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POLICY INTERVENTION IN DEBT RENEGOTIATION:
EVIDENCE FROM THE HOME AFFORDABLE MODIFICATION PROGRAM

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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 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 increase in the intensity of renegotiations while adversely affecting effectiveness of renegotiations performed outside the program. Renegotiations induced by the program resulted in a modest reduction in rate of foreclosures but did not alter the rate of house price decline, durable consumption, or employment in regions with higher exposure to the program. The overall impact of the program will be substantially limited since it will induce renegotiations that will reach 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 cannot be accounted by differences in contract, borrower, or regional characteristics of mortgages across servicers. Instead, their low renegotiation activity—which is also observed before the program—reflects servicer specific factors that appear to be related to their preexisting organizational capabilities. Our findings reveal that the ability of government to quickly induce changes in behavior of large intermediaries through financial incentives is quite limited, underscoring significant barriers to the effectiveness of such polices.

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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 polices—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 redistributional 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 a distressed loan 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 intensity of a few servicers explains a large part of this shortfall. Their low renegotiation activity, which is also observed before the program, cannot be accounted by differences in contract, borrower, or regional characteristics of mortgages across servicers. Rather, servicer specific factors--which seem to be related to their preexisting organizational capabilities--had a significant role in hampering program implementation. Our findings reveal that the ability of government to quickly induce changes in behavior of large intermediaries through financial incentives is limited

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 large financial incentives[2] to servicers, relative to their regular compensation, in an attempt to alleviate several perceived barriers to renegotiation—such as the inability of the private market to

[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 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–$1,000 per year for a $200,000 outstanding loan balance mortgage).

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; Agarwal et al., 2011).

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. This helps us to generate systematic evidence on the effects of this intervention—and makes this 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.

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 renegotiation rates on mortgages 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 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]

[3] Anecdote-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.
[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,

We start our analysis by showing that, on average, control and treatment groups in both empirical strategies are very similar on 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, and interest 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.

Our main analysis begins by analyzing the extensive margin—that is, additional loan renegotiations (contract modifications) induced by the program. We find that there were non-negligible HAMP modifications offered in the eligible group of loans. However, this does not imply, per se, that the program increased the overall rate of modifications performed by servicers since HAMP may also indirectly adversely impact modifications performed by the servicers outside of the program (i.e., "private modifications"). Therefore, we also examine the impact of the program on private modifications in the treatment and control groups. We find no evidence of decline in the rate of private modifications in the eligible group relative to the control 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.

While we do not find evidence of substitution on the extensive margin, we do find some evidence of substitution on the intensive margin in the treatment group—that is, in the composition of types of renegotiations and effectiveness of renegotiation as measured by default rate subsequent to the modification. In particular, private permanent modifications offered in the treatment group after the program is introduced are 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 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 around the program.

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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 consist of owner-occupied properties with similar loan balances.

[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 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 duration of the program (i.e., through December 2012)—substantially lower than the program target. In addition, because of limited coverage of the post-program period in our data, it is difficult to conclude how many of these foreclosures would be permanently prevented.

We also evaluate the impact of HAMP on other economic outcomes that are available to us only at the zip code level. We find no evidence that the program affected house prices in regions more exposed to the program. Concurrently, we also find no significant changes in growth rates of auto sales and employment, or in delinquency rates on credit card loans and auto loans in regions more exposed to the program. These results suggest that the program generated limited spillovers, at least in the near term.

In sum, the first part of our paper establishes that servicers responded to the program by conducting more modifications among eligible loans, though 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 we exploit response across intermediaries to shed light on potential barriers to program implementation.

We find a 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 their more active counterparts. Further investigation shows that 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 by differences in 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.

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 the program may not have been inadequate per se. Rather, the policy failed to account for firm level factors that resulted in muted program response of some servicers. Notably, our analysis does not allow us to comment on the exact nature of these firm level factors or how they lead to inertia in behavior of these servicers. For instance, servicers with low renegotiation activity in the pre-program period may not have responded to the program since doing so would involve 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 appropriate infrastructure and hiring and training servicing staff. Regardless of what these exact factors may be, our analysis does reveal that their presence limits the ability of the government to quickly influence intermediaries through provision of financial incentives.

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; Keys et al. 2010, 2011; Rajan et al. 2010 and Demyanyk and Van Hemert 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 and falling house prices on economic outcomes (e.g., Mian and Sufi 2010; Melzer, 2010; Mian et al. 2011; Campbell et al. 2011 and Mian and Sufi 2012).[6]

Finally, our findings investigating the possibility of substitution 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 occurring in subsequent years (see Credit Suisse Foreclosure

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

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 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.[7] Second, policymakers noted that the non-agency securitized market—i.e., 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---and 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.[8]

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. HAMP was first authorized under the Emergency Economic Stabilization Act of 2008 and then amended by the American Recovery and Reinvestment Act of 2009. The HAMP outline was 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

[7] 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).

[8] 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).

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 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 affected more by HAMP (treatment group) and those who are not (control group).[9] We note that verification of these criteria require 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 the HAMP program, 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

[9] In addition, servicers were required to screen candidates for loan modification to ensure that these borrowers were at 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.

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.[10]

When the program was introduced, new borrowers were to be accepted under the program 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 policymakers--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 for the analysis come from a unique data set known as the OCC Mortgage Metrics data. This 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. 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 also know if the loan was modified under HAMP—either as a trial or permanent modification—or if it was privately modified by servicers. The data set also contains information about the change in contract terms when a modification occurs as well as modified terms and repayment behavior before and after the renegotiation (e.g., reduction in interest rate, amount of principal deferred or

[10] In addition, a cost-sharing arrangement with mortgage investors was designed to help reduce first-lien mortgage payments to 31% of a homeowner's gross household income.
[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 US GAO Report, July 2009.

forgiven etc.), 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.

Finally, in our zip-code-level analysis, we use zip-code-level house price indices from Zillow (an online real estate database), zip code auto sales growth data from Mian and Sufi (2010), employment data from the U.S. Bureau of Labor Statistics (BLS), and data on consumer credit performance from a credit bureau (Equifax).

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.[12] 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 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 rely on the following difference-in-difference specification to estimate the effect of HAMP:

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

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 and 0 otherwise. Loans for

[12] 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).

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 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—i.e., whether or not the loan got 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—i.e., 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*).

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. Both panels report the statistics in the pre-HAMP period, i.e., from July 2008 to March 2009.

As one can observe, the control group is very similar to the treatment group. 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%). Importantly, the renegotiation rates in the two groups are similar in the pre-HAMP period as well—about 0.3% of loans obtain private permanent modifications per quarter in the control group and about 0.4% in the treatment group. Notably, not only the means but the computed standard deviations of the two groups are quite similar for these variables as well. We note that our data consist of the 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.

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. Notably, 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 (Figures 2(a)-2(c)). Additionally, Figure 4 (a) confirms that the renegotiation rates in the two groups are similar (up to a constant difference) in the period before the program. This analysis suggests that the observables are relatively well matched for treatment and control groups. Nevertheless, we provide evidence for robustness of our results by using an alternative empirical strategy in Section VI that is better matched across treatment and control groups--since both groups consist of owner-occupied properties with similar observables.

Second, like other studies on program evaluation that use 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 since such effects will be differenced out.[13]

V. Impact of HAMP on Extensive and Intensive Margins

V.A Extensive Margin

V.A.1 HAMP Trial and HAMP Permanent Modifications

We start our analysis by demonstrating that the program resulted in a higher intensity of HAMP renegotiations in the treatment group than in the control 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 control and treatment groups 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 control and treatment groups as defined by our main

[13] There may be a concern that 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 would inflate the program effect since our estimate in the treatment group is measured relative to the control group, which would concurrently have lower modification rates due to the program. We find no evidence for this scenario in the data. First, in Section V we show that, on average, there is no reduction in the modifications done in the control group across all servicers during program period (if anything there is an increase). Second, we stratify the treatment and control group based on servicer renegotiation activity prior to the program. To the extent that prior renegotiation activity proxies for servicer capacity, we find no reduction in modifications in the control group even for servicers with lower activity (Section VII). Finally, we also employ a specification to assess how treatment effect changes conditional on number of loans in the control group for a given servicer and find no evidence of this scenario either.

empirical strategy. There is a substantial increase in HAMP permanent modifications in the treatment group 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 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 reason for a rate that is smaller than 100% lies with the program guidelines that require the conversion from trial to permanent HAMP 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 help servicers to verify borrowers' eligibility for the program. We summarize these findings in Panel A of Table 2, 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 Panel B 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 zero otherwise.[15] Columns (3)–(4) present the corresponding results for permanent HAMP modifications.

