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Working Paper 2009-17a
April 2010

WORKING PAPER SERIES

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Abstract: Securitization does not explain the reluctance among lenders to renegotiate home mortgages. We focus on seriously delinquent borrowers from 2005 through the third quarter of 2008 and show that servicers renegotiate similarly small fractions of securitized and portfolio loans. The results are robust to several different definitions of renegotiation and hold in subsamples where unobserved heterogeneity is likely to be small. We argue that information issues endemic to home mortgages where lenders negotiate with large numbers of borrowers lead to barriers to renegotiation fundamentally different from those present with other types of debt.

JEL classification: D11, D12, G21

Key words: mortgage, foreclosure, modification, renegotiation

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The authors thank John Campbell, Chris Foote, Scott Frame, Anil Kashyap, Chris Mayer, Eric Rosengren, Julio Rotemberg, Amit Seru and seminar audiences at the NBER Capital Markets and the Economy Meeting, the NBER Monetary Economics Meeting, Freddie Mac, Harvard Business School, University of Connecticut, LSE, Brandeis University, Bentley University, University of California, Berkeley, Standford University, University of Virginia, University of Chicago (Booth School of Business), Penn State University, Chicago Fed, AREUEA winter meetings, the SAET meetings in Ischia, ESSET-Gerzensee, the FDIC/FHFA Symposium on credit risk, and the MIT Finance Lunch for thoughtful comments, Kyle Carlson and Suzanne Lorant for editorial help, and the Studienzentrum Gerzensee, where the authors completed this paper, for their generous hospitality. The views expressed here are the authors' and not necessarily those of the Federal Reserve Banks of Atlanta or Boston or the Federal Reserve System. Any remaining errors are the authors' responsibility.

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Many commentators have attributed the severity of the foreclosure crisis in the United States in the 2007–2009 period to the unwillingness of lenders to renegotiate mortgages, and, as a consequence, have placed renegotiation at the heart of the policy debate. Several major policy actions to date have involved encouraging lenders, in one way or another, to renegotiate loan terms in order to reduce borrower debt loads. The Obama Adminstration's Making Home Affordable Plan, announced in February of 2009, provided financial incentives to servicers to renegotiate loans on the condition that the lenders reduce the interest rate for a significant period of time.[1]

The appeal of renegotiation to policy makers is simple to understand. Foreclosure can be a costly outcome post delinquency for both the borrower and the lender. If a lender makes a concession to a borrower by renegotiating a loan this can be beneficial to both parties, as long as the value to the lender of the new contract exceeds the value of the property in foreclosure. According to proponents, renegotiation of home mortgages is a type of public policy holy grail, in that it helps both borrowers and lenders at little or no cost to the government (some examples of this view include Congressional Oversight Panel (2009) and Geanakoplos and Koniak (2008)). However, if this logic is correct, lenders should find renegotiation attractive, even in the absence of government prodding. Yet, we observe very little renegotiation—less than two percent of all delinquent loans receive payment reducing modifications in the first year following delinquency, whereas approximately 30 percent of delinquent loans are in foreclosure one year after missing two mortgage payments.

The leading explanation attributes the reluctance of lenders to renegotiate to the process of securitization.

> Unfortunately, this win-win solution is not possible today. Your mortgage has been sold and repackaged in an asset-backed security pool and sold in tranches with different priorities. There is disagreement on who has the right to renego-

[1]See "$275 Billion Plan Seeks To Address Crisis In Housing," *New York Times*, Feb. 18, 2009.

> tiate and renegotiation might require the agreement of at least 60% of the debt holders, who are spread throughout the globe. This is not going to happen. Furthermore, unlike your local bank, distant debt holders cannot tell whether you are a good borrower who has been unlucky or somebody just trying to take advantage of the lender. In doubt, they do not want to cut the debt for fear that even the homeowners who can easily afford their mortgage will ask for debt forgiveness. (Zingales 2008)

With securitization the claims on the cash flows from large pools of mortgages become dispersed. The existing theory on dispersed debt ownership suggests that this makes renegotiation following default harder (Bolton and Scharfstein 1996 and Asquith, Gertner, and Scharfstein 1994). For loans that are held on banks' balance sheets this problem should not arise, leading many to conclude that the existence of securitization trusts is one of the key obstacles to mortgage renegotiation.

More precise institutional evidence appears to confirm the role of securitization in impeding renegotiation. As mentioned in more detail below, PSAs do sometimes place global limits on the number of modifications a servicer can perform for a particular pool of mortgages. In addition, the rules by which servicers are reimbursed for expenses may provide a perverse incentive to foreclose rather than modify. Furthermore, because servicers do not internalize the losses on a securitized loan, they may not behave optimally. Another issue is the possibility that those investors whose claims are adversely affected by modification will take legal action. Finally, historically, SEC rules have stated that contacting a borrower who is fewer than 60-days delinquent constitutes an ongoing relationship with the borrower and jeopardizes the off-balance sheet status of the loan.

But some market observers express doubts about the renegotiation-limiting role of securitization. One important difference between dispersed corporate debt and mortgage-backed securities is that delinquent borrowers have to negotiate with multiple parties in the case of

corporate debt, whereas securitization trusts have an agent (the servicer) who acts on their behalf. Hunt (2009) conducted an exhaustive review of a sample of PSAs and concluded, "it appears that large-scale modification programs may be undertaken without violating the plain terms of PSAs in most cases." Although some servicers have expressed concern about lawsuits, of the more than 800 lawsuits filed by investors in subprime mortgages through the end of 2008, not one involved the right of a servicer to modify a loan.[2] Even the Congressional Oversight Panel (2009), which did view securitization as a problem in general, conceded, "The specific dynamics of servicer incentives are not well understood." Finally, the SEC ruled in 2008 that if default was "reasonably foreseeable," then contact with a borrower prior to 60-day delinquency would not affect the accounting status of the loan.

In this paper, we explore the role of securitization in preventing the renegotiation of home mortgages using a dataset from Lender Processing Services (LPS), a large, detailed sample of residential mortgages.[3] Measuring renegotiation in the LPS data is a challenge because there is no field in the data that identifies whether or not a servicer has changed the terms of, or "modified," the loan. We overcome this difficulty by developing an algorithm to identify modifications and we validate this algorithm on an unrelated dataset that includes a modification flag.

The LPS dataset includes loans that are serviced for private securitization trusts that are not sponsored by any of the government sponsored enterprises (GSEs), so-called "private-label" loans, which are subject to all of the contract frictions described above. It also includes loans owned by servicers, so-called "portfolio" loans, which are immune to such problems. We compare renegotiation rates of seriously delinquent loans (i.e. that missed two or more payments) in the two groups, controlling for observable characteristics of the loans. Our LPS data run from the first quarter of 2005 through the third quarter of 2008, and thus has the benefit of covering much of the housing and foreclosure crisis period,

[2]Navigant report, Congressional Oversight Panel (2009).
[3]Until 2008, the dataset was known as McDash.

while excluding the period of dramatic government intervention in financial markets that occurred beginning in September of 2008 with the conservatorship of the housing GSEs and the failure of Lehman Brothers that was followed by the large-scale capital injections in the banking sector from the Troubled Asset Relief Program (TARP).

Our empirical analysis provides strong evidence against the role of securitization in preventing renegotiation. For our narrowest definition of renegotiation, payment-reducing modifications[4], we find that the differences in the likelihood of renegotiation in the 12 months subsequent to the first 60-day delinquency between the two types of loans is neither economically nor statistically significant. When we consider a broader definition that includes any modification, the data even more strongly reject the role of securitization in preventing renegotiation. It is worth noting that the pooling and servicing agreements (PSAs), which govern the conduct of servicers when loans are securitized, often place limits on the total number of modifications a servicer can perform, so we would expect the metric of "all modifications" to be most affected by securitization. We also find no differences between private-label and portfolio loans when we include in our definition of renegotiation the transactions whereby lenders allow borrowers to extinguish their liabilities by repaying less than the outstanding balance of the loan.[5]

Our results are highly robust. One potential problem with the data is that there is unobserved heterogeneity in the characteristics of portfolio and private-label loans. To address this, we exploit subsets of the LPS data, in which servicers provide an exceptional amount of information about borrowers. When we exclude observations where the servicer failed to report whether the borrower fully documented income at origination, or what the debt-to-income ratio was at origination, our results even more strongly reject the role of securitization in inhibiting renegotiation. When we focus only on loans for which the

[4]These include any changes to the contract terms that reduce the borrower's monthly payment.

[5]These transactions are known as short payoffs, short sales, or deeds-in-lieu of foreclosure, depending on the structure. We measure this component of renegotiation by counting the number of seriously delinquent loans that the servicer reports as "paid off."

borrower fully documented income, we obtain results that are broadly consistent or, in some cases, stronger than the results for the full sample. Finally, we limit our sample to only subprime loans (as defined in LPS). These loans comprise only 7 percent of the LPS data, but they account for more than 40 percent of all serious delinquencies and almost 50 percent of the modifications that we identify in the data. The results that we obtain for the subprime sample are also consistent with our results for the full sample.

Another potential issue with our focus on 60-day delinquent loans is that portfolio lenders can contact borrowers at any time, whereas some securitization agreements forbid lenders from contacting borrowers until they are at least 60 days delinquent (two missed payments). When we shift our focus to 30-day delinquent borrowers (one missed payment), our results continue to show no meaningful difference between renegotiation of private-label and portfolio loans.

One other possibility is that our algorithm for identifying modifications is somehow missing a class of loss-mitigation actions taken by servicers. Forbearance agreements and repayment plans, for example, would not necessarily show up in our data. However, neither of these actions constitutes renegotiation in any classic sense, because the lender still expects the borrower to repay in full, including interest on any delayed payment. In addition, unlike modifications, PSAs never place any limits on the use of forbearance agreements or repayment plans, so, *a priori*, we would have less reason to expect a difference in their use across private-label and portfolio loans. Finally, most successful forbearance agreements conclude with a modification to allow the borrower to repay the arrears incurred in forbearance. With all of that said, we test the proposition that servicers engage in other loss mitigation actions by looking at the "cure rate." This is the percentage of loans that transition to current status in the year subsequent to becoming 60-days delinquent. We find that in the full sample, private-label loans are unconditionally less likely to cure, but that when we correct for observable characteristics, we estimate a cure rate of around 55 percent for

the typical portfolio loan and a cure rate about two percentage points *more* for an otherwise equivalent private-label loan. This difference is similar in other subsamples of the data. For example, in the subsample of subprime loans, the subsample with information about documentation and debt-to-income (DTI) status, and the sample of fully documented loans, we again find that private-label loans are significantly more likely to cure (both unconditionally and conditioning on observable characteristics).

The policy debate has focused exclusively on the manner in which securitization impedes renegotiation and implicitly assumes that portfolio lenders face no institutional impediments, but this is not realistic. Portfolio lenders complain about accounting rules, including the need to identify modifications, even when the borrowers are current on their mortgage payments prior to the modification, as "troubled debt restructurings", which leads to reduction of the amount of Tier II capital and increased scrutiny from investors and cumbersome accounting requirements. The shortage of qualified staff, an oft-heard complaint from borrowers seeking renegotiation, affects servicers of portfolio loans and private label loans equally. Finally, the interests of the managers of a loan portfolio are not necessarily any more likely to be aligned with their investors than are the interests of the trustees of a mortgage pool; many have attributed the catastrophic failures of financial institutions like AIG in 2008 to misaligned incentives of managers and shareholders.

