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Abstract

Surprisingly little is known about the importance of mortgage payment size for default, as efforts to measure the treatment effect of rate increases or loan modifications are confounded by borrower selection. We study a sample of hybrid adjustable-rate mortgages that have experienced large rate reductions over the past years and are largely immune to these selection concerns. We show that interest rate reductions dramatically affect repayment behavior, even for borrowers who are significantly underwater on their mortgages. Our estimates imply that cutting a borrower's payment in half reduces his hazard of becoming delinquent by about 55 percent, an effect approximately equivalent to lowering the borrower's combined loan-to-value ratio from 145 to 95 (holding the payment fixed). These findings shed light on the driving forces behind default behavior and have important implications for public policy.

Key words: mortgage finance, delinquency, adjustable-rate mortgages, Alt-A

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1 Introduction

Measuring the relative importance of payment size and negative equity is a central question in the analysis of the mortgage default decision. Ongoing policy debates have pitted proponents of principal reductions who argue that only the latter matters against opponents who argue that monthly payment reductions are sufficient to prevent most defaults.[1] Early in the recent crisis, the dominant view was that the increase in foreclosures was almost entirely the result of rising monthly payments (for example, Bair 2007 and Eakes 2007). However, others such as Foote, Gerardi, and Willen (2012) have argued that payment increases of adjustable-rate loans were not a major driving factor behind the crisis, based on the fact that the number of defaults does not seem to react much even to large payment increases.

In this paper, we contribute to this debate by exploiting the resets of Alt-A hybrid adjustable-rate mortgages (ARMs) over the period 2008–2011. Hybrid ARMs have fixed payments for 3, 5, 7, or 10 years and then adjust annually or semiannually until the mortgage matures, meaning that the borrower's required monthly payment can adjust substantially at a particular moment in the life of the mortgage.[2] What makes our sample unique is that, because of the changed macroeconomic environment, required payments on most of these loans *fell* at the reset, often dramatically (see Panel A of Figure 1). This gives us an advantage over previous work, because, as we explain in Section 2, the prepayment option makes it impossible to use payment *increases* to measure the effects of payment changes on mortgage defaults.

We compare the performance of mortgages before and after payment reductions to the performance of otherwise similar mortgages that did not receive a contemporaneous payment reduction, because they had a different fixed payment period. Our analysis also uses variation in the size of payment reductions coming from different origination dates across loans. We find that payment reductions have very large effects. Panel B of Figure 1 plots the hazard of becoming 60-days delinquent for three types of loans as a function of the number of months since the origination of the loan. It shows the hazard for ARMs that reset in month 61 (5-year or "5/1" ARMs[3]) dropping from 1.7 percent in month 58 (three months prior to reset)

[1] A recent *Wall Street Journal* article illustrates the debate: "Economists are split. 'There's no question that in many cases, [principal forgiveness] is the only way to assure people will stay in the house,' says Kenneth Rosen of the University of California, Berkeley. Others say what really matters to borrowers is an affordable monthly payment. 'If people have a huge debt burden but the mortgage is not the problem, why are we reducing the mortgage?' asks Thomas Lawler, an independent housing economist in Leesburg, Va." ("How Forgiveness Fits in Housing-Fix Toolkit," p. A2, July 30, 2012)

[2] We restrict our sample to "interest-only" (IO) loans that do not amortize principal over the first ten years of their life. As discussed later, the IO feature was very common among Alt-A hybrid ARMs.

[3] We follow industry convention in referring to loans that reset after X years as "$X/1$" with the 1 referring to the annual frequency of subsequent adjustments, generating a slight abuse of terminology as a majority

to 0.5 percent by month 64 (after the reset). Payments for these borrowers had fallen on average by more than 3 percentage points, or 50 percent. The ARMs that reset after 7 or 10 years ("7/1+") and thus had not yet reset in our observation period display no similar drop around month 61. Until month 60, shortly prior to the reset, the hazard of the 7/1+ loans was lower than that of the 5/1s, but after the reset the default hazard comes in dramatically lower for the 5/1s.

While this figure is strongly suggestive, it obviously does not provide conclusive evidence on the strength of the effects of payment reductions. In the remainder of this paper, we use statistical techniques to show that the payment reductions indeed caused the changes in the default hazards. In particular, we control for the possibility that changes in macroeconomic conditions may differentially affect different loan types, and explore various alternative specifications to ensure that our effects are not driven by pre-reset trends or omitted variables. We robustly find a large effect of payment reductions.

To quantify the size of the effect, we focus on comparing the effects of an interest rate reduction with that of reducing a borrower's negative equity position (while holding the payment size constant). Our estimates indicate that a 2-percentage point reduction in the interest rate charged to a borrower has effects on the default hazard approximately equivalent, for instance, to reducing the borrower's combined loan-to-value ratio (CLTV) from 135 to 105.[4] A reduction of 4 percentage points or more, which indeed applied to about 20 percent of 5/1s in our sample, has approximately the same predicted effect on the delinquency hazard as a reduction in the CLTV from 155 to 80. As an alternative way to quantify the effect, our estimates imply that an interest rate decrease of 3 percentage points for a group of "typical" 5/1s at age 61 months (close to the mean reduction such loans actually experienced) with a CLTV between 130 and 140 reduces the number of delinquencies for these loans over the year after the reset by about 9 percentage points, or more than half.[5] This illustrates the broader and important finding that our estimated effects are similar if we look at only a subset of borrowers in our sample who are severely underwater. As we show, this is consistent with basic finance theory and goes against the intuition held by some commentators that once a borrower's mortgage is sufficiently far underwater, it is always optimal for him to default.

An interesting question is at what point in time the effects of a predictable interest rate

of the ARMs in our sample actually adjust every 6 months.

[4] A major advantage of the dataset we use over most of the previous literature is that for a large fraction of loans, we have updated information on the current CLTV, including all liens on a property.

[5] Such counterfactuals account for the fact that payment reductions also reduce the hazard of prepayment, as shown in Panel C of Figure 1. For underwater loans, the prepayment hazard is very low, so reductions in the default hazard translate almost directly into reductions in the number of defaults.

decrease actually occur. The default decision of a borrower who understands the terms of his mortgage, tracks the underlying index (for example, the six-month LIBOR), and is not liquidity constrained should not be affected by the actual occurrence of the reset. Instead, such a borrower would, in each period, consider the expected rate path over all future periods and decide accordingly whether default is optimal today, given his equity position and his expectations of future house prices. We find little evidence for effects of interest rate reductions on delinquency occurring much ahead of the actual reset, suggesting that either many borrowers do not actively anticipate the much lower rate they will be paying after the reset, or that they are so liquidity constrained that even foreknowledge of the reset cannot prevent them from defaulting if they are short of cash a few months before the reset occurs.

We also study the effect of a decrease in the interest rate on the probability that a loan that is at least 60-days delinquent "cures" (meaning that it becomes current again or pays off voluntarily). As Panel D of Figure 1 illustrates, we see large effects there as well, with the cure probability for 5/1s roughly doubling in month 63 relative to what it was before the reset.[6] Econometrically, we estimate that a 2-percentage point reduction in the interest rate increases the probability of cure by about 50 percent, which is comparable to the effect of reducing a loan's CLTV from 135 to 110.

In sum, all our evidence suggests that the size of the required monthly payment is an important determinant of mortgage delinquencies and cures for the borrower population we study.[7] This is not to say that a borrower's equity position is unimportant: in fact, we document very substantial effects of the CLTV on the likelihood of delinquency, and show that using the more accurate information on second liens available in our dataset leads to a higher estimated sensitivity of default to the level of negative equity.

Our results have important policy implications. A number of government-supported programs such as HAMP (Home Affordable Modification Program) and HARP (Home Affordable Refinance Program) attempt to reduce mortgage delinquencies and foreclosures by lowering the payments to "affordable" levels. However, empirical evidence on the success of such programs is scarce (for exceptions, see Adelino, Gerardi, and Willen 2009, Haughwout, Okah, and Tracy 2010, and Agarwal et al. 2011, who all study modifications, with a focus on

[6]The cure rate in this figure may seem low; this is due to the fact that the denominator includes all 60+ days delinquent loans that have not foreclosed yet (including those that are in the foreclosure process). In the appendix, we also look at newly 60-days delinquent loans separately, and find that those have a much higher cure rate (which, for 5/1s, goes from about 12 percent in month 50 to a high of 30 percent after the reset). It may also be surprising that the cure hazard falls again after month 63, but this can be explained by the fact that the borrowers most likely to cure when the rate is reduced do so relatively quickly.

[7]This is somewhat at odds with Amromin et al. (2010), who argue that for borrowers with "complex mortgages" (including interest-only mortgages, to which we restrict our sample), payment resets do not have important effects on delinquencies. However, over the period that they study, payment resets were relatively unimportant, unlike the case in our sample.

how payment reductions perform relative to principal reductions in affecting re-default rates) and somewhat difficult to interpret, because servicers and lenders *choose* the borrowers to whom they offer a modification or a refinancing (and on what terms).[8] As a consequence, it is very difficult to know to what extent any observed effect is driven by selection or treatment, and therefore one cannot reliably extrapolate the resulting estimates of intervention effectiveness to either larger-scale modification programs or policy interventions aimed at reducing delinquency in the first place.

The identification of the effects of payment reductions in our setting is cleaner in that regard, as the payment reduction for borrowers with a certain mortgage type at a certain loan age is *unconditional* on any other borrower covariates that may have changed since origination. Absent the ideal scenario of completely randomized payment reductions—which unfortunately have not occurred—this seems to provide as good a natural laboratory to look at the effects of substantial payment reductions as we can think of. On the other hand, the Alt-A hybrid ARM borrower population we focus on is obviously not necessarily representative of the broader market. That said, contemporaneous work by Tracy and Wright (2012) documents similar effects of interest rate reductions on the delinquency rates of ARM borrowers in the prime segment.[9] Zhu (2012) studies HARP refinances using internal Freddie Mac data, and also finds that these payment-reducing refinances substantially lower subsequent default rates.

This paper is organized as follows: in the next section, we discuss the effects of payment size and negative equity on mortgage defaults from a theoretical perspective, and the difficulties one faces when trying to cleanly identify these effects empirically. Section 3 describes the empirical methods and data we use, while Section 4 presents the results from our analysis. Section 5 first discusses what our results tell us about the driving forces behind defaults, and then moves on to policy implications. Section 6 offers a brief conclusion.

[8] Also, servicers tend to only modify delinquent mortgages; it is not clear to what extent payment reductions reduce the default probability of nondelinquent borrowers, which are the target of HARP.

[9] Tracy and Wright's main goal is to predict the effects of a large-scale refinancing program such as HARP on subsequent defaults and credit losses. Among other differences, Tracy and Wright do not focus on large payment resets as we do, and do not explicitly discuss the relative importance of liquidity versus negative equity in causing defaults. (As they are using the LPS data, measuring the precise extent of negative equity is difficult, as that dataset contains no information on second liens.)

2 Theoretical considerations and identification

2.1 Why payment size would matter for default

In theoretical analyses of mortgage delinquency, researchers (for example, Deng, Quigley, and Van Order 2000; Schelkle 2012) typically distinguish between a "frictionless" model in which households are assumed to be able to borrow freely at the risk-free rate and default is completely costless, and more realistic models in which borrowers are constrained and subject to shocks, or where there are costs to defaulting beyond the loss of the house.

In both types of theory, negative equity is a necessary but not sufficient condition for default. Likewise, payment size matters in both worlds. In a frictionless world, the payment matters because it affects the total discounted cost of the mortgage (which the rational borrower compares to the expected net present value of the house). In the appendix, we present a barebones, frictionless model that demonstrates the following points: first, mortgages can usefully be thought of as call options on a call option on the house. Second, negative equity is basically never sufficient for default to be optimal except in a situation where a borrower with negative equity at time t also has negative equity for all $s > t$ along every possible path for prices. Unless that is the case, there is always a monthly payment low enough such that it is optimal for the borrower to not default; therefore, payment reductions should lower default rates. Importantly, this statement holds even if the borrower is free to default and then repurchase the same house at a lower price, as long as there is no arbitrage in the economy.[10] And third, we show that changes in the size of the monthly payment affect repayment behavior more when borrowers have negative equity than when they have positive equity (as it is never optimal for a borrower with positive equity to default; he is better off selling the house).

Frictions such as borrowing constraints and income shocks make the analysis more complicated and less elegant than the option-theoretic frictionless case. The economic mechanism in such models (for example, Campbell and Cocco 2011) is typically that borrowers get hit by "liquidity shocks" that make them more impatient (for example, they lose their job or are subject to large medical expenses and cannot borrow sufficient amounts to smooth their consumption) such that the effective cost of having to make a payment today weighs more than the expected future value of the option on the house. This model is often referred to as "double trigger," because the combination of negative equity and some shock drives

[10]The intuition for this result is that if it is optimal for the borrower to exercise his option by making the payment, the overall expected value of the call option on the house must be positive, while any new option the borrower could acquire in the market must ex ante have zero net expected value, as otherwise it represents an arbitrage opportunity.

defaults. In such a world, having to make a smaller monthly payment makes it less likely that for a shock of a given size, a borrower finds it optimal to default (or alternatively, the shock size that makes defaulting optimal increases). The further underwater a borrower is, the lower the payment that makes it worthwhile for him to continue paying after being hit by a shock, but generally there again exists a payment size sufficiently low so that it remains in the borrower's interest to keep making payments rather than default.[11] Payment reductions should, therefore, again affect the default decision for any level of negative equity; whether these reductions have larger effects the more negative equity a borrower has is theoretically ambiguous.

2.2 Why we don't know how much payment size matters for default

The proposition that researchers do not really know the precise effect of payment size and affordabilty more broadly on the decision by a borrower to default on a mortgage may seem surprising, given the attention to the topic of mortgage default in recent years and the wealth of data on the subject provided by the worst foreclosure crisis in U.S. history.[12] In this section, we first review why it is so difficult to identify these effects and explain why the resets of hybrid ARMs since 2008 present a unique opportunity to address the question.

One could try to identify the effect of payment size on delinquency simply by exploiting the sizable heterogeneity among the monthly payments required of borrowers at a given point in time. However, such an analysis would be plagued by very serious selection concerns: lenders may require some borrowers to pay a higher interest rate precisely because these borrowers are at a higher risk of default (so the lender requires a risk premium), or conversely, some borrowers may be willing to pay more points upfront to reduce their interest rate because they plan to stay in their house for a longer time. An alternative strategy one might consider would be to rely purely on time-series variation in interest rates, but such an analysis would be confounded by the fact that economic conditions also vary over time and may affect default rates.

When thinking about the effects of affordability, or liquidity shocks, summary variables that are often considered are the payment-to-income or the broader debt-to-income ratios. A logical strategy to study the link between affordability (or liquidity) and default would

[11] We say "generally" because one can, of course, imagine extreme shocks and borrowing constraints such that a borrower prefers to spend all his money on food rather than make even a very low mortgage payment.

[12] Of course, researchers have been trying to quantify the relative importance of negative equity and affordability for mortgage default since much before the recent crisis—see, for example, von Furstenberg (1969) or Campbell and Dietrich (1983) for important early contributions.

therefore be to look at income shocks, in particular those due to unemployment. However, this is again a difficult endeavor: while large fractions of respondents in surveys of delinquent borrowers report suffering shocks, including spells of unemployment and illness (for example, Cutts and Merrill 2008), to identify the effects of those shocks we would need similar survey data on the population of nondelinquent borrowers. Unfortunately, no publicly available dataset combines detailed information on mortgage performance and employment histories.[13]

Recently, researchers have gained access to matched samples of credit bureau data and loan-level mortgage data. While these datasets also do not allow direct observation of income shocks, they do contain updated information on the status and availability of credit. Elul et al. (2010), for example, use borrowers' bankcard utilization rates as a proxy for their liquidity constraints, and find that these have a statistically and economically significant effect on the likelihood of default, especially for borrowers with high CLTV ratios (consistent with double trigger theories). While this is a very interesting approach, it does not directly identify the effects of liquidity shocks, as borrowers' bankcard utilization rates are to some extent endogenous to their behavior and their type.

Where does this leave us? Ideally, one would have a randomized experiment in which some mortgage borrowers are required to make lower payments than others.[14] As far as we know, such data are not available, so we rely on perhaps the next best thing: a situation in which different borrowers' payments adjust at different times and by different amounts, depending on when they took out their mortgage and exactly what type of mortgage they got, but not conditional on their current equity position or other characteristics that may have changed since origination. Such a situation is provided by hybrid ARMs with different fixed-rate periods, different reset times, and different index rates.[15] One needs not only any type of resets, however, but *downward resets*: as we explain more formally now, the prepayment option makes it impossible to use upward resets to reliably estimate the causal

[13]The loan-level servicer and trustee data used by researchers to study mortgage performance typically only provide information about the borrower's income at the time of origination of the loan. The Panel Study of Income Dynamics (PSID) or the Survey of Income and Program Participation (SIPP) provide data on employment and mortgage default at annual frequencies but contain no information on the timing of default, the terms of the mortgage, or precise information on the location and value of the property. Hsu, Matsa, and Melzer (2013) use the SIPP to document that an increase in a state's unemployment insurance generosity significantly lowers the likelihood of mortgage delinquency and eviction of unemployed homeowners, consistent with liquidity being an important driver of default.

[14]For an example of such an experiment in the context of unsecured micro-credit, see Karlan and Zinman (2009).

[15]An alternative clever identification strategy is used by Anderson and Dokko (2011), who exploit random variation in the due date of property taxes to study the causal effect of liquidity reductions on early payment default of subprime borrowers. In a different context, Almeida et al. (2012) use ex-ante variation in the maturity of long-term debt (similar to the ex-ante variation in reset dates used here) together with the disruption in credit markets in the third quarter of 2007 to measure the causal effect of financial contracting on firm behavior.

effect of payment size on defaults.

2.2.1 Selection versus treatment effects

Consider a situation with a continuum of borrowers divided into two types $i \in \{g, b\}$ for good and bad, with prepayment and defaults hazards of p_t^i and d_t^i, respectively. We assume that $d_t^b > d_t^g$, that is, the bad types default more. We make no similar assumption about prepayment. At time t the share of bad borrowers is σ_t, meaning that the prepayment and default hazards in the population are $p_t = \sigma_t p_t^b + (1-\sigma_t)p_t^g$ and $d_t = \sigma_t d_t^b + (1-\sigma_t)d_t^g$, respectively.

Consider a reset that occurs at time $t+1$ and assume that it affects the default hazard multiplicatively so that $d_{t+1}^i = \phi d_t^i$ for both types of borrowers. In case of an upward reset we expect $\phi > 1$, while for a downward reset we expect $\phi < 1$. The goal of this paper is to estimate ϕ, but the challenge we face is that we cannot tell the two borrower types apart and can only observe d_t and d_{t+1} (as well as p_t and p_{t+1}).

Two equations illustrate the key identification issues in estimating the effect of resets on the default hazard. The first shows the treatment and selection effects on the change in the default hazard:

$$\frac{d_{t+1}}{d_t} \equiv \hat{\phi} = \underbrace{\phi}_{\text{Treatment effect}} \left[\underbrace{1 + \frac{(\sigma_{t+1}-\sigma_t)(d_t^b - d_t^g)}{\sigma_t d_t^b + (1-\sigma_t)d_t^g}}_{\text{Selection effect}} \right]. \tag{1}$$

Clearly, the treatment effect will be overestimated by $\hat{\phi}$ if $\sigma_{t+1} - \sigma_t > 0$, that is, if the share of bad borrowers is larger after the reset than before.

