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Source: *American Economic Journal: Economic Policy*, November 2017, Vol. 9, No. 4 (November 2017), pp. 167-191

Published by: American Economic Association

Stable URL: <https://www.jstor.org/stable/26598350>

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Payment Size, Negative Equity, and Mortgage Default[†]

By ANDREAS FUSTER AND PAUL S. WILLEN*

This paper studies the treatment effect of monthly payment size on mortgage default, using a sample of adjustable-rate loans that experienced large payment reductions thanks to the recent low interest rate environment. Payment size has an economically large effect on repayment behavior; for instance, cutting the required payment in half reduces the delinquency hazard by about 55 percent. Importantly, the link between payment size and delinquency is equally strong for borrowers that are significantly underwater on their mortgage. Relying on payment reductions for identification circumvents the selection concerns due to prepayments that would be associated with rate increases. (JEL D14, G21, R31)

Do changes in a homeowner's monthly mortgage bill significantly affect the likelihood that they will default? The answer to this question is of critical importance to policymakers when designing interventions to reduce foreclosures, as well as for regulation and the conduct of monetary policy. For instance, during the recent US foreclosure crisis, policymakers had to decide between inducing lenders to reduce payments or principal in order to prevent defaults. When it chose to focus on the former, the government endured harsh criticism from commentators, who argued that reducing payments for underwater borrowers would not reduce the likelihood of default.¹

More broadly, the sensitivity of default to payment changes is central for questions of mortgage market design (Campbell 2013). In particular, a high sensitivity of default to payments provides a justification for the strong regulatory bias in the US toward fixed-rate mortgages (FRMs), which has resulted in an exceptionally high FRM share compared to other countries. At the same time, while FRMs prevent

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[†]Go to <https://doi.org/10.1257/pol.20150007> to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

¹For instance, John Geanakoplos and Susan Koniak wrote in a *New York Times* op-ed on March 5, 2009 (p. A31): "The [Making Home Affordable] plan announced by the White House will not stop foreclosures because it concentrates on reducing interest payments, not reducing principal for those who owe more than their homes are worth. The plan wastes taxpayer money and won't fix the problem." The Making Home Affordable initiative encompasses the Home Affordable Modification Program (HAMP) as well as the Home Affordable Refinancing Program (HARP) and some smaller programs.

payment increases, they may blunt the effectiveness of monetary policy: to benefit from interest rate reductions, FRM borrowers must refinance, which requires them to fulfill underwriting criteria for a new loan. This tends to be most difficult for precisely those borrowers at the highest risk of default—job-losers and homeowners with negative equity. Borrowers with adjustable-rate mortgages (ARMs), in contrast, benefit automatically from rate cuts.

Remarkably, given the importance of these issues, there has been no clear empirical evidence on the treatment effect of payment size on mortgage default because major selection problems historically made measuring this effect difficult. In this paper, we exploit a unique combination of events generated by the financial crisis to circumvent these problems. We study a sample of privately securitized hybrid ARMs over the period 2005–2011. Hybrid ARMs have fixed payments for 3, 5, 7, or 10 years and then adjust or “reset” periodically until the mortgage matures, meaning that the borrower’s required monthly payment can change substantially at a particular moment in the life of the mortgage. Historically, these resets almost always increased the monthly payment, and many borrowers responded by refinancing. As we explain in Section I, these refinances introduce a selection effect, since more creditworthy borrowers are more likely to refinance, and this makes it impossible to cleanly measure the treatment effect of the payment increase.

For two reasons, our sample avoids this selection problem. First, because of the macroeconomic environment over 2008–2011, required payments on most of the loans that reset *decreased*, often dramatically (see panel A of Figure 1). Second, house prices had fallen since origination of the mortgage, so that most borrowers in our sample were underwater on their mortgage at the time it reset. The combination of falling house prices and falling payments in this period meant that the typical borrower had neither the incentive nor the ability to refinance.²

The key to our identification is that borrowers in our sample receive a payment reduction as a function only of their loan type and of short-term interest rates at a fixed time after the origination date of their loan. In other words, the payment reduction is unconditional on any borrower covariates that may have changed since origination. This also makes our approach cleaner than if we measured the effect of payment changes from loan modifications, where borrowers need to apply and fulfill certain eligibility criteria, and servicers generally have some discretion regarding the terms they offer their borrowers.

We find that payment reductions have very large effects in our sample. Panel B of Figure 1 provides graphical evidence for our main finding. It 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, following industry convention, “5/1” ARMs) dropping from 1.7 percent in month 58 (three months prior to the reset) to 0.5 percent by month 64 (after the reset). Payments for these borrowers had on average roughly been cut in half by the reset. The ARMs that reset after 7 or 10 years (“7/1+”), and thus had not yet reset, display no similar drop in defaults around month 61. Until

²The loans in our sample were all sold into private-label securities and thus were ineligible for HARP, which only applied to loans securitized by Fannie Mae and Freddie Mac and fulfilling other criteria.

