregate effect we consider lending in all counties in the mainland United States, and not just border counties. Between 2001 and 2006 there were around 300 thousand applications to jumbo loans in judicial states, with around $150 billion in 2006 U.S. dollars. Therefore, a 2 percentage point increase in the rejection rate corresponds to around 6 thousand loans with a value of around $3 billion.

Importantly, however, this number would have been much larger according to our results had the GSEs not subsidized judicial states. Applying, as a counter-factual example, this 2 percentage point rejection rate on the total number of loans (conforming and jumbo) during that period yields around 60 thousand loans with a total volume of around $10 billion. These calculations provide a useful illustration of the impact of GSEs' cross-subsidization on overall lending. They suggest that absent the GSEs' cross-subsidization, and everything else equal, mortgage lending would have been smaller by $ 7 billion over that period (computed as the difference between the two scenarios).[15] Clearly, the cross-subsidization by the GSEs became very costly during the foreclosure crisis when foreclosure delays in states like New York reached over 800 days. This has prompted the Federal Housing Finance Agency (FHFA)

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[15]It is important not to confuse the impact of the cross-subsidization with its net cost to the GSEs which is clearly much more challenging to estimate.

to plan an increase in guarantee fees on a number of states with the longest processing delays (all of them requiring judicial foreclosure) in December 2013, a decision that was later put on hold for further study following the appointment of a new director.

C.5   Bank heterogeneity

How do bank characteristics influence the sensitivity of banks' lending decisions to foreclosure laws? In what follows we aim to shed light on this question. We are motivated by the fact that the banking industry has been, up until the crisis, on a trend of increased concentration, geographical diversification, and reliance on private securitization. One thus wonders whether these characteristics affect the way banks respond to local regulations that impose costs on their operations. In the empirical banking literature, bank size is often viewed as a potentially important explanatory variable in view of the vast heterogeneity in size. Particularly, large banks are typically viewed as special. We therefore examine the impact of bank size both linearly as well as by exploring the impact of banks in the top tier using a dummy variable.

We also explore whether banks' sensitivity to judicial foreclosure correlates with the extent of their geographical diversification. There are two motivations for this. First, geographical diversification might allow banks to cherry-pick loans based on local factors and regulations, a privilege that local banks do not have. Second, and in a similar vein to Loutskina and Strahan (2011), geographically diversified banks might invest less in soft information and hence their evaluation of risk could be disproportionally based on public information such as local regulations.

The third characteristic we study is the extent to which banks rely on private securitization. We have already established that judicial laws' effect on credit supply is mostly felt in the jumbo segment of the market. These loans are either kept on banks' balance sheet or sold for private securitization. One therefore wonders whether the sensitivity of supply to judicial laws is greater for banks that rely more on private securitization, i.e., when the unconditional probability of the loan being securitized, based on the type of the lender, is higher. One might argue that private investors are, like diversified lenders, more likely to respond to observable information since they do not have access to soft information.

Clearly, the three bank characteristics we discussed are correlated. Larger banks are also more diversified ($\rho = 0.6$) and more diversified banks tend to rely somewhat more on private securitization ($\rho = 0.15$). We therefore need to control jointly for these factors. Since our earlier regressions have shown that the impact of judicial foreclosures are mainly felt on jumbo loans, and in order to avoid using triple interactions in our regressions, we focus in this exercise on the jumbo segment of the market. We estimate the following regression using

the same cross-border sample:

$$R_{i,k,c,s,t} = \alpha_k + \beta_c + \rho_t + \theta X_{i,k,c,s,t} + \gamma_1 Reg_s Size_k +$$
$$+ \gamma_2 Reg_s Diversification_k + \gamma_2 Reg_s Securitization_k + \epsilon_{i,k,c,s,t} \qquad (4)$$

where $R_{i,k,c,s,t}$ is a dummy variable that takes the value one if the jumbo loan application i, at bank k, on a property in county c, in state s, at time t, was rejected by the bank. We control for bank and county fixed effects and for the characteristic of the applications available from HMDA (X) as discussed earlier. The key coefficients are those on the interaction between bank characteristics and the judicial foreclosure variable.

