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Recourse restrictions and judicial foreclosures: Effects of mortgage law on loan price and collateralization

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ABSTRACT

Borrower-friendly laws, such as recourse restrictions and judicial foreclosures, impose higher costs and risks to lenders. Yet, there is little evidence on how lenders transfer them to borrowers at the mortgage origination. By exploiting the mortgage law heterogeneity across U.S. states, I show that recourse restrictions trigger a collateral channel, through which lenders require a 1.6 to 1.9 percentage points lower loan-to-value ratio to compensate for worse recovery opportunities and respective higher expected loss. This effect holds both before and after the Great Recession, and is robust to a regression discontinuity design approach. I also find that lenders do not penalize strategic defaults when recourse is not allowed. Regarding the impact of judicial requirements, the findings are mixed.

1. Introduction

Does mortgage law influence loan contract terms? There are reasons to think that it does. If the law imposes higher risks or costs to lenders, it is reasonable to assume that lenders will pass on these costs to borrowers by raising interest rates or requiring higher loan collateralization. Still, not all aspects of mortgage law should induce the same effects. While some aspects may change the borrower's probability of default, others may lead to higher recovery costs or longer recovery processes.

Despite its relevance to policy decisions, there is little empirical evidence of how different laws affect mortgage price and collateralization. Most of the contributions studying the influence of the law on mortgage credit focus on the impact on loan size, ignoring other loan contract terms. This is generally due to a lack of microdata on interest rates and house values. In this paper, I address this gap by using an alternative loan-level dataset for the U.S. mortgage market to trace how recourse restrictions and judicial foreclosures influence the mortgage interest rate and the loan-to-value ratio at the origination.

Similar to prior research (e.g., Pence, 2006; Ghent and Kudlyak, 2011; Mian et al., 2015; and Milonas, 2017), I exploit mortgage law heterogeneity across different U.S. states. I classify each state based on two aspects of the law: recourse (formally known as deficiency judgments) and foreclosure procedure. First, state law might not allow for deficiency judgments. If so, the lender cannot seize other assets or income of the borrower to recover the debt. Besides limiting recovery opportunities, this encourages strategic defaults. I classify such states as non-recourse. Second, state law might require a court's approval to initiate foreclosure, which delays the process and makes it costlier. I classify such states as judicial.

In order to illustrate how recourse restrictions and judicial foreclosures affect loan contract terms, I introduce a conceptual framework for mortgage origination that considers the distinct motivations of lenders and borrowers. This framework highlights the role of the mortgage interest rate and the loan-to-value ratio in determining the loan's price and risk. Depending on how the law impacts the loan's expected loss, lenders might either raise the mortgage interest rate (price channel) or demand greater collateralization (collateral channel).

As for the data, comprehensive datasets for the U.S. mortgage market, such as the Home Mortgage Disclosure Act dataset (HMDA),

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do not have historical information on mortgage pricing and, thus, are unsuitable for this study. To analyze the effect on mortgage interest rates and loan-to-value ratio, I use Freddie Mac's Single-Family Loan-Level data from 2001 to 2019 and show that qualitative findings hold for a broader representation of the mortgage market. Given the mortgage market crisis and the Federal Housing Finance Administration's (FHFA) decision to place Freddie Mac under conservatorship, two distinct sample periods are utilized, with loans granted during the Great Recession (2007 to 2011) being excluded. The interventions during that period were part of a sizable mortgage finance reform with significant changes in the mortgage market (see, for example, Duca et al., 2016).

After examining the potential endogeneity concerns related to the law and the simultaneity of the loan-to-value ratio and the mortgage interest rate, I find that the impacts of mortgage laws are not always similar before and after the Great Recession. Consistently, non-recourse laws led to more collateralized loans with, on average, loan-to-value ratios 1.6 to 1.9 percentage points lower and no significant effect on the interest rate. Therefore, lenders require more collateral to compensate for a lower expectation of recovery, activating a collateral channel. On the other hand, judicial foreclosure requirements seem to prompt higher interest rates and lower loan-to-values before the Great Recession, and a counterintuitive reduction of the interest rates after 2011. This puzzling result hints that studying the implications of the judicial foreclosure processes during the Great Recession might prove to be a promising avenue for future research. Nevertheless, this is out of the scope of this study.

By focusing on the impact of recourse restrictions on the loan's collateralization levels, I apply a regression discontinuity design approach that furthers robust the findings. I also examine if lenders demand more collateral in non-recourse states as a way to decrease the likeliness of mortgages going into negative equity, and so deter strategic defaults. To investigate this, I use a quantile regression model. Without evidence of this phenomenon, the findings suggest that lenders require more collateral in non-recourse states only to compensate for fewer recovery opportunities.

This paper relates to extensive empirical literature studying the effects of mortgage law heterogeneity on the supply of mortgage credit, mortgage default, and foreclosure.

Regarding the supply of mortgage credit, most contributions measure the law's impact through loan size. Pence (2006) studies loans originated between 1994 and 1995 and finds that loan sizes are 3 to 7 percent smaller in judicial states. Mian et al. (2015) apply a similar methodology to loans originated in 2005 but find no evidence that average loan sizes or total lending are significantly lower in judicial states. They argue that the effect described by Pence (2006) is diluted over time. A few studies also explore the impact of laws on loan prices, though using aggregate data at the state level. Ghent and Kudlyak (2011) and Pruszkowski (2017) find no difference in mortgage interest rates between recourse and non-recourse states. Ambrose et al. (2004) use loan-level data on 26,179 loans from 1995 to 1997, provided by a national lender. Surprisingly, they find lower interest rates and higher loan-to-value ratios in non-recourse states.

As to the role of mortgage law heterogeneity in mortgage default and foreclosure, in the case of judicial requirement, Mian et al. (2015) find that in non-judicial states, lenders were twice as likely to proceed with the foreclosure during the 2007 mortgage crisis, even if default rates were no different. Desai et al. (2013) show that the effects of judicial requirements and non-recourse laws on default and foreclosure rates are strongest for subprime mortgages and adjustable-rate mortgages. Gerardi et al. (2013) argue that judicial requirements delay the foreclosure process, which for Melzer (2017) creates a debt-overhang problem. In the case of non-recourse law, Gete and Zecchetto (2022) assume that recourse discourages default and argue that countries with mortgage recourse systems face higher nominal rigidities that lead to deeper and more persistent recessions. Other authors ask if negative equity is enough for default (known as strategic default). Ghent and Kudlyak (2011) find that borrowers facing negative equity are more likely to default in non-recourse states, but only for home values above $200,000. Guiso et al. (2013) find evidence of strategic default and conclude that personal values also play a role in the decision. Demiroglu et al. (2014) inspect whether judicial foreclosures interact with non-recourse to affect the likelihood of default on residential mortgages and conclude that it is only significant for borrowers with negative equity.