[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 suggests 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.

[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 non-linearity 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.

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. This is because the program may also have affected the modifications performed by the servicers outside of the program (that is, private modifications). We formally investigate this question in the next section.

V.A.2 Private Permanent Modifications

We now explore the effects of HAMP on renegotiations done by servicers based on their private incentives outside the program (private modifications). As discussed earlier, HAMP could broadly affect the rate of private renegotiations in two ways. First, in the presence of government incentives, lenders may substitute some of the modifications they were going to conduct based on their private incentives with HAMP ones. Alternatively, it is possible that HAMP modifications do not substitute private modifications, but rather the program encourages lenders and servicers to modify additional loans, for instance, by subsidizing their investment in fixed costs required to achieve economies of scale across renegotiations.[17]

In Table 3, we test whether HAMP affected the rate of permanent private modifications in the treatment group.[18] 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

[16] We also investigate the relationship of incidence of HAMP modification received by a loan and its ownership—that is, 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 non-negligible number of HAMP modifications (both trial and permanent) in all of the ownership categories. This suggests that, consistent with one of its objective, HAMP did, enhance modification activity on securitized loans.

[17] This increase may also be on account of a larger number of borrowers applying for modification in the treatment group, with some borrowers who applied for HAMP modification receiving a private modification instead.

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

modifications in the treatment group during our sample period. This evidence suggests that the program did not result in substitution of private modifications with HAMP ones.[19]

We also observe these patterns clearly in Figure 4(a), 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 monthly modification rates in the two groups display very 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. The numbers in the figure suggest, on average, that the monthly private modification rates range from 0.1% to 0.6% (20,000-60,000 modifications per month). These plots provide additional support that the implementation of the program did not lead overall to any significant substitution of private permanent modifications with HAMP ones.

Overall, 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 (Panel A of Table 2). Moreover, this increase does not mask any substitution of private modifications with HAMP modifications, as our estimates imply an additional increase of 0.014% to 0.020% in the quarterly permanent modification rate in the treatment group due to additional private permanent modifications induced by the program. Taken together, these estimates suggest that HAMP led to an increase in the annual modification rate of about 0.72%. This accounts to about a 40% increase relative to the pre-program mean modification rate in the treatment group. We confirm this conjecture in Figure 4(b), 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 duration (i.e., through December 2012)—significantly short of the government expectations of three to four million modifications.[20]

Finally, we note that the findings above are robust to performing inferences separately in each of the loan ownership categories (i.e., whether a loan is bank held, is privately securitized, or is GSE securitized). In particular, we find no evidence of substitution on the extensive margin in each of the loan ownership categories, suggesting that the program resulted in an overall increase in modifications in both bank-held as well as securitized mortgages.

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[19] At a first glance our finding 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).

[20] 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 U.S. This involves applying the same estimate for potentially eligible loans that are not covered in our data. As well, we project using the same rate from the end of our sample period till the end of the program period.

V.B Intensive Margin

V.B.1 Contract Terms

We now study the effect of HAMP on contract terms offered to the borrowers whose loans were renegotiated. While the earlier analysis showed an increase in the rate of renegotiations (extensive margin) subsequent to the program, we now want to evaluate the changes on the intensive margin—i.e., on the type 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 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 modifications on 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 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, while 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. While this change could be consistent with several conjectures--such as incentive payments by the program prompting servicers to channel better loans toward aggressive modifications under the program--it is clear that the program led to the overall permanent modifications in the treatment group becoming less aggressive relative to those in the control group.

V.B.2 Effectiveness of Modifications (Redefault Rates)

An important metric to evaluate the effectiveness of renegotiating loan terms is the rate of default of the renegotiated loans (see Haughwout et al. 2010). We now examine "redefault rate" in the context of renegotiations that occur as a result of HAMP's introduction and compare them with the redefault rate on renegotiations done before the program.

In Panel B of Table 4, we use whether or not a modified mortgage redefaults within six months of receiving the renegotiation as the dependent variable. Our specification is similar to the main one, with the sample confined to loans that received a modification. 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.

There could be several reasons behind these findings. For instance, these results could simply reflect the fact that servicers channeled more promising loans (on unobservables) to be modified under the program, since their incentive payments were higher if a loan did not redefault after a modification. Alternatively, the results could reflect that servicers had to redirect some resources to conducting HAMP modifications that might have adversely impacted the effectiveness of private modifications. Regardless of what factors drive these findings, this evidence clearly shows that there was an adverse impact of the program on effectiveness of private modifications in the treatment group.

Overall, the findings from Section V.B suggest that the program induced some substitution 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. It is worth noting that these findings do not immediately imply that the program did not have any 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 Outcomes

V.C.1 Foreclosure Rates

We now turn to examining the impact HAMP had on the outcome it was designed to ultimately affect, i.e., the rate at which loans are foreclosed. In 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).[21] 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 duration of the program (i.e., through December 2012).[22] As Column (3) of Table 5 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 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, since 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 on 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) of Table 5 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 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 beyond this horizon. It is possible that the decline in foreclosure rates may be temporary. For

[21] 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.

[22] 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 U.S. This involves applying the same estimate for potentially eligible loans that are not covered in our data. As well, we project using the same rate from the end of our sample period till the end of the program period.

instance, servicers may just be delaying foreclosures while the program is being implemented. Nevertheless, we note that even if the reduction in foreclosure rates due to HAMP were 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 Table 6, 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 just an increase of 1.5% in relative terms when compared to 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.

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.[23]

[23] 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.

VI. Alternative Identification Strategy

In this section we 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 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 balances outstanding above \$729,750 are ineligible for the program.[24] 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 a low power, since few mortgages with loan balances in the vicinity of \$729,750 face economic hardship and receive modifications. Regardless, this strategy provides a valuable consistency check for the results obtained earlier.

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

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

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 zero 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.[25] As before, we estimate these regressions on all mortgages and employ the same outcome variables.

Table 1 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 Figure 1 (d)–(f), which plot the kernel densities of FICO credit score, LTV, and interest rates for the borrowers in

[24] 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).

[25] The results we obtain from this strategy are robust to tightening the bound around the balance threshold (e.g., when we consider loans within \$50,000 of the threshold).

the treatment and control groups defined based on the loan amount threshold. 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 2 (d)–(f)). Moreover, Figure 4(c) shows that the renegotiation rates in the two groups follow almost identical pattern in the period before the program.

In Figure 3(b), 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 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 Table 7. First, consistent with results in Table 3, we find no evidence that the program substituted private modifications by HAMP ones (Column (1) of Table 7). If anything, there is a small increase in the quarterly rate of permanent private modifications (about 0.06%). Column (2) of Table 7 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 4(d). 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 Table 7 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 Table 7 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.

[26] We observe a few rare instances of program modifications in the control group. These modifications are on mortgages 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 remain unchanged, regardless of whether these few cases are excluded or included in the analysis.

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.

VII. House Prices and Other Outcome Variables

In this section, we assess how growth rates of house prices, auto sales, employment, and delinquency rates on other categories of consumer credit were impacted by HAMP. We rely on a coarser unit of observation (zip code), since we do not have more micro data for these variables.

VII.A Treatment and Control Groups

We confine our analysis to zip codes that have at least 100 mortgages in the OCC database and where we have information on all the relevant variables (including zip-code-level house price data). This allows for reliable estimates and leaves us with time series for 6,616 zip codes. Because only owner-occupied homes were eligible for HAMP, we use the share of owner-occupied homes in a zip code as of March 2009 to construct an ex ante measure of a zip code's exposure to the program.

We first verify that this ex ante measure of regional HAMP exposure indeed correlates with the treatment. 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 control variable is a fraction of homes with outstanding mortgages that were owner-occupied in a zip code as of March 2009. There is a strong positive association between the fraction of HAMP-modified loans and the share of owner-occupied homes in a zip code. A 10% absolute increase in the fraction of owner-occupied homes results in a 0.23% absolute increase in the fraction of HAMP-modified loans in a zip code (around 8.5% relative increase with respect to the mean zip code fraction of HAMP-modified loans). These results demonstrate that zip codes with ex ante (as of March 2009) larger shares of owner-occupied homes indeed did subsequently receive more treatment ex post (2009:Q2–2010:Q4).

