Housing Collateral Values and the Discretionary Component of a Bank's Regulatory Capital Ratio

PRELIMINARY AND INCOMPLETE. COMMENTS ARE WELCOME.

Felix Irresberger
Durham University Business School
Durham University
Mill Hill Lane, DH13LB, UK

Francesco Vallascas*
Durham University Business School
Durham University
Mill Hill Lane, DH13LB, UK

Abstract

Using an identification strategy based on the exogenous increase in house prices following the legalization of Home Equity Loans in Texas, we show that banks decrease their regulatory capital ratio when the value of housing collaterals increases. Consistent with our findings reflecting a decrease in the riskiness a bank assigns to its mortgage portfolio, we find a larger decrease in the regulatory capital ratios for banks more exposed to recovery risk and to lower-income households. Further, banks do not respond to higher housing collateral values by increasing their risk-taking in other loan segments or by changing their asset structure. Thus, our findings suggest that the discretionary component of the regulatory capital ratio of banks incorporates variation in the value of housing collaterals in their portfolios prior to the introduction of mandatory capital requirements tied to LTV ratios.

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* Corresponding author. Email address: francesco.vallascas@durham.ac.uk

1. Introduction

The housing markets and the banking sector are invariably intertwined. Real estate booms are often a precursor to financial crises (Calomiris, 2009), drive bank asset growth (Flannery et al. 2022) and might influence the allocation of bank credit in the economy (Chakraborty et al., 2018). At the same time, bank credit, and loose credit standards, can fuel housing bubbles and lead to instability (Favara and Imbs, 2012; Justiniano et al., 2019). Over the past two decades, this interplay between the housing market and the banking sector has motivated a growing number of regulatory initiatives, including new rules that link capital requirements to the value of housing collateral held by banks (Basten, 2020; Benetton et al., 2021). These rules lead to lower requirements for banks with higher collateral values relative to the size of their mortgage portfolio by anchoring mandatory capital requirements to the loan-to-value (LTV) ratio of a mortgage contract.[2]

The growing emphasis on housing collateral values as a driver of borrower risk in mortgage contracts is a relatively recent phenomenon in capital regulation. However, regulatory prescriptions might not be the only motivation that induces banks to adjust their regulatory capital ratio in response to variation in housing collateral values. Indeed, in this study we exploit a regulatory context where these prescriptions are absent to show that the discretionary component of a bank's regulatory capital ratio accounts for such risk factor. We document that banks decrease the discretionary component of their regulatory capital ratios when the value of housing collaterals increases. Our findings allow us to draw implications on how impactful mandatory capital requirements linked to housing collateral values might be for banks and their borrowers.

Our investigation on the influence of housing collateral values of bank regulatory capital ratios in the absence of explicit mandatory rules is motivated by two stylized facts that are well documented in the literature. First, the implications arising for mortgage risk from variation in

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[2] Such rules, first introduced in the international capital standards known as Basel II, were refined in the context of the revised capital framework (Basel III) and are also now part of US regulators' 2023 proposal to strengthen capital requirements for large banks. See https://www.fdic.gov/news/board-matters/2023/2023-07-27-notice-dis-a-fr.pdf.

housing collateral values are known to banks and are well documented in numerous mortgage pricing models and theoretical models focusing on borrower defaults (Agarwal et al., 2005; Campbell and Cocco, 2015). Second, on average, banks operate with regulatory capital ratios well above minimum regulatory requirements, and this discretionary component has been partly linked to the risk that a bank assigns to its asset portfolio (Berger et al., 2008). Thus, if the (relative) values of collaterals underlying mortgage contracts correlate with borrower default risk and influence bank losses in the case of a borrower default, the discretionary part of the regulatory capital ratio a bank decides to hold should also depend on housing collateral values. Hence, mandatory requirements sslinked to housing collateral values might, on average, have only a small impact on how banks allocate their regulatory capital across mortgages and decide mortgage costs (cf. Basten, 2020).

Documenting a causal link between the values of housing collaterals in a mortgage portfolio and the regulatory capital ratios is, however, problematic due to endogeneity issues. Changes in the economic environment, monetary policy, or regulatory standards might simultaneously affect bank lending practices, house prices and credit demand (Meeks, 2017; Justiniano et al., 2019). For instance, an increase in housing collateral values might be associated with credit booms, making it difficult to isolate the housing collateral effects from the credit supply effect on bank regulatory capital ratios. Additionally, the regulatory capital held by a bank might also influence its lending practices with the consequence to affect the value of collaterals a bank holds in its portfolio.

To overcome this intrinsic endogeneity challenge, we employ the Texas Home Equity Law (HEL) in 1998, and the subsequent surges in house prices, as a quasi-natural experiment. The law provided borrowers for the first time the option to pledge their house as a collateral against additional home equity loans while keeping unchanged the maximum debt capacity relative to the house value. Zevelev (2021) shows that this option causally increased house prices via a larger demand for owned properties and contributed to lowering the average LTV ratio also of newly originated mortgages. Crucially for our analysis, the law was motivated by federal tax reform and a circuit court ruling and was not passed with the intention to stimulate the economy, for instance, via additional lending flowing from the banking sector (Forrester, 2002). Additionally, the law did not increase the rate of homeownerships in Texas relative to other states. Therefore, this event leads to sources of (positive) changes in the collaterals already pledged in favour of banks and a decrease in the LTV of new mortgages that are plausibly exogenous with respect to bank lending policies and unrelated to any credit boom. We exploit this variation for identification purposes and to alleviate endogeneity concerns.

We base our analysis on a generalized difference-in-differences (DID) model through which we compare the regulatory capital ratio of banks whose branch network is entirely based in Texas (henceforth, Texas banks) with a control group of geographically proximate banks matched across several business characteristics, but with no branches in Texas. Using this econometric setting, we provide evidence of a relative decline of the regulatory capital ratio of Texas banks after the adoption of the HEL 1998 law. This decline is equal to about 80 basis points, equivalent to 4% of the pre-shock average value of the regulatory ratio. This result holds across numerous alternative specifications based on i) different control samples, ii) estimation windows, and iii) estimation methods. Importantly, our main result remains unchanged when we reduce the possibility that unobserved demand and supply factors related to the local economy might affect our inference by employing a tight geographic matching based on contiguous counties.

Next, we provide further support to the interpretation of our results being driven by the impact of the Texas law on the collateral value of a bank mortgage portfolio. We begin by modelling the treating effect under a non-binary setting where the exposure of Texas banks to the law depends on the importance of housing collateral for capital requirements prior to the law. Under this setting, and consistent with the initial interpretation of our findings, we observe that Texas banks where this importance was higher experience the larger decrease in the regulatory capital ratios following the law.

We next explore cross-sectional heterogeneity in our sample. The tests we implement share the same intuition: if a positive variation in collateral values of a mortgage portfolio is indeed behind our findings, we should observe that the relative decline in the regulatory capital ratio in Texas banks becomes more pronounced when this effect is expected to be more salient. Our tests rely on the widely documented influence of housing collateral values on different facets of mortgage risk (see, for instance, Agarwal et al., 2015; Campbell and Cocco, 2015; Gerardi et al., 2018).

First, uncertainty about collateral values increases a bank's recovery risk in the case of a borrower's default (Qi and Yang, 2009; Jiang and Zhang, 2023). Therefore, if a collateral value effect matters for our results, banks that are more exposed to such uncertainty prior to the adoption of the new law should show a larger relative decline of their regulatory capital ratios. We measure a bank's exposure to uncertainty in housing collateral values using granular house price data at the county level, combined with branch-level data for banks, to construct a bank-level exposure measure to house price volatility prior to the adoption of the law. We show that Texas banks more exposed to higher house price volatility before the law are those exhibiting a larger relative decline in their regulatory capital ratio post the law.

Second, we focus on another dimension of the risk effects related to the value of the LTV ratio. Specifically, jointly with the decrease in financial constraints due to the law, lower average LTV values as those determined by the law, are generally associated with a reduced propensity of borrowers to default (Guiso et al., 2013; Gerardi et al., 2018). Thus, if our evidence is driven by housing collateral values via an LTV effect, the finding should be mostly driven by banks more exposed to borrowers with stronger propensity to default ex ante. Along these lines, we find that our results are stronger when banks are more exposed ex-ante to households with lower income. Additionally, consistent with higher housing collateral values lower the riskiness of the mortgage portfolio of Texas banks, we show that several proxies of the mortgage risk decline for Texas banks relative to the control group after the enactment of the law.

In a final battery of tests, we rule out potential alternative explanations of our results. First, it might be suggested that the increase in housing collateral values induces banks to engage in more aggressive risk-taking. The lower regulatory capital ratios we observe in Texas banks might be simply a consequence of a broader change in risk-taking by banks after the law. However, when we exploit heterogeneity in our results due to a bank's risk attitude, we find that this explanation is unlikely to be valid. Second, it might be argued that the law creates incentives for households to borrow more in the mortgage market and for banks to substitute other loans with mortgages, thus shifting the composition of their portfolio towards assets with lower regulatory risk weights. Our results might then be the consequence of a mechanical effect due to portfolio adjustments. However, against this interpretation, we do not find an increase in the relative importance of loans secured by residential properties in the sample of treated banks relative to the control group after the law, or any evidence of a significant shift in the composition of the asset structure. Taken together, these two groups of tests are consistent with a demand-side effect from the real estate market driving our results as in Zevelev (2021), and do not provide evidence of a supply-side effect arising from the local banking sector.

Our contribution to the literature is manifold. First, we add to the literature on what drives regulatory capital ratios above the minimum requirements. This literature provides several explanations for this regularity in bank behaviour related to the perceived risk exposure of a bank portfolio and bankruptcy costs (Ayuso et al., 2004; Berger et al., 2008; Flannery and Rangan, 2008; Memmel and Raupach, 2010;), profitability and tax incentives (Berger et al., 1995; Collins et al., 1995), regulatory arbitrage (Acharya et al., 2013; Vallascas and Hagendorff, 2013; Gropp et al., 2023), government guarantees (Duchin and Sosyura, 2014), competition (Allen et al., 2011), the business cycle (Ayuso et al., 2004; Repullo and Suarez, 2013), financing constraints (Boyson et al., 2016) and the exposure to idiosyncratic funding shocks (Corbae and D'Erasmo, 2021). We show that a key risk-factor of the mortgage portfolio contributes to drive variation in the discretionary component of the regulatory capital ratio of banks despite being ignored by the design of the regulatory risk-weights. Our results are consistent with the evidence indicating that capital regulation is not necessarily binding (Flannery and Rangan, 2008; Gropp and Heider, 2010) and with recent work showing the importance of collaterals on how banks manage their capital requirements (Degryse et al., 2021).

Our work is closely related also to the literature on the role of collaterals as a crucial risk-factor for mortgage defaults (see, among others, Agarwal et al., 2005; Guiso et al., 2013; Campbell and Cocco, 2015; Gerardi et al., 2018), and to the stream of studies focusing on banking regulation and the mortgage market. A first group of studies shows the consequences arising from an increase in mandatory capital requirements for mortgages (Basten, 2020; Benetton et al., 2021). In particular, Basten (2020) document a relatively small increase in mortgage costs after the introduction of a countercyclical capital buffer requiring additional capital for residential mortgages. This effect is mostly driven by banks with a low capital cushion and is not strengthened for mortgages with higher LTV. These findings are consistent with our evidence indicating that the de-facto capital requirements of banks indeed account already for the importance of collaterals. Other studies have emphasized the role of LTV as a macroprudential tool (see Morgan et al., 2019; Acharya et al., 2022), while more recent work has highlighted the distortions that can be generated when banks can endogenously determine and manipulate house appraisals. This behavior results in the build-up of hidden risks and in regulatory arbitrage via overstated regulatory ratios (Galán and Lamas, 2023; Mayordomo et al., 2023). Different from these studies, our analysis focuses on a setting where these types of distortions are unlikely to emerge as the variations in the LTV that we implicitly capture are driven by the demand side rather than by bank decision-making. Therefore, our evidence is more informative on the regulatory implications that can arise when bank discretion in valuation is constrained. In this respect, our results relate to the findings in Agarwal et al. (2020) on the importance of rules, such as the Home Valuation Code of Conduct, to reduce overstated valuations for an effective regulatory role of the LTV ratio.

Our work can be also framed in the context of the literature that employs the appreciation of real estate values as collateral shocks. These studies take the borrower perspective and document how these shocks alleviate financial constraints for firms and household by facilitating access to credit (Chaney et al., 2012; Cvijanovic, 2014; Schmalz et al., 2017; DeFusco, 2018; Campello et al. 2022). In contrast, we take the perspective of the lending bank and derive implications on how the collateral shock matters for its financing decision within the capital regulation framework. Furthermore, in our experiment the borrower overall debt capacity remains capped by the regulatory setting, and this limits contamination effects on a bank's funding choice from excessive lending growth in the banking sector that indeed we do not observe.

