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Securitization and distressed loan renegotiation: Evidence from the subprime mortgage crisis ☆

Tomasz Piskorski [a], Amit Seru [b,*], Vikrant Vig [c]

[a] Columbia University, Graduate School of Business, USA
[b] University of Chicago, Booth School of Business, USA
[c] London Business School, UK

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ABSTRACT

We examine whether securitization impacts renegotiation decisions of loan servicers, focusing on their decision to foreclose a delinquent loan. Conditional on a loan becoming seriously delinquent, we find a significantly lower foreclosure rate associated with bank-held loans when compared to similar securitized loans: across various specifications and origination vintages, the foreclosure rate of delinquent bank-held loans is 3% to 7% lower in absolute terms (13% to 32% in relative terms). There is a substantial heterogeneity in these effects with large effects among borrowers with better credit quality and small effects among lower quality borrowers. A quasi-experiment that exploits a plausibly exogenous variation in securitization status of a delinquent loan confirms these results.

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* Corresponding author.
E-mail addresses: tp2252@columbia.edu (T. Piskorski), amit.seru@chicagobooth.edu (A. Seru), vvig@london.edu (V. Vig).

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1. Introduction

This paper is motivated by the recent foreclosure crisis. The non-agency securitized market (i.e., securitized mortgages issued without a guarantee from government-sponsored entities) has been at the core of this debate, as it has accounted for more than half of the foreclosure starts, despite its relatively small size.[1] This could simply reflect the greater risk of these mortgages, since many were "subprime loans" granted to borrowers with low credit ratings. There has been a concern among policymakers, however, that the high foreclosure rate on securitized mortgages might also be driven by other factors. One factor that has generated a great amount of controversy and has been a subject of ongoing debate[2] is whether dispersed ownership and potential agency frictions brought about by securitization of residential mortgages inhibited renegotiation of loans at risk of foreclosure, thereby aggravating the current foreclosure crisis.

This paper contributes to this debate by empirically investigating the impact of securitization on renegotiation decisions of loan servicers, focusing on their decision to foreclose a delinquent loan. Using a large database of mortgages that has information on whether a delinquent loan is held on the banks' balance sheets or securitized, we find that securitization does induce a foreclosure bias.[3] Controlling for contract terms and regional conditions, we find that seriously delinquent loans that are held by the bank[4] (henceforth called "portfolio" loans) have lower foreclosure rates than comparable securitized loans (between 3% (13%) and 7% (32%) in absolute (relative) terms).

There are several reasons why securitized loans might be serviced differently from those directly held on the banks' balance sheets. First, servicers may have different financial incentives to service securitized loans relative to the portfolio loans as, in the latter case, a servicer fully internalizes the costs and benefits of the decision to foreclose a delinquent loan (Jensen and Meckling, 1976).[5] Second, even if the incentives were well aligned, PSAs may legally restrain servicers from performing certain types of renegotiations.[6] Third, securitization creates dispersion in property rights—cash flow rights on a mortgage are held by several bondholders with varying seniority of claims. This raises concerns that complex capital structure, brought about by securitization, may create a coordination problem amongst investors making it harder for servicers to alter mortgage contracts.[7] It is important to note that this coordination problem not only makes it harder to renegotiate debt contracts, but it may also make it harder for the investors to correct the servicer incentive structure and the ensuing agency problem.[8] Finally, securitization could also affect some of the institutional constraints faced by lenders. For example, lenders may postpone foreclosures on their own delinquent loans to delay accounting recognition of their losses.[9]

It is of course possible that these constraints do not exist or that borrowers and investors are able to circumvent these frictions. As a result, securitization may not affect the decision of servicers to foreclose a delinquent loan. Ultimately, whether securitization affects this decision is an empirical question, one which we investigate in this paper. We do so by examining differences in servicing of securitized loans at risk of foreclosure relative to the loans held on the banks' balance sheets for every loan originated in 2005 and 2006. The main test of the paper assesses whether differences in foreclosure rates of delinquent loans depend on their securitization status.

Since loans that are securitized might differ on observables (such as credit scores) from those banks keep on their balance sheet, it is important to control for ex ante characteristics of the loan (i.e., when loans are originated). Our data set provides rich information for each loan in the sample, allowing us to use a relatively flexible specification with a host of loan and borrower

[1] The size of the market is about 15 percent of all outstanding mortgages. These numbers are as of January 2009. Source: Federal Reserve Bank of New York, Credit Conditions in the United States, http://www.newyorkfed.org/regional/subprime.html.

[2] See, among others, Adelino, Gerardi, and Willen (2009), Gelpern and Levitin (2009), Mayer, Morrison, and Piskorski (2009), Posner and Zingales (2009), and White (2009a, 2009b).

[3] We use the term bias in comparing the rate of foreclosure of securitized loans with the corresponding rate for portfolio mortgages without any efficiency implications.

[4] Throughout our paper we denote the loans owned by the lending institutions as "bank-held" irrespective whether these institutions have a formal bank status.

[5] In the case of a securitized loan, the servicer is an agent of the investors, and its rights, duties, and compensation are set out in a "Pooling and Servicing Agreement" (PSA). Typically, servicers are compensated by fees, which are annually about 20–50 basis points of the outstanding loan balance. Moreover, they are reimbursed for costs incurred during the foreclosure process but typically are not reimbursed for costs incurred during renegotiation of loans—benefiting only through the extension of servicing fees. In general, these renegotiation costs may be quite substantial and can easily cost as much as $1,000 per loan (see Barclays, 2008 Global Securitization Annual). Thus, to break even on a $100,000 mortgage loan can take anywhere between two and five years absent any re-default or prepayment. In other words, servicers may incur up front costs in exchange for uncertain fees when they renegotiate a loan. Foreclosure, by contrast, allows servicers an immediate, low-cost exit.

[6] For instance, some outstanding subprime and Alternative A-paper (Alt-A) mortgages have explicit restrictions that forbid servicers to alter the loan contract terms. Even when there are no explicit restrictions, the servicer is required to follow some vaguely specified instructions when deciding to renegotiate a mortgage (e.g., "best interest of certificate holders"). See, for example, Credit Suisse Fixed Income Research, The Day After Tomorrow: Payment Shock and Loan Modifications, April 5, 2007.

[7] Gilson, John, and Lang (1990), Asquith, Gertner, and Scharfstein (1994), Franks and Tourus (1994), Bolton and Scharfstein (1996), and Zingales (2008) are some related papers that highlight coordination problems brought about my dispersion of financial claims.

[8] For example, even if the bank does not service its own loans it might renegotiate the contract with the outside servicers in order to change their incentives. Alternatively, a bank can freely sell the delinquent loans in its portfolio to entities that might specialize in servicing of distressed mortgages. Such a change of servicing contract or transfer of loans to other servicers might be much harder to implement in the case of securitized loans due to coordination problems among dispersed owners of a mortgage pool.

[9] Alternatively, it might easier for policymakers to exert political pressure aimed at reducing foreclosure on banks; servicers of securitized loans whose behavior is bound by contractual arrangements with a large group of dispersed investors might be less prone to such pressure. Securitized loans might therefore be foreclosed at a higher rate due to lack of such considerations.

characteristics and regional dummies. We estimate the regressions separately for each quarter to alleviate concerns about changing macroeconomic conditions at the time of origination. Conditional on a loan becoming seriously delinquent, we find that the foreclosure rate of bank-held loan is lower as compared to securitized loans by around 3% to 7% in absolute terms (13% to 32% in relative terms).

Of course, besides observable differences, securitized and bank-held loans may also differ in unobservable characteristics. It is worth noting that our focus on the sample of delinquent loans and numerous controls should alleviate this concern to some degree. To further assuage these concerns, we restrict our analysis to a sample of loans that are better quality on the dimension of hard information characteristics such as credit score and documentation level. The reason to focus on these loans is that several studies provide evidence that potential screening on unobservables is less important for these types of loans (Keys, Mukherjee, Seru, and Vig, 2009; Keys, Mukherjee, Seru, and Vig, 2010; Rajan, Seru, and Vig, 2008). Our results suggest that the foreclosure bias due to securitization is larger among better quality loans with bank-held loans of better initial credit quality foreclosed, on average, at a rate 8.5% lower in absolute terms (34% in relative terms). More generally, we find substantial heterogeneity in the foreclosure differences across the cross-section of credit distribution: the effects are larger among borrowers of better credit quality, as measured by initial creditworthiness of the borrowers, and very small among borrowers of lower creditworthiness. In Section 7, we discuss that this pattern could be consistent with some economic arguments that suggest that renegotiation should be undertaken more intensively for borrowers of high credit quality.

To address the concern of selection more seriously, the main identification strategy of the paper relies on a quasi-experiment. The empirical design exploits a particular institutional feature of this market where the originators are obligated to purchase back securitized loans that become seriously delinquent or violate representations and warranties in a post-sale audit, typically within 90 days of being securitized. We use this feature to construct two groups: securitized loans that become delinquent just before 90 days and are taken back by the originator form the *treatment* group since the originators have the right to service these loans like any other loan on their balance sheet; and securitized loans that become delinquent just after 90 days form the *control* group since these loans continue to be serviced as securitized loans.

Since both types of loans are securitized to start with, this test allows us to circumvent any ex ante selection on unobservable concerns by providing us with a plausibly exogenous variation in the securitization status of a delinquent loan. By comparing the foreclosure rates of loans in the *treatment* and *control* groups, we examine whether securitization causally induces a bias in the foreclosure decision of servicers. Despite the obvious selection that delinquent loans sent back by securitization trusts to lenders (treatment loans) may be of worse quality, we find that these loans foreclose at a rate that is 6.5% *lower* in absolute terms (17.8% in relative terms) as compared to delinquent securitized loans that remain securitized. We conduct several tests to rule out alternative explanations that might drive these results, but in each of the tests our results remain.

Our analysis is agnostic about what tools servicers might be using to achieve the differences in foreclosure rates between bank-held and securitized loans. In principle, there are a variety of tools servicers may use with respect to troubled mortgages in order to prevent foreclosure. These tools include, among others: repayment plans, forbearance plans, short-sales, foreclosure moratoria, refinancing borrowers into more affordable loans, explicit modification of contractual terms (like principal reduction, term extension, or adjustment of the mortgage rate), a transfer of a loan to a specialized servicer who among other things might engage in a workout with the borrower and a wait-and-see approach.[10] Each of these actions could result in foreclosure differences between bank-held and securitized loans with the difference coming from different tools employed in servicing of bank loans relative to servicing of securitized loans, or similar tools being used in servicing with different efficiency. Since these actions affect the nature of the relationship between borrower, lender, and the servicer, we broadly interpret them as representing contract renegotiation.

Our estimate of foreclosure bias in servicing of securitized loans is measured relative to foreclosures by banks. As a result, any explanation must vary with the securitization status of the loans, since omitted variables that affect both the securitized and portfolio loans in a similar manner get differenced out. For instance, factors like servicer capacity constraint to handle renegotiations that do not vary with securitization status of a loan are not likely to explain our results. As discussed above, securitization can affect the servicers' decision to foreclose, through several channels such as potential agency conflicts, legal constraints, coordination issues among multiple investors, and different accounting treatment of losses. While each of these channels is consistent with securitization affecting the foreclosure rate on seriously delinquent loans, our empirical analysis is largely agnostic about the exact channel through which this effect takes place. In Section 7 we discuss results that shed some light on this issue.

Finally, our analysis is largely agnostic about efficiency. Answering this question is difficult since it requires knowledge of expected recovery for foreclosed loans as well as expected repayment in case of renegotiation at the time of the servicing decision. We discuss some of our results that speak to the efficiency issue in Section 7.

Our paper contributes to the research studying loan servicer incentives. The paper closest to ours is Adelino, Gerardi, and Willen (2009) which uses the same data set

[10] Our interviews with industry experts suggest that the issue of transferring a loan to a specialized servicer may be especially relevant for repurchased loans (such as the treatment group in our quasi-experiment) that might later be re-securitized in the private market following a successful workout.

and attempts to identify the use of loan modifications by servicers and investigates renegotiation in general by examining the cure rates of delinquent loans. They interpret their findings as showing that renegotiation was infrequent, independent of the securitization status of the loan. Their conclusion is at odds with independent studies from government agencies (Office of the Comptroller of the Currency (OCC) and Office of Thrift Supervision (OTS)) that show that renegotiation comes in many forms besides loan modification and varies in both frequency and efficiency with the securitization status as is predicted by our study. In Section 7, we discuss some limitations of Adelino, Gerardi, and Willen (2009) and provide alternative interpretation of their findings.

Our paper is also related to recent literature on servicer incentives (Pennington-Cross and Ho, 2006; Cordell, Dynan, Lehnert, Liang, and Mauskopf, 2009; Gan and Mayer, 2006). More broadly, we contribute to the literature that debates the costs and benefits of securitization (Dell'Ariccia, Igan, and Laeven, 2008; Demyanyk and Van Hemert, 2009; Keys, Mukherjee, Seru, and Vig, 2010; Keys, Mukherjee, Seru, and Vig, 2009; Rajan, Seru, and Vig, 2008; Loutskina, 2006; Loutskina and Strahan, 2007; Mian and Sufi, 2009; Morrison, 2005; Parlour and Plantin, 2008). Finally, the paper is also related to the literature that empirically examines renegotiation (see Benmelech and Bergman, 2008; Roberts and Sufi, 2009 in the context of corporate default, and Matvos, 2009 for renegotiation in NFL football contracts).

The rest of the paper is organized as follows. Data and sample construction are described in Section 2. We discuss our empirical methodology in Section 3. Our main empirical findings are presented in Sections 4 and 5. Section 6 discusses a quasi-experiment that helps confirm our findings. Section 7 concludes.

2. Data

The data for this study come from Lender Processing Services (LPS) (formerly called McDash Analytics) and include loan-level data reported by mortgage servicing firms. The data set has detailed information on the loan at the time of origination, such as the loan amount, term, loan-to-value ratio (LTV), credit score, and interest rate type—data elements that are typically disclosed and form the basis of contracts for both securitized and portfolio loans. We now describe some of these variables in more detail. The borrower's credit quality is captured by a summary measure called the FICO score. The FICO score has increasingly become the most recognizable measure used by lenders, rating agencies, and investors to assess borrower quality (Gramlich, 2007). The software used to generate the score from individual credit reports is licensed by the Fair Isaac Corporation (FICO) to the three major credit repositories—TransUnion, Experian, and Equifax. These repositories, in turn, sell FICO scores and credit reports to lenders and consumers.

FICO scores provide a ranking of potential borrowers by the probability of having some negative credit event in the next two years. Probabilities are rescaled into a range of 400 to 900, though nearly all scores are between 550 and 800, with a higher score implying a lower probability of a negative event. The negative credit events foreshadowed by the FICO score can be as small as one missed payment or as large as bankruptcy. Borrowers with lower scores are proportionally more likely to have all types of negative credit events than are borrowers with higher scores.

Borrower quality can also be gauged by the level of documentation collected by the lender when taking the loan. The documents collected provide historical and current information about the income and assets of the borrower. Documentation in the market (and reported in the database) is categorized as full, limited, or no documentation. Borrowers with full documentation provide verification of income as well as assets. Borrowers with limited documentation provide no information about their income but do provide some information about their assets. No-documentation borrowers provide no information about income or assets.

The data also provide information on the features of the loan contracts. Specifically, we have information on the type of mortgage loan (fixed rate, adjustable rate, balloon, or hybrid), and the LTV of the loan, which measures the amount of the loan expressed as a percentage of the value of the home. To better account for regional conditions, such as local house price variation, we focus only on loans originated in the Metropolitan Statistical Areas (MSAs) for which we have such information. Information about the geography where the loan is located (MSA) is also available in the database. Finally, LPS provides information on whether the loan is securitized to private investors or is a bank-held (or portfolio) loan.

We restrict our sample to first-lien non-agency mortgages originated in 2005 and 2006. We focus on loans originated in or after 2005 since the LPS coverage prior to this year is less representative. We track the payment status of loans till the end of first quarter of 2008 since the behavior of participants in the market may have changed after several government interventions subsequent to this time period (e.g., Bear Sterns bailout or the Obama Administration's Making Home Affordable Program). Accordingly, we only consider loans that are originated till the end of 2006 in order to have sufficient data to evaluate subsequent loan performance.

We drop loans that have incomplete information about original credit scores, original interest rates, origination amounts, and property values. We focus on loans with maturities of 15, 20, and 30 years since this constitutes most of the sample. To avoid survivorship bias, we limit our sample to those loans that entered the LPS database within four months of the origination date. To exclude outliers and possible data errors, we only consider loans with FICO scores between 500 and 850 and LTV less than 150. In addition, we also exclude loans in Alaska, Hawaii, and other non-continental areas.

