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Essays in Mortgage Finance and Securitization

by

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Professor Nancy Wallace, Chair
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Abstract

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The Great Recession renewed focus on various stages of a mortgage's life—how they are originated, why borrowers default on them, and how default is ultimately resolved. More specifically, the chapters of this dissertation shed light on the factors determining the success of mortgage renegotiation, and on the rise in the origination of complex mortgage instruments. Features of the securitized mortgage market are either explicitly studied, or provide the foundation for the empirical methodologies I develop.

During the housing crisis regulators faced impediments in their unprecedented intervention to promote large-scale mortgage renegotiation. What hampered renegotiation in the wake of the crisis? To answer this question, in **Chapter 1**, **The Limited Benefits of Mortgage Renegotiation** I study the expected gains from renegotiation for both sides of a mortgage contract: investors and borrowers. To overcome selection bias, I use plausibly exogenous variation in the propensity of intermediaries to renegotiate mortgages. I find that loan modification helped investors recover 3.5% more of the principal balance outstanding at the time of delinquency relative to foreclosing upon the borrower. However, there was substantial variation around this mean—a 12.5% (3.6 times the mean) standard deviation—which highlights the high degree of uncertainty about the realization of these gains. Thus, despite expected gains to borrowers—higher credit scores and a $115 increase in monthly consumption—regulators' attempts to promote mortgage renegotiation have proven to be ineffective, exacerbating debt overhang and its consequences.

The setting of Residential Mortgage Backed Securitization (RMBS) provides an ideal testing ground for theories of debt-structure, agency problems and their effect on debt renegotiation. Via the tranching of cash flows from underlying mortgages, an RMBS transaction creates multiple securities with claims to the underlying collateral. Moreover, tax law mandates the hiring of an agent, the Servicer, to manage the underlying collateral. In **Chapter 2**, **Multiple Tranches, Information Asymettry and the Impediments to Mortgage Renegotiation**, I first develop a simple conceptual framework to outline the channels via which multiple claim-holders induce fewer than optimal loan modification by worsening the agency problem between mortgage Servicers and RMBS Sponsors. Then, using within deal variation in the number and structure of tranches, I find that loans in pools collateralizing fewer tranches are more likely to be modified conditional upon being seriously delinquent. I also find that modified loans in such loan pools were likely to receive more aggressive loan

modifications. The results provide evidence for one channel via which the securitization of mortgages inhibited the renegotiation of delinquent mortgages in the wake of the housing crisis, and complement the results of Chapter 1.

In **Chapter 3**, **Partial Deregulation and Competition: Evidence from Risky Mortgage Origination**, co-authored with Amir Kermani (University of California, Berkeley) and Marco Di Maggio (Harvard University), we exploit the OCC's preemption of national banks from state laws against predatory lending as a quasi-experiment to study the effect of deregulation and its interaction with competition on the supply of complex mortgages. Following the preemption ruling, national banks significantly increased their origination of loans with prepayment penalties by comparison with national banks in states without predatory-lending laws. We highlight a competition channel: in counties where OCC-regulated lenders had larger market shares, non-OCC lenders responded by increasing their use of riskier contract features, such as deferred amortization, adjustable rates and interest-only payments, which were not restricted by the state predatory-lending laws.

Dedicated to my parents for all their support through the years.

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Chapter 1

The Limited Benefits of Mortgage Renegotiation

I have long advocated a systematic and streamlined approach to loan modification that puts borrowers into long term, sustainable mortgages. I support the industry plan as a means to allow borrowers to remain in their homes, provide investors with higher returns than can be obtained under foreclosure, and strengthen local neighborhoods where foreclosures are already driving down property values. It is my hope that this plan will be implemented in a way that delivers real progress on these important policy goals.
- Sheila Bair, Chairman of FDIC, in foreword to The Case for Loan Modification

1.1 Introduction

During the housing crisis, thousands of borrowers were unable to make the monthly payments on their mortgages and became seriously delinquent, with significant consequences on the broader economy (68, 75, 70, 69, 71). At the onset of the crisis, regulators such as FDIC Chairman Sheila Bair strongly promoted widespread mortgage renegotiation, although others were hesitant to do so, citing concerns about strategic behaviour by borrowers (67). Academic economists and legal scholars alike put forth proposals to encourage renegotiation (80, 66). Eventually, regulators initiated an unprecedented intervention in debt markets to encourage loan modification, but they remained disappointed by its efficacy. 10 show that the flagship Home Affordable Modification Program (HAMP) resulted in permanent modifications of only about 15% of all delinquent loans. In fact, towards the end of 2009, the Obama administration began to apply pressure on mortgage companies to ramp up loan modifications.[1]

[1] "After months of playing pretend, the Treasury Department conceded last week that the Home Affordable Modification Program, its plan to aid troubled homeowners by changing the terms of their mortgages, was a dud." – New York Times, December 6th, 2009. "The Obama administration on Monday plans to announce a campaign to pressure mortgage companies to reduce payments for many more troubled homeowners, as evidence mounts that a $75 billion taxpayer-financed effort aimed at stemming foreclosures is foundering." – New York Times, November 29th, 2009.

Ultimately, the completion of renegotiation will depend on whether the expected gains available to the agents on *both* sides of this debt contract are sufficient to induce them to participate.[2] Surprisingly, despite significant government resources being directed to encourage mortgage debt renegotiation, little work has been done to understand whether these gains were in fact achievable. Understanding these gains is crucial to appropriately design market interventions. For programs such as HAMP to be more successful, is it constraints on borrowers or investors that need to be relaxed? To shed light on the decision to renegotiate debt, I estimate the expected gains from modification relative to immediate foreclosure to both sides of the contract—the investor and the borrower.

My results show that there was likely little resistance from borrowers to the renegotiation of debt. However, the participation constraint of investors were often not met due to relatively small gains and a high variance around these gains. Previous explanations, both theoretical and empirical, for the perceived low rate of debt renegotiation have revolved around agency problems in securitization (8, 78, 72, 62, 90, 11, 63) or adverse selection (2, 4). Using the findings of this literature to motivate an appropriate empirical strategy, I provide evidence for an alternative channel which held up renegotiation. My findings complement the above literature by demonstrating that even in the absence of such agency problems renegotiation may have been subdued because of low expected gains to investors.

Quantifying the effect of renegotiation on the number of monthly payments completed by the borrower is vital to estimate the expected gains to investors from debt renegotiation. Expected gains from renegotiation are defined as the present value of the incremental cash flows that arise when a mortgage is renegotiated relative to those that arise when it is not. I determine the mean and the variance of these expected gains by combining an estimate of this effect with assumptions about house prices and discount rates. The higher the number of monthly payments completed by the borrower, the longer the time to re-default, and the more the investor gains from the modification. Continued mortgage payments maintain amortization and reduce the probability of subsequent foreclosure. Loan modification, however, delays the terminal cash flow from the mortgage, imposing a time-value-of-money related cost on the investor.

I estimate gains from modification relative to foreclosure for the borrower—the other side of the mortgage contract—by testing for the effect of loan modification on the borrower's durable and non-durable consumption. Following renegotiation of his mortgage a borrower's monthly payment decreases, allowing him to overcome liquidity constraints, improve consumption smoothing, and avoid the costs arising from default. I argue that these effects will be reflected in a borrower's consumption choices. Hence, the analysis of gains from modification to both sides of the contract relies on the careful identification of how renegotiation affects borrowers' decisions on whether to make mortgage payments and how much to consume.

The challenge to estimating both these causal effects arises because loans are not randomly renegotiated, as highlighted in the following example. Suppose two identical groups

[2] While debt renegotiation may have positive externalities—e.g. reducing the externalities that arise from foreclosure 27—these are unlikely to be internalized by privately optimizing agents on either side of the mortgage contract.

of borrowers become delinquent because they lose their jobs and cannot afford their monthly payments. Now suppose one group is able to obtain a renegotiation because, unobservable to the econometrician, they line up new jobs and can credibly promise to remain current. They would continue to make a large number of monthly payments. Those who do not get a renegotiation make two or three additional monthly payments but eventually end up in foreclosure. A naive comparison of the means of their outcome variables would result in an upward biased estimate of the causal effect of renegotiation precisely because it is not randomly given to borrowers.[3] Hence, to overcome the endogeneity concern in this simple context, I require a variable, or a set of variables, that are correlated with whether a borrower receives a modification, but uncorrelated with whether a borrower is able to find employment.

To overcome such selection bias, I use a unique feature of the mortgage market; namely that the mortgage is monitored not by the investor, but by a third party, the mortgage servicer. In this market the servicer has discretion over the decision to renegotiate. I then draw upon the results of 72, who theoretically model the consequences of the agency problem between the investors and the servicer, and 8, 78, 62, 84, 10, 59 who provide evidence that this agency problem manifests itself in substantial heterogeneity across servicers in their propensity to modify mortgage debt. This motivates the use of such variation to instrument for whether a loan gets modified.[4]

Two aspects of the market validate this strategy. First, borrowers do not choose who their mortgage servicers are, mitigating concerns about endogenous selection of borrowers into servicers. Second, borrowers are unlikely to be aware of their servicer's propensity to modify a mortgage, how this propensity compares to other servicers, and why such variation arises in the first place. Thus, conditional on observables, this variation will be exogenous to a borrower's decisions to make an additional monthly payment or change consumption. In the context of the simple example constructed above, the identity of the borrower's servicer will be unrelated to whether the borrower finds a job or not.

First, I show that loan modification predicts the completion of 56 additional monthly payments by the borrower. Given this finding, the present value of gains to investors from modification relative to foreclosure amounts to about 3.5% of the outstanding balance at entry into serious delinquency. This equates to about $6,700 for the average balance of $202,700. Not only are the expected gains from modification relatively low, but there is also substantial variation around them. From the perspective of the investor who observes key characteristics of the loan pool[5], the standard deviation of these gains is 12.5% (i.e., 3.6 times the mean). This variation is larger than that resulting from spatial and business-cycle

[3]The bias may go the other way as well. For example, if the servicer knows that a borrower will be re-employed he might not give him a modification as he will be able to self-cure. In this case, the naive estimate of causal effect of loan modification will be biased downwards. Such selection biases arise because I cannot observe the counterfactual outcome for those borrowers who received or did not receive a loan modification.

[4]In practice, I implement the first stage of a two-staged least squares by using either servicer-by-time-of-delinquency fixed effects or servicer-by-time-since-delinquency fixed effects to predict whether a loan receives a modification or not.

[5]These will include borrowers credit score, property value and loan-to-value-ratio at origination, the location of the property and the timing of the delinquency.

variation (7.2% across-CBSA-by-time standard deviation) which highlights the importance of borrower-specific heterogeneity.[6] Overall, the participation constraints of investors were just about met, if investors were risk neutral, and are unlikely to have been met if they were risk-averse.

The failure of the investor's participation constraint to hold will be sufficient to preclude debt renegotiation. As a result, if gains from renegotiation do exist for the other side of the contract—the borrower—they would remain unrealized. To understand whether borrowers wished to participate in renegotiations, I test for the availability of these gains. Moreover, policy-making can be designed to target the agent whose participation constraint needs to be relaxed in order to induce loan modification. In a result that is novel to the literature on mortgage renegotiation, I show that borrowers increase consumption by $115 per month following loan modification, which amounts to $5,700 in present value terms.[7] For every dollar decrease in the monthly payment 32 cents will be consumed, implying an elasticity which is higher than that estimated using interest rate resets on adjustable rate mortgages (34, 58) or from obtaining refinancing (12).

Overall, while the participation constraints of investors are unlikely to be met, there is evidence of substantial gains to borrowers. This demonstrates that contracting frictions are not the only impediment to debt renegotiation in mortgage markets. Insufficient gains to investors may have precluded debt renegotiation even without such frictions. Moreover, while it is important to align the incentives of servicers and investors, or subsidize servicers' costs of making loan modifications, interventions to encourage renegotiation must ensure that investors are willing to participate in the first place.

Hence, my paper is related to a broader literature understanding and assessing countercyclical policies employed in the wake of the financial crisis. Unconventional monetary policy had a profound impact on housing and mortgage markets through the large-scale asset purchases of the quantitative easing program, which lowered mortgage rates and fueled refinancing activity.[8] However, it was only the most credit-worthy borrowers who benefitted from these policies.[9] The government also intervened more directly to assist borrowers who were current on their mortgages but deeply underwater and so unable to obtain a refinancing. This took the form of the Home Affordable Refinancing Program (HARP), whose effect on interest rates and refinancing volume was mitigated by a flawed design which introduced competition related distortions into the mortgage market.[10]

Yet another attempt to mitigate the fallout from the housing crisis involved renegotiating mortgages of borrowers who were unable to make monthly payments and faced foreclosure.[11]

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[6]The unconditional standard deviation of these gains is about 22%.

[7]Assuming discount rate of 4.9% in annual terms. This assumption is based on the average interest rates on 30 Year Fixed Rate mortgages at time of modification.

[8]61 document that Q.E.1 lowered prepayment risk borne by investors, and 43 show that lenders passed this decrease onto borrowers by lowering mortgage rates.

[9]See 23 and 33 for a study of the real effects of quantitative easing and an examination of which borrowers and regions did or did not benefit from the program.

[10]20 and 12 study the effects of HARP on refinancing and show that the program changed the competitive landscape of the refinancing market with adverse effects on both interest rates and the volume of refinancing.

[11]38 provide a simple framework to conceptualize the tradeoffs between renegotiating a loan or not and

While mortgage renegotiation was observed prior to the government intervention in the form of the Home Affordable Modification Program (HAMP), several papers argued that the low rates of loan modification were due to agency problems and distorted incentives within the securitization chain.[12] My findings complement this literature and suggest an alternative channel that prevented mortgage modification—insufficient gains to investors.

In addition to further understanding the efficacy of loan modifications as a response to the housing crisis, my paper builds upon the work of 64 and 10 who describe the ex-post effects of loan modification. 64 studies the ex-post realized losses on privately securitized loans and finds that renegotiated loans had lower realized losses. First, this paper does not provide a view of the gains available to investors at the time at which the mortgage becomes delinquent, which is the relevant metric to understand the decision to renegotiate. The mean and variance of the gains I estimate fill this gap and provide this perspective. Moreover, this leaves us with an incomplete view as his results do not account for potential gains and losses to those on the other side of the contract, the borrowers.

10 demonstrate that geographies where servicers were more likely to modify loans experienced smaller house price declines, lower rates of delinquency on non-mortgage debt and higher levels of automobile purchases. While these results are informative of the social benefits of debt renegotiation and so justify intervention on the basis of realizing these externalities, they do not tell us about why such intervention would be needed in the first place. My results show that investors' limited gains made them unlikely to want to modify, which in turn pushed the government to intervene in this large debt market.

The rest of the paper proceeds as follows. Section 1.2 lays out a simple conceptual framework to inform the empirical analysis, Section 1.3 outlines the empirical frameworks used to model the effects of debt renegotiation on investors and borrowers, Section 1.4 describes the sources of data used, Section 1.5 presents the results of the analysis, Section 1.6 discusses robustness checks and extensions and Section 1.7 concludes.

1.2 Conceptual Framework

A mortgage contract is a complex instrument. The cash flows that it generates to investors and the utility that it gives borrowers will be driven by several micro- and macro-economic factors. This section builds a simple conceptual framework to highlight the key quantities I need to estimate from the data in order to measure gains to both sides of the contract, and to draw attention to assumptions I make in the subsequent analysis.

1.2.1 Servicing of mortgage debt

One of the unique features of the mortgage market, is the mechanism in place for post-origination monitoring of the debt. Neither the originator (lender) nor the investors in a securitized mortgage transaction maintain a relationship with the borrower after the issuance

describe loan modifications that may be optimal.

[12]Most recently 10 suggest that pre-existing institutional frictions related to the operating capacity and infrastructure of mortgage services may have impeded the success of HAMP.

of the mortgage debt. A third party—the mortgage servicer—maintains a direct relationship with the borrower, obtaining monthly payments of principal and interest and passing them onto investors. The mortgage servicer is an agent of the securitization trust. His actions and duties are governed by the pooling and servicing agreement (PSA), the contract in place between the servicer and the trust. The servicer, not the lender or investor, has discretion over whether the mortgage gets renegotiated or not.

In modeling the servicer's decision to renegotiate the loan, I assume that the servicer shares the investor's objective function and seeks to maximise cash flows from the mortgage pool that collateralizes the bonds held by the investor.[13] Making this assumption allows me to focus on estimating the gains to the investor from renegotiation rather than modeling the compensation structure of servicers. There will be some variation around this assumption, and this is variation that I can use to my advantage to identify the effects of renegotiation. I discuss this further in Section 1.3.4.[14]

1.2.2 Representation of gains to investors and borrowers

By assuming the shared objective function between servicer and investor I can model the cash flows to the investor and the utility of the borrower.

Consider an environment with three periods $t = 0, 1, 2$ where a mortgage has already been originated at $t = 0$ to fund 100% of the purchase of a property worth P_0. In general, P_t represents the value of the property at time t. The servicer monitors the mortgage in time $t = 1, 2$. Assume that there is no uncertainty or asymmetric information in the model. The borrower has utility over consumption at $t = 1, 2$. Each period he has Cobb-Douglas utility over units of goods, c_t, and units of housing, c_{th}, consumed. The borrower's utility function will be given by:

$$U(c_1, c_{1h}, c_2, c_{2h}) = (c_1)^{1-\alpha}(c_{1h})^{\alpha} + (c_2)^{1-\alpha}(c_{2h})^{\alpha} \text{ with } \alpha \in (0, 1)$$

One additional assumption will be that borrowers cannot adjust their consumption of housing c_{th} after it has been chosen at $t = 0$ upon origination of the loan (i.e., $c_{1h} = c_{2h} = c_h$). Thus, I focus on the units of consumption goods consumed by the borrower, c_t, a proxy for which I can observe in the data. The borrower receives income $\bar{y}$ in each period from which he makes his mortgage payments, and consumes some minimum level of the consumption good $\underline{v}$. Assume the mortgage contract is such that the borrower has to make two equal periodic payments, d, at times $t = 1, 2$ and consequently repay all outstanding principal, D_2, at the end of $t = 2$.[15]

[13] 52 surveys a representative sample of private label PSAs. He finds that the most common condition placed on a servicer contemplating renegotiation, is that the servicer act in the best interest of certificate-holders. Also note that in practice, there may be multiple investors who hold the bonds that are collateralized by the loan pool. I assume there is one representative investors who receives all the cash flows from a particular mortgage.

[14] Such variation will not be a concern for the subsequent reduced form analysis of the causal effect of loan modification if I restrict attention to dependent variables which, in the absence of modification, are independent of the identity and practices of the servicer.

[15] The initial lending takes place in a perfectly competitive mortgage market, with all lenders earning

At $t = 1$, there will be a permanent unexpected income shock with the income realization now being $y_1 = \tilde{y} < \bar{y}$. I restrict attention to the interesting case where $\tilde{y} < d + \underline{v}$[16]. This formulation captures the inherent incompleteness of a mortgage contract.[17] Now the servicer makes a decision about whether to modify the loan or not.

Suppose the mortgage is not renegotiated and the borrower is foreclosed upon. Then, the borrower's consumption bundle is $c_1 = \tilde{y} - \rho c_h - \theta$; $c_2 = \tilde{y} - \rho c_h$ where ρ is the rental cost of one unit of housing. At $t = 1$, the borrower does not make his monthly payment, but consumes the same level of housing c_h for which he now pays rent. Additionally, he bears a cost of being in default of θ. Let θ represent costs such as loss of access to credit markets, relocation costs and other fees and expenses the borrower bears by being in foreclosure. If the servicer forecloses upon the borrower, the investor simply recovers ϕP_1 where $1 - \phi \in (0, 1)$ represents the property value recovered from a foreclosure sale. The investor receives nothing at $t = 2$.

Now consider the borrower's consumption bundle and the investor's cash flows when the loan does get renegotiated. Let the renegotiation involve an adjustment of the borrower's monthly payment, Δ, such that he is able to remain current; i.e., Δ s.t. $\tilde{y} \geq d + \Delta + \underline{v}$.[18] Then, the borrower's consumption bundle is $c_1 = \tilde{y} - (d + \Delta)$; $c_2 = \tilde{y} - (d + \Delta)$. Note here that the borrower also does not bear the cost of default, θ. If he has paid down enough of his principal, $D_2 \leq 90\% \times P_2$, I assume that he simply refinances the mortgage and repays the principal outstanding. The investor receives D_2 at the end of $t = 2$. However, if the borrower is sufficiently underwater, $D_2 > 90\% \times P_2$, he will be unable to refinance, and will enter foreclosure to repay the principal outstanding. In this case, the investor receives ϕP_2. Let the function $G(P, D)$ denote this terminal cash flow, where $G(P, D) = \mathbf{1}_{\{0.9 \times P < D\}} \cdot \phi P + \mathbf{1}_{\{0.9 \times P \geq D\}} \cdot D$. Following a loan modification, the investor's cash flows can be expressed as $d + \Delta + d + \Delta + G(D_2, P_2)$.

Let $V(\Delta)$ refer to the investor's cash flows from $t = 1$ onwards assuming the loan is modified, and $V(0)$ denote the investor's cash flows assuming the loan is not modified. I express the gains to the investor as:

$$V(\Delta) - V(0) = d + d + \Delta + \Delta + G(D, P_2) - \phi P_1 \tag{1.1}$$

The ability of debt renegotiation to generate gains for the investor will depend on what he can earn from not renegotiating (ϕP_1); on the number and size of additional monthly

zero profits, therefore I have the condition $d + d + D_2 = P_0$. Moreover, assume the asset pricing equation, $P_0 = \rho c_h + \rho c_h + P_2$ holds, where ρ is the rental cost of housing and c_h is the flow of housing units from a property worth P_0. Therefore, in equilibrium $d = \alpha \bar{y} = \rho c_h$ and $D_2 = P_2$ and $c_1 = (1 - \alpha)\bar{y}$; $c_2 = (1 - \alpha)\bar{y}$; $c_h = \frac{\alpha \bar{y}}{\rho}$.

[16]If $\bar{y} > \tilde{y} \geq d + \underline{v}$ the borrower can still make his monthly payment. The inefficiency that arises from not being able to rewrite the contract is the failure of the borrower to smooth consumption over period 1 and period 2.

[17]In this setting, given the assumed lack of ex-ante uncertainty, the contract will be non-contingent when originated. I follow 38 in modeling the unexpected income shock to capture such incomplete contracting frictions in a reduced form manner.

[18]Note that this assumption of a permanent income shock is not crucial to my goal of estimating the benefits to investors and borrowers. It will just affect what the optimal loan modification might look like.

payments $(d + \Delta)$ he collects; on the borrower's ability to pay down the debt until he refinances or redefaults $(G(D, P_2))$. In the case of redefault; the property value at $t = 2$ will also affect the gains. In a setting with discounting, the more delayed the termination, the lower will be the contribution of $G(D, P_2)$ to the present value of gains. On average, a loan modification extends the time over which mortgage debt is repaid (38) by lowering the interest rate (often to a level below the average rate on a new mortgage), extending the term of the loan, capitalizing missed payments into the balance (increasing D_2), and engaging in principal forbearance.[19] It keeps the borrower current on the mortgage, but delays recovery of principal.

Similarly, defining the consumption bundle of the borrower with modification as $W(\Delta)$ and without modification as $W(0)$, I can write down the incremental consumption bundle from loan modification as:

$$W(\Delta) - W(0) = (-2\Delta + \theta) \tag{1.2}$$

Borrowers consume from the decrease in monthly payments due to the loan modification (-2Δ). Additionally, they avoid the costs of being in foreclosure, θ.[20] In this case, θ might represent the continued access to credit markets due to the avoidance of foreclosure, which better allows borrowers to smooth consumption.

1.2.3 Translating the framework to data

Additional assumptions will now be required to facilitate the estimation of the quantities represented in Equations (1.1) and (1.2).

In the empirical setting there will be variation in the number of payments that borrowers complete depending on whether or not their loan gets modified. Let T_{Mod} denote the expected number of payments completed by the borrower if his mortgage is modified following entry into serious delinquency, and T_{NoMod} be the expected number of payments completed if it is not modified. $T_{NoMod} \geq 0$ either because the borrower attempts to recover from the delinquency, or because he self-cures and continues to make payments on his mortgage. The mortgage remains active until $t = 1 + T_{Mod}$ if modified and $t = 1 + T_{NoMod}$ if not modified.

Adding uncertainty about the realization of house prices to the framework above requires me to make an assumption about how these prices evolve. I assume $E_1[P_{1+k}] = P_1$ for all k; i.e., that house prices follow a random walk. Making this assumption, I only need to estimate the property value at the time of the borrower's entry into serious delinquency. Finally, I incorporate discounting into the framework, assuming that all cash flows from $t > 1$ onwards will be discounted at rate R_1.

I decompose the gains from renegotiation into those which arise from the present value of continued payments by the borrower due to loan modification, denoted $\Delta PV(\text{PMTs})$; and those from the present value of gains from termination of the mortgage contract, denoted

[19] Principal forbearance happens when some portion of the outstanding balance is converted into interest free debt, without the amortization term of the loan being changes. This results in a balloon payment upon maturity of the contract.

[20] Also note that ρc_h and d cancel out. The initial contract in this frictionless world will result in $d = \rho c_h$ in equilibrium

ΔPV(Termination). These can be calculated as:

$$\Delta PV(\text{PMTs}) = \sum_{k=1}^{T_{Mod}} \frac{d+\Delta}{(1+R_1)^k} - \sum_{k=1}^{T_{NoMod}} \frac{d}{(1+R_1)^k} \tag{1.3}$$

$$\Delta \text{PV(Termination)} = \frac{G(P_1, D_{T_{mod}})}{(1+R_1)^{T_{mod}}} - \frac{\phi P_1}{(1+R_1)^{T_{NoMod}}} \tag{1.4}$$

where $V(\Delta) - V(0) = \Delta PV(\text{PMTs}) + \Delta\text{PV(Termination)}$. To estimate $(\widehat{V(\Delta) - V}(0))_i$ for each loan in the sample, I require estimates of $T_{i,Mod}$ and $T_{i,NoMod}$. They can be obtained by estimating a model of the causal effect of loan modification on the number of monthly payments completed by the borrower and using predicted values from this model.[21] For renegotiated mortgages, I have data on how each of the contract terms change as a result of the renegotiation and can directly obtain Δ, the change in the monthly payment. For those that are not renegotiated, I have to impute $\widehat{\Delta}$.[22]

On the borrower's side, I decompose the benefit from loan modification into two components:

$$W(\Delta) - W(0) = (w(\Delta) - w(0)) \times (T_{Mod} - T_{NoMod})$$

where $w(\Delta) - w(0)$ is the causal effect of loan modification on monthly consumption, and $(T_{Mod} - T_{NoMod})$ is the causal effect of loan modification on the number of months that the borrower remains in the existing mortgage contract. I am unable to generate reliable estimates of $\widehat{W(\Delta) - W}(0)$ at the loan level due to limitations of the data that I use to construct proxies for consumption. However, I test the null hypothesis that loan modifications had no effect on the consumption of borrowers relative to not getting their loans modified.[23]

1.3 Empirical Frameworks

In the previous section, a simple conceptual framework highlights the two models that I need to estimate; a model of the effect of debt renegotiation on the number of monthly payments completed following 90+ days delinquency, and on the monthly consumption of the borrower. It is crucial to use the appropriate empirical frameworks to model these effects. Otherwise the estimates of $T_{i,Mod}$, $T_{i,NoMod}$, and $(w(\Delta) - w(0))$ will be biased as they will not fully account for the nuances of the data-generating processes in this setting.

1.3.1 The causal effect of renegotiation on payments completed

An important determinant of the gains from loan modifications is the expected number of monthly payments a delinquent borrower will complete depending on whether or not he receives a loan modification. To estimate the effect of renegotiation on the number of

[21] Section 1.3.1 discusses the methodology employed to do so.
[22] I do so in Section 1.3.2.
[23] I discuss the methodology used to test this hypothesis in Section 1.3.3.

payments completed, I depart from the widely used least squares frameworks employed in the literature on mortgage renegotiation.

Let $Modify_i$ be a variable equal to 1 if loan i has been modified. $Modify_i$ is an endogenous variable and potentially correlated with characteristics of the borrower that remain unobservable to the econometrician. Failure to account for this will result in a biased estimate of the causal effect of loan modification. A second concern is the right censoring inherent in the data. This arises because I only observe the loan histories through to December 2013 and do not observe how many more payments borrowers completed beyond this date. Not accounting for this feature of the data will bias the estimate downwards. Therefore, I estimate a censored regression model of the number of payments completed following delinquency, with an endogenous dummy variable which determines whether a loan is modified or not:

$$T_i^* = Modify_i \beta + X_i' \zeta_1 + \epsilon_i \text{ where } \epsilon_i \sim N(0, \sigma_\epsilon^2) \tag{1.5}$$

$$T_i = \begin{cases} T_i^* & \text{if } Censored_i = 0 \\ T_i^{max} & \text{if } Censored_i = 1 \end{cases} \tag{1.6}$$

$$Modify_i = \mathbf{1}\left\{Z_i'\gamma + X_i'\zeta_2 + \upsilon_i > 0\right\} \text{ where } \upsilon_i \sim N(0, \sigma_\upsilon^2) \tag{1.7}$$
$$\text{and where } Cov\left(\epsilon_i, \upsilon_i \mid X_i\right) \neq 0 \neq 0$$

This is a cross-sectional setting, with one observation in the dataset for each mortgage i. X_i represents a set of borrower level characteristics that I can observe in the data.[24] Equations (1.5) and (1.6) lay out the censored regression framework and Equation (1.7) models the endogeneity of $Modify_i$. Equation (1.5) is the structural equation of interest. The latent variable T_i^* denotes the number of monthly payments completed by a delinquent borrower following entry into 90+ days delinquency.

The true realization of T_i^* is not always observable in the data. Let T_i be the count observed in the data. Loan histories are truncated at December 2013. If a loan i is current at this date, the data only tells me that the borrower has completed at least T_i monthly payments following entry into 90+ days delinquency. Such a loan is considered to be censored, i.e., $Censored_i = 1$.[25] Another loan history might have the borrower foreclosed upon before December 2013, and so he stops making additional payments. In this case, I do observe the

[24] The following are included as control variables in all regressions: For the following variables, a spline with knots at each quintile: loan to value ratio, loan amount, credit score, original interest rate, house price change over the 12 month period prior to entry into serious delinquency. I also include dummy variables for the purpose of the loan (cash out refinance, rate refinance, purchase, or unknown); whether it is private label or GSE securitized; whether information on debt to income ratio is missing. I also include the debt to income ratio as a control if it is not missing. Finally, CBSA fixed effects, time of delinquency fixed effects, and originator by agency (PLS or GSE) fixed effects will be included.

[25] Note here that the definition of censoring differs from that of the classical mortgage setting. For example, consider a hazard model of loan default. Here, a loan's time series observations would be considered censored if the loan terminates due to prepayment, or leaves the sample for other reasons, such as the transferring of Servicing rights. In the case of this hazard model, the latent variable which measures time to default will not be observed by the econometrician if the loan leaves the sample for these alternative assumptions. However, similar to the setting of the hazard rate of default, T_i^* will be assumed to be censored if I do not observe the loan history due to the fact that I stop observing loan histories in December 2013.

true realization of variable T_i^*. These possibilities are reflected in Equation (1.6). Equations (1.5) and (1.6) together correspond to a Tobit model with right censoring, where the right censoring point T_i^{max} varies from one individual loan to the other.

Equation (1.7) states that the decision to renegotiate the loan will be based on X_i (which also appear in Equation (1.5)), a shock v_i, and a variable or vector Z_i that is excluded from the structural equation (1.5). Importantly, the variation in Z_i is assumed to affect the decision to renegotiate the loan, but not the decision of the borrower to make monthly payments following delinquency. By assuming v_i are normally distributed, I am modeling the decision to renegotiate the loan as a probit regression. The endogeneity problem arises through the assumption $Cov\left(\epsilon_i, v_i \mid X_i\right) \neq 0$. This assumption states that, for example, a high realization of v_i might lead to the loan being renegotiated, but will also drive T_i^* in a way that cannot be captured by observable covariates X_i.

In order for the estimate of coefficient β to be free of bias from endogeneity, Z_i must satisfy two assumptions. First, $Cov\left(Z_i, Modify_i \mid X_i\right) \neq 0$ and second, $Cov\left(Z_i, \epsilon_i \mid X_i\right) = 0$. The first states that conditional on observable X_i, Z_i affects whether the loan gets renegotiated. The second assumption states that the only way the variation in Z_i can affect the borrower's decision to make monthly payments is through its effect on the decision to renegotiate the loan. The variables that satisfy these restrictions are discussed in Section 1.3.4. Having found such a Z_i, the system of equations can be estimated using maximum likelihood.[26]

I use the parameters of the model to form estimates of estimates of $T_{i,Mod}$ and $T_{i,NoMod}$ at the loan level[27]:

$$\begin{aligned}
\widehat{T_{i,Mod}} &= E\left[T_i^* \mid T_i^* > 0,\, X_i,\, Modify_i = 1\right] && (1.8)\\
\widehat{T_{i,NoMod}} &= E\left[T_i^* \mid T_i^* > 0,\, X_i,\, Modify_i = 0\right] && (1.9)
\end{aligned}$$

1.3.2 Imputing modifications for loans that are not modified

While the success of loan modification depends on the ability of the servicer to extend the life of the mortgage by reducing monthly payments by Δ, this reduction also has a negative effect on the cash flows to the investors. For loans that were modified, I simply observe Δ in the data. For those that were not, I infer the change in the mortgage contract using the parameters from two regressions:

$$\begin{aligned}
\mathbf{1}_{\{\Delta Contract_i \neq 0\}} &= X_i'\eta_1 + \epsilon_i \quad \text{if } Modify_i = 1 && (1.10)\\
\Delta Contract_i &= X_i'\eta_2 + \epsilon_i \quad \text{if } Modify_i = 1 \text{ and } \Delta Contract_i \neq 0 && (1.11)
\end{aligned}$$

[26] Appendix A.1 derives the log-likelihood function for both the censored regression model, and the censored regression with endogenous dummy variable model. The discussion in 92 demonstrates why a simple two step estimator using predicted values of $Modify_i$ from a first step linear probability model cannot be used as it is an endogenous dummy variable. Hence, one has to estimate the system using full information maximum likelihood.

[27] Note that T_{NoMod} will be adjusted to take into account the time lag between entry into serious delinquency and completion of the loan modification.

where $Contract_i \in \{Rate_i, Balance_i, RemainingTerm_i, InterestFree_i\}$ and represents various terms of a mortgage contract, and $\Delta Contract_i$ is the change in the term from before to after loan modification. The first three contract terms are standard.[28] $InterestFree_i$ represents the percentage of outstanding balance at the time of serious delinquency that was converted to interest free debt as a result of the modification. I estimate Equation (1.10) using a probit regression, and Equation (1.11) using ordinary least squares. X_i includes borrower and loan level observables at origination, and also includes time of delinquency fixed effects, CBSA fixed effects and servicer by time of delinquency fixed effects.

I use predicted probabilities from the first regression and multiply them by predicted values from the second regression to infer $\Delta Rate_i$, $\Delta Balance_i$, $\Delta RemainingTerm_i$, and $\Delta InterestFree_i$ for loans with $Modify_i = 0$, and then construct $\widehat{\Delta}_i$ for each of these loans.

With the estimates from Section 1.3.1 and 1.3.2 in hand, I will be able to compute the gains to investors at the loan level, i.e., compute $\left(\widehat{V(\Delta) - V(0)}\right)_i$. To facilitate comparison across mortgages, I normalize this estimate by the balance outstanding at the time of entry into serious delinquency to obtain $\frac{\left(\widehat{V(\Delta)-V(0)}\right)_i}{D_{1,i}}$. With borrower level estimates, I can characterize both the mean and variance of these gains.

1.3.3 Estimating the gains to borrowers

Without understanding whether the participation constraints of borrowers were satisfied, one obtains an incomplete view of the impediments to renegotiation in the wake of the crisis. In order to quantify gains to borrowers, I test for the effect of debt renegotiation on the durable and non-durable consumption of borrowers.[29]

First, I estimate an event study to compare the consumption of a borrower before and after he receives a loan modification. I compare their consumption to a control group who have also become 90+ days delinquent, do not receive a loan modification, and are equally deep into their delinquency. The estimating equation for the event study is:

$$Y_{it} = \eta_{ct} + \psi_{(t-t_0(i))} + X_{it}'\beta_1 + \sum_{k=-4;k\neq-1}^{4} Modify_i \cdot \mathbf{1}_{t=t_m(i)+k} \cdot \beta_{2k} + \epsilon_{ict} \tag{1.12}$$

where Y_{it} denotes a measure of borrower level durable or non-durable consumption.[30] η_{ct} are county-by-time fixed effects to ensure that I control for all time-varying unobservable heterogeneity at the county level; $\psi_{(t-t_0(i))}$ are fixed effects for the time since delinquency, which control for the time trend in the outcome variable that is common to loans that are or are not modified. $Modify_i$ is an indicator variable equal to 1 if the loan was modified, and $\mathbf{1}_{t=t_m(i)+k}$ is an indicator variable for whether time t is k periods ahead of time $t_m(i)$, i.e., the date when the loan was modified. Note that coefficient $\beta_{2,-1}$ is restricted to be 0. Each time period t will cover 6 months.

[28] $Rate_i$ is the interest rate on the mortgage, $Balance_i$ is the log of the outstanding balance, $RemainingTerm_i$ is the number of months until maturity of the loan.

[29] Section 1.4 describes the construction of the proxy variables used to measure consumption.

[30] I will also consider the credit score of the borrower as an outcome variable.

The estimated $\{\beta_{2k}\}_{k=-4}^{4}$ from this specification will be biased due to endogenous selection of loans into renegotiation, i.e., $Cov\left(Modify_i \times \mathbf{1}_{t=t_m(i)+k}, \epsilon_{ict} \mid \eta_{ct}, \psi_{(t-t_0(i))}, X_{it}\right) \neq 0$. To overcome this selection bias, I will use a two stage least squares framework, incorporating an instrumental variable to obtain exogenous variation in loan modification. Since the data on consumption is rather noisy, I move away from an event study setting, and test for the change in consumption from before the loan modification to after the loan modification. Yet, I maintain the essential ingredients of Equation (1.12). The structural equation to be estimated is:

$$\begin{aligned} Y_{it} &= \eta_{ct} + \psi_{(t-t_0(i))} + X_{it}'\beta_1 \\ &+ \underbrace{Modify_i\beta_2 + Modify_i \cdot \mathbf{1}_{t_m(i)>t} \cdot \beta_3}_{\text{Instrumented}} + \epsilon_{ict} \end{aligned} \tag{1.13}$$

The coefficient of interest is β_3. The two first stage regressions will be:

$$\begin{aligned} Modify_i &= \eta_{1,ct} + \psi_{1,(t-t_0(i))} \\ &Z_{it}'\lambda_1 + X_{it}'\lambda_2 + \xi_{ict} \end{aligned} \tag{1.14}$$

$$\begin{aligned} Modify_i \cdot \mathbf{1}_{t_m(i)>t} &= \eta_{2,ct} + \psi_{2,(t-t_0(i))} \\ &+ Z_{it}'\gamma_1 + X_{it}'\gamma_2 + \upsilon_{ict} \end{aligned} \tag{1.15}$$

where Z_{it} is a vector that is excluded from Equation (1.13).[31] In other words, variation in Z_{it} is assumed to be independent of a borrower's consumption decisions. That is, Z_{it} only drives them through its effect on whether and when a loan gets modified. Using predicted values from the first stage regressions in place of $Modify_i$ and $Modify_i \cdot \mathbf{1}_{t_m(i)>t}$ in Equation (1.13) will allow me to estimate β_3.

1.3.4 Instrumental variables approach

The validity of the analysis described above hinges on the appropriate choice of variables Z_i or Z_{it}. Without these instrumental variables, any estimate of the gains from modification will be biased. This section discusses the strategy used to overcome this concern. I will be estimating regressions of the type:

$$Y_i = Modify_i\beta + X_i'\zeta + \epsilon_i \tag{1.16}$$

where i denotes each individual loan. Y_i denotes the outcome variable of interest and X_i represents loan, borrower, and geography related control variables. Note that I have suppressed time related subscripts in the above equation. To identify β using Ordinary Least Squares, the assumption $Cov(Modify_i, \epsilon_i \mid X_i) = 0$ needs to be satisfied. That is, conditional on loan and borrower characteristics, loan modification should be as good as randomly assigned. Satisfaction of this assumption appears unlikely given that the servicers

[31]21 suggest the use of a linear probability model in the first stage to avoid model mis-specification.

have a larger information set than I do and will select borrowers into loan modification based on characteristics that are unobservable to me. To correctly identify β, I need to isolate variation in the probability that a loan gets modified which is uncorrelated with shocks to the borrower, ϵ_i.

In Section 1.2.1, I describe the unique feature of the mortgage market in that loans are monitored not by the investors but by a third party—the mortgage servicer—who has discretion over the decision to renegotiate or not. In Figure 1.1 I document variation across servicers in my sample in their propensity to modify a loan that has become 90+ days delinquent. In particular, I run the regression:

$$Y_{ict_0(i)} = \alpha + \sum_{s \in S} \sum_{t} \beta_{0,s,t} \mathbf{1}_{Servicer=s \text{ and } t_0(i)=t} + X_i' \beta_1 + \gamma_{ct_o(i)} + \epsilon_{ict_0(i)} \tag{1.17}$$

where Y_i is an indicator variable for whether loan i, that went delinquent at time $t_0(i)$ gets renegotiated; $\mathbf{1}_{Servicer=s \text{ and } t_0(i)=t}$ is an indicator variable for whether the loan was monitored by servicer s and went delinquent for the first time at $t_0(i)$; and $\gamma_{ct_0(i)}$ are CBSA by time of serious delinquency fixed effects. Figure 1.1 plots the $\beta_{0,s,t}$ coefficients from this regression, with each line corresponding to a given servicer s.[32] I also observe variation across servicers in the hazard rate to loan modification. To document this variation I estimate a proportional hazards model of entry into loan modification conditional on being seriously delinquent. I allow for servicer specific baseline hazard functions and plot them in Figure 1.2.

These figures highlight that substantial heterogeneity exists in servicer behaviour even after controlling for a comprehensive set of covariates. The variation is not driven purely by the mix of borrowers serviced by each intermediary. The partial F-statistic of the joint test of significance of all fixed effects in Figure 1.1 equals 145, showing that they are important predictors of the propensity to modify a loan.

The literature suggests that agency problems, the mechanisms and contracts to alleviate them, and other important institutional features of mortgage securitization lead to such variation across servicers. In a theoretical model 72 show that the optimal contract which overcomes asymmetric information and aligns the servicer's and investors' incentives will influence the rate of loan modification. Parties within the securitization chain may be affiliated with each other based on decisions about which securitization an originator sells his mortgage pools into, or depending on who retains the servicing rights.[33] 11 show that affiliation between the owner of a borrower's second lien mortgage and the servicer of the first lien loan can affect loss mitigation decisions (i.e. whether to foreclosure, modify, or do nothing).[34] 51

[32]The omitted category here are $\mathbf{1}_{Servicer=s \text{ and } t_0(i)=t}$ for which the servicer is recorded as "unknown". Thus the coefficients can be interpreted as the propensity of each servicer to modify a loan relative to the group of loans with missing data on servicers.

[33]For example, Wells Fargo can originate loans and then sell them into a securitization being organized by Bank of America (who is termed the deal sponsor). However, Wells Fargo may choose to retain servicing rights and continue to service this mortgage pool. Now there is an affiliation between the originator and servicer of the mortgages. Consider another example. Countrywide can be the deal sponsor of a securitization, acquire mortgage pools from a range of bank and non-bank lenders, and also purchase the servicing rights for these loans. In this case, the deal sponsor and servicer are affiliated.

[34]My results are robust to controlling for whether the property had a second lien on it or not. This should

provide evidence that when a servicer and the investor in the equity tranche of a mortgage backed securitization deal are the same entity, the equity tranche sees improved performance through aggressive loan modifications or a delay in foreclosing upon the borrower. Servicers take these actions to avoid recognizing losses that would first affect the equity tranche.

Both legal and economic scholarship has discussed how servicers' contracts (the pooling and servicing agreements) and their cost structure can impede renegotiation. 52 documents substantial variation in a sample of these contracts, and argues that while most agreements do not outright ban loan modifications they may still put up obstacles to it. He comments that the heterogeneity in these contracts does leave open the possibility that servicers faced varying levels of liability risk from failure to modify in accordance with the PSA terms. 62 studies a sample of PSAs and shows that they do affect the rate of loan modification. Servicers would have been differentially exposed to restrictive or not restrictive PSAs which would contribute to the variation documented in Figures 1.1 and 1.2. Finally 10 show that servicers' varying operational characteristics also drive heterogeneity in propensity to renegotiate loans.

It is doubtful that these complex arrangements and institutional features of the securitization chain will be well understood by borrowers. Borrowers may have been aware of who their servicer was, but are unlikely to have known his propensity to renegotiate, and how his practices differed from other servicers.[35] This is precisely the variation that will be used in the application of the instrumental variables approach. I argue that the exclusion restriction, $Cov(Z_i, \epsilon_i \mid X_i) = 0$, will be satisfied as variation across servicers Z_i, conditional on observable X_i, will be exogenous to borrowers' decisions on the number of payments to complete and how much to consume—it will be uncorrelated with ϵ_i. In other words the servicer's propensity to modify, Z_i, can affect the outcome variable Y_i only through its effect on whether a particular mortgage is renegotiated.[36] I use the identity of the mortgage servicer interacted with the timing of the delinquency as instrumental variables for whether a loan receives a modification. In other words, let $\Lambda_{S_i \times t_0(i)}$ denote the servicer by time of delinquency fixed effects and let $Z_i = \Lambda_{S_i \times t_0(i)}$.[37]

One challenge to the exclusion restriction arises from the possibility of endogenous sorting or matching of borrowers and servicers on dimensions that will not be captured by covariates. However servicers are assigned to loans just before closing of residential mortgage backed securitization deals and the borrower does not have the ability to choose who his mortgage servicer is.[38]

account for the effect of the second lien on decisions to make payments and consume. However, as I will shortly discuss, I assume that the ownership of this second lien, and whether the owner is affiliated with the servicer are exogenous to these outcome variables.

[35]Moreover, given that the longer the borrower stays in delinquency, the larger the negative effect on their credit score, it would have been costly for borrowers to learn this propensity by remaining delinquent without trying to recover.

[36]In the context of the simple example in the introduction; the servicer's propensity to modify cannot influence whether or not a borrower finds employment.

[37]Note that when I estimate the effect of loan modification on borrowers, I will be in a panel rather than cross-sectional setting. Thus, I will use $Z_{it} = \Lambda_{S_i \times (t - t_0(i))}$ i.e. servicer by time since delinquency fixed effects. Intuitively, this is using the variation that has been documented in Figure 1.2.

[38]One can contrast this setting with that of corporate debt, where a firm may choose whether to borrower

While the exclusion restriction can never explicitly be tested, I provide some reassurance about its satisfaction with a test carried out in Section 1.6.1. In particular, I use the sample of all originated mortgages and show that controlling flexibly for observable covariates, there is little remaining variation across servicers' portfolios in the probability that a loan becomes 90+ days delinquent.

1.4 Data

In order to perform the tests outlined in the previous section, I require mortgage data that satisfies a few key requirements. First, I need to construct T_i, a measure of the number of payments completed by borrowers after they become seriously delinquent. To do so requires—for every borrower—monthly data on whether or not they make their mortgage payment on time. Second, I need to know the identity of the mortgage servicer to construct the instrument Z_i. Third, I will require a way to measure consumption to test for the borrower's response to loan modification. Fourth, the data should include details on when the modification was completed, and how the mortgage contract changed as a result. Finally, the data should provide me with a rich set of covariates to control for observable differences between borrowers in my sample.

1.4.1 Data sources

I use three datasets which satisfy the above requirements. The first two are used to estimate the causal effect of loan modification on the number of monthly payments made by borrowers. The third will allow me to construct the required measures of consumption.

The first dataset is the ABSNet Loan database, which covers over 90% of the loans that provided collateral for private label residential mortgage backed securitizations. This data is compiled using detailed reports from the securitization trustees. They include information about the borrower and the mortgage contract at origination, identify loans that were modified, and describe how they were modified. Moreover, they also include a count, for every month that the loan remains active, of the number of payments missed by the borrower. This allows me construct T_i. Finally, the dataset includes the name of the mortgage servicer. The second and third datasets are the publicly available data on Fannie Mae and Freddie Mac 30 Year Fixed Rate mortgages. These agencies publish data on a subset of the mortgages that reside in their securitizations. Like the ABSNet Loan data, it includes detailed information about the borrowers and contracts at origination, and provides me with a count of the number of payments completed by the borrower while also identifying the mortgage servicer. While these data identify when a loan is modified, the change in the contract has to be inferred from the monthly performance data.

Finally, to analyse the consumption response, I use the McDash Loan Performance Services (LPS) data matched to credit bureau data from Equifax. The LPS data covers about

from public markets or from a bank based on the fact that each channel possesses different monitoring and renegotiation technologies. For example, see 82.

65% of U.S. mortgage originations, with reliable coverage from June 2005 onwards. This data is reported by mortgage servicers who are part of the LPS platform. The dataset contains loans that are securitized (private label as well as GSE, FHA and Ginnie Mae loans) and those held on banks' balance sheets. One disadvantage of this data is that it does not identify the mortgage servicer. To do so, I merge this data with the above two datasets.[39] I use the contract change algorithm of Adelino et al. (2013) to identify modifications from the monthly performance data.

