

Collateral Enforcement and the Secondary Market

by

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Department of Business Administration
Duke University

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David T. Robinson

David W. Berger

Dissertation submitted in partial fulfillment of the requirements for the degree of
Doctor of Philosophy in the Department of Business Administration
in the Graduate School of Duke University
2023

ABSTRACT

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Abstract

This dissertation investigates the role of the secondary market in the enforcement of a creditor's security interest. In the first chapter, I examine how creditors respond to ex-post higher foreclosure costs. I find that when repossessing collateral becomes costly, creditors choose to sell their delinquent debt on the secondary market rather than renegotiate with borrowers. Only when repossession becomes prohibitively expensive, thus impeding sale, do creditors offer forbearance. These results highlight a novel channel to explain the lack of mortgage renegotiation, namely, the very presence of a robust secondary market. Subsequent chapters further explore this relationship. In the second chapter, I study how banks respond to riskier collateral enforcement ex-ante. I find that banks exposed to enforcement risk reduce lending for portfolio loans, which are precisely those loans that banks are liable to enforce. In the third chapter, I study how creditors respond to the delayed sale of delinquent debt. I find that suspending the early sale of delinquent debt reduces the incidence of forbearance, increases foreclosures, and limits creditor incentives to cure loans.

Dedication

To my parents and my wife, for your enduring love and support

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Acknowledgements

I begin by thanking God and sending peace and blessings upon His Noble Messenger.

Next, I would like to thank my advisor, Manuel Adelino, and my committee members Sasha Indarte, David Robinson, and David Berger. By challenging me to improve, my committee developed me into the researcher that I am today. Anything of value from my work is a reflection of their dedication and patience.

I am grateful to the faculty at Duke for generously offering me their time and support throughout my graduate studies. I have appreciated the particular interest of Nuno Clara, Simon Gervais, John Graham, and David Hsieh, all of whom were excellent mentors and friends. Completing my various milestones would also not have been possible without the generous help of MJ Oles. I am also thankful to my professors from college, the staff at the Federal Reserve Board, and the economists at the FDIC.

I would like to thank James Pinnington and Chenyu Wang for sticking through this six-year journey with me. I will always remember our time both in and out of the classroom very fondly. To the rest of my friends at Duke, I have sincerely appreciated your company and kindness.

I am also grateful for the continuous help of my economist friends outside of Duke who would offer me advice and grounding throughout my studies. In particular, Meraj Allahrakha, Jack Bao, Sergio Correia, Ahmet Degerli, Gazi Kabas, Gazi Kara, Ben McCartney, and Brett McCully would never hesitate to answer my phone call during

times of great need. Along these lines, I am also indebted to Attorney Thomas Cox for spending countless hours over the past few years reviewing details of the law with me to make this dissertation possible. Outside of academia, I would also like to thank my teachers in religion for providing spiritual grounding throughout my studies.

Lastly, I would like to thank my family, my parents, my wife, my parents-in-law, my sisters, and my brothers. In particular, I am grateful to my mother and father for encouraging me throughout my life to succeed. They didn't need to know the details of my path to know that they wanted the best for me. I am also indebted to my wife for her continuous support and companionship. Her patience through my graduate studies has been a testament to her character and I am forever grateful for it. Finally, I would like to thank my daughter and my son for bringing our family unfathomable joy. I pray that you both will always be the coolness of our eyes.

Introduction

This dissertation investigates the role of the secondary market in the enforcement of a creditor's security interest. During the financial crisis, families lost their homes in record numbers even as governments imposed significant costs on collateral enforcement. In the first chapter, I seek to understand why creditors failed to renegotiate their mortgage debt more often in the face of such costly enforcement. Prior research has argued that securitization, financial constraints, and information asymmetries explain the paucity of renegotiation. I identify a novel channel to explain this puzzle. Exploiting quasi-experimental variation generated by Maine's 2014 Greenleaf judgment, I show that when repossessing collateral becomes costly, creditors choose to sell their delinquent debt on the secondary market rather than renegotiate with borrowers. Only when repossession becomes prohibitively expensive, thus impeding sale, do creditors offer forbearance. These results suggest that the demand for delinquent debt and the foreclosure appetite of a loan's future creditors is key to assessing the full impact of forbearance policies.

If creditors respond to enforcement costs ex-post by selling their debt, then how do lenders respond to uncertainty over enforcement costs ex-ante? While the extant

literature provides evidence that uncertainty, in general, can curtail the credit supply, in the second chapter, I study how banks respond to risk in the enforcement of collateral itself. Maine's Greenleaf judgment once again provides a unique setting to test uncertainty in enforcement. While the judgment increased enforcement costs on existing creditors, it mechanically left new lenders unaffected. Nevertheless, I show that originating banks exposed to the judgment restricted lending by 21%, almost exclusively for portfolio loans not intended for sale. This result implies that when banks face uncertainty over collateral enforcement, they will curtail lending on precisely those loans that they are liable to enforce.

If delinquent debt sale can help creditors offload debt when enforcement becomes costly, then what happens to enforcement when delinquent debt sale becomes delayed? In the third chapter, I study how the suspension of an early buyout option on delinquent debt affects foreclosure outcomes. I exploit an unanticipated 2002 Ginnie Mae policy change that suspended the right for debt issuers to buy out their loans from MBS pools before entering severe delinquency. Prior to the policy change, these issuers could buy this debt from initial investors when it was in early delinquency, help the borrower exit default, and resell the debt to new investors. I find that the suspension of this early buyout option led to an increase in foreclosures due to a reduction in forbearance on the part of issuers. These results suggest that early buyouts help financially constrained borrowers avoid foreclosure and give issuers incentives to preserve loan quality to resell delinquent debt.

Red Tape, Greenleaf: Creditor Behavior Under Costly Collateral Enforcement

2.1 Introduction

The ability to repossess collateral plays a central role in determining the credit supply (Rampini and Viswanathan, 2010; Fostel and Geanakoplos, 2015). Nevertheless, regulations intended to protect borrowers impose significant costs on creditors enforcing their security interest (Pence, 2006). Ultimately, these costs may fall short of preventing foreclosure or motivating renegotiation, as evidenced by the 2008 financial crisis (Piskorski and Seru, 2021). A deeper understanding of how the cost of enforcement affects creditor behavior is critical to policymakers aiming to target relief to borrowers.

Using a hand-collected dataset of mortgages and a quasi-experimental research design, I study how higher foreclosure costs affect creditor behavior. I show that when foreclosure becomes costly, creditors choose to sell their loans on the secondary market instead of renegotiating with borrowers. However, I find that this market unwinds for loans with prohibitively high foreclosure costs, at which point the value

of renegotiation exceeds that of costly foreclosure. For these loans that are too expensive to foreclose upon, I identify a stark reduction in delinquency that resembles forbearance on the part of creditors.

Causally identifying these effects is empirically challenging for several reasons. First, in the cross-section, local economic conditions are often correlated with both enforcement costs and loan outcomes. Second, over time, creditors may anticipate an increase in foreclosure costs following lengthy efforts to enact debt relief policy. Finally, a creditor's choice to foreclose is an equilibrium outcome that occurs jointly with a borrower's choice to default. Disentangling these two dynamics is difficult, even when relying on exogenous changes in foreclosure costs.

To explicitly 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 Greenleaf judgment increased foreclosure costs for creditors holding mortgage contracts associated 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 the ultimate creditor. The Greenleaf judgment suspended a creditor's right to foreclose due to an unorthodox and unexpected interpretation of a single sentence within the standard MERS mortgage contract. The judgment required affected creditors to request reassignment of the original mortgage contract from the original lender. Only then would creditors of MERS loans regain their legal standing to foreclose. The ruling imposed (1) non-pecuniary costs associated with searching for original lenders and (2) material legal costs in obtaining mortgage assignments from lenders that filed for bankruptcy.

The Greenleaf judgment provides an appealing empirical setting to identify the causal effect of foreclosure costs for several reasons. First, the judgment hinged upon the unorthodox interpretation of a single sentence. The subtle variation in the

construction of this sentence was random relative to other similar sentences across the country and, by definition, independent of loan outcomes. Furthermore, the judgment was unanticipated, as it overturned a century of Maine common law and occurred well after the financial crisis. Finally, the judgment was not an advertised government policy and was highly technical in nature, plausibly reducing the salience of moral hazard among borrowers.

I show that when foreclosure costs rise, creditors sell their loans on the secondary market in lieu of renegotiating with borrowers. For two years following the Greenleaf judgment, I estimate that the foreclosure rate among MERS loans relative to non-MERS loans dropped by at least 26% (0.088 pp). Simultaneously, the rate of delinquent debt sales increased by 57% (0.051 pp) relative to the baseline for treated loans. These effects are robust to the inclusion of loan-level fixed effects, exploiting the panel structure of my data to account for unobserved heterogeneity correlated with selection into MERS. Finally, I find no evidence of explicit modifications or short sales.

Critical to my research design, I find that higher foreclosure costs did not cause the delinquency rate among MERS loans to increase relative to non-MERS loans. If borrowers with MERS loans were fully aware of the Greenleaf judgment, then the delinquency rate should rise due to moral hazard on the part of borrowers (Mayer et al., 2014; O'Malley, 2021). Instead, I find that the differential effect of the Greenleaf judgment on the delinquency rate of MERS loans is small and statistically insignificant. The estimated effect remains insignificant even after accounting for investor status, past delinquency, and financial shocks. These results suggest that the effect of the judgment was likely not salient among borrowers, plausibly due to the technical nature of the ruling.

To explore the demand side for delinquent debt, I document several facts using a sample of MERS loan sales recorded in the Maine Registry of Deeds. First, I find that

only 25% of these loans enter initial foreclosure after an average waiting period of 36 months since the initial sale. This suggests that the Greenleaf judgment had a lasting impact on affected mortgages, despite being sold on the secondary market. Second, I find that 90% of loans are initially purchased by just three investors and Fannie Mae. Fannie Mae likely purchases these loans due to its capacity to identify original lenders (Housing Finance Policy Center, 2022), its large security interest in residential real estate (Favara and Giannetti, 2017), and its role as an intermediary of debt sale (Lane, 2018). Finally, outside of Fannie Mae, these investors exhibit substantial heterogeneity in foreclosure appetite that correlates with purchase volume. Loans sold to the largest buyer experience an initial foreclosure 30% more often and two years sooner than those loans sold to the second largest investor.

To explore the mechanism driving the effect of the Greenleaf judgment, I test how changes in loan outcomes correlate with measures of original lender quality. Underlying my main results is the assumption that the cost of foreclosure reflects the difficulty of finding the original lender to obtain a mortgage assignment. To that end, my results should be concentrated on loans originated by lenders that are relatively unresponsive or out of business. If an original lender filed for bankruptcy, creditors faced several complications that made obtaining reissued mortgages prohibitively expensive. These issues included ambiguity around the power of attorney, the location of a debtor's assets, and the cooperation of the trustee presiding over the case.

I find that, even after accounting for loans originated in the same year, the effect of the Greenleaf judgment is concentrated on lenders operating in the peak boom years prior to the financial crisis. These results are consistent with market players in the early part of the boom being more robust and established than those that came in the waning years (Drechsler et al., 2021; Mian and Sufi, 2022). I further find evidence suggesting that the reduction in foreclosure is driven by subprime lenders. Once again, under the assumption that this reflects search costs in locating the original

lender to assign the mortgage following the Greenleaf judgment, my results support the notion that subprime lenders were financially constrained and likely shuttered following the 2008 financial crisis (Purnanandam, 2010).

I further explore this channel by explicitly identifying lenders in my sample that filed for bankruptcy. I estimate my main specification using indicator variables that distinguish between MERS loans originated by lenders with and without a bankruptcy filing. If current creditors faced higher costs when the original lender filed for bankruptcy, then I would expect monotonically larger effects for loans issued by these high foreclosure cost MERS lenders.

Ultimately, I find that the effect of foreclosure costs on loan sales is not monotonic. I estimate that loans originated by bankruptcy filers experience a drop in foreclosure that is four times larger than the drop for other MERS loans. However, I do not find a similarly larger increase in sales. Instead, delinquent debt sales exclusively increased for loans issued by lenders without a bankruptcy filing, one-for-one relative to the drop in foreclosure. For loans issued by bankruptcy filers, I instead find a reduction in delinquencies that reflects the drop in foreclosure.

These results suggest that there was no demand for a subset of MERS loans that were prohibitively expensive to foreclose upon. Instead, creditors likely offered forbearance to this subset, evidenced by the reduction in delinquency. Given that creditors prefer lower enforcement costs, my estimates reveal a pecking order of creditor behavior under costly collateral enforcement. Namely, creditors prefer selling delinquent debt rather than renegotiating, offering forbearance only when foreclosure becomes prohibitively expensive and sale is impeded.

As a final step, I validate the robustness of my results by addressing concerns over selection into MERS. First, I expand my sample to include loans from the neighboring state of New Hampshire. If MERS loans differed systematically from non-MERS loans in the period following the Greenleaf judgment, this would be reflected in loan

outcomes across the two states. However, I find that common trends across the two states fail to explain the drop in foreclosure and increase in loan sales. In a second test, I restrict identification to within lender variation across the two states. Once again, I find that the drop in foreclosure and increase in sales cannot be explained by common shocks across MERS loans originated by the same lender. Finally, in a third exercise, I conduct a placebo test using Maine MERS loans that did not include the key sentence from the Greenleaf judgment. If MERS loans differed from non-MERS loans in a manner that correlated with the timing of the judgment, then this would be reflected in both treated and placebo loans. Instead, I find that these untreated loans did not experience a drop in foreclosure.

Taken together, I show that, when faced with higher foreclosure costs, creditors sell their loans on the secondary market instead of renegotiating with borrowers. I find that only when loans become prohibitively expensive to foreclose upon, thus impeding sale, do creditors offer forbearance. This likely occurs when the value of renegotiation exceeds that of costly foreclosure. Ultimately, these results suggest that policymakers should consider how enforcement costs influence initial creditor behavior as well as future investor demand.

Related Literature

This paper contributes to three strands of the literature. First, it contributes to the literature on creditor decision making around renegotiation (Roberts and Sufi, 2009; Nini et al., 2012). Roberts (2015) estimates that the typical bank loan is renegotiated every nine months. In contrast, modifications in the mortgage market have been persistently rare (Ghent, 2011). Explanations proposed by the literature for this paucity of modifications include securitization (Piskorski et al., 2010; Agarwal et al., 2011; Kruger, 2018), financial constraints (Aiello, 2022), and asymmetric information (Adelino et al., 2013). In this paper, I focus on a novel channel, namely,

selling delinquent debt on the secondary market. By exploiting heterogeneity in enforcement costs, I show that the secondary market functions as a direct substitute for renegotiation. This explains the lack of renegotiation in the presence of a robust secondary market—if investors offer a price higher than the value of renegotiation, a creditor will choose to sell their debt.

Second, I contribute to the literature on mortgage debt relief policy.¹ Following the 2008 financial crisis, state governments implemented foreclosure moratoria that likely led to reduced foreclosure rates (Collins and Urban, 2018; Gabriel et al., 2020; Artavanis and Spyridopoulos, 2022). However, the efficacy of these policies has been called into question. First, foreclosure suspension can lead to moral hazard on the part of borrowers, as borrowers may choose to default strategically and benefit from government-mandated debt relief (Mayer et al., 2014; O’Malley, 2021). Second, these policies may simply delay foreclosure instead of preventing it outright (Gerardi et al., 2013). I contribute to this literature by identifying the precise mechanism through which foreclosure may be delayed. Namely, if the secondary market is composed of primarily specialized, patient investors, then delinquent debt sales may mute the long-term effects of a temporary foreclosure suspension.

This insight sheds new light on the efficacy of crisis-era policies promoting borrowers to refinance (HARP) and creditors to offer mortgage modifications (HAMP). HARP helped three million eligible borrowers refinance, expanded the breadth of debt instruments, and reduced delinquency (Agarwal et al., 2015; Ehrlich and Perry, 2015; Karamon et al., 2017; Abel and Fuster, 2021). HAMP similarly reduced foreclosure for about 600,000 borrowers and delinquencies more broadly (Hembre, 2014; Scharlemann and Shore, 2016; Agarwal et al., 2017; Ganong and Noel, 2020). However, these policies likely produced different types of future investor demand; the latter for refi-

¹ Indarte (2022) provides a systematic review of the costs and benefits of consumer debt relief more broadly.

nanced debt and the former for seasoned debt with a delinquency history. My paper suggests that the foreclosure appetite of a loan’s future creditors is key to assessing the full impact of forbearance policies. Indeed, recent research corroborates the importance of a creditor’s forbearance propensity in the context of COVID-19 policies (Cherry et al., 2021; Kim et al., 2021).² My work builds on this by emphasizing the role of future creditors.

Third, my paper contributes to a broad literature on loan sales. To explain the differences in loan performance between loans retained on lender balance sheets and those sold to investors, the literature provides several explanations related to adverse selection, moral hazard, and reputation building (Drucker and Puri, 2008; An et al., 2011; Jiang et al., 2013; Begley and Purnanandam, 2016; Hartman-Glaser, 2017; Adelino et al., 2019; Ashcraft et al., 2019). More relevant to this paper, there exists a strand of the literature that studies the sale of seasoned debt. In this context, creditors sell their debt when facing liquidity shortfalls (Irani and Meisenzahl, 2017), stricter capital requirements (Irani et al., 2020), and leverage constraints (Kundu, 2021; Elkamhi and Nozawa, 2022). Complementing the previous literature, my paper focuses on the sale of delinquent debt when collateral enforcement becomes costly.³

The closest research to my paper is contemporaneous work by Giannetti and Meisenzahl (2021), who also study the relationship between the secondary market and renegotiation. Giannetti and Meisenzahl (2021) find that when banks face regulatory pressure to sell loans, investors with existing loan shares buy this debt, increasing

² Recent work by Cherry et al. (2021) demonstrates that 70 million borrowers holding \$2.3 trillion dollars of mortgage debt benefited from government-provided forbearance during the COVID-19 pandemic. This debt relief benefited those borrowers most impacted by the pandemic (An et al., 2021; Gerardi et al., 2022) and generated positive externalities, such as increasing refinancing rates (Capponi et al., 2021) and house prices (Anenberg and Scharlemann, 2021).

³ Several papers study the demand side of these transactions, examining the role of distressed debt investors in commercial credit (Hotchkiss and Mooradian, 1997; Jiang et al., 2012; Ivashina et al., 2016; Feldhütter et al., 2016) and debt collectors in consumer lending (Fedaseyev, 2020; Cheng et al., 2020; Romeo and Sandler, 2021; Fonseca, 2022).

their concentration in a manner that favors renegotiation. In contrast, I focus on the relationship between renegotiation and debt sale under costly collateral enforcement, not regulatory pressure. Therefore, in my context, initial creditors are not forced by regulation but, rather, have a choice to renegotiate or sell their debt. I find that creditors overwhelmingly choose to sell their debt, only renegotiating when the secondary market unwinds because of prohibitively expensive foreclosure. Hence, I identify a novel mechanism whereby access to the secondary market functions to replace the initial creditor’s need to renegotiate. Importantly, both papers highlight that the benefit of debt sale fundamentally depends on the composition of the demand side. In the setting observed by Giannetti and Meisenzahl (2021), the demand side already has an established relationship with the debtor and likely renegotiates with the borrower. In my setting, the demand side is dominated by investors specializing in foreclosure.

The remainder of this paper proceeds as follows. Section 2.2 describes the institutional details of MERS and the Maine Greenleaf judgment. Section 4.3 describes the data, sample construction, and summary statistics. Section 2.4 estimates the effect of the Greenleaf judgment on loan outcomes. Section 4.6 explores the mechanism driving the effect of the Greenleaf judgment. Section 4.7 investigates the robustness of my results. Section 4.9 concludes.

2.2 Institutional Background

In this section, I outline the institutional details related to the Mortgage Electronic Registration Systems (MERS) and the Greenleaf judgment of July 2014. In the first subsection, I outline the development of MERS as a response to the traditional method of recording mortgage contracts in the public registry of deeds. In the second subsection, I describe the details of the Greenleaf judgment, its implications, and attempts to ameliorate its fallout through the law.

2.2.1 Mortgage Electronic Registration Systems

A mortgage loan is the combination of two legal contracts. The first of these contracts is a promissory note, which establishes the obligation of the borrower to repay the owner of the promissory note. The second document is referred to as the mortgage contract securitizing the promissory note. This security interest grants the owner of the promissory note the right to foreclose on a borrower's home upon failure to repay the original loan.

When a lender chooses to sell a loan on the secondary market, the buyer must take possession of the note,⁴ while the seller must publicly register the mortgage contract to the new owner. This public registration is known as a mortgage assignment. To foreclose on a home after default, the current owner of the promissory note must be registered in the public registry of deeds as the rightful mortgagee. Panel A of Figure 2.1 provides a graphical representation of this traditional method of selling a mortgage loan.

Legal scholars have debated the necessity of this process for selling mortgage debt under the common law assumption that “the mortgage follows the note.” Under certain conditions, the promissory note itself guarantees a loan's security interest. In the context of the mortgage market, this would imply that a mortgage assignment is unnecessary. Hunt et al. (2011) posit that the continued concern over mortgage assignment likely relates to doubts over whether a note alone can supersede junior claims on real property when the first lien is unrecorded. In its most extreme example, as in the 2011 Massachusetts Ibanez judgment, state courts may demand that all mortgage assignments be recorded prior to foreclosure. In such a scenario, without registering proper assignment of the mortgage contract, the current owner of the promissory note essentially holds an unsecured consumer loan. Here, the promissory

⁴ In the case of electronic documents, this is referred to as authoritative control.

note alone will not guarantee the legal right to foreclose on a borrower’s home.

In response to this dated system of mortgage assignment, the Mortgage Electronic Registration Systems (MERS) was created to function as a private registry of deeds. Panel B of Figure 2.1 details the process by which a lender sells a mortgage loan with the assistance of MERS. After the introduction of MERS, a lender could now originate a loan jointly with MERS and split the two legal contracts originally bound together. The lender would hold the promissory note while registering MERS as the mortgagee in the public registry of deeds. Thereafter, MERS would never assign the mortgage contract to new owners of the promissory note until necessary, such as upon a loan discharge, foreclosing on the home, or sale of the promissory note to a non-MERS member. Upon any of these events, MERS would assign the mortgage contract in the public registry of deeds to the ultimate owner of the promissory note, thereby ending its responsibility of holding on to the mortgage contract.

MERS replaced the need for constantly assigning the mortgage contract at the public registry following every new sale of the promissory note. While the promissory note changed hands from one owner to the next, the mortgage contract stayed with MERS. The introduction of MERS greatly reduced the transaction cost of extending mortgage credit and allowed lenders to expand the volume and number of new loans (Lewellen and Williams, 2021). Indeed, Berg et al. (2022) identify operational improvements as one of the primary goals of financial technology adoption more broadly (Fuster et al., 2019; Buchak et al., 2018).

2.2.2 The Greenleaf judgment

The contract used to originate loans with MERS as the nominee for assignment was virtually equivalent across all 50 states. For one of these states, Maine, a single sentence in an almost twenty-page document read: “For purposes of recording this mortgage, MERS is the mortgagee of record.” Panel A of Figure 2.2 displays an

excerpt of this declaration from a sample Maine MERS mortgage contract.

This sentence outlined what all parties knew to be true at the time, that MERS “executes and publicly records a written assignment of mortgage to the foreclosing entity” (Boudreau et al., 2020). For example, Panel B of Figure 2.2 displays the analogous sentence in a sample New Hampshire MERS mortgage contract. Here, a variation of the key declaratory sentence reads: “MERS is the mortgagee under this Security Instrument.” Ultimately, there was no meaningful rationale in any given state’s MERS mortgage contract behind the particular construction of this declaratory sentence. To corroborate this point, Hunt et al. (2011) analyze MERS mortgage forms across the US and find that these contracts at the time did not display any legally problematic language. In sum, creditors holding MERS loans did not suspect any serious legal consequence from subtle differences in wording.

However, in July 2014, the Supreme Judicial Court of Maine ruled in *Bank of America, N.A. v. Scott A. Greenleaf et al.* (the Greenleaf judgment) that all Maine MERS mortgage contracts were never properly assigned due to Maine’s key declaratory sentence mentioned above. The Court interpreted the sentence to mean that MERS only held the right to record and not the right to assign the mortgage contract. While the difference in wording seems subtle, the implication meant that the original lender failed to grant MERS the right to assign the mortgage to all future note holders. Therefore, while the promissory note had passed from one owner to the next, the mortgage contract securitizing the note legally never left the original lender. The original lender was the only other party with whom the mortgage was registered.

This ruling was unexpected and monumental, affecting 15 years of mortgage originations. In one critique of the judgment, Aromando (2014) argues that it was “illogical to require ‘ownership’ of the mortgage, separate and distinct from the note, as a condition of standing to foreclose. Maine law has been clear on this for many years:

the mortgage follows the note.” In private conversation with the defense attorney who won the case himself, I find corroborating evidence that attorneys across Maine were dumbfounded. To be explicit, Maine would have gone the way of every other state in its treatment of MERS if not for the Greenleaf judgment. In Section 2.6.5, I provide greater detail on the litigation history of MERS, the legal basis for the Greenleaf judgment, and evidence that the ruling served as a radical departure from precedence and expectation.

For non-MERS mortgage loans, the ruling resulted in no change. These traditional mortgage contracts did not include MERS as a nominee of assignment, nor did they have the particular language outlined in the Greenleaf judgment. More importantly, these non-MERS mortgage contracts were assigned in a traditional manner through the public registry of deeds. The right to foreclose was preserved because the preceding owner of the promissory note publicly recorded the assignment of the mortgage contract to the subsequent owner of the promissory note.

For MERS mortgage contracts, however, current owners of the promissory note could not receive mortgage assignment at the time of foreclosure because they relied on MERS to grant them that assignment. Following the Greenleaf judgment, Maine no longer recognized MERS as a valid party to assign the mortgage for no reason other than an unorthodox interpretation of a single sentence within the mortgage contract.

Since the court believed that the mortgage contract was never properly assigned to MERS in the first place, the mortgage contract never left the ownership of the original lender, even though the promissory note changed hands. Therefore, the current owner of the promissory note would have to obtain a proper assignment from the original lender. While in some instances, this process would take a few months, obtaining a proper assignment became increasingly costly if the original lender was difficult to find or shut down. MERS mortgage contracts now carried both material legal costs,

as well as non-pecuniary costs associated with waiting on collateral enforcement.

In the wake of the Greenleaf judgment, Maine’s legislature aimed to validate past transactions that relied on MERS while maintaining the benefit to consumers the decision entailed. Therefore, past foreclosure proceedings involving MERS and home purchases that required MERS to execute an assignment were granted clear title. This meant that homeowners were shielded from uncertainty around their property rights due to circumstances outside of their control. However, the legislation required “future foreclosures to meet the requirements of the Greenleaf decision.”⁵ Thus, all MERS-related foreclosure proceedings that were placed on hold following the judgment and any filings following the new legislation would still require assignment from the original lender.

At the time of this writing, eight years after the Greenleaf judgment, there exists no ruling, neither judicial nor legislative, that has granted note holders a means to foreclose without obtaining a mortgage assignment from the original lender. Nevertheless, certain justices in Maine’s High Court have begun to openly question the legal basis for the Greenleaf ruling. In Maine’s 2020 Gordon judgment, recently appointed High Court Justice Andrew Marcus Horton explicitly stated in an opinion that he would be interested in revisiting Maine’s recent mortgage law jurisprudence. Justice Horton goes as far as to argue that the Greenleaf judgment departed substantively from “longstanding precedent and from the modern rule regarding transfer of mortgages” (Boudreau et al., 2020). Key to this paper, however, the Gordon judgment did not erase the requirement that the current note holder of a MERS loan obtain a proper assignment from the original lender.

⁵ David Sherwood, “Maine lawmakers tighten foreclosure rules in win for consumers”, Reuters, July 1, 2015.

2.3 Data

2.3.1 *CoreLogic Loan-Level Market Analytics*

I use the CoreLogic Loan-Level Market Analytics (LLMA) data to study loan performance over my sample period. The LLMA data contains detailed information on mortgage origination and loan performance characteristics for about 45% of all mortgages originated in the US between 2002 and 2007. The data is provided to CoreLogic through the 25 largest mortgage servicers. The dataset includes origination characteristics such as the initial interest rate, uncombined loan-to-value ratio (LTV) at origination, and initial mortgage balance. Also included are indicators for prime loan status, term length, low or no documentation of income status, refinancing status, primary residence status, and single-family residence status. In addition to origination data, CoreLogic's LLMA also includes the current balance and the current delinquency status.

2.3.2 *Registry of Deeds*

I use mortgage contract data from the public registry of deeds of Maine and New Hampshire, available at the Maine Registry of Deeds and New Hampshire Registry of Deeds websites, respectively. Data is available for each mortgage originated over my sample period across all of Maine's 16 counties and 8 of New Hampshire's 10 counties.⁶ 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

⁶ Registry of deeds data is not readily accessible from Carroll and Coos counties.

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.

2.3.3 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 highest organizational level.⁷

2.3.4 Other Data Sources

I use the CoreLogic Deeds data to supplement the registry of 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. Other data sources include data from the Bureau of Economic Analysis, containing annual measures of county-level GDP, income, and total population. For annual county-level unemployment, I use the year-end unemployment rate provided by the Bureau of Labor Statistics. I also use county-level HPI data from the Federal Housing Finance Agency. In order to identify census county and census tracts in the CoreLogic LLMA dataset, I rely upon zip-to-county and zip-to-tract crosswalks provided by the Department of Housing and Urban Devel-

⁷ Available upon request at Robert.Avery@fhfa.gov.

opment (HUD). Finally, to identify mortgage lender bankruptcy filings, I rely upon Bloomberg’s PACER database.

2.3.5 Dataset Construction

I merge CoreLogic Deeds data with public registry of deeds data in several steps using one-to-one matches. I first merge on deeds book number, deeds page number, and county. With the remaining unmatched records, I merge on county, note date, loan amount, and zip. I next add end year and end month separately for greater precision. In order to account for unmatched loans and loans where the end dates are unavailable, I merge on borrower name. I infer census tracts based on CoreLogic Deeds latitude and longitude where available. Otherwise, I rely upon the HUD zip-tract crosswalk.

I merge HMDA data to the intermediate dataset using one-to-one matches on the 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 observations 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 the first eight characters of the lender name. Finally, this entire procedure is repeated for any remaining unmatched observations based on lender names provided by the CoreLogic Deeds data.

Following this intermediary step, I turn to the CoreLogic LLMA data. I impute MERS status using unique matches on zip code, loan amount, and origination date. Each subsequent match increases in precision by adding a new variable to identify unique matches of unmatched MERS and CoreLogic LLMA data left over from the previous iteration. These variables include mortgage end year, mortgage end month,

sale price, and interest rate, respectively.

I compare the rate of MERS-affiliated origination between my sample and that of Lewellen and Williams (2021). The sample used in Lewellen and Williams (2021) represents the universe of Massachusetts mortgages obtained from the Massachusetts Registry of Deeds for 6 of the state's 14 counties. Before comparing the time series, I address some of the shortcomings with my merged dataset. First, as mentioned earlier, the CoreLogic LLMA data represents a selected sample of loans associated with a number of major servicers. While this represents nearly half of all loans originated over the sample period, it will naturally be a biased sample. In contrast, the Massachusetts data is more comprehensive in that it represents the universe of MERS mortgage originations for the counties it covers. Second, imputed MERS status will depend on the presence of unique observations using a select number of variables. While a small number of loan characteristics are sufficient to match the vast majority of MERS loans, I will naturally misclassify some pivotal mortgages as non-MERS loans. Finally, rendering MERS mortgage contracts into text will leave some margin of error on correctly translating important loan information.

Figure 2.3 provides evidence that my sample is representative and well classified for both Maine and New Hampshire. Here, I plot the time series of mortgages that are registered with MERS as a fraction of all mortgages originated in a given year between 2001 and 2012 across Maine, New Hampshire, and Massachusetts. At first glance, all three time series follow the same trend across year of origination. Nevertheless, for both Maine and New Hampshire, there exists a wide gap in MERS share of originations relative to Massachusetts in the early years. This is likely due to the fast adoption of MERS in large metropolitan areas, such as Boston, relative to less densely populated localities across Maine and New Hampshire. Overall, MERS adoption seems to have taken off during the end of the pre-crisis boom. This was a period when the industry started to coalesce around MERS as a standard technology for mortgage

origination. It seems reasonable that the gap in MERS share between Massachusetts and the rest of New England would shrink precisely at a time when a significant number of mortgage companies and non-bank intermediaries originated using MERS. Finally, any gap across the three states appears to vanish after 2007. Taken together, the evidence in Figure 2.3 suggests that my sample is likely representative of the population of interest. Differences between my sample and that of Lewellen and Williams (2021) are likely driven by local economic factors.

2.3.6 Summary Statistics

In Figure 2.4, I plot origination characteristics for MERS and non-MERS loans, pooled across Maine and New Hampshire. I identify 61,429 MERS and 84,659 non-MERS originations in the state of Maine. In New Hampshire, I identify 225,700 MERS and 144,945 non-MERS originations. With a higher LTV and loan balance across quarter of origination, MERS loans appear riskier. However, these loans also appear to have higher FICO credit scores relative to non-MERS loans at origination, particularly during the late boom period. During this late period, non-MERS loans entered delinquency faster than MERS loans, whereas the reverse was true early on. Ultimately, MERS loans still had higher delinquency rates relative to non-MERS loans, despite their higher FICO scores. This largely confirms what the previous literature found to be true, namely that MERS loans were relatively riskier than non-MERS loans. Lewellen and Williams (2021) find that localities with high MERS share had higher long-term foreclosure rates and lower borrower income.

Given all of this, the validity of my research design depends on understanding how loan quality evolved over time. Since the Greenleaf judgment took place years after origination for the loans in my sample, the loan outcomes relevant to my setting will naturally reflect both cross-sectional variation across MERS and non-MERS status, as well as differences in MERS share over time. Prior to widespread MERS adoption,

non-MERS loans represented the vast majority of loans originated in the early 2000s. Furthermore, non-MERS loans originated later in the sample appeared to exit the sample at a faster rate. Hence, loans that remain in my sample by April 2013 will be predominantly early-originated non-MERS loans and late-originated MERS loans. This suggests significant differences across treated and control units by the time of the Greenleaf judgment. However, in the context of my research design, identification requires that differences in loan characteristics should not affect the evolution of loan outcomes in a manner that is correlated with the shock itself.

To test this assumption, I borrow from O'Malley (2021) and model loan outcomes of MERS and non-MERS loans over time as transition rates. The empirical foreclosure rate is defined as the rate at which a loan transitions into foreclosure. Once a loan enters foreclosure, it drops out of the sample. Furthermore, in order to measure transition rates into either a foreclosure or an alternative outcome, I require a loan to be previously 60 days delinquent or worse to register a transition. Doing so allows me to interpret transition into a loan outcome as an alternative to foreclosure, creating consistent measurement across transition rates while also retaining loans which are not current at the start of the sample.

Figure 2.5 plots the empirical foreclosure rate across MERS status and state of jurisdiction. The sample covers 4 quarters prior to and 8 quarters following the baseline quarter. The baseline quarter is defined as the quarter immediately preceding the judgment and estimates are demeaned relative to this period. Panel A plots the time series of the foreclosure rate across MERS status in the state of Maine. Outside of slight variation, MERS and non-MERS loans follow parallel trends for five quarters prior to the Greenleaf judgment. Thereafter, however, MERS loans experience a stark drop in foreclosure relative to non-MERS loans for eight quarters following the judgment. Relative to a baseline foreclosure rate of 34 basis points, the Greenleaf judgment reduced MERS foreclosures by a third.

In order to further attribute this drop in foreclosure to the Greenleaf judgment, Panel B plots the time series for foreclosure rates across MERS and non-MERS loans in the neighboring state of New Hampshire. Under the assumption that differences between MERS and non-MERS loans should be similar across a local geography, Panel B presents a plausible counter-factual relative to Maine. Here, the New Hampshire MERS foreclosure rate appears to follow parallel to the non-MERS foreclosure rate throughout both the period preceding the Greenleaf judgment, as well as the period following the ruling. Hence, this evidence suggests that Maine MERS foreclosure rates would likely have continued to follow parallel to non-MERS foreclosure rates in the post-period, but for the Greenleaf judgment.

As a final piece of suggestive evidence, I want to ensure that the drop in MERS foreclosure is not due to differences in loan quality attributable to survival bias. Figure A.1 plots the empirical foreclosure rate for MERS and non-MERS loans across year of origination and FICO score quintile using data from Maine. Panels A and B plot the empirical foreclosure rate for the pre-period. Panels C and D plot the empirical foreclosure rate for the post-period. Across year of origination and FICO quintile, MERS and non-MERS loans experience similar rates of foreclosure in the pre-period. In the post-period, however, this relationship no longer holds, whereby MERS loans experience a distinct reduction in foreclosure relative to non-MERS loans. In Section 2.5.1, I provide a formal analysis of how heterogeneity in the treatment effect across the timing of loan origination and loan quality relates to the underlying mechanism for the drop in foreclosure.

In sum, the preceding evidence suggests that variation across MERS status appears uncorrelated with the Greenleaf judgment. Given this, Table 4.1 reports summary statistics for loan characteristics of my primary sample of Maine loans. Here, I restrict the sample to conventional loans originated between 2002 and 2007 and observable in CoreLogic between April 2013 and June 2016. I further retain loans with

an initial mortgage balance less than or equal to \$2 million, an initial uncombined loan-to-value ratio less than or equal to 150, and an initial interest rate less than 20 percentage points. I require that these loans have an initial term length of 30, 20, or 15 months, as well as a non-missing prime or subprime status. My final sample consists of 10,192 MERS loans and 21,353 non-MERS loans. Comparing treated and control units, MERS loans have a \$15,000 higher loan balance, a 3 percentage point lower prime status, a 3 percentage point higher LTV, and an interest rate over 30 basis points higher. Ultimately, while MERS status reflects differences in loan quality, the preceding evidence suggests that these differences do not vary systematically around the time of the judgment. Nevertheless, Section 4.7 deals with concerns related to selection directly.

2.4 Main Results

This section outlines the empirical strategy and reports results from OLS regressions. I find a strong negative effect of the Greenleaf judgment on the foreclosure rate. Furthermore, creditors sell their loans when foreclosure becomes costly. I find no evidence of changes in explicit modifications on the part of creditors or increased delinquency on the part of borrowers. Finally, I find that the market for delinquent debt sale is highly concentrated and features significant heterogeneity in foreclosure propensity.

2.4.1 Empirical Design

To identify the causal effect of an increase in foreclosure costs on creditor behavior, I use a difference-in-differences research design. Here, I compare loan outcomes of MERS loans (treated group) and non-MERS loans (control group) before and after the Greenleaf judgment.

I run OLS regressions using the loan-month panel. I estimate a difference-in-

differences regression of loan outcomes:

$$Y_{it} = \alpha_{zip,t} + \alpha_{vintage,t} + X'_{it}\gamma + \beta MERS_i + \delta MERS_i \times Post_t + \epsilon_{it} \quad (2.1)$$

In the above specification, Y_{it} takes a value of one at time t if both of the following conditions hold: (i) the loan experiences a particular credit event of interest and (ii) the loan had a previous delinquency status of 60 days or worse. A loan drops out of the sample upon experiencing any foreclosure, debt sale, short sale, and when $Y_{it} = 1$. Therefore, I measure the transition rate of loan outcomes as opposed to any cumulative effect. The *Post* dummy takes a value of one if a loan is observed on or after the month of the judgment (July 2014). X_{it} is a vector of loan, borrower, and regional characteristics. Loan-level controls include the origination interest rate, initial LTV, and log of the original mortgage balance. Further loan-level controls include indicators for prime loan status, term length, absent or minimal income verification status, refinancing status, primary residence status, single-family status, and log of the previous period's balance. Annual county-level controls at the time of event include the log of the house price index, GDP, population, and income, as well as the year-end unemployment rate. Standard errors are clustered at the zip code-level to account for within-zip code residual correlation.

