ASYMMETRIC INFORMATION IN THE ADJUSTABLE-RATE MORTGAGE MARKET*

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Abstract

We explore the existence of information asymmetries in the US adjustable-rate mortgage market using detailed loan level data linked to high quality individual credit histories. To isolate selection and moral hazard effects, we distinguish between ex-ante known terms that borrowers accept when taking out a mortgage and the ex-ante unknown interest rates paid upon mortgage reset. We exploit administrative details of contract terms—including differences in the choice of financial index tied to loans and the lookback period which determines the precise interest rate chosen upon reset—to generate exogenous variation in reset rates. We find explicit evidence of moral hazard effects: a 1 percentage point increase increase in interest rates gives a 0.6–1.2 percentage point increase in the probability of foreclosure within a year. These effects persist for non-liquidity constrained individuals and are concentrated among underwater borrowers, suggesting that such foreclosures are due to strategic actions, rather than unavoidable illiquidity. More preliminary evidence on selection shows that individuals who accept less favorable contract terms default at higher rates, although the importance of different contract terms differs across mortgage products.

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1 Introduction

There is a well established and influential literature on the importance of information asymmetries in credit markets, following Stiglitz and Weiss (1981). However, empirical tests of these asymmetries are less common, and a major hindrance is the difficulty of separately identifying different informational issues—in particular, the distinction between moral hazard and adverse selection.

Being able to separately identify these effects is of particular importance in mortgage markets. Since the subprime mortgage crisis which began in late 2007, central issues in public debates regarding the appropriate response to rises in mortgage default have revolved around the relative importance of borrower characteristics and strategic defaults. One common line of argument suggests that the relaxation of lending standards driven in part by increased securitization created a lower quality pool of borrowers (e.g. Mian and Sufi (2009)). Others suggest that defaults were primarily generated by rising monthly payments for adjustable-rate borrowers and the inability of those borrowers to refinance to preferential rates (e.g. Bair (2007)). These different explanation have markedly different policy implications. If a risky borrower pool was responsible for rises in mortgage default, policies aimed at improving screening requirements for borrowers and aligning incentives along the chain of securitization are necessary to ensure that more creditworthy borrowers receive credit. Alternatively, if the wave of defaults was set off by more strategic choices on the part of borrowers, then loan modifications and balance reductions may be effective ways to keep borrowers in homes; and changes to the recourse nature of mortgages may be effective ways to mitigate the incentive to default.

Understanding the relative importance of these two explanations ultimately comes down to distinguishing between selection effects and moral hazard effects. The classic adverse selection problem in credit markets results when borrowers have private information on their risk levels. In practice, those who know that they are risky may be willing to accept higher interest rates in exchange for reduced levels of collateral. Alternatively, moral hazard exists when lenders cannot hold borrowers accountable for the full value of their loans. In mortgage markets, the common prevalence of non-recourse loans—under which lenders are unable to come after borrowers for the difference between loan and house value in the event of default—places strong limits on the ability of creditors to contract against default. Under these incomplete contracts, higher interest rates provide an increased incentive to default on the part of the borrower.

What is common to both adverse selection and moral hazard is a common empirical prediction that higher interest rates should be associated with higher default. If adverse selection is present, those who select into higher interest rates ex-ante will default more frequently. If moral hazard is present, those who face higher interest rates will default on loans more frequently. In either case, theory would predict a positive correlation between interest rates and default.

In this paper, we develop a novel instrument that allows for a clean test for the existence of moral hazard in adjustable-rate mortgage markets. We follow recent papers, notably Karlan and Zinman (2009), which have developed strategies to tease apart moral hazard from selection by contrasting the ex-ante terms under which borrowers select into contracts with the ex-post terms those borrowers face. By contrasting the rates borrowers agreed to when originating loans with the rates they faced when their mortgages adjusted, and utilizing only variation in the post-adjustment ("reset") rate due to contract details, we are able to isolate plausibly exogenous—and ex-ante unknown—changes in interest rates. As they are unpredictable at the time of origination, a correlation between these changes and the probability of default identifies a moral hazard effect untainted by the possibility of selection. We supplement this analysis with a test for adverse selection based on Finkelstein and Poterba (2014).

We identify two forms of contract details—the lookback date at reset, and the index choice. Among resetting adjustable-rate loans, the lookback date refers to the number of days prior to reset from which the market index is computed. This interest rate applies to the loan for the subsequent six (or twelve) months. Importantly, there is sizable variation in lookback dates, and in times of considerable variation in inter-day interest rates; this results in sizable differences in paid interest rates following reset. The existence of lookback periods is generally a remnant of pre-computer processing times for updating schedules. While the precise lookback date is not purely randomized across loans; there was little ex-ante reason to prefer one lookback period instead of another from the perspective either of borrowers or lender.

A second form of variation comes from the choice of index. Loans were typically indexed to either a LIBOR or Treasury rate. The precise choice of index was a relatively minor aspect of loan contracting prior to the financial crisis since the spreads between the two indices were relatively low. However, the spread between the two indices grew during to financial crisis to a maximum of 300 basis points, resulting in large differences in mortgage payments between similar mortgages originated and resetting on the same month that were indexed to different indices. For both forms of contract variation we use, we argue that it is unlikely that the precise choice of contract term resulted in a forecastable difference in interest rate; yet there was a large resulting ex-post difference in paid interest rates across resetting loans.

We focus on two samples—a relatively low-quality group of loans which reset after two or three years (2/28 + 3/27) for which we instrument for the reset rate using lookback variation among LIBOR loans, and a higher-quality group of borrowers which reset after five years (5/1s) for which we use variation resulting both from lookback variation as well as index choice. Among the 2/28 + 3/27 sample—we find that a 1 percentage point increase in interest rates raise the probability of missing at least one payment by 4 percentage points, and increases the probability of foreclosure by 2.6 percentage points. The effects are somewhat smaller for 5/1 loans—a 1.5 percentage point increase in missing one payment, or a 0.6 percentage point increase in the probability of foreclosure.