Next, we analyze the outcome differences in regions classified based on their exposure to the program. To do so, we follow the strategy similar to Mian and Sufi (2010) and define a zip code as being in the HAMP treatment group if it is in the top quartile of zip codes in terms of owner-occupancy percentage (as of March 2009). Likewise, we create a control group of zip codes in the bottom quartile in terms of owner-occupancy. We then limit our sample to only the zip codes in the treatment or control group, leaving us with 3,308 zip codes. The treatment group has a mean investor-occupancy homes percentage of 7.1% %, compared with 24.9% for the control group. Column (2) of Panel A of Table 8 verifies that in these sets of zip codes we also observe a strong positive association between the ex ante share of owner-occupied homes and fraction of HAMP modifications received ex post.[27]

VII.B House Price Growth

We analyze the differences in house price growth in regions classified based on 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, given the results of the previous section, we are interested in examining if regions with more exposure to HAMP—which are also the regions with lower foreclosure rates in the program period—experienced lower house price declines relative to regions with limited program exposure.

Figure 5(a) presents the results from our analysis where we present mean quarterly house price growth in treatment and control zip codes. The zip-code-level house price data come from CoreLogic. While the difference between control and treatment zip codes remains relatively stable before the program announcement, if anything, the gap between control and treatment groups shrinks from mid-2009. In other words, zip codes where more borrowers qualified for HAMP—and where they were more likely to receive HAMP modifications—did not see any meaningful relative increase in house prices after the program's introduction, at least in the near term. To get a better sense of magnitudes, we estimate a simple regression with quarterly house price growth as the dependent variable for the period 2008:Q3–2010:Q4. The estimates in Panel B of Table 8 (Column (1)) suggest no change in quarterly rate of house price decline is smaller for treatment zip codes after the HAMP program.

It is possible that part of this house price change could reflect a change in the composition of transacted properties in the treatment zip codes due to lower intensity of foreclosure sales. To assess the robustness with respect to this concern we repeat this exercise using CoreLogic house price index that excludes distressed transactions. The estimates using this series are presented in Column (2). Our inferences remain unchanged. Figure 5(b) demonstrates the same results.

VII.C Auto Sales, Delinquencies, and Employment

[27] As a verification test we examine if the inferences on foreclosure rates for the treatment and control groups of zip codes is similar to the individual loan-level analysis in Section V.B.2. In unreported tests we find that, relative to the foreclosure rate in the control group, the foreclosure rate in the treatment group declines after the program's introduction and the magnitude is broadly in line with our loan-level results.

Next, we investigate the impact of HAMP on other outcome variables, such as durable consumption (e.g., growth rates of auto sales), employment, and delinquencies on other credit categories for borrowers in the treatment and control zip codes. 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 as well as spur employment.

In Figure 5(c), we plot the growth rates in new auto sales in treatment and control zip codes, classified as in the previous section. We first note that there is no differential change in the growth rate of auto sales between treatment and control groups in the close vicinity of the program announcement. This suggests that treatment and control zip codes faced similar economic conditions around the introduction of the program—yielding further support to our empirical design. Figure 5(c) also shows no discernible change in auto sales in the zip codes affected by HAMP relative to those that are not.

Figure 5(d) plots the average employment growth in the treatment and control zip codes. Again, we find no significant relative change in employment growth between treatment and control zip codes after the program's introduction. Finally, we also find no significant change in the delinquency rates on credit cards (from the Federal Reserve Bank of New York's Credit Panel) in the control and treatment zip codes. For brevity, we do not report these results here. All the patterns discussed are robust to regressions of the form as reported in Panel B of Table 8.

These findings suggest that distressed borrowers who found mortgages to be more affordable after HAMP renegotiation did not significantly alter their other spending patterns, at least in the near term—possibly because of continued tight budget constraints. This suggests that the program did not result in other economic spillovers, at least in the near term.

There are two caveats with our analysis in Section VII. First, the low implementation rate of the program makes it difficult to detect its effects at the regional level. This should not be taken to imply that a meaningful decline in foreclosure rate would not impact house prices or other economic variables. Second, it is also possible that debt relief programs like HAMP may impact economic variables such as consumption at longer horizons than we are able to study.

VIII. The Role of Servicers

Our analysis suggests that the take-up rate, i.e., the number of trial modifications being granted and the conversion rate of trial modifications to permanent modifications, under HAMP was significantly lower compared with policymakers' expectations. Though 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.

VIII.A Heterogeneity in take-up rates across servicers under HAMP

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 by differences in contract, borrower, or regional characteristics of mortgages across servicers.[28] 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 (~ less than 30% to 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 by differences in contract, borrower, or regional characteristics of mortgages across servicers. This is illustrated in Figure 6(b) that plots the average quarterly permanent private modification rates across the servicing entities in our sample. These servicer specific rates in the pre-program period are obtained based on servicer fixed effects in a regression similar to column (4) of Table 2, Panel B, but estimated on pre-program data. The quarterly rates of permanent private modifications before the program vary across these servicing entities (~less than 0.04% to 1.4%).

In Panel A of Table 9 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 9 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

[28] See Glaeser, Gyourko and Siaz (2008) and Siaz (2010) for discussion on implications of geography for urban development and house prices.

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 9, 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 180% (126%) higher rate relative to the mean modification rate for servicers classified as 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 a permanent HAMP modification: the corresponding likelihood is higher by 1.73% in absolute terms.[29] 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 accounted by the heterogeneity in observable characteristics of loans in their servicing portfolio.

For completeness, in Panel B of Table 9, we also examine foreclosure decisions across servicers. The results in Columns (1) and (2) suggest that the foreclosure rate in the treatment group is lower for both the high- and low-experience servicers. However, the foreclosure rates are differentially lower for high-experience servicers relative to low-experience ones for delinquent loans. 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 substitution 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 substitution on intensive margin for both servicers. These results are not reported for brevity.

In sum, our findings indicate that 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[30], the differential and persistent patterns of renegotiation across servicers cannot be accounted for by these factors. This persistence in renegotiation behavior of servicers over time is in line with the finding that the productivity of firms is, in general, persistent (Syverson 2010).

[29] We also estimated specifications in Columns (5)–(7) of Panel A (Table 9) for trial HAMP modifications. The findings and inferences are similar to those for permanent modifications (unreported for brevity).

[30] 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.

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.

VIII.B Heterogeneity in pre-program organization design of servicers

We now investigate the potential reasons behind differences in the renegotiation activity of servicers before the program. One possibility is that contract, borrower and regional characteristics of mortgages vary significantly across servicers. This reason is unlikely because these patterns have been obtained using renegotiation intensity of servicers that already accounts for differences in contract, borrower, or regional characteristics of mortgages across servicers.

Another possibility that could explain the nature of servicer renegotiation experience in the pre-program period relates to organizational capability of the servicers themselves. Organizational factors, such as the quality of the work force, incentives, and technology, have been found to be responsible for differences in firm productivity across manufacturing firms (Syverson 2010). Recall, that 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. 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. Thus, in our context, these factors include the size of the servicing staff, the workload of the servicing staff, and the characteristics of servicing technology employed by the servicer.

In order to evaluate whether these organizational differences are related in any manner with the renegotiation experience of the servicer, we collect information on each servicer 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. We use the reports to extract information on the organizational variables of servicers around the introduction of the program. This information includes the characteristics of the servicing staff and the type of servicing technology used by the servicers.

In Table 10, we relate servicer organizational characteristics with pre-program renegotiation experience. As reported in Panel A, we find significant relationships between several variables. In particular, in Column (1), the number of full-time servicing staff is positively correlated with the intensity of renegotiations conducted by the servicer in the pre-program period. While this correlation is consistent with the notion that servicers with more renegotiation experience also have less-constrained servicing staff, it is possible that these servicers also service more loans. Therefore, we also evaluate how the number of loans serviced per number of full-time employees relates to the renegotiation experience of the servicer. Indeed, in Column (2), we find that servicers that conducted more renegotiations did have less-constrained staff, as measured by loans per full-time employee.