After filtering out the data set as described above, there were approximately 6.2 million unique mortgages. From here, we split the data into quarters, yielding between 650,000 and one million loans in each quarter. Of these loans, roughly 75% were portfolio loans at the time of

origination, i.e., the loans that were not securitized. Because it takes some time to securitize loans, however, most of the loans initially recorded as portfolio are securitized within a few months.[11]

For our regressions, we consider a subsample of the loans defined above that become 60+ days delinquent as reported by the servicers. In the paper, we use the Mortgage Bankers Association's (MBA) (2008) definition of 60+ days delinquency, though all our results hold if we used the Office of Thrift Supervision's definition of 60+ delinquency instead.[12] A loan is 60+ days delinquent if the borrower is behind by two mortgage payments. The missed payments do not necessarily have to be consecutive. There were about 327,000 delinquent loans in our entire sample. For these loans we record their ownership status, that is whether they are securitized or bank-held (portfolio), at the first time of their 60+ days delinquency. About 11.3% of these loans were bank-held as of the time of delinquency. A loan is considered foreclosed when it enters foreclosure post-sale or REO (real estate owned) status during the course of the loan's payment history.[13]

In our analysis we also consider a subsample of higher-quality loans with full documentation and a FICO credit score of at least 680. Using this classification, there were about 1.3 million such loans in the subsample (125,000 to 200,000 per quarter). This sample contains approximately 16,500 delinquent loans, of which 20.4% were portfolio-held at the time of delinquency.

3. Empirical methodology

Servicers of mortgages make the crucial decision whether to foreclose a delinquent mortgage. In our empirical analysis we want to estimate the impact securitization has on this servicing decision. The most simple approach to doing this would be to use the following specification:

$$Pr(Y_i = 1|Delinquency) = \Phi(\alpha + \beta \times Portfolio_i + \gamma.X_i + \delta_m + \varepsilon_i), \quad (1)$$

where the dependent variable is an indicator variable for a *delinquent* loan i that takes a value of 1 if the loan is foreclosed, and 0 otherwise. Conditioning on delinquency of a loan seems natural given that we are interested in a servicer's decision to renegotiate or foreclose a distressed loan. X_i is a vector of loan and borrower characteristics that includes variables such as FICO scores, interest rate, loan-to-value ratio (LTV), and origination amount, and γ a vector of coefficients. *Portfolio* is a dummy variable that takes the value 1 if the delinquent loan was held on the lender's balance sheet, and 0 if the loan was securitized. In this specification, β would measure the impact of securitization on a servicer's decision to foreclose the delinquent property or engage in a workout.

The causal interpretation of these results would rely on the assumption that, conditional on observables, there is a random assignment of portfolio and securitized loans at the time of delinquency. Following this, we ensure that the empirical specification conditions on a plethora of explanatory variables that might be important. In particular, besides the observables listed above, we also use the term length, whether the loan was fixed term, whether it was insured, and the age of the loan at the time of delinquency. To account for regional factors we include MSA fixed effects (δ_m). Moreover, we make the specification (1) very flexible by including squares of LTV ratio and loan amount as well as dummies of different FICO ranges.

It is nevertheless possible that after conditioning on a host of observables, the assumption of random assignment may be violated, making the estimate β biased. In particular, if lenders collect unobservable private information about borrower quality at the time of origination and securitize loans of worse quality, β would be biased, i.e., securitized loans would foreclose at a higher rate. Notably, restricting the analysis to the sample of delinquent loans alleviates this concern to some degree. Specifically, if lenders obtain signals about the likelihood of delinquency only during the origination process (i.e., the signals are of short-term prospects), differences in foreclosure rates of delinquent securitized and portfolio loans cannot be attributed to selection on unobservables at the time of origination. It is conceivable, however, that lenders might also obtain long-term signals when they screen the borrower. We circumvent this issue by restricting our analysis to borrowers for whom such information is likely to be less valuable at the time of loan origination, i.e., borrowers that are better quality on the dimension of hard information characteristics, such as credit score and documentation level. The reason to focus on these loans is that studies show that screening on unobservables is less important for these types of loans (Keys, Mukherjee, Seru, and Vig, 2010) Table A1.[14]

It is also plausible to conjecture that lenders obtain additional information about the borrower and the property between origination and delinquency. The differences in foreclosure rates between delinquent securitized and portfolio loans might simply reflect worse

[11] For example, if we examine ownership status of loans six months after origination in our sample, we find that only 20% of loans are still held as portfolio loans.

[12] Under the MBA definition, a loan increases its delinquency status if a monthly payment is not received by the end of the day immediately preceding the loan's next due date. Under the OTS definition, a loan increases its delinquency status if a monthly payment is not received by the loan's due date in the following month.

[13] Since loans frequently transition from portfolio to securitized, one might worry that our definition of bank-held loans might generate a bias in how we measure our foreclosure results (if banks bought some delinquent securitized loans after delinquency). Note that if banks were to subsequently purchase some of the delinquent securitized loans, these loans would still be treated as securitized loans in our analysis. Similarly if banks securitize some of their delinquent loans these loans would still be treated as bank-held by us. Note that this makes it harder for us to show that bank-held loans are serviced differently relative to securitized loans, since some of the loans that are serviced as bank-held are treated as securitized and the loans that were securitized subsequent to their delinquency are treated as bank-held.

[14] One could wonder why these high-quality borrowers (FICO > 680 and full documentation loans) ended up in the non-agency market. In Table A1 we discuss in more detail that this is not likely to be a concern for our findings (also discussed in Section 5.1).

Table 1
Summary statistics of all loans.

The sample only includes first lien loans. The investor is either private (securitized) or portfolio (bank balance sheet) at the time of the first observed month of 60+ days delinquency. Delinquent is defined as 60+ days MBA delinquent. Default is defined as a loan that enters into foreclosure post-sale or REO status. Age at delinquency is the number of months since origination when a loan becomes 60+ days delinquent. All loans in the sample are originated between 2005 and 2006.

Panel A: Delinquent loans

Origination quarter	2005 Q1	2005 Q2	2005 Q3	2005 Q4	2006 Q1	2006 Q2	2006 Q3	2006 Q4
% Portfolio	13.8%	12.5%	13.5%	10.7%	8.9%	8.6%	10.4%	11.7%
Original credit score	628.0	630.9	639.8	638.0	637.6	636.6	634.4	632.8
LTV	80.1	80.3	79.8	79.1	79.6	80.0	79.9	80.5
Original interest rate	7.02%	7.13%	7.13%	7.56%	8.08%	8.26%	8.45%	8.29%
Original loan amount	217,526	231,752	252,690	254,366	251,435	256,711	261,184	272,667
Age at delinquency	17.5	16.9	16.9	15.4	13.4	12.0	10.6	9.17
% Default/Foreclosure	24.19%	23.52%	22.73%	24.70%	26.27%	22.29%	19.93%	16.18%
N	35,585	46,521	46,907	45,133	42,978	42,354	37,386	30,574

Panel B: Delinquent loans by investor status

Portfolio	2005 Q1	2005 Q2	2005 Q3	2005 Q4	2006 Q1	2006 Q2	2006 Q3	2006 Q4
Original credit score	639.2	656.2	656.3	662.9	664.7	660.1	634.0	641.6
LTV	78.9	79.2	79.4	79.1	80.3	81.3	82.2	83.2
Original interest rate	6.16%	6.29%	6.50%	6.67%	6.97%	7.54%	7.97%	7.64%
Original loan amount	248,033	282,570	271,062	305,099	297,276	286,659	249,147	264,680
Age at delinquency	17.4	16.9	14.9	14.2	12.8	11.0	9.0	8.0
% Default/Foreclosure	19.22%	19.26%	18.80%	20.00%	22.63%	19.18%	16.01%	15.35%
N	4,921	5,837	6,313	4,811	3,822	3,654	3,892	3,570

Securitized	2005 Q1	2005 Q2	2005 Q3	2005 Q4	2006 Q1	2006 Q2	2006 Q3	2006 Q4
Original credit score	626.2	627.2	637.2	635.0	634.9	634.4	634.4	631.6
LTV	80.3	80.5	79.9	79.1	79.5	79.9	79.7	80.2
Original interest rate	7.15%	7.26%	7.23%	7.67%	8.19%	8.32%	8.50%	8.37%
Original loan amount	212,631	224,461	249,833	248,313	246,960	253,884	262,583	273,723
Age at delinquency	17.5	16.9	17.2	15.6	13.5	12.1	10.8	9.3
% Default/Foreclosure	24.99%	24.14%	23.35%	25.27%	26.62%	22.58%	20.38%	16.29%
N	30,664	40,684	40,594	40,322	39,156	38,700	33,494	27,004

information obtained for securitized loans. We alleviate this concern by conditioning on the credit score and loan-to-value ratio of the borrower *at the time* of delinquency for a subsample of loans with this information. Since it takes, on average, about one and one-half years for a borrower to become delinquent (see Table 1), we expect credit scores and loan-to-value ratios at the time of delinquency to capture some of the information regarding quality of the borrower that is revealed between origination and delinquency.[15] If the conjecture is true, this test should reduce the bias in β (i.e., reduce the magnitude of β).

Note that in our sample, some loans exit the database as their servicing rights are transferred to servicers outside LPS coverage (around 1.8% of the delinquent loans in our sample transfer within the first six months of delinquency, 2.2% transfer within 12 months, and 2.3% transfer within 24 months). As a result, payment history for these loans is not available subsequent to the time of their transfer.[16] This poses a challenge for the empirical analysis that relies on a logit specification since we do not have an outcome variable (i.e., whether these loans were foreclosed or not subsequent to the transfer date) for these loans. For the regressions that rely on this specification, we exclude these loans from our sample. Our analysis suggests, however, that these transfers are not random, and delinquent loans that are transferred are more likely to be bank-held loans. Consequently,

[15] Fair Isaac reports that credit scores get updated, on average, every three months.

[16] Conversations with the data vendor revealed that there are several loans where servicing transfers occur within the set of servicers who provide information to LPS. These loans do not pose any complications since their entire payment history is available.

excluding these loans altogether could potentially bias our results that use the logit specification.[17]

To assess the robustness of our results we employ two strategies. First, we examine how our results vary with inclusion of payment history that we observe until the transfer date of these loans. Second, we employ a hazard model that accounts for attrition of the transfer loans while giving us an estimate on the differences of outcome rates between securitized and portfolio loans. The benefit of this approach over the first strategy is that the hazard model also employs information on the payment history of transferred loans (till their transfer date). As we will discuss later in Section 4, our results on foreclosure bias are qualitatively unaffected regardless of the empirical strategy we employ.

Finally, while the tests discussed above might alleviate concerns about selection, they might not be able to fully account for unobservables. As a result, our estimates could be biased. To address this concern, we rely on a quasi-experiment that is discussed in more detail in Section 6.

4. Descriptive statistics and main tests

4.1. Descriptive statistics

We start the empirical analysis by providing summary statistics of some of the key variables used in our analysis in Tables 1 and 2. We use all the delinquent loans in Table 1, while only delinquent loans that were of high-quality at the time of origination (fully documented loans with FICO >680) are considered in Table 2. As can be observed from Panel A of Tables 1 and 2, there seem to be differences in the proportion of loans that are securitized depending on the riskiness of the loans. About 11.2% in the sample of all delinquent loans are held on portfolio, compared with 20.3% of the fully documented loans (averaged over the sample period). In addition to higher FICO scores, the fully documented loans had slightly lower LTV ratios and larger origination amounts on average. In both samples, the origination amounts increase from 2005 Q1 through 2006 Q4. In most quarters, the sample of all loans foreclose more often than the sample of fully documented loans. Foreclosures mechanically fall as we get closer to the end of the sample (2006 Q3 and 2006 Q4 origination vintages) since a long history for these loans is not available.[18]

Panels B of Tables 1 and 2 split the respective samples by securitization status at the time of delinquency. The panels show that portfolio loans have higher FICO scores and lower interest rates than securitized loans. On the other hand, portfolio loans usually have slightly higher LTV ratios and origination amounts. In both the sample of all loans and the subsample of fully documented loans, loans held on portfolio foreclose less often than securitized loans. However, portfolio loans take less time to become delinquent than do securitized loans. Since these are univariate statistics, we next turn to multivariate regressions to assess what differences exist in foreclosure rates between portfolio and securitized loans after we condition for observables of the loan.

4.2. Comparing foreclosure rates of securitized and portfolio loans

We now describe the results from our first test. We estimate Eq. (1) and report the marginal effects of a logit regression performed for the entire sample in Table 3. The dependent variable is whether or not the loan is foreclosed conditional on the loan becoming delinquent. We estimate the regressions separately for each quarter of origination to alleviate concerns that macroeconomic conditions might have changed substantially during our sample period. MSA fixed effects are included in all the specifications to account for regional variation across the country.

As can be seen in columns 1–8, the coefficient on (*Portfolio*) dummy is consistently negative and significant for all quarters. This suggests that, conditional on being delinquent, a loan on a lender's balance sheet is less likely to be foreclosed than a loan that is securitized. The effects are large: keeping all the variables at their mean values, being on portfolio reduces the likelihood of foreclosure for a delinquent loan in absolute terms by around 3.8% to 7% (between 18% and 32% relative to the mean foreclosure rate of securitized loans; see also Fig. 1 for a modified test).

The coefficients on most other variables are also as expected. For instance, loans with higher LTV ratios are more likely to foreclose. Interestingly, the coefficient on FICO suggests that, conditional on being delinquent, loans with lower FICO default less. This is in contrast to the negative relationship one typically observes between FICO and delinquencies.[19] One interpretation of this finding is that if a high FICO loan becomes seriously delinquent, it is most likely that the borrower has received a larger credit shock, given initial credit quality. As a result, conditional on delinquency, a higher credit score may be proxying for the size of the credit shock in these regressions.[20]

[17] Also note that our conversations with the data vendor (LPS) suggested that delinquent loans that are transferred out of LPS are not typically done for the purpose of foreclosing them. Consequently, not observing payment history on a sample of delinquent loans that are largely bank-held loans and are transferred for reasons other than foreclosing may make it harder for us to demonstrate that bank-held loans are foreclosed less intensively.

[18] Note that our data run until the end of 2008 Q1, and as a result, loans in 2006 Q3 and 2006 Q4 are tracked for less than two years.

[19] In unreported tests, we confirm that there is a strong negative relationship between FICO and the likelihood of a loan becoming delinquent.

[20] We also conduct a test using information on the credit score at the time of delinquency and with updated loan-to-value values in addition to other controls in specification (1). The results are presented in Fig. 1, where we report β, the coefficient on *Portfolio* and its 95% confidence interval. β is negative and significant for all the quarters in our sample.

Table 2
Summary statistics of high-quality loans (full documentation and FICO of at least 680).

The sample only includes first lien loans. The investor is either private (securitized) or portfolio (bank balance sheet) at the time of the first observed month of 60+ days delinquency. Delinquent is defined as 60+ days MBA delinquent. Default is defined as a loan that enters into foreclosure post-sale or REO status. Age at delinquency is the number of months since origination when a loan becomes 60+ days delinquent. All loans in the sample are originated between 2005 and 2006.