The remainder of the paper proceeds as follows. The next section describes the mortgage law heterogeneity in the U.S. In Section 3, I present a conceptual framework of mortgage origination and the respective influence of mortgage law. Section 4 describes the data, Section 5 the empirical strategy, and Section 6 the results. Section 7 shows the regression discontinuity design approach on the impact of recourse restrictions on the loan-to-value ratio. Section 8 explores the effect of non-recourse along the conditional distribution of the loan-to-value ratio. Section 9 presents the conclusions.

2. Mortgage law heterogeneity in the U.S.

A mortgage contract gives the lender the right to foreclose the home if the borrower defaults. In the U.S., the foreclosure procedure is governed mostly by state law and reveals a significant heterogeneity among states. After a depth review of U.S. mortgage law history, Ghent (2014) concluded that the roots of its heterogeneity go back to the nineteenth century and have no connection with economic factors.[2]

Following the National Mortgage Servicer's Reference Directory (NMSRD) published by USFN (2019), I characterize differences across states along two aspects of the law: deficiency judgments and foreclosure procedure.[3,4] First, as for *deficiency judgments*, when the debt value exceeds the property's market value, the lender may collect a deficiency judgment to pursue the borrower personally. With a deficiency judgment, the lender can seize the borrower's unsecured personal assets and future income to recover the debt value (Harris and Meir, 2015). This possibility is usually known as recourse and might be automatic, require a judicial decision, or even be forbidden. States that do not allow recourse are more pro-borrower (Ghent, 2014).

The dummy variable $NonRecourse$ describes the recourse restriction classification. If a state allows deficiency judgments through a process without significant obstacles, it is classified as recourse ($NonRecourse = 0$). All other cases are classified as non-recourse ($NonRecourse = 1$).

Second, as for the *foreclosure procedure*, when a borrower becomes delinquent on his mortgage, and the lender wants to proceed with the foreclosure, the law may require a court's approval or allow a nonjudicial procedure. In states that allow procedures that do not require courts' approval (for example, power-of-sale foreclosures), lenders often choose a nonjudicial option to speed up the process and avoid judicial costs. Fig. 1 shows the estimated foreclosure timelines published

[2] According to Ghent (2014), laws show "remarkable persistence", as they require changes to the civil code of procedures and cannot be promptly adjusted to the economic conditions.

[3] The NMSRD is a reference in the mortgage industry and provides information on state foreclosure processes and regulations. I consider the 50 U.S. states plus the federal District of Columbia to study state-level differences. Appendix A presents state classification for each mortgage law aspect. Appendix B presents differences in the classification across different sources and studies.

[4] Another aspect of the law is homestead exemptions, which protect homeownership after a bankruptcy filing. The value of the exemption varies according to state and only protects households in case of unsecured debt. It does not directly apply to mortgages (secured loans), and it does not prevent foreclosure. However, some authors recognize that homestead exemptions may influence mortgage default incentives or delay the foreclosure process if households simultaneously hold a positive equity mortgage and unsecured debt (see, for example, Li et al., 2011; Cao, 2014; and Hintermaier and Koeniger, 2016). This indirect effect only applies to a small, yet not identifiable, group of households, and, thus, homestead exemption values are not considered in this study.

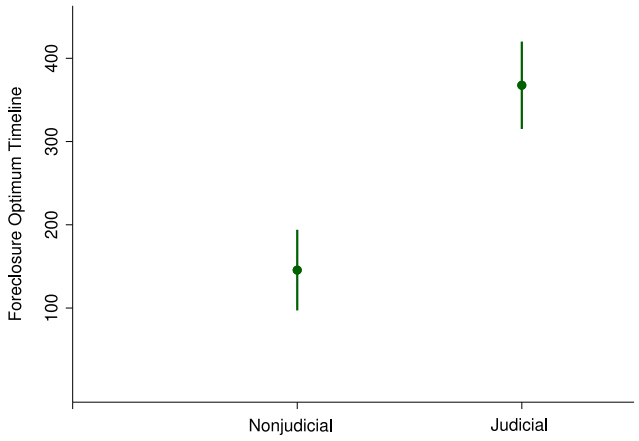

Fig. 1. Estimated foreclosure timeline for nonjudicial and judicial states.
This figure presents the estimated foreclosure optimum timeline for a 95% confidence interval in judicial and nonjudicial states classified as of USFN (2019) and USFN (2022) data. The foreclosure process is, on average, 222 days longer in judicial states. The foreclosure timelines used in this estimation apply to uncontested foreclosure actions that have been referred to with all necessary documentation and are based on the best-case scenario without delays.

by USFN (2022) according to the distribution of judicial states. On average, the foreclosure process is 222 days longer in judicial than in nonjudicial states, which implies significant losses for the lenders. Thus, states that demand judicial procedures are more pro-borrower (Ghent, 2014).

The dummy variable $Judicial$ describes the foreclosure procedure classification. If a state requires a court's approval to proceed with foreclosure, it is classified as judicial ($Judicial = 1$). If judicial foreclosure is not mandatory or nonjudicial is the only option, then the state is classified as nonjudicial ($Judicial = 0$).

Fig. 2 shows the distribution of recourse restrictions and judicial requirements across U.S. states. The distribution of $Judicial$ is balanced, but there is a noticeable minority of $Non-Recourse$ states. Only three states concurrently endorse the two borrower-friendly laws.

Given the heterogeneity of laws, the question that follows is if states with different mortgage laws are also different regarding other essential dimensions. Although Ghent (2014)'s study has established that there is no simultaneous endogeneity in the adoption of laws due to their *remarkable persistence*, it is essential to consider the structural and systematic differences across states that could undermine the assumption that variations in mortgage laws are orthogonal to relevant economic and social conditions. To ensure that mortgage laws are not endogenous to other attributes that independently influence the loan price and the collateralization, I follow an approach similar to Mian et al. (2015). Specifically, I examine whether states with non-recourse (judicial) laws are significantly distinct from other states in terms of a set of economic and social attributes grouped into four categories: (1) demographic and income; (2) GDP; (3) housing market; and (4) mortgage market variables. As shown in Appendix C, the results indicate that the orthogonality assumption holds for most indicators with some exceptions as for the mortgage balance delinquency rate, particularly in the period after the Great Recession.

3. Mortgage origination: a conceptual framework

A mortgage loan is a complex contract between the lender and the borrower, that needs agreement on four key features: loan amount (quantity), interest rate (price), house value (collateral), and loan maturity (term). To provide a clear and concise understanding of the relationships between these variables, assume that lenders operate in perfect competition and with unrestricted access to funding at the rate of r in the interbank market. Assume also a one-period maturity for all loans.

