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Regulating Zombie Mortgages

By *Jonathan Lee, Duc Duy Nguyen, and Huyen Nguyen*

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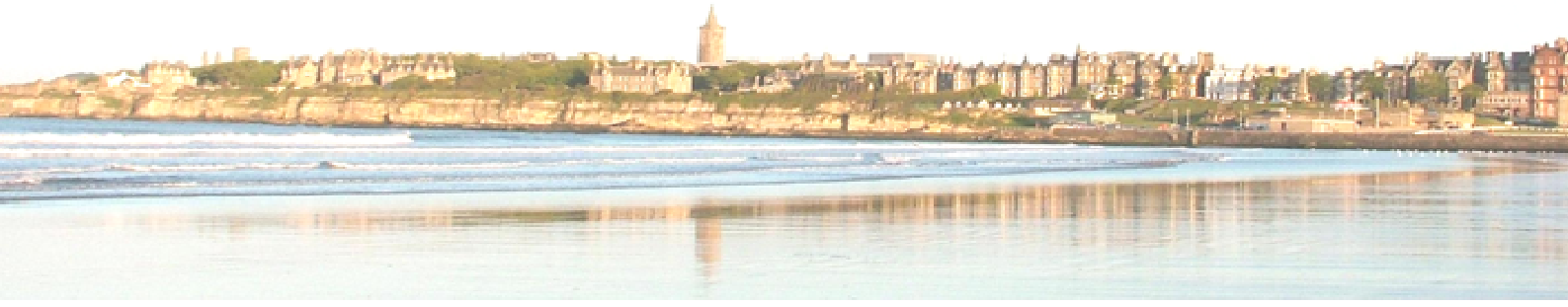

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Jonathan Lee, Duc Duy Nguyen, and Huyen Nguyen†

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Abstract

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Keywords: Zombie lending, Mortgage screening, Mortgage renegotiation.

JEL Codes: G21, G28, K25.

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1 Introduction

The United States stands out as one of a few high-income countries that does not have a federal law to regulate "zombie properties"— foreclosed homes abandoned in an incomplete foreclosure process and left to deteriorate over time.[1] As of 2023, there are at least eight million zombie properties in the US, costing local governments more than $50 billion to clean up.[2] Zombie properties pose significant environmental and health risks, attract crime and vandalism, reduce property values in the neighborhood, and cost public funds to clean up (Campbell et al., 2011). In the absence of a federal law, several US states have enacted their own Zombie Property Laws requiring lenders to maintain vacant properties in pending foreclosure cases. Other states are actively considering adopting similar regulations. Although these laws are central to an ongoing policy debate, their impact on mortgage lending decisions and standards remains largely unexplored, a gap this paper aims to address.

Economic theory suggests that Zombie Property Laws may improve mortgage lending standards by increasing lenders' skin in the game. This increased accountability is expected to prompt lenders to exercise greater caution when originating riskier loans that carry a higher probability of default, potentially creating a future burden for lenders (Ibragimov, 2019). However, there is concern that these policies may place excessive maintenance and legal costs on lenders, which could in turn restrict credit availability and raise borrowing costs (Conlin, 2013). After mortgage origination, lenders may also be incentivized to extend additional credit to distressed borrowers to avoid property maintenance responsibilities, thereby exacerbating the long-lasting effect of foreclosure.

Our paper contributes to this debate by providing some of the first evidence on the impact of Zombie Property Laws on mortgage lending decisions and standards. We analyze how these laws influence lending practices across a loan's lifecycle, from origination to the point

[1] Lenders often struggle to sell these houses, especially during periods of low housing demand and low potential returns (Conlin, 2013).

[2] Zombie Property Statistics, Statistic Stats, April 10, 2023, https://shorturl.at/fNkG7

at which a loan enters distress. While previous studies primarily focus on bank lending to "zombie firms" (Acharya et al., 2019; Bonfim et al., 2023; Favara et al., 2024), our research shifts attention to zombie property laws—a prevalent issue in the U.S. and other countries with significant implications for borrowers, lenders, neighborhoods, and legal systems.

Specifically, we exploit the adoption of Zombie Property Law (ZL) in New Jersey and New York in 2014 and 2016, respectively, and examine the effect of the law on mortgage lending behavior.[3] The law requires lenders or mortgage servicers to inspect and maintain vacant or abandoned residential properties that are subject to a pending foreclosure, and to promptly address any code violation or nuisances on the property. In most cases, ZL is applied to the lender that originates the mortgage because most originators continue to be the mortgage servicers even if they securitize the mortgage.[4] Thus, following the passage of ZL, lenders face substantial costs of maintaining the foreclosed properties. Lenders that fail to comply with ZL face a penalty of up to $500 and $2,500 per day per property in New York and New Jersey, respectively. Both states have issued substantial fines and filed lawsuits against non-complying lenders.

To answer our research questions, we design an empirical strategy that compares mortgages originated within a narrow geographical bandwidth of five miles on either side of state borders, where one state experiences a change in ZL while the other does not. This methodology resembles a regression discontinuity design because economic and housing conditions and foreclosure patterns are likely to be similar across state borders within a small homogeneous area. All specifications include region $\times$ year fixed effects, where a region is defined as a rectangular area consisting of two squares, each measuring five miles by five miles, one square is located in a state that eventually passes ZL and the other in a non-ZL

[3]In addition to New Jersey and New York, California also enacted a similar regulation in 2008. We do not include California passed the law at the onset of the 2008-09 crisis, making it difficult to isolate the effect of ZL from other concurrent federal and local regulatory changes enacted in response to the crisis. Moreover, because of the large time gap between the passage of ZL in California in 2008 and New Jersey and New York respectively in 2014 and 2016, this can lead to heterogeneity in treatment effects across lenders over time which could potentially bias our estimation (Baker et al., 2022).

[4]Report to the Congress on the Effect of Capital Rules on Mortgage Servicing Assets, Federal Reserve Board, 2016

state. The inclusion of region × year fixed effects allows us to compare mortgage outcomes across ZL and non-ZL states within the same region in the same year, which is likely to share similar economic and housing conditions. We also include lender × year fixed effects in specifications where we are able to identify the lender.

Consistent with the hypothesis that ZL exposes lenders to higher costs of maintaining foreclosed properties and greater litigation risk, we find that lenders respond to ZL by reducing lending and imposing a higher interest rate on borrowers. Specifically, following the passage of ZL, mortgage applications in ZL states face a modest 1.7% lower acceptance likelihood compared to those in non-ZL states. Similarly, the interest rate spreads on mortgage applications for properties located in ZL states increase by 4.5 basis points relative to the counterfactual after ZL.

To interpret the economic magnitude of the estimates, we evaluate the extent to which the increase in interest rates offset the potential costs lenders may incur upon foreclosure. Based on households' spending on home repairs and maintenance, we infer that lenders would incur approximately $5,277 to maintain each foreclosed property until being able to sell them.[5] In contrast, our estimate of the 4.5 basis point increase in interest rate spreads indicates that lenders have approximately $4,284 to maintain each *foreclosed* property.[6] Thus, the increase in lending rates adequately compensates lenders for the costs of property maintenance after foreclosures. This implies that lenders are able to pass most of the costs of complying with ZL to borrowers.

We conduct additional tests to further understand the lending response to ZL. First, we show that the effects of ZL on mortgage acceptance rates and interest rate spreads are

[5]Specifically, we use household survey data from the Panel Study of Income Dynamics (PSID) and find that households in New York and New Jersey spend average of $1,759 on home repairs and maintenance in 2013. Assuming that lenders incur similar maintenance costs and take, on average, three years to sell the properties, this implies a maintenance cost of $5,277 (=$1,759 x 3) for each foreclosed property. We acknowledge that there can be additional factors that influence the estimates. For instance, lenders could incur other types of operating costs, such as the cost to hire additional staff, in addition to the maintenance expenses.

[6]Our estimate indicates that lenders collect an additional $84 per mortgage per year after ZL. Because 5% borrowers in our sample eventually default and the median defaulter takes on average three years from origination to default, this works out to $4,284 (=($71.4 x 3)/5%).

stronger among riskier borrowers, that is, borrowers with a lower FICO score, a lower income, and higher LTI and LTV ratios. These borrowers face a higher probability of eventual default and therefore present a greater risk for lenders in the future. Second, the effects are more pronounced among lenders with more skin in the game, i.e., smaller lenders and those that rely more heavily on lending. These lenders have less diversified customer bases and income sources and are thus more exposed to the law. Finally, we find that mortgages originated after ZL have better *ex-post* performance, that is, they are 14% less likely to become seriously delinquent relative to the counterfactual.

Collectively, our results suggest that by tying lenders' responsibility to the vacant properties in foreclosure, ZL increases lenders' skin in the game for issuing risky mortgages and incentivizes them to exert more effort to screen mortgage applications during origination. In this respect, our findings have important implications beyond the ZL law. Specifically, we highlight that initiatives that hold lenders responsible for mortgages that they service may increase their skin in the game and potentially have real effects on lending standards.

Despite enhancing lender screening at origination, our further analyses show that ZL also affects lender behavior when borrowers enter distress. Specifically, the higher foreclosure costs could incentivize lenders to continue lending to distressed borrowers to avoid assuming the responsibility of maintaining the foreclosed properties. Consistent with this, we find that after ZL, lenders are 8.1% more likely to allow their distressed borrowers to renegotiate their mortgage terms to receive, for instance, an extended repayment period or a lower interest rate relative to the counterfactual. Importantly, distressed loans that are renegotiated after ZL are *not* more likely to eventually emerge from bankruptcy relative to the counterfactual. This suggests that the renegotiation decisions after ZL are not more efficient.

We perform various tests to ensure the validity of our inferences. We confirm that the two key assumptions of our DiD model is likely to hold. First, we find that borrowers do not strategically avoid buying properties in ZL states after the law. Specifically, there is no changes in the total number of mortgage applications and the loan amount requested

in ZL states after the law is enacted. Second, we verify the parallel trend assumption. To do this, we calculate the first differences of all outcome variables in 2012-2014 before ZL is passed, and find that there is no statistically significant differences in the pre-trends of these variables between the control and treatment observations.

We confirm that our results survive a large set of robustness checks, including placebo tests that alter the treatment timing and treatment location. Moreover, our findings are not sensitive to the way standard errors are clustered, are not driven by other state laws or time-varying housing and economic characteristics, and do not depend on whether the mortgage is securitized. Finally, although our sample does not include observations with changing treatment status over time, we allow for heterogeneous treatment effect using the estimator proposed by Sun and Abraham (2021) and find robust results.

We contribute to several active strands of the literature. First, we advance the literature on the real effects of foreclosures and foreclosure legislation on credit supply and housing markets. The existing literature on foreclosure laws mostly focuses on the judicial process that lenders need to follow to reclaim the property. Mian and Sufi (2009) use state requirements for a judicial process as an instrument for foreclosures to show that foreclosures lead to lower house prices and demotivate household investment. Dagher and Sun (2016) and Pence (2006) show that judicial foreclosure laws affect credit supply whereas McGowan and Nguyen (2023) find that lenders respond to judicial foreclosure laws by either adjusting interest rates or securitizing mortgages depending on whether a mortgage is eligible for sale to Government Sponsored Enterprises. Instead of focusing on laws governing the foreclosure procedure, we study foreclosure laws that seek to regulate zombie properties—a phenomenon widespread across the US and has long lasting implications for borrowers, lenders, local neighborhoods, and the legal systems. It is especially important to understand the impact of zombie property laws since many US states are also actively considering implementing similar regulations. Consequently, our study's implications extend beyond the impact of ZL

in New York and New Jersey and broadly contribute to the active debate on the costs and benefits of holding lenders accountable during the foreclosure process.

Second, we contribute to literature on the determinants of mortgage screening. Purnanandam (2010), Keys et al. (2010), Keys et al. (2012), and McGowan and Nguyen (2023) show that securitization leads to lax screening in mortgage markets. A key takeaway from our findings is that Zombie Property Laws (ZL) hold lenders accountable, regardless of whether the mortgage remains on the lender's balance sheet or has been securitized, as long as the lender continues to act as the mortgage servicer—a role that the majority of lenders retain even after selling the mortgage. This accountability gives lenders "skin in the game" when issuing riskier mortgages, motivating them to conduct more rigorous screening of mortgage applications at origination. In this respect, our findings have important implications beyond the ZL law and the US context. Specifically, we highlight that initiatives that hold lenders responsible for mortgages they service could increase their skin in the game, and potentially having real effects on lending standards.

Third, we speak to the evolving literature on zombie lending. Notably, Choi and Choi (2021) and Caballero et al. (2008) show that during the Japanese stagnation of 1990s, zombie lending reduces restructuring and delays recovery because it impedes reallocation of assets from low productivity firms to high productivity firms. More recently, Acharya et al. (2019) document that zombie firms that receive loans from weak banks do not use these funds to undertake real economic activities but to build cash reserves, thereby hindering impact of unconventional monetary policies on economic growth. Using data on Portuguese banks, Bonfim et al. (2023) illustrate that onsite regulatory inspections could reduce bank lending to zombie firms because these inspected banks are forced to realize losses. Unlike prior studies that focus on bank lending to nonviable firms, we show, for the first time, that lenders also have an incentive to continue lending to keep nonviable mortgages alive to delay assuming the responsibility post-foreclosure.