We further explore our basic results along several dimensions. First, we compare our results across levels of equity and liquidity. We find that our results are strongest among borrowers with high levels of negative equity, regardless of liquidity levels—consistent with a moral hazard interpretation of our results. We also perform a variety of robustness checks. Overall, our results suggest a sizable role for both moral hazard and adverse selection as important information frictions in the adjustable-rate mortgage market.

Our paper is related to the broader literature on testing for information asymmetries in credit markets. Chiappori and Salanie (2000) and Chiappori et al. (2006) outline robust tests for the presence of general information asymmetries, which have been commonly used in several financial markets (Dobbie and Skiba (2013)). The fundamental insight is that, controlling for all observables to the principal (the lender, or the insurance company in their analysis), and conditional on appropriate assumptions on the competitiveness of the market; asymmetric information necessitates a positive correlation between the interest rate offered and the probability of default. However, this insight does not in general allow for a separation of exact type of information asymmetry that is operating.

The paper closest to ours is Karlan and Zinman (2009), who are able to separate separate moral hazard from selection effects via an explicit randomization in a consumer credit market in South Africa. They used direct mailings to randomly assign potential borrowers different "offer" rates, then randomly assigned a portion of those who chose to take a loan into lower "contract rates." The authors argue that, conditional on the offer rate, any correlation between the contract rate and the default probability represents a moral hazard effect, while any correlation between the offer rate and the default probability represents a selection effect. Our paper differs in that we consider a natural experiment resulting from adjustable-rate contract features in a existing marketplace of residential mortgages in the United States.

Finally, our paper is also linked to the growing literature on exploiting reset variation among adjustable-rate mortgages. Tracy and Wright (2012), Fuster and Willen (2012), and Keys et al. (2014) all use differences in behavior among adjustable-rate loans which have experienced a reset and those which have not yet reset to understand reset behavior. Our paper differs from this literature by focusing on information asymmetries, and by identifying an instrument that allows us to examine differences *among* loans resetting in a given month.

2 Data and Background on Adjustable-Rate Mortgages

2.1 Background on Adjustable-Rate Mortgages

The typical adjustable-rate mortgage (ARM) in the United States is a hybrid ARM. For these loans, the interest rate is fixed for some initial period—often 2, 3, or 5 years—and resets on a pre-determined schedule for the remainder of the loan. The most common reset frequencies are 6 months or 1 year, although other durations exist. At origination, the borrower agrees upon an initial rate and a series of guidelines that determine all future resets. Under these guidelines, the rate of reset is determined based upon the cost of funds to the bank, proxied by some financial index (typically either Treasury or LIBOR), plus a fixed margin agreed upon at origination. Resets are often subject to other rules, for example per-period caps on the size of the rate change.

Though 30-year fixed rate contracts dominate the US mortgage market, adjustable-rate mortgages comprise a substantial share of mortgages, and were especially popular in the mid-2000s—especially among subprime borrowers. Given their predominance among borrowers where concerns of both selection and strategic behavior are largest, adjustable-rate mortgages constitute a particularly interesting environment in which to study informational frictions.

2.2 Data

Our primary dataset combines loan-level information provided by BlackBox Logic, as well as borrower-level information provided by Equifax, a major credit bureau.

The BlackBox dataset contains information on over 90% of private-label securitized mortgages (ie, those not securitized by a government-sponsored entity like Fannie Mac or Freddie Mae) starting around 2000. The data include records on over 20 million mortgages, which are typically either subprime, Alt-A, or jumbo-prime in credit risk. Information about loans includes static information taken at the time of origination, such as the credit score at time of loan application, loan balance, and the purpose of the loan. The static data also describe many features of the loan contract upon which we draw heavily. These include the length of the mortgage, the interest type (fixed or adjustable-rate), the schedule of timing of resets if it is an adjustable-rate mortgage, and other loan contract details. The data also include dynamic information updated monthly on fields like the interest rate, mortgage payments, and the mortgage balance. We also rely heavily on the mortgage payment field, which allows us to calculate the delinquency and refinancing status of the loan.

The Equifax data is a borrower-level data and contain information about the consolidated debt position of the household. This includes information about the presence of second liens, which we combine with zip-level estimates of house prices from Zillow to compute a combined, current loan-to-value estimate for the property. The data also include information about the current credit score and credit card utilization of the household. We use the credit utilization numbers in particular to compute the free credit available to individuals and attempt to isolate credit-constrained individuals.

The two datasets were linked through a proprietary merge algorithm maintained by Equifax.[1] To ensure accurate matches, we further required that the Equifax record list only one first mortgage, and that the zip code listed in BlackBox (corresponding to the property address) matched the zip code listed in Equifax (corresponding to the mailing address of the the borrowers for those with only one first mortgage), suggesting that the record was well matched.

After linking the two datasets, we made the following restrictions to isolate loans for our analysis. We restricted to adjustable-rate mortgages with a recognizable reset date (ie, two, three, or five years after origination) and present indicator for the index type the loan is scheduled to reset to (LIBOR or treasury). We identified two samples: a 5/1 sample consisting of loans which reset yearly after five years to either a treasury or LIBOR rate, and a 2/28 + 3/27 sample consisting of loans which reset every six months after two or three years to a LIBOR rate.[2] For both samples,

[1] See Mayer et al. (2014) and Piskorski, Seru and Witkin (2013) for more details about this merge algorithm.

[2] For the 2/28 + 3/27 sample, we additionally restrict to loans for which the interest rate cap was not binding— those loans for which the scheduled interest rate hike was less than 300 basis points. Loans facing a greater interest rate hike than this would typically have an interest rate hike which is capped at that amount, eliminating any variation in the amount paid resulting from our instruments. This cap was rarely binding for the 5/1 sample.

we identified loans that reset appropriately as those loans which experienced their first interest rate change in a four month window around the scheduled reset date, and had no other interest rate resets within that window. We used the actual date of interest rate hike as reflecting the reset date (this coincided with the scheduled date change in the majority of cases). We further required that loans be current three months prior to this reset date. We focus on the first reset, and keep loan outcomes for the subsequent two years (for the 5/1 sample) or one year (for the 2/28 + 3/27 sample).

Our final sample consists of 119,846 loans in the 5/1 sample and 248,050 loans in the 2/28 + 3/27 sample. Table I shows summary statistics our two samples. Panel A focuses on covariates, which are all static and taken at the time of origination. Panel B shows outcome variables for the 12 months following reset.

