The Effect of Mortgage Securitization on Foreclosure and Modification

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June 2013

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Abstract

Does securitization affect the foreclosure and modification decisions of mortgage servicers? I exploit the third quarter 2007 freeze of private mortgage securitization as an exogenous change in probability of securitization for jumbo mortgages. The unanticipated freeze left banks holding mortgages that were intended for securitization when they were originated. Loans made shortly before the freeze are similar to loans made earlier in the year but were significantly less likely to be securitized. Using origination-month as an instrument for jumbo securitization, I estimate that before the Home Affordable Modification Program (HAMP) private securitization increased foreclosure probability (by 8.7 ppt for foreclosure initiation and 5.2 ppt for foreclosure completion) and decreased modification probability (by 2.3 ppt). After implementation of HAMP, privately securitized loans are no longer biased against modification, but the foreclosure bias persists.

JEL classification:

1 Introduction

Mortgage securitization divorces mortgage ownership from mortgage servicing, potentially affecting foreclosure and modification decisions. Many policy makers and economists worry that securitization impedes mortgage modification and leads to unnecessary foreclosures.[1] Empirical assessment of this issue is challenging because securitization is an endogenous decision, and securitized loans are likely lower quality than portfolio loans even after controlling for observable characteristics.[2] Previous studies fail to fully account for this endogeneity and disagree about whether securitization biases foreclosure and modification decisions.[3]

I instrument for securitization by exploiting a sudden and unexpected freeze in private mortgage securitization in the third quarter of 2007. As a result of the freeze, jumbo mortgages made shortly before the market freeze were disproportionately stuck on bank balance sheets even though many of them were intended for private securitization. Because the market freeze was unanticipated, loans made shortly before the freeze are otherwise similar to loans made earlier in 2007. I further control for changes in the lending environment over time using a difference-in-differences methodology with high quality non-jumbo loans, which are primarily securitized by Fannie Mae and Freddie Mac (the GSEs) and were thus unaffected by the private securitization freeze.

The results are striking. Prior to government intervention, private securitization increased the probability of foreclosure initiation by 8.7 ppt (12% on a relative basis) during the six months following first serious (60+ day) delinquency relative to portfolio loans. Similarly, securitization increased the probability of foreclosure completion by 5.2 ppt (39% on a relative basis) and decreased the probability of modification by 2.3 ppt (39% on a relative basis).[4] My instrumental variables strategy is critical for estimating these coefficients. For foreclosure initiation and completion, IV estimates are twice as large as corresponding OLS estimates, suggesting that previous studies

[1] Posner and Zingales (2009) were early proponents of the view, arguing that securitization is partially responsible for the high level of foreclosure witnessed over the past few years. Frictions in modifying securitized loans were part of the rationale for the Home Affordable Modification Program (HAMP).

[2] For example, Keys, et al. (2010) argue that moral hazard leads to looser screening standards for securitized loans.

[3] Piskorski, Seru, and Vig (2010) estimate that delinquent privately securitized loans are foreclosed 3-7% more frequently than portfolio loans and Agarwal, et al. (2011) estimate that delinquent privately securitized loans are modified 4-6% less frequently than portfolio loans, but Adelino, Gerardi, and Willen (2011a) estimate that privately securitized loans and portfolio loans are modified with equal frequency.

[4] My identification strategy is specific to jumbo loans originated between January and August of 2007. Results represent the average treatment effect of private securitization relative to portfolio ownership for this population.

significantly underestimated the impact of securitization on foreclosure.

Understanding the impact of securitization on mortgage servicing is important for at least three reasons. First, it informs policy responses to the current foreclosure crisis. Second, it is an important part of the debate about the costs and benefits of securitization. Third, it illustrates more general corporate finance theory regarding separation of ownership and control and incomplete contracts.

The magnitude of the foreclosure crisis is well known. Since the start of the financial crisis, 4.4 million U.S. homes have been foreclosed, causing turmoil in the lives of mortgagees and potentially damaging surrounding communities (cf. Campbell, Giglio, and Pathak, 2011).[5] Half of foreclosure originations stem from privately securitized mortgages.[6] Given my estimates that securitization increased foreclosure initiation by 12% and foreclosure completion by 39%, securitization significantly exacerbated the foreclosure crisis and needs to be considered in any policy response.

Securitized mortgage servicing is part of a debate about securitization more generally. The tradeoffs of securitized financing include liquidity creation, increased availability of financing, decreased lending standards, and securitization's role in the financial crisis.[7] Securitization's impact on how assets are managed has received less attention but is also important, especially where management practices have externalities, as they likely do in the case of mortgage servicing.[8]

Biased servicing of securitized loans also illustrates one of the central precepts of corporate finance: separation of ownership and control matters. The importance of managerial incentives is theoretically well-established and universally accepted as a basic premise.[9] Yet, empirical applications remain controversial. Are managers of public companies overpaid? Do compensation and governance provisions affect firm performance? Are private firms managed better than public firms?

These questions are unsettled because empirical identification is often difficult if not impossi-

[5] Foreclosure data is from CoreLogic National Foreclosure Report, April 2013.

[6] The estimate that half of foreclosure initiations are privately securitized mortgages comes from Piskorski, Seru, and Vig (2011) and Mayer (2009) based on Federal Reserve reports and private market data.

[7] Gorton and Metrick (2011) address liquidity creation and the financial crisis. Loutskina (2011), Loutskina and Strahan (2009), and Mian and Sufi (2009) address financing availability. Keys, et al. (2010) and Rajan, Seru, and Vig (2012) address loan quality.

[8] Gan and Mayer (2007) address servicing of securitized commercial mortgages and conclude that when a special servicer owns a first-loss position it services delinquent loans more efficiently.

[9] The idea that incentives matter is as old as economics itself. Modern applications to managerial incentives date to at least Jensen and Meckling (1976).

ble. In particular, comparing firms with different financial structures (e.g., public and private firms) introduces omitted variable bias because financial structure is an endogenous decision. Bernstein (2012) offers a rare example of a satisfying instrument for public versus private ownership: NASDAQ returns shortly after an IPO announcement are uncorrelated with firm prospects but predict whether the IPO will be completed. My identification strategy is analogous. Mortgage ownership is endogenous (thereby compromising previous empirical investigations) but is influenced by effectively random changes to securitization market conditions after mortgage origination. Thus, my setting offers a rare laboratory for well-identified comparison of direct asset control to delegated management.

Similarly, mortgage securitization is a good example of incomplete contracts. The incomplete contracts theory of Grossman and Hart (1986) and Hart and Moore (1990) is well-established, but empirical research with actual contract details is rare. Mortgage securitization is a good setting for illustrating incomplete contracts because the relationship between the parties is clear (mortgage trusts passively own the mortgages, and servicers manage them) and the contracts are publicly disclosed.

The institutional details of mortgage servicing (described in Section 5) suggest that current loans and active foreclosures are mechanical to service whereas loss mitigation (including modification) for delinquent loans involves significant discretion. In the language of Grossman and Hart (1986), loss mitigation decisions represent uncontractible residual rights. These residual rights are universally held by the mortgage servicer, effectively making the servicer the "owner" of the mortgages even though the trust holds the legal titles most cash flow rights.[10] The disconnect between control and marginal cashflows creates two problems. First, servicers have an incentive to underinvest in loss mitigation. Second, when servicers do pursue loss mitigation, they may employ practices that enhance servicing income at the expense of principal and interest payments to the trust. This is essentially a multitasking problem, akin Holmstrom and Milgrom (1991). Efforts to limit the underinvestment problem by incentivizing loss mitigation would be expensive and would exacerbate the multitasking problem.

In my review of securitization contracts, I find that servicing agreements do little to overcome

[10] Grossman and Hart (1986) define ownership as control of residual rights. Similarly, Hart and Moore (1990) define ownership as the ability to fire "employees," a right that the trust does not have because servicers can only be removed in exceptional situations.

the underinvestment problem. Servicers are required to follow accepted industry practices, but servicing agreements provide no explicit incentives for loss mitigation. The agreements actually do the opposite. By universally reimbursing foreclosure expenses but not loss mitigation expenses, servicing agreements create an extra incentive to pursue foreclosure instead of loss mitigation. Servicing agreements address the multitasking problem by explicitly prohibiting some practices that could harm trusts. For example, modifications are frequently limited to cases where a mortgage is in default or default is foreseeable, and about a third of contracts categorically prohibit some types of modifications. Ex-post renegotiation is precluded by trust passivity and investor dispersion (as in Bolton and Scharfstein, 1996). Thus, incomplete servicing contracts have real effects. Privately securitized loans are modified less and foreclosed more than they would be if they were held as portfolio loans. Contractual modification restrictions likely account for some of this bias, but they are too rare and insufficiently binding to explain the full bias. Most of securitization's impact on foreclosures and modifications comes from misaligned incentives.

To incentivize mortgage modifications and make modification practices more uniform, the Obama administration enacted the Home Affordable Modification Program (HAMP) in February of 2009. The program was rolled out over the course of 2009 and was fully operational by the end of the year. Potential HAMP modifications are evaluated using a standardized NPV test. If the NPV test indicates that modification is more beneficial to the lender than foreclosure would be, the servicer employs a four-step waterfall to reduce monthly payments to 31% of income by first capitalizing past-due balances, then reducing interest rates to as low as 2%,[11] then extending loan terms to up to 40 years from the modification date, and then forbearing principal. Servicers receive $1000 of incentive compensation per HAMP modification and success fees of up to $1000 per year for performing modifications. Borrowers can also earn up to $1000 in principal forgiveness per year for five years for keeping modified mortgages current. HAMP does not override specific contractual restrictions, but it does create safe harbors for servicers by deeming the HAMP NPV tests to be the appropriate measure of investor welfare and deeming the waterfall modification methodology to be standard industry practice. HAMP is a voluntary program, but all major servicers participate, and participating servicers are required to use HAMP modification guidelines

[11] Interest rate reductions are permanent unless they are reduced below prevailing interest rates, which establish an Interest Rate Cap. If interest rates are reduced below the cap, they stay at the reduced level for five years and then are gradually increased to the cap.

for all qualifying non-GSE mortgages, whether they are privately securitized or held as portfolio loans.

HAMP's efficacy is the subject of an ongoing debate.[12] My methodology does not provide a way to test whether HAMP has succeeded in reducing foreclosures, but I can test whether foreclosure and modification probabilities were consistent across securitized and portfolio loans after HAMP. For modification, this appears to be the case. For 2010 and 2011 delinquencies, securitization does not significantly affect the probability that a loan will be modified. However, HAMP left intact the bias of securitized loans towards foreclosure. Among 2010 and 2011 delinquencies, securitization increased the six-month probability of foreclosure initiation by 9.1 ppt (26% on a relative basis) and foreclosure completion by 1.8 ppt (44% on a relative basis).

2 Existing Evidence

Previous studies of the effect of securitization on servicing decisions have regressed the probability of foreclosure or modification conditional on serious delinquency on securitization status, controlling for observable loan characteristics. For example, Piskorski, Seru, and Vig (2010) consider mortgages originated in 2005 and 2006 that become 60+ days delinquent. Compared to privately securitized mortgages, portfolio mortgages have foreclosure rates that are 4-7% lower after controlling for observable loan characteristics.[13] Using a similar approach, Agarwal, et al. (2011) estimate that portfolio mortgages that became seriously (60+ days) delinquent in 2008 are 4.2% more likely to be renegotiated within 6 months relative to comparable privately-securitized mortgages.[14] In contrast, Adelino, Gerardi, and Willen (2011a) find that for mortgages originated starting in January of 2005 that become seriously (60+ days) delinquent by September of 2007,

[12] For example, Agarwal, et al. (2012) argue that HAMP increased modifications but has fallen short of program goals because of mixed servicer compliance.

[13] Results taken from Table 3 of Piskorski, Seru, and Vig (2010) in which the authors estimate quarterly (by origination date) logit regressions of the form: $\Pr(Foreclose_i|Delinquency_i) = \text{logit}(\alpha + \beta * portfolio_i + \gamma X_i + \varepsilon_i)$, where X_i is a vector of observable loan characteristics, including origination FICO score, origination loan-to-value ratio, origination amount, mortgage terms, mortgage age at delinquency, and MSA fixed effects. β, the coefficient of interest, has a highly significant marginal effect of -4 to -7% depending on the origination quarter. The authors use Lender Processing Services (LPS) data.

[14] Result taken from Table 3, Panel A, column (3) of Agarwal et al. (2011) in which the authors estimate an OLS (linear probability model) regression of the form: $\Pr(Foreclose_i|Delinquency_i) = \alpha + \beta * portfolio_i + \gamma X_i + \varepsilon_i$, where X_i is a vector of observable loan characteristics, including FICO score upon delinquency, loan-to-value ratio upon delinquency, origination amount, mortgage terms, origination year fixed effects, zip code interacted with calendar quarter fixed effects, and servicer fixed effects. The authors use OCC-OTS Mortgage Metrics data, which is similar to LPS data but also includes servicer identifiers and direct indicators for loan renegotiation.

differences in loan modification rates between comparable portfolio and privately securitized loans over the twelve months following their first serious delinquency are small. If anything, privately securitized loans are modified slightly more frequently (0.6% to 2.1%) than portfolio loans.[15]

The conflicting results of these papers are attributable to two factors. First, securitization has a larger impact on foreclosure than it does on modification. I find this in my analysis, and Agarwal, et al. find the same thing in their Appendix A. Securitization impacts foreclosures in more ways than just preventing formal modifications, which explains why Piskorski, Seru, and Vig find large foreclosure effects while Adelino, Gerardi, and Willen do not find significant modification effects in a largely equivalent data sample. The second factor is the sample time period. Due to data availability, Agarwal, et al. focus on a later time period than the other two papers. Securitization appears to have a bigger impact in the later time period. Adelino, Gerardi, and Willen show this is the case by replicating the Agarwal, et al. result in the later time period. Similarly, when Agarwal, et al. analyze foreclosure probabilities in their later sample, they get a larger coefficient estimate than Piskorski, Seru, and Vig.

