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

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Abstract

We study how employment documentation requirements and out-of-pocket closing costs constrain mortgage refinancing. These frictions, which bind most severely during recessions, may significantly inhibit monetary policy pass-through. To study their effects on refinancing, we exploit an FHA policy change that excluded unemployed borrowers from refinancing and increased others' out-of-pocket costs substantially. These changes dramatically reduced refinancing rates, particularly among the likely unemployed and those facing new out-of-pocket costs. Our results imply that unemployed and liquidity-constrained borrowers have a high latent demand for refinancing. Cyclical variation in these factors may therefore affect both the aggregate and distributional consequences of monetary policy.

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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., 2019), 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, 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

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[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 latent 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 baseline estimates imply that the policy reduced the monthly probability that an FHA borrower refinanced her mortgage by a full percentage point, which is a decline of roughly 80 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 results that are similar to the event study analysis. Across a range of specifications, we estimate that the policy led to a reduction in the monthly FHA refinancing rate of roughly 0.7 percentage points, which is more than 50 percent of the baseline rate. 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 0.05 percentage points following the policy change. These estimates are robust to our full set of controls, and the timing of the effect is consistent with the change in FHA policy. Importantly, these results are also not conflated by the simultaneously imposed new requirement that negative equity FHA borrowers pay for closing costs out-of-pocket. We estimate all of our specifications only in the subsample of borrowers with positive equity and, in the preferred specification, allow for the level of home equity to have a fully flexible and time-varying independent effect on relative refinancing rates of FHA borrowers.

Taken together, our estimates imply that unemployed borrowers have a high demand for refinancing that is constrained by the standard mortgage underwriting process. Back of the envelope calculations suggest that allowing unemployed borrowers to refinance at the rates implied by our estimates would have generated approximately 2.4 million additional refinances in 2009. Together, these refinances would have saved unemployed borrowers roughly $7.2 billion that year. This represents almost 10 percent of the entire amount paid out to unemployed people through the unemployment insurance system in 2009. These implied payment savings are also of roughly the same order of magnitude as what was achieved by the Home Affordable Modification and and Home Affordable Refinancing Programs (HAMP and HARP), both of which notably excluded unemployed borrowers from participating (Agarwal et al., 2017a,b).

In the last part of the paper, we study 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. To avoid conflating this effect with the employment documentation requirements, we estimate these regressions only in the subsample of counties with below median increases in unemployment and, in some specifications, also allow for

county-level changes in unemployment to have a flexible and time-varying effect on FHA refinancing. We find that the inability for low-equity borrowers to roll the closing costs into the loan had very large negative effects. Our preferred estimate suggests that this friction reduced monthly refinancing rates among FHA borrowers by at least 0.5 percentage points. 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 similar to those in our full sample, which suggests that lack of liquidity 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. (2019) emphasize the role that home equity plays in amplifying and mediating interest rate changes through the mortgage refinancing channel. Similarly, Di Maggio et al. (2016) show that the effects of quantitative easing during the Great Recession operated in large part through mortgage refinancing. 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. The frictions we document, because they limit the pass-through of interest rate changes to households with fixed interest mortgages, help quantify how much less economic stimulus is being effected through both conventional and unconventional monetary policy. Our work is also related to the mechanisms highlighted by Greenwald (2018), who emphasizes the way payment-to-income restrictions affect the ability of interest rate changes to affect credit growth. Agarwal et al. (2018), and Scharfstein and

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Sunderam (2016) examine how frictions arising from market structure, lack of competition, and bank incentives affect the pass-through of monetary policy to households through several channels including refinancing.[3] Similarly, Bond et al. (2017) show that the presence of second mortgages can inhibit the ability for borrowers to refinance into to a lower interest rate on their first mortgage when junior creditors refuse to be re-subordinated. Finally, Auclert (2017) and Coibion et al. (2017) 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 at the frequency that would be expected in a fully rational frictionless benchmark model(Andersen et al., 2015; Agarwal et al., 2015; 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 contribute to this literature by quantifying the effects of two important 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 program 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., 2013; 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

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[3]Agarwal et al. (2017b) similarly show that lack of competition in the refinancing market inhibited the effectiveness of HARP by both lowering take-up rates and reducing interest rate savings on refinances completed through the program.

from competitive and moral hazard frictions restricting uptake. The benefits of the stream-line 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 along the lines of the FHA SLR program.

The rest of the paper proceeds as follows. Section II describes the institutional back-ground 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. 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

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[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.

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 that does not require either employment documentation or positive equity 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 just over 80% of all FHA refinances went through the SLR program. In 2009, which is when the policy change that we study occurred, FHA streamline refinances represented roughly 6% of all refinances in the U.S.[6]

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

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

[6]These figures are based on the authors' calculations (available on request) using data from HUD's FHA Single-Family Outlook Reports and the Federal Reserve Board's report on the 2009 HMDA data. The majority (60%) of the FHA refinances that did not go through the SLR program were cash-out refinances, which do not qualify for SLR. While the FHA does not report statistics for the remaining 40% of these loans, they were likely made up primarily of conventional-to-FHA refinances, which are also excluded from the SLR program.

[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. 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).

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 that 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 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.[11]

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

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[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).

[11]Unemployed borrowers were likely the largest class of borrowers shut out from the market by the new policy. However, those whose income is difficult to verify and who may have also been affected include self-employed borrowers without a W-2, retirees drawing income from a retirement account, and borrowers with a significant fraction of income coming from tips or gratuities.

that no closing costs could be rolled into the new mortgage.[12] 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. 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]

It is somewhat difficult to document the exact rationale for why the FHA implemented these policy changes. In the press release describing the changes, the FHA indicated that they were intended to "bring documentation standards in line with other FHA loan origination guidelines" and would "[ensure] the borrower's capacity to repay the new mortgage."[14] However, the actual economic benefits of the policy changes to the FHA are unclear. While preventing underwater borrowers from rolling closing costs into the new mortgage may potentially reduce the FHA's liability, precluding these borrowers as well as those who are unemployed from refinancing into lower interest rates would likely increase that liability. The reason for this is simple: lower interest rates will at least weakly reduce default risk, and any borrower using the SLR program would have already had that risk insured by the FHA. The SLR changes were announced two months after the swearing in of a new FHA commissioner, who was forced to address the serious deterioration in the insurance fund's capital reserves. It is possible that these changes were instituted as part of this broader effort. In particular, the FHA began treating SLR originations as new loans for the purposes of its actuarial review around this time and may therefore have needed to harmonize underwriting guidelines between the SLR program and new originations as a result.[15] To the best of our knowledge, there is no clear documentation explicitly outlining why these steps were taken at this particular point in time, apart from statements like those described above.

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

[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.

[14]See https://archives.hud.gov/news/2009/pr09-177.cfm.

[15]See Aragon et al. (2010) for a discussion of how streamline refinances were treated in the 2009 actuarial review.

Regardless of the exact reasons, these changes were likely to have important effects. In Figure I we plot trends in several key measures of aggregate economic conditions at the time. These trends indicate that the policy changes were indeed likely to have had a significant effect on refinancing in the FHA market. Panels A. and B., which plot the federal funds rate and the going rate on new 30-year fixed rate mortgages, indicate that the incentive to refinance and the associated benefits were likely to have been quite large. The SLR changes were announced just shortly after the Federal Reserve had begun the first round of quantitative easing and aggressively lowered its key policy rate to near zero percent, which was accompanied by a sharp 1 percentage point drop and continuing downward trend in mortgage rates. Yet, as Panels C. and D. indicate, many borrowers in the FHA market would be shut out from these benefits by the new policy changes. The national unemployment rate had just reached its peak of nearly 10 percent and house prices were continuing their precipitous fall from their peak in 2006. All of these unemployed borrowers would be excluded from the SLR program and those with limited cash-on-hand living in parts of the country experiencing the largest house price declines would very likely have found it prohibitively costly to refinance through the program due to the new need to pay for closing costs out-of-pocket.

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 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.[16] Estimates from various sources suggest that almost all FHA refinances would

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[16]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. Re-

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.

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 rely on three distinct 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. Using this merged data, Corelogic is able to determine whether or not a paid off loan is a refinance or a sale. 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 Zillow's county-level house price indices. In each month, we impute the current value of the borrower's home and subtract

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finances 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.

the observed remaining balance to determine 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 (ACS) 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 restrict our attention to a set of relatively standard mortgages that were active at some point during the window of time spanning 6 months prior to the policy announcement and 6 months after the implementation date (March, 2009–July, 2010). To construct our analysis sample, we begin with an initial 20 percent random sample of all first-lien, fixed-rate, 30-year, single-family, owner-occupied mortgages active during this period for which CoreLogic reports a non-missing interest rate, LTV, and FICO score at origination.[17] From this sample, we then exclude any loans that would be prevented from participating in the updated SLR program for mechanical reasons related to payment history. This includes loans that are less than 6 months old and loans with recent payment histories that do not meet the requirements described in Section II or with insufficient information to determine whether these requirements would be met. Finally, we keep only loans in counties for which both Zillow house price indices and ACS unemployment rates are available. The full details of our sample selection procedure including the number of observations dropped at each step are described in Appendix A.1.

Our final analysis sample contains approximately 1.3 million loans that are observed for a total of 15.6 million loan-months. Table II reports summary statistics for this sample. In Panel A the unit of analysis is the individual loan and summary statistics are measured in the month of origination. In Panel B the unit of analysis is the loan-month and summary statistics are measured across all months for which a loan appears in our sample. The first column pools across all loans, whereas columns 2 and 3 report statistics separately for conventional and FHA loans, respectively. As expected, FHA loans tend to be smaller and have lower FICO scores and higher LTVs than conventional loans. However, FHA and

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[17]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.

conventional borrowers have roughly similar debt-to-income (DTI) ratios and (unconditional) interest rates. The average monthly probability of refinancing in the full sample is 0.84 percentage points and is slightly lower for FHA borrowers, who constitute approximately 17 percent of the 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 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 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:

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

13

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.[18] 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.[19] 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 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 equation (1):

$$
\begin{aligned}
Refinance_{it} = \alpha + X_{it}'\gamma + \beta_0 \cdot Post_t + \delta_0(t - \tau) + \delta_1(t - \tau) \cdot Post_t \\
+ \beta_1 \cdot Post_t^{News} + \delta_2(t - \tau^{News}) \cdot Post_t^{News} + \epsilon_{it},
\end{aligned}
\tag{2}
$$

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 estimate of β_1 would suggest that there was relatively little anticipation of the policy.

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[18]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.

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

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:

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

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, house prices, and interest rates were all falling, 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 II 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 1.5 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 1 percentage point 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 1.04 percentage points. This estimate lines up closely with the raw averages reported in Figure II and is large relative to the pre-period refinancing rate of roughly 1.25 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 loan age (one-year bins), interest rate (one-percentage point bins), and estimated home equity ($10,000 bins) of the borrower. 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 0.96 percentage points.

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 negative, and small in absolute value 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 changes led to a reduction in refinancing among existing FHA borrowers of roughly 1 percentage point 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 0.8 percentage points, similar to the 1 percentage point reduction from the event study analysis. This effect 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 falls slightly but is statistically indistinguishable from 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. Allowing for this additional flexibility does not affect 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 roughly 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:

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

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.[20] 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 III, we plot these coefficients along with their 95 percent confidence intervals. The figure shows that, conditional on the controls, trends in refinancing rates between FHA and conventional borrowers evolved in rough parallel up until the month of the policy change.[21] 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 0.7 percentage points, which is economically large and similar to the estimates from Table IV. Compared to the 1.25 percent rate of refinancing in the month prior to the policy change, it suggests that the new restrictions to the SLR program reduced FHA refinancing rates by just over 50 percent of the baseline.