The second key equation is the law of motion for the share of bad borrowers in the population:

$$\sigma_{t+1} - \sigma_t = \frac{\sigma_t(1-p_t^b-d_t^b)}{\sigma_t(1-p_t^b-d_t^b) + (1-\sigma_t)(1-p_t^g-d_t^g)} - \sigma_t,$$

which we can re-write as:

$$\sigma_{t+1} - \sigma_t = \sigma_t \left[\sigma_t + (1-\sigma_t) \cdot \frac{1-p_t^g-d_t^g}{1-p_t^b-d_t^b} \right]^{-1} - \sigma_t. \tag{2}$$

This expression will be positive if $p_t^g + d_t^g > p_t^b + d_t^b$, that is, if a larger fraction of good borrowers than bad borrowers leaves the population during period t.

This illustrates why it is difficult to get an accurate estimate of ϕ from looking at upward resets: a large fraction of good borrowers typically prepays before or at the reset (p_t^g is high), meaning that the quality of the borrower pool is lower after the reset, and as a consequence $\hat{\phi} > \phi$.[16] In other words, the increase in the default hazard after the reset that is typically observed in the data (for example, Ambrose, LaCour-Little, and Huszar 2005; deRitis, Kuo, and Liang 2010; or Pennington-Cross and Ho 2010) confounds the treatment effect of higher payments with the selection of higher-quality borrowers into prepayment.

This simple model also shows how selection can explain why researchers (for example, Sherlund 2008 or Foote, Gerardi, and Willen 2012) found that when rates went up, the *number* of defaults stayed relatively constant, and why this does not teach us much about the effect of the reset on the *hazard* of default. Essentially, even if the hazard of default goes up at the reset, the number of borrowers at risk falls, and these two effects can cancel out. Letting D_t be the number of defaults at time t, we have

$$\frac{D_{t+1}}{D_t} = \phi \left[1 - \frac{d_t^b \sigma_t (p_t^b + d_t^b) + d_t^g (1 - \sigma_t)(p_t^g + d_t^g)}{d_t^b \sigma_t + d_t^g (1 - \sigma_t)} \right].$$

The expression in brackets is smaller than 1, and potentially substantially so, for instance if p_t^g and $1 - \sigma_t$ are large (a large fraction good types prepay before the reset and these good types represent a significant part of the borrower population). As a consequence, even with ϕ much larger than 1, it is possible that the number of defaults stays relatively constant around the reset. This is discussed in more detail in appendix A.3, which relates our findings to earlier work arguing that upward resets of ARMs were not a major contributor to the mortgage crisis.

The downward resets we consider in our empirical analysis are not subject to the selection problem of good types avoiding the payment reset, but selection remains a potential issue. In this case, we are most concerned with selection that would lead us to estimate $\hat{\phi} < \phi$, meaning that our estimates would exaggerate the extent to which lowering the interest rate reduces the default hazard. Thus, going back to equation (1), we are concerned with situations in which $\sigma_{t+1} - \sigma_t < 0$, that is, the share of bad borrowers is lower after the reset than before.

As we show in our empirical analysis, the reset affects both default and prepayment behavior. Consider our law of motion for the share of bad borrowers and assume that the reset affects the prepayment hazard by a factor π and the default hazard again by a factor ϕ:

$$\sigma_{t+1} - \sigma_t = \sigma_t \left[\sigma_t + (1 - \sigma_t) \cdot \frac{1 - \pi p^g - \phi d^g}{1 - \pi p^b - \phi d^b} \right]^{-1} - \sigma_t. \tag{3}$$

[16] Of course, upward resets need not lead to an increase in prepayments, if the rate that prepaying borrowers could get on a new loan is not sufficiently below the rate on their current loan after the reset to make it worthwhile to incur the transaction cost. However, initial rates on new ARMs (or FRMs) have historically been sufficiently low to cause large spikes in prepayment rates when the rate reset up.

Suppose we assume that initially π and ϕ equal 1 and there is a constant termination hazard for both types, meaning that $p^g - d^g = p^b - d^b$ (omitting time subscripts for simplicity). How do shocks to π and ϕ affect the evolution of the share of bad types?

$$\left.\frac{\partial(\sigma_{t+1} - \sigma_t)}{\partial \pi}\right|_{p^b+d^b=p^g+d^g,\pi=\phi=1} = \frac{\sigma(1-\sigma)(p^g - p^b)}{1 - p^b - d^b} > 0 \tag{4}$$

$$\left.\frac{\partial(\sigma_{t+1} - \sigma_t)}{\partial \phi}\right|_{p^b+d^b=p^g+d^g,\pi=\phi=1} = \frac{\sigma(1-\sigma)(d^g - d^b)}{1 - p^b - d^b} < 0 \tag{5}$$

What this means is that if the reset lowers the probability of prepayment, then it reduces the growth in the number of bad types; if it lowers the probability of default, then it increases the growth in the number of bad types. In other words, a reduction in the prepayment hazard due to the reset may lead us to overstate the effect of the reset on the default hazard, while a reduction in the true default hazard may attenuate our estimate of the effect of the reset on the default hazard towards 1.

We argue that while we cannot show theoretically that our estimated treatment effect is perfectly unbiased, it is unlikely that selection effects lead to exaggerated estimates of the effect of the downward resets in our setting. First, since the default hazard is higher (and in some cases much higher) than the prepayment hazard around the resets in our data, we would generally expect the net effect of the reset to *increase* the growth in the number of bad types in the population, meaning that the bias would attenuate our estimated effect. Second, a subsample of the borrowers in our sample is so deeply underwater that the prepayment hazard before and after the reset is practically zero. This means that any effect of π is effectively turned off, and as a consequence, the average quality of loans in this subsample, if anything, worsens after the reset relative to what would have happened without the reset, and we may underestimate the true change in ϕ.

In sum, the loans in our sample offer a unique combination. Since payments are *falling*, the borrowers in our sample have little incentive to refinance around the reset. Furthermore, a majority of the borrowers in our sample are deeply underwater and therefore unable to refinance or sell, even if they wanted to do so for reasons unrelated to interest rates. As a consequence, the borrower population in our sample stays approximately fixed around the reset, and our analysis is thus much less plagued by potential selection effects than if we studied upward resets.

3 Empirical methods

In the remainder of the paper, we estimate versions of the equation

$$y_{it} = \mathrm{F}\left(\beta_1 X_{1,i} + \beta_2 X_{2,t} + \beta_3 X_{3,it} + \gamma R_{it} + \epsilon_{it}\right), \tag{6}$$

where y_{it} is the probability of default, prepayment, or cure.[17] The index i denotes a loan and t is a measure of time (which will correspond to the number of months since origination for delinquency or prepayment, or to the number of months since becoming delinquent for the cure analysis). The X variables are our explanatory variables: $X_{1,i}$ is a vector of characteristics of the loan at origination; $X_{2,t}$ are macroeconomic variables like the unemployment rate; and $X_{3,it}$ are time-varying, borrower-specific variables like the amount of equity the borrower has in the property. Our main focus is on R_{it}, the interest rate faced by borrower i at time t, which we measure relative to the initial interest rate on the loan for delinquency and prepayment, or relative to the interest rate at the time when the borrower became delinquent for the cure analysis. In this section, we discuss key details of our various regression specifications including the data we use, the precise specification of the model, and the timing of the relevant variables.

3.1 Measuring defaults, prepayments and cures

As mentioned, we consider three different outcome variables: default, prepayment, and cure.

We define default as occurring when the servicer reports a borrower as 60-days delinquent using the MBA (Mortgage Bankers Association) definition of delinquency.[18] Specifically, the MBA definition says that a borrower's delinquency status increases by 30 days every time the borrower fails to make a scheduled payment before the next payment is due. For example, using the MBA method, a servicer would report a borrower who is current, has a payment due on June 1 and makes no payments in June as current in June and 30-days delinquent in July. Depending on whether the borrower makes no payment, one payment, or two payments in July, he will transition to 60 days, stay at 30 days or become current, respectively. This means that a borrower who resumes making payments but fails to make up missed payments can remain in a particular delinquency bin indefinitely. Historically, and even in this crisis, borrowers who become 30-days delinquent are very likely to become current again and thus

[17] We also consider the probability of modification for delinquent loans (as modification is a competing hazard for cure). However, as we do not emphasize these results in the main text, we omit modifications from the discussion in this section.

[18] There is an alternative delinquency definition, the OTS/FFIEC rule. See www.securitization.net/pdf/content/ADC_Delinquency_Apr05.pdf for details.

30-day delinquency is not considered evidence of serious stress. On the other hand, in our data, 83 percent of borrowers who become 60-days delinquent also enter foreclosure over the sample period, meaning that 60-day delinquency is a good indicator of serious stress.

A prepayment occurs when borrower repays the loan in full. In our framework, default and prepayment are competing risks, meaning that a borrower who has prepaid cannot default and a borrower who has defaulted cannot prepay. This means that for the purpose of our delinquency analysis, a loan "dies" the first time it becomes 60-days delinquent.

Such loans that are at least 60-days delinquent do not become irrelevant for us, however, because we consider them separately in our cure analysis. For our purposes, cure of a delinquency occurs when the servicer reports the borrower as current or prepaid after the borrower has become 60-days delinquent. Note that to cure, the borrower cannot simply resume making scheduled payments but must remit all the missed payments as well. In other words, to go from 60-days delinquent to current in one month, a borrower must make three payments.

3.2 Data

We use a sample of 221,000 Alt-A, interest-only (IO) adjustable-rate mortgages (ARMs) originated between January 1, 2005 and June 30, 2006. The sample comes from the CoreLogic LoanPerformance (LP) dataset, which contains data on pools of loans sold in the private-label securitization market, meaning that the loans were not insured by Fannie Mae, Freddie Mac, or the Federal Housing Administration. We now describe the data in detail, discuss the reasons for our sample restrictions, and provide some descriptive statistics.

The LP dataset includes basic origination information, including borrower FICO score, the zip code of the property, the terms of the loan, the original loan-to-value ratio, whether the loan was used to purchase a property or to refinance another loan, and whether the borrower plans to occupy the property or is an investor.[19]

For ARMs, the information about the terms of the loan includes the number of months after which the loan resets for the first time, the frequency of subsequent resets (6 or 12 months), the interest rate to which the loan is indexed,[20] the margin over the index rate,[21]

[19] Information about whether the borrower is an investor is generally not considered highly reliable; as Haughwout et al. (2011) document, many investors pretended to be occupying the property underlying the mortgage, presumably in order to obtain more favorable loan terms. That said, Alt-A mortgages are traditionally considered an "investor product," so misrepresentation in our sample may be less severe than in the subprime segment.

[20] In our sample, 69 percent of loans are indexed to the 6-month LIBOR, 29 percent to the 1-year LIBOR, and 2 percent to the 1-year Treasury bill rate.

[21] The most common margins in our sample are 225 basis points (71 percent of loans) and 275 basis points (19 percent of loans). Almost all of the remaining loans have margins between 250 and 500 basis points. After

and bounds on the admissible level of or changes to the interest rate (commonly referred to as "caps" and "floors").[22] The information provided is (nearly) sufficient to predict the evolution of the interest rate as a function of the index to which it is linked.[23]

Most importantly for us, the LP dataset contains dynamically updated information on a loan's current interest rate and delinquency status, and flags for loan modifications, allowing us to distinguish scheduled changes to the terms of a loan from unscheduled ones.

Our version of LP also includes a new, dynamically updated measure of a borrower's leverage in the property, which should be much more accurate than the information that has traditionally been used in mortgage research. Since the dataset was created 20 years ago, LP has reported the origination LTV of most loans. For a subset of loans, LP also reported the initial CLTV which took into account second liens taken out at origination (known as "piggy-backs"). LP traditionally did not update LTV or CLTV over time to account for new liens or for changes in the price of the house.[24] However, LP recently augmented their data with a new measure of updated CLTV called "TrueLTV." TrueLTV uses information from state-level public records databases to measure all liens, including both simultaneous second liens taken out at origination and second liens taken out later, and uses an automated valuation model to update the value of the property. The TrueLTV of a mortgage is updated either monthly (for most loans) or annually but is not available for all loans (and, even within a loan, not necessarily for all months); as a consequence, we also compare the predicted effects of CLTV on delinquency with the predicted effect from an alternative measure that updates the LTV of a first-lien loan based on local house price changes, as is frequently done in the literature (for example, Bajari, Chu, and Park 2008; Elul 2011; Tracy and Wright 2012).[25] With the exception of Goodman et al. (2010), we are, to the best of our knowledge, the first

adding the margin to the index rate, the interest rate is rounded to the nearest one-eight of one percentage point. The margins on subprime loans made over this period were typically substantially higher, with an average around 600 basis points (Demyanyk and Van Hemert 2011).

[22] An example from an actual loan contract: "The interest rate I am required to pay at the first Change Date will not be greater than 12.625% or less than 2.25%. Thereafter, my interest rate will never be increased or decreased on any single Change Date by more than Two percentage points (2%) from the rate of interest I have been paying for the preceding six months. My interest rate will never be greater than 12.625%."

[23] The only piece of information that is missing is the exact date at which the index rate is taken to determine the borrower's subsequent interest rate after the reset. Typically, this is the first business day of the month prior to the reset date, but we have also encountered loan contracts where the relevant index rate was measured 45 days prior to the reset date. When imputing interest rates (or forecasts thereof) we assume that the relevant index rate is taken as of the first business day of the month.

[24] The same is true for other popular datasets used in this literature, such as the one provided by LPS Applied Analytics (formerly known as "McDash"), which furthermore does not contain information on initial CLTVs (when there are multiple liens) for any loans.

[25] We use the CoreLogic zip-code-level indices based on sales of nondistressed properties.

to use TrueLTV for academic research.[26]

The LP dataset contains both Alt-A and subprime mortgages; we focus on Alt-A rather than subprime because subprime ARM contracts typically contained floors such that the interest rate could not go lower than the initial rate (Bhardwaj and Sengupta 2011). Alt-A mortgages are also referred to as "near prime" and are linked to borrowers who are characterized by either minor credit quality issues or an inability or unwillingness to provide full documentation of income and assets. Adelson (2003), Mayer, Pence, and Sherlund (2009), and Sengupta (2010) provide an overview of the Alt-A market and how it compares to subprime; we also discuss some comparisons below.

We focus on 30-year hybrid ARMs with fixed-rate periods of 3, 5, 7, or 10 years and a 10-year interest-only (IO) feature, originated between January 1, 2005, and June 30, 2006. An IO period means that over that time, the borrower only pays interest, without amortizing the mortgage. This leads to an initially lower monthly payment, but allows the borrower to avoid building equity in the property.[27] We study IO mortgages because for these loans the interest rate change directly corresponds to the payment change, and an interest rate decrease of a given magnitude will have the largest impact on the payment. For instance, payment reductions of regular amortizing 5/1s are not all that large, because after 5 years a substantial part of the payment is principal amortization. Also, we choose mortgages with the 10-year-IO feature because 5-year-IO mortgages (which are also quite popular) start amortizing after 5 years and so may in fact see payment increases even if the interest rate resets substantially lower. Finally, the 10-year-IO feature was very popular among Alt-A hybrid ARMs.[28]

Our origination date range was chosen for two reasons. First, when these loans reset, the majority of them see large reductions in interest rates, as the 6-month and 1-year LIBOR as well as the constant-maturity 1-year Treasury bill rate, to which these loans are indexed when they reset, have been very low since early 2009 (see Panel A of Figure 2) after an initial drop in early 2008. Second, for those that have reset (the 3/1s and 5/1s), we have at least

[26] Elul et al. (2010) and Bond et al. (2013) use updated CLTV estimates based on information on second liens from credit bureau data.

[27] Barlevy and Fisher (2010) argue that IOs are the perfect product to speculate during a bubble. An alternative type of "exotic" mortgage that was popular during the boom years was the negative amortization or "option" ARM, which allowed the borrower to make less than his scheduled monthly payment and to add the difference to the mortgage balance. This feature was mostly combined with otherwise regular amortization, that is, non-IO ARMs.

[28] Among hybrid ARMs with fixed-rate periods of three years or longer originated in 2005/6, the share of 10-year-IOs is approximately 59 percent. About 20% were 5-year-IOs, while only about 13% were regularly amortizing over the full loan term. Overall, the value-weighted shares of Alt-A originations over 2005/6 are approximately as follows: Fixed-rate mortgages (FRMs) 29 percent, amortizing ARMs (mostly without an initial fixed-rate period) 37.5 percent, 5-year-IO ARMs 8.5 percent, and 10-year-IO ARMs 21.3 percent, with the rest going to ARMs with different or unknown IO periods, or balloon mortgages.

an additional five months of performance data (unless they prepay or foreclose, of course).

We retain only first-lien mortgages on single-family homes, condominiums, and townhouses, with origination amounts between \$40,000 and \$1,000,000 (roughly corresponding to the 1st and 99th percentile of our initial sample) and with an origination LTV between 20 and 100 percent. Also, we restrict our sample to loans that enter the dataset within six months of origination, in order to minimize selection bias. This leaves us with a total of 221,561 loans, of which about 59 percent are 5/1s, 18 percent are 10/1s, 16 percent are 3/1s, and 7 percent are 7/1s. As of November 2011, the last month of performance data in our sample, slightly fewer than one-third (70,422) of our original loans were still open (they had not prepaid and were not currently in foreclosure).

Panel B of Figure 2 shows the distribution of interest rate changes at the first reset, as well as subsequent resets. For the 5/1s, almost two-thirds of loans saw a reduction of 3 percentage points or more at the first reset (with the heterogeneity mostly due to differences in floors and caps, as well as the initial rate). Subsequent resets for these loans tended to be small. For the 3/1s, the pattern is somewhat different, as only about 20 percent of those loans saw interest rate reductions of 2 percentage points or more at the first reset (which happened between January 2008 and June 2009), but subsequent resets tended to be more substantial than for the 5/1s (as the index rates kept decreasing).

Table 1 shows other basic information about our sample. Panel A shows that the market for Alt-A hybrid ARMs grew markedly over our sample, with 88,500 loans originated in the first half of 2006 compared with only 60,400 in the same period one year earlier. The mix of loans changed considerably as well, with a doubling in the origination of longer-duration ARMs offsetting a more than halving of the market for 3/1s.[29]

Panel B shows some key statistics about characteristics at origination. Overall, borrowers in our sample were highly levered and unlikely to provide full documentation when obtaining their mortgage. More than half the involved properties were located in the so-called sand states: Arizona, California, Florida, or Nevada. The average FICO score for all loan types is above 710, which is close to the median FICO score in the U.S. population. Compared with subprime loans originated around the same time, the characteristics of which are summarized in Mayer, Pence, and Sherlund (2009), the loans in our sample are similarly highly levered, have higher FICO scores (the median FICO score of subprime loans in this period was around 617), are more likely to provide low or no documentation (subprime: 37 percent), are more likely to be on (declared) investor or second home properties (subprime: 7 percent), are

[29] The change in market shares of the different types over the origination period is most likely due to changes in the term structure of interest rates during this time: the yield curve flattened substantially over the course of 2005, making long-duration loans relatively cheaper and thus prompting borrowers to move into hybrid ARMs with longer fixed-rate periods (or into FRMs).

more likely to be purchase loans (subprime: 41 percent), and are less likely to be subject to a prepayment penalty (subprime: 71 percent).[30] Comparing the loans with different fixed-rate periods in our sample, one notices that the 10/1s, although larger, are somewhat less risky than the others, based on characteristics at origination.

Panel C shows that by the beginning of 2008, most borrowers in the sample had negative equity and by 2010 and especially 2011, were deeply underwater with a mean CLTV of 145 percent for the whole sample.[31]

The Alt-A ARMs originated in 2005 and 2006 are an exceptionally troubled group of loans. Panel D shows that by November of 2011, lenders had foreclosed or arranged for short sales on more than a third of the loans. The 5/1 and 7/1 ARMs performed significantly worse than either the 3/1s or the 10/1s. As we discuss below, the stronger performance of those loans reflects the earlier resets for the 3/1s, as well as the better initial credit quality for the 10/1 borrowers, who had both higher credit scores and significantly more equity at and after origination than the rest of the sample.