Size is computed as the log of total assets. For lender geographic diversification, we follow closely the method used in Loutskina and Strahan (2011). The index equals one minus the sum of squared shares of loans made by a bank in each county in which it operates, where the share of loans is defined as the ratio of total volume of loans in one county to the total volume of loans in all counties the bank operates in. The diversification measure ranges from 0 for lenders operating in a single county to near 1 for lenders whose lending is equally spread over a very large number of counties. Private ecuritization is computed as the share of loans that are sold to non-GSE private buyers.[16]

In order to capture potentially non-linear effects, we employ both continuous as well as dummy variables. For each of the three continuous variables discussed above, which we compute as an average of the 2001-06 period, we create a dummy variable that indicates whether the bank is in the top third of the distribution.

The results are shown in Table 7. In the first three columns we use the continuous measure. We iterate between the judicial dummy variable, the USFN measure, and the Freddie Mac measure in columns (1), (2), and (3), respectively. We find a positive and significant coefficient on the interaction between processing timelines and securitization, suggesting that banks that rely more on private securitization tend to reduce their credit more when delays are longer. Specifically, a one standard deviation (0.26) increase in securitization is associated with an increase of around 4 percentage point increase in the rejection rate when moving from a state with 0 delays to a state with maximum delays. This relation does not hold when we use the judicial dummy (first column). In columns (4)-(6) we use the dummy variable instead to capture bank characteristics. We continue to find no significant coefficients when bank characteristics are interacted with the judicial foreclosure dummy. The interactions with the timelines, on the other hand, show positive and significant coefficients on both

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[16]In order to compute the share of private securitization we rely on an entry in HMDA that indicates whether and to which type of entity the loan was sold to during the calendar year. While this could underestimate the level of securitization what matters for us is a relative measure.

the interactions with the diversification and with the private securitization dummies. The coefficients are large in magnitude, suggesting that these top tier banks in diversification and securitization are much more likely to curtail their lending in areas with long foreclosure delays.

IV   Conclusion

This paper studied the impact of foreclosure laws on the supply of mortgage credit. Specifically, we sought to understand whether and to what extent borrower-friendly foreclosure laws can have a negative impact on the willingness of banks to accept a loan application, everything else constant. Our empirical methodology has addressed the usual challenges stemming from potentially confounding demand factors. It also takes into account the institutional characteristics of the mortgage market in the U.S., specifically the role of the GSEs, using an exogenous cutoff in loan eligibility for purchase and securitization by these entities. Overlooking the role of the GSEs could lead one to underestimate the impact of borrower friendly laws on the supply of credit, given the cross-subsidization by the GSEs of borrower friendly states. Our findings point to a significant and meaningful impact of judicial foreclosure delays on the supply of mortgages. Our findings illustrate the negative indirect effect of borrower protection laws on credit and therefore help inform future cost-benefit analyses of these regulations. They also illustrate how the indirect subsidy by the GSEs to borrower-friendly states, which is currently being reconsidered, has helped mitigate the overall impact of borrower-friendly laws on credit supply.

Figure 1: States with Judicial Requirement
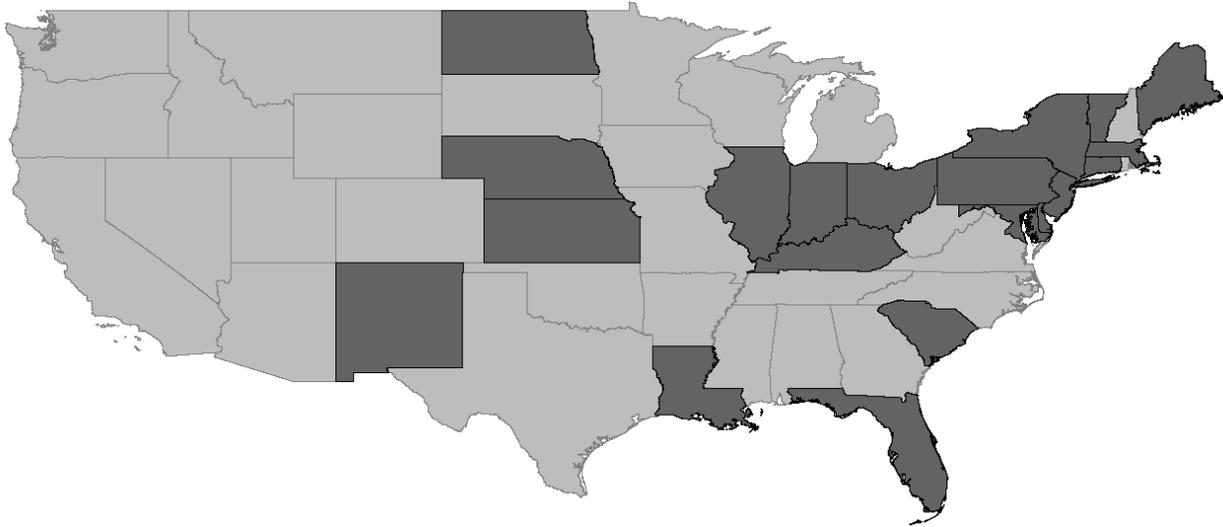
Note: This map of the U.S. mainland indicates in dark gray states that require judicial foreclosure based on RealtyTrac.