The Cost of Not Refinancing

Together, these results imply that imposing employment documentation requirements and requiring borrowers to pay for closing costs out-of-pocket significantly constrains mortgage refinancing. The economic consequences of this reduced refinancing depend in part on how beneficial refinancing would have been for the individual borrowers who are prevented from doing so. If all of the borrowers excluded from the market by these constraints would have only marginally benefited from being able to refinance, then both the aggregate costs and distributional consequences of these constraints are likely to be small. However, if a large fraction of borrowers prevented from refinancing would have meaningfully benefited from access to a lower interest rate, then the consequences of these constraints are potentially substantial. In this section, we sort borrowers according to their potential benefits from refinancing and document that the fall in refinancing among FHA borrowers was driven in large part by borrowers who would have greatly benefited from being able to do so.

One way to measure the potential benefits of refinancing for an individual borrower would be to simply compare the interest rate they are currently paying to an estimate of the rate they would pay if they were to refinance their mortgage today. However, this comparison fails to take into account the potentially significant option value of waiting to refinance later. Among other things, this option value depends critically on expectations regarding future interest rates and the remaining life of the loan. To account for this, we follow Keys et al. (2016) and rely on the model of refinancing behavior provided by Agarwal et al. (2013). This

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[20]In Appendix B we also report results from specifications that are analogous to those from columns 1–3 of Table IV.

[21]While there is some evidence of a differential pre-trend in the earlier months this effect dissipates quickly, leading the trends to be roughly identical in the 6 months leading up to the policy change.

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. 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 refinance.[22] For simplicity, we ignore the role of closing costs in the calibration and return to this below in Section V when discussing mechanisms.

One of the more critical inputs into this model 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 calculate the median observed interest rate on new loans originated within each of these cells.[23] We then assume that the medians 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. With these potential rates in hand, we can construct a measure of how beneficial it would be for a borrower to refinance at a given point in time by comparing the observed gap between their current and potential interest rate with the rate gap at which it would be optimal for the borrower to refinance according to Agarwal et al. (2013) (the "ADL rate gap"). The larger the difference between the borrower's current rate gap and the ADL rate gap, the more beneficial refinancing is for that borrower.

In Figure IV, we sort borrowers according to this measure and plot their refinancing rates before and after the policy change. Each dot in the figure is the average refinancing rate among borrowers in a given 5-percentile bin of the distribution of differences between

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[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 30-year 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: ≥ 800, (800,740], (740,670], (670, 580], and ≤ 580. Similarly, for LTV we group loans into the following bins: > 90, [90, 80), [80, 70), [70, 60), and ≤ 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.

current rate gaps and ADL rate gaps. The relationship is plotted separately for loan-month observations prior to the SLR policy change (orange circles) and afterward (blue triangles). The dashed lines are the predictions from a quadratic fit of the refinancing rate to the difference in rate gaps across bins in a given time period. Negative values on the x-axis are borrowers for whom refinancing would be sub-optimal according to ADL, whereas positive values are borrowers who should refinance.

Panel A plots the results for FHA borrowers. Two features of this figure are worth pointing out. First, refinancing rates fall after the policy change in every single bin. This is consistent with the large average fall in refinancing among FHA borrowers documented above. Second, and more importantly, the fall in refinancing is largest among precisely the set of borrowers who would benefit most from being able to refinance. Monthly refinancing rates fall from approximately 2 percent to 0.5 percent for borrowers with the largest potential benefits to refinancing. In contrast, the differences for borrowers who would either not benefit from refinancing or only marginally benefit, though positive, are negligible in economic magnitude. This implies that the majority of the overall fall in refinancing documented above is driven by borrowers with a substantial benefit to refinancing. For completeness, Panel B plots the same relationships for conventional borrowers. Consistent with the difference-in-differences analysis, there is no evidence of a systematic fall in refinancing after the policy change for these borrowers. Moreover, in this segment of the market the relationship between refinancing and the estimated benefits from doing so is generally upward sloping and similar both before and after the policy change.[25]

As an alternative way to gauge the costs of these constraints for FHA borrowers we can calculate the potential foregone monthly savings from not refinancing. Given their current interest rates, loan sizes, and estimated potential interest rates, FHA borrowers for whom the ADL model implies it would be optimal to refinance could have saved an average of $160 per month if they were to have refinanced into a new 30-year loan (a 17 percent reduction in monthly payments). This is a substantial increase in disposable income. The average FHA borrower only earns approximately $66,000 per year (Ehrlich and Perry, 2015) and most of the borrowers prevented from refinancing by these constraints are likely earning far less given that the constraints apply specifically to *unemployed* borrowers. Similar differences in monthly payment burdens have been shown to have large effects on mortgage default rates and consumption in populations that likely include a larger fraction of employed households (Ehrlich and Perry, 2015; Agarwal et al., 2017b; Di Maggio et al., 2017; Ganong and Noel,

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[25]The non-monotonicity at high values of the benefits to refinancing in Panel B is likely driven by the fact that many underwater conventional borrowers were prevented from refinancing during this period. As interest rates continued to fall over time, these borrowers experienced growing potential benefits to refinancing that they were unable to act on.

2017). These effects would presumably be even larger among unemployed and liquidity constrained households who have higher marginal propensities to consume and are most directly affected by the constraints we consider.

V Mechanisms

Our results thus far indicate that the new employment documentation requirements and restrictions on financing closing costs collectively led to a large reduction in FHA refinancing that was likely to have been economically costly for the affected borrowers. 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 occurring at the same time, these two changes affected observably distinct sets of borrowers, which allows us to plausibly trace out their respective effects. However, given that these two changes occurred simultaneously and likely interact, we will not be able to decompose the overall effect into the portion that is driven by each restriction. Instead, we will focus on establishing whether or not each mechanism had economically significant and independent effects on refinancing.

V.A Employment Documentation Requirements

Graphical Evidence

The change in employment 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 V 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 quarter of the distribution of these changes across counties.[26] In Panels A and B, we plot monthly refinancing rates for FHA borrowers

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[26]We use changes in unemployment as our proxy rather than levels to address the fact that county-level unemployment rates exhibit substantial noise that is highly correlated with the level of unemployment.

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 roughly 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 by the new employment documentation requirements.

Empirical Strategy

To more formally analyze the effect of the change in employment documentation requirements, we use a triple-differences strategy that is directly motivated by the results in Figure V 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 V, 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

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Taking the change alleviates this issue as the ACS-reported measurement error is only weakly correlated with the change in unemployment ($\rho = 0.16$) despite the fact that the change in unemployment is highly correlated with the level ($\rho = 0.70$). Moreover, when regressing the level of unemployment on the change the coefficient on the change is effectively one. To further alleviate issues of measurement error, we take two year averages on either end of the difference. Formally, we calculate the 2006 to 2009 change in unemployment as $\Delta UR_{09-06} = (UR_{10} + UR_{09})/2 - (UR_{06} + UR_{05})/2$.

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 employment documentation requirements were an important barrier to refinancing during this period, we should expect to find $\beta_5 < 0$.

One key confound that needs to be considered when estimating equation (5) is 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, when estimating (5), we will always focus only on the sample of borrowers who are in positive equity in a given month. 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. Second, because our estimate of home equity is measured with error, we will also 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 current equity ($10,000 bins). 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 residual independent effect of the new closing cost requirements arising from the fact that some borrowers who we deem to be in positive equity are not.[27]

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[27] In Appendix B, we also estimate an alternative to this specification in the full sample of loans allowing

Finally, it is important to note that the results from these triple-difference regressions will only pick up the *differential* fall in refinancing between FHA borrowers in high- relative to low-unemployment counties. If the introduction of the employment documentation requirements also reduced refinancing rates among employed borrowers, perhaps due to the increased psychological hassle costs associated with providing documentation, then our results will not reflect this effect.[28] Unfortunately, we are not aware of any reliable proxy for cross-sectional variation in these types of costs that would allow for a direct measurement of the role that such hassle costs may have played. While these costs are likely to be relatively small in comparison to the substantial potential benefits to refinancing discussed in Section IV, we will discuss how their presence may affect the interpretation of our results below.

Results

Columns 2–5 of Table V present our triple-difference estimates of the effect of the change in employment documentation requirements on FHA refinancing. For reference, in column 1 we also report results from a difference-in-differences specification that is identical to the specification used to estimate the overall effect in column 4 of Table IV but estimated only in the subsample of borrowers with positive equity. The effect of the policy in this subsample is roughly two-thirds as large as the corresponding effect in the full sample. This difference is exactly what would be expected given that the closing cost requirements are non-binding for borrowers with positive equity.

Column 2 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 0.06 percentage points for each one percentage point increase in the county-level unemployment rate. The average loan in our sample was in a county that experienced an increase in unemployment of roughly 4 percentage points. At that change in unemployment rates, this effect is enough to account for a reduction in refinancing of 0.23 percentage points per month, which is roughly half the size of the overall effect reported in column 1.

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 3, we include the same detailed set of

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for both the employment documentation and closing costs to separately affect FHA refinancing rates. The results from this specification are qualitatively similar to those from our preferred estimates in the restricted sample of borrowers who are in positive equity.

[28]These effects would, however, be reflected in the difference-in-difference and event study estimates.

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 bin, and LTV-by-FICO bins). Column 4 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 2. In column 5, we allow the controls for home equity to enter even more flexibly by interacting each equity bin 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 nearly identical to that in column 4, which provides confidence that these results primarily reflect the effect of the change in employment documentation requirements.

To further explore the robustness of this relationship, in Panel A of Figure VI we report estimates from an alternative and more flexible parameterization that allows for the effect to vary by month of observation. Specifically, we plot coefficient estimates from the following specification:

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

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 change. Although the estimates have relatively wide confidence intervals, there is a noticeable level shift that occurs 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 that is correlated with unemployment.

Interpreting the Magnitudes

While our results provide compelling evidence that the new employment documentation requirements were a substantial barrier to refinancing for FHA borrowers during this period, 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 column 5 by 1.25 would yield an estimate of 0.0376. This implies that the likelihood of refinancing for the average borrower in our sample was roughly 0.15 percentage points lower than it otherwise would have been as a result of the fall in employment between 2006 and 2009.[29] This effect would be even larger in counties hit by above average employment shocks, which has important implications for the distributional impact of stimulative policies, such as the first round of quantitative easing (QE1), that operate in large part through the mortgage refinancing channel (Di Maggio et al., 2016).

As an alternative way to interpret our triple difference estimates, we can also convert them into an implied rate at which unemployed borrowers would refinance if permitted. This rate provides a direct measure of the latent demand for refinancing among unemployed borrowers. For clarity of exposition, we work with a simplified expression for the probability of refinancing. In particular, let r_0^{FHA} and r_1^{FHA} denote the monthly probabilities that an FHA borrower refinances during the pre- and post-policy periods, respectively. If we assume that employed and unemployed borrowers each refinance at constant rates when permitted, then we can express these two FHA refinancing rates as weighted averages of the refinancing

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[29]This calculation is based on a 4 percent average change in county-level unemployment: $\frac{-0.047}{1.25} \times 4.0 = -0.15$.

rates of employed (r_E) and unemployed (r_U) borrowers:

$$r_0^{FHA} = r_U \times UR + r_E \times (1 - UR)$$
$$r_1^{FHA} = r_E \times (1 - UR),$$

(7)

where UR denotes the unemployment rate and the second line follows from the fact that unemployed FHA borrowers are prohibited from refinancing after the policy change.