The bigger issue with the existing evidence is that causal interpretation of all three papers requires the assumption that securitization status is randomly assigned conditional on observed loan characteristics that are explicitly controlled for. This is a problematic assumption because there is good reason to believe that securitized loans are unobservably different from portfolio loans. Origination and securitization are endogenous decisions, and both are made based on a larger set of information than the observed control variables. One channel, highlighted by Keys, et al. (2010), is that moral hazard may have caused originators to screen loans that were likely to be securitized less diligently than similar loans that were likely to be held as portfolio loans. Originators may have also intentionally selected mortgages for securitization based on characteristics that are unobservable to econometricians.[16] Unobserved quality differences are a big problem for causal inference because they likely affect optimal foreclosure and modification decisions.

Piskorski, Seru, and Vig (2010), Agarwal, et al. (2011), and Adelino, Gerardi, and Willen

[15] Table VI, Panel B of Adelino, Gerardi, and Willen (2011a) shows results from logit regressions of modification probability on securitization status and observable loan characteristics at origination and upon delinquency. Depending on how modification is defined, private securitization increases modification probability by 0.6% to 2.1% relative to portfolio loans. The authors use Loan Processing Services (LPS) data. Because LPS data does not explicitly flag modifications, the authors identify modifications based on observed changes to loan terms.

[16] Selection on unobservable characteristics could come from adverse selection or joint agreement with equally-informed MBS sponsors.

(2011a) all explicitly recognize the potential bias presented by unobserved quality. Yet, all three papers ultimately adopt causal interpretations of their evidence for or against securitization affecting servicing decisions. Their first rationale for a causal interpretation is that conditioning on serious delinquency mitigates the unobserved quality problem. Market participants may have unobserved information about probability of delinquency or loan quality conditional on delinquency. If unobserved information is solely about probability of delinquency, then conditioning on delinquency gets rid of the problem. Unfortunately, there is no reason to believe that unobserved information is solely, or even primarily, about delinquency probability. There is actually good reason to believe the opposite because FICO scores (which are one of the most important observable quality measures) predict only the probability of a negative credit event, not the losses associated with the event. The second rationale the papers advance is that their results are qualitatively similar for high quality loans (i.e., loans with high FICO scores and full income documentation), which should have less potential for unobserved quality differences.[17] Though not clearly documented, smaller unobserved quality differences for high quality loans seem likely on an unconditional basis. However, the relevant unobserved difference is quality conditional upon delinquency, and this could be just as large for high quality loans as for low quality loans.

Finally, Piskorski, Seru, and Vig (2010) conduct a "quasi-experiment." They note that early payment default (EPD) clauses require some originators to buy back loans that become delinquent within 90 days of securitization. Loans that become delinquent shortly before and after this 90-day threshold differ in their probability of remaining securitized but are otherwise very similar. The authors attempt to exploit this discontinuity by comparing loans that become delinquent shortly before 90 days and are bought back and kept by the originator to loans that become delinquent shortly after 90 days and remain securitized. Unfortunately, Piskorski, Seru, and Vig (2010) do not use conventional instrumental variables or fuzzy regression discontinuity tools. Instead, their empirical design contaminates the plausibly orthogonal variation in securitization probability (timing of delinquency relative to the 90 day threshold) with endogenous decisions (whether the loan is bought back by the originator and whether it remains on the originator's balance sheet) by

[17] Piskorski, Seru, and Vig (2010) and Agarwal, et al. (2011) use high quality loans as a robustness test. Adelino, Gerardi, and Willen (2011a) avoid this approach and argue that unobserved heterogeneity may actually be greater for loans that appear to be high quality because these loans were not securitized by the GSEs for some unobserved reason.

directly comparing the two groups described above. Because repurchases are based on factors other than delinquency status (for example, a loan could unobservably violate another representation) and originators decide whether to retain or re-securitize repurchased loans, the resulting comparison is just as prone to omitted variable bias as the original securitization regressions. Adelino, Gerardi, and Willen (2011b) discuss this issue more fully and argue that early default is not a good instrument even if it is properly implemented. My survey of securitization contracts suggests another reason for skepticism: EPD clauses appear to be rare. In my sample of 37 prime mortgage securitization deals, none had EPD clauses.[18]

3 Data and Methodology

Loan Performance Data

My data on mortgage loans comes from Lender Processing Services (LPS).[19] The dataset consists of detailed monthly data on individual loans provided by large mortgage servicers, including at least seven of the top ten servicers. As of 2007, the dataset included 33 million active mortgages, representing approximately 60 percent of the U.S. mortgage market. Importantly, the dataset spans all mortgages serviced by the participating servicers, including portfolio loans, GSE securitized loans, and privately securitized loans. My analysis focuses on first lien loans originated between January and August of 2007. To avoid survivor bias, I only consider loans that enter the LPS dataset within four months of origination. I drop government sponsored loans like VA and FHA loans because these loans may have different servicers requirements and incentives. To eliminate outliers and focus on reasonably typical prime (or near prime) loans I further restrict the sample to loans with origination FICO scores between 620 and 850, origination loan-to-value ratios of less than 1.5, and terms of 15, 20, or 30 years that are located in U.S. metropolitan statistical areas (MSAs) outside of Alaska and Hawaii. Finally, I drop a small set of loans that are at some point transferred to a servicer that doesn't participate in the LPS data because the data doesn't always reveal how delinquencies were ultimately resolved for these loans. Other than my exclusion of low FICO score loans and inclusion of GSE loans, these restrictions are largely consistent with

[18] By contrast, in their internet appendix Piskorski, Seru, and Vig (2010) find that 13% subprime deals in 2005 and 2006 had some kind of EPD clause.

[19] LPS data was previously known as McDash data.

Piskorski, Seru, and Vig (2010), Agarwal, et al. (2011), and Adelino, Gerardi, and Willen (2011a). The resulting sample consists of 1.9 million loans.

Table 1 describes the sample. It includes 264 thousand jumbo loans (i.e. loans over $417,000, which are not eligible for GSE securitization)[20] and 1.6 million non-jumbo loans. As of six months after origination, most (70% of) jumbo loans are privately securitized. Almost all of the rest (27%) are held as portfolio loans. By contrast, 81% of non-jumbo loans are securitized by the GSEs. Delinquency is common in both sub-samples. 6% of jumbo loans become seriously (60+ days) delinquent within 1 year, and 36% become seriously delinquent within five years. Similarly, 4% of non-jumbo loans become seriously delinquent within 1 year and 27% become delinquent within 5 years. My regressions analyze loans that become seriously delinquent during two different time periods. The pre-HAMP sample consists of loans that became seriously delinquent within 12 months of origination.[21] This sample has 16 thousand jumbo loans and 61 thousand non-jumbo loans. The post-HAMP sample, which consists of loans that first became seriously delinquent in 2010 or 2011, has 26 thousand jumbo loans and 132 thousand non-jumbo loans. The jumbo and non-jumbo loans clearly differ in size. Jumbo loans also tend to have slightly higher FICO scores. Loan-to-value (LTV) ratios are almost identical across jumbo and non-jumbo loans.

Identifying delinquencies is straight-forward because LPS includes data on payment status. Consistent with previous studies, I use the Mortgage Bankers Association's (MBA) definition of 60+ day delinquency. Foreclosures are also identified in the LPS data. I consider both foreclosure initiation, the referral of a loan to an attorney for foreclosure, and foreclosure completion, indicated by postsale foreclosure or real estate owned (REO) status. Piskorski, Seru, and Vig (2010) and Adelino, Gerardi, and Willen (2011a) study foreclosure completion, which has the nice property of being a final resolution. On the other hand, foreclosure initiation is a more direct servicer decision and is more common within my six-month window of analysis. As reported in Table 1, in the pre-HAMP sample foreclosure is initiated within six months of serious delinquency for 70% of jumbo loans and completed for 14%. Foreclosure rates are slightly lower for non-jumbo loans and decrease in the post-HAMP sample.

[20] The conforming loan limit in 2007 was $417,000 in all states except Alaska and Hawaii, which are excluded from my sample.

[21] I use delinquency date relative to origination date to equalize age upon delinquency across origination months. The twelve month cutoff combined with a six-month analysis window ends the analysis in February of 2009, before HAMP was implemented.

Identifying loan modifications is more complicated because they are not directly recorded in the LPS data. Nonetheless, modifications can be imputed from month-to-month changes in interest rates, principal balances, and term lengths. For example, absent errors in the data, an interest rate reduction on a fixed rate mortgage must be due to a mortgage modification. My algorithm for identifying loan modifications, described in detail in Appendix A, is essentially the same as the algorithm employed by Adelino, Gerardi, and Willen (2011a). Broadly, I consider two (potentially overlapping) types of modifications: concessionary modifications that reduce monthly payments by decreasing interest rates, decreasing principal, or extending the loan's term; and modifications to make a loan current by capitalizing past due balances. The loan modification algorithm looks for evidence of either of these patterns. In contrast to the LPS data, Agarwal, et al. (2011) employ OCC-OTS Mortgage Metrics data, in which servicers explicitly flag loan modifications. This may or may not be an advantage. While the explicit modification flags are cleaner, they rely on self-reporting by servicers. Adelino, Gerardi, and Willen (2011a) address this trade-off and conclude that the algorithmic approach to modification identification is reasonably accurate and produces results consistent with Agarwal, et al. (2011) when used to analyze the same time period. In my pre-HAMP jumbo sample, 5.9% of seriously delinquent jumbo loans were modified within six months, and these modifications are overwhelmingly principal-increasing as opposed to concessionary. Post-HAMP, the six-month modification rate increased to 8.5%, and interest rate reductions (3.5%) and term extensions (2.1%) became more common.

Instrumental Variables Design

Mortgage securitization comes in two forms. Most residential mortgages are securitized by Fannie Mae and Freddie Mac (the GSEs). However, not all mortgages qualify for GSE securitization. A loan may fail to conform to GSE standards either because it fails their underwriting standards (subprime loans) or because it exceeds their loan limits (jumbo loans). Starting in the 1990s and growing rapidly in the early 2000s, liquid private markets arose to securitize subprime and jumbo loans. In 2006, $1.1 trillion of private mortgage-backed securities were issued, including $200 billion backed by jumbo mortgages.[22]

Private securitization abruptly halted in the third quarter of 2007 and has essentially remained

[22] Source: Inside Mortgage Finance.

frozen since then. Figure 1 plots private securitization volume from 2000 to 2011. Jumbo prime MBS issuance topped $55 million dollars in quarters 1 and 2 of 2007 then crashed to $38 million in Q3 and $18 million in Q4, followed by almost no issuance after 2007. The private securitization freeze was simultaneous with the August 2007 collapse of asset-backed commercial paper, previously a $1.2 trillion market that was heavily invested in MBS. Both freezes were unanticipated and appear to have been caused by sudden increases in investor apprehension of mortgage backed securities, particularly subprime MBS.[23] Consistent with this view, ABX indices for AAA MBS (plotted in Figure 2) fell below unity for the first time shortly before the market freezes.[24] GSE credit guaranties prevented similar fears in the GSE MBS market, which continued to issue securities uninterrupted throughout 2007 and the rest of the financial crisis.

I use the August 2007 private securitization freeze as a natural experiment for jumbo securitization. Because the freeze was unanticipated, it did not affect origination decisions until after it occurred. This is the exclusion restriction underlying my identification strategy. To confirm that it is a reasonable assumption, I plot monthly mortgage originations in my sample by month in Figure 3. Jumbo originations track non-jumbo originations and stay in the neighborhood of 30,000 originations per month until August of 2007. Jumbo lending then dramatically falls in September of 2007 while non-jumbo lending (which is largely unaffected by private securitization) remains steady. This is exactly the response we would expect from an unexpected freeze in private securitization.

Though the freeze did not afect pre-freeze origination decisions, it did affect the probability that those mortgages were securitized. Assembling a pool of loans, selling them to a MBS sponsor, and closing on the MBS deal often takes a few months. Table 2 highlights this lag. Within my sample of January 2007 originations, only 12% of jumbo loans were privately securitized in their origination month. By two months after origination, 66% are privately securitized. Private securitization further increased to 79% by six months after origination. As 2007 progressed, less and less time was available to securitize new originations before the freeze. As a result, the probability

[23] Kacperczyk and Schnabl (2010) document the collapse of asset backed commercial paper and identify the July 31, 2007 bankruptcy filing two Bear Stearns hedge funds that invested in subprime mortgages and the August 7, 2007 suspension of withdrawals at three BNP Paribus funds as the catalysts of the collapse. Calem, Covas, and Wu (2011) and Fuster and Vickery (2012) discuss the private MBS issuance freeze, which they date to August 2007 and exploit as a liquidity shock to jumbo lending.

[24] ABX indices track prices of credit default swaps on the underlying mortgage backed securities. See Stanton and Wallace (2011) for more information.

of securitization dropped dramatically in the summer of 2007. Figure 4 plots ownership status six months after origination for jumbo loans in my sample by origination month. This is essentially the first stage regression for my identification strategy. Private securitization rates are around 80% until April and then start to decline, with dramatic drops in the summer to 65% in June, 54% in July, and 36% in August. Over this time period, the volume of portfolio loans increased from 6,500 in April to 17,900 in August, consistent with lenders being stuck holding portfolio loans they had anticipated securitizing.

One potential concern with this identification strategy is that the mortgage lending environment may have changed over the course of 2007 resulting in differences between origination-month cohorts even though the securitization freeze was unanticipated. Fortunately, I have a natural control group that was not affected by the securitization freeze. Prime non-jumbo loans are predominately securitized by the GSEs, and GSE securitization was uninterrupted throughout 2007. Figure 5 plots ownership status six months after origination for non-jumbo loans in my sample by origination month. While jumbo securitization drops from March to August, GSE securitization rates of non-jumbo loans remained steady at around 85%.

