Black Box, Greenleaf: Lender Behavior Under Uncertain Collateral Enforcement*

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Abstract

This paper uses a shock to downside risk in contract enforcement to study lender behavior when collateral enforcement becomes uncertain. I exploit quasi-experimental variation from Maine's 2014 Greenleaf judgment that increased enforcement costs on existing creditors but mechanically left new lenders unaffected. I estimate that the most exposed banks restricted lending by 21%, almost exclusively for portfolio loans. I provide evidence that this contraction in credit was not driven by balance sheet losses or capital flight on the part of investors. Exploring the mechanism, I find that banks likely tightened lending standards following the judgment. Consequently, exposed banks issued safer loans and denied high debt-to-income loan applications above the conforming loan limit. Furthermore, the salience of the shock increased in proximity to the judgment, size of the bank, and bank portfolio concentration. Lastly, following the judgment, I identify spillovers to other bank operations and the local economy. Small business lending increased in bank branch localities, house prices fell in the most exposed zip codes, and exposed counties experienced an increase in unemployment.

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1 Introduction

Protecting the security interest of creditors is fundamental to a well functioning lending market. Both theoretically and empirically, the literature has proven that creditors will limit access to finance when collateral enforcement becomes costly (Pence (2006), Rampini and Viswanathan (2010)). Less explored, however, is how lenders respond to changes in confidence over the state's commitment to protect a creditor's security interest. This commitment is often compromised during a crisis, where policy makers are weighing between creditor rights and borrower protection. In theoretical work, Simsek (2013) and Geanakoplos (2010) prove that such uncertainty could precipitate a credit crunch. They argue that lenders demand collateral to insure against all states of the world, which means that loan terms should anchor themselves to the worst-case scenario to guarantee such protection. Therefore, news of low-probability disaster, such as the breakdown of contract enforcement, should significantly restrict the credit supply.

Given this theoretical framework, the current study aims to identify the causal effect of contract enforceability on the credit supply. The perception of commitment to collateral enforcement changes sharply during crisis when a foreclosure moratorium may be imminent. However, this risk also evolves over the business cycle. For example, the risk of a foreclosure moratorium may increase due to fear of a recession, an election, or a neighboring state's policies. Therefore, identifying the effect of this channel on the credit supply proves challenging because perceived enforceability covaries with asset prices, employment, income, and general uncertainty.

In order to address the challenges mentioned above, I use a difference-in-differences identification strategy that exploits the timing of a 2014 Maine Supreme Court case (the Greenleaf judgment) and cross-sectional variation in exposure to the judgment. The judgment increased foreclosure costs upon creditors holding mortgage contracts originated with the Mortgage Electronic Registration Systems (MERS). MERS is a private electronic registry that maintains a record of mortgage ownership and acts as a nominee to assign mortgages on behalf of creditors. Due to an unexpected interpretation of a single sentence within the MERS mortgage contract, the Greenleaf judgment required affected creditors to request a reissue of the original mortgage contract by the original lender. Only then would creditors of MERS loans have legal standing to foreclose.

While this imposed significant costs on creditors holding loans from the secondary market, banks issuing new loans could easily meet the requirement of the judgment by simply editing the problematic sentence. However, the unorthodox interpretation of a subtle sentence in the MERS mortgage contract likely broke the confidence that lenders held in their ability to recover collateral. Perhaps foreclosure court would use other sentences in mortgage contracts to dismiss foreclosure cases. What previously seemed like a highly unlikely scenario now became a serious possibility for banks.

The Greenleaf judgment provides an appealing empirical setting to help identify the causal effect of enforcement uncertainty. First, the judgment was unanticipated, hinging upon a subtle variation in a single sentence unrelated to local conditions. Furthermore, a third of Maine's outstanding mortgages issued prior to 2008 were affected. Therefore, the judgment served as a plausibly exogenous and significant shock to perceived contract enforceability. Second, banks issuing new loans were virtually unaffected by the judgment because they could easily change the problematic sentence in their mortgage contract. Therefore, the judgment likely affected banks via an information channel. Finally, the judgment occurred well after the financial crisis, thereby providing clean estimates to identify the effect of changes in perceived collateral enforceability.

I hand collect MERS identifying information from the Maine Registry of Deeds. I impute MERS loan status within data from the Home Mortgage Disclosure Act by using loan size, lender name, census tract, and loan type. Doing so, I identify banks exposed to the Greenleaf judgment through Maine MERS lending as a share of total New England lending. The advantage of my research design is that I compare lending between exposed and unexposed banks in localities outside of Maine. Comparing lending outcomes before and after the Greenleaf judgment in localities outside of Maine, I restrict identifying variation to an information channel driven by bank exposure. Since the consequences of the Greenleaf judgment are plausibly less salient for unexposed banks operating outside of Maine, differences in lending across treated and untreated banks should identify the effect of an increase in perceived enforcement risk.

Using a difference-in-differences design, I find that above median exposure to the Greenleaf judgment leads to a 21% reduction in tract-level lending. Furthermore, I find that this reduction is driven entirely by changes in portfolio lending. Accounting for time-varying shocks common across treated and untreated bank lending, I find that a one standard deviation increase in exposure for observationally identical banks leads to a 37% drop in portfolio lending following the judgment. In contrast, changes in non-portfolio lending are small and statistically insignificant. These results suggest that an increase in enforcement risk causes banks to exclusively reduce lending on those loans that they are liable to enforce.

In order to provide evidence of an information channel, I investigate several possible alternative explanations. First, I demonstrate that exposed banks are not more likely to experience balance sheet shocks in the wake of the Greenleaf judgment. If anything, exposed banks are more likely to see reduced losses following the Greenleaf judgment, plausibly due to tightening lending standards. Second, I find no evidence that outside investors restricted capital to banks operating in Maine, the epicenter of the Greenleaf judgment. Namely, I find statistically insignificant changes in securitized bank lending in Maine. These results suggest that bank lending did not decline as a response to financial constraints following the Greenleaf judgment. Finally, I find that treated non-banks experienced no decline in lending precisely because they operate in non-portfolio lending, confirming that the drop in portfolio lending is not due to an overall reduction in lending. In fact, exposed non-banks sort into similar localities as exposed banks, therefore picking up the slack in credit supply when banks reduce lending. Above median exposure to the Greenleaf judgment leads non-banks to increase lending by 24% following the Greenleaf judgment.

Exploring the broader mechanism, I study several distinct channels driving my results. First, I exploit institutional details related to MERS lending to provide a sharp test of the primary mechanism. MERS as an entity facilitates secondary market transaction by eliminating the need to register a mortgage after each loan sale. Therefore, loans registered as MERS loans are intended for distribution and not retention. If exposed banks are concerned with collateral enforcement risk, then the effect of the Greenleaf judgment should be concentrated on non-MERS lending since MERS loans are explicitly intended for sale. Along these lines, I find that exposed banks exclusively reduce loan size for non-MERS loans. This is a striking result given the role of MERS in the Greenleaf ruling, whereby 25% of loans affiliated with the third-party registry were rendered unenforceable (Ahsin (2021)). Nonetheless, banks did not reduce MERS lending following the judgment, plausibly due to their intention to sell those very loans on the secondary market and remove any collateral enforcement risk associated with MERS.

Second, I test for changes in lending standards across exposed and unexposed banks. I find that exposed banks reduce loan size for portfolio loans, even after accounting for time-varying shocks within a given census-tract and within a given bank. The advantage of this test is that the loan-level micro-data allows me to account for unobserved time-varying bank balance sheets shocks, thereby corroborating the hypothesis that losses are likely not driving my results. I find that banks with above median exposure reduce portfolio loan size by 21% following the judgment even after accounting for common shocks across loan types. In addition to loan size, I find that exposed banks likely improved their overall borrower quality. A one standard deviation increase in county-level exposure led to a 0.256 percentage point drop in county-level delinquency rates. This result is corroborated by changes in exposed bank denial rates. I estimate that banks with above median exposure increased loan denial rates for non-conforming loans by 5.9%. This result is driven entirely by mortgage applications with a high loan-to-income ratio, whereby the point estimate is roughly twice as large as the overall effect. Taken together, my evidence suggests that exposed banks tightened lending standards in order to restrict portfolio lending following an increase in collateral enforcement risk.

Third, I study how geographic distance affects the transmission of a shock to collateral enforcement risk. Exposed banks may assume that adjacent states are more likely to follow one another in collateral enforcement than states farther away. I find that one standard deviation of exposure leads to a 16% differentially larger reduction in portfolio lending for nearby census tracts relative to all other localities. Restricting identifying variation to the same bank-year, I demonstrate that this result is not mechanically driven by sorting. Extending these results, I present graphical evidence that the change in portfolio lending monotonically decreases in distance, whereas the effect on non-portfolio lending is statistically insignificant and homogeneous across distance.

Finally, I demonstrate that the effect is driven by large and portfolio-focused banks. Splitting the sample along total assets, large banks with above median exposure reduce overall lending by 19%. In contrast, small banks with above median exposure increase overall lending by 24%. This result is qualitatively similar to that of non-banks, whereby exposed small banks are mechanically sorted into similar localities as exposed large banks, thereby picking up slack in credit supply. Importantly, portfolio lending exclusively falls for large banks and remains unchanged for small banks. Splitting the sample along share of volume in portfolio loans, I find that portfolio-focused banks see the largest drop in lending. A one standard deviation increase in exposure leads portfolio-focused banks to drop overall lending by 35%, driven by portfolio lending in particular. In contrast, securitization-focused banks experience no statistically significant change in lending. These results do not follow traditional dynamics following a balance sheet shock because the treatment is affecting perception of collateral and not realized losses.

I conclude by studying spillover effects of increased enforcement risk on bank operations and the local economy. I find that above median tract-level exposure leads to a 5% increase in small business lending to large loan recipients following the Greenleaf judgment. In contrast, the market for small loans experiences no meaningful change in lending. Furthermore, this effect is concentrated on localities with bank branch deposits. These results suggest that banks reallocate credit from collateral with risky enforceability to collateral with less enforcement risk. Turning to the local economy, I find that a standard deviation of exposure leads to a 1% drop in zip-code level house prices and a 0.437 percentage point increase in the unemployment rate. This result is qualitatively similar to previous work demonstrating the tight link between mortgage credit, house prices, and employment.

Related Literature

I contribute to the literature on expectations and bank lending in several ways. First, I provide causal evidence on the importance of the informational channel in bank lending, as it relates to collateral enforcement risk. In particular, I find that uncertainty over enforcing collateral rights fundamentally determines the credit supply. Second, my research complements established facts related to balance sheet shocks by studying a shock to collateral enforcement risk. I find that this type of shock affects larger banks, non-securitizing lenders, and geographically nearby subsidiaries. Furthermore, credit is exclusively reduced for loans that are liable to enforcement risk and reallocated to other bank operations.

My research contributes to the broad empirical literature on lender experiences and credit provision. Research has shown that lender experiences are fundamental to the credit supply, affecting loan provision, perception of risk, capitalization, and covenant strictness (Ma (2015), Cheng et al. (2014), Bouwman and Malmendier (2015), Murfin (2012)). Furthermore, banks facing uncertainty reduce lending and increase capital surpluses (Kara and Yook (2019), Eckley et al. (2019), Gissler et al. (2016)).

Studying downside risk in particular, Ma et al. (2021) find that banks with worse expectations over unemployment and house prices experience weaker firm-level loan growth in the subsequent year. Surprisingly, this effect is salient exclusively for a severe stress test scenario and not baseline conditions. In related work studying downside risk, Capponi et al. (2022) find that tail risk better explains the large margins in the CDS market when compared to the standard value-at-risk models. Capponi et al. (2022) further build on this by developing a general equilibrium model to ground these results.

Causally identifying the effect of beliefs is empirically challenging due to the tight link between lender beliefs and balance sheets. To overcome this empirical challenge, Carvalho et al. (2021) isolate variation from local house price growth to identify the effect of loan officer beliefs on credit issuance. They find that loan officers price risk more generously when experiencing plausibly exogenous house price growth. Most closely related to the current paper, Koudijs and Voth (2016) test the implications of Simsek (2013) and Geanakoplos (2010) by demonstrating that rare disasters can causally affect the pessimism of lenders and therefore increase collateral requirements. Studying the 1772 fallout of an investor syndicate's bankruptcy, Koudijs and Voth (2016) find that, although lenders experienced no losses from investor bankruptcy, margins nonetheless increased.

Second, my research contributes to the literature on foreclosure risk and the mortgage credit supply. Using a regression discontinuity framework, Pence (2006) causally identifies the effect of foreclosure laws on loan size. Dagher and Sun (2016) build on this work and find that foreclosure laws also affect mortgage denial rates. Bongaerts et al. (2021) further find that banks lend more in localities where foreclosure externalities are better internalized (either by the bank itself or other market players) and where the likelihood of own-bank foreclosure risk does not covary with that of other market participants.

Most closely related to this paper, Huo et al. (2021) identify the effect of higher perceived foreclosure risk on lending standards. They find that lenders operating near a county court are more likely to reject mortgage applications and extend less credit due to exposure to foreclosure auctions. Controlling for local conditions via time varying fixed effects, these results are concentrated on loans associated with high debt-to-income ratios, negative house price growth, and small banks. Furthermore, these results are only salient during periods of high foreclosure and when foreclosure auctions are held outside.

Third, this paper relates to the large literature on internal capital markets. Within this literature, many studies have found that changes in collateral value, financial constraints, and agency costs can affect the credit supply through bank networks. For example, Loutskina and Strahan (2015) study the effect of credit flows via collateral on local economic conditions. They find that plausibly exogenous changes in loan supply increase house prices, particularly for localities well integrated via the secondary mortgage market. Chakraborty et al. (2018) further find that these changes in collateral value can affect other asset classes in the form of credit flowing from firms to households. Furthermore, Gilje et al. (2016) find that banks exposed to the shale boom via deposit growth extended new credit in the form of portfolio loans and in markets with branch presence. In complementary work, Cortés and Strahan (2017) identify the effect of a negative liquidity shock on bank lending. They measure how lending changes in regions unaffected by natural disasters due to exposure to disaster lending. They find that banks reduced overall lending, while protecting core markets with significant ex-ante market share. Finally, Rehbein and Ongena (2022) find that these effects are concentrated on weakly capitalized banks, implying that low bank capital can generate financing frictions.

Related to contract enforcement in particular, several papers have studied how regulation, supervision, and tax policy can transmit shocks through internal capital markets. Ongena et al. (2013) present evidence on how home-country bank regulation can influence lending standards abroad. Using a cross-sectional survey of firm credit constraints matched to bank subsidiaries, the authors find that home-country competition and lending restrictions lead intermediaries to lower lending standards abroad for opaque firms. Houston et al. (2012) find that capital inflows are positively related to creditor rights, property rights, and contract enforcement. Smolyansky (2019) demonstrates that firm exposure to rising taxes leads to an increase in a firm's credit supply, employment, and income via the bank lending channel.

These effects are robust to bank, county, and industry level regressions, as well as local credit demand.

Most related to the current paper, Fazio and Silva (2021) study the effect of stronger creditor rights on the distribution of credit. The authors find that Brazil's 2004 collateral reform dramatically strengthened bank security interest in real estate collateral and led to an increase in credit to high homeownership localities. While this led to more lending, lower interest rates, and beneficial real economic effects for high homeownership regions, localities with low homeownership experienced the opposite due to a reallocation of credit and limited collateral.

The remainder of this paper proceeds as follows. Section 2 provides institutional details on MERS and the Maine Greenleaf judgment. Section 3 details my data, sample construction, and summary statistics. Section 4 estimates the effect of the Greenleaf judgment on bank lending. Section 5 explores alternative explanations. Section 6 investigates the mechanism driving the reduced lending. Section 7 studies spillovers into alternative bank operations and the local economy. Section 8 concludes.

2 Institutional Background

2.1 MERS

An underappreciated feature of the US real estate market is that the colloquial term "mortgage" actually refers to two contracts. The first contract is a loan or a promissory note which represents a borrower's promise to repay the owner of the promissory note. The second contract is a mortgage contract securitizing the promissory note. In the case of default, the mortgage contract guarantees that the creditor will recover the loan's associated collateral, which in this case would be the borrower's house.