Using similar expressions for the refinancing probabilities in the conventional market, we can express our difference-in-differences estimate as

$$DiD \triangleq \left(r_1^{FHA} - r_0^{FHA}\right) - \left(r_1^{Conv} - r_0^{Conv}\right)$$
$$= \left(r_1^{FHA} - r_0^{FHA}\right) - 0$$
$$= -r_U \times UR,$$

(8)

where the second line follows from the assumption that the change in FHA policy had no effects on conventional refinancing rates.[30] Finally, the coefficient of interest reported in our regressions is a triple differences estimate, which is simply the partial derivative of the difference-in-differences estimate with respect to the unemployment rate:

$$\frac{\partial DiD}{\partial UR} = -r_U = -0.047 \implies r_U = 0.047.$$

(9)

Rescaled by 100 to account for the units of the unemployment rate, our estimates imply that unemployed borrowers would refinance at a monthly rate of about 4.7 percent if permitted. This is roughly 5 times the refinancing rate of the average borrower during this period. Alternatively, if we use the same 1.25 scaling factor as above, the implied rate at which an unemployed borrower would refinance if permitted would be 3.76 percent.

The derivation of equation (9) above relies on the assumption that the refinancing rate for employed FHA borrowers is the same before and after the policy change. However, if employed borrowers also reduced their refinancing rates, perhaps due to the added psychological hassle associated with providing documentation, then our results would actually *underestimate* the true refinancing rate among unemployed borrowers. To see this, suppose that employed FHA refinancing falls from r_E to $r_E - \rho$ after the policy change, where ρ represents the fall in refinancing due to hassle costs. In this case, the second line of equation (7) would be $r_1^{FHA} = (r_E - \rho) \times (1 - UR)$, which would yield an alternative expression for the difference-in-difference estimate: $DiD \triangleq -r_U \times UR - \rho \times (1 - UR)$. Differentiating

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[30] For simplicity, we omit borrower characteristics as well as secular trends in refinancing and unemployment rates from this analysis as they would simply difference out in the end.

this with respect to the unemployment rate would imply that our triple-difference estimate is actually equal to $\frac{\partial DiD}{\partial UR} = -r_U + \rho = -0.047$, which underestimates the true refinancing rate for unemployed borrowers by exactly the amount of the (unobserved) fall in refinancing among employed borrowers due to hassle costs.

Implied Aggregate Effects of Employment Documentation Requirements

Thus far, our analysis has focused on the direct effect of the changes to the SLR program on FHA borrowers themselves. However, the estimate from equation (9) also provides us with a way to evaluate how the standard practice of prohibiting unemployed borrowers from refinancing may have affected aggregate refinancing outside the FHA market during the Great Recession. In this section, we extrapolate our estimate to the general population to perform a simple back-of-the-envelope calculation demonstrating that this effect may indeed have been substantial.

As a starting point for this calculation, note that the Bureau of Labor Statistics (BLS) estimated that there were 15.1 million unemployed persons in the U.S. as of December 2009.[31] Using data from the 2009 SCF, we calculate that roughly 38 percent of these unemployed persons also held mortgages.[32] However, even if they were permitted to refinance, not all of these unemployed mortgage borrowers would have been able to do so. In particular, many of them were likely to be in negative equity, which *was* a binding constraint on refinancing outside the FHA market during this time. Using information on self-reported house values and the balance of outstanding mortgages reported in the SCF, we estimate that roughly 25 percent of unemployed borrowers were in negative equity as of 2009.[33] Together, this means that there were approximately 4.3 million unemployed mortgage borrowers in positive equity who were potentially prevented from refinancing in 2009 due to employment documentation requirements. Our estimates from equation (9) imply that these borrowers would have refinanced at a monthly rate of 4.7 percent if permitted. This means that there would have been roughly 2.4 million more refinances in 2009 had these unemployed borrowers been able to refinance. Our data on potential interest rates discussed in Section IV indicate that the

---

[31]Source: U.S. Bureau of Labor Statistics, Unemployment Level [UNEMPLOY], retrieved from FRED, Federal Reserve Bank of St. Louis; https://fred.stlouisfed.org/series/UNEMPLOY, August 19, 2019.

[32]Strictly speaking, the SCF is a survey of households, not persons. We are, however, unaware of any other data set that would allow us to calculate the share of unemployed persons with a mortgage in 2009.

[33]In performing this calculation we aggregate balances on both first and second mortgages outstanding and compare them to an adjusted estimate of the home value that takes into account the well-known upward bias in self-reported housing wealth. In particular, we multiply the self-reported house value by 0.9, reflecting the findings in Benítez-Silva et al. (2015) that homeowners over-report housing values by about 8 percent on average and in Chan et al. (2016) that this bias is larger during housing busts. Using this correction factor, the implied share of *all* mortgage borrowers (not just the unemployed) that were in negative equity in 2009 is 23.9 percent, which is very close to the aggregate estimates reported by CoreLogic at the time.

average borrower who refinanced in 2009 would have lowered their interest rate by roughly 104 basis points, resulting in annual savings of approximately \$3000. Thus, over the course of 2009, our estimates imply that allowing unemployed borrowers to refinance would have provided these already cash-strapped borrowers with an additional \$7.2 billion in disposable income. This represents almost 10 percent of the entire \$79.5 billion paid out to unemployed people through the unemployment insurance system in 2009.[34]

As an alternative way of putting these numbers into context, we can also compare them to the increase in refinancing and mortgage modification that was achieved by the two largest household debt relief policies implemented during the financial crisis: the Home Affordable Refinancing, and Home Affordable Modification Programs (HARP and HAMP). These policies aimed to provide debt relief to underwater and distressed borrowers by extending government-backed credit guarantees to cover refinances of negative equity GSE loans and by providing incentives for private servicers to renegotiate terms on delinquent mortgages. Together, Agarwal et al. (2017a,b) estimate that HARP and HAMP generated an additional 3 million refinances and 1 million mortgage modifications between March 2009 and December of 2012, and that these refinances and modifications saved borrowers between \$3000–\$4000 annually. Importantly, both of these programs explicitly excluded unemployed borrowers from participating. Thus, our estimates suggests that implementing a broad-based streamline refinancing program aimed at easing employment documentation requirements could have potentially generated an additional increase in disposable income of roughly the same order of magnitude as that achieved by policies focused on relieving negative equity constraints and agency conflicts in the secondary mortgage market.

Employment Documentation Requirements vs. Negative Equity Constraints

One of the key features of our empirical setting is that it allows us to isolate the importance of employment documentation requirements from that of other potentially correlated barriers to refinancing. Chief among these is negative equity. As noted by Beraja et al. (2019), negative equity was both highly correlated with unemployment during the Great Recession and a significant barrier to refinancing. Because underwater FHA borrowers were always permitted to refinance throughout our entire sample period, our estimates reveal the direct effect of employment documentation requirements among a subset of households who were unaffected by these negative equity constraints. However, this does not directly answer the question of whether employment documentation requirements are an independently important barrier to refinancing in the general population. If all unemployed borrowers are in negative equity, then

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[34]Source: U.S. Department of Labor, Monthly Program and Financial Data, retrieved from https://oui.doleta.gov/unemploy/claimssum.asp, August 19, 2019.

relaxing employment documentation requirements would have no effect on the refinancing rate outside the FHA market. The statistic we cited from the SCF above—that only 25 percent of households with a mortgage and an unemployed member were in negative equity in 2009—suggests that this is unlikely to be the case. In Figure VII we provide further evidence indicating that employment documentation requirements are an independent constraint on refinancing that, in many recessions, may even be more important than negative equity.

Panel A. of Figure VII plots the cross-sectional relationship between county-level unemployment and negative equity for borrowers in our main analysis sample. For each county, we calculate the share of loans that were ever in negative equity at some point in our sample period and then group counties into bins according to these shares. Within these groups we then plot the cross-county distribution of changes in unemployment between 2006 and 2009. Each box plot in the figure reports the 5th, 25th, 50th, 75th and 95th percentiles of the distribution of unemployment rate changes among counties with negative equity shares in the ranges indicated on the x-axis. The mean of this distribution is indicated by the orange diamond associated with each box plot. As is well-known, the two measures are closely related on average; mean unemployment rises with the negative equity share. However, the figure also makes clear that there is enormous variation around this trend. For example, unemployment rates increased by up to almost 10 percentage points even in counties where the share of negative equity loans never exceeded the national average of 25 percent. Moreover, regardless of negative equity share, there were always some counties experiencing increases in unemployment above the national average of 5 percent. Thus, consistent with the evidence from the SCF, there are many borrowers in our sample for whom employment documentation requirements were likely to bind even though negative equity constraints did not.

Panel B. of Figure VII further demonstrates that the positive relationship between unemployment and negative equity that existed during the Great Recession was atypical of recent U.S. experience. The figure plots time series patterns in unemployment and house prices at the national level over the longest horizon for which we can reliably measure aggregate house prices. The unemployment rate is plotted on the left axis using data from the BLS and house prices are plotted on the right axis using the Federal Housing Finance Agency (FHFA) House Price Index. The time periods shaded in gray correspond to NBER-dated recessions. As expected, the unemployment rate spikes in all five recessions shown. The patterns for house prices, however, are remarkably different. It is only during the Great Recession that national house prices fall. In all four of the other recessions during which it is possible to measure aggregate house prices, there is no meaningful or sustained depreciation. This means that it was employment documentation requirements, not negative equity, that

were more likely to be the binding constraint on refinancing during the "typical" recession. In that sense, we think that the estimates in this paper, which focus on employment documentation requirements, are a strong complement to existing evidence on the importance of negative equity as a barrier to refinancing during the Great Recession.

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. Thus, while negative equity FHA borrowers were still fully permitted to refinance through the SLR program, the out-of-pocket costs associated with doing so were significantly higher after the policy change. To study the effects of this change on 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 VIII 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 three 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 bor-

rowers 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:

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

(10)

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 key 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. This could lead us to conflate the effect of the new closing cost requirements with the employment documentation effect. To reduce the likelihood that our estimates merely reflect variation in unemployment,

when estimating equation (10) we will always restrict attention to the subsample of counties with below median increases in unemployment. We will also estimate specifications that allow for the effect of unemployment to flexibly and differentially affect FHA borrowers before and after the policy change. In particular, our preferred specification will include controls for the complete interaction between the FHA dummy, the Post indicator, and a set of fixed effects for each decile of the distribution of county-level unemployment changes. 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 the effect of requiring borrowers to pay for upfront costs out-of-pocket. For reference, column 1 reports baseline results from a difference-in-differences specification identical to our preferred specification used to measure the overall effect in Table IV but restricted to the set of counties with below median increases in unemployment. As expected, the effect in this subsample is substantially smaller than in the full sample due to the fact that the employment documentation requirements are less likely to bind for borrowers in these counties.

The remaining columns present our main triple-difference estimates. Column 2, which controls only for month fixed effects, CBSA fixed effects and FHA-specific linear time trends, suggests that FHA borrowers who were required to pay for closing costs out-of-pocket were 0.8 percentage points less likely to refinance subsequent to the policy change relative to their positive equity counterparts. Column 3 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 4 further interacts these controls with the FHA and Post indicators. The coefficient estimate falls to −0.56 when these controls are included but remains statistically significant and is still economically quite large. In column 5 we directly control for the independent effect of the employment documentation requirements by fully interacting each unemployment change decile fixed effect with the the FHA and Post indicators. Including these controls has a minimal effect on the coefficient, which provides assurance that our sample restriction and other controls are effectively isolating the effect of the closing cost requirement. 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 Panel B of Figure VI, we present results from a more flexible version of the triple difference regression that allows for the effect to

vary by month of observation. These results parallel the results presented for unemployment changes in Panel A and are derived from specification directly analogous to equation (6). In particular, we fully interact the indicator for low-equity with month fixed effects and the FHA dummy, which allows for the effect of negative equity to vary freely over time. We omit the December 2009 month fixed effect so that all results can be interpreted relative to the month just prior to the policy change. While the estimates from this specification are noisy, there is a visible level shift down in refinancing that occurs in precisely the month that the policy takes effect. All but one of the coefficient estimates in the pre-period are statistically indistinguishable from zero, whereas the post-period coefficients are all negative, significant, and of roughly the same magnitude as the pooled effects reported in Table VI.

