Red Tape, Greenleaf: Creditor Behavior Under Costly Collateral Enforcement*

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Abstract

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. I exploit quasi-experimental variation from an increase in foreclosure costs due to Maine's Greenleaf judgment. To foreclose on loans associated with an electronic registry, the judgment required affected creditors to request reassignment of their mortgage from the initial lender. For treated loans, I estimate that foreclosures fell by 26% (0.09 pp) and debt sales increased by 57% (0.05 pp). I find no evidence of an increase in formal modifications on the part of creditors or default on the part of borrowers. In cases where the initial lender filed for bankruptcy, treated loans experienced no increase in sales. Instead, these loans benefited from reduced delinquency in a manner resembling creditor forbearance.

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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 renego-

tiating, 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 refinanced 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

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

[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).

(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 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 describes the institutional details of MERS and the Maine Greenleaf judgment. Section 3 describes the data, sample construction, and summary statistics. Section 4 estimates the effect of the Greenleaf judgment on loan outcomes. Section 5 explores the mechanism driving the effect of the Greenleaf judgment. Section 6 investigates the robustness of my results. Section 7 concludes.

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.

[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 (Fedaseyeu, 2020; Cheng et al., 2020; Romeo and Sandler, 2021; Fonseca, 2022).

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 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 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 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.

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

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 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 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 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 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

[5]David Sherwood, "Maine lawmakers tighten foreclosure rules in win for consumers", Reuters, July 1,

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.

3 Data

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.

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

2015.

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

is originated, the mortgage contract is publicly registered at the county registry of deeds in order to preserve its chain of title. Each additional sale of the mortgage loan requires that the associated securitizing contract be publicly reassigned to the new creditor as well. In the case of a MERS mortgage, within the very originating documents themselves, the original lender immediately assigns the mortgage to MERS. Hence, MERS is listed alongside the original lender in the public registry for each MERS-affiliated loan. For each mortgage contract, I record the book and page number, the borrower's name, the address, the lender, the date of origination, the date of termination, the loan amount, and the MERS ID.

3.3 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]

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

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

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 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 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.

3.6 Summary Statistics

In Figure 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 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 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 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 6 deals with concerns related to selection directly.

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.

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:

$$Y_{it} = \alpha_{zip,t} + \alpha_{vintage,t} + X_{it}'\gamma + \beta MERS_i + \delta MERS_i \times Post_t + \epsilon_{it} \quad (1)$$

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.

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 systemati-

cally 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 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 indicators variables for 4 quarters before and 8 quarters after the baseline quarter preceding the judgment:

$$Y_{it} = \alpha_{zip,t} + \alpha_{vintage,t} + X'_{it}\gamma + \beta MERS_i + \sum_{s=-4}^{8} \delta_s MERS_i \times \{t = s\} + \epsilon_{it} \tag{2}$$

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 6, I deal with any remaining concerns over systematic variation directly by conducting my analysis using New Hampshire loans, lender identifiers, and placebo loans.

4.3 Effect on Foreclosure and Debt Sale

Table 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 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, 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 6 plots estimates of the regression Equation (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. 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.

4.4 Effect on Delinquency

Given the results in Section 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 (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 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 6 plots estimates of the regression Equation (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. 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.

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 3 test the effect of the Greenleaf judgment on alternative loan outcomes. I estimate regression Equation (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 and 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.

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 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 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 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.

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.

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 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 (1):

$$\begin{aligned} Y_{it} = \alpha_{zip,t} + \alpha_{vintage,t} + X_{it}'\gamma + \beta_1 MERS_i + \beta_2 Absent\ Lender_i \\ \beta_3 MERS_i \times Absent\ Lender_i + \beta_4 Absent\ Lender_i \times Post_t \\ +\delta MERS_i \times Post_t + \varphi MERS_i \times Absent\ Lender_i \times Post_t + \epsilon_{it} \end{aligned} \tag{3}$$

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 5 reports estimates of regression Equation (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 hypothesis 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.

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 (1):

$$\begin{aligned} Y_{it} = \alpha_{zip,t} + \alpha_{vintage,t} + X'_{it}\gamma + \beta_1 Low\ Cost_i + \delta_1 Low\ Cost_i \times Post_t \\ + \beta_2 High\ Cost_i + \delta_2 High\ Cost_i \times Post_t + \epsilon_{it} \end{aligned} \tag{4}$$

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 costs MERS loans, respectively, relative to untreated loans.

Table 6 presents the estimates of regression Equation (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.

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 (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:

$$\begin{aligned} Y_{i,t+n} = \alpha_{zip} + \alpha_{vintage} + \alpha_{t+n} + X'_{it+n}\gamma + \beta_1 Low\ Cost_i + \delta_1 Low\ Cost_i \times Post_{t+n} \\ + \beta_2 High\ Cost_i + \delta_2 High\ Cost_i \times Post_{t+n} + \epsilon_{i,t+n} \end{aligned} \tag{5}$$

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 7 plots estimates of regression Equation (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 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 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 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.

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.

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:

$$\begin{aligned} Y_{it} = \alpha_{zip,t} + \alpha_{vintage,t} + X'_{it}\gamma + \beta_1 MERS_i + \beta_2 MERS_i \times Maine_i \\ + \delta MERS_i \times Post_t + \varphi MERS_i \times Maine_i \times Post_t + \epsilon_{it} \end{aligned} \tag{6}$$

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 (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. 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 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:

$$\begin{aligned} Y_{it} = \alpha_{zip,t} + \alpha_{vintage,t} + X_{it}'\gamma + \beta_1 Low\ Cost_i + \beta_2 Low\ Cost_i \times Maine_i \\ + \delta_1 Low\ Cost_i \times Post_t + \varphi_1 Low\ Cost_i \times Maine_i \times Post_t \\ + \beta_3 High\ Cost_i + \beta_4 High\ Cost_i \times Maine_i \\ + \delta_2 High\ Cost_i \times Post_t + \varphi_2 High\ Cost_i \times Maine_i \times Post_t + \epsilon_{it} \end{aligned} \tag{7}$$

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 (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 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 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.

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 interstate 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, 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:

$$Y_{it} = \alpha_{lender,zip} + \alpha_{lender,vintage,t} + X'_{it}\gamma + \delta Maine_i \times Post_t + \epsilon_{it} \tag{8}$$

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 (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 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.

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 (1):

$$\begin{aligned} Y_{it} = \alpha_{zip,t} + \alpha_{vintage,t} + X'_{it}\gamma + \beta_1 Treated_i + \delta_1 Treated_i \times Post_t \\ + \beta_2 Placebo_i + \delta_2 Placebo_i \times Post_t + \epsilon_{it} \end{aligned} \tag{9}$$

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 qualitatively similar to those in Table 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 6.2, given that the differential effect of the Greenleaf judgment

[8]Kevin Hudspeth, "MERS's 'Maine' Purpose: Recognizing Key Differences Between MERS Mortgages", American Bar Association, April 18, 2020.