When a lender originates a loan, the lender physically holds the promissory note and thereafter publicly records the mortgage contract with the registry of deeds. If the lender chooses to sell the mortgage loan, they will sell both the promissory note and the mortgage contract together. Upon a sale, the promissory note must physically change hands while the mortgage contract must be publicly assigned to the new creditor in the registry of deeds.

With the advent of large securitization trusts, investors who pooled millions of mortgages together found it costly to process each individual mortgage assignment given the scale of their operations and how often mortgages were traded. As a solution, in the 1990's, the largest financial institutions at the time created the Mortgage Electronic Registration Systems (MERS). MERS essentially privatized the chain of title by serving as a nominee for mortgage assignment. Following the creation of MERS, the promissory note continued to physically change hands between creditors upon a loan's sale. However, the mortgage contract was now assigned to MERS at origination and remained with MERS throughout the life of the loan. Only upon repayment, foreclosure, or the request of the creditor would MERS need to visit the county registry of deeds again to publicly declare a change in the chain of title.

In its most streamlined form, the mortgage contract would be publicly registered once at origination and again upon full repayment. All the while countless sales would occur privately and undeclared. This represented a significant cost saving for financial intermediaries, as demonstrated in work by Lewellen and Williams (2021). They find that the introduction of MERS was associated with a 10% increase in origination volume and a 4% increase in loan approval. They also document that 20% of the total credit supply increase observed by Mian and Sufi (2009).

2.2 The Greenleaf judgment

Across all of the United States and for almost two decades, a mortgage contract affiliated with MERS has included a declaratory sentence defining the role of MERS as a nominee for mortgage assignment. Subtle variations in this declaratory sentence across contracts, states, and time bore virtually no weight on the goal of defining the clear responsibility entrusted to MERS. Figure 1 provides examples of these declaratory sentences within the mortgage contracts of New Hampshire and Maine. In Panel A, the New Hampshire MERS contract states: "MERS is the mortgagee under this Security Instrument". In Panel B, the key sentence within Maine's contract states: "For purposes of recording this mortgage, MERS is the mortgagee of record". In plain language, these two sentences appear identical in that they identify MERS as the mortgagee. However, on July 3, 2014, the Maine Supreme Court ruled in Bank of America vs. Greenleaf that the declaratory sentence used for Maine's MERS mortgage contracts did not grant MERS the right to assign the mortgage contract. Rather, the sentence implied that MERS only held the right to record a mortgage assignment.

Prior to the judgment, if a creditor chose to initiate foreclosure proceedings, they would request that MERS assign the mortgage in the creditor's name. This would likely be the first time that the mortgage contract was assigned in the registry of deeds to a mortgagee other than MERS or the original lender. Once the creditor reunited the mortgage contract with its associated promissory note, only then would a creditor stand to foreclose. Due to the judgment, however, the Court deemed that MERS did not have the right to assign the mortgage contract based on the literal meaning of the key sentence. Within a single day, the Court's judgment implied that a third of Maine's mortgages contracts from prior to the 2008 financial crisis never transferred to MERS, even though current creditors held the associated promissory note.

In order to resolve this issue for creditors holding MERS loans, the Greenleaf judgment required that a current creditor obtain a reissue of the mortgage contract from the only other party with the right to assign the mortgage—the original lender. Namely, the current creditor would have to request that the original lender assign the mortgage contract in the current creditor's name through the county registry of deeds, a responsibility previously held by MERS. Ahsin (2021) demonstrates that this proved costly, both in non-pecuniary terms via wait time and search costs, but also in the form of legal fees. Ahsin (2021) finds that creditors faced a 25% drop in foreclosures and managed to sell only about half of the delinquent mortgages that they would have otherwise foreclosed on. The market for mortgage sale broke down for loans that were prohibitively expensive to foreclose, forcing creditors to offer forbearance to this subset of loans.

In sharp contrast, new loans could easily resolve the issue introduced by the Greenleaf judgment. Lenders originating new loans following the judgment could simply change a handful of words in their mortgage contract to grant MERS the right to assign the mortgage instead of the right to record. Alternatively, the lender could register a separate assignment to MERS in the registry of deeds. Ultimately, simple steps rendered the judgment virtually costless for originating new loans.

3 Data

3.1 Home Mortgage Disclosure Act

The Home Mortgage Disclosure Act requires that the near universe of mortgage lenders in the US report each mortgage application's loan, property, and borrower characteristics to regulators. There are few lenders exempt from this rule based on size, location, and loan volume. Loan characteristics include loan size, type, purchaser, lien status, high interest rate indicator, lender identifier, and action taken. Borrower characteristics include income, race, ethnicity, and gender. Property characteristics include property type, occupancy status, state, county, and census tract. I use a crosswalk maintained by Robert Avery to identify parent companies associated with a given subsidiary so that analysis is at the bank level[1].

3.2 Bank Data

I collect bank data from the Consolidated Report of Condition and Income (Call Reports) provided by Wharton Research Data Services (Drechsler et al. (2021)). I use data from the

[1] Available upon request at Robert.Avery@fhfa.gov.

fourth quarter of 2009 to the fourth quarter of 2016. The Call Reports provide quarterly data on income and balance sheet variables for all U.S. commercial banks. This data includes total assets, total deposits, cash holdings, federal funds purchases, trading securities, total equity capital, commercial loans, real estate loans, core deposits, and the interest expense on core deposits. The Call Reports also include information on charge-offs, non-performing loans, and loan loss funds. In a given event year, I associate a bank with its previous year's fourth quarter balance sheet position.

3.3 MERS Public Registry Data

I use mortgage contract data from the Maine public registry of deeds, available at the Maine Registry of Deeds website. Data is available for each mortgage originated over my sample period across all 16 counties. When a mortgage loan is originated, the mortgage contract is publicly registered at the county registry of deeds in order to preserve its chain of title. Each additional sale of the mortgage loan requires that the associated securitizing contract be publicly reassigned to the new creditor as well. In the case of a MERS mortgage, within the very originating documents themselves, the original lender immediately assigns the mortgage to MERS. Hence, MERS is listed alongside the original lender in the public registry for each MERS affiliated loan. For each mortgage contract, I record the book and page number, the borrower's name, the address, the lender, the date of origination, the date of termination, the loan amount, and the MERS ID.

3.4 Other Data Sources

In order to identify MERS loans in the HMDA dataset, I supplement the Public Registry data with the CoreLogic Deeds data. The CoreLogic Deeds data provides information on home purchases and mortgage transactions through deed-level recorder and assessor data. The dataset includes information on borrower name, sale amount, mortgage amount, mortgage due date, mortgage interest rate, and lender name. I collect data from the Bureau of Economic Analysis containing annual measures of county level GDP, the employment population, the total population, and aggregate income. For annual county level unemployment, I use the unemployment rate provided by the Bureau of Labor Statistics. I also use county- and zip-level HPI data from the Federal Housing Finance Agency. Finally, to extend my analysis to commercial credit, I collect data on annual small business lending at the census-tract level through the Community Reinvestment Act. I supplement this data with branch level deposits provided by the FDIC.

3.5 Dataset Construction and Summary Statistics

I merge CoreLogic Deeds data with MERS Public Registry Data in several steps using one-to-one matches. I first merge on deeds book number, deeds page number, and county. With remaining unmatched records, I merged on county, note date, loan amount, and zip. I next add year end and month end separately for greater precision. In order to account for unmatched loans and loans where the end dates are not available, I merge on borrower name. I infer census tracts based on CoreLogic Deeds latitude and longitude where available. Otherwise, I rely upon a zip-tract crosswalk available at Department of Housing and Urban Development.

I merge HMDA data to the intermediate dataset using one-to-one matches on year of origination, county, census tract, loan amount, original lender name, purchase status, and conventional loan status. I iterate on this process by removing precision as loans are eliminated from both datasets. Namely, I merge unmatched observation on one-to-one combinations of all previous variables excluding conventional status. I then exclude purchase status, as well. I loop through this process again, except with a shortened lender name, removing extraneous terms and using first eight characters. Finally, this entire procedure is repeated for any remaining unmatched observations based on CoreLogic Deeds provided lender names. I match Call Reports data and county-level economic variables to associated lenders and counties. Income and balance sheet data is aggregated to the parent company level using the Avery crosswalk.

Table 1 reports summary statistics for bank balance sheet and lending characteristics. My main sample includes banks with loans originated in New England (excluding Maine), a balance less than \$2 million, and a loan-to-income ratio less than 8. My sample comprises of 349 unexposed banks and 85 banks with some exposure to Maine's Greenleaf judgment. I define exposure as:

$$\omega_b = \left(\frac{Maine\ MERS\ Volume}{New\ England\ Volume} \right)_b \tag{1}$$

Here, exposure ω_b measures total Maine MERS loan volume as a share of total loan originations across New England for a given bank b in the year prior to the 2014 Greenleaf judgment. Figure 2 plots exposure across the New England area. Mechanically, exposure is larger in localities closest to the Maine Greenleaf judgment.

To test for any systematic differences in bank balance sheets across bank exposure, Figure 3 plots standardized estimates form a regression of balance sheet variables on bank exposure to the Greenleaf judgment. A one standard deviation increase in exposure appears not to be a statistically significant predictor of bank balance sheet variables, except for total assets. Exposed banks appear to be relatively larger than unexposed banks. This relationship seems mechanical in that banks with exposure to Maine MERS lending by definition must be interstate lenders to appear in the New England sample. Small banks, in contrast, may appear in my sample with no exposure to the Greenleaf judgment precisely because these banks have no exposure beyond their particular locality. Section 6.4.1 accounts for this explicitly by re-estimating my results across bank size.

4 Main Results

4.1 Empirical Design

Identifying the causal effect of contract enforceability on lending is empirically challenging. Expectations over collateral enforcement are correlated with changes in asset prices or delinquency rates, especially during times of economic crisis. An ideal experiment would test the lending outcomes of two intermediaries, equivalent along every dimension except for their weight on the downside risk of contract enforcement. In this experiment, one bank would be treated with perceiving a greater probability of some rare breakdown of collateral enforcement. Such an experiment would be a direct test of the effect of heightened downside risk discussed in Simsek (2013) and Geanakoplos (2010).

In order to approximate this experiment, I exploit the timing of the Greenleaf judgment and cross-sectional variation in exposure to the judgment across banks. With regards to the first source of variation, as noted earlier, the Greenleaf judgment was unexpected and unrelated to local conditions, therefore serving as a plausibly exogenous shock. The direct effect of the shock was large, whereby the foreclosure rate fell by 25% (Ahsin (2021)). Moreover, the shock was explicitly related to a breakdown in enforceability, hence providing greater precision in attributing the shock to news about enforceability as opposed to general uncertainty. Finally, bank lenders issuing new mortgages mechanically experienced the shock through an information channel as opposed to a material breakdown in enforcement. A new mortgage could easily account for the Greenleaf judgment by changing a single sentence in the standard MERS mortgage contract. Hence, the effect of the shock on new loans was plausibly isolated to a jump in the perceived downside risk of contract enforcement.

My second source of variation is cross-sectional. The salience of the shock likely varied based on bank exposure to Maine MERS lending. Research by Malmendier and Nagel (2015) suggests that individuals overweight personal experiences in forming expectations. Therefore, a bank with a higher share of total lending conducted in Maine MERS loans would likely lose greater confidence in enforceability than banks with less exposure to the market. This assumption seems reasonable given that the judgment received little coverage and was not an intentional policy that perhaps less exposed bank lenders would be aware of. Ahsin (2021) demonstrates that the judgment had virtually no effect on any measurable estimate of strategic default, suggesting that even affected borrowers were plausibly unaware of the judgment.

Finally, I focus on lending in localities outside of Maine in order to restrict identification to exclusively the time-series and cross-sectional variation discussed above. Doing so has several advantages. First, I abstract from any direct consequences of the Greenleaf judgment on Maine borrowers. Ahsin (2021) demonstrates that the Greenleaf judgment led to a drop in foreclosures, an increase in borrowers reducing their delinquency status, and creditor provision of forbearance (Ahsin (2021)). Such outcomes should affect borrower wealth, asset prices, and, ultimately, demand for credit. Second, by studying lending outside of Maine, I restrict attention to an information channel driven by bank level exposure. Within Maine, it is likely that local banks became aware of the Greenleaf judgment simply because of its dramatic consequences across the state. Outside of Maine, it becomes less likely that a remote bank lender in Massachusetts or Connecticut should be aware of such a nuanced and unadvertised judgment, if not for exposure to Maine MERS lending.

To estimate the effect of a shock to downside enforcement risk on bank lending, I run ordinary least squares (OLS) regressions using the bank-tract-year panel. I estimate a difference-in-differences regression of lending volume:

$$Y_{c,b,t} = \alpha + X'_{c,b,t}\gamma + \delta D_b \times Post_t + \epsilon_{c,b,t} \tag{2}$$

In the above specification, $Y_{c,b,t}$ is the logarithm of loan volume made by bank b in event year t and census tract c. The $Post_t$ dummy takes a value of one if bank lending is observed in or after the year of the 2014 judgment. $X_{c,b,t}$ is a vector of bank and regional characteristics. I control for borrower characteristics at the bank-tract-year level, namely share of loans issued to female borrowers, black borrowers, one- to four-family homes, conventional mortgages, and primary residences. I also account for the average loan-to-income ratio for a given bank-tract-year combination. I control for bank balance sheet variables, namely deposits, liquid assets, tangible equity, commercial loan volume, real estate loan volume, and net income, all scaled by total assets. In addition, I include log of total assets and the core deposit rate determined by the interest expense on core deposits divided by core deposits. I include time varying county-year characteristics, namely the employment to population ratio, the log of GDP, the year-end unemployment rate, and the log of aggregate income. Depending on the specification, the variable D_b represents either a discrete or continuous measure of exposure, $Treat_b$ and $Exposure_b$, respectively. The continuous variable $Exposure_b$ measures bank lender exposure to the Greenleaf judgment as defined in Equation (1). This variable is standardized so as to interpret a value of one as a one standard deviation increase in exposure relative to the mean. The dummy variable $Treat_b$ equals one if the bank had above median non-zero exposure to 2013 Maine MERS lending.

All regressions include fixed effects for the year of observation, the census tract, and the bank holding company. I further include bank-tract fixed effects to account for bank sorting into particular census tracts. Finally, I account for local demand by including tract-year fixed effects. This also accounts for changes in local economic conditions, such as house prices and unemployment, that would affect bank lending. Standard errors are clustered at the bank level to account for within-bank residual correlation.

The coefficient of interest is δ, which measures the differential change in lending among exposed banks (treated banks) relative to unexposed banks (control banks) following the Greenleaf judgment (treatment). I identify δ by exploiting the timing of the Greenleaf judgment, using within-tract, -bank, and -year variation in lending volume over time, across exposed and unexposed banks. I further restrict the identifying variation using bank-tract fixed effects and time-varying tract fixed effects. The key identifying assumption is that, conditional on observables, lending by treated and untreated banks would have evolved in parallel if not for an exogenous increase in downside enforcement risk due to treatment (the Greenleaf judgment).

My research design provides support for this assumption in several ways. First, my main specification addresses concerns related to changes in lending opportunities available to treated banks due to unobserved heterogeneity. For example, including bank-tract fixed effects, I control for local lending relationships and sorting into particular localities. Furthermore, restricting identification to variation within a given tract-year, I account for factors affecting credit demand that could be correlated with the timing of the judgment. Second, I address concerns over reverse causality by outlining the institutional details related to the judgment. Namely, the judgment was based on a subtle variation in a single sentence within a standard mortgage contract, unrelated to local economic conditions. Third, to assess the degree to which lending by treated and untreated banks would have differed systematically absent the Greenleaf judgment, I replace the $Post_t$ variable in the specification above with indicator variables for three years before and four years after the baseline year preceding the judgment:

$$Y_{c,b,t} = \alpha + X'_{c,b,t}\gamma + \sum_{s=-3}^{4} \delta_s D_b \times \{t = s\} + \epsilon_{c,b,t} \tag{3}$$

In the above specification, the baseline year, 2013, is the omitted category. Therefore, δ_s is interpreted as the differential change in lending by treated banks relative to the baseline year. To test for common pre-trends, δ_s should be statistically indifferent from zero for years prior to the judgment. If lending by treated and untreated banks evolved in parallel, then deviations from zero in the years following the judgment can plausibly be interpreted as a consequence of increased enforcement risk rather than anything systematically different across exposure.