Optimality vs. 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, 1991; Carroll, 2001; Zeldes, 1989). These constraints could prevent some borrowers from being able to pay the upfront costs needed to refinance.[35] Second, being forced to pay costs upfront instead of financing them with 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 attempt 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 need 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 draw on the same approach used in Section IV and compute the implied rate gap at which it would be optimal for a borrower to refinance according to the model in Agarwal et al. (2013). However, for this exercise we also directly incorporate the role of closing costs. Specifically, whereas before we assumed that all closing costs could be rolled into the new loan when calculating the ADL

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[35]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 constraints.

rate gaps, here we require that borrowers pay a substantial sum out-of-pocket. We then re-calculate the ADL rate gaps and construct an indicator for whether it would be optimal for a borrower to refinance in each month given her current rate, potential rate, and the estimated upfront cost. Because there is no widely available 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.

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 5 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 in this subsample is still substantially negative and in fact larger than the analogous results for the full sample in column 5. The fact that refinancing rates fall even in this subsample suggests that lack of liquidity is an important driver of the fall in refinancing for negative equity borrowers following the policy change.

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 meaningful frictions to mortgage refinancing. This suggests that the pass-through of monetary or other policies that aim to stimulate consumption through the mortgage refinancing channel may be less efficient in recessions, when unemployment is higher and households have less liquidity. Moreover, these frictions are likely to bind most for precisely the households whose expenditures may be most sensitive to reduced rates—those with little cash-on-hand or who recently experienced a negative income shock. This fact may exacerbate the already unequal impacts of recessions 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. However, our results suggest that there are a significant number of borrowers that would refinance their mortgages when lower rates are on offer but cannot because of these large frictions in the mortgage market.

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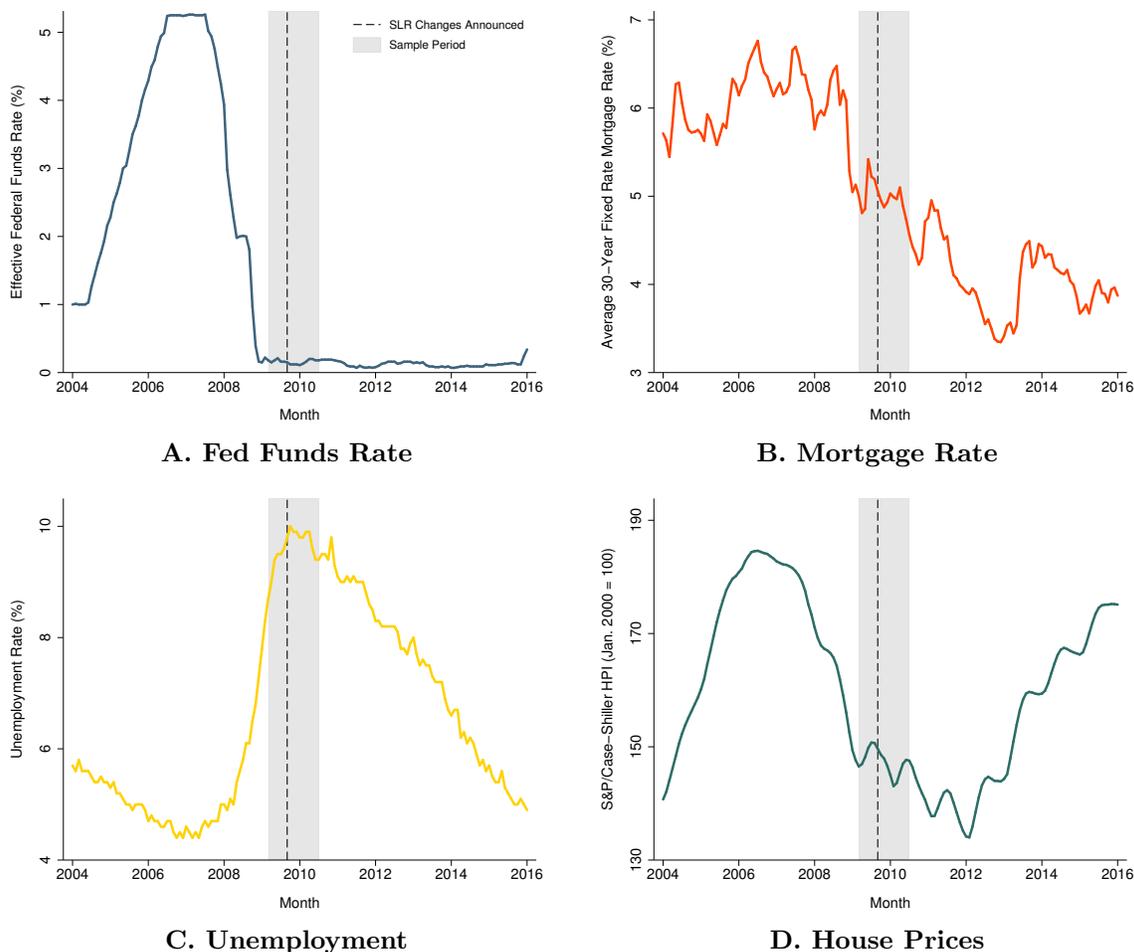
FIGURE I

Aggregate Economic Conditions Around the Time of the FHA Policy Changes

NOTE.—This figure plots monthly time series trends for several key measures of aggregate economic conditions around the time of the FHA SLR policy changes. The vertically dashed line in each panel marks the month of the policy announcement (September 2009). The shaded grey area marks our primary sample period, which covers the window of time spanning 6 months prior to the policy announcement and 6 months after its implementation (March, 2009–July 2010). Panel A. plots the effective federal funds rate and is sourced from the Board of Governors' H.15 Release on Selected Interest Rates. Panel B. reports the average offered interest rate on new 30-year fixed rate mortgaged from Freddie Mac's Primary Mortgage Market Survey. Panel C. plots the Civilian Unemployment Rate from the Bureau of Labor Statistics' monthly Employment Situation Summary. Panel D. plots the S&P/Case-Shiller U.S. National House Price Index and is normalized to a value of 100 in January of 2000.
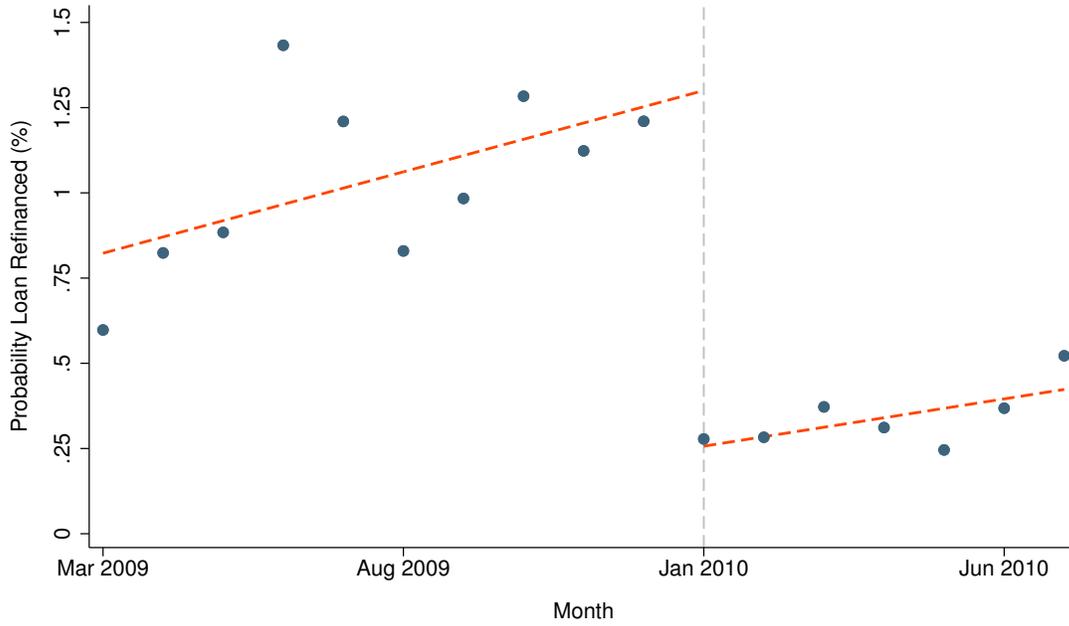
A. FHA Loans
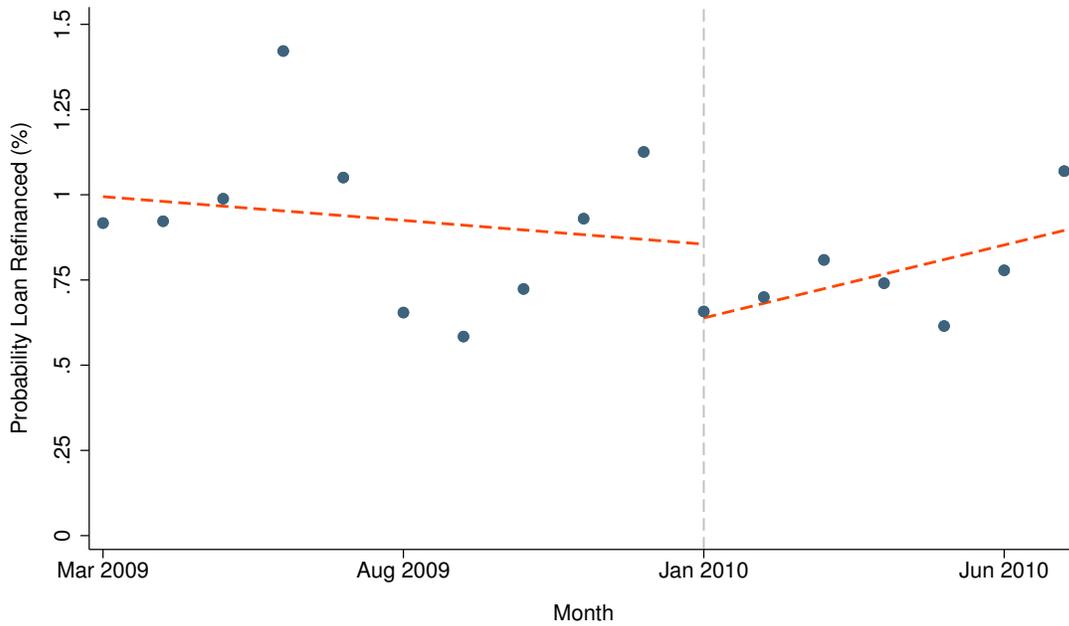
B. Conventional Loans

