The Impact of Loan Modifications on Repayment, Bankruptcy, and Labor Supply: Evidence from a Randomized Experiment*

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July 2015

Abstract

This paper estimates the impact of ex-post loan modifications on repayment, bankruptcy, and employment using a randomized experiment merged to administrative data. A large non-profit credit counseling organization and eleven unsecured creditors offered lower interest rates and longer repayment periods to a random subset of 80,000 distressed borrowers. Borrowers offered a lower interest rate were more likely to repay their debts and less likely to file for bankruptcy. For the most heavily indebted borrowers, lower interest rates also increased the probability of being employed. In contrast, there was little impact of a longer repayment period on debt repayment, bankruptcy, or employment.

*We are extremely grateful to Ann Woods and Robert Kaplan at Money Management International, David Jones at the Association of Independent Consumer Credit Counseling Agencies, Ed Falco at Auriemma Consulting Group, and Gerald Ray and David Foster at the Social Security Administration for their help and support. We thank Tal Gross, Matthew Notowidigdo, and Jialan Wang for providing the bankruptcy data used in this analysis. We also thank Hank Farber, John Friedman, Paul Goldsmith-Pinkham, Tal Gross, Ben Keys, Patrick Kline, Alex Mas, Conrad Miller, Roland Rathelot, Jesse Shapiro, Steve Woodbury, Crystal Yang, Seth Zimmerman, Jon Zinman, Eric Zwick, and numerous seminar participants for helpful comments and suggestions. Samsun Knight, Kevin Tang, and Daniel Van Deusen provided excellent research assistance. Financial support from the Washington Center for Equitable Growth is gratefully acknowledged. Correspondence can be addressed to the authors by e-mail: wdobbie@princeton.edu [Dobbie] or jae.song@ssa.gov [Song]. Any opinions expressed herein are those of the authors and not those of the Social Security Administration.

More than 14 percent of American consumers have a debt in collections and more than $200 billion in non-mortgage debt goes into default each year (Federal Reserve Bank of New York 2014). There are significant deadweight losses to such financial distress. Lenders must either write off delinquent debt or attempt to collect it directly or through wage garnishment and asset seizure orders. Borrowers, in turn, have an incentive to avoid these collection efforts through potentially costly strategies, such as changing jobs to force creditors to reinstate a garnishment order or leaving the formal banking system to hide their assets from seizure.[1] In theory, ex-post loan modifications can benefit both borrowers and lenders by completing debt contracts and lowering default rates (e.g. Bolton and Rosenthal 2002, Bolton and Scharfstein 2006). To date, however, there is little empirical evidence that loan modifications provide economically significant benefits to either party.[2]

This paper estimates the impact of two different kinds of ex-post loan modifications on repayment, bankruptcy, and employment using a large randomized experiment merged to administrative data. The experiment was designed and implemented by the largest non-profit credit counseling organization in the United States. Eleven unsecured creditors agreed to offer lower interest rates and lower minimum monthly payments to approximately 40,000 distressed borrowers that contacted the non-profit organization between January 2005 and August 2006. The median interest rate reduction of 3.69 percentage points shortened the typical borrower's repayment period by about four months and decreased the total amount to be repaid by $1,712. In contrast, the median monthly payment reduction of $26.68 (or 0.14 percent of initial debt) lengthened the typical borrower's repayment period by four months and increased the total amount to be repaid by $289.

We separately identify the effects of lower interest rates and longer repayment periods using two

[1] There is also evidence that financial distress imposes negative externalities on nearby individuals. For example, home foreclosures can reduce nearby home values (e.g. Campbell, Giglio, and Pathak 2011, Mian, Sufi, and Trebbi forthcoming) and consumer debt overhang can depress regional consumption and employment (e.g. Guerrieri and Lorenzoni 2011, Hall 2011, Midrigan and Philippon 2011, Eggertson and Krugman 2012, Farhi and Werning 2013, Mian, Rao, and Sufi 2013, Mian and Sufi 2014).

[2] Of course, the availability of ex-post loan modifications may also have important ex-ante effects. There is a large literature documenting the ex-ante effects of debtor protections in a variety of contexts. Mayer et al. (2014) find that distressed homeowners respond strategically to news of mortgage modification programs. Pence (2006) finds that mortgage sizes are three to seven percent smaller in states with foreclosure laws that are more debtor friendly. Ghent and Kudlyak (2011) find that borrowers are more likely to default in non-recourse states. Gropp et al. (1996) and Lin and White (2001) examine the cross-sectional relationship between bankruptcy laws and borrowing costs, while Severino, Brown, and Coates (2014) use within-state variation in bankruptcy law to show that that an increase in Chapter 7 exemption levels increases unsecured borrowing. Li et al. (2011) and Kuchler and Stroebel (2009) show that bankruptcy exemption levels also affect mortgage default, and Li, Tewari, and White (2014) find that mortgage strip-down affects both interest rates and approval rates. See Bolton and Rosenthal (2002) for a discussion of the ex-ante and ex-post efficiency of debt relief when debt contracts are incomplete.

unique features of the randomized experiment. First, each of the eleven creditors participating in the experiment offered a different bundle of interest rate and monthly payment reductions. Second, individual borrowers in our sample owed different amounts to these creditors. As a result, otherwise similar borrowers received very different interest rate and monthly payment reductions. This sizable cross-borrower variation allows us to isolate the effects of each modification by comparing the impact of the randomized experiment across borrowers that differed in their "potential treatment intensity," that is, the interest rate and monthly payment reductions that they would have received if treated.

We measure the effects of the randomized experiment using three administrative datasets matched for the purposes of this study. Debt repayment is measured using data from the credit counseling organization, which recorded enrollment in and completion of the structured repayment program offered during the experiment. Financial distress is measured using court bankruptcy records. Labor supply and 401k contributions are measured using tax data from the Social Security Administration (SSA). The matched dataset allows us to estimate the effects of the loan modifications on a wide range of outcomes up to five years after the experiment.[3]

In our empirical analysis, we find compelling evidence that lower interest rates (i.e. shorter repayment periods and lower repayment costs) had significant ex-post benefits for both lenders and borrowers. Borrowers offered the median interest rate reduction were 1.77 percentage points more likely to complete a structured repayment plan, a 14.83 percent increase from the control group mean of 11.93 percent. Back-of-the-envelope calculations suggest that the interest rate reductions subsequently increased lender profits by about $23 per borrower. We also find that borrowers offered lower interest rates were also 1.07 percentage points less likely to file for bankruptcy over the first five post-experiment years, a 10.35 percent decrease from the control group mean of 10.36 percent. For borrowers with above median debt-to-income ratios, lower interest rates increased the probability of completing repayment by 3.25 percentage points, decreased the probability of filing for bankruptcy protection by 1.36 percentage points, and increased the probability of being

[3]It is important to note that our measure of debt repayment is based on records from the credit counseling organization. We are unable to measure any repayment that occurs outside of the experiment, such as to a third-party debt collector or the original creditor. Second, our bankruptcy data only capture one particularly severe form of financial distress. Loan modifications may influence many other forms of financial distress not captured in our data, such as loan delinquency or the amount of debt in collections. To partially address these concerns, we are in the process of adding individual-level credit bureau records to our data.

employed by 1.70 percentage points. There were no detectable effects of lower interest rates on earnings or 401k contributions for any borrowers in our sample.

Conversely, we find no positive effects of lower monthly payments (i.e. longer repayment periods and higher repayment costs). Borrowers offered the median monthly payment reduction of 0.14 of initial debt were neither more or less likely to complete a structured repayment plan, with the 95 percent confidence interval ruling out effects larger than 1.5 percentage points. The median monthly payment reduction also *increased* the probability that a borrower filed for bankruptcy over the first five years by a statistically insignificant 0.70 percentage points, a 6.75 percent increase from the control group mean. For borrowers with above median debt-to-income ratios, employment also *decreased* by 1.68 percentage points, a 2.14 percent change from the control group mean of 82.02 percent.

Our results suggest that liquidity constraints are not an important driver of borrower behavior in our data, or that a longer repayment period is an ineffective way to alleviate these liquidity constraints. These findings stand in sharp contrast to an influential literature that has documented liquidity constraints in a variety of contexts (e.g. Gross and Souleles 2002, Johnson, Parker, and Souleles 2006, Agarwal et al. 2007, Parker et al. 2013). For example, there is evidence that reductions in a borrower's mortgage payments induced by anticipated rate resets both decreases the probability of mortgage default and increases the amount of non-durable consumption (e.g. Di Maggio, Kermani, and Ramcharan 2014, Keys et al. 2014, Fuster and Willen 2015). Importantly, there is no evidence of forward looking behavior before the rate resets, suggesting that borrowers are so liquidity constrained that they cannot prevent involuntarily defaults just before the monthly payment reduction.