4.2 Bank Lending

Table 2 presents the estimates of regression Equation (2) where the dependent variable is the logarithm of lending at the bank-tract-year level. In all specifications, I control for bank and time-varying county-level controls. Column (1) includes fixed effects for census tract, bank, and year of observation. Doing so allows me to control for any macroeconomic conditions and unobservable time-invariant heterogeneity. I find that above median treated banks reduced lending by 18%, significant at the 5% level.[2]

In Columns (2) and (3), I increasingly restrict the identifying variation to account for unobserved heterogeneity. Column (2) includes bank-tract fixed effects to control for bank sorting and lending opportunities. The magnitude of the effect increases to 22%, statistically significant at the 1% level. Column (3) further restricts identification to a given census tract and year pair by incorporating tract-year fixed effects. Treated banks may be concentrated in census tracts experiencing lower house prices and employment precisely in the year of the judgment. To account for this, I control for changes in local economic conditions common across treated and control banks. The size of the effect is stable, equaling 21%, statistically significant at the 5% level.

Columns (4) to (6) repeat the preceding analysis using a continuous measure of exposure. Here, the exposure variable is standardized so that the coefficient can be interpreted as the effect of a one standard deviation increase in exposure to treatment. The results generally mirror those in Columns (1) to (3). The effect is negative across all columns and statistically different from zero at the 5% level in all but the least restrictive specification. Under the most restrictive specification, a one standard deviation in exposure leads banks to reduce lending by 16%.

4.3 Bank Lending Across Loan Type

While these results demonstrate that treated banks restricted lending in the period following the Greenleaf judgment, they may be underestimating the effect due to heterogeneity in the response of treated banks to the Greenleaf judgment. The presence of a secondary

[2] I follow DeFusco (2018) and Kennedy (1981) to convert log points into an implied percentage increase using the point estimate and its standard error, namely $\%\triangle = 100 \times [exp(\delta - 0.5 \times \sigma_{\delta}^2) - 1]$.

market facilitates the ability to offload default risk to outside investors. Therefore, a shock to enforcement should be most salient for portfolio lending, as it leaves banks exposed to downside enforcement risk. Furthermore, it is unclear whether banks expand securitized lending to replace portfolio lending or if banks simply restrict lending in response to increased enforcement risk. To test these hypotheses and understand the direction of credit growth, I estimate my main specification using a split sample of portfolio and non-portfolio lending.

Table 3 tests whether treated bank lenders respond to the increased enforcement risk asymmetrically across lending type. I mirror Table 2 and estimate regression Equation (2) separately for portfolio loans and non-portfolio loans. Panel A of Table 3 presents results where the dependent variable is the logarithm of portfolio lending at the bank-tract-year level. As before, Columns (1) to (3) report estimates using a discrete measure of exposure. In Column (1), I find that an above median exposure to the Greenleaf judgment decreases portfolio lending by 32%, statistically significant at the 10% level. In Columns (2) and (3), I find a stronger decline of 40% and 41%, respectively. These estimates are both significantly different from zero at the 5% level. corroborating these results using continuous measure of exposure, Columns (4) to (6) report large, negative, and statistically significant estimates. Under the most restrictive specification, I estimate that a one standard deviation increase in exposure to higher collateral enforcement risk leads to a 37% decline in portfolio lending, significantly different from zero at the 1% level.

Panel B of Table 3 presents results where the dependent variable is the logarithm of non-portfolio lending at the bank-tract-year level. Across all specifications, I find point estimates that are small, positive, and statistically insignificant. Using the discrete measure and under the most restrictive specification, the effect is estimated to be one percentage point. These results suggest that the effect of higher collateral enforcement risk is only salient for loans retained on bank balance sheets, not for loans originated to sell on the secondary market. If the treated group experiences an increase in enforcement risk, then these banks will reduce lending precisely for those loans that they are liable to enforce.

Figure 4 compares the effect of the Greenleaf judgment on portfolio lending across treated and control banks for three years prior to the baseline year and four years following the baseline year. The figure plots estimates and 95% confidence intervals from a flexible difference-in-difference specification given by Equation 3 using the same set of controls as in Column (6) of Table 3. Estimates from the baseline year (2013) are normalized to zero so that lending is compared relative to the year prior to the Greenleaf judgment. In Figure 4, treated and control banks evolved portfolio lending roughly in parallel prior the Greenleaf judgment. However, treated banks experienced a sharp and statistically significant decline in portfolio lending over the four years following the baseline year. Relative the baseline year, treated banks reduced portfolio lending by almost 30% in the first year alone. In contrast, in Figure 5, non-portfolio lending is statistically insignificant from zero for the years prior to and following the Greenleaf judgment.

4.4 Aggregate Lending

While exposed banks may restrict lending in response to a shock to enforcement risk, it is unclear whether unexposed banks are able to make up for the credit shortfall. Alternatively, the contraction of portfolio lending by treated banks may spillover into aggregate securitized lending by way of consumer demand. Namely, a contraction of credit should lead to reduced household liquidity, expenditures, and home purchase. Hence, reduced portfolio lending should affect credit demand for non-portfolio loans as local economic conditions deteriorate. This section presents estimates of the effect of increased enforcement risk on aggregate lending.

I define aggregate exposure as the average bank-level exposure, weighted by 2013 market share. More formally, exposure at the census-tract level is given by:

$$\omega_c = \frac{\sum_{b=1}^{N} \omega_b \times volume_{c,b}}{\sum_{b=1}^{N} volume_{c,b}} \tag{4}$$

Here, ω_c measures aggregate exposure for census-tract c, ω_b is exposure for a given bank b, and $volume_{c,b}$ is total lending volume by bank b in census-tract c. I aggregate control variables using Equation (4), replacing ω_b with bank balance sheet variables or bank-tract-year lending characteristics. Doing so allows me to account for unobserved heterogeneity correlated with bank wealth and borrower composition.

Table A.1 tests whether treated census tracts experience a reduction in overall lending following an increase in enforcement risk. I estimate regression Equation (2) separately for the full sample of loans, as well as restricted samples of only portfolio loans and only non-portfolio loans. All columns include fixed effects for census tract and event year.

Columns (1) and (2) presents results where the dependent variable is the logarithm of total lending at the tract-year level. In Column (1), I find that an above median exposure to the Greenleaf judgment decreases tract-level lending by 5%, statistically significant at the 1% level. Column (2) corroborates this result using a continuous measure of exposure. I estimate that a one standard deviation increase in exposure to higher collateral enforcement risk leads to a 3% decline in tract-level lending, significantly different from zero at the 1% level.

Columns (3) and (4) present results where the dependent variable is the logarithm of portfolio lending at the tract-year level. I find that above median exposure decreases tract-level portfolio lending by 7% and a one standard deviation increase in exposure leads to a 5% decline in tract-level portfolio lending. Both estimates are statistically significant at the 1% level. Columns (5) and (6) report estimates where the dependent variable is the logarithm of non-portfolio lending at the tract-year level. In sharp contrast to the previous columns and corroborating the bank-level results, exposed census tracts experience little change in non-portfolio lending. The point estimate in Column (5) is statistically significant at the 10% level, however effects on non-portfolio lending are small and generally ambiguous.

The results in Table A.1 imply that while treated banks reduced lending significantly following the Greenleaf judgment, the aggregate effect of the higher enforcement risk was attenuated. The estimated coefficient is statistically significant and negative using both the sample of all loans as well as the sample of portfolio loans. As before, non-portfolio lending is generally unchanged. However, these results imply that the bank-level treatment effect does not translate to a more aggregated level. This is likely due to market competition, whereby control banks make up for the shortfall in credit supply. However, when banks experience and transmit enforcement risk from multiple sources during a financial crisis or economic recession, then there may be fewer banks willing to replace the gap in lending.

5 Robustness

5.1 Wealth Shock

In order to associate the changes in bank lending to an increase in collateral enforcement risk, treated banks must not be affected by the Greenleaf judgment through any other avenue but for an information channel. A threat to this assumption would be that banks exposed to Maine MERS lending suffered a wealth shock following the ruling. A wealth shock would reduce the capital that banks have at their disposal to extend credit, thereby leading to a reduction in mortgage credit.

This seems unlikely for several reasons. First, as mentioned previously, current bank lenders were mechanically unaffected by the judgment because the ruling affected loans that were held by precisely those creditors that did not originate the loan. Loans affected were likely held by securitizing trusts, as MERS was constructed to facilitate secondary market sale for those very investors. Second, if banks were holding delinquent loans associated with MERS, Ahsin (2021) demonstrates that those loans self-cured and reduced delinquency. Therefore, creditors holding MERS loans were not subject to losses on their balance sheets following the judgment. Finally, even if banks suffered a loss whereby they could not finance new credit, then this would have translated into at least some reduced non-portfolio lending.

The fact that exclusively loans associated with enforcement risk saw a contraction in credit suggests that exposed banks were treated with heightened enforcement risk rather than anything material.

Nevertheless, I can formally test whether exposed banks experienced differentially higher charge-offs, non-performing loan losses, or an increase in loan loss funds relative to unexposed banks. Estimating the effect of the judgment on balance sheets serves as the most straightforward test of a wealth shock since it estimates the size of bank losses directly. In Table A.2, I estimate Equation (2) using the bank-quarter panel. All columns include bank balance sheet variables, as well as time and bank fixed effects.

Columns (1) and (2) present results where the dependent variable measures total charge-offs scaled by total assets for a given bank and event quarter. The outcome variable is multiplied by 100 for ease of interpretation. For this test, I examine whether exposed banks are more likely to experience charge-offs in the period after the Greenleaf judgment. Perhaps banks curtailed lending precisely when they experienced an increase in losses. I find that my scaled measure of charge-offs fell by 0.2 for above median exposure and 0.033 for a one standard deviation increase in exposure. These estimates are statistically different from zero at the 1% and 5% levels, respectively.

Columns (3) and (4) present results where the dependent variable measures total non-performing loans scaled by total assets for a given bank and event quarter. Once again, the outcome variable is multiplied by 100 for ease of interpretation. For this test, I examine whether exposed banks are more likely to experience delinquencies in the period after the Greenleaf judgment. I find that my scaled measure of nonperforming loans fell by 0.293 for above median exposure and 0.029 for a one standard deviation increase in exposure. The first estimate is statistically significant at the 1% level.

Columns (5) and (6) present results where the dependent variable measures total loan loss funds scaled by total assets for a given bank and event quarter. Once again, the outcome variable is multiplied by 100 for ease of interpretation. For this test, I examine whether exposed banks are more likely to increase their capital buffers for potential losses. Perhaps exposed banks reduced lending following the Greenleaf judgment because of expected delinquencies or losses moving forward. I find that my scaled measure of loan loss funds fell by 0.115 for above median exposure and 0.014 for a one standard deviation increase in exposure. The first estimate is statistically lower than zero at the 5% level.

Taken together these estimates reject the hypothesis that exposed banks experienced higher current and expected losses. In fact, the results in Table A.2 suggest that the banks with above median exposure experienced reduced current and expected losses relative to unexposed banks. Given the sharp decline in portfolio lending following the Greenleaf judgment, banks plausibly improved quality of their borrower pool in response to an increase in enforcement risk. I explore this mechanism further in Section 6.2.

5.2 Outside Capital

Perhaps treated banks relied upon outside investors to finance their mortgage lending. Following the Greenleaf judgment, these outside investors might have tightened capital provision by cutting loan purchases or increasing the fees associated with loan purchase. Furthermore, the prospect of losses associated with the Greenleaf judgment might have heightened the risk that investors would "put back" purchased loans to originating banks. If bank lenders and investors were concerned with loan sales due to the regulatory environment in Maine, then Maine securitized lending should drop in the aftermath of the Greenleaf judgment.

I test for this by estimating my main specification on Maine non-portfolio lending. I mirror Panel B of Table A.3 and estimate regression Equation (2), where the dependent variable is the logarithm of non-portfolio lending at the bank-tract-year level using the Maine sample. Columns (1) to (3) report estimates using a discrete measure of exposure and Columns (4) to (6) use a continuous measure. Across all specifications, the point estimates are relatively small, positive, and statistically insignificant. These results reject the hypothesis that exposed banks experienced reduced securitized lending in Maine due to capital flight following the Greenleaf judgment.

5.3 Non-Banks and Credit Unions

While the Greenleaf judgment also affected non-bank mortgage lenders and credit unions, the current research design focuses on bank lending in particular for several reasons. First, banks are highly interconnected across borders, hold large footprints in the markets that they operate in, and lend across multiple credit categories beyond just real estate finance. Therefore, understanding how different types of shocks are transmitted by banks across regions is important for financial stability merely due to the scope of bank operations. Second, banks originate loans that are either retained on their balance sheet or securitized. Therefore, understanding bank lending provides insight into the role of a secondary market in managing systemic risk.

In contrast, non-bank mortgage companies and credit unions operate under two distinct business models of financial intermediation. Non-bank mortgage companies originate loans using lines of credit offered by warehouse lenders and sell loans immediately on the secondary market. Credit unions, in contrast, originate loans using deposit funding and are obliged to retain loans on balance sheet. Only in June 2017 did the National Credit Union Association issue a safe harbor rule for lenders interested in securitizing loans. With this context in mind, a natural robustness test of my previous results would assess whether the portfolio and non-portfolio lending dynamics are reproducible in intermediaries that exclusively operate using one form of lending versus the other. Given their local nature, credit unions do not offer sufficient variation in exposure across state lines to generate meaningful results. Nonetheless, in unreported regressions, I re-estimate Panel A of Table 3 with the inclusion of unexposed credit unions as a control group and produce identical estimates to my main results. This implies that untreated credit unions did not experience a reduction in lending precisely because they were not exposed to the Greenleaf judgment.

Turning to non-bank lending, I re-estimate the regressions in Table A.4 with the lender-tract-year panel of non-banks. Table A.4 shows the estimates where the dependent variable is the logarithm of non-bank lending at the lender-tract-year level. As before, Columns (1) to (3) use a discrete measure of exposure and Columns (4) to (6) use a continuous measure. In Column (1), above median exposure to the Greenleaf judgment leads to a 15% increase in non-bank lending. This is statistically larger than zero at the 10% level. In Columns (2) and (3), the point estimates increase to 26% and 24%, respectively, both statistically larger than zero at the 5% level. In Columns (4) to (6), the point estimates are positive but only statistically larger than zero in Column (5). Taken together, the effect of the judgment on treated non-bank lending is generally unclear. However, highly exposed non-bank lenders unambiguously increased their supply of credit.

The results from Table A.4 confirm the hypothesis that non-bank lenders do not decrease their lending following an increase in collateral enforcement risk precisely because they operate exclusively under a non-portfolio lending model. However, in contrast to the estimates in Panel B of Table 3 using the sample of bank loans, I find that highly exposed non-bank lenders actually increase their lending following the Greenleaf judgment. This result does not contradict the thesis that enforcement risk leads to reduced lending when a lender is liable to enforce collateral rights. However, it is not immediately obvious why non-bank lenders will increase lending while bank lenders fail to do so.

To resolve this, I test for whether treated non-bank lenders sort into the same census tracts as treated bank lenders. Exposed lenders likely operate in similar markets since exposure is a function of how much a lender concentrates in Maine MERS lending. Therefore, non-bank lenders with above median exposure mechanically sort into the same tracts as highly exposed bank lenders. Hence, if banks reduce portfolio lending and fail to replace it with non-portfolio lending, non-bank lenders situated in the same localities are likely to pick up the slack in credit supply.