My specific empirical strategy is to estimate equations of the form:

$$Pr\left(Y_i|Delinquency_i\right) = \alpha + \gamma Sec_i + \beta_1 Jumbo_i + OrigMonth_i\beta_2 + X_i\beta_3 + \varepsilon_i \tag{1}$$

using $Jumbo_i * OrigMonth_i$ indicator variables as instruments for private securitization (Sec_i). Y_i is an indicator for foreclosure or modification within six months within first serious (60+ day) delinquency.[25] Sec_i is an indicator for a mortgage being privately securitized six months after origination. $Jumbo_i$ is an indicator for jumbo status. $OrigMonth_i$ is a vector of origination-month dummy variables. X_i is a vector of observable loan characteristics. The implied linear probability model accommodates standard IV regression techniques and more readily incorporates fixed effects without biasing coefficient estimates.[26]

The reduced form of this empirical strategy is a difference in differences regression of Y_i (foreclo-

[25] I use a six month window so that my pre-HAMP analysis ends in February of 2009, before HAMP took effect.

[26] Angrist and Pischke (2009) advocate using linear IV (two stage least squares) even when the outcome and endogenous regressor are both binary, as they are here. The alternative is to estimate a bivariate probit model, which requires more restrictive distributional assumptions and cannot accommodate a large number of fixed effects (e.g., MSA fixed effects) without biasing results.

sure or modification) on origination month exploiting differences between jumbo loans (the treated group) and non-jumbo loans (the control group). I plot these relationships in Figures 6 (modification), 7 (foreclosure initiation), and 8 (foreclosure completion). The regressions also control for loan characteristics and delinquency month, but the basic relationship is visible in the figures. Up to April, jumbo and non-jumbo loans follow parallel trends. Then, as non-jumbo securitization falls between April and August, jumbo foreclosures decline and modifications increase relative to non-jumbo control loans. By using origination month is an instrument for jumbo securitization, the IV design extracts the causal impact of securitization on foreclosure and modification from this reduced-form difference in differences relationship.

Strictly speaking, the identification strategy does not require any control variables other than an indicator for jumbo loans and indicators for origination months (i.e., X_i is not needed). Nonetheless, I include a rich set of observable loan characteristics in X_i to increase equation (1)'s explanatory power and make it more directly comparable with previous studies. I control for borrower credit worthiness with an indicator for origination FICO scores above 680. I include origination loan-to-value (LTV) ratio as well as an indicator for LTV of exactly 0.8 because mortgages with an LTV of 0.8 are more likely to have concurrent second-lien mortgages (Adelino, Gerardi, and Willen, 2011a). The loan terms I control for are origination amount (through its log), origination interest rate, an indicator for fixed rate mortgages, indicators for the term length of the mortgage, an indicator for mortgage insurance, and an indicator for option ARM mortgages. I control for the quality of underwriting with indicators for low income documentation and no income documentation, and I control for loan purpose with indicators for refinancing, primary residence, and single family homes. I also control for MSA fixed effects and month of delinquency fixed effects. Finally, I include interactions between jumbo status and all control variables except MSA fixed effects to allow for the possibility that these characteristics affect jumbo and non-jumbo loans differently.

4 Results

Foreclosure and Modification Probabilities

I start by estimating the effect of jumbo securitization on foreclosure and modification before implementation of HAMP. This time period is most directly comparable to previous studies and is

relatively free of policy interventions designed to correct the perceived foreclosure bias for securitized loans. The sample is loans that became seriously (60+ days) delinquent within twelve months of origination. I use delinquency date relative to origination as my cutoff rule to ensure that age upon delinquency is similar across origination-months. The twelve-month delinquency window combined with my six-month analysis window ensures that the last month analyzed is February of 2009, which is before HAMP was implemented. Later, I consider loans that became delinquent after HAMP to test whether HAMP changed the effect of securitization on foreclosure and modification.

Before implementing my instrumental variables strategy, I first estimate equation (1) using OLS for the pre-HAMP sample of delinquent jumbo loans. Coefficient estimates and standard errors (clustered by MSA) are reported in Table 3. After controlling for observable loan characteristics, seriously delinquent securitized loans are 3.9 ppt more likely to have foreclosure initiated, 2.2 ppt more likely to have foreclosure completed, and 2.1 ppt less likely to be modified within six months. 97% of sample jumbo loans are privately securitized or held as portfolio loans so the coefficients estimate differences between these two groups. The coefficients are slightly lower than Piskorski, Seru, and Vig's (2010) 4-7% foreclosure bias estimate and Agarwal, et al.'s (2011) -4.2% modification bias estimate. Given that I analyze only jumbo loans instead of all loans and that my sample covers a slightly different time period and uses a shorter analysis window than Piskorski, Seru, and Vig (2010), my OLS results are generally consistent with these previous findings. By contrast my results conflict with the approximately equal modification rates of Adelino, Gerardi, and Willen (2011a). This is likely due to the sample period since Adelino, Gerardi, and Willen (2011a) show that the modification gap between portfolio loans and privately securitized loans grew over time.

Like previous studies, my OLS regressions are not conducive to causal interpretation because securitization status may be correlated with unobserved (and thus omitted) loan characteristics that explain part of the residual of equation (1). Can we at least determine the direction of the bias? The short answer is no. As explained in the previous section, the leading concern is that originators and issuers endogenously securitize loans that are unobservably lower quality. This quality gap means securitized loans should be more likely to become delinquent than portfolio loans after controlling for observable loan characteristics. However, the impact of loan quality on foreclosure and modification decisions conditional on delinquency is ambiguous. Some quality

dimensions likely favor foreclosure, while others favor modification or inaction. For example, low borrower quality may favor foreclosure by making it less likely that a borrower will meet future obligations (with or without a modification). In contrast, low property quality may diminish foreclosure proceeds, making foreclosure less attractive. Ambiguities like this are apparent in the OLS control variable coefficient estimates. Some measures of quality increase foreclosure probability while others decrease it. For example, a high FICO score increases the probability of foreclosure initiation within six months by 8.7 ppt whereas a low loan-to-value ratio decreases the same probability (see column (1) of Table 3).

Table 4 addresses the omitted variable problem by using jumbo origination month as an instrument for securitization status. Coefficients are estimated using two stage least squares. Standard errors are clustered by MSA. In columns (1) to (3), I estimate the instrumental variables regression on jumbo loans only. The sample and dependent variables are identical to the OLS regressions in Table 3 except that I now use origination-month indicator variables as instruments for private securitization instead of as control variables. Columns (4) to (6) implement my baseline methodology of instrumenting for jumbo securitization with the interaction between jumbo status and origination month while controlling for origination-month fixed effects with non-jumbo loans. In addition to the control variables in Table 3, columns (4) to (6) of Table 4 include indicator variables for jumbo loans and interactions between the jumbo indicators and all independent variables in Table 3 except MSA fixed effects and origination-month fixed effects. Including origination-month fixed effects does not significantly impact the securitization coefficient in the foreclosure regressions, but it does change the modification securitization coefficient. Throughout the rest of the paper, I employ regressions analogous to columns (4) to (6) unless otherwise noted.

Turning to the baseline coefficient estimates (columns (4) to (6) of Table 4), private securitization increases the six-month probability of foreclosure initiation by 8.7 ppt and foreclosure completion by 5.2 ppt. Securitization decreases the six-month probability of modification by 2.3 ppt. The coefficient estimates are all statistically significant (standard errors range from 1 ppt to 1.5 ppt). Moreover, they are economically meaningful. Compared to average rates of 70% for foreclosure initiation, 14% for foreclosure completion, and 6% for modification (see Table 1), private securitization increases foreclosure initiation by 12%, increases foreclosure completion by 39%, and decreases modification by 39% on a relative basis. Comparing columns (4) to (6) of

Table 4 to Table 3 reveals the omitted variable bias of the OLS regressions. For foreclosure, the IV securitization coefficient estimates are over twice as large as their OLS counterparts. On the other hand, the OLS and IV estimates are similar for modification. It appears that unobserved quality differences between securitized and portfolio loans make securitized loans less likely to be foreclosed without having much affect on modification. As a result OLS underestimates the causal impact of securitization on foreclosure.

One difference between my empirical design and Piskorski, Seru, and Vig (2010) and Gerardi, Adelino, and Willen (2011a) is that I use a six month analysis window instead of considering loans for a longer period of time after delinquency.[27] The shorter window is desirable because it ends before HAMP, but it creates the possibility that I am picking up acceleration or deceleration in foreclosure and modification instead of changes in their ultimate probability. Columns (1) to (3) of Table 5, Panel A address this concern by replicating my baseline results with twelve-month windows instead of six-month windows. The coefficient estimates are consistent with my baseline results. The foreclosure start coefficient is 1.5 ppt lower, and the foreclosure completion coefficient is 0.8 ppt lower. Both differences are within the baseline confidence interval. The modification coefficient changes more significantly from -2.3 ppt to -5.0 ppt, suggesting that my baseline six-month coefficient estimate is conservative.

Another potential concern is that non-jumbo origination-month fixed effects are insufficient to control for changes to the jumbo lending environment in 2007 or that the securitization freeze was anticipated, particularly late in the sample. The best evidence against this concern is that the jumbo private securitization rate stayed stable in the 80-85% range from January to April and then dropped dramatically to 36% by August without a significant drop in originations until September (see Figures 3 and 4). Loan volume would have dropped sooner if the securitization freeze was anticipated, and other changes to jumbo lending this sudden and large are unlikely especially since I control for changes in the overall lending environment with non-jumbo fixed effects. Nonetheless, I address the concern by restricting my sample to loans originated between May and July of 2007. The probability of securitization dropped significantly over these three months from 77% in May to 54% in July, and ending the sample before August reduces the concern that securitization market

[27] Piskorski, Seru, and Vig (2010) consider all foreclosure actions up to the first quarter of 2008, which could be as much as three years after a loan becomes seriously delinquent. Gerardi, Adelino, and Willen (2011a) use a twelve-month analysis window.

changes may have been anticipated at the time of origination. Columns (4) to (6) of Table 5, Panel A show regression estimates for the restricted sample. Standard errors are larger, but the coefficient estimates are consistent with my baseline results. If anything, the foreclosure completion and modification results are slightly larger in the restricted sample.

Panel B of Table 5 reports two additional robustness tests. In columns (1) to (3), I estimate bivariate probit models. As discussed by Wooldridge (2002), this specification implements instrumental variables identification while bounding outcome (foreclosure or modification) and treatment (securitization) probabilities between 0 and 1 with probit functions. To get the models to converge (and to avoid biases associated with a large number of fixed effects), I limit the analysis to jumbo loans and drop origination-month and MSA fixed effects. Marginal effects of private securitization (6.8 ppt for foreclosure initiation, 4.1 ppt for foreclosure completion, and -3.7 ppt for modification) are consistent with my baseline results. Columns (4) to (6) of Panel B, rerun my baseline regressions without loan characteristic controls. The coefficient estimates for foreclosure completion and modification (4.8 ppt and -3.3 ppt, respectively) are close to my baseline results. The foreclosure initiation coefficient estimate of 17.6 ppt exceeds my baseline estimate of 8.7 ppt, suggesting that the baseline estimate may be conservative.

Modification Details and Effectiveness

In addition to impacting probability of modification, securitization may also affect how loans are modified. Some PSAs place limits on principal and interest reductions and modifications that extend loan terms beyond the terms of the bonds financing them (typically 30 years). Further, servicers of securitized loans may have an incentive to keep delinquent loans alive longer through principal-increasing modifications that capitalize past due balances. Finally, servicers of securitized loans may have less incentive to invest in thoughtful screening and negotiation to give modifications the best chance of successfully preventing future default.

To assess the impact of securitization on modification terms, I employ my IV regression strategy on the subset of pre-HAMP delinquencies that are modified. First, I consider indicators for different types of modifications as my dependent variables, thereby estimating the probability of a certain type of modification conditional on there being a modification of some kind. Except for the sample reduction and different dependent variables, the regressions are identical to my baseline regressions.

Panel A of Table 6 shows the results. Securitization has essentially no impact on the incidence of interest and term modifications (securitization coefficients are an insignificant -0.5 ppt and -2.6 ppt respectively). On the other hand, securitization does impact principal modifications, decreasing the share of modifications involving a principal decrease by 7.9 ppt and increasing the share of modifications involving a principal increase by 13.5 ppt.

I also consider how securitization affects net changes to interest rates, term lengths, principal levels, and monthly payments during modification. Panel B of Table 6 shows results for regressions of net changes on the same variables considered in Panel A. None of the securitization coefficients are significant, which is consistent with the general lack of significance in similar OLS regressions estimated by Agarwal, et al. (2011). It appears that securitization favors principal increases over principal reductions without having much impact on average modification terms.

Finally, I compare the success of securitized and portfolio modifications by estimating the probability of redefault (return to 60+ day delinquency) in the six months following modifications that cured delinquencies. Table 7 reports the results. In column (1), I estimate an OLS regression of redefault on private securitization and standard controls in the pre-HAMP sample. Redefault is 12.4 ppt higher for securitized loans compared to portfolio loans, and this difference is largely unexplained by the type of modification employed (see column (2)). These OLS results are qualitatively similar to Agarwal, et al.'s (2011) OLS estimate that redefault is 3.5% higher for securitized modifications relative to portfolio modifications. To assess whether these differences are causal, I employ my IV regression methodology in columns (3) and (4). The resulting coefficient estimates are lower and indistinguishable from zero, but they have large standard errors of approximately 6 ppt. The bottom line is that my IV strategy does not have sufficient power to assess the relative performance of securitized and portfolio modifications.

Post-HAMP Results

As discussed in the introduction, one of HAMP's goals was to make modification and foreclosure decisions more consistent across loans. In particular, policy makers wanted to correct the perceived bias of securitized loans towards foreclosure and away from modification. Was HAMP successful at mitigating these biases? To assess post-HAMP securitization biases, I repeat my baseline empirical strategy on jumbo loans originated between January and August of 2007 that became

seriously (60+ days) delinquent for the first time in 2010 or 2011, after HAMP was implemented. Table 8 shows the results. HAMP seems to have eliminated most if not all of the bias against modification of securitized loans. Post-HAMP, the securitization coefficient on modification is an insignificant -1.1 ppt (-13% on a relative basis) compared to a pre-HAMP coefficient of -2.3 ppt (-39% on a relative basis). However, there is no evidence that HAMP changed the securitization foreclosure bias. Post-HAMP securitization coefficients are 9.1 ppt (26% on a relative basis) for foreclosure initiation and 1.8 ppt (44% on a relative basis) for foreclosure completion compared to pre-HAMP coefficients of 8.7 ppt (12% on a relative basis) for foreclosure initiation and 5.2 ppt (39% on a relative basis) for foreclosure completion.