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.

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:

$$\begin{aligned} Y_{it} = \alpha_{zip,t} + \alpha_{vintage,t} + X_{it}'\gamma + \beta MERS_i + \delta_1 MERS_i \times \textit{Pre-Law}_t \\ + \delta_2 MERS_i \times \textit{Post-Law}_t + \epsilon_{it} \end{aligned} \tag{10}$$

In the equation above, the $\textit{Pre-Law}_t$ variable takes a value of one if a loan is observed following the Greenleaf judgment but prior to the law change. The $\textit{Post-Law}_t$ 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 $\textit{Pre-Law}_t$ and $\textit{Post-Law}_t$ 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 (10) using outcomes of foreclosure and debt sale, respectively. The estimated coefficients on the $MERS_i \times \textit{Pre-Law}_t$ interaction term and $MERS_i \times \textit{Post-Law}_t$ interaction are statistically pairwise equivalent

and individually significant at the 1% level. These estimates are also qualitatively similar to those in Table 2 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 5.3. Second, the forbearance identified in Section 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.

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) 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 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.

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 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.

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.

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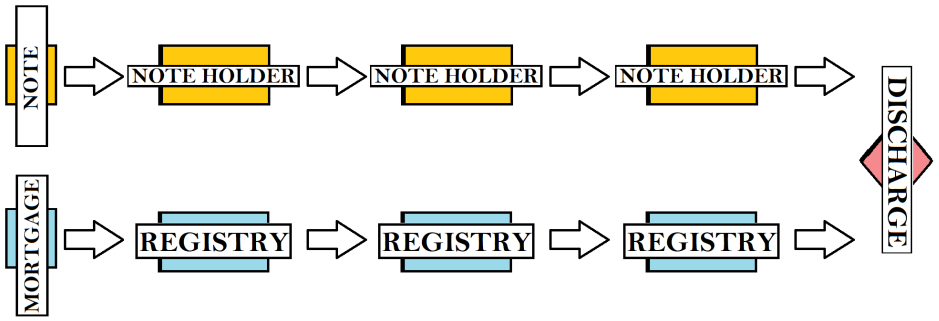
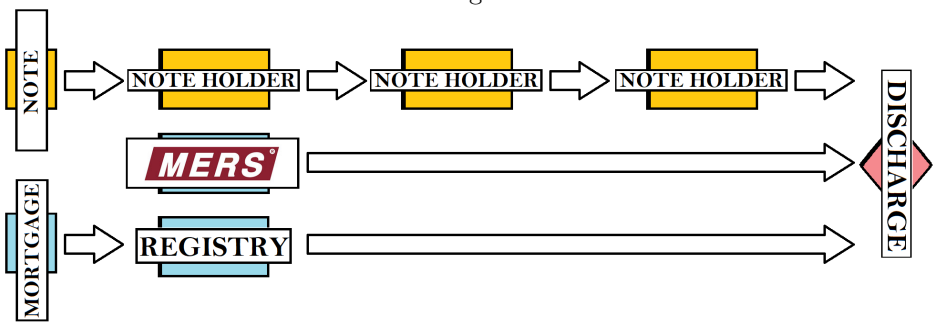
Figure 1. Process of Selling a Mortgage Loan

Panel A: Selling Without MERS



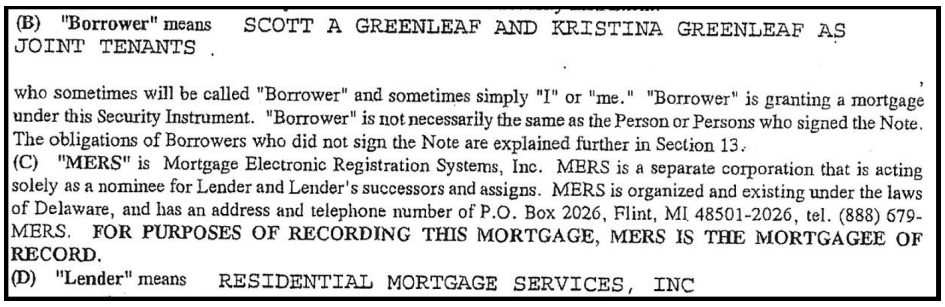
Panel B: Selling With MERS



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.

Figure 2. Variation in MERS Mortgage Contracts

Panel A: Maine MERS Contract



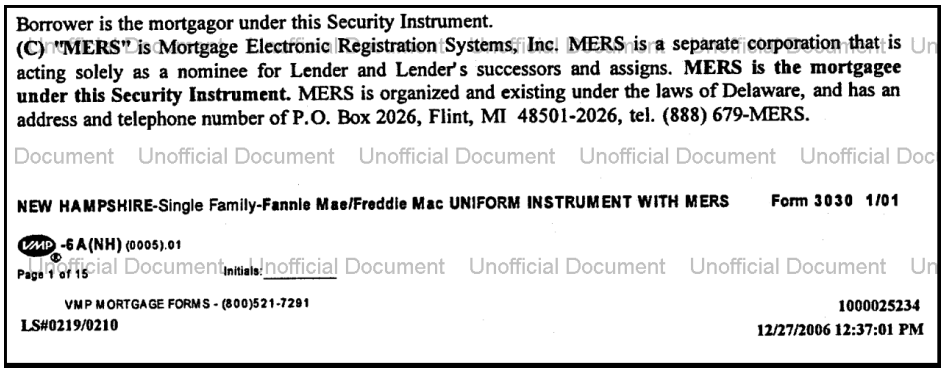
(B) "Borrower" means SCOTT A GREENLEAF AND KRISTINA GREENLEAF AS JOINT TENANTS .