Lenders operating within this framework are willing to offer loans at an interest rate of r^L under the condition that the difference between this rate and the cost of funds ($r^L - r$) is sufficient to cover the expected loss in case of borrower's default. This expected loss is the product of two variables: the borrower's probability of default PD and the loss given default LGD. The first depends on the borrower's characteristics and incentives to default. In contrast, the latter depends on the features of the loan (namely, the loan-to-value ratio LTV) and the ability of the lender to recover the debt. In this setting, both the return and the risk of the loan are measured in relative terms and thus expressed as the mortgage interest rate spread ($r^L - r$) and the LTV.

On the borrowers' side, households look for a loan that puts the purchase of the desired house within reach of their budget and, thus, simultaneously decide the loan amount and the house value.[5] In other words, when choosing a house, households also consider the LTV that ensures loan acceptance, while evaluating the mortgage cost r^L, and their preferences and needs. Altogether, for both the lender and the borrower, we can resume the deal as a combination of two jointly determined variables: the mortgage interest rate spread and the LTV.

This conceptual framework provides a structured approach that helps to understand the potential impact of mortgage law on loan price and collateralization. On the one hand, if mortgage law promotes borrowers' incentives to default, then the PD at the mortgage origination will be higher. On the other hand, if mortgage law imposes higher costs and risks to lenders, then the estimation of the LGD at the mortgage origination will be higher. From the combination of both, in states with borrower-friendly laws, one will expect that lenders react to the increased expected loss by adjusting the mortgage price r^L to get a higher risk premium (price channel) and/or changing the collateral requirements LTV to decrease the expected loss of the loan (collateral channel).

In non-recourse states, the house value (collateral value) limits debt recovery. With lower recovery opportunities, lenders face a higher LGD when compared to recourse states. In addition, borrowers might have incentives to default strategically if they go into negative equity, which would increase the PD. According to Ghent and Kudlyak (2011), borrowers with properties appraised at more than $\$200,000$ are 30% more likely to default in non-recourse states.

In judicial states, lenders face additional costs. Different authors recognize this increase in the foreclosure costs, which can go as high as 10% of the loan balance (Pence, 2006). Cerqueiro et al. (2016) also recognize that legal mechanisms influence the value of the collateral as they determine when and how the secured assets can be seized. Higher costs in recovering the debt imply a higher LGD when compared to nonjudicial states.

On the borrowers' side, judicial foreclosure provides more protection and delays the foreclosure process, but it should not change borrowers' incentives to default.

4. Data

This paper combines a variety of data sources. Besides NMSRD for the mortgage law, I use Freddie Mac's Single-Family Loan-Level Dataset for data on individual mortgages and FRED Economic Data for market interest rates.[6]

Freddie Mac Single-Family Loan-Level Dataset comprises a portion of single-family mortgages acquired or guaranteed by Freddie Mac.

[5] The idea of a simultaneous decision on the loan amount and house value is asserted by Brueckner (1994).

[6] Freddie Mac's Single Family Loan-Level Dataset available in http://www.freddiemac.com/research/datasets/sf_loanlevel_dataset.page as of December 2022. Details on the dataset are available in Freddie Mac (2020).

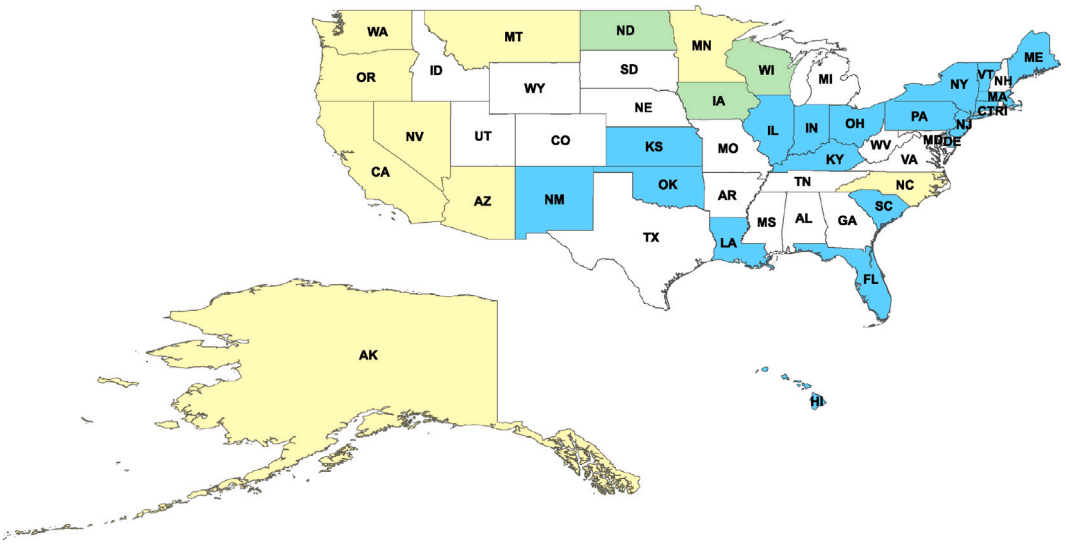

Fig. 2. Distribution of recourse restrictions and judicial requirements.
This figure presents the distribution of recourse restrictions and judicial requirements across states based on USFN (2019) data as of 2019. States in yellow have recourse restrictions, states in blue require a judicial foreclosure, and states in green have both. In 2009, the state of Nevada passed a new law that abolished deficiency judgments on primary residence single-family mortgages originated after October 1, 2009 (Li and Oswald, 2017). However, this change in the law emerged as a response to the mortgage crisis and cannot be considered a random (not policy-oriented) change in the law. From 2001 to 2019, only three states – District of Columbia, Hawaii, and Vermont – justified changes in the judicial classification, and all of them from nonjudicial to judicial. For more details on the classification and changes between 2001 and 2019, see Appendix A and Appendix B. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

The data is publicly available for research; it includes fully amortizing fixed-rate mortgages and has the comparative advantage of having information on interest rates.[7]

Regarding the time frame, I focus on two sample periods: the pre-Great Recession period (2001–2006) and the subsequent and pre-pandemic period (2012–2019). The loan origination information is available by quarter/year. The selected sample includes mortgages granted for the purchase of a single-family primary residency, a minimum amount of $\$50,000$, original term between 25 and 35 years, an identified seller, and whose associated property is located in one of the states considered in Section 2. In addition to the variables directly provided, I compute the borrower's monthly income ($Income$) through the monthly installments and the debt-to-income ratio. I also compute the loan-to-income ratio as the loan amount per dollar of monthly income (LTI).