Figure 2: USFN Foreclosure Timelines Index
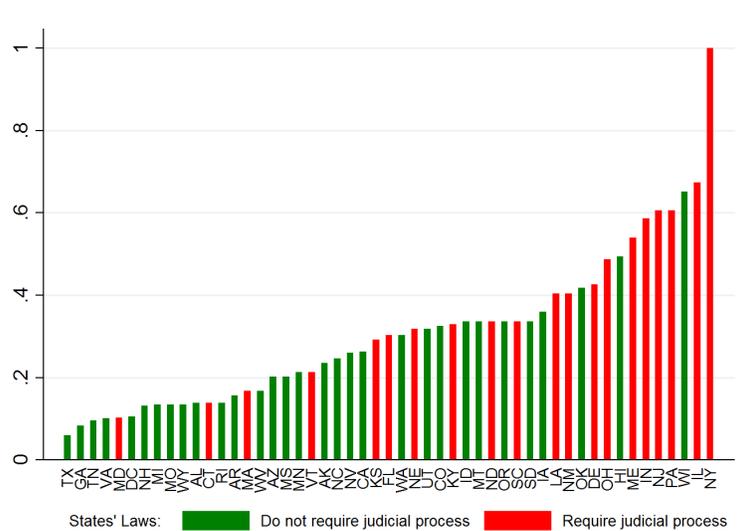
Note: This bar figure plots the foreclosure timelines index we use in our regressions. It is based on timelines in days made available by the U.S. Foreclosure Network based on states' regulatory requirements which are shown in the Online Appendix. The red (green) bars indicate that the state does (not) require judicial foreclosure.

Figure 3: Urban Areas in the Cross-border Sample
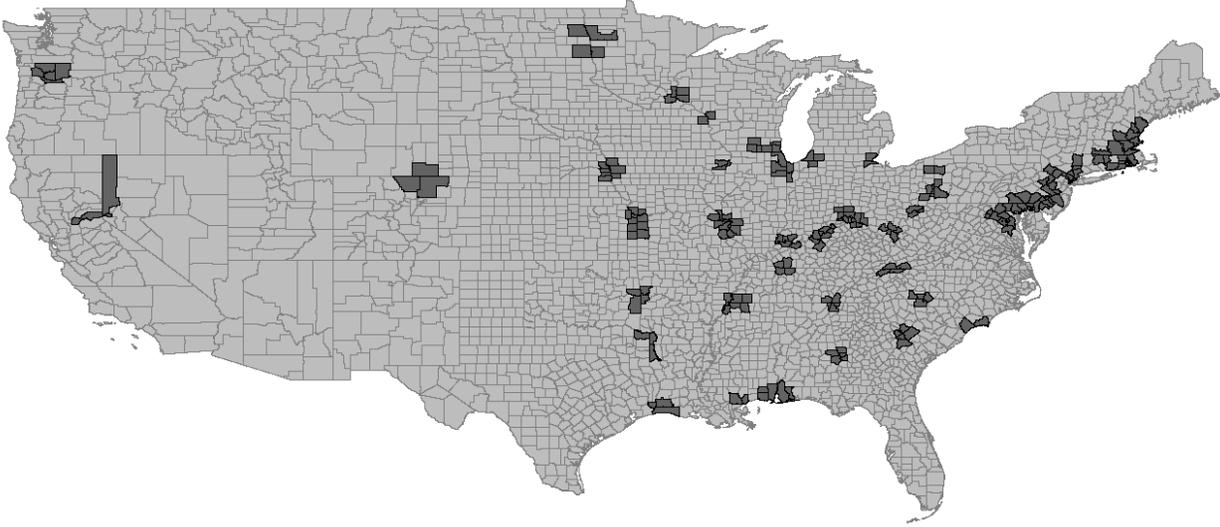
This map of the U.S. mainland indicates in dark gray our sample of urban areas which are made of contiguous metropolitan counties that straddle state borders.

