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Do Large-Scale Refinancing Programs Reduce Mortgage Defaults? Evidence From a Regression Discontinuity Design*

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Abstract

In 2012, the Federal Housing Administration (FHA) reduced fees to refinance FHA-insured mortgages obtained before—but not after—a retroactive deadline. We use a natural experiment to study how reduced mortgage payments affect default rates. Using a regression discontinuity design, we find that reducing payment size by 1 percent lowers conditional default rates by 2.75 percent. Evidence suggests that those effects are larger for borrowers with negative equity and lower credit scores. We estimate that the policy will prevent more than 35,000 defaults of FHA-insured mortgages, saving FHA nearly $1 billion in present-value terms.

JEL Codes: G18, G21, E65, H50

1 Introduction

In March 2012, the Federal Housing Administration (FHA) announced that it would reduce the premiums it charges to participate in its streamline refinance (SLR) program.[1] The reduction in premiums was substantial: A borrower with a $200,000 mortgage and a loan-to-value (LTV) ratio greater than 95 percent could expect to save $3,480 in up-front premiums and $1,200 per year in annual premiums. However, only borrowers whose mortgages FHA endorsed by May 31, 2009, were eligible for the reduced premiums. Therefore, borrowers with endorsement dates on opposite sides of the cutoff date faced different financial incentives to participate in the SLR program. This retroactive eligibility rule creates a natural regression discontinuity (RD) design with which to measure how reduced mortgage payments affect borrower behavior. The results suggest that reduced mortgage payments lower default rates substantially and that the reduced fees more than doubled the number of SLRs between July 2012 and December 2013. We estimate that the policy will prevent more than 35,000 defaults of FHA-insured mortgages.

Policymakers may wish to reduce mortgage defaults for several reasons. First, defaults cause losses to taxpayers through the mortgage guarantee programs of FHA, Fannie Mae, and Freddie Mac.[2] Reducing default losses was one of FHA's stated goals in announcing reduced fees on SLRs (Department of Housing and Urban Development [HUD], 2012). Second, defaults and subsequent foreclosures not only have adverse spillover effects on nearby properties and neighborhoods but also entail significant deadweight losses. For instance, Anenberg and Kung (2014) estimate that listing a foreclosed property for sale reduces home prices within a third of a mile by 1.5 percent, although Gerardi and others (2012) find that such effects last no more than one year. Ellen, Lacoe, and Sharygin (2013) estimate that an additional foreclosure leads to a 1 percent increase in crime in the block of the foreclosed property. Considering many of these effects and the administrative costs associated with a foreclosure, HUD in 2010 estimated the deadweight loss associated with a foreclosure to be approximately $50,000. Third, defaults and foreclosures may reduce real economic activity by reducing the value of households' assets, in turn reducing consumer demand. For instance, Mian, Sufi, and Trebbi (2014) estimate that between 2007 and 2009, foreclosures accounted for one-third of the decline in house prices and one-fifth of the decline in residential investment and auto sales.

Yet according to Fuster and Willen (2015), "Surprisingly little is known about the im-

[1] The press release announcing the reduction in premiums is available at http://go.usa.gov/3w92m.

[2] Although Fannie Mae and Freddie Mac were historically private companies, the federal government now owns a majority stake in both companies.

portance of mortgage payment size for default" (n.p. [see abstract]). The major obstacle to estimating how mortgage payment size influences borrower behavior has historically been the lack of random variation in payment size. As Fuster and Willen express the problem, "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" (p. 7). The lack of random variation in payment size makes it difficult to isolate the effect of payment size on borrower behavior from possible confounding factors. Such factors include borrowers' potentially unobserved risk characteristics or their willingness to default on a mortgage even if they can afford to continue making payments. This paper aims to help fill that gap by using quasi-experimental methods to analyze the effects of FHA's reduction in fees to encourage SLRs.

Lacking experimental data, recent studies of the effects of payment reductions have generally taken three approaches:[3]

1. Study the behavior of loans that receive modifications privately negotiated between the lender or servicer and the borrower.

2. Compare performance between loans that participate in the Home Affordable Refinance Program (HARP) and loans that do not.

3. Examine the behavior of loans whose payments change because of features of the original mortgage contract, such as an adjustable interest rate or a nonstandard amortization schedule.

Using approach 1, Adelino, Gerardi, and Willen (2013) find that the probability that a previously delinquent loan that receives a modification will redefault within six months is 49 percent in their full sample of modified loans. But for "concessionary" modifications that involve a payment reduction, the probability was only 42 percent. Haughwout, Okah, and Tracy (2010) find that a 10 percent reduction in monthly payment reduces the probability of redefault in the next year by 13 percent. Agarwal and others (2010) find that a payment reduction of 10 percent reduces the probability of redefault within six months by 3 percentage points in relation to a baseline probability of approximately 40 percent. They argue that affordability, rather than negative equity, is the primary driver of redefault in their data.[4]

Approach 2 requires modeling the selection of borrowers into HARP, since participation in the program is not random. Using a propensity score model to predict borrowers' selec-

[3]See Appendix Table A1 for a summary of the papers this section surveys.

[4]Haughwout, Okah, and Tracy (2010) define a redefault as a 90-day delinquency, whereas Agarwal and others (2010) define a redefault as a 60-day delinquency.

tion into HARP, Zhu (2012) finds that a HARP refinance lowers default probabilities by 54 percent over a 15-month period. Zhu (2014) uses logistic regression analysis to control for observable loan and borrower characteristics and finds that a HARP refinance reduces default probabilities by 53 percent, also over a 15-month period. Finally, Zhu and others (2014) control for borrower selection by using an inverse probability weighting approach to account for borrower selection into HARP, finding that a 10 percent payment reduction reduces monthly default probabilities by around 10 percent to 11 percent.

Nonrandom selection of borrowers into payment reduction programs such as HARP could bias the results of observational studies of their effects in at least two ways. First, borrowers at higher risk of default may be more likely to participate because of their need for assistance. Controlling for observed indicators of borrower distress may not correct for this issue if borrowers have private information regarding their likelihood to default. If program participants are disproportionately likely to be at high risk of default, payment reductions will appear less effective at reducing default rates. Second, more financially sophisticated borrowers are more likely to participate in payment reduction programs because they are more likely to be aware of their existence. For instance, Keys, Pope, and Pope (2014) present evidence that many households do not refinance when doing so appears financially advantageous. More sophisticated borrowers may also be less likely to default conditional on their observed characteristics. If participants are disproportionately financially sophisticated borrowers, payment reductions may appear more effective at reducing default rates.

The evidence suggests that nonrandom selection of borrowers may be a meaningful concern for both groups of studies. For instance, Adelino, Gerardi, and Willen (2013) find that servicers are more likely to modify loans of borrowers whom they expect to default otherwise, whereas Agarwal and others (2010) find that "weaker" borrowers receive more favorable modification terms. Zhu and others (2014) find that, of the variables they consider, the identity of the borrower's loan servicer is the strongest predictor of HARP participation.

Using approach 3, Tracy and Wright (2012) study a sample of prime adjustable rate mortgages (ARMs) that experienced downward interest rate adjustments. The authors estimate that a 10 percent reduction in monthly payments reduces conditional default rates by 17 percent to 22 percent. Fuster and Willen (2015) compare subprime hybrid interest-only ARMs that have different initial terms over which the interest rate is fixed.[5] Because of this variation, some loans in their sample experienced large payment reductions due to falling

[5] A hybrid ARM begins with a fixed interest rate for a certain period, after which the rate becomes adjustable. The mortgage contract specifies the length of the fixed term.

interest rates, whereas other loans' payments remained fixed. In that study, a 50 percent reduction in monthly payments lowers monthly default probabilities by about 55 percent. Amromin and others (2013) examine "complex mortgages" that feature zero or negative amortization at the beginning of the life of the loan but that later reset into amortizing payments. That study finds that a 38 percent increase in monthly payment increases the probability of default by 23 percent.

An attractive feature of the studies using approach 3 is that the payment variations result from terms of the mortgage contracts; lenders, servicers, or borrowers do not choose those variations after the mortgages might become distressed. Therefore, those studies should be less susceptible to concerns that borrowers who receive payment reductions may differ systematically from borrowers who do not in potentially unobservable ways. Nonetheless, borrowers who select nonstandard loan products such as interest-only or subprime ARMs may differ from standard borrowers in systematic, and potentially unobservable, ways.

Our work here exploits the quasi-experimental variation in rates of streamline refinancing provided by the discontinuous eligibility rules for reduced premiums to participate in FHA's SLR program. Eligibility for reduced fees has a large effect on the rate at which borrowers streamline refinance. Whereas 30 percent of loans endorsed in the month before the cutoff date had streamline refinanced by the end of 2013, only 14 percent of loans endorsed in the month after the cutoff date had done so. Those differences in refinancing led to different default rates.

The discontinuity in eligibility for reduced fees allows us to estimate the effect of mortgage payment reductions on default rates by using a fuzzy RD design. Our preferred specifications imply that streamline refinancing reduces the monthly conditional default rate by 63 percent and that the elasticity of the conditional default rate with respect to payment size is 2.75. Those estimated effects are somewhat larger than other estimates in the literature. We estimate that the lower fees will cost FHA $1.7 billion in income, but the reduced defaults will save FHA $2.6 billion in losses, for a net budgetary savings of $900 million.[6]

[6]Those estimates are calculated in the spirit of the procedures prescribed by the Federal Credit Reform Act of 1990, which expresses the cost to the government in net-present-value terms by using Treasury rates to discount cash flows to the date of refinance. CBO has also provided the Congress with supplemental estimates of FHA's costs on a fair-value basis, which includes an adjustment for the cost of market risk in the present-value calculation. In this case, a fair-value estimate would show larger savings, because default risk would be lower as a result of the policy. An analysis of the full budgetary effect or of the broader costs and benefits of refinancing to borrowers, mortgage investors, and other stakeholders is beyond the scope of this paper. See Remy, Lucas, and Moore (Congressional Budget Office 2011) for a discussion of those issues.

2 FHA's Streamline Refinancing Program

Through FHA's SLR program, borrowers can refinance existing FHA-insured mortgages with less stringent documentation and underwriting than loans typically require to qualify for FHA insurance.[7] The SLR program itself long predates the housing crisis, but in 2012 FHA substantially reduced the fees it charges some borrowers—but not others—to participate. That fee change, discussed later in this section, provides the natural experiment we study in this paper. To qualify a borrower for an SLR, FHA requires neither an updated appraisal on the mortgaged property nor a credit report.[8] In April 2011, FHA stopped requiring lenders to verify borrowers' employment and income on SLRs, although before that time borrowers had to show that they could make the new mortgage payments. As the website *The Mortgage Reports* describes the program (Green 2014):

> When you put it all together, you can be (1) out-of-work, (2) without income, (3) carry a terrible credit rating, and (4) have no home equity. Yet, you can *still* be approved for an FHA Streamline Refinance.

SLR transactions must meet certain other requirements, however: The borrower must have made at least six payments on the outstanding mortgage. At least six months must have passed since the outstanding mortgage's first payment due date, and at least 210 days must have passed from the outstanding mortgage's closing date. If the outstanding mortgage has fewer than 12 months' payment history, the borrower must have made all payments within the month due. Otherwise, the borrower must not have made more than one 30-day-late payment over the previous 12 months and must have made all payments within the month due for the previous three months. No more than $500 cash back may be taken out by using an SLR. Furthermore, the SLR must provide a "net tangible benefit" to the borrower, which for fixed rate mortgages (FRMs) is defined as a reduction of at least 5 percent in the borrower's principal and interest (P&I) payment plus annual mortgage insurance premium (MIP).

For SLRs and other mortgages, borrowers must pay both an up-front MIP and an annual MIP in exchange for FHA insurance. FHA has raised its insurance premiums substantially since 2009. For SLRs originated in fiscal year 2009, FHA charged an up-front MIP of 150 basis points and an annual MIP of 55 basis points on a 30-year FRM with a loan amount less than $625,000 and an LTV ratio greater than 95 percent (HUD 2014).[9] By June 2012,

[7] See HUD Handbook 4155.1.6.C for program eligibility requirements.

[8] The discussion of the program in this section pertains only to non–credit-qualifying SLRs. However, credit-qualifying SLRs have different program rules and features.

[9] A basis point is one one-hundredth of a percentage point.

the up-front MIP on such a loan had risen to 175 basis points and the annual MIP had risen to 115 basis points. The annual MIP increased to 135 basis points in April 2013. Figure 1 shows how FHA's MIPs have evolved, along with average interest rates on FHA-insured mortgages.

On March 6, 2012, FHA announced that it would reduce MIPs for SLRs of mortgages endorsed by May 31, 2009 (HUD 2012). The change took effect June 11, 2012. The reduced up-front MIP for those loans was 1 basis point of the loan amount and the reduced annual MIP was 55 basis points. Borrowers with mortgages endorsed June 1, 2009, and later did not qualify for the lower premiums and therefore faced substantially higher premiums to participate in the SLR program. In the program announcement, FHA estimated that streamline refinancing could save the average eligible borrower approximately $3,000 per year.

The volume of streamline refinancing increased markedly after the change in fees took effect. FHA endorsed an average of 22,100 SLRs per month between January and June 2012, compared with an average of 41,200 per month between July and December 2012.[10] Nearly 410,000 borrowers participated in the SLR program under the reduced fee schedule through December 2013.[11]

3 Data

This paper uses mainly a loan-level data set that FHA generated in May 2014. The data set contains the universe of FHA-guaranteed loans in the credit subsidy cohorts 2009 to 2013, as well as the loans guaranteed in the 2014 cohort for which data were available when the data set was created.[12] The data set includes the terms of the loan, such as the original mortgage amount, amortization start date, loan term, interest rate, annual MIP, ARM status, and loan purpose (for example, purchase or refinance, including a code for SLRs). The data set also includes several borrower and property characteristics used to model loan performance, such as the original LTV ratio, borrower's annual effective income, borrower credit score, and the state and metropolitan statistical area where the mortgaged property

[10] The average interest rate on newly endorsed FHA mortgages fell from 4.10 percent in January 2012 to 3.52 percent in December 2012, which would have contributed to the increase in streamline refinancing volume apart from the reduction in fees.

[11] For comparison, 3.1 million loans refinanced under HARP through the end of 2013 (http://go.usa.gov/3AMBe). That program was in place well before FHA's fee reductions on SLRs took effect, so borrowers had more time to participate in it by the end of 2013.