1.4.2 Data restrictions

Mortgage contracts are complicated objects and come in various forms, from the standard 30 Year Fixed Rate Mortgage to more complex products such as adjustable-rate or interest-only mortgages. The parsimony of my framework points me to focus on the 30 Year Fixed Rate mortgage, the simplest of these contracts with a more straightforward repayment structure. In making such a restriction, I wish to minimize the distance between assumptions made in my calculations and the actual nature of cash flows to the investor. Additionally, making this assumption will reduce the false positive and false negative errors of the modification detection algorithm employed in the LPS data.[40] Finally, two of these datasets do not directly identify principal forbearance, i.e., where a portion of the outstanding balance is converted into interest-free debt. Principal forbearance can easily be inferred from data on 30 Year Fixed Rate loans using the standard mortgage annuity formulae.[41]

A number of papers have identified the difference in the rate of modification between loans that were securitized and those that were held on banks' balance sheets, and so I restrict my analysis to loans that were securitized (either in private-label securitizations or securitized by Fannie Mae and Freddie Mac). Furthermore, I restrict my sample to loans that were originated between and including the years 2004 to 2007.[42] I further restrict the sample to loans that went seriously delinquent before 2012. Finally, a loan will enter my analysis when it becomes 90+ days delinquent, i.e., the borrower misses three or more monthly payments.

[39] The procedure for the merge is described in Appendix Section A.2. Also note that I propose an alternative test which does not involve knowing the identity of the servicer.

[40] For example, if for a fixed rate loan I observe a change in contract terms in the monthly mortgage data, it must have arisen due to a renegotiation of the contract term rather than the triggering of an interest-reset in the mortgage contract.

[41] One potential disadvantage of this restriction is that delinquency rates for 30 Year Fixed Rate Mortgages were lower than those for loans with features such as adjustable rates (19). In drawing positive conclusions, restricting attention to a simpler contracting space is beneficial. In drawing normative conclusions however, concerns about external validity to other types of mortgage contracts will caveat the results.

[42] When using data from LPS I restrict to years 2005 to 2007 due to poor availability of data prior to 2005. Additionally, I leave loans that enter the LPS data after more than 12 months from origination to reduce bias from seasoning effects.

1.4.3 Variable construction

1.4.3.1 Number of post-delinquency payments completed

One of the key dependent variables will be the number of payments completed by the borrower following his entry into serious delinquency, i.e., T_i. In order to construct this variable I use the ABSNet Loan, Fannie Mae and Freddie Mac data. This measure is created by keeping a count of the number of payments missed, and subtracting this from the number of months since serious delinquency.

1.4.3.2 Proxy for durable and non-durable consumption

In order to estimate the gains to borrowers, I require a measure of consumption. While I do not directly observe borrowers' consumption levels, I construct proxies for their durable and non-durable consumption using data on the liability side of the borrowers' balance sheet. The proxy for durable consumption is constructed using data on automobile financing accounts as in 58 and 34. If I observe a discrete change in the balance of automobile finance accounts that is greater than \$5,000 and is accompanied by an increase in the count of automobile financing accounts, I record this as an automobile purchase.[43]

To construct a measure of non-durable consumption, I would ideally require data on the monthly payments made by borrowers on debt such as bank cards or other consumer debt. Unfortunately, the data does not include these variables, although I do observe the outstanding balance on these accounts at a monthly frequency. Thus, I follow the methodology of 34 who proxy for non-durable consumption using the measure $NonDur_{it} = \mathbf{1}_{\{Unsecured_{it} - Unsecured_{i,t-1} > \$500\}} \cdot (Unsecured_{it} - Unsecured_{i,t-1})$ for borrower i in month t. I consider the borrower to have increased expenditures on non-durable consumption if I observe the unsecured credit balance recorded in Equifax ($Unsecured_{it}$) increase by more than \$500 in a given month. The use of such a proxy suggests that the estimated response of loan modification on borrower's consumption will be a lower bound on what the true response is likely to be.

1.4.4 Summary statistics

Table 1.1 presents summary statistics for the mortgages in my sample. Panel A displays summary statistics on loans that appear in the ABSNet, Fannie Mae and Freddie Mac dataset, while Panel B presents summary statistics on loans that appear in the LPS dataset. Comparing GSE securitized and private-label mortgages, the loans look broadly similar, with privately securitized mortgages having lower credit scores and higher interest rates.

What is the change of mortgage terms implemented for an average loan modification? Figure 1.3 restricts attention to renegotiated loans and plots the average mortgage terms relative to a year before the loan was modified ($t = -12$ on the x axis). $t = 0$ corresponds to

[43] 34 describe how about 80% of car purchases are financed, with this proportion remaining consistent over time. Moreover, given that my sample consists of delinquent borrowers, I expect this proportion to be higher in this data. Any automobile purchases that are carried out using cash would not be captured by this proxy variable, but they are likely to be small in number.

the quarter in which the loan is modified. The overall effect of the loan modification can be seen in the top left graph; the renegotiation reduces monthly mortgage payments by \$400, on average. This is brought about by changing the three main mortgage terms—interest rate, outstanding balance, and maturity. Interest rates decrease by about 250 basis points following loan modification; outstanding balance increases by about \$6000; and the maturity of the loan is extended by 30 months. Note that the loan modification may involve a principal forbearance wherein a portion of the principal balance will be converted to interest free debt.[44] About 15% of loan modifications involve principal forbearance.

In general, these data suggest that investors trade off increases in principal balances, decreases in interest rates and increases in the mortgage maturity in order to reduce the monthly payment.

1.5 Results

With the main elements of the methodology now established, this section presents the results of the paper.

1.5.1 Do loan modifications result in gains to investors?

As outlined above, the gains to investors can be characterised by combining an estimate of the additional cash flows that result from loan modification with assumptions about discount rates and house prices.

1.5.1.1 Estimating the effect on payments completed following delinquency

The first model I estimate is that of the causal effect of loan modification on the number of monthly payments completed by the borrower following entry into serious delinquency. To build intuition, consider Figure 1.4 which shows the empirical cumulative density function of T_i for two separate groups of delinquent loans—those that were and were not renegotiated. The figure shows that if you are a delinquent borrower who does not receive a loan modification, there is a 10% probability that you make greater than 20 additional monthly payments. However, if you did receive a loan modification, this probability increases to 60%. This pattern is also reflected in the averages shown on Table 1.1.

As described earlier, a naive comparison of these averages will not identify the effect of loan modification. First, such a comparison will not take into account the endogenous selection into receiving a modification. Second, since I only observe loan performance until December 2013, a comparison of the averages will not account for the payments that are completed after this date.

[44] While these loan modifications are not directly identified in the data, I can use the mortgage formulas for the computation of the monthly payments to impute the amount of forbearance. The balance of the mortgage might also increase after modification due to the capitalization of missed payments into the outstanding balance of the mortgage.

To tackle this problem, I estimate the model specified in Equations (1.5) to (1.7), which accounts for both the endogeneity and the right-censoring. To better understand the effects of each of these elements of the data-generating process, I estimate a range of specifications. The results appear in Table 1.2. Column 1 presents the results from a simple OLS regression of T_i on an indicator variable for whether the loan was modified, loan level covariates, CBSA fixed effects, time of delinquency fixed effects and originator by agency fixed effects.[45] This specification ignores both the endogeneity of treatment and the right-censoring - hence I refer to it as the naive estimate. The OLS estimate suggests that modification will result in 19 additional monthly payments made by the borrower. In Column 2 I repeat this analysis with CBSA by Time of Delinquency fixed effects, and demonstrate that the result in Column 1 is not biased by time varying CBSA level unobserved heterogeneity.

In Column 3, I present results from a censored regression framework, which accounts for the right censoring in the dependent variable but not for the endogenous selection into loan modification. The coefficient on $Modify_i$ is 35, however the appropriate statistic to compare to the estimate in Column 1 is the average partial effect, which here will be 27. In other words, this specification tells us that loan modification will increase the number of payments completed by 27. As expected, the right censoring has biased my naive estimate downwards.

In Column 4, I move back to a least-squares linear regression specification which ignores the right-censoring but now accounts for the endogenous selection into treatment. This specification implies that loan modifications lead to 38 additional monthly payments from the borrower. Failure to account for endogenous selection biases the naive estimate (Column 1) downwards. In other words, the selection bias is negative and the counterfactual expected number of payments completed by borrowers who received a loan modification will be lower than the expected payments completed by those who did not. The direction of the bias suggests that servicers chose to modify loans of those borrowers who would really have struggled to complete additional monthly payments without a renegotiation. If the bias went the other way, it would indicate that they renegotiated mortgages which were more likely to have self-cured in the absence of a modification.[46] In Column 5, I repeat the analysis with CBSA by time of delinquency fixed effects, and show that it is robust to controlling for all CBSA level time-varying heterogeneity.

Finally, in Column 6, I estimate the model which accounts for both the endogeneity in the decision to renegotiate, and the right censoring in the data-generating process. The β coefficient in Equation (1.5) is estimated to be 73.7. The resulting average partial effect reflects that, on average, renegotiation of the mortgage leads to 56 additional monthly payments from borrowers who become 90+ days delinquent. Note that the censoring framework takes into account that although a large number of borrowers re-default following entry into serious delinquency, there still are those who continue to make a large number of monthly payments. The nature of loan-level mortgage data precludes the observation of these additional payments which they would complete. It is crucial to correctly quantify these payments as they

[45] Here agency simply refers to whether the loan was in a Fannie Mae/Freddie Mac securitization, or whether it was in a private-label securitization.

[46] There is suggestive evidence of this in the data, with selection into modification on observables such as credit score being negative.

represent monthly cash flows to investors for interest and amortization of principal.

Next using (1.8) and (1.9), I construct estimates of $\widehat{T_{i,Mod}}$ and $\widehat{T_{i,NoMod}}$, the expected number of payments completed by a delinquent borrower with and without renegotiation, respectively. Figure 1.5 below plots the densities of these constructed measures. On average, the difference between the means will be approximately 60 monthly payments, which is close to the Average Partial Effect estimated above. These estimates allow me to compute the gains to investors from renegotiation.

1.5.1.2 Imputing loan modifications for non-modified loans

In order to estimate the gains from loan modification, I need to construct, for loans that did not get renegotiated, an estimate of the counterfactual change in monthly payment as if they had been renegotiated. In order to do so, I follow the procedure outlined in Section 1.3.2. Essentially, I estimate a series of regressions on loans with $Modify_i = 1$, and having estimated the parameters of these specifications, use predicted values from them to impute the counterfactual change in interest rate, outstanding balance, remaining term, and principal forbearance for those loans with $Modify_i = 0$. In Table 1.3, I report summary statistics on the distribution of $\frac{d+\Delta}{d}$, the ratio of post-modification payments to pre-modification monthly payments. The first row presents summary statistics for loans that were not modified, for which this quantity has been imputed. The second row presents summary statistics on modified loans as they appear in the data. The table demonstrates that the two distributions appear to be similar. Using these inputs, I compute $\widehat{\frac{V(\Delta)-V(0)}{D_1}}_i$, the loan level gains from loan modification to investors.

1.5.1.3 Estimating the gains to investors

Before I use the components computed thus far to measure the gains from mortgage renegotiation, I require three additional assumptions. First, I compute house prices as at the date of delinquency, P_1 , by applying CBSA level, or state level, house price indices from the Federal Housing Finance Agency (FHFA) to the property value at origination. Second, I assume that the foreclosure discount is $\phi = 1 - 0.27 = 0.73$, following 27.[47] And finally, I assume that the annual discount rate will be based on the prevailing 30 Year Fixed Rate Mortgage rate in the FHFA's monthly Mortgage Interest Rate Survey. Figure A.1, in the Appendix, shows the time series of the assumed discount rate. Given these assumptions, I am able to compute the gains to investors as depicted in Equations (1.3) and (1.4).

I form an estimate of these gains, $V(\Delta) - V(0)$, at the loan level and normalize it by the balance outstanding as at first entry into serious delinquency, D_1. The sample mean and standard deviation of each component of the gains and of the total gains are represented as bars and vertical lines in Figure 1.6. In estimating the standard deviation I take into account the fact that investors observe borrower characteristics, including where they are

[47]Later I present a robustness check to this assumption where I show the results assuming perfect foresight house prices. I also perform a sensitivity analysis on the assumption of ϕ, which appears in the Appendix.

located and when they went seriously delinquent. Therefore, I estimate the conditional standard deviation of each component of the gains.[48]

The first component ΔPV(Interest from PMTs) represents the amount that investors earn in interest as borrowers continue to make additional payments every month following mortgage renegotiation. It has a mean of 20.2% of the balance as at 90+ days delinquency and a standard deviation of 6%. As borrowers continue to make payments principal is amortized and gains to investors from this component (ΔPV(Principal from PMTs)) are represented by the second bar. This component has a mean of 8.4% of the balance as at 90+ days and a standard deviation of 2%. Variation across the sample in these components arises from differences in the expected number of payments completed ($T_{i,Mod}$, $T_{i,NoMod}$) and differences in the original and renegotiated terms of the mortgage.[49]

The third component (ΔPV(Termination)) estimates the expected value to investors from the terminal cash flow from a modified loan relative to one that does not get renegotiated. This component too is estimated at the loan level. For example, consider a borrower who makes 56 additional monthly payments as a result of receiving a loan modification. After these 56 months the borrower may either redefault and enter foreclosure or he can prepay the outstanding balance.[50] Thus loan modification extends the life of the mortgage and delays termination of the contract. The -25% mean of this component largely represents this time value of money cost of delaying the recovery of the outstanding balance. The principal recovered after the borrower makes these additional payments will differ from that recovered upon immediate foreclosure due to the rate at which the modified loans amortize.[51]

Overall investors expected gains from loan modification of only 3.5% of the outstanding mortgage balance at 90+ days delinquency. The standard deviation around this mean is substantially larger at 12.5%, which is 3.6 times the mean.[52] The average balance as at 90+ days delinquency in the sample is \$202,700, implying average gains of \$7095. The estimate is smaller compared to the one based on realized losses in 64. The difference in my estimates would arise from the fact that I consider the present value of expected gains rather than realized losses. Additionally, I adopt a different approach to estimation, and explicitly account for the right censoring in the data generating process when computing these gains.

When renegotiating a mortgage investors expose themselves to borrower-level variation as well as spatial and business-cycle variation. The standard deviations reported above represent borrower-level variation within a CBSA at a particular point of time in the business cycle. To contextualize the within borrower standard deviation of 12.5% I contrast it to across

[48] I do so by estimating the regression $\widehat{Y_i} = \alpha + X_i^{'}\beta + \gamma_{ct} + \epsilon_{ict}$, where Y_i is one of the components described above. Then, I compute the standard deviation of the residuals $\widehat{\epsilon}_{ict}$.

[49] Appendix Figure A.2 further decomposes this quantity to show that the smaller monthly payments (Δ in the framework) cost the investors about 14% of the balance at 90+ days, but the continuation of payments over a longer period of time helps them recover 43% of the balance at 90+ days.

[50] Here I assume that if a borrower's LTV at the end of making these additional monthly payments is below 90% he will be able to refinance or else he will enter foreclosure. The right-censoring in the data necessitates this assumption. In the data, I observe that 30% of loans that are modified will enter foreclosure within 4 years.

[51] Appendix Figure A.3 further decomposes ΔPV(Termination) to show that the time-value-of-money effect dominates.

[52] The unconditional standard deviation is about 20%.

CBSA by time variation. I estimate the regression $((\widehat{V(\Delta) - V}(0)_i = \alpha + X_i'\beta + \gamma_{ct} + \epsilon_{ict})$ and plot the resulting $\widehat{\gamma_{ct}}$ in a histogram. I also plot the density of $\widehat{\epsilon}_{ict}$. The results appear in Figure 1.7 and show that across CBSA by Time variation is around 7%. Investors were exposed not only to variation in expected gains across borrowers but also to variation across geographies

1.5.2 Do borrowers gain from debt renegotiation?

From the perspective of the investors, the estimated gains do not appear to justify regulators' enthusiasm for debt renegotiation. However, stopping the analysis here will leave us with outstanding questions about the other side of the mortgage contract, the borrowers. Was the failure to complete a renegotiation costly for borrowers? Or, did they too not wish to participate in loan modifications? While it is true that most delinquent loans that did not get renegotiated ended up in foreclosure, it is unclear whether this helped the borrower by allowing him to forego all further debt payments, or whether it hurt him by imposing the costs of being in default. As the framework makes clear, the gains from modification will be reflected in borrowers' consumption changes. Hence, I test for the effect of loan modification on durable and non-durable consumption of borrowers.

As a first test, ignoring endogeneity from selection into loan modification, Figure 1.8 presents results from the event study specification described in Equation (1.12). I test for the effect of loan modification on four variables, clockwise from the top right; dollars spent on automobile purchases, an indicator variable equal to 1 if an automobile was purchased in a given time period, the proxy for non-durable spending $NonDur_{it}$, and the credit score of the borrower. A time period consists of a 6-month long time interval. Each graph plots the coefficients $\{\beta_{2t}\}_{t=-4}^{4}$ from the event study, where $t = 0$ is the time of loan modification. The coefficient for $t = -1$ has been normalized to zero to facilitate interpretation.

Prior to renegotiation the automobile purchases of borrowers who did receive a loan modification look very similar to those of the control group. None of the coefficients for $t < -1$ are significantly different from zero. When the loan is renegotiated at $t = 0$, constraints on borrowers begin to be lifted, evident in the increase in consumption. 6 months after the loan modification, borrowers purchase \$61.6 per month of automobiles over and above the amount purchased by those who do not receive a modification. This increase is also reflected in the top right graph, which shows the effect of renegotiation on the probability that a borrower purchases an automobile. 6 months after renegotiating his mortgage the borrower is 0.33% more likely to purchase a car in a given month relative to a borrower who has not received the modification. Together these estimates suggest that conditional upon purchasing an automobile a borrower with a modified loan spends about \$19,000.[53]

It is particularly striking that the two groups of borrowers look similar to each other right until the point at which they receive their loan modification. The sharp response of durable purchases to renegotiation points to a substantial relaxation of a delinquent borrower's liq-

[53] This is between the average price of a used car (\$15,300) and the average price of a new car (\$32,000) as per industry sources in 2013.

uidity constraint.[54] While it may be true that those who do not receive a modification and simply enter foreclosure will stop making monthly payments, they do not appear to be in a position to smooth consumption. If this were not the case, I would expect to see no effect of loan modification on consumption.

To test for whether renegotiation changes consumption of goods other than automobiles, I turn to the data on the borrower's unsecured debt balances. $NonDur_{it}$ measures discrete increases in the monthly balance that are greater than \$500, and aggregates these increases over a 6 month interval. The event study in the bottom right graph demonstrates that those who did not receive modifications were more likely to run up unsecured balances, as evidenced by the $t < -1$ coefficients below zero. However, following renegotiation, borrowers are able to tap into unsecured credit markets and increase non-automobile consumption as well.

Borrowers can overcome liquidity constraints and smooth consumption either by consuming out of the decrease in monthly payments or through improved access to credit markets. Remaining in delinquency and entering foreclosure can harm credit scores which are the basis for a wide variety of secured and un-secured lending. The event study shows that although the effect of modification on the credit scores is modest it does appear to halt their decline.

While the event-study results are informative in providing evidence that gains do exist for borrowers, and lend insight into when these gains start to be realized, they are still subject to the critique that there will be endogenous selection into renegotiation. To overcome this critique, I estimate the response of borrowers using the specification laid out in Equations (1.13), (1.14), and (1.15). I estimate the specification on the four variables considered above. The results appear in 1.4.

Panel A and Panel B show OLS and two-stage-least squares results for the indicator for automobile purchase and for dollars spent on automobiles, respectively. Panel C presents results on Non-Durable consumption, and Panel D on the credit score. For each dependent variable I estimate a number of specifications to show that the results are robust to the inclusion of fixed effects and controls for various levels of heterogeneity. Column 2 of each panel includes as dependent variables ($Modify_i$ and $Modify_i \cdot \mathbf{1}_{t>t_m(i)}$). Column 3 adds county by time fixed effects and time since delinquency fixed effects to control for time-varying unobserved heterogeneity at the county level. In Column 3, I also add control variables as well as linear trends interacted with control variables to account for the possibility that borrowers with different levels of original monthly payments, for example, may have had consumption that trended differentially over time. Column 4 adds originator by agency fixed effects to soak up any variation across borrowers which is correlated with borrower quality as reflected by underwriting standards of the mortgage originator. Finally Column 5 implements instrumental variables approach using two stage least squares.[55]

Overall, the two stage least squares (2SLS) estimates confirm the findings of the event study and OLS estimates. The higher 2SLS estimates indicate that, similar to the results on the effect of loan modification on the number of monthly payments completed, the naive

[54] A large literature has tested for the effects on consumption of shocks to the borrower. See 17, 53, 76 for some recent examples outside the context of mortgage markets.

[55] Note that I used the matched sample for Column 5, hence the reduction in sample size.

OLS estimate is biased downwards. It appears to be that servicers modify the loans of those who are least likely to self-cure or recover from the delinquency, and thus face the most difficulty in smoothing consumption following entry into serious delinquency. Durable consumption is predicted to increase by \$72 a month following debt renegotiation, and non-durable consumption by \$43 a month, implying a total effect of \$115 per month.

Earlier, I estimated that the borrower completes 56 additional monthly payments as a result of loan modification. Hence I can estimate the total gain to borrowers as $W(\Delta) - W(0) = 56 \times 115 = \$6440$. This is the total ex-post gain to the borrower. In present value terms this will amount to \$5,750 which amounts to 2.8% of the borrower's outstanding balance as at entry into 90+ days delinquency.[56] While the gains are small relative to the outstanding balance, perhaps the more appropriate comparison is to estimates from the household finance literature that compute borrowers' propensity to consume out of decreases in monthly obligations due to ARM resets (34, 58) or refinancing (12).

My estimates show that borrowers consume 32 cents of every dollar decrease in monthly payment, which is larger than estimates in the literature cited above. This is consistent with studies showing that more liquidity constrained individuals should have a larger response to income shocks (for example 93). The borrowers in my sample are particularly constrained because they are seriously delinquent and thus not able to refinance their loans or easily access other credit markets. The ability to renegotiate their debt is particularly helpful as they would not have been eligible for post-crisis refinancing programs, such as the Home Affordable Refinancing Program, due to their poor credit records. As 43 and 33 show, this group of borrowers was also unlikely to have benefited from quantitative easing.

1.5.3 Summarizing the results

So far, the literature has argued that the presence of contracting frictions within the securitization chain hampered loan modifications, taking as given that, at least on average, investors and borrowers both wanted to participate in rewriting the contract. However, even in a frictionless setting, a modification will not be completed unless there are sufficient expected gains from renegotiation relative to foreclosure for both sides of the debt contract. There has been little work done to understand these expected gains. Since insufficient gains to *any* one side of the contract are enough to preclude renegotiation, I study both investors and borrowers.

My results show that while borrowers gain from loan modification relative to foreclosure by being able to better smooth consumption, there do not appear to be substantial gains available to investors. They only recover 3.5% more of the outstanding balance, on average, relative to foreclosure and these gains have a high variance both due to borrower level heterogeneity within geographies, and due to spatial variation across geographies. Investors benefit from the renegotiation as the borrower continues to make his now lower monthly payments, but are hurt as the interest rate reduces dramatically to try and keep the borrower

[56]Assuming an annual discount rate of 4.9%. I find this discount rate by finding, for each delinquent loan, the average FHFA MIRs rate at the time of first entry into serious delinquency, and then taking an average over the sample.

current. Thus, the mortgage takes longer to amortize and investors do not appear to be appropriately compensated for this.

These limited gains to investors would have hindered mortgage renegotiation despite government's efforts to promote them. As I have found, there is a wide distribution of these gains, and agency problems may still have had a negative effect on the rate of loan modification. However such frictions would not be the only reason we see a subdued response to government intervention. Programs such as HAMP subsidized the servicer and compensated them for the costs of engaging in loan modification. However, since expected gains to investors were limited this likely did not translate into a large increase in the rate of loan modification.

1.6 Robustness Checks and Extensions

1.6.1 Investigating the exclusion restriction

In order to successfully apply the instrumental variables approach (described in Section 1.3.4), the exclusion restriction must hold true. The exclusion restriction asserts that the servicer's loan modification strategies affect borrower level outcomes only through their effect on whether the loan gets modified. In other words, it asserts that there is no correlation between servicer fixed effects (the instrument) and unobservable variation in borrower level outcomes. While the exclusion restriction cannot be directly tested, in this section I propose a test which can at least reassure me of its satisfaction. The test revolves around the idea that although my analysis has been performed on loans that become 90+ days delinquent I still have in my dataset 30 Year Fixed Rate Mortgages that did not become seriously delinquent. Hence, I can test for whether there are differences between the portfolios of various servicers in the probability that their loans are becoming seriously delinquent.

A quick glance at the characteristics of the portfolios of the servicers in my sample demonstrates that we might expect them to perform differently. After all, some of them specialized in subprime segments of the market (e.g. Ocwen, Countrywide), while others serviced mostly prime loans (e.g. J.P. Morgan Chase). These observable differences among servicers do not pose a challenge to identification because they can be controlled for. The remaining concern will be that loans across servicers' portfolios are substantially different after controlling for a rich set of observables. Studying borrowers' entry into delinquency is one way to get a sense, at least ex-post, of the quality of the servicer's portfolio.

This formulation of this test uses a propensity score matching method. First, I select a particular servicer, Wells Fargo for example, and estimate a probit model where the dependent variable is an indicator for whether a loan was serviced by Wells Fargo.[57] I then form propensity scores using the estimated probit model, i.e., the propensity score predicts whether a loan in another servicer's portfolio was similar to one in Wells Fargo's portfolio.

[57] In the probit specification, I include as control variables a spline for loan amount, credit score, interest rate, LTV, and the change in house prices over the year prior to origination of the mortgage. I also include an indicator variable for whether the loan was a private label securitized loan, indicator variables for various loan purposes, CBSA fixed effects, and origination date fixed effects.

After forming a sample of Wells Fargo loans matched to other servicers' loans (by keeping non-Wells Fargo loans with propensity scores in the highest quartile of the distribution) I perform a t-test comparing the probability that a Wells Fargo loan entered serious delinquency within 36 months of origination to the probability that a loan from the matched sample entered serious delinquency within 36 months of origination. Standard errors are clustered at the state level. I then repeat this for each of the top 15 servicers in the sample. I report the results of each t-test and the associated 95% confidence intervals in Figure 1.9.

I divide this figure into two panels; the panel on the left shows the servicers who satisfy this robustness check while the one on the right shows those who don't. On the x axis I show the market share of a given servicer. As can be seen, those servicers that do have portfolios that look different from their competitors only hold a small portion of the market share. All my results go through once I drop these servicers. On the left hand side panel I also plot the average rate of entry into 90+ days delinquency in the sample of 14%, highlighting that even if these servicers' portfolios are statistically different from their competitors, the size of this difference appears economically insignificant.

1.6.2 Do borrowers who consume more also re-default more?

The interpretation of the results on the effect of loan modifications on borrower welfare rest on the assumption that increase in consumption corresponds to an increase in welfare. This interpretation may be undone if borrowers who receive loan modifications and then consume more are riskier, or behaving recklessly. In other words, if higher post-modification automobile purchases predict higher rate of entry into redefault, one would be hard pressed to interpret this increase in consumption as being entirely welfare improving.

Suggestive evidence indicates that this is not the case. There is a weak or no correlation between post-modification purchase of an automobile and subsequent re-entry into delinquency. Table 1.5 shows results from a regression of an indicator variable for mortgage redefault following modification on an indicator variable for whether a borrower with a modified loan made an automobile purchase following renegotiation. The results allay concerns that I might be mis-interpreting the results on the response of individual borrowers to loan modification.

1.6.3 Measuring the consumption response using within borrower variation

The instrumental variables approach to estimating the consumption response of borrowers rests on the key assumption of the exclusion restriction which, if violated, would cast doubts on the results. While the test above helps reassure one of the validity of the instrument, I perform one additional robustness test in the context of estimating the gains to the borrower.

The panel structure of the data affords me the opportunity to use an alternative methodology, i.e., restricting analysis to loans that received modifications and using borrower-fixed-effects to soak up time invariant heterogeneity at the individual borrower level. While this method will not correct for selection into loan modification, it will help me understand the extent to which the OLS coefficients may have been biased due to omitted variables.[58] Now identification is obtained using time series variation *within* each individual borrower. More specifically, the estimating equation employed for the event study will be:

$$Y_{ict} = \eta_{ct} + \gamma_i + \psi_{(t-t_0(i))} + \beta_1 X_{it} + \sum_{k=-4}^{4} \beta_{2k} Modify_i \cdot \mathbf{1}_{t=t_m(i)+k} + \epsilon_{ict} \tag{1.18}$$

where γ_i represents the borrower level fixed effects. The advantage of this approach is that all borrower level heterogeneity that does not vary with time is absorbed by the individual fixed effect. This would capture variation from factors such as pre-delinquency household income or unobserved liquidity constraints. Figure 1.10 presents the results of these event studies.

It is reassuring to see that the magnitude of the auto-mobile purchase response is similar to that obtained using 2SLS. Similar to the OLS event study, I see that the change in consumption occurs precisely at the time at which the borrower's loan is renegotiated. The estimated response of credit score is higher in this empirical setting, perhaps suggesting that in the OLS specification some portion of borrowers who did not receive a loan modification began to self cure and saw an increase in credit score. Within borrower effects on the non-durable consumption proxy demonstrate a pre-modification trend. It appears that these borrowers increasingly rely on unsecured credit markets as they get deeper into their delinquency. However, there is a distinct break in this relationship at $t = 0$.

1.6.4 How does consumption response vary by modification type?

Past studies of mortgage renegotiation have studied the effect of changes in specific loan terms due to renegotiation on the performance of loans, measured as the rate of entry into redefault (88, 48). My data give me the ability to study their effect on borrower's consumption response to loan modification. One of the challenges of doing so arises from the fact that multiple contract terms can change simultaneously, each having a different effect on consumption.[59] Thus, in testing for the effect of a change in monthly payment, I control for the change in the outstanding balance. Similarly, to test for the effect of an increase in the outstanding balance, I compare loans that received a similar decrease in the monthly payment.

In this section I restrict attention to loans that were renegotiated, and investigate the consumption response to two dimensions of the modification—percentage changes in the monthly payment ($\Delta PMT_i = ln(PMT_{i,PostMod}) - ln(PMT_{i.PreMod})$) and the principal outstanding ($\Delta Bal_i = ln(Bal_{i,PostMod}) - ln(Bal_{i,PreMod})$). I consequently standardize these measures of relative changes ($\Delta PMT_{i,z} = \frac{\Delta PMT_{i,z} - \Delta \bar{PMT}}{Var(\Delta PMT)}$ and $\Delta Bal_{i,z} = \frac{\Delta Bal_{i,z} - \Delta \bar{Bal}}{Var(\Delta Bal)}$). I then augment Equation 1.18 and regress an indicator variable for whether an automobile

[58] It does not correct for selection because by restricting the analysis to loans that were modified, I now have a sample selection problem.

[59] For example, the average loan modification resulted in a decrease in the monthly payment and an increase in the balance.

was purchased in a given 6 month interval on indicator variables for each time period before the loan modification, and indicator variables for each period post-modification that are interacted with the standardized measures.

The results appear in Figure 1.11 and show that controlling for a change in the outstanding balance, a larger decrease in the monthly payment is correlated with a stronger consumption response. Surprisingly, controlling for changes in the monthly payment, an increase in the principal balance correlates with a larger probability of an automobile purchase. This speaks to the role of principal forbearance in generating gains to borrowers. 38 derive this to be the optimal loan modification as it backloads the repayment of the mortgage, allowing borrowers to better smooth consumption.

1.6.5 Heterogeneity in gains to investors

I find that gains to investors were small with a standard deviation of gains almost four times larger than the sample mean. As Table 1.1 shows, there is variation across mortgages on dimensions such as credit score and loan size, along with variation in the location of borrowers. To better understand what drives these gains, I estimate them across various subsets of my sample. This approach will also reveal how robust the main result is to the homogenous treatment effect assumed in Equation (1.5).

I divide the sample into groups based on quartiles of credit score, loan size, and change in collateral value (estimated using FHFA house price indices) between origination and first date of entry into 90+ days delinquency, and estimate the gains to investors from loan modification for each group. The results appear in Appendix Table A.2. Overall, the estimate of the standard deviation of gains is similar across groups. Although, the larger the decline in house prices from origination to 90+ days delinquency (Panel B, Column 1) the lower is the variation in these gains. At the same time the average gains from modification are the largest for this group, potentially due to a lower ϕP_1 as per my framework. These results suggest the importance of liquidation values in determining the gains from modification. Loan modifications have larger and less variant benefits for those delinquent borrowers who have experienced a large decline in house prices. This is consistent with the higher probability that such borrowers receive a loan modification.

1.6.6 Sensitivity to assumptions

Estimating the gains to investors involved making assumptions about some of the parameters in my model. To assess how sensitive the results are to these assumptions, I re-estimate the gains to borrowers under a series of alternative assumptions. The results appear in Appendix Table A.1. Columns 1 to 4 present the mean and conditional standard deviation of the various components of the gains to loan modification. Column 5 presents across CBSA by time of delinquency variation in the gains to investors (the mean of which appears in Column 4). I first assess the sensitivity to the assumption on ϕ. Then, I compute gains under the assumption of perfect foresight in house prices. I also estimate these gains by explicitly accounting for foreclosure timelines. Finally, I change the threshold which has been incorporated in the $G(P, D)$ function of Section 1.2 to generate a higher implied rate of mortgage redefault. As the results show, the changing these assumptions will affect the mean of the gains to investors in the sample. However, the conditional standard deviation of the gains always remains in the 10% to 17% range.

1.7 Conclusion

As the collapse of house prices turned into a widespread economic downturn, more and more borrowers began to become delinquent on their mortgages. To combat this debt overhang, various regulators and government agencies poured resources into renegotiation of contracts. Their efforts, however, were met with a muted response from participants in the mortgage market.

Agency problems in the securitization chain are among the main reasons proposed for this response (8, 78, 59) along with restrictive contracts faced by mortgage servicers (63, 90, 62). I highlight that the decision to renegotiate will depend primarily on the availability of sufficient gains from modification relative to foreclosure to both sides of the mortgage contract. Insufficient expected gains to a single party can be enough to preclude renegotiation. In this paper, I estimate and characterize these gains to investors and borrowers.

The challenge in doing so arises because loan modification is not randomly assigned to borrowers. There are observable and unobservable differences between borrowers who receive loan modifications and those who do not. A simple comparison of these two groups which fails to account for this will result in a biased estimate of the expected gains. Therefore, to identify these gains I develop an estimation framework which exploits variation in the propensity of intermediaries to modify loans. Crucially, I rely on reduced form specifications that use as dependent variables the outcome of individual borrowers' decision-making on how many monthly payments to complete and how much and when to consume. Notably, when making these decisions, borrowers are unlikely to take into account how their servicer would be different from others.

I find that through modification of a mortgage investors expect to recover, on average, 3.5% more of the outstanding balance as at 90+ days delinquency relative to what they might expect to recover through the foreclosure process. The uncertainty about realizing these gains is highlighted by their 12.5% standard deviation. Once a loan is renegotiated, borrowers continue to make monthly payments of interest and principal and maintain mortgage amortization. However, as a result of the modification, interest rates paid by borrowers decrease by an average of 250 basis points, thus imposing a cost to investors. The loan modification extends the period of time over which principal is repaid but does not compensate investors sufficiently for doing so.

Borrowers, on the other hand, would not resist the loan modification. Renegotiation is accompanied by a sharp increase in durable consumption (measured as automobile purchases) and a slower increase in consumption using unsecured credit. Borrowers consume out of the decrease in monthly payments and do not lose access to credit markets which allows them to overcome liquidity constraints and smooth consumption.

The results show that if gains to investors are insufficient and uncertain servicers would be unwilling to renegotiate loans on their behalf. Despite their being substantial gains to borrowers, renegotiation would often not be completed. Hence, explanations for the subdued response to government intervention do not have to solely rely on agency problems.

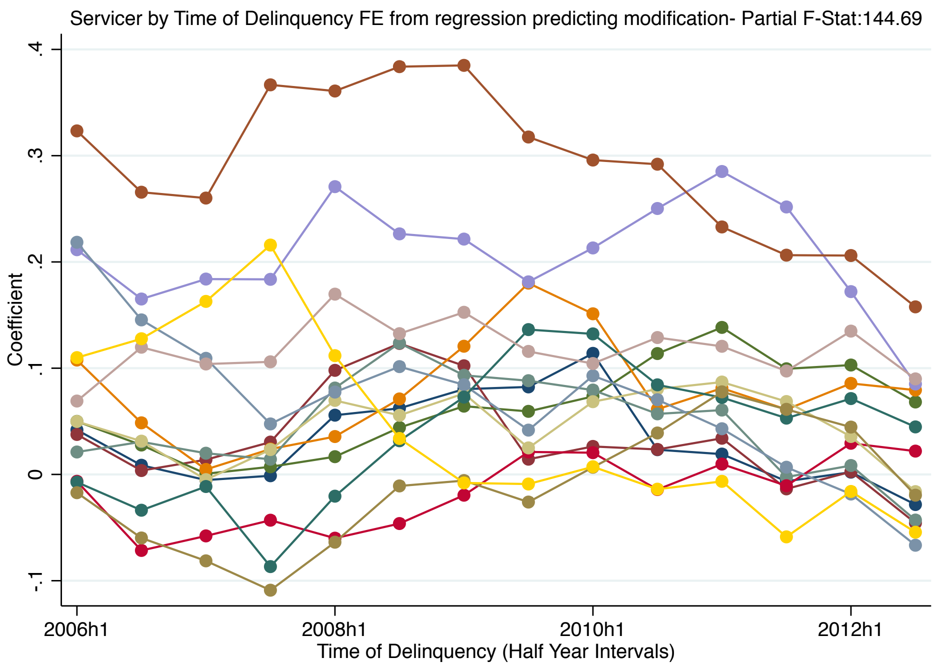


Figure 1.1: Servicer by Calendar Time of Delinquency Fixed Effects

The graph above displays plots, for every servicer s, the time series formed by the coefficients $\beta_{0,s,t}$ estimated from the regression $Y_{ict_0(i)} = \alpha + \sum_{s \in S} \sum_t \beta_{0,s,t} \mathbf{1}_{Servicer=s \text{ and } t_0(i)=t} + \beta_1 \cdot X_i + \gamma_c + \eta_{t_0(i)} + \epsilon_{ict_0(i)}$. The coefficients $\beta_{0,s,t}$ are those on the servicer by Time of Serious Delinquency Fixed Effects from a regression where the dependent variable is equal to 1 if loan i that became delinquent at time $t_0(i)$ is modified at any point in its subsequent loan history. The loans used in the estimation are 30 Year Fixed Rate mortgages from the ABSNet Loan Private Label Securitization Data and the Fannie Mae and Freddie Mac publicly available 30 Year Fixed Rate Mortgage data. The sample is further restricted to loans that become 90+ days delinquent at some point in their history.

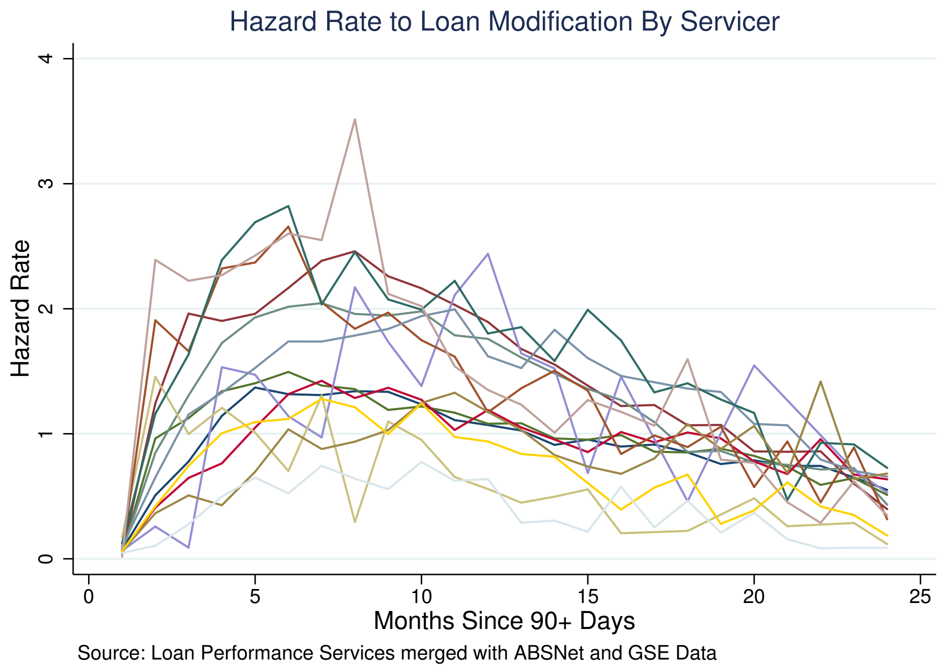


Figure 1.2: Servicer Specific Baseline Hazard Rate from Proportional Hazards Model of Loan Modification

The graph above displays plots, for every servicer s, the baseline hazard function from a proportional hazard model estimated using maximum likelihood. Loans enter analysis when they become 90+ days delinquent. Failure in the hazard model is specified to be the entry of a delinquent loan into a completed renegotiation. Loans that prepay, self-cure, or enter foreclosure are assumed to be censored. The loans used in the estimation are 30 Year Fixed Rate mortgages from the ABSNet Loan Private Label Securitization Data and the Fannie Mae and Freddie Mac publicly available 30 Year Fixed Rate Mortgage data that have been successfully merged with McDash Loan Performance Data so as to obtain accurate information on entry of the loan into foreclosure.

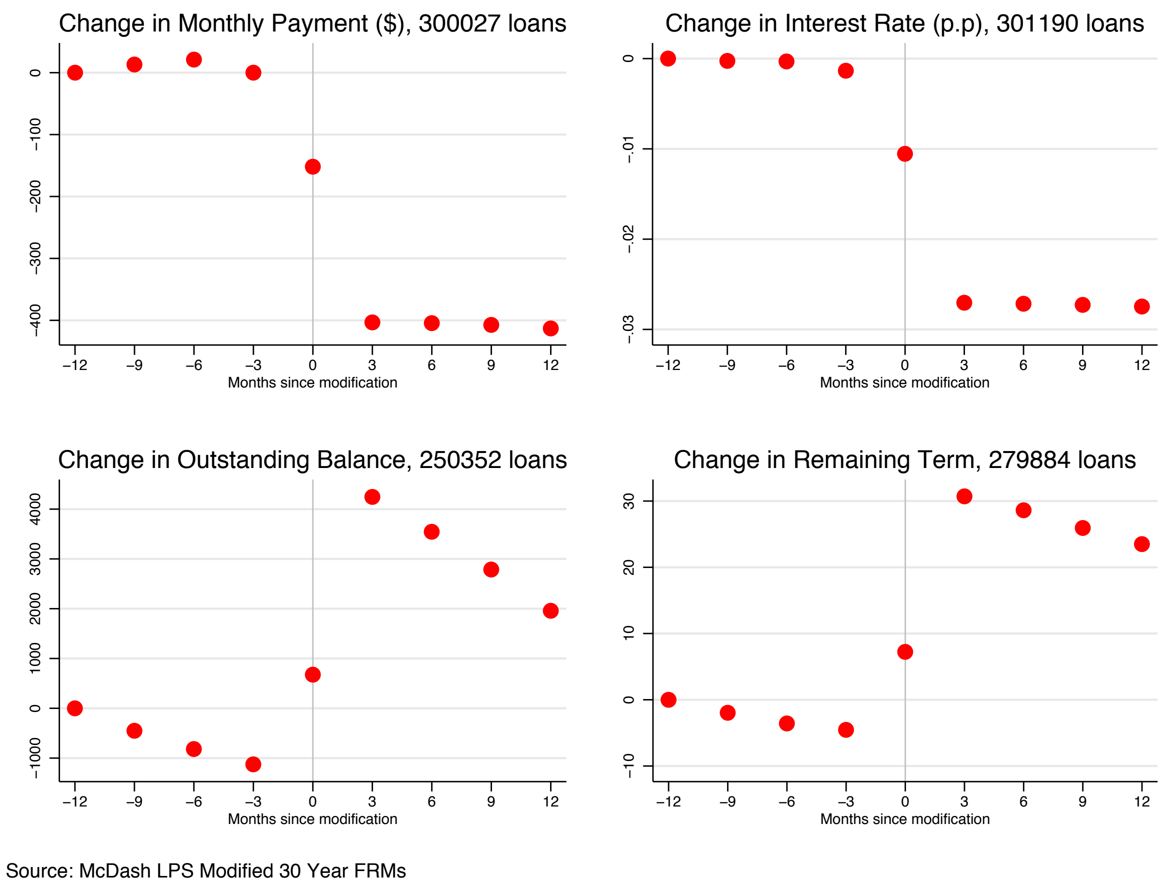


Figure 1.3: Average change in mortgage contract terms

This figure shows the change in mortgage contract terms before and after loan modification. The loans used in the estimation are 30 Year Fixed Rate mortgages from McDash Loan Performance Services Data. that become modified. Mortgage terms plotted include (clockwise from top left) monthly principal and interest payment, interest rate, outstanding principal balance and remaining mortgage term. Each plot normalizes the loan term as at 4 quarters before loan modification to 0, and plots the average loan term for 3 quarters prior to and 3 quarters after loan modification. Note that due to the aggregating of monthly performance data into quarterly intervals, the adjustment of the loan term following loan modification is not instantaneous at time 0, but the full effect manifests itself by quarter 1.

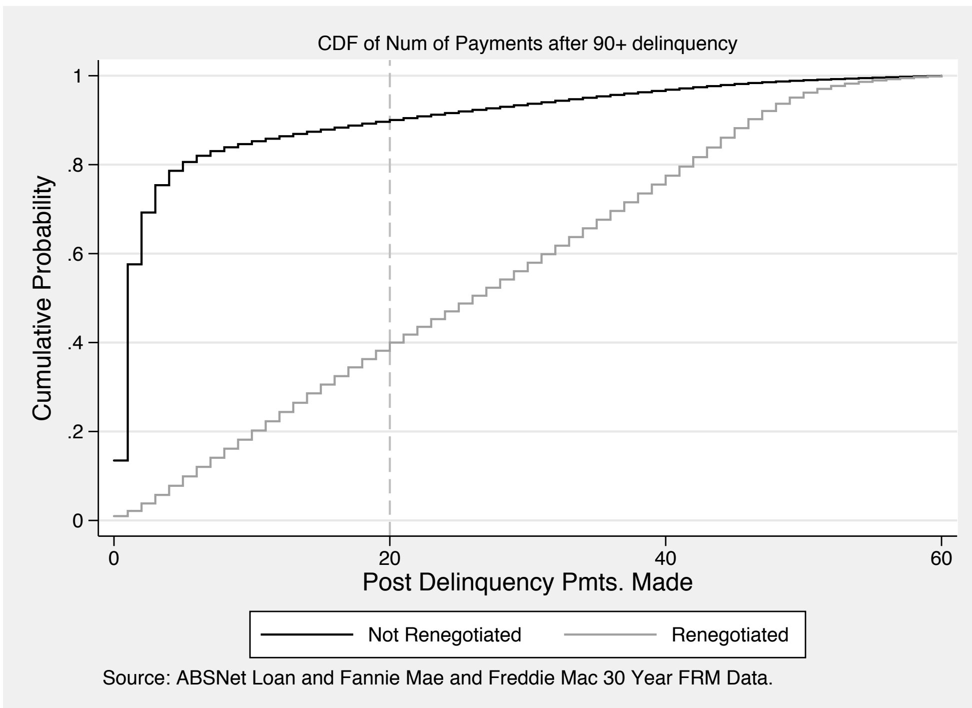


Figure 1.4: Empirical Cumulative Distribution Function of No. of Payments Made After Delinquency

This graph plots the empirical cumulative distribution function of the variable "Number of completed monthly payments following 90+ days delinquency". The sample used is 30 Year Fixed Rate mortgages from the ABSNet Loan Private Label Securitization Data and the Fannie Mae and Freddie Mac publicly available 30 Year Fixed Rate Mortgage data. The sample is further restricted to loans that become at least 90+ days delinquent. The solid line plots the empirical CDF of this variable for loans that are not modified. The dashed line plots the empirical CDF of this variable for loans that are eventually modified.

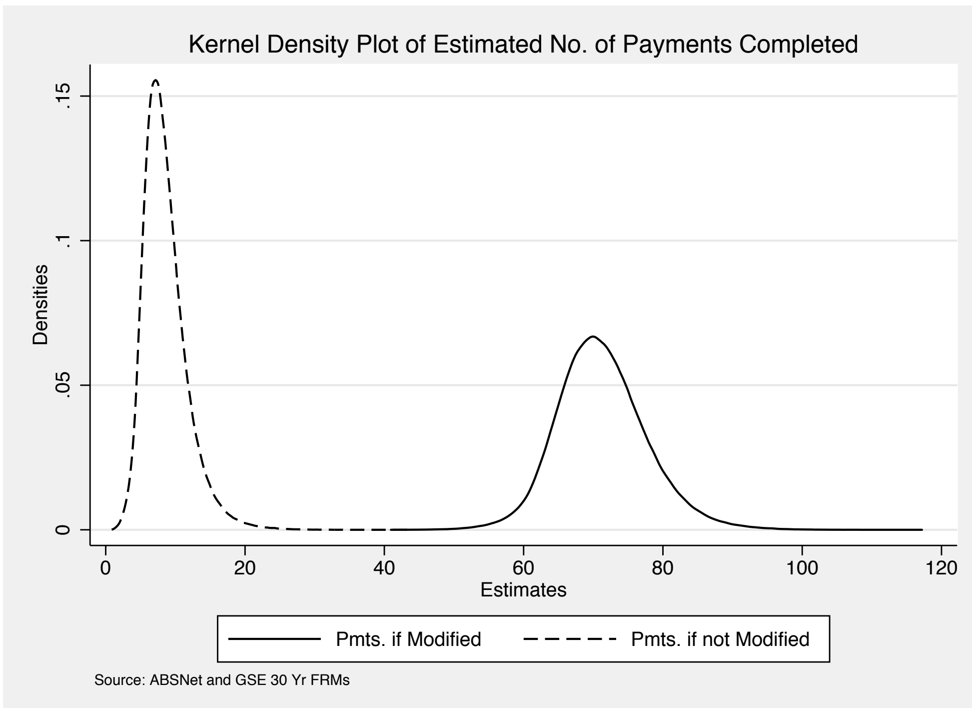


Figure 1.5: Distribution of $\widehat{T_{i,Mod}}$ and $\widehat{T_{i,NoMod}}$

This graph plots the empirical kernel density estimates of the predicted number of payments completed based on whether the loans would modified or not modified. $\widehat{T_{i,Mod}}$ and $\widehat{T_{i,NoMod}}$ are the predicted values from the estimated structural equation which takes into account both the endogeneity of selection into treatment on unobservables and the right censoring inherent in the data. The sample used is 30 Year Fixed Rate mortgages from the ABSNet Loan Private Label Securitization Data and the Fannie Mae and Freddie Mac publicly available 30 Year Fixed Rate Mortgage data. The sample is further restricted to loans that become at least 90+ days delinquent. The solid line plots predicted values assuming the loans were modified. The dashed line plots predicted values for all loans assuming the loans were not modified.