There are at least three possible explanations for the lack of evidence for liquidity constraints in our setting. First, we estimate the impact of lower monthly payments on unsecured credit card debt, not secured mortgage debt. It is possible that liquidity constraints are a more important determinant of repayment behavior on mortgage debt, while strategic concerns dominate for unsecured credit card debt. Second, a longer repayment period for unsecured credit card debt has a more modest impact on disposable earnings compared to a mortgage rate reset. The median monthly payment reduction in our sample was \$26.68, while the mortgage rate resets examined in the prior literature decreased monthly payments by up to \$900 (Di Maggio, Kermani, and Ramcharan 2014). It is possible that borrowers only benefit from monthly payment reductions large enough to deal with particularly severe liquidity shocks, such as those created by a job loss or uninsured medical emergency. Finally, it is possible that the results documented in the mortgage rate reset literature are due to borrower inattention or ignorance about the rate reset, not liquidity constraints. While lenders are supposed to inform borrowers by mail before the index rate changes, it is not clear if borrowers read or understand these notices.

Our paper is also closely related to recent work examining the impact of mortgage modifications on borrower outcomes. There is evidence that mortgage modifications made through the Home Affordable Modification Program modestly decreased foreclosure rates and default on non-mortgage debt, although it is unclear whether the effects were driven by lower interest rates, principal reductions, or term extensions (Agarwal et al. 2012). However, cross-sectional comparisons suggest that there are larger decreases in re-default rates when monthly mortgage payment reductions are achieved through principal forgiveness (Haughwout, Okah, and Tracy 2010), and recent theoretical work suggests that payment deferrals are likely to increase the probability of default unless paired with some sort of principal forgiveness or lower interest rates (Eberly and Krishnamurthy 2014). Our results are broadly consistent with these descriptive findings, although in a very different context.

This paper is also related to an emerging literature estimating the effects of debt relief provided by consumer bankruptcy protection. There is evidence that the debt relief provided by consumer bankruptcy protection can increase labor supply and decrease both mortality risk and financial distress (Dobbie and Song 2015, Dobbie, Goldsmith-Pinkham, and Yang 2015), and that the consumer bankruptcy system can provide implicit health insurance (Mahoney 2015) and generate positive spillovers during a financial crisis (Dobbie and Goldsmith-Pinkham 2014). However, none of these papers are able to identify the precise mechanisms through which bankruptcy protection benefits debtors, or whether lenders might also benefit from ex-post debt relief.

The remainder of this paper is structured as follows. Section I describes the institutional setting and experimental design. Section II details the data used in our analysis. Section III presents our empirical design and main results. Section IV concludes.

I. Background and Experimental Design

A. Background

The randomized trial was implemented by Money Management International (MMI), the largest non-profit credit counseling agency in the United States. Founded in 1958, MMI provides financial guidance, credit counseling, bankruptcy counseling, and housing counseling to its clients via phone and in-person sessions. In 2013, MMI counseled over 160,000 clients and conducted over 2,000 community educational programs.

The main product MMI offers is a debt management plan (DMP), a structured repayment program that simultaneously repays all of a borrower's unsecured creditors. Enrolled borrowers make a single monthly payment to MMI that is then disbursed to each unsecured creditor. The monthly payment also includes a small fee that partially covers the costs of administering the plan, with the remaining costs covered by "fair-share" payments from creditors that are proportional to the amount of debt repaid. In exchange for voluntarily enrolling in the repayment program, creditors will reduce the borrower's monthly payments, lower or eliminate interest payments and late fees, and stop recording the debt as delinquent. The entire repayment process usually takes about three to five years, with the exact length depending on the terms offered by creditors and the amount of debt to be repaid. The monthly payment to each creditor typically ranges from two to three percent of the initial debt. In our sample, the average monthly payment for the control group is 2.38 percent of initial debt holdings, or about $437.

Creditor participation in the repayment program is voluntary. From a creditor's perspective, there are at least two reasons to prefer the DMP to outside options such as insisting on full repayment or negotiating a bilateral workout with the borrower. First, the DMP allows participating creditors to internalize many of the externalities associated with bi-lateral loan modifications, including positive effects on the ability to repay debts and negative effects on the incentive to repay non-modified debts. Second, MMI credit counselors screen borrowers on behalf of all of the participating creditors, eliminating the need for each creditor to conduct their own eligibility screens.

Creditors have a number of options to collect the unpaid debt if a delinquent borrower does not enroll in a repayment program or fails to make the required payments. These options include collection letters or phone calls, in-person visits at home or work, wage garnishment orders, and asset seizure orders (Hynes, Dawsey, and Ausubel 2013, Dobbie and Song 2015). Borrowers can make these collection efforts more difficult by ignoring collection letters and calls, changing their telephone number, or moving without leaving a forwarding address. Borrowers can also leave the formal banking system to hide their assets from seizure, change jobs to force creditors to reinstate a garnishment order, or work less so that their earnings are not subject to garnishment. Finally, borrowers can discharge unsecured debts through the consumer bankruptcy system.[4]

Each year, MMI administers over 75,000 DMPs that repay nearly $600 million in unsecured debt. Nationwide, it is estimated that non-profit credit counselors administer approximately 600,000 DMPs that repay unsecured creditors between $1.5 and $2.5 billion each year (Hunt 2005, Wilshusen 2011). In comparison, 1.0 to 1.5 million individuals file for bankruptcy protection each year.

B. Experimental Design

In 2003, MMI and eleven large unsecured creditors agreed to offer lower interest and lower monthly payments rates to a subset of borrowers enrolled in the structured repayment program. The purpose of the experiment was to evaluate the effect of more borrower-friendly loan terms on repayment rates and the average recovery amount. The eleven participating creditors are among the largest and most well-known unsecured creditors in the United States, collectively holding over 50 percent of borrowers' unsecured debt in our sample. The resulting randomized experiment was conducted between January 2005 and August 2006, before being discontinued due to the financial crisis.

The experimental population consisted of approximately 80,000 prospective clients that contacted MMI during the sample period. Each client was randomly assigned to a credit counselor, conditional on the client's state of residence, reference type, and contact date. In two week intervals, each credit counselor rotated between assigning every client to either the control or treatment group. Counselors were strictly instructed not to inform prospective clients of the randomized trial or whether the client was assigned to the treatment or control group. MMI conducted frequent audits of the counselors to ensure that the experimental procedures were followed.

Clients assigned to the treatment group were offered a repayment program with lower interest rates and lower minimum monthly payments than they otherwise would have received. In the

[4]Cross-sectional comparisons suggest that individuals enrolled in a DMP are less likely to file for bankruptcy (Staten and Barron 2006) and less likely to report financial distress (O'Neill et al. 2006) compared to otherwise similar individuals.

context of the experiment, lower interest rates decreased the total cost of the repayment program by shortening the repayment period, not by reducing monthly payments. This feature of the experiment is the result of monthly payment amounts being calculated using the initial balance of debt, not the total cost of repaying the debt. The lower monthly payments associated with the experiment instead came about by lengthening the repayment period. As a result, the lower monthly payments also modestly increased the total cost of completing the repayment program, making it possible that the lower payments could actually exacerbate financial distress for some borrowers.

Conditional on having at least one debt with a participating creditor, the median interest rate reduction was 3.69 percentage points, a 43.5 percent decrease from the control group mean of 8.50 percent. The median monthly payment reduction was 0.14 percent of initial debt, a 5.8 percent decrease from the control group mean of 2.38 percent of initial debt. To fix ideas, Appendix Table 1 further details the effect of these treatments on various repayment program attributes. We first calculate program attributes using the control means for debt ($18,212), the monthly payment (2.38 percent of debt), and the interest rate (8.50 percent). We then show how each attribute changes with various interest rate and monthly payment changes. The median interest rate reduction would shorten the repayment period by about four months, a 7.99 percent change, and decrease the total amount repaid by about $1,712, a 7.89 percent change. For the same borrower, the median monthly payment change would lengthen the repayment period by four months and increase the total amount repaid by about $289, a 1.33 percent change.

Importantly, each of the eleven creditors participating in the experiment offered a different bundle of interest rate and monthly payment reductions. Interest rate reductions for treated borrowers ranged from 4.0 to 9.9 percentage points, while minimum monthly payment reductions ranged from 0.0 to 0.5 percent of the initial debt. Moreover, borrowers owed different amounts to each of the participating creditors. As a result of these two institutional features, otherwise similar borrowers in our data received very different interest rate and monthly payment reductions when treated. The standard deviation of the interest rate change was 2.5 percentage points, or 29.4 percent of the control group mean, and the standard deviation of the monthly payment change was 0.17 percent of initial debt, 7.1 percent of the control group mean. Over 30 percent of eligible borrowers received above median reductions for both interest rates and monthly payments, 19.4 percent received above median reductions for only interest rates, 9.9 percent received above median reductions for only monthly payment reductions, and 40.0 percent received below median reductions for both interest rates and monthly payments. See Appendix Table 2 for additional details on the treatment bundles offered by each creditor, and Appendix Figure 1 for additional information on the distribution of potential treatment intensities for borrowers in our sample.

