No Job, No Money, No Refi: Frictions to Refinancing in a Recession*

Anthony A. DeFusco† **John Mondragon**‡

PRELIMINARY
PLEASE DO NOT CITE OR CIRCULATE.

Abstract

Frictions that prevent households from refinancing their mortgages during times of economic distress can significantly inhibit policy efforts aimed at curtailing the costs of recessions. In this paper, we study two important and counter-cyclical refinancing frictions: the need to document employment and the need to pay upfront closing costs. To quantify the effect of these frictions on refinancing, we exploit a sharp policy change introduced by the Federal Housing Administration (FHA) during the height of the Great Recession that eliminated the ability for unemployed borrowers to refinance and increased the out-of-pocket closing costs for many others. We find that this policy change had very large effects on FHA borrowers; it led to a reduction in the monthly probability of refinancing of about 0.7 percentage points, which is more than 50 percent of the pre-shock average. This reduction in refinancing is concentrated among borrowers likely to be unemployed and among those newly required to pay for closing costs out-of-pocket. Taken together, our results imply a high latent demand for refinancing among the unemployed and underscore the importance of liquidity constraints as a potential barrier to credit access.

*This version: March 21, 2018. We would like to thank Brian Chappelle and George Baker for very helpful conversations. We also thank seminar participants at Northwestern, BYU, UC Berkeley (Real Estate), the Philadelphia Fed, and the Federal Reserve Board for comments and suggestions. The authors gratefully acknowledge financial support from the Guthrie Center for Real Estate Research at Northwestern University.

†Northwestern University, Kellogg School of Management: anthony.defusco@kellogg.northwestern.edu

‡Northwestern University, Kellogg School of Management: john.mondragon@kellogg.northwestern.edu

I Introduction

Frictions that prevent households from refinancing their debt during times of economic distress can significantly inhibit policy efforts aimed at curtailing the costs of recessions. This was particularly true during the Great Recession, when frictions in the U.S. mortgage market held back a broad array of policies directed at providing debt relief and economic stimulus to households. These frictions ranged from widespread levels of negative equity, which limited the ability for many households to benefit from accommodative monetary policy (Beraja et al., 2017), to competitive barriers in the mortgage market, which suppressed the take-up of federal mortgage modification and refinancing programs (Agarwal et al., 2017a,b).[1]

In this paper, we study how two previously overlooked but important frictions may contribute to a lack of refinancing during recessions. To refinance a mortgage, borrowers typically need to both document that they are employed and pay upfront, out-of-pocket closing costs. While always present, these constraints may be especially binding during recessions, when unemployment is high, income risk is elevated, and cash-on-hand is low. They are also likely to have significant distributional implications. The households who are most affected—the unemployed and the liquidity constrained—are precisely those who would benefit most from refinancing into a lower interest rate. Yet, despite their potential importance, surprisingly little is known about the extent to which these constraints actually bind in practice.

To quantify the effect of these frictions on refinancing in a recession, we exploit a sharp policy change introduced by the Federal Housing Administration (FHA) during the height of the Great Recession. Prior to late 2009, borrowers with an FHA mortgage were typically *not* constrained by out-of-pocket closing costs or employment documentation requirements. Instead, these borrowers were allowed to roll all closing costs into their new mortgage and were not required to provide any income or employment documentation so long as they refinanced into a new FHA mortgage through the FHA's Streamline Refinance (SLR) program. However, in response to the general deterioration in the mortgage market, the FHA eliminated both of these provisions from the SLR program in late 2009. Under the revised program, borrowers with negative equity had to pay for any upfront refinancing fees out-of-pocket, and unemployed borrowers were prohibited from refinancing altogether.[2] Changes in refinancing rates among FHA borrowers following the policy change should therefore be

[1] See Piskorski and Seru (2018) for a comprehensive review of the literature studying how mortgage market frictions interacted with household debt relief and restructuring attempts during the Great Recession.

[2] Crucially, the FHA did not change its policy on home equity and refinancing. FHA borrowers with negative equity were still permitted to refinance through the SLR program as long as they could pay for the closing costs and prove that they were employed.

informative about both the demand for refinancing among the unemployed and the extent to which upfront costs inhibit refinancing during a recession.

To identify the combined effect of these changes to the SLR program, we begin with a simple event study that exploits the sharp timing of the policy change. Graphical analysis reveals that refinancing rates among FHA borrowers experienced an exceptionally large and discrete fall in precisely the month that the policy changes took effect. This drop in refinancing persists even after controlling flexibly for time trends and a large set of borrower- and loan-level observables. Our estimates imply that the policy reduced the monthly probability that an FHA borrower refinanced her mortgage by at least 0.7 percentage points, which is a decline of more than 50 percent relative to the pre-shock average.

While these results strongly suggest that the policy change had a negative effect on refinancing, the event study approach cannot completely rule out the possibility that the drop in FHA refinancing was driven by concomitant macroeconomic shocks. To address this issue, we estimate difference-in-differences specifications that use the unaffected conventional (non-FHA) market as a control group. This approach is motivated by a similar graphical analysis of refinancing in the conventional market, which does not reveal any discrete changes around the time of the policy change. Including the conventional borrowers as a control group allows us to fully and non-parametrically control for aggregate trends in refinancing rates and yields very similar results to the event study analysis. Finally, to further support our approach, we estimate flexible specifications that allow the effect on FHA refinancing to vary by month and find that the differential fall in refinancing among FHA borrowers coincides exactly with the implementation of the policy change. Taken together, these results provide strong evidence that the policy changes had a large negative effect on FHA refinancing rates.

Having documented the combined effect of the new employment documentation and closing cost requirements on refinancing rates among FHA borrowers, we then turn to examining the effects of these two provisions separately. We identify these effects using a triple differences approach that compares how the post-policy fall in FHA refinancing relative to conventional refinancing varies across groups of borrowers who are more or less likely to be affected by each of the two constraints. To isolate the effect of the employment documentation requirement, we use variation in the likelihood that a borrower is unemployed based on changes in county-level unemployment rates. Specifically, we take the difference in refinancing rates between borrowers in high- and low-unemployment counties, before and after the policy, and across FHA and conventional borrowers. Our estimates show that the post-policy fall in refinancing among FHA borrowers was substantially larger in high- relative to low-unemployment counties, but that there was no differential change in refinancing behavior among conventional borrowers across these two groups of counties. Our baseline

estimate suggests that raising the county-level unemployment rate by one percentage point reduces the monthly probability that an FHA borrower refinances by about 0.1 percentage point following the policy change. These estimates are robust to the full set of controls including home equity, and the timing of the effect is consistent with the change in FHA policy. To reduce the possibility that we are picking up residual correlation between unemployment and the new need for negative equity FHA borrowers to pay for closing costs out-of-pocket, we also estimate the the effect on a subsample of borrowers that have more than sufficient levels of equity to be able to roll any closing costs into their new mortgage. These results are similar to the baseline estimates, and suggest that the differential fall in FHA refinancing in high-unemployment counties is being driven by the employment documentation requirements. Taken together, our estimates imply that unemployed borrowers have a high demand for mortgage refinancing that is constrained by the standard mortgage underwriting process requiring employment documentation.

Next, we turn to the effects of the change in how upfront costs are financed. Following the policy change, borrowers with low or even negative levels of equity could still refinance their loans through the SLR program. However, if there was insufficient equity to roll the upfront costs into the new loan, borrowers would have to pay these costs out-of-pocket. To the extent that paying the closing costs upfront was either unaffordable or suboptimal, this change could lead to a meaningful reduction in FHA refinancing even among employed borrowers. To measure this effect, we identify borrowers who likely have insufficient equity based on their initial loan-to-value ratios and changes in local house prices. We then estimate a similar triple-difference model, taking the difference between borrowers with high- and low-equity levels, before and after the policy, and across the FHA and conventional markets. We find that the inability for low-equity borrowers to roll the closing costs into the loan had very large negative effects. Our baseline estimate suggests that this friction reduced monthly refinancing rates among FHA borrowers by at least 0.6 percentage points. This estimate is robust to a broad set of controls, and the estimates are even larger when we limit the sample to counties where unemployment was low. Comprehensive data on closing costs for FHA streamlines are not generally available, but estimates of the average range from $2,000-3,000 depending on the state (Woodward, 2008). Survey evidence suggests many households would have difficulty accessing this amount of cash even in an emergency, which may explain why we find such large effects (Lusardi et al., 2011).

Forcing households to pay for closing costs out-of-pocket could also reduce refinancing even among those with sufficient liquid assets. In particular, increases in up-front costs can push the refinancing option out of the money for households who discount cash flows at a rate higher than that at which they are able to borrow. To separate this mechanism from

the liquidity effect, we construct estimates of the optimality of the refinancing option for each borrower and in each month based on the model in Agarwal et al. (2013). We then re-estimate our effects on the sample of borrowers for whom the refinancing option is still optimal even after having to pay for closing costs. The results for this subset of borrowers are nearly identical to those in our full sample, which suggests that the liquidity effect is the dominant driver of the drop in refinancing following the policy change.

Related Literature

Our paper is closely related to a growing body of work studying the relationship between household financial frictions and monetary policy. Bernanke and Gertler (1995) were among the first to emphasize the household balance sheet channel as a way of understanding how monetary policy affects the real economy. Caplin et al. (1997) and more recently Beraja et al. (2017) emphasize the role that home equity plays in amplifying and mediating interest rate changes through the mortgage refinancing channel. We build on this work by quantifying the effects of both employment documentation and closing costs on refinancing, both of which likely become more important in typical recessions. Di Maggio et al. (2017) show the large effects on household expenditures for borrowers with adjustable mortgages where declines in interest rates pass through to payments quickly. The frictions we document, because they limit the pass-through of these rates to households with fixed interest mortgages, help quantify how much less economic stimulus is being effected through interest rate reductions. Our work is also related to the mechanisms highlighted by Greenwald (2016), who emphasizes the way payment-to-income restrictions affect the ability of interest rate changes to affect credit growth. Agarwal et al. (2015b), Di Maggio et al. (2016), and Scharfstein and Sunderam (2016) examine how frictions arising from market structure and bank incentives affect the pass-through of monetary policy to households through several channels including refinancing. Finally, Auclert (2017) and Coibion et al. (2012) argue that monetary policy can have heterogeneous effects on households due to variation in wealth and income. We document that variation in income and liquidity can lead to large differences in mortgage refinancing, highlighting another channel through which differences across households interact with the transmission of monetary policy.

Our work is also related to the vast literature studying households' mortgage refinancing decisions. Much of this literature documents that households do not refinance optimally (Andersen et al., 2015; Agarwal et al., 2015a; Campbell, 2006; Chang and Yavas, 2009; Deng and Quigley, 2012; Deng et al., 2000; Green and LaCour-Little, 1999; Johnson et al., 2015; Keys et al., 2016). We depart from this approach by quantifying two real frictions that can help explain some part of observed sluggish refinancing behavior. Our results on the effects

of closing costs provide empirical support for the results in, among many others, Agarwal et al. (2013), Dunn and Spatt (2005), and Stanton (1995) who demonstrate the important role of upfront costs on refinancing behavior. We also rely on the characterization of refinancing optimality from Agarwal et al. (2013) to test for the relative importance of liquidity and upfront costs. Our emphasis on the role of income and employment documentation relates to Archer et al. (1996), who emphasize the role of payment-to-income constraints in reducing refinancing as well as Campbell and Dietrich (1983), Dickinson and Heuson (1994), and Pavlov (2001). Both our emphasis on refinancing and the FHA SLR relates our paper to Ehrlich and Perry (2015), who also study the SLR program, but focus on quasi-experimental variation in premiums to show the effects that reduced payments have on mortgage performance.

Finally, the SLR program presents an interesting complement to mortgage modification programs, which have been emphasized in the wake of the 2009 financial crisis (Adelino et al., 2009; Agarwal et al., 2011, 2017a,b; Eberly and Krishnamurthy, 2014; Ganong and Noel, 2017; Haughwout et al., 2016; Mayer et al., 2014). Our work suggests that streamlined refinancing may be a useful alternative to modification programs, which potentially suffer from competitive and moral hazard frictions restricting uptake. The benefits of the streamline program in reducing payments quickly, irrespective of property valuations and incomes, potentially apply to the GSE market as well since explicit guarantees against credit risk are also made by Fannie and Freddie when those loans are securitized. As such, our results are directly informative about the large-scale refinancing programs proposed by Lucas et al. (2011) and Boyce et al. (2012), both of which advocate for a relaxation of refinancing standards in the conventional market using elements similar to the FHA SLR program.

The rest of the paper proceeds as follows. Section II describes the institutional background for our analysis and the details of the policy shock we examine. Section III describes the data and sample we use. Section IV provides estimates of the overall effect of the policy on FHA refinancing rates. Section V presents results on the two mechanisms, unemployment and upfront costs. Section VI concludes.

II Institutional Background

The FHA was founded in 1934 to help stabilize the mortgage market during the Great Depression. Now regulated by the Department of Housing and Urban Development (HUD), one of the primary functions of the FHA is to provide access to homeownership for households unlikely to satisfy conventional mortgage underwriting requirements. To accomplish this goal, the FHA provides insurance to originators of FHA loans that fully protects against

any principal losses associated with borrower default.[3] To pay for the default insurance, the FHA charges borrowers a mortgage insurance premium (MIP). One part of the MIP is collected upfront (UFMIP) and often rolled into the mortgage, while a second part is added to the interest rate and collected monthly throughout the life of the loan. As a result, FHA mortgages typically have higher interest rates than comparable conventional loans but generally allow for higher LTVs and flexible income and credit requirements. In addition to purchase mortgages, the FHA also offers refinances, reverse mortgages, and cash-out refinances, along with both fixed and adjustable rates. During the period we study, the FHA was involved in financing nearly one out of every five new mortgages in the U.S.[4]

II.A The FHA Streamline Refinance Program

When interest rates began to fall rapidly in 1981, the FHA faced new and substantial demand to refinance a large stock of high-interest loans. In response to this demand, the FHA created the Streamline Refinance (SLR) program in October 1982. In its announcement of the program the FHA outlined that "certain types of applications to refinance existing [FHA] mortgages need not contain a standard credit report and the regular verifications of deposit and employment."[5] Later, the FHA relaxed these standards even further by dropping the requirement that borrowers obtain an appraisal for the property being financed. From the FHA's perspective, the justification for a refinancing program with such relaxed underwriting criteria is relatively straightforward. If a borrower has an FHA mortgage, then the FHA has already insured that mortgage against default. By allowing the borrower to refinance and reduce their payment, the FHA has weakly reduced the probability of default. The SLR program quickly became a standard and popular option for FHA borrowers looking to refinance. For example, during the refinancing boom from 2001–2003 nearly 70% of all FHA refinances were through the SLR program. In 2009, which is when the policy change that we study occurred, FHA streamline refinances represented roughly 6% of the total dollar volume of all refinances in the U.S., or nearly $75 billion dollars.[6]

[3]To encourage faithful underwriting, lenders are exposed to indemnification risk if their underwriting for a loan is found to be faulty or fraudulent. HUD may also ban the lender from originating new FHA loans if their default rates are significantly higher (usually 200 percent) than the average among other lenders within that same HUD "field office" jurisdiction.

