



Federal Reserve Bank of Chicago

**Policy Intervention in Debt
Renegotiation: Evidence from
the Home Affordable
Modification Program**

*Sumit Agarwal, Gene Amromin, Itzhak Ben-David,
Souphala Chomsisengphet, Tomasz Piskorski, and
Amit Seru*

November 2013

WP 2013-27

Policy Intervention in Debt Renegotiation: Evidence from the Home Affordable Modification Program

Sumit Agarwal^a
Souphala Chomsisengphet^c

Gene Amromin^a
Tomasz Piskorski^d

Itzhak Ben-David^b
Amit Seru^e

June 2013

ABSTRACT

The main rationale for policy intervention in debt renegotiation is to enhance such activity when foreclosures are perceived to be inefficiently high. We examine the ability of the government to influence debt renegotiation by empirically evaluating the effects of the 2009 Home Affordable Modification Program (HAMP) that provided intermediaries (servicers) with sizeable financial incentives to renegotiate mortgages. A difference-in-difference strategy that exploits variation in program eligibility criteria reveals that the program generated an overall increase in the intensity of renegotiations while adversely affecting the effectiveness of renegotiations performed outside the program. Renegotiations induced by the program resulted in a modest reduction in the rate of foreclosures and reached just one-third of its targeted 3 to 4 million indebted households. This shortfall is in large part due to low renegotiation intensity of a few large servicers that responded at half the rate than others. The muted response of these servicers—which is also observed before the program—does not reflect differences in contract, borrower, or regional characteristics of mortgages across servicers. Instead, it reflects servicer-specific factors that appear to be related to their preexisting organizational capabilities. We exploit regional variation in the share of loans serviced by intermediaries with high pre-program renegotiation activity to assess the economic effects in areas more exposed to the program. Regions where HAMP was used intensively saw a lower rate of house price decline as well as an increase in the pay-down rate on consumer debt. There was no change in non-durable and durable consumption in these regions, suggesting that distressed borrowers who are in the process of debt deleveraging may have a relatively low spending multiplier from moderate debt reduction. We conclude by discussing implications of our findings for debt relief programs in general and for other policy responses to crises that also require intermediaries for implementation.

JEL: E60, E65, G18, G21, H3

Keywords: Government intervention, Debt renegotiation, Mortgage modification, Foreclosures, Housing crisis, HAMP, Servicers

The views presented in the paper do not necessarily reflect those of the FRB of Chicago, the Federal Reserve System, the Office of the Comptroller of the Currency, or the U.S. Department of the Treasury. We thank Raphael Bostic, John Campbell, John Cochrane, Dennis Glennon, Andrew Haughwout, Bruce Kruger, Chris Mayer, Atif Mian, Uday Rajan, Kristopher Rengert, Rik Sen, Amir Sufi, Francesco Trebbi, Joe Tracy, Kostas Tzioumis, Wilbert van der Klaauw and Luigi Zingales. We also thank the seminar participants at Berkeley, Chicago Booth, Chicago Fed, Cleveland Fed, Columbia, Kellogg, NYU Stern, Penn State and Office of the Comptroller of the Currency, as well as participants at the AEA, NBER Summer Institute, the NYC Real Estate Conference, and the UCLA Real Estate Conference for helpful suggestions. We are indebted to Regina Villasmil and James Witkin for outstanding research assistance. Piskorski acknowledges the funding from the Paul Milstein Center for Real Estate at Columbia Business School and the NSF. Seru acknowledges the funding from the Initiative on Global Markets at Booth School of Business at the University of Chicago.

a: National University of Singapore; b: Fisher College of Business, The Ohio State University and NBER; c: Office of the Comptroller of the Currency; d: Graduate School of Business, Columbia University; and e: Booth School of Business, University of Chicago and NBER (contact author).

I. Introduction

At least since the Great Depression, federal and state governments have regularly intervened in the functioning of mortgage markets—through household debt relief and foreclosure prevention policies—during times of exceptionally harsh economic circumstances (e.g., Rucker and Alston 1987). There has been a long-standing debate among economists on the effects of such interventions. On the one hand, proponents argue that such policies prevent excessive foreclosures that may not only lead to deadweight losses for borrowers and lenders, especially if debt contracts are incomplete (Bolton and Rosenthal 2002), but also generate negative externalities for the society (Campbell et al. 2010; Mian, Sufi, and Trebbi 2011; Guiso, Sapienza and Zingales 2011). Moreover, these policies also help reduce high levels of debt that may distort household consumption and investment decisions (Mian and Sufi 2012). On the other hand, critics argue that such policies potentially generate moral hazard problems that are likely to raise the cost of credit in the long run, and may also have undesirable redistributive consequences (Becker 2009; Posner 2009). Remarkably, despite the economic importance of and controversy surrounding such interventions, empirical evidence on the consequences of such policy programs is scant.¹ This paper attempts to fill this gap by empirically evaluating the effects of the largest government intervention concerning mortgage debt renegotiation in the aftermath of the recent crisis.

We exploit unique micro data concerning the policy program that provided intermediaries (servicers) who handle distressed loans with sizeable financial incentives to renegotiate residential mortgages. Employing a difference-in-difference strategy, we estimate that the impact of this program will fall significantly short of its target. We show that low renegotiation activity of a few large servicers, which is also observed before the program and seems to be related to their preexisting organizational capabilities, explains a large part of this shortfall. We use regional variation in the share of loans serviced by intermediaries with high pre-program renegotiation activity to assess economic effects in areas more exposed to the program. Regions where the program was used most intensively saw a lower rate of house price decline and an increase in the pay-down rate on consumer debt while experiencing no change in non-durable and durable consumption. These findings have implications for debt relief programs in general and for other policy responses to crisis that require private intermediaries for implementation.

We study the Home Affordable Modification Program (HAMP), a large-scale government effort that was unveiled in early 2009 in response to the foreclosure crisis. The program provided substantial financial incentives² to servicers, relative to their regular compensation, in an attempt

¹ This is in contrast to large literature that examines the role of fiscal stimulus in stimulating economic activity (e.g., Johnson, Parker, and Souleles 2006; Mian and Sufi 2010; Christiano et al. 2009; Auerbach and Gorodnichenko 2011; Parker 2011; Parker et al. 2011; Ramey 2011; and Nakamura and Steinsson 2012).

² HAMP committed to one-time incentive payments to servicers of \$1,000 for each completed renegotiation under the program. Servicers were also eligible for up to \$1,000 in annual, ongoing pay-for-success incentive payments that would accrue if mortgage payments were made on time for three years after the renegotiation. These incentive

to alleviate several perceived barriers to renegotiation—such as the inability of the private market to internalize negative externalities imposed by foreclosures (e.g., Campbell et al., 2010) and the frictions induced by non-agency securitization (e.g., Piskorski et al., 2010).

Our paper has two objectives. First, we undertake a detailed evaluation exercise to assess the impact of the program by examining how HAMP affected various margins related to renegotiation decision by servicers, studying both the renegotiations done under the program as well as those outside it. Moreover, we exploit the variation induced by program exposure—which potentially facilitated contract renegotiation in some regions, while leaving contracts relatively unaffected in others—to examine the impact of HAMP on broader outcomes such as house prices, consumption, and delinquency rates on non-mortgage debt in areas more exposed to the program. This helps us to generate systematic evidence on the effects of this intervention, with implications for debt relief programs in general, making ours the first paper to go beyond the typical anecdote-based discussions of such programs.³ Second, we document and exploit the significant heterogeneity in program response across intermediaries. This allows us to understand their role in implementing the program and generate implications for other policy responses to crises that also require intermediaries for implementation.

We use the unique MortgageMetrics data set from the Office of the Comptroller of the Currency (OCC). This data set contains precise information on performance and renegotiation outcomes for more than 60% of outstanding residential mortgages in the United States, and it is a loan-level panel that has detailed information on loan, property, and borrower characteristics (e.g., interest rates, location of the property, credit scores), payment history (e.g., delinquent or not), renegotiation actions taken (e.g., principal reduction), whether the renegotiation was undertaken under HAMP, as well as the servicer responsible for the mortgage. The richness of this data set provides us a unique opportunity to assess the effects of the program.

The biggest obstacle, however, in evaluating the impact of the program on mortgage renegotiation rates is getting an estimate of the counterfactual level of renegotiation rates for these mortgages in the absence of the program. We circumvent this issue by using an empirical design that exploits variation in exposure of similar borrowers to the program. We follow two strategies to classify borrowers into treatment and control groups. The main empirical strategy exploits variation in owner-occupancy status and uses the notion that borrowers whose properties are classified as investor-owned during program implementation are ineligible for HAMP. Therefore, we use such borrowers as a control group for the eligible group of borrowers whose property is classified as owner-occupied (treatment group). The second strategy, employed for

payments are sizeable relative to the regular annual fees for servicing, which amount to about twenty to fifty basis points of the outstanding loan balance (~\$400 to \$1,000 per year for a \$200,000 outstanding loan balance mortgage). See Section II.C for more discussion.

³Anecdote-based discussions on HAMP are aplenty. For instance, in July 2010, Neil Barofsky, special inspector general for the TARP, argued that HAMP had been perceived to be an outright failure. However, Christina Romer, former chair, CEA, argued around the same time that though a bit slow, the program was making steady progress.

robustness, uses the notion that among borrowers with properties that are owner-occupied during program implementation, mortgages with outstanding balances above \$729,750 are ineligible for HAMP. We use such borrowers to construct the control group for the eligible group of borrowers with loan balances just below \$729,750 (treatment group).⁴

We start our analysis by showing that, on average, control and treatment groups in both empirical strategies are similar on most observables before the program. In addition, the treatment and control groups of loans have no differential pre-trends. This holds for various observables such as credit score, loan-to-value ratio, interest rates, delinquency rates as well as rate of renegotiations offered in the two groups before the program. As a validation of our empirical design, we also verify that our classification of loans into treatment group based on the program guidelines corresponds to the loans where we observe modifications performed under HAMP. Notably, the second empirical strategy exploits program eligibility criteria based on loan amount *within the group of loans for owner-occupied properties*, thereby addressing the potential criticism of the first empirical strategy that the treatment and control loans might differ on unobservables because they differ on owner-occupancy status.

Next, we focus on analyzing the extensive margin—that is, additional loan renegotiations (contract modifications) induced by the program. Doing so requires taking into account the potential of the program to crowd out modifications performed by the servicers outside of the program (i.e., “private modifications”). We find that there were non-negligible HAMP modifications offered in the eligible group of loans, but no evidence of decline in the rate of private modifications in the eligible group relative to the control group. The potential crowding out of some private modification activity by the program in the treatment group appears to be compensated by an overall increase in applicants due to outreach initiatives of the program, with some of borrowers who did not qualify for permanent HAMP modification receiving a private modification instead.

We next analyze the intensive margin in the treatment group—that is, in the composition of types of renegotiations and their effectiveness, as measured by default rate subsequent to the modification. We provide evidence suggesting that servicers did channel some loans that they would have modified based on their private incentives to be modified under HAMP instead. In particular, private permanent modifications offered in the treatment group after the program is introduced become less aggressive (e.g., fewer rate reduction and interest capitalizations) and suffer a drop in their effectiveness. These patterns are observed concurrently with an increase in aggressiveness and effectiveness of modifications done under the program. The drop in

⁴ Our data consists of loans serviced by large institutions and, in general, includes loans of better credit quality than typical investor loans used to finance speculative investments in the non-agency market. As a result, in our sample, the treatment and control groups formed based on owner-occupancy status are very comparable. The second strategy is even better on this front since both groups of loans consist of similar sized owner-occupied properties.

effectiveness of private modifications is offset by higher effectiveness of HAMP modifications, resulting in no change in the average effectiveness of modifications in the treatment group. Overall, when considering all the renegotiations—regardless of whether they were done privately or under HAMP—we find that the program led to an increase in the annual rate of permanent modifications of about 0.7%.⁵ At this rate, the program would induce about 1.2 million additional permanent modifications over its duration (i.e., through December 2012)—falling significantly short of its goal of three to four million modifications for the severely indebted households targeted by the intervention.

We address various alternatives that might bias our findings. In particular, we investigate if our treatment effects are inflated because servicers may use up some of their resources for conducting HAMP modifications in the treatment group at the expense of modifications in the control group, given the program incentives. We investigate, among others, trends in the control group for servicers in our sample around the time of program implementation. We further compare these trends with those exhibited by loans that would have been included in the control group *had* their servicers chosen to participate in HAMP. The analysis yields a consistent picture: servicers modified more loans in the treatment group—with the more promising candidates for modifications channeled under HAMP to take advantage of program incentive payments—leaving modifications in the control group relatively unchanged. We further discuss servicing technology that may lead to such effects.

We then turn to examining the impact of HAMP on the outcome it was designed to ultimately affect—that is, the rate at which loans are foreclosed. We find that HAMP resulted in a moderate decrease in the rate of completed foreclosures in the treatment group, reflecting the change in extensive margin induced by the program. In particular, we observe a differential 0.48% decrease in the annual foreclosure rate across the loans in the treatment group. This rate would translate into about 800,000 fewer foreclosures in the treatment group over the original duration of the program (i.e., through December 2012)—substantially lower than the program target. In addition, limited coverage of the post-program period in our data makes it difficult to conclude how many of these foreclosures would be permanently prevented.

In sum, the first part of our paper establishes that servicers responded to the program by conducting more modifications among eligible loans, although the increase fell significantly short of the target of this intervention. Moreover, there was an adverse impact on the effectiveness of renegotiations performed outside the program. While it is difficult to know what the optimal response to the program incentives should have been, in the second part of the paper

⁵ The program also induced several trial modifications—renegotiations that had to be necessarily offered under the program for a trial period before permanent ones could be offered. The rate of trial HAMP modifications is higher than permanent ones, and only 38% of trial modifications were converted into permanent ones. This conversion rate reflects several criteria that had to be satisfied before a trial modification could be made permanent.

we exploit response across intermediaries to shed light on potential barriers to program implementation as well as on broader economic effects in areas most exposed to the program.

We find substantial heterogeneity across servicers in terms of their response to HAMP, with a few large servicers offering modifications at half the rate of others. A simple counterfactual computation shows that this is a large effect—the program would have induced about 70% more permanent modifications if all the loans by less active servicers were renegotiated at the same rate as those of their more active counterparts. Further investigation shows that the renegotiation activity of servicers during the program closely tracks their pre-program renegotiation behavior. While contract, borrower, and regional characteristics of mortgages are important determinants of renegotiation activity of a servicer, these differential patterns of renegotiation across servicers cannot be accounted for by these factors. Instead, servicer-specific factors—which seem to be related to their preexisting organizational capabilities—are responsible for differences in pre-program renegotiation activity across servicers. Servicers with lower (higher) renegotiation activity had pre-program organizational design that was less (more) conducive to conducting renegotiations on dimensions such as size and workload of the servicing staff, staff training effort, and servicing call-center capability.⁶

Finally, we explore the impact of the program on regional outcome variables such as house prices, consumption, and delinquency rates on other categories of consumer debt. The broad goal of this exercise is to help understand the effect of debt relief programs such as HAMP, when implemented intensively, on economic outcomes. To do so, we exploit regional variation in the share of loans serviced by intermediaries with high pre-program renegotiation activity to assess the economic effects in areas more exposed to the program. Because servicer concentration in a region is determined prior to the program and is very persistent over time in the data, using this variation to examine effects of HAMP in areas most exposed to the program seems reasonable. Consistent with our earlier evidence, regions with high concentration of loans serviced by active servicers in the pre-program period were more likely to experience a significant amount of program modifications. Importantly, these regions saw a lower rate of house price decline as well as an increase in the pay-down rate on consumer debt, relative to similar regions with low program exposure. There was no concurrent relative change in non-durable consumption (such as groceries) or durable consumption (auto sales) in regions with higher exposure to the program.

Our findings suggest that debt relief programs, when used with sufficient intensity, may have a meaningful impact on foreclosure rates and house prices, similar to inferences made in Campbell et al. (2010) and Mian, Sufi, and Trebbi (2011). Moreover, our results also suggest that such

⁶ The fact that some servicers—with similar loans as servicers with low program response rate—actively conducted modifications under the program suggests that the incentive structure of HAMP may not have been inadequate per se. Rather, the policy may have failed to account for firm-level factors that resulted in muted program response of some servicers. Our analysis does not allow us to comment on the exact nature of these firm-level factors or how they led to inertia in the behavior of these servicers. For instance, servicers with low renegotiation activity in the pre-program period may not have responded to the program because doing so would have involved changing their business focus from processing and channeling payments to actively renegotiating loans. In addition, this may have involved significantly altering their organizational capabilities, such as building infrastructure and appropriate staff.

programs targeted at distressed borrowers may not necessarily result in a sizeable increase in non-durable or durable consumption, at least in the near term. Thus, the results in our paper clarify that distressed borrowers who are in the process of debt deleveraging may initially have a relatively low spending multiplier from moderate mortgage debt reduction, and use additional resources to pay down their other debt instead. These findings are consistent with arguments and empirical evidence in Mian, Sufi, and Rao (2011) and corroborates their view that large accumulation of household debt prior to the crisis is an important factor adversely affecting household consumption.

Our paper is related to the small body of empirical literature that evaluates the impact of government intervention in distressed debt markets. This literature, among others, examines the federal and state government interventions during the Great Depression through debt moratoria of farm mortgages (Alston 1983, 1984; Rucker and Alston 1987) and impact on debtor value generated by the devaluation of debt contracts (Kroszner 1998).

Our work also relates to the literature on the housing crisis (e.g., Mian and Sufi 2009; Mayer et al. 2009; Gyourko et al., 2009, Keys et al. 2010, 2011; Rajan et al. 2010; and Demyanyk and Van Hemert 2011, Gyourko and Fernando 2011). In this area, our findings on the impact of government intervention in mortgage renegotiation are closely related to the work that examines loan renegotiation in mortgage markets (see Agarwal et al. 2011; Piskorski et al. 2010) and work that studies the effects of mortgage modification programs on household behavior (e.g., Mayer et al. 2011). It is also related to the studies evaluating the impact of foreclosures, falling house prices, and high levels of debt on economic outcomes (e.g., Mian and Sufi 2010; Melzer 2010; Mian et al. 2011; Campbell et al. 2011; and Mian and Sufi 2012).⁷

Finally, our findings investigating the possibility of crowding-out of private activity by government intervention in the context of mortgage renegotiation broadly relate to the literature on government spending and Ricardian equivalence (e.g., Barro 1989; Johnson, Parker, and Souleles 2006; Agarwal et al. 2007; Mian and Sufi 2010; Auerbach and Gorodnichenko 2011; Parker 2011; Parker et al. 2011; Ramey 2011; and Nakamura and Steinsson 2012).

II. HAMP: Background, Eligibility, Incentive Plan, and Overall Budget

II.A Background

The housing crisis unfolded around 2007, with the number of foreclosures reaching unprecedented levels. More than 700,000 foreclosures were started in 2007, with another two million in 2008 and even more in subsequent years (see Credit Suisse Foreclosure Update 2010). Foreclosures are considered costly—either because they result in significant deadweight losses for borrowers and lenders or because they result in negative externalities for the society (see

⁷ See also recent models by Piazzesi and Schneider (2009), Favilukis et al. (2010), Burnside et al. (2011), Philippon and Midrigan (2011), and Landvoigt et al. (2012) on origins and consequences of housing boom and busts.

Posner and Zingales 2009 and Campbell et al. 2011). Thus, federal and state government efforts were aimed at encouraging mortgage renegotiations through loan modifications instead of foreclosing loans.

There were several reasons why the rate of mortgage modifications was perceived to be low. First, since foreclosures may exert significant negative externalities, it could be socially optimal to modify mortgage contracts to a greater extent than servicers were choosing to do privately.⁸ Second, policy makers noted that the non-agency securitized market—that is, securitized mortgages issued without a guarantee from government-sponsored entities (GSEs)—accounted for more than half of the foreclosure starts, despite their relatively small market share. The worry was that high foreclosure rates on these securitized mortgages reflected factors other than their greater inherent credit risk. In particular, a servicer—an intermediary who makes the crucial decision to pursue a foreclosure or renegotiate a delinquent mortgage—is an agent who acts on behalf of the investor in case of a securitized loan. Thus, servicers' contractual incentives and legal uncertainty on the course of action allowed by investors could have inhibited renegotiation of securitized loans.⁹

These economic arguments prompted the federal government to intervene in the mortgage market by providing financial incentives to lenders to renegotiate residential mortgages. On February 19, 2009, President Obama announced the Home Affordable Modification Program (HAMP), which became a central policy tool aimed at bolstering the rate of modifications of residential loans. The program guidelines were presented on March 4, 2009.

II.B Borrower Eligibility

According to HAMP guidelines, borrowers' eligibility during the program was based on a number of factors. First, the property had to be owner-occupied and the borrower's primary residence. Vacant and investor-owned properties were excluded. Second, the property had to be a single-family (one- to four-unit) property, with a maximum unpaid principal balance on the unmodified first-lien mortgage equal to or less than \$729,750 for a one-unit property. Third, the loans had to have been originated on or before January 1, 2009. Fourth, the first-lien mortgage payment had to be more than 31% of the homeowner's gross monthly income in order for the program to reduce the household monthly debt burden to a target of 31%. Finally, the program rules require the servicers to offer a trial modification first, which may be subsequently converted into a permanent modification only if the modification is successful during the trial

⁸ In times of adverse economic conditions, renegotiating some mortgages instead of foreclosing them could create value for both borrowers and lenders (Bolton and Rosenthal 2002; Piskorski and Tchistyi 2011).

⁹ Moreover, coordination frictions between multiple investors of securitized debt can make it hard to change the contracts between them and the servicers. Existing research has been consistent with the view that securitization adversely impacted incentives to renegotiate mortgages (Piskorski et al. 2010 and Agarwal et al. 2011).

period (i.e., borrowers make payments per the changed contract that was offered on a trial basis, which typically takes about six months).

In our empirical analysis, we use some of these eligibility criteria to classify borrowers into those who are likely to be affected by HAMP (treatment group) and those who do not qualify (control group).¹⁰ We note that verification of these criteria requires servicers to employ appropriate infrastructure and sufficiently trained staff. For instance, processing applications for program modifications involves direct contact between servicer and borrower, potentially through a call center, in order to collect relevant information.

II.C Incentives for Servicers

We now discuss the incentive payments for the servicers and lenders who participate in the HAMP program. In discussing these payments, we focus primarily on the first-lien modification program, which has been the largest component of HAMP, and will be the focus of our analysis.