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: New Hampshire 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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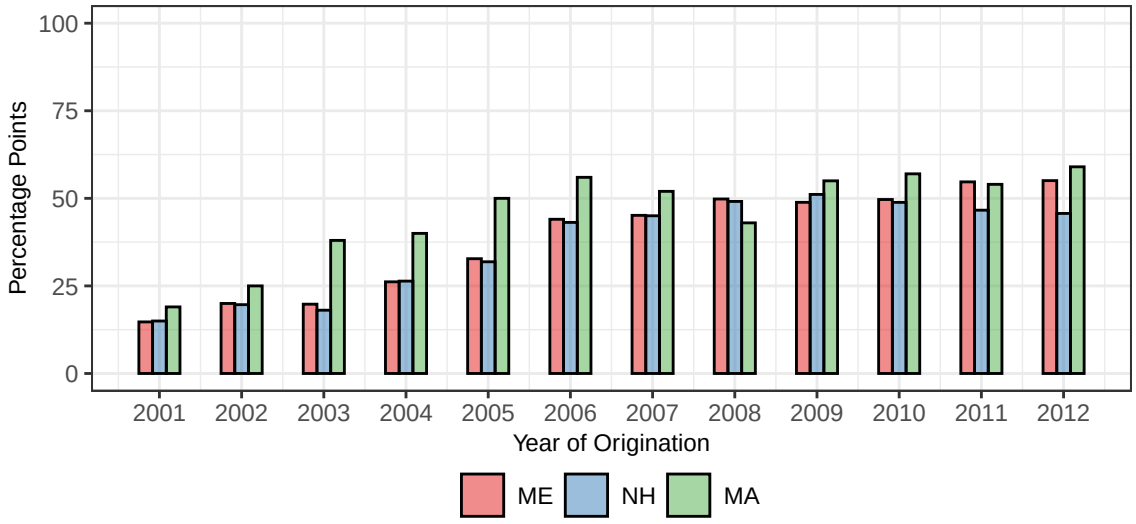
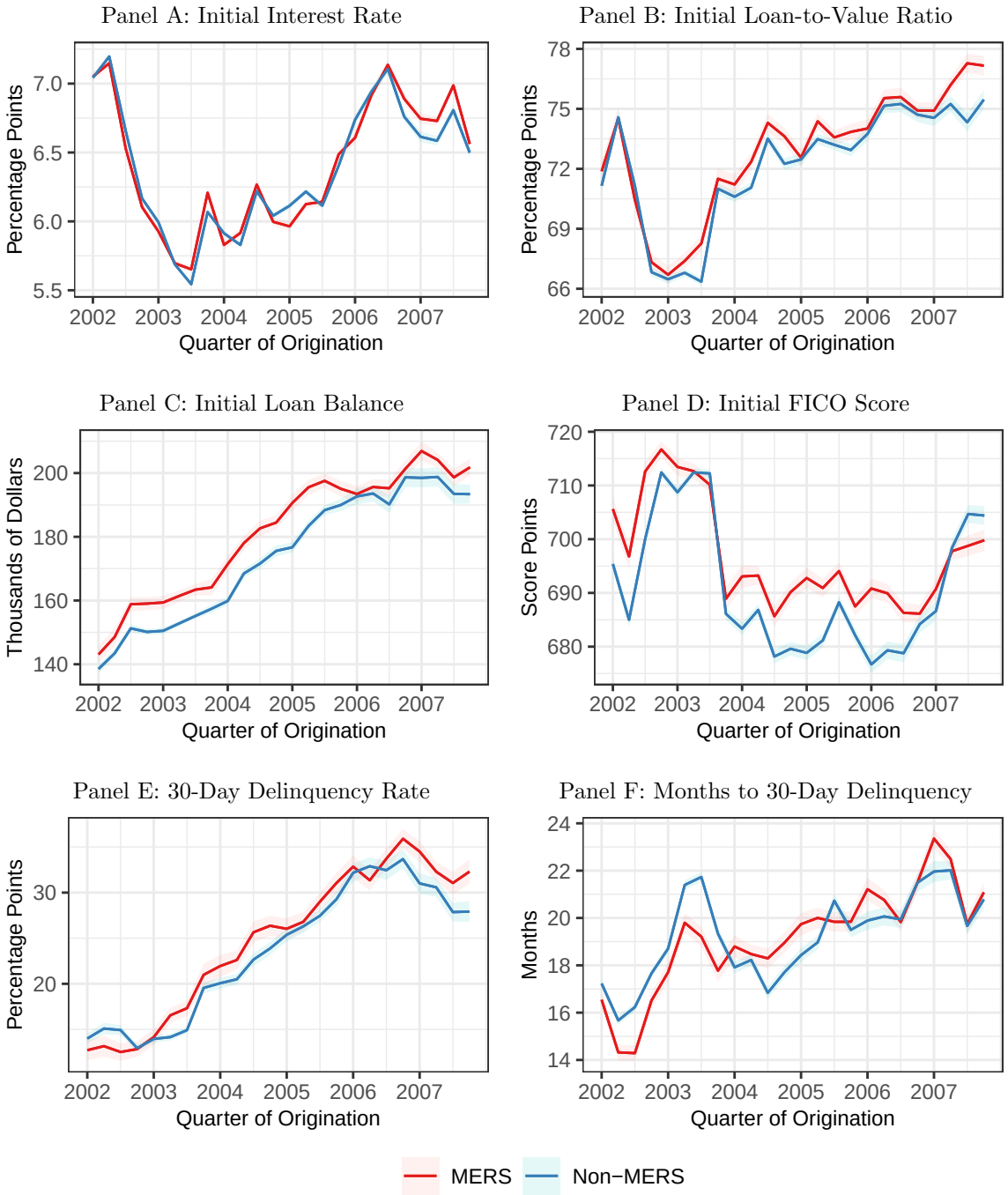
Note: This figure presents excerpts from MERS mortgage contracts across Maine and New Hampshire. MERS mortgage contracts are standardized within a given state and each excerpt identifies the portion of the state-specific contract that designates MERS as the note holder's nominee. Panel A presents an 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 3. MERS Loans as a Percent of All Originations