Panel A: Delinquent loans

Origination quarter	2005 Q1	2005 Q2	2005 Q3	2005 Q4	2006 Q1	2006 Q2	2006 Q3	2006 Q4
% Portfolio	17.6%	20.8%	21.5%	19.3%	20.7%	21.2%	17.8%	24.2%
Original credit score	716.5	718.3	718.8	718.5	717.6	716.2	715.6	717.8
LTV	79.9	80.2	79.6	78.7	78.4	79.1	79.0	79.4
Original interest rate	6.09%	6.29%	6.12%	6.53%	6.86%	7.16%	7.31%	7.19%
Original loan amount	250,483	256,730	280,300	276,557	276,597	297,623	311,906	320,919
Age at delinquency	21.2	20.0	19.4	17.8	15.8	13.5	11.7	9.8
% Default/Foreclosure	25.45%	25.08%	20.02%	21.01%	23.48%	20.44%	16.95%	13.67%
N	2,008	2,911	2,452	2,228	1,793	2,099	1,793	1,207

Panel B: Delinquent loans by investor status

Portfolio	2005 Q1	2005 Q2	2005 Q3	2005 Q4	2006 Q1	2006 Q2	2006 Q3	2006 Q4
Original credit score	723.2	726.0	727.6	728.2	721.9	722.4	719.9	722.0
LTV	80.5	80.5	80.5	79.4	78.5	80.1	81.8	79.5
Original interest rate	5.13%	5.66%	5.44%	6.18%	6.54%	6.76%	6.89%	6.65%
Original loan amount	257,893	266,009	292,939	290,574	273,631	294,194	305,043	342,780
Age at delinquency	21.1	20.6	18.5	15.9	14.7	12.3	10.7	9.4
% Default/Foreclosure	19.26%	19.64%	14.61%	14.22%	18.28%	13.03%	8.44%	10.96%
N	353	606	527	429	372	445	320	292

Securitized	2005 Q1	2005 Q2	2005 Q3	2005 Q4	2006 Q1	2006 Q2	2006 Q3	2006 Q4
Original credit score	715.1	716.3	716.4	716.2	716.4	714.6	714.7	716.5
LTV	79.8	80.2	79.4	78.5	78.4	78.9	78.3	79.4
Original interest rate	6.29%	6.46%	6.31%	6.62%	6.94%	7.27%	7.40%	7.36%
Original loan amount	248,903	254,290	276,839	273,214	277,374	298,546	313,397	313,942
Age at delinquency	21.2	19.8	19.6	18.3	16.2	13.8	11.9	10.0
% Default/Foreclosure	26.77%	26.51%	21.51%	22.62%	24.84%	22.43%	18.81%	14.54%
N	1,655	2,305	1,925	1,799	1,421	1,654	1,473	915

4.3. High-quality loans

As discussed in Section 3, though we have controlled for all the relevant observable characteristics of the loans, differences in foreclosure rates between securitized and portfolio loans could be driven by some unobservable information about quality that lenders obtain at the time of origination. While focusing on the sample of financially distressed loans, under some assumptions, could alleviate some of these concerns, we now examine a subset of the data where we believe this would be less of a problem. We focus on a subsample of loans of higher quality: loans that are fully documented and also have good initial credit quality as represented by a FICO credit score of at least 680 (more than half of the fully documented loans have FICO greater than 680). We do so since any selection on unobservables at the time of origination is likely to be of less concern for these types of loans (Keys, Mukherjee, Seru, and Vig, 2010).

We present the estimates using the specification (1) for this subsample in Table 4. As can be observed from columns 1 to 8, the coefficient on the portfolio dummy is negative and significant for all but one quarter. In other words, conditional on being delinquent, loans that are of higher quality at the time of origination foreclose at a rate that depends on the securitization status of the loan. The estimates are, once again, economically meaningful. For example, in 2006 Q4, being on portfolio decreases the probability of foreclosure in absolute terms by about 4.5%, nearly a 31% decrease relative to the mean foreclosure rate of 14.5% among securitized loans. Similarly, in 2006 Q3, the probability of foreclosure for portfolio loans is lower by about 47% in relative terms. The estimates on other variables are qualitatively similar to those reported in Table 3.

We also find that, similar to the entire sample, the magnitude of our results is stronger in the periods of house price declines. For instance, the difference between foreclosure rates of portfolio and securitized loans is about 14.5% and 21.5% in relative terms in 2005 Q1 and 2005 Q2—a significantly smaller number when compared to 47% and 31% in 2006 Q3 and 2006 Q4. This evidence again

Table 3
Logit regression of default conditional on 60+ days delinquency (all loans).

This table reports the marginal effects of a logit regression. Coefficients on discrete variables represent the effect of moving from zero to one. Coefficients on continuous variables represent the effect of moving one standard deviation from the mean. Portfolio is a dummy which indicates that the loan was bank-held at the time of first 60+ days delinquency. Age at delinquency is the age of the loan at the time of first 60+ days delinquency. The excluded variables are private investor, FICO $>=$ 680, 30-year term, and missing insurance information. Standard errors are clustered at MSA level and resulting *t*-statistics are reported in parentheses. All loans in the sample are originated between 2005 and 2006. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Origination quarter	2005 Q1	2005 Q2	2005 Q3	2005 Q4	2006 Q1	2006 Q2	2006 Q3	2006 Q4
Dependent variable: Foreclosure								
Mean securitized	0.2499	0.2414	0.2335	0.2527	0.2662	0.2258	0.2038	0.1629
Portfolio (d)	−0.046***	−0.048***	−0.046***	−0.070***	−0.059***	−0.060***	−0.066***	−0.038***
	(−8.12)	(−8.86)	(−8.21)	(−10.91)	(−8.21)	(−12.99)	(−12.97)	(−14.25)
FICO < 620 (d)	−0.109***	−0.133***	−0.127***	−0.145***	−0.155***	−0.124***	−0.108***	−0.069***
	(−11.15)	(−18.42)	(−17.92)	(−23.61)	(−19.81)	(−15.24)	(−16.43)	(−12.51)
620 < = FICO < 680 (d)	−0.025***	−0.037***	−0.034***	−0.038***	−0.042***	−0.030***	−0.028***	−0.017***
	(−3.57)	(−8.01)	(−6.36)	(−8.01)	(−7.82)	(−6.41)	(−4.97)	(−4.37)
LTV	0.579***	0.280***	0.501***	0.535***	0.553***	0.401***	0.100***	0.055***
	(6.47)	(4.50)	(7.10)	(6.68)	(7.37)	(5.14)	(3.56)	(3.18)
LTV squared	−0.405***	−0.163***	−0.342***	−0.361***	−0.373***	−0.265***	−0.035	−0.015
	(−5.73)	(−3.24)	(−6.20)	(−5.53)	(−6.16)	(−4.17)	(−1.45)	(−1.04)
Origination amount	−0.003	0.000	−0.001	−0.003	0.001	0.007	0.003	0.009**
	(−0.47)	(0.08)	(−0.19)	(−0.84)	(0.09)	(1.08)	(0.62)	(2.21)
Origination amount squared	0.009	0.001	−0.002	−0.001	0.001	−0.011	−0.008	−0.016**
	(1.64)	(0.26)	(−0.28)	(−0.16)	(0.19)	(−1.52)	(−1.42)	(−2.12)
Original interest rate	0.015***	0.012***	0.020***	0.018***	0.021***	0.015***	0.013***	0.010***
	(6.71)	(5.40)	(9.89)	(8.89)	(8.44)	(9.01)	(9.04)	(8.13)
FIX (d)	−0.081***	−0.070***	−0.058***	−0.060***	−0.053***	−0.046***	−0.036***	−0.026***
	(−15.34)	(−12.52)	(−13.62)	(−13.93)	(−7.24)	(−10.27)	(−7.55)	(−6.97)
15-Year term (d)	0.013	−0.047**	−0.074***	−0.060***	−0.108***	−0.028	0.114***	0.072*
	(0.48)	(−2.21)	(−3.12)	(−2.69)	(−5.50)	(−1.06)	(3.59)	(1.94)
20-Year term (d)	0.022	−0.053	−0.073*	−0.074	−0.086	−0.104***	−0.046	−0.050***
	(0.35)	(−1.27)	(−1.88)	(−1.47)	(−1.32)	(−3.31)	(−0.87)	(−2.91)
No insurance (d)	−0.018***	−0.016***	−0.002	0.004	0.013**	0.024***	0.014**	−0.002
	(−3.53)	(−2.81)	(−0.37)	(0.64)	(2.32)	(4.42)	(2.23)	(−0.59)
Insurance (d)	−0.019	−0.011	−0.015	0.009	−0.005	−0.019	−0.013	−0.004
	(−1.55)	(−0.98)	(−1.40)	(0.64)	(−0.27)	(−1.06)	(−0.99)	(−0.38)
Age at delinquency	−0.085***	−0.096***	−0.109***	−0.135***	−0.163***	−0.136***	−0.127***	−0.097***
	(−13.51)	(−17.02)	(−26.89)	(−32.76)	(−44.74)	(−51.75)	(−60.27)	(−126.99)
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	35,365	46,279	46,636	44,904	42,789	42,050	37,008	29,939

suggests that declining house prices eroded borrowers' ability to renegotiate their contract through refinancing, thereby aggravating the foreclosure bias.

Overall, the magnitude of our findings for the sample of higher quality is larger than what we obtained for the entire sample. Moreover, the entire increase in the foreclosure difference for the higher quality loans is driven by lower foreclosure rates of portfolio loans in this sample. In other words, if we move from low quality sample to a high quality sample, then there is statistically no difference in the foreclosure rates of securitized loans, but the loans held by the bank are foreclosed at a much higher rate (see Panel B of Tables 1 and 2). We discuss in Section 7 what possible economic reasons might drive these differences.

4.4. Other tests

4.4.1. Attrition due to loan transfers

We first assess the robustness of our results to inclusion of data on the loans whose servicing rights were transferred to servicers outside LPS coverage. We use a Cox-proportional hazard model to explicitly incorporate the payment history that is available for the loans before they are transferred. In the model we consider three states depending on whether the loan is: foreclosed, not foreclosed, or transferred to a servicer outside the LPS data. In Panel A of Table 5 we present the results using all the loans in our sample. Panel B does the same exercise using the high-quality loans. All the regressions are estimated using time and MSA fixed effects. The coefficients in the table present the hazard ratio of portfolio loans relative to securitized loans for the foreclosure and the transfer states.

As can be observed from Panel A, our results remain affected after accounting for this attrition. Specifically, even though delinquent bank-held loans are 4.76 times more likely to be transferred relative to comparable securitized loans, accounting for this attrition still shows that bank-held loans are about 24% less likely to be foreclosed (6% in absolute terms). In addition, consistent with our previous results, we find that this difference is accentuated among high-quality mortgages. As is evident from Panel B, the high-quality portfolio loans are about

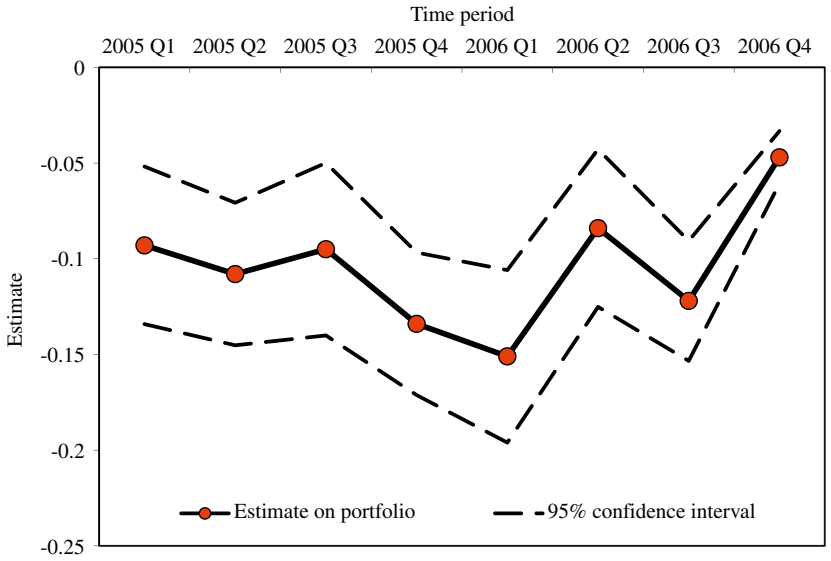

Fig. 1. Estimates on portfolio from logit regression using credit scores and LTV at time of delinquency. This figure reports the estimate (marginals) on the portfolio dummy using a specification similar to Table 3. We use FICO scores and LTV *at the time* of delinquency instead of using credit scores and LTV at the time of origination as in Table 3. Also plotted in the graph are the 95% confidence interval bands around the estimate.

34% less likely to be foreclosed relative to the comparable high-quality securitized mortgages.[21]

4.4.2. Other robustness

We now discuss some additional tests which confirm the robustness of our findings. Some of these tests are unreported for brevity and are available upon request. First, there might be concerns that some of the results might be sensitive to the particular definition of delinquency we have chosen. To alleviate this concern, we estimated our regressions using alternative MBA definitions of delinquency (30+ and 90+). Our results are qualitatively very similar.

Second, even though we controlled for regional dummies, there may be concerns that the house price index changed quite dramatically during the sample period, which might not be reflected in the MSA fixed effects. For instance, one might be worried that perhaps borrowers with securitized and portfolio loans belonged to very different neighborhoods and faced different house price changes over the sample period. To address this concern, we re-estimate the baseline regressions controlling for house price movements at the MSA level and for *zip-code level fixed effects*. In running this specification, note that we are unable to estimate zip-code regressions quarter by quarter since they do not have enough power to capture within zip-code variation. More specifically, the number of delinquent loans per quarter (there are about 9,300 delinquent loans, on average, per quarter) is small relative to parameters being estimated in a zip-code fixed regression (there are about 13,293 unique zip-codes, on average, spanned by our delinquent loans). We are able to exploit, however, within zip-code variation between bank-held and securitized loans using data from all the quarters in a pooled regression. As is reported in columns 1 and 2 of Table 6, we find that the results are similar to those obtained in the paper. In particular, we find that delinquent bank-held loans are more likely to be foreclosed by about 5.7% in absolute terms as compared to delinquent securitized loans (24.5% in relative terms) and that these effects are larger for higher-quality loans.

Third, as a robustness check, we also expanded the definition of foreclosure to include foreclosure starts in addition to foreclosure post-sale, and REO. Using this more liberal definition of foreclosure, we re-estimate our regressions and report these results in columns 3 and 4 of Table 6. As can be observed, we still find a negative and significant effect (at 1% level) on the portfolio estimate: the bank-held delinquent loans are foreclosed at the 10% lower rate in absolute terms (18% lower in relative terms) compared to similar securitized mortgages.

Fourth, we investigate whether omission of information on second liens in LPS might impact our results. Note that this bias may affect our estimates if delinquent securitized loans had more combined loan-to-value (CLTV) relative to comparable bank-held loans, since this would make delinquent securitized loans more risky and therefore more likely to foreclose. To address this concern, we re-estimated our results including a dummy that takes a value 1 if the loan has an LTV of 80% (LTV=80%).[22] The reason to do so stems from the notion that since most

[21] As mentioned earlier, we also employ an alternative empirical strategy to evaluate the robustness of our findings by inclusion of payment history that we observe until the transfer date of loans that are transferred out of our sample. More precisely, we re-estimate the logit regressions of the form used in Table 3 that allow comparison between the rate at which delinquent bank-held loans resume making payments as compared to securitized loans at different horizons. At each different horizon we include all the transfer loans that leave the database after a given horizon so that their payment history is available. Our results are qualitatively similar to those reported in this section.

[22] We thank the referee for suggesting this test.

Table 4
Logit regression of default conditional on 60+ days delinquency (high-quality loans).