The major feature of the first-lien modification program is its incentive payment structure. The funds from the program were to provide one-time and ongoing “pay-for-success” incentives to loan servicers, mortgage holders/investors, and borrowers. First, there were to be one-time incentive payments to servicers of \$1,000 for each completed permanent modification under HAMP. Second, servicers were also eligible for up to \$1,000 in annual, ongoing pay-for-success incentive payments that would accrue when monthly mortgage payments were made on time for three years after the borrower’s monthly mortgage payment was permanently modified. In addition, servicers would receive an additional current borrower bonus incentive payment of \$500 when a loan was permanently modified for a borrower whose loan was current. As noted earlier, these incentive payments are quite substantial relative to the regular fees for servicing, which amount to about twenty to fifty basis points of the outstanding loan balance per year (roughly \$400 to \$1,000 per year for a mortgage with \$200,000 of outstanding loan balance; see Barclays 2008 Global Securitization Annual).

Mortgage holders/investors would also receive this type of incentive as a one-time payment of \$1,500 for each modification agreement executed with a borrower who was current on mortgage payments upon entering HAMP. Finally, borrowers who remained current on their mortgage payments would be eligible for up to \$1,000 in annual, ongoing “pay-for-performance” incentives for five years—to be used to pay down the mortgage principal. There was also a cost-sharing arrangement with mortgage investors for help in reducing first-lien mortgage payments.

¹⁰ In addition, servicers were required to screen candidates for loan modification to ensure that these borrowers were in danger of imminent default. Subsequent to such a determination, an NPV (net present value) test was required on each loan that was in imminent default or was sixty-plus days delinquent under the Mortgage Bankers Association (MBA) delinquency calculation. This test compares the NPV of cash flows expected from a modification to the net present value of cash flows expected in the absence of modification. If the payments after modification are greater, the NPV test result is deemed positive, warranting a modification under HAMP.

We note that while the servicer participation in the program was voluntary, many major bank servicers in the United States decided to participate. This includes all the servicers in our main data set. However, as we corroborated in conversations with the economists at the U.S. Department of Treasury, some servicers of non-agency securitized mortgages associated with RMBS deals issued by foreign underwriters opted out of the program. We use an alternative dataset consisting of renegotiations conducted by such servicers to better assess renegotiation activity in the absence of the program.

At the time of its introduction, the program was to remain in force until December 31, 2012. Program payments were to be made for up to five years after the date of entry into a Home Affordable Modification. According to the Government Accounting Office (2009), the overall funds allocated to HAMP were \$75 billion. The expectation of policy makers—given the number of severely indebted households—was that about three to four million homeowners would receive assistance with their mortgages during forty-five months of the program.¹¹

III. Data

Our main data source for the analysis is the OCC Mortgage Metrics data. This unique data set includes origination and servicing information for U.S. mortgage servicers owned by large banks supervised by the OCC. The data consist of monthly observations of over 34 million mortgages totaling \$6 trillion, which make up about 64% of U.S. residential mortgages. About 11% of these loans are bank-held, and 89% are sold to investors through GSEs as well as through the private market. Because of various restrictions implied by our empirical design and the availability of relevant loan characteristics in the data, we end up using about 20.8 million of these loans in our analysis.¹² We study loans over the period July 2008 through December 2010. Since HAMP was implemented in March 2009, we have data that span nine months in the period before HAMP was implemented and twenty-one months of the program period.

The origination details in the data set are similar to those found in other loan-level data (e.g., First CoreLogic LoanPerformance or LPS data). In particular, there is information on original loan terms as well as mortgage, property, and borrower characteristics (e.g., credit score, owner-occupancy, balance, and interest rate). The servicing information is collected monthly and includes details about actual payments, loan status, and changes in loan terms.

The data set contains detailed information about the workout resolution for borrowers. We know if the loan was modified under HAMP—either as a trial or permanent modification—or if it was

¹¹ This estimate was based on the number of homeowners who were likely to be at risk of default (over 10 million homes), to have unaffordable loans (more than 8 million homes), to apply for a loan modification (5.5 million homes), and to pass the NPV test (about 4 million homes). See U.S. GAO Report, July 2009.

¹² The reason for this attrition is due to the missing values for loan characteristics in the data, mainly their owner-occupancy status. As will become clear, this field is needed to classify the loans into treatment and control groups. We will discuss later why, despite this attrition, we think our sample is reasonably representative of the population.

privately modified by servicers. The data set contains information about the change in contract terms when a modification occurs (e.g., reduction in interest rate, amount of principal deferred or forgiven etc.), and the repayment history before and after the action (current, delinquent, etc.). It also provides information on the identity of the sixteen main servicing entities responsible for the mortgage. This allows us to exploit within-servicer variation as well as variation across servicers.

We also use a loan-level data set provided by BlackBox Logic that covers almost all securitized mortgages issued without government guarantees. In addition to origination and payment data for each of these loans, this data set also reports whether a mortgage received a private modification in a given month. By merging this data with underwriter data provided by ABSNet, we are able to separately analyze private modification rates for loans in deals handled by servicers who opted out of the program. As we will discuss later, this analysis will help investigate the modification trends among servicers who did not participate in the program.

Finally, in our zip-code-level analysis, we use zip-level house price indices from CoreLogic, zip-level auto sales growth data from Mian and Sufi (2010), zip-level data on non-durable spending growth from Nielsen data at Chicago Booth (Kilts Center) and data on consumer credit performance from a credit bureau.

IV. Empirical Methodology

IV.A Research Design

The biggest obstacle in evaluating the impact of the program on outcome variables is to get an estimate of the counterfactual level in the absence of the program. We circumvent this obstacle by exploiting variation in exposure of similar borrowers to HAMP. The key to our empirical design is defining the groups of borrowers that are eligible for HAMP. The main empirical strategy (called Strategy 1) exploits variation in owner-occupancy criteria for receiving renegotiation under HAMP to form these groups. Specifically, we argue that borrowers whose properties are classified as investor-owned during program implementation are ineligible for HAMP and, therefore, can serve as a control group for the treatment group—namely, the group of borrowers whose properties are classified as owner-occupied.

We investigate the validity of this assertion in the data and find support for it when we evaluate various borrower and contractual observables.¹³ In particular, we show that there are no differential trends in how the treatment group compares with the control group before the program is passed (see Meyer 1995). The identification assumption is that in the absence of

¹³ Our data consists of loans serviced by main banking institutions and, in general, includes mortgages of much better average credit quality than typical loans that were used to finance speculative investments in the non-agency securitized markets. As we will show, this makes the treatment and control groups formed based on owner-occupancy status very comparable in our data (see Haughwout et al. 2011, who show differences between owner-occupied and investor loans when they investigate the sample of largely non-agency securitized mortgages).

HAMP, the difference between treatment and control groups would display similar payment and renegotiation patterns (up to a constant difference) during the period of the program as they did before it. We provide evidence on the validity of this assumption in Sections IV.B and Section VI.

We rely on the following difference-in-difference specification to estimate the effect of HAMP:

$$Y_{it} = \alpha + \beta \times T_i + \gamma \times T_i * 1(\text{After})_{it} + X_{it} \delta + \varepsilon_{it},$$

where T takes a value of 1 for loans in the treatment group and 0 for the loans in the control group. *After* takes the value of 1 for the quarters after 2009:Q1 (the program period), and 0 otherwise. Loans for owner-occupied properties take a value of $T=1$, while the investor-occupied loans take a value of $T=0$. The occupancy status of these properties is based on information gathered at origination of the loan. In addition, we require that loans in the treatment group have an outstanding balance below the program eligibility cutoff of \$729,750. The coefficient γ measures the effect of the program on the treatment group relative to the control group, while the coefficient β measures the pre-program differences between the treatment and control groups.

We estimate these regressions on all mortgages. The reason is that the only requirement of HAMP is that borrowers must “face economic hardship and a danger of imminent default.” The program guidelines do not have any specific requirement that a loan has to be delinquent or under water to be eligible. In fact, the program provides additional financial incentives to servicers to actively modify loans that are currently making payments (but may not do so in the future). Nevertheless, one could potentially also conduct the analysis only on delinquent loans, arguing that borrowers with these loans are those most likely to satisfy these criteria. While our results are qualitatively similar to those reported in the paper, we are cautious in following this route. The reason is that, as discussed in Section V.C.2, delinquency status of a loan may itself be a response variable to HAMP—since the program design may itself induce borrowers who would otherwise continue making payments to default (see Mayer et al. 2011).

The first outcome variable employed in these regressions is to assess the extensive margin—that is, whether or not the loan was modified (i.e., $Y_{it}=1$ if loan i was modified in time t). We use several variants of this variable, such as whether the loan was privately modified or was modified under HAMP. To ensure that we track the rate of modifications on loans rather than the cumulative effect, we drop loan observations subsequent to the modification when we use a loan in a panel setting. In our regressions, we account for different loan-level attributes that capture observable idiosyncratic differences across borrowers. In particular, X_{it} is a vector of loan and borrower characteristics that includes variables such as initial FICO credit score, initial and current loan-to-value ratio (LTV), and initial interest rate and loan balance. We also include controls for loan ownership status: whether a loan is securitized into GSE-backed pools (agency

loan), is securitized without government guarantees (private-label loan), or is bank held (portfolio loan). In addition, we also employ origination year and servicer fixed effects to absorb any aggregate effects driven by the times at which loans were originated and to capture idiosyncratic servicer-related effects.

In our subsequent specifications, we also investigate the intensive margin—that is, we employ similar regressions to evaluate the likelihood of receiving different types of contractual modifications conditional on receiving one (i.e., $Y_{it}=1$ if loan i was modified in time t and the modification was of a certain type). Similar regressions are also employed to assess the efficiency of renegotiations by tracking the likelihood of redefault of a loan subsequent to receiving a modification (i.e., $Y_{it}=1$ if loan i was modified in time t and the loan redefaulted within a certain time period from t) and the likelihood a loan is foreclosed (i.e., $Y_{it}=1$ if loan i was foreclosed in time t).

Finally, we note that in our main specifications that investigate change in renegotiation rate, the loans that default (e.g., become seriously delinquent) do not exit the estimation sample. Only when these loans are foreclosed do they exit the estimation sample. We include these loans since delinquent mortgages could be considered as plausible candidates for renegotiation. Similarly, in specifications that investigate the change in foreclosure rate, loans that are renegotiated do not exit the estimation sample. Again, these loans are included since they may be plausible candidates for getting foreclosed. In Section VI.C we discuss the robustness of our findings with respect to these choices.

IV.B Potential Concerns

We confront several challenges in the identification of our key estimates. First, we need to show that the treatment and control groups are comparable before the program was implemented. Table 1 presents the descriptive statistics for important observables at the quarterly frequency in the treatment and control groups as defined by our empirical strategy. It reports the statistics in the pre-HAMP period—that is, from July 2008 to March 2009.

As one can observe, the control group is very similar to the treatment group on most observables. In particular, the control group has loans that have, on average, a slightly higher FICO credit score relative to the treatment group (717 versus 710). The mean LTV is about 70%, and about 1.7% of loans are seriously delinquent (payments that are at least two months past due) in both groups. Moreover, interest rate, a statistic that captures the overall riskiness of the borrower pool in the two groups, is very similar across the two groups (the mean for both is slightly above 6%). The renegotiation rates in the two groups differ a bit in the pre-HAMP period—about 0.3% of loans obtain private permanent modifications per quarter in the control group and about 0.4% in the treatment group—but, importantly for our identification, as we will show in Figure 3(b), there are no pre-trends in this difference. It is worth noting that not only the means but the computed

standard deviations of the two groups are quite similar for all these variables as well. These patterns are also visible in Figure 1. In particular, in Panels 1(a)–(c), we plot the kernel densities of FICO credit score, LTV, and interest rates for the borrowers in the treatment and control groups defined based on owner-occupancy status. The borrowers in treatment and control groups look remarkably similar on all these dimensions.

The observables in the treatment and control groups are not only well matched across time in the pre-treatment period, but they are also matched period by period (Figures 2(a)–2(d)). For instance, Figure 2(d) plots the evolution of the monthly delinquency rate of treatment and control group in the pre-program period and illustrates that these rates track each other very closely up to a constant difference (we revert to more formal tests of this assertion in Section VI.C). Similarly, Figure 3(b) confirms that the renegotiation rates in the two groups follow similar pattern in the period before the program.

In general, one might be worried that borrowers in our treatment and control groups may differ significantly. However, our data consist of mortgages serviced by main banking institutions, which are known to be on average of a better quality than the entire population of U.S. mortgages (see Piskorski et al. 2010). This could explain why loans in the control group (investor-owned properties) are well matched with those in the treatment group (owner-occupied properties) in our data. Nevertheless, we provide evidence for robustness of our results by using an alternative empirical strategy in Section VI.A that allays these concerns—both treatment and control groups in this strategy consist of owner-occupied properties with similar observables. We also conduct several other robustness tests to deal with related concerns in Section VI.

Second, like other studies on program evaluation that use the difference-in-difference strategy (e.g., Mian and Sufi 2010), we will not be able to comment on any economy-wide effects introduced by the program. For instance, we will not be able to detect any across-the-board improvement or worsening in renegotiation process and standards due to the program because such effects will be differenced out.

V. Impact of HAMP: Loan-Level Analysis

V.A Impact on Extensive Margin: HAMP and Private Modifications

V.A.1 HAMP Trial and Permanent Modifications

We start our analysis by discussing the renegotiations induced by the program in the treatment group. We first focus on renegotiations that are offered in the form of “trial modifications,” and may be subsequently converted into “permanent modifications” if the modification is successful during the trial period (i.e., borrowers make payments according to the changed contract that was offered on a trial basis).

In Figure 3(a), we present the fraction of loans that enter trial and permanent HAMP modifications for the first time in a given month in the treatment group as defined by our main empirical strategy. There is a substantial increase in HAMP trial modifications in the treatment group just after the introduction of the program in March 2009. As shown in Figure 3(a), the rate of HAMP trial modifications peaks around late 2009 and then starts to decline. We note that the sharp decline in the number of HAMP trial modifications in the second half of 2010 was likely related to the tightening of program eligibility rules for such modifications. Prior to June 1, 2010, trial modifications could be initiated even if borrowers did not provide all required documentation to potentially roll them over into permanent modifications. Borrowers had to submit the required documentation in order to enter the trial modification subsequent to this date. (See Supplemental Directive 10-01 of the U.S. Department of the Treasury.)

As we observe from Figure 3(a), on average, about 0.144% of loans enter a HAMP trial modification in a month in the treatment group (with the peak being around 0.35% per month). This translates into a 1.74% annual modification rate. This rate implies that, during our sample period, about 522,000 loans received a trial HAMP modification in the treatment group.

In Figure 3(a), we also present the fraction of loans that enter permanent HAMP modifications for the first time in a given month in the treatment group. In our sample, a permanent HAMP modification resulted, on average, about 20% reduction in monthly payment--a saving of \$350 per month. There is a substantial increase in HAMP permanent modifications, starting a few months after the program was introduced in March 2009. This pattern is mechanical because, as we discussed earlier, a loan could be given a permanent HAMP modification only subsequent to a successfully completed trial HAMP modification, which usually took at least three months. On average, about 0.055% of loans per month received a permanent HAMP modification in the treatment group (with a peak of about 0.14% per month). This translates into about a 0.66% annual modification rate. This rate implies that during our sample period, about 200,000 loans received a permanent HAMP modification in the treatment group. We note that, as a validation of our empirical design, we verify that our classification of loans into the treatment group based on the program guidelines corresponds to the loans where we observe modifications performed under HAMP.¹⁴

It is also worth noting that using these estimates we can get a sense of the “conversion rate” from trial modifications to permanent ones. In particular, our findings suggest that about 38% of HAMP trial modifications were converted into permanent ones. The rate is smaller than 100% because the program guidelines require the conversion from trial to permanent HAMP

¹⁴ There are a few program modifications that we observe in the control group. These cases are relatively rare and, importantly, excluding or including them does not impact our inferences. Conversations with servicers suggest that these cases reflect program guidelines that allow for modifications under the program to be offered to borrowers that, at the time of applying for a modification, could credibly show that the property was now their main residence.

modification to be based on several criteria. These include the borrower making the scheduled payments under the terms of the trial modification, as well as the borrower providing the necessary documentation that helps servicers to verify borrowers' eligibility for the program. We summarize these findings in Table 1, where we present the average quarterly rates of trial and permanent HAMP modifications in the treatment group based on owner-occupancy status.

Next, we further explore the characteristics of mortgages that were more likely to receive a modification under HAMP. To do so, we assess how the likelihood of receiving a trial or a permanent modification under the program relates to observables on a given loan in the treatment group. Columns (1)–(2) of Table 2 present the estimates from specifications that employ a dependent variable that takes the value of 1 if a given loan in the treatment group (defined by Strategy 1) received a trial HAMP modification during the program period (2009:Q2 to 2010:Q4) and is 0 otherwise.¹⁵ Columns (3)–(4) present the corresponding results for permanent HAMP modifications.

As we observe, mortgages given to borrowers with lower FICO credit score, higher loan-to-value ratios, higher interest rates, and higher loan amounts, as well as lower documentation level, are more likely to receive both trial and permanent HAMP modifications. These results are not surprising given that the program targeted loans at risk of default, and these characteristics are broadly indicative of the higher risk of default.¹⁶

Overall, these results indicate that HAMP induced a sizeable number of modifications in the eligible group of loans. However, this does not necessarily mean that the program increased the overall rate of modifications performed by the servicers, as it may also have affected the modifications outside of the program (that is, private modifications). We formally investigate this question in the next section.

V.A.2 Private Permanent and Overall Modifications

We now explore the effects of HAMP on renegotiations done by servicers based on their private incentives outside the program (private modifications). In Table 3, we test whether HAMP

¹⁵ Throughout the paper we estimate our specifications using the OLS despite the binary nature of several of the dependent variables. The reason is that we have a large number of fixed effects along several dimensions, and using logit or probit results in an incidental parameters problem. Our OLS specification with flexible controls to capture nonlinearity allows us to estimate our coefficients consistently even with multiple fixed effects (Dinardo and Johnston 1996). Regardless, we have verified that we obtain qualitatively similar inferences when employing logit specification without employing as many fixed effects.

¹⁶ We also investigate the relation between incidence of HAMP modification received by a loan and its ownership—i.e., whether loan is securitized into GSE-backed pools (agency loans), is securitized without government guarantees (private-label loans), or is bank-held loans (portfolio loans). We find significant number of HAMP modifications (both trial and permanent) in all ownership categories. These results, along with those in Section V.A.2, suggest that, consistent with one of its objectives, HAMP did enhance modification activity on securitized loans.

affected the rate of permanent private modifications in the treatment group.¹⁷ We focus on permanent private modifications, since these renegotiations have been shown to be the main renegotiation tool for loss mitigation in the period before the program (Agarwal et al. 2011). As described in the section discussing our empirical strategy, the impact of HAMP on private modification rates in the treatment group relative to the control group can be identified by the coefficient on $T*After$. The coefficient estimates in Columns (1)–(3) suggest that the rate of private permanent modifications in the treatment group slightly increased relative to the control group after the program’s introduction (0.014% to 0.020% on a quarterly basis). This would translate into between 17,000 and 24,100 extra private permanent mortgage modifications in the treatment group during our sample period. This evidence suggests that the program did not result in a decline in the rate of private modifications in the treatment group.¹⁸

We also observe these patterns in Figure 3(b), where we present the fraction of loans that enter permanent private modification for the first time in any given month in the control and treatment groups. Private permanent modification rates in the two groups display similar patterns before the introduction of HAMP in March 2009. This is consistent with earlier evidence that showed that treatment and control groups are comparable in the pre-treatment period, further validating our empirical design. The numbers in the figure suggest, on average, that the quarterly private modification rates range from 0.3% to 1.8% (60,000–180,000 modifications per quarter). As discussed, there is an increase of 0.014% to 0.020% in the quarterly private permanent modification rate in the loans in the treatment group during the program. In addition, the program resulted in an absolute increase of 0.165% in the quarterly permanent modification rate in the treatment group because of the permanent HAMP modifications (Table 1). Taken together, these estimates suggest that HAMP led to an increase in the annual modification rate of about 0.72%. This amounts to about a 40% increase relative to the pre-program mean modification rate in the treatment group.

We confirm these findings in Figure 3(c), which presents the combined (private and HAMP) permanent modification rates in treatment and control—and more formally in Column (4) of Table 3, where we estimate the overall impact of the program on the rate of permanent modifications (private and HAMP). At this rate, the program would induce about 1.2 million additional permanent modifications over its original duration (i.e., through December 2012)—significantly short of the government expectations of three to four million modifications.¹⁹

¹⁷ Throughout the paper we cluster standard errors at the state level corresponding to the location of the property backing the loan. The results are also robust to clustering at the loan level.

¹⁸ The fact that there is no adverse impact of the program on the rate of private modifications may seem surprising. However, we do find significant evidence that the program adversely impacted the aggressiveness and effectiveness of renegotiations performed outside the program (Section V. B).

¹⁹ We arrive at 1.2 million additional permanent modifications induced by the program, assuming that our estimates are valid for the entire stock of 45 million potentially eligible loans for the program in the U.S. This involves

These findings are robust to performing inferences separately among agency and non-agency loans (i.e., among loans issued with or without guarantees of government-sponsored entities such as Fannie Mae or Freddie Mac). Panel B of Table 3 shows these results. As we observe, there is no decline in private permanent modifications in the treatment group in either category of loans. Moreover, the program resulted in a similar increase in the overall rate of permanent modifications in the treatment group for agency and non-agency loans (0.13% and 0.15% increases in the quarterly permanent modification rate, respectively). Similar inferences hold when we further split non-agency loans into privately securitized and bank-held loans. These results suggest that the program resulted in an overall increase in the permanent modification rate regardless of the loan ownership categories (i.e., whether a loan is bank held, is privately securitized, or is securitized with government guarantees).

At a first glance, the finding of no decline in the intensity of private modifications in the treatment group during the program period may appear surprising. However, note that the program could broadly affect the rate of private renegotiations in two ways. First, in the presence of government incentives, lenders may substitute some of the private modifications with HAMP ones. This crowding-out of private activity with government subsidized one would lead to a decline in the rate of private renegotiations in the treatment group. However, there may be a second countervailing force, which could potentially blunt the first effect. In particular, the program, through its outreach effort, could increase the pool of borrowers in the treatment group who apply for modifications. Consequently--to the extent that some of the additional applicants who did not receive a HAMP modification could end up getting a private one--the program could also positively impact the intensity of private modifications in the treatment group.