I include two sets of fixed effects to account for time-varying common shocks. First, the dummy variable $\alpha_{zip,t}$ represents fixed effects for the quarter of observation interacted with a loan's zip code. Including this, I control for changes in unemployment, income, and house prices, factors that may affect loan outcomes independent of treatment. Second, the dummy variable $\alpha_{vintage,t}$ represents fixed effects for the quarter of observation interacted with quarter of origination. Here, I account for systematic changes associated with a particular vintage that may correlate with the timing of the Greenleaf judgment.

2.4.2 Identification

The coefficient of interest is δ , which measures the differential change in loan outcomes among MERS loans relative to non-MERS loans following the Greenleaf judgment. The key identifying assumption is that, conditional on observables, loans in the treated and control groups would have faced similar outcomes if not for an exogenous increase in foreclosure costs due to treatment (the Greenleaf judgment). This assumption would be violated if the treatment was endogenous to loan outcomes or if loan outcomes for treated loans systematically differed from untreated loans following the ruling for reasons unrelated to the Greenleaf judgment.

The nature and timing of the Greenleaf ruling assuage these concerns. First, the judgment hinged upon the unorthodox interpretation of a single sentence in Maine’s MERS mortgage contract. The subtle variation in the construction of this declaratory sentence was random relative to other similar sentences across the country and, by definition, independent of loan outcomes. Furthermore, the judgment was unanticipated, as it overturned a century of Maine common law (Aromando, 2014). Finally, as presented previously in Section 2.3.6, I find suggestive evidence that treated and untreated loans would have evolved in parallel, but for the Greenleaf judgment. Loan outcomes in New Hampshire, for example, do not present any meaningful differences across MERS status and quarter of observation.

To formally assess the degree of any systematic variation, I replace $Post_t$ in the specification above with indicator variables for 4 quarters before and 8 quarters after the baseline quarter preceding the judgment:

$$Y_{it} = \alpha_{zip,t} + \alpha_{vintage,t} + X'_{it}\gamma + \beta MERS_i + \sum_{s=-4}^8 \delta_s MERS_i \times \{t = s\} + \epsilon_{it} \quad (2.2)$$

The lack of a preexisting differential trend coupled with a sharp break at the time of treatment would imply that changes following the ruling are likely not due to

systematic differences in the evolution of treated and untreated loans. In Section 4.7, I deal with any remaining concerns over systematic variation directly by conducting my analysis using New Hampshire loans, lender identifiers, and placebo loans.

2.4.3 Effect on Foreclosure and Debt Sale

Table 2.2 reports difference-in-differences estimates of the effect of the Greenleaf judgment on the monthly probability of loan outcomes. In Columns (1) to (3), I estimate the equivalent of a first-stage, using foreclosure as the credit event of interest. All columns include origination, borrower, and time-varying county-level controls. The specification in Column (1) also includes fixed effects to account for time-varying shocks to loan vintage and zip code, separately. I find that the MERS foreclosure rate declines by 0.088 percentage points relative to the pre-period. Relative to the MERS baseline foreclosure rate, this represents a 26% drop, economically large and statistically different from zero at the 1% level.

In Columns (2) and (3), I attempt to restrict variation so as to limit concerns over selection into MERS. In Column (2), I include fixed effects for time-varying shocks to loan vintage and zip code, jointly. Insofar as loan vintage predicts unobserved loan quality, this controls for any variation associated with a particular loan vintage in a particular period and a particular zip code. Here, I require a weaker identifying assumption, namely that MERS and non-MERS loans originated at the same time and in the same location do not significantly differ in outcomes over time. In Column (3), my most demanding specification, I exploit the full panel structure of my data with the inclusion of fixed effects for a particular loan itself. This specification deals with selection directly, whereby I account for time-invariant factors that may influence a borrower's choice into MERS. Across all specifications, the effect of the Greenleaf judgment is negative, large, and statistically different from zero at the 1% significance level.

Given that creditors are unable to enforce their security interest, they may be inclined to sell their assets. If the price offered by the secondary market exceeds the value of holding a mortgage, then a creditor should prefer to sell their delinquent debt. To that end, I next estimate my main specification using an outcome variable equal to one when a loan experiences a debt sale. As before, I require that this variable equal one only when the loan is previously 60 days delinquent or worse.

As an aside, CoreLogic's LLMA data does not measure debt sale explicitly but, rather, records the period in which a servicer outside of the sample purchases the servicing rights of a loan. I argue that my measurement of servicing sale can be interpreted as a loan sale for three reasons. First, my outcome variable equals one when a loan transition into servicing sold from a delinquency status of 60 days or worse. Hence, the motivation of why a servicer would sell its servicing rights becomes more clearly related to delinquency and suspended foreclosure. Second, the CoreLogic sample records a servicing sale only when a loan is sold outside of one of its data providing partners. Therefore, such a sale likely indicates that the loan was also sold off of the balance sheets of the largest investors that provide data to CoreLogic, such as Fannie Mae and Freddie Mac. Even when these large investors purchase these loans themselves, they are likely placed in specialized trusts that deal particularly with non-performing loans. Third, in Section 2.4.6, I provide evidence that, indeed, my outcome variable corresponds to recorded sales in the public registry of deeds. Taken together, the nature of my measurement appears to indicate that I identify the sale of delinquent debt.

In Columns (4) to (6) of Table 2.2, I present results using the outcome variable of debt sale. The specifications in these columns are analogous to the regressions in Columns (1) to (3), respectively. In Column (4), I find that the MERS debt sale rate increases by 0.051 percentage points following the Greenleaf judgment. This represents a 57% increase relative to the baseline MERS debt sale rate. Furthermore,

this accounts for 58% of the decline in foreclosures. In Column (2), I account for time-varying shocks to a given vintage in a particular zip code. Here, I estimate a 0.068 percentage point increase in the debt sale rate. In Column (3), I control for a loan's time-invariant unobserved heterogeneity and estimate that the debt sale rate increased by 0.056 percentage points. Across all specifications, the effect of the Greenleaf judgment on debt sale is positive, large, and statistically different from zero at the 1% significance level.

Figure 2.6 plots estimates of the regression Equation (2.2) using 95% confidence intervals. The plot shows the degree to which MERS loans experienced differential trends in the rate of foreclosure and debt sale prior to the Greenleaf judgment. Normalizing estimates to the baseline quarter before the ruling, I plot estimates from four quarters in the pre-period and eight quarters in the post-period.

Panels A and B plot estimates using foreclosure and debt sale as the outcome variables, respectively. The specification here is analogous to Column (3) of Table 2.2. Under the key identifying assumption that MERS loans would have evolved in parallel to non-MERS loans if not for the Greenleaf judgment, there should be no significant difference between the two loan types across the pre-period. Conditional on observables, the figure shows that the treatment and control groups evolved roughly in parallel prior to the Greenleaf judgment for both outcomes.

In Panel A, however, MERS loans experienced a stark reduction in foreclosure propensity relative to non-MERS loans immediately following the Greenleaf judgment. Furthermore, in Panel B, MERS loans experienced an increase in debt sale propensity relative to non-MERS loans in the period following the Greenleaf judgment. Taken together these results suggest that when creditors faced higher enforcement costs due to the Greenleaf judgment, they chose to sell their delinquent debt.

2.4.4 *Effect on Delinquency*

Given the results in Section 2.4.3, an increase in enforcement costs would naturally affect both borrowers and creditors alike. Disentangling these effects may be complicated due to strategic behavior by both parties in the wake of the Greenleaf judgment. Namely, following an exogenous drop in foreclosure, borrowers with a MERS loan may default on their mortgages due to moral hazard. O'Malley (2021) presents evidence that, following a foreclosure moratorium in Ireland, the delinquency rate increased by 60% precisely due to the reduced cost of default associated with foreclosure. Moral hazard, therefore, would make it difficult to distinguish between changes in creditor behavior due to higher foreclosure costs versus changes due to increased borrower delinquency.

I argue that given the unadvertised and nuanced legal nature of the Greenleaf judgment, borrowers were left unaware of the rise in foreclosure costs. As a first step, simply understanding the difference between a mortgage contract and its associated promissory note requires specialized knowledge. To then understand the legal arbitrage associated with the subtle wording of a single sentence in a given mortgage contract adds another layer of complexity. Further still, even after familiarizing oneself with the esoteric details surrounding mortgage assignment, a borrower would have to then assess how likely and how quickly a creditor can contact their original lender. Lastly, all of this hinges upon a borrower paying attention to local court cases affecting foreclosure. This seems highly unlikely given that only a single news article out of Maine's eight local daily newspapers reported on the drop in foreclosure.

To test this hypothesis, I estimate regression Equation (2.1), where the outcome of interest Y_{it} takes a value of one if loan i at time t transitions into a delinquency status of 90 days following a delinquency status of 60 days, upon which it drops out of the sample. Hence, I am effectively measuring the rate of entry into severe delinquency.

I restrict my sample to those loans that were either current, 30 days delinquent, or 60 days delinquent in the period prior to the start of my sample.

In this setting, the coefficient of interest is δ , which measures the differential change in delinquency among MERS loans relative to non-MERS loans following the Greenleaf judgment. If borrowers with MERS loans were fully aware of the Greenleaf judgment, then the delinquency rate should rise to mirror the results in Section 2.4.3 (O’Malley, 2021). If delinquency failed to grow in response to such a large and persistent drop in foreclosure, then it is likely that the effect of the Greenleaf judgment was not salient among borrowers.

Table A.1 presents my results. Column (1) presents estimates for all loans in my sample. I find a statistically insignificant and economically small change in delinquencies in the post-period. I supplement these results with three additional tests that may be representative of the marginal borrower affected by the Greenleaf judgment. Column (2) reports estimates interacting $MERS_i \times Post_t$ with an indicator for loans associated with non-primary residences. This test is motivated by the assumption that default may be most relevant to investors instead of borrowers living in their mortgaged home (Bhutta et al., 2017). Column (3) reports estimates interacting $MERS_i \times Post_t$ with an indicator for loans that experienced any delinquency in the year prior to the start of my sample. The non-pecuniary and psychological costs that could prevent borrowers from defaulting may be weaker for borrowers who already defaulted at least once. Column (4) reports estimates interacting $MERS_i \times Post_t$ with indicators for negative income growth and negative equity. This test is motivated by the “double-trigger” theory of mortgage default (Foote and Willen, 2018). Across all columns, estimates are statistically insignificant.

To provide additional reassurance that there is no change in severe delinquency for MERS loans following the ruling, Panel C of Figure 2.6 plots estimates of the regression Equation (2.2) using the rate of severe delinquency as an outcome variable.

Here, I plot the specification analogous to the regression in Column (3) of Table 2.2. As before, there are no differential trends prior to the Greenleaf judgment. Furthermore, there appears no significant change in the delinquency rate across all quarters following the Greenleaf judgment.

This result has several important implications. First, given that the Greenleaf judgment led to Maine’s worst foreclosure crisis in a century (outside of the financial crisis), insignificant estimates on the transition into delinquency provide strong evidence that the effect of the judgment was not salient among borrowers. Second, if the effect was salient among borrowers, then I would be unable to attribute creditor behavior to higher enforcement costs alone. This is due to creditors suffering losses from increased borrower delinquency. Given that this does not appear to be the case, further tests of loan outcomes should be interpreted as revealing what alternatives creditors rely upon when enforcement becomes costly, independent of borrower behavior.

2.4.5 Effect on Alternative Outcomes

My previous results indicate that the Greenleaf judgment reduced the incidence of foreclosure and led creditors to sell their debt. In this section I want to explore whether creditors also offered to renegotiate with borrowers when faced with costly foreclosure. The regressions in Table 2.3 test the effect of the Greenleaf judgment on alternative loan outcomes. I estimate regression Equation (2.1), where the outcome of interest Y_{it} takes a value of one if loan i at time t transitions into a cure, short sale, or modification, upon which it drops out of the sample. The coefficient of interest is δ , which measures the differential change in loan outcomes for MERS loans relative to non-MERS loans following the Greenleaf judgment.

For the purposes of comparison, in Column (1), I first reproduce estimates for delinquency reported in Table A.1. Column (2) reports estimates where the outcome

variable equals one if loan i at time t transitions into a delinquency status of 30 days or less following a delinquency status of 60 days, upon which it drops out of the sample. In essence, I am measuring the rate of cure, namely, when a loan begins to reperform through creditor intervention or borrower repayment. In Column (3), the outcome variable is equal to one when a loan transitions to a zero balance from a delinquency status of 60 days or more. Here, given the loan's delinquency status, the outcome variable may be interpreted as a short sale, whereby a creditor negotiates with a distressed borrower to sell the mortgaged home and repay the remaining balance. In Column (4), the outcome variable is equal to one when a loan transitions into a modification (an interest rate reduction, principal balance reduction, or a principal balance increase) from a delinquency status of 60 days or more. Here, I restrict the sample to fixed-rate mortgages to limit mismeasurement of modifications. Note that this measure of modification does not include changes to the term length of the loan, as CoreLogic does not include current remaining months of payment. Across all columns, I estimate statistically insignificant estimates.

Overall, the results in Tables 2.2 and 2.3 suggest that when faced with the choice of selling their assets or negotiating with the borrower, creditors appear to sell their delinquent debt. This is consistent with the evidence in Table A.1 that suggests that creditors do not seem to forgo renegotiation simply because borrowers would, in the absence of foreclosure, fall deeper into delinquency. Rather, there exists a secondary market that provides recourse to creditors for delinquent loans that are costly to foreclose upon.

Not only does this result imply that loan sales function as an alternative to foreclosure, mortgage modifications, and self-cures, but it may also explain why securitized mortgages experience fewer modifications. Namely, lenders originate mortgages with the securitization infrastructure in mind, which includes its intermediaries, servicers, and investors. In fact, MERS was constructed to facilitate the securitization process

itself, whereby creditors could easily buy and sell without a break in the chain of title. The fact that securitized mortgages are part of a highly liquid market would make debt sale an attractive alternative to foreclosure and modification, particularly in the face of financial constraints and high foreclosure costs.

2.4.6 Measuring Outcomes Following Delinquent Debt Sale

The evidence so far suggests that when current creditors face higher enforcement costs, they sell their debt on the secondary market. Measuring outcomes following delinquent debt sale can clarify how future creditors behave when current creditors face higher enforcement costs. To that end, I refer back to the Maine Registry of Deeds to identify the ultimate purchasers of these loans. Out of 122 delinquent debt sales from my primary sample of MERS loans in the post-period, I hand collect information on 106 recorded purchases from the Maine Registry of Deeds.

Panel A of Table 2.4 presents summary statistics of loan outcomes following delinquent debt sale. I find that a given investor will continue to buy delinquent debt for an average of 8 months following their first purchase. This means that investors buy debt through multiple offerings, not through a single transaction. Furthermore, only 24% of the loans purchased ultimately experience an initial foreclosure filing and 41% ultimately experience a discharge. Conditional on initiating enforcement, investors file for foreclosure after an average of 36 months since initial purchase. Similarly, loans that experience a discharge do so after 47 months since initial sale. These results suggest that, despite debt sale, creditors still faced difficulty initiating foreclosure.

The largest buyer of delinquent debt in my sample is Fannie Mae, the demand side investor in 43% of recording transactions. Fannie Mae likely has an incentive to purchase this debt for three reasons. First, over 60% of all total mortgage debt outstanding is owned by the three agencies Fannie Mae, Freddie Mac, and Ginnie Mae (Housing Finance Policy Center, 2022). Hence, Fannie Mae is likely specialized in

its infrastructure and network to facilitate obtaining a speedy mortgage assignment from the original lender. Second, given its outsized role in the mortgage market, Fannie Mae possesses security interests in a large share of residential real estate. With the potential for negative spillover effects to other collateral across the state of Maine (Favara and Giannetti, 2017; Gupta, 2019), Fannie Mae's position likely translates to a vested interest in ultimate loan outcomes. To that end, I find that loans purchased by Fannie Mae indeed experience a foreclosure rate 75% smaller than average. Third, in the case that Fannie Mae itself cannot obtain a proper assignment, it has the facilities to sell this debt to outside investors. For example, between 2016 and 2018, Fannie Mae sold Goldman Sachs and its subsidiaries over \$10 billion in non-performing and reperforming mortgage debt through multiple offerings (Lane, 2018).

Panel B of Table 2.4 supports the preceding discussion. Here, I find that Fannie Mae only retains 23% of the loans that it initially purchased. Of the loans that Fannie Mae ultimately sells, the time to foreclosure and discharge is almost half that of the larger sample. Hence, Fannie Mae plausibly functions as an intermediary that distributes loans to specialized debt collectors. The largest investor that ultimately buys 42% of Fannie Mae's initial purchases never initiates foreclosure on any of these purchased loans. Furthermore, almost all transactions by this large investor occur in the span of one month. In stark contrast, the second largest investor has a foreclosure rate of 40%. Here the debt sale occurs over the course of almost two years. Taken together, the preceding evidence suggests that Fannie Mae plays an active role in selecting investors best suited to purchase its debt (Bhardwaj, 2021).

Returning to Panel A of Table 2.4, I find that, outside of loans sold to Fannie Mae, purchase volume correlates with foreclosure specialization. The first largest investor purchased 25% of all loans, foreclosed on 42% of these loans, and initiated foreclosure within 24 months. This represents a foreclosure rate that is 30% higher and

a speed that is 50% faster than the second largest investor. Finally, about 90% of all delinquent debt is purchased by four initial investors and almost 80% of Fannie Mae's purchases are subsequently acquired by only three investors. In sum, the market for delinquent debt appears to be highly concentrated and highly variable in foreclosure appetite across investors.

2.5 Mechanism

In this section, I explore the mechanism driving my main results. First, I show that the effect of the Greenleaf judgment is concentrated on lenders operating later in the business cycle and within the subprime mortgage market. Second, I demonstrate that for those loans where the original lender filed for bankruptcy, the Greenleaf judgment rendered these loans unenforceable and likely led to forbearance in lieu of debt sale. Finally, I show that these results are persistent, lasting well over a year after initial delinquency.

2.5.1 Timing of Origination and Loan Quality

To some degree, the broad mechanism is well understood. Namely, the Greenleaf judgment made foreclosure costly by forcing creditors holding MERS loans to obtain proper assignment from original lenders. However, understanding the channel through which the judgment drove the drop in foreclosure can shed useful insight into important dynamics within lending markets. To that end, I study how the timing of origination and lender quality impacted the transmission of treatment.

With regards to the timing of origination, a budding research area has argued that mortgage companies, shadow banks, and privately securitized mortgages began to grow in market share in the latter part of the business cycle prior to the financial crisis (Drechsler et al., 2021; Mian and Sufi, 2022). Mortgage companies appearing in these peak boom years included new market players, some of whom subsequently went

out of business in the wake of the crisis. In contrast, MERS members participating in the earlier part of the business cycle were major market players that developed the very electronic registry itself. Hence, loans issued earlier in the business cycle were originated by lenders that held an important role in the expansion of mortgage credit and the development of lending infrastructure. With such a robust foundation, I hypothesize that, a decade later, these original lenders were readily available to assign mortgages to current note holders for the purposes of foreclosure in the wake of the Greenleaf judgment. In contrast, I predict that lenders that originated primarily in the tail end of the business cycle were likely difficult to find, unresponsive, or shutdown entirely.

As a second test of the mechanism, I am interested in learning how lender quality affected loan outcomes in the wake of the Greenleaf judgment. As discussed earlier, borrowers were likely unaware of the Greenleaf judgment given its complex and unadvertised nature. Therefore, heterogeneity in treatment effects across credit scores should reflect the quality of the original lender and not that of the borrower. Indeed, the literature has found the mortgage market to be highly segmented along loan quality, where subprime mortgage companies fundamentally differ from regular lenders along the dimensions of incentives, borrower pool, and balance sheets (Mayer and Pence, 2009; Gerardi and Willen, 2009; Nadauld and Sherlund, 2013). I hypothesize that constrained subprime lenders that financed their originations via wholesale funding likely went out of business when their funding dried up following the financial crisis (Purnanandam, 2010). Therefore, the effect of Greenleaf judgment should be concentrated on low credit score loans, reflecting high search costs to obtain assignment from subprime lenders.

As previously discussed in Section 2.3.6, Figure A.1 provides suggestive evidence in support of the preceding hypotheses. Panels A and B plot the empirical foreclosure rate for the pre-period across origination year and FICO credit score quintile,

respectively. Panels C and D plot the analogous empirical foreclosure rate for the post-period. Relative to the pre-period, MERS loans in the period following the Greenleaf judgment experience a distinct break in foreclosure across year of origination and FICO quintile. However, this reduction in foreclosure is concentrated on precisely the late originated loans (later market entrants) and the low FICO score loans (subprime lenders).

In order to formally test how market entry and lender quality affected the transmission of the Greenleaf judgment, I augment Equation (2.1):

$$\begin{aligned}
 Y_{it} = & \alpha_{zip,t} + \alpha_{vintage,t} + X'_{it}\gamma + \beta_1 MERS_i + \beta_2 Absent Lender_i \\
 & \beta_3 MERS_i \times Absent Lender_i + \beta_4 Absent Lender_i \times Post_t \quad (2.3) \\
 & + \delta MERS_i \times Post_t + \varphi MERS_i \times Absent Lender_i \times Post_t + \epsilon_{it}
 \end{aligned}$$

The variable $Absent Lender_i$ is an indicator equal to one for loans associated with lenders that were likely unresponsive in the wake of the Greenleaf judgment. Table 2.5 reports estimates of regression Equation (2.3). I restrict my analysis to the primary outcomes of interest, namely, foreclosure and debt sale. I also include estimates using severe delinquency as an outcome variable to ensure that the effect is not salient among borrowers. The coefficient of interest is φ , which measures the differential change in loan outcomes among MERS loans originated with an absent lender relative to both non-MERS loans and MERS loans issued by likely more responsive lenders. Since all Maine MERS loans belong to the treated group, φ identifies heterogeneity in treatment.

In Columns (1) to (3), I replace $Absent Lender_i$ with $Late Vintage_i$, a proxy variable for late market entrants that equals one for loans originated after January 2005. This specification controls for common trends across MERS status and origination timing, separately. Here, the effect of the judgment appears concentrated on precisely those MERS loans originated late in the business cycle. This result confirms the hy-

pothesis that for MERS loans that were issued in the early part of the business cycle, there was no differential effect of treatment. Rather, late market entrants were likely the least responsive to requests by current note holders for mortgage assignments.

In Columns (4) to (6), I replace *Absent Lender_i* with *Low FICO_i*, a proxy variable for subprime lenders that equals one for loans originated with a below median FICO score. Once again, this specification controls for common shocks to MERS loans and low quality lenders, separately. Here, the effect appears concentrated on precisely those MERS loans with below median credit scores. This evidence suggests that the impact of the Greenleaf judgment was likely concentrated on those loans issued by subprime MERS lenders.

The Greenleaf judgment left the burden upon current note holders to obtain proper assignment from original lenders for all MERS loans, independent of year of origination or credit score. The drop in foreclosure activity reflects the increase in foreclosure costs discussed earlier, both pecuniary and non-pecuniary, in locating the original lender. Obtaining proper assignment became prohibitively expensive for loans originated by late market entrants and subprime lenders, composed of mortgage companies that were difficult to identify or out of business entirely. Hence, loans issued by these opaque intermediaries were likely responsible for the drop in foreclosure that current note holders faced almost a decade later.

2.5.2 Lender Bankruptcy

Given the nature of foreclosure moratoria and legal regimes of collateral enforcement, it becomes difficult to study how heterogeneous changes in foreclosure costs impact loan outcomes. A foreclosure moratorium indiscriminately blocks all creditors from seizing collateral on a subset of loans; legal regimes of foreclosure are either judicial or non-judicial in nature. In this subsection, I present novel evidence on the impact of heterogeneity in foreclosure costs on loan outcomes by exploiting a unique feature

of the Greenleaf judgment.

Since the judgment forced creditors to obtain mortgage assignments from original lenders, loans originated by lenders who ultimately filed for bankruptcy proved problematic. The judgment required that creditors work through bankruptcy court in order to obtain proper assignment for these loans. In such cases, there was an open question as to whether bankruptcy terminated the power of attorney for the original servicer to assign the mortgage contract. Furthermore, it was unclear as to whether the unassigned mortgage contract reverted back to either (a) the debtor as an unclaimed asset or (b) the liquidating trust presiding over the bankruptcy. In some cases, bankruptcy plans stated explicitly that no asset transferred to the liquidating trustee would revert back to the debtor. In other cases, bankruptcy plans stated that any remaining unadministered assets would revert to the 25 largest creditors without specifying who they were. Finally, bankruptcy judges and trustees are generally reluctant to cooperate with requests to reopen cases or to assign mortgage contracts.

Hence, obtaining a proper assignment when the original lender filed for bankruptcy proved prohibitively costly due to issues related to closed cases, unreliable trustees, and non-cooperative bankruptcy judges. Compared to instances where the lender did not file for bankruptcy, these loans were significantly more expensive to foreclose upon. In order to leverage this heterogeneity, I augment Equation (2.1):

$$\begin{aligned}
 Y_{it} = & \alpha_{zip,t} + \alpha_{vintage,t} + X'_{it}\gamma + \beta_1 Low Cost_i + \delta_1 Low Cost_i \times Post_t \\
 & + \beta_2 High Cost_i + \delta_2 High Cost_i \times Post_t + \epsilon_{it}
 \end{aligned}
 \tag{2.4}$$

The variable *High Cost_i* is an indicator for MERS loans originated by lenders who ultimately filed for bankruptcy, namely high foreclosure cost MERS lenders. The variable *Low Cost_i* is an indicator for MERS loans originated by all other lenders, namely low foreclosure cost MERS lenders. I classify lenders as high cost by identifying lender bankruptcy filings in Bloomberg's PACER database. I find that roughly 6% of MERS

mortgages were originated by high cost lenders. The coefficients of interest are δ_1 and δ_2 , which measure the differential change in loan outcomes for low cost and high cost MERS loans, respectively, relative to untreated loans.

Table 2.6 presents the estimates of regression Equation (2.4). Each column defines a distinct credit event, namely foreclosure, debt sale, severe delinquency, self-cure, short sale, and modification, respectively, as defined earlier. For each outcome variable, the last row reports a pairwise t-test statistic for the equivalence of coefficients δ_1 and δ_2 .

Column (1) reports estimates of the effect of the Greenleaf judgment on foreclosures. I estimate that the foreclosure rate for low cost MERS loans fell by 0.063 percentage points, statistically significant at the 5% level. Furthermore, I find that the foreclosure rate for high cost MERS loans fell by 0.231 percentage points, statistically different from zero at the 1% level. The pairwise t-test statistic comparing the two coefficients is statistically significant at the 5% level. These results suggest that MERS loans originated by bankruptcy filers indeed experienced higher foreclosure costs following the Greenleaf judgment.

Column (2) reports estimates where the outcome variable is debt sale. Here, the estimate for low cost debt sale is positive, significant at the 1% level, and roughly equivalent to the analogous drop in low cost foreclosures. In stark contrast, however, for high cost loans, the point estimate is small and statistically insignificant. The pairwise t-test statistic comparing the two coefficients is statistically significant at the 5% level. These results suggest that the increase in debt sale is not monotonic in foreclosure costs. Rather, when foreclosure becomes prohibitively expensive, such as when the original lender filed for bankruptcy, demand dries up.

Nevertheless, Column (3) reveals that these high foreclosure cost MERS loans experience a reduction in severe delinquency. This reduction reflects the analogous drop in foreclosure from Column (1). At the same time, low foreclosure cost MERS

loans experience no significant change in delinquency. The pairwise t-test statistic comparing the two coefficients is statistically significant at the 10% level. Finally, Columns (4) to (6) further report statistically insignificant point estimates for self-cures, short sales, and modifications, respectively.

Under the assumption that the reduction in delinquency is not due to changes in borrower behavior, these results suggest that high foreclosure cost loans benefited from forbearance following the Greenleaf judgment. Creditors likely offered forbearance when the secondary market broke down for these loans, evidenced by the lack of increase in debt sales. Given that note holders prefer lower foreclosure costs, the Greenleaf judgment reveals a pecking order of creditor behavior under costly collateral enforcement. Namely, creditors prefer selling delinquent debt rather than renegotiating, offering forbearance only when foreclosure becomes prohibitively expensive and sale is impeded.

2.5.3 Long-Term Effects

Given the results in the previous section, changes in loan outcomes may be short-lived, ultimately no different relative to the pre-period when viewed at longer horizons. To that end, in this subsection, I am interested in learning whether my main results are representative of foreclosure delay instead of foreclosure foregone (Gerardi et al., 2013). Should foreclosure simply be delayed, I want to measure how long it takes a creditor to locate the original lender and obtain a mortgage assignment.

In the spirit of Jordà (2005), I separately estimate Equation (2.4) using local projections for horizon lengths of one through 12 months since an initial 60-day delinquency. For each horizon, I define my sample at the loan-level, restricting my analysis to those loans experiencing an initial 60-day delinquency. Note that this sample is no longer defined as a loan-month panel but, rather, represents the loan-level cross-section of future outcomes for a given time horizon. Table A.2 reports summary

statistics of loan outcomes at various horizons. I run OLS regressions using this loan-level sample and estimate a difference-in-differences regression of loan outcomes for each horizon:

$$\begin{aligned}
 Y_{i,t+n} = & \alpha_{zip} + \alpha_{vintage} + \alpha_{t+n} + X'_{it+n}\gamma + \beta_1 Low Cost_i + \delta_1 Low Cost_i \times Post_{t+n} \\
 & + \beta_2 High Cost_i + \delta_2 High Cost_i \times Post_{t+n} + \epsilon_{i,t+n}
 \end{aligned}
 \tag{2.5}$$

In these regressions, the outcome variable takes a value of one if a loan experiences a credit event n months following an initial delinquency status of 60 days. Furthermore, $Post_{t+n}$ is a dummy variable equal to one if a loan outcome is measured in the period following the Greenleaf judgment. Here, the treatment effect measures the differential change in loan outcomes n months since initial delinquency. I include fixed effects to account for time-invariant shocks to loan outcomes at the level of a loan's zip code and quarter of origination. I also account for macroeconomic shocks common across all loans by including quarter of observation fixed effects. Finally, I adjust the sample in two ways to account for the limited number of delinquencies. First, I account for uneven weighting towards the early part of the sample by expanding the set of measured delinquencies three years after the judgment instead of only two years. Second, the sample is less restricted in that I do not condition on initial mortgage term and prime status.

Figure 2.7 plots estimates of regression Equation (2.5) across time horizons of one to 12 months. Panels A through F present estimates where the outcome variable is equal to one when, after n months since initial delinquency, a loan's status is recorded as foreclosure, debt sale, delinquency (60- or 90-days delinquent), self-cure (30-days delinquent or current), short sale (loan balance equal to zero), and modification, respectively. Here, foreclosure, debt sale, and short sale function as terminal states and are measured as of the event date. In contrast, self-cure, delinquency, and modification are measured as of $t + n$. All outcomes are mutually exclusive of one

another for a given n -month horizon, except for modification. When a loan experiences a modification, it may simultaneously experience a cure or still currently be in delinquency.

In Panel A, I find high foreclosure cost MERS loans experience an increasing reduction in foreclosure, growing over 12 months to -11.6 percentage points, significant at the 1% level. For low foreclosure cost MERS loans, there appears to be a relatively stable reduction in foreclosure, close to 5 percentage points, with varying degrees of statistical significance.

This evolution in foreclosure provides valuable insight into the nature of the Greenleaf judgment. First, it confirms that the initial drop in foreclosure identified in the panel setting does not disappear even after 12 months. Therefore, the increase in foreclosure cost does not amount to simply a delay in foreclosures, but, rather, a persistent and significant gap. Second, this result reveals that requiring creditors to obtain proper legal assignment of mortgages is sufficient to suspend foreclosures for over 12 months since an initial 60-day delinquency. This demonstrates that should legal challenges to the authority of MERS hold up in court, then creditors could face a foreclosure crisis whereby their collateral rights are rendered unenforceable. Finally, the evolution in foreclosures reveals significant search costs in communicating with the original lender. Not until almost a year since an initial 60-day delinquency do creditors finally begin to slightly recover in their propensity to foreclose on a delinquent loan. This implies that locating initial lenders and obtaining proper assignment bears a non-trivial cost. Such a vulnerability demonstrates the importance of maintaining the legal integrity of documentation for collateral enforcement, despite the ex-ante costs that may ensue (Defusco and Mondragon, 2020).

It is also important to note that this cost is born due to a uniquely recent mortgage technology. Had lenders assigned mortgages according to the traditional registry system, then ultimate creditors would not have faced deficiency in their standing to

foreclose. Hence, this result demonstrates that while advances in financial technology may certainly facilitate credit availability for borrowers, such changes may also jeopardize the creditor's security interest and introduce uncertainty around collateral enforceability.

Panel B plots estimates where the variable of interest is debt sale. Once again, the two loan types experience vastly different outcomes. Across all time horizons, I estimate a monotonically increasing effect of the judgment on debt sale for low cost MERS loans, largely reflecting the reduction in foreclosures for this subset. At 12 months after an initial 60-day delinquency, low foreclosure cost MERS loans experience a 6 percentage point increase in debt sale, statistically significant at the 1% level. Turning to high cost loans, estimates of δ_2 are insignificant when measured across all months. As demonstrated in Table 2.6, this evidence suggests that creditors have a robust market to sell their loans only if the cost of foreclosure does not exceed some threshold.

Panel C plots estimates where the outcome variable is equal to one if the borrower remains delinquent (60- or 90-days delinquent). Immediately following an initial 60-day delinquency, high foreclosure cost MERS loans experience a sharp decline in delinquency. The size of the effect is negative and statistically significant. As mentioned earlier, these results suggest that creditors plausibly intervened following an initial 60-day delinquency. I reinforce this hypothesis in Panel D, where the outcome variable is equal to one if a loan enters a 30-day delinquency or becomes current. Within only one month, the size of the increase in self-cure is much larger than the reduction in foreclosure. In stark contrast, low foreclosure cost MERS loans experience no systematic pattern of delinquency or cure. Taken together, these facts suggest that creditors intervened to cure high foreclosure cost MERS loans, likely before foreclosure is typically initiated.

Finally, in Panels E and F, I find limited evidence of short sales and mortgage

modifications, respectively. Here, across high and low foreclosure costs, there appears to be little change for these alternative outcomes. Only in the last few months does there appear to be some increase in short sales and modifications. However, even in this case, the increase in short sales and formal modifications appears exclusively offered to high foreclosure cost MERS loans alone. This delay in negotiated short sale and formal modifications may be in response to the waning effect of initial forbearance.

As an aside, it does appear as though almost all loans that counter-factually would have experienced foreclosure instead experience a self-cure. Figure A.2 plots estimates where the outcome variable is equal to one if the borrower experienced any cure (30-day delinquency or current). Alongside these results, I also plot the estimates for the drop in foreclosure presented in Panel A of Figure 2.7, multiplied by negative one for ease of interpretation as a counter-factual. In Panel A of Figure A.2, I find that eventually all low foreclosure cost MERS loans experience a cure at some point over 12 months since an initial delinquency. This is not reflected in Panel D of Figure 2.7 due to loan sales censoring the cumulative effect. Furthermore, this evidence suggests that self-curing borrowers would have been foreclosed on if not for the Greenleaf judgment, possibly due to financial constraints (Aiello, 2022; Kim et al., 2021). In Panel B of Figure A.2, high foreclosure cost loans experience an initial increase in self-cures, followed by some attenuation. This likely reflects the fact that some loans that experience a cure following delinquency would have eventually cured independent of creditor intervention.

Studying the long-term effect of the Greenleaf judgment reveals that creditors face significant search costs in identifying original lenders. This is evidenced by the persistence of my main results for months after a loan enters delinquency. The timing of these results is also informative. For high foreclosure cost MERS loans, I identify an immediate reduction in delinquency, prior to the timing of foreclosure. This suggests that creditors offer forbearance preemptively, prior to further deterioration in loan

quality. These results reinforce the proposition that only when enforcement costs impede both foreclosure and debt sale do creditors offer forbearance.

2.6 Robustness

In this section, I perform a series of robustness tests to reinforce my main results. First, I show that my results are unchanged when including MERS loans from a neighboring jurisdiction unaffected by the Greenleaf judgment. Second, I find that my results are robust to explicitly controlling for lender identifiers. Third, I find that MERS loans that happened to exclude the key Greenleaf sentence did not experience a drop in foreclosure. I also present evidence that Maine's 2015 legislative response to the Greenleaf judgment failed to make creditors whole. Exploring the judicial history of MERS more broadly, I validate the assumption that the Greenleaf judgment was unanticipated. Finally, I provide suggestive evidence to support the external validity of my research design.

2.6.1 Controlling for Trends Across Jurisdiction

In this subsection, I augment the robustness of my baseline specification by including loans from the neighboring state of New Hampshire, excluding loans from Carroll and Coos counties due to data limitations. The standard difference-in-differences design cannot account for differential changes in outcomes for treated loans that would have occurred independent of the treatment. If MERS loans differed systematically from non-MERS loans in the period following the ruling, then this would bias my estimates of δ . However, if this difference was uniform around a local geography, then including New Hampshire loans would allow me to account for differences shared across state lines. Naturally, MERS loans in New Hampshire were left unaffected by the Greenleaf judgment. Therefore, accounting for shared difference-in-differences across the two states leaves unexplained variation that is likely due to the rise in MERS foreclosure

costs in Maine. To test this, I run OLS regressions and estimate a difference-in-difference-in-differences (DDD) regression of loan outcomes:

$$Y_{it} = \alpha_{zip,t} + \alpha_{vintage,t} + X'_{it}\gamma + \beta_1 MERS_i + \beta_2 MERS_i \times Maine_i + \delta MERS_i \times Post_t + \varphi MERS_i \times Maine_i \times Post_t + \epsilon_{it} \quad (2.6)$$

The variable $Maine_i$ is an indicator equal to one for loans originated in Maine and zero otherwise. The coefficient on $MERS_i \times Post_t$ no longer identifies the causal effect of changes in foreclosure costs. Instead, δ captures the average change in loan outcomes for MERS loans across Maine and New Hampshire following the Greenleaf decision. The coefficient of interest is φ , which measures the differential change in loan outcomes for Maine MERS loans relative to both Maine non-MERS loans and New Hampshire MERS loans following the Greenleaf judgment. I identify φ using variation in loan outcomes over time across MERS and non-MERS loans, controlling for systematic time-varying shocks to MERS and Maine loan outcomes separately.

Table A.3 presents the estimates of regression Equation (2.6). All columns include origination, borrower, and time-varying county-level controls. I further include fixed effects to account for time-varying shocks to loan vintage and zip code, separately. Each column defines a distinct credit event, namely foreclosure, debt sale, severe delinquency, self-cure, short sale, and modification, respectively. In Column (1), I find that MERS loans experience a 0.081 percentage point decline in the foreclosure rate relative to the pre-period, statistically significant at the 5% level. This result is similar to the 0.088 percentage point decline measured in Table 2.2. In Column (2), I find that MERS loans experience a 0.036 percentage point increase in the debt sale rate relative to the pre-period. This is statistically different from zero at the 10% level and represents a 40% increase relative to the baseline MERS rate. Lastly, I find statistically insignificant values for other loan outcomes. Taken together, these results mirror the estimates reported earlier.

Next, I augment the robustness of my earlier specification exploiting heterogeneity in foreclosure costs. In Section 2.5.2, I could not account for time-varying shocks to outcomes for high and low foreclosure cost MERS loans. This is likely a relevant concern, given that variation in the cost of foreclosure is a direct consequence of an original lender’s bankruptcy status. In so far as a lender’s bankruptcy status is correlated with its loan quality, my results might fail to identify the true causal effect of interest. To account for time-varying shocks common within bankruptcy status, I run OLS regressions and estimate a difference-in-difference-in-differences (DDD) regression of loan outcomes:

$$\begin{aligned}
Y_{it} = & \alpha_{zip,t} + \alpha_{vintage,t} + X'_{it}\gamma + \beta_1 Low Cost_i + \beta_2 Low Cost_i \times Maine_i \\
& + \delta_1 Low Cost_i \times Post_t + \varphi_1 Low Cost_i \times Maine_i \times Post_t \\
& + \beta_3 High Cost_i + \beta_4 High Cost_i \times Maine_i \\
& + \delta_2 High Cost_i \times Post_t + \varphi_2 High Cost_i \times Maine_i \times Post_t + \epsilon_{it}
\end{aligned} \tag{2.7}$$

The coefficients of interest are φ_1 and φ_2 , which measure the differential change in loan outcomes for Maine’s low and high foreclosure cost MERS loans, respectively, relative to both Maine non-MERS loans and New Hampshire MERS loans following the Greenleaf judgment.

