# 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 this 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 1.55 percent. We estimate that the policy will prevent nearly 35,000 defaults of FHA-insured mortgages, at a present-value cost to the agency of $8,645 per prevented default.

**JEL Codes**: G18, G21, E65, H50

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 would 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 very 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 greatly increased the number of SLRs between July 2012 and July 2016. We estimate that the reduction in fees will prevent nearly 35,000 defaults of FHA-insured mortgages, at a cost to FHA of $8,645 per prevented default.

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. Reducing default losses was one of FHA's stated goals in announcing reduced fees on SLRs (Department of Housing and Urban Development, 2012, hereafter HUD).

Second, defaults and subsequent foreclosures not only have adverse spillover effects on nearby properties and neighborhoods but also entail significant deadweight losses. 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. Ellen et al. (2013) estimate that an additional foreclosure leads to a 1 percent increase in crime in the block of the foreclosed property. HUD in 2010 estimated the deadweight loss associated with a foreclosure to be approximately $50,000 (Department of Housing and Urban Development, 2010), including transaction costs, such as legal fees, of nearly $12,800, avoidable depreciation of the foreclosed home of $13,500, neighborhood externalities of $14,500, and household expenses, such as moving costs,

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

of $10,300.

Third, defaults and foreclosures may reduce real economic activity by reducing the value of households' assets, in turn reducing consumer demand. For instance, Mian et al. (2015) 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.

Policymakers may also be interested in reducing borrowers' payments as a way to bolster consumer demand following a recession. Eberly and Krishnamurthy (2014) argue that reducing borrowers' monthly payments in the aftermath of a severe downturn in the housing market is a more efficient use of the government's resources than reducing the principal balances on borrowers' mortgages, both in terms of preventing foreclosures and in terms of supporting consumer spending. Keys et al. (2014) and Di Maggio et al. (2014) both estimate that households experiencing payment reductions from downward adjustments in adjustable-rate mortgage interest rates significantly increase their spending on automobile purchases.

Despite the clear policy relevance of the topic, according to Fuster and Willen (forthcoming), "Surprisingly little is known about the importance of mortgage payment size for default." The major obstacle to estimating how mortgage payment size influences borrower behavior has historically been the lack of random changes in the size of mortgage payments. 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." 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 mortgage payment reductions on subsequent default behavior. We exploit the variation in the propensity to streamline refinance provided by the discontinuous eligibility rules for reduced premiums. Those differences were substantial: borrowers with mortgages endorsed in the months prior to the cutoff saved an average of \$224 per month, or 18.8 percent, by refinancing, versus an average of \$137 per month, or 11.2 percent, for borrowers with mortgages endorsed in the months after the cutoff. In contrast to much of the previous literature, the difference in eligibility was driven by a policy decision of the mortgage insurer, FHA, rather than by prespecified mortgage terms or by the decisions of economic actors closer to the default decision, such as the borrower, mortgage lender, or mortgage servicer. Eligibility for reduced fees had a large effect on the rate at which borrowers streamline refinance. Forty percent of eligible loans endorsed in the months before the cutoff date had streamline refinanced by July 2016, versus only 15 percent of loans endorsed in the months after the cutoff date.

We estimate the causal effect of payment reductions on default rates using a fuzzy RD design. The basic logic of the design is that loans endorsed on opposite sides of, but close to, the cutoff date for eligibility for reduced fees should have exhibited similar default rates in the absence of the policy change. Moreover, those loans' different propensities to streamline refinance should be unrelated to loan characteristics other than eligibility for the reduced fees. Comparing the size of the discontinuity in default rates at the cutoff date to the size of the discontinuity in refinancing probabilities or mortgage payment reductions thus provides an estimate of the causal effect of those treatments on default rates.

Our preferred specifications imply that refinancing reduces the monthly conditional default rate by 40 percent and that the elasticity of the conditional default rate with respect to payment size is $-1.55$. We estimate that the lower fees will cost FHA \$1.8 billion in income, while the reduced defaults will save FHA \$1.5 billion in losses, for a net budgetary cost of \$300 million.[2] The effects we estimate are well within the range of estimates elsewhere in the literature, adding quasi-experimental evidence to a literature that has generally found large-scale refinancing programs to be an effective means of reducing mortgage defaults.

[2]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.

# I Previous Literature

Recent studies of the effects of mortgage payment reductions have generally taken three approaches:

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.

We review the previous literature briefly here and provide a summary of the studies we discuss in Appendix Table A1.

Using approach 1, Adelino et al. (2013) find that receiving a "concessionary" loan modification reduces the probability that a previously delinquent loan will re-default within six months by 14 percent. Haughwout et al. (2016) find that a 10 percent reduction in monthly payment reduces the probability of redefault in the next year by 13 percent, corresponding to an elasticity of defaults with respect to payment reductions of $-1.3$. Agarwal et al. (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, an elasticity of $-0.75$.

Approach 2 typically requires modeling the selection of borrowers into HARP, because participation in the program is not random. Using a propensity score model to predict borrowers' selection into HARP, Zhu (2012) finds that a HARP refinance lowers default probabilities by 54 percent over a 15-month period. Zhu et al. (2015) control for borrower selection into HARP using inverse probability weighting, and find that a 10 percent payment reduction reduces monthly default probabilities by 10 to 11 percent. Agarwal et al. (2015) use an instrumental variable strategy to account for selection into HARP, and estimate that a reduction of 15 basis points in average mortgage interest rates reduces zipcode-level foreclosure rates by one basis point. Karamon et al. (2016) use a fuzzy RDD that builds on the design in an earlier version of this paper to estimate the causal effect of HARP refinancing on default rates, and estimate that it reduces monthly conditional default rates by 48 to 62 percent.

Using approach 3, Tracy and Wright (2012) study a sample of prime adjustable rate mortgages (ARMs) that experienced downward interest rate adjustments. They estimate that a 10 percent reduction in monthly payments reduces conditional default rates by 17 to 22 percent, an elasticity of $-1.7$ to $-2.2$. Fuster and Willen (forthcoming) compare nonprime, hybrid, interest-only ARMs that have different initial terms over which the interest rate is fixed.[3] They estimate that a payment reduction of 50 percent lowers monthly default probabilities by about 55 percent, an elasticity of $-1.1$. Amromin et al. (2013) examine "complex mortgages" that initially feature zero or negative amortization but later reset into amortizing payments, and estimate that a payment increase of 38 percent raises the probability of default by 23 percent, an elasticity of $-0.6$. Keys et al. (2014) also compare hybrid ARMs with different periods of fixed interest rates, and find that a twenty percent payment reduction reduces 60-day mortgage delinquencies by roughly 40 percent two years after the reduction, an elasticity of $-2.0$.

# II 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.[4] 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 provides the natural experiment we study in this paper.

[3] A hybrid ARM begins with a fixed interest rate for a certain period, after which the rate becomes adjustable.

[4] See HUD Handbook 4155.1.6.C (Department of Housing and Urban Development, 2011) for program eligibility requirements.

## II.A Program Description

FHA requires neither an updated appraisal on the mortgaged property nor a credit report as part of an SLR.[5] In April 2011, FHA stopped requiring lenders to verify borrowers' employment and income on SLRs. 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.[6]

For SLRs and other mortgages, borrowers must pay both an up-front mortgage insurance premium (MIP) and an annual MIP in exchange for FHA insurance. Figure 1 shows how FHA's MIPs have evolved over time, along with the average interest rates on FHA-insured mortgages. FHA raised its insurance premiums substantially between 2009 and 2012. 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 (HUD 2014).[7] By June 2012, 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, where it stayed until January 2015, at which point it declined to 85 basis points.

[5]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.

[6]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.

[7]Unless otherwise noted, all MIPs apply to 30-year FRMs with a loan amount less than $625,000 and an LTV ratio greater than 95 percent.

## II.B Policy Change

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, shown in the dark dashed line in Fiugre 1; the reduced annual MIP, shown in the light dashed line, 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 announcement of the fee reduction, FHA estimated that streamline refinancing could save the average eligible borrower approximately $3,000 per year, including both the expected reduction in the mortgage interest rate and the savings from reduced fees. We calculate that loans originally endorsed from January to May 2009 had the potential to save an average of $2,742 in the upfront premium and $946 per year in annual premiums solely from the fee reduction.[8] We calculate that the net present value to the borrower of streamline refinancing a loan that had an original principal balance of $200,000, an original loan-to-value ratio of 97 percent, an original interest rate of 5 percent, and a refinanced interest rate of 4 percent was $12,150 if the borrower was eligible for the reduced fees.[9] In contrast, the net present value of streamline refinancing the same mortgage if the borrower was ineligible for the reduced fees was $3,547. Therefore, borrowers whose original mortgages were endorsed on opposite sides of the cutoff faced very different incentives to streamline refinance.[10]

The volume of streamline refinancing nearly doubled after the fees were reduced, from roughly 22,000 per month in the months before the reduction to 41,000 per month in the months afterward. Figure 2 shows the cumulative internal refinance rate for loans originally endorsed within 14 business days prior to and after the cut-

[8] Eligible borrowers actually saved an average of $1,120 per year in annual premiums, as borrowers with larger potential savings were more likely to refinance.

[9] Those calculations assume prepayment rates from Castelli et al. (2014).

[10] The savings from refinancing realized by borrowers on opposite sides of the cutoff date were more similar than these calculations imply because borrowers selected into refinancing based on their potential savings.

off date of May 31, 2009.[11] The refinance rates for the two groups of loans tracked each other fairly closely through June 2012. The pace of streamline refinancing accelerated sharply after June 2012 for the group of loans endorsed shortly prior to the cutoff date. In contrast, the pace of streamline refinancing did not accelerate meaningfully after June 2012 for the group of loans endorsed shortly after the cutoff. By July 2016, 36 percent of loans endorsed shortly before the cutoff had internally refinanced, compared to only 20 percent of loans endoresed shortly afterward. The difference in refinance rates between the two groups provides the first stage of our fuzzy RD design.

An interesting question is to what extent the substantial differences in refinance rates for eligible and ineligible borrowers reflect rational, full-information responses to different incentives, rather than differences in information sets or possible behavioral biases. Keys et al. (2016) estimate that 20 percent of unconstrained households with outstanding mortgages had failed to refinance in 2010 when it would have been advantageous to do so, foregoing an average present-value savings of $11,500. One obstacle to assessing the rationality of borrowers' responses to the fee reduction we study is the difficulty of obtaining reliable data on the closing costs charged by lenders for FHA refinances. Closing costs of even $2,000 would reduce the average present-value savings of streamline refinancing for ineligible borrowers by 56 percent, versus only 16 percent for eligible borrowers. Therefore, we are inclined to view the observed response to the reduced fees as consistent with rational and informed decision-making.

# III Data

This paper uses a loan-level data set that FHA generated in August 2016. The data set contains the universe of FHA-guaranteed loans in the credit subsidy cohorts 2009 to 2015, as well as the loans guaranteed in the 2016 cohort for which data

[11] The overwhelming majority of refinances internal to FHA were streamline refinances, but we consider non-streamline refinances within FHA along with streamline refinances throughout the analysis because non-streamline refinances could also reduce monthly payments.

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, such as the original LTV ratio, annual effective income, borrower credit score, and the state and metropolitan statistical area where the mortgaged property 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-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, but we also consider 60-day delinquency episodes, and 90-day episodes that eventually result in a claim against FHA.[13] Because there is a slight delay in the reporting of delinquency episodes, we censor our study in July 2016.[14]

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

[13]Tracy and Wright (2012) and Haughwout et al. (2016) also adopted the 90-day delinquency definition. Fuster and Willen (forthcoming) and Agarwal et al. (2010) adopted a 60-day delinquency threshold.

[14]We restricted our initial sample to loans endorsed within five months of the cutoff date, or January through October 2009. From those, we dropped non-fixed rate mortgages, loans with terms other than 30 years, loans for which the mortgaged properties were located in the Virgin Islands or Guam, loans with a termination code of "Cancellation," loans with duplicate case numbers, and loans with beginning amortization dates before December 2008 or after November 2009.

## III.A Descriptive Statistics

Table 1 displays descriptive statistics for the sample. The first column shows loans endorsed within 14 business days prior to May 31, 2009, the second column shows loans endorsed within 14 business days after May 31, 2009, the third column shows loans endorsed from January to May, 2009, the fourth column shows loans endorsed from June to October, 2009, and the fifth column shows the entire sample.

A few general features stand out from comparing columns 1 through 4. First, the characteristics of loans endorsed between 14 business days before and after the cutoff, in columns 1 and 2, are quite similar. We assess this claim formally in section IV.A. Second, there are some systematic differences between loans endorsed from January through May and loans endorsed from June through October. Loans endorsed prior to the cutoff have lower average borrower FICO scores, are more likely to have been refinances of previous FHA loans, and are less likely to remain active in July 2012 than loans endorsed after the cutoff. The lower credit quality of loans endorsed prior to the cutoff and the greater proportion of refinances stem from the increasing stress in other segments of the mortgage market over the course of 2009, which drove some borrowers who would previously have obtained mortgage credit through other channels to acquire FHA-insured mortgages instead.

We define a *loan chain* as an originally endorsed loan and any later internal refinances; Table 2 summarizes the outcomes of loan chains as of July 1, 2016 for loans that remained active as of July 2012. The table shows that loans endorsed after the cutoff date were substantially more likely to have repaid in full, probably due to higher rates of external refinancing. The gap in prepayment rates was 9 percentage points in the full sample and 5.4 percentage points for loans endorsed within 14 business days of the cutoff. Those loans were unable to default against FHA after that point, although loans that refinanced to non-FHA mortgages may still have defaulted. Rows 3 through 5 display the proportions of loan chains that are ever 60 days delinquent, 90 days delinquent, and that ever result in a claim against FHA. Very similar proportions of loan chains endorsed within 14 days before and 14 days after the cutoff defaulted, despite the lower prepayment rates of the loans endorsed prior to the cutoff. We attribute this seemingly puzzling similarity to differences in streamline refinancing. Indeed, mortgage default and prepayment are often described as "competing risks" when modeling mortgage behavior, as in Deng et al. (2000).

Table 3 shows the characteristics of loan chains before and after internally refinancing. Average mortgage amounts after refinancing are higher than the amortized balance prior to refinancing, reflecting the common practice of financing the upfront insurance premium into the refinanced mortgage.[15] Borrowers with mortgages endorsed prior to the cutoff experienced average monthly payment reductions of $224 (18.8 percent), while borrowers with loans endorsed after the cutoff experienced reductions of $137 (11.2 percent).[16]

# IV Fuzzy Regression Discontinuity Design

To estimate how payment reductions affect 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, producing a *fuzzy RD design*.

