The Effect of Mortgage Securitization on Foreclosure and Modification

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Abstract: Did securitization exacerbate the foreclosure crisis by altering mortgage servicing practices? I exploit the unanticipated freeze of private mortgage securitization in 2007 to provide new evidence that securitization increases foreclosure probability and decreases modification probability. These effects are economically large and persist over time even after implementation of the Home Affordable Modification Program (HAMP) in 2009. Using hand-collected data on the contractual terms of servicing agreements, I show that servicers typically have broad discretion to modify loans but face significant incentives favoring foreclosure. The evidence implies that securitization significantly increased foreclosure rates during and after the crisis.

JEL Classification: G01, G21, G32

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1. Introduction

Since the start of the financial crisis, 5.3 million U.S. homes have been foreclosed. Roughly half of these foreclosures stemmed from privately securitized mortgages, prompting policy makers and economists to worry that securitization alters how loans are serviced and leads to unnecessary foreclosures. Previous studies find that private securitization increases foreclosure rates and decreases modification rates (Piskorski, Seru, and Vig, 2010; Agarwal, et al., 2011). These studies focus on the run-up to and early stages of the financial crisis. I reassess this issue with a long time series and different approach focused on the later stages of the crisis when delinquencies became more common.

I contribute to the literature in four ways. First, I use a long time series to reassess the quantitative importance, persistence, and economic significance of securitization's effect on foreclosure and modification in the aftermath of the financial crisis. Consistent with Piskorski, Seru, and Vig, (2010) and Agarwal, et al. (2011), I find that securitization significantly increases foreclosure probability and decreases modification probability, and when securitized loans are modified, the modifications tend to be less concessionary and more prone to redefault. The effects are permanent and economically large. Second, I show that securitization increased foreclosure probability throughout 2007 to 2012, even after significant government intervention through the Home Affordable Modification Program (HAMP) in 2009. Third, I base these conclusions on a new identification strategy that exploits the freeze of private securitization markets starting in August of 2007. Because my results are based on an entirely new source of variation, they provide strong confirmation that the effect of securitization on foreclosure and modification is causal. Finally, I use hand-collected data on the contractual terms of servicing agreements to show that servicers typically have broad discretion to modify loans but face significant incentives favoring foreclosure. Where contractual modification restrictions exist, foreclosure probabilities are higher, but the effects are not large enough to explain the full impact of securitization on foreclosure and modification.

Relative to portfolio loans held directly on bank balance sheets, private securitization increases the probability of foreclosure initiation within twelve months of a mortgage's first serious delinquency by 11.2 ppt (19% of the mean foreclosure initiation rate). Similarly, securitization increases the probability of foreclosure completion by 7.5 ppt (43% of the mean) and decreases the probabil-

ity of modification by 5-11 ppt (33-78% of the mean), depending on how modification is measured. These results imply that securitization significantly exacerbated the foreclosure crisis. Taken at face value, they imply that approximately 950,000 of the 5.3 million foreclosures experienced since the start of the financial crisis were caused by securitization. This is the partial equilibrium effect of securitization through changes in loan servicing. Securitization also affects loan origination, and foreclosures could be amplified in general equilibrium due to spillover on house prices. As a result, this estimate likely understate the full impact of securitization on foreclosure.

In part motivated by the high foreclosure rate 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 consistent across mortgages. I test the consistency of foreclosure and modification practices across securitized and portfolio loans before and after HAMP and find that private securitization increased foreclosure probability throughout 2007 to 2012. Using a triple difference identification strategy, I find that HAMP mitigated the foreclosure completion difference between securitized and portfolio loans that qualify for HAMP modifications. However, securitized foreclosure rates remained elevated after 2009 even for qualifying loans. HAMP's impact on securitized modifications depends on how modifications are identified. Whereas reported modification rates are more consistent after 2009, imputed modification rates based on observed changes to loan terms remain lower for securitized loans throughout the sample. The overall evidence suggests that HAMP mitigated but did not eliminate foreclosure and modification differences between securitized and portfolio loans. As a result, securitized loans are substantially more likely to be foreclosed and less likely to be modified compared to portfolio loans despite HAMP. To the extent that HAMP mitigated these differences, the 950,000 foreclosures caused by securitization would be even higher without HAMP.

I estimate the causal effect of securitization on foreclosure and modification of delinquent loans by exploiting the sudden and unexpected freeze of private mortgage securitization starting in August of 2007. 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 also control for potential changes to the lending environment over time using a difference-in-differences methodology with high quality non-

jumbo loans, which are primarily securitized by Fannie Mae and Freddie Mac and were unaffected by the private securitization freeze.

Securitization's impact on foreclosure and modification of delinquent loans stems from the principal-agent relationship between MBS investors and mortgage servicers. Because dispersed investors cannot directly manage mortgages, securitization involves delegating loan management to a servicer, subject to an incomplete contract. Servicing current mortgages is relatively straightforward to contract, but loss mitigation (including modification) for delinquent mortgages involves significant discretion. This gives servicers an incentive to underinvest in costly loss mitigation.

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, servicing agreements create an extra incentive to pursue foreclosure instead of loss mitigation. Where they are present, modification restrictions increase foreclosure probability in loan-level regressions, but they are too rare and insufficiently binding to explain the full effect of securitization on foreclosure and modification. Ex-post renegotiation of servicing contracts is precluded by trust passivity, stringent contract amendment requirements, and investor dispersion. 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.

My results relate most directly to recent work by Piskorski, Seru, and Vig (2010), Agarwal, et al. (2011), and Adelino, Gerardi, and Willen (2013). Piskorski, Seru, and Vig find that privately securitized loans are more likely to foreclosed, and Agarwal et al. find that privately securitized loans are less likely to be modified. To control for potential endogeneity of securitization, both papers condition their samples on loans becoming delinquent, control for observable characteristics, and restrict their samples to high quality loans. Agarwal, et al. also control for servicer fixed effects using servicer identifiers, which are available in their data. Piskorski, Seru, and Vig complement their baseline analysis with a quasi-experiment for securitization based on early payment default clauses, which require some originators to buy back loans that become delinquent within 90 days of securitization. In contrast, Adelino, Gerardi, and Willen find that private securitization does not impede modification and differences in modification rates are economically small relative to larger economic forces that kept one-year modification rates under 10% until 2008 for all loans, including

portfolio loans. My results confirm the findings of Piskorski, Seru and Vig and Agawal et al. in a new sample with a new identification strategy and extended time horizon. I discuss my results relative to the previous literature in more detail in Section 3.6.

This paper adds to a growing literature on the relation between mortgage securitization and the high level of foreclosure observed during the recent crisis. In part motivated by foreclosure externalities [see Campbell, Giglio, and Pathak (2011); Anenberg and Kung (2014)], economists and policymakers worry that unnecessary foreclosures caused by securitization may have exacerbated the foreclosure crisis. Securitization can increase foreclosures through two channels. First, securitization may decrease the quality of loans being originated [Keys et al. (2010); Purnanandam (2011)]. Second, securitization may change how loans are serviced by distorting incentives or introducing contractual frictions.

I focus on securitization's impact on how loans are serviced. Zingales (2008), Posner and Zingales (2009), and White (2009) propose that servicing distortions may explain why foreclosure is common despite being costly to lenders. Cordell, et al. (2008) note that servicers lack adequate loss mitigation capacity and have incentives that may not be aligned with investors. Mayer, Morrison, and Piskorski (2009); Gelpern and Levitin (2009); and Levitin and Twomey (2011) note that some mortgage servicing contracts restrict modifications and incentivize servicers to pursue foreclosure over modification. On the other hand, Foote, et al. (2009) note that modification may be more costly to investors than foreclosure, and Adelino, Gerardi, and Willen (2013) find that modifications are rare even for portfolio loans. Supporting this idea, Ghent (2011) finds that modifications were also rare during the Great Depression. Zhang (2013) finds that securitized loan modifications redefault at higher rates than portfolio loan modifications. Maturana (2016) finds that foregone modifications of securitized loans would have benefitted investors.

Securitization is not the only friction faced by mortgage servicers. Agarwal et al. (2016) find that lack of loan modification capability significantly constrains HAMP modifications by many mortgage servicers. Roshak (2015) finds that reluctance to realize losses also decreases modifications for portfolio loans. These complementary findings give even more reason to fear that foreclosure levels are unnecessarily high.

2. Data and methodology

To test the effect of securitization on foreclosure and modification, I use loan-level data from Lender Processing Services (LPS). To analyze the mechanism through which securitization affects mortgage servicing, I supplement this data with hand-collected data on servicing agreement terms, which I match to loan-level Core Logic data on privately securitized loans. I discuss these additional data sources in Section 4.

The LPS data consist 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 data included 33 million active mortgages, representing approximately 60% of the U.S. mortgage market. Importantly, the data span all mortgages serviced by the participating servicers, including portfolio loans, loans securitized by Fannie Mae and Freddie Mac (the Government Sponsored Enterprises, GSEs), and privately securitized loans.

My analysis focuses on first lien loans originated between January and August of 2007. To avoid survivor bias, I only consider loans that enter the LPS data within four months of origination. I drop government sponsored loans like VA and FHA loans because these loans may have different servicer requirements and incentives. To eliminate outliers and focus on reasonably typical prime and 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 do not 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 (2013). The resulting sample consists of 1.9 million loans.

