The Effect of Mortgage Securitization on Foreclosure and Modification

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Abstract

Did securitization exacerbate the foreclosure crisis? I exploit the third quarter 2007 freeze of private mortgage securitization as an exogenous change in the probability of securitization for jumbo mortgages. The unanticipated freeze left banks holding mortgages that were intended for securitization at the time they were originated. Loans originated shortly before the freeze are similar to loans originated earlier in the year but were significantly less likely to be securitized. Using origination month as an instrument for securitization, I find that private securitization substantially increases foreclosure probability and decreases modification probability for delinquent loans. My estimates imply that over 500,000 of the 4.4 million foreclosures experienced since the start of the financial crisis were caused by securitization.

1 Introduction

Since the start of the financial crisis, 4.4 million U.S. homes have been foreclosed, inflicting losses on mortgage investors, causing turmoil in the lives of mortgagors, and damaging surrounding communities. Roughly half of these foreclosures stemmed from privately securitized mortgages, prompting policy makers and economists to worry that securitization impedes mortgage modification and leads to unnecessary foreclosures. Unfortunately, evaluating the impact of securitization on foreclosures is challenging because securitization is an endogenous decision, and securitized mortgages likely differ from mortgages held on bank balance sheets even after controlling for observable characteristics.

I estimate the causal effect of securitization on foreclosure and modification by exploiting the sudden and unexpected freeze of private mortgage securitization in the third quarter of 2007.[1] Jumbo mortgages originated shortly before the freeze were disproportionately stuck on bank balance sheets even though many of them were intended for private securitization at the time they were originated. Because the freeze was unanticipated, loans originated shortly before the freeze are similar to loans originated earlier in 2007. I further control for changes to the lending environment over time using a difference-in-differences methodology with non-jumbo loans, which are primarily securitized by Fannie Mae and Freddie Mac and were unaffected by the private securitization freeze.

The results are striking. Relative to portfolio loans held directly on bank balance sheets, private securitization increases the probability of foreclosure initiation within six months of a mortgage's first serious delinquency by 8.0 ppt (12% of the mean foreclosure initiation rate) . Similarly, securitization increases the probability of foreclosure completion by 4.7 ppt (35% of the mean) and decreases the probability of modification by 3.6 ppt (69% of the mean). My instrumental variables (IV) strategy is critical for estimating these effects. For foreclosure initiation and completion, IV estimates are twice as large as corresponding ordinary least squares (OLS) estimates. These results suggest that securitization significantly exacerbated the foreclosure crisis and needs to be considered in any policy response. Taken at face value, they imply that over 500,000 of the 4.4

[1] Purnanandam (2011) also documents and exploits loans being stuck on bank balance sheets in 2007. Purnanandam exploits cross sectional differences in bank exposure to originate-to-distribute lending to estimate the impact of securitization on origination quality. In contrast, I exploit time series variation in loan origination to estimate the impact of securitization on mortgage servicing.

million foreclosures experienced since the start of the financial crisis were caused by securitization.

In part motivated by the high foreclosure rates of privately securitized mortgages, the federal government enacted the Home Affordable Modification Program (HAMP) in February of 2009 to incentivize modifications and make modification practices more uniform across mortgages. My methodology does not provide a way to test whether HAMP succeeded in reducing foreclosures, but I can test the uniformity of foreclosure and modification practices across securitized and portfolio loans before and after HAMP. I find that private securitization increased foreclosure probability and decreased modification probability throughout the 2007 to 2011 time period, suggesting that HAMP did little to make foreclosure and modification practices more consistent across securitized and portfolio loans.

In addition to their relevance for foreclosure policy, these results speak to the 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.[2] 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 foreclosures (cf., Campbell, Giglio, and Pathak, 2011).

Securitization's impact on foreclosures and modifications illustrates one of the central precepts of corporate finance: separation of ownership and control matters. The importance of ownership structure and managerial incentives is universally accepted as a basic premise.[3] 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 impossible. My setting offers a rare laboratory for well-identified assessment of the effects of adding a layer of delegated management through securitization.

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

[2] 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.

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

for analyzing 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 pending 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 non-contractible residual rights. These residual rights are universally held by mortgage servicers, effectively making the servicer the "owner" of a mortgage even though the trust holds the legal title and most of the cash flow rights.[4] 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 to 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 examination of securitization contracts, I find that servicing agreements do little to overcome 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. 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.

[4] Grossman and Hart (1986) define ownership as control of residual rights.

2 Existing Evidence

Posner and Zingales (2009) were early advocates of the view that securitization impedes loan modifications and causes foreclosures. Three previous studies test this hypothesis by regressing foreclosure and modification probability on securitization status using OLS or logit regressions. Piskorski, Seru, and Vig (2010) consider mortgages originated in 2005 and 2006 that became seriously delinquent, defined as a delinquency of at least 60 days. Compared to portfolio mortgages, privately securitized mortgages had foreclosure rates that were 4-7 ppt higher after controlling for observable loan characteristics.[5] Using a similar approach, Agarwal, et al. (2011) estimate that privately securitized mortgages that became seriously delinquent in 2008 were 4.2 ppt less likely to be renegotiated within 6 months relative to comparable portfolio mortgages.[6] In contrast, Adelino, Gerardi, and Willen (2011a) find that differences in twelve-month loan modification rates between privately securitized mortgages and comparable portfolio mortgages were small for mortgages that were originated after 2004 and became seriously delinquent by September of 2007.[7] The conflicting results of these papers appear to be mainly a function of the outcome variables and samples analyzed.[8]

The main limitation of the existing evidence is that causal interpretation requires the assumption that securitization status is randomly assigned conditional on observed loan characteristics. This is a problematic assumption because origination and securitization are endogenous decisions, and both are made based on a larger set of information than the observed characteristics econometricians can control for, thereby introducing omitted variable bias.

Can we at least determine the direction of the bias? The answer is no. First, privately securitized loans could be lower or higher quality than observably similar portfolio loans. Originator adverse selection and screening moral hazard push in the direction of securitized loans being

[5] See Table 3 of Piskorski, Seru, and Vig (2010).

[6] See Table 3, Panel A of Agarwal et al. (2011).

[7] Adelino, Gerardi, and Willen (2011a) estimate that if anything, privately securitized loans were modified slightly more frequently (0.6 to 2.1 ppt) than portfolio loans. See Panel B of their Table VI.

[8] Securitization has a larger impact on foreclosure than it does on modification. I find this in my analysis, and Agarwal, et al. (2011) find the same thing in their Appendix A. This explains why Piskorski, Seru, and Vig (2010) find large foreclosure effects while Adelino, Gerardi, and Willen (2011a) do not find significant modification effects in a largely equivalent sample. Agarwal, et al. (2011) focus on a later time period than the other two papers, which may explain why their modification results differ from Adelino, Gerardi, and Willen (2011a).

lower quality.[9] On the other hand, mortgage backed security (MBS) sponsors also have access to unobserved information, which they could use to select higher quality loans.[10] Second, the impact of loan quality on foreclosure and modification decisions conditional on delinquency is ambiguous. Some quality dimensions favor foreclosure, while others favor modification or inaction. For example, borrower resilience discourages foreclosure because a resilient borrower is likely to regain his financial footing and repay his mortgage. By contrast, borrower reliability encourages foreclosure because a reliable borrower must have suffered a large shock before becoming delinquent on his loan.

The existing literature recognizes the potential bias presented by unobserved quality. Yet, all three papers discussed above 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 the probability of delinquency or loan quality conditional on delinquency. If unobserved information is solely about the probability of delinquency, 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 similar for high quality loans (e.g., loans with high FICO scores and full income documentation), which should have less potential for unobserved quality differences.[11] 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

[9] Using evidence from credit score cutoffs, Keys, et al. (2010) propose that originators employ less diligent screening for loans that are likely to be securitized. Bubb and Kaufman (2013) question the credit score cutoff evidence. Purnandam (2010) finds that banks with higher exposure to originate-to-distribute lending were stuck holding loans intended for securitization when securitization froze in 2007 and subsequently suffered higher delinquency rates and charge offs, consistent with securitization decreasing loan origination quality.

[10] Jiang, Nelson, Vytlacil (2010) present evidence that screening moral hazard is more than offset by selection of higher quality loans for securitization. The selection is in part facilitated by information that emerges during the time period between origination and securitization. Similarly, Agarwal, Chang, and Yavas (2012) show that for prime loans default risk is lower for GSE securitized loans than for portfolio loans.

[11] 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.

delinquency, and this could be just as large for high quality loans as for low quality loans.

Finally, Piskorski, Seru, and Vig (2010) analyze a quasi-experiment for securitization status. 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 similar. The authors exploit this discontinuity by comparing loans that became delinquent shortly before 90 days and were bought back and kept by the originator to loans that became delinquent shortly after 90 days and remained securitized. Importantly, Piskorski, Seru, and Vig do not use instrumental variables or fuzzy regression discontinuity tools. Instead, they directly compare the two groups described above. This 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). Because repurchases are based on factors other than delinquency status (for example, a loan could unobservably violate another representation or warranty) and originators decide whether to retain or re-securitize repurchased loans, the resulting comparison is subject to omitted variable bias. Piskorski, Seru, and Vig argue that repurchase decisions are less endogenous than securitization decisions, but it is not clear this is the case. Adelino, Gerardi, and Willen (2011b) discuss this issue more fully and argue that early payment default is not a good instrument even if it is implemented using traditional tools.