Relatedly, we also contribute to the nascent literature examining the factors that affect lenders' decision to renegotiate distressed mortgages. Mortgage renegotiation has been rare in the US market. A common explanation of the low renegotiation rate is the frictions introduced by securitization. While Agarwal et al. (2011) and Piskorski et al. (2010) document that bank-held loans get renegotiated more than securitized loans, Ghent (2011) argues that banks do not renegotiate loans during bad times and securitization did not contribute to the lack of concessionary mortgage renegotiation. We contribute to this literature by showing how foreclosure costs influence the likelihood and efficiency of mortgage renegotiation decisions.

Our results are further related to the literature on renegotiation and underinvestment. Specifically, if lenders cannot commit to cutting funds to inefficient projects, this could lead to too much investment in bad projects and insufficient investment in good projects (Bolton, 1990; Holden, 1999). In our case, when lenders strategically keep delinquent mortgages alive, this could crowd out available mortgage credit for other clients and attract high risk borrowers assuming that they can renegotiate their mortgage terms when falling behind their payments. Our findings therefore have implications for the design of policy interventions aiming to regulate mortgage renegotiation.

2 Institutional Setting

2.1 Zombie Properties

The term "zombie mortgage" refers to a mortgage on a residential property where the foreclosure process was initiated but never finalized (Ibragimov, 2019). The foreclosure process begins when the borrower fails to make mortgage payments and the foreclosure notice is automatically issued. Upon receiving the foreclosure notice, most borrowers vacate the house. However, lenders often struggle to sell these houses, especially during periods of low housing demand. They soon realize that following through with the sales may not yield

anything close to what is owed on them. By walking away, lenders can at least reap some of the insurance, tax, and accounting benefits from documenting the loss without having to take on the additional costs and responsibilities of ownership (Conlin, 2013). As a result, lenders have little incentive to maintain the vacant houses.[7] Meanwhile, the title of the property remains with the borrower, and the property is left to deteriorate over time.

The zombie property phenomenon was particularly acute during and after the 2008-09 financial crisis. The crisis sent house prices plummeting, causing many borrowers to default on their mortgages, followed by a wave of foreclosures. More recently, zombie properties are on the rise again and have increased every quarter since early 2022. For example, in 2023, the number of zombie properties in the US is up 16% from a year ago.[8] As shown in Figure 1, on average, 40% of mortgage foreclosures are not completed, and the number of pending foreclosures goes up sharply since 2021. Widespread across the US and several countries, zombie properties are harmful to the communities because they are magnets for crime, impose health hazards, and reduce the property value of neighboring houses. It also costs public funds to secure, clean, and stabilize vacant houses that generate no tax revenue. Thus, these properties create substantial negative externalities that warrant public intervention.

[Insert Figure 1]

2.2 Zombie property law

Because of the negative externalities of zombie properties, several countries have long established regulations requiring lenders to maintain residential properties in foreclosure. For instance, Singapore, Canada, the United Kingdom, many European countries, and several states in Australia mandate that lenders take proactive measures to prevent damage once

[7] The compensation structure of loan officers further contributes to this problem. Because loan officers' salary and bonuses mainly depend on the number of new customers brought in and loan performance (Lim et al., 2023), loan officers have limited incentives to take care of the properties post foreclosure.

[8] Zombie Properties are increasing in 2023, Propstream, July 21, 2023, https://shorturl.at/bqCEY

borrowers default and vacate the properties. In contrast, the US is one of the few high-income countries without a federal law to regulate zombie properties.

There is an active debate in the US on the economic implications of policies that address zombie properties. At the federal level, although the Zombie Property Relief Act of 2016 was introduced in the 114th Congress with the aim to give the Consumer Financial Protection Bureau the authority to fine lenders for failing to maintain vacant houses in foreclosure, the bill did not receive a vote. This is partly due to lobbying effort from lenders to block the law. For instance, the Mortgage Bankers Association has strongly objected to suggestions for creating registries that require lenders to take more responsibility for vacant houses, arguing that this would place "unreasonable" and "onerous" pressure on lenders and would consequently hurt the mortgage lending business (Conlin, 2013). Politicians are also concerned that combating zombie properties can be an expensive, multiyear process and could put them at odds with lenders.

Given these obstacles, as of 2019, there are three states that have Zombie Property Law (ZL) that require lenders to maintain foreclosed properties: California, New Jersey, and New York.[9] California is the first state to introduce this law in 2008, followed by New Jersey and New York in 2014 and 2016, respectively. We focus our analyses on the adoption of ZL in New Jersey and New York in 2014 and 2016, respectively. We do not include California because it passed ZL in 2008 at the onset of the 2008-09 financial crisis. This makes it difficult to isolate the effect of ZL from other concurrent federal and local regulatory changes enacted in response to the crisis. Moreover, because there is a large time gap between the passage of ZL in California in 2008 and New Jersey and New York in 2014 and 2016, this can lead

[9]Ohio and Oregon also have ZL, but instead place the responsibility on borrowers to maintain the abandoned properties. These states threaten to impose harsh actions, including possible jail time, on borrowers if they fail to remedy the problems associated with their abandoned properties. However, actions against borrowers are difficult to enforce. Most borrowers only realize they have maintenance responsibilities several years after leaving the house, and many struggle to pay the maintenance bills. This leads to endless court battles and puts an immense strain on borrowers and the legal system (Conlin, 2013). The properties, meanwhile, continue to rot.

to heterogeneity in treatment effects across lenders over time which potentially can lead to a biased estimation (Baker et al., 2022).

New Jersey passed the Assembly Bill A347 in July 2014. The bill authorizes New Jersey municipalities to penalize lenders who do not address code violations or abate nuisances on vacant or abandoned residential property subject to a pending foreclosure. Upon receiving notice of a nuisance or code violation occurring on the abandoned property, the responsible lender will have 30 days to remedy the violation. In most cases, the lender that originates the mortgage will assume the maintenance responsibility under ZL. This is because even if the originating lender securitizes the mortgage to a third party, they continue to be its servicer. This is in line with a 2016 report from the Federal Reserve Board indicating that originating lenders retain servicing rights for almost every mortgage that they sell. Lenders who do not comply will be subject to penalties ranging from a fine up to $2,500 per property per day, community service, or imprisonment for not more than 90 days. The law proved to be more than just a threat. For instance, in 2014, East Orange (a small town in New Jersey) has issued court summons for a total fine of $320,000 on lenders of vacant and abandoned properties.[10]

New York independently passed the Abandoned Property Relief Act of 2016 in December 2016. The law requires lenders or their servicing agents to inspect one-to-four family residential properties within 90 days after a mortgage loan falls into delinquency. If the lender determines that the property is vacant and abandoned, they must assume maintenance obligations for the property. Failing to do so, lenders may face financial penalties or be sued by cities. For example, lenders in New York have an obligation to report their efforts at maintaining the properties to the New York Department of Financial Services (NYDFS). If the NYDFS believes that lenders fail to maintain a property, they may issue fines of $500 per day per property in violation, implying a fine of $15 million if lenders fail to take care of one thousand abandoned properties for 30 days. The city of New York has filed lawsuits

[10]N.J. Towns Tackle Abandoned Homes as 'Zombie Foreclosures' Climb, Wall Street Journal, November 10, 2015, https://shorturl.at/hjlvM

for over $1 million in penalties against several lenders for failing to comply with the state's ZL in one single case.

Several states are considering adopting similar legislation that would require lenders to maintain foreclosed properties. For instance, Illinois recently enacted the Vacant and Abandoned Property Law in 2023, while Florida, Minnesota, Ohio, and Virginia are discussing proposals to increase mortgage servicers' responsibilities to upkeep vacant and abandoned properties. Our findings could therefore provide valuable insights for these ongoing policy discussions, especially given the rising number of vacant homes across the US.

2.3 Hypothesis development

Based on the institutional setting, we proceed to develop our hypotheses. Our first hypothesis concerns the effect of Zombie Property Law on mortgage acceptance rates and interest rates. After the passage of ZL, lenders face increased accountability in the foreclosure process (Ibragimov, 2019). They also incur additional costs of maintaining foreclosed properties and face potential penalties and litigation costs for failure to comply with the regulation. As a result, we expect lenders to exercise more caution in extending loans to properties in ZL states, especially those that could potentially be abandoned. This could involve using a stricter mortgage origination process, such as more rigorous property inspections, before approving a loan. This raises the bar for a given loan to be approved and reduces mortgage acceptance rates as a result. Moreover, to compensate for the increased risk and higher costs associated with ZL, lenders could charge higher interest rates on loans for properties located in ZL states. We further hypothesize that the effects of ZL on acceptance rates and interest rates would be more pronounced among the riskier mortgages, that is, those that have a higher probability of eventual default and present a greater burden for lenders in the future. We formulate our hypothesis as follows:

Hypothesis 1: *Zombie Property Law increases lender accountability in the foreclosure process and leads to a more tightened origination process, especially among risky borrowers.*

Our second hypothesis focuses on the effect of ZL on lender behavior post-origination. We hypothesize that, after the adoption of ZL law, lenders will have an incentive to continue lending to distressed borrowers to avoid taking the responsibility of maintaining the foreclosed properties. As a result, instead of heading towards default, borrowers are allowed to renegotiate their mortgage terms. We therefore expect the propensity of mortgage renegotiation among distressed borrowers to increase after the passage of ZL. These considerations lead to our second hypothesis:

Hypothesis 2: *Lenders strategically keep delinquent mortgages alive to avoid the maintenance cost and legal risk imposed by the Zombie Property Law.*

3 Data, Identification Strategies, and Setting Validity

3.1 Data

HMDA: To analyze the effect of ZL on credit supply, we use data from the HMDA database, a loan-level dataset that covers all mortgage applications that have been assessed by qualified financial institutions. Specifically, an institution is required to disclose any mortgage lending under the HMDA if it has at least one branch office in any metropolitan statistical area and meets the minimum asset size threshold. For instance, in 2016, this reporting threshold was $44 million in book assets.[11] Given the relatively low asset size reporting threshold, this dataset covers the majority of lenders and accounts for approximately 95% of US mortgage originations.

Each observation in HMDA corresponds to a unique mortgage application and provides borrower demographic characteristics (e.g., income, gender, and race), loan characteristics (e.g., loan amount and purpose), property location, property type (single- or multi-family),

[11]The HMDA's reporting criteria can be found at https://www.ffiec.gov/hmda/reporterhistory.htm.

the decision on the loan application (e.g., accepted or rejected) and a lender identifier. Our sample period is between 2012 and 2018, which covers two years around the enactment of ZL in New Jersey in 2014 and New York in 2016. To have a homogeneous sample of loans, we follow the prior literature and restrict our sample to conventional, single-family, and home purchase mortgages. Moreover, to ensure that we compare mortgages in areas that share very similar economic and social conditions, we restrict our sample to observations that are within a five-mile distance from the border between states that have the ZL (i.e., New York, and New Jersey) and states that do not have such a law (i.e., Connecticut, Massachusetts, Pennsylvania, and Vermont). We also exclude the observations at the New Jersey-New York state borders because they experience changing treatment status over time. Subsection 3.2 provides further rationales for these sampling choices.

Our HMDA sample includes 199,076 observations from 2012 to 2018. Summary statistics for the HMDA loan-level dataset are shown in Table 1. On average, 78% of mortgage applications were accepted. The average applicant earns approximately $100,000 annually and requests a mortgage loan of $198,000. The average loan-to-income ratio is 2.37.

[Insert Table 1]

McDash: To examine the effect of ZL on mortgage pricing and renegotiation, we use loan-level mortgage data from the Black Knight Financial Services Group's McDash dataset. Covering approximately two-thirds of the mortgage market in the US, McDash provides several loan-level characteristics that are not available in HMDA.[12] These include, for instance, several mortgage characteristics (e.g., interest rate, loan amount, and maturity), risk characteristics of borrowers (e.g., FICO score and loan-to-value ratio), and mortgage performance since origination (e.g., information on repayments, delinquencies, and mortgage renegotiation).

[12]Starting from 2018, the HMDA database has started to record additional variables such as loan-to-value ratio and loan spread. We are unable to leverage this feature of the HMDA data because it covers the period after ZL has been enacted.

To ensure comparability with the HMDA sample, our McDash sample includes conventional, single-family, and home-purchase mortgages. To minimize possible data errors, we follow Agarwal et al. (2013) to exclude observations with FICO scores below 300 or above 900, observations with reported loan-to-value ratios above 100%, and adjustable rate mortgages. Our McDash sample includes 30,734 observations between 2012 and 2018. As shown in Table 1, the average interest rate spread (defined as the difference between the interest rate on a mortgage and the 30-year US treasury bond yield) is 0.946%. The average borrower in the McDash sample has a FICO score of 717, borrows 87% of the property's appraisal value, and requests a mortgage loan of $230,150.