All loans in the sample are fixed-rate mortgages. To compare fixed interest rates from different periods and isolate the credit risk premium, I follow an approach similar to Basten et al. (2018) and compute the $Spread$ as the difference between the mortgage interest rate and the refinancing costs under full hedging of interest rate risk. In their work, the interest rate risk immunization strategy contemplates the repricing period as a reference for the interest rate swap maturity. However, in the U.S., fixed-rate mortgages do not have interest rate resetting or prepayment penalties.[8] Therefore, I assume that the quarterly average 30-year interest rate swap is the adequate hedging instrument to compute the $Spread$, given that the loan prepayment is unknown at origination. Since mortgage rates are usually defined before closing the deal, I lag the 30-year interest rate swap one quarter.

Tables 1 and 2 present summary statistics of the loan-level data used for the two sample periods.[9] For a similar number of observations, the tables reveal an increase in the $Spread$ charged after the Great Recession, greater leverage (either measured by the LTV or LTI), and a generalized increase in all absolute variables (such as $Amount$, $Value$, and $Income$). There was also an increase in the number of first-time and single borrowers. The sample is fairly distributed among recourse and non-recourse states, with 26% and 29% of loans subject to a non-recourse regime. It is well distributed among judicial and nonjudicial states, with 45% and 43% of loans subject to judicial foreclosure. Tables E1 and E2 in Appendix E present the summary statistics by the aspect of the law.

5. Empirical strategy

Identifying the effect of mortgage law is challenging because there is no significant and exogenous variation in state laws over time. Moreover, there are at least three dimensions of endogeneity that pose challenges to the identification. First, if legislators are prone to enact borrower-friendly laws by simultaneously adopting judicial foreclosure and recourse restrictions, it can be challenging to disentangle the effects of both laws. Second, states with judicial foreclosure laws or recourse restrictions may be systematically different regarding other unobserved variables. Finally, the mortgage interest rate and the LTV ratio are jointly determined.

The first two endogeneity dimensions are approached in Section 2. The endogeneity related to the simultaneity of the outcome variables is bypassed through a reduced-form model of equations, as recommended by Wooldridge (2019). The approach is similar to Gambacorta and Mistrulli (2014) and implies the estimation of reduced-form equations for the mortgage interest rate spread and the loan-to-value ratio. Eqs. (1)

[7] It excludes government-insured mortgages, affordable mortgages, adjustable-rate mortgages, mortgages with credit enhancements other than primary mortgage insurance, and mortgages without verified documentation. Appendix D discusses the caveats and opportunities of using Freddie Mac's data.

[8] To immunize interest rate risk, lenders consider the possibility of principal prepayment. However, as it is an uncertain event that varies with the interest rate level and economic environment, the duration computation will depend on the specification of the option exercise (Mattey, 2000). As a robustness check, I assume that the prepayment occurs after ten years and compute the $Spread$ as the difference between the mortgage interest rate and the 10-year Treasury yield.

[9] As Tables 1 and 2 show, some loans have a negative $Spread$. These are very few, resulting from computing the $Spread$ as the difference to the swap rate. I decided to maintain the loans in the sample to avoid discretionary adjustments.

Table 1
Sample I (2001–2006) - Summary statistics.

	mean	sd	min	max	Description
Spread	73	41	−356	474	Mortgage spread to previous period 30Y swap rate (bps)
Amount	171,923	78,185	50,000	802,000	Mortgage amount ($)
Value	230,537	125,074	50,000	3,878,571	House value ($)
LTV	78.08	13.95	7.00	105.00	Loan-to-value ratio (pp)
LTI	57.89	18.41	1.27	129.37	Loan-to-income ratio
CreditScore	724	54	300	850	Credit score
Income	3,320	2,508	429	264,418	Borrowers' monthly income ($)
DFirstTime	0.25	0.43	0	1	=1 if First-time homebuyer
DSingleBorrower	0.40	0.49	0	1	=1 if Single borrower
NonRecourse	0.26	0.44	0	1	=1 if in a non-recourse state
Judicial	0.45	0.50	0	1	=1 if in a judicial state
Observations		1,593,119			

Table 2
Sample II (2012–2019) - Summary statistics.

	mean	sd	min	max	Description
Spread	155	51	−54	454	Mortgage spread to previous period 30Y swap rate (bps)
Amount	249,365	124,942	50,000	1,397,000	Mortgage amount ($)
Value	306,211	174,453	50,505	4,276,471	House value ($)
LTV	84.23	12.58	6.00	105.00	Loan-to-value ratio (pp)
LTI	71.47	18.33	1.92	118.75	Loan-to-income ratio
CreditScore	749	41	517	840	Credit score
Income	3,727	2,302	449	210,719	Borrowers' monthly income ($)
DFirstTime	0.47	0.50	0	1	=1 if First-time homebuyer
DSingleBorrower	0.52	0.50	0	1	=1 if Single borrower
NonRecourse	0.29	0.45	0	1	=1 if in a non-recourse state
Judicial	0.43	0.50	0	1	=1 if in a judicial state
Observations		1,589,049			

and (2) set the estimation of $Spread$ and LTV for loan i, seller j, and year t

$$\begin{aligned} Spread_{i,j,t} = {} & \beta_{10} + \beta_{11} Log(Income)_i + \beta_{12} Log(Income)_i^2 + \beta_{13} LTI_i \\ & + \beta_{14} CreditScore_i + \beta_{15} DSingleBorrower_i + \beta_{16} DFirstTime_i \\ & + \beta_{17} NonRecourse_{i,t} + \beta_{18} Judicial_{i,t} \\ & + \Phi_{1t} + \Omega_{1j} + \epsilon_{1i,j,t}, \end{aligned} \tag{1}$$

$$\begin{aligned} LTV_{i,j,t} = {} & \beta_{20} + \beta_{21} Log(Income)_i + \beta_{22} Log(Income)_i^2 + \beta_{23} LTI_i \\ & + \beta_{24} CreditScore_i + \beta_{25} DSingleBorrower_i + \beta_{26} DFirstTime_i \\ & + \beta_{27} NonRecourse_{i,t} + \beta_{28} Judicial_{i,t} \\ & + \Phi_{2t} + \Omega_{2j} + \epsilon_{2i,j,t}. \end{aligned} \tag{2}$$

Time fixed-effects Φ control for nationwide shocks that might influence demand or supply. Seller fixed-effects Ω control for heterogeneity in interest rate setting and collateral requirements due to seller's specific characteristics of liquidity, capitalization, and relationship lending (Gambacorta, 2008). Residuals ϵ are clustered by the seller to correct for time-varying seller's strategies. The relevant coefficients from a policy perspective are β_{17} and β_{27} for recourse restrictions, and β_{18} and β_{28} for judicial foreclosure requirements.