FIGURE II

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.
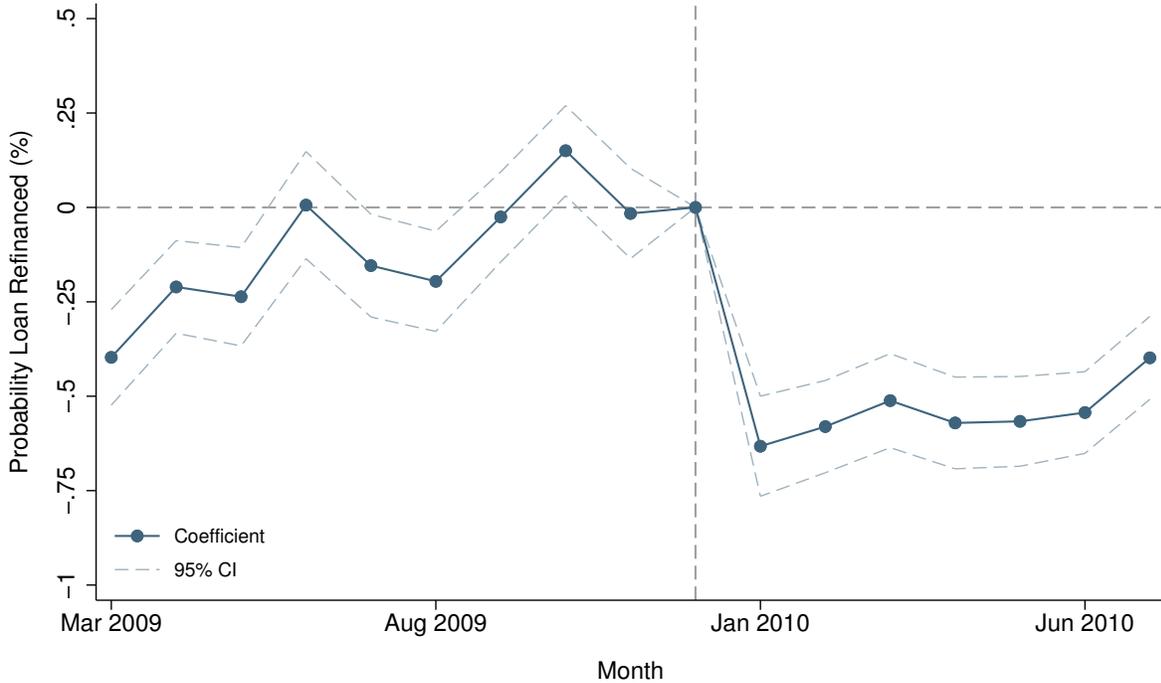
FIGURE III

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 loan age (one-year bins), interest rate (one-percentage point bins) and the borrower's estimated home-equity ($10,000 bins), 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.

FIGURE IV

The Relationship between Refinancing and the Benefits to Refinancing

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A. FHA: High ΔUnemployment

B. FHA: Low ΔUnemployment

C. Conventional: High ΔUnemployment

D. Conventional: Low ΔUnemployment

FIGURE V

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 quarter of the 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.
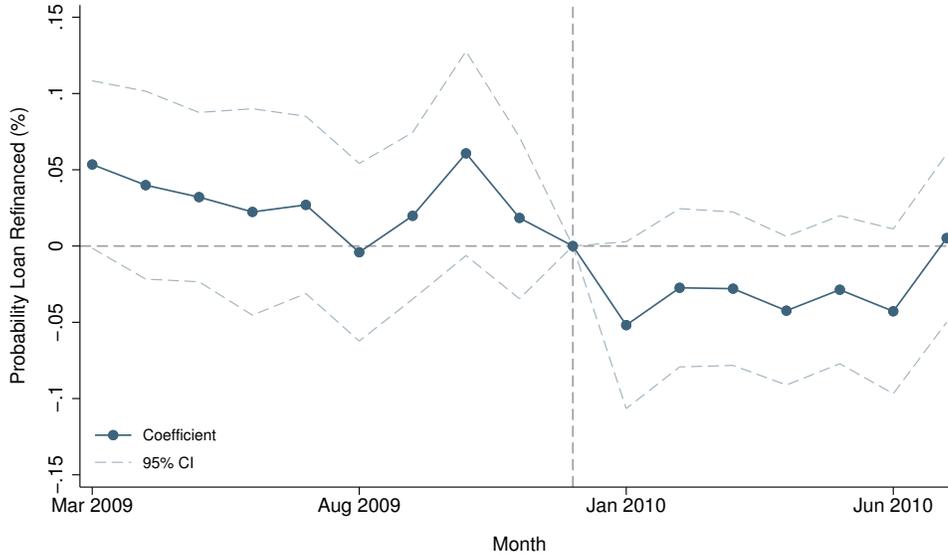
A. Employment Documentation Effect
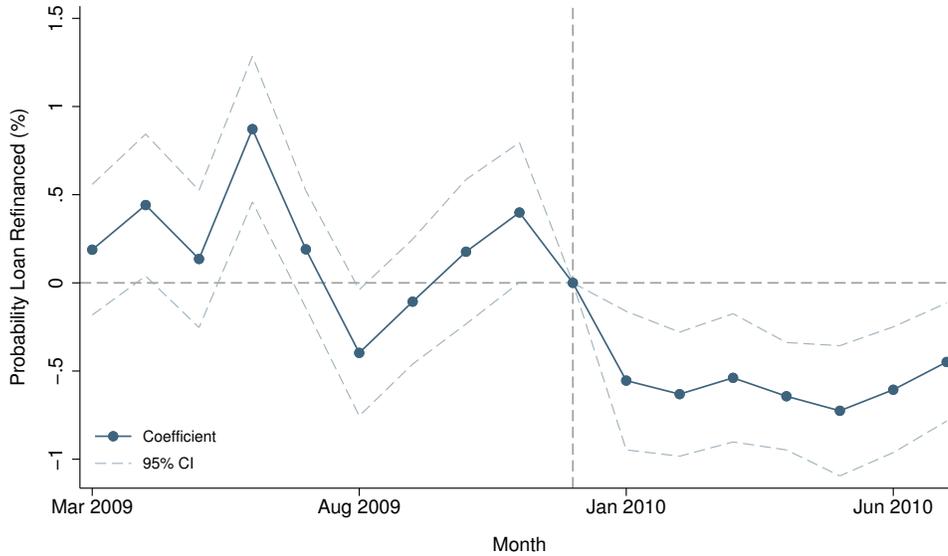
B. Upfront Costs Effect

FIGURE VI

Triple Difference Estimates by Month of Observation

NOTE.—This figure reports estimates of how the effect of the change in FHA policies on FHA refinancing varied across differences in county-level unemployment rates (Panel A) and borrower equity (Panel B). Estimates are derived from a flexible triple difference specification that allows the effect to vary freely by month of observation. 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 instead interacts the FHA dummy and month fixed effects with an indicator for whether the borrower's estimated home equity is less than or equal to zero. Both regressions include fixed effects for the month of observation and the CBSA of the property. The coefficient for December 2009 is normalized to zero, so that all estimates can be interpreted as the differential change in the monthly probability of refinancing relative to the month prior to when the policy changes went into effect. The 95% confidence intervals are based on standard errors that are clustered at the CBSA level.
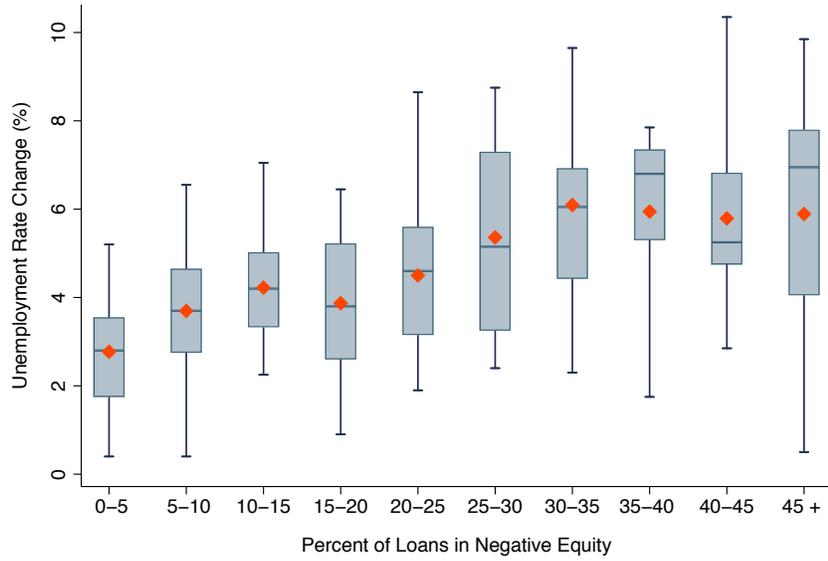
A. Unemployment and Negative Equity in the Cross Section
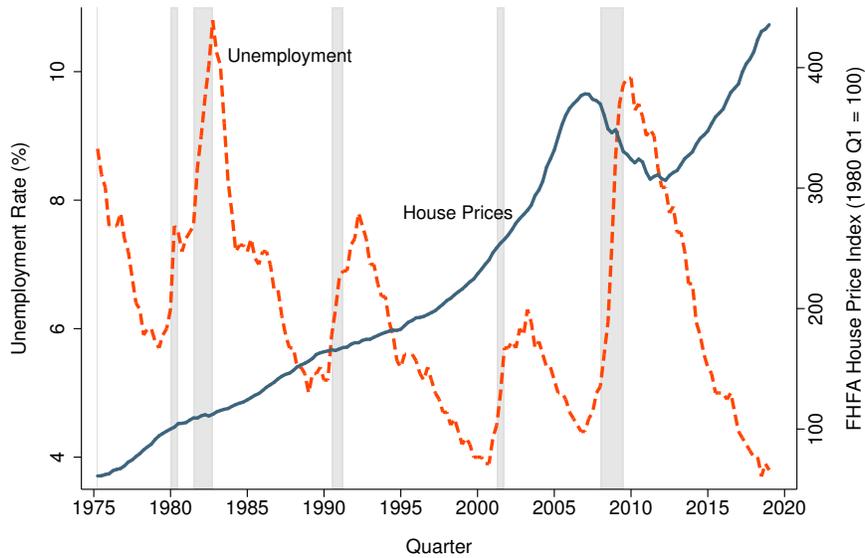
B. Unemployment and House Prices in the Time Series