Table 1 also reports summary statistics on a sample of mortgages that become delinquent for at least 90 days, i.e., seriously delinquent mortgages. We focus on delinquent mortgages originated before the enactment of ZL to isolate the law's effects on mortgage acceptance rates and to examine how ZL influences lenders' willingness to renegotiate mortgage terms. On average, 11% of seriously delinquent mortgages have their mortgage terms renegotiated within six months of becoming seriously delinquent. Expectedly, the average borrower in this sample has weaker credit scores (a FICO score of 670 and borrows 94% of the property's appraisal value) compared to the average borrower in the full McDash sample. The summary statistics are comparable to those reported in prior studies analyzing mortgage renegotiation (e.g., Agarwal et al. (2011), Kruger (2018)).

Other datasets: In addition to the mortgage datasets, we also use several additional data sources that are common in the literature. Specifically, we obtain zip-code-level data on home foreclosure rates, house prices, and mortgage delinquencies from Corelogic and Zillow.com. We collect county-level data on violent crime rates, the share of the population living in poverty, and the share of the population with a college degree from the US Census Bureau, and unemployment rates from the Bureau of Economic Analysis. Finally, we collect bank-level Call Reports from the Bank Regulatory Database provided through the Wharton Research Data Services.

3.2 Identification Strategy

Our paper seeks to examine the effects of the passage of ZL in New Jersey and New York in 2014 and 2016, respectively, on mortgage lending outcomes. We use a difference-in-differences (DiD) estimator that compares the evolution of mortgage acceptance rates, interest rates, and renegotiation rates between mortgages that are subject to the ZL and mortgages that are not. We estimate the following equation:

$$y_{ilrst} = \alpha + \beta ZombieLaw_{st} + \varphi W_{ilrst} + \delta_{rt} + \delta_{lt} + \varepsilon_{ilrst}, \tag{1}$$

where y_{ilrst} is the dependent variable (either the acceptance dummy, renegotiation dummy, or interest rate spreads) for loan i originated by lender l in region r of state s in year t; $ZombieLaw_{st}$ defines treatment status and equals to 1 if a property is in a state that has a Zombie Property Law at time t, 0 otherwise. W_{ilrst} is a vector of control variables; ε_{ilrst} is the error term.

However, simply regressing mortgage acceptance rates or interest rate spreads on the ZL indicator could result in biased estimates. Specifically, the decision to pass ZL could be driven by state-level economic conditions (e.g., foreclosure rates, housing conditions, labor market conditions), as well as state-level political factors such as party preferences. To overcome this challenge, we design a test that compares mortgages originated within a narrow geographical bandwidth of five miles on either side of state borders, where one state experiences a change in ZL and the other does not. This methodology resembles a regression discontinuity design because economic and housing conditions are likely to be similar across state borders within a small homogeneous area whereas ZL changes discontinuously.

Figure 2 displays the map of states with ZL and nearby states without ZL. Observations included in the analyses are colored, where those located in ZL states of New Jersey and New York are colored in light red, and those in non-ZL states of Connecticut, Massachusetts, Pennsylvania, and Vermont are in light green.

[Insert Figure 2]

Figure 3 illustrates our definition of a "region" by zooming into observations around the Pennsylvania/New York state border. Specifically, we define a region as a rectangular area measuring 10 miles in length and five miles in width. It consists of two squares, each measuring five miles by five miles—one located in a state that eventually passes ZL and the other in a non-ZL state.

[Insert Figure 3]

All specifications in the paper include δ_{rt} region×year fixed effects, allowing us to compare mortgage outcomes across ZL and non-ZL states in the same region in the same year. This within-region analysis is key to our identification because observations within the same region in the same year are likely to share similar geography, climate conditions, transportation routes, and, importantly, similar economic and housing conditions. This suggests that unobserved economic shocks tend to affect all observations within the same region symmetrically since economic conditions are likely to change smoothly across state borders (see, for instance, Pence (2006) and McGowan and Nguyen (2023)).[13]

In all specifications using HMDA data, we also include δ_{lt} lender×year fixed effects, which absorb all time-varying lender specific characteristics such as size, capital level, risk appetite, managerial quality, regulatory differences and business models that may influence lending behavior. In specifications using McDash data, because our data license does not allow us to identify the specific lender that originates each mortgage, we are unable to include δ_{lt} lender×year fixed effects in these specifications. We instead control for several county-year

[13]To validate this, we compare treatment and control observations across a large set of location-, loan-, and lender-level characteristics over the 2012-2014 period before the enactment of ZL. Appendix B shows that the normalized differences across all characteristics are below the Imbens and Wooldridge (2009) critical threshold of 0.25, indicating that there is no significant difference between the groups. In particular, we find that there are no significant differences in the average zip-code-level home foreclosure and mortgage delinquency rates between treatment and control observations before ZL is enacted. Thus, our results are unlikely to be driven by increasing foreclosure patterns or, more broadly, deteriorating economic conditions in ZL states. Further, treatment and control observations also share similar lender-level characteristics as well as loan-level characteristics, such as mortgage acceptance rates, interest rate spreads, loan renegotiation rates, loan amount, applicant income, and credit scores.

measures of the following lender characteristics: *Ln(Total assets)*, *ROA*, *Equity-to-assets*, *Deposit-to-assets*, and *Loan-to-assets* ratios. These variables are calculated based on the weighted average of the deposit market share of all lenders in a given county-year. Our main coefficient of interest β captures the local average treatment effect of the passage of ZL on mortgage acceptance rates, interest rate spreads, and renegotiation outcomes of seriously delinquent mortgages.

One concern with using a staggered treatment design is that it could lead to a biased estimation of causal effects. This bias arises when the treatment status changes over time, typically when control observations in earlier time periods become treated observations later on (Baker et al., 2022). This causes the estimated effect to capture the treatment effect that is in the process of materializing in the control observations. This is not a concern in our set up because our sample already excludes the observations around the New Jersey-New York state border, the only border that contains some units with changing treatment status over time. We later show that our results are robust to using the estimator proposed by Sun and Abraham (2021) that allows for heterogeneous treatment effects over time.

Finally, clustering standard errors requires special consideration. One option is to cluster the error terms at the state-level since our treatment ZL is at the state-level. However, given that our sample includes only six states, corresponding to six clusters, inferences based on clustered standard errors could be unreliable (see, e.g., MacKinnon and Webb (2018)). Clustering at the lender-level is also not viable because we do not have a lender identifier in McDash. As a result, we opt for the next best alternative option, which is to cluster our standard errors at the county-level in all main analyses. Subsection 6.1 shows that our results are robust to using wild bootstrap standard errors (MacKinnon and Webb, 2018) and double clustering the standard errors at the state- and year level.

3.3 Setting validity

The validity of our empirical strategy depends on two identifying assumptions: (1) borrowers do not manipulate the assignment variable, and (2) there is no difference in the pre-treatment trends of the dependent variables between the treatment and control groups. In this subsection, we show that both assumptions are likely to hold in our setting.

We start by validating the manipulation assumption that borrowers do not manipulate the assignment variable, which is the location of the mortgage property that determines the applicability of ZL. Conceptually, this assumption is likely to hold for several reasons. First, it is highly unlikely that borrowers would apply for a mortgage across state lines just to benefit from ZL. Rather, their decision is a function of commuting, education, and health care access. Second, budget constraints may also prevent borrowers from choosing where they live.

To empirically verify this assumption, we perform several tests and display the results in Table 2. First, we perform census tract-year-level regressions to examine whether the passage of ZL affects the application flow quantity.[14] If ZL incentivizes borrowers to strategically choose to reside in ZL states, we should observe a reduction in the number of loan applications and the total loan amounts sought after the passage of ZL.

[Insert Table 2]

To evaluate this assumption, we calculate for each census-tract-year the natural logarithm of the total number of mortgage applications submitted (*Ln(Number of applications)*) and the natural logarithm of the total loan amount requested (*Ln(Total Loan Amount)*), and regress these variables on the *Zombie Law* dummy. As shown in Table 2, the estimated coefficients on *Zombie Law* are statistically insignificant for both outcome variables, indicating that the passage of ZL does not affect the application flow quantity. This confirms our conceptual evidence that the manipulation assumption is likely to hold.

[14] We aggregate the data at the census-tract-level because it is the most granular geographical level in HMDA.

Next, we evaluate whether the parallel assumption holds in our sample. The parallel assumption states that in the absence of treatment (ZL), the coefficient on the DiD estimator is zero. Thus, it requires a similar pre-event trend for both treatment and control observations. Conceptually, this assumption is likely to hold because we focus on observations near the state borders, which are likely to share similar social and economic characteristics.

[Insert Table 3]

To empirically verify this, we compare the evolution of all outcome variables between the treatment observations in ZL states and control observations in non-ZL states over the 2012-2014 period prior to the enactment of ZL. To calculate these pre-trends, we aggregate our data to census tracts (HMDA) and 5-digit ZIP code (McDash) and compare the first differences of all outcome variables. We find in Table 3 that there is no statistically significant differences in the pre-trends of acceptance rates, interest rates, and seriously delinquent rates between the treatment and control observations. The treatment and control groups also experience similar pre-trends in foreclosure rates, renegotiation rates, and cure rates. Thus, the results indicate that the parallel trend assumption is likely to hold in our data. We further validate the parallel trend assumption in Figure 4, showing that the effects of ZL on mortgage lending outcomes only take place after the law has been passed.

4 The effect of zombie law on mortgage supply and interest rates

4.1 Baseline results

In Table 4, we present our baseline regression results on the effect of ZL on mortgage acceptance rates and interest rate spreads. Columns (1)-(3) display the results on mortgage

acceptance rates using HMDA data. The dependent variable is *Accept*, a dummy variable that equals to 1 if a loan is accepted, and 0 otherwise.[15] Model specifications vary by the set of fixed effects and control variables included. The control variables are *Ln(Applicant Income), Ln(Loan Amount)*, Loan-to-value ratio (*LTI*), and dummy variables indicating whether the applicant is *Male, Minority*, has a *Coappliant*, and whether the loan is *Jumbo*.

[Insert Table 4]

We find that, following the passage of ZL, the acceptance rate for mortgage applications in ZL states decreases relative to those in non-ZL states. Across Columns (1)-(3), the coefficients on ZL are negative and statistically significant below the 10% level. In the model that includes the full set of fixed effects and control variables (Column (3)), the coefficient estimate is 0.013. This implies that mortgage applications in ZL states face a 1.7% (= -0.013/0.784) lower acceptance rate after the law becomes effective.

Moreover, the magnitude of the coefficient estimates on ZL is generally stable as we progressively include more control variables and fixed effects in the model. For example, the coefficient on ZL changes minimally from -0.011 in Column (2) to -0.013 in Column (3) when we replace lender fixed effects with lender×year fixed effects. This suggests that our estimates are not sensitive to time-varying lender characteristics such as size, capital level, or risk appetite.

Columns (4)-(5) of Table 4 display the results for interest rate spreads using data from McDash. The dependent variable *Loan Spread* is the difference between the interest rate on a mortgage and the 30-year US treasury bond yield. Model specifications mirror those in Columns (1)-(3), except we are unable include lender or lender×year fixed effects because there is no lender identifier in McDash. In the absence of lender×year fixed effects, the specifications in Columns (4)-(5) control for county-year measures of several lender characteristics: *Ln(Total assets)*, *ROA*, *Equity-to-assets*, *Deposit-to-assets*, and

[15]The main advantage of using the acceptance rate is that it captures lenders' willingness to accept the mortgage. Other outcome variables such as loan amounts tend to confound lenders' decisions and borrower preferences.

Loan-to-assets ratios. We further control for *Ln(Loan Amount)*, the applicant's *FICO* and their Loan-to-value ratio (LTV), and dummy variables indicating whether the loan has *Low Documentation*, is *Jumbo*, or has a *Prepayment Penalty*.[16] The inclusion of *FICO* score and LTV ratio is especially important and allows us to control for borrowers' creditworthiness. Thus, the estimates on the ZL dummy can be viewed as capturing incremental variation over and above common borrower risk characteristics.

As shown in Columns (4)-(5), following the passage of ZL, lenders charge higher interest rate spreads on mortgage applications for properties located in ZL states. In the specification that includes the full set of fixed effects and control variables (Column (5)), the estimated coefficient indicates that ZL increases interest rate spreads by 0.045 percentage points (or 4.5 basis points) and the effect is statistically significant at the 1% level. We later show in Section 4.2 that this increase fairly compensates lenders' costs of complying with ZL.

Figure 4 displays the dynamic timing effects of the passage of ZL on the mortgage acceptance rate and interest rate spreads. We plot the coefficients of ZL for the period 4 years before and after the passage of ZL. The legend indicates the 95% confidence intervals of the coefficient estimates. As shown in Figure 4, the effects of ZL on the acceptance rate and rate spreads become statistically significant only after the passage of ZL, further confirming the validity of our inferences.

[Insert Figure 4]

Overall, the baseline results in Table 4 are consistent with our hypothesis that ZL exposes lenders to greater litigation risk and higher costs of maintaining foreclosed properties. This causes lenders to *ex-ante* pass some of these costs to borrowers in the form of a lower acceptance likelihood and higher interest rates. We later show that these effects are particularly salient among riskier borrowers and among lenders that are more exposed to the law, in line with ZL motivating lenders to take more effort to screen mortgage applications.