C. When Should Loan Modifications Matter?

In this section, we motivate our empirical analysis by describing how the effects of lower interest rates and lower monthly payments will depend on the types of constraints that borrowers face.

Interest Rates: Lower interest rates are most likely to affect repayment decisions through the total cost of the repayment program. Recall that the median interest rate reduction decreases the total amount to repaid by about 7.89 percent, about $1,712 for the typical borrower in our sample. This lower repayment cost increases both a borrower's incentive and ability to repay the debt. The effects of lower interest rates are therefore likely to be largest for borrowers who are more financially distressed, as these are exactly the types of borrowers most likely to be suffering from debt overhang. Conversely, the effects are likely to be smallest for borrowers with binding liquidity constraints or a high discount rate, as lower interest rates do not change the monthly payments in the short run.

Lower interest rates may also impact a borrower's financial distress due to changes in the repayment rate and cost of repayment. Higher repayment rates mechanically decrease loan default and collections activity, which may then increase a borrower's credit score and access to credit, further decreasing financial distress. Lower repayment burdens can have additional effects even when repayment rates are unchanged. Decreasing debt service costs may increase a borrower's ability to repay debts not included in the repayment plan, or help prevent any sharp drops in consumption that have important long-term consequences such as becoming sick due to a lack of medical care.

Finally, lower interest rates may impact labor supply decisions through opposing substitution and wealth effects. The decreased cost of debt service increases borrowers' wealth, potentially decreasing the incentive to work. On the other hand, lower default rates may increase the incentive to work by decreasing the incidence of wage garnishment.[5] Taken together, lower interest rates will increase labor supply if the substitution effect associated with the lower implicit tax rate dominates the wealth effect associated with the lower cost of debt service. However, lower interest rates are likely to decrease labor supply if borrowers are not subject to wage garnishment or the wealth effect dominates.

Monthly Payments: Lower monthly payments may impact repayment decisions through increased liquidity or the total cost of the repayment program. Decreasing the minimum payment amount mechanically increases disposable earnings in the short run, increasing a borrower's ability to smooth his or her consumption following a liquidity shock. All else equal, lower monthly payments should therefore increase repayment rates if borrowers are liquidity constrained. On the other hand, a longer repayment period also modestly increases the total cost of the repayment program, potentially decreasing a borrower's incentive and ability to repay the debt. In our data, the median payment reduction increases the total repayment cost by about 1.33 percent, or $289 for the typical borrower in our sample.

Lower monthly payments can also impact a borrower's financial distress and labor supply decisions due to changes in the repayment rate. If lower monthly payments increase repayment rates, financial distress is likely to fall and labor supply is likely to increase for the reasons discussed above. Conversely, financial distress is likely to increase and labor supply fall if repayment rates fall or are unchanged.

II. Data

A. Data Sources and Sample Construction

To estimate the impact of the randomized loan modifications, we match counseling data from MMI to administrative tax and bankruptcy records. In ongoing work, we are adding individual-level

[5]Wage garnishments occur when an employer is compelled by a court order to withhold a portion of the employee's disposable earnings to repay a particular debt. Federal law limits the amount that may be garnished in any one week to the lesser of 25 percent of weekly disposable earnings, or the amount by which weekly disposable earnings exceed 30 times the federal minimum wage. Repaying a debt stops all garnishment orders associated with that debt, potentially increasing the marginal return to work. Consistent with this idea, Dobbie and Song (2015) find that consumer bankruptcy protection – which stops all wage garnishment orders – increases annual earnings by $5,562, with larger effects for borrowers subject to higher wage garnishment rates. Exploiting within- and across-state variation in the marginal garnishment rate, they find that the implied earnings elasticity with respect to potential garnishment is 0.94.

credit bureau data to the matched dataset. This section describes the construction and matching of each dataset.

The counseling data provided by MMI include information on all prospective clients eligible for the randomized trial. The data include detailed information on each individual's unsecured debts, assets, liabilities, monthly income, monthly expenses, homeownership status, number of dependents, treatment status, enrollment in a repayment program, and completion of a repayment program. The data also include information on the date of first contact, state of residence, who referred the individual to MMI, and the assigned counselor. Finally, the MMI data include an internal risk score that captures the probability of finishing a repayment program. We normalize the risk score to have a mean of zero and standard deviation of one in the control group, and top-code all other continuous variables at the 99th percentile.

We make two sample restrictions to the MMI data. First, we drop any individuals that MMI does not randomly assign to counselors because they are likely to need a specialized service such as bankruptcy counseling or housing assistance. Second, we drop individuals with less than $850 in unsecured debt or more than $100,000 in unsecured debt to minimize the influence of outliers. These cutoffs correspond to the 1st and 99th percentiles of the control group, respectively. The resulting estimation sample consists of 39,243 individuals in the treatment group and 40,496 individuals in the control group.

In this estimation sample, we use the MMI data to calculate potential treatment intensity for each individual in our data. Recall that there is significant variation in potential interest rate and monthly payment reductions as a result of the participating creditors offering different concessions to treated borrowers. To measure this variation in treatment intensity, we first calculate the interest rate and monthly payment for all individuals as if they had been assigned to the control group. We then calculate the interest rate and monthly payment as if they had been assigned to the treatment group. For both the control and treatment calculations, we assume an interest rate of 6.7 percent and monthly payment of 2.25 percent for initial debt holdings for any debt held by non-participating creditors.[6] For debt held by participating creditors, we use the concessions detailed in Appendix Table 2. Finally, we calculate the difference between the control interest rate and the treatment

[6]These assumptions follow MMI's internal guidelines for calculating expected DMP payments. Results are robust to a wide range of alternative assumptions.

interest rate for each individual, and the control monthly payment and treatment monthly payment for each individual. These interest rate and monthly payment differences are our individual-level measures of potential treatment intensity.

Information on bankruptcy filings comes from individual-level PACER bankruptcy records. The bankruptcy records are available from 2000 to 2011 for the 81 (out of 94) federal bankruptcy courts that allow full electronic access to their dockets. These data represent approximately 87 percent of all bankruptcy filings during our sample period.[7] All specifications control for state fixed effects to account for the fact that we do not observe filings in all states. We match the credit counseling data to PACER data using name and the last four digits of the social security number. We assume that unmatched clients did not file for bankruptcy protection during the sample period. Our sample for bankruptcy outcomes is therefore identical to the estimation sample described above.

Information on labor supply and 401k contributions comes from administrative tax records at the SSA. The SSA data are remarkably complete and include every individual who has ever acquired a SSN, including those who are institutionalized. Illegal immigrants without a valid SSN are not included in these data. Information on earnings, employment, and annual 401k contributions come from annual W-2s. Individuals with no W-2 in any particular year are assumed to have had no earnings or 401k contributions in that year. Individuals with zero earnings are included in all regressions throughout the paper. We match the credit counseling data to the tax data using the full social security number. We are able to successfully match 95.3 percent of the counseling data to the SSA data. The probability of being matched to the SSA data is not significantly related to treatment status (see Panel D of Table 1). Our sample for all labor supply and 401k outcomes consists of the 76,008 individuals matched to the SSA data.

To provide additional information on repayment and financial distress, we are in the process of adding individual-level credit reports from TransUnion to our data. The TransUnion data are derived from public records, collections agencies, and trade lines data from lending institutions. The public records data contain records of bankruptcies, tax liens, and civil judgments. The collections data contain information on any unpaid bills that have been sent to collection agencies, including the date of collections and the current amount owed. The trade lines data include nearly all credit

[7]We are extremely grateful to Tal Gross, Matt Notowidigdo, and Jialan Wang for sharing the bankruptcy records used in our analysis. See Gross, Notowidigdo, and Wang (2014) for additional details on the data.

provided by banks, finance companies, credit unions, and other institutions. Each record includes the account opening date, outstanding balances, credit limit, and payment history for revolving credit, mortgages, and installment loans.

B. Descriptive Statistics and Experiment Validity

Table 1 presents descriptive statistics for the treatment and control groups. The average borrower in our sample is just over 40 years old with 2.15 dependents. Sixty-four percent of borrowers are women, 63.5 percent are white, 17.2 percent are black, and 8.9 percent are Hispanic. Forty-one percent are homeowners, 44.1 percent are renters, and the remainder live with either a family member or friend. The typical borrower in our data has just over $18,000 in unsecured debt, with about $9,600 of that debt being held by a creditor participating in the randomized trial. Monthly household incomes average about $2,450, and monthly expenses average about $2,150.

Panel B of Table 1 presents baseline outcomes for the year before contacting MMI. Individual earnings w approximately $23,500, slightly lower than the self-reported household earnings reported in Panel A, suggesting that at least some individuals in our sample are not the sole earner in the household. Eight-five percent of borrowers in our data are employed at baseline. Baseline bankruptcy rates are very low, 0.3 percent, likely because individuals are unlikely to enroll in a repayment program if they have already received bankruptcy protection.