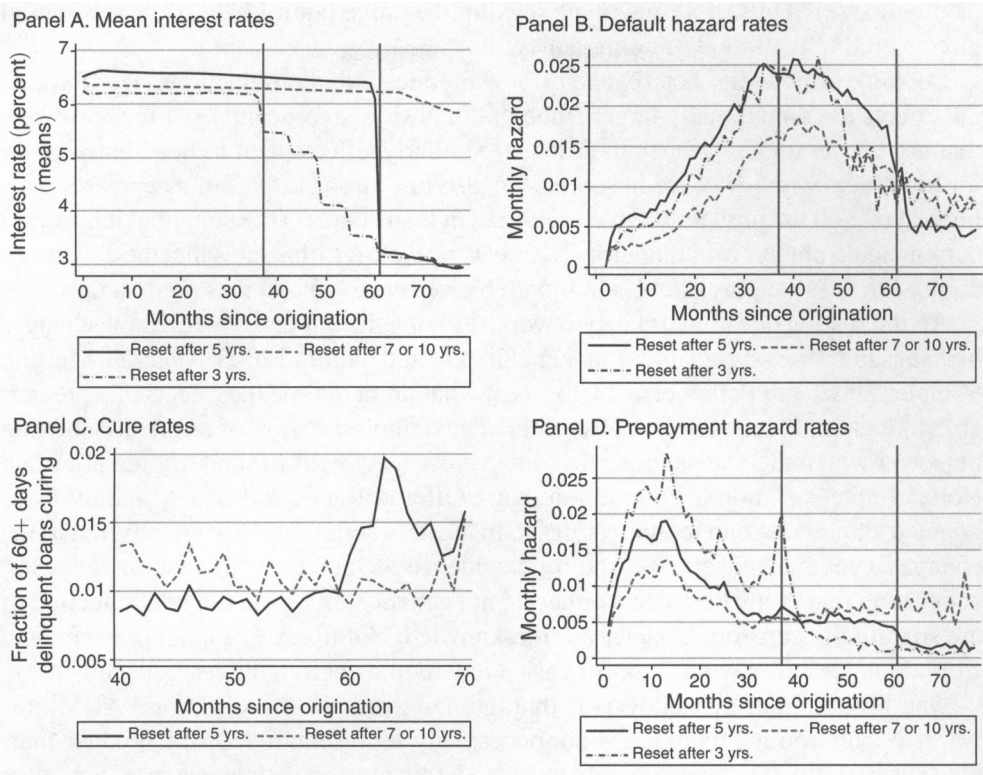


FIGURE 1. RESETS, DEFAULTS, CURES, AND PREPAYMENTS OVER THE LIFE OF THE LOAN

Notes: Based on a sample of hybrid Alt-A loans with first reset after 3, 5, 7, or 10 years, and with interest-only feature for 10 years, originated between January 2005 and June 2006. Except for exclusions based on origination characteristics as explained in online Appendix B, the figure includes all active unmodified 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.

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 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 hazard. We robustly find large effects. For instance, a 3-percentage-point reduction in the interest rate, which in our sample roughly corresponds to cutting the monthly payment in half, is estimated to reduce the monthly delinquency hazard by about 55 percent. Since our data includes updated information on all liens on a property, we can compare the size of this effect with that of a borrower's equity position (while holding the payment size constant). For example, the 55 percent reduction in the delinquency hazard is approximately equivalent to the difference in hazard rates between a borrower with combined loan-to-value ratio (CLTV) of 135 and one with CLTV of 90 (but otherwise identical observables and

payment size). Thus, both payment size and negative equity have very substantial effects on the likelihood of delinquency.

Looking at potential heterogeneity of the effects of rate reductions, we find that the effects are directionally larger (though not always significantly so) for borrowers that are deeply underwater on their loans. Similarly, effects tend to be slightly larger for borrowers with lower credit scores. We also find strong effects of a decrease in the interest rate on the probability that a delinquent loan “cures” (meaning that it becomes current again or pays off voluntarily). Panel C of Figure 1 illustrates that the cure probability for 5/1s roughly doubles in month 63 relative to what it was pre-reset.

An interesting question, related to work studying the effects of predictable changes in disposable cash on consumption (e.g., Shapiro and Slemrod 2003; Agarwal, Liu, and Souleles 2007; and Parker et al. 2013), is at what point in time the effects of a predictable interest rate decrease actually occur. The default decision of a forward-looking borrower who understands the terms of his mortgage, tracks the underlying index rate (for example, six-month London Interbank Offered Rate), and is not liquidity constrained should respond to innovations to his beliefs about future payments, not to the change in payments itself. We find little evidence of such forward-looking behavior, suggesting that many borrowers either do not actively anticipate the rate reduction or 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.

One limitation of our analysis is that the non-prime (“Alt-A”) hybrid ARM borrower population we focus on is not necessarily representative of the broader market. Specifically, borrowers may have chosen this mortgage type because they were particularly liquidity constrained, and the effects we find might be smaller for a priori less constrained borrowers. While this is possible, we note that our effects are identified from payment changes that occur three or more years after origination, and we would expect the most liquidity constrained borrowers to already have defaulted at that point. Additionally, we replicate our main analysis using a sample of prime borrowers (many of them with “jumbo” loans) and find similar-sized effects.

A set of contemporaneous related papers also suggests that the effects we document are not confined to non-prime or ARM borrowers: Tracy and Wright (2016) and Keys et al. (2014) find similar effects of interest rate reductions on the delinquency rates of ARM borrowers in the prime conforming segment. Turning to FRMs, Zhu (2012) uses internal Freddie Mac data to study the effects of HARP, and Ehrlich and Perry (2015) exploit a discontinuity in eligibility for FHA streamline refinancing; both papers find that payment-reducing refinances under these programs substantially lower subsequent delinquency rates. In addition, Keys et al. (2014) and Di Maggio, Kermani, and Ramcharan (2014) build on our identification strategy to document that the downward rate resets also affect durable purchases and debt balances.

Aside from these papers, which focus on resets or refinancings, our analysis also complements other recent work that has focused on the role of income and employment shocks (Gerardi et al. 2013; Hsu, Matsa, and Melzer 2014) or liquidity constraints (Elul et al. 2010, Anderson and Dokko 2011) in generating defaults. This body of work provides an important input for discussions of optimal policies in a housing debt crisis (Remy, Lucas, and Moore 2011; Eberly and Krishnamurthy

2014); we return to some of the policy implications that follow from our results in the final section of the paper.

I. Theoretical Considerations and Identification

A. Predicted Effects of Payment Size on Default

In theoretical analyses of mortgage default, 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 online Appendix A, we present a barebones, frictionless model in the spirit of Kau et al. (1992) that demonstrates, among other things, 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. Unless that is the case, there is always a monthly payment low enough such that it is optimal for the borrower to not default.

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 2015) is typically that borrowers get hit by “liquidity shocks” (for instance, job loss) such that the effective cost of having to make a payment today weighs more than the expected future value of the house. This model is often referred to as “double trigger,” because the combination of negative equity and some shock drives defaults. In such a world, a smaller required 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 the borrower finds it optimal to keep making payments rather than default.

B. Identification Issues

In this section, we review why it is so difficult to identify the effects of payment size on mortgage default 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 default 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: for instance, lenders may require some borrowers to pay a higher

interest rate precisely because these borrowers are at a higher risk of default. Similarly, a strategy relying purely on time-series variation in interest rates would be confounded by the fact that economic conditions also vary over time and affect default rates.

Research on loan modifications, such as Adelino, Gerardi, and Willen (2013); Agarwal et al. (2011); and Haughwout, Okah, and Tracy (2016), has studied how payment reductions perform relative to principal reductions in affecting re-default rates. While studying the effects of modifications is of course directly policy relevant, it is difficult to control for selection effects, since the set of borrowers to receive modifications, and the terms they receive, are generally not determined exogenously. Therefore, it is challenging to extrapolate the findings from such studies either to larger-scale modification programs or to policy interventions aimed at reducing delinquency in the first place.