This table reports the marginal effects of a logit regression. Coefficients on discrete variables represent the effect of moving from zero to one. Coefficients on continuous variables represent the effect of moving one standard deviation from the mean. Portfolio is a dummy which indicates that the loan was bank-held at the time of first 60+ days delinquency. Age at delinquency is the age of the loan at the time of first 60+ delinquency. The excluded variables are private investor, FICO $>=760$, 30-year term, and missing insurance information. Standard errors are clustered at MSA level and resulting t-statistics are reported in parentheses. All loans in the sample are originated between 2005 and 2006. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Origination quarter	2005 Q1	2005 Q2	2005 Q3	2005 Q4	2006 Q1	2006 Q2	2006 Q3	2006 Q4
Dependent variable: Foreclosure								
Mean securitized	0.2677	0.2651	0.2151	0.2262	0.2484	0.2243	0.1881	0.1454
Portfolio (d)	−0.039	−0.057***	−0.041**	−0.066***	−0.079***	−0.096***	−0.089***	−0.045***
	(−1.29)	(−3.04)	(−1.98)	(−3.85)	(−2.88)	(−4.81)	(−7.20)	(−3.25)
680 < = FICO < 720 (d)	−0.028	0.002	0.050*	0.027	−0.023	0.037	−0.028	0.010
	(−0.87)	(0.06)	(1.89)	(1.20)	(−0.59)	(1.27)	(−1.15)	(0.35)
720 < = FICO < 760 (d)	−0.005	0.024	0.113**	0.026	−0.013	0.046	−0.028	0.010
	(−0.15)	(0.76)	(2.49)	(1.05)	(−0.32)	(1.25)	(−1.25)	(0.33)
LTV	0.529**	0.007	0.448***	0.213**	0.236**	0.351**	0.112*	−0.045
	(2.37)	(0.10)	(4.71)	(2.07)	(2.04)	(2.00)	(1.85)	(−1.48)
LTV squared	−0.439**	0.034	−0.355***	−0.143	−0.157	−0.281*	−0.067	0.058**
	(−2.19)	(0.55)	(−4.14)	(−1.56)	(−1.48)	(−1.74)	(−1.07)	(1.98)
Origination amount	−0.055	0.008	0.003	0.057	0.006	0.025	0.009	0.059**
	(−1.48)	(0.65)	(0.18)	(1.51)	(0.33)	(1.18)	(0.54)	(2.14)
Origination amount squared	0.164***	−0.012	0.001	−0.162*	0.002	−0.025	−0.010	−0.070*
	(2.63)	(−1.48)	(0.09)	(−1.73)	(0.10)	(−1.23)	(−0.64)	(−1.80)
Original interest rate	0.022	0.036***	0.026***	0.034***	0.007	0.040***	0.015*	0.015**
	(1.45)	(3.53)	(2.57)	(3.53)	(0.74)	(3.58)	(1.96)	(2.37)
FIX (d)	−0.085***	−0.108***	−0.049**	−0.053***	−0.078***	−0.006	−0.036**	0.001
	(−3.71)	(−6.34)	(−2.35)	(−2.83)	(−3.15)	(−0.38)	(−2.32)	(0.08)
15-Year term (d)	−0.040	−0.145***	−0.018	−0.103**	−0.067	−0.086	0.489**	0.040
	(−0.28)	(−3.03)	(−0.16)	(−2.31)	(−0.55)	(−1.23)	(2.12)	(0.43)
20-Year term (d)		0.041	−0.074	−0.032				
		(0.32)	(−0.85)	(−0.31)				
No insurance (d)	−0.014	−0.055**	−0.022	−0.041**	−0.006	0.011	−0.001	0.000
	(−0.58)	(−2.30)	(−1.22)	(−2.27)	(−0.25)	(0.39)	(−0.09)	(−0.01)
Insurance (d)	−0.040	−0.017	−0.024	−0.072***	0.091	−0.003	−0.028	−0.048**
	(−0.62)	(−0.45)	(−0.59)	(−2.89)	(0.93)	(−0.06)	(−0.62)	(−2.24)
Age at delinquency	−0.100***	−0.104***	−0.109***	−0.121***	−0.183***	−0.139***	−0.130***	−0.089***
	(−6.40)	(−9.09)	(−18.96)	(−16.17)	(−23.29)	(−23.59)	(−35.71)	(−30.00)
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,758	2,631	2,123	1,978	1,555	1,826	1,518	905

loans in the subprime market had a combined LTV in excess of 80%, it is plausible that a loan with an LTV of 80% on its first lien is likely to have other liens. In other words, such loans are more likely to have a combined LTV which is not reported. If so, this dummy variable should capture some effects due to omission of combined LTV in our foreclosure regressions. Correspondingly, including this dummy variable should reduce the magnitude and significance of the *Portfolio* dummy in the foreclosure regression. As shown in columns 5 and 6 of Table 6, our results are virtually unchanged when we include the LTV=80% dummy, suggesting that omission of second liens might not be biasing our estimate.

Next, we also re-estimate our regressions defining high-quality loans using different FICO breakpoints (e.g., FICO of 700 instead of FICO of 680), using a more flexible specification (squares and cubes of all variables), and adding more explanatory variables. In each instance, we find results qualitatively similar to those reported in this paper.

Finally, we also estimated the logit and hazard specifications controlling for the quarter of delinquency of the mortgage. Our results from logit specification suggest that loans held on portfolio are 3% less likely to be foreclosed upon compared to securitized loans (13% in relative terms) after controlling for observable risk characteristics, MSA, quarter of origination and quarter of delinquency fixed effects. Correspondingly, results from hazard specification indicate that portfolio loans are foreclosed at the 5.7% lower rate in absolute terms (23% in relative terms). These results are reported in columns 7 to 10 in Table 6 (and in more detail in the internet appendix).

5. Additional evidence

5.1. Alternative measure[23]

So far we have focused our attention on analyzing the rate at which seriously delinquent loans are foreclosed depending on their securitization status. We now follow Adelino, Gerardi, and Willen (2009) and investigate the

[23] We thank an anonymous referee and the editor for suggesting this test.

Table 5
Hazard regression of default conditional on 60+ days delinquency (all loans and high-quality loans).

This table reports the estimated hazard ratios from a Cox-proportional hazard model of the transition from delinquency to foreclosure/transfer. Estimates on discrete variables represent the effect of moving from zero to one. Portfolio is a dummy which indicates that the loan was bank-held at the time of first 60+ days delinquency. All loans in the sample are originated between 2005 and 2006. Robust *t*-statistics are reported. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

	Panel A: All loans		*Panel B: High-quality loans*	
Dependent variable:	Foreclosure	Transfer	Foreclosure	Transfer
Mean securitized	0.25	0.02	0.25	0.01
Portfolio (d)	0.759***	5.76***	0.662***	4.797***
	(−21.34)	(79.86)	(−8.28)	(12.17)
FICO	1.006***	1.000***	1.000	1.006***
	(91.77)	(−2.80)	(0.82)	(3.62)
LTV	1.162***	1.011*	1.113***	0.973
	(18.23)	(1.79)	(3.37)	(−1.27)
LTV squared	0.999***	1.000	0.999***	1.000
	(−16.18)	(−1.15)	(−2.80)	(1.48)
Origination amount	1.000***	1.000	1.000	1.001*
	(7.19)	(−0.09)	(−0.15)	(1.70)
Origination amount squared	1.000***	1.000***	1.000	1.000*
	(−4.41)	(−3.46)	(0.18)	(−1.74)
Original interest rate	1.162***	1.196***	1.110***	1.040
	(50.75)	(23.44)	(7.62)	(1.12)
FIX (d)	0.68***	0.82***	0.627***	1.66***
	(−39.57)	(−7.79)	(−11.53)	(4.09)
15-Year term (d)	0.778***	0.837	0.576*	1.318
	(−4.17)	(−1.60)	(−1.87)	(0.66)
20-Year term (d)	0.507***	0.485***	0.788	0.000***
	(−4.48)	(−2.74)	(−0.58)	(−143.93)
No insurance (d)	0.999	1.89***	0.852***	0.614***
	(−0.16)	(28.49)	(−4.10)	(−3.48)
Insurance (d)	0.903***	1.292***	0.734***	0.534**
	(−5.01)	(5.25)	(−3.26)	(−2.20)
Time fixed effects	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes
N	316,772	316,772	15,203	15,203

payment behavior of the borrowers of seriously delinquent loans focusing on the rate at which borrowers of bank-held loans resume making payments relative to borrowers of comparable securitized loans (i.e., whether these loans are cured). To conduct our tests, we estimate the hazard specification similar to the one used in Section 4.4.1 to also account for the attrition of loans that are transferred out of the sample. In particular, the loan is assigned to one of three states: it is cured, not cured, or it is transferred. The loan is assigned a cured state if a 60+ delinquent loan's payment history becomes better than 60+ during the period we track the loans. For the purpose of this subsection, we track the loans until 12 months after their delinquency.

Before we analyze the cure rate, we note that this evidence should be interpreted cautiously with respect to our findings on foreclosure rate differences. In particular, no differences in the realized cure rates between delinquent portfolio and securitized loans does not necessarily indicate that these loans are serviced in a similar manner. The reason is that this measure treats both foreclosed loans and loans that have not yet cured in a certain time frame as "not cured". From the perspective of renegotiation, however, these are quite different. A loan that is not foreclosed has an option value that is associated with a potential future renegotiation. Alternatively, loans that have been foreclosed cannot be renegotiated in the future. To give a simple example of how this can confound inferences on servicing decisions related to renegotiation, suppose a lending institution decides not to foreclose a part of their delinquent portfolio due to the option to renegotiate them in the future. Also, suppose the servicers of securitized loans decide to foreclose similar loans, because they value the same option less, due to the presence of renegotiation frictions. In this example, there would be no large difference in the observed cure rates of portfolio and securitized loans in a given time horizon, while there is actually a substantial difference in servicing decisions due to renegotiation frictions imposed by securitization.[24]

Panel A of Table 7 presents the results for all the loans while Panel B displays the results for high-quality loans. The coefficients in the table present the hazard ratio of portfolio loans relative to securitized loans for cured and transfer states. As is evident from Panel A, 60+ delinquent borrowers with loans that are bank-held are more likely to resume

[24] In addition, it is not obvious how to define a loan as being cured since it is not a clearly defined state in the LPS data. One could define a loan as being cured differently depending on the number of payments that the borrower makes after delinquency (e.g., one, two, five, etc.), the order of these payments (e.g., consecutive, anytime after delinquency, etc.), and the time horizon over which to track the loan after delinquency (e.g., three months, six months, 12 months, etc.).

Table 6
Additional robustness tests.

This table reports the estimates (marginals) on Portfolio dummy using a specification similar to Table 3. The dependent variable is Foreclosure. The regression includes all the controls that were used in Table 3. Time and MSA (or zip-code) fixed effects are included in all specifications. High-quality loans consist of loans with FICO greater than or equal to 680 and full documentation. Coefficients on discrete variables represent the effect of moving from zero to one. All loans in the sample are originated between 2005 and 2006. More details on regressions in column (7) to (10) are provided in the internet appendix. Regressions in column (9) and (10) also include transferred loans. Standard errors are clustered at MSA level and resulting *t*-statistics are reported in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

	Zip-code Fixed Effects		Alternative foreclosure definition		LTV=80 Dummy		Quarter of Delinquency Fixed Effects			
							Logit regressions		*Hazard regressions*	
	(1) All loans	(2) High-quality	(3) All loans	(4) High-quality	(5) All loans	(6) High-quality	(7) All loans	(8) High-quality	(9) All loans	(10) High-quality
Mean securitized	0.23	0.23	0.56	0.54	0.23	0.23	0.23	0.23	0.25	0.25
Portfolio (d)	−0.057***	−0.063***	−0.101***	−0.124***	−0.051***	−0.057***	−0.030***	−0.053***	0.772***	.715***
	−24.73	−4.03	−19.69	−9.96	−15.04	−6.57	(−14.13)	(−14.03)	(−19.55)	(−6.40)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter of origination fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter of delinquency fixed effects	No	No	No	No	No	No	Yes	Yes	Yes	Yes
Clustering unit	MSA	MSA	MSA	MSA	MSA	MSA	MSA	MSA	MSA	MSA
Other fixed effects	Zip	Zip	MSA	MSA	MSA	MSA	MSA	MSA	MSA	MSA
N	327,438	16,491	327,372	16,272	327,401	16,106	327,438	13,822	316,772	15,203

making payments relative to borrowers of comparable securitized loans. In the entire sample of loans, the 60+ delinquent bank-held loans are cured at a rate 6% higher in absolute terms relative to comparable securitized loans within a year after delinquency (12.9% higher in relative terms).[25] From Panel B it is also clear that the differences in cure rate between bank-held loans and securitized loans are higher for better quality loans: these 60+ delinquent bank-held loans resume making payments a year after delinquency at a rate 8.2% higher in absolute terms relative to comparable securitized loans (20.5% higher in relative terms).

To get a better sense of magnitudes, we examine the rate at which delinquent loans resume making payments relative to the rate at which loans foreclose. As we know from Table 5, in a similar specification we found that bank-held delinquent loans foreclose at a rate 6% lower in absolute terms relative to comparable securitized loans. Correspondingly, the rate at which delinquent bank-held loans resume making payments relative to comparable securitized loans is higher in absolute terms by 6% as well.[26] Taken together, these results show that bank-held delinquent loans are not only foreclosed at a lower rate compared to securitized loans but also that bank-held delinquent loans resume making payments at a comparably higher rate. These differences are accentuated among better-quality loans.[27]

5.2. Cross-sectional evidence

We now examine how our results vary with the initial creditworthiness of the borrower. To do this we estimate the difference in cure and foreclosure rates between bank-held and securitized loans from the hazard specification of the form used in Section 4 for different subsamples and present the results in Table 8. More concretely, we divide the loans based on the initial creditworthiness of the borrower into three groups: lowest credit quality (with FICO credit score less than 620), medium credit quality (with FICO credit score between 620 and 680), and highest credit quality (with FICO credit score greater than 680).

Two facts emerge from the results: (a) cure rate differences between bank-held and securitized loans (with higher cure rates for bank-held loans) and foreclosure rates

[25] The number in relative terms matches well with Adelino, Gerardi, and Willen (2009) who report an 8.5% higher cure rate in relative terms for portfolio loans when they estimate a logit specification in their entire sample.

[26] Similarly, in the better-quality sample, bank-held delinquent loans foreclose at a rate 8.5% lower in absolute terms relative to comparable securitized loans. Correspondingly, in this sample the rate at which delinquent bank-held loans resume making payments is higher in absolute terms by 8.2% during a year after delinquency relative to comparable securitized loans.

[27] One could wonder why these high-quality borrowers (FICO > 680 and full documentation loans) ended up in the non-agency market. A potential concern could be that these borrowers are risky along some unobservable dimensions, which prevents them from qualifying for Government Sponsored Enterprise (GSE) loans. If lending institutions employ this information in deciding which loans to securitize vs. retain on the portfolio, it could partly explain the much lower foreclosure rates among the sample of high-quality portfolio loans. We address this issue in Table A1 and argue that this concern is unlikely driving all the results of the higher-quality sample.

Table 7
Hazard regression of cure rate conditional on 60+ days delinquency (all loans and high-quality loans).

This table reports the estimated hazard ratios from a Cox-proportional hazard model of the transition from delinquency to cure/transfer. A mortgage is considered cured if it makes another payment following delinquency. Estimates on discrete variables represent the effect of moving from zero to one. Portfolio is a dummy which indicates that the loan was bank-held at the time of first 60+ days delinquency. All loans in the sample are originated between 2005 and 2006. Robust *t*-statistics are reported. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

	Panel A: All loans		*Panel B: High-quality loans*	
Dependent variable:	Cure	Transfer	Cure	Transfer
Mean securitized	0.47	0.02	0.40	0.01
Portfolio (d)	1.129***	6.226***	1.205***	8.261***
	(17.15)	(71.91)	(6.69)	(14.31)
FICO	0.996***	1.00**	1.000	1.006***
	(−98.15)	(−2.08)	(−0.22)	(3.44)
LTV	0.991***	0.996	0.992	0.965*
	(−8.67)	(−0.57)	(−1.64)	(−1.71)
LTV squared	1.000***	1.000	1.000*	1.000
	(−7.01)	(0.54)	(−1.78)	(1.55)
Origination amount	1.000***	1.000**	1.000***	1.002**
	(−20.62)	(−2.18)	(−3.19)	(2.13)
Origination amount squared	1.000***	1.000*	1.000	1.000**
	(8.08)	(−1.81)	(1.28)	(−2.13)
Original interest rate	0.892***	1.166***	0.907***	1.046
	(−73.59)	(16.78)	(−12.52)	(1.23)
FIX (d)	1.227***	0.999	1.416***	2.655***
	(39.79)	(−0.02)	(14.44)	(6.92)
15-Year term (d)	1.125***	0.812	1.178**	1.828
	(6.10)	(−1.39)	(2.25)	(1.33)
20-Year term (d)	1.177***	0.458**	1.159	0.000***
	(4.22)	(−2.18)	(1.41)	(−127.51)
No insurance (d)	1.011**	2.114***	1.071***	0.406***
	(2.03)	(28.05)	(2.82)	(−5.49)
Insurance (d)	1.196***	1.259***	1.258***	0.259***
	(13.56)	(3.78)	(4.11)	(−3.51)
Time fixed effects	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes
N	316,772	316,772	15,203	15,203

differences between bank-held and securitized loans (with lower foreclosure rates for bank-held loans) show up together, and (b) the differences in cure rates and foreclosure rates are larger for loans that are of better initial credit quality as measured by the initial credit score. More specifically, for loans of the lowest credit quality (FICO score less than 620) there is an economically very small difference in foreclosure and cure rates. In contrast, for loans with medium initial credit quality these differences are large. For instance, in loans with medium initial credit quality, the foreclosure rate for bank-held loans is lower in absolute terms by 7.4% (26.6% in relative terms) and the cure rate is higher by 7% (17.1% in relative terms). The differences are even larger for loans with FICO greater than 680: the foreclosure rate for bank-held loans is lower in absolute terms by 9.2% (33% in relative terms) and the cure rate is higher by 12.1% (35% in relative terms) within a year after delinquency.[28]

Together, these findings highlight the value of understanding differences in cure rates between delinquent bank-held and comparable securitized loans in conjunction with differences in the foreclosure rates between these loans. Clearly, in the subsample of worst initial quality, there are small differences in cure rates between bank and securitized loans. For the same subsample, there is also a small difference in the foreclosure rates.[29] Conversely, cure rates and foreclosure rates are consistently different in loans of medium or high initial quality. Overall, these findings are consistent with absence of foreclosure bias in servicing decisions for securitized loans on lowest initial credit quality while being present in loans of medium and higher initial credit quality.[30]

6. Evidence from a quasi-experiment

In this section we exploit a particular institutional feature of this market to generate a plausibly exogenous variation in the securitization status of a delinquent loan. We then use this variation to identify the causal impact of securitization on the foreclosure bias in renegotiation decisions of loan servicers.