3 Empirical Strategy and Background on Adjustable-Rate Mortgages

To identify and test for the presence of moral hazard, we would ideally like to compare borrowers who selected into identical loans, but who faced different terms—in particular different interest rates—ex-post. Adjustable-rate mortgages provide an attractive setting for such an analysis: most loans are adjusted annually or semi-annually, generating substantial variation in interest rates both within and across borrowers. However, much of this variation is driven by factors that are hard to justify as exogenous. While loans that adjust in different periods typically reset to different rates, the factors that influence the adjustment date—the date of origination or timing of resets—are likely to be related to the probability of defaulting. Comparing borrowers who chose different loan structures creates similar problems. While it is not plausible to impose random assignment of interest rates in mortgage markets, most adjustable-rate products are quite detailed, and many of these details may be unknown or unimportant to the borrower upon loan origination. Our identification relies on two contract details that we argue satisfy the following criteria:

(i) Irrelevant to borrowers and lenders in terms of predicting the future path of the interest rate.

(ii) Capable of generating significant differences in the interest rate as realized.

3.1 Index Choice

The first contract detail that we exploit is the financial index that determines the reset rate. There are many potential choices of index, but treasury and and LIBOR rates are dominant, and our analysis focuses on contrasting two of these: the one year LIBOR rate and the one year constant maturity Treasury.

Prior to 2007, Libor and Treasury rates moved quite closely, with a spread of under 50 basis points. While loans tied to Libor typically had slightly lower margins to account for the spread, the difference in risk between the two was perceived to be low and both were viewed as valid proxies of the risk free rate. [3] Borrowers may have had some choice of index type; it was typically more often a function of lender intentions on the secondary market. Many investors in mortgage-backed securities (for instance, European banks) had a cost of funds denominated in LIBOR, and were interested in purchasing assets with a payment structure also determined by LIBOR. In our sample, the loan servicer explains more than 50% of the variance in index; however many originators provided loans in both categories.

In the pre-crisis period, the precise choice of interest might generate modest differences in interest rates for borrowers in the same product class resetting in the same month, but there was little ex-ante reason to prefer one over another. However, as seen in figure I, the two rates diverged sharply during the crisis, reaching a maximum spread of over 3 percentage points, and averaging over 1 percentage point between 2007 and 2009.

3.2 Lookback Period

The second contract detail is what we refer to as the lookback period. At each adjustment, it is necessary for the lender compute a new interest rate using the relevant index value. The existence of lookback periods is generally a remnant of pre-computer processing times for updating schedules. To allow for processing and notification of the borrower, lenders typically do not take the contemporaneous value of the index on the day of reset, but instead use the index value at a set period in the past. However there is no set standard for this period: 15, 25, and 45 days are all common durations. While there is some connection between originator identity and the precise choice of lookback period, there is again no strong ex-ante reason to prefer one lookback period rather than another and in general the choice of lookback period does not forecast future interest rates.

In periods with relatively low fluctuations in interest rates, the choice of lookback period makes little difference. However, when index rates are volatile, differences in lookback period can generate significant differences in the reset rate actually paid ex post. Figure II shows the implications of different lookback periods at a time of significant fluctuation: A loan resetting September 1st 2007 could experience a greater than 80 basis point difference in rate depending on the lookback.

3.3 Empirical Approach

Our goal is to test for the existence of moral hazard as a result of interest rate changes. We face two primary challenges in doing so. The first is the difficulty of separating moral hazard effects from selection effects. We address this by controlling for all ex-ante known contract terms, including the original rate (r^o), and the margin (m). The second is the need for exogenous variation in the interest rate faced by the borrower (the reset rate r^r). To accomplish this, we explicitly limit variation in the reset rate to that created by the contract terms discussed above. We do so by constructing

[3] Hull (2010): "There is a small chance that an AA-rated financial institution will default on a LIBOR loan. However, they are close to risk-free. Derivatives traders regard LIBOR rates as a better indication of the “true” risk-free rate than Treasury rates, because a number of tax and regulatory issues cause Treasury rates to be artificially low."

an instrument z_{it}, equal—at each reset– to the value of borrower i's chosen index at the relevant lookback period prior to the reset. Conditional on all known contract terms, a positive relationship between variation in the reset rate caused by z_{it} and the probability of default or delinquency is evidence of moral hazard.

Our primary analysis considers several delinquency outcomes in loans in the period between their first and second resets. We analyze binary outcomes, equal to one if the delinquency event occurs at any point in that period. Our delinquency events are: being 30, 60, or 90 days past due on your mortgage, as well as the mortgage entering foreclosure.[4] We also analyze the outcome of prepayment, which occurs when a borrower refinances or sells the property and repays the entire outstanding amount of the mortgage. We cluster 30 year mortgages into two broad product classes and conduct our analyses separately within each class. We refer to the first class as 5/1s. These are loans with a fixed rate for a 5 year initial period, that update yearly thereafter. We refer to the second class as 2/28s and 3/27s, which are loans with a 2 or 3 year initial period, that update every 6 months thereafter.

To conduct our test for the existence of moral hazard, we consider IV models with a second stage of the following form:

$$Y_{it} = \beta_0 + \beta_1 r_{it}^r + \beta_2 r_{it}^o + \beta_3 m_{it} + x_{it}'\gamma + \alpha_t + \varepsilon_{it}$$

Where Y_{it} is a delinquency event for individual i originating his mortgage in month t, r_{it}^r i the observed reset rate at the time of the first rate reset, r_{it}^o is the interest rate at origination, and m_{it} is the margin. x_{it}' is a vector of other covariates, which include geographic fixed effects—metropolitan statistical area (MSA) and region $\times$ quarter of year—and the following loan level characteristics: original and time-of-reset balance, loan to value, loan to value squared, credit category, borrower type, reason for loan, whether the contract is interest only, no-documentation, or has a prepayment penalty, and FICO score at origination. α_t denotes origination month fixed effects.[5]

The first stage of this regression of our estimation simply regresses the reset rate on our instrument z_{it}:

$$r_{it}^o = \gamma_0 + \gamma_1 z_{it} + u_{it}.$$

Our identifying assumption is $E[\varepsilon_{it}|z_{it}] = 0$. Under this assumption, our test for the existence of moral hazard comes down to the simple test of:

$$H_0 : \beta_1 = 0$$

against:

$$H_1 : \beta_1 > 0.$$

[4]We include bankruptcies in the foreclosure measure.