[12] A credit subsidy cohort is the group of loans that FHA guarantees under its budget authority for a particular fiscal year.

is located. Finally, the data set includes variables relating to loan performance, including the status of the loan when the data set was generated (for example, active, terminated, or claim) and data concerning the eventual loss FHA bore on loans that resulted in a claim. Crucially, the data set also includes the endorsement date of each loan, as well as a case number for each loan and an old case number for loans that were refinances of previous FHA loans. The presence of both the new and old case numbers allows us to link the loans within a refinancing chain.

A supplementary data set records all 60- and 90-day-or-longer delinquency events in the life of each loan, including episode start and end dates. That information allows us to calculate the conditional probability that a loan enters serious delinquency status. We follow much of the literature in focusing on a loan's first 90-day delinquency episode as our default event of interest.[13] Because our data reflect a slight delay in the reporting of delinquency episodes, we censor our study at the end of 2013.

To focus on the period around the cutoff date, we restricted our initial sample to the 340,126 loans originally endorsed in May or June 2009. From those, we dropped 880 loans that were not FRMs; 15,105 loans that were not 30-year mortgages; 2,342 loans for which the mortgaged properties were located in Puerto Rico, the Virgin Islands, or Guam; 13 loans with a termination code of "Cancellation"; 3 loans with duplicate case numbers, and 6,203 loans with beginning amortization dates before April 1, 2009, or after July 31, 2009. Excluding those left 315,580 loans.

3.1 Descriptive Statistics

Table 1 displays descriptive statistics for loans endorsed in May, loans endorsed in June, and the whole sample. FHA's endorsement volume rose from 141,243 loans in May to 174,337 loans in June. That increase in endorsement volume reflects the increasing stress in the conventional mortgage market in spring 2009. Figure 2 shows the number of loans by business date of endorsement.

The average original mortgage amount in the sample is $186,433, and the difference between months is not statistically significant. The standard deviation is $93,492, and the distribution is positively skewed, with a median of $166,920. The average FICO score of all borrowers is 689 in the whole sample, rising statistically significantly from 687 in May

[13] Tracy and Wright (2012) and Haughwout, Okah, and Tracy (2010) also adopted this definition. Fuster and Willen (2015) also focused on a loan's transition to serious delinquency but adopted a 60-day delinquency threshold.

to 691 in June.[14] That increase in borrower credit scores probably reflects the increasing difficulty of obtaining conventional mortgages during this period, which is thought to have led borrowers with relatively high credit scores to take out FHA mortgages instead. Average mortgage interest rates fell slightly from an average of 5.2 percent in May to 5.1 percent in June, while the average original LTV ratio rose slightly from 93.5 percent to 93.6 percent. Both differences are statistically significant. The share of endorsements that were refinances as opposed to purchase loans fell from 52.7 percent in May to 50.2 percent in June, also a statistically significant difference.[15] The estimated monthly P&I payment was $1,012 and fell $8 from May to June. Finally, average annual household income was $66,209 and the difference between May and June was not statistically significant. Figure 3 shows the average value for each of those variables by business date of endorsement.

California accounts for the largest share of loans, with 9.6 percent, followed by Texas, with 6.8 percent, Georgia with 4.1 percent, and Illinois and Florida with 4.0 percent each (see Table A2). The top 10 states by share of endorsements account for 46.1 percent of all endorsements. The geographical distribution is stable from May to June.

Table 2 summarizes loan outcomes as of December 1, 2013, again broken out by endorsement month. The table contains information both on outcomes for the original loans in the sample and for the subsequent outcomes of internally refinanced loans. For convenience, we will define a *loan chain* as the originally endorsed loan and any later internal refinances. Approximately 80 percent of all loan chains in the original sample remained active as of December 2013, with 80.5 percent originally endorsed in May 2009 (*May chains*) and 79.3 percent in June 2009 (*June chains*). By December 2013, 13.0 percent of May chains had been paid in full, compared with 14.9 percent of June chains.[16] By that time, 6.4 percent of May chains had resulted in a claim against FHA, compared with 5.9 percent of June chains, whereas 4.5 percent of May chains remained active but were delinquent by 90 or more days, compared with 4.3 percent of June chains.

The original loan in the chain remained active as of December 2013 in 51.2 percent of May chains versus 64.8 percent of June chains. Conversely, 30.0 percent of May chains had streamline refinanced by that time versus 14.1 percent of June chains. As of June 1, 2012, shortly before the fee reduction went into effect, 83.3 percent of the original loans in

[14] Some loans in the sample were themselves SLRs of loans FHA had previously guaranteed and are therefore missing data for some characteristics. All loans have information on original mortgage amount, interest rate, and refinance status. Roughly 75 percent of loans have information on the other characteristics.

[15] Those percentages count all refinances, not merely SLRs or refinances of previously FHA-insured loans.

[16] We will refer to the event of a loan being paid in full as an external refinance, although in theory some borrowers may not take out another mortgage to repay their FHA-insured mortgage.

a May chain remained active, as opposed to 85.1 percent of June chains. Between July 2012 and December 2013, 22.1 percent of loans in May chains streamline refinanced, compared with 7.4 percent of loans in June chains. Thus, whether a loan's endorsement date was before or after the cutoff date was an important determinant of whether that loan eventually streamline refinanced.

Table 2 shows the status of chains that streamline refinanced in July 2012 or later. A higher proportion of those chains remained active as of December 2013: 96.9 percent of May chains versus 97.2 percent of June chains. The borrower later refinanced externally in 3.0 percent of May chains and 2.7 percent of June chains, whereas approximately 0.1 percent of both types had gone to claim by December 2013. A further 0.5 percent of May chains and 0.9 percent of June chains were seriously delinquent as of December 2013.

Table 3 shows the characteristics of chains before and after streamline refinancing. Chains that do streamline refinance tend to have larger original loan amounts, higher credit scores, higher original LTV ratios, and higher borrower incomes than chains that never streamline refinance. May chains and June chains have roughly the same interest rates and annual premiums before refinancing. For outcomes of loans after refinancing, Table 3 shows that the average interest rate fell from 5.09 percent to 3.73 percent for May chains and from 5.11 percent to 3.58 percent for June chains. In contrast, the annual MIP stayed roughly constant for May chains, rising from 0.53 percent to 0.55 percent, but rose substantially for June chains, from 0.54 percent to 1.21 percent. That pattern reflects the May chains' exemption from the substantial increases in FHA's annual MIPs from 2009 to 2012. Overall, the May chains experienced a combined P&I plus annual MIP payment reduction of $221 per month after refinancing, substantially larger than the $119 reduction for June chains.

4 Fuzzy Regression Discontinuity Design

To estimate how FHA's program of reducing fees to encourage refinancing affects loan performance, we use a standard regression discontinuity (RD) design framework. Although borrowers with endorsement dates on different sides of the cutoff date have sharply different probabilities of participating in the SLR program, eligibility for reduced fees does not completely determine whether a borrower participates. Some, but not all, borrowers with endorsement dates on both sides of the cutoff date participate, producing a *fuzzy RD design*.

In the notation of Lee and Lemieux (2010), we estimate systems of equations of the

following form:

$$Y = \alpha + \tau D + f(X - c) + h_Y(M) + \varepsilon \tag{1}$$

$$D = \gamma + \delta T + g(X - c) + h_D(M) + \nu \tag{2}$$

$$T = \mathbb{1}[X < c]. \tag{3}$$

In this system, Y represents an outcome of interest, such as the conditional default rate over the study period. D represents a treatment of interest, such as whether a loan streamline refinanced or the loan's payment reduction. T represents the intent to treat—here, eligibility for reduced fees to streamline refinance—which takes the value one if a loan's endorsement date is before the cutoff date and zero otherwise. X represents the running variable, here the original loan's endorsement date, and c represents the cutoff date. We normalize X such that c equals zero (see the appendix for more detail). Because X is defined in terms of endorsement dates, our running variable is discrete. $f(\cdot)$ and $g(\cdot)$ are polynomial functions of the running variable. M is the number of months after June 2012, and $h_Y(\cdot)$ and $h_D(\cdot)$ are polynomial functions. ε and ν are uncorrelated random errors.

Lee (2008) shows that when individuals have imprecise control over the running variable X, the intent to treat T is "as good as randomized" in the area of the cutoff. At first glance, the imprecise control assumption may seem odd in this setting: Presumably borrowers control the date at which they obtain their mortgages. However, X is defined not as the endorsement date of the mortgage itself but rather the endorsement date in relation to the cutoff date, which was not announced until nearly three years after the cutoff date. Therefore, we view the imprecise control assumption as quite natural in this setting. Furthermore, Lee (2008) and McCrary (2008) show that the local randomization result produces testable implications. The appendix shows our tests of those implications and discusses the validity of the RD design.

Following the approach of Hahn, Todd, and van der Klaauw (2001), we use two-stage least squares to estimate the system of equations (1) and (2). Those authors show that two assumptions are required to interpret the $\hat{\tau}$ estimated from this system as an average treatment effect. In Lee and Lemieux's (2010) terminology, the first assumption is monotonicity, which in our context amounts to the assumption that eligibility for reduced fees did not decrease the likelihood of any borrowers to streamline refinance. The second assumption is excludability, which in our context states that whether a loan's endorsement date was before or after June 1, 2009, affects its performance only by affecting the probability of streamline refinancing. Both assumptions seem intuitively reasonable.

When those conditions hold, $\hat{\tau}$ is an estimate of the weighted local average treatment effect (LATE) for compliers, that is, borrowers whose eligibility for reduced fees affected the decision to refinance (Lee and Lemieux 2010). The weights are the ex ante probabilities that a borrower's endorsement date was near the cutoff date before the eligibility rule was determined.

We exploit the panel structure of the data by following a suggestion of Lee and Lemieux (2010) to conduct the estimation on a pooled cross-section of the data. The specifications control for a polynomial time trend as a covariate in the regressions but do not include individual loan dummies. An observation in this system of equations is a loan-month. We use fourth-degree polynomials for $h_Y(\cdot)$ and $h_D(\cdot)$ regardless of the degree of polynomials used for the functions $f(\cdot)$ and $g(\cdot)$. We allow the polynomial $h_D(\cdot)$ to be estimated separately on both sides of the cutoff in the treatment equation (2) to account for the difference in take-up of treatment induced by the reduction in fees. However, we estimate the polynomial $h_Y(\cdot)$ jointly on both sides of the cutoff. This specification amounts to assuming that the time trend in default rates would have affected loans on both sides of the cutoff date equally if not for the differential take-up of the treatment, that is, participation in the SLR program.

Exploiting the panel structure of the data allows for more precise inference regarding the effects of payment reductions because the reduction in fees on SLRs led to different rates of streamline refinancing only over time. In July 2012, immediately after the reduction in fees became effective, a similar proportion of eligible and ineligible loans had streamline refinanced, whereas by December 2013, a substantially higher proportion of loans eligible for the reduced fees had done so. Therefore, if payment reductions do lead to lower default rates, the conditional default probabilities of eligible and ineligible loans should be similar in July 2012, but the eligible loans should have lower default rates by December 2013.

One drawback of the specifications presented here is the linearity assumption they entail. A loan's conditional default rate over the study period cannot be below zero, yet the fitted values of the linear model may yield such a prediction. Likewise, whether a loan streamline refinanced or defaulted in a particular month is a binary variable. The specifications discussed above employ a linear probability model for those outcomes, which again can yield inadmissible fitted values. In practice, however, these concerns appear minor. Appendix Figure A2 displays the fitted values for key variables in our preferred specifications. Either the predicted values are entirely within the admissible range or the proportion of inadmissible predicted values is small.[17]

[17]Bivariate probit regressions, which account for the potential endogeneity of streamline refinancing and in

5 Results

We discuss the results of our estimates of the system of equations (1)-(2) by using two measures of D, the treatment of interest: whether a loan had streamline refinanced and the payment reduction a loan experienced by streamline refinancing. We first discuss Figures 4 and 5, which show the data in the regressions that follow. We then discuss the regression results in Tables 4 and 5. Figure 4 examines loan-level average monthly outcomes for July to December 2013, by which time the reduction in fees had given rise to substantially different proportions of streamline refinances between May and June loan chains. The effect on defaults is more apparent in these two-dimensional cross-sectional graphs than in the three-dimensional panel graphs that follow. Figure 5 and Tables 4 and 5 present loan-month–level analysis for July 2012 through December 2013. The sample used in the RD analysis has been confined to chains in which the original loan had not refinanced and had not been delinquent for 90 or more days before July 2012.

5.1 Graphical Evidence

Figure 4A shows by endorsement date the average proportion of loans that had streamline refinanced through December 2013. Roughly 30 percent of loans endorsed before the cutoff date had streamline refinanced, compared with about 10 percent for loans endorsed on or after the cutoff date.[18] A striking discontinuity is evident at the cutoff date.

Figure 4B shows by endorsement date the average percent reduction in effective monthly payment (that is, P&I plus annual premium) for the loans that streamline refinanced. The average reduction is roughly 18 percent for loans eligible for reduced fees but less than 10 percent for ineligible loans.

Figure 4C shows by endorsement date the weighted-average effective payment reduction that loans received from streamline refinancing during July through December 2013. In other words, Figure 4C displays the product of the payment reductions displayed in Figure 4B and the proportion of months for which loans were streamline refinanced. Therefore, Figure 4C shows the reduction in total effective monthly payments due to streamline refinancing over the period. May chains were more likely to streamline refinance and received larger effective payment reductions when they did. Thus, the weighted-average payment

which the predicted probabilities of streamline refinancing and default are constrained to be between zero and one, generate point estimates for the average treatment effects that are somewhat larger than the local average treatment effects reported in the main specifications. Those results are available on request.

[18] These numbers differ slightly from the descriptive statistics in Table 2 because of the additional sample restrictions described above.

reduction is substantially larger for those chains than for June chains; the discontinuity at the cutoff is roughly 4 percentage points.[19]

Figure 4D shows by endorsement date the average monthly conditional default rate between July and December 2013. The monthly conditional default rates range between 0.25 percent and 0.45 percent on both sides of the cutoff date but are higher on average after the cutoff. Without the difference in fees, we would expect the conditional default rate to be lower for loans with a later original endorsement date, since borrower FICO scores rose from the beginning of May to the end of June, as shown in Figure 3E. If streamline refinancing reduces default rates, however, we should expect to see the decline in default rates interrupted by a jump upward at the cutoff date. Figure 4D shows such a pattern, suggesting that streamline refinancing does reduce default rates. To get a rough estimate of how streamline refinancing affects conditional default rates, one can compare the size of the upward shift in the default rate in Figure 4D with the sizes of the discontinuities in the treatment measures in Figure 4A and 4C.