Did HAMP make modification details more consistent across securitized and portfolio loans? I replicate my modification-detail regressions on the post-HAMP sample. Table 9 reports the results. HAMP's impact is mixed. The securitization coefficient for principal decreases falls from -7.9 ppt before HAMP to an insignificant -2.7 ppt. On the other hand, the securitization coefficient for principal increases goes up from 13.5 ppt before HAMP to 27.2 ppt after HAMP, and securitization decreases the prevalence of term extensions after HAMP (this is clear in both the modification type (Panel A, column (2)) and net term length change (Panel B, column (2)) regressions).

The post-HAMP collapse of the principal-decrease coefficient is consistent with servicers using a more uniform methodology across securitized and portfolio loans. In contrast, HAMP likely made limits on term extensions more binding by making term extensions part of the standard modification process. The higher post-HAMP principal-increase coefficient is difficult to rationalize with HAMP making modifications more consistent across loans. One possibility is that servicers treat capitalizing past due balances to restore loans to current status as a separate tool from HAMP modifications. If so, HAMP would have little impact on these modifications, and servicers would have similar incentives to pursue these modifications before and after HAMP.

Direct comparisons between pre-HAMP and post-HAMP coefficients are somewhat problematic because it is not clear exactly what the counterfactuals should be. Even aside from HAMP policy changes, the regressions consider different time periods and the loans analyzed have different ages. Nonetheless, the fact that the modification bias decreased and became insignificant post-HAMP while the modification rate increased strongly suggests that HAMP made modification decisions more consistent across securitized and portfolio loans. By contrast, the foreclosure bias coefficients

remain significant post-HAMP and increased according to some measures. This evidence suggests that HAMP largely corrected the modification bias without having much impact on the foreclosure bias. Perhaps this should not be surprising. HAMP is almost exclusively focused on modifications, which represent only 8% of sample delinquencies even post-HAMP. Lenders do not face a binary decision between foreclosure and modification. They can also agree to short sales, refinance loans, give temporary relief, or take no action at all in the hope that a loan will self-cure. HAMP does little if anything to address incentives for these other actions relative to foreclosure so it is entirely consistent for HAMP to correct the modification bias without making much of a dent in the foreclosure bias.

5 Mechanism

The preceding section establishes that securitized loans are modified less and foreclosed more than comparable portfolio loans. Specifically, before HAMP securitized loans were 2.3 ppt less likely to be modified, 8.7 ppt more likely to have foreclosure initiated, and 5.2 ppt more likely to have foreclosure completed within six months of becoming seriously delinquent. Why do servicers treat securitized loans differently from portfolio loans?

Servicing securitized mortgages is a classic principal-agent problem. Securitized mortgages are owned by trusts that are explicitly passive (in part for tax reasons) and managed by third party servicers. Servicing current mortgages is relatively straight-forward. Servicers bill mortgagees, collect and forward payments, and maintain records. These functions can be readily standardized and specified in servicing contracts. By contrast, servicing delinquent loans is highly discretionary. Collection, modification, and foreclosure involve unobservable actions and loan-specific decisions that are difficult to specify in advance.

As in other principal-agent settings, servicer practices can deviate from investor interests either because of contract rigidity or because servicer incentives differ from investor incentives. The most obvious case of contract rigidity is explicit prohibitions of certain practices, particularly modification. These restrictions are meant to protect investors but may end up hurting them in some situations. Incentive differences are primarily manifested in an incentive for servicers to underinvest in practices that could enhance mortgage value but would be costly to servicers. Servicers may also

have an incentive not to deviate from default practices. For example, if foreclosure is the default practice for delinquent loans, servicers may perceive that alternatives invite investor scrutiny and liability risk. In some principal-agent settings, deviations from the principal's preferred actions can be corrected with ex-post renegotiation. This is all but impossible for MBS because dispersed investors lack the ability and incentive to monitor servicer practices.[28] Amending servicing contracts is also a difficult process, requiring super-majorities of certificateholders.

Previous discussions have focused mainly on securitization impeding mortgage modification, often with an emphasis on contractual modification restrictions, and this spilling over into increased foreclosure rates. This is an incomplete view of how securitization impacts delinquent mortgage servicing. Binding contractual restrictions on modifications are actually quite rare, and spillover from decreased modifications is only a small part of the bias of securitized loans towards foreclosure. We have already seen three pieces of evidence to this effect. First, securitization has a larger impact on foreclosure (8.7 ppt for foreclosure initiation and 5.2 ppt for foreclosure completion) than on modification (-2.3 ppt). If the foreclosure bias was a spillover from modification frictions, it would be smaller than the modification bias. Second, HAMP largely eliminated the modification bias without having much impact on the foreclosure bias. Finally, HAMP's modification impact came despite not overriding contractual terms, suggesting that the original modification bias was due to misaligned incentives as opposed to contractual restrictions.

To better understand how securitization affects mortgage servicing, I survey the contractual terms of actual servicing agreements and link those terms to panel data on modifications and foreclosures. I find that reimbursement policies universally incentivize foreclosure over modification and other effort-intensive loss mitigation practices. In contrast, binding modification restrictions are rare and have only moderate impact on modification rates.

Servicing Practices

Before discussing frictions associated with servicing securitized loans, it is important to understand the options available to servicers when dealing with delinquent loans. Many discussions treat foreclosure and modification as binary responses to delinquency. This is not at all the case.

[28] MBS trusts have a trustee that theoretically represents the interests of certificateholders, but the actual power and responsibility of trustees are limited, and servicers can only be removed in exceptional situations. Moreover, the trustee is just another agent of the underlying investors with its own conflicts of interest.

In addition to foreclosure and modification, servicers have a wide range of notification, collection, relief, and loss mitigation options. Securitization has the potential to bias whether and how all of these options are used.

FNMA's 2006 Servicing Guide offers one window into the breadth of delinquency management practices available to servicers. Notification options include late payment notices, payment reminder notices, reminder phone calls, letters (preferably individually-written as opposed to form letters), and face-to-face interviews. If communication alone does not suffice, FNMA has procedures for debt collection by attorneys, acceptance or rejection of partial payments, referral to counseling agencies, and direct delinquency counseling. In parallel with these efforts, servicers are to communicate with junior lien-holders. If a temporary hardship is identified, the servicer may offer special relief in the form of a 30-day grace period, longer forbearance agreement, or repayment plan to pay past-due balances over time on top of regular monthly payments. With FNMA approval, servicers can also negotiate more formal "Loss Mitigation Alternatives," including loan modifications, short sales, deed-in-lieu of foreclosures, assumptions of mortgages by new homebuyers, and assignment of mortgages to mortgage insurers.

Choosing among these options requires significant servicer discretion. Optimal practices depend on loan-specific soft information that is difficult to document and essentially impossible to contract on ex-ante. Moreover, most delinquency management practices involve personal interaction with borrowers, which makes them costly and dependent on unobservable effort. Modification is particularly challenging because it requires servicers to negotiate new mortgage terms, which have the potential to harm investors.

Levitin and Twomey (2011) contrast foreclosure with other delinquency management tools. Foreclosure is unique in that once undertaken it involves little discretion and can be largely outsourced and automated. For example, Levitin and Twomey describe a widely used software platform that automatically refers mortgages to approved local attorneys once certain delinquency benchmarks (e.g., 60 days past due) are reached. The software uploads required documents for the attorneys and generates specific instructions and timelines without any human contact.

All servicers face a decision as to how much they should automate delinquent loan servicing. At one extreme, decisions can be highly formulaic and push most delinquent borrowers into foreclosure. At the other extreme, servicing can be hands-on with significant personal interaction and solutions

tailored to specific borrower circumstances. The basic trade-off is overhead cost versus higher recovery rates on individual loans. Levitin and Twomey (2011), argue that faced with this tradeoff most servicers chose the scale efficiencies of heavy automation. They further argue that the tradeoff between automation and hands-on discretion changed as delinquency rates climbed in 2007 and 2008 but that servicers were ill-equipped to quickly ramp up non-foreclosure delinquency management capabilities.

Securitization introduces three additional elements into this tradeoff. First, because it involves less discretion, soft information, and unobservable effort, automation mitigates principal-agent conflicts. Second, because it is cheaper, third-party servicers will naturally choose automation. Overcoming the bias towards automation requires costly interventions such as incentive payments or contractual restrictions of servicer actions. These elements both make automation more ex-ante efficient for securitized servicing relative to portfolio servicing. The final element is that servicing agreements are locked in when a deal closes and are difficult, if not impossible, to alter in response to changing market conditions. Thus, automation is sticky for securitized servicing even if market conditions change to favor more hands-on discretion.

Servicing Agreements

Securitized mortgage servicing is governed by servicing agreements, which are incorporated into more general pooling and servicing agreements (PSAs). To understand how these agreements operate, I survey the terms of actual PSAs. My sample consists of all prime MBS deals between January and August of 2007 that exceeded $1B. 37 deals meet this criteria, which collectively represent $70B, 48% of total prime MBS issuance during this period.[29] For deals that involve multiple servicing agreements, I describe the agreement that is relevant to the most loans. The sample covers nine deal sponsors and seven servicers.

The PSAs give servicers broad authority for managing loans coupled with responsibility to follow accepted industry practices. Servicers bear most costs of servicing the loans and are compensated with a servicing fee, which is typically around 25 bps annualized for prime mortgages.

[29] Data on MBS issuance volumes comes from Inside Mortgage Finance. Classifications of individual MBS deals come from Inside Mortgage Finance and review of prospectuses and rating agency reports for individual deals. In addition to the 37 deals in my sample, Inside Mortgage Finance identifies another 10 deals as prime that are described as Alt-A by the ratings agencies.

Servicing fees are payable from loan proceeds and (in case of default) from the trust more generally so they function as a senior interest only strip for the life of a loan. Servicers also retain late fees and other ancillary fee income. Servicers generally have discretion to pursue modifications and other loss mitigation alternatives, but they have no direct incentive to do so because these tools require unreimbursable expenses and may involve waiving fee income. By contrast, foreclosure expenses are fully reimbursed. As long as they comply with accepted practices, servicers have an incentive to shade their delinquency management practices away from modification and loss mitigation and toward foreclosure. This incentive is compounded by the fact that foreclosure is universally specified as a default practice for delinquent loans, which may make it less risky for servicers from an investor liability point of view. Some PSAs contractually prohibit certain modifications, but these restrictions are relatively uncommon.

Table 10 summarizes the incidence of specific PSA terms. Sample PSAs universally require servicers to follow accepted servicing practices, generally defined as the practices of other responsible mortgage lenders. One source of these practices is FNMA servicing guidelines, which are explicitly incorporated into 38% of PSAs. 68% of PSAs also require that loans be serviced equivalently to portfolio loans, and in one case the PSA explicitly requires that servicing be in the best interest of certificateholders. In other PSAs this is implicit in general and sometimes an explicit standard for individual servicing decisions.

The PSAs also universally establish a default responsibility to foreclose on sufficiently delinquent loans and provide reimbursement for foreclosure expenses. PSAs allow foreclosure to be postponed or avoided altogether if it is not in the best interest of certificateholders (for example if modification is more valuable or if hazardous materials make foreclosure more expensive than the property's value), but these are always exceptions to the general rule of foreclosure.

By contrast, modification and other loss mitigation practices are never explicitly required and are not reimbursed through regular loan payments or by the trust. Instead, the servicers "may" pursue these alternatives and modify loans under certain conditions. The closest the PSAs come to requiring modification is a term in seven deals that requires the servicer to "consider" alternatives to foreclosure. In lieu of reimbursement from the trust, servicers are allowed to charge borrowers a modification fee. This is explicit in 22% of PSAs and implicit in the other PSAs by virtue of FNMA's 2006 servicing guide allowing servicers to charge borrowers a $500 modification fee and

some modification-related expenses. 59% of PSAs also disincentivize modification by requiring servicers to advance deferred or forgiven principal and interest payments for any modification that alters mortgage payments. These advances will eventually be reimbursed out of the loan's future proceeds or from the trust more generally, but in the mean time they constitute interest-free loans from the servicer to the trust.[30]

Of all the terms summarized in Table 10, modification restrictions vary the most and are of most interest. Some of these terms appear to be innocuous. 62% of PSAs explicitly prohibit principal, interest, or term modifications unless a mortgage is in default or default is foreseeable. This restriction is unlikely to be binding (it certainly does not bind for the seriously delinquent loans I analyze) and is probably implicit in accepted servicing practices even where it is not explicitly included. 22% of PSAs require the expected value of modified loans to exceed the expected value of foreclosure proceeds. This is also unlikely to bind and is implied by accepted servicing practices.

Binding modification restrictions come in the form of limitations on principal forgiveness, interest reductions, and term extensions. 22% of PSAs prohibit modifications that decrease principal balances or permanently decrease interest rates. 14% of PSAs prohibit modifications that increase loan maturity beyond the maturity of other loans in the trust or the maturity of the trusts' certificates. Because loans in a deal almost always have similar maturities (typically 30 years), this effectively prohibits term extensions. Importantly, these restrictions are uncommon compared with the universal incentive differences described above, and they still permit many kinds of modifications. For example, temporary interest rate reductions and principal forbearance are permitted under all PSAs. To explain the modification bias documented in Section 4, these restrictions would need to prevent all modifications.[31] The next subsection shows this is not the case.

Finally, amendment is difficult under all of the PSAs. General amendments require at least majority approval of certificateholders, and in all but one PSA they require either a supermajority of all certificateholders or a majority vote within each class of affected certificateholders.[32] Moreover, all PSAs expressly outlaw any amendment that would decrease or delay payments without universal

[30] Servicers similarly advance scheduled principal and interest payments while a loan is in default until the advances are deemed uncollectable.