Our results are consistent with the hypothesis that securitization does impede renegotiation but that a different set of impediments leads to similar problems with portfolio loans and generates our finding that there is no difference. However, the small differences would represent a remarkable coincidence.[6] More importantly, the low overall levels of renegotiation mean that even if contract frictions cut the overall number of concessionary modifications in half, 94 percent of seriously delinquent borrowers would still fail to receive

[6]Yet another possible explanation is that equal treatment provisions in PSAs force servicers to modify similar numbers of portfolio and private-label loans and that servicers are reluctant to modify portfolio loans in spite of the fact that they internalize the benefits because they must then modify private label loans for which they do not.

a concessionary modification. So the puzzle remains why so few loans are renegotiated.

To our knowledge, this paper is the first to estimate directly the likelihood of renegotiation of private-label and portfolio-held mortgages. Piskorski, Seru, and Vig (2009) address the question of the effects of securitization on renegotiation, but rather than directly identifying renegotiation, they argue that observed differences in foreclosure rates *imply* differences in renegotiation activity. Our results contradict this interpretation. For renegotiation to explain the differences in foreclosure rates, there would have to be large errors in our algorithm for identifying renegotiation, and those errors would have to be significantly biased toward portfolio loans, a possibility that is particularly problematic given that the renegotiations we focus on are precisely the type that PSAs supposedly prevent. In addition, most of the loan histories in the LPS sample are right-censored, meaning that the borrowers have neither lost their homes nor paid off their mortgages when the data end, making it impossible to equate the absence of a foreclosure with successful renegotiation. By contrast, a "cure" is a necessary condition for renegotiation, and thus the differences we report in cure rates across portfolio and private-label loans that are neither large nor of consistent sign contradict the claim that securitization is a major obstacle to renegotiation.

If contract frictions induced by securitization are not a significant problem, then what is the explanation for why lenders do not renegotiate with delinquent borrowers more often? Researchers have puzzled over this since well before the advent of non-agency securitization and the the subprime crisis. Wang, Young, and Zhou (2001) write that:

> ...Given the literature, a bank should be more likely to work out a solution with the mortgage borrower than to foreclose on the property in the case of a mortgage default. However, Riddiough and Wyatt (1994b) observe that in dealing with mortgage defaults, many lenders take a hard-line approach and are reluctant to grant workout concessions." (pp. 960-1)

Wang, Young, and Zhou (2001) and Riddiough and Wyatt (1994b) both argue that infor-

mation issues make foreclosure a better option from the lender's perspective and we view this as a plausible explanation.

The first way in which information issues affect the decision to modify is through the uncertainty that the lender faces about the outcomes of each loan, namely the possibility that borrowers will "self-cure" or that borrowers will re-default after receiving a modification. As we mentioned above, about 55 percent of seriously delinquent borrowers in our sample "cure" without receiving a modification; if taken at face value, this means that, in expectation, 55 percent of the money spent on a given modification is wasted. Regarding the possibility of redefault, our results show that a large fraction of borrowers who receive modifications end up back in serious delinquency within six months. For them, the lender has simply postponed foreclosure; in a world with rapidly falling house prices, the lender will now recover even less in foreclosure. Documentary evidence shows that lenders explicitly took both self-cure risk and redefault risk into account when making their renegotiation decisions.

The second problem is that borrowers have private information about their willingness and ability to repay the loan and the value of the collateral. In making a renegotiation decision, lenders must weigh the benefits of preventing a particular foreclosure with the cost of providing that same concession to all observationally equivalent borrowers. Suppose the lender knows that some of the seriously delinquent borrowers need a 30% reduction in principal to make repayment attractive but others only need a 10% reduction. The lender may only offer 10% because the cost of giving 30% reductions to borrowers who only need 10% offsets the benefit of fewer foreclosures. Furthermore, the lender may worry that more generous renegotiation will induce borrowers currently making payments to join the ranks of the seriously delinquent.

In sum, our research suggests that in understanding distressed debt renegotiation, the case of residential mortgages and other debts need to be viewed differently. As Wang, Young, and Zhou (2001) write:

> ...previous models [of debt renegotiation] addressed the coordination and agency issues when a distressed firm faces multiple debt holders. Our model addresses the problems when one debt holder faces many potential defaulters. In this regard, our model is most relevant to an environment where a debt holder could face a large group of potential defaulters, as in a residential property market with falling prices... (p. 962)

Our results provide some insight into the perceived failure of the the U.S. governments Home Affordable Modification Program (HAMP) to stem the tide of foreclosure in 2009 and 2010.[7] HAMP was based on the premise that the barriers to renegotiation were largely institutional and not economic – i.e. renegotiation was in the interests of the owners of the loans but institutional barriers including securitization were preventing them from occuring. To address these institutional issues, HAMP diverted significant resources toward the servicers as opposed to the investors.[8] Subsequent updates to HAMP implicitly recognized this by providing increased financial incentives to investors to renegotiate loans.[9]

I. Related Literature and Existing Evidence

Our research draws on existing literature in several different fields. First, there has been substantial interest in the question of renegotiation of home mortgages among real estate economists, both prior to, and as a result of the current crisis. Riddiough and Wyatt (1994a), Riddiough and Wyatt (1994b), Ambrose and Capone (1996), and Wang, Young, and Zhou (2001) addressed informational issues that inhibit efficient renegotiation. We draw extensively on this research in Section IV. Springer and Waller (1993), in an early example, explores patterns in the use of forbearance as a loss mitigation tool. Capone

[7] "U.S. Loan Effort Is Seen as Adding to Housing Woes," *New York Times*, Jan 1, 2010.
[8] http://www.ustreas.gov/initiatives/eesa/homeowner-affordability-plan/FactSheet.pdf
[9] "Mortgage Plan Remodeled Again ," *Wall Street Journal*, March 29, 2010.

(1996) and Cutts and Green (2005) both discuss the institutional issues, with the former study providing historical evidence and focusing on issues in the mid-1990s, and the latter study discussing innovations since then.

The issue of dispersed ownership and debt renegotiation has received a fair amount of attention in the corporate finance literature. Gan and Mayer (2006), for example, focus on commercial mortgages, and find that servicers delay liquidation of delinquent mortgages when they are also the holders of the equity tranche of the deal. This suggests that participating in the losses due to liquidation may alleviate some of the agency problems posed by the separation of ownership and servicing pointed out before. However, it may also lead to conflicts of interest between holders of different tranches. In their setting, Gan and Mayer (2006) find that the servicers' behavior is consistent with asset substitution, as servicers seek to benefit from the option-like payoff of their position. Also, the contractual restrictions imposed by PSAs (discussed above) and standard economic arguments on the effects of dispersed ownership of debt (as in Bolton and Scharfstein 1996 and Asquith, Gertner, and Scharfstein 1994) further reduce the incentives of servicers to modify mortgages.

The start of the subprime crisis in 2007 led to a resurgence of interest in the topic of modifications among economists and aroused new interest from other fields, in particular, the field of law. Legal researchers, White (2008) and White (2009), for example, have addressed empirical questions about the frequency and characteristics of loan modifications, closely related to the analysis in this paper. In addition, they have also looked at issues related to the restrictions imposed by contracts (Hunt 2009 and Gelpern and Levitin 2009) and the interactions among foreclosure, renegotiation, and personal bankruptcy (Levitin 2009a and Levitin 2009b). In real estate, Quercia, Ding, and Ratcliffe (2009), Cutts and Merrill (2008), Stegman, Quercia, Ratcliffe, Ding, Davis, Li, Ernst, Aurand, and Van Zandt (2007), and Mason (2007), all discuss issues with contemporary loss mitigation approaches.

More broadly, previous research explored the factors that lead delinquent mortgages to

transition to foreclosure or to cure, one of which is renegotiation. Pre-crisis papers include Ambrose and Capone (1998), Ambrose, Buttimer Jr., and Capone (1997), Ambrose and Capone (2000), Lauria, Baxter, and Bordelon (2004), Danis and Pennington-Cross (2005), Pennington-Cross (2009), and Pennington-Cross and Ho (2006). Mulherin and Muller (1987) discusses conflicts between mortgage insurers and owners that may lead servicers to induce or postpone foreclosure inefficiently. In light of the crisis, Piskorski, Seru, and Vig (2009) and Cordell, Dynan, Lehnert, Liang, and Mauskopf (2008a) have revisited the question.

II. Data

We use a dataset constructed by LPS. This is a loan-level dataset that covers approximately 60 percent of the U.S. mortgage market and contains detailed information on the characteristics of both purchase-money mortgages and mortgages used to refinance existing debt.[10] This dataset is especially useful in the context of this paper, as it includes both securitized mortgages and loans held in portfolio.[11] The LPS data specifically denote whether a mortgage is held in portfolio, or securitized by a non-agency, private institution.[12] If institutional constraints are restricting the modification process for private-label, securitized loans, we would expect to see relatively few modifications among them, as compared to portfolio loans. Unfortunately, our LPS sample does not include direct information regarding loan modifications.[13] However, LPS does provide monthly updates to loan terms, so it is possible

[10]We use a 10 percent random sample of the LPS data when estimating all of our empirical models. The dataset is simply too big to use in its entirety from a computational standpoint. However, we have checked the robustness of our results to using different sample sizes, and we do not find substantial differences.

[11]For a more detailed discussion of the LPS data, we direct the reader to Foote, Gerardi, Goette, and Willen (2009).

[12]The LPS data also denote when a loan is securitized by a GSE (Government Sponsored Enterprise) such as Freddie Mac or Fannie Mae. We eliminate this class of loans, since the GSEs hold all credit risk, and thus are not subject to any modification restrictions.

[13]The Office of Thrift Supervision (OTS), in collaboration with the Office of the Comptroller of Currency (OCC), used data from LPS to analyze the outcomes of recent mortgage modification programs (OCC and OTS Mortgage Metrics Report, Third Quarter 2008). In this report, they had access to supplementary data from servicers that include the identification of loans in the LPS data that had been modified. We have not

to identify loan modifications indirectly (and imperfectly). Table III shows two examples of modifications in the data. In the first example, the servicer cuts the interest rate, capitalizes arrears into the balance of the loan, and extends the term of the loan to 40 years. In the second example, the servicer just capitalizes arrears into the balance of the loan. In both cases the loan is reported as "current" after the modification, whereas before it was 90+ days delinquent.

We denote a loan as being modified if there is a change in its terms that was not stipulated by the initial terms of the contract. Such modifications include interest-rate reductions, principal-balance reductions, and term extensions. We can also identify principal-balance and mortgage-payment *increases* that reflect the addition of arrears into the balance of a loan.[14] We spell out our algorithm for identifying modifications in more detail in Appendix A.

There are two potential mistakes we can make in this exercise. First, we may falsely identify modifications ("false positives") because of measurement error in the data (for example, a mistake in the updated balance or interest rate) or some endogenous behavior on the part of the borrower (for example, a borrower making extra principal payments). Second, we could miss modifications ("false negatives") because our algorithm for finding modifications is incomplete. In order to test our algorithm, we use data from the Columbia files put together by Wells Fargo's CTSLink service. This dataset includes a similar set of variables to those in the LPS dataset (on performace of the loans and characteristics of the borrower at origination) but is limited to private-label loans. These files do include, however, explicit flags for modifications. This allows us to use the same algorithm described in Appendix A and compare the modifications we identify to the "true" modifications.

been able to obtain access to this data.

[14] One of the major types of loan modifications that we are largely unable to identify are interest rate freezes for subprime ARMs, which reset after two or three years. However, the reason that we cannot identify those freezes is because many are not binding; the fully-indexed rate is lower than the initial rate. These modifications will have no major effect on the current terms of the mortgage, so we do not view this as a major drawback.

Results are reported in Table IV. Overall our algorithm performs well, with 17 percent false negatives (that is, we do not identify around 17 percent of the "true" modifications) and around the same percentage of false positives (that is, approximately 17 percent of the modifications we identify are not flagged as modifications on the CTSLink data). By type of modification, our algorithm performs best for principal reductions, term increases, and fixed-rate mortgage reductions, and comparatively worse for ARM rate reductions and for principal increases.