More generally, our findings shed light on the interplay between the housing market and the banking sector where the direction of influence goes from the housing market to the banking sector. For instance, Chakraborty et al. (2018) show that housing boom can induce loan portfolio adjustments that penalize the corporate sector and Flannery et al. (2022) document that house demand shocks can account for a large share of bank asset growth from 2001 to 2006. Our analysis expands this evidence by focusing on the regulatory capital decision of banks in a setting that facilitates causal claims.

2. Background and Hypothesis Development

To model the causal impact of housing collateral values on bank regulatory capital ratios, we use an identification strategy based on the state-level constitutional change that legalizes Home Equity Loans (HEL) in Texas from the first quarter of 1998. In the next two sections, we discuss the key features of this law and explain why it generates exogenous variations in house prices that contributes to reducing on, average, the LTV ratio of existing and newly originated mortgages. We then present the theoretical arguments that should lead to an impact of these variations on the regulatory capital ratio of a bank, despite in the period we investigate the regulatory capital regime

(Basel I) did not link the risk-sensitivity of capital requirements to the value of housing collateral through rules based on the LTV ratios.

2.1. The Texas HEL Legalization and its Implications on the Housing Market

The 1998 Texas law provided local households for the first time the option to borrow against the value of the property they already own via HEL (Abdallah and Lastrapes, 2012; Forrester, 2002; Kumar and Liang, 2019; Zevelev, 2021). The law did not have any impact on the borrowing capacity at the time of the purchase that remained capped at 80% of the total market value of a property. Importantly, the overall debt exposure of each borrower, including the home-based lending, was also capped at 80% of the fair value of the property.[3]

Several studies have documented the effects of the Texas law by taking mostly a macro perspective. Particularly relevant for our work is the evidence reported in Zevelev (2021) of an average increase of around 4% to 6.2% in Texas house prices following HEL legalization, relative to a wide range of control geographic groups. In Figure 1, we confirm this evidence. Similar to Zevelev (2021), we plot the demeaned percent change in house prices in Texas versus the average observed in contiguous states (Arkansas, Louisiana, New Mexico, Oklahoma) and in what we define as proximate states (contiguous states plus Colorado and Kansas – representing the closest states to the Texas border) from 1995 to 2001. The figure shows that while prior to the law, Texas did not exhibit any increase in house prices higher than in the nearby states, after the law the growth in price was indeed higher in Texas.

[FIGURE 1 HERE]

Zevelev (2021) documents that the increase in house prices is consistent with a rise in demand for residential housing already occupied, rather than being driven by other economic outcomes

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[3] The law did not allow home equity loans in the form of an open-end account (that is, an account where the credit granted is available as long as the outstanding balance is repaid, and interests are charged on the unpaid balance) and did not allow to require more than one HEL by a homeowner. For more details, see https://occc.texas.gov/sites/default/files/uploads/disclosures/b98-2-home-equity-regulatory-commentary.pdf.

that could spur price increases indirectly. In other words, the law did not influence other economic factors, including home ownership rates that remained largely unchanged, but simply gave (credit-constrained) households the future option to cash-out some of their home equity through HELs. In turn, this option increases the value of the property as a collateral that was then reflected in the prices of residential properties. Therefore, the aggregate value of housing collaterals pledged in a bank's mortgage portfolio should increase after the law. Furthermore, since it is not plausible that *all* existing mortgagors saturate their new borrowing capacity after the law, thus increasing again their LTV ratio to the maximum limit of 80% of the property value, the aggregate LTV ratio of this portfolio should decrease after the law.

This latter prediction is further justified by the fact that the increase in house prices was accompanied also by a decrease in the average LTV ratio of *newly* originated single-family mortgages in Texas relative to other geographic areas (Zevelev (2021). We confirm this second key fact in Figure 2 where we plot the average LTV ratios from 1995 to 2001 in Texas again versus the geographic aggregates based on contiguous states and proximate states. Taken together, the two figures are inconsistent with more aggressive risk-taking by banks following the law (which should not justify a decline in the average LTV ratio of mortgages). Instead, they are consistent with a facilitated access to future borrowing via HEL for local households that reduces financial constraints and the incentive to saturate debt capacity at the time of the house purchase.

[FIGURE 2 HERE]

The highlighted interpretation based on financial constraints is supported by studies that do not focus on the impact of the HEL law on the housing market. For instance, Abdallah and Lastrapes (2012) show that retail spending in Texas increased following the law and Kumar and Liang (2019, 2023) find evidence for lower labor market participation as a side-effect of households being able to use their house to take out HELs. However, none of these additional effects are likely to contaminate our identification strategy as, plausibly, they do not have any direct effects on the regulatory capital held by banks. There is not any evidence that the event led to a credit boom that might have significant effects on the regulatory capital ratio of the affected banks.

2.2. Hypotheses: The Impact on the Regulatory Capital Buffer of Banks

Over the past two decades, the regulatory capital framework on banks has progressively introduced rules that link the granularity of capital requirements to housing collateral values via the LTV ratio of a mortgage contract. Initially, rules based on this ratio were mildly established by the revised version of the Basel Accord (Basel II) in 2004. Loans secured by a residential property showed a reduction of their risk weight from a flat 50% to a range up to a maximum of 45% depending on the LTV ratio of the loan (Calem and LaCour-Little, 2004; Benetton et al., 2021).[4] The granularity was then further strengthened in the following revisions of the Accord (Basel III) where the risk-weights for a mortgage range from a minimum of 20% when the LTV is below 20% to a maximum of 70% for a LTV above 100%.[5] Additionally, recent proposals to increase the capital requirements for large banks by US regulators in July 2023 also recognize the importance of LTV as a risk factor and aim to introduce even more penalizing LTV-linked risk-weights than Basel III.[6] However, the motivation behind these risk-based rules would justify an effect of housing collateral on the regulatory capital ratio independently of the regulatory framework in place. There are at least two reasons that lead to this prediction.

First, the theoretical and empirical literature has unanimously identified housing collateral values as an important risk-mitigating factor for banks independently of the regulatory regime. For instance, in conventional option pricing models on mortgages, borrower default risk increases when the collateral value declines relative to the mortgage amount (Agarwal et al., 2005). In the

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[4] Basel Committee on Banking Supervision, 2004, https://www.bis.org/publ/bcbs107.htm.
[5] See Basel Committee on Banking Supervision, 2011, https://www.bis.org/publ/bcbs189.htm; 2017).
[6] For instance, US regulators indicate that "*LTV ratios can be a useful risk indicator because the amount of a borrower's equity in a real estate property correlates inversely with default risk and provides banking organizations with a degree of protection against losses. Therefore, exposures with lower LTV ratios generally would receive a lower risk weight than comparable real estate exposures with higher LTV ratios under the proposal*", see https://www.fdic.gov/news/board-matters/2023/2023-07-27-notice-dis-a-fr.pdf.

model of Campbell and Cocco (2015) the probability of observing negative equity for the borrower might trigger defaults in a bank's mortgage portfolio. In a similar vein, Gerardi et al. (2018) show empirically that when the LTV increases, there are growing incentives for borrowers to strategically default, especially if they have low residual income. Guiso et al. (2013) confirm that considerations about property values do matter for strategic defaults by borrowers.

Second, it is widely established that banks hold regulatory capital in excess to the minimum requirements and this is due in part to their aim to lower the risk of breaching the minimum requirements as well as reducing the cost of failure (Ayuso et al., 2004; Berger et al., 2008; Repullo and Suarez, 2013). Indeed, several studies suggest that the perceived risk exposure of a bank influences the choice of how much regulatory capital to hold above the minimum requirements and conclude that capital regulation is not binding (Berger et al., 2008; Flannery and Rangan 2008). Thus, the highlighted risk-effects of the 1998 Texas law on the mortgage market might induce adjustments in the discretionary choice of the regulatory capital buffer by banks given their implications for the loss distribution of the mortgage portfolio. [7]

In particular, a decrease in the discretionary part of the regulatory capital ratio due to exogenous positive variations in housing collateral values would be consistent with banks altering their preferences for capital buffers when their risk exposure changes to maintain target regulatory capital buffers commensurate with the underlying credit risk (Plosser and Santos, 2023). This prediction can be justified also in the context of models where banks have a target regulatory capital ratio that accounts for the mandatory prescriptions from regulators, as well as adjustments costs, bankruptcy costs and capital remuneration costs (Froot and Stein, 1998; Ayuso et al., 2004). The downward shift in the loss distribution of the mortgage portfolio might lower bankruptcy

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[7] This would be consistent with the US regulatory agencies' view offered in 2005 in the consultation process on potential improvement in the risk sensitivity of capital requirements including for the first time the possibility to use risk-weights based on LTV ratios. They stated: "*Agencies believe that the use of LTV ratios to measure risk sensitivity would not increase regulatory burden for banking organizations since this data is readily available and is often utilized in the loan approval process and in managing mortgage portfolios*". See, https://www.federalregister.gov/documents/2005/10/20/05-20858/risk-based-capital-guidelines-capital-adequacy-guidelines-capital-maintenance-domestic-capital.

costs for banks as well as adjustment costs, thus making it more likely for a bank to operate closer to the minimum regulatory capital ratio.

Evidence of an impact of housing collateral values on the discretionary component of the regulatory ratio during the Basel I regime would have implications for our understanding of the effects of mandatory regulatory requirements that link risk-weights on mortgages to LTV ratios. In short, banks were already internalizing partly the effects of these dynamics when they decide their regulatory capital policy and the actual effects of such rules would simply depend on how the sensitivity to LTV implicit in bank choices is distinct from what is required by regulators.

3. Data and Identification Strategy

3.1. Sample of Treated and Control Banks

To implement our analysis, we begin with the full sample of commercial banks with call report data available at Q3/1997 (that is, the quarter before the introduction of the HEL law in Texas) and branch deposit data in the FDIC summary of deposits (SOD) in Q2/1997 (the last available period with branch deposit data prior to the treatment timing). Next, we identify banks affected by the law (*the treated group*) by imposing two preliminary conditions. First, banks must operate branches in Texas. Second, their entire branch network must be located in this state. These two conditions ensure that the banks in the treated group are exposed to the Texas HEL law, but they are plausibly not much exposed to local economic forces from other states, which can influence their lending and deposit taking.

To select the banks that satisfy the two conditions above, we rely on the SOD data. Specifically, we identify banks with branches in Texas and compute for each of these banks the share of their deposits that is concentrated in this state. Finally, we retain in the sample only banks with a Texas deposit share equal to 100% in Q2/1997. Next, we follow Dell'Ariccia et al. (2021) and retain in the sample only commercial banks (variable RSSD9331 equal to 1). The application of these criteria leads to an initial sample of 847 treated banks.

We progress by constructing the sample of control banks. To this end, we follow three steps. First, we impose the condition that the control banks do not have branches in Texas. This condition is necessary to avoid that the control banks are affected by the HEL law. Second, we reduce heterogeneity in the geographic exposure of these banks by requiring that their branch network is located in the bordering states (Arkansas, Louisiana, New Mexico, and Oklahoma) or in states with the closest distance to the Texas border (Colorado and Kansas). Figure 3 illustrates the banks operating in states included in our treated (Texas) and control sample.

[FIGURE 3 HERE]

Third, in line with the selection of treated banks, we require that the potential control banks operate in only one state and are commercial banks. Essentially, we compare single state banks in Texas with single state banks in nearby states. Using these criteria, we identify 1,371 potential control banks and have a sufficiently large number of potential control banks for implementing the third and final step of our selection process.

The final step consists of matching the 1,371 control banks with the treated banks along several characteristics. This is done using a 1:1 propensity score matching without replacement between treated banks and control banks. The matching is based on the following characteristics measured at Q3/1997: 1) Size (the log transformation of total assets); 2) ROA (net income scaled by total assets; 3) Loans (total loans in percentage to total assets); 4) Regulatory Capital Ratio (regulatory capital scaled by total assets); 5) NPL (non-performing loans divided by total assets); 6) C&I Loans (measured in proportion to total loans) 7) Residential Mortgages (measured in proportion to total loans); 8) Insured Deposits (the ratio between insured deposits and total deposits). We employ a caliper of 0.01 in our matching strategy to ensure that the two groups of banks are sufficiently similar in the pre-shock period. Using this criterion, we match 666 treated banks to 666 control banks. These 666 Texas banks represent 65% of all branch deposits in Texas in Q2/1997. It is important to note that in additional tests, we extensively document that our main finding does not depend on the way we construct the control group and, more importantly, it is robust to changes in the matching strategies we follow and when we consider the full sample of 847 Texas banks that we identify in the first step of our identification strategy.