Table 1 describes the sample. It includes 262,000 jumbo loans (i.e., loans over $417,000, which are not eligible for GSE securitization)[1] 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 (28%)

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

were held as portfolio loans. By contrast, 81% of non-jumbo loans were securitized by the GSEs. Delinquency is common in both sub-samples. Six percent 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. The delinquent loan sample, which consists of all loans that became seriously delinquent by June of 2013, has 100,000 jumbo loans and 453,000 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 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 post-sale foreclosure or real estate owned status. Piskorski, Seru, and Vig (2010) 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 the twelve-month window I use for most of my analysis. As reported in Table 1, foreclosure is initiated within one year of first serious delinquency for 59% of jumbo loans and completed for 18%. Foreclosure rates are about the same for non-jumbo loans.

Identifying loan modifications is more complicated because not all servicers report modifications. I use two measures for loan modifications. First, I impute modifications based on 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 the internet appendix, is essentially the same as the algorithm employed by Adelino, Gerardi, and Willen (2013). 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 interest rates, term to maturity, or principal balances. In particular, it does not capture temporary payment plans or principal forbearance. 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 imputed modification analysis to these loans.

Second, following Agarwal, et al. (2011) and Zhang (2013), I use servicer-reported modifications, which are available in the LPS data starting in 2008 for servicers representing approximately 75% of the observations in my delinquent jumbo loan sample. As reported on Table 1, imputed and reported modification rates are about the same. By both measures, approximately 15% of delinquent loans are modified within twelve months of becoming seriously delinquent. Where the imputed and reported modification samples overlap (i.e., post-2007 delinquencies for loans with term to maturity data that are serviced by servicers that report modifications), imputed and reported modifications have a correlation of 81%.

2.1. Instrumental variables methodology

Empirically identifying securitization's impact on mortgage servicing requires holding loan quality constant. Securitization's impact on mortgage origination quality and originators' adverse selection incentive to securitize low-quality loans make this identification challenging because securitized loans may differ from portfolio loans on unobserved dimensions. Moreover, it is not clear which direction unobserved quality would bias the results. 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 lower quality. On the other hand, mortgage backed security (MBS) sponsors also have access to unobserved information, which they could use to select higher quality loans [Jiang, Nelson, Vytlacil (2014); Agarwal, Chang, and Yavas (2012)]. The impact of loan quality on foreclosure and modification decisions conditional on delinquency is also 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 re-

liability encourages foreclosure because a reliable borrower must have suffered a large shock before becoming delinquent on his loan.

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 (2015) 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 exploits loans being stuck on bank balance sheets in 2007. Using bank-level call report data, Purnanandam shows that in the cross section, 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 securitization endogeneity 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 Enterprises, 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 (Inside Mortgage Finance, 2008).

Private mortgage securitization abruptly decreased in the third quarter of 2007, disappeared by the end of 2007, and has essentially remained frozen since then. Fig. 1 plots prime securitization volume from 2000 to 2011 based on data from Inside Mortgage Finance. 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 freeze of asset backed commercial paper, previously a $1.3 trillion market that was heavily invested in MBS. Both freezes were unanticipated and appear to have been caused by sudden increases in investor apprehension and lack of liquidity for mortgage backed securities, particularly subprime MBS. Acharya, Schnabl, and Suarez (2013) and Kacperczyk and Schnabl (2010) date the collapse of asset backed commercial paper to the August 9, 2007 suspension of withdrawals at three BNP Paribus funds. Calem, Covas, and Wu (2013) and Fuster and Vickery (2015) discuss the private MBS issuance freeze, which they date to August 2007 and exploit as a liquidity shock to jumbo lending. Consistent with these shocks being unanticipated, asset backed commercial paper was liquidly rolled over at overnight spreads of 10 basis points until immediately before August 9, 2007, at which point spreads jumped to 150 basis points within one day (Acharya, Schnabl, and Suarez, 2013). Similarly, ABX price indices for AAA subprime MBS fell below unity for the first time only shortly before the market freeze (see Fig. 2).[2] GSE credit guarantees prevented similar fears in the GSE MBS market, which continued to issue securities uninterrupted throughout 2007 and the rest of the financial crisis (see Fig. 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. The exclusion restriction underlying my analysis is that after conditioning on serious delinquency and controlling for observable characteristics, foreclosure and modification are uncorrelated with origination month except through its effect on securitization probability. This is a natural implication of an unanticipated shock to private securitization and is less restrictive than requiring that the shock was completely unanticipated by all agents. The evidence on asset backed commercial paper spreads and the ABX index indicates that the shock was unanticipated by asset market participants. Even if some investors or sponsors foresaw potential turmoil, the exclusion restriction is valid as long as loan origination was unaffected until after the shock. Origination volumes, interest rates, and loan characteristics indicate this was the case.

To confirm that the exclusion restriction is a reasonable assumption, I plot mortgage originations by month in Fig. 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 dramat-

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

ically fell in September of 2007 while non-jumbo lending (which was largely unaffected by private securitization) remained steady. This is the response we would expect from an unexpected freeze in private securitization. The internet appendix (Fig. A1-14) includes plots of loan characteristics by origination month. This evidence supports the origination volume data in Fig. 3. Loan characteristics were fairly stable from January to August of 2007, and jumbo and non-jumbo loans followed similar patterns. Of particular interest, jumbo interest rates tracked non-jumbo interest rates from January to August of 2007 and then increased in September relative to non-jumbo interest rates, consistent with a shock to jumbo financing starting in September.

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. Internet Appendix Table A1 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. Fig. 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 80% throughout 2007.

A drop in originations after August 2007 (Fig. 3) and a decrease in securitization rates for loans originated in the three months leading up to August 2007 (Fig. 4) is exactly what we would expect from an unanticipated shock to securitization in August and is difficult to reconcile with other stories. In particular, flat origination volumes are inconsistent with originators anticipating the freeze or changing lending standards before August. Similarly, jumbo origination volumes and securitization rates show no signs of being affected by New Century's collapse and bankruptcy in March and April of 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 one year of first serious delinquency. 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 (Angrist and Pischke, 2009).

Identification of securitization status in Eq. (1) comes entirely from variation in when a loan was originated. The implied second stage regression exploits decreasing securitization rates by origination month leading up to August 2007 as opposed to the actual securitization status of individual loans. As a result, the identification strategy is robust to securitization being endogenous and even to securitization selection criteria changing over time.

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. Nonetheless, I include a rich set of observable loan characteristics in X_i to increase Eq. (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, 2013). 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.

Ideally, I would like to follow Agarwal et al. (2011) and include servicer fixed effects to control for differences in capabilities and practices across servicers. Unfortunately, the LPS data do not include servicer identifiers. This is a limitation of the LPS data relative to the OCC-OTS data used by Agarwal et al. Piskorski, Seru, and Vig (2010) and Adelino, Gerardi, and Willen (2013) face the same limitation. As with other control variables, servicer fixed effects only matter if they correlated with origination month.

One potential concern with this identification strategy is that the mortgage lending environment may have changed over the course of 2007 resulting in differences between origination month cohorts even though the securitization freeze was unanticipated. Fortunately, I have a natural control group that was not affected by the securitization freeze. Prime non-jumbo loans are predominately securitized by the GSEs, and GSE securitization was uninterrupted throughout 2007. 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 twelve 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, which allows for the possibility that private securitization has a different impact on jumbo and non-jumbo loans. $OrigMonth_i$ is a vector of origination-month dummy variables. X_i is a vector of the same loan characteristics and fixed effects included in Eq. (1).

Conceptually, Eq. (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 Eq. (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 (presented in Fig. 3 and the internet appendix) indicates that jumbo lending was stable and moved in parallel with non-jumbo lending until after August of 2007. In robustness tests, I control for time-series changes to the lending environment and limit my analysis to loans that may be less subject to changing unobservable characteristics. Additionally, monthly changes to foreclosure and modification probabilities track monthly changes to securitization probability. Even if there were time-series changes specific to jumbo lending, they are unlikely to rival the large drop in jumbo private securitization rates from 80% in April to 36% in August.

3. 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 by June of 2013. Before implementing my instrumental variables strategy, I first estimate Eq. (1) with origination month fixed effects using OLS regressions. Coefficient estimates and standard errors (clustered by MSA) are reported in Table 2. After controlling for observable loan characteristics, seriously delinquent securitized loans are 8.4 ppt more likely to have foreclosure initiated, 6.1 ppt more likely to have foreclosure completed, 8.4 ppt less likely to have an imputed modification, and 4.6 ppt less likely to have a reported modification within a year of becoming seriously delinquent. 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 four regressions are identical with two exceptions. As discussed in the previous section, I can only consistently impute modifications for loans that report their term to maturity on a monthly basis, and reported modification data start in 2008 and is not available for all servicers. I drop observations for which I do not have modification data, which decreases the sample size by about 50% for imputed modifications and 30% for reported modifications.

Like previous studies, my OLS regressions are subject to potential omitted variable bias. The direction of the 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 5.1 ppt whereas a low loan-to-value ratio decreases the same probability (see column (1) of Table 2).

Table 3 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 2 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. 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 May. Origination month is a powerful predictor for securitization. The within-MSA adjusted R-squared for the first stage regression is 0.25, and the Kleibergen-Paap F statistic is 423. In short, weak identification is not a problem.

Columns (2) to (5) of Table 3 report instrumental variables estimates for Eq. (1). Conditional on serious delinquency, private securitization increases the twelve-month probability of foreclosure initiation by 11.2 ppt and foreclosure completion by 7.5 ppt. Private securitization decreases the twelve-month probability of imputed modification by 10.9 ppt and decreases the probability of reported modification by 5.0 ppt. The coefficient estimates are all highly statistically significant and economically large. As percentages of mean rates, the foreclosure initiation coefficient is 19%, the foreclosure completion coefficient is 43%, the imputed modification coefficient is -78%, and the reported modification coefficient is -33%. Using origination month indicator variables results in seven instruments for private securitization. The bottom rows of columns (2) to (4) report Hansen's J-statistic tests for overidentification, which are insignificant at the 5% level.