Figure 5 shows three-dimensional panel graphs organized by the endorsement date of the original loans in the chain and the calendar months in the study period, from July 2012 to December 2013. Figure 5A shows the cumulative rate of streamline refinancing, and Figure 5B shows the cumulative proportional payment reduction that borrowers received from refinancing. Because the analysis is restricted to chains that had not streamline refinanced before the reduction of fees on June 11, 2012, the proportional payment reduction is approximately zero in July 2012 for both May chains and June chains. The proportion of May chains that had streamline refinanced grows more rapidly than the proportion of June chains, and the May chains paid lower annual guarantee fees, resulting in a substantial difference in payment reductions by the end of the sample period. By December 2013, the average payment reduction is roughly 6 percent for the May chains and roughly 1 percent for the June chains. Figure 5C shows monthly conditional default rates during the period, that is, how many loans default in a given month as a proportion of all active loans that had never defaulted. Essentially no discontinuity was present in conditional default rates in July 2012, before the loans in the sample had much time to streamline refinance. By the end of the sample period, when substantially more May chains had refinanced than June chains, a visible discontinuity is evident in conditional default rates, with a jump upward around the cutoff interrupting a generally downward trend over the original endorsement dates.

[19]The treatments in Figure 4 may understate the treatment intensity relevant to the borrower's decisionmaking. For instance, if a loan refinances in December 2013, the calculated effective payment reduction is only one-sixth the reduction in the P&I plus annual MIP payment. However, the borrower's payment will be lower for the lifetime of the loan.

Figure 5D shows the predicted monthly conditional default rates from the second stage of our preferred specification, reported in Table 5. May and June chains have similar predicted default rates in July 2012, but May chains are predicted to have lower average default rates by December 2013.

5.2 Regression Results

Table 4 shows our estimates of how streamline refinancing affected the conditional default rate between July 2012 and December 2013. In the first stage, we instrument for streamline refinance status by using eligibility for reduced fees. In the second stage, we regress conditional default rates on the predicted streamline refinance status. Each stage includes a polynomial in the number of business days between the original endorsement date and the cutoff date, estimated separately on each side of the cutoff. All specifications feature a fourth-degree polynomial in calendar month, which is estimated separately on each side of the cutoff date in the first stage but estimated jointly on both sides of the cutoff date in the second stage. That specification accounts for the different time trends in streamline refinance status between May and June loan chains with the implicit assumption that the time trend in default rates would have affected both types of chains identically if not for the differential trend in refinancing.[20]

Column 1 shows results from a specification featuring a first-degree polynomial in the endorsement date. The estimated semielasticity of default rates with respect to having streamline refinanced is -0.62 when evaluated at the predicted default rate for loans endorsed at the cutoff date. The estimated effects in column 2, which features a second-degree polynomial, is -0.63. The estimated effects in columns 3 and 4, which use third- and fourth-degree polynomials, respectively, are -0.67 and -0.72. Each estimated semielasticity is statistically significant at the 1 percent level.

Formal information criteria such as the Akaike information criterion and Bayesian information criterion for selecting the polynomial order give conflicting results depending on the criterion used and the stage of the regression examined. Fortunately, the estimated effect of streamline refinancing is not sensitive to the polynomial order used in the regressions; all the estimated effects are within one standard deviation of each other. Since Gelman and Imbens (2014) show that an RD design should use lower-order polynomials in the running variable, we take the second-order specification in column 2 as our preferred specification.

Columns 5 and 6 of Table 4 use a sample restricted to loans for which we have data

[20]This specification leads to the model being overidentified, because the second stage has more excluded instruments (five) than endogenous regressors (one).

on the covariates shown in Figure 3. Both columns use the second-order polynomial specification from column 2, whereas column 6 also includes the covariates in the regression. Including the covariates results in an estimated semielasticity of default rates with respect to streamline refinancing of -0.68, which is larger in absolute value than in column 2 and is statistically significant at the 1 percent level. However, we prefer the specification in column 2 that does not include covariates, because the loans that are missing information on covariates are unlikely to be a random sample of all loans. In particular, loans that were themselves originally streamline refinances of older loans when endorsed in 2009 are generally missing information on borrower credit score and LTV ratio. The similar results in columns 5 and 6 suggest that the RD design effectively emulates random assignment to treatment.

Table 5 shows our estimates of how payment reductions affect the conditional default rate between July 2012 and December 2013. In the first stage, we instrument for payment reductions by using eligibility for reduced fees. In the second stage, we regress conditional default rates on the predicted payment reductions. Otherwise, the specifications follow the pattern in Table 4. Using the payment reduction as the treatment variable allows us to calculate the elasticity of the conditional default rate with respect to the size of payment reduction.

The results are similar to those in Table 4. In column 1, the estimated elasticity of defaults with respect to payment reductions is -2.77. In column 2, which features a second-degree polynomial, the estimated elasticity is -2.75. The third-degree specification in column 3 gives an estimated elasticity of -2.92, and the fourth-degree specification in column 4 gives an estimated elasticity of -3.16. All four estimates are statistically significant at the 1 percent confidence level. Columns 5 and 6 restrict the estimation sample to loans that have complete covariate data. As in Table 4, the estimated elasticities in specifications 5 and 6 are similar but slightly larger than the estimate from the full sample in column 2.

Figure 5D shows the predicted monthly conditional default rates from the second-stage regression in column 2 of Table 5. Those rates do not show a noticeable discontinuity in July 2012, consistent with few loans having streamline refinanced by then. By the end of 2013, a noticeable discontinuity is evident in the predicted default rates. Although the discontinuity may appear to be small in the figure, compare its magnitude with the size of the treatment. The relevant comparison may be more easily seen in Figure 4, which shows a discontinuity in conditional default rates of approximately 3.4 basis points, about 9 percent of the conditional default rate for loans endorsed on June 1. For comparison, the discontinuity in the proportion of loans that streamline refinanced is 17 percentage points,

whereas the discontinuity in the average payment reduction is approximately 4 percentage points. Therefore, the discontinuity in default rates is meaningful in relation to the size of the treatment.

5.3 Variability of Effects by Loan or Borrower Characteristics

Here we examine whether the effects of streamline refinancing vary by mark-to-market (MTM) LTV ratios or borrowers' FICO scores.[21] We split the sample into groups with MTM LTV ratios above or below the median value of 98.1 percent as of June 1, 2012, and, separately, into groups with borrower FICO scores above or below the median value of 687. Appendix Figure A3 shows the distribution of MTM LTV ratios and borrower FICO scores in the sample. Because many loans endorsed from May 1, 2009, to June 30, 2009, were themselves streamline refinances of previously FHA-insured mortgages, MTM LTV ratios and borrower FICO scores are missing for approximately one-quarter of the sample.

Figures 6 and 7 show loan outcomes split by MTM LTV ratio and borrower FICO score, respectively, in relation to the median. The figures illustrate the proportion of possible months for which the loans were streamline refinanced, the weighted-average payment reduction that the loans experienced, and the average monthly conditional default rate from July to December 2013. The figures are analogous to Figure 4 in that they display results for loan-level averages rather than at the loan-month level.

Figure 6 (panels A and B) shows that loans with MTM LTV ratios both below and above the median had sharply different propensities to streamline refinance depending on their eligibility for reduced fees. The discontinuity is larger for loans with MTM LTV ratios above the median. Figure 6 also shows that the discontinuity in default rates is much more pronounced for loans with MTM LTV ratios above the median (Figure 6F) than below the median (Figure 6E), consistent with the larger discontinuity in streamline refinancing for loans with higher MTM LTV ratios.

Similarly, Figure 7 (panels A and B) shows that eligibility for reduced fees strongly affected streamline refinancing for borrowers with FICO scores both below and above the median, although the discontinuity is larger for borrowers with FICO scores above the median. That difference may reflect a higher likelihood that borrowers with higher FICO scores are eligible to streamline refinance.[22] The discontinuity in conditional default rates is larger

[21] We calculate the loans' MTM LTV ratios by applying an appreciation factor derived from the Federal Housing Finance Agency all-transactions index at the metropolitan statistical area level to estimate current property values as of June 1, 2012. The MTM LTV ratio is calculated as the ratio of the amortized loan balance to the estimated current property value.

[22] As discussed in Section 2, to be eligible to streamline refinance, borrowers generally must not have made

for loans with borrower FICO scores above the median, consistent with the larger discontinuity in streamline refinancing for those loans.

Table 6 presents evidence of heterogeneity in the effects of payment reductions arising through streamline refinancing. The table examines loan-month–level data from July 2012 to December 2013 and displays results of instrumental variable regressions with the same specification as column 2 of Table 5, which is repeated in column 1 for comparison.[23] Columns 2 and 3 show results for loans with MTM LTV ratios below and above the median as of June 2012, respectively. The estimated elasticity of the conditional default rate with respect to payment reductions is -0.91 and not statistically different from zero for loans with MTM LTV ratios below the median. The estimated elasticity is -4.37 and statistically different from zero for loans with MTM LTV ratios above the median. That pattern is consistent with the results of Tracy and Wright (2012) and Zhu and others (2014), who find that the default rates of borrowers with higher current LTV ratios respond more to payment reductions than the default rates of borrowers with lower current LTV ratios. Columns 4 and 5 of Table 6 show results for loans with borrower FICO scores below and above the median, respectively. The estimated elasticity is -3.88 and statistically different from zero for loans with borrower FICO scores below the median, but the estimated elasticity is -2.24 and statistically indistinguishable from zero for loans with borrower FICO scores above the median.

Table 6 also gives the results of formal tests of the hypothesis that streamline refinancing exerts equal effects across groups.[24] A test of the null hypothesis that the effects are equal across loans with MTM LTV ratios below and above the median yields a p-value of .11, whereas the same test for loans with borrower FICO scores below and above the median yields a p-value of .04. We view these results as suggestive but imprecise evidence that the effects of streamline refinancing vary across groups.[25]

more than one 30-day-late payment over the previous 12 months and must have zero 30-day-late payments in the last three months. Borrowers with higher FICO scores may be more likely to meet those requirements and therefore to be eligible to take advantage of the reduced fees to streamline refinance. Our data set does not contain information on 30-day delinquencies, so we cannot verify eligibility to streamline refinance on a loan level. However, such a pattern would be consistent with the higher conditional default rates for borrowers with FICO scores below the median shown in Figure 7E and 7F.

[23]We display loan-level average data in Figures 6 and 7 because the visual presentation is much clearer than for loan-month–level data.

[24]The tests are from full-sample regressions that include indicator variables for above-median MTM LTV ratio or FICO score status and interactions of those indicators with the other regressors.

[25]The results of the subgroup analysis are similar when we use streamline refinancing status instead of payment reductions as our measure of treatment.

6 Discussion

The effects of payment reductions on mortgage default rates estimated here are somewhat larger than other such estimates in the literature. We estimate that streamline refinancing reduces monthly conditional default rates by 63 percent and that the elasticity with respect to payment size is 2.75 (that is, a 10 percent payment reduction reduces conditional default rates by 27.5 percent). Some studies estimate effects nearly as large as those we estimate. Zhu (2014) finds that participation in HARP reduces default by 53 percent.[26] Tracy and Wright (2012) estimate that the elasticity of conditional default rates with respect to payment size is 2.25 for loans with current LTV ratios above 80 percent.[27] Other studies find smaller but still substantial effects. Fuster and Willen (2015) estimate that a 50 percent payment reduction leads to a 55 percent reduction in default rates, whereas Zhu and others (2014) estimate that a 10 percent payment reduction leads to a 10 percent to 11 percent reduction in default rates. Agarwal and others (2010) find that a 10 percent payment reduction reduces the probability of redefault by approximately 7 percent—the low end of the range of estimated effects.

Several possible reasons aside from simple sampling variability account for the variation in estimated effect sizes in the literature. First, payment reductions probably do not have a linear effect on default rates. Standard financial models of default—for instance, Kau and others (1992)—emphasize that the ability to default serves as a put option on the value of the house. It is advantageous for the borrower to exercise the option when the value of the house falls far enough below the present value of the future mortgage payments. In that case, reducing the mortgage payment by more than necessary to keep the borrower from exercising the default option will not further reduce the default probability, leading the elasticity with respect to payment reductions to decline as those reductions become larger.

Second, the sample examined in this paper differs from those in other studies. The MTM LTV ratios are generally higher and the borrower FICO scores are on average lower in our sample than those in most other studies.[28] Section 5.3 presents suggestive evidence that those characteristics should be associated with a larger effect of payment reductions on default rates. Zhu and others (2014) and Tracy and Wright (2012) also estimate larger

[26] Zhu (2014) does not report the size of the payment reductions in her data, but Zhu and others (2014), who use a similar data set, report that the average monthly payment reduction is approximately 15 percent, similar to the average in our data.

[27] A total of 96.4 percent of the loans in our estimation sample had MTM LTV ratios above 80 percent as of June 2012.

[28] However, the combined LTV ratios in Fuster and Willen's (2015) sample in 2010 and 2011 are higher than those in our sample.

effects of payment reductions for loans with higher MTM LTV ratios. Furthermore, the loans in our sample are 30-year FRMs, whereas the samples in Haughwout, Okah, and Tracy (2010); Tracy and Wright (2012); and Fuster and Willen (2015) are either mostly or entirely ARMs. Because interest rates on ARMs can adjust upward, downward payment adjustments may affect default rates less for ARMs than for FRMs, which feature a permanently lower payment after refinancing.

Third, our estimates come from a quasi-experimental design that should avoid potential problems of selection bias. To the extent that selection bias accounts for the larger effects we estimate than those in the literature, it would indicate that borrowers who receive payment reductions are at higher risk of default than borrowers who do not, conditional on observable characteristics. Such a selection effect would lead to higher default rates among treated borrowers, reducing the measured treatment effect. Tests of treatment endogeneity in Tables 4 and 5 generally reject the null hypothesis that streamline refinancing is exogenous, although several exceptions exist.[29]

Fourth, the estimated effect in our study is the LATE for compliers, the group whose eligibility for reduced fees determined their participation in the SLR program. The LATE may vary from the average treatment effect for all borrowers, the effect estimated in studies of ARM resets such as Tracy and Wright (2012) and Fuster and Willen (2015). A payment reduction program with a different level of participation might have a different average effect on default rates from that of the program we study. The appendix discusses a test proposed by Bertanha and Imbens (2014), which fails to reject the null hypothesis that the LATE is equal to the average treatment effect for all borrowers.

7 Defaults Prevented and Budgetary Effect on FHA

We estimate the number of defaults prevented by reduced fees for FHA's streamline refinance program as well as the policy's budgetary effects. Those calculations require using several additional simplifying assumptions and sources of information. Perhaps the most important simplifying assumption here is that we use the estimated LATE from section 5.2 to calculate the reduction in default rates for all loans that streamline refinanced. Therefore, the results in this section only roughly estimate the policy's true effect.