Table A.4 presents triple difference estimates of regressions analogous to Equation (2.7) using the sample of New Hampshire and Maine loans. For each outcome variable, the last row reports a pairwise t-test statistic for the equivalence of coefficients φ_1 and φ_2 .

In Column (1), I find that Maine high foreclosure cost loans experience a 0.199 percentage point decline in foreclosures, statistically significant at the 5% level. Maine’s low foreclosure cost MERS loans experience a 0.059 percentage point decline in foreclosures, statistically significant at the 10% level. Once again, these point estimates are similar to the point estimates measured in Table 2.6. The pairwise t-test statistic

for the equivalence of coefficients is -1.487. In Column (2), I find that Maine’s low foreclosure cost MERS loans experience a 0.035 percentage point increase in the debt sale rate. In contrast, Maine’s high foreclosure cost MERS loans experience no statistically significant change. Here, the pairwise t-test statistic for the equivalence of coefficients is 0.409.

As before, I estimate that Maine’s high foreclosure cost MERS loans experience a 0.354 percentage point decline in severe delinquencies, statistically different from zero at the 1% level. Maine’s low foreclosure cost MERS loans experience no change in entry into severe delinquency. The pairwise t-test statistic for the equivalence of coefficients is -2.774. Finally, there appears to be no significant change in other loan outcomes.

Next, I test the robustness of my results in Figure 2.7 by re-estimating the results from my local projections exercise using the sample of both Maine and New Hampshire loans. Figure A.3 plots estimates across time horizons of one to 12 months. I find that my results are robust to controlling for common shocks within bankruptcy status across jurisdiction. As before, high foreclosure cost MERS loans experience a larger decline in foreclosure, low foreclosure cost MERS loans experience an increase in debt sale, and high foreclosure cost loans exclusively benefit from what appears to be forbearance.

2.6.2 Exploiting Within Lender Variation

In this subsection, I account for unobserved heterogeneity associated with a particular lender. My research design assumes that creditors face higher foreclosure costs following the Greenleaf judgment due to difficulty in finding original lenders. However, the responsiveness of an originating lender likely correlates with the outcomes of the loans that it originates. If changes in loan outcomes reflected unobserved heterogeneity associated with a particular lender, then the internal validity of my results would

suffer.

This concern is unlikely to be salient for two reasons. First, as demonstrated by the specification including New Hampshire loans, my estimates do not seem to reflect systematic changes in MERS loans across the Northeast region. Rather, the effects that I identify are exclusively identified for Maine MERS loans alone. If lender quality were to bias my estimates, then Maine and New Hampshire mortgage lending would have to be highly segmented to explain the drop in foreclosure exclusive to Maine MERS loans. This is unlikely for MERS loans in particular given that early MERS participants were associated with inter-state mortgage lenders, not localized community banks. Second, under the assumption that any variation associated with a particular lender is time invariant, then including loan-level fixed effects, as in Table 2.2, accounts for concerns related to lender quality by absorbing this variation.

Nevertheless, I can account for time-varying heterogeneity at the lender-level directly by identifying MERS lenders in the data. Here, I take advantage of HMDA data to identify lenders at the highest organizational level. I restrict my analysis to MERS loans due to limited coverage of lender identifiers for non-MERS loans. Furthermore, I focus on loans originated after January 2005 to account for stability in identified lenders after this point. I run OLS regressions using the loan-month sample of exclusively MERS loans originated across Maine and New Hampshire. I estimate a difference-in-differences regression of loan outcomes:

$$Y_{it} = \alpha_{lender,zip} + \alpha_{lender,vintage,t} + X'_{it}\gamma + \delta Maine_i \times Post_t + \epsilon_{it} \quad (2.8)$$

The coefficient of interest is δ , which measures the differential change in loan outcomes among Maine MERS loans relative to New Hampshire MERS loans following the Greenleaf judgment. I identify δ using within lender variation in loan outcomes across Maine and New Hampshire. I include fixed effects for the lender interacted with the zip code and the quarter of origination, separately. I also interact quarter of

origination with quarter of observation, jointly with the lender identifier. Including an interaction of zip code with quarter of observation would render the treatment effect unidentified. This is because my specification does not include non-MERS loans, meaning that identification relies on time-variation in loan outcomes at the level of geography. Finally, standard errors are clustered at the level of the lender.

Table A.5 shows results from difference-in-differences regressions that interact the Maine dummy with the Post variable. In Columns (1) to (3), I present results using regression Equation (2.8) using outcomes of foreclosure, debt sale, and delinquency, respectively. In Columns (1) and (2), the estimated coefficients for foreclosure and debt sale are qualitatively similar to those in Table 2.2 and Table A.3, statistically significant at the 1% level and 10% level respectively. Delinquency, as before, is statistically insignificant.

The estimates presented in Table A.5 do not directly compare to earlier results because I restrict the sample to (i) exclusively MERS loans (ii) loans originated after January 2005 and (iii) loans matched to a lender identifier. Furthermore, the specification in Columns (1) to (3) of Table A.5 do not include fixed effects for the quarter of observation interacted with the zip code. In order to better compare these point estimates to earlier results, I take the full sample of MERS loans originated after 2005 across New Hampshire and Maine, independent of whether assigned a lender identifier. In Columns (4) to (6), I present results without restricting identification to with-lender variation. Here I find results similar to those in Columns (1) to (3). Taken together, this would suggest that the fully specified model exploiting the full sample likely reflects limited bias without lender-level fixed effects.

2.6.3 Placebo Test Using Untreated MERS Loans

In this subsection, I further support my results from earlier by exploiting a unique feature within a subset of Maine MERS mortgage contracts. While most MERS

mortgages in Maine had the sentence “For purposes of recording this mortgage...,” there were some mortgages that did not. The MERS mortgage contract without the Greenleaf wording had no exposure to treatment, just as non-MERS loans, purely by the letter of the law. Indeed, an article published by the American Bar Association in 2020 highlights this alternative wording that protects a subset of MERS mortgage contracts from the Greenleaf judgment. Unlike mortgages with the key language from the Greenleaf judgment, “mortgages that do not limit MERS’s interest to recording purposes should not create similar impediments to foreclosure.”⁸

I use MERS loans without the key sentence as a placebo test for the sample of MERS loans with the key sentence. In order to leverage this heterogeneity, I augment Equation (2.1):

$$\begin{aligned}
 Y_{it} = & \alpha_{zip,t} + \alpha_{vintage,t} + X'_{it}\gamma + \beta_1 Treated_i + \delta_1 Treated_i \times Post_t \\
 & + \beta_2 Placebo_i + \delta_2 Placebo_i \times Post_t + \epsilon_{it}
 \end{aligned}
 \tag{2.9}$$

The variable $Placebo_i$ is an indicator for MERS loans originated without the key sentence while the variable $Treated_i$ is an indicator for all other MERS loans. I classify MERS loans as $Treated_i$ by searching through mortgage contracts that contained some variation of the Greenleaf sentence. I find that roughly 90% of MERS mortgage contracts contained the key Greenleaf language. If MERS loan outcomes changed systematically for reasons unrelated to the Greenleaf judgment, the MERS loans without the key sentence would also reflect this change.

Table A.6 shows results from difference-in-differences regressions that interact Treated and Placebo dummies with the Post variable. For each outcome variable, the last row reports a pairwise t-test statistic for the equivalence of coefficients δ_1 and δ_2 .

Columns (1) and (2) use the outcome variables of foreclosure and debt sale, respectively. The estimated coefficients on the $Treated_i \times Post_t$ interaction term are

⁸ Kevin Hudspeth, “MERS’s ‘Maine’ Purpose: Recognizing Key Differences Between MERS Mortgages”, American Bar Association, April 18, 2020.

qualitatively similar to those in Table 2.2 and Table A.3, statistically significant at the 1% level. The pairwise t-test statistic comparing the two coefficients in Column (1) is statistically significant at the 5% level. Coefficient estimates for the $Placebo_i \times Post_t$ interaction term for all loan outcomes are small and statistically insignificant, except for in the case of debt sale.

For debt sale, I find that placebo loans experience a 0.054 percentage point increase in debt sale, statistically different from zero at the 5% level and statistically equivalent to the estimate for treated loans. This result is puzzling given the fact that untreated MERS loans experienced no drop in foreclosure. I provide two possible explanations for this rise in the debt sale rate. First, creditors may choose to sell untreated MERS loans due to balance sheet losses associated with the Greenleaf judgment. This seems unlikely to be the case, as discussed in Section 2.6.2, given that the differential effect of the Greenleaf judgment on Maine’s MERS loans is *larger* than the baseline when compared to New Hampshire MERS loans in Table A.3. This would require creditors to be highly segmented along state lines to explain the increase in debt sales exclusive to Maine MERS loans. As a second explanation, creditors holding Maine MERS loans plausibly sold untreated MERS loans in their portfolio preemptively. This is evidenced by the lack of any reduction in the foreclosure rate, suggesting that the loans that were sold would not have been foreclosed on in any case.

2.6.4 Measuring the Effect Across Legislative Periods

In this subsection I show that my results are not driven by creditors anticipating legislative action in the wake of the Greenleaf judgment. If creditors believed that the legislature would remove the requirements imposed by the ruling, then a drop in foreclosure would reflect an anticipatory pause instead of higher foreclosure costs. Indeed, in July 2015, the legislature passed a law in response to the Greenleaf judgment to address title issues related to loans that were discharged via MERS prior

to the ruling. This would ensure clean title to all Maine properties if MERS was ever an affiliated party in past foreclosure cases. However, the legislature intentionally maintained the requirement that the Greenleaf judgment imposed on creditors for the benefit of consumers. Therefore, I hypothesize that the effect of the Greenleaf judgment should be unaffected by the state legislature's actions. To test this, I augment my main specification:

$$Y_{it} = \alpha_{zip,t} + \alpha_{vintage,t} + X'_{it}\gamma + \beta MERS_i + \delta_1 MERS_i \times Pre-Law_t + \delta_2 MERS_i \times Post-Law_t + \epsilon_{it} \quad (2.10)$$

In the equation above, the *Pre-Law_t* variable takes a value of one if a loan is observed following the Greenleaf judgment but prior to the law change. The *Post-Law_t* variable takes a value of one for the period following the law change. The coefficients of interest are δ_1 and δ_2 , which identify the differential change in loan outcomes in the pre-law and post-law period, separately.

Table A.7 shows results from difference-in-differences regressions that interact the MERS dummy with the *Pre-Law_t* and *Post-Law_t* variables, separately. Each column defines a distinct credit event, namely foreclosure, debt sale, severe delinquency, self-cure, short sale, and modification, respectively. For each outcome variable, the last row reports a pairwise t-test statistic for the equivalence of coefficients δ_1 and δ_2 .

In Columns (1) and (2), I report estimates of regression Equation (2.10) using outcomes of foreclosure and debt sale, respectively. The estimated coefficients on the $MERS_i \times Pre-Law_t$ interaction term and $MERS_i \times Post-Law_t$ interaction are statistically pairwise equivalent and individually significant at the 1% level. These estimates are also qualitatively similar to those in Table 2.2 and Table A.3. Estimates across all other outcomes, except for self-cure, are statistically insignificant.

For self-cures, I find that MERS loans experience a 0.096 percentage point increase, statistically different from zero at the 5% level. I provide two possible explanations

for this rise in the self-cure rate. First, loans that failed to enter foreclosure due to the Greenleaf judgment likely took time to self-cure. This is evidenced by Figure A.2, as discussed in Section 2.5.3. Second, the forbearance identified in Section 2.5.3 might have been a consequence of creditors learning about the legislature’s action. In light of this, my earlier results may warrant an additional condition—creditors only offer forbearance when foreclosure becomes prohibitively expensive, the secondary market unwinds, and the legislature fails to make creditors whole.

2.6.5 Judicial History of MERS

In this subsection, I examine the degree to which the Greenleaf judgment served as a plausible shock to creditor expectations based on the judicial history of MERS. I begin by recognizing that creditors holding MERS loans faced varied legal treatment across the country. Meek (2015) classifies the treatment of MERS as either strict or lenient depending on the jurisdiction. In the most lenient category, which includes states such as Florida, Minnesota, and Nevada, MERS has standing to foreclose, independent of the ultimate creditor identified on foreclosure documentation. To highlight the strictest treatment, Meek (2015) uses the Massachusetts 2011 Ibanez judgment as a case study. In this well-advertised ruling, as in similar rulings across the US, the Massachusetts state court required that the current note holder be identified and assigned the mortgage prior to foreclosure.

In this same spirit, Maine’s 2010 Saunders judgment forced creditors to foreclose in their own name as opposed to foreclosing in the name of MERS. The basis for this was the very sentence used in the Greenleaf judgment four years later. At the time, however, the role of the sentence was never considered to imply that creditors holding MERS loans lost standing to foreclose entirely. In a literal sense, it simply meant that creditors with MERS mortgages would adjust business-as-usual in name only. While frustrating, this did not fundamentally alter the nature of foreclosure for creditors

that held valid promissory notes with a MERS registered mortgage. Indeed, holding both the note and the mortgage is the hallmark of a perfected security interest. Where Greenleaf took a radical departure was to rely upon the unorthodox interpretation of a single word to dismantle this seemingly perfected security interest.

There are several reasons to believe that the 2014 Greenleaf judgment was unanticipated by the 2010 Saunders judgment. First, Aromando (2014) confirms what all Maine attorneys knew at the time—the key sentence in the initial Saunders case was only problematic in so far as it required substituting one party of interest with another in name only. In fact, Aromando (2014) notes that prior to the Greenleaf ruling, the Maine Supreme Court itself sided with its own century-old legal principle that “the mortgage follows the note” in several cases following the Saunders judgment. Second, in a review of MERS case law at the time, Hunt et al. (2011) affirm that there appeared to be no major issues in variation across MERS form documents that would render them unenforceable. Even more, the authors claim that, despite a colored litigation history following the financial crisis, courts have largely validated the authority held by MERS to ultimately assign mortgages. Finally, in its 2020 Gordon judgment, the High Court’s opinion supported what Aromando (2014) argued 6 years prior. Here, Justice Horton explicitly stated that the High Court never relied on the Saunders judgment to dismiss foreclosure cases prior to the Greenleaf ruling. Hence, across practicing attorneys, legal scholars, and the High Court itself, the implication of the Saunders decision failed to translate into its ultimate interpretation prior to the 2014 Greenleaf judgment.

To further corroborate this, Figure A.4 plots estimates of the regression Equation (2.2) using foreclosure as the outcome of interest and a full four year window around the timing of the Greenleaf judgment. The narrow window of just one year preceding the judgment in Figure 2.6 limited mismeasurement by focusing on all loans active around the time of the ruling. In contrast, using a wider window, my sample censors

loans that would reappear within a narrower bandwidth due to incomplete foreclosure, biasing my results. Nevertheless, the benefit of a longer sample is that I can capture the long judicial history of MERS throughout the post-financial crisis period.

If creditors faced difficulty in foreclosing on MERS loans in the state of Maine prior to the Greenleaf judgment, the foreclosure rate for MERS loans, relative to non-MERS loans, would reflect this. Instead, in Figure A.4, I find no differential trends prior to the timing of the Greenleaf judgment. Following Maine's 2010 Saunders judgment and the 2011 Ibanez ruling out of Massachusetts, there appears to be no significant difference across Maine MERS and non-MERS foreclosure rates. Only in the wake of the Greenleaf judgment does there appear to be a break in trend for MERS loans.

2.6.6 External Validity

The preceding research design may be subject to concerns over external validity along two dimensions. First, my results are naturally a product of a highly stylized lending market. As a state, Maine is the tenth least populated, the farthest northeast, and largely rural. Hence, I need to demonstrate that the insights drawn from the Greenleaf judgment can translate to other markets. Second, even if Maine serves as a valid laboratory, the question remains whether other periods of costly foreclosure can corroborate the dynamics identified in my setting. Along these lines, substitution into debt sale may be muted during times of crisis when other mortgage market frictions are more salient. Therefore, to prove the broader relevancy of the secondary market, debt sale should substitute for foreclosure during periods of costly foreclosure more broadly.

To address both of these concerns I plot the foreclosure and debt sale rate over time in Figure A.5. Here, I use the full CoreLogic LLMA sample, spanning all originations between 2002 and 2007 across the entire United States. I measure the rate at which a 60-day delinquent loan enters foreclosure or debt sale. My sample of delinquent

debt represents 9.5 million loans and covers loan outcomes between 2003 and 2012. For comparison, in addition to spanning the entire United States, Figure A.5 also includes the equivalent foreclosure and debt sale rate for Maine loans exclusively.

To extrapolate to other localities, Figure A.5 demonstrates that Maine loans experience rates of foreclosure and debt sale in a manner similar to that of the rest of the country. This is evident by the near lock step changes in both foreclosure and debt sale over time. Despite having a unique legal regime surrounding mortgage law, Maine loans experience a rise and fall in foreclosures resembling that of the broader United States, both in relative and absolute terms. Debt sale similarly rises for both Maine and the rest of the country following the financial crisis. The strong relationship between the local lending market and the broader economy should add confidence to the external validity of my research design beyond the state of Maine.

To extrapolate to other time periods, Figure A.5 also provides suggestive evidence of a substitution between debt sale and foreclosure following the financial crisis. By the end of 2010, several major servicers suspended evictions across the country due to accusations of “robo-signing”, namely, falsifying legal documents to expedite foreclosure proceedings. Naturally, these mortgage servicers, and by extension investors, suffered increased foreclosure costs due to both waiting to foreclose and adjusting a previously streamlined foreclosure process. Figure A.5 marks the quarter preceding the robo-signing scandal with a dashed black line. Immediately following the scandal, debt sale rose and foreclosures fell, plausibly to resolve the unexpected increase in foreclosure costs.

The preceding discussion comes with a caveat on measurement. First, the debt sale rate plotted in Figure A.5 suffers from the same measurement issue discussed in Section 2.4.3. Namely, my measure of debt sale is derived from a variable identifying when a loan exits the sample due to the sale of servicing rights. This can be due to either the true sale of servicing rights or the sale of the loan off of a creditor’s

balance sheet entirely. As demonstrated earlier, this sale of servicing rights likely corresponds to the sale of the loan itself, evidenced by Maine’s registry of deeds data. However, even if the variable fails to correspond entirely to debt sale, it likely underestimates the true level of debt sale since loans can change investors while still providing performance data to CoreLogic through shared servicers managing the same loan.

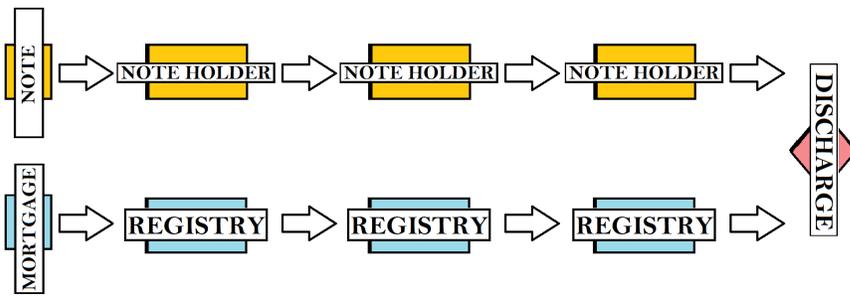
Taken together, Figure A.5 provides suggestive evidence that the insight drawn from the Greenleaf judgment can apply to other settings, both across markets and time. Anecdotally, the practice of buying seasoned mortgage debt is commonplace. For example, Goldman Sachs has purchased over \$10 Billion in non-performing and reperforming mortgage debt from Fannie Mae and Freddie Mac through several deals over the past five years (Lane, 2018). Ultimately, the key insight of the Greenleaf judgment lies in revealing the preference that creditors hold for selling debt rather than renegotiating when foreclosure becomes costly. Given how well Maine reflects national mortgage market trends in foreclosure and debt sale, this insight is likely broadly applicable beyond my particular setting alone.

2.7 Conclusion

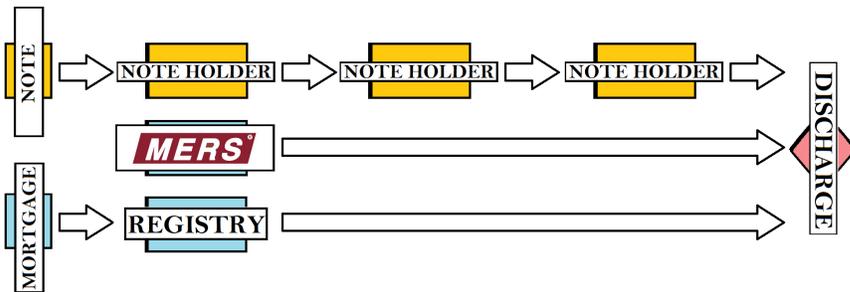
This paper identifies a novel mechanism, whereby access to the secondary market functions to replace an initial creditor’s need to renegotiate. Using a hand-collected dataset of mortgage contracts and a quasi-experimental research design, I show that when faced with costly enforcement, creditors rely upon the secondary market to sell their delinquent debt instead of renegotiating with borrowers. Only when foreclosure becomes prohibitively expensive, at which point the market unwinds, do creditors appear to offer forbearance. Furthermore, I demonstrate that the secondary market for delinquent debt is highly concentrated and features significant variation in foreclosure propensity. Of the loans that are sold in lieu of renegotiation, 90% are purchased by

four investors alone. Finally, I provide a battery of robustness tests that account for differential trends across jurisdiction, lender, and legislative periods.

The results in this paper have several implications for future research and policy design. A central concern since the financial crisis has revolved around understanding renegotiation in mortgage markets. The evidence in this paper suggests the secondary market functions as a direct substitute for renegotiation. If investors offer a price higher than the value of renegotiation, a creditor will choose to sell their debt. To that end, lawmakers should assess how debt relief policy can affect the foreclosure appetite of a loan's future creditors. In sum, this paper presents novel insight into the relationship between collateral enforcement and the secondary market.



Panel A: Selling Without MERS



Panel B: Selling With MERS

FIGURE 2.1. Process of Selling a Mortgage Loan

Note: This figure illustrates the process of selling a mortgage loan. Panel A provides an example where a lender originates a loan without MERS. The note holder must take possession of the promissory note and the mortgage contract is recorded in the public registry of deeds. To sell a mortgage loan, the buyer must take possession of the promissory note and the seller must assign the mortgage to the new note holder in the public registry of deeds. Panel B provides an example where a lender originates a loan with MERS. While the seller still must physically pass the promissory note, MERS will act as the mortgagee for all current and future note holders until the time of discharging the mortgage.

(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

Panel A: Maine MERS 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: New Hampshire MERS Contract

FIGURE 2.2. Variation in MERS Mortgage Contracts

Note: This figure presents excerpts from MERS mortgage contracts across Maine and New Hampshire. MERS mortgage contracts are standardized within a given state and each excerpt identifies the portion of the state-specific contract that designates MERS as the note holder's nominee. Panel A presents an example from Maine's MERS mortgage contract. Panel B presents an example from New Hampshire's MERS mortgage contract. In a given state, virtually every MERS mortgage contract will contain the same standardized language.

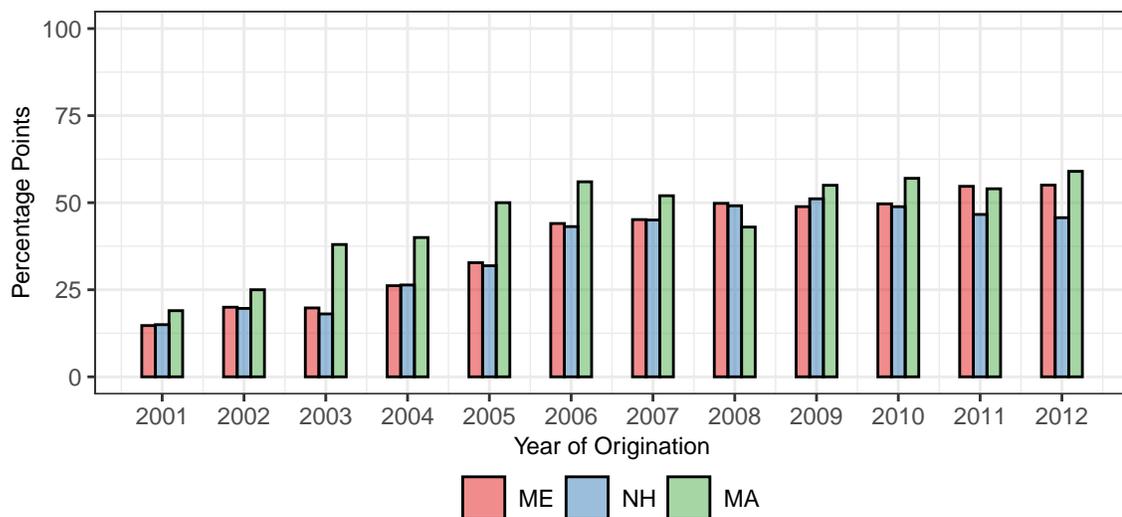


FIGURE 2.3. MERS Loans as a Percent of All Originations

Note: This figure plots the time series of mortgages that are registered with MERS as a fraction of all mortgages originated in a given year between January 2001 and December 2012 using data from the Maine Registry of Deeds, the New Hampshire Registry of Deeds, CoreLogic, and Table 1 of Lewellen and Williams (2021). Each bar provides the percentage of newly originated loans that were registered with MERS at origination.

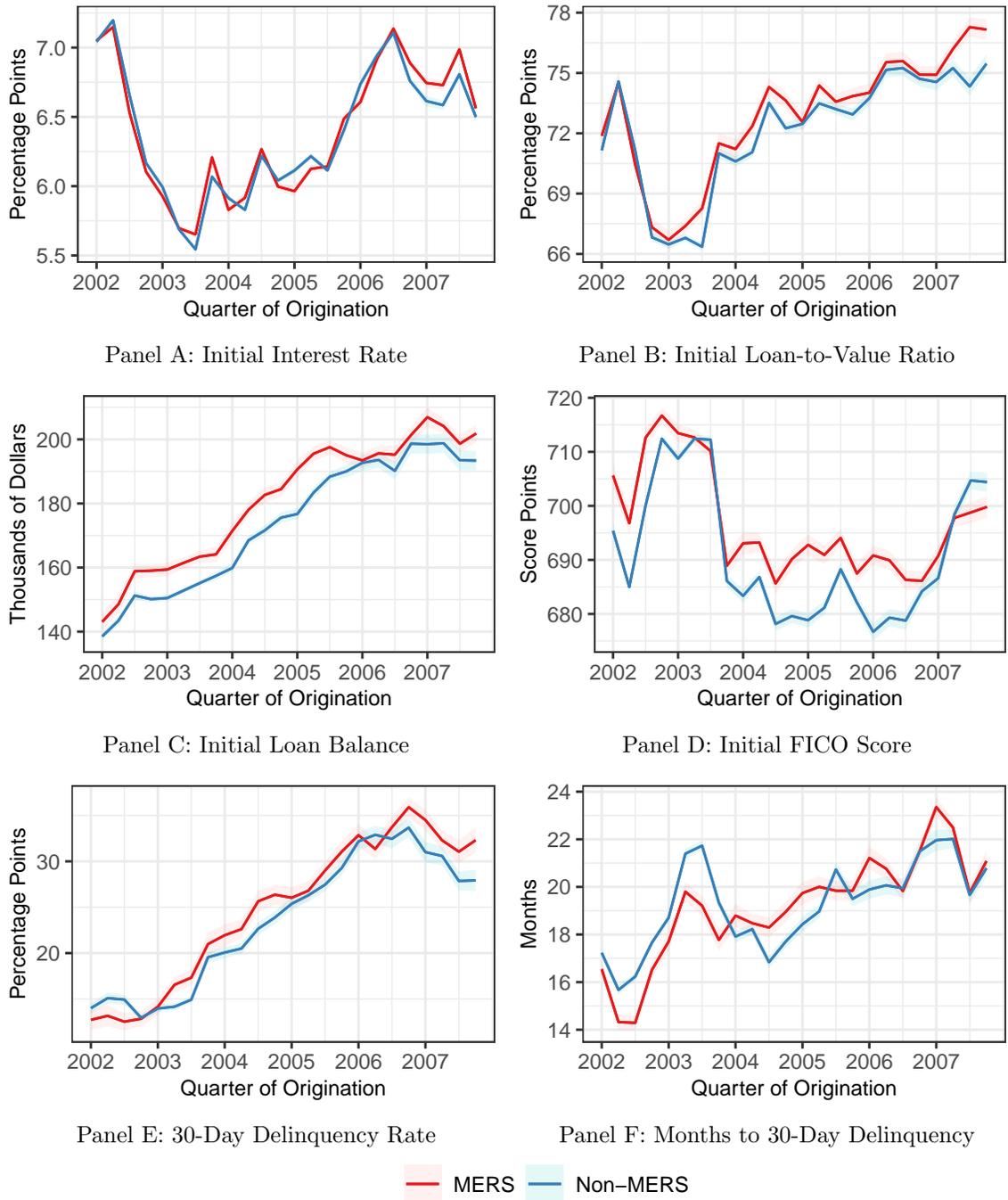
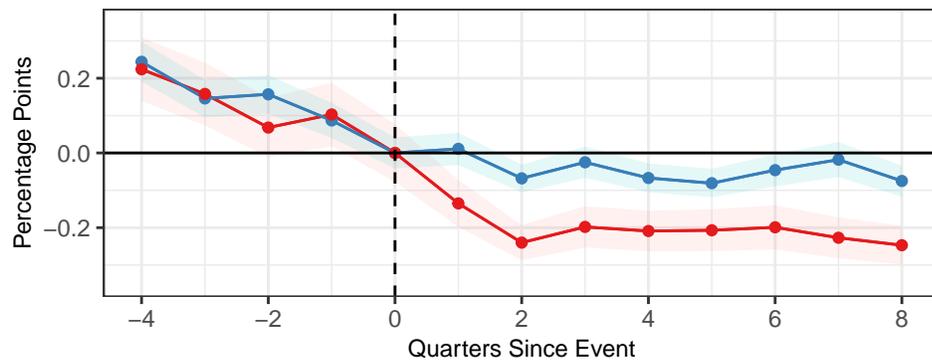


FIGURE 2.4. Loan Characteristics Across Origination Quarter and MERS Status

Note: This figure plots loan characteristics of MERS and non-MERS loans across quarter of origination. Loans are originated between January 2002 and December 2007. Panel A plots the initial interest rate. Panel B plots the initial (uncombined) loan-to-value ratio. Panel C plots the initial FICO credit score. Panel D plots the initial loan balance. Panel E plots the 30-day delinquency rate. Panel F plots the average time to delinquency.



Panel A: Maine

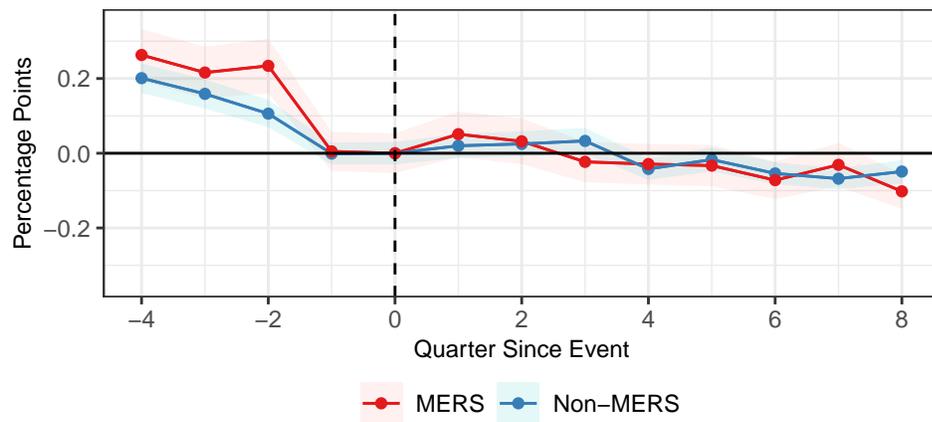
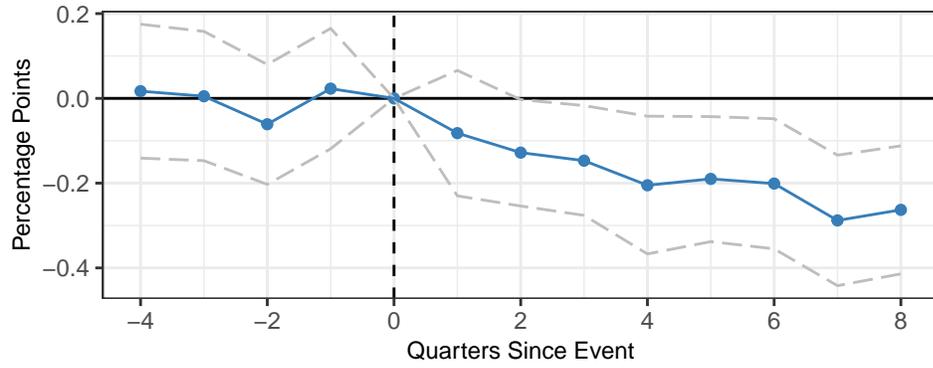
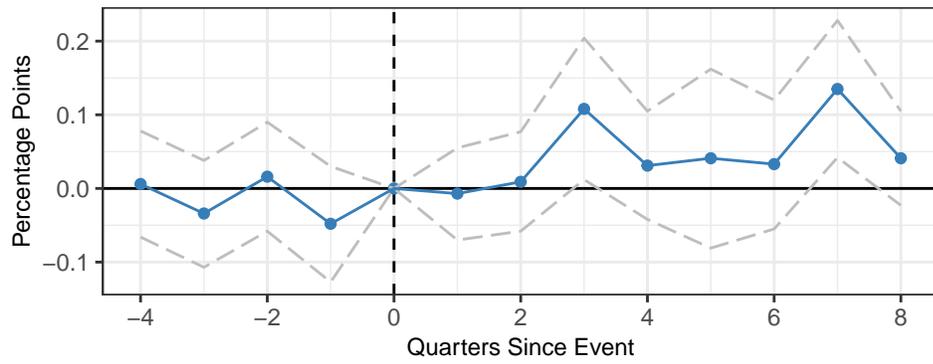


FIGURE 2.5. Empirical Foreclosure Rate Relative to the Baseline Quarter

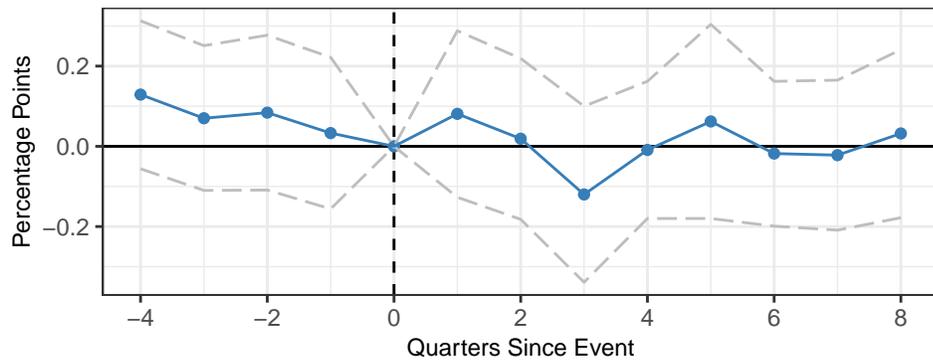
Note: This figure plots the empirical foreclosure rate of MERS and non-MERS loans across quarters since the Greenleaf judgment. Loans are originated between January 2002 and December 2007. Foreclosure is observed between April 2013 and June 2016. Panel A plots the foreclosure rate for Maine. Panel B plots the foreclosure rate for New Hampshire. Each dot represents a quarterly transition rate into foreclosure, demeaned relative to the baseline quarter preceding the judgment. The black dashed vertical line indicates the date of the baseline quarter. The light shaded region represents 95% confidence intervals calculated using standard errors. Data is collected from the Maine Registry of Deeds, the New Hampshire Registry of Deeds, and CoreLogic.



Panel A: Foreclosure



Panel B: Debt Sale



Panel C: Delinquency

FIGURE 2.6. Difference-in-Differences Estimates

Note: This figure reports difference-in-difference estimates of the effect of the Greenleaf judgment on the probability of a credit event. Estimates are derived from a difference-in-differences regression that interacts MERS and quarter fixed effects. The coefficient for the baseline quarter preceding the judgment is normalized to zero. The black dashed vertical line indicates the date of the baseline quarter. The light dashed line represents 95% confidence intervals calculated using zip code-clustered standard errors. Outcomes are observed between April 2013 and June 2016 for loans originated between 2002 and 2007 in Maine. Data is collected from the Maine Registry of Deeds and CoreLogic.

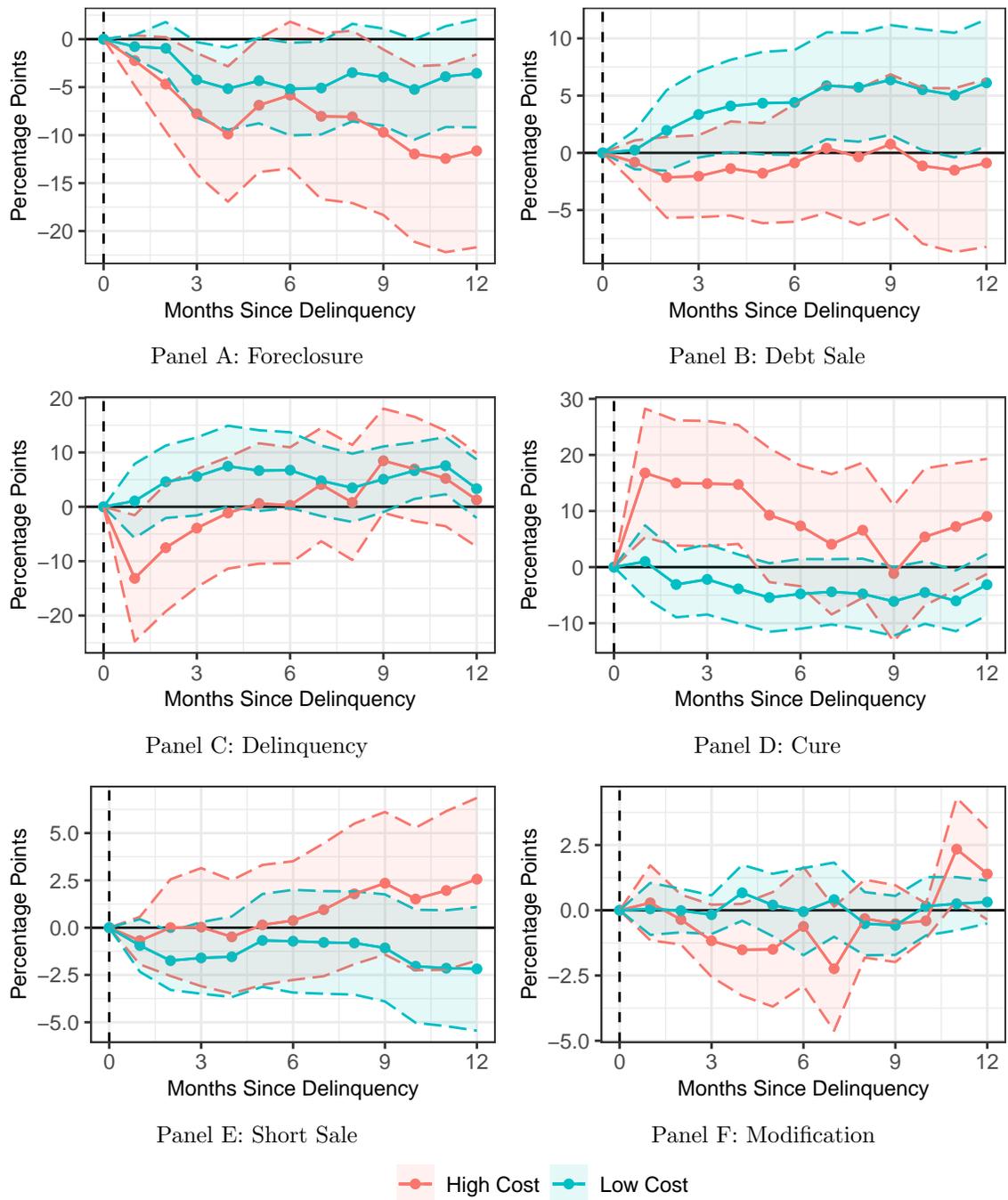


FIGURE 2.7. Heterogeneous Impulse Response Following Initial 60-day delinquency

Note: This figure reports difference-in-difference estimates of the heterogeneous effect of the Greenleaf judgment on the probability of a credit event over multiple time horizons after an initial 60-day delinquency. Results are derived from an estimation of Equation (2.5), separately estimated at each time horizon. The shaded region represents 95% confidence intervals calculated using zip code-clustered standard errors.