Fig. 5 plots baseline sample first stage and reduced form origination month fixed effects for Eq. (1). Jumbo foreclosure initiation (panel A), foreclosure completion (panel B), imputed modification (panel C), and reported modification (panel D) 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. Fig. 5 also plots the reduced form of Eq. (1) for non-jumbo loans. Non-jumbo foreclosure and modification origination month fixed effects were mostly flat and unrelated to jumbo securitization probability over the sample period, suggesting that any changes to the lending environment between January and August of 2007 did not have a major impact on foreclosure and modification practices.

3.1. Robustness checks

The baseline results reported in Table 3 are based on a linear probability model with two stage least squares regressions. As an alternative specification, I consider bivariate probit models that jointly estimate securitization status and foreclosure or modification. 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. Columns (1) to (4) of Table 4, Panel A, report marginal effect estimates from the bivariate probit models. The marginal effects of private securitization on foreclosure initiation (10.5 ppt), foreclosure completion (7.2 ppt), imputed modification (-9.5 ppt), and reported modification (-5.2 ppt) are all close to my baseline estimates.

Two potential concerns with my baseline empirical strategy are that the jumbo lending environment changed between January and August of 2007 or the securitization freeze was anticipated, particularly late in the sample. The best evidence against these concerns 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 Fig. 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, estimating origination-month fixed effects with non-jumbo loans, and controlling for time-trends related to unobservable loan characteristics.

I first consider a restricted sample of 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 (5) to (8) of Table 4, Panel A show regression estimates for the restricted sample. The coefficient estimates are similar to the baseline results and remain highly significant even in the reduced sample.

To explicitly control for changes to the lending environment over time, I estimate Eq.(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 (4) of Table 4, Panel B. Foreclosure initiation (6.1 ppt), foreclosure completion (3.8 ppt), imputed modification (-10.4 ppt), and reported modification (-3.9 ppt) coefficient estimates are all close to their baseline values.

It remains possible that the jumbo lending environment changed differentially for jumbo loans relative to nonjumbo loans along unobservable dimensions. Recent work by Piskorski, Seru, and Witkin (2015) and Griffin and Maturana (2016a) showing that securitized loan characteristics were frequently misreported highlights one potential source of omitted variable bias. Given that loan misreporting declined during 2007, it could be correlated with my time-series instrument for securitization. To account for this possibility, I control for the level of second-lien misreporting in the month a loan was originated using data from Griffin and Maturana (2016a). Results, reported in Columns (5) to (8) of Table 4, Panel B, are essentially the same as in my baseline specification. In the internet appendix (Table A2), I control for a linear origination-month time trend with similar results.

As an additional precaution against omitted variable bias, I analyze restricted samples that may be less subject to misreporting and less sensitive to unobserved characteristics more generally. First, I follow Piskorski, Seru, and Vig (2010) and Agarwal et al. (2011) and limit my analysis to high quality loans, defined as loans with full income documentation and FICO scores of at least 680. Second, I restrict my analysis to refinance loans because Piskorski, Seru, and Witkin (2015) and Griffin and Maturana (2016a) find that misreporting is less common for refinance loans. Third, I restrict my analysis to zip codes with combined second-lien and owner-occupancy misreporting rates of less than the median rate of 7.5% based on data from Piskorski, Seru, and Witkin. Fourth,

I restrict my analysis to zip codes in which originators with a high incidence of misreporting have a market share of less than 5% based on data from Griffin and Maturana (2016b). Fifth, I drop MSAs with more than a 1% share of total 2007 LPS loan origination volume because Piskorski, Seru, and Witkin find that MSA loan share is associated with misreporting. Finally, I drop loans in California, Florida, Nevada, and Arizona because these states exhibited more extreme housing booms than other parts of the country and Piskorski, Seru, and Witkin find that they have higher levels of owner-occupany misreporting. Results for all six restricted samples are reported in Table A2 of the internet appendix. With the exception of foreclosure completion in the sample without CA, FL, NV, and AZ, all results are close to the baseline coefficient estimates and highly statistically significant.

Table A2 of the internet appendix also reports pre-HAMP results for foreclosure and modification within six months of delinquency in a sample restricted to loans that became delinquent within one year of origination. Finally, Table A2 considers: (i) dropping loan characteristic control variables; (ii) including mortgages that are transferred to non-LPS servicers; (iii) estimating Eq. (2) without the $NonJumbo_i * Sec_i$ interaction term; and (iv) estimating Eq. (2) on a restricted sample of loans near the jumbo threshold with origination values between $300,000 and $550,000. Results are consistent with my baseline estimates.

3.2. Long term impact

Private securitization increases the probability of foreclosure and decreases the probability of modification within 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 July of 2011. Table 5 reports the results. Private securitization increases the three-year probability of foreclosure initiation by 9.9 ppt, increases three-year probability of foreclosure completion by 12.3 ppt, decreases the three-year probability of imputed modification by 11.5 ppt, and decreases the three year probability of reported modification by 6.3 ppt. As a fraction of mean rates for privately securitized jumbo loans, these represent impacts of

13% for foreclosure initiation, 36% for foreclosure completion, -54% for imputed modification, and -24% for reported modification.

Since September of 2008, 5.3 million home foreclosures have been completed, roughly half of which were privately securitized.[3] If 36% of privately securitized foreclosures were caused by securitization, this implies that approximately 950,000 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 private securitization. That said, 950,000 could be a conservative estimate because it is limited to the partial equilibrium effect of securitization through changes in loan servicing. Keys et al. (2010) and Purnanandam (2011) find that securitization decreases loan origination quality, which is another channel through which securitization can cause foreclosures. Moreover, Campbell, Giglio, and Pathak (2011) and Anenberg and Kung (2014) find that foreclosures decrease neighborhood house prices, which could amplify foreclosures. Finally, other frictions such the lack of servicer modification capability identified by Agarwal et al. (2016) could also inhibit modification and encourage foreclosure.

3.3. Interpreting the results

The IV estimates of Tables 3-5 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, 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

[3] Foreclosure data is from CoreLogic National Foreclosure Reports (April 2013, March 2014, and October 2014). Piskorski, Seru, and Vig (2010) and Mayer (2009) estimate that half of foreclosure initiations were privately securitized mortgages based on Federal Reserve reports and private market data.

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 change 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. I focus on 2007 originations solely for identification purposes. My estimates should be valid for jumbo loans originated at other times as well. The estimates are also informative about private securitization of non-jumbo loans (e.g., subprime loans). 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.

While my identification strategy is specific to jumbo loans, I can check its external validity to some extent by estimating OLS regressions on an expanded sample. In the internet appendix (Table A3), I estimate three-year window regressions on an expanded sample that includes non-jumbo loans and loans with FICO scores below 620. The coefficient estimates are similar to the Table 5 jumbo IV regressions, supporting my estimate of the overall impact of securitized servicing in the previous subsection. Table A3 also reports OLS estimates for jumbo and subprime loans separately with similar results.

3.4. HAMP

To incentivize mortgage modifications and make modification practices more uniform, the federal government 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%, then extending loan terms to up to 40 years from the modification date, and then forbearing principal. In 2010, HAMP was expanded to include principal reductions. 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 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. For example, Agarwal, et al. (2016) conclude that HAMP increased modifications but has fallen short of program goals because of mixed servicer compliance. My instrumental variables 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. Policymakers were particularly concerned about the perceived bias of securitized loans towards foreclosure and away from modification. Did HAMP eliminate this bias?

I first examine how securitized and portfolio loan foreclosure and modification rates changed over time as HAMP was implemented. Fig. 6 plots twelve-month foreclosure initiation (Panel A), foreclosure completion (Panel B), imputed modification (Panel C), and reported modification (Panel D) rates for jumbo loans by quarter of delinquency. Over time, foreclosure rates fell, and modification rates increased, with a particular uptick in modification in 2009 around the time HAMP was implemented. Panel D plots reported HAMP modifications in addition to overall reported modification rates. Consistent with HAMP ramping up in mid-to-late 2009, HAMP modification rates were less than 1% for loans that became seriously delinquent in the first quarter of 2009, and by the fourth quarter of 2009, HAMP modification rates were 4.3% for privately securitized loans and 11.4% for other jumbo loans.

Throughout 2007 to 2013, privately securitized loans had higher foreclosure rates and lower imputed modification rates compared to loans that were not privately securitized. If anything,

differences between privately securitized and portfolio loans increased over time, particularly for foreclosure completion and imputed modification. However, the story for reported modifications is less clear. In early 2008, privately securitized loans were half as likely to receive a reported modification (5.4% compared to 10.6%). By contrast, 2010 reported modification rates were the same for securitized and non-securitized loans, suggesting that HAMP may have closed the gap in reported modification rates. On the other hand, reported HAMP modifications were lower for privately securitized loans in 2010, and overall reported modifications for privately securitized loans again dropped below other loans in 2011.

To more formally assess differences between privately securitized loans and other loans over time, I repeat my baseline empirical strategy on sub-samples of jumbo loans split by the year in which they became delinquent. Table 6 reports the results. Foreclosure initiation coefficients (Panel A) had no clear trend over time. Foreclosure completion coefficients (Panel B) declined in 2009 and then increased, both overall and as a percentage of mean foreclosure completion rates. Imputed modification coefficients (Panel C) grew throughout 2007 to 2012 but were reasonably stable as a percentage of mean imputed modification rates, which also grew during this time period. With the exception of the reduced sample size in 2013, private securitization increased foreclosure and decreased imputed modification probability by statistically significant and economically meaningful amounts in all years.[4] In short, there is no evidence that HAMP eliminated the bias of privately securitized loans toward foreclosure and away from imputed modification. By contrast, reported modification coefficients (Panel D) shrank after 2009 and are statistically indistinguishable from zero after 2010.