[28] We find qualitatively similar results when we conduct these tests on a sample restricted to fully documented loans.

[29] In this subsample, bank-held loans foreclosure (cure) at a slightly higher (lower) rate than securitized loans.

[30] Note that more than 50% of loans in the sample used in our main tests have an initial credit score greater than 620. The analysis in this section shows that the sample of loans with medium or high initial creditworthiness largely drives the differences between foreclosure and cure rates among securitized and portfolio loans that we find when we use the entire sample.

Table 8
Hazard regression of default and cure conditional on 60+ days delinquency (cross-sectional evidence).

This table reports the estimated hazard ratios from Cox-proportional hazard models of the transition from delinquency to foreclosure/transfer and of the transition from delinquency to cure/transfer for different FICO segments. Estimates on discrete variables represent the effect of moving from zero to one. Portfolio is a dummy which indicates that the loan was bank-held at the time of first 60+ days delinquency. All loans in the sample are originated between 2005 and 2006. Robust *t*-statistics are reported. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

	FICO < 620		620 < *FICO* < 680		*FICO* > 680	
Dependent variable:	Foreclosure	Transfer	Foreclosure	Transfer	Foreclosure	Transfer
Mean securitized	0.21	0.02	0.28	0.02	0.28	0.02
Portfolio (d)	1.044*	9.859***	0.734***	5.39***	0.67***	1.915***
	(1.95)	(76.57)	(−14.24)	(44.33)	(−18.32)	(12.46)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	131,466	131,466	111,638	111,638	73,668	73,668

	FICO < 620		620 < *FICO* < 680		*FICO* > 680	
Dependent variable:	Cure	Transfer	Cure	Transfer	Cure	Transfer
Mean securitized	0.58	0.02	0.41	0.02	0.34	0.02
Portfolio (d)	0.967***	9.462***	1.171***	6.35***	1.349***	2.829***
	(−3.01)	(61.16)	(12.53)	(4.79)	(22.53)	(17.76)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	131,466	131,466	111,638	111,638	73,668	73,668

In a number of deals, the originators are legally obligated to purchase back any securitized loans that become delinquent, typically within 90 days of the loan being securitized, due to the presence of repurchase clauses (called early pay default (EPD) clauses). In addition, originators are also legally obligated to purchase back any securitized loans that violate the general representations and warranties, evaluated in a post-sale audit that typically lasts up to 90 days after the sale. A large random sample of securitized pools in our data confirms the presence of these features in deal documents, though the exact clauses may vary (as can be seen in the internet appendix). These features are also described in several industry reports (e.g., BasePoint Analytics, 2007; American Securitization Forum, 2009). Once a loan has been sold back, the originators have the right to service these loans like any other loan on their balance sheet.

To understand the intuition behind how these clauses help us identify the impact of differential servicing between portfolio and securitized loans, consider the expected foreclosure (Y) of a loan that is securitized and becomes delinquent for the first time *just after* 90 days of being securitized (henceforth, control group, C):

$$E[Y_{imt}=1|i\in C,X_{imt},Z_{imt},\alpha_t,\delta_m]=\alpha+\alpha_t+\beta\cdot 1_{\{i\in T\}}+\gamma\cdot X_{imt}+\delta\cdot Z_{imt}+\delta_m, \quad (2)$$

where m is the MSA, t is the quarter of origination, X_{imt} is a vector of loan and borrower characteristics that includes variables such as FICO scores, interest rates, LTVs, and origination amounts, Z_{imt}'s are variables that are unobservable to the econometrician (omitted variables), $1_{\{i\in T\}}$ is an indicator variable that takes on a value of 1 if the delinquent loan belongs to the treatment group, and 0 otherwise, and α_t and δ_m are time and MSA fixed effects, respectively.

In contrast, consider the expected foreclosure (Y) of a loan that is securitized and becomes delinquent for the first time *just before* 90 days of being securitized (henceforth, treated group, T):

$$E[Y_{imt}=1|i\in T,X_{imt},Z_{imt},\alpha_t,\delta_m]=\alpha+\alpha_t+\beta\cdot 1_{\{i\in T\}}+\gamma\cdot X_{imt}+\delta\cdot Z_{imt}+\delta_m. \quad (3)$$

Note that the difference in the expected foreclosure of the two loans comes from the fact that a securitized loan that becomes delinquent just before the 90-day threshold is repurchased by the originator. At that time the originators have the right to service these loans as any other loan on the balance sheet (i.e., it becomes a *portfolio* loan). Differencing Eqs. (3) and (2) and applying the law of iterated expectations gives us

$$E[Y_i=1|i\in T]-E[Y_i=1|i\in C]=\beta. \quad (4)$$

To the extent that the securitization status is randomly assigned around the 90-day threshold, and that the treatment and control groups are similar, the parameter β gives us the causal effect of securitization on the foreclosure decision of loan servicers.

In our empirical analysis we estimate the following specification:

$$Pr(Y_{imt}=1|Delinquency)=\Phi(\alpha+\alpha_t+\beta\cdot 1_{\{i\in T\}}+\gamma\cdot X_{imt}+\delta_m+\varepsilon_{imt}), \quad (5)$$

where the dependent variable is an indicator variable for a *delinquent* loan i that takes a value of 1 if the loan is

foreclosed, and 0 otherwise. Conditioning on delinquency of a loan seems natural given that we are interested in the servicer's decision to foreclose a distressed loan. X_i are explanatory variables as defined in Eq. (2), and γ is a vector of coefficients. $1_{\{i\in T\}}$ is a dummy variable that takes the value 1 if the delinquent loan belongs to the *treatment* group, and 0 if the delinquent loan belongs to the *control* group. In addition, we also include MSA fixed effects (δ_m) and quarter fixed effects (α_t), and cluster the standard errors at the MSA level. In this specification, β measures the causal impact of securitization on a servicer's decision to foreclose the delinquent property.

6.1. Comments on empirical design

There are several challenges that threaten the construction of comparable treatment and control groups. First, the representations and warranties, and EPD clauses are typically applicable up to 90 days (see internet appendix and evidence in Section 6.2.3). However, loans that become delinquent closer to the securitization date and are sent back by securitization trusts to lenders are likely to be severely worse on unobservables as compared to loans that become delinquent after 90 days.[31] Consequently, comparing loans that became delinquent anytime within 90 days after being securitized and are repurchased with loans that became delinquent just after 90 days of being securitized might generate a severe bias against finding any improvement in eventual foreclosure rate due to loan servicing for repurchased loans. Our main test focuses on the close vicinity of 90 days to reduce this heterogeneity between the treatment and the control group.

Second, there is variation in how long it may take delinquent loans which are subject to repurchase clauses to eventually come back on the balance sheet of the lender. The reason for this variation is that some of these provisions (e.g., EPD clauses) typically allow originators to try and cure the loan. Consequently, this feature of the contract could introduce heterogeneity in payment history of a delinquent loan that is repurchased by the lender vs. delinquent loans that remain securitized (for example, the monthly payment sequence of a delinquent loan in the treatment group could be 30+, 60+, 90+ days delinquent before it is repurchased by the lender vs. 30+, 30+, 30+ days delinquent for the delinquent loan that forms a part of the control group).[32] We circumvent this complication by focusing on loans with identical payment history around the vicinity of the 90-day threshold. Keeping the tracking horizon after delinquency very short allows us to construct the control and treatment groups with identical payment histories. There is, however, a trade-off in following this approach. While we are able to construct control and treatment groups that are identical in terms of their payment histories and other contractual terms, we lose power in our tests, as we have fewer observations in both the control and treatment groups from what is potentially available around the vicinity of the threshold. As a compromise, we settle for a tracking horizon of three months, although our results are qualitatively similar when we use a one-month tracking horizon instead.[33]

Our treatment group, therefore, consists of loans that become 30+ days delinquent just before the 90-day trigger, transition to 60+ in the next month and are recorded on a bank's balance sheet in the subsequent three months, while the control group consists of loans that become 30+ days delinquent just after the 90-day trigger, transition to 60+ in the next month, and remain securitized in the subsequent three months.

There are two other issues that deserve some discussion. First, we do not exactly know the types of clauses or representations and warranties associated with each loan since LPS data do not report which deal a particular loan is securitized into. While focusing on loans that became delinquent just before the 90-day trigger and *actually came back* on the bank's balance sheet subsequent to being delinquent gives us our treatment group, data do not allow us to evaluate the extent to which different types of representations and warranties and/or EPD clauses drive our findings. In Section 6.2.3, we discuss this issue in more detail. Second, our analysis assumes that the incidence of delinquency is random around the three-month threshold. We discuss the validity of this assumption in Section 6.2.4.

6.2. Empirical results

6.2.1. Main tests

We begin by summarizing the contractual characteristics of the treatment and control groups as defined above. The treatment groups consists of securitized delinquent loans that are delinquent just before the 90-day trigger and return to the balance sheet within three months of being 60+ days delinquent, while the control group consists of securitized delinquent loans that were delinquent in the fourth month, i.e., just after the 90-day trigger and stay securitized for the next three months. As can be seen from Fig. 2, about 9% of securitized loans that became delinquent within 90 days after being securitized are repurchased by banks within three months after the first instance of serious delinquency. Moreover, consistent with the notion that most of these repurchase agreements are typically applicable to loans in the first three months after securitization, we also find that a much smaller fraction of securitized loans that

[31] Research studies and anecdotal evidence suggest that loans that become delinquent very quickly and are repurchased by the originators are of worse quality on both observable and unobservable dimensions (see Mayer, Morrison, and Piskorski, 2009; BasePoint Analytics, 2007).

[32] We also exclude from our sample the loans that were securitized at the time of origination. The reason is that this category of loans are quite different on average observable risk characteristics and consequently including these loans introduces a heterogeneity in observables in treatment and control group. Nevertheless, we assess the robustness of our results to inclusion of these loans and find that the results remain unchanged (see internet appendix).

[33] A six-month tracking horizon introduces significant heterogeneity, both in the contract terms and payment histories for the treatment and control groups. To account for this heterogeneity, we could match loans in the treatment sample with comparable loans in the control sample. Doing so gives qualitatively similar results.

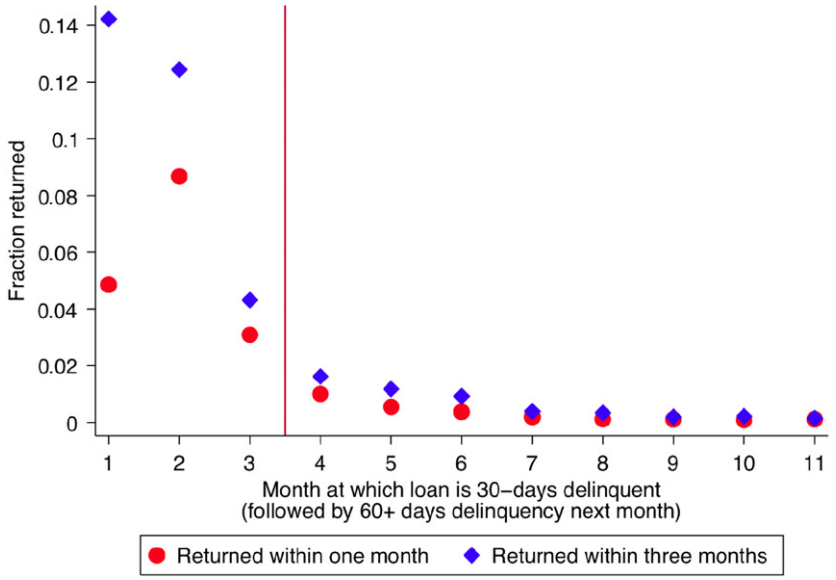

Fig. 2. Fraction of delinquent loans coming back to lenders. This figure plots the fraction of loans that return on portfolio as a function of the first time they become delinquent after securitization. More precisely, the figure shows a fraction of loans that becomes 30-day delinquent followed by 60-day delinquency for the first time in a given month after securitization that return on portfolio. We present the graph for two horizons–returns within one month after the loan hits 60-day delinquency and returns within three months after the loan hits 60-day delinquency. Consistent with the time horizon over which repurchase clauses are applicable (see Section 6), we observe that most returns for delinquent loans occur in the first three months after securitization.

became delinquent after the third month came back on portfolio (only about 1% of loans that become delinquent after three months are repurchased by the lender vs. 9% that are repurchased if the loan becomes delinquent within the first three months).

Table 9 shows that the treatment and control groups have similar contractual terms such as the LTV ratio (82.1 for the treatment group vs. 80.7 for the control group in last four columns of Panel B) and FICO score (617 for the treatment group and 611 for the control group in last four columns of Panel B). Most importantly, the interest rate, which captures the inherent risk characteristics of these loans, is identical for the treatment and control groups (8.2% for the treatment as compared to 8.3% for the control group). This pattern can also be visually observed if we plot Epanechnikov kernel densities of observable characteristics of loans in Fig. 3.

While this figure concentrates only on loans in the treatment and control groups, we next examine the characteristics of loans in these groups relative to characteristics of other loans around the 90-day threshold. To construct this figure, we represent average characteristics of loans in each quarter which became 30+ delinquent in a given month, transitioned to 60+ in the next month, and were repurchased by the originator in the next three months on the left of the cutoff. To the right of the cutoff, we represent characteristics of loans in each quarter that became 30+ delinquent in a given month, transitioned to 60+ in the next month, and remained securitized in the next three months. By construction, the loan characteristics in months three and four in the graph represent the characteristics of loans in the treatment and control groups of the main test. To remove any time effects, we regress loan characteristics on origination quarter dummies and the time since securitization to control for the mechanical relationship that loans that become delinquent later in time tend to have lower foreclosure rates. We then plot the corresponding residuals against the time since securitization and present the results in Fig. 4. Fig. 4(a) plots FICO scores, Fig. 4(b) plots interest rates, and Fig. 4(c) plots LTV ratios. As is confirmed in these figures, there is no statistical jump in the other observables around the cutoff. Nevertheless, in regressions we condition on observable characteristics when we make inferences on differences in foreclosure rates across the two groups.

Before formally conducting our test, we also plot the foreclosure rate of delinquent loans around the 90-day threshold and present the results in Fig. 5. As can be seen, loans in the treatment group are less likely to be foreclosed relative to loans in the control group. This pattern is also consistent with summary default rate differences reported in Table 9. For instance, 32% of the delinquent loans in the treatment group are foreclosed. This is significantly smaller than the 39% of the delinquent loans that are foreclosed in the control group. We next turn to regressions to test this relationship more formally.

We estimate Eq. (1) using a logit specification and present the results in Table 10. The estimate of interest is β, the coefficient on *Portfolio*, where *Portfolio* takes a value of 1 for loans in the treatment group and a value of 0 for loans in the control group. In all the tests we include observables as defined in Eq. (2). In addition, we also include MSA and quarter fixed effects and cluster the standard errors at the MSA level. We call this test a "3:4" test for ease of reference. As is reported in column 1, β is negative and significant. In column 2, we add more controls including loan age since securitization and time fixed effects interacted with state

Table 9
Summary statistics of sample of loans used in quasi-experiment.

This table reports summary statistics of a sample of loans used to conduct the test that exploits the repurchase clauses. The vertical headers indicate how many months after securitization the loan first becomes 30+ days delinquent, followed by 60+ days the next month. The top (bottom) panel tracks the loan for one (three) month(s) after the month of 60+ days delinquency. The treatment group consists of securitized loans that return to portfolio either in the month of 60+ days delinquency or within the corresponding time horizon. The control group consists of loans that remain securitized throughout the corresponding time horizon.