Panel C of Table 1 presents measures of treatment intensity calculated using the MMI data. Fifty-three point seven percent of the treatment group and 53.4 percent of the control group would have lower monthly minimum monthly payments if eligible for treatment. Treatment reduces monthly payments by an average of 0.09 percent of initial debt, a 3.78 percent change from the control group mean of 2.38 percent of initial debt. The median reduction is slightly higher at 0.14 percent of initial debt. Sixty-six point three percent of the treatment group and 65.9 percent of the control would have lower interest rates if eligible for treatment. Treatment reduces interest rates by an average of 2.7 percentage points, a 31.7 percent change from the control mean of 8.50 percent. The median reduction is again slightly higher at 3.69 percentage points.

Column 3 of Table 1 tests for balance. We report the difference between the treatment and control group controlling for state by reference group by date fixed effects – the level at which clients were randomly assigned to counselors. Standard errors are clustered at the counselor level.

The means of all of the baseline and treatment intensity variables are similar in the treatment and control groups. Only one of the 24 baseline differences is statistically significant at the ten percent level and the p-value from a F-test of the joint significance of all of the variables listed is 0.691, suggesting that the randomization was successful.

To provide further evidence on the experimental validity, Appendix Table 3 presents results where we follow our main empirical specification described below and regress each characteristic or outcome on the interaction of treatment eligibility and potential treatment intensity. All regressions control for potential treatment intensity and strata fixed effects, and cluster standard errors at the counselor level. Consistent with our results from Table 1, we find no statistically significant relationships between our baseline measures and the interaction of treatment eligibility and potential treatment intensity.

III. Empirical Strategy and Results

A. Empirical Strategy

We estimate the impact of lower interest rates and minimum monthly payments using the following regression:

$$y_{it} = \alpha + \beta_1 treat_i \cdot \Delta rate_i + \beta_2 treat_i \cdot \Delta payment_i + \beta_3 \Delta rate_i + \beta_4 \Delta payment_i + \gamma \mathbf{X}_i + \varepsilon_{it} \quad (1)$$

where y_{it} is the outcome of interest for individual i in year t, $treat_i$ is an indicator variable equal to one if individual i was assigned to the treatment group, $\Delta rate_i$ is the difference between the control and treatment interest rate for individual i, $\Delta payment_i$ is the difference between the control and treatment monthly payment for individual i, and $\mathbf{X}_i$ is a vector of state by reference group by date fixed effects that account for the stratification used in the randomization of individuals to counselors. We estimate equation (1) first without any additional controls, then with the individual controls listed in Table 1, and finally with the individual controls listed in Table 1 and counselor fixed effects. Standard errors are adjusted for clustering at the counselor level.[8]

[8]Equation (1) implicitly assumes that the impact of each type of loan modification is linear and additively separable. Appendix Table 4 presents results using treatment intensity bins that do not rely on these functional form assumptions. The results are broadly consistent with linear and additively separable treatment effects, although large standard errors makes a precise test of these assumptions impossible. Equation (1) also implicitly assumes that there are no direct effects of treatment eligibility. This assumption is consistent with the experimental design discussed in

Equation (1) isolates the effect of each loan modification by comparing the impact of the randomized experiment across borrowers that differed in their potential treatment intensities.[9] We therefore interpret any treatment effect differences across these borrowers as the causal effect of the different treatment intensities. One potential threat to our interpretation of the results is that the observed treatment effect differences may be the result of other, unrelated factors. For example, it is possible that individuals with greater sensitivity to interest rate or monthly payment changes are more likely to borrow from the creditors who offered more generous loan modifications during the randomized experiment. In this scenario, estimates of equation (1) would be biased upwards because we would attribute the larger treatment effect solely to the more generous loan modification, not the greater sensitivity of the individuals who chose that creditor. Conversely, our estimates would be biased downwards if these individuals with greater sensitivities are less likely to borrow from the creditors who offered more generous loan modifications.

The observed differences in potential treatment intensity are the result of borrowers endogenously choosing different creditors before the experiment began. Columns 1-2 of Appendix Table 5 describe the correlates of the potential change in interest rate if treated, and columns 3-4 do the same for the potential change in monthly payment if treated. All specifications control for the level of randomization using state by reference group by date fixed effects, and cluster standard errors at the counselor level. Borrowers with larger potential interest rate changes are less likely to be black, less likely to have children, more likely to be homeowners, and have higher baseline earnings. Borrowers with larger potential monthly payment changes are also less likely to be black, are at lower risk of default as measured by MMI's standardized risk score, and have lower baseline earnings. Finally, and not surprisingly, borrowers with larger potential treatment intensities have more debt with creditors participating in the experiment and less debt with creditors not participating in the experiment.

Section II.B. Counselors were strictly instructed not to inform prospective clients of the randomized trial and MMI conducted frequent audits of the counselors to ensure that the experimental procedures were followed. Moreover, our main results are unchanged when we include an indicator for treatment eligibility, and the coefficient on the indicator for treatment eligibility is always small and not statistically different from zero.

[9] Our empirical strategy is similar to earlier work using variation in treatment exposure interacted with state or federal law changes. For example, Card (1992) estimates the impact of minimum wage laws on wages, employment, and education using across-state variation in the fraction of workers earning less than a new federal minimum wage. Similarly, Currie and Gruber (1996) estimate the impact of health insurance eligibility on health care utilization and child health using across-state and across-group variation in the number of children eligible for Medicaid. In contrast to these earlier studies, our treatment and control groups are determined by random assignment, not state or federal law changes.

To partially test the validity of this identifying assumption, Appendix Table 6 presents subsample results by predicted treatment intensity. We use the descriptive results from columns 2 and 4 from Appendix Table 5 to calculate predicted treatment intensity for all borrowers in our sample. We then estimate results interacting our treatment effect with an indicator for having an above or below median predicted treatment intensity. There are larger effects of interest rate changes for borrowers with low predicted treatment intensity, although only the point estimate on starting repayment is statistically significant. For monthly payments, we find results that are more negative for borrowers with low predicted treatment intensity, but again only the earnings result is statistically significant. These results suggest that our main results may be modestly biased towards zero, and, more importantly, are unlikely to be biased upwards. Nevertheless, our estimates should be interpreted with these potential issues in mind.[10]

B. Debt Repayment

Table 2 presents estimates of the impact of lower interest rates and lower minimum monthly payments on starting and completing a repayment program. The dependent variable for columns 1-3 is an indicator variable for starting a repayment program through MMI. The dependent variable for columns 4-6 is an indicator variable for completing a repayment program. Columns 1 and 4 report results controlling only for potential treatment intensity and strata fixed effects. Columns 2 and 5 add the baseline controls listed in Table 1. Columns 3 and 6 add counselor fixed effects. All specifications cluster standard errors at the counselor level. We report the coefficients on the interaction of treatment eligibility and potential treatment intensity.

There is an economically significant impact of lower interest rates (i.e. shorter repayment periods and lower repayment costs) on both starting and completing repayment. Borrowers offered the median interest rate reduction of 3.69 percentage points were 1.77 to 2.03 percentage points more likely to start a repayment program, a 5.57 to 6.38 percent increase from the control group mean of 31.85 percent. Lower interest rates also increased the probability of completing repayment by 1.77 to 1.99 percentage points, a 14.88 to 16.74 percent increase from the control mean of 11.93

[10] An alternative test of our identifying assumption is to compare the treatment effects of borrowers with identical predicted treatment intensities but with different creditors. For example, there are three different creditors that have predicted interest rate reductions of 9.9 percentage points and predicted monthly payment reductions if 0.4 percentage points. Unfortunately, there are very few borrowers whose debts are held primarily by these creditors, making the resulting placebo tests too imprecise to be informative.

percent.

An important question is whether lenders benefit from offering lower interest rates. To shed light on this question, we conduct a back-of-the-envelope calculation of the expected value of debt with and without the lower interest rate. To simplify the calculation, we assume that the lender is risk neutral and does not discount future payments. We also assume that borrowers repaid ten percent of any outstanding debt that is not repaid through the repayment program. Unfortunately information is not available on the exact repayment rates on outside debt for the borrowers in our sample. Creditors participating in the experiment suggested that the average repayment rate for observably similar borrowers ranged from 6.5 percent to 14.5 percent during our sample period. Given the wide range of estimates, we also report expected value calculations assuming repayment rates of zero and 20 percent to explore the robustness of our results to this assumption.

The average borrower in the control group repays 19.97 percent of his or her debt through the structured repayment program. Assuming a ten percent repayment rate on the 80.03 percent of debt that is not repaid through the program, this implies that the average borrower in the control group repays approximately $5,790 of his or her debt. The median interest rate cut increases the amount repaid by 2.00 percent, implying that the average treated borrower repays approximately $5,813 of his or her debt. Thus, lenders gain approximately $23 for each borrower offered the median interest rate reduction. If the outside repayment rate is zero percent, lenders gain approximately $58 per borrower. If the outside repayment rate is 20 percent, however, lenders lose approximately $14 for each borrower offered the median interest rate reduction. These calculations suggest that there is likely a modest ex-post benefit to creditors of reducing interest rates, but that we cannot rule out small ex-post losses for creditors under reasonable assumptions.