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. To test whether HAMP changed the effect of securitization on foreclosure and modification, I follow Agarwal et al. (2016), and exploit the fact that loans with a principal balance of over $729,750 are not eligible for HAMP modifications. Specifically, I estimate triple difference OLS regressions comparing the effect of private securitization on foreclosure and modification rates before and after

[4] The sample ends with June 2013 delinquencies because I analyze one-year foreclosure and modification rates, and my loan data ends in June of 2014.

HAMP for loans below and above the principal limit as of the time of their delinquency. To avoid inference challenges due to HAMP's gradual phase in, I drop loans that became delinquent in 2009 and use 2008 delinquencies as my pre-HAMP sample and 2010 delinquencies as my post-HAMP sample.

Table 7 reports the results. The primary coefficient of interest is the triple interaction of private securitization, post-HAMP, and principal balance below $729,750. For foreclosure completion, this coefficient is a highly significant -7.7 ppt, suggesting that HAMP mitigated the tendency of privately securitized loans to have higher foreclosure rates for qualified loans. While HAMP mitigated the effect of private securitization on foreclosure completion, it did not eliminate it. To calculate the effect of private securitization on foreclosure completion for loans under $729,750 in 2010, one needs to add the private securitization, private securitization $*$ post, private securitization $*$ (principal <= $729,750), and triple-interaction coefficients for a total effect of 5.9 ppt. Though not statistically significant, foreclosure initiation also has a negative triple interaction coefficient. The modification triple-interaction coefficients are both close to zero and statistically insignificant, but with standard errors of 2.1 ppt for imputed modifications and 1.9 ppt for reported modifications, I cannot rule out moderate effects. In the internet appendix (Table A4), I repeat the same regressions using HAMP's owner-occupancy requirement as an alternative eligibility cutoff with similar results.

The evidence suggests that HAMP led to more consistent reported modification rates and more consistent foreclosure completion rates for qualifying loans, but the overall bias of privately securitized loans toward foreclosure and away from imputed modification persisted even after HAMP.

3.5. 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 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 screening and negotiation to give modifications the best chance of successfully preventing future default. Agarwal, et al. (2011) test these predictions with OLS regressions of modification terms and redefault rates on securitization status and find that privately securitized loans are modified more generously

but have higher redefault rates. Zhang (2013) also estimates that redefault rates are higher for securitized loans.

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 2014 and had an imputed modification within twelve months of delinquency. 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 8 reports the results. Securitization decreases the incidence of interest modifications, term extensions, and principal decreases, and increases the incidence of principal increases.

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. In contrast to Agarwal et al.'s (2011) results, I find that privately securitized modifications are less concessionary across all terms. Securitized modifications have smaller interest rate cuts (by 27 bps), shorter term extensions (by 78 months), smaller principal decreases (by 2.1%), and smaller payment decreases (by 4.9%).

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 imputed modifications that cured delinquencies. Table 9 reports the results. In column (1), I estimate an IV regression of redefault on private securitization and control variables in the full sample of jumbo loans. Redefault is 7.0 ppt higher for privately securitized loans (compared to a mean redefault rate of 23%). This result is consistent with the redefault differences of Agarwal et al. (2011) and Zhang (2013). The higher redefault rate of securitized modifications is partially explained by the types of modifications employed. Column (2) includes controls for modification type. This decreases the private securitization coefficient to 4.1 ppt. Interest decreases, term extensions, and principal decreases are associated with lower redefault rates.

3.6. Comparison to previous literature

My results support Piskorski, Seru, and Vig's (2010) finding that securitization increases foreclosure rates and Agarwal, et al.'s (2011) finding that securitization decreases modification rates and contrast with Adelino, Gerardi, and Willen's (2013) finding that modification rates are similar for securitized and portfolio loans. My analysis differs from previous studies in that I use a longer time series, different sample, and different source of variation in securitization status. Thus, my results provide strong confirmation that securitized mortgage servicing has a causal impact on modifications and foreclosures that is economically large and has persisted over time.

In contrast to the low modification rates reported by Adelino, Gerardi, and Willen (2013) prior to 2008, modification is reasonably common following the financial crisis. In my sample, 14% of delinquent loans are modified within one year of delinquency, and 25% are modified within three years. This is consistent with modification rates rising, as shown by Adelino, Gerardi and Willen. Further highlighting the importance of modifications, Keys et al. (2013) show that 4 million loans were modified during the recent crisis, most of which were performed without government support. As a result, frictions inhibiting modification of securitized loans have a large economic effect.

Adelino, Gerardi, and Willen (2013) advocate using a sample that ends in September of 2008 because government intervention in the mortgage market may have increased after Lehman Brothers declared bankruptcy. I find that securitization decreases modification rates and increases foreclosure rates even for loans that became delinquent in 2007. Specifically, Table 10 shows that for 2007 delinquencies, securitization increases foreclosure initiation within one year by 8.7 ppt (10% of the mean), increases foreclosure completion within one year by 5.4 ppt (12% of the mean), and decreases imputed modification within one year by 2.5 ppt (132% of the mean). This is consistent with Agarwal, et al.'s (2011) finding that securitization decreases modification rates for loans that became delinquent both early and late in 2008. In contrast, Adelino, Gerardi, and Willen (2013) find that securitization does not decrease modification in a sample that ends in September of 2008. Limited government intervention in the fourth quarter of 2008 is unlikely to drive the one-year regression results for my sample of 2007 delinquencies. To confirm this is the case, in Table A5 of the internet appendix I repeat the 2007 delinquency regressions using only modifications that were completed by September of 2008. Results are close to those reported in Table 10, indicating that

securitization's impact on modification and foreclosure predates the Lehman Brothers bankruptcy.

Given the different methodologies, samples, and time periods involved, my estimates are remarkably consistent with Piskorski, Seru and Vig (2010) and Agarwal et al. (2011). For example, Piskorski, Seru, and Vig find that securitization increases foreclosure probability by 4-7 ppt, and Agarwal et al. find that securitization decreases reported modification probability by 4.2 ppt. My baseline estimates for these outcomes in Table 3 are 7.5 ppt and -5.0 ppt, respectively. As a percentage of mean foreclosure rates for securitized mortgages, Piskorski, Seru and Vig's coefficients range from 18% to 33%. Applying the methodology described in Section 3.2, this implies 500,000 to 850,000 foreclosures were caused by securitization, which is similar to the 950,000 foreclosures my estimates attribute to securitization.

4. Mechanism

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

Securitized mortgages are owned by passive trusts on behalf of dispersed investors and managed by servicers subject to servicing contracts. 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. Servicer ownership of mortgage backed securities potentially mitigates incentive problems, but principal-agent incentive differences are inherent to the economics of securitization as long as servicers have less than complete ownership of the mortgages. Moreover, Huang and Nadauld (2014) find that equity retention promotes modifications that are harmful to investors.

To better understand how securitization affects mortgage servicing, I hand collect the contractual terms of servicing agreements and link these terms to loan-level data on modifications and foreclosures. I find that servicing agreements universally incentivize foreclosure over modification and other effort-intensive loss mitigation practices. Binding modification restrictions are less common and have a moderate impact on foreclosure probabilities.

4.1. Servicing practices

Foreclosure and modification are not binary responses to delinquency. Servicers have a wide range of loss mitigation options. For example, Fannie Mae's 2006 Servicing Guide describes notification letters, telephone calls, face-to-face interviews, debt collection, partial payments, delinquency counseling, grace periods, forbearance agreements, repayment plans, short sales, and deeds-in-lieu of foreclosure in addition to formal loan modification and foreclosure. Choosing among these alternatives requires significant servicer discretion, and most options involve personal interaction with borrowers. Securitization can bias whether and how all of these options are used.

Servicer automation and loss mitigation capacity further complicate delinquent loan servicing. Whereas modification and informal loss mitigation require significant discretion, foreclosure can be largely outsourced and automated. Levitin and Twomey (2011) describe this automation and argue that heavy reliance on automation left servicers ill-equipped to deal with rising delinquency rates. Similarly, Agarwal, et al. (2016) find that lack of organizational capability significantly impeded HAMP modifications.

4.2. 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 hand collect and analyze the terms of PSAs. My sample consists of all prime MBS deals between January and August of 2007 that exceeded one billion dollars. Thirty seven deals meet this criteria, collectively representing $70 billion, which is 48% of total prime MBS issuance during this period based on data from Inside Mortgage Finance. The sample covers nine deal sponsors and seven servicers.

Table 10 summarizes the PSA servicing terms. Servicers generally have 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. The PSAs universally establish a responsibility to foreclose on sufficiently delinquent loans and provide reimbursement for foreclosure expenses. They allow foreclosure to be postponed or avoided if it is not in the best interest of certificate-holders, but this

is always an exception to the general rule of foreclosure.

Servicers 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. Modification and other loss mitigation practices are never explicitly required and are not reimbursed through regular loan payments or by the trust. 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.

PSAs frequently limit modifications to mortgages in or near default and sometimes require that modified loan values exceed expected foreclosure proceeds, but modification prohibitions are uncommon. Some PSAs restrict specific types of modifications. Decreasing principal balances or permanently decreasing interest rates is prohibited by 22% of PSAs, and increasing loan maturity beyond the maturity of other loans in the trust or the maturity of the trusts' certificates is prohibited by 14% of PSAs. Most PSAs lack these restrictions, and many kinds of modifications are permitted even when they are present. For example, temporary interest rate reductions and principal forbearance are permitted under all of the PSAs.