[16]The loan-level control variables in Columns (4)-(5) are different from those in Columns (1)-(3) because the variables available in McDash are slightly different from those in HMDA.

4.2 Interpreting the economic magnitude

Having shown that ZL causes lenders to charge borrowers higher interest rates, a natural question that arises is the extent to which the increase in interest rates offsets the potential costs that lenders may incur upon foreclosure. Because we cannot directly observe the actual costs lenders incur to maintain the foreclosed properties, we make our inferences based on households' spending on home repairs and maintenance.

We obtain data on households' spending on home repairs and maintenance (including both labor and material costs) from the University of Michigan's Panel Study of Income Dynamics (PSID). Our sample includes 498 households living in the two ZL states of New York and New Jersey in 2013 before the adoption of ZL law.[17] These households spend an average of $1,759 in the previous 12 months on home repairs and maintenance. Assuming that lenders incur similar costs to maintain the vacant properties, and that lenders need to maintain the properties for three years before being able to sell them, this translates a total maintenance cost of $5,277 (=$1,759 x 3) for each foreclosed property.

We now compare this $5,277 cost with the increase in interest rate spreads documented in Table 4. Our estimate in Table 4 indicates that, on average, borrowers face an annual increase of $71.4 in borrowing costs after ZL.[18] Given that 5% borrowers in our sample eventually default and the median defaulter takes on average three years from origination to default, the increase in interest rates implies that lenders have approximately $4,284 (=($71.4 x 3)/5%) to maintain each foreclosed property. This is reasonably close to the

[17] In the public version of the PSID, state is the most granular level at which households are geocoded. As such, we are unable to limit our calculations on households living five miles from the state borders and instead focus on households living in the two ZL states of New York and New Jersey.

[18] The average loan size in our sample is $230,150, the average annual interest rate (r) is 3.928%, and the duration of the mortgage is 30 years (30 * 12 = 360 months). We obtain the monthly payments using the simple present value formula, where the total mortgage cost is the sum of all monthly payments (pmt*360). At an interest rate of 3.928%, the total mortgage cost is $392,126 ($1,089.24 * 360). In contrast, at an interest rate of 3.973%, the total mortgage cost is $394,268 ($1,095.19 * 360).

estimated maintenance cost of $5,277, suggesting that the increase in rate spreads fairly compensates lenders' costs of maintaining the vacant properties after ZL.[19]

4.3 Heterogeneous effects across borrower creditworthiness

Our results so far are consistent with ZL exposes lenders to greater litigation risk and higher costs of maintaining foreclosed properties, causing them to reduce lending and impose higher interest rates on borrowers. If this interpretation is true, the effects of ZL law should naturally be more pronounced among the riskier mortgages, that is, those that have a higher probability of eventual default and present a greater burden for lenders in the future. To provide direct evidence on this, we interact the *Zombie Law* indicator with various measures of loan risk and display the results in Table 5.

[Insert Table 5]

Columns (1)-(2) report results for mortgage acceptance rate. We use two standard measures of loan risk in HMDA: (1) *High LTI*, a dummy variable that equals to 1 if the loan's LTI ratio is above the top quartile (i.e., 75th percentile) in a given year, and zero otherwise. A higher LTI ratio indicates that the loan is riskier because borrowers are less able to use their regular income to service the loan (Dagher and Sun, 2016); and (2) *High Income*, a dummy variable that equals to 1 if the applicant's income is above the top quartile in a given year, and 0 otherwise. Columns (3)-(4) report results for interest rate spreads using McDash data. We use two proxies for loan risk: *High LTV* and *High FICO*, dummy variables equal to 1 if the applicant's LTV ratio and their FICO score, respectively, are above the top quartile in a given year, and 0 otherwise.

[19]We acknowledge that there can be additional factors that influence the estimates. For instance, lenders could benefit from economies of scale when maintaining multiple abandoned properties simultaneously, potentially reducing costs. However, lenders may also incur other types of operating costs in addition to the maintenance expenses. For example, they may need to invest in hiring additional staff, implementing new monitoring system, conducting more frequent property inspections, all of which could lead to higher operating expenses.

In line with our expectation, we find that the effects of ZL on both acceptance rates and interest rate spreads are stronger among the riskier mortgages. As shown in Column (1), the interaction term between the ZL dummy and *High LTI* is -0.025 and statistically significant. Thus, the results in Column (1) indicate that the effect of ZL on acceptance rates is concentrated on risky borrowers in the highest LTI quartile. Likewise, the effect become muted for safe borrowers in the highest income quartile (Column (2)). We similarly find that the effect of ZL on interest rate spreads is also concentrated on riskier borrowers, i.e., those having a higher loan-to-value ratio (Column (3)), and a lower FICO score (Column (4)). Overall, our results indicate that after the passage of ZL, lenders exert more effort to screen risky mortgage applications, resulting in lower acceptance rates and higher interest rate spreads.

4.4 Heterogeneous effects across lender characteristics

To further understand lenders' response to ZL, we next exploit cross-sectional variations across different lender characteristics. We expect that lenders will react more strongly to ZL when they have more "skin in the game" and are thus more affected by the law. We use several proxies to capture lenders' skin in the game: (1) *High lender assets*, (2) *High loan-to-assets ratio*, (3) *High equity-to-assets ratio*, which are dummy variables that equal to 1 if the lender's total assets, loan-to-assets ratio, equity-to-assets ratio, respectively, are above the top quartile in a given year, and 0 otherwise; and (4) *Out of state lenders*, a dummy that equals to 1 if the lender's headquarters state differs from the state of the property, and 0 otherwise. We expect the effect of ZL to be less salient among larger lenders since they have more diversified borrower bases and income sources and are therefore less exposed to ZL. In contrast, lenders that rely more heavily on lending are more exposed to the law, and those with higher equity-to-assets ratios could be more prudent in screening applications after ZL. We therefore expect these lenders to react more strongly to ZL. We perform the

analyses only on mortgage acceptance rates using HMDA data because there is no lender identifier in McDash.

[Insert Table 6]

Table 6 displays the results. In line with our expectation, we find that the effects of ZL on mortgage acceptance rates are more pronounced for lenders with more skin in the game, i.e., lenders that are smaller (Column (1)), rely more heavily on lending (Column (2)), and have higher equity-to-asset ratio (Column (3)). We also find the effect to be marginally stronger for out of state lenders that lend to properties in New Jersey (Column (4)). The result is consistent with the fact that New Jersey requires out-of-state lenders to comply with additional requirements, such as appointing an in-state representative, and also imposes heavier fines on out-of-state lenders for non-compliance with ZL ($2,500 per property per day compared to $1,500 for in-state lenders).[20]

4.5 Zombie Law and Mortgage Default Rates

Having shown that lenders exert more effort to screen applications at origination, a natural question that arises is whether mortgages originated after the adoption of Zombie Property Law exhibit better *ex-post* performance. We test for this using loan-level data from McDash. The dependent variables are dummy variables that equal to 1 if a mortgage (1) becomes seriously delinquent, i.e., past 90 days delinquent or (2) goes into foreclosure, and 0 otherwise. The sample, control variables, and fixed effects are similar to those Columns (4)-(5) of Table 4 that uses McDash data.

[Insert Table 7]

As shown in Column (1) of Table 7, after the adoption of ZL law, loans originated in treated states are 0.7 pp less likely to become seriously delinquent compared to the

[20]See "New Law Clears Way to Force Cleanups of Derelict Properties", NJ Spotlight News, September 3, 2014. https://shorturl.at/fqPZ8

counterfactual, albeit the effect is marginally significant at the 10% level. Relative to the average delinquency rate of 5%, the estimate corresponds to an economically significant marginal effect of 14%. The results are thus consistent with the view that the adoption of the ZL incentivizes lenders to screen mortgage applicants more carefully during origination, resulting in lower ex-post default rates. Overall, by holding lenders accountable to the mortgage they originate even after the borrower has defaulted and left, ZL gives lenders more skin in the game for issuing risky mortgages and incentivizes them to screen mortgage applications more carefully during origination. Thus, an important takeaway of our finding is that any initiatives that hold lenders responsible for the mortgage they service would have potential real effects on lending standards.

We find that ZL does not impact the likelihood of mortgage foreclosure (Column (2)). While ZL decreases the likelihood of serious delinquency, it does not affect the probability of foreclosure. In the next section, we demonstrate that this is because ZL reduces lenders' incentives to foreclose on seriously delinquent mortgages.

5 Zombie Law and Mortgage Renegotiation

So far, we show that the adoption of ZL law affects lender behavior at loan origination: it causes lenders to reduce lending and charge borrowers higher interest rates to offset some of the maintenance and litigation costs they may incur in the future. In this section, we focus on lender behavior post origination and examine the impact of ZL law on the likelihood of mortgage renegotiation. We hypothesize that, after the adoption of ZL law, lenders will have an incentive to continue lending to distressed borrowers to avoid taking the responsibility of maintaining the foreclosed properties. As a result, instead of having their property foreclosed, borrowers are allowed to renegotiate their mortgage terms to receive, for instance, an extended repayment period or a lower interest rate.

To examine this hypothesis, we obtain data from the monthly loan updates from McDash. Following the prior literature (Favara and Giannetti, 2017), our sample includes mortgages originated in 2012-2014 that become seriously delinquent for at least 90 days in 2012-2018. This allows us to directly examine how ZL affects lenders' propensity to renegotiate mortgage terms. The dependent variable is *Renegotiated (6 months)*, a dummy that equals to 1 if a loan is renegotiated within six months of entering distress, and 0 otherwise. The control variables and fixed effects are similar to those in Columns (4)-(5) of Table 4 that use McDash data.

[Insert Table 8]

Table 8 displays the results. As shown in Column (1), following the adoption of ZL law, the rate of renegotiation within six months of delinquency is 8.1% higher for distressed loans in ZL states compared to distressed loans in non-ZL states. This effect is statistically significant and economically substantial. Thus, consistent with our hypothesis, after the adoption of ZL law, lenders are more likely to "revive" their distressed borrowers and allow them to renegotiate their mortgage terms.

Having shown that lenders are more likely to allow distressed borrowers to renegotiate mortgage terms after ZL, we next evaluate the efficiency of such decision. To do so, we analyze the likelihood that distressed loans are "cured" from their distress after mortgage renegotiation. The dependent variable *Cure in 6 months* is a dummy variable that equals to 1 if the borrower avoids falling behind payment again in the next six months after having their mortgage terms renegotiated, and 0 otherwise (Columns (2)). In alternative specifications, we also extend the definition of being "cured" to 12 months instead of six months from renegotiation (Columns (3)). We regress *Cured (6 months)* on the interaction between the ZL dummy and *Renegotiated (6 months)* dummy, in addition to the usual set of controls and fixed effects.

We first note that the coefficients on *Renegotiated (6 months)* is positive and statistically significant. This indicates that mortgage renegotiation is, on average, effective in "curing"

distress and increasing the probability that distressed borrowers exit default. Importantly, the interaction coefficients between the ZL dummy and *Renegotiated (6 months)* are not statistically significant in any of the specifications, indicating that distressed loans renegotiated after ZL are *not* more likely to emerge from bankruptcy relative to the counterfactual. Thus, there is no evidence that lender's decision to allow their distressed borrowers to renegotiate their mortgages after ZL is more efficient.

6 Robustness Tests and Further Results

This section presents various robustness tests on our baseline findings.

6.1 Methodological checks

Although our sample does not include observations with changing treatment status over time, we nevertheless follow the staggered DiD literature and allow for heterogeneous treatment effect using the estimator proposed by Sun and Abraham (2021). As shown in Columns (1)-(2) of Table 9, the effects of ZL on the acceptance rate and rate spreads become statistically significant after the passage of ZL. This further confirms the robustness of our inferences.

[Insert Table 9]

In Columns (3)-(4) of Table 9, we show that our results are not sensitive to how standard errors are clustered. In the baseline regressions, we cluster our standard errors at the county-level instead of the state-level to avoid the problem of having too few clusters. As a robustness test, we alternatively cluster our standard errors at the state-level, but circumvent the small cluster problem using wild bootstrap standard errors developed by MacKinnon and Webb (2018). By resampling the data with replacement, the wild bootstrap method allows for a more accurate estimation of standard errors in situations where observations are

correlated within small clusters, leading to more reliable inferences. In Columns (5) amd (6), we double cluster our standard errors at the state- and the year-level. Our results remain robust regardless of the way we cluster our standard errors.

6.2 Falsification Tests

We next conduct several placebo tests. If the enactment of ZL is plausibly random, then in placebo tests where there is no difference in ZL, we should not observe discontinuities in the outcome variables. Our first placebo test alters the treatment date and assumes treatment starts earlier and lasts until the actual implementation date of ZL. Accordingly, *Placebo ZL* is a dummy variable that equals to 1 for all mortgages that are originated from 2013 onwards in New Jersey and from 2015 onwards in New York, and 0 otherwise. Since placebo treatment lasts until the true implementation date, we exclude observations after the passage of ZL. Our second placebo test alters the treatment location by artificially "moving" state borders inside the non-ZL states. In this case, both the placebo treated and placebo control observations are located in non-ZL states and are within 10 miles from the state border. For both placebo tests, we re-estimate equation (1) using *Placebo ZL* as the main explanatory variable.