Next, we proxy for the quality of the servicing staff by the average training hours received by the staff. As reported in Column (3), we find that servicers with more renegotiation experience also are the ones who devote more hours to training their employees.

We also assess the nature of servicing processes employed by the servicers by evaluating the relation between renegotiation experience and two variables—the percentage of calls dropped by the servicer's call center and the average hold time in each customer call (in seconds). As shown 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 correlations are also visible in Panel B of Table 10 when we present the means in the servicer groups classified on the basis of pre-HAMP modifications.

Overall, our analysis suggests that the nature of renegotiation activity conducted by servicers before the program 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, these same servicers were able to extend modifications under the program.

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 largely 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 by differences in contract, borrower, or regional characteristics of mortgages across servicers. The fact that some other servicers, with similar portfolio 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 resulted in muted program 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.[31]

Our findings also suggest that the reallocation of resources that could help 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 commercial

[31] These factors may include costs of hiring and training of staff and setting up the requisite technological facilities to conduct renegotiations. And, these costs may display significant variation across intermediaries.

real estate sector, or through the entry of better and more capable servicers—must have faced significant hurdles. 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.[32] Admittedly, this would be a very naive computation, since it ignores other costs (or benefits) of program implementation, as well as any aggregate or redistributional 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.

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[32] 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).

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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 (July 2008 to March 2009) where treatment and control groups are formed on the basis of owner occupancy status (Strategy 1) and loan amount (Strategy 2). In particular, for the first strategy, the treatment group consists of owner-occupied loans, while the control group consists of investor-occupied loans. For the second strategy, 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. 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.

	Strategy 1 (Owner Occupied)				**Strategy 2 (Loan Amount)**			
	Control		*Treatment*		*Control*		*Treatment*	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
FICO	717	1.0	710	1.0	729	0.0	728	1.0
LTV %	70.3	0.4	70.6	0.2	64.5	0.1	65.6	0.1
Interest rate %	6.1	0.1	6.2	0.1	5.5	0.1	5.5	0.1
60+ delinquency % [Quarterly]	1.7	0.2	1.6	0.2	2.4	0.7	2.8	0.7
Private Permanent modifications % (all loans) [Quarterly]	0.3	0.1	0.4	0.2	0.6	0.2	0.6	0.2
Foreclosure complete % (all loans) [Quarterly]	0.4	0.001	0.3	0.001	0.2	0.1	0.2	0.001
Foreclosure complete % (delinquent loans) [Quarterly]	2.6	2.0	1.6	1.0	1.0	0.7	0.8	0.7
Number of loans	3,005,537		17,778,672		62,373		126,717	

Table 2: Trial and Permanent HAMP Modifications – Summary Statistics

The table presents summary statistics for the trial and permanent HAMP quarterly modification rates in the treatment group during the program period Q2:2009 to Q4:2010. Column (1) shows the results for the treatment group formed on the basis of owner occupancy status (Strategy 1) while Column (2) classifies treatment based on the loan amount (Strategy 2).

Panel A: Descriptive Statistics on Trial and Permanent HAMP Modifications

	Strategy 1 (Owner Occupied)	**Strategy 2 (Loan Amount)**
	Trial HAMP Modifications	
Modification rate (%) [Quarterly]	0.432	0.565
Number of Trial HAMP modification	522,365	4,489
	Permanent HAMP Modifications	
Modification rate (%) [Quarterly]	0.165	0.226
Number of Permanent HAMP modification	199,515	1,796
Conversion Rate: Trial to Permanent HAMP	38.2%	40.04%
Number of Loans	17,273,971	113,493

Table 2 (contd.): 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 modification was offered to a loan and various borrower and contract level characteristics. The sample includes those that are eligible 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 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. The *FICO* is the variable capturing the borrower's credit score at loan origination. The *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 zero 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. 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 (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).

Panel B: Trial and Permanent HAMP Modifications and Borrower and Contract Characteristics

	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	-0.021	-0.008	-0.008
	(9.70)	(9.42)	(7.80)	(7.58)
LTV	1.167	2.397	0.426	0.954
	(4.29)	(4.14)	(4.15)	(3.92)
Interest Rate	0.356	0.385	0.069	0.081
	(3.64)	(4.67)	(1.61)	(2.27)
Origination Amount	0.003	0.001	0.001	0.0004
	(4.56)	(1.07)	(5.38)	(1.15)
Low Doc	0.781	0.720	0.326	0.303
	(11.44)	(11.30)	(6.67)	(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 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. The control 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 control 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 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.

	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	0.213	0.209	0.205
	(8.39)	(8.40)	(6.43)	(7.21)
T* After	0.014	0.021	0.020	0.144
	(1.3)	(1.27)	(1.27)	(5.40)
After	0.471	0.454	0.453	0.492
	(12.57)	(13.03)	(13.33)	(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 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 is 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. 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 control 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 control 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 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	1.23	1.12	-7.14
	(4.40)	(0.70)	(2.91)	(4.80)
T* After	-11.14	-9.33	-2.16	9.76
	(2.52)	(2.22)	(3.30)	(3.61)
After	29.63	17.53	3.73	8.18
	(6.81)	(4.30)	(3.02)	(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 (contd.):

Panel B: Redefault conditional on Modification

	Dependent variable: Whether a modified loan re-defaults within six months after receiving a modification				
	(1)	(2)	(3)	(4)	(5)
T	0.15	0.39	0.42	0.49	0.46
	(1.21)	(2.02)	(2.11)	(3.13)	(3.24)
T* After	-0.02	-0.14	-0.18	-0.35	-0.35
	(0.21)	(1.02)	(1.31)	(2.30)	(2.30)
After	-0.21	-0.05	0.04	0.28	0.27
	(1.31)	(0.21)	(0.13)	(1.30)	(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

The table presents OLS estimates from regressions that track whether or not a loan was foreclosed around the program implementation. The sample consists of all loans in columns (1)–(3) and delinquent loans in columns (4)–(6). 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 control 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 control 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 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. *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.

	Dependent variable: Whether a loan was foreclosed in a quarter					
	Sample: All loans			Sample: Delinquent loans		
	(1)	(2)	(3)	(4)	(5)	(6)
T	-0.164	-0.133	-0.120	-1.32	-0.619	-0.603
	(5.21)	(3.72)	(3.55)	(2.36)	(1.08)	(1.43)
T* After	-0.127	-0.126	-0.129	-2.03	-1.92	-1.96
	(3.13)	(3.13)	(3.15)	(5.39)	(5.45)	(5.64)
After	0.364	0.372	0.372	3.858	3.808	3.936
	(5.03)	(5.15)	(5.17)	(6.03)	(6.04)	(6.63)
Observations	178,917,320	178,917,320	178,917,320	13,658,925	13,658,925	13,658,925
Adj. R-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 6: Delinquency

The table 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 control 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 control 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 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 the parentheses. The estimates are expressed in percentage terms.

	Dependent variable: Whether a loan becomes delinquent in a quarter			
	(1)	(2)	(3)	(4)
T	-0.132	0.013	-0.039	-0.02
	(1.33)	(0.14)	(0.32)	(0.32)
T*After	0.088	0.035	0.024	0.027
	(1.81)	(0.72)	(0.52)	(0.43)
After	-0.245	-0.147	-0.143	-0.171
	(2.80)	(1.81)	(1.70)	(1.83)
Observations	179,871,929	179,871,929	179,871,929	179,871,929
Adj. *R*-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 7: Alternative Empirical Strategy -- Modifications, Redefault Rates, and Foreclosure Rates

The table presents OLS estimates from regressions that track whether or not a 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. 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 control variable *T* takes the value of 1 if a loan belongs to the treatment group as defined by Strategy 2 (loan balance cutoff) and is 0 otherwise. The control 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 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 are clustered at the state level; *t*-statistics are in parentheses. The estimates are expressed in percentage terms.