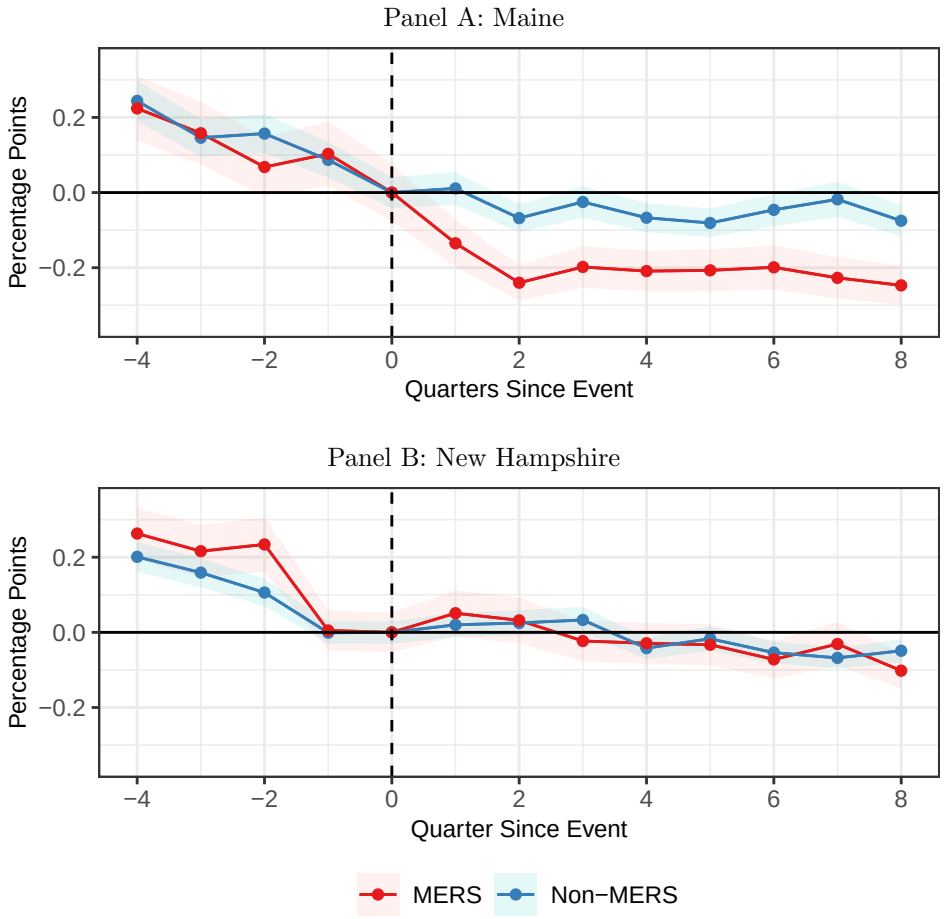
Note: This figure plots the time series of mortgages that are registered with MERS as a fraction of all mortgages originated in a given year between January 2001 and December 2012 using data from the Maine Registry of Deeds, the New Hampshire Registry of Deeds, CoreLogic, and Table 1 of Lewellen and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

Figure 4. Loan Characteristics Across Origination Quarter and MERS Status



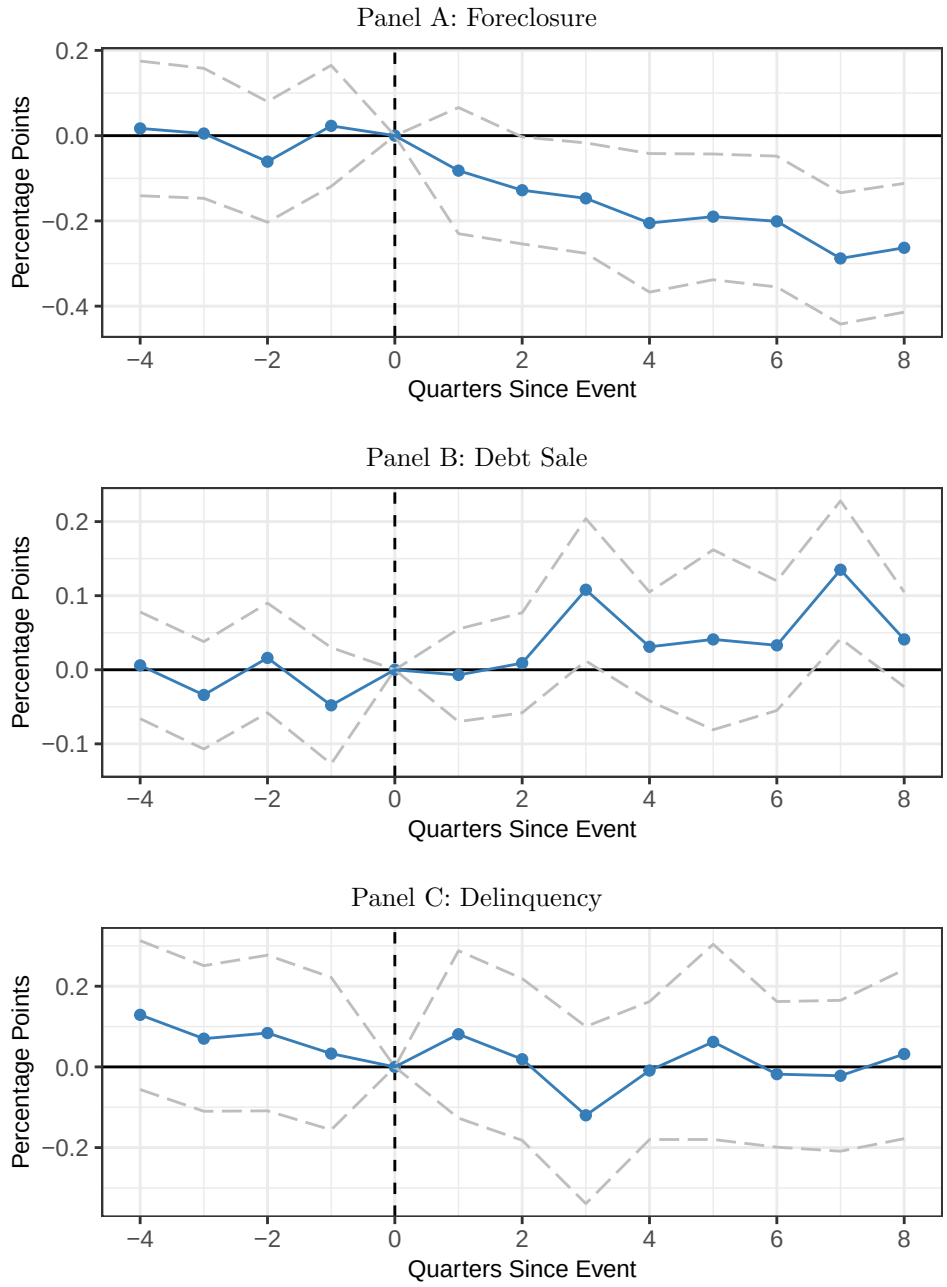
Note: This figure plots loan characteristics of MERS and non-MERS loans across quarter of origination. Loans are originated between January 2002 and December 2007. Panel A plots the initial interest rate. Panel B plots the initial (uncombined) loan-to-value ratio. Panel C plots the initial FICO credit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