3 Data and Methodology

Loan Performance Data

My data on mortgage loans comes from Lender Processing Services (LPS).[12] 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% of the U.S. mortgage market. Importantly, the dataset spans all mortgages serviced by the participating servicers, including portfolio loans, loans securitized by Fannie Mae and Freddie Mac (the Government Sponsored Entities, GSEs), and privately securitized

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

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 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 264,000 jumbo loans (i.e., loans over $417,000, which are not eligible for GSE securitization)[13] and 1.6 million non-jumbo loans. As of six months after origination, 70% of the jumbo loans were privately securitized. Almost all of the rest (27%) were held as portfolio loans. By contrast, 81% of non-jumbo loans were securitized by the GSEs. Delinquency is common in both sub-samples. 6% of jumbo loans became seriously (60+ days) delinquent within 1 year, and 36% became seriously delinquent within five years. Similarly, 4% of non-jumbo loans became seriously delinquent within 1 year and 27% became seriously delinquent within 5 years.

All of my analysis is conditional on mortgages becoming seriously delinquent, which I define as delinquencies of at least 60 days. I split the sample based on when a loan first became seriously delinquent. The baseline sample consists of loans that became seriously delinquent within twelve months of origination. I use the twelve month delinquency cutoff to focus on a time period before significant government intervention in the mortgage market.[14] The baseline sample has 16,000

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

[14] The twelve month cutoff combined with a six month analysis window ends the analysis in February of 2009, before the Home Affordable Modification Program (HAMP) was implemented.

jumbo loans and 61,000 non-jumbo loans. The full sample, which consists of all loans that became seriously delinquent before the end of 2011, has 93,000 jumbo loans and 426,000 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 baseline sample foreclosure is initiated within six months of first serious delinquency for 70% of jumbo loans and completed for 14%. Foreclosure rates are slightly lower for non-jumbo loans and decrease over time, driving down foreclosure rates in the full sample.

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, an interest rate reduction on a fixed rate mortgage must be due to a mortgage modification. My algorithm for identifying loan modifications, described 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 balances, or extending loan terms; and modifications to make loans current by capitalizing past due balances. The loan modification algorithm looks for evidence of either of these patterns.

A limitation of the loan modification algorithm is that it does not identify modifications that do not change interests rates, term to maturity, or principal balances. In particular, it does not capture temporary payment plans or principal forbearance. In order to work, the algorithm requires monthly data on interest rates, term to maturity, and principal balances. This is universally available for interest rates and principal balances. Monthly term to maturity data, on the other

hand, is only available for about half of the loans in my sample. I limit my modification analysis to these loans.

In my baseline jumbo sample, 5.2% of seriously delinquent jumbo loans were modified within six months. These modifications were overwhelmingly principal-increasing as opposed to concessionary. In the full sample, the six-month jumbo modification rate was 7.1% and included interest rate reductions (2.4%), term extensions (2.7%), and principal increases (3.6%).

Instrumental Variables Methodology

I exploit the sudden and unexpected freeze of private mortgage securitization in the third quarter of 2007 to identify private securitization. Loans originated shortly before the freeze are similar to loans originated earlier in the year but were significantly less likely to be securitized. My identification strategy is analogous to Bernstein's (2012) instrument for public ownership. Bernstein exploits the fact that NASDAQ returns shortly after an IPO announcement are uncorrelated with firm prospects but predict whether the IPO will be completed. In both Bernstein's setting and my own, ownership structure is endogenous but is influenced by effectively random shocks to related asset markets.

Purnanandam (2011) also documents and exploits loans being stuck on bank balance sheets in 2007. Using bank-level call report data, Purnanandam shows that banks with heavy exposure to originate-to-distribute lending were stuck holding loans that were intended for sale. These banks subsequently suffered higher delinquency rates and charge offs than other banks, consistent with originate-to-distribute loans being lower quality than other loans. In contrast, I exploit time series variation in securitization rates by loan origination month to control for origination quality differences and estimate the impact of securitization on mortgage servicing.

Mortgage securitization comes in two forms. Most residential mortgages are securitized by Fannie Mae or Freddie Mac (the Government Sponsored Entities, 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

(MBS) were issued, including $200 billion backed by jumbo mortgages.[15]

Private mortgage securitization abruptly halted in the third quarter of 2007 and has essentially remained frozen since then. Figure 1 plots prime securitization volume from 2000 to 2011. Jumbo prime MBS issuance topped $55 billion dollars in quarters 1 and 2 of 2007 then crashed to $38 billion in Q3 and $18 billion 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.[16] Consistent with this view, ABX price indices for AAA subprime MBS fell below unity for the first time shortly before the market freeze (see Figure 2).[17] 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 (see Figure 1).

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 by month in Figure 3. Jumbo originations tracked non-jumbo originations and stayed in the neighborhood of 30,000 originations per month until August of 2007. Jumbo lending then dramatically fell in September of 2007 while non-jumbo lending (which was largely unaffected by private securitization) remained steady. This is exactly the response we would expect from an unexpected freeze in private securitization. The online appendix includes plots of loan characteristics by origination month. This evidence supports the origination volume data in Figure 3. Loan size, credit scores, loan-to-value ratios, and documentation levels were fairly stable from January to August of 2007, and jumbo and non-jumbo loans followed similar patterns. Jumbo interest rates tracked non-jumbo interest rates from January to

[15] Source: Inside Mortgage Finance.

[16] 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.

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

August of 2007 and then increased in September relative to non-jumbo interest rates.

Though the freeze did not affect pre-freeze origination decisions, it did affect the probability that these mortgages were securitized. Assembling a pool of loans, selling them to an MBS sponsor, and closing on an 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% were 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 of securitization dropped dramatically in the summer of 2007. Figure 4 plots private securitization rates six months after origination for jumbo loans in my sample by origination month. This is essentially the first stage regression for my identification strategy. Jumbo private securitization rates were around 80% until April and then started 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. By contrast, non-jumbo GSE securitization rates remained steady at around 85% throughout 2007.

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

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

using origination month indicator variables as instruments for private securitization (Sec_i). The regression is conditional upon loans becoming seriously delinquent. Y_i is an indicator for foreclosure or modification within six months of first serious delinquency.[18] Sec_i is an indicator for a mortgage being privately securitized six months after origination. X_i is a vector of observable loan characteristics including MSA and delinquency month fixed effects. The implied linear probability model accommodates standard IV regression techniques and readily incorporates fixed effects without biasing coefficient estimates.[19]

[18] I use a six month window so that my baseline analysis ends in February of 2009, before the Home Affordable Modification Program (HAMP) took effect.

[19] 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. As a robustness check, I estimate bivariate probit models and find

Strictly speaking, the identification strategy only requires control variables to the extent that they are correlated with origination month. Delinquency month fixed effects are important because foreclosure and modification practices changed over time and delinquency month is correlated with origination month. Other control variables are less important.[20] 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 to 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 term lengths, 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.

Figure 5 plots baseline sample first stage and reduced form origination month fixed effects for equation (1).[21] Jumbo foreclosure initiation (panel A), foreclosure completion (panel B), and modification (panel C) origination month fixed effects were fairly constant until April 2007. After April, jumbo foreclosure probability decreased and jumbo modification probability increased as jumbo private securitization probability (the first stage) decreased. The IV regressions in the next section add coefficient estimates and standard errors, but the basic relationships are clear from the reduced form plots. Private securitization increases the probability of foreclosure and decreases the probability of modification.

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 pre-

that they produce similar results.

[20] In the online appendix I estimate a version of equation (1) without loan characteristics. Results are consistent with my baseline estimates.

[21] Figure 5 corresponds to the IV regressions reported in Table 4. The first stage is identical across the three regressions except that the modification regression is limited to loans that report term length data. This results in slightly different jumbo private securitization fixed effects in Panel C.

dominately securitized by the GSEs, and GSE securitization was uninterrupted throughout 2007. Figure 5 also plots the reduced form of equation (1) for non-jumbo loans. Non-jumbo foreclosure and modification origination month fixed effects were largely flat over the sample period, suggesting that any changes to the lending environment between January and August of 2007 did not have a major impact foreclosure and modification practices.

As a robustness check, I control for origination month fixed effects by estimating equations of the form:

$$\begin{aligned} Pr\left(Y_i|Delinquency_i\right) &= \alpha + \gamma Sec_i + \beta_1 Jumbo_i + \beta_2 NonJumbo_i * Sec_i \\ &\quad + OrigMonth_i\beta_3 + X_i\beta_4 + NonJumbo_i * X_i\beta_5 + \varepsilon_i \end{aligned} \tag{2}$$

using $Jumbo_i * OrigMonth_i$ indicator variables as instruments for private securitization (Sec_i). As before, Y_i is an indicator for foreclosure or modification within six months of first serious delinquency, and Sec_i is an indicator for a mortgage being privately securitized six months after origination. $Jumbo_i$ is an indicator for jumbo status. $NonJumbo_i * Sec_i$ is the interaction between private securitization and non-jumbo status.[22] $OrigMonth_i$ is a vector of origination-month dummy variables. X_i is a vector of the same loan characteristics and fixed effects included in equation (1). Conceptually, equation (2) estimates separate regressions for jumbo and non-jumbo loans except that the origination-month fixed effects estimated with non-jumbo loans are applied to the jumbo regressions. The reduced form of equation (2) is a difference in differences regression of Y_i (foreclosure or modification) on origination month exploiting differences between jumbo loans (the treated group) and non-jumbo loans (the control group).