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. 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. Furthermore, to identify the effect of payment size, only *downward resets* will suffice: as we explain more formally now, the prepayment option makes it impossible to use upward resets to reliably estimate the causal effect of payment size on defaults.

Selection versus Treatment Effects.—Consider a vintage of ARM borrowers divided into two types $i \in \{g, b\}$ for good and bad, with prepayment and default 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 , so that the prepayment and default hazards in the population are $p_t = \sigma_t p_t^b + (1 - \sigma_t) p_t^g$ and $d_t = \sigma_t d_t^b + (1 - \sigma_t) d_t^g$.

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}).

To illustrate the selection issue, equation (1) decomposes the change in the observed default hazard into treatment and selection effects:

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

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. This will happen if a larger fraction of good borrowers than bad borrowers leaves the population during period t , for instance if $p_t^g > p_t^b$. (In reality, the share of bad borrowers could also grow “exogenously” if economic conditions are worsening.)

For illustrative purposes, we now study the special case where only good types prepay ($p^b = 0$ and $p^g > 0$) and only bad types default ($d^b > 0$ and $d^g = 0$). In that case, equation (1) simplifies to the following, where the second equality follows from the definitions of p_t and d_t :

$$(2) \quad \hat{\phi} = \phi \cdot \frac{\sigma_{t+1}}{\sigma_t} = \phi \cdot \frac{1 - d_t^b}{1 - p_t - d_t}.$$

It is easy to see from equation (2) that, all else equal, an increase in the prepayment hazard prior to the reset will increase the estimated effect of the reset. How significant is this in practice? To see, we consider subprime “2/28” mortgages originated in the first quarter of 2005. These loans, which first adjust in the 25th month, played a particularly prominent role in the crisis as they had very high default rates and their resets, which increased the average interest rate from 7.2 percent to 9.7 percent, coincided with the collapse of the subprime market in 2007. Since the reset would potentially start affecting delinquencies in the 27th month, we define months 21–26 to be the pre-reset period and months 27–32 as the post-reset period.

Table 1 shows that in month 21, there were 143,444 active loans, of which 8.5 percent defaulted and 40.2 percent prepaid over the subsequent six months. In the six months after the reset, 13,454 or 18.3 percent of the remaining loans defaulted, meaning that $\hat{\phi} = 2.16$.³ How much is selection and how much is treatment? That depends on what assumptions we make. If we assume as above that only good types prepay and bad types default, and, for the purpose of discussion, that bad types are half the population, then the selection effect is $(1 - 2 \cdot 0.085)/(1 - 0.402 - 0.085) = 1.62$, and the treatment effect of the reset is 1.33. However, if we did not believe there was any selection effect at all (meaning $d^b = d^g$ in equation (1)), then the treatment effect ϕ would equal 2.16. In other words, the observed data is consistent with a treatment effect of the reset as small as 33 percent and as large as 116 percent.

Equation (2) also illustrates why our focus on downward resets helps so much. As shown in panel D of Figure 1, the prepayment hazard around the reset of 5/1s in our sample is about 0.5 percent per month, or 3 percent over six months. If we redo the calculations above under the assumption of a strong selection effect, we would get a selection effect of $(1 - 2 \cdot 0.085)/(1 - 0.03 - 0.085) = 0.94$ and thus a treatment effect of 2.30, which is very close to what one would have calculated if one assumed that prepayments and defaults were random. In other words, with such low prepayment rates, whether one believes there is a strong selection effect or not makes little difference.

³ Previous researchers (for example, Ambrose, LaCour-Little, and Huszar 2005; deRitis, Kuo, and Liang 2010; or Pennington-Cross and Ho 2010) have found similar effects of upward resets on default and prepayment hazards. We present additional evidence from an Alt-A vintage in online Appendix E.

TABLE 1—DEFAULTS AND PREPAYMENTS AROUND UPWARD RESETS: AN EXAMPLE

	#	Hazard
Active loans in month 21	143,444	
– Defaults in months 21–26	12,164	8.5%
– Prepayments in months 21–26	57,707	40.2%
= Active loans in month 27	73,573	
– Defaults in months 27–32	13,454	18.3%

Notes: Default and prepayment around the time of the reset of subprime 2/28 mortgages originated in the first quarter of 2005. First payment at higher interest rate due in month 26; this payment affects delinquency status in month 27 (see Section IID for a detailed discussion on timing).

Source: CoreLogic LoanPerformance

As a final point, this analysis illustrates that the finding of a large treatment effect of payment shocks does not necessarily contradict the finding that resets of subprime ARMs did not lead to a large increase in defaults (for example, Sherlund 2008 or Foote, Gerardi, and Willen 2012). Table 1 shows that there were 12,164 defaults in the six months prior to the reset and 13,454 in the six months after. As we have shown, that relatively small increase in defaults (11 percent) is consistent with a large treatment effect of the resets (if we assume that prepayment was random) as well as a more modest one (if we assume that good types are more likely to prepay around the reset, as seems plausible).

II. Empirical Methods

In the remainder of the paper, we estimate hazard models of default and prepayment at the loan level. 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.

A. Data

Our main analysis is based on a sample of 221,000 Alt-A 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. We provide an overview of the data here; online Appendix B discusses additional details.

The LP dataset includes basic origination information such as the borrower FICO score, the original LTV, the zip code of the property, or the loan purpose (purchase or refinance). Most importantly for us, it 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 dataset also includes a new, dynamically updated measure of a borrower’s leverage in the property (called “TrueLTV” by LP), which is based on information on all liens. This provides us with a more accurate measure of CLTV than what is generally available in other mortgage datasets, which for the most part only contain information on the first lien. Furthermore, the home value is estimated

by an automated valuation model, which should be less noisy than local home price indices.⁴

The LP dataset contains both Alt-A and subprime mortgages; we focus on Alt-A because subprime ARM contracts typically contained “floors” such that the interest rate could not fall below the initial rate (Bhardwaj and Sengupta 2012). Alt-A (or “near prime”) mortgages were targeted at borrowers with (what were perceived to be) relatively minor credit quality issues or an inability or unwillingness to provide full documentation of income and assets. Mayer, Pence, and Sherlund (2009) and Sengupta (2010) provide an overview of the Alt-A market and how it compares to subprime.

