

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

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At least since the Great Depression, federal and state governments have regularly intervened in the functioning of mortgage markets during economic crises—through household debt relief and foreclosure prevention policies. 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, but also generate negative externalities for the society. Moreover, these policies also help reduce high levels of debt that may distort household consumption and investment decisions. On the other hand, critics argue that such policies potentially generate moral hazard problems that are likely to raise the cost of credit in the long run, and may also have undesirable redistributive consequences. Remarkably, despite the economic importance of and controversy surrounding such interventions, empirical evidence on the consequences of such policy programs is scant.

This paper attempts to fill this gap by empirically evaluating the effects of the largest government intervention -- the Home Affordable Modification Program (HAMP) -- concerning mortgage debt renegotiation in the aftermath of the recent crisis. The program provided large financial incentives<sup>1</sup> 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 internalize negative externalities imposed by foreclosures and the frictions induced by non-agency securitization.

We use the unique MortgageMetrics data set from the Office of the Comptroller of the Currency (OCC). This loan level panel data set contains information on loan and borrower attributes for more than 60% of outstanding residential mortgages in the United States, including precise information on loan payments and renegotiation actions taken (e.g., principal reduction), whether the renegotiation was undertaken under HAMP, as well as the servicer responsible for the mortgage.

The biggest obstacle 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

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<sup>1</sup> 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).

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).

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.

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. In addition, 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%.<sup>2</sup> 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

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<sup>2</sup> 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.

of HAMP modifications, resulting in no change in the average effectiveness of modifications in the treatment group around the program.

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 of significant changes in growth rates of house prices, auto sales and employment, or in delinquency rates on credit card loans and auto loans in regions more exposed to the program.

While it is difficult to know what the optimal response to the program incentives should have been, in the next 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. 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.