Table 2.1. Summary Statistics

	All Loans		MERS		Non-MERS	
	Mean	SD	Mean	SD	Mean	SD
<i>Panel A: Loan Characteristics</i>						
Initial Rate	6.23	1.20	6.47	1.26	6.11	1.16
Original LTV	71.28	17.28	73.34	16.68	70.30	17.47
Balance (000's)	146.16	86.42	156.02	87.83	141.45	85.34
15 Month Term	0.18	0.39	0.12	0.33	0.21	0.41
20 Month Term	0.07	0.26	0.04	0.19	0.09	0.29
Prime Borrower	0.80	0.40	0.78	0.41	0.81	0.39
Refinance Loan	0.54	0.50	0.51	0.50	0.55	0.50
Primary Occupancy	0.83	0.37	0.82	0.38	0.84	0.37
Single-Family	0.88	0.32	0.88	0.33	0.89	0.32
Low Doc/No Doc	0.30	0.46	0.30	0.46	0.30	0.46
<i>Panel B: Loan Performance: All Loans</i>						
Foreclosure	0.24	4.84	0.34	5.83	0.19	4.31
Debt Sale	0.07	2.71	0.09	3.05	0.06	2.54
Delinquency	0.29	5.38	0.25	5.03	0.31	5.56
Cure	0.29	5.36	0.31	5.57	0.28	5.27
Short Sale	0.01	1.21	0.02	1.24	0.01	1.20
Modification	0.13	3.66	0.17	4.16	0.12	3.41
Number of Obs	31,545		10,192		21,353	

Note: This table reports mean and standard deviation values for loan characteristics and loan performance for all conventional mortgages observable between April 2013 and June 2016, originated between January 2002 and December 2007, for which CoreLogic reports non-missing prime status, an original term of 30, 20, or 15 months, an interest rate below 20 percentage points, an LTV below 150 percentage points, and a loan balance below \$2 million. MERS status is identified using the Maine Registry of Deeds. Panel A reports values for loan characteristics. Panel B reports values for loan outcomes using the full sample. The outcome variable takes a value of one if a loan experiences a particular credit event while holding a delinquency status of 60 days or worse, upon which it drops out of the sample. Each row defines a distinct credit event, namely foreclosure, debt sale (servicing sold), delinquency (90-days delinquent from 60-days delinquent), cure (30-days delinquent or current from 60-days delinquent), short sale (zero balance), and modification, respectively. In Panel B, the outcome variable is measured in the month preceding the Greenleaf judgment and multiplied by 100 in order to interpret as percentage points.

Table 2.2. Effect of Greenleaf Judgment on Foreclosure and Sale: Difference-in-Differences

	Foreclosure			Debt Sale		
	(1)	(2)	(3)	(4)	(5)	(6)
MERS	0.005 (0.025)	-0.006 (0.030)		-0.020 (0.013)	-0.026 (0.016)	
MERS×Post	-0.087*** (0.027)	-0.105*** (0.033)	-0.171*** (0.030)	0.050*** (0.016)	0.067*** (0.020)	0.056*** (0.018)
Vintage-Time FE	X			X		
Zip-Time FE	X			X		
Vintage-Zip-Time FE		X	X		X	X
Loan FE			X			X
Obs	726,526	726,526	726,526	726,526	726,526	726,526

Note: This table reports difference-in-differences estimates of the effect of the Greenleaf judgment on the monthly probability of foreclosure or debt sale. Observations are at the loan-month level. The outcome variable takes a value of one if a loan experiences a particular credit event while holding a delinquency status of 60 days or worse, upon which it drops out of the sample. In Columns 1 to 3, the credit event of interest is foreclosure. In Columns 4 to 6, the credit event of interest is debt sale (servicing sold). The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Columns 1 to 6 report estimates from difference-in-differences regressions that interact MERS and Post dummies. The Post dummy takes a value of one if a loan is observed on or after the month of the judgment (July 2014). Loan level controls include the origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, single-family status, and log of the previous period's balance. Annual county level controls at the time of event include the log of the house price index, GDP, population, and income, as well as the year-end unemployment rate. All columns include fixed effects for the quarter of observation interacted with quarter of origination and the zip code, separately. Columns 2 and 5 include fixed effects for the quarter of observation interacted with quarter of origination and the zip code, jointly. Columns 3 and 6 include fixed effects for a particular loan. Standard errors are reported in parentheses and are clustered at the zip code level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of April 2013 to June 2016 for loans originated between 2002 and 2007 in Maine.

Table 2.3. Effect of Greenleaf Judgment on Alternative Outcomes: Difference-in-Differences

	Delinquency	Cure	Short Sale	Modification
	(1)	(2)	(3)	(4)
MERS	-0.051* (0.029)	-0.078** (0.031)	-0.015 (0.010)	-0.011 (0.014)
MERS×Post	0.006 (0.035)	0.044 (0.035)	0.004 (0.012)	-0.012 (0.017)
Vintage-Time FE	X	X	X	X
Zip-Time FE	X	X	X	X
Obs	592,033	689,291	726,526	713,008

Note: This table reports difference-in-differences estimates of the effect of the Greenleaf judgment on the monthly probability of loan outcomes. Observations are at the loan-month level. The outcome variable takes a value of one if a loan experiences a particular credit event while holding a delinquency status of 60 days or worse, upon which it drops out of the sample. Each column defines a distinct credit event, namely delinquency (90-days delinquent from 60-days delinquent), cure (30-days delinquent or current from 60-days delinquent), short sale (zero balance), and modification, respectively. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Columns 1 to 4 report estimates from difference-in-differences regressions that interact MERS and Post dummies. The Post dummy takes a value of one if a loan is observed on or after the month of the judgment (July 2014). Loan level controls include the origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, single-family status, and log of the previous period's balance. Annual county level controls at the time of event include the log of the house price index, GDP, population, and income, as well as the year-end unemployment rate. All columns include fixed effects for the quarter of observation interacted with quarter of origination and the zip code, separately. Standard errors are reported in parentheses and are clustered at the zip code level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of April 2013 to June 2016 for loans originated between 2002 and 2007 in Maine.

Table 2.4. Foreclosure and Discharge Rate After Debt Sale

	All Buyers	Fannie Mae	First	Second	Third
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Initial Purchases</i>					
Percent Bought	100.0	43.4	24.5	17.0	5.7
Average Months Since First Purchase	8.1	10.1	8.4	5.0	9.7
Foreclosure Rate	23.6	8.7	42.3	33.3	33.3
Discharge Rate	40.6	43.5	30.8	50.0	66.7
Average Months to Foreclosure	36.4	49.8	24.0	50.8	43.0
Average Months to Discharge	47.0	40.6	38.9	61.1	66.8
<i>Panel B: Purchases from Fannie Mae</i>					
Percent Bought	100.0	-	42.3	19.2	15.4
Average Months Since First Purchase	5.4	-	0.4	25.4	1.5
Foreclosure Rate	15.4	-	0.0	40.0	25.0
Discharge Rate	23.1	-	27.3	0.0	75.0
Average Months to Foreclosure	14.8	-	-	10.0	16.0
Average Months to Discharge	27.5	-	24.3	-	30.7

Note: This table reports summary statistics for a sample of 106 MERS loans originated between January 2002 and December 2007 that experience a sale recorded in the Maine Registry of Deeds between July 2014 and June 2017. Panel A reports statistics for the entire sample. Panel B reports statistics for subsequent trades executed by Fannie Mae. Column 1 presents statistics for all loans. Column 2 presents statistics for Fannie Mae. Column 3 to 5 present statistics for the three largest purchasers, not including Fannie Mae, respectively. In Panel A, the three largest purchasers are BNY Mellon, Ditech Financial, and WSFS Bank. In Panel B, the three largest purchasers are Nationstar Mortgage, US Bank, and MTGLQ Investors. Outcomes are measured independent of whether the initial purchaser sold the loan again prior to an outcome occurring.

Table 2.5. Effect of Greenleaf Judgment Across Markets: Difference-in-Differences

	Fce.	Debt Sale	Delinq.	Fce.	Debt Sale	Delinq.
	(1)	(2)	(3)	(4)	(5)	(6)
MERS	-0.026 (0.031)	0.009 (0.017)	-0.032 (0.037)	-0.060** (0.029)	-0.011 (0.013)	-0.108*** (0.032)
Late Vintage	0.047 (0.046)	-0.044* (0.026)	-0.031 (0.053)			
MERS×Post	-0.021 (0.032)	0.006 (0.023)	-0.022 (0.041)	0.015 (0.035)	0.007 (0.018)	0.058 (0.039)
MERS×Late Vintage×Post	-0.104** (0.052)	0.069** (0.033)	0.044 (0.067)			
Low FICO				0.080 (0.053)	0.003 (0.027)	0.039 (0.067)
MERS×Low FICO×Post				-0.181*** (0.060)	0.077** (0.034)	-0.057 (0.075)
Vintage-Time FE	X	X	X	X	X	X
Zip-Time FE	X	X	X	X	X	X
Obs	726,841	726,841	592,033	592,072	592,072	484,209

Note: This table reports regression estimates of the effect of the Greenleaf judgment on the monthly probability of loan outcomes. Observations are at the loan-month level. The outcome variable takes a value of one if a loan experiences a particular credit event while holding a delinquency status of 60 days or worse, upon which it drops out of the sample. Each column defines a distinct credit event, namely foreclosure, debt sale (servicing sold), and delinquency (90-days delinquent from 60-days delinquent). The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Columns 1 to 6 report estimates that control for differential trends across MERS status, FICO score, and loan vintage. Columns 1 to 3 interact MERS and Post dummies with a Late Vintage dummy. The Late Vintage dummy takes a value of one if the loan was originated between 2005 and 2007. Columns 4 to 6 interact MERS and Post dummies with a Low FICO dummy. The Low FICO dummy takes a value of one if the loan has a FICO score below the median FICO score. The Post dummy takes a value of one if a loan is observed on or after the month of the judgment (July 2014). Loan level controls include the origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, single-family status, and log of the previous period's balance. Annual county level controls at the time of event include the log of the house price index, GDP, population, and income, as well as the year-end unemployment rate. All columns include fixed effects for the quarter of observation interacted with quarter of origination and the zip code, separately. Standard errors are reported in parentheses and are clustered at the zip code level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of April 2013 to June 2016 for loans originated between 2002 and 2007 in Maine.

Table 2.6. Effect of Greenleaf Judgment Across Lender Bankruptcy: Difference-in-Differences

	Fce.	Debt Sale	Delinq.	Cure	Short Sale	Mod.
	(1)	(2)	(3)	(4)	(5)	(6)
High Cost	0.144** (0.063)	-0.020 (0.029)	0.152** (0.074)	0.027 (0.060)	-0.034* (0.019)	-0.029 (0.031)
Low Cost	-0.019 (0.025)	-0.019 (0.014)	-0.084*** (0.030)	-0.096*** (0.032)	-0.012 (0.010)	-0.008 (0.015)
High Cost × Post	-0.231*** (0.071)	-0.028 (0.035)	-0.289*** (0.090)	0.040 (0.084)	0.007 (0.025)	-0.023 (0.039)
Low Cost × Post	-0.063** (0.026)	0.064*** (0.017)	0.053 (0.036)	0.044 (0.034)	0.003 (0.011)	-0.010 (0.018)
Vintage-Time FE	X	X	X	X	X	X
Zip-Time FE	X	X	X	X	X	X
Obs	726,526	726,526	592,033	689,291	726,526	713,008
Pairwise t-test	-2.434**	-2.408**	-3.725***	-0.062	0.175	-0.327

This table reports difference-in-differences estimates of the effect of the Greenleaf judgment on the monthly probability of loan outcomes. Observations are at the loan-month level. The outcome variable takes a value of one if a loan experiences a particular credit event while holding a delinquency status of 60 days or worse, upon which it drops out of the sample. Each column defines a distinct credit event, namely foreclosure, debt sale (servicing sold), delinquency (90-days delinquent from 60-days delinquent), cure (30-days delinquent or current from 60-days delinquent), short sale (zero balance), and modification, respectively. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Columns 1 to 6 report estimates from difference-in-differences regressions that interact the High Cost and Low Cost dummies with the Post variable. The High Cost dummy takes a value of one if a MERS loan was originated by a lender that initiated bankruptcy proceedings prior to the Greenleaf judgment. The Low Cost dummy takes a value of one for all other MERS loans. The Post dummy takes a value of one if a loan is observed on or after the month of the judgment (July 2014). Loan level controls include the origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, single-family status, and log of the previous period's balance. Annual county level controls at the time of event include the log of the house price index, GDP, population, and income, as well as the year-end unemployment rate. All columns include fixed effects for the quarter of observation interacted with quarter of origination and the zip code, separately. The pairwise t-statistic of the difference between coefficients is reported in the last row. Standard errors are reported in parentheses and are clustered at the zip code level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of April 2013 to June 2016 for loans originated between 2002 and 2007 in Maine.

Black Box, Greenleaf: Lender Behavior Under Uncertain Collateral Enforcement

3.1 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 3.2 provides institutional details on MERS and the Maine Greenleaf judgment. Section 4.3 details my data, sample construction, and summary statistics. Section 3.4 estimates the effect of the Greenleaf judgment on bank lending. Section 4.7 explores alternative explanations. Section 4.6 investigates the mechanism driving the reduced lending. Section 3.7 studies spillovers into alternative bank operations and the local economy. Section 4.9 concludes.

3.2 Institutional Background

3.2.1 Mortgage Electronic Registration Systems

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).

3.2.2 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 3.1 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.

3.3 Data

3.3.1 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 level¹.

3.3.2 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 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.

3.3.3 Registry of Deeds

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

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

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.

3.3.4 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.

3.3.5 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 3.1 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:

$$\omega_b = \left(\frac{\textit{Maine MERS Volume}}{\textit{New England Volume}} \right)_b \tag{3.1}$$

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 3.2 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 3.3 plots standardized estimates from 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 inter-state 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 3.6.4 accounts for this explicitly by re-estimating my results across bank size.

3.4 Main Results

3.4.1 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:

$$Y_{c,b,t} = \alpha + X'_{c,b,t}\gamma + \delta D_b \times Post_t + \epsilon_{c,b,t} \quad (3.2)$$

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 (3.1). 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 Green-

leaf 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:

$$Y_{c,b,t} = \alpha + X'_{c,b,t}\gamma + \sum_{s=-3}^4 \delta_s D_b \times \{t = s\} + \epsilon_{c,b,t} \quad (3.3)$$

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.

3.4.2 Bank Lending

Table 3.2 presents the estimates of regression Equation (3.2) 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.

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 $\% \Delta = 100 \times [\exp(\delta - 0.5 \times \sigma_\delta^2) - 1]$.

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%.

3.4.3 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 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 3.3 tests whether treated bank lenders respond to the increased enforcement risk asymmetrically across lending type. I mirror Table 3.2 and estimate regression Equation (3.2) separately for portfolio loans and non-portfolio loans. Panel A of Table 3.3 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 3.3 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 3.4 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 3.3 using the same set of controls as in Column (6) of Table 3.3. 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 3.4, 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 3.5, non-portfolio lending is statistically insignificant from zero for the years prior to and following the Greenleaf judgment.

3.4.4 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:

$$\omega_c = \frac{\sum_{b=1}^N \omega_b \times volume_{c,b}}{\sum_{b=1}^N volume_{c,b}} \quad (3.4)$$

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 (3.4), 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.8 tests whether treated census tracts experience a reduction in overall lending following an increase in enforcement risk. I estimate regression Equation (3.2) 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.8 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.

3.5 Robustness

3.5.1 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.9, I estimate Equation (3.2) 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.9 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 3.6.2.

3.5.2 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.10 and estimate regression Equation (3.2), 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.

3.5.3 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 3.3 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.11 with

the lender-tract-year panel of non-banks. Table A.11 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.11 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 3.3 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.12, 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 (3.4). 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 3.7.1.

3.6 Mechanism

3.6.1 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:

$$\begin{aligned}
 Y_{c,b,t,i} = & \alpha + X'_{c,b,t,i}\gamma + \delta D_b \times Post_t + \varphi MERS_i \times D_b \times Post_t \\
 & + \beta MERS_i \times Post_t + \epsilon_{c,b,t,i}
 \end{aligned}
 \tag{3.5}$$

Note the above equation is an abbreviated version of the fully interacted specification. In the above specification, I modify Equation (3.2) 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 (3.2). 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 3.4 presents estimates using regression Equation (3.5). 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 3.3. Here I restrict the sample to lending in New Hampshire and using loan level data. The results are similar to those reported

in Table 3.3, 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 3.4 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.

3.6.2 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 mini-

mizing 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.

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:

$$Y_{c,b,t,i} = \alpha + X'_{c,b,t,i}\gamma + \delta D_b \times Post_t + \varphi D_b \times Portfolio_i \times Post_t + \epsilon_{c,b,t,i} \quad (3.6)$$

As with Equation (3.5), 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 3.5 presents estimates of the effect of the Greenleaf judgment on loan size across bank exposure and loan type. I estimate Equation (3.6) 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 3.5 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.

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.13 tests whether treated counties experience a change in credit worthiness of newly issued loans. I estimate regression Equation (3.2) 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 (3.4) 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.13 suggest that exposed banks likely worked to reduce mortgage delinquency rates. This corroborates results in Table A.9 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.13, 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.

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:

$$\begin{aligned}
 Y_{c,b,t,i} = & \alpha + X'_{c,b,t,i}\gamma + \delta D_b \times Post_t + \phi Above Limit_i \times Post_t \\
 & + \beta_1 Above Limit_i + \beta_2 Above Limit_i \times D_b \quad (3.7) \\
 & + \varphi Above Limit_i \times D_b \times Post_t + \epsilon_{c,b,t,i}
 \end{aligned}$$

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.14 presents the estimates of regression Equation (3.7), 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.

3.6.3 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:

$$Y_{c,b,t} = \alpha + X'_{c,b,t}\gamma + \delta D_b \times Post_t + \varphi Near_c \times D_b \times Post_t + \epsilon_{c,b,t} \quad (3.8)$$

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.15 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.15, 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:

$$Y_{c,b,t} = \alpha + X'_{c,b,t}\gamma + \delta D_b \times Post_t + \sum_{s=1}^9 \delta_s D_b \times Post_t \times \{c \in decile_s\} + \epsilon_{c,b,t} \quad (3.9)$$

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 3.6 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 (3.9) using the same set of controls as in Column (6) of Table 3.3. 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 3.6 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 3.6 demonstrates, whereby changes in non-portfolio lending across distance are statistically insignificant.

3.6.4 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 3.6.3, 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.

Bank Size

Table 3.6 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 3.3 but restricted to banks with either below or above the 2013 median asset size. Columns (1) and (2) estimate Equation (3.2) 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 3.6, 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.11, 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 3.6, I report estimates using the sample of large banks. Here, the results largely mirror the estimates using the full sample, reported in Table 3.3. 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.

Business Model

Table 3.7 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 (3.2) 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 3.7, I restrict the sample to Retainers. Here, the estimates mirror the results in Table 3.3. 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 3.3 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.

3.7 Spillover Effects

3.7.1 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 3.6.2, 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 (3.2) 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.16 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 (3.2) using the tract-year panel:

$$Y_{c,t} = \alpha + X'_{c,t}\gamma + \delta D_c \times Post_t + \varphi Branch_c \times D_c \times Post_t + \epsilon_{c,t} \quad (3.10)$$

In the abbreviated specification above, $Branch_c$ equals one if a census tract contains bank branch deposits. Figure 3.7 plots estimates of regression Equation (3.10). 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.16. 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.16 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.

3.7.2 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.17 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 3.8 plots estimates and 95% confidence intervals from a flexible difference-in-differences specification given by Columns (2) and (4) of Table A.17. In Panel A of Figure 3.8, 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 3.8, 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.

3.8 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.

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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NEW HAMPSHIRE-Single Family-Fannie Mae/Freddie Mac UNIFORM INSTRUMENT WITH MERS Form 3030 1/01

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Initials: _____

VMP MORTGAGE FORMS - (800)521-7291

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Panel A: New Hampshire 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

Panel B: Maine Contract

FIGURE 3.1. MERS Mortgage Contract Across States

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.

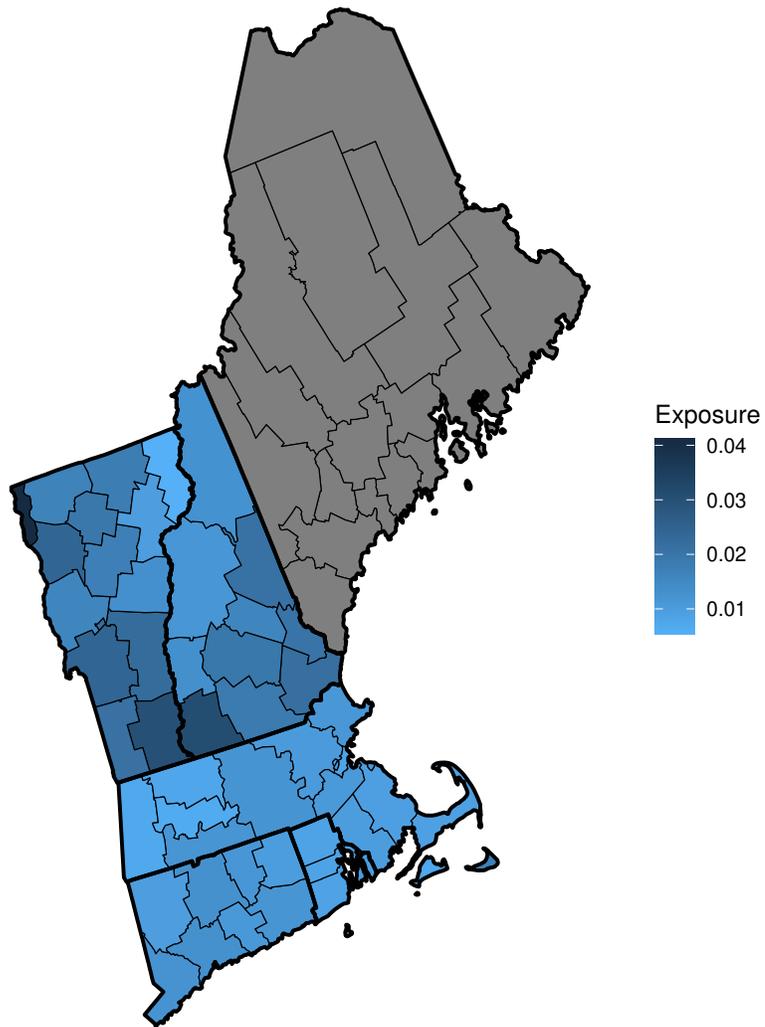


FIGURE 3.2. Exposure to Greenleaf judgment Across New England

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.

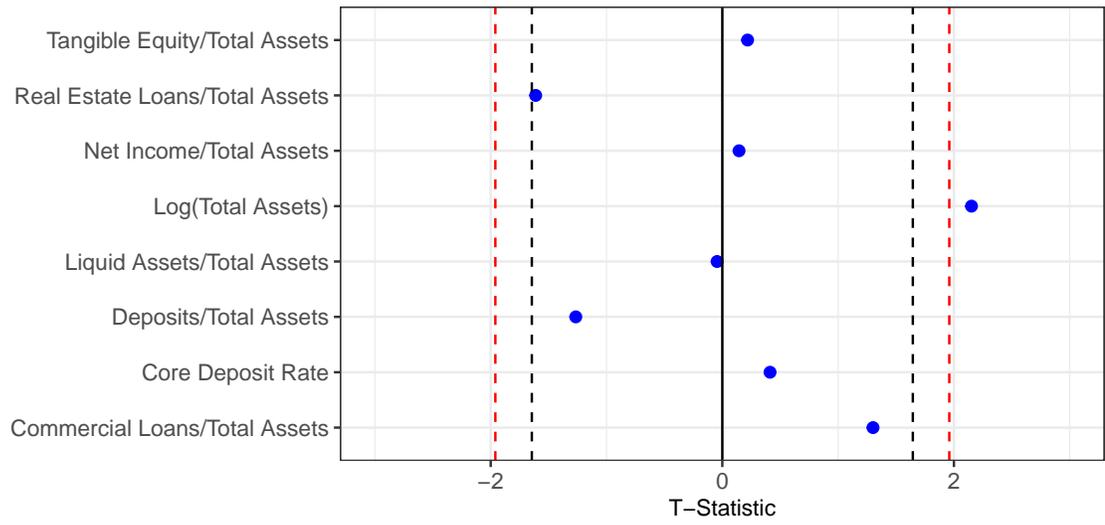


FIGURE 3.3. Balance Test

Note: This figure plots standardized estimates from 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.

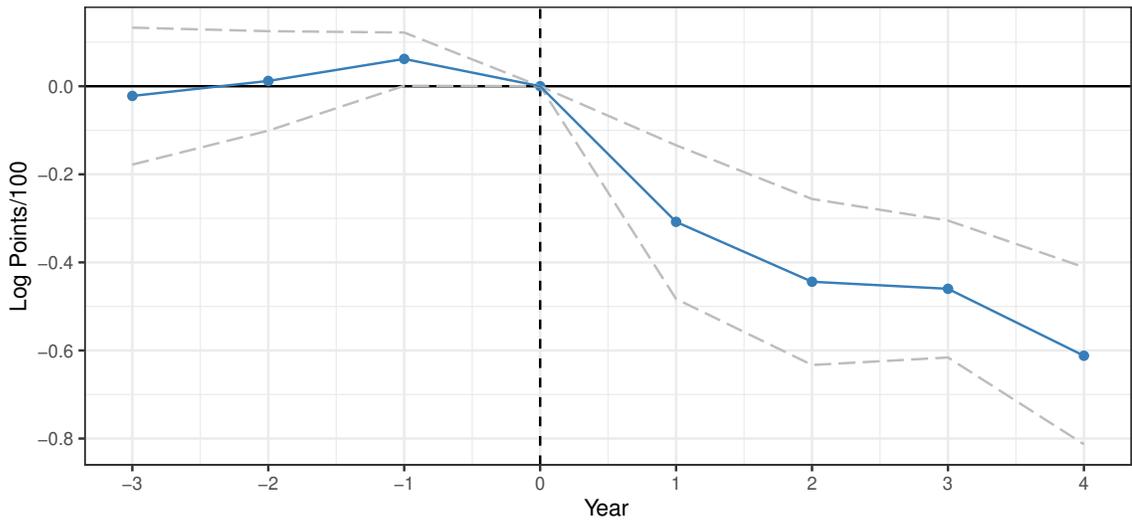


FIGURE 3.4. Difference-in-Differences Estimates: Log(Portfolio Loan Volume)

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.

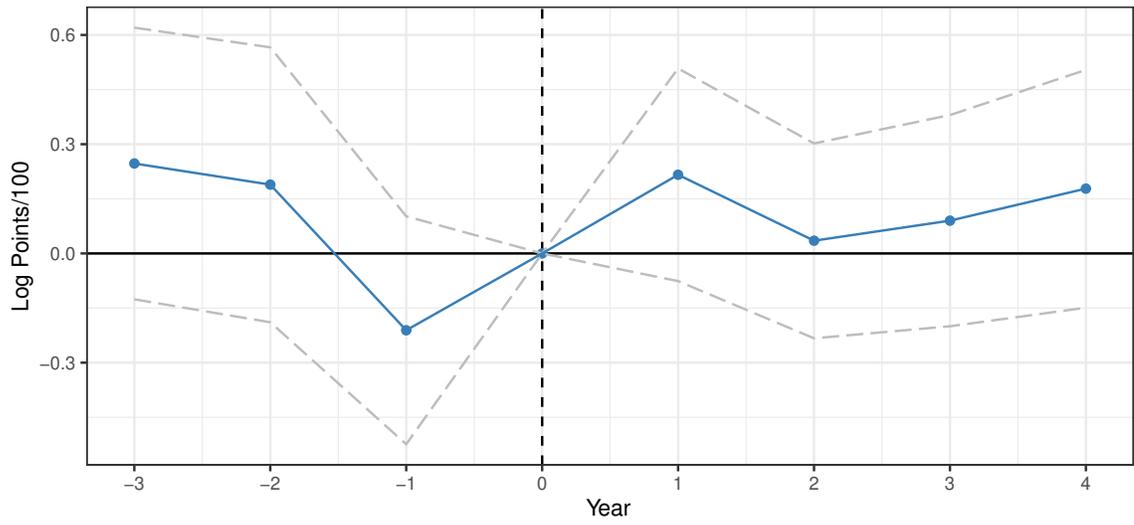


FIGURE 3.5. Difference-in-Differences Estimates: Log(Non-Portfolio Loan Volume)

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.

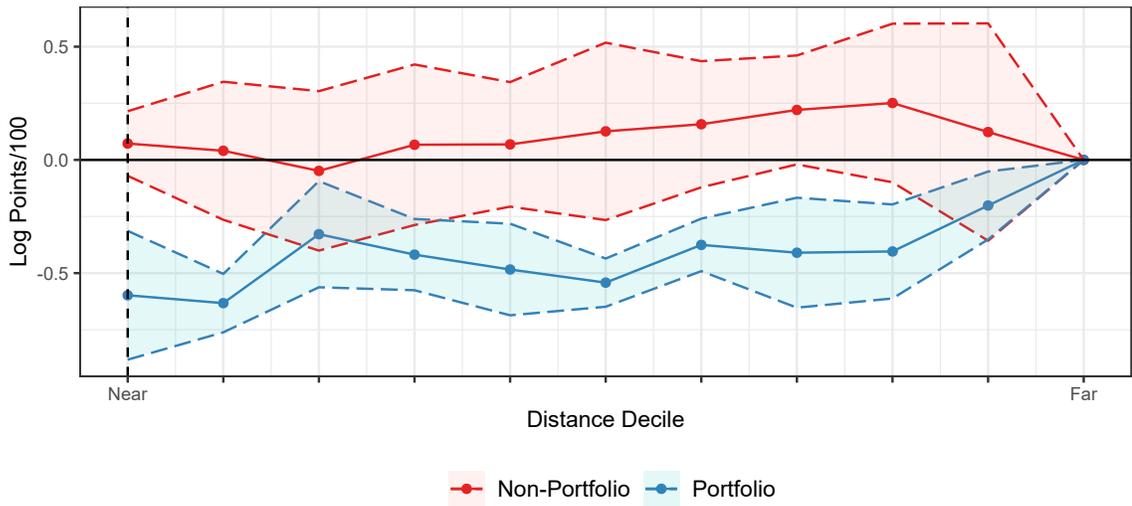


FIGURE 3.6. Difference-in-Differences Estimates by Distance Decile and Loan Type: Log(Loan Volume)

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.

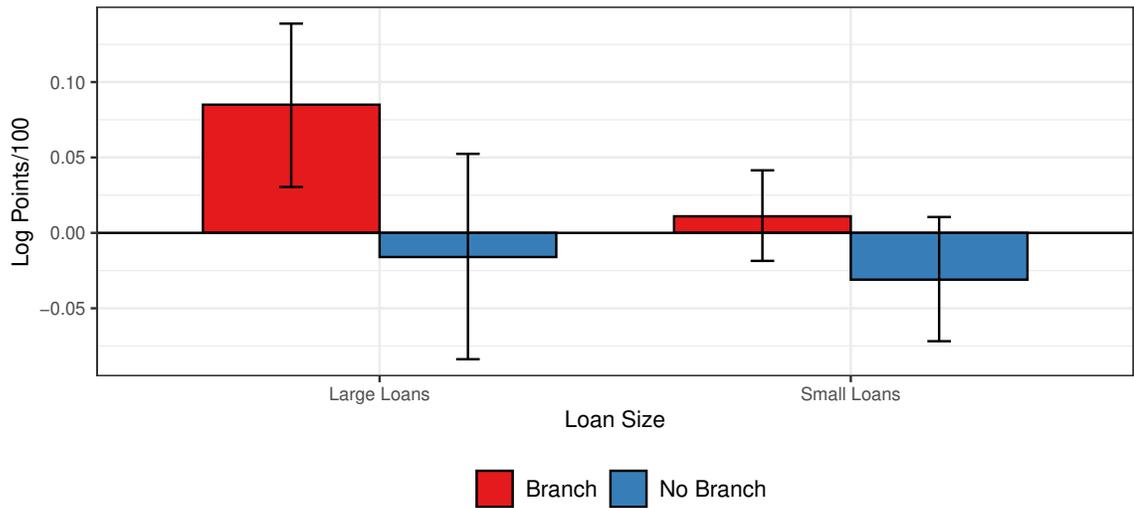
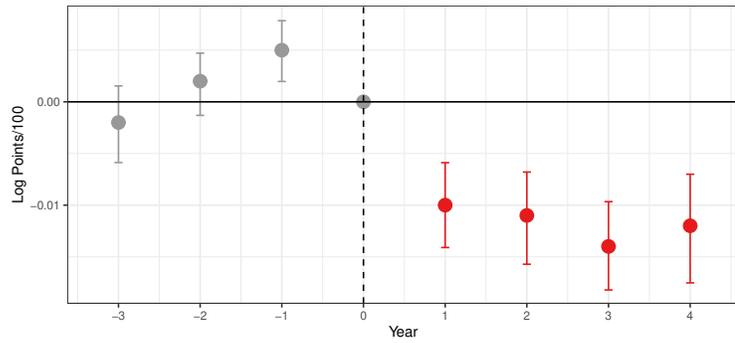
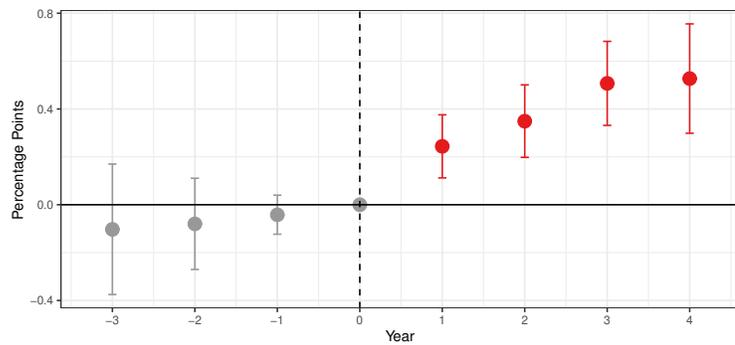


FIGURE 3.7. Difference-in-Differences Estimates Across Loan Size and Branch Presence: Log(Small Business Lending)

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.



Panel A: Log(House Price Index)



Panel B: Unemployment Rate

FIGURE 3.8. Difference-in-Differences Estimates: Real Effects

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 3.1. Summary Statistics

	Exposed		Unexposed	
	Mean	SD	Mean	SD
<i>Panel A: Balance Sheet Characteristics</i>				
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
<i>Panel B: Lending Characteristics</i>				
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 3.2. Effect of the Greenleaf Judgment on Bank Lending: Difference-in-Differences

	log(Loan Volume)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat \times Post	-0.195** (0.098)	-0.239*** (0.091)	-0.226** (0.092)			
Exposure \times Post				-0.102 (0.070)	-0.183** (0.076)	-0.173** (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 \times Tract FE		X	X		X	X
Tract \times Time FE			X			X
Implied $\% \Delta$	-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 3.3. Effect of the Greenleaf Judgment on Bank Lending Across Loan Type: Difference-in-Differences

	log(Loan Volume)					
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A:</i>						
<i>Portfolio Loans</i>						
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 %Δ	-32%	-40%	-41%	-26%	-36%	-37%
Obs	222,689	222,689	222,689	222,689	222,689	222,689
<i>Panel B:</i>						
<i>Non-Portfolio Loans</i>						
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 %Δ	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. 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 3.4. Effect of the Greenleaf Judgment on Bank Loan Size Across MERS Status: Difference-in-Differences

	log(Loan Size)				
	(1)	(2)	(3)	(4)	(5)
Treat \times Post	-0.186*** (0.066)			-0.423*** (0.155)	
Exposure \times Post		-0.103*** (0.035)			-0.119** (0.055)
MERS \times Post			0.000 (0.045)	0.011 (0.024)	0.030 (0.053)
MERS \times Treat \times Post				0.404** (0.183)	
MERS \times Exposure \times 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 \times Tract FE	X	X	X	X	X
Tract \times Time FE	X	X	X	X	X
Implied $\% \Delta$	-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 3.5. Effect of the Greenleaf Judgment on Bank Loan Size: Difference-in-Differences

	log(Loan Size)			
	(1)	(2)	(3)	(4)
Treat \times Post	0.018 (0.057)			
Portfolio \times Treat \times Post	-0.241*** (0.092)		-0.233* (0.128)	
Exposure \times Post		0.042 (0.072)		
Portfolio \times Exposure \times Post		-0.219*** (0.055)		-0.318*** (0.101)
Tract FE	X	X	X	X
Time FE	X	X	X	X
Lender FE	X	X	X	X
Lender \times Tract FE	X	X	X	X
Tract \times Time FE	X	X	X	X
Lender \times Time FE			X	X
Implied $\% \Delta$	-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 3.6. Effect of the Greenleaf Judgment on Bank Lending Across Size: Difference-in-Differences

	All Loans		Portfolio		Non-Portfolio	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A:</i>						
<i>Small Banks</i>						
Treat × Post	0.221*** (0.082)		0.086 (0.097)		0.240*** (0.083)	
Exposure × Post		0.175 (0.122)		0.515 (1.596)		0.352*** (0.108)
Implied %Δ	24%	18%	8%	—	27%	41%
Obs	91,734	91,734	43,615	43,615	60,931	60,931
<i>Panel B:</i>						
<i>Big Banks</i>						
Treat × Post	-0.211** (0.097)		-0.390* (0.213)		0.057 (0.151)	
Exposure × Post		-0.158* (0.083)		-0.461*** (0.081)		0.142 (0.125)
Implied %Δ	-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. 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 3.7. 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)
<i>Panel A:</i>						
<i>Retainers</i>						
Treat × Post	-0.519*** (0.135)		-0.603*** (0.175)		-0.117 (0.106)	
Exposure × Post		-0.424*** (0.031)		-0.508*** (0.039)		0.098 (0.159)
Implied %Δ	-41%	-35%	-46%	-40%	-12%	9%
Obs	124,912	124,912	102,001	102,001	42,650	42,650
<i>Panel B:</i>						
<i>Sellers</i>						
Treat × Post	-0.165 (0.137)		-0.147 (0.179)		0.046 (0.170)	
Exposure × Post		-0.064 (0.135)		-0.155 (0.136)		0.109 (0.131)
Implied %Δ	-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. 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.

Yellow Light Foreclosures: Collateral Enforcement and the Sale of Delinquent

4.1 Introduction

The enforceability of creditor rights is fundamental to determining the credit supply (Pence (2006), Rampini and Viswanathan (2010)). While lending decisions are made ex-ante, a creditor's security interest is enforced ex-post following a debtor's default. At that point, creditors face a choice of either renegotiating the terms of their debt contract or liquidating the associated collateral. However, recent work by Ahsin (2021) demonstrates that creditors may choose a third option—selling their debt when other options become costly. This paper aims to extend this novel insight by studying how the sale of debt following borrower delinquency affects loan outcomes.

Empirically studying the effect of loan sales on foreclosure and renegotiation is challenging due to asymmetric information. A rich body of work explores how the selling of newly originated loans is riddled by a lemons problem (Adelino et al. (2019), Begley and Purnanandam (2016) To signal loan quality, buyers demand costly signals, such as a seller's equity stake or longer hold times. Naturally, informational asym-

metrics are amplified when a borrower defaults. For the econometrician, therefore, a primary challenge is disentangling the effect delinquent loan sale from that of loan quality. If creditors only sell low quality loans, then the relationship between loan sale and loan performance will be biased down. Alternatively, if creditors sell the best performing loans due to reputational concerns, then the opposite will be true.