3.3 Econometric methods

3.3.1 Default and prepayment

As is standard in the literature, we conduct a competing risk analysis of prepayment and default (for example, Deng, Quigley, and Van Order 2000; Foote et al. 2010; Krainer and Laderman 2011).[32] We use a Cox proportional hazard framework, which asserts that the hazard rate of borrower j at loan age t for outcome $n \in$ (default, prepayment) is given by

$$h^n(t|\mathbf{X}_{it}) = h_0^n(t) \cdot exp(\mathbf{X}_{it}\beta^n), \tag{7}$$

where $\mathbf{X}_{it}$ is the vector of borrower-specific controls (some of which are time-varying, such as the current CLTV and the interest rate). The baseline hazard $h_0(t)$ for both outcomes is unrestricted, and we typically let it vary by origination quarter of the mortgage (that is, we have six different baseline hazards) in order to pick up differences in underwriting standards or other unobservables. The default and prepayment hazards are assumed to be independent.

Our population of interest is loans that are either current or 30-days delinquent. When estimating the Cox model for delinquency, we treat mortgages that prepay as censored, and

[30] For about 95 percent of loans with a prepayment penalty, the penalty applies for three years or less, with three years as the modal length.

[31] This includes delinquent loans. Among loans that are current, the mean CLTV in November 2011 was 137.5, with a median of 129.6.

[32] Classic analyses of the prepayment decision include Schwartz and Torous (1989) and Stanton (1995).

vice-versa.[33] Importantly, we also treat as censored mortgages that are subject to an interest rate *increase*; as explained in Section 2, such upward resets give rise to potentially important selection biases. That said, in our data only about 15,000 loans ever see their interest rate increase, most of them at either age 37 or 43 months (for 3/1s) or 67 months (for 5/1s, which, at age 61 months, see large decreases).

In our baseline specification, we only retain loans that are older than 30 months. This is because we are primarily interested in the effects of rate resets, which start occurring in month 37, and we want to avoid that the "comparison group" of loans with no reset is dominated by young loans, where the relative performance of different loan types may be different from the relative performance just before the reset. That said, we will see later that not imposing this restriction, or instead looking only at a narrow time window around the reset, leaves the results almost unchanged, suggesting that this issue is of minor importance in our sample.

3.3.2 Cures

To study the determinants of cures of delinquent mortgages, we again use a Cox proportional hazard model, this time on only the population of loans that is 60+ days delinquent. The possible competing outcomes are now $n \in$ (cure, foreclosure, modification); we discuss mainly the results on the cure hazard. The index t now refers to the number of months a loan has been delinquent, since the number of periods a loan has been in delinquency will strongly affect the likelihood that it cures. We additionally add dummies for each loan age to allow for age dependence in the likelihood of cures. We retain all "delinquency episodes" that start with a 60-day delinquency; if a borrower cures after his first episode, he may appear in more than one episode. Consequently, we cluster the standard errors at the loan level (while for the delinquency and prepayment analysis, we cluster at the state level).

In addition to the Cox models, we also run linear probability models on the determinants of cures for (only) newly 60-days delinquent loans. The results, which are given in the appendix, provide an alternative perspective on the size of the effects, test for forward-looking effects, and allow easy comparative statics exercises.

[33] In both cases, we also treat mortgages as censored when they are marked in the LP data as being subject to a loan modification, which often leads the mortgage to become a fixed-rate mortgage or to reset at different dates than those specified in the original contract. Only about 3,000 uncensored loans in our data are modified while current or 30-days delinquent.

3.4 Control variables

Panel B of Figure 3 shows the main borrower-specific control variables that we include in our regressions. Our main variables of interest are the borrower's current interest (relative to his initial rate) and his updated CLTV. To allow for nonlinear effects in a parsimonious and easy-to-interpret manner, we use indicators for bins of values these variables take. We also include bins of the interest rate at origination (of width 25 basis points) to control for initial differences in payment size, as well as the interest rate spread at origination (SATO, calculated relative to the median rate of loans of the same type originated in the same month) and original LTV to account for potential selection effects. For the same reason, we add loan type dummies (3/1, 5/1, 7/1+), and we also interact these with calendar quarter dummies, to account for the possibility that borrowers in different loan types are affected differentially by changing economic conditions.

In addition, we include the FICO score at origination, dummies for the current number of open liens, the log of the origination amount, a dummy for there being an active prepayment penalty, a condo dummy, purpose type (dummies for rate-term refi and cash-out refi, with purchase as the omitted category), documentation (dummies for full and no documentation, with low documentation as the omitted category), and a dummy for whether the loan is on a second home or investment property. We also put in some proxies for local economic conditions and income shocks of the borrower: zip-code-level house price growth over the past 12 months (measured by the CoreLogic house price index), the local (MSA or state-level) unemployment rate, and the six-month change in the unemployment rate. We add the current Freddie Mac 30-year FRM rate, which one would expect to matter in particular in the prepayment regression. Finally, to account for differences in the legal environment across states (Ghent and Kudlyak 2011), we include state dummies in our regressions.

3.5 Timing

The central question of this paper is how a reduction in the required monthly payment affects payment behavior, but linking a particular monthly payment to default is not as straightforward as it sounds, as the following simple example illustrates. Suppose a borrower has a loan that resets from a fixed rate to an adjustable rate on June 1. In the typical loan in our dataset, the new interest rate for June will depend on the value of the index interest rate on the first business day of the month prior—that is, May 1 in our example. The reset means that on June 1, interest starts accruing at, for example, the six-month LIBOR on May 1 plus 225 basis points. But, since mortgage payments are made in arrears, the first monthly payment using the new rate will not be due until July 1. Furthermore, recall from our earlier discussion that a missed payment changes delinquency status only in the month *after* the missed payment. Therefore, if a loan reset on June 1 affects a borrower's ability to make the payment, we will not detect this as a change in delinquency status until August.

The note holder is required by law to deliver a written notice to the borrower with the details of the new payment before the change becomes effective. Thus, a borrower whose interest rate changes in June (so the higher payment becomes due in July) would be notified about the change in May.

In our baseline specification for delinquency, we assume that the borrower's payment behavior is affected in the month the payment is due, as described above. In other words, we use the two-month-lagged interest rate as our independent variable—for example, the June rate for delinquency in August. On the other hand, when analyzing the prepayment hazard, we do not lag the interest rate, because prepayment is recorded the month it occurs.

The baseline specification is not the only natural model to use. In a frictionless model (discussed verbally in Section 2 and formally in Appendix A.2), the value of the call option on the house depends on the price of future call options. Therefore, if a borrower's expected required payment in any future period decreases, he should become less likely to default today (with the magnitude of the effect depending on the borrower's discount factor). Below, we test to what extent interest rate reductions affect delinquency before they actually occur. To do so, we need to impute an expectation for our borrowers. Following the discussion above, a borrower who knows the terms of his mortgage will always know the interest rate that will affect his delinquency status two months out. For additional months in the future, we assume that the borrower bases his expectation on the current value of the index rate underlying his mortgage (for example, the six-month LIBOR) on the first of the month and assumes that this rate follows a random walk.[34] Using information on a borrower's margin above the index rate, as well as caps and floors on interest rate changes that are specified in the loan contract, we then impute what the borrower would likely expect the interest rate to be up to six months in the future.

4 Results

In this section, we review the results of estimating equation (6) for defaults, prepayments, and cures. The main question is whether formal statistical analysis confirms the visual evidence from Figure 1 that interest rate reductions reduce the likelihood of defaults and

[34] Koijen, Van Hemert, and Van Nieuwerburgh (2009) study time-series determinants of demand for fixed-rate versus adjustable-rate mortgages and find that a backward-looking specification for interest rate expectations explains borrower choices well.

increase the likelihood of cures. After presenting results from a baseline specification, we address potential concerns related to pre-reset trends, selection, and differential sensitivity of different loan types to economic conditions. We also present a variety of other robustness checks.

In proportional hazard models, the exponential functional form implies that the effect of a change in a control variable is assumed to be multiplicative. In our figures as well as the complete regression tables, which are given in the appendix, we report hazard ratios, which can be interpreted as the multiplicative effect of a one-unit increase in a variable (while to get, say, the predicted effect of a two-unit decrease, one needs to calculate $1/(\text{hazard ratio})^2$).

4.1 Defaults

Figure 3 shows the estimation results for our baseline default regression. The top panel shows that rate resets have an effect on the default hazard that is both statistically and economically highly significant. For instance, a 2-percentage point reduction in the interest rate on the loans in our sample lowers the probability of default by about 40 percent while a 4-percentage point reduction lowers it by two-thirds, effects that are statistically significant at the 0.1-percent level.

To give a better sense of the economic magnitude of these estimates, we add a plot of the effect of different levels of CLTV to the top panel of Figure 3. A 2-percentage point reduction in the interest rate has an effect roughly comparable to a reduction in CLTV from our baseline level of 135 to around 105, *holding the payment fixed*. Alternatively, a 3-percentage point reduction, which is close to the mean and median reduction experienced by 5/1s at their first reset, and approximately cuts their required monthly payment in half, corresponds approximately to moving the CLTV from 145 to 95.[35]

It is important to stress here that whereas one can plausibly interpret the effect of the interest rate changes as a causal treatment effect, it is more difficult to do the same for the CLTV because the variation in CLTV is not random: reductions in house prices proxy for local economic conditions that also affect the default hazard. That said, we try to proxy for these economic conditions by directly adding the change in local house prices as well as the unemployment rate and recent changes therein as explanatory variables in our regressions.

The coefficients on the control variables, reported in Panel B of Figure 3, behave largely as expected. Despite controlling for interest rates and many other observables directly, the SATO positively predicts defaults, suggesting that riskier borrowers obtain higher interest loans. The fact that origination LTV has an effect despite the fact that we control for CLTV

[35]One caveat here is that we do not observe the interest rate and required payment on second liens. Thus, a 3-percentage point reduction may actually reduce the monthly payment by less than half on average.

after origination implies a similar selection into high LTV loans. Finally, while we do not report the coefficients on loan type × calendars quarters, the default hazards for 5/1s and 3/1s tend to significantly exceed that of 7/1 and 10/1 loans, suggesting that borrowers with longer fixed-rate periods are less risky.

In Section 2, we argued that the size of the monthly payment should affect the default hazard even for borrowers with negative equity, and the data overwhelmingly confirm this prediction. First, remember that at the time of the resets of the 5/1 ARMs, the average LTV in our sample is over 140, making it hard to imagine that our results are driven by borrowers with positive equity. But to analyze this more formally, we repeat our baseline regression with a restricted sample of borrowers with CLTV greater than 140. Panel A of Figure 4 shows that there is no meaningful difference between the estimated effect of payment reductions for the whole sample and for the sample of borrowers with deep negative equity. The same panel also shows that not controlling for CLTV (or other loan characteristics aside from interest rates) has little effect on the estimated effect of rate reductions. This illustrates one of the main advantages of the setting we are analyzing, namely that the rate reductions are pre-determined by the original mortgage contract and the current index rates, and thus effectively orthogonal to a mortgage's current characteristics.

4.1.1 Alternative specifications

There are a number of possible reservations about our baseline regression specification. We address three potential issues: (1) differential pre-reset trends across different loan types; (2) correlation in the size of the reset with unobservable variation in borrower characteristics; (3) differential responses across loan-types to changes in macro conditions. In addition, we consider a variety of other alternative specifications.

One potential concern is that loans of different types may have differential pre-reset trends in default hazards (in a way not absorbed by the loan-type-specific calendar quarter dummies), and that including data away from the resets biases the estimated effects. In our baseline specification, we do not use the first 30 months of data, in order to make sure they do not drive the estimated effects.[36] We now provide further graphical and econometric evidence that this issue is not confounding our estimates, focusing on the stark resets of the 5/1s relative to the 7/1+ loans. First, Panel B of Figure 4 plots the "raw" default hazard of 5/1s relative to 7/1+ (i.e., the ratio of the solid and dashed lines in Panel B of Figure 1). We are plotting the ratio rather than the difference because that is what the proportional hazard model we estimate identifies the effects from. We note that from loan ages 40 to 59,

[36] As column (6) of Table A-1 shows, however, the results are very similar if we do include the first 30 months after origination.

this ratio is remarkably stable. It then drops over the course of four months from about 1.4 to 0.5, and remains stable after that.[37] This again seems strongly suggestive in favor of the reset causing the drop, as there are no trends in relative default hazards either before or after. To further investigate this point, we conduct an "event study" of the effect of the 5/1 reset, by retaining only five months of data before and after the reset, and by comparing only the 5/1 and the 7/1+ loans. The resulting coefficient estimates are plotted in Panel B of Figure 4 and show that for the common rate reductions of 2.5 percentage points or more, the estimated effects are very similar in size to those in the baseline specification.[38]

A second potential concern is that our estimates are confounded by a type of selection different from the one discussed in section 2.2: the size of the reset for borrowers within a loan type may be affected by the riskiness of a borrower. For instance, riskier borrowers get higher initial interest rates and therefore larger resets (assuming they have no binding rate floor). There is also some heterogeneity across borrowers in terms of index rates, margins, and rate floors. To eliminate this potential confound, we re-run our regression using the median interest rate (relative to the initial rate) of loans of the same type and originated the same month as loan i in lieu of loan i's actual relative interest rate. The resulting coefficients, also shown in Panel B of Figure 4 are overall again similar to those in the baseline, meaning that the relevant variation really comes from two sources: when a loan was originated, and how long its fixed-rate period was.[39]

The third main concern is that different loan types are differentially sensitive to changes in economic conditions—for instance, the default hazard of 5/1 borrowers relative to that of 7/1+ borrowers could fall around the time of the 5/1 resets (in 2010 and 2011) because the economy improves around that time and the borrowers who chose the 5/1s, who are riskier than those in the 7/1+ loans, may benefit more from this improvement in economic conditions. However, this effect would be soaked up in the loan type $\times$ calendar quarter dummies, so it should not affect the estimated effect of the rate reduction.[40]

[37]Recall from Section 3.5 that the reset of the 5/1 loans, which for the majority of them occurs in month 61 (but in some cases is marked in the data as occurring in months 60 or 62) becomes directly relevant for the delinquency status two months later, i.e. generally in month 63. The fact that the relative hazard starts dropping in month 60 is consistent with "anticipation effects" as discussed in the next subsection.

[38]We only plot the estimated effects for rate reductions of 1.5 percentage points or more, as there are very few 5/1s with smaller reductions. The estimated hazard ratios are also shown in Column (3) of Table A-1.

[39]As an alternative way to eliminate concerns about selection, we run our baseline regression on a "uniform" subsample of loans with the following characteristics: their rate must be indexed to the six-month LIBOR with a margin of 225 basis points and no binding rate floor; furthermore their SATO must be between -50 and 50 basis points. The results for this subsample, shown in column (5) of Table A-1, are again very similar to the baseline.

[40]Not including these interacted dummies, as done in an earlier version of this paper, leads to slightly larger estimated effects of the rate reduction. Using calendar month instead of quarter barely affects the results, but significantly increases the time it takes for the maximum likelihood estimation to converge.

Furthermore, we note that the estimation does *not* need to rely on the inclusion of the 7/1+ loans as comparison group: we can also identify the effects of resets solely from comparing the 3/1 loans, which start resetting 37 months after origination, to the 5/1 loans. Comparing these two loan groups may be appealing because they are very similar in terms of origination characteristics, and also because their default hazards over the first 30 months track each other quite closely. What is particularly interesting, however, is that the relative rate differences between 3/1s and 5/1s vary across origination cohorts, both in magnitude and in timing, and we can check visually whether this variation is also reflected in relative default hazards. This is done in Figure 5. This figure shows, for each of the six origination-quarter-vintages, how the differences in average rates between 3/1s and 5/1s (top line), as well as the relative default hazards of 3/1s (bottom line), change over (calendar) time.[41] What the figure shows is that the relative performance of 3/1s and 5/1s is closely aligned with the resets: when the rate of the 3/1s falls, they default less than the 5/1s; once the 5/1s also reset down, the relative performance of the 5/1s improves. Also, this clearly happens at different calendar times for different vintages, going against the hypothesis that the effect we pick up in the aggregate is driven by differential sensitivity of the different loan types. Quantitatively, Table A-2 shows that removing the 7/1+ loans from the sample, and only identifying the effects of interest rate changes from the 3/1s and 5/1s, leads to very similarly sized effects as in our baseline. The same is true if we alternatively drop the 3/1s or the 5/1s (columns (1) to (3)).

Table A-2 also contains some additional alternative specifications. In column (4), we replace the measure of the borrower's equity position by the "self-updated" estimate of a loan's LTV ratio, which is what researchers in this area have usually done when information on additional liens or the updated estimate of the home value was not available. This measure is based on the loan amount of the first lien only, and updates the denominator based on the zip-code house price index.[42] As the table shows, the coefficients on the different interest rate bins are very similar to the baseline. However, the predicted effects of LTV on the probability of delinquency are now smaller. For instance, a loan with an estimated LTV between 130 and 140 is now only 1.6 times as risky as a loan with an LTV between 90 and 100, while in the baseline it was 2.1 times as risky. In a sense, these attenuated effects are of course expected, as they are a well-known consequence of measurement error in the right-hand-side variable. Nevertheless, the results illustrate to what extent the quantitative importance of

[41] To reduce noise, we plot three-quarter moving average relative default hazards, and to enhance comparability across cohorts, we normalize the relative hazard to 1 for the tenth quarter of each cohort's history.

[42] Our regression controls again also include a quadratic function of LTV at origination, as well as a dummy for this LTV being equal to 80, which is a good proxy for the presence of a second lien (see Foote et al. 2010).

negative equity for mortgage delinquency may have been underestimated when information on second liens was unavailable.

Columns (5) to (7) look at different subsamples. First, in column (5) we study the effects of interest rate changes and CLTV on the delinquency of (self-declared) investors and second-home owners. While the effects are qualitatively similar to our baseline results, some of the coefficients suggest that investor delinquency could be slightly less responsive to interest rate decreases than what we observe for the whole sample. Similarly, in column (6), we exclude borrowers from the sand states and find that the resulting coefficients are largely similar to our baseline results, although some of the effects are slightly smaller. Finally, column (7) restricts the sample to borrowers that provided full documentation at origination, with the idea that this might tell us how applicable the results are to borrowers who might usually get prime loans. The table shows that the estimated effects are very similar to those in the baseline specification.

4.1.2 Do borrowers anticipate the reset?

In the results shown so far, we have studied the effect of the required monthly payment on contemporaneous repayment behavior, but as explained earlier, there are good theoretical reasons to think that payment changes should affect behavior before they actually occur. To test this, we estimate a version of equation (6) in which we include forward-looking changes in payments. In other words, our explanatory variables now include not only the interest rate that affects the current delinquency status but also six additional months of forward-looking interest rates (all relative to a loan's initial rate). In order to make the estimation simpler, we use quadratic functions of these interest rates (relative to the initial rate) rather than rate bins. Based on the resulting coefficients, we can then calculate the predicted effect of an interest rate reduction becoming delinquency-status-relevant in x months, for x ranging from 0 (this month) to 6, and the associated confidence interval.

For the interpretation of the estimated effects, it is important to remember our discussion from Section 3.5 above. During the month that we call "2 months ahead of the reset" (for example, June when looking at the delinquency status in August), the required payment in fact is accruing at the lower rate already, and the borrower has received a letter informing him of the lower rate in the month prior (May). If the borrower was not aware of the reset prior to receiving the letter, but learning about the reset affects his payment choice, then this should be reflected in the delinquency status two months prior to the reset.