[TABLE 1 HERE]

Panel A of Table 1 reports the characteristics of the treated and control banks before and after the matching. The Panel shows also normalized differences between the two groups computed as below:

$$\text{NDIFF}=\frac{\overline{X}_i - \overline{X}_j}{\sqrt{S_i^2+S_j^2}} \tag{1}$$

where $\overline{X}_i$ (S_i^2) is the mean (variance) of a given variable for the control group and $\overline{X}_j$ (S_j^2) is the mean (variance) of the same variable for the treated group. Imbens and Wooldridge (2009) highlight that a normalized difference below a threshold value of 0.25 is indicative of homogeneity between two groups.

As documented by the first two columns of Table 1, prior to the matching, the two groups are significantly different especially in terms of lending composition. Nevertheless, the matching makes the two groups very similar. In Column (4) we show that after the matching the normalized differences in characteristics between the treated and control groups, which are measured in the last quarter prior to the law (Q3/1997), are well below the threshold value of 0.25.

Overall, the matched treated and control banks are very similar in terms of regulatory capital ratio, size, and business models in the period prior to the adoption of the new Texas law. This high degree of similarity significantly reduces the possibility that the two groups of banks differ along *unobservable* dimensions (Roberts and Whited, 2013).

3.2. Econometric Method

We estimate the causal effect of variation in housing collateral values on the regulatory capital ratio of banks (under Basel I) using a generalized difference-in-differences approach. Our baseline model takes the following functional form:

$$\text{Regulatory Capital Ratio}_{i,t} = \alpha + \beta_1 \text{Treated}_i \times \text{Post}_t + \textbf{BANK} + \textbf{TIME} + \varepsilon_{i,,t}\ , \quad (2)$$

Where the dependent variable is the total regulatory capital ratio of bank *i* at time *t*; $Treated_i$ is a dummy that equals one if bank *i* is located in Texas and zero if it belongs to the control group; $Post_t$ is a dummy equal to one in the 12-quarter (3-year) period following the HEL law (from Q1/1998 to Q4/2000) and zero from Q1/1996 to Q4/1997. The choice of the estimation window is motivated by the regulatory capital ratio not being available before 1996. Furthermore, we opt for a longer post estimation window as the effect of the law on banks plausibly requires a sufficiently long period to materialize. However, in section 4.2 we show that the length of the estimation window does not affect our findings. In addition, whenever data are available, we consistently employ as symmetric estimation window of (-12;+12) quarters. The largest sample we employ includes a total of 24,896 bank-year observations. This number reflects the fact that not all banks are observed for the full sample period; namely, we employ an unbalanced panel. We do not impose that the panel must be balanced to avoid selection bias in the identification of banks that might affect our inference.

The coefficient of interest is β_1 that measures the difference in the change of the dependent variable from the pre-shock to the post-shock period between treated banks and control banks. In equation (2), we cluster standard errors at the bank level to control for within bank correlation in the evolution of the regulatory capital ratio and include bank (BANK) and year (TIME) fixed effects. The first set of fixed effects controls for bank-specific time-invariant omitted variables. The inclusion of time fixed effects accounts for the evolution of the business cycle that is common across states that could affect the risk exposure of banks and their consequent choice of the regulatory capital ratio.

Initially, we estimate equation (2) without bank-level controls. If the Texas law on HEL affects also other bank-level outcomes, the inclusion of controls would make it more difficult to interpret the coefficient of $Treated_i \times Post_t$ (Gormley and Matsa, 2011). Nevertheless, to mitigate concerns over omitted variables, we also report two additional specifications that include 1-quarter lagged bank controls computed from quarterly Call Reports. The first specification controls for *Size*. The second specification includes as controls all the bank characteristics that we have employed for our matching strategy described in section 3.1.

3.3. Parallel Trends and Identifying Assumption

Our difference-in-differences analysis assumes that absent the law, the regulatory capital ratio would have evolved in a similar way for both treated and control banks (i.e., *the parallel trends assumption*). We cannot directly validate this assumption because we do not observe the evolution of the regulatory ratio in the treated group in the absence of the law. Nevertheless, if the regulatory capital ratio follows similar trends in the two groups of banks in the period prior to the new law, this assumption is regarded as credible (Lemmon and Roberts, 2010).

We conduct two analyses to investigate pre-shock trend dynamics for both the treated and the control group. First, we follow Lemmon and Roberts (2010) and compare average changes in the regulatory ratios in the pre-event period in the two groups of banks (Panel B of Table 1). We initially perform t-tests, reported in Column (3), to assess if these average changes differ significantly by considering the full pre-shock period. For the parallel trends assumption to be plausible, there should not be any clear statistical difference in the changes in the regulatory ratio between both groups. The last column of Panel B shows that this is the case. Next, we repeat the analysis for each quarterly change in the pre-shock period. Overall, we do not find evidence of systematic differences between the two groups of banks. Indeed, only one difference test in the average quarterly change in the initial part of the period is marginally significant at the 10% level.

[FIGURE 4 HERE]

Second, Figure 4 plots the trends for treated and control banks in the pre-shock period that we estimate from a linear model with the set of bank controls reported in equation (2). The estimated values of the regulatory capital ratio in Figure 4 do not reveal any discernable differences in trends between the two groups before the event. However, after the regulatory event, we observe a significant change in the evolution of the regulatory capital ratio between the two groups, with treated banks operating with a much lower average regulatory capital ratio than control banks. The results of both tests suggest that the parallel trends assumption is plausible in our setting.

A further assumption of our identification strategy is the exogeneity of the law with respect to bank regulatory capital. In this respect, the extant literature is unanimous in suggesting that the legislation was not targeted towards the banking sector and, more importantly, was not referring in its design and preparation to bank regulatory capital (see, Forrester, 2002; Zevelev, 2021). More specifically, Zevelev (2021) provides evidence indicating that the adoption of the law was motivated by federal tax reform and a circuit court ruling while Forrester (2002) shows that the law was not passed with the intention to stimulate the economy (for instance, via additional lending flowing from the banking sector). As a result, the regulatory capital ratios of Texas banks should not have any influence on the decision to design and implement the law.

3.4. The Implementation of Capital Requirements for Market Risk in 1998

In 1998, the US banking system was characterized also by the adoption of capital requirements for market risk (Holod et al., 2020). As a result, it might be suggested that this concurrent regulatory change can contaminate our analysis. Nevertheless, the rules on capital requirements for market risk, as detailed below, had a very limited scope in the US and the banks in our sample were not much affected by their introduction.

Specifically, the additional capital requirements for market risk apply only to banks with a position in trading accounts (trading assets plus trading liabilities) that is above \$1 billion or 10% of total assets (Holod et al., 2020). However, the largest treated (control) bank in our sample has a value of *total assets* of about \$6 billion (\$8 billion). Additionally, the average trading ratio in the treated (control) group in the estimation window we consider for the baseline analysis is equal to 0.01% (0.01%). More importantly, over the entire estimation period none of the treated banks is close to the 10% threshold and only in 2 cases (in year 2000) a bank in the control group shows a trading account position above 10% of total assets. The exclusion of these banks does not have any meaningful impact on our findings.

4. The Impact of Housing Collateral on Regulatory Capital Ratios

This section presents our difference-in-difference regression results and reports on various additional tests that establish robustness and underscore the causality of our findings.

4.1. Baseline Results

We begin by presenting our baseline results on how variation in the value of housing collateral causally impacts the regulatory capital ratio of banks. As a preview, Panel A of Table 2 shows a univariate difference-in-differences analysis to estimate the average treatment effect. We compute the average difference in the regulatory capital ratio between the post-shock and the pre-shock period for treated and control banks and then perform t-tests on whether these averages differ in the two groups. In this very simplified setting, we find evidence of a decrease in the regulatory capital ratio in the group of treated banks relative to the control group. This preliminary result is consistent with banks accounting for the dynamics of the value of these collaterals within their real estate portfolio, and the related effects on a bank's mortgage risk exposure, when they chose the discretionary component of the regulatory capital buffer.

[TABLE 2 HERE]

Next, Panel B reports the results of our main analysis based on equation (2). In column (1), we present a model that does not include control variables, but only bank and time fixed effects. In column (2), we add bank size as an explanatory variable and in column (3) we include the full set of matching variables as controls. Consistent with the findings reported in Panel A, all specifications show a decrease in the regulatory capital ratio of treated banks relative to the control group. The relative (average) decline is approximately equal to 80 basis points. This is equivalent to 4% of the average regulatory capital ratio in the treated group in the pre-shock period. The decrease seems to be economically plausible given the observed relative increase in property prices estimated by Zevelev (2021) of about 4-6% and the observed dynamics in the LTV of newly originated mortgages. Jointly with the results of Zevelev (2021), our findings imply an elasticity of the regulatory capital ratio with respect to the value of housing collateral between 13% and 19%.

Overall, the results in this section show that the value of housing collateral matters for the regulatory capital choices of banks also in the low risk-sensitive capital framework based on Basel I. When the value of housing collateral increases, banks lower their (discretionary part of the) regulatory capital buffer. This finding is consistent with a role of collateral as a shelter against a bank's risk exposure and with theories that identify the perceived risk exposure of a bank as a key driver of its decision to hold discretionary regulatory capital (Berger et al., 2008). Ultimately, our results show that the value of housing collaterals matters for bank regulatory capital well beyond the regulatory prescriptions.

4.2. Robustness Tests

Table 3 reports additional specifications that document the robustness of our main results. In Columns (1) and (2), we employ a symmetric estimation window 3 (-8;+8) around the event and repeat the estimation without and with control variables, respectively. We find that our results remain intact. Next, we address concerns related to standard errors. Bertrand et al. (2004) argue that biased standard errors might arise in a difference-in-differences setting due to serially correlated outcomes. To mitigate this potential bias, we follow their approach and collapse the estimation period to one period before and one period after the law using the average values of our dependent variable as well as the other variables employed in our main test computed for the initial pre and post event window. Our results remain similar, independently of whether controls are added or not to the model.

[TABLE 3 HERE]

In columns (5) and (6), we implement an alternative estimation strategy based on Arkhangelsky et al. (2021). We estimate a synthetic difference-in-differences (SDiD) model using the same estimation window as in our initial test. The SDiD incorporates within the difference-in-differences set up the benefits of the synthetic control group approach. This is done by assigning more importance in the estimation to periods where the parallel trend assumption is more likely to hold and to observations in the two groups that are more similar in terms of a set of covariates. One limitation of this setting is that it requires a balanced panel, and this marginally reduces the number of observations in our sample. As in our baseline analysis, we estimate SDiD with and without control variables. The results confirm our main conclusion: following the new Texas law, we observe a relative decrease in the regulatory capital ratio of treated banks.

[FIGURE 5 HERE]

We progress by examining if our results are supported by a dynamic model based on the joint inclusion of the interaction of pre and post quarterly dummies with the treated dummy. We plot in Figure 5 the estimated coefficients obtained from this model and the related 95% confidence interval. While in the pre-period there is no evidence of significance of any of the interaction terms, in the post period the difference between treated and control banks becomes evident. Notably, this chart offers further support to the plausibility of the parallel trend assumption in the context of our analysis. Finally, we test if our results hold when we measure differently the regulatory capital ratios of banks. Specifically, in the last two columns of Table 3, we repeat the analysis by using the Tier 1 regulatory ratio (that is, tier 1 regulatory capital scaled by risk-weighted assets) as the dependent variable. We find again that our main result is confirmed.

4.3. Alternative Matching Strategies

In this section we examine how sensitive our results are to changes to the matching strategy. We focus on two different aspects of the matching strategy. First, we focus on the bank characteristics used to match treated and control banks. We show that keeping constant our initial geographic matching, our results remain similar if a) we remove some of the matching variables, or b) we add further matching variables to our initial list of bank characteristics. More precisely, in the first two columns Panel A of Table 4, we start by replicating our initial analysis by using only three variables in the matching strategy: Size, ROA and NPL. In the next two column, we add the loan ratio as a matching variable. In all columns we find that our results remain largely unchanged. In the following four columns, we add further variables to our initial list of bank characteristics. In columns (5) and columns (6), we add the ratio between unused loan commitments and bank assets as they also influence the regulatory capital ratios. In the last two columns we also include the ratio between mortgage back securities and bank total assets. We still observe that our findings are not affected by these modifications.