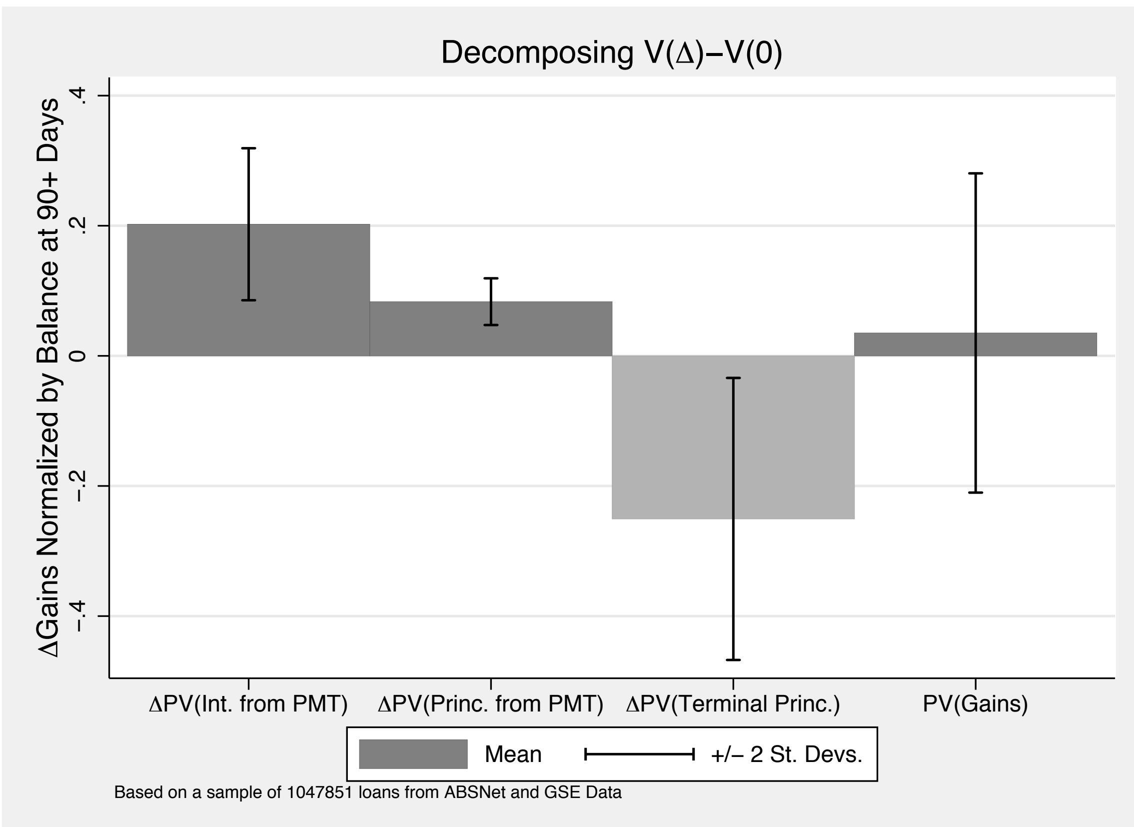
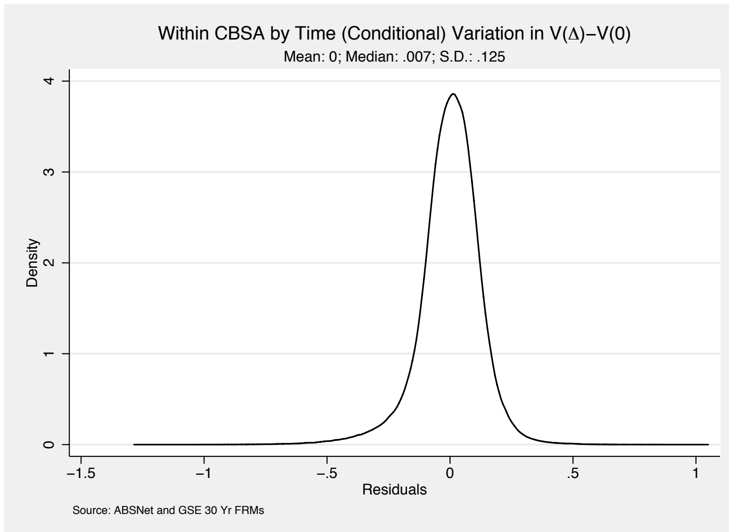


Figure 1.6: Decomposing the benefits from loan modification

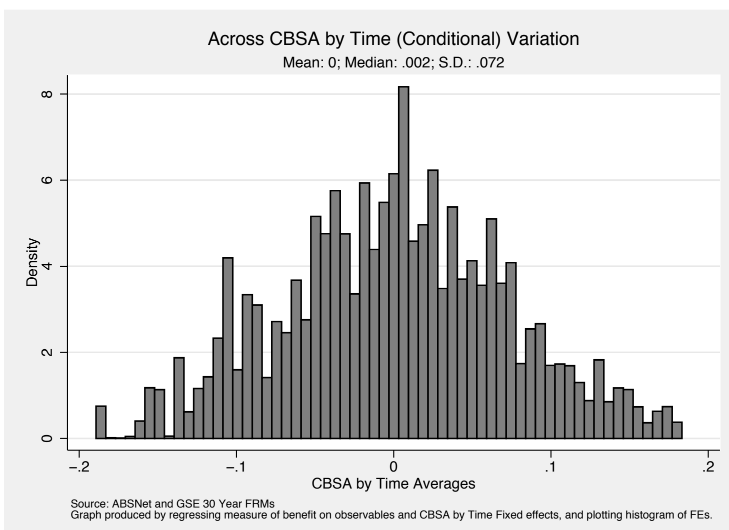
This graph shows the mean and variance of the gains from modification to investors relative to not modifying the mortgage. The bar graphs represent the means of normalized estimated gains which are measured at the loan level. The lines represent 95% confidence intervals based on the conditional standard deviation of the loan level estimates of gains from modification. The first component the present value of gains from interest earned through continued completion of monthly payments. The second component represents incremental amounts recovered of principal from continued collection of monthly payments. The third component represents the amount recovered from the termination of the mortgage after renegotiation, in present value terms, relative to the amount recovered from termination if the loan is not renegotiated. The estimates are based on my analysis on data on 30 Year Fixed Rate mortgages from ABSNet Loan, and the publicly available Fannie Mae and Freddie Mac data.

Figure 1.7: Within CBSA by time of delinquency variation



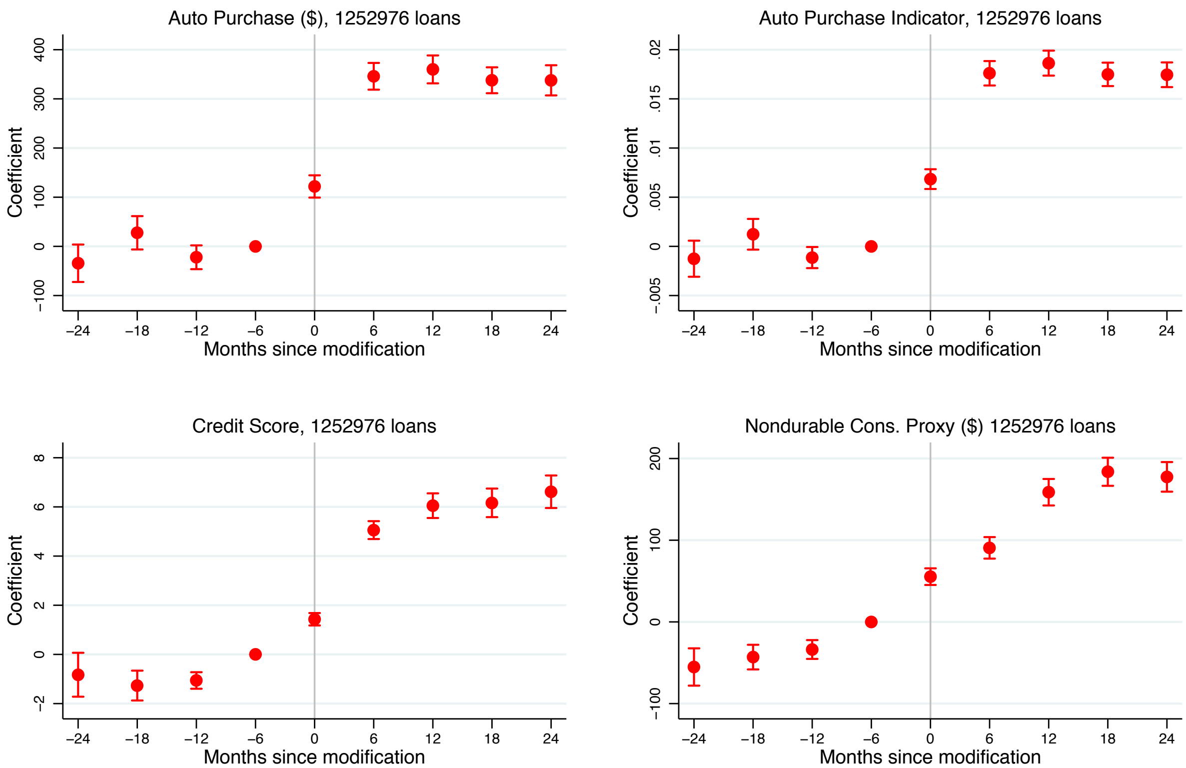
(a) Within CBSA by time of delinquency variation

The graph plots the density of results from a regression of my estimate of normalized gains from loan modification, i.e., I plot $\widehat{\epsilon}_{ict}$ from the regression $V(\widehat{\Delta) - V}(0)_{ict} = \alpha + X_i'\beta + \gamma_{ct} + \epsilon_{ict}$. The estimates are based on my analysis on data on 30 Year Fixed Rate mortgages from ABSNet Loan, and the publicly available Fannie Mae and Freddie Mac data.



(b) Across CBSA by time of delinquency variation

The graph plots the density of results from a regression of my estimate of normalized gains from loan modification, i.e., I plot $\widehat{\gamma}_{ct}$ from the regression $V(\widehat{\Delta) - V}(0)_{ict} = \alpha + X_i'\beta + \gamma_{ct} + \epsilon_{ict}$. The estimates are based on my analysis on data on 30 Year Fixed Rate mortgages from ABSNet Loan, and the publicly available Fannie Mae and Freddie Mac data.



The graphs above show estimates from an OLS regression with county by calendar halfyear fixed effects.
Includes loan level controls and allows for differential time trends for different observables.
Source: LPS–Equifax 30 Yr FRMs

Figure 1.8: Borrower's Consumption Response to Loan Modification - OLS Estimates
The graphs above plot results from an event study estimation of the effect of loan modification on borrower-level observables. The dependent variables used in the event studies are (clockwise from top): automobile purchases proxy variable (constructed using credit bureau data), indicator variable for whether an auto-mobile purchase was made; non-durable purchases proxy variable; and Equifax Vantage score (credit score) of the borrower from Equifax. The x-axis plots the time since loan modification in 6 month intervals, with the effect of the loan modification in the 6 month interval before modification ($t = -1$) normalized to 0. The loans used in the estimation are 30 Year Fixed Rate mortgages from McDash Loan Performance Services Data that become 90+ days delinquent at some point in their history. Standard errors are clustered at the county level.

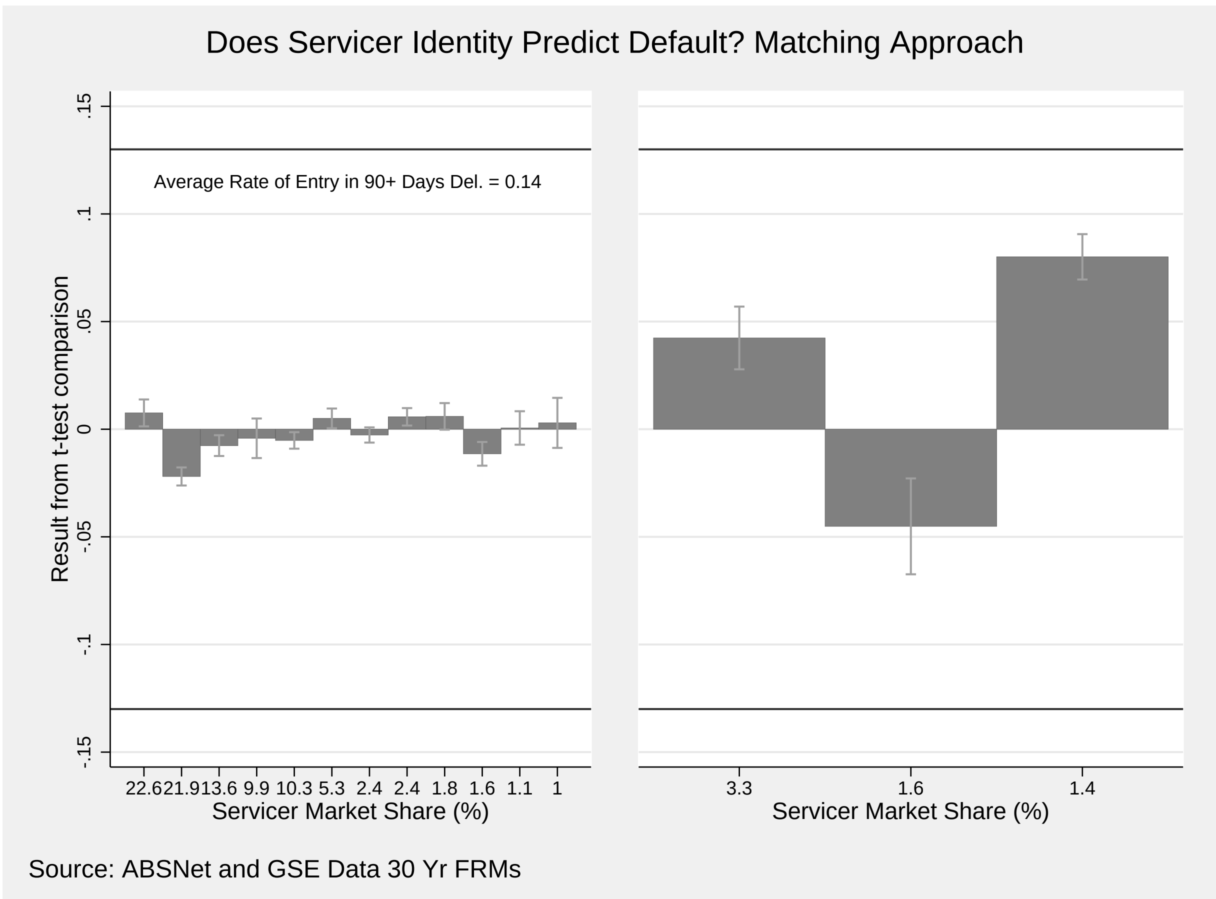


Figure 1.9: Assessing the exclusion restriction

This graph compares the loan performance (probability of entry into 90+ days delinquency within 36 months) for loans in the portfolios across different mortgage servicers. The y-axis shows results of a t-test comparing the loan performance of a sample of loans that belong to a servicer with market share denoted on the x-axis, with that of a matched sample (using propensity score matching) of mortgages from other servicers. The vertical lines on each bar show the clustered standard error on the difference in means from each t-test. The horizontal lines across the graphs denote the average rate of entry into 90+ days delinquency of around 14%. The panel on the left shows the set of servicers for whom I consider this robustness test to be valid. The panel on the right denotes the servicers for whom the exclusion restriction is unlikely to hold.

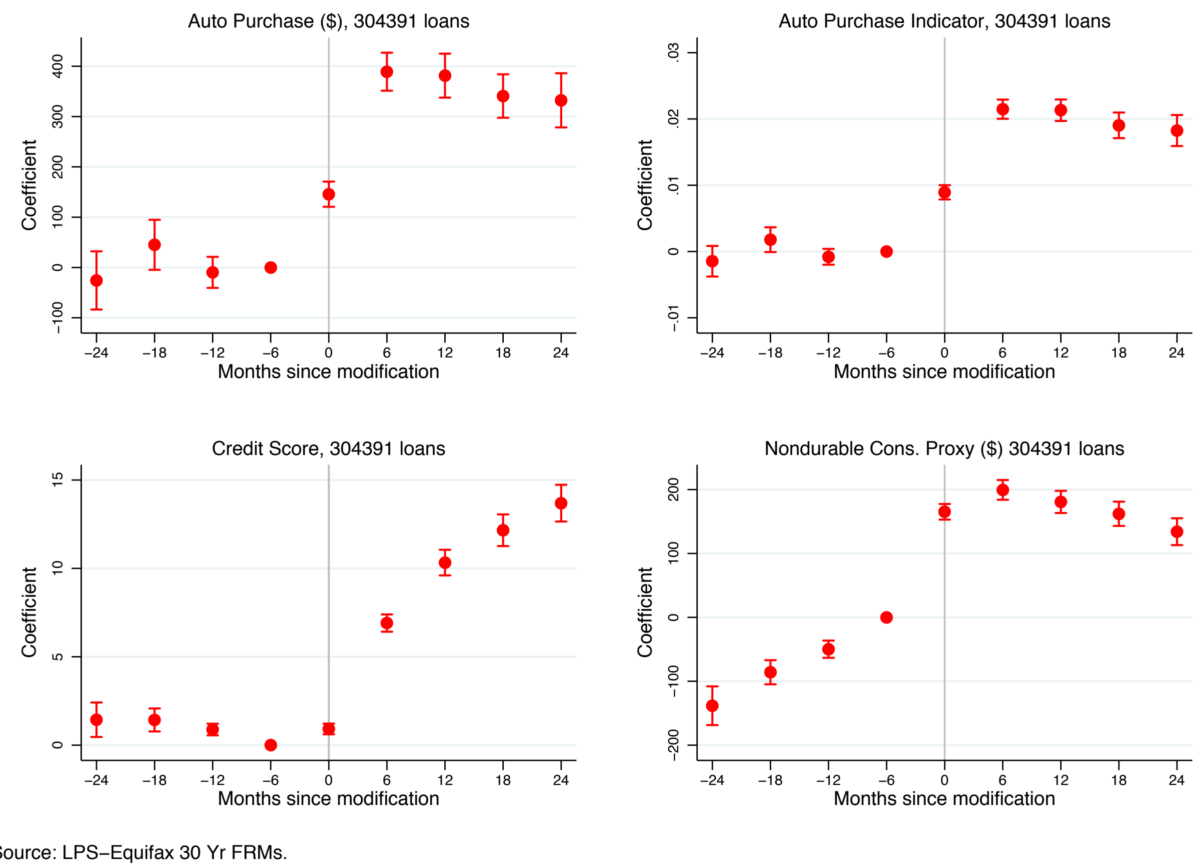


Figure 1.10: Event Study of the effect of loan modification using within borrower variation
The graphs above plot results from an event study estimation of the effect of loan modification on borrower-level observables. The dependent variables used in the event studies are (clockwise from top): automobile purchases proxy variable (constructed using credit bureau data); indicator variable for whether an auto-mobile purchase was made; Equifax Vantage score (credit score) of the borrower; and the non-durable purchases proxy variable. The x-axis plots the time since loan modification in 6 month intervals, with the effect of the loan modification in the 6 month interval before modification ($t = -1$) normalized to 0. The loans used in the estimation are 30 Year Fixed Rate mortgages from McDash Loan Performance Services Data that have been modified. Standard errors are clustered at the county level.

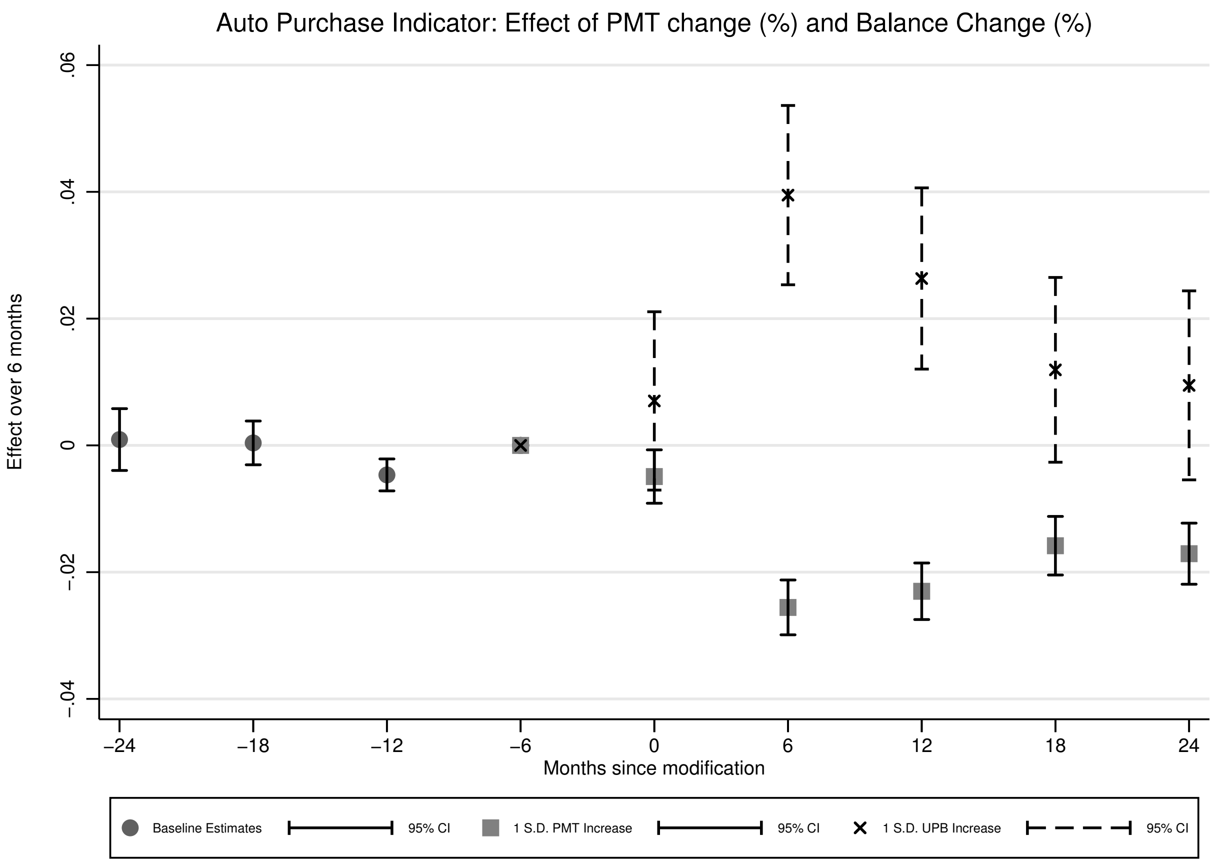


Figure 1.11: Heterogeneity in loan modifications and consumption

The graph above plots estimates from an OLS event study regression. The sample of loans used for this analysis comes from the McDash LPS data and consists of 30 Year Fixed Rate mortgages that have been modified. The dependent variable is an indicator for whether or not an automobile purchase took place within a given 6 month time period. I control for borrower level fixed effects, indicator variable for 6 month period before each loan was modified (coefficient plotted as a circle), indicator variables for every 6 month period after a loan was modified interacted with a standardized measure of the relative change in mortgage monthly payments (coefficient plotted as a cross) and indicator variables for every 6 month period after a loan was modified interacted with a standardized measure of the relative change in mortgage outstanding balance (coefficient plotted as a square). I also control for county by time fixed effects and time since delinquency fixed effects.

Table 1.1: Summary Statistics for 90+ Days Delinquent Mortgages (Panel A)

The table below displays summary statistics on mortgages that enter the analysis. The loans used to construct the summary statistics are 30 Year Fixed Rate mortgages from the ABSNet Loan Private Label Securitization Data and the Fannie Mae and Freddie Mac publicly available data (Panel A) and the LPS Data (Panel B). The sample is restricted to those loans that become at least 90+ days delinquent at some point in their history. The table displays summary statistics as at the origination of the mortgage, or summary statistics on the type of loan modification obtained.

	Fannie Mae/Freddie Mac GSE Loans			**ABSNet Loan Private Label Securitized**		
	N	**Mean**	**SD**	**N**	**Mean**	**SD**
FICO>=680 (%)	781296	0.50	0.50	559970	0.36	0.48
620<=FICO<680 (%)	781296	0.39	0.49	559970	0.37	0.48
FICO<620 (%)	781296	0.11	0.32	559970	0.27	0.44
Origination LTV	783687	77.01	12.77	580165	78.34	14.36
Interest Rate	783693	6.25	0.47	580175	7.65	1.49
DTI	783693	39.58	12.98	580175	0.00	0.00
DTI Missing (%)	783693	0.03	0.17	580175	1.00	0.00
Purchase Loan (%)	783693	0.34	0.47	580175	0.37	0.48
Cash Out Refi (%)	783693	0.47	0.50	580175	0.49	0.50
Rate Refi (%)	783693	0.20	0.40	580175	0.13	0.34
Modified within 6 mths. of 90+ (%)	783693	0.13	0.34	580175	0.11	0.32
Modified within 12 mths. of 90+ (%)	783693	0.23	0.42	580175	0.18	0.38
Modified overall (%)	783693	0.38	0.48	580175	0.31	0.46
Principal Increase (%)	294224	0.41	0.49	178374	0.75	0.43
Rate Decrease (%)	294224	0.77	0.42	178374	0.73	0.45
Term Increase (%)	294224	0.66	0.47	178374	0.04	0.20
Payment Decrease (%)	294224	0.94	0.24	178374	0.87	0.34
Number of Additional Monthly Payments After 90+ Days Delinquency						
Modified Loans	294224	**29.82**	14.89	178374	**20.49**	15.03
Not Modified Loans	489469	**8.16**	14.88	401771	**4.65**	10.29

Table 1.1: Summary Statistics for 90+ Days Delinquent Mortgages (Panel B)

	LPS GSE Loans			**LPS Private Label Securitized**		
	N	**Mean**	**SD**	**N**	**Mean**	**SD**
FICO>=680 (%)	685159	0.51	0.50	255692	0.41	0.49
620<=FICO<680 (%)	685159	0.34	0.47	255692	0.36	0.48
FICO<620 (%)	685159	0.15	0.36	255692	0.23	0.42
Origination LTV	846021	0.80	0.14	301823	0.78	0.12
LTV at 90+	715485	1.01	0.26	257582	0.98	0.25
Interest Rate	846717	0.07	0.01	303838	0.07	0.02
DTI	846718	21.82	23.00	303838	17.11	21.30
DTI Missing (%)	846718	0.43	0.50	303838	0.55	0.50
Purchase Loan (%)	846718	0.43	0.50	303838	0.38	0.48
Cash Out Refi (%)	846718	0.13	0.33	303838	0.21	0.41
Rate Refi (%)	846718	0.04	0.19	303838	0.02	0.14
Single Family (%)	846718	0.80	0.40	303838	0.80	0.40
Not Primary Residence (%)	846718	0.19	0.39	303838	0.18	0.38
Modified within 6 mths. of 90+ (%)	846718	0.12	0.32	303838	0.16	0.37
Modified within 12 mths. of 90+ (%)	846718	0.19	0.39	303838	0.22	0.42
Modified overall (%)	846718	0.26	0.44	303838	0.29	0.45
Principal Increase (%)	214244	0.88	0.33	79869	0.84	0.36
Rate Decrease (%)	217072	0.83	0.37	77453	0.69	0.46
Term Increase (%)	217760	0.44	0.50	81400	0.14	0.35
Payment Decrease (%)	215935	0.94	0.23	77208	0.81	0.39

Table 1.2: Effect of Loan Modification on the Additional Number of Monthly Payments

The table displays estimates of the effect of loan modification on the number of additional monthly payments completed by the borrower following entry into serious delinquency. The table displays estimates from various specifications. The loans used in the estimation are 30 Year Fixed Rate mortgages from the ABSNet Loan Private Label Securitization Data and the Fannie Mae and Freddie Mac publicly available 30 Year Fixed Rate Mortgage data. The sample is further restricted to loans that become 90+ days delinquent. "Modified" is the regressor of interest in the specifications and is a variable equal to 1 if a loan is modified. The row "Modified" corresponds to parameter estimates from the various specifications. To facilitate comparison across specifications, I also compute the average partial effect as implied by the coefficient estimate from the non-linear models. Columns 1 and 2 show the estimate from an OLS regression, Column 3 shows the results from a censored regression model, Columns 4 and 5 show the results from a two-stage least squares estimation and column 6 shows the results from full maximum likelihood estimation of a censored regression model with an endogenous dummy variable.

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	Cens. Reg.	IV-2SLS	IV-2SLS	IV-Cens. Reg.
VARIABLES	No. of Payments	No. of Payments	No. of Payments	No. of Payments	No. of Payments	No. of Payments
Modified	19.3236***	19.2983***	34.8775***	37.9798***	33.3944***	73.7148***
	(0.3495)	(0.3523)	(0.3457)	(0.9079)	(1.0547)	(0.9299)
Average Partial Effect			26.93			55.91
Observations	1,129,593	1,129,279	1,163,585	1,129,593	1,123,368	1,129,620
R-squared	0.4122	0.4178	-	0.1467	0.2570	-
Controls	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	No	Yes	Yes	No	Yes
Time FE	Yes	No	Yes	Yes	No	Yes
CBSA by Time FE	No	Yes	No	No	Yes	No
Originator by Agency FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	CBSA	CBSA	CBSA	CBSA	CBSA	CBSA
Partial F-Stat	-	-	-	217	208	-

Table 1.3: Comparing imputed and actual change in monthly payments

The table present summary statistics on the distribution of $\frac{d+\Delta}{d}$, the ratio of post-modification payments to pre-modification monthly payments. The first row presents summary statistics for loans that were not modified, for which this quantity has been imputed. The second row presents summary statistics on modified loans as they appear in the data.

			Percentiles				
Change in \$'s Monthly Payment	Mean	S.D.	10th	25th	Median	25th	90th
Imputed (Not Modified Loans)	0.707	0.089	0.608	0.639	0.690	0.762	0.839
Actual (Modified Loans)	0.704	0.137	0.527	0.578	0.695	0.812	0.913

Table 1.4: Borrower's Response to Loan Modification - OLS and IV Estimates

The table displays estimates of the effect of loan modification on borrower level observables. The tables plot estimates of the coefficient β_2 and β_3 from the structural equation $Y_{it} = \eta_{ct} + \psi_{(t-t_0(i))} + \beta_1 X_{it} + \beta_2 Modify_i + \beta_3 Modify_i \cdot \mathbf{1}_{t_m(i)>t} + \epsilon_{ict}$. Note that the data are in a panel setting with multiple time serious observations (t) for each loan (i). For columns 2 and 3 the sample used is 30 Year Fixed Rate Mortgages from the McDash LPS data that become 90+ days delinquent at some point in their history. For columns 4 and 5, the sample is further restricted to loans for which a match is available in the ABSNet Loan and GSE datasets so as to obtain the identity of the mortgage servicer and Originator. "Modify" is an indicator variable equal to 1 if the loan is modified at any point following serious delinquency. "Modify x Post" is equal to 1 if the loan i is modified following serious delinquency and the time period t corresponds to one following the loan modification of mortgage i. Column 1 presents, for a comparison, the average change in 6 months worth of monthly payments are a result of the loan modification. Column 2 presents results from an OLS estimation of the structural equation, with no additional control variables, Column 3 adds County by Time Fixed Effects, Time Since Delinquency Fixed Effects, and control variables. Column 4 adds Originator by Securitizer (PLS or GSE) Fixed Effects. Column 5 implements the instrumental variables approach using two stage least squares. Panel A has as the dependent variable the auto-purchase indicator variable while Panel B has the dollar value of auto purchases as captured by the credit bureau data based proxy. Panel C has as dependent variable the unsecured spending proxy variable, while Panel D presents results using the Equifax Vantage Credit Score as a dependent variable.

Panel A: Auto Purchase Indicator

	(1)	(2)	(3)	(4)	(5)
		OLS	OLS	OLS	2SLS
VARIABLES		New Car Ind.	New Car Ind.	New Car Ind.	New Car Ind.
Avg. Change in Mthly. Pmt.	**-2160**				
Modify		-0.0047***	-0.0004	-0.0005	-0.0021
		(0.0003)	(0.0003)	(0.0005)	(0.0047)
Modify x Post		0.0313***	0.0173***	0.0165***	0.0237***
		(0.0004)	(0.0005)	(0.0007)	(0.0089)
Observations		6,829,752	6,093,074	2,377,684	2,441,457
R-squared		0.0031	0.0157	0.0259	0.0252
Controls		No	Yes	Yes	Yes
Controls x Linear Trend		No	Yes	Yes	Yes
County by Time FE		No	Yes	Yes	Yes
Time Since Delinquency FE		No	Yes	Yes	Yes
Originator by Securitizer FE		No	No	Yes	Yes
Partial 1st Stage F-Stat		-	-	-	95
Clustering		County	County	County	County

Table 1.4: Borrower's Response to Loan Modification - OLS and IV Estimates

Panel B: Auto Purchase ($)

	(1)	(2)	(3)	(4)	(5)
		OLS	OLS	OLS	2SLS
VARIABLES		New Car $	New Car $	New Car $	New Car $
Avg. Change in Mthly. Pmt.	**-2160**				
Modify		-97.3885***	-17.3017**	-14.6708	40.8895
		(6.7271)	(7.6317)	(13.0669)	(100.0215)
Modify x Post		625.6580***	337.6254***	337.8889***	427.6917**
		(11.2206)	(10.2331)	(15.4260)	(198.3280)
Observations		6,829,752	6,093,074	2,377,684	2,441,457
R-squared		0.0014	0.0088	0.0292	0.0282
Controls		No	Yes	Yes	Yes
Controls x Linear Trend		No	Yes	Yes	Yes
County by Time FE		No	Yes	Yes	Yes
Time Since Delinquency FE		No	Yes	Yes	Yes
Originator by Securitizer FE		No	No	Yes	Yes
Partial 1st Stage F-Stat		-	-	-	95
Clustering		County	County	County	County

Table 1.4: Borrower's Response to Loan Modification - OLS and IV Estimates

Panel C: Non-Durable Consumption (Proxy) ($)

	(1)	(2)	(3)	(4)	(5)
		OLS	OLS	OLS	2SLS
VARIABLES		NonDur	NonDur	NonDur	NonDur
Avg. Change in Mthly. Pmt.	**-2160**				
Modify		-83.7063***	-46.7914***	-33.9333***	-110.6914*
		(8.0843)	(4.0093)	(6.8132)	(66.8499)
Modify x Post		22.8642***	56.9709***	73.8934***	263.9607***
		(4.0232)	(6.7047)	(8.4656)	(78.4031)
Observations		6,829,752	6,093,074	2,377,684	2,441,457
R-squared		0.0002	0.0440	0.0553	0.0550
Controls		No	Yes	Yes	Yes
Controls x Linear Trend		No	Yes	Yes	Yes
County by Time FE		No	Yes	Yes	Yes
Time Since Delinquency FE		No	Yes	Yes	Yes
Originator by Securitizer FE		No	No	Yes	Yes
Partial 1st Stage F-Stat		-	-	-	95
Clustering		County	County	County	County

Table 1.4: Borrower's Response to Loan Modification - OLS and IV Estimates

Panel D: Credit Score

	(1)	(2)	(3)	(4)	(5)
		OLS	OLS	OLS	2SLS
VARIABLES		Credit Score	Credit Score	Credit Score	Credit Score
Avg. Change in Mthly. Pmt.	**-2160**				
Modify		-0.9687	8.6261***	10.3303***	27.7345***
		(0.7752)	(0.5281)	(0.6416)	(3.4293)
Modify x Post		30.7287***	10.8214***	9.7222***	4.1742
		(0.5311)	(0.2774)	(0.3908)	(3.4479)
Observations		6,829,563	6,092,903	2,377,636	2,441,405
R-squared		0.0208	0.1235	0.1320	0.1279
Controls		No	Yes	Yes	Yes
Controls x Linear Trend		No	Yes	Yes	Yes
County by Time FE		No	Yes	Yes	Yes
Time Since Delinquency FE		No	Yes	Yes	Yes
Originator by Securitizer FE		No	No	Yes	Yes
Partial 1st Stage F-Stat		-	-	-	95
Clustering		County	County	County	County

Table 1.5: Consumption and Redefault

The table displays estimates of the effect of automobile purchases on subsequent redefault of a mortgage following loan modification. The table displays estimates from a linear probability model. The loans used in the estimation are 30 Year Fixed Rate mortgages from the LPS McDash dataset that were modified after becoming 90+ days delinquent. The dependent variable is an indicator variable equal to 1 if the borrower becomes 90+ days delinquent at any point following loan modification. "Car Purchase After Mod" is an indicator variable equal to 1 if the borrower purchased an automobile at any point following loan modification, and 0 otherwise. "Car Purchase Before Mod" is an indicator variable equal to 1 if the borrower has purchased an automobile at any point prior to loan modification. Columns 1 to 5 all present estimates from an OLS regression. Each column adds additional sets of control variables. The preferred estimate is from Column 5.

	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	OLS	OLS	OLS
VARIABLES	Re-Default	Re-Default	Re-Default	Re-Default	Re-Default
Car Purchase After Mod?	-0.0656***	-0.0663***	-0.0494***	-0.0431***	-0.0451***
	(0.0026)	(0.0028)	(0.0027)	(0.0026)	(0.0027)
Car Purchas Before Mod?		0.0039	0.0156***	0.0201***	0.0136***
		(0.0029)	(0.0025)	(0.0024)	(0.0025)
Observations	177,027	177,022	177,022	177,022	149,743
R-squared	0.0036	0.0037	0.1357	0.1594	0.1772
Controls	No	No	No	No	Yes
County FE	No	No	Yes	Yes	Yes
Month of Del FE	No	No	No	Yes	Yes
Month of Mod. FE	No	No	Yes	Yes	Yes
Mod Type FE	No	No	No	No	Yes
Cluster	County	County	County	County	County
Mean of Dep Var	0.59	0.59	0.59	0.59	0.59

Chapter 2

Multiple Tranches, Information Asymmetry and the Impediments to Mortgage Renegotiation

2.1 Introduction

The years prior to the financial crisis witnessed a large increase in the origination of household credit, and mortgage debt in particular, followed by a rapid and widespread increase in delinquencies. Innovations in the institutions of mortgage origination and the accompanying agency problems (56, 57, 81) were an important contributor to these phenomena. Renegotiation of debt (37, 10) has been one avenue to alleviate the adverse effects of the Great Recession on households. However, an important question arises; do post-origination agency problems in credit markets affect the rate of household debt renegotiation?

A study of a specific agency problem within the securitized residential mortgage market sheds light on this question. This market lends itself well to the study of frictions to renegotiation for two reasons. Firstly, tax laws prohibit the RMBS Deal Sponsor from managing the pool of collateral himself, and so he is required to employ an agent, the Servicer (for more details see Section 2.2) to do so. The Servicer collects payments from borrowers who are current, and disburses the bond coupons and principal repayments to investors. While this activity involves little of their own discretion; they are the agents who are responsible for managing loss mitigation efforts when it comes to borrowers who remain seriously delinquent. Servicers have superior information about borrowers due to their continuos monitoring of them. Hence, information asymmetries between Sponsor and the Servicer create an agency problem. Secondly, securitization involves the tranching of cash flows which generates multiple claims to the pool of mortgage collateral. This multiplicity has the potential to exacerbate the agency problem between the Sponsor and the Servicer.

In this paper, I explicitly study whether multiplicity of tranches affects both the probability of loan modification and the types of loan modifications offered. The paper's contribution is twofold. First, it develops a framework to demonstrate how the multiplicity of tranches and the agency problem induced by use of the Servicer interact and influence the rate of loan modification. The framework, although stylized, incorporates elements of securitization that have not been examined in theoretical work on the matter (91, 72). Second, augmenting insight from the framework with findings from the existing literature, I motivate and test two main hypotheses. I test these hypotheses using within-RMBS-deal variation in the multiplicity of tranches. I find that loans in mortgage pools collateralizing more tranches were less likely to be modified. Loans in these pools that were modified received less aggressive loan modifications. Overall, the results suggest that this agency problem in mortgage securitization affects how borrower delinquency is resolved.

The framework consists of a simple principal-agent model, augmented by an audit mechanism. The principal is the RMBS deal Sponsor, who aims to maximise the cash flow from a pool of delinquent mortgages.[1] The Servicer has private information about what the cash-flow maximising action with regards to a pool of delinquent borrower is. That is, will the cash flow from the mortgage be maximised by loan modification or foreclosure. The Servicer also faces convex costs of loan modification and so has an incentive to foreclose upon a property when, in fact, modification of the mortgage could deliver a higher pay-off to the investors. The cost of ex-post auditing the agent's (Servicer's) actions increases with the number of tranches, and consequently, the number of investors.[2] This assumption captures, in a reduced form way, the ex-post frictions in disciplining the Servicer which arise in the presence of multiple investors (50, 35).[3] Therefore, the effect of multiplicity of tranches on the rate of loan modification works through the agency problem involving the Servicer.[4]

An optimal contract resolves the incentive misalignment at the cost of the principal conceding rents to the agent in the form of higher compensation for modification. Increasing the number of investors interacts with this agency problem. A larger number of tranches, or investors, makes it more expensive to coordinate to discipline the Servicer, and reduces the probability of ex-post audit. Thus, the Servicer must be given more rents in order to implement the optimal rate of loan modification. While increasing the number of tranches on a given pool has it's benefits to the deal Sponsor (74, 29) it may hinder loan modification if it becomes too costly to incentivize the Servicer appropriately.[5] The framework allows me to motivate two hypotheses about the multiplicity of tranches and the rate and types of loan modifications.

There are three challenges to identifying the effect of multiple tranches on loan renegotiation. First, one needs to construct a measure of the multiplicity of claim-holders to

[1]A similar assumption is made in 60

[2]For the purpose of the theoretical exercise, assume that there is a one-to-one relationship between the number of tranches and the number of investors.. Note that the investors are assumed to be passive and I do not model any interaction between the investors.

[3]This is an attempt to model in a reduced form manner, the free-rider problem in ex-post monitoring of the agent that exists in the presence of multiple investors. One can also think of this as representing the collective action problem faced by the investors.

[4]This agency problem was inherent in Private Label Securitization. In Private Label Securitization, the Servicer is needed to ensure that the securitization conduit maintains it's REMIC status to avail of the tax benefits which made the structure profitable. To do so, the trustees could not actively manage the portfolio of mortgages, and thus were required to hire the mortgage Servicer.

[5]Additionally, this intuition can explain why Sponsor's may have preferred rigid contracts, which tied the Servicer's hands, by either prohibiting loan modifications, or disincentivizing the modification of mortgages.

a particular loan pool. Secondly, there are several elements of the structure of an RMBS deal that may affect outcomes of delinquent mortgages, yet are either unobservable, or pose a challenge to quantify. For example, residential mortgage backed securitization deals had varying levels of protection for Senior Tranche investors in the form of varying levels of Subordination or differing Credit Enhancement mechanisms used. More importantly, as highlighted by the framework, it will be important to control for the contract that is signed with the Servicer. This contract is challenging to codify.

The first challenge is overcome by using two measures of tranche multiplicity. I use the data to determine a mapping between the loan pools of a mortgage backed securitization deal and the tranches that the loan pools collateralize. The first measure simply counts up the tranches that have claims to a particular loan pool. The second measure is based on the Herfindahl Hirschman Index methodology and varies on the interval $(0, 1]$.[6] This measure aims to take into account the relative sizes of the tranches that are associated with a particular loan pool. It captures more of the observed variation in RMBS deal structures.

I overcome the second challenge by using deal fixed effects to control for all deal-level unobservables that stay constant over time. Thus, identification is obtained from comparing loans in a pool that collateralized fewer tranches (as measured by one of the two methods) with those in a pool of the same deal that had collateralized more tranches. Firstly, note that the measures developed are pool level measures. This is precisely so that I can employ such a fixed effects strategy. Further note that the the contract that governs the incentives of the Servicer is in place at the deal level. To the extent that this contract does not vary significantly over time, controlling for the deal fixed effects allows me to "hold it constant". Additionally, I am able to use a Servicer fixed effect to control for all servicer-specific unobservables (for e.g. servicer's infrastructure, competence, size, etc.) that may influence the probability of loan modification.

First, consistent with the predictions of the framework, I find that a higher number of tranches predicts a lower probability of loan modification. A one standard deviation increase in the count of tranches that have claims to a particular loan pool predicts a 100 bps lower probability of loan modification. However looking at a simple standardized count of tranches has it's shortcomings. Once I use the HHI based measure, I find that moving from the 25th to the 75th percentile of this measure predicts a 128 bps increase in the probability of loan modification.[7] One can draw a parallel between these results and those of 78 and 8. A loan held in a bank's portfolio would correspond to one in a PLS pool collateralizing a single tranche. They find that portfolio loans have lower foreclosure rates and higher modification rates compared to securitized loans. My results corroborate their evidence (albeit in the context of private label securitization) by showing that a loan pool with fewer tranches was more likely to see its loans modified (after controlling for all relevant loan level observables). Estimates from a hazard rate model of foreclosure suggest that the multiplicity of tranches may account for about a third of the higher foreclosure rates of securitized mortgages as

[6] 89 uses a similar approach in his study of syndicated loans.

[7] These results are confirmed in a proportional hazard rate specification as well. Moreover, I find that the estimated effect is stronger when I restrict the sample to mortgages which did not have complex features (19) such as Negative Amortization or Interest Only payments.

compared to portfolio loans.

Next, I find that a higher number of tranches predicts less aggressive loan modifications, conditional on a loan having received a modification. I focus on two types of loan modifications. First, those that reduced the monthly payment without a subsequent increase in the principal balance. Second, I focus on those that increased principal without a subsequent decrease in the monthly payment (one of the most common ones, which was achieved by simply capitalizing missed payments).[8] Overall, conditional on receiving a loan modification, decreasing the multiplicity of tranches predicts more aggressive changes in the contract terms. More specifically, a decrease in the number of tranches predicts a larger decrease in the monthly payment, conditional on receiving such a modification; predicts a lower probability of a modification that only increases the balance without reducing the monthly payment; and predicts a decrease in the interest rate.

In summation, the results show that agency problems in the intermediation and post-origination monitoring of household debt may impede loan modifications. Failure to renegotiate loans and lower monthly payments does not relax a borrower's liquidity constraint, leaving him worse off (60). 60 and 64 suggest that such frictions also prevent investors from realizing gains from renegotiation particularly when collateral values have declined substantially.[9] The originate-to-distribute model continues to be prevalent in the U.S. mortgage market, and so do the agency problems that arise with it. Understanding potential frictions in the system contributes to the subsequent discussion on how best to design U.S. mortgage markets so as to maximise welfare in the event of a housing crisis.

My work relates to a few different strands of the existing literature. 8, 78, 4, 3 and 62 examine whether delinquent loans loans in securitized pools were modified at a different rate as compared to those that were held on banks' balance sheets. While they predominantly find that securitized loans were less likely to be modified, they remain agnostic on the specific channel through which this may have occurred. I contribute to that literature by corroborating their evidence in the setting of private label mortgage backed securitization, and providing evidence of the mechanism through which the effects they measure manifest themselves.

This paper also contributes to the literature on agency problems in the securitization chain, and their effects on loan origination and loan performance. 57 and 81 show that securitization, and the fact that originators did not subsequently hold onto any risk led to relaxed screening of borrowers. 30 show that the default rates are lower for deals in which the originator is affiliated either with the Sponsor or the Servicer, and that these affiliations are priced into the initial yields of the RMBS. 51 demonstrate that the affiliation between the Servicer and the deal Sponsor predicts life of the equity tranche through the channels of modifications and foreclosures. My paper differs from these in demonstrating that the agency problem between the Sponsor and the Servicers/Investors interacts with the number

[8] I focus on these, as the two most important outcomes of a modification for a borrower are the effect on the outstanding balance and the monthly payment.

[9] The main result of 60 is not incongruous with the results presented here. For one to *a priori* expect an effect of tranche multiplicity there must exist some states of the world in which positive gains from renegotiation could be realized in a frictionless world. 60 shows that such states exist even though on average gains from renegotiation to investors are limited.

of tranches written on the underlying collateral to influence the rate of loan modification.

My identification strategy is motivated by the work of 1 who use the setting of Residential Mortgage Backed Securitization to test for the effects of blockholding on the performance of agents. Similarly to this paper, I use a deal fixed effect to ensure I am controlling appropriately for deal structure, Servicer contracting and other deal-level unobservables, and make use of the fact that loan pools within the same deal collateralize different tranche structures.

Section 2.2 describes the role of tranching and the Servicer in RMBS deals. Section 2.3 lays out the conceptual framework. 2.4 describes the hypotheses. Section 2.5 lays out the Empirical strategy to test the hypotheses. Section 2.6 outlines the results. 2.7 includes the Robustness checks performed.

2.2 Institutional Details

Securitization involves the pooling of mortgage loans, which are eventually held by the securitization trust. A deal Sponsor incorporates the securitization trust. The trust funds the purchase of the loans by issuing rated mortgage backed securities. The deal Sponsor also designs the tranched structure of the bonds collateralized by the underlying pool of mortgages. The Sponsor appoints a Servicer to manage the cash flows from the loans. The next two subsections delve further into the multiplicity of tranches in private-label RMBS, and the role of the mortgage Servicer.