[5]Note that for borrowers within a fixed loan duration, say 5/1, the origination month and 1st reset month are perfectly collinear, so there is no need to control for both. In any regression in which loan products of different durations are used, we include fixed effects for both the origination and reset month.

We would also like to make the claim that findings of $\beta_2 > 0$ or $\beta_3 > 0$ is evidence of an ex-ante adverse selection effect. However, there are several issues that prevent us from making such a claim. Firstly, there is no way to separate a pure selection effect from moral hazard effects that occur within the period prior to the first reset. Secondly, and perhaps more importantly, when borrowers select into loans with contract terms, it may be the case that they prioritize one over the other: for example, a borrower who has private information that they will default with high probability early in the loan may choose a lower initial rate, and a higher margin as compared to a borrower who believes himself to have a low probability of default. This would create a situation in which $\beta_3 > 0$ but $\beta_2 < 0$. Given this, any claims we make regarding adverse selection from these regressions must be interpreted as suggestive.

4 Results

4.1 Univariate Graphical Comparisons

We first consider a simplified graphical "proof of concept" that only utilizes the distinction between loans indexed to LIBOR and Treasury rates, but does not yet incorporate the full IV approach. Figure III plots two lines. The black line shows the spread in interest rates between loans linked to Libor indices and loans linked to treasury indices in our sample. Note that loans reset monthly, and may be subject to caps and other restrictions, so the fluctuations in this spread are dampened in relation to those shown in Figure I. The red line shows the spread in the percentage of defaults between those linked to Libor indices and those linked to treasury indices. Consistent with the existence of moral hazard, we see a spike in this default spread coinciding with the spike in interest rates.

4.2 Primary Specifications

We next turn to estimating OLS versions of the second stage outlined in section 4. Tables II and III show these results for 5/1 loans, and the combination of 2/28 and 3/27 loans, respectively. Our primary coefficient of interest is that on the reset rate. Note first that, across all outcomes, this coefficient is positive and statistically significant, suggesting the presence of moral hazard. Further, the effect sizes are much larger for those in the 2/28 and 3/27 sample, despite the fact that the period covered for this sample is 6 months, as opposed to 1 year for 5/1s. For 2/28s and 3/27s, OLS results suggest that a 1 percentage point increase in interest rates increases the probability of the least extreme form of delinquency we explore—being more than 30 days past due—by 4 percentage points, or just over 10% of the mean. For the most extreme form of delinquency we observe, the property entering foreclosure, a 1 percentage point increase in interest rates gives a 2.6 percentage point increase in the likelihood, which is an increase of just under 25% of the mean. The effect sizes for 5/1 loans, though more modest, are still of economic interest. A one percentage point increase in the reset rate gives a 1.5 percentage point increase in the probability of being 30 days past due

($\approx$ 14% of the mean), and a 0.6 percentage point increase in the probability of entering foreclosure ($\approx$ 21% of the mean).

The coefficients of the initial contract terms displayed in our table—the original rate and margin—are less consistent across the two product categories. For 5/1 loans, we see positive and significant coefficients on the original interest rate, suggesting that those accepting higher interest rates are more likely to default. Coefficients on the margin are close to zero and only marginally significant in one instance. Conversely, for 2/28 and 3/27 loans, coefficients on the original interest rate are negative and significant, while coefficients on the margin are positive and significant for the outcomes of being 30, 60 or 90 days past due. The impact of a one percentage point higher margin on the probability of foreclosure appears to be negative, but is only marginally significant.

We also include the outcome of prepayment, which in our sample is mostly a choice to refinance. For both product categories, we see that an increase in the interest rate significantly increases the probability of prepayment. The impact is about 4 times larger for 5/1 loans, for which a one percentage point increase in the reset rate gives a 11 percentage point increase in the probability of repayment, despite the fact that the mean prepayment level is lower for 5/1 loans. This likely reflects fundamental differences in the borrower types that choose each product category, and potentially a greater difficulty amongst 2/28 and 3/27 borrowers in securing credit for refinancing.

We next turn to our IV specifications. For both loan categories, our instrument z_{it} is a strong predictor of the reset rate.[6] Tables IV and V display the results of the second stage of our IV regressions for both samples. In terms of sign and significance, our results are exactly consistent with those seen in tables II and III. Additionally, for all outcomes, the magnitude of the impact of the reset rate is higher in our IV specifications than in our OLS specifications: a one unit increase in the reset rate giving a 0.6 percentage point higher increase in the probability of both being 30 days past due and foreclosure than our OLS specification. Overall, across all specifications we see a positive and both economically and statistically significant coefficient on the reset rate, indicating the presence of moral hazard. While the effect is stronger for less serious types of delinquency, for example falling 30 days behind on a mortgage, that do not necessarily indicate that the borrower will ultimately default on their loan, it persists through the outcome of foreclosure. The results on selection effects are more suggestive. For 5/1 loans, we see a pattern that is consistent with what is often taken to be indicative of adverse selection: a positive correlation between the contracted interest rate and the probability of delinquency. However, for 2/28 and 3/27 loans, we see the opposite (although there appears to be a positive correlation between the margin and the probability of default).

4.3 Moral Hazard or Liquidity Constraints

There is an alternative explanation for what we are terming a moral hazard effect. If borrowers are liquidity constrained, an increase in interest rates could lead to a default through a mechan-

[6]In the 5/1 loan category, the regression $r^o_{it} = \gamma_0 + \gamma_1 z_{it} + u_{it}$ gives $\gamma_1 = 0.91$ with an R^2 of 0.90. In the 2/28 and 3/27 loan category, $\gamma_1 = 0.53$ with an R^2 of 0.18.

ical channel: borrowers want to continue paying their mortgage, but are simply unable to afford payments at higher rates. To test for this possibility, we cut our results across the distributions of liquidity and equity. If we see effects amongst individuals who we can observe as having access to liquidity, than it is easier to interpret defaults as a strategic choice, or what we term moral hazard, since such individuals could always tap into other sources of credit to finance continued mortgage payments.[7] However, observing defaults amongst illiquid individuals, and particularly amongst illiquid individuals with high equity, is suggestive of a mechanical channel.