In contrast to the interest rate results discussed above, we find little impact of lower minimum payments (i.e. longer repayment periods and higher repayment costs) on repayment rates. The point estimates for both starting and completing a repayment program are small and not statistically different from zero. The 95 percent confidence intervals rule out treatment effects larger than 2.4 percentage points for starting a repayment program, and 1.5 percentage points for completing a repayment program. These results suggest that either liquidity constraints are not an important driver of borrower behavior in our data, or that a longer repayment period is an ineffective way to alleviate these liquidity constraints.

Table 5 presents our main subsample estimates for borrowers with above and below median baseline debt-to-income ratio, a proxy for financial distress. There are two reasons why lower interest rates and lower minimum payments may have larger effects on more financially distressed borrowers. First, these borrowers have the largest absolute changes in the repayment period (in months) and repayment costs (in dollars). Second, these borrowers may be more likely to be suffering from debt overhang or liquidity constraint problems. We calculate debt-to-income ratio using self-reported household income. Results are identical using individual earnings from the tax data to calculate debt-to-income.

The results from Table 5 are largely consistent with these predictions. Borrowers with above median debt-to-income ratios were 3.18 percentage points more likely to start and 3.25 percentage points more likely to complete repayment if offered the median interest rate cut. In comparison, there were no statistically significant effects of lower interest rates on borrowers with below median debt-to-income ratios. We also find no effect of lower minimum payments for borrowers with either above or below median debt-to-income ratios.

Appendix Tables 7-9 present additional subsample results by gender, ethnicity, and homeownership. For each of these three subgroups, there are no clear theoretical predictions as to which group will benefit most from either lower interest rates or lower monthly payments. We find that the effect of interest rates on repayment was larger for female borrowers, but did not systematically differ by ethnicity or homeownership. Lower monthly payments had little impact on all borrowers.

Our null result on monthly payments, even for the most distressed borrowers, is surprising given the large literature documenting liquidity constraints in a variety of contexts (e.g. Gross and Souleles 2002, Johnson, Parker, and Souleles 2006, Agarwal et al. 2007, Parker et al. 2013). For example, in the setting most similar to our own, there is evidence that reductions in a borrower's mortgage payments induced by anticipated rate resets both decreases the probability of mortgage default and increases the amount of non-durable consumption (e.g. Di Maggio, Kermani, and Ramcharan 2014, Keys et al. 2014, Fuster and Willen 2015). Importantly, there is no evidence of forward looking behavior before the rate resets, suggesting that borrowers are so liquidity constrained that they cannot prevent involuntarily defaults just before the monthly payment reduction.

There are at least three possible explanations for the lack of evidence for liquidity constraints in our setting. First, we estimate the impact of lower monthly payments on unsecured credit card debt, not secured mortgage debt. It is possible that liquidity constraints are a more important determinant of repayment behavior on mortgage debt, while strategic concerns dominate for unsecured credit card debt. Second, a longer repayment period for unsecured credit card debt has a much more modest impact on disposable earnings as compared to a mortgage rate reset. The median monthly payment reduction in our sample was $26.68, while the mortgage rate resets examined in the prior literature decreased monthly payments by up to $900 (Di Maggio, Kermani, and Ramcharan 2014). It is possible that borrowers only benefit from monthly payment reductions large enough to deal with particularly severe liquidity shocks, such as those created by a job loss or uninsured medical emergency. Finally, it is possible that the results documented in the mortgage rate reset literature are due to borrower inattention or ignorance about the rate reset, not liquidity constraints. While lenders are supposed to inform borrowers by mail before the index rate changes, it is not clear if borrowers read or understand these notices.

C. Bankruptcy

Table 3 presents estimates of the effect of loan modifications on bankruptcy filing in any of the first five years following the experiment. Table 5 presents results for borrowers with below and above median debt-to-income levels.

There was a modest impact of lower interest rates on bankruptcy filing. Over the first five years, borrowers offered the median interest rate reduction were 0.88 to 1.07 percentage points less likely to file for bankruptcy, a 8.57 to 10.35 percent decrease from the control mean of 10.36 percent. The decrease in bankruptcy filing is largely driven by reductions in the second and third post-randomization years, approximately when most repayment plans are completed.

Consistent with our repayment results, we find larger effects for borrowers with above median debt-to-income levels. The median interest rate reduction decreases the probability of filing for bankruptcy by 1.36 percentage points for these borrowers, a 9.67 percent decrease from the control mean for that subset of individuals. There are much more modest effects for borrowers with below median levels of debt, although relatively large standard errors means that the difference is not statistically significant (p-value = 0.152). The bankruptcy filing effects are also somewhat larger for female and non-white borrowers, though neither difference is statistically significant.

We find no impact of lower monthly payments on bankruptcy filing. Over the first five years following the experiment, the median monthly payment reduction increased the probability of filing for bankruptcy by a statistically insignificant 0.70 percentage points, with slightly larger point estimates for borrowers with above median debt-to-income ratios. In results available upon request, there are statistically significant increases in the probability of filing in the fourth and fifth post-experiment years, suggesting that lower monthly payments slightly delay the onset of financial distress.

D. Labor Supply and 401k Contributions

Table 4 presents estimates of the effect of loan modifications on annual earnings, employment, and 401k contributions. Table 5 presents analogous results for borrowers with below and above median debt-to-income levels. The dependent variable for each regression is the outcome averaged over the first five years following the experiment.

The estimated effects of both interest rates and monthly payments on labor supply and 401k contributions are small and relatively imprecisely estimated in the full sample of borrowers. There is a positive effect of lower interest rates on employment, but the point estimates lose statistical significance when we add baseline controls. In our specification with baseline controls and counselor fixed effects, the upper limit of the 95 percent confidence interval is positive 1.16 percentage points, or 1.42 percent of the control mean. Similarly, there is a negative effect of lower monthly payments on employment that loses statistical significance when we add counselor fixed effects. In our specification with baseline controls and counselor fixed effects, the lower limit of the 95 percent confidence interval is negative 1.65 percentage points, or 2.02 percent of the control mean. There are no statistically significant effects on earnings or 401k contributions in any specification.

The effect of loan modifications on employment is larger for borrowers with above median debt-to-income ratios, although the effects on earnings and 401k contributions remain small and imprecisely estimated. For these indebted borrowers, the median interest rate reduction increased employment rates by 1.36 percentage points over the first five post-randomization years, a 1.75 percent increase from the control mean for that subset of borrowers. Conversely, the median monthly payment reduction decreased employment rates by 1.68 percentage points, or 2.14 percent, for these borrowers.

IV. Conclusion

This paper uses a randomized experiment to estimate the impact of loan modifications on repayment, bankruptcy, and labor supply. We find that lower interest rates increased repayment rates and decreased bankruptcy rates. Lower interest rates also modestly increased employment rates among the most heavily indebted borrowers. In contrast, lower minimum payments had little positive impact on any of the observed outcomes, suggesting that either liquidity constraints are not an important driver of borrower behavior in our data or that a longer repayment period does not alleviate the kinds of liquidity constraints present in our sample.

Our estimates suggest that there may be significant ex-post benefits of voluntary debt forgiveness for both lenders and borrowers. A simple back-of-the-envelope calculation suggests that the median interest rate reduction increased lender profits by about $23 per borrower. Moreover, borrowers appear to benefit from lower interest rates due to the lower bankruptcy rates and higher employment rates. These results suggest that policies that lower the barriers to voluntary debt forgiveness, such as lower tax penalties on debt write-offs and new mechanisms to help lenders coordinate, may be welfare improving.

These findings also inform a recent debate on the use of loan modifications to increase consumption and employment during economic downturns. Recent work suggests that excessive household debt can affect the real economy due to nominal or labor market rigidities (e.g. Guerrieri and Lorenzoni 2011, Hall 2011, Midrigan and Philippon 2011, Eggertson and Krugman 2012, Farhi and Werning 2013, Mian, Rao, and Sufi 2013, Mian and Sufi 2014), and that ex-post debt forgiveness can help mitigate the harmful effects of debt during a financial crisis (e.g. Dobbie and Goldsmith-Pinkham 2014). This paper suggests that debt forgiveness may be welfare improving even in the absence of such macro-rigidities.

The main limitation of our analysis is that we are not able to estimate the impact of loan modifications on ex-ante borrower behavior or borrowing costs. There may also be important ex-post impacts of loan modifications on outcomes such as credit availability that we are unable to measure with our data. These issues remain important areas for future research.