In Table A.5, I test for sorting across lender type and exposure. In Column (1), I regress the logarithm of tract-level bank exposure on the logarithm of tract-level non-bank exposure. Tract-level exposure by lender is defined as the market-share weighted exposure of a given lender type, as defined by Equation (4). I find that a 1% increase in tract-level non-bank exposure is correlated with a 0.209% increase in tract-level bank exposure. This estimate is statistically different from zero at the 1% level. In Column (2), I replace the tract-level non-bank exposure measure with a given tract's highest non-bank exposure. Here I find that a 1% increase in lead non-bank exposure is correlated with a 0.023% increase in tract-level bank exposure. This estimate is statistically different from zero at the 5% level. These results confirm the hypothesis that exposed non-banks sort into similar tracts as exposed banks. Furthermore, the estimate in Column (2) suggests that this is not due to a single non-bank, but rather something more systematic.

While the analysis above explains why non-bank lenders pick up the slack in credit supply, it does not explain why bank lenders fail to increase non-portfolio lending as well. In order to explain the absence of an effect on non-portfolio bank lending, I refer back to the difference between bank and non-bank lenders. While non-bank lenders operate exclusively under an originate-to-distribute model within the real estate market, banks have complex business operations across a variety of markets and asset classes. Given this context, perhaps bank lenders reallocate capital from mortgages to other credit classes. I formally test this channel in Section 7.1.

6 Mechanism

6.1 Retaining Risk

The general mechanism behind the preceding results relies upon the assumption that banks are curtailing portfolio lending due to the enforcement risk associated with balance sheet loans. To pin this mechanism down, I compare changes in lending across treated and control banks and across MERS and non-MERS loans following the Greenleaf judgment. MERS loans are originated with the intention of sale, since the electronic registry was constructed precisely for the purposes of streamlining secondary market transactions. Therefore, the effect of the Greenleaf judgment should be concentrated on non-MERS loans. This serves as the strongest test of the mechanism since the Greenleaf judgment was adjudicated on a MERS mortgage contract in particular. Therefore, if treated bank lenders leave MERS lending unchanged even after such a shock to contract enforceability, it should imply that lenders are plausibly more concerned with retaining enforcement risk than selling it off.

I run OLS regressions using a disaggregated, loan-level sample. I estimate a difference-in-difference-in-differences (DDD) regression of loan size:

$$Y_{c,b,t,i} = \alpha + X'_{c,b,t,i}\gamma + \delta D_b \times Post_t + \varphi MERS_i \times D_b \times Post_t \\ +\beta MERS_i \times Post_t + \epsilon_{c,b,t,i} \tag{5}$$

Note the above equation is an abbreviated version of the fully interacted specification. In the above specification, I modify Equation (2) to account for the loan-level sample. Here, $X_{c,b,t,i}$ includes the same county-level economic characteristics, bank-level balance sheet variables, and fixed effects from Equation (2). Instead of using continuous measures of bank-tract-year loan characteristics, I instead include fixed effects for borrower race, gender, property type, loan type, and owner occupancy, along with a loan-level measure of the loan-to-income ratio.

Table 4 presents estimates using regression Equation (5). All columns include fixed effects for lender-tract and tract-year. Here I identify the effect of increased enforcement risk across exposure and MERS status using the loan-level sample. For reference, Columns (1) and (2) report results from difference-in-differences regressions that mirror Columns (3) and (6) from Table 3. Here I restrict the sample to lending in New Hampshire and using loan level data. The results are similar to those reported in Table 3, whereby banks with above median exposure reduce average loan size by 17% and banks with a one standard deviation increase in exposure reduce loan size by 10%. These results are significantly different from zero at the 1% level. Column (3) of Table 4 replaces the exposure variable with an indicator for a loan's MERS status. Here, the estimated coefficient reports the effect of the Greenleaf judgment on MERS loan size, pooled across both exposed and unexposed banks. The coefficient estimate is quantitatively and statistically indistinguishable from zero.

The next two columns present estimates interacting the exposure variable with a MERS dummy. In Column (4), banks with above median exposure reduce non-MERS loan size by 35%, statistically different from zero at the 1% level. In Column (5), banks with a one standard deviation increase in exposure reduce non-MERS loan size by 11%, statistically different from zero at the 5% level. I identify the effect of the Greenleaf judgment on exposed bank MERS loan size by adding the difference-in-differences estimate to the triple-difference estimate. Standard errors are calculated using the covariance matrix. In Columns (4) and (5), I find that exposed banks do not change MERS loan size following the judgment in any qualitatively or statistically significant way. Given that banks register a mortgage with MERS to facilitate loan sale, these results indicate that increased enforcement risk is relevant only for those loans intended to stay on bank balance sheets. This is a sharp test of the mechanism, whereby lenders leave MERS loan size unchanged even though MERS was at the center of the Greenleaf judgment's unprecedented decision.

6.2 Tightening Lending Standards

Naturally, treated lenders would aim to reduce the need to rely upon collateral enforcement when faced with higher enforcement risk. Banks enforce their security interest in collateral when a borrower defaults on the associated promissory note. Therefore, treated lenders may want to reduce exposure to enforcement risk by minimizing the probability of having to enforce collateral rights in the first place, namely by reducing delinquency. Banks may achieve this by limiting the loan size itself or screening for better borrower quality. In this section, I estimate the effect of the Greenleaf judgment on bank lending standards.

6.2.1 Loan Size

In this first section, I test how exposed banks respond to the Greenleaf judgment by restricting the size of their loans. Here, I estimate my main specification on loan-level data using the pooled sample of portfolio and non-portfolio loans. I run OLS regressions and estimate a triple-difference regression of loan size:

$$Y_{c,b,t,i} = \alpha + X'_{c,b,t,i}\gamma + \delta D_b \times Post_t + \varphi D_b \times Portfolio_i \times Post_t + \epsilon_{c,b,t,i} \tag{6}$$

As with Equation (5), the above equation is an abbreviated version of the fully-interacted specification. The variable D_b measures a bank's exposure to Maine MERS lending, either as a discrete or continuous measure depending upon the specification. The coefficient on $D_b \times Post_t$ identifies the average treatment effect of increased downside risk for collateral enforcement. Here, δ captures the average change in loan size for loans issued by exposed banks across portfolio and non-portfolio loans following the Greenleaf decision. The coefficient of interest is φ, which measures the differential change in loan size among treated portfolio loans relative to both untreated portfolio loans and treated non-portfolio loans following the Greenleaf judgment. I identify φ using variation in the downside risk of collateral enforcement over time across loans issued by exposed and unexposed bank, controlling for systematic time-varying shocks to portfolio and exposed bank loan size separately. Once again, instead of continuous measures of the borrower pool, I include fixed effects for borrower race, gender, property type, loan type, and owner occupancy, along with a loan-level measure of the loan-to-income ratio.

There are three advantages to using a triple-difference specification. The first advantage is that I can control for loan level covariates directly to identify more precise estimates of the effect of the Greenleaf judgment on loans issued by treated banks. In addition to the fixed effects employed earlier, these new covariates include race, ethnicity, property type, owner occupancy, loan type, borrower income, and the debt-to-income ratio. In an ideal experiment, two loans would be equivalent to one another across every dimension except for treatment of downside enforcement risk. My research design approximates this ideal experiment by restricting identifying variation to two observationally equivalent loans which differ in exposure to the Greenleaf judgment through their originating lender. Using loan-level data allows a closer approximation to this ideal experiment as I can control for unobserved heterogeneity associated with borrower and loan characteristics.

The second advantage of my specification is that I can account for shared difference-in-differences across treated portfolio and non-portfolio loans. Doing so enhances the fixed effects used in my previous estimates. For example, when estimating a difference-in-differences regression of loan size on a sample of portfolio loans, I can control for time-varying shocks to loan demand and local economic conditions but only as they pertain to portfolio loans. If treated banks adjusted their portfolio lending in a manner that correlated with loan demand or local conditions affecting non-portfolio loans, then the fixed effects from earlier would fail to account for correlated time-series variation in treated bank lending. Instead, using a pooled sample, I can account for common shocks to loan demand across both loan types.

Third, my specification can control for shocks to bank balance sheets common across portfolio and non-portfolio lending using bank-year fixed effects. As mentioned earlier, the internal validity of my research design relies upon identifying variation associated with a shock to downside risk and not a shock to balance sheets following the ruling. Bank-year fixed effects can assuage these concerns by identifying the differential change in loan size for portfolio loans relative to non-portfolio loans within the same bank. Of course, this cannot perfectly control for the wealth channel of the Greenleaf judgment because exposed banks may not restrict lending equally across retained and securitized loans.

Table 5 presents estimates of the effect of the Greenleaf judgment on loan size across bank exposure and loan type. I estimate Equation (6) using the loan level sample. Here, the dependent variable is the logarithm of loan size. All columns include lender-tract and tract-year fixed effects. In Columns (1) and (2), I find that any change in average bank loan size is both quantitatively and statistically insignificant following the Greenleaf judgment. Under the assumption that this estimate identifies the average treatment effect across portfolio and non-portfolio loans, this would imply that exposed banks did not restrict loan size on average. Turning to the triple-difference estimates in Columns (1) and (2), I find that above median exposure leads to a 22% reduction in portfolio loan size and one standard deviation of exposure leads to a 20% reduction in portfolio loan size, respectively. These estimates are statistically different from zero at the 1% level. These results suggest that not only did exposed banks reduce their supply of portfolio credit, but exposed banks also reduced the size of each originated portfolio loan.

To assess the robustness of these results, Columns (3) and (4) of Table 5 present estimates with lender-year fixed effects. These fixed effects account for common changes across loan type within the same lender. If lenders faced losses correlated with the timing of the Greenleaf judgment, then controlling for this variation should attenuate the size of the estimates identified in Columns (1) and (2). Instead, after accounting for unobserved time-varying bank shocks, I find that the effect is quantitatively similar or larger than the baseline estimates. In Column (3), above median exposure leads to a 21% reduction in portfolio loan size, statistically different from zero at the 10% level and quantitatively similar to the estimate in Column (1). In Column (4), one standard deviation of exposure leads to a 28% reduction in loan size, statistically significant at the 1% level and 40% larger in magnitude relative to the estimate in Column (2).

These results make it increasingly unlikely that the effect of the Greenleaf judgment is driven by a material shock to banks. There is a fundamental difference between a shock to bank balance sheets and a shock to downside collateral enforcement risk. A balance sheet shock is fungible in that banks do not face a barrier that prevents them from curtailing both securitized and portfolio lending simultaneously. In contrast, a shock to the downside risk of enforcement should only affect portfolio lending since banks are directly liable for enforcing collateral rights over the promissory note. This is not the case for non-portfolio loans, whereby the right to enforce the promissory note is transferred upon sale of the loan. Hence, stable estimates across the inclusion of various fixed effects suggest that perhaps heightened downside risk of enforcement explains the drop in lending.

6.2.2 Credit Quality

In addition to loan size, I explore exposed banks credit quality for new originations by analyzing changes in county level delinquency rates following the Greenleaf judgment. If exposed banks are treated with higher perceived downside risk, then these banks would naturally aim to prevent the worst-case scenario from materializing. Banks could achieve this end by minimizing the probability that borrowers default, thereby limiting their reliance on enforcement in the first place. By extension, changes in delinquency rates in counties associated with high bank exposure to the Greenleaf judgment would be suggestive of banks screening on credit worthiness.

Table A.6 tests whether treated counties experience a change in credit worthiness of newly issued loans. I estimate regression Equation (2) using the county-month panel. The dependent variable is the 30- to 89-day mortgage delinquency rate for a given county and event month. Modifying Equation (4) for the county-level, exposure is defined as market share weighted average of bank exposure. Once again, I include weighted averages of bank and borrower characteristics. All columns also use county level economic controls, county fixed effects, and event year fixed effects.

In Column (1), I find that an above median exposure to the Greenleaf judgment decreases county-level delinquency rates by 0.235, statistically significant at the 5% level. In Column (2), a one standard deviation increase in exposure leads to a 0.25 decline in county-level delinquency rates, significantly different from zero at the 1% level. Given a baseline delinquency rate of 2.534%, this represents close to a 10% decrease in delinquency.

The results in Table A.6 suggest that exposed banks likely worked to reduce mortgage delinquency rates. This corroborates results in Table A.2 indicating that banks with above median exposure reduced charge-offs, non-performing loans, and loan loss funds following the Greenleaf judgment. If exposed banks extended credit more cautiously as evidenced by the county-level estimates in Table A.6, then exposed banks would naturally experience reduced losses in the wake of Maine's ruling. As banks suffered fewer losses, their balance sheets similarly improved.

6.2.3 Application Denial Rates

The previous results seem to suggest that exposed banks limited credit based on borrower quality. In this section, I further explore bank screening behavior by directly estimating changes to application denial rates following the Greenleaf judgment. Assuming that portfolio lending declined for exposed banks, I hypothesize that denial rates should also increase on loan applications that failed to qualify for investor purchase. The Federal Housing Finance Agency (FHFA) sets loan size thresholds that loans cannot exceed in order to qualify for Fannie Mae and Freddie Mac purchase on the secondary market. I exploit this variation in FHFA limits to identify the effect of the Greenleaf judgment on application denial for loans with heightened enforcement risk.

I test how exposed banks respond to the Greenleaf judgment by restricting application approvals. Here, I estimate my main specification on application-level data. I run OLS regressions and estimate a triple-difference regression of application denial:

$$\begin{aligned} Y_{c,b,t,i} = \alpha + X'_{c,b,t,i}\gamma + \delta D_b \times Post_t + \phi Above\ Limit_i \times Post_t \\ + \beta_1 Above\ Limit_i + \beta_2 Above\ Limit_i \times D_b \\ + \varphi Above\ Limit_i \times D_b \times Post_t + \epsilon_{c,b,t,i} \end{aligned} \tag{7}$$

The outcome variable is equal to one if an application is denied. The *Above Limit*$_i$ dummy takes a value of one if a loan is above the conforming loan limit (CLL). The application-level sample is restricted to applications with a loan size within 5% of the CLL. Within this sharp bandwidth, I assume that borrowers do not systematically select into CLL compliance in any systematic way across exposed and unexposed banks over time. Therefore, β_1 and β_2 identify the time-invariant differential effect of violating the CLL on loan denial rates across unexposed and exposed banks, respectively. Furthermore, I assume that time-varying differences are shared across exposed and unexposed banks. Under these assumption, φ identifies the causal effect of increased enforcement risk on denial rates above the conforming limit. Note that this does not assume that borrowers do not select into above or below the CLL. Rather, I assume that such selection is accounted for through time-invariant differences across exposure and time-varying common shocks across exposure.

Table A.7 presents the estimates of regression Equation (7), where the dependent variable is a dummy variable equaling one if a loan application was denied. All columns include lender-tract and tract-year fixed effects. In Column (1), the triple difference interaction term shows that loans just above the CLL experience a 5.9% increase in rejection among above median exposure banks. This result is statistically significant at the 5% level. In Column (2), the estimated treatment effect using a continuous measure of exposure is 9%, statistically larger than zero at the 1% level. Given a baseline denial rate of 18.57%, these estimates of the differential effect represent an increase in denial rates between one third and one half.

I conduct additional tests to confirm that these results are driven by tightening lending standards. To do so, I split the sample into loans with a loan-to-income (LTI) ratio above or below the 2013 median LTI. Columns (3) and (4) restrict the sample to loans with a LTI ratio below the 2013 median. Here, the estimated treatment effect is not statistically significant. Columns (5) and (6) restrict the sample to loans with a LTI ratio above the 2013 median. In Column (5), I find that above median exposure leads to a 10% increase in denial rates for high LTI loans above the conforming loan limit issued following the Greenleaf judgment, statistically different from zero at the 10% level. In Column (6), the estimated treatment effect using a continuous measure of exposure is 16.9%, statistically larger than zero at the 1% level. Importantly, the point estimates of the difference-in-differences estimate is relatively larger and negative, although statistically insignificant. Therefore, the overall effect on a loan application above the CLL is attenuated, likely due to reallocation of loan approvals to loans below the CLL.