Figure 4: The Market Share of Banks in Neighboring Cross-border Counties
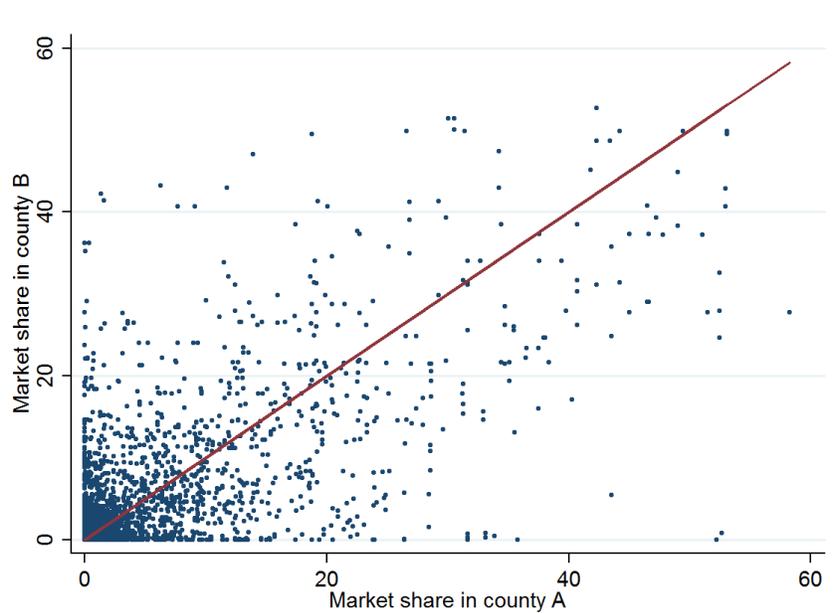
Note: For each county pair and each bank with positive lending in either of the two counties we calculate the market share of the bank in each county. The figure shows these points for all county-pairs in our sample.

Figure 5: The Distribution of Rejection Rates within a Band of Jumbo Cutoff
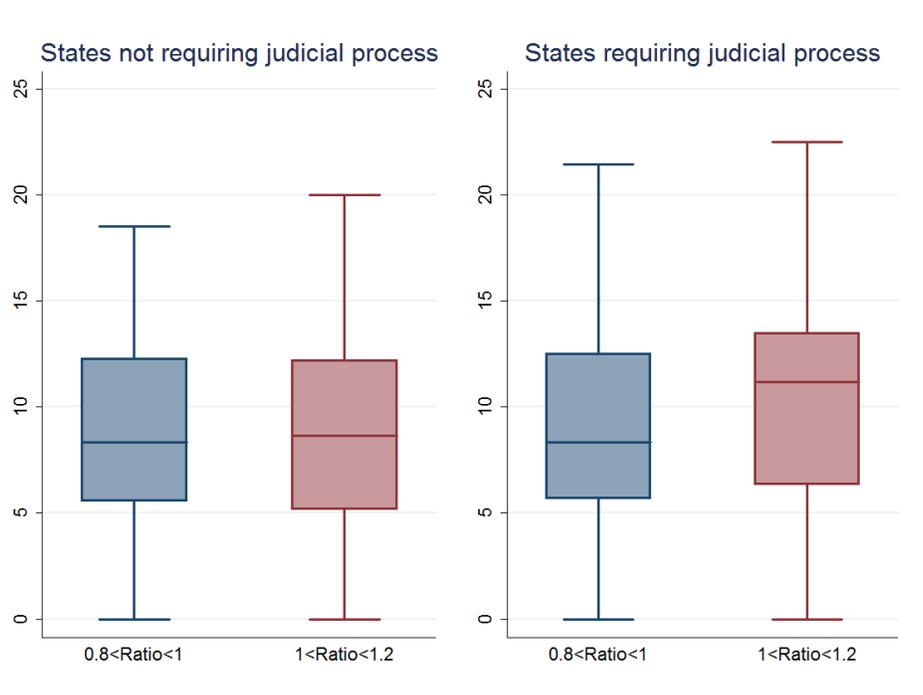
Note: This figure shows the distribution of rejection rates for both conforming loans and jumbo loans in the 20% bandwidth sample, in non-judicial states (left panel) and judicial states (right panel). In each panel the left box plot shows the distribution of rejection rate on loans that are above 80 percent and below 100 percent of the jumbo cutoff, while the right box plot shows the distribution of rejection rate on loans that are above 100 percent and below 120 percent of the jumbo cutoff.