	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 re-defaults 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	0.01	-3.54	-0.04	-0.16
	(0.22)	(0.43)	(2.01)	(2.86)	(1.21)
T* After	0.06	0.215	1.54	-0.03	-0.59
	(1.88)	(2.34)	(1.23)	(1.67)	(3.11)
After	0.64	0.69	-14.64	0.62	3.23
	(2.53)	(2.61)	(7.62)	(3.13)	(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 8: Foreclosures, House Price Growth and Auto Sales – Zipcode Level Analysis

Panel A reports OLS estimates of regression where the dependent variable is the fraction of modified loans under the program in a zip code during Q2 2009 to Q4 2010 period. Column (1) consists of all zipcodes with house price indexes while Column (2) presents the regression for bottom and top quartile of zip codes in terms of % owner-occupied loans. The control variable is a fraction of owner-occupied homes (with outstanding mortgage) in a zip code as of March 2009. Panel B reports OLS estimates of regressions evaluating the relationship between exposure to HAMP in a zipcode and quarterly house price growth rate, auto sales growth rate and employment growth rate around the implementation of HAMP. *T* takes the value of 1 if a zipcode belongs to the treatment group (top quartile of % owner-occupied loans) and is zero otherwise. The control variable *After* takes the value of 1 for the quarters after Q1 2009 and is zero otherwise. Estimation period is 2008:Q3-2010:Q4; Standard errors are clustered at the state level in Panel B; *t*-statistics are in parentheses. The estimates are expressed in percentage terms.

Panel A: Zip Code Ex Post HAMP Modifications and Ex Ante Exposure to HAMP (% Owner-Occupied Loans)

	Fraction of modified loans under HAMP during the sample period	
	(1)	(2)
% Owner-Occupied	0.0230	0.0225
	(6.57)	(5.70)
State Fixed Effects	Yes	Yes
Number of Zipcodes	6,616	3,308
Adj. R-squared	0.253	0.211

Panel B: Zip Code Outcomes and Ex Ante Exposure to HAMP

	HPI growth rate	HPI growth rate (Distressed sales excluded)	Auto sales growth rate	Employment growth rate
	(1)	(2)	(3)	(4)
T	0.850	0.839	1.801	-0.0897
	(1.16)	(1.45)	(1.55)	(-0.44)
T*After	-0.156	-0.336	-1.520	0.125
	(-0.28)	(-0.87)	(-0.78)	(0.57)
After	3.921	2.726	15.23	1.442
	(7.06)	(6.80)	(17.68)	(9.74)
Number of Observations	33080	28050	33080	33080
Pre-HAMP Mean	-3.89	-2.79	-5.77	-1.59
Adj. R-squared	0.209	0.147	0.00447	0.101

Table 9: Heterogeneity Using Servicer Pre-HAMP Renegotiation Experience

In Panel A, 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) of Panel A show the results for treatment loans as defined by Strategy 1. Column (5) and Column (6) of Panel A show the results for treatment loans (as defined by Strategy 1) in California and Florida, respectively. Column (7) of Panel A shows the results for treatment loans as defined by Strategy 2. In Columns (1) and (2) of Panel B the dependent variable is 1 in the quarter a given loan is foreclosed. Column (1) of Panel B shows the results for all loans, while Column (2) of Panel B shows the results for the sample of delinquent loans. 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 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. In Panel B, *T* takes a value of 1 if a loan belongs to the treatment group as defined by Strategy 1 (owner-occupied) and is 0 otherwise. *After* takes a value of 1 for the quarters after Q1 2009 and is 0 otherwise. The additional controls in Panel B also include interaction terms of *High Experience* dummy with *T* and *After*. 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 the 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	1.15	0.92	0.98	2.41	1.72	1.738
	(3.33)	(3.35)	(4.08)	(4.03)	(4.39)	(4.12)	(5.71)
Observations	17,273,971	17,273,971	17,273,971	17,273,971	2,848,540	1,113,040	126,717
Adj. R-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 9 (contd.):

Panel B: Foreclosures and Servicer Pre-HAMP Renegotiation Experience

	All loans	Delinquent loans
	Dependent variable: Whether a loan is foreclosed in given quarter	Dependent variable: Whether a loan is foreclosed in given quarter
	(1)	(2)
T	-0.072	-1.495
	(2.73)	(2.84)
T* After	-0.126	-1.762
	(2.28)	(4.48)
T* After*High Experience	-0.064	-1.110
	(2.03)	(2.10)
After	0.372	4.062
	(4.73)	(6.25)
Observations	179,871,929	13,658,925
Adj. R-squared	0.008	0.02
Other Controls & Origination FE	Yes	Yes
State FE	Yes	Yes

Table 10: Servicer Pre-HAMP Renegotiation Experience and Servicer Organization Variables

In Panel A, 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. In Panel B, we present the means in two groups of servicers based on their prior renegotiations. The groups are sorted based on *High Experience* dummy that takes the value of 1 if a loan is serviced by a servicer whose estimated renegotiation intensity in the pre-HAMP period (*pre-HAMP mod rate*) is above median and is 0 otherwise. *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. *Average hold time* refers to the hold time (in seconds) a customer has to wait on a servicing call.

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

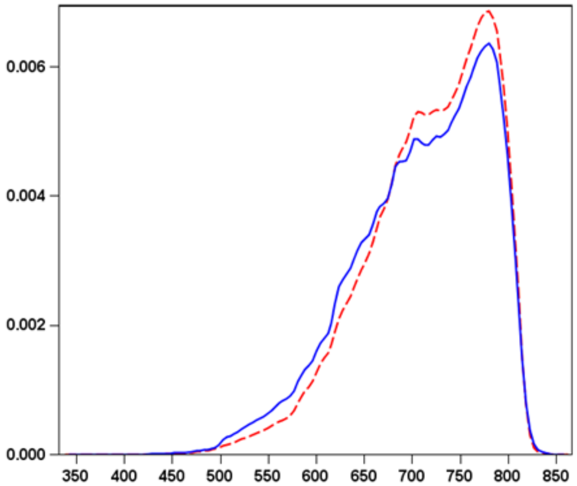
	Full time staff (FTE)	Loans per FTE	Average Tranining Hours	% Call Dropped	Phone hold time (sec)
	(1)	(2)	(3)	(4)	(5)
pre-HAMP mod rate	52%	-57%	14%	-43%	-49%

Panel B: Mean of Servicer Organization Variables in Groups Formed Based on Servicer Pre-HAMP Renegotiation Experience

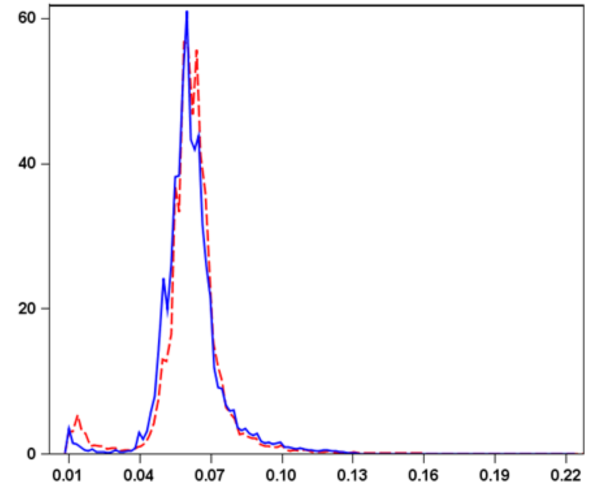
	Full time staff (FTE)	Loans per FTE	Average Tranining Hours	% Call Dropped	Phone hold time (sec)
	(1)	(2)	(3)	(4)	(5)
High Experience =1	4118	1216	87	2.7	42
High Experience =0	1129	2235	61	4.7	64
Difference	2989	-1019	26	-2.0	-22

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

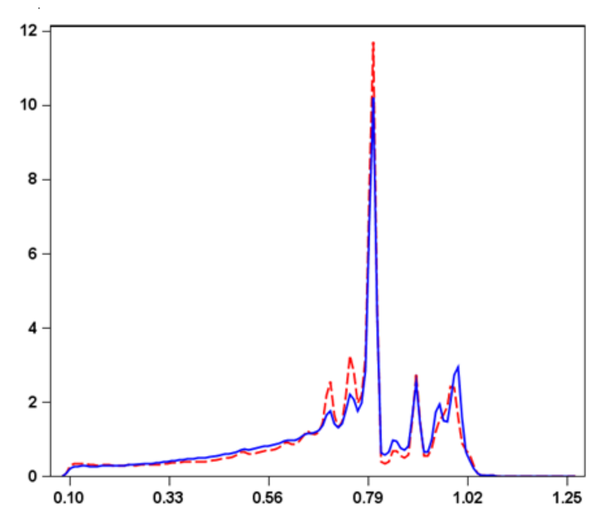
The figure shows the kernel density plots for loan origination FICO credit score (Figures (a) and (d)), interest rate (Figures (b) and (e)), and LTV (Figures (c) and (f)) in the treatment and control groups. Figures (a)–(c) define these groups based on Strategy 1 (owner occupancy), while Figures (d)–(f) define these groups based on Strategy 2 (loan amount). The plots are based on pre-program data. The treatment group is represented by solid line, and the control group is represented by dashed line.