We explore several different definitions of renegotiation in the data. Our first definition of "renegotiation" is concessionary modifications that serve to reduce a borrower's monthly payment. These may be reductions in the principal balance or interest rate, extensions of the term, or combinations of all three. This definition of renegotiation is a key focus of our analysis because there is a consensus among many market observers that concessionary modifications are the most, or possibly the only, effective way of preventing foreclosures. As the Congressional Oversight Panel (COP) for the Troubled Asset Recovery Program (TARP) has written, "Any foreclosure mitigation plan must be based on a method of modifying or refinancing distressed mortgages into affordable ones. Clear and sustainable affordability targets achieved through interest rate reductions, principal write-downs, and/or term extensions should be a central component of foreclosure mitigation."[15]

Because the pooling and servicing agreements (PSAs), which govern the conduct of servicers when loans are securitized, often place limits on the number of modifications a servicer can perform, we broaden our definition of renegotiation to include any modification, regardless of whether it lowers the borrower's payment. Modifications are often thought to always involve concessions to the borrower, but many, and in some subsets most, modifications involve the capitalization of arrears into the balance of the loan, and thus lead to increased

[15]See the Congressional Oversight Panel (2009). This view is widely held and is the main focus of the Administration's Making Home Affordable foreclosure prevention plan was to encourage servicers to modify loans to reduce monthly payments to 31 percent of income.

payments.

Finally, we attempt to include in our definition of renegotiation the transactions whereby lenders allow borrowers to extinguish their liabilities by repaying less than the outstanding balance of the loan.

We focus on loans originated after January of 2005, in line with most studies that use the LPS dataset.[16] We restrict attention to loans that have missed two or more payments and track the performance of those loans (and whether they were modified) until the end of the third quarter of 2008. Given that the purpose of this paper is to test for the effect of contract frictions and dispersed debt ownership on the renegotiation of mortgages, we choose to exclude the period following September of 2008. While the financial crisis of 2008-2009 and the bankruptcy of Lehman Brothers may be an interesting setting to study the behavior of financial institutions, the post-September 2008 period is also a time when political intervention in financial markets is likely to have significantly changed the incentives of banks and servicers. Our interest in this paper is to test for the effect of the incentives put in place by securitization and not the interaction of those incentives with political intervention and financial distress. The advantage from a research perspective of the period we consider is that it includes an early time period where house prices are increasing and a later time period (from December of 2006) where national house prices declined significantly.

We exclude loans that enter the database more than three months after being originated, as well as those with missing FICO, origination amount, interest rate information, and LTV, as well as those with missing information about whether they are subprime or prime and whether they are purchase or refinance loans.

[16]LPS added a few large national servicers in January of 2005, which created an attrition bias for the mortgage data prior to 2005. When a servicer enters the LPS dataset it only provides information on active loans. Thus, if one uses data prior to 2005 from these services, it will include only the mortgages that survived until 2005.

A. Summary Statistics from the Data

Table V reports the number of modifications performed each quarter from the first quarter of 2007 through the third quarter of 2008, disaggregated by the type of modification. Each of the numbers is a multiple of 10 because we used a 10 percent random sample and scaled up the numbers we found. The first column of Table V simply reports the total number of loan modifications made. Not surprisingly, modifications have become more common as the housing market has weakened. There appear to be more than 6 times as many modifications performed in the third quarter of 2008 as in the first quarter of 2007. In addition to the rapid growth in loan modifications, the composition of modifications has changed over time. This can be seen in the remaining columns of Table V, which list the incidence of modifications of different types.[17]

An interesting finding is that most modifications entailed *increases* in the principal balance of a mortgage. Such increases are likely due to the addition of arrears to the outstanding mortgage balance for delinquent borrowers, and these often increase the monthly mortgage payment by a nontrivial amount. While the absolute numbers of balance-increasing modifications are still rising, they are falling as a percentage of total modifications. In the last few quarters, interest-rate reductions, which necessarily involve a decrease in the monthly mortgage payment, have become more frequent, rising to more than 20 percent of all modifications performed in 2008:Q3. Table V provides further information regarding the behavior of monthly mortgage payments for loans that have undergone a modification. There are several notable patterns in this table. First, as of 2008:Q3, modifications that involved payment decreases were more common than those that involved payment increases. Furthermore, the average and median magnitude of payment decreases has recently increased

[17] In many cases a mortgage will experience multiple types of modifications at the same time. For example, we see cases in the data in which the interest rate is decreased and at the same time the term of the loan is extended. Thus, the percentages in Table V are not calculated with respect to the number of loans modified, but rather with respect to the number of modifications performed.

in our sample. From 2007:Q1 to 2008:Q2, the median payment decrease ranged from approximately 10 percent to 14 percent, but then increased to approximately 20 percent in 2008:Q3. Based on the logic from our simple framework above, it is likely that these will have more success than modifications involving increases in the payment and/or balance.

Another interesting observation from Table V is that the incidence of principal reductions is quite low in our data. This is likely due to two factors. First, the LPS dataset underrepresents the subprime mortgage market.[18] A few servicers that focus almost exclusively on subprime mortgages have recently begun modification programs that involve principal reduction.[19] In addition, from a theoretical perspective, principal reduction plans suffer from the severe incomplete-information problem noted earlier. Balance reductions are appealing to both borrowers in danger of default and those who are not. In a recent paper, we argued that to avoid such moral hazard concerns, lenders have a strong incentive to only provide modifications to those borrowers who are most likely to default.[20] Table V contains summary statistics regarding the characteristics at origination of both the sample of modified mortgages and the sample of all loans in the LPS dataset. The patterns that emerge from the table are consistent with such an argument. We discuss this point in more detail below. The sample of modified mortgages is characterized by substantially lower credit scores, higher loan-to-value (ltv) ratios, and slightly higher debt-to-income ratios. The discrepancy in ltv ratios may be underestimated, as the percentage of mortgages with an ltv ratio of exactly 80 percent is significantly higher in the modification sample than in the full sample. This likely implies a larger fraction of highly leveraged loans, for which the second liens are not observable in the data. In addition, the modification sample includes a higher fraction of mortgages with non-traditional amortization schedules, such as interest-only loans, option

[18]The majority of subprime mortgages are securitized by non-agency firms, and the LPS dataset includes approximately 35 percent of mortgages securitized by non-agency corporations.

[19]According to an October 2008 report by Credit Suisse, Ocwen Loan Servicing, LLC and Litton Loan Servicing LP were the only subprime servicers that had performed a nontrivial number of principal reduction modifications. Neither of these servicers contributes to the LPS dataset.

[20]See Foote, Gerardi, and Willen (2008) for a more detailed discussion.

ARMS, hybrid ARMs, and subprime loans.

In Table VI we compare the size of payment decrease and payment increase modifications for loans held in private-label trusts and loans held in portfolio. The size (as a percentage of the original payment) of the median payment decrease due to modification is larger for private-label loans in the first three quarters of 2008. We see a similar pattern for the median payment increase due to modification, while the differences are small for the mean and median payment increase.

III. Differences in Modification Behavior

In this section, we directly address the question of whether the incidence of modification is impeded by the process of securitization. We show evidence that private-label loans and portfolio loans perform similarly, both unconditionally and when observable differences between securitized and portfolio-held loans are controlled for, using both a logit model with a 12-month horizon and a Cox proportional hazard model that takes into account the problem of right censoring in the data.

To make sure that our results are robust to the type of modification performed, we use several different definitions of modification in this section. Our first measure is the number of concessionary modifications, which we define as reductions in the interest rate, reductions in the principal balance, extensions of the term, or combinations of all three. Any or a combination of these serves to reduce a borrower's monthly mortgage payment. We use this as our primary definition of modification in our analysis, as there is a consensus among most market observers that concessionary modifications are the most, or perhaps the only, effective way of preventing foreclosures. Because pooling and servicing agreements, which govern the conduct of servicers when loans are securitized, often limit modifications that change *any* of the contract terms (not just those that result in payment decreases), we

broaden our definition of renegotiation to include any modification, regardless of whether it lowers the borrower's payment. As we discussed above, many, and in some subsets, most modifications, involve the capitalization of arrears into the balance of the loan and thus lead to increased payments. Finally, we attempt to include in our measure of renegotiation the number of times that lenders allow borrowers to extinguish their liabilities by repaying less than the outstanding balance of the loan. These transactions are known as short payoffs, short sales, or deeds-in-lieu of foreclosure, depending on the structure. We do this by counting the number of seriously delinquent loans that the servicer reports as paid off, and including these observations in our definition of modification.

Before turning to the regressions, however, it is instructive to look at the unconditional frequencies of modifications in the data. Panel A of Table VII shows the unconditional frequencies for each type of investor. The first takeaway from the table is the extremely low percentages of modifications for *both* types of mortgages. Less than 2 percent of 60-day delinquent loans received concessionary modifications in the 12 months following the first serious delinquency, and only 8.6 percent of the delinquent loans received *any* type of modification in the same period. These are low levels of modifications compared to the frequency of foreclosure (foreclosure proceedings were initiated on approximately half of the loans in the sample and completed for almost 30 percent of the sample.), and they suggest that even if there are contract frictions that are preventing modifications in securitized trusts, the economic effects are small.

The second takeaway from the table is that the unconditional differences between portfolio loans and private-label loans are very small in absolute terms. There is a difference of approximately 0.8 percentage points and 0.1 percentage points for concessionary modifications and all modifications, respectively. These are very small differences, and they suggest that contract frictions do not play an important role in inhibiting the renegotiation process for loans in securitized trusts. However, these are unconditional statistics, and it

is possible that once observable differences in the characteristics of each type of loan and borrower are accounted for, the results may change.[21] Thus, we now estimate differences in modification behavior while controlling for observable loan and borrower characteristics. These characteristics include the contract interest rate at origination; the credit score of the borrower at origination; the loan-to-value ratio of the mortgage (not including second or third liens) at origination[22]; the logarithm of the nominal dollar amount of loan; an indicator of whether the purpose of the loan was a refinance of a previous mortgage or a home purchase; an indicator of whether the loan was considered to be subprime[23]; a measure of the amount of equity in the property at the time of delinquency, specified as a percentage of the original loan balance and updated by state-level house price indexes calculated by the Federal Housing Finance Agency (FHFA)[24] (and an indicator for a borrower who is in a position of negative equity at the time of delinquency, where the value of the mortgage exceeds the value of the home); and the unemployment rate of the county in which the borrower resides, calculated by the Burea of Labor Services (BLS).[25] We also include, but do not show because of space considerations, a set of cohort dummies that control for the quarter when the mortgage was originated, information regarding the amortization schedule of the mortgage (interest-only or negative amortization, including mortgages commonly referred to as option ARMs), an indicator for whether the size of the mortgage is greater than the GSE conforming loan limits, an indicator for whether the house is a primary residence,

[21] For example, if private-label loans are significantly riskier, and thus better candidates for modification on average, then the unconditional difference will significantly understate things.

[22] Because of the lack of information on second liens in the LPS data and the prevalence of second mortgages as a way to avoid paying mortgage insurance, we include an indicator variable if the ltv ratio is exactly equal to 80 percent. These are the borrowers who likely took out second mortgages, as the requirement for mortgage insurance occurs at ltv ratios above 80 percent. Our experience with other, more complete datasets also confirms that many of these borrowers are likely to have second mortgages that bring the cumulative ltv ratio up to 100 percent.

[23] This definition of subprime comes from the mortgage servicers that contribute to the LPS dataset.

[24] House prices are measured at the state level using the FHFA index. We also tried using Case-Shiller metropolitan area house price indexes and found no substantive differences. We chose to use the FHFA prices for our primary specifications because of their greater sample coverage.