The remaining concern is that something changed between January and August of 2007 differentially in the jumbo lending environment relative to the non-jumbo lending environment. I cannot fully rule this out, but the overall evidence suggests that jumbo lending was fairly stable and moved in parallel with non-jumbo lending until August of 2007. Even if there were time-series changes specific to jumbo lending, they are unlikely to rival the drop in jumbo private securitization

[22] Including the $NonJumbo_i * Sec_i$ interaction allows for the possibility that private securitization has a different impact on jumbo and non-jumbo loans. I include this interaction variable directly in the regression (i.e., without an instrument) even though it is endogenous. This is less of a problem because I am not interested in the β_2 coefficient. In the appendix, I estimate a version of equation (2) without $NonJumbo_i * Sec_i$ and obtain larger γ estimates, suggesting that equation (2) is a conservative specification.

from 80% in April to 36% in August.

4 Results

Baseline Results

I start by estimating the effect of private securitization on foreclosure and modification in my baseline sample of jumbo loans that became seriously delinquent within one year of origination. This time period is most directly comparable to previous studies and is relatively free of government policy interventions. Because the last originations in my sample are in August of 2007, 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 the Home Affordable Modification Program (HAMP) was implemented. Later, I consider all loans that became seriously delinquent before 2012 to assess whether the effect of securitization on foreclosure and modification changed over time.

Before implementing my instrumental variables strategy, I first estimate equation (1) with origination month fixed effects using OLS regressions. 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 3.1 ppt less likely to be modified within six months.[23] 97% of sample jumbo loans are privately securitized or held as portfolio loans so the coefficients estimate differences between these two groups. The samples for the three regressions are identical with one exception. As discussed in the previous section, I can only consistently identify modifications for loans that report their term to maturity on a monthly basis. This decreases the modification regression sample size by about 50%.

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

[23] The coefficients are slightly lower than Piskorski, Seru, and Vig's (2010) 4-7 ppt foreclosure bias estimate and Agarwal, et al.'s (2011) -4.2 ppt 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.

explain part of the residual of equation (1). As discussed in the previous section, the direction of the omitted variable bias is theoretically ambiguous. Even assuming securitized loans are unobservably lower quality, the impact of loan quality on foreclosure and modification conditional on delinquency could be positive or negative. This ambiguity is 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 origination month to instrument for jumbo securitization status. Coefficients are estimated using two stage least squares. Standard errors are clustered by MSA. Control variables are the same as in the Table 3 OLS regression except that origination month is now used as an instrument for private securitization.

Column (1) reports the first stage regression of private securitization on origination month.[24] As discussed earlier, securitization probability decreased dramatically during the summer of 2007. The first stage regression shows the same pattern after controlling for observable loan characteristics. Origination month fixed effects decreased over the course of 2007 with a particularly sharp decline after April. The August origination month fixed effect is -69.5 ppt compared to loans originated in January. Origination month is a powerful predictor for securitization. The within-MSA adjusted R-squared for the first stage regression is 0.32, and the Kleibergen-Paap F statistic is 396. In short, weak identification is not a problem.

Columns (2) to (4) of Table 4 report instrumental variables estimates for equation (1). Conditional on serious delinquency, private securitization increases the six-month probability of foreclosure initiation by 8.0 ppt and foreclosure completion by 4.7 ppt. Private securitization decreases the six-month probability of modification by 3.6 ppt. The coefficient estimates are all statistically significant (standard errors range from 0.9 ppt to 1.6 ppt). Moreover, they are economically large. As percentages of mean rates, the foreclosure initiation coefficient is 12%, the foreclosure completion coefficient is 35%, and the modification coefficient is 69%. Comparing columns (2) to (4) of Table 4 to Table 3 reveals the omitted variable bias of the OLS regressions. For foreclosure

[24] The reported first stage results use the entire jumbo baseline sample, which is also used for the foreclosure initiation and foreclosure completion regressions. The modification regression uses a reduced sample and has slightly different first stage estimates, which are plotted in Figure 5.

initiation and completion, the IV securitization coefficient estimates are about 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 effect on modification. As a result OLS underestimates the causal impact of securitization on foreclosure.

Interpreting the Results

The IV estimates of Table 4 estimate the Local Average Treatment Effect (LATE) of private securitization on foreclosure and modification. The securitization freeze instrument affected securitization probability for loans that would have been securitized after a delay. The IV methodology cannot estimate the impact of securitization on non-compliers, in this case mortgages that never would have been securitized and mortgages that were securitized quickly enough to avoid the freeze. Is LATE likely to differ from the Average Treatment Effect (ATE) of securitization on all loans? No. First, the instrument is very strong (e.g., the August first stage fixed effect is -69.5 ppt), suggesting that most mortgages are compliers. Second, there is no a priori reason to think that speed of securitization is correlated with the treatment effect. If the treatment effect does vary across loans, the loans and originators with the smallest treatment effect are likely the most inclined to securitization (because a smaller treatment effect makes securitization less costly). Thus, if anything LATE is likely conservative relative to ATE.

The treatment itself is also slightly nuanced in the IV regression. Specifically, the IV treatment is being stuck holding loans intended for securitization. If pre-planning aids portfolio loan servicing or if the entities stuck holding the loans don't typically engage in portfolio lending, this treatment is slightly different from a planned changed in securitization practices. To the extent that it matters, the lack of pre-planning likely decreases an owner's ability to differentially service portfolio loans, thereby making the IV estimates conservative.

A final issue of interpretation is how broadly to extrapolate the results. Strictly speaking, my baseline regressions estimate the impact of private securitization on foreclosure and modification of jumbo loans originated in 2007 that became delinquent within one year of origination. In later regressions, I show that similar results also hold for loans that became delinquent at other times. I focus on 2007 originations solely for identification purposes. As far as I know, there is nothing

special about 2007 origination practices so my coefficient estimates should be valid for jumbo loans originated at other times. The estimates are also informative about private securitization of non-jumbo loans (e.g., subprime loans). Exact magnitudes may differ, but the same basic frictions of private securitization likely apply there as well. My results are less informative about GSE securitization because GSE securitization involves different contracts and leaves a single entity (the GSE) with full credit exposure for the underlying mortgages.

Robustness Checks

One difference between my empirical design and that of Piskorski, Seru, and Vig (2010) and Adelino, Gerardi, and Willen (2011a) is that I use a six month analysis window instead of considering loans for a longer period of time after delinquency.[25] 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 as opposed to changes to their ultimate probability. Columns (1) to (3) of Table 5, Panel A address this concern by replicating my baseline results with a twelve-month window instead of a six-month window. The coefficient estimates are consistent with my baseline results. The foreclosure start coefficient (8.1 ppt) is almost identical. The foreclosure completion coefficient is somewhat larger (6.1 ppt compared to 4.7 ppt). The modification coefficient is more significantly larger (-6.6 ppt compared to -3.6 ppt). The increases are likely due to the higher incidence of foreclosure completion and modification within the twelve-month window. In short, my baseline results appear to reflect permanent effects as opposed to changes in timing.

Another potential concern is that the jumbo lending environment changed between January and August of 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 after controlling for observable characteristics. Nonetheless, I address the concern by restricting the sample and estimating origination-month fixed

[25] 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. Adelino, Gerardi, and Willen (2011a) use a twelve-month analysis window.

effects with non-jumbo loans.

The restricted sample focuses on 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 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 foreclosure coefficient estimates are nearly identical to my baseline results. The modification coefficient is larger in the restricted sample (-8.0 ppt compared to -3.6 ppt), suggesting that my baseline results are conservative.

To explicitly control for changes to the lending environment over time, I estimate equation (2) using interactions between origination month indicator variables and jumbo status as instruments for private securitization. As discussed earlier, this difference in differences strategy controls for origination month fixed effects using non-jumbo loans while using the interacted version of origination month to instrument for jumbo securitization. Results are reported in Columns (1) to (3) of Table 5, Panel B. Foreclosure initiation (9.7 ppt), foreclosure completion (5.9 ppt), and modification (-2.7 ppt) coefficient estimates are all close to their baseline values.

In Columns (4) to (6) of Table 5, Panel B, I report marginal effect estimates from 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 avoid biases associated with a large number of fixed effects, I drop the MSA fixed effects. The marginal effects of private securitization on foreclosure initiation (6.8 ppt) and foreclosure completion (4.1 ppt) are close to my baseline estimates. The modification marginal effect (-1.9 ppt) is lower than my baseline estimate.

In the online appendix, I consider three additional robustness tests: dropping loan characteristic control variables, estimating equation (2) without the $NonJumbo_i * Sec_i$ interaction term, and including mortgages that are transferred to non-LPS servicers. Results are consistent with my baseline estimates.