[4]See Table 3 of https://www.hud.gov/sites/documents/FHA_SF_MARKETSHARE_2016Q2.PDF, which indicates that FHA loans constituted 21.1% and 17.5% of all new mortgages issued in 2009 and 2010 respectively.

[5]See https://www.hud.gov/sites/documents/82-23ML.TXT.

[6]These calculations are the authors' using data from the Actuarial Reviews of the FHA (available at https://www.hud.gov/program_offices/housing/rmra/oe/rpts/actr/actrmenu) and the FHA mortgage market share reports at https://www.hud.gov/program_offices/housing/rmra/oe/rpts/fhamktsh/fhamktqtrly. Calculations available upon request.

To use the SLR program, borrowers need to be refinancing an existing FHA mortgage and they cannot receive more than $500 cash-back, which is typically used to cover small discrepancies in prepayments or estimated escrow costs. Streamline refinances must also lower the borrower's payment unless there is a substantial reduction in the term of the mortgage. Prior to the policy change we study, lenders participating in the program were not required to document any cash that might be needed for closing nor were there any limits on the borrower's combined loan-to-value (CLTV) ratio so long as all subordinate financing retained its junior lien position.

Within the SLR program there are two primary types of refinance: non-credit qualifying with appraisal, and non-credit qualifying without appraisal.[7] The most important distinction between these two options involves restrictions on the size of the new mortgage. In the first column of Table I we provide a detailed layout of the maximum loan amounts that were permitted under both types of streamline before the policy change that we study. Without an appraisal, a borrower could finance all closing costs as well as any discount points so long as the new mortgage amount did not exceed the original principal balance of the mortgage being paid off. This was true regardless of whether the borrower's current house value placed them in positive or negative equity. If the borrower did get an appraisal, then the new mortgage was allowed to exceed the original principal balance up to a maximum of 97.75% of the newly appraised value, which could also be used to pay for any closing costs associated with the loan.[8] Neither type of streamline required lenders to check income or employment.

II.B Major Changes to the SLR Program

On September 18, 2009, HUD announced sweeping changes to the streamline program, taking effect 60 days later.[9] We focus on the two major changes to the program, which fundamentally altered access for unemployed borrowers and for borrowers with low levels of equity. First, lenders had to begin certifying that the borrower was employed with an income before extending a streamline refinance.[10] While no strict income limits were imposed, this

[7] In addition to the two non-credit qualifying options, there is also a third category of SLR referred to as credit-qualifying. Unlike the non-credit qualifying options, credit-qualifying SLRs require documentation of income, a minimum 620 FICO score, and underwriting to income ratios. However, this refinance represents a small share of FHA business and is primarily used when deleting a borrower from the mortgage or if the new refinance has substantially larger payments (due to a term reduction, for example).

[8] These maximum LTVs were imposed starting in early 2009, see https://www.nclc.org/images/pdf/foreclosure_mortgage/loan_mod/hope/lmp_hope_refinance_transactions.pdf.

[9] For the full text of the announcement see http://portal.hud.gov/hudportal/documents/huddoc?id=09-32ml.doc.

[10] In practice lenders now had to fill out and certify the income sections of the Uniform Residential Loan Application (URLA).

new requirement explicitly excluded any borrower that was unemployed or had income that was difficult to document from refinancing their mortgage, irrespective of the borrower's equity or credit score.

The second change we examine prevented borrowers with low levels of equity from rolling closing costs into the new mortgage. This resulted from a change in the treatment of refinances without appraisals. Prior to the policy change, the loan amount for SLRs without an appraisal was allowed to increase dollar for dollar with any increase in closing costs up to the original principal balance of the loan being paid off. This meant that a borrower would be able to finance her closing costs even if she had negative equity since the maximum loan amount was determined based on the amount of the original loan and not the value of the house. The change in policy eliminated this option entirely. As shown in column 2 of Table I, the maximum loan amount for streamlines without appraisals was reduced such that no closing costs could be rolled into the new mortgage.[11] In contrast, streamlines *with* an appraisal were still allowed to roll closing costs into the mortgage up to a maximum of 97.75% of the newly appraised value. Therefore if a borrower wanted to finance closing costs using the new loan, she would have to order an appraisal and that appraisal would need to indicate that the house was worth more than the remaining unpaid balance.[12] That is, she would need to have positive equity.

To summarize, the policy change completely eliminated the ability for unemployed FHA borrowers to refinance through the SLR program and increased the out-of-pocket costs of refinancing substantially for borrowers with insufficient equity. After these changes were announced, lenders in the FHA market noted that the employment and appraisal changes would likely be very important. One lender stated that these changes were a "landscape shifter," and summarized the effects as "No job? No money? No FHA loan."[13]

II.C Other Changes to SLR

In addition to the major changes outlined above, there were several other small changes to the SLR program that were announced at the same time but are unlikely to affect our results. These changes were directed primarily at reducing the extent of refinance "churning," a practice by which mortgage originators would aggressively market refinances to existing

[11] In addition to this change, HUD also began requiring that any funds needed for closing be directly verified by the lender.

[12] At the same time, HUD also began imposing a maximum CLTV of 125 percent for both types of streamlines, which could have precluded even borrowers willing to finance their closing costs from refinancing if there were junior liens present.

[13] Originally available at https://themortgagereports.com/3231/fha-streamline-refi-changes, but an archived version is housed at https://web.archive.org/web/20120604014910/https://themortgagereports.com/3231/fha-streamline-refi-changes.

borrowers to capture new origination fees despite generating no real benefit for the borrower.

To avoid this practice, HUD began imposing requirements limiting the set of outstanding FHA loans that were eligible for a streamline based on both the age of the loan and the potential benefits to the borrower. In particular, following the policy change, only loans that were at least 6 months old and for which the refinance would lead to a "net tangible benefit" for the borrower were eligible for the SLR program. The net tangible benefit requirement varied somewhat based on both the type of loan that was being refinanced (fixed-rate versus variable) and the type of loan that would be replacing it. However, for the vast majority of SLR transactions, which are fixed-to-fixed refinances, the net tangible benefit standard only required that the new monthly payment be at least five percent lower than the payment on the current loan.[14] Estimates from various sources suggest that almost all FHA refinances would have satisfied this requirement (Ehrlich and Perry, 2015; Agarwal et al., 2017b; Lambie-Hanson and Reid, 2017). However, to limit the effect of these changes on our analysis, we will restrict our sample to include only fixed rate mortgages that had been outstanding for at least 6 months as of the date of the policy announcement.

In addition to the changes targeting refinance churning, HUD also started requiring that borrowers have satisfactory payment histories to qualify for a streamline refinance. In particular, if the loan was less than 12 months old at the time of application, then the borrower was required to have made all payments on time to participate in SLR. If the loan was older than 12 months, then all payments in the last three months must have been on time and no more than one payment in the last year may have been 30 days late. In our analysis, we will also restrict our sample to include only loans that met these requirements as of the policy announcement date.

III Data and Sample

III.A Data Sources

We rely on the Loan-Level Market Analytics (LLMA) data from Corelogic for our primary analysis. The data are collected from large mortgage servicers and cover about 60% of first liens originated over the period we examine in both the agency and non-agency markets. We use a 20 percent sample of all active loans during that period. We rely on three distinct

[14] If refinancing from an adjustable-rate mortgage (ARM) to a fixed-rate mortgage (FRM), then the new rate could not be more than 200 basis points greater than the current rate on one-year FHA ARMs. Refinances from ARMs to hybrids required that the payment not increase by more than 20 percent. Finally, FRMs refinancing into ARMs required a rate that was at least 200 basis points less than the rate on the current loan.

files from the dataset. The first is a static file containing information recorded at the time of origination, including borrower characteristics (e.g., FICO, DTI, occupancy status), loan characteristics (e.g., loan amount, interest rate, LTV), property characteristics (e.g., ZIP code, property type), and an indicator for whether or not the loan is FHA insured. The second file is dynamic and records monthly performance information over the life of the loan. The performance data allow us to observe when a loan is delinquent or paid off, but does not distinguish between payoffs resulting from sales versus refinances. To address this issue we rely on the Supplemental Loan Analytics file, which uses merges (conducted by Corelogic) of the originations and performance data to public deeds records. Corelogic is then able to determine whether or not a paid off loan is a refinance or a sale so long as the new loan also appears in Corelogic's database or if this information can be inferred from the deeds data alone. Our sample is restricted to loans for which we are able to determine the payoff reason.

To construct estimates of a borrower's current equity we use the reported LTV at origination along with the house price appreciation implied by county-level house price indexes from Zillow. We then impute the current value of the borrower's home and, given the observed remaining balance from performance data, the borrower's level of equity. This estimate will suffer from error for at least two reasons. First, if the borrower's home has experienced idiosyncratic (with respect to the county) appreciation or depreciation this will not be reflected in the county-level price changes. Second, if the borrower has taken out a junior lien against the house after origination of the first loan this additional debt will not be reflected in the performance data. This means we will tend to overstate the level of equity. These issues should, if anything, attenuate our estimates. Finally, we use estimates of county-level annual unemployment rates available from the American Community Survey to measure differences in the likelihood that a borrower is unemployed. There are a number of issues with error in these estimates, which we address in Section V.

III.B Sample Selection and Description

We select our sample primarily to exclude loans that are either unlikely to refinance or that will be prevented from participating in the updated SLR program for mechanical reasons (for example, loans less than 6 months old or with recently missed payments). In this way we focus on a set of loans that are candidates for refinancing and that will only be affected by the changes in employment documentation and closing costs.

We limit the sample to owner-occupied loans secured by single-family homes as the FHA program has distinct procedures for condos and investor or second homes that also changed over this period. We also limit our analysis to fixed rate interest loans as it simplifies the

question of whether or not a borrower is likely to benefit from a refinance. Within this subsample we drop any loans with an interest rate below 6 percent to help ensure that refinancing would substantially reduce the payment so that the net tangible benefit requirement is not binding. Changing this restriction does not substantially alter our analysis. We drop loans that do not have a recorded payoff reason, loans with initial balances or sales prices that are less than $30,000, loans that appear to have a invalid ages, invalid amortization behavior, or loans with insufficient information to check if they satisfy the payment history requirements.

Table II reports statistics for the set of loans satisfying our selection criteria and active in August 2009, one month before the policy change. FHA loans tend to be smaller, younger, have lower FICO scores, and higher LTVs (and so less equity). However, FHA and conventional borrowers have similar DTIs and interest rates (conditional on having a rate above six). FHA borrowers make up about 18 percent of all active loans in our sample.

IV The Combined Effect of SLR Policy Changes

The changes to the SLR program announced by the FHA in 2009 may have led to a reduction in refinancing among FHA borrowers for two primary reasons. First, the new requirement that lenders document income explicitly excluded unemployed borrowers from refinancing through the program. Second, the reduction in the maximum loan amount for streamlines without an appraisal meant that underwater FHA borrowers who wanted to refinance would now need to pay for any upfront closing costs out-of-pocket. In this section, we estimate the combined effect of these two policy changes on FHA refinancing rates. Later, we will also examine the importance of each of these two channels separately.

IV.A Empirical Strategy

Event Study

To estimate the overall average effect of the policy changes, we use two alternative empirical strategies that leverage different aspects of our data. The first is a simple event study that compares refinancing behavior before and after the policy change for a group of similar FHA borrowers while flexibly controlling for aggregate trends in refinancing as well as a broad set of loan-level and time-varying observables that are typically considered to be important inputs into a household's decision to refinance. This approach exploits the discrete timing of the policy change as the primary source of identification. The key identifying assumption is that the probability an FHA loan refinances would have evolved smoothly over time in the

absence of the policy change. We will provide direct graphical evidence in support of this assumption below by showing that FHA refinancing rates tended to evolve smoothly in all months during our sample period except the month that the policy went into effect, when there was a large and discrete drop.

To implement this approach, we estimate versions of the following monthly, loan-level panel regression:

$$Refinance_{it} = \alpha + X'_{it}\gamma + \beta_0 \cdot Post_t + \delta_0(t-\tau) + \delta_1(t-\tau) \cdot Post_t + \epsilon_{it}, \tag{1}$$

where $Refinance_{it}$ is an indicator variable denoting whether or not loan i refinances in month t and X_{it} is a vector of loan-level and possibly time-varying observables. The indicator variable $Post_{it}$ takes the value one if month t falls on or after January 2010, the first month after the policy change.[15] The coefficient of interest is β_0, which measures the change in the average rate of refinancing among FHA borrowers after the policy has taken effect. To ensure that this coefficient will reflect only the discontinuous change in refinancing induced by the policy, we also include linear time trends which we allow to differ before and after the date of the policy change (τ = January 2010). These trends control for general changes in the likelihood of refinancing over time. If income documentation requirements or the need to pay for closing costs out-of-pocket are important barriers to refinancing, then we should expect to find $\beta_0 < 0$. Standard errors are clustered by Core-based statistical area (CBSA) in all specifications.