[29] Conceptually, those tests measure whether the results of the two-stage least-squares regressions are the same as the results of ordinary least-squares regressions of conditional default rates on treatment status. A rejection of the null hypothesis that the two types of regressions produce the same results suggests the presence of selection bias due to treatment endogeneity.

The first step in calculating how the policy affects defaults is to estimate how many defaults would have occurred without the policy. We use the projected lifetime rates reported in Castelli and others (Congressional Budget Office 2014).[30] We extrapolate the cumulative claim rates of mortgages active when the policy took effect through the end of the mortgage term from those projections.[31]

The second step is to estimate how many additional refinances resulted from the reduced fees. The reduction in fees should have induced additional streamline refinances while discouraging refinances outside FHA, so we distinguish the two. We separately regress the log conditional streamline and external refinance rates of each cohort over the period January 2010 to December 2013 on the following variables:

- A set of cohort dummies,
- The appreciation of the Federal Housing Finance Agency purchase-only U.S. house price index since the January of each cohort year,
- The average reduction of the interest rate plus annual premium (the effective interest rate) that the average loan in each cohort would be expected to experience by streamline refinancing (the effective spread), and
- The spread between each cohort's average effective interest rate and the average monthly value of Freddie Mac's Primary Mortgage Market Survey for 30-year FRMs (the external spread).

The unit of observation in the regressions is the cohort-month. We split the 2009 cohort into loans eligible and ineligible for the reduced fees. The regressions include the 2003–2010 cohorts; prior cohorts contained relatively few eligible loans when the policy was enacted.

[30] We obtain similar results by using the estimates from the *Actuarial Review of the Federal Housing Administration Mutual Mortgage Insurance Fund, Forward Loans, for Fiscal Year 2013* (HUD 2013).

[31] Accounting for a cohort's lifetime claim rate is complicated by the budget practice of attributing a default that occurs on a refinanced mortgage to the cohort in which the refinancing occurred rather than to the cohort in which the loan was originally made. We adjust the reported default rates for each cohort by the proportion of loans expected to refinance internally to FHA to calculate the total number of defaults that would be expected to occur on the loan *chains* active in July 2012 (without a payment reduction). For notational convenience, we denote F_i^{loan} as the cumulative default rate for cohort i reported in the *Actuarial Review* and denote ϕ_i as the proportion of cohort i expected to internally refinance. We calculate this proportion by using the reported cumulative prepayment rates for each cohort, and we assume that the proportion of prepayments that will be streamline refinances will be the same in the future as in the loan-level data set used throughout the analysis in this paper. Then the projected cumulative claim rate for loan chains in each cohort i is $F_i^{chain} = \frac{1}{1-\phi_i} F_i^{loan}$. This calculation assumes that without a payment reduction, loans that internally refinanced would default at the same rates as those that did not.

To calculate how many streamline refinances would have occurred without the reduced fees, we calculate a counterfactual effective spread under a scenario in which the fees were not reduced and use the estimated regression coefficients to predict how many streamline and external refinances would have occurred. We assume that loans that would have externally refinanced if not for the reduced fees would have received the same payment reduction as they did by streamline refinancing and therefore would have defaulted at the same rates.[32] The results imply that from July 2012 to the end of 2013, the reduced fees caused about 179,000 excess refinances after we account for loans that would have refinanced externally without the policy change.

The reduction in fees will reduce default rates even for those streamline refinances that would have occurred without the policy. For those loans, the policy change resulted in lower annual premiums, but the loans would have experienced an interest rate reduction and a lengthening of the term of the mortgage even without the policy change. We estimate that nearly 190,000 streamline refinances of loans eligible for the reduced fees would have occurred even without the reduced fees.

The final step is to apply the estimated elasticity of defaults with respect to payment reductions from section 5.2 to the total payment reduction that the policy induced. One complication that arises in this step is that the elasticity estimated in section 5 applies to the monthly conditional default rate, whereas we are ultimately interested in the effect on lifetime defaults. Denoting the elasticity of the monthly conditional default rate as θ, Zhu and others (2014) show that the elasticity Θ of the lifetime cumulative default rate F^{chain} can be bounded by $\theta(1 - F^{chain})$ and θ. Another complication arises from the need to apply the elasticity estimated from loans near the discontinuity to a much broader set of loans, most of which experience substantially larger payment reductions. Although those larger payment reductions may be associated with smaller default elasticities, we apply our estimated elasticity uniformly across cohorts.

Call the number of streamline refinances that the program induced M and the remaining eligible streamline refinances N. Let the proportional payment reduction for the induced streamline refinances be ρ_m and the reduction for the others be ρ_n. Then the point estimate

[32] This assumption is conservative in that those borrowers' choice to streamline refinance indicates that the payment reduction from doing so would probably have been larger than the payment reduction from externally refinancing.

of the total number of defaults prevented, Δ, is bounded by:

$$\Delta_{LB} = F^{chain}\theta(1 - F^{chain})(M\rho_m + N\rho_n) \tag{4}$$

$$\Delta_{UB} = F^{chain}\theta(M\rho_m + N\rho_n). \tag{5}$$

The results imply that the reduced fees prevented 35,500–49,400 defaults over the affected loans' lifetimes. Of those totals, 17,900–25,400 are loans that the reduced fees caused to streamline refinance, net of streamline refinances that would otherwise have refinanced externally. Of the prevented defaults, 17,600–24,000 are loans that would have streamline refinanced without the reduced fees but experienced a larger payment reduction because of the policy. Because we assume no effect on defaults for loans that would have refinanced externally without the reduced fees, these results apply to the mortgage finance system as a whole rather than to FHA specifically.

We use the midpoint of the estimated range of prevented defaults to assess the policy's budgetary effects. We estimate that the present value of the prevented default losses is $2.6 billion, whereas the present value of the reduced fee income is $1.7 billion. Therefore, the net savings to FHA have a present value of $900 million.[33] We do not estimate the program's effects on other stakeholders, such as borrowers or investors in mortgage-backed securities. Because the U.S. government owns mortgage-backed securities whose values the policy may have adversely affected, the policy's budgetary effect on FHA does not reflect the policy's total budgetary effect on the U.S. government. An analysis of the full budgetary effect is beyond the scope of this paper. See Remy, Lucas, and Moore (Congressional Budget Office 2011) for a discussion of those issues.

8 Conclusion

This paper offers quasi-experimental evidence that mortgage payment reductions substantially reduce default rates. The estimated effects are larger than those generally found in the literature. Suggestive, though noisy, evidence indicates that the reduced payments exert

[33] We use default, prepayment, and loss given default rates from Castelli and others (Congressional Budget Office 2014) as the basis for the calculations. Interest rates on U.S. Treasury securities in 2013 are used to discount future cash flows to 2013 values, in the spirit of the Federal Credit Reform Act of 1990. As noted in Section 1, the savings would be larger if the cash flows were accounted for on a fair-value basis, which includes an adjustment for the market price of risk. The lower default risk induced by the smaller mortgage payments would reduce the additional compensation in relation to Treasury rates of return that private investors would demand to insure the refinanced loans. Adjusting for that reduction in the market price of risk would generate larger savings to FHA. If we use default, prepayment, and loss given default rates from the 2013 *Actuarial Review* (HUD 2013) instead, the net savings to FHA are estimated to be $580 million.

larger effects on default rates for loans with higher MTM LTV ratios and lower borrower FICO scores. We estimate that the reduction in fees to participate in FHA's streamline refinance program induced nearly 180,000 additional loans to streamline refinance by the end of 2013, which will prevent more than 17,000 defaults over those loans' lifetimes. When accounting for how the reduced fees affect borrowers who would have streamline refinanced without the program, we estimate that the program will prevent more than 35,000 defaults. The results indicate that large-scale refinancing programs could reduce defaults materially in the wake of a severe downturn in the housing market.

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Table 1.
Descriptive Statistics on Mortgage Loan Characteristics

Variable	Mean for Group		
	Endorsed in May	**Endorsed in June**	**Total**
Original Mortgage Amount, Dollars	186,403	186,457	186,433
	(93,379)	(93,583)	(93,492)
Borrower FICO Score	686.9	690.9	689.1
	(57.3)	(56.4)	(56.8)
Mortgage Interest Rate, Percent	5.2	5.1	5.1
	(0.4)	(0.4)	(0.4)
Original Loan-to-Value Ratio, Percent	93.5	93.6	93.5
	(7.6)	(7.5)	(7.5)
Refinance Share, Percent	52.7	50.2	51.3
	(49.9)	(49.9)	(50.0)
Monthly P&I Payment, Dollars	1,016	1,008	1,012
	(506)	(504)	(505)
Annual Borrower Income, Dollars	66,208	66,211	66,209
	(39,054)	(39,929)	(39,540)

Note: Data reflect a total of 315,580 loans, with 141,243 endorsed in May 2009 and 174,337 endorsed in June 2009. Monthly P&I payment is principal and interest payment as calculated by authors. Standard deviations are in parentheses.

Table 2.
Loan Outcomes as of December 1, 2013

Percent

Outcome	Endorsed in May	Endorsed in June	Total
		Proportion of All Loans	
Borrower Remained Active[a]	80.5	79.3	79.8
Paid in Full	13.0	14.9	14.0
Went to Claim	6.4	5.9	6.1
Borrower Remained Active, 90+ Days Delinquent	4.5	4.3	4.4
Original Loan Remained Active	51.2	64.8	58.7
Ever Streamline Refinanced	30.0	14.1	21.2
Active as of June 2012	83.3	85.1	84.3
Streamline Refinanced After June 2012	22.1	7.4	13.9
	Proportion of Loans That Streamline Refinanced July 2012 or Later		
Borrower Remained Active[a]	96.9	97.2	97.0
Paid in Full	3.0	2.7	2.9
Went to Claim	0.07	0.10	0.08
Borrower Remained Active, 90+ Days Delinquent	0.5	0.9	0.6

Note: In both groups of loans, percentages of borrowers who remained active, paid in full, and went to claim sum to 100.
a. "Active" means that the original loan, an internal refinance of the original loan, or a later internal finance remained active.

Table 3.
Characteristics of Streamline Refinances

Variable	Mean for Group		
	Endorsed in May	**Endorsed in June**	**Total**
		Before Refinancing	
Original Mortgage Amount, Dollars	213,894	221,498	216,087
	(91,675)	(92,696)	(92,034)
Borrower FICO Score	702.2	700.9	701.8
	(56.0)	(55.4)	(55.9)
Original Loan-to-Value Ratio, Percent	94.2	94.4	94.2
	(5.3)	(5.4)	(5.4)
Refinance Share, Percent	57.1	62.3	58.6
	(49.5)	(48.5)	(49.3)
Annual Borrower Income, Dollars	73,925	74,169	73,990
	(38,469)	(40,111)	(38,909)
Mortgage Interest Rate, Percent	5.09	5.11	5.10
	(0.30)	(0.29)	(0.30)
Annual FHA Insurance Premium, Percent	0.53	0.54	0.53
	(0.02)	(0.02)	(0.02)
Monthly P&I Payment Plus FHA Insurance Premium, Dollars	1,247	1,296	1,261
	(529)	(538)	(532)
		After Refinancing	
New Mortgage Amount, Dollars	202,216	212,637	205,220
	(87,037)	(89,067)	(87,753)
Mortgage Interest Rate, Percent	3.73	3.58	3.69
	(0.29)	(0.26)	(0.29)
Annual FHA Insurance Premium, Percent	0.55	1.21	0.74
	(0.05)	(0.07)	(0.30)
Monthly P&I Payment Plus FHA Insurance Premium, Dollars	1,026	1,177	1,069
	(440)	(493)	(461)
Reduction in Monthly Payment, Dollars	221	119	192
	(103)	(65)	(104)
Proportional Reduction in Monthly Payment, Percent	17.8	9.3	15.4
	(3.5)	(3.4)	(5.2)

Note: Monthly P&I payment plus FHA insurance premium is principal and interest payment plus FHA's annual insurance premium as calculated by the authors. The payment before refinancing is calculated by using the original mortgage amount for the principal and interest and the new mortgage amount for FHA's annual premium. Standard deviations are in parentheses.

Table 4.
Instrumental Variable Regressions of Conditional Default Rate on Streamline Refinance Status, Loan-Month Level, July 2012–December 2013

	(1)	(2)	(3)	(4)	(5)	(6)
			First Stage			
			Dependent Variable: Streamline Refinance Status			
Endorsement Date Before Cutoff	-0.0355	-0.0537	-0.0689	-0.0642	-0.0646	-0.0555
	(0.0011)	(0.0046)	(0.0050)	(0.0044)	(0.0059)	(0.0040)
			Second Stage (IV)			
			Dependent Variable: Monthly Conditional Default Rate			
Streamline Refinance Status	-0.0026	-0.0025	-0.0028	-0.0031	-0.0024	-0.0024
	(0.0005)	(0.0006)	(0.0007)	(0.0008)	(0.0008)	(0.0008)
Degree of Polynomial in Business Days in Relation to Cutoff	First	Second	Third	Fourth	Second	Second
Sample Restricted to Loans With All Covariates?	No	No	No	No	Yes	Yes
Regression Includes Covariates?	No	No	No	No	No	Yes
Number of Loan-Months	3,780,337	3,780,337	3,780,337	3,780,337	2,873,144	2,873,144
Number of Loans	229,785	229,785	229,785	229,785	174,966	174,966
p-Value of Test of Exogeneity of Streamline Refinancing	.0453	.0666	.1783	.3938	.2007	.1596
First-Stage F-Statistic	751	1,294	1,743	1,087	1,547	1,172
Semielasticity of Defaults With Respect to Streamline Refinancing	-0.6237	-0.6292	-0.665	-0.721	-0.6688	-0.6767
	(0.1312)	(0.1588)	(0.1679)	(0.1825)	(0.2128)	(0.2159)

Note: First stage is regression of loans' streamline refinance status month by month on a fourth-degree polynomial in number of months since July 2012, a polynomial in business days of loan endorsement in relation to the cutoff date, and a dummy indicating whether the endorsement date is before the cutoff date of June 1, 2009. Both polynomials are estimated separately on each side of the cutoff date. Second stage is regression of whether loan defaults in month on refinance status predicted from the first stage. Standard errors clustered at the business day of endorsement reported in parentheses. The six covariates included in the regression in column 6 are original loan-to-value ratio, mortgage interest rate, share of original loans that were refinances, borrower income, borrower FICO score, and original mortgage amount. Endorsement period of original loans is May 1, 2009, through June 30, 2009. Exogeneity test is from Wooldridge (1995).