Full Sample Results

So far my analysis has focused on my baseline sample of loans that became seriously delinquent within twelve months of origination. The rationale for starting with this sample is that it ends the analysis in February of 2009, before significant government intervention into the mortgage market. The baseline sample time period (primarily 2007 and 2008) also represents the heart of the financial crisis and was a time when servicers may have been overwhelmed by a surge in delinquencies.

To assess whether my baseline results are specific to 2007 and 2008, I repeat my analysis on the full sample of all jumbo loans that became seriously delinquent before 2012. Table 6 reports the results. The full sample private securitization coefficient estimates are 12.4 ppt for foreclosure initiation, 2.8 ppt for foreclosure completion, and -5.1 ppt for modification (25%, 49%, and 72%, respectively, as a percent of mean rates). Compared to the baseline sample (Table 4) results, the foreclosure initiation coefficient is larger both in absolute terms and as a fraction of the mean foreclosure initiation rate. The foreclosure completion coefficient is lower in absolute terms but is higher as a fraction of the mean foreclosure completion rate. The modification coefficient is larger on an absolute basis and about the same size as a fraction of the mean modification rate.

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%,[26] 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 three years 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

[26] 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.

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

HAMP's efficacy is the subject of an ongoing debate.[27] My methodology does not provide a way to test whether HAMP succeeded in reducing foreclosures, but I can assess whether it made foreclosure and modification decisions more uniform across securitized and portfolio loans. Policy makers were particularly concerned about the perceived bias of securitized loans towards foreclosure and away from modification. Was HAMP successful at mitigating this bias?

To assess post-HAMP securitization biases, I repeat my empirical strategy on sub-samples of jumbo loans split by the year in which they became delinquent. Table 7 reports the results. Foreclosure initiation coefficients (Panel A) had no clear trend over time. If anything, they were higher in 2010 and 2011 after HAMP was implemented, especially when considered as a fraction of mean foreclosure initiation rates, which declined over time. Foreclosure completion coefficients (Panel B) declined over time on an absolute basis but increased as a fraction of mean foreclosure rates. Modification coefficients (Panel C) had no trend over time on an absolute basis and decreased moderately as a fraction of mean modification rates. With the sole exception of modification in 2010, private securitization increased foreclosure and decreased modification probability by statistically significant and economically meaningful amounts in all years. In short, there is no evidence that HAMP mitigated the bias of privately securitized loans toward foreclosure and away from modification.

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 foreclosure and modification biases persisted after HAMP suggests that HAMP had little impact on them. At the very least we can conclude that HAMP did not fully eliminate the bias of privately securitized loans toward foreclosure and away from modification.

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

Long Term Impact

Private securitization increases the probability of foreclosure and decreases the probability of modification within six and twelve months of first serious delinquency. Do these effects also show up in longer term foreclosure and modification probabilities? How large are the long term effects? What is the total impact of private securitization on foreclosures?

To answer these questions, I estimate the impact of private securitization on foreclosure and modification over a three-year analysis window. The analyzed sample includes all jumbo loans that became seriously delinquent before 2010. Table 8 reports the results. Private securitization increases the three-year probability of foreclosure initiation by 8.7 ppt, increases three-year probability of foreclosure completion by 11.3 ppt, and decreases the three year probability of modification by 5.9 ppt. As a fraction of mean rates these represent impacts of 11% for foreclosure initiation, 31% for foreclosure completion, and -25% for modification.

Since September of 2008, 4.4 million homes have been foreclosed, half of which were privately securitized.[28] If private securitization increased the incidence of foreclosure by 31%, this means over 500,000 foreclosures are attributable to private securitization. Admittedly, this is a rough estimate. It requires extrapolation from jumbo private securitization to private securitization more generally, and it ignores the general equilibrium effects of curtailing private securitization. That said, 500,000 could actually be a conservative estimate. Subprime private securitization frictions may be even larger than jumbo frictions, and curtailing securitization may have increased loan quality, further decreasing delinquencies and foreclosures.

Modification Details and Effectiveness

In addition to impacting the probability of modification, securitization also affects how loans are modified. Some securitized servicing contracts place limits on principal and interest reductions and modifications and term extensions. 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

[28] Foreclosure data is from the CoreLogic National Foreclosure Report, April 2013. Piskorski, Seru, and Vig (2011) and Mayer (2009) estimate that half of foreclosure initiations were privately securitized mortgages based on Federal Reserve reports and private market data.

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 delinquencies that are modified. For this analysis I include all jumbo loans that became seriously delinquent before 2012 and were modified within six months. 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 different sample and dependent variables, the regressions are identical to my previous IV regressions. Panel A of Table 9 reports the results. Securitization increases the incidence of interest modifications and principal increases, decreases the incidence of term modifications, and has no significant impact on the incidence of principal decreases.

I also consider how securitization affects net changes to interest rates, term lengths, principal balances, and monthly payments. Panel B of Table 9 reports results for regressions of net changes on the same variables considered in Panel A. Across all terms, privately securitized modifications are less concessionary. Even though a higher fraction of privately securitized modifications involve interest rate decreases, the average interest rate decrease is 39 bps lower for securitized mortgages. Similarly, term extensions and payment cuts are smaller and principal increases are larger for privately securitized mortgages.

Finally, I compare the effectiveness of securitized and portfolio modifications by analyzing the probability of redefault (return to 60+ day delinquency) in the twelve months following modifications that cured delinquencies. Table 10 reports the results. In column (1), I estimate an IV regression of redefault on private securitization and standard controls in the full sample of jumbo loans. Redefault is 7.6 ppt higher for privately securitized loans (compared to a mean redefault rate of 30%). The difference is partially explained by the types of modifications employed. Column (2) includes controls for modification type. This decreases the private securitization coefficient to (a statistically insignificant) 4.2 ppt. Interest and principal decreases are associated with lower redefault rates. Principal increases are associated with higher redefault rates.[29]

[29] These 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.

5 Mechanism

The preceding section established that privately securitized loans are foreclosed more and modified less than comparable portfolio loans. Why do servicers treat securitized loans and portfolio loans differently?

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 mortgagors, 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, servicing 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 a mortgage's value but would be costly to the servicer. Servicers may also have an incentive to not 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 servicers.[30] 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 rare, and spillovers from decreased

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

modifications are only part of the bias of securitized loans towards foreclosure. We have already seen one piece of evidence to this effect. Securitization has a larger impact on foreclosure (8.0 ppt for foreclosure initiation and 4.7 ppt for foreclosure completion) than on modification (-3.6 ppt). If the foreclosure bias was solely a spillover from modification frictions, it should be smaller than the modification bias.

To better understand how securitization affects mortgage servicing, I examine the contractual terms of actual servicing agreements and link these terms to loan-level 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 focussing on frictions associated with servicing securitized loans, it is important to understand the options available to servicers when dealing with delinquent loans. Foreclosure and modification are not binary responses to delinquency. Servicers also 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.

Fannie Mae's 2006 Servicing Guide offers a 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, Fannie Mae 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, servicers 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 Fannie Mae approval, servicers can also negotiate more formal "Loss Mitigation Alternatives," including loan modifications, short sales, deeds-in-lieu of foreclosure, assumptions of mortgages by new homebuyers, and assignment of mortgages to mortgage insurers.

Choosing among these options requires significant servicer discretion. Optimal practices de-

pend 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 servicing cost versus higher recovery rates. 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, securitized 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 analyze 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.[31] 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. 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 little 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 industry 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 11 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 Fannie Mae 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

[31] 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.

the best interest of certificateholders. In other PSAs this is implicit in general and sometimes an explicit standard for specific 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, 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 Fannie Mae'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.[32]

Of all the terms summarized in Table 11, 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 bind (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 industry 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

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

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 to 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.

Finally, amendment is difficult under all of the PSAs. General amendments require at least a majority approval of certificateholders, and in all but one PSA they require either a supermajority of certificateholders or a majority vote within each class of affected certificateholders.[33] Moreover, all PSAs expressly outlaw any amendment that would decrease or delay payments without the universal 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) surveyed 20 subprime deals in 2006 and found that 67% limit modifications to loans in default or where default is foreseeable or imminent and 10% prohibit modifications altogether. Credit Suisse (2007) surveyed 31 deals between 2004 and 2007 and found that nearly all PSAs permit modification of 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) surveyed approximately 20 deals and found that 10% of deals prohibit modifications and another 40% of deals require ratings agency approval if more than 5% of a loan pool is changed.

[33] 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.

PSA Term Regressions

To assess how modification restrictions affect servicer behavior, I link PSAs to individual loans in Core Logic panel data.[34] For comparability to my earlier analysis, I limit the dataset to jumbo loans and impose the restrictions described in Section 3.[35] As described in Table 12, 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 seriously days delinquent within 1 year and 21% became seriously 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 that became seriously delinquent between 2007 and 2011. Foreclosure initiation (51.7% within six months), foreclosure completion (6.7% within six months) and modification (6.3% within six months) rates are similar to the previous full jumbo sample. 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 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 should 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. (2012a)) have demonstrated that servicers employ different modification and foreclosure practices. One caveat is that within-servicer

[34] 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.