To overcome this challenge, I exploit a policy change in the Ginnie Mae mortgage market related precisely to delinquent loan purchase. When a Ginnie Mae bond issuer securitizes a pool of loans, the issuer retains the right to buy out any individual mortgage from Ginnie Mae bond investors under one of the following scenarios: (i) when a loan holds the status of at least one missed payment for a consecutive four-month period (rolling delinquency) or (ii) when a loan enters a status of three missed payments (serious delinquency). In November 2002, however, Ginnie Mae abruptly released a memorandum that eliminated the buy out option under scenario (i) for all loans securitized as of January 1, 2003.

Exploiting this policy change, I compare outcomes for loans originated closer to the memorandum and those originated earlier in the year. Identification requires that the vintage month not directly correlate with loan performance except through buyout. Using month of origination satisfies this conditions in several ways. First, based on conversations with a former Ginnie Mae executive, the memorandum was intentionally announced in a manner that would prevent market anticipation. Hence, loans originated shortly before the announcement were similar to those originated earlier in the year but for the probability of buyout. Second, the former executive confirmed that the policy change was not precipitated by any fraudulent behavior or material loan quality. By definition, the buyout suspension was assigned independent of loan outcomes. To further corroborate this, I provide graphical evidence that ex-ante loan characteristics and ex-post delinquency does not experience any stark change across vintage months. Third, I restrict identification to within-delinquency

month and within-MSA variation. Theoretically, outcomes should not differ significantly for loans that experience equivalent macroeconomic conditions at the time of a rolling delinquency and differ in vintage by only a short span of time. Finally, I rely upon placebo tests and pre-trends to demonstrate that vintage month does not correlate with loan outcomes except when loans are treated with exposure to the policy shock.

In what effectively serves as the first stage of my analysis, I find that buyout probability precipitously falls for loans originated in the months immediately preceding the policy shock relative to loans originated months earlier. Exploiting within-MSA and within-delinquency time variation, I estimate the differential effect of vintage month on buyout rates, conditional on observables. Given a baseline buyout rate of 52%, I find that the buyout rate for loans issued in the two-month period immediately preceding the policy announcement experienced a 18.6 percentage point reduction. The estimate for the last vintage month alone represents close to a 60% reduction relative to the baseline. In sharp contrast, the buyout rate for 90-day delinquent loans experienced no break in trend for months before and after the policy announcement, as shown in Figure 4.1. Based on estimates of buyout activity earlier in the calendar year, my first-stage estimates strongly suggest that the buyout rate would have evolved in parallel with seriously delinquent loans, if not for the Ginnie Mae policy change.

Given the stark drop in buyout activity preceding the policy, I next estimate the differential effect of vintage month on foreclosure 18 months following a rolling delinquency. Exploiting variation within-delinquency time, I find that Ginnie Mae loans experience a monotonic increase in foreclosures across vintage month, peaking to a 5.7 percentage point increase relative to the earliest month in my sample. Widening the control group to include the first four months of my sample, I find that the last two months experience a differentially 2.6 percentage point higher foreclosure rate.

Taken together, the drop in the buyout rate induced by the policy change coincided with an increase in the foreclosure rate in the months preceding the announcement.

Exploring the mechanism, I find that within three months of a rolling delinquency, loans exit delinquency differentially less in the months immediately prior to the policy announcement. I measure cures using a three month window immediately following a rolling delinquency in order to differentiate these early cures from those that follow a buyout due to serious delinquency. I find that point estimates of the differential effect decline monotonically to -7.4 percentage points. Under the null that the last two-month period should experience no significant deviation from the first four vintage months in my sample, I find a statistically significant differential decline of 4.6 percentage points.

Next, I confirm that the reduction in early cures is related to the suspension of buyouts. To this end, I estimate the differential effect of vintage month on late cures using a longer 12-month window following rolling delinquency. If issuers still retained the buyout option for loans experiencing a serious delinquency, then I should see the differential effect from my previous exercise attenuate using a longer horizon, capturing the timing of two more missed payments. Along these lines, I find that most vintage months exhibit no statistically significant differential effect. The two-month estimate is 40% smaller than the estimate for early cures. Essentially, the effect of the buyout suspensions appears to affect foreclosure through early cures, not cures in general.

Given that the reduction in early cures, as opposed to late cures, appears to drive the increase in foreclosures, I investigate how early intervention prevents loan quality from deteriorating. I find that the reduction in early cures is uncorrelated with changes to a borrower's interest rate, principal balance, and required payment. Instead, point estimates for the differential effect of vintage month on modifications and payment changes are close to zero and generally statistically insignificant. This

implies that the reduction in early cures must be related to a reduction in either term extensions or payment deferrals, two alternatives not measured in the data. Of course, there remains the possibility that modifications occur in a manner undetected by my sample.

My analysis rests upon the key identifying assumption that an omitted variable, such as loan quality, does not correlate with both vintage month and loan performance. This may be violated if, for example, less credit-worthy borrowers happen to obtain Ginnie Mae loans right before the policy announcement. Naturally, this would bias my estimates, spuriously indicating that loans experience higher foreclosures precisely in vintage months preceding the 2002 memorandum. While evidence using ex-ante loan characteristics, ex-post loan quality, and the nature of the shock suggest that this is unlikely, I provide several additional robustness checks to further validate my research design.

First, I re-estimate my main specifications across several placebo samples. In an initial test, I construct a sample of loans originated prior to a 2003 placebo shock, one year after the true policy announcement. Since the buyout suspension was in full effect by 2003, loans issued around the 2003 placebo shock should experience no variation in rolling delinquency buyout. In a second set of placebo tests, I construct a sample of loans for which there exists no buyout incentive. These loans have a value below par at the time of delinquency, whereby the initial interest rate is below the average market rate at the time of delinquency. Across both samples, I test whether vintage month differentially affects loan outcomes when either buyout is entirely suspended or the incentive to buyout does not exist. Indeed, I find no systematic pattern in variation across origination time, validating my key identifying assumption that vintage month does not correlate with long-term loan outcomes, except through treatment. The point estimates for the final two-month period in both samples are statistically and economically indistinguishable from zero across all placebo tests.

In order to further validate my research design, I exploit a wider sample of vintage months to test for the presence of pre-trends. I expand my sample to the entire calendar year of origination and provide graphical evidence on differential effects in loan outcomes prior to the start of my primary sample. If loan outcomes differed across origination time in later vintage months, then outcomes should also vary earlier in the calendar year. I find that there exists no meaningful pre-trends across both 2002 and 2003 issued Ginnie Mae loans. Instead I find a monotonic pattern in loan outcomes only in the last months of the 2002 vintage sample, consisting of precisely the loans exposed to treatment. The limited variation in early vintage months combined with a systematic pattern in later 2002 vintages makes the possibility of another confounding channel less likely.

Finally, I estimate my main specification using an analogous sample of GSE loans issued in 2002. If my main results were due to unobserved correlation between loan performance and vintage month unrelated to the Ginnie Mae announcement, then I should identify this differential effect using a sample of non-Ginnie Mae loans, as well. Instead, using the GSE sample, I find point estimates statistically insignificant and close to zero for the two-month period preceding the policy shock across all tests. In my most demanding specification, I use a pooled sample of 2002 issued Ginnie Mae and GSE loans, estimating a specification that interacts vintage month with Ginnie Mae status. I identify the treatment effect of the Ginnie Mae policy across vintage month using vintage month fixed effects. This specification takes advantage of the pooled nature of the sample and accounts for common shocks to vintage month directly. Here, I find symmetric point estimates, whereby the differential increase in foreclosure mirrors the differential decline in early cures. In the two-month period preceding the policy announcement, Ginnie Mae loans experience a 2.7 percentage point increase in foreclosures and 2.8 percentage point decline in early cures. The equivalency across point estimates strongly suggests that buyouts reduce foreclosures

due to early cures.

I conclude by employing an instrumental variables (IV) approach that exploits the quasi-experimental variation in buyout propensity preceding the Ginnie Mae shock. Using an IV approach is useful as it provides (i) policy-relevant causal estimates of the elasticity of buyout and loan performance and (ii) a framework to interpret the external validity of my results. I estimate two-stage least squares (2SLS) regressions using vintage month as the set of excluded instruments. Under the assumption that vintage month affects loan outcomes only through quasi-random variation in buyout propensity, I estimate that buyout reduces the probability of foreclosure by at least 11.6 percentage points and increases early cures by at least 13 percentage points. Given that my research design exploits the suspension of buyout, this one-sided non-compliance facilitates an interpretation of average treatment effect on the treated. In words, my estimates represent the degree to which buyout reduces foreclosure for those loans that ultimately experience buyout. Finally, exploring the heterogeneity, I find that financially constrained borrowers and borrowers with unencumbered collateral benefit most from buyout. Ultimately, creditors appear to respond to financial incentives, whereby loans providing the largest interest rate spread reperform most effectively after initial delinquency.

Literature Review

This paper bridges two broad research areas in corporate finance. First, I contribute to the literature on creditor behavior under debtor delinquency. Work by Piskorski et al. (2010), Agarwal et al. (2011), and Kruger (2018) find that securitization leads to fewer modifications and more foreclosures, whereas Adelino et al. (2013) argue that, prior the financial crisis, there was little difference in renegotiation. The paper proposes self-cure risk and moral hazard as alternative explanations for the lack of renegotiation. Aiello (2022) demonstrates that financial constraints on the part of ser-

vicers lead to foreclosures and modifications that reduce value to investors and replace alternative borrower actions. Finally, Ahsin (2021), Gabriel et al. (2020), and Collins and Urban (2018) study the effect of foreclosure costs on foreclosures, renegotiations, and borrower repayment. My paper complements preceding work by demonstrating the efficacy of buyout as an alternative to foreclosure and renegotiation.

This paper also contributes to the literature on loan sales. Previous work has studied the role of asymmetric information in selling debt. An et al. (2011) find that Conduit CMBS sales feature a lower lemons discount than portfolio sales even after controlling for various economic conditions and loan characteristics. The authors argue that this is likely due to the informational advantage enjoyed by portfolio lenders, which subjects sales to a problem of adverse selection. Jiang et al. (2012) further find that balance sheet loans perform worse than loans that are ultimately sold, likely due to the time buyers have to observe ex-post loan performance.

Adelino et al. (2019) operationalize this insight and find that loans sold earlier perform worse than loans sold later in the private label securities market. The authors show that signaling is most important for those loans with limited hard information to identify quality. Begley and Purnanandam (2016) further demonstrate that RMBS deals with higher levels of equity perform better ex-post, precisely for those loans that are most opaque. In complimentary work, Ashcraft et al. (2019) find that retention by B-piece buyers of CMBS is associated with high default probabilities of senior tranches. The authors demonstrate that this result is due to moral hazard as opposed to adverse selection on the part of selling underperforming securities. In addition to adverse selection and moral hazard, Drucker and Puri (2008) and Hartman-Glaser (2017) study reputation effects in the market for loan sales. Finally, beyond loan quality, work by Irani and Meisenzahl (2017) find that banks with liquidity shortfalls increase loan sales. While previous work has focused on sale at the time origination, my work instead focuses on loan sale around delinquency.

Outside of loan sales and creditor behavior, this paper relates to the literature on Ginnie Mae bonds and FHA loan issuance. The Ginnie Mae market has evolved significantly over the past two decades. Adelino et al. (2020) note that borrowers served by the Ginnie Mae market switched into the private securitization market prior to the financial crisis. Since the crisis, Gete and Reher (2020) find that the favorable regulatory status held by Ginnie Mae bonds led to an increase in non-bank market share in the post-crisis period. Similarly, Bhutta and Ringo (2021) find that the credit supply expanded in 2015 following reductions in insurance premiums for FHA loans. In recent years, Kim et al. (2018) document significant liquidity concerns related to issuers and servicers within the broader Ginnie Mae market. They document growth in issuance, concerns over financial stability, and significant costs borne by Ginnie Mae issuers. As of March 2022, Ginnie Mae made up about 25% of the \$8.5 trillion in agency MBS outstanding. This is over two times the share of Ginnie Mae issuance outstanding as of June 2007.

This paper contributes to previous work by studying the buyout option, a particular feature of the Ginnie Mae mortgage market. Importantly, this option exists for all agency debt when a loan experiences serious delinquency. While \$8 trillion worth of outstanding agency debt retain this buyout option, there remains a dearth of research on its consequences. Relevant to the current paper, Bandyopadhyay et al. (2021) find that creditors buy back loans in Ginnie Mae securitization pools when the loan has a relatively higher interest rate spread over the ten-year treasury rate. The paper provides suggestive evidence that issuers remove loans in order to re-securitize them under declining interest rate environments. The authors posit that buyout creates a friction between servicers and borrowers due to a loan's change of ownership from origination to securitization to buyout and then to re-securitization again. The current paper aims to extend this research by asking how buyouts affects loan performance directly.

The remainder of this paper proceeds as follows. Section 4.2 describes the institutional background of the Ginnie Mae loan market and the delinquency buyout option. Section 4.3 details the data, sample construction, and summary statistics. Section 4.4 estimates the baseline relationship between buyout and loan performance. Section 4.5 estimates the effect of the Ginnie Mae policy change on buyout and foreclosure. Section 4.6 explores the mechanism underlying the increase in foreclosures. Section 4.7 tests the robustness of my main results. Section 4.8 implements an IV approach that estimates the causal relationship between buyout and loan performance. Section 4.9 concludes.

4.2 Institutional Background

4.2.1 The Government National Mortgage Association

Established in 1968 by the Department of Housing and Urban Development (HUD), the Government National Mortgage Association (GNMA or Ginnie Mae) facilitates a secondary market for securitization of loans issued to high LTV borrowers. Ginnie Mae serves as a platform for Ginnie Mae-certified issuers to securitize loans insured by the Federal Housing Authority (FHA), Department of Veteran Affairs (VA), the Rural Housing Service (RHS), or the Office of Public and Indian Housing (PIH). Primarily, issuers are responsible for forwarding payments to investors and Ginnie Mae guarantees the full and timely payment of principal and interest in case of issuer default. In case of borrower default, one of the four insuring agencies will compensate the issuer. The Ginnie Mae market is unique in that Ginnie Mae does not purchase loans from the issuers directly. Instead, it offers insurance and a platform while issuers continue to act as custodians over timely payment to investors. In contrast, the Government-Sponsored Enterprises (GSE's) purchase the loans outright from lenders.

4.2.2 The Delinquency Buyout Option

Issuers of Ginnie Mae and GSE MBS retain a right to buy out loans from the securitized mortgage pool when a loan experiences a 90-day delinquency, also known as serious delinquency. This means that the issuer may return the remaining principal balance to investors at par in order to purchase the loan. Furthermore, within the Ginnie Mae market, an issuer may re-securitize a buyout loan when it begins to reperform.

In addition to the 90-day delinquency condition, prior to 2003, Ginnie Mae allowed loan buyouts for a rolling delinquency. This type of delinquency occurred when a borrower remained non-current for four periods. As an extreme example of this, a borrower might miss a payment in period one while servicing payments in periods two, three, and four. As long as the borrower failed to make investors whole by the fourth period, the issuer had the right to buy the loan out of the pool prior to the traditional 90-day delinquency measure.

Ginnie Mae intended that the buyout option on delinquent mortgages target severe cases of deterioration in loan quality. Providing this option benefits investors through timely repayment, helps issuers maintain pool quality, and uplifts the broader market by lowering Ginnie Mae's exposure to bad loans. However, allowing issuers to retain a buyout option naturally lowers MBS prices. In general, prepayment lowers the price of a bond since investors demand lower prices to compensate for faster pay-off speeds. Buyouts function in the same manner and therefore exacerbate this problem. While this discount is worthwhile for 90-day delinquent mortgages, rolling delinquencies do not necessarily fulfill the spirit of the policy. Rolling delinquency borrowers by definition pay every period except for a single missed payment at least four months earlier. Therefore, these loans contribute far less risk to the broader market than seriously delinquent loans both due to the level of delinquency and the probability of further

deterioration. Hence, the systematic benefit of buying out rolling delinquencies is unclear.

With this context in mind, investors petitioned Ginnie Mae to remove the buyout option in order to raise MBS prices. Ginnie Mae found the argument to suspend the buyout option compelling, especially given that lower bond prices would hurt its mission to facilitate housing finance. On November 6, 2002, Ginnie Mae's Executive Vice President, George Anderson, released All Participants Memorandum 02-24, which restricted buyout to exclusively loans that were at least 90 days delinquent. This applied to all new Ginnie Mae bond issuance as of January 1, 2003.

Usually, Ginnie Mae collaborates with a variety of stakeholders over the key features of a policy change. This may include the Mortgage Bankers Association (MBA), the broader issuer community, and MBS investors. A policy change of major importance would be anticipated by at least 6 months in order to provide all participants ample time to adjust to a new setting and prevent operational failure. In stark contrast, one Ginnie Mae executive close to the matter described the 2002 memorandum as an announcement to "stop immediately". In private conversation, the executive stated that, at most, Ginnie Mae inquired about the value of the buyout option with the MBA, with no revelation of any intention to suspend the policy. Ultimately, the policy announcement was intended to prevent anticipation on the part of the very issuers exploiting the letter of the law in the first place.

The Ginnie Mae executive confirmed that the policy change was not precipitated by fraudulent exercise of the option. Ginnie Mae would regularly conduct field reviews to ensure that issuers were fulfilling all responsibilities associated with general Ginnie Mae policy. Of course, these reviews would include auditing for compliance with the buyout policy. An article in HousingWire corroborates this, confirming that early Ginnie Mae audits found no abuse of the rolling delinquency buyout option¹.

¹ Linda Lowell, "Ginnie Buyouts Rattle Investor Nerves," HousingWire, October 28, 2009.

Ultimately, while issuers failed to fulfill the spirit of the original policy, the Ginnie Mae announcement was not precipitated by fraudulent behavior.

Importantly, the Ginnie Mae executive also confirmed that the policy change was unrelated to loan performance, such as delinquencies or foreclosures. In fact, Ginnie Mae at the time was not monitoring delinquencies and payoffs in a manner similar to today. Since issuers would continue to advance payments to investors after a mortgage default, Ginnie Mae simply ensured that issuers were current on the pass through to investors. Ultimately, the policy change was implemented independent of loan outcomes.

For mortgages originated early in 2002, the policy change had no material effect. These loans likely securitized well before the January 1, 2003 deadline. Therefore, issuers of these loans still retained an option to buy out the mortgage following a rolling delinquency. For loans experiencing a 90-day delinquency, there was no change to the option to buy out the mortgage, both preceding and following the policy announcement. This was due to the fact that the policy applied to rolling delinquencies, in particular. Ultimately, the loans most affected by this policy were precisely those mortgages originated closer to the timing of the memorandum. For these loans, there was an increasingly small chance of securitizing before the January deadline.

The intuition underlying this change is best captured by Figure 4.1. Here, I plot the buyout rate for loans across vintage month and delinquency type, both rolling delinquency and serious delinquency. Up until July 2002, buyout rates appear indistinguishable across either delinquency type for each month of origination. However, starting August 2002, the buyout rate on rolling delinquencies experiences a stark break, precipitously dropping towards zero as origination month approaches the timing of the policy announcement. In contrast, buyout rates on serious delinquencies experience no change across vintage month. The break in trend presented in Figure

4.1 is a direct consequence of the suspension of buyouts, whereby loans originated later are unable to securitize in time to avoid the suspension.

4.3 Data

4.3.1 CoreLogic Loan-Level Market Analytics Data

I use the CoreLogic Loan-Level Market Analytics (LLMA) data to study loan performance over my sample period. The data is obtained through the 25 largest mortgage servicers in the US and represents about 45% of all mortgages originated in the US over the sample period of 2002. The dataset includes origination characteristics such as FICO score, origination balance, initial interest rate, original LTV, original term, origination month, origination year, and zip code. The dataset also includes monthly performance data such as unpaid mortgage balance and delinquency status.

4.3.2 Dataset Construction and Summary Statistics

In order to construct my primary sample, I identify all loans that were originated between June 2002 and November 2002. I retain loans that identify Ginnie Mae as an investor within 6 months of origination. While the data does not provide a measure of securitization date, identifying the investor in this manner allows me to associate a loan with the appropriate securitizing agency. In order to eliminate outliers, I retain fixed-rate loans with 30-, 20-, and 15-year terms, loan-to-value ratios below 1.5, and an associated Metropolitan Statistical Area (MSA) located in the contiguous United States. Finally, since Corelogic LLMA data has incomplete information on credit score, I retain loans with an inferred collateral type, which measures a loan's prime or subprime status. Along these lines, I also drop loans that are censored due to sale of servicing rights.

My sample is further restricted by two conditions. I first restrict my sample to loans that enter a rolling delinquency by July 2007. I use this cutoff date since it

was the last month prior to the freeze on asset backed commercial paper. Doing so, I abstract from concerns related to the broader financial crisis influencing the onset of delinquencies. I define a rolling delinquency as four consecutive months of an uncured 30-day delinquency. This identifies the moment that a loan enters the fourth consecutive month that the borrower failed to make up one single missed payment. I condition on rolling delinquency because this represents the relevant population affected by the memorandum.

I next restrict my sample to loans valued at above par upon experiencing a rolling delinquency. A loan is valued above par when its initial interest rate is above the average market rate at the time of delinquency. Hence, issuers receive a profit from buying out and resecuritizing the mortgage, thereby paying new investors less for the same loan because of the lower market rate. This sampling restriction is necessary to study loan outcomes when buyout is a worthwhile option. In some specifications, I exploit this incentive compatibility constraint by conducting placebo tests using the sample of loans that experience a rolling delinquency when the buyout incentive is not present, namely when the loan is valued below par.

In order to identify buyout status, I measure a change in investor status from Ginnie Mae to non-Ginnie Mae within one month of a qualifying credit event. A qualifying event will be either a rolling delinquency or a 90-day delinquency. Note that, this measure does not identify buyout activity for GSE securitized loans because these loans do not change their status following delinquency. Instead, the GSE's repurchase the loans directly and retain them on balance sheet. In contrast, Ginnie Mae does not purchase loans nor issue securities directly, but rather functions as a platform for other issuers. Issuers will purchase a loan from the Ginnie Mae security pool directly upon a qualifying credit event, hence removing the loan's Ginnie Mae status in the data.

As an aside, from a methodological standpoint, my measure of buyout is novel

to the literature. To the best of my knowledge, this is the first use of a proprietary dataset to identify buyout status among Ginnie Mae loans. It is important to reiterate that every agency loan retains a buyout option for its issuer upon serious delinquency. Given that Ginnie Mae loans represent 25% of a \$8.5 trillion market, my measure should prove useful for future research on the Ginnie Mae market, the broader RMBS market, and the economics of distressed debt sale.

Panel A of Table 4.1 reports summary statistics for origination characteristics of loans in my primary sample. My primary sample consists of loans issued between June and November of 2002, conditioning on a rolling delinquency and a value above par. There are several noticeable differences across buyout status. First, buyout loans have an interest rate 14 basis points higher than non-buyout loans. This is consistent with issuer incentives to buy loans with a higher interest rate in order to profit from resecuritizing a loan when valued above par (Bandyopadhyay et al. (2021)). Second, buyout loans hold better observable characteristics along some dimensions, such as loan size, prime status, and documentation status. This is unsurprising given the weight that investors place on observable loan quality when purchasing Ginnie Mae bonds. Naturally, an issuer will choose to buyout those loans that will facilitate bond sale following resecuritization.

Panel B of Table 4.1 displays estimates for medium- and long-term loan outcomes across buyout status. Here I use the sample of loans issued in the pre-period, namely between January and May of 2002. Doing so, I measure baseline loan outcomes unrelated to the Ginnie Mae policy. I find that loans across buyout status experience near identical foreclosure rates 18 months following a rolling delinquency. Furthermore, cure rates are equivalent both within a 3-month window around a rolling delinquency, as well as a 12-month window. Finally, modification and payment changes are relatively rare following a rolling delinquency across buyout status.

4.4 Baseline Results

In my baseline model, I compare differences in loan outcomes across mortgages that are bought out of Ginnie Mae pools and those that remain. Conditional on rolling delinquency and a value above par, I estimate ordinary least squares (OLS) regressions of the following form:

$$Foreclosure_i = X_i'\gamma + \beta Buyout_i + \alpha_t + \alpha_m + \varepsilon_i \quad (4.1)$$

$Foreclosure_i$ measures whether a loan experiences a foreclosure start within 18 months following a rolling delinquency. $Buyout_i$ is an indicator for a mortgage being bought out of the Ginnie Mae pool. X_i is a vector of loan, borrower, and regional characteristics. Following Kruger (2018), I control for loan terms such as the origination interest rate, LTV, an indicator for inferred prime loan status, log of the original mortgage balance, and an indicator for term length. I also account for underwriting quality by including indicators for low-income documentation or no income documentation. In addition, I control for loan purpose by including indicators for refinancing, primary residence, and single-family homes. Finally, I include measures of local economic activity at the time of delinquency, such as the monthly county-level unemployment rate, the log of the monthly county-level labor force, and the log of the annual county-level house price index, GDP, population, and income. In various specifications, I include fixed effects for month of delinquency α_t and the borrower's MSA α_m . Standard errors are clustered at the MSA level to account for within-MSA residual correlation.

Table 4.2 reports estimates of Equation (4.1) using the sample of Ginnie Mae loans issued in 2002 that experienced a rolling delinquency while valued above par. For this exercise, I derive my estimates using loans issued between January and May of 2002, prior to the start of my primary sample. Doing so, I abstract from any effect of the Ginnie Mae policy and measure a stable benchmark relationship between buyout and foreclosure. Column (1) shows results from a univariate regression of foreclosure

on buyout. The estimated effect is economically and statistically indistinguishable from zero. In Column (2), after including control variables, the estimate increases slightly and remains statistically insignificant. In Columns (3) and (4), separately including MSA and month of delinquency fixed effects, respectively, changes the size and significance little. Even after the inclusion of both fixed effects simultaneously, the point estimate remains below one percentage point and statistically insignificant. In summary, the baseline results presented in Table 4.2 suggest that there exists no meaningful relationship between buying a loan out and foreclosure initiation.

4.5 Main Results

The analysis in the previous section naturally suffers from estimation bias due to asymmetric information in loan sales. For example, an issuer with private information may choose to buyout a low-quality loan in order to prevent high delinquency in its Ginnie Mae portfolio. Alternatively, an issuer may buyout precisely those loans most likely to re-perform after a rolling delinquency. This would provide an issuer with the opportunity to profit from resecuritizing the mortgage after it becomes current. Hence, the relationship between foreclosure and buyout may be biased up or down depending on what incentives dominate.

In order to differentiate the effect of loan sale from that of loan quality, an ideal experiment would treat one of two identical delinquent loans with a sale and, thereafter, measure loan performance. Such an experiment would account for both loan quality as well as local economic conditions faced by the borrowers. In order to approximate this, I exploit the timing of the 2002 Ginnie Mae memorandum that eliminated the buyout option on rolling delinquencies. My strategy relies upon the fact that loans originated closer in time to the memorandum were less likely to experience a buyout relative to those originated earlier in the year. This is a mechanical consequence of the memorandum, which declared that any loan securitized after January 1, 2003

would not retain a buyout option for rolling delinquencies. Given the lag between origination and securitization, loans issued closer in time to the memorandum were less likely to be securitized prior to January in order to retain the buyout option.

In theory, the month of origination should correlate with buyout propensity but should not vary with loan quality within a short span of time. The policy was intentionally delivered so as to be unanticipated by market participants. If the memorandum was unanticipated and loan quality did not differ over a short horizon, then origination month (relative to the timing of the policy) should have no effect on future loan performance except through quasi-experimental variation in buyout propensity. I discuss the validity of this assumption in Section 4.5.1 below.

Conditional on rolling delinquency and a value above par, I estimate OLS regressions of the following form:

$$Y_i = X_i' \gamma + \sum \pi_\tau Z_\tau + \alpha_t + \alpha_m + \xi_i \quad (4.2)$$

In the above specification, Y_i is an indicator variable measuring loan performance. Z_τ is equal to one if a loan is originated τ months prior to policy announcement. As before, X_i is a vector of loan, borrower, and regional characteristics. Furthermore, I include fixed effects for month of delinquency and the borrower's MSA. Finally, standard errors are clustered at the MSA level to account for within-MSA residual correlation.

4.5.1 Identification

The coefficient of interest, π_τ , measures the differential change in loan outcomes over month of origination due to the policy change. I identify π_τ by exploiting the timing of the Ginnie Mae memorandum, holding fixed variation within a MSA and within a month of delinquency. The key identifying assumption requires that the month of origination is as good as randomly assigned. Under this assumption, loan

outcomes should have evolved smoothly across vintage month, if not for the Ginnie Mae policy announcement. Z_τ satisfies this condition in that the memorandum was unanticipated and released after loan origination. Hence, the decision to originate a loan in a particular month was done independently relative to the effect of the policy change.

However, my assumption may be difficult to support if origination month is correlated with time series variation in loan characteristics, such as interest rates and house prices. In order to assuage these concerns, I employ two weaker assumptions. I assume that origination month is quasi-randomly assigned after (i) conditioning on observables and (ii) restricting my sample to a short span of time. Conceptually, I am comparing two identical loans differing in origination date by only a few months. Since economic conditions should vary little in the immediate short run, origination month assignment should be as good as random.

Nevertheless, unobservable loan quality may still correlate with the timing of loan origination even after the battery of controls outlined above. To further rule this possibility out, I refer to the time series variation in ex-ante observable loan characteristics across Ginnie Mae status. A stark change in observable characteristics prior to the announcement would indicate some change in unobservable quality. In Figure 4.2, I estimate the average interest rate, loan-to-value ratio, credit score, and loan balance across origination time and loan type. For this exercise, my sample consists of all loans originated between January 2002 and January 2003. There seems to be little variation in loan characteristics within and across Ginnie Mae status in the months immediately preceding the announcement, except for the average interest rate. While the average interest rate falls by 50 basis points from July 2002 to November 2002, this trend is shared across Ginnie Mae and GSE loans. Hence, the evidence in Figure 4.2 suggests that, conditional on observables, including the initial interest rate and the timing of delinquency, loan quality should vary little over origination month.

As a final piece of suggestive evidence, I refer to institutional details related to loan buyouts. If the month of origination only affects loan performance through the policy change, then loan outcomes unrelated to the policy should be independent of origination month. Since the original Ginnie Mae policy affected a loan's performance *after* experiencing a rolling delinquency, loan performance *before* a rolling delinquency should be constant across vintage. Figure 4.3 plots the delinquency rate across origination time and Ginnie Mae status using the sample of all loans originated between January 2002 and January 2003. In the months preceding the policy announcement, loans across Ginnie Mae and GSE loans vary little in their likelihood of experiencing any delinquency. Given that delinquency correlates with unobservable loan quality, this supports the assumption that origination month should be uncorrelated with loan outcomes except through the Ginnie Mae policy change.

4.5.2 *Effect on Buyout*

Table 4.3 presents estimates of regression Equation (4.2) using buyout as the dependent variable. This specification is equivalent to a first stage in so far as I am interested in the effect of buyout on secondary loan outcomes. Column (1) reports estimates for the sample of Ginnie Mae loans issued between June and November of 2002, conditional on rolling delinquency and a value at above par. The estimated coefficients measure the differential change in buyout activity across month of origination relative to loans issued in June 2002. This specification exploits variation within-MSA and within-delinquency month. I am effectively comparing buyout rates across vintage month for loans experiencing a rolling delinquency in the same period, conditioning on average local buyout rates. Hence, this controls for both the time-series and cross-sectional variation that would generally affect loan outcomes based on economic conditions.

In Column (1), the estimate for the earliest month is 1 percentage point, statis-

tically indistinguishable from zero. The size of the estimate increases monotonically for months closer to the timing of the policy. In three months prior to the policy announcement, the estimate is -4.2 percentage points, statistically different from zero at the 5% level. In the last row, I find that, on average, the Ginnie Mae policy caused loans issued in November to experience a 30.6 percentage point decline in buyouts relative to loans issued in June of that year, statistically different than zero at the 1% level.

In Column (2), I relax the assumption that month of origination is as good as randomly assigned. Instead of using a full set of vintage indicator variables, I estimate Equation (4.2) using a single indicator variable $Z_{\tau > -3}$ for loans issued in the two-month period immediately prior to the policy announcement. In particular, I estimate regressions of the following form:

$$Y_i = X_i' \gamma + \pi_{t > -3} Z_{\tau > -3} + \alpha_t + \alpha_m + \xi_i \quad (4.3)$$

Here, the coefficient of interest, $\pi_{t > -3}$, identifies the differential change in buyout activity in the last two-month period relative to the previous four months. The key identifying assumption is that loans issued in the two-month period before the policy would have reflected loan outcomes similar to the previous four vintage months, if not for the Ginnie Mae policy shock. This assumption seems more defensible in that loan quality within a short six-month period should not deviate significantly in the last two-month period relative to the first four months of the sample. However, if the Ginnie Mae policy change should affect loan outcomes, then this would be most stark precisely in the last two-month period before the announcement. Indeed, Column (2) presents an 18.6 percentage point drop in buyout for the two-month period preceding the policy relative to the first four months. This estimate is statistically different from zero at the 1% level.

The estimates in Columns (1) and (2) confirm that buyout activity fell significantly

following the Ginnie Mae policy change for loans issued immediately prior to the announcement. While the drop in buyout activity is stark, there still remains the possibility that these estimates reflect some trend in buyout activity unrelated to the treatment. In order to falsify my results, I perform a placebo test using a sample of Ginnie Mae loans issued in 2003. Here, τ equals month of origination relative to a placebo shock one year following the announcement. Hence, I assess whether calendar month correlates with buyout activity using a year that had no policy change. I report estimates in Columns (3) and (4) using the sample of Ginnie Mae loans issued six months prior to a 2003 placebo shock. I find no meaningful pattern in the differential effect of origination month on buyout activity. Point estimates are smaller than 0.5 percentage points and generally statistically indistinguishable from zero.

In Columns (5) and (6), I report analogous estimates using the sample of Ginnie Mae loans issued in 2002 that experienced a rolling delinquency while valued below par. If vintage month should relate to buyout activity only due to the Ginnie Mae policy change, then vintage month is irrelevant to loan outcomes when the issuer does not have an incentive to buy the loan out in the first place. When a loan is trading below par, its initial interest rate at the time of origination is below the average market rate at the time of delinquency. Therefore, after buyout and resecuritization, issuers would owe new investors more than what was previously paid. This means that the issuer has no incentive to buy the mortgage if its value falls below par at the time of delinquency. The estimates in Columns (5) and (6) are close to 1 percentage point and generally statistically indistinguishable from zero. For loans valued below par, I find no meaningful pattern in the differential effect of origination month on buyout activity.

Figure 4.4 plots the coefficients in Columns (1) and (3) using 95% confidence intervals. The plot shows the degree to which 2002 vintage and 2003 vintage Ginnie Mae loans experienced differential trends in buyout activity across origination month.

Previously, in order to restrict variation in loan quality, I estimated Equation (4.2) using a limited six-month window of origination. The key identifying assumption hinged upon constant loan quality within a short window of origination. Now, I expand the set of vintage months to January of the sample's origination year. Doing so provides confidence in the internal validity of my estimates. Normalizing to June 2002, I plot estimates from five months prior to June 2002 and five months preceding the policy announcement. Under the key identifying assumption that Ginnie Mae loans would have experienced similar buyout activity relative to June 2002 if not for the Ginnie Mae policy shock, there should be no significant difference in outcomes across vintage months. Indeed, buyout activity remained relatively stable for months earlier in the calendar year. However, starting in the months immediately prior to the 2002 shock, there is a stark drop in buyout for 2002 Ginnie Mae loans. In contrast, Ginnie Mae loans issued in 2003 experience no change in buyout activity for an entire calendar year preceding the 2003 placebo shock.

Taken together, the estimates reported in Table 4.3 and Figure 4.4 suggest that the Ginnie Mae policy severely restricted buyout activity for loans issued in the months preceding the policy announcement relative to earlier originated loans, loans issued in 2003, and loans valued below par at delinquency.

4.5.3 Effect on Foreclosure

The drop in buyout activity identified in the previous section was likely due to the lag between origination and securitization. Loans issued immediately prior to the policy shock could not securitize in time to bypass the suspension of buyouts on rolling delinquencies. With this in mind, the Ginnie Mae policy shock provides an opportunity to test how the buyout option affects long term loan outcomes. An issuer can expect that after buying out a loan, the mortgage can be res securitized into a new Ginnie Mae pool if the borrower becomes current following a rolling or serious

delinquency. Therefore, the buyout option naturally generates an incentive for issuers to maintain high loan quality among repooled loans in order to raise investor appetite. Eliminating the buyout option would therefore unwind these incentives and possibly cause loan quality to deteriorate.

The regressions in Table 4.4 test the effect of the Ginnie Mae policy change on loan quality by estimating regression Equation (4.2) using foreclosure as the the outcome of interest. Here, Y_i takes a value of one if loan i experiences a foreclosure start within 18 months of a rolling delinquency. I choose an 18-month window as opposed to a 12-month window in order to accommodate the long transition from a 30-day rolling delinquency into foreclosure. This contrasts with the literature, which usually measures transition from a 60- or 90-day delinquency into foreclosure. Moreover, loans experiencing a rolling delinquency should take longer to enter foreclosure simply because the borrower is current on all payments but a single payment from earlier in the year. In order to fall into worse delinquency and prompt foreclosure proceedings, a borrower would need to begin missing payments again after three periods of current payment.

Column (1) reports estimates for the sample of Ginnie Mae loans issued in 2002 and valued above par at the time of rolling delinquency. The coefficient of interest, π_τ , measures the differential effect of the Ginnie Mae policy shock on foreclosures for late originated loans relative to loans issued earlier in June 2002. As before, this specification controls for time-series and cross-sectional variation in economic conditions using delinquency month and MSA fixed effects. I estimate that the differential effect of vintage month on foreclosure grew monotonically from 1.8 percentage points, statistically significant at the 10% level, to 5.7 percentage points, statistically significant at the 1% level. This represents a three-fold increase in the foreclosure rate up to the month preceding the policy announcement. In Column (2), I find that, on average, the Ginnie Mae policy change caused loans issued in the two-month period preced-

ing the announcement to experience a 2.6 percentage point increase in foreclosures relative to the first four months in my sample. This estimate is statistically different from zero at the 1% level.

As before, the estimates in Columns (1) and (2) may suffer from systematic shocks to foreclosure due to some unobserved factor correlated with vintage month but unrelated to the Ginnie Mae policy. I once again conduct placebo tests that address this concern. As an aside, the placebo tests in the previous section were not entirely informative given the nature of the samples. For example, loans issued after the January 1, 2003 cutoff had no buyout activity following a rolling delinquency precisely because the policy change entirely eliminated this option. For loans issued before the cutoff but valued below par at delinquency, the incentive to buyout never existed in the first place, independent of the policy. Hence, there is mechanically limited variation in buyout across vintage month for both samples. These placebo tests gain traction when studying loan outcomes other than buyout. Specifically, if the tests in the previous section confirmed that a subset of loans were unaffected by the policy shock, then these are precisely the loans that should similarly experience no differential change in secondary loan outcomes, such as foreclosure.

To this end, I report estimates in Columns (3) and (4) using the sample of Ginnie Mae loans issued six months prior to a 2003 placebo shock. Once again, I find no meaningful pattern in the differential effect of origination month on foreclosure. Point estimates are all economically and statistically indistinguishable from zero. I report estimates in Columns (5) and (6) using the sample of Ginnie Mae loans issued in 2002 and valued below par at the time of rolling delinquency. Here too, point estimates are statistically indistinguishable from zero across all vintage months. Overall, these results parallel those presented in Table 4.3 and provide confidence in the validity of the key identifying assumption.

In Figure 4.5, I plot the coefficients in Columns (1) and (3) along with their 95%

confidence intervals. As before, I expand the sample from a six-month window to the full calendar year preceding the policy announcement. For Ginnie Mae loans originated in both 2002 and 2003, I find no differential change in foreclosure across vintage months preceding June of each respective sample's calendar year. This stability is persistent in the control group, whereby Ginnie Mae loans experience no significant differential change in foreclosure relative to the baseline month of June across the entire calendar year preceding the 2003 placebo shock.