The evidence in the data is consistent with the second force outweighing the first effect. In particular, we find that more than a third of borrowers who applied for a HAMP modification in the treatment group--and received a trial HAMP modification that did not become permanent -- subsequently received a permanent private modification. Since attracting and evaluating potential borrowers for a modification is costly, it may be profitable for servicers to offer a private modification to some applicants who, upon evaluation, did not qualify for a permanent HAMP modification. As HAMP triggered an increase in borrower solicitation through its outreach effort, it also expanded the pool of applicants who did not qualify for the program. Once the costs of attracting and evaluating these borrowers--a significant component that determines profitability of a modification--became sunk, it may have been profitable to offer

applying the same estimate for potentially eligible loans that are not covered in our data and projecting the same rate from the end of our sample period until the end of the program period. Notably, our estimated number of HAMP modifications is very close to the *actual program modifications* released by the administration in 2013. This fact lends credibility to representativeness of our sample.

private modifications to some of them.²⁰ While the presence of the second effect masks the potential substitution on extensive margin, we next provide evidence that servicers did channel some loans that they would have modified privately through HAMP instead.²¹

V.B Impact on Intensive Margin: Contract Terms and Redefault Rates

In this section we evaluate the changes on the intensive margin—that is, on the type and effectiveness of modification offered, conditional on the loan receiving a modification. In general, lenders can change more than one dimension of the contract term when they renegotiate a loan. For example, a lender may offer an interest rate reduction on the loan, as well as writing down the principal. We focus on the key categories of such changes, evaluating the change in the rates of these modification types around the program. In addition, we examine the impact of the program on the rate of default of renegotiated loans (“redefault rate”), a commonly used metric to evaluate the effectiveness of renegotiations (see Haughwout et al. 2010).

In Panel A of Table 4, we follow a specification similar to the main one, with the analysis confined to modified loans. In terms of the outcome variable, we are now interested in measuring the type of contract changes in both HAMP and private modifications after the passage of the program. Accordingly, the $T*After$ interaction term in the present context captures the change in the contract terms associated with both private and HAMP permanent modification in the treatment group relative to the control group. The results in Columns (1)–(4) show that overall permanent modifications in the treatment group became less aggressive relative to ones in the control after the program was introduced. In particular, the incidence of more aggressive tools like rate reduction, term extension, and principal reduction decrease (by about 11%, 9%, and 2%, respectively), while the incidence of less-aggressive tools, like capitalization of unpaid interest in the principal amount due, increases (by close to 10%).

To better understand the composition of modification tools, in unreported results we also separately consider the permanent private modifications and HAMP modifications. We observe

²⁰ Given the program incentives, servicers may have been willing to ex-ante spend resources on borrowers to learn about their program eligibility even if they know that a sizeable proportion of these borrowers would not qualify for the program once necessary information had been collected. This investment may have positive expected value for servicers, with program benefits earned on qualifying borrowers compensating servicers for costs incurred on evaluating borrowers who would end up failing to qualify for the program.

²¹ A simple example can illustrate this point. Suppose that in the absence of the program servicers would have performed 100 private modifications in the pool of loans controlled by them. Subsequent to the program implementation, and given that the incentive payments under the program are higher if a loan did not redefault after a modification, the servicers will channel 20 most promising and eligible of these modifications to be performed under the program. The remaining 80 loans would be modified privately. This is the first crowding out effect. However, the program through its outreach results in additional 50 borrowers applying for modification. After spending resources on evaluating these borrowers suppose that only 10 are eligible for HAMP modification. Of the remaining 40, given that the costs of attracting and evaluating these borrowers—a significant component that determines profitability of a modification—became sunk, it may have been profitable to offer private modifications to 20 of them. This is the second effect. Consequently, after the program introduction we end up with 130 modifications of which 30 are program ones and 100 are being done privately. Note that the number of private modifications does not decline despite the fact that the program resulted in channeling of some the modifications that would have been conducted privately to be performed under the program.

that servicers offered more comprehensive modification terms for renegotiations done under HAMP. There is a significantly higher incidence of rate reductions observed on HAMP modifications relative to the private permanent modifications in the treatment group (55% higher). This pattern is consistent with the program requiring participating servicers to make mortgages more affordable for borrowers with economic hardship and facing imminent default. The incidence of term extensions and principal write-downs is also higher for HAMP modifications, but the magnitudes are smaller (27% and 3%, respectively). These results suggest that although HAMP modifications appear to be more aggressive in terms of concessions offered to the borrower, concurrently, private permanent modifications performed in the treatment group became less aggressive after the program's introduction relative to the control group.

In Panel B of Table 4, we use an indicator of whether or not a modified mortgage redefaults within six months of renegotiation as the dependent variable. Our specification is similar to the main one, with the sample confined to modified loans. Note that there is a significant downward trend in redefault rates for both treatment and control group loans over time. More importantly, as is evident from Column (2) we find that the program did not affect average redefault rates in the treatment group relative to the control group.

To better understand these results, in unreported tests we evaluate the change in redefaults separately for private and HAMP modifications. We find that the redefault rate of HAMP-modified loans is significantly lower, around 5%, than that of private permanent modified loans in the treatment group. This effect is sizable relative to the mean redefault rate of about 20% for permanent private modifications in the treatment group in the pre-program period. However, this increase in efficiency (as measured by the redefault rate) due to HAMP modifications is entirely offset by concurrent reduction in efficiency on private permanent modifications.

The findings from this section suggest that the program had an effect on the intensive margin. In particular, we find that subsequent to the program introduction, private modifications done outside the program in the treatment group became less aggressive in their composition as well as in their effectiveness relative to the control group. This drop in effectiveness of private modifications appears to be offset by higher effectiveness of HAMP modifications, resulting in no change in the average effectiveness of modifications in the treatment group around the program. This analysis shows that servicers may have channeled more promising loans (on unobservables) to be modified under the program, since the incentive payments under the program were higher if a loan did not redefault after a modification. As a result of this channeling of promising loans to the program, there was an adverse impact of the program on effectiveness of private modifications in the treatment group.

It is worth noting that these findings do not imply that the program did not have an effect on other economic outcomes since there was an increase in the overall rate of modifications in the treatment group (i.e., due to expansion on the extensive margin). We now investigate this aspect.

V.C Impact on Foreclosures and Delinquency Rates

V.C.1 Foreclosure Rates

We now turn to examining the impact HAMP had on the outcome it was designed to ultimately affect—that is, the rate at which loans are foreclosed. In Panel A of Table 5, we assess HAMP’s effectiveness in preventing foreclosures by examining how the rate at which a loan was foreclosed in a given quarter varies across the treatment and control groups. As before, the coefficient of interest in these regressions is $T*After$.

The results indicate that there was a decrease in the rate of completed foreclosures in the treatment group during the program period. In particular, among all the loans, we observe a 0.12% decrease in the quarterly foreclosure rate (about 17% lower than the foreclosure rate in the control group during the program period).²² This would translate into a decrease of 0.48% in the annual foreclosure rate and about 145,000 fewer foreclosures in the treatment group during our sample period. This rate would translate into about 800,000 fewer foreclosures in the treatment group over the original duration of the program (i.e., through December 2012).²³ As Column (3) indicates, the estimated reduction in the foreclosure rate is robust to inclusion of state fixed effects for the location of the property backing the mortgage.

In the next three columns we conduct an alternative test in which we evaluate the change in foreclosure rates for delinquent loans instead of using all the loans. Note that as explained earlier, we prefer to do our analysis on all loans, because delinquency status of a loan is itself an endogenous variable that could be affected by HAMP (also further discussed in Section V.C.2). With this caveat in mind, the test does give us an assessment of how foreclosure rates change on distressed loans. Among delinquent loans we observe about a 2% absolute reduction in the quarterly foreclosure rate (8% decrease in the annual foreclosure rate). Notably, Column (6) shows that the estimated reduction in the foreclosure rate among delinquent loans is also robust to inclusion of the state fixed effects for the location of the property backing the mortgage.

Finally, note that these estimates represent the overall impact of the program on foreclosure rates during our sample period. Hence, they represent the combined effect of trial and permanent HAMP modifications, changes in the number and composition of private modifications, and the program’s impact on other servicing actions and outcomes that may impact foreclosure rates. It is also worth noting that these estimates are obtained for our sample period ending in December 2010. As a result, we cannot quantify the overall effect of the program on foreclosure rates

²² Alternatively, this estimate represents a 40% relative decrease with respect to the mean foreclosure rate in the treatment group prior to the program. Note that the relative reduction in foreclosure rate relative to the control group during the program period is smaller than this estimate because foreclosure rates have been trending upward.

²³ We arrive at 800,000 fewer foreclosures induced by the program, assuming that our estimates are valid for the entire stock of 45 million potentially eligible loans for the program in the United States. This involves applying the same estimate for potentially eligible loans that are not covered in our data. In addition, we project using the same rate from the end of our sample period until the end of the program period.

beyond this horizon. It is possible that the decline in foreclosure rates may be temporary. For instance, servicers may just be delaying foreclosures while the program is being implemented. In addition, it is also possible that some of the effects we document may reflect inter-temporal substitution since some renegotiations that may have been done in the future could have been pulled into program period by HAMP. Thus, the long-run impact of program on modifications and foreclosure rates could be even smaller. Nevertheless, we note that even if the reduction in foreclosure rates due to HAMP was temporary and confined to our sample period, such a reduction may have some social benefits by spreading the incidence of foreclosures over a longer horizon (see Mian, Sufi, and Trebbi 2011).

V.C.2 Delinquency Rates

An important concern regarding mortgage modification programs is that they may induce borrowers who would otherwise continue making payments to default in order to increase their chances of receiving help (e.g., see the discussion of such behavior in the context of the Countrywide modification program in Mayer et al. 2011). We now examine whether we find any evidence that HAMP induced such strategic behavior on the part of borrowers. In particular, we examine if the program increased the propensity of some borrowers to become delinquent in order to benefit from reduced debt payments under the program.

We estimate a regression in which the dependent variable is the probability that a loan becomes 60 days past due in a given quarter, conditional upon making payments (being current) two months earlier. In other words, we estimate the transition rate of a loan moving from being current to 60 days delinquent. Again, the main focus is on $T*After$, which estimates the change in this transition rate in the treatment group relative to the control group in the period after HAMP was introduced.

As reported in Panel B of Table 5, there is a relative increase in the delinquency rate in the treatment group in the pre-HAMP period. However, this increase is very small, on the order of about 0.027% per quarter. This is an increase of just 1.5% in relative terms when compared with the pre-program mean in the treatment group. These results suggest that the program did not induce a significant wave of defaults by potentially eligible borrowers relative to those who were ineligible for the program.

To investigate the timing of these effects further, we re-estimate the specification in Table 6 and present the results in Internet Appendix (A.1). Here, we replace the *After* dummy with quarterly dummies and their interactions with the treatment dummy (the excluded category includes observations from 2008:Q3). This specification allows us to investigate the quarter-by-quarter changes in default patterns between the treatment and control groups. As is shown, we again find no evidence that the program resulted in an increase in defaults in the treatment group relative to the control one in any quarter during the program period. The estimated differential quarterly changes in default rates are insignificant and economically small, with the effect ranging at the

maximum to about 0.15% change in the quarterly default rate in the treatment group (on a base of 1.60% mean quarterly default rate in the treatment group in the pre-program period).

These results seem sensible and may provide guidance for designing large-scale renegotiation programs in the future. In particular, HAMP guidelines contained multiple eligibility requirements that required borrowers to produce documentation of their economic hardship and danger of imminent default. In addition, there was also an evaluation trial period prior to permanently changing the contract with the borrower. Moreover, the program provided additional compensation to servicers for modifying the loans that were current. This suggests that our findings of limited strategic behavior induced by HAMP may have to do with extensive screening related to its eligibility criteria and its design of incentives for servicers.²⁴

VI. Extensions and Robustness

VI.A Alternative Identification Strategy

One potential criticism of our empirical strategy is that even though the control and treatment loans are comparable on observable dimensions, the two sets of loans might still differ on unobservables because they differ on owner-occupancy status. We now refine our empirical strategy to provide additional support for the findings derived using treatment and control groups that are formed based on owner-occupancy status.

This alternative empirical strategy (called Strategy 2) exploits program eligibility criteria based on loan amount *within the group of loans for owner-occupied properties*. Specifically, among borrowers with properties that are owner-occupied during program implementation, those with mortgages with outstanding balances above \$729,750 are ineligible for the program.²⁵ Therefore, we use these loans to construct the control group to measure the counterfactual level of renegotiations for mortgages with balances just below \$729,750 (treatment group) in the absence of HAMP.

It is important to note that relative to our main empirical strategy, this alternative strategy is likely to consist of loans in the control group that match better with those in the treatment group. The reason is that both groups consist of loans for owner-occupied properties with relatively similar balances. However, this empirical strategy potentially suffers from low power, because few mortgages with loan balances in the vicinity of \$729,750 face economic hardship and

²⁴ These findings are in contrast to strategic behavior induced by simpler modification programs. In particular, Mayer et al. (2011) show that the simple modification program by Countrywide Financial Corporation led to significant strategic defaults. Unlike HAMP, the Countrywide modification program did not employ extensive screening of borrowers. Instead, it relied only on serious delinquency of the borrower as the key eligibility criterion.

²⁵ The \$729,750 figure equals the temporarily increased maximum conforming loan eligibility limit for high-cost areas that was incorporated into the 2008 economic stimulus package. The new jumbo-conforming program was adopted by Fannie Mae and Freddie Mac, effective April 1, 2008, until December 31, 2010. Because the vast majority of loans in our sample were originated before April 2008, this loan limit had no particular meaning during their origination process (e.g., all loans in close range of this limit were not eligible for conforming loan status).

receive modifications. Regardless, this analysis provides a valuable consistency check for the results obtained earlier.

Specifically, similar to our main empirical specification, we estimate:

$$Y_{it} = \alpha + \beta \times T_i + \gamma \times T_i * 1(\text{After})_{it} + X_{it} \delta + \varepsilon_{it},$$

where T takes a value of 1 for loans in the treatment group and 0 for the loans in the control group. *After* takes a value of 1 for the quarters after 2009:Q1 and is 0 otherwise. Loans for owner-occupied properties whose amount outstanding is below \$729,750 as of the date of announcement of the program (March 2009) take a value of $T=1$, while loans for owner-occupied properties with the balance above this threshold take the value of $T=0$. To make the comparisons of loans in the treatment and control groups in the second strategy comparable, we restrict attention to loans that are within \$100,000 of the threshold. As before, we estimate these regressions on all mortgages and employ the same outcome variables.

Panel A of Table 6 confirms that loans in the control group are very similar to those in the treatment group in terms of their observable characteristics. These patterns are also visible in kernel densities of FICO credit score, LTV, and interest rates for the borrowers in the treatment and control groups defined based on the loan amount threshold (Internet Appendix A.2). Notably, as before, not only are the observables in the treatment and control groups well matched across time in the pre-treatment period, but they are also matched period by period (Figure 4(a)–(d)). For instance, Figure 4(d) plots the evolution of the monthly delinquency rate of treatment and control group in the pre-program period and illustrates that these rates track each other very closely (up to a constant difference). Moreover, Figure 5(b) shows that the renegotiation rates in the two groups follow almost an identical pattern in the period before the program.

In Figure 5(a), we present the fraction of loans that enter the trial and permanent HAMP modifications for the first time in a given month in the control and treatment groups as defined by this alternative strategy. The patterns in the plots suggest inferences similar to those obtained with our main empirical strategy. As was the case with our main empirical strategy, we verify that our classification of loans into the treatment group based on the program guidelines corresponds to the loans where we observe modifications performed under HAMP.²⁶

We now discuss salient results from this strategy, presented in Panel B of Table 6. First, consistent with results in Table 3, we find no evidence that the program resulted in a decline of the rate of private modifications in the treatment group (Column (1) of Panel B of Table 6). If

²⁶ We observe a few rare instances of program modifications in the control group. These modifications are on mortgages that were classified in the control group based on the loan amount as of program announcement but that became eligible for HAMP due to a progressive reduction in the loan amount implied by a loan amortization schedule. Our inferences are similar, regardless of whether these few cases are excluded or included in the analysis.

anything, there is a small increase in the quarterly rate of permanent private modifications (about 0.06%). Column (2) of Panel B in Table 6 confirms this by estimating the overall impact of the program on the rate of permanent modifications (private and HAMP together). As we observe, the quarterly rate of permanent modifications in the treatment group increases by about 0.21% relative to the control one (about a 30% increase relative to the mean permanent modification rate in the treatment group). These findings are also visible in Figure 5(c). Overall, these results are consistent with our previous findings of a significant positive effect of the program on the extensive margin (the number of permanent modifications).

Second, Column (3) of Panel B of Table 6 presents the results on redefault for the alternative identification strategy. Consistent with our previous results, we find no change in the overall efficiency of modifications in the treatment group relative to the control after the program was implemented.

Finally, Columns (4) and (5) of Panel B of Table 6 present the foreclosure results for the alternative strategy. Again, we find qualitatively similar evidence as before: the program reduced the number of foreclosures in the treatment group relative to that of the control group. As we see from Column (5) among delinquent loans, the estimated decline in the quarterly foreclosure rate equals 0.59% per quarter—a reduction of 14% relative to the foreclosure rate in the control group in the program period.

Two comments about this empirical analysis are worth noting. First, the results using this alternative empirical strategy are qualitatively consistent with those obtained in Sections V.A–V.B. This is despite the fact that analysis with this strategy suffers from potentially low power.

Second, we note that in our analysis, we classify borrowers as potentially in the treatment or control group based on their loan status as of program announcement. However, a borrower in the control group with a loan balance above the \$729,750 threshold may strategically become eligible for HAMP if the borrower pays down the loan’s principal over time. There are several reasons why this is not likely to be an issue in our analysis. One, we note that few loans in our data cross the balance threshold in our program period from the control group to the treatment group. Two, most of these loans appear to cross the threshold because of the mechanical amortization schedule implied by their mortgage payments before the program announcement. Three, we classify borrowers as potentially in the treatment or control group based on their loan status as of the program announcement, allowing us to circumvent the issue of potential manipulation of loan balance by borrowers to become eligible for the program.

VI.B Potential Bias due to Reallocation

There may be an additional concern that our estimated treatment effects are biased because servicers may use up some of their resources for conducting HAMP modifications in the treatment group at the expense of modifications in the control group, given the program

incentives. This channel, if operational, could inflate the program effect since our estimate in the treatment group is measured relative to the control group, which would concurrently see lower modification rates due to reallocation of resources by servicers. In this section we use several approaches to investigate if there is evidence for this concern.

First, we examine if there are differential trends in the control group around the program implementation. The thought experiment is that reallocation of resources by servicers from the control group should change the intensity of modifications in this set of loans after the program is implemented. We use the baseline regression in Table 3, analyzing the time trend in modification activity in the control group of loans around the program implementation. Table A.3 in Internet Appendix presents such a regression. As can be observed, we find no evidence for this conjecture. The qualitative inferences are similar when we do a quarter-by-quarter analysis instead (unreported for brevity).

Second, we analyze how modifications in the control group of servicers in our sample evolve relative to loans that would qualify as control group loans for servicers who did not participate in HAMP. As explained in Section II, the latter are mainly servicers sponsored by foreign underwriters. The idea here is that if servicers implementing modifications under HAMP do reallocate resources from the control group, we should expect a difference in modification activity after the program implementation between the two sets of servicers. We use the baseline regression in Table 3, analyzing the modification activity only in the control group of loans. We explore if there are differences in the modification activity between the two sets of servicers after the program implementation by including the interaction term *After*Foreign*, where *Foreign* is an indicator variable which takes a value 1 if a loan belongs to deals underwritten by foreign underwriter whose servicers opted out of the program and 0 otherwise. The results presented in Table A.4 in the Internet Appendix show that there is no such difference.

Finally, we assess if the treatment effect changes with a higher proportion of treatment group loans in the portfolio of the servicer. The idea here is that a higher proportion of treatment group loans might result in greater reallocation of resources by servicers from the control group after the program is implemented, thereby changing the treatment effect differentially. To explore this possibility we again employ our baseline specification from Table 3 but also include an interaction of *T* with a variable *Share*, which is the proportion of treatment group loans in the portfolio of that servicer. The results, presented in Table A.5 in the Internet Appendix, show that there is no evidence for such a scenario.

Overall, the analysis in this section suggests that servicers may not have shifted resources from servicing loans in the control group to the treatment group. These results suggest that the servicing technology is such that the marginal cost of offering an additional modification for a given servicer is roughly constant. This is likely to be the case if the main costs of performing private modifications were mostly of the fixed type, such as setting infrastructure.²⁷ Under this

²⁷ This is not the only theoretically possible servicing technology. For example, another possible servicing technology could be that marginal cost of modifying an additional loan is increasing in the total number of

scenario, servicers would simply modify more loans in the treatment group as a result of the program subsidies, leaving their operations in the control group unchanged. Combining the results of this section with those we found in Section V paints a picture that is consistent with this inference. Servicers modified more loans in the treatment group—with the more promising candidates for modifications channeled under HAMP to take advantage of program incentive payments—leaving modifications in the control group relatively unchanged.

VI.C Other Tests

The main results presented in earlier sections survive several additional robustness tests. We discuss these very briefly in this section, not reporting details for brevity reasons.

The first set of tests ensure that our findings using the main empirical strategy are not driven by differences in treatment and control group loans in the pre-program period. In particular, as discussed earlier, our analysis in Internet Appendix A.1 shows that the delinquency rates in the treatment and control groups track each other closely in the pre-program period with no apparent differential pre-trend in this period. We obtain similar inferences if we employ other observables instead of delinquency and perform the analysis like Internet Appendix A.1. Next, we estimate our specification for a sample of treatment and control loans that are closely matched on observables, including their past delinquency history. In particular, in each of the quarters we only include loans in the control group that are nearest neighbor-matched with treatment loans based on the Mahalanobis distance metric. Estimating our regressions on this sample again yields very comparable results. In addition, we also re-estimate our main specification allowing for pre-existing trend and find qualitatively as well as quantitatively similar results. For example, using a specification with pre-existing trend, we find that the program resulted in the overall increase in the quarterly modification rate of 0.131% compared to our base estimate of 0.144% increase.

The second set of tests assess if the (constant) pre-program difference in modification rates between the treatment and control group for first empirical strategy could be driving some of our findings. In particular, the concern is that if a certain group of loans—say those in the treatment sample—were more sensitive to a macro state variable, the propensity to modify these loans could continue grow at a faster pace relative to those in the control sample. Note that our second empirical strategy does not suffer from this concern since there are virtually no pre-treatment differences in treatment and control samples including their renegotiation rates (Figure 5(b)). Nevertheless, to address this issue for our main empirical strategy, we also re-estimate our key specifications using more flexible, non-linear specifications such as logistic regression. We find that the estimates obtained from these specifications are similar to those reported in the paper. For instance, using logistic specification we find that the program resulted in the overall increase in the quarterly modification rate of 0.128% compared to our base estimate of 0.144% increase (Table 3). Moreover, as a placebo test, we explore if there is differential change in the private

modifications performed by a servicer. In such a scenario, one would expect servicers to reduce their modifications in the control group because the marginal cost of modifying loans would increase as more renegotiations are performed in the treatment group due to program subsidies.

modification rates between loans in the treatment and control groups handled by servicers who did not participate in HAMP. These loans are classified based on criteria of our main empirical strategy. We find no evidence for any change in the private modifications between the two groups of loans around the program.