Taken together, these results suggest that exposed banks increased loan denials particularly for high LTI loans, as these loans are relatively riskier. With a larger loan size and lower income, borrowers are more likely to miss a payment, therefore increasing the likelihood that a creditor will have to enforce their collateral rights on the loan. Since loans above the conforming loan limit are more likely to stay on bank balance sheets, the increased risk of enforcement precipitated by the Greenleaf judgment is particularly heightened for these high LTI and non-conforming mortgages. These are precisely the conditions for which loan denial rates should increase.

6.3 Distance from Shock

Literature on internal bank capital markets emphasize the role that geography plays in transmitting shocks (Nguyen (2019)). Naturally, the effect of the shock should be most salient in localities nearby. In the context of the Greenleaf judgment, the preceding analysis assumes that lenders are most concerned with new information related to collateral enforcement risk. A more involved hypothesis would predict that this new information is most relevant for geographies immediately adjacent to the shock itself. This may be due to similarities in enforcement regimes across local geographies. It seems plausible that a lender believes that Massachusetts is more likely to follow Maine than New York in its commitment to contract enforcement. I test this hypothesis by estimating my main specification across distance. I run OLS regressions using the bank-tract-year panel. I estimate a triple-difference regression of lending volume:

$$Y_{c,b,t} = \alpha + X'_{c,b,t}\gamma + \delta D_b \times Post_t + \varphi Near_c \times D_b \times Post_t + \epsilon_{c,b,t} \tag{8}$$

In the triple-differences specification above, I interact the exposure variable with a dummy variable $Near_c$. This dummy variable equals one when a census tract is in the first decile of distance from Maine's capital. Here I compare changes in exposed bank lending before and after the Greenleaf judgment across census tracts near the epicenter of the judgment and census tracts farther away. I focus on portfolio lending in order to determine how distance affects bank perception of enforcement risk on loans that they are liable to enforce. I hypothesize that banks will reduce lending more in localities near the epicenter than those farther from it.

Table A.8 presents estimates of the triple difference specification where the dependent variable is the logarithm of portfolio lending at the bank-tract-year level. All columns include bank-tract and tract-year fixed effects. In Column (1), I find that the drop in portfolio lending is twice as large in nearby tracts relative to those farther away. Above median exposure leads to a 37% reduction in overall bank portfolio lending, statistically significant at the 5% level. Census tracts within the first decile of distance from Maine experience a 37% larger reduction in lending, statistically significant at the 1% level. Column (2) further corroborates these results, where a one standard deviation increase in exposure is associated with a 35% drop in bank lending, statistically significant at the 1% level. Nearby census tracts differentially experience a 16% greater drop in lending, significant at the 10% level.

While the preceding analysis demonstrates that perceived enforcement risk diminishes with distance, it may also be the case that exposed lenders simply concentrate in localities near the shock. Therefore, distance is mechanically correlated with exposure. Incorporating lender-year fixed effects should account for this mechanical correlation by restricting variation to a given bank and event year. Conceptually, I am accounting for time-varying shocks common across near and far census tracts within the same bank. Therefore, the triple-difference estimate should identify the differential effect of the Greenleaf judgment on nearby census tracts relative to any average treatment effect within the same bank.

In Column (3) of Table A.8, I find that above median exposed banks reduce portfolio lending by 27% more in census tracts within the first decile of distance from Maine, statistically significant at the 1% level. In Column (4), I find that a one standard deviation increase in exposure leads to a differentially larger 21% drop in bank portfolio lending in nearby census tracts, statistically significant at the 1% level. Ultimately, these results support the conclusion from earlier, that nearby census tracts experienced a stronger reduction in lending when compared to localities farther away.

In order to assess the degree to which lending by treated and untreated banks varies across space, I replace the $Near_c$ variable with an indicator variables for deciles in distance from Maine's capital:

$$Y_{c,b,t} = \alpha + X'_{c,b,t}\gamma + \delta D_b \times Post_t + \sum_{s=1}^{9} \delta_s D_b \times Post_t \times \{c \in decile_s\} + \epsilon_{c,b,t} \tag{9}$$

In the above specification, the most distant decile is the omitted category. Therefore, δ_s is interpreted as the differential change in lending by treated banks following Greenleaf relative to the baseline decile.

Figure 6 compares the effect of the Greenleaf judgment on treated and control banks across distance deciles. The figure plots estimates and 95% confidence intervals from a flexible triple difference specification given by Equation (9) using the same set of controls as in Column (6) of Table 3. The regression is estimated separately for portfolio and non-portfolio lending. Estimates from the baseline decile are normalized to zero so that lending is compared relative to the farthest distance from Maine's capital. I identify the effect of the Greenleaf judgment on exposed bank lending by adding the difference-in-differences estimate to the triple-difference estimate. Standard errors are calculated using the covariance matrix. For non-portfolio lending, treated and control banks evolved lending roughly in parallel across distance deciles. However, for portfolio lending, treated banks increasingly reduced portfolio lending closer to the Greenleaf epicenter relative to control banks. The reduction in portfolio lending by treated banks was over two times as large in the first decile when compared to the decile farthest away.

These results suggest that the effect of increased enforcement risk is most salient in localities closest to the shock's epicenter. This supports the hypothesis that bank perception of collateral enforcement is relatively continuous across a local geography. Furthermore, Figure 6 corroborates previous results indicating that non-portfolio loans are unaffected by this judgment. If portfolio lending is differentially affected across geographic distance from a shock due to enforcement risk, then non-portfolio lending should be unchanged across geography precisely because banks are not liable for sold loans. This is exactly what Figure 6 demonstrates, whereby changes in non-portfolio lending across distance are statistically insignificant.

6.4 Lender Type

It is well-established that the organizational form of a bank has meaningful consequences for how lenders transmit balance sheet shocks (Berger et al. (2005); Gilje et al. (2016)). For example, relative to large banks, small banks may have less access to capital markets and suffer greater financial constraints. Therefore, a small bank is expected to curtail lending more following a balance sheet shock. Furthermore, banks that function under an originate-to-distribute model are often liquidity constrained and less capitalized to deal with significant capital short falls (Purnanandam (2010)). Indeed, Ahsin (2021) provides evidence that creditors had more difficulty locating precisely these lenders, likely due to their bankruptcy following 2008.

In the context of enforcement risk, however, these well-established dynamics may reverse. Namely, smaller banks may be better informed about collateral enforcement regimes relative to larger banks. Therefore, larger banks may be less likely to trust local judiciaries when compared to small banks. As demonstrated in Section 6.3, bank perception of enforcement risk is plausibly continuous across local geographies. Therefore, private information about local collateral rights likely ameliorates these concerns. Second, lenders operating on an originate-to-distribute model may be less likely to curtail lending since their loans are sold off-balance sheet. In contrast, originate-to-retain lenders are more likely to limit credit provision due to an increase in enforcement risk. If 'sellers' sell away any enforcement risk, then 'retainers' on average retain most of this risk on balance sheet. To test these hypotheses, I estimate my main specification on restricted samples across lender type.

6.4.1 Bank Size

Table 6 presents estimates of the effect of higher enforcement risk on bank lending across bank size. The dependent variable measures the logarithm of lending at the bank-tract-year level. Each panel reports estimates from difference-in-differences regressions equivalent to my preferred specification in Table 3 but restricted to banks with either below or above the 2013 median asset size. Columns (1) and (2) estimate Equation (2) using the primary sample, Columns (3) and (4) restrict the sample to portfolio lending, and Columns (5) and (6) restrict the sample to non-portfolio lending. Due to limited variation among small bank portfolio lending across exposure, I eliminate lender-tract and tract-time fixed effects in Column (5) across both panels. Otherwise, all columns include bank-tract and tract-year fixed effects.

In Panel A of Table 6, I report estimates using the sample of small banks. I do not find evidence that small banks reduced their portfolio lending. In fact, mirroring the estimates using the sample of non-banks in Table A.4, I find mixed evidence that exposed small banks increased overall lending following the Greenleaf judgment. These results are driven by an increase in non-portfolio lending. In Columns (5) and (6), I find that above median exposure led to 27% higher non-portfolio lending and one standard deviation of exposure led to 41% higher non-portfolio lending, respectively. These estimates are both statistically larger than zero at the 1% level.

In Panel B of Table 6, I report estimates using the sample of large banks. Here, the results largely mirror the estimates using the full sample, reported in Table 3. In Column (1), I find that above median exposure decreases big bank lending by 19%, statistically different from zero at the 5% level. In Column (2), a one standard deviation increase in exposure leads to a 15% decline in big bank lending, statistically different from zero at the 10% level. Turning to portfolio lending, in Column (3), I find that above median exposure decreases big bank portfolio lending by 34%, statistically significant at the 10% level. In Column (4), a one standard deviation increase in exposure leads to a 37% decline in big bank portfolio lending, statistically significant at the 1% level. Finally, in Columns (5) and (6), I find statistically insignificant estimates of the effect of higher enforcement risk on non-portfolio lending for big banks.

These results support the hypothesis that the Greenleaf judgment mainly affected large banks. If the drop in portfolio lending reflects a shock to expected enforcement in the down state, then the preceding evidence suggests that big banks bore the largest increase in perceived risk. This supports previous studies that emphasize the role of localized knowledge in lending. Distance from local markets likely contributed to worse perceptions about enforcement risk.

6.4.2 Business Model

Table 7 presents the results of the effect of the Greenleaf judgment across exposure and business model. The dependent variable is the logarithm of lending at the bank-tract-year level. I estimate Equation (2) separately for banks above median scaled portfolio lending (Retainers) and banks with below median scaled portfolio lending (Sellers). I use 2013, the year prior to the ruling, as the reference date and scale portfolio lending by a bank's total New England volume in 2013. All columns include bank-tract and tract-year fixed effects.

In Panel A of Table 7, I restrict the sample to Retainers. Here, the estimates mirror the results in Table 3. Naturally, since Retainers mainly operate in portfolio lending, the overall effect will be larger for these lenders when compared to the full sample. In Columns (1) and (2), using the sample of Retainers and pooling across all loan types, the point estimates are 41% and 35%, respectively. Both estimates of the overall effect are statistically significant at the 1% level. In Columns (3) and (4), above median exposure leads to a 46% reduction in portfolio lending and one standard deviation of exposure leads to a 40% reduction in portfolio lending, respectively. Both estimates are statistically significant at the 1% level. Finally, corroborating previous results using the full sample, I find no statistically significant change in non-portfolio lending among Retainers. In contrast to Panel A, Panel B of Table 3 shows that Sellers experience no statistically significant change in lending, both estimated using the pooled sample and across loan type.

These results imply that the effect of higher enforcement risk is most salient for Retainers as opposed to Sellers. For banks that do a majority of their business in portfolio lending, bad news about collateral enforcement is a grave threat to their business model. In contrast, banks that sell most of their loans do not face any relevant increase in collateral enforcement risk since this risk is sold off to investors. This result is counter-intuitive because banks operating under an originate-to-distribute model are most vulnerable to balance sheet shocks given their reliance on outside capital. However, when the shock is concentrated on collateral, then the originate-to-distribute model protects Sellers from liability to rely upon enforcement rights.

7 Spillover Effects

7.1 Reallocation of Credit

Underexplored in the literature is the transmission of shocks from one credit type to another. Chakraborty et al. (2018) find that positive shocks to house prices lead lenders to reallocate commercial credit to mortgage lending. Fieldhouse (2021) finds reinforcing evidence that regulatory shocks to subsidized mortgage credit crowds out commercial credit. Along these lines, I test for whether shocks to the downside enforcement risk of housing collateral leads banks to reallocate credit to commercial lending. Not only does this test the substitution channel of bank lending, but the particular mechanism is unique to the literature. Namely, in this section I study how a shock to the enforcement risk of one type of collateral affects lending securitized by another type of collateral.

Furthermore, I predict that reallocated credit by exposed banks should increase to borrowers with a banking relationship. If exposed banks were to reallocate capital to commercial credit due to enforcement risk, then banks may be more sensitive to the credit worthiness of the new recipients of the reallocated credit. This heightened sensitivity is highlighted in Section 6.2, where banks seem to tighten lending standards to reduce their expected reliance on enforcing collateral rights. Therefore, I test for this heterogeneity by splitting the sample across loan size. Namely, I estimate regression Equation (2) at the tract-level for large and small loans separately. When banks issue small business loans above \$250,000, the borrower likely has an established relationship with the bank to receive a loan of that size. Therefore, separately testing for the effect of the Greenleaf judgment across large and small loan volume should be suggestive of a relationship-lending channel.

Table A.9 shows estimates of the effect of the Greenleaf judgment on small business lending. Due to the limited number of county-bank pairs in the New England area, I focus on aggregate lending at the census tract level. All columns include tract and year fixed effects. Columns (1) and (2) presents results where the dependent variable is the logarithm of total lending at the tract-year level using the sample of small business loans above \$250,000. In Column (1), I find that an above median exposure to the Greenleaf judgment increases tract-level lending by 5%, statistically significant at the 5% level. Column (2) corroborates this result using a continuous measure of exposure. I estimate that a one standard deviation increase in exposure to higher collateral enforcement risk leads to a 3% increase in tract-level lending, significantly different from zero at the 5% level. In Columns (3) and (4), I find no statistically significant change in small business lending using the sample of loans below \$250,000. Ultimately, these results suggest that exposed banks did increase commercial credit but plausibly only to those borrowers that had established banking relationships.

I further explore the degree to which banking relationships affected the reallocation of credit following the Greenleaf judgment using data on bank branch deposits. I estimate a modified version of Equation (2) using the tract-year panel:

$$Y_{c,t} = \alpha + X'_{c,t}\gamma + \delta D_c \times Post_t + \varphi Branch_c \times D_c \times Post_t + \epsilon_{c,t} \tag{10}$$

In the abbreviated specification above, $Branch_c$ equals one if a census tract contains bank branch deposits. Figure 7 plots estimates of regression Equation (10). Here, I compare the effect of the Greenleaf judgment on small business lending across bank branch presence and loan size. The figure plots estimates and 95% confidence intervals from the specification above using the same set of controls as in Column (1) of Table A.9. The regression is estimated separately for large and small loans. The dependent variable is the logarithm of loan volume for a given census-tract and event year.

I identify the effect of the Greenleaf judgment on exposed small business lending by adding the difference-in-differences estimate to the triple-difference estimate. Standard errors are calculated using the covariance matrix. For large loans, treated census tracts increased large loan volume almost exclusively for loans within tracts with bank branch deposits. For small loans, treated and exposed tracts experience no statistically significant change in commercial credit, even when containing bank branch deposits.

These results imply a robust reallocation channel that complements the estimates in previous sections. The evidence in Table A.9 suggests that exposed census tracts increase small business credit following an increase in enforcement risk. The increase in credit to large loan recipients instead of small loan borrowers further implies that banks plausibly shifted toward safer collateral, whereby large loans are often provided to trustworthy borrowers with an established banking relationship. This mechanism is further solidified by assessing the role of bank branch deposits. Banks appear to increase credit to large loan borrowers exclusively in localities with a branch present, supporting the importance of relationship lending following a shock to enforcement risk.

7.2 Real Economic Effects

Following the financial crisis, significant research has focused on understanding the relationship between the mortgage credit supply and the real economy. The literature has linked increases in mortgage lending to house prices (Favara and Imbs (2015), Mian and Sufi (2011), Adelino et al. (2020)) and aimed to understand the effect of mortgage credit on employment (Mian and Sufi (2014), Adelino et al. (2015), Di Maggio and Kermani (2017),Chodorow-Reich (2013), Mondragon (2020)). With this context in mind, I test whether shocks to the downside enforcement risk of housing collateral affects house prices and employment. The results in the preceding sections suggest that that higher enforcement risk leads to a drop in portfolio lending. If local house prices and employment are tightly linked to the availability of mortgage credit, then the reduction in portfolio lending should have significant consequences for the local economy.