Panel A: One-month tracking horizon

Months after securitization delinquent	One month				Two months				Three months				Three vs. Four months			
	Treatment		Control		Treatment		Control		Treatment		Control		Treatment		Control	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
FICO	615.3	63.3	613.6	65.5	618.7	63.3	613.3	65.6	614.7	60	612.7	63.8	614.7	60	610.8	62.2
LTV	82.2	11.6	80.5	12.3	82.2	11.8	80.3	11.2	81.7	12.2	80.4	11.1	81.7	12.2	80.5	11.4
Interest rate	0.089	0.016	0.083	0.015	0.086	0.015	0.084	0.016	0.082	0.014	0.083	0.015	0.082	0.014	0.083	0.015
Foreclosure	0.46	0.5	0.36	0.48	0.41	0.49	0.39	0.49	0.31	0.46	0.37	0.48	0.31	0.46	0.35	0.48
N	390	390	7,610	7,610	1,041	1,041	10,849	10,849	394	394	12,345	12,345	394	394	12,824	12,824

Panel B: Three-month tracking horizon

Months after securitization delinquent	One month				Two months				Three months				Three vs. Four months			
	Treatment		Control		Treatment		Control		Treatment		Control		Treatment		Control	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
FICO	616.1	60.4	611.1	65.1	618.2	61.6	613.4	65.6	617.6	61.1	613.1	63.5	617.6	61.1	611.2	62.0
LTV	82.7	12.2	80.4	11.8	81.7	11.8	80.5	11.1	82.1	11.5	80.5	11.1	82.1	11.5	80.7	11.2
Interest rate	0.087	0.014	0.084	0.015	0.085	0.015	0.084	0.016	0.082	0.015	0.083	0.015	0.082	0.015	0.083	0.015
Foreclosure	0.41	0.49	0.4	0.49	0.4	0.49	0.41	0.49	0.32	0.47	0.39	0.49	0.32	0.47	0.36	0.48
N	1,044	1,044	6,240	6,240	1,489	1,489	9,934	9,934	526	526	11,636	11,636	526	526	12,157	12,157

dummies to account for changing macroeconomic conditions at the state level and find qualitatively similar results. In particular, the estimate in column 2 suggests that delinquent securitized loans that are taken back on the bank's balance sheet foreclose at a rate that is 6.5% lower in absolute terms as compared to delinquent securitized loans that continue to be securitized (17.8% lower in relative terms).[34]

6.2.2. Selection concerns

As mentioned above, an impediment to our research design is that the LPS data set does not allow us to match the loans to their respective deals. This is problematic since there may be heterogeneity introduced in treatment and control groups due to variation in the repurchase clauses across deals. Specifically, on examination of PSAs (Pooling Service Agreements) and prospectuses from Securities and Exchange Commission (SEC) public records of around 400 deals underwritten in 2005 and 2006, we find that while the representations and warranties are present in all deals, the EPD clauses are found in only 10% to 15% of deals.

To understand the nature of this problem and the bias generated by it, we employ another data set that provides information on the deal a given securitized loan belongs to and also allows us to examine the pattern of delinquent loans returning to a bank's portfolio.[35] The pattern of delinquent loans returning to a bank's portfolio for all the 400 deals reveals substantially higher returns in the first three months after securitization, consistent with the time stated in a typical repurchase clause (see internet appendix). Moreover, loan returns occur in the presence of both representations and warranties and EPD clauses, suggesting that the treatment group consists of loans due to both types of repurchase clauses. While the patterns of returns provides validation to our research design, it also exposes another plausible selection issue–not all delinquent loans with repurchase clauses return back on the balance sheet. This explains why, as observed in Section 6.2.1, only around 10% loans returned back.

[34] We also augment the specification by including house price changes in the loan's MSA from the time of origination to delinquency, which might have accounted for the foreclosure of the delinquent loan. We also add quarter of delinquency fixed effects in the specification (see internet appendix). Our estimates are robust to these changes.

[35] This data set, however, suffers from another drawback. Since it reports information on only securitized loans, it does not allow us to track a given loan after the loan exits the database.

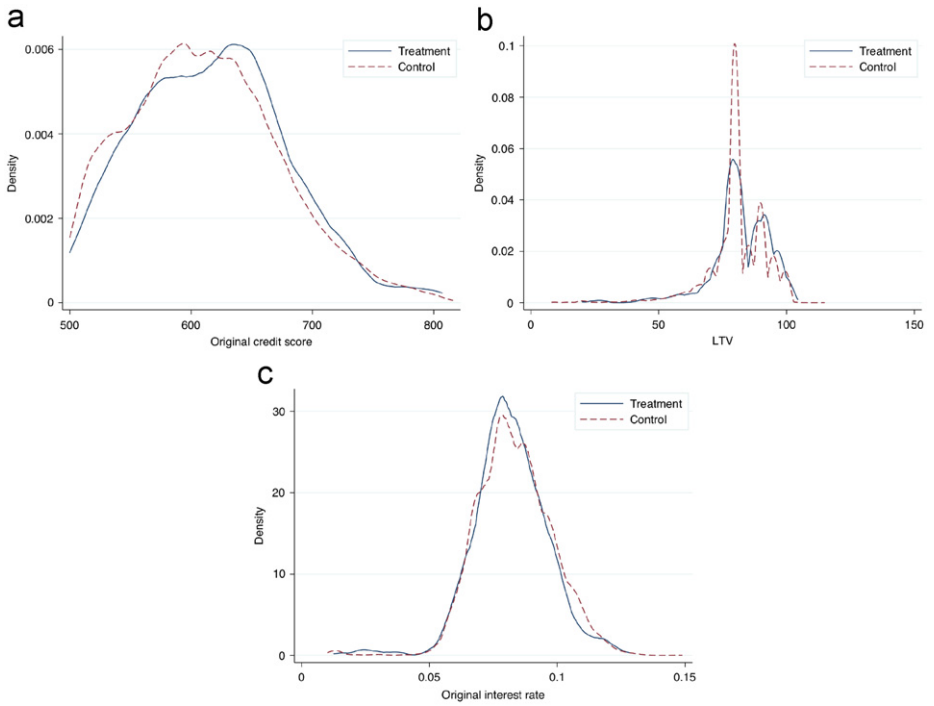

Fig. 3. Kernel densities of characteristics of loans in treatment and control group used in the main (3:4) test in the quasi-experiment. This figure shows Epanechnikov kernel densities of characteristics of the loan sample used to conduct the test that exploits the repurchase clauses. The treatment group consist of securitized loans which became 30+ days delinquent three months after securitization, transitioned to 60+ in the next month, and were repurchased by the originator in the following three months, while the control group consist of securitized loans which became 30+ days delinquent four months after securitization, transitioned to 60+ in the next month, and remained securitized in the next three months. The bandwidth employed has the width that minimizes the mean integrated squared error as if the data were Gaussian and a Gaussian kernel were used.

Our discussion above clarifies the two selection issues that confront us. First, there is a potential selection due to the heterogeneity in the presence of EPD clauses across deals, since not all deals have this clause. Second, one might worry that there might be selection as conditional on having a repurchase clause not all delinquent loans return to the balance sheet of the bank. We now elaborate how we deal with these issues in turn.

The first selection problem stems from the fact that not all loans have EPD clauses associated with them. Our analysis compares loans from deals with repurchase clauses in the treatment group with loans in the control group which may potentially come from both deals with and deals without repurchase clauses. We therefore need to worry that our results might be driven by difference in quality of loans across deals with different repurchase provisions. While it is true that the test is potentially confounded, it is important to understand the sign of the bias that this generates. For instance, if deals with repurchase clauses tend to have riskier loans, then this would bias against finding anything, because we find these risky loans are foreclosed at a lower rate. Thus, for this selection to create a bias against our results, it would have to be the case that loans from deals with EPD clauses are inherently *safer* than the loans from deals without these provisions.

A comparison of characteristics of loans across deals reveals that deals with EPD clauses tend to contain loans that are riskier on observables. In particular, as can be seen in Table 11, deals with EPD clauses tend to be have loans with lower FICO scores and higher interest rates. For example, the average FICO score for non-EPD deals in our sample was 646, while it was 617 for EPD deals. Similarly, the interest rate for non-EPD loans was 7.43 compared to 8.37 for EPD deals. Furthermore, controlling for observables, there is no difference in the delinquency rate of loans across deals.[36] Our analysis thus suggests that EPD clauses are driven by observable risk

[36] Specifically, to conduct this test, we took a random sample of around 400 deals underwritten during 2005 and 2006 and collected information on whether these deals had EPD clauses from publicly available deal documents on the SEC website. We then conducted tests using all loans in these deals. Loan-level data come from BlackBox and include all loans originated in 2005 and 2006 with payment history on these loans tracked until February 2009. BlackBox data include data on characteristics of subprime mortgages and cover 80–90% of all securitized subprime mortgage pools (similar to LoanPerformance). In the sample, 99,809 loans were securitized in deals with explicit EPD clauses and 484,398 loans were securitized in deals without explicit EPD clauses. Our primary test examines whether, conditional on observables, loans in deals with explicit EPD clauses differ on ex post performance (potentially capturing unobservables) from loans in deals with no EPD clauses. The dependent variable in the logit regression is a dummy that takes a value 1 if the loan becomes 60+ delinquent (alternatively, 30+ delinquent) during the tracking period, and 0 otherwise. The regression, besides a dummy variable that indicates whether or not the loan has an EPD clause, also includes controls for observables such as FICO, LTV, interest rate, loan amount, and time and MSA fixed effects. The results are reported in Table 11.

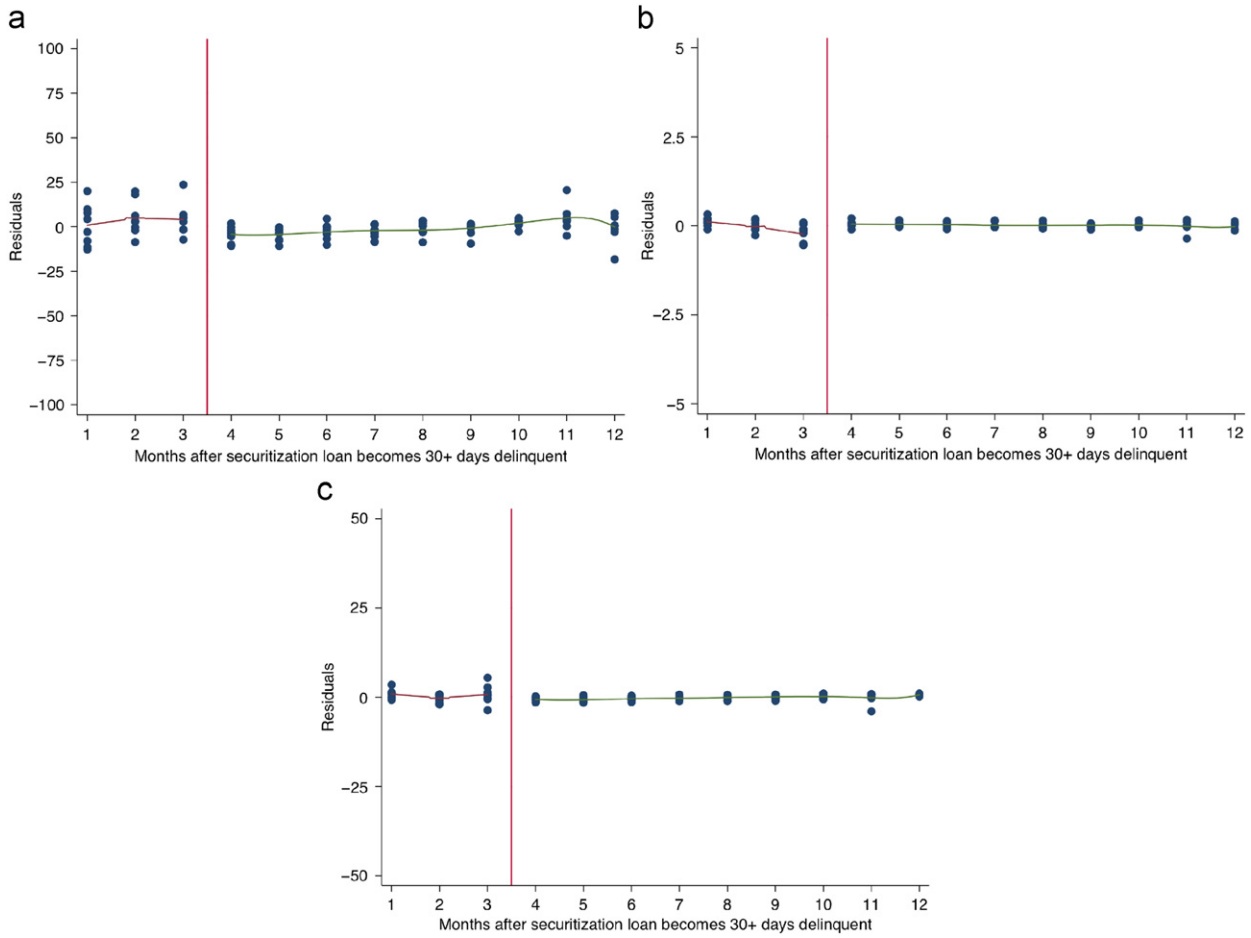

Fig. 4. Scatter plot of loan characteristics around 90-day threshold. This figure shows the scatter plot of characteristics of loans around the 90-day threshold. To the left of the cutoff, we represent average characteristics of loans in every quarter which became 30+ days delinquent in a given month, transitioned to 60+ in the next month, and were repurchased by the originator in the subsequent month (tracked for three months after 60+). To the right of the cutoff, we represent characteristics of loans in every quarter which became 30+ days delinquent in a given month, transitioned to 60+ in the next month, and remained securitized in the subsequent month (tracked for three months after 60+). By construction, characteristics of loans in month three and month four in the graph represent the characteristics of loans in the treatment and control group of the 3:4 test. To remove any time effects, we regress loan characteristics on quarter dummies and plot the residuals. Figure (a) plots FICO scores, Figure (b) plots interest rates, while Figure (c) plots LTV ratios.

characteristics of the overall pool and there are no differences in loan-level unobservable risk between EPD and non-EPD deals.[37] Note that the interest rate around the three-month cutoff for treatment and control loans is similar. To the extent that interest rate captures the summary statistics of the inherent riskiness of the loans, the similarity in interest rates also tells us that the loans do not differ in their inherent ex ante overall risk characteristics.

The final set of factors that might lead to this selection relates to the inherent quality of originators and servicers associated with the deal. We are able to control for lender fixed effects in our tests using additional data where we have information on lenders originating the loans. This helps account for unobservable lender-level characteristics like screening technology and capacity constraints. Moreover, we only need to worry about servicer fixed effects to the extent that there is heterogeneity in servicers used across deals by the *same* lender. To examine if this is the case, we use BlackBox data and find that lenders that securitize loans in the data are significantly likely to have the same servicer across deals they originate (perhaps driven by relationships). In particular, in our data there is an 83% chance that a typical lender in our sample has the same servicer across deals. In other words, accounting for lender fixed effects accounts to a large extent for unobserved quality of originators and servicers associated with the deal. Our results are qualitatively similar when we conduct our tests with lender fixed effects.

The second selection problem originates from the fact that only some delinquent loans are sent back

[37] This issue should also be clear from the fact that repurchase agreements are deal-level variables and not loan-level variables. In other words, the presence of EPD clauses for a given loan are likely to be driven by pool- or group-level characteristics rather than loan-level characteristics.

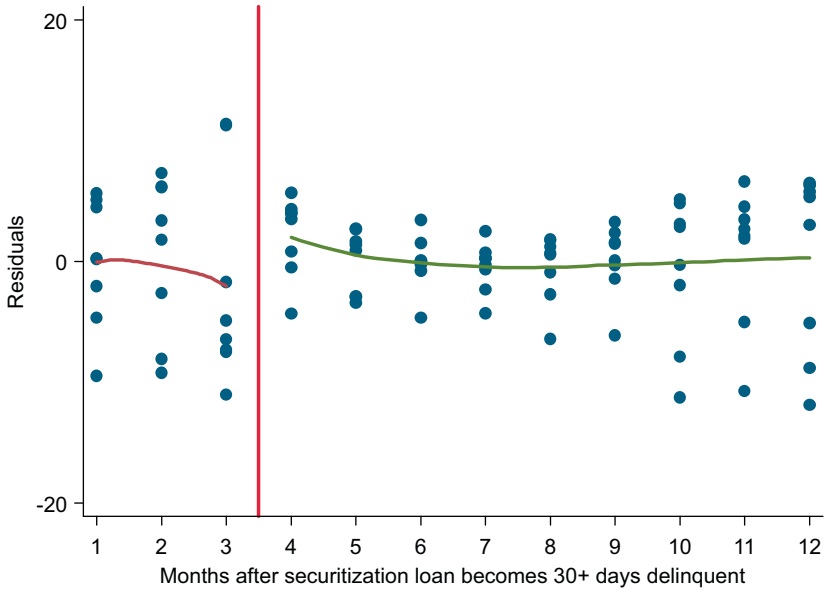

Fig. 5. Scatter plot of foreclosure rates around 90-day threshold. This figure shows the scatter plot of foreclosure rate of loans around the 90-day threshold. To the left of the cutoff, we represent average foreclosure rate of loans in every quarter which became 30+ days delinquent in a given month, transitioned to 60+ in the next month, and were repurchased by the originator in the subsequent month (tracked for three months after 60+). To the right of the cutoff, we represent foreclosure rate of loans in every quarter which became 30+ delinquent in a given month, transitioned to 60+ in the next month, and remained securitized in the subsequent month (tracked for three months after 60+). By construction, foreclosure rates of loans in month three and month four in the graph represent the foreclosure rate of loans in the treatment and control groups of the 3:4 test. To remove any time effects, we regress foreclosure rates on origination quarter dummies. The regression also conditions on other observables such as FICO, LTV, origination amount and interest rates and plot the residuals.

by securitization trusts.[38] However, this issue may not be a concern since the delinquent loans sent back by securitization trusts to lenders (treatment loans) are likely to be of worse quality, thereby biasing us against finding that these loans are foreclosed at a lower rate. In addition, as discussed in the next subsection, the fact that we find foreclosure differences become larger for loans of high credit quality is hard to reconcile with this selection argument. Finally, we also find that interest rate of delinquent loans that return in a given month is very comparable to interest rate of delinquent loans that do not return in the same month (see Table 9)—suggesting that loans that are sent back relative to those kept by securitization trusts had similar perceived risk at origination.