Our measure of liquidity for a borrower is based upon linked credit histories. We define liquidity based upon available credit from sources of revolving credit, which generally reduces to credit available from home equity lines of credit (HELOCs) and credit cards. We normalize the total amount of credit by the monthly mortgage payment, our measure is therefore equivalent to the total number of monthly mortgage payments an individual could make using only official credit sources. We take this as a lower bound on the liquidity of an individual, as it ignores all assets and informal credit sources. We define equity based upon the combined loan to value (CLTV) of the borrower, which includes junior mortgage liens on the property. In this analysis, we are simply interested in whether the borrower has positive or negative equity in the home, that is, whether the CLTV is greater than 100%.

We provide a basic look into the impacts of liquidity and equity by comparing borrowers in four categories: those above and below the median level of liquidity, with CLTVs above and below 100%. Tables VI - IX present results from these categories for borrowers in the 2/28 and 3/27 categories. Note first that the impact of the reset rate on delinquency and default is concentrated in the pool of borrowers with negative equity. This is consistent with the existence of moral hazard, although not definitive, as borrowers with equity typically have an option to refinance even when they have no liquidity from other sources. The importance of equity for refinancing is reinforced by these tables, as the entirety of the impact of changes in reset rates on refinancing is concentrated amongst borrowers with positive equity. Perhaps more compelling is the similarity in effect sizes amongst those with negative equity, regardless of the level of liquidity. For the outcomes of being 30, 60 and 90 days past due, both high and low liquidity borrowers with negative equity show positive, significant, and substantial impacts of an increase in the reset rate on the probability of defaulting. The impact of an increase in interest rates on the probability of actually entering foreclosure does appear to be lower amongst those with high liquidity.

4.4 Robustness

Jumbo Loans

We first focus on borrowers with jumbo loans, that is, loans that exceed the conforming loan limits for securitization by the GSEs. Table X presents results for 5/1 jumbo loans. For all delinquency outcomes, the magnitude of the impact of changes in their reset rate is smaller than for our full

[7] There may be costs to tapping other forms of credit, as suggested in Cohen-Cole and Morse (2009). However, our primary interest lies identifying only whether individuals have the capacity to continue making mortgage payments.

sample, although they remain positive and statistically significant. The impact on the probability of foreclosure is especially notable, as the impact of a one percentage point increase in the reset rate gives a 0.6 percentage point increase in the probability of default, as opposed to a 1.2 percentage point increase for the full sample. However, 0.6 percentage point increase is still notable, as it represents 32% of the mean.

Small Banks

We next limit our analysis to loans that were not originated by the top 5 national banks, shown in table XI. Results here are quite similar to those found in our main tables, although the impact of interest rate changes on the prepayment probability is slightly lower.

Investors

Our third robustness check focuses on borrowers who explicitly identify themselves as investors in loan documentation. We expect investors to be more sophisticated, more strategic, and to have a lower value of the housing stock itself as compared to the general population. As a result, we expect them to be particularly sensitive to changes in the interest rate. Table XII, provides modest evidence for this hypothesis, as investors demonstrate marginally greater responsiveness to changes in the reset rate for minor delinquency behavior.

Common Servicers

Table XIII presents results limiting only to servicers that tie loans to both the LIBOR and treasury indexes. Note that we significant effects amongst this category, suggesting that differences are not due to differences across servicer.

5 Selection: An alternative approach

As discussed in our methodology, our approach does not cleanly identify an adverse selection effect. While we would like to argue that a positive correlation between the original "teaser" rate and the probability of default is evidence of selection, it is also possible that any correlation simply reflects a moral hazard effect resulting from higher teaser rates.

To provide further evidence on the existence of selection effects, we follow Finkelstein and Poterba (2014). We exploit an observable that was unused in setting interest rates to test for selection. In our context, one potential variable is an indicator of being an "unreported investor" as defined in Piskorski, Seru and Witkin (2013). An unreported investor is an individual who lists his or her mortgaged property as their primary place of residence, but who never actually resides in the home. These individuals are identified by contrasting the mortgage documentation with the zipcode of residence (available in our dynamic data). Piskorski, Seru and Witkin (2013) document both that this phenomena is substantial (on the order of 10% of loans drawing on the same BlackBox data source), and that unreported investors represent a different risk category from homeowners.

Finkelstein and Poterba (2014) suggest the following test: If C denotes the terms of the loan (the original interest rate), L denotes the default probability, W denotes a candidate unused observable variable, and X is a vector of observables used in setting the loan terms, then we estimate

$$C_i = X_i\beta + W_i\alpha + \epsilon_i \tag{1}$$

$$L_i = X_i\gamma + W_i\delta + \mu_i \tag{2}$$

and test $\{\alpha = 0, \delta = 0\}$. Table 1 shows results of regressions of this form, which support the existence of a selection effect. Finkelstein and Poterba (2014) argue that, formally, this test is a test of asymmetric information, and may still conflate moral hazard and selection effects. However, if one can completely control for the terms of the contract, it seems a remaining correlation between the unused observable and the default probability identifies a selection effect distinct from a moral hazard effect. Table XIV shows results from this analysis. The first two columns represent equation 1 above, with the contract terms being the original rate and the margin. Note that there is a strong positive correlation between the initial interest rate and being an unreported investor. Furthermore, being an unreported investor has a positive and statistically significant relationship with all default outcomes (and no significant relationship with prepayment). Our results suggest that unreported investors have a 1.5 percentage point higher probability of defaulting as compared to non-investors. In line with the approach ofFinkelstein and Poterba (2014) approach, the finding that being an unreported investor is correlated with both accepting a higher original rate and being more likely to become delinquent provides evidence that adverse selection exists in this market.