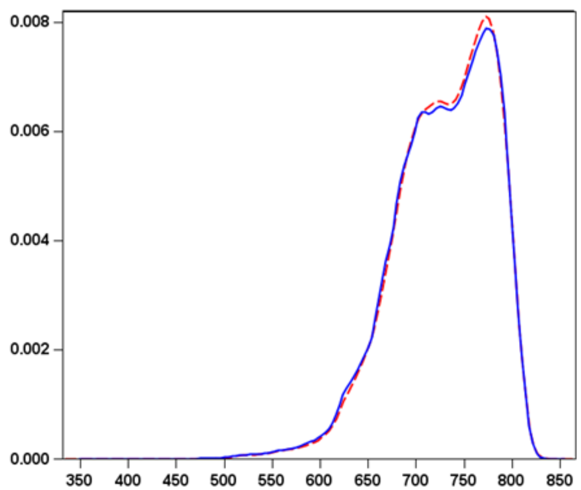
1(a): FICO credit score (Strategy 1)



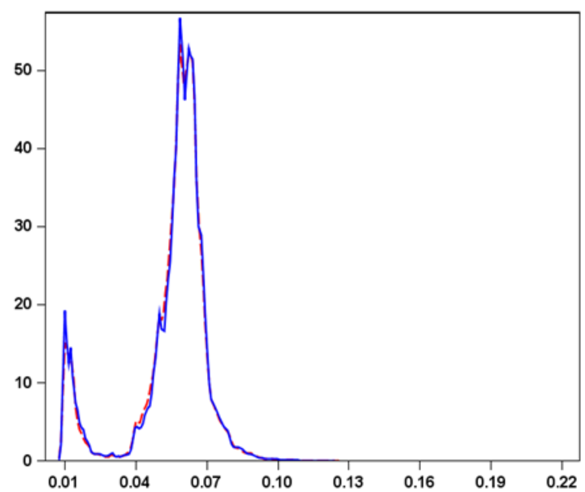
1(b): Interest rate (Strategy 1)



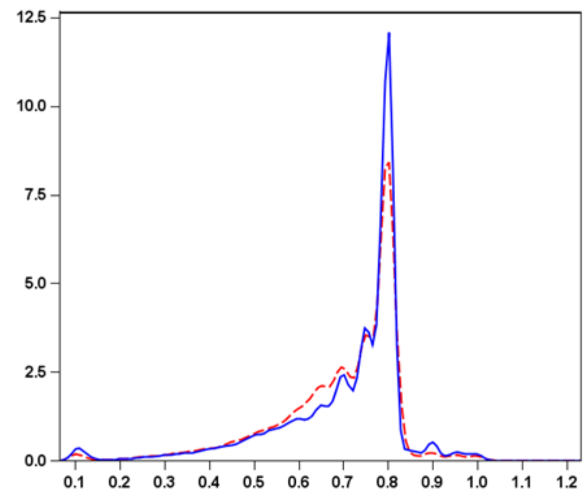
1(c): LTV (Strategy 1)



1(d): FICO credit score (Strategy 2)



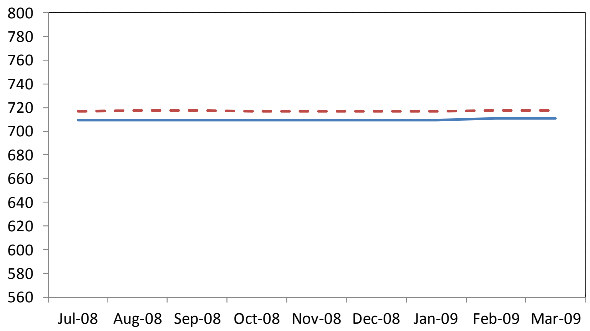
1(e): Interest rate (Strategy 2)



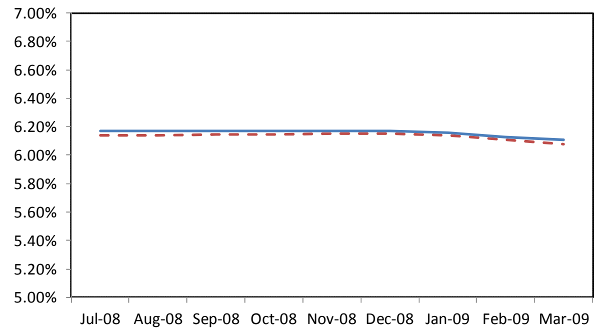
1(f): LTV (Strategy 2)

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

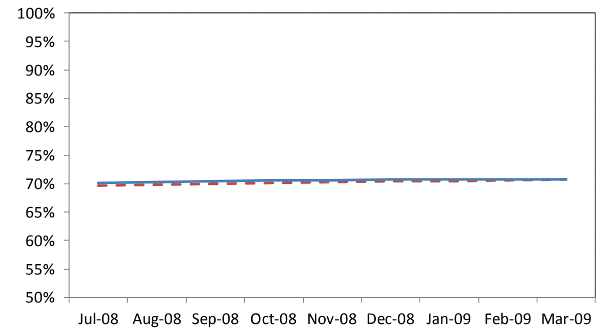
The figure shows the monthly evolution of mean origination FICO credit score (Figures (a) and (d)), interest rate (Figures (c) and (e)), and LTV (Figures (c) and (f)) in the treatment and control groups. Figures (a)–(c) define these groups based on Strategy 1 (owner occupancy), while Figures (d)–(f) define these groups based on Strategy 2 (loan amount). The plots are for the pre-program period. The treatment group is represented by solid line, and the control group is represented by dashed line.



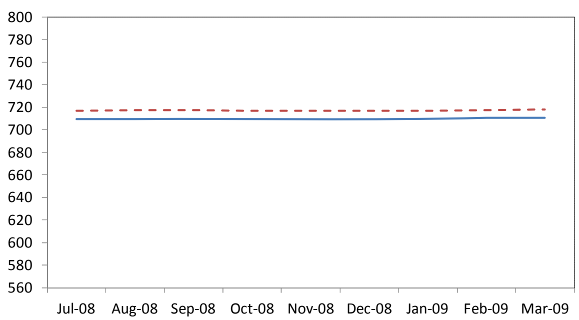
2(a): FICO credit score (Strategy 1)



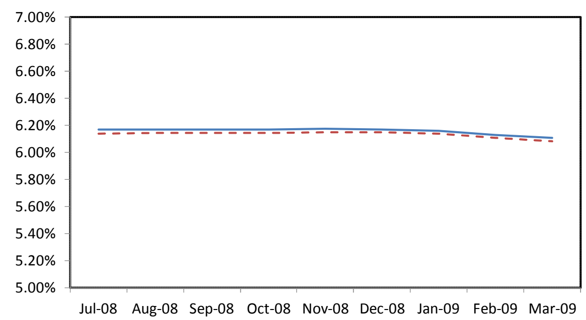
2(b): Interest rate (Strategy 1)



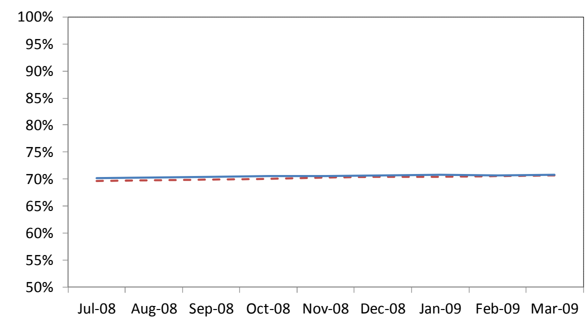
2(c): LTV (Strategy 1)



2(d): FICO credit score (Strategy 2)