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

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

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

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

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

[15]98 percent of loans in our sample financed at least 1.5 percentage points of the upfront MIP, and 76 percent financed the entire upfront MIP. The increase in the mortgage amount is much smaller for loans endorsed prior to the cutoff date because the upfront MIP was reduced to a single basis point for streamline refinances.

[16]The monthly payment reductions include annual mortgage insurance premiums, and in the vast majority of cases in which the upfront premium is financed into the mortgage, the amortized cost of the upfront premium as well.

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.[17] Because $X$ is defined in terms of endorsement dates, our running variable is discrete. $\varepsilon$ and $\nu$ are uncorrelated random errors.

A large literature studies how best to estimate the functions $f(\cdot)$ and $g(\cdot)$. We use the method of Calonico et al. (2014), which entails nonparametrically estimating the functions near the cutoff using local linear regressions. We estimate the functions $f(\cdot)$ and $g(\cdot)$ as separate local linear polynomials on each side of the cutoff using triangular kernels and mean square error-optimal bandwidths. The procedure also corrects for the bias of the RD estimator in constructing the confidence intervals for the estimate of $\hat{\tau}$ using quadratic local polynomial estimates. We cluster our standard errors at the business day to reflect our discrete running variable.

Hahn et al. (2001) show that two assumptions are required to interpret the $\hat{\tau}$ estimated from this system as an average treatment effect. In the terminology of Lee and Lemieux (2010), 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 borrower to streamline refinance. The second assumption is excludability, which in our context states that a loan's eligibility affects its performance only by affecting the probability of streamline refinancing.

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 start by presenting simple cross-sectional specifications in which the unit of observation is the individual loan, the outcome of interest is whether the loan ever defaults in the policy period, and the treatment is either the proportion of months for which the loan had internally refinanced, or the proportion of months refinanced times the proportional payment reduction. Despite their appealing clarity, those specifications do not account for the fact that loan chains endorsed after

[17] We normalize $X$ such that $c$ equals zero.

the cutoff were more likely to prepay, ending their potential to default against FHA.

To better account for the competing risks of prepayment and default, our preferred specifications take the loan-month as the unit of observation, and estimate treatment effects using pooled cross-sections of the data without imposing any additional structure. The outcome of interest in these regressions is whether a loan defaulted in a particular month, and the treatment of interest is either whether the loan had refinanced or its proportional payment reduction as of that month. These specifications allow us to study the effects of refinancing or payment reduction on the conditional default rate, a standard object of analysis in the mortgage modeling literature.

## IV.A Assessing the Validity of the RDD

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, 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 natural in our setting.

Lee (2008) shows that the local randomization result produces testable implications for the validity of the RD design: no discontinuities should be found in observable covariates at the cutoff date. Such discontinuities might suggest that borrowers sorted around the cutoff in anticipation of the policy change, contradicting the assumption of imprecise control.

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 quartic polynomials in $X$ on both sides of the cutoff date. In general, the graphical evidence supports the notion that there are no systematic differences between loans endorsed before and after the cutoff date. One exception arises in panel E, in which average borrower FICO scores appear to be higher for loans endorsed after the cutoff than for loans endorsed beforehand.[18]

To conduct formal balancing tests for discontinuities at the cutoff date, we estimate sharp RD designs using the method of Calonico et al. (2014) for each of the six covariates. Table 4 displays the results of the balance tests. None of the discontinuities at the cutoff date is close to being statistically significant, using either conventional standard errors or the robust (bias-corrected) standard errors of Calonico et al. (2014). In particular, the conventional and robust p-values for the discontinuity in the borrower FICO score are 0.362 and 0.517, respectively. Thus, the higher average FICO scores to the right of the cutoff in panel E of Figure 3 do not indicate a statistically significant discontinuity. We take these results as consistent with the null hypothesis of no discontinuities in the covariates at the cutoff date.

An additional test of the assumption of imprecise control is the density manipulation test of McCrary (2008). In our context, this test examines whether there is a discontinuity in the number of loan endorsements at the cutoff date. Such a discontinuity would suggest that borrowers sorted to one side of the cutoff in anticipation of the reduced fees. Figure 4 displays the number of endorsements per business day relative to the cutoff. It is evident from the figure that no such discontinuity exists.

To test formally for a possible discontinuity, we use the procedure of Frandsen (2016), who adapts McCrary's (2008) test to allow for a discrete running variable. Frandsen's test requires choosing a "bound coefficient" $k$. To choose $k$ in our application, we conducted Frandsen's test at each business day within 75 days before and after the cutoff for each value of $k$ on the grid $0.01, 0.02, \ldots, 0.30$. The proportion of business days in which the test rejects the null of no sorting is closest to 5 percent at the value $k = 0.25$. The p-value of the Frandsen test with $k = 0.25$ is 1.000, consistent with the visual evidence in Figure 4.

[18]A positive discontinuity in FICO scores at the cutoff would bias us against finding a large treatment effect, as more creditworthy borrowers just to the right of the cutoff should exhibit lower default rates.

# V Results

We present the results of our estimates of the system of equations (1)-(2) using two measures of $D$, the treatment of interest: first, refinance status, and second, proportional monthly payment reductions. We first examine graphical evidence in Figure 5, which shows the data underlying the regressions that follow. We then examine regression results in Table 5, which uses refinance status as the treatment of interest, and Table 6, which uses proportional payment reductions as the treatment of interest.

## V.A Graphical Evidence

Figure 5A shows by endorsement date the average proportion of loans that had refinanced within FHA through July 2016, weighted by the proportion of possible months for which they had refinanced. A striking discontinuity is evident at the cutoff date: roughly 30 percent of loans endorsed just before the cutoff had refinanced internally to FHA, compared with about 12.5 percent for loans endorsed just afterward. Figure 5B shows the weighted average percent reduction in effective monthly payment (P&I plus annual MIP), averaged over loans that did and did not refinance. The reduction is weighted by the proportion of possible months that the loans had refinanced between August 2012 and July 2016.[19] The weighted-average reduction is 5.3 percent just before the cutoff, and roughly 1.2 percent just afterward.

Figure 5C displays the proportion of loans that ever experienced a 90-day delinquency episode from August 2012 to July 2016. The ever 90-day delinquency rate displays a downward trend across endorsement dates, from more than 18 percent for loans endorsed 100 business days prior to the cutoff to approximately 12 percent for loans endorsed 100 business days after the cutoff. That trend is consistent with the improving credit quality of the average borrowers seen in Figure 3. There appears to be a faint jump upward in delinquency rates at the cutoff date.

Figure 5D shows the monthly conditional 60-day delinquency rate, which is

[19]For instance, a loan that experienced a 20 percent payment reduction by refinancing in August 2012 would enter as a 20 percent in the figure, but a loan that experienced a 20 percent payment refinancing halfway through the period would enter as a 10 percent.

falling over most of the range of endorsement dates, from roughly 60 basis points per month 100 business days prior to the cutoff to roughly 45 basis points per month 100 business days after the cutoff. The downward trend appears to be interrupted by a jump upwards at the cutoff date, consistent with payment reductions reducing conditional default rates. The discontinuity in the conditional 60-day delinquency rate is much more pronounced than the discontinuity in the cumulative 90-day delinquency rate in Figure 5C. Figures 5E and 5F display the same information as Figure 5D, but using 90-day delinquencies and 90-day delinquencies that result in a claim against FHA as the measures of default. Figure 5F mimics Figures 5D and 5E, but restricts the default definition to only those 90-day delinquency episodes that end in a claim against FHA. The patterns in panel D are apparent in panels E and F as well.

## V.B Regression Results

Table 5 shows our estimates of how refinancing affected default rates of FHA-insured loans between August 2012 and July 2016; Table 6 shows analagous estimates for the effects of the payment reductions resulting from refinancing. The estimates and inference are performed using the `rdrobust` package provided by Calonico et al. (forthcoming). The first stage of our fuzzy RD design entails estimating the effect of eligibility for reduced fees on refinancing activity or payment reductions. The reduced form entails estimating the discontinuity in default rates at the cutoff. The second stage entails estimating the effect of the treatments on default rates, by dividing the estimated reduced form discontinuities in default rates at the cutoff by the first-stage discontinuities in the treatments.

The bandwidths used to estimate the local polynomials are chosen to be mean square error-optimal as in Calonico et al. (2014), building on Imbens and Kalyanaraman (2012). A single bandwidth for both the first and second stages, and on both sides of the cutoff, is used in each specification. We use triangular kernel functions in the estimation.

Table 5 considers a loan's refinance status as the treatment of interest. Column 1 takes the individual loan as the unit of observation. The treatment of interest is the proportion of possible months for which each loan had refinanced over the sample period, as displayed in Figure 5A. The outcome of interest is whether the loan ever entered 90-day delinquency. This specification implies a semielasticity of defaults with respect to refinancing of $-0.22$, but the effect is not statistically significant at standard confidence levels. One limitation of this specification is that it does not account for the varying lengths of time for which mortgages endorsed on opposite sides of the cutoff were potentially able to default against FHA. Because loans endorsed after the cutoff externally refinanced more rapidly, they had fewer total months in which to default. Therefore we consider conditional default rates as the outcome of interest in the remainder of the table.

The unit of observation in columns 2 through 6 is the loan-month, and the treatment is coded as a one if the loan had internally refinanced by the month in question and a zero otherwise. As described in section IV, the analysis is conducted on a pooled dataset across months. Loans are excluded from the sample after they pay in full or when they default according to the default definition in each column.

Column 2 of Table 5 shows results using 60-day delinquency as the default definition. The estimated semielasticity of the monthly conditional default rate is $-0.64$, and is statistically significant at the 1 percent level. Examining conditional default rates thus improves the precision of the estimates substantially relative to cumulative default rates. Using 90-day delinquency as the default definition in column 3 reduces the estimated elasticity to $-0.40$. The statistical significance of the estimate is also reduced: the robust p-value of Calonico et al. (2014) is 0.072, indicating marginal significance. Column 4 shows results using claims against FHA as the default definition.[20] The estimated semielasticity is $-1.63$, and is highly statistically significant. It is puzzling that the absolute value of the estimated semielasticity is larger than one, implying that each additional refinance prevents more than one claim. One possible explanation is that eligibility for reduced fees may have lowered borrowers' willingness to default even prior to refinancing.[21]

[20]To account for the long and variable delays in delinquent loans generating claims against FHA, the defaults are dated to the month in which the loan enters 90-day delinquency.

[21]Borrowers may have been prevented from streamline refinancing immediately if they had more than one 30-day delinquency in the preceding 12 months; we do not observe 30-day delinquencies in our data.

Column 5 restricts the sample to loans for which information on all covariates is available.[22] The default definition is again 90-day delinquency. The estimated semielasticity of the monthly conditional default rate to refinancing falls to $-0.26$, and is not statistically significant. Column 6 uses the same specification as in column 5, but includes the covariates in the estimation procedure. The results do not change substantially from column 5, although the precision of the estimates improves marginally. The results in section IV.A and the similarity of the estimates in columns 5 and 6 both suggest that the RD design effectively emulates random assignment to treatment, but including the covariates reduces the sample size by nearly one-fifth, which diminishes the precision of the estimates. Therefore, we take the results in column 3 to be our preferred estimates.

Table 6 considers a loan's proportional payment reduction from refinancing as the treatment of interest. Using the proportional payment reduction as the treatment variable allows us to calculate the elasticity of the default rate with respect to the size of the payment reduction. The specifications follow the pattern in Table 5. The treatment of interest in column 1 is the proportional payment reduction weighted by the proportion of possible months for which each loan had refinanced over the sample period, as displayed in Figure 5B. This specification implies an elasticity of defaults with respect to payment reductions of $-0.87$, which as in column 1 of Table 5 is not statistically significant at standard confidence levels.

The estimated elasticity of the monthly conditional default (60-day delinquency) rate with respect to the month-by-month proportional payment reduction measure in column 2 is $-2.63$, and is highly statistically significant. Using 90-day delinquency as the default definition in column 3 produces an estimated elasticity of $-1.55$, which is marginally statistically significant at standard confidence levels. Using claims against FHA as the default definition in column 4 produces an estimated elasticity of $-5.64$, which again is highly significant. As in table 5, restricting the sample to loans with full information on all covariates reduces the estimated elasticity in column 3 to $-1.07$, and the estimate is no longer statistically significant. The estimated elasticity is slightly larger, $-1.28$, when including the covariates in

[22]This sample will be disproportionately purchase loans, as loans that were streamline refinances in 2009 of previous FHA loans did not have full underwriting.

column 6, and the standard error is slightly smaller, but the estimate remains statistically insignficant. We again take the results in column 3 as our preferred estimates, for the reasons described above.

Appendix Section A assesses potential heterogeneity in the estimated treatment effects by examining different subsamples of the data. The results are noisy, but the point estimates suggest that loans with higher mark-to-market LTV ratios and lower borrower FICO scores exhibit larger reductions in default rates in response to refinancing or payment reductions. A stronger result is that mortgages that were refinance loans in 2009 show larger treatment effects than mortgages that were originally purchase loans. There is some evidence that borrowers who refinanced with FHA in 2009 had weaker credit profiles than borrowers who took out purchase loans. Overall, we interpret the results from the data subsamples as suggesting that borrowers with weaker credit profiles are more responsive to payment reductions, although the evidence is not conclusive. To the extent that these differences are meaningful, our data sample may produce larger estimated treatment effects than studies of conventional borrowers with stronger average credit profiles.

## V.C Discussion

Our preferred estimate of the elasticity of default rates with respect to payment reductions is $-1.55$, well within the range of estimates in the studies in Appendix Table A1, approximately $-0.6$ to $-2.2$.[23] Our results therefore provide quasi-experimental validation for the previous literature, suggesting that borrower selection into payment reduction programs does not drive the large reductions in defaults that are typically observed. Additionally, our results suggest that payment reductions can meaningfully reduce default rates in a previously understudied borrower population, FHA borrowers, that has weaker credit histories and less equity in their homes than conventional borrowers; indeed, payment reductions may be even more effective in this setting. Finally, our study examines loan behavior for up to four years

[23] Our preferred estimate of the semielasticity of default rates with respect to refinancing, $-0.40$, is at the low end of the range in Appendix Table A1, approximately $-0.48$ to $-0.62$. One complication that arises from comparing estimated semielasticities is that the size of proportional payment reductions across different programs may differ substantially.

after payment reductions occur, substantially longer than in most of the previous literature. Our results suggest that the effects of payment reductions persist over an extended period of time.