This is the largest survey of PSA terms that I am aware of and is the only one that focuses on prime MBS. Three studies survey subprime modification restrictions with similar results. Hunt (2009) finds that 67% of subprime deals limit modifications to loans in default or where default is foreseeable or imminent and 10% prohibit modifications. Credit Suisse (2007) finds that nearly all PSAs permit modification of loans in default or where default is reasonably foreseeable. Finally, a Bear Stearns study described by Bajaj (2007) and Hunt (2009) finds that only 10% of deals prohibit modifications. Thompson (2011) reviews the evidence on PSA terms and concludes that PSAs generally permit modification but incentivize foreclosure. In their internet appendix, Piskorski, Seru, and Vig (2010) describe the early payment default buyback terms of subprime MBS deals.

4.3. PSA term regressions

To assess how modification restrictions affect servicer behavior, I link PSAs to individual loans in Core Logic panel data. For comparability to my earlier analysis, I limit the sample to jumbo loans and impose the restrictions described in Section 3.[5] As described in Table 11, the linked data include 85,000 loans with an aggregate origination value of $60 billion. The loans are similar to the jumbo loans analyzed in Section 4 but default less. This difference may 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 21 thousand loans that became seriously delinquent between 2007 and 2013. Foreclosure and modification rates are similar to the baseline sample. Foreclosure is defined and identified as before. Imputed modification is the same as before except that I cannot identify term extensions in the Core Logic data. Thus, term modifications are missing from the PSA-linked data. I do not have servicer modification reports for the Core Logic data so my modification analysis in this section is limited to imputed modifications.

I regress foreclosure initiation, foreclosure completion, and imputed modification within twelve 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. If these terms bind, we should expect them to reduce modifications and 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. (2016)] find that servicers employ different modification and foreclosure practices. An important caveat is that within-servicer PSA 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.

[5] 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. Sample selection is the same as for the LPS sample except that I do not 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.

Table 12 reports the results. Prohibitions on permanent principal and interest reductions are associated with increased foreclosure (14.0 ppt for foreclosure initiation and 11.4 ppt for foreclosure completion). Prohibitions on term extensions are also associated with increased foreclosure (6.8 ppt for foreclosure initiation and 7.0 ppt for foreclosure completion). Point estimates suggest that both restrictions reduce imputed modifications, but the effects (-2.8 ppt and -3.6 ppt) are smaller and statistically insignificant. Because I am unable to identify modifications that solely extend mortgage terms, this likely underestimates the full impact of modification restrictions on modification probability. These results are directionally what we should expect. However the magnitudes are too small to explain overall foreclosure and modification differences. For example, the -2.8 ppt and -3.6 ppt modification effects applied to the 22% and 14% of securitized loans with these terms explain -1.1 ppt of the -10.9 ppt baseline imputed modification effect for securitized loans. Similarly, the foreclosure coefficient estimates, combined with the incidence of these terms explain 35% of the foreclosure initiation securitization effect and 45% of the foreclosure completion securitization effect.

5. Conclusion

This paper's contribution is fourfold. First, I use a long time series to reassess the quantitative importance, persistence, and economic significance of securitization's effect on foreclosure and modification in the aftermath of the financial crisis. Private securitization increases foreclosure probability (by 11.2 ppt for foreclosure initiation and 7.5 ppt for foreclosure completion) and decreases modification probability (by 10.9 ppt for imputed modifications and 5.0 ppt for reported modifications), and when securitized loans are modified, the modifications tend to be less concessionary and more prone to redefault. Second, I assess how the effect of securitization on foreclosure and modification changed over time, including periods before and after government intervention. The Home Affordable Modification Program (HAMP) succeeded to some extent at mitigating differences in reported modification and foreclosure completion rates between securitized and portfolio loans. However, securitization continued to increase foreclosure probability and decreased imputed modification probability throughout 2007 to 2012, even after implementation of HAMP in 2009. Third, I base these conclusions on a new identification strategy that exploits the freeze of private

securitization markets starting in August of 2007. Because my results are based on an entirely new source of variation, they provide strong confirmation that the effect of securitization on foreclosure and modification is causal. Finally, I describe the mechanisms through which securitization effects foreclosure and modification, highlighting that incentive differences are at least as important as contractual prohibitions.

The tendency of securitized loans to be foreclosed and not modified helps to explain why foreclosure is so prevalent. In my sample of 2007 jumbo mortgages, 36% of privately securitized foreclosure completions within three years of serious delinquency are attributable to securitized servicing. Extrapolated to all privately securitized mortgages, this adds up to approximately 950,000 of the 5.3 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. Moreover, this effect is purely from securitization's impact on mortgage servicing, which is additive to securitization's impact on loan origination quality.

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 are systematically foreclosed more and modified less. This is an important factor in the debate about securitized lending both in the mortgage market and elsewhere. Sub-optimal servicing is a cost of securitization that 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 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. 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.

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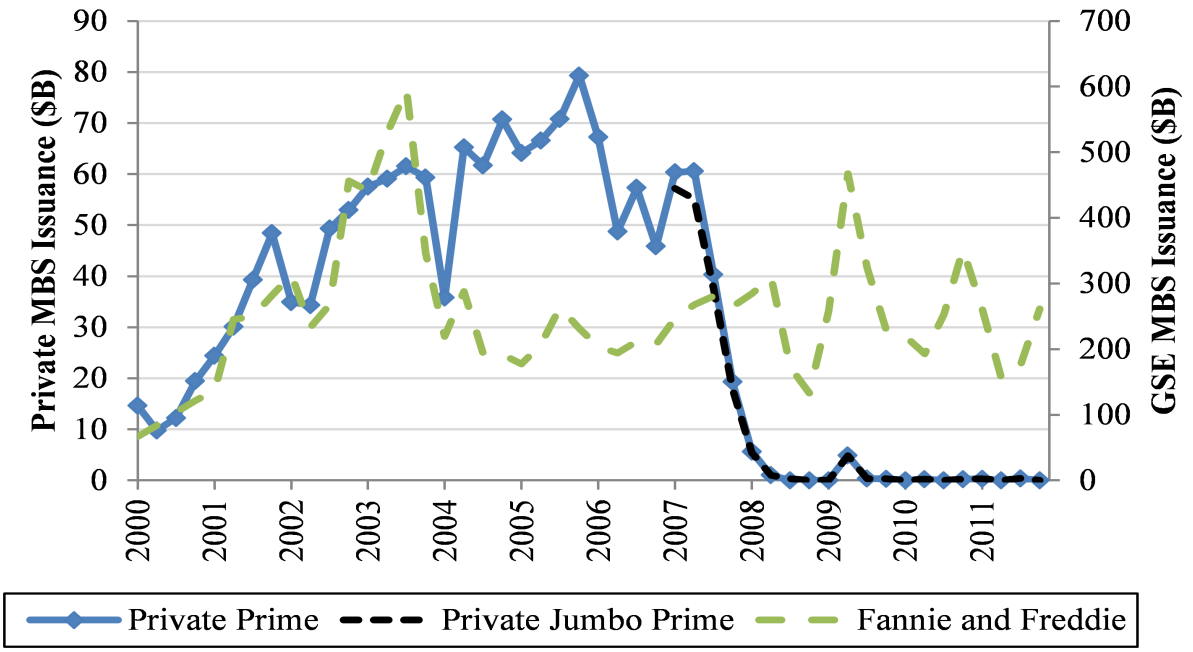

Fig. 1. MBS issuance. Prime mortgage backed security (MBS) issuance volume by quarter. Private issuance is plotted on the left axis. Fannie Mae and Freddie Mac (GSE) issuance is plotted on the right axis.

Fig. 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.

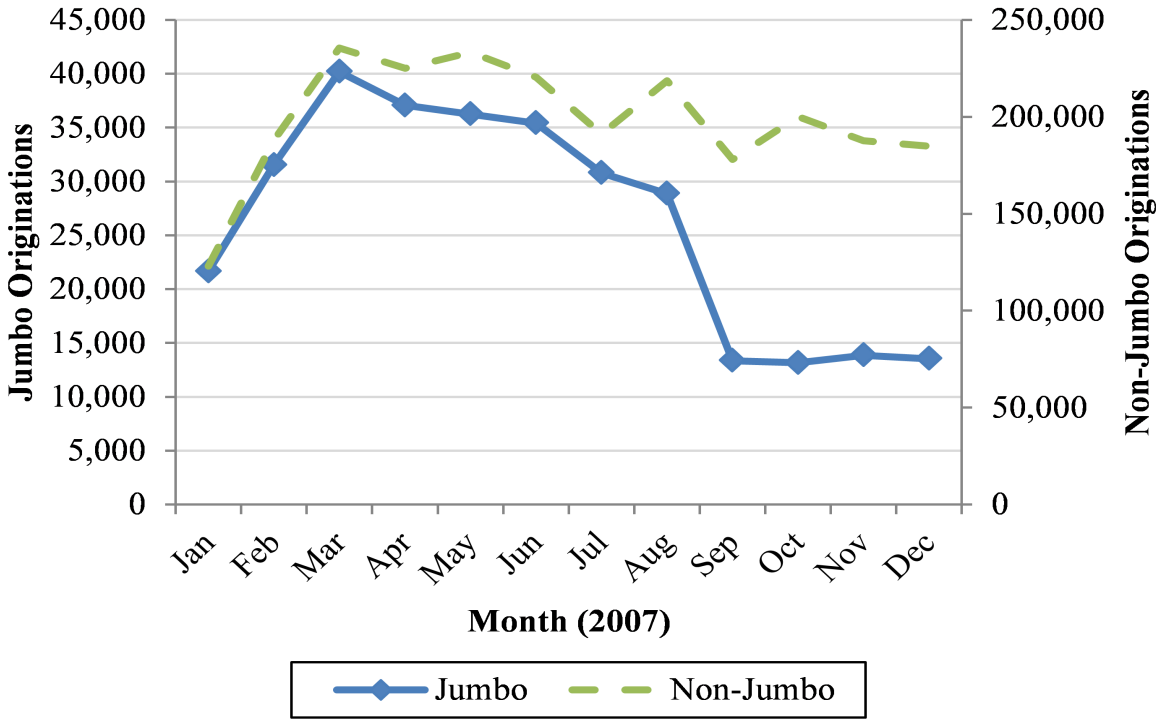

Fig. 3. Mortgage originations. Sample loan originations by month and size. Jumbo mortgages are loans over $417K, the conforming limit for Fannie Mae and Freddie Mac.