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) period. An IO period means that over that time, the borrower only pays interest, without amortizing the mortgage. 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. Also, we chose 10-year IO mortgages because 5-year-IOs (which are also quite common) 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.⁵ See Amromin et al. (2010) and Barlevy and Fisher (2010) for recent analyses of IOs and related types of mortgages.

Panel A of Figure 2 shows the short-term rates to which the loans in our sample are indexed (most commonly with a margin of 225 basis points over the index). Panel B 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 rate floors and caps, as well as the initial rate). Subsequent resets (happening every 6 or 12 months) 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 2 shows other basic information about our sample. Panel A shows that the market for Alt-A hybrid ARMs grew markedly over the origination period we consider, and that the mix of loans changed toward longer-duration ARMs, likely due to a flattening of the yield curve. Panel B of Table 2 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

⁴In earlier versions of this paper, we compare the effects of different CLTV measures in more detail, and document results that are indeed consistent with the TrueLTV measure containing less measurement error than a “self-updated” LTV based on local home price indices.

⁵Among hybrid ARMs with fixed-rate periods of three years or longer originated in 2005/2006, the share of 10-year-IOs is approximately 59 percent. About 20 percent were 5-year-IOs, while only about 13 percent were regularly amortizing over the full loan term. Overall, the value-weighted shares of Alt-A originations over 2005/2006 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.

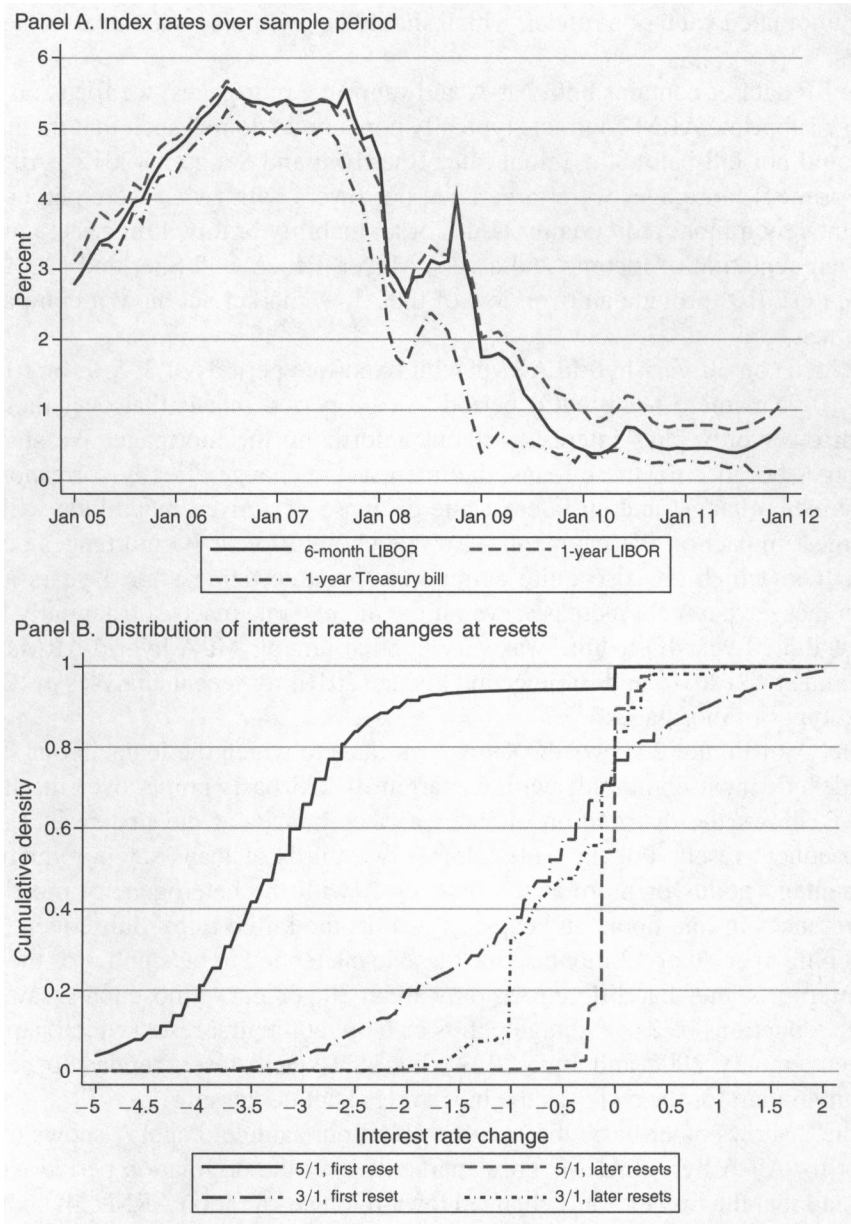


FIGURE 2. INDEX RATES AND RESETS

Notes: 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: FRED). In our sample, 69 percent of loans are indexed to the six-month LIBOR, 29 percent to the one-year LIBOR, and 2 percent to the one-year Treasury bill rate. 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).

than half of 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 US population. Comparing the loans with different fixed-rate periods in our sample, one notices that the 10/1s,

TABLE 2—SUMMARY STATISTICS

	3/1 ARMs		5/1 ARMs		7/1 ARMs		10/1 ARMs		Total
	# (000s)	Share (%)	# (000s)	Share (%)	# (000s)	Share (%)	# (000s)	Share (%)	# (000s)
<i>Panel A. Distribution of loan types</i>									
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
					3/1s	5/1s	7/1s	10/1s	Total
<i>Panel B. Origination characteristics</i>									
Origination amount (\$1,000s)					294 (170)	272 (164)	345 (200)	414 (218)	306 (187)
LTV on first lien					78 (8)	77 (9)	77 (11)	74 (12)	77 (9)
CLTV (TrueLTV)					93 (20)	94 (20)	93 (20)	88 (22)	93 (21)
Number of liens					1.7 (0.5)	1.7 (0.5)	1.6 (0.5)	1.5 (0.5)	1.7 (0.5)
FICO score					714 (42)	710 (45)	717 (46)	721 (46)	713 (45)
Initial interest rate (%)					6.2 (0.7)	6.6 (0.8)	6.6 (0.6)	6.3 (0.5)	6.5 (0.7)
Margin over index rate (%)					2.5 (0.5)	2.5 (0.5)	2.4 (0.3)	2.4 (0.2)	2.5 (0.5)
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
<i>Panel C. Mean CLTV (active loans only) at different points over sample period (percent)</i>									
January 2008					109 (26)	108 (26)	107 (25)	102 (26)	107 (26)
January 2010					144 (47)	142 (48)	139 (44)	130 (43)	139 (47)
November 2011					150 (50)	147 (50)	146 (48)	137 (45)	145 (49)
<i>Panel D. Outcomes (as of November 2011)</i>									
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

Note: Standard deviations for continuous variables are in parentheses.

although larger, are somewhat less risky than the others, based on characteristics at origination.