2.2.1 Multiple Tranches in RMBS

The focus of this paper is on the idea that RMBS involved creating multiple claims to cash flows from a pool of mortgages. A securitization transaction typically involves two or more loan pools that provide the underlying collateral. Figure 1 diagrammatically depicts a typical Residential Mortgage Backed Security transaction. This particular transaction is the MASTR Adjustable Rate Mortgages Trust 2004-11. It was an Alt-A deal of size $709 million dollars.

As can be seen in Figure 1, there are two loan pools underlying this deal. The blue boxes denote the AAA rated tranches in this deal. The tranches denoted with '1-A' are collateralized exclusively by loan pool 1. The tranches denoted with '2-A' are collateralized exclusively by loan pool 2. The tranches in green are collateralized by both loan pools. The two groups of AAA rated securities depicted are different on two dimensions. Firstly, there is variation in the number of tranches in each stack. Secondly, there is variation in the size of each tranche relative to its stack. For example, the largest tranche in the 1-A stack, 1-A-1, is about 50% of the size of the stack, while 2-A-1 is about 90% of the size its stack. Thus, the two different loan pools have different counts of tranches that they collateralize. Additionally, there is heterogeneity in the relative sizes of the tranches that have claims to each loan pool.

Therefore, the structuring that takes place in securitization induces complexities beyond just increasing the number of potential claim-holders to cash flows from a mortgage pool. It imposes a waterfall structure, outlining the priority of the different securities in receiving cash flows arising from the underlying mortgages. In this paper, I will not be able to address explicitly the effect of creating securities with an order of seniority, i.e. a subordination structure. However, I will focus on the fact that such structuring inherently increases the number of securities which have claims to the underlying collateral.

There are several existing theories of why this tranching structure is optimal. 74 describes how the potential for market segmentation and price discrimination encourages the Sponsor to tranche the cash flows and create securities with different characteristics to sell to heterogenous investors.29 suggests that a "risk diversification" effect motivates tranching, as it allows an informed RMBS Sponsor to create a low risk, high rating debt security that has enhanced liquidity. In this paper I take such a tranching structure as given.

2.2.2 Servicer

The Sponsor appoints a Servicer at the inception of the deal to manage the cash flows from the loans. The need for the Servicer arises, firstly, from the need of the securitization trust to maintain it's REMIC (Real Estate Mortgage Investment Conduit) status. To receive the tax benefits associated with this legal status, the trustee cannot actively manage the assets and must appoint another entity to do so. The Servicer for the MASTR deal in the example above is Wells Fargo.

The primary role of the servicer involves little of his own discretion. The servicer is required to pass on payments received from borrowers to the investors as per the waterfall structure of the deal. The servicer must also manage loans that are in default. If a borrower does not make their mortgage payments, the servicer advances these amounts to the trancheholder from his own pocket to prevent disruptions in cash flows to the rated bonds. These advances are recovered when the borrower resumes making payments, or from the proceeds of any liquidation or foreclosure on the loans. Thus, the advances constitute an interest-free loan made by the servicer to the investors. If the borrower remains seriously delinquent beyond a specified period of time, the servicer will be called upon to determine the action to be taken. The servicer can initiate foreclosure upon the property, engage in negotiations to modify the terms of the loans, or simply take no action and wait for the borrower to "selfcure". The servicer may also have discretion in the methods he can use to modify the loans. The contract that governs this relationship between the trust (that represents investors) and the servicer is the Pooling and Servicing Agreement (henceforth the "PSA").[10]

[10]Note that the trust is established by the deal Sponsor, and the PSA is in place at the closing of the RMBS deal. Broadly speaking the PSA contains general guidelines and servicing principles to be followed, a description of what the agent may or may not do in certain situations, and an outline of the compensation scheme. Often there may be multiple Servicers appointed under a single deal to manage different sets of loans. In my data, I will aggregate this information at the deal or the loan pool level, depending on the identification strategy employed, and obtain the name of the modal servicer pertaining to each deal or loan pool. There are, to my knowledge, only two studies that document the heterogeneity across PSAs in Subprime RMBS deals. 52 documents the contents of 65 PSAs. 62 considers the contents of a sample of 35 to 40 deals to understand whether they impede loan modifications. He finds that they do not explain a substantial amount of the difference between the rate of modification between securitized loans and loans held on bank balance sheets.

I propose two main channels via which the creation of multiple tranches interacts with the presence of the mortgage Servicer. Firstly, the PSA does not instruct the Servicer to take into account the payoffs of one particular investor. Modifying a group of delinquent mortgages will involve higher costs, yet may deliver no benefit to the protected senior bond holder, making them averse to such an action on the part of the Servicer. However, the junior bond-holder may prefer the loan modifications and would want the servicer to incur the necessary cost. Such a degree of ambiguity about whose interests are to be maintained may induce the Servicer to simply take the lowest cost action of foreclosing upon the property. Secondly, ex-post monitoring of the Servicer may suffer from free-rider problems, such as those which motivate the delegated monitoring model of 35. The conceptual framework developed in the next section looks to further this intuition.

In summary, note that there are two key features of this setting. Firstly, that there exists, due to institutional reasons and prevailing securities law, a separation of ownership (tranche-holders) and control (mortgage Servicer) of the underlying collateral pool, and secondly, that there are not one but multiple "owners" of a particular pool of underlying collateral. The next section provides a simple conceptual framework which ties these elements together, shows how they may influence the rate of loan modification; and consequently sets the stage for the hypothesis development and empirical analysis that follows.

2.3 Conceptual Framework

The conceptual framework developed in this section highlights how multiplicity of tranches worsens the agency problem between the Sponsor and the Servicer of an RMBS securitization deal, thus resulting in a lower rate of loan modification. Additionally, it highlights important endogeneity concerns that any empirical analysis of this setting will need to address. Thus, it informs the hypotheses and the empirical strategy developed in the subsequent sections. Sections 2.3.1 to Section 2.3.3 setup and solve the framework assuming the existence of a single Sponsor, or Investor. Section 2.3.4 incorporates frictions from the presence of multiple investors. Section 2.3.5 discusses related models of mortgage servicing.

2.3.1 Setup

There are two states of the world $\omega \in \{\omega_H, \omega_L\}$. Let p denote the probability of the high state ω_H. The principal is the Deal Sponsor while the agent is the Servicer. The principal chooses a contract to maximise it's payoff subject to a set of participation and incentive compatibility constraints. The objective functions of the Sponsor and the Servicer will be described in more detail in Sections 2.3.1.2 and 2.3.1.3.

2.3.1.1 Mortgage and Borrower

In this setting, I take a simple approach to modeling the borrower and the mortgage, and follow closely the framework of 42. Assume α_0 is the probability of a mortgage becoming seriously delinquent. In this setting I assume $\alpha_0 = 1$ so that the analysis begins with a pool of seriously delinquent mortgages.[11]

Following 42 define a loss mitigation plan as a triple (α_1, m^*, f) where α_1 is the probability of redefault following the mortgage modification, m^* is the modified mortgage payment to be made, and f is the value of the property in foreclosure. Let $f = \gamma V_\omega - \lambda$, where γ is a fire-sale discount on the value of the property sold in foreclosure and λ is the dead weight loss from foreclosure (lawyers fees, administrative costs etc.) Assume the components of this triple are simply parameters of the model.

Given the realization of the state ω, the amount recovered if foreclosure is initiated and completed is $V_\omega - \lambda$, and the expected amount recovered following a modification is: $\alpha_1(\gamma V_\omega - \lambda) + (1 - \alpha_1)m^*$. For further simplification, assume $\gamma = 1$ and set V_L and V_H such that $V_L < (\lambda + m^*)$ and $V_H > (\lambda + m^*)$.

This assumption simply indicates that in the low state of the world, the value of the mortgage is maximised by modification and in the high state of the world, the value of the mortgage is maximised by foreclosing upon the property. 60 shows that while on average gains to investors from loan modification were limited, they are substantially higher when the borrower has experienced decreases in collateral value or when losses in foreclosure are higher.[12] The low state, ω_L, captures such states of the world. Also note that I do not explicitly model self-cure. Here the servicer must take an action, either modify or foreclose, and cannot simply wait for the mortgage to self-cure.[13]

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[11] By imposing this assumption, I am to some extent abstracting away from one side of the adverse selection problem in loan modifications. This assumption imposes that modifications will never be given to those borrowers who would have self-cured in its absence.

[12] Consider a simple example of a mortgage that has become delinquent, and the three options available to a servicer. The cash flows from doing nothing are:

$$P(\text{Self-cure}) \sum_t \frac{CF}{(1+r)^t} + (1 - P(\text{Self-cure}))(\text{CF from Mod or Foreclosure})$$

Cash flows from a loan modification (assuming no re-default) are:

$$\sum_t \frac{CF^{Mod} - CF^{Original}}{(1+r)^t}$$

which is likely to be less than 0. Finally cash flows from foreclosure will be:

$$\text{Recovered Value of Property - Outstanding Debt}$$

Thus, it can be seen that if $P(\text{Self-cure}) \to 0$ or Recovered Value $\to 0$, then the investor too would find it optimal to engage in a loan modification.

[13] In this setting, the borrower is simply a passive entity. I assume no strategic interaction between the borrower and the creditor (such as in the model of 91). Additionally, I abstract away from the choice of modification strategy used by assuming that m^* is simply a parameter of the model. Previous work such as 38 explore the effects of different types of modifications. We use insights from this literature in considering the outcome variables of the empirical analysis.

2.3.1.2 Deal Sponsor

The Deal Sponsor represents the principal in this principal-agent setup. In this note, the Investor is not explicitly modeled. Instead, I assume that there is no information asymmetry between the Investor and the Deal Sponsor with regards to the loan pool (for example, the primary friction in models such as DeMarzo and Duffie (1999)). The Sponsor and the Investors are aligned in this regard. The Sponsor's has to choose a contract with the Servicer which maximises the payoff from the pool of delinquent mortgages. The Sponsor writes the contract with the Servicer at the inception of a RMBS deal and then has limited or no involvement in the deal. Let $Z(\phi, \omega)$ denote the payoff from the pool of delinquent loans given some rate of modification (ϕ) and the realized state (ω).

$$Z(\phi, \omega) = \phi(\alpha_1(V_\omega - \lambda) + (1 - \alpha_1)m^*) + (1 - \phi)(V_\omega - \lambda)$$

Until Section 2.3.4 I assume that there exists one investor, and that the Sponsor's interests are aligned with his. In Section2.3.4, I lay out the frictions that arise in the presence of multiple tranches.

2.3.1.3 Servicer

The Servicer is the agent of the deal Sponsor. There are two key assumptions to be made here. Firstly, an assumption about the information available to the servicer, and secondly, an assumption about the cost structure faced by the Servicer.

Assumption 1. *The servicer privately observes the realization of the state ω.*

In this framework, the servicer observes whether the value of the property is V_L or V_H. The framework here attempts to model in a simple way the notion that the Servicer is privately aware of whether the decision to modify or foreclose maximises the value of the mortgage. This knowledge needs to be elicited from him via the provision of incentives. Note that following the inception of an RMBS deal, it is the Servicer who primarily communicates with the borrowers. The Sponsor and Investors simply receive reports from the Servicer. As 90 writes, it is not easy for the investor to have enough information about the costs and benefits associated with a loan modification for a particular borrower. The Servicer's action involves choice of rate of modification, $\phi \in [0, 1]$. This can be interpreted as fraction of a pool of delinquent mortgages that will be modified rather than being foreclosed upon.[14][15]

Let the Servicer's cost function be denoted by $C(\phi, \omega)$; i.e. it is a function of ϕ, the servicer's action and the state of the world.

The following assumptions are made with regards to the cost function:

[14]Note that this may be incongruous with the assumption that the value of the property is private information. The setting is maintained however to simplify the exposition. This assumption is similar to that which appears in 72 except that it is the Servicer, and not the borrower, who has this private information.

[15]An alternative assumption, in the spirit of 42 would be to model the value of α_1 as being state dependent. However, this requires making, and justifying, further parametric assumptions regarding the value of γ and the value of the property. More precisely, one has to assume, for the problem to be non-trivial, that $\gamma < 1$ and that in the good and bad state of the world, the delinquent borrower will be underwater on their mortgage $\gamma V - \lambda < V - \lambda < m^*$

Assumption 2. ***(a)*** *$C_{\phi\phi} > 0$;* ***(b)*** *$C_{\phi\omega} > 0$;* ***(c)*** *$\lim_{\phi \to 0} C_\phi = -\infty$ and (d) $C_\phi > 0$ for some ϕ. (e) C is continuos and differentiable.*

The first part of this assumption implies convexity of the cost function.[16] It says that the marginal cost of more modifications is increasing in the rate of modification ϕ. The second assumption indicates that $C_\phi(\phi, \omega_H) > C_\phi(\phi, \omega_L)$, i.e. the marginal cost of an additional modification is higher in the high state of the world, when $V = V_H$. Firstly, this assumption ensures that the Spence-Mirlees (Single Crossing) condition holds in this setting. Secondly, it attempts to capture the fact that often, making a larger volume of modifications involves an up-front infrastructure cost. Thus, in a good state of the world, when the Servicer is less likely to have made this investment, the marginal cost of modification will be higher. In the low state of the world, they are more likely to be prepared to make a larger number of of modifications. The third assumption is required to ensure consistency with the parametric assumption $V_H > (\lambda + m^*)$.[17] Intuitively, it suggests that initially, there are economies of scale to making modifications, however, as the Servicer becomes overburdened with modifications, the marginal costs eventually become positive.

Additionally, let the cost function be parametrized as $C(\phi, \omega) = g(\omega)C(\phi)$. Additionally, let $g(\omega) = V_\omega$. This separates the cost function into two components, the state specific component and the action specific component.[18]

2.3.1.4 Contract

The contract between the Sponsor and the Servicer consists of a transfer $t(\hat{\omega})$ and a specified rate of modification $\phi(\hat{\omega})$ for any given report by the agent $\hat{\omega} \in \{\hat{\omega}_H, \hat{\omega}_L\}$. Let $t_H = t(\hat{\omega}_H)$ and $\phi_H = \phi(\hat{\omega}_H)$ and define t_L, ϕ_L similarly. Invoking the Revelation Principle, I can restrict my attention to contracts in which the agent truthfully reports. Thus, let K denote the set of incentive compatible and feasible contracts, i.e. choice of pairs $(\{t_H, \phi_H\}, \{t_L, \phi_L\})$ which satisfy the following constraints:

$$\begin{aligned}
t_H - V_H C(\phi_H) &\geq t_L - V_H C(\phi_L) \; (ICH) \\
t_L - V_L C(\phi_L) &\geq t_H - V_L C(\phi_H) \; (ICL) \\
t_H - V_H C(\phi_H) &\geq 0 \; (PCH) \\
t_L - V_L C(\phi_L) &\geq 0 \; (PCL)
\end{aligned}$$

The contract here takes a rather simple form. The framework does not explicitly include advances and servicing fees, which may also be part of the servicer's costs and compensation,

[16] It is made to facilitate analysis of the First Order Conditions which will be expressed in terms of the marginal costs of carrying out a particular rate of loan modification

[17] this assumption implies that the marginal benefit of a modification will be negative in ω_H. Thus, when analysing the first-best allocation, when marginal costs need to be equal to marginal benefit, we require the marginal cost to be defined for a range that includes the negative part of the real line.

[18] Alternatively, one can simply assume that there are two values of g (g_H and g_L) such that $g_H > g_L$, and all the results would go through.

respectively.[19] I abstract away from these various components of the contract, and assume they are encompassed in the transfer t.[20]

2.3.1.5 Timing

At $T = 0$ the Servicer and the Agent contract on the action and the transfer to be made. At $T = 1$ the state is realized, the agent (Servicer) makes his report, and the transfers and actions are carried out as per the contract.

2.3.2 Contracting Problem

The contracting problem to be solved by the principal can be summarized as follows:

$$\begin{aligned}\max_{\{t_H,\phi_H\},\{t_L,\phi_L\}\in K} & p\left(Z(\phi_H, V_H) + (1-p)\left(Z(\phi_L, V_L)\right)\right. \\ & -pt_H - (1-p)t_L\end{aligned}$$

where the set of contracts to be chosen from, K, incorporates the constraints delineated above - ICH, ICL, PCH, PCL.

2.3.3 Solution

2.3.3.1 Full Information Benchmark

Here, I am interested in characterizing the relationship between ϕ_L^{FB} and ϕ_H^{FB}, the first best rates of loan modification in each state of the world. Such characterization will serve as a benchmark. The IC constraints in the full information case are irrelevant, and the participation constraints will be satisfied by simply reimbursing the agent for the costs incurred. The solution in the full information case will be characterized by ϕ_L^{FB} and ϕ_H^{FB} that satisfy:

$$\begin{aligned}C'(\phi_H^{FB}) &= \frac{1}{V_H}\left[(1-\alpha_1)(m^* + \lambda - V_H)\right] < 0 \\ C'(\phi_L^{FB}) &= \frac{1}{V_L}\left[(1-\alpha_1)(m^* + \lambda - V_L)\right] > 0\end{aligned}$$

[19]The decision to modify or foreclose a mortgage has contrasting implications for the earning of Servicing Fees, and recovery of Servicing Advances made. A servicer makes advances when a loan becomes seriously delinquent, and is required to continue making these Advances until he deems them to be irrecoverable. Modifying a mortgage allows for the continuation of the Servicing Fee to be earned by the Servicer, but may not permit a quick recovery of advances. On the other hand, foreclosing upon a property allows for the quick recovery of advances made. However, as the mortgage is now removed from the loan pool, it reduces the flow of compensation that arises from the servicing fee.

[20]Related to the previous footnote, the assumption that $\lim_{\phi\to 0} C_\phi = -\infty$ might capture to some extent, the idea that the Servicer want's to modify some mortgages, even in the high state of the world, as this leads to more continuation income. Thus, ϕ may not fall all the way to zero even in the high state of the world.

where the inequalities follow from the assumptions made in Section 1.2.[21] I am most interested in the comparison of ϕ_L between the full information case and the asymmetric information case.

The FOCs imply that $\phi_L^{FB} > \phi_H^{FB}$; i.e. the optimal level of modification is higher in the lower state of the world that in the higher state of the world. First, rather intuitively, the more likely the modification is to reduce value of the delinquent mortgages (higher V_H), the lower is ϕ_H^{FB}. In the low state of the world, the higher the rate of redefault, α_1, the lower the first best rate of modification (the intuition here is in line with 4) . This results from the fact that redefault will lead to a dead-weight loss of λ following the loan modification. In the high state of the world ω_H, I find that the higher the rate of redefault, the higher is the first best rate of loan modification. This result arises from two assumptions that I make. Firstly, the assumption with regards to the cost function (Assumption 2c), and secondly from the assumption that $V_H > m^* + \lambda$.[22]

Next, I compute the solution in the case with asymmetric information.

2.3.3.2 Asymmetric Information (Optimal Contracting)

Let ϕ_ω^* denote the solution to the constrained first best case; i.e. the case solved under Assumption 2 made above. Let U_H and U_L denote utility to the servicer in the low and high states under truth-telling. Thus, we rewrite the constraints in terms of U_L, U_H and the cost functions and substitute $t_\omega = U_\omega + V_\omega C(\phi_\omega)$ into the principal's objective function. This simplifies the problem to:

$$\max_{\{U_H,\phi_H\},\{U_L,\phi_L\}} p\left(Z(\phi_H, V_H) + (1-p)\left(Z(\phi_L, V_L)\right)\right.$$
$$-pU_H - (1-p)U_L - pV_H C(\phi_H) - (1-p)V_L C(\phi_L)$$

subject to ICH, ICL, PCH, PCL.

The first order conditions that arise in the solution to this problem are:

$$C'(\phi_H^*) = \frac{p}{V_H - (1-p)V_L}\left(1-\alpha_1\right)\left(m^* + \lambda - V_H\right) < 0 \qquad (FOCH)$$
$$C'(\phi_L^*) = \frac{1}{V_L}\left(1-\alpha_1\right)\left(m^* + \lambda - V_L\right) > 0 \qquad (FOCL)$$

A detailed solution to the problem appears in the Appendix.

The first implication of the First Order Conditions is that $\phi_L^* = \phi_L^{FB}$, i.e. in the low state of the world there is no distortion away from the first best level of loan modification.

[21] Note here that the marginal cost must be defined on the negative part of the real line due to the assumption $V_H > m^* + \lambda$.

[22] In the high state of the world, cash flows from the loan pool do not become negative if foreclosure is initiated, and so, knowing that the same amount can be recovered following a redefault as following a loan modification, a planner may want to modify the mortgage to reduce the dead weight loss. Consider the alternative case whereby we impose the assumption $\gamma < 1$. As $\gamma \to 0$ the right hand side of $C'(\phi_H) \to \frac{(1-\alpha_1)(m^*+\lambda)}{V_H} - 1$. This expression is decreasing in α_1 suggesting that in this case, the first best level of modification would be decreasing in α_1. Looking at the First best FOC for the low state suggests that the inequality above will hold as $\gamma \to 0$ as long as $V_L < (1-\alpha_1)(m^* + \lambda)$.

However, note that this is true only with optimal contracting. Thus, this suggests that one should seek to understand frictions to optimal contracting so as to understand distortions away from the first best level of loan modification, particularly so in state ω_L, when they are most valuable.

Additionally, $\phi_H^* > \phi_H^{FB}$, since $V_H > V_L$ implies $\frac{p}{V_H-(1-p)V_L} < \frac{1}{V_H}$, and because $V_H > m^* + \lambda$. The intuition here is that the principal wants to reduce the rents conceded to the agent, which will depend on the level of ϕ_H^* as per the expression for U_L and the fact that U_L binds. Since $C'(\phi_H^{FB}) < 0$ the rents to the agent can be reduced by increasing the rate of loan modification from ϕ_H^{FB}, thus leading to a distortion upwards. However, potential distortions in the low state of the world are the most concerning to policy-makers.

A second implication follows from the expressions for the transfers to be made to the agent. The solution implies $t_L^* = V_L C(\phi_L^*) + (V_H - V_L)$ and $t_H^* = V_H C(\phi_H^*)$. This implies that in order to obtain the first best level of loan modification in the low state of the world, the agent must be conceded some rents since $t_L^* > V_L C(\phi_L^{FB})$. However, in the high state of the world, the agent is driven to his participation constraint.

Thus, the optimal contracting setting demonstrates one possible channel for distortions away from the first best level of modifications in the low state of the world; the right level of incentives may have been too expensive to provide. Such intuition is also captured by the model of 72.

2.3.4 Multiple Investors

In this section, I incorporate the presence of multiple tranches. To do so, I augment my framework with an audit mechanism, wherein the cost of auditing the action of the agent is increasing in both the probability that an audit is conducted and in the number of investors, N. Here, I impose that the larger the number of investors, the more costly it is to provide incentives to modify, and this results in more distortions away from the first best level of loan modification.

In this setting, the contract will additionally involve a probability of audit in each state (γ_ω) and a punishment if the reported state is found to be different from the true state ($P(\omega, \hat{\omega})$). Following an audit (incurred at a cost $\chi(\gamma, N)$; which depends on the probability of auditing) the principal learns the true state of the world. Make the following assumption.

Assumption 3. *$\chi_\gamma > 0$ and $\chi_N > 0$. Additionally, $\chi(0, N) = 0$ $\forall N$.*

The first is a standard assumption. The second captures increasing frictions to coordination between multiple tranche-holders. The third simply says that there are not costs to a zero probability of audit. Let N now denote the number of investors. In the RMBS market, one can assume that the investors would not know each other's identities. As described earlier, a larger number of investors raises the difficulty of coordinating to either guide the Servicer, or discipline him ex-post. As in the delegated monitoring model of 35 such costs arise due to a free-rider problem. Such free-rider problems may arise because of a simple externality. An investor exerting effort to monitor the Servicer benefits all the other investors, however, he does not internalize this externality. It is these costs of monitoring that this assumption aims to capture. Similarly, as pointed out in 90, to take an action against the Servicer, often the majority of Investors must be in agreement, and often this becomes impractical as different investors may have different views on what the appropriate response of the investor should have been. This assumption attempts to capture in a reduced form the coordination, or collective action problem, that will arise with multiple tranche-holders.

The contracting problem to be solved now becomes:

$$\begin{aligned}\max_{\{U_H,\phi_H,\gamma_H,P_H\},\{U_L,\phi_L\gamma_L,P_L\}} & p\left(Z(\phi_H,V_H)+(1-p)\left(Z(\phi_L,V_L)\right)\right. \\ & -pU_H-(1-p)U_L-pV_HC(\phi_H)-(1-p)V_LC(\phi_L) \\ & -p\chi(\gamma_H,N)-(1-p)\chi(\gamma_L,N)\end{aligned}$$

subject to:

$$\begin{aligned}U_H\geq U_L-(V_H-V_L)C(\phi_L)-\gamma_LP_H\ (ICH) \\ U_L\geq U_H+(V_H-V_L)C(\phi_H)-\gamma_HP_L\ (ICL) \\ U_H\geq 0\ (PCH) \\ U_L\geq 0\ (PCL) \\ P_L\leq l \text{ and } P_H\leq l\end{aligned}$$

Note the differences in the objective function and the incentive compatibility constraints. The audit costs enter in the objective function in both the high and low states. In (ICL), for example, the expression on the right hand side of the inequality includes the expected cost that will be borne if the agent reports that the low state has occurred and gets audited. He will face penalty P_L. The solution to this contracting problem has the same rates of loan modifications (ϕ_H^*,ϕ_L^*) as in 2.3.3.2.[23] However it gives rise to the following additional first order constraint:

$$\chi_\gamma(\gamma_H,N)=\frac{1-p}{p}l$$

Examining this condition, in conjunction with Assumption 3, shows that as N increases, γ_H decreases. As γ_H decreases, ICL becomes more binding, and U_L has to increase to ensure the ICL is satisfied. In other words, as N increases, a higher level of rents need to be conceded to the Servicer in order to achieve the optimal level of loan modifications.

The ex-ante expected cost with N investors is:

$$\begin{aligned}E\left[\tilde{t^*}\right]+E\left[\text{Audit Cost}\right]=&\left(pV_HC(\phi_H^*)+(1-p)V_LC(\phi_L^*)+(V_H-V_L)-\gamma_H^*(N)l\right) \\ &+p\chi(\gamma_H^*(N),N)\end{aligned}$$

where the terms in the first set of parentheses relate to the expected equilibrium payment, and the last term captures the cost of auditing in the high state of the world. As described above

[23] First note that it is not necessary to conduct an audit if $\hat{\omega}_L$ is reported, since ICH is slack in the no audit case, and so $\gamma_L=0$. This in turn makes the choice of P_H irrelevant. Also see that in order to relax ICL as much as possible one can set $P_L=l$. I obtain the solution under the conjecture that PCH and ICL are the only remaining relevant constraints and confirm later the conditions under which this will hold.

$\gamma_H^*(N)$ will depend on N due to the first order condition. Note that $\frac{\partial(E[\tilde{t}]+E[\text{Audit Cost}])}{\partial N} > 0$. Increasing N raises the audit cost, and increases the rents that need to be conceded to the agent due to the effect on γ_H. This suggests that multiple investors make implementation of the optimal contract more costly. Additionally note that if we restrict the contract to be state contingent in this manner ($\phi_N \neq \phi_H$), the expected payoff to the investors $E[Z(\phi_\omega, V_\omega)]$ does not vary with N.

2.3.4.1 Rigid contract with no incentives ($\phi_H = \phi_M = \bar{\phi}$)

Let us now assume that the Sponsor has the option to provide a contract that specifies the same rate of loan modification in either state of the world, $\phi_H = \phi_M = \bar{\phi}$. With such a contract, no rents need to be conceded to the Servicer. Such a contract would prescribe $\phi_H^{FB} < \bar{\phi} < \phi_L^{FB}$ (see Appendix for F.O.C.). It would achieve below optimal rate of loan modification in the low state of the world. Under this contract there is no requirement to audit the agent and $\gamma_H = \gamma_L = 0$. Therefore;

$$E\left[\tilde{t^R}\right] + E\left[\text{Audit Cost}^R\right] = \left(pV_H C(\bar{\phi}) + (1-p)V_L C(\bar{\phi})\right) + 0$$

where the R subscript denotes the outcomes for the rigid contract case. In this case, because the audit mechanism does not enter the equation, both the expected costs and the expected payoff from the pool of delinquent mortgages will not change with N. Comparison of this with the expression for $E\left[\tilde{t^*}\right] + E\left[\text{Audit Cost}\right]$ above shows that, under appropriate parameter restrictions, if the N is sufficiently high, the Deal Sponsor might prefer to implement this rigid contract instead.[24]

In summary, increasing N will lead to deviations from the first best rate of loan modification in the bad state, ϕ_L^{FB}, for two reasons. Firstly, in the case of implementing a state contingent contract, increasing N raises the costs of auditing the agent. If such a contract is implemented and audit not properly conducted, the agent's actions will likely diverge from first best. Secondly, if N is sufficiently large, the Deal Sponsor may choose to implement a rigid contract. The empirical analysis will not directly test for the implication of increasing N on the form of the contract - this is left for future work. In the subsequent sections, I will focus on testing the implications of the framework for the rate of loan modification. However, the empirical strategy used will have to explicitly account for the fact that Pooling and Servicing Agreements (the contracts between Deal Sponsor and Servicer) will have a direct effect on the rate of loan modification.

[24]How the payoffs to investors, $E[Z(\phi_\omega, V_\omega)]$, differs under the two contracts will depend on the assumptions made about the cost functions C as well as on whether $\bar{\phi} \lesseqgtr \phi_H^*$. Also note that if the Sponsor places value on reducing payments to the Servicer, the more likely we are to see below optimal rate of loan modification arise in this setting. One situation in which this may be the case is when the Servicer and Sponsor are unaffiliated entities 51 studied the effect of Sponsor and Servicer affiliation on the rate of loan modification. Under the assumption that Sponsor's held the equity piece of the RMBS deal, they find that affiliation between the Sponsor and the Servicer translates to a wealth transfer to the equity tranche via either delay in liquidation of delinquent mortgages, or aggressive mortgage modification.

2.3.5 Theoretical Literature on Mortgage Servicing

Although stylized, the model highlights aspects of agency problems in mortgage securitization that previous theoretical literature has not addressed. One related model is 91. They model one investor, or lender, and a continuum of borrowers, who have private information about their ability to make the mortgage payments. Some borrowers can become current without a loan modification, while others need a modification in order to recover from their delinquency. The single lender chooses a modification policy which applies to all borrowers that remain delinquent and renegotiate with the lender. i.e. the "distressed" borrowers. Borrowers with the ability to become current without the modification, may "strategically" default and mimic the "distressed" type so as to try and get a concession. This possibility of strategic default induces the lender to reduce the rate of loan modification away from the first best. While their work carefully models the information asymmetry between the borrower and the lender, they do not account for the fact that the Servicer is responsible for the modification decision, and thus constitutes an additional layer of information asymmetry. They do not account for the fact that Securitization involves multiple investors rather than a single investor. My framework adopts a simpler approach to modeling the borrower. However, the distortion to first best rate of loan modification comes from the unwillingness to concede high rents to the Servicer.

Secondly, the framework relates to the model of 72. Using an incomplete contracting framework to model the renegotiation problem between a borrower and a lender, they introduce a Servicer as an intermediary who has to be incentivized to gather the right amount of information, and carry out the optimal level of loan modification. Thus a Servicer is offered a contract that provides him some "skin-in-the-game", i.e. his compensation is a fraction of the cash flow that arises from the mortgage pool less a price of obtaining this risky position. However, if the value of the risky position to the Servicer is lower than the price, the investor optimally makes him commit to a no information gathering equilibrium in which no renegotiations are carried out. Here, instead of considering how variation in the systematic risk of underlying collateral affects the contract with the Servicer, I consider the effect of multiple tranches on this contract. However, I model the contract in a much simpler manner.

2.3.6 A note on empirical implementation

A key aspect of the framework is the number of investors, N. In examining the empirical implications of the framework, to be outlined below, I will in effect be assuming that the number of tranches in a particular securitization deal is a proxy for the number of investors. It is difficult to determine, using existing data, the exact ownership of tranches in RMBS transactions. In the empirical analysis, I will use two different measures, for the number of investors.

2.4 Hypotheses

The framework highlights two key points that inform the empirical analysis. Firstly, multiplicity of tranches worsens the incentive compatibility constraint and increases the fees or payments that need to be conceded to the Servicer to implement the optimal level of loan modifications. Secondly, the rate of loan modifications implemented will be closely tied to the contract provided to the Servicer, and thus, one must control for this contract in any estimation framework. Recent literature on loan modifications has failed to take this into account. For example, the Sponsor might wish to put in place a right contract that ties the Servicers hands and reduces the rate of loan modification. These insights allow us to form the two main hypotheses.

Earlier studies examining the rate of loan modification abstract away from the differences between securitization deals. Additionally, apart from 62, they remain agnostic on the potential channel via which these effects may manifest themselves. 78 highlight the role of dispersion of ownership inherent in securitization - "securitization creates dispersion in property rights—cash flow rights on a mortgage are held by several bondholders with varying seniority of claims. This raises concerns that complex capital structure, brought about by securitization, may create a coordination problem amongst investors making it harder for servicers to alter mortgage contracts". The framework developed shows how such "dispersion of ownership", inherently tied to the multiplicity of tranches, interacts with the agency problem created by the use of the Servicer to influence the rate of loan modification. This motivates the first hypothesis.

Hypothesis 1. *Extensive Margin: All else being equal, the more the number of tranches on a particular pool of mortgages, the lower the probability of loan modification conditional on delinquency.*

8 build upon the previous paper by studying the various types of loan modifications used, in addition to just the rate of loan modification. Their evidence suggests that modified portfolio held loans involved smaller interest rate and payment decreases, and smaller principal increases, relative to loans that were in private label securitizations. In my setting, the analogy to being portfolio held is having a single tranche on a mortgage pool. Thus, the insight of 8 suggests that higher number of debtors predicts more aggressive modifications in terms of principal balance, and less aggressive modifications in terms of interest rate and principal payments.

However, my framework suggests that one also needs to take into account the fact that the PSA design will influence the Servicer's action. This is completely abstracted away from in the analysis of 8. The main component of a Servicer's compensation is the Servicing Fee. Among his main costs of maintaining delinquent mortgages is the foregone interest rate on Advances made to debt-holders (since Advances essentially constitute an interest-free loan to investors). Servicers prefer implementing modifications that increase the outstanding balance for two main reasons. Firstly, they increase the basis for their main source of income - the servicing fee received based on the outstanding balance of mortgages serviced. Secondly, once arrears are capitalized, the borrower can be classified as being Current, which then allows the Servicer to speedily recover any Advances made for missed payments, or any expenses incurred for the modification. However, these types of modifications lead to a higher rate of redefault as documented by 48. Another option that the Servicer has is a modification which reduces the interest rate, without leaving other terms changed; thus resulting in the relaxing of a borrower's liquidity constraint. However, such modifications reduce the rate at which the Servicer make recover advances made. Additionally, a reduction in the interest rate implies that the mortgage principal is paid down more speedily, which further reduces the Servicer's compensation via the servicing fee. Thus if creating multiple tranches, increasing N, worsens the agency problem, I would expect Servicers to implement modifications that are more likely to raise their compensation. That is, one would expect to observe more modifications that increased the principal balance, and less likely to observe modifications that decreased the interest rate. This discussion allows me to motivate Hypothesis 2.

Hypothesis 2. *Intensive Margin: The higher the number of tranches - the lower the probability of a modification involving only a reduction in principal, and the lower is the predicted decrease in the principal balance conditional on receiving such a modification. The effect of multiple tranches on modifications involving changes in interest rates remains ambiguous.*

There are two required steps to test the hypotheses above. Firstly, I need to develop a measure of tranche multiplicity that sufficiently captures the variation in the different types of deal structures. Secondly, I need to delineate an empirical strategy which allows me to control for the incentives provided to the Servicer. In the next Section, I outline my empirical approach.

2.5 Empirical Strategy

2.5.1 Measures of Multiplicity

2.5.1.1 Count of Tranches Mapping to Each Pool

As a first measure of multiplicity, I take the simplest approach, which is simply to count the tranches that have claims to each individual loan pool in a particular RMBS deal. Therefore, for the example deal described earlier, the count for loan pool 1 would be 8 and the count for loan pool 2 would be 6. I standardize the variable so that it has mean 0 and variance 1. This approach does have it's shortcomings, and thus, a more nuanced approach will be needed.

2.5.1.2 Herfindahl Hirschman Index (HHI) based Measure

One shortcoming of the measure delineated above is that simply considering the count of tranches does not capture the rich variation in deal structure. Consider the earlier example. Suppose the '1-A' stack now had only two tranches, each equal in size to the other. Even though stacks 1-A and 2-A would have the same number of tranches, stack 2-A has one tranche which is dominant. To capture these differences in structuring, I use a more nuanced approach.

I use the additional data on the size of the tranches obtained from ABSNet. I want to construct a measure that captures not only the number of tranches, but also takes into account their relative size. Thus, I take an approach inspired by the Herfindahl Hirschman Index used in studies such as 89, where it is used to measure the concentration of the members in a syndicated lending deal. Essentially the Index is a weighed average of the the face value of each tranche that has a claim to the loan pool, where the weights are equal to the share of the tranche's face value among all tranches that have claims to the loan pool. First, let us construct a simple example based on the structure of MASTR Adjustable Rate Mortgages Trust 2004-11. Table 2.1summarizes the debt structure. Column 1 lists the tranches of the deal, Column 2 denotes the loan pool collateralizing each tranche, Column 3 denotes the class balance of the tranche at deal closing.

The total balance of tranches that have claims to loan pool 1 is \$361 million, and the total balance to tranches with claims to loan pool 2 is \$418 million. As column 4 shows, tranche 1-A-1 has a weight of 0.4 with respect to loan pool 1 (\$145 million/\$361 million). Similarly, tranche 2-A-1 has a weight of 76% with respect to loan pool 2. Taking a sum of the squared weights that appear in Column 4 and 5, I obtain a HHI based measure of multiplicity of debt claims for pool 1 and pool 2 respectively. The measure is 0.27 for pool 1 and 0.6 for pool 2. This suggests that loan pool 2 has a lower multiplicity of debt claims.

Such a measure also gets us closer to capturing the "dispersion of ownership" as mentioned by 78. However, it is only a proxy for ownership, as there does not exist any comprehensive data on the identity of the investors in RMBS transactions. More formally, the measure is constructed as follows. Assume that a deal has N tranches given by the set $T = \{T_1, \ldots, T_N\}$ collateralized by K loan pools given by the set $P = \{P_1, \ldots, P_K\}$. The data provides a mapping $M(P_i) = \{T_k \in T | T_k \text{ receives cash flows from } P_i\}$ which determines the tranches of a deal that receive cash flows from the loan pool i. The measure of multiplicity $C(P_i)$ is calculated as:

$$C(P_i) = \sum_{T_k \in M(P_i)} \left(\frac{V_{T_k}}{\sum_{T_k \in M(P_i)} V_{T_k}} \right)^2$$

where V_{T_k} is the principal balance at origination of the tranche T_k.

Figure 1 below depicts the distribution of this measure. The measure $C(P_i) \in [0, 1]$ and has mean 0.43 and standard deviation 0.23. The lower the measure the higher the multiplicity of tranches, and the more dispersed the "ownership" of the underlying collateral. One shortcoming of this measure, however, is that it may be difficult to interpret in a regression framework.

The preferred measure of tranche multiplicity will be $C(P_i)$, the Herfindahl index based measure.

2.5.1.3 Advantages of using pool-level measures

The advantage of using such pool-level measures of deal structure instead of tranche-level measures (for example, subordination) is that we can unambiguously assign an individual mortgage to a particular loan pool. The complex waterfall structure of RMBS deals makes it difficult to assign the cash flows from an individual mortgage to a particular tranche. Thus, there arises a challenge in associating these tranche level measures of deal structure to individual loan modification outcomes. However, each mortgage in our data includes an identifier for the loan pool that it belonged to. The approach of using this loan pool level measure has its drawbacks as well. One pertinent drawback is that a particular mortgage pool often cross-collateralized others in the same RMBS deal. These cross-collateralization measures are not always captured in the data as they are often described only in the deal prospectus.

To better understand what drives variation in these measures, I estimate regressions with each of them as the dependent variable, and a vector of pool level averages of mortgage characteristics at origination as the independent variables. Table 2.2 presents the results. Column 1 and 4 performs the baseline regression, in Column 2 and 5 I control for vintage fixed effects, and in Column 3 and 6, I control for deal fixed effects. Overall, the table suggests that riskier mortgages (higher LTV, Cash Out Refinance, Non-Owner Occupied, ARMs) were in loan pools with lower number of tranches (higher $C(P_i)$ measure). The results indicate that the structuring of the tranches was not randomly assigned. However, given the richness of the data-set, I control for all these loan level observables in running the regressions in Section 2.6.

2.5.2 Estimation Frameworks

The main linear probability model specification used to test Hypothesis 1 and 2 is:

$$Y_{ipd} = \alpha + \gamma_{deal} + \eta_{CBSA \times t} + \beta_1 \cdot Multiple_p + \beta_2' X_{ipd} \tag{2.1}$$

where the left hand side is an indicator for whether a loan i from loan pool p of RMBS deal d that was at least 60+ days delinquent was modified (in the case of testing Hypothesis. 1) or for whether a loan received a particular type of modification; γ_{deal} are deal fixed effects; $\eta_{CBSA \times t}$ are CBSA by quarter of serious delinquency fixed effects; $Multiple_p$ measures multiplicity of tranches and X_{ipd} are loan and borrower level control variables.[25] The use of γ_{deal} is crucial here, and is utilized in a similar fashion to 1. As described in Section 2.2, and highlighted by the framework, one must control appropriately for the provision of incentives to the Servicer; which are determined by the PSA at the deal level. A reading of PSAs demonstrates that the guidelines and compensation provided to the Servicer are not loan pool or tranche specific, but make reference to all mortgages underlying a particular RMBS deal. While the contents of the PSAs are public information, they are challenging to codify, and thus I assume the PSA is unobservable to the econometrician. Using γ_{deal} implies that identification of β_1 comes from comparing the modification outcomes of loans *within the same deal* but in loan pools that have different degrees of multiplicity of tranches. Thus, I am holding constant not only provision of incentives to the Servicer, but also other

[25] I control for whether the loan was Owner Occupied or not; the presence of private mortgage insurance, whether there was a second lien present on the property, CLTV at origination, CLTV at origination squared, Log of the original appraised value, the interest rate, the age of the loan (in months) at delinquency, and a set of indicators for loan contract features (ARM, IO, Negative Amortization, Balloon, Prepayment Penalties).

aspects of the deal that are common to all loan pools. One may be concerned that affiliation between parties of the deal (for e.g. the Sponsor and the Servicer) may have implications for the decision to modify a delinquent mortgage. Using γ_{deal} mitigates such concerns.

I define $Multiple_p$ as either the count of tranches or as $Multiple_p = C(p)$, the Hefindahl based measure of multiplicity.

In addition to the Linear Probability Model above, I also estimate a proportional hazards specification to test Hypothesis 1, I use the framework from 75. The latent variable is time to modification. The instantaneous probability of modification at time t for loan i, of loan pool p in deal d, year of serious delinquency c and zip-code z will be given by:

$$\lambda(X_{ipdcz}(t), t) = exp(X'_{ipdcz}(t)\beta)\lambda_0(t)$$

where $\lambda_0(t)$ is the baseline hazard function that only depends on time since serious delinquency, t. The specification for the covariates is:

$$X'_{ipdcz}(t)\beta = \gamma_c + \gamma_d + W_{B,i}\theta_B + W_{L,i}\theta_L + \mu \cdot \Delta HPChange_z(t) + \eta \cdot C(p)$$

where γ_c are year of serious delinquency fixed effects, γ_d are deal-level fixed effects, $W_{B,i}$ and $W_{L,i}$ are a set of borrower and loan level controls as at origination or serious delinquency. The only time varying variable in $X_{icz}(t)$ is the House Price Change ($\Delta HPChange_z(t)$) over the past three months for zip-code z which is computed using Zillow House Price Index data. I estimate this specification on a 5% random sample of seriously delinquent mortgages.

2.5.3 Data

The main source of data used here is ABSNet Loan. ABSNet Loan aggregates data from Residential Mortgage Backed Securitization (RMBS) Trustees, and covers the majority of Private Label RMBS issuances. Two aspects of the data are crucial for my purposes. Firstly, it provides us a detailed description of the pooling and tranching structure of the deal, as well as data on the loans underlying each loan pool of the deal. Crucially, I am able to match each loan to it's corresponding loan pool, and each loan pool to a tranche that draws cash flows from it. For each loan, I observe characteristics of the mortgage contract and the borrower, and a detailed history of the payment status of the loan. Also, importantly, I observe all modifications made to the loans in the loan pool, as well as the type of modification (deferral of payment, missed payment capitalized, principal forgiven, etc.). This data is augmented with data on the original balance and rating at origination of the bond tranches. The baseline sample will consist of loans that went at least 60+ days delinquent before January 2009. I use this restriction to ensure that the effects of government interventions to encourage modifications, i.e. the HAMP program, do not confound my results. I also obtain the name of the Servicer for each loan in the pool. The final sample consists of about 3500 deals and 2,700,000 mortgages originated between 2002 and 2007.

Table 2.3 shows us summary statistics for the sample of loans used in the regressions. Since these loans are those that go at least 60+ days delinquent, one sees the borrowers were not as credit worthy, with a FICO score of about 625. Additionally, about 22% of the loans in this sample receive some form of modification. This rate of modification considers all modifications occurring at any length of time following the delinquency. It does not restrict to only modifications made within the first or second year of the loan modification. About 57% of the loans see the mortgage foreclosed upon at any point following delinquency. Note that this figure includes loans that were modified and then subsequently foreclosed upon. On average, the loans are about a year and a half old by the time they become seriously delinquent.

Loans in the sample may be modified multiple times. If a loan is modified multiple times, the average length of time between modifications is on average 9 months. The linear probability model regressions in Section 2.6 and Section 2.7 are performed on the cross section of loan modifications. That is each observation corresponds to one mortgage, and an indicator for whether or not it has been modified at least once. If the mortgage has been modified more than once, the data aggregates these multiple modifications. For example, I create the indicator variables for the various modification types by comparing the loan features (outstanding balance, monthly payment, interest rate) prior to the first modification and following the last modification recorded for each particular mortgage in the dataset. Among modified mortgages, the median number of modifications is 1. 75% of modified mortgages received 2 or fewer modifications, and 90% of modified mortgages received 3 or fewer modifications.

In addition to the sources above, I use county level house price indices from Zillow to control for county level changes in house prices.

2.6 Results

2.6.1 Extensive Margin of Loan Modification

2.6.1.1 Linear Probability Model

Table 2.4 shows the results from estimating the specification in equation (1). Columns (1) to (3) show the results using the Tranche Count as the measure of tranche multiplicity. Columns (4) to (6) implement the regressions with the $C(p)$ as the measure of tranche multiplicity. Columns (7) to (9) use, as a robustness check, the measure 'GSE Pool' as per Adelino et al. (2015). Columns (1),(4) and (7) use the specification with CBSA by Quarter of Delinquency Fixed Effects and Deal Fixed Effects. In Column (2), (5) and (7) I add as a control $\Delta HP_{county,t}$ computed as $\Delta HP_{county,t} = ln(HP_{c,t}) - ln(HP_{c,t-6})$ using the Zillow county level house price index. This will be the preferred specification. Finally, In Columns (3), (6) and (9) I use Deal by Servicer Fixed Effects as well.

First, consider the result in Column (2). This indicates that a one standard deviation increase in the count of tranches to a loan pool predicts an increase in the probability of loan modification of 105 basis points. In Column (5) and (6) the coefficient on $C(p)$, the HHI based measure, suggests that increasing the concentration from the 25th percentile of the measure to the 75th percentile of the measure predicts an increase in the probability of a loan modification by 134 bps. This corresponds a 1.5 standard deviation increase in this measure. As a point of comparison, the in-sample probability of loan modification conditional on delinquency is about 21.4%.

As per my identification strategy, I have controlled for all deal-level unobservables that do not vary with time. Thus, I am holding constant various elements of the deal-structure such as initial Credit Enhancement features (Subordination, Overcollateralization and Net Excess Spread). Additionally, I am holding constant features and structure of the contract between the deal Sponsor and the agent responsible for the loan modification and foreclosure decision, the Servicer. This contract is put in place at the origination of the deal. One may be concerned by the fact that within a deal, Loans may have had different Servicers. Thus, the effect estimated may arise from differences in Servicing practices related to the identities of the Servicers in loan pools with different concentrations. In order to mitigate these concerns, I construct fixed effects at the Deal by Loan Servicer level. Thus, I am now identifying the coefficient β_1 comparing loans in different pools of the same deal that were serviced by the same Servicer. In other words I am controlling for differences in Servicing practices of each servicer. The results using these fixed effects are reported in Columns (3), (6) and (9). As can be seen, my estimates remain robust to this specification as well. To further illustrate this point, I separately estimate the $\hat{\beta}_1$ coefficient on each of the Servicers among the top 10 in the sample. The top 10 servicers account for 62% of the loans in the sample. As observed in Figure 3, the coefficient on $C(p)$ is positive and statistically significant for 7 of the top 10 Servicers. The coefficient on Tranche Count is negative and statistically significant for 6 of the top 10 Servicers in the sample. While the effect of multiple tranches is heterogenous across Servicers, the direction of the effect is consistent with the estimate from the pooled regression.

One concern might be that the results are susceptible to an omitted variable bias. For example, a Servicer looking to increase it's exposure to the subprime market may have purchased Mortgage Servicing Rights for a particularly risky pool of mortgages, which consequently appear in our sample. Once these loans start becoming delinquent the Servicer may target their resources towards modifying this set of mortgages. To mitigate concerns about such biases driving our results, I first restrict attention to loans that might be considered of a higher quality and loans where the origination decision is less likely to have been made primarily on the basis of "soft" or unobservable information. I consider three different subsamples, Full Documentation loans, loans with balances below the conforming loan limit, and 30 Year Fixed Rate Mortgages. The results of these regressions appear in Appendix Table B.1. Looking at Columns 1 to 6 of Panel A indicates that the coefficients on $Multiple_p$ are comparable to, and in most cases, stronger than the baseline results presented in Table 2.4. This is suggestive evidence that the results are not simply driven by a particularly risky group of borrowers.