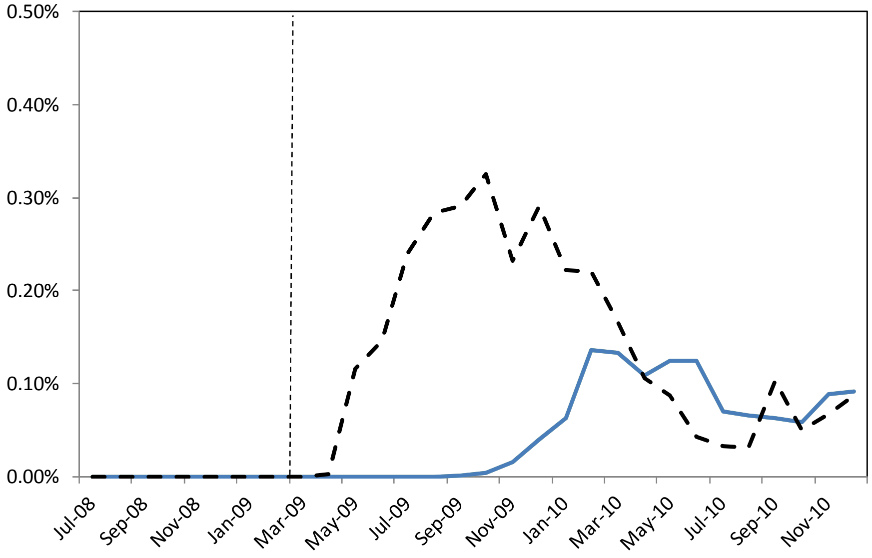
2(e): Interest rate (Strategy 2)



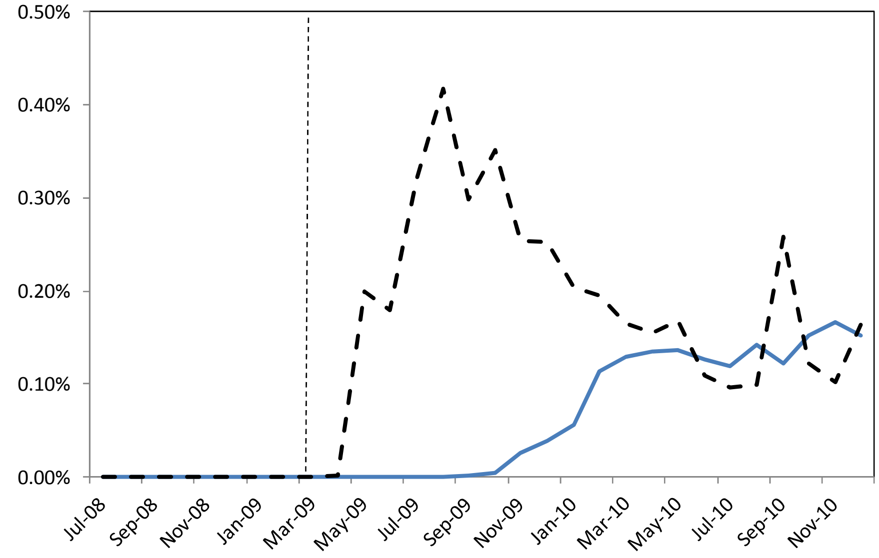
2(f): LTV (Strategy 2)

Figure 3: Evolution of Monthly Trial and Permanent HAMP Modification Rate

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. Figure (a) defines the treatment group using Strategy 1 (owner-occupancy status), while Figure (b) defines the treatment group using Strategy 2 (loan amount).



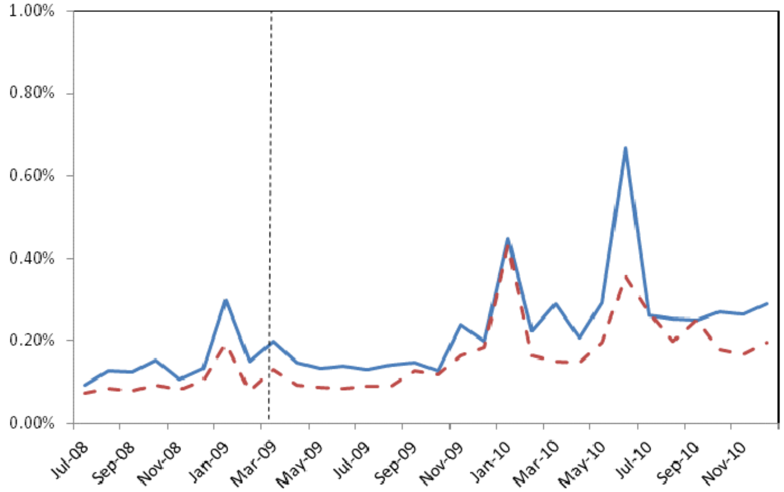
3(a): Trial and permanent HAMP modification (Strategy 1)



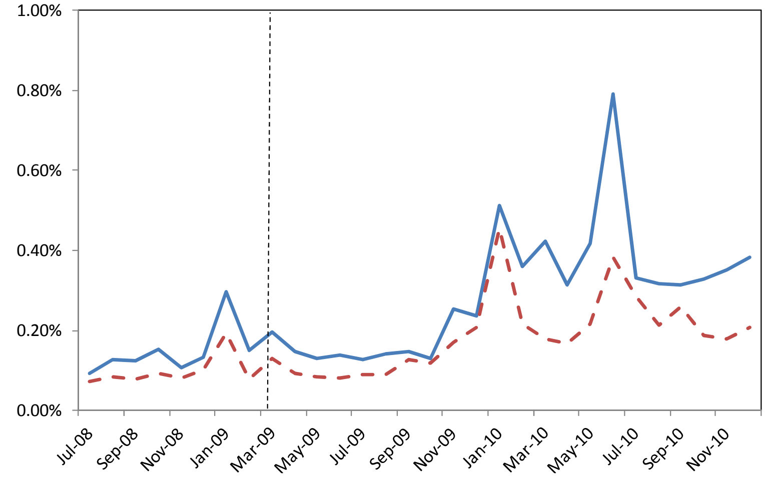
3(b): Trial and permanent HAMP modification (Strategy 2)

Figure 4: Evolution of Monthly Private Permanent and Combined Permanent (Private and HAMP) Modification Rate

The figure shows the percentage of loans receiving a permanent private modification for the first time in a given month in the treatment and control groups (Figure (a) and (c)) and the percentage of loans receiving a combined permanent modification (private and HAMP) in the treatment and control groups (Figures (b) and (d)) during the period from July 2008 to December 2010. Figures (a) and (b) show the treatment and control groups defined based on Strategy 1, while Figures (c) and (d) show the results for treatment and control groups defined based on Strategy 2. The treatment group is represented by solid line, and the control group is represented by dashed line.

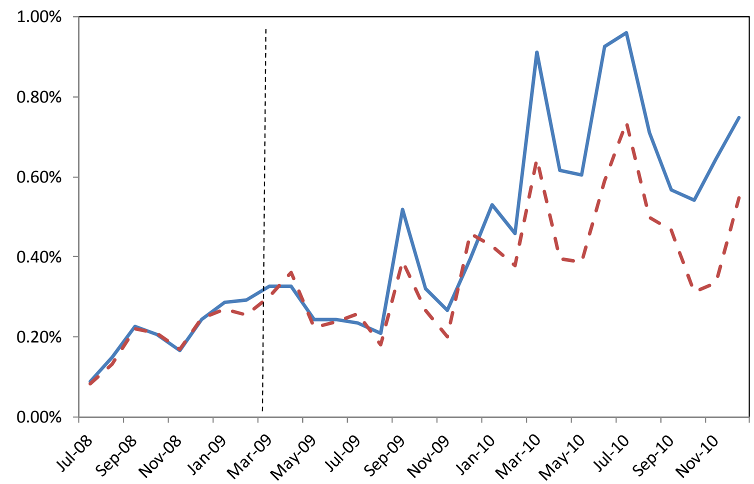


4(a): Private permanent modification (Strategy 1)



4(b): Combined private and HAMP permanent modification (Strategy 1)