The treatment and control loans in the second strategy are much better matched. Nevertheless, we also assess the robustness of our findings based on the second strategy by making the control and treatment groups more comparable. In particular, we tighten the bound around the balance threshold from which the treatment and control groups are constructed—instead of using loans within \$100,000 of the threshold, we consider only loans within \$50,000 of the threshold. These results, presented in Internet Appendix A.6 and A.7, again show that our inferences are similar.

We also investigate whether our modeling choices regarding when loans exit estimation sample affect our estimates from both empirical strategies. We note that our specifications do already control for the variety of loan and borrower characteristics. Thus, to the extent the composition of our sample changes because of exit which is a function of loan observables, we do capture such changes, at least in part, by controlling in our specifications for the characteristic of the loans that remain in the sample. Nevertheless, since unobservables may matter for exit as well, we conduct several other tests to investigate the robustness of our findings. In particular, we estimate specifications that investigate changes in modification rate where we include loans in the estimation sample even after their foreclosure. Note that by definition such loans cannot be renegotiated since they have been foreclosed. The estimates from this specification are very similar to those reported in the paper.²⁸

Finally, we investigate whether the announcement of the program affected the behavior of servicers prior to the program implementation. We note that there was a relatively small time interval between announcement of the program and its implementation: the program was announced in February 2009 and the details of the program as well as its implementation started in March 2009. Nevertheless, we investigate whether there was a relative change in the modification and foreclosure rate between treatment and control group from February 2009 to March 2009. In particular, we follow the same specifications as in the paper (Panel A of Table 3 and Panel A of Table 5) but also include interaction of T with a dummy Pre that takes a value 1 in the period between February 2009 and March 2009 and is 0 otherwise. This interaction is supposed to capture any differential changes in the behavior of servicers with respect to treatment group relative to the control group in the period after the program announcement but

²⁸ Next, we perform our analysis of various outcomes among only delinquent loans, complementing our analysis of foreclosures that we already performed in this subsample (see Table 5). The inference we obtain in this sample is qualitatively similar but different in terms of economic significance when compared to estimates that are based on the sample of all the loans. The reason is that delinquent loans are more likely to be modified or foreclosed. As discussed in Section IV.A, we prefer conducting our analysis on all loans since delinquency of a loan was not the criteria for giving a program modification per se and because delinquency could itself be an endogenous response to the program.

before its implementation. Our results reveal no evidence of any differential change in the modification and foreclosure rates in the treatment and control groups between February 2009 and March 2009. It is also worth noting that there was considerable uncertainty surrounding the possible national modification program prior to its announcement in February 2009 regarding both its timing as well as its scope. Consistent with this notion, we find no significant relative change in the modification and foreclosure rates in the treatment and control groups before February 2009 as well. These results suggest that there was no differential change in the behavior of servicers with respect to the treatment and control group loans in anticipation of the program.

VII. The Role of Servicers

Our analysis suggests that the take-up rate—that is, the number of trial modifications being granted and the conversion rate of trial modifications to permanent modifications—under HAMP was significantly lower compared with policy makers’ expectations. Although it is hard to know what the optimal response to the program should have been, we now exploit heterogeneity in response across servicers to try to understand some of the potential barriers to program implementation.

The program’s effect on the extensive margin is not uniform across servicers in our sample. In particular, there is significant variation in the rate of trial and permanent HAMP modifications across servicers, with some servicers modifying at a rate that is more than four times the rate of others. Importantly, this variation cannot be accounted for by differences in contract, borrower, or regional characteristics of mortgages across servicers.²⁹ To illustrate this, Figure 6(a) plots the average quarterly trial and permanent HAMP modification rates across the sixteen main servicing entities in our sample. These servicer-specific rates are obtained based on servicer fixed effects in column (2) and column (4) of Table 2, Panel B. As we observe, the quarterly rates of trial HAMP modifications vary from as little as 0.03% to almost 1% across servicers. Similarly, the quarterly rates of permanent modifications vary substantially across servicers, from about 0.02% to almost 0.8%. Together, these results imply substantial variation in the conversion rates from trial modifications to permanent modifications across servicers (from less than 30% to about 80%).

Interestingly, there was similar heterogeneity in the rate of private modifications offered across these servicing entities in the pre-program period. Again, this variation cannot be accounted for by differences in contract, borrower, or regional characteristics of mortgages across servicers. This is illustrated in Figure 6(b), which plots the average quarterly permanent private modification rates across the servicing entities in our sample. These servicer-specific pre-program rates are obtained from servicer fixed effects in a regression similar to column (4) of Table 2, Panel B, but estimated on pre-program data. The pre-HAMP quarterly rates of permanent private modifications vary substantially across these servicing entities (from less than 0.04% to 1.4%).

²⁹ See Glaeser, Gyourko, and Saiz (2008) and Saiz (2010) for discussion on regional factors and house prices.

In Panel A of Table 7 we investigate whether there is a relation between renegotiation intensity of servicers in the pre-program period and the rate of permanent modifications induced by the program across these entities. To do so we first construct an indicator variable, *High Experience*, that takes a value of 1 for servicers that are above the median in terms of renegotiation intensity in the pre-program period, and 0 otherwise. The servicer-specific renegotiation intensity in the pre-program period is obtained as in Figure 6(b).

We start by using a specification using loans in the treatment group (as defined by Strategy 1), where the dependent variable takes the value of 1 if a loan has received a given HAMP modification and 0 otherwise. Columns (1) and (2) (Columns (3) and (4)) of Panel A of Table 7 show that loans serviced by servicers that did more renegotiations in the pre-program period are much more likely to receive a trial (permanent) HAMP modification: the corresponding likelihood is bigger by more than 1% (0.98%). These are large effects, since they suggest an increase of about 58% (117%) relative to the overall mean trial (permanent) HAMP modification rate for low-experience servicers in our sample period. It is worth reiterating that in these specifications we control for all the observable collateral characteristics (FICO, LTV, interest rates), loan ownership status (securitized or bank held), and for geography (state fixed effects).

We further assess the robustness of this finding by restricting our attention to treatment loans in California and Florida, respectively. Focusing on loans in a specific state allows us to better control for local economic conditions and variation in state laws. Moreover, we also account for regional effects within these states by including zip code fixed effects corresponding to property location in these specifications. The results are presented in Columns (5) and (6) of Panel A in Table 7, where the standard errors are clustered at the zip code level. Again, even with more refined controls for geography we find that servicers with high pre-program renegotiation experience perform many more permanent HAMP modifications. Strikingly, the permanent HAMP modification rate among loans in California (Florida) during the program period is about 2.4% (1.7%) higher for high-experience servicers. This amounts to about a 180% (126%) higher rate relative to the mean modification rate for servicers classified as having low experience.

Finally, as another robustness check, we estimate the specification restricting our attention to treatment loans classified according to Strategy 2. Recall that this sample consists of better-quality mortgages given to owner-occupants with similar loan balances. Consistent with our earlier results, Column (7) of Panel A shows that high-experience servicers are much more likely to offer permanent HAMP modification: the corresponding likelihood is higher by 1.73% in absolute terms.³⁰ We find similar effects if we cluster at the level of servicers in the regressions that are presented (unreported for brevity). Overall, our results show that the persistent lower renegotiation activity of some servicers—both before and during the program—cannot be

³⁰ We also estimated specifications in Columns (5)–(7) of Panel A (Table 7) for trial HAMP modifications. The findings and inferences are similar to those for permanent modifications (unreported for brevity).

accounted by the heterogeneity in observable characteristics of loans in their servicing portfolios.³¹

For completeness, we also examine foreclosure decisions across servicers. We find evidence that the foreclosure rates are measurably lower for high-experience servicers relative to low-experience ones for delinquent loans consistent with their higher renegotiation activity (see Internet Appendix A.8). We also assess the changes in extensive and intensive margins based on servicer experience redoing the analysis similar to Section V.A and V.B. Our results suggest that there is no adverse effect on extensive margin across servicers, but those with lower pre-program renegotiation experience display a much smaller increase in the combined (private and HAMP) permanent modification rate due to the program. Moreover, similar to results in Section V, we find evidence of adverse impact on intensive margin of permanent modifications for both types of servicers (unreported for brevity).

In sum, our findings indicate that there is a strong positive relationship between renegotiation intensity of servicers in the pre-program period and the rate of permanent modifications induced by HAMP across these entities. While contract, borrower, and regional characteristics of mortgages are important determinants of renegotiation activity of a servicer,³² the differential and persistent patterns of renegotiation across servicers cannot be accounted for by these factors. Another possibility that could explain the nature of servicer renegotiation experience in the pre-program period relates to the organizational capability of the servicers. Organizational factors, such as the quality of the workforce, incentives, and technology, have been found to be responsible for differences in productivity across manufacturing firms (Syverson 2010). Recall that the program requires the servicers to verify numerous eligibility criteria regarding the applicant status prior to offering modification. This requires servicers to employ appropriate infrastructure and sufficiently trained staff.³³ Thus, we evaluate whether such organizational differences are related with renegotiation experience of servicers.

In Panel B of Table 7, we relate servicer organizational characteristics with pre-program renegotiation experience and find significant relationships between several variables. We collect information on organizational variables of servicers around the introduction of the program from the residential mortgage servicer reports generated by the three rating agencies (Standard & Poor's, Moody's, and Fitch). We aggregate the servicers affiliated with the same institution—the level at which many of these servicer reports are available—to conduct this analysis. In Column (1), the number of full-time servicing staff is positively correlated with the intensity of

³¹ We also note that each of the servicers in our sample services significant number of loans both issued with and without government guarantees (e.g., on average the high experience servicers have around 50% loans issued with government guarantee while the other servicers have around 62% loans issued with government guarantee). Thus, it is unlikely that the results on high renegotiation activity in the pre-program period persisting into the program period can be explained by some servicers that primarily service (or do not service) GSE loans.

³² For instance, Agarwal et al. (2011) use within servicer variation to show that servicers renegotiate loans they own at a faster rate relative to similar loans that are securitized. Similarly, factors such as credit score of the borrower and loan-to-value of the mortgage are also important determinants of renegotiation rates.

³³ For instance, processing applications for program modifications involves direct contact between servicer and borrower, potentially through a call center, in order to collect relevant information.

renegotiations conducted by the servicer in the pre-program period. Column (2) confirms that servicers that conducted more renegotiations did have less-constrained staff, as measured by loans per full-time employee. Next, in Column (3), we find that servicers with more renegotiation experience also are the ones who devote more hours to training their employees—a proxy for the quality of the servicing staff. Finally, in Columns (4) and (5) we find that servicers who are more efficient in handling the phone queries—as proxied by the lower percentage of calls dropped and the smaller average call holding time—also conducted more renegotiations. These patterns are also visible in Internet Appendix A.9.

Overall, our analysis provides suggestive evidence that the nature of pre-program renegotiation activity conducted by servicers is related to their organizational capabilities. In particular, servicers with higher pre-program renegotiation activity appear to have the specialized skills and infrastructure that is conducive to conducting loan workout. It seems reasonable to conjecture that given these skills and infrastructure before the program, these same servicers were able to extend more modifications under the program.

We end this section by doing a naive counterfactual computation: we compute what the effect of the program would be if the low-experience servicers were to renegotiate the loans at the same rate as their high-experience counterparts. Since 75% of the loans are serviced by low-experience servicers, our estimates imply that HAMP would have induced about 70% more permanent HAMP modifications, if the loans by low-experience servicers were renegotiated at the same rate as their high-experience counterparts. This would translate into about 800,000 more modifications induced by the program tracked until its original end date (December 2012).

VIII. Impact of HAMP on House Prices and Other Outcome Variables

In this section, we explore the impact of the program on regional outcome variables such as house prices, consumption, and delinquency rates on other categories of consumer debt. The goal from this exercise is to inform on the effect of debt relief programs such as HAMP, on broader set of economic outcomes when such programs are implemented intensively. The challenge for using HAMP as an episode to infer such a connection is that, as we have shown, there was a relatively muted response to the program. We circumvent this challenge by using the results from the previous section, and exploiting regional heterogeneity in the share of loans in a region that are serviced by “high-experience” servicers just prior to the program. Because servicer concentration in a region is determined prior to the program and is very persistent over time in the data, we can trace out the effects of HAMP on different economic outcomes using variation in this ex-ante measure of the program exposure.

Exploiting such regional variation in HAMP exposure allows us to assess what the impact of this program on various outcomes was when it was implemented intensively. The idea is to compare the economic outcomes in regions that had high concentration of loans serviced by high-experience servicers before the program—and therefore were also regions more likely to receive HAMP modifications—to otherwise similar regions with a low concentration of loans serviced

by these servicers. This approach is similar to that used by Mian and Sufi (2010) in their study of the effects of the “Cash-for-Clunkers” program.

VIII.A Empirical Design

Our empirical strategy of exploring the impact of the program on regional outcome variables relies on zip code data, because we do not have more micro data for variables like house prices. We confine our analysis to zip codes that have at least 250 mortgages in the OCC database, and this leaves us with a sample of about 10,000 zip codes. Imposing this restriction, which allows for reliable estimates, does not change the sample composition much—for instance, the mean share of loans serviced by high-experience servicers in a zip code in the restricted sample is very close to the overall share of these loans in an entire data set (roughly 25%).

We first verify that our *ex ante* measure of regional HAMP exposure based on a share of loans serviced by high-experience servicers in a zip code before the program indeed correlates with the subsequent treatment from the program. We note that servicer concentration in a region is very persistent over time, with around 95% of loans continuing to have the same servicer that handled these loans at their origination. In Column (1) of Panel A of Table 8, we present the results of a regression in which the dependent variable is a fraction of modified loans under HAMP in a zip code during 2009:Q2 and 2010:Q4, and the explanatory variable is a fraction of loans serviced by institutions classified as high experience in a zip code as of March 2009 (*High Experience Share*). As we observe, there is a strong positive association between the fraction of HAMP-modified loans and the share of loans serviced by high-experience servicers in a zip code. A one-standard-deviation increase in the high experience share (about 25% relative increase) is associated with a 0.12% absolute increase in the fraction of HAMP-modified loans in a zip code (around a 13% increase in relative terms with respect to the mean zip code fraction of HAMP-modified loans). This is consistent with our results from the loan-level analysis (Section VI) and demonstrates that zip codes with a larger *ex ante* measure of the program exposure—a higher share of loans serviced by high-experience servicers as of March 2009—did subsequently receive more treatment *ex post* (2009:Q2–2010:Q4).

In our main analysis, we want to compare regions with large differences in how intensively the program was implemented. We use share of loans serviced by high experience servicers in a zip code before the program to generate such variation. More specifically, we construct such regions by restricting our sample to zip codes in the top quartile (high exposure group) and bottom quartile (low exposure group) in terms of a share of loans in the zip code serviced by high-experience servicers as of March 2009. While these regions provide us with significant variation in program exposure -- and consequently in the intensity of program implementation -- they may differ on several dimensions such as collateral quality being serviced. Accordingly, we need to make sure that we focus on zip codes that are otherwise as similar as possible. We do so by selecting regions from the high and low exposure groups using propensity score matching. In particular, we construct the nearest neighbor-matched sample of control zip codes based on the Mahalanobis distance metric. This approach employs a large set of matching covariates,

including zip-code-level averages of the FICO score of borrowers, interest rates, LTV, and delinquency rates in the pre-program period (2008:Q3 to 2009:Q1). We end up with 990 zip codes, equally split between high and low exposure groups, after this matching exercise.

Figure 7 presents the time series evolution of characteristics of matched high and low exposure zip codes. The mean FICO score, interest rates, LTV, and fraction of loans transitioning from current to 60-day delinquency are close to each other across these group of zip codes and follow a similar pattern in the pre-program period. Internet Appendix A.10(a) presents private modifications in the two groups around the program. Since the high exposure group has a higher proportion of loans serviced by high-experience servicers, consistent with evidence in Section VII, the level of private modifications is higher in this group. However, more importantly, there is constant difference in the private modification rates between the high and low exposure zip codes in the pre-program period. Notably, at the same time, the percentage of loans serviced by high experience intermediaries in a zip code ranges from more than 50% in the high exposure group to 6% in the low exposure group.

Columns (2) and (3) reveal that the strong positive association between the fraction of HAMP-modified loans and the share of loans serviced by high-experience servicers in a zip code is also found in the matched sample. In this sample, a one-standard-deviation increase in the high experience share (about 33% relative increase) is associated with a 0.23% absolute increase in the fraction of HAMP-modified loans in a zip code (about 16% relative increase with respect to the mean fraction of HAMP-modified loans in a zip code). Internet Appendix A.10(b) confirms this inference by plotting the average combined permanent program and private modifications in the matched sample of high and low exposure zip codes. It shows that there is a sizeable increase in the permanent modifications in the high exposure group relative to the low exposure one during the program period. Notably, when combined with patterns in Internet Appendix A.10(a), it is clear that the differential increase in the rate of permanent modifications in the high exposure zip codes during the program period is driven by more intensive program implementation in these zip codes.

VIII.B Impact on Foreclosures and House Prices

We start by analyzing how the quarterly rate of foreclosures varies with program exposure in regions with large differences in how intensively the program was implemented. To do so, we use our matched sample and estimate a regression with change in the zip code quarterly rate of foreclosures between the program and pre-program period as the dependent variable, and *High Experience Share* as the explanatory variable. As we observe from Column (1) of Panel B of Table 8, zip codes with a larger high experience share saw a more decline in the foreclosure rate. The estimates suggest that a one-standard-deviation increase in high-experience share (33% relative increase) is associated with about 0.07% decline in a quarterly foreclosure rate (about 18% decrease relative to the mean foreclosure rate in the pre-program period). Column (2) shows that these results are robust to including controls. These results are similar in spirit to those obtained with loan-level analysis in Section V.

Next, we examine the differences in house price growth in regions classified on the basis of their exposure to the program. Several recent papers argue that foreclosures create downward pressure on house prices (Campbell et al. 2010; Mian, Sufi, and Trebbi 2011). Accordingly, we are interested in examining if regions with more exposure to HAMP—which are also the regions that experienced relative decline in foreclosure rates in the program period—saw an increase in house prices relative to regions with limited program exposure.

Similar to analysis with foreclosure rates, we use the matched sample and estimate a regression with a change in the quarterly house price growth between the program and pre-program period as the dependent variable and *High Experience Share* as the explanatory variable. The estimate in Panel B of Table 8 (Column (3)) indicates that zip codes with a larger high experience share saw an increase in the growth rate of house prices. In particular, a one-standard-deviation increase in the high-experience share is associated with about a 0.45% relative increase in the quarterly growth rate of house prices. As Column (4) shows, this estimate is robust to adding zip-code-level controls.

An alternative way to illustrate these findings is to exploit only the differences between high and low exposure regions. Figure 8(a) plots the mean quarterly house price growth in high and low exposure zip codes. The zip-code-level house price data come from CoreLogic. While the difference between low and high exposure zip codes remains relatively stable before the program announcement, the gap between these groups grows from mid-2009. In other words, zip codes with significant exposure to the program saw a meaningful relative increase in house prices after the program's introduction, at least in the near term. Moreover, the increase in the growth rate of house prices in the high exposure group during the program period broadly coincides with the timing and intensity of program modifications, including trial ones (see Figure 3(a)).

It is, of course, possible that part of this house price change reflects a change in the composition of transacted properties due to the relative lower intensity of foreclosure sales in the high exposure zip codes relative to low exposure ones. To assess the robustness of our results to this concern, we repeat this exercise using the CoreLogic house price index, which excludes distressed transactions. The estimates using this series are presented in Columns (5) and (6) of Panel B of Table 8. Our inferences remain unchanged. Figure 8(b) demonstrates the same results between high and low exposure zip codes graphically.

Finally, note that the timing of the results on house prices compare well with an uptick in trial and permanent program modifications in the high exposure group of zip codes relative to low exposure ones. As Internet Appendix 10.(b) shows there is an increase in permanent modifications due to the program in the high exposure group relative to low exposure one from the beginning of 2010. A similar plot for trial modifications shows an uptick six months earlier (unreported for brevity).

VIII.C Impact on Consumption and Delinquencies on Other Consumer Credit

Next, we investigate the impact of HAMP on other outcome variables, such as durable (e.g., growth rates of auto sales) and non-durable consumption and delinquencies on other consumer credit. The motivation for looking at these variables follows from arguments made by proponents of household debt relief programs that suggest that lowering household debt level during the crisis may help alleviate distortions in consumption and investment decisions of households.

In Figure 8(c), we plot the time-series evolution of growth rates in new auto sales in high and low exposure zip codes. We first note that there is no differential change in the growth rate of auto sales between these groups in the close vicinity of the program announcement. This provides additional evidence that the zip codes in our matched sample faced similar economic conditions around the introduction of the program—yielding further support to our empirical design. Importantly, Figure 8(c) shows no discernible change in auto sales in high exposure zip codes relative to low exposure ones during the program period. Panel C of Table 8 (Columns (1) and (2)) confirms this inference in a regression setting.

Similarly, columns (3) and (4) of Panel C of Table 8 reveal no statistical relation between growth rate of consumer spending on non-durables -- i.e., groceries including food and beverages, drugs, health and beauty care and general merchandise -- and high experience share. This is also corroborated in Internet Appendix A.11 where we plot the quarterly growth rates of overall non-durable spending as well as when it is broken into groceries and non-grocery categories in the high and low exposure zip codes. We also note that our results on durable and non-durable consumption are not likely due to potentially low power induced by our empirical strategy since we do find significant effects for house-prices, foreclosures and, in what follows, delinquencies on other consumer debt.

Figure 8(d) plots the time-series evolution of change in the quarterly delinquency rate on consumer credit in the high and low exposure zip codes. This figure suggests that there was a meaningful relative decline in the delinquency rates of consumer credit in zip codes with high program exposure. Panel C of Table 8 (Columns (5) and (6)) confirms these results in a regression setting. The estimates in the table suggest that a one-standard-deviation increase in high-experience share in a zip code is associated, on average, with about a 0.23% decrease in the zip code quarterly delinquency rate on consumer debt in the program period relative to the pre-program period. In additional tests, we separate consumer credit in separate categories such as home equity line of credit (Columns (7) and (8)), and auto loans and credit cards (unreported for brevity), and find similar significant effects in these categories.