Panel D of Figure 4 shows that anticipated rate reductions do affect delinquency, but that the effect is strong only shortly before the resets. For a 1-percentage point reduction in rates, there is a small reduction in the default hazard one and two months prior to the reset;

however, the effects are only marginally statistically significant. For a 3-point reduction in the interest rate, the effects appear sooner and are stronger. The default hazard falls by about 15 percent three months before the reset (and already by up to 10 percent during the months before, though these estimates are not all statistically significant), which is consistent with there being some borrowers whose repayment behavior is affected by future (large) changes in the interest rate on the loan. Two months prior to the reset becoming delinquency-status relevant, the default hazard is about 30 percent lower than with no anticipated change in the rate, consistent with the receipt of the information mattering. However, the estimated total effect once the reduction has actually occurred is twice as large, at 60 percent. As discussed later, this points toward liquidity constraints as an important driver of borrower default.

4.2 Prepayment and the incidence of default

Figure 6 shows the estimation results for the prepayment hazard. As we discussed in Section 2, ignoring the prepayment hazard can generate incorrect inference when looking at resets. Specifically, when payments increase, we often see a spike in prepayments, as creditworthy borrowers select out of the sample, meaning that the change in the default hazard confounds both selection and treatment effects.

The top panel of Figure 6 shows that prepayments respond strongly to both CLTV and to interest rate changes, but the pattern is quite different from that observed for defaults. Whereas reduced CLTV and payments work in the same direction for defaults, they work in opposite directions for prepayment. As the figure, which is in logs, shows, reducing CLTV from 135 to below 80 increases the likelihood of prepayment by an order of magnitude, whereas reducing the interest rate by 2.5 percentage points roughly cuts the likelihood of prepayment in half.

On the face of it, one might conclude that reductions in prepayments due to the decrease in rates could offset much of the decrease in the default hazard. However, the figure illustrates why this is not of first-order importance here. Essentially, for the borrowers most likely to default (those with high CLTV), the prepayment hazard is so low that changes to it have little or no economically meaningful effect. Even for all remaining loans in the sample, one can see in Panels B and C of Figure 1 that the default hazard for 5/1s shortly before the reset is approximately four times the size of the prepayment hazard.

In appendix A.4, we quantify the combined effect of the change in default and prepayment hazards on the *incidence* of defaults, using our estimated coefficients to predict the cumulative fraction of delinquency for a fixed population of 5/1 loans with "typical" characteristics, and for different assumptions about their CLTV. Our estimates imply that for loans with a CLTV between 130 and 140, a 3-percentage point reduction in the interest rate, which corresponds approximately to cutting the payment in half, is predicted to reduce the incidence of default by about 9 percentage points (or more than 50 percent) over the span of one year after the reset.

4.3 Cure rates

Having discussed transitions into delinquency, we now turn our attention to the effect of payment reductions on transitions out of delinquency. Figure 7 shows the results of our baseline specification and, as with the defaults, formal statistical analysis confirms the visual evidence in Figure 1 and shows that rate changes have statistically and economically significant effects on the likelihood of cure.

The top panel of Figure 7 shows that a 2–2.5-percentage point reduction in the interest rate leads to a 75-percent increase in the cure hazard, and a 3-point or higher reduction more than doubles the probability of cure. As with the default hazard, the effect of payment reduction is comparable to the effect of large changes in CLTV, with the same caveats about the interpretation of such changes. Also, as shown in Table A-3 in the appendix, negative equity does not attenuate and may in fact enhance the effect of rate reduction on cures. For instance, for borrowers with CTLV>140, the estimated effect of a 3 percentage point rate reduction increases from 200 percent to about 240 percent.

One might worry that the measured effect of rate reductions on cures is confounded by concurrent changes in servicers' propensity to modify delinquent loans. If, for instance, servicers tended to modify the loans that are most likely to cure (even without a modification), and then reduced the number of loans they modify after the interest rates decrease, then the measured effect of rate reductions on cures would be due at least in part to selection. Table A-3 shows that we find little systematic effect of rate reductions on the probability of a loan's receiving a modification.

5 Discussion

We divide our discussion of the implications of the results of Section 4 into two parts, focusing first on the general decision of the borrower to default on a mortgage and second on policies to prevent foreclosure.

5.1 What do the resets tell us about borrower behavior?

In our view, the evidence in Section 4 holds a number of lessons. First, many researchers (for example, Bhutta, Dokko, and Shan 2010) have attempted to identify some critical value of negative equity at which borrowers default. But our results illustrate that the answer to such a question is not straightforward. In Figure 8 we use the estimates from Figure 3 to construct "iso-default curves," with each line representing combinations of CLTV and rate reduction that are equally likely to lead to default. For instance, the line labeled "1" shows that a borrower with a CLTV of 90 paying 6 percent, a borrower with CLTV of 120 paying 4 percent, and a borrower with a CLTV of 140 paying 3 percent are all approximately equally likely to default. The line labeled "1.5" represents combinations of CLTV and rate reduction that make borrowers 1.5 times more likely to default than the borrowers in the line labeled 1, and so on.

Figure 8 shows that the threshold level of negative equity is highly sensitive to the size of the monthly payment. Therefore, if one asks the question "If the value of your mortgage exceeded the value of your house by $50K [$100K/$150K], would you walk away from your house (that is, default on your mortgage) even if you could afford to pay your monthly mortgage?" as Guiso, Sapienza, and Zingales (2013) do, one should get wildly different answers depending on whether the borrower is paying 3, or 4, or 6 percent in interest.

We now turn to the question of what the results tell us about borrower decisionmaking more broadly. It will come as a surprise to many that Figure 8 is completely consistent with the concept of "strategic default." Kau et al. (1992) show that a borrower making a purely financial decision about whether to default in a completely frictionless world compares the value of the property with the *value* of the mortgage, which is the present discounted value of all future mortgage payments, controlling for the fact that the borrower has options to prepay or default on the mortgage. Simple bond math says that the value of the mortgage will depend on the size of the monthly payment and thus that borrowers making lower payments are less likely to default. In other words, iso-default curves as shown in Figure 8 are exactly what the frictionless theory would predict.

That said, while Figure 8 does not provide evidence against strategic default, it does not necessarily provide evidence in its favor either. First, a combination of a simple nonoptimizing double trigger model and mismeasured house prices could generate the patterns we observe. According to the double trigger model, a borrower gets hit with an income shock that makes it impossible to pay the mortgage and then either sells or refinances the house if he or she has positive equity, and defaults otherwise. All else being equal, lower mortgage payments reduce the likelihood that a given shock is sufficient to cause nonpayment. Mismeasured house prices mean that measured equity provides only a noisy signal for whether the borrower is actually unable to sell or refinance; however, the probability of that being the case is higher the higher the measured CLTV.

Second, it is difficult to square the evidence on forward-looking behavior in Figure 4 with a pure strategic default story. Recall that Panel D of Figure 4 shows that even a large payment reduction does not have an economically meaningful effect on repayment behavior until 2–3 months prior to the reset and, even one month prior to reset, the effect is still only about half as strong as it is when the rate actually changes. A strategic default story would ascribe this to the difference between the value of the mortgage one month prior to the reset and one month after, given by the difference between one payment at the higher rate and one at the lower rate. For a 3-percentage point rate cut, we divide this by 12 months, meaning that the value of the mortgage is 25 basis points higher one month prior to the reset. This small difference cannot possibly account for the enormous change in behavior.

To make sense of Figure 4, we need to understand why borrower behavior changes discontinuously at the time of the reset. At least two logical explanations present themselves. The first is that borrowers are behaving strategically but are inattentive and fail to realize that the rate is going to change. As mentioned in Section 3.5, lenders are supposed to inform borrowers by mail before the index rate changes, which would mean that the notice should arrive prior to the due date of the last pre-reset payment.[43] Prior to that, the borrowers could forecast the expected change, but to do so they would need to know the terms of the mortgage, including the index used and the margin, and Bucks and Pence (2008) raise serious questions about whether average borrowers understand these terms. That said, it seems hard to imagine that a borrower making a potentially life-changing decision to default on a mortgage would not find out. Thus, the implications of the combination of ignorance and strategic default are odd. Because we know that the borrowers choose to continue paying when confronted with the true monthly payment, the implication is that their ignorance leads them to walk away from mortgages that they would prefer to pay.

An alternative explanation is that borrowers face liquidity shocks, for instance due to unemployment or illness. In standard consumption-portfolio choice models with constraints, liquidity shocks drive up the marginal utility of current consumption to make borrowers behave as if they were highly impatient. One potential explanation for the response to the resets is that some share of borrowers face such liquidity shocks every period, causing them to default, and that the lowered payment shrinks the set of liquidity shocks sufficient to induce default. This intuitively appealing model is thus qualitatively consistent with our findings. An interesting question for future research is whether a quantitative model with realistic parameters (along the lines of Campbell and Cocco 2011 or Schelkle 2012) would

[43]Of course, it is debatable whether borrowers will read or understand such a notice.

quantitatively match the effects of rate reductions that we find in the data.

5.2 Policy implications

Our results indicate that payment reductions, if sufficiently large, are an effective tool to reduce mortgage defaults and increase cures, even if a borrower is massively underwater. This suggests that government or lender programs that allow underwater borrowers to refinance at a lower rate, or loan modifications that lower the interest rate, have the potential to significantly reduce delinquencies, and that the view that principal reduction is the *only* way to meaningfully reduce defaults is incorrect.

A logical and important follow-up question is whether, given our estimates, it is more cost-efficient from an investor perspective to reduce an underwater loan's interest rate (and thus the required monthly payment) or the principal (which lowers the CLTV *and* the required payment).[44] We leave this question for further research, as it is nontrivial to analyze: the answer will depend on investors' discount rate, the recovery rate in case of default, and notably also on the length of time the borrower is assumed to stay in the mortgage in case he does not default, which will itself depend on market interest rates and the borrower's equity position.[45]

Furthermore, one needs to keep in mind two things when trying to apply our results to broader policy questions. First, the interest reductions we study are not necessarily permanent, as the benchmark rates may increase again in the future.[46] If they were permanent, the resulting reductions in the default hazard might be even larger. Second, the effects of an interest rate reduction of x percent on the required monthly payment would be smaller for amortizing mortgages than for the interest-only mortgages we study, and so the reduction in the default hazard following a fixed cut in the interest rate would likely be smaller than for the loans in our sample.

In any event, we do not mean to argue that payment reductions are generally more (cost) effective than principal reductions when it comes to reducing defaults of underwater homeowners—we simply show that they are certainly not ineffective, as argued by some

[44] See Das (2011) for a theoretical, option-based analysis that argues that principal reductions are preferable to rate reductions.

[45] The present value of a loan with an interest rate above the current market rate will be lower the sooner the borrower prepays. As a consequence, if a principal reduction causes the borrower to prepay sooner, this effect needs to be taken into account when assessing the cost of the principal reduction. Furthermore, the reduction in the principal the investor receives as a consequence of the modification is more costly in present-value terms the sooner it is realized.

[46] Such temporary reductions are also a characteristic of the HAMP program, where a borrower pays a low fixed interest rate for five years, while after that, the interest rate increases by 1 percent a year until it reaches the lesser of the Freddie Mac Primary Mortgage Market Survey rate or the originally contracted rate.

commentators, and are a potentially valuable tool when principal reductions are impossible as a result of institutional constraints.

From a broader perspective, a key feature of the payment reductions in our sample is that they came about because of the historically low interest rates, which are arguably tied closely to the state of the economy and also to monetary policy. Our results thus show that, with ARMs, monetary policy can have large effects on mortgage delinquency, and by extension, on the health of the housing market as a whole. In principle, to the extent that monetary policy affects long-term rates (either through the expectations channel, or more recently, through expansion of the Fed's balance sheet), the same would be true for FRMs. However, a painful realization of the period since 2008 is that in case of a credit crunch with tight underwriting standards, many borrowers are not able to take advantage of the lower rates. In that sense, FRMs make the transmission of monetary policy more fragile. On the other hand, should benchmark rates increase without a contemporaneous improvement in house prices and economic conditions more broadly (for example, in a stagflation episode), ARMs would be at a higher risk of default again.

Finally, in terms of regulation of mortgage products, our results suggest that one might want to limit the ability of lenders to offer ARMs with asymmetric floors and caps on interest rates. As mentioned earlier, such asymmetries were prevalent for subprime ARMs, where at the end of the fixed-rate period, the interest rate could only increase but not decrease. This means that decreases in short-term interest rates due to economic conditions did not get passed through to subprime ARM borrowers to the same extent they did to Alt-A and prime ARM borrowers, and this likely caused subprime defaults to be higher than they would have been without the rate floors. To prevent this from happening in future cycles, one could imagine a regulation along the following lines: if an ARM's interest rate can increase by up to x percentage points relative to the initial rate, it must also be possible for the rate to *decrease* by x percentage points relative to the initial rate. x would then be an easy-to-understand indicator of the nominal interest rate risk borne by the household (with $x = 0$ corresponding to an FRM).

6 Conclusion

In this paper, we have studied the effects of payment size and negative equity on mortgage borrowers' likelihood of becoming delinquent. After arguing that because of the prepayment option, one needs to study payment reductions to get accurate estimates of the treatment effect of changes in the required payment, we exploit a sample for which large payment reductions took place due to the low-interest-rate environment over the period 2008–2011.

Our results show that the size of the monthly payment matters strongly for delinquency and cures, even for borrowers who are deeply underwater. These findings, which we argue are consistent with theoretical predictions, shed light on the driving forces behind mortgage default and have a variety of policy implications.

In terms of related research, our results could be used to calibrate or discipline quantitative models of mortgage delinquency in which the effects of different policy options are simulated. Also, our findings should be useful for the pricing of mortgage-backed securities based on ARMs.

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Table 1: Summary statistics

A. Distribution of loan types

	3/1 ARMs		5/1 ARMs		7/1 ARMs		10/1 ARMs		Total
	# (000s)	Share (%)	# (000s)	Share (%)	# (000s)	Share (%)	# (000s)	Share (%)	# (000s)
2005H1	19.1	32	32.4	54	1.4	2	7.6	13	60.4
2005H2	8.3	11	43.5	60	4.0	6	17.1	23	72.9
2006H1	8.2	9	55.4	63	9.6	11	15.3	17	88.5
Total	35.6	16	131.2	59	15.0	7	40.0	18	221.6

B. Origination characteristics

	3/1s	5/1s	7/1s	10/1s	Total
Origination amount	294	272	345	414	306
(std. dev.)	*(170)*	*(164)*	*(200)*	*(218)*	*(19)*
LTV on first lien	78	77	77	74	77
(std. dev.)	*(8)*	*(9)*	*(11)*	*(12)*	*(9)*
CLTV (TrueLTV)	93	94	93	88	93
(std. dev.)	*(20)*	*(20)*	*(20)*	*(22)*	*(21)*
Number of Liens	1.7	1.7	1.6	1.5	1.7
(std. dev.)	*(0.5)*	*(0.5)*	*(0.5)*	*(0.5)*	*(0.5)*
FICO score	714	710	717	721	713
(std. dev.)	*(42)*	*(45)*	*(46)*	*(46)*	*(45)*
Initial interest rate	6.2	6.6	6.6	6.3	6.5
(std. dev.)	*(0.7)*	*(0.8)*	*(0.6)*	*(0.5)*	*(0.7)*
Condo	0.21	0.21	0.22	0.21	0.21
Investor or 2nd home	0.24	0.28	0.19	0.15	0.24
Low documentation	0.73	0.69	0.63	0.74	0.70
No documentation	0.04	0.06	0.04	0.06	0.06
CA, NV, FL, or AZ	0.57	0.52	0.57	0.67	0.56
Purchase mortgage	0.68	0.70	0.61	0.57	0.67
Resets every 6 months	0.85	0.79	0.45	0.28	0.69
Prepayment penalty	0.32	0.38	0.30	0.34	0.35

C. Mean CLTV (active loans only) at different points over sample period (%)

	3/1s	5/1s	7/1s	10/1s	Total
January 2008	109	108	107	102	107
(std. dev.)	*(26)*	*(26)*	*(25)*	*(26)*	*(26)*
January 2010	144	142	139	130	139
(std. dev.)	*(47)*	*(48)*	*(44)*	*(43)*	*(47)*
November 2011	150	147	146	137	145
(std. dev.)	*(50)*	*(50)*	*(48)*	*(45)*	*(49)*

D. Outcomes (as of November 2011)

	3/1s	5/1s	7/1s	10/1s	Total
Goes 60+ days delinquent	0.37	0.46	0.45	0.36	0.43
Foreclosure / short sale	0.30	0.38	0.35	0.26	0.34
Voluntary prepayment	0.46	0.36	0.32	0.35	0.37
Modified at least once	0.04	0.07	0.08	0.07	0.07

Figure 1: Resets, defaults, prepayments and cures over the life of the loan

Based on sample of hybrid Alt-A loans with interest-only feature for ten years, originated between January 2005 and June 2006. Except for exclusions based on origination characteristics as explained in text, includes all loans (also those with upward resets). After loan age 65 months, the sample changes because loans originated toward the end of the origination period are no longer observed (which explains the kink in the dashed line in Panel A). Vertical lines indicate loan ages 37 months (when 3/1s reset for the first time) and 61 months (when 5/1s reset for the first time). A small percentage of loans are recorded as resetting one month before or after the scheduled reset month.

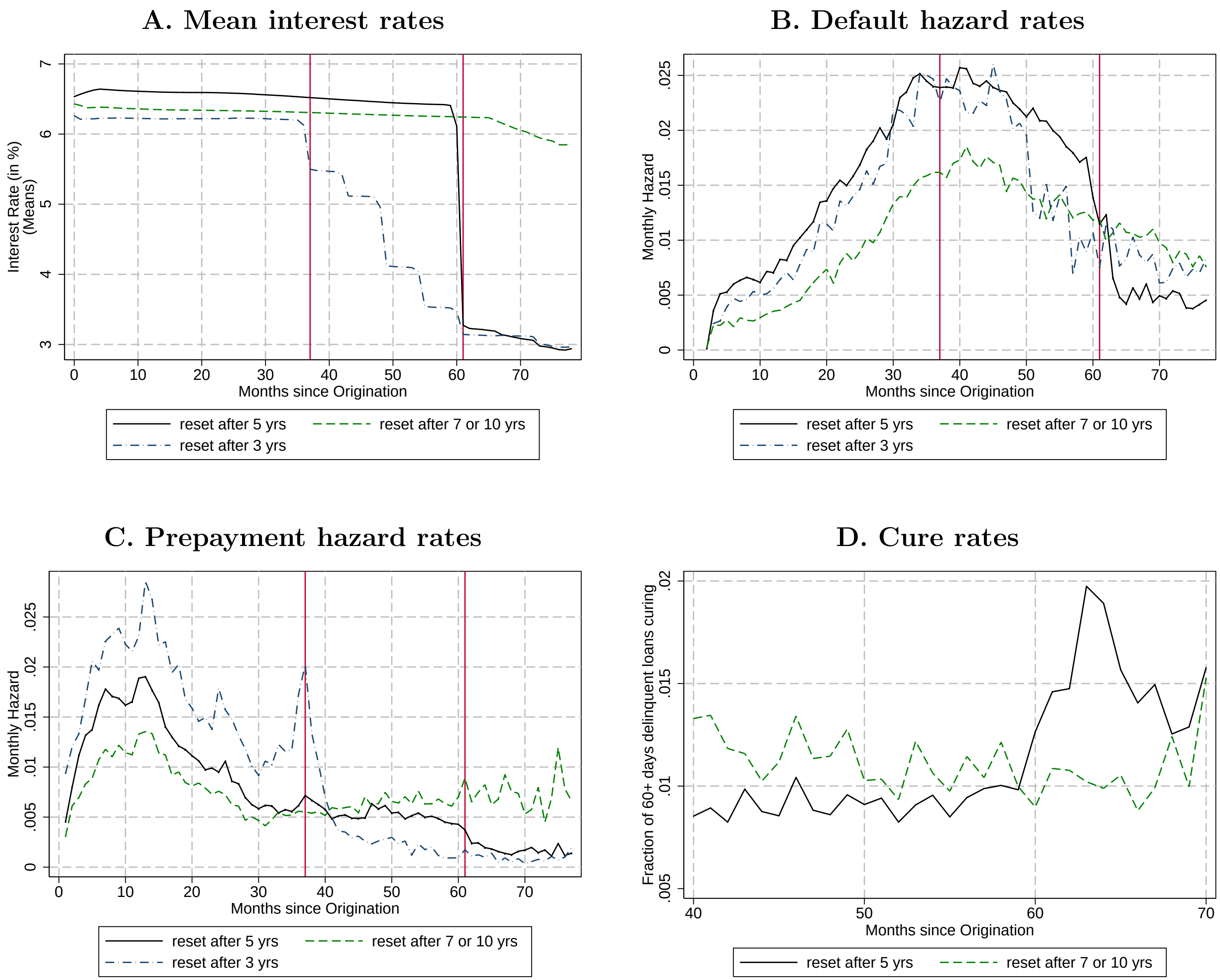
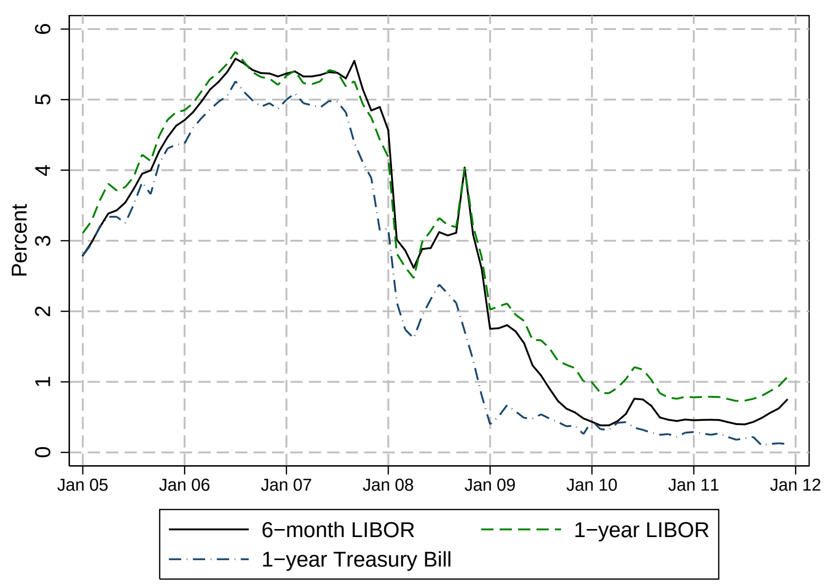


Figure 2: Index rates and resets

Panel A displays the evolution over our sample period of the three interest rates to which the mortgages in our sample are indexed (data source: Haver Analytics). The first months relevant for resets are, respectively, January 2008 and January 2010 for 3/1s and 5/1s originated in January 2005. Values displayed are of the first of each month. Panel B shows the distribution of interest rate changes at the first reset (month 37 for 3/1s, month 61 for 5/1s) as well as subsequent resets (every 6 or 12 months after the initial reset).