[35] 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.

term variation is limited to two servicers. Servicer A has prohibitions of 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 13 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, significant at the 10% level). Because I am 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 should 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 -3.6 ppt baseline modification bias for securitized loans. Similarly, the foreclosure coefficient estimates, combined with the incidence of these terms explain 56% of the foreclosure start bias and 46% 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. Private securitization increases foreclosure probability (by 8.0 ppt for foreclosure initiation and 4.7 ppt for foreclosure completion) and decreases modification probability (by 3.6 ppt). Second, I estimate the effect of securitization on foreclosure and modification over time, including periods before and after government intervention. Securitization increased foreclosure probability and decreased modification probability throughout 2007 to 2011, even after implementation of the Home Affordable Modification Program (HAMP) in 2009. Third, I identify the mechanisms through which securitization effects foreclosure and modification, highlighting that

incentive differences are more important than contractual prohibitions and that the foreclosure bias is more than just a spillover from modification frictions.

The bias of securitized loans towards foreclosure and away from modification helps to explain why foreclosure is so prevalent. Securitization increases the incidence of foreclosure completion within three years by 31%. Extrapolated to all privately securitized mortgages, this adds up to over 500,000 of the 4.4 million foreclosures experienced since the start of the financial crisis. Securitization does not explain all foreclosures, but many foreclosures would have been prevented if mortgages had been held directly on bank balance sheets instead of being securitized.

The differential treatment of securitized and portfolio loans serves as an example of how ownership structure can affect how assets are managed. Despite contracts designed to protect MBS investors from differential servicing treatment, securitized loans were systematically foreclosed more and modified less. This is an important factor in 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 should be considered for both regulatory reforms and improvements to private contracts.

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 perfectly 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 the policy goal should be. The private and social costs and benefits of foreclosure and modification remain important topics for future research to address 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, an interest rate change 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 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 ppt 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 ppt would have a fully indexed rate of LIBOR + 2 ppt 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 ppt 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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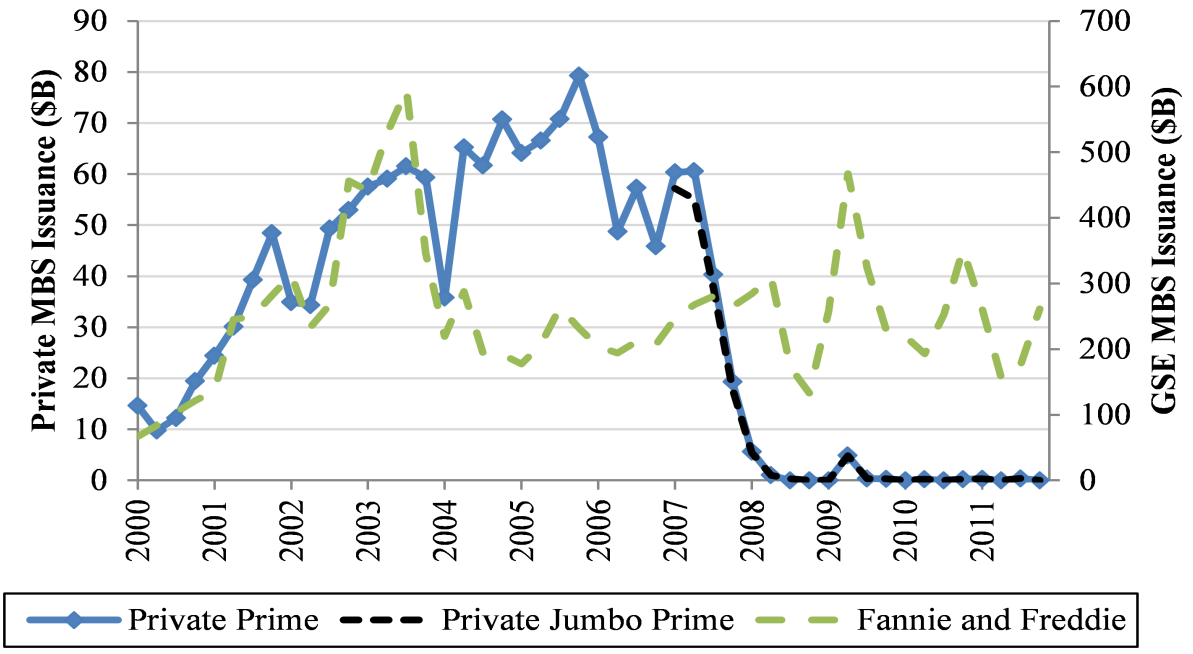

Figure 1: MBS Issuance. Prime mortgage backed security (MBS) issuance volume by quarter. Private issuance is plotted on the left axis. Fannie Mae and Freddie Mac (GSE) issuance is plotted on the right axis.

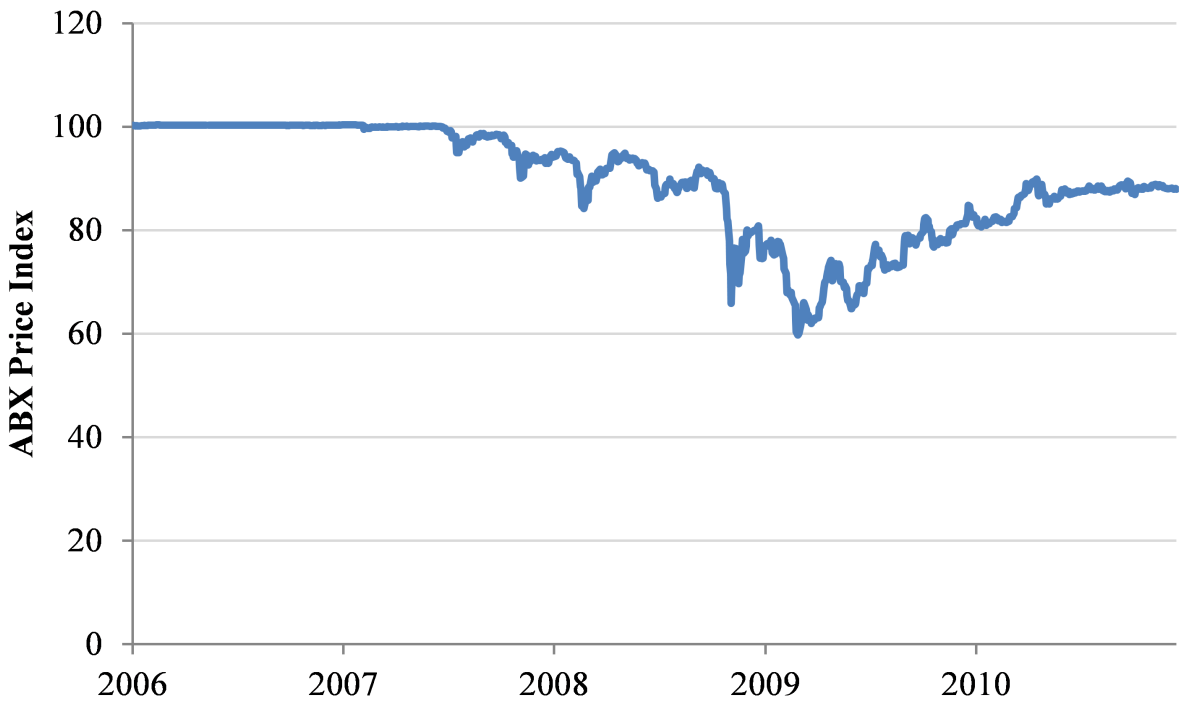

Figure 2: ABX Price Index. Daily prices of the Markit ABX.HE.06-1 AAA index, which consists of Credit Default Swaps (CDS) on AAA supbrime MBS issued in the second half of 2005.

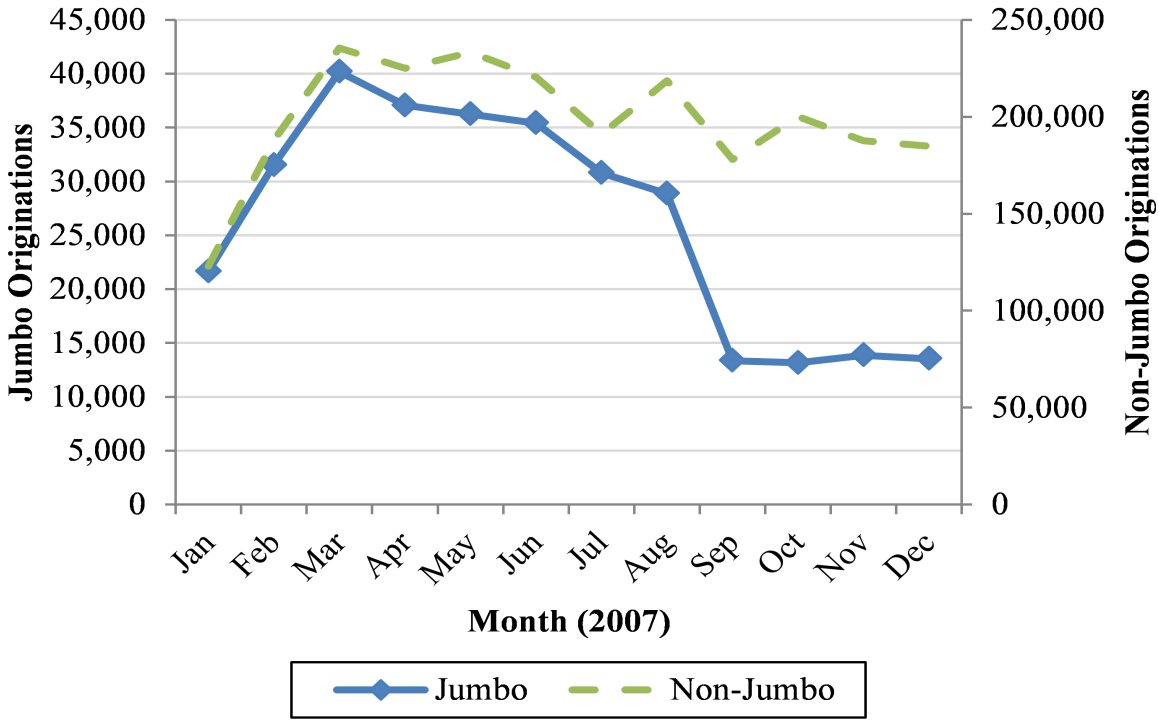

Figure 3: Mortgage Originations. Sample loan originations by month and size. Jumbo mortgages are loans over $417K, the conforming limit for Fannie Mae and Freddie Mac.