VIII.D Implications

A number of insights emerge from the analysis in this section. First, our evidence suggests that mortgage debt relief programs, when used with sufficient intensity, may have a meaningful impact on foreclosure rates, delinquencies on non-targeted consumer debt, and house prices. In particular, recall that on average a permanent HAMP modification resulted in about 20%

reduction in payments in our sample, a saving in the order of \$350 per month. Hence our estimates from Table 8 imply that a one-standard-deviation increase in the high-experience share—which would translate into such reduction of payments for 16% more borrowers in relative terms during the program period—would be associated with about 1.8% annual increase in house prices and about 1% annual decrease in consumer debt delinquencies. In this respect, this evidence supports the recent studies that show that establish a link between foreclosures and house prices (e.g., Campbell et al. 2010; Mian, Sufi, and Trebbi 2011).

Second, and more important, our results also suggest that such programs targeted at distressed borrowers may not necessarily result in a sizeable increase in consumption, at least in the near term. As we saw, distressed borrowers who found mortgages to be more affordable after HAMP renegotiation did not significantly alter their consumption patterns. Instead, these borrowers used the additional cash to service and pay down their consumer debt. Thus, the results in our paper suggest that distressed borrowers who are in the process of debt deleveraging may have a relatively low spending multiplier from moderate debt reduction, at least in the near term. These findings are consistent with arguments and empirical evidence in Mian, Sufi, and Rao (2011) and corroborates their view that large accumulation of household debt prior to the crisis is an important factor adversely affecting household consumption.

It is important to stress that our findings do not imply that a widespread debt relief program would not stimulate household consumption. It is possible that the program targeting the general population instead of a select group of distressed borrowers could have such consequences. Moreover, there may be a more pronounced impact on consumption at longer horizons than what we study in the paper. Despite these caveats, our findings provide valuable guidance on what the effect of debt relief programs on broader economic outcomes might be, were such programs implemented intensively.

IX. Conclusion

We find that renegotiations induced by HAMP and its effects will fall significantly short (two-thirds) of the target of this intervention. This is mostly because a few large servicers, with pre-program organizational design that was less conducive to conducting renegotiations, responded at half the rate of others. The muted response of these servicers cannot be accounted for by differences in contract, borrower, or regional characteristics of mortgages across servicers. The fact that some other servicers with similar portfolios of distressed loans actively conducted modifications under the program suggests that the incentive structure of the program may not have been inadequate per se. Rather, the program failed to account for firm level factors that inhibited the response of some servicers. The presence of these factors—and the lack of understanding of their specific nature—poses a significant challenge to the ability of the government to quickly influence such intermediaries through provision of financial incentives, thus hampering policies that require voluntary participation of such firms. This lesson is not only applicable to HAMP but may also apply to other initiatives undertaken by the administration in

response to the foreclosure crisis. For example, effective policies aimed at reducing the cost of debt through mortgage refinancing, such as HARP or quantitative easing initiatives, require significant refinancing activity by intermediaries. Our paper suggests that such policies may also face significant hurdles due to the limited organizational capabilities of some financial intermediaries.

Our findings also suggest that the reallocation of resources that could promote more effective implementation of the program—for example, through private contracting to allow the transfer of distressed mortgages to more efficient servicers, similar to provisions that exist in the commercial real estate sector, or through the entry of better and more capable servicers—must have faced significant hurdles. Figuring out what these challenges that prevent reallocation of resources are, especially in times of crisis, is an interesting avenue for future research.

Our results also provide guidance for designing large-scale renegotiation programs in the future. In particular, our evidence suggests that HAMP did not lead to widespread strategic defaults, likely because of the extensive screening related to its eligibility criteria and its design of incentives for servicers. However, these factors may also have stalled the pace of the program. For example, verification of extensive eligibility criteria may have been challenging for servicers with less renegotiation experience, contributing to their low response to the program. These findings can be compared to the results from a simple modification program that employed only serious delinquency as its main eligibility criterion, as studied in Mayer et al. (2011), which led to significant strategic behavior. Consequently, there is a likely tradeoff between screening more intensively to reduce strategic behavior, which limits the unintended effects of the program, and the reach and pace of the program.

Finally, because incentive payments were triggered only by permanent HAMP modifications, one could use the ratio of estimated permanent modifications induced by the program to foreclosures prevented in assessing the program's success.³⁴ Admittedly, this would be a very naive computation, since it ignores other costs (or benefits) of program implementation, as well as any aggregate or redistributive effects in the economy. Likewise, such a computation would not account for the potential impact of the program on the behavior of borrowers and lenders in the future or whether these foreclosures would be prevented in the longer term. As a result, we refrain from this exercise. More generally, in the absence of a model of what optimal level of renegotiations and foreclosures should be, we cannot determine whether HAMP helped correct a “market failure.” Devising such a model is a fruitful area of future research.

³⁴ In particular, our results suggest that for every ten permanent modifications induced by the program there are about seven fewer foreclosures. One could potentially compute the benefit of the program based on studies that quantify the deadweight losses of foreclosures. Such benefits could be compared with the direct cost of providing incentives for the additional HAMP modification (around \$4,500 per modification).

References

- Agarwal, Sumit, Chunlin Liu, and Nicholas S. Souleles, 2007. The Reaction of Consumer Spending and Debt to Tax Rebates-Evidence from Consumer Credit Data, *Journal of Political Economy* 115, 986-1019.
- Agarwal, Sumit, Gene Amromin, Itzhak Ben-David, Souphala Chomsisengphet, Douglas D. Evanoff, 2011, The Role of Securitization in Mortgage Renegotiation, *Journal of Financial Economics*, 102(3), 559-578.
- Alston, Lee J., 1984, Farm Foreclosure Moratorium Legislation: A Lesson from the Past, *American Economic Review* 74, 445-457.
- Alston, Lee J., 1983, Farm Foreclosures in the United States During the Interwar Period, *Journal of Economic History*, 43, 885-903.
- Auerbach, Alan, and Yuriy Gorodnichenko, 2011, Fiscal Multipliers in Recession and Expansion, in *Fiscal Policy after the Financial Crisis*, University of Chicago Press.
- Barro, Robert J., 1989, The Ricardian Approach to Budget Deficits, *Journal of Economic Perspectives* 3, 37-54.
- Becker, Gary, 2009, On the Obama Mortgage Plan, Becker-Posner Blog.
- Bolton, Patrick, and Howard Rosenthal, 2002, Political Intervention in Debt Contracts, *Journal of Political Economy*, 110 (5), 1103-1134.
- Burnside Craig, Martin Eichenbaum, and Sergio Rebelo, 2011, Understanding Boom and Busts in Housing Markets, NBER Working Paper 16734.
- Campbell, John Y., Stefano Giglio, and Parag Pathak, 2011, Forced Sales and House Prices, forthcoming, *American Economic Review* 101, 2108-2131.
- Christiano, Lawrence, Martin Eichenbaum, and Sergio Rebelo, 2009. When Is the Government Spending Multiplier Large? *Journal of Political Economy*.
- Demyanyk, Yuliya, and Otto Van Hemert, 2011, Understanding the Subprime Mortgage Crisis, *Review of Financial Studies* 24, 1848-1880.
- Dinardo, John, and Jack Johnston, 1996, *Econometric Methods*, 4th edition, McGraw-Hill.
- Favilukis, Jack, Sydney C. Ludvigson, and Stijn Van Nieuwerburgh, 2010, The Macroeconomic Effects of Housing Wealth, Housing Finance, and Limited Risk-Sharing in General Equilibrium, NBER Working Paper 15988.
- Glaeser, Edward, Joseph Gyourko and Albert Siaz, 2008, Housing Supply and Housing Bubbles, *Journal of Urban Economics*.
- Guiso, Luigi, Paola Sapienza and Luigi Zingales, 2011, The Determinants of Attitudes towards Strategic Default on Mortgages, *Journal of Finance*, forthcoming.
- Gyourko, Joseph, Fernando Ferreira, and Joseph Tracy, 2009, Housing busts and household mobility, *Journal of Urban Economics* 68, 34-45.
- Gyourko, Joseph, and Fernando Ferreira, 2011, Anatomy of the Beginning of the Housing Boom: U.S. Neighborhoods and Metropolitan Areas, working paper.

Haughwout, A., E. Okah, and J. Tracy, 2010, Second Chances: Subprime Mortgage Modification and Re-Default, NY FED working paper.

Haughwout, A., Donghoon Lee, Joseph Tracy, and Wilbert van der Klaauw, 2011, Real Estate Investors, the Leverage Cycle, and the Housing Market Crisis, *Federal Reserve Bank of New York Staff Reports*, 514.

Johnson, David S., Jonathan A. Parker, and Nicholas S. Souleles, 2006. Household Expenditure and the Income Tax Rebates of 2001, *American Economic Review* 96, 1589-1610.

Keys, Benjamin J., Tanmoy Mukherjee, Amit Seru, and Vikrant Vig, 2010, Did Securitization Lead to Lax Screening: Evidence from Subprime Loans, *Quarterly Journal of Economics* 125, 307-362.

Keys, Benjamin J., Amit Seru, and Vikrant Vig, 2011, Lender Screening and Role of Securitization: Evidence from Prime and Subprime Mortgage Markets, *Review of Financial Studies*, forthcoming.

Kroszner, Randall S., 1998, Is It Better to Forgive than to Receive? Repudiation of the Gold Indexation Clause in Long-Term Debt During the Great Depression, Working paper.

Landvoigt, Tim, Monika Piazzesi, and Martin Schneider, 2012, The Housing Market(s) of San Diego, NBER Working Paper 17723.

Mayer, Christopher, Karen Pence, and Shane Sherlund, 2009, The Rise in Mortgage Defaults, *Journal of Economic Perspectives* 23, 27-50.

Mayer, Christopher, Edward Morrison, Tomasz Piskorski, and Arpit Gupta, 2011, Mortgage Modification and Strategic Behavior: Evidence from a Legal Settlement with Countrywide, NBER Working Paper 17065.

Meyer, Bruce D., 1995, Natural and Quasi-Experiments in Economics, *Journal of Business and Economic Statistics* 13, 151-161.

Melzer, Brian, 2010, Mortgage Debt Overhang: Reduced Investment by Homeowners with Negative Equity, Working Paper.

Mian, Atif, and Amir Sufi, 2009, The Consequences of Mortgage Credit Expansion: Evidence from the U.S. Mortgage Default Crisis, *Quarterly Journal of Economics* 124, 1449-1496.

Mian, Atif, and Amir Sufi, 2010, The Effects of Fiscal Stimulus: Evidence from the 2009 'Cash for Clunkers' Program, *Quarterly Journal of Economics*, forthcoming.

Mian, Atif, Amir Sufi, and Francesco Trebbi, 2011, Foreclosures, House Prices, and the Real Economy, University of Chicago, Working Paper.

Mian, Atif, Kamalesh Rao, and Amir Sufi, 2011, Household Balance Sheets, Consumption, and the Economic Slump, Working Paper.

Mian, Atif, and Amir Sufi, 2012, What Explains High Unemployment? The Aggregate Demand Channel, NBER Working Paper 17830.

Nakamura, Emi, and Jon Steinsson, 2012, Fiscal Stimulus in a Monetary Union: Evidence from US Regions, Working Paper.

Office of the Comptroller of the Currency and Office of Thrift Supervision, 2009, OCC and OTS Mortgage Metrics Report, Quarterly reports.

- Parker, Jonathan A., Nicholas S. Souleles, David S. Johnson, and Robert McClelland, 2011, Consumer Spending and the Economic Stimulus Payments of 2008, NBER Working Papers 16684.
- Parker, Jonathan, 2011, On Measuring the Effects of Fiscal Policy in Recessions, *Journal of Economic Literature*, 49, 703-718.
- Philippon, Thomas, and Virgiliu Midrigan, 2011, Household Leverage and the Recession, NBER Working Paper 16965.
- Piazzesi, Monika, and Martin Schneider, 2009, Momentum Traders in the Housing Market: Survey Evidence and a Search Model, *American Economic Review* 99, 406-411.
- Piskorski, Tomasz, Amit Seru, and Vikrant Vig, 2010, Securitization and Distressed Loan Renegotiation: Evidence from the Subprime Mortgage Crisis, *Journal of Financial Economics* 97(3), 369-397.
- Piskorski, Tomasz, and Alexei Tchisty, 2011, Stochastic House Appreciation and Optimal Mortgage Lending, *Review of Financial Studies* 24, 1407-1446.
- Posner, Richard, 2009, The President's Plan for Mortgage Relief, Becker-Posner Blog.
- Posner, Richard, and Luigi Zingales, 2009, A Loan Modification Approach to the Housing Crisis, *American Law and Economics Review* 11, 575-607.
- Rajan, Uday, Amit Seru, and Vikrant Vig, 2010, The Failure of Models That Predict Failure: Distance, Incentives and Defaults, University of Chicago, Working Paper.
- Ramey, Valerie A. 2011. Can Government Purchases Stimulate the Economy? *Journal of Economic Literature*, 49(3), 673-85.
- Rucker, Randal R., and Lee J. Alston, 1987, Farm Failures and Government Intervention: A Case Study of the 1930's, *American Economic Review* 77, 724-730.
- Saiz, Albert, The Geographic Determinants of Housing Supply, *Quarterly Journal of Economics*, 125, 1253-1296.
- Syverson, Chad, 2010, What Determines Productivity?, *Journal of Economic Literature*, 326-365.

Table 1: Summary Statistics for Control and Treatment Group in the Pre-Program Period

This table presents summary statistics of key variables in the pre-HAMP period (2008: Q3 to 2009: Q1) in the treatment and control groups formed on the basis of Strategy 1 and the trial and permanent HAMP quarterly modification rates in the treatment group during the program period (2009: Q2 to 2010: Q4). The treatment group consists of loans with owner-occupied status and with outstanding balance below \$729,750, while the control group consists of loans with non-owner-occupied (investor) status.

<i>Pre-program period:</i>	<i>Control</i>		<i>Treatment</i>	
	Mean	SD	Mean	SD
FICO	717	1.0	710	1.0
LTV %	70.3	0.4	70.6	0.2
Interest rate %	6.1	0.1	6.2	0.1
60+ delinquency % [Quarterly]	1.7	0.2	1.6	0.2
Private Permanent modifications % (all loans) [Quarterly]	0.3	0.1	0.4	0.2
Foreclosure complete % (all loans) [Quarterly]	0.4	0.001	0.3	0.001
Foreclosure complete % (delinquent loans) [Quarterly]	2.6	2.0	1.6	1.0
Number of loans as of March 2009	3,005,537		17,778,672	
<i>Program period:</i>	<i>Trial HAMP Modifications</i>			
Modification rate (%) [Quarterly]	0.432			
Number of Trial HAMP modification	522,365			
	<i>Permanent HAMP Modifications</i>			
Modification rate (%) [Quarterly]	0.165			
Number of Permanent HAMP modification	199,515			
Conversion Rate: Trial to Permanent HAMP	38.2%			

Table 2: Trial and Permanent HAMP Modifications: Relation with Borrower and Contract Characteristics

The table presents OLS estimates from regressions that relate whether or not a trial or a permanent HAMP modification was offered to a loan and various borrower and contract-level characteristics. The sample includes loans that are eligible for HAMP based on owner-occupancy status (Strategy 1). In Columns (1)–(2) the dependent variable takes the value of 1 if a given loan received a trial HAMP modification during the program period (2009:Q2–2010:Q4) and is 0 otherwise. In Columns (3)–(4) the dependent variable takes the value of 1 if a given loan received a permanent HAMP modification during the program period and is 0 otherwise. *FICO* is the borrower’s credit score at loan origination. *LTV* is the loan origination loan-to-value ratio. *Interest Rate* is the loan interest rate in percentage terms. *Origination Amount* is the loan initial balance (in thousands of dollars). *Low Doc* is the dummy that takes value of 1 if a loan was originated with limited documentation and is 0 otherwise. *Other Controls* include origination variables such as loan type (ARM, option ARM) and the loan ownership status. *Origination FE* includes loan origination year fixed effects, while *Servicer FE* includes loan servicers fixed effects. *State FE* includes fixed effects for the location (state) of the property. Standard errors are clustered at the state level; *t*-statistics are in parentheses. The estimates are expressed in percentage terms (e.g., -0.02 estimate reported for FICO in Column (1) means that an increase of FICO by 1 is associated with a 0.02% absolute decrease in the likelihood of loan receiving a HAMP trial modification).

	Dependent variable: Whether a loan gets a trial HAMP during the program period		Dependent variable: Whether a loan gets a permanent HAMP during the program period	
	(1)	(2)	(3)	(4)
FICO	-0.02 (9.70)	-0.021 (9.42)	-0.008 (7.80)	-0.008 (7.58)
LTV	1.167 (4.29)	2.397 (4.14)	0.426 (4.15)	0.954 (3.92)
Interest Rate	0.356 (3.64)	0.385 (4.67)	0.069 (1.61)	0.081 (2.27)
Origination Amount	0.003 (4.56)	0.001 (1.07)	0.001 (5.38)	0.0004 (1.15)
Low Doc	0.781 (11.44)	0.720 (11.30)	0.326 (6.67)	0.303 (5.87)
Observations	17,273,971	17,273,971	17,273,971	17,273,971
Adj. R-square	0.039	0.041	0.016	0.018
Other Controls	Yes	Yes	Yes	Yes
Origination FE	Yes	Yes	Yes	Yes
Servicer FE	No	Yes	No	Yes

Table 3: Rate of Permanent Modifications

The table presents OLS estimates from regressions that track whether or not a permanent modification is offered to a loan around the program implementation. The dependent variable takes the value of 1 in the quarter a given loan receives a modification for the first time and is 0 otherwise. The modified loans exit the estimation sample. Panel A shows the results for all loans. The variable *T* takes the value of 1 if a loan belongs to the treatment group as defined by Strategy 1 (owner-occupied loan) and is 0 otherwise. The variable *After* takes the value of 1 for the quarters after Q1 2009 and is 0 otherwise. *Other Controls* include origination variables such as FICO credit score, LTV, interest rates and their squares, loan documentation status, loan type (ARM, option ARM), and loan ownership status. *Origination FE* includes loan origination year fixed effects, while *Servicer FE* includes loan servicers fixed effects. *State FE* includes fixed effects for the location (state) of the property. Estimation period is 2008:Q3–2010:Q4. Standard errors are clustered at the state level; *t*-statistics are in parentheses. The estimates are expressed in percentage terms.

Panel A: Rate of Permanent Modifications: All Loans

	Dependent variable: Whether a loan gets a private permanent modification in a quarter			Dependent variable: Whether a loan gets a combined permanent modification (private and HAMP) in a quarter
	(1)	(2)	(3)	(4)
T	0.190 (8.39)	0.213 (8.40)	0.209 (6.43)	0.205 (7.21)
T* After	0.014 (1.3)	0.021 (1.27)	0.020 (1.27)	0.144 (5.40)
After	0.471 (12.57)	0.454 (13.03)	0.453 (13.33)	0.492 (12.11)
Observations	175,910,892	175,910,892	175,910,892	175,166,092
Adj. R-square	0.012	0.013	0.014	0.015
Other Controls	Yes	Yes	Yes	Yes
Origination FE	Yes	Yes	Yes	Yes
Servicer FE	No	Yes	Yes	Yes
State FE	No	No	Yes	Yes

Table 3: Rate of Permanent Modifications (contd.)

The table presents OLS estimates from regressions that track whether or not a permanent modification is offered to a loan around the program implementation. The dependent variable takes the value of 1 in the quarter a given loan receives a modification for the first time and is 0 otherwise. The modified loans exit the estimation sample. Panel B shows the results in a sample of loans issued without guarantees from the government-sponsored entities (non-agency) and a sample of loans issued with such guarantees (agency loans), respectively. The variable *T* takes the value of 1 if a loan belongs to the treatment group as defined by Strategy 1 (owner-occupied loan) and is 0 otherwise. The variable *After* takes the value of 1 for the quarters after Q1 2009 and is 0 otherwise. *Other Controls* include origination variables such as FICO credit score, LTV, interest rates and their squares, loan documentation status, loan type (ARM, option ARM), and loan ownership status. *Origination FE* includes loan origination year fixed effects, while *Servicer FE* includes loan servicers fixed effects. *State FE* includes fixed effects for the location (state) of the property. Estimation period is 2008:Q3–2010:Q4. Standard errors are clustered at the state level; *t*-statistics are in parentheses. The estimates are expressed in percentage terms.

Panel B: Rate of Permanent Modifications: Agency and Non-Agency Loans

	Dependent variable: Whether a loan gets a private permanent modification in a quarter		Dependent variable: Whether a loan gets a combined permanent modification (private and HAMP) in a quarter	
	Agency	Non-Agency	Agency	Non-Agency
	(1)	(2)	(3)	(4)
T	0.107 (7.04)	0.367 (6.44)	0.112 (7.16)	0.381 (6.16)
T* After	0.001 (1.02)	0.056 (0.74)	0.134 (4.92)	0.151 (4.38)
After	0.473 (12.04)	0.178 (8.07)	0.492 (11.59)	0.181 (8.89)
Observations	110,306,351	41,653,494	109,964,456	41,245,687
Adj. R-square	0.06	0.013	0.08	0.016
Other Controls	Yes	Yes	Yes	Yes
Origination FE	Yes	Yes	Yes	Yes
Servicer FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Table 4: Composition of Modifications and Redefault Conditional on a Modification

The table presents OLS estimates from regressions that track the composition of modifications and redefault rate conditional on a loan having received a modification around the program implementation. The sample consists of permanently modified loans. In Panel A, we assess the composition of modifications. In Column (1) the dependent variable takes the value of 1 if a given loan modification includes rate reduction and is 0 otherwise. In Column (2) the dependent variable takes the value of 1 if a given loan modification includes term extension and is 0 otherwise. In Column (3) the dependent variable takes the value of 1 if a given loan modification includes principal write-down and is 0 otherwise. In Column (4) the dependent variable takes the value of 1 if a given loan modification includes interest rate capitalization. The variable *T* takes the value of 1 if a loan belongs to the treatment group as defined by Strategy 1 (owner-occupied loan) and is 0 otherwise. The variable *After* takes the value of 1 if the modification took place after Q1 2009 and is 0 otherwise. *Other Controls* include origination variables such as FICO credit score, LTV, interest rates and their squares, loan documentation status, loan type (ARM, option ARM), and loan ownership status. *Origination FE* includes loan origination fixed effects, while *Servicer FE* includes loan servicers fixed effects. *State FE* includes fixed effects for the location (state) of the property. Estimation period is 2008:Q3–2010:Q4. Standard errors are clustered at the state level; *t*-statistics are in the parentheses. The estimates are expressed in percentage terms.