The Alt-A ARMs that originated in 2005 and 2006 are an exceptionally troubled group of loans. Panel C of Table 2 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. Panel D of Table 2 shows that

by November of 2011, the last month of performance data in our sample, 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.

In online Appendix D, we repeat our analysis on a sample of privately-securitized prime mortgages (many of them “jumbo” loans that were too large to be securitized by Fannie Mae or Freddie Mac). These loans have much lower default rates than the Alt-A loans we focus on in the main text, yet we find effects of rate resets of similar size (in relative terms) as those presented in the next section.

B. Econometric Methods

We conduct a competing risk analysis of prepayment and default, following other papers in the literature (for example, Deng, Quigley, and van Order 2000; Foote et al. 2010). We use a Cox proportional hazard framework, which posits that the hazard rate of loan i at loan age t months for outcome $n \in (\text{default, prepayment})$ is given by

$$(3) \quad h^n(t | \mathbf{X}_{it}, R_{it}) = h_0^n(t) \cdot \exp(\beta_1^n X_{1,i} + \beta_2^n X_{2,t} + \beta_3^n X_{3,it} + \gamma^n R_{it}).$$

Our specifications include different types of explanatory variables: $X_{1,i}$ is a vector of characteristics of the loan at origination like the borrower’s FICO score; $X_{2,t}$ are macroeconomic variables like the unemployment rate; and $X_{3,it}$ are time-varying, loan-specific variables like the updated CTLV. Our main focus is on R_{it} , the interest rate on loan i at time t , which we measure relative to the loan’s initial interest rate.

In proportional hazard models, the exponential functional form implies that the effect of a change in a control variable is assumed to be multiplicative. As discussed further in Section IIIA, this implies that the absolute effect on the hazard of a change in a regressor depends on the levels of all other regressors. The baseline hazard $h_0(t)$ for both outcomes is unrestricted, and we let it vary by origination quarter of the mortgage (that is, we have six different baseline hazards) in order to allow for differences in underwriting standards or other unobservables. The default and prepayment hazards are assumed to be independent.

For the purpose of our analysis, we define default as occurring when the servicer reports a borrower as 60 days delinquent. In our data, four out of five 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 the borrower repays the loan in full. In our framework, default and prepayment are competing risks, meaning that for the purpose of our analysis, a loan “dies” when it prepays or the first time it becomes 60 days delinquent.⁶ Such loans that are

⁶As explained in more detail in online Appendix B, we treat mortgages as censored that are subject to an interest rate increase, since as explained in Section I, such upward resets give rise to potentially important selection biases. We also treat mortgages as censored when they are marked in the LP data as being subject to a loan modification.

60 or more days delinquent do not become irrelevant for us, however, because we consider them separately in our cure analysis, described in online Appendix G.

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 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 differ from the relative performance just before the reset. However, we note that not imposing this restriction leaves the results almost unchanged (see Table A.1 in the online Appendix), suggesting that this issue is of minor importance in our sample.

C. Control Variables

Our main variables of interest are the borrower’s current interest rate (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, the log of the origination amount, and dummies for the current number of open liens, the presence of an active prepayment penalty, purpose type, documentation, and for whether the loan is on a second home or investment property, or a condo. We also include 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 monthly Freddie Mac 30-year FRM rate, which may affect prepayments. Finally, to account for differences in the legal environment across states (Ghent and Kudlyak 2011), we include state dummies in our regressions.

For the sake of brevity, our discussion in the main paper focuses on the effects of rate reductions and CLTV; the coefficients on other controls are shown in the tables in the online Appendix and are generally in line with other research.

D. 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. Suppose a borrower has a loan that resets from a fixed rate to an adjustable rate on June 1, when interest starts accruing at the new rate. Since mortgage payments are made in arrears, the first monthly payment using

the new rate will not be due until July 1. Furthermore, 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.

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). In contrast, 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 formally in online Appendix A), the net present value of staying in the mortgage depends on the level of all future payments. Therefore, if a borrower's expected required payment in any future period decreases, he should become less likely to default today. 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. We assume that a borrower always knows the interest rate that will affect his delinquency status two months out. This is because 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—in our example, the borrower would be notified sometime in May, since the new interest rate for June typically depends on the value of the index interest rate on the first business day of the month prior, i.e., May 1. 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 on the first of the month and assumes that this rate follows a random walk.⁷ Using information on a borrower's contract terms we then impute what the borrower would likely expect the interest rate to be up to six months in the future.

III. Results

This section presents the results of estimating equation (3) for defaults and prepayments. After presenting results from a baseline specification, we study potential heterogeneity of the effects on defaults for different borrower/loan characteristics, and the timing of the effects. In our figures as well as the complete regression tables, which are given in the online 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 $1/(\text{hazard ratio})^2$).

A. Defaults

Figure 3 displays the main coefficients of interest from our baseline default regression, along with 95 percent confidence intervals. The figure shows that rate

⁷ Kojen, Van Hemert, and Van Nieuwerburgh (2009) study time-series determinants of demand for FRMs versus ARMs and find that a backward-looking specification for interest rate expectations explains borrower choices well.