FIGURE VII

The Relationship Between Unemployment and Negative Equity

NOTE.—This figure plots cross-sectional and time series patterns in the relationship between unemployment, negative equity, and house prices. Panel A. plots the relationship between county-level unemployment changes and negative equity during the Great Recession. Counties in the primary analysis sample are grouped into bins based on the share of loans in the county that were ever in negative equity at some point during the sample period. Each box plot in the figure reports the 5th, 25th, 50th, 75th, and 95th percentiles of the distribution of unemployment rate changes between 2006 and 2009 among counties with negative equity shares in the rages indicated on the x-axis. The mean change in unemployment for counties in a given bin is indicated with an orange diamond. Panel B. plots the quarterly time series of national unemployment from the BLS (left axis) and house prices as measured by the FHFA single-family house price index (right axis). The house price index is normalized to 1 in the first quarter of 1980. Shaded grey areas correspond to NBER-dated recessions.
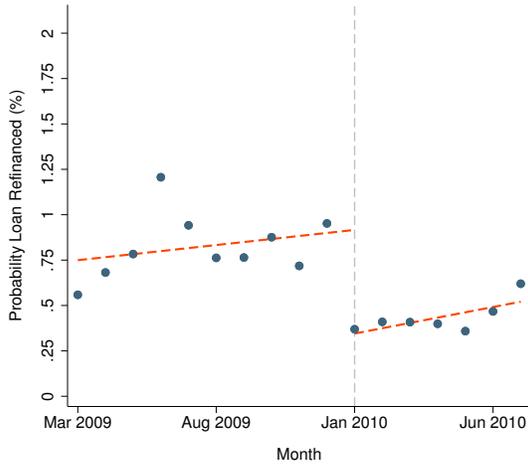
47
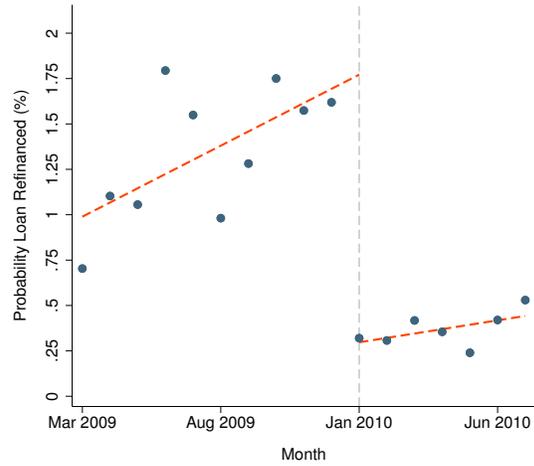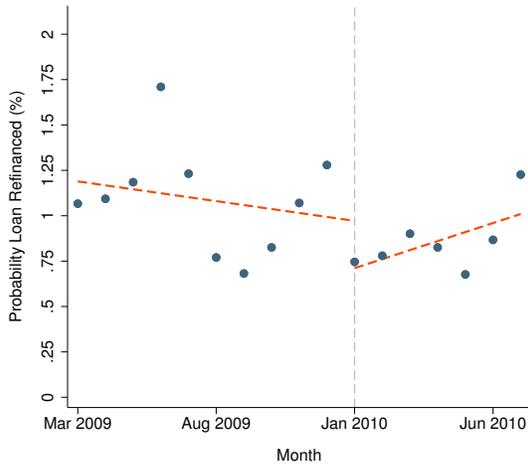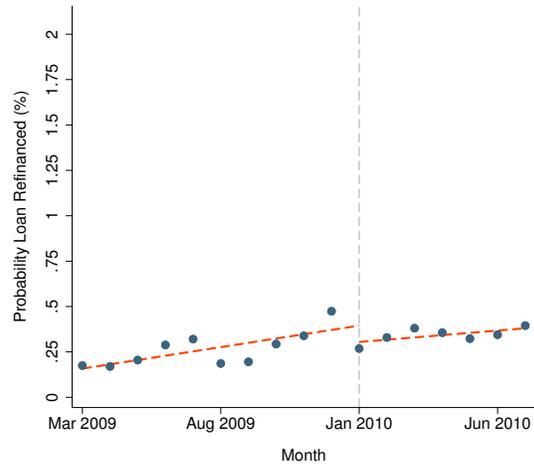
FIGURE VIII

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. Home equity is estimated by subtracting 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.

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 pre-payment 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

	All Loans	Conventional	FHA
Panel A. Loan-level Characteristics at Origination			
Loan Amount ($1000's)	216.91	226.01	171.17
	(139.75)	(145.86)	(90.80)
FICO Score	722.82	732.21	675.56
	(61.48)	(55.90)	(66.29)
Loan-to-Value	74.29	70.15	95.10
	(18.66)	(17.41)	(7.48)
Back-End Debt-to-Income	37.52	37.14	39.68
	(15.49)	(16.08)	(11.37)
Interest Rate	5.90	5.91	5.82
	(0.81)	(0.83)	(0.71)
Panel B. Loan-month Characteristics			
Current Balance ($1000's)	195.57	201.46	155.45
	(134.71)	(139.52)	(85.50)
Estimated Equity ($1000's)	96.05	109.42	4.98
	(174.82)	(183.06)	(31.03)
Current Loan-to-Value	75.41	72.20	97.30
	(25.97)	(25.43)	(17.74)
Loan Age (Years)	3.79	3.99	2.44
	(2.33)	(2.26)	(2.32)
Percent Refinanced	0.84	0.86	0.70
	—	—	—
Number of Loans	1,309,393	1,092,163	217,230
Number of Loan-months	15,645,645	13,643,184	2,002,461

NOTE.—This table reports descriptive statistics for loans in the final analysis sample. All table entries represent sample means or, in parentheses, standard deviations. Summary statistics are presented pooling across all loan types (column 1) as well as separately for conventional (column 2) and FHA loans (column 3). In Panel A the unit of analysis is the individual loan and summary statistics are measured in the month of origination. In Panel B the unit of analysis is the loan-month and summary statistics are measured across all months for which a loan appears in our sample.

TABLE III

The Effect of the Policy Changes on FHA Refinancing: Event Study

	(1)	(2)	(3)	(4)
Post	−1.041***	−0.960***	−1.026***	−1.013***
	(0.077)	(0.071)	(0.073)	(0.073)
Post News			−0.157***	−0.112***
			(0.039)	(0.040)
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	2,002,461	2,002,461	2,002,461	2,002,461

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 loan age (one-year bins), interest rate (one-percentage point bins) and the borrower's estimated home-equity ($10,000 bins), 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	0.511***	0.827***	0.813***	2.041***
	(0.062)	(0.062)	(0.055)	(0.315)
FHA × Post	−0.804***	−0.727***	−0.709***	−0.708***
	(0.065)	(0.062)	(0.049)	(0.045)
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	15,645,645	15,645,645	15,645,645	15,645,645

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 loan age (one-year bins), interest rate (one-percentage point bins) and the borrower's estimated home-equity ($10,000 bins), 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

TABLE V

REFINANCING AND UNEMPLOYMENT: TRIPLE DIFFERENCE ESTIMATES

	(1)	(2)	(3)	(4)	(5)
FHA	1.498***	−0.054	0.353***	1.321***	1.208***
	(0.356)	(0.055)	(0.065)	(0.355)	(0.349)
FHA × Post	−0.476***	−0.280***	−0.231***	−0.315***	−0.349***
	(0.046)	(0.081)	(0.073)	(0.068)	(0.077)
FHA × ΔUR		0.046***	0.042***	0.047***	0.047***
		(0.011)	(0.012)	(0.012)	(0.012)
FHA × ΔUR × Post		−0.057***	−0.047***	−0.046***	−0.047***
		(0.015)	(0.013)	(0.014)	(0.014)
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
Number of Observations	13,250,266	13,250,266	13,250,266	13,250,266	13,250,266

NOTE.—This table reports estimates of the effect of the change in SLR income documentation requirements on the monthly probability that an FHA loan refinances. In all specifications the sample includes only loan-months for which the borrower is estimated to be in positive equity. The outcome variable is an indicator for whether or not a loan refinances in the month of observation and is multiplied by 100, so that all coefficients can be interpreted as percentage point changes. For reference, column 1 reports estimates from a baseline difference-in-difference regression that is directly analogous to the specification in column 4 of Table IV, but which only includes loan-months in positive equity. Coefficients are reported for the FHA "treatment" dummy as well as its interaction with the Post dummy. Columns 2–5 report estimates from triple difference regressions that further interact the FHA and Post dummies with the change in county-level employment, which is also included linearly in the regression. 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 3 adds fixed effects for the current loan age (one-year bins), interest rate (one-percentage point bins) and the borrower's estimated home-equity ($10,000 bins), as well as the full pairwise interaction between the borrower's LTV (10-point bins) and FICO score (50-point bins) at origination. Column 4 further interacts all of the additional fixed effects contained in column 3 with the Post dummy and the FHA indicator. Column 5 allows for an unrestricted effect of home equity by interacting each $10,000 home equity bin fixed effect with the complete interaction between the FHA and Post indicators. 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	0.627	0.060	0.299***	0.650	0.522	1.246**
	(0.432)	(0.044)	(0.055)	(0.423)	(0.436)	(0.618)
FHA × Post	−0.551***	−0.403***	−0.357***	−0.458***	−0.272***	−1.107***
	(0.059)	(0.066)	(0.061)	(0.050)	(0.067)	(0.270)
FHA × Low Equity		1.089***	0.969***	0.899***	0.869***	1.076***
		(0.165)	(0.187)	(0.187)	(0.187)	(0.283)
FHA × Low Equity × Post		−0.800***	−0.746***	−0.560***	−0.510***	−0.801***
		(0.127)	(0.132)	(0.130)	(0.127)	(0.258)
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
Optimal Refi Subsample						X
Number of Observations	5,441,498	5,441,498	5,441,498	5,441,498	5,441,498	884,809

NOTE.—This table reports estimates of the effect of the change in SLR closing cost requirements on the monthly probability that an FHA loan refinances. In all specifications the sample includes only counties for which the 2006–2009 change in unemployment was below the median across counties. The outcome variable is an indicator for whether or not a loan refinances in the month of observation and is multiplied by 100, so that all coefficients can be interpreted as percentage point changes. For reference, column 1 reports estimates from a baseline difference-in-difference regression that is directly analogous to the specification in column 4 of Table IV, but which only includes counties with below median increases in unemployment rates. Coefficients are reported for the FHA "treatment" dummy as well as its interaction with the Post dummy. Columns 2–6 report estimates from triple difference regressions that further interact the FHA and Post dummies with an indicator for whether the borrower's estimated home equity is less than zero ("Low Equity"), which is also included in the regression. 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 3 adds fixed effects for the current loan 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 4 further interacts all of the additional fixed effects contained in column 3 with the Post dummy and the FHA indicator. Column 5 allows for an unrestricted effect of unemployment changes by interacting each unemployment change decile fixed effect with the complete interaction between the FHA and Post indicators. 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.

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

Online Appendices

Anthony A. DeFusco   John Mondragon

A Data Appendix

A.1 Sample Selection

To conduct our empirical analysis we make use of a loan-level data set containing detailed information on individual mortgages active during the 6 months prior to the SLR policy change announcement date and 6 months after its implementation. The raw data was purchased from CoreLogic and is provided to them by a consortium of participating mortgage servicers.

Because the data are very large, we select our primary analysis sample from an initial 20 percent random sample of relatively "standard" mortgages. This initial sample includes 20 percent of all first-lien, fixed-rate, 30-year, single-family, owner-occupied mortgages with non-missing interest rates, LTVs, and FICO scores at origination that were active at some point between March, 2009 and July 2010. This initial sample contains 1,845,744 unique loans and 25,876,460 loan-month observations as reported in the first row of Table A.1.

Starting from this sample, we then impose a series of restrictions to make sure that we are able to accurately determine both whether a loan was paid off in a particular month and the reason for that payoff. First, we drop any loans for which the payoff status cannot be known with certainty during our entire sample period. This includes loans that were transferred to servicers not in our data set as well as loans with missing or unknown delinquency information.[1] We then drop any loan-month observations observed after the recorded payoff date. In the vast majority of cases, these observations report a zero remaining balance. Finally, we drop a small number of loans where the payoff information is inconsistently recorded across differing variables in the data.[2] The number of loans and loan-month observations dropped by each of these filters are reported in rows 2–4 of Table A.1. Together, they eliminate 86,138 loans and their associated 1,402,766 loan-month observations.

From the remaining sample of loans, we then impose a set of additional restrictions to ensure that all observations in our final analysis sample meet the payment history requirements imposed under the updated SLR program. Specifically, as described in Section II, to be eligible for the SLR program after the policy change a loan was required to be at least 6 months old and to have a satisfactory payment history. The payment history requirement

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[1]Specifically, we drop any loan for which the `mba_delinquency_status` variable in the LLMA data ever takes the values: S (Servicing Sold/Released), T (Loan Status No Longer Provided/Available), X (Unknown/missing LPI Date), or Z (Incompatible).