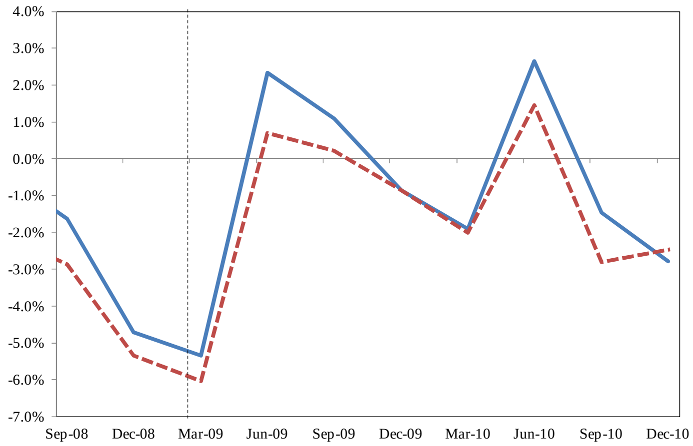
4(c): Private permanent modification (Strategy 2)



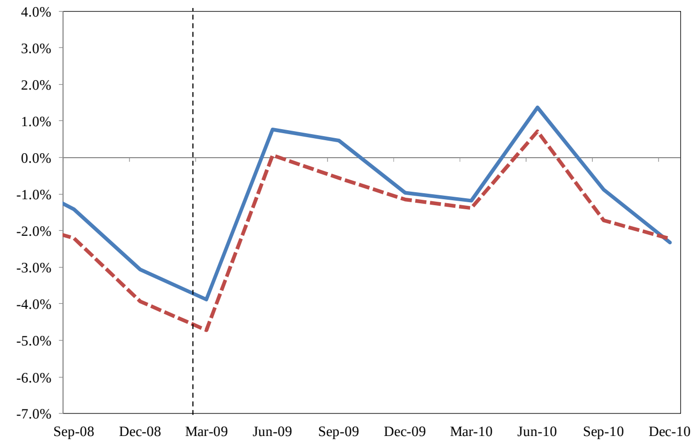
4(d): Combined private and HAMP permanent modification (Strategy 2)

Figure 5: Evolution of Quarterly HPI Growth, Auto Sales, and Employment Growth Rates in Regions sorted by HAMP Exposure

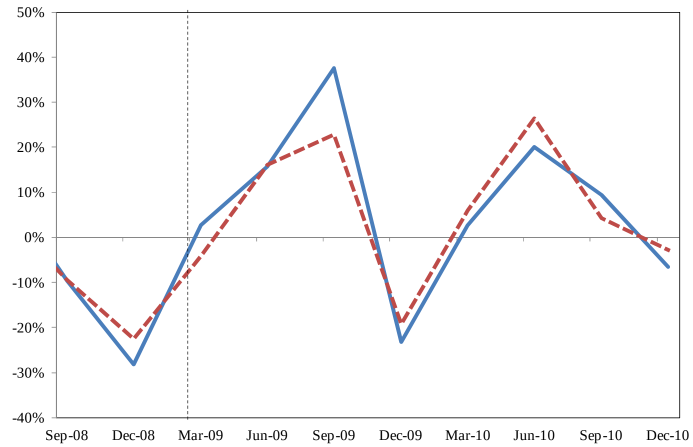
The figure shows the average house price growth rates (Figures (a) and (b)), auto sales growth (Figure (c)), and employment growth (Figure (d)) in the treatment and control zip codes from Q2:2008 to Q4:2010. The treatment and control groups are defined based on the share of owner-occupied loans (ex ante exposure to HAMP). Zip-code-level house price growth is computed using CoreLogic (Figure (a)) and CoreLogic excluding distressed sales (Figure (b)) price indices, and auto sales growth data come from Mian and Sufi (2010). The employment growth data are from the BLS. Treatment group is represented by solid line, and the control group is represented by dashed line.



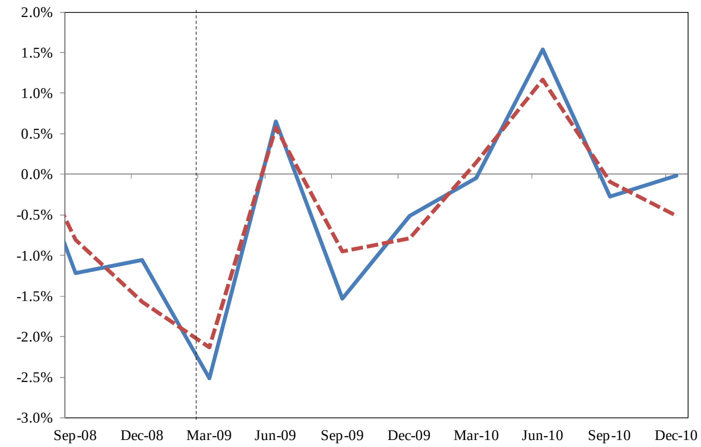
5(a): House price growth



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



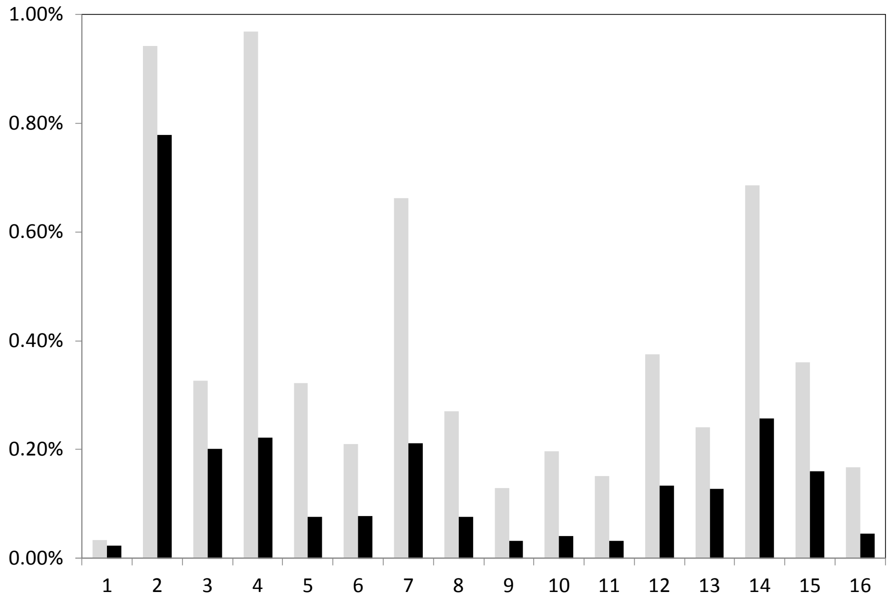
5(c): Auto sales growth



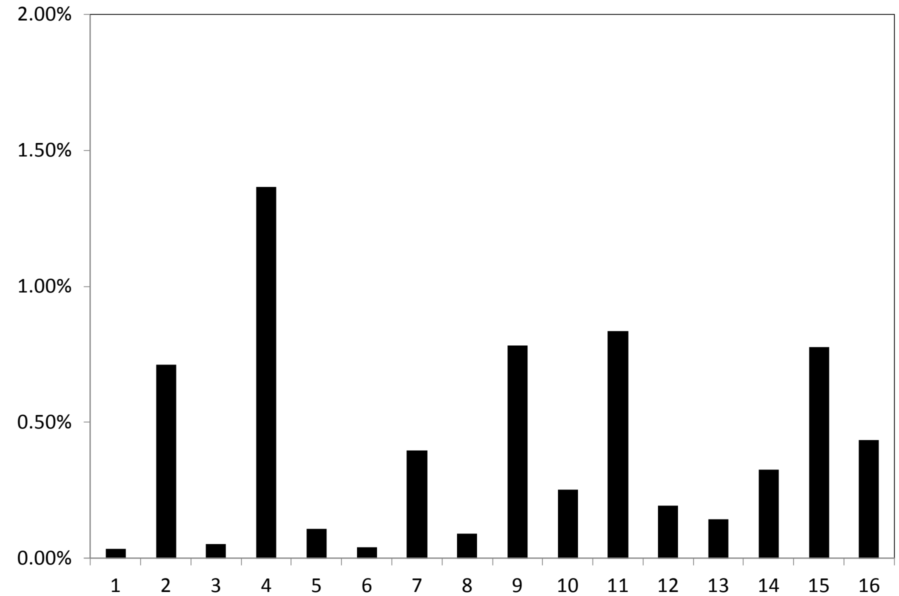
5(d): Employment growth

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