Table 5.
Instrumental Variable Regressions of Conditional Default Rate on Payment Reduction, Loan-Month Level, July 2012–December 2013

	(1)	(2)	(3)	(4)	(5)	(6)
			First Stage			
			Dependent Variable: Proportional Payment Reduction			
Endorsement Date Before Cutoff	-0.0085	-0.0114	-0.0105	-0.0109	-0.0091	-0.0091
	(0.0007)	(0.0007)	(0.0006)	(0.0009)	(0.0007)	(0.0007)
			Second Stage (IV)			
			Dependent Variable: Monthly Conditional Default Rate			
Proportional Payment Reduction	-0.0014	-0.0111	-0.0121	-0.0134	-0.0111	-0.0112
	(0.0024)	(0.0027)	(0.0030)	(0.0033)	(0.0035)	(0.0035)
Degree of Polynomial in Business Days in Relation to Cutoff	First	Second	Third	Fourth	Second	Second
Sample Restricted to Loans With All Covariates?	No	No	No	No	Yes	Yes
Regression Includes Covariates?	No	No	No	No	No	Yes
Number of Loan-Months	3,780,337	3,780,337	3,780,337	3,780,337	2,873,144	2,873,144
Number of Loans	229,785	229,785	229,785	229,785	174,966	174,966
p-Value of Test of Exogeneity of Streamline Refinancing	.0000	.0001	.0008	.0061	.0089	.0053
First-Stage *F*-Statistic	1,157	2,676	2,483	1,940	2,097	1,687
Elasticity of Defaults With Respect to Streamline Refinancing	-2.7694	-2.7549	-2.9161	-3.1586	-3.0606	-3.0882
	(0.5729)	(0.6842)	(0.7195)	(0.7827)	(0.9623)	(0.9739)

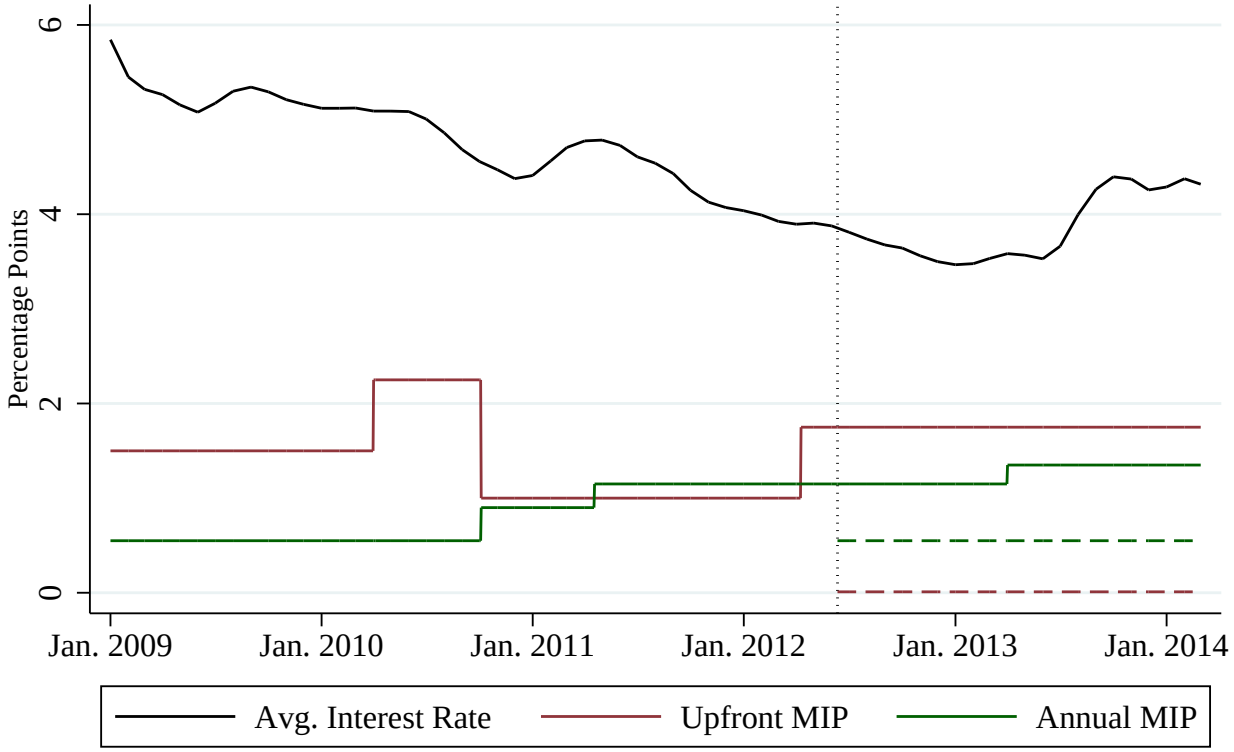
Note: First stage is regression of loans' payment reduction month by month on a fourth-degree polynomial in number of months since July 2012, a polynomial in business days of loan endorsement in relation to the cutoff date, and a dummy indicating whether the endorsement date is before the cutoff date of June 1, 2009. Both polynomials are estimated separately on each side of the cutoff date. Second stage is regression of whether loan defaults in month on payment reduction predicted from the first stage, polynomial in months since July 2012, and polynomial in business days in relation to cutoff date. Standard errors clustered at the business day of endorsement reported in parentheses. The six covariates included in the regression in column 6 are original loan-to-value ratio, mortgage interest rate, share of original loans that were refinances, borrower income, borrower FICO score, and original mortgage amount. Endorsement period of original loans is May 1, 2009, through June 30, 2009. Exogeneity test is from Wooldridge (1995).

Table 6.
Instrumental Variable Regressions of Conditional Default Rate on Payment Reduction for Different Subpopulations, Loan-Month Level, July 2012–December 2013

	(1)	(2)	(3)	(4)	(5)
			First Stage		
			Dependent Variable: Proportional Payment Reduction		
Endorsement Date Before Cutoff	-0.0114	-0.0075	-0.0109	-0.0081	-0.0111
	(0.0007)	(0.0008)	(0.0009)	(0.0009)	(0.0010)
			Second Stage (IV)		
			Dependent Variable: Monthly Conditional Default Rate		
Proportional Payment Reduction	-0.0111	-0.003	-0.0176	-0.0211	-0.0041
	(0.0027)	(0.0066)	(0.0051)	(0.0075)	(0.0033)
Sample	All Observations	MTM LTV Ratio Below Median	MTM LTV Ratio Above Median	FICO Score Below Median	FICO Score Above Median
Degree of Polynomial in Business Days in Relation to Cutoff	Second	Second	Second	Second	Second
Number of Loan-Months	3,780,337	1,454,055	1,489,443	1,487,724	1,485,121
Number of Loans	229,785	89,618	89,621	90,398	90,490
p-Value of Test of Exogeneity of Streamline Refinancing	.0001	.0231	.3031	.3417	.0352
Elasticity of Defaults With Respect to Streamline Refinancing	-2.7549	-0.9122	-4.368	-3.8776	-2.2403
	(0.6842)	(1.9908)	(1.2806)	(1.3748)	(1.8224)
p-Value of Test of Effect Homogeneity		.1142		.0396	

Note: All specifications correspond to column 2 of Table 5 (column 1 is identical to column 2 of Table 5 to facilitate comparison). Standard errors clustered at the business day of endorsement reported in parentheses. The median mark-to-market (MTM) loan-to-value (LTV) ratio as of June 2012 was 98.1 percent. The median borrower FICO score was 687.

Figure 1: FHA Interest Rates and Premiums
January 2009 to March 2014



Note: Dashed lines represent reduced premiums for streamline refinances of loans originally endorsed May 31, 2009 or earlier.
Premiums shown for streamline refinances of loans of $625,000 or less with LTV>95%.

Figure 2: FHA Endorsements by Business Days in Relation to Cutoff
May 1, 2009, to June 30, 2009

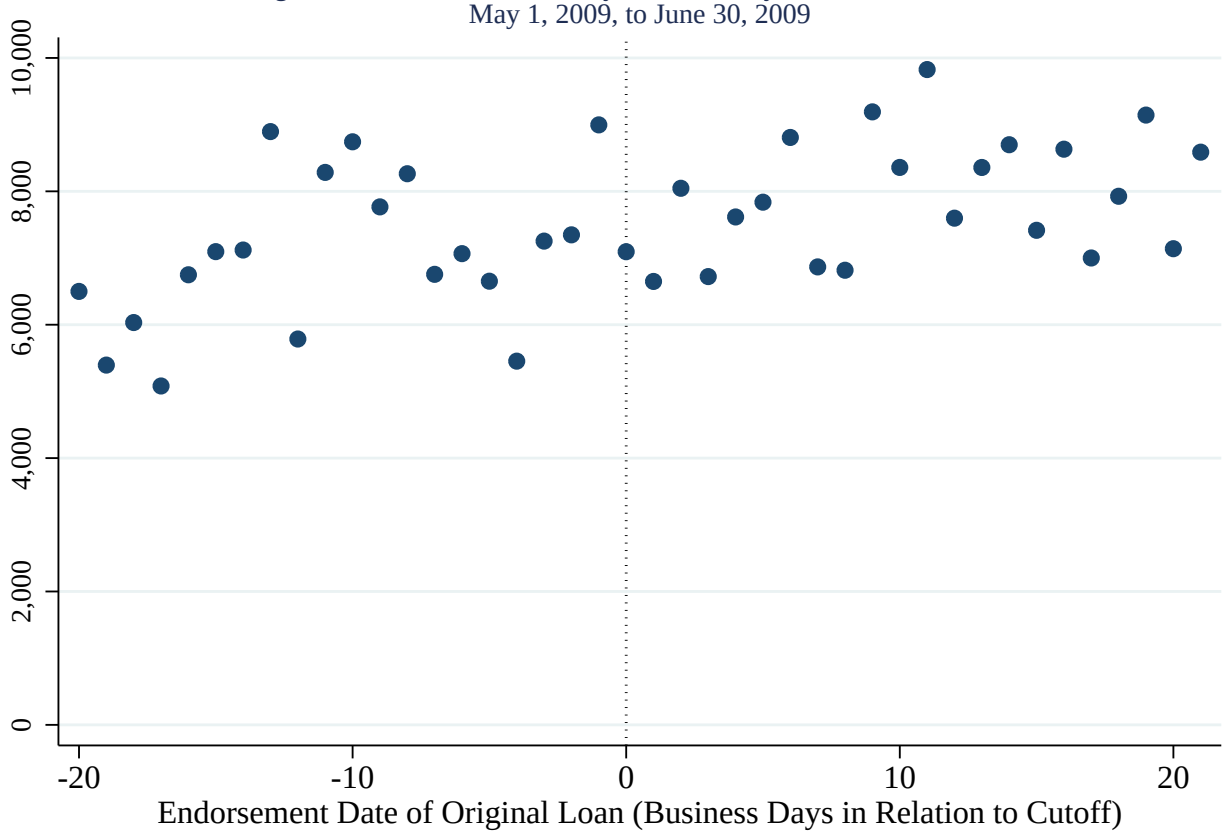
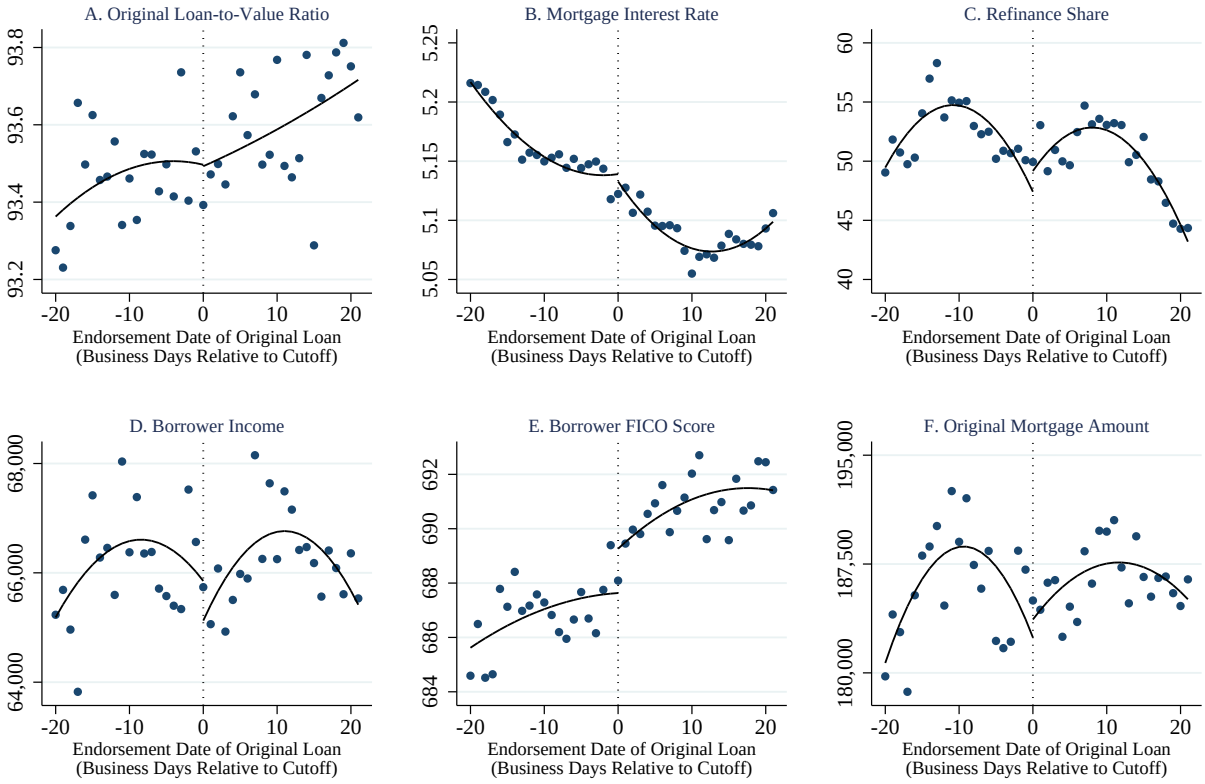
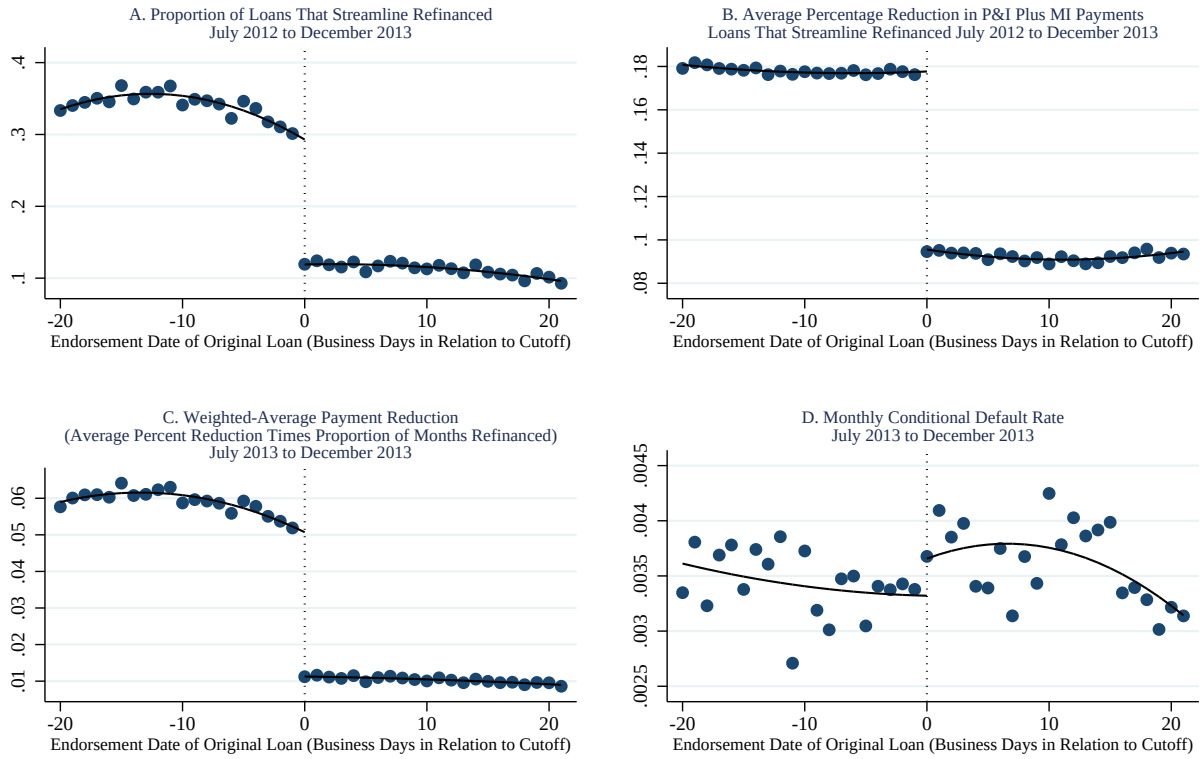