Second, we focus on the geographic dimension of our matching. A first concern is that the narrow geographic focus we have employed, while beneficial to remove heterogeneity in terms of local market characteristics, has reduced the number of potential bank matches for each treated bank. Consequently, this might have decreased the chances to identify control banks that are very similar to treated banks. Although the evidence reported in section 2 shows that any degree of heterogeneity between the two groups of banks has remained within an acceptable range, we make additional steps to mitigate this concern. First, we repeat the 1:1 matching without replacement and with a caliper of 0.01 by considering the full US population of single state banks as possible controls. Second, to further increase the sample size of the control group, we re-match the treated banks with the full population of single state banks with a 1:3 matching with replacement and with a caliper of 0.01. In both cases, we observe that our results remain largely unchanged.

A second and opposite concern in terms of geography is that our focus beyond the bordering states to form the control group might add a source of heterogeneity due to differences across local markets that we are not able to remove. We alleviate this concern in two ways. First, we repeat the analysis by including in the control group only the full sample of banks in the bordering states. Second, we replicate the analysis by employing a 1:1 matching between Texas banks and banks in the bordering states with a caliper of 0.01. This restricts both the size of the treated and control groups, but it allows us to reach a higher degree of similarity between the two groups of banks. The results, reported in the last two columns of Panel B, confirm our main conclusion: we still observe a decline in the regulatory capital ratio of treated banks relative to the control group.

In summary, despite the many alternative matching strategies, choice of specifications and samples we employ, we systematically confirm our main result. We observe a relative decline in the regulatory capital ratios of treated Texas banks after the adoption of a new law that has significantly affected house values in the local real estate market.

4.4. An Alternative Identification Strategy based On Contiguous Counties

We have made several attempts to mitigate the influence of differences in local economic conditions on our results, including an investigation based on a very narrow geographic setting (*contiguous states*). However, state proximity might not be sufficient to completely rule out the effects of local differences that can correlate with the evolution of the housing market, thus making the interpretation of our findings more problematic. In this section, we implement an even tighter identification strategy based on geographic proximity at the county level. This alternative identification strategy is based on the following steps and illustrated in Figure 6.

[FIGURE 6 HERE]

First, we identify counties in Texas that share their borders with counties in Arkansas, Louisiana, New Mexico, and Oklahoma (the contiguous states, solid black lines in the figure). Second, for these counties in Texas, and the related counties in contiguous states, we identify the treated and control banks that are part of the matched sample used in the last two columns of Panel B of Table 4 and have a significant share of their deposit business in these counties (red for Texas, blue for bordering states' counties). The intuition here is that banks operating in adjacent counties, although across different states, are likely to be exposed to more similar economic conditions and this should help us rule out contamination effects from other economic factors.

More precisely, we use information from the SOD from the FDIC to quantify the share of a bank's branches (deposits) in these counties. We then maintain in the analysis only those treated and control banks with a branch (deposit) share in the identified counties of at least 50%. This choice aims to ensure that the activities of these banks in the identified counties are relevant enough for outcomes at the bank-level being affected by changes in the local real estate market. Notably, the assumption here is that the branch share and the deposit share in these counties provide a good representation of the importance of the lending business for banks in these geographic markets. This assumption seems plausible since the period we investigate was not characterized by a significant role for digital banking and the banks we examine are relatively small and primarily fund their lending via the deposit market. Approximately 15% of the sample of banks in contiguous states remain in the analysis after we impose this condition. Thus, the benefits of a narrower geographic focus come at the cost of a significantly reduced sample size. Finally, we use this smaller sample to replicate our investigation. Table 5 reports the results.

[TABLE 5 HERE]

Despite the much smaller sample size, we still observe a decrease in the regulatory capital ratio of treated banks relative to the control group after the Texas HEL came into effect. This result is confirmed independently of whether we measure the exposure of the bank business in these adjacent counties in terms of branches or via the volume of deposits. Additionally, it holds independently of whether we include or not control variables in the model.

Ultimately, this section offers further support for the presence of a causal relationship between variation in house values and regulatory capital buffers held by banks.

5. Does the Value of Housing Collateral in the Mortgage Portfolio Drive Our Results?

We have interpreted our findings as reflecting the impact of the dynamics of collateral values of a mortgage portfolio held by a bank on the regulatory capital ratio. In the following sections, we provide further support to this interpretation and rule out alternative explanations of our results. To this end, we proceed in three ways.

We begin with a test that measures the treatment effect through the exposure of the capital requirements of Texas banks to housing collateral values prior to the event. The revised setting replaces, therefore, the exposure measure based on a dummy variable with a more refined non-binary proxy for the treatment effect. Under this revised setting, we should find that the impact of the law on the regulatory capital ratio is more negative for higher values of our exposure measure.

Next, we present a series of tests that exploit cross-sectional heterogeneity in our sample to corroborate housing collateral values as the main channel of our finding. This battery of tests shares the same intuition: if our results are driven by the collateral value effect, they should mostly emerge in banks for which this effect is expected to be more important. To implement the tests, we rely on the broadly documented influence of housing collaterals on mortgage risk (see, for instance, Agarwal et al., 2015; Campbell and Cocco, 2015; and Gerardi et al., 2018; Jiang and Zhang, 2023).

First, more uncertainty about collateral values in a mortgage might lead to a larger bank's exposure to losses and in mortgage contracts that are perceived as riskier (Jiang and Zhang (2023). Therefore, if our results are driven by a collateral value effect, they should be stronger for banks that are more exposed to recovery risk prior to the adoption of the new law. Second, the increase valuation of housing collaterals and the consequent decrease in the average LTV of mortgage contracts, jointly with the decrease in financial constraints due to the law, should reduce the propensity of borrowers to default (Guiso et al., 2013; Gerardi et al., 2018). Thus, if the excess regulatory capital held by banks is driven by a housing collateral value effect, the finding should be mostly driven by banks more exposed to borrowers that are likely to exhibit a stronger propensity to default ex ante.

In a second group of tests, we intend to rule out the possibility that our results can be linked to other potential effects of the law on bank policies. For instance, it might be suggested that the lower regulatory capital ratios we observe are simply an indication of a broader change in risk-taking by banks due to boosting house prices. We exploit heterogeneity in our results due to a bank's risk attitude to assess how plausible this explanation can be.

Finally, the law might also induce portfolio adjustments by a bank that are mechanically reflected in its regulatory capital ratio. Specifically, if the law creates incentives for borrowing more in the mortgage market by households, the asset composition of a bank can shift towards assets with a lower regulatory risk weight. To rule out this explanation, we expand our baseline analysis to a wider range of additional dependent variables that refer to the asset structure of a bank.

5.1. Evidence Based on a Non-Binary Treatment Measure

We begin by constructing a non-binary treatment measure based on a proxy of the ex-ante importance of housing collateral values for the capital requirements of treated banks. We base this measure on the value of first lien single-family mortgages that a bank has provided in the local markets (call report item: RCON5367) scaled by the total values of RWA in the same market at Q3/1997. Given that in the regulatory capital framework of Basel I all mortgages received a regulatory risk weight of 50%, a higher value of this ratio indicates that a larger proportion of the capital requirements of a bank is linked to housing collateral.

[TABLE 7 HERE]

We employ this ratio to construct the following exposure measure that replaces our treated dummy in our baseline setting:

$$\text{Exposure}=\begin{cases} 0 \text{ for banks in the control group} \\ \frac{\text{Single Family Mortgages}}{\text{RWA}} \text{ for banks in the treated group} \end{cases} \quad (3)$$

We then use the interaction of this measure with the post dummy to capture the relative impact of the event on treated banks. We report the results of this test in Table 6. Specifically, as in Table 2, initially we estimate a model without control variables, we then control only for banks size and finally we add the full set of controls.

Across all specifications, we find that a higher exposure of capital requirements to housing collateral in treated banks prior to the law is associated with a decrease in the regulatory capital ratio of these banks relative to the control group. This finding offers some preliminary evidence in favor of our results being driven by the valuation effects of the law on the housing collateral pledged to the mortgage portfolio.

5.2. The Exposure of Banks to Recovery Risk

Next, we examine the importance of a bank's exposure to recovery risk on our results. We build on the evidence in Jiang and Zhang (2023) showing that mortgages characterized by higher uncertainty about collateral values are perceived as riskier and thus are more likely to be rejected, receive worse rates, and show lower loan to value. These findings indicate that this uncertainty is perceived by a bank as a factor that amplifies its exposure to recovery risk in the case of a mortgage default. Using this finding as a starting point, we construct a measure of a bank's exposure to uncertainty in the value of residential collateral prior to the law and examine how this measure influences our main findings.

Precisely, we rely on the yearly house price index available at the county level on the website of the Federal Housing Finance Agency and quantify for each bank the exposure to house price volatility prior to the regulatory change. To this end, we compute for each county the 5-year volatility in the growth rate of the house price index with 1997 representing the last year of the estimation window. We then use the distribution of branches at the county level from the SOD from the FDIC to collapse this measure at the bank level as follows:

$$\text{Bank Price Volatility}_{i,1997} = \sum_{j=1}^{n} w_{i,j} \times \text{County Price Volatility}_{j,1997} \quad (4)$$

Where w represents the branch (deposit) of a bank *i* in a county *j* scaled by the total number of branches in a state, and County Price Volatility is the 5-year volatility in the growth of house prices in a county *j* where a bank operates branches. Finally, we classify as highly exposed to uncertainty in house prices those banks in the last quintile of the sample distribution and estimate the following model:

$$\text{Regulatory Capital Ratio}_{i,t} = \alpha + \beta_1 \text{Treated}_i \times \text{Post}_t + \beta_2 \text{Treated}_i \times \text{Post}_t \times \text{High Price Volatility} + \mathbf{BANK} + \mathbf{TIME} + \varepsilon_{i,,t}\ , \quad (5)$$

Where High Volatility is a dummy equal to one for banks more exposed to high volatile residential markets. The coefficient of interest in $\beta_{2.}$ and captures the difference in the average treatment effect for banks more exposed to recovery risk (that is, when the dummy High Volatility is equal to one). Notably, the High Volatility dummy does not enter directly in the model as its effect is subsumed by bank-fixed effects.

We report the results of this test in Table 7. As in our baseline analysis, we begin by estimating a model without controls and then we add the full set of bank controls employed in our previous investigation. Consistent with our results being driven by a collateral value effect, we find that the treatment effect emerges in the group of Texas banks more exposed to uncertainty about house valuation prior to the new law. This finding is robust to the inclusion of control variables and does not depend on whether we estimate a bank's presence in a county through branch shares (columns (1) and (2)) or through deposit shares (columns (3) and (4)).

[TABLE 7 HERE]

The results of all the tests shown in Table 7 add confidence to the initial interpretation of our findings. We show evidence consistent with a role for a bank's ex-ante exposure to recovery risk as a key driver of our findings and this risk is expected to be mitigated by the positive impact of the law on housing collateral values.

5.3. Mortgage Borrowers' Default Propensity

Another way to support the initial interpretation of our findings is based on inspecting cross-sectional heterogeneity due to the propensity of borrowers to default on their mortgage. For instance, Gerardi et al. (2018) show that when the LTV increases, there are growing incentives for borrowers to strategically default, especially if they have low residual income. The interplay between exposure of borrowers to income shocks and LTV makes the propensity to default especially higher. Similarly, Guiso et al. (2013) confirm empirically that considerations regarding property values do matter for strategic defaults. Additionally, also in Campbell and Cocco (2015) the propensity to default is highly, and jointly, influenced by LTV and mortgage affordability.

Although it is intrinsically difficult to quantify borrower propensity to default and how this propensity changes over time, we present below two groups of tests bas*f*ed on measures that indirectly should allow us to capture variations in such propensity and its implications. These measures offer complementary information, and we argue that jointly the tests we present can be sufficiently informative for our analysis.

The first set of tests rely on the fact that low-income borrowers are more concerned about income shocks and are likely to have more (less) incentives to strategically default in the presence of an increase (decrease) in house values (Gerardi et al., 2018). Therefore, if our results are driven by a collateral value effect, banks more exposed to these borrowers should assign more importance to the consequences of a decrease in the average LTV of their mortgage portfolio. This importance should be reflected in a more pronounced decrease in the regulatory capital ratio of these banks as compared to other treated banks.

We quantify a bank's specific exposure to low-income borrowers following a similar empirical strategy as in the previous section. Specifically, we combine branch and deposit data from the SOD by the FDIC and county level information about median household income, to construct the following variable:

$$\text{Bank Household Income}_{i,1997} = \sum_{j=1}^{n} w_{i,j} \times \text{MHI}_{j,1997} \quad (6)$$

Where *w* represents the branch (deposit) a bank *i* has in a county *j* scaled by the total number of branches in a state, and MHI is the median household income in a county *j* where a bank operates branches. Using this weighted average household income, we then construct a dummy (Low Income) that takes a value of 1 if a bank is in the first quintile of the 1997 sample distribution (lowest values) that we interact with the post and treated dummies.