One potential issue with this specification is that it does not allow for any anticipation effects. The policy changes were announced in late September 2009, which was a full two months before they took effect. There is some anecdotal evidence that lenders were aware of this and took efforts to notify potential clients of the need to refinance ahead of the changes.[16] To the extent that this behavior was widespread and borrowers decided to refinance early, this could lead us to overestimate the effect of the policy since it would generate a higher refinancing rate in the pre-period. To account for this, we will also estimate specifications that include an additional indicator variable marking periods of time subsequent to the announcement of the policy. In particular, we estimate the following modified version of

[15] While December 2009 was the first full month when SLR applications had to abide by the new rules, due to the amount of time it takes for loans to close, many of the loans with applications prior to the deadline would likely not be recorded as refinanced until 30 or more days later. Therefore, we will always treat January 2010 as the first "post-policy" month.

[16] For example, https://themortgagereports.com/3231/fha-streamline-refi-changes.

equation (1):

$$Refinance_{it} = \alpha + X'_{it}\gamma + \beta_0 \cdot Post_t + \delta_0(t-\tau) + \delta_1(t-\tau) \cdot Post_t + \beta_1 \cdot Post_t^{News} + \delta_2(t-\tau^{News}) \cdot Post_t^{News} + \epsilon_{it}, \quad (2)$$

where $Post_t^{News}$ is an indicator variable that takes the value of one if month t falls on or after September 2009. As in the baseline specification, we allow the linear time trend to differ for months following the policy announcement (τ^{News} = September 2009). A small and statistically insignificant estimate of β_1 would suggest limited evidence of borrower anticipation.

Difference-in-Differences

One disadvantage of the event study approach is that it cannot account for sharp changes in outcomes that would have occurred even in the absence of the policy change. This is an especially important concern in our context because refinancing probabilities often exhibit large changes when interest rates begin to rise or fall. To address this issue, we also provide estimates based on a difference-in-differences strategy leveraging the fact that the policy changes had no effect on refinancing options for borrowers with conventional (non-FHA) mortgages. If movements in household expectations about interest rates or other macroeconomic factors caused a large change in refinancing at the same time as the policy change, this effect should manifest itself similarly among both conventional and FHA borrowers. Therefore, by netting out any changes in refinancing among conventional borrowers, we will be able to isolate the effect of the policy change alone.

The baseline specification that we use to implement this approach is a standard differences-in-differences regression estimated at a monthly frequency using the full sample of both conventional and FHA loans. Specifically, we estimate regressions of the following form:

$$Refinance_{it} = \alpha + \delta_t + X'_{it}\gamma + \beta_0 \cdot FHA_i + \beta_1 \cdot FHA_i \times Post_t + \epsilon_{it}, \quad (3)$$

where δ_t is a vector of fixed effects for the month of observation and FHA_i is an indicator for whether or not loan i is FHA insured. The coefficient of interest is β_1, which measures the differential change in refinancing among FHA borrowers relative to conventional borrowers following the implementation of the SLR policy changes. This difference is conditional on a broad set of loan and borrower characteristics as well as time and geographic-specific factors.

The standard identifying assumption in this framework is that trends in FHA and conventional refinancing would have evolved in parallel in the absence of the policy change. In our context, the interpretation of this assumption requires some care. The nature of the

policy change that we study was to make underwriting standards in the FHA market more similar to those in the conventional market. Prior to the policy change, FHA borrowers had easier access to refinancing than conventional borrowers. In particular, during the pre-period unemployed and underwater conventional borrowers would have typically been shut out of the market, whereas FHA borrowers would have still been able to refinance through the SLR program. Because employment and house prices were both falling during that time, this may have led to a decline in refinancing among conventional borrowers relative to FHA borrowers. This would violate the parallel trends assumption and lead us to *underestimate* any relative decline in FHA refinancing subsequent to the policy change. To account for this possibility, our set of control variables will always include a linear time trend for FHA borrowers. As in the event study analysis, this trend will be allowed to vary freely before and after the policy change. Below, we will provide graphical evidence showing that, conditional on theses trends and the other controls that we include, refinancing rates in the two market segments evolved in parallel prior to the policy change.

IV.B Results

Graphical Evidence

As motivation for our empirical strategy, we begin by presenting simple graphical evidence indicating that the refinancing rates of FHA borrowers experienced a discontinuous and dramatic decline in exactly the month that the SLR policy changes went into effect. In Figure I we plot the raw unconditional probability that a loan refinanced during each month leading up to and after the policy changes. These refinancing rates are plotted separately for FHA (Panel A) and conventional loans (Panel B). The vertically dashed grey line in January 2010 marks the first post-policy month. In this figure and throughout the paper we multiply all refinancing rates by 100, so that a value of one would imply a one percent probability of refinancing in a given month.

Panel A of the figure shows that FHA refinancing rates fluctuated between roughly 0.6 and 1 percent prior to the policy, but then dropped sharply in January 2010 to 0.25 percent. For visual reference, the orange dashed lines plot the fitted values from a regression of the monthly refinancing probabilities on a linear time trend fit separately on either side of the policy change. These trends indicate that the refinancing rate among FHA borrowers fell by roughly 0.7 percentage points in precisely the month that the new restrictions to the SLR program went into effect and remained low for the remainder of the sample period. The large and discontinuous nature of this drop provides strong evidence in support of our event study approach. In panel B, we plot the analogous figure for conventional loans. While there is a

slight difference in pre-trends between the two groups of loans, both appear to evolve roughly linearly prior to the policy change and there is no evidence of a drop in refinancing among conventional borrowers. Because we will always allow for separate linear trends between FHA and conventional loans, these results also lend support for the difference-in-differences strategy.

Event Study Results

Table III presents our main results from the event study analysis. The first two columns report estimates from the baseline specification given by equation (1). In column one, we include only CBSA fixed effects and the linear trends. The coefficient on the *Post* dummy indicates that the change in policy reduced the monthly probability that an FHA loan refinanced by 0.7 percentage points. This estimate lines up closely with the raw averages reported in Figure I and is large relative to the pre-period refinancing rate of roughly 0.6 to 1 percent. In the second column, we control non-parametrically for a host of loan and borrower characteristics that may also be important determinants of the likelihood of refinancing. To control for time-varying drivers of the demand for refinancing, we include fixed effects for the current age (one-year bins), interest rate (one-percentage point bins), and decile of the distribution of estimated home equity associated with the loan. To control for differences in borrower characteristics at origination, we further include a full set of 50-point FICO score bins, 10-point LTV bins and the pairwise interaction between the two. Including these controls has no meaningful effect on the result. The estimate reported in column two remains statistically significant at the one-percent level and implies that the policy changes led to a reduction in FHA refinancing rates of roughly 0.75 percentage points.[17]

In columns 3 and 4 we report analogous estimates from the modified event study specification given by equation (2). This specification allows for the possibility that borrowers may have tried to front-run the policy changes by refinancing early in response to the news that was released several months before changes actually took effect. The results suggest limited evidence of this type of anticipation effect. The coefficient on the $Post^{News}$ dummy is statistically insignificant, negative, and close to zero in both specifications. Moreover, including this coefficient and allowing for a separate linear time trend during the period between the announcement and implementation of the policy changes has essentially no effect on the magnitude of the main coefficient reported in the top row. Taken together, the estimates reported in this table suggest that the new constraints introduced by the SLR policy

[17] Adding these controls reduces the sample size since not all controls are available for every loan. This is due primarily to home equity which is constructed from local house price indexes that are not always available. We include these loans in the previous specification for completeness, but our results are not sensitive to this choice.

changes led to a reduction in refinancing among existing FHA borrowers of roughly 0.7 to 0.75 percentage points per month.

Difference-in-Differences Results

The event study results are largely confirmed by our difference-in-differences analysis, which compares not only how refinancing behavior changes following the implementation of the policy, but also whether the change in behavior is differential across FHA and conventional borrowers. In the first column of Table IV, we report estimates from a baseline version of the difference-in-differences specification given by equation (3). In this baseline regression, we control only for the month of observation, the CBSA of the property, and a linear time trend for FHA borrowers that is allowed to vary before and after the policy change. The coefficient of interest is reported in the second row and implies that the changes to the SLR program reduced FHA refinancing rates by 0.68 percentage points. This estimate is statistically indistinguishable from the 0.7 percentage point reduction implied by the event study analysis. It is also large enough to more than offset the gap in refinancing rates that existed between FHA and conventional borrowers just prior to the policy change as indicated by coefficient estimate on the FHA dummy reported in the first row.

FHA and conventional borrowers differ along a broad set of observables. Because of this, one concern might be that differences in these observables would lead to large differences in refinancing rates that could confound our estimates. In column 2, we begin to address this issue by controlling flexibly for all of the same characteristics included in our event study analysis (loan age, interest rate, current equity, LTV, and FICO). When we include these controls, the resulting estimate is statistically indistinguishable and nearly identical in magnitude to the baseline effect reported in the first column. In column 3, we further interact all of the additional controls added in column 2 with the Post dummy. This allows for each borrower or loan characteristic to have a separate and time-varying effect on the likelihood of refinancing. If anything, allowing for this additional flexibility only increases the size of the implied drop in FHA refinancing caused by the policy change. Finally in column 4, we further interact all of the borrower and loan-level controls with the FHA indicator. This allows for the possibility that FICO scores, for example, are differentially informative about refinancing behavior for FHA borrowers relative to conventional borrowers. Allowing for these observables to vary with the type of loan gives an almost identical estimate. Across all of the specifications, we find robust evidence that borrowers with FHA loans are much less likely to refinance after the policy change relative to conventional borrowers. The size of this gap is large and indicates that the change in SLR policies led to a reduction in FHA refinancing of rough 0.7 to 0.8 percentage points.

Finally, to give a sense of the dynamics of this effect, we estimate a more flexible version of the difference-in-differences specification that allows for the effect to vary by month. Specifically, we estimate a regression of the following form:

$$Refinance_{it} = \alpha + \delta_t + X'_{it}\gamma + \sum_{\tau} \left[\beta_\tau \cdot FHA_i \times \mathbb{1}_{t=\tau} \right] + \epsilon_{it}, \tag{4}$$

where $\mathbb{1}_{t=\tau}$ is an indicator variable taking the value one if month t is equal to τ (e.g. December 2009). The β_τ coefficients from this regression provide a non-parametric measure of the differential trend in refinancing rates among FHA borrowers relative to conventional borrowers. We normalize the coefficient for December 2009 to zero, so that all estimates can be interpreted as the difference in refinancing rates between FHA and conventional borrowers in a given month relative to the corresponding difference in the month just prior to the policy changes. We include all of the same controls as in column 4 of Table IV but, instead of interacting these controls with just a single Post dummy, we allow for a full set of interactions with each of the month fixed effects. If these observables are able to effectively control for any differences in pre-trends, then we should expect to find $\beta_\tau = 0$ in all months prior to December 2009.

In Figure II, we plot these coefficients along with their 95 percent confidence intervals. The figure shows that, conditional on the controls we include, trends in refinancing rates between FHA and conventional borrowers evolved in parallel up until the month of the policy change.[18] However, starting in immediately the month of the policy change, there is a discrete drop in refinancing among FHA borrowers. The magnitude of this drop is roughly 0.5 percentage points, which is slightly smaller than the estimates from Table IV but still economically quite large. Compared to the pre-policy rate, it suggests that the new restrictions to the SLR program reduced FHA refinancing rates by at least 50 percent of the baseline.

V Mechanisms

Our results thus far indicate that the new income documentation requirements and restrictions on financing closing costs collectively led to a large reduction in FHA refinancing. In this section, we investigate heterogenity in this response across borrowers to study how these two major program changes separately contributed to the drop in refinancing. Although oc-

[18] While the small spike in October 2009 provides some evidence of borrower anticipation that is not apparent from the event study analysis, the magnitude of this spike is small relative to the sustained drop following the policy change and is therefore unlikely to meaningfully affect our estimates.

curring at the same time, these two changes affected observably distinct sets of borrowers, which allows us to plausibly trace out their respective effects.

V.A Income Documentation Requirements

Graphical Evidence

The change in income documentation requirements introduced in 2009 fundamentally altered the nature of the SLR program. Prior to this change, FHA borrowers were able to qualify for a streamline refinance regardless of their income or employment status. This meant that unemployed borrowers had the same access to refinancing as any other FHA borrower. However, the FHA changed this when it began requiring lenders to verify employment as a condition for receiving a streamline refinance.

As evidence that this new constraint was binding, Figure III plots unconditional refinancing rates by month separately for FHA and conventional loans and across groups of borrowers that are more or less likely to be unemployed at the time of the policy change. To proxy for the likelihood that a borrower is unemployed, we use changes in county-level unemployment between 2006 and 2009 and categorize loans into "high" and "low" unemployment groups based on whether they fall into the top or bottom third of the (loan-count weighted) distribution of these changes.[19] In Panels A and B, we plot monthly refinancing rates for FHA borrowers in the high- and low-unemployment groups, respectively. Panels C and D plot the analogous refinancing rates for conventional borrowers.

Comparing across the panels in the top row shows that FHA borrowers in high unemployment counties had substantially higher rates of refinancing than FHA borrowers in low unemployment counties before the policy change. This suggests that unemployed borrowers refinance at a higher rate than their employed counterparts when they are able to do so. After the policy change, however, there is a discrete drop in refinancing and both groups of FHA borrowers begin to refinance at similar rates. The fact that the drop in refinancing was nearly twice as large for the high-unemployment group is consistent with the idea that the new income documentation requirements were more binding for this group. In contrast, refinancing rates in the conventional market (Panels C and D) remain constant around the time of the policy change and are always somewhat higher in counties with smaller increases in unemployment. While not conclusive, these results suggest that unemployed FHA borrowers had a high demand for refinancing during the pre-period that was substantially constrained

[19]We use changes in unemployment as our proxy rather than levels to address the fact that county-level unemployment rates exhibit substantial noise, particularly in counties with high unemployment. Taking the change alleviates this issue as the ACS reported measurement error is uncorrelated with the change in most of the sample.

by the new income documentation requirements.

Empirical Strategy

To more formally analyze the effect of the change in income documentation requirements, we use a triple-differences strategy that is directly motivated by the results in Figure III but which allows us to control for many other factors that are correlated with unemployment and also related to refinancing. The idea behind this strategy is to compare changes in refinancing behavior before and after the policy change across groups of FHA borrowers who are more or less likely to be unemployed while using similar changes in the conventional market as a counterfactual for what would have happened in the absence of the policy. As in Figure III, we use changes in county-level unemployment as a proxy for the likelihood that a borrower is unemployed. The identifying assumption in this context is that, conditional on the controls we include, the differential change in FHA refinancing rates across counties that experienced high and low changes in unemployment would have paralleled that in the conventional market in the absence of the policy.