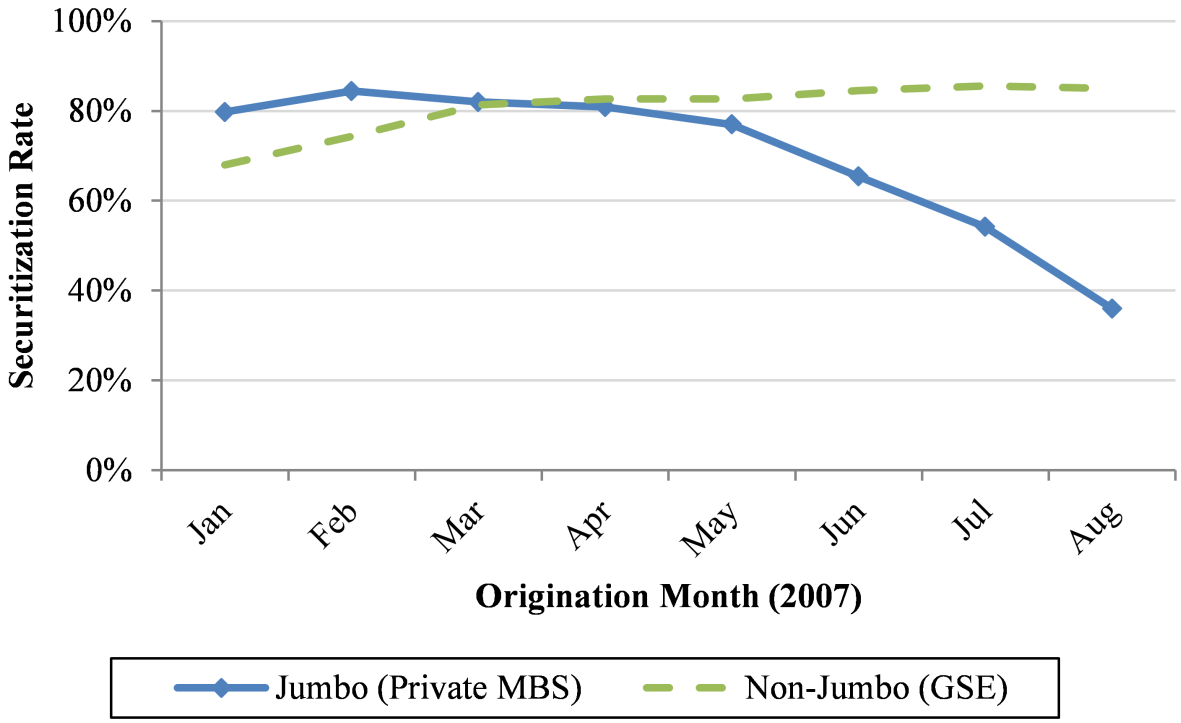

Fig. 4. Securitization rates by origination month. Percent of jumbo sample loans that are privately securitized and percent of non-jumbo sample loans that are securitized by Fannie Mae and Freddie Mac (the GSEs) by origination month. Securitization is measured as of six months after origination.

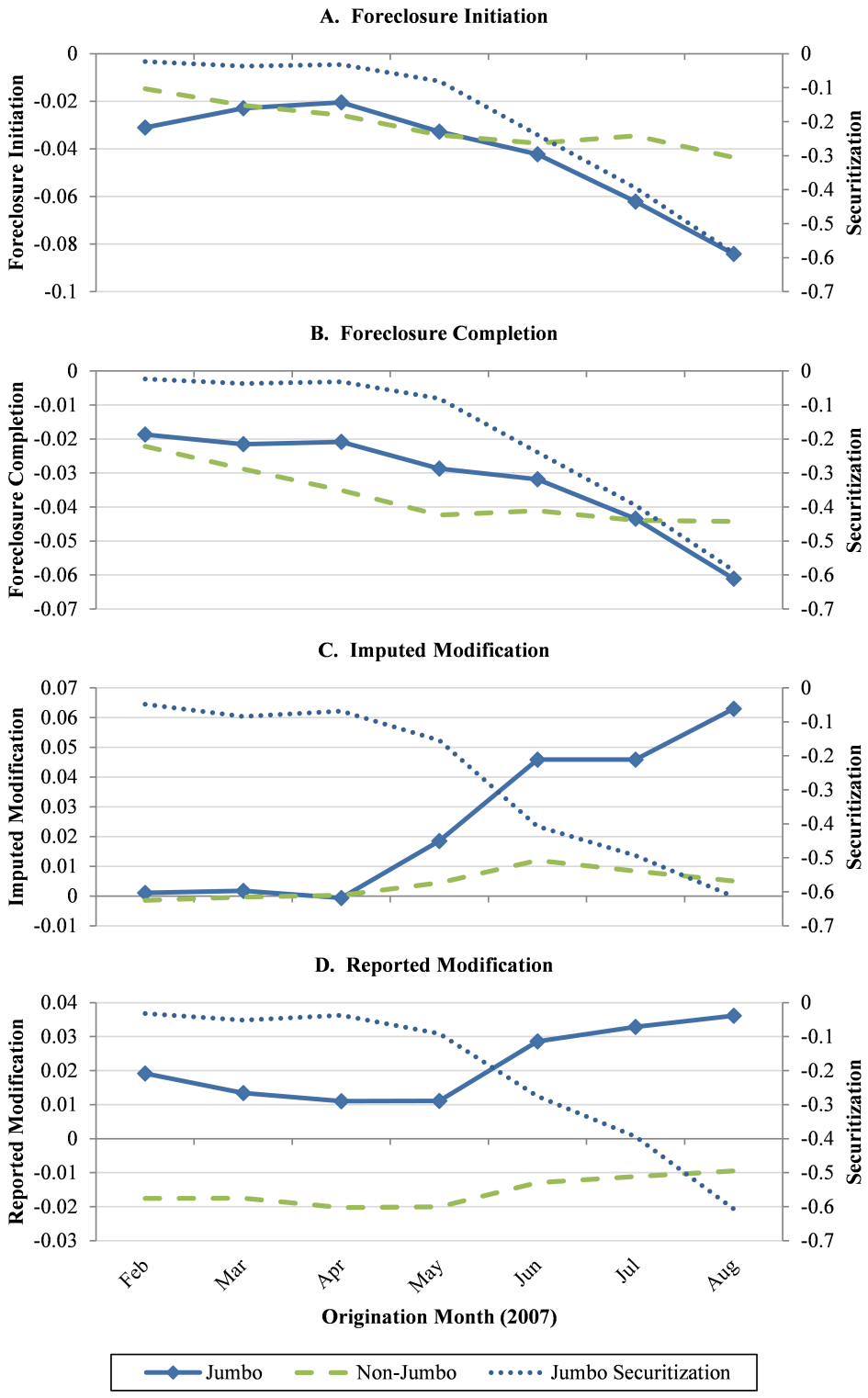

Fig. 5. Reduced form regression fixed effects. Jumbo fixed effects are from the reduced form of the baseline IV regressions reported in Table 3. Non-jumbo fixed effects are for identical regressions estimated for non-jumbo loans. All fixed effects are relative to January.