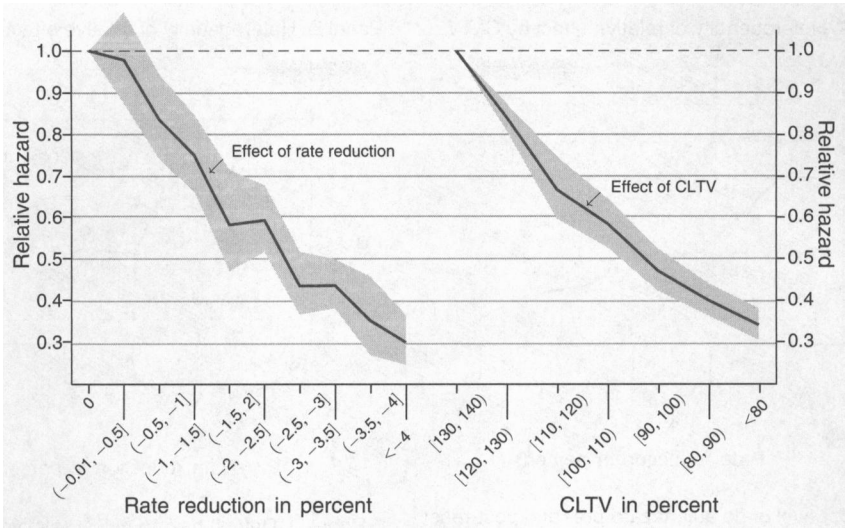


FIGURE 3. ESTIMATED EFFECTS OF RATE REDUCTION AND CLTV ON DEFAULT HAZARD

Notes: Figure 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 regressions of 60-day delinquency. Bands are 95 percent confidence intervals (based on standard errors clustered at state level). Regressions control for a large set of additional variables, as described in Section IIC. Coefficients and standard errors are shown in column 1 of Table A.1 in the online Appendix.

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 lowers the probability of default by about 40 percent (the estimated hazard ratio is close to 0.6), with a 95 percent confidence interval of about 30–50 percent. A 4-percentage-point reduction lowers the default hazard by two-thirds, with a confidence interval for the effect of 60–75 percent.

To provide additional perspective on the economic magnitude of these estimates, the plot includes the estimated effects of different levels of CLTV. For instance, a 3-percentage-point rate 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, is estimated to lower the default hazard by about 55 percent. This effect is roughly comparable to the difference in hazard rates between two borrowers with CLTV of 135 and 90, respectively, holding other covariates and, in particular, the payment size fixed.

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.⁸ That said, we try to control 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.

⁸Scharlemann and Shore (2016) make progress on identifying the causal effect of a borrower’s negative equity position on default by exploiting kinks in HAMP principal reduction schedules.

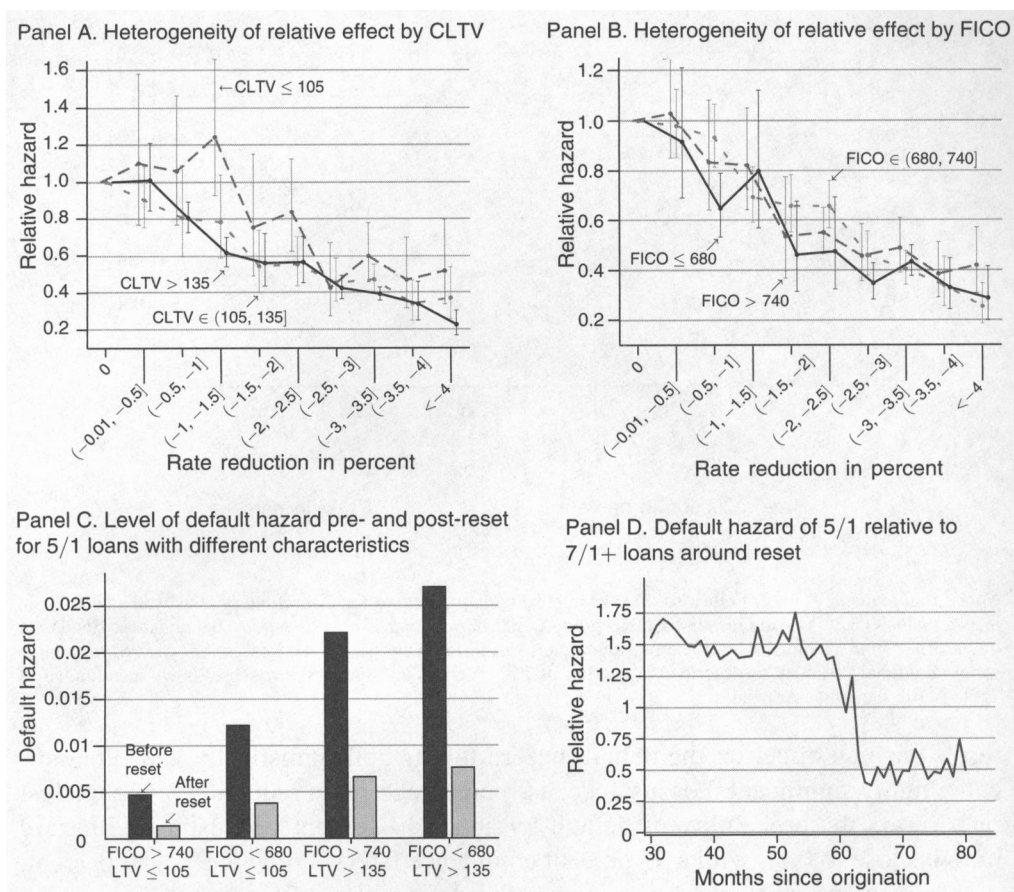


FIGURE 4. DEFAULT HAZARD: HETEROGENEITY BY CLTV AND FICO, AND ABSENCE OF PRE-TRENDS

Notes: Panels A and B show estimated effects on default hazard of rate reductions when effects are allowed to vary across three CLTV bins or FICO bins (otherwise regression is identical to baseline specification). Vertical bars are 95 percent confidence intervals (based on standard errors clustered at state level). Panel C displays average monthly default hazards for 5/1s before the reset (months 53–60) and after the reset (months 63–70). Panel D displays the ratio of observed delinquency hazards of 5/1 and 7/1+ loans.

Heterogeneity of Effects.—While the results from the baseline specification show that there are strong effects of rate reductions on default probabilities for the sample as a whole, it is interesting to consider whether the effects vary with loan characteristics such as the CLTV or the borrower’s FICO score. In Figure 4, we plot the results of additional specifications in which we interact the rate reduction bins with either three CLTV bins or FICO bins. The CLTV bins were chosen to have roughly the same number of observations in each (over the most relevant loan age range) while for the FICO bins we chose commonly used thresholds.