Figure 5. Empirical Foreclosure Rate Relative to the Baseline Quarter



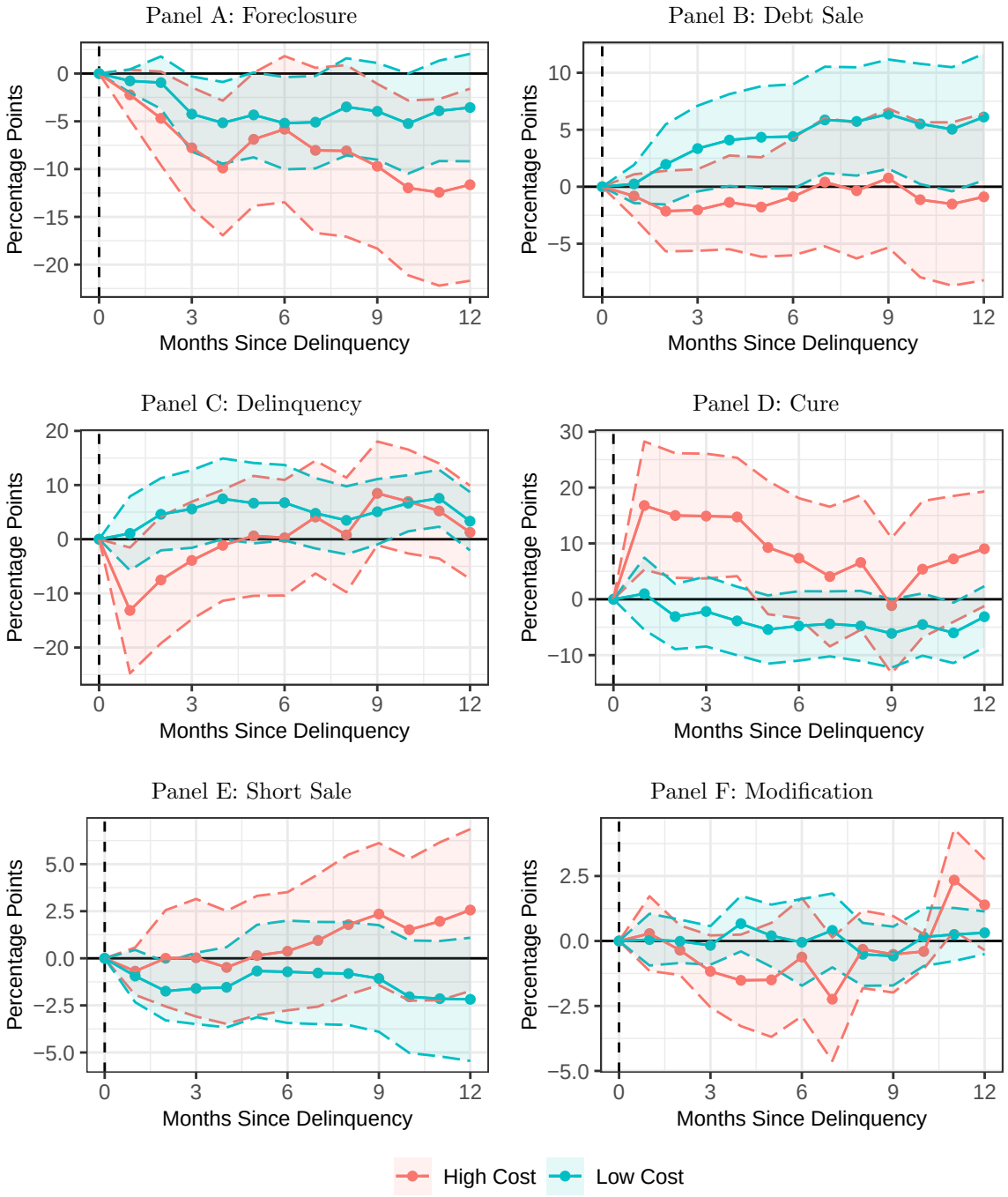
Note: This figure plots the empirical foreclosure rate of MERS and non-MERS loans across quarters since the Greenleaf judgment. Loans are originated between January 2002 and December 2007. Foreclosure is observed between April 2013 and June 2016. Panel A plots the foreclosure rate for Maine. Panel B 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.

Figure 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. 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 panel corresponds to a distinct outcome, namely foreclosure, debt sale (servicing sold), and delinquency (90-days delinquent from 60-days delinquent), respectively. 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 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 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 (5), separately estimated at each time horizon. For a particular time horizon, the outcome variable takes a value of one in the period that it was last recorded as experiencing a particular credit event. Each panel 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, respectively. The shaded region represents 95% confidence intervals calculated using zip code-clustered standard errors. Data is collected from the Maine Registry of Deeds and CoreLogic.

Table 1. Summary Statistics

	All Loans		MERS		Non-MERS	
	Mean	SD	Mean	SD	Mean	SD
Panel A: Loan Characteristics						
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
Panel B: Loan Performance: All Loans						
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. Effect of Greenleaf Judgment on Foreclosure and Sale: Difference-in-Differences

	Foreclosure			Debt Sale		
	(1)	(2)	(3)	(4)	(5)	(6)
MERS	0.005	-0.006		-0.020	-0.026	
	(0.025)	(0.030)		(0.013)	(0.016)	
MERS×Post	-0.087***	-0.105***	-0.171***	0.050***	0.067***	0.056***
	(0.027)	(0.033)	(0.030)	(0.016)	(0.020)	(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 3. Effect of Greenleaf Judgment on Alternative Outcomes: Difference-in-Differences

	Delinquency	Cure	Short Sale	Modification
	(1)	(2)	(3)	(4)
MERS	-0.051*	-0.078**	-0.015	-0.011
	(0.029)	(0.031)	(0.010)	(0.014)
MERS×Post	0.006	0.044	0.004	-0.012
	(0.035)	(0.035)	(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 4. Foreclosure and Discharge Rate After Debt Sale

	All Buyers	Fannie Mae	First	Second	Third
	(1)	(2)	(3)	(4)	(5)
Panel A: Initial Purchases					
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
Panel B: Purchases from Fannie Mae					
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 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	0.009	-0.032	-0.060**	-0.011	-0.108***
	(0.031)	(0.017)	(0.037)	(0.029)	(0.013)	(0.032)
Late Vintage	0.047	-0.044*	-0.031			
	(0.046)	(0.026)	(0.053)			
MERS×Post	-0.021	0.006	-0.022	0.015	0.007	0.058
	(0.032)	(0.023)	(0.041)	(0.035)	(0.018)	(0.039)
MERS×Late Vintage×Post	-0.104**	0.069**	0.044			
	(0.052)	(0.033)	(0.067)			
Low FICO				0.080	0.003	0.039
				(0.053)	(0.027)	(0.067)
MERS×Low FICO×Post				-0.181***	0.077**	-0.057
				(0.060)	(0.034)	(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.