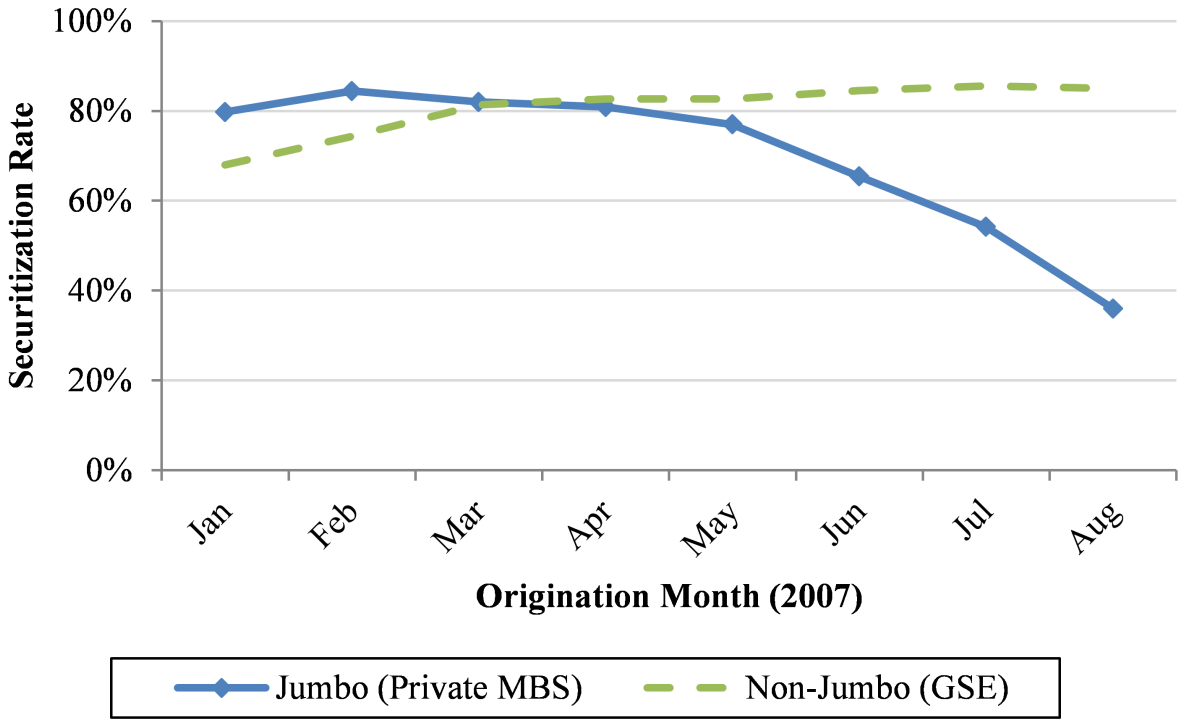

Figure 4: Securitization Rates by Origination Month. Percent of jumbo sample loans that are privately securitized and percent of non-jumbo sample loans that are securitized by Fannie Mae and Freddie Mac (the GSEs) by origination month. Securitization is measured as of six months after origination.

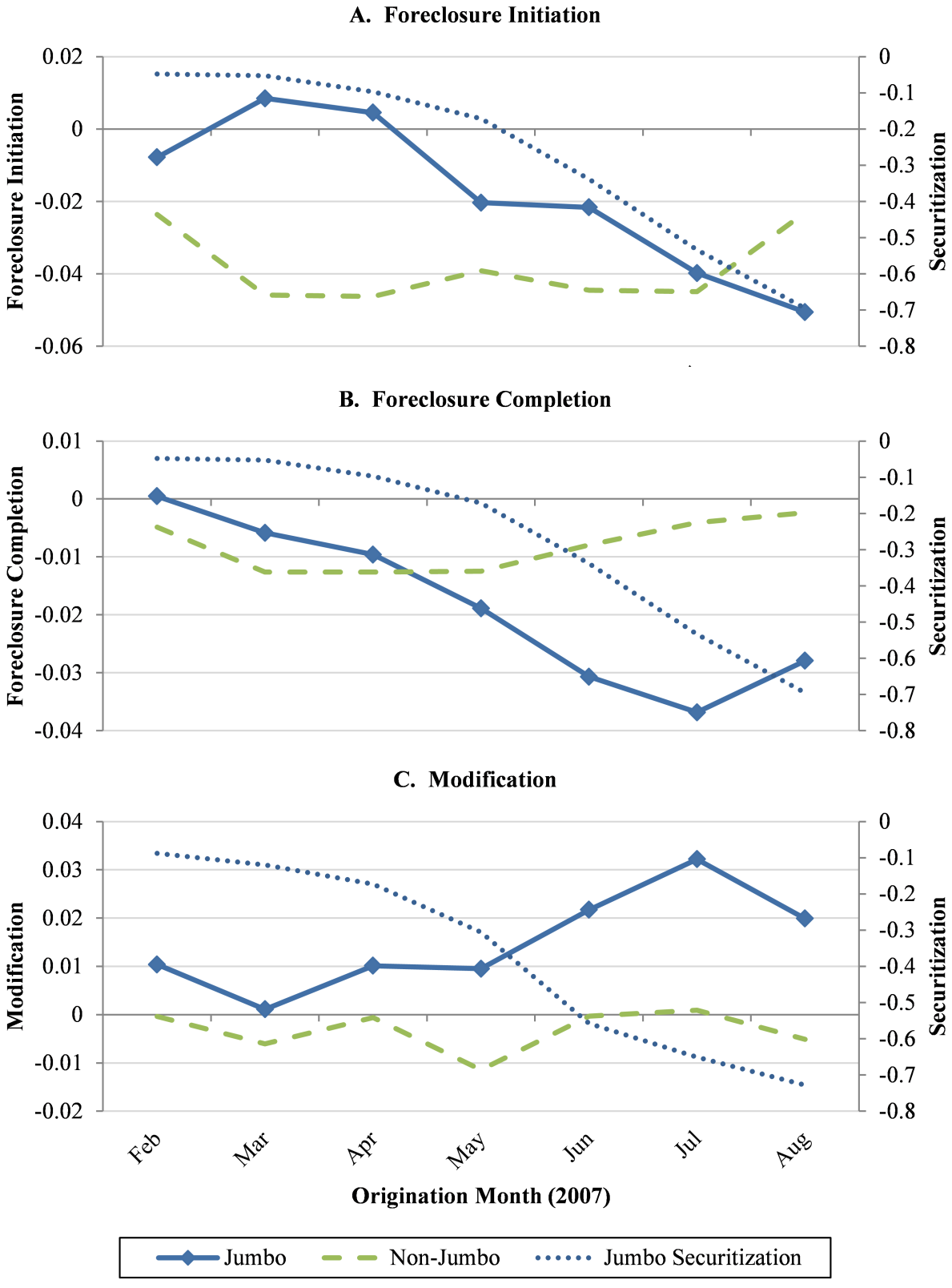

Figure 5: Reduced Form Regression Fixed Effects. Jumbo fixed effects are from the reduced form of the baseline IV regressions reported in Table 4. Non-jumbo fixed effects are for identical regressions estimated for non-jumbo loans. All fixed effects are relative to January.