Table A.10 presents estimates of the effect of the Greenleaf judgment on house prices and employment. All columns include county and year fixed effects. In Columns (1) and (2), my dependent variable is the logarithm of the house price index for a given zip-code and event year. I use the HUD zip-tract cross walk to construct a weighted average of zip-code exposure based on census-tract exposure. For this exercise, I use the residential ratio of a given zip-tract pair in June of 2014 to serve as the weight. I find that above median exposure leads to a 3% reduction in house prices, statistically significant at the 1% level. Furthermore, a one standard deviation increase in exposure is associated with a 1% drop in house prices, statistically significant at the 1% level. In Columns (3) and (4), my dependent variable is the unemployment rate for a given county and event month. I find that above median exposure leads to a 0.899 percentage point increase in the unemployment rate, statistically significant at the 1% level. Furthermore, a one standard deviation increase in exposure is associated with a 0.437 increase in the unemployment rate, statistically significant at the 1% level. Given a baseline unemployment rate of 4.98%, these estimates represent an increase between one tenth and one fifth.

Figure 8 plots estimates and 95% confidence intervals from a flexible difference-in-differences specification given by Columns (2) and (4) of Table A.10. In Panel A of Figure 8, I plot changes in the logarithm of the house price index around the 2013 baseline year. I find there is relatively little difference in house prices across exposed and unexposed zip codes for three years prior to the Greenleaf judgment. Following the judgment, however, there is a statistically significant reduction in house prices for four years in treated zip codes. In Panel B of Figure 8, I plot changes in the unemployment rate around the baseline year. Here, the baseline year refers to the full year immediately prior to the July 2014 judgment. Once again, I find that there is relatively little difference in unemployment across exposed and unexposed counties. However, following July 2014, I estimate a statistically significant increase in the unemployment rate in treated counties for four years following the judgment.

8 Conclusion

This paper provides causal evidence that increases to collateral enforcement risk can have dramatic consequences for the credit supply. I first show that banks exposed to the Greenleaf judgment reduce lending across the New England area, particularly for portfolio loans. I argue that this reduction is due to heightened enforcement risk and neither a wealth shock nor capital flight. Exploring the mechanism, I provide evidence that banks reduce lending by tightening loan terms, namely by increasing borrower quality and shrinking the average loan size. Ultimately, large banks, portfolio lenders, and geographically nearby subsidiaries are most affected by collateral enforcement uncertainty.

The evidence in this paper suggests that downside risk to collateral enforcement is fundamental to lending markets. Previous studies have focused on the importance of material enforcement costs (Pence (2006)), perhaps through legal regimes or a foreclosure moratorium. In contrast, this paper demonstrates that even absent anything material, banks may curtail lending due to the perception of heightened enforcement risk. This implies that as banks lend later into the business cycle and the risk of a recession increases, lenders will increasingly worry about their ability to repossess collateral. Such concern, as demonstrated by the evidence in this paper, may precipitate a credit crunch. While providing guarantees to creditor rights may assuage these concerns, policy makers must ultimately balance between reinforcing the credit supply and protecting borrowers.

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Figure 1. MERS Mortgage Contract Across States

Panel A: New Hampshire Contract

Borrower is the mortgagor under this Security Instrument.
(C) "MERS" is Mortgage Electronic Registration Systems, Inc. MERS is a separate corporation that is acting solely as a nominee for Lender and Lender's successors and assigns. MERS is the mortgagee under this Security Instrument. MERS is organized and existing under the laws of Delaware, and has an address and telephone number of P.O. Box 2026, Flint, MI 48501-2026, tel. (888) 679-MERS.

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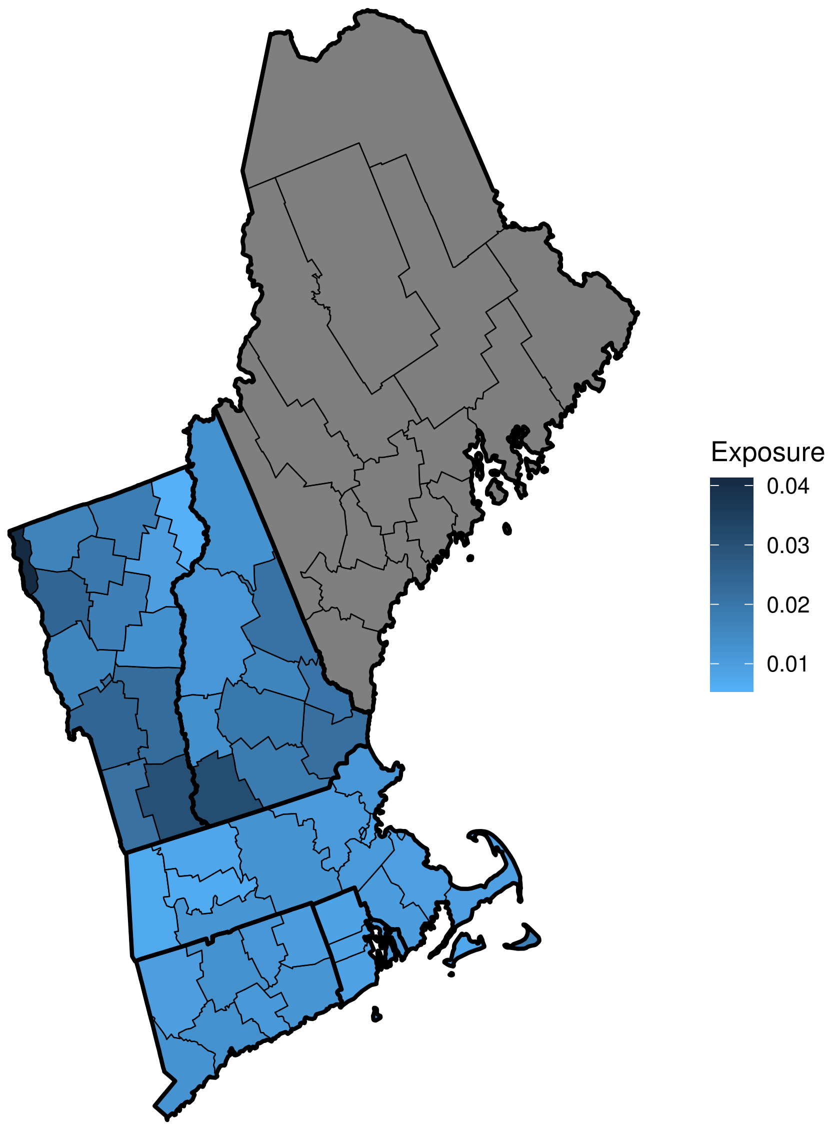
Panel B: Maine Contract

(B) "Borrower" means SCOTT A GREENLEAF AND KRISTINA GREENLEAF AS JOINT TENANTS .

who sometimes will be called "Borrower" and sometimes simply "I" or "me." "Borrower" is granting a mortgage under this Security Instrument. "Borrower" is not necessarily the same as the Person or Persons who signed the Note. The obligations of Borrowers who did not sign the Note are explained further in Section 13.
(C) "MERS" is Mortgage Electronic Registration Systems, Inc. MERS is a separate corporation that is acting solely as a nominee for Lender and Lender's successors and assigns. MERS is organized and existing under the laws of Delaware, and has an address and telephone number of P.O. Box 2026, Flint, MI 48501-2026, tel. (888) 679-MERS. FOR PURPOSES OF RECORDING THIS MORTGAGE, MERS IS THE MORTGAGEE OF RECORD.
(D) "Lender" means RESIDENTIAL MORTGAGE SERVICES, INC

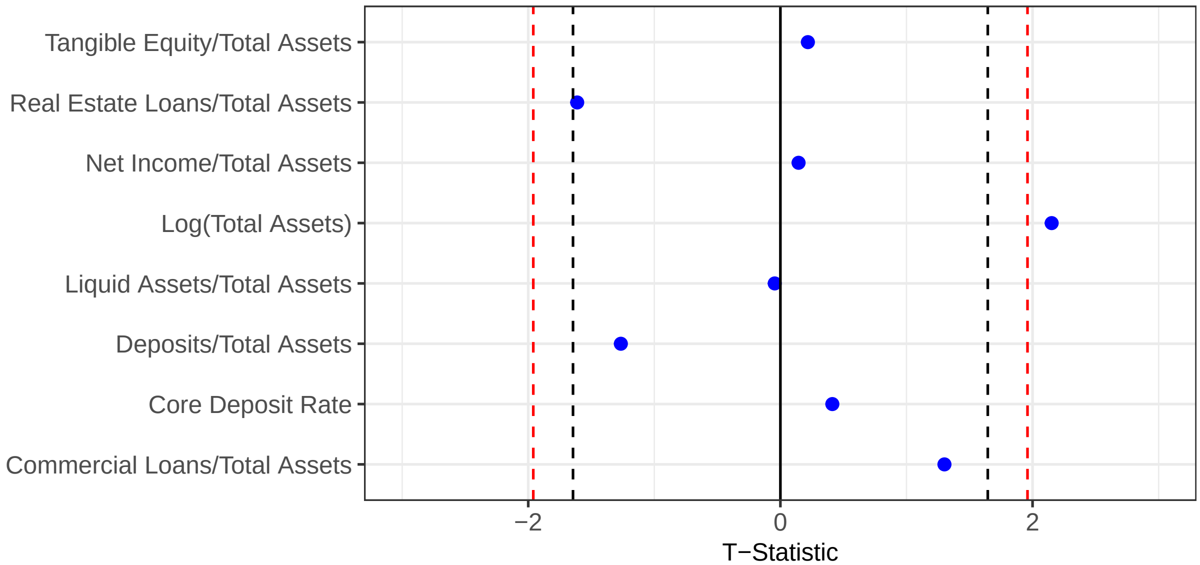
Note: This figure presents variations in the key sentence declaring MERS as the mortgagee of interest. Panel A presents the standard MERS mortgage contract in New Hampshire. Panel B presents the standard MERS mortgage contract in Maine. Panel B is taken directly from an exhibit used in the Greenleaf judgment.

Figure 2. Exposure to Greenleaf judgment Across New England



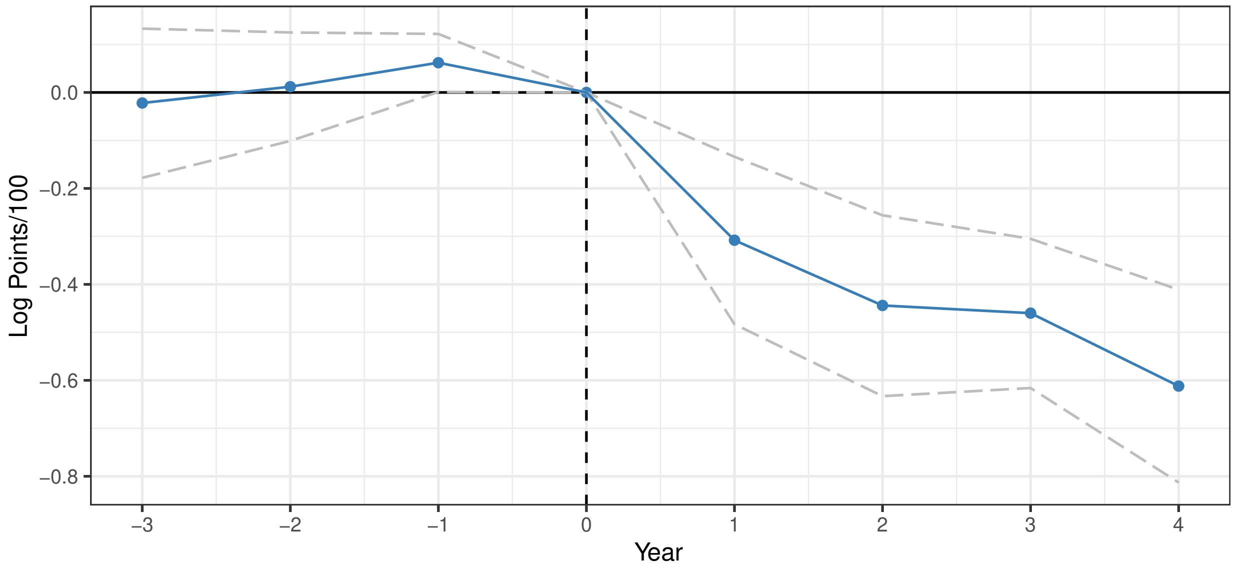
Note: This figure plots exposure to the Greenleaf judgment across New England counties. Exposure at the bank level is measured as 2013 Maine MERS lending as a share of total New England lending by a given bank. Exposure at the county level is average bank exposure, weighted by 2013 market share. State lines are in bold.

Figure 3. Balance Test



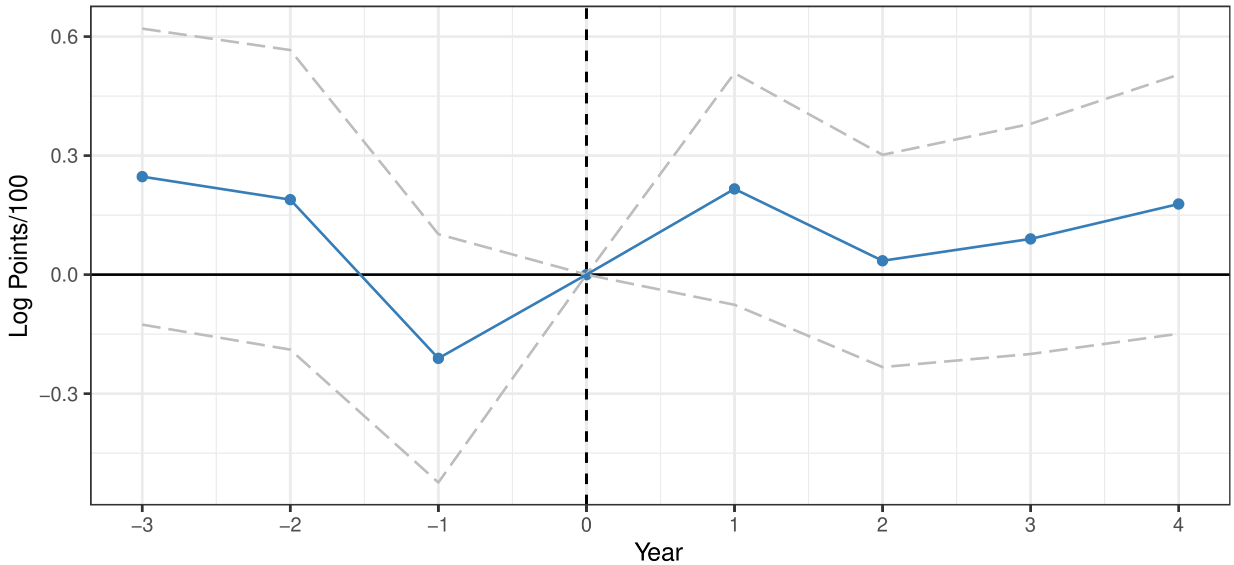
Note: This figure plots standardized estimates form a regression of balance sheet variables on bank exposure to the Greenleaf judgment. Estimates are divided by their associated standard error, so that each point measures a t-statistic.