Table 1: Summary Statistics

This table provides descriptive statistics of our final cross-border dataset on individual loans from HMDA. The first four columns show the number of urban areas, state borders, counties and banks. The following columns show the average income of applicants, the average loan amount originated, the ratio of the loan amount to income for originated loans. The table also shows the ratio of jumbo (non-conforming) loans and the average rejection rate, which is the ratio of rejected applications to the total number of approved and rejected applications.

Year	Number of urban areas (1)	Number of state borders (2)	Number of counties (3)	Number of banks (4)	Applicant income (thou. $) (5)	Loan amount (thou. $) (6)	Loan-to-income ratio (7)	Minority (%) (8)	Jumbo loans (%) (9)	Rejection rate (%) (10)
2001	52	48	188	430	91.28	145.21	2.01	13.87	8.65	11.66
2002	52	49	185	432	93.46	160.21	2.15	14.07	9.70	11.21
2003	52	48	186	432	90.48	169.04	2.29	14.32	9.57	10.92
2004	52	49	189	409	93.74	199.93	2.50	17.01	13.57	10.03
2005	52	49	187	412	98.55	208.88	2.45	19.91	13.95	11.91
2006	52	49	189	362	104.01	199.51	2.25	22.62	8.05	12.50

Table 2: Comparison of Counties in Judicial and Non-judicial States

This table provides a comparison of county characteristics between judicial and non-judicial states in the selected urban areas. Panel A compares county characteristics in the judicial and non-judicial subsamples and presents a mean t-test. Panel B presents the coefficient and the p-value from a regression of these characteristics on the judicial dummy (first pair of columns) controlling for urban area fixed effects. The second pair of columns repeats the exercise replacing the judicial dummy with the USFN timelines. Similarly, we use the Freddie Mac timelines in the last two columns.

Panel A

Variables	Judicial States	Non-judicial States	Mean Test
	Mean	Mean	p-value
Applicant income (thou. $)	81.43	83.62	0.60
Median household income (thou. $)	50.60	50.51	0.97
Per capita personal income (thou. $)	33.90	34.20	0.86
Unemployment rate (%)	5.30	5.22	0.80
Poverty (%)	11.08	11.88	0.41
Minority (%)	0.14	0.10	0.14
New house built per 1000 persons	6.95	6.69	0.85
Delinquency ratio	1.20	1.22	0.85
Foreclosure filing per 1000 properties	0.25	0.23	0.70

Panel B

Dependent variables	Judicial dummy as independent variable		USFN timelines as independent variable		Freddie Mac timelines as independent variable	
	Coefficient	P-value	Coefficient	P-value	Coefficient	P-value
Applicant income (thou. $)	-0.723	0.865	5.829	0.610	0.568	0.941
Median household income (thou. $)	1.127	0.364	-4.895	0.219	-4.834	0.146
Per capita personal income (thou. $)	0.218	0.874	0.918	0.864	-0.265	0.926
Unemployment rate (%)	0.021	0.953	-0.061	0.921	0.071	0.914
Poverty (%)	-0.604	0.325	1.524	0.529	1.234	0.479
Minority (%)	0.047	0.199	0.001	0.986	-0.014	0.664
New house built per 1000 persons	0.176	0.816	0.208	0.910	0.178	0.895
Delinquency ratio	-0.049	0.715	0.219	0.435	0.221	0.444
Foreclosures per 1000 properties	0.010	0.663	-0.005	0.933	-0.008	0.855