[25] Equity and periods of unemployment are very important determinants of a borrower's decision to default, and thus should also be important factors in the modification decision.

an indicator for adjustable rate mortgages that contain a reset provision (so-called "hybrid ARMs"), and, finally, an indicator for a borrower who does not use the corresponding property as a principal residence (this includes both properties used strictly for investment purposes, and vacation homes).

A. Canonical Specification Results

Panel B of Table VII displays the estimated marginal effects from a set of logit models for the three different types of modification definitions. The dependent variable is 1 if a 60-day deliquent loan is modified at any point in the 12 months following the first delinquency.[26] The first column considers payment-reducing (concessionary) modifications, the second column includes both payment-reducing and payment-increasing modifications, and the third column contains all modifications considered before, as well as prepayments. In all regressions, the group of portfolio-held loans is omitted from the estimation and is thus assumed to be the reference group. We cluster the standard errors at the zip code level to account for the fact that loans in the same geographical area are likely to suffer correlated (unobserved) shocks.

According to the estimates in the first column, private-label loans were approximately 0.6 percentage points more likely to receive concessionary modifications than loans held in portfolio. This estimate is economically small but statistically significant at the 10 percent level. When we consider all modifications the point estimate becomes one percentage point (statistically significant at the 10 percent level), while for the third specification, private-label loans were 2 percentage points more likely to receive concessionary modifications (statistically significant). As discussed above, all of these specifications include a number of additional loan characteristics that are important in the underwriting process and, thus, likely to play an important role in the modification decision. The first obser-

[26]The 12-month horizon implies that only mortgages that become delinquent before September 2007 are considered in the logit estimation.

vation to make regarding the results reported in Panel B is that the difference between the incidence of modification for portfolio-held loans and private-label loans becomes even smaller when these variables are controlled for in the estimation. The results also imply that loans with higher credit scores were modified less, loans more likely to have a second mortgage were modified less, larger loans were modified more (although jumbo loans were modified less), and loans with more equity at the time of delinquency were modified less. We find a sizeable difference in terms of the frequency of modification for both refinances and subprime loans for the two specifications in the second and third columns. Conditional on being 60-days delinquent, subprime loans were modified about 2 percentage points less than prime loans (second column). We estimate a model separately for subprime loans in Table VIII.

Censoring is an important issue for any sample of mortgages, as there are currently many delinquent loans that are, or will soon be, good candidates for modification, as the housing market continues to decline. For this reason, we estimate a Cox proportional hazard model of the transition from serious delinquency to modification. The Cox model is very common in the survival analysis literature, and it has the advantage of being both flexible in terms of functional form considerations, as the baseline hazard function can be treated as an incidental parameter, and easy to estimate in terms of computational considerations. The results, expressed as hazard ratios, are reported in Panel C. A hazard ratio less than 1 indicates that private-label loans were less likely to receive a modification compared to portfolio loans, while a ratio greater than 1 signifies the opposite. The estimates are consistent with what we report for the logits in the previous panel. Private-label loans were less likely to receive concessionary modifications, but this coefficient estimate is statistically insignificant. For the other two modification definitions the sign flips, but again the result is not statistically significant. All three specifications include the same covariates that were included in the logit models.

B. Subsample Results

Table VIII contains further logit estimation results for various subsamples of interest to see if there are different probabilities than in the full sample. Since the subprime indicator seems to be an important predictor of modification conditional on serious delinquency in Table VII, we report the estimated marginal effects for only the sample of subprime loans in the second column of Table VIII. The subprime sample also has the advantage that the agencies (Fannie Mae and Freddie Mac) were unlikely to be the marginal investor for this type of loan, so it is less likely that the portfolio and private-label samples differ significantly on unobservable characteristics. In the third column, we report results from the sample of LPS mortgages for which the borrower had a FICO score of less than 620, since automated underwriting systems generally instruct lenders to engage in increased scrutiny for such loans because of increased default risk. In the fourth and fifth columns, we focus on samples of loans that we believe contain the most information regarding the borrowers, in order to try to minimize the amount of unobservable heterogeneity that could potentially be biasing the results. In the fourth column, we focus on the sample of loans for which both the DTI ratio and the documentation status contain non-missing values, while the fifth column contains results for only the loans that were fully documented (in terms of income and assets) at origination. Panel A contains both unconditional means and estimated marginal effects for concessionary modifications, while Panel B contains results for the broader definition that also includes non-concessionary modifications.

The results are largely consistent with those contained in Table VII. We redisplay the results from the full sample in the first column of Table VIII for ease of comparison. The difference in modification frequency between private-label and portfolio-held, subprime mortgages for 60-day delinquent loans is small, and not statistically different from zero for both definitions of modification. Using a FICO cutoff of 620 as an alternative definition of subprime does not seem to make much difference. The unconditional means are similar

(for both types of loans) compared to the LPS subprime sample, as are the marginal effects of private-label loans estimated from the logit models (although the result for all mods is statistically significant at the 10 percent level). Finally, we find small but statistically significant results at the 10 percent level for the last two subsamples (displayed in the fourth and fifth columns of Table VIII), indicating that private label loans were *more* likely to be modified, not less.

C. Alternative Delinquency Definition

As an additional robustness check, we broaden our definition of delinquency and focus on modifications performed on loans subsequent to their first 30-day delinquency, which corresponds to one missed mortgage payment. While waiting until a borrower becomes seriously delinquent (defined as 60-days) to renegotiate is common practice in the servicing industry, there are no direct contractual stipulations (to our knowledge) that restrict a servicer from modifying the loan of a borrower who is 30-days delinquent. Thus, in Table IX we repeat our analysis of Tables VII and VIII, but condition on 30-days delinquency rather than 60-days. The table contains three panels of estimation results, one for each of our modification definitions, and all of the subsamples considered in Table VIII. The unconditional means, logit marginal effects, and Cox hazard ratios are all reported for each combination of subsample and modification definition.

The results are very similar to those from the analysis of 60-day delinquent loans. According to the full sample and subprime sample logit models, private-label loans received slightly more concessionary modifications, and the difference for the subprime subsample (0.5 percentage points) is statistically significant at the 10 percent level. Similarly, in the non-missing documentation and DTI sample and the full documentation sample Cox models, private-label loans received more concessionary modifications, although those differences

are also small.[27] The results for our second modification definition are similar, and we find no significant differences between the incidence of portfolio and private-level modifications. The samples of portfolio loans with non-missing information for DTI and documentation status were modified at similar rates as the corresponding sample of private-label loans, with small differences both when we consider concessionary modifications and all modifications. Finally, in Panel C, we see strong evidence for both the logit and Cox specifications, that delinquent private-label loans prepayed more often than portfolio loans. The differences are statistically significant for every one of the subsamples.

D. Redefault Probabilities and Cure Rates

In the previous subsections, we showed that there is little difference in the frequency of mortgage loan modifications between servicers of loans held in a private trust versus loans held in portfolio. There are two potential reasons that may explain the failure of those exercises to pick up important differences in servicer behavior that may truly exist. First, it may be that contract frictions in securitization trusts do not result in substantial differences in the frequency of modifications (the extensive margin) but do result in significant differences in the intensive margin, with respect to the types of modifications performed, the extent to which contract terms are modified, and, more broadly, the care or effort expended in each modification by private-label servicers compared to that expended by portfolio servicers. Second, there may be a type of renegotiation that our algorithm does not identify, but that is used to a large extent in loss mitigation efforts and used differently by servicers of private-label loans than by servicers of portfolio loans. For example, forms of forbearance, which are often called repayment plans in the industry, would not be picked up by our algorithm.[28]

[27] The logit marginal effects correspond to percentage point differences, while the Cox hazard ratios correspond to percent differences. If one expresses the logit marginal effects as a percent change of the unconditional means, those percent changes are very similar in magnitude to the Cox results.

[28] However, as we argued above, PSAs do not contain restrictions on repayment plans, because they do not involve changing the terms of the mortgage. Thus, we would argue that differences in forbearance

In this subsection, we use the LPS data to attempt to address these possibilities.

We perform two separate empirical exercises to address each of these concerns in turn. First, we compare redefault rates of private-label modified loans with those of portfolio modified loans. We define redefault as a loan that is 60 days delinquent or more, in foreclosure process or already foreclosed and now owned by the lender (REO for "real-estate-owned") six months after the time of the modification. If there are important differences in the manner by which servicers of private-label loans modify mortgages relative to the foreclosure procedures of servicers of portfolio loans, then we would expect to see significant differences in the subsequent performance of modified loans.

Second, to address the possibility that our algorithm misses an important aspect of renegotiation, we compare the cure rates of seriously delinquent, private-label loans to those of seriously delinquent portfolio loans. The idea behind this exercise is that any appreciable difference in servicer renegotiation behavior will manifest itself in differences in cure rates. As we mentioned above, Piskorski, Seru, and Vig (2009) focus on differences in foreclosure rates in an attempt to identify differences in the extent of renegotiation between servicers of portfolio loans and servicers of private-label loans. We view the cure rate as the more relevant outcome to study, as differences in foreclosure rates could be due to many other explanations, such as the unwillingness of banks to recognize losses or political pressure to reduce foreclosure numbers, perhaps by imposing foreclosure moratoria. In the current version of their paper, Piskorski, Seru, and Vig also estimate very similar cure regressions to those we report below, and after finding similar differences in cure rates compared to differences in foreclosure rates, interpret it as evidence that securitization may impede renegotiation. We find small differences (much smaller than the differences in foreclosure rates documented in Piskorski, Seru, and Vig (2009)), due in part to the different treatment of right-censored observations and to different control variables, which

behavior that might exist could not be the result of contract frictions in securitization trusts.

we discuss in detail in Appendix B and in Adelino, Gerardi, and Willen (2010). It is important to stress however, that differences in servicer renegotiation behavior are also only one potential explanation for differences that may exist in cure rates. To put this idea in the terms of logical reasoning, differences in cure rates are a necessary condition for significant differences in renegotiation behavior, but they are not a sufficient condition.

Table X contains the results of the redefault analysis. The first observation to note from the table is that the unconditional probability that a modified mortgage redefaults in this six-month period is very large, at about 35–45 percent for payment-reducing modifications (Panel A), and about 45–60 percent for all modifications (Panel B). We argue below that the high level of redefault rates could explain why we observe so few modifications — very often they do not lead to successful outcomes even as little as six months after the modification. The second observation to note is that there is no statistically significant difference between the redefault rates of private-label loans and those of portfolio loans, once the observable characteristics of the mortgages are taken into account (except for the subprime subsample, and when we consider all modifications, where private label is actually associated with fewer defaults). These results, combined with the statistics displayed in Table VI suggest that there are no substantial differences in either the type of modification employed or in the care/effort expended by the two types of servicers.

Table XI shows the results of both logit and cox proportional hazard models for the probability that a seriously delinquent loan subsequently cures. Accounting for right-censored observations is very important here, which is why we also include results from the hazard specification in Table XI (we discuss this issue in detail in Appendix B). Our definition of a cure in the logit models is that the loan is either current, 30-days delinquent, or prepaid at any point during the first 12 months following the first 60-day delinquency. The first important point to make is that the unconditional cure probabilities in our sample are large (around 55 percent). Given that the unconditional modification probability is about 8 per-

cent, this means that many loans cured without any intervention on the part of servicers. The second important observation to note in this table is that the cure probabilities for portfolio loans and private-label loans are quite similar. The unconditional cure probability is smaller by about 1 percentage point for private-label loans in the whole sample, but that difference reverses to 3 percentage points larger (statistically significant) when we control for observable characteristics of the loans and borrowers, which is approximately 5 percent of the unconditional mean for private-label loans. The estimated hazard ratio shows the opposite sign, as it implies that private-label loans are about 3 percent less likely to cure relative to portfolio loans, but is not statistically significant at conventional levels.