Figure 3: FHA Loan Characteristics, May 1, 2009, to June 30, 2009



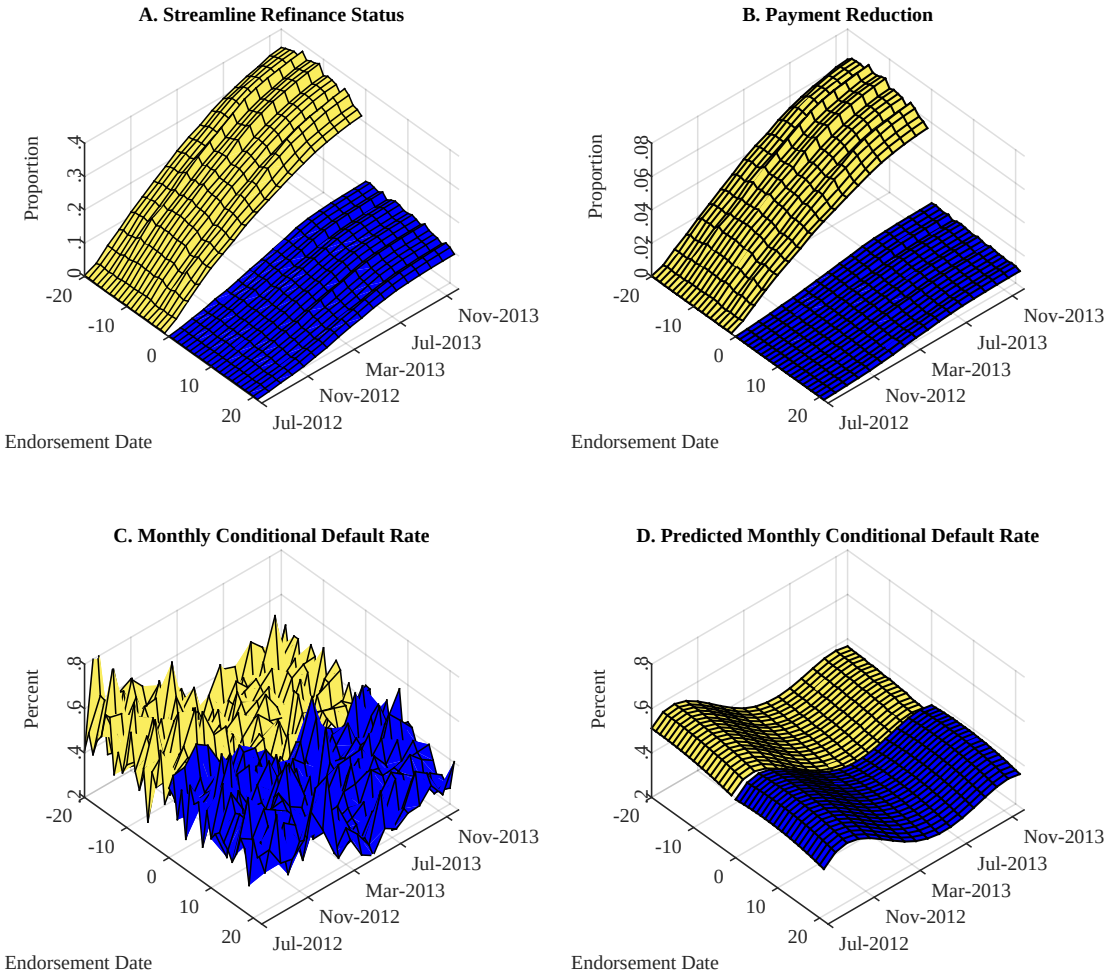
Note: Dots are average values by endorsement date of original loan. Lines are fitted quadratic polynomials.

Figure 4: Average Loan Outcomes by Endorsement Date of Original Loan



Note: Dots are average values by endorsement date of original loan. Lines are fitted quadratic polynomials. P&I abbreviates principal and interest, and MI abbreviates mortgage insurance.

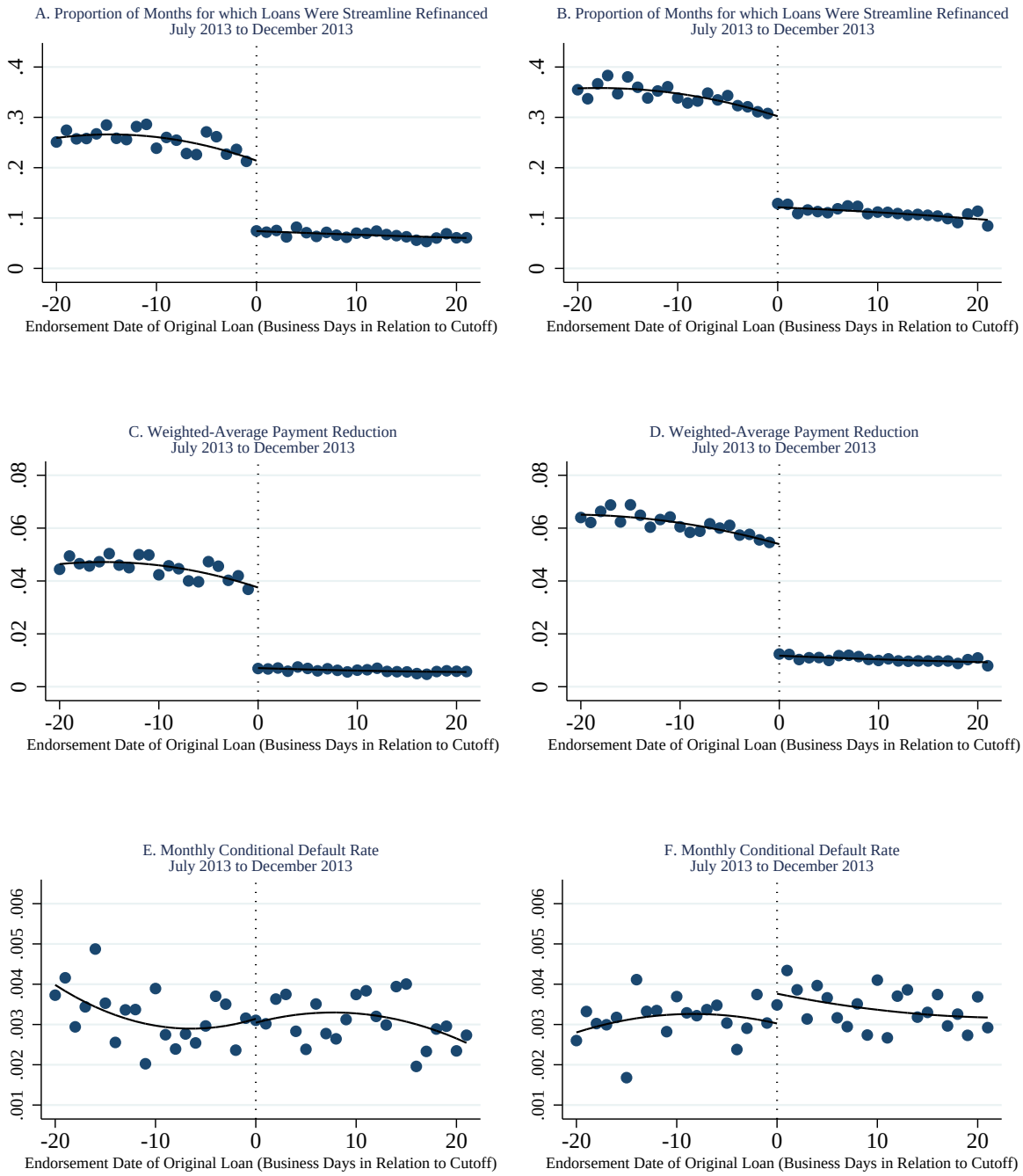
Figure 5: Average Loan Outcomes by Endorsement Date and Calendar Month



Note: Yellow indicates May chains; blue indicates June chains.

Figure 6: Streamline Refinancing, Payment Reductions, and Default Probabilities

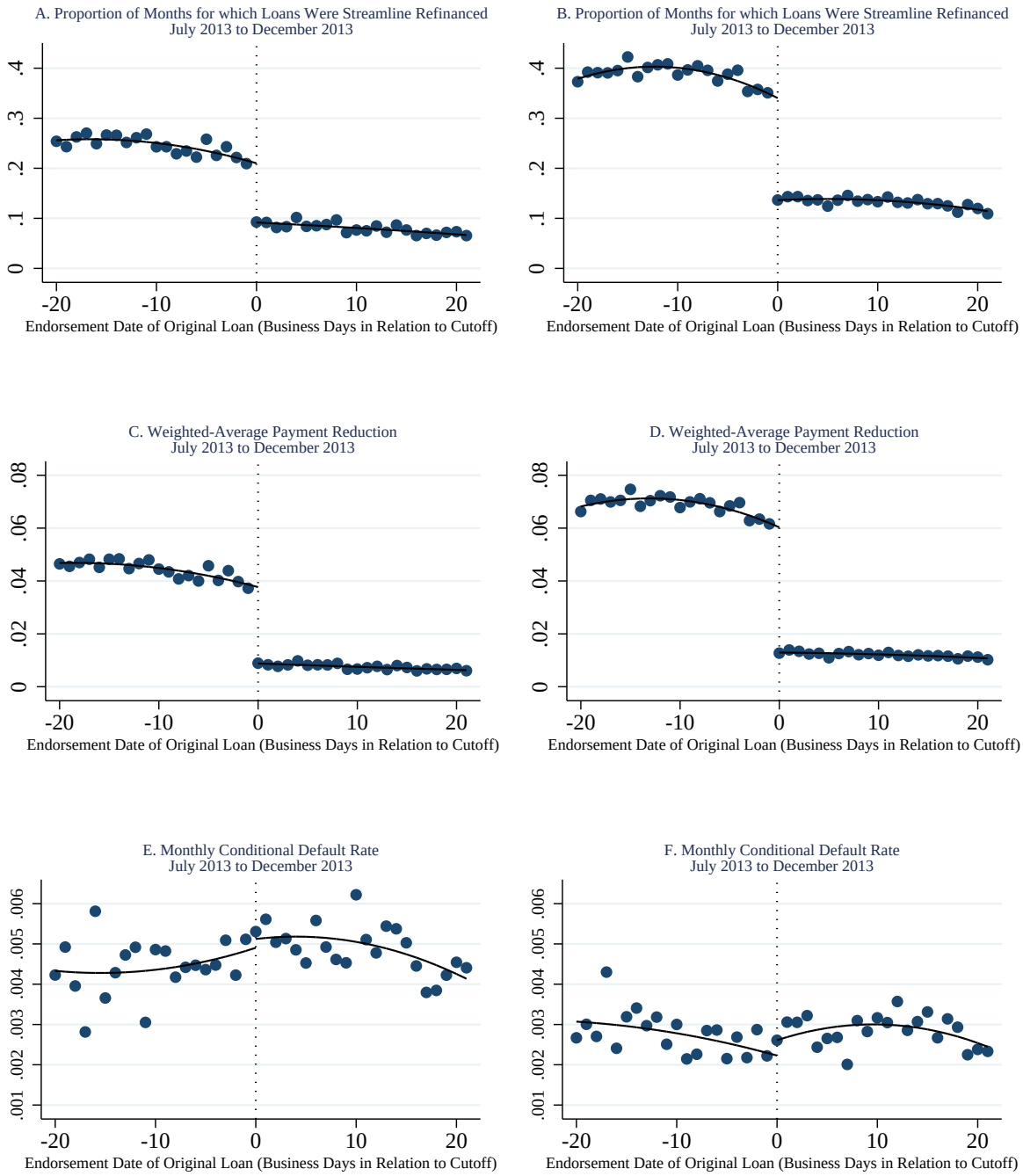
MTM LTV Ratio Below Median | MTM LTV Ratio Above Median



Note: Dots are average values by endorsement date of original loan. Lines are fitted quadratic polynomials. The median mark-to-market (MTM) loan-to-value (LTV) ratio as of June 2012 was 98.1.

Figure 7: Streamline Refinancing, Payment Reductions, and Default Probabilities

FICO Score Below Median | FICO Score Above Median



Note: Dots are average values by endorsement date of original loan. Lines are fitted quadratic polynomials. The median borrower FICO score was 687.