6 Conclusion

We develop a novel instrument based on the spread between LIBOR-Treasury rates during the Financial Crisis, as well as differences in interest rates deriving from variation in lookback days among resetting adjustable-rate loans. We argue that these idiosyncratic contract terms did not lead to forecastable differences in payment sizes prior to reset, but led to large ex-post differences in realized interest rates among mortgages resetting in the same month.

We use this variation in interest rate shocks to disentangle different types of information asymmetries operating in the adjustable-rate mortgage market. While prior work, such as Karlan and Zinman (2009) have adopted similar strategies using differences between prior and subsequent rates to differentiate between different types of information asymmetries, our work is the first attempt to isolate these informational problems in an existing financial market: the US residential mortgage market.

Understanding the relative importance of informational frictions is of critical importance in this market due to the persistent impacts of the subprime mortgage crisis and associated rise in consumer defaults. Both in understanding the historical nature of the crisis, as well as in maintaining the flow of consumer credit in the future, there remains a debate regarding the nature of consumer defaults. Policies aimed at enhancing screening policies or further aligning bank incentives in securitization are ultimately driven by a concern that adverse selection is a pervasive feature of credit markets. On the other hand, policies aimed at changing default incentives are motivated by a view of the existence of moral hazard in credit markets.

Our work contributes to this debate by using our novel instrument to separate interest rates paid at origination from those paid upon reset and identify separate moral hazard and adverse selection effects. Among relatively low-quality 2/28s and 3/27s; we find that a 1 percentage point increase in interest rates raises the probability of missing at least one payment by 4 percentage points, or the probability of foreclosure by 2.6 percentage points. We find smaller, but still substantial effects when focusing on relatively higher-quality 5/1s. Importantly, we find that these effects are largely concentrated among individuals with negative equity—those with positive equity are better able to escape the reset shock through refinancing. We also find suggestive evidence of adverse selection using both the impact of the original interest rate paid, as well as supplemental approach focusing on risk characteristics unavailable to banks—though we find that the nature of adverse selection depends on the product category.

These results suggest that both forms of information asymmetries are important in mortgage markets. Our effects are economically quite large and suggest that households are quite sensitive to payment shocks, especially when the choice of default is financially advantageous.

Figure I
Libor - Treasury Spread

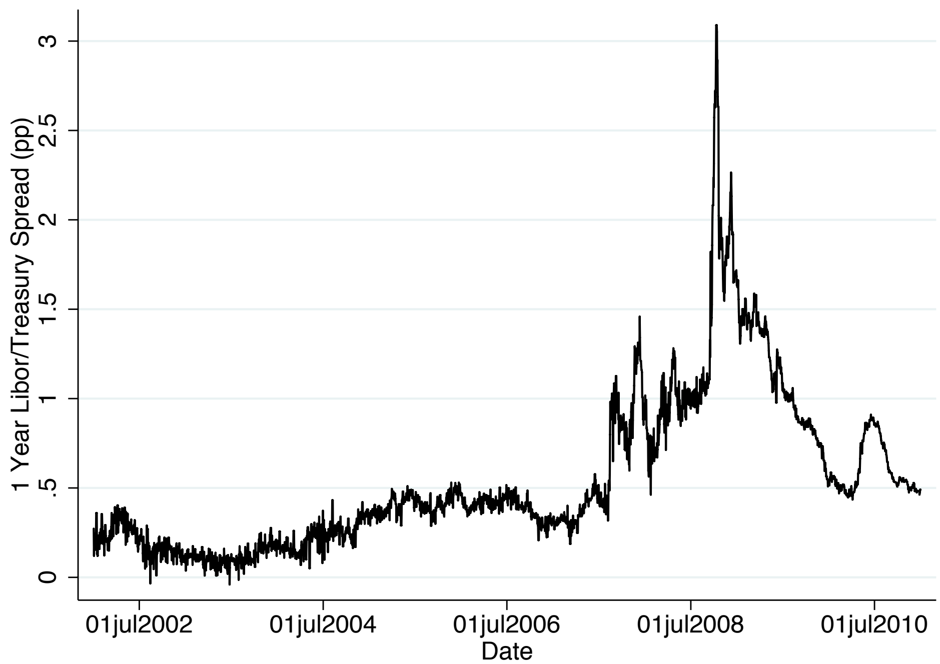

Figure II
Illustration of Lookback Period

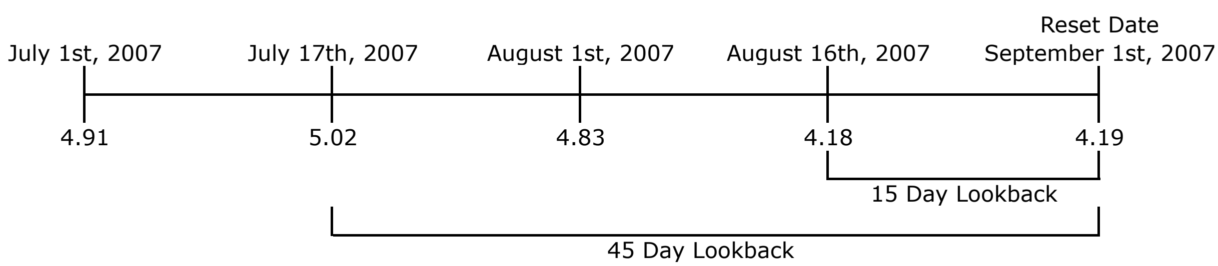

Figure III
Proof of Concept

Table I

Panel A: Summary Statistics, Covariates

	5/1 Sample		2/28 - 3/27 Sample	
	Mean	N	Mean	N
Margin	2.46	119846	5.13	248050
Fico Credit Score	730.7	83823	635.2	221991
Original Balance (100k)	4.58	119846	2.05	248050
Original Loan to Value	71.4	117831	80.8	243212
Interest Only Loan	0.71	109365	0.29	220910
45 Day Lookback	0.93	119846	0.28	248050
Loan for Primary Home	0.86	119846	0.84	248050
Loan Used for Purchase	0.53	119846	0.51	248050
No Documentation Loan	0.55	119846	0.53	248050
Serviced By Top 5 Bank	0.46	119846	0.24	248050