Panel A compares the effect of the rate reduction on the hazard ratio across CLTV bins, and shows that the effects tend to be largest for the most underwater borrowers ($CLTV > 135$). Thus, even for borrowers with highly negative equity, the propensity to enter delinquency is strongly reduced by a cut in the required monthly payment. The point estimates of the effects of rate reductions tend to be smallest for

borrowers that are not (or only mildly) underwater; however, as illustrated by the 95 percent confidence interval bars, the difference between the groups is only significant for some of the rate bins. Similarly, panel B shows that the hazard ratios tend to be lowered most strongly for the borrowers with the lowest origination FICO scores, but again, the differences relative to the highest FICO bin are generally statistically indistinguishable. These results imply that the proportional hazard assumption from the baseline specification without interactions is reasonable.

Importantly, the previous statements are all about *relative* effects—meaning how the rate reduction affects the default hazard within a certain group of loans. If we instead compare the effects on the *absolute* default hazard, the reductions are of course much larger for borrowers with a higher risk of default prior to the reset. This is illustrated in panel C of Figure 4, which plots the average observed (not estimated) default hazard for different subsamples of 5/1 loans pre-reset and post-reset. The bars show that loans with low FICO and high CLTV are at much higher risk of default, and that the absolute reduction in the default hazard (the difference in height of the before and after bars) is largest for these loans as well, even though the relative effects (the ratio of the heights of the bars) vary little across these categories.

In separate regressions reported in online Appendix C, we find similar estimated effects of rate reductions when restricting the sample to include only deeply underwater borrowers, or only borrowers with “prime characteristics.” As already mentioned above, we also repeat our analysis on a separate sample of prime (instead of Alt-A) privately securitized loans, and find similar effects as well.

The online Appendix additionally presents a variety of robustness tests of our baseline specification. For instance, we explicitly address the possibility that our results could be influenced by differential pre-reset trends across different loan types. To illustrate that this is unlikely to be an issue, panel D of Figure 4 plots the observed default hazard of 5/1s relative to 7/1+ loans, and shows a sharp drop in this relative hazard at the time of the reset, while in the surrounding months, the ratio is remarkably stable. This strongly suggests that it is indeed the rate reduction the 5/1s benefit from that causes the drop. Furthermore, we show that the estimated effects of rate reductions are not driven by the size of the reset being correlated with certain borrower characteristics, and that they are robust to only including two of the three loan types in the regressions or to not controlling for loan characteristics (other than loan type) at all.

B. 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 (3) in which we include 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. The resulting coefficients then allow us to calculate the predicted effect of an interest rate

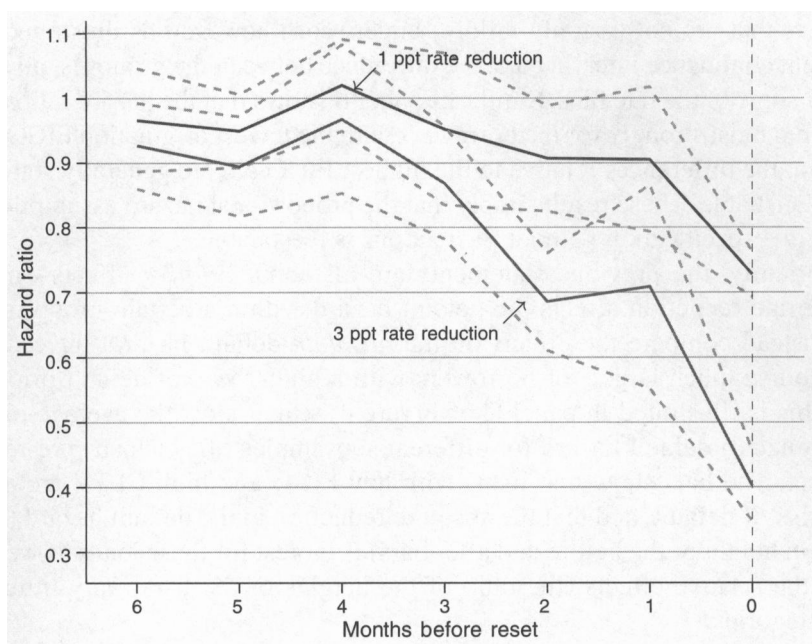


FIGURE 5. DO BORROWERS ANTICIPATE THE RESET?

Notes: Figure displays the cumulative predicted effect of interest rate changes from 0 to 6 months before the delinquency-status-relevant payment changes. Dashed lines represent 95 percent confidence intervals (based on standard errors clustered at state level).

reduction becoming delinquency-status-relevant in x months, for x ranging from 0 (this month) to 6.

For the interpretation of the estimated effects, it is important to remember our discussion from Section IID above. During the month that we call “two 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.

Figure 5 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 the displayed confidence intervals do not always exclude that there is no effect), 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.

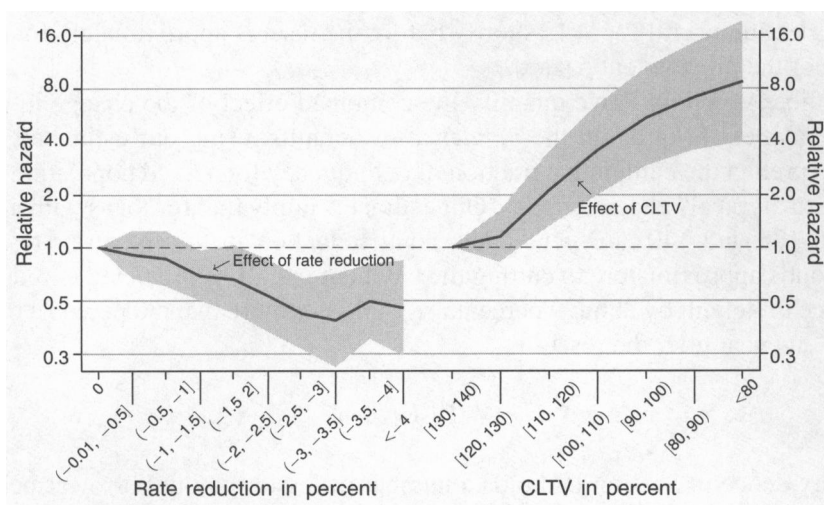


FIGURE 6. ESTIMATED EFFECTS OF RATE REDUCTION AND CLTV ON PREPAYMENT HAZARD

Notes: Figure 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 regressions of prepayment. Bands are 95 percent confidence intervals (based on standard errors clustered at state level). Regression controls for a large set of additional variables, as described in Section IIC. Coefficients and standard errors are shown in column (5) of Table A.1 in the online Appendix. Vertical axis has a log scale.