Table 1: Data Summary

Data comes from LPS. The sample consists of 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. Jumbo loans are larger than the GSE conforming limit ($417K). Portfolio loans are not securitized. Privately securitized loans are securitized in non-GSE mortgage backed 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 initiation is the referral of a mortgage to an attorney to initiate foreclosure proceedings. Foreclosure completion is 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		**Baseline Sample (Delinquent in First Year)**		**Full Sample (Delinquent Before 2012)**	
	Jumbo	**Non-Jumbo**	**Jumbo**	**Non-Jumbo**	**Jumbo**	**Non-Jumbo**
Number	263,544	1,644,346	15,985	61,242	93,379	425,543
Size (mean)	$691,219	$210,294	$653,155	$230,861	$650,601	$230,892
FICO (mean)	733	726	700	686	712	699
LTV (mean)	0.73	0.72	0.79	0.81	0.77	0.79
Ownership						
Portfolio	27.4%	9.2%	33.2%	16.4%	25.4%	11.3%
Private Security	70.2%	9.4%	63.8%	18.6%	71.5%	15.9%
GSE	1.7%	80.9%	1.6%	64.5%	2.2%	72.4%
Delinquency						
Within 1 year	6.1%	3.7%				
Within 5 years	36.4%	26.6%				
Foreclosure Initiation						
Within 6 months			69.5%	60.1%	48.8%	49.9%
Within 1 year			80.7%	72.2%	60.7%	62.1%
Within 3 years			90.3%	86.2%	78.9%	78.9%
Foreclosure Completion						
Within 6 months			13.5%	12.4%	5.7%	6.6%
Within 1 year			36.9%	29.3%	17.9%	18.4%
Within 3 years			58.1%	54.7%	36.9%	42.0%
Modification						
Within 6 months			5.2%	3.0%	7.1%	7.3%
interest decrease			0.4%	0.6%	2.4%	4.7%
term extension			0.2%	0.7%	2.7%	3.5%
principal decrease			0.1%	0.0%	0.4%	0.3%
principal increase			4.8%	2.2%	3.6%	2.9%
Within 1 year			8.5%	6.3%	13.6%	15.5%
Within 3 years			12.3%	13.9%	23.5%	26.5%
Redefault						
Within 1 year			71.5%	73.2%	30.2%	27.5%

Table 2: Securitization by Age for 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	% Privately Securitized
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

The dependent variables are indicators for foreclosure initiation, foreclosure completion, and modification within six months of first serious (60+ days) delinquency. All regressions are OLS. Privately securitized is an indicator for private securitization as of six months after origination. The regressions analyze baseline sample jumbo loans, which became seriously (60+ days) delinquent within one year of origination. The modification regression is restricted to mortgages with term length data. R-squared statistics are calculated within MSAs. Clustered (by MSA) standard errors are in parentheses. * represents 10% significance, ** represents 5% significance, *** represents 1% significance.

	(1) OLS	(2) OLS	(3) OLS
	Foreclose Start	**Foreclose**	**Modify**
Mean	0.695	0.135	0.052
Privately Securitized	0.039***	0.022***	-0.031***
	(0.011)	(0.008)	(0.005)
FICO >= 680	0.087***	0.032***	-0.044***
	(0.008)	(0.008)	(0.006)
LTV Ratio	0.630***	0.046	0.018
	(0.051)	(0.040)	(0.045)
LTV = 80	0.031***	0.018**	-0.008*
	(0.010)	(0.007)	(0.004)
log(Origination Amount)	-0.0003	-0.028***	0.000
	(0.013)	(0.011)	(0.008)
Origination Interest Rate	0.003	-0.001	-0.023***
	(0.002)	(0.001)	(0.003)
Fixed Interest Rate	-0.095***	-0.067***	0.002
	(0.012)	(0.009)	(0.007)
Term = 15 Years	-0.180***	-0.049	-0.042***
	(0.068)	(0.036)	(0.013)
Term = 20 Years	-0.233	0.008	-0.007
	(0.157)	(0.107)	(0.026)
Insurance	-0.091***	0.010	0.018
	(0.018)	(0.009)	(0.011)
Refinancing Loan	-0.075***	-0.038***	-0.001
	(0.008)	(0.007)	(0.005)
Option ARM	0.009	0.006	0.063***
	(0.012)	(0.007)	(0.009)
Single Family Home	0.006	-0.013	-0.018***
	(0.010)	(0.009)	(0.006)
Primary Residence	0.009	-0.001	-0.002
	(0.016)	(0.011)	(0.008)
No Income Documentation	0.0001	0.009	-0.004
	(0.014)	(0.010)	(0.008)
Low Income Documentation	-0.085***	-0.027***	0.005
	(0.012)	(0.006)	(0.004)
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
Observations	15,945	15,945	7,893
Adusted R-Squared	0.083	0.030	0.089

Table 4: Baseline IV Regressions

The dependent variables are indicators for foreclosure initiation, foreclosure completion, and modification within six months of first serious (60+ days) delinquency. The regressions estimate linear probability models for these indicators using origination month indicators 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 analyze baseline sample jumbo loans, which became seriously (60+ days) delinquent within one year of origination. The modification regression is restricted to mortgages with term length data. The weak identification test is a Kleibergen-Paap F statistic. R-squared statistics are calculated within MSAs. Clustered (by MSA) standard errors are in parentheses. * represents 10% significance, ** represents 5% significance, *** represents 1% significance.

	(1) OLS	**(2) IV**	**(3) IV**	**(4) IV**
	Privately Securitized	**Foreclose Start**	**Foreclose**	**Modify**
Mean	0.638	0.695	0.135	0.052
Privately Securitized		0.080***	0.047***	-0.036***
		(0.016)	(0.012)	(0.009)
February Origination	-0.048***			
	(0.015)			
March Origination	-0.053***			
	(0.015)			
April Origination	-0.097***			
	(0.016)			
May Origination	-0.171***			
	(0.021)			
June Origination	-0.338***			
	(0.022)			
July Origination	-0.533***			
	(0.020)			
August Origination	-0.695***			
	(0.019)			
Loan Characteristic Controls	Yes	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes	Yes
Origination Month FE		No	No	No
MSA FE	Yes	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	No	No	No	No
Observations	15,945	15,945	15,945	7,893
Adusted R-Squared	0.324	0.082	0.029	0.090
Weak Identification F-stat	396			

Table 5: Robustness Checks

Regressions are the same as columns 2-4 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 A analyze only loans originated between May and July of 2007. Columns 1-3 of Panel B control for origininationmonth fixed effects using non-jumbo loans. Columns 4-6 of Panel B estimate bivariate probit models without MSA fixed effects. R-squared statistics are calculated within MSAs. Clustered (by MSA) standard errors are in parentheses. * represents 10% significance, ** represents 5% significance, *** represents 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.)**	**Modify (12 mos.)**	**Foreclose Start (6 mos.)**	**Foreclose (6 mos.)**	**Modify (6 mos.)**
Mean	0.807	0.369	0.085	0.669	0.109	0.061
Privately Securitized	0.081***	0.061***	-0.066***	0.078**	0.042*	-0.080***
	(0.013)	(0.016)	(0.014)	(0.035)	(0.023)	(0.025)
Loan Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Origination Month FE	No	No	No	No	No	No
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	No	No	No	No	No	No
Observations	15,945	15,945	7,893	6,443	6,443	3,259
Adusted R-Squared	0.045	0.072	0.100	0.074	0.017	0.066

B. Non-jumbo origination month control regressions and bivariate probit models

	(1) IV	(2) IV	(3) IV	(4) Bivarite Probit	(5) Bivarite Probit	(6) Bivarite Probit
	Foreclose Start (6 mos.)	**Foreclose (6 mos.)**	**Modify (6 mos.)**	**Foreclose Start (6 mos.)**	**Foreclose (6 mos.)**	**Modify (6 mos.)**
Mean	0.695	0.135	0.052	0.695	0.135	0.052
Privately Securitized	0.097***	0.059***	-0.027**	0.068***	0.041**	-0.019
	(0.019)	(0.015)	(0.012)	(0.016)	(0.014)	(0.012)
Loan Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Origination Month FE	Yes	Yes	Yes	No	No	No
MSA FE	Yes	Yes	Yes	No	No	No
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	Yes	Yes	Yes	No	No	No
Observations	77,160	77,160	35,934	15,980	15,980	7,931
Adusted R-Squared	0.083	0.037	0.073			

Table 6: Full Sample IV Regressions

Regressions are the same as in Table 4 except that sample is expanded to include all jumbo sample loans that became delinquent prior to 2012. The dependent variables are indicators for foreclosure initiation, foreclosure completion, and modification within six months of first serious (60+ days) delinquency. The regressions estimate linear probability models for these indicators using origination month indicators as instruments for private securitization status six months after origination. All observable loan characteristics shown in Table 3 are included as unreported controls. R-squared statistics are calculated within MSAs. Clustered (by MSA) standard errors are in parentheses. * represents 10% significance, ** represents 5% significance, *** represents 1% significance.

	(1) IV	**(2) IV**	**(3) IV**
	Foreclose Start	**Foreclose**	**Modify**
Mean	0.488	0.057	0.071
Privately Securitized	0.124***	0.028***	-0.051***
	(0.007)	(0.003)	(0.005)
Loan Characteristic Controls	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes
Origination Month FE	No	No	No
MSA FE	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	No	No	No
Observations	93,330	93,330	48,289
Adusted R-Squared	0.119	0.049	0.022

Table 7: IV Regressions by Delinquency Year

Regressions are the same as in Table 6 except that the sample is split by the year in which a mortgage first becomes seriously (60+ days) delinquent. The dependent variables are indicators for foreclosure initiation (panel A), foreclosure completion (panel B), and modification (panel C) within six months of first serious delinquency. The regressions estimate linear probability models for these indicators using origination-month indicators as instruments for private securitization status six months after origination. All observable loan characteristics shown in Table 3 are included as unreported controls. R-squared statistics are calculated within MSAs. Clustered (by MSA) standard errors are in parentheses. * represents 10% significance, ** represents 5% significance, *** represents 1% significance.