We also include results for the subsamples of interest in columns 2–5. For many of the subsamples private-label loans were *more* likely to cure. This is an important robustness check, as we argued above that unobserved heterogeneity is likely to be less of a problem in the subsamples (especially for the non-missing documentation status and DTI ratios sample and the full documentation sample). Thus, the change in the sign of the differences in cure rates between private-label servicers and portfolio servicers suggests that unobserved heterogeneity between the two loan types plays an important role.

In addition to the subsamples reported above, we also include results from three other subsamples that focus on borrowers with high credit scores at origination (FICO $>$ 680) in columns 6–8. Piskorski, Seru, and Vig (2010) focus much of their attention on the sample of loans for which the borrower's FICO score was above 680, based on their belief that borrowers with good credit histories are better candidates for renegotiation from the lender's perspective. We do not share the same sentiment, and in fact believe that this sample of borrowers likely contains a significant amount of unobserved heterogeneity. The reason is that the majority of mortgages in this subsample are below the GSE conforming loan limits, and thus are potential candidates for acquisition and securitization by Fannie Mae or Freddie Mac. We believe the fact that these mortgages were not sold to the GSEs

implies that they are likely characterized by some risky element (unbeknownst to us) that makes them unattractive. Column 6 shows that the difference in cure rates between private-label and portfolio loans is only negative in the high FICO subsample (approximately 17 percent) compared to the full sample of loans. But, the results from the hazard estimates in columns 7 and 8 suggest that the difference between the high FICO sample and the full sample is largely being driven by the sample of conforming loans that we believe to be characterized by significant unobserved heterogeneity. The difference in cure rates for the non-conforming sample is consistent with the difference in cure rates in the full sample of mortgages (column 1).

E. Causality

We have been careful to interpret the estimates discussed above in terms of correlations rather than as causal effects. That is, we are not claiming that causality runs explicitly from securitization or portfolio lending to modification or cure probabilities, because we do not have any exogenous variation in the decision to securitize loans to obtain clean identification. We are simply saying that the fact that we do not see large observable differences in modification and cure probabilities does not support the story that the institution of securitization is impeding mortgage renegotiation. With that said, we believe that this study offers more than a purely descriptive analysis, because unobserved heterogeneity likely works against our finding of small differences in modification rates and cure rates. To the extent that privately securitized loans are worse on unobservables compared to portfolio-held loans, we would expect to see lower modification and cure rates for private-label loans, and thus a larger difference in renegotiation between the two types of loans. Of course if the opposite relationship held, then we'd expect lower modification rates of portfolio loans, which is why we are reluctant to interpret our estimates in a causal manner. But we would argue that the majority of the literature regarding this crisis suggests that securitized loans suffer from

this issue more than portfolio-held loans.[29]

Piskorski, Seru, and Vig (2010) claim to have found a source of exogenous variation in the decision to securitize a mortgage, which they then use to study differences in foreclosure rates between private-label and portfolio loans. The source of the variation comes from rules in securitization contracts regarding mortgages that default early in their payment history, or so-called "early-payment defaults." In Adelino, Gerardi, and Willen (2010) we argue in detail that this is not a valid instrument for securitization as lenders choose which loans to repurchase and, in fact, repurchase very few. The identification thus hinges on the assumption that lenders randomly choose which loans to repurchase which is no more plausible than the assumption that lenders randomly select loans to select loans for securitization in the first place.

IV. Understanding the Empirical Results

If securitization does not block renegotiation, then why is it so rare? In this section, we discuss the incentives behind the renegotiation decision from the lender's point of view, which, in a stylized way, mirrors the net present value (NPV) calculation that servicers are supposed to perform when deciding whether to offer a borrower a modification. We show that servicer uncertainty about the true ability or willingness of borrowers to repay their loans (and thus their ability to cure without needing a modification), as well as about whether the borrower will redefault even after successful renegotiation can dramatically affect the NPV calculation, ruining what a naive observer might think of as a "win-win" deal for the borrower and lender. In addition, we argue that moral hazard may play an important role in the lender's modification decision. Specifically, as a lender offers a more generous modification to its eligible borrowers, it produces a financial incentive for ineligible borrowers to take hidden actions in order to gain eligibility. For example, many lenders

[29] See Krainer and Laderman (2009).

require that borrowers are delinquent on their mortgage before qualifying for a modification. If the benefits of missing mortgage payments to qualify outweigh the costs, which include restricted access to future credit, then even borrowers who are not seriously considering default will have an incentive to become delinquent.

We also provide institutional evidence in this section that supports our arguments and findings above. This includes evidence of low modification frequencies in previous housing busts, well before the advent of securitization trusts; the equal treatment provision statements contained in the PSAs, which direct the servicer to behave as if it was in fact the investor of the mortgage-backed security and thus the owner of the mortgages; and finally, the absence of lawsuits to date directed at servicers by investors in mortgage-backed securities, which one would expect to find if modifications were unambiguously better than foreclosures.

A. Renegotiation and Asymmetric Information

In this section, we argue that avoiding the deadweight loss of foreclosure through renegotiation is far more difficult than it may at first appear. Many commentators have argued that the large losses in foreclosure relative to the costs of modification mean that the only possible explanations for foreclosures are either contract frictions from securitization or simple economic irrationality. Foreclosure can dominate renegotiation even when the costs of foreclosure far exceed the proposed concession by the lender, in particular when there is uncertainty about the effectiveness of renegotiation and when borrowers have private information about their ability and willingness to repay or about the value of their property.

There are two sets of problems. The first relate to uncertainty about the outcome of the loan with and without renegotiation and emerge even in situations where the borrower has no private information. To illustrate these problems, we offer a slide from a PowerPoint presentation by an executive from Indymac, a lender involved in a high profile program

to renegotiate mortgages (Figure 2). What the flow chart on the slide illustrates is that lenders take into account two important facts: (i) not all borrowers who renegotiate avoid foreclosure and (ii) some borrowers who fail to get modifications still manage to avoid foreclosure. Both of these "errors" make renegotiation less attractive, the former, known as redefault risk, because the renegotiation postpones foreclosure, delaying recovery and leading to further deterioration of the property and the latter, known as self-cure risk, because the lender will make a concession to a borrower who would have repaid the loan in full without assistance. It is important to understand that both redefault risk and self-cure risk emerge in situations where the lender and borrower have symmetric information. In other words, the borrower has no better idea whether he or she will cure or redefault than the lender.

Adding asymmetric information makes renegotiation much more difficult as shown in Riddiough and Wyatt (1994b) and Wang, Young, and Zhou (2001). Adelino, Gerardi, and Willen (2010) (henceforth AGW) consider a model in which borrowers have private information about their willingness and ability to repay the loan. Formally, AGW summarize a borrower's private information as a reservation amount that they are willing to repay; if the cost of repaying the loan exceeds the amount the borrower owes, default ensues and otherwise he or she repays. A lender that can observe the borrower's reservation value will set the repayment amount equal to it and thus avoid foreclosure and the associated deadweight losses. But a lender who cannot observe the borrower's valuation faces the familiar problem of a non-discriminating monopolist. In this case the marginal cost of renegotiation equals the liquidation value of the property but the profit-maximizing lender, like the monopolist, sets the price above marginal cost and thus allows foreclosures to proceed even in the absence of self-cure or redefault risk. The intuition is that by increasing the concession in renegotiation, the lender prevents additional foreclosures but incurs losses because it must extend that same concession to borrowers with higher reservation valuations, who would

have accepted a much smaller concession.

It is crucial to understand that in this setup, the lender will walk away from positive NPV modifications. If we assume that there is no self cure risk and no redefault risk, then as long as the renegotiated repayment amount exceeds the liquidation value of the property, renegotiation has positive NPV and is, in that sense, profitable. But the goal of the lender is to maximize profits and not just to achieve non-negative profits, and the optimal concession to the borrower typically exceeds the minimum profitable concession.

To understand the mechanics of asymmetric information in practice, we return to Figure 2, the Indymac presentation. There is no mention of asymmetric information in the picture but it's pernicious effect will be felt by borrowers who receive modification offers that are too small to allow them to keep their homes. Such borrowers will promise to default unless a larger modification is offered but the lender will rationally proceed with foreclosure. The costs savings on smaller modification offers accepted by observationally equivalent borrowers will offset the losses from the foreclosure.

But the problem is even worse than the previous paragraphs indicates because we assumed, up until now, that the distribution of reservation values is invariant to the size of the modifications offered. AGW show that the costs of modification grow even larger relative to foreclosure if we allow the distribution to change. In this case, the decision to offer more generous terms in renegotiation induces some borrowers who previously did not seek renegotiation to do so by, for example, deliberately becoming delinquent on their loans. This problem is well-known in the industry and is the moral hazard problem of modifications. As one lender described it in an article in *ABA Banking Journal*, "We have not to date forgiven any principal. We are wary of the consequences of being known as a bank that forgives principal."[30]

[30] "Bankers' view of the new Hope for Homeowners program," *ABA Banking Journal*, October 2008.

B. Institutional Evidence

While the results from Section III may be surprising to market commentators who believe that contract frictions inherent in securitization trusts are preventing large-scale modification efforts in mortgage markets, we argue in this section that both historical evidence and evidence from securitization contracts actually support our findings.

First, we look at history. If securitization, or more precisely private-label securitization, inhibits renegotiation, then we would expect that renegotiation would have been common in the 1990s, when there was little private-label securitization, or in the 1970s, when securitization itself was rare. But, the historical evidence we have does not bear that out. In 1975, Touche Ross surveyed loss mitigation activities at savings and loans and found, "Lenders... were unwilling to either modify loans through extended terms or refinancing to a lower rate."[31] In the 1990s, a report commissioned by Congress to study foreclosure alternatives, said, "Along with loan modifications, long-term forbearance/repayment plans are the most under utilized foreclosure avoidance tools currently available to the industry."[32]

Second, many observers have focused on institutional factors that inhibit loan modification when the loan is securitized, but other factors may play a similar role for portfolio lenders as well. In particular, accounting rules force lenders (i) to take writedowns at the time of the modification (reducing Tier II capital), (ii) to identify modified loans as troubled debt restructurings (under FAS 15), and (iii) to impose burdensome reporting requirements on modified loans including loan-specific allowances for potential losses (under FAS 114). Additionally, payments made by borrowers for loans that are subject to "troubled debt restructurings" are recognized only as principal repayments and generate no interest income until the bank can demonstrate that a borrower is "performing." All of the above accounting requirements potentially make modifications costly for a bank. Downey Financial, for

[31] Capone (1996), p. 20–21.
[32] Capone (1996).

example, attempted to refinance current borrowers out of risky option ARMs into safer, fixed-rate instruments and argued that the change should not affect their balance sheet because the borrowers had never missed payments. However, their accountants viewed the refinancings as "troubled debt restructurings," and forced the firm to restate the share of nonperforming assets for November 2007 to 5.77 percent from 3.65 percent.[33]

If modifications were truly in the best financial interest of investors in mortgage-backed-securities (MBS) as many commentators have alleged, we would expect to see concern on their part regarding the low levels of modifications performed to date. But, according to Cordell, Dynan, Lehnert, Liang, and Mauskopf (2008b), who interviewed a number of MBS investors, they (the investors) are not concerned that servicers are foreclosing on many more mortgages than they are modifying. Thus, there does not seem to be much concern by market participants that either incentives or contract frictions are inhibiting servicers from performing loan modifications. The evidence in the literature seems to suggest a small role for contract frictions in the context of renegotiation. In a 2007 study of a small sample of PSAs, Credit Suisse found that fewer than 10 percent of the contracts ruled out modifications completely, while approximately 40 percent allowed modifications, but with quantity restrictions,[34] and the rest, about half, contained no restrictions on renegotiation behavior. Hunt (2009) also analyzed a sample of subprime PSAs and concluded that outright modification bans were extremely rare. A 2008 report by the COP analyzed a number of securitized mortgage pools with quantity restrictions and concluded that none of the restrictions were binding. In terms of incentive issues, Hunt (2009) found that most of the contracts in his sample explicitly instructed the mortgage servicer to behave as if it were the owner of the pool of the loans:

> The most common rules [in making modifications] are that the servicer must

[33] http://www.housingwire.com/2008/01/14/downey-financial-accounting-rules-suck/

[34] The quantity restrictions often took the form of a limit (usually 5 percent) on the percentage of mortgages in the pool that could be modified without requesting permission from the trustee.