[2]The data includes both a static variable indicating the date of the payoff (for loans that do pay off) as well as a dynamically updated variable that indicates whether a loan is paid off in a particular month. We drop any loans for which these two methods for determining the payoff status ever disagree.

depended on the age of the loan. For loans older than 12 months, all payments within the last 3 months must have been on time and no more than one payment in the last year may have been 30 days late. For loans between 6 and 12 months old, all payments since origination must have been on time. We drop any loan-month observations that do not meet these requirements. The fifth row of Table A.1 shows that this filter eliminates 237,550 loans, which is roughly 13 percent of the initial sample and is consistent with aggregate mortgage delinquency rates during our sample period.[3]

Finally, there are several important variables that we require to be non-missing for all loans in our final analysis sample. These include an estimate of current home equity, county-level unemployment rate changes, and an estimate of the potential interest rate available to each borrower if she were to refinance in a given month. To calculate current equity in each month, we need an estimate of house price appreciation between origination and the current month. Thus, we drop any loans in our data for which the county-level Zillow house price indices are unavailable either in the month of origination or during every month in our sample period. Similarly, because the remaining loan balance reported in the data set is an end of month number, we also drop any observations for which the lag of the remaining balance is missing since such observations would have a missing value for estimated current equity as well. Together these restrictions eliminate 148,579 loans. An additional 62,162 loans are dropped because they cannot be matched to county-level unemployment estimates from the ACS. Finally, we drop 160,412 loan-months for which we cannot calculate a potential interest rate due to a lack of observed refinances on similar loans in the candidate month. This leaves us with a final analysis sample containing 1,309,393 loans and 15,645,645 loan-month observations. Descriptive statistics for this sample are reported in Table II and discussed in Section III of the paper.

A.2 Measurement Error in County-Level Unemployment

B ADDITIONAL RESULTS AND ROBUSTNESS CHECKS

B.1 Quadratic Time Trends in the Difference-in-Differences Regressions

One potential concern with our main difference-in-differences analysis is that changes in economic conditions prior to the policy change may have differentially affected the refinancing rate of FHA borrowers relative to conventional borrowers since FHA borrowers had easier access to refinancing through the SLR program during that period. This would violate the

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[3]Table 1193 in the 2011 Statistical Abstract of the United States reports that 13.9 percent of all mortgages by dollar volume were delinquent or in foreclosure in 2009.

parallel trends assumption and introduce bias in our estimate of the effect of the policy change. As we discuss in Section IV.A, our main approach to dealing with this concern is to control for a set of linear time trends for FHA borrowers that are allowed to vary freely before and after the policy change. Here we show that our main results are also robust to more flexible quadratic trends, which may do a better job of capturing differential trends in the FHA market during the period surrounding the policy change.

In Figure A.1 we begin by replicating the results presented in Figure II but allowing for quadratic trends in time. As the figure makes clear, including these trends has no effect on the qualitative conclusions we draw from Figure II. Panel A. shows that there is still a large and discontinuous drop in refinancing in the FHA segment of the market that occurs in precisely the month of the policy change. The size of the drop in FHA refinancing implied by the quadratic trends is slightly smaller than in the linear case but is nonetheless still visually apparent and economically similar in magnitude. Panel B. plots the same trends in the conventional market and, as before, shows little evidence of a meaningful drop in refinancing in that segment. Relative to the linear trends, the quadratic trends imply a slightly larger drop in conventional refinancing; however, in both cases the size of this drop pales in comparison to the drop in the FHA market and is of similar magnitude to many other month-over-month changes in the raw averages shown by the blue dots.

Consistent with this evidence, Table A.2 shows that our main difference-in-differences estimates are also robust to including more flexible time trends. This table replicates Table IV from the main text, with the only difference being that here we include quadratic rather than linear time trends for FHA borrowers that are allowed to vary freely before and after the policy change. In all specifications, we continue to find a statistically significant and economically large fall in FHA refinancing after the policy change. The size of this drop is roughly 50 basis points, which is slightly smaller than the 70 basis point fall we find in Table IV but still constitutes a 40 percent fall relative to average FHA refinancing rate in the month just prior to the policy change. Taken together, we view this as strong evidence that our main results are not affected by the specific manner in we attempt to control for potential differential trends.

B.2 Alternate Specifications for Flexible Difference-in-Differences

Figure III in the main text plots coefficient estimates from a version of equation (4) that is analogous to our most conservative difference-in-differences estimates from column 4 of Table IV. For robustness, in Figure A.2 we plot similar coefficient estimates from less conservative specifications that mirror those from columns 1–3 of Table IV. For completeness, Table A.3 also reports these coefficient estimates numerically.

Each set of coefficients reported in the figure comes from a different version of equation (4) that includes an increasingly conservative set of controls. In "Model 1," we control only for the CBSA of the property. "Model 2" adds a further set of fixed effects for the current loan age (one-year bins), interest rate (one-percentage point bins) and the borrower's estimated home-equity ($10,000 bins), as well as the full pairwise interaction between the borrower's LTV (10-point bins) and FICO score (50-point bins). In "Model 3" we further interact all but the CBSA fixed effects with the dummies for the month of observation. This allows each control variable to have a fully non-parametric effect on refinancing over time. Finally, in "Model 4" we repeat our preferred estimates from Figure III for reference. This specification further interacts all of the control variables with the FHA dummy and produces results that are nearly identical to those from "Model 3." While the pre-trends differ somewhat across specifications, these trends are largely eliminated when we allow for our control variables to differentially affect refinancing rates over time. Moreover, across all four specifications, we find that the fall in FHA refinancing after the policy change is persistent and of roughly the same magnitude.

B.3 Pooled Full-Sample Triple-Difference Regressions

The triple-difference regressions we present in Section V are estimated in restricted samples. For example, when studying the role of the income documentation requirement, we restrict the sample to include only loan-months for which the borrower is in positive equity. Similarly, when estimating the effect of the upfront costs, we include only properties in counties that experienced below median increases in unemployment. The reason we restrict our samples in this way is because the two "treatments" are highly correlated. Counties that experienced large increases in unemployment also experienced on average significant declines in house prices during this period. By restricting the sample to include borrowers who are only likely to face one of the two constraints, we are able to plausibly isolate their separate effects. To be even more conservative, in our most stringent specifications we also control directly for any remaining effect of negative equity (in the income documentation analysis) or county-level unemployment (in the upfront costs analysis) on FHA refinancing after the policy change.

An alternative approach to isolating the separate effects of each constraint would be to estimate a pooled regression in the full sample that allows for both county-level unemployment changes and negative equity to separately effect FHA refinancing. This regression would be similar to our most conservative specifications (column 5 in Table V and Table VI) but be estimated in a non-restricted sample. In Table A.4 we present results from various versions of this type of regression. In columns 1–3 we estimate specifications that are directly analogous to those in Table V and Table VI. Broadly speaking, the estimates for the

triple interaction coefficients in rows 3 and 4 are qualitatively similar to our main estimates. For example, the results in column 3 indicate that the differential fall in FHA refinancing after the policy change was 46 basis points larger among FHA borrowers in negative equity and grows by roughly 3.1 basis points with each additional one percentage point increase in the county-level unemployment rate. These estimates are slightly smaller but economically similar to the 51 and 4.7 basis point estimates from the most directly analogous regressions in column 5 of Table V and Table VI.

The largest difference between the estimates in full sample and those from the restricted samples in the main text are for the coefficient estimates on the triple interaction with unemployment changes in columns 2 and 3 of Table A.4. These effects are roughly half the size of their counterparts from Table V and are no longer statistically significant at conventional levels. However, this attenuation is exactly what would be expected given the fact that county-level unemployment is both highly correlated with home equity in the full sample and a very weak proxy for individual employment status. As a result, much of the "signal" from the county-level unemployment change variable is likely being picked up by the borrower-level measure of negative equity, leading to an attenuation of the coefficient on the triple interaction with unemployment. Nonetheless, at 2.3 basis points, these coefficients are still economically quite large and indicate that employment documentation requirements were likely a significant barrier to FHA refinancing after the policy change.

In columns 4–6, we present a slightly different version of the specification. Instead of entering the county-level unemployment change linearly, in these specifications we simply include a dummy for whether the borrower lives in a county experiencing an above-median increase in unemployment. This coarser measure of unemployment helps to address some of the issues with measurement error and correlation between unemployment and negative equity mentioned above. The results in the bottom row are statistically significant at the one percent level in all three specifications and indicate that living in a high unemployment county is associated with a roughly 20 basis point larger fall in FHA refinancing subsequent to the policy change. As a way of comparing these estimates to those from columns 1–3 and our main estimates in Table V we can divide this coefficient by the difference between the average increase in unemployment in low and high unemployment counties, which is 3.025 percentage points. Doing this suggests that the FHA refinancing rate falls by roughly $0.22/3.025 = 0.07$ percentage points more for each one percentage point increase in the county-level unemployment rate. This estimate is slightly larger but economically similar in magnitude to our preferred estimate of 4.7 basis points from Table V. Together, we think these results provide strong evidence that our main estimates are robust to alternative methods for isolating the independent effect of each of the two policy changes and indicate

that both played an important role in contributing to the overall fall in FHA refinancing after the policy change.
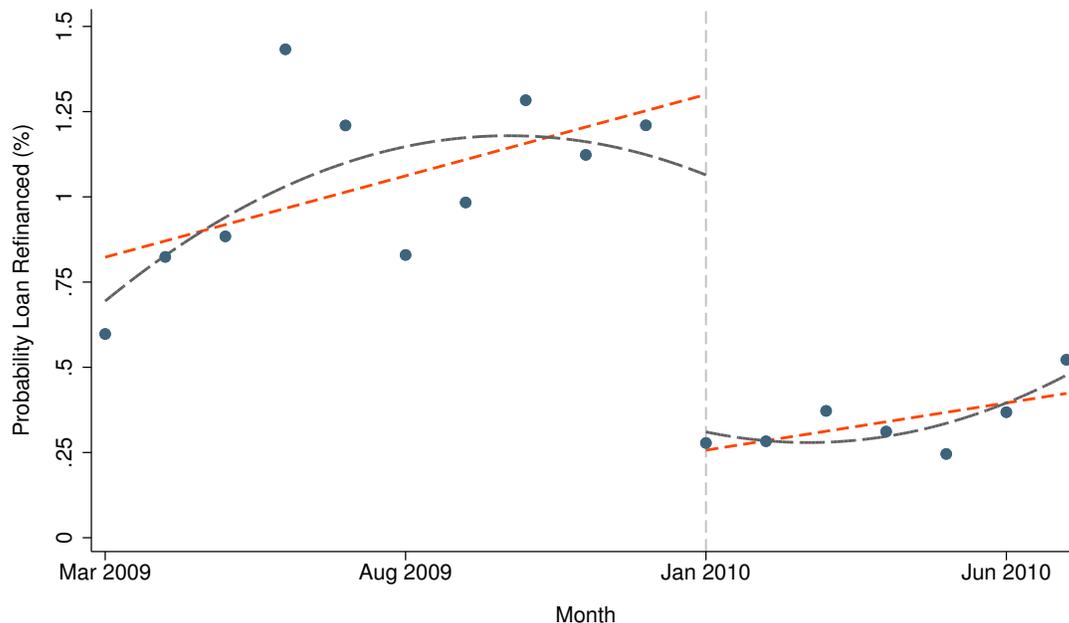
A. FHA Loans
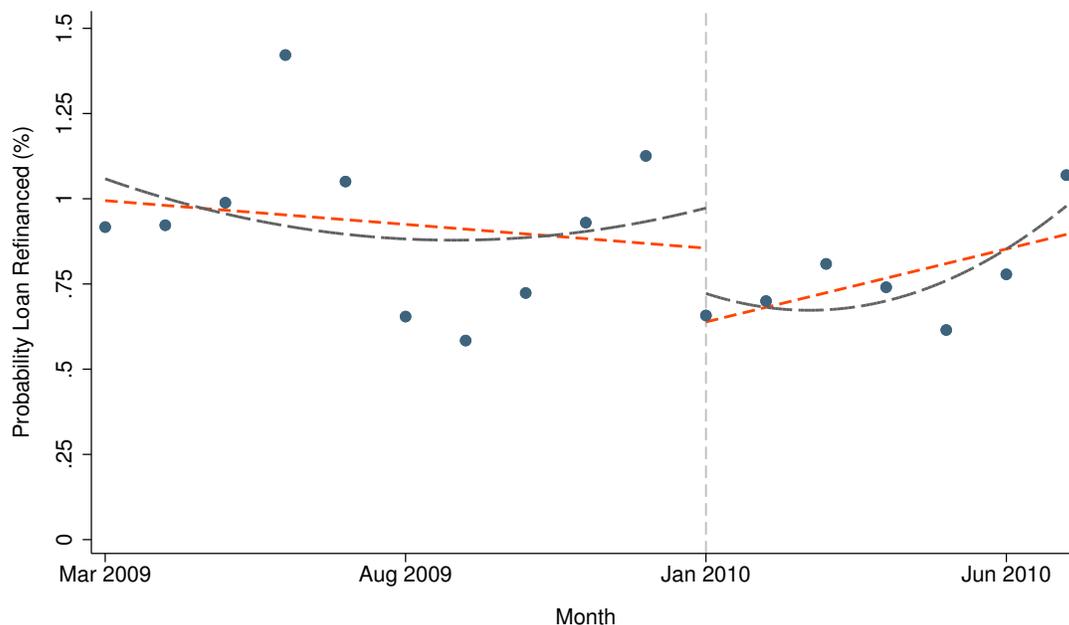
B. Conventional Loans