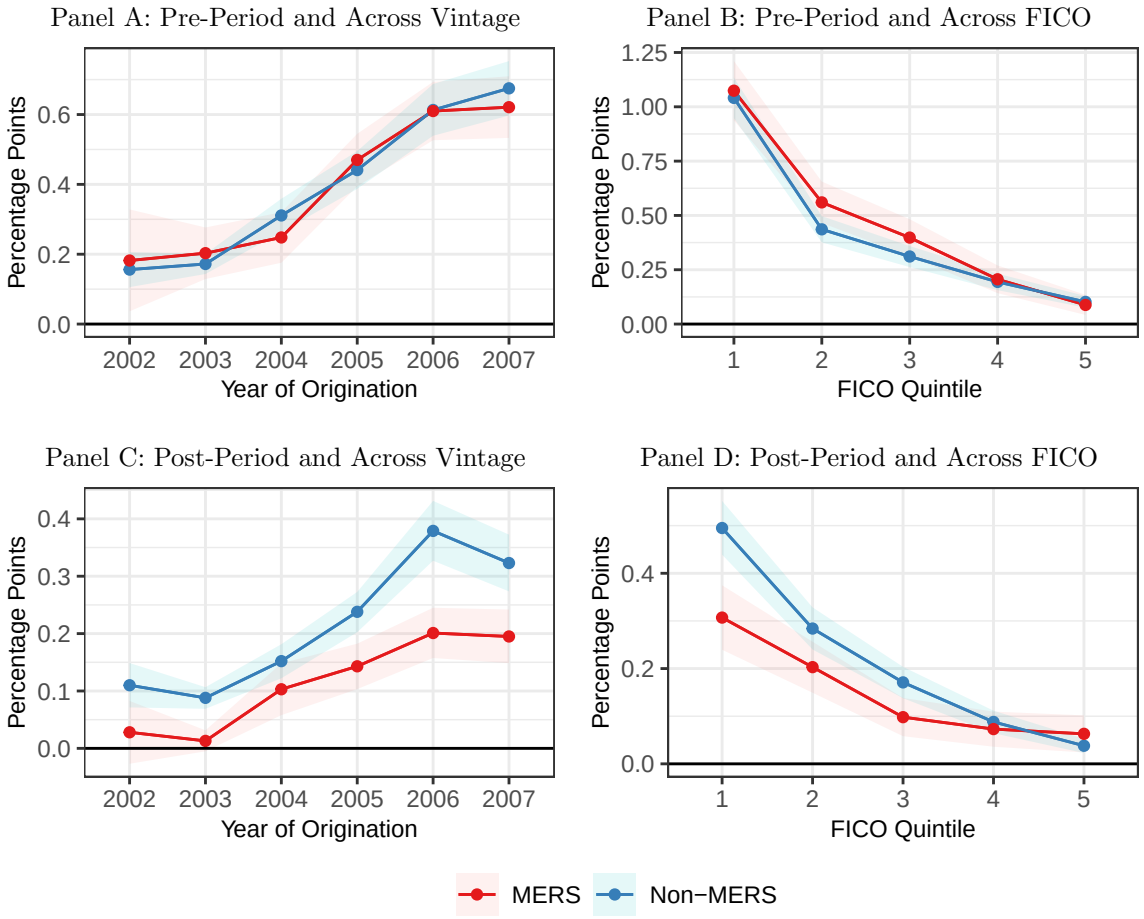
Table 6. Effect of Greenleaf Judgment Across Lender Bankruptcy: Difference-in-Differences

	Fce.	Debt Sale	Delinq.	Cure	Short Sale	Mod.
	(1)	(2)	(3)	(4)	(5)	(6)
High Cost	0.144**	-0.020	0.152**	0.027	-0.034*	-0.029
	(0.063)	(0.029)	(0.074)	(0.060)	(0.019)	(0.031)
Low Cost	-0.019	-0.019	-0.084***	-0.096***	-0.012	-0.008
	(0.025)	(0.014)	(0.030)	(0.032)	(0.010)	(0.015)
High Cost×Post	-0.231***	-0.028	-0.289***	0.040	0.007	-0.023
	(0.071)	(0.035)	(0.090)	(0.084)	(0.025)	(0.039)
Low Cost×Post	-0.063**	0.064***	0.053	0.044	0.003	-0.010
	(0.026)	(0.017)	(0.036)	(0.034)	(0.011)	(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.