In additional results documented in Columns 7 and 8 of Appendix Table B.1I document that the effect is larger if one considers only the sample of loans that were Fixed Rate Mortgages or Adjustable Rate Mortgages without any complex features such as Negative Amortization or Interest Only payments. There are valid reasons to restrict attention to these less complex mortgages; complex mortgages were likely to be obtained by more sophisticated borrowers (19) and were susceptible to predatory lending (32). One might also argue that if such complex mortgages went to those who borrowed beyond their means; we should be most concerned about the consequences of securitization on those who did not enter into these particularly "risky" contracts. The results show that this mechanism of multiple tranches is particularly important exactly borrowers who did not have complex contracts.

In addition to understanding the effect of multiplicity on various subsamples of mortgages, I take another approach to mitigate concerns that the results are driven by characteristics of the borrower which are unobservable to the econometrician. In a first step, I estimate a regression of a 1-0 indicator for 90+ day delinquency on a sample of loans that are in all the RMBS deals in my sample (i.e. I consider all originated loans, without conditioning on them reaching any state of delinquency). I include as regressors all the loan and borrower covariates included above. I also include CBSA by Quarter of Origination FE and Deal FE. I then use the residuals from this regression as an additional regressor in equation (1). In essence, I am attempting to capture all the unobservables that might predict default. The use of the LPM might be problematic here. One might use a logit or probit framework. However, it becomes challenging to estimate a binary outcomes model with a large number of fixed effects. One can think of the residuals from the first stage as an index of the unobserved quality of the mortgage. As can be seen in Appendix Table B.3, the results remain robust to the addition of these residuals as additional covariates, suggesting that the coefficient $\hat{\beta}_1$ is not substantially biased due to the correlation between loan quality and it's placement in particular loan pools.

A similar analysis was carried out to assess the probability of an increase in the multiplicity of tranches on the probability that following serious delinquency a loan is foreclosed upon. The results demonstrate that tranche multiplicity (as measured by either metric) predicts a lower probability of foreclosure. These results appear in the Appendix Table B.4.

Henceforth, I use $C(p)$, the HHI based measure as my preferred measure.

2.6.1.2 Proportional Hazards Model

The results appear in Tables 4A and 4B. Table 2.5 uses as the regressor of interest the HHI based measure of multiplicity, while Table 2.6 uses the standardized Tranche Counts. Column (1) shows the baseline specification without any fixed effects added. Column (2) includes the CBSA fixed effects. Column (3) includes the Deal fixed effects that form the crux of the identification strategy. I drop the CBSA fixed effects in Column 3 so as to keep the model parsimonious. Moreover, looking across Column 1 and Column 2 it does not appear that the CBSA fixed effects vary the estimates substantially. However, I cluster standard errors at the CBSA level. In Column (4) I add the borrower and loan level control variables. I interpret the estimates in Columns 4 of each table. Exponentiating the coefficient to obtain the hazard ratio demonstrates that moving from the 25th to the 75th percentile of the HHI distribution predicts an increase in the hazard rate of loan modification of 7 percentage points. Similarly, a 1 S.D. decrease in the Tranche Count measure reflects an increase in the hazard rate of loan modification of 11.2 percentage points.[26]

[26] In other words, the above increase in the HHI measure makes the loan 7% more likely to be modified, and a 1 standard deviation decrease in the Tranche Count measure makes a loan 11.2 percentage points more likely to be modified.

The results confirm the hypothesis on the hazard rate of loan modification. These results complement those of 78 and 8. A loan held on a bank's portfolio has one investor, the bank itself. Such a mortgage would have an HHI measure of 1, the highest possible value of $C(p)$. This corresponds to a higher hazard rate of loan modification, qualitatively confirming their findings. Additionally, lower "dispersion of ownership", to use their term, lowers the hazard rate of loan foreclosure.

2.6.1.3 Comparison to other estimates in the literature

78 estimate in a Cox Proportional Hazard model the effect of securitization on the hazard rate of foreclosure. They find (Table 5 in their paper) that portfolio held loan is 24 percentage points less likely to be foreclosed upon as compared to a securitized loan (i.e. the "Portfolio" dummy variable has a hazard ratio of 0.759). Estimates from my hazard rate of foreclosure specification (see Appendix Table B.5) shows that a 1 S.D. decrease in the Tranche Count measure indicates that a mortgage is 10.6 percentage points less likely to be foreclosed upon. Similarly moving from the mean $C(p)$ of 0.43 to a $C(p) = 1$ (i.e. the hypothetical $C(p)$ of a portfolio held loan) indicates that the delinquent mortgage is 8.1 percentage points less likely to be foreclosed upon. Thus, the multiplicity of tranches in securitization accounts for about 34% to 44% of the effect captured by 78.

2.6.2 Intensive Margin of Loan Modification

In the previous section, I observe that mortgages in loan pools with fewer tranches on the underlying collateral were more likely to be modified conditional on being seriously delinquent. Next, I look at the intensive margin of loan modification, i.e; I look to test Hypothesis 2 outlined in Section 2.4. For the tests that follow, I restrict my sample to mortgages that were modified.

As a first step in the investigation of the intensive margin I wish to understand the effect of tranche multiplicity on the probability of a modification involving a change of payment or a change in the outstanding balance (note, this may be either an increase/decrease in the balance or monthly payment). Additionally, I also wish to estimate an elasticity with which I can perform a back of the envelope calculation to obtain an understanding of how much decrease in the multiplicity of tranches would change the dollar value of relief given to borrowers. To that end, I first estimate a regression of $P(PMT\,Change|Modification)$, and $P(Balance\,Change|Modification)$ on the Tranche Counts, and the HHI measure $C(p)$ for loan pool p. I also estimate regressions of $ln\left(\frac{PMT_{i,post-mod}}{PMT_{i,pre-mod}}\right)$ and $ln\left(\frac{Balance_{i,post-mod}}{Balance_{i,pre-mod}}\right)$ on ln(Tranche Count) and $ln\,C(p)$ (where i indexes a particular mortgage in loan pool p).[27] This regression provides me with an elasticity measure. The results of the regression appear in Table 2.7. First note that since most modifications involved a change in payment or a change in the outstanding balance, there is a negligible effect of tranche multiplicity on the probability of these changes. However, as column 3, suggests there is a relationship between tranche structure and the generosity of such loan modifications.

[27] where $\frac{PMT_{i,post-mod}}{PMT_{i,pre-mod}}$ is the ratio of post modification payment to pre-modification monthly payment.

Consider the results in Column (3). The elasticity coefficient is estimated to be -0.0089. There are about 600,000 modifications in my sample, 84% of which had a change in payment. The average pre-modification payment is $1570 and the average ratio of post modification to pre-modification payment is 0.72. This suggests that a 100% increase in the HHI based measure (approximately moving from the 25th percentile to the 75th percentile) predicts an aggregate decrease in monthly payments by $5,070,522. Next consider the results in Column (7). Of the 600,000 modifications, 96% had changes in the outstanding balance. The average pre-modification balance is 213,801 and the average ratio of post-modification to pre-modification balance is 1.026. Thus, a 100% increase in the multiplicity measure predicts a decrease in the post-modification balances by $252,846,850.

Next, I restrict my attention to modifications that reduce monthly payments but do not offset these with an increase in the mortgage balance. 38 argue that these types of modifications are likely to be most beneficial to the borrower since they relax the liquidity constraint without pushing the borrower's default option further in the money. However, Servicers may not prefer such a modification as it reduces the recovery of Advances they would have made for the delinquent mortgages. Thus, to the extent that tranche multiplicity (i.e. a proxy for less concentrated ownership) exacerbates the agency problem, we should expect that a lower Tranche Count, or a higher $C(p)$ measure, predicts more of these modifications, and more aggressive ones as well. To test this hypothesis, I estimate the main specification with the dependent variable as a 1-0 indicator for whether a modification involved a reduction in the payment without an increase in the outstanding balance. Additionally, as before, I estimate an elasticity coefficient. The results appear in Table 2.8.

Table 2.8 indicates that multiplicity of tranches does not predict a higher probability of such modifications. However, the elasticity coefficient is statistically significant. The elasticity coefficient suggests that a 100% increase in the HHI based measure predicts an aggregate decrease in monthly payments by about $2,500,000 (600,000 loan modifications, 27.5% receiving payment reductions without balance increase, $1690 average pre-mod payment, 0.58 average ratio of post mod to pre-mod payment).

Next, I restrict my attention to modifications that increased the mortgage balance without reducing the monthly mortgage payments. 48 suggests that such modifications lead to higher redefault. However, they are preferred by the Servicer as they lead to an increase in the basis for the servicing fee that forms the main component of its compensation. I estimate similar specifications to Table 2.8, but now consider a 1-0 indicator for whether the loan received a modification that increased the mortgage balance without reducing the monthly payments. If multiplicity of tranches worsens the agency problem, then a lower Tranche Count, or a higher $C(p)$ measure should predict a lower probability of such modifications, and a smaller increase in principal balances conditional on receiving such a modification. The results appear in Table 2.9.

Table 2.9 suggests that a decrease in the multiplicity of tranches predicts a lower probability of a modification that increases the borrower's balance without reducing payments. Moving from the 25th to the 75th percentile of the HHI measure predicts a 96 bps decrease in the probability of such a modification. Of the 600,000 modifications, 20% involved such a modification in which the outstanding balance was on average reduced by $16,000. Thus the estimated benefit to delinquent borrowers from a 96 bps reduction in the probability of such a modification amounts to about \$93 million. However, there does not appear to be any differences in the generosity of such modifications once we compare loans with a higher multiplicity of tranches to a lower multiplicity of tranches.

2.7 Robustness and Extensions

2.7.1 Effect of Multiplicity on GSE Pools

The mechanism proposed so far indicates that multiple claim holders find it difficult to coordinate and discipline the agent, i.e. the mortgage Servicer. There may an outstanding concern that these measures of multiplicity might proxy for other features of the underlying collateral or the borrowers which in fact predict the rate of loan modification. This next test examines the situation where for some loan pools all the tranches were likely to be held by the same investor. In this case, multiplicity of tranches should not matter for the rate of loan renegotiation. If multiplicity predicted the rate of loan modification only via some underlying, unobserved and unaccounted feature; we would still expect the multiplicity measure to matter for loan renegotiation of mortgages in such loan pools.

The test is motivated by, and uses, the blockholder measure created by 1 for the context of RMBS. They use the fact that GSEs were large investors in private label securitization, and that private label securitization deals had separate mortgage pools specifically created to collateralize AAA tranches that were ultimately held by the GSEs. For e.g. in 2004 Fannie Mae purchased \$90.8 billion of PLS, while Freddie Mac purchased \$121.1 billion. 1 test for the presence of large investors on the quality of assets. Ownership of a loan pool by the GSEs (via ownership of the tranches) proxies for lower debtor multiplicity for that particular pool. Thus, I hypothesize that if a pool is “owned” by the GSEs, multiplicity of tranches should not predict the rate of loan modification and foreclosure.

I follow the algorithm of 1 to determine whether the a loan pool was used as collateral for tranches purchased by the GSEs. In particular, the algorithm exploits the fact that the GSEs are only allowed to acquire mortgages below the conforming loan limit, a fixed dollar amount set annually by the government. The algorithm determines a loan pool to be collateral to GSE-owned bonds if at least 99% of the loans in the loan pool have a balance below the conforming loan limit of the given year at the time of deal closing, and if the number of loans that are not first lien mortgages make up less than 75% of the loan pool.[28] On average, depending on the deal vintage considered, about 20% to 30% of the loan pools in our sample are indicated to be GSE Pools by the applied algorithm. On average, these loan pools make up about 12% to 16% of the total collateral in their respective deals. The correlation between the GSE Pool indicator and the HHI-based measure $C(P_i)$ is about 35%.

[28] There is some error associated with this. If data is not available on the balance of the loan at the exact month of deal closing we look out up to 6 months after the deal closed. Thus I may be overestimating slightly the number of loans with a conforming loan balance. See Internet Appendix of 1 for more details on the algorithm.

I then combine the multiplicity measure $C(p)$ and the GSE Pool Indicator into a single regression. I estimate equation (1) but with:

$$Multiple_p = \begin{array}{c} C(p) \\ GSEPool_p \\ C(p) \times GSEPool_p \end{array}$$

or

$$Multiple_p = \begin{array}{c} TrancheCount_p \\ GSEPool_p \\ TrancheCount_p \times GSEPool_p \end{array}$$

Table 2.10 shows the results. The first two columns have as dependent variable a 1-0 indicator for whether the delinquent loan was modified. The second two columns have as a dependent variable a 1-0 indicator for whether the delinquent loan entered foreclosure. Columns 1 and 3 use the HHI based measure as the measure of tranche multiplicity. Columns 2 and 4 use Tranche Count measure. As can be seen, a pool being classified as a "GSE Pool" offsets the effect of tranche multiplicity on loan modification and foreclosure.

Using this measure does, after all, have it's limitations that one must be mindful of. Firstly, we do not know with certainty whether a GSE held the tranches collateralized by such loan pools. Secondly, we may be capturing an effect that is correlated with debtor multiplicity, but which affects loan modification via a political economy type channel. The GSE's were large participants in the market, and being government sponsored entities, they would have preferred to modify delinquent mortgages. Alternatively, they may have induced such a response from the Sponsors and Servicers by virtue of a reputation channel. Such a channel may be correlated with tranche multiplicity (63).

2.7.2 Rate of Mortgage Redefault: Assessment of Modification Success

So far, we have seen that lower multiplicity of tranches predicts more modifications, and more aggressive modifications conditional on receiving one. To more completely assess the effectiveness of this multiplicity in improving outcomes from delinquency, I test for it's effect on the rate of redefault of modified mortgages. To do so, I restrict the sample to loans that were modified and classified as "Current" in the month following the modification. Then for each modified mortgage, I construct a 1-0 indicator for whether the mortgage become 90+ days delinquent at any point following the modification.

4 classify borrowers who may potentially receive modifications into three groups. The first group are those such that they would have defaulted whether or not they would have received a modification. The second group are those who would have repaid the mortgage with or without a loan modification. The third group are those who would have defaulted without receiving a modification, but who can now repay the mortgage following the modification. Motivated by their framework, I argue that if lower multiplicity of tranches predicts more

modifications being given to the borrowers from the first group, we should expect to see a higher rate of redefault for modifications from pools with lower number of tranches.

The results of the test appear in Table 2.11. As can be seen, neither the HHI based measure nor the Tranche Count measure predict a higher rate of mortgage redefault following modification. One can interpret this result as suggesting that the additional, and more aggressive, modifications resulting from lower multiplicity go either to borrowers who truly require them so as to become current, or those borrowers that would have repaid the mortgage with or without the modification.

2.7.3 House price rebound

One concern may be that the geographic distribution of loans may be different between loan pools with more tranches and loan pools with fewer tranches.[29] This might suggest that the effect on loan modifications is driven by the housing market rebound in areas which were more likely to have loans in loan pools collateralizing fewer tranches. Why might this be a concern for our results? Servicers might be more willing to modify mortgages in areas that were likely to experience a house price rebound, so as to ensure continuation of the flow of servicing income. Alternatively, perhaps they were simply more likely to wait for self-cure in these areas. Similarly, the servicers may have been inclined to foreclose upon those properties in areas not expected to experience house price rebounds. They would be inclined to do this to firstly recover their Servicing Advances, and secondly to prevent future advances from further delinquencies that may arise.

To ensure that results are not driven by such variation in geographic concentration between loan pools with different tranche multiplicity, I follow the methodology of 64 and construct zip-code level house price rebounds. I compute for each zip code the log difference between the minimum of the house price index in 2009 and the house price index in December 2012. Then I form three groups. The first group did not see a rebound. The second and third groups are based on the median rebound of the remaining loans. The second group contains the loans that experienced a small rebound, while the third group contains loans that experienced a large rebound.

The results appear in Appendix Table B.6. The first three columns of the table suggest that the effect of multiplicity appears to be more acute in zip codes that saw no or a low rebound. However, once combined in a single regression, we can see that the interaction terms between diffusion and the indicators for Group 1 and Group 2 (low or no rebound) are either not statistically significant, or are small in economic magnitude.

2.7.4 Linear Probability Models and Bootstrapped Standard Errors

Finally, I perform a robustness check to address the use of linear probability models (LPMs). As 21 describes, LPMs provide consistent estimates of the marginal effects of a dependent

[29] Albeit in a different context, 72 show that geographic distribution of underlying mortgages can affect design of the deal structure

variable of interest on the probability of loan modification. The fact that the regression errors are by definition heteroskedastic can be addressed by the use of cluster-robust standard errors. However, this still leaves unresolved the issue that the errors in an LPM are not normally distributed. Thus, to ensure inference is robust, I estimate my main specification, but cluster bootstrap the standard errors instead of using the cluster-robust variance-covariance matrix estimator. All the results remain robust to inference using the cluster-bootstrapped procedure.

2.8 Conclusion

Agency problems in financial intermediation can have adverse effects on households. In this paper, I ask whether post-origination agency problems affect the rate of household debt renegotiation. To answer this question I study a specific agency problem that arises when borrowers become delinquent in the securitized residential mortgage market. An RMBS deal Sponsor—who wishes to maximise cash flows from a loan pool—employs an agent, the Servicer, who has superior information about the delinquent borrower. This information asymmetry interacts with the multiplicity of claims on the underlying mortgage collateral created by the pooling and tranching of cash flows.

In a conceptual framework I assume that creating multiple tranches makes it difficult for investors to ex-post coordinate and discipline the mortgage Servicer. I show that this makes it more expensive to efficiently contract with the Servicer who makes the decision to either modify the loan or foreclose upon the borrower. Non-optimal contracting will lead to a lower rate of loan modification. In addition, the framework sheds light on why a Deal Sponsor may choose to place restrictions on the actions of the Servicer.

The framework motivates two hypotheses about the extensive and intensive margins of loan modification. I test these hypotheses using within-RMBS-deal variation in the multiplicity of tranches. I find that loans in mortgage pools collateralizing more tranches (measured in two ways) were less likely to be modified. Loans in these high multiplicity pools that were modified received less aggressive loan modifications. Estimates from a hazard rate model of foreclosure suggest that the multiplicity of tranches may account for about a third of the higher foreclosure rates of securitized mortgages as compared to portfolio loans (78).

This paper demonstrates that the pooling and tranching of cash flows interacted with the agency problem between the Servicer and Sponsor and potentially impeded the completion of markets via debt renegotiation. Failure to renegotiate the loan leaves a borrower more constrained and less likely to cure from their delinquency. Moreover, it leaves investors worse off, particularly when the borrower has experienced a substantial decline in collateral value (60, 64).

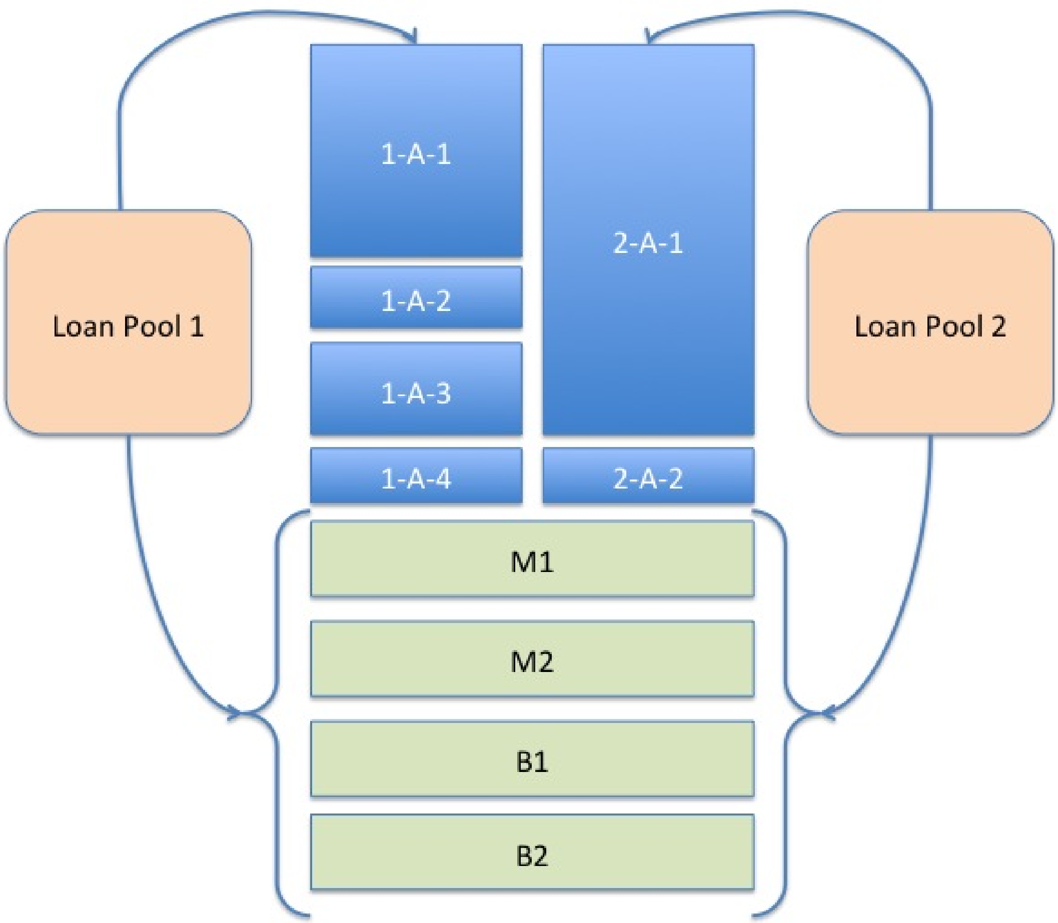


Figure 2.1: Diagram of typical RMBS transaction (Source: ABSNet)

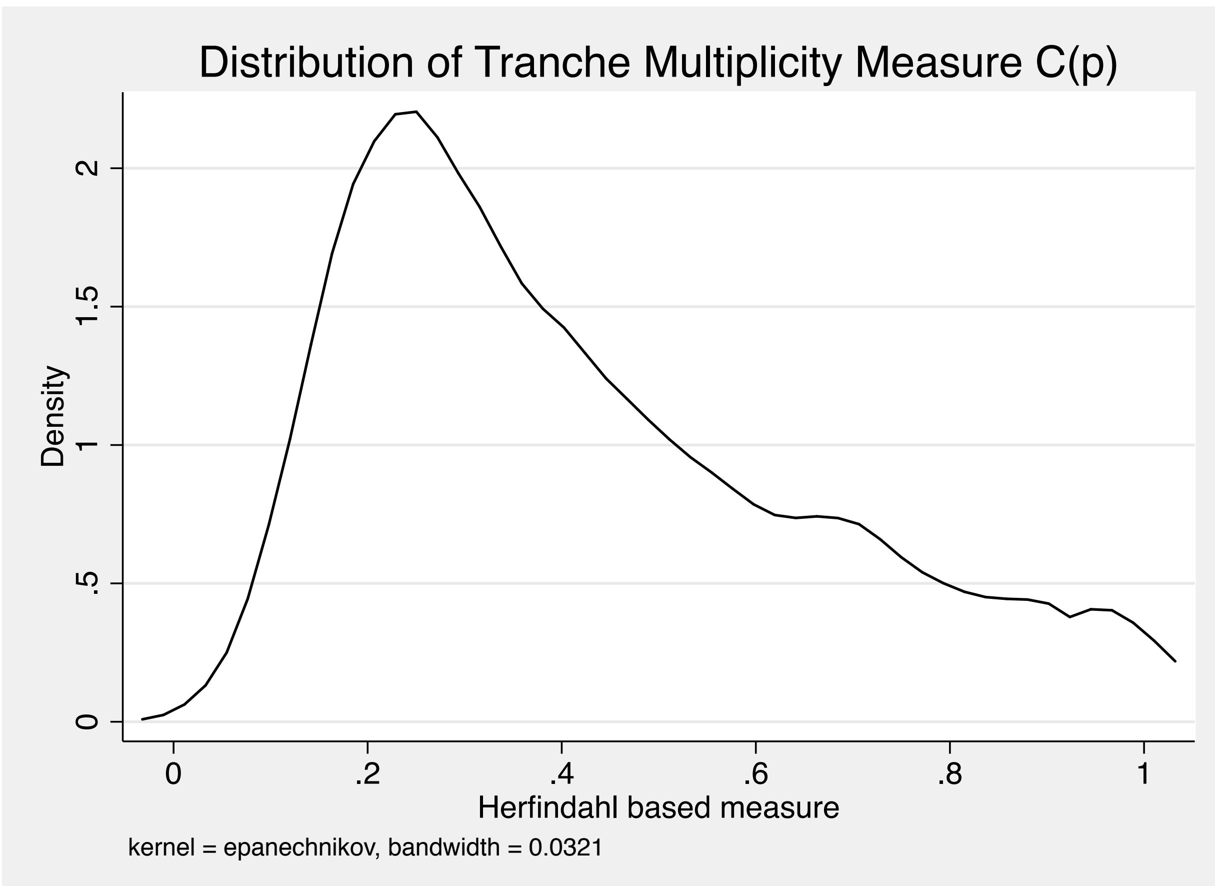
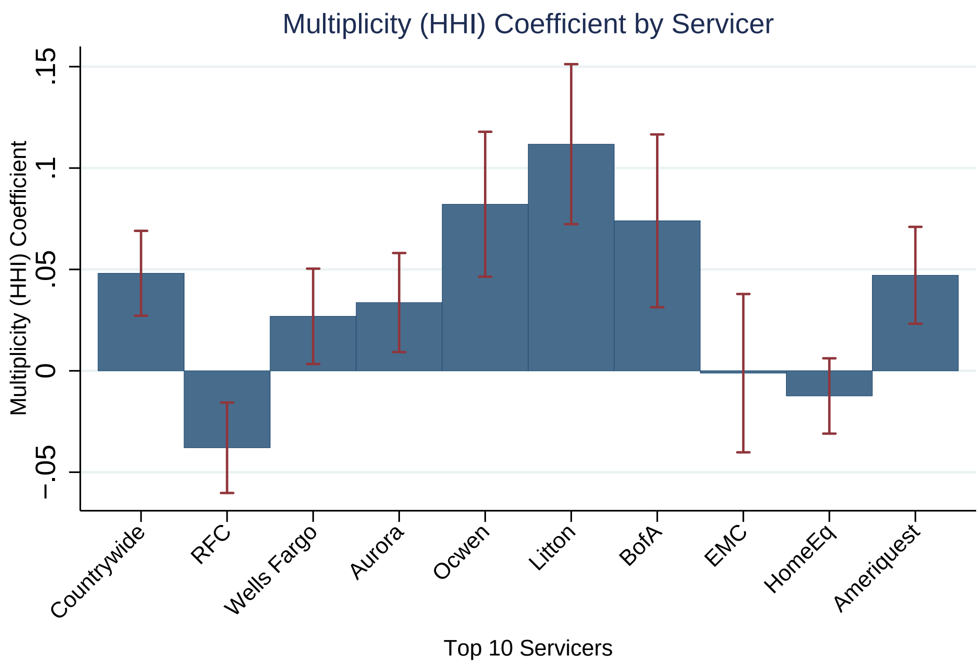


Figure 2.2: Distribution of $C(P_i)$ measure



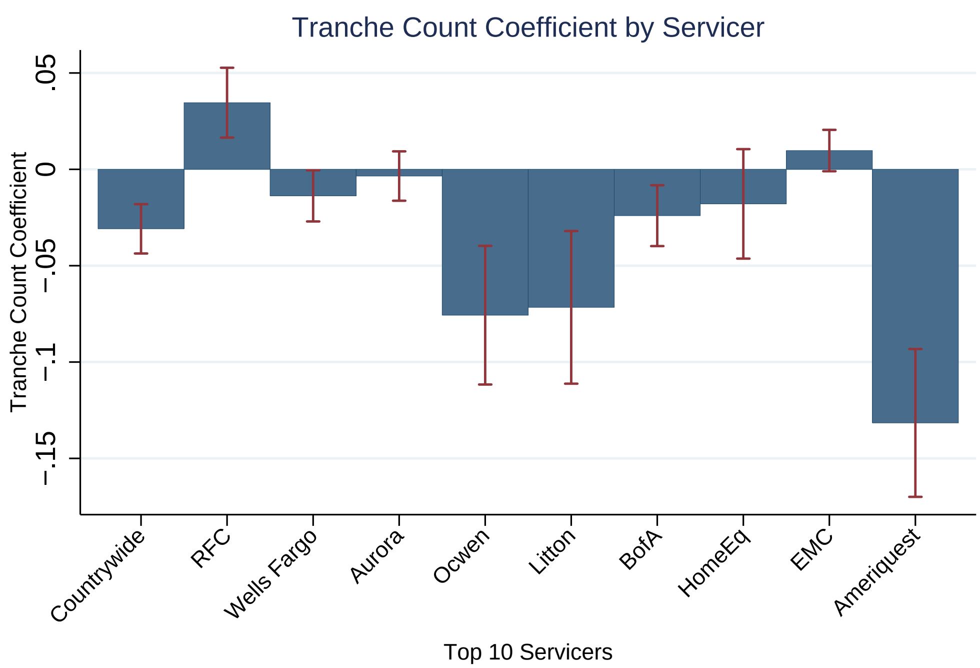


Figure 2.3: Multiplicity (HHI) and Tranche Count Coefficients by Servicer (Top 10 Servicers)

Table 2.1: Tranche Structure: MASTR Adjustable Rate Mortgages Trust 2004-11

The table below summarizes the debt structure of securitization deal MASTR Adjustable Rate Mortgages Trust 2004-11. Column 1 lists the tranches of the deal, Column 2 denotes the loan pool collateralizing each tranche, Column 3 denotes the class balance of the tranche at deal closing. The total balance of tranches that have claims to loan pool 1 is \$361 million, and the total balance to tranches with claims to loan pool 2 is \$418 million. As column 4 shows, tranche 1-A-1 has a weight of 0.4 with respect to loan pool 1 (\$145 million/\$361 million). Similarly, tranche 2-A-1 has a weight of 76% with respect to loan pool 2. Taking a sum of the squared weights that appear in Column 4 and 5, I obtain a HHI based measure of multiplicity of debt claims for pool 1 and pool 2 respectively.

(1)	(2)	(3)	(4)	(5)
Tranche	**Pools**	**Class Balance**	**Weight for Pool 1**	**Weight for Pool 2**
1-A-1	1	145,078,000	0.40	0
1-A-2	1	16,000,000	0.04	0
1-A-3	1	105,000,000	0.29	0
1-A-4	1	26,000,000	0.07	0
2-A-1	2	318,985,000	0	0.76
2-A-2	2	30,000,000	0	0.07
M-1	Both	40,357,000	0.11	0.10
M-2	Both	15,714,000	0.04	0.04
B-1	Both	7,143,000	0.02	0.02
B-2	Both	5,714,000	0.02	0.01

Table 2.2: Correlation between Tranche Multiplicity Measures and Loan Features

The table below presents results from a regression of the each of the tranche multiplicity measures on mortgage features at origination. The unit of observation for this regression is a loan pool. Each covariate is a loan pool average of the mortgages in the particular loan pool. The regressions are weighted least square regressions with weights equal to the number of loans within each loan pool. Standard errors are clustered at the deal level. *** p<0.01, ** p<0.05, * p<0.1

	(1)	(2)	(3)	(4)	(5)	(6)
	Tranche Count	Tranche Count	Tranche Count	HHI	HHI	HHI
LTV>0.8	-0.530***	-0.352***	-0.229***	0.088***	0.042*	0.119***
	(0.101)	(0.091)	(0.069)	(0.025)	(0.023)	(0.028)
FICO<660	0.511***	0.258***	-0.393***	-0.249***	-0.184***	0.167***
	(0.094)	(0.086)	(0.150)	(0.019)	(0.018)	(0.045)
Cash Out Refi	-0.580***	-0.464***	-0.612***	0.230***	0.199***	0.334***
	(0.084)	(0.077)	(0.078)	(0.023)	(0.021)	(0.034)
Not Owner Occupied	-0.317***	-0.406***	-0.571***	0.071***	0.083***	0.147***
	(0.076)	(0.071)	(0.079)	(0.021)	(0.017)	(0.027)
ARM	-0.488***	-0.366***	-0.192***	0.151***	0.125***	0.122***
	(0.044)	(0.037)	(0.028)	(0.010)	(0.010)	(0.015)
PMI	-0.066	-0.059	-0.141	0.084***	0.081***	0.062
	(0.143)	(0.129)	(0.207)	(0.024)	(0.021)	(0.134)
CLTV>LTV	-0.161*	-0.247***	-0.333***	0.019	0.041***	0.180***
	(0.089)	(0.077)	(0.059)	(0.014)	(0.013)	(0.027)
CLTV	-0.005*	0.002	0.039***	-0.002	-0.004***	-0.032***
	(0.003)	(0.003)	(0.011)	(0.001)	(0.001)	(0.007)
Prepayment Penalty	0.104*	-0.117**	-0.062	-0.076***	-0.032***	0.020
	(0.060)	(0.053)	(0.081)	(0.014)	(0.012)	(0.039)
HELOC	-1.212***	-1.115***	-0.674***	0.353***	0.324***	0.412***
	(0.114)	(0.102)	(0.136)	(0.057)	(0.061)	(0.073)
IO	0.114	-0.223***	0.102	-0.031*	0.057***	-0.098***
	(0.071)	(0.070)	(0.082)	(0.017)	(0.016)	(0.030)
Negative Amortization	0.591***	0.388***	0.402***	-0.134***	-0.079***	-0.274***
	(0.090)	(0.086)	(0.121)	(0.023)	(0.022)	(0.064)
Low or No Doc	0.222***	0.120*	-0.031	-0.020	0.004	-0.121***
	(0.077)	(0.069)	(0.092)	(0.016)	(0.015)	(0.030)
Observations	11,516	11,516	11,516	11,516	11,516	11,516
R-squared	0.076	0.173	0.889	0.142	0.234	0.732
Vintage FE	N	Y	N	N	N	Y
Deal FE	N	N	Y	N	N	Y

Table 2.3: Summary Statistics

Panel A: Loan Outcomes

Variable	Mean	Std. Dev.
Modified	0.217	0.412
Foreclosed	0.572	0.495
Prepaid	0.170	0.375

Panel B: Control Variables

Variable	Mean	Std. Dev.
FICO Score	626.03	63.05
CLTV (Pct. Points)	84.80	12.10
Appraised Val.	281609.50	202599.20
Not Own. Occ.	0.19	0.39
Age at Delinquency	19.29	11.04
Second Lien	0.22	0.41
Rate (pct. Points)	7.80	2.09
Purchase	0.43	0.50
Prepayment Penalty	0.43	0.50
HELOC	0.00	0.00
IO	0.20	0.40
Balloon	0.11	0.32
Neg Am.	0.06	0.23
Low/No Doc	0.41	0.49
ARM	0.79	0.41
PMI	0.08	0.27

Panel C: Types of Modification

Variable	N	% of Mods
Rate Change	488,987	80.77%
Payment Change	512,404	84.63%
Capitalization	486,921	80.43%
Deferral	10,395	1.72%
Principal Forgiveness	112,265	18.54%
Interest Forgiveness	2,204	0.36%

Table 2.4: Multiplicity of Tranches and the Extensive Margin of Loan Modification

The table below shows the results of a linear probability model estimation. The sample of mortgages used are those that were originated between and including 2002 and 2007, and those which went delinquent before January 2009. Loan and Borrower Level Controls are as at the origination of the mortgage. The dependent variable in each of the columns is an indicator for whether the mortgage in the loan pool was modified. House Price Change is calculated as the three month change in house prices at the county level (using Zillow data) prior to the incidence of early delinquency. Columns 1,2,4,7 and 8 control for CBSA by Quarter of Delinquency Fixed Effects and Deal Fixed Effects. Column 3, 6 and 9 additionally control for Deal by Loan Level Servicer Fixed Effects. Standard errors are clustered at the deal level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	P(Modify)	P(Modify)	P(Modify)	P(Modify)	P(Modify)	P(Modify)
Tranche Count	-0.0110***	-0.0105***	-0.0098***			
	(0.0031)	(0.0032)	(0.0033)			
Multiplicity (HHI)				0.0373***	0.0396***	0.0361***
				(0.0050)	(0.0049)	(0.0051)
House Price Change		-0.0538***	-0.0480***		-0.0537***	-0.0479***
		(0.0181)	(0.0180)		(0.0181)	(0.0181)
Observations	2,682,632	2,225,480	2,224,566	2,682,632	2,225,480	2,224,566
R-squared	0.1763	0.1773	0.2019	0.1763	0.1773	0.2020
CBSA x Quarter FE	Y	Y	Y	Y	Y	Y
Deal FE	Y	Y	N	Y	Y	N
Deal by Servicer FE	N	N	Y	N	N	Y
Additional Controls	Y	Y	Y	Y	Y	Y
Cluster	Deal	Deal	Deal	Deal	Deal	Deal
Mean of Dep Var	0.218	0.214	0.214	0.218	0.214	0.214

Table 2.5: Multiplicity of Tranches and Hazard Rate of Loan Modification

The table below shows the results of a continuous time proportional hazard model estimation based on Palmer (2014). The estimation sample is a random 15% sample of mortgages that were originated between and including 2002 and 2007, which went delinquent before January 2009. Failure is defined as a loan receiving a modification. A loan is considered to be censored either if it "self-cures" without any action by the Servicer, or if it enters into a foreclosure and is subsequently terminated. Once a loan receives a modification it leaves the sample. In addition to controlling for zip code level 3 month house price changes, I control for the standard set of loan and borrower level characteristics used in the previous regressions. Standard errors are clustered at the CBSA level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

	(1)	(2)	(3)	(4)
VARIABLES				
Cohort of Delinquency 2003	0.288	0.259	0.049	-0.312
	(0.177)	(0.176)	(0.214)	(0.235)
Cohort of Delinquency 2004	0.713***	0.677***	0.542**	-0.026
	(0.170)	(0.170)	(0.237)	(0.259)
Cohort of Delinquency 2005	1.441***	1.376***	1.016***	0.261
	(0.165)	(0.165)	(0.239)	(0.262)
Cohort of Delinquency 2006	2.168***	2.069***	1.586***	0.611**
	(0.175)	(0.175)	(0.245)	(0.271)
Cohort of Delinquency 2007	2.795***	2.706***	2.298***	1.039***
	(0.181)	(0.182)	(0.248)	(0.275)
Cohort of Delinquency 2008	3.197***	3.151***	2.785***	1.313***
	(0.183)	(0.183)	(0.248)	(0.272)
House Price Index Change	-3.893***	-8.059***	-4.123***	-4.351***
	(0.347)	(0.382)	(0.340)	(0.327)
Multiplicity (HHI)	-0.196***	-0.275***	0.293***	0.146**
	(0.041)	(0.034)	(0.079)	(0.060)
Observations	7,074,157	7,074,157	7,015,350	6,893,323
CBSA FE	N	Y	N	N
Deal FE	N	N	Y	Y
Loan Chars	N	N	N	Y
Borrower Chars	N	N	N	Y
Cluster	CBSA	CBSA	CBSA	CBSA
Log likelihood	-205565	-204028	-199466	-192995

Table 2.6: Tranche Count and Hazard Rate of Loan Modification

The table below shows the results of a continuous time proportional hazard model estimation based on Palmer (2014). The estimation sample is a random 15% sample of mortgages that were originated between and including 2002 and 2007, which went delinquent before January 2009. Failure is defined as a loan receiving a modification. A loan is considered to be censored either if it "self-cures" without any action by the Servicer, or if it enters into a foreclosure and is subsequently terminated. Once a loan receives a modification it leaves the sample. In addition to controlling for zip code level 3 month house price changes, I control for the standard set of loan and borrower level characteristics used in the previous regressions. Standard errors are clustered at the CBSA level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

VARIABLES	(1)	(2)	(3)	(4)
Cohort of Delinquency 2003	0.277	0.246	0.048	-0.312
	(0.178)	(0.177)	(0.214)	(0.235)
Cohort of Delinquency 2004	0.702***	0.663***	0.541**	-0.026
	(0.171)	(0.171)	(0.237)	(0.258)
Cohort of Delinquency 2005	1.442***	1.377***	1.014***	0.260
	(0.165)	(0.165)	(0.239)	(0.261)
Cohort of Delinquency 2006	2.152***	2.048***	1.586***	0.612**
	(0.176)	(0.176)	(0.244)	(0.271)
Cohort of Delinquency 2007	2.779***	2.684***	2.298***	1.039***
	(0.182)	(0.183)	(0.247)	(0.274)
Cohort of Delinquency 2008	3.179***	3.127***	2.785***	1.313***
	(0.184)	(0.185)	(0.248)	(0.272)
House Price Index Change	-3.887***	-8.061***	-4.127***	-4.351***
	(0.347)	(0.382)	(0.340)	(0.327)
Tranche Count	0.065***	0.093***	-0.204***	-0.106***
	(0.010)	(0.010)	(0.039)	(0.029)
Observations	7,074,157	7,074,157	7,015,350	6,893,323
CBSA FE	N	Y	N	N
Deal FE	N	N	Y	Y
Loan Chars	N	N	N	Y
Borrower Chars	N	N	N	Y
Cluster	CBSA	CBSA	CBSA	CBSA
Log likelihood	-205556	-204011	-199456	-192992

Table 2.7: Multiplicity of Tranches and the Type of Modification

Columns 1, 2, 5, and 6 are linear probability models. The dependent variable in these columns is an indicator for whether the mortgage in the loan pool was modified. In Columns 3,4,7, and 8 I regress the log of the ratio of post-modification to pre-modification payments on the the log of the Multiplicity measure. The sample of mortgages used are those that were originated between and including 2002 and 2007, and those which went delinquent before January 2009 and subsequently received a loan modification Loan and Borrower Level Controls are as at the origination of the mortgage. Standard errors are clustered at the deal level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	P(Pay Change)	P(Pay Change)	Pay Elasticity	Pay Elasticity	P(Bal Change)	P(Bal Change)	Bal Elasticity	Bal Elasticity
Multiplicity (HHI)	0.0187***				0.0082**			
	(0.0052)				(0.0033)			
Tranche Count		-0.0114***				-0.0048***		
		(0.0034)				(0.0017)		
Ln(HHI)			-0.0089***				-0.0018	
			(0.0026)				(0.0012)	
Ln(Tranche Count)				0.0037*				0.0014
				(0.0022)				(0.0010)
Observations	484,378	484,378	410,154	301,684	484,378	484,378	468,054	337,432
R-squared	0.3839	0.3839	0.2043	0.2019	0.0998	0.0999	0.1668	0.1763
CBSA x Quarter FE	Y	Y	Y	Y	Y	Y	Y	Y
Deal FE	Y	Y	Y	Y	Y	Y	Y	Y
Additional Controls	Y	Y	Y	Y	Y	Y	Y	Y
Cluster	Deal	Deal	Deal	Deal	Deal	Deal	Deal	Deal
Mean of Dep Var	0.848	0.848	-0.430	-0.442	0.967	0.967	-0.00551	-0.0127

Table 2.8: Multiplicity of Tranches and Payment Reducing Modifications

Columns 1 to 2 are linear probability models. The dependent variable in these columns is an indicator for whether the mortgage in the loan pool received a modification that reduced payments without a consequent increase in the mortgage balance. In Columns 3 and 4 I regress the log of the ratio of post-modification to pre-modification payments on the the log of the Multiplicity measures. The sample of mortgages used are those that were originated between and including 2002 and 2007, and those which went delinquent before January 2009 and subsequently received a loan modification. Loan and Borrower Level Controls are as at the origination of the mortgage. Standard errors are clustered at the deal level. *** p<0.01, ** p<0.05, * p<0.1

	(1)	(2)	(3)	(4)
VARIABLES	P(Pmt. Dec.)	P(Pmt. Dec.)	Elasticity	Elasticity
Multiplicity (HHI)	0.0041			
	(0.0071)			
Tranche Count		0.0027		
		(0.0046)		
Ln(HHI)			-0.0154***	
			(0.0053)	
Ln(Tranche Count)				0.0074*
				(0.0041)
Observations	484,378	484,378	131,469	99,489
R-squared	0.1649	0.1649	0.3027	0.3026
CBSA x Quarter FE	Y	Y	Y	Y
Deal FE	Y	Y	Y	Y
Deal by Servicer FE	N	N	N	N
Additional Controls	Y	Y	Y	Y
Cluster	Deal	Deal	Deal	Deal
Mean of Dep Var	0.275	0.275	-0.680	-0.695

Table 2.9: Multiplicity of Tranches and Balance Increasing Modifications

Columns 1 to 2 are linear probability models. The dependent variable in these columns is an indicator for whether the mortgage in the loan pool received a modification that increased the outstanding balance without reducing monthly payments. In Columns 3 and 4 I regress the log of the ratio of post-modification to pre-modification balance on the the log of the Multiplicity measures. The sample of mortgages used are those that were originated between and including 2002 and 2007, and those which went delinquent before January 2009 and subsequently received a loan modification. Loan and Borrower Level Controls are as at the origination of the mortgage. Standard errors are clustered at the deal level. *** p<0.01, ** p<0.05, * p<0.1

	(1)	(2)	(3)	(4)
VARIABLES	P(Bal Inc.)	P(Bal Inc.)	Elasticity	Elasticity
Multiplicity (HHI)	-0.0285***			
	(0.0063)			
Tranche Count		0.0125***		
		(0.0048)		
Ln(HHI)			-0.0004	
			(0.0009)	
Ln(Tranche Count)				0.0003
				(0.0007)
Observations	484,378	484,378	95,793	63,351
R-squared	0.2181	0.2181	0.2052	0.2056
CBSA x Quarter FE	Y	Y	Y	Y
Deal FE	Y	Y	Y	Y
Additional Controls	Y	Y	Y	Y
Cluster	Deal	Deal	Deal	Deal
Mean of Dep Var	0.201	0.201	0.0832	0.0832

Table 2.10: Multiplicity of Tranches and Block Ownership (GSE Pool)

The table below shows the results of a linear probability model estimation. The sample of mortgages used are those that were originated between and including 2002 and 2007, those which went delinquent before January 2009 and were subsequently modified. Loan and Borrower Level Controls are as at the origination of the mortgage. The dependent variable in each of the columns is an indicator for whether the mortgage in the loan pool went back to being 90+ days delinquent following the modification. Standard errors are clustered at the deal level. *** p<0.01, ** p<0.05, * p<0.1

	(1)	(2)	(3)	(4)
VARIABLES	P(Modify)	P(Modify)	P(Default)	P(Default)
GSE Pool	0.0307***	0.0203***	-0.0279***	-0.0219***
	(0.0036)	(0.0017)	(0.0035)	(0.0018)
Multiplicity (HHI)	0.0279***		-0.0493***	
	(0.0055)		(0.0063)	
Multiplicity (HHI) x GSE Pool	-0.0330***		0.0239***	
	(0.0069)		(0.0082)	
Tanche Count		-0.0033		0.0139***
		(0.0030)		(0.0027)
Tranche Count x GSE Pool		0.0104***		-0.0060**
		(0.0021)		(0.0025)
Observations	2,682,632	2,682,632	2,682,632	2,682,632
R-squared	0.1765	0.1765	0.2077	0.2076
CBSA x Quarter FE	Y	Y	Y	Y
Deal FE	Y	Y	Y	Y
Additional Controls	Y	Y	Y	Y
Cluster	Deal	Deal	Deal	Deal
Mean of Dep Var	0.218	0.218	0.572	0.572

Table 2.11: Multiplicity of Tranches and Redefault

The table below shows the results of a linear probability model estimation. The sample of mortgages used are those that were originated between and including 2002 and 2007, those which went delinquent before January 2009 and were subsequently modified. Loan and Borrower Level Controls are as at the origination of the mortgage. The dependent variable in each of the columns is an indicator for whether the mortgage in the loan pool went back to being 90+ days delinquent following the modification. Standard errors are clustered at the deal level. *** p<0.01, ** p<0.05, * p<0.1

	(1)	(2)
VARIABLES	P(90+)	P(90+)
Tranche Count	0.0015	
	(0.0036)	
Multiplicity (HHI)		-0.0097
		(0.0067)
House Price Change	-2.7552***	-2.7553***
	(0.0272)	(0.0272)
Observations	391,712	391,712
R-squared	0.1680	0.1680
CBSA x Quarter FE	Y	Y
Deal FE	Y	Y
Additional Controls	Y	Y
Cluster	Deal	Deal
Mean of Dep Var	0.728	0.728

Chapter 3

Partial Deregulation and Competition: Evidence from Risky Mortgage Originations

3.1 Introduction

The financial deregulation of the last two decades has been the subject of heated political and academic debate, insofar as it may have played an important role in creating a permissive lending environment. In fact, critics maintain that regulators incentivized laxer underwriting standards in order to encourage the origination of increasingly marginal loans, whereas effective regulation of lending practices could have prevented aggressive lenders from abusing vulnerable borrowers by offering riskier and more complex mortgages.[1] Moreover, it is not clear that this market could regulate itself. On the one hand, market forces and lenders' reputation concerns may discipline banks' behavior, but on the other, fiercer banking competition could induce lenders to originate even riskier loans to preserve their market shares in the short term.

Hence, in this paper we address the following questions empirically: how does financial deregulation affect the credit supply and the use of complex loans features? How do regulated intermediaries react to the deregulation of other lenders? One of the major difficulties in empirically identifying the effects of deregulation on the types of mortgages banks originate is that policy interventions usually affect all lenders at once, making it impossible to distinguish between the direct effects of the policy and other confounding factors affecting mortgage originations. This paper overcomes these problems by exploiting the 2004 preemption of state laws against predatory lending for lenders regulated by the Office of Comptroller

[1] President Barack Obama justified the need for a Consumer Financial Protection Agency with the argument that predatory lending by unregulated mortgage brokers was one of the causes of the financial crisis: "Part of what led to this crisis were not just decisions made on Wall Street, but also unsustainable mortgage loans made across the country. While many folks took on more than they knew they could afford, too often folks signed contracts they didn't fully understand offered by lenders who didn't always tell the truth" (White House news release, September 19, 2009, available at www.whitehouse.gov/the_press_office/Weekly-Address-President-Obama-Promotes-Tougher-Rules-on-Wall-Street-to-Protect-Consumers). .

and Currency as an exogenous shock to the competitive landscape. Specifically, this shock expanded the set of loans that OCC-regulated lenders were allowed to originate but did not alter the set permitted to other lenders. The pre-emption ruling creates an ideal environment to test for the effects of deregulation by providing us with a clean set of affected banks and states, i.e. those regulated by the OCC in states with anti-predatory laws, and a set of unaffected banks, i.e. those regulated by the state regulators, as well as by the Department of Housing and Urban Development (HUD), and those in states without laws against predatory lending. We can exploit this setting to analyze how lenders respond to partial deregulation and detect the spillover effects on still-regulated lenders due to local mortgage market competition.