In summary, our analysis provides validity to our research design—the treatment and control groups seem identical in all respects (if anything treatment group might be riskier) and the securitization status is randomly assigned to these groups.

[38] Note that for deals with only reps and warranties, it is reasonable that not all delinquent loans within three months should return. The reason is that not all these loans would have violated some reps and warranty as specified in the clause. There may also be reasons which explain why not all delinquent loans return in deals which have EPD clauses in addition to reps and warranties. For instance, most of the EPD clauses allow the servicer discretion to wait and try to cure the loan if possible. Alternatively, the trustees may want to incur the transaction costs only if the benefit of sending the loan for the investors is large enough.

6.2.3. Placebo and other tests

Having established the validity of our empirical design, we now conduct several additional tests to bolster our claims. We begin by comparing the foreclosure rates of loans that became delinquent in the third month of securitization, *but stayed securitized*, to those that were delinquent in the fourth month and stayed securitized. This constitutes a placebo test since we replace the loans in the treatment group in the main test with those that *did not receive the treatment*. A lower foreclosure rate for the loans that were delinquent in the third month, as compared to the control group, would suggest that there is something specific to the cutoff around the third month, and that our main results are not driven by actions taken by banks to reduce foreclosures. We present the results from this analysis in column 3 of Table 10. As can be observed, the difference in foreclosure rates, in contrast to the main test, is *positive*, although not statistically significant. This confirms that the results in the 3:4 test are likely driven by some differential treatment that is accorded to the bank held loans.

Next, following our discussion from the previous section, we exploit the fact that inherent riskiness of loans that return to the lender is likely to be similar or worse than those that remain securitized to conduct a "3:3" test. In this test, we compare the foreclosure rates of loans that were delinquent in the third month and came back on the balance sheet of banks with those that were delinquent in the third month but stayed securitized. This test can alternatively be viewed as a difference in foreclosure rates of the 3:4 test (our main test), with the placebo test

Table 10
Regression estimates using a quasi-experiment.

This table reports the estimates (marginals) using a specification and controls as used in Table 3. The dependent variable is Foreclosure. In columns (1) and (2), the treatment group consist of securitized loans which became 30+ days delinquent three months after securitization, transitioned to 60+ in the next month, and were repurchased by the originator in the following three months, while the control group consist of securitized loans which became 30+ days delinquent four months after securitization, transitioned to 60+ in the next month, and remained securitized in the next three months. In column (3), the treatment group consists of securitized loans which became 30+ days delinquent three months after securitization, transitioned to 60+ in the next month, and remained securitized in the next three months, while the control group consists of securitized loans which became 30+ days delinquent four months after securitization, transitioned to 60+ in the next month, and remained securitized in the next three months. In column (4), the treatment group consist of securitized loans which became 30+ days delinquent three months after securitization, transitioned to 60+ in the next month, and were repurchased by the originator in the following three months, while the control group consist of securitized loans which became 30+ days delinquent three months after securitization, transitioned to 60+ in the next month, and remained securitized in the next three months. In column (5), the treatment group consists of securitized loans which became 30+ days delinquent within three months after securitization, transitioned to 60+ in the next month, and were repurchased by the originator in the following three months, while the control group consists of securitized loans which became 30+ days delinquent within three months after securitization, transitioned to 60+ in the next month, and remain securitized in the three months following delinquency. In column (6), the treatment group consists of securitized loans which became 30+ days delinquent within three months after securitization, transitioned to 60+ in the next month, and were repurchased by the originator in the following three months, while the control group consists of securitized loans which became 30+ days delinquent between four and six months after securitization, transitioned to 60+ in the next month, and remain securitized in the three months following delinquency. In column (7), the treatment group and the control group are same as in column (5). In addition to the Portfolio dummy, the regression includes FICO, LTV, interest rate, origination amount, squared terms of these variables, insurance and maturity dummies, age of the loan at delinquency, and MSA and origination-quarter fixed effects. Marginal effects are reported for the logit regression. Coefficients on discrete variables represent the effect of moving from zero to one. Standard errors are clustered at MSA level and resulting *t*-statistics are reported in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Test (3-month tracking horizon)	3:4 (Main)	3:4 (Main)	3:4 (Placebo)	3:3 (Main)	1:1+2:2+3:3 (Pooled)	123:456 (Pooled)	1:1+2:2+3:3 (Pooled)
Mean control	0.364	0.364	0.364	0.391	0.401	0.367	0.401
Portfolio (d)	−0.049*	−0.065**	0.011	−0.069**	−0.041**	−0.033**	−0.031*
	(−1.80)	(−2.49)	(1.39)	(−2.44)	(−2.32)	(−1.97)	(1.93)
Portfolio (d)*High-quality (d)							−0.115***
							(−2.63)
High-quality (d)							−0.028**
							(−2.30)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan age	No	Yes	Yes	Yes	Yes	Yes	Yes
Loan origination period	05Q1–06Q4	05Q1–06Q4	05Q1–06Q4	05Q1–06Q4	05Q1–06Q4	05Q1–06 Q4	05Q1–06Q4
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering unit	MSA	MSA	MSA	MSA	MSA	MSA	MSA
Treatment/Control	481/11,196	481/11,196	10,816/11,313	474/10,672	2,880/25,913	2,887/59,211	2,880/25,913

described above (i.e., it is like a difference-in-difference test between 3:4 and the placebo sample above). It can be seen from our results reported in column 4 of Table 10, that there is a difference of about 6.9% lower foreclosure rate on loans that become delinquent in the third month and return to banks relative to those that remain securitized. In addition to the 3:3 test, we also conduct a "1:1+2:2+3:3" test. Here we pool all the securitized loans that were delinquent in the first three months and came back on the balance sheet of banks in the treatment group. The control group comprises securitized loans that were delinquent in the first three months of being securitized and stayed securitized. The pooling of loans increases the power of the test, as there are more loans in the treated group. The flip side, however, is that loans that are delinquent earlier tend to be of worse quality and therefore are likely to have a lowered magnitude of the treatment effect. The results of this test are reported in column 5. As can be observed, the treatment effect remains statistically and economically significant.

In column 6, for robustness, we conduct another variant of this test, which we refer to as "123:456". We pool all the securitized loans that were delinquent in the first three months and came back on the balance sheet of banks in the treatment group. The control group comprises securitized loans that were delinquent in fourth, fifth, and sixth months of being securitized and stayed securitized. As is the case in the test in column 5, while pooling the data increases the statistical power of our analysis, it introduces substantial heterogeneity in the treatment and the control groups. A loan that is delinquent in the first month tends to be of a much inferior quality than a loan that is delinquent, for example, in the sixth month. The heterogeneity introduced, however, biases against finding a result. As expected, with the trade-off highlighted above, moving away from the trigger of three months makes the results economically weaker (the treatment effect is around 3.3% in absolute terms).

Next, we assess if the effects we document vary with the initial credit worthiness of the borrower to match our results in Section 4.3. The results reported in column 7 support our earlier results. The differences in foreclosure rates are accentuated for the sample of high-quality loans,

Table 11
Characteristics of EPD and non-EPD deals.

Panel A presents summary statistics for loans originated in securitization pools with and without early payment default clauses. Panel B reports the estimates (marginals) of a logit regression. The dependent variable is 60+ days delinquency in the first two columns and 30+ delinquency in the last two columns. Control variables include FICO score, FICO score squared, LTV, interest rate, loan amount, MSA fixed effects, and origination time fixed effects where indicated. Standard errors are clustered at MSA level and resulting t-statistics are reported in parentheses. ***, **, and * represents significance at 1%, 5%, and 10% respectively. Data comes from the BlackBox loan database and spans 2005 and 2006.

Panel A: Summary statistics

	EPD Deals	Non-EPD Deals	Difference
FICO	616.8	645.7	28.93***
LTV	78.7	78.6	−.1
Interest Rate	8.37	7.43	−.94***
Observations	99,809	484,398	

Panel B: Logit regressions

	Pr (60+ =1)	Pr (60+ =1)	Pr (30+ = 1)	Pr (30+ = 1)
EPD dummy	0.009	−0.015	−0.001	−0.023
	(0.33)	(−0.57)	(−0.03)	(−0.83)
FICO	−0.138	−0.157*	0.017	−0.001
	(−1.41)	(−1.67)	(0.20)	(−0.01)
FICO squared	−0.017	−0.002	−0.208**	−0.194**
	(−0.18)	(−0.02)	(−2.42)	(−2.32)
LTV	−0.112**	−0.125**	−0.090*	−0.103**
	(−1.97)	(−2.23)	(−1.79)	(−2.09)
Interest rate	0.208***	0.160***	0.042***	0.035**
	(3.49)	(3.01)	(2.91)	(2.51)
Other controls	Yes	Yes	Yes	Yes
Observations	584,183	584,183	584,204	584,204
Time fixed effects	No	Yes	No	Yes
MSA fixed effects	Yes	Yes	No	Yes

where high quality is defined as before to be loans that have both the full-documentation status and have FICO scores that are above the 75th percentile in the sample. This result is similar to the finding in the aggregate sample, and as we will discuss in Section 7, is consistent with some economic arguments that suggest that renegotiation should be undertaken more intensively for borrowers of high credit quality.

Finally, we also estimate a hazard model similar to the one we estimated in Section 4.4.1 to take into account the effect of loans that are transferred out of the sample. In conducting the test with transfer loans, we first have to match the treatment and control sample since adding transfer loans into the sample introduces substantial heterogeneity in the control and treatment groups (see internet appendix for evidence). We match the treatment and control groups within the transferred and non-transferred samples based on the interest rate and the FICO score of a loan. The results from the 3:4 and 3:3 tests are qualitatively similar to those reported from our earlier specifications. Our tests from the hazard model also suggest that the treatment loans cure faster as compared to the loans in the control group in the 3:4 and 3:3 tests (results reported in internet appendix). We next discuss the validation of our identification assumption.

6.2.4. Identification assumption

Our identification strategy hinges on the incidence of delinquency being random around the three-month threshold. Since this threshold is known to lenders in advance, it is plausible that they may take steps to game this rule. In particular, based on unobservable information, the lenders may keep worse loans on "life support" until they cross over the 90-day trigger threshold. The reason is that among delinquent loans, ones that are worse, would on the margin, result in more losses for the lender if the lender had to take them back. Note, however, that this gaming by lenders is considered illegal and punishable by law since it violates the representations and warranty agreements of lenders with investors (see, for instance, Oklahoma Teachers v. American Home Mortgage).[39] Nevertheless, to address this concern, we conduct two tests.

First, Fig. 6(a) and (b) show the scatter plot of percentage of securitized loans becoming 30+ days delinquent around the 90-day threshold. If there was lender manipulation, one would expect an under-reporting of delinquencies just before the cutoff and a bunching of delinquencies just after the cutoff. As can be observed in both the figures, this is not the case: the rate of delinquency is similar around the 90-day cutoff, supporting the identification assumption of no manipulation around the threshold.

[39] Yet another possibility is that borrowers may strategically decide the timing of their delinquency—in anticipation of being able to extract better terms once the loan returns to the bank, they might time their delinquency before the 90-day threshold. This possibility is, however, ruled out since borrowers rarely know the securitization status of their loan.

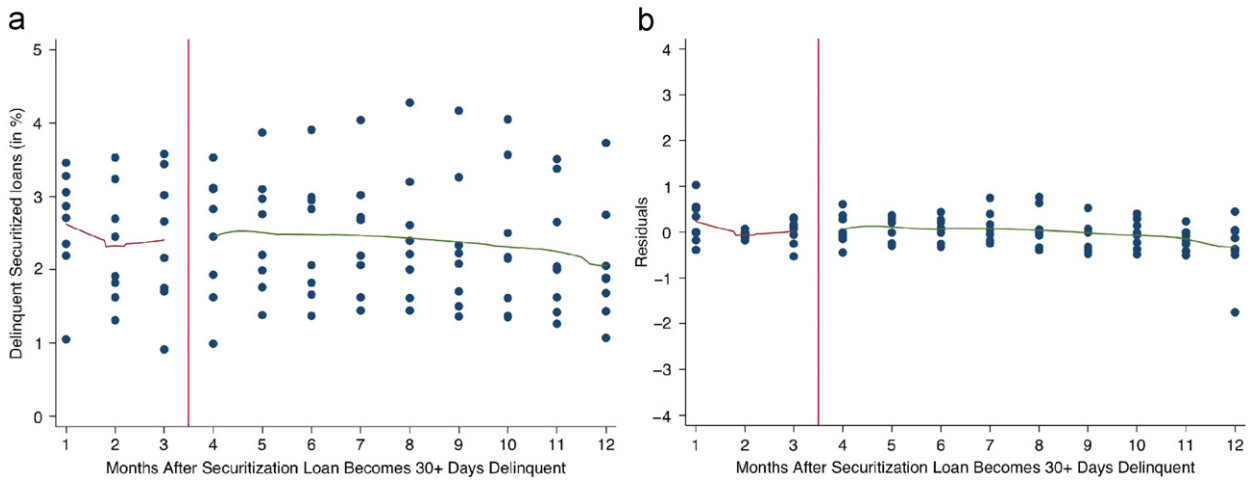

Fig. 6. Scatter plot of percentage of loans becoming 30+ days delinquent around 90-day threshold. This figure shows the scatter plot of percentage of securitized loans in every quarter that become 30+ days delinquent around the 90-day threshold. We present both the raw percentages (a) and residuals obtained after regressing the raw percentages on origination quarter dummies to remove the time effects (b).

Second, we also conduct a more formal test based on the following idea. If our results are entirely driven by lender manipulation, they should disappear if we remove loans which look as though lenders kept them on life support just to ensure they crossed the 90-day trigger from the control group. We identify these loans as ones which immediately transition to worse states as soon as they cross the three-month trigger.[40] In unreported tests, we find that the qualitative nature of our results does not change.

6.2.5. *Final comments*

We would like to conclude this section with two important remarks. First, while the estimates based on the tests from this section should be interpreted locally (i.e., it is a local average treatment effect, (LATE)), they may convey information about what the effect for an average delinquent loan (i.e., the average treatment effect, (ATE)) is likely to be. It is important to note that relative to the population's average delinquent borrower, the sample's average delinquent borrower used in this section (Table 9) tends to be of worse credit quality (Section 4). As we will discuss in Section 7, there may be compelling arguments that suggest that renegotiation is likely to be undertaken more aggressively for borrowers with higher initial credit quality. Consequently, under this interpretation of our findings, the ATE is likely to be similar (if not higher) than the estimates obtained in this section.

Second, we also track loans subsequent to their repurchase by the lenders. We find that a substantial fraction of loans that contribute to the difference in foreclosure rates in our tests were re-securitized within six months of returning to the bank's balance sheet. While we do not observe the identity of the investors, there are some reports that state that some of these loans are resold at heavy discounts to investors in what is called the "scratch-and-dent" market (see Fitch Ratings Report, December 2005). It is important to highlight that this pattern is consistent with our hypothesis that securitization creates a foreclosure bias.

For instance, quick re-securitization of some loans could simply reflect that these repurchased loans might have been cured quickly by the lenders and resold. Alternatively, lenders might be able to re-securitize loans to investors who could employ a specialized servicer that is more effective in workouts with the borrower.[41] This alternative may have been easier to implement only because originators had the right to service these loans as portfolio loans.[42] Each of these actions could result in foreclosure differences between bank-held and securitized loans and since these actions affect the nature of the relationship between borrower, lender, and the servicer, we can broadly interpret them as representing a contract renegotiation.