[Insert Table 10]

As shown in Table 10, the coefficients on *Placebo ZL* are statistically insignificant across all specifications. Thus, there is no difference in mortgage acceptance rates or rate spreads at the placebo year when ZL has not been enacted (Columns (1)-(2)) or when the placebo location is used (Columns (3)-(4)). If our results are driven by omitted variables such as differences in foreclosure patterns or economic conditions between ZL and non-ZL states, we should continue to find significant effects when using the placebo year or placebo location. Overall, this evidence reinforces our argument that ZL law is plausibly exogenous to the outcome variables and that our findings are unlikely to be driven by omitted variables.

6.3 Does securitization undermine the effectiveness of ZL?

Recent studies suggest that mortgage securitization may weaken the effectiveness of housing market policies by reducing lenders' skin in the game. This is unlikely to be the case for ZL because the law requires the mortgage servicers to bear the maintenance responsibilities in foreclosure. Since the originating lenders continue to be the mortgage servicers of nearly every mortgage that they sell (Federal Reserve Board, 2016),[21] they are responsible to maintain the foreclosed property even after the mortgage is sold. Nevertheless, to further explore the role of mortgage securitization, we examine in Table 11 whether the effects of ZL on mortgage acceptance rates and interest rates depend on lenders' securitization decisions.

[Insert Table 11]

Because we can only observe securitization in accepted mortgages, we perform this analysis in two steps. In the first step in Column (1), we estimate the probability that a mortgage will be securitized using a sample of accepted mortgages. In the second step, we re-estimate the baseline regressions in Table 4, this time including an interaction term between *Zombie Law* and *Likely To Be Sold*, a dummy variable that equals to 1 if the mortgage's estimated probability of being sold is above the median in a given year. As shown in Columns (2) and (3), the interaction terms are statistically insignificant, indicating that the likelihood of securitization does not weaken the effects of ZL on either acceptance rates (Column (2)) or interest rates (Column (3)). Moreover, the single coefficients on *Zombie Law* remain statistically significant in both columns, further suggesting that mortgage securitization is unlikely to affect our inferences.

[21] Report to the Congress on the Effect of Capital Rules on Mortgage Servicing Assets, Federal Reserve Board, 2016

6.4 Other State Laws and Economic Conditions

We next address the concern that our results could be driven by other confounding state-level laws by controlling for several state-level laws that are known to affect mortgage lending practices. Specifically, we control for (1) *State corporate tax rate*, as it has been shown to affect mortgage origination and securization behavior (Han et al., 2015); (2) *Broker restriction index*, since broker laws may affect the relationship between lenders and borrowers, where lenders tend to originate riskier mortgages in states with more lenient broker laws (Agarwal et al., 2021), (3) *Zoning index*, as zoning restrictions may lead to lower mortgage origination and higher interest rates; (4) *Land use regulation index*, since more restrictive residential land use regulations could cause house prices to become sensitive to boom and bust cycle and affect household wealth (Huang and Tang, 2012); and (5) *Ln(Homestead exemption)*, as personal bankruptcy laws could influence how lenders supply mortgages and how they renegotiate with borrowers (Berkowitz and Hynes, 1999).

[Insert Table 12]

As shown in Table 12, our findings are robust to controlling for all state-level laws. In addition, to address the concern that Massachusetts does not have judicial foreclosure laws in place while all other states in the sample do (Dagher and Sun, 2016), we exclude observations located in the state of Massachusetts and display the results in Column (6) for acceptance rates and Column (12) for interest rate spreads. Our results remain robust.[22]

Finally, although the state border design has mitigated the concern that our results are driven by local housing and economic conditions to a large extent, we perform additional tests to further control for several time-varying county-level housing and economic characteristics across ZL and non-ZL states. In particular, we control for the (1) ratio of owner-occupied houses over the total number of residential properties, (2) number of houses per capita, and (3) unemployment rate. This mitigates the concern that ZL states may have fewer available

[22]All states in our sample have the same recourse laws.

owner-occupied houses and worse economic conditions, which could affect the supply and cost of mortgage credit.

[Insert Table 13]

As shown in Table 13, we continue to find ZL to reduce mortgage acceptance rates and increase interest rate spreads after controlling for all of the time-varying county-level housing and economic characteristics. Moreover, with one exception, the coefficients on the county characteristics are statistically insignificant. Collectively, the results in Tables 12 and 13 confirm that our main findings are not driven by other state-level laws or local economic conditions.

6.5 Assessing potential bias from unobservable omitted variables

We use a methodology developed by Oster (2019) to assess the potential bias from unobservable omitted variables. This test computes the degree of selection on unobservables relative to observables that would reduce the effect of interest to zero. This ratio is denoted as δ. For instance, δ=2 would indicate that unobservables need to be two times as important as observables for omitted variable bias to explain away the entire effect of ZL on mortgage lending outcomes and reduce the coefficient of interest to zero.

[Insert Table 14]

We follow Oster (2019) in setting the estimation inputs and display the results in Table 14. In the acceptance rate regressions, the absolute value of the δ is 27. This is substantially higher than the robustness benchmark of one recommended by Oster (2019) and indicates the unobservables would need to be around 27 times as important as the observables to reduce the coefficient on ZL to zero. This is unlikely given that our regressions already include many important determinants of acceptance rates. The δ estimate in the interest rate regressions is three, indicating that unobservable factors must be three times as important as observable ones to bring the effect of ZL to 0.

An alternative approach to assess the robustness of the results is to estimate a possible ranges for the main coefficient β which is $[\beta^*, \beta_{\text{Full}}]$, where β_{Full} is the ZL coefficient in the baseline specification in Table 4 and β^* is the bias-adjusted treatment effect. If 0 does not fall within the range of β, the main findings are considered robust. Table 14 shows that our β ranges for both the acceptance rates and interest rates regressions do not include 0. Therefore, our findings are unlikely to be influenced by omitted variable bias.

6.6 Further Results

In this subsection, we explore the broader economic implications of the Zombie Property Law by evaluating the effect of ZL on local house prices and crime rate. On the one hand, by making lenders responsible for the mortgages they originate, ZL puts some pressure on lenders to carefully screen risky mortgage applications and maintain the property upon foreclosure. This could lead to higher house prices and a lower crime rate as the neighborhood quality improves. On the other hand, we also see that ZL incentivizes lenders to continue lending to distressed borrowers to avoid the maintenance duties. This could have an adverse effect on house prices and crime rates since houses owned by risky borrowers do not get revitalized quickly enough.

To test this, we run regressions on property prices at the 5-digit ZIP code level and on crime rates at the county level. All regressions control for the county-level averages of sold properties-to-property stock, unemployment rate, population-to-property stock, and mortgage foreclosure and delinquency rates (Favara and Giannetti, 2017).

Appendix C displays the results. The dependent variables in Columns (1) and (2) are the natural logarithm of the five-digit postcode Property Price Index obtained from Corelogic and Zillow, respectively. In Columns (3) and (4), the dependent variables are the share of criminal court cases (Column (3)) and housing-related criminal court cases (Column (4)) in the county per 100,000 population. As shown in Appendix C, the coefficients on *ZL* are

statistically insignificant across all outcome variables, suggesting that ZL does not have any real effects on local house prices or crime rates.

7 Conclusion

In this paper, we study the effect of zombie property laws, which require lenders to maintain properties after they enter foreclosure, on mortgage lending decisions and standards. Using a DiD design that compares mortgages originated within a narrow geographical bandwidth around the state borders, our study offers two key insights. First, we find that lenders respond to Zombie Law by screening mortgage applications more prudently. They deny more mortgage applications, impose higher interest rates on originated mortgages, especially risky mortgages. Thus, by tying lenders' responsibility to the foreclosure property, ZL incentivizes them to exert more effort to carefully screen mortgage applications during origination. One unique feature of our findings is that securitization does not weaken the effect of ZL because the law requires mortgage servicers to maintain the vacant properties during the foreclosure process and all most all mortgages in the U.S are still serviced by the original loan originators. Our findings highlight that any initiatives that hold lenders responsible for mortgages that they service could increase their skin in the game and potentially have real effects on lending standards.

Second, beyond prompting more rigorous screening at origination, we find that zombie property laws also influence lender behaviours when borrowers enter financial distress. Specifically, to avoid the obligation of maintaining vacant properties during the foreclosure process, lenders are more likely to keep delinquent mortgages alive, even though these mortgages are not significantly more likely to recover after these modification efforts. This behavior has important implications, as renegotiating distressed loans can lead to inefficient resource allocation, with excessive capital tied up in unviable projects and inadequate investment in viable ones (Bolton, 1990; Holden, 1999). By highlighting this unintended

moral hazard, our findings suggest a need for policies that address this inefficiency in lender behavior. Exploring ways to mitigate this moral hazard in lender behavior could be a valuable avenue for future research.

Our results inform regulators on the real effects of zombie property law. Understanding this is especially important for policymaking as the foreclosure rate is on the rise again, not only in the US, but globally, in the aftermath of the Covid-19 crisis. This understanding is essential for policymakers aiming to craft effective responses to rising foreclosure rates and market instability. By highlighting the impact of these laws on lending standards and lender behavior, our work broadens the discussion on zombie lending and underscores the importance of regulatory frameworks that address lender accountability in the housing sector.

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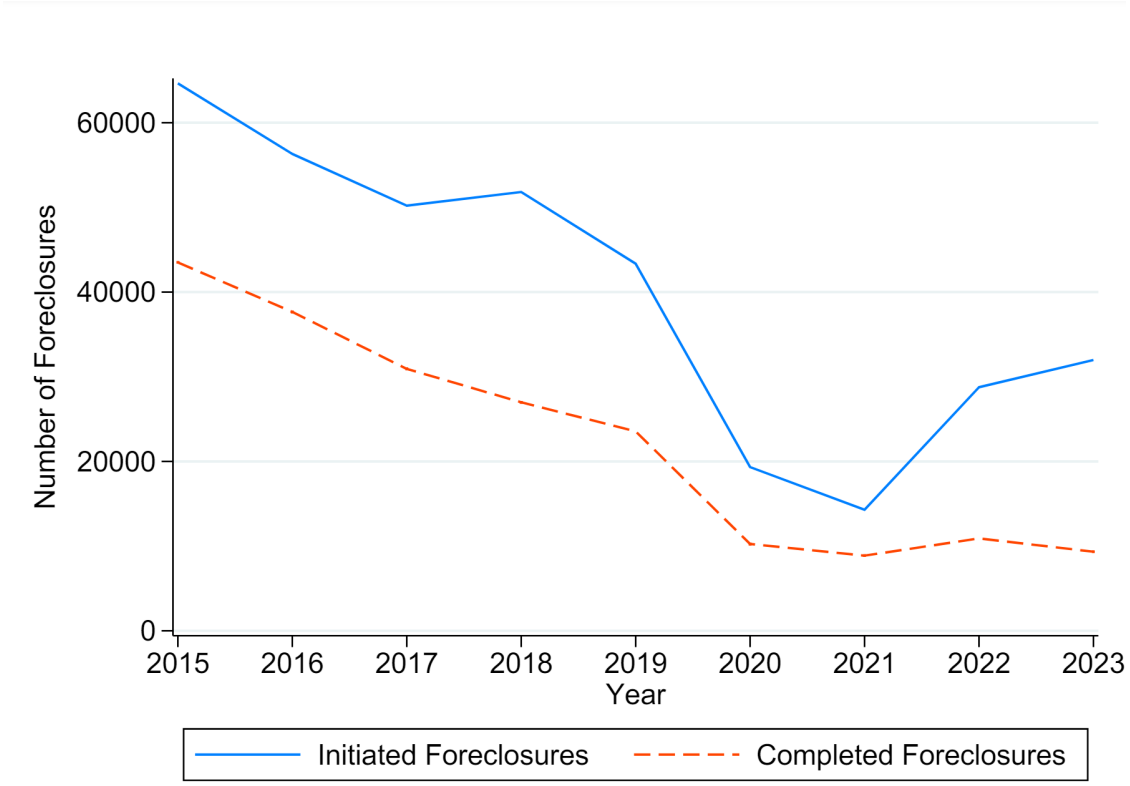
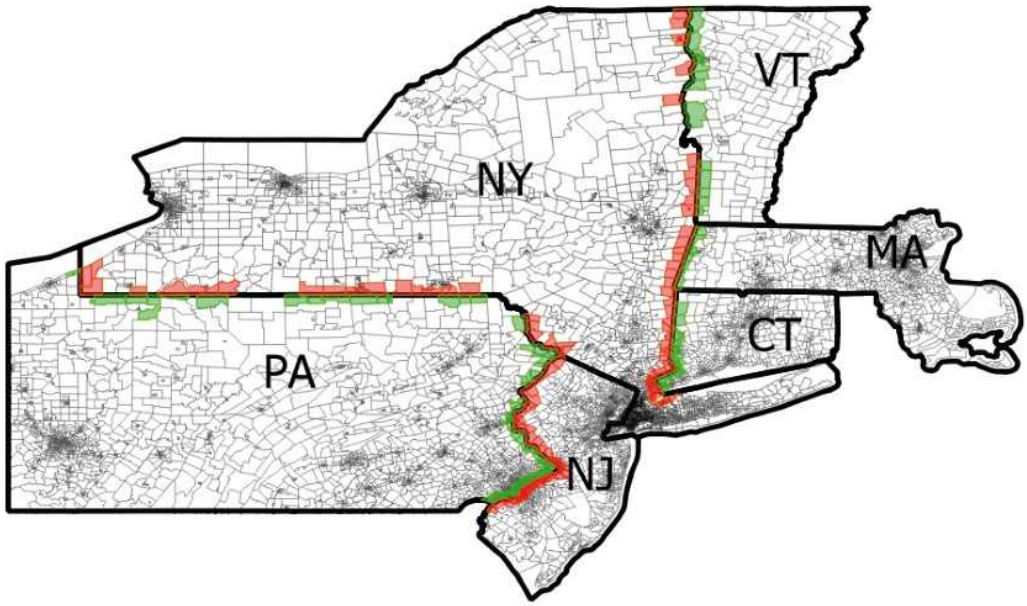
Figures