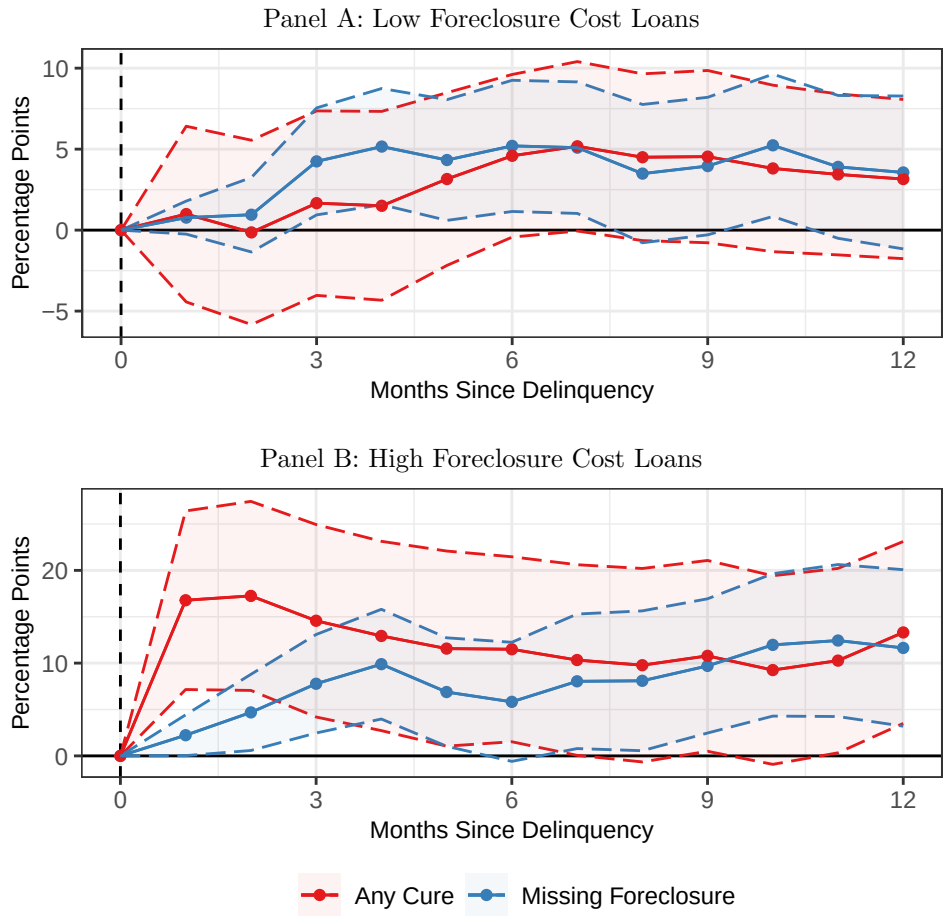
Appendix A Additional Results

Figure A.1. Heterogeneity in the Empirical Foreclosure Rate



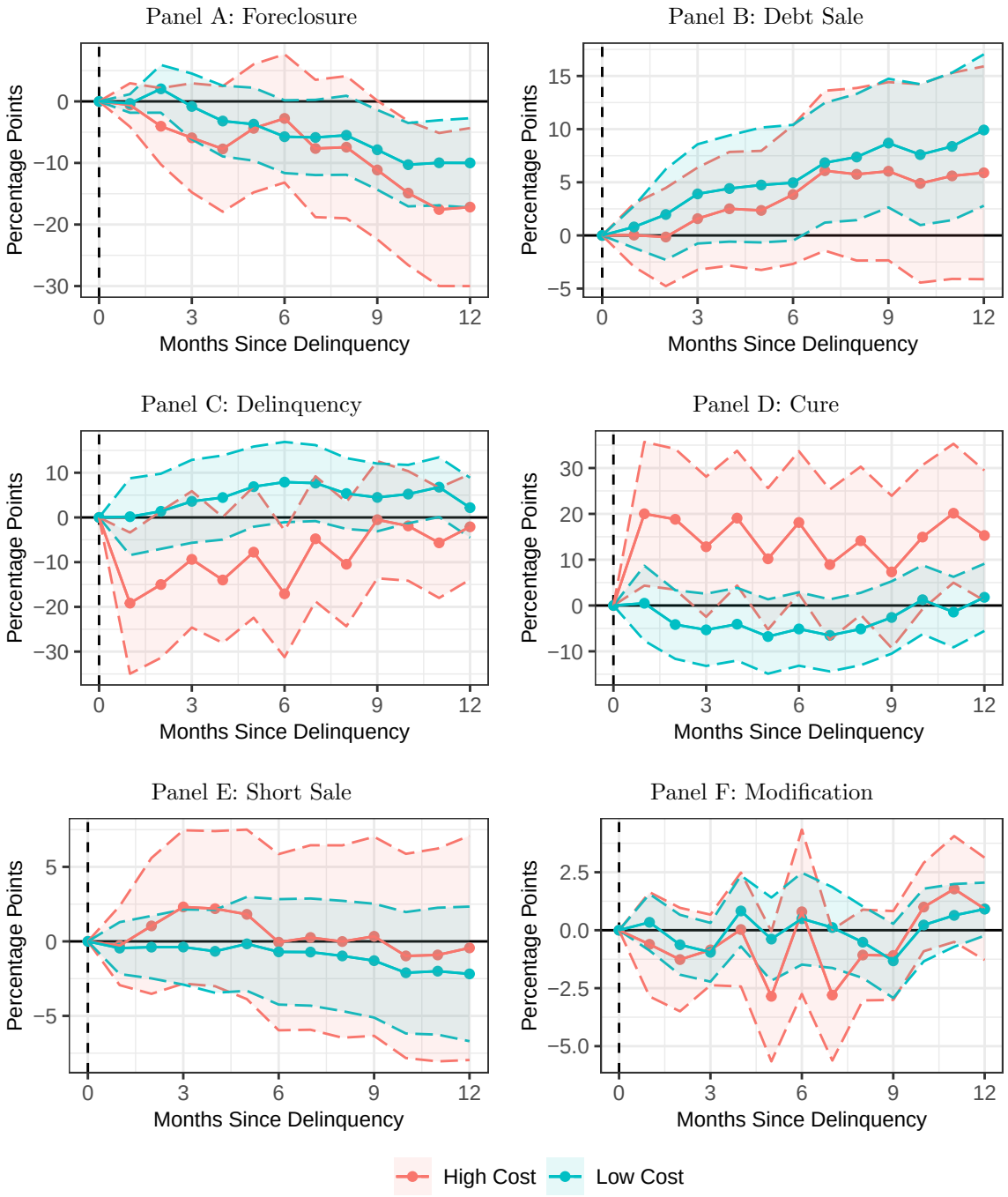
Note: This figure plots heterogeneity in the empirical foreclosure rate of MERS and non-MERS loans before and after the Greenleaf judgment. Loans are originated between January 2002 and December 2007. Foreclosure is observed between April 2013 and June 2016. Panel A plots the pre-period foreclosure rate 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.

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



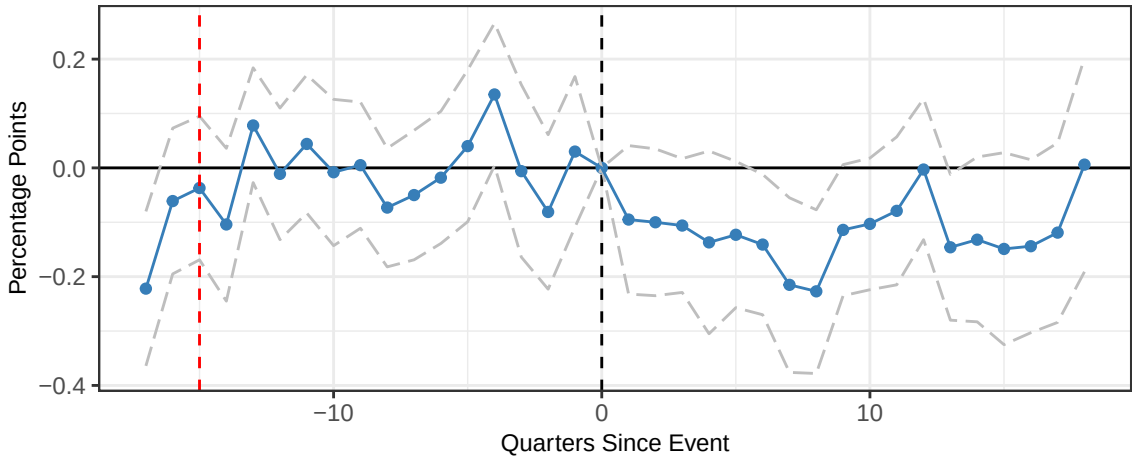
Note: This figure reports difference-in-difference estimates of the effect of the Greenleaf judgment on the probability of a credit event over multiple time horizons after an initial 60-day delinquency. Results are derived from an estimation of Equation (5), separately estimated at each time horizon. 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