Non-recourse might induce strategic defaults and limit debt recovery options. As a result, lenders might: (1) require a risk premium (price channel: $\beta_{17} > 0$) to compensate for the higher expected loss; (2) demand higher collateralization (collateral channel: $\beta_{27} < 0$), whether to compensate for the higher LGD or decrease the probability of a negative equity scenario; (3) or not react ($\beta_{17} = \beta_{27} = 0$). On the other hand, judicial foreclosures imply higher costs in case of default. Therefore lenders might: (1) require a risk premium (price channel: $\beta_{18} > 0$) to compensate for the higher recovery costs; (2) require more collateral (collateral channel: $\beta_{28} < 0$); (3) or not react ($\beta_{18} = \beta_{28} = 0$). The two channels are coherent with the evidence in Cerqueiro et al. (2016) on collateral functions and the impact of legal tools.

6. Results

Table 3 presents the OLS estimation of the reduced-form Eqs. (1) and (2) for the period between 2001 and 2006. Before the Great Recession, there was no evidence that non-recourse law increased the mortgage spread (β_{17} not significantly different from 0). In contrast, judicial requirements significantly increased the mortgage spread (positive β_{18}). This suggests lenders required monetary compensation for the increased foreclosure costs but not for the lower recovery opportunities and increased probability of default. On average, the mortgage interest rate in judicial states is approximately 4.4 to 5.0 basis points higher than in nonjudicial ones.

During this period, law effects on the collateralization level are also evident. Both non-recourse laws and judicial requirements decrease the LTV at origination (negative β_{27} and β_{28}); however, the effect of the former is more than triple. Lenders require more collateralization for the lower recovery opportunities and increased probability of default. On average, the loan-to-value ratio is approximately 1.6 percentage points lower in non-recourse states.

Given the significant real estate and mortgage market changes during the Great Recession, laws might have distinctly impacted mortgage features. Table 4 presents the same estimation applied between 2012 and 2019. As for the recourse restrictions, the effects previously observed remain. In states where lenders cannot seize other households' assets or income, mortgages have, on average, a 1.9 percentage points lower loan-to-value ratio.

However, the judicial requirement has a counterintuitive, negative, and significant impact on mortgage interest rates. This begs the question: what insights can we learn from judicial foreclosures during the financial crisis? For example, despite incurring higher costs and longer processing times, did lenders recover a more significant percentage of the debt owed? Examining the mortgage recovery rates could be a promising avenue for future research. This could shed light on whether

Table 3
Sample I (2001–2006) - Effects of law on Spread and LTV.

	Spread	Spread	LTV	LTV
LogIncome	−34.91***	−32.65***	49.76***	48.83***
	(−4.54)	(−4.99)	(16.47)	(14.53)
LogIncome^2	1.350**	1.252**	−3.093***	−3.021***
	(3.18)	(3.64)	(−15.98)	(−13.75)
LTI	−0.304***	−0.289***	0.0610***	0.0573***
	(−13.57)	(−14.40)	(7.08)	(7.58)
CreditScore	−0.116***	−0.113***	−0.0588***	−0.0573***
	(−5.34)	(−5.30)	(−28.62)	(−29.08)
dSingleBorrower	3.873***	3.619***	2.181***	2.203***
	(5.36)	(4.93)	(11.67)	(15.40)
dFirstTime	2.018	3.660*	4.832***	4.771***
	(1.50)	(2.79)	(12.27)	(21.08)
NonRecourse	−1.045	−0.404	−1.575***	−1.557***
	(−1.68)	(−0.61)	(−5.32)	(−5.39)
Judicial	4.987***	4.391***	−0.248	−0.422**
	(11.11)	(9.94)	(−1.18)	(−2.95)
FE Year	Yes	Yes	Yes	Yes
FE Seller	No	Yes	No	Yes
N	1,554,691	1,554,691	1,554,645	1,554,645
R^2	0.164	0.180	0.115	0.138

This table presents coefficients of the OLS specification for $Spread$ and LTV on law dummies—$NonRecourse$ and $Judicial$. T statistics in parentheses. Standard errors are clustered by the seller. Coefficients marked with ***, ** and * are statistically different from zero at the 0.1%, 1%, and 5% confidence level, respectively

Table 4
Sample II (2012–2019) - Effects of law on Spread and LTV.

	Spread	Spread	LTV	LTV
LogIncome	−21.19*	−21.39*	70.28***	71.21***
	(−2.11)	(−2.42)	(15.13)	(18.99)
LogIncome^2	0.959	0.961	−4.353***	−4.388***
	(1.63)	(1.84)	(−15.90)	(−19.69)
LTI	−0.150***	−0.162***	0.00675	0.0110
	(−5.31)	(−5.75)	(0.94)	(1.69)
CreditScore	−0.282***	−0.276***	−0.0220***	−0.0209***
	(−39.98)	(−33.68)	(−11.40)	(−10.83)
dSingleBorrower	2.997***	2.673***	1.275***	1.250***
	(8.86)	(7.99)	(9.96)	(18.14)
dFirstTime	0.971	0.806	4.551***	4.562***
	(0.97)	(0.81)	(23.51)	(33.36)
NonRecourse	1.459**	0.933	−1.850***	−1.855***
	(3.21)	(2.01)	(−4.24)	(−5.63)
Judicial	−1.960***	−1.729***	−0.231	−0.0889
	(−4.35)	(−5.15)	(−1.44)	(−0.65)
FE Year	Yes	Yes	Yes	Yes
FE Seller	No	Yes	No	Yes
N	1,588,718	1,588,718	1,588,718	1,588,718
R^2	0.646	0.657	0.078	0.090

This table presents coefficients of the OLS specification for $Spread$ and LTV on law dummies—$NonRecourse$ and $Judicial$. T statistics in parentheses. Standard errors are clustered by the seller. Coefficients marked with ***, ** and * are statistically different from zero at the 0.1%, 1%, and 5% confidence level, respectively

the benefits of the judicial process outweigh its costs and ultimately inform policy decisions regarding foreclosure procedures.

What can we conclude based on the observation of the two periods? Consistently, recourse restrictions trigger a collateral channel through which lenders demand higher collateralization to reduce the expected loss of the loan, which can occur through a lower LGD (as lenders can only claim the collateralized property) or a lower PD (as by making negative equity less probable, the likelihood of borrower's strategic default diminishes).[10] Differently, judicial requirements do not generate a clear effect as we observe contradictory signs before and after the Great Recession.[11]

In the next sections, I further explore the identified effect of recourse restrictions, by applying a regression discontinuity design as a robustness measure and by assessing the median effect along the conditional distribution of the collateralization level.