Panel A: Composition of Modifications

	Dependent variable: Whether a modified loan in a quarter gets a rate reduction	Dependent variable: Whether a modified loan in a quarter gets a term extension	Dependent variable: Whether a modified loan in a quarter gets a principal write-down	Dependent variable: Whether a modified loan in a quarter gets a capitalization
	(1)	(2)	(3)	(4)
T	10.04 (4.40)	1.23 (0.70)	1.12 (2.91)	-7.14 (4.80)
T* After	-11.14 (2.52)	-9.33 (2.22)	-2.16 (3.30)	9.76 (3.61)
After	29.63 (6.81)	17.53 (4.30)	3.73 (3.02)	8.18 (3.01)
Observations	1,198,049	1,198,049	1,198,049	1,198,049
R-square	0.165	0.245	0.656	0.239
Other Controls	Yes	Yes	Yes	Yes
Origination FE	Yes	Yes	Yes	Yes
Servicer FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Table 4: Composition of Modifications and Redefault Conditional on a Modification (contd.)

The table presents OLS estimates from regressions that track the composition of modifications and redefault rate conditional on a loan having received a modification around the program implementation. The sample consists of permanently modified loans. In Panel B, we assess the change in redefault rate conditional on a loan having received a modification. The dependent variable takes the value of 1 if a loan status becomes 60 days past due or worse on payments in the first six months after modification and is 0 otherwise. The variable *T* takes the value of 1 if a loan belongs to the treatment group as defined by Strategy 1 (owner-occupied loan) and is 0 otherwise. The variable *After* takes the value of 1 if the modification took place after Q1 2009 and is 0 otherwise. *Other Controls* include origination variables such as FICO credit score, LTV, interest rates and their squares, loan documentation status, loan type (ARM, option ARM), and loan ownership status. *Origination FE* includes loan origination fixed effects, while *Servicer FE* includes loan servicers fixed effects. *State FE* includes fixed effects for the location (state) of the property. Estimation period is 2008:Q3–2010:Q4. Standard errors are clustered at the state level; *t*-statistics are in the parentheses. The estimates are expressed in percentage terms.

Panel B: Redefault Conditional on Modification

Dependent variable: Whether a modified loan redefaults within six months after receiving a modification					
	(1)	(2)	(3)	(4)	(5)
T	0.15 (1.21)	0.39 (2.02)	0.42 (2.11)	0.49 (3.13)	0.46 (3.24)
T* After	-0.02 (0.21)	-0.14 (1.02)	-0.18 (1.31)	-0.35 (2.30)	-0.35 (2.30)
After	-0.21 (1.31)	-0.05 (0.21)	0.04 (0.13)	0.28 (1.30)	0.27 (1.30)
Observations	1,064,296	921,871	921,871	921,871	921,871
R-square	0.0001	0.0023	0.0023	0.0051	0.0061
Other Controls	No	Yes	Yes	Yes	Yes
Origination FE	No	No	Yes	Yes	Yes
Servicer FE	No	No	No	Yes	Yes
State FE	No	No	No	No	Yes

Table 5: Foreclosures and Delinquencies

Panel A presents OLS estimates from regressions that analyze whether or not a loan was foreclosed around the program implementation. The dependent variable takes the value of 1 in the quarter that a given loan is foreclosed (completed) and is 0 otherwise. The foreclosed loans exit the estimation sample. The sample consists of all loans in columns (1)–(3) and delinquent loans in columns (4)–(6). The variable *T* takes the value of 1 if a loan belongs to the treatment group as defined by Strategy 1 (owner-occupied loan), and is 0 otherwise. The variable *After* takes the value of 1 for the quarters after Q1 2009 and is 0 otherwise. *Other Controls* include origination variables such as FICO credit score, LTV, interest rates and their squares, loan documentation status, loan type (ARM, option ARM), and loan ownership status. *Origination FE* includes loan origination year fixed effects, while *Servicer FE* includes loan servicers fixed effects. *State FE* includes fixed effects for the location (state) of the property backing the loan. Estimation period is 2008:Q3–2010:Q4. Standard errors are clustered at the state level; *t*-statistics are in parentheses. The estimates are expressed in percentage terms.

Panel A: Foreclosures

Dependent variable: Whether a loan was foreclosed in a quarter						
	Sample: All loans			Sample: Delinquent loans		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>T</i>	-0.164 (5.21)	-0.133 (3.72)	-0.120 (3.55)	-1.32 (2.36)	-0.619 (1.08)	-0.603 (1.43)
<i>T* After</i>	-0.127 (3.13)	-0.126 (3.13)	-0.129 (3.15)	-2.03 (5.39)	-1.92 (5.45)	-1.96 (5.64)
<i>After</i>	0.364 (5.03)	0.372 (5.15)	0.372 (5.17)	3.858 (6.03)	3.808 (6.04)	3.936 (6.63)
Observations	178,917,320	178,917,320	178,917,320	13,658,925	13,658,925	13,658,925
Adj. <i>R</i> -square	0.001	0.001	0.002	0.012	0.014	0.022
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Origination FE	Yes	Yes	Yes	Yes	Yes	Yes
Servicer FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes

Table 5: Foreclosures and Delinquencies (contd.)

Panel B presents OLS estimates from regressions that track whether or not a loan becomes delinquent around the program implementation. The dependent variable takes the value of 1 in the quarter that a given loan transitions for the first time to serious delinquency (60 days past due on payments) and is 0 otherwise. Once a loan reaches a serious delinquency status for the first time, it exits the estimation sample. The variable *T* takes the value of 1 if a loan belongs to the treatment group as defined by Strategy 1 (owner-occupied loan), and is 0 otherwise. The variable *After* takes the value of 1 for the quarters after Q1 2009 and is 0 otherwise. *Other Controls* include origination variables such as FICO credit score, LTV, interest rates and their squares, loan documentation status, loan type (ARM, option ARM), and loan ownership status. *Origination FE* includes loan origination year fixed effects, while *Servicer FE* includes loan servicers fixed effects. *State FE* includes fixed effects for the location (state) of the property backing the loan. Estimation period is 2008:Q3–2010:Q4. Standard errors are clustered at the state level; *t*-statistics are in parentheses. The estimates are expressed in percentage terms.

Panel B: Delinquencies

	Dependent variable: Whether a loan becomes delinquent in a quarter			
	(1)	(2)	(3)	(4)
<i>T</i>	-0.132 (1.33)	0.013 (0.14)	-0.039 (0.32)	-0.02 (0.32)
<i>T*After</i>	0.088 (1.81)	0.035 (0.72)	0.024 (0.52)	0.027 (0.43)
<i>After</i>	-0.245 (2.80)	-0.147 (1.81)	-0.143 (1.70)	-0.171 (1.83)
Observations	179,871,929	179,871,929	179,871,929	179,871,929
Adj. <i>R</i> -squared	0.013	0.014	0.015	0.017
Other Controls	Yes	Yes	Yes	Yes
Origination FE	No	Yes	Yes	Yes
Servicer FE	No	No	Yes	Yes
State FE	No	No	No	Yes

Table 6: Alternative Empirical Strategy: Modifications, Redefault Rates, and Foreclosure Rates

Panel A presents summary statistics of key variables in the pre-HAMP period (2008:Q3 to 2009:Q1) in the treatment and control groups formed using Strategy 2 and the trial and permanent HAMP quarterly modification rates in the treatment group during the program period 2009:Q2 to 2010:Q4. Owner-occupied loans whose amount outstanding is below \$729,750 as of the date of announcement of the program (March 2009) form the treatment group, while owner-occupied loans with the balance above this threshold form the control group. We restrict attention to loans that are within \$100,000 of the threshold.

Panel A: Alternative Strategy: Summary Statistics for Control and Treatment Group

<i>Pre-program period:</i>	<i>Control</i>		<i>Treatment</i>	
	Mean	SD	Mean	SD
FICO	729	0.0	728	1.0
LTV %	64.5	0.1	65.6	0.1
Interest rate %	5.5	0.1	5.5	0.1
60+ delinquency % [Quarterly]	2.4	0.7	2.8	0.7
Private Permanent modifications % (all loans) [Quarterly]	0.6	0.2	0.6	0.2
Foreclosure complete % (all loans) [Quarterly]	0.2	0.1	0.2	0.001
Foreclosure complete % (delinquent loans) [Quarterly]	1.0	0.7	0.8	0.7
Number of loans as of March 2009	62,373		126,717	
<i>Program period:</i>	<i>Trial HAMP Modifications</i>			
Modification rate (%) [Quarterly]	0.565			
Number of Trial HAMP modification	4,489			
	<i>Permanent HAMP Modifications</i>			
Modification rate (%) [Quarterly]	0.226			
Number of Permanent HAMP modification	1,796			
Conversion Rate: Trial to Permanent HAMP	40.04%			

Table 6: Alternative Empirical Strategy: Modifications, Redefault Rates, and Foreclosure Rates (contd.)

Panel B presents OLS estimates from regressions that track whether or not a permanent modification is offered to a loan, redefault rate conditional on receiving a modification, and whether or not the loan was foreclosed, around the program implementation in the treatment and control groups. Owner-occupied loans whose amount outstanding is below \$729,750 as of the date of announcement of the program (March 2009) form the treatment group, while owner-occupied loans with the balance above this threshold form the control group. We restrict attention to loans that are within \$100,000 of the threshold. Column (1) uses the dependent variable that takes the value of 1 in the quarter that a given loan receives the permanent private modification for the first time and is 0 otherwise. The modified loans exit the estimation sample. Column (2) uses the dependent variable that takes the value of 1 in the quarter that a given loan receives the permanent modification (private or HAMP) for the first time and is 0 otherwise. The modified loans exit the estimation sample. Column (3) presents the redefault estimates for the sample of permanently modified loans. The dependent variable takes the value of 1 if a loan status becomes 60 days past due or worse on payments in the first six months after modification and is 0 otherwise. Columns (4) and (5) present the OLS estimates for the sample of all loans (Column 3) and the sample of delinquent loans (Column 4). The dependent variable takes the value of 1 in the quarter that a given loan is foreclosed and is 0 otherwise. The foreclosed loans exit the estimation sample. The variable *T* takes the value of 1 if a loan belongs to the treatment group and is 0 otherwise. The variable *After* takes the value of 1 for the quarters after Q1 2009 and is zero otherwise. *Other Controls* include origination variables such as FICO credit score, LTV, interest rate and their squares, loan documentation status, loan type (ARM, option ARM), and loan ownership status. Origination FE includes loan origination year fixed effects, while *Servicer FE* includes loan servicers fixed effects. Estimation period is 2008:Q3–2010:Q4. Standard errors clustered at the state level; *t*-statistics are in parentheses. Estimates are expressed in percentage terms.

Panel B: Alternative Strategy: Modifications, Redefault Rates, and Foreclosure Rates

	All loans	All loans	Modified loans	All loans	Delinquent loans
	Dependent variable: Whether a loan gets a private permanent modification in given quarter	Dependent variable: Whether a loan gets a combined permanent modification (private and HAMP) in given quarter	Dependent variable: Whether a modified loan redefaults within six months after receiving a modification	Dependent variable: Whether a loan was foreclosed in a quarter	Dependent variable: Whether a loan was foreclosed in a quarter
	(1)	(2)	(3)	(4)	(5)
T	0.005 (0.22)	0.01 (0.43)	-3.54 (2.01)	-0.04 (2.86)	-0.16 (1.21)
T* After	0.06 (1.88)	0.215 (2.34)	1.54 (1.23)	-0.03 (1.67)	-0.59 (3.11)
After	0.64 (2.53)	0.69 (2.61)	-14.64 (7.62)	0.62 (3.13)	3.23 (2.03)
Observations	1,518,352	1,518,352	12,084	1,559,665	194,987
Adj. R-squared	0.012	0.013	0.057	0.008	0.017
Other Controls	Yes	Yes	Yes	Yes	Yes
Origination FE	Yes	Yes	Yes	Yes	Yes
Servicer FE	Yes	Yes	Yes	Yes	Yes

Table 7: Servicer Pre-HAMP Renegotiation Experience and HAMP Renegotiations

Panel A of the table shows the OLS estimates where the dependent variable takes the value 1 if a given loan received a trial HAMP (or permanent HAMP) modification during the program period and is 0 otherwise. Columns (1)–(4) show the results for treatment loans as defined by Strategy 1. Column (5) and Column (6) show the results for treatment loans (as defined by Strategy 1) in California and Florida, respectively. Column (7) shows the results for treatment loans as defined by Strategy 2. The *High Experience* dummy takes the value of 1 if a loan is serviced by a servicer whose estimated renegotiation intensity in the pre-HAMP period is above median and is 0 otherwise. The estimated renegotiation intensity of each servicer is obtained based on servicer fixed effects in a regression similar to column (4) of Table 2, Panel B, but estimated on pre-HAMP data. *Other Controls* include FICO credit score, LTV, interest rates, their squares, loan doc status, loan type (ARM, option ARM), the loan ownership status, and the loan origination year fixed effects. *State FE* includes fixed effects for the location (state) of the property backing the loan. *After* takes a value of 1 for the quarters after Q1 2009 and is 0 otherwise. Estimation period 2008:Q3–2010:Q4. Standard errors are clustered at the state level or at the zip code level (Column (5) and (6)); *t*-statistics are in parentheses. The estimates are expressed in percentage terms.

Panel A: Servicer Pre-HAMP Renegotiation Experience and HAMP Renegotiations

	Sample: Treatment loans (Strategy 1)		Sample: Treatment loans (Strategy 1)		Sample: Treatment loans in California (Strategy 1)	Sample: Treatment loans in Florida (Strategy 1)	Sample: Treatment loans (Strategy 2)
	Dependent variable: Whether a loan gets a trial HAMP modification in a quarter		Dependent variable: Whether a loan gets a permanent HAMP modification in a quarter		Dependent variable: Whether a loan gets a permanent HAMP modification in a quarter	Dependent variable: Whether a loan gets a permanent HAMP modification in a quarter	Dependent variable: Whether a loan gets a permanent HAMP modification in a quarter
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Experience	1.02 (3.33)	1.15 (3.35)	0.92 (4.08)	0.98 (4.03)	2.41 (4.39)	1.72 (4.12)	1.738 (5.71)
Observations	17,273,971	17,273,971	17,273,971	17,273,971	2,848,540	1,113,040	126,717
Adj. <i>R</i> -square	0.04	0.046	0.017	0.019	0.035	0.022	0.018
Other Controls & Origination FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes	-	-	Yes
Zip Code FE	No	No	No	No	Yes	Yes	No

Table 7: Servicer Pre-HAMP Renegotiation Experience and HAMP Renegotiations (contd.)

In Panel B, we present correlation coefficients between renegotiation experience of servicers prior to HAMP (*pre-HAMP mod rate*) and servicer organization variables. The pre-HAMP mod rate of servicers is obtained based on servicer fixed effects in a regression similar to column (4) of Table 2, Panel B, but estimated on pre-HAMP data. *Full-time staff (FTE)* is the number of employees employed in servicing the loans. *Loans-per-FTE* is the average number of loans serviced by an employee in a year. *Average training hours* refers to the hours dedicated by the servicing entity to training new (induction training) and old employees (continual training). *% calls dropped* refer to the percentage of calls dropped by the call center receiving calls related to loan servicing. *Phone hold time* refers to the average hold time (in seconds) a customer has to wait on a servicing call (see also Internet Appendix A.9).

Panel B: Correlation between Servicer Pre-HAMP Renegotiation Experience and Servicer Organizational Variables

	Full time staff (FTE)	Loans per FTE	Average Traning Hours	% Call Dropped	Phone hold time (sec)
	(1)	(2)	(3)	(4)	(5)
pre-HAMP mod rate	52%	-57%	14%	-43%	-49%

Table 8: Foreclosures, House Price Growth and Auto and Non-Durable Consumption Growth – Zip Code Level Analysis

Panel A reports OLS estimates of regression where the dependent variable is the percentage of modified loans under the program in a zip code during the program period (2009:Q2 to 2010:Q4). The variable *High Servicer Share* is the fraction of loans serviced by high experience servicers in a zip code as of March 2009 (based on our classification). Column (1) presents results for an overall sample of zip codes, while Column (2) and (3) present the results for the matched sample of zip codes. Panel B reports OLS estimates of regressions evaluating the relationship between exposure to HAMP in a zip code and the change in the quarterly house price growth and the foreclosure rate in a zip code. The change is between the program period (2009:Q2 to 2010:Q4) and the pre-program period (2008:Q3 to 2009:Q1). The sample consists of matched zip codes as explained in Section VII.A. The estimates are scaled by one standard deviation of *High Servicer Share* variable and expressed in percentage terms. *t*-statistics are in parentheses.

Panel A: Zip Code Ex Post HAMP Modifications and Ex Ante Exposure to HAMP (Share of Loans Serviced by High Experience Servicers)

	Percentage of loans modified under HAMP		
	All loans	Matched Sample	
	(1)	(2)	(3)
High Servicer Share	0.12	0.24	0.23
	(14.23)	(5.54)	(5.75)
Propensity Score Controls	No	No	Yes
Mean HAMP Percentage	0.92	1.45	1.45
Number of Observations	9,995	990	990
Adj. <i>R</i> -squared	0.02	0.04	0.19

Panel B: Zip Code Outcomes and Ex Ante Exposure: House Prices and Foreclosures

	Foreclosure rate		HPI growth		HPI growth (excluding distressed sales)	
	(1)	(2)	(3)	(4)	(5)	(6)
High Servicer Share	-0.07	-0.08	0.45	0.47	0.51	0.53
	(5.88)	(7.26)	(3.66)	(4.11)	(4.28)	(4.51)
Propensity Score Controls	No	Yes	No	Yes	No	Yes
Adj. <i>R</i> -squared	0.047	0.30	0.028	0.16	0.048	0.068

Table 8: Foreclosures, House Price Growth and Auto and Non-Durable Consumption Growth – Zip Code Level Analysis (contd.)

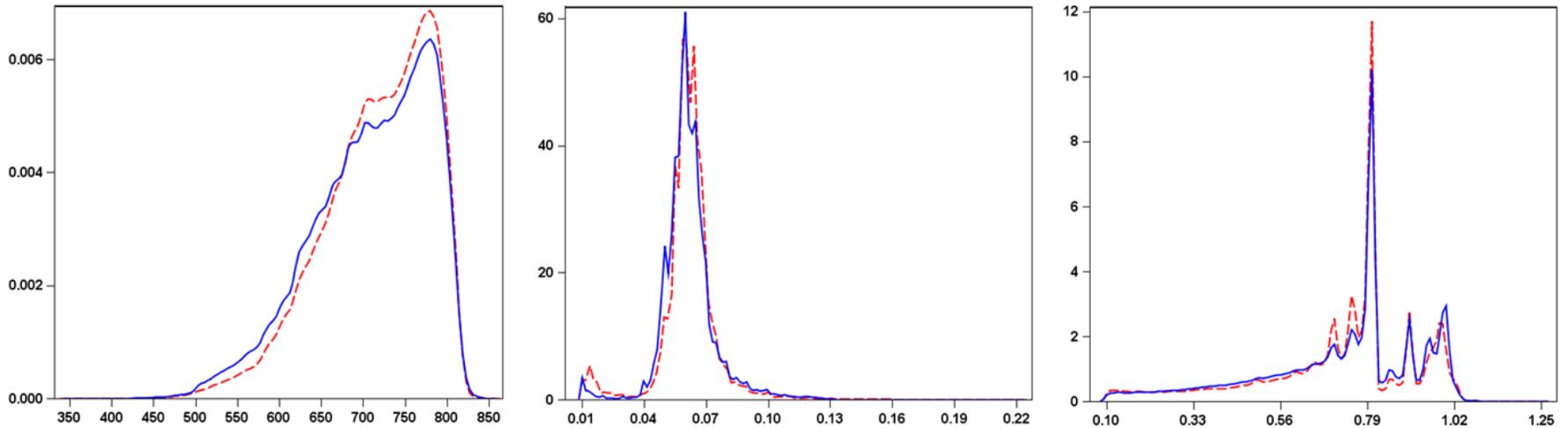
Panel C reports OLS estimates of regressions evaluating the relationship between exposure to HAMP in a zip code and the change in quarterly auto sales growth, non-durable spending growth, consumer delinquencies, and home equity line of credit delinquencies (HELOCs) in a zip code. The quarterly change is between the program period (2009:Q2 to 2010:Q4) and the pre-program period (2008:Q3 to 2009:Q1). The variable *High Servicer Share* is the fraction of loans serviced by high experience servicers in a zip code as of March 2009 (based on our classification). The sample consists of matched zip codes as explained in Section VII.A. The estimates are scaled by one standard deviation of the *High Servicer Share* variable and in percentage terms. *t*-statistics are in parentheses.

Panel C: Zip Code Outcomes and Ex Ante Exposure: Auto Sale Growth, Non-durable Spending Growth and Consumer Delinquencies

	Auto sales growth		Non-durable spending growth		Consumer delinquencies (All accounts)		HELOC Delinquencies	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High Servicer Share	0.22 (0.43)	0.22 (0.41)	-0.80 (0.50)	-0.85 (0.53)	-0.23 (4.32)	-0.21 (4.44)	-0.18 (1.94)	-0.17 (1.84)
Propensity Score Controls	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R-squared	0.000	0.010	0.010	0.020	0.025	0.183	0.003	0.012

Figure 1: Comparability of Treatment and Control Groups -- Kernel Density of Observables

The figure shows the kernel density plots for (a) loan origination FICO credit score, (b) interest rate, and (c) Loan to Value (LTV) in the treatment and control groups defined using Strategy 1 (owner-occupancy status). The treatment group is represented by the solid line, and the control group is represented by the dashed line.