We implement this approach by estimating versions of the following triple-differences regression:

$$\begin{aligned} Refinance_{it} = \alpha + \delta_t + X_{it}'\gamma + \beta_0 \cdot FHA_i + \beta_1 \cdot \Delta UR_i \\ + \beta_2 \cdot FHA_i \times Post_t + \beta_3 \cdot \Delta UR_i \times Post_t + \beta_4 \cdot FHA_i \times \Delta UR_i \\ + \beta_5 \cdot FHA_i \times \Delta UR_i \times Post_t + \epsilon_{it}. \end{aligned} \tag{5}$$

In this specification, the variable ΔUR_i measures the change in the unemployment rate from 2006 to 2009 in borrower i's county, and all other terms are as previously defined. As in the difference-in-differences analysis above, the set of controls X_{it} will always include FHA-specific linear time trends that are allowed to differ before and after the policy change. These trends are included to adjust for the fact that FHA borrowers may have had a differential capacity to refinance in response to the deterioration in economic conditions leading up to the policy change. The coefficient of interest is β_5, which provides a measure of how much FHA refinancing rates fall relative to conventional loans following the policy change and as the likelihood of unemployment increases. If income documentation requirements were an important barrier to refinancing during this period, we should expect to find $\beta_5 < 0$.

One key advantage of this specification is that it allows us to control completely flexibly for the borrower's level of home equity. While FHA borrowers could qualify for a streamline refinance regardless of home equity throughout the entire sample period, only borrowers with positive equity were able to roll closing costs into their loans subsequent to the policy change.

Because house prices and unemployment are highly correlated during this period, a simple comparison that does not control effectively for home equity would risk conflating the effect of the income documentation requirements with the increased upfront costs for negative equity borrowers. We will address this issue in two ways. First, we will estimate versions of the specification that include controls for the complete interaction between the FHA indicator, the Post indicator, and a set of dummies for the borrower's equity. This will allow for home equity to have a separate effect on refinancing for FHA and conventional borrowers both before and after the policy change and should therefore control for any independent effect of the new closing cost requirements. Second, we will also provide results based on a restricted subset of borrowers for whom we estimate relatively high levels of equity. This group of borrowers is able to finance their closing costs using the new loan both before and after the policy change and should therefore only be affected by the change in income documentation requirements.

Results

Table V presents our estimates of the effect of the change in income documentation requirements on FHA refinancing. Column 1 reports estimates from a baseline version of the triple-differences regression that includes only the FHA time trends and a set of month and CBSA fixed effects as controls. The coefficient estimate on the triple interaction term in the bottom row implies that the policy-induced drop in refinancing rates for FHA borrowers increases by roughly 0.13 percentage points for each one percentage point increase in the county-level unemployment rate. For reference, we also report the average change in county-level unemployment in the top row of the bottom panel, which was 2.71 percentage points. At that change in unemployment rates, this effect is enough to account for a reduction in refinancing of 0.36 percentage points per month, which is roughly half the size of the overall effect reported in Table IV.

In the remaining columns of the table, we add a series of control variables that increasingly restrict the nature of the variation being used to identify how the fall in FHA refinancing depends on local employment conditions. In column 2, we include the same detailed set of fixed effects for loan-level characteristics that were included in our analysis of the overall effect of the policy change (loan age, interest rate, current equity decile, and LTV-by-FICO bins). Column 3 further interacts these controls with the FHA dummy and the post indicator. In both cases, the coefficient on the triple interaction term remains negative, qualitatively similar to, and statistically indistinguishable from the baseline estimate in column 1. In column 4, we allow the controls for home equity to enter even more flexibly by interacting each equity decile fixed effect not only with the FHA and Post dummies, but also with their

interaction. This specification explicitly controls for the differential effect that the change in FHA policy may have had on FHA borrowers through the importance of equity and its relation to closing costs. The estimate is slightly smaller but statistically indistinguishable from the baseline, which provides confidence that these results primarily reflect the effect of the change in income documentation requirements.

Finally, in column 5, we restrict the sample to a set of borrowers that are estimated to have more than sufficient equity to finance any reasonable closing costs (at least $20,000). For this group of borrowers, the only relevant change in FHA policy was to the income documentation requirements. The coefficient estimate in this subsample is similar to column 4 and implies that each one percentage point increase in county-level unemployment is associated with an additional 0.1 percentage point fall in FHA refinancing relative to conventional refinancing following the policy change.

To further explore the robustness of this relationship, in Figure IV we report estimates from two alternative and more flexible parameterizations that allow for the effect to vary either by month of observation or non-linearly with the change in local unemployment. Specifically, in Panel A, we plot coefficient estimates from the following specification:

$$\begin{aligned} Refinance_{it} = \alpha + \delta_t + X'_{it}\gamma \\ + \sum_{\tau} \mathbb{1}_{t=\tau} \cdot \Big[\beta_{0\tau} \cdot FHA_i + \beta_{1\tau} \cdot \Delta UR_i + \beta_{2\tau} \cdot FHA_i \times \Delta UR_i\Big] + \epsilon_{it}. \end{aligned} \tag{6}$$

This specification allows for separate monthly coefficients on FHA status, changes in local unemployment, and the interaction between the two. In the figure, we plot the $\beta_{2\tau}$ coefficients, which measure how the gap between FHA and conventional refinancing is related to local unemployment rates during each month in our sample period. As before, we normalize the coefficient for December 2009 to zero, so that each estimate can be interpreted as the effect relative to the month just prior to the policy changes. Although the estimates have relatively wide confidence intervals, there is a clear, large, and discrete drop in precisely the month that the policy takes effect. This provides confidence that the results in Table V are being driven directly by the policy change and not some other omitted factor.

In Panel B, we report results from a similar regression which instead allows the effect to vary non-linearly with the size of the change in local unemployment. Specifically, we group local unemployment changes into quintiles $q \in \{1, ..., 5\}$, and plot coefficients from the following specification:

$$Refinance_{it} = \alpha + \delta_t + X'_{it}\gamma + \sum_{q} \mathbb{1}_{\Delta UR_i \in q} \cdot \Big[\beta_{0q} \cdot FHA_i + \beta_{1q} \cdot FHA_i \times Post_t\Big] + \epsilon_{it}, \tag{7}$$

In this specification, the indicator variable $\mathbb{1}_{\Delta UR_i \in q}$ denotes whether the change in county-level unemployment for borrower i falls into quintile q. The β_{1q} coefficients from this regression provide a non-parametric measure of how the effect of the policy varies as the change in local unemployment rises. These coefficients are plotted in Panel B along with their 95 percent confidence intervals. We normalize the coefficient for the first quintile to zero so that all effects can be interpreted relative to the effect in the set of counties experiencing the smallest increases in unemployment. The coefficients show large and weakly monotonic declines in FHA refinancing rates as unemployment changes become larger. This provides reassurance that the results in Table V are not simply an artifact of the linear functional form that we use.

Taken together, we view these results as compelling evidence that the new income documentation requirements were a substantial barrier to refinancing for FHA borrowers during this period. However, it is important to note that the *magnitude* of these estimates may not extrapolate to the general population. In particular, given their demographic characteristics, it is likely that changes in local unemployment load more strongly on FHA borrowers relative to the average household. That is, a one percentage point increase in county-level unemployment may translate into a greater than one percentage point increase in unemployment among FHA borrowers. If true, this would lead us to over-estimate the effect of employment documentation requirements for the typical borrower.

To provide a sense of how large this bias may be, we use data from the Survey of Consumer Finances (SCF) to measure how the change in unemployment between 2007 and 2009 among households with an outstanding FHA mortgage in 2007 compared to the same change for all households over that period. For FHA borrowers, the unemployment rate in the SCF increased by 7.8 percentage points over this period, whereas the increase among all households was only 6.2 percentage points. If we assume that changes in county-level unemployment load similarly on the two groups of borrowers, this would imply that a one percentage point increase in the local unemployment rate translates into a roughly 1.25 (7.8/6.2) percentage point increase in unemployment for FHA borrowers. Even with this scaling, however, the results in Table V imply substantial effects of employment documentation requirements on refinancing rates. For example, dividing the coefficient estimate in the bottom row of the first column by 1.25 would imply that the likelihood of refinancing for a borrower in the average county was roughly 0.29 percentage points lower than it otherwise would have been as a result of the fall in employment between 2006 and 2009.[20]

[20]This calculation is based on the average change in county-level unemployment reported in the bottom panel of Table V: $\frac{-0.133}{1.25} \times 2.71 = -0.29$.

V.B Upfront Costs

The second major change to the SLR program was the reduction in maximum loan amounts for streamline refinances without an appraisal. As discussed in Section II, this change eliminated the ability for negative equity borrowers to roll the upfront closing costs of refinancing into their new loan. Instead, after the policy change, these borrowers would now have to pay for any upfront costs out-of-pocket. To study the effects of this change on FHA refinancing rates, we proceed in the same manner as in our analysis of the employment documentation requirements. First, we present simple graphical evidence indicating that this new constraint appears to have had a larger effect on refinancing among borrowers that were more likely to have been affected by it. Second, we estimate triple-differences regressions that are motivated by this evidence and which allow us to more precisely quantify the extent to which the need to pay for closing costs out-of-pocket constrains refinancing.

Graphical Evidence

Figure V presents unconditional monthly refinancing rates for FHA and conventional borrowers with differing levels of home equity. We categorize borrowers into "high," and "low" equity groups based on whether their estimated home equity in the month of observation is greater than or equal to $20,000 or less than or equal to $0, respectively.

Panels A and B plot refinancing rates separately for high- and low-equity FHA borrowers. Comparing across these panels reveals that both positive and negative equity FHA borrowers experienced a discrete fall in refinancing in the month of the policy change. However, this fall was nearly four times as large for negative equity borrowers. Importantly, this differential fall is not a direct result of negative equity itself, since both groups of borrowers were still permitted to refinance through the SLR program provided that they could pay the upfront costs. Rather, it is consistent with the idea that the new need to pay for closing costs out-of-pocket, which only affected negative equity borrowers, was a binding constraint on refinancing during this period.

Panels C and D of the figure plot the analogous refinancing rates for conventional borrowers and show that there was essentially no change in refinancing among either group around the date of the policy change. These figures also make clear that negative equity itself *was* a binding constraint in the conventional market. Unlike in the FHA market, high-equity conventional borrowers refinance at substantially higher rates than their low-equity counterparts in every month. Taken together, these results suggest that requiring negative equity FHA borrowers to pay for closing costs out-of-pocket may have created a substantial barrier to refinancing.

Empirical Strategy

To more precisely quantify the magnitude of these effects, we use a triple-differences framework that directly parallels the approach we used to estimate the income documentation effects in Section V.A. The idea is to compare changes in refinancing between high- and low-equity FHA borrowers relative to conventional borrowers following the policy change while controlling flexibly for other potential drivers of refinancing. To do this, we categorize borrowers into "high" and "low" equity groups and estimate the following triple-differences regression:

$$\begin{aligned} Refinance_{it} = \alpha + \delta_t + X'_{it}\gamma + \beta_0 \cdot FHA_i + \beta_1 \cdot LowEquity_{it} \\ + \beta_2 \cdot FHA_i \times Post_t + \beta_3 \cdot LowEquity_{it} \times Post_t + \beta_4 \cdot FHA_i \times LowEquity_{it} \\ + \beta_5 \cdot FHA_i \times LowEquity_{it} \times Post_t + \epsilon_{it}. \end{aligned} \tag{8}$$

In this specification, $LowEquity_{it}$ is an indicator for whether borrower i's estimated level of home equity in month t is less than or equal to zero and all other terms are as previously defined. The coefficient of interest is β_5, which measures the difference in refinancing probability for FHA borrowers with low equity relative to FHA borrowers with high equity after the policy is in effect, relative to the same difference in the conventional market. As before, the identifying assumption is that the change in refinancing rates for high- and low-equity FHA borrowers would have evolved in parallel with the same change in refinancing for conventional borrowers in the absence of the policy. To increase the likelihood that this assumption holds, we will continue to include FHA-specific linear time trends that are allowed to vary freely before and after the policy change among our set of controls.

One potential problem with our estimates of borrower equity is that we cannot incorporate information on subsequent equity extraction through second liens or home equity lines of credit, which means that our estimates of borrower equity may be biased upwards. Bhutta and Keys (2016) show that equity extraction was very prevalent before the crisis with as many as 20 percent of likely homeowners borrowing against their home. This will attenuate our estimates of the policy effect since we may be misclassifying some negative-equity borrowers as having positive levels of equity.

Another potential concern, as with our results on the effects of unemployment, is that shocks to income and movements in house prices are correlated over this period. To reduce the likelihood that our estimates pick up residual variation in unemployment, we will estimate specifications that control flexibly for changes in county-level unemployment and also allow for the effect of unemployment to differentially affect FHA borrowers before and after the

policy changes relative to conventional borrowers. We will also show that our results are similar when we restrict the sample to counties that had relatively low levels of unemployment in late 2009. These steps should reduce the likelihood that our analysis is conflating the need for negative equity borrowers to pay for closing costs out-of-pocket with their potential inability to document employment.

Results

Table VI presents our estimates of equation (8). Column one, which controls only for month fixed effects, CBSA fixed effects and FHA-specific linear time trends, suggests that FHA borrowers without equity were 0.76 percentage points less likely to refinance subsequent to the policy change. Column 2 controls flexibly for a detailed set of fixed effects for various loan-level characteristics (loan age, interest rate, and LTV-by-FICO bins) as well as a set of fixed effects denoting which decile of the distribution of county-level unemployment changes the loan falls into. Column 3 further interacts these controls with the FHA and Post indicators. The coefficient estimate falls slightly when these controls are included but remains statistically significant and economically similar to the baseline effect in column 1. In column 4 we control for the independent effect of the employment documentation requirements by by interacting each unemployment change decile fixed effect with the complete interaction between the FHA and Post indicators. This causes the coefficient to fall somewhat to -0.57, which is still economically quite large and represents more than half of the overall effect of the policy change reported in Table IV. In column 5, we restrict the sample to counties with 2009 unemployment rates less than or equal to 7 percent. This substantially reduces the sample (leaving us only with 59 CBSAs), but we estimate an even larger decline in this subsample. Taken together, these results suggest that the restrictions on financing upfront costs for negative equity borrowers posed a substantial barrier to refinancing during this period.