7. An RDD approach to the impact of recourse restrictions

Regression discontinuity design models (RDD) allow for estimating the average effect of a binary treatment through different local approaches. In this specific case, I estimate the causal effect of recourse restrictions (treatment variable, T) on the LTV ratio at origination (outcome variable).[12]

As the treatment is assigned according to the state law where the loan is granted, there are no unmeasured confounders, and the assignment is independent of the potential outcomes. The optimal approach would involve using a geographic RDD, which considers the spatial proximity to the border and the fact that the treatment jumps discontinuously along this boundary (Imbens and Kalyanaraman, 2011). This would control for the second dimension of endogeneity mentioned in Section 5.

However, due to the poor georeferencing of data (Freddie Mac Single-Family Loan-Level Dataset only provides a three-digit zip code), the option is to follow a two-dimensional nongeographic RDD, with bandwidth forced to zero. In other words, I apply an RDD setting that compares loans granted in contiguous three-digit zip code areas with different recourse restrictions, X_z. The major limitation is the fact that these areas have heterogeneous dimensions and do not allow for a bandwidth choice. Fig. 3 present the identified adjacent areas.

Additionally, I substitute the role of distance in a geographic RDD by the exact matching of observations at X_z, year t, and seller j.[13] This matching method controls for the confounding influence of pre-treatment control variables, such as the contiguous area's demographic and economic characteristics, the year's conjuncture, and the seller's specificity. Still, it does not control for all the other variables related to the features of the loan, given that the richness of covariates would not allow for a significant number of matches.

To find the sample average treatment effect on the treated, I apply a linear regression considering the weights resulting from the matching estimation while controlling for the differences in the covariates included in Eq. (2), and not considered in the matching exercise.

Table 5 shows the average treatment effect of recourse restrictions before and after the Great Recession. The analysis compares loans granted by a given lender in contiguous three-digit zip code areas with varying recourse restrictions in a particular year. The findings support the assertion that recourse restrictions lead to greater collateralization levels, as evidenced by the negative and economically significant coefficients of non-recourse in the LTV ratio for both periods.

[10] For a lower LTV, it is less likely that the debt value will exceed the house value during the life of the loan.

[11] The results are robust to an alternative definition of $Spread$ that considers the possibility of mortgage prepayment and to the combined loan-to-value ratio. Tables available in Appendix F.

[12] In this setting of a sharp RDD, the treatment is determined solely by the value of the forcing variable ($NonRecourse$). Therefore, T is explicitly referred to as a binary treatment variable. $T = 1$ if the state law imposes recourse restrictions, and 0 otherwise.

[13] I use the Coarsened Exact Matching with zero cut points, which means that I match precisely for the three variables.

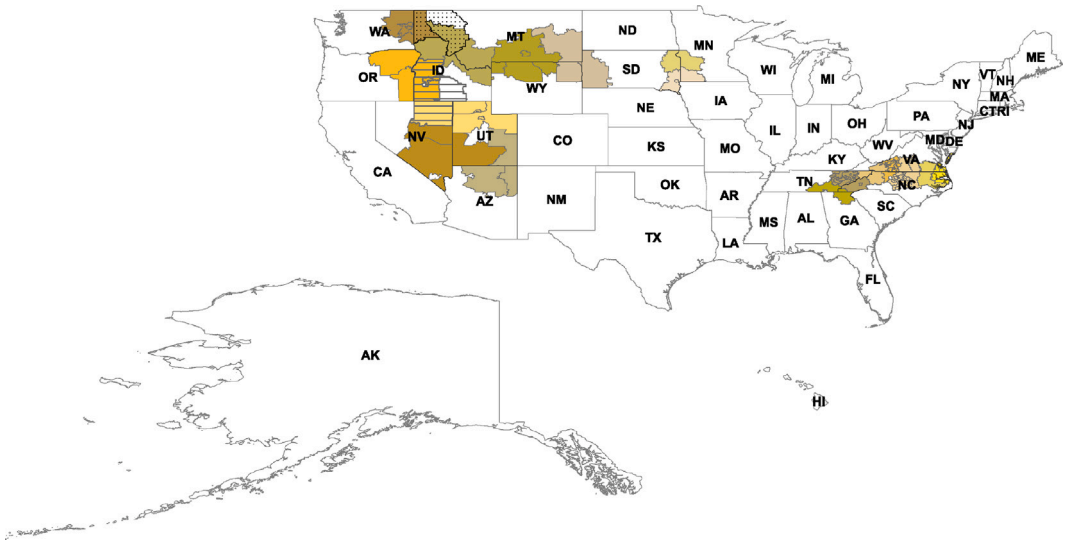

Fig. 3. Contiguous three-digit zip code areas with different recourse restrictions.
This figure depicts contiguous three-digit zip code areas with varying recourse restrictions. Each colored area contains at least one three-digit zip code with recourse restriction and one without. The areas are not uniform due to the different dimensions of the zip codes. Some three-digit zip codes are included in two contiguous areas; in such cases, the second area is represented by dashes instead of color. The law changes mentioned in Fig. 2 were considered.

Table 5
Average effects on the treated.

	Sample I: 2001–2006	Sample II: 2012–2019
	LTV	LTV
NonRecourse	−0.601***	−0.427**
	(−2.31)	(−1.95)

This table presents coefficients of the OLS specification for LTV on law dummy $NonRecourse$, after matching the sample at contiguous three-zip code with law discontinuity, year, and seller. T statistics in parentheses. Standard errors are robust. Coefficients marked with ***, ** and * are statistically different from zero at the 0.1%, 1%, and 5% confidence level, respectively

8. Strategic defaults and the impact of recourse restrictions

As stated in Section 5, in non-recourse states, lenders may demand increased loan collateralization as a means to: (1) reduce their exposure to property devaluations that could threaten the recovery of the debt value in a default event (lower the LGD of the loan) or; (2) minimize the likelihood of an event of negative equity that would increase borrower's incentives to engage in strategic default (lower the PD of the borrower).

Focusing on the latter, I inspect if the impact of non-recourse on the collateralization level at origination varies along the distribution of the LTV ratio. The underlying hypothesis is that a lower LTV decreases the probability of a negative equity event resulting from a given property devaluation, thereby reducing the borrower's likelihood to choose to default strategically. As so, if lenders demanded increased collateralization for loans with higher LTV ratios, this would suggest that they would be trying to deter strategic defaults.

To study this hypothesis, I apply a quantile regression model that allows for the estimation of the effects of explanatory variables at different points of the LTV conditional distribution.[14] To define the quantiles of interest, I start by inspecting the empirical cumulative distribution of the LTV ratio for the two sample periods portrayed in Fig. 4.