Panel B: Summary Statistics, Outcome Variables

	5/1 Sample		2/28 - 3/27 Sample	
	Mean	N	Mean	N
Indexed to Libor	0.606	119846	1.000	248050
30 DPD Within 12m of Reset	0.111	119846	0.376	248050
60 DPD Within 12m of Reset	0.064	119846	0.243	248050
90 DPD Within 12m of Reset	0.051	119846	0.181	248050
Foreclosure Within 12m of Reset	0.028	119846	0.110	248050
Prepayment Within 12m of Reset	0.149	119846	0.215	248050

Table II

OLS - 5/1 Loans at First Reset

	30 DPD	60 DPD	90 DPD	Foreclosure	Prepayment
Reset Rate	0.015***	0.015***	0.012***	0.006**	0.112***
	(0.005)	(0.004)	(0.004)	(0.003)	(0.007)
Original Interest Rate	0.029***	0.015***	0.013***	0.008***	-0.056***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)
Margin	0.016*	0.002	-0.003	0.004	-0.065***
	(0.008)	(0.006)	(0.005)	(0.004)	(0.009)
Month FEs	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes
Western US x Quarter FEs	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Avg. of Dep. Var.	0.111	0.0636	0.0506	0.0284	0.149
R^2	0.068	0.060	0.054	0.037	0.261
N	119846	119846	119846	119846	119846

** denotes 5% significance, *** denotes 1% significance. Std. errors are clustered at the msa level.

Table III
OLS - 2/28 and 3/27 Loans at First Reset

	30 DPD	60 DPD	90 DPD	Foreclosure	Prepayment
Reset Rate	0.040***	0.047***	0.043***	0.026***	0.028***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.001)
Original Interest Rate	-0.033***	-0.042***	-0.042***	-0.021***	-0.024***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Margin	0.017***	0.009***	0.007***	-0.003*	-0.007***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Reset Month FEs	Yes	Yes	Yes	Yes	Yes
Orig. Month FEs	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes
Western US x Quarter FEs	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Avg. of Dep. Var.	0.376	0.243	0.181	0.110	0.215
R^2	0.164	0.162	0.152	0.094	0.320
N	248050	248050	248050	248050	248050

** denotes 5% significance, *** denotes 1% significance. Std. errors are clustered at the msa level.

Table IV
IV - 5/1 Loans at First Reset

	30 DPD	60 DPD	90 DPD	Foreclosure	Prepayment
Reset Rate	0.021***	0.023***	0.019***	0.012***	0.120***
	(0.007)	(0.006)	(0.005)	(0.003)	(0.010)
Original Interest Rate	0.029***	0.015***	0.013***	0.008***	-0.056***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)
Margin	0.009	-0.005	-0.009	-0.001	-0.071***
	(0.009)	(0.007)	(0.006)	(0.005)	(0.011)
Month FEs	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes
Western US x Quarter FEs	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Avg. of Dep. Var.	0.111	0.0636	0.0506	0.0284	0.149
R^2	0.067	0.059	0.054	0.037	0.260
N	119831	119831	119831	119831	119831

** denotes 5% significance, *** denotes 1% significance. Std. errors are clustered at the msa level.

Table V
IV - 2/28 and 3/27 Loans at First Reset

	30 DPD	60 DPD	90 DPD	Foreclosure	Prepayment
Reset Rate	0.020*	0.036***	0.031***	0.036***	0.055***
	(0.010)	(0.009)	(0.008)	(0.007)	(0.008)
Original Interest Rate	-0.032***	-0.042***	-0.041***	-0.022***	-0.026***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)
Margin	0.036***	0.018**	0.017**	-0.011*	-0.032***
	(0.010)	(0.009)	(0.008)	(0.006)	(0.006)
Reset Month FEs	Yes	Yes	Yes	Yes	Yes
Orig. Month FEs	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes
Western US x Quarter FEs	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Avg. of Dep. Var.	0.376	0.243	0.181	0.110	0.215
R^2	0.163	0.162	0.152	0.094	0.318
N	248050	248050	248050	248050	248050

** denotes 5% significance, *** denotes 1% significance. Std. errors are clustered at the msa level.

Table VI
IV Regressions – 2/28 and 3/27 – Negative Equity – Low Liquidity

	30 DPD	60 DPD	90 DPD	Foreclosure	Prepayment
Reset Rate	0.066**	0.090***	0.065***	0.078***	0.013
	(0.027)	(0.028)	(0.020)	(0.020)	(0.012)
Original Interest Rate	-0.066***	-0.085***	-0.072***	-0.030***	-0.004*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.002)
Margin	0.009	-0.003	0.011	-0.049**	0.002
	(0.025)	(0.026)	(0.019)	(0.019)	(0.011)
Month FEs	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes
Western US x Quarter FEs	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Avg. of Dep. Var.	0.600	0.427	0.332	0.186	0.0622
R^2	0.107	0.144	0.143	0.084	0.093
N	20288	20288	20288	20288	20288

Table VII
IV Regressions – 2/28 and 3/27 – Negative Equity – High Liquidity

	30 DPD	60 DPD	90 DPD	Foreclosure	Prepayment
Reset Rate	0.067**	0.059**	0.069**	0.027	0.016
	(0.028)	(0.027)	(0.027)	(0.026)	(0.015)
Original Interest Rate	-0.078***	-0.080***	-0.073***	-0.039***	-0.013***
	(0.007)	(0.007)	(0.006)	(0.004)	(0.004)
Margin	0.026	0.029	0.013	0.013	0.007
	(0.025)	(0.023)	(0.023)	(0.023)	(0.013)
Month FEs	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes
Western US x Quarter FEs	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Avg. of Dep. Var.	0.419	0.305	0.248	0.149	0.0914
R^2	0.170	0.193	0.186	0.121	0.141
N	21529	21529	21529	21529	21529

Table VIII
IV Regressions – 2/28 and 3/27 – Positive Equity – Low Liquidity

	30 DPD	60 DPD	90 DPD	Foreclosure	Prepayment
Reset Rate	-0.042	0.019	0.026	0.031	0.150***
	(0.040)	(0.039)	(0.030)	(0.020)	(0.026)
Original Interest Rate	-0.016**	-0.031***	-0.027***	-0.009**	-0.035***
	(0.006)	(0.006)	(0.005)	(0.004)	(0.005)
Margin	0.066*	0.015	0.004	-0.019	-0.099***
	(0.035)	(0.034)	(0.026)	(0.018)	(0.023)
Month FEs	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes
Western US x Quarter FEs	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Avg. of Dep. Var.	0.418	0.226	0.144	0.0737	0.251
R^2	0.085	0.076	0.068	0.034	0.208
N	22012	22012	22012	22012	22012