[31] As shown in Table 1, 6% of delinquent jumbo loans were modified within six months before HAMP. If PSA restrictions reduced this to 0% in the 36% of deals with these restrictions, that would explain approximately 2 ppt of the 2.3 ppt modification bias documented in Table 4.

[32] Amendments to cure or correct ambiguities and conflicts are allowed without shareholder consent, and some PSAs (32%) allow more general amendments without consent if they don't adversely impact certificateholders.

consent of all certificateholders. Any amendment inducing modification or other loss mitigation activity over foreclosure would presumably trigger this prohibition. If a PSA is substantively modified, this would necessitate an 8-K filing with the SEC. I observed no such filing for any of the 37 deals I investigated.

This is the largest survey of PSA terms that I am aware of and the only one that focuses on prime MBS. It also describes a wider range of PSA terms than any previous study. Three other studies survey subprime PSAs with consistent results. Hunt (2009) surveys 20 subprime deals in 2006 and finds that 67% limit modifications to loans in default or where default is foreseeable or imminent and 10% prohibit modifications altogether. Credit Suisse (2007) surveys 31 deals between 2004 and 2007 and finds that nearly all loans permit modifications loans in default or where default is reasonably foreseeable and 60% had no other modification restrictions. A Bear Stearns study described by Bajaj (2007) and Hunt (2009) surveys approximately 20 deals and finds that 10% of deals prohibit modifications and another 40% of deals require ratings agency approval if more than 5% of a pool is changed.

PSA Term Regressions

To assess how modification restrictions affect servicer behavior, I link PSAs to individual loans in Core Logic panel data.[33] For comparability to my earlier analysis, I limit the dataset to jumbo loans and impose the restrictions described in Section 3.[34] As described in Table 11, the linked dataset includes 85,000 loans with an aggregate origination value of $60B. The loans are similar to the jumbo loans analyzed in Section 4 but are slightly larger ($708K on average compared to $691K) and have slightly higher FICOs (742 compared to 733) and lower LTVs (0.71 compared to 0.73). The linked sample also defaults less than the earlier sample (1% became 60+ days delinquent within 1 year and 21% became 60+ days delinquent within five years compared to 6% and 36%). These differences likely stem from the linked sample being entirely from prime MBS whereas my earlier sample included all jumbo mortgages with FICOs above 620. My analysis focuses on 18,000 loans

[33] Core Logic mortgage data is similar to the LPS data used for my previous analysis but is limited to privately securitized mortgages. Unlike LPS, Core Logic contains identifiers for servicers, originators, and deals, which allows me to link loans to PSAs.

[34] The only changes are that I no longer require loans to enter the dataset within four months of origination and I do not require loans to be originated in 2007. Survivor bias is not an issue in the Core Logic data because all loans enter the dataset when a deal closes.

that became seriously (60+ days) delinquent by the end of 2011. For descriptive purposes I break out pre-HAMP (within 12 months of origination) and post-HAMP (2010 and 2011) delinquencies, but my regression analysis is of the full sample of 18,000 delinquencies.[35] Foreclosure initiation (52% within six months) and completion (7% within six months) rates are similar to the previous sample. Modification rates (6% within six months) are similar to the previous sample overall but are lower than the previous sample prior to HAMP (1% within six months compared to 6%). Foreclosure and modification are defined and identified as before with one significant difference. I cannot identify term extensions in the Core Logic data. Thus, term modifications are missing from the PSA-linked data.

Having linked PSAs to individual delinquencies, I regress foreclosure and modification probability on indicators for PSA terms. Specifically, I regress foreclosure initiation, foreclosure completion, and modification within six months of first serious (60+ day) delinquency on indicators for prohibitions of (1) permanent principal and interest reductions and (2) term extensions beyond the term of the MBS certificates or other mortgages. As discussed earlier, these terms vary across PSAs. To the extent that they bind, we would expect them to reduce modifications and potentially increase foreclosures. The regressions are OLS and include the same control variables as previous regressions plus servicer fixed effects. The servicer fixed effects are important because PSA terms vary across servicers and previous studies (e.g., Agarwal, et al. (2011) and Agarwal, et al. (2012)) have demonstrated that servicers employ different modification and foreclosure practices. One caveat is that within-servicer term variation is limited to two servicers. Servicer A has prohibitions on permanent principal and interest reductions in the seven deals it sponsors but not in the three deals it services for other sponsors. Servicer B has prohibitions on term extensions in the four deals it sponsors but not in the three deals it services for other sponsors.

Table 12 reports the results. Prohibitions on permanent principal and interest reductions are associated with increased foreclosure (13.7 ppt for foreclosure initiation and 7.5 ppt for foreclosure completion) and no change in modification. Prohibitions on term extensions are associated with increased foreclosure (11.0 ppt for foreclosure initiation and 3.9 ppt for foreclosure completion) and slightly decreased modification (-2.0 ppt, which is almost significant at the 5% level). Because I am

[35] Splitting the data into pre- and post-HAMP samples (as I did in my previous analysis) produces samples that are too small to generate meaningful coefficient estimates.

unable to identify modifications that solely extend mortgage terms, this likely underestimates the full impact of term extension prohibitions on modifications. These results are directionally what we would expect. Modification restrictions decrease modifications and increase foreclosures. However the magnitudes, particularly for modification, are too small to explain the overall bias of securitized loans toward foreclosure and away from modification. For example, the -2.0 ppt modification bias applied to the approximately 14% of securitized loans with this term only explains -0.3 ppt of the -2.3 ppt overall modification bias for securitized loans. Similarly, the foreclosure coefficient estimates, combined with the incidence of these terms explain 52% of the foreclosure start bias and 42% of the foreclosure completion bias.

6 Conclusion

This paper's contribution is threefold. First, I propose a novel instrument for jumbo securitization and provide the first well-identified assessment of securitization's impact on foreclosure and modification rates. Before the implementation of HAMP, private securitization made foreclosure more likely (by 8.7 ppt for foreclosure initiation and 5.2 ppt for foreclosure completion) and modification less likely (by 2.3 ppt). Second, I illustrate the mechanisms through which securitization impacts foreclosure and modification, highlighting that incentive differences are more important than contractual prohibitions and the foreclosure bias is more than just a spillover from modification frictions. Third, I compare the effect of securitization on foreclosure and modification before and after HAMP. HAMP mitigated the modification difference between securitized and portfolio loans without having much impact on the bias of securitized loans towards foreclosure.

The bias of securitized loans towards foreclosure and away from modification helps to explain why foreclosure is so prevalent despite its large cost. On a relative basis, securitization increased foreclosure initiation by 12% and foreclosure completion by 39% within six months of first serious delinquency. This does not explain all foreclosures, but it does mean many foreclosures would have been prevented if mortgages were held directly on bank balance sheets instead of being securitized.

The differential treatment of securitized and portfolio loans also serves as an example of how ownership can affect how assets are managed. Despite agreements designed to protect MBS investors from differential servicing treatment, securitized loans were systematically foreclosed more

and modified less. This needs to be part of the debate about the welfare implications of securitized lending both in the mortgage market and elsewhere. Previously, most assessments of mortgage securitization have focused on origination, comparing the benefit of increased funding availability with the cost of lower-quality underwriting. Sub-optimal servicing is another channel through which securitization can be harmful, and this needs to be considered for both regulatory reforms and improvements to private contracts.

HAMP mitigated the differential treatment of privately securitized loans relative to portfolio loans by making modification decisions more consistent. This is a victory for HAMP, but it is a partial victory. HAMP's broad goal was to reduce foreclosure, and securitized loans remained biased towards foreclosure even after HAMP despite more equal modification rates. HAMP's partial success in my analysis is consistent with more general assessments of HAMP. The accepted wisdom seems to be that HAMP increased modification rates but fell short of proponents' overall foreclosure-prevention goals (cf. Agarwal, et al. 2012). In part, this may be a reflection of the complex options servicers have for managing delinquent loans. Modification is not the only alternative to foreclosure, and it is not even used very often. Foreclosures can also be prevented or delayed through short sales, temporary relief arrangements, debt counseling, and more intensive collection techniques. If the policy goal is to reduce foreclosures, these practices should be incentivized just as much as modification.

Finally, a word about welfare. In a first-best world where all loans are optimally managed, a loan's ownership status should not affect foreclosure and modification decisions. Thus, my results reject the hypothesis that mortgage servicing is efficient. However, this does not mean that eliminating securitization (or correcting its biases) would make servicing efficient. Portfolio lending is also subject to principal-agent problems, and externalities (particularly for foreclosure) could drive a wedge between private and social welfare. Properly interpreted, my results show the effect of adding a layer of principal-agent conflict through securitization and highlight a mechanism that has increased foreclosure rates. This understanding is critical for achieving the policy goal of reducing foreclosures, but it does not pin down what that policy goal should be. The private and social costs and benefits of foreclosure and modification are important topics for future research to answer the broader welfare question.

A Modification Algorithm Appendix

The LPS dataset lacks an explicit modification flag but contains enough detailed panel information to identify changes to loan terms over time. My loan-modification algorithm differs in a few details but is essentially the same as the algorithm employed by Adelino, Gerardi, and Willen (2011a). The purpose of the algorithm is to identify changes to loan terms that are consistent with modification and do not have other likely explanations. Some changes are enough to identify a modification on their own. For example, absent errors in the data, interest rate changes to a fixed rate loan must stem from modification. Other changes require confirmatory evidence. For example, a principal reduction could be from a modification or from a prepayment. The size of the reduction, changes in monthly payments, and other simultaneous modifications all inform whether the reduction stems from a modification. In all cases, the loans in question are seriously (60+ days) delinquent at the time of the potential modification, adding to the likelihood that the algorithm is identifying true modifications. The algorithm separately identifies four types of modifications: interest rate reductions, term extensions, principal decreases, and principal increases. These modifications are not mutually exclusive and often take place simultaneously. I consider a loan to be modified if the algorithm flags it with any of the four modification types.

Interest rate reductions

Interest rate reductions are easiest to identify in fixed-rate loans and adjustable-rate loans that are still in their introductory fixed-rate period. For these loans, I define an interest rate reduction as a change that reduces a loan's interest rate to at least 0.5% below the previous month's rate and the loan's origination interest rate.

For adjustable-rate mortgages, I first compute a fully indexed interest rate for each loan in each month using LPS data on the loan's reference index and spread combined with time-series data on the index rates. For example, a loan that references LIBOR and has a spread of 2% would have a fully indexed rate of LIBOR + 2% in any month. I abstract from details on exactly how frequently rates reset and consider any loan to be adjustable if it is past or within 2 months of the end of its introductory period. To be flagged as an interest rate reduction, a loan's interest rate must decrease to at least 0.5% below the previous month's rate, the origination interest rate, and

the fully indexed rate.

Term extensions

To be flagged as a term extension, a loan's remaining term to maturity must increase by at least 20 months or rise above its initial term to maturity. The term change must also be contemporaneous with a monthly payment decrease, principal increase, or explicit loss mitigation flag in the data.

Principal decreases

To be flagged as a principal decrease, the mortgage must have had outstanding principal of at least $25K in the previous month, and the principal balance must have decreased by between 10% and 30% and be accompanied by a payment decrease or term extension. The 10-30% range is used to differentiate modifications from scheduled principal decreases and prepayments. Adelino, Gerardi, and Willen (2011a) experiment with the 30% cutoff and find that results are not sensitive to its exact value.

Principal increases

To be flagged as a principal increase, principal must increase by at least 1% (0.5% for option ARM mortgages) and be accompanied by either a payment increase or a term length decrease.

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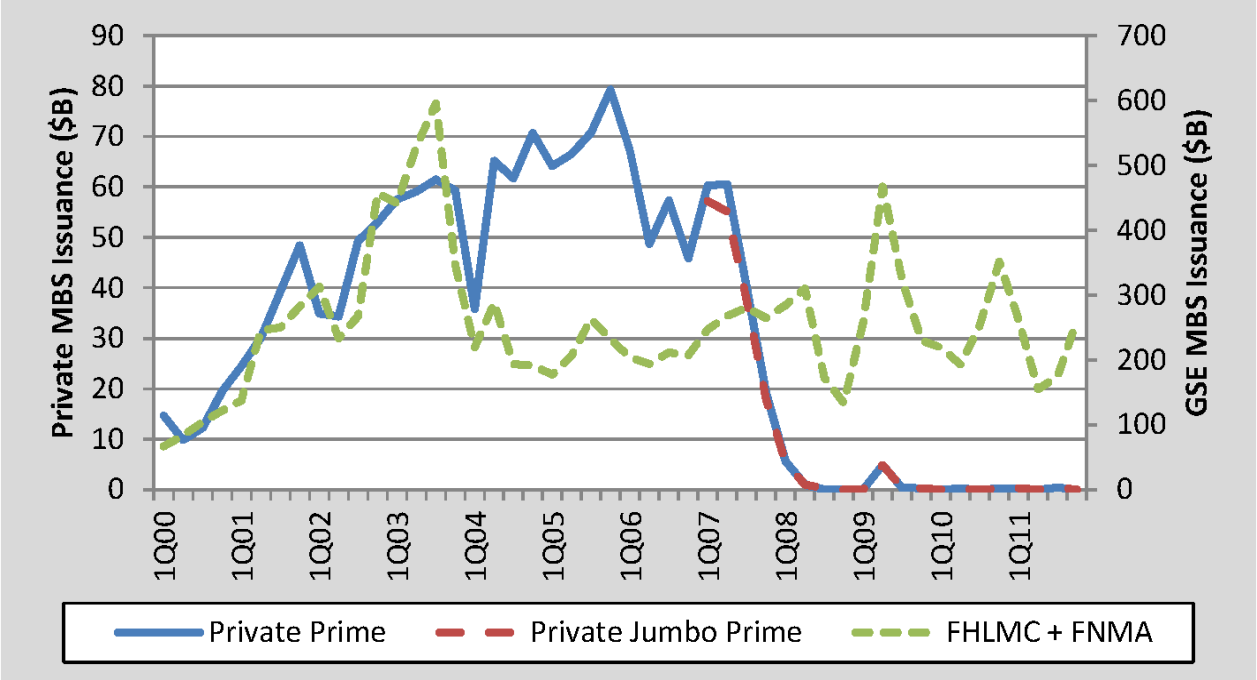

Figure 1: MBS Issuance. Source: Inside Mortgage Finance.