[TABLE 8 HERE]

Panel A of Table 8 shows the results. We find support for our prior: the impact of the new law for the regulatory capital ratio of treated banks is higher if these banks are more exposed to low-income borrowers. As in the previous section, the results do not depend on whether we construct the measure based on branch share (columns (1) and (2)) or deposit share (columns (3) and (4)). Furthermore, we reach a similar conclusion in the last four columns of the Panel A if we use earnings per capita as an alternative proxy for low-income borrowers.

The underlined mechanism the test above suggests is that the increase in collateral values leads banks to expect a lower default frequency by borrowers especially if they have low income. Next, we assess if this expectation is supported by our data and look at the evolution of the riskiness of real estate loans in a bank's portfolio after the new law is implemented. Our intuition is as follows. If the propensity of mortgage borrowers to default decrease due to LTV benefits, we should observe at least some evidence of a decrease in the riskiness of the real estate portfolio held by a bank. Accordingly, we repeat the baseline analysis using as dependent variables proxies of the riskiness of real estate lending by a bank and using a symmetric estimation period (starting in Q1/1995 and ending in Q4/2000).

More precisely, we initially employ a measure of the residential assets repossessed by the bank scaled by total assets in the balance sheet based on domestic offices. The repossessed assets are captured through the call report data items RCON5011 and RCON5012 that refer to residential properties owned by a bank in the US. Indeed, these items include, although they are not limited to, residential properties that a bank has acquired via foreclosures. As an alternative measure we refer to the value of other real estate owned by a bank (RCON2150) scaled by total assets, which, in spite of not referring only to residential assets, it might be a more direct proxy of foreclosures in the real estate market.[8] Finally, we employ the ratio between non-performing real estate loans and total real estate loans as a final proxy of the riskiness of the real estate portfolio. While this is

[8] The item includes: "The book value (not to exceed fair value), less accumulated depreciation, if any, of all real estate other than bank premises owned by the bank and its consolidated subsidiaries. Includes as other real estate owned: (1) real estate acquired in any manner for debts previously contracted (including, but not limited to, real estate acquired through foreclosure and real estate acquired by deed in lieu of foreclosure), even if the bank has not yet received title to the property (hereafter referred to as "foreclosed real estate"); (2) real estate collateral underlying a loan when the bank, branch or agency has obtained possession of the collateral, regardless of whether formal foreclosure proceedings have been instituted against the borrower". See https://www.federalreserve.gov/apps/mdrm/data-dictionary/search/item?keyword=2150&show_short_title=False&show_conf=False&rep_status=All&rep_state=Opened&rep_period=Before&date_start=99991231&date_end=99991231.

a more conventional risk proxies, it does not allow us to focus only on residential properties. We report the results of these tests in Panel B of Table 8 where we estimate models akin to our baseline equation without and with controls. Across all specifications, we find that the treated banks exhibit a relative decrease in their risk exposure after the law change.

Overall, the results are consistent with a demand side interpretation of risk attitude in line with the evidence in Zevelev (2021) on what drove the increase in house prices after the introduction of the HEL Texas law. The findings are instead inconsistent with a supply side story, wherein banks respond to the regulatory change by increasing their risk taking in the real estate market to take advantage of the increase in house values. We further explore this aspect in the next sections.

5.4. Bank Risk Taking Attitude

It might still be argued that the results reported in the previous sections do not necessarily exclude an impact of the law on the overall risk attitude of the bank. In fact, the decrease in the real estate risk exposure of the bank might have increased the incentives to take additional risks on other business lines, thus making the overall bank risk higher. According to this interpretation, the change in the regulatory capital ratio we observe is simply a reflection of a broader more aggressive risk-taking attitude that treated banks should show because of the observed increase in house prices.

To rule out the interpretation above, we begin by examining how our results depend on a bank's equity ratio prior to the event. Our intuition here is as follows. If our results are (at least in part) driven by a more generalized shift upward in bank risk-taking, we should observe stronger results in banks where these incentives are expected to be higher; namely, in banks with extremely low equity ratios that might have more incentives for *gambling for resurrection* (Admati et al., 2018; Freixas et al., 2004). In contrast, if, consistent with our current interpretation, our results reflect a perceived lower risk exposure of the bank in the real estate segment that leads to a decrease in regulatory capital held for prudential motives, we should observe stronger findings for more risk adverse banks; that is, banks that operate with extremely high equity ratios also before the event.

[TABLE 9 HERE]

To test these contrasting arguments, we construct two dummy variables based on the distribution of the equity ratio in Q3/1997. The first dummy (Overcapitalized) takes the value of one if this ratio is in the upper quintile of the sample distribution and zero otherwise. The second dummy (Undercapitalized) assumes instead a value of 1 if the equity ratio is in the lowest quintile of the 1997 sample distribution. We then add these dummies separately in our baseline regression to create triple interaction terms with the dummies Post and Treated.

We report the results of these tests in Panel A of Table 9. The first two columns focus on overcapitalized banks while the remaining columns report the results for undercapitalized banks. We observe that the impact of law on the regulatory capital ratio is significantly more negative in banks that used to follow more prudent capital choices prior to the event. In contrast, there is no evidence that the effect is mor pronounced for weakly capitalized banks. If any different, the decreasing effect on these banks is weaker than in the rest of the sample of treated banks.

To offer further support for this finding, we employ an alternative setting based on a measure of risk propensity by banks obtained by scaling the equity ratio by earnings volatility computed for a 4-quarter window ending in Q3/1997. This variable, therefore, represents the equity ratio expressed per unit of bank risk and it is the key capital component of the default risk of a bank when computed via a z-score. Using this alternative variable above, we construct two dummies that identify banks with high-risk propensity as those in the last quintile of the sample distribution and banks with high-risk propensity as those in the first quintile of the same distribution. We finally replicate the analysis in Panel A and report the results in Panel B. The results confirm the evidence documented in Panel B: banks that appear to be more risk adverse before the new law are those more affected by the law and its implications on house value.

Both group of tests presented in this section are inconsistent with an interpretation of our findings based on more aggressive risk-taking attitude by banks after the HEL Texas law.

5.5. Mechanical Effects of Portfolio Adjustments

A further alternative explanation of our findings is related to portfolio adjustments by banks. For example, Chakraborty et al. (2018) find that banks shift their portfolio composition from commercial loans to mortgages when house prices increase. In terms of regulatory capital ratios, this strategic choice would imply a decrease in the relative importance of loans with higher risk weights and a consequent decrease in regulatory capital ratios. This explanation seems, however, inconsistent with the lack of impact of the law on economic outcomes, as reported by Zevelev (2021). In fact, these outcomes should be affected by the law in the presence of a reduced access to credit by firms. Additionally, this interpretation does not seem to align with the lack of impact of the law on home ownership as documented by Zevelev (2021). In the rest of this section, we provide additional evidence that goes against this explanation of our findings.

We focus on changes in the asset composition of banks. If portfolio adjustments are at work, we should observe significant changes in the asset structure of Texas banks after the new law is adopted. To conduct this test, we consider as dependent variables: 1) residential real estate loans; 2) commercial real estate loans; 3) C&I loans; and 4) consumer loans. We estimate models without and with control variables which consist of Size, NPL, Insured Deposits (in addition to bank and quarter fixed effects). We do not include variables related to assets structure/loan portfolio composition as controls to avoid mechanical relationships in our model.

[TABLE 10 HERE]

The results reported in Table 10 do not show any evidence of a change in the asset structure of the treated banks relative to the control group in the post period. These non-results are again inconsistent with our results being a consequence of Texas banks reshaping their asset structure in favor of mortgages.

6. Conclusion

We show that housing collateral values are causally linked to banks' choice of their regulatory capital ratio even under the simplified regulatory capital framework of Basel I where residential collaterals did not significantly influence mandatory capital requirements beyond the favorable lower risk weights assigned to mortgages. Our findings suggest that banks incorporate a crucial risk factor of the mortgage portfolio in how they manage their discretionary component of the regulatory capital ratio.

We document that our results are consistent with the importance of collateral risk for banks by showing stronger evidence for banks more exposed to uncertainty in collateral valuation and fragile homeowners. Ultimately, our findings highlight that some of the elements that have been only recently at the core of the re-design of mandatory capital requirements for the mortgage portfolio, such as the size of housing collateral values relative to a bank's credit exposure, already matter for the discretionary component of the regulatory capital ratio chosen by banks.

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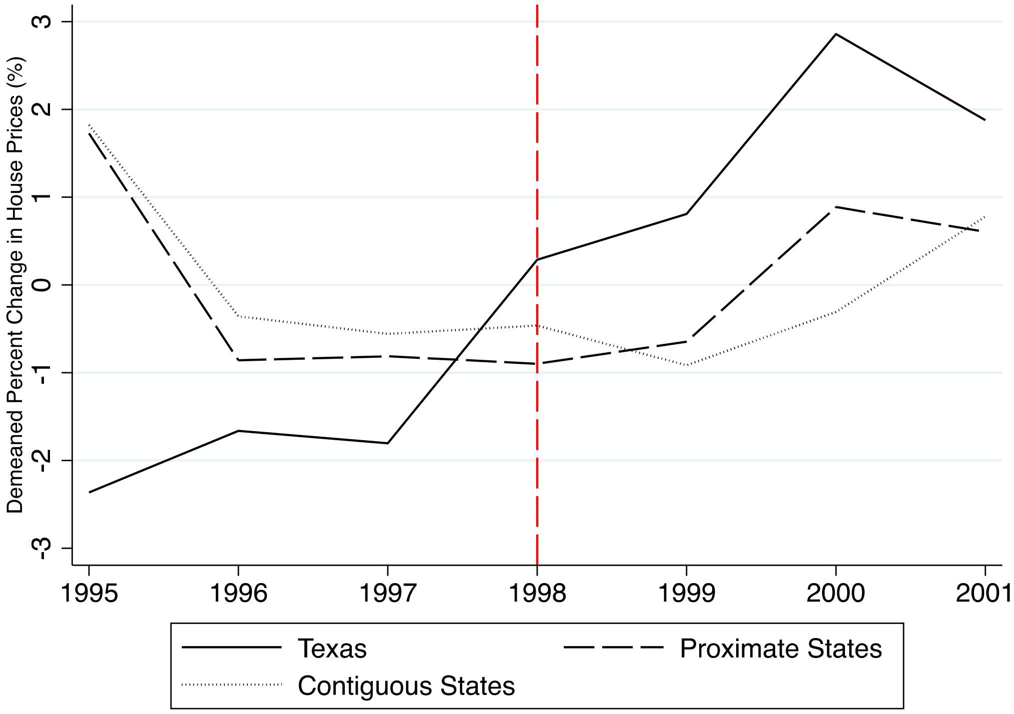

Figure 1: Evolution of House Prices Around the HEL Law in Texas

This figure plots the yearly demeaned percent change in house price in Texas and in contiguous states (Arkansas, Louisiana, New Mexico, and Oklahoma) and proximate states (contiguous states plus Colorado and Kansas). Changes in house prices are measured through the annual all transactions house price index from the federal housing finance agency (FHFA) available at https://www.fhfa.gov/DataTools/Downloads/Pages/House-Price-Index-Datasets.aspx#qat.

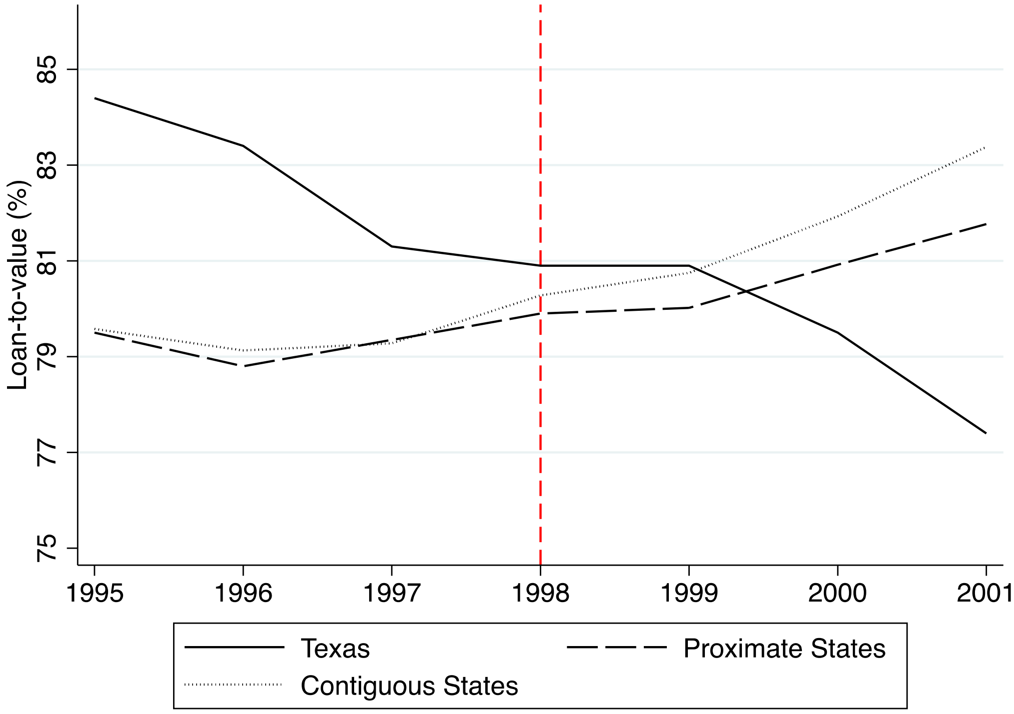

Figure 2: Evolution of the Loan-to-Value Ratio of Single-Family Mortgages Around the HEL Law in Texas

This figure plots the yearly loan-to-value (LTV) ratio for single-family mortgages in Texas and in contiguous states (Arkansas, Louisiana, New Mexico, and Oklahoma) and proximate states (contiguous state plus Colorado and Kansas). The LTV ratio at the state level is provided by the federal housing finance agency (FHFA) and it is available at https://www.fhfa.gov/DataTools/Downloads/Pages/Monthly-Interest-Rate-Data.aspx.