# VI 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 V.B as a proxy for the average treatment effect (ATE) for all loans that streamline refinanced. This assumption seems defensible given that borrowers from pre-2009 credit subsidy cohorts had weaker average FICO scores and likely lost more equity in their homes than borrowers in the 2009 cohort. Nonetheless, the results in this section only roughly estimate the policy's true effect. We summarize our methodology and describe the results in this section; Appendix Section C provides more detail.

We start with a baseline calculation of how many defaults would have occurred in the absence of the reduced fees for SLRs using standard methods. Next, we calculate how many additional streamline refinances and fewer external refinances resulted from the reduced fees. We estimate that the reduced fees caused nearly $200,000$ additional refinances and that a further $263,000$ streamline refinances would have occurred even without the fee reduction.[24] We then apply our preferred estimate of the elasticity of defaults with respect to payment reductions of $-1.55$ to these loans. Our point estimate of the number of defaults prevented by the reduction in fees is $34,841$.

We estimate that the present value of the prevented default losses is \$1.5 billion, whereas the present value of the reduced fee income is \$1.8 billion. Therefore, the net cost to FHA has a present value of \$300 million, or \$8,645 per prevented default.

[24] The latter group defaulted at lower rates because the lower fees led to larger payment reductions than would have occurred with the original fees.

# VII Conclusion

This paper offers quasi-experimental evidence that mortgage payment reductions substantially reduce default rates. Our preferred estimates are within the range typically found in the previous literature, suggesting that selection effects were not a major factor in producing the default reductions observed in previous studies. We estimate that FHA's June 2012 reduction in fees for its streamline refinance program prevented nearly 35,000 defaults, at a present-value cost to the agency of $8,645 per prevented default. The results indicate that large-scale refinancing programs can 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 at Origination**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Loan Characteristic** | **Mean for Group Endorsed:** | | | | |
| | Within 14 Business Days Prior to Cutoff | Within 14 Business Days After Cutoff | January-May 2009 | June-October 2009 | January-October 2009 |
| Loan-to-Value Ratio (%) | 93.4 | 93.5 | 93.4 | 93.7 | 93.6 |
| | (7.7) | (7.4) | (7.8) | (7.6) | (7.7) |
| | [79,745] | [81,880] | [525,473] | [688,925] | [1,214,398] |
| Interest Rate (%) | 5.2 | 5.1 | 5.4 | 5.3 | 5.3 |
| | (0.4) | (0.4) | (0.5) | (0.4) | (0.4) |
| | [107,549] | [111,513] | [656,753] | [826,807] | [1,483,560] |
| Refinance Share (%) | 53.3 | 51.9 | 53.2 | 38.5 | 45.0 |
| | (49.9) | (50.0) | (49.9) | (48.7) | (49.8) |
| | [107,549] | [111,513] | [656,753] | [826,807] | [1,483,560] |
| Borrower Income ($) | 66,239 | 66,224 | 66,057 | 66,091 | 66,077 |
| | (39,661) | (39,511) | (37,971) | (40,253) | (39,300) |
| | [80,117] | [83,618] | [517,010] | [699,891] | [1,216,901] |
| Borrower FICO Score | 687 | 690 | 679 | 693 | 687 |
| | (58) | (57) | (59) | (56) | (58) |
| | [79,410] | [83,181] | [507,572] | [686,990] | [1,194,562] |
| Mortgage Amount ($) | 187,103 | 186,229 | 183,632 | 182,042 | 182,746 |
| | (94,272) | (93,110) | (88,983) | (94,620) | (92,171) |
| | [107,549] | [111,513] | [656,753] | [826,807] | [1,483,560] |
| Monthly Payment ($) | 1,019 | 1,008 | 1,026 | 1,004 | 1,013 |
| | (511) | (502) | (496) | (519) | (509) |
| | [107,549] | [111,513] | [656,753] | [826,806] | [1,483,559] |
| Prop. Active July 2012 (%) | 74.4 | 76.6 | 67.5 | 75.7 | 72.1 |

Note: Cutoff date is May 31, 2009. Standard deviations are in parentheses. Counts of loans with nonmissing characteristics are in brackets. Monthly P&I payment is principal and interest payment as calculated by authors. Proportion active in July 2012 refers to loans that had not refinanced or otherwise paid in full, and were not 90 or more days delinquent as of that date. Sample is restricted 30-year fixed rate mortgages. Refinance share refers to proportion of loans that were refinances of previous mortgages as opposed to purchase loans at origination in 2009, not to the share of originations that subsequently refinanced.

**Table 2.**
**Loan Chain Outcomes as of July 1, 2016 for Policy Sample**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Outcome** | **Percentage for Group Endorsed:** | | | | |
| | Within 14 Business Days Prior to Cutoff | Within 14 Business Days After the Cutoff | January-May 2009 | June-October 2009 | January-October 2009 |
| Refinances | 36.6 | 15.6 | 39.3 | 14.6 | 24.8 |
| Ever Pays in Full | 26.5 | 31.9 | 24.3 | 33.7 | 29.8 |
| Ever 60 Days Delinquent | 15.9 | 16.1 | 17.6 | 15.3 | 16.3 |
| Ever 90 Days Delinquent | 13.5 | 13.5 | 15.2 | 12.8 | 13.8 |
| Ever Goes to Claim | 3.2 | 3.3 | 3.6 | 3.0 | 3.3 |
| Number of Loans | 79,583 | 84,949 | 440,775 | 622,284 | 1,063,059 |

Note: Cutoff date is May 31, 2009. Policy sample includes loans that had not refinanced or otherwise paid in full, and were not 90 or more days delinquent as of July 2012. Loan chains include originally endorsed loans and any internal refinances.

**Table 3.**
**Characteristics of Internal Refinances in Policy Sample, August 2012 to July 2016**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Variable** | **Mean for Group Endorsed:** | | | | |
| | Within 14 Business Days Prior to Cutoff | Within 14 Business Days After Cutoff | January-May 2009 | June-October 2009 | January-October 2009 |
| Mortgage Amount ($) | | | | | |
| Before Refinancing | 196,321 | 195,160 | 186,614 | 190,942 | 188,105 |
| | (86,925) | (84,731) | (79,126) | (86,565) | (81,791) |
| After Refinancing | 197,404 | 200,243 | 187,568 | 196,061 | 190,494 |
| | (87,337) | (86,434) | (79,590) | (88,181) | (82,749) |
| Interest Rate (%) | | | | | |
| Before Refinancing | 5.1 | 5.1 | 5.3 | 5.3 | 5.3 |
| | (0.3) | (0.3) | (0.4) | (0.3) | (0.4) |
| After Refinancing | 3.8 | 3.7 | 3.9 | 3.7 | 3.8 |
| | (0.4) | (0.4) | (0.4) | (0.4) | (0.4) |
| Annual Mortgage Insurance Premium (%) | | | | | |
| Before Refinancing | 0.53 | 0.54 | 0.53 | 0.54 | 0.53 |
| | (0.02) | (0.02) | (0.03) | (0.02) | (0.03) |
| After Refinancing | 0.56 | 1.10 | 0.56 | 1.09 | 0.74 |
| | (0.10) | (0.22) | (0.10) | (0.23) | (0.29) |
| Monthly Payment Amount ($) | | | | | |
| Before Refinancing | 1,228 | 1,233 | 1,189 | 1,220 | 1,200 |
| | (535) | (526) | (493) | (542) | (511) |
| After Refinancing | 1,011 | 1,104 | 965 | 1,084 | 1,006 |
| | (444) | (478) | (405) | (490) | (440) |
| Payment Reduction ($) | 217 | 128 | 224 | 137 | 194 |
| | (115) | (99) | (112) | (102) | (116) |
| Payment Reduction (%) | 17.6 | 10.3 | 18.8 | 11.2 | 16.2 |
| | (5.7) | (7.8) | (5.7) | (7.8) | (7.4) |
| Number of Loans | 29,152 | 13,264 | 173,122 | 90,972 | 264,094 |

Note: Policy sample includes loans that had not refinanced or otherwise paid in full, and were not 90 or more days delinquent as of July 2012. Monthly payment amount includes annual mortgage insurance premium.

**Table 4.**

**Regression Discontinuity Balance Tests of Loan Characteristics at Origination**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | **Loan Characteristic:** | | | | | |
| | Loan-to-Value Ratio (%) | Interest Rate (%) | Refinance Share (%) | Borrower Income ($) | Borrower FICO Score | Mortgage Amount ($) |
| Estimated Discontinuity at Cutoff | -0.031 | 0.002 | 0.59 | -458 | 0.84 | -365 |
| | (0.066) | (0.011) | (0.91) | (443) | (0.92) | (1,859) |
| Conventional P-value | 0.636 | 0.856 | 0.516 | 0.302 | 0.362 | 0.844 |
| Robust P-value | 0.600 | 0.644 | 0.575 | 0.309 | 0.517 | 0.924 |
| | | | | | | |
| Bandwidth | 21.5 | 9.6 | 11.1 | 25.1 | 14.0 | 14.2 |
| Robust Bandwidth | 41.3 | 16.8 | 20.3 | 46.3 | 25.8 | 30.4 |
| Number of Loans | 1,214,398 | 1,483,560 | 1,483,560 | 1,216,901 | 1,194,562 | 1,483,560 |

Note: Cutoff date is May 31, 2009. Discontinuity at cutoff is estimated using the method of Calonico et al. (2014) using triangular kernels, first-order polynomials to estimate the discontinuities, and second-order polynomials to estimate biases. Bandwidths are selected to be MSE-optimal for the treatment effect estimator. Standard errors clustered at the endorsement date are reported in parentheses. Sample includes loans originated from January through October 2009.

**Table 5.**
**Fuzzy Regression Discontinuity Estimates of the Effect of Internal Refinance Status on Default Rates, August 2012–July 2016**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | | **First Stage** | | | |
| | | | Dependent Variable: Refinance Status (%) | | | |
| Originated after Cutoff | -14.8 | -17.5 | -17.1 | -15.4 | -16.7 | -16.7 |
| | (0.17) | (0.15) | (0.13) | (0.25) | (0.38) | (0.36) |
| Conventional P-value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Robust P-value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| | | | **Reduced Form** | | | |
| | | | Dependent Variable: Monthly Conditional Default Rate (%) | | | |
| Originated after Cutoff | 0.43 | 0.05 | 0.03 | 0.03 | 0.01 | 0.01 |
| | (0.48) | (0.02) | (0.01) | (0.01) | (0.02) | (0.01) |
| Conventional P-value | 0.364 | 0.001 | 0.050 | 0.002 | 0.433 | 0.163 |
| Robust P-value | 0.382 | 0.005 | 0.079 | 0.001 | 0.363 | 0.311 |
| | | | **Second Stage** | | | |
| | | | Dependent Variable: Monthly Conditional Default Rate (%) | | | |
| Refinance Status | -2.94 | -0.30 | -0.15 | -0.18 | -0.08 | -0.09 |
| | (3.25) | (0.09) | (0.08) | (0.06) | (0.11) | (0.06) |
| Conventional P-value | 0.365 | 0.001 | 0.051 | 0.002 | 0.433 | 0.164 |
| Robust P-value | 0.371 | 0.004 | 0.072 | 0.001 | 0.359 | 0.299 |
| Semielasticity of Defaults with Respect to Refinancing | -0.22<br>(0.24) | -0.64<br>(0.19) | -0.40<br>(0.21) | -1.63<br>(0.52) | -0.26<br>(0.33) | -0.27<br>(0.20) |
| Bandwidth | 12.2 | 4.5 | 11.3 | 5.2 | 9.7 | 20.0 |
| Robust Bandwidth | 19.8 | 7.6 | 19.2 | 7.9 | 13.2 | 32.5 |
| Default Definition | Cum. 90 Day | 60 Day | 90 Day | Claim | 90 Day | 90 Day |
| Covariates Included | No | No | No | No | No | Yes |
| Covariate Sample | No | No | No | No | Yes | Yes |
| Number of Loans | 1,063,059 | 1,036,668 | 1,063,059 | 1,063,413 | 857,234 | 857,234 |
| Number of Loan-Months | -- | 37,304,553 | 39,269,514 | 37,294,125 | 31,537,628 | 31,537,628 |