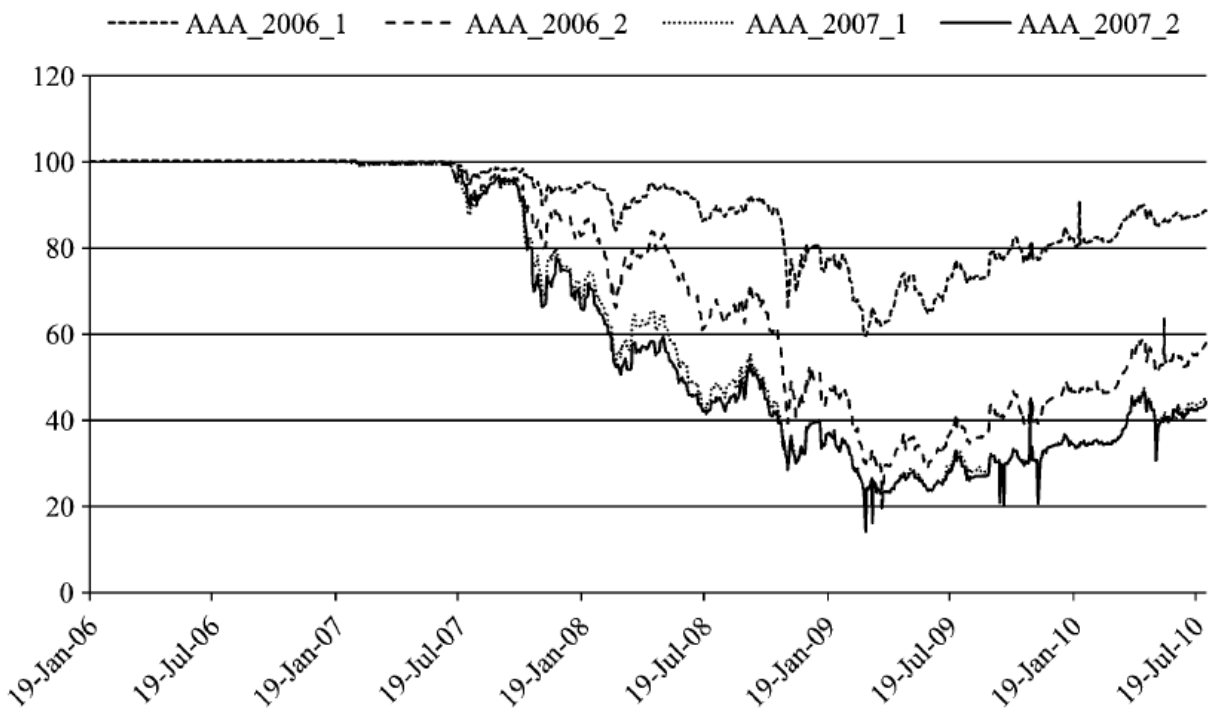

Figure 2: ABX Index. Source: Markit ABX.HE price data. Figure reproduced from Stanton and Wallace (2007, Figure 1).

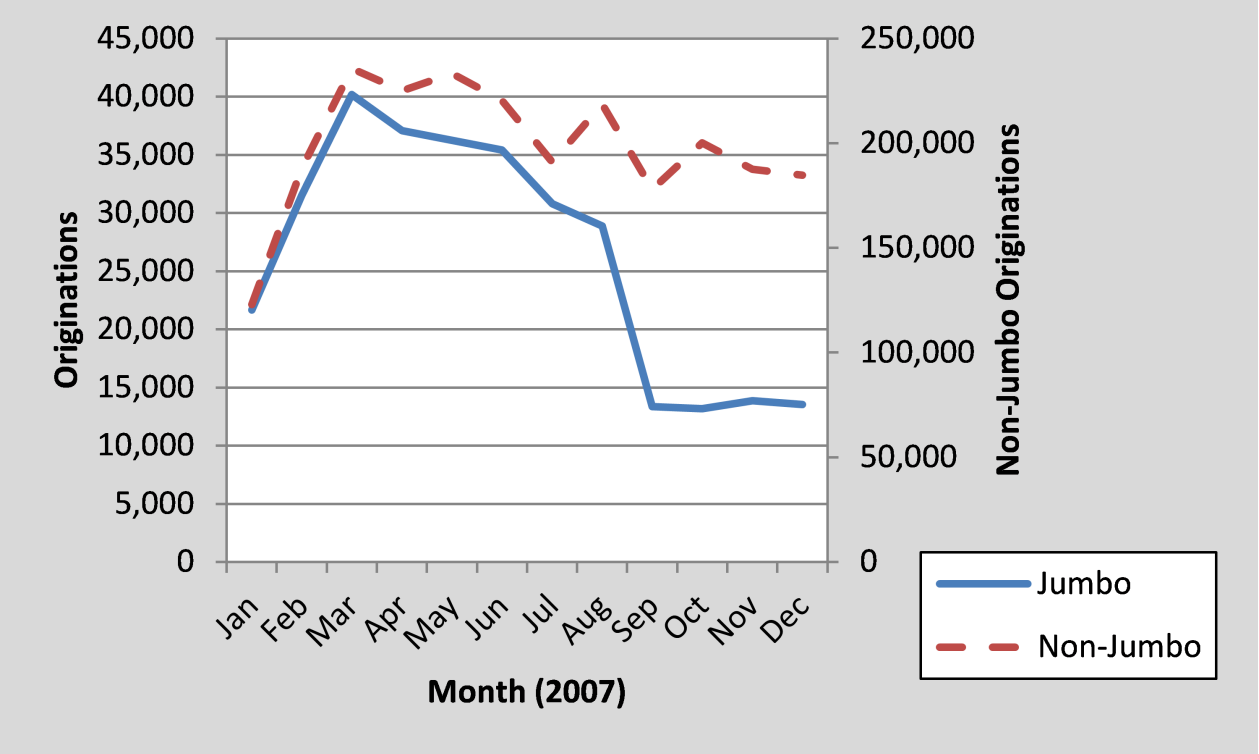

Figure 3: Mortgage Originations. Sample loans by origination month and size.

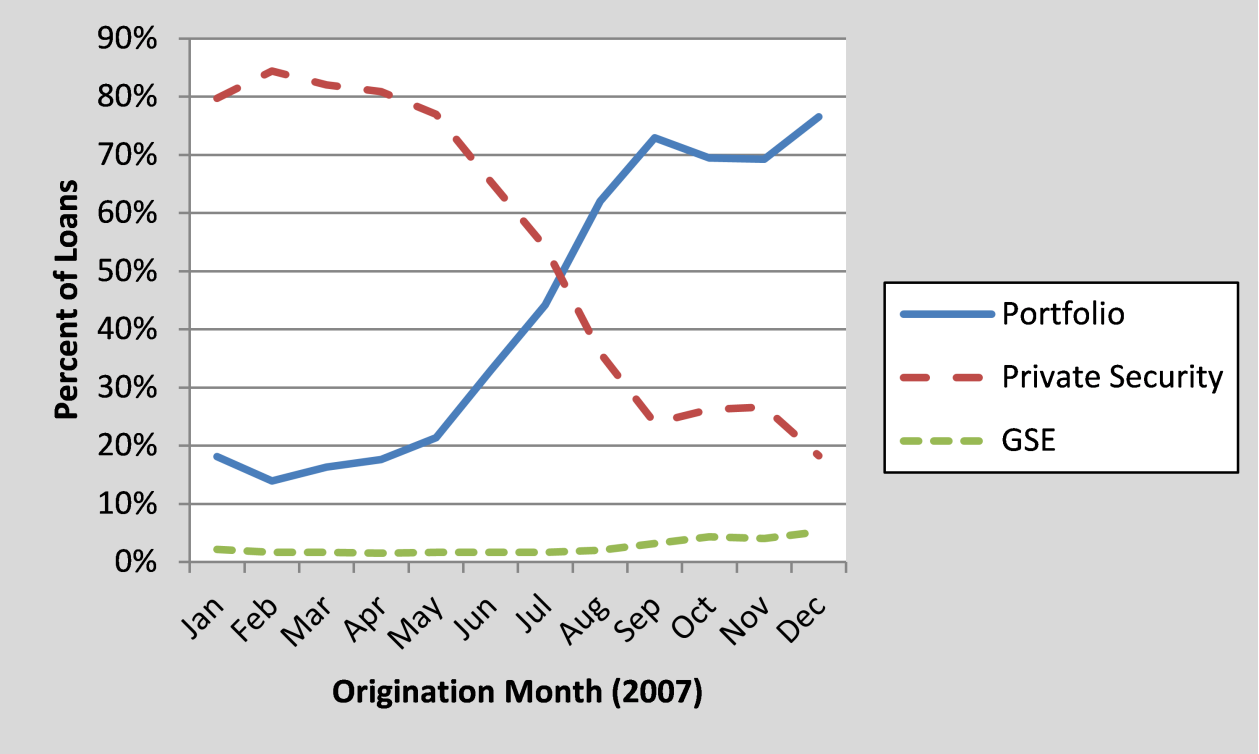

Figure 4: Jumbo Securitization. Percent of jumbo sample loans with portfolio, private security and GSE ownership status six months after origination by origination month.

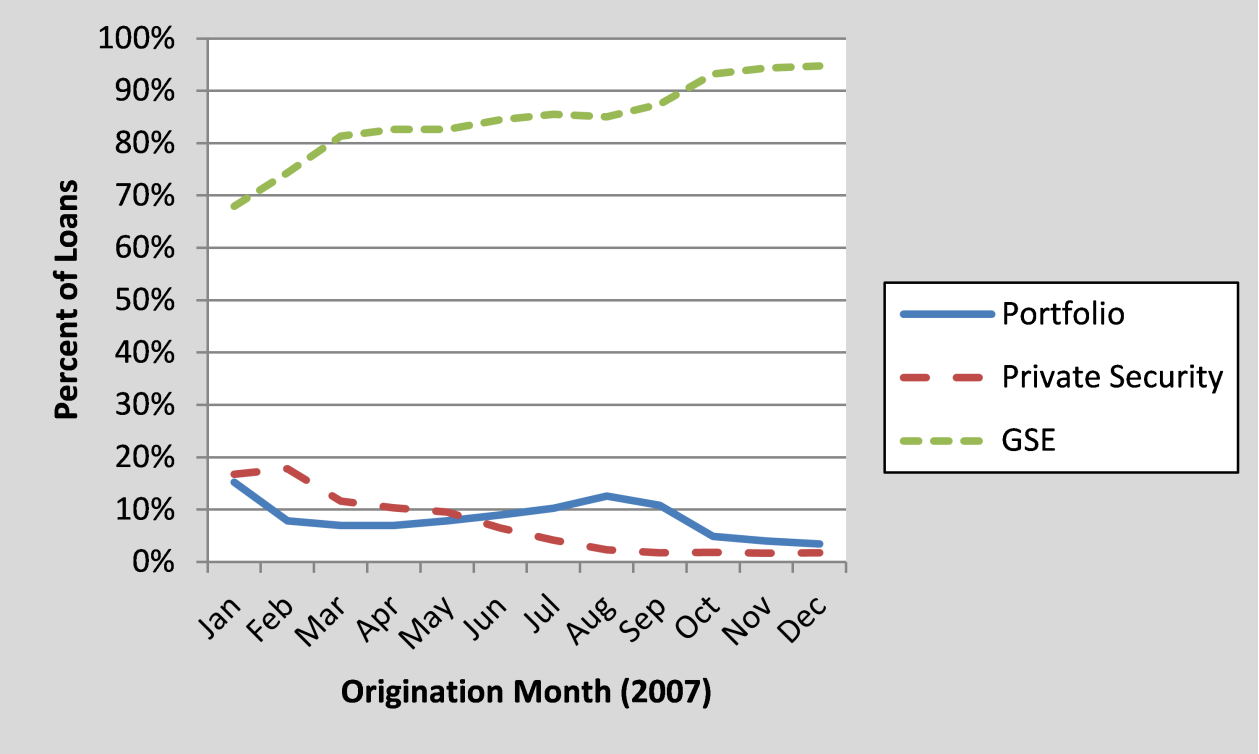

Figure 5: Non-Jumbo Securitization. Percent of non-jumbo sample loans with portfolio, private security and GSE ownership status six months after origination by origination month.

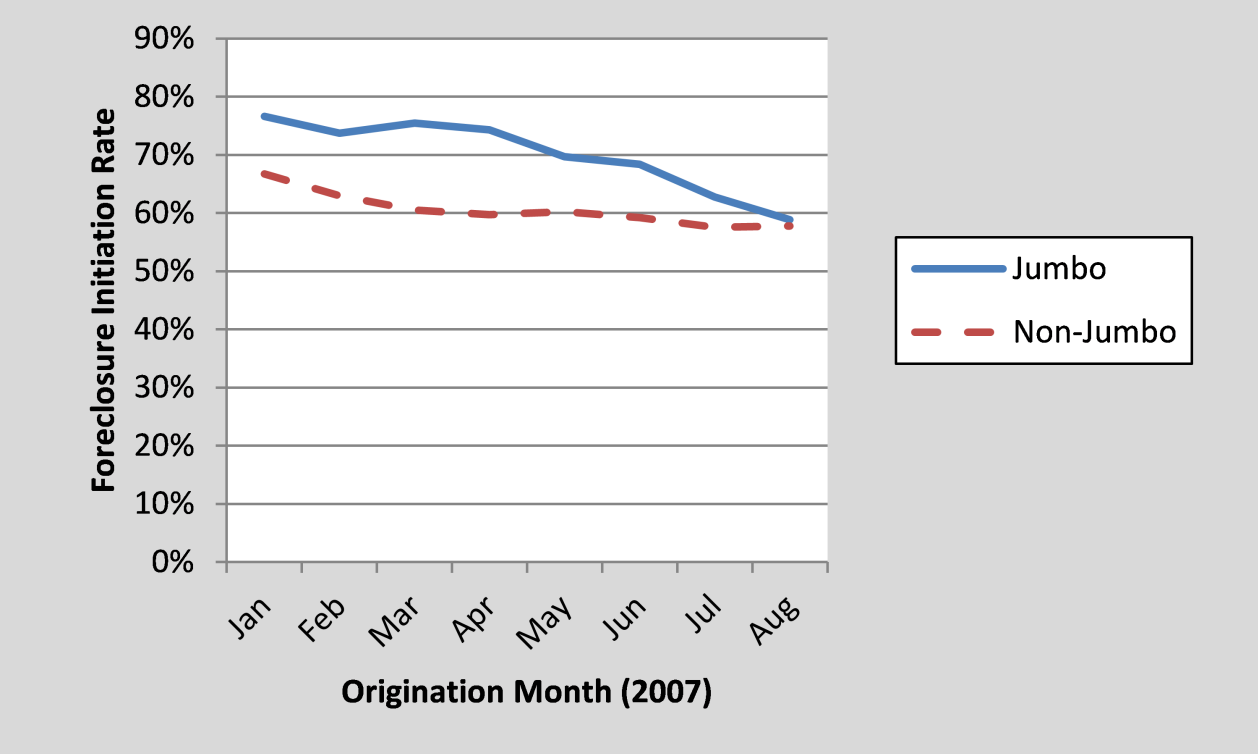

Figure 6: Foreclosure Initiation Rates. Percent of pre-HAMP serious (60+ day) delinquencies for which foreclosure is initiated within six months by loan size and origination month.