Figure 1: US Mortgage Foreclosures between 2015 and 2023



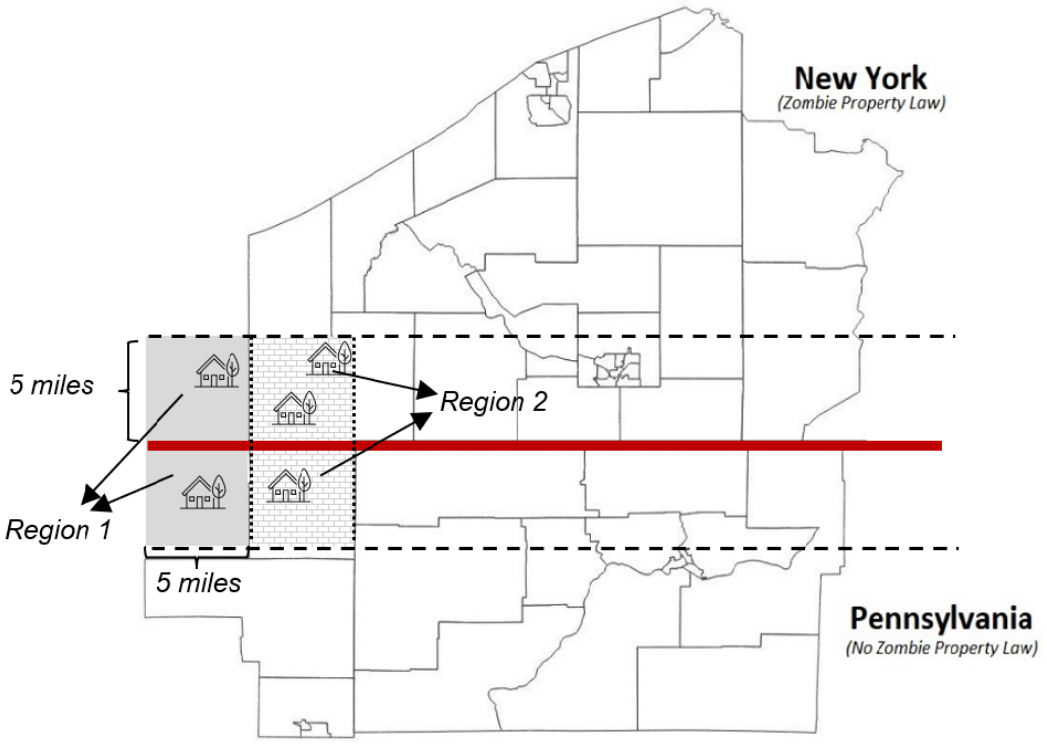
Notes: The figure shows the evolution of foreclosure cases in the US between 2015 and 2023. The solid line shows the initiated foreclosure cases, whereas the dashed line shows the completed foreclosure cases.

Figure 2: Data Sample



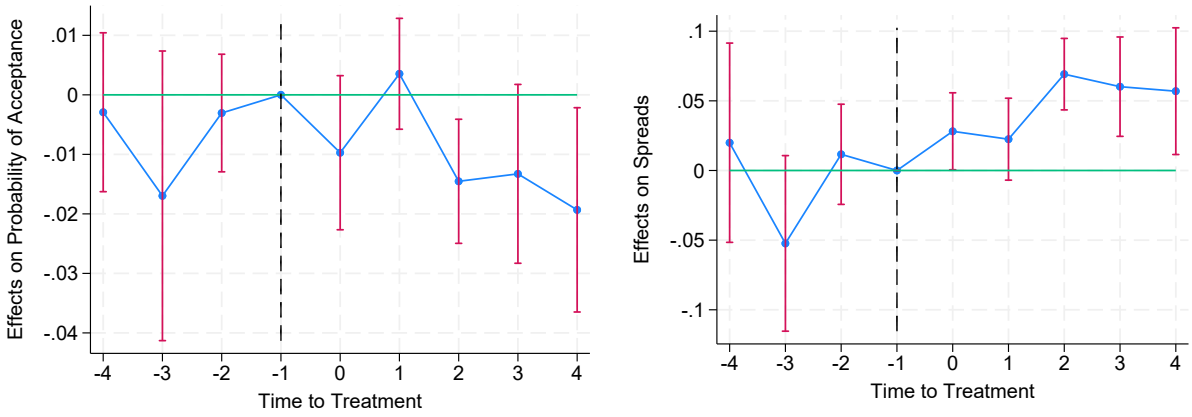
Notes: This figure shows census tracts that are located 5 miles on either side of the state borders between ZL (NY and NJ) and non-ZL (PA, VT, MA, and CT) states which are included in our analysis. The border between NY and NJ where the treatment status changes over time is excluded from the analysis.

Figure 3: Region Year Fixed Effects



Note: The figure shows an example of how we construct our region to use region × year fixed effects in our empirical analysis. We define a region as an area 10 miles long by 5 miles wide that overlaps the borders between ZL and non-ZL states.

Figure 4: Dynamic treatment effects



Notes: The Figure plots two-way fixed effects event-study coefficient estimates for relative-time periods from 4 years before to 4 years after the passage of ZL. The Figure on the left shows the dynamic treatment effects of ZL on mortgage acceptance rates. The Figure on the right shows the dynamic treatment effects of ZL on interest rate spreads. The vertical lines indicate 95% confidence intervals.

Tables

Table 1: Summary Statistics

	N	Mean	SD	Min	Max
Panel A: Sample on mortgage applications between 2012 and 2018					
Data source: HMDA					
Accept	199,076	0.7835	0.4118	0	1
Zombie Law	199,076	0.1767	0.3815	0	1
Ln(Loan Amount)	199,076	12.1959	0.9287	6.9078	17.5073
Ln(Applicant Income)	199,076	11.514	0.8515	6.9078	16.4262
LTI	199,076	2.37	1.3093	0.0833	7.5
Male	199,076	0.6143	0.4868	0	1
Minority	199,076	0.2834	0.4506	0	1
Coapplicant	199,076	0.4398	0.4964	0	1
Jumbo	199,076	0.1638	0.3701	0	1
State corporate tax (%)	199,076	9.1017	1.1499	6.5	9.99
Broker restriction index	199,076	7.9781	3.006	4	13
Zoning index	199,076	7.9918	2.9988	2	12
Land-use regulation index	199,076	12.4681	5.6318	2	23
Homestead exemption (ln)	199,076	10.9456	0.5369	10.6066	13.1224
Panel B: Sample on originated mortgages between 2012 and 2018					
Data source: McDash					
Loan Spread (%)	30,734	0.9458	0.4822	0	7.17
Serious Delinquency	30,734	0.0488	0.2155	0	1
Foreclosure	30,734	0.0299	0.1704	0	1
Zombie Law	30,734	0.2858	0.4518	0	1
FICO	30,734	716.9317	56.6386	457	850
LTV	30,734	87.2464	14.2568	17.32	99.4600
Ln(Loan Amount)	30,734	12.3465	0.8034	8.8537	16.2751
Low Documentation	30,734	0.0299	0.1704	0	1
Jumbo	30,734	0.1655	0.3716	0	1
Prepayment Penalty	30,734	0.0211	0.1438	0	1
State corporate tax (%)	30,734	8.7670	1.1166	6.5	9.99
Broker restriction index	30,734	9.2696	3.704	4	13
Zoning index	30,734	9.3167	3.0544	2	12
Land-use regulation index	30,734	13.0228	5.9585	2	23
Homestead exemption (ln)	30,734	10.9394	0.5021	10.6066	13.1224
Panel C: Sample on mortgages becoming seriously delinquent (89+days) pre-ZL					
Data source: McDash					
Renegotiated (6 months)	892	0.1132	0.317	0	1
Cured in 6 months	892	0.4473	0.4975	0	1
Cured in 12 months	892	0.352	0.4779	0	1
Ln(Loan Amount)	892	11.893	0.4233	10.5187	13.3847
Loan Spread (%)	892	1.031	0.547	0.05	3.76
Zombie Law	892	0.4709	0.4994	0	1
FICO	892	669.861	42.9156	479	819
LTV	892	93.9238	7.2065	40.35	99.94
Low Documentation	892	0.0168	0.1287	0	1
Prepayment Penalty	892	0.0022	0.0473	0	1
Jumbo	892	0.0056	0.0747	0	1

Notes: This table provides descriptive statistics for the variables used in the empirical analysis. 'Ln' denotes that a variable is measured in natural logarithms. Variable descriptions are in Appendix A.

Table 2: Manipulation Tests

	1	2	3	4
Dependent variable	Ln(Number of applications)		Ln(Total Loan Amount)	
Zombie Law	0.017	0.016	0.008	0.004
	(0.130)	(0.074)	(0.302)	(0.120)
Ln(Applicant Income)		0.402***		0.986***
		(0.041)		(0.055)
Male		-0.074***		-0.119***
		(0.023)		(0.031)
Minority		-0.332***		-0.565***
		(0.032)		(0.051)
Coapplicant		0.052*		0.080*
		(0.030)		(0.048)
Jumbo		0.065		0.472***
		(0.054)		(0.076)
Observations	6,263	6,263	6,263	6,263
Adjusted R^2	-0.000	0.218	-0.000	0.445

Notes: This table reports manipulation tests. Regressions are at the census-tract level. The dependent variable in Columns (1) and (2) is the natural logarithm of the total number of mortgage applications submitted in a given census-tract in a given year. The dependent variable in Columns (3) and (4) is the natural logarithm of the total loan amount requested in a given census-tract in a given year. Standard errors clustered at the county-level are reported in parentheses. Variable descriptions are in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3: Parallel trends

	(1)	(2)	(3)	(4)	(5)	(6)
	Non ZL states	N	ZL states	N	Differences	t-stat
△ Accept	0.025	1093	0.012	683	0.013	1.357
△ Loan Spread	-0.049	110	0.006	247	-0.055	-0.662
△ Serious Delinquency	-0.025	110	-0.020	247	-0.004	-0.224
△ Foreclosure	-0.004	110	-0.010	247	0.006	0.478
△ Renegotiated (6 months)	0.004	89	0.005	226	-0.001	-0.634
△ Cured (6 months)	-0.066	89	-0.048	226	-0.018	-1.141
△ Cured (12 months)	-0.064	89	-0.061	226	-0.003	-0.162

Notes: This table presents the results of parallel trend tests for all dependent variables. The data are aggregated at the census tract level (HMDA) and 5-digit ZIP code level (McDash). Columns (1) and (3) report the mean annual change in the dependent variables in non-ZL states and ZL states, respectively. Columns (2) and (4) display the number of observations in non-ZL and ZL states, respectively. Column (5) shows the differences in mean values between non-ZL and ZL states, while Column (6) reports the t-statistics for these differences. Variable descriptions are in Appendix A.