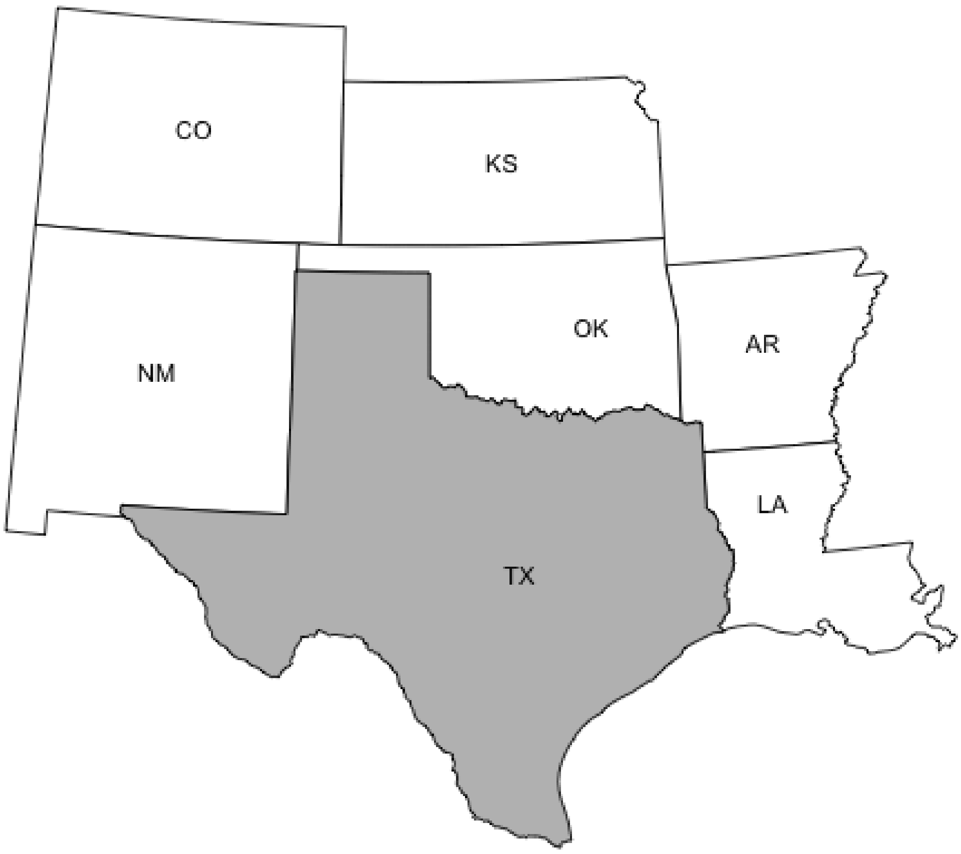

Figure 3: Texas and Nearby States

This figure shows a map of the U.S. state of Texas and bordering states (Arkansas, Louisiana, New Mexico, and Oklahoma) as well states with the closest distance as compared to the Texas border (Colorado and Kansas), which are used to construct our sample of treated (grey) banks and control banks (white) operating exclusively in respective states.

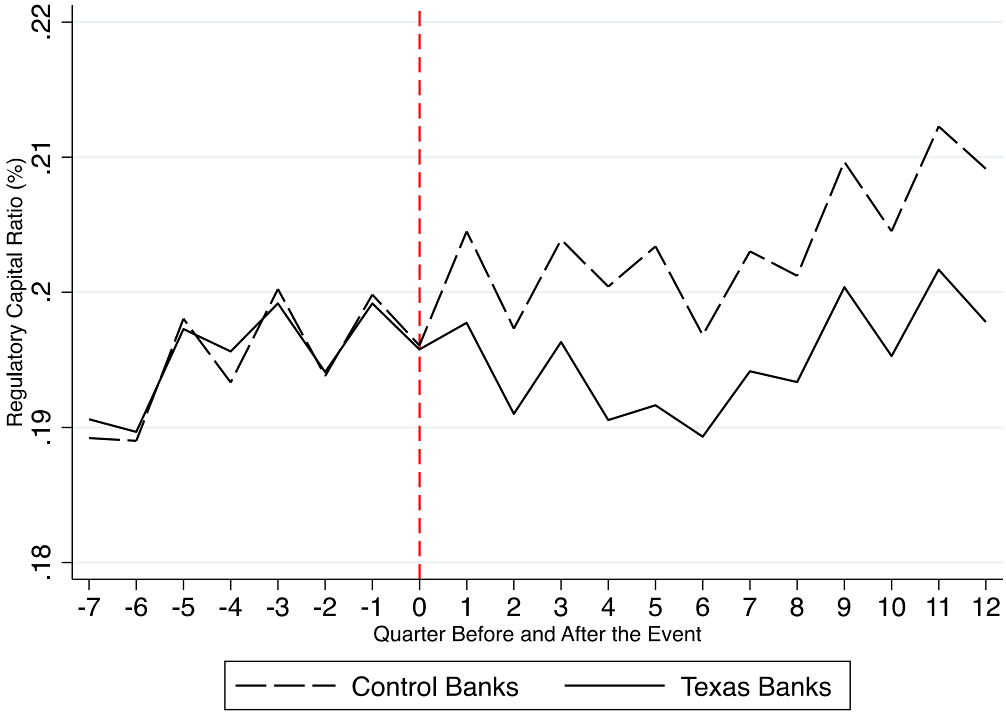

Figure 4: Parallel Trend Analysis

This figure plots the fitted values of the trend in the regulatory capital ratio for treated and control banks for the period from Q1/1996 to Q4/2000. The trend estimates are obtained from a linear model with bank controls.

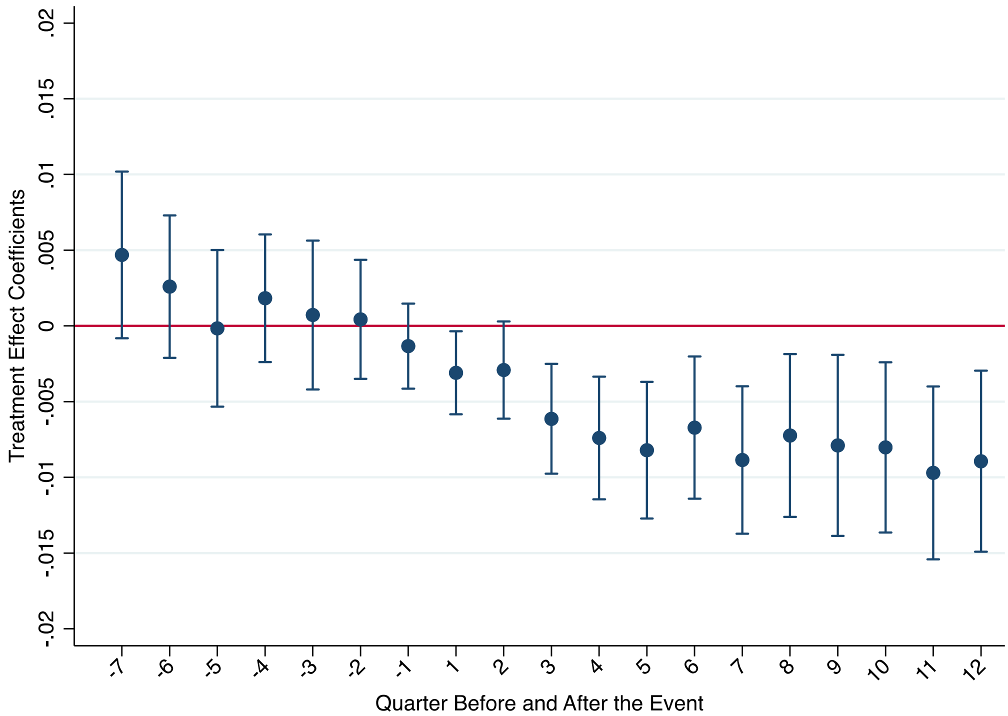

Figure 5: Coefficient Plot from a Dynamic Fixed Effect Model

This figure plots the fitted values of the trend in the regulatory capital ratio for treated and control banks for the period from Q1/1996 to Q4/2000. The trend estimates are obtained from a linear model with bank controls.

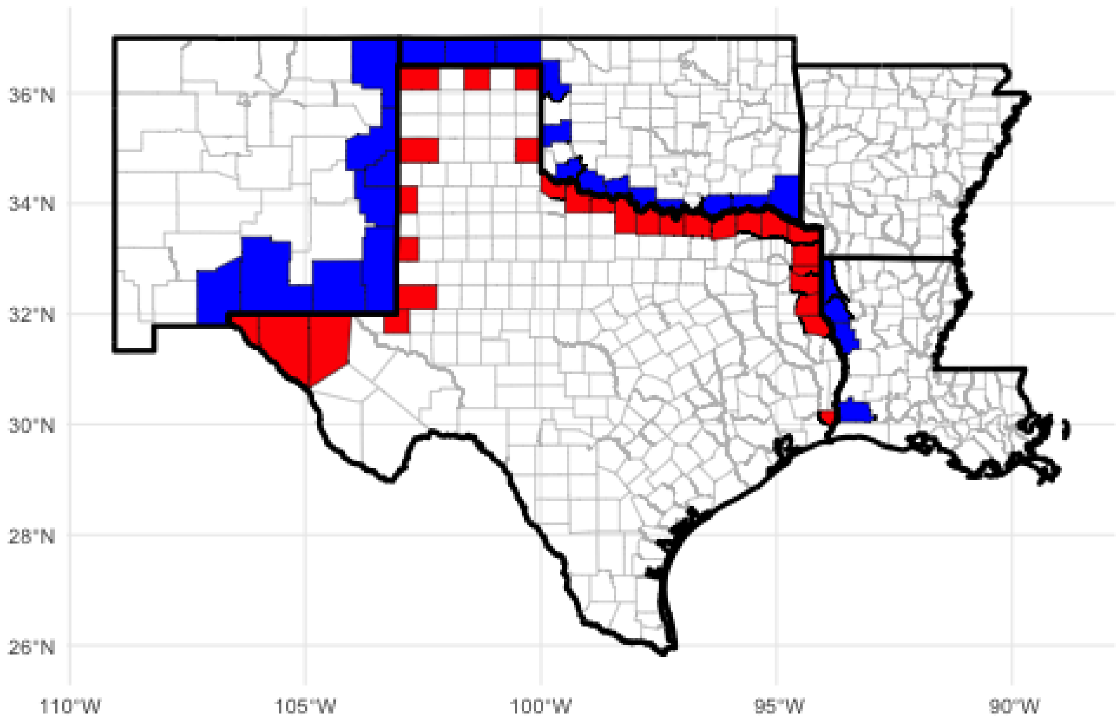

Figure 6: Illustration of Identification Strategy Based on Contiguous Counties

This figure shows a map of US counties in Texas and bordering states (Arkansas, Louisiana, New Mexico, and Oklahoma) to illustrate our identification strategy based on contiguous counties. Counties colored in red are those where treated Texas banks hold over 50% of their deposits (branches) in that county; adjacent counties outside of Texas highlighted in blue are those where matched control banks hold significant shares (>50%) of their deposits (branches) in that county.