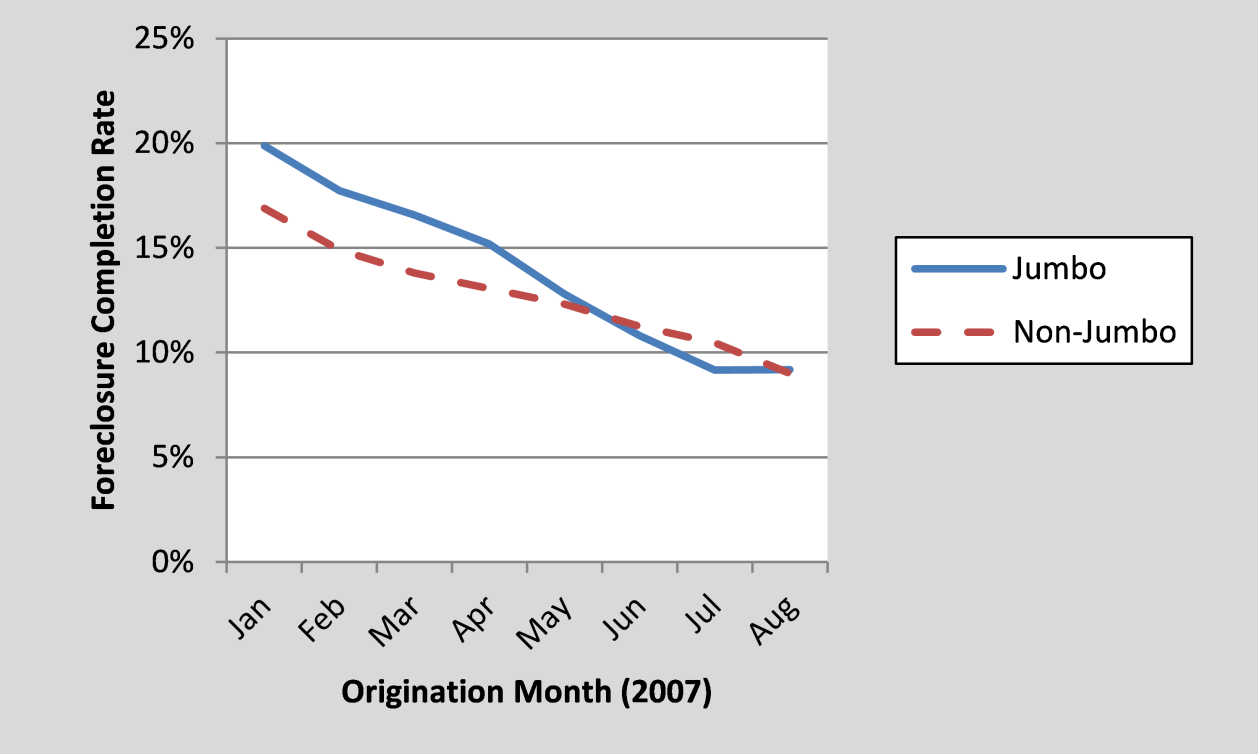

Figure 7: Foreclosure Completion Rates. Percent of pre-HAMP serious (60+ day) delinquencies for which foreclosure is completed within six months by loan size and origination month.

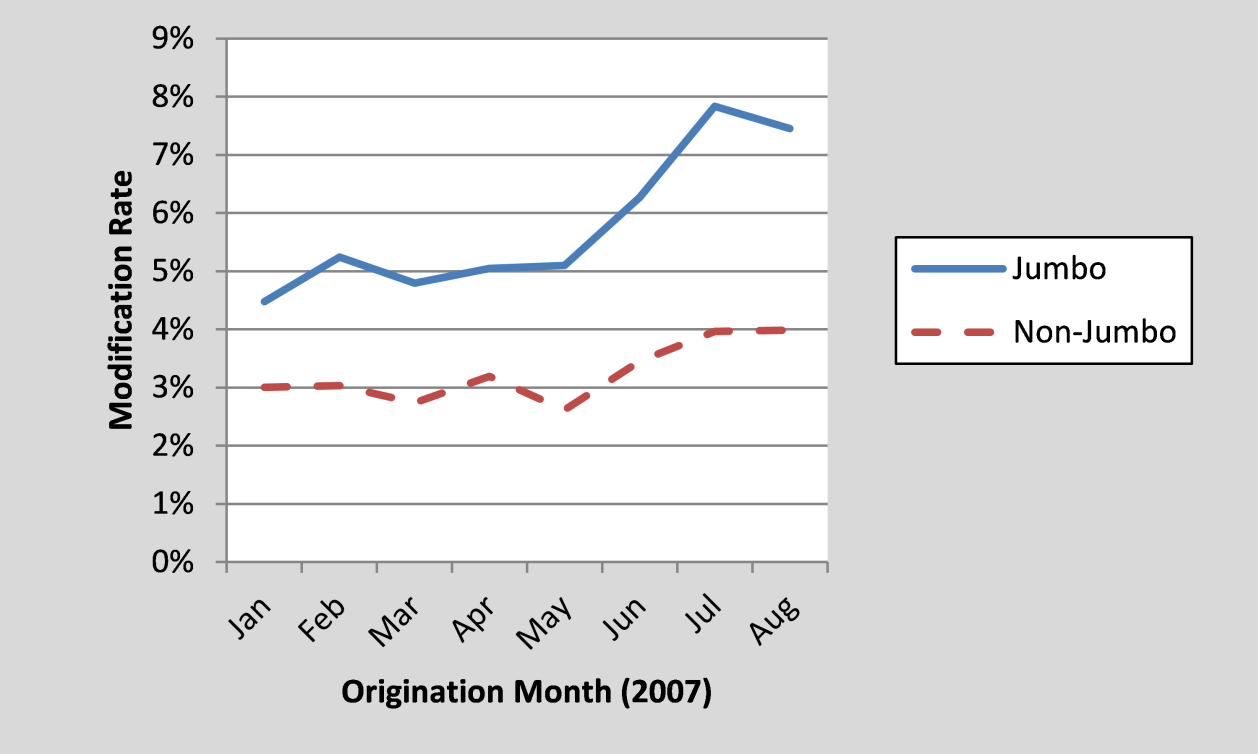

Figure 8: Modification Rates. Percent of pre-HAMP serious (60+ day) delinquencies that are modified within six months by loan size and origination month.

Table 1: Data Summary

Data comes from LPS. The analyzed sample is first-lien conventional loans originated between January and August of 2007 that enter the dataset within 4 months of origination, have orgination FICO scores between 620 and 850, have origination loan-to-value ratios of less than 1.5, have terms of 15, 20, or 30 years, are located in U.S. MSAs outside of Alaska and Hawaii, and are not transferred to a non-LPS servicer. Pre-HAMP delinquencies are loans that become 60+ days delinquent within six months of origination. Post-HAMP delinquencies are loans that become 60+ days delinquent for the first time in 2010 or 2011. Jumbo loans are larger than the GSE conforming limit ($417K). Portfolio loans are non-securitized. Private security loans are securitized in non-GSE securities. GSE loans are predominantly FHLMC and FNMA but also include some GNMA and Federal Home Loan Bank loans. Delinquency is 60+ day delinquency. Foreclosure start is the referral of a mortgage to an attorney to initiate foreclosure proceedings. Foreclosure is foreclosure completion identified by post-sale foreclosure or REO status. Modifications are identified based on observed changes to loan terms. Redefault is a return to 60+ day delinquency after a modification cures an initial delinquency.

	All Loans		**Pre-HAMP Delinq.**		**Post-HAMP Delinq.**	
	Jumbo	**Non-Jumbo**	**Jumbo**	**Non-Jumbo**	**Jumbo**	**Non-Jumbo**
Number	263,544	1,644,346	15,985	61,242	25,555	132,275
Size (mean	$691,219	$210,294	$653,155	$230,861	$655,002	$222,327
FICO (mean)	733	726	700	686	722	708
LTV (mean)	0.73	0.72	0.79	0.81	0.75	0.77
Ownership						
Portfolio	27%	9%	33%	16%	23%	10%
Private Security	70%	9%	64%	19%	73%	13%
GSE	2%	81%	2%	65%	2%	77%
Delinquency						
w/i 1 year	6%	4%				
w/i 5 years	36%	27%				
Foreclosure start						
w/i 6 months			70%	60%	35%	49%
w/i 12 months			81%	72%	47%	59%
Foreclosure						
w/i 6 months			14%	12%	4%	7%
w/i 12 months			37%	29%	11%	16%
Modification						
w/i 6 months			6%	3%	8%	7%
w/i 12 months			10%	7%	17%	16%
Redefault						
w/i 6 months			23%	12%	3%	3%
w/i 12 months			30%	16%	5%	4%

Table 2: Securitization by Age - January Jumbo Loans

Data includes all jumbo sample loans that were originated in January of 2007. Age is months since origination. Loans are added to the LPS data over time and can change ownership. Number of loans and percent of loans privately securitized is reported by age.

Age (months)	Loans	Private Securitization (%)
0	12,715	12%
1	18,208	43%
2	19,069	66%
3	20,338	75%
4	21,023	78%
5	21,558	79%
6	21,811	79%

Table 3: OLS Regressions (Pre-HAMP)

The dependent variables are indicators for foreclosure start, foreclosure completion, and modification within six months of first serious (60+ days) delinquency. All regressions are OLS, which implies linear probability models. Private security is an indicator for private securitization as of six months after origination. The other independent variables are other loan characteristics. The regressions analyze jumbo loans in my sample that became seriously (60+ days) delinquent within six months of origination. Clustered (by MSA) standard errors are in parentheses. * represents 5% significance, ** represents 1% significance, *** represents 0.1% significance.

	(1) OLS	(2) OLS	(3) OLS
	Foreclose Start	**Foreclose**	**Modification**
Private Security	0.0385***	0.0219**	-0.0212***
	(0.0106)	(0.00821)	(0.00451)
FICO >= 680	0.0873***	0.0324***	-0.0401***
	(0.00832)	(0.00805)	(0.00538)
LTV Ratio	0.630***	0.0462	0.0202
	(0.0513)	(0.0405)	(0.0472)
LTV = 0.8	0.0315**	0.0178*	-0.00917
	(0.0105)	(0.00728)	(0.00477)
log(Origination Amount)	-0.000346	-0.0279*	-0.00790
	(0.0132)	(0.0108)	(0.00551)
Origination Interest Rate	0.00313	-0.00127	-0.0156***
	(0.00217)	(0.00135)	(0.00266)
Fixed Interest Rate	-0.0946***	-0.0671***	0.0309***
	(0.0125)	(0.00944)	(0.00502)
Term = 15 Years	-0.180**	-0.0486	-0.0172
	(0.0679)	(0.0357)	(0.0280)
Term = 20 Years	-0.233	0.00841	-0.0314*
	(0.158)	(0.107)	(0.0151)
Insurance	-0.0908***	0.00959	0.0102
	(0.0182)	(0.00887)	(0.00845)
Refinancing Loan	-0.0750***	-0.0383***	0.00700
	(0.00792)	(0.00698)	(0.00514)
Option ARM	0.00917	0.00622	0.110***
	(0.0116)	(0.00736)	(0.00660)
Single Family Home	0.00614	-0.0130	-0.00127
	(0.0105)	(0.00913)	(0.00522)
Primary Residence	0.00918	-0.000736	0.00205
	(0.0161)	(0.0108)	(0.00794)
No Income Documentation	0.000117	0.00901	-0.0159**
	(0.0137)	(0.00987)	(0.00573)
Low Income Documentation	-0.0846***	-0.0270***	0.0155**
	(0.0116)	(0.00645)	(0.00478)
Constant	0.0391	0.465**	0.441***
	(0.194)	(0.168)	(0.130)
Delinquency Month FE	Yes	Yes	Yes
Origination Month FE	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	No	No	No

Table 4: IV Regressions (Pre-HAMP)

The dependent variables are indicators for foreclosure start, foreclosure completion, and modification within six months of first serious (60+ days) delinquency. The regressions estimate linear probability models for these indicators using the interaction of origination-month indicators and an indicator for jumbo status as instruments for private securitization status six months after origination. All observable loan characteristics shown in Table 3 are included as unreported controls. The regressions with non-jumbo loans also include an indicator for jumbo status and interactions between jumbo status and all control variables except MSA fixed effects and origination-month fixed effects. The regressions analyze jumbo (and where indicated non-jumbo) loans in my sample that became seriously (60+ days) delinquent within six months of origination. Clustered (by MSA) standard errors are in parentheses. * represents 5% significance, ** represents 1% significance, *** represents 0.1% significance.