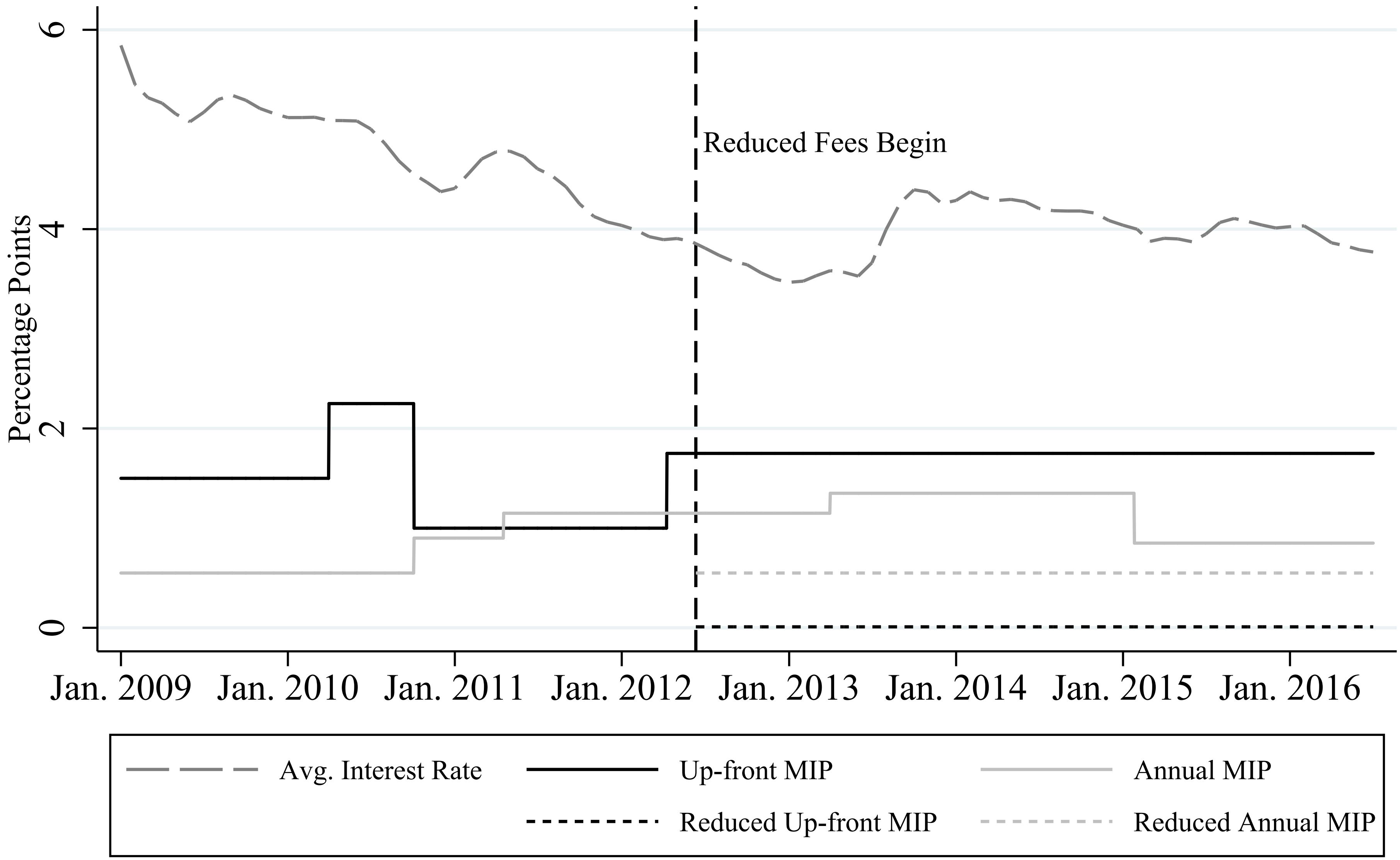
Note: Cutoff date is May 31, 2009. Discontinuity at cutoff is estimated using the method of Calonico et al. (2014) using triangular kernels, first-order polynomials to estimate discontinuities, and second-order polynomials to estimate biases. Bandwidths are selected to be MSE-optimal for the treatment effect estimator. Standard errors clustered at the endorsement date are reported in parentheses. Sample includes loans originated from January through October 2009 that had not paid in full and were not in default according to each column's default definition as of July 2012. Effect in column 1 is estimated on a single cross-section, which measures refinance status as proportion of active months during which loan chain was refinanced, and measures default status as whether the loan chain ever enters 90-day delinquency. Effects in columns 2-6 are estimated using repeated cross-sections of active loan chains, with refinance and default status measured monthly. Column 4 dates defaults to the month a loan goes 90 days delinquent prior to resulting in a claim, and ends the sample December 2015 due to delays in defaults producing claims.

**Table 6.**
**Fuzzy Regression Discontinuity Estimates of the Effect of Payment Reductions on Default Rates, August 2012–July 2016**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | | **First Stage** | | | |
| | | | Dependent Variable: Proportional Payment Reduction (%) | | | |
| Originated after Cutoff | -3.42 | -4.04 | -3.97 | -3.57 | -3.72 | -3.69 |
| | (0.03) | (0.04) | (0.03) | (0.03) | (0.08) | (0.07) |
| Conventional P-value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Robust P-value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| | | | **Reduced Form** | | | |
| | | | Dependent Variable: Monthly Conditional Default Rate (%) | | | |
| Originated after Cutoff | 0.40 | 0.05 | 0.02 | 0.02 | 0.01 | 0.02 |
| | (0.43) | (0.01) | (0.01) | (0.01) | (0.02) | (0.01) |
| Conventional P-value | 0.342 | 0.001 | 0.060 | 0.005 | 0.437 | 0.206 |
| Robust P-value | 0.379 | 0.002 | 0.098 | 0.007 | 0.470 | 0.275 |
| | | | **Second Stage** | | | |
| | | | Dependent Variable: Monthly Conditional Default Rate (%) | | | |
| Payment Reduction (%) | -11.80 | -1.24 | -0.57 | -0.60 | -0.35 | -0.42 |
| | (12.45) | (0.37) | (0.31) | (0.21) | (0.45) | (0.33) |
| Conventional P-value | 0.343 | 0.001 | 0.061 | 0.005 | 0.439 | 0.209 |
| Robust P-value | 0.366 | 0.001 | 0.091 | 0.006 | 0.469 | 0.274 |
| Elasticity of Defaults with Respect to Payment Reductions | -0.87<br>(0.92) | -2.63<br>(0.78) | -1.55<br>(0.83) | -5.64<br>(2.01) | -1.07<br>(1.38) | -1.28<br>(1.02) |
| Bandwidth | 15.2 | 6.7 | 13.3 | 9.1 | 11.2 | 14.8 |
| Robust Bandwidth | 24.6 | 10.3 | 20.4 | 12.9 | 14.7 | 19.6 |
| Default Definition | Cum. 90 Day | 60 Day | 90 Day | Claim | 90 Day | 90 Day |
| Covariates Included | No | No | No | No | No | Yes |
| Covariate Sample | No | No | No | No | Yes | Yes |
| Number of Loans | 1,063,059 | 1,036,668 | 1,063,059 | 1,063,413 | 857,234 | 857,234 |
| Number of Loan-Months | -- | 37,304,553 | 39,269,514 | 37,294,125 | 31,537,628 | 31,537,628 |

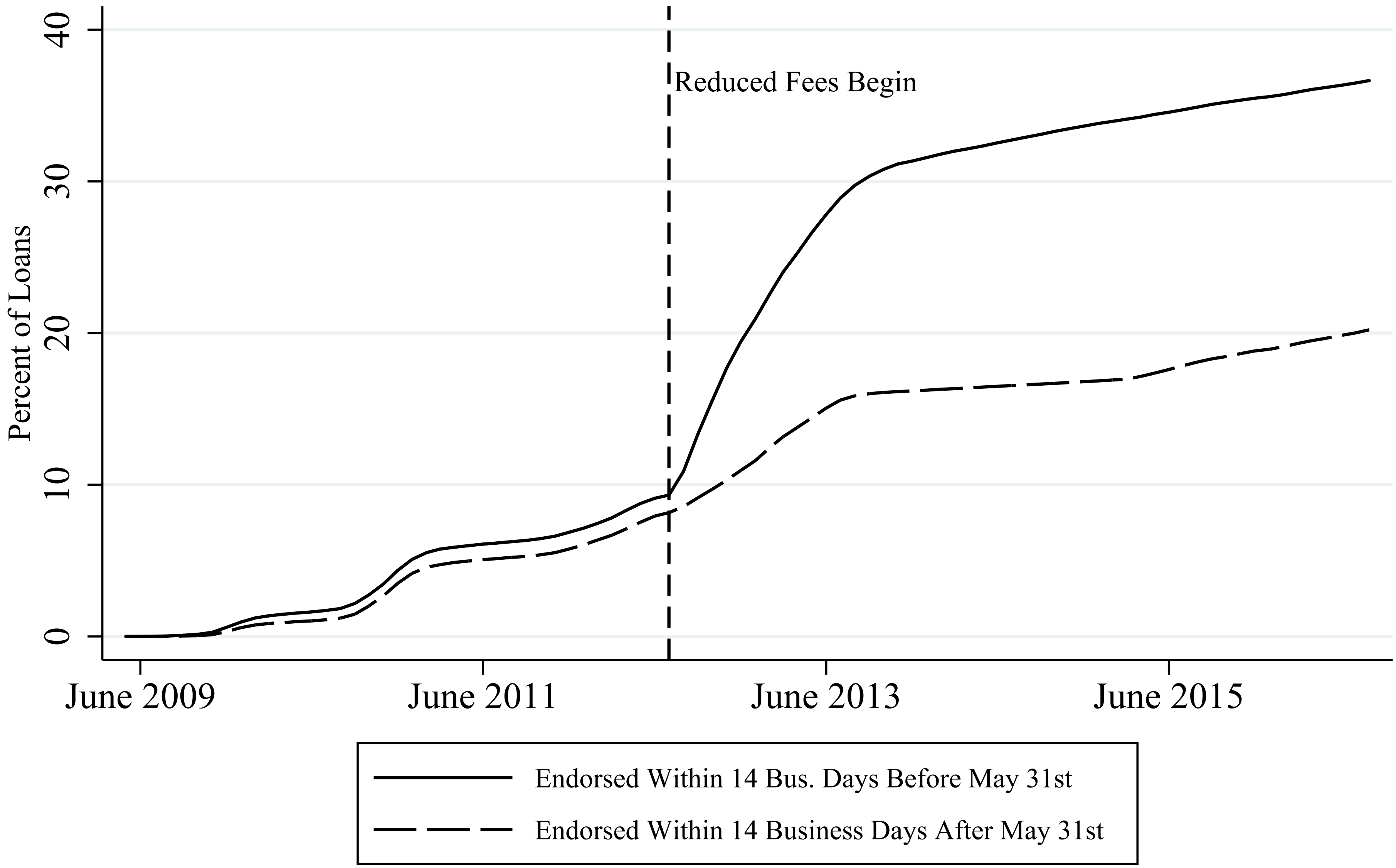
Note: Cutoff date is May 31, 2009. Discontinuity at cutoff is estimated using the method of Calonico et al. (2014) using triangular kernels, first-order polynomials to estimate discontinuities, and second-order polynomials to estimate biases. Bandwidths are selected to be MSE-optimal for the treatment effect estimator. Standard errors clustered at the endorsement date are reported in parentheses. Sample includes loans originated from January through October 2009 that had not paid in full and were not in default according to each column's default definition as of July 2012. Effect in column 1 is estimated on a single cross-section, which measures payment reduction as the proportional payment reduction if refinanced times the proportion of active months during which loan chain was refinanced, and measures default status as whether the loan chain ever enters 90-day delinquency. Effects in columns 2-6 are estimated using repeated cross-sections of active loan chains, with payment reduction and default status measured monthly. Column 4 dates defaults to the month a loan goes 90 days delinquent prior to resulting in a claim, and ends the sample in December 2015 due to delays in defaults producing claims.

Figure 1: FHA Interest Rates and Premiums
January 2009 to July 2016



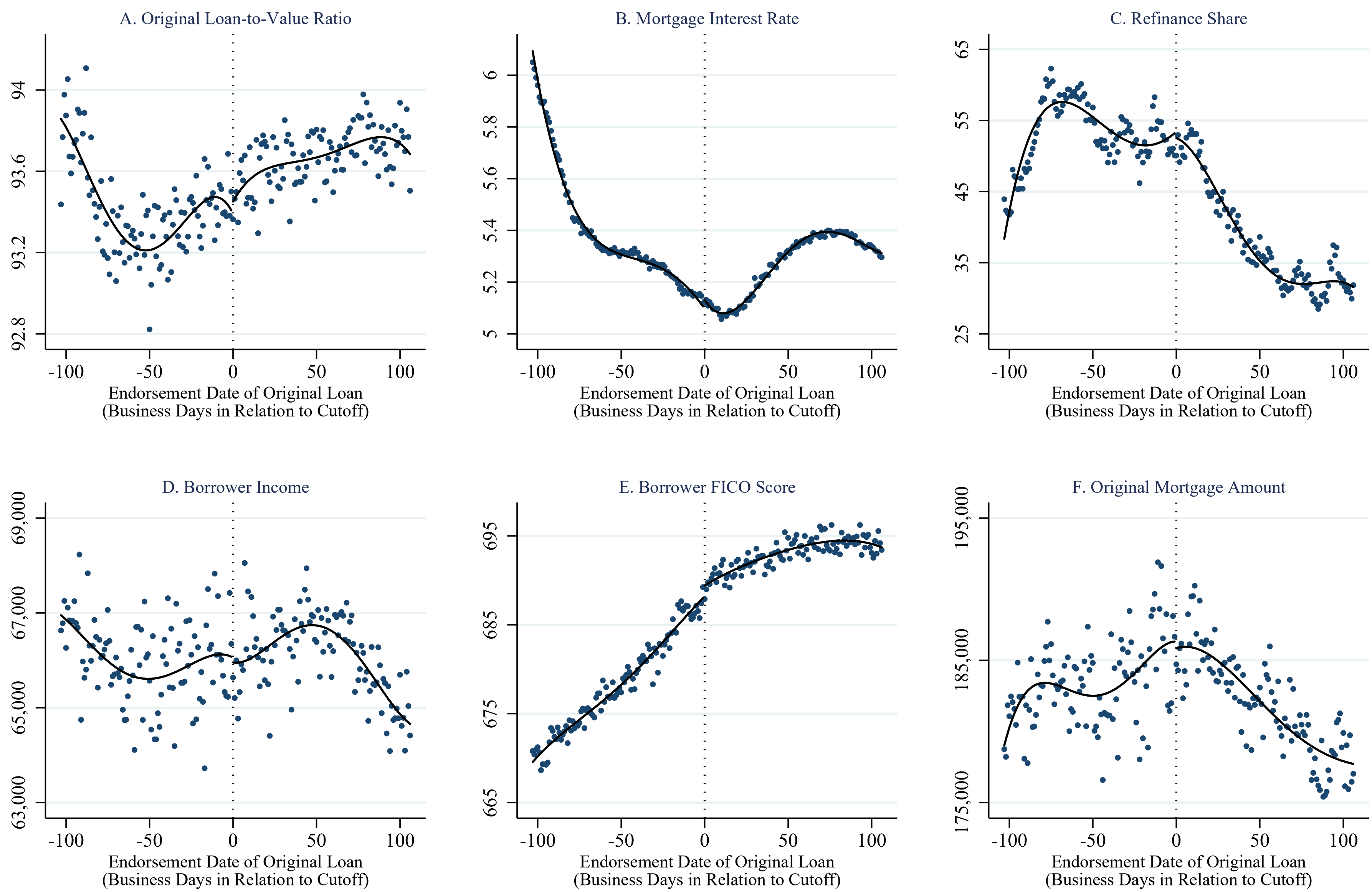

Note: Dashed lines represent reduced premiums for streamline refinances of loans originally endorsed May 31, 2009, or earlier.
Premiums shown for streamline refinances of loans of $625,000 or less with loan-to-value ratios of greater than 95 percent.