To check the robustness of these effects, in Figure VI, we estimate two sets of flexible parameterizations that allow for the effect to vary either by month of observation or non-linearly with borrower equity. These results parallel the results presented for unemployment changes in Figure IV and are derived from specification directly analogous to equations (6) and (7). The top panel interacts the indicator for low-equity with month fixed effects and the FHA indicator, allowing for the effect of negative equity to vary freely over time. Once again, there is a clear, large, and discrete drop in refinancing for FHA borrowers with low levels of equity occurring in precisely the month that the policy takes effect. The bottom panel breaks equity levels into quintiles and interacts these quintiles with the FHA and post-shock indicators. We index the equity quintiles so that higher values represent households that are

further underwater. These coefficients show a large and monotonic decrease in refinancing moving from the first quintile, where all borrowers are estimated to have positive equity, to the fifth quintile, where borrowers tend to be in deeply negative equity. The size of the effect is very large; FHA borrowers in the fifth quintile are more than one percentage point less likely to refinance than FHA borrowers in the first quintile.

Optimality and Liquidity

Our results thus far show that having to pay for upfront costs out-of-pocket reduced refinancing rates for negative-equity FHA borrowers substantially. This decline may be due to two distinct mechanisms. First, there is a long literature arguing for the presence and importance of liquidity constraints (Deaton, 1989; Carroll, 2001; Zeldes, 1989). These constraints could prevent some borrowers from being able to pay the upfront costs needed to refinance.[21] Second, being forced to pay costs upfront instead of financing them into the loan may change the optimality of the refinancing decision even for borrowers with ample liquidity. This can happen when the subjective discount rate of the borrower differs from the actual interest rate on the loan. In this section, we provide evidence attempting to distinguish between these two channels.

While we cannot observe household liquidity or the upfront costs of refinancing directly, we are able to identify a group of households for whom the refinancing decision is still likely to be "optimal" even if they needed to pay for closing costs out-of-pocket. Within this set of households, changes in refinancing behavior caused by the policy should be driven primarily by the liquidity effect. Therefore, by comparing the behavior of this group of households to the entire sample, we are able to gauge the relative importance of the liquidity effect.

To measure the optimality of the household's refinancing option, we follow Keys et al. (2016) and rely on the model of refinancing behavior provided by Agarwal et al. (2013). This model takes standard loan and borrower characteristics as inputs and produces a threshold for the differential between a borrower's current rate and the rate on a new loan at which it would be optimal to refinance. If the gap between the prevailing rate and the borrower's current rate exceeds this threshold it is optimal for the borrower to refinance in the sense that it will reduce the expected net present value of her obligations to the lender (including potential closing costs). We calculate these thresholds in each month and for each borrower in our sample using the same baseline calibration of the model used by Agarwal et al. (2013) and Keys et al. (2016), which takes a conservative view on how many households should

[21] Borrowers may literally have the cash available to pay the costs, but if the borrower's precautionary motives imply this level of liquidity would not leave a large enough liquidity buffer then we would ascribe this failure to refinance to liquidity costs.

refinance.[22]

There are two critical components of this formula for our application. The first is the size of the upfront costs of refinancing. Because there is no widely avaialbe data on closing costs, we follow Agarwal et al. (2013) and calibrate this cost to be $2,000 plus one percent of the loan balance being refinanced. The second critical input is the "prevailing" rate to which a borrower could refinance if she chose to. We construct monthly estimates of this potential rate for each borrower in our sample using the set of observed refinances in each month among borrowers with similar characteristics. Specifically, we categorize observed refinances into FICO, LTV, state, month, and FHA cells and estimate the mean observed interest rate within each of these cells.[23] We then assume that the estimated means within each cell give the potential rate for a borrower with the same observables.[24] While clearly a simplification, this procedure helps to address the substantial dispersion in observed interest rates across borrowers and incorporates important correlations between borrower characteristics and potential rates. Given that the estimated potential rates vary from 3.75 percent to 9.13 percent, simply using the mean aggregate observed rate would likely introduce substantial measurement error. With these potential rates in hand, we can construct an indicator for whether it would be optimal for a borrower to refinance in each month given her current rate and the estimated upfront cost.

In column 6 of Table VI, we use this measure to explore the extent to which the effect of forcing borrowers to pay for upfront costs is driven by changes in the optimality of refinancing relative to liquidity. Specifically, we re-estimate the specification from column 4 in the subsample of borrowers for whom it would still be optimal to refinance even if they were required to pay for closing costs out-of-pocket. Changes in refinancing behavior in this sample should be driven primarily by liquidity. The estimated effect is economically very similar and indeed slightly larger than the analogous results for the full sample in column 4. The fact that refinancing rates fall even in this subsample suggests that lack of liquidity rather than changes in the optimality of refinancing is the dominant driver of the fall in refinancing for negative equity borrowers following the policy change.

[22]The specific calibration we use assumes the real discount rate used by households is 5 percent (annual), the marginal tax rate is 28 percent, the relocation rate is 10 percent, the rate of events with full deductibility of expenses is 20 percent, the standard deviation of mortgage rates is 0.0109, the inflation rate is 3 percent, and we assume all borrowers are refinancing into fixed rate mortgages. Actuarial data about the streamline program suggest that almost all streamline refinances are into fixed rate loans.

[23]We bin FICO scores into standard categorizations used by lenders: $\geq$ 800, (800,740], (740,670], (670, 580], and $\leq$ 580. Similarly, for LTV we group loans into the following bins: $>$ 90, [90, 80), [80, 70), [70, 60), and $\leq$ 60.

[24]If there is no observed rate for a given cell (no refinances were originated with those characteristics in that month and state) we assume there is no potential rate for that kind of borrower and omit them from our analysis.

Decomposing the Overall Effect

Our estimates show that FHA borrowers were much less likely to refinance after the policy changes and that both dimensions of the policy, employment documentation and upfront costs, were important drivers of this decline. Here we provide a simple back-of-the-envelope accounting for the relative contribution of these two mechanisms. We take the baseline 0.68 percentage point decline in FHA refinancing rates from the first column of Table IV as the overall effect to be explained and decompose it as

$$\Delta P(Refinance)_{it} = \beta_{UR} * \overline{\Delta UR} + \beta_{LE} * \%\text{FHA with Low Equity} + \text{other}, \tag{9}$$

where β_{UR} is the baseline estimated effect on unemployment from the first column of Table V (-0.133) and we take the average change in unemployment from the bottom panel of that table to be 2.71 percentage points. For the closing costs component, we set β_{LE} to -0.767 using the baseline estimate from column 1 of Table VI, and the fraction of FHA borrowers with low equity is 30 percent. Plugging these values in gives a total decline in FHA refinancing of 0.59 percent, which is roughly 87 percent of the overall decline that we observe. Within this amount, the unemployment effect is a little more than 50 percent larger than the equity effect, consistent with the idea that unemployed borrowers have a very high demand for refinancing.

VI Conclusion

Using large changes in the FHA streamline refinance program, we present evidence that requiring borrowers to document employment and pay upfront costs introduce economically large frictions to mortgage refinancing. This suggests that the pass-through of monetary policy to households may be less efficient in recessions, when unemployment is higher and households have less liquidity. Moreover, the households that might increase expenditures the most in response to reduced rates, those with little cash-on-hand or who recently experienced a negative income shock, may be the most affected by these frictions. This might exacerbate the unequal impacts of recessions on households by limiting the extent to which reductions in interest rates or other policies that operate through mortgage refinancing benefit lower income households directly. Evaluating the feasibility and welfare impacts of a broader streamline refinance program that is accessible to conventional or private-label borrowers is well beyond the scope of this paper. But our results suggest that, despite the well-documented mistakes made by borrowers when refinancing (or not refinancing), there are large numbers of borrowers that would refinance their mortgages, but do not because of these

large frictions in the mortgage market.

References

Adelino, Manuel, Kristopher Gerardi, and Paul S Willen, "Why don't lenders renegotiate more home mortgages? Redefaults, self-cures and securitization," Technical Report, National Bureau of Economic Research 2009.

Agarwal, Sumit, Gene Amromin, Itzhak Ben-David, Souphala Chomsisengphet, and Douglas D Evanoff, "The role of securitization in mortgage renegotiation," *Journal of Financial Economics*, 2011, *102* (3), 559–578.

_ , _ , _ , _ , **Tomasz Piskorski, and Amit Seru**, "Policy intervention in debt renegotiation: Evidence from the home affordable modification program," *Journal of Political Economy*, 2017, *125* (3), 654–712.

_ , _ , **Souphala Chomsisengphet, Tim Landvoigt, Tomasz Piskorski, Amit Seru, and Vincent Yao**, "Mortgage Refinancing, Consumer Spending, and Competition: Evidence from the Home Affordable Refinancing Program," 2017. NBER Working Paper No. 21512.

_ , **John C Driscoll, and David I Laibson**, "Optimal Mortgage Refinancing: A Closed-Form Solution," *Journal of Money, Credit and Banking*, 2013, *45* (4), 591–622.

_ , **Richard J Rosen, and Vincent Yao**, "Why Do Borrowers Make Mortgage Refinancing Mistakes?," *Management Science*, 2015, *62* (12), 3494–3509.

_ , **Souphala Chomsisengphet, Neale Mahoney, and Johannes Stroebel**, "Do banks pass through credit expansions? The marginal profitability of consumer lending during the great recession," 2015.

Andersen, Steffen, John Y Campbell, Kasper Meisner Nielsen, and Tarun Ramadorai, "Inattention and inertia in household finance: Evidence from the Danish mortgage market," Technical Report, National Bureau of Economic Research 2015.

Archer, Wayne R, David C Ling, and Gary A McGill, "The effect of income and collateral constraints on residential mortgage terminations," *Regional Science and Urban Economics*, 1996, *26* (3), 235–261.

Auclert, Adrien, "Monetary policy and the redistribution channel," Technical Report, National Bureau of Economic Research 2017.

Beraja, Martin, Andreas Fuster, Erik Hurst, and Joseph Vavra, "Regional heterogeneity and monetary policy," Technical Report, National Bureau of Economic Research 2017.

Bernanke, Ben S and Mark Gertler, "Inside the Black Box: The Credit Channel of Monetary Policy Transmission," *Journal of Economic Perspectives*, 1995, *9* (4), 27–48.

Bhutta, Neil and Benjamin J Keys, "Interest rates and equity extraction during the housing boom," *The American Economic Review*, 2016, *106* (7), 1742–1774.

Boyce, Alan, Glenn Hubbard, Chris Mayer, and James Witkin, "Streamlined refinancings for up to 14 million borrowers," 2012.

Campbell, John Y, "Household finance," *The journal of finance*, 2006, *61* (4), 1553–1604.

Campbell, Tim S and J Kimball Dietrich, "The determinants of default on insured conventional residential mortgage loans," *The Journal of Finance*, 1983, *38* (5), 1569–1581.

Caplin, Andrew, Charles Freeman, and Joseph Tracy, "Collateral Damage: Refinancing Constraints and Regional Recessions," *Journal of Money, Credit and Banking*, 1997, *29* (4 Part 1), 496–516.

Carroll, Christopher D, "A theory of the consumption function, with and without liquidity constraints," *The Journal of Economic Perspectives*, 2001, *15* (3), 23–45.

Chang, Yan and Abdullah Yavas, "Do Borrowers Make Rational Choices on Points and Refinancing?," *Real Estate Economics*, 2009, *37* (4), 635–658.

Coibion, Olivier, Yuriy Gorodnichenko, Lorenz Kueng, and John Silvia, "Innocent bystanders? Monetary policy and inequality in the US," Technical Report, National Bureau of Economic Research 2012.

Deaton, Angus, "Saving and liquidity constraints," Technical Report, National Bureau of Economic Research 1989.

Deng, Yongheng and John M Quigley, "Woodhead behavior and the pricing of residential mortgages," *Working Paper*, 2012.

_ , _ , and Robert Order, "Mortgage terminations, heterogeneity and the exercise of mortgage options," *Econometrica*, 2000, *68* (2), 275–307.

Di Maggio, Marco, Amir Kermani, and Christopher Palmer, "Unconventional Monetary Policy and the Allocation of Credit," 2016. Columbia Business School Research Paper No 16-1.

Dickinson, Amy and Andrea Heuson, "Mortgage prepayments: past and present," *Journal of Real Estate Literature*, 1994, *2* (1), 11–33.

Dunn, Kenneth B and Chester S Spatt, "The effect of refinancing costs and market imperfections on the optimal call strategy and the pricing of debt contracts," *Real Estate Economics*, 2005, *33* (4), 595–617.

Eberly, Janice and Arvind Krishnamurthy, "Efficient Credit Policies in a Housing Debt Crisis," *Brookings Papers on Economic Activity*, 2014, *2014* (2), 73–136.

Ehrlich, Gabriel and Jeffrey Perry, "Do Large-Scale Refinancing Programs Reduce Mortgage Defaults? Evidence From a Regression Discontinuity Design," 2015. Working Paper 2015-06.

Ganong, Peter and Pascal Noel, "The Effect of Debt on Default and Consumption: Evidence from Housing Policy in the Great Recession," 2017. Working Paper.

Green, Richard K and Michael LaCour-Little, "Some truths about ostriches: Who doesn't prepay their mortgages and why they don't," *Journal of Housing Economics*, 1999, *8* (3), 233–248.

Greenwald, Daniel L, "The Mortgage Credit Channel of Macroeconomic Transmission," *Working Paper*, 2016.

Haughwout, Andrew, Ebiere Okah, and Joseph Tracy, "Second Chances: Subprime Mortgage Modification and Redefault," *Journal of Money, Credit and Banking*, 2016, *48* (4), 771–793.

Johnson, Eric, Stephan Meier, and Olivier Toubia, "Money Left on the Kitchen Table: Exploring Sluggish Mortgage Refinancing Using Administrative Data, Surveys, and Field Experiments."," 2015.

Keys, Benjamin J, Devin G Pope, and Jaren C Pope, "Failure to refinance," *Journal of Financial Economics*, 2016, *122* (3), 482–499.