Table 3: Benchmark Regressions

This table presents the coefficient estimates from a linear probability model (LPM) regressing the rejection dummy on application characteristics, foreclosure laws, and bank, year and urban area fixed effects. In the first two columns the judicial foreclosure dummy is used. In the second and third pair of columns we replace the judicial dummy with the USFN and the Freddie Mac timelines, respectively. In columns (1), (3), and (5) we only control for foreclosure laws in levels, while in columns (2), (4), and (6) we additionally control for their interaction with the jumbo dummy. t-stats are reported in brackets. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Variables	(1)	(2)	(3)	(4)	(5)	(6)
Log of tract median income	-0.0449***	-0.0450***	-0.0450***	-0.0452***	-0.0449***	-0.0451***
	(-7.49)	(-7.55)	(-7.53)	(-7.64)	(-7.44)	(-7.51)
Log of applicant income	-0.0391***	-0.0391***	-0.0391***	-0.0392***	-0.0391***	-0.0392***
	(-7.00)	(-7.07)	(-7.01)	(-7.08)	(-7.01)	(-7.11)
Loan-to-income ratio	0.00405***	0.00406***	0.00406***	0.00407***	0.00405***	0.00406***
	(2.66)	(2.64)	(2.65)	(2.65)	(2.66)	(2.65)
Minority	0.0581***	0.0580***	0.0580***	0.0580***	0.0580***	0.0580***
	(7.70)	(7.64)	(7.64)	(7.63)	(7.67)	(7.67)
Female	-0.000915	-0.000943	-0.000923	-0.000970	-0.000914	-0.000961
	(-0.39)	(-0.40)	(-0.39)	(-0.41)	(-0.39)	(-0.41)
Jumbo dummy	0.0455***	0.0282***	0.0454***	0.0306***	0.0455***	0.0283***
	(4.27)	(2.94)	(4.31)	(3.23)	(4.29)	(2.78)
Judicial dummy	-0.00231	-0.00533				
	(-0.46)	(-1.02)				
USFN timelines			0.0182	0.0124		
			(0.94)	(0.70)		
Freddie Mac timelines					0.00472	0.00272
					(0.32)	(0.19)
Redemption dummy	0.0179	0.0197	0.0171	0.0179	0.0167	0.0176
	(0.85)	(0.95)	(0.79)	(0.82)	(0.80)	(0.83)
Deficiency prohibition dummy	0.00740	0.00227	0.0105	0.00712	0.00848	0.00589
	(0.77)	(0.23)	(1.26)	(0.84)	(1.00)	(0.69)
Jumbo dummy*Judicial dummy		0.0230***				
		(2.87)				
Jumbo dummy*USFN timelines				0.0339***		
				(2.75)		
Jumbo dummy*Freddie Mac timelines						0.0329***
						(2.68)
Jumbo dummy*Redemption dummy		-0.0111		-0.0109		-0.0109
		(-0.83)		(-0.83)		(-0.83)
Jumbo dummy*Deficiency prohibition dummy		0.0302*		0.0179		0.0174
		(1.70)		(1.27)		(1.26)
Observations	1300232	1300232	1300232	1300232	1300232	1300232
R-squared	0.102	0.102	0.102	0.102	0.102	0.102