In contrast, treated loans experience a monotonic increase in foreclosure across vintage months immediately preceding the 2002 policy shock. Given the stability in foreclosure rates for 2003 originated loans, Figure 4.5 reinforces the key identifying assumption that loans issued immediately prior to the 2002 memorandum would have experienced outcomes similar to loans originated earlier in the year and loans originated in 2003, but for the effect of the Ginnie Mae policy change. The results in Table 4.4 and Figure 4.5 seem to suggest that the increase in foreclosure is likely due to the suspension of the buyout option and not unobserved factors correlated with vintage month.

4.6 Mechanism

4.6.1 Effect on Early Cure

As an immediate consequence of the Ginnie Mae policy shock, issuers could no longer execute the option to buyout a loan upon suffering a rolling delinquency. In the long run, loans originated immediately prior to the policy shock appear to experience differentially higher foreclosure rates following a rolling delinquency relative to loans originated earlier in the year. The underlying mechanism is straightforward in that the lag between origination and securitization likely prevented loans originated later in the year from securitizing before the January 1, 2003 deadline, thus failing to retain the buyout option. Hence, buyout can help prevent foreclosure.

In this section, I further examine this relationship by studying the medium run impact of the policy shock on loan outcomes. After a loan experiences a buyout, the borrower must be current before the issuer can resecuritize the mortgage. Hence, the issuer will have a material incentive to make the loan reperform after buyout, likely through renegotiation of the terms of the mortgage or forbearance. However, if the Ginnie Mae shock eliminated the buyout option, I expect to see a reduction in cures immediately following a rolling delinquency.

The regressions in Table 4.5 test the effect of the Ginnie Mae policy shock on early cures. I estimate regression Equation (4.2) using early cure as the dependent variable. Here, the outcome of interest Y_i takes a value of one if loan i experiences a cure within 3 months of a rolling delinquency. I use a 3-month window in order to differentiate between a cure associated with a rolling delinquency and a cure associated with a serious delinquency. While the Ginnie Mae policy shock eliminated the buyout option on rolling delinquencies, issuers still had the right to buyout a mortgage following a serious delinquency. Hence if a borrower missed two more payments following a rolling-delinquency and entered 90-day delinquency, then the issuer could execute their buyout option even if the loan securitized after the January 1, 2003 deadline. By restricting the window to cures within 3 months, I identify the effect of the Ginnie Mae policy shock on early cures as opposed to cures in general. The coefficient of interest, π_τ , measures the differential effect of the Ginnie Mae policy shock on immediate cures for late originated loans relative to loans issued earlier in June 2002.

Column (1) reports estimates for the sample of Ginnie Mae loans issued in 2002 and valued above par at the time of rolling delinquency. I estimate that the differential effect of vintage month on early cure decreased monotonically from 1.1 percentage points, statistically insignificant, to -7.4 percentage points, statistically significant at the 1% level. In Column (2), I find that, on average, the Ginnie Mae policy change caused loans issued in the two-month period preceding the announcement to

experience a 4.6 percentage point drop in early cure relative to the first four months in my sample. This estimate is statistically different from zero at the 1% level.

In Columns (3) and (4), using the sample of Ginnie Mae loans issued in 2003 and valued above par at the time of rolling delinquency, I report estimates from specifications equivalent to the first two columns. As before, I present results from a falsification test around a placebo shock in 2003 to determine the validity of my key identifying assumption. If vintage month correlates with the early cure rate in a manner unrelated to the true Ginnie Mae policy shock, then I should identify a reduction in early cures across origination time. In Column (3), most point estimates are statistically insignificant and overall present no systematic pattern as shown in Column (1). In fact, Column (4) reports a statistically and economically insignificant coefficient for the two-month period preceding the placebo shock, equal to a differential effect of -0.1 percentage points relative to the first four months.

In Columns (5) and (6), I estimate Equations (4.2) and (4.3), respectively, using the sample of Ginnie Mae loans issued in 2002 and valued below par at the time of rolling delinquency. Here, I estimate the effect of vintage month on a sample of loans that should be unaffected by the elimination of the buyout option due to the lack of a buyout incentive in the first place. I hypothesize that, in contrast to Columns (1) and (2), there should be no differential effect of vintage month on early cures using this sample. Indeed, Column (5) reports a statistically insignificant coefficient for each vintage month in 2002. Here, I fail to reject the null that loans valued below par experienced no differential change in early cures across vintage months. In Column (6), this is reenforced by testing for any differential effect in the two-month period preceding the policy shock. I find that the estimated coefficient is small and not statistically different from zero.

Figure 4.6 plots the estimates from Columns (1) and (3) using 95% confidence intervals. I now expand the window of vintage months to the entire calendar year

preceding the shock associated with each respective sample. In the case of loans originated in 2002, this shock is represented by the true policy shock. In the case of loans issued in 2003, the shock is represented by a placebo shock, one year from the date of the Ginnie Mae memorandum. I find that most estimates across vintage months preceding June of the calendar year are statistically not different from zero across both samples. Furthermore, for loans issued in 2003, most estimates following June are also statistically insignificant, presenting no systematic pattern across vintage months.

In contrast, loans issued in 2002 experience a monotonic decrease in early cures across vintage months following June 2002 and immediately preceding the policy shock. Taken together, Figure 4.6 validates the key identifying assumption that the reduction in early cures is not driven by some unobserved factors correlated with vintage month. Rather, as evidenced in Table 4.5, this reduction is likely a direct consequence of the Ginnie Mae policy change. These results suggest that when issuers lose the option to buyout a mortgage, those loans experience reduced cures immediately following a rolling delinquency. In contrast, if a loan either held no option for buyout (2003 Ginnie Mae loans) or no incentive for buyout (2002 Ginnie Mae loans valued below par), then there appears to be no systematic reduction in early cures across vintage months.

4.6.2 Effect on Late Cures and Modifications

The previous section suggests that the suspension of buyout likely increased the foreclosure rate for loans experiencing a rolling delinquency due to a reduction in early cures. In this section I investigate this channel further by trying to pin down how early cures increase loan quality sufficiently to prevent foreclosure among loans unaffected by the buyout suspension.

As a first pass, I focus on the difference between early and late cures. If an issuer

fails to cure a loan following a rolling delinquency due to Ginnie Mae's policy change, then the same issuer can still exercise the buyout option by waiting only two more periods of missed payments. The suspension of buyouts reduced cures that would otherwise occur due to a rolling delinquency loan buyout. Hence, measuring cures using a longer horizon should attenuate the estimated effect due to buyouts associated with a serious delinquency. If the effect identified in the previous section relates to the Ginnie Mae policy shock, then there should be no differential effect across vintage months for late cures due to the absence of any treatment.

In Column (1) of Table 4.6, I explore this channel using a longer window of observation. Here, I estimate regression Equation (4.2), where the outcome of interest Y_i takes a value of one if loan i experiences a cure within 12 months of a rolling delinquency. The goal of estimating around a 3-month window earlier was to identify an immediate change in cure rates following a rolling delinquency. Using a 12-month window, I measure changes in cures following both a a rolling delinquency and a serious delinquency, hereafter termed late cures.

In Column (1), I present estimates using the sample of Ginnie Mae loans issued in 2002, valued above par at the time of rolling delinquency. Estimates for all vintage months are statistically insignificant, with the exception of the month immediately preceding the policy change. Even then, this estimate is close to half the analogous estimate in Column (1) of Table 4.5. The same is true in Column (2), where the coefficient estimate for the two-month period immediately prior to the policy shock is close to 40% of the analogous estimate in Column (2) of Table 4.5. Overall, the results in the first two columns of Table 4.6 suggest that the reduction in cures is only present under a short horizon. Hence, the increase in foreclosures identified in the previous section can be attributed to the reduction in early cures as opposed to cures in general.

The difference in estimated effects between early and late cures has policy im-

plications. Assuming that foreclosures increased for late originated loans due to the reduction in immediate cures, then these results imply that the timing of a cure matters. To see this, note that there is no systematic reduction in late cures prior to the policy announcement. If timing for loan cure was immaterial, then the foreclosure rate would have remained constant across vintage month since the late cure rate is relatively constant. And yet I find that the foreclosure rate increased in line with the reduction in *early* cures. Therefore, the results above imply that for a loan that may eventually cure, curing earlier increases its long term loan performance.

In Columns (3) and (4), I explore whether a reduction in modifications can explain the decline in early cures following the suspension of buyouts. Here, I test whether loans cure following a buyout due to issuers modifying the terms of the mortgage to incentivize repayment. Naturally, if the buyout option is suspended for rolling delinquencies, issuers will no longer offer borrowers these modifications, thus reducing the cure rate for mortgages most affected by the policy change. To this end, I estimate regression Equation (4.2) and Equation (4.3), where the outcome of interest Y_i takes a value of one if loan i experiences a modification within 3 months of a rolling delinquency. Once again, a 3-month window associates an identified modification with a rolling delinquency instead of a serious delinquency. Modifications are identified by measuring any changes to the principal balance or reduction in interest rates. Due to data limitations, I cannot identify term extensions. The results in Columns (3) and (4) suggest that there is no differential effect of vintage month on modifications. Point estimates are close to zero and statistically insignificant.

In Columns (5) and (6), I test for any changes in required loan payments. If the mortgage contract was modified along dimensions other than the interest rate and principal balance, then this may be reflected in the required monthly payments owed by the borrower. Namely, the issuer may negotiate with the borrower to make the loan current by adjusting the per period payment. Hence, a reduction in buyout

activity should reduce the rate at which the issuer offers a payment change. I estimate regression Equation (4.2) and Equation (4.3), where the outcome of interest Y_i takes a value of one if loan i experiences a change in loan payments of at least \$50 within 3 months of a rolling delinquency. The results in Columns (3) and (4) are generally statistically insignificant and close to zero, suggesting that there is no differential effect of vintage month on payment changes.

Taken together, while the results in this section suggest that the timing of a loan cure affects long term loan performance, the precise mechanism is unclear. Estimates of the differential effect on renegotiation are statistically and economically indistinguishable from zero. Thus, there appears to be no observable change in loan terms that would explain the relationship between vintage month and early cure. Issuers, therefore, likely induce the early cures that I identify through at least one of the following remaining forbearance channels: (i) extending a subordinate lien on the property to compensate the payments owed to the issuer, (ii) extending the length of the mortgage to make up for the missed payment, or (iii) negotiating terms in a manner unobserved in the data.

Of these three possibilities, given the specialized nature of a rolling delinquency, I posit that issuer incentives are best aligned with the use of a subordinate lien. To understand this argument, consider that when a borrower enters serious delinquency due to a heavy debt burden, then reducing the required payment through a modification can plausibly induce reperformance. In contrast, a rolling delinquency, by definition, occurs when a borrower is *already* paying all recent periods except for a single missed earlier payment. Therefore, the issuer may assume that the borrower will continue to extend future payments under the current terms of the mortgage. Reducing the interest or principal under this assumption would be redundant and amount to a relative loss. Alternatively, if the issuer were to capitalize the arrears into a principal balance increase, the borrower would face higher per period pay-

ments. For the marginally constrained borrower, increasing the debt burden will also increase the risk of default due to the borrower’s limited capacity to service the debt. In contrast to a modification, placing arrears into a subordinate lien can make an issuer whole, cure a loan, and maintain the present value of future payments without further burdening the borrower.

In sum, I find suggestive evidence that intervening early through buyout can reduce foreclosure rates with no obvious change in payments and loan terms. I argue that issuers instead rely upon extending a second lien on the property to reinstate the delinquent loan. In Section 4.8.3, I explore the implications of this argument by studying how the effect of buyout varies with financial slack.

4.7 Robustness

The results from the previous section hinged upon the assumption that the vintage month only affects the outcome through treatment. This assumption would be violated if loan quality fundamentally differed across month of origination in a manner not accounted for in loan characteristics, even after conditioning on delinquency. While I provide evidence that this is unlikely given trends in loan characteristics over time, there still may be some unaccounted variation in the lending environment from one month to the next. In order to capture this variation, my empirical strategy requires a control group that can account for common trends in loan quality within a given month of origination. GSE loans serve as a natural candidate to control for such common variation. Hence, I augment Equation (4.2) using regressions of the following form:

$$Y_i = X_i' \gamma + \sum \pi_\tau Z_\tau \times \text{GNMA}_i + \theta \text{GNMA}_i + \alpha_\tau + \alpha_t + \alpha_m + \xi_i \quad (4.4)$$

In the above, Y_i measures loan performance and Z_τ is equal to one if a loan is originated in month τ , as before. Now, I include an interaction term, GNMA_i , which

equals one if loan i is associated with Ginnie Mae. I retain the control variables and fixed effects used in previous regressions. I now account for shocks common to loans issued in month τ by including vintage fixed effects α_τ . Note that these fixed effects absorb Z_τ . Finally, I include an indicator variable GNMA_i to measure Ginnie Mae status in order to account for heterogeneity in loan outcomes across loan type. Standard errors are clustered at the MSA level to correct for within-MSA residual correlation.

4.7.1 Identification

The coefficient of interest, π_τ , measures the differential change in loan outcomes among Ginnie Mae loans relative to both loans issued earlier in the year as well as relative to non-Ginnie loans. I identify π_τ by restricting variation to within a given vintage month. Hence, after using α_t and α_m to account for time-series and cross-sectional variation, I can further control for common shocks within a given month of origination through α_τ . Furthermore, the Ginnie Mae memorandum generates the quasi-experimental variation necessary for identification by virtually eliminating the buyout option on loans issued immediately prior to the policy announcement. I assume that loan performance across vintage month and Ginnie Mae status would have remained relatively constant in the absence of the Ginnie Mae policy change.

The key identifying assumption is relaxed relative to Section 4.5. Here, vintage month must be as good as randomly assigned after accounting for common shocks to loan outcomes across Ginnie Mae and GSE loans within a given month of origination. Practically, this means that if some unobserved quality correlated with both the timing of origination as well as loan outcomes, then this factor must be accounted for in both Ginnie Mae and GSE loans. This assumption would be violated if some unobserved factor significantly predicted both vintage and loan outcomes for Ginnie Mae loans uniquely. This seems unlikely given the evidence in Figure 4.2 and Figure

4.3, where both ex-ante and ex-post loan quality seems to move in parallel prior to the memorandum across Ginnie Mae and GSE loans.

4.7.2 Results

Table 4.7 presents estimates using the sample of all agency loans and foreclosure as the outcome of interest. Column (1) first reports estimates of regression Equation (4.2) for the sample of exclusively GSE loans issued in 2002, conditional on a rolling delinquency and a value at above par. The vintage month coefficients are generally statistically insignificant, indicating that there is no systematic differential effect of month of origination on foreclosure. This is mirrored in Column (2), where loans issued in the two-month period immediately prior to the policy shock experience no differential effect in foreclosures relative to loans issued in the first four months. These results validate the notion that GSE loans do not experience any meaningful changes in foreclosure rates across vintage months. Therefore, GSE loans function as a suitable control to Ginnie Mae loans issued in 2002.

In Column (3), I estimate Equation (4.4) using the sample of GSE and Ginnie Mae loans issued in 2002, conditional on a rolling delinquency and a value above par. As before, I include origination, borrower, and time-varying controls, in addition to MSA level and month of delinquency fixed effects. Given that my sample now includes both loans securitized through Ginnie Mae (treatment group) and loans securitized through the GSE's (control group), I can also include vintage month fixed effects while still identifying the treatment effect of interest. Here, I restrict identifying variation to the differential effect of vintage month on loan outcomes for Ginnie Mae loans in particular, independent of common shocks across all mortgages originated in a particular month. This specification allows me to isolate the treatment effect from the effect of unobserved factors correlated with both vintage month and foreclosure.

Column (3) reports coefficient estimates for the interaction term $Z_\tau \times \text{GNMA}$. The

coefficient estimate for the first month in my sample is close to zero and statistically insignificant. Three months prior to the policy shock, I estimate that the foreclosure rate increased by 2.6 percentage points for Ginnie Mae loans relative to both Ginnie Mae loans issued in June 2002 and all agency debt issued in the same month. This point estimate is statistically different from zero at the 5% level. One month immediately preceding the policy change, the coefficient estimate on the interaction term increases 60% to 4.2 percentage points, statistically different from zero at the 1% level. The point estimates in Column (3) indicate that the 2002 policy change led to a monotonic increase in foreclosure rates for Ginnie Mae loans across vintage months.

In Column (4), I find that, on average, the Ginnie Mae policy change caused loans issued in the two-month period preceding the announcement to experience a 2.7 percentage point increase in foreclosures relative to the first four months in my sample and relative to all agency debt issued in the same period. This estimate is statistically different from zero at the 1% level. This specification relaxes the assumption that Ginnie Mae loans experienced a differential effect across vintage months preceding the policy. Instead I assume that loans issued in the two-month period preceding the announcement would have experienced foreclosure rates relatively similar to loans issued in the first four months and relative to other GSE loans within the same period. The estimate in Column (4) is virtually identical to the corresponding estimate in Column (2) of Table 4.4 using the sample of Ginnie Mae loans, further bolstering confidence in my underlying assumptions.

Table 4.8 presents results from estimating regression Equation (4.4) using early cures as the outcome of interest. The columns in this table are analogous to those presented in Table 4.7. As before, Column (1) reports estimates using regression Equation (4.2) and the sample of exclusively GSE loans. Across all vintage months, the estimates are close to zero and statistically insignificant. Column (2) corroborates

the results in Column (1), whereby the coefficient estimate for the two-month period immediately prior to the policy shock is close to zero and statistically insignificant. Taken together, the results in Columns (1) and (2) indicate that GSE loans experience no differential effect in early cures across vintage month. Hence, they serve as a plausibly valid control group to test the effect of the 2002 policy shock on early cures among Ginnie Mae loans relative to agency loans in general.

Columns (3) reports coefficient estimates of regression Equation (4.4). While the coefficients are individually statistically insignificant, they present a clear monotonic decline in early cure rates. In contrast, the point estimates in Column (1) are small and present no systematic pattern across vintage months. The differential decline in early cure rates among Ginnie Mae loans is most stark using a pooled specification. In Column (4), I estimate that loans issued in the two-month period prior to the policy experienced a 2.8 percentage point decline in early cures relative to the first four months and relative to all agency loans issued in the same period. This estimate is significantly different from zero at the 5% level. The point estimate measuring the reduction in early cures for Ginnie Mae loans in Column (4) of Table 4.8 is nearly equivalent to the point estimate measuring the increase in foreclosures for Ginnie Mae loans in column (4) of Table 4.7. These results provide confidence that the change in foreclosure and early cure rates are tightly linked to the Ginnie Mae policy change.

4.8 Instrumental Variables

The previous sections established that the Ginnie Mae policy change led to a stark reduction in buyout activity after a rolling delinquency. In the absence of buyout, loans instead experienced an increase in foreclosures and decrease in early cures. In the following section, I formalize these results using an instrumental variables approach. Doing so, I can provide policy-relevant causal estimates of the elasticity of buyout and loan performance.

Using vintage month as an instrument for buyout activity, I estimate two-stage least squares (2SLS) regressions to identify the effect of loan buyout on loan performance. I discuss the validity of using month of origination as an instrument for buyout in Section 4.8.1 below. Conditional on rolling delinquency and value at above par, the first-stage is represented by regressions of the the following form:

$$Buyout_i = X_i' \gamma_1 + \sum_{\tau} \pi_{\tau} Z_{\tau} + \alpha_t + \alpha_m + \xi_i \quad (4.5)$$

where Z_{τ} is the instrumental variable, equal to one if a loan is originated in month τ . In the second-stage, conditional on rolling delinquency and value at above par, I regress loan performance on fitted values for buyout:

$$Y_i = X_i' \gamma_2 + \rho \widehat{Buyout}_i + \alpha_t + \alpha_m + \eta_i \quad (4.6)$$

Here, \widehat{Buyout}_i represents the predicted values from Equation (4.5). Under the assumption that Z_{τ} is a valid instrument, ρ identifies the causal effect of buyout on loan performance.

4.8.1 Identification

LATE

In order to identify the causal effect of interest, the instrument I employ must satisfy certain conditions. Namely, month of origination must vary with buyout activity, must be as good as randomly assigned, and must affect loan performance only through buyout. In addition, my research design must accommodate heterogeneity in instrument response. In particular, if there existed only a subset of loans for which month of origination affects buyout, my instrument must satisfy an additional condition, whereby month of origination must affect all loans in the same direction. In this section, I discuss the validity of these assumptions and their implications for external validity.

Relevance To satisfy the relevance condition, an instrument must affect the probability of treatment assignment. Therefore, the month of origination satisfies the relevance condition if a loan's origination month differentially affects the probability that a loan is bought out. As shown in Section 4.2, Figure 4.1 demonstrates that the buyout rate for loans experiencing a rolling delinquency drops precipitously across vintage months starting in August 2002. In contrast, the buyout rate for serious delinquent loans experiences no change across vintage month. Finally, regression estimates from Table 4.4 and Figure 4.4 further validate the argument that vintage month differentially affects buyout propensity even after accounting for ex-ante loan characteristics.

Independence A valid instrument must be as good as randomly assigned. Given that the the memorandum was unanticipated and released after loan origination, vintage month is a function of conditions exogenous to the policy shock itself. Hence, the timing of origination is not driven by the suspension of the buyout option. As done in Section 4.5.1, I adopt an assumption more flexible than pure random assignment. I assume that origination month is as good as randomly assigned after (i) conditioning on observables and (ii) restricting my sample to a short span of time. Within a short span of time and controlling for factors observable at origination, the precise timing of origination should matter little.

Exclusion Restriction In order to satisfy the exclusion restriction, an instrument must not affect the outcome variable except through treatment. In the context of my research design, this means that, conditional on a rolling delinquency, a loan's month of origination should not affect its long-term performance except through buyout.

Immediately, the nature of the instrument helps support this assumption. Loan quality and, by extension, loan outcomes should be unrelated to the timing of the policy since the announcement was unanticipated. Furthermore, conditional on observables and within a short period of time, whether a loan is originated in one month or the next should not affect long term loan performance. My analysis further conditions on rolling delinquency, month of delinquency, and MSA of origination. Therefore, I restrict identification to two observationally identical loans, both experiencing a rolling delinquency and the same economic conditions at the time of delinquency.

Turning to graphical evidence, as presented in Section 4.5.1, Figure 4.2 and Figure 4.3 present ex-ante loan characteristics and ex-post loan quality, respectively. If loan quality varied across vintage month in a manner that would reflect significant changes in long-term loan outcomes, then this would be apparent here. Figure 4.2 demonstrates that Ginnie Mae loans experience no discrete break in ex-ante loan characteristics across vintage months immediately preceding the announcement. Similarly, Figure 4.3 presents little variation in ex-post delinquency rates across Ginnie Mae status and origination months.

Turning to the analysis in the previous section, Figures 4.5 and 4.6 present evidence that loan outcomes varied little in the early months of the calendar year for loans issued in both 2002 and 2003. For loans issued in 2003, origination timing remained uncorrelated with loan outcomes even in later months of the calendar year. In contrast, loans issued immediately prior to the 2002 policy shock uniquely experienced a stark change in long-term performance. If vintage month correlated with loan outcomes in a manner unrelated to treatment, then this would be apparent in outcomes for loans issued during unexposed periods. Instead, I find that outcomes differ exclusively for loans issued in the most exposed months.

Monotonicity In order to identify a local average treatment effect, I require an

additional assumption of monotonicity, whereby units do not select out of treatment due to assignment to the instrument (defiers). In the context of my research design, this requires that if a loan (i) received a late origination date and (ii) experienced a buyout, then it would be equally or more likely to experience a buyout if it received an early origination date. This is satisfied mechanically given the nature of the instrument. The instrument exploits the fact that loans originated closer in time to the policy announcement would be less likely to securitize prior to the deadline—thus subject to the ban on buyouts. If a late originated loan happened to securitize prior to the deadline, then having more time to securitize would make the loan more or equally likely to securitize prior to the deadline, by definition.

Local Average Treatment Effect Under the assumption that vintage month satisfies the conditions outlined above, I identify the local average treatment effect (LATE) for those loans whose treatment varies due to variation in the instrument. Namely, this subset represents those loans that experienced a buyout due to early origination and would not have experienced a buyout if originated later (compliers). Note that this research design will not identify the effect of buyouts for those loans with (i) a short time-to-securitization (always-takers) and (ii) an excessively long time-to-securitization (never-takers), since the timing of origination would be unaffected by the policy.

External Validity Importantly, time is not the only dimension that determines the subset of loans affected by the instrument. In particular, the treatment effect is unidentified for those loans that would never be bought out after a rolling delinquency, independent of securitization time and, hence, instrument intensity. Note that there does not exist an equivalent group of always-takers, since treatment fundamentally depends on the timing of securitization—if some subset of always-takers

fail to securitize in time, they no longer function as always-takers in the context of my research design. Therefore, my instrument facilitates identifying an effect with a relevant population that includes those loans that would otherwise contribute no variation, if not for the timing of the policy. Under the assumption that time-to-securitization is random within a short span of time, this one-sided non-compliance allows the LATE to be interpreted as the average treatment effect on the treated (ATT).

Weak Instrument Test

Asymptotic properties of the 2SLS estimator depend on the instrument's correlation with the endogenous variable. The bias of the estimated treatment effect will be inversely proportional to how well the instrument predicts treatment. Therefore, small violations of the exclusion restriction will be exacerbated by a weak first-stage. Furthermore, the asymptotic variance of the 2SLS estimator is also inversely proportional to the correlation between the instrument and treatment. Hence, a weak relationship between the treatment and instrument will produce an inefficient estimate of the treatment effect, even if the estimate is consistent.

To test for weak instruments, I obtain the multivariate F-statistic, namely the Cragg-Donald Wald statistic, and its robust counterpart, the Kleibergen-Paap Wald test statistic (Bazzi and Clemens (2013)). Intuitively, these statistics are used to test the null hypothesis that the first-stage coefficients are zero. Stock and Yogo (2005) provide critical values defined by a 10% rejection region under the null that 2SLS bias relative to OLS exceeds 10%, in addition to critical values for the null that I falsely reject $\rho = 0$ in a two-tailed 5% t-test.

Overidentification Test

When an instrumental variables model has more instruments than endogenous variables, then the model is overidentified. In such a case, there is no longer a unique estimator for the coefficient of interest. This is apparent from estimates plotted in Figure 4.4 and Figure 4.5. Taking the ratio of estimates for each vintage month across the two plots, the estimated treatment effect for November is close to half the treatment effect estimated for October. Accounting for this heterogeneity, 2SLS obtains a consistent estimator of the weighted average of individual treatment effects, whereby the weights represent the influence of a particular instrument on the treatment. In the context of my research design, this would imply that estimates for the two-month period prior to the policy should have the greatest weight since loans originated during this period were most severely affected.

In addition to increasing precision, an overidentified model provides a means to test the exclusion restriction under certain assumptions. The Sargan-Hansen overidentification test assumes that at least one instrument is valid and holds a null hypothesis that additional instruments are exogenous. Failing to reject the null, along with confidence in the validity of at least one instrument, would provide strong evidence that all instruments are valid.

Importantly, rejecting the null hypothesis would still leave room for at least one instrument to be valid, given the baseline assumption of the test. In the context of my research design, this would most likely hold for the last two-month period since buyout activity broke most sharply in this period relative to other loan vintages. For this reason, I present estimates for both the overidentified model and the just identified model.

4.8.2 Results

Effect on Foreclosure

Panel A of Table 4.9 presents second-stage estimates from the instrumental variable estimation using foreclosure as the dependent variable. Column (1) reports estimates for the sample of Ginnie Mae loans issued in 2002, conditional on a rolling delinquency and a value at above par. Here, I use the full set of vintage months as instruments. Note that first-stage and reduced-form estimates are effectively reported in Column (1) of Table 4.3 and Table 4.4, respectively, from Section 4.5. In Column (1) of Table 4.9, I estimate Cragg-Donald and Kleibergen-Paap Statistics of over 100, a strong rejection of the null hypothesis that the set of instruments is weak. This is unsurprising given the results from Section 4.5, indicating that vintage month strongly correlates with buyout activity, dropping starkly in the last months prior to the policy announcement. Given that these diagnostic statistics far exceed the critical values provided by Stock and Yogo (2005), the worst-case bias of 2SLS should be limited and extreme outlier 2SLS estimates are highly unlikely.

In Column (1) of Panel A, I also report the p-value for the Sargan–Hansen test of overidentifying restrictions using foreclosure as the outcome of interest. Importantly, this test assumes that at least one instrument is exogenous. While inherently untestable, the evidence presented in Section 4.5 strongly suggests that, at a minimum, the two-month period preceding the policy announcement should satisfy the exclusion restriction. Hence, assuming that this two-month period provides exogenous variation in buyout activity, I fail to reject the null hypothesis that additional instruments are exogenous at the 5% level. This provides confidence in the validity of the full set of instruments.

Column (1) of Panel A in Table 4.9 presents estimates of the second-stage regression given by Equation (4.6). I find that buyout reduces foreclosure by 12.4 percentage

points, statistically different from zero at the 1% level. This specification accounts for the same set of loan, borrower, and regional characteristics as earlier. Here, I restrict identification to within-delinquency month and within-MSA variation in foreclosure across vintage months. Under the assumptions outlined in Section 4.8.1, I identify the average treatment effect on the treated. Therefore, the estimate in Column (1) implies that loans experiencing a buyout would have counter-factually experienced an 80% increase in foreclosure.

Column (2) of Panel A in Table 4.9 presents just-identified 2SLS estimates using first-stage regressions given by Equation (4.3). Here, the excluded instrument is $Z_{\tau>-3}$, an indicator variable equal to one if a loan experiences a rolling delinquency in the two-month period immediately prior to the Ginnie Mae policy announcement. As before, the coefficient from the first-stage regression, $\pi_{t>-3}$, identifies the differential change in buyout activity in the last two-month period relative to the previous four months. The choice of $Z_{\tau>-3}$ as the strongest candidate for the just-identified 2SLS estimate assumes that loans issued in the two-month period immediately preceding the policy announcement would have experienced virtually equivalent outcomes to those loans issued in the first four months, if not for the Ginnie Mae policy shock.

Indeed, the first stage seems to suggest this to be the case. As before, I borrow from Section 4.5, where I present first-stage and reduced-form estimates, reported in Column (2) of Table 4.3 and Table 4.4, respectively. In Column (2) of Table 4.9, I estimate Cragg-Donald and Kleibergen-Paap Statistics of over 200, strongly rejecting the the null hypothesis that the set of instruments is weak.

Turning to the second-stage, the estimate in Column (2) indicates that buyout reduces foreclosure by 14 percentage points, statistically different from zero at the 1% level. This estimate is 1.6 percentage points larger than the estimate in Column (1). The just-identified 2SLS estimator has the desirable property that its distribution is centered at the population parameter value, hence it is approximately median

unbiased (Angrist and Krueger (1999)). The similarity in estimates across Columns (1) and (2) suggests that the bias of the overidentified 2SLS estimate should be limited relative to the population parameter.

In Column (3), I present second-stage estimates for the sample of all agency loans issued in 2002, conditional on a rolling delinquency and a value at above par. Here, I augment the first-stage regression Equation (4.5) to include interaction terms with an indicator for Ginnie Mae status and vintage month fixed effects. The effective first-stage regression is equivalent to Equation (4.4). In theory, I am comparing foreclosure rates between Ginnie Mae and GSE loans after controlling for variation attributable to vintage, delinquency time, and MSA.

In Column (3) of Panel A, I report the p-value for the Sargan–Hansen test of overidentifying restrictions using foreclosure as the outcome of interest. Assuming at least one exogenous instrument, I fail to reject the null hypothesis that additional instruments are exogenous at the 10% level. The assumption of at least one exogenous instrument is relaxed relative to the specification in Column (1) since I interact vintage month with Ginnie Mae status. Hence the p-value in Column (3) provides strong reassurance regarding the validity of the set of instruments.

The estimate in Column (3) indicates that buyout reduces foreclosure by 11.6 percentage points, statistically different from zero at the 1% level. This estimate is 0.8 percentage points smaller than the estimate in Column (1) and 2.4 percentage points smaller than the estimate in Column (2). Assuming that including GSE loans corrects for any omitted variable bias associated with vintage month, estimates using Ginnie Mae loans alone are relatively close to the more precise estimate given by Column (3). In fact, the coefficient in Column (3) still indicates that loans experiencing a buyout would have counter-factually experienced a foreclosure rate close to 80% higher.

Finally, Column (4) of Panel A in Table 4.9 presents just-identified 2SLS estimates using a first-stage regression similar to Equation (4.4). Here, the excluded instrument

is $Z_{\tau > -3} \times \text{GNMA}$, an indicator variable equal to one if a Ginnie Mae loan experiences a rolling delinquency in the two-month period immediately prior to the Ginnie Mae policy announcement. The estimate in Column (4) indicates that buyout reduces foreclosure by 12.5 percentage points, statistically different from zero at the 1% level. This final specification holds the most desirable properties, as it is approximately median unbiased and accounts for within-vintage variation. Importantly, this estimate is within almost 10% of the just-identified estimate in Column (2) using the sample of only Ginnie Mae loans.

Effect on Early Cures

Panel B of Table 4.9 presents second-stage estimates from the instrumental variable estimation using early cure as the dependent variable. Column (1) reports estimates using the full set of vintage month instruments Z_τ for the sample of Ginnie Mae loans issued in 2002, conditional on a rolling delinquency and a value at above par. The first stage estimates are the same as in Panel A and the reduced-form estimates are reported in Column (1) of Table 4.5 from Section 4.5.

Column (1) of Panel B presents estimates of the second-stage regression given by Equation (4.6) using early cure as the outcome of interest. I find that buyout increases early cure by 21.5 percentage points, statistically different from zero at the 1% level. Column (1) of Panel B also reports the p-value for the Sargan–Hansen test of overidentifying restrictions using early cure as the dependent variable. Assuming at least one instrument is exogenous, I reject the null hypothesis that additional instruments are exogenous at the 1% level. In the context of measuring early cures, vintage month fails to identify the same population parameter. This is likely due to limited variation in the first few months of my primary sample, as show in Figure 4.6. Importantly, this test does not reject the validity of at least one instrument, which I argue is best represented by the two-month period immediately preceding the policy

announcement.

With this in mind, Column (2) of Panel B in Table 4.9 presents just-identified 2SLS estimates using first-stage regressions given by Equation (4.3). Here, the excluded instrument is $Z_{t>-3}$, an indicator variable equal to one if a loan experiences a rolling delinquency in the two-month period immediately prior to the Ginnie Mae policy announcement. Once again, first-stage estimates are the same as in Panel A and I report reduced-form estimates in Column (2) of Table 4.5. The estimate in Column (2) of Panel B in Table 4.9 indicates that buyout increases early cures by 24.8 percentage points, statistically different from zero at the 1% level. Given that the 2SLS estimator is a weighted average of individual treatment effects based on the influence of each instrument, the overidentified estimate is naturally close to the just-identified estimate.

In Column (3), I present second-stage estimates for the sample of all agency loans issued in 2002, conditional on a rolling delinquency and a value at above par. I report the p-value for the Sargan–Hansen test of overidentifying restrictions using early cure as the outcome of interest. Assuming at least one exogenous instrument, I fail to reject the null hypothesis that additional instruments are exogenous at the 10% level. The p-value in Column (3) reinforces the the validity of the set of instruments in estimating the treatment effect when including all agency loans. Given the failure of this test in Column (1), the result in Column (3) implies that accounting for vintage month fixed effects absorbs significant variation necessary to ensure that the set of instruments is exogenous.

The size of the estimate in Column (3) further supports this conclusion. Here I estimate that buyout increases early cures by 13.2 percentage points, statistically different from zero at the 5% level. This estimate is almost half of those presented in Columns (1) and (2). The estimate in Column (4) of Panel B does not vary much from Column (3), where I report a just-identified 2SLS estimate of 13.0 percentage

points, statistically different from zero at the 5% level. Given that these estimates account for vintage month fixed effects, the results in Panel B suggest that unobserved factors may correlate with both vintage month and incidence of early cure. Hence, accounting for this variation is important in identifying the treatment effect for early cures in particular. Note that these estimates are virtually equivalent to the analogous specification in Panel A, reinforcing the argument that buyouts reduce foreclosure due to an increase in early cures.

4.8.3 Heterogeneous Treatment Effects

In the previous section, I estimated the causal elasticity of buyout and loan performance. Under the assumptions of one-sided non-compliance, my results indicate that buyouts reduce foreclosure for loans normally selected into buyout. Given that foreclosure rates are equivalent across buyout status, I argue that issuers likely buyout loans with worse counter-factual performance relative to non-buyout loans. In other words, the evidence in the previous section suggests that issuers select low-quality loans for buyout. While the interpretation above explains selection into treatment, in this section, I explore heterogeneity in the treatment effect to better characterize the channel through which loans are affected by buyout.

Debtor Financial Constraints

In this subsection, I examine the degree to which financial constraints can explain the results in the previous section. Borrowers with relatively more liquid savings or stable lines of credit may find it easier to prevent worsening delinquency after an initial missed payment. Therefore, unconstrained borrowers may prevent foreclosure independent of selection into buyout. In contrast, I hypothesize that early intervention should benefit those borrowers that are most at risk to worsen in delinquency due to limited access to finance. If an issuer provides forbearance that reduces the

burden of repayment upon a constrained debtor, then perhaps that borrower will be incentivized to continue making future payments. For example, Melzer (2017) finds that homeowners at risk of default cut back mortgage principal payments due to limited incentive to maintain loan quality. Here, I directly test whether early cures can prevent worsening delinquency for precisely these most at-risk borrowers.

In order to explore this hypothesis, I reestimate my main IV specification on various subsamples derived using proxy variables for financial constraints. I construct these restricted samples by first identifying key demographic characteristics that may correlate with financial constraints due to economic or historical reasons. The variables I measure include the percent of adults that (i) hold less than a high school education, (ii) are below the poverty line, (iii) are unemployed, and (iv) identify as non-white. These factors proxy for difficulty in accessing financing due to issues related to employment, income, wealth, and discrimination. For example, borrowers in localities with above median unemployment may find it difficult to refinance when interest rates decline (Defusco and Mondragon (2020)). Similarly, borrowers in primarily non-white counties may fall into worse delinquency following an income shock due to limited liquid savings (Ganong et al. (2020)). I obtain county-level values for each threshold variable using the 2000 decennial census. Finally, I split my primary sample into loans from counties with above and below median values of each threshold variable.

In Panel A of Figure 4.7, I report just-identified 2SLS estimates using subsamples derived from each measure of financial constraints. I follow the specification outlined in Column (2) of Table 4.4. I include all controls, as well as fixed effects for MSA and delinquency month. Here, I plot IV estimates that measure the causal effect of buyout on foreclosure for constrained and unconstrained localities, separately. Across all measures, I find that borrowers in financially constrained localities experience a larger treatment effect relative to unconstrained counties. Counties with less educational

attainment experience a 25% larger reduction in foreclosures relative to counties with more graduates. Localities with a higher rate of poverty experience a 70% larger treatment effect relative to more affluent counties. High unemployment counties experience a treatment effect that is three times larger than the effect estimated for low unemployment counties. Finally, the effect of buyout appears exclusively salient for counties with an above-median non-white population.

Assuming that these variables proxy for financial constraints, these results suggest that limited access to finance likely increases the impact of the reduction in foreclosure risk following a loan buyout. For each proxy variable, loans in constrained counties appear to benefit most from selection into buyout. Taken together, these results reinforce the argument that buyout reduces foreclosure by incentivizing issuers to cure loans.

Creditor Financial Slack

While financially constrained borrowers benefit most from buyout, issuers may fail to provide effective foreclosure relief when financial slack is limited. For example, Aiello (2022) finds that financially constrained servicers perform foreclosures and modifications that are value reducing for both investors and borrowers. In the context of buyout, the most relevant constraint should correlate with the availability of collateral. This is due to the use of subordinate debt to capitalize arrears and cure loans, as suggested by evidence presented in Section 4.6.2. I hypothesize that when the credit supply contracts or the underlying asset (the borrower's home) is over-levered, then an issuer has less financial slack to extend a second lien to reinstate the loan. Thus, buyout should benefit borrowers residing in localities with greater slack in the mortgage credit supply. If the credit supply is less constrained, then an issuer has more flexibility to capitalize arrears into a subordinate loan to cure a delinquent loan. Since the subordinate loan is inherently tied to collateral, whether issuers can cure a

loan through subordination depends on the availability and quality of the underlying collateral.