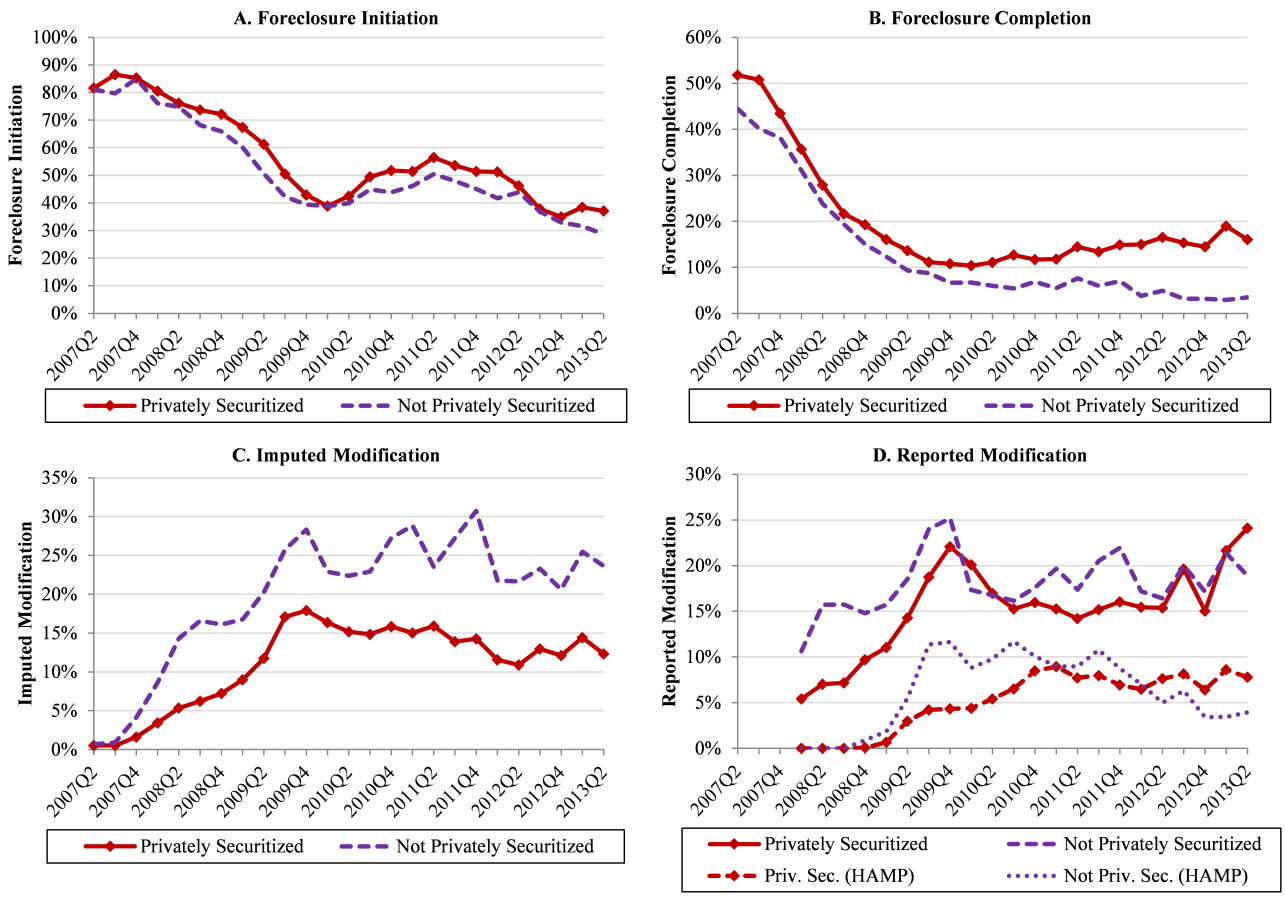

Fig. 6. Foreclosure and modification rates by delinquency month. Percent of loans that are foreclosed or modified within twelve months of becoming seriously delinquent by the month in which they became delinquent. The sample consists of jumbo loans originated between January and August of 2007.

Table 1.
Data summary.

Data come 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. Imputed modifications are identified based on observed changes to loan terms. Reported modifications are based on servicer modification reports. Redefault is a return to 60+ day delinquency after a modification cures an initial delinquency.

	All Loans		**Delinquent Loans**	
	Jumbo	**Non-Jumbo**	**Jumbo**	**Non-Jumbo**
Number	262,407	1,636,836	99,929	452,584
Size (mean)	$691,402	$210,355	$650,500	$229,391
FICO (mean)	733	726	713	700
LTV (mean)	0.73	0.72	0.77	0.78
Ownership				
Portfolio	27.5%	9.2%	25.4%	11.5%
Private Security	70.1%	9.4%	71.6%	15.9%
GSE	1.7%	80.9%	2.2%	72.0%
Delinquency				
Within 1 year	6.1%	3.7%		
Within 5 years	36.5%	26.5%		
Foreclosure Initiation				
Within 6 months			47.8%	49.5%
Within 1 year			59.4%	61.3%
Within 3 years			74.1%	75.9%
Foreclosure Completion				
Within 6 months			5.8%	6.7%
Within 1 year			17.5%	18.3%
Within 3 years			32.6%	38.8%
Imputed Modification				
Within 6 months			7.1%	7.0%
Within 1 year			14.0%	15.4%
Within 3 years			25.0%	26.9%
Reported Modification				
Within 6 months			7.6%	6.5%
Within 1 year			15.1%	14.5%
Within 3 years			27.6%	26.1%
Redefault				
Within 1 year			22.2%	24.1%

Table 2.
OLS regressions.

The dependent variables are indicators for foreclosure initiation, foreclosure completion, imputed modification, and reported modification within one year 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 before July of 2013. The imputed modification regression is restricted to mortgages with term length data. The reported modification regression is restricted to mortgages whoses servicers reported modifications as of the month the loan became 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) OLS	**(2) OLS**	**(3) OLS**	**(4) OLS**
	Foreclose Start	**Foreclose**	**Imputed Modify**	**Reported Modify**
Mean	0.594	0.175	0.140	0.151
Privately Securitized	0.084***	0.061***	-0.084***	-0.046***
	(0.007)	(0.006)	(0.005)	(0.003)
FICO >= 680	0.051***	0.042***	-0.029***	-0.029***
	(0.005)	(0.005)	(0.005)	(0.004)
LTV Ratio	0.001***	0.001***	0.002***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
LTV = 80	0.017***	0.015***	-0.010***	-0.010***
	(0.004)	(0.002)	(0.003)	(0.003)
log(Origination Amount)	0.037***	-0.008	-0.034***	-0.047***
	(0.007)	(0.006)	(0.003)	(0.003)
Origination Interest Rate	0.003**	0.002*	0.008***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Fixed Interest Rate	-0.159***	-0.129***	0.049***	-0.022***
	(0.009)	(0.010)	(0.004)	(0.005)
Term = 15 Years	0.026	0.020	-0.012	0.000
	(0.022)	(0.017)	(0.030)	(0.025)
Term = 20 Years	-0.080*	0.010	0.056	-0.035
	(0.046)	(0.025)	(0.050)	(0.030)
Insurance	-0.007	-0.004	-0.046***	-0.033***
	(0.008)	(0.006)	(0.010)	(0.007)
Refinancing Loan	-0.041***	-0.040***	0.032***	0.020***
	(0.004)	(0.004)	(0.003)	(0.002)
Option ARM	-0.063***	-0.044***	0.004	-0.039***
	(0.004)	(0.005)	(0.007)	(0.005)
Single Family Home	-0.003	-0.012***	0.025***	0.034***
	(0.006)	(0.004)	(0.005)	(0.003)
Primary Residence	-0.084***	-0.058***	0.060***	0.067***
	(0.006)	(0.007)	(0.005)	(0.005)
No Income Documentation	0.053***	-0.004	-0.009*	-0.017***
	(0.006)	(0.005)	(0.005)	(0.003)
Low Income Documentation	0.035***	0.018***	-0.011**	-0.031***
	(0.006)	(0.005)	(0.005)	(0.004)
Delinquency Month FE	Yes	Yes	Yes	Yes
Origination Month FE	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	No	No	No	No
Observations	99,891	99,891	51,984	70,291
Adjusted R-Squared	0.122	0.116	0.049	0.027

Table 3.
Baseline IV regressions.

The dependent variables are indicators for foreclosure initiation, foreclosure completion, imputed modification, and reported modification within one year 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 2 are included as unreported controls. 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 before July of 2013. The imputed modification regression is restricted to mortgages with term length data. The reported modification regression is restricted to mortgages whoses servicers reported modifications as of the month the loan became seriously delinquent. 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**	**(5)** **IV**
	Privately Securitized	**Foreclose Start**	**Foreclose**	**Imputed Modify**	**Reported Modify**
Mean	0.716	0.594	0.175	0.140	0.151
Privately Securitized		0.112***	0.075***	-0.109***	-0.050***
		(0.008)	(0.008)	(0.008)	(0.005)
February Origination	-0.023***				
	(0.005)				
March Origination	-0.037***				
	(0.005)				
April Origination	-0.032***				
	(0.005)				
May Origination	-0.081***				
	(0.007)				
June Origination	-0.239***				
	(0.010)				
July Origination	-0.395***				
	(0.010)				
August Origination	-0.587***				
	(0.013)				
Loan Characteristic Controls	Yes	Yes	Yes	Yes	Yes
Delinquency Month FE	Yes	Yes	Yes	Yes	Yes
Origination Month FE		No	No	No	No
MSA FE	Yes	Yes	Yes	Yes	Yes
Origination Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Include Non-Jumbo Loans	No	No	No	No	No
Observations	99,891	99,891	99,891	51,984	70,291
Adjusted R-Squared	0.251	0.121	0.115	0.048	0.027
Weak Identification F-stat	423				
Hansen's J-stat (p-value)		11.38	10.49	12.06	11.85
		(0.08)	(0.11)	(0.06)	(0.07)

Table 4.
Robustness checks.