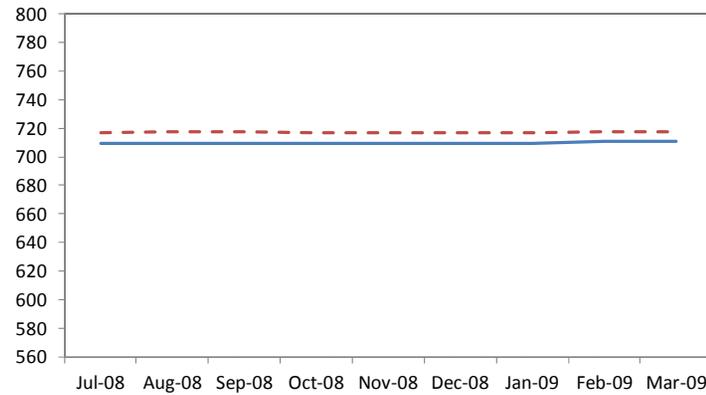
1(a): FICO credit score

1(b): Interest rate

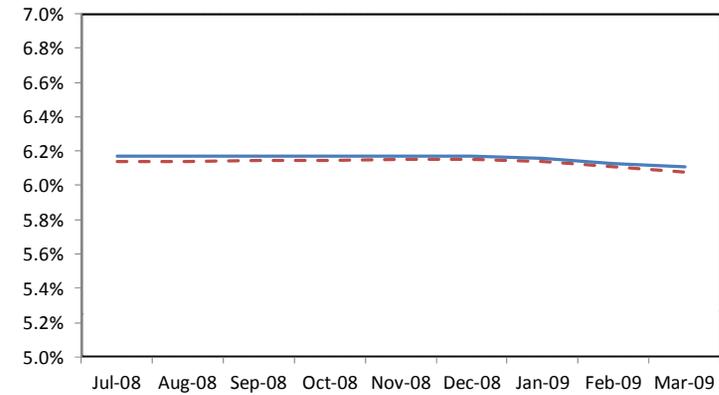
1(c): Loan to Value (LTV)

Figure 2: Comparability of Treatment and Control Groups: Evolution of Observables

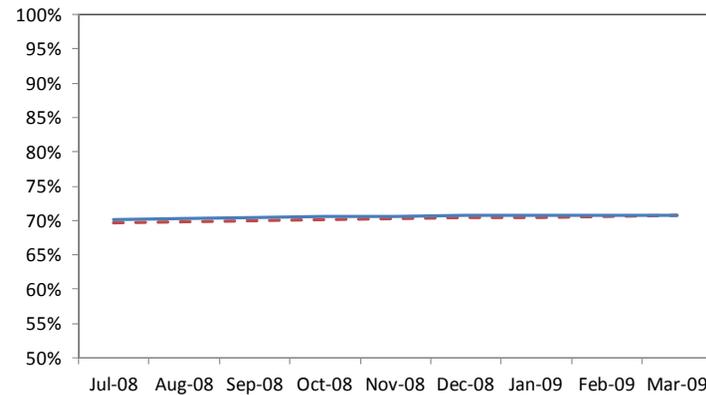
The figure shows the pre-program evolution of monthly evolution of mean (a) origination FICO credit score, (b) interest rate, (c) Loan to Value (LTV) , and (d) fraction of current loans that become seriously delinquent for the first time in the treatment and control groups defined using Strategy 1 (owner-occupancy status). The treatment group is represented by the solid line, and the control group is represented by the dashed line.



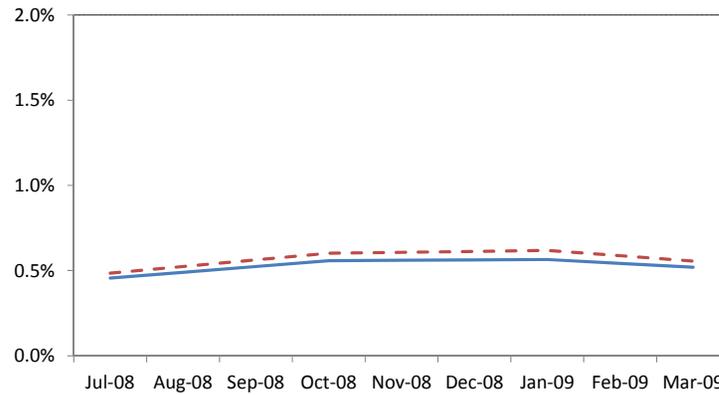
2(a): FICO credit score



2(b): Interest rate



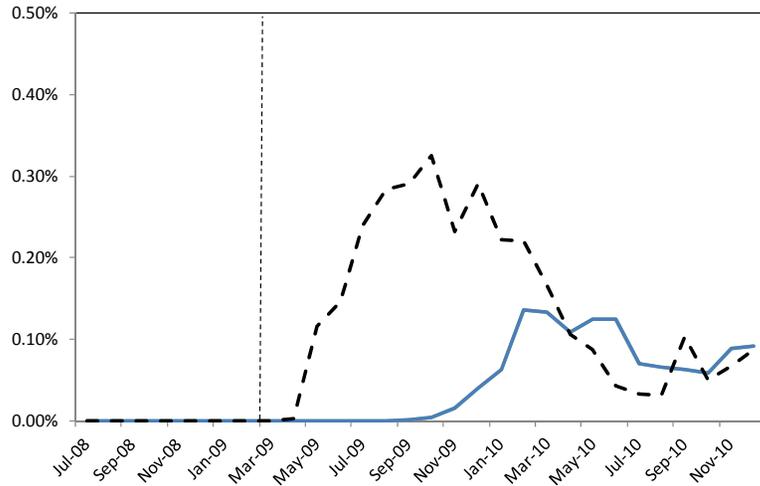
2(c): Loan to Value (LTV)



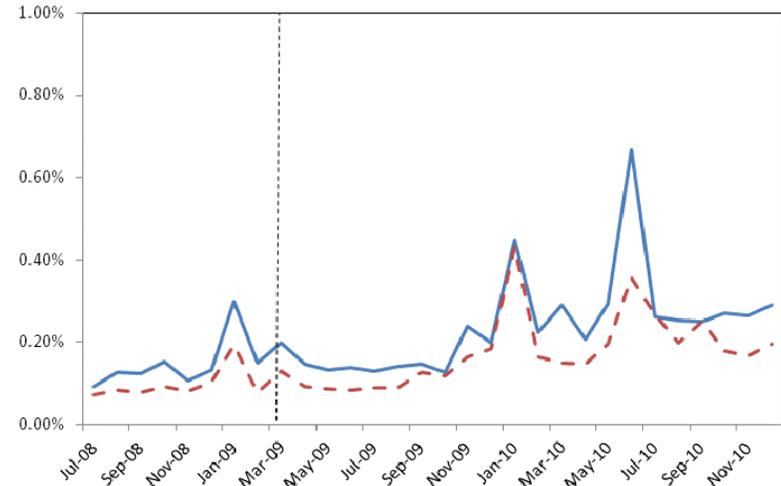
2(d): Delinquency rate

Figure 3: Evolution of Private, HAMP, and Combined (Private and HAMP) Modification Rates

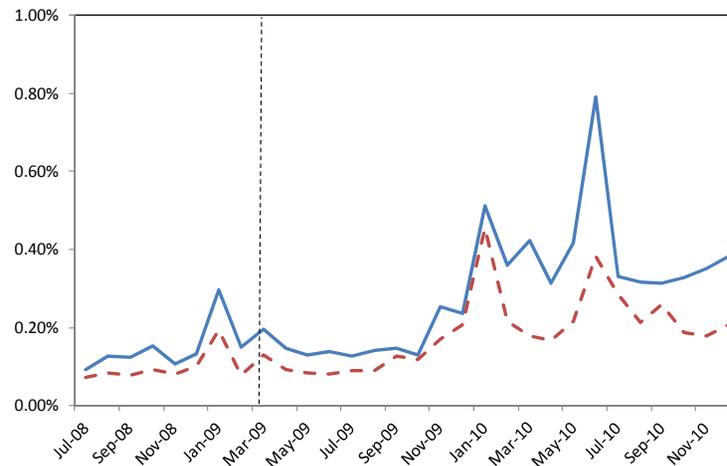
Panel (a) of the figure shows the percentage of loans receiving a trial (dashed line) and permanent (solid line) HAMP modification for the first time in a given month in the treatment group. Panel (b) shows the percentage of loans receiving a permanent private modification for the first time in a given month in the treatment and control groups defined using Strategy 1. Panel (c) shows the percentage of loans receiving a combined permanent modification (private and HAMP) in these groups. In Panels (b) and (c) the treatment group is represented by the solid line, and the control group is represented by the dashed line.



3(a): Trial and permanent HAMP modification



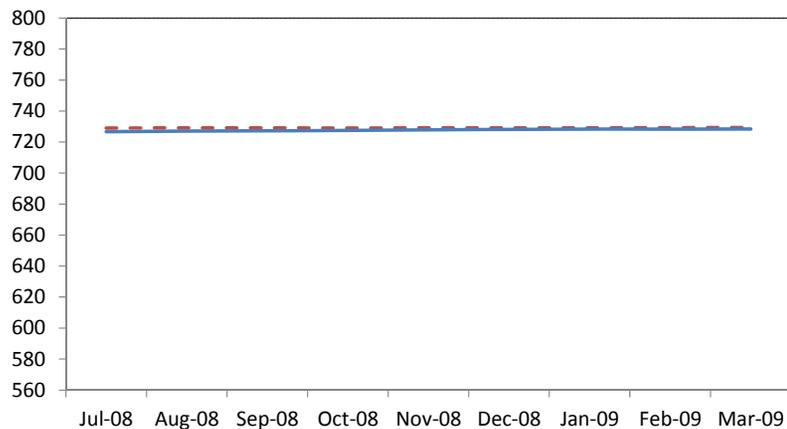
3(b): Private permanent modification



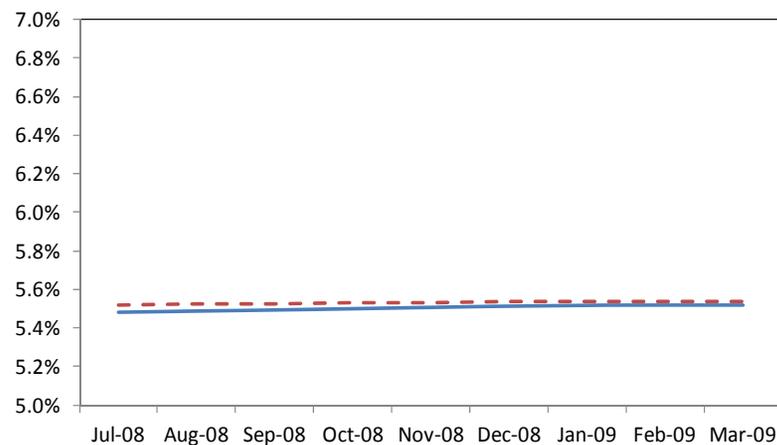
3(c): Combined private and HAMP permanent modification

Figure 4: Alternative Strategy: Comparability of Treatment and Control Groups: Evolution of Observables

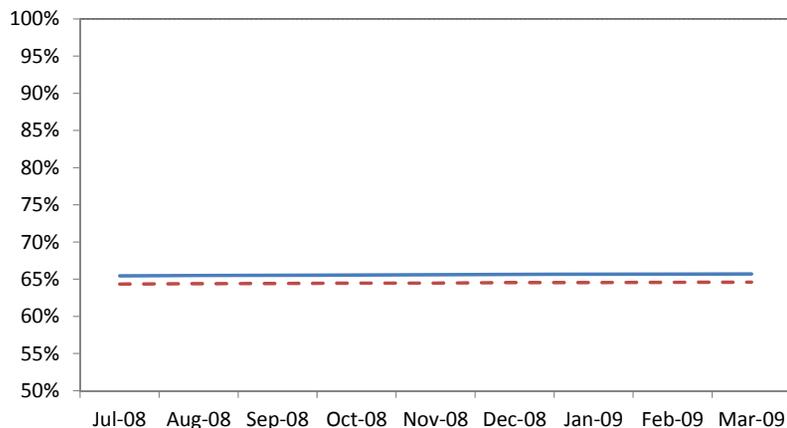
The figure shows the monthly evolution of mean (a) origination FICO credit score, (b) interest rate, (c) Loan to Value (LTV) , and (d) fraction of current loans that become seriously delinquent for the first time in the treatment and control groups defined using Strategy 2 (based on loan amount). The treatment group is represented by the solid line, and the control group is represented by the dashed line.



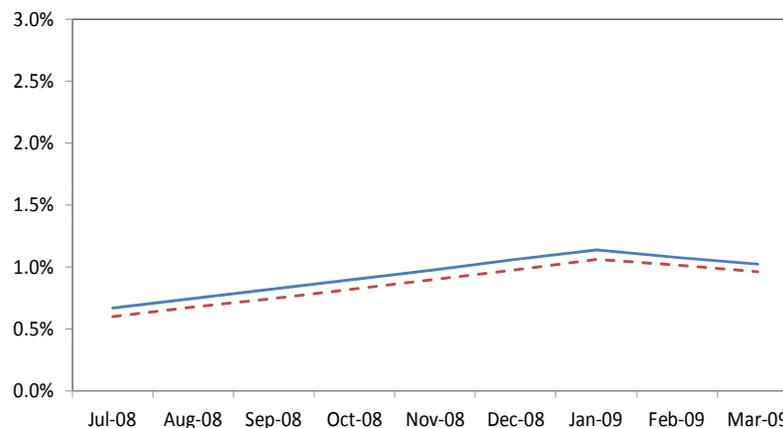
4(a): FICO credit score



4(b): Interest rate



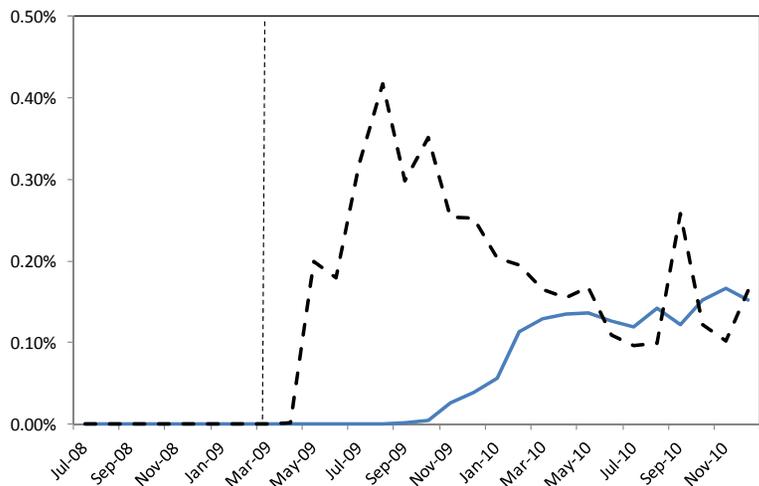
4(c): Loan to Value (LTV)



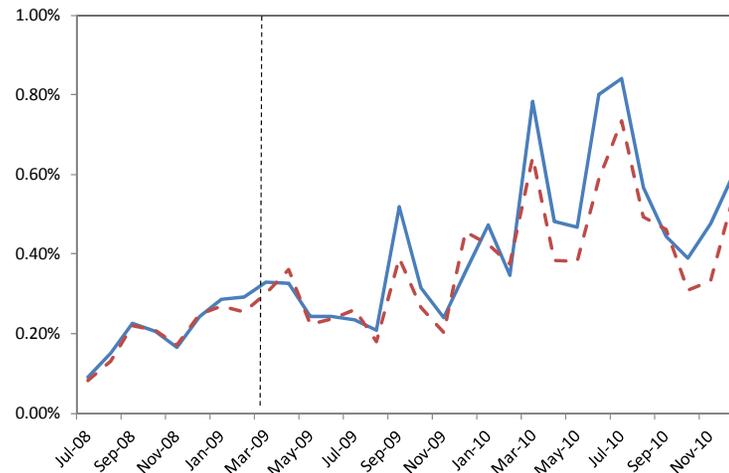
4(d): Delinquency rate

Figure 5: Alternative Strategy; Evolution of Private, HAMP, and Combined (Private and HAMP) Modification Rates

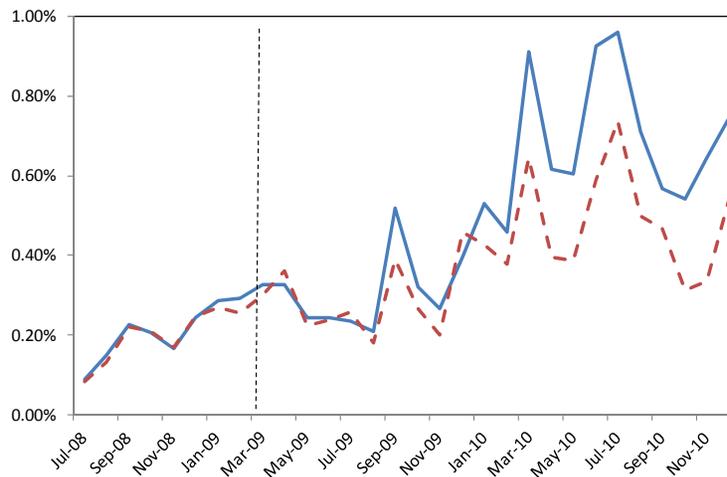
Panel (a) of the figure shows the percentage of loans receiving a trial (dashed line) and permanent (solid line) HAMP modification for the first time in a given month in the treatment group. Panel (b) shows the percentage of loans receiving a permanent private modification for the first time in a given month in the treatment and control groups defined using Strategy 2. Panel (c) shows the percentage of loans receiving a combined permanent modification (private and HAMP) in these groups. In Panels (b) and (c) the treatment group is represented by the solid line, and the control group is represented by the dashed line.



5(a): Trial and permanent HAMP modification



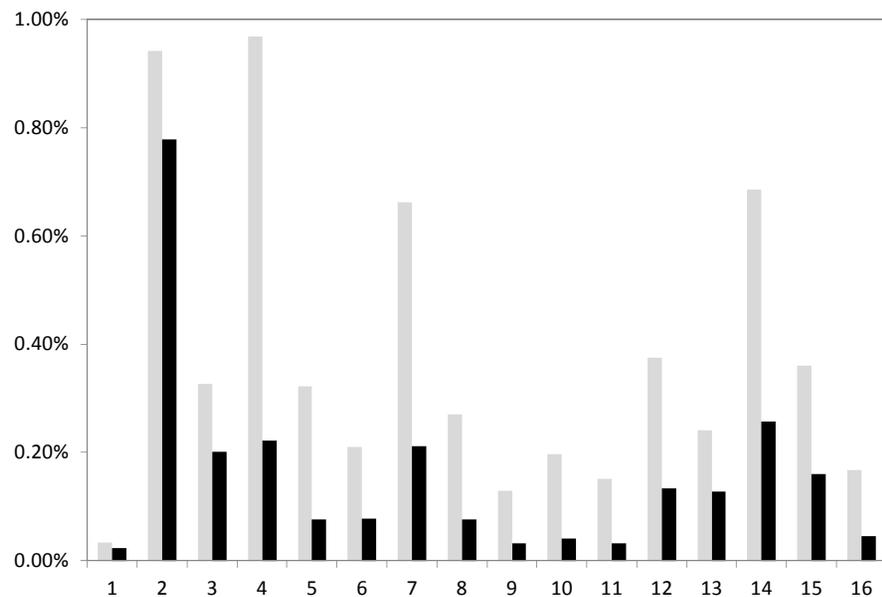
5(b): Private permanent modification



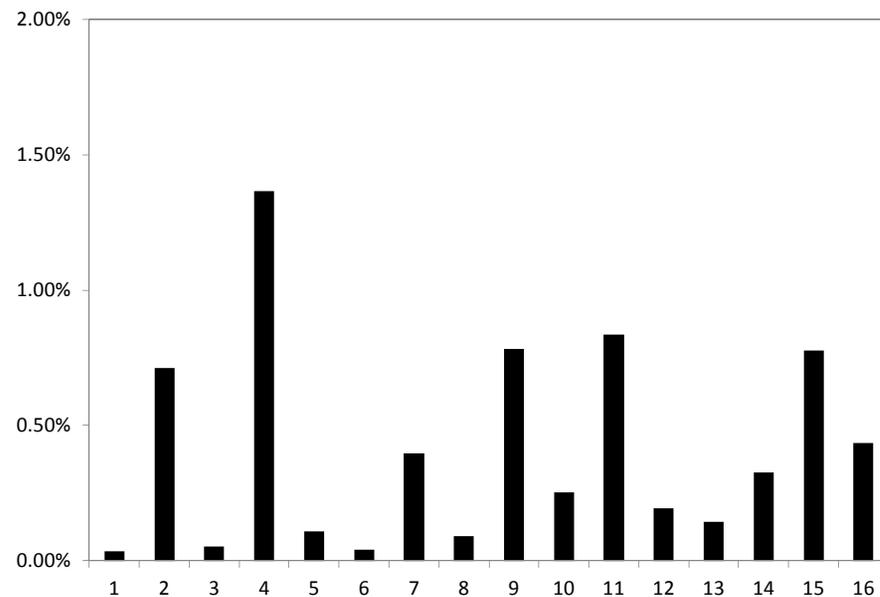
5(c): Combined private and HAMP permanent modification

Figure 6: Quarterly HAMP Modification Rates and Pre-HAMP Private Modification Rates Across Servicers

The figure shows the heterogeneity in modification rates across sixteen servicers in our data. Figure (a) presents quarterly trial and permanent HAMP modification rates by servicer. These servicer-specific rates are obtained based on servicer fixed effects in column (2) and column (4) of Table 2, Panel B. Figure (b) presents quarterly pre-HAMP private permanent modification rate by servicer; this rate is obtained based on servicer fixed effects in a regression similar to column (4) of Table 2, Panel B, but estimated on pre-HAMP data.