I test this hypothesis by reestimating my main IV specification on subsamples derived using proxy variables for slack in the mortgage credit supply. As before, I construct restricted samples by first generating measures of financial slack. The variables I use include the county-level loan denial rate and the county-level loan-to-income ratio for originated loans. The average denial rate measures the degree to which lenders restrict the credit supply in one county relative to another. The average loan-to-income ratio measures the availability of collateral to secure a second lien. I obtain measures for each threshold variable using 2002 HMDA loan application data. Finally, as before, I split my primary sample into loans from counties with above and below median values for each threshold variable.

In Panel B of Figure 4.7, I report just-identified 2SLS estimates using subsamples derived from each measure of financial slack. As before, I follow the specification in Column (2) of Table 4.5, exploiting within-MSA and within-delinquency month variation to identify the effect of buyout on foreclosure for each subsample. In the first two rows, I plot IV estimates that measure the casual effect of buyout on foreclosure across counties with above and below median slack in the mortgage credit supply. I find that across both threshold variables, the treatment effect is larger in counties where the mortgage credit supply is slack. In counties with below median denial rates, loans experience a treatment effect that is 30% larger than in counties with less slack. Loans issued in counties with an average loan-to-income ratio below median experience a treatment effect that is four times as large as counties with above median ratios. In both instances, estimates obtained using counties with less slack appear statistically insignificant. Ultimately, the close relationship between the salience of the buyout effect and quality of collateral seems to support the hypothesis that issuers rely upon capitalizing arrears into subordinate debt.

In order to distinguish between financial slack in the mortgage credit supply and borrower financial constraints, I repeat the exercise above using a measure of financial slack that abstracts from mortgage credit in particular. I obtain the aggregate debt-to-income ratio for each county in my primary sample using the Enhanced Financial Accounts (EFA) data provided by the Federal Reserve. The EFA data represents a combination of household debt data from the Equifax/Federal Reserve Bank of New York Consumer Credit Panel and income data from Bureau of Labor Statistics (Ahn et al. (2018)). I split my primary sample into loans from counties with above and below median values for this debt-to-income measure.

Whereas aggregate loan-to-income signals the degree of slack available for mortgage credit in particular, aggregate debt-to-income is more broad since it includes all consumer debt, such as auto and credit card debt. Hence, debt-to-income functions as a signal for borrower financial constraints more than slack for any individual credit type. Given this context, I hypothesize that loans issued in high debt-to-income counties experience larger treatment effects relative to loans originated in low debt-to-income counties. If consumer debt-to-income, as opposed to mortgage loan-to-income, is representative of borrower financial constraints, then this test should resemble the tests in the Section 4.8.3.

In the final row of Panel B in Figure 4.7, I report just-identified 2SLS estimates using subsamples derived from aggregate debt-to-income. I plot IV estimates that measure the causal effect of buyout on foreclosure for loans issued in counties with above and below median debt-to-income, separately. In contrast to the first two rows of Panel B in Figure 4.7, I find that borrowers in counties with above median debt-to-income experience a 70% larger treatment effect relative to loans issued in low debt-to-income counties. The estimate for below median debt-to-income counties is statistically insignificant. These results support the interpretation reached in Section 4.8.3 that financially constrained borrowers experience the largest reduction in fore-

closure following buyout. These results also contrast with the first two rows of Panel B in Figure 4.7 because, instead of measuring borrower financial constraints, the first two rows measure financial slack as it relates to collateral quality and availability.

Profit Motive

My main results condition on a positive difference between the initial interest rate and the average mortgage rate at the time of delinquency. This positive interest rate premium implies that I identify the effect of buyout on loan performance when the buyout incentive exists. Naturally, if the issuer resecuritizes the buyout loan for a lower rate but receives the same per period amount from the borrower, then the issuer profits from passing through less per period to investors.

In this section, I explore how this profit motive affects the quality of loan cure. I first decompose my primary sample into loans with above and below interest rate premiums. Doing so, I classify loans based on whether issuer profit motive provides a strong or weak incentive to resecuritize. I hypothesize that loans with a strong incentive to resecuritize will likely perform better than loans with a weaker incentive. Prior literature has demonstrated that the sellers of debt often signal loan quality in order to facilitate sale (Begley and Purnanandam (2016)). Creditors may increase effort into curing loans in order to signal loan quality ex-post, building reputation of high-quality cures in newer mortgages. Work by Hartman-Glaser (2017) and Adelino et al. (2017) argue that reputation is fundamental to MBS markets in particular.

In Figure 4.8, I report just-identified 2SLS estimates using subsamples derived from above and below median interest rate premiums. In the first row, I plot IV estimates that measure the causal effect of buyout on foreclosure for loans with a strong and weak buyout incentive, separately. I find that loans with a strong incentive to buyout and resecuritize experience an 80% larger reduction in foreclosures relative to loans with a weak profit motive. This result suggests that issuers plausibly put

greater effort to cure precisely the loans that have the greatest payoff.

However, an alternative interpretation hinges upon the results in Section 4.8.3. The loans with the highest interest rate premium are also the loans with the highest interest rate. In that case, one interpretation of my result is that loans with high interest rates, and therefore borrowers that are financially constrained, benefit most from loan buyout. To test this channel, I repeat the previous exercise and decompose my primary sample into loans with no delinquency and loans with some delinquency over the past year. Doing so, I exploit ex-post loan quality to identify the effect of loan buyout on foreclosure for high and low risk loans separately. In Figure 4.8, I report just-identified 2SLS estimates using these. I find that ex-post riskier loans experience an 60% larger reduction in foreclosures relative to ex-post less risky loans. The point estimate for low risk loans is statistically insignificant at the 5% level. This result fails to reject the null hypothesis that high premium loans experience a larger reduction in foreclosure due to loan risk instead of issuer incentives.

However, this last result may mask treatment effect heterogeneity across both interest rate premium and loan risk. A sharp rejection of my null hypothesis would therefore condition on both dimensions simultaneously. If high risk, high premium loans in particular experienced a larger treatment effect, then I would definitively fail to reject the null hypothesis that my results are driven by borrower financial constraints instead of profit motives. To test this, I once again repeat the previous exercise by decomposing my primary sample into loans with no delinquency and loans with some delinquency over the past year. I then decompose each subsample into loans with above and below median interest rate premiums.

Table 4.10 presents just-identified 2SLS estimates that are equivalent to those in in Column (2) of Table 4.5. These estimates measure the casual effect of buyout on foreclosure across loans with above and below median interest rate premium and across high and low risk. I find that among loans with a high interest rate premium,

low risk loans experience a two-fold larger reduction in foreclosures relative to high risk loans. The estimate for high risk loans is statistically insignificant. When the interest rate premium is below median, I find that the effect is exclusively salient for high risk loans. Taken together, these results imply that when issuer incentives are low, then borrower financial constraints determine the size of the treatment effect. When issuer incentives are strong, then the best performing loans experience the largest reduction in foreclosures.

4.9 Conclusion

This paper examines the role of delinquent loan buyout on loan performance. I exploit quasi-experimental variation in buyout propensity across the vintage months preceding a suspension on rolling delinquency buyout. The policy announcement was unanticipated and affected those loans securitized following a January 1, 2003 deadline. Within a short span of time preceding the shock, loan quality and economic conditions should vary little across vintage months. However, due to the lag between origination and securitization, loans issued in the months immediately prior to the announcement were far less likely to securitize in time to avoid the suspension. I estimate that in the last month alone, a mortgage experiencing a rolling delinquency was 30.6 percentage points less likely to experience a buyout compared to only five months earlier. In response to this drop in buyout activity, I find that the foreclosure rate for loans issued preceding the announcement monotonically increased by 5.7 percentage points and early cures monotonically fell by 7.4 percentage points. I demonstrate that this reduction in cures is not due to interest rate, principal balance, or payments modifications, but likely due to a reduction in term extensions or payment deferrals. Finally, using an instrumental variables framework, I identify policy-relevant causal estimates of the elasticity of buyout and loan performance.

The results in this paper have implications for several areas of future research

and policy discussion. First, preventing worsening delinquency and foreclosure in mortgage markets has been of primary importance since the financial crisis. The main result of this paper speaks directly to this concern, whereby I find that delinquent debt sale creates incentives for better loan quality. In order for issuers to resell delinquent debt, they renegotiate with borrowers in a manner that reduces long-term foreclosure risk. Importantly, I find that early cures in particular are effective at reducing foreclosure, as opposed to late cures. Second, the efficacy of the buyout option has recently come into question again in the wake of the recent COVID pandemic. Ginnie Mae for example recently introduced stricter criteria by which an issuer could resecuritize a buyout loan. This paper, in contrast, points to the benefit of the agency buyout option on delinquent debt. I find that this benefit would accrue to investors through better loan quality, and, more directly, benefit borrowers through foreclosure prevention. This finding is of great consequence even beyond Ginnie Mae loans given that \$8.5 trillion worth of agency debt outstanding holds a buyout option on seriously delinquent mortgages. Third, with a face value close to \$1 trillion, financing of distressed debt outside of the mortgage market represents an essential component of asset markets. While my setting is based on agency debt in particular, the dynamics that I identify have implications for creditors in other lending markets as well.

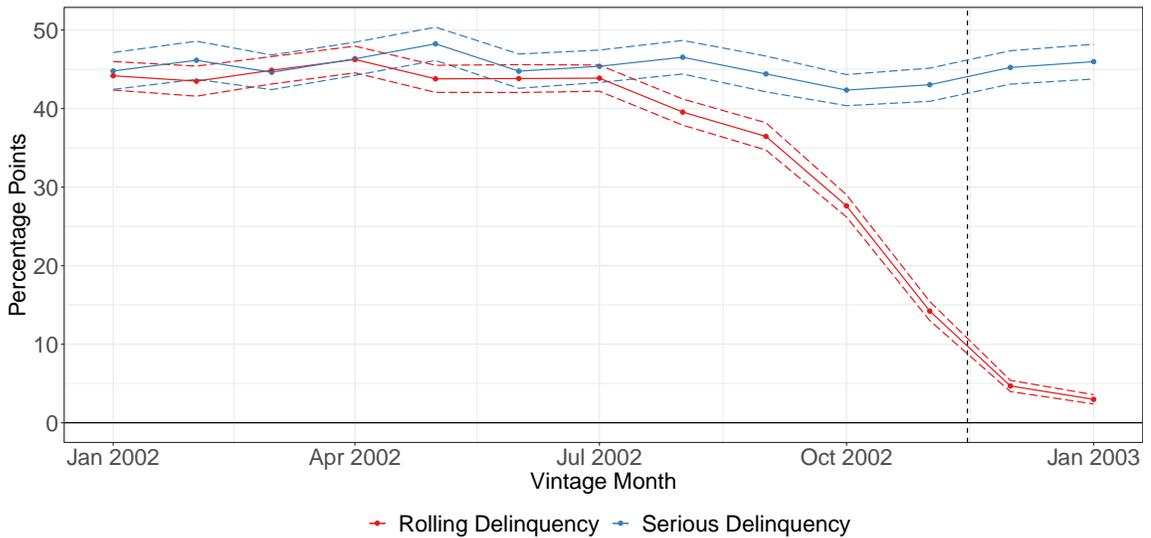
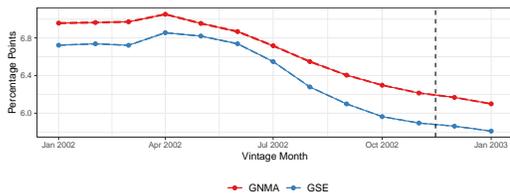
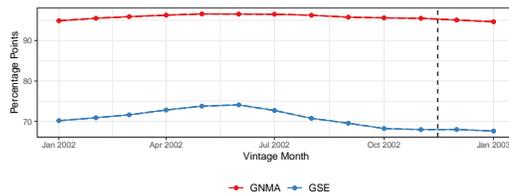


FIGURE 4.1. Buyout Rate of Rolling Delinquencies

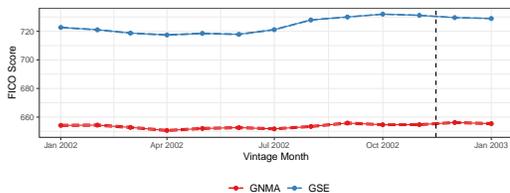
Note: This figure plots the buyout rate of GNMA loans across month of origination, conditional on delinquency. Loans are originated between January 2002 and January 2003. Delinquency is observed between the time of origination and July 2007. Rolling delinquency occurs when a loan holds the status of at least one missed payment for a consecutive four periods. Serious delinquency occurs when a loan enters a status of three missed payments. Each dot represents an estimated average for a given month. The colored dotted lines represent 95% confidence intervals calculated using standard errors. The black dashed vertical line indicates the date of the policy change. Data is collected from Corelogic.



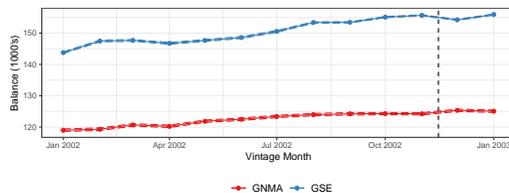
Panel A: Interest Rates



Panel B: LTV



Panel C: FICO Score



Panel D: Original Balance

FIGURE 4.2. Loan Characteristics Across Origination Month and Investor

Note: This figure plots loan characteristics of GNMA and GSE loans across month of origination. Loans are originated between January 2002 and January 2003. Panel A plots the interest rate. Panel B plots the loan-to-value ratio. Panel C plots the FICO credit score. Panel D plots the loan balance. Each dot represents an estimated average for a given month. The colored dotted lines represent 95% confidence intervals calculated using standard errors. The black dashed vertical line indicates the date of the policy change. Data is collected from Corelogic.

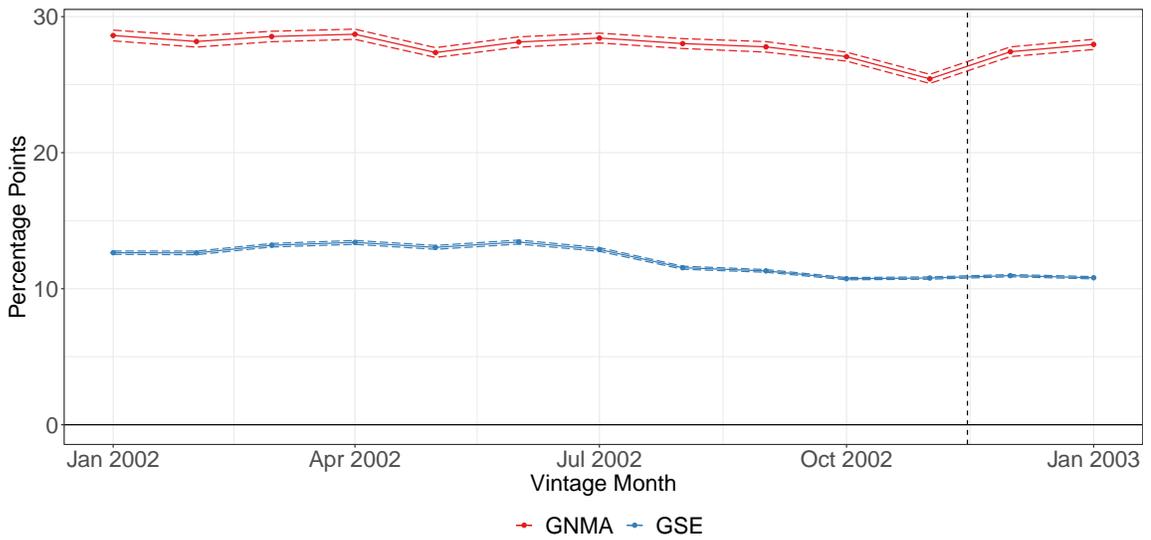


FIGURE 4.3. Delinquency Rate Across Origination Month and Investor

Note: This figure plots the delinquency rate of GNMA and GSE loans across month of origination. Loans are originated between January 2002 and January 2003. Delinquency is observed between the time of origination and July 2007. Delinquency is defined as missing at least one payment since the time of origination. Each dot represents an estimated average for a given month. The colored dotted lines represent 95% confidence intervals calculated using standard errors. The black dashed vertical line indicates the date of the policy change. Data is collected from Corelogic.

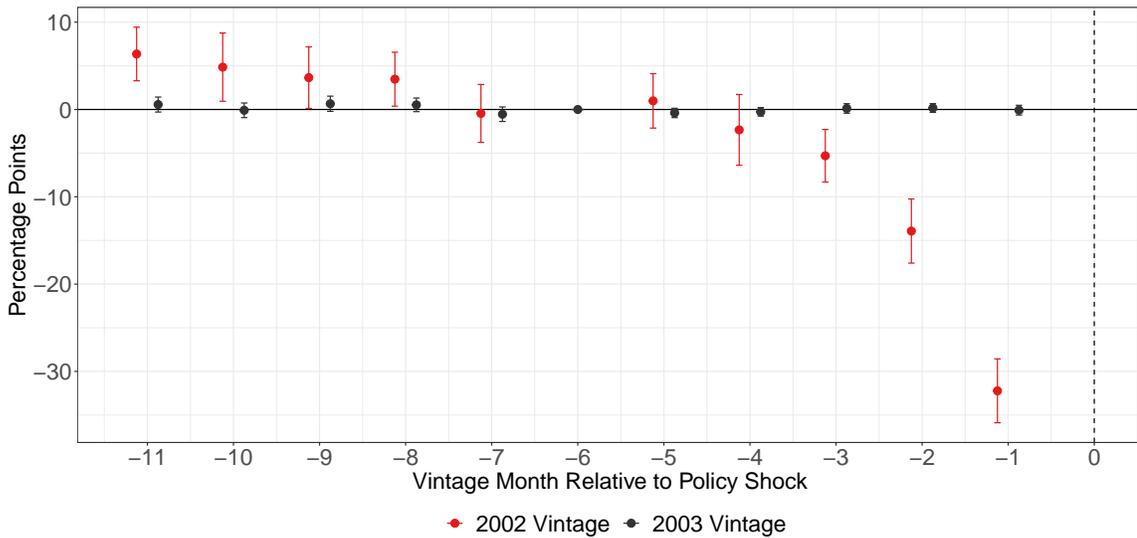


FIGURE 4.4. Effect of GNMA Policy Change on Buyout

Note: This figure reports OLS regression estimates of the effect of the GNMA policy change on the probability of a buyout, conditional on rolling delinquency and value at above par. The outcome variable takes a value of one if a loan experiences a buyout within 1 month after a rolling delinquency. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Vintage month measures the month of origination relative to the policy or placebo shock, which occurs in November of the sample's calendar year. The coefficient for relative month -6 is normalized to zero. The series in red plots the coefficient estimates using the sample of loans originated in 2002. The series in black plots the coefficient estimates using the sample of loans originated in 2003. The black dashed vertical line indicates the date of the policy change. The error bar represents 95% confidence intervals calculated using MSA-clustered standard errors. Loans are originated between January and November of the sample's calendar year. Outcomes are observed between the time of origination and July 2007. Data is collected from Corelogic.

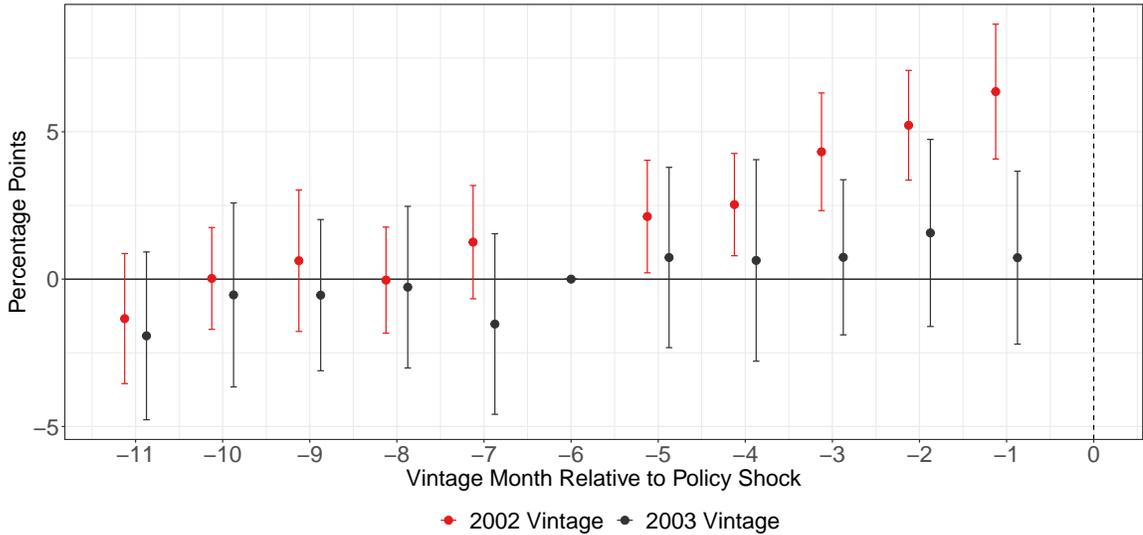


FIGURE 4.5. Effect of GNMA Policy Change on Foreclosure

Note: This figure reports OLS regression estimates of the effect of the GNMA policy change on the probability of a foreclosure, conditional on rolling delinquency and value at above par. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Vintage month measures the month of origination relative to the policy or placebo shock, which occurs in November of the sample's calendar year. The coefficient for relative month -6 is normalized to zero. The series in red plots the coefficient estimates using the sample of loans originated in 2002. The series in black plots the coefficient estimates using the sample of loans originated in 2003. The black dashed vertical line indicates the date of the policy change. The error bar represents 95% confidence intervals calculated using MSA-clustered standard errors. Loans are originated between January and November of the sample's calendar year. Outcomes are observed between the time of origination and July 2007. Data is collected from Corelogic.

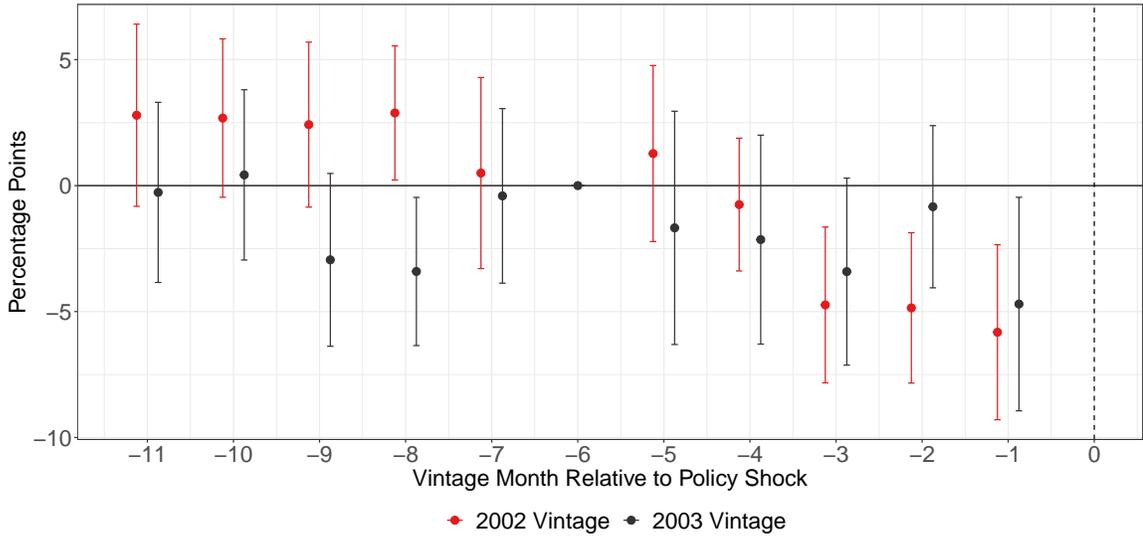
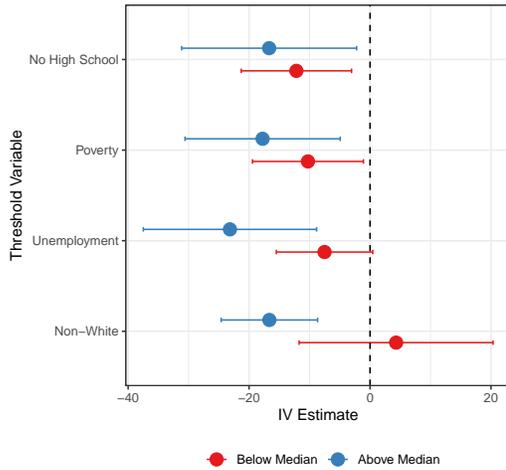
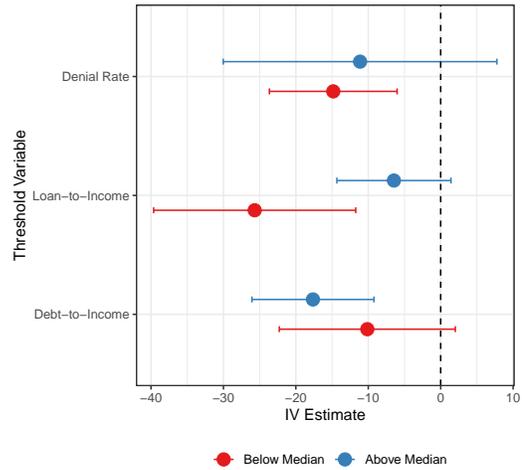


FIGURE 4.6. Effect of GNMA Policy Change on Early Cure

Note: This figure reports OLS regression estimates of the effect of the GNMA policy change on the probability of a cure, conditional on rolling delinquency and value at above par. The outcome variable takes a value of one if a loan experiences a cure within 3 months after a rolling delinquency. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Vintage month measures the month of origination relative to the policy or placebo shock, which occurs in November of the sample's calendar year. The coefficient for relative month -6 is normalized to zero. The series in red plots the coefficient estimates using the sample of loans originated in 2002. The series in black plots the coefficient estimates using the sample of loans originated in 2003. The black dashed vertical line indicates the date of the policy change. The error bar represents 95% confidence intervals calculated using MSA-clustered standard errors. Loans are originated between January and November of the sample's calendar year. Outcomes are observed between the time of origination and July 2007. Data is collected from Corelogic.



Panel A: Financial Constraints



Panel B: Financial Slack

FIGURE 4.7. Effect of GNMA Policy Change on Foreclosure: Heterogeneity

Note: This figure reports heterogeneous treatment effect estimates of buyout on foreclosure. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. Buyout is instrumented with $Z_{\tau > -3}$ from Column 2 in Table 4.4. The $Z_{\tau > -3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. Point estimates are obtained by adding the baseline treatment effect and the corresponding interaction. The error bar represents 95% confidence intervals calculated using MSA-clustered standard errors. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007. Data is collected from Corelogic.

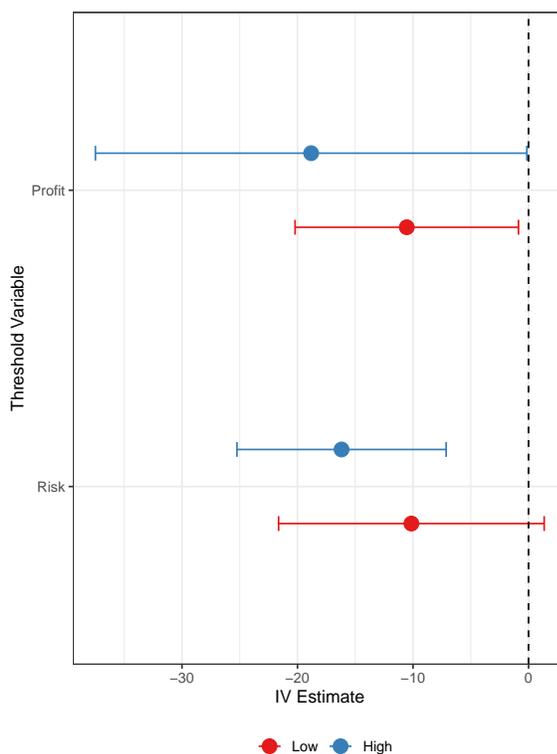


FIGURE 4.8. Effect of GNMA Policy Change on Foreclosure: Profit Motive

Note: This figure reports 2SLS estimates of regressions of foreclosure the GNMA policy announcement. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. Buyout is instrumented with $Z_{\tau > -3}$ from Column 2 in Table 4.4. The $Z_{\tau > -3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. High and low profit is defined by the rate spread on the loan. High and low risk is defined by any prior delinquency in the past year. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. Point estimates are obtained by adding the baseline treatment effect and the corresponding interaction. The error bar represents 95% confidence intervals calculated using MSA-clustered standard errors. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007. Data is collected from Corelogic.

Table 4.1. Summary Statistics

	All Loans		Buyout		No Buyout	
	Mean	SD	Mean	SD	Mean	SD
<i>Panel A: Primary Sample</i>						
Initial Rate	6.76	0.44	6.84	0.40	6.70	0.46
Original LTV	96.13	7.07	96.10	6.87	96.15	7.21
Balance (000's)	112.74	43.12	109.57	42.09	115.13	43.73
15 Month Term	0.02	0.14	0.02	0.14	0.02	0.14
20 Month Term	0.03	0.16	0.03	0.16	0.03	0.16
Prime Borrower	0.45	0.50	0.46	0.50	0.44	0.50
Refinance Loan	0.24	0.43	0.21	0.41	0.27	0.44
Primary Occupancy	0.83	0.37	0.82	0.38	0.84	0.37
Single-Family	0.84	0.37	0.85	0.35	0.82	0.38
Low Doc/No Doc	0.26	0.44	0.21	0.40	0.31	0.46
Number of Obs	13,432		5,758		7,674	
<i>Panel B: Pre-Period</i>						
Buyout	0.52	0.50	1.00	0.00	0.00	0.00
Foreclosure	0.15	0.36	0.15	0.36	0.15	0.36
Early Cure	0.47	0.50	0.47	0.50	0.47	0.50
Late Cure	0.74	0.44	0.75	0.43	0.74	0.44
Modification	0.00	0.02	0.00	0.02	0.00	0.01
Payment Change	0.03	0.17	0.02	0.13	0.04	0.19
Number of Obs	11,439		5,905		5,534	

Note: This table reports mean and standard deviation values of loan characteristics and outcomes for GNMA loans, conditional on rolling delinquency and value at above par. Panel A includes loans originated between June 2002 and November 2002. Panel B includes loans originated between January 2002 and May 2002. Outcomes are observed between the time of origination and July 2007. Buyout is measured within 1 month of rolling delinquency. Foreclosure is measured within 18 months of rolling delinquency. Early cure, modification, and payment change are measured within 3 months of rolling delinquency. Late cure is measured within 12 months of rolling delinquency. Data is collected from Corelogic.

Table 4.2. Effect of Buyout on Foreclosure

	Foreclosure				
	(1)	(2)	(3)	(4)	(5)
Buyout	-0.000 (0.007)	-0.003 (0.007)	-0.003 (0.007)	0.006 (0.007)	0.005 (0.007)
Controls		X	X	X	X
MSA FE			X		X
Month FE				X	X
Obs	11,439	11,236	11,236	11,236	11,236

Note: This table reports OLS regression estimates of the effect of the buyout on foreclosure, conditional on rolling delinquency and value at above par. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. Buyout takes a value of one if a loan experiences a buyout within 1 month after a rolling delinquency. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. Columns 2 includes controls. Column 3 includes fixed effects for MSA. Column 4 includes fixed effects for month of delinquency. Column 5 includes fixed effects for MSA and month of delinquency. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between January 2002 and May 2002 and outcomes observed between the time of origination and July 2007.

Table 4.3. Effect of the GNMA Policy Change on Buyout

	Buyout					
	(1)	(2)	(3)	(4)	(5)	(6)
Z_{-5}	0.010 (0.016)		-0.003** (0.002)		0.014 (0.009)	
Z_{-4}	-0.014 (0.022)		-0.003** (0.001)		0.011 (0.007)	
Z_{-3}	-0.042** (0.017)		-0.001 (0.002)		0.011 (0.007)	
Z_{-2}	-0.126*** (0.023)		-0.003** (0.001)		0.013** (0.007)	
Z_{-1}	-0.306*** (0.022)		-0.002 (0.002)		0.011* (0.006)	
$Z_{\tau>-3}$		-0.186*** (0.012)		-0.001 (0.001)		0.001 (0.003)
MSA FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Obs	13,204	13,204	7,109	7,109	3,153	3,153

Note: This table reports OLS regression estimates of the effect of the vintage month on buyout, conditional on rolling delinquency and value at above par. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a buyout within 1 month after a rolling delinquency. The Z_{τ} dummy takes a value of one if loan is originated τ months relative to the policy change. The $Z_{\tau>-3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. The sample in Columns 1 and 2 consists of loans originated in 2002. The sample in Columns 3 and 4 consists of loans originated in 2003. The sample in Columns 5 and 6 consists of loans originated in 2002, but valued at below par. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between January and May of the sample's calendar year and outcomes observed between the time of origination and July 2007.

Table 4.4. Effect of the GNMA Policy Change on Foreclosure

	Foreclosure					
	(1)	(2)	(3)	(4)	(5)	(6)
Z_{-5}	0.018*		0.004		-0.051	
	(0.010)		(0.016)		(0.048)	
Z_{-4}	0.022**		0.009		-0.031	
	(0.010)		(0.019)		(0.044)	
Z_{-3}	0.037***		0.016		0.011	
	(0.011)		(0.015)		(0.038)	
Z_{-2}	0.045***		0.023		-0.029	
	(0.010)		(0.018)		(0.041)	
Z_{-1}	0.057***		0.010		-0.015	
	(0.012)		(0.016)		(0.042)	
$Z_{\tau > -3}$		0.026***		0.008		-0.016
		(0.007)		(0.009)		(0.012)
MSA FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Obs	13,204	13,204	7,109	7,109	3,153	3,153

Note: This table reports OLS regression estimates of the effect of the vintage month on foreclosure, conditional on rolling delinquency and value at above par. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. The Z_{τ} dummy takes a value of one if loan is originated τ months relative to the policy change. The $Z_{\tau > -3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. The sample in Columns 1 and 2 consists of loans originated in 2002. The sample in Columns 3 and 4 consists of loans originated in 2003. The sample in Columns 5 and 6 consists of loans originated in 2002, but valued at below par. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between January and May of the sample's calendar year and outcomes observed between the time of origination and July 2007.

Table 4.5. Effect of the GNMA Policy Change on Early Cure

	Early Cure					
	(1)	(2)	(3)	(4)	(5)	(6)
Z_{-5}	0.011 (0.018)		-0.013 (0.024)		-0.011 (0.106)	
Z_{-4}	-0.013 (0.015)		-0.023 (0.022)		-0.035 (0.086)	
Z_{-3}	-0.058*** (0.017)		-0.041** (0.020)		-0.036 (0.078)	
Z_{-2}	-0.064*** (0.017)		-0.009 (0.018)		-0.034 (0.086)	
Z_{-1}	-0.074*** (0.019)		-0.046** (0.022)		-0.041 (0.084)	
$Z_{\tau > -3}$		-0.046*** (0.010)		-0.001 (0.011)		-0.005 (0.019)
MSA FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Obs	13,204	13,204	7,109	7,109	3,153	3,153

Note: This table reports OLS regression estimates of the effect of the vintage month on cure, conditional on rolling delinquency and value at above par. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a cure within 3 months after a rolling delinquency. The Z_{τ} dummy takes a value of one if loan is originated τ months relative to the policy change. The $Z_{\tau > -3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. The sample in Columns 1 and 2 consists of loans originated in 2002. The sample in Columns 3 and 4 consists of loans originated in 2003. The sample in Columns 5 and 6 consists of loans originated in 2002, but valued at below par. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between January and May of the sample's calendar year and outcomes observed between the time of origination and July 2007.

Table 4.6. Effect of the GNMA Policy Change on Loan Performance: Mechanism

	Late Cure		Modification		Payment Change	
	(1)	(2)	(3)	(4)	(5)	(6)
Z_{-5}	0.001 (0.014)		-0.001 (0.001)		-0.003 (0.005)	
Z_{-4}	-0.008 (0.013)		-0.001 (0.001)		-0.014*** (0.005)	
Z_{-3}	-0.020 (0.014)		-0.001 (0.001)		-0.010** (0.004)	
Z_{-2}	-0.018 (0.015)		-0.000 (0.001)		-0.006 (0.005)	
Z_{-1}	-0.041** (0.017)		-0.001 (0.001)		-0.006 (0.005)	
$Z_{\tau > -3}$		-0.018** (0.008)		0.000 (0.000)		0.002 (0.003)
MSA FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Obs	13,204	13,204	13,204	13,204	13,204	13,204

Note: This table reports OLS regression estimates of the effect of the vintage month on loan outcomes, conditional on rolling delinquency and value at above par. Observations are at the loan level. In Columns 1 and 2, the outcome variable takes a value of one if a loan experiences a cure within 12 months after a rolling delinquency. In Columns 3 and 4, the outcome variable takes a value of one if a loan experiences an interest rate reduction, principal increase, or principal reduction within 3 months after a rolling delinquency. In Columns 4 and 5, the outcome variable takes a value of one if a loan experiences a payment change within 3 months after a rolling delinquency. The Z_{τ} dummy takes a value of one if loan is originated τ months relative to the policy change. The $Z_{\tau > -3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007.

Table 4.7. Effect of the GNMA Policy Change on Foreclosures: All Agency Loans

	Foreclosure			
	(1)	(2)	(3)	(4)
Z_{-5}	0.020*			
	(0.011)			
Z_{-4}	0.003			
	(0.011)			
Z_{-3}	0.016			
	(0.011)			
Z_{-2}	0.016			
	(0.012)			
Z_{-1}	0.022**			
	(0.010)			
$Z_{-5} \times \text{GNMA}$			0.001	
			(0.015)	
$Z_{-4} \times \text{GNMA}$			0.025*	
			(0.014)	
$Z_{-3} \times \text{GNMA}$			0.026**	
			(0.013)	
$Z_{-2} \times \text{GNMA}$			0.037***	
			(0.014)	
$Z_{-1} \times \text{GNMA}$			0.042***	
			(0.013)	
$Z_{\tau > -3}$		0.008		
		(0.007)		
$Z_{\tau > -3} \times \text{GNMA}$				0.027***
				(0.008)
MSA FE	X	X	X	X
Month FE	X	X	X	X
Vintage FE			X	X
GNMA FE			X	X
Obs	14,463	14,463	27,667	27,667

Note: This table reports OLS regression estimates of the effect of the vintage month on foreclosure, conditional on rolling delinquency and value at above par. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. All columns include fixed effects for MSA and month of delinquency. Columns 3 and 4 include fixed effects for vintage month and GNMA status. The sample in Columns 1 and 2 consists of loans associated with the GSE's. The sample in Columns 3 and 4 consists of all agency loans. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007.

Table 4.8. Effect of the GNMA Policy Change on Early Cure: All Agency Loans

	Early Cure			
	(1)	(2)	(3)	(4)
Z_{-5}	-0.001 (0.015)			
Z_{-4}	-0.012 (0.014)			
Z_{-3}	-0.007 (0.014)			
Z_{-2}	-0.014 (0.014)			
Z_{-1}	-0.008 (0.017)			
$Z_{-5} \times \text{GNMA}$			0.017 (0.021)	
$Z_{-4} \times \text{GNMA}$			0.013 (0.017)	
$Z_{-3} \times \text{GNMA}$			-0.027 (0.020)	
$Z_{-2} \times \text{GNMA}$			-0.021 (0.020)	
$Z_{-1} \times \text{GNMA}$			-0.033 (0.021)	
$Z_{\tau > -3}$		-0.005 (0.010)		
$Z_{\tau > -3} \times \text{GNMA}$				-0.028** (0.013)
MSA FE	X	X	X	X
Month FE	X	X	X	X
Vintage FE			X	X
GNMA FE			X	X
Obs	14,463	14,463	27,667	27,667

Note: This table reports OLS regression estimates of the effect of the vintage month on cure, conditional on rolling delinquency and value at above par. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a cure within 3 months after a rolling delinquency. All columns include fixed effects for MSA and month of delinquency. Columns 3 and 4 include fixed effects for vintage month and GNMA status. The sample in Columns 1 and 2 consists of loans associated with the GSE's. The sample in Columns 3 and 4 consists of all agency loans. Standard errors are reported in parentheses and are clustered by MSA. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007.