Table 4: Robustness Check: Band Sample

This table presents the coefficient estimates from the LPM regression of the rejection dummy on application characteristics and the interaction between foreclosure laws and the jumbo dummy. The regressions also control for *county*, bank, and year fixed effects. The regressions are ran on subsamples of applications that fall within a band of the jumbo cutoff. For example, the 20% band sample only includes loans with size between 80% and 120% of the jumbo cutoff. In the first three columns we use the judicial dummy variable. We then replace this by the USFN measure in (4)-(6) and the Freddie Mac measure in (7)-(9). Note that we only show the coefficients of interests. We cluster standard errors at both the urban area and bank level. t-stats are reported in brackets. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Variables	(1) 20% Band	(2) 10% Band	(3) 5% Band	(4) 20% Band	(5) 10% Band	(6) 5% Band	(7) 20% Band	(8) 10% Band	(9) 5% Band
Jumbo dummy*Judicial dummy	0.0184***	0.0163**	0.0200**						
	(2.61)	(1.98)	(2.09)						
Jumbo dummy*USFN timelines				0.0290***	0.0261**	0.0396**			
				(3.09)	(2.23)	(2.30)			
Jumbo dummy*Freddie Mac timelines							0.0287***	0.0268**	0.0410**
							(2.84)	(2.14)	(2.35)
Jumbo dummy*Redemption dummy	-0.00226	-0.00452	-0.0198	-0.00248	-0.00476	-0.0192	-0.00210	-0.00429	-0.0184
	(-0.16)	(-0.24)	(-0.98)	(-0.19)	(-0.26)	(-0.88)	(-0.15)	(-0.23)	(-0.86)
Jumbo dummy*Deficiency prohibition dummy	0.0156*	0.0217	0.0167	0.00626	0.0135	0.00748	0.00563	0.0131	0.00682
	(1.88)	(1.58)	(0.91)	(0.72)	(1.12)	(0.43)	(0.64)	(1.07)	(0.39)
Observations	151625	69156	33364	151625	69156	33364	151625	69156	33364
R-squared	0.139	0.147	0.156	0.139	0.147	0.156	0.139	0.147	0.156

Table 5: Robustness Check: Falsification Test

Each panel of this table presents the coefficient estimates from the same regressions in Table 4 with the exception that jumbo cutoff is chosen to be, as a falsification test, at 80% (Panel A) or 120% (Panel B) of the actual cutoff. We continue to control for application characteristics, county, bank, and time fixed effects and cluster standard errors at both the urban area and bank level. t-stats are reported in brackets. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Panel A: 80% of jumbo cutoff	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	20% Band	10% Band	5% Band	20% Band	10% Band	5% Band	20% Band	10% Band	5% Band
False dummy*Judicial dummy	0.00723	0.00332	-0.000594						
	(1.35)	(0.59)	(-0.07)						
False dummy*USFN timelines				0.00889	0.00429	0.00377			
				(1.38)	(0.52)	(0.35)			
False dummy*Freddie Mac timelines							0.00553	0.00199	0.00362
							(0.92)	(0.24)	(0.34)
False dummy*Redemption dummy	-0.00513	-0.00171	0.00258	-0.00545	-0.00181	0.00317	-0.00574	-0.00206	0.00325
	(-0.59)	(-0.23)	(0.27)	(-0.57)	(-0.24)	(0.34)	(-0.60)	(-0.27)	(0.34)
False dummy*Deficiency prohibition dummy	-0.00234	-0.00689	-0.00647	-0.00635	-0.00870	-0.00571	-0.00680	-0.00897	-0.00582
	(-0.25)	(-0.88)	(-0.53)	(-0.74)	(-1.23)	(-0.54)	(-0.80)	(-1.27)	(-0.54)
Observations	223607	109493	54314	223607	109493	54314	223607	109493	54314
R-squared	0.125	0.132	0.139	0.125	0.132	0.139	0.125	0.132	0.139

Panel B: 120% of jumbo cutoff	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	20% Band	10% Band	5% Band	20% Band	10% Band	5% Band	20% Band	10% Band	5% Band
False dummy*Judicial dummy	0.00117	-0.00578	-0.000187						
	(0.18)	(-0.82)	(-0.02)						
False dummy*USFN timelines				0.00995	-0.0108	-0.00456			
				(1.19)	(-0.92)	(-0.22)			
False dummy*Freddie Mac timelines							0.0105	-0.0107	-0.00603
							(1.11)	(-0.86)	(-0.27)
False dummy*Redemption dummy	0.0209	0.0138	0.0369	0.0219	0.0136	0.0364	0.0222	0.0135	0.0361
	(1.11)	(0.68)	(1.45)	(1.20)	(0.67)	(1.42)	(1.19)	(0.66)	(1.40)
False dummy*Deficiency prohibition dummy	-0.000662	-0.00951	-0.000198	-0.000156	-0.00692	-0.000677	-0.000329	-0.00666	-0.000733
	(-0.05)	(-0.58)	(-0.01)	(-0.01)	(-0.38)	(-0.03)	(-0.02)	(-0.36)	(-0.03)
Observations	114193	43311	21112	114193	43311	21112	114193	43311	21112
R-squared	0.145	0.144	0.145	0.145	0.144	0.145	0.145	0.144	0.145