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.

C. Prepayment and the Incidence of Default

Figure 6 shows the main coefficients of interest from the estimation of the prepayment hazard. As we discussed in Section I, ignoring the prepayment hazard can generate incorrect inferences when looking at resets. Specifically, when ARMs reset upward, we often see a spike in prepayments as creditworthy borrowers select out of the sample, and as a consequence, the change in the default hazard confounds both selection and treatment effects.

Figure 6 shows that prepayments respond strongly to both CLTV and to interest rate reductions, 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 Figure 6, 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, 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 D 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 online Appendix F, 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. 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.

IV. Discussion

The evidence in Section III holds a number of lessons about borrower behavior. First, some 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 3, for example, one can interpret the line labeled “Effect of rate reduction” as showing that the default probability for a borrower with a specific level of negative equity can vary by a factor of three depending on the size of the borrower’s 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.

Second, the evidence in Figure 3 is completely consistent with standard frictionless models of default. Kau et al. (1992), for example, show that a borrower making a purely financial decision about whether to default in a 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, Figure 3 is exactly what the frictionless theory would predict.

However, while Figure 3 does not provide evidence against standard frictionless models of default, it does not necessarily provide evidence in their favor either. In particular, it is difficult to square the evidence on anticipation effects in Figure 5 with the frictionless model. Recall from Figure 5 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. According to the frictionless model, the difference in behavior results from 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 change in the value of the mortgage cannot possibly account for the large change in default propensity that we observe in the data.

To make sense of Figure 5, 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 behave according to the frictionless model but fail to realize that the rate is going to change. As mentioned in Section IID, 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. Before 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.

An alternative deviation from the frictionless model is that borrowers are not ignorant but rather 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 2015 or Schelkle 2012) would quantitatively match the effects of rate reductions that we find in the data.

V. Summary and Policy Implications

In this paper, we have exploited ARM downward rate resets during the low-interest-rate period 2008–2011 to measure the treatment effect of payment size on mortgage default. Studying payment reductions allows us to overcome the selection problems associated with payment increases due to borrowers' prepayment option. We find that payment reductions substantially lower mortgage defaults, even for borrowers that are deeply underwater.

Our results inform government policy toward the mortgage market along two main dimensions. First, during the crisis, policymakers debated ways to prevent mortgage defaults and foreclosures among at-risk borrowers. Implemented policies mostly focused on lowering payments to "affordable" levels through programs such as HAMP and HARP, rather than writing down principal. We show that payment reductions alone can dramatically lower the hazard of default without any change in the principal balance, and therefore are a powerful policy tool. For instance, the average HAMP modification reduced payments by 36 percent (US Department of the Treasury 2014, 24) and, roughly, our estimates imply that for a borrower who was current at the time of the modification, such a reduction would lower the default hazard by about 40 percent versus no modification at all.

An 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).⁹ 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.

Furthermore, one needs to keep in mind two things when extrapolating from our results. First, the interest reductions we study are not necessarily permanent, as the benchmark rates may increase again in the future.¹⁰ 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 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.

The second way in which our research informs policy relates to the costs and benefits of policies that encourage the use of fixed-rate mortgages. Policies in the United States have long favored FRMs and that bias has become stronger since the financial crisis. One way to interpret our results is that the payment stability of FRMs is desirable, because it avoids defaults due to payment increases that can occur with ARMs, thereby potentially justifying a policy preference for FRMs. However, ARMs have the advantage that borrowers (such as the ones in our sample) directly benefit from decreases in short-term interest rates due to monetary policy actions; this may not only lower their default propensity but also increase their consumption (Di Maggio, Kermani, and Ramcharan 2014; Keys et al. 2014). In principle, to the extent that monetary policy affects long-term rates, it would also pass through to FRM borrowers who can refinance their loan. However, the period since 2008 has illustrated that in case of a credit crunch with tight underwriting standards, many FRM borrowers are not able to benefit from the lower rates by refinancing. In that sense, FRMs make monetary policy transmission more fragile.

In weighing the benefits and costs of FRMs versus ARMs, one needs to consider the states of the world in which they materialize. Given the positive correlation between interest rates and economic conditions (and the health of the housing market), upward rate resets of ARMs tend to occur when most borrowers have sufficient equity in their home to be able to refinance or sell the home if the ARM becomes unaffordable. Conversely, downward ARM resets tend to occur in bad states of the world, such as the recent crisis, when their delinquency-reducing effects (which likely also spill over to the housing market and aggregate demand) are especially valuable.

⁹See Das (2012) and Eberly and Krishnamurthy (2014) for theoretical analyses. Das uses an option-based analysis to argue that principal reductions are preferable to rate reductions; Eberly and Krishnamurthy, in a model with borrowing constraints, obtain the opposite result.

¹⁰Such 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 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.

That said, it is of course possible that upward resets could occur during a particularly bad economic situation, such as a stagflation episode, where widespread reliance on ARMs would make things even worse and may also tie the hands of the central bank to some extent. Since at least the 1970s, there has not been a period where prices were falling in the overall US housing market while interest rates were rising, although episodes with falling prices and rising rates have occurred regionally.¹¹

Both with respect to default prevention and fixed-rate versus adjustable-rate mortgages, the overall cost-benefit calculation goes well beyond the scope of this paper. For example, the benefits of preventing foreclosures depend on much broader questions of externalities and financial stability. Similarly, the cost-benefit calculation of FRMs versus ARMs is complex, and should also include, among other considerations, the aggregate duration risk created by FRMs that needs to be borne by the financial system. All these questions are fertile ground for future research.

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¹¹To get a sense of the frequency of such episodes, we study two-year and five-year changes in state-level CoreLogic home price indices (starting in 1976:I) from some assumed origination quarter. There are 1,481 state-quarter pairs (out of 7,700 total) where nominal house prices fell over a two-year period. Out of those, in 245 cases (or 16.5 percent) did one-year Treasury rates increase over the same period (meaning the payments of fully-indexed hybrid ARMs with a two-year fixed rate period would increase at the reset). This happened most commonly for origination quarters 1986:II–1988:II (almost half of all cases). Over a five-year horizon, there are 1,201 state-quarter pairs (out of 7,089) where house prices fell (most of them in the 2000s). But only in 54 cases (or 4.5 percent) did one-year Treasury rates increase over the same time span.

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