A. Index rates over sample period



B. Distribution of interest rate changes at resets

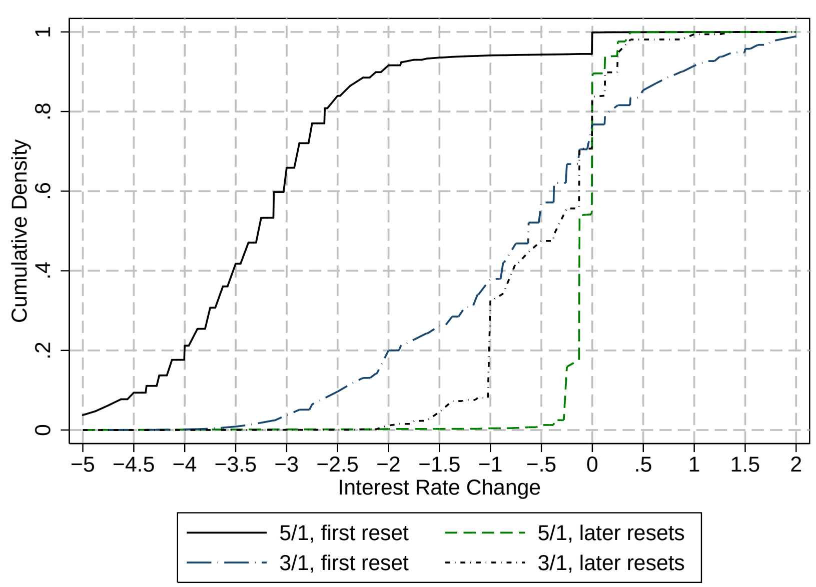
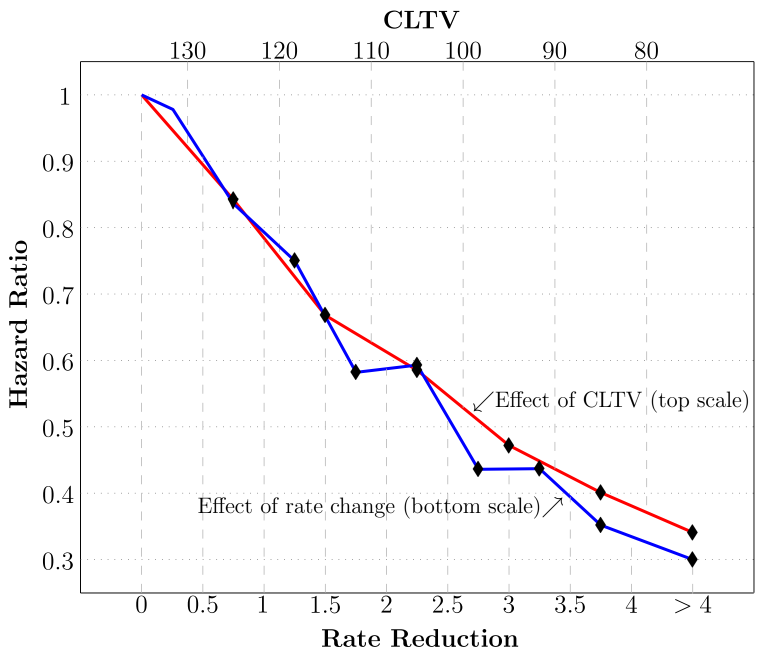


Figure 3: Results of baseline default hazard estimation

Panel A graphically displays hazard ratios for bins of interest rates (relative to loan's original rate) as well as combined loan-to-value (CLTV) ratios in our baseline proportional hazard regression of 60-day delinquency. Coefficients and standard errors are also given in Table A-1 in the appendix. Panel B shows hazard ratios and standard errors for other control variables, and provides details about the regression. Sample is restricted to loan ages > 30 months.

A. Effects of CLTV and rate reduction



(▼ $p < 0.05$, ▲ $p < 0.01$, ♦ $p < 0.001$)

B. Effects of control variables

SATO	1.191***	Origination LTV	1.048***	Not owner-occupied	1.019
	(0.046)		(0.007)		(0.068)
FICO/100	0.591***	(Orig. LTV)^2/100	0.977***	Condo	0.854***
	(0.009)		(0.004)		(0.037)
Open liens = 2	1.190***	Full doc.	0.600***	12-month HPA	0.985***
	(0.028)		(0.017)		(0.002)
Open liens $\geq$ 3	0.980	No doc.	1.087**	Unemp. rate	1.000
	(0.054)		(0.031)		(0.006)
Prepayment penalty active	1.065***	Purpose=cashout refi	0.954***	6-month Δ(Unempl. rate)	1.004
	(0.012)		(0.012)		(0.008)
Log(loan amount)	1.062*	Purp.=non-cashout refi	0.989	30-year FRM rate	1.148**
	(0.033)		(0.016)		(0.056)
Baseline hazard strat.	Closing q.	Observations	1,890,615		
State dummies	✓	# Loans	75,123		
Loan type × calendar q. dummies	✓	# Incidents	30,377		
Initial interest rate bins	✓				

Exponentiated coefficients; Standard errors (clustered at state level) in parentheses
* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Figure 4: Understanding the default hazard

Panels A and C displays hazard ratios for bins of interest rates (relative to loan's original rate) for different samples/specifications of our proportional hazard regression of 60-day delinquency. The corresponding coefficients and standard errors are in columns (1), (2), and (7) (Panel A) and (1), (3), and (4) (Panel C) of Table A-1 in the appendix. Panel B displays the ratio of hazard rates of 5/1 and 7/1+ loans. Panel D displays the cumulative predicted effect of interest rate changes from 0 to 6 months before the delinquency-status relevant payment changes.

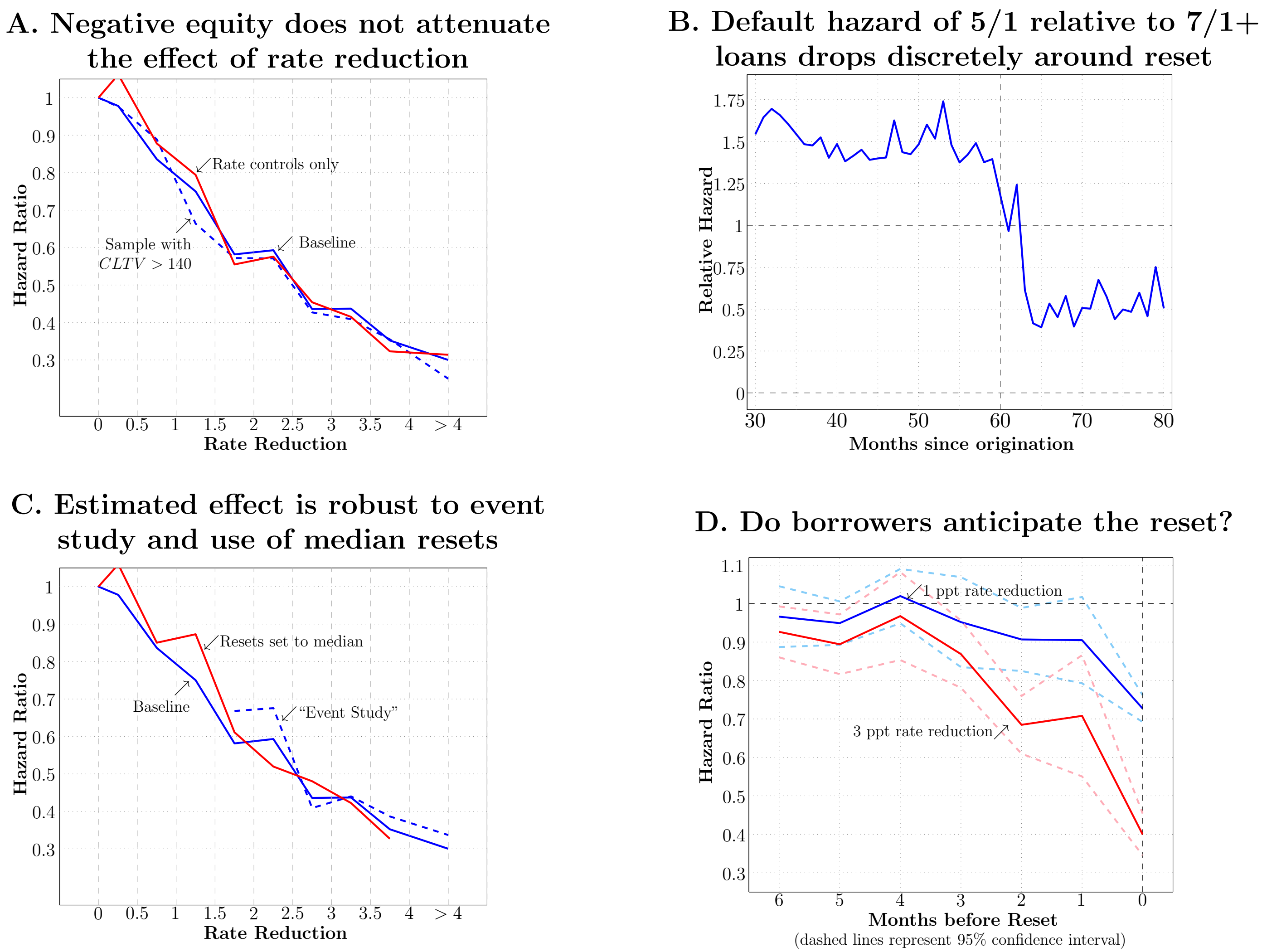


Figure 5: Timing of rate resets and defaults hazards of 3/1s vs. 5/1s

Figure shows how the differences in average rates between 3/1s and 5/1s (top line), as well as the relative quarterly default hazards of 3/1s relative to 5/1s (bottom line), evolve over (calendar) time, for each of the six origination-quarter-vintages. To reduce noise, three-quarter moving average relative default hazards are plotted, and to enhance comparability across cohorts, the relative hazard is normalized to 1 for the tenth quarter of each cohort's history.

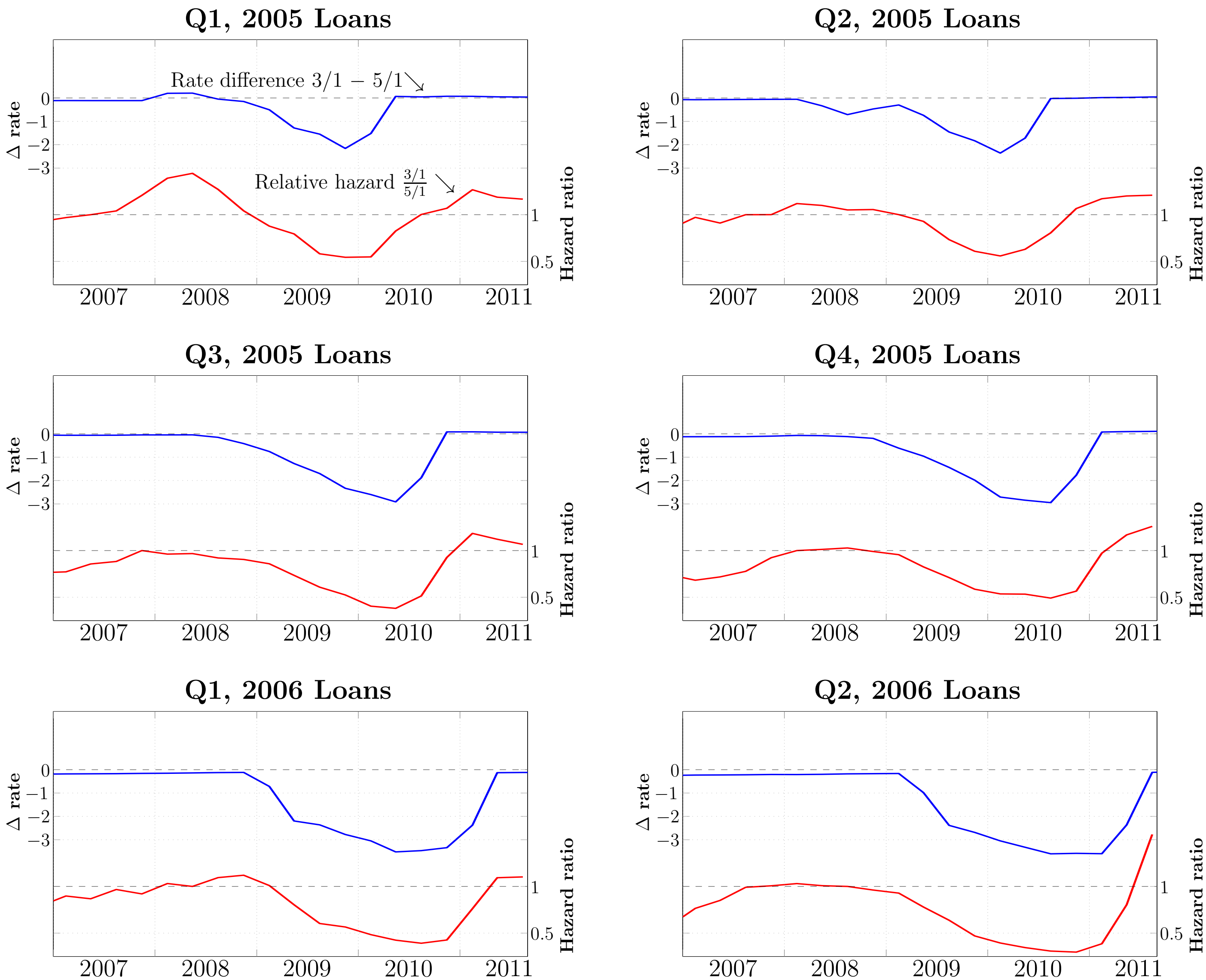
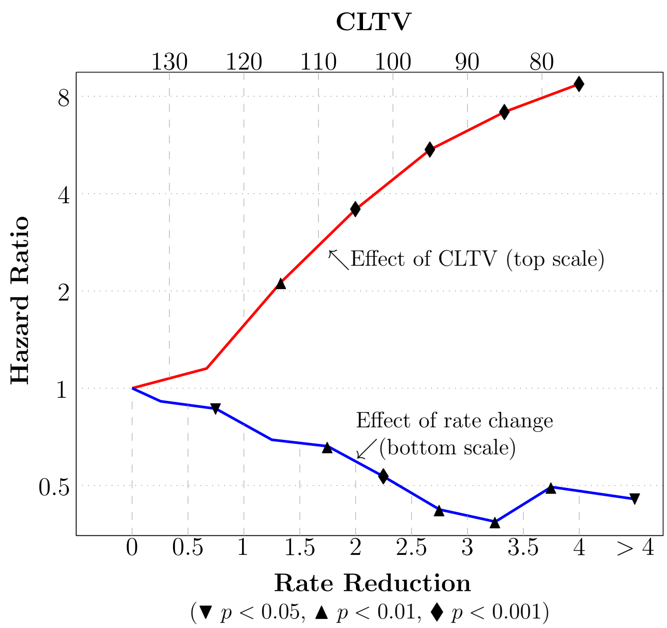


Figure 6: Prepayment hazard as a function of rate reduction

Panel A graphically displays hazard ratios for bins of interest rates (relative to loan's original rate) as well as combined loan-to-value (CLTV) ratios in our baseline proportional hazard regression of prepayment. Vertical axis has a log scale. Panel B shows hazard ratios and standard errors for other control variables, and provides details about the regression. Sample is restricted to loan ages > 30 months.

A. Effects of CLTV and rate reduction



B. Effects of control variables

SATO	1.225*	Origination LTV	0.969***	Not owner-occupied	0.589***
	(0.099)		(0.004)		(0.052)
FICO/100	1.682***	(Orig. LTV)^2/100	1.013**	Condo	0.899
	(0.066)		(0.004)		(0.055)
Open liens = 2	0.944	Full doc.	1.387***	12-month HPA	1.015*
	(0.055)		(0.041)		(0.006)
Open liens ≥ 3	1.823***	No doc.	1.021	Unemp. rate	0.964
	(0.306)		(0.049)		(0.023)
Log(loan amount)	1.331***	Purpose=cashout refi	0.724***	6mon Δ(Unemp. rate)	1.045
	(0.067)		(0.028)		(0.027)
Prepayment penalty active	0.542***	Purp.= Non-cashout refi	0.822***	30-year FRM rate	0.460***
	(0.040)		(0.032)		(0.054)
Baseline hazard strata	Closing q.	Observations	1,890,611		
State dummies	✓	# Loans	75,123		
Loan type × calendar q. dummies	✓	# Incidents	6,878		
Initial interest rate bins	✓				

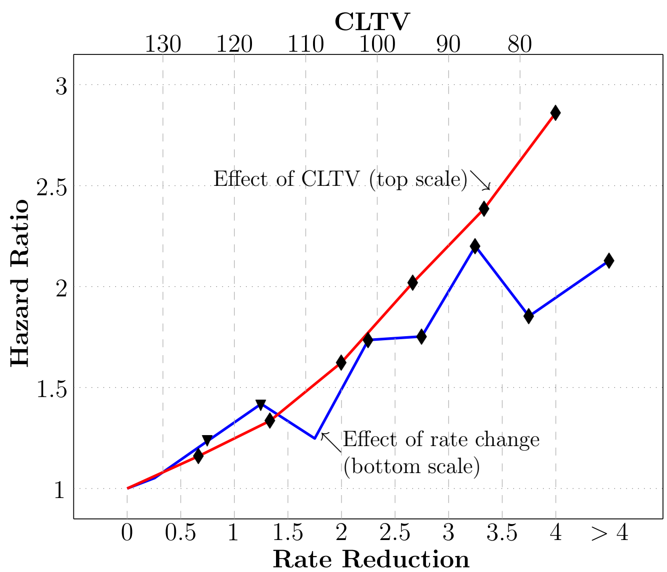
Exponentiated coefficients; Standard errors (clustered at state level) in parentheses

* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Figure 7: Cure hazard as a function of rate reduction

Panel A graphically displays hazard ratios for bins of interest rates (relative to mortgage rate at which borrower became 60-days delinquent) as well as combined loan-to-value (CLTV) ratios in our baseline proportional hazard regression of curing (= becoming current again or prepaying voluntarily). Coefficients and standard errors are also given in column (1) of Table A-3 in the appendix. Panel B shows hazard ratios and standard errors for other control variables, and provides details about the regression.