Regressions are the same as columns 2-5 of Table 3 except where noted. Columns 1-4 of Panel A estimate bivariate probit models without MSA fixed effects. Columns 5-8 of Panel A analyze only loans originated between May and July of 2007. Columns 1-4 of Panel B control for origininationmonth fixed effects using non-jumbo loans. Columns 5-8 of Panel B control for time-series changes in second-lien misreporting and use indicator variables for June, July, and August originations as instruments for private securitization. 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. Bivariate probit and restricted origination-month sample

	(1) Bivarite Probit	(2) Bivarite Probit	(3) Bivarite Probit	(4) Bivarite Probit	(5) IV	(6) IV	(7) IV	(8) IV
	Foreclose Start	**Foreclose**	**Imp. Modify**	**Rep. Modify**	**Foreclose Start**	**Foreclose**	**Imp. Modify**	**Rep. Modify**
Mean	0.594	0.175	0.140	0.151	0.579	0.161	0.156	0.155
Priv. Sec.	0.105***	0.072***	-0.095***	-0.052***	0.094***	0.047***	-0.092***	-0.077***
	(0.008)	(0.007)	(0.008)	(0.005)	(0.017)	(0.014)	(0.020)	(0.019)
Loan Char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Delinq. Mo. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Orig. Mo. FE	No	No	No	No	No	No	No	No
MSA FE	No	No	No	No	Yes	Yes	Yes	Yes
Orig. Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug	May-Jul	May-Jul	May-Jul	May-Jul
Non-Jumbo	No	No	No	No	No	No	No	No
Obs.	99,929	99,929	52,024	70,338	37,948	37,948	19,618	27,580
R-Squared					0.113	0.097	0.039	0.022

B. Non-jumbo origination month and misreporting time series controls

	(1) IV	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV	(7) IV	(8) IV
	Foreclose Start	**Foreclose**	**Imp. Modify**	**Rep. Modify**	**Foreclose Start**	**Foreclose**	**Imp. Modify**	**Rep. Modify**
Mean	0.609	0.181	0.151	0.146	0.594	0.175	0.140	0.151
Priv. Sec.	0.061***	0.038***	-0.104***	-0.039***	0.104***	0.061***	-0.107***	-0.049***
	(0.009)	(0.008)	(0.010)	(0.006)	(0.009)	(0.008)	(0.010)	(0.007)
Loan Char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Misrep. Rate	No	No	No	No	Yes	Yes	Yes	Yes
Delinq. Mo. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Orig. Mo. FE	Yes	Yes	Yes	Yes	No	No	No	No
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Orig. Months	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug	Jan-Aug
Non-Jumbo	Yes	Yes	Yes	Yes	No	No	No	No
Obs.	552,474	552,474	263,959	392,890	99,891	99,891	51,984	70,291
R-Squared	0.081	0.075	0.063	0.038	0.121	0.116	0.048	0.027

Table 5.
IV regressions with a 3-year analysis window.

Regressions are the same as in Table 3 except that the dependent variables are now foreclosure and modification within three years instead of one year. The sample is jumbo loans that became delinquent prior to July of 2011. 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 2 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**	**(4) IV**
	Foreclose Start (3 Years)	**Foreclose (3 Years)**	**Imputed Modify (3 Years)**	**Reported Modify (3 Years)**
Mean	0.741	0.326	0.250	0.276
Privately Securitized	0.099***	0.123***	-0.115***	-0.063***
	(0.007)	(0.011)	(0.009)	(0.009)
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	88,774	88,774	45,905	61,373
Adjusted R-Squared	0.097	0.159	0.083	0.048

Table 6.
IV regressions by delinquency year.

Regressions are the same as in Table 3 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), imputed modification (panel C), and reported modification (panel D) within one year 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 2 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	(6) IV	(7) IV
Deliquency Year	**2007**	**2008**	**2009**	**2010**	**2011**	**2012**	**2013**
Mean	0.846	0.736	0.551	0.438	0.517	0.424	0.356
Privately Securitized	0.087***	0.110***	0.108***	0.163***	0.085***	0.072*	0.113
	(0.024)	(0.014)	(0.014)	(0.024)	(0.027)	(0.040)	(0.098)
Observations	7,628	27,447	32,460	15,918	9,526	5,657	1,012

B. Foreclosure completion within one year

	(1) IV	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV	(7) IV
Deliquency Year	**2007**	**2008**	**2009**	**2010**	**2011**	**2012**	**2013**
Mean	0.445	0.236	0.123	0.100	0.116	0.123	0.134
Privately Securitized	0.054**	0.093***	0.053***	0.084***	0.076***	0.121***	0.226***
	(0.026)	(0.016)	(0.010)	(0.015)	(0.014)	(0.021)	(0.071)
Observations	7,628	27,447	32,460	15,918	9,526	5,657	1,012

C. Imputed modification within one year

	(1) IV	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV	(7) IV
Deliquency Year	**2007**	**2008**	**2009**	**2010**	**2011**	**2012**	**2013**
Mean	0.019	0.093	0.167	0.182	0.188	0.148	0.166
Privately Securitized	-0.025**	-0.092***	-0.118***	-0.131***	-0.133***	-0.147***	-0.034
	(0.012)	(0.013)	(0.013)	(0.019)	(0.028)	(0.025)	(0.096)
Observations	3,836	13,445	17,753	8,079	4,927	3,011	698

D. Reported modification within one year

	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV	(7) IV
Deliquency Year	**2008**	**2009**	**2010**	**2011**	**2012**	**2013**
Mean	0.098	0.171	0.173	0.164	0.167	0.219
Privately Securitized	-0.057***	-0.070***	-0.028*	-0.033	0.000	0.057
	(0.011)	(0.010)	(0.015)	(0.024)	(0.031)	(0.068)
Observations	19,622	24,911	12,622	7,545	4,553	858

Table 7.
HAMP regressions.

The dependent variables are indicators for foreclosure initiation, foreclosure completion, imputed modification, and reported modification within one year of first serious (60+ days) delinquency. All regressions are OLS. All observable loan characteristics shown in Table 2 are included as unreported controls. The regressions analyze baseline sample jumbo loans, which became seriously (60+ days) delinquent in 2008 (pre-HAMP sample) or 2010 (post-HAMP sample). Privately securitized is an indicator for private securitization as of six months after origination. Post is an indicator for loans that became seriously delinquent in 2010, after HAMP was implemented. Principal <= $729,750 is an indicator for having a principal balance below that level (which is the cutoff for HAMP modification eligibility) as of the month the loan became seriously delinquent. The imputed modification regression is restricted to mortgages with term length data. The reported modification regression is restricted to mortgages whoses servicers reported modifications as of the month the loan became 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) OLS	**(2) OLS**	**(3) OLS**	**(4) OLS**
	Foreclose Start	**Foreclose**	**Imputed Modify**	**Reported Modify**
Mean	0.627	0.186	0.127	0.127
Privately Securitized	0.074***	0.002	-0.060***	-0.076***
	(0.015)	(0.014)	(0.013)	(0.014)
Privately Securitized * Post	0.015	0.072***	-0.005	0.078***
	(0.020)	(0.016)	(0.019)	(0.020)
Privately Securitized * Post * (Principal <= $729,750)	-0.043	-0.077***	0.009	-0.008
	(0.029)	(0.019)	(0.021)	(0.019)
Principal <= $729,750	-0.005	-0.049***	0.002	-0.020
	(0.017)	(0.015)	(0.013)	(0.014)
Post * (Principal <= $729,750)	-0.098***	0.043***	0.080***	0.082***
	(0.028)	(0.017)	(0.020)	(0.015)
Privately Securitized * (Principal <= $729,750)	0.016	0.062***	-0.027**	0.001
	(0.016)	(0.014)	(0.013)	(0.015)
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	43,404	43,404	21,560	32,280
Adjusted R-Squared	0.140	0.089	0.045	0.029

Table 8.
Modification details.

All regressions are conditional on loans having an imputed modification within one year of becoming seriously delinquent. 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 2 are included as unreported controls. 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	0.879	0.392	0.070	0.385
Private Security	-0.052**	-0.475***	-0.035***	0.186***
	(0.022)	(0.023)	(0.013)	(0.028)
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	7,282	7,282	7,282	7,282
Adjusted R-Squared	0.049	0.282	0.017	0.328

B. Net changes

	(1) IV	**(2) IV**	**(3) IV**	**(4) IV**
	Interest Change (ppt)	**Term Change (mos.)**	**Principal Change (%)**	**Payment Change (%)**
Mean	-3.219	48.760	0.164	-30.633
Privately Securitized	0.274***	-77.677***	2.071***	4.861***
	(0.067)	(3.873)	(0.463)	(0.958)
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	7,281	6,727	7,246	6,993
Adjusted R-Squared	0.089	0.322	0.032	0.063

Table 9.
Modification effectiveness.

All regressions are conditional on a loan being cured of initial delinquency with an imputed modification within one year of becoming seriously delinquent before July of 2013. 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 2 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**
	Redefault	**Redefault**
Mean	0.234	0.234
Privately Securitized	0.070***	0.041*
	(0.017)	(0.024)
Interest Decrease		-0.088***
		(0.015)
Term Increase		-0.037*
		(0.019)
Principal Decrease		-0.066***
		(0.020)
Principal Increase		0.016
		(0.013)
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	6,875	6,875
Adjusted R-Squared	0.145	0.152

Table 10.
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. Thirty seven 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**
General servicing responsibilities:		
Follow accepted industry practices	37	100%
Service equivalently to portfolio loans	25	68%
Pursue best interest of certificateholders	1	3%
Follow 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%

Table 11.
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 June of 2013. 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	84,987	20,715
Size (mean)	$707,606	$670,976
FICO (mean)	742	723
LTV (mean)	0.71	0.75
Ownership		
Private Security	100%	100%
Delinquency		
Within 1 year	1.1%	
Within 5 years	20.8%	
Foreclosure Initiation		
Within 6 months		49.3%
Within 1 year		57.8%
Foreclosure Completion		
Within 6 months		7.0%
Within 1 year		20.2%
Imputed Modification		
Within 6 months		6.0%
Within 1 year		12.6%

Table 12.
PSA term regressions.

The dependent variables are indicators for foreclosure initiation, foreclosure completion, imputed modification, and reported modification within one year 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 2 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 June of 2013. 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**	**Imputed Modify**
Mean	0.584	0.204	0.126
Permanent Principal and Interest Reductions Prohibited	0.140***	0.114***	-0.028
	(0.0256)	(0.0216)	(0.0185)
Term Extensions Limited	0.068**	0.070***	-0.036
	(0.0279)	(0.0199)	(0.0219)
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	20,362	20,362	20,362
Adjusted R-Squared	0.125	0.068	0.040