Appendix

Assessing the Validity of the Regression Discontinuity Design

Here we conduct tests of the internal and external validity of the RD design in the spirit of McCrary (2008), Lee (2008), and Bertanha and Imbens (2014). The first test examines whether a discontinuity exists in the number of endorsements around the cutoff date, whereas the second test examines whether any discontinuities occur in the observable borrower and loan characteristics around the cutoff date. Discontinuities of either sort might suggest that borrowers sorted around the cutoff in anticipation of the policy change or for some other reason, which could contradict the assumption of imprecise control that underlies the interpretation of $\hat{\tau}$ as a causal effect. The third test examines whether discontinuities are present in conditional default rates around the cutoff date within the groups of borrowers who did and did not streamline refinance. Such discontinuities could indicate that the local average treatment effect (LATE) we estimate for compliers does not generalize to other subpopulations.

Figure 2 shows the number of endorsements by business day, expressed in terms of business days in relation to the cutoff. June 1, 2009, is defined as day zero, so that business days assigned negative numbers correspond to May endorsements and business days with (weakly) positive numbers correspond to June endorsements. The figure shows clearly that the distribution is continuous at the cutoff, which we confirm with statistical tests. Appendix Figure A1 displays the number of endorsements by day, with weekends and federal holidays shaded in gray.

McCrary (2008) suggests to test for discontinuities in the distribution of endorsements around the cutoff date. We cannot perform McCrary's precise test because our running variable is discrete rather than continuous. Instead, we run regressions of the form:

$$N = h(X) + \beta_T T + u \tag{A.1}$$

where N is the number of endorsements on a particular business day, T is the intent-to-treat indicator defined in equation 3, $h(\cdot)$ is a polynomial function, and u is a random error. We then test the statistical significance of $\hat{\beta}_T$. A positive value for this coefficient may indicate that borrowers sorted to the early side of the cutoff date in anticipation of the eligibility rule. We perform the test for polynomials from degrees one to four.

Table A3 shows the results of those tests of the continuity of endorsement date distribution at the cutoff date. We cannot reject the null hypothesis that $\hat{\beta}_T = 0$ for any polynomial order, with p-values ranging from .45 (for a quadratic polynomial) to .79 (for a quartic polynomial). We view these results as strongly consistent with the null hypothesis that the distribution of endorsement volumes is continuous at the cutoff date.

Lee (2008) suggests testing for discontinuities in the distribution of observable covariates (borrower and loan characteristics) around the cutoff date. Figure 3 shows average

values by endorsement date for six covariates: original loan-to-value ratio, mortgage interest rate, the proportion of loans that were refinances, borrower household income, borrower FICO score, and the original mortgage amount. The figure also shows trends for these variables estimated separately as quadratic polynomials in X on both sides of the cutoff date. To conduct a formal test for discontinuities in these covariates at the cutoff date, we follow Lee and Lemieux (2010) and estimate the following system of seemingly unrelated regressions:

$$\begin{aligned} w_1 &= h_1(X) + \beta_1 T + \varepsilon_1 \\ &\vdots \\ w_6 &= h_6(X) + \beta_6 T + \varepsilon_6 \end{aligned} \tag{A.2}$$

where w_1,...,w_6 represent the business-day averages of each of the six covariates, T represents the intent-to-treat dummy, and $h_1(\cdot)$,...,$h_6(\cdot)$ represent polynomial functions. We again consider polynomials of orders one through four estimated separately on each side of the cutoff and impose that the polynomials all have the same order in each system of equations. Because the unit of observation in these equations is the business day rather than the loan, we weight each observation by the number of endorsements in that day.

We test for discontinuities in the covariates at the cutoff date by testing the hypothesis that $\beta_1 = \ldots = \beta_6 = 0$. Table A3 lists the results of F-tests of that hypothesis.[34] For the first-degree polynomial, the null hypothesis of no discontinuities in the covariates at the cutoff date is rejected at the 5 percent confidence level, but the null hypothesis is not rejected at the 5 percent or 10 percent confidence level for the other polynomial orders. We take these results as consistent with the null hypothesis that no discontinuities exist in the distributions of the covariates around the cutoff date.

The linear model in equations (1) and (2) may result in inadmissible predicted values for the probability of streamline refinancing, the conditional default probability, and payment reductions. The true probability of streamline refinancing and conditional default probability must be bounded between zero and one, whereas payment reductions are bounded below by zero—restrictions that a linear model may not respect. A linear model may nonetheless yield a good approximation to the true data-generating process and has compensating virtues that others have discussed elsewhere (for example, Angrist [2001] and Angrist and Pischke [2009]). However, many predicted values that fall outside the admissible range could indicate that a linear model poorly approximates the underlying data generating process.

Figure A2 shows the predicted values for probability of streamline refinancing, conditional default probability, and payment reduction from our preferred specifications in Tables 4 and 5. Panels A and B show the predicted probability of streamline refinancing and conditional default probabilities, respectively, from column 2 of Table 4. Although 5.7 percent

[34] Because we have only 42 observations (business days), we conduct F-tests rather than χ^2-tests. We also apply a small-sample adjustment to the estimated covariance matrix of the residuals, dividing by $n - k$, where n is the sample size and k is the numbers of parameters per equation, instead of dividing by n. The test results do not change at the 5 percent confidence level.

of predicted probabilities of streamline refinancing are less than zero, no predicted conditional default probabilities were less than zero. Panels C and D show the predicted payment reduction and conditional default probability, respectively, from column 2 of Table 5; 7.2 percent of predicted payment reductions were in the inadmissible range, whereas less than 0.1 percent of the predicted conditional default probabilities were less than zero. Overall, we take these results to indicate that the linear models used in the main analysis generally predict values consistent with the limited values that the predicted variables can take.

Figure A3 shows the distributions of mark-to-market loan-to-value ratios and borrower FICO scores. The median mark-to-market loan-to-value ratio is 98.1 percent, with an average of 98.8 percent and a standard deviation of 11.0 percent. The median borrower FICO score is 687, with an average of 692 and a standard deviation of 56.

Figure A4 shows graphs of monthly conditional default rates conditional on whether a borrower had refinanced by December 2013. The top panel shows the conditional default rate for borrowers who did not refinance, whereas the bottom panel shows the conditional default rate for borrowers who did refinance. Bertanha and Imbens (2014) argue that discontinuities in these graphs at the cutoff date would indicate that the estimated LATE for compliers from the fuzzy regression discontinuity design is unlikely to be externally valid for the subpopulations of noncompliers. Bertanha and Imbens's (2014) setting is not strictly comparable to ours, because the reduction in annual premiums for the May chains that streamline refinanced is larger than the reduction for the June chains. Therefore, the "treatment intensity" differs meaningfully for loans with endorsement dates on each side of the cutoff, in contrast to the setup in Bertanha and Imbens (2014), which has no notion of treatment intensity. A formal test of the joint null hypothesis of no discontinuities in conditional default rates at the cutoff date in either plot has a p-value of .28, leading us not to reject the null hypothesis, so we cannot reject the null hypothesis that the LATE for compliers is valid for other subpopulations.

Table A1.
Literature Review

Study	Data Set	Approach	Results
		Approach 1: Study Private Loan Modifications	
Adelino, Gerardi, and Willen	Lender Processing Services data (approximately 60 percent of U.S. mortgage market); focus on mortgages delinquent by at least 60 days	Compare performance between loans that receive payment reductions as part of modification and loans that do not	42 percent of loans that receive "concessionary" modifications featuring payment reductions redefault within six months, compared with 49 percent of all loans receiving modifications
Haughwout, Okah, and Tracy (2010)	FirstAmerican CoreLogic Loan Performance ABS data; securitized subprime loans	Compare redefault probabilities among mortgages that receive different types of modifications	A 10 percent payment reduction reduces probability of redefault within one year by 13 percent. Payment reductions achieved by reducing the principal balance of the mortgage are substantially more effective than those achieved solely by reducing mortgage interest rate
Agarwal and others (2010)	OCC/OTS Mortgage Metrics data (approximately 64 percent of U.S. mortgage market); focus on "troubled" mortgages	Compare redefault probabilities among mortgages that receive different types of modifications	A 10 percent payment reduction reduces probability of redefault within one year by 3 percentage points from the baseline redefault probability of 40 percent
		Approach 2: Study Home Affordable Refinancing Program (HARP) Refinances	
Zhu (2012)	Freddie Mac data	Compare performance between loans that participate in HARP and those that do not. Use propensity score model to control for selection into HARP	HARP refinancing lowers default probability by 54 percent
Zhu (2014)	Freddie Mac data	Compare performance between loans that participate in HARP and those that do not. Control for observable borrower and loan characteristics	HARP refinancing lowers default probability by 53 percent
Zhu and others (2014)	Freddie Mac data	Compare performance between loans that participate in HARP and those that do not. Use inverse probability weighting approach to control for selection into HARP	A 10 percent payment reduction reduces monthly default probability by 10 percent to 11 percent
		Approach 3: Study Payment Reductions Arising From Prespecified Loan Terms	
Amromin and others (2013)	Lender Processing Services data; focus on "complex mortgages" such as interest-only and negative amortization mortgages	Compare performance between complex and noncomplex mortgages, and examine performance of mortgages that experience payment changes due to contractual terms such as the end of interest-only or negative amortization period	A 38 percent increase in monthly payment increases default probability by 23 percent
Tracy and Wright (2012)	Lender Processing Services data; focus on prime, first-lien, owner-occupied adjustable rate mortgages (ARMs) held by Fannie Mae or Freddie Mac	Compare performance between loans that experience downward interest rate adjustments and loans that do not	A 10 percent reduction in monthly payment reduces default probability by 17 percent to 22 percent
Fuster and Willen (2015)	CoreLogic Loan Performance data; focus on Alt-A, interest-only subhprime ARMs	Compare performance between loans that experience downward interest rate adjustments and loans that do not	A 50 percent reduction in monthly payment reduces default probability by 55 percent

Table A2.
Geographical Distribution of Loans

Percent

State	Endorsed May	Endorsed June	Combined
California	9.5	9.7	9.6
Texas	6.8	6.9	6.8
Georgia	4.3	4.0	4.1
Illinois	4.0	4.0	4.0
Florida	3.9	4.1	4.0
Ohio	3.9	3.7	3.8
Pennsylvania	3.7	3.5	3.6
Arizona	3.5	3.5	3.5
Virginia	3.3	3.4	3.3
Colorado	3.3	3.3	3.3
All Others	53.8	53.9	53.9

Table A3.
Validity of Regression Discontinuity Design

	(1)	(2)	(3)	(4)
	Tests of Continuity of Endorsement Date Distribution at Cutoff Date			
	Dependent Variable: Number of Endorsements per Day			
p-Value of *F*-Test of Coefficient on Dummy for Endorsement Date Before Cutoff	.518	.451	.630	.792
	Tests of Continuity of Covariate Distributions at Cutoff Date			
	Dependent Variable: Daily Average Covariate Values—Seemingly Unrelated Regression			
p-Value of *F*-Test of Coefficient on Dummy for Endorsement Date Before Cutoff	.001	.221	.178	.108
Degree of Polynomial in Business Days in Relation to Cutoff	First	Second	Third	Fourth
Number of Business Days in Endorsement Period	42	42	42	42

Note: "Tests of Continuity of Endorsement Date Distribution at Cutoff Date" shows results from regressions of number of endorsements per business day on a dummy indicating whether the endorsement date is before the cutoff date of June 1, 2009, and a polynomial in business days in relation to the cutoff date of the order specified. "Tests of Continuity of Covariate Distributions at Cutoff Date" shows results from a seemingly unrelated regression of the daily average values of six covariates on the same cutoff dummy and business day polynomial. The polynomial is estimated separately on each side of the cutoff. The six covariates are original loan-to-value ratio, mortgage interest rate, share of original loans that were refinances, borrower income, borrower FICO score, and original mortgage amount. Endorsement period of original loans is May 1, 2009, to June 30, 2009.