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Table 1
Descriptive Statistics and Balance Tests

	Treatment	Control	Difference
Panel A: Baseline Characteristics	(1)	(2)	(3)
Age	40.516	40.626	−0.271
Male	0.361	0.363	0.008
White	0.635	0.636	0.010
Black	0.174	0.171	−0.008*
Hispanic	0.088	0.090	−0.001
Home owner	0.410	0.412	−0.003
Renter	0.442	0.440	0.003
Number of dependents	2.156	2.159	−0.006
Monthly income	2.448	2.453	0.010
Monthly expenses	2.158	2.168	0.003
Total assets	71.545	71.635	−0.373
Total liabilities	68.101	68.488	−0.125
Unsecured debt	18.368	18.212	0.299
Debt eligible for modification	9.615	9.568	0.163
Standardized risk score	0.003	0.000	0.003
Panel B: Baseline Outcomes			
Bankruptcy	0.003	0.004	−0.001
Earnings	23.518	23.447	−0.108
Employment	0.850	0.848	0.004
401k contributions	0.373	0.372	−0.008
Panel C: Treatment Intensity			
Potential payment change (x100)	9.371	9.513	0.081
Potential interest rate change	2.650	2.641	0.034
Panel D: Data Quality			
Matched to SSA data	0.954	0.953	0.003
Missing age	0.071	0.072	−0.005
p-value from joint F-test	–	–	0.691
Observations	39,243	40,496	79,739

Notes: This table reports descriptive statistics and balance tests for the estimation sample. Information on age, gender, race, earnings, employment, and 401k contributions is only available for individuals matched to the SSA data. Risk score is standardized to have a mean of zero and standard deviation of one in the control group. Each baseline outcome is for the year before the experiment. Earnings and employment outcomes come from 1978 - 2012 W-2s, where employment is an indicator for non-zero wage earnings. 401k contributions come from annual W-2s. Potential minimum payment and interest rate changes if treated are calculated using the amount of debt held by each creditor and the rules listed in Appendix Table 2. All dollar amounts are divided by 1,000. Column 3 reports the difference between the treatment and control groups, controlling for strata fixed effects and clustering standard errors at the counselor level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. The p-value is from an F-test of the joint significance of the variables listed.

Table 2
Loan Modifications and Debt Repayment

	Start Payment			Complete Payment		
	(1)	(2)	(3)	(4)	(5)	(6)
Interest rate change	0.0055**	0.0050**	0.0048**	0.0054***	0.0049***	0.0048***
	(0.0025)	(0.0023)	(0.0023)	(0.0017)	(0.0017)	(0.0017)
Payment change (x100)	0.0003	0.0005	0.0006	−0.0001	0.0000	0.0001
	(0.0006)	(0.0006)	(0.0006)	(0.0005)	(0.0005)	(0.0005)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	No	Yes	Yes	No	Yes	Yes
Counselor FE	No	No	Yes	No	No	Yes
Observations	79,739	79,739	79,739	79,739	79,739	79,739
Mean in Control Group	0.3185	0.3185	0.3185	0.1193	0.1193	0.1193

Notes: This table reports reduced form estimates of the impact of loan modifications on debt repayment. Information on repayment comes from records at the credit counseling organization. We report coefficients on the interaction of treatment eligibility and potential interest rate change if treated, and the interaction of treatment eligibility and potential monthly payment change (x 100) if treated. All specifications control for the potential minimum payment and interest rate changes if treated and cluster standard errors at the counselor level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Table 1 notes for details on the baseline controls and sample.

Table 3
Loan Modifications and Bankruptcy

	Bankruptcy in Years 1-5		
	(1)	(2)	(3)
Interest rate change	-0.0024^{*}	-0.0027^{*}	-0.0029^{**}
	(0.0014)	(0.0014)	(0.0014)
Payment change (x100)	0.0005	0.0005	0.0005
	(0.0003)	(0.0003)	(0.0003)
Strata FE	Yes	Yes	Yes
Baseline Controls	No	Yes	Yes
Counselor FE	No	No	Yes
Observations	79,739	79,739	79,739
Mean in Control Group	0.1036	0.1036	0.1036

Notes: This table reports reduced form estimates of the impact of loan modifications on bankruptcy. Information on bankruptcy comes from court records. We report coefficients on the interaction of treatment eligibility and potential interest rate change if treated, and the interaction of treatment eligibility and potential monthly payment change (x 100) if treated. All specifications control for the potential minimum payment and interest rate changes if treated and cluster standard errors at the counselor level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Table 1 notes for details on the baseline controls and sample.

Table 4
Loan Modifications and Labor Supply and 401k Contributions

	Employment			Earnings			401k Contributions		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Interest rate change	0.0022*	0.0013	0.0012	0.0290	−0.0403	−0.0357	−0.0056	−0.0053	−0.0054
	(0.0017)	(0.0009)	(0.0010)	(0.1309)	(0.0689)	(0.0702)	(0.0056)	(0.0041)	(0.0042)
Payment change (x100)	−0.0007*	−0.0004*	−0.0004	−0.0075	0.0053	0.0042	0.0008	0.0012	0.0011
	(0.0005)	(0.0002)	(0.0003)	(0.0334)	(0.0182)	(0.0183)	(0.0014)	(0.0010)	(0.0010)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Counselor FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	76,008	76,008	76,008	76,008	76,008	76,008	76,008	76,008	76,008
Mean in Control Group	0.8202	0.8202	0.8202	26.8915	26.8915	26.8915	0.4643	0.4643	0.4643

Notes: This table reports reduced form estimates of the impact of loan modifications on earnings, employment, and 401k contributions. Information on all outcomes comes from records at the Social Security Administration. We report coefficients on the interaction of treatment eligibility and potential interest rate change if treated, and the interaction of treatment eligibility and potential monthly payment change (x 100) if treated. All specifications control for the potential minimum payment and interest rate changes if treated and cluster standard errors at the counselor level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Table 1 notes for details on the baseline controls and sample.

Table 5
Results by Debt-to-Income Ratio

	Start Payment	Complete Payment	Bankrupt	Earnings	Employed	401k Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Interest x high debt/income	0.0086***	0.0088***	−0.0037**	0.1135	0.0046**	−0.0041
	(0.0029)	(0.0023)	(0.0019)	(0.1473)	(0.0021)	(0.0068)
(2) Interest x low debt/income	0.0020	0.0016	−0.0008	−0.0613	−0.0003	−0.0072
	(0.0031)	(0.0023)	(0.0015)	(0.1662)	(0.0021)	(0.0075)
P-value for (1)-(2)	[0.0595]	[0.0188]	[0.1526]	[0.3146]	[0.0655]	[0.7220]
(3) Payment x high debt/income	0.0001	−0.0005	0.0006	−0.0266	−0.0012**	0.0009
	(0.0007)	(0.0006)	(0.0004)	(0.0389)	(0.0006)	(0.0016)
(4) Payment x low debt/income	0.0006	0.0003	0.0003	0.0137	−0.0002	0.0007
	(0.0007)	(0.0006)	(0.0004)	(0.0410)	(0.0006)	(0.0017)
P-value for (3)-(4)	[0.5283]	[0.2620]	[0.4540]	[0.3643]	[0.1184]	[0.8941]
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No	No	No
Counselor FE	No	No	No	No	No	No
Observations	79,739	79,739	79,739	76,008	76,008	76,008
Mean if high debt/income	0.3204	0.1300	0.1415	26.1384	0.7821	0.4874
Mean if low debt/income	0.3167	0.1086	0.0658	27.6495	0.8586	0.4410

Notes: This table reports reduced form estimates by baseline debt-to-income ratio. We report coefficients on the interaction of treatment, potential treatment intensity, and an indicator for having above and below median debt-to-income. All specifications control for an indicator for high debt-to-income and the potential minimum payment and interest rate changes if treated. Standard errors are clustered at the counselor level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Table 1 notes for details on the baseline controls and sample.

Appendix Table 1
Experimental Loan Modifications and Repayment Program Attributes

Treatment	Monthly Cost	Total Cost	Total Months
Panel A: Baseline Case			
–	\$433.45	\$21,694.46	50.05
Panel B: With Interest Rate Reduction			
Δ1.96%	\$433.45	\$20,735.11	47.84
Δ3.69%	\$433.45	\$19,982.00	46.10
Δ5.63%	\$433.45	\$19,214.64	44.33
Panel C: With Monthly Payment Reduction			
Δ0.07%	\$420.14	\$21,832.71	51.97
Δ0.14%	\$406.77	\$21,983.43	54.04
Δ0.25%	\$387.92	\$22,219.20	57.28

Notes: This table describes the effect of treatment eligibility on repayment program attributes. Monthly cost is the minimum required payment of the program. Total cost is the total amount that is repaid including interest. Total duration is the total number of months before the program is complete. All program characteristics are calculated using the control means for debt (\$18,212), monthly payment amount (2.38% of debt), and interest rate (8.5%). Panel A reports program characteristics for the baseline case with no reductions. Panel B reports program characteristics after 25th, 50th, and 75th percentile interest rate reductions. Panel C reports program characteristics after 25th, 50th, and 75th percentile monthly payment reductions.