A. Effects of CLTV and rate reduction



(▼ $p < 0.05$, ▲ $p < 0.01$, ♦ $p < 0.001$)

B. Effects of control variables

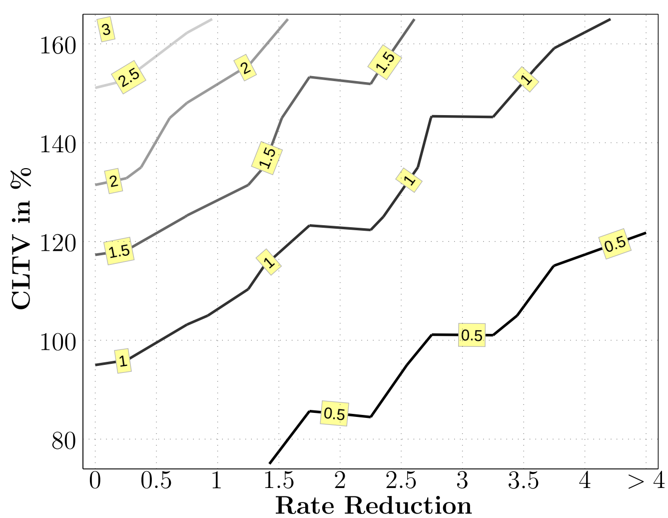
SATO	1.002	Origination LTV	0.959***	Not owner-occupied	0.897***
	(0.035)		(0.006)		(0.024)
FICO/100	0.812***	(Origination LTV)^2	1.000***	Condo	0.954
	(0.019)		(0.000047)		(0.02)
Open liens = 2	0.936**	Full doc.	1.209***	12-month HPA	1.019***
	(0.024)		(0.028)		(0.001)
Open liens ≥ 3	1.156**	No doc.	1.014	Unemp. rate	0.989
	(0.057)		(0.038)		(0.007)
Log(loan amount)	0.867***	Purpose = Cashout refi	1.098***	6mon Δ(Unemp. rate)	1.000
	(0.018)		(0.028)		(0.010)
Prepayment penalty active	0.942*	Purp. = Non-cashout refi	0.987	30-year FRM rate	1.163
	(0.023)		(0.029)		(0.108)
Loan age dummies	✓	Baseline hazard strat.	Closing q.		
State dummies	✓	Observations	847,262		
Missed interest rate bins	✓	# Loans	65,900		
Loan type × calendar q. dummies	✓	# Incidents	14,867		

Exponentiated coefficients; Standard errors (clustered at individual level) in parentheses

* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Figure 8: Iso-default curves

Lines on plot represent the combinations of rate reductions and CLTVs that lead to the same probability of default according to our baseline specification. Number on line measures the default probability relative to other lines; for example, borrowers with combinations of CLTV and rate reduction on the line marked "0.5" are half as likely to default as borrowers on the line marked "1" and one-third as likely to default as borrowers on the line marked "1.5."



Appendix

A.1 Additional tables

Table A-1: Proportional hazard models of 60-day delinquency

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Loan ages (months)	>30	>30	$58-67$	>30	>30	all	>30
Sample restrictions		CLTV≥ 140	no 3/1		unif. sample		
Use median resets?				✓			
Interest rate $-$ initial rate (omitted bin: $[-0.01, 0.01]$):							
$(-0.01, -0.5]$	0.978	0.976	0.695	1.059	1.048	1.013	1.062
	(0.0556)	(0.0775)	(0.728)	(0.0491)	(0.107)	(0.0475)	(0.0587)
$(-0.5, -1]$	0.836**	0.889	1.140	0.850**	0.903	0.831**	0.878***
	(0.0515)	(0.0686)	(0.542)	(0.0420)	(0.0590)	(0.0535)	(0.0291)
$(-1, -1.5]$	0.750***	0.664***	0.476	0.873	0.787	0.740***	0.794***
	(0.0470)	(0.0369)	(0.206)	(0.0695)	(0.116)	(0.0497)	(0.0377)
$(-1.5, -2]$	0.582***	0.572***	0.668	0.611***	0.598*	0.590***	0.555***
	(0.0618)	(0.0929)	(0.159)	(0.0800)	(0.131)	(0.0590)	(0.0418)
$(-2, -2.5]$	0.593***	0.571***	0.676**	0.520***	0.616***	0.617***	0.576***
	(0.0408)	(0.0745)	(0.0827)	(0.0422)	(0.0794)	(0.0433)	(0.0472)
$(-2.5, -3]$	0.436***	0.427***	0.409***	0.481***	0.407***	0.455***	0.454***
	(0.0383)	(0.0378)	(0.0299)	(0.0305)	(0.0620)	(0.0426)	(0.0371)
$(-3, -3.5]$	0.437***	0.409***	0.440***	0.422***	0.416***	0.450***	0.415***
	(0.0302)	(0.0242)	(0.0464)	(0.0271)	(0.0479)	(0.0350)	(0.0280)
$(-3.5, -4]$	0.352***	0.355***	0.386***	0.327***	0.298***	0.346***	0.323***
	(0.0476)	(0.0620)	(0.0767)	(0.0301)	(0.0622)	(0.0518)	(0.0425)
≤ -4	0.300***	0.250***	0.337***		0.240***	0.265***	0.314***
	(0.0298)	(0.0373)	(0.0478)		(0.0524)	(0.0272)	(0.0315)
Current CLTV (omitted bin: $[130, 140)$) :							
< 80	0.341***		0.445***	0.341***	0.321***	0.254***	
	(0.0193)		(0.0609)	(0.0193)	(0.0340)	(0.0110)	
$[80, 90)$	0.401***		0.541***	0.401***	0.369***	0.313***	
	(0.0197)		(0.0620)	(0.0197)	(0.0387)	(0.0245)	
$[90, 100)$	0.472***		0.558***	0.472***	0.486***	0.393***	
	(0.0255)		(0.0463)	(0.0255)	(0.0439)	(0.0230)	
$[100, 110)$	0.585***		0.717***	0.585***	0.574***	0.537***	
	(0.0302)		(0.0412)	(0.0303)	(0.0425)	(0.0296)	
$[110, 120)$	0.668***		0.710***	0.667***	0.654***	0.686***	
	(0.0381)		(0.0699)	(0.0382)	(0.0423)	(0.0281)	
$[120, 130)$	0.842***		0.881**	0.842***	0.850***	0.849***	
	(0.0167)		(0.0420)	(0.0167)	(0.0292)	(0.0102)	
$[140, 150)$	1.076*	0.693***	1.075	1.076*	1.081*	1.127***	
	(0.0357)	(0.0159)	(0.0892)	(0.0357)	(0.0349)	(0.0303)	
$[150, 160)$	1.246***	0.815***	1.163***	1.246***	1.196***	1.273***	
	(0.0561)	(0.0122)	(0.0492)	(0.0560)	(0.0426)	(0.0542)	
≥ 160	1.473***	1	1.507***	1.472***	1.453***	1.491***	
	(0.0565)		(0.0781)	(0.0563)	(0.0493)	(0.0625)	
FICO/100	0.591***	0.693***	0.651***	0.590***	0.601***	0.543***	
	(0.00948)	(0.0101)	(0.0312)	(0.00953)	(0.0156)	(0.0126)	

Continued on next page

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SATO	1.191***	1.092	1.303	1.171***	1.114	1.263***	
	(0.0458)	(0.0855)	(0.265)	(0.0424)	(0.152)	(0.0576)	
Open liens = 2	1.190***	1.124***	1.199***	1.191***	1.161***	1.232***	
	(0.0279)	(0.0304)	(0.0462)	(0.0281)	(0.0489)	(0.0269)	
Open liens $\geq$ 3	0.980	0.903	0.859	0.981	0.966	0.910	
	(0.0537)	(0.0510)	(0.104)	(0.0540)	(0.0722)	(0.0516)	
Ppmt. penalty active	1.065***	1.043**	1.213	1.065***	0.969	1.065***	
	(0.0124)	(0.0151)	(0.145)	(0.0126)	(0.0361)	(0.0114)	
Log(loan amount)	1.062*	1.033	0.855***	1.061	1.135***	1.189***	
	(0.0327)	(0.0202)	(0.0328)	(0.0324)	(0.0328)	(0.0387)	
Origination LTV	1.048***	1.082***	1.059***	1.049***	1.064***	1.043***	
	(0.00681)	(0.0125)	(0.0132)	(0.00686)	(0.0146)	(0.00544)	
(Orig. LTV)^2/100	0.977***	0.957***	0.969***	0.977***	0.967***	0.978***	
	(0.00353)	(0.00606)	(0.00750)	(0.00357)	(0.00794)	(0.00267)	
Full documentation	0.600***	0.660***	0.712***	0.601***	0.566***	0.596***	
	(0.0175)	(0.0166)	(0.0257)	(0.0175)	(0.0237)	(0.0147)	
No documentation	1.087**	1.106**	1.099	1.089**	1.113*	1.085***	
	(0.0309)	(0.0379)	(0.0681)	(0.0306)	(0.0492)	(0.0176)	
Cashout Refi	0.954***	0.874***	1.068	0.954***	0.937***	0.850***	
	(0.0120)	(0.0102)	(0.0424)	(0.0120)	(0.0178)	(0.0253)	
Non-cashout refi	0.989	0.970	0.974	0.988	0.998	0.949*	
	(0.0159)	(0.0242)	(0.0781)	(0.0158)	(0.0271)	(0.0194)	
Not owner-occupied	1.019	0.935	0.840*	1.022	0.987	1.060	
	(0.0681)	(0.0593)	(0.0627)	(0.0681)	(0.0957)	(0.0642)	
Condo	0.854***	0.870***	0.973	0.854***	0.864***	0.843***	
	(0.0370)	(0.0307)	(0.0410)	(0.0364)	(0.0290)	(0.0406)	
12-month HPA	0.985***	0.990***	1.001	0.985***	0.983***	0.983***	
	(0.00157)	(0.00124)	(0.00414)	(0.00160)	(0.00142)	(0.00170)	
Unempl. rate (U)	1.000	0.989*	1.000	1.000	0.996	1.011*	
	(0.00579)	(0.00531)	(0.00577)	(0.00574)	(0.00635)	(0.00480)	
6-month ΔU	1.004	1.001	1.036	1.004	1.013	1.003	
	(0.00810)	(0.0114)	(0.0225)	(0.00807)	(0.0145)	(0.00858)	
30-year FRM rate	1.148**	1.041	0.621**	1.148**	1.122	1.129*	
	(0.0556)	(0.0675)	(0.100)	(0.0540)	(0.160)	(0.0660)	
State dummies	✓	✓	✓	✓	✓	✓	
Initial interest rate bins	✓	✓	✓	✓	✓	✓	✓
Loan type × calendar q. dummies	✓	✓	✓	✓	✓	✓	✓
Observations	1890615	493284	315482	1890615	589163	4790556	2823245
# Loans	75123	29541	34469	75123	23973	138077	116866
# Incidents	30377	14197	3152	30377	9404	55238	49914
Log Likelihood	-267200.0	-108759.6	-26520.1	-267249.2	-71531.4	-499127.1	-470664.5

Exponentiated coefficients; Standard errors (clustered at state level) in parentheses.
In all regressions, baseline hazard allowed to vary by origination quarter.
Significance: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A-2: Proportional hazard models of 60-day delinquency — Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Loan ages (months)	> 30	> 30	> 30	> 30	> 30	> 30	> 30
Sample restrictions	no 3/1	no 7/1+	no 5/1		Inv. only	no AZ,CA,FL,NV	Full doc only
Use "self-updated" LTV				✓			
Interest rate − initial rate (omitted bin: $[-0.01, 0.01]$):							
$(-0.01, -0.5]$	0.265	0.982	0.963	0.950	1.428**	0.799	0.932
	(0.203)	(0.0494)	(0.0544)	(0.0449)	(0.192)	(0.151)	(0.163)
$(-0.5, -1]$	1.411*	0.844*	0.791***	0.810***	0.791	0.918	0.771
	(0.210)	(0.0659)	(0.0458)	(0.0350)	(0.108)	(0.156)	(0.133)
$(-1, -1.5]$	1.049	0.771***	0.692***	0.783***	0.915	0.845	0.766
	(0.251)	(0.0447)	(0.0555)	(0.0387)	(0.114)	(0.118)	(0.117)
$(-1.5, -2]$	0.531***	0.603***	0.574***	0.578***	0.496***	0.607**	0.639***
	(0.0902)	(0.0676)	(0.0881)	(0.0512)	(0.0975)	(0.103)	(0.0864)
$(-2, -2.5]$	0.629***	0.613***	0.549***	0.556***	0.647***	0.603***	0.651***
	(0.0826)	(0.0526)	(0.0598)	(0.0428)	(0.0680)	(0.0828)	(0.0689)
$(-2.5, -3]$	0.452***	0.453***	0.402***	0.432***	0.517***	0.497***	0.457***
	(0.0362)	(0.0528)	(0.0575)	(0.0349)	(0.0639)	(0.0750)	(0.0492)
$(-3, -3.5]$	0.430***	0.465***	0.449***	0.410***	0.488***	0.529***	0.427***
	(0.0393)	(0.0417)	(0.0421)	(0.0257)	(0.0384)	(0.0500)	(0.0466)
$(-3.5, -4]$	0.353***	0.385***	0.352***	0.333***	0.511***	0.374***	0.415***
	(0.0613)	(0.0589)	(0.0457)	(0.0428)	(0.0620)	(0.0477)	(0.0594)
≤ -4	0.326***	0.334***	0.233***	0.322***	0.315***	0.335***	0.319***
	(0.0385)	(0.0398)	(0.0420)	(0.0313)	(0.0512)	(0.0645)	(0.0610)
Current CLTV (omitted bin: $[130, 140)$) :							
< 80	0.343***	0.359***	0.317***	0.440***	0.314***	0.313***	0.368***
	(0.0184)	(0.0254)	(0.0218)	(0.0447)	(0.0292)	(0.0312)	(0.0542)
$[80, 90)$	0.403***	0.412***	0.392***	0.530***	0.407***	0.387***	0.425***
	(0.0181)	(0.0259)	(0.0267)	(0.0389)	(0.0245)	(0.0339)	(0.0407)
$[90, 100)$	0.476***	0.507***	0.417***	0.633***	0.528***	0.482***	0.457***
	(0.0228)	(0.0283)	(0.0249)	(0.0343)	(0.0284)	(0.0352)	(0.0287)
$[100, 110)$	0.585***	0.603***	0.568***	0.756***	0.709***	0.597***	0.612***
	(0.0243)	(0.0401)	(0.0267)	(0.0288)	(0.0346)	(0.0426)	(0.0387)
$[110, 120)$	0.667***	0.715***	0.606***	0.873***	0.718***	0.721***	0.682***
	(0.0345)	(0.0388)	(0.0321)	(0.0113)	(0.0407)	(0.0490)	(0.0535)
$[120, 130)$	0.827***	0.865***	0.845***	0.948*	0.899*	0.827***	0.858***
	(0.0167)	(0.0288)	(0.0154)	(0.0241)	(0.0391)	(0.0476)	(0.0372)
$[140, 150)$	1.062	1.058*	1.134**	1.089**	1.072	1.062	1.096
	(0.0392)	(0.0281)	(0.0436)	(0.0286)	(0.0449)	(0.0627)	(0.0635)
$[150, 160)$	1.222***	1.237***	1.307***	1.139***	1.099	1.146	1.271**
	(0.0521)	(0.0683)	(0.0375)	(0.0205)	(0.0575)	(0.109)	(0.103)
≥ 160	1.454***	1.457***	1.527***	1.240***	1.363***	1.272*	1.580***
	(0.0513)	(0.0620)	(0.0658)	(0.0267)	(0.0589)	(0.141)	(0.0652)
FICO/100	0.589***	0.615***	0.558***	0.608***	0.588***	0.540***	0.526***
	(0.0107)	(0.0137)	(0.0119)	(0.0134)	(0.0227)	(0.0149)	(0.0209)
SATO	1.192***	1.178***	1.349***	1.184***	1.254	1.159	1.034
	(0.0552)	(0.0568)	(0.0651)	(0.0382)	(0.145)	(0.0898)	(0.0716)
Open liens = 2	1.184***	1.201***	1.187***		1.365***	1.079	1.206***

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	(0.0267)	(0.0397)	(0.0228)		(0.0391)	(0.0654)	(0.0460)
Open liens ≥ 3	0.976	1.016	0.955		1.067	0.838	0.961
	(0.0483)	(0.0709)	(0.0743)		(0.140)	(0.0973)	(0.0749)
Ppmt. penalty active	1.084***	1.024*	1.085***	1.075**	1.033	1.047	1.083**
	(0.0179)	(0.0119)	(0.0223)	(0.0272)	(0.0495)	(0.0517)	(0.0313)
Log(loan amount)	1.058	1.118***	1.013	1.050	0.996	1.074**	0.931
	(0.0364)	(0.0270)	(0.0390)	(0.0593)	(0.0274)	(0.0258)	(0.0417)
Origination LTV	1.051***	1.049***	1.040***	1.097***	1.065***	1.031**	1.075***
	(0.00733)	(0.00858)	(0.00680)	(0.00936)	(0.0144)	(0.0100)	(0.0128)
(Orig. LTV)^2/100	0.975***	0.976***	0.983***	0.945***	0.967***	0.985*	0.961***
	(0.00371)	(0.00448)	(0.00443)	(0.00554)	(0.00826)	(0.00646)	(0.00680)
Orig. LTV = 80				1.085**			
				(0.0291)			
Full documentation	0.598***	0.581***	0.643***	0.624***	0.605***	0.568***	
	(0.0161)	(0.0226)	(0.0176)	(0.0174)	(0.0220)	(0.0276)	
No documentation	1.085**	1.062	1.171***	0.937**	1.211**	1.064	
	(0.0312)	(0.0399)	(0.0398)	(0.0204)	(0.0817)	(0.0531)	
Cashout Refi	0.971*	0.927***	0.948*	0.919***	1.078	0.985	1.052
	(0.0132)	(0.0171)	(0.0216)	(0.0134)	(0.0456)	(0.0423)	(0.0446)
Non-cashout refi	1.001	0.986	0.977	1.012	1.130	0.983	1.033
	(0.0172)	(0.0213)	(0.0338)	(0.0176)	(0.0735)	(0.0320)	(0.0465)
Not owner-occupied	1.001	1.046	1.004	0.844*		1.132	1.060
	(0.0692)	(0.0713)	(0.0675)	(0.0620)		(0.0851)	(0.0752)
Condo	0.863***	0.833***	0.867**	0.898***	0.878***	0.760***	0.898*
	(0.0354)	(0.0366)	(0.0446)	(0.0243)	(0.0325)	(0.0234)	(0.0489)
12-month HPA	0.985***	0.984***	0.986***	0.993***	0.988***	0.983***	0.985***
	(0.00152)	(0.00155)	(0.00215)	(0.00167)	(0.00221)	(0.00456)	(0.00202)
Unempl. rate (U)	1.001	1.000	0.996	1.008	0.998	1.015	1.000
	(0.00597)	(0.00710)	(0.00503)	(0.00452)	(0.0123)	(0.0167)	(0.0105)
6-month ΔU	1.007	1.004	0.998	1.007	1.029	0.984	1.000
	(0.00882)	(0.00975)	(0.00667)	(0.00590)	(0.0190)	(0.0231)	(0.0203)
30-year FRM rate	1.103	1.283***	1.020	1.132**	1.383*	1.183	1.131
	(0.0740)	(0.0712)	(0.0598)	(0.0464)	(0.193)	(0.120)	(0.125)
State dummies	✓	✓	✓	✓	✓	✓	✓
Initial interest rate bins	✓	✓	✓	✓	✓	✓	✓
Loan type × calendar q. dummies	✓	✓	✓	✓	✓	✓	✓
Observations	1733461	1206985	840784	2484935	432029	796231	624331
# Loans	64353	51643	34250	103516	16479	29945	22105
# Incidents	27666	21281	11807	45278	6037	8846	6245
Log Likelihood	-240862.8	-178577.0	-94225.9	-413728.2	-43854.7	-69928.9	-47456.5