> follow generally applicable servicing standards, service the loans in the interest of the certificate holders and/or the trust, and service the loans as it would service loans held for its own portfolio. Notably, these conditions taken together can be read as attempting to cause the loans to be serviced as if they had not been securitized. (p. 8, insertion added)

V. Conclusion

There is widespread concern that an inefficiently low number of mortgages have been modified during the current crisis, and that this has led to excessive foreclosure levels, leaving both families and investors worse off. We use a large dataset that accounts for approximately 60 percent of mortgages in the United States originated between 2005 and 2007, to shed light on the claim that delinquent loans have different probabilities of renegotiation depending on whether they are securitized by private institutions or held in a servicer's portfolio. By comparing the relative frequency of renegotiation between private-label and portfolio mortgages, we are able determine whether institutional frictions in the secondary mortgage market are inhibiting the modification process from taking place.

Our first finding is that renegotiation in mortgage markets during this period was rare. In our full sample of data, less than 2 percent of the seriously delinquent borrowers received a concessionary modification in the year following their first serious delinquency, while approximately 8 percent received some type of modification. These numbers are low, considering that foreclosure proceedings were initiated on approximately half of the loans in the sample and completed for almost 30 percent of the sample.

Our second finding is that a comparison of renegotiation rates for private-label loans and portfolio loans, while controlling for observable characteristics of loans and borrowers, yields economically small, and for the most part, statistically insignificant differences. This

finding holds for a battery of robustness tests we consider, including various definitions of modification, numerous subsamples of the data, including subsamples for which we believe unobserved heterogeneity to be less of an issue, and consideration of potential differences along the intensive margin of renegotiation.

Since we conclude that contract frictions in securitization trusts are not a significant problem, we attempt to reconcile the conventional wisdom held by market commentators, that modifications are a win-win proposition from the standpoint of both borrowers and lenders, with the extraordinarily low levels of renegotiation that we find in the data. We argue that the data are not inconsistent with a situation in which, on average, lenders expect to recover more from foreclosure than from a modified loan. At face value, this assertion may seem implausible, since there are many estimates that suggest the average loss given foreclosure is much greater than the loss in value of a modified loan. However, we point out that renegotiation exposes lenders to two types of risks that are often overlooked by market observers and that can dramatically increase its cost. The first is "self-cure risk," which refers to the situation in which a lender renegotiates with a delinquent borrower who does not need assistance. This group of borrowers is non-trivial according to our data, as we find that approximately 50 percent of seriously delinquent borrowers "cure" in our data without receiving a modification. The second cost comes from borrowers who default again after receiving a loan modification. We refer to this group as "redefaulters," and our results show that a large fraction (between 40 and 60 percent) of borrowers who receive modifications, end up back in serious delinquency within six months. For this group, the lender has simply postponed foreclosure, and, if the housing market continues to decline, the lender will recover even less in foreclosure in the future. In addition to self-cure and redefault risk, we discuss how asymmetric information between the borrower and lender can significantly reduce the financial incentives for a lender to renegotiate.

We believe that our analysis has some important implications for policy. First, "safe har-

bor provisions," which are designed to shelter servicers from investor lawsuits, are unlikely to have a material impact on the number of modifications, and thus will not significantly decrease foreclosures. Second, and more generally, if the presence of self-cure risk, redefault risk, and moral hazard stemming from asymmetric information do make renegotiation less appealing to investors, the number of easily "preventable" foreclosures may be far smaller than many commentators believe.

A. Appendix: Identifying Modifications in the LPS Dataset

In this section we discuss in detail the assumptions that we used to identify modified loans in the LPS dataset. The LPS dataset is updated on a monthly basis, and the updated data include both new mortgages originated and a snapshot of the current terms and delinquency status of outstanding mortgages. Essentially, for a given mortgage, we compare the updated terms to the terms at origination, as well as the change in terms from the proceeding month, and if there is a material change over and above the changes stipulated in the mortgage contract, then we assume that the contract terms of the mortgage have been modified.

A. Interest Rate Reductions

We use a different set of rules to identify reduced interest rates for fixed-rate mortgages (FRM) and adjustable-rate mortgages (ARM). In principle, identifying a rate change for an FRM should be easy, since by definition the rate is fixed for the term of the mortgage. However, after a detailed inspection of the LPS data, it became apparent that some of the smaller rate fluctuations were likely due to measurement error rather than to an explicit modification. Thus, we adopt a slightly more complex criterion: The difference between the rate at origination and the current rate must be greater than 50 basis points; *and* the difference between the rate in the previous month and the current rate must be greater than 50 basis points; *and* either the mortgage must be 30-days delinquent with the loan currently in loss mitigation proceedings (as reported by the servicer) or the difference between the rate in the previous month and the current rate must be greater than 300 basis points (which allows for the possibility that a loan that is current could feasibly qualify for a modification).

Identifying interest rate reductions for ARMs is slightly more complicated, since by definition the interest rate is variable and can move both up and down. The LPS data contain the information necessary to figure out how much the interest rate should move from

month to month. This rate is often referred to as the fully indexed rate, as it is normally specified as a fixed spread above a common nominal interest rate. The LPS dataset contains information regarding the initial rate, the appropriate index rate, and the spread between the index and the mortgage rate. In addition, the majority of ARMs are characterized by a period at the beginning of the contract in which the interest rate is held constant (these mortgages are often referred to as hybrid ARMs). At the end of this period, the interest rate adjusts (or resets) to a certain spread above an index rate and then subsequently adjusts at a specific frequency. The LPS dataset also contains information regarding the length of the initial fixed period, enabling us to identify this period in the data and determine the point at which the interest rate should begin to adjust (we refer to this period as the reset date). Our criterion for identifying an interest rate reduction for an ARM is as follows: The difference between the rate at origination and the current rate must be greater than 50 basis points; *and* the difference between the rate in the previous month and the current rate must be greater than 50 basis points; *and* if the reset date has passed, then the difference between the fully-indexed rate and the current rate must be at least 100 basis points ; *and* either the mortgage must be 30-days delinquent with the loan currently in loss mitigation proceedings (as reported by the servicer) or the difference between the rate in the previous month and the current rate must be greater than 300 basis points (which allows for the possibility that a loan that is current could feasibly qualify for a modification). In addition, we allow for more modest month-to-month decreases in the interest rate (200 to 300 basis points) as long as there is also a positive change in the delinquency status of the loan (that is, the loan is reported to be less delinquent). Our inspection of the data suggests that the majority of modifications involve a resetting of the delinquency status back to current, or a minor delinquency, so conditioning on this change likely eliminates many false positives.

B. Term Extensions

In theory, it should be straightforward to identify term extensions in the LPS data, but it can be tricky to do so because of possible measurement error in the variable that measures the remaining maturity of each loan. We defined a term extension in the LPS dataset to be a case in which the loan was at least 30-days delinquent at some point and the number of years remaining increases by at least 20 months *or* the change in number of years remaining is greater than the difference between the original term of the loan and the remaining term (for example, if the original maturity is 360 months, and the loan has 350 months remaining, then the increase in length must be at least 10 months) and, finally, either the monthly payment decreases *or* the principal balance increases *or* the loan is in loss mitigation.

C. Principal Balance Reductions

A reduction in the remaining balance of a mortgage is perhaps the most difficult type of modification to identify because of the prevalence of "curtailment" or partial prepayment among mortgage borrowers. For example, it is common for borrowers to submit extra mortgage payments in order to pay down the loan at a faster rate. For this reason, we were forced to adopt strict criteria to limit the number of false positives. Our criterion for identifying a principal balance reduction is as follows: The month-to-month decrease in the remaining principal balance must be at least -10 percent and cannot be more than -30 percent (the upper bound does not matter as much as the lower bound—we experimented with -40 percent and -50 percent, but did not find a substantial difference); the principal balance recorded in the previous month must be greater than $25,000 (since we throw second liens out, and look only at mortgages originated after 2004, this cutoff does not bind often); the month-to-month payment change must be negative (there are only a few cases in which the principal balance is reduced without a corresponding decrease in the payment, but in these cases the term is extended, and thus is picked up in our code for identifying term

extensions); and, finally, the mortgage must be either 30-days delinquent or currently in loss mitigation proceedings (as reported by the servicer).

D. Principal Balance Increases

For interest-only and fully-amortizing mortgages, identifying an increase in the principal balance due to the addition of arrears is relatively straightforward. It becomes trickier for mortgages that allow for negative amortization, as the principal balance is allowed to increase over the course of the contract, by definition. For interest-only and fully-amortizing mortgages our criterion is: The month-to-month principal balance must increase by at least 0.5 percent (to rule out measurement error in the data); the loan must have been at least 30-days delinquent at the time of the balance increase; and, finally, the month-to-month payment change must be positive unless there is also a corresponding increase in the term of the loan. For mortgages that allow for negative amortization, the criterion is similar, except that the balance increase must be at least 1 percent and there must be a positive change in the delinquency status of the loan.

B. Appendix: Transferred Loans

There are loans in the LPS data for which the servicing rights are transferred to a servicer that does not contribute to the LPS dataset. When the servicing rights are transferred, the loans drop out of the data, and we do not observe their subsequent performance. While they only account for a small subsample of the loans in the data, unfortunately the transferred loans do not appear to be randomly selected. This is evident from a comparison of their observable characteristics at origination and their relative performance up until the time of transfer. In addition, a significantly higher percentage of portfolio-held loans are transferred compared to private-label loans. These observations imply that the sample of transferred

loans cannot be simply dropped from the data, as doing so could result in a non-trivial bias of the estimated difference in cure rates (and foreclosure rates) between portfolio and private-label mortgages. In this appendix, we document some of the observable differences between transferred loans and non-transferred loans, show the bias introduced into the estimates of cure rate differences when transferred loans are dropped from the data, and discuss the appropriate way to deal with this sample of loans. Most of this discission is taken from a correspondence with Piskorsi, Seru, and Vig. In earlier versions of their paper, they in fact dropped the transferred loans, while in earlier versions of our paper (as well as in this draft), we kept them in the sample, but assumed that they did not cure. As we discuss below, dropping the transfers altogether creates a large bias, while assuming that the transfers do not cure does not seem to create bias, and in our opinion is a valid assumption for reasons discussed below.

In our sample, there are 3,922 loans that are transferred within 12 months of the date of the first 60-day delinquency. Approximately 4 percent of seriously delinquent, private-label loans transfer within 12 months, while 6.5 percent of seriously delinquent, portfolio loans transfer. Table I displays the the mean and median of the FICO score and DTI ratio at origination for the sample of transferred loans. The mean and median FICO score is lower for the entire sample of transferred loans compared to non-transferred loans, but the difference between portfolio transfers and non-transfers is much larger than the difference between private-label transfers and non-transfers. The same pattern emerges for DTI ratios.