Table 4: The Effect of Zombie Law on Mortgage Acceptance and Interest Rates

	1	2	3	4	5
Dependent variables	Accept			Loan Spread	
Zombie Law	-0.011*	-0.011**	-0.013***	0.084***	0.045***
	(0.006)	(0.005)	(0.005)	(0.021)	(0.010)
LTI		-0.058***	-0.058***		
		(0.010)	(0.010)		
Male		-0.013***	-0.013***		
		(0.002)	(0.002)		
Minority		-0.061***	-0.060***		
		(0.006)	(0.006)		
Coapplicant		0.018***	0.018***		
		(0.003)	(0.003)		
Ln(Applicant Income)		-0.062***	-0.062***		
		(0.016)	(0.016)		
Ln(Loan Amount)		0.167***	0.167***		-0.175***
		(0.008)	(0.008)		(0.008)
Jumbo		-0.075***	-0.077***		0.021
		(0.016)	(0.016)		(0.013)
FICO					-0.001***
					(0.000)
LTV					-0.001
					(0.001)
Low Documentation					-0.027
					(0.016)
Prepayment Penalty					0.045
					(0.041)
Observations	199,076	199,076	199,076	30,734	30,734
Region × Year FE	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	No	No	No
Lender × Year FE	No	No	Yes	No	No
Occupancy FE	Yes	Yes	Yes	Yes	Yes
County-level lender controls	No	No	No	Yes	Yes
Adjusted R^2	0.125	0.162	0.168	0.333	0.390
Clustering	County	County	County	County	County
Data	HMDA	HMDA	HMDA	McDash	McDash

Notes: This table shows the estimation for equation 1 and illustrates how ZL affects mortgage acceptance rates and loan spreads. In Columns (1), (2), and (3), the dependent variable is *Accept* and in Columns (4) and (5), the dependent variable is *Loan Spread*. Standard errors clustered at the county level are reported in parentheses. Variable descriptions are in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Heterogeneous Effects of Zombie Law across Borrower Creditworthiness

Dependent variables	(1) Accept	(2) Accept	(3) Loan Spread	(4) Loan Spread
Zombie Law	-0.003	-0.014***	0.035***	0.068***
	(0.006)	(0.005)	(0.011)	(0.010)
Zombie Law × High LTI	-0.025**			
	(0.011)			
Zombie Law × High Income		0.007*		
		(0.004)		
Zombie Law × High LTV			0.032***	
			(0.010)	
Zombie Law × High FICO				-0.096***
				(0.012)
High LTI	-0.051***			
	(0.008)			
High Income		-0.052***		
		(0.003)		
High LTV			-0.032***	
			(0.004)	
High FICO				-0.051***
				(0.005)
Observations	199,076	199,076	30,734	30,734
Region × Year FE	Yes	Yes	Yes	Yes
Lender × Year FE	Yes	Yes	No	No
Occupancy FE	Yes	Yes	Yes	Yes
County-level lender controls	No	No	Yes	Yes
Adjusted R^2	0.162	0.168	0.390	0.377
Clustering	County	County	County	County
Data	HMDA	HMDA	McDash	McDash

Notes: This table shows how ZL affects mortgage acceptance rates and loan spreads for high vs low risk borrowers. In Columns (1) and (2), the dependent variable is *Accept* and in Columns (3) and (4), the dependent variable is *Loan Spread*. *High LTI* and *High Income* are dummy variables that equal to 1 if the loan's LTI ratio and the applicant's income, respectively, are above the top quartile (i.e., 75th percentile) in a given year, and 0 otherwise. *High LTV* and *High FICO* are dummy variables that equal to 1 if the applicant's LTV ratio and their FICO score, respectively, are above the top quartile in a given year, and 0 otherwise. Standard errors clustered at the county level are reported in parentheses. Variable descriptions are in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Bank characteristics and Heterogeneous Effects of Zombie Law

	(1)	(2)	(3)	(4)	(5)
Dependent variable	Accept				
Sample	Full			NJ pairs	NY pairs
Zombie Law	-0.052***	-0.009	-0.008	0.008	-0.029
	(0.014)	(0.014)	(0.015)	(0.018)	(0.028)
Zombie Law × High lender assets	0.049**				
	(0.023)				
Zombie Law × High loan-to-assets ratio		-0.074***			
		(0.027)			
Zombie Law × High equity-to-assets ratio			-0.033**		
			(0.015)		
Zombie Law × Out of state banks				-0.033*	0.039
				(0.017)	(0.028)
High lender assets	-0.106***				
	(0.025)				
High loan-to-assets ratio		0.031**			
		(0.014)			
High equity-to-assets ratio			0.031		
			(0.021)		
Out of state banks				0.014	-0.052***
				(0.014)	(0.012)
Observations	106,862	106,862	106,862	63,583	43,279
Region × Year FE	Yes	Yes	Yes	Yes	Yes
Occupancy FE	Yes	Yes	Yes	Yes	Yes
Lender-level controls	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.104	0.102	0.102	0.134	0.060
Clustering	County	County	County	County	County
Data	HMDA	HMDA	HMDA	HMDA	HMDA

Notes: This table shows how ZL affects mortgage acceptance rates across different bank characteristics. In all columns, the dependent variable is *Accept*. *High lender assets*, *High loan-to-assets*, and *High equity-to-assets* ratio are dummy variables that equal to 1 if the lender's total assets, loan-to-assets ratio, and equity-to-assets ratio, respectively, are above the top quartile in a given year, and 0 otherwise. *Out of state lenders* is a dummy variable that equals to 1 if the lender's headquarters state differs from the state of the property, and 0 otherwise. Standard errors clustered at the county level are reported in parentheses. Variable descriptions are in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7: The Effect of Zombie Law on Mortgage Default and Foreclosure Rates

Dependent variables	(1) Serious delinquency	(2) Foreclosure
Zombie Law	-0.007*	-0.002
	(0.004)	(0.003)
Jumbo	0.008	0.007*
	(0.005)	(0.004)
FICO	-0.001***	-0.000***
	(0.000)	(0.000)
LTV	0.000**	0.000
	(0.000)	(0.000)
Ln(Loan amount)	-0.008**	-0.007**
	(0.003)	(0.003)
Low documentation	-0.008	-0.001
	(0.006)	(0.004)
Prepayment Penalty	-0.010**	-0.011
	(0.004)	(0.009)
Observations	30,734	30,734
Region × Year FE	Yes	Yes
Occupancy FE	Yes	Yes
County-level lender controls	Yes	Yes
Adjusted R^2	0.068	0.044
Clustering	County	County
Data	McDash	McDash

Notes: This table shows how ZL affects mortgage default and foreclosure rates. In Column (1), the dependent variable is *Serious delinquency* and in Column (2), the dependent variable is *Foreclosure*. Standard errors clustered at the county level are reported in parentheses. Variable descriptions are in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8: The Effect of Zombie Law on Mortgage Renegotiation and Cure Rates

	1	2	3
Dependent variables	Renegotiated (6 months)	Cured (6 months)	Cured (12 months)
Zombie Law	0.081**	-0.013	-0.004
	(0.037)	(0.037)	(0.039)
Renegotiated		0.365***	0.203***
		(0.074)	(0.036)
Zombie Law x Renegotiated		0.078	0.045
		(0.084)	(0.054)
FICO	0.000	-0.001	-0.001
	(0.000)	(0.001)	(0.001)
Original LTV	-0.000	0.000	-0.001
	(0.001)	(0.001)	(0.002)
Loan amount (ln)	0.014	-0.032	0.041
	(0.039)	(0.048)	(0.041)
Low documentation	0.154	-0.122	-0.052
	(0.098)	(0.111)	(0.116)
Jumbo	-0.051	0.369	-0.204
	(0.065)	(0.304)	(0.179)
Prepayment Penalty	-0.069	0.389	0.027
	(0.044)	(0.450)	(0.027)
Observations	892	892	892
Region × Year FE	Yes	Yes	Yes
Occupancy FE	Yes	Yes	Yes
County-level lender controls	Yes	Yes	Yes
Adjusted R^2	-0.017	0.136	0.286
Clustering	County	County	County
Data	McDash	McDash	McDash

Notes: This table shows how ZL affects mortgage renegotiation and cure rates. The sample includes seriously delinquent mortgages that originated in the pre-ZL period, i.e., 2012-2014. The dependent variables are *Renegotiated (6 months)* (Column (1)), *Cured (6 months)* (Column (2)), and *Cured (12 months)* (Column (3)). Standard errors clustered at the county level are reported in parentheses. Variable descriptions are in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 9: Methodology Robustness Checks

	1	2	3	4	5	6
	Staggered DID		State Wild Bootstrap		Double Clustering	
Dependent variable	Accept	Loan Spread	Accept	Loan Spread	Accept	Loan Spread
Zombie Law			-0.013*	0.045***	-0.013**	0.045***
			(0.003)	(0.005)	(0.006)	(0.013)
Lag 4	-0.003	0.021				
	(0.006)	(0.020)				
Lag 3	-0.003	-0.018				
	(0.008)	(0.025)				
Lag 2	0.001	0.022				
	(0.005)	(0.021)				
Zero	0.006	0.023				
	(0.006)	(0.034)				
Lead 1	-0.020***	0.070***				
	(0.006)	(0.011)				
Lead 2	-0.009	0.067*				
	(0.008)	(0.036)				
Lead 3	-0.013*	0.060***				
	(0.008)	(0.018)				
Lead 4	-0.019**	0.057**				
	(0.009)	(0.023)				
Observations	199,076	30,734	199,076	30,734	199,076	30,734
Region × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender × Year FE	Yes	No	Yes	No	Yes	No
Occupancy FE	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
County-level lender controls	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.162	0.390	0.125	0.168	0.168	0.390
Clustering	County	County	State	State	State and Year	State and Year
Data	HMDA	McDash	HMDA	McDash	HMDA	McDash

Notes: This table reports robustness tests on the effect of ZL on acceptance rates and interest rate spreads. In Columns (1) and (2), we document the effect of ZL on acceptance rates and interest rate spreads using Sun and Abraham (2021)'s approach. *Lag 4*, *Lag 3*, *Lag 2* are dummy variables that respectively indicate four, three, and two years before ZL is passed; Zero is a dummy variable that equals 1 for the year when ZL is passed; *Lead 1*, *Lead 2*, *Lead 3*, and *Lead 4* are dummy variables that respectively indicate one, two, three, and four years after ZL is passed. In Columns (3) and (4), we use the state wild bootstrap methodology by MacKinnon and Webb (2018) to cluster our standard errors. In Columns (5) and (6), we double cluster our standard errors at the state and year level. The dependent variable in Columns (1) and (3) is *Accept* and the dependent variable in Columns (2) and (4) is *Loan Spread*. Variable descriptions are in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 10: Falsification Tests

	(1)	(2)	(3)	(4)
Dependent variable	Accept	Loan Spread	Accept	Loan Spread
Placebo types:	Placebo timing		Placebo location	
Placebo ZL	-0.007	0.019	-0.002	-0.010
	(0.006)	(0.013)	(0.006)	(0.007)
Observations	163,770	21,930	228,600	32,390
Region × year FE	Yes	Yes	Yes	Yes
Lender × Year FE	Yes	No	Yes	No
Occupancy FE	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes
County-level lender controls	No	Yes	No	Yes
Adjusted R^2	0.172	0.322	0.173	0.367
Cluster	County	County	County	County
Data	HMDA	McDash	HMDA	McDash

Notes: This table reports results from placebo tests. In Columns (1) and (3), the dependent variable is *Accept* and in Columns (2) and (4), the dependent variable is *Loan Spread*. In Columns (1) and (2), we assume treatment starts earlier and finishes before the actual implementation of ZL. *Placebo ZL* is a dummy variable that equals to 1 for all mortgages that are originated from 2013 onwards in New Jersey and from 2015 onwards in New York, and 0 otherwise. The sample in Columns (1) and (2) excludes observations after the passage of ZL. In Columns (3)-(4), we alter the treatment location. The sample includes observations from non-ZL states and are located within 10 miles from the state border. *Placebo ZL* is a dummy variable that equals 1 for observations in areas where the distance to the state border is larger than or equal to the sample's median, i.e., 2.873 miles, and is originated after 2014 (2016) if its neighboring ZL state is NJ (NY), 0 otherwise. Standard errors clustered at the county level are reported in parentheses. Variable descriptions are in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 11: Does Mortgage Securitization Moderate the Effects of Zombie Law?