Table IX
IV Regressions – 2/28 and 3/27 – Positive Equity – High Liquidity

	30 DPD	60 DPD	90 DPD	Foreclosure	Prepayment
Reset Rate	-0.047	-0.052	-0.043	0.018	0.151***
	(0.042)	(0.034)	(0.030)	(0.017)	(0.035)
Original Interest Rate	-0.016***	-0.015***	-0.012***	-0.007***	-0.049***
	(0.004)	(0.004)	(0.004)	(0.002)	(0.006)
Margin	0.071*	0.065**	0.053**	-0.010	-0.091***
	(0.037)	(0.030)	(0.027)	(0.016)	(0.030)
Month FEs	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes
Western US x Quarter FEs	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Avg. of Dep. Var.	0.177	0.0871	0.0578	0.0320	0.401
R^2	0.084	0.068	0.059	0.038	0.305
N	20771	20771	20771	20771	20771

Table X
IV Regressions – 5/1 – Jumbo Loans Only

	30 DPD	60 DPD	90 DPD	Foreclosure	Prepayment
Reset Rate	0.020***	0.013**	0.010*	0.006*	0.160***
	(0.007)	(0.006)	(0.005)	(0.003)	(0.012)
Original Interest Rate	0.030***	0.012***	0.009***	0.006***	-0.067***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.005)
Margin	0.021	-0.000	-0.004	-0.000	-0.085***
	(0.021)	(0.013)	(0.012)	(0.009)	(0.016)
Month FEs	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes
Western US x Quarter FEs	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Avg. of Dep. Var.	0.0874	0.0474	0.0371	0.0186	0.180
R^2	0.053	0.049	0.043	0.027	0.243
N	59258	59258	59258	59258	59258

** denotes 5% significance, *** denotes 1% significance. Std. errors are clustered at the msa level.

Table XI
IV Regressions – 5/1 – Small Banks

	30 DPD	60 DPD	90 DPD	Foreclosure	Prepayment
Reset Rate	0.020*	0.028***	0.019**	0.012**	0.089***
	(0.011)	(0.010)	(0.008)	(0.006)	(0.011)
Original Interest Rate	0.033***	0.018***	0.016***	0.009***	-0.060***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)
Margin	0.010	-0.011	-0.010	-0.000	-0.044***
	(0.012)	(0.012)	(0.010)	(0.007)	(0.011)
Month FEs	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes
Western US x Quarter FEs	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
Avg. of Dep. Var.	0.132	0.0778	0.0624	0.0352	0.140
R^2	0.078	0.067	0.062	0.041	0.310
N	65011	65011	65011	65011	65011

** denotes 5% significance, *** denotes 1% significance. Std. errors are clustered at the msa level.

Table XII
IV Regressions – 5/1 – Investors Only

	30 DPD	60 DPD	90 DPD	Foreclosure	Prepayment
Reset Rate	0.037***	0.026***	0.022***	0.011***	0.103***
	(0.004)	(0.004)	(0.003)	(0.002)	(0.008)
Original Interest Rate	0.055***	0.033***	0.026***	0.016***	-0.083***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.005)
Margin	-0.001	-0.002	-0.005	0.002	-0.050***
	(0.005)	(0.004)	(0.003)	(0.002)	(0.006)
Month FEs	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes
Western US x Quarter FEs	Yes	Yes	Yes	Yes	Yes
Avg. of Dep. Var.	0.111	0.0636	0.0506	0.0284	0.149
R^2	0.036	0.033	0.031	0.023	0.177
N	119846	119846	119846	119846	119846

** denotes 5% significance, *** denotes 1% significance. Std. errors are clustered at the msa level.

Table XIII
IV Regressions – 5/1 – Common Servicer

	30 DPD	60 DPD	90 DPD	Foreclosure	Prepayment
Reset Rate	0.022***	0.019***	0.015***	0.008**	0.067***
	(0.006)	(0.005)	(0.004)	(0.003)	(0.010)
Original Interest Rate	0.045***	0.025***	0.019***	0.013***	-0.067***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)
Margin	0.015**	0.016***	0.010**	0.004	-0.055***
	(0.007)	(0.005)	(0.005)	(0.003)	(0.009)
Month FEs	Yes	Yes	Yes	Yes	Yes
MSA FEs	Yes	Yes	Yes	Yes	Yes
Western US x Quarter FEs	Yes	Yes	Yes	Yes	Yes
Avg. of Dep. Var.	0.0946	0.0532	0.0417	0.0247	0.144
R^2	0.029	0.027	0.025	0.022	0.149
N	69053	69053	69053	69053	69053

** denotes 5% significance, *** denotes 1% significance. Std. errors are clustered at the msa level.

Table XIV
Selection: An alternative Approach

	Original Rate	Margin	30 DPD	60 DPD	90 DPD	Foreclosure	Prepayment
Unreported Investor	0.061***	-0.007***	0.015***	0.010***	0.006*	0.004*	0.001
	(0.010)	(0.002)	(0.005)	(0.004)	(0.003)	(0.002)	(0.003)
Margin			-0.032**	-0.038***	-0.033***	-0.015***	-0.014*
			(0.015)	(0.011)	(0.009)	(0.006)	(0.007)
Interest Rate			0.052***	0.035***	0.031***	0.014***	0.029***
			(0.012)	(0.009)	(0.006)	(0.003)	(0.004)
Original Interest Rate			0.000	0.004	0.004	0.003	-0.011***
			(0.004)	(0.003)	(0.002)	(0.002)	(0.002)
MSA FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg. of Dep. Var.	5.547	2.430	0.0812	0.0426	0.0302	0.0117	0.0223
R^2	0.327	0.680	0.036	0.027	0.024	0.019	0.028
N	27751	27751	27751	27751	27751	27751	27751

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