There is a growing household finance literature on the demand-side determinants of the different loan contracts observed in the data. This literature takes important steps towards understanding what types of borrowers take on different forms of debt, such as adjustable rate mortgages (ARM), fixed rate mortgages (FRM) and interest-only mortgages (IO).[2] Much less is known about the supply side, however. The 2004 deregulation, by affecting different types of originator differentially, offers a unique chance to determine whether the supply of these mortgages changed significantly in the run-up to the crisis.

Our first result explores whether the preemption of these APL laws for OCC-regulated lenders led them to increase loans with more complex terms. We adopt a differences-in-difference strategy by comparing loans made in states with and without laws against predatory lending (henceforth "APL laws") to show that the preemption of these laws for OCC-regulated lenders led them to significantly increase the origination of mortgages featuring prepayment penalties. The idea is to use only within OCC lenders variation and compare their behavior in states with APL laws ("APL states"), where the preemption ruling should have had an effect, to their behavior in states without these laws ("non-APL states"). This identification strategy does not suffer from the potential problems associated with comparing lenders regulated by different agencies (e.g. differential pre-trends); moreover, the control group (i.e. OCC lenders in non-APL states) is not affected by the treatment.

The most conservative estimate shows that, following the preemption ruling, OCC-regulated lenders were about 6% more likely than other lenders to make mortgage loans with prepayment penalties. Compared with the unconditional probability of about 30% in our sample, this represents an economically significant increase. These prepayment penalties are particularly important, as they represent an optimal way for banks to make it more costly for borrowers to refinance when their creditworthiness improves. These results are very consistent with the fact that these laws were indeed binding for national banks, since the limits on prepayment penalties, both on their magnitude and on the prepayment term, were the most common feature of these anti-predatory laws. Overall, these findings support the thesis that the deregulation significantly increased the supply of complex mortgages.

Having established that the deregulation had a *direct* effect on the supply of riskier mortgages from national banks, we can now ask whether it also had an *indirect* effect on the non-OCC lenders. Intuitively, the deregulation altered the competitive landscape by giving

[2]See 26 for a survey of this literature. A more detailed discussion of the literature is provided in the next section.

national banks an advantage. Their lending was now unconstrained by APL laws, while the lending of their competitors who were not regulated by the OCC remained constrained. Hence, since prepayment penalties are used to make a loan more affordable, for instance by lowering the monthly payments, we should expect non-OCC lenders to try to defend their market shares by offering loans with features catering to the same pool of borrowers without violating the law. Alternative mortgage terms that increase affordability, and increase barriers to refinancing, include the use of deferred amortization and interest-only mortgages.[3]

We test for the presence of these spillover effects of the preemption rule by comparing the behavior of non-OCC lenders in APL states and in non-APL states before and after the preemption. We should expect the non-OCC lenders to react in the states with anti-predatory laws, while we should observe no effects in the states without APL laws. The reason is that while in the non-APL states both OCC and non-OCC lenders were not facing any regulatory constraint, in APL states the non-OCC lenders had to abide by these laws, while after the preemption rule (which only affected the OCC lenders) OCC lenders did not. This led us to hypothesize that non-OCC lenders in APL-states should then react to the preemption rule by originating mortgages with characteristics that were not explicitly prohibited by the APL.

We find that non-OCC lenders responded to the preemption by increasing the origination of ARMs and loans with negative amortization (i.e. dimensions not restricted under the APL). We show that these effects remain statistically and economically significant even when we control for key regional characteristics related to the credit supply, such as the fraction of subprime borrowers, the borrowers' median income and the changes in house prices. All specifications also include the loan-to-value ratio at origination, the log of the appraised value, the borrower's FICO score, an indicator for the presence of second liens, a low or no documentation indicator, an indicator for loan purpose (i.e. cash out refinance, rate refinance or other), and an indicator for the presence of private mortgage insurance (i.e. which denotes loan-to-value ratios higher than 80%). In the most conservative specification, we control for bank-quarter fixed effects which means that we compare -for instance- New Century's origination strategy in a state with an anti-predatory law (e.g. California) to New Century's origination strategy in another state without an APL (e.g. Arizona) within the same quarter. It is then very unlikely that our results are confounded by movement in the control group or arguments about treatment and control being on differential trends.

If the mechanism is working through a competition channel, this effect should be stronger in markets where OCC lenders have a more dominant presence. We test this hypothesis using the fraction of loan volume originated by OCC lenders in the pre-period as proxy for their market dominance.[4] We investigate the non-OCC lenders' response by separately considering the response in counties with different levels of competition from OCC lenders. Consistent with the hypothesis, we find that after the preemption ruling non-OCC lenders were 3 percentage points more likely to grant adjustable rate mortgages and 3 to 4.5 percentage points more likely to grant deferred amortization and interest-only mortgages, especially

[3] See 24 for a similar argument.

[4] In robustness checks available from the authors we also show that similar results hold when we proxy for competition by computing the Herfindahl-Hirschman index using data on deposits from the FDIC.

in counties where OCC lenders had larger market shares and absent in the counties where they had little market power. Even in this case, we show that the results are not due to differential trends in house prices across these regions. Our results indicate that, rather than attenuating the effects of deregulation, competition may have led even the banks that were not directly affected to turn to riskier and more complex mortgages as a best-response to the increased risk of losing market share.

We complement the previous results by investigating the defaults and the interest rates of these complex loans. We find that loans with complex features originated by both OCC and non-OCC lenders are significantly more likely to default in states with anti-predatory laws after the preemption. This confirms the hypothesis that loans with complex features such as prepayment penalties or deferred amortization are indeed riskier. We also find that the OCC-originated loans exhibit a reduction in interest rates of 7.5 basis points as a result of the pre-emption ruling, whereas non-OCC lenders loans with deferred amortization and interest-only features are offered to borrowers at significantly higher interest rates, about 20 basis points.[5] The results for the national banks echo the framework of 65, which shows that a borrower of a given quality who previously qualified for a loan without prepayment penalties might be better off by choosing one with a prepayment penalty because of the lower interest rates. However, the results for the non-OCC lenders suggest that borrowers who get lured into a mortgage with lower monthly payments, such as those with deferred amortization or interest-only features offered by non-OCC lenders, end up paying significantly more in interest expenses.

Finally, we run additional robustness checks. First, to show that the results are not contaminated by regional differences, we restrict the sample to counties on the state borders with different APL laws controlling for county pair fixed effects and confirm the main results. This test makes sure that the counties in the treatment (i.e. the ones in APL states) and in the control (i.e. the ones in non-APL states) are similar to each other, as bordering counties should be affected by similar shocks. Second, we adopt a propensity-score matching approach to address the concern that regions with higher national banks' market share in APL states may also differ in other important ways from the ones in non-APL states, which independently influence the origination of complex mortgages. We match the high OCC market share counties in APL and non-APL states on a range of demographic, economic, and mortgage characteristics. This matching exercise allows us to compare similar counties across a range of pre-deregulation characteristics and trends. Third, we confirm our results by adopting a triple differences-in-difference strategy where we compare the response of OCC lenders to the preemption rule across states compared to the response of non-OCC lenders. To be valid, this approach requires that the *difference* between OCC and non-OCC lenders would be on parallel trends in the absence of the preemption rule. Consistent with the results presented above, the OCC expanded their issuance of mortgages with prepayment penalties and longer prepayment terms, but reduced their issuance of ARMs and deferred

[5]The mortgages without prepayment penalties also exhibit lower interest rates after the preemption in APL states, because while lower quality borrowers are attracted by the affordability of the loans with prepayment penalties, the plain contracts tend to attract the higher-quality borrowers and so demand even lower rates.

amortization mortgages compared to the non-OCC lenders. This is because the control group in this specification (i.e. non-OCC lenders) is issuing more mortgages with those characteristics.

Taken together, our findings indicate two main channels through which the effects of deregulation in mortgage markets may manifest themselves. First, it directly increases OCC-regulated lenders' origination of loans with features otherwise prohibited under state APL laws, particularly prepayment penalties, an effect that can explain about 10 percent of the increase in the use of these features. Second, it also induces a response from the lenders still subject to the regulation in the same markets by increasing their origination of loans with other complex features that were not prohibited under the state laws. The picture that emerges is of a competition channel that began with the OCC-regulated lenders, worked its way through the local mortgage market, and obliged the non-OCC regulated lenders to alter their own mortgage terms as a competitive response. These results complement and shed new light on other mechanisms that have been proposed to explain the rise of riskier mortgages, such as the boom in securitization (56 and 73).

Related Literature. Our key contribution consists in directly estimating the effect of deregulation on the supply of complex mortgages both through a direct channel, namely the behavior of the deregulated national banks, and more importantly through an indirect one, the response of their state-regulated competitors.

After the crisis, a novel literature emerged relating changes in mortgage market terms to the real economy. For instance, in their seminal paper, 70 show that zip codes with a higher fraction of subprime borrowers experienced unprecedented relative growth in mortgage credit and a corresponding increase in delinquencies. More recently, 6 argue that the middle-class and high-FICO borrowers also played a significant role in the mortgage crisis. We find evidence consistent with both narratives, in fact; although banks' credit supply significantly shifted during the years preceding the crisis, it is also true that prime borrowers were steered to riskier contracts, which defaulted with higher probability.

79 and 45 have shown that about one out of every ten loans exhibits some form of asset quality misrepresentation, such as misreported occupancy status of the borrower and unreported second liens. They also provide evidence that a good part of this misrepresentation is the work of the financial institutions themselves and not of the borrowers. Our results contribute to this debate by exploiting an exogenous shock to credit supply and the competitive environment to show that deregulation played a significant role in the mortgage crisis by heightening the incentive for lenders to issue riskier mortgages, especially in highly competitive markets.[6]

Our paper relates directly to 19 and 41. 19 analyze the *demand* for complex mortgages, i.e. the type of borrowers who are more prone to take on complex mortgages, during the years preceding the crisis. 41, instead, find that banks can influence customers' mortgage

[6]Our paper also relates to a number of studies that investigate the changes in lending behavior during pre-crisis years. Other papers, such as 55, 9, 47, 28 and 22, have held that the relaxation of lending standards was one of the main causes of the subprime crisis; others, such as 83, 81, 73 and 56, have highlighted the failure of ratings models and the rapid expansion of non-agency securitization markets as driving factors. We complement these studies with evidence that deregulation might have triggered a race to the bottom among lenders in the years preceding the crisis.

choices, between fixed-rate and adjustable-rate loans, through an advice channel in addition to pricing. A few other papers have analyzed riskier mortgages during the boom period. 9 test whether predatory lending was a key element in fueling the subprime crisis, investigating the effect of an anti-predatory pilot program in Chicago. Similarly, 7 explore the effects of mandatory third-party review of mortgage contracts on consumer choice, including the terms and demand for mortgage credit. 46, instead, show evidence that lenders advertise to steer unsophisticated consumers into bad choices by highlighting the initial interest rate and shrouding the reset rate.[7] We complement these findings by showing how the supply side is shaped by changes in the regulatory environment. We also show that when competition is more intense, the lenders not directly affected by the preemption rule tend to adjust not only the interest rate but also a variety of other mortgage features.[8]

We adopt the identification strategy followed by 31, based on the OCC's introduction of the preemption rule in 2004 and the variation between states with and without anti-predatory laws. However, the present paper differs in both focus and results. The main results of 31 relate to the real effects of an outward shift of credit supply, and in particular the possibility of inducing a boom-and-bust economic cycle at county level. The present paper, instead, exploits individual-level data to investigate the response of the non-OCC regulated banks, such as state banks and credit unions, to show how competition shapes their response to the deregulation. The results have important policy implications. In fact, they suggest that deregulating only a subset of market participants might have unintended consequences as the still-regulated market participants might react by offering contract terms that could potentially be even more detrimental for borrowers' welfare. More generally, banking regulations have to take into account indirect effects working through changes in the competitive landscape.

Other related papers on the effect of mortgage deregulation include 54 and 40. 54 show that per capita growth rates in income and output rose significantly following the relaxation of bank branching restrictions in the United States. Like 40, we use deregulation as a quasi-experiment; 40 exploit the passage of the Interstate Banking and Branching Efficiency Act (IBBEA) in 1994 and show that this deregulation triggered an increase in the demand for housing, that is, that house prices rose because of the expanded supply of credit in the deregulating states. The main difference from the current paper is that we document an increase in credit supply due to the preemption rule of 2004, which - unlike the 1994 IBBEA– targeted subprime lending and riskier borrowers. In other words, the deregulation

[7] Also related is 13, which examines the role of loan officers' incentives, describing a controlled corporate experiment in which loan officers' compensation structure was altered from fixed salary to volume-based pay, and shows that the incentives made mortgage origination more aggressive.

[8] Two recent papers have investigated different policy interventions in the mortgage market. First, we share the focus on the effect of policy changes on the competitive landscape with 20, who explore whether market competitiveness affects mortgage interest rates by exploiting the introduction of the Home Affordable Refinancing Program (HARP), which gave lenders that were servicing eligible loans substantial advantages over their competitors. Second, 14 analyze the effect of the Community Reinvestment Act (CRA) on banks' lending activity. They find that adherence to the act led to an increase in lending by banks; in fact, during the six quarters surrounding the CRA exams lending is 5 percent higher, but these loans default more often. We share the focus on the effect of deregulation on pre-crisis loan origination, but we exploit loan-level data to study how lenders modified key features of their mortgages to remain competitive.

investigated here expanded the range of mortgage contracts that national banks could offer to subprime borrowers; which is a far different form of deregulation with radically different implications.[9]

The rest of the paper is organized as follows. Section 3.2 gives background on the US credit market and regulation. Section 3.3 provides details on the data sources, while Section 3.4 illustrate our research design. Section 3.5 provides the first results on the effect of the deregulation on the mortgage terms and on the composition of borrowers. Section 3.6 investigates a competition mechanism by which non-OCC lenders also changed their mortgage origination behavior. Section 3.7 shows additional evidence on interest rates and defaults. Finally, Section 3.8 presents several robustness checks, while Section 3.9 concludes.

3.2 The Regulatory Framework

3.2.1 Mortgage Regulators

In the United States, residential mortgage lenders are regulated by both national and local agencies. National banks, federal thrift institutions and their subsidiaries are supervised by the OCC or the Office of Thrift Supervision (OTS). State banks and state-chartered thrifts are supervised by the Federal Reserve System, the Federal Deposit Insurance Corporation (FDIC) or their own state banking authority. Credit unions are supervised by the National Credit Union Administration (NCUA), while non-depository mortgage companies are regulated by the Department of Housing and Urban Development (HUD) and the Federal Trade Commission.

Since our identification strategy depends on this classification, it is important to make sure that lenders cannot somehow circumvent their assigned regulator. One particular source of concern is that lending institutions might be able to shop around for the most lenient regulator. 16 show that federal regulators are significantly less lenient, downgrading supervisory ratings about twice as frequently as state supervisors, and that banks under federal regulators report higher nonperforming loan ratios, more delinquent loans, higher regulatory capital ratios, and lower ROA. If they are allowed to, then, banks have an incentive to switch from federal to state supervision, which means that even if this were possible, it would bias the results against our hypothesis. Moreover, 87 and 85 explore switching between regulatory agencies between 1970 and 2012 and find that at first most of the switches were due to new banking policies, such as the relaxation of the ban on interstate banking, but that after the initial period the main reason was merger with a bank chartered at a different level. Further, the banks that switched tended to be small banks (assets of less than $1 billion), which are not in our sample as we exclude banks with fewer than 1,000 loans. The only exceptions are JP Morgan and HSBC, which switched from the state to the national regulator in 2004, and to avoid biasing our estimates, we classify these two institutions as national lenders prior to 2004 as well.

[9] Other recent papers on credit supply include 44, which investigates the importance of the credit channel for employment by assessing the role of bank lending to small businesses, and 5, which exploits changes in the conforming loan limit to gauge the effect of cheaper financing on house prices.

These findings corroborate our identification strategy. And the granularity of our data allows us to track the banks that changed regulatory agencies, so that we can address any other concerns relating to this issue.

3.2.2 Predatory-lending laws

This dual banking system generated conflicting regulations when several states passed anti-predatory-lending laws and the OCC issued a preemption rule for national banks. The first attempt to limit predatory lending practices was the 1994 Home Ownership and Equity Protection Act (HOEPA), which imposed substantial restrictions on terms and practices for high-priced mortgages, based either on APR or on total percentage points of interest and fees. The aim was to redress abusive high charges for refinancing and home equity loans. However, the thresholds for classifying mortgages as predatory or "high cost" were very high, which significantly narrowed the scope for applying the restrictions. These "high cost" mortgages, in fact, accounted for just 1 percent of subprime residential mortgages; they represented the most abusive sector of the subprime mortgage market (25).

Many states later adopted stricter predatory lending regulations than federal law requires. Such legislation is intended to prevent various unfair and deceptive practices, such as steering borrowers to loans with a higher interest rate than they could qualify for, making loans regardless of repayment capacity, charging exorbitant fees, or adding abusive early repayment penalties – all of which can significantly aggravate the risk of foreclosure.[10] The first comprehensive state law against predatory lending, or APL, was passed by North Carolina in 1999, targeted at the subprime mortgage market. As of January 2007, 20 states and the District of Columbia had APL laws in effect.

Potentially, predatory lending laws may have various effects on mortgage market outcomes. They might ration credit and raise the price of subprime loans, or else allay consumer fears about dishonest lenders and ensure that creditors internalize the cost of any negative externalities, which could increase the demand for credit. There is strong recent evidence that predatory lending laws played an important role in the subprime market. 36, for instance, find that they are associated with a 43% reduction in early repayment penalties and a significant decrease in adjustable-rate mortgages; they are also correlated with a significant reduction in riskier borrowers' probability of default. In subprime regions (those with a higher fraction of borrowers with FICO scores below 680) these effects are even stronger.

Using 2004 HMDA data, 49 find that subprime loans originating in the states with predatory lending laws had lower APRs than in unregulated states. 77 provide additional evidence, focusing on border counties of adjacent states with and without APL to control for labor and housing market characteristics. Using a legal index, they examine the effect of APLs on the probability of subprime applications, originations, and rejections. They find that more restrictive regulations reduced the likelihood of origination and application. Similarly, 39, using a proprietary database of subprime loans by eight large lenders from 1999

[10] 15 give evidence of unscrupulous behavior by lenders – such as predatory lending – during the housing boom of the 2000s. They show that lenders steered higher-quality borrowers to affiliates offering subprime-like loans, with APR between 40 and 60 basis points higher.

to 2004, find that the presence of a law was associated with fewer subprime originations.

We follow this literature employing the measure constructed by 36, which considers only the states with predatory lending laws that were not just small-scale home ownership and equity protection acts passed to prevent local regulation.

3.2.3 Preemption Rule

On January 7, 2004 the OCC issued sweeping regulations preempting, for national banks, a broad range of state laws designed to regulate the "terms of credit": laws regulating loan terms and lending and deposit relationships or requiring a state license to lend. The final rule also mandated preemption where the state law would "obstruct, impair, or condition a national bank's exercise of its lending, deposit-taking, or other powers granted to it under federal law", either directly or through subsidiaries. The new regulations effectively barred the application of all state laws to national banks, except where Congress has expressly incorporated state-law standards in federal statutes or where the effect of the state laws on national banks is only "incidental." The OCC has clarified that state laws will be deemed to have a permissible "incidental" effect only if they are part of "the legal infrastructure that makes it practicable" for national banks to conduct their federally-authorized activities and "do not regulate the manner or content of the business of banking authorized for national banks," such as contracts, torts, criminal law, the right to collect debts, property acquisition and transfer, taxation, and zoning.

Specifically, the OCC preempted all regulations pertaining to terms of credit, including repayment schedules, interest rates, amortization, payments due, minimum payments, loan-to-value ratios, the aggregate amount that may be lent with real collateral and the term to maturity, including the circumstances under which a loan may be called due and payable after a certain time or upon a specified external event.

This means that starting in 2004 the subprime mortgage market in states with predatory lending laws was no longer a level playing field: national banks were significantly less constrained by APLs in providing credit to riskier borrowers. For instance, New Century, one of the biggest non-OCC mortgage originators, in its 2004 10-K filing complains as follows: "Several states and cities are considering or have passed laws, regulations or ordinances aimed at curbing predatory lending practices. In general, these proposals involve lowering the existing federal HEPA thresholds for defining a "high-cost"loan, and establishing enhanced protections and remedies for borrowers who receive such loans. [...] This would effectively preclude us from continuing to originate loans that fit within the newly defined thresholds. [...] Moreover, some of our competitors who are, or are owned by, national banks or federally chartered thrifts may not be subject to these laws and may, therefore, be able to capture market share from us and other lenders. For example, the Office of the Comptroller of the Currency issued regulations effective January 7, 2004 that preempt state and local laws that seek to regulate mortgage lending practices by national banks."[11]

[11] Available at http://www.sec.gov/Archives/edgar/data/1287286/000119312505052506/d10k.htm pag. 45.

3.3 The Data

We collected data from a variety of sources. The main one is the ABSNet Loan Database, which covers almost 90% of private-label Residential Mortgage Backed Securities and provides data on the underlying loans and key borrowers' characteristics. The main advantage of this dataset over the other standard datasets used in the literature, such as LPS and Blackbox, is the possibility of identifying the mortgage originator, which is crucial to our identification strategy. This enables us to classify lenders into those who were and were not regulated by federal agencies (respectively "OCC" and "non-OCC" lenders).[12] We also identify the lenders regulated by the OTS. As of 2004, OTS-regulated lenders had been preempted from following state-lending laws due to a regulation passed in 1996.[13] Therefore, to ensure that our control group consists only of lenders who still had to adhere to state laws, we exclude loans originated by OTS-regulated lenders. We consider all first-lien mortgages originated in the pre-period, January 2001 to January 2004, and in the post-period, February 2004 to December 2006, with a final sample of almost 8 million loans. Another advantage of this fine-grained data is the ability to observe all the specific features of the loans at the date of origination. We exploit this, for instance, by analyzing how financial institutions changed their provision for prepayment penalties, the length of prepayment penalty terms, balloon payments, negative amortization, and interest rates in response to the preemption rule.

Table 3.1 and Table 3.2 give summary statistics for our sample of loans. Of all the loans in the sample, 4.3 million were originated in states that had APL laws. Panel A focuses on the covariates used in our specification, Panel B on the mortgage features at origination. Table 3.1 shows the statistics for the period before the preemption rule (2001-January 2004), Table 3.2 those for the post-period February 2004-December 2006. As our sample consists of loans in private label securitizations, which were the way in which a large quantity of subprime and non-conforming loans were securitized, we have an average FICO score of 687 for OCC lenders in the pre-period and slightly lower for other financial institutions. The score declined slightly in the post period, probably reflecting the general deterioration of lending standards. In the pre-period the average LTV was 72% for OCC and 76% for non-OCC lenders. Subsequently, it remained stable for non-OCC lenders but rose to 75.8% for OCC lenders. In addition, some 7% of OCC loans have a second lien in the pre-period, with this number increasing to 14% in the post-period. Finally, 40% of the loans have little or no documentation and 15% have private mortgage insurance. Unconditionally, 29% of the loans in our sample have prepayment penalties, a key focus of the analysis; 64% are ARMs and 17% are interest-only.

3.4 Research Design

We start our analysis by discussing our strategy for identifying the direct effect of the deregulation on OCC lenders. We then move to the central part of the paper: our empirical

[12]This classification has been graciously provided to us by Nancy Wallace and the Fisher Center for Real Estate and Urban Economics at the Haas School of Business.

[13]See Code of Federal Regulations Title 12 Section 560.2.

strategy to identify the presence of spillover effects to other lenders.

By lifting the existing laws against predatory lending, the preemption rule removed a constraint for lenders that want to charge prepayment penalties. These are of particular importance as highlighted by 65 which sets out a dynamic lending model with costly default in which riskier loans are more likely to exhibit prepayment penalties in equilibrium, because such penalties represent an optimal way for banks to make it more costly for the -ex post- higher-quality borrowers to refinance when their creditworthiness improves. This suggests the following empirical hypothesis:

Hypothesis I Controlling for borrower characteristics, loans by OCC lenders in APL states are more likely to have prepayment penalties after the preemption rule than those by OCC lenders in non-APL states.

We test this hypothesis by estimating the following differences-in-difference specification on the sample of loans originated by OCC lenders:

$$\begin{aligned} Loan\ Features_{i,c,t} &= \alpha + \beta_1 \cdot Post_{2004} \cdot APL_{g,t} + \beta_2 \cdot APL_{g,t} + \\ &\quad \Gamma_1 \cdot X_c \cdot Post_{2004} + \Gamma_2 \cdot \Sigma_i + \eta_t + \theta_c + \epsilon_{i,c,t} \end{aligned} \tag{3.1}$$

where $Loan\ Features_{i,c,t}$ are mortgages characteristics for loan i in a county c in month t, $APL_{g,t}$ indicates whether or not state g has an APL law in place at time t, and $Post_{2004}$ is a dummy equal to 1 after the preemption rule. The coefficient of interest is β_1. The idea behind this specification is to use only *within OCC* lenders variation and compare their behavior in states with APL laws, where the preemption rule should have had an effect, to their behavior in non-APL states where there no laws to restrict lending practices. This is a powerful identification strategy because it does not suffer from the potential problems associated with comparing lenders regulated by different agencies, and because the control group (i.e. OCC lenders in non-APL states) is not affected by the treatment, i.e. the preemption rule.

We include controls X_c and their interactions with $Post$ to capture heterogeneity across regions that might be correlated with the demand for complex mortgages: the fraction of subprime borrowers (i.e. those with FICO below 660) and the log of the median income in the county. Intuitively, we would expect that in regions with a more significant presence of riskier borrowers, banks would be more likely to originate complex mortgages that might better cater this segment of the demand. Similarly, borrowers in lower income regions might be more likely to need features such as lower monthly payments for the first few years, which would increase the demand for interest-only or adjustable-rate mortgages. Finally, we also include the house price change in the year before the loan was originated, because previous changes in housing prices increase demand for credit by boosting the value of collateral and by potentially affecting borrowers' expectations about future house appreciation.

We also exploit the granularity of our data to include a vector Σ_i of borrower-level characteristics: an indicator for inadequate or absent documentation, an indicator for the presence of private mortgage insurance, the loan-to-value ratio, FICO score, a second-lien indicator, and a loan purpose indicator (i.e. cash out refinance, rate refinance or other).

While the direct effect of the preemption rule on the OCC lenders is expected, the partial deregulation introduced by the OCC might have also an *indirect* and less expected effect on the non-OCC lenders by creating an un-level playing field whereby lenders not regulated by the OCC still had to comply with state APL laws. On the one hand, non-OCC lenders may have responded to the change in the competitive landscape by specializing in less risky borrowers and loans. In other words, the preemption rule may have heightened market segmentation, especially in regions where OCC lenders have a dominant position, deterring non-OCC lenders from competing for the same borrowers. On the other hand, non-OCC lenders could have increased their origination of complex loans, such as ARMs and deferred amortization mortgages, that were not directly governed by the APL laws. Accordingly, we test the following hypothesis:

Hypothesis II: After the preemption rule, non-OCC lenders increase the issuance of complex mortgages along dimensions not regulated by state laws, i.e. ARMs, interest-only and deferred amortization loans, significantly more in states with APL than in those without.

We can test this hypothesis by running a similar specification to (3.1) but for the sample of loans originated by non-OCC lenders. Leveraging on the same idea, we consider the spillover effect of the preemption of OCC lenders from APL laws on non-OCC lenders by comparing the behavior of non-OCC lenders in APL states and in non-APL states before and after the preemption. We also include the same set of controls as the ones reported in (3.1).

In addition, in the most conservative specification we exploit *within bank-quarter variation* by including bank times quarter fixed effects in (3.1). This is a powerful test because it allows us to compare - for instance- New Century's origination strategy in a state with an anti-predatory law (e.g. California) to New Century's origination strategy in another state without an APL (e.g. Arizona) within the same quarter. This means that the assumption of parallel trends required by this strategy is quite weaker: we only have to argue that New Century's origination strategy in different states would have been on parallel trends in absence of the preemption rule.

Furthermore, if the mechanism is working through a competition channel, the reaction by non-OCC lenders should be stronger in markets where OCC lenders have a more dominant position. It is precisely in these markets where non-OCC lenders would have had their existing market share threatened, and thus be expected to respond by changing their product portfolio. This suggests the following hypothesis:

Hypothesis III: After the preemption rule, non-OCC lenders increase their issuance of riskier mortgages along dimensions not regulated by state laws, i.e. ARMs, interest-only and deferred amortization loans, significantly more in regions where they face fiercer competition by OCC lenders.

As a proxy for degree of competition with OCC lenders, we construct the *OCC Share*, defined as the fraction of loans (by volume) originated by OCC lenders in 2003. Intuitively, if national banks have a higher market share, then non-OCC lenders might be even more adversely affected by the preemption rule as OCC lenders exploit their position to issue riskier mortgages and capture an even higher market share.[14] Then, we run the following specification on the sample of loans originated by non-OCC lenders:

$$\begin{aligned} Loan\ Features_{i,c,t} \;=\;& \alpha + \beta_1 \cdot Post_{2004} \cdot APL_{g,t} \cdot OCC\ Share_c + \\ & \beta_2 \cdot APL_{g,t} \cdot OCC\ Share_c + \\ & \beta_3 \cdot Post_{2004} \cdot OCC\ Share_c + \\ & \beta_4 \cdot Post_{2004} \cdot APL_{g,t} + \beta_5 \cdot APL_{g,t} + \\ & \Gamma_1 \cdot X_c \cdot Post_{2004} + \Gamma_2 \cdot \Sigma_i + \eta_t + \theta_c + \epsilon_{i,c,t} \end{aligned} \tag{3.2}$$

where the main coefficient of interest is β_1, which – if positive– would provide evidence supporting the hypothesis that non-OCC lenders were indeed more likely to respond to the preemption rule by increasing their origination of complex mortgages in more competitive regions. This would also highlight an interesting interaction between banking deregulation and competition, which might lead the deregulation to have even more widespread effects than expected.

3.5 OCC Banks' Response to the Preemption Rule

In this section, we focus on the effect of the deregulation on the OCC lenders' mortgage origination before and after the preemption by testing whether the features of the OCC mortgages given to these riskier borrowers changed significantly with the preemption. In Table 3.3 our dependent variables include a prepayment penalty indicator, the length of the prepayment term (e.g. the borrower is subject to prepayment penalties if he repays the mortgage within the first two years from origination), and whether the mortgage is adjustable-rate or with deferred amortization as defined by APL laws (i.e. negative amortization or balloon features) or with an interest-only repayment period. The most important of these characteristics is the prepayment penalty, for at least two reasons. First, this is a contract feature that is restricted by all APL laws. Second, as is argued by 65, loans to riskier borrowers tend to have prepayment penalties; otherwise borrowers would refinance as soon as their creditworthiness improves, which would lead them to leave the pool of mortgages and make it riskier.[15] Thus, limiting their use also limits the profitability of lending to riskier borrowers.

Table 3.3 presents the results divided in two panels: Panel A presents the baseline specification without regional controls, while Panel B includes the main controls for the fraction of subprime borrowers, median income and the change in house prices between origination and two years prior to origination, to ensure that our findings are not driven by different fundamentals or house prices growth in APL and non-APL states. Both specifications include borrower-level controls to make sure that we are comparing mortgages with similar characteristics across states. Column 1 in Panel A shows that after the preemption an OCC

[14] In robustness checks (available from the authors) we also show that similar results hold when we proxy for competition by computing the Herfindahl-Hirschman index using data on deposits from the FDIC.

[15] This idea is related to the empirical prepayment literature that found path dependence of prepayment (see, for instance, 86).

mortgage lender in an APL state was about 6% more likely than an OCC lender in a non-APL state to impose a prepayment penalty, while the unconditional mean for presence of prepayment penalties was 31.1%. That is, the preemption resulted in an economically important increase in prepayment penalties in states with APL. Additionally, as is shown in Column 2, OCC lenders' prepayment penalty terms were 2–3 months longer in APL states than those in non-APL states (with an unconditional mean of 8 months). These lenders were also about 5% more likely to originate interest-only mortgages, while we find no change in the origination of mortgages with adjustable rates or deferred amortization features.

Panel B confirms that the results are not significantly affected by controlling for regional heterogeneity. As expected, we do find that in lower income regions the use of prepayment penalties is higher as well as in regions in which house prices increase more significantly. However, even in these specifications we find that OCC lenders increased the origination of mortgages with prepayment penalties and longer prepayment terms as well as interest-only mortgages. Overall, these results show that there is a significant expansion of credit towards riskier contracts in response to the preemption rule.

3.6 Non-OCC Lenders' Response to the Preemption Rule

We can now present the main results of the paper by testing for the presence of spillovers to non-OCC lenders. By creating an un-level playing field whereby lenders not regulated by the OCC still had to comply with state APL laws, the partial deregulation introduced by the OCC might have an *indirect* effect on the non-OCC lenders' credit supply. To test for this hypothesis, we compare the contract features used by non-OCC lenders in states with and without APL. The idea is that non-OCC lenders might respond to the increased competition due to the preemption rule by trying to attract borrowers with more complex features that are not limited by the APL. If this is true, they will do so in states where the preemption has an effect, that is, in states with laws against predatory lending. Moreover, we should not expect non-OCC lenders to originate loans with higher prepayment penalties, because they still have to comply with the state predatory laws. Thus, these spillovers should manifest themselves in other mortgage features.

We can start our analysis by plotting in Figure 3.1 the fraction of loans with different contract features for APL (the blue line) and non-APL states (the red one) over our sample period 2002-2007.[16] There are three things worth noticing. First, the first two panels show that the fraction of loans with preemption penalties was higher in states without predatory laws and that there is no significant differential increase after the preemption, which further confirms that APLs were indeed binding and that the preemption had no effect on non-OCC lenders on those regulated dimensions. Second, the remaining three panels show the fraction of loans with ARMs, deferred amortization and interest-only features and highlight that there has been a significant increase of this type of loans after the preemption rule in APL states. Third, along these three dimensions the pre-period shows no significant differences across states with and without predatory laws, which confirms the validity of our identification

[16]Figure C.1 in the appendix reports similar graphs for the OCC lenders.

approach, i.e. by exploiting within-non-OCC lenders variation we make sure that treatment and control groups are on parallel trends.

We quantify these effects in Table 3.4. Similarly to Table 3.3, we report our results without and with regional controls in Panel A and B, respectively. Both panels show that after the preemption rule non-OCC lenders significantly increased the origination of ARMs by 3%, of mortgages with deferred amortization by 4% and interest-only mortgages by 3% in states with laws against predatory lending. These results remain statistically and economically significant when we include additional regional controls, such as the change in house prices between origination and two years prior to origination, in addition to county and month fixed effects as well as borrowers' characteristics.

It is important to note that non-OCC lenders do not respond by changing their use of prepayment penalties, which is precisely the dimension that OCC lenders adjusted their mortgage terms following the pre-emption ruling. Similarly, OCC lenders do not change their use of adjustable rate and deferred amortization loans following the pre-emption. The exception to this is interest-only mortgages. Thus, we interpret these findings as spillover effects. Overall, these results provide supporting evidence for Hypothesis II by showing that non-OCC lenders expanded their credit supply of complex mortgages to respond to the preemption rule.

To identify the heightened competition as the main mechanism, we can analyze how our results differ across regions with a different degree of market competition. As a proxy for degree of competitiveness, we employ the fraction of loans (by volume) originated by OCC lenders in the pre-period, i.e. in 2003. We denote this fraction the *OCC Share*. Before analyzing the non-OCC lenders origination behavior depending on the local credit market competition, we report in Table 3.5 the coefficient estimates of cross-sectional regressions relating the presence of national banks to a number of county characteristics. The fraction of loans originated by national banks is correlated with several important characteristics of the county. Less populous counties (Column 2) and those with more elastic housing supply (Column 3), less intense securitization activity (Column 4) and lower house price change between January 2001 and December 2003 (Column 6) are those with larger fractions of loans originated by national banks. However, these correlations do not differ significantly between states with and without anti-predatory laws, as is shown by the non-significance of the coefficient on the interaction $High\ OCC\ Share \times APL_{g,2004}$, where $High\ OCC\ Share$ is an indicator variable equal to 1 if the county's fraction of lending by OCC lenders lies above the median of the distribution of this measure. Figure C.2 reports house prices for different terciles of this OCC Share measure showing that there are no significant differences between them. In other words, the correlation between fraction of OCC and county characteristics does not vary with the presence or absence of a state predatory lending law. This reassures us that *Fraction OCC* does not proxy for other possibly relevant mortgage market characteristics.

To understand the role of competition, we first show how OCC lenders' origination strategy changes depending on the degree of competition. In Table 5, we run the baseline specification (3.1) separately for regions with above and below median degree of competitiveness.[17]

[17]Los Angeles County falls exactly on the median of our measure. Given that it is among the largest

By comparing the results in Panel A and Panel B, we find that national banks increased their use of prepayment penalties similarly in counties above and below the median of our measure, but were more aggressive in using them in areas where they had a larger market share. Hence, there is potential for the spillover effect of the deregulation to be stronger in high OCC share counties, and we test this hypothesis next.

Tables 3.7 and 3.8 report the effect of the preemption on non-OCC lending behavior for different regions. Panel A presents the results for the loans originating in a county in the top half of the distribution of *OCC Share*, while Panel B reports the results for the bottom half. Panel A shows that in counties in APL states where OCC lenders have a larger market share, non-OCC lenders respond by issuing mortgages with features not directly restricted by the law. Specifically, after the preemption they make significantly more adjustable-rate and deferred-amortization mortgages as well as interest-only loans. As hypothesized, these effects are concentrated in counties where OCC lenders have larger market shares; these patterns are not generally found in the counties where OCC lenders have little market power (Panel B). The effects are statistically and economically significant even after controlling for county characteristics. Specifically, we find that non-OCC lenders originate 5% more adjustable-rate mortgages, 6% more deferred-amortization mortgages and 3% more interest-only mortgages. Interestingly, there is no significant effect on prepayment penalties or term length, which are the clauses governed by predatory lending laws. This is important as additional confirmation that non-OCC lenders do not react along the same dimensions as the treatment group of OCC lenders. Table 3.8 employs a different strategy by interacting *Post*APL* with the continuous standardized measure *OCC Share*. We confirm that even in this empirical specification we find that a one standard deviation increase in *OCC Share* makes non-OCC lenders 6-8% more likely to originate ARMs and deferred amortization mortgages after the preemption in the APL states.

These findings suggest that, as a result of fiercer competition with national banks, non-OCC lenders were offering more complex mortgages after the preemption. Thus, rather than attenuating the effects of deregulation, competition might induce even lenders not directly affected by the preemption to compete by offering riskier loan contracts. These results are also consistent with a recent study by 18 showing that, after the Interstate Banking and Branching Efficiency Act (IBBEA) in 1994, as a result of intensified competition, banks fully exploited consumers' inattention in the pricing of ARMs as they started to shroud the loans' key attributes (add-on prices).

3.7 Effect of Deregulation on Interest Rates and Defaults

Having shown the results on the changes in the use of different mortgage features in response to the preemption rule, we now explore the changes in rates and defaults to provide further evidence about the mechanism. Specifically, do lenders move down the demand curve and extend credit to borrowers who would earlier not have qualified for mortgages? Or do they

counties in our sample, we exclude it from the analysis to avoid ambiguity. Our results remain robust to including Los Angeles in the estimation as being above the median measure.

shift borrowers from a mortgage without a prepayment penalty, for example, to one with a prepayment penalty? Do these new or existing borrowers subsequently benefit from lower interest rates? Although a complete welfare analysis is outside the scope of this paper, an investigation of interest rates, borrowers' characteristics and ex-post default rates can provide suggestive evidence of whether borrowers benefited from the deregulation.

Modeling the pricing and default behavior of different types of mortgages involves complexities that are challenging to capture fully in a reduced form setting. Hence, to facilitate the analysis, we estimate our difference-in-difference specification on various sub-samples of the data. For example, first we restrict attention to loans originated by OCC lenders that did have pre-payment penalties, and then estimate the regression on OCC-originated loans without these features. Table 3.9 Panel A displays results from the specification where the dependent variable is equal to one if the mortgage became 90+ days delinquent at any point in its history prior to December 2009. Panel B of the same table uses the original interest rate of the mortgage as the dependent variable. Control variables include borrower and loan level characteristics used in our main specifications (Tables 2 and 3), as well as regional controls such as the change in house prices between origination and two years prior to origination. Controlling for house prices helps us ruling out the alternative story that changes in default rates might be driven by cohort effects, i.e. borrowers buying houses later in the cycle were more likely to default.

First, we turn our attention to the OCC-regulated lenders. The results show that loans with prepayment penalties originated by these lenders see a reduction in interest rates of 7.5 basis points as a result of the pre-emption ruling, and begin to default more (2.2 percentage points increase, as compared to a sample average of 27%). The default results of loans without prepayment penalties (i.e they default less) originated by OCC lenders strongly suggest that after the preemption OCC lenders pushed some of the marginal borrowers that otherwise would have borrowed through a plain vanilla contract to a contract with prepayment penalty. Therefore, the remaining pull of loans without prepayment penalty performed better (compared to plain vanilla contracts before the preemption). The decline in the interest rate for OCC loans without prepayment penalty is also consistent with this result. For the OCC loans with prepayment penalty, since the default rate goes up and the interest rate goes down, the interest rates for risky borrowers declined on a risk-adjusted basis. This is consistent with the theoretical framework of 65, which demonstrates that a borrower of a given quality who previously qualified for a loan without prepayment penalties would be better off by choosing one with a prepayment penalty to take advantage of lower interest rates. These lower interest rates are a result of lenders being less exposed to adverse selection of the mortgage pool, as borrowers who receive positive credit quality shocks are prevented from prepaying the loan. These results suggest that the deregulation may have benefited OCC lenders' borrowers.[18]

Note, however, that 65 does not consider what might occur in a setting such as ours, which involves partial deregulation. Our paper contributes by exploring this interaction between

[18]Note that a borrower with a choice between a plain vanilla loan and a more complex one is shifting his preferences, but this still represents a supply-side story, as this change is induced by a shift in the product portfolio of the lenders.

competition and deregulation. Thus, we consider the effect of the pre-emption ruling on the interest and ex-post default rates of non-OCC lenders. The effects of the preemption on adjustable rate mortgages are directionally similar to the effects on loans with prepayment penalties made by OCC lenders; however, as seen from Table 3.4, the largest change in the product portfolio of non-OCC lenders comes from the increased use of deferred amortization and interest-only features. Loans with these features (which we denote to be "Complex") not only default more (Panel A Column 4), but are now offered to borrowers at higher interest rates (Panel A Column 5).

These results suggest that the non-OCC lenders' best response to the new environment created by the preemption was to absorb riskier borrowers by offering them riskier contracts, like contracts with deferred amortization. Deferred amortization helps non-OCC lenders to compete with OCC lenders in two ways. First, the deferred amortization feature of the contract works as a barrier to refinancing (since the remaining principal grows over time) and from that point of view works very similarly to prepayment penalties. Second, by offering lower monthly payments, non-OCC lenders can attract cash-constrained borrowers without giving them any discount on interest rates (in fact we find that the rate goes up by about 20 basis points for these contracts). If these riskier borrowers are sensitive to the size of the monthly payments, they would borrow from non-OCC lenders. However, they may have been made worse off as their balances grow over time and they do not build equity in their homes thus putting them at a larger risk of being underwater.

While a more structured model will be required to conclusively answer whether the increase in interest rates offsets the increase in default rates, we can see that while customers of OCC-lenders benefit from the deregulation via lower interest rates, the same cannot be said for those of non-OCC lenders. They too are being offered loans that put up barriers to refinancing or prepayment, but do not share the benefits of lower interest rates. This suggests that if the benefits from lower monthly payments resulting from deferred-amortizing loans do not outweigh the benefits from lower interest payments, risky borrowers may have been worse off as a result of partial deregulation compared to full deregulation.

To what extent does this change in lending behavior reflect a shift down the demand curve towards lower quality borrowers - i.e. an adjustment along the extensive margin? In Table C.3 in the appendix, we estimate our difference-in-difference specification on the sample of OCC loans and non-OCC loans, now considering as dependent variables credit score and combined loan-to-value ratio at origination, and whether the loan was a cash-out refinance or has a second lien. These characteristics are typically believed to be associated with underwriting quality. We find inconclusive evidence that the preemption ruling resulted in a substantial change in the type of borrower who receives a mortgage, since the effects are small when significant. This is also consistent with Mayer et al. (2013), since their model predicts that banks would change the terms offered to both the existing customers and the new one, with unclear predictions for the average borrower's quality.

Overall, the results indicate that the preemption ruling induced OCC-lenders to shift borrowers into contracts with prepayment penalties, offering them lower rates in return. As the market share of non-OCC lenders is threatened, they increased the adoption of risky contract features, like deferred amortization, as their way of defending their market shares while not violating the state APL laws. These new products ultimately end up defaulting at higher rates. In other words, partial deregulation via competition shifts borrowers into riskier contract types.

3.8 Robustness Checks

In this section, we further test the validity of our identification strategy and examine several alternative explanatory hypotheses.

3.8.1 Bordering Counties

In the main specifications, we have controlled for a number of county characteristics that could potentially affect the credit demand and supply of complex mortgages. However, in order to control even more conservatively for potential heterogeneity across counties, we can focus on the counties that lie on the borders between states with and without predatory laws. The assumption is that these bordering counties should be very similar to each other to begin with, and should also be more likely to be subject to the same shocks, with the only main difference being the presence of predatory lending laws.

Table 3.10 shows the results for the same regression as in (3.1) for the counties at the border, but also including county-pair fixed effects. In other words, this allows us to compare the non-OCC response to the preemption rule in these neighboring counties.[19] Although the sample size shrinks considerably, our main interaction coefficient β_1 on the $Post \cdot APL_{g,t}$ is still highly significant and also slightly larger in magnitude than the one presented in Table 3.4. Specifically, we confirm that also non-OCC lenders respond to the deregulation introduced in 2004 by issuing a larger fraction of complex mortgages.

3.8.2 Within-Bank Variation

The previous specifications have always exploited the possibility to compare non-OCC lenders across state lines. However, we can go one step further and control for bank-time fixed effects which means that we can compare, for instance, New Century's origination strategy in a state with an anti-predatory law (e.g. California) to New Century's origination strategy in another state without an APL (e.g. Arizona) within the same quarter.[20] This is possible because these financial institutions are active across multiple markets at the same time, and it has the key advantage of significantly reducing the heterogeneity between treatment and control groups both cross-sectionally and over time. That is, we do not have to worry about potential time-varying shocks affecting our estimates, e.g. a sudden increase in securitization activity that might affect the willingness of specific banks to issue more complex mortgages. At the same time, by exploiting within-bank variation, we are also able to avoid comparing different banks and their responses to the deregulation.

[19] Table C.1 in the Appendix reports the same specification for the OCC sample.
[20] Table C.2 in the Appendix reports the same specification for the OCC sample.

Table 3.11 reports the main results by running a similar specification to (3.1), but including originator times quarter fixed effects. Overall, this specification also reveals that non-OCC lenders responded to the preemption rule by originating a higher fraction of ARMs, interest-only and deferred amortization loans.

3.8.3 Matching Estimator

One might be concerned that regions with higher national banks' market share in APL states may also differ in other important ways from the regions with high national banks' share in non-APL states, that independently influence the origination of complex mortgages. We address endogeneity concerns in several ways. First, we have shown in Table 3.7 and 3.8 that our results are robust to a battery of controls including county and time fixed effects, as well as median income, share of subprime borrowers, house price changes and detailed mortgage characteristics. Thus, higher competition increases the likelihood of non-OCC banks' making riskier mortgage loans after the preemption in APL states, even after allowing for the possibility that the preemption might differentially affect counties that are heterogenous on these dimensions.

Second, we use a propensity score matching procedure to ensure that the counties with different degrees of competition are similar along the observable dimensions. We consider counties with OCC lending in the top half and estimate, using a logit model, the probability of a county's state having an APL law in place, based on observable characteristics. We do the same for a county with an OCC share in the bottom half of the distribution of this measure. Specifically, we include as covariates in the logit regression unemployment rate, fraction of households with FICO scores below 620 and below 680, average debt to income ratio, the log of median income, employment rate in different industries, the home ownership rate, and the shares of households with college and high school education - all measured in 2000. We then include the propensity scores as weights in a regression, effectively ensuring that the effect is estimated using as a control group those non-APL counties that are most similar to APL counties. Table 3.12 reports the results for counties above and below the median OCC share. We find that, even on this matched sample, in more competitive markets non-OCC lenders were significantly more likely to issue ARMs and riskier mortgages with interest-only or deferred amortization clauses.