Figure 2: Cumulative Internal Refinance Rate
May 2009 to July 2016



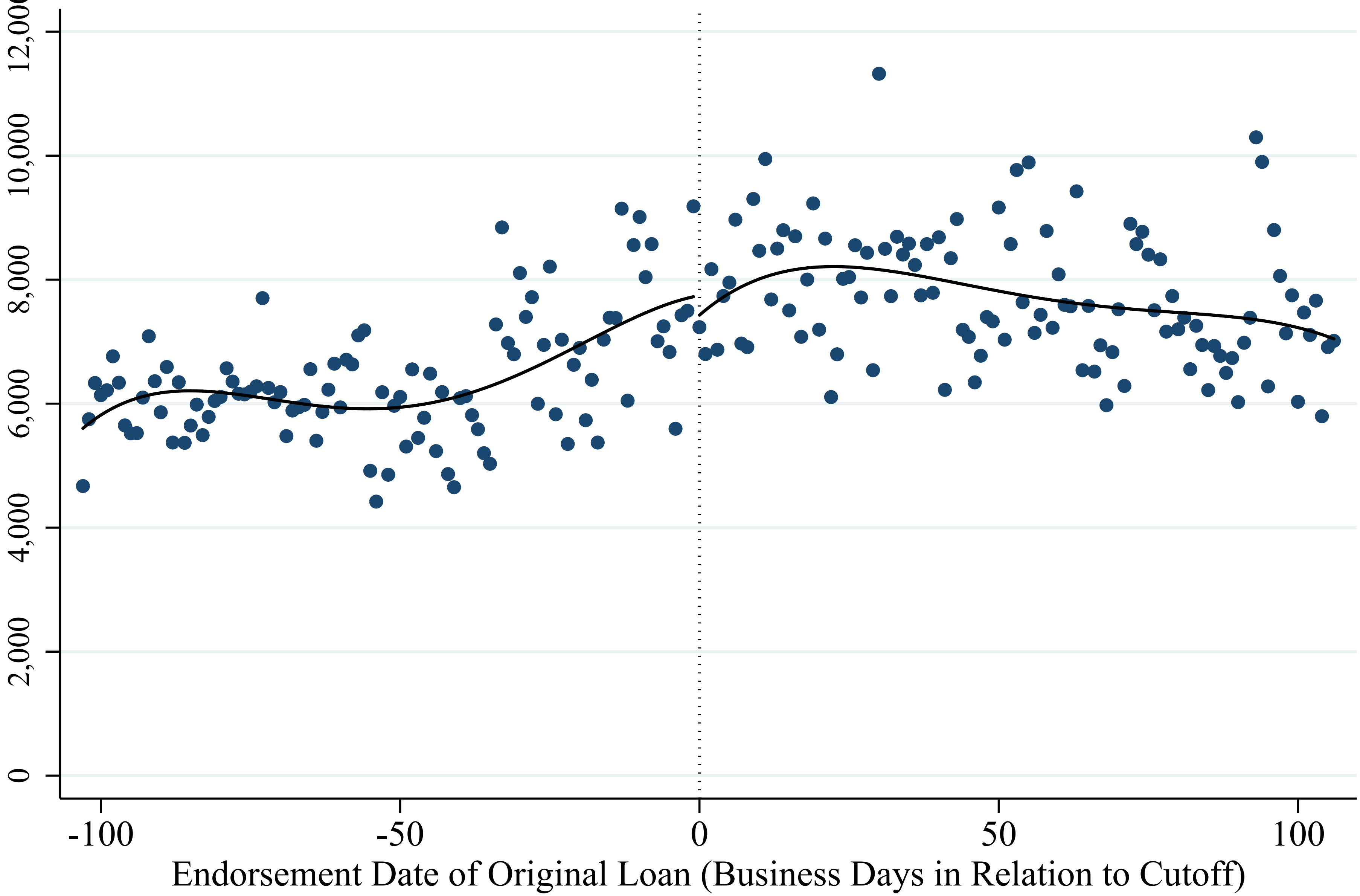

Note: Loans endorsed May 31, 2009, or earlier were eligible for reduced fees.

Figure 3: FHA Loan Characteristics, January 1, 2009 to October 31, 2009



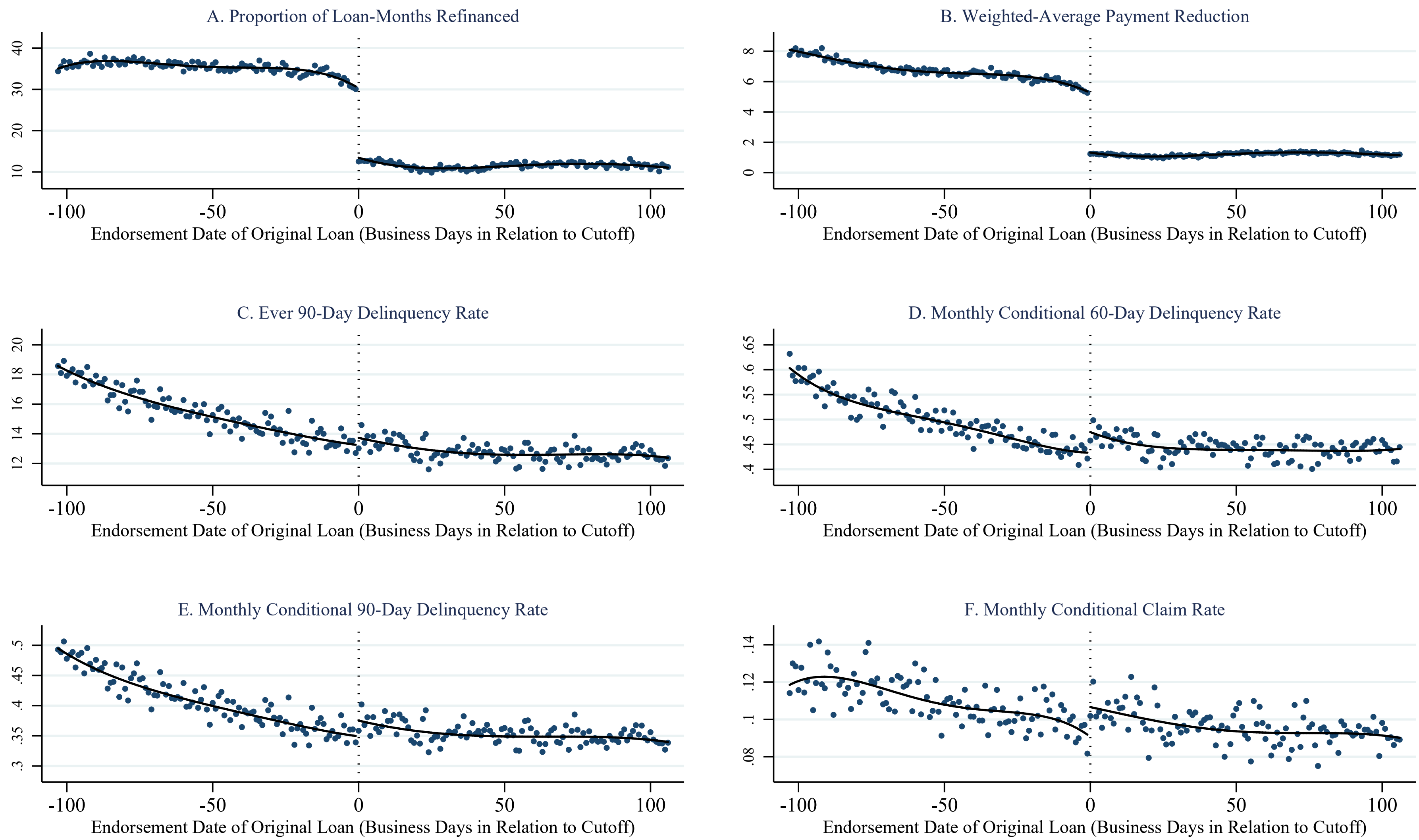

Note: Dots are average values by endorsement date of original loan. Lines are fitted quartic polynomials.

Figure 4: FHA Endorsements by Business Days in Relation to Cutoff
January 1, 2009 to October 31, 2009



## Figure 5: Average Loan Outcomes by Endorsement Date of Original Loan

August 2012 to July 2016, Percentage Points




Note: Dots are average values by endorsement date of original loan. Lines are fitted quartic polynomials, estimated separately on either side of the cutoff. Monthly conditional claim rate measures final transition to 90-day delinquency conditional on the mortgage terminating in a claim, and is measured through December 2015.

# Appendix for Online Publication

## A Variability of Effects by Loan and Borrower Characteristics

Here we examine the effects of streamline refinancing for various subpopulations of the loans in our data sample. We divide the data into subpopulations along three different dimensions: low vs. high mark-to-market loan-to-value ratios (MTM LTVs), low vs. high borrower FICO scores, and mortgages that were purchase loans at endorsement in 2009 vs. loans that were refinances of previous loans at endorsement in 2009.

Table A2 displays regressions following our preferred specification in column 3 of Table 5, which estimate the semielasticity of the monthly conditional default (90-day delinquency) rate with respect to internal FHA refinancing. The first column shows estimates on the subpopulation of loans with MTM LTVs below the sample median of 107.9 percent as of August 2012, and the second column shows estimates for loans with MTM LTVs above the median.[25] Neither estimate is close to achieving statistical significance. The point estimates do imply, however, that loans with higher MTM LTVs are more responsive to payment reductions. Figure A3 displays the regression discontinuity estimates graphically for the two subpopulations. Table A3 shows regressions that follow our preferred specification in column 3 of Table 6, which takes the proportional payment reduction from refinancing as the treatment of interest. The results in Table A3 parallel the results in Table A2 throughout all six columns, so we omit further discussion here.

Columns 3 and 4 show estimates on the subpopulations with borrower FICO scores below and above, respectively, the sample median of 683; Figure A4 illustrates these results. Once again, neither estimate is statistically significant, and the estimates are too noisy to distinguish the estimated treatment effects from one another. The lack of precision in these estimates is unsurprising in light of the results in column 5 of table 5, which show that omitting loans without full covariate information eliminates the statistical significance of the estimated treatment.

[25]Figure A2 shows the distribution of MTM LTVs and borrower FICO scores in the sample as of August 2012. MTM LTVs were estimated by dividing each mortgage's amortized unpaid principal balance by our estimate of the market value of the mortgaged home as of August 2012. Those market values were calculated by assuming that each home's market value evolved proportionally to values in the home's 3-digit ZIP code as estimated by the Federal Housing Finance Agency in Bogin et al. (2016).

Columns 5 and 6 show estimates for mortgages that were purchase loans and mortgages that were refinance loans at endorsement in 2009, as illustrated in Figure A5. Column 5 shows that the estimated effect of refinancing for the purchase loans is approximately zero. In contrast, the estimated effects for the loans that were refinances as of 2009 is estimated to be quite large, with a semielasticity of $-0.79$, and highly statistically significant.

Drawing solid inferences from the contrast between the purchase and refinance loans is difficult. Unfortunately, the data are missing borrower FICO scores for nearly 42 percent of mortgages that were originally refinance loans in 2009, many of which were themselves streamline refinances without full underwriting.[26] Nonetheless, there are three reasons to believe that the credit profile of borrowers who refinanced with FHA in 2009 was weaker than the credit profile of borrowers who took out purchase loans. First, we do observe borrower FICO scores for over half of refinances, even if they are unlikely to be a random sample of all refinances. The average observed FICO score for refinances is 679, versus 691 for purchase loans. Second, the average FICO score of FHA borrowers began increasing sharply in early 2008 (the continuation of that trend into 2009 is visible in our Figure 3E). Therefore, the pool of internal refinancers in 2009 would likely have had a weaker average credit profile than FHA home purchasers in 2009. Third, the bottom panels of Figure A5 show that the conditional default rates of the refinances are almost uniformly higher than the default rates of the purchases. As seen in Figure A4, higher conditional default rates are strongly associated with lower borrower FICO scores. It therefore seems reasonable to conclude that refinance loans originated in 2009 had weaker borrower credit profiles than purchase loans originated over the same period.

That conclusion is complicated by the fact that underwritten FHA refinance loans have substantially lower LTV ratios than purchase loans, which make a modal down payment of 3.5 percent. In our data, underwritten refinance loans in 2009 had an average original LTV ratio of 89.1 percent, versus an average of 95.7 percent for purchase loans.[27] However, it is possible that streamline refinances in 2009 had substantially higher LTV ratios than underwritten loans. For instance, Aragon et al. (2010) estimate that more than 50 percent of streamline refinances in this period had negative equity, or loan-to-value ratios above 100 percent, upon streamline refinancing.

Overall, we interpret the evidence in Tables A2 and A3 as providing suggestive,

[26]For comparison, just over one percent of the purchase loans in 2009 are missing borrower FICO scores.

[27]The data are missing original LTV ratios for 40.3 percent of refinance loans, as FHA does not require this information for streamline refinances. In contrast, only three purchase loans out of more than 800,000 are missing this information.

though not conclusive, that mortgage payment reductions have a larger effect on default rates for borrowers who face greater credit constraints.

# B Robustness of Estimated Effects

The regressions in Tables A4 and A5 assess the robustness of the results in Tables 5 and 6, respectively. Columns 1 and 2 of both tables examine the robustness of our preferred specifications in column 3 of Tables 5 and 6 to different bandwidths. Column 1 uses one-half the optimally chosen bandwidth, and column 2 uses twice the optimally chosen bandwidth. In Table A4, the estimated semielasticity of the monthly conditional default rate in column 1 is $-0.56$, larger than the baseline estimate, and is also more statistically significant. The estimated semielasticity in column 2 is $-0.38$, nearly equal to the baseline estimate, but also has higher statistical significance. In Table A5, the estimated elasticity of the monthly conditional default rate in column 1 is $-2.14$, again larger and more significant than the baseline estimate. The estimated elasticity in column 2 is $-1.73$, also close to the baseline estimate, but more statistically significant.

Columns 3 and 4 show the results of placebo tests using April 30, 2009 and June 30, 2009, respectively, as cutoff dates. The columns otherwise mimic the preferred specifications in column 3 of Tables 5 and 6. There are not statistically significant discontinuities in the second stage at either placebo cutoff date in Table A5, with robust p-values of $0.415$ and $0.524$. The reduced form in column 4 shows a marginally significant discontinuity with a robust p-value of $0.078$, but there is no first-stage discontinuity in refinancing. There are also not statistically significant discontinuities in the second stage at either placebo cutoff date in Table A5, with robust p-values of $0.577$ and $0.948$. Column 4 again shows a discontinuity in the reduced form, but virtually no discontinuity in the first stage.