	(1) **IV**	**(2)** **IV**	**(3)** **IV**	**(4)** **IV**	**(5)** **IV**	**(6)** **IV**
	Foreclose Start	**Foreclose**	**Mod.**	**Foreclose Start**	**Foreclose**	**Mod.**
Private Security	0.0800***	0.0467***	-0.0427***	0.0868***	0.0523***	-0.0229*
	(0.0158)	(0.0124)	(0.00942)	(0.0183)	(0.0151)	(0.0107)
Loan Characteristic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Origination Month FE	No	No	No	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	No	No	No	Yes	Yes	Yes

Table 5: Robustness Checks

Regressions are the same as columns 4-6 of Table 4 except where noted. Columns 1-3 of Panel A consider foreclosure and modification within twelve months instead of six months. Columns 4-6 of Panel Ainclude only loans originated between May and July of 2007. Columns 1-3 of Panel B estimate bivariate probit models on jumbo loans and exclude MSA and origination-month fixed effects. Columns 4-6 of Panel B omit loan characteristic control variables. Clustered (by MSA) standard errors are in parentheses. * represents 5% significance, ** represents 1% significance, *** represents 0.1% significance.

A. 12-month analysis window and restricted origination-month sample

	(1) IV	**(2) IV**	**(3) IV**	**(4) IV**	**(5) IV**	**(6) IV**
	Foreclose Start (12 mos.)	**Foreclose (12 mos.)**	**Mod. (12 mos.)**	**Foreclose Start (6 mos.)**	**Foreclose (6 mos.)**	**Mod. (6 mos.)**
Private Security	0.0717***	0.0603**	-0.0496***	0.0743	0.0862**	-0.0417
	(0.0171)	(0.0191)	(0.0143)	(0.0407)	(0.0288)	(0.0274)
Loan Characteristic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Origination Month FE	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	May-Jul	May-Jul	May-Jul
Include Non-Jumbo Loans	Yes	Yes	Yes	Yes	Yes	Yes

B. Bivariate probit regressions and regressions without loan characteristic controls

	(1) Bivarite Probit IV	**(2) Bivarite Probit IV**	**(3) Bivarite Probit IV**	**(4) IV**	**(5) IV**	**(6) IV**
	Foreclose Start (6 mos.)	**Foreclose (6 mos.)**	**Mod. (6 mos.)**	**Foreclose Start (6 mos.)**	**Foreclose (6 mos.)**	**Mod. (6 mos.)**
Private Security	0.0677***	0.0409**	-0.0372***	0.176***	0.0479**	-0.0327*
	(0.0160)	(0.0137)	(0.0079)	(0.0254)	(0.0178)	(0.0146)
Loan Characteristic Controls	Yes	Yes	Yes	No	No	No
Delinquency Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Origination Month FE	No	No	No	Yes	Yes	Yes
MSA FE	No	No	No	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	No	No	No	Yes	Yes	Yes

Table 6: Modification Details (Pre-HAMP)

All regressions are conditional on a loan being modified. The dependent variables in Panel A are indicators for interest rate modification, term modification, principal decrease and principal increase. Panel A regressions estimate linear probability models for these indicators. The dependent variables in Panel B are net changes to interest rates, terms, principal, and monthly payments. Private securitization status six months after origination is instrumented with interaction of origination-month indicators and an indicator for jumbo status. All observable loan characteristics shown in Table 3 are included as unreported controls. The regressions also include an indicator for jumbo status and interactions between jumbo status and all control variables except MSA fixed effects and origination-month fixed effects. The regressions analyze jumbo and non-jumbo loans in my sample that became seriously (60+ days) delinquent within six months of origination and are modified within six months of becoming seriously delinquent. Clustered (by MSA) standard errors are in parentheses. * represents 5% significance, ** represents 1% significance, *** represents 0.1% significance.

A. Type of modification

	(1) IV	**(2) IV**	**(3) IV**	**(4) IV**
	Interest Modification	**Term Modification**	**Principal Decrease**	**Principal Increase**
Private Security	-0.00477	-0.0263	-0.0790*	0.135*
	(0.0646)	(0.0402)	(0.0308)	(0.0563)
Loan Characteristic Controls	Yes	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes	Yes
Origination Month FE	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	Yes	Yes	Yes	Yes

B. Net changes

	(1) IV	**(2) IV**	**(3) IV**	**(4) IV**
	Interest Change (%)	**Term Change (mos.)**	**Principal Change (%)**	**Payment Change (%)**
Private Security	0.225	3.502	1.55	1.32
	(0.293)	(9.322)	(0.893)	(2.75)
Loan Characteristic Controls	Yes	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes	Yes
Origination Month FE	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	Yes	Yes	Yes	Yes

Table 7: Modification Effectiveness (Pre-HAMP)

All regressions are conditional on a loan being cured of initial delinquency with a loan modification. The dependent variable is an indicator for redefault, defined as a return to 60+ day delinquent status within twelve months of modification. The regressions estimate linear probability models for this indicator. For the IV regressions, private securitization is instrumented with the interaction of origination-month indicators and an indicator for jumbo status. Indicators for modification type are included where indicated. All observable loan characteristics shown in Table 3 are included as unreported controls. The regressions with non-jumbo loans include an indicator for jumbo status and interactions between jumbo status and all control variables except MSA fixed effects and origination-month fixed effects. The regressions analyze jumbo (and where indicated non-jumbo) loans in my sample that became seriously (60+ days) delinquent within six months of origination and are cured through modification within six months of becoming seriously delinquent. Clustered (by MSA) standard errors are in parentheses. * represents 5% significance, ** represents 1% significance, *** represents 0.1% significance.

	(1) **OLS**	**(2)** **OLS**	**(3)** **IV**	**(4)** **IV**
	Redefault	**Redefault**	**Redefault**	**Redefault**
Private Security	0.124***	0.110**	0.0123	0.00144
	(0.0364)	(0.0367)	(0.0608)	(0.0621)
Interest Mod		-0.0895		-0.101***
		(0.0670)		(0.0300)
Term Mod		-0.0810		0.0762
		(0.249)		(0.0836)
Principal Decrease		-0.362		-0.347**
		(0.298)		(0.134)
Principal Increase		0.0699		0.0959
		(0.251)		(0.0887)
Loan Characteristic Controls	Yes	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes	Yes
Origination Month FE	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	No	No	Yes	Yes

Table 8: IV Regressions (Post-HAMP)

Regressions are the same as columns 4-6 of Table 4 except that the sample is now post-HAMP delinquencies. The dependent variables are indicators for foreclosure start, foreclosure completion, and modification within six months of first serious (60+ days) delinquency. The regressions estimate linear probability models for these indicators using the interaction of origination-month indicators and an indicator for jumbo status as instruments for private securitization status six months after origination. All observable loan characteristics shown in Table 3 are included as unreported controls. The regressions also include an indicator for jumbo status and interactions between jumbo status and all control variables except MSA fixed effects and origination-month fixed effects. The regressions analyze jumbo and non-jumbo loans in my sample that became seriously (60+ days) delinquent for the first time in 2010 or 2011. Clustered (by MSA) standard errors are in parentheses. * represents 5% significance, ** represents 1% significance, *** represents 0.1% significance.

	(1) **OLS**	**(2)** **OLS**	**(3)** **OLS**
	Foreclose Start	**Foreclose**	**Modification**
Private Security	0.0906*** (0.0168)	0.0178** (0.00663)	-0.0108 (0.0109)
Loan Characteristic Controls	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes
Origination Month FE	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	Yes	Yes	Yes

Table 9: Modification Details (Post-HAMP)

All regressions are conditional on a loan being modified. The dependent variables in Panel A are indicators for interest rate modification, term modification, principal decrease and principal increase. Panel A regressions estimate linear probability models for these indicators. The dependent variables in Panel B are net changes to interest rates, terms, principal, and monthly payments. Private securitization status six months after origination is instrumented with interaction of origination-month indicators and an indicator for jumbo status. All observable loan characteristics shown in Table 3 are included as unreported controls. The regressions also include an indicator for jumbo status and interactions between jumbo status and all control variables except MSA fixed effects and origination-month fixed effects. The regressions analyze jumbo and non-jumbo loans in my sample that became seriously (60+ days) delinquent for the first time in 2010 or 2011 and are modified within six months of becoming seriously delinquent. Clustered (by MSA) standard errors are in parentheses. * represents 5% significance, ** represents 1% significance, *** represents 0.1% significance.

A. Type of modification

	(1) **IV**	**(2)** **IV**	**(3)** **IV**	**(4)** **IV**
	Interest Modification	**Term Modification**	**Principal Decrease**	**Principal Increase**
Private Security	0.0478	-0.329***	-0.0266	0.272***
	(0.0355)	(0.0466)	(0.0356)	(0.0496)
Loan Characteristic Controls	Yes	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes	Yes
Origination Month FE	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	Yes	Yes	Yes	Yes

B. Net changes

	(1) **IV**	**(2)** **IV**	**(3)** **IV**	**(4)** **IV**
	Interest Change (%)	**Term Change (mos.)**	**Principal Change (%)**	**Payment Change (%)**
Private Security	-0.111	-93.32***	0.678	2.14
	(0.181)	(9.602)	(0.962)	(2.01)
Loan Characteristic Controls	Yes	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes	Yes
Origination Month FE	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	Yes	Yes	Yes	Yes

Table 10: Summary of PSA Terms

Sample consists of all prime non-agency MBS deals in excess of $1B closed between January and August of 2007. 37 MBS deals with a total of value of $70B meet this criteria. These deals represent 48% of total January - August 2007 prime non-agency MBS volume. For deals with multiple servicing and purchase agreements (e.g., deals involving multiple originators or sponsors), the sample includes the agreements relevant to the most loans. The sample includes nine sponsors and seven servicers.

	Number of PSAs	Percent of PSAs
Representations and Warranties:		
Early payment default:	0	0%
Loan schedule accurate:	37	100%
Current:	31	84%
Limited past delinquency:	22	59%
Servicing:		
General Responsibility:		
Accepted practices	37	100%
Equivalent to portfolio loan	25	68%
Best interest of certificateholders	1	3%
Fannie Mae Servicing Guide	14	38%
Obligation to foreclose	37	100%
Foreclosure reimbursement	37	100%
Obligation to modify	0	0%
Obligation to consider modification	7	19%
Modification reimbursement:		
From trust	0	0%
From mortgagor	8	22%
Payment advances:		
Must advance delinquent monthly payments	37	100%
If principal or interest deferred, must advance difference	22	59%
Modification restrictions:		
In default or default is forseeable	23	62%
Must expect modification value to exceed foreclosure proceeds	8	22%
May not decrease principal or permanenty decrease interest rate	8	22%
May not extend term beyond term of certificates	1	3%
May not extend term beyond maturity of last-maturing loan	4	11%
Ammendment:		
w/o consent		
cure/correct terms	37	100%
alter without adversely affecting certificateholders	12	32%
Required consent for other changes		
Overall	17	46%
In each class	0	0%
In affected classes	26	70%
In differentially impacted classes	0	0%
In adversely impacted classes	18	49%
Prohibition on decreasing or delaying payments w/o universal consent	37	100%

Table 11: PSA-Linked Loan Sample

Data comes from Core Logic loan data linked to my sample of prime non-agency PSAs. The analyzed sample is jumbo (over $417K) first-lien conventional loans that have orgination FICO scores between 620 and 850, have origination loan-to-value ratios of less than 1.5, have terms of 15, 20, or 30 years, are located in U.S. MSAs outside of Alaska and Hawaii. Pre-HAMP delinquencies are loans that become 60+ days delinquent within six months of origination. Post-HAMP delinquencies are loans that become 60+ days delinquent for the first time in 2010 or 2011. Delinquency is 60+ day delinquency. Foreclosure start is the referral of a mortgage to an attorney to initiate foreclosure proceedings. Foreclosure is foreclosure completion identified by post-sale foreclosure or REO status. Modifications are identified based on observed changes to loan terms.

		Delinquent Loans		
	All Loans	**Total**	**Pre-HAMP**	**Post HAMP**
Number	85,036	18,049	918	6,959
Size (mean)	$707,542	$671,927	$705,395	$668,457
FICO (mean)	742	722	708	730
LTV (mean)	71	75	77	75
Delinquency				
w/i 1 year	1.1%			
w/i 5 years	20.9%			
Foreclosure start				
w/i 6 months		51.7%	73.1%	40.5%
w/i 12 months		60.6%	80.1%	50.8%
Foreclosure				
w/i 6 months		6.7%	18.6%	6.4%
w/i 12 months		20.6%	41.8%	18.6%
Modification				
w/i 6 months		6.3%	1.1%	8.6%
interest decrease		*5.4%*	*0.3%*	*7.9%*
term extension				
principal decrease		*0.0%*	*0.0%*	*0.0%*
principal increase		*2.5%*	*1.1%*	*2.9%*
w/i 12 months		13.3%	4.8%	16.5%

Table 12: PSA Term Regressions

The dependent variables are indicators for foreclosure start, foreclosure completion, and modification within six months of first serious (60+ days) delinquency. All regressions are OLS, which implies linear probability models. The reported independent variables are indicators for the presence of these terms. All observable loan characteristics in previous regressions are included as unreported controls. The regressions analyze jumbo loans in my sample that became seriously (60+ days) delinquent within six months of origination. Clustered (by MSA) standard errors are in parentheses. * represents 5% significance, ** represents 1% significance, *** represents 0.1% significance.

	(1) **OLS**	**(2)** **OLS**	**(3)** **OLS**
	Foreclose Start	**Foreclose**	**Mod.**
Permanent P&I Reductions Prohibited	0.137***	0.0745***	0.00108
	(0.0245)	(0.0131)	(0.0105)
Term Extensions Limited	0.110***	0.0392**	-0.0195
	(0.0283)	(0.0126)	(0.0101)
Loan Characteristic Controls	Yes	Yes	Yes
Servicer FE	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes
Origination Month FE	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes
Number of Deals	37	37	37
Number of Delinquent Loans	18,049	18,049	18,049