Table 1: Summary Statistics and Parallel Trend

The table reports summary statistics, means and standard deviations, of treated versus control bank samples before and after propensity score matching (Panel A) and average values of quarterly changes in banks' regulatory capital ratios (Panel B). Treated banks are banks in Texas that operate all their branches within Texas only in June 1997 (based on FDIC Summary of Deposits); Non-treated banks are banks that operate no bank branches inside Texas and have operating branches in bordering states (Arkansas, Louisiana, New Mexico, and Oklahoma) and states with the closest distance as compared to the Texas border (Colorado and Kansas), and only operate branches in a single state. Propensity score matching is performed as 1:1 matching without replacement of treated banks and control banks in terms of: 1) Size (the log transformation of total assets); 2) ROA (net income scaled by total assets; 3) Loans (loans in percentage to total assets); 4) Regulatory Capital Ratio (regulatory capital scaled by total assets); 5) NPL (non-performing loans divided by total assets); 6) C&I Loans (measured in proportion to total loans) 7) Residential Mortgages (measured in proportion to total loans); 8) Insured Deposits (the ratio between insured deposits and total deposits).

Panel A: Summary Statistics

					Matched Sample				
	Treated Banks (847)		Non-Treated Banks (1371)		Treated Banks (666)		Non-Treated Banks (666)		**Covariate Balance (Normalized Diff.)**
	Mean	Std.Dev	Mean	Std.Dev	Mean	Std.Dev	Mean	Std.Dev	
Regulatory Capital Ratio	0.208	0.112	0.193	0.108	0.200	0.107	0.204	0.102	-0.028
Size	11.095	1.103	10.913	1.074	11.009	0.987	11.016	1.122	-0.005
ROA	0.003	0.002	0.003	0.002	0.003	0.002	0.003	0.002	0.025
Loans	0.479	0.154	0.563	0.145	0.512	0.143	0.507	0.142	0.023
NPL	0.011	0.013	0.011	0.013	0.011	0.014	0.011	0.014	-0.001
C&I Loans	0.188	0.102	0.164	0.098	0.185	0.103	0.182	0.107	0.023
Residential Mortgages	0.210	0.122	0.265	0.144	0.227	0.126	0.230	0.130	-0.018
Insured Deposits	0.640	0.115	0.640	0.110	0.642	0.114	0.637	0.115	0.027

Panel B: Parallel Trend – Mean Change in the Regulatory Capital Ratio

	Treated Banks	Non-Treated Banks	Difference	P-Value
Δ Regulatory Capital Ratio full pre-shock period	-0.002	-0.001	-0.001	0.296
Δ Regulatory Capital Ratio$_{t-7}$	0.001	0.004	-0.003	0.301
Δ Regulatory Capital Ratio $_{t-6}$	0.001	0.004	-0.003	0.081
Δ Regulatory Capital Ratio $_{t-5}$	-0.005	-0.005	0.000	0.802
Δ Regulatory Capital Ratio $_{t-4}$	0.003	0.002	0.001	0.737
Δ Regulatory Capital Ratio $_{t-3}$	-0.005	-0.003	-0.002	0.293
Δ Regulatory Capital Ratio $_{t-2}$	-0.003	-0.002	-0.001	0.555
Δ Regulatory Capital Ratio $_{t-1}$	-0.005	-0.007	0.002	0.199

Table 2: The Impact of the HEL Texas Law on the Regulatory Capital Ratio of Local Banks

The table reports difference-in-differences analyses of the impact of the 1998 Texas Home Lending Law on the regulatory capital ratio of local banks. The estimation period ranges from Q1/1996 to Q4/2000. Panel A shows the results of a univariate difference-in-differences analysis to estimate the average treatment effect. The t-test of equality of means compares the average difference in the regulatory capital ratio between the post and the pre-event period for groups of treated and untreated banks and then test whether these differences significantly differ between the two groups. Panel B reports the results of a multivariate analysis (based on equation (2)). *Treated* is a dummy that equals one if a bank has its branch deposits only in Texas and zero for single state banks operating in bordering states (Arkansas, Louisiana, New Mexico, and Oklahoma) and states with the closest distance as compared to the Texas border (Colorado and Kansas). *Post* is a dummy equal to one in the post-shock window (up to 12 quarters after the shock). The difference-in-differences estimate of the coefficient of *Treated* × *Post* is the difference between the changes in the regulatory capital ratio of treated banks and control banks from the pre- to the post-shock period. Size is the logarithmic transformation of bank total assets in thousands of US$. ROA is the ratio between net income and total assets. NPL is the fraction of non-performing loans with respect to total loans. Loans is constructed as total loans divided by total assets. C&I loans is the ratio between commercial and industrial loans and total loans. Residential Mortgages is the ratio between mortgages secured by residential properties and total loans and Insured Deposits is the ratio between insured deposits and total deposits. All models include bank and quarter-year fixed effects. Standard errors given in parentheses are corrected for heteroskedasticity and are clustered at the bank-level. ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Panel A: Univariate Difference in Differences

	Regulatory Capital Ratio		
	Treated	Untreated	DID
	(1)	(2)	(3)
Average Diff. Post-Pre	-0.013***	-0.005***	-0.008***
T-value	(6.90)	(2.81)	(2.96)

Panel B: Regression Results

	Regulatory Capital Ratio		
	(1)	(2)	(3)
Treated ×Post	-0.008***	-0.007***	-0.008***
	(0.003)	(0.002)	(0.002)
$Size_{t-1}$		-0.049***	-0.039***
		(0.007)	(0.005)
ROA_{t-1}			1.169***
			(0.283)
$Loans_{t-1}$			-0.198***
			(0.010)
NPL_{t-1}			0.109*
			(0.059)
C&I $Loans_{t-1}$			-0.001
			(0.015)
Residential $Mortgages_{t-1}$			0.050**
			(0.020)
Insured $Deposits_{t-1}$			-0.019
			(0.015)
Constant	0.204***	0.731***	0.713***
	(0.001)	(0.071)	(0.065)
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	24,896	24,876	24,858
Adjusted R^2	0.05	0.10	0.19

Table 3: The Impact of the HEL Texas Law on the Regulatory Capital Ratio of Local Banks - Robustness

The table reports additional specifications of the difference-in-differences analyses of the impact of the 1998 Texas Home Lending Law on the regulatory capital ratio of local banks. *Treated* is a dummy that equals one if a bank has its branch deposits only in Texas and zero for single state banks operating in bordering states (Arkansas, Louisiana, New Mexico, and Oklahoma) and states with the closest distance as compared to the Texas border (Colorado and Kansas). *Post* is a dummy equal to one in the post-shock window (up to 12 quarters after the shock). The difference-in-differences estimate of the coefficient of *Treated* × *Post* is the difference between the changes in the regulatory capital ratio of treated banks and control banks from the pre- to the post-shock period. The set of bank controls includes Size (the logarithmic transformation of bank total assets in thousands of US$), ROA (the ratio between net income and total assets), NPL (the fraction of non-performing loans with respect to total loans), Loans (total loans divided by total assets), C&I loans (the ratio between commercial and industrial loans and total loans), Residential Mortgages (the ratio between mortgages secured by residential properties and total loans) and Insured Deposits (insured deposits scaled by total deposits). In the first two columns, we reduce the post estimation period to 8 quarters. In columns (3) and (4), we follow the approach proposed by Bertrand et al. (2004) and collapse the pre and post estimation to one period and employ average of the variables computed over our initial estimation window. In columns (5) and (6) we employ the synthetic difference-in-differences estimator proposed by Arkhangelsky et al. (2021). In the last two columns, we repeat our baseline analysis by using the Tier 1 Regulatory Capital Ratio as dependent variable. All models include bank and quarter-year fixed effects. Standard errors given in parentheses are corrected for heteroskedasticity and are clustered at the bank-level. ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

	-8 quarters,+8 quarters		Bertrand et al. (2004)		Synthetic DID		Alternative Dependent Variable (Tier 1 Regulatory Ratio)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated ×Post	-0.007***	-0.007***	-0.007**	-0.006***	-0.006**	-0.007***	-0.008***	-0.008***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Constant	0.203***	0.753***	0.205***	0.950***			0.193***	0.705***
	(0.001)	(0.076)	(0.001)	(0.125)			(0.001)	(0.065)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,409	20,372	2,613	2,613	21,540	21,460	24,896	24,858
Adjusted R^2	0.04	0.16	0.07	0.34			0.05	0.19

Table 4: The Impact of the HEL Texas Law on the Regulatory Capital Ratio of Local Banks - Alternative Matching and Control Groups

The table reports additional specifications of the difference-in-differences analyses of the impact of the 1998 Texas Home Lending Law on the regulatory capital ratio of local banks. The estimation period ranges from Q1/1996 to Q4/2000. *Treated* is a dummy that equals one if a bank has its branch deposits only in Texas. *Post* is a dummy equal to one in the post-shock window (up to 12 quarters after the shock). The difference-in-differences estimate of the coefficient of *Treated* × *Post* is the difference between the changes in the regulatory capital ratio of treated banks and control banks from the pre- to the post-shock period. The set of bank controls includes Size (the logarithmic transformation of bank total assets in thousands of US$), ROA (the ratio between net income and total assets), NPL (the fraction of non-performing loans with respect to total loans), Loans (total loans divided by total assets), C&I loans (the ratio between commercial and industrial loans and total loans), Residential Mortgages (the ratio between mortgages secured by residential properties and total loans) and Insured Deposits (insured deposits scaled by total deposits). In Panel A, we report the results with different matching variables to obtain the control group. In Panel B, we employ the same set of matching variables of Table 1, but we modify the geographic selection of the control banks. In the first four columns of Panel B, we employ all single state banks as potential controls while in the last four columns we employ only banks located in adjacent states ((Arkansas, Louisiana, New Mexico, and Oklahoma). All models include bank and quarter fixed effects. Standard errors given in parentheses are corrected for heteroskedasticity and are clustered at the bank-level. ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Panel A: Alternative Matching Variables								
	Size, ROA, NPL		Size, ROA, NPL, Loans		Adding Loan Commitments		Adding Loan Commitments & MBS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated ×Post	-0.007***	-0.007***	-0.007***	-0.007***	-0.007***	-0.005**	-0.010***	-0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Constant	0.201***	0.751***	0.201***	0.751***	0.202***	0.776***	0.203***	0.794***
	(0.001)	(0.061)	(0.001)	(0.061)	(0.001)	(0.072)	(0.002)	(0.075)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	31,129	31,088	31,129	31,088	24,478	24,448	22,619	22,591
Adjusted R^2	0.05	0.17	0.05	0.17	0.05	0.19	0.05	0.19
Panel B: Alternative Matched Samples								
	All States				Contiguous States			
	1-1 Matching		1-3 Matching with replacement		No Matching		1-1 Matching	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated ×Post	-0.008***	-0.006***	-0.008***	-0.008***	-0.007***	-0.007***	-0.011***	-0.009***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Constant	0.204***	0.854***	0.200***	0.746***	0.204***	0.886***	0.201***	0.785***
	(0.001)	(0.079)	(0.001)	(0.067)	(0.001)	(0.084)	(0.002)	(0.072)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	30,482	30,444	29,817	29,775	29,914	29,844	18,903	18,876
Adjusted R^2	0.04	0.18	0.04	0.17	0.04	0.18	0.05	0.18

Table 5: County Based Analysis

The table reports difference-in-differences analyses of the impact of the 1998 Texas Home Lending Law on the regulatory capital ratio of local banks focusing on adjacent counties. The estimation period ranges from Q1/1996 to Q4/2000. *Treated* is a dummy that equals one if a bank in the matched sample, employed in the last two columns of Panel B of Table 4, has at least 50% of its deposits (columns (1) and (2)) or branches (columns (3) and (4)) in adjacent counties between Texas and the contiguous states (Arkansas, Louisiana, New Mexico, and Oklahoma). Post is a dummy equal to one in the post-shock window (up to 12 quarters after the shock). The difference-in-differences estimate of the coefficient of Treated × Post is the difference between the changes in the regulatory capital ratio of treated banks and control banks from the pre- to the post-shock period. The set of bank controls includes Size (the logarithmic transformation of bank total assets in thousands of US$), ROA (the ratio between net income and total assets), NPL (the fraction of non-performing loans with respect to total loans), Loans (total loans divided by total assets), C&I loans (the ratio between commercial and industrial loans and total loans), Residential Mortgages (the ratio between mortgages secured by residential properties and total loans) and Insured Deposits (insured deposits scaled by total deposits). All models include bank and quarter fixed effects. Standard errors given in parentheses are corrected for heteroskedasticity and are clustered at the bank-level. ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

	Based on Branch Share in Adjacent Counties >=50%		Based on Deposit Share in Adjacent Counties >=50%	
	(1)	(2)	(3)	(4)
Treated ×Post	-0.016**	-0.010*	-0.016**	-0.010*
	(0.007)	(0.006)	(0.008)	(0.006)
Constant	0.200***	0.774***	0.196***	0.809***
	(0.004)	(0.176)	(0.004)	(0.173)
Controls	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	2,380	2,377	2,272	2,269
Adjusted R^2	0.04	0.19	0.04	0.20