3.8.4 Triple Difference-in-Differences

The key identification assumption required by the main difference-in-differences specification is that banks would be on parallel trends in states with and without APLs in the absence of the preemption rule. We can now relax this assumption by adopting a triple differences-in-difference framework where we compare the response of OCC lenders to the preemption rule across states compared to the response of non-OCC lenders. To be valid, this approach only requires that the *difference* between OCC and non-OCC lenders would be on parallel trends in the absence of the preemption rule. Formally, we use the following specification:

$$\begin{aligned} Y_{i,c,t} &= \beta_0 + \beta_1 \cdot Post_t \cdot OCC_i \cdot APL_{g,t} + \beta_2 \cdot Post_t \cdot OCC_i + \beta_3 \cdot OCC_i \cdot APL_{g,t} \\ &\quad + \beta_4 \cdot OCC_i + \Gamma_1 \cdot X_c \cdot Post + \Gamma_2 \cdot \Sigma_i + \eta_c + \theta_t + \epsilon_{i,c,t} \end{aligned} \tag{3.3}$$

where OCC_i is a dummy equal to one for the loans originated OCC lenders and the relevant coefficient is β_1. It estimates:

$$\begin{aligned} &\left(\left[\bar{Y}^{APL}_{OCC,Post} - \bar{Y}^{APL}_{OCC,pre}\right] - \left[\bar{Y}^{Non-APL}_{OCC,Post} - \bar{Y}^{Non-APL}_{OCC,pre}\right]\right) \\ &- \\ &\left(\left[\bar{Y}^{APL}_{Non-OCC,Post} - \bar{Y}^{APL}_{Non-OCC,Pre}\right] - \left[\bar{Y}^{Non-APL}_{Non-OCC,Post} - \bar{Y}^{Non-APL}_{Non-OCC,Pre}\right]\right), \end{aligned}$$

which effectively compares loans originated by OCC and non-OCC lenders across states with and without APL around the preemption rule. To capture time-varying variation that might differentially affect institutions regulated by different agencies, e.g. increase in securitization activity, we include month-agency fixed effects. We also saturate the specification with county-agency fixed effects to compare loans within the same county and type of regulatory agency (e.g. OCC vs. HUD). Table 3.13 shows that OCC lenders are significantly more likely to originate loans with prepayment penalties and longer terms even when compared to the non-OCC lenders, whereas the non-OCC lenders are more likely to expand their offering of ARMs and deferred amortization mortgages as captured by the negative coefficients on the triple interaction in Columns (3) and (4). Both approaches yield broadly consistent results, so that we can be confident that we are capturing the effect of deregulation and not preexisting trends or confounding factors.

3.9 Conclusion

In this paper, we use the preemption of state laws against predatory lending for banks regulated by the OCC, as a quasi-experiment to test for the effect of deregulation on the supply of complex mortgages. This was a shock that expanded the range of permissible loans by OCC-regulated lenders while leaving the set available to non-OCC lenders unchanged. This deregulation allows us to take advantage of two different sources of variation. We exploit the heterogeneity among OCC and non-OCC regulated mortgage originators before and after the preemption rule. Moreover, we also exploit the fact that the preemption only affected a subset of US states, namely those that had predatory lending laws in place.

We obtain two main results. First, the national banks' supply of loans with prepayment penalties and longer prepayment terms increased significantly in response to the deregulation. Second, we inquire into the potentially perverse effects of local mortgage market competition between lenders regulated by different agencies. We find that in highly competitive counties, those where OCC lenders had a higher market share, non-OCC lenders became more aggressive in originating loans with deferred amortization, ARMs and interest-only payments, all features not directly controlled by the state laws against predatory lending.

This is all the more striking in that these non-OCC regulated lenders were not directly affected by the preemption ruling, which means their response can be seen as essentially an effort to defend their market position. Our evidence suggests the existence of a competition channel that induced a potentially adverse response even by banks that remained subject to state regulation.

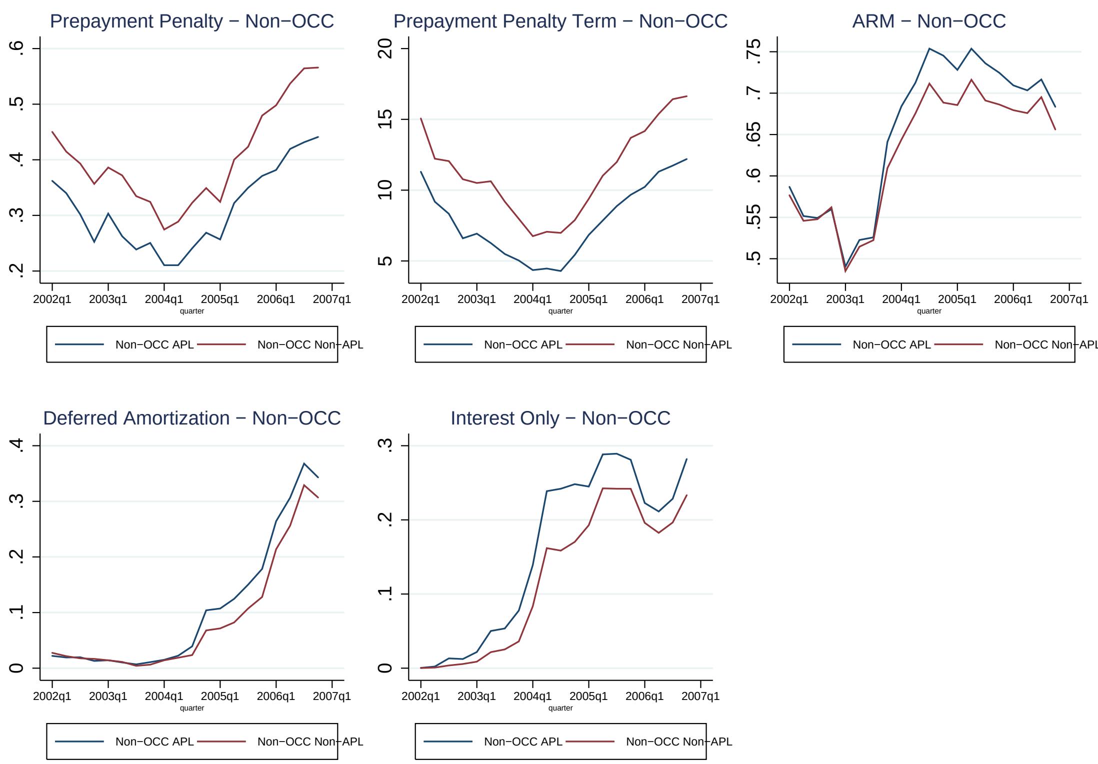


Figure 3.1: Loan terms for Non-OCC lenders
This figure plots the fraction of non-OCC loans with different contract features for APL (the blue line) and non-APL states (the red line) over our sample period 2002-2007.

Table 3.1: Summary Statistics Loan Level (January 2001 to January 2004)

The table below presents Summary Statistics by Regulatory Agency of Lender for Loans that were originated between and including January 2001 and January 2004. OCC refers to loans originated by national banks who were regulated by the OCC. Non-OCC includes all state charted banks and state chartered savings and loans institutions as well as mortgage companies, funding companies and credit unions. Note that we exclude mortgages originated by lenders regulated by the OTS. Credit Score, LTV Ratio and Appraised Value have been winsorized at the 1st and 99th percentile. Second Lien Present is an indicator variable for whether the property had a second lien at the time of origination. PMI is an indicator variable equal to one if the mortgage had private mortgage insurance. Prepayment Penalty Term Violation is an indicator variable capturing whether a loan issued was in violation of the maximum prepayment penalty term length stipulated in the APL as classified by Bostic et al. (2009). Prepayment Penalty, Interest Only and ARM are indicator variables equal to 1 if the mortgage had each of these features respectively. Deferred Amortization is an indicator variable equal to one if the mortgage had a negative amortization or a balloon payment feature.

	States with APL Laws by Feb 2004				States without APL Laws by Feb 2004			
	OCC		*Non-OCC*		*OCC*		*Non-OCC*	
	Mean	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*
Panel A: Covariates								
CreditScore	686.875	77.632	637.305	70.406	672.037	76.971	630.697	67.892
LTV Ratio	0.721	0.194	0.792	0.141	0.794	0.163	0.820	0.133
Appraised Value	266642	236584	246102	180121	157455	134844	165060	127394
Second Lien Present	0.075	0.263	0.081	0.272	0.051	0.221	0.073	0.260
Low or No Doc	0.484	0.500	0.347	0.476	0.378	0.485	0.286	0.452
PMI	0.146	0.353	0.121	0.326	0.148	0.355	0.148	0.355
Panel B: Loan Contract Features								
Prepayment Penalty	0.177	0.382	0.275	0.447	0.332	0.471	0.367	0.482
Prepayment Penalty Term Violation	0.120	0.325	0.167	0.373	-	-	-	-
Deferred Amortization	0.019	0.136	0.016	0.124	0.024	0.154	0.018	0.131
Interest Only Loan	0.013	0.113	0.036	0.187	0.003	0.056	0.016	0.126
ARM Loan	0.224	0.417	0.549	0.498	0.222	0.416	0.536	0.499
Observations	75112		990193		66151		773020	

Table 3.2: Summary Statistics Loan Level (February 2004 to December 2006)

The table below presents Summary Statistics by Regulatory Agency of Lender for Loans that were originated between and including February 2004 and December 2006 in those states that had implemented APL laws by February 2004. OCC refers to loans originated by national banks who were regulated by the OCC. Non-OCC includes all state charted banks and state chartered savings and loans institutions as well as mortgage companies, funding companies and credit unions. Note that we exclude mortgages originated by lenders regulated by the OTS. Credit Score, LTV Ratio and Appraised Value have been winsorized at the 1st and 99th percentile. Second Lien Present is an indicator variable for whether the property had a second lien at the time of origination. PMI is an indicator variable equal to one if the mortgage had private mortgage insurance. Prepayment Penalty Term Violation is an indicator variable capturing whether a loan issued was in violation of the maximum prepayment penalty term length as classified by Bostic et al. (2009). Prepayment Penalty, Interest Only and ARM are indicator variables equal to 1 if the mortgage had each of these features respectively. Deferred Amortization is an indicator variable equal to one if the mortgage had a negative amortization or a balloon payment feature.

	States with APL Laws by Feb 2004				States without APL Laws by Feb 2004			
	OCC		*Non-OCC*		*OCC*		*Non-OCC*	
	Mean	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*
Panel A: Covariates								
CreditScore	674.048	70.165	652.977	68.824	667.689	69.999	647.095	69.293
LTV Ratio	0.758	0.150	0.781	0.127	0.787	0.133	0.803	0.121
Appraised Value	327011	256682	342702	235484	234337	188551	240956	176498
Second Lien Present	0.138	0.345	0.225	0.418	0.113	0.316	0.211	0.408
Low or No Doc	0.412	0.492	0.451	0.498	0.391	0.488	0.386	0.487
PMI	0.193	0.395	0.039	0.194	0.199	0.399	0.046	0.208
Panel B: Loan Contract Features								
Prepayment Penalty	0.263	0.440	0.332	0.471	0.375	0.484	0.431	0.495
Prepayment Penalty Term Violation	0.156	0.363	0.201	0.401	-	-	-	-
Deferred Amortization	0.046	0.210	0.175	0.380	0.052	0.223	0.143	0.350
Interest Only Loan	0.198	0.398	0.250	0.433	0.163	0.369	0.200	0.400
ARM Loan	0.500	0.500	0.724	0.447	0.506	0.500	0.687	0.464
Observations	307082		2956710		301487		2345248	

Table 3.3: OCC in APL and non-APL states

The table reports coefficient estimates from a linear probability model relating the presence of various mortgage terms to the pre-emption ruling of national banks. The sample contains loans originated in states with and without APL laws. We restrict the sample to loans originated by OCC regulated lenders. The dependent variables are as follows: Column 1: an indicator variable for whether the loan has a prepayment penalty; Column 2: length of the prepayment penalty term, with 0 if there is no prepayment penalty; Column 3: indicator variable for whether a loan has an ARM feature; Column 4: indicator variable for whether a loan has either negative amortization or a balloon feature. Column 5: Indicator variable for whether a mortgage had an interest only feature. APL is a time varying indicator variable for whether the state in which the loan was originated had an APL law in place at time of origination. Post is an indicator variable equal to 1 for months after January 2004. All columns include the following controls: the LTV ratio at origination, the Log of appraised value at origination, the borrower's FICO score, an indicator for the presence of second liens, a low or no documentation indicator, an indicator for loan purpose (i.e. cash out refinance, rate refinance or other), and an indicator for the presence of PMI. Panel B also includes as controls the fraction of subprime borrowers and the county median income interacted with Post and the change in house price between origination and two years prior to origination. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A. Difference in Difference OCC Sample APL and Non-APL States

	(1) Prepayment Penalty	(2) Prepayment Term	(3) ARM	(4) Deferred Amort	(5) IO
$Post_t$ x APL_{gt}	0.058***	2.440***	-0.003	0.000	0.046***
	(0.013)	(0.440)	(0.008)	(0.005)	(0.015)
APL_{gt}	-0.163***	-5.252***	-0.023***	-0.004	-0.053***
	(0.015)	(0.422)	(0.007)	(0.003)	(0.011)
Observations	735,443	703,960	735,443	735,443	735,443
R-squared	0.264	0.234	0.276	0.074	0.232
Mean of Dep Var	0.311	8.062	0.449	0.0447	0.151

Panel B. Difference in Difference OCC Sample APL and Non-APL States

	(1) Prepayment Penalty	(2) Prepayment Term	(3) ARM	(4) Deferred Amort	(5) IO
$Post_t$ x APL_{gt}	0.067***	2.654***	-0.016*	-0.002	0.030*
	(0.014)	(0.480)	(0.009)	(0.005)	(0.016)
APL_{gt}	-0.177***	-5.395***	-0.018**	-0.005	-0.052***
	(0.016)	(0.465)	(0.009)	(0.004)	(0.012)
$Post_t$ x Subprime $Borrowers_c$	0.014	-0.933	0.092	-0.049**	-0.137
	(0.085)	(2.594)	(0.061)	(0.020)	(0.121)
$Post_t$ x Median $Income_c$	-0.065*	-1.724	0.068**	-0.004	0.101*
	(0.039)	(1.116)	(0.027)	(0.005)	(0.058)
House Price $Change_{ict}$	0.074***	2.734***	0.007	0.012	-0.028
	(0.021)	(0.688)	(0.033)	(0.020)	(0.040)
Observations	557,261	536,070	557,261	557,261	557,261
R-squared	0.256	0.224	0.275	0.080	0.236
Mean of Dep Var	0.291	7.576	0.458	0.0476	0.176
County FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes

Table 3.4: Non-OCC in APL and non-APL states

The table reports coefficient estimates from a linear probability model relating the presence of various mortgage terms to the pre-emption ruling of national banks. The sample contains loans originated in states with and without APL laws. We restrict the sample to loans originated by non-OCC regulated lenders (excludes OTS regulated lenders). The dependent variables are as follows: Column 1: an indicator variable for whether the loan has a prepayment penalty; Column 2: length of the prepayment penalty term, with 0 if there is no prepayment penalty; Column 3: indicator variable for whether a loan has an ARM feature; Column 4: indicator variable for whether a loan has either negative amortization or a balloon feature. Column 5: Indicator variable for whether a mortgage had an interest only feature. APL is a time varying indicator variable for whether the state in which the loan was originated had an APL law in place at time of origination. Post is an indicator variable equal to 1 for months after January 2004. All columns include the following controls: the LTV ratio at origination, the Log of appraised value at origination, the borrower's FICO score, an indicator for the presence of second liens, a low or no documentation indicator, an indicator for loan purpose (i.e. cash out refinance, rate refinance or other), and an indicator for the presence of PMI. Panel B also includes as controls the fraction of subprime borrowers and the county median income interacted with Post and the change in house price between origination and two years prior to origination. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Difference in Difference Non-OCC Sample APL and Non-APL States

	(1) Prepayment Penalty	(2) Prepayment Term	(3) ARM	(4) Deferred Amort	(5) IO
$Post_t$ x APL_{gt}	0.008	0.545	0.031**	0.041***	0.034**
	(0.009)	(0.342)	(0.015)	(0.014)	(0.016)
APL_{gt}	-0.105***	-3.195***	-0.039***	-0.040***	-0.013
	(0.009)	(0.331)	(0.010)	(0.009)	(0.010)
Observations	6,819,918	6,465,752	6,819,918	6,819,918	6,819,918
R-squared	0.146	0.143	0.137	0.177	0.187
Mean of Dep Var	0.367	9.923	0.667	0.124	0.179

Difference in Difference Non-OCC Sample APL and Non-APL States

	(1) Prepayment Penalty	(2) Prepayment Term	(3) ARM	(4) Deferred Amort	(5) IO
$Post_t$ x APL_{gt}	0.008	0.542	0.029*	0.045***	0.031*
	(0.010)	(0.376)	(0.015)	(0.013)	(0.017)
APL_{gt}	-0.114***	-3.481***	-0.037***	-0.048***	-0.010
	(0.011)	(0.369)	(0.010)	(0.010)	(0.011)
$Post_t$ x Subprime $Borrowers_c$	-0.124**	-3.687**	-0.085	-0.270***	-0.122
	(0.057)	(1.756)	(0.069)	(0.068)	(0.104)
$Post_t$ x Median $Income_c$	-0.017	-0.291	0.004	0.009	0.049
	(0.014)	(0.374)	(0.015)	(0.015)	(0.038)
House Price $Change_{ict}$	0.008	-0.747	0.073***	0.036	0.115***
	(0.022)	(0.683)	(0.018)	(0.026)	(0.022)
Observations	5,396,594	5,100,577	5,396,594	5,396,594	5,396,594
R-squared	0.146	0.143	0.131	0.187	0.186
Mean of Dep Var	0.373	10.02	0.682	0.136	0.202
County FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes

Table 3.5: Examining the Competition Measure (Fraction OCC in 2003)

The table reports coefficient estimates of weighted cross-sectional regressions relating the county level coviariates to our measure of competition- the fraction OCC lending in each county between, and including, 2001 and 2003 in the ABSNet sample. The dependent variables are as follows. Column 1: Fraction of Subprime is estimated from HMDA as the fraction of originations to borrowers with FICO Score below 680; Column 2: The log of the County Population as at 2003; Column 3: A measure of elasticity of housing supply provided by Saiz (2010); Column 4: Fraction Securitized, estimated by dividing the number of loans in the BlackBox data on private securitizations by the total number of loans for each county in HMDA as at 2003; Column 5: Log of the County's Median Income. Column 6: house price change at the county level between January 2001 and December 2003. "APL in 2004" is equal to 1 if the state has an anti-predatory-lending law in place by 2004 and zero otherwise. High OCC Share is an indicator variable equal to 1 if the county's fraction of lending by OCC lenders lies above the median of the distribution of this measure. All regressions are weighted by the number of loans in ABSNet for each county between 2001 and 2003. Standard errors are clustered at the county level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1) Fraction of Subprime	(2) Ln(Pop)	(3) Elasticity	(4) Fraction Securitized	(5) Ln(Median Income)	(6) House Prices
APL in 2004_g x High OCC $Share_c$	0.030	-0.576	-0.207	-0.003	-0.014	-0.020
	(0.020)	(0.417)	(0.207)	(0.024)	(0.045)	(0.015)
APL in 2004_g	-0.001	0.907**	0.032	0.060***	0.071*	0.023*
	(0.017)	(0.370)	(0.158)	(0.020)	(0.037)	(0.012)
High OCC $Share_c$	-0.005	-0.530**	1.001***	-0.036***	-0.027	-0.025***
	(0.014)	(0.223)	(0.122)	(0.008)	(0.029)	(0.008)
Constant	0.444***	12.610***	1.250***	0.134***	10.661***	0.086***
	(0.013)	(0.182)	(0.083)	(0.007)	(0.025)	(0.007)
Observations	2,217	2,217	769	2,160	2,217	873
R-squared	0.014	0.136	0.188	0.155	0.027	0.171

Table 3.6: Competition and OCC Contract Features

The table below reports coefficient estimates of regressions relating the pre-emption of state anti-predatory lending laws for national banks and features of mortgages originated. The sample contains loans originated in states with and without APL laws. We restrict the sample to loans originated by OCC regulated lenders. The dependent variables are as follows: Column 1: Indicator variable for whether a mortgage had an interest-only feature; Column 2: indicator variable for whether a loan has an ARM feature; Column3: indicator variable for whether a loan has either negative amortization or a balloon feature; Column 4: length of the prepayment penalty term, with 0 if there is no prepayment penalty; Column 5: an indicator variable for whether the loan has a prepayment penalty. We divide our sample of mortgages in two depending on the share of OCC lending in the county of origination between 2001 and 2003 based on the ABSNet Sample. Note that we exclude Los Angeles from the results as it is a large county that lies at the median of our measure. Panel A presents the results for the loans originated in a county in the top half of the distribution of this measure, while Panel B reports the results for the bottom half. Note that we exclude Los Angeles from the results as it is a large county that lies at the median of our measure. APL is a time varying indicator variable for whether the state in which the loan was originated had an APL law in place at time of origination. Post is an indicator variable equal to 1 for months after January 2004. All columns include the following controls: the LTV ratio at origination, the Log of appraised value at origination, the borrower's FICO score, an indicator for the presence of second liens, a low or no documentation indicator, an indicator for loan purpose (i.e. cash out refinance, rate refinance or other), and an indicator for the presence of PMI. The columns also include as controls the fraction of subprime borrowers and the county median income interacted with Post and the change in house price between origination and two years prior to origination. Standard errors are clustered at the county level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A. OCC Sample Above the Median					
	(1) Prepayment Penalty	(2) Prepayment Term	(3) ARM	(4) Deferred Amort	(5) IO
$Post_t$ x APL_{gt}	0.058***	2.616***	0.012	-0.003	0.056***
	(0.015)	(0.470)	(0.010)	(0.004)	(0.020)
APL_{gt}	-0.155***	-4.656***	-0.009	-0.005	-0.037**
	(0.016)	(0.493)	(0.012)	(0.004)	(0.015)
Observations	315,576	302,685	315,576	315,576	315,576
R-squared	0.279	0.246	0.294	0.066	0.256
Controls	Yes	Yes	Yes	Yes	Yes
Mean of Dep Var	0.293	7.558	0.446	0.0390	0.165
Panel B. OCC Sample Below the Median					
	(1) Prepayment Penalty	(2) Prepayment Term	(3) ARM	(4) Deferred Amort	(5) IO
$Post_t$ x APL_{gt}	0.061**	1.809**	-0.057***	-0.006	-0.031
	(0.025)	(0.803)	(0.013)	(0.007)	(0.027)
APL_{gt}	-0.178***	-5.394***	-0.001	-0.007	-0.042*
	(0.030)	(0.842)	(0.012)	(0.006)	(0.023)
Observations	212,692	220,431	220,431	220,431	220,431
R-squared	0.188	0.216	0.244	0.091	0.213
Mean of Dep Var	0.294	7.664	0.484	0.0595	0.188
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
County Controls X Post	Yes	Yes	Yes	Yes	Yes
House Price Change	Yes	Yes	Yes	Yes	Yes

Table 3.7: Competition and Non-OCC Contract Features

The table below reports coefficient estimates of regressions relating the pre-emption of state anti-predatory lending laws for national banks and features of mortgages originated. The sample contains loans originated in states with and without APL laws. We restrict the sample to loans originated by non-OCC regulated lenders (excludes OTS regulated lenders). The dependent variables are as follows: Column 1: Indicator variable for whether a mortgage had an interest-only feature; Column 2: indicator variable for whether a loan has an ARM feature; Column3: indicator variable for whether a loan has either negative amortization or a balloon feature; Column 4: length of the prepayment penalty term, with 0 if there is no prepayment penalty; Column 5: an indicator variable for whether the loan has a prepayment penalty. We divide our sample of mortgages in two depending on the share of OCC lending in the county of origination between 2001 and 2003 based on the ABSNet Sample. Panel A presents the results for the loans originated in a county in the top half of the distribution of this measure, while Panel B reports the results for the bottom half. Note that we exclude Los Angeles from the results as it is a large county that lies at the median of our measure. APL is a time varying indicator variable for whether the state in which the loan was originated had an APL law in place at time of origination. Post is an indicator variable equal to 1 for months after January 2004. All columns include the following controls: the LTV ratio at origination, the Log of appraised value at origination, the borrower's FICO score, an indicator for the presence of second liens, a low or no documentation indicator, an indicator for loan purpose (i.e. cash out refinance, rate refinance or other), and an indicator for the presence of PMI. The columns also include as controls the fraction of subprime borrowers and the county median income interacted with Post and the change in house price between origination and two years prior to origination. Standard errors are clustered at the county level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A. Non-OCC Sample Above the Median					
	(1)	(2)	(3)	(4)	(5)
	Prepayment Penalties	Prepayment Term	ARM	Deferred Amort	IO
Post_t x APL_{gt}	0.009	0.259	0.049***	0.057***	0.032*
	(0.012)	(0.329)	(0.015)	(0.016)	(0.019)
APL_{gt}	-0.111***	-3.097***	-0.009	-0.027***	0.021**
	(0.012)	(0.322)	(0.011)	(0.009)	(0.010)
Observations	2,432,526	2,316,205	2,432,526	2,432,526	2,432,526
R-squared	0.140	0.131	0.142	0.176	0.188
Mean of Dep Var	0.364	9.986	0.656	0.122	0.187
Panel B. Non-OCC Sample Below the Median					
	(1)	(2)	(3)	(4)	(5)
	Prepayment Penalties	Prepayment Term	ARM	Deferred Amort	IO
Post_t x APL_{gt}	-0.015	-0.065	-0.010	0.017	0.004
	(0.012)	(0.448)	(0.017)	(0.016)	(0.025)
APL_{gt}	-0.115***	-3.355***	-0.033***	-0.053***	-0.017
	(0.014)	(0.501)	(0.011)	(0.015)	(0.015)
Observations	2,659,277	2,507,998	2,659,277	2,659,277	2,659,277
R-squared	0.154	0.157	0.122	0.193	0.185
Mean of Dep Var	0.373	9.826	0.702	0.142	0.206
County FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
County Controls X Post	Yes	Yes	Yes	Yes	Yes
House Price Change	Yes	Yes	Yes	Yes	Yes

Table 3.8: Competition and Non-OCC Contract Features

The table below reports coefficient estimates of regressions relating the pre-emption of state anti-predatory lending laws for national banks and features of mortgages originated. The sample contains loans originated in states with and without APL laws. We restrict the sample to loans originated by non-OCC lenders (excludes OTS regulated lenders). The dependent variables are as follows: Column 1: Indicator variable for whether a mortgage had an interest-only feature; Column 2: indicator variable for whether a loan has an ARM feature; Column3: indicator variable for whether a loan has either negative amortization or a balloon feature; Column 4: length of the prepayment penalty term, with 0 if there is no prepayment penalty; Column 5: an indicator variable for whether the loan has a prepayment penalty. We divide our sample of mortgages into terciles depending on the share of OCC lending in the county of origination between 2001 and 2003 based on the ABSNet Sample. "OCC share" is the standardized measure of the fraction of loans originated by OCC lenders in a county in 2003. APL is a time varying indicator variable for whether the state in which the loan was originated had an APL law in place at time of origination. Post is an indicator variable equal to 1 for months after January 2004. All columns include the following controls: the LTV ratio at origination, the Log of appraised value at origination, the borrower's FICO score, an indicator for the presence of second liens, a low or no documentation indicator, an indicator for loan purpose (i.e. cash out refinance, rate refinance or other), and an indicator for the presence of PMI. The columns also include as controls the fraction of subprime borrowers and the county median income interacted with Post and the change in house price between origination and two years prior to origination. Standard errors are clustered at the county level. Standard errors are clustered at the county level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)	(4)	(5)
	Prepayment Penalties	Prepayment Term	ARM	Deferred Amort	IO
$Post_t$ x APL_{gt} x $OCCShare_c$	0.003	-0.767	0.076***	0.058***	0.030
	(0.020)	(0.630)	(0.028)	(0.022)	(0.022)
APL_{gt} x $OCCShare_c$	0.021	1.278*	0.018	0.007	0.026
	(0.021)	(0.662)	(0.014)	(0.013)	(0.016)
$Post_t$ x $OCCShare_c$	-0.059***	-1.064***	-0.053**	-0.066***	-0.039**
	(0.012)	(0.326)	(0.022)	(0.013)	(0.016)
$Post_t$ x APL_{gt}	0.004	0.075	0.062***	0.068***	0.043***
	(0.013)	(0.408)	(0.017)	(0.016)	(0.015)
APL_{gt}	-0.105***	-2.881***	-0.024**	-0.044***	0.005
	(0.010)	(0.331)	(0.011)	(0.008)	(0.010)
Observations	5,396,577	5,100,560	5,396,577	5,396,577	5,396,577
R-squared	0.145	0.141	0.13	0.185	0.185
County FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
County Controls X Post	Yes	Yes	Yes	Yes	Yes
Mean of Dep Var	0.373	10.02	0.682	0.136	0.202

Table 3.9: Ex-Post Default and Interest Rate Response

he table below reports coefficient estimates of regressions relating the pre-emption of state anti-predatory lending laws for national banks and, in Panel A, the ex-post default rate of originated loans, and in Panel B, the interest rate at origination of these mortgages. The sample contains loans originated by OCC lenders (Columns 1 to 2) and non-OCC lenders (excludes OTS regulated lenders) (Columns 3 to 5). The dependent variable in Panel A is equal to 1 if the loan becomes seriously delinquent at some point in its history before 2009. The dependent variable in Panel B is the interest rate at origination (note this is not the teaser rate for loans with ARM or other variable interest features). "Complex" refers to mortgages that have deferred amortization features or interest only features. APL is a time varying indicator variable for whether the state in which the loan was originated had an APL law in place at time of origination. Post is an indicator variable equal to 1 for months after January 2004. All columns include the following controls: the LTV ratio at origination, the Log of appraised value at origination, the borrower's FICO score, an indicator for the presence of second liens, a low or no documentation indicator, an indicator for loan purpose (i.e. cash out refinance, rate refinance or other), and an indicator for the presence of PMI. The columns also include as controls the fraction of subprime borrowers and the county median income interacted with Post and the change in house price between origination and two years prior to origination. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A: Ex-Post Default Response

	(1) OCC With Prepay-Pen	(2) OCC No Prepay-Pen	(3) Non OCC Only ARMs	(4) Non OCC Only Complex	(5) Non OCC Not ARM or Complex
$Post_t$ x APL_{gt}	0.022*	-0.015***	0.029***	0.059***	0.005
	(0.012)	(0.005)	(0.009)	(0.013)	(0.008)
APL_{gt}	-0.001	0.021***	-0.032***	-0.078***	-0.002
	(0.010)	(0.005)	(0.008)	(0.015)	(0.006)
Observations	162,348	394,913	3,680,511	1,793,443	1,475,912
R-squared	0.181	0.148	0.173	0.193	0.112
County FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
County x Post Controls	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Mean of Dep Var	0.270	0.165	0.313	0.358	0.169

Panel B: Interest Rate Response

	(1) OCC With Prepay-Pen	(2) OCC No Prepay-Pen	(3) Non OCC Only ARMs	(4) Non OCC Only Complex	(5) Non OCC Not ARM or Complex
$Post_t$ x APL_{gt}	-0.075***	-0.141***	-0.078*	0.200***	0.069***
	(0.026)	(0.0318)	(0.040)	(0.061)	(0.015)
APL_{gt}	0.048	0.172***	0.260***	-0.079	0.010
	(0.029)	(0.0370)	(0.030)	(0.076)	(0.014)
Observations	162,339	394,911	3,679,995	1,793,132	1,475,912
R-squared	0.409	0.517	0.401	0.316	0.477
County FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
County x Post Controls	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Mean of Dep Var	7.481	6.7468	6.909	5.802	7.306

Table 3.10: Non-OCC in APL vs non-APL States (Bordering Counties)

The table below reports coefficient estimates of regressions relating the pre-emption of state anti-predatory lending laws for national banks and features of mortgages originated. We restrict the sample to loans originated by non-OCC lenders (excludes OTS regulated lenders) in counties that lie on state borders such that one side of the border has an APL law in place and the other side does not. The dependent variables are as follows: Column 1: an indicator variable for whether the loan has a prepayment penalty; Column 2: length of the prepayment penalty term, with 0 if there is no prepayment penalty; Column 3: indicator variable for whether a loan has an ARM feature; Column 4: indicator variable for whether a loan has either negative amortization or a balloon feature. Column 5: Indicator variable for whether a mortgage had an interest only feature. APL is a time varying indicator variable for whether the state in which the loan was originated had an APL law in place at time of origination. Post is an indicator variable equal to 1 for months after January 2004. All columns include the following controls: the LTV ratio at origination, the Log of appraised value at origination, the borrower's FICO score, an indicator for the presence of second liens, a low or no documentation indicator, an indicator for loan purpose (i.e. cash out refinance, rate refinance or other), and an indicator for the presence of PMI. The columns also include as controls the fraction of subprime borrowers and the county median income interacted with Post and the change in house price between origination and two years prior to origination. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)	(4)	(5)
	Prepayment Penalty	Prepayment Term	ARM	Deferred Amort	IO
Post_t x APL_{gt}	-0.016	-0.339	0.053***	0.051**	0.035
	(0.023)	(0.834)	(0.017)	(0.021)	(0.023)
APL_{gt}	-0.145***	-4.111***	-0.049***	-0.057***	-0.045***
	(0.018)	(0.767)	(0.011)	(0.015)	(0.012)
Post_t x Subprime Borrowers_c	-0.205	-3.662	0.202*	0.065	0.323**
	(0.129)	(4.030)	(0.107)	(0.114)	(0.153)
Post_t x Median Income_c	-0.067	-1.129	0.147***	0.095**	0.223***
	(0.052)	(1.600)	(0.040)	(0.041)	(0.049)
House Price Change_{ict}	-0.045	-2.428**	0.051*	0.051	0.067***
	(0.036)	(1.103)	(0.031)	(0.049)	(0.023)
Observations	1,625,110	1,545,343	1,625,110	1,625,110	1,625,110
R-squared	0.153	0.150	0.125	0.177	0.187
County Pair FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
County X Post Controls	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Mean of Dep Var	0.333	8.690	0.690	0.133	0.196

Table 3.11: Non OCC in APL vs non-APL States (with Originator by Quarter FE)

The table below reports coefficient estimates of regressions relating the pre-emption of state anti-predatory lending laws for national banks and features of mortgages originated, controlling for originator times quarter fixed effects. The sample contains loans originated by non-OCC lenders (excludes OTS regulated lenders). The dependent variables are as follows: Column 1: an indicator variable for whether the loan has a prepayment penalty; Column 2: length of the prepayment penalty term, with 0 if there is no prepayment penalty; Column 3: indicator variable for whether a loan has an ARM feature; Column 4: indicator variable for whether a loan has either negative amortization or a balloon feature. Column 5: Indicator variable for whether a mortgage had an interest only feature. APL is a time varying indicator variable for whether the state in which the loan was originated had an APL law in place at time of origination. Post is an indicator variable equal to 1 for months after January 2004. All columns include the following controls: the LTV ratio at origination, the Log of appraised value at origination, the borrower's FICO score, an indicator for the presence of second liens, a low or no documentation indicator, an indicator for loan purpose (i.e. cash out refinance, rate refinance or other), and an indicator for the presence of PMI. The columns also include as controls the fraction of subprime borrowers and the county median income interacted with Post and the change in house price between origination and two years prior to origination. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)	(4)	(5)
	Prepayment Penalty	Prepayment Term	ARM	Deferred Amort	IO
$Post_t$ x APL_{gt}	-0.001	0.026	0.029*	0.029***	0.032**
	(0.009)	(0.318)	(0.015)	(0.011)	(0.015)
$Post_t$	0.075	0.912	0.106	0.061	-0.331
	(0.128)	(3.881)	(0.164)	(0.125)	(0.353)
APL_{gt}	-0.114***	-3.099***	-0.034***	-0.039***	-0.014
	(0.011)	(0.306)	(0.010)	(0.008)	(0.010)
$Post_t$ x Subprime $Borrowers_c$	-0.023	0.172	-0.117*	-0.208***	-0.127
	(0.054)	(1.562)	(0.068)	(0.058)	(0.089)
$Post_t$ x Median $Income_c$	-0.006	-0.045	-0.003	0.003	0.038
	(0.011)	(0.340)	(0.015)	(0.011)	(0.031)
House Price $Change_{ict}$	0.019	-0.162	0.079***	-0.021	0.129***
	(0.023)	(0.771)	(0.021)	(0.026)	(0.017)
Observations	5,302,030	5,008,176	5,302,030	5,302,030	5,302,030
R-squared	0.332	0.304	0.226	0.326	0.289
County FE	Yes	Yes	Yes	Yes	Yes
Originator by Quarter FE	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Mean of Dep Var	Yes	Yes	Yes	Yes	Yes

Table 3.12: Competition and Non-OCC Contract Features (Matching Estimator)

The table below reports coefficient estimates of weighted regressions relating the pre-emption of state APL laws for national banks and features of mortgages originated. The sample contains loans originated in states with and without APL laws. We match counties without APL laws to counties with APL laws using a propensity score matching procedure. We match counties based on key observables such as unemployment rate, fraction subprime, median income, average debt to income, home ownership rate and college and high school graduation rate. We further restrict the sample to loans originated by non-OCC lenders (excludes OTS regulated lenders). The dependent variables are as follows: Column 1: an indicator variable for whether the loan has a prepayment penalty; Column 2: length of the prepayment penalty term, with 0 if there is no prepayment penalty; Column 3: indicator variable for whether a loan has an ARM feature; Column 4: indicator variable for whether a loan has either negative amortization or a balloon feature; Column 5: indicator variable for whether a mortgage had an interest only feature. We divide our sample of mortgages in two depending on the share of OCC lending in the county of origination between 2001 and 2003 based on the ABSNet Sample. Note that we exclude Los Angeles from the results as it is a large county that lies at the median of our measure. Panel A. presents the results for the loans originated in a county in the top half of the distribution of this measure, while Panel B reports the results for the bottom half. APL is a time varying indicator variable for whether the state in which the loan was originated had an APL law in place at time of origination. Post is an indicator variable equal to 1 for months after January 2004. All columns include the following controls: the LTV ratio at origination, the Log of appraised value at origination, the borrower's FICO score, an indicator for the presence of second liens, a low or no documentation indicator, an indicator for loan purpose (i.e. cash out refinance, rate refinance or other), and an indicator for the presence of PMI. The columns also include as controls the fraction of subprime borrowers and the county median income interacted with Post and the change in house price between origination and two years prior to origination. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A. Non-OCC Sample Above the Median

	(1) Prepayment Penalty	(2) Prepayment Term	(3) ARM	(4) Deferred Amort	(5) IO
$Post_t$ x APL_{gt}	0.001	0.069	0.045***	0.043**	0.032
	(0.013)	(0.373)	(0.017)	(0.020)	(0.020)
APL_{gt}	-0.114***	-3.219***	0.006	-0.013	0.033**
	(0.014)	(0.382)	(0.013)	(0.012)	(0.013)
House Price $Change_{it}$	0.044	0.421	0.102***	0.086*	0.106***
	(0.046)	(1.387)	(0.028)	(0.050)	(0.027)
Observations	2,364,309	2,252,086	2,364,309	2,364,309	2,364,309
R-squared	0.141	0.131	0.144	0.173	0.186
County FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Mean of Dep Var	0.346	9.464	0.656	0.118	0.186

Table 3.12: Competition and Non-OCC Contract Features (Matching Estimator)

Panel B. Non-OCC Sample Below the Median

	(1)	(2)	(3)	(4)	(5)
	Prepayment Penalty	Prepayment Term	ARM	Deferred Amort	IO
$Post_t$ x APL_{gt}	-0.013	-0.060	-0.003	0.020	0.013
	(0.011)	(0.391)	(0.019)	(0.019)	(0.028)
APL_{gt}	-0.125***	-3.704***	-0.028**	-0.046**	-0.013
	(0.014)	(0.445)	(0.013)	(0.018)	(0.021)
House Price $Change_{it}$	-0.066**	-2.569**	0.044*	0.013	0.082**
	(0.031)	(1.114)	(0.026)	(0.039)	(0.039)
Observations	2,655,655	2,504,614	2,655,655	2,655,655	2,655,655
R-squared	0.155	0.158	0.127	0.194	0.191
County FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Mean of Dep Var	0.348	8.975	0.705	0.137	0.202

Table 3.13: Triple Difference-In-Difference Estimator

The table below reports coefficient estimates of regressions relating the pre-emption of state anti-predatory lending laws for national banks and features of mortgages originated. The sample contains loans made in those states with and without APL laws. We include both loans originated by OCC regulated lenders and those originated by non-OCC regulated lenders (but excluding OTS regulated lenders). The dependent variables are as follows: Column 1: an indicator variable for whether the loan has a prepayment penalty; Column 2: length of the prepayment penalty term, with 0 if there is no prepayment penalty; Column 3: indicator variable for whether a loan has an ARM feature; Column 4: indicator variable for whether a loan has either negative amortization or a balloon feature. Column 5: Indicator variable for whether a mortgage had an interest only feature. OCC is an indicator for whether the mortgage was originated by an OCC regulated lender. APL is a time varying indicator variable for whether the state in which the loan was originated had an APL law in place at time of origination. Post is an indicator variable equal to 1 for months after January 2004. All columns include the following controls: the LTV ratio at origination, the Log of appraised value at origination, the borrower's FICO score, an indicator for the presence of second liens, a low or no documentation indicator, an indicator for loan purpose (i.e. cash out refinance, rate refinance or other), and an indicator for the presence of PMI. The columns also include as controls the fraction of subprime borrowers and the county median income interacted with Post and the change in house price between origination and two years prior to origination. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)	(4)	(5)
	Prepay Pen	Term Length	ARM	Def Amort	IO
$Post_t$ x APL_{gt} x OCC_i	0.053***	1.834***	-0.033*	-0.043***	0.011
	(0.010)	(0.333)	(0.017)	(0.011)	(0.010)
OCC_i x $Post_t$	-0.077**	-3.821***	0.099***	-0.267***	-0.020**
	(0.032)	(1.035)	(0.023)	(0.021)	(0.010)
APL_{gt} x OCC_i	-0.068***	-2.063***	0.015	0.039***	-0.038***
	(0.014)	(0.374)	(0.014)	(0.008)	(0.009)
$Post_t$ x APL_{gt}	0.008	0.569	0.028*	0.044***	0.030*
	(0.010)	(0.374)	(0.015)	(0.013)	(0.017)
APL_{gt}	-0.113***	-3.460***	-0.037***	-0.048***	-0.011
	(0.011)	(0.368)	(0.010)	(0.010)	(0.011)
$Post_t$ x Subprime $Borrowers_c$	-0.113**	-3.455**	-0.070	-0.252***	-0.123
	(0.057)	(1.721)	(0.065)	(0.063)	(0.104)
$Post_t$ x Median $Income_c$	-0.020	-0.405	0.009	0.008	0.053
	(0.015)	(0.385)	(0.014)	(0.013)	(0.040)
House Price $Change_{ict}$	0.014	-0.448	0.067***	0.034	0.103***
	(0.021)	(0.647)	(0.018)	(0.025)	(0.019)
Observations	5,953,855	5,636,647	5,953,855	5,953,855	5,953,855
R-squared	0.157	0.150	0.163	0.187	0.191
County FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
County by Agency FE	Yes	Yes	Yes	Yes	Yes
Month by Agency FE	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes

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Appendix A

Appendix to Chapter 1

A.1 Censored regression analysis

Deriving the log-likelihood and average partial effect

Recall that the model was given by

$$T^* = \beta \cdot m + \epsilon \text{ where } \epsilon \sim N(0, \sigma_\epsilon^2) \tag{A.1}$$

$$T = \begin{cases} T^* & \text{if } Censored = 0 \\ T^{max} & \text{if } Censored = 1 \end{cases} \tag{A.2}$$

$$Modify = \mathbf{1}_{\gamma Z + \upsilon > 0} \text{ where } \upsilon_i \sim N(0, \sigma_\upsilon^2) \tag{A.3}$$
$$\text{and where } Cov(\epsilon, \upsilon) \neq 0$$

where m is an indicator variable equal to 1 if the loan is modified. $T^{max} = 360$ for loans that were not censored and T^{max} equals the observed data for loans that are censored. First, ignore the endogeneity (equations A.3) and consider the censored regression model of equations A.1 and A.2. I wish to derive the log-likelihood function, and the expression for obtaining the average partial effect of loan modification on the number of monthly payments made by a borrower following entry into serious delinquency. I abstract away from other control variables used in the model. First, I obtain an expression for the likelihood of observing a given T_i depending on whether a loan observation is censored (i.e. loan has not left the sample as at December 2013) or not censored.

The cdf of the latent variable T^* will be:

$$\begin{aligned} P(T^* \leq \tau) &= F_{T^*}(\tau) \\ &= P(m \cdot \beta + u \leq \tau) \\ &= \Phi\left(\frac{\tau - m \cdot \beta}{\sigma}\right) \end{aligned}$$

which implies that the pdf is:

$$f_{T^*}(\tau) = \frac{1}{\sigma}\phi\left(\frac{\tau - m\cdot\beta}{\sigma}\right)$$

If the loan is censored, the true realization of the latent variable T^* is not observed. Rather, some loan specific upper bound, T^{Max} will be observed.

$$\begin{aligned} P(Censored = 1) = P(T^* > T_i^{max}) &= 1 - F_{T^*}(T^{max}) \\ &= 1 - \Phi\left(\frac{T^{max} - m\cdot\beta}{\sigma}\right) \\ &= \Phi\left(-\left(\frac{T^{max} - m\cdot\beta}{\sigma}\right)\right) \end{aligned}$$

Therefore, the log likelihood for observation i can be written as:

$$\begin{aligned} \log\, f(T_i \mid \beta, \sigma) = Censored_i \cdot \log\,\Phi\left(-\left(\frac{T_i^{max} - m\cdot\beta}{\sigma}\right)\right) \\ + (1 - Censored_i)\cdot \log\left(\frac{1}{\sigma}\phi\left(\frac{T_i - m_i\cdot\beta}{\sigma}\right)\right) \end{aligned}$$

Since m is a binary variable, the average partial effect cab be expressed as $E[T \mid m = 1] - E[T \mid m = 0]$. $E[T \mid m]$ can be expressed as:

$$\begin{aligned} E[T \mid m] &= P(Censored = 1 \mid m)\cdot T^{max} + P(Censored = 0)\cdot E[T \mid T < T^{max}, m] \\ &= \Phi\left(-\left(\frac{T_i^{max} - m\beta}{\sigma}\right)\right)\cdot T^{max} + \Phi\left(\frac{T^{max} - m\beta}{\sigma}\right)\cdot [m\beta + E[u \mid T < T^{max}, m]] \\ &= \Phi\left(-\left(\frac{T_i^{max} - m\beta}{\sigma}\right)\right)\cdot T^{max} + \Phi\left(\frac{T^{max} - m\beta}{\sigma}\right)\cdot\left(m\beta - \sigma\frac{\phi\left(\frac{T^{max}-m\beta}{\sigma}\right)}{\Phi\left(\frac{T^{max}-m\beta}{\sigma}\right)}\right) \\ &= T^{max} - (T^{max} - m\beta)\cdot\Phi\left(\frac{T^{max} - m\beta}{\sigma}\right) - \sigma\phi\left(\frac{T^{max} - m\beta}{\sigma}\right) \end{aligned}$$

Now, using data on $\{T_i, T_i^{max}, m_i, Censored_i\}_{i=1,\ldots,N}$ the average partial effect can be computed as:

$$\begin{aligned} N^{-1}\sum_{i=1}^{N}\hat{\beta}\Phi\left(\frac{T_i^{max} - \hat{\beta}}{\hat{\sigma}}\right) + T_i^{max}\left(-\Phi\left(\frac{T_i^{max} - \hat{\beta}}{\hat{\sigma}}\right) + \Phi\left(\frac{T_i^{max}}{\hat{\sigma}}\right)\right) \\ + \hat{\sigma}\left(-\phi\left(\frac{T_i^{max} - \hat{\beta}}{\hat{\sigma}}\right) + \phi\left(\frac{T_i^{max}}{\hat{\sigma}}\right)\right) \end{aligned}$$

Deriving the log-likelihood function for censored regression model with endogenous dummy variable

Note that if loan modification were randomly assigned, the average partial effect, would allow me to capture the average treatment effect of loan modification. However, loan modification is not randomly assigned and so I will augment this censored regression model with an endogenous dummy variable. m will be the endogenous dummy variable in this case. I assume that there exists a vector Z_i that is excluded from (A.1) and is independent of ϵ_i. I assume $m = \mathbf{1}\{\gamma Z + v > 0\}$.