Table 4.9. Effect of the GNMA Policy Change on Loan Performance: Instrumental Variables

	Loan Performance			
	(1)	(2)	(3)	(4)
<i>Panel A: Foreclosure</i>				
Buyout	-0.124*** (0.034)	-0.140*** (0.040)	-0.116*** (0.035)	-0.125*** (0.041)
Sargan-Hansen Test (p-value)	0.075	-	0.331	-
<i>Panel B: Early Cure</i>				
Buyout	0.215*** (0.046)	0.248*** (0.057)	0.132** (0.053)	0.130** (0.062)
Sargan-Hansen Test (p-value)	0.001	-	0.404	-
Excluded Instrument	Z_τ	$Z_{\tau > -3}$	$Z_\tau \times \text{GNMA}$	$Z_{\tau > -3} \times \text{GNMA}$
Cragg-Donald Statistic	106	355	168	633
Kleibergen-Paap Statistic	116	232	167	406
MSA FE	X	X	X	X
Month FE	X	X	X	X
Vintage FE			X	X
GNMA FE			X	X
Obs	13,204	13,204	27,667	27,667

Note: This table reports instrumental variables (two-stage least-squares) estimates of regressions of foreclosure and cure preceding the GNMA policy announcement. Observations are at the loan level. The outcome variable in Panel A takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. The outcome variable in Panel B takes a value of one if a loan experiences a cure within 3 months after a rolling delinquency. Buyout is instrumented with Z_τ , $Z_{\tau > -3}$, $Z_\tau \times \text{GNMA}$, and $Z_{\tau > -3} \times \text{GNMA}$ in Columns 1 to 4, respectively. The Z_τ dummy takes a value of one if loan is originated τ months relative to the policy change. The $Z_{\tau > -3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. The GNMA dummy takes a value of one if a loan is associated with GNMA. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. Columns 3 and 4 include fixed effects for vintage month and GNMA status. Standard errors are reported in parentheses and are clustered by MSA. The sample in Columns 1 and 2 consists of loans associated with GNMA. The sample in Columns 3 and 4 consists of all agency loans. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007.

Table 4.10. Effect of the GNMA Policy Change on Loan Performance: Sample Splits

	Foreclosure			
	Low Profit		High Profit	
	Low Risk	High Risk	Low Risk	High Risk
	(1)	(2)	(3)	(4)
Buyout	-0.018 (0.070)	-0.190*** (0.068)	-0.275* (0.159)	-0.122 (0.099)
MSA FE	X	X	X	X
Month FE	X	X	X	X
Obs	3,647	4,703	2,460	2,394

Note: This table reports instrumental variables (two-stage least-squares) estimates of regressions of foreclosure and cure preceding the GNMA policy announcement. Observations are at the loan level. The outcome variable takes a value of one if a loan experiences a foreclosure within 18 months after a rolling delinquency. Buyout is instrumented with $Z_{\tau > -3}$ from Column 2 in Table 4.4. The $Z_{\tau > -3}$ dummy takes a value of one if loan is originated within the two-month period prior to the policy change. High and low profit is defined by the rate spread on the loan. High and low risk is defined by any prior delinquency in the past year. Loan level controls include origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for inferred prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, and single-family status. Annual county level controls at the time of delinquency include the log of the house price index, GDP, population, and income. Monthly county level controls at the time of delinquency include the unemployment rate and the log of labor force. All columns include fixed effects for MSA and month of delinquency. Standard errors are reported in parentheses and are clustered by MSA. The sample in Columns 1 and 2 consists of loans associated with GNMA. The sample in Columns 3 and 4 consists of all agency loans. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers all loans originated between June 2002 and November 2002 and outcomes observed between the time of origination and July 2007.

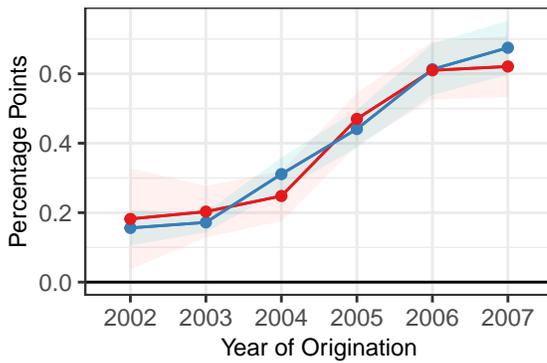
5

Conclusion

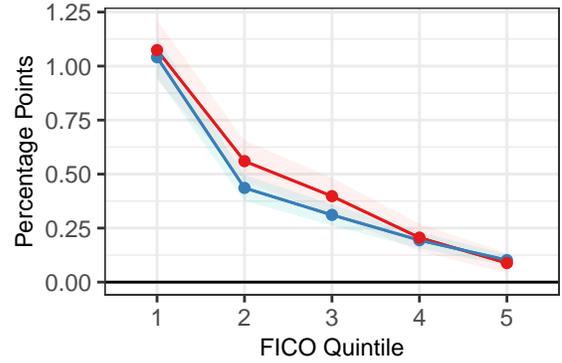
This dissertation explores how the presence of a robust secondary market interacts with collateral enforcement. I find that loan markets facilitate debt sale in lieu of renegotiation when collateral enforcement becomes costly. When future enforcement becomes uncertain, lenders reduce their supply of credit, but only for balance sheet loans. Finally, when debt sale becomes delayed, the profit motive to cure loans is suspended and foreclosures increase. Taken together, these results demonstrate the important role played by the secondary market in shaping loan outcomes. Accounting for this intimate relationship in models of creditor behavior should provide rich insight into lending market dynamics.

Appendix A

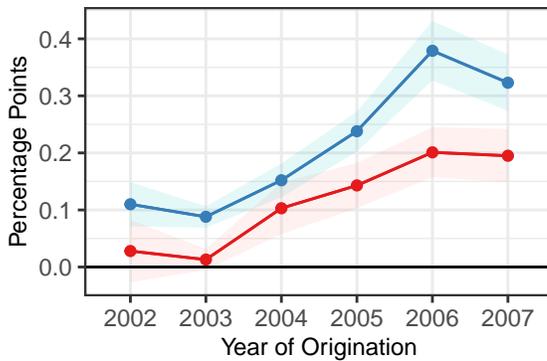
Appendix



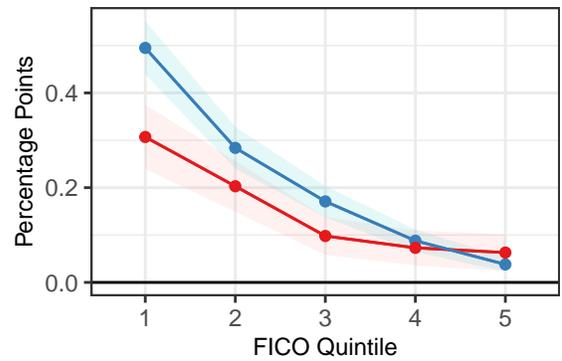
Panel A: Pre-Period and Across Vintage



Panel B: Pre-Period and Across FICO



Panel C: Post-Period and Across Vintage

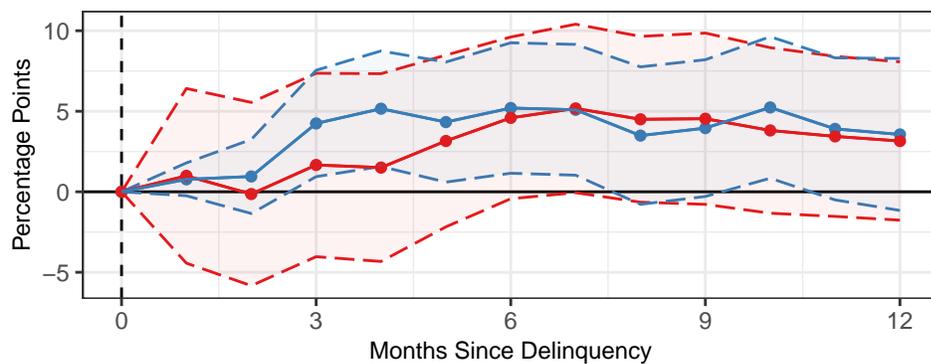


Panel D: Post-Period and Across FICO

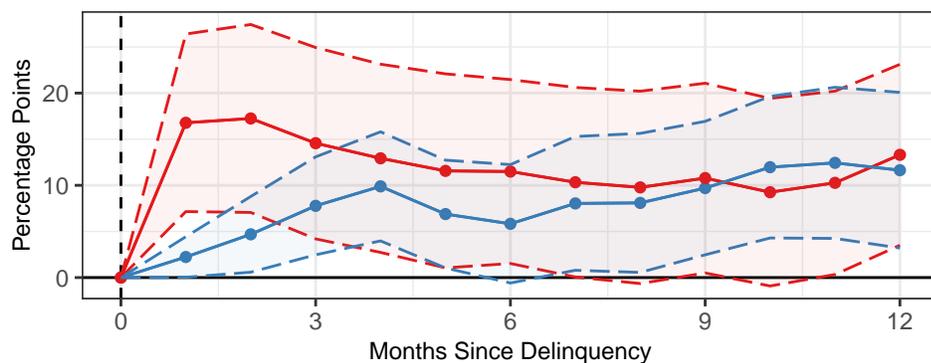
● MERS ● Non-MERS

FIGURE A.1. Heterogeneity in the Empirical Foreclosure Rate

Note: This figure plots heterogeneity in the empirical foreclosure rate of MERS and non-MERS loans before and after the Greenleaf judgment. Loans are originated between January 2002 and December 2007. Foreclosure is observed between April 2013 and June 2016. Panel A plots the pre-period foreclosure rate across year of origination. Panel B plots the pre-period foreclosure rate across FICO credit score quintile. Panel C plots the post-period foreclosure rate across year of origination. Panel D plots the post-period foreclosure rate across FICO credit score quintile. Pre- and post-periods are defined as before and after the Greenleaf judgment, respectively. Each dot represents the average foreclosure rate. The light shaded region represents 95% confidence intervals calculated using standard errors. Data is collected from the Maine Registry of Deeds and CoreLogic.



Panel A: Low Foreclosure Cost Loans



Panel B: High Foreclosure Cost Loans

—●— Any Cure —●— Missing Foreclosure

FIGURE A.2. Impulse Response Following Initial 60-day delinquency: Any Cure and Foreclosure

Note: This figure reports difference-in-difference estimates of the effect of the Greenleaf judgment on the probability of a credit event over multiple time horizons after an initial 60-day delinquency. Results are derived from an estimation of Equation (2.5), separately estimated at each time horizon. For a particular horizon, the outcome takes a value of one in the period that it was last recorded experiencing the outcome, upon which it drops out of the sample. Outcomes of interest include cure (30-days delinquent or current) and foreclosure. Foreclosure estimates are multiplied by negative one to interpret results as the counter-factual foreclosure rate. High Cost loans are those MERS loans whose lender filed for bankruptcy prior to July 2014. Low Cost loans are all other MERS loans. The shaded region represents 90% confidence intervals calculated using zip code-clustered standard errors. Data is collected from the Maine Registry of Deeds and CoreLogic.

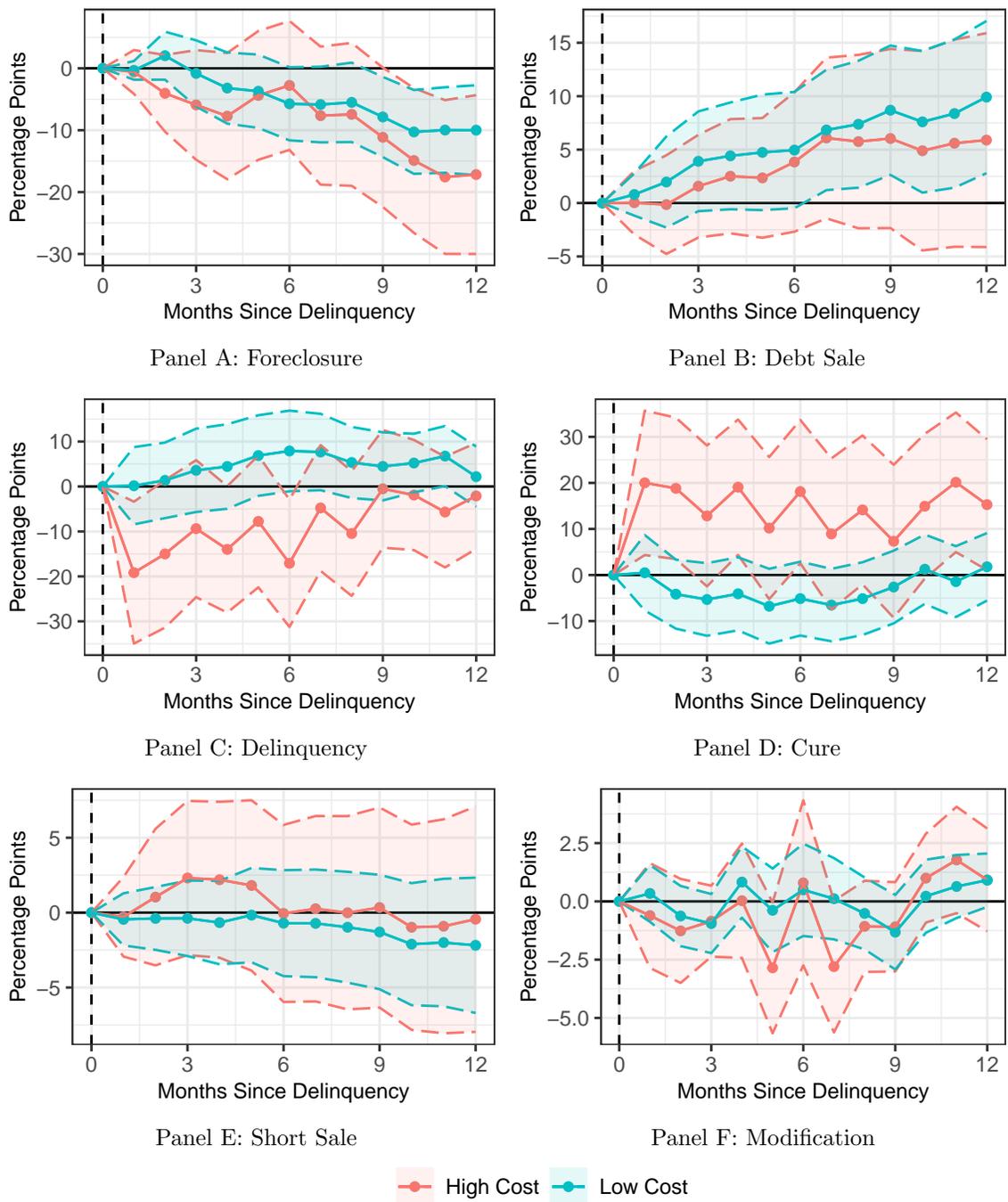


FIGURE A.3. Heterogeneous Impulse Response Following Initial 60-day delinquency: Triple-Difference

Note: This figure reports triple-difference estimates of the effect of the Greenleaf judgment on the probability of a credit event over multiple time horizons after an initial 60-day delinquency. Results are derived from an estimation of Equation (2.7), separately estimated at each time horizon.

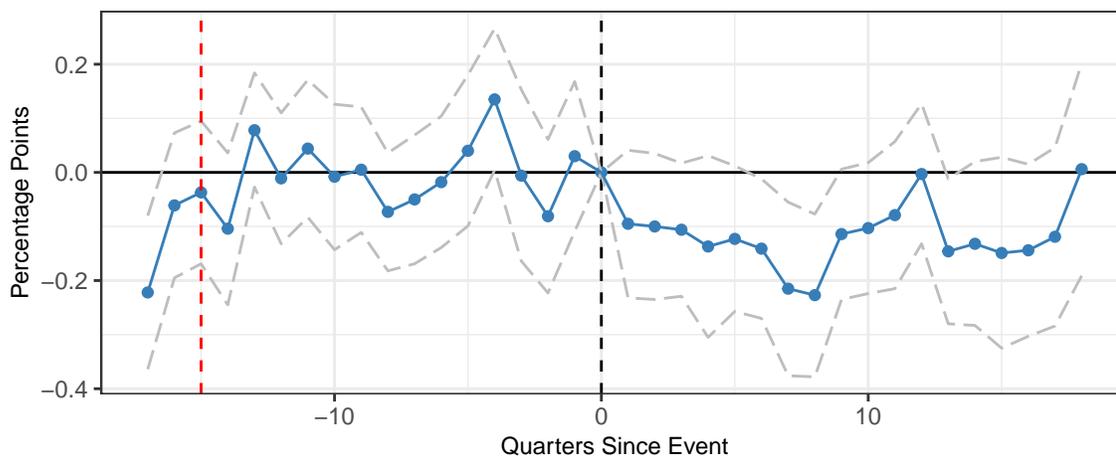
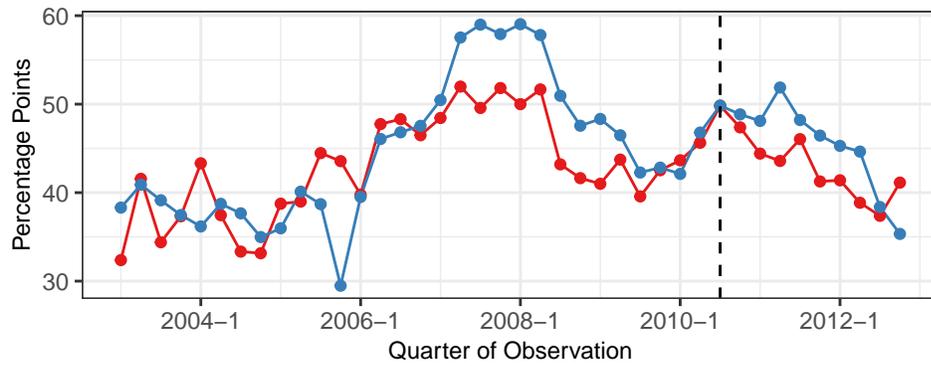
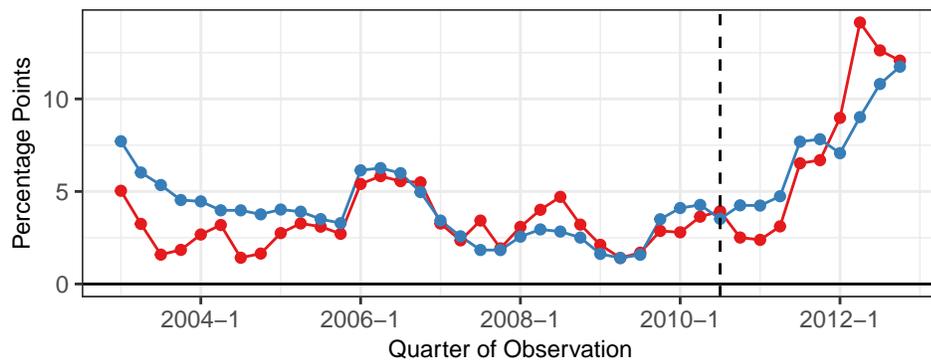


FIGURE A.4. Difference-in-Differences Estimates: Wider Bandwidth

Note: This figure reports difference-in-difference estimates of the effect of the Greenleaf judgment on the probability of foreclosure. The outcome variable takes a value of one if a loan experiences foreclosure from a previous delinquency status of 60 days or worse, upon which the loan drops out of the sample. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Estimates are derived from a difference-in-differences regression that interacts MERS and quarter fixed effects. The coefficient for the baseline quarter preceding the judgment is normalized to zero. The black dashed vertical line indicates the date of the baseline quarter. The red dashed vertical line indicates the date of the Saunders judgment. The light dashed line represents 95% confidence intervals calculated using zip code-clustered standard errors. Outcomes are observed between January 2010 and December 2018 for loans originated between 2002 and 2007 in Maine. Data is collected from the Maine Registry of Deeds and CoreLogic.



Panel A: Foreclosure Rate



I — Maine — All Loans

FIGURE A.5. Foreclosure and Debt Sale Rate: External Validity

Note: This figure plots the empirical foreclosure rate and debt sale rate for all loans originated across the United States after entering 60-day delinquency. Loans are originated between January 2002 and December 2007. Loan outcomes are observed 12 months following an initial 60-day delinquency between January 2003 and December 2012. Panel A plots the foreclosure rate. Panel B plots the debt sale rate. Each dot represents a quarterly rate of transition into a loan outcome of interest. The black dashed vertical line indicates the quarter prior to the 2010 robo-signing scandal. Data is collected from CoreLogic and covers the entire United States.

Table A.1. Effect of Greenleaf Judgment on Delinquency: Difference-in-Differences

	Delinquency			
	(1)	(2)	(3)	(4)
MERS	-0.051*	-0.041	-0.112***	-0.061*
	(0.029)	(0.031)	(0.025)	(0.034)
MERS×Post	0.006	0.007	0.041	-0.005
	(0.035)	(0.038)	(0.032)	(0.044)
MERS×Investor×Post		-0.005		
		(0.083)		
MERS×Past Delinq.×Post			-0.187	
			(0.187)	
MERS×Neg. Equity×Post				-0.145
				(0.501)
MERS×Neg. Inc.×Post				-0.004
				(0.076)
MERS×Neg. Equity×Neg. Inc.×Post				0.373
				(0.762)
Vintage-Time FE	X	X	X	X
Zip-Time FE	X	X	X	X
Obs	592,033	592,033	592,033	592,033

Note: This table reports regression estimates of the effect of the Greenleaf judgment on the monthly probability of delinquency. Observations are at the loan-month level. The outcome variable takes a value of one when a loan becomes 90-days delinquent from 60-days delinquent, upon which it drops out of the sample. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Columns 1 to 4 report estimates from regressions that interact MERS and Post dummies with indicators for strategic default. The Post dummy takes a value of one if a loan is observed on or after the month of the judgment (July 2014). The Investor dummy takes a value of one if the loan was originated for a second home. The Past Delinquency dummy takes a value of one if the borrower experienced a delinquency in the year prior to the start of the sample. The Negative Equity dummy takes a value of one if the current loan-to-value ratio is above 100%. The Negative Income dummy takes a value of one if the loan's county level income growth is negative. Loan level controls include the origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, single-family status, and log of the previous period's balance. Annual county level controls at the time of event include the log of the house price index, GDP, population, and income, as well as the year-end unemployment rate. All columns include fixed effects for the quarter of observation interacted with quarter of origination and the zip code, separately. Standard errors are reported in parentheses and are clustered at the zip code level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of April 2013 to June 2016 for loans originated between 2002 and 2007 in Maine.

Table A.2. Loan Outcomes After Initial 60-Day Delinquency

	All Loans		MERS		Non-MERS	
	Mean	SD	Mean	SD	Mean	SD
<i>Panel A: 3-Month Horizon</i>						
Foreclosure	14.38	35.10	15.55	36.26	13.71	34.41
Debt Sale	4.60	20.95	5.07	21.95	4.33	20.36
Delinquency	45.94	49.85	45.28	49.81	46.33	49.88
Cure	32.78	46.95	31.11	46.32	33.73	47.29
Short Sale	1.88	13.59	2.19	14.64	1.71	12.95
Modification	0.25	5.00	0.12	3.39	0.33	5.72
<i>Panel B: 6-Month Horizon</i>						
Foreclosure	25.52	43.61	25.84	43.80	25.35	43.52
Debt Sale	8.39	27.74	8.57	28.01	8.29	27.59
Delinquency	28.50	45.15	28.38	45.11	28.57	45.19
Cure	33.97	47.37	32.80	46.98	34.64	47.60
Short Sale	3.12	17.40	3.48	18.34	2.92	16.84
Modification	0.78	8.80	0.94	9.64	0.69	8.29
<i>Panel C: 9-Month Horizon</i>						
Foreclosure	35.57	47.89	35.49	47.89	35.63	47.91
Debt Sale	13.21	33.87	11.83	32.32	14.04	34.76
Delinquency	17.79	38.25	17.03	37.62	18.24	38.64
Cure	28.44	45.12	29.81	45.78	27.60	44.72
Short Sale	4.34	20.39	4.73	21.25	4.11	19.85
Modification	0.24	4.87	0.32	5.61	0.19	4.37
<i>Panel D: 12-Month Horizon</i>						
Foreclosure	45.57	49.82	45.65	49.86	45.52	49.83
Debt Sale	18.32	38.70	17.19	37.77	19.00	39.25
Delinquency	10.50	30.67	10.47	30.65	10.51	30.69
Cure	18.62	38.94	18.58	38.93	18.64	38.96
Short Sale	6.11	23.95	6.72	25.06	5.73	23.26
Modification	0.37	6.09	0.00	0.00	0.60	7.71

Note: This table reports mean and standard deviation values for loan performance at various time horizons since an initial 60-day delinquency. Loans are conventional mortgages observable between April 2013 and June 2017, originated between January 2002 and December 2007, for which CoreLogic reports an interest rate below 20 percentage points, an LTV below 150 percentage points, and a loan balance below \$2 million. MERS status is identified using the Maine Registry of Deeds. Panel A reports values for a 3-month horizon. Panel B reports values for a 6-month horizon. Panel C reports values for a 9-month horizon. Panel D reports values for a 12-month horizon. Outcomes corresponds to a distinct credit event, namely foreclosure, debt sale (servicing sold), delinquency (60- or 90-days delinquent), cure (30-days delinquent or current), short sale (zero balance), and modification. Foreclosure, debt sale, and short sale serve as absorbing states. The outcome variable is measured in the pre-period.

Table A.3. Effect of Greenleaf Judgment Across Jurisdiction: Triple-Difference

	Fce.	Debt Sale	Delinq.	Cure	Short Sale	Mod.
	(1)	(2)	(3)	(4)	(5)	(6)
MERS	-0.018 (0.017)	0.018* (0.011)	-0.060** (0.025)	-0.035* (0.019)	-0.019** (0.009)	0.009 (0.012)
MERS×Maine	0.043 (0.029)	-0.037** (0.016)	0.022 (0.037)	-0.030 (0.034)	0.003 (0.013)	-0.018 (0.018)
MERS×Post	-0.030 (0.020)	0.014 (0.012)	0.059* (0.032)	0.022 (0.025)	0.009 (0.011)	-0.011 (0.015)
MERS×Maine×Post	-0.081** (0.032)	0.035* (0.019)	-0.053 (0.047)	0.017 (0.041)	-0.006 (0.015)	-0.002 (0.022)
Vintage-Time FE	X	X	X	X	X	X
Zip-Time FE	X	X	X	X	X	X
Obs	1,786,518	1,786,518	1,479,360	1,697,529	1,786,518	1,753,695

This table reports triple-difference estimates of the effect of the Greenleaf judgment on the monthly probability of loan outcomes. Observations are at the loan-month level. The outcome variable takes a value of one if a loan experiences a particular credit event while holding a delinquency status of 60 days or worse, upon which it drops out of the sample. Each column defines a distinct credit event, namely foreclosure, debt sale (servicing sold), delinquency (90-days delinquent from 60-days delinquent), cure (30-days delinquent or current from 60-days delinquent), short sale (zero balance), and modification, respectively. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Columns 1 to 6 report estimates from triple-difference regressions that control for MERS and non-MERS trends across both New Hampshire and Maine. Regressions interact MERS and Post dummies with a Maine dummy. The Maine dummy takes a value of one if the loan is originated in the state of Maine, which is the state affected by the judgment. The Post dummy takes a value of one if a loan is observed on or after the month of the judgment (July 2014). Loan level controls include the origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, single-family status, and log of the previous period's balance. Annual county level controls at the time of event include the log of the house price index, GDP, population, and income, as well as the year-end unemployment rate. All columns include fixed effects for the quarter of observation interacted with quarter of origination and the zip code, separately. Standard errors are reported in parentheses and are clustered at the zip code level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of April 2013 to June 2016 for loans originated between 2002 and 2007 in Maine and New Hampshire.

Table A.4. Effect of Greenleaf Judgment Across Lender Bankruptcy: Triple-Difference

	Fce.	Debt Sale	Delinq.	Cure	Short Sale	Mod.
	(1)	(2)	(3)	(4)	(5)	(6)
High Cost	-0.003 (0.050)	0.020 (0.035)	-0.082 (0.064)	0.060 (0.057)	0.002 (0.025)	-0.030 (0.029)
Low Cost	-0.020 (0.018)	0.018 (0.011)	-0.057** (0.026)	-0.047** (0.020)	-0.022** (0.010)	0.015 (0.012)
High Cost × Maine	0.176** (0.080)	-0.047 (0.045)	0.250** (0.098)	-0.007 (0.080)	-0.035 (0.030)	0.004 (0.042)
High Cost × Post	-0.059 (0.063)	-0.081** (0.036)	0.072 (0.084)	-0.018 (0.073)	-0.005 (0.030)	0.009 (0.039)
Low Cost × Maine	0.019 (0.029)	-0.035** (0.017)	-0.015 (0.039)	-0.037 (0.036)	0.009 (0.014)	-0.021 (0.019)
Low Cost × Post	-0.026 (0.020)	0.028** (0.013)	0.056* (0.034)	0.028 (0.026)	0.011 (0.011)	-0.014 (0.016)
High Cost × Maine × Post	-0.199** (0.094)	0.056 (0.049)	-0.354*** (0.123)	0.051 (0.109)	0.011 (0.037)	-0.032 (0.055)
Low Cost × Maine × Post	-0.059* (0.031)	0.034 (0.021)	-0.002 (0.048)	0.012 (0.041)	-0.008 (0.015)	0.003 (0.023)
Vintage-Time FE	X	X	X	X	X	X
Zip-Time FE	X	X	X	X	X	X
Obs	1,786,518	1,786,518	1,479,360	1,697,529	1,786,518	1,753,695
Pairwise t-test	-1.487	0.409	-2.774***	0.352	0.527	-0.609

This table reports triple-difference estimates of the effect of the Greenleaf judgment on the monthly probability of loan outcomes. Observations are at the loan-month level. The outcome variable takes a value of one if a loan experiences a particular credit event while holding a delinquency status of 60 days or worse, upon which it drops out of the sample. Each column defines a distinct credit event, namely foreclosure, debt sale (servicing sold), delinquency (90-days delinquent from 60-days delinquent), cure (30-days delinquent or current from 60-days delinquent), short sale (zero balance), and modification, respectively. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Columns 1 to 6 report estimates from triple-difference regressions that control for High Cost, Low Cost, and non-MERS trends across both New Hampshire and Maine. Loan level controls include the origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, single-family status, and log of the previous period's balance. Annual county level controls at the time of event include the log of the house price index, GDP, population, and income, as well as the year-end unemployment rate. All columns include fixed effects for the quarter of observation interacted with quarter of origination and the zip code, separately. Standard errors are reported in parentheses and are clustered at the zip code level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of April 2013 to June 2016 for loans originated between 2002 and 2007 in Maine and New Hampshire.

Table A.5. Effect of Greenleaf Judgment Within Lender: Difference-in-Differences

	Fce.	Debt Sale	Delinq.	Fce.	Debt Sale	Delinq.
	(1)	(2)	(3)	(4)	(5)	(6)
Maine×Post	-0.262*** (0.072)	0.047* (0.028)	0.013 (0.072)	-0.206*** (0.045)	0.043* (0.024)	-0.045 (0.066)
Zip FE				X	X	X
Vintage-Time FE				X	X	X
Zip-Lender FE	X	X	X			
Vintage-Time-Lender FE	X	X	X			
Obs	247,573	247,573	205,601	367,882	367,882	309,738

Note: This table reports difference-in-differences estimates of the effect of the Greenleaf judgment on the monthly probability of loan outcomes. Observations are at the loan-month level. The outcome variable takes a value of one if a loan experiences a particular credit event while holding a delinquency status of 60 days or worse, upon which it drops out of the sample. Each column defines a distinct credit event, namely foreclosure, debt sale (servicing sold), and delinquency (90-days delinquent from 60-days delinquent). The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Columns 1 to 6 report estimates from difference-in-differences regressions that interact Maine and Post dummies. The Maine dummy takes a value of one if the loan is originated in the state of Maine, which is the state affected by the judgment. The Post dummy takes a value of one if a loan is observed on or after the month of the judgment (July 2014). Loan level controls include the origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, single-family status, and log of the previous period's balance. Annual county level controls at the time of event include the log of the house price index, GDP, population, and income, as well as the year-end unemployment rate. Columns 1 to 3 include fixed effects for the quarter of observation interacted with quarter of origination and the lender ID, jointly, and the zip code interacted with the lender ID, jointly. Columns 4 to 6 include fixed effects for the quarter of observation interacted with quarter of origination and a separate fixed effect for the zip code. Standard errors are reported in parentheses and, in Columns 1 to 3, clustered at the lender level and, in Columns 4 to 6, clustered at the zip code level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of April 2013 to June 2016 for MERS loans originated between 2005 and 2007 in Maine and New Hampshire.

Table A.6. Effect of Greenleaf Judgment Across Contract Type: Difference-in-Differences

	Fce.	Debt Sale	Delinq.	Cure	Short Sale	Mod.
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.010 (0.025)	-0.015 (0.014)	-0.049 (0.030)	-0.081*** (0.031)	-0.015 (0.010)	-0.010 (0.015)
Placebo	-0.047 (0.053)	-0.064*** (0.018)	-0.074 (0.058)	-0.053 (0.064)	-0.020 (0.019)	-0.025 (0.028)
Treated×Post	-0.102*** (0.027)	0.050*** (0.017)	0.003 (0.036)	0.051 (0.036)	0.002 (0.012)	-0.012 (0.019)
Placebo×Post	0.052 (0.064)	0.054** (0.025)	0.027 (0.074)	-0.021 (0.076)	0.018 (0.028)	-0.010 (0.033)
Vintage-Time FE	X	X	X	X	X	X
Zip-Time FE	X	X	X	X	X	X
Obs	726,526	726,526	592,033	689,291	726,526	713,008
Pairwise t-test	-2.381**	-0.136	-0.305	0.962	-0.491	-0.071

Note: This table reports difference-in-differences estimates of the effect of the Greenleaf judgment on the monthly probability of loan outcomes. Observations are at the loan-month level. The outcome variable takes a value of one if a loan experiences a particular credit event while holding a delinquency status of 60 days or worse, upon which it drops out of the sample. Each column defines a distinct credit event, namely foreclosure, debt sale (servicing sold), delinquency (90-days delinquent from 60-days delinquent), cure (30-days delinquent or current from 60-days delinquent), short sale (zero balance), and modification, respectively. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Columns 1 to 6 report estimates from difference-in-differences regressions that interact the Treated and Placebo dummies with the Post variable. The Treated dummy takes a value of one if a MERS loan had the key sentence that was the subject of the Greenleaf judgment. The Placebo dummy takes a value of one for all other MERS loans. The Post dummy takes a value of one if a loan is observed on or after the month of the judgment (July 2014). Loan level controls include the origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, single-family status, and log of the previous period's balance. Annual county level controls at the time of event include the log of the house price index, GDP, population, and income, as well as the year-end unemployment rate. All columns include fixed effects for the quarter of observation interacted with quarter of origination and the zip code, separately. The pairwise t-statistic of the difference between coefficients is reported in the last row. Standard errors are reported in parentheses and are clustered at the zip code level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of April 2013 to June 2016 for loans originated between 2002 and 2007 in Maine.

Table A.7. Effect of Greenleaf Judgment Across Legislative Period: Difference-in-Differences

	Fce.	Debt Sale	Delinq.	Cure	Short Sale	Mod.
	(1)	(2)	(3)	(4)	(5)	(6)
MERS	0.005 (0.025)	-0.020 (0.013)	-0.051* (0.029)	-0.078** (0.031)	-0.015 (0.010)	-0.011 (0.014)
MERS×Pre-Law	-0.086*** (0.030)	0.049*** (0.018)	-0.020 (0.040)	0.005 (0.040)	0.001 (0.013)	-0.031 (0.021)
MERS×Post-Law	-0.089*** (0.030)	0.052** (0.021)	0.038 (0.046)	0.094** (0.041)	0.008 (0.014)	0.011 (0.022)
Vintage-Time FE	X	X	X	X	X	X
Zip-Time FE	X	X	X	X	X	X
Obs	726,526	726,526	592,033	689,291	726,526	713,008
Pairwise t-test	-0.088	0.112	1.138	2.232**	0.553	1.600

Note: This table reports difference-in-differences estimates of the effect of the Greenleaf judgment on the monthly probability of loan outcomes. Observations are at the loan-month level. The outcome variable takes a value of one if a loan experiences a particular credit event while holding a delinquency status of 60 days or worse, upon which it drops out of the sample. Each column defines a distinct credit event, namely foreclosure, debt sale (servicing sold), delinquency (90-days delinquent from 60-days delinquent), cure (30-days delinquent or current from 60-days delinquent), short sale (zero balance), and modification, respectively. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Columns 1 to 6 report estimates from difference-in-differences regressions that interact the Pre-Law and Post-Law dummies with the MERS variable. The Pre-Law dummy takes a value of one if a loan is observed on or after the month of the judgment (July 2014) but before the legislative action. The Post-Law dummy takes a value of one if a loan is observed on or after the month of the legislative action (July 2015). Loan level controls include the origination interest rate, LTV, and log of the original mortgage balance. Further loan level controls include indicators for prime loan status, term length, low-income or no income documentation status, refinancing status, primary residence status, single-family status, and log of the previous period's balance. Annual county level controls at the time of event include the log of the house price index, GDP, population, and income, as well as the year-end unemployment rate. All columns include fixed effects for the quarter of observation interacted with quarter of origination and the zip code, separately. The pairwise t-statistic of the difference between coefficients is reported in the last row. Standard errors are reported in parentheses and are clustered at the zip code level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of April 2013 to June 2016 for loans originated between 2002 and 2007 in Maine.

Table A.8. Effect of the Greenleaf Judgment on Tract Level Lending: Difference-in-Differences

	All Loans		Portfolio		Non-Portfolio	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat \times Post	-0.053*** (0.010)		-0.076*** (0.016)		-0.021* (0.012)	
Exposure \times Post		-0.031*** (0.007)		-0.051*** (0.009)		-0.012 (0.008)
Tract FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Implied $\% \Delta$	-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.9. 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*** (0.054)		-0.293* (0.153)		-0.115** (0.047)	
Exposure × Post		-0.033** (0.016)		-0.029 (0.043)		-0.014 (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.10. Effect of the Greenleaf Judgment on Non-Portfolio Lending (Maine): Difference-in-Differences

	log(Loan Volume)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat \times Post	0.078 (0.089)	0.135 (0.115)	0.108 (0.113)			
Exposure \times Post				0.010 (0.017)	0.023 (0.024)	0.016 (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 \times Tract FE		X	X		X	X
Tract \times Time FE			X			X
Implied $\% \Delta$	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.11. Effect of the Greenleaf Judgment on Non-Bank Lending: Difference-in-Differences

	log(Loan Volume)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat \times Post	0.144*	0.233**	0.221**			
	(0.081)	(0.104)	(0.111)			
Exposure \times 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 \times Tract FE		X	X		X	X
Tract \times Time FE			X			X
Implied $\% \Delta$	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.12. 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.13. Effect of the Greenleaf Judgment on County Level Delinquency: Difference-in-Differences

	Delinquency Rate	
	(1)	(2)
Treat \times Post	-0.236** (0.092)	
Exposure \times 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.14. 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 (0.030)		0.026 (0.062)		-0.081 (0.050)	
Above Limit × Treat × Post	0.059** (0.024)		-0.034 (0.069)		0.100* (0.057)	
Exposure × Post		-0.015 (0.033)		-0.010 (0.046)		-0.062 (0.059)
Above Limit × Exposure × Post		0.090*** (0.030)		0.027 (0.056)		0.169*** (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.15. 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*** (0.072)		-0.309*** (0.105)	
Exposure × Post		-0.426*** (0.074)		
Near × Exposure × Post		-0.171* (0.095)		-0.231*** (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 %Δ	-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.16. 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 \times Post	0.047** (0.022)		-0.007 (0.013)	
Exposure \times Post		0.028** (0.011)		0.001 (0.008)
Tract FE	X	X	X	X
Time FE	X	X	X	X
Implied $\% \Delta$	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.17. Effect of the Greenleaf Judgment on House Prices and Employment: Difference-in-Differences

	Log(House Price Index)		Unemployment Rate	
	(1)	(2)	(3)	(4)
Treatment \times Post	-0.026*** (0.005)		0.899*** (0.189)	
Exposure \times Post		-0.013*** (0.002)		0.437*** (0.100)
Geography FE	X	X	X	X
Time FE	X	X	X	X
Implied $\% \Delta$	-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.

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Biography

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