We view the results of these robustness tests as showing that our estimated treatment effects are fairly robust to different bandwidth choices. Furthermore, the placebo tests support the notion that end-of-month differences do not drive our results, and that something special happened at the May 31, 2009 cutoff. Visual inspection of Figure 5 also supports this notion.

Figure A1 shows estimates of the first-stage, reduced form, and second stage coefficients and confidence intervals of RD regressions analagous to column 3 of Table 6, but performed month-by-month from August 2011 to July 2016. The estimates prior to the fee reduction in July 2012 serve as a pre-treatment placebo test. The top panel of Figure A1, which displays the estimated first-stage discontinuities, shows that there was no discontinuity in internal refinancing at the cutoff prior to July 2012, after which time the discontinuity grew quickly. The reduced form estimates in panel B show that from August 2011 to July 2012, the estimated discontinuity in default rates was noisy, with many point estimates very nearly close to zero, and a roughly even number of estimates above and below zero. Most of the point estimates for the months after the fee reduction indicate positive discontinuities, implying that loans endorsed shortly after the cutoff defaulted at higher rates than loans endorsed shortly beforehand. The monthly estimates are quite noisy, with few individual point estimates statistically different from zero, and several negative point estimates. Pooling the months improves the statistical precision of the estimates substantially. The second stage estimates in panel C are omitted prior to the fee reduction in July 2012 because the lack of a first stage discontinuity leads to extremely noisy estimates; the estimates remain quite noisy for the first year or so after the fee reduction. The point estimates for the months after mid-2013 tell essentially the same story as the reduced form estimates in panel B, with a majority of monthly point estimates below zero, suggesting that payment reductions reduced defaults. Again, though, pooling the months is necessary to gain statistical significance.

# C Details of Accounting for Defaults Prevented and Budgetary Effects

The first step in calculating how FHA's fee reduction for SLRs affects defaults is to estimate how many defaults would have occurred without the policy. We use the projected lifetime rates reported in Castelli et al. (2014), and extrapolate the cumulative claim rates of mortgages active when the policy took effect through the end of the mortgage term from those projections.[28]

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). We denote $F_i^{loan}$ as the cumulative default rate for cohort $i$ reported in the *Actuarial Review* and denote $\phi_i$ 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

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

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.

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 internal and external refinance rates of each cohort over the period January 2010 to July 2016 on: a set of cohort dummies; 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.

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.[29] The results imply that from August 2012 to July 2016, the reduced fees caused 199,754 additional refinances.

The reduction in fees reduced 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 262,586 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 V.B to the total payment reduction that the policy induced. One complication that arises in this step is that the elasticity estimated in

[29]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.

section V 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 $\theta$, Zhu et al. (2015) show that the elasticity $\Theta$ of the lifetime cumulative default rate $F^{chain}$ can be bounded by $\theta(1 - F^{chain})$ and $\theta$.

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 $\rho_m$ and the reduction for the others be $\rho_n$. Then the point estimate of the total number of defaults prevented, $\Delta$, is bounded by:

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

$$\Delta_{UB} = F^{chain}\theta(M\rho_m + N\rho_n). \tag{A.2}$$

The results imply that the reduced fees prevented 27,253–42,428 defaults over the affected loans' lifetimes. Of those totals, 13,352–21,432 are loans that the reduced fees caused to streamline refinance, net of streamline refinances that would otherwise have refinanced externally. The remainder 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 on FHA. 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 et al. (2011) for a discussion of those issues.

**Table A1.**
**Literature Review**

| Study | Data Set | Approach | Results |
|---|---|---|---|
| | | **Approach 1: Study Private Loan Modifications** | |
| Adelino, Gerardi, and Willen (2013) | Lender Processing Services data (approximately 60 percent of U.S. mortgage market); focus on mortgages delinquent by at least | 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 (elasticity of -1.3) |
| 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 (elasticity of -0.75) |
| | | **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 and others (2014) | Freddie Mac data | Compare performance between loans that participate in HARP and those that do not. Use inverse probability weighting approach | A 10 percent payment reduction reduces monthly default probability by 10 percent to 11 |
| Agarwal and others (2015) | Proprietary database of conforming mortgages merged with consumer credit bureau records | Use instrumental variables strategy to compare outcomes of ZIP codes with large amounts of HARP refinancing to ZIP codes with smaller amounts of HARP refinancing | A 15 basis point reduction in average mortgage interest rates reduces ZIP code-level foreclosure rates by one basis point |
| Karamon and others (2016) | Freddie Mac data | Compare performance between loans that participate in HARP and those that do not. Use fuzzy RD to estimate causal effect | HARP refinancing lowers default probability by 48 to 62 percent |
| | | **Approach 3: Study Payment Reductions Arising From Prespecified Loan Terms** | |
| Amromin and others (2013) | Lender Processing Services data; focus on "complex mortgages" | 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 (elasticity of -0.61) |
| Tracy and Wright (2012) | Lender Processing Services data; focus on 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 (elasticity of -1.7 to -2.2) |
| Keys and others (2014) | Proprietary dataset with loan-level panel data matched to consumer credit bureau records | Compare performance between loans that experience downward interest rate adjustments and loans that do not | A 20 percent reduction in monthly payment reduces default probability by 40 percent |
| Fuster and Willen (2015) | CoreLogic Loan Performance data; focus on Alt-A, interest-only 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.**
**Fuzzy Regression Discontinuity Estimates of the Effect of Internal Refinance Status on Default Rates, August 2012–July 2016**

| | (1) MTM LTV Below Median | (2) MTM LTV Above Median | (3) FICO Score Below Median | (4) FICO Score Above Median | (5) Orginally Purchase Loan | (6) Originally Refinance |
|---|---|---|---|---|---|---|
| | | | **First Stage** | | | |
| | | | Dependent Variable: Proportional Payment Reduction (%) | | | |
| Originated after Cutoff | -14.5 | -17.5 | -12.3 | -21.1 | -15.6 | -18.7 |
| | (0.26) | (0.46) | (0.42) | (0.36) | (0.33) | (0.57) |
| Conventional P-value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Robust P-value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| | | | **Reduced Form** | | | |
| | | | Dependent Variable: Monthly Conditional Default Rate (%) | | | |
| Originated after Cutoff | -0.01 | 0.03 | 0.01 | 0.00 | 0.00 | 0.07 |
| | (0.02) | (0.02) | (0.02) | (0.01) | (0.02) | (0.01) |
| Conventional P-value | 0.575 | 0.227 | 0.448 | 0.745 | 0.990 | 0.000 |
| Robust P-value | 0.437 | 0.366 | 0.599 | 0.979 | 0.970 | 0.000 |
| | | | **Second Stage** | | | |
| | | | Dependent Variable: Monthly Conditional Default Rate (%) | | | |
| Refinance Status | 0.06 | -0.16 | -0.11 | -0.02 | 0.00 | -0.35 |
| | (0.11) | (0.13) | (0.14) | (0.06) | (0.12) | (0.05) |
| Conventional P-value | 0.577 | 0.225 | 0.448 | 0.745 | 0.990 | 0.000 |
| Robust P-value | 0.436 | 0.353 | 0.587 | 0.981 | 0.970 | 0.000 |
| Semielasticity of Defaults with Respect to Refinancing | 0.20<br>(0.36) | -0.46<br>(0.38) | -0.22<br>(0.29) | -0.11<br>(0.35) | 0.00<br>(0.38) | -0.79<br>(0.11) |
| Bandwidth | 14.6 | 8.7 | 17.5 | 11.6 | 11.5 | 7.9 |
| Robust Bandwidth | 22.3 | 15.1 | 30.0 | 21.1 | 15.2 | 18.2 |
| Default Definition | 90 Day | 90 Day | 90 Day | 90 Day | 90 Day | 90 Day |
| Covariates Included | No | No | No | No | No | No |
| Covariate Sample | No | No | No | No | No | No |
| Number of Loans | 448,039 | 448,043 | 440,256 | 439,494 | 617,140 | 445,919 |
| Number of Loan-Months | 16,067,114 | 16,901,857 | 16,172,253 | 16,223,555 | 22,280,849 | 16,988,665 |

Note: All specifications are identical to column 3 of table 5. Column 1 includes loans with mark-to-market loan-to-value ratios below the sample median as of August 2012; column 2 includes loans with mark-to-market LTVs above the median. Column 3 includes loans with borrower FICO scores below the sample median as of August 2012; column 4 includes loans with borrower FICO scores above the median. Column 5 includes mortgages that were purchase loans at endorsement in 2009; column 6 includes mortgages that were refinances of previous mortgages at endorsement in 2009.

**Table A3.**
**Fuzzy Regression Discontinuity Estimates of the Effect of Payment Reductions on Default Rates, August 2012–July 2016**

| | (1)<br>MTM LTV Below Median | (2)<br>MTM LTV Above Median | (3)<br>FICO Score Below Median | (4)<br>FICO Score Above Median | (5)<br>Orginally Purchase Loan | (6)<br>Originally Refinance |
|---|---|---|---|---|---|---|
| | | | **First Stage** | | | |
| | | | Dependent Variable: Refinance Status (%) | | | |
| Originated after Cutoff | -3.1 | -4.1 | -2.9 | -4.5 | -3.4 | -4.6 |
| | (0.07) | (0.08) | (0.08) | (0.09) | (0.07) | (0.09) |
| Conventional P-value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Robust P-value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| | | | **Reduced Form** | | | |
| | | | Dependent Variable: Monthly Conditional Default Rate (%) | | | |
| Originated after Cutoff | 0.00 | 0.03 | 0.01 | 0.00 | 0.00 | 0.05 |
| | (0.02) | (0.02) | (0.02) | (0.01) | (0.01) | (0.01) |
| Conventional P-value | 0.871 | 0.165 | 0.475 | 0.858 | 0.921 | 0.000 |
| Robust P-value | 0.443 | 0.192 | 0.629 | 0.849 | 0.973 | 0.000 |
| | | | **Second Stage** | | | |
| | | | Dependent Variable: Monthly Conditional Default Rate (%) | | | |
| Payment Reduction (%) | -0.10 | -0.66 | -0.43 | -0.05 | 0.04 | -0.99 |
| | (0.60) | (0.47) | (0.60) | (0.29) | (0.40) | (0.21) |
| Conventional P-value | 0.871 | 0.167 | 0.476 | 0.858 | 0.921 | 0.000 |
| Robust P-value | 0.441 | 0.190 | 0.621 | 0.851 | 0.974 | 0.000 |
| Elasticity of Defaults with Respect to Payment Reductions | -0.31<br>(1.92) | -1.84<br>(1.33) | -0.86<br>(1.21) | -0.31<br>(1.73) | 0.13<br>(1.32) | -2.25<br>(0.47) |
| Bandwidth | 8.7 | 12.2 | 18.5 | 10.5 | 17.9 | 14.9 |
| Robust Bandwidth | 10.5 | 19.3 | 28.9 | 19.2 | 22.8 | 31.9 |
| Default Definition | 90 Day | 90 Day | 90 Day | 90 Day | 90 Day | 90 Day |
| Covariates Included | No | No | No | No | No | No |
| Covariate Sample | No | No | No | No | No | No |
| Number of Loans | 448,039 | 448,043 | 440,256 | 439,494 | 617,140 | 445,919 |
| Number of Loan-Months | 16,067,114 | 16,901,857 | 16,172,253 | 16,223,555 | 22,280,849 | 16,988,665 |

Note: All specifications are identical to column 3 of table 5. Column 1 includes loans with mark-to-market loan-to-value ratios below the sample median as of August 2012; column 2 includes loans with mark-to-market LTVs above the median. Column 3 includes loans with borrower FICO scores below the sample median as of August 2012; column 4 includes loans with borrower FICO scores above the median. Column 5 includes mortgages that were purchase loans at endorsement in 2009; column 6 includes mortgages that were refinances of previous mortgages at endorsement in 2009.

**Table A4.**
**Fuzzy Regression Discontinuity Estimates of the Effect of Internal Refinance Status on Default Rates, August 2012–July 2016**

| | (1) One-Half Bandwidth | (2) Two Times Bandwidth | (3) Placebo Test: Cutoff April 30 | (4) Placebo Test: Cutoff June 30 |
|---|---|---|---|---|
| | **First Stage** | | | |
| | Dependent Variable: Refinance Status (%) | | | |
| Originated after Cutoff | -16.7 | -17.6 | -2.7 | 0.4 |
| | (0.19) | (0.39) | (0.01) | (0.38) |
| Conventional P-value | 0.000 | 0.000 | 0.000 | 0.328 |
| Robust P-value | 0.000 | 0.000 | 0.000 | 0.714 |
| | **Reduced Form** | | | |
| | Dependent Variable: Monthly Conditional Default Rate (%) | | | |
| Originated after Cutoff | 0.03 | 0.03 | -0.02 | 0.04 |
| | (0.02) | (0.01) | (0.00) | (0.01) |
| Conventional P-value | 0.030 | 0.008 | 0.000 | 0.000 |
| Robust P-value | 0.019 | 0.030 | 0.259 | 0.078 |
| | **Second Stage** | | | |
| | Dependent Variable: Monthly Conditional Default Rate (%) | | | |
| Refinance Status | -0.21 | -0.14 | 0.82 | 11.94 |
| | (0.10) | (0.05) | (0.02) | (12.18) |
| Conventional P-value | 0.030 | 0.009 | 0.000 | 0.327 |
| Robust P-value | 0.018 | 0.027 | 0.415 | 0.524 |
| Semielasticity of Defaults with Respect to Refinancing | -0.56 | -0.38 | 2.24 | 31.83 |
| | (0.26) | (0.15) | (0.06) | (32.46) |
| Bandwidth | 5.6 | 22.6 | 2.5 | 6.7 |
| Robust Bandwidth | 9.6 | 38.3 | 5.5 | 45.7 |
| Default Definition | 90 Day | 90 Day | 90 Day | 90 Day |
| Covariates Included | No | No | No | No |
| Covariate Sample | No | No | No | No |
| Number of Loans | 1,063,059 | 1,063,059 | 1,063,059 | 1,063,059 |
| Number of Loan-Months | 39,269,514 | 39,269,514 | 39,269,514 | 39,269,514 |

Note: All specifications are identical to column 3 of table 5, except column 1 uses a bandwidth one-half the size, column uses a bandwidth twice the size, column 3 uses a cutoff of April 30, 2009, and column 4 uses a cutoff of June 30, 2009.