The two graphs plot different realities. In the first sample period, most mortgages had an LTV ratio of 80%, which is consistent with the argument that lenders prefer to grant conforming loans that can be easily traded in the secondary market.[15] Loans with LTV ratios above 80% might classify as conforming loans and can be sold to Freddie Mac, but require additional protection in case of the borrower's default. The second sample period depicts a significant increase in loans with LTV ratios above 80%.

Taking the conforming criterion as the reference for the standard loan, I classify each loan according to the conditional distribution of the LTV ratio and to the type of borrower:

- Type I (LTV ratio below 80%) - the borrower is not collateral constrained, as they could have asked for a higher loan to buy the house.
- Type II (LTV ratio of 80%) - the borrower might be collateral constrained, as they asked for the maximum loan amount that did not require additional protection to classify as conforming.
- Type III (LTV ratio above 80%) - the borrower is collateral constrained, as they chose to obtain a higher loan, even if that meant paying for private mortgage insurance.

Tables 6 and 7 present the estimation results for the LTV ratio quantile regression for the two sample periods.[16] For the Type I borrowers, I analyze within-type effects, whereas, for the Type II and III, I analyze the mid-quantiles. As expected, the coefficients of some explanatory variables vary across quantiles.

[14] A quantile regression model is more robust in the presence of outliers as it considers median regression, rather than mean regression. Moreover, and following (Koenker and Bassett, 1978), quantile regression models do not make assumptions on the parametric distribution of residuals, which makes them more efficient than OLS estimates in the presence of a violation of the normal distribution of residuals. After inspecting the residuals in Eq. (2), I have concluded that the normality assumption is violated.

[15] The Federal Housing Finance Agency (FHFA) sets, annually, the limits on the loan amount for conforming loans, and Freddie Mac and Fannie Mae set the guidelines on loan characteristics. Conforming loans are easily traded either through a government-sponsored enterprise or a private issue of mortgage-backed securities.

[16] I use the STATA module *xtqreg* to estimate quantile regressions according to the method proposed by Machado and Santos Silva (2019) with seller and year fixed effects. Instead of clustering the errors by the seller (not available for the function *xtqreg*), I use bootstrap standard errors as recommended by Baum (2013).

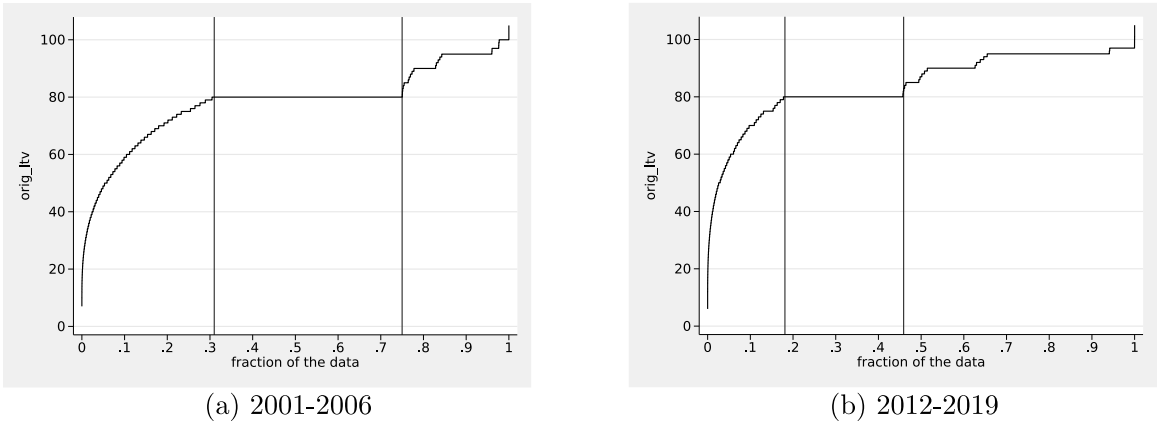

Fig. 4. Cumulative distribution of the LTV ratio.
This figure presents the empirical cumulative distribution of the LTV ratio for the two sample periods. Between 2001 and 2006, approximately 30% of mortgages had a ratio below 80%, and 25% had a ratio above 80%. From 2012 to 2019, there was a general increase in the LTV ratios, as 18% of mortgages had a ratio below 80% and more than 50% of new loans had a ratio above 80%.

Table 6
Sample I (2001–2006) - Effects of law on the conditional distribution of LTV.

	OLS	Type I Q(0.10)	Type I Q(0.15)	Type I Q(0.20)	Type I Q(0.25)	Type I/II Q(0.30)	Type II Q(0.50)	Type III Q(0.85)
LogIncome	48.83***	128.3***	102.6***	84.79***	72.40***	63.24***	39.52***	−6.140
	(14.53)	(15.80)	(17.21)	(16.38)	(13.49)	(16.97)	(13.03)	(−1.55)
LogIncome^2	−3.021***	−7.638***	−6.144***	−5.112***	−4.391***	−3.859***	−2.480***	0.174
	(−13.75)	(−15.16)	(−16.81)	(−15.41)	(−12.90)	(−15.63)	(−12.56)	(0.74)
LTI	0.0573***	0.187***	0.145***	0.116***	0.0958***	0.0809***	0.0420***	−0.0327***
	(7.58)	(9.33)	(9.47)	(8.64)	(9.51)	(8.72)	(5.17)	(−3.93)
CreditScore	−0.0573***	−0.0737***	−0.0684***	−0.0647***	−0.0622***	−0.0603***	−0.0554***	−0.0460***
	(−29.08)	(−14.43)	(−22.50)	(−24.52)	(−25.72)	(−26.71)	(−19.48)	(−11.66)
dSingleBorrower	2.203***	3.797***	3.281***	2.925***	2.676***	2.492***	2.016***	1.099***
	(15.40)	(14.78)	(14.83)	(12.74)	(13.21)	(14.65)	(10.50)	(6.06)
dFirstTime	4.771***	9.150***	7.733***	6.754***	6.071***	5.566***	4.259***	1.742**
	(21.08)	(15.45)	(17.02)	(20.84)	(20.88)	(15.76)	(12.80)	(3.23)
NonRecourse	−1.557***	−3.368***	−2.782***	−2.377***	−2.094***	−1.886***	−1.345***	−0.304
	(−5.39)	(−5.49)	(−5.15)	(−4.13)	(−4.57)	(−4.43)	(−4.09)	(−1.27)
Judicial	−0.422**	−1.344***	−1.046***	−0.840**	−0.696***	−0.589**	−0.314*	0.216*
	(−2.95)	(−3.53)	(−3.32)	(−2.94)	(−3.33)	(−3.05)	(−2.32)	(2.04)
FE Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Seller	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,554,645	1,554,645	1,554,645	1,554,645	1,554,645	1,554,645	1,554,645	1,554,645