Lambie-Hanson, Lauren and Carolina Reid, "Stuck in Subprime? Examining the Barriers to Refinancing Mortgage Debt," 2017.

Lucas, Deborah, Damien Moore, and Mitchell Remy, "An Evaluation of Large-Scale Mortgage Refinancing Programs: Working Paper 2011-04," Technical Report 2011.

Lusardi, Annamaria, Daniel Schneider, and Peter Tufano, "Financially fragile households: evidence and implications," *Brookings Papers on Economic Activity*, 2011, pp. 83–151.

Maggio, Marco Di, Amir Kermani, Benjamin J Keys, Tomasz Piskorski, Rodney Ramcharan, Amit Seru, and Vincent Yao, "Interest Rate Pass-Through: Mortgage Rates, Household Consumption, and Voluntary Deleveraging," *American Economic Review*, 2017.

Mayer, Christopher, Edward Morrison, Tomasz Piskorski, and Arpit Gupta, "Mortgage Modification and Strategic Behavior: Evidence from a Legal Settlement with Countrywide," *The American Economic Review*, 2014, *104* (9), 2830–2857.

Pavlov, Andrey D, "Competing risks of mortgage termination: who refinances, who moves, and who defaults?," *The Journal of Real Estate Finance and Economics*, 2001, *23* (2), 185–211.

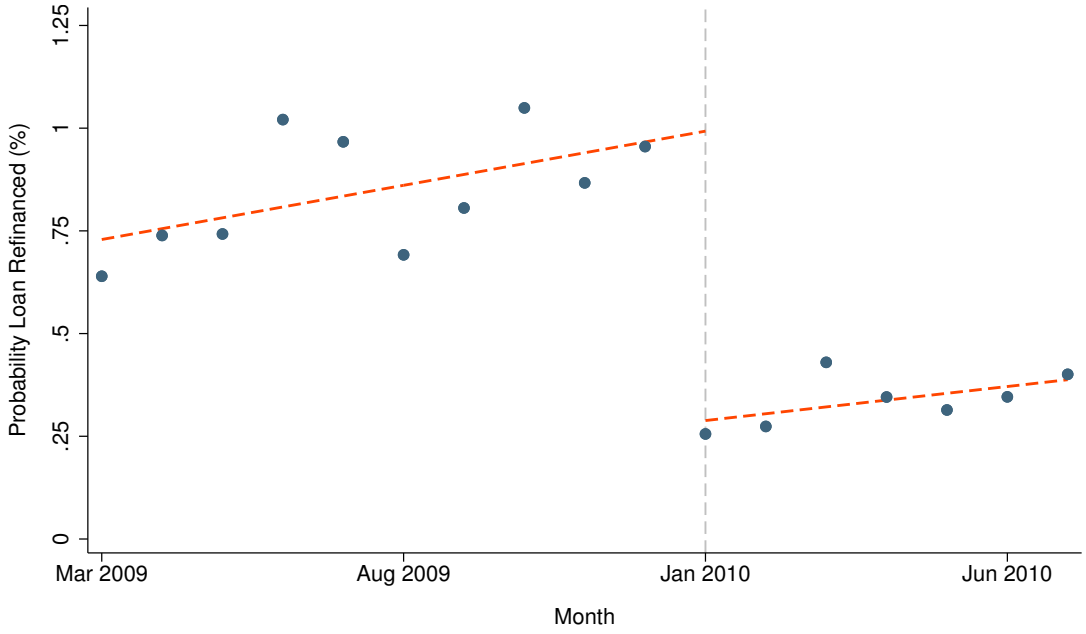
Piskorski, Tomasz and Amit Seru, "Mortgage Market Design: Lessons from the Great Recession," *Brookings Papers on Economic Activity*, 2018, *2018* (2).

Scharfstein, David and Adi Sunderam, "Market Power in Mortgage Lending and the Transmission of Monetary Policy," 2016. Working Paper.

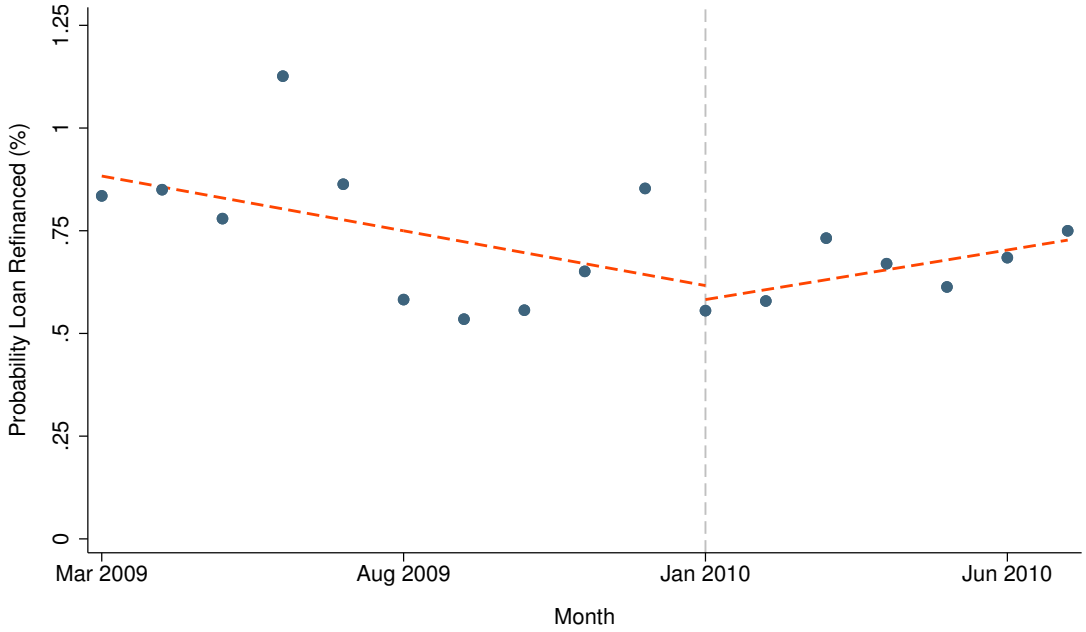
Stanton, Richard, "Rational prepayment and the valuation of mortgage-backed securities," *The Review of financial studies*, 1995, *8* (3), 677–708.

Woodward, Susan E, "A study of closing costs for FHA mortgages," *Department of Housing and Urban Development*, 2008.

Zeldes, Stephen P, "Consumption and liquidity constraints: an empirical investigation," *Journal of political economy*, 1989, *97* (2), 305–346.



A. FHA Loans



B. Conventional Loans

FIGURE I
FHA and Conventional Refinancing Trends

NOTE.—This figure plots monthly unconditional refinancing rates between March 2009 and July 2010. Each dot represents the percent of outstanding mortgages of a given type that refinanced in the indicated month. Refinancing rates are calculated separately for FHA (Panel A) and Conventional loans (Panel B). The vertically dashed grey line in January 2010 marks the first month that the SLR policy changes went into effect. The dashed orange lines are the predicted values from a regression of the plotted refinancing rates on a linear time trend fit separately on either side of the cutoff date.

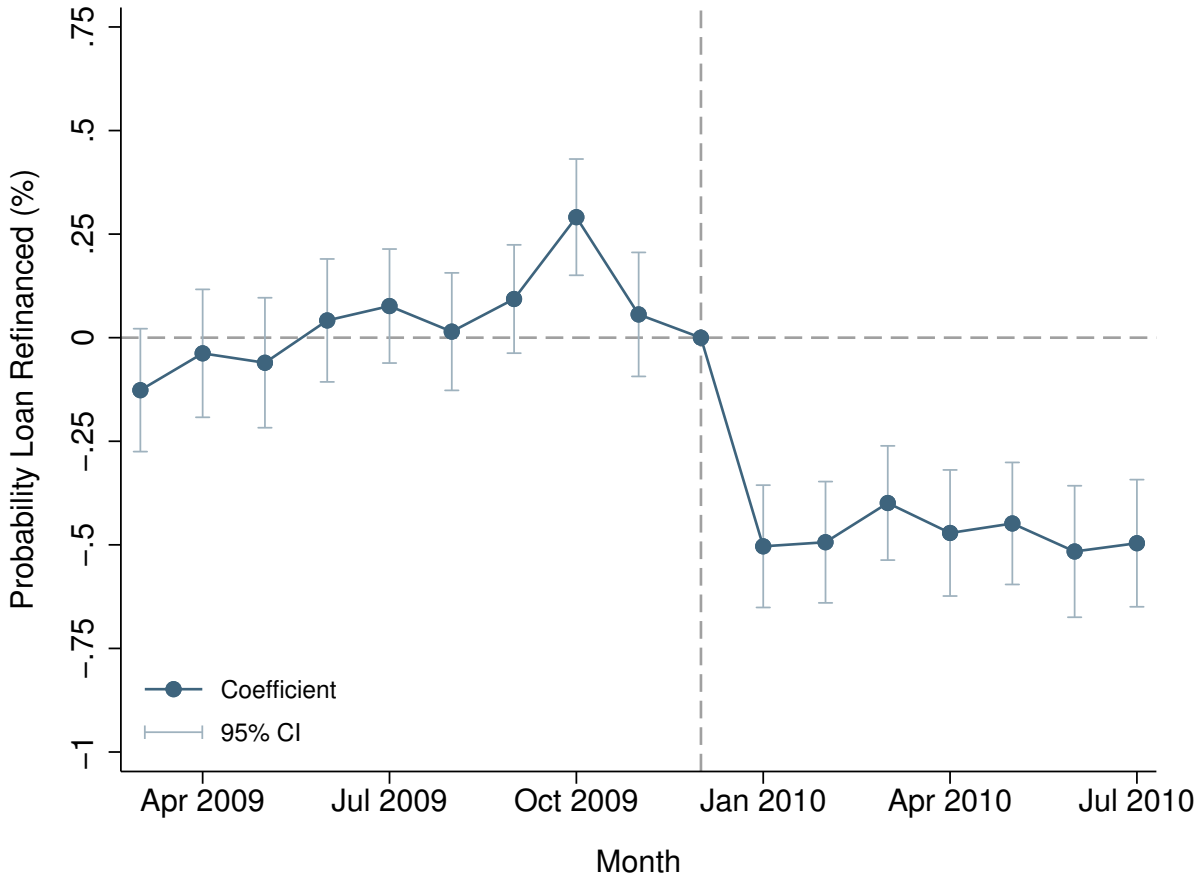
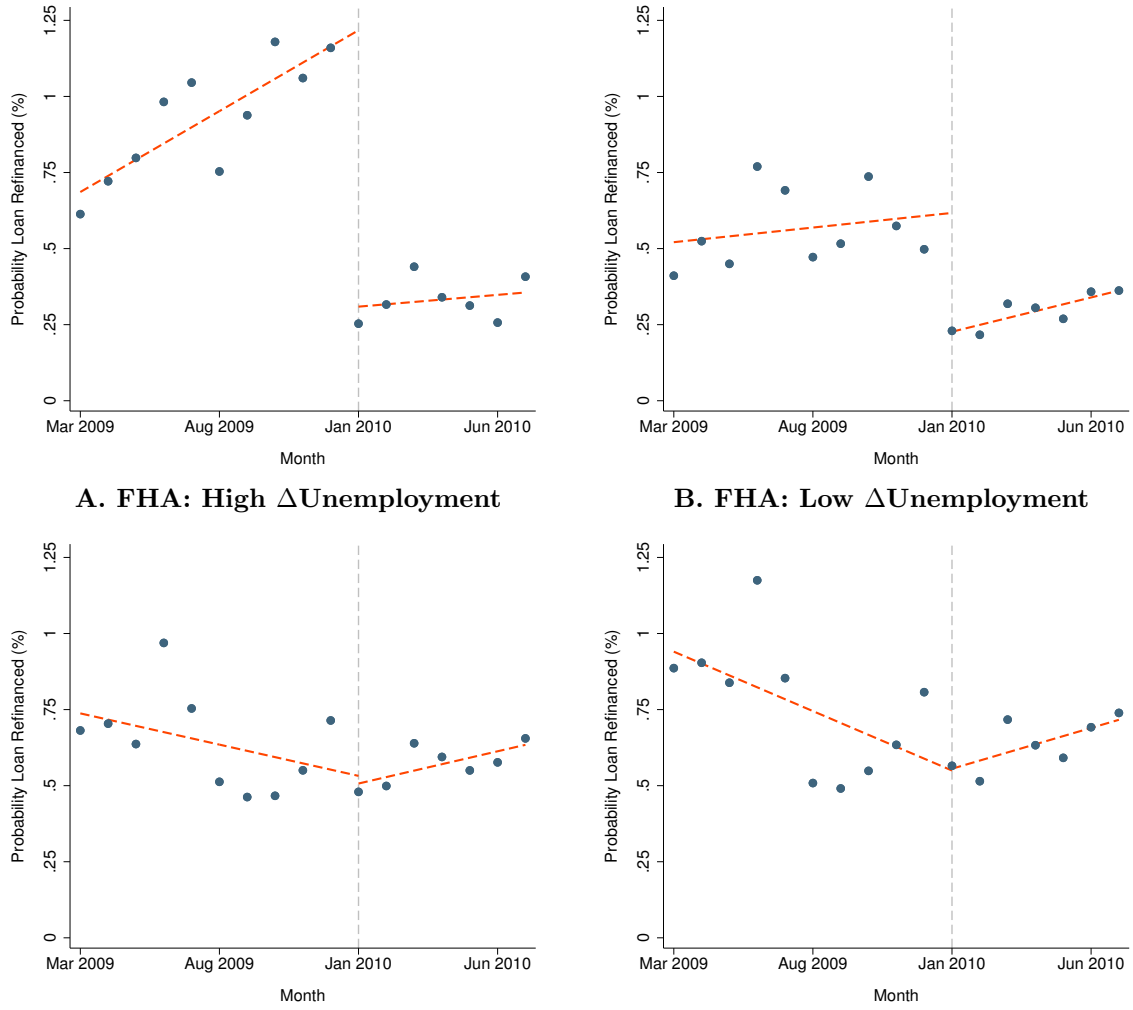


FIGURE II

Flexible Diff-in-Diff Estimates of the Effect of the FHA Policy Changes on Refinancing

NOTE.—This figure reports estimates of the effect of the change in FHA policies on FHA refinancing derived from a flexible difference-in-differences specification that allows the effect to vary freely by month of observation. Estimates were constructed by regressing an indicator for whether or not a loan refinances in a given month on a dummy variable denoting whether the loan was FHA insured and the interaction of that FHA dummy with a series of dummy variables indicating the month of observation. The coefficient for December 2009 is normalized to zero, so that all estimates can be interpreted as the change in the monthly probability of refinancing relative to the month prior to when the policy changes went into effect, which is marked by the vertically dashed grey line. The regression also included fixed effects for the CBSA of the property, the current age (one-year bins), interest rate (one-percentage point bins) and home-equity decile associated with the loan, as well as the full pairwise interaction between the borrower's LTV (10-point bins) and FICO score (50-point bins) at origination. With the exception of the CBSA fixed effects, all of these controls were also separately interacted with the FHA dummy and with the dummies for the month of observation. The 95% confidence intervals are based on standard errors that are clustered at the CBSA level.