Table 6: Robustness Check: Test for Relative Demand

This table presents coefficient estimates from regressions of ratios of jumbo to conforming loans on foreclosure laws, and urban and year fixed effects. The subsample used is made of loans that are within the 20% band of the jumbo cutoff. In columns (1)-(4) we use the judicial dummy as the key explanatory variable. The dependent variables in these columns are: the ratio of the number of jumbo loans to that of conforming loans, the ratio of their volume, the ratio of the average applicant income in each of these segments, and the ratio of the average tract median income. In columns (5)-(8) we replace the judicial dummy with the USFN timelines, while we use the Freddie Mac timelines in (9)-(12). We cluster standard errors at the urban area level. t-states are reported in brackets. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Variables	(1) Number of Loans	(2) Volume of Loans	(3) Applicant income	(4) Tract median income	(5) Number of Loans	(6) Volume of Loans	(7) Applicant income	(8) Tract median income
Judicial dummy	0.0233	0.00487	0.00249	-0.00145				
	(0.82)	(0.23)	(0.69)	(-0.98)				
USFN timelines					-0.0538	-0.0640	0.00925	-0.00371
					(-0.67)	(-1.23)	(1.12)	(-1.00)
Redemption dummy	0.0243	0.0320	0.00322	0.00181	0.0256	0.0273	0.00465	0.00115
	(0.40)	(0.75)	(0.37)	(0.61)	(0.50)	(0.69)	(0.58)	(0.40)
Deficiency prohibition dummy	0.00562	0.0233	0.00220	-0.00225	-0.0158	0.0106	0.00233	-0.00214
	(0.10)	(0.51)	(0.19)	(-0.56)	(-0.26)	(0.21)	(0.22)	(-0.56)
Observations	1151	1167	965	948	1151	1167	965	948
R-squared	0.156	0.143	0.073	0.094	0.156	0.145	0.074	0.094

Variables	(9) Number of Loans	(10) Volume of Loans	(11) Applicant income	(12) Tract median income
Freddie Mac timelines	-0.0428	-0.0397	0.00502	-0.00434
	(-0.78)	(-1.03)	(0.69)	(-1.30)
Redemption dummy	0.0245	0.0275	0.00442	0.000933
	(0.48)	(0.68)	(0.56)	(0.35)
Deficiency prohibition dummy	-0.00748	0.0207	0.000949	-0.00167
	(-0.13)	(0.39)	(0.10)	(-0.47)
Observations	1151	1167	965	948
R-squared	0.156	0.144	0.073	0.095

Table 7: Bank Characteristics and Sensitivity to Foreclosure Laws

This table presents coefficient estimates from an LPM regression of the rejection dummy on application characteristics, the interaction of judicial foreclosure with bank characteristics, and county, bank, and year fixed effect. Bank size is the log of banks' total assets. Diversification is one minus the sum of squared shares of loans made by a bank in each county in which it operates. Securitization is computed as the share of loans that are sold to private securitization. In the columns indicating 'Continuous variable' we use bank characteristics as measured. In the columns indicating 'Dummy variable' we instead use a dummy variable that takes one if the bank is in the top third of the distribution. Standard errors are clustered at the bank and urban area level. t-stat are reported in brackets. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Continuous variable			Dummy variable		
Judicial dummy*Bank size	0.00148			0.00208		
	(0.30)			(0.08)		
Judicial dummy*Diversification	0.00615			0.0185		
	(0.07)			(0.85)		
Judicial dummy*Securitization	0.0365			0.0219		
	(1.36)			(1.00)		
USFN timelines*Bank size		0.000454			-0.0162	
		(0.05)			(-0.51)	
USFN timelines*Diversification		-0.0621			0.162***	
		(-0.29)			(3.74)	
USFN timelines*Securitization		0.161***			0.0995***	
		(7.94)			(5.04)	
Freddie Mac timelines*Bank size			-0.00214			-0.0157
			(-0.20)			(-0.49)
Freddie Mac timelines*Diversification			-0.0538			0.210*
			(-0.30)			(1.93)
Freddie Mac timelines*Securitization			0.158***			0.0971***
			(6.25)			(4.67)
Observations	112941	112941	112941	112948	112948	112948
R-squared	0.141	0.143	0.142	0.141	0.143	0.142

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