7. Discussion and conclusion

Our analysis strictly focuses on establishing that delinquent securitized loans are foreclosed at a much higher rate as compared to similar delinquent bank-held loans. We next briefly discuss some of the implications of our results, commenting on the evidence related to our findings, on the mechanism that might be driving our results and on the possible policy implications of our findings.

[40] More specifically, we drop loans which transition immediately from 60+ after the cutoff to worse payment states in subsequent (consecutive) months until they reach foreclosure.

[41] This is not far-fetched since interviews with industry experts in the scratch-and-dent market suggest that this is especially relevant for loans that might be re-securitized in this market.

[42] Note that our identification does not impose any requirement on the time the repurchased loans need to be serviced like a portfolio loan. All it requires is that, at the time the loans are repurchased, originators have the right to service these loans as any other loan on the balance sheet.

7.1. Evidence related to our findings

Our paper cannot comment on the tools servicers may have used differently across portfolio and securitized loans. In other words, we measure output from the servicing activity rather than the actual servicing that was performed on these loans. The difference in foreclosure rates could come either from different tools employed in servicing of bank loans relative to servicing of securitized loans, or similar tools being used in servicing with different intensity and/or efficiency. In principle, there are a variety of tools servicers may use when renegotiating troubled mortgages: repayment plans, forbearance, short-sales, foreclosure moratoria, refinancing borrowers into more affordable loans, an explicit modification of contractual terms (like principal reduction, term extension, or adjustment of the mortgage rate), a transfer of a loan to a specialized servicer who among other things might engage in a workout with the borrower or a wait-and-see approach (Cutts and William, 2008).[43]

While data limitations prevent us from analyzing the tools servicers might be using, recent reports by government agencies that have collected this data provide evidence on both the usage of servicing tools and their efficiency depending on the securitization status of a loan. In particular, the recent OCC and OTS Mortgage Metrics Reports (2009a, 2009b) analyze the data on actual number and type of renegotiations performed by servicers and report substantial differences in renegotiation rates of portfolio and securitized loans, with more renegotiations for portfolio loans.[44] There is also evidence in these reports that the rate of success on performed renegotiation varies with securitization status. For example OCC and OTS Mortgage Metrics Reports (2009b) point out that the re-default rate for renegotiated loans serviced by third parties was significantly higher than the re-default rate for loans held in the servicers' own portfolios (for example, 70% higher after six months).

Contrary to the evidence from OCC and OTS, a study by Adelino, Gerardi, and Willen (2009) argues that loan modifications during this period were quite rare. Moreover, they maintain that these modifications not only occurred at roughly the same rate for both portfolio and securitized loans, but they also had similar effectiveness associated with them. It is important to note that these findings, even if they were to be true, are potentially consistent with our results. In particular, our results could be explained by a host of other tools besides explicit loan modifications that servicers may choose to renegotiate with the borrowers. Furthermore, not only could these other tools be different for the securitized and portfolio lenders, but their intensity or effectiveness may as well vary depending on the securitization status of the loan. In other words, Adelino, Gerardi, and Willen (2009) are simply looking at a subset of tools that are employed by the servicers and their results on modifications are not in anyways at odds with our current findings.

Adelino, Gerardi, and Willen (2009) further argue that in certain sub-samples of the data there are no differences between the cure rates of delinquent securitized and portfolio loans, and use this evidence to claim that there are no differences in the renegotiation rates between securitized and portfolio loans. This line of argument has two shortcomings. First, it is important to emphasize that differences in observed cure rates are not necessary for the presence of differences in renegotiation rates. As discussed in Section 5.1, cure rate treats both foreclosed loans and loans that have not yet cured in a certain time frame as "not cured". For instance, if a servicer were to foreclose a securitized loan and renegotiate a portfolio loan, then both these loans would be treated as "not cured" if the renegotiated loan had not fully cured in a given time frame, even though the portfolio loan in this example was renegotiated. In other words, observing no difference in cure rates does not automatically imply that there are no differences in renegotiation rates. On the other hand differences in foreclosure rates documented in this paper inform us that there was something different done for securitized loans as compared to bank-held loans.

Moreover, it is important to note that while there are sub-samples of the data where there are no differences in cure rates between securitized and bank-held loans, the distribution of the data that drives the bulk of our foreclosure results also has a significant difference in cure rates.[45] In other words, there is almost a one- to-one correspondence between the sample where we observe lower foreclosure rates for bank-held loans and where we observe higher cure rates for these loans. The sample where Adelino, Gerardi, and Willen (2009) do not find a difference in cure rates (lower end of credit quality distribution) is also one with no differences in foreclosure rates (see Table 8)—suggesting that there was no different action for the portfolio and securitized loans for this subsample.

While some of the findings of our paper and Adelino, Gerardi, and Willen (2009) can be reconciled as discussed above, a few comments about their analysis are in order. First, we would like to emphasize that the LPS database, utilized both in our paper as well as the paper by Adelino, Gerardi, and Willen (2009), *does not* include a direct measure of whether a mortgage is modified or not. To back this information out from the data, Adelino, Gerardi,

[43] For more details on some of these tools, see Cutts and William (2008).

[44] For example, during 2009 Q1, bank-held loans were renegotiated to a much larger degree relative to securitized loans. In this quarter, explicit modification occurred in 57,733 bank-held loans and 102,079 non-agency securitized loans. Assuming these data have a similar ratio of delinquent non-agency securitized loans for every delinquent portfolio loan as the entire LPS data as of April 2009 (roughly three delinquent securitized loans per delinquent bank-held loan), these numbers suggest that bank-held loans were renegotiated at least 50% more relative to securitized loans. These reports also show that principal write-downs and other aggressive renegotiations were done far more often on bank-held loans as compared to securitized loans. A simple statistic reveals how stark the differences are: during 2009 Q1, portfolio lenders wrote down principal in over 3,300 mortgages; servicers of securitized loans did this in only three mortgages.

[45] The magnitudes of the differences in cure rates are quite comparable to the differences in foreclosure rates for this sample.

and Willen (2009) employ a heuristic that is tested on securitized loans only. This heuristic, however, as reported in their paper, has large Type I and Type II errors. In other words, not only is their heuristic not able to detect actual loan modifications, but it also detects loan modifications with a high error when actually no modification took place. Importantly, these errors vary with observable characteristics in the data such as whether a loan is an adjustable rate mortgage (ARM) or a fixed rate mortgage (FRM) and may also vary across securitization status of the loan. These errors could potentially bias the reported estimates from regressions that examine how the rate of modifications (their y variable) varies with different covariates, since the measurement error in their y variable is correlated with these covariates (i.e., the measurement error is non-classical). Understanding the nature of this bias, and in particular how it varies across securitized and bank-held loans, is important in order to compare the Adelino, Gerardi, and Willen (2009) results with OCC and OTS study (see also Agarwal et al. (2010) that use direct data from lenders and find higher modification rates for bank-held loans relative to securitized loans).

Second, given the high incidence of errors of their heuristic, it is unclear how to interpret their results on the efficiency of the loan modification. A large portion of the loan modifications identified by their heuristic do not actually occur, and those that actually occur are not identified. Consequently, comparing the efficiency of modifications imputed by their heuristic, without a regard to whether the modification actually occurred makes it hard to interpret their findings. It is important to address both these issues since, as Mayer (2010) notes, "This conclusion [servicers of securitized mortgages foreclosed on properties at a much higher rate than portfolio lenders did] is supported by independent studies showing that modifications come in many forms and are not nearly as rare as described in Adelino et al. As well, portfolio lenders seem to be more successful with the modifications they undertake."

7.2. Mechanism driving the results

Our empirical analysis is largely agnostic about the exact channel through which the effects we document take place. There are several channels through which securitization can affect the decision of a servicer to foreclose. Some of these include potential agency conflicts brought about the separation of ownership and control, legal constraints and/or uncertainty in servicer contracts, coordination problems amongst multiple investors and different accounting/regulatory treatment of securitized and portfolio loans. We believe that nailing down the exact channel is an important area for future research.

It is important to note that our paper makes no welfare claims. Our estimate of foreclosure bias in securitized loan servicing is measured relative to foreclosures by banks. As banks are likely to fully internalize the costs and benefits of the decision to foreclose a delinquent loan, it is natural to interpret our results as suggesting that securitization has imposed renegotiation frictions that have resulted in a higher foreclosure rate than would be desired by investors. It might, however, be the case that banks face soft-budget constraints due to regulation, accounting, or political pressure relative to servicers of securitized loans, and therefore the differences in foreclosure rates we report would not necessarily indicate inefficient renegotiation of securitized loans. It is difficult to fully investigate this alternative hypothesis as it would require knowledge of expected recovery for foreclosed loans as well as expected repayment in case of renegotiation at the time of the servicing decision.

Our empirical results, however, might shed some light on this issue. In particular, we find stronger evidence of foreclosure bias among the loans of better initial quality. This finding is consistent with economic arguments that suggest that renegotiation should be undertaken more aggressively for these borrowers.[46] If there are substantial barriers to renegotiation of securitized loans, these arguments predict a larger difference between foreclosure rate of bank-held and securitized loans. To the extent that there are no differential accounting/capital requirements or capacity constraints for high and low quality loans, these results suggest that foreclosure differences are not entirely driven by these institutional constraints. In addition, we find that delinquent portfolio loans are not only foreclosed at a lower rate, but also resume making payments at a higher rate relative to comparable securitized loans. This might suggest that some investors could have benefited if their loans were serviced similarly to portfolio loans.

7.3. Policy implications

While our findings are agnostic on efficiency questions, they suggest that there may be a role for government intervention for at least two reasons. First, it is well known that ex post debt renegotiation can create perverse ex ante incentive effects. As a result, a commitment not to renegotiate, for example through dispersion of claims, can be an outcome of an optimal contract design since it deters strategic default. However, there are compelling arguments that in times of significant adverse macro shocks, debt forgiveness and loan renegotiation can create value for both borrowers and investors (Bolton and Rosenthal, 2002; Kroszner, 2003; Piskorski and Tchistyi, 2008).[47] It is possible that the magnitude of the current crisis was not fully anticipated by investors and borrowers and so they did not provision for this contingency. Consequently, to the extent that the investors are not able to change the nature of servicing contracts due to coordination problems or other institutional frictions, government initiatives facilitating renegotiation of secur-

[46] The economic intuition for why lenders might be more willing to renegotiate better-quality loans follows from the idea that potential benefits of renegotiation might be larger for borrowers of better initial credit quality due to their lower expected probability of re-default. In addition, theories such as Piskorski and Tchistyi (2008) suggest that lenders would be less willing to renegotiate with borrowers of worse credit quality due to moral-hazard concerns stemming from potential adverse impact on incentives of other borrowers to pay.

[47] In this context one can think about renegotiation as part of an implementation of a state-contingent optimal contract.

itized loans could benefit both borrowers and investors (Hart and Zingales, 2008).

Second, foreclosures exert significant negative externalities, such as negative neighborhood effects, and the reduction in collateral prices can further aggravate financial distress (Campbell, Giglio, and Pathak, 2009). As a result, the foreclosure bias in decisions of servicers of securitized loans may have exacerbated these social costs of the crisis, warranting intervention.

In the end, relative merits of any policy intervention should depend on a careful evaluation of its social benefits as well as its potential costs. This task is complicated by the need to take into account the impact of policy intervention on incentives of current borrowers to repay as well as on the behavior of borrowers and lenders in the future. Our paper contributes to this policy debate by demonstrating that securitization induces a foreclosure bias in decisions of loan servicers and by quantifying the magnitudes related to this bias.

Appendix A. Description of variables

This table describes main variables used in the analysis.

Variable	Description
Portfolio	Investor status at the time of delinquency. Portfolio=1 if status is held on portfolio. Portfolio=0 if status is privately securitized.
FICO	Credit score at origination.
LTV	Loan-to-value ratio at origination.
Origination amount	Origination amount in thousands of dollars.
Original interest rate	Monthly interest rate at origination in percent.
FIX	A variable whose value is one if mortgage is fixed-rate mortgage; otherwise value is zero.
15-Year term	A variable whose value is one if original term length is 15 years; otherwise value is zero.
20-Year term	A variable whose value is one if original term length is 20 years; otherwise value is zero.
Insurance	A variable whose value is one if borrower has mortgage insurance; otherwise value is zero. The variable is defined to be one if the mortgage insurance company's id (MICompanyId) takes either of the values: "GE", "MGIC", "PMI", "UGIC", "RMIC", "Radian", "Integon", "Triad", "CMG" or if id indicated there is mortgage insurance but by some Unknown/Other Company.
No insurance	A variable whose value is one if borrower does not have mortgage insurance; otherwise value is zero.
Age at delinquency	Number of months since origination when loan first becomes 60+ days delinquent.
HPI change from origination to delinquency	Percentage change in the Office of Federal Housing Enterprise Oversight House Price Index (HPI) from origination to time of 60+ days delinquency at MSA level.
Delinquency	A variable whose value is one if the borrower becomes 60+ days delinquent; otherwise value is zero.
Default/Foreclosure	A variable whose value is one if the borrower enters foreclosure complete, foreclosure post-sale or REO; otherwise value is zero.
Cure	A variable whose value is one if a 60+ delinquent loan's payment history improves in delinquency status at the end of a prespecified window; otherwise value is zero.

Table A1
Robustness of high-quality loans result.

This table reports the estimated hazard ratios from Cox-proportional hazard models of the transition from delinquency to foreclosure/transfer and of the transition from delinquency to cure/transfer. Estimates on discrete variables represent the effect of moving from zero to one. Portfolio is a dummy that indicates that the loan was bank-held at the time of first 60+ days delinquency. All loans in the sample are originated between 2005 and 2006. Robust *t*-statistics are reported. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

	FICO > 680 *and full documentation*		*FICO* > 680, *full doc*, *and* "*conforming*"		*FICO* > 680, *full doc*, *and non-conforming*	
Dependent variable:	Foreclosure	Transfer	Foreclosure	Transfer	Foreclosure	Transfer
Mean securitized	0.25	0.01	0.24	0.02	0.26	0.01
Portfolio (d)	0.662***	4.797***	0.679***	5.537***	0.599***	2.38***
	(−8.28)	(12.17)	(−6.97)	(11.89)	(− 4.30)	(2.63)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	15,203	15,203	11,674	11,674	3,101	3,101
	FICO > 680 *and full documentation*		*FICO* > 680, *full doc*, *and* "*conforming*"		*FICO* > 680, *full doc*, *and non-conforming*	
Dependent variable:	Cure	Transfer	Cure	Transfer	Cure	Transfer
Mean securitized	0.39	0.01	0.38	0.02	0.40	0.01
Portfolio (d)	1.205***	8.261***	1.178***	9.286***	1.23***	3.708***
	(6.69)	(14.31)	(5.12)	(13.46)	(3.33)	(3.39)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	15,203	15,203	11,674	11,674	3,101	3,101

One could wonder why some high-quality borrowers (FICO > 680 and full documentation loans) end up in the non-agency market. A potential concern could be that these borrowers are risky along some unobservable dimensions, which prevents them from qualifying for GSE loans. If lending institutions employ this information in deciding which loans to securitize and which loans to retain on the portfolio, this could partly explain the much lower foreclosure rates among the sample of high-quality loans. To alleviate this concern, we split the sample of high-quality loans into two subsamples: loans with balances that conform to GSE guidelines ("potentially conforming" loans) and loans with balances above the GSE conforming limit ("potentially non-conforming" loans). Note that we call these loans "potentially conforming" since we do not know if these loans are truly "conforming" on dimensions other than loan amount (e.g., debt to income of the borrower).

The idea behind the partition is that we expect that any selection on unobservables at the time of origination or securitization to be less of a concern for "potentially non-conforming" high-quality loans since there is an observable reason for why these loans got excluded from the GSE market. Table A1 displays the results of cure and foreclosure hazard specification for the two high-quality subsamples. As we observe, the delinquent "potentially conforming" high-quality loans that are held by the banks resume making payments at a 17% higher rate relative to the comparable "potentially conforming" loans that are securitized. Interestingly, the delinquent "potentially non-conforming" high-quality loans that are held by the banks resume making payments at a 23% higher rate relative to comparable "potentially non-conforming" securitized loans. A similar pattern emerges for foreclosure differences between bank-held and securitized loans (difference is larger for "potentially non-conforming" sample). In other words the difference in the cure and foreclosure rates between delinquent bank-held loans and comparable securitized loans is larger in the sample where the selection concerns are less likely. This test suggests that it is unlikely that the higher rate at which delinquent bank-held loans resume making payments (or the lower rate at which they foreclose) relative to securitized loans is entirely driven by differences in unobservable loan characteristics.

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