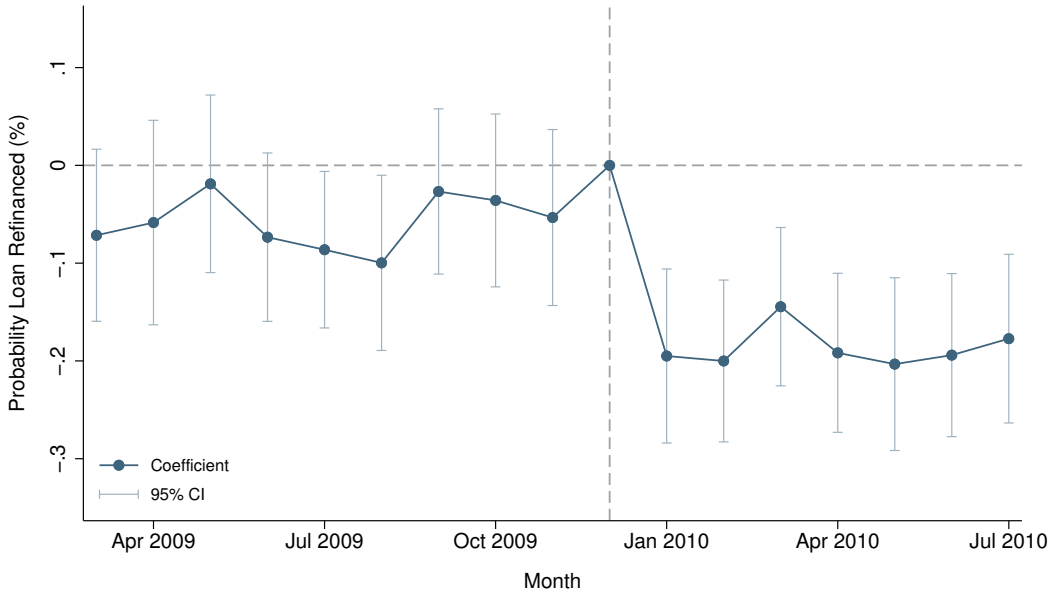
A. FHA: High ΔUnemployment **B. FHA: Low ΔUnemployment**

C. Conventional: High ΔUnemployment **D. Conventional: Low ΔUnemployment**

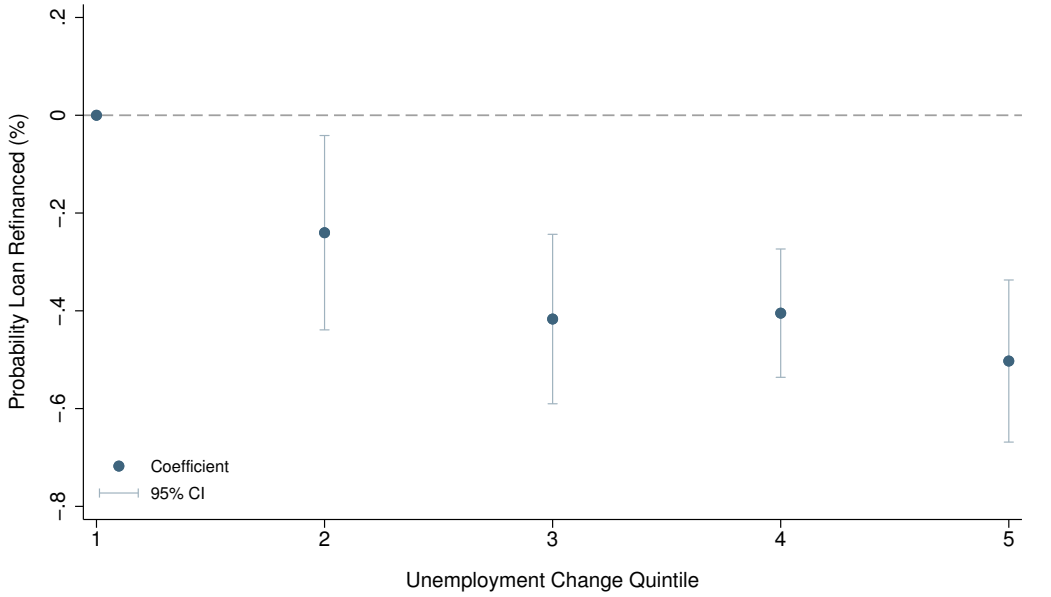
FIGURE III

FHA and Conventional Refinancing Trends by County Unemployment Change

NOTE.—This figure plots unconditional monthly refinancing rates between March 2009 and July 2010 among FHA and conventional borrowers and across counties experiencing differing changes in unemployment. Loans are categorized into "high" and "low" unemployment change groups based on whether they fall into the top or bottom third of the (loan count weighted) distribution of changes in county-level unemployment rates between 2006 and 2009. Each dot represents the percent of outstanding mortgages in a given group that refinanced in the indicated month. Refinancing rates are calculated separately for FHA (Panels A and B) and Conventional loans (Panels C and D). The vertically dashed grey line in January 2010 marks the first month that the SLR policy changes went into effect. The dashed orange lines are the predicted values from a regression of the plotted refinancing rates on a linear time trend fit separately on either side of the cutoff date.



A. Triple Difference Estimates by Month of Observation

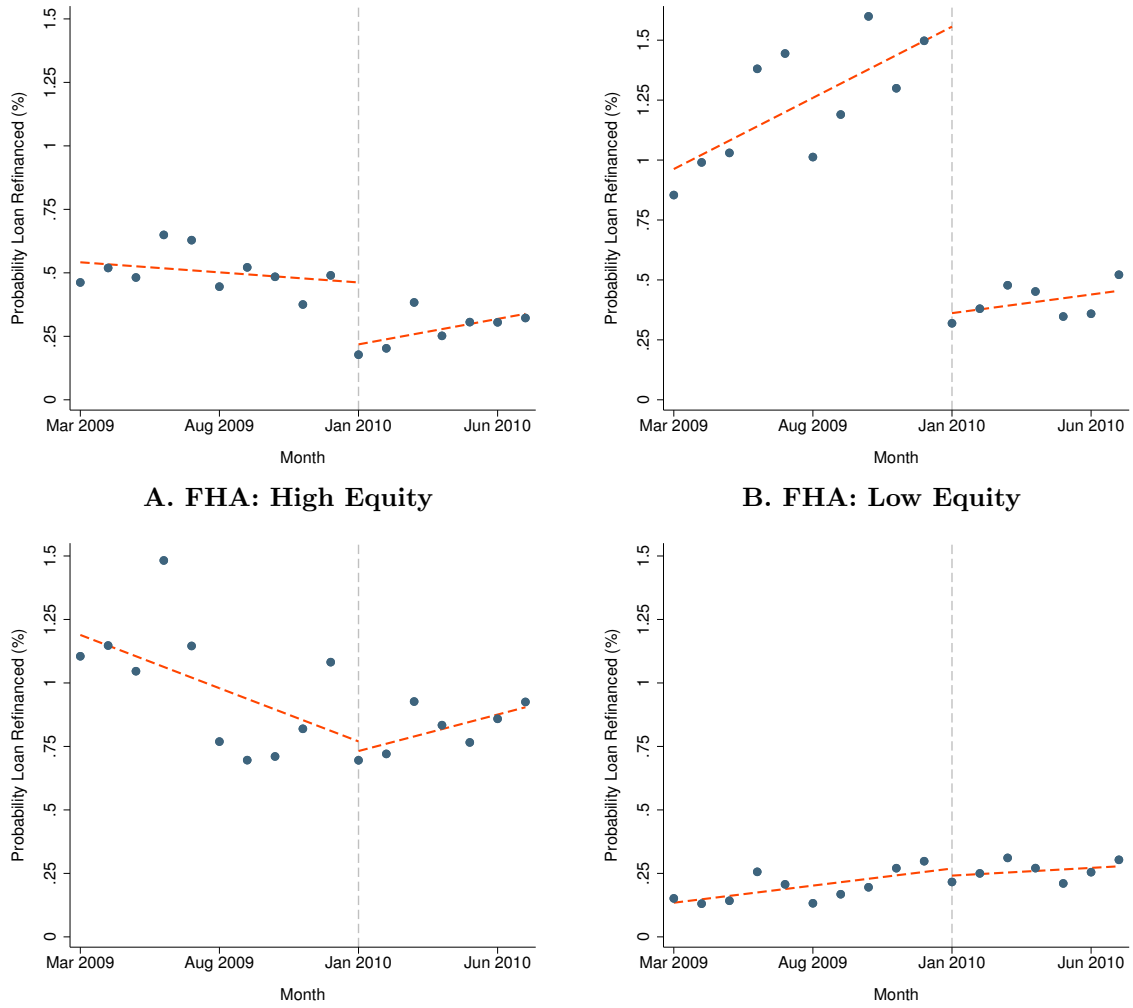


B. Triple Difference Estimates by Unemployment Change Quintile

FIGURE IV

Flexible Triple-Difference Estimates: Unemployment

NOTE.—This figure presents two sets of non-parametric estimates of the effect of change in SLR income documentation requirements on FHA refinancing. Panel A reports coefficient estimates from a regression of an indicator for refinancing on the interaction between the FHA dummy, county-level unemployment changes, and month fixed effects. Panel B reports estimates from a similar regression that splits unemployment changes into quintiles and interacts these bins with FHA and post-policy indicators. Both regressions include fixed effects for the month of observation and the CBSA of the property as well as linear time trends for FHA borrowers that are allowed to vary freely before and after the policy change. The 95% confidence intervals are based on standard errors that are clustered at the CBSA level.



A. FHA: High Equity

B. FHA: Low Equity

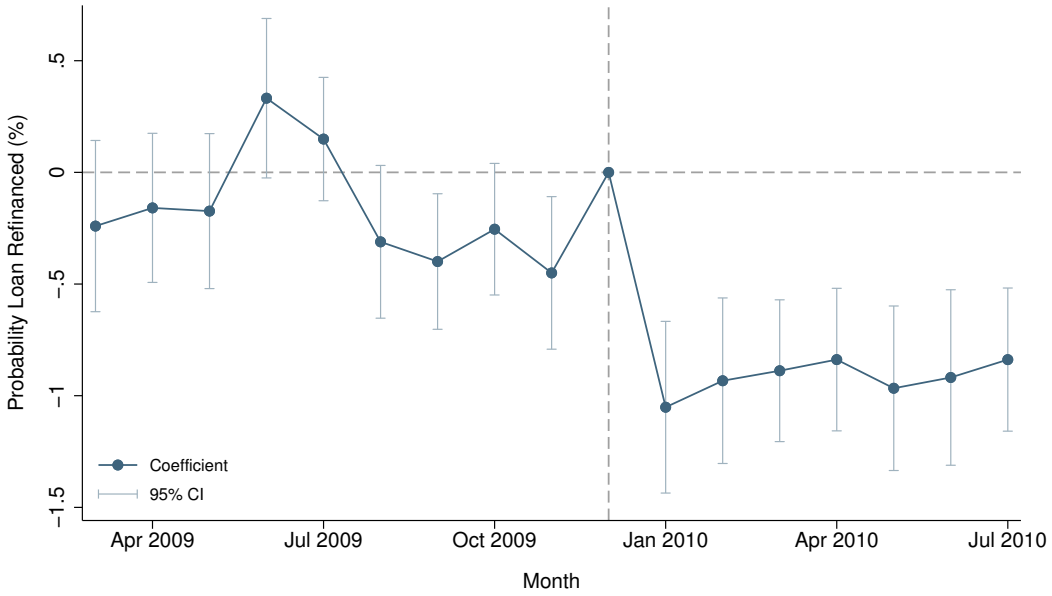
C. Conventional: High Equity

D. Conventional: Low Equity

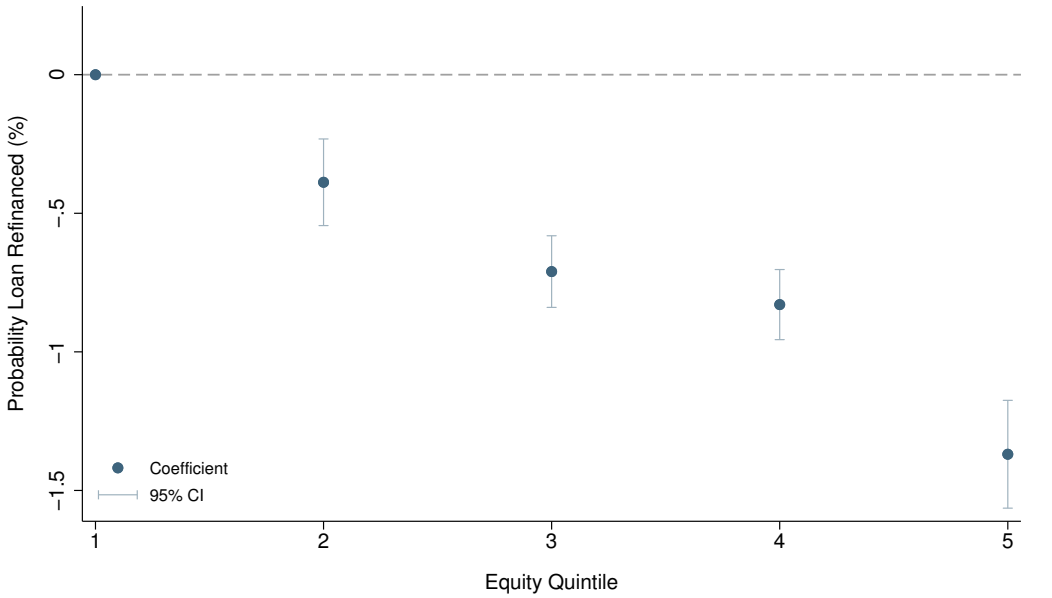
FIGURE V

FHA and Conventional Refinancing Trends by Borrower's Equity

NOTE.—This figure plots unconditional monthly refinancing rates between March 2009 and July 2010 among FHA and conventional borrowers with differing levels of home equity. Borrowers are categorized as "high" equity if their estimated level of home equity is greater than or equal to $20,000 and "low" equity if it is less than or equal to $0. To estimate home equity, we subtract the borrower's current outstanding balance from an estimate of the current home value derived from the initial purchase price and subsequent growth implied by the relevant county-level Zillow home price index. Each dot represents the percent of outstanding mortgages in a given group that refinanced in the indicated month. Refinancing rates are calculated separately for FHA (Panels A and B) and Conventional loans (Panels C and D). The vertically dashed grey line in January 2010 marks the first month that the SLR policy changes went into effect. The dashed orange lines are the predicted values from a regression of the plotted refinancing rates on a linear time trend fit separately on either side of the cutoff date.