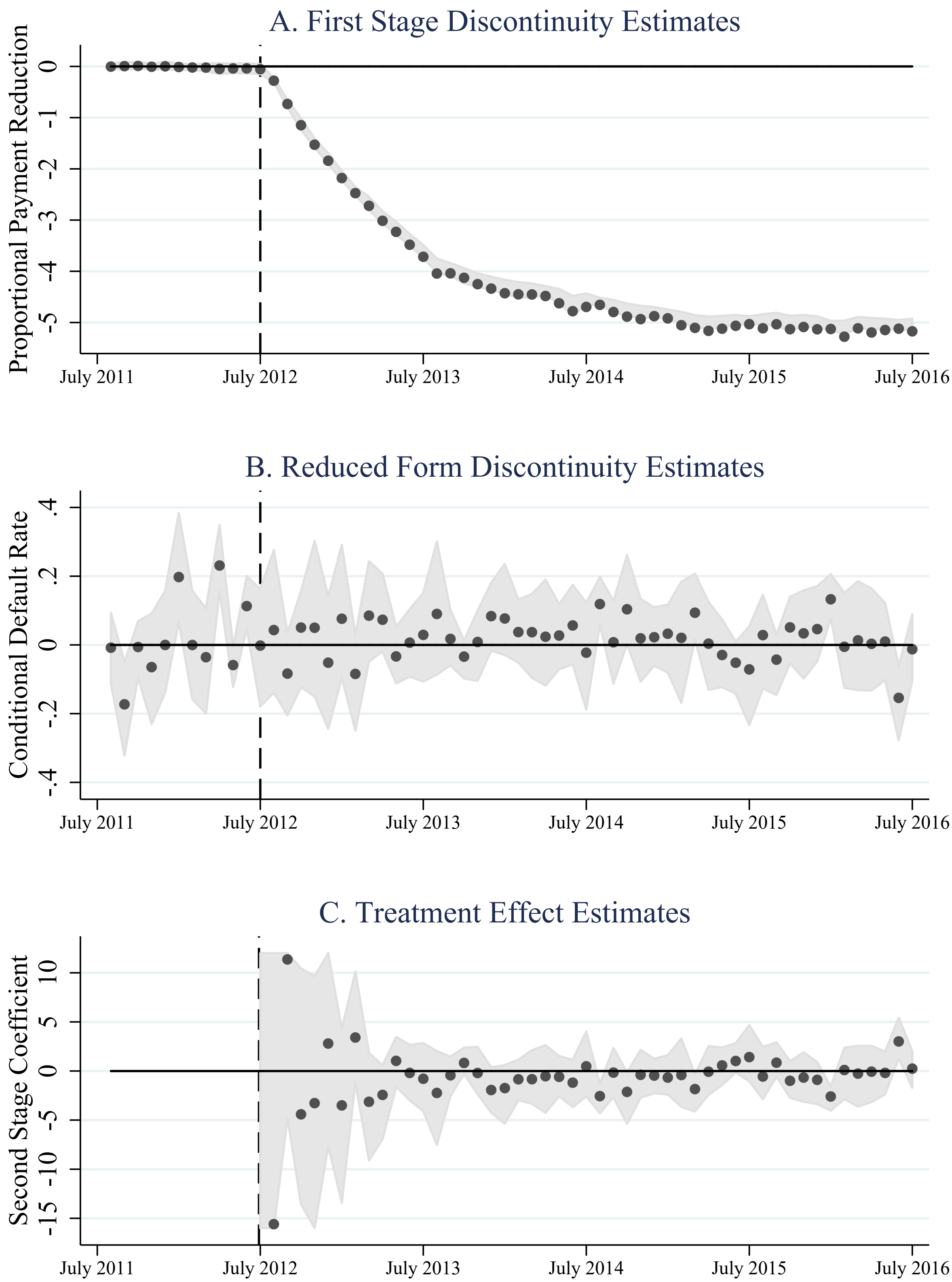
**Table A5.**
**Fuzzy Regression Discontinuity Estimates of the Effect of Payment Reductions on Default Rates, August 2012–July 2016**

| | (1) One-Half Bandwidth | (2) Two Times Bandwidth | (3) Placebo Test: Cutoff April 30 | (4) Placebo Test: Cutoff June 30 |
|---|---|---|---|---|
| | **First Stage** | | | |
| | Dependent Variable: Proportional Payment Reduction (%) | | | |
| Originated after Cutoff | -3.9 | -4.3 | -0.2 | 0.0 |
| | (0.03) | (0.08) | (0.10) | (0.06) |
| Conventional P-value | 0.000 | 0.000 | 0.035 | 0.989 |
| Robust P-value | 0.000 | 0.000 | 0.014 | 0.929 |
| | **Reduced Form** | | | |
| | Dependent Variable: Monthly Conditional Default Rate (%) | | | |
| Originated after Cutoff | 0.03 | 0.03 | 0.01 | 0.05 |
| | (0.01) | (0.01) | (0.01) | (0.01) |
| Conventional P-value | 0.030 | 0.000 | 0.540 | 0.001 |
| Robust P-value | 0.026 | 0.001 | 0.516 | 0.001 |
| | **Second Stage** | | | |
| | Dependent Variable: Monthly Conditional Default Rate (%) | | | |
| Payment Reduction (%) | -0.80 | -0.65 | -3.96 | -5739.79 |
| | (0.37) | (0.17) | (6.85) | (419238.16) |
| Conventional P-value | 0.030 | 0.000 | 0.563 | 0.989 |
| Robust P-value | 0.025 | 0.001 | 0.577 | 0.948 |
| Elasticity of Defaults with Respect to Payment Reductions | -2.14 | -1.73 | -11.05 | -15327.70 |
| | (0.99) | (0.46) | (19.10) | (1119545.40) |
| Bandwidth | 9.2 | 36.7 | 7.9 | 7.1 |
| Robust Bandwidth | 13.5 | 54.2 | 16.9 | 17.4 |
| Default Definition | 90 Day | 90 Day | 90 Day | 90 Day |
| Covariates Included | No | No | No | No |
| Covariate Sample | No | No | No | No |
| Number of Loans | 1,063,059 | 1,063,059 | 1,063,059 | 1,063,059 |
| Number of Loan-Months | 39,269,514 | 39,269,514 | 39,269,514 | 39,269,514 |

Note: All specifications are identical to column 3 of table 6, except column 1 uses a bandwidth one-half the size, column uses a bandwidth twice the size, column 3 uses a cutoff of April 30, 2009, and column 4 uses a cutoff of June 30, 2009.

# Figure A1: Monthly Effect Estimates

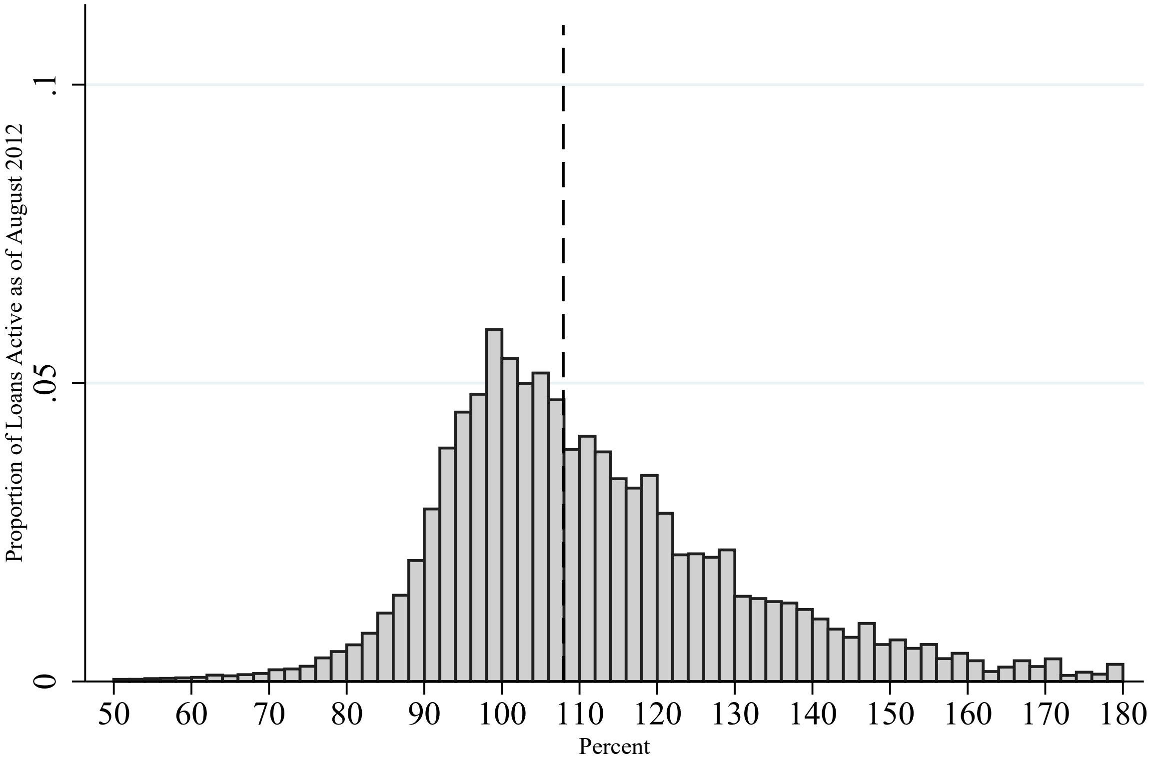
August 2011 to July 2016, Percentage Points




Note: 95 percent robust confidence regions shaded in grey. Confidence region in treatment effect estimates extends beyond graph range in August and September 2012 and is censored for clarity.

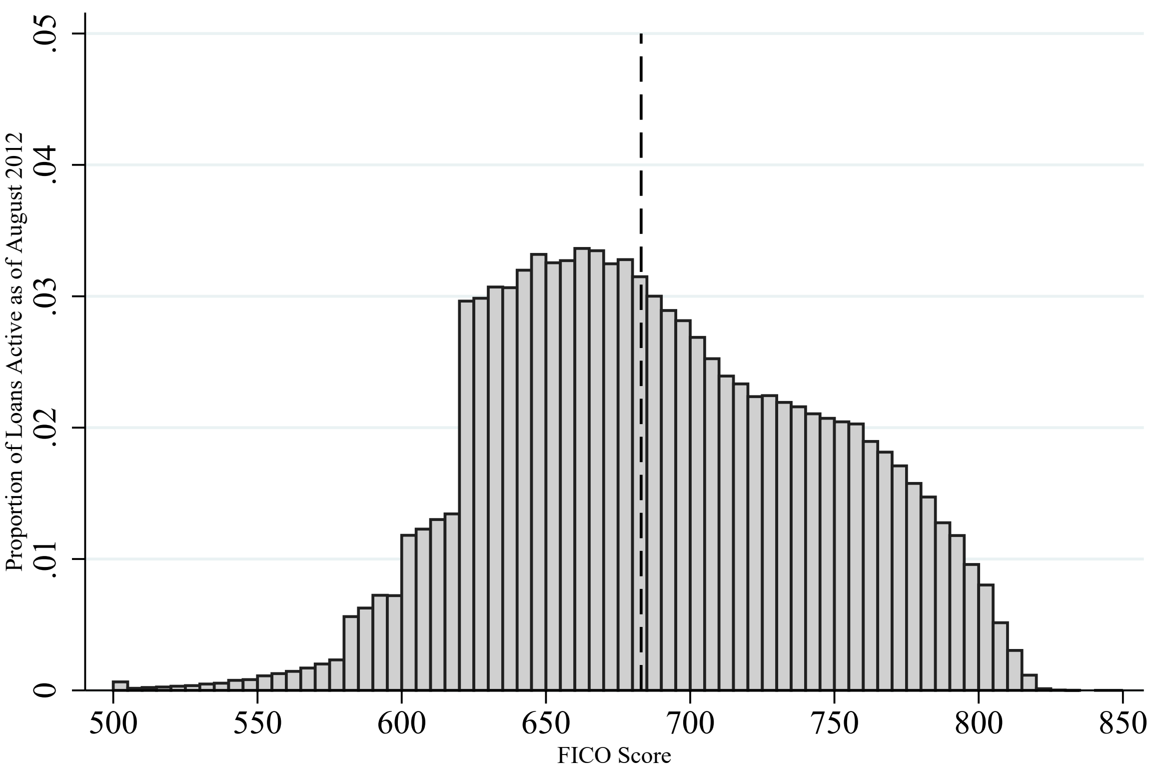
## Figure A2: Mark-to-Market LTV Ratio and Borrower FICO Score Distributions

A. Mark-to-Market LTV Ratios as of August 2012




Dashed line shows median mark-to-market loan-to-value (LTV) ratio of 107.9 percent.