Figure A1: FHA Endorsements by Calendar Day, May 1, 2009, to June 30, 2009

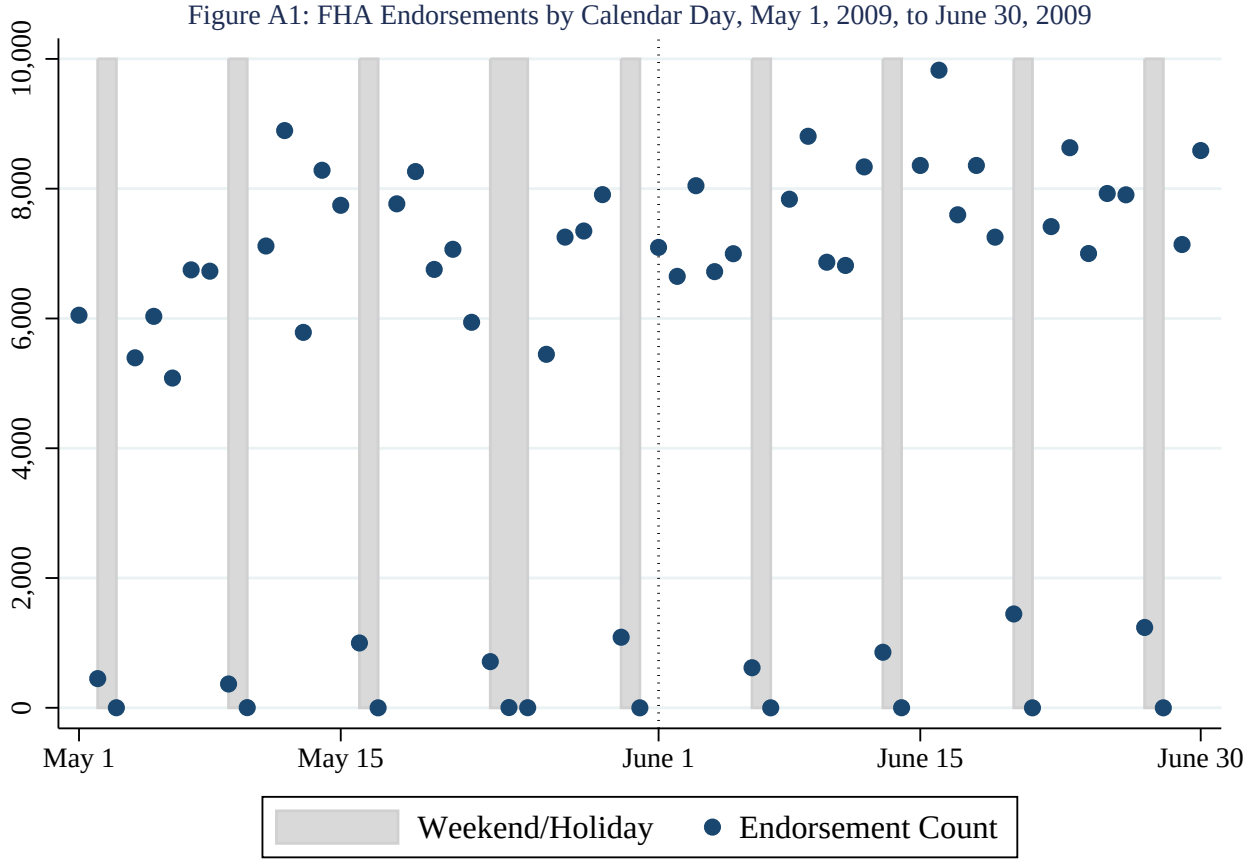


Figure A2: First- and Second-Stage Predictions From Tables 4 and 5

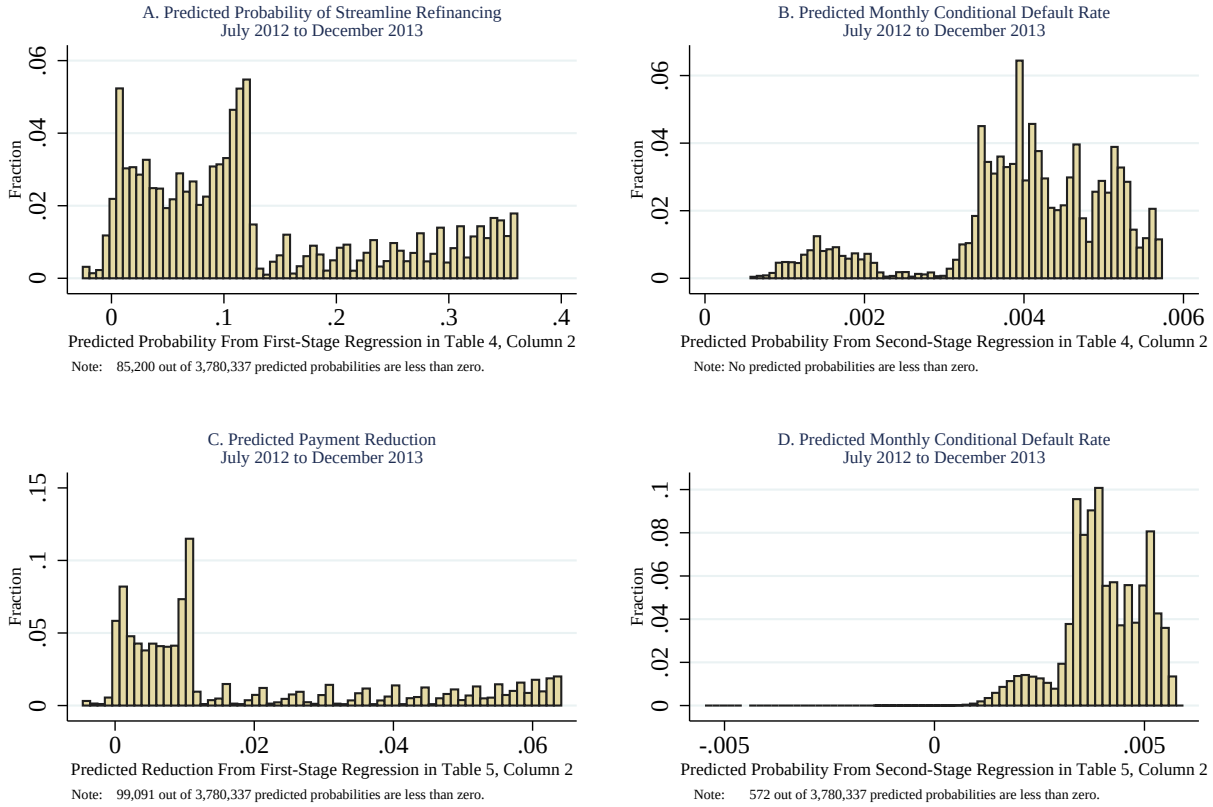
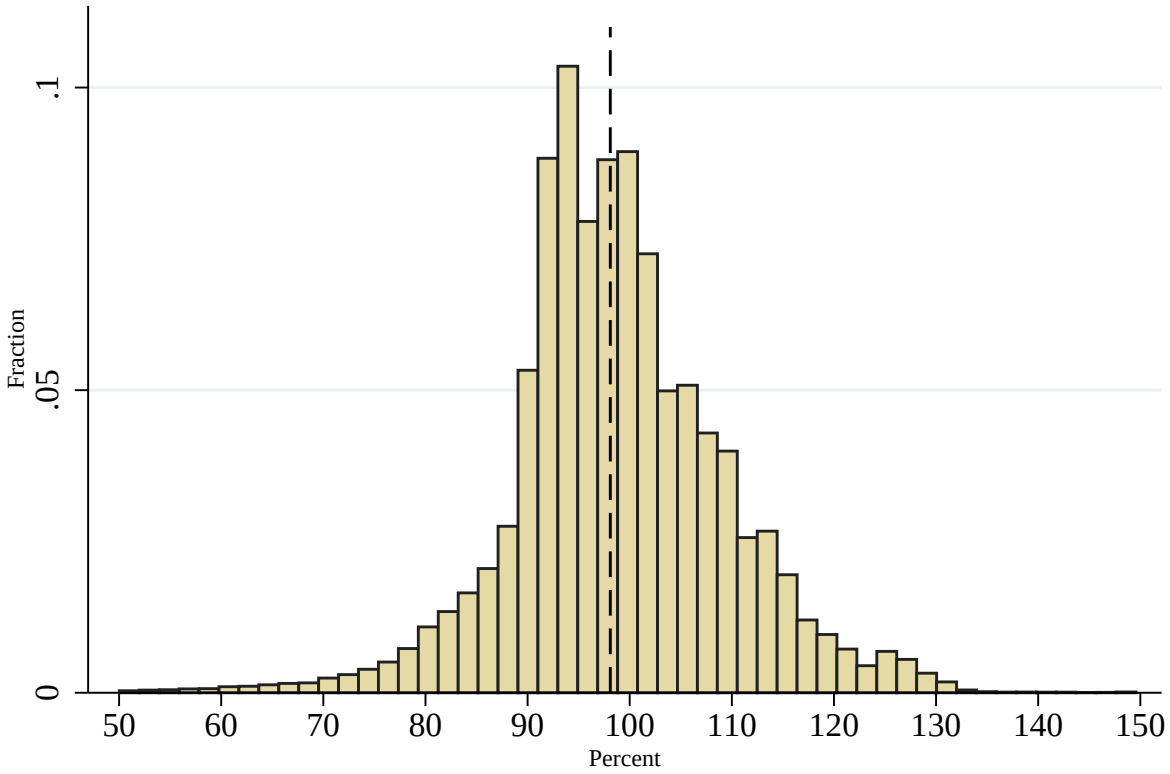


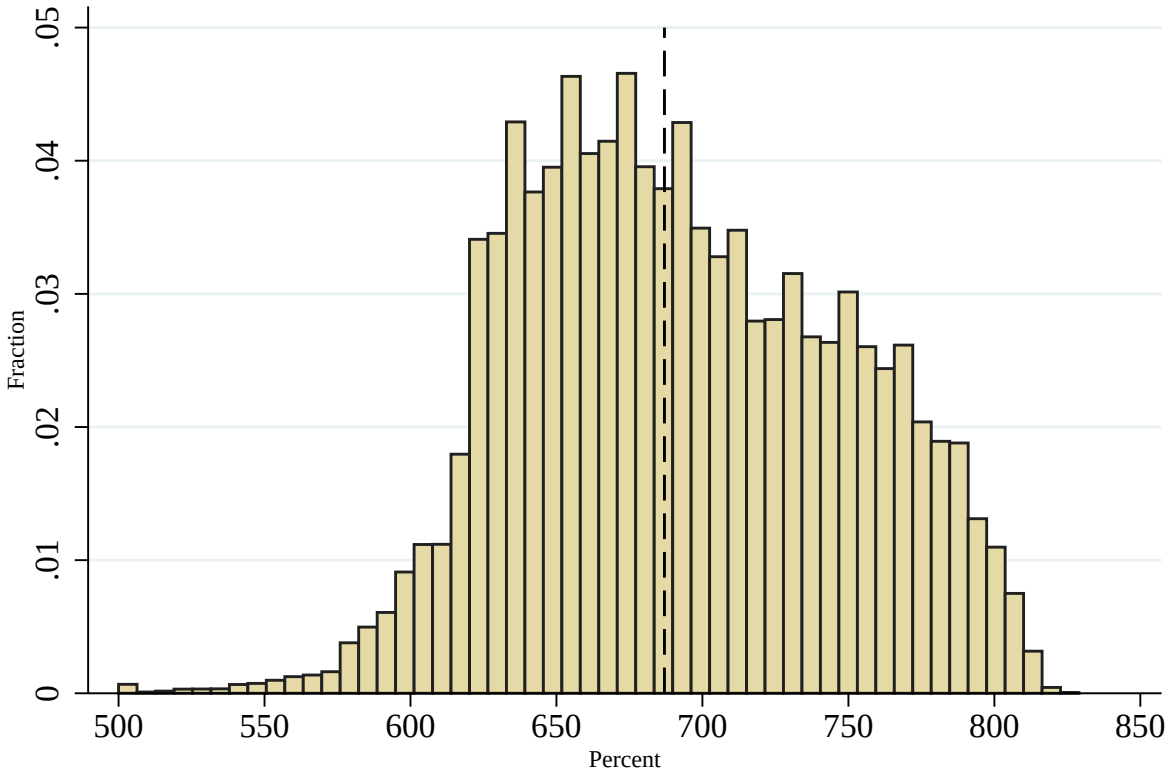
Figure A3: Mark-to-Market LTV Ratio and Borrower FICO Score Distributions

A. Mark-to-Market LTV Ratios as of June 1, 2012



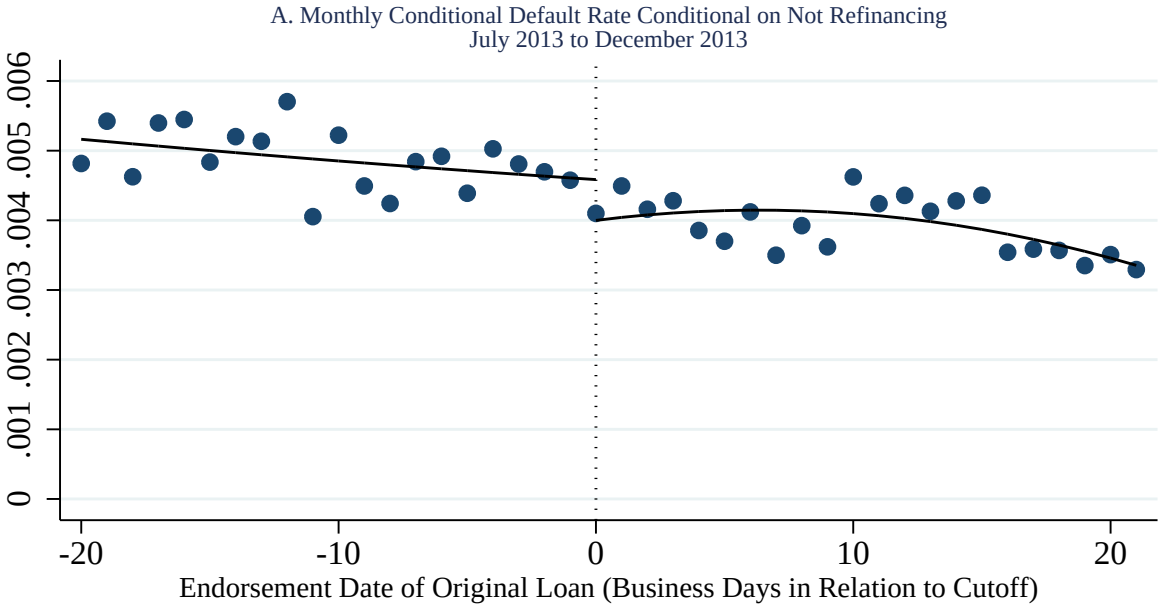
Dashed line shows median mark-to-market loan-to-value (LTV) ratio of 98.1 percent.

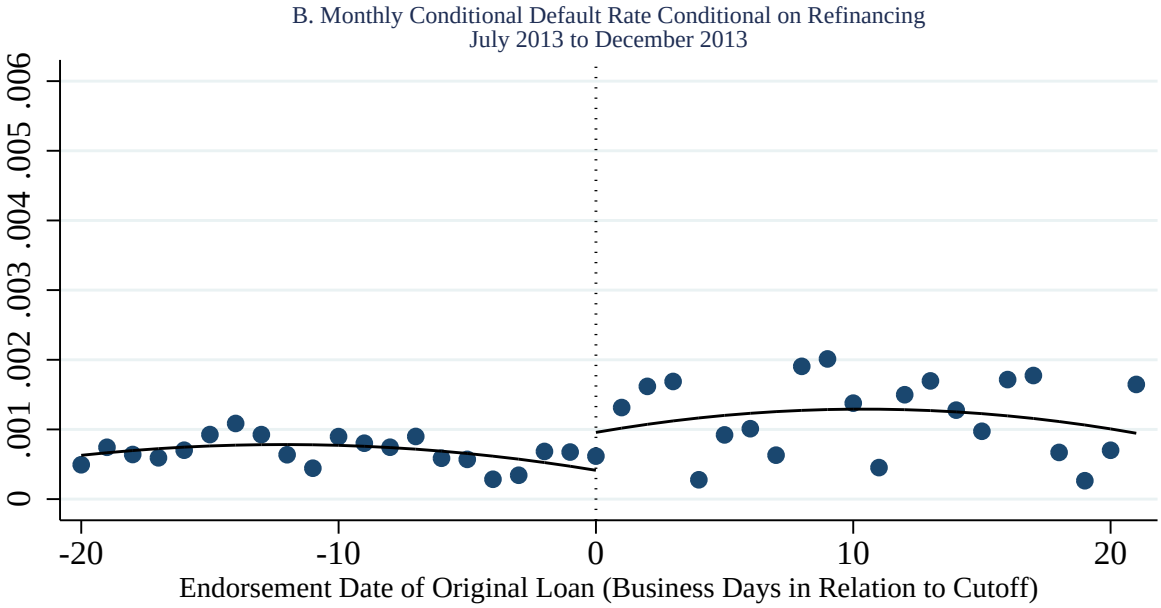
B. Borrower FICO Scores



Dashed line shows median borrower FICO score of 687.

Figure A4: Bertanha and Imbens (2014) Test of External Validity





Note: The *p*-value of the joint test of discontinuity in both refinanced and non-refinanced loans is .28.