Figure 4. Difference-in-Differences Estimates: Log(Portfolio Loan Volume)



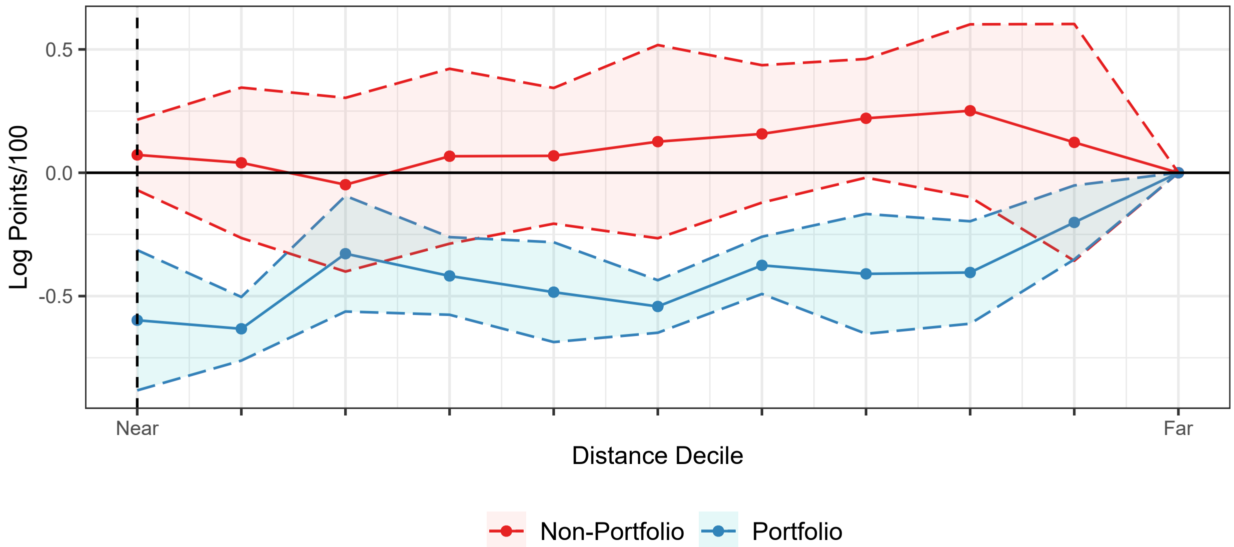
Note: This figure reports difference-in-difference estimates of the effect of the Greenleaf judgment on bank portfolio lending. The outcome variable measures the logarithm of total portfolio lending at the bank-tract-year level. Estimates are derived from a difference-in-differences regression that interacts exposure to the Greenleaf judgment and year fixed effect. The coefficient for the year preceding the judgment is normalized to zero. The black dashed vertical line indicates the date of the Greenleaf judgment. The light dashed line represents 95% confidence intervals calculated using bank-clustered standard errors. Data covers the period of 2010 to 2017 for loans originated across the New England area.

Figure 5. Difference-in-Differences Estimates: Log(Non-Portfolio Loan Volume)



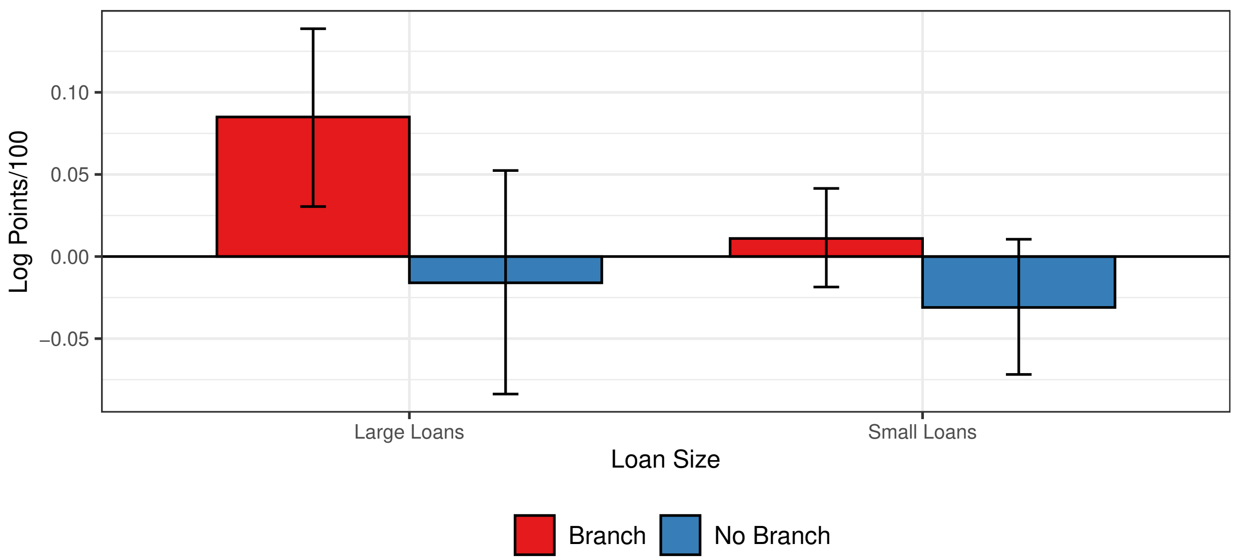
Note: This figure reports difference-in-difference estimates of the effect of the Greenleaf judgment on bank non-portfolio lending. The outcome variable measures the logarithm of total non-portfolio lending at the bank-tract-year level. Estimates are derived from a difference-in-differences regression that interacts exposure to the Greenleaf judgment and year fixed effect. The coefficient for the year preceding the judgment is normalized to zero. The black dashed vertical line indicates the date of the Greenleaf judgment. The light dashed line represents 95% confidence intervals calculated using bank-clustered standard errors. Data covers the period of 2010 to 2017 for loans originated across the New England area.

Figure 6. Difference-in-Differences Estimates by Distance Decile and Loan Type: Log(Loan Volume)



Note: This figure reports triple difference estimates of the effect of the Greenleaf judgment on bank lending across distance deciles. The outcome variable measures the logarithm of total lending at the bank-tract-year level. Estimates using the sample of non-portfolio loans are highlighted in red and estimates using the sample of portfolio loans are highlighted in blue. Estimates are derived from a triple-difference regression that interacts exposure to the Greenleaf judgment, distance decile, and a Post dummy. The coefficient for the decile farthest from the judgment is normalized to zero. Each point represents the sum of the difference-in-differences estimate and the triple-difference estimate. Bank-clustered standard errors are calculated using the covariance matrix. The light dashed lines represent 95% confidence intervals. Data covers the period of 2010 to 2017 for loans originated across the New England area. The right-most point is expositional and does not originate from any regression.

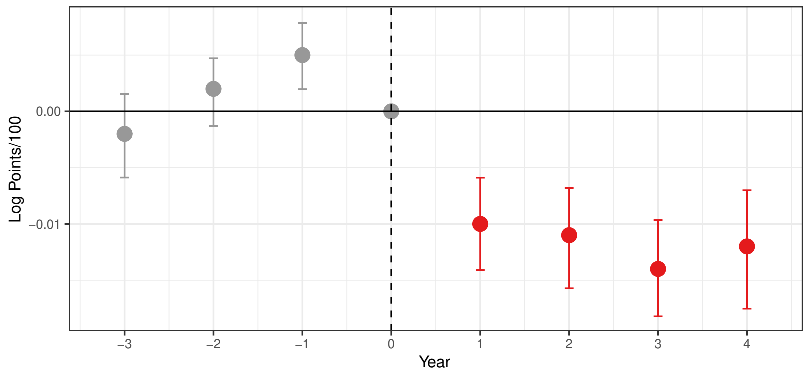
Figure 7. Difference-in-Differences Estimates Across Loan Size and Branch Presence: Log(Small Business Lending)



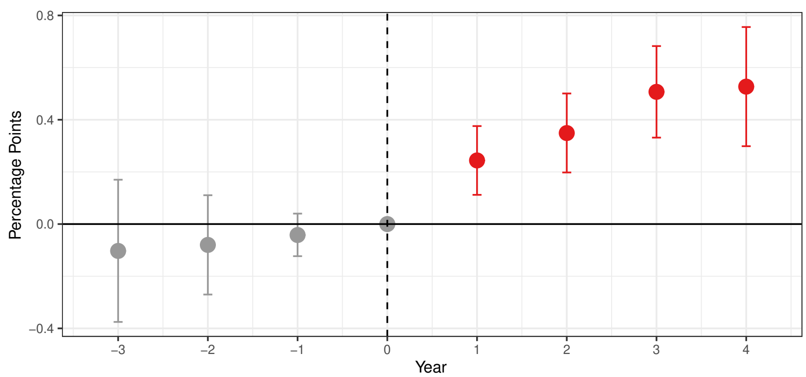
Note: This figure reports triple difference estimates of the effect of the Greenleaf judgment on small business lending across bank branch presence. The outcome variable measures the logarithm of total bank lending at the tract-year level. Estimates using the sample of large loans are plotted on the left and estimates using the sample of small loans are plotted on the right. Estimates of bank branch deposit presence are highlighted in red and estimates of no branch presence are highlighted in blue. Estimates are derived from a triple-difference regression that interacts exposure to the Greenleaf judgment, bank branch deposit presence, and a Post dummy. The coefficient for the no presence is normalized to zero. Each estimate is the sum of the difference-in-differences estimate and the triple-difference estimate. Bank-clustered standard errors are calculated using the covariance matrix. The black error bars represent 95% confidence intervals. Data covers the period of 2010 to 2017 for loans originated across the New England area.

Figure 8. Difference-in-Differences Estimates: Real Effects

Panel A: Log(House Price Index)



Panel B: Unemployment Rate



Note: This figure reports difference-in-difference estimates of the effect of the Greenleaf judgment on house price growth and the unemployment rate. In Panel A, the outcome variable measures the logarithm of the zip-code-year level house price index. In Panel B, the outcome variable measures the unemployment rate at the county-month level. Estimates are derived from a difference-in-differences regression that interacts exposure to the Greenleaf judgment and year fixed effect. The coefficient for the year preceding the judgment is normalized to zero. The black dashed vertical line indicates the date of the Greenleaf judgment. The error bars represent 95% confidence intervals calculated using geography-clustered standard errors. Data covers the period of 2010 to 2017 for the New England area.

Table 1. Summary Statistics

	Exposed		Unexposed	
	Mean	SD	Mean	SD
Panel A: Balance Sheet Characteristics				
Log(Total Assets)	14.93	2.34	13.59	1.74
Deposits/Total Assets	0.75	0.14	0.81	0.08
Core Deposit Rate	0.01	0.00	0.01	0.00
Liquid Assets/Total Assets	0.26	0.17	0.28	0.14
Tangible Equity/Total Assets	0.11	0.03	0.11	0.03
Commercial Loans/Total Assets	0.06	0.08	0.06	0.06
Real Estate Loans/Total Assets	0.34	0.18	0.36	0.16
Net Income/Total Assets	0.01	0.01	0.01	0.01
Panel B: Lending Characteristics				
Tract Volume (000's)	770.59	1185.83	871.30	1716.98
Portfolio Volume	707.41	1247.46	738.43	1622.61
Non-Portfolio Volume	599.01	751.96	694.60	1003.90
Tract LTI	2.40	1.01	2.14	1.09
Loan Size (000's)	262.17	202.71	256.05	235.89
Portfolio Size	316.65	313.24	291.06	304.66
Non-Portfolio Size	241.30	132.92	226.91	151.10
LTI	2.34	1.25	2.09	1.26
Number of Banks	85		349	
Number of Bank-Tracts	219,371		201,674	
Number of Loans	656,788		651,968	

Note: This table reports mean and standard deviation values for 2013 bank balance sheet and lending characteristics for all banks observable in HMDA between January 2010 and December 2017 across the New England area (excluding Maine). Maine MERS exposure is defined as Maine MERS lending as a share of overall New England lending by a given bank.

Table 2. Effect of the Greenleaf Judgment on Bank Lending: Difference-in-Differences

	log(Loan Volume)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	-0.195**	-0.239***	-0.226**			
	(0.098)	(0.091)	(0.092)			
Exposure×Post				-0.102	-0.183**	-0.173**
				(0.070)	(0.076)	(0.078)
Tract FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Lender FE	X	X	X	X	X	X
Lender×Tract FE		X	X		X	X
Tract×Time FE			X			X
Implied %△	-18%	-22%	-21%	-10%	-17%	-16%
Obs	420,448	420,448	420,448	420,448	420,448	420,448

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on bank lending volume. The outcome variable equals the log of mortgage lending at the lender-tract-year level. The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treatment dummy takes a value of one if a lender has above median non-zero exposure to 2013 Maine MERS lending. The Exposure variable equals lender exposure to 2013 Maine MERS lending, standardized. Lender-tract level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. Bank level controls include total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation, the census tract, and the lender. Columns 2 and 5 include fixed effects for the census tract interacted with the lender. Columns 3 and 6 include fixed effects for the census tract interacted with year. Standard errors are reported in parentheses and are clustered by lender. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for bank loans originated in the New England area.

Table 3. Effect of the Greenleaf Judgment on Bank Lending Across Loan Type: Difference-in-Differences

	log(Loan Volume)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Portfolio Loans						
Treat×Post	-0.371*	-0.485**	-0.496**			
	(0.211)	(0.224)	(0.213)			
Exposure×Post				-0.293**	-0.445***	-0.452***
				(0.121)	(0.082)	(0.080)
Implied %△	-32%	-40%	-41%	-26%	-36%	-37%
Obs	222,689	222,689	222,689	222,689	222,689	222,689
Panel B: Non-Portfolio Loans						
Treat×Post	0.038	-0.000	0.018			
	(0.122)	(0.138)	(0.144)			
Exposure×Post				0.069	0.079	0.099
				(0.060)	(0.116)	(0.125)
Implied %△	3%	-1%	1%	7%	7%	10%
Obs	295,527	295,527	295,527	295,527	295,527	295,527
Tract FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Lender FE	X	X	X	X	X	X
Lender×Tract FE		X	X		X	X
Tract×Time FE			X			X

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on bank lending volume across loan type. Panel A limits the sample to portfolio loans. Panel B limits the sample to non-portfolio loans. The outcome variable equals the log of mortgage lending at the lender-tract-year level. The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treatment dummy takes a value of one if a lender has above median non-zero exposure to 2013 Maine MERS lending. The Exposure variable equals lender exposure to 2013 Maine MERS lending, standardized. Lender-tract level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. Bank level controls include total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation, the census tract, and the lender. Columns 2 and 5 include fixed effects for the census tract interacted with the lender. Columns 3 and 6 include fixed effects for the census tract interacted with year. Standard errors are reported in parentheses and are clustered by lender. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for bank loans originated in the New England area.

Table 4. Effect of the Greenleaf Judgment on Bank Loan Size Across MERS Status: Difference-in-Differences

	log(Loan Size)				
	(1)	(2)	(3)	(4)	(5)
Treat×Post	-0.186***			-0.423***	
	(0.066)			(0.155)	
Exposure×Post		-0.103***			-0.119**
		(0.035)			(0.055)
MERS×Post			0.000	0.011	0.030
			(0.045)	(0.024)	(0.053)
MERS×Treat×Post				0.404**	
				(0.183)	
MERS×Exposure×Post					0.146***
					(0.048)
Tract FE	X	X	X	X	X
Time FE	X	X	X	X	X
Lender FE	X	X	X	X	X
Lender×Tract FE	X	X	X	X	X
Tract×Time FE	X	X	X	X	X
Implied %△	-17%	-10%	—	-35%	-11%
Obs	95,299	95,299	95,299	95,299	95,299

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on bank loan size across MERS status. The outcome variable equals the log of loan size. The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treatment dummy takes a value of one if a lender has above median non-zero exposure to 2013 Maine MERS lending. The Exposure variable equals lender exposure to 2013 Maine MERS lending, standardized. The MERS dummy takes a value of one for MERS loans. Lender-tract level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. Bank level controls include total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation, the census tract, the lender, the census tract interacted with the lender, and the census tract interacted with year. Standard errors are reported in parentheses and are clustered by lender. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for bank loans originated in the State of New Hampshire.

Table 5. Effect of the Greenleaf Judgment on Bank Loan Size: Difference-in-Differences

	log(Loan Size)			
	(1)	(2)	(3)	(4)
Treat×Post	0.018			
	(0.057)			
Portfolio×Treat×Post	-0.241***		-0.233*	
	(0.092)		(0.128)	
Exposure×Post		0.042		
		(0.072)		
Portfolio×Exposure×Post		-0.219***		-0.318***
		(0.055)		(0.101)
Tract FE	X	X	X	X
Time FE	X	X	X	X
Lender FE	X	X	X	X
Lender×Tract FE	X	X	X	X
Tract×Time FE	X	X	X	X
Lender×Time FE			X	X
Implied %△	-22%	-20%	-21%	-28%
Obs	1,308,030	1,308,030	1,308,030	1,308,030

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on bank loan size. The outcome variable equals the log of loan size. The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treatment dummy takes a value of one if a lender has above median non-zero exposure to 2013 Maine MERS lending. The Exposure variable equals lender exposure to 2013 Maine MERS lending, standardized. The Portfolio dummy takes a value of one for portfolio loans. Lender-tract level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. Bank level controls include total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation, the census tract, the lender, the census tract interacted with the lender, and the census tract interacted with year. Columns 2 and 4 include fixed effects for the lender interacted with year. Standard errors are reported in parentheses and are clustered by lender. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for bank loans originated in the New England area.