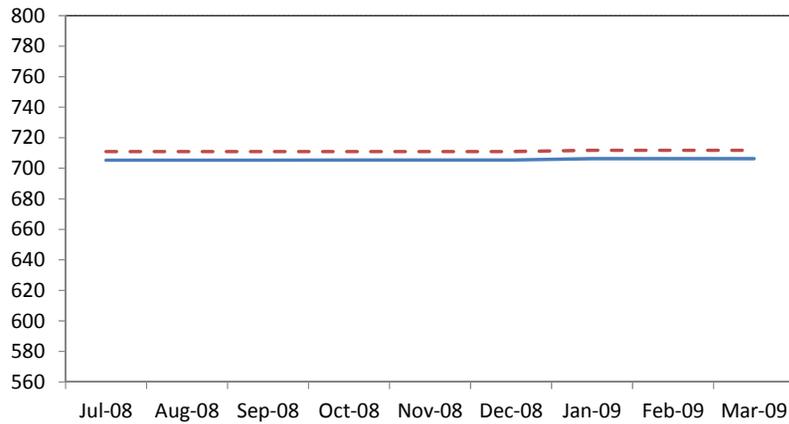
6(a): Trial (grey) and permanent (black) HAMP modification rates



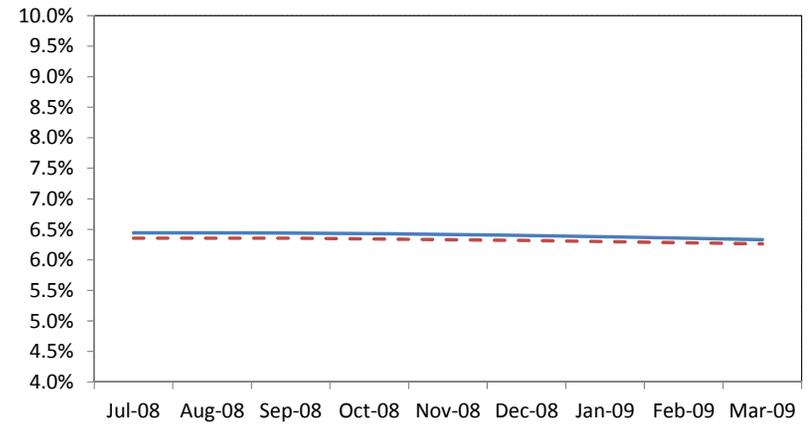
6(b): Pre-HAMP permanent modification rates

Figure 7: Comparability of High Exposure and Low Exposure Zip Codes formed based on High Experience Servicer Share

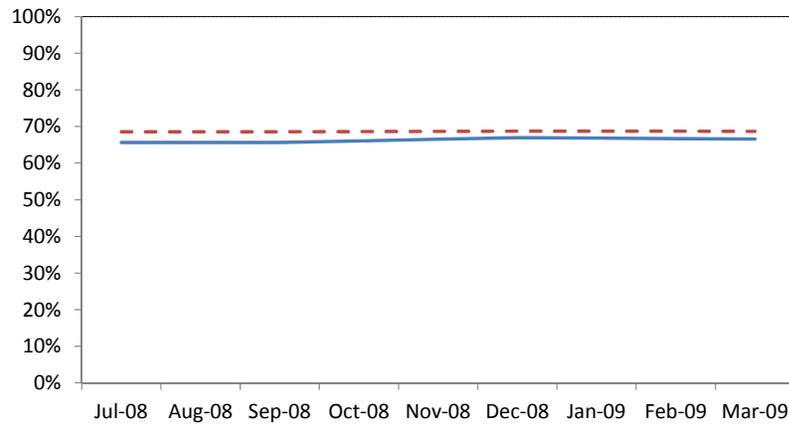
The figure shows the evolution of mean (a) origination FICO credit score, (b) interest rate, (c) Loan to Value (LTV) , and (d) fraction of current loans that become seriously delinquent for the first time in the treatment and control groups in the matched zip code sample in the pre-program period. The high and low exposure groups are defined based on share of loans handled by high experience servicers in the pre-program period. The high exposure group is represented by the solid line, and the low exposure group is represented by the dashed line.



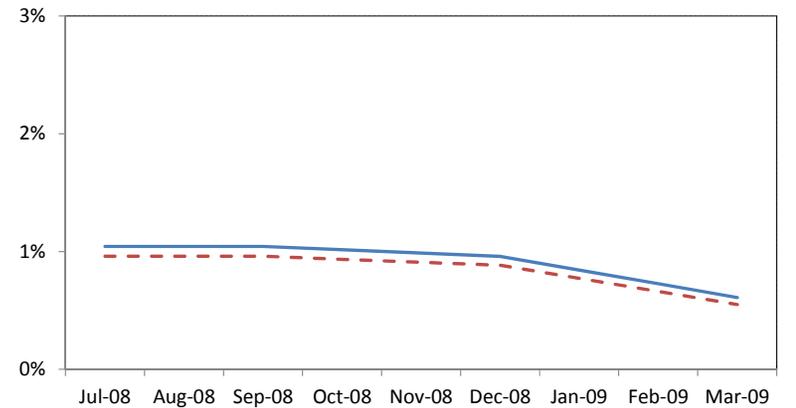
7(a): FICO



7(b): Interest rate



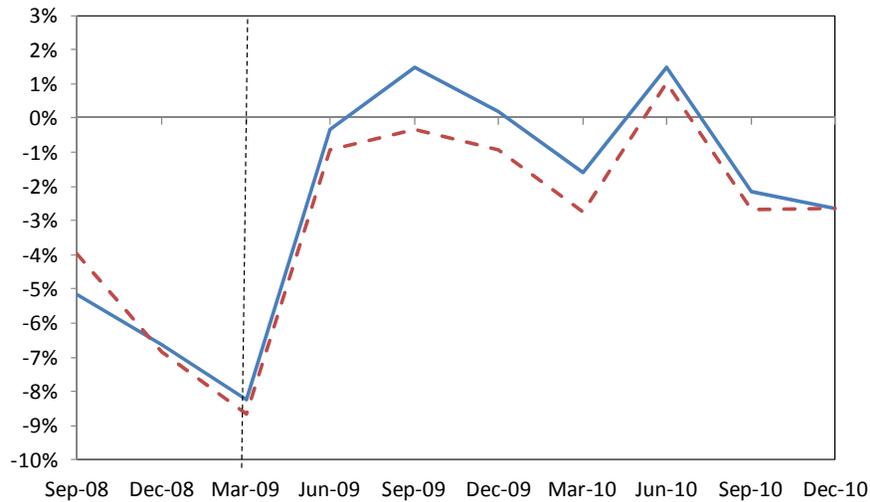
7(c): Loan to Value (LTV)



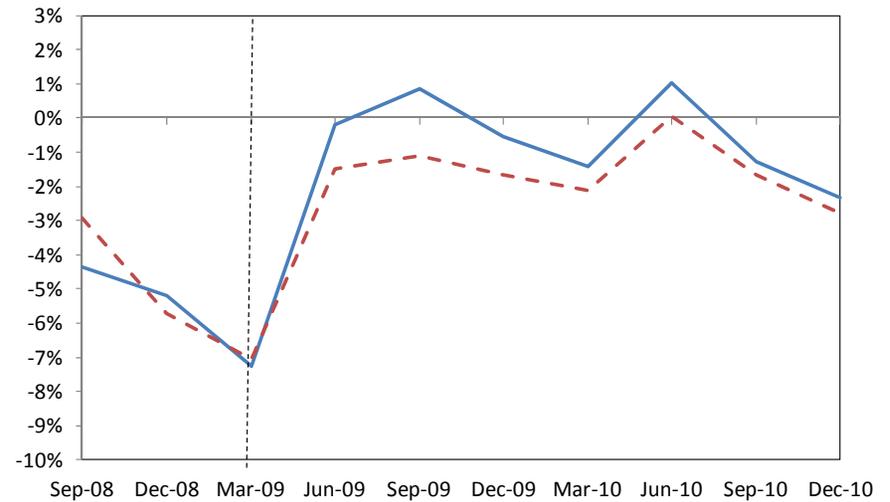
7(d): Delinquency rate

Figure 8: Quarterly HPI Growth, Auto Sales Growth, and Consumer Credit Delinquency Rates in High and Low Exposure Zip Codes

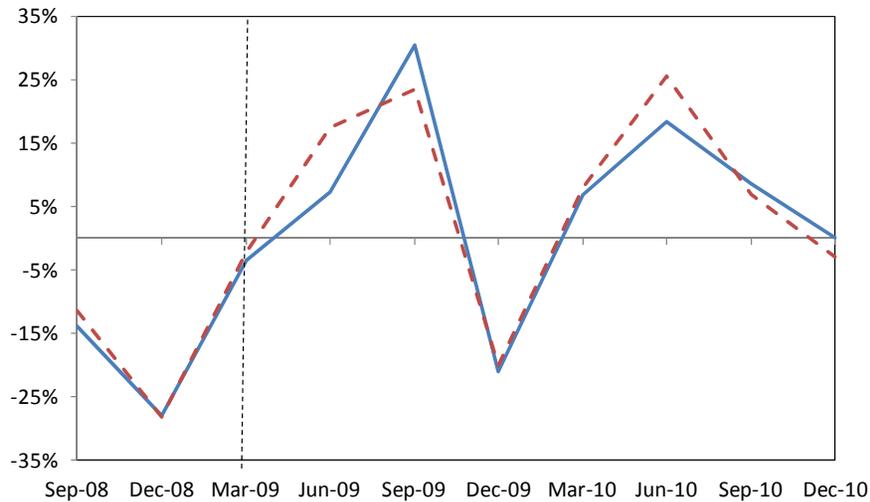
The figure shows the average house price growth rates (Panels (a) and (b)), auto sales growth (Panel (c)), and the delinquency rate on all consumer accounts (Panel (d)) in the high and low exposure groups in the matched zip code sample. Zip-code-level house price growth is computed using CoreLogic (Panel (a)) and CoreLogic excluding distressed sales (Panel (b)) price indices, auto sales growth data come from Mian and Sufi (2010) (Panel (c)), and the rate of consumer delinquencies on all accounts is from c'o clqt 'etgf k'dwtgcw(Panel (d)). The high exposure group is represented by the solid line, and the low exposure group is represented by the dashed line.



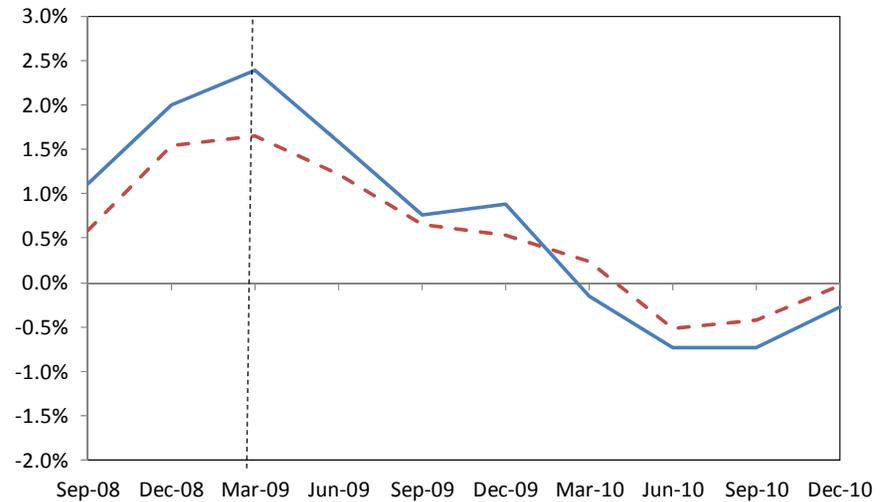
8(a): House price growth



8(b): House price growth (excluding distressed sales)



8(c): Auto sales growth



8(d): Delinquency rate (other debt besides mortgages but including HELOCs)

Working Paper Series

A series of research studies on regional economic issues relating to the Seventh Federal Reserve District, and on financial and economic topics.

Comment on “Letting Different Views about Business Cycles Compete” <i>Jonas D.M. Fisher</i>	WP-10-01
Macroeconomic Implications of Agglomeration <i>Morris A. Davis, Jonas D.M. Fisher and Toni M. Whited</i>	WP-10-02
Accounting for non-annuitization <i>Svetlana Pashchenko</i>	WP-10-03
Robustness and Macroeconomic Policy <i>Gadi Barlevy</i>	WP-10-04
Benefits of Relationship Banking: Evidence from Consumer Credit Markets <i>Sumit Agarwal, Souphala Chomsisengphet, Chunlin Liu, and Nicholas S. Souleles</i>	WP-10-05
The Effect of Sales Tax Holidays on Household Consumption Patterns <i>Nathan Marwell and Leslie McGranahan</i>	WP-10-06
Gathering Insights on the Forest from the Trees: A New Metric for Financial Conditions <i>Scott Brave and R. Andrew Butters</i>	WP-10-07
Identification of Models of the Labor Market <i>Eric French and Christopher Taber</i>	WP-10-08
Public Pensions and Labor Supply Over the Life Cycle <i>Eric French and John Jones</i>	WP-10-09
Explaining Asset Pricing Puzzles Associated with the 1987 Market Crash <i>Luca Benzoni, Pierre Collin-Dufresne, and Robert S. Goldstein</i>	WP-10-10
Prenatal Sex Selection and Girls’ Well-Being: Evidence from India <i>Luoqia Hu and Analía Schlosser</i>	WP-10-11
Mortgage Choices and Housing Speculation <i>Gadi Barlevy and Jonas D.M. Fisher</i>	WP-10-12
Did Adhering to the Gold Standard Reduce the Cost of Capital? <i>Ron Alquist and Benjamin Chabot</i>	WP-10-13
Introduction to the <i>Macroeconomic Dynamics</i> : Special issues on money, credit, and liquidity <i>Ed Nosal, Christopher Waller, and Randall Wright</i>	WP-10-14
Summer Workshop on Money, Banking, Payments and Finance: An Overview <i>Ed Nosal and Randall Wright</i>	WP-10-15
Cognitive Abilities and Household Financial Decision Making <i>Sumit Agarwal and Bhashkar Mazumder</i>	WP-10-16

Working Paper Series *(continued)*

Complex Mortgages <i>Gene Amromin, Jennifer Huang, Clemens Sialm, and Edward Zhong</i>	WP-10-17
The Role of Housing in Labor Reallocation <i>Morris Davis, Jonas Fisher, and Marcelo Veracierto</i>	WP-10-18
Why Do Banks Reward their Customers to Use their Credit Cards? <i>Sumit Agarwal, Sujit Chakravorti, and Anna Lunn</i>	WP-10-19
The impact of the originate-to-distribute model on banks before and during the financial crisis <i>Richard J. Rosen</i>	WP-10-20
Simple Markov-Perfect Industry Dynamics <i>Jaap H. Abbring, Jeffrey R. Campbell, and Nan Yang</i>	WP-10-21
Commodity Money with Frequent Search <i>Ezra Oberfield and Nicholas Trachter</i>	WP-10-22
Corporate Average Fuel Economy Standards and the Market for New Vehicles <i>Thomas Klier and Joshua Linn</i>	WP-11-01
The Role of Securitization in Mortgage Renegotiation <i>Sumit Agarwal, Gene Amromin, Itzhak Ben-David, Souphala Chomsisengphet, and Douglas D. Evanoff</i>	WP-11-02
Market-Based Loss Mitigation Practices for Troubled Mortgages Following the Financial Crisis <i>Sumit Agarwal, Gene Amromin, Itzhak Ben-David, Souphala Chomsisengphet, and Douglas D. Evanoff</i>	WP-11-03
Federal Reserve Policies and Financial Market Conditions During the Crisis <i>Scott A. Brave and Hesna Genay</i>	WP-11-04
The Financial Labor Supply Accelerator <i>Jeffrey R. Campbell and Zvi Hercowitz</i>	WP-11-05
Survival and long-run dynamics with heterogeneous beliefs under recursive preferences <i>Jaroslav Borovička</i>	WP-11-06
A Leverage-based Model of Speculative Bubbles (Revised) <i>Gadi Barlevy</i>	WP-11-07
Estimation of Panel Data Regression Models with Two-Sided Censoring or Truncation <i>Sule Alan, Bo E. Honoré, LuoJia Hu, and Søren Leth-Petersen</i>	WP-11-08
Fertility Transitions Along the Extensive and Intensive Margins <i>Daniel Aaronson, Fabian Lange, and Bhashkar Mazumder</i>	WP-11-09
Black-White Differences in Intergenerational Economic Mobility in the US <i>Bhashkar Mazumder</i>	WP-11-10

Working Paper Series *(continued)*

Can Standard Preferences Explain the Prices of Out-of-the-Money S&P 500 Put Options? <i>Luca Benzoni, Pierre Collin-Dufresne, and Robert S. Goldstein</i>	WP-11-11
Business Networks, Production Chains, and Productivity: A Theory of Input-Output Architecture <i>Ezra Oberfield</i>	WP-11-12
Equilibrium Bank Runs Revisited <i>Ed Nosal</i>	WP-11-13
Are Covered Bonds a Substitute for Mortgage-Backed Securities? <i>Santiago Carbó-Valverde, Richard J. Rosen, and Francisco Rodríguez-Fernández</i>	WP-11-14
The Cost of Banking Panics in an Age before “Too Big to Fail” <i>Benjamin Chabot</i>	WP-11-15
Import Protection, Business Cycles, and Exchange Rates: Evidence from the Great Recession <i>Chad P. Bown and Meredith A. Crowley</i>	WP-11-16
Examining Macroeconomic Models through the Lens of Asset Pricing <i>Jaroslav Borovička and Lars Peter Hansen</i>	WP-12-01
The Chicago Fed DSGE Model <i>Scott A. Brave, Jeffrey R. Campbell, Jonas D.M. Fisher, and Alejandro Justiniano</i>	WP-12-02
Macroeconomic Effects of Federal Reserve Forward Guidance <i>Jeffrey R. Campbell, Charles L. Evans, Jonas D.M. Fisher, and Alejandro Justiniano</i>	WP-12-03
Modeling Credit Contagion via the Updating of Fragile Beliefs <i>Luca Benzoni, Pierre Collin-Dufresne, Robert S. Goldstein, and Jean Helwege</i>	WP-12-04
Signaling Effects of Monetary Policy <i>Leonardo Melosi</i>	WP-12-05
Empirical Research on Sovereign Debt and Default <i>Michael Tomz and Mark L. J. Wright</i>	WP-12-06
Credit Risk and Disaster Risk <i>François Gourio</i>	WP-12-07
From the Horse’s Mouth: How do Investor Expectations of Risk and Return Vary with Economic Conditions? <i>Gene Amromin and Steven A. Sharpe</i>	WP-12-08
Using Vehicle Taxes To Reduce Carbon Dioxide Emissions Rates of New Passenger Vehicles: Evidence from France, Germany, and Sweden <i>Thomas Klier and Joshua Linn</i>	WP-12-09
Spending Responses to State Sales Tax Holidays <i>Sumit Agarwal and Leslie McGranahan</i>	WP-12-10

Working Paper Series *(continued)*

Micro Data and Macro Technology <i>Ezra Oberfield and Devesh Raval</i>	WP-12-11
The Effect of Disability Insurance Receipt on Labor Supply: A Dynamic Analysis <i>Eric French and Jae Song</i>	WP-12-12
Medicaid Insurance in Old Age <i>Mariacristina De Nardi, Eric French, and John Bailey Jones</i>	WP-12-13
Fetal Origins and Parental Responses <i>Douglas Almond and Bhashkar Mazumder</i>	WP-12-14
Repos, Fire Sales, and Bankruptcy Policy <i>Gaetano Antinolfi, Francesca Carapella, Charles Kahn, Antoine Martin, David Mills, and Ed Nosal</i>	WP-12-15
Speculative Runs on Interest Rate Pegs The Frictionless Case <i>Marco Bassetto and Christopher Phelan</i>	WP-12-16
Institutions, the Cost of Capital, and Long-Run Economic Growth: Evidence from the 19th Century Capital Market <i>Ron Alquist and Ben Chabot</i>	WP-12-17
Emerging Economies, Trade Policy, and Macroeconomic Shocks <i>Chad P. Bown and Meredith A. Crowley</i>	WP-12-18
The Urban Density Premium across Establishments <i>R. Jason Faberman and Matthew Freedman</i>	WP-13-01
Why Do Borrowers Make Mortgage Refinancing Mistakes? <i>Sumit Agarwal, Richard J. Rosen, and Vincent Yao</i>	WP-13-02
Bank Panics, Government Guarantees, and the Long-Run Size of the Financial Sector: Evidence from Free-Banking America <i>Benjamin Chabot and Charles C. Moul</i>	WP-13-03
Fiscal Consequences of Paying Interest on Reserves <i>Marco Bassetto and Todd Messer</i>	WP-13-04
Properties of the Vacancy Statistic in the Discrete Circle Covering Problem <i>Gadi Barlevy and H. N. Nagaraja</i>	WP-13-05
Credit Crunches and Credit Allocation in a Model of Entrepreneurship <i>Marco Bassetto, Marco Cagetti, and Mariacristina De Nardi</i>	WP-13-06

Working Paper Series *(continued)*

Financial Incentives and Educational Investment: The Impact of Performance-Based Scholarships on Student Time Use <i>Lisa Barrow and Cecilia Elena Rouse</i>	WP-13-07
The Global Welfare Impact of China: Trade Integration and Technological Change <i>Julian di Giovanni, Andrei A. Levchenko, and Jing Zhang</i>	WP-13-08
Structural Change in an Open Economy <i>Timothy Uy, Kei-Mu Yi, and Jing Zhang</i>	WP-13-09
The Global Labor Market Impact of Emerging Giants: a Quantitative Assessment <i>Andrei A. Levchenko and Jing Zhang</i>	WP-13-10
Size-Dependent Regulations, Firm Size Distribution, and Reallocation <i>François Gourio and Nicolas Roys</i>	WP-13-11
Modeling the Evolution of Expectations and Uncertainty in General Equilibrium <i>Francesco Bianchi and Leonardo Melosi</i>	WP-13-12
Rushing into American Dream? House Prices, Timing of Homeownership, and Adjustment of Consumer Credit <i>Sumit Agarwal, Luojia Hu, and Xing Huang</i>	WP-13-13
The Earned Income Tax Credit and Food Consumption Patterns <i>Leslie McGranahan and Diane W. Schanzenbach</i>	WP-13-14
Agglomeration in the European automobile supplier industry <i>Thomas Klier and Dan McMillen</i>	WP-13-15
Human Capital and Long-Run Labor Income Risk <i>Luca Benzoni and Olena Chyruk</i>	WP-13-16
The Effects of the Saving and Banking Glut on the U.S. Economy <i>Alejandro Justiniano, Giorgio E. Primiceri, and Andrea Tambalotti</i>	WP-13-17
A Portfolio-Balance Approach to the Nominal Term Structure <i>Thomas B. King</i>	WP-13-18
Gross Migration, Housing and Urban Population Dynamics <i>Morris A. Davis, Jonas D.M. Fisher, and Marcelo Veracierto</i>	WP-13-19
Very Simple Markov-Perfect Industry Dynamics <i>Jaap H. Abbring, Jeffrey R. Campbell, Jan Tilly, and Nan Yang</i>	WP-13-20
Bubbles and Leverage: A Simple and Unified Approach <i>Robert Barsky and Theodore Bogusz</i>	WP-13-21

Working Paper Series *(continued)*

The scarcity value of Treasury collateral: Repo market effects of security-specific supply and demand factors <i>Stefania D'Amico, Roger Fan, and Yuriy Kitsul</i>	WP-13-22
Gambling for Dollars: Strategic Hedge Fund Manager Investment <i>Dan Bernhardt and Ed Nosal</i>	WP-13-23
Cash-in-the-Market Pricing in a Model with Money and Over-the-Counter Financial Markets <i>Fabrizio Mattesini and Ed Nosal</i>	WP-13-24
An Interview with Neil Wallace <i>David Altig and Ed Nosal</i>	WP-13-25
Firm Dynamics and the Minimum Wage: A Putty-Clay Approach <i>Daniel Aaronson, Eric French, and Isaac Sorkin</i>	WP-13-26
Policy Intervention in Debt Renegotiation: Evidence from the Home Affordable Modification Program <i>Sumit Agarwal, Gene Amromin, Itzhak Ben-David, Souphala Chomsisengphet, Tomasz Piskorski, and Amit Seru</i>	WP-13-27