Exponentiated coefficients; Standard errors (clustered at state level) in parentheses.
In all regressions, baseline hazard allowed to vary by origination quarter.
Significance: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A-3: Proportional hazard models of cures and modifications of 60+ days delinquent loans

	Cure			Modification		
	(1) All	(2) CLTV>140 only	(3) All	(4) All	(5) CLTV>140 only	(6) All
Interest rate − missed rate (omitted bin: $[-0.01, 0.01]$):						
$\geq +0.01$	0.840	0.817	0.868	1.214	1.191	1.345*
	(0.0836)	(0.153)	(0.0707)	(0.216)	(0.284)	(0.200)
$(-0.01, -0.5]$	1.052	1.156	0.975	1.159	1.288	1.035
	(0.0880)	(0.160)	(0.0689)	(0.200)	(0.283)	(0.158)
$(-0.5, -1]$	1.235**	1.341*	1.299***	1.088	0.960	1.118
	(0.0993)	(0.193)	(0.0881)	(0.137)	(0.172)	(0.120)
$(-1, -1.5]$	1.419**	1.709**	1.343**	1.164	1.132	1.048
	(0.161)	(0.309)	(0.133)	(0.173)	(0.233)	(0.141)
$(-1.5, -2]$	1.247	1.382	1.173	1.166	1.076	1.107
	(0.145)	(0.268)	(0.113)	(0.130)	(0.166)	(0.107)
$(-2, -2.5]$	1.736***	1.786**	1.407***	1.259*	1.252	1.139
	(0.191)	(0.316)	(0.134)	(0.137)	(0.178)	(0.112)
$(-2.5, -3]$	1.753***	2.242***	1.538***	1.072	1.102	1.095
	(0.158)	(0.317)	(0.117)	(0.101)	(0.138)	(0.0901)
$(-3, -3.5]$	2.200***	2.551***	1.908***	1.000	0.851	1.027
	(0.186)	(0.352)	(0.137)	(0.0925)	(0.110)	(0.0825)
$(-3.5, -4]$	1.853***	2.084***	1.640***	0.898	0.901	0.904
	(0.184)	(0.339)	(0.137)	(0.0927)	(0.123)	(0.0808)
≤ -4	2.128***	2.114***	1.852***	0.801	0.889	0.821
	(0.233)	(0.384)	(0.165)	(0.0948)	(0.136)	(0.0847)
Current CLTV (omitted bin: $[130, 140)$) :						
< 80	2.861***			0.794*		
	(0.143)			(0.0860)		
$[80, 90)$	2.386***			0.878		
	(0.108)			(0.0770)		
$[90, 100)$	2.019***			0.987		
	(0.0831)			(0.0650)		
$[100, 110)$	1.624***			1.008		
	(0.0638)			(0.0542)		
$[110, 120)$	1.335***			1.054		
	(0.0526)			(0.0500)		
$[120, 130)$	1.160***			0.975		
	(0.0470)			(0.0453)		
$[140, 150)$	0.898*	1.226***		0.958	1.190***	
	(0.0423)	(0.0576)		(0.0443)	(0.0482)	
$[150, 160)$	0.950	1.304***		0.902*	1.114**	
	(0.0473)	(0.0610)		(0.0433)	(0.0462)	
≥ 160	0.738***	1		0.835***	1	
	(0.0306)			(0.0324)		
SATO	1.002	1.036		0.833**	0.866	
	(0.0346)	(0.0650)		(0.0466)	(0.0665)	
FICO/100	0.812***	0.749***		0.815***	0.819***	

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	Cure			Modification		
	(1) All	(2) CLTV>140 only	(3) All	(4) All	(5) CLTV>140 only	(6) All
	(0.0194)	(0.0368)		(0.0231)	(0.0311)	
Open liens = 2	0.936**	0.880*		0.874***	0.973	
	(0.0235)	(0.0497)		(0.0260)	(0.0435)	
Open liens ≥ 3	1.156**	1.137		0.895	1.044	
	(0.0566)	(0.0926)		(0.0530)	(0.0802)	
Ppmt. penalty active	0.942*	0.973		1.076	1.061	
	(0.0226)	(0.0679)		(0.0550)	(0.0775)	
Log(loan amount)	0.867***	0.894*		0.927**	0.884**	
	(0.0179)	(0.0455)		(0.0267)	(0.0387)	
Origination LTV	0.959***	0.863***		1.065***	1.058*	
	(0.00625)	(0.0178)		(0.0155)	(0.0300)	
(Orig. LTV)^2/100	1.026***	1.101***		0.963***	0.968	
	(0.00483)	(0.0151)		(0.00949)	(0.0181)	
Full documentation	1.209***	1.195***		0.824***	0.772***	
	(0.0278)	(0.0576)		(0.0271)	(0.0353)	
No documentation	1.014	0.993		0.933	0.918	
	(0.0382)	(0.0997)		(0.0512)	(0.0782)	
Cashout Refi	1.098***	1.280***		1.082**	1.215***	
	(0.0277)	(0.0668)		(0.0318)	(0.0481)	
Non-cashout refi	0.987	1.075		1.109**	1.108	
	(0.0294)	(0.0727)		(0.0417)	(0.0608)	
Not owner-occupied	0.897***	0.999		0.436***	0.426***	
	(0.0239)	(0.0623)		(0.0203)	(0.0301)	
Condo	0.954	0.917		0.603***	0.579***	
	(0.0229)	(0.0443)		(0.0206)	(0.0265)	
12-month HPA	1.019***	1.020***		1.017***	1.016***	
	(0.00123)	(0.00304)		(0.00184)	(0.00276)	
Unempl. rate (U)	0.989	1.020*		0.986*	0.972**	
	(0.00680)	(0.0100)		(0.00699)	(0.00839)	
6-month ΔU	1.000	1.003		1.001	1.026	
	(0.0104)	(0.0173)		(0.0131)	(0.0170)	
30-year FRM rate	1.163	0.988		1.817***	2.003***	
	(0.108)	(0.203)		(0.237)	(0.356)	
Age dummies	✓	✓	✓	✓	✓	✓
State dummies	✓	✓		✓	✓	
Missed interest rate bins	✓	✓	✓	✓	✓	✓
Loan type × calendar q. dummies	✓	✓	✓	✓	✓	✓
Observations	847262	424887	1354863	847262	424887	1354863
# Loans	65900	35322	106971	65900	35322	106971
# Incidents	14867	3349	23493	8649	4688	11678
Log-Likelihood	-128812.6	-26307.3	-218350.3	-70489.4	-35076.9	-103219.1

Exponentiated coefficients; Standard errors (clustered at loan level) in parentheses
In all regressions, baseline hazard allowed to vary by origination quarter.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.2 A simple model of mortgage default

This section presents a barebones model of a borrower's default decision, in order to derive qualitative predictions for what we should expect to see in the data. This model is a simplified version of other frictionless models in the literature, such as Kau et al. (1992).

We consider the following transaction. A homeowner owns a house priced S_0 and gets a three-period mortgage. The terms of the loan are that the lender advances some amount L_0 at time 0 and the borrower promises to make a periodic payment of m dollars at time 1 and to repay e dollars at time 2. The house price evolves stochastically but the payments m and e are deterministic. There is a market interest rate r and the borrower can borrow and lend unlimited amounts at that rate. If the borrower fails to pay m at time 1 or e at time 2, the lender sells the house to recover the money owed. We abstract here from the possibility of prepayment but this is, otherwise, a standard promissory note: the borrower promises to make a series of payments and the lender sells the collateral in the event that the borrower defaults. There are no frictions and the lender has no recourse to the borrower's other assets nor are there penalties for default. At time 1 the borrower can, if he so desires, default on his promised payment and buy an identical house with a new, smaller mortgage.

We now show three key propositions about mortgages. First, we show that we can characterize the mortgage described above as a call option on a call option on the house. Second, we show that negative equity is basically never sufficient for default to be optimal except in a situation where a borrower with negative equity at time t also has negative equity for all $s > t$ along every possible path for prices. And third, we show that changes in the size of the monthly payment affect repayment behavior more when borrowers have negative equity than positive and that for any reduction in principal, there is an equivalent reduction in the monthly payment that will reduce default by the same amount.

We stress that the purpose of this model is not to describe an actual borrower's decision. At the very least, a complete description of the borrower's choice would include borrowing limits on the riskless asset, liquidity shocks, and some sort of penalty for default. A more sophisticated model would differentiate the consumption of housing from that of other goods.[47] For our purposes, one can think of the household as having some fixed level of housing consumption and assume that the

[47] See e.g. Campbell and Cocco (2011) for a more realistic model of the default decision.

payments made by the borrower are net of rent. In other words, at time 1, the borrower actually pays $r + m$ on the mortgage and r if he opts to default. Despite these omissions, the model described below illustrates some basic principles of the mortgage default problem that apply in any model and also some common errors that economists and others make in thinking about the problem.

A.2.1 A formal model

Consider a three-period model with $t = 0, 1, 2$, with a finite sample space $\Omega = \{\omega_1, ..., \omega_K\}$, with a probability measure P, and a filtration $\mathcal{F}$. Suppose we have a security S with adapted price process $S_t, t = 0, 1, 2$ and a riskless asset with return r. Let M be a security in which the investor gets the option to pay m at time 1 for an option to buy security S at time 2 for price e. Let C be a call option on the house with strike price e exercised at time 2. Let M be a call option on C with strike price m exercised at time 1. Importantly, we assume absence of arbitrage, which implies the existence of an equivalent martingale measure Q defined on $\mathcal{F}$ and Ω. For simplicity, we assume the borrower does not discount the future.

Our first insight is that selling the house and buying the call option M at time 0 is an identical problem to the mortgage choice problem described above. The coincidence of these two strategies is a simple example of put-call parity and results from the fact the mortgage contract includes an embedded put option. To see the mechanics, start at time 2. If the borrower made the mortgage payment m at time 1, then his payoff would be:

$$(S_2 - e)^+,$$

which is also the payoff for the call option C, which the investor has the option to buy for price m. In other words, buying the call option and making the mortgage payment at time 1 are identical investments. At time 0, the borrower receives L_0 by receiving a loan of that amount and the buyer of M also receives L_0, but, in his case, it comes from selling the house for S_0, paying M_0 for the call option.

A.2.2 Equity and default

Going forward, we focus on the call option formulation, as it is far easier to work with. We now turn our attention to the question of the relationship between negative equity and default. As explained above, default at time 1 consists of failing to exercise the call option M. That is to say:

$$\text{Borrower Defaults} \Leftrightarrow C_1 < m. \tag{8}$$

What is surprising and somewhat counterintuitive is that neither the price process of the house S nor the outstanding balance of the loan e appear in equation (8). But, of course, C_1 depends on S, and, by absence of arbitrage, we can re-write equation (8) as

$$\text{Borrower Defaults} \Leftrightarrow C_1 = \tfrac{1}{(1+r)}\mathrm{E}_Q\left(S_2 - e\right)^+ < m. \tag{9}$$

Equation (9) allows us to establish a proposition that is central to understanding the default decision:

Proposition 1 *If and only if there exists $\omega \in F \in \mathcal{F}_1$ such that $S_2(\omega) > e$, then there exists $m > 0$ such that $C_1 > m$ and default is not optimal.*

Proposition 1 leads to two significant conclusions. First, suppose all we observe about a borrower is that he has negative equity, that is, that $S_1 < e$. What can we say about whether it is optimal to default? Not much. The sufficient condition for default is that the borrower must have negative equity in every possible future state of the world, and $S_1 < e$ is not a sufficient condition for that. In fact, the historical evolution of house prices indicates that nominal house prices often surpass their previous peaks over fairly short horizons even after deep busts. In other words, negative equity today has not, historically, been sufficient to eliminate the possibility of positive equity at some point in the future.

The second key point, and the one most relevant to this paper, is that as long as there is some state of the world in which the borrower has positive equity, then that borrower will continue making mortgage payments if we lower the payment enough. So long as $C_1 > 0$, then, we can set the payment at $C_1/2$ and ensure continued payment.

Before continuing, it is important to stress that we have not, in any way, ruled out the possibility that the borrower could opt against exercising M and go and buy another house with a new mortgage. However, the existence of such a strategy has no effect on our results; as long as there is no arbitrage, it is *always* the case that if $C_1 > m$, “walking away” and making any other investment is strictly wealth reducing relative to exercising the call option. To see why, note that if $C_1 > m$, then the borrower has an opportunity to buy an asset worth C_1 dollars but pay less than C_1. By exercising the option the borrower increases his wealth by $C_1 - m$ dollars.

What investment could possibly dominate that? No investment. Unless an arbitrage opportunity exists, the most valuable alternative investment a borrower can make with the m dollars will be worth m dollars, which is strictly less than C_1 and thus will reduce the borrower's wealth.

We conclude this discussion of negative equity by making one thing crystal clear. Proposition 1 does not imply that borrowers will never default. For a fixed m, a sufficiently large fall in prices will reduce the value of the call option so that $C_1 < m$ and default is optimal. But to deduce that default is optimal for a given borrower, we need to know the borrower's beliefs about the stochastic process for house prices, the discount rate, and the size of the monthly payment and, in a multi-period world, future monthly payments. Thus, the existence of borrowers with negative equity making their monthly payments is fully consistent with rationality in every sense.

A.2.3 Taking the model to the data

What does our simple model tell us to expect in the data? First, as we have already explained, Proposition 1 implies that unless there is no state of nature in which the borrower will have positive equity, borrowers will continue to make mortgage payments if the monthly payment is sufficiently low. Furthermore, changes in the monthly payment will affect repayment behavior, no matter how negative the equity. In fact, in the extreme, payment reduction should be *more* effective, the more negative the equity. To formalize this, imagine that we have a continuum of borrowers with the same S_1 and e but indexed by different levels of C_1. If $S_1 - e > m$, then $C_1 > m$ for all i and no borrower will want to default, meaning that perturbing the monthly payment will have no effect on default behavior. For lower levels of equity and a sufficiently high monthly payment, a small perturbation of the monthly payment should have no effect but a sufficiently large change will always affect borrower repayment behavior.

We can also think about the dynamics of default using the model. Up to now, we have focused on the decision to default at time 1, but we can think about default at time 0 by imagining that the borrower has to pay m_0 to buy security M at time 0. The default decision at time 0 is

$$\text{Borrower Defaults} \Leftrightarrow M_0 < m_0, \tag{10}$$

which, by absence of arbitrage is

$$\text{Borrower Defaults} \Leftrightarrow M_0 = \frac{1}{(1+r)} \mathrm{E}_Q \left(C_1 - m\right)^+ < m_0. \tag{11}$$

Default at time 0 obviously depends on the level of the current payment m_0 but also on the future payment m. In other words, a future reduction in the monthly payment should affect current willingness to pay.

A.3 Comparison with earlier work on upward resets

Researchers studying the causes of the mortgage crisis have argued that upward resets of ARMs played only a small role, a claim that may seem at odds with the results in this paper. For example, Foote, Gerardi and Willen (2012) write that for loans originated in January 2005,

> ...the initial interest rate was 7.5 percent for the first two years. Two years later, in January 2007, the interest rate rose to 11.4 percent, resulting in a payment shock of 4 percentage points, or more than 50 percent in relative terms. However, (...) delinquencies for the January 2005 loans did not tick up when this reset occurred. In fact, the delinquency plot shows no significant problems for the 2005 borrowers two years into their mortgages when their resets occurred.

On the face of it, the statement that there were "no significant problems for the 2005 borrowers... when their resets occurred..." appears to contradict the basic thesis of this paper that resets have an economically large effect on the default hazard. The statements are, however, both correct — there is no contradiction here. Figure A-1, which shows the performance of subprime ARMs that reset for the first time 24 months after origination (known as "2/28"), illustrates why. Panel A shows that the median rate paid rose by more than 200 basis point in month 24, and Panel B shows, consistent with this paper (and other work such as Pennington-Cross and Ho 2010), that the default hazard also jumped sharply. However, Panel C shows that consistent with the quote above, the incidence of defaults seems little affected by the reset: almost two-thirds of the defaults in this pool occurred prior to the reset, and the share of borrowers of the original pool who default just before the reset is about the same as the share who default just after.

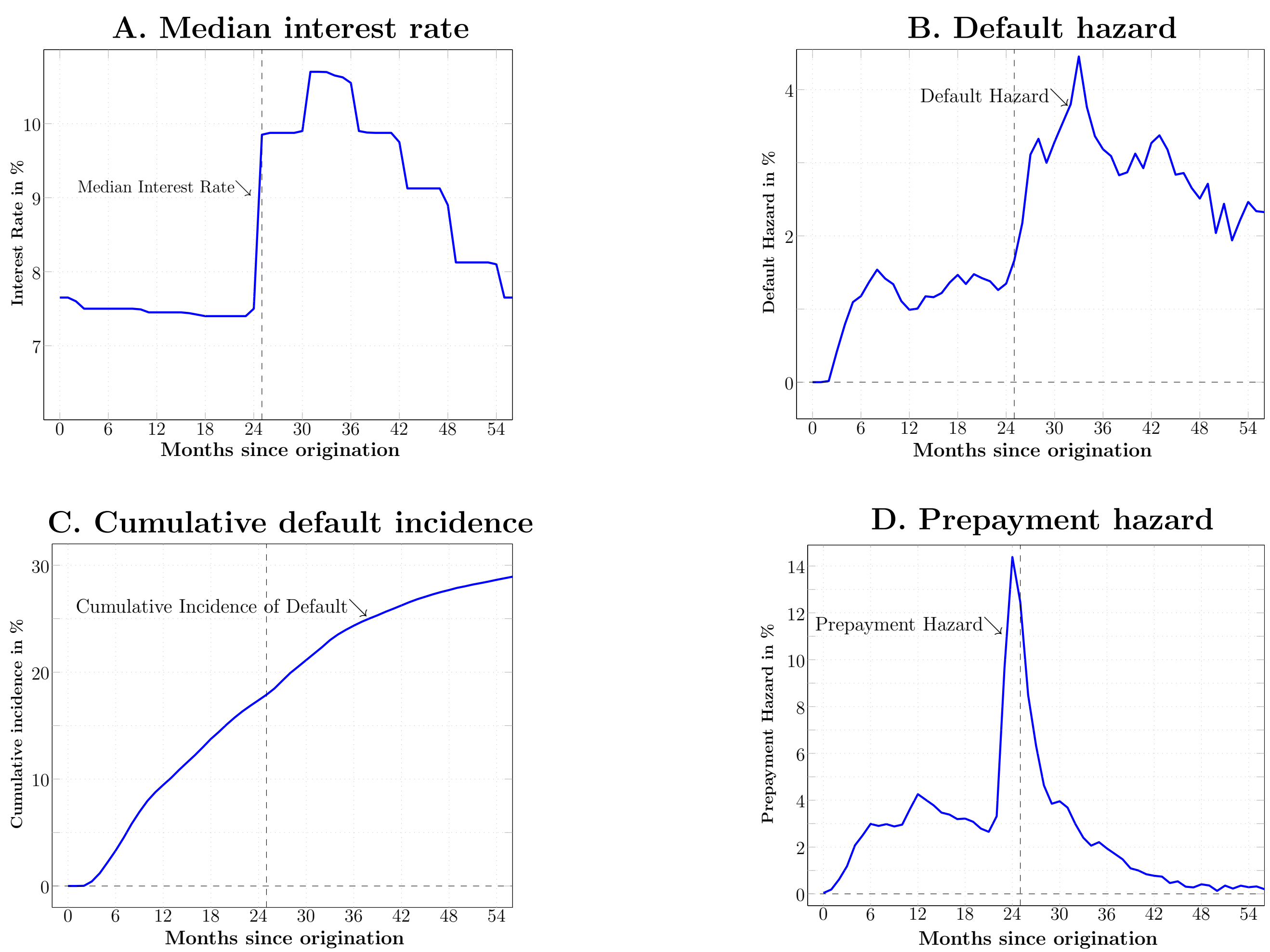
How can Panels B and C be consistent with one another? The key is prepayments, which are shown in Panel D: between months 21 and 25, about half the remaining loans in the pool prepay as borrowers either sell or get new mortgages to avoid the higher payments. The result is that the increase in the default hazard and the reduction in the number of loans in the pool roughly offset one another, which explains why there is no noticeable change in the path of the cumulative incidence shown in Panel C.