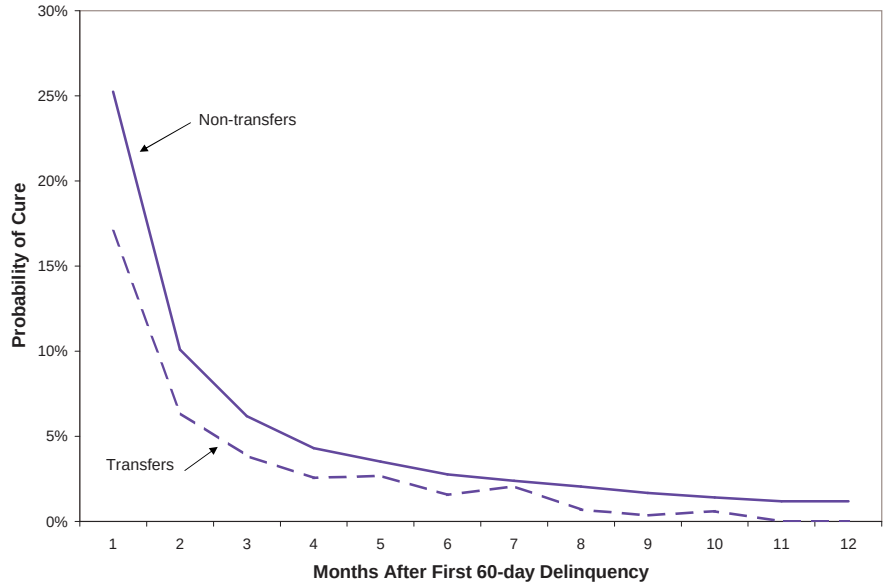
In addition to the transferred loans having worse observable characteristics, they also perform significantly worse before they are transferred than loans that are not transferred. Figure 1 displays the Kaplan Meier cure hazards for the sample of transferred loans and non-transferred loans.[35] It is clear from the figure that the transferred loans cure significantly less frequently than loans that are not transferred.

[35] We define cures in the same way that we defined them in section III.D above, where we assume in the figure that the cure state is absorbing.

Table I
FICO and DTI Distribution of Transferred versus Non-transferred Loans

		Transfers			Non-Transfers		
		#	Mean	Median	#	Mean	Median
FICO	Private-label	1,276	628	627	68,442	632	632
	Portfolio	708	622	620	9,793	652	654
	Total	1,984	626	624	78,235	634	634
DTI	Private-label	1,636	42	41	40,821	40	41
	Portfolio	457	39	41	7,913	35	36
	Total	2,093	42	41	48,734	39	40

Figure 1. Kaplan Meier Estimator for Cures: Transfers versus Non-Transfers



In addition, Table II shows that transferred loans are significantly worse than non-transferred loans in the period immediately before they are transferred. The table compares the status of the transferred loans in the month before transfer to the status of all loans in the first twelve months after origination. A much lower percentage of transferred loans are

current, and a much higher percentage are in foreclosure or seriously delinquent. This is especially true of the portfolio transfers.

Table II
Status of Loan in Preceding Period: Transferred versus Non-transferred Loans

Status in Previous Month	Transfer Observations		All Observations	
	Private-label (%)	Portfolio (%)	Private-label (%)	Portfolio (%)
30-days delinquent	7.1	6.8	8.6	9.3
60-days delinquent	23.0	16.0	12.9	12.9
90-days delinquent	20.8	27.4	20.9	21.6
Current	9.5	7.5	14.0	23.1
In Foreclosure Proceedings	32.9	39.3	24.6	20.6
REO Status	6.6	2.8	16.6	10.1

As a result of the differences in observed characteristics and performance, the transferred loans cannot simply be thrown out of the sample. Doing so creates a serious bias in the comparison between the cure rates of portfolio loans versus private-label loans. The bias works in the direction of the portfolio loans, since the portfolio loans that are transferred are of worse quality than the private-label loans that are transferred. Thus, by throwing out all transferred loans, the cure rate of seriously delinquent portfolio mortgages is artificially inflated relative to the cure rate of private-label loans. This can actually be clearly seen from Panel A2 in the August 2009 version of Piskorski, Seru, and Vig. The table shows the results of logit cure regressions at different horizons. At each horizon the authors estimated two regressions, with each regression corresponding to a different sample of loans. The first sample excludes the transferred loans completely (this is the column titled "original sample"), while the second sample excludes loans that were transferred before the relevant horizon, but includes loans that were transferred after the horizon (this is the column titled "original + transferred"). While this does not solve the problem completely for horizons greater than 1 month (since transferred loans are still being excluded), the 1 month logits

should be free of bias, as most of the transferred loans are still present. The estimated marginal effect falls from 0.045 to 0.026, when the transferred loans are kept in the sample which corresponds to a 46% decrease. This implies that holding the horizon constant (at 1 month), excluding transferred loans increases the estimated difference in cure rates between portfolio and private-label from just over 7% of the unconditional mean of the cure rate for portfolio loans to over 12%. In addition, at the twelve month horizon (the last two columns of the table) the bias can no longer be seen because almost all loans that are transferred in the LPS data are transferred within 12 months of the first serious delinquency.[36]

As a result of the bias discussed above from throwing out the sample of transferred loans, we conclude that the appropriate way to deal with the issue is to estimate a hazard model rather than a discrete-choice model like a logit or probit. A hazard model appropriately accounts for right-censored observations, and thus, has the advantage of being able to use observable information before the loans are transferred. Of course, it must be noted that the hazard model would not be able to control for scenarios in which the transferred loans perform worse after they are transferred (which we cannot observe). An implicit assumption in a hazard model is that right-censored observations perform the same as non-censored observations after they drop out of the data. Given that transferred loans perform worse while we observe them in the data, it is likely the case that they perform worse after we stop observing them. As a result, a hazard model specification for cure rates might still contain a bias in favor of portfolio loans. Piskorski, Seru, and Vig instead argue in an earlier draft of their paper (August 2009) that the sample of transferred portfolio loans may have been transferred to other servicers of portfolio loans who renegotiate many of their loans, in which case it would be more difficult to show that portfolio loans cure more than private-label loans. They say:

> Second, there are at least two large servicers (Ocwen Loan Servicing and Litton

[36] In fact the bias is present in both columns.

> Loan Servicing) who are widely known to renegotiate substantial amount of loans that are not in the LPS database. Consequently, not including a sample of largely bank-held loans that might be transferred for renegotiation to these servicers might actually make it harder for us to demonstrate that portfolio loans are renegotiated more intensively.

However, the servicers that they mention, Ocwen and Litton (who are not providers to the LPS data), service private-label loans almost exclusively. Thus, if the private-label loans are being transferred to Ocwen and Litton, then the estimates of the hazard model will actually be biased in favor of finding larger cure rates for portfolio loans, rather than smaller cure rates!

As mentioned above, in earlier versions of this paper, as well as in the current draft, we estimate logit models and treat the transfers as non-cures (as opposed to throwing them out of the data). We made this assumption on the grounds that the servicer that we observe in the data did not modify these loans while they were in the data. While some may argue that this assumption could have the effect of biasing the estimates of the cure differences in the opposite direction – that is, creating a downward bias in the probability of cure for portfolio loans – the estimates from the hazard specification in Table XI are very similar to the estimates from the logit models, implying little bias.

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Table III
Examples of modifications in the data.

Example 1: Servicer cuts interest rate, capitalizes arrears in the balance of the loan and extends term to 40 years.

Date	MBA Delinq. Stat.	Interest Rate	Monthly Payment	Outstanding Balance	Remaining Term in Months
2008m4	9	6.5	907	141,323	340
2008m5	9	6.5	907	141,323	339
2008m6	9	6.5	907	141,323	338
2008m7	C	4.5	660	146,686	479

Example 2: Servicer capitalizes arrears into the balance of the loan but otherwise leaves the loan unchanged.

Date	MBA Delinq. Stat.	Interest Rate	Monthly Payment	Outstanding Balance	Remaining Term in Months
2008m5	6	9.25	1,726	208,192	346
2008m6	9	9.25	1,726	208,192	346
2008m7	9	9.25	1,726	208,192	346
2008m8	C	9.25	1,815	218,316	341
2008m9	C	9.25	1,815	218,184	340

Table IV
Robustness of the modifications algorithm

False positives by type of modifications

	# of Modifications Using WF CTS Data	False Positives
FRM Rate Reduction	5,381	8.0%
ARM Rate Reduction	8,951	22.0%
Principal Reductions	470	1.9%
Principal Increases	13,010	12.8%
Term Increases	394	2.3%

Overall success of algorithm

	No Mod Using Our Algorithm	Mod Using Our Algorithm	Total
No Mod in WF Data	2,329,187	3,559	2,332,746
Mod in WF Data	3,627	17,514	21,141
Total	2,332,814	21,073	2,353,887

Notes: We test our algorithm on a dataset of securitized mortgages in which the trustee has identified modifications (data is from Wells Fargo Trustee Services). The lower panel shows that about 17.2% of our modifications are false positives, meaning that we identify modifications but the trustee does not and about 16.9% are false negatives, meaning that the trustee identifies a modification but we do not.

Table V
Modification Statistics

(1) By Type of Modification: 2007:Q1–2008:Q4

	# Loans Modified	Interest Rate Reductions		Principal Balance Reductions		Principal Balance Increases		Term Extensions	
		#	(% total)	#	(% total)	#	(% total)	#	(% total)
2007:Q1	10,940	600	5.3	700	6.2	8,660	76.4	1,380	12.2
2007:Q2	14,600	820	5.4	550	3.7	11,630	77.3	2,050	13.6
2007:Q3	17,720	770	4.1	810	4.3	15,170	81.2	1,940	10.4
2007:Q4	27,150	2,990	9.7	700	2.3	22,520	72.8	4,740	15.3
2008:Q1	36,230	6,010	13.8	900	2.1	32,100	73.8	4,500	10.3
2008:Q2	44,750	9,050	16.4	1,300	2.4	39,750	72.1	5,030	9.1
2008:Q3	62,190	16,280	20.3	940	1.2	56,940	70.9	6,110	7.6

(2) By Payment Change

	Payment Decreases					Payment Increases				
	#	mean Δ		median Δ		#	mean Δ		median Δ	
		$	%	$	%		$	%	$	%
2007:Q1	2,080	-492	-13.2	-157	-10.0	5,020	106	6.7	62	4.4
2007:Q2	2,060	-464	-12.7	-141	-9.6	7,710	120	7.0	63	4.4
2007:Q3	2,470	-290	-12.9	-125	-9.7	10,380	110	6.7	60	4.3
2007:Q4	5,600	-367	-15.3	-159	-11.7	14,540	100	5.9	59	3.9
2008:Q1	11,500	-358	-14.0	-210	-13.2	18,720	108	6.5	62	4.3
2008:Q2	18,660	-425	-16.1	-239	-14.1	20,770	124	7.4	69	4.1
2008:Q3	31,770	-562	-21.5	-365	-20.2	26,400	124	6.3	63	3.6

(3) Loan Characteristics of Modified Mortgages

	All Loans					Modifications				
	#	mean	p25	p50	p75	#	mean	p25	p50	p75
FICO (at origination)	1,892,777	706	660	713	762	17,533	622	580	621	662
LTV (at origination)	2,250,162	75	67	79	85	21,675	82	78	80	90
DTI (at origination)	1,346,093	37	28	38	45	13,945	41	35	41	47
Mortgage balance (at origination)	2,267,497	231K	121K	185K	288K	21K	234K	121K	186K	294K
% characterized as										
LTV = 80	14.4					21.7				
Subprime	6.8					47.4				
Fixed	71.2					39.7				
Hybrid ARM	7.7					26.2				
IO-ARM	11.3					13.1				
IO-Fixed	2.1					2.7				
Option-ARM	5.1					12.0				
Option-Fixed	0.3					1.4				
Owner	89.3					96.0				
Investor	7.1					2.6				
Vacation Home	3.7					1.1				
Purchase	51.9					49.0				
Low/no documentation	29.2					20.4				

Notes: These statistics were computed using a 10% random sample of the LPS data. Quantities obtained from the data are multiplied by a factor of 10. The percentages in panels (1) and (2) are taken with respect to the total number of modifications, and *not* loans modified. Thus, there is double-counting in the sense that some loans received multiple types of modifications in a given quarter.