A. Triple Difference Estimates by Month of Observation



B. Triple Difference Estimates by Current Equity Quintile

FIGURE VI
Flexible Triple-Difference Estimates: Equity

NOTE.—This figure presents two sets of non-parametric estimates of the effect of the change in SLR closing costs requirements on FHA refinancing. Panel A reports coefficient estimates from a regression of an indicator for refinancing on the interaction between the FHA dummy, an indicator for whether the borrower's estimated home equity is less than or equal to zero, and month fixed effects. Panel B reports estimates from a similar regression that splits equity levels into quintiles and interacts these bins with FHA and post-policy indicators. Equity quintiles are indexed so that higher values represent borrowers that are further underwater. Both regressions include fixed effects for the month of observation and the CBSA of the property as well as linear time trends for FHA borrowers that are allowed to vary freely before and after the policy change. The 95% confidence intervals are based on standard errors that are clustered at the CBSA level.

TABLE I

MAXIMUM LOAN AMOUNTS FOR FHA STREAMLINE REFINANCES BEFORE AND AFTER THE POLICY CHANGE

Pre-Policy Change	Post-Policy Change
(1)	(2)
Streamline without Appraisal	
Minimum of:	*Minimum of:*
Original principal balance + Net UFMIP	Original principal balance + Net UFMIP
or	*or*
Unpaid Principal balance + Net UFMIP + Interest payoff + **Closing costs** + **Net pre-paid expenses** + **Discount points**	Unpaid principal balance + Net UFMIP + Interest payoff
Streamline with Appraisal	
Minimum of:	*Minimum of:*
97.75% of appraised value + Net UFMIP	97.75% of appraised value + Net UFMIP
or	*or*
Unpaid principal balance + Net UFMIP + Interest payoff + Closing costs + Net pre-paid expenses + **Discount points**	Unpaid principal balance + Net UFMIP + Interest payoff + Closing costs + Net Pre-paid expenses

NOTE.—This table presents the FHA-mandated loan calculations for streamline refinances with and without an appraisal, before and after the policy changes we study. Net UFMIP refers to the upfront mortgage insurance payment required by the FHA minus any refund due to the borrower for the UFMIP on the original loan. The interest payoff reflects the fact that interest on FHA loans is guaranteed at the beginning of each new month. This means that if a loan is refinanced in the middle of a month the borrower is still responsible for the remaining days interest on the old loan. Closing costs include origination fees and other underwriting costs in addition to title, attorney, and recording fees. Net pre-paid expenses will reflect any prepayment of interest, insurance or taxes minus any amount still due for those costs. Discount points are upfront payments the borrower may make to the lender in exchange for a reduced rate, which the FHA mandated be "reasonable." The boldfaced items indicated in orange were eliminated from the loan calculations by the policy change.

TABLE II
Summary Statistics

	Conventional	FHA
Loan Amount ($1000's)	193.41	135.83
	(149.42)	(76.74)
Estimated Equity ($1000's)	72.18	17.08
	(153.72)	(76.64)
FICO Score	711.86	656.94
	(60.74)	(70.17)
Loan-to-Value at Origination	75.13	94.88
	(17.20)	(8.40)
Back-End Debt-to-Income	37.72	37.84
	(13.71)	(12.31)
Interest Rate	6.72	6.71
	(0.79)	(0.59)
Loan Age (Years)	3.90	3.51
	(2.94)	(3.76)
Number of Observations	465,127	84,918

Note.—This table reports summary statistics for the loans in the analysis sample as of August 2009, one month before the policy changes were announced.

TABLE III
THE EFFECT OF THE POLICY CHANGES ON FHA REFINANCING: EVENT STUDY

	(1)	(2)	(3)	(4)
Post	−0.701***	−0.747***	−0.694***	−0.765***
	(0.069)	(0.072)	(0.081)	(0.087)
Post News			−0.053	−0.041
			(0.045)	(0.047)
Time Trends	X	X	X	X
CBSA FEs	X	X	X	X
Loan Age FEs		X		X
Interest Rate FEs		X		X
LTV × Fico FEs		X		X
Equity FEs		X		X
Number of Observations	1,121,511	1,004,247	1,121,511	1,004,247

NOTE.—This table reports event study estimates of the effect of the change in FHA policies on the monthly probability that an FHA loan refinances. Each column reports the estimated coefficients from a separate regression where the dependent variable is an indicator for whether or not a loan refinances in the month of observation. The outcome is multiplied by 100, so that all coefficients can be interpreted as percentage point changes. In all specifications, coefficients are reported for the Post dummy denoting whether the month of observation is after the implementation of the policy changes (January 2010). The specifications in columns 3 and 4 also include an indicator for whether the month of observation was after September 2009, which was the month that the policy changes were announced (Post News). All specifications include fixed effects for the CBSA of the property as well as linear time trends which are allowed to differ on either side of the policy implementation date. In columns 3 and 4, an additional linear time trend is included for the period of time subsequent to the announcement date. Columns 2 and 4 include fixed effects for the current age (one-year bins), interest rate (one-percentage point bins) and home-equity decile associated with the loan as well as the full pairwise interaction between the borrower's LTV (10-point bins) and FICO score (50-point bins) at origination. Standard errors are reported in parentheses and are clustered at the CBSA level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE IV

THE EFFECT OF THE POLICY CHANGES ON FHA REFINANCING: DIFFERENCE-IN-DIFFERENCES

	(1)	(2)	(3)	(4)
FHA	0.474***	0.674***	0.702***	1.052*
	(0.070)	(0.067)	(0.066)	(0.618)
FHA × Post	−0.677***	−0.670***	−0.778***	−0.798***
	(0.063)	(0.065)	(0.065)	(0.064)
Month FEs	X	X	X	X
CBSA FEs	X	X	X	X
FHA Time Trends	X	X	X	X
Loan Age FEs		X	X	X
Interest Rate FEs		X	X	X
LTV × Fico FEs		X	X	X
Equity FEs		X	X	X
Controls × Post			X	X
Controls × FHA				X
Number of Observations	7,641,922	6,857,715	6,857,715	6,857,715

NOTE.—This table reports difference-in-differences estimates of the effect of the change in FHA policies on the monthly probability that an FHA loan refinances. Each column reports estimates from a separate regression where the dependent variable is an indicator for whether or not a loan refinances in the month of observation. The outcome is multiplied by 100, so that all coefficients can be interpreted as percentage point changes. Coefficients are reported for the FHA "treatment" dummy as well as its interaction with an indicator for whether the month of observation was after the implementation of the policy changes (Post), which occurred in January 2010. All specifications include fixed effects for the month of observation and the CBSA of the property as well as linear time trends for FHA borrowers that are allowed to vary freely before and after the policy change. Column 2 adds fixed effects for the current age (one-year bins), interest rate (one-percentage point bins) and home-equity decile associated with the loan as well as the full pairwise interaction between the borrower's LTV (10-point bins) and FICO score (50-point bins) at origination. Column 3 further interacts all of the additional fixed effects contained in column 2 with the Post dummy. Column 4 adds an additional set of interactions between these fixed effects and the FHA dummy. Standard errors are reported in parentheses and are clustered at the CBSA level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE V

REFINANCING AND UNEMPLOYMENT: TRIPLE DIFFERENCE ESTIMATES

	(1)	(2)	(3)	(4)	(5)
FHA	0.024	0.221***	−0.046	0.038	0.333
	(0.073)	(0.071)	(0.505)	(0.498)	(0.620)
FHA × Post	−0.278***	−0.217***	−0.354***	−1.495***	−0.327*
	(0.075)	(0.081)	(0.080)	(0.229)	(0.194)
FHA × ΔUR	0.137***	0.145***	0.110***	0.095***	0.048***
	(0.020)	(0.019)	(0.019)	(0.020)	(0.016)
FHA × ΔUR × Post	−0.133***	−0.150***	−0.150***	−0.111***	−0.104***
	(0.017)	(0.019)	(0.020)	(0.021)	(0.021)
Month FEs	X	X	X	X	X
CBSA FEs	X	X	X	X	X
FHA Time Trends	X	X	X	X	X
Loan Age FEs		X	X	X	X
Interest Rate FEs		X	X	X	X
LTV × Fico FEs		X	X	X	X
Equity FEs		X	X	X	X
Controls × Post			X	X	X
Controls × FHA			X	X	X
Equity FEs × FHA × Post				X	X
High Equity Subsample					X
Mean ΔUR	2.71	2.70	2.70	2.70	2.53
Number of Observations	5,588,474	5,013,637	5,013,637	5,013,637	3,291,551

NOTE.—This table reports triple-differences estimates of the effect of the change in SLR income documentation requirements on the monthly probability that an FHA loan refinances. Each column reports estimates from a separate regression where the dependent variable is an indicator for whether or not a loan refinances in the month of observation. The outcome is multiplied by 100, so that all coefficients can be interpreted as percentage point changes. Coefficients are reported for the FHA "treatment" dummy as well as its interaction with the Post dummy, the change in county-level employment, and the triple interaction between the three. The Post dummy takes the value one if the month of observation is after the implementation of the policy changes (January 2010). All specifications include fixed effects for the month of observation and the CBSA of the property as well as linear time trends for FHA borrowers that are allowed to vary freely before and after the policy change. Column 2 adds fixed effects for the current age (one-year bins), interest rate (one-percentage point bins) and home-equity decile associated with the loan as well as the full pairwise interaction between the borrower's LTV (10-point bins) and FICO score (50-point bins) at origination. Column 3 further interacts all of the additional fixed effects contained in column 2 with the Post dummy and the FHA indicator. Column 4 allows for an unrestricted effect of home equity by interacting each home equity decile fixed effect with the complete interaction between the FHA and Post indicators. Column 5 restricts the sample to borrowers with estimated home equity greater than $20,000. Standard errors are reported in parentheses and are clustered at the CBSA level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE VI

Refinancing and Upfront Costs: Triple Difference Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
FHA	0.149***	0.333***	−0.632	−0.750	−3.701*	−1.114*
	(0.052)	(0.052)	(0.476)	(0.474)	(2.070)	(0.566)
FHA × Post	−0.460***	−0.467***	−0.600***	−0.407***	−0.507***	−0.589***
	(0.053)	(0.057)	(0.057)	(0.066)	(0.160)	(0.106)
FHA × Low Equity	1.249***	0.964***	0.889***	0.852***	0.969***	1.055***
	(0.114)	(0.111)	(0.112)	(0.111)	(0.307)	(0.126)
FHA × Low Equity × Post	−0.767***	−0.701***	−0.652***	−0.567***	−0.808***	−0.732***
	(0.085)	(0.081)	(0.083)	(0.077)	(0.262)	(0.098)
Month FEs	X	X	X	X	X	X
CBSA FEs	X	X	X	X	X	X
FHA Time Trends	X	X	X	X	X	X
Loan Age FEs		X	X	X	X	X
Interest Rate FEs		X	X	X	X	X
LTV × Fico FEs		X	X	X	X	X
ΔUR FEs		X	X	X	X	X
Controls × Post			X	X	X	X
Controls × FHA			X	X	X	X
ΔUR FEs × FHA × Post				X	X	X
Low Unemployment Subsample					X	
High Optimality Subsample						X
Number of Observations	5,588,474	5,058,744	5,058,744	5,058,744	651,144	3,283,720

Note.—This table reports triple-differences estimates of the effect of the change in SLR closing cost requirements on the monthly probability that an FHA loan refinances. Each column reports estimates from a separate regression where the dependent variable is an indicator for whether or not a loan refinances in the month of observation. The outcome is multiplied by 100, so that all coefficients can be interpreted as percentage point changes. Coefficients are reported for the FHA "treatment" dummy as well as its interaction with the Post dummy, an indicator for whether the borrower's estimated home equity is less than zero ("Low Equity"), and the triple interaction between the three. The Post dummy takes the value one if the month of observation is after the implementation of the policy changes (January 2010). All specifications include fixed effects for the month of observation and the CBSA of the property as well as linear time trends for FHA borrowers that are allowed to vary freely before and after the policy change. Column 2 adds fixed effects for the current age (one-year bins), interest rate (one-percentage point bins) and county-level unemployment change decile associated with the loan as well as the full pairwise interaction between the borrower's LTV (10-point bins) and FICO score (50-point bins) at origination. Column 3 further interacts all of the additional fixed effects contained in column 2 with the Post dummy and the FHA indicator. Column 4 allows for an unrestricted effect of unemployment changes by interacting each unemployment decile fixed effect with the complete interaction between the FHA and Post indicators. Column 5 restricts the sample to counties with 2009 unemployment rates less than 7 percent. Column 6 drops borrowers for whom refinancing is not optimal if they have to pay closing costs. See the text for more details on how estimates of refinancing optimality are constructed. Standard errors are reported in parentheses and are clustered at the CBSA level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.