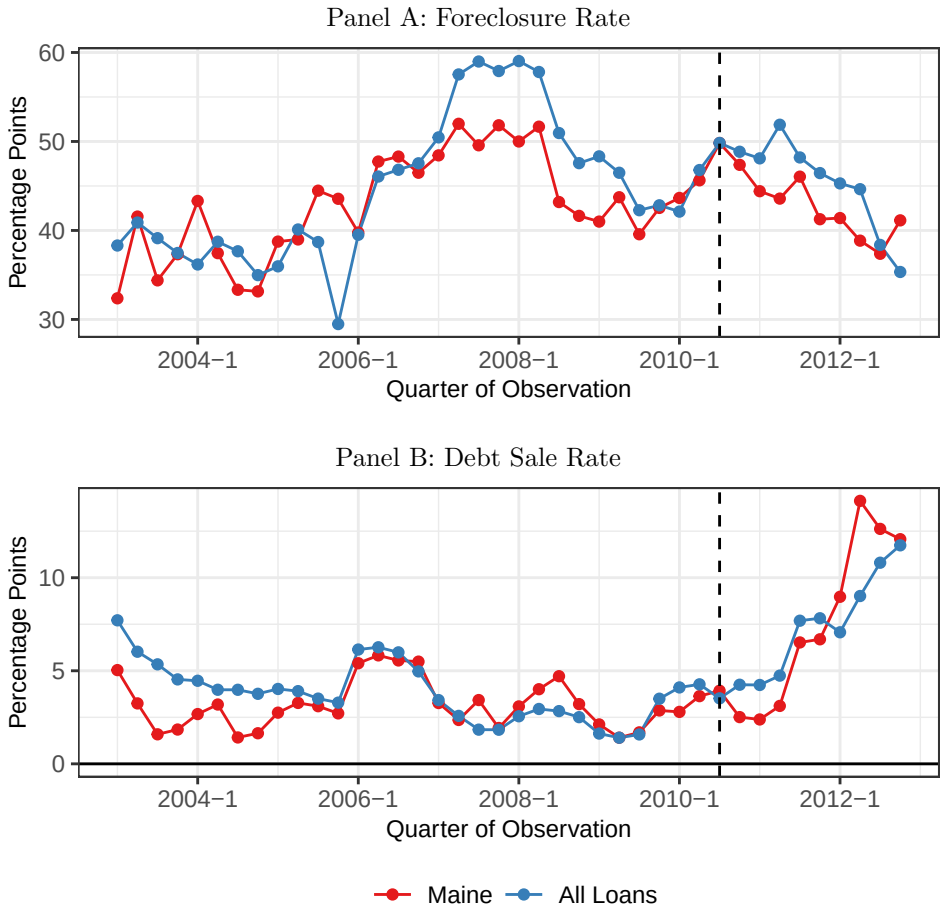
Note: This figure reports triple-difference estimates of the effect of the Greenleaf judgment on the probability of a credit event over multiple time horizons after an initial 60-day delinquency. Results are derived from an estimation of Equation (7), separately estimated at each time horizon. For a particular time horizon, the outcome variable takes a value of one in the period that it was last recorded as experiencing a particular credit event. Each panel 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, respectively. The shaded region represents 95% confidence intervals calculated using zip code-clustered standard errors. Data is collected from the Maine Registry of Deeds, the New Hampshire Registry of Deeds, and CoreLogic.

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.

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 Untied 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
Panel A: 3-Month Horizon						
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
Panel B: 6-Month Horizon						
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
Panel C: 9-Month Horizon						
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
Panel D: 12-Month Horizon						
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 15 months preceding the Greenleaf judgment and multiplied by 100 in order to interpret as percentage points.

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	0.018*	-0.060**	-0.035*	-0.019**	0.009
	(0.017)	(0.011)	(0.025)	(0.019)	(0.009)	(0.012)
MERS×Maine	0.043	-0.037**	0.022	-0.030	0.003	-0.018
	(0.029)	(0.016)	(0.037)	(0.034)	(0.013)	(0.018)
MERS×Post	-0.030	0.014	0.059*	0.022	0.009	-0.011
	(0.020)	(0.012)	(0.032)	(0.025)	(0.011)	(0.015)
MERS×Maine×Post	-0.081**	0.035*	-0.053	0.017	-0.006	-0.002
	(0.032)	(0.019)	(0.047)	(0.041)	(0.015)	(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	0.020	-0.082	0.060	0.002	-0.030
	(0.050)	(0.035)	(0.064)	(0.057)	(0.025)	(0.029)
Low Cost	-0.020	0.018	-0.057**	-0.047**	-0.022**	0.015
	(0.018)	(0.011)	(0.026)	(0.020)	(0.010)	(0.012)
High Cost×Maine	0.176**	-0.047	0.250**	-0.007	-0.035	0.004
	(0.080)	(0.045)	(0.098)	(0.080)	(0.030)	(0.042)
High Cost×Post	-0.059	-0.081**	0.072	-0.018	-0.005	0.009
	(0.063)	(0.036)	(0.084)	(0.073)	(0.030)	(0.039)
Low Cost×Maine	0.019	-0.035**	-0.015	-0.037	0.009	-0.021
	(0.029)	(0.017)	(0.039)	(0.036)	(0.014)	(0.019)
Low Cost×Post	-0.026	0.028**	0.056*	0.028	0.011	-0.014
	(0.020)	(0.013)	(0.034)	(0.026)	(0.011)	(0.016)
High Cost×Maine×Post	-0.199**	0.056	-0.354***	0.051	0.011	-0.032
	(0.094)	(0.049)	(0.123)	(0.109)	(0.037)	(0.055)
Low Cost×Maine×Post	-0.059*	0.034	-0.002	0.012	-0.008	0.003
	(0.031)	(0.021)	(0.048)	(0.041)	(0.015)	(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. Regressions interact High Cost, Low Cost, 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 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 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***	0.047*	0.013	-0.206***	0.043*	-0.045
	(0.072)	(0.028)	(0.072)	(0.045)	(0.024)	(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	-0.015	-0.049	-0.081***	-0.015	-0.010
	(0.025)	(0.014)	(0.030)	(0.031)	(0.010)	(0.015)
Placebo	-0.047	-0.064***	-0.074	-0.053	-0.020	-0.025
	(0.053)	(0.018)	(0.058)	(0.064)	(0.019)	(0.028)
Treated×Post	-0.102***	0.050***	0.003	0.051	0.002	-0.012
	(0.027)	(0.017)	(0.036)	(0.036)	(0.012)	(0.019)
Placebo×Post	0.052	0.054**	0.027	-0.021	0.018	-0.010
	(0.064)	(0.025)	(0.074)	(0.076)	(0.028)	(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	-0.020	-0.051*	-0.078**	-0.015	-0.011
	(0.025)	(0.013)	(0.029)	(0.031)	(0.010)	(0.014)
MERS×Pre-Law	-0.086***	0.049***	-0.020	0.005	0.001	-0.031
	(0.030)	(0.018)	(0.040)	(0.040)	(0.013)	(0.021)
MERS×Post-Law	-0.089***	0.052**	0.038	0.094**	0.008	0.011
	(0.030)	(0.021)	(0.046)	(0.041)	(0.014)	(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.