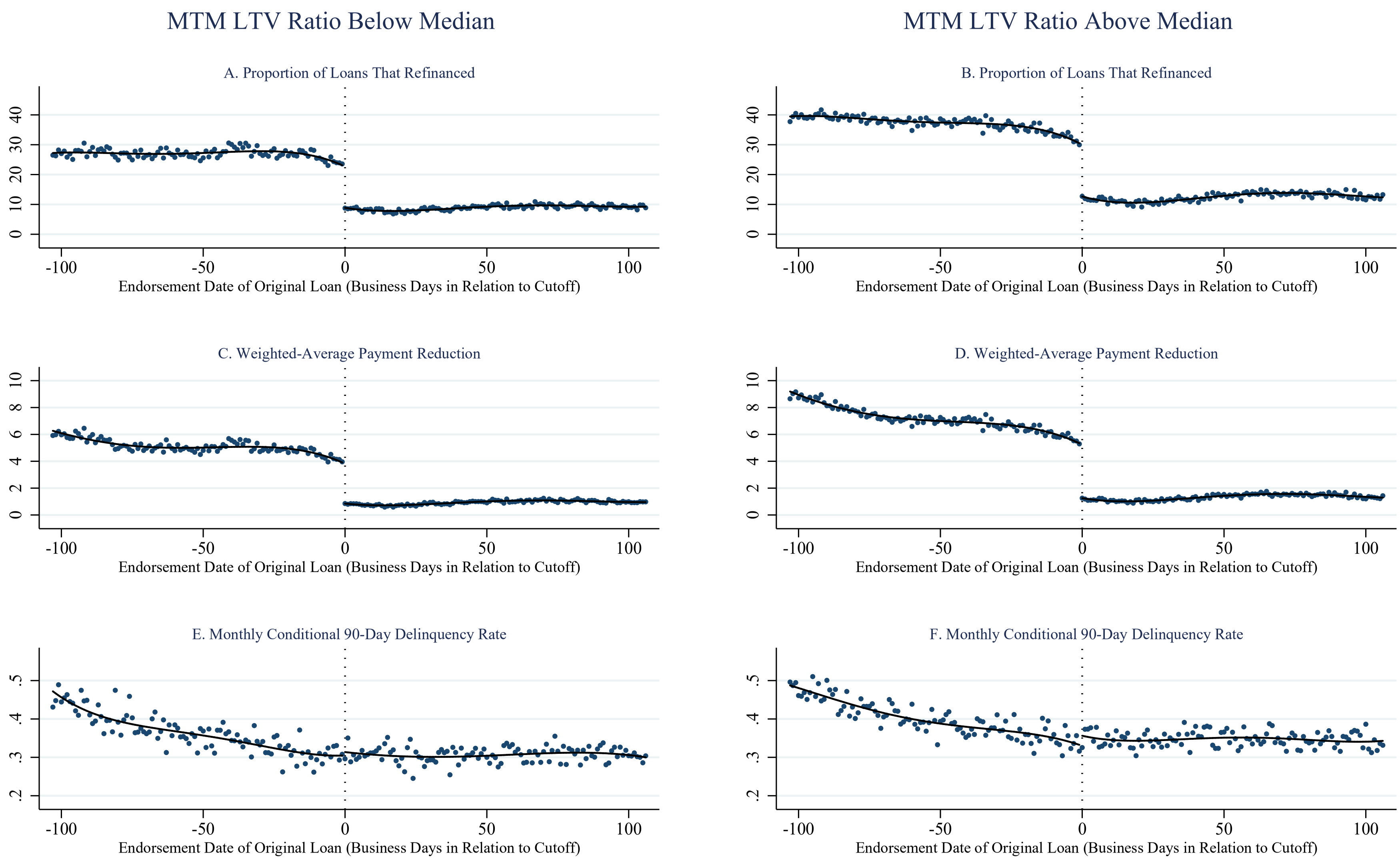
B. Borrower FICO Scores




Dashed line shows median borrower FICO score of 683.

# Figure A3: Average Loan Outcomes by Mark-to-Market LTV Ratio

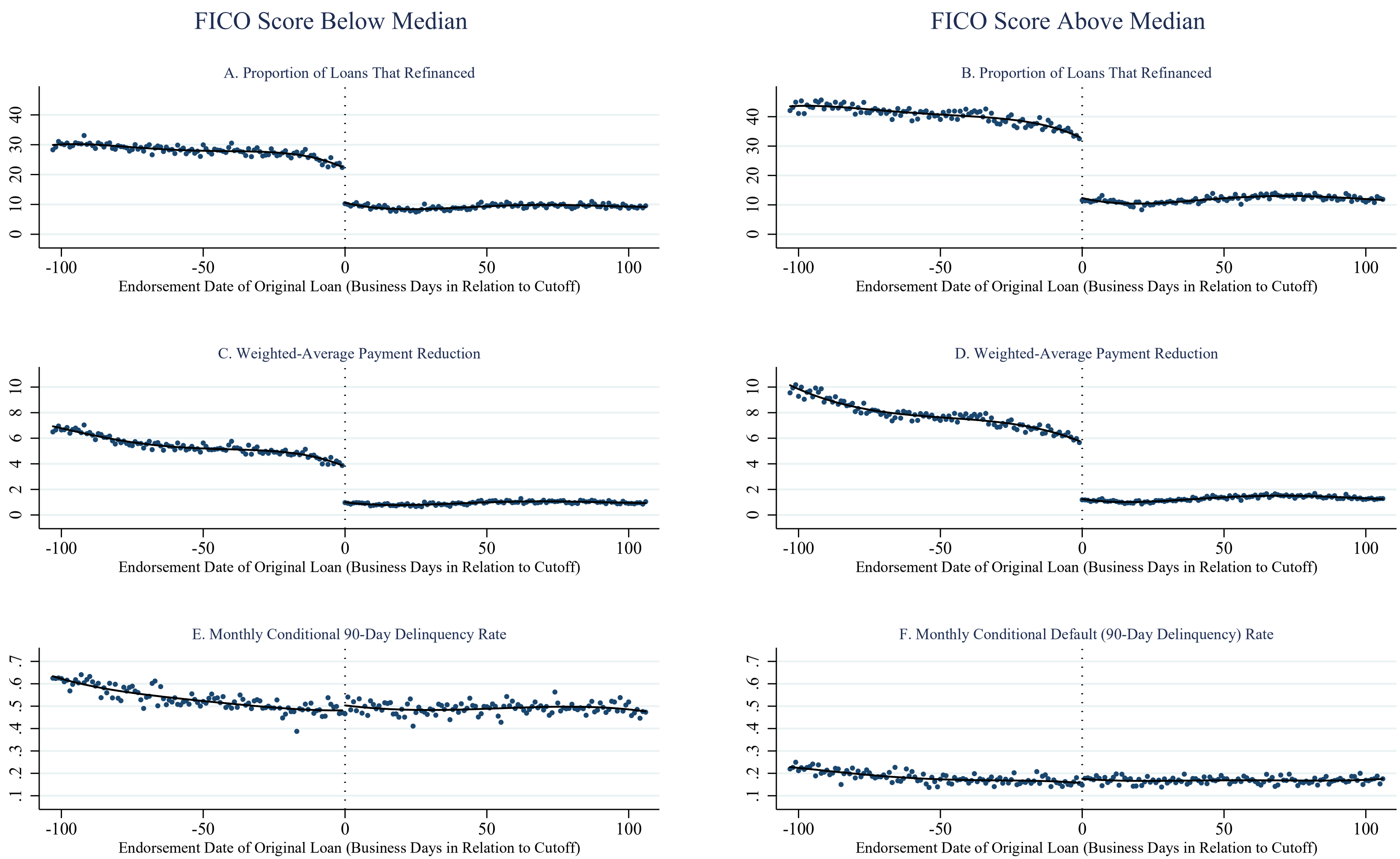
August 2012 to July 2016, Percentage Points




Note: Dots are average values by endorsement date of original loan. Lines are fitted quartic polynomials, estimated separately on either side of the cutoff.

Figure A4: Average Loan Outcomes by Borrower FICO Score

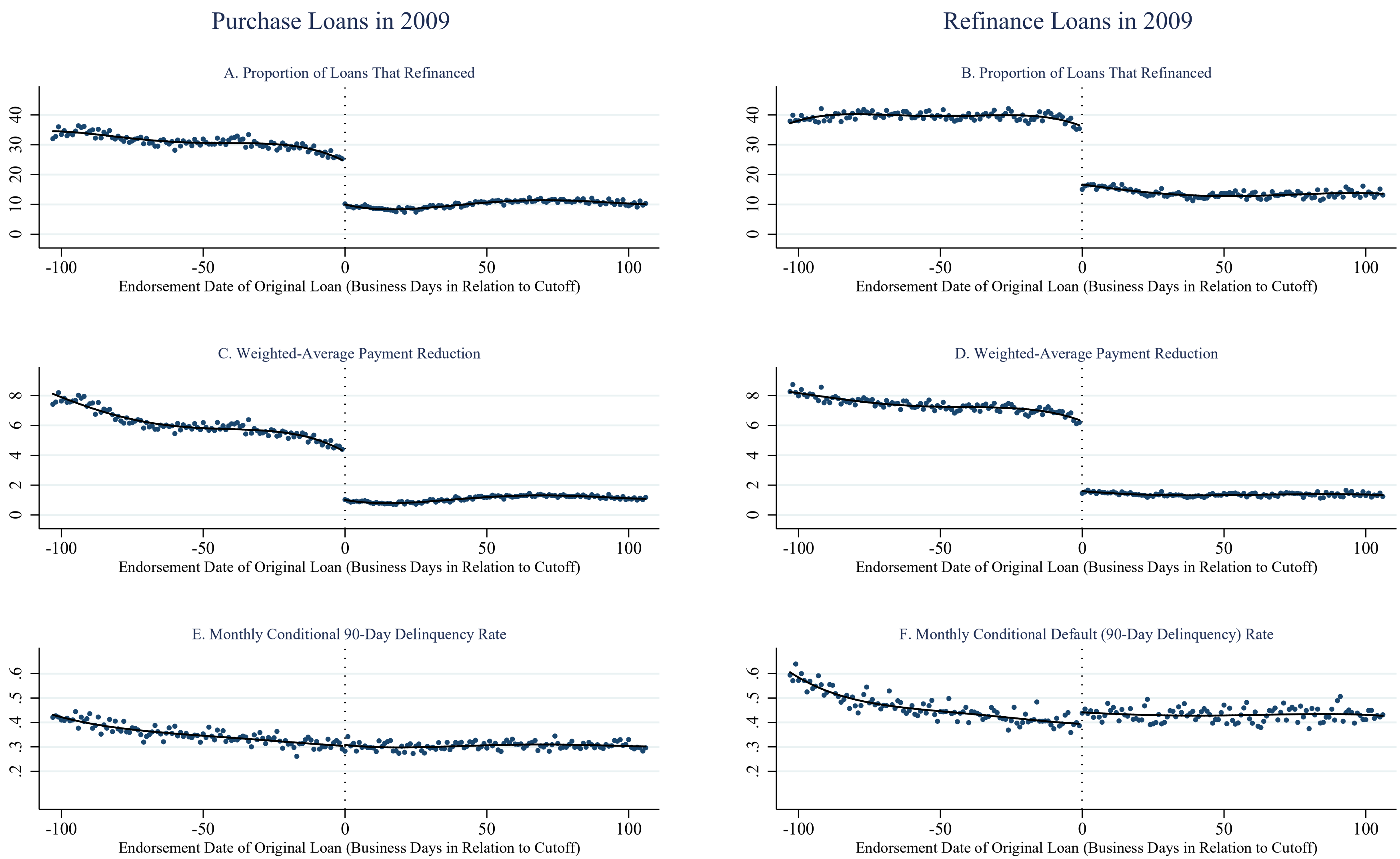
August 2012 to July 2016, Percentage Points




Note: Dots are average values by endorsement date of original loan. Lines are fitted quartic polynomials, estimated separately on either side of the cutoff.

# Figure A5: Average Loan Outcomes by Original Loan Type

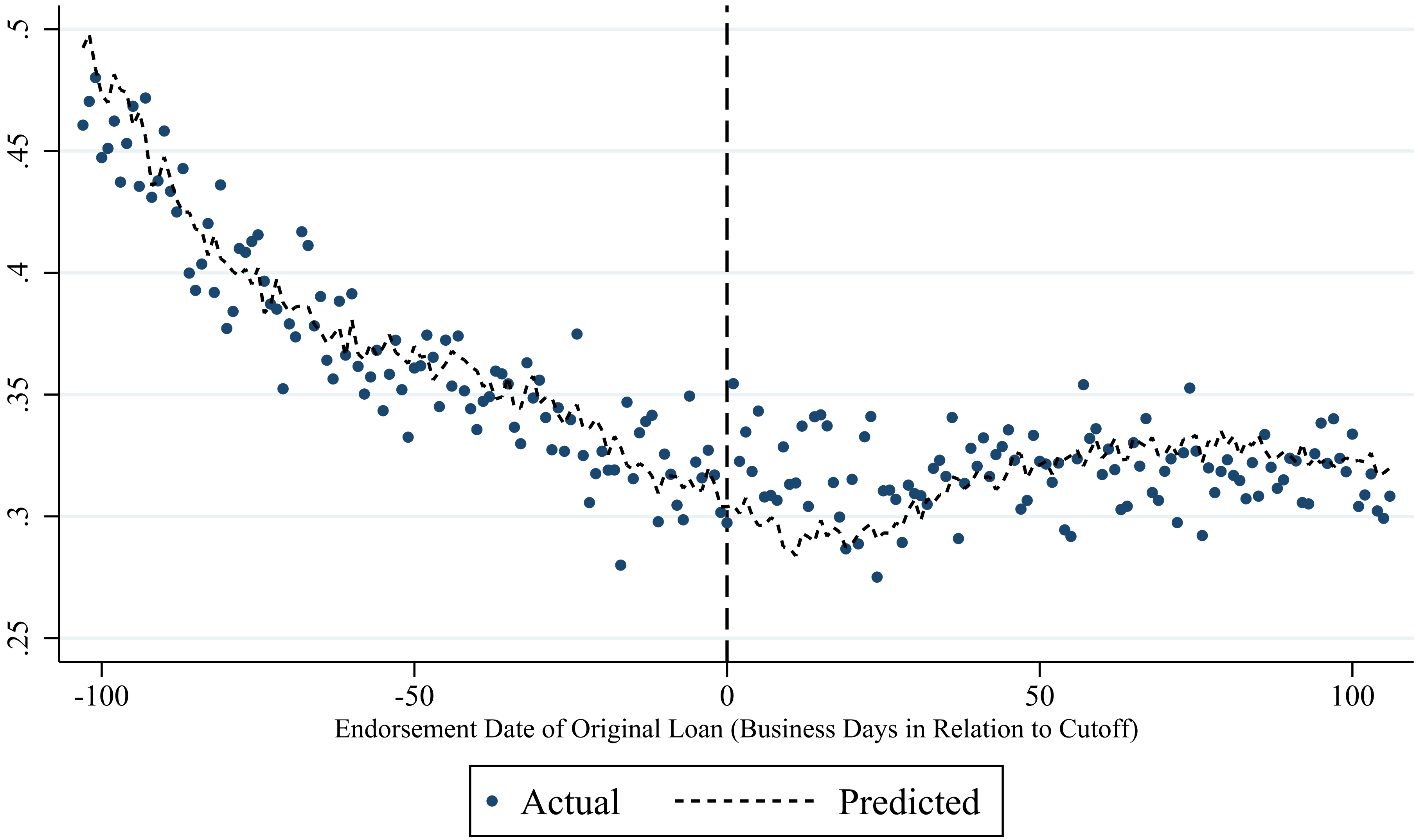
August 2012 to July 2016, Percentage Points




Note: Dots are average values by endorsement date of original loan. Lines are fitted quartic polynomials, estimated separately on either side of the cutoff.

Figure A6: Actual and Predicted Monthly 90-Day Conditional Delinquency Rates

August 2012 to July 2016, Covariate Sample, Percentage Points




Note: Covariate sample contains loans with full information for original LTV ratio, mortgage interest rate, borrower FICO score, borrower income, original refinance status, and original mortgage amount. Predicted conditional delinquency rate is from a regression of the delinquency rate on those covariates.