This table presents coefficients of the OLS and the Quantile Regression specification for LTV on law dummies—$NonRecourse$ and $Judicial$. Standard errors are bootstrapped by the seller with 100 repetitions. T statistics in parentheses. Coefficients marked with ***, **, and * are statistically different from zero at the 0.1%, 1%, and 5% confidence level, respectively

The overall results are consistent with the OLS estimation. The significant and negative impact of non-recourse on all quantiles further supports the conclusion that recourse restrictions activate a collateral channel. However, the coefficient variation across quantiles raises some questions about its underlying reasoning. As the absolute value of $NonRecourse$ coefficients decreases with the quantile, the hypothesis that lenders require a lower LTV ratio to discourage strategic defaults is questionable. Put differently, collateral-constrained borrowers (Type III) should be the most affected if lenders seek more protection against negative equity situations. Yet, Type III borrowers in non-recourse states exhibit an impact on the LTV ratio of 0 to −0.9 percentage points (sample I vs. sample II), whereas Type II borrowers have a −1.3 to −2.3 percentage points impact (sample I vs. sample II). The results are even more remarkable for differences within the Type I borrowers, as the effect of non-recourse on loans with low LTV ratios should be almost negligible. For borrowers in the 10th quantile, the LTV ratio impact is −3.4 percentage points in both samples.

9. Conclusion

Mortgage law influences the debt recovery process in case of default and, thus, impacts the cost and risk of mortgages to lenders. Depending on the type of law, the effects might materialize in a higher probability of default, a higher loss given default, or both. By focusing on the U.S. mortgage market, I analyze recourse restrictions and judicial foreclosures as borrower-friendly laws that increase mortgage cost and risk. I use a loan-level dataset to assess how those laws influence loan contract terms and consider two possible channels: price and collateral channels.

My findings suggest that non-recourse laws trigger a collateral channel, with new mortgages having a lower loan-to-value ratio of about 1.6 to 1.9 percentage points. This happens because lenders require more collateral to compensate for the higher loan's expected loss when recourse is not allowed. I further show that this effect is robust to a regression discontinuity design approach, both before and after the Great Recession.

Table 7
Sample II (2012–2019) - Effects of law on the conditional distribution of LTV.

		Type I	Type I	Type I	Type I/II	Type II	Type III	Type III
	OLS	Q(0.05)	Q(0.10)	Q(0.15)	Q(0.18)	Q(0.30)	Q(0.75)	Q(0.85)
LogIncome	71.21***	147.2***	117.3***	103.5***	98.42***	83.51***	42.48***	37.45***
	(18.99)	(20.78)	(19.92)	(19.87)	(19.60)	(21.63)	(15.31)	(16.33)
$LogIncome^2$	−4.388***	−8.746***	−7.031***	−6.241***	−5.949***	−5.094***	−2.741***	−2.453***
	(−19.69)	(−20.75)	(−19.60)	(−19.65)	(−19.73)	(−22.09)	(−16.49)	(−17.97)
LTI	0.0110	0.0526***	0.0362***	0.0287***	0.0259***	0.0177*	−0.00471	−0.00745
	(1.69)	(5.90)	(4.00)	(3.73)	(3.53)	(2.44)	(−0.94)	(−1.39)
CreditScore	−0.0209***	−0.0369***	−0.0306***	−0.0277***	−0.0266***	−0.0235***	−0.0148***	−0.0137***
	(−10.83)	(−12.49)	(−12.29)	(−12.24)	(−12.77)	(−11.67)	(−7.13)	(−8.20)
dSingleBorrower	1.250***	2.940***	2.275***	1.968***	1.855***	1.523***	0.611***	0.499***
	(18.14)	(15.60)	(12.88)	(13.32)	(15.08)	(18.47)	(13.37)	(11.19)
dFirstTime	4.562***	9.210***	7.381***	6.538***	6.227***	5.315***	2.805***	2.498***
	(33.36)	(18.80)	(18.14)	(20.19)	(23.99)	(29.36)	(35.84)	(31.50)
NonRecourse	−1.855***	−4.447***	−3.427***	−2.957***	−2.783***	−2.275***	−0.875***	−0.704***
	(−5.63)	(−4.78)	(−4.81)	(−5.21)	(−5.19)	(−5.56)	(−6.15)	(−5.57)
Judicial	−0.0889	0.254	0.119	0.0568	0.0338	−0.0334	−0.219***	−0.241***
	(−0.65)	(0.63)	(0.40)	(0.24)	(0.15)	(−0.20)	(−3.60)	(−4.74)
FE Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE Seller	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,588,718	1,588,718	1,588,718	1,588,718	1,588,718	1,588,718	1,588,718	1,588,718

This table presents coefficients of the OLS and the Quantile Regression specification for LTV on law dummies—$NonRecourse$ adn $Judicial$. Standard errors are bootstrapped by the seller with 100 repetitions. T statistics in parentheses. Coefficients marked with ***, **, and * are statistically different from zero at the 0.1%, 1%, and 5% confidence level, respectively

To explore the underlying reasons why lenders demand more collateral to mitigate the loan's expected loss, I study the effect of non-recourse along the conditional distribution of the loan-to-value ratio. I find that collateral requirements are less stringent for borrowers who are collateral-constrained. This suggests that the lower loan-to-value ratios observed in non-recourse states are not intended to discourage borrowers' strategic default. Rather, the higher collateralization is likely due to the lower opportunities to recover the debt, as lenders can only seize the collateral property and not other assets and income of the borrower.

Regarding the judicial foreclosure requirements, the results are varied and seemingly contradictory for the post-Great Recession period. Prior to the Great Recession, lenders raised interest rates in judicial states to compensate for the higher foreclosure costs. However, after 2011, the opposite effect was observed. For this reason, we cannot clearly infer the impact of judicial foreclosures on mortgage characteristics at origination. What we can suspect, though, is that the underlying mechanism behind judicial foreclosures might have changed during the Great Recession, and that raises new questions. How did recovery rates compare in judicial vs nonjudicial states? Despite the higher costs associated with judicial fees and longer foreclosure processes, did lenders end up with higher recovery values?

Finally, it is outside this study's objectives to set any normative conclusion on what type of law should be considered more beneficial. Nevertheless, the results presented provide new policy-relevant insights on non-recourse laws that contribute to improving the policy assessment and aligning the policymaker's objectives.

Suppose a policymaker wants to protect borrowers in a vulnerable situation after a house foreclosure and enacts recourse restrictions. In that case, the policymaker should also consider that lenders will protect themselves by requiring higher collateralization levels for new mortgages, ultimately limiting households' access to the mortgage market. All in all, borrower-friendly laws come at a cost.

Data availability

Data will be made available on request

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.irle.2023.106142.

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