Table VI
Modification Comparison by Payment Change

Private-label Modifications

	Payment Decreases					Payment Increases				
	#	mean		median		#	mean		median	
		$	%	$	%		$	%	$	%
2007:Q1	106	-614	-14.42	-162	-10.85	239	121	6.02	76	3.37
2007:Q2	110	-505	-12.02	-222	-9.30	364	168	7.96	76	3.49
2007:Q3	128	-261	-11.82	-131	-8.42	558	145	7.52	75	3.65
2007:Q4	288	-313	-13.38	-163	-12.36	741	125	6.24	74	3.52
2008:Q1	634	-393	-16.12	-261	-15.65	938	133	6.76	79	4.08
2008:Q2	1,014	-540	-18.94	-334	-17.89	1,241	152	8.14	83	4.08
2008:Q3	1,778	-641	-22.01	-423	-19.95	1,805	137	6.22	70	3.31

Portfolio Modifications

	Payment Decreases					Payment Increases				
	#	mean		median		#	mean		median	
		$	%	$	%		$	%	$	%
2007:Q1	28	-759	-20.90	-428	-17.19	128	106	7.78	52	5.46
2007:Q2	19	-1172	-25.17	-656	-28.07	222	81	6.11	55	5.28
2007:Q3	31	-395	-17.13	-168	-15.29	255	71	6.13	43	5.37
2007:Q4	90	-474	-11.11	-90	-2.48	292	70	5.50	37	4.29
2008:Q1	187	-369	-10.00	-183	-8.08	331	80	6.59	33	3.97
2008:Q2	309	-304	-10.90	-117	-6.64	405	63	5.59	34	3.56
2008:Q3	376	-585	-25.19	-295	-17.85	359	105	7.04	39	4.26

Table VII
Modifications (Main Sample)

Panel A: Unconditional Percentages

	Concessionary Mods	All Mods	All Mods + Prepayments
Portfolio	0.010	0.086	0.154
Private-label	0.018	0.087	0.159

Panel B: Logit Regressions (12 month horizon)

	Concessionary Mods	All Mods	All Mods + Prepayments
Private-label	0.006	0.010	0.021
	1.88	1.97	3.24
Initial Rate	0.002	-0.001	-0.004
	3.00	-1.14	-2.62
LTV Ratio	0.000	-0.001	-0.002
	2.44	-2.02	-2.86
LTV = 80	-0.001	-0.012	-0.035
	-0.79	-3.14	-7.78
FICO	0.000	-0.001	-0.003
	-0.88	-1.02	-4.00
$FICO^2$	0.000	0.000	0.000
	0.78	0.96	3.80
FICO < 620	0.009	0.058	0.060
	1.27	3.76	3.56
620 ≤ FICO < 680	0.005	0.022	0.024
	1.11	2.34	2.15
Log Original Amount	0.004	0.014	0.029
	2.33	4.00	6.41
Equity at Delinquency	0.000	-0.032	0.001
	-0.22	-2.58	0.12
Negative Equity	-0.004	-0.020	-0.034
	-0.58	-1.14	-1.32
Unemployment	0.000	-0.004	-0.007
	0.35	-3.64	-4.69
Refi	0.002	0.007	0.034
	0.95	1.89	6.22
Subprime	0.001	-0.018	-0.017
	0.23	-3.86	-2.70
Other Controls	Y	Y	Y
# Mortgages	28,574	28,574	28,574

Panel C: Duration Model

	Concessionary Mods	All Mods	All Mods + Prepayments
Private-label	0.91	1.00	1.02
	1.43	0.10	0.68
# Mortgages	77,676	77,676	77,676

Notes: Other controls include indicator variables for Jumbo, Option, Hybrid and Interest-Only mortgages, as well as for condos and multifamily homes. Panel B shows the marginal effects of logit regressions with a 12-month horizon, t-statistics shown below the coefficients. Standard errors are clustered at the zip code level. Panel C shows hazard ratio estimates from a Cox proportional hazards model.

Table VIII
Modifications (Robustness tests with alternative samples)

Panel A: Concessionary Modifications

	All Loans	Subprime	$FICO < 620$	Non-missing Documentation and DTI	Full Documentation
Portfolio Mean	0.010	0.014	0.012	0.008	0.008
Private-label Mean	0.018	0.020	0.022	0.017	0.019
Private Label Mg. Eff. (Logit)	0.006	0.004	0.004	0.007	0.009
	1.88	0.87	0.76	1.81	1.74
Private Label Haz. Ratio (Cox)	0.91	0.86	0.89	1.19	1.07
	1.43	1.56	1.05	1.99	0.63
# Mortgages	28,574	18,099	13,101	15,206	10,900

Panel B: All Modifications

	All Loans	Subprime	$FICO < 620$	Non-missing Documentation and DTI	Full Documentation
Portfolio Mean	0.086	0.069	0.080	0.086	0.076
Private-label Mean	0.087	0.092	0.104	0.093	0.104
Private Label Mg. Eff. (Logit)	0.010	0.010	0.017	0.014	0.027
	1.97	1.12	1.74	1.89	2.53
Private Label Haz. Ratio (Cox)	1.00	0.96	1.04	1.17	1.13
	0.10	0.69	0.55	3.28	1.94
# Mortgages	28,574	18,242	13,223	15,317	10,981

Panel C: All Mods + Prepayment

	All Loans	Subprime	$FICO < 620$	Non-missing Documentation and DTI	Fully Documented
Portfolio Mean	0.154	0.145	0.150	0.152	0.141
Private-label Mean	0.159	0.164	0.189	0.164	0.179
Private Label Mg. Eff. (Logit)	0.021	0.026	0.032	0.025	0.039
	3.24	2.47	2.61	2.68	3.13
Private Label Haz. Ratio (Cox)	1.02	1.01	1.06	1.13	1.12
	0.68	0.24	1.18	2.96	2.15
# Mortgages	28,574	18,242	13,223	15,317	10,981

Notes: Portfolio and private-label means are unconditional probabilities of modification in each sample. Marginal effects are computed from logit models with a 12-month horizon that include all the controls in Table VII. Standard errors are clustered at the zip code level. t-statistics are reported below the marginal effects.

Table IX
Modifications Conditional on 30 Days Delinquency (Logits)

Panel A: Concessionary Mods

	All Loans	Subprime	$FICO < 620$	Non-missing Documentation and DTI	Fully Documented
Portfolio Mean	0.007	0.005	0.005	0.005	0.005
Private-label Mean	0.010	0.011	0.012	0.010	0.011
Private Label Mg. Eff. (Logit)	0.001	0.005	0.005	0.003	0.004
	0.55	1.77	1.68	1.65	1.71
Private Label Haz. Ratio (Cox)	0.988	0.941	0.949	1.128	1.034
	0.39	1.16	0.94	2.86	0.62
# Mortgages	64,857	31,402	24,004	33,341	23,752

Panel B: All Mods

	All Loans	Subprime	$FICO < 620$	Non-missing Documentation and DTI	Fully Documented
Portfolio Mean	0.036	0.037	0.042	0.036	0.031
Private-label Mean	0.036	0.043	0.047	0.037	0.041
Private Label Mg. Eff. (Logit)	-0.002	-0.001	-0.001	-0.004	0.000
	-0.98	-0.20	-0.28	-1.60	-0.05
Private Label Haz. Ratio (Cox)	0.94	0.91	0.91	1.22	1.10
	1.04	1.08	0.94	2.75	1.01
# Mortgages	65,256	31,513	24,197	33,590	23,969

Panel C: All Mods + Prepayment

	All Loans	Subprime	$FICO < 620$	Non-missing Documentation and DTI	Fully Documented
Portfolio Mean	0.158	0.188	0.158	0.156	0.133
Private-label Mean	0.184	0.196	0.213	0.183	0.191
Private Label Mg. Eff. (Logit)	0.026	0.014	0.027	0.021	0.033
	6.28	1.80	3.08	3.65	4.29
Private Label Haz. Ratio (Cox)	1.12	1.00	1.09	1.16	1.20
	6.32	0.12	2.51	6.22	5.73
# Mortgages	65,256	31,513	24,197	33,590	23,969

Notes: Portfolio and private-label means are unconditional probabilities of modification in each sample. Marginal effects are computed from logit models with a 12-month horizon that include all the controls in Table VII. Hazard ratios are computed from Cox proportional hazard models with the same controls as in Table VII. z-statistics are shown below the coefficients, and t-statistics are reported below the marginal effects. Standard errors are clustered at the zip code level. Sample sizes refer to the logit regressions. The sample sizes for the Cox models are slightly larger.

Table X
Redefault Conditional on Modification

Panel A: Concessionary Mods

	All Loans	Subprime	$FICO < 620$	Non-missing Documentation and DTI	Fully Documented
Portfolio Mean	0.348	0.479	0.392	0.299	0.338
Private-label Mean	0.447	0.473	0.465	0.457	0.458
Private Label Mg. Eff. (Logit)	0.037	-0.037	-0.019	0.030	-0.001
	1.43	-0.84	-0.37	0.82	-0.01
# Mortgages	2,970	1,848	1,338	1,633	1,239

Panel B: All Mods

	All Loans	Subprime	$FICO < 620$	Non-missing Documentation and DTI	Fully Documented
Portfolio Mean	0.455	0.661	0.520	0.461	0.467
Private-label Mean	0.553	0.591	0.590	0.573	0.577
Private Label Mg. Eff. (Logit)	0.011	-0.066	-0.041	0.011	0.001
	0.71	-2.34	-1.52	0.50	0.02
# Mortgages	8,561	5,209	4,626	4,855	3,809

Notes: redefault is defined as loans that are 60 days delinquent, 90 days delinquent, in the process of foreclosure or in REO 6 months after the modification. Marginal Effects refer to the marginal effects of a logit model with a horizon of 6 months. t-statistics shown below the marginal effects. Standard errors are clustered at the zip code level.

Table XI
Cure Conditional on 60 Days Delinquency

	All Loans	Subprime	$FICO < 620$	Non-missing Documentation and DTI	Fully Documented	$FICO > 680$		
						Full Sample	Conforming	Non-conforming
Portfolio Mean	0.558	0.503	0.586	0.553	0.575	0.534	0.549	0.493
Private-label Mean	0.544	0.561	0.634	0.567	0.614	0.436	0.449	0.409
Private Label Mg. Eff. (Logit)	0.028	0.068	0.079	0.047	0.062	-0.034	-0.030	-0.046
	3.40	5.53	6.00	4.16	4.39	2.20	1.56	1.71
Private Label Haz. Ratio (Cox)	0.97	1.08	1.05	1.00	1.04	0.93	0.91	0.99
	1.53	2.71	1.59	0.20	1.61	2.29	2.42	0.27
# Mortgages	28,574	18,242	13,223	15,317	10,981	5,702	3,842	1,845

Notes: The dependent variable ("Cure") is defined as a loan that is either current, 30 days delinquent, or prepaid 12 months after the first 60-day delinquency. Portfolio and Private-label means are unconditional probabilities of modification in each sample. Marginal effects are computed from logit models with a 12-month horizon that include all the controls in Table VII. Standard errors are clustered at the zip code level. t-statistics are reported below the marginal effects.

Figure 2. Slide from a presentation by about the Indymac foreclosure prevention program.