Appendix Table 2
Creditor Concessions and Dates of Participation

Creditor	Interest Rate		Monthly Payment		Dates of Participation
	Treatment	Control	Treatment	Control	
1	1.00%	7.30%	2.00%	2.00%	Jan. 2005 to Aug. 2006
2	0.00%	9.90%	1.80%	2.20%	Jan. 2005 to Aug. 2006
3	0.00%	9.00%	1.80%	2.00%	Jan. 2005 to Aug. 2006
4	0.00%	8.00%	2.44%	2.44%	Feb. 2005 to Aug. 2006
5	2.00%	6.00%	1.80%	2.30%	Jan. 2005 to Aug. 2006
6	0.00%	9.90%	2.25%	2.25%	Apr. 2005 to Aug. 2006
7	1.00%	10.00%	1.80%	2.00%	May 2005 to Oct. 2005
8	2.00%	6.00%	1.80%	2.30%	Sept. 2005 to Aug. 2006
9	0.00%	9.90%	1.80%	2.20%	Jan. 2005 to Aug. 2006
10	0.00%	9.90%	1.80%	2.20%	Jan. 2005 to Aug. 2006
11	0.00%	9.90%	1.80%	2.20%	Jan. 2005 to Aug. 2006

Notes: This table details the terms offered to the treatment and control groups by the eleven creditors participating in the randomized trial. Monthly payments are a percentage of the total debt enrolled. See text for additional details.

Appendix Table 3
Additional Tests of Random Assignment

	Control Mean	Treated x Δ Interest	Treated x Δ Payment	p-value on joint test
Panel A: Baseline Characteristics	(1)	(2)	(3)	(4)
Age	40.6256	−0.0314	0.0034	0.8785
	(13.4135)	(0.0759)	(0.0199)	
Male	0.3631	0.0020	−0.0002	0.7004
	(0.4809)	(0.0029)	(0.0007)	
White	0.6363	0.0031	−0.0000	0.2217
	(0.4811)	(0.0026)	(0.0006)	
Black	0.1712	−0.0003	−0.0004	0.1719
	(0.3767)	(0.0019)	(0.0004)	
Hispanic	0.0904	−0.0027	0.0005	0.2617
	(0.2868)	(0.0017)	(0.0004)	
Home owner	0.4123	−0.0019	0.0006	0.5496
	(0.4923)	(0.0023)	(0.0006)	
Renter	0.4395	0.0024	−0.0007	0.4936
	(0.4963)	(0.0025)	(0.0006)	
Number of dependents	2.1590	−0.0017	0.0009	0.8749
	(1.3852)	(0.0070)	(0.0018)	
Monthly income	2.4534	0.0066	−0.0012	0.6796
	(1.4452)	(0.0076)	(0.0020)	
Monthly expenses	2.1682	0.0014	−0.0001	0.9542
	(1.2944)	(0.0068)	(0.0018)	
Total assets	71.6355	−0.6294	0.1267	0.5651
	(109.8651)	(0.5893)	(0.1463)	
Total liabilities	68.4875	−0.3651	0.0966	0.6785
	(86.2506)	(0.4472)	(0.1140)	
Unsecured debt	18.2120	0.1233	−0.0107	0.1775
	(16.9388)	(0.0761)	(0.0195)	
Debt eligible for modification	9.5679	0.0813	−0.0110	0.3257
	(12.6572)	(0.0566)	(0.0154)	
Standardized risk score	0.0000	−0.0010	0.0007	0.8118
	(1.0000)	(0.0051)	(0.0012)	
Panel B: Baseline Outcomes				
Bankruptcy	0.0038	−0.0002	0.0000	0.7922
	(0.0614)	(0.0003)	(0.0001)	
Employment	0.8478	0.0028	−0.0005	0.3700
	(0.3593)	(0.0020)	(0.0005)	
Earnings	23.4466	0.0272	−0.0041	0.9714
	(21.1752)	(0.1188)	(0.0302)	
401k contributions	0.3717	−0.0019	−0.0002	0.7577
	(0.9688)	(0.0056)	(0.0014)	

Panel C: Data Quality				
Matched to SSA data	0.9526	0.0005	0.0001	0.5749
	(0.2124)	(0.0011)	(0.0003)	
Missing age	0.0720	−0.0016	0.0001	0.2141
	(0.2585)	(0.0013)	(0.0003)	
Observations	40,496	79,739		

Notes: This table reports additional tests of random assignment. The dependent variable for each regression is the listed baseline variable. We report coefficients on the interaction of treatment and potential treatment intensity. All regressions control for potential treatment intensity and strata fixed effects, and cluster standard errors at the counselor level. Column 4 reports the p-value from an F-test that both interactions of treatment and potential treatment intensities are jointly equal to zero. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Table 1 notes for additional details on the sample and variable construction.

Appendix Table 4

Results by Treatment Intensity Bins

Panel A: Start Payment

	Interest Rate Change		
	0.00-0.01	0.01-3.69	3.70-9.90
Payment Change	(1)	(2)	(3)
(1) 0.00-0.01	0.0042	0.0157	0.0457
	(0.0089)	(0.0239)	(0.0289)
(2) 0.01-0.14	–	0.0067	0.0274
		(0.0151)	(0.0213)
(3) 0.15-0.50	–	0.0177	0.0453***
		(0.0209)	(0.0132)

Panel B: Complete Payment

	Interest Rate Change		
	0.00-0.01	0.01-3.69	3.70-9.90
Payment Change	(1)	(2)	(3)
(1) 0.00-0.01	−0.0039	0.0361**	0.0310
	(0.0061)	(0.0162)	(0.0199)
(2) 0.01-0.14	–	0.0127	0.0297
		(0.0104)	(0.0189)
(3) 0.15-0.50	–	0.0099	0.0290***
		(0.0163)	(0.0110)

Panel C: Bankruptcy

	Interest Rate Change		
	0.00-0.01	0.01-3.69	3.70-9.90
Payment Change	(1)	(2)	(3)
(1) 0.00-0.01	0.0016	−0.0217*	−0.0106
	(0.0054)	(0.0120)	(0.0154)
(2) 0.01-0.14	–	0.0036	0.0125
		(0.0099)	(0.0149)
(3) 0.15-0.50	–	0.0055	−0.0109
		(0.0137)	(0.0080)

Panel D: Employment

	Interest Rate Change		
	0.00-0.01	0.01-3.69	3.70-9.90
Payment Change	(1)	(2)	(3)
(1) 0.00-0.01	0.0042	0.0190	0.0123
	(0.0071)	(0.0121)	(0.0198)
(2) 0.01-0.14	–	0.0057	−0.0157
		(0.0103)	(0.0178)
(3) 0.15-0.50	–	0.0005	−0.0078
		(0.0152)	(0.0091)

Panel E: Earnings

		Interest Rate Change		
		0.00-0.01	0.01-3.69	3.70-9.90
Payment Change		(1)	(2)	(3)
(1)	0.00-0.01	0.2419	0.5388	−0.3226
		(0.4949)	(0.9152)	(1.3761)
(2)	0.01-0.14	–	0.1876	−1.1255
			(0.6991)	(1.3022)
(3)	0.15-0.50	–	−0.3801	0.2310
			(1.1851)	(0.7171)

Panel F: 401k Contributions

		Interest Rate Change		
		0.00-0.01	0.01-3.69	3.70-9.90
Payment Change		(1)	(2)	(3)
(1)	0.00-0.01	0.0050	−0.0313	0.0069
		(0.0204)	(0.0383)	(0.0596)
(2)	0.01-0.14	–	0.0181	−0.0241
			(0.0306)	(0.0509)
(3)	0.15-0.50	–	−0.0235	−0.0158
			(0.0505)	(0.0302)

Notes: This table reports estimates separately by treatment intensity bin. We report coefficients on the interaction of treatment eligibility and an indicator for having potential treatment intensity in the indicated ranges. All specifications control for an exhaustive set of potential treatment intensity fixed effects and cluster standard errors at the counselor level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table 5
Correlates of Potential Treatment Intensity