Table 6. Effect of the Greenleaf Judgment on Bank Lending Across Size: Difference-in-Differences

	All Loans		Portfolio		Non-Portfolio	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Small Banks						
Treat×Post	0.221***		0.086		0.240***	
	(0.082)		(0.097)		(0.083)	
Exposure×Post		0.175		0.515		0.352***
		(0.122)		(1.596)		(0.108)
Implied %△	24%	18%	8%	—	27%	41%
Obs	91,734	91,734	43,615	43,615	60,931	60,931
Panel B: Big Banks						
Treat×Post	-0.211**		-0.390*		0.057	
	(0.097)		(0.213)		(0.151)	
Exposure×Post		-0.158*		-0.461***		0.142
		(0.083)		(0.081)		(0.125)
Implied %△	-19%	-15%	-34%	-37%	5%	14%
Obs	321,628	321,628	176,018	176,018	229,657	229,657
Tract FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Lender FE	X	X	X	X	X	X
Lender×Tract FE	X	X		X	X	X
Tract×Time FE	X	X		X	X	X

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on bank lending volume across bank size. Panel A limits the sample to banks with below median 2013 total assets. Panel B limits the sample to banks with above median 2013 total assets. The outcome variable equals the log of mortgage lending at the lender-tract-year level. The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treatment dummy takes a value of one if a lender has above median non-zero exposure to 2013 Maine MERS lending. The Exposure variable equals lender exposure to 2013 Maine MERS lending, standardized. Lender-tract level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. Bank level controls include total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation, the census tract, the lender, the census tract interacted with the lender (not Column 3), and the census tract interacted with year (not Column 3). Standard errors are reported in parentheses and are clustered by lender. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for bank loans originated in the New England area.

Table 7. Effect of the Greenleaf Judgment on Bank Lending Across Business Model: Difference-in-Differences

	All Loans		Portfolio Loans		Non-Portfolio Loans	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Retainers						
Treat×Post	-0.519***		-0.603***		-0.117	
	(0.135)		(0.175)		(0.106)	
Exposure×Post		-0.424***		-0.508***		0.098
		(0.031)		(0.039)		(0.159)
Implied %△	-41%	-35%	-46%	-40%	-12%	9%
Obs	124,912	124,912	102,001	102,001	42,650	42,650
Panel B: Sellers						
Treat×Post	-0.165		-0.147		0.046	
	(0.137)		(0.179)		(0.170)	
Exposure×Post		-0.064		-0.155		0.109
		(0.135)		(0.136)		(0.131)
Implied %△	-16%	-7%	-15%	-15%	3%	11%
Obs	295,536	295,536	120,688	120,688	252,877	252,877
Tract FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Lender FE	X	X	X	X	X	X
Lender×Tract FE	X	X	X	X	X	X
Tract×Time FE	X	X	X	X	X	X

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on bank lending volume across business model. Panel A limits the sample to banks with below median 2013 non-portfolio lending. Panel B limits the sample to banks with above median 2013 non-portfolio lending. The outcome variable equals the log of mortgage lending at the lender-tract-year level. The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treatment dummy takes a value of one if a lender has above median non-zero exposure to 2013 Maine MERS lending. The Exposure variable equals lender exposure to 2013 Maine MERS lending, standardized. Lender-tract level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. Bank level controls include total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation, the census tract, the lender, the census tract interacted with the lender, and the census tract interacted with year. Standard errors are reported in parentheses and are clustered by lender. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for bank loans originated in the New England area.

Appendix A Additional Results

Table A.1. Effect of the Greenleaf Judgment on Tract Level Lending: Difference-in-Differences

	All Loans		Portfolio		Non-Portfolio	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	-0.053***		-0.076***		-0.021*	
	(0.010)		(0.016)		(0.012)	
Exposure×Post		-0.031***		-0.051***		-0.012
		(0.007)		(0.009)		(0.008)
Tract FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Implied %△	-5%	-3%	-7%	-5%	-2%	-1%
Obs	22,899	22,899	22,521	22,521	22,764	22,764

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on tract level bank lending volume. The outcome variable equals the log of mortgage lending at the tract-year level. The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treatment dummy takes a value of one if a tract has above median volume-weighted bank exposure to 2013 Maine MERS lending. The Exposure variable equals volume-weighted bank exposure to 2013 Maine MERS lending, at the tract level and standardized. Tract level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level economic controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. Tract level bank controls include volume-weighted measures of total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further volume-weighted bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation and the census tract. Standard errors are reported in parentheses and are clustered by census tract. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for census tracts in the New England area.

Table A.2. Effect of the Greenleaf Judgment on Bank Balance Sheets: Difference-in-Differences

	Charge-Offs/Assets		NPL/Assets		Loan Loss Fund/Assets	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	-0.200***		-0.293*		-0.115**	
	(0.054)		(0.153)		(0.047)	
Exposure×Post		-0.033**		-0.029		-0.014
		(0.016)		(0.043)		(0.011)
Time FE	X	X	X	X	X	X
Lender FE	X	X	X	X	X	X
Obs	12,892	12,892	12,892	12,892	12,892	12,892

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on bank balance sheets. The outcome variables are named at the top of each column and include charge-offs, non-performing loans, and total loan loss fund. Each variable is scaled by total assets at the lender-quarter level and multiplied by 100 for ease of interpretation. The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treat dummy takes a value of one if a lender has above median non-zero exposure to 2013 Maine MERS lending. The Exposure variable equals lender exposure to 2013 Maine MERS lending, standardized. Bank level controls include total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation and the lender. Standard errors are reported in parentheses and are clustered by lender. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for banks originating loans in the New England area.

Table A.3. Effect of the Greenleaf Judgment on Non-Portfolio Lending (Maine): Difference-in-Differences

	log(Loan Volume)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	0.078	0.135	0.108			
	(0.089)	(0.115)	(0.113)			
Exposure×Post				0.010	0.023	0.016
				(0.017)	(0.024)	(0.024)
Tract FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Lender FE	X	X	X	X	X	X
Lender×Tract FE		X	X		X	X
Tract×Time FE			X			X
Implied %△	8%	14%	11%	1%	2%	2%
Obs	23,027	23,027	23,027	23,027	23,027	23,027

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on non-portfolio lending in the State of Maine. The outcome variable equals the log of mortgage lending at the lender-tract-year level. The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treatment dummy takes a value of one if a lender has above median non-zero exposure to 2013 Maine MERS lending. The Exposure variable equals lender exposure to 2013 Maine MERS lending, standardized. Lender-tract level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. Bank level controls include total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation, the census tract, and the lender. Columns 2 and 5 include fixed effects for the census tract interacted with the lender. Columns 3 and 6 include fixed effects for the census tract interacted with year. Standard errors are reported in parentheses and are clustered by lender. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for bank loans originated in the State of Maine.

Table A.4. Effect of the Greenleaf Judgment on Non-Bank Lending: Difference-in-Differences

	log(Loan Volume)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	0.144*	0.233**	0.221**			
	(0.081)	(0.104)	(0.111)			
Exposure×Post				0.027	0.111*	0.094
				(0.048)	(0.065)	(0.063)
Tract FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Lender FE	X	X	X	X	X	X
Lender×Tract FE		X	X		X	X
Tract×Time FE			X			X
Implied %△	15%	26%	24%	3%	12%	10%
Obs	350,058	350,058	350,058	350,058	350,058	350,058

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on non-bank lending volume. The outcome variable equals the log of mortgage lending at the lender-tract-year level. The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treatment dummy takes a value of one if a lender has above median non-zero exposure to 2013 Maine MERS lending. The Exposure variable equals lender exposure to 2013 Maine MERS lending, standardized. Lender-tract level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. All columns include fixed effects for the year of observation, the census tract, and the lender. Columns 2 and 5 include fixed effects for the census tract interacted with the lender. Columns 3 and 6 include fixed effects for the census tract interacted with year. Standard errors are reported in parentheses and are clustered by lender. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for non-bank loans originated in the New England area.

Table A.5. Sorting Across Banks and Non-Banks Based on Exposure to Greenleaf Judgment

	log(Tract Bank Exposure)	
	(1)	(2)
Log(Tract Nonbank Exposure)	0.209***	
	(0.033)	
Log(Lead Nonbank Exposure)		0.023**
		(0.009)
Obs	2,941	2,805

Note: This table reports estimates of the correlation between bank exposure and non-bank exposure. The outcome variable measures the logarithm of tract-level average bank exposure using market share as weights. Tract non-bank exposure measures the logarithm of tract-level average non-bank exposure, weighted by market share. Lead non-bank exposure measures the logarithm of tract-level average non-bank exposure, weighted by market share. Exposure equals 2013 Maine MERS lending as a share of a lenders overall New England loan volume. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers 2013 for census tracts in the New England area.

Table A.6. Effect of the Greenleaf Judgment on County Level Delinquency: Difference-in-Differences

	Delinquency Rate	
	(1)	(2)
Treat×Post	-0.236**	
	(0.092)	
Exposure×Post		-0.250***
		(0.065)
County FE	X	X
Time FE	X	X
Obs	2,340	2,340

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on county level mortgage delinquency. The outcome variable measures the 30-89 day mortgage delinquency rate at the county-month level. The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treat dummy takes a value of one if a county has above median volume-weighted bank exposure to 2013 Maine MERS lending. The Exposure variable equals volume-weighted bank exposure to 2013 Maine MERS lending, at the county level and standardized. County level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level economic controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. County level bank controls include volume-weighted measures of total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further volume-weighted bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation and the county. Standard errors are reported in parentheses and are clustered by county. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for counties in the New England area.

Table A.7. Effect of the Greenleaf Judgment on Denial Rates Within 5% of CLL Limit: Difference-in-Differences

	All Loans		Low DTI		High DTI	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	-0.026		0.026		-0.081	
	(0.030)		(0.062)		(0.050)	
Above Limit×Treat×Post	0.059**		-0.034		0.100*	
	(0.024)		(0.069)		(0.057)	
Exposure×Post		-0.015		-0.010		-0.062
		(0.033)		(0.046)		(0.059)
Above Limit×Exposure×Post		0.090***		0.027		0.169***
		(0.030)		(0.056)		(0.062)
Tract FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Lender FE	X	X	X	X	X	X
Lender×Tract FE	X	X	X	X	X	X
Tract×Time FE	X	X	X	X	X	X
Obs	49,892	49,892	20,194	20,194	29,698	29,698

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on denial rates. The outcome variable equals one if a loan is rejected and zero otherwise. The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treatment dummy takes a value of one if a lender has above median non-zero exposure to 2013 Maine MERS lending. The Exposure variable equals lender exposure to 2013 Maine MERS lending, standardized. The Above Limit dummy takes a value of one when loan size is above the conforming loan limit. Lender-tract level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. Bank level controls include total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation, the census tract, the lender, the census tract interacted with the lender, and the census tract interacted with year. Standard errors are reported in parentheses and are clustered by lender. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for bank loans originated in the New England area that are within 5 percent of the conforming loan limit.

Table A.8. Effect of the Greenleaf Judgment on Bank Portfolio Lending Across Distance: Difference-in-Differences

	log(Loan Volume)			
	(1)	(2)	(3)	(4)
Treat×Post	-0.447**			
	(0.201)			
Near×Treat×Post	-0.453***		-0.309***	
	(0.072)		(0.105)	
Exposure×Post		-0.426***		
		(0.074)		
Near×Exposure×Post		-0.171*		-0.231***
		(0.095)		(0.042)
Tract FE	X	X	X	X
Time FE	X	X	X	X
Lender FE	X	X	X	X
Lender×Tract FE	X	X	X	X
Tract×Time FE	X	X	X	X
Lender×Time FE			X	X
Implied %△	-37%	-16%	-27%	-21%
Obs	217,290	217,290	217,290	217,290

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on bank portfolio lending volume across distance. The outcome variable equals the log of mortgage lending at the lender-tract-year level. The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treatment dummy takes a value of one if a lender has above median non-zero exposure to 2013 Maine MERS lending. The Exposure variable equals lender exposure to 2013 Maine MERS lending, standardized. The Near dummy takes a value of one for tracts within the first distance decile from Maine's capital. Lender-tract level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. Bank level controls include total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation, the census tract, the lender, the census tract interacted with the lender, and the census tract interacted with year. Columns 2 and 4 include fixed effects for the lender interacted with year. Standard errors are reported in parentheses and are clustered by lender. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for bank portfolio loans originated in the New England area.

Table A.9. Effect of the Greenleaf Judgment on Tract Level Small Business Lending: Difference-in-Differences

	Log(Large Loan Volume)		Log(Small Loan Volume)	
	(1)	(2)	(3)	(4)
Treat×Post	0.047**		-0.007	
	(0.022)		(0.013)	
Exposure×Post		0.028**		0.001
		(0.011)		(0.008)
Tract FE	X	X	X	X
Time FE	X	X	X	X
Implied %△	5%	3%	-1%	0%
Obs	15,998	15,998	22,890	22,890

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on tract level small business lending volume. The outcome variable equals the log of small business lending volume at the tract-year level. The outcome variables are named at the top of each column, measuring large loan volume (loans of sizes between $250,000 and $1,000,000) and small loan volume (loans of sizes below $250,000). The Post dummy takes a value of one if an outcome is observed on or after the year of the judgment (2014). The Treatment dummy takes a value of one if a tract has above median volume-weighted bank exposure to 2013 Maine MERS lending. The Exposure variable equals volume-weighted bank exposure to 2013 Maine MERS lending, at the tract level and standardized. Tract level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level economic controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. Tract level bank controls include volume-weighted measures of total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further volume-weighted bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation and the census tract. Standard errors are reported in parentheses and are clustered by census tract. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for census tracts in the New England area.

Table A.10. Effect of the Greenleaf Judgment on House Prices and Employment: Difference-in-Differences

	Log(House Price Index)		Unemployment Rate	
	(1)	(2)	(3)	(4)
Treatment×Post	-0.026***		0.899***	
	(0.005)		(0.189)	
Exposure×Post		-0.013***		0.437***
		(0.002)		(0.100)
Geography FE	X	X	X	X
Time FE	X	X	X	X
Implied %△	-3%	-1%	—	—
Obs	8,470	8,470	4,590	4,590

Note: This table reports difference-in-difference estimates of the effect of the Greenleaf judgment on real effects. In Columns 1 and 2, the outcome variable measures the log of the house price index at the zip-code-year level. In Columns 3 and 4, the outcome variable measures the unemployment rate at the county-month level. The Post dummy takes a value of one if an outcome is observed on or after the year or month of the judgment (July 2014). The Treatment dummy takes a value of one if a geography has above median volume-weighted bank exposure to 2013 Maine MERS lending. The Exposure variable equals volume-weighted bank exposure to 2013 Maine MERS lending, at the geography level and standardized. Geography level borrower controls include percent of loans issued to female borrowers, black borrowers, one to four-family homes, conventional mortgages, and principal dwellings. County level economic controls include the employment to population ratio, log of GDP, year-end unemployment rate, log of aggregate income, and log of HPI. Geography level bank controls include volume-weighted measures of total deposits, liquid assets, tangible equity, commercial lending, real estate lending, and net income, all scaled by total assets. Further volume-weighted bank controls include the core deposit rate and log of total assets. All columns include fixed effects for the year of observation and the geography. Standard errors are reported in parentheses and are clustered by geography. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of 2010 to 2017 for the New England area.