The pdf of the joint distribution of m and T conditional on Z_i will be:

$$f(T, m \mid Z) = f(T \mid m, Z) \cdot f(m \mid Z) \tag{A.4}$$

$f(m \mid Z)$ will be given by the standard likelihood function for a probit model. Note that there will be four cases in the data.

$$\begin{aligned}
&\text{Case 1: } Censored_i = 0;\ m_i = 0\\
&\text{Case 2: } Censored_i = 0;\ m_i = 1\\
&\text{Case 3: } Censored_i = 1;\ m_i = 0\\
&\text{Case 4: } Censored_i = 1;\ m_i = 1
\end{aligned}$$

The density$f(T \mid m, Z)$ can be derived for each of these cases. First, the equation for the latent variable T^* can be written as:

$$T^* = \beta \cdot m + \theta v + e_1$$

where, by the joint normality assumption;

$$\epsilon = \theta v + e_1$$

where $\theta = \frac{Cov(\epsilon,v)}{Var(v)} = \frac{\rho_1}{\sigma_v^2}$ and where $Var(e_1) = \sigma_\epsilon^2 - \frac{\rho^2}{\sigma_v^2} \equiv \mu^2$. Upon making this substitution, the density $f(T^* \mid m, Z, v)$ takes the usual censored regression form as derived above. For example, consider the density of T^*, conditional on m, $Z_{S\times t_0}$and v, in Case 1 and 2 where the data is not censored:

$$f(T \mid m, Z, v) = \frac{1}{\mu}\phi\left(\frac{\tau - m \cdot \beta - \theta v}{\mu}\right)$$

and subsequently in Cases 3 and 4, where the data is censored:

$$f(T \mid m, Z, v) = \Phi\left(-\left(\frac{T^{max} - m \cdot \beta - \theta v}{\mu}\right)\right)$$

Having obtained the likelihood function conditional on v, I now use the fact that $m_i = 1$ if the shock to the latent variable underlying the model, v, is realized to be greater than $-\gamma Z$. In this case, the density of T conditional on m, and the density of m can be written as:

$$\begin{gathered}
f(T \mid m, Z) = \tfrac{1}{\Phi(\gamma Z)} \int_{-\gamma Z}^{\infty} f(T \mid m, Z, \xi)\phi(\xi)d\xi\\
f(m \mid Z) = \Phi(\gamma Z)
\end{gathered}$$

Alternatively, if $m_i = 0$:

$$f(T \mid m, Z) = \tfrac{1}{1-\Phi(\gamma Z)} \int_{-\infty}^{-\gamma Z} f(T \mid m, Z, \xi)\phi(\xi)d\xi$$
$$f(m \mid Z) = (1 - \Phi(\gamma Z))$$

Putting these expressions together, and considering equation A.4, the likelihood functions for each of the four cases of the data can be written as:

$$\text{Case 1:} f^1(T_i; \beta, \gamma, \sigma_e, \sigma_v, \rho_1) = \int_{-\infty}^{-\gamma\Lambda_{S\times t_0}} \frac{1}{\mu}\phi\left(\frac{T_i - m_i \cdot \beta - \theta\xi_i}{\mu}\right)\phi(\xi_i)d\xi_i$$
$$\text{Case 2:} f^2(T_i; \beta, \gamma, \sigma_e, \sigma_v, \rho_1) = \int_{-\gamma\Lambda_{S\times t_0}}^{\infty} \frac{1}{\mu}\phi\left(\frac{T_i - m_i \cdot \beta - \theta\xi_i}{\mu}\right)\phi(\xi_i)d\xi_i$$
$$\text{Case 3:} f^3(T_i; \beta, \gamma, \sigma_e, \sigma_v, \rho_1) = \int_{-\infty}^{-\gamma\Lambda_{S\times t_0}} \Phi\left(-\left(\frac{T_i^{max} - m_i \cdot \beta - \theta\xi_i}{\mu}\right)\right)\phi(\xi_i)d\xi_i$$
$$\text{Case 4:} f^4(T_i; \beta, \gamma, \sigma_e, \sigma_v, \rho_1) = \int_{-\gamma\Lambda_{S\times t_0}}^{\infty} \Phi\left(-\left(\frac{T_i^{max} - m_i \cdot \beta - \theta\xi_i}{\mu}\right)\right)\phi(\xi_i)d\xi_i$$

The range of the integration depends on whether the loan has been modified or not modified, and the expression that enters the integration depends on whether the observation for the loan in the data is considered to be censored. Average Partial Effects can be computed as above, using the estimated values of β and σ_e that result from the full maximum likelihood procedure. The maximum likelihood procedure is implemented in Stata using the "cmp" command.

A.2 Matching LPS to ABSNet and GSE Data

In order to obtain the names of servicers and originators for loans in the LPS data, I employ a simple algorithm to match the LPS dataset to the ABSNet data on privately securitized mortgages, and the data on 30 Year Fixed Rate Mortgages from Fannie Mae and Freddie Mac. First, I will describe the methodology used to match the LPS dataset to the ABSNet dataset, and then discuss how this is modified when matching to the GSE datasets.

First, for every loan in the LPS dataset, I find loans in the ABSNet dataset that have the same interest rate, five digit zip code, loan amount and first month that a mortgage payment is due. This will result in pairs of mortgages that are identical on these characteristics but might differ on others. At this stage, each LPS loan will be potentially mapped to more than one ABSNet loan. I keep only those pairs for which both borrowers have the exact same FICO score at origination. Then, I keep those pairs for which the loan purpose is the same. Next, I keep only those pairs where the loans have loan to value ratio at origination which is within 2 percentage points of each other. Among the set of pairs that a given LPS loan may still be in, I keep the pair with the least difference in the loan-to-value ratios and the least difference in the credit score. I achieve a match rate of 52%.

Next, I match the LPS sample to the GSE data. I first follow a similar procedure as above. In the first round of matching I obtain pairs of loans with exact matches on interest rate, three digit zip code, loan amount and first month that a mortgage payment is due. Then, I keep only those pairs for which both borrowers have exact same FICO score at origination; then keep those for whom the loan purpose is the same, and then those for whom the LTV at origination is within 2 percentage points of each other. I drop all LPS loans that have not been uniquely paired at this point. Since the data does not go into more granular geographic detail than a 3 digit zip code, I want to minimize matching errors. I trade-off precision of the match with a lower match rate. I achieve a match rate of 47%.

A.3 Appendix Graphs and Tables for Chapter 1

Table A.1: Sensitivity to Key Assumptions

	(1)	(2)	(3)	(4)	(5)
	ΔPV(Interest PMTs)	ΔPV(Principal PMTs)	ΔPV(Principal Termination)	Gains	Across CBSA by Time SD of Gains
Foreclosure Discount $\phi = 1$	0.2030	0.0835	-0.5025	-0.2160	
	(0.0598)	(0.0184)	(0.1616)	(0.1735)	(0.1540)
Foreclosure Discount $\phi = 0.65$	0.2030	0.0835	-0.1879	0.0986	
	(0.0598)	(0.0184)	(0.0939)	(0.1098)	(0.0440)
Perfect Foresight House Prices	0.2030	0.0836	-0.2684	0.0182	
	(0.0598)	(0.0184)	(0.1037)	(0.1178)	(0.1070)
Foreclosure Timelines	0.2030	0.0836	-0.1894	0.0972	
	(0.0598)	(0.0184)	(0.1079)	(0.1227)	(0.0690)
Higher rate of post-mod redefault	0.2030	0.0836	-0.2690	0.0176	
	(0.0598)	(0.0184)	(0.1015)	(0.1171)	(0.0710)

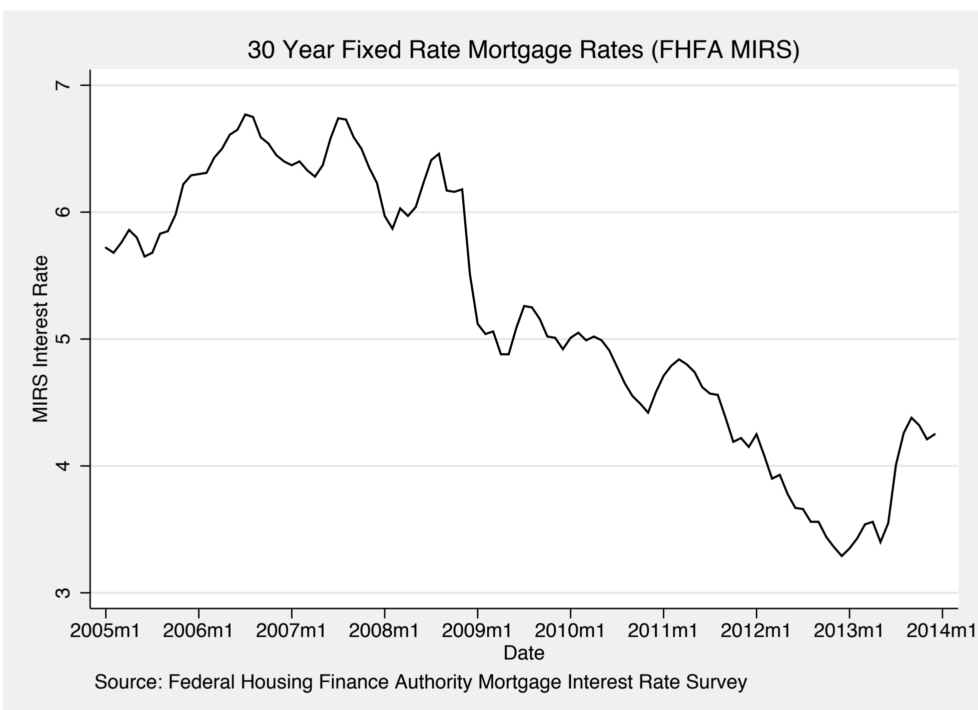

Figure A.1: Across CBSA by time of delinquency variation

The graph above plots the time series of the Federal Housing Finance Authority's Mortgage Interest Rate Survey.

$$\underbrace{\sum_{k=1}^{T_{Mod}} \frac{\Delta}{(1+R_1)^k}}_{1} + \underbrace{\sum_{k=TNoMod}^{T_{Mod}} \frac{d}{(1+R_1)^k}}_{2} = \underbrace{\Delta PV(\text{PMTs})}_{3}$$

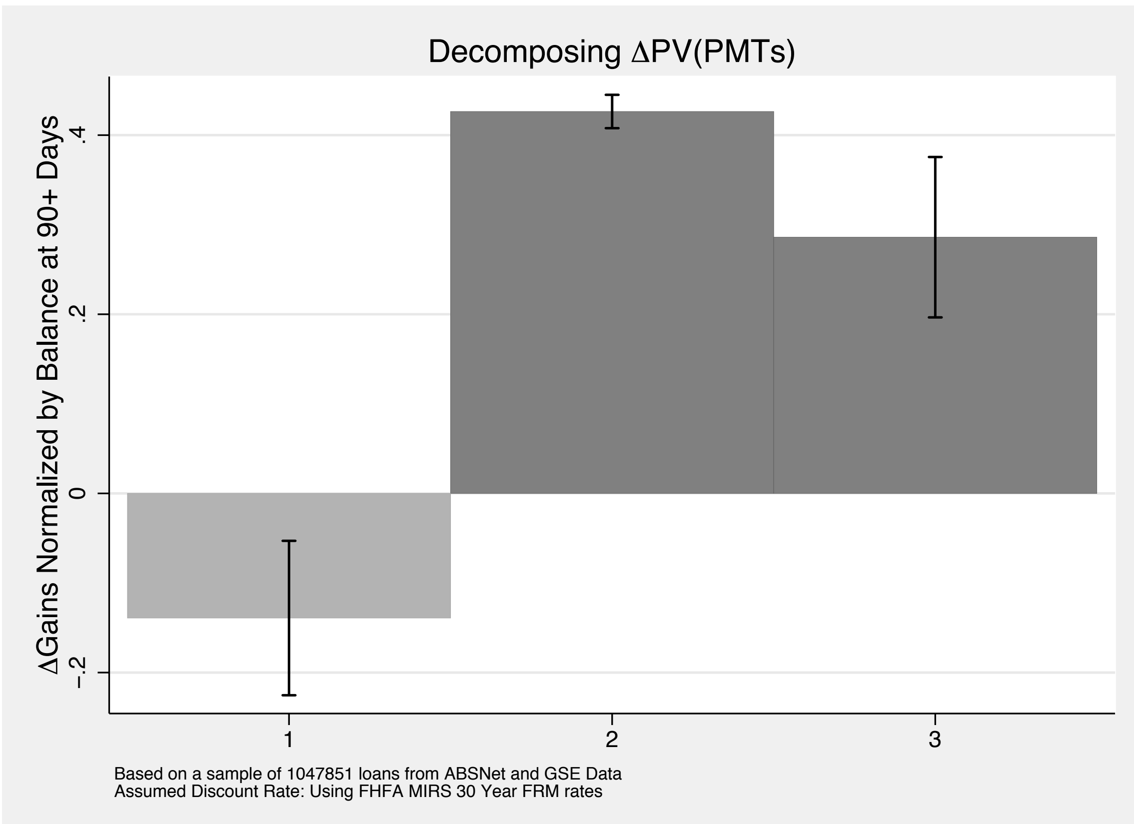

Figure A.2: Decomposing the gains from monthly payments

This graph shows the mean and variance of the gains from modification to investors relative to not modifying the mortgage that arise from the continued payment of monthly interest and principal. The bar graphs represent the means of normalized estimated gains which are measured at the loan level. The lines represent 95% confidence intervals based on the conditional standard deviation of the loan level estimates of gains from modification. The bars denote various components of $\Delta PV(PMTs)$ as depicted in the formula above the chart. The estimates are based on my analysis on data on 30 Year Fixed Rate mortgages from ABSNet Loan, and the publicly available Fannie Mae and Freddie Mac data.

$$\underbrace{\underbrace{\frac{(1+R_1)^{T_{NoMod}}}{(1+R_1)^{T_{Mod}}}}_{1}\underbrace{\left(\frac{G(P_1, D_{T_{Mod}})}{(1+R_1)^{T_{NoMod}}}\right)}_{2}}_{3} - \underbrace{\frac{\phi P_1}{(1+R_1)^{T_{NoMod}}}}_{4} = \underbrace{\Delta PV(\text{Termination})}_{5}$$

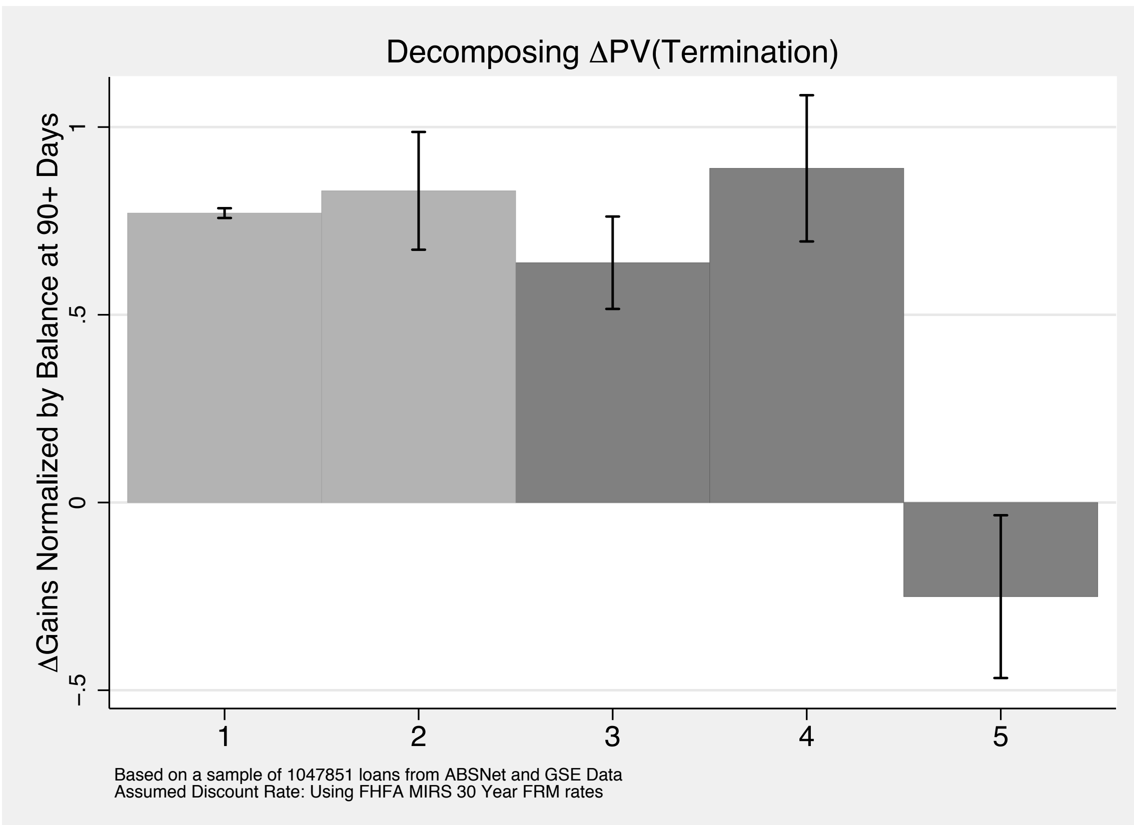

Figure A.3: Decomposing the gains from termination

This graph shows the mean and variance of the gains from modification to investors relative to not modifying the mortgage that arise from the continued payment of monthly interest and principal. The bar graphs represent the means of normalized estimated gains which are measured at the loan level. The lines represent 95% confidence intervals based on the conditional standard deviation of the loan level estimates of gains from modification. The bars denote various components of $\Delta PV(Termination)$ as depicted in the formula above the chart. The estimates are based on my analysis on data on 30 Year Fixed Rate mortgages from ABSNet Loan, and the publicly available Fannie Mae and Freddie Mac data.

Table A.2: Robustness to Heterogenous Treatment Effects

	Ex-Ante Credit Score			
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Incremental Cash Flows From:				
Interest from PMTs	0.2233	0.2047	0.1820	0.1644
	(0.0759)	(0.0657)	(0.0570)	(0.0488)
Principal from PMTs	0.0813	0.0866	0.0858	0.0830
	(0.0201)	(0.0188)	(0.0179)	(0.0167)
Principal at Termination	-0.2922	-0.2602	-0.2386	-0.2187
	(0.1145)	(0.1095)	(0.1094)	(0.1091)
Gains to Investor	0.0152	0.0328	0.0305	0.0306
	(0.1369)	(0.1257)	(0.1206)	(0.1165)
	Change in House Prices between Orig. and 90+			
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Incremental Cash Flows From:				
Interest from PMTs	0.1450	0.1697	0.2148	0.2205
	(0.0493)	(0.0560)	(0.0645)	(0.0689)
Principal from PMTs	0.0715	0.0729	0.0806	0.0994
	(0.0167)	(0.0174)	(0.0183)	(0.0189)
Principal at Termination	-0.1378	-0.1903	-0.2596	-0.4147
	(0.0573)	(0.0572)	(0.0488)	(0.0921)
Gains to Investor	0.0824	0.0529	0.0380	-0.0891
	(0.0738)	(0.0803)	(0.0807)	(0.1153)
	Origination Loan Amount			
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Incremental Cash Flows From:				
Interest from PMTs	0.0953	0.1898	0.1801	0.1704
	(0.0313)	(0.0597)	(0.0601)	(0.0603)
Principal from PMTs	0.0382	0.0866	0.0857	0.0844
	(0.0115)	(0.0182)	(0.0182)	(0.0183)
Principal at Termination	-0.1635	-0.2496	-0.2313	-0.2181
	(0.1096)	(0.1076)	(0.1044)	(0.1014)
Gains to Investor	-0.0298	0.0308	0.0365	0.0378
	(0.1173)	(0.1218)	(0.1174)	(0.1144)

Appendix B

Appendix to Chapter 2

B.1 Framework Solutions

B.1.1 Lemma 1

To reiterate, the contracting problem will be given by:

$$\max_{\{U_H,\phi_H\},\{U_L,\phi_L\}} p\left(Z(\phi_H, V_H) + (1-p)\left(Z(\phi_L, V_L)\right)\right.$$
$$-pU_H - (1-p)U_L - pV_H C(\phi_H) - (1-p)V_L C(\phi_L)$$

subject to:

$$U_H \geq U_L - (V_H - V_L)C(\phi_L)\ (ICH)$$
$$U_L \geq U_H + (V_H - V_L)C(\phi_H)\ (ICL)$$
$$U_H \geq 0\ (PCH)$$
$$U_L \geq 0\ (PCL)$$

Lemma 1 is used to solve the optimal contracting problem.

Lemma 1. ***(a)*** *ICL and PCH imply PCL* ***(b)*** $\phi_L^* \geq \phi_H^*$ ***(c)*** *ICL binds* ***(d)*** *ICL binding and (b) implies ICH holds* ***(e)*** *PCH binds.*

Proof:
(a) is self-explanatory (b) is obtained by adding together the ICH and ICL constraints (c) by contradiction. Suppose ICL does not bind, then can reduce U_L without violating any constraints and improve the objective function, hence it cannot be optimal (d) is self-explanatory (e) by contradiction and using the fact that we can ignore ICH by part (d).

The lemma allows us to ignore ICH and PCL and shows that ICL and PCH will indeed bind in equilibrium. Thus we can substitute ICL and PCH into the objective function and take first order conditions with respect to ϕ_H and ϕ_L.

B.1.2 Insufficient Incentives (Non-Optimal Contracting)

As an extreme case, consider the provision of a contract that provides a fixed payment $\bar{t}$ no matter the state of the world. Maintaining the assumption that ex-post participation constraints need to hold, this implies a single rate of loan modification ($\bar{\phi}$) in both states of the world. Thus, the IC constraints will trivially hold, and the PC will bind in the high state of the world, and will be slack in the low state of the world (since $V_H > V_L$). . The FOC that arises from the optimization problem is rather intuitive and straightforward:

$$C'(\bar{\phi}) = \frac{(1-\alpha_1)}{V_H}\left[p\left(m^* + \lambda - V_H\right) + (1-p)\left(m^* + \lambda - V_L\right)\right]$$

Expressing it in terms of the first best levels of loan modification ϕ_H and ϕ_L may provide more insight:

$$C'(\bar{\phi}) = pC'(\phi_H^{FB}) + (1-p)\frac{V_L}{V_H}C'(\phi_L^{FB})$$

The first implication that arises from this first order condition is that $\phi_L^{FB} > \bar{\phi} > \phi_H^{FB}$ for $p < 1$. Thus, with such a contract, we see under-modification relative to the first best in the low state of the world. We still see over-modification relative to the first best in the high state of the world. For higher values of p, $\bar{\phi}$ moves towards the first best ϕ_H^{FB} but away from ϕ_L^{FB}. Additionally, consider a more dispersed distribution of the private information available to the Servicer (i.e larger ratio $\frac{V_H}{V_L}$). As this ratio increases, it reduces the weight on $C'(\phi_L^{FB})$ in the expression above and lowers the implemented level of modifications away from ϕ_L^{FB}.

Another comparison to perform is to think about the payments to the Servicer with the incentive contract from section 2.2 and the low incentive contract outlined here. We see that $U_L^* > U_L^R$ when the costs $C(\phi_H^*) > C(\bar{\phi})$. Thus, which contract is preferred will depend on whether $\phi_H^* \lesseqgtr \bar{\phi}$ and the specification of the cost function.

Thus, if the parties contracted under the incorrect assumption that p was high enough, or that V_H was high enough, this would mean that such a contract with "insufficient incentives" would come closer to implementing the first best level of loan modifications in the state believed to be the most likely. However, once again, this conclusion hinges crucially on assumptions made about the cost function $C(\phi)$.

B.2 Appendix Tables for Chapter 2

Table B.1: Robustness Check: Effect of Multiplicity on Modification by Subsamples (Modify)

The table below shows the results of a linear probability model estimation. The sample of mortgages used are those that were originated between and including 2002 and 2007, and those which went delinquent before January 2009. Loan and Borrower Level Controls are as at the origination of the mortgage. The dependent variable in each of the columns is an indicator for whether the mortgage in the loan pool was modified. House Price Change is calculated as the three month change in house prices at the county level (using Zillow data) prior to the incidence of early delinquency. Columns 1,2,4,7 and 8 control for CBSA by Quarter of Delinquency Fixed Effects and Deal Fixed Effects. Column 3, 6 and 9 additionally control for Deal by Loan Level Servicer Fixed Effects. Standard errors are clustered at the deal level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Doc	Full Doc	Below CLL	Below CLL	30 Yr FRM	30 Yr FRM	Non-Complex	Non-Complex
VARIABLES	P(Modify)	P(Modify)	P(Modify)	P(Modify)	P(Modify)	P(Modify)	P(Modify)	P(Modify)
Tranche Count	-0.0260***		-0.0135***		-0.0111***		-0.0259***	
	(0.0042)		(0.0036)		(0.0032)		(0.0033)	
Multiplicity (HHI)		0.0486***		0.0446***		0.0291***		0.0490***
		(0.0065)		(0.0060)		(0.0091)		(0.0062)
House Price Change	-0.0935***	-0.0938***	-0.0220	-0.0269	-0.1471***	-0.1472***	-0.0213	-0.0217
	(0.0257)	(0.0258)	(0.0202)	(0.0203)	(0.0539)	(0.0539)	(0.0230)	(0.0230)
Observations	1,249,467	1,249,467	1,848,317	1,849,013	306,425	306,425	1,579,797	1,579,797
R-squared	0.1859	0.1859	0.2052	0.1803	0.1629	0.1629	0.1850	0.1850
CBSA x Quarter FE	Y	Y	Y	Y	Y	Y	Y	Y
Deal FE	Y	Y	Y	Y	Y	Y	Y	Y
Additional Controls	Y	Y	Yes	Yes	Y	Y	Y	Y
Cluster	Deal	Deal	Deal	Deal	Deal	Deal	Deal	Deal
Mean of Dep Var	0.238	0.238	0.218	0.218	0.252	0.252	0.232	0.232

Table B.2: Robustness Check: Effect of Multiplicity on Default by Subsamples (Foreclosure)

The table below shows the results of a linear probability model estimation. The sample of mortgages used are those that were originated between and including 2002 and 2007, and those which went delinquent before January 2009. Loan and Borrower Level Controls are as at the origination of the mortgage. The dependent variable in each of the columns is an indicator for whether the mortgage in the loan pool was modified. House Price Change is calculated as the three month change in house prices at the county level (using Zillow data) prior to the incidence of early delinquency. Columns 1,2,4,7 and 8 control for CBSA by Quarter of Delinquency Fixed Effects and Deal Fixed Effects. Column 3, 6 and 9 additionally control for Deal by Loan Level Servicer Fixed Effects. Standard errors are clustered at the deal level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Doc	Full Doc	Below CLL	Below CLL	30 Yr FRM	30 Yr FRM	Non-Complex	Non-Complex
VARIABLES	P(Default)	P(Default)	P(Default)	P(Default)	P(Default)	P(Default)	P(Default)	P(Default)
Tranche Count	0.0400***		0.0205***		0.0111***		0.0383***	
	(0.0046)		(0.0035)		(0.0039)		(0.0034)	
Multiplicity (HHI)		-0.0794***		-0.0558***		-0.0549***		-0.0779***
		(0.0065)		(0.0057)		(0.0103)		(0.0066)
House Price Change	-0.4986***	-0.4982***	-0.5624***	-0.5625***	-0.5112***	-0.5111***	-0.6000***	-0.5995***
	(0.0310)	(0.0310)	(0.0249)	(0.0249)	(0.0601)	(0.0601)	(0.0275)	(0.0275)
Observations	1,249,467	1,249,467	1,849,013	1,849,013	306,425	306,425	1,579,797	1,579,797
R-squared	0.2036	0.2037	0.2153	0.2154	0.2144	0.2145	0.2012	0.2013
CBSA x Quarter FE	Y	Y	Y	Y	Y	Y	Y	Y
Deal FE	Y	Y	Y	Y	Y	Y	Y	Y
Additional Controls	Y	Y	Yes	Yes	Y	Y	Y	Y
Cluster	Deal	Deal	Deal	Deal	Deal	Deal	Deal	Deal
Mean of Dep Var	0.535	0.535	0.574	0.574	0.487	0.487	0.528	0.528

Table B.3: Robustness Checks: Controlling for Unpredicted Entry into Delinquency

The table below shows the results of a linear probability model estimation. The sample of mortgages used are those that were originated between and including 2002 and 2007, and those which went delinquent before January 2009. Loan and Borrower Level Controls are as at the origination of the mortgage. The dependent variable in each of the columns is an indicator for whether the mortgage in the loan pool was modified. *** p<0.01, ** p<0.05, * p<0.1

	(1)	(2)
VARIABLES	P(Modify)	P(Modify)
Panel B: Using Unpredicted 90+ Days Delinquency		
Tranche Count	-0.0093***	
	(0.0034)	
Multiplicity (HHI)		0.0361***
		(0.0051)
Default Residuals	-0.0040	-0.0042
	(0.0299)	(0.0299)
(Default Residuals)2	0.1292**	0.1289*
	(0.0658)	(0.0658)
(Default Residuals)3	-0.0615	-0.0606
	(0.0412)	(0.0412)
House Price Change	-0.0585***	-0.0584***
	(0.0188)	(0.0188)
Observations	2,098,526	2,098,526
R-squared	0.2027	0.2028
CBSA x Quarter FE	Y	Y
Deal by Servicer FE	Y	Y
Additional Controls	Y	Y
Cluster	Deal	Deal
Mean of Dep Var	0.214	0.214

Table B.4: Multiplicity of Tranches and the Probability of Foreclosure

The table below shows the results of a linear probability model estimation. The sample of mortgages used are those that were originated between and including 2002 and 2007, and those which went delinquent before January 2009. Loan and Borrower Level Controls are as at the origination of the mortgage. The dependent variable in each of the columns is an indicator for whether the mortgage in the loan pool entered foreclosure. House Price Change is calculated as the three month change in house prices at the county level (using Zillow data) prior to the incidence of early delinquency. Columns 1,2,4 and 5 control for CBSA by Quarter of Delinquency Fixed Effects and Deal Fixed Effects. Column 3 and 6 additionally control for Deal by Loan Level Servicer Fixed Effects. Standard errors are clustered at the deal level. *** p<0.01, ** p<0.05, * p<0.1

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	P(Default)	P(Default)	P(Default)	P(Default)	P(Default)	P(Default)
Tranche Count	0.0228***	0.0212***	0.0216***			
	(0.0028)	(0.0031)	(0.0033)			
Multiplicity (HHI)				-0.0623***	-0.0640***	-0.0619***
				(0.0050)	(0.0054)	(0.0054)
House Price Change		-0.5423***	-0.5428***		-0.5424***	-0.5427***
		(0.0228)	(0.0226)		(0.0228)	(0.0226)
Observations	2,682,632	2,225,480	2,224,566	2,682,632	2,225,480	2,224,566
R-squared	0.2075	0.2158	0.2279	0.2076	0.2159	0.2280
CBSA x Quarter FE	Y	Y	Y	Y	Y	Y
Deal FE	Y	Y	N	Y	Y	N
Deal by Servicer FE	N	N	Y	N	N	Y
Additional Controls	Y	Y	Y	Y	Y	Y
Cluster	Deal	Deal	Deal	Deal	Deal	Deal
Mean of Dep Var	0.572	0.586	0.586	0.572	0.586	0.586

Table B.5: Multiplicity of Tranches and Hazard Rate of Foreclosure

The table below shows the results of a continuous time proportional hazard model estimation based on Palmer (2014). The estimation sample is a random 15% sample of mortgages that were originated between and including 2002 and 2007, which went delinquent before January 2009. Failure is defined as a loan entering into a foreclosure. A loan is considered to be censored either if it "self-cures" without any action by the Servicer, or if it is modified. Once a loan receives a modification it leaves the sample and we do not follow the subsequent history. In addition to controlling for zip code level 3 month house price changes, I control for the standard set of loan and borrower level characteristics used in the previous regressions. Standard errors are clustered at the CBSA level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cohort of Delinquency 2003	-0.016	0.024	-0.406***	-0.305***	-0.013	0.024	-0.405***	-0.304***
	(0.067)	(0.066)	(0.073)	(0.078)	(0.065)	(0.065)	(0.073)	(0.077)
Cohort of Delinquency 2004	-0.211***	-0.164***	-0.549***	-0.365***	-0.232***	-0.187***	-0.547***	-0.364***
	(0.059)	(0.060)	(0.077)	(0.086)	(0.059)	(0.060)	(0.077)	(0.086)
Cohort of Delinquency 2005	-0.118*	-0.051	-0.639***	-0.364***	-0.137**	-0.070	-0.637***	-0.363***
	(0.062)	(0.065)	(0.085)	(0.096)	(0.062)	(0.065)	(0.084)	(0.096)
Cohort of Delinquency 2006	0.134*	0.239***	-0.635***	-0.294***	0.075	0.177**	-0.633***	-0.293***
	(0.074)	(0.073)	(0.095)	(0.109)	(0.076)	(0.075)	(0.095)	(0.109)
Cohort of Delinquency 2007	0.433***	0.577***	-0.352***	-0.075	0.368***	0.509***	-0.351***	-0.073
	(0.097)	(0.090)	(0.097)	(0.117)	(0.101)	(0.094)	(0.096)	(0.116)
Cohort of Delinquency 2008	0.381***	0.556***	-0.400***	-0.245**	0.314***	0.486***	-0.399***	-0.244**
	(0.093)	(0.084)	(0.092)	(0.121)	(0.096)	(0.087)	(0.091)	(0.121)
House Price Index Change	-6.095***	-4.391***	-5.452***	-3.819***	-6.042***	-4.368***	-5.441***	-3.815***
	(0.707)	(0.510)	(0.689)	(0.556)	(0.707)	(0.512)	(0.686)	(0.556)
Multiplicity (HHI)	-0.083	-0.122*	-0.290***	-0.149***				
	(0.071)	(0.065)	(0.073)	(0.047)				
Tranche Count					0.100***	0.112***	0.202***	0.112***
					(0.010)	(0.009)	(0.038)	(0.026)
Observations	7,044,507	7,044,507	7,044,507	6,912,502	7,044,507	7,044,507	7,044,507	6,912,502
CBSA FE	N	Y	N	N	N	Y	N	N
Deal FE	N	N	Y	Y	N	N	Y	Y
Loan Chars	N	N	N	Y	N	N	N	Y
Borrower Chars	N	N	N	Y	N	N	N	Y
Cluster	CBSA	CBSA	CBSA	CBSA	CBSA	CBSA	CBSA	CBSA
Log likelihood	-335831	-333756	-326616	-317657	-335699	-333597	-326594	-317649

Table B.6: Robustness to House Price Rebounds (Modification results)

The table below shows the results of a linear probability model estimation. The sample of mortgages used are those that were originated between and including 2002 and 2007), and those which went delinquent before January 2009. Group 1 is an indicator variable for whether a zip code saw no house price rebound between 2009 and 2012. Group 2 is an indicator variable for whether a zip code saw a low house price rebound between 2009 and 2012. Loan and Borrower Level Controls are as at the origination of the mortgage. The dependent variable in each of the columns is an indicator for whether the mortgage in the loan pool was modified. Column 1 shows the baseline specification for the sample with zip code level price indices available from Zillow. Columns 2 to 4 provide the results for each of the subsamples. Column 5 combines the samples into a single regression. Standard errors are clustered at the deal level. *** p<0.01, ** p<0.05, * p<0.1

	(1)	(2)	(3)	(4)	(5)
	P(Modify)	P(Modify)	P(Modify)	P(Modify)	P(Modify)
VARIABLES	Baseline	Group1	Group2	Group3	Combined
Multiplicity (HHI)	0.0466***	0.0531***	0.0439***	0.0204**	0.0554***
	(0.0067)	(0.0074)	(0.0106)	(0.0095)	(0.0079)
Multiplicity (HHI) X Group 1					-0.0138**
					(0.0057)
Multiplicity (HHI) X Group 2					-0.0017
					(0.0065)
Group 1					0.0246***
					(0.0025)
Group 2					0.0116***
					(0.0026)
House Price Change	-0.0784***	-0.1509***	-0.1096*	-0.0496	-0.0967***
	(0.0258)	(0.0370)	(0.0640)	(0.0411)	(0.0258)
Observations	1,031,655	615,295	173,083	242,623	1,031,655
R-squared	0.1912	0.1987	0.2008	0.1929	0.1914
CBSA x Quarter FE	Y	Y	Y	Y	Y
Deal FE	Y	Y	Y	Y	Y
Additional Controls	Y	Y	Y	Y	Y
Cluster	Deal	Deal	Deal	Deal	Deal
Mean of Dep Var	0.275	0.301	0.253	0.226	0.275

Table B.7: Robustness to House Price Rebounds (Foreclosure results)

The table below shows the results of a linear probability model estimation. The sample of mortgages used are those that were originated between and including 2002 and 2007), and those which went delinquent before January 2009. Group 1 is an indicator variable for whether a zip code saw no house price rebound between 2009 and 2012. Group 2 is an indicator variable for whether a zip code saw a low house price rebound between 2009 and 2012. Loan and Borrower Level Controls are as at the origination of the mortgage. The dependent variable in each of the columns is an indicator for whether the mortgage in the loan pool was modified. Column 1 shows the baseline specification for the sample with zip code level price indices available from Zillow. Columns 2 to 4 provide the results for each of the subsamples. Column 5 combines the samples into a single regression. Standard errors are clustered at the deal level. *** p<0.01, ** p<0.05, * p<0.1

	(1)	(2)	(3)	(4)	(5)
	P(Default)	P(Default)	P(Default)	P(Default)	P(Default)
VARIABLES	Baseline	Group1	Group2	Group3	Combined
Multiplicity (HHI)	-0.0449***	-0.0495***	-0.0474***	-0.0222**	-0.0533***
	(0.0063)	(0.0071)	(0.0111)	(0.0095)	(0.0076)
Multiplicity (HHI) X Group 1					0.0138**
					(0.0062)
Multiplicity (HHI) X Group 2					-0.0005
					(0.0071)
Group 1					-0.0172***
					(0.0026)
Group 2					-0.0086***
					(0.0028)
House Price Change	-0.1871***	-0.0608	-0.0671	-0.2878***	-0.1756***
	(0.0285)	(0.0401)	(0.0675)	(0.0459)	(0.0284)
Observations	1,031,655	615,295	173,083	242,623	1,031,655
R-squared	0.1956	0.1875	0.2187	0.2260	0.1957
CBSA x Quarter FE	Y	Y	Y	Y	Y
Deal FE	Y	Y	Y	Y	Y
Additional Controls	Y	Y	Y	Y	Y
Cluster	Deal	Deal	Deal	Deal	Deal
Mean of Dep Var	0.657	0.624	0.684	0.719	0.657

Appendix C

Appendix to Chapter 3

C.1 Appendix Graphs and Tables for Chapter 3

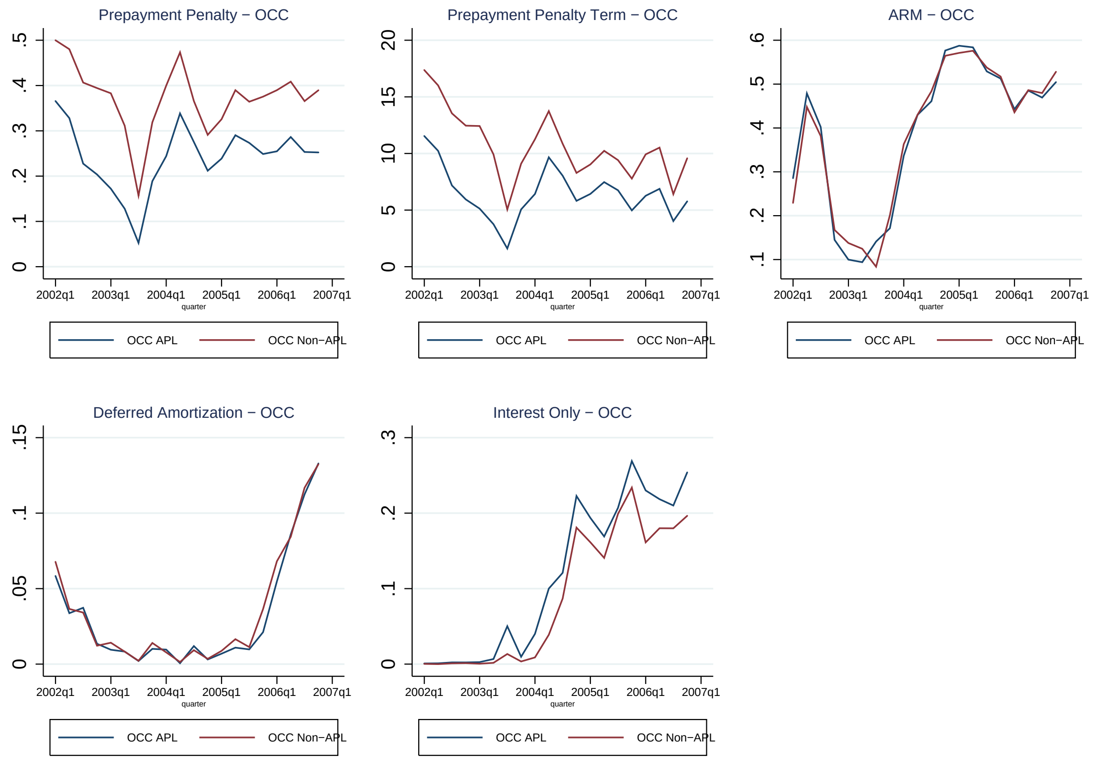

Figure C.1: Loan terms for OCC lenders

This figure plots the fraction of OCC loans with different contract features for APL (the blue line) and non-APL states (the red line) over our sample period 2002-2007.

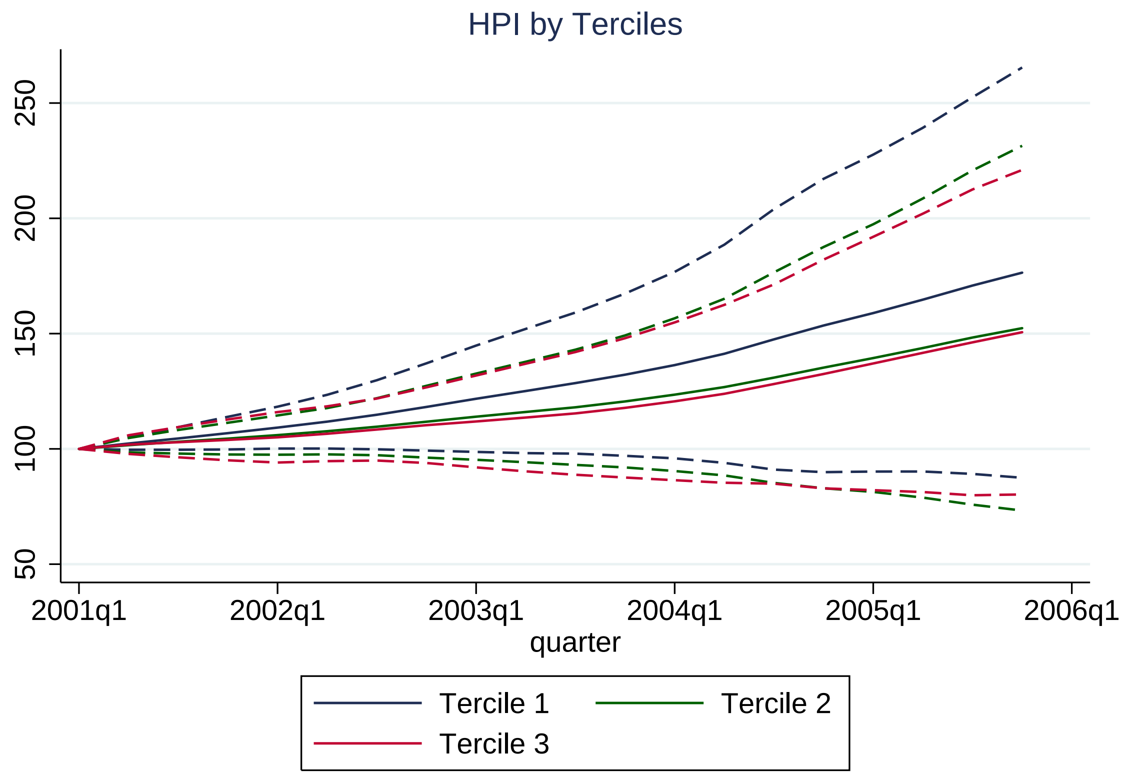

Figure C.2: House Price Indices by Competition Measure Terciles
This Figure plots a house price index from Zillow for each county in the sample between 2002 and 2005 for different level of OCC lenders' market share. Specifically, the index is normalized to be equal to 100 in the first quarter of 2001 and for each tercile of the OCC Share measure, we compute the population weighted average index value across counties in that group. We then plot these aggregated indices with 95% confidence intervals.

Table C.1: OCC in APL vs non-APL States (Bordering Counties)

The table below reports coefficient estimates of regressions relating the pre-emption of state anti-predatory lending laws for national banks and features of mortgages originated. The sample contains loans originated by OCC regulated lenders in counties that lie on state borders. The dependent variables are as follows: Column 1: an indicator variable for whether the loan has a prepayment penalty; Column 2: length of the prepayment penalty term, with 0 if there is no prepayment penalty; Column 3: indicator variable for whether a loan has an ARM feature; Column 4: indicator variable for whether a loan has either negative amortization or a balloon feature. Column 5: Indicator variable for whether a mortgage had an interest only feature. APL is a time varying indicator variable for whether the state in which the loan was originated had an APL law in place at time of origination. Post is an indicator variable equal to 1 for months after January 2004. All columns include the following controls: the LTV ratio at origination, the Log of appraised value at origination, the borrower's FICO score, an indicator for the presence of second liens, a low or no documentation indicator, an indicator for loan purpose (i.e. cash out refinance, rate refinance or other), and an indicator for the presence of PMI. The columns also include as controls the fraction of subprime borrowers and the county median income interacted with Post and the change in house price between origination and two years prior to origination. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)	(4)	(5)
	Prepay Penalties	Term Length	ARM	Def Amort	IO
$Post_t$ x APL_{gt}	0.041**	1.987***	-0.000	0.013***	0.048*
	(0.018)	(0.619)	(0.013)	(0.004)	(0.025)
APL_{gt}	-0.158***	-4.693***	-0.017	-0.009**	-0.092***
	(0.027)	(0.792)	(0.010)	(0.005)	(0.016)
$Post_t$ x Subprime $Borrowers_c$	0.023	0.128	0.080	0.059***	0.252*
	(0.113)	(3.160)	(0.067)	(0.020)	(0.142)
$Post_t$ x Median $Income_c$	-0.125**	-2.597*	0.082***	0.031***	0.251***
	(0.050)	(1.481)	(0.025)	(0.009)	(0.049)
House Price $Change_{ict}$	0.057*	1.494*	0.009	-0.032	-0.035
	(0.030)	(0.842)	(0.033)	(0.023)	(0.034)
Observations	177,861	171,139	177,861	177,861	177,861
R-squared	0.267	0.243	0.269	0.070	0.226
County Pair FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
County X Post Controls	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Mean of Dep Var	0.306	7.930	0.468	0.0440	0.155

Table C.2: OCC in APL vs non-APL States (with Originator by Quarter FE)

The table below reports coefficient estimates of regressions relating the pre-emption of state anti-predatory lending laws for national banks and features of mortgages originated, controlling for originator times quarter fixed effects. The sample contains loans originated by OCC regulated lenders. The dependent variables are as follows: Column 1: an indicator variable for whether the loan has a prepayment penalty; Column 2: length of the prepayment penalty term, with 0 if there is no prepayment penalty; Column 3: indicator variable for whether a loan has an ARM feature; Column 4: indicator variable for whether a loan has either negative amortization or a balloon feature. Column 5: Indicator variable for whether a mortgage had an interest only feature. APL is a time varying indicator variable for whether the state in which the loan was originated had an APL law in place at time of origination. Post is an indicator variable equal to 1 for months after January 2004. All columns include the following controls: the LTV ratio at origination, the Log of appraised value at origination, the borrower's FICO score, an indicator for the presence of second liens, a low or no documentation indicator, an indicator for loan purpose (i.e. cash out refinance, rate refinance or other), and an indicator for the presence of PMI. The columns also include as controls the fraction of subprime borrowers and the county median income interacted with Post and the change in house price between origination and two years prior to origination. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)	(4)	(5)
	Prepayment Penalty	Prepayment Term	ARM	Deferred Amort	IO
$Post_t$ x APL_{gt}	0.012	1.037***	-0.002	0.001	0.048***
	(0.008)	(0.222)	(0.006)	(0.002)	(0.012)
$Post_t$	0.260	4.342	-0.445***	-0.012	-0.551
	(0.220)	(5.215)	(0.152)	(0.033)	(0.401)
APL_{gt}	-0.108***	-3.023***	-0.008	-0.005	-0.055***
	(0.014)	(0.340)	(0.007)	(0.004)	(0.011)
$Post_t$ x Subprime $Borrowers_c$	0.102**	1.792	0.017	-0.021*	-0.258***
	(0.051)	(1.408)	(0.034)	(0.012)	(0.080)
$Post_t$ x Median $Income_c$	-0.036*	-0.731	0.037***	0.000	0.060*
	(0.019)	(0.458)	(0.013)	(0.003)	(0.036)
House Price $Change_{ict}$	0.013	0.531	-0.020	-0.010	-0.007
	(0.020)	(0.595)	(0.026)	(0.011)	(0.035)
Observations	529,235	508,488	529,235	529,235	529,235
R-squared	0.438	0.422	0.449	0.156	0.443
County FE	Yes	Yes	Yes	Yes	Yes
Originator by Quarter FE	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Mean of Dep Var	0.290	7.455	0.457	0.0377	0.174

Table C.3: Effect of Pre-Emption Ruling on Borrower Characteristics at Origination

The table below reports coefficient estimates of regressions relating the pre-emption of state anti-predatory lending laws for national banks and borrowers' characteristics. The sample contains loans originated by OCC lenders in Panel A and loans originated by non-OCC lenders in Panel B. Each column in a panel shows the coefficients on the interaction term Post x APL from a series of regressions, each regression having a different borrower characteristic as a dependent variable. APL is a time varying indicator variable for whether the state in which the loan was originated had an APL law in place at time of origination. Post is an indicator variable equal to 1 for months after January 2004. The dependent variables in these regressions are Credit Score, CLTV, Indicator for whether a Second Lien was present, and indicator for whether the loan was a cash out refinance. Each column corresponds to a sub-sample of OCC or non-OCC loans depending on whether the mortgage did or did not have prepayment penalties (Panel A) or whether the mortgage did or did not have complex features (Panel B). The columns include as controls the fraction of subprime borrowers and the county median income interacted with Post, the change in house price between origination and two years prior to origination, County Fixed Effects and Month of Origination FE. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Coefficients on Post x APL in Difference and Difference Regression

	Panel A: OCC Lenders	
	(1)	(2)
	With Prepayment Penalty	*Without Prepayment Penalty*
Credit Score	0.932	-0.667
	(1.531)	(2.187)
CLTV	0.002	0.023***
	(0.004)	(0.005)
Second Lien	0.020***	-0.009
	(0.006)	(0.007)
Cash Out Refinance	0.022	0.050***
	(0.015)	(0.014)
N	162,338	405,109
County FE	Yes	Yes
Month FE	Yes	Yes
County Controls X Post	Yes	Yes
House Price Change	Yes	Yes

		Panel B: Non OCC Lenders	
	(1)	(2)	(3)
	ARMs	*Complex*	*No ARMs/No Complex*
Credit Score	1.042	-9.115***	-1.359
	(1.459)	(1.846)	(0.868)
CLTV	-0.001	-0.010**	-0.008
	(0.003)	(0.004)	(0.009)
Second Lien	-0.002	-0.076***	-0.021**
	(0.006)	(0.014)	(0.009)
Cash Out Refinance	0.003	0.085***	-0.001
	(0.009)	(0.016)	(0.017)
N	3,801,978	1,848,707	1,528,614
County FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
County Controls X Post	Yes	Yes	Yes
House Price Change	Yes	Yes	Yes