FIGURE A.1

FHA and Conventional Refinancing Trends, Allowing for a Quadratic

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. The dark grey dashed lines are the predicted values from a similar regression that allows for a quadratic trend on either side of the cutoff.
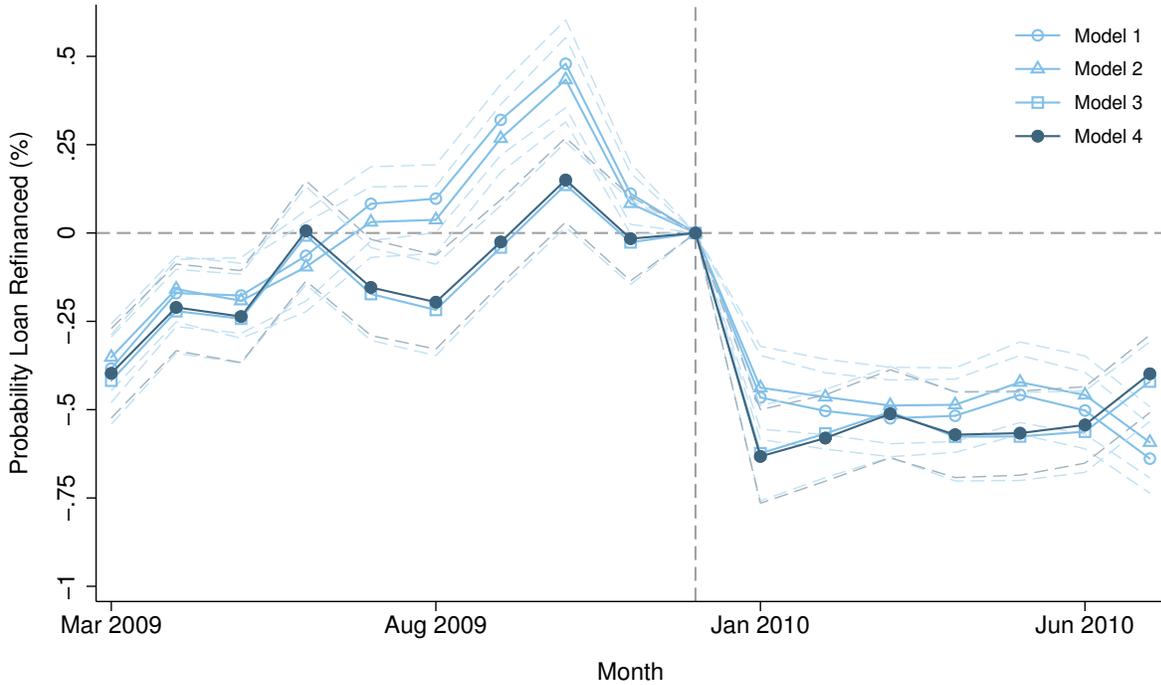
FIGURE A.2

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. In "Model 1" the regression also included fixed effects for the CBSA of the property. The regression in "Model 2" adds a further set of fixed effects for the current loan age (one-year bins), interest rate (one-percentage point bins) and the borrower's estimated home-equity ($10,000 bins), 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 are then separately interacted with the dummies for the month of observation in "Model 3" and the FHA dummy in "Model 4." The 95% confidence intervals are based on standard errors that are clustered at the CBSA level.

TABLE A.1

NUMBER OF OBSERVATIONS DROPPED DURING SAMPLE SELECTION

	Loans	Loan-Months
Original Count	1,845,744	25,876,460
Dropped Observations:		
Unknown Payoff Status	48,924	705,074
Payment Date after Payoff Date	34,077	663,771
Inconsistent Payoff Information	3,137	33,921
Payment History Ineligible for SLR	237,550	6,000,497
Missing Zillow Price Index	115,760	1,431,789
Missing Current Equity	32,819	506,016
Missing County Unemployment	62,162	729,335
Missing Potential Interest Rate	1,922	160,412
Final Count	1,309,393	15,645,645

NOTE.— This table reports the number of observations dropped during each stage of our sample selection procedure. Column 1 reports observation counts at the loan level, whereas column 2 is at the loan-month level.

TABLE A.2

DIFFERENCE-IN-DIFFERENCES ESTIMATES ALLOWING FOR QUADRATIC FHA TRENDS

	(1)	(2)	(3)	(4)
FHA	0.161***	0.551***	0.555***	1.796***
	(0.058)	(0.060)	(0.055)	(0.319)
FHA × Post	−0.490***	−0.485***	−0.484***	−0.490***
	(0.060)	(0.060)	(0.051)	(0.051)
Month FEs	X	X	X	X
CBSA FEs	X	X	X	X
Quadratic 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	15,645,645	15,645,645	15,645,645	15,645,645

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 quadratic 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 loan age (one-year bins), interest rate (one-percentage point bins) and the borrower's estimated home-equity ($10,000 bins), 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 A.3

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

	(1)	(2)	(3)	(4)
Mar. 2009	−0.384***	−0.352***	−0.418***	−0.397***
	(0.049)	(0.048)	(0.063)	(0.065)
Apr. 2009	−0.170***	−0.159***	−0.221***	−0.211***
	(0.048)	(0.047)	(0.060)	(0.063)
May. 2009	−0.177***	−0.192***	−0.242***	−0.236***
	(0.055)	(0.054)	(0.064)	(0.066)
Jun. 2009	−0.064	−0.096	−0.007	0.006
	(0.066)	(0.065)	(0.071)	(0.072)
Jul. 2009	0.083	0.031	−0.173**	−0.154**
	(0.053)	(0.051)	(0.067)	(0.069)
Aug. 2009	0.097**	0.037	−0.218***	−0.196***
	(0.049)	(0.049)	(0.066)	(0.068)
Sep. 2009	0.320***	0.268***	−0.041	−0.025
	(0.051)	(0.049)	(0.060)	(0.061)
Oct. 2009	0.479***	0.434***	0.137**	0.150**
	(0.063)	(0.061)	(0.061)	(0.061)
Nov. 2009	0.111**	0.084*	−0.026	−0.016
	(0.044)	(0.044)	(0.061)	(0.061)
Jan. 2010	−0.466***	−0.438***	−0.624***	−0.632***
	(0.061)	(0.060)	(0.069)	(0.068)
Feb. 2010	−0.504***	−0.464***	−0.568***	−0.580***
	(0.055)	(0.054)	(0.064)	(0.062)
Mar. 2010	−0.525***	−0.488***	−0.506***	−0.512***
	(0.055)	(0.055)	(0.066)	(0.064)
Apr. 2010	−0.517***	−0.486***	−0.576***	−0.571***
	(0.053)	(0.053)	(0.064)	(0.062)
May. 2010	−0.458***	−0.422***	−0.576***	−0.566***
	(0.057)	(0.058)	(0.064)	(0.061)
Jun. 2010	−0.502***	−0.459***	−0.562***	−0.543***
	(0.055)	(0.056)	(0.059)	(0.055)
Jul. 2010	−0.639***	−0.592***	−0.420***	−0.398***
	(0.050)	(0.051)	(0.058)	(0.056)
Month FEs	X	X	X	X
CBSA FEs	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 × Month FEs			X	X
Controls × FHA				X
Number of Observations	15,645,645	15,645,645	15,645,645	15,645,645

NOTE.—This table reports flexible 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 interaction between the FHA "treatment" dummy and a dummy for 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. All specifications include fixed effects for the month of observation and the CBSA of the property. Column 2 adds fixed effects for the current loan age (one-year bins), interest rate (one-percentage point bins) and the borrower's estimated home-equity ($10,000 bins), 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 added in column 2 with the dummies for the month of observation. 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 A.4
POOLED FULL-SAMPLE TRIPLE-DIFFERENCE ESTIMATES

	(1)	(2)	(3)	(4)	(5)	(6)
FHA	0.078	0.504***	0.689***	0.078	0.463***	0.640**
	(0.080)	(0.095)	(0.254)	(0.052)	(0.064)	(0.270)
FHA × Post	−0.481***	−0.397***	−0.380***	−0.462***	−0.385***	−0.375***
	(0.096)	(0.088)	(0.081)	(0.071)	(0.066)	(0.060)
FHA × Low Equity × Post	−0.679***	−0.689***	−0.462***	−0.657***	−0.674***	−0.458***
	(0.065)	(0.068)	(0.061)	(0.064)	(0.067)	(0.061)
FHA × ΔUR × Post	−0.023	−0.023	−0.031*			
	(0.019)	(0.018)	(0.018)			
FHA × High ΔUR × Post				−0.220***	−0.200***	−0.220***
				(0.075)	(0.069)	(0.071)
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
Interest Rate FEs		X	X		X	X
LTV × FICO FEs		X	X		X	X
Equity FEs		X	X		X	X
ΔUR FEs		X	X		X	X
Controls × Post			X			X
Controls × FHA			X			X
Number of Observations	15,645,645	15,645,645	15,645,645	15,645,645	15,645,645	15,645,645

NOTE.—This table reports estimates of the effect of the changes in SLR income documentation requirements and upfront costs on the monthly probability that an FHA loan refinances. The outcome variable is an indicator for whether or not a loan refinances in the month of observation and is multiplied by 100, so that all coefficients can be interpreted as percentage point changes. The Post dummy takes the value one if the month of observation is after the implementation of the policy changes (January 2010). In columns 1–3 the county-level unemployment change is entered linearly. In columns 4–6 it is entered as a dummy indicating whether the borrower lives in a county that experienced an above median increase in unemployment. 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. Columns 2 and 4 add fixed effects for the current loan age (one-year bins), original interest rate (one-percentage point bins), the borrower's estimated home-equity ($10,000 bins), and county-level unemployment change decile as well as the full pairwise interaction between the borrower's LTV (10-point bins) and FICO score (50-point bins) at origination. Columns 3 and 6 further interact the loan age, interest rate, LTV, and FICO fixed effects with the Post dummy and the FHA indicator. Standard errors are reported in parentheses and are clustered at the CBSA level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.