As we explained in Section 2.2.1, interpreting the default hazard in Panel B is not easy, and this cohort of loans illustrates precisely why. On one hand, Panel B could reflect a situation where there is no treatment effect of the payment increase, but only the borrowers who are not going to default can select into prepayment. Alternatively, if prepayments were randomly assigned, then the change in the default hazard would be a pure treatment effect of the higher monthly payment. The motivation for this paper is that we were able find a sample of borrowers who faced a reset but had no incentive and, in many cases, no ability to prepay.

The median payment increase for the borrowers in the 2005:Q1 sample depicted in Figure A-1 at the first reset is about 27%, which is roughly consistent with about a 1.5 percentage point rate change for the borrowers in our sample (who face lower payments to begin with, as their rates are lower and their loans are not amortizing over our sample period). According to Figure 3, such a payment increase would lead approximately to a $1/0.66 = 1.5$ fold increase in the default hazard. Since the actual hazard in Panel B more than doubles, we can attribute about half of the increase in the hazard in Panel B to the treatment effect of higher mortgage payments and the other half to the selection effect of more creditworthy borrowers prepaying. This means that of the one-third of the defaults in the pool that occurred after the reset, we can attribute about a quarter to the treatment effect of the payment increase (as half of the defaults would have happened even with a constant default hazard). If we look at the pool as a whole, it follows that we can only attribute approximately $1/12 = 1/3 \times 1/4$ of the defaults directly to the increase in payment size.

Figure A-1: The role of the resets in the foreclosure crisis

Sample is all amortizing subprime 2/28 mortgages originated in the first quarter of 2005 in the CoreLogic Loan-Performance ABS database. Default is defined as the first transition to 60-day delinquency.



A.4 The incidence of delinquency

The results in Section 4 of the paper show that interest rate reductions i) strongly reduce the default hazard, even for borrowers who are deeply underwater, and ii) also strongly reduce the prepayment hazard, although this is relative to a lower base rate than for the default hazard. In this appendix, we illustrate the combined effect on the cumulative number or *incidence* of defaults, which is affected by both default and prepayment hazards (because a loan that prepays can no longer default). To do so, we use our estimated coefficients to predict the cumulative fraction of delinquency for a fixed population of 5/1 loans with certain characteristics, starting at loan age 55 months.

The loan characteristics we chose are shown in Panel A of Figure A-2. These are close to the modal characteristics of loans that are still in the sample at age 55. Panel B shows the cumulative incidence of 60-day delinquency implied by the combination of our estimated baseline default and prepayment hazard models, with and without a 3-percentage point reduction in the interest rate occurring at loan age 61 months, and for two different assumptions about a loan's CLTV.[48] The upper two lines show that for a CLTV between 130 and 140, our estimates imply that at the initial interest rates, about one-third of loans that are not delinquent at age 55 would become 60-days delinquent by age 75. With the 3-percentage point reduction at the reset, however, this predicted fraction goes down to about 23 percent. If we compare the predicted incidence of delinquency only after loan age 63, when the reset becomes relevant, we see that only about 8 percent of loans become delinquent with the reset, but that about 17 percent become delinquent without the reset. Thus, over the span of one year after the reset, a 3-percentage point reduction, which corresponds approximately to cutting the payment in half, is predicted to reduce the incidence of default by about 9 percentage points, or more than 50 percent.

The lower set of lines in Panel B shows the predicted cumulative incidence of delinquency for loans that are not underwater, with a CLTV between 80 and 90. Unsurprisingly, such loans are predicted to go delinquent at a much lower though still non-trivial rate. While without the reset at age 61, the model predicts that about 14 percent of loans go delinquent by age 75, with the reduction the predicted fraction is below 10 percent. While the relative impact on delinquency rates is similar to the impact for the underwater loans, the absolute reduction in the number of delinquent

[48] In calculating the competing hazards, we take equal-weighted averages across the possible origination months, thereby accounting for the fact that the baseline hazards are allowed to vary at the origination-quarter level and that our regressions include calendar-quarter effects.

loans due to the reset is much lower than for underwater loans.

Finally, one might think that the model-implied incidence of delinquency seems unreasonably high, but Panel C shows that this is not the case. The panel shows the actual cumulative fraction of loans with a CLTV above 130 at loan age 55 that become 60-days delinquent by age 75, for our three different loan types. We see that among loans in the ARM 7/1 and 10/1 category, one quarter become 60-days delinquent over that time span. For 3/1s, which have benefitted from a number of downward rate resets, the corresponding number is below 20 percent. Most interestingly, the 5/1s, to which the counterfactuals in Panel B apply, were on a higher path than the 7/1+ loans prior to loan age 61, and it is plausible that 30 percent or more of these loans would have become delinquent without the reset. However, in actuality we see a notable change in the slope of the incidence function for these loans around age 62 (with a slight reduction in the slope already occurring around age 60, consistent with the findings from Section 4.1.2), so that by age 75 fewer than 20 percent defaulted.

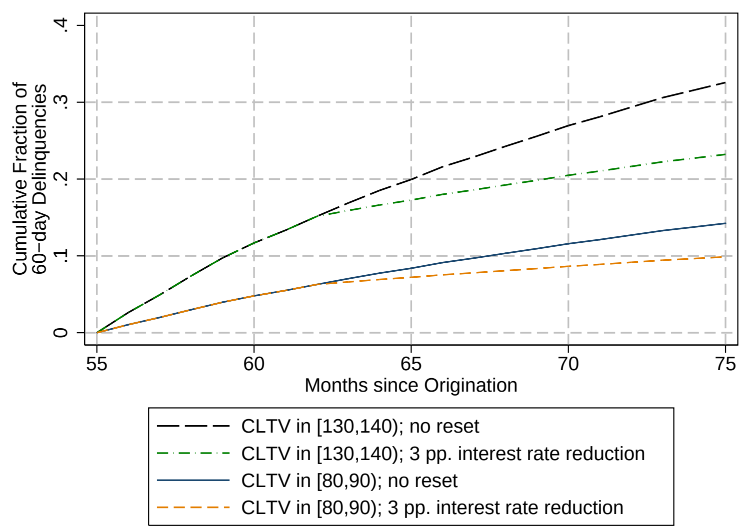
Figure A-2: Cumulative incidence of 60-day delinquency

Panel B shows the model-predicted incidence of default for a set of loans with characteristics given in Panel A, starting at loan age 55 months. Panel C shows the actual cumulative incidence of default for loans in our data with current CLTV of 130 at age 55 months. The fractions in this panel are based on loans in our sample originated up to August 2005 only, as loans originated after that are in the sample for less than 75 months.

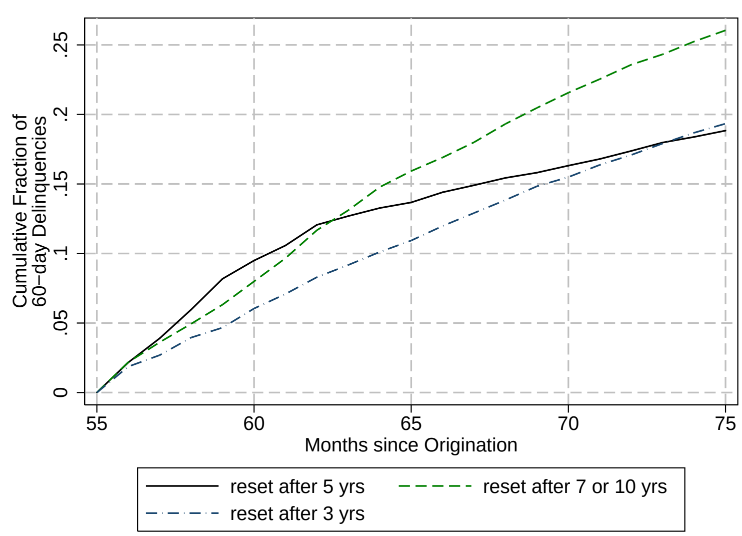
A. Characteristics of counterfactual loans

Loan type	5/1	Documentation	Low
Initial rate	6.25%	Investor	No
FICO	720	Condo	No
Open liens	2	State	California
Loan amount	200,000	Unemployment	8% (fixed)
Original LTV	80	12-month HPA	-4%
Purpose	Purchase	FRM rate	4.50%
Ppmt penalty	No		

B. Predicted cumulative incidence of delinquency



C. Actual cumulative incidence of delinquency for loans with CLTV>130



A.5 An alternative analysis of cures

Here, we provide an alternative analysis of cures, which is meant to complement the hazard analysis in the main text.

Now, we focus only on loans that have just become 60-days delinquent (that is, they have missed two payments). As Figure A-3 suggests, although the cure rate of these loans is much higher than that of all delinquent loans, these loans are still not very likely to cure—for loan ages between 25 and 55 months, 60-days delinquent loans without interest rate resets cure within three months at a rate of only 10 to 15 percent. However, as the figure also suggests, 5/1s become much more likely to cure around the time of the large interest rate reduction they witness.[49]

We estimate linear probability models where the dependent variable is whether a loan cures within the next three months. The independent variable of interest is the change in the borrower's interest rate during the month in which he becomes 60-days delinquent, relative to the interest rate at which the borrower failed to make his last payment.[50] In some specifications we also use leads and lags of these changes, as will be explained below.

We exclude from the estimation loans that in the LP modification dataset are marked as having received a modification in the month when the loan became 60-days delinquent or the following three months. We also drop loans that are not marked as getting modified but nevertheless experience an unscheduled interest rate decrease. In all regressions, we include a loan-type specific quartic function of the loan age in months, to account for the fact, clearly visible in Figure A-3, that younger loans are more likely to cure (if there is no interest rate reduction). We also include loan type $\times$ calendar quarter dummies.

Table A-4 displays the results. In column (1), we simply regress the cure indicator on the change in a delinquent borrower's interest rate. The coefficient of -0.047 (strongly statistically significant) means that borrowers whose interest rate decreases by 1 percentage point are predicted to have a probability of curing within three months that is 4.7 percentage points higher; for an interest rate decrease of 3

[49] In the figure as well as in the regressions in this section, we only retain a 60-days delinquent loan for the first month it is 60-days delinquent. However, the same loan—if it cures at least once—can be in the sample more than once, at different ages. In our regressions, we cluster the standard errors at the loan level.

[50] For instance, a borrower who is recorded as going delinquent during month 62 failed to make a payment during month 61, and the payment that was due at that time was based on the interest rate during month 60. His new payment is determined by the interest rate that applied during month 61. Thus, the variable "Δ_t rate" would be the change in the contract interest rate between months 60 and 61. We exclude interest rate increases.

percentage points the corresponding effect size would be 14.1 percentage points. This is a large effect: as can be seen in Figure A-3, it corresponds roughly to a doubling of the probability of curing, which corresponds closely to the findings of our analysis in the main text.

In the remaining columns, we allow for the effect of the interest rate decrease to occur before or after the actual decrease. The coefficients on the variables "Δ_{t+j} rate" should be interpreted as the effect of a 1-percentage point interest rate change in period $t + j$ on the probability that a borrower who goes 60-days delinquent in period t cures within three months. For $j \geq 4$, such an effect would be due to an anticipation effect: the cure occurs before the required scheduled payment is actually reduced. For $j < 0$, the coefficient measures the effect of the relatively lower interest rate on delinquent borrowers who went 60-days delinquent after the lower interest rate was already in effect; it can thus be interpreted as measuring to what extent a lower required payment makes it easier to recover from delinquency if delinquency occurred despite this lower payment.

In column (2), we control for the missed interest rate in addition to six leads and lags of interest rate changes. We see that the coefficient on the contemporaneous interest rate change is now of slightly larger magnitude, such that a 3-percentage point interest rate decrease in period t is now predicted to increase the probability of a 60-days delinquent loan in t curing by $t + 3$ by 17.7 percentage points. We also see that the coefficients on changes in $t - j$ for $j = 1, \ldots, 6$ are negative and significant, although somewhat smaller in magnitude. This is likely explained by the fact that borrowers who become delinquent under a lower required payment are "worse types" than those who become delinquent under the earlier, higher payment. Yet, as noted in the main text, the overall effect of the rate reduction is still to increase cures, even for borrowers becoming delinquent under low payments. For $t + 1$ and $t + 2$, the coefficients are negative and of economically significant magnitude; this is not necessarily due to anticipation but consistent with borrowers' making double or triple payments once the rate reduction occurs and thereby curing within three months. For an interest rate reduction in $t+3$ this would not suffice, and the detected (relatively small) effect on cures is either due to borrowers' making up payments in anticipation of the rate reduction, or a triple payment during the first month for which the interest rate decreases.[51] For $t + 4$ and higher, the coefficient approaches

[51] An example may be helpful: for a borrower who goes 60-days delinquent in month 59, "Δ_{t+3} rate" is the change in the contract interest rate between months 60 and 61, affecting the payment due in month 62, which will be reflected in the delinquency status of month 63, and thus after the three-month cure window we are considering. However, he may make up the missed earlier

zero, consistent with borrowers mostly (though not completely) failing to anticipate future decreases. This is qualitatively similar to the findings in Section 4.1.

In columns (3) and (4), we run the same regressions in two separate samples: 5/1s only and the other types only. The resulting coefficients are similar, although they tend to be of somewhat smaller magnitude and less significant when 5/1s are excluded. This suggests a potentially nonlinear effect of interest rate changes on cure probabilities, as interest rate decreases tend to be larger for 5/1s than for 3/1s. In column (5) we add the same control variables we were using in Section 4, which tends to increase the magnitude of the coefficients (a 3-percentage point decrease in the interest rate is now predicted to increase the cure probability by more than 20 percentage points). As a point of comparison, this effect on the probability of curing is similar to the difference between a borrower who has more than 20 percent equity in his property and one who is more than 20 percent underwater.

Column (6) restricts the sample to borrowers who are severely underwater (with a CLTV exceeding 140 percent) and finds coefficients of similar magnitude (although with larger standard errors due to the smaller sample), indicating that even severely underwater borrowers become much more likely to cure if their interest rate is reduced substantially.

Figure A-3: Cure rate of newly 60-days delinquent loans, by loan type

Cure = become current or pay off mortgage within 3 months of becoming 60 days delinquent.

payments during month 61, as he knows that the required payment during that month is lower.

Table A-4: OLS regressions of the probability to cure within 3 months

	(1) All	(2) All	(3) 5/1s only	(4) w/o 5/1s	(5) All w/Controls	(6) CLTV>140 only
Δ_t rate	-0.0467***	-0.0588***	-0.0589***	-0.0463***	-0.0722***	-0.0731***
	(0.00591)	(0.00687)	(0.00775)	(0.0142)	(0.00883)	(0.0128)
Δ_{t-1} rate		-0.0379***	-0.0428***	-0.00623	-0.0339***	-0.0257*
		(0.00875)	(0.0103)	(0.0155)	(0.0108)	(0.0149)
Δ_{t-2} rate		-0.0371***	-0.0473***	-0.00312	-0.0287***	-0.00450
		(0.00876)	(0.0108)	(0.0135)	(0.0103)	(0.0105)
Δ_{t-3} rate		-0.0419***	-0.0481***	-0.0209	-0.0429***	-0.0526***
		(0.00910)	(0.0109)	(0.0153)	(0.0111)	(0.0156)
Δ_{t-4} rate		-0.0307***	-0.0312***	-0.0260	-0.0290***	-0.0134
		(0.00808)	(0.00930)	(0.0172)	(0.00981)	(0.0129)
Δ_{t-5} rate		-0.0164**	-0.0137	-0.0186	-0.0193**	-0.00657
		(0.00770)	(0.00909)	(0.0149)	(0.00942)	(0.0110)
Δ_{t-6} rate		-0.0248**	-0.0331**	-0.00565	-0.0233*	-0.0151
		(0.0106)	(0.0139)	(0.0158)	(0.0128)	(0.0162)
Δ_{t+1} rate		-0.0406***	-0.0401***	-0.0265**	-0.0511***	-0.0439***
		(0.00593)	(0.00676)	(0.0122)	(0.00756)	(0.0109)
Δ_{t+2} rate		-0.0345***	-0.0355***	-0.0229*	-0.0401***	-0.0282***
		(0.00503)	(0.00560)	(0.0118)	(0.00638)	(0.00848)
Δ_{t+3} rate		-0.0139***	-0.0165***	0.00253	-0.0137***	0.00343
		(0.00394)	(0.00438)	(0.00878)	(0.00504)	(0.00576)
Δ_{t+4} rate		-0.00993***	-0.0117***	-0.000594	-0.0126***	-0.00761
		(0.00375)	(0.00415)	(0.00843)	(0.00479)	(0.00600)
Δ_{t+5} rate		-0.00441	-0.00423	-0.00861	-0.00569	-0.00692
		(0.00350)	(0.00376)	(0.00984)	(0.00466)	(0.00621)
Δ_{t+6} rate		-0.00293	-0.00307	-0.00433	-0.00257	-0.00266
		(0.00330)	(0.00357)	(0.00890)	(0.00431)	(0.00590)
CLTV < 80					0.196***	
					(0.0116)	
CLTV $\in [80, 90)$					0.128***	
					(0.00924)	
CLTV $\in [90, 100)$					0.0833***	
					(0.00675)	
CLTV $\in [100, 110)$					0.0485***	
					(0.00554)	
CLTV $\in [110, 120)$					0.0211***	
					(0.00484)	
CLTV $\in [120, 130)$					0.0146***	
					(0.00458)	
CLTV $\in [140, 150)$					0.00486	0.0124***
					(0.00461)	(0.00446)
CLTV $\in [150, 160)$					0.00407	0.00971**
					(0.00496)	(0.00454)
CLTV ≥ 160					-0.00572	0
					(0.00436)	
Loan-type-specific quartic fn. of loan age	✓	✓	✓	✓	✓	✓
Loan type × calendar q. dummies	✓	✓	✓	✓	✓	✓
Missed interest rate bins		✓	✓	✓	✓	✓
Other controls					✓	✓
Observations	122284	117375	76455	40920	71846	24132
# unique loans	92080	89288	58226	31062	54141	20483
Adj. R^2	0.0690	0.0728	0.0717	0.0740	0.107	0.0481

Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$