A. Foreclosure initiation within six months

	(1) IV	**(2) IV**	**(3) IV**	**(4) IV**	**(5) IV**
Deliquency Year	**2007**	**2008**	**2009**	**2010**	**2011**
Mean	0.766	0.573	0.460	0.318	0.396
Privately Securitized	0.079***	0.125***	0.119***	0.167***	0.085***
	(0.029)	(0.016)	(0.013)	(0.018)	(0.028)
Observations	7,647	27,520	32,514	15,937	9,537
Adusted R-Squared	0.042	0.086	0.088	0.066	0.027

B. Foreclosure completion within six months

	(1) IV	**(2) IV**	**(3) IV**	**(4) IV**	**(5) IV**
Deliquency Year	**2007**	**2008**	**2009**	**2010**	**2011**
Mean	0.173	0.064	0.036	0.035	0.048
Privately Securitized	0.062***	0.038***	0.013**	0.036***	0.024**
	(0.022)	(0.006)	(0.006)	(0.009)	(0.010)
Observations	7,647	27,520	32,514	15,937	9,537
Adusted R-Squared	0.002	0.032	0.013	0.022	0.031

C. Modification within six months

	(1) IV	**(2) IV**	**(3) IV**	**(4) IV**	**(5) IV**
Deliquency Year	**2007**	**2008**	**2009**	**2010**	**2011**
Mean	0.045	0.064	0.068	0.078	0.105
Privately Securitized	-0.022*	-0.070***	-0.042***	-0.021	-0.072**
	(0.013)	(0.009)	(0.010)	(0.014)	(0.033)
Observations	3,844	13,486	17,787	8,091	4,930
Adusted R-Squared	0.112	0.041	0.026	0.010	0.024

Table 8: IV Regressions with a 3-Year Analysis Window (Full Sample)

Regressions are the same as in Table 6 that the dependent variables are now foreclosure initiation, foreclosure completion, and modification within three years instead of six months. The sample is jumbo loans that became delinquent prior to 2010. The regressions estimate linear probability models using origination month indicators as instruments for private securitization status six months after origination. All observable loan characteristics shown in Table 3 are included as unreported controls. R-squared statistics are calculated within MSAs. Clustered (by MSA) standard errors are in parentheses. * represents 10% significance, ** represents 5% significance, *** represents 1% significance.

	(1) **IV**	**(2)** **IV**	**(3)** **IV**
	Foreclose Start (3 Years)	**Foreclose (3 Years)**	**Modify (3 Years)**
Mean	0.789	0.369	0.235
Privately Securitized	0.087***	0.113***	-0.059***
	(0.008)	(0.012)	(0.010)
Loan Characteristic Controls	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes
Origination Month FE	No	No	No
MSA FE	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	No	No	No
Observations	67,780	67,780	35,189
Adusted R-Squared	0.082	0.149	0.085

Table 9: Modification Details (Full Sample)

All regressions are conditional on loans 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, term lengths, principal balances, and monthly payments. Private securitization status six months after origination is instrumented with origination-month indicators. All observable loan characteristics shown in Table 3 are included as unreported controls. The regressions analyze jumbo loans that became seriously delinquent before 2012 and are modified within six months of becoming seriously delinquent. The net change (Panel B) regressions exclude observations with extreme changes (rate changes over 10 ppt, term changes over 20 years, principal changes over 50%, and payment changes over 75%). R-squared statistics are calculated within MSAs. Clustered (by MSA) standard errors are in parentheses. * represents 10% significance, ** represents 5% significance, *** represents 1% significance.

A. Type of modification

	(1) IV	(2) IV	(3) IV	(4) IV
	Interest Modification	**Term Modification**	**Principal Decrease**	**Principal Increase**
Mean	0.343	0.375	0.058	0.509
Private Security	0.058**	-0.427***	-0.021	0.448***
	(0.023)	(0.030)	(0.020)	(0.039)
Loan Characteristic Controls	Yes	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes	Yes
Origination Month FE	No	No	No	No
MSA FE	Yes	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	No	No	No	No
Observations	3,378	3,378	3,378	3,378
Adusted R-Squared	0.642	0.322	0.021	0.244

B. Net changes

	(1) IV	(2) IV	(3) IV	(4) IV
	Interest Change (ppt)	**Term Change (mos.)**	**Principal Change (%)**	**Payment Change (%)**
Mean	-2.792	25.733	0.259	-27.302
Privately Securitized	0.385**	-68.693***	1.573**	4.034**
	(0.161)	(5.555)	(0.717)	(2.047)
Loan Characteristic Controls	Yes	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes	Yes
Origination Month FE	No	No	No	No
MSA FE	Yes	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	No	No	No	No
Observations	3,377	3,052	3,361	3,205
Adusted R-Squared	0.240	0.334	0.028	0.205

Table 10: Modification Effectiveness (Full Sample)

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 one year of modification. The regressions estimate linear probability models using origination month indicators as instruments for private securitization status six months after origination. Indicators for modification type are included where indicated. All observable loan characteristics shown in Table 3 are included as unreported controls. The regressions analyze jumbo loans that were cured through modification before 2012 within six months of becoming seriously delinquent. R-squared statistics are calculated within MSAs. Clustered (by MSA) standard errors are in parentheses. * represents 10% significance, ** represents 5% significance, *** represents 1% significance.

	(1) **IV**	**(2)** **IV**
	Redefault	**Redefault**
Mean	0.302	0.302
Privately Securitized	0.076**	0.042
	(0.034)	(0.045)
Interest Decrease		-0.094***
		(0.027)
Term Increase		-0.046
		(0.031)
Principal Decrease		-0.096***
		(0.032)
Principal Increase		0.048*
		(0.029)
Loan Characteristic Controls	Yes	Yes
Delinquency Month FE	Yes	Yes
Origination Month FE	No	No
MSA FE	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	No	No
Observations	3,058	3,058
Adusted R-Squared	0.199	0.210

Table 11: Summary of PSA Terms

The 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 pooling and servicing agreements (PSAs) (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 warranty	0	0%
Loan schedule is accurate	37	100%
Loans are current	31	84%
Loans had only limited past delinquency	22	59%
Servicing:		
General servicing responsibilities:		
Accepted industry practices	37	100%
Equivalent to portfolio loans	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:		
Must be in default or default is forseeable	23	62%
Must expect modification value to exceed foreclosure proceeds	8	22%
May not permanently decrease principal or 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:		
Without consent:		
Cure/correct terms	37	100%
Alter without adversely affecting certificateholders	12	32%
Required consent for other changes:		
Overall majority consent	37	100%
Overall supermajority (over 66%) consent	10	27%
Majority or supermajority consent in all affected classes	26	70%
Prohibition on decreasing or delaying payments without universal consent	37	100%

Table 12: PSA-Linked Loan Sample

Data comes from Core Logic loan data linked to my sample of PSAs from prime non-agency MBS deals closed between January and August of 2007. The sample consists of 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, and are located in U.S. MSAs outside of Alaska and Hawaii. The delinquent loan sample includes loans that became seriously (60+ days) delinquent between 2007 and 2011. Delinquency is 60+ day delinquency. Foreclosure initiation is the referral of a mortgage to an attorney to initiate foreclosure proceedings. Foreclosure completion is identified by post-sale foreclosure or REO status. Modifications are identified based on observed changes to loan terms.

	All Loans	**Delinquent Loans**
Number	85,036	18,049
Size (mean)	$707,542	$671,927
FICO (mean)	742	722
LTV (mean)	0.71	0.75
Ownership		
Private Security	100%	100%
Delinquency		
Within 1 year	1.1%	
Within 5 years	20.9%	
Foreclosure Initiation		
Within 6 months		51.7%
Within 1 year		60.6%
Foreclosure Completion		
Within 6 months		6.7%
Within 1 year		20.6%
Modification		
Within 6 months		6.3%
interest decrease		5.4%
term extension		
principal decrease		0.0%
principal increase		2.5%
Within 1 year		13.3%

Table 13: PSA Term Regressions

The dependent variables are indicators for foreclosure initiation, foreclosure completion, and modification within six months of first serious (60+ days) delinquency. All regressions are OLS. The reported independent variables are indicators for the presence of servicing contract terms. All observable loan characteristics shown in Table 3 are included as unreported controls. The regressions also control for MSA, origination month, delinquency month, and servicer fixed effects. The regressions analyze sample jumbo loans that became seriously (60+ days) delinquent between 2007 and 2011. R-squared statistics are calculated within MSAs. Clustered (by MSA) standard errors are in parentheses. * represents 10% significance, ** represents 5% significance, *** represents 1% significance.

	(1) **OLS**	**(2)** **OLS**	**(3)** **OLS**
	Foreclose Start	**Foreclose**	**Modify**
Mean	0.517	0.067	0.063
Permanent Principal and Interest Reductions Prohibited	0.137***	0.075***	0.001
	(0.025)	(0.013)	(0.011)
Term Extensions Limited	0.110***	0.039***	-0.020*
	(0.028)	(0.013)	(0.010)
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
Observations	18,049	18,049	18,049
Adusted R-Squared	0.138	0.049	0.034