Dependent variables	(1) Sold	(2) Accept	(3) Spread
Sample	Mortgage Originated before ZL	Full	Full
Sample	HMDA	HMDA	McDash
Zombie Law		-0.021**	0.040***
		(0.009)	(0.012)
ZL x Likely To Be Sold		0.014	
		(0.009)	
Likely To Be Sold		0.026**	
		(0.010)	
ZL × Sold Mortgage			0.011
			(0.015)
Sold Mortgage			0.027***
			(0.009)
LTI	-0.045***	-0.054***	
	(0.006)	(0.009)	
Male	-0.002	-0.012***	
	(0.003)	(0.002)	
Minority_app	-0.011***	-0.057***	
	(0.003)	(0.007)	
Coapplicant	0.010**	0.016***	
	(0.005)	(0.003)	
Ln(Applicant Income)	-0.119***	-0.050***	
	(0.018)	(0.012)	
Ln(Loan_Amount)	0.141***	0.149***	0.177***
	(0.013)	(0.009)	(0.009)
Jumbo	-0.374***	-0.047*	0.025*
	(0.044)	(0.028)	(0.014)
FICO			-0.001***
			(0.000)
LTV			-0.000
			(0.001)
Low documentation			-0.048***
			(0.018)
Prepayment Penalty			0.099
			(0.069)
Observations	55,797	199,076	25,764
Bank × Year FE	Yes	Yes	No
Region × Year FE	Yes	Yes	Yes
Occupancy FE	Yes	Yes	Yes
County-level lender controls	No	No	Yes
Clustering	County	County	County
Adjusted R^2	0.574	0.168	0.407

Notes: This table reports the heterogeneous effects of ZL on spreads for sold mortgages. Column (1) estimates the likelihood that a mortgage is sold after origination. The dependent variable *Sold* is a dummy variable that equals to 1 if a mortgage is sold, and 0 otherwise. The sample in Column (1) includes mortgages that are originated before ZL. Columns (2) and (3) analyze the impact of ZL on acceptance rates and interest rates, respectively. *Likely To Be Sold* is a dummy variable that equals 1 if the loan's estimated probability of being sold (based on Column (1)) is above the median in a given year, and 0 otherwise. Standard errors clustered at the county level are reported in parentheses. Variable descriptions are in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 12: Robustness Checks on Other State Laws

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Accept						Loan Spread					
Sample	(All)	(All)	(All)	(All)	(All)	(Without MA)	(All)	(All)	(All)	(All)	(All)	(Without MA)
Zombie law	-0.010**	-0.013**	-0.011**	-0.013***	-0.013**	-0.013**	0.041***	0.043***	0.035***	0.049***	0.046***	0.043***
	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.010)	(0.014)	(0.008)	(0.011)	(0.010)	(0.010)
State corporate tax (%)	0.282						-0.007					
	(0.296)						(0.008)					
Broker restriction index		-0.000						0.000				
		(0.001)						(0.001)				
Zoning index			-0.001						0.006**			
			(0.001)						(0.002)			
Land-use regulation index				-0.000						0.001		
				(0.000)						(0.001)		
Ln(homestead exemptions)					-0.005						-0.029**	
					(0.007)						(0.014)	
Observations	199,076	199,076	199,076	199,076	199,076	197,771	30,734	30,734	30,734	30,734	30,734	30,659
Region × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupancy FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No	No
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Yes	Yes											
County-level lender controls	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.168	0.168	0.168	0.168	0.168	0.168	0.390	0.390	0.390	0.390	0.390	0.390
Clustering	County	County	County	County	County	County	County	County	County	County	County	County
Data	HMDA	HMDA	HMDA	HMDA	HMDA	HMDA	McDash	McDash	McDash	McDash	McDash	McDash

Notes: This table reports results that control for several state-level laws. Columns (1) and (7) control for state-level corporate tax. Data on state-level corporate tax come from from the Tax Foundation. Columns (2) and (8) control for the Broker Restrictiveness Index as in Pahl et al. (2007). Columns (3) and (9) control for the single-family home zoning restriction index collected from Calder (2017). Columns (4) and (10) control for the Ln(homestead exemptions) as in (Corradin et al., 2016). Columns (5) and (11) control for the land-use regulation index collected from Calder (2017). In Columns (6) and (12), we exclude observations from Massachusetts, the only state in our sample that does not have judicial foreclosure laws. The dependent variable in Columns (1)-(6) is *Accept* and in Columns (7)-(12) is *Loan Spread*. Standard errors clustered at the county level are reported in parentheses. Variable descriptions are in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 13: Robustness Checks on Economic and Housing Market Conditions

Dependent Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Accept				Loan Spread			
ZL	-0.012**	-0.013**	-0.013**	-0.012**	0.046***	0.040***	0.042***	0.042***
	(0.006)	(0.005)	(0.006)	(0.005)	(0.007)	(0.007)	(0.008)	(0.015)
Owner Occuppied	0.001			-0.001	0.001***			0.001*
	(0.000)			(0.000)	(0.000)			(0.000)
Houses Per Capita		0.001		-0.000		-0.003		0.001
		(0.001)		(0.002)		(0.004)		(0.004)
Unemployment Rates			0.003	-0.003			0.006***	-0.003
			(0.003)	(0.004)			(0.002)	(0.005)
Observations	173,107	173,107	173,107	173,107	29,326	29,326	29,326	29,326
Region × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupancy FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender × Year FE	Yes	Yes	Yes	Yes	No	No	No	No
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-level lender controls	No	No	No	No	Yes	Yes	Yes	Yes
Adjusted R^2	0.177	0.177	0.177	0.177	0.392	0.392	0.392	0.392
Cluster	County	County	County	County	County	County	County	County
Data	HMDA	HMDA	HMDA	HMDA	McDash	McDash	McDash	McDash

Notes: This table reports results that control for time-varying county-level housing and economic characteristics. Columns (1) and (5) control for the ratio of owner-occupied houses over the total number of residential properties. Columns (2) and (6) control the number of houses per capita. Columns (3) and (7) control for unemployment rates. Columns (4) and (8) include all county-level housing and economic characteristics in the same regressions. The dependent variable in Columns (1)-(4) is *Accept* and in Columns (5)-(8) is *Loan Spread*. Standard errors clustered at the county level are reported in parentheses. Variable descriptions are in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 14: Robustness tests: Bias from unobservables

Dependent variable	Full model	δ	Beta range
Accept	Borrower characteristics, and bank controls	-27.466	[-0.014,-0.013]
Loan Spread	Borrower characteristics, and bank controls	2.969	[0.030, 0.045]

Notes: This table reports the results of Oster's (2019) test for the amount of variation in unobservables relative to observables needed to bring the estimated effect on the explanatory variable *ZombieLaw* to zero. Following Oster (2019), δ is calculated as:

$$\delta = \frac{\beta_{\text{Full}}}{\beta_{\text{Restrict}} - \beta_{\text{Full}}} \times \frac{R_{\text{Full}} - R_{\text{Restrict}}}{R_{\text{Max}} - R_{\text{Full}}}$$

where β_{Restrict} is the coefficient on the dependent variables from the model using a restricted set of controls, and β_{Full} is the coefficient on the dependent variables from the model using a full set of controls. The restricted model does not control for borrower characteristics, state laws, and bank characteristics. The full models correspond to Columns (7) and (14) in Table 5. Following Oster (2019), we specify $R_{\text{Max}} = 1.3 \times R_{\text{Full}}$, where R_{Max} is the R^2 from a hypothetical regression that includes both observed and unobserved controls, and R_{Full} is the R^2 from a regression that includes a full set of controls, i.e., Columns (7) and (14) in Table 5. The beta range is $[\beta^*, \beta_{\text{Full}}]$, where the bias-adjusted treatment effect is:

$$\beta^* = \beta_{\text{Full}} - (\beta_{\text{Restrict}} - \beta_{\text{Full}}) \times \frac{R_{\text{Max}} - R_{\text{Full}}}{R_{\text{Full}} - R_{\text{Restrict}}}$$

Appendix A

Variable descriptions

Variable	Descriptions
Data source: Trackbill.com	
Zombie Law	= 1 if a mortgage application has been filed for a property in a state with an effective Zombie Property Law, = 0 otherwise.
Data source: HMDA	
Accept	= 1 if a mortgage application is accepted, = 0 otherwise.
Ln(Loan Amount)	Natural logarithm of the loan amount.
Ln(Applicant Income)	Natural logarithm of applicant's gross annual income.
LTI	Loan amount divided by the applicant's gross annual income.
Male	= 1 if the main applicant's reported sex is male, = 0 otherwise.
Minority	= 1 if the main applicant's reported race is non-white, = 0 otherwise.
Coapplicant	= 1 if there is a coapplicant, = 0 otherwise.
Jumbo	= 1 if the loan amount exceeds the conforming loan limit, = 0 otherwise.
Data source: McDash	
Loan Spread (%)	The difference between the interest rate on a mortgage and the 30-year US treasury bond yield
Serious Delinquency	= 1 if the mortgage becomes 89+ days delinquent, = 0 otherwise.
Foreclosure	= 1 if the mortgage enters foreclosure, = 0 otherwise.
FICO	The FICO score reported in the application.
LTV	The application's loan-to-value ratio.
Low Documentation	= 1 if the application has less than full documentation, = 0
Prepayment Penalty	= 1 if the application has a prepayment penalty, = 0
Renegotiated (6 months)	= 1 if a seriously delinquent loan is renegotiated within 6 months of entering distress, = 0 otherwise
Cured (6 months)	= 1 if the borrower avoids falling behind payment again in the next 6 months from renegotiation.
Cured (12 months)	= 1 if the borrower avoids falling behind payment again in the next 12 months from renegotiation.
Data source: Tax foundation	
State corporate tax	The top marginal state corporate income tax rate in the state of the mortgage
Data source: Pahl et al. (2007)	
Broker restriction index	A state-level index measuring the intensity of restrictions imposed on mortgage broker in seeking a license or registration
Data source: Calder (2017)	
Zoning index	A state-level index measuring the intensity of restrictions on building single unit homes
Land-use regulation index	A state-level index measuring the intensity of land-use regulations.
Data source: Corradin et al. (2016)	
Homestead exemption (ln)	The natural logarithm of the maximum value of property that is exempt from bankruptcy in the state where loan i is located is located

Notes: This table provides definitions and sources of variables used in the paper.

Appendix B

Similarity between Treated and Control Groups

	Non-ZL states		ZL states		
	Mean	SD	Mean	SD	ND
Sample: HMDA					
Accept	0.755	0.430	0.765	0.424	-0.02
Ln(Loan Amount)	12.125	0.983	12.219	0.946	-0.07
Ln(Applicant Income)	11.470	0.876	11.617	0.825	-0.12
LTI	2.349	1.341	2.158	1.130	0.11
Male	0.628	0.483	0.655	0.475	-0.04
Minority	0.279	0.449	0.207	0.405	0.12
Coapplicant	0.444	0.497	0.495	0.500	-0.07
Jumbo	0.144	0.351	0.192	0.394	-0.09
Observation	47,161		26,539		
Sample: McDash					
Loan Spread (%)	0.721	0.403	0.740	0.394	-0.03
Serious Delinquency	0.068	0.252	0.084	0.277	-0.04
Foreclosure	0.042	0.200	0.054	0.226	-0.04
FICO	715.323	56.420	714.893	54.863	0.01
LTV	87.624	15.092	88.001	14.076	-0.02
Ln(Loan Amount)	12.292	0.802	12.233	0.746	0.05
Low documentation	0.016	0.127	0.019	0.136	-0.01
Jumbo	0.158	0.365	0.122	0.328	0.07
Prepayment Penalty	0.001	0.036	0.003	0.051	-0.02
Observation	4,675		8,715		
Sample: Seriously delinquent(89+days) mortgages (McDash)					
Ln(Loan Amount)	11.859	0.351	11.927	0.398	-0.13
Loan Spread (%)	0.609	0.401	0.650	0.528	-0.06
Renegotiated (6 months)	0.049	0.218	0.085	0.279	-0.10
Cured (6 months)	0.607	0.493	0.475	0.501	0.19
Cured (12 months)	0.590	0.496	0.610	0.489	-0.03
FICO	664.672	28.361	671.593	44.142	-0.13
LTV	94.286	6.049	92.973	8.038	0.13
Low Documentation	0.016	0.128	0.028	0.166	-0.06
Observation	61		177		
Sample: Lender-level data					
Total asset (ln)	14.042	1.650	14.151	1.740	-0.05
ROA(%)	0.705	0.839	0.658	0.775	0.04
Deposit-to-asset ratio (%)	78.208	9.770	78.750	10.892	-0.04
Loan-to-asset ratio (%)	66.626	15.314	63.472	15.911	0.14
Equity-to-asset ratio (%)	11.306	4.522	10.631	3.003	0.12
Observation	441		311		

Notes: This table compares the characteristics of the treatment observations in ZL states to the control observations in non-ZL states over the pre-ZL period (2012-2014). Following Imbens and Wooldridge (2009), an absolute difference smaller than 0.25 indicates no significant difference between the groups. Variable descriptions are in Appendix A.

Appendix C

The effect of ZL on Property Prices and Crime Rates

	(1)	(2)	(3)	(4)
Dependent variable	Corelogic House Prices	Zillow House Prices	Overall Crime Rates	ZL related Crime Rates
Zombie Law	-0.013	0.001	13.363	10.904
	(0.045)	(0.044)	(11.461)	(9.633)
Sold properties-to-property stocks	0.102**	0.052	95.584	93.918*
	(0.048)	(0.044)	(66.735)	(52.908)
Foreclosure rate (%)	0.072**	0.060**	-3.261	-11.025
	(0.029)	(0.024)	(10.495)	(11.294)
Delinquency rate (%)	-0.138***	-0.114***	-1.127	4.637
	(0.020)	(0.018)	(7.594)	(6.831)
Unemployment rate (%)	-0.053*	-0.081**	25.240**	18.076***
	(0.030)	(0.031)	(14.299)	(11.975)
Population-to-property stocks	-0.007	-0.104	27.752*	24.158*
	(0.091)	(0.108)	(14.299)	(11.975)
Observations	15,215	15,215	271	172
Region× Year FE	Yes	Yes	No	No
State FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
Adjusted R^2	0.709	0.904	0.230	0.282
Clustering	County	County	County	County

Notes: This table shows how ZL affects house prices, crime rates, and ZL related crime rates. In Column (1), house prices are collected at the 5-digit zip code level from CoreLogic. In Column (2), house prices are collected at the 5-digit zip code level from Zillow. In Column (3), Overall crime rates are calculated as number of crimes over 10,000 population. In Column (4), ZL-related crime rates are calculated as a number of crimes related to housing issues, abandoned properties, and vandalism over 10,000 population. Standard errors clustered at the county level are reported in parentheses. Variable descriptions are in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

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