Table 6: The Impact of the HEL Texas Law on the Regulatory Capital Ratio of Local Banks – Continuous Exposure Measure

The table reports difference-in-differences analyses of the impact of the 1998 Texas Home Lending Law on the regulatory capital ratio of local banks. The estimation period ranges from Q1/1996 to Q4/2000. Panel A shows the results of a univariate difference-in-differences analysis to estimate the average treatment effect. The t-test of equality of means compares the average difference in the regulatory capital ratio between the post and the pre-event period for groups of treated and untreated banks and then test whether these differences significantly differ between the two groups. Panel B reports the results of a multivariate analysis (based on equation (2)). *Exposure* is equal to zero for the control group (single state banks operating in bordering states (Arkansas, Louisiana, New Mexico, and Oklahoma) and in states with the closest distance as compared to the Texas border (Colorado and Kansas)) and equal to the ratio between first linen single-family mortgages in the domestic market and total RWA for domestic activities for treated banks (single states banks in Texas). *Post* is a dummy equal to one in the post-shock window (up to 12 quarters after the shock). The difference-in-differences estimate of the coefficient of *Treated* × *Post* is the difference between the changes in the regulatory capital ratio of treated banks and control banks from the pre- to the post-shock period. Size is the logarithmic transformation of bank total assets in thousands of US$. ROA is the ratio between net income and total assets. NPL is the fraction of non-performing loans with respect to total loans. Loans is constructed as total loans divided by total assets. C&I loans is the ratio between commercial and industrial loans and total loans. Residential Mortgages is the ratio between mortgages secured by residential properties and total loans and Insured Deposits is the ratio between insured deposits and total deposits. All models include bank and quarter-year fixed effects. Standard errors given in parentheses are corrected for heteroskedasticity and are clustered at the bank-level. ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)
Exposure ×Post	-0.031***	-0.027***	-0.027***
	(0.009)	(0.009)	(0.008)
Constant	0.204***	0.732***	0.717***
	(0.001)	(0.071)	(0.066)
Size	No	Yes	Yes
Other Controls	No	No	Yes
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	24,896	24,876	24,858
Adjusted R^2	0.05	0.10	0.18

Table 7: The Impact of the HEL Texas Law on the Regulatory Capital Ratio of Local Banks – Recovery Risk

This table reports difference-in-differences analyses of the impact of the 1998 Texas Home Lending Law on the regulatory capital ratio of local banks by bank exposure to house price volatility. The estimation period ranges from Q1/1996 to Q4/2000 focusing on banks operating in contiguous counties. In first three columns Treated is a dummy that equals one if a bank has its branch deposits only in Texas and a value of zero for single state banks operating in operating in bordering states (Arkansas, Louisiana, New Mexico, and Oklahoma) and in states with the closest distance as compared to the Texas border (Colorado and Kansas)) . The difference-in-differences estimate of the coefficient of Treated × Post is the difference between the changes in the regulatory capital ratio of treated banks and control banks from the pre- to the post-shock period. The set of bank controls includes Size (the logarithmic transformation of bank total assets in thousands of US$), ROA (the ratio between net income and total assets), NPL (the fraction of non-performing loans with respect to total loans), Loans (total loans divided by total assets), C&I loans (the ratio between commercial and industrial loans and total loans), Residential Mortgages (the ratio between mortgages secured by residential properties and total loans) and Insured Deposits (insured deposits scaled by total deposits). All models include bank and quarter-year fixed effects. Standard errors given in parentheses are corrected for heteroskedasticity and are clustered at the bank-level. ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Treated ×Post	0.006	0.003	0.005	0.002
	(0.006)	(0.004)	(0.006)	(0.004)
Treated ×Post ×High Price Volatility	-0.017**	-0.012**	-0.015**	-0.010**
	(0.007)	(0.005)	(0.007)	(0.005)
Constant	0.190***	0.739***	0.190***	0.740***
	(0.002)	(0.075)	(0.002)	(0.075)
Controls	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	17,772	17,740	17,772	17,740
Adjusted R^2	0.05	0.21	0.05	0.21

Table 8: The Impact of the HEL Texas Law on the Regulatory Capital Ratio of Local Banks – Heterogeneity Tests by Local Market Features

The table reports difference-in-differences analyses of the impact of the 1998 Texas Home Lending Law on the regulatory capital ratio of local banks by bank exposure to county income characteristics. The estimation period ranges from Q1/1996 to Q4/2000. Treated is a dummy that equals one if a bank has its branch deposits only in Texas and zero for single state banks operating in bordering states (Arkansas, Louisiana, New Mexico, and Oklahoma) and states with the closest distance as compared to the Texas border (Colorado and Kansas). Post is a dummy equal to one in the post-shock window (up to 12 quarters after the shock). The difference-in-differences estimate of the coefficient of Treated × Post is the difference between the changes in the regulatory capital ratio of treated banks and control banks from the pre- to the post-shock period. We obtain bank-specific measures of Median Household income and Earnings Per Capita by the weighted average of the county variables in the counties where a bank operates. In Panel A the weights are based on deposit shares of a bank in each county and in Panel B on branch shares. The bank-specific variables refer to 1997 and Low is a dummy equal to one if a bank falls in the first quintile of the sample distribution. The set of bank controls includes Size (the logarithmic transformation of bank total assets in thousands of US$), ROA (the ratio between net income and total assets), NPL (the fraction of non-performing loans with respect to total loans), Loans (total loans divided by total assets), C&I loans (the ratio between commercial and industrial loans and total loans), Residential Mortgages (the ratio between mortgages secured by residential properties and total loans) and Insured Deposits (insured deposits scaled by total deposits). All models include bank and quarter fixed effects. Standard errors given in parentheses are corrected for heteroskedasticity and are clustered at the bank-level. * * * ,**, and * indicate statistical significance at the 1%, 5% and 10% levels, respectively. treated banks and control banks from the pre- to the post-shock period. In the first two columns, the dependent variable is the ratio between non-performing loans related to real estate loans and total real estate loans. In columns (3) and (4) the dependent variable is the ratio between repossessed real estate assets and total assets and in columns (5) and (6) is the ratio between.

Panel A: Pre-shock Bank Specific Exposure to Low Income Households								
	Median Household Income				**Earnings per capita**			
	Based on Branch Share by County		Based on Deposit Share by County		Based on Branch Share by County		Based on Deposit Share by County	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated ×Post	-0.005*	-0.005**	-0.006*	-0.005**	-0.005*	-0.005**	-0.005*	-0.005**
	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Treated ×Post ×Low Income	-0.013**	-0.016***	-0.011*	-0.012**	-0.011*	-0.015**	-0.015**	-0.015***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Constant	0.204***	0.719***	0.204***	0.719***	0.204***	0.715***	0.204***	0.714***
	(0.001)	(0.065)	(0.001)	(0.066)	(0.001)	(0.066)	(0.001)	(0.066)
Controls	No	Yes	No	Yes	No	No	No	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24,896	24,858	24,896	24,858	24,896	24,858	24,896	24,858
Adjusted R^2	0.05	0.19	0.05	0.19	0.05	0.19	0.05	0.19
Panel B: Effects on Actual Mortgage Risk								
	Repossessed Residentials		**Repossessed Real Estate**		**Real Estate NPL**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated ×Post	-0.012**	-0.011**	-0.036***	-0.034**	-0.002**	-0.002***		
	(0.005)	(0.005)	(0.014)	(0.014)	(0.001)	(0.001)		
Constant	0.053***	0.068	0.248***	0.286	0.012***	-0.004		
	(0.003)	(0.073)	(0.008)	(0.223)	(0.000)	(0.013)		
Controls	No	Yes	No	Yes	No	Yes		
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes		
Time FE	Yes	Yes	Yes	Yes	Yes	Yes		
Observations	30,134	30,090	29,998	29,954	28,223	28,184		
Adjusted R^2	0.00	0.01	0.04	0.06	0.01	0.01		

Table 9: The Impact of the HEL Texas Law by Bank Risk Attitude

The table reports difference-in-differences analyses of the impact of the 1998 Texas Home Lending Law on the regulatory capital ratio of local banks. The estimation period ranges from Q1/1996 to Q4/2000. Treated is a dummy that equals one if a bank has its branch deposits only in Texas and zero for single state banks operating in bordering states (Arkansas, Louisiana, New Mexico, and Oklahoma) and states with the closest distance as compared to the Texas border (Colorado and Kansas). Post is a dummy equal to one in the post-shock window (up to 12 quarters after the shock). The difference-in-differences estimate of the coefficient of Treated × Post is the difference between the changes in the regulatory capital ratio of treated banks and control banks from the pre- to the post-shock period. Size is the logarithmic transformation of bank total assets in thousands of US$. ROA is the ratio between net income and total assets. NPL is the fraction of non-performing loans with respect to total loans. Loans is constructed as total loans divided by total assets. C&I loans is the ratio between commercial and industrial loans and total loans. Residential Mortgages is the ratio between mortgages secured by residential properties and total loans and Insured Deposits is the ratio between insured deposits and total deposits. All models include bank and quarter fixed effects. Standard errors given in parentheses are corrected for heteroskedasticity and are clustered at the bank-level. * * * , **, and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Panel A: Heterogeneity by pre-shock Equity Ratio				
	(1)	(2)	(3)	(4)
Treated ×Post	-0.004*	-0.004**	-0.010***	-0.010***
	(0.002)	(0.002)	(0.003)	(0.003)
Treated ×Post × Overcapitalized	-0.023**	-0.022**		
	(0.011)	(0.009)		
Treated ×Post × Undercapitalized			0.004	0.007*
			(0.005)	(0.004)
Constant	0.204***	0.719***	0.204***	0.736***
	(0.001)	(0.065)	(0.001)	(0.067)
Controls	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	24,896	24,858	24,896	24,858
Adjusted R^2	0.05	0.19	0.05	0.19
Panel B: Heterogeneity by pre-shock Equity Ratio Scaled by Earnings Volatility				
	(1)	(2)	(3)	(4)
Treated ×Post	-0.005*	-0.005**	-0.010***	-0.009***
	(0.003)	(0.002)	(0.003)	(0.002)
Treated ×Post × Low Risk Propensity	-0.015**	-0.015***		
	(0.006)	(0.005)		
Treated ×Post × High Risk Propensity			0.008	0.003
			(0.008)	(0.006)
Constant	0.204***	0.658***	0.204***	0.656***
	(0.001)	(0.058)	(0.001)	(0.058)
Controls	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	24,741	24,718	24,741	24,718
Adjusted R^2	0.05	0.18	0.05	0.18

Table 10: The Impact of the HEL Texas Law on Bank Asset Structure

The table reports difference-in-differences analyses of the impact of the 1998 Texas Home Lending Law on asset structure. The estimation period ranges from Q1/1995 to Q4/2000. We report the results of a multivariate analysis (based on equation (1)). Treated is a dummy that equals one if a bank has its branch deposits only in Texas and zero for single state banks operating in bordering states (Arkansas, Louisiana, New Mexico, and Oklahoma) and states with the closest distance as compared to the Texas border (Colorado and Kansas). Post is a dummy equal to one in the post-shock window (up to 12 quarters after the shock). The difference-in-differences estimate of the coefficient of Treated × Post is the difference between the changes in the dependent variable of treated banks and control banks from the pre- to the post-shock period. In the first two columns, the dependent variable is the ratio between non-performing loans related to real estate loans and total real estate loans. In columns (3) and (4) the dependent variable is the ratio between repossessed real estate assets and total assets and in columns (5) and (6) is the ratio between. We include the following set of controls: Size (the logarithmic transformation of bank total assets in thousands of US$), ROA (the ratio between net income and total assets), NPL (the fraction of non-performing loans with respect to total loans), and Insured Deposits (insured deposits scaled by total deposits). All models include bank and quarter fixed effects. Standard errors given in parentheses are corrected for heteroskedasticity and are clustered at the bank-level. * * * ,**, and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

	Residential Loans/Total Assets		Commercial Real Estate /Total Assets		C&I Loans/Total Assets		Consumer Loans/ Total Assets	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated ×Post	-0.001	-0.001	0.001	0.000	-0.000	-0.000	0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Constant	0.108***	0.091	0.106***	-0.260***	0.087***	-0.016	0.096***	0.098**
	(0.001)	(0.082)	(0.001)	(0.064)	(0.001)	(0.051)	(0.001)	(0.045)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	30,134	30,090	30,134	30,090	30,134	30,090	30,134	30,090
Adjusted R^2	0.04	0.05	0.20	0.23	0.04	0.05	0.00	0.01