	Δ Interest		Δ Payment	
	(1)	(2)	(3)	(4)
Age	0.0013	-0.0021^{*}	0.0309^{***}	0.0279^{***}
	(0.0015)	(0.0012)	(0.0065)	(0.0055)
Male	0.0270	-0.0179	0.4037^{***}	0.3417^{***}
	(0.0314)	(0.0269)	(0.1501)	(0.1291)
White	0.0047	-0.0383	0.3866	0.3758
	(0.0593)	(0.0542)	(0.2720)	(0.2479)
Black	-0.3151^{***}	-0.1853^{***}	-1.1664^{***}	-0.4426^{*}
	(0.0703)	(0.0599)	(0.3068)	(0.2642)
Hispanic	-0.0923	-0.0441	-0.4339	-0.2218
	(0.0732)	(0.0644)	(0.3435)	(0.3025)
Home owner	0.2174^{***}	0.1935^{***}	0.2142	-0.2851
	(0.0555)	(0.0500)	(0.2506)	(0.2257)
Renter	-0.0325	-0.0204	-0.1088	-0.0341
	(0.0469)	(0.0402)	(0.2097)	(0.1797)
Number of dependents	-0.0580^{***}	-0.0382^{***}	-0.1782^{***}	-0.0449
	(0.0106)	(0.0102)	(0.0519)	(0.0491)
Monthly income	0.0097	0.0424^{**}	-0.2943^{**}	-0.3166^{***}
	(0.0266)	(0.0209)	(0.1194)	(0.0936)
Monthly expenses	-0.0162	-0.0301	0.1246	0.1619
	(0.0291)	(0.0238)	(0.1291)	(0.1054)
Total assets	-0.0009^{***}	-0.0010^{***}	0.0004	0.0025^{**}
	(0.0003)	(0.0003)	(0.0013)	(0.0011)
Total liabilities	0.0008^{**}	0.0007^{**}	0.0005	-0.0013
	(0.0004)	(0.0003)	(0.0018)	(0.0015)
Unsecured debt	-0.1002^{***}	-0.0608^{***}	-0.3541^{***}	-0.1241^{***}
	(0.0019)	(0.0017)	(0.0099)	(0.0083)
Debt eligible for modification	0.1956^{***}	0.1181^{***}	0.6966^{***}	0.2473^{***}
	(0.0029)	(0.0028)	(0.0130)	(0.0124)
Standardized risk score	-0.0557^{***}	0.0155	-0.6405^{***}	-0.5126^{***}
	(0.0163)	(0.0138)	(0.0729)	(0.0618)
Bankruptcy	-0.5295^{*}	-0.2958	-2.1013^{**}	-0.8848
	(0.2784)	(0.2335)	(0.9460)	(0.7737)
Employment	0.0326	0.0343	-0.0152	-0.0902
	(0.0528)	(0.0422)	0.2363)	(0.1885)
Earnings	0.0022^{**}	0.0023^{***}	-0.0014	-0.0065^{*}
	(0.0010)	(0.0009)	(0.0041)	(0.0037)
401k contributions	-0.0193	-0.0166	-0.0247	0.0196
	(0.0170)	(0.0147)	(0.0748)	(0.0646)
Matched to SSA data	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Missing age	0.0931	-0.0919	1.6633	1.4494
	(0.2747)	(0.2661)	(1.3284)	(1.2697)
Potential payment change (x100)		0.1112^{***}		
		(0.0023)		
Potential interest rate change				2.2972^{***}
				(0.0356)
Observations	79,739	79,739	79,739	79,739

Notes: This table describes correlates of potential treatment intensity. The dependent variable for columns 1-2 is the potential change in interest rates. The dependent variable for columns 3-4 is the potential change in monthly payments (x 100). All regressions control for strata fixed effects and cluster standard errors at the counselor level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Table 1 notes for additional details on the sample and variable construction.

Appendix Table 6

Results by Predicted Treatment Intensity

	Start Payment	Complete Payment	Bankrupt	Earnings	Employed	401k Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Interest x high predicted change	0.0025	0.0038**	−0.0020	0.0019	−0.0458	−0.0060
	(0.0027)	(0.0019)	(0.0016)	(0.0018)	(0.1422)	(0.0064)
(2) Interest x low predicted change	0.0133***	0.0091***	−0.0018	0.0032	0.2987	−0.0031
	(0.0043)	(0.0034)	(0.0023)	(0.0031)	(0.2143)	(0.0083)
P-value for (1)-(2)	[0.0209]	[0.1501]	[0.9317]	[0.6979]	[0.1454]	[0.7530]
(3) Payment x high predicted change	0.0010	0.0003	0.0012	−0.0006	0.0051	0.0009
	(0.0006)	(0.0005)	(0.0009)	(0.0005)	(0.0366)	(0.0015)
(4) Payment x low predicted change	−0.0005	−0.0010	0.0018**	−0.0029***	−0.0356	0.0007
	(0.0012)	(0.0008)	(0.0009)	(0.0010)	(0.0721)	(0.0028)
P-value for (3)-(4)	[0.2445]	[0.1408]	[0.2157]	[0.0137]	[0.5894]	[0.9439]
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No	No	No
Counselor FE	No	No	No	No	No	No
Observations	79,739	79,739	79,739	76,008	76,008	76,008
Mean if high predicted rate change	0.3904	0.1673	0.1055	0.8266	29.6198	0.5482
Mean if low predicted rate change	0.2463	0.0710	0.1018	0.8138	24.1524	0.3801
Mean if high predicted payment change	0.4021	0.1710	0.1069	0.8087	28.5052	0.5280
Mean if low predicted payment change	0.2354	0.0679	0.1003	0.8318	25.2787	0.4006

Notes: This table reports results by predicted potential treatment intensity. The predicted potential interest rate and monthly payment change is calculated using all baseline variables listed in Table 1. We report coefficients on the interaction of treatment, actual potential treatment intensity, and an indicator for having above and below median predicted potential treatment intensity. All specifications control for an indicator for high predicted potential treatment intensity and the potential minimum payment and interest rate changes if treated. Standard errors are clustered at the counselor level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Table 1 notes for details on the baseline controls and sample.

Appendix Table 7

Results by Gender

	Start Payment	Complete Payment	Bankrupt	Earnings	Employed	401k Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Interest x male	0.0016	0.0012	−0.0013	0.0660	0.0031	−0.0072
	(0.0034)	(0.0025)	(0.0022)	(0.1890)	(0.0025)	(0.0077)
(2) Interest x female	0.0081***	0.0083***	−0.0033**	−0.0180	0.0017	−0.0050
	(0.0030)	(0.0022)	(0.0017)	(0.1305)	(0.0020)	(0.0066)
P-value for (1)-(2)	[0.0935]	[0.0180]	[0.3981]	[0.6510]	[0.6105]	[0.8068]
(3) Payment x male	0.0013	0.0003	0.0004	−0.0133	−0.0006	0.0003
	(0.0009)	(0.0006)	(0.0005)	(0.0456)	(0.0007)	(0.0020)
(4) Payment x female	−0.0002	−0.0001	0.0005	−0.0014	−0.0008	0.0012
	(0.0008)	(0.0006)	(0.0004)	(0.0351)	(0.0005)	(0.0016)
P-value for (3)-(4)	[0.1486]	[0.6242]	[0.7995]	[0.7997]	[0.8779]	[0.6733]
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No	No	No
Counselor FE	No	No	No	No	No	No
Observations	79,739	79,739	79,739	76,008	76,008	76,008
Mean if male	0.3121	0.1100	0.1252	32.0416	0.8430	0.5633
Mean if female	0.3203	0.1211	0.0993	23.9590	0.8073	0.4079

Notes: This table reports reduced form estimates by gender. We report coefficients on the interaction of treatment, potential treatment intensity, and gender. All specifications control for gender and the potential minimum payment and interest rate changes if treated. Standard errors are clustered at the counselor level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Table 1 notes for details on the baseline controls and sample.

Appendix Table 8
Results by Race

	Start Payment	Complete Payment	Bankrupt	Earnings	Employed	401k Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Interest x white	0.0039	0.0051***	−0.0015	0.0149	0.0026	−0.0071
	(0.0030)	(0.0020)	(0.0017)	(0.1416)	(0.0018)	(0.0059)
(2) Interest x non-white	0.0093***	0.0065**	−0.0049**	0.0523	0.0016	−0.0023
	(0.0035)	(0.0029)	(0.0020)	(0.1830)	(0.0029)	(0.0081)
P-value for (1)-(2)	[0.1777]	[0.6375]	[0.1328]	[0.8381]	[0.7408]	[0.5353]
(3) Payment x white	0.0002	−0.0000	0.0002	−0.0047	−0.0007	0.0003
	(0.0007)	(0.0005)	(0.0004)	(0.0350)	(0.0005)	(0.0015)
(4) Payment x non-white	0.0008	0.0002	0.0009*	−0.0132	−0.0007	0.0021
	(0.0009)	(0.0007)	(0.0005)	(0.0530)	(0.0008)	(0.0021)
P-value for (3)-(4)	[0.5516]	[0.7785]	[0.2264]	[0.8716]	[0.9766]	[0.3938]
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No	No	No
Counselor FE	No	No	No	No	No	No
Observations	79,739	79,739	79,739	76,008	76,008	76,008
Mean if white	0.3330	0.1290	0.1155	27.1763	0.8187	0.4673
Mean if non-white	0.2858	0.0932	0.0951	26.3202	0.8234	0.4583

Notes: This table reports reduced form estimates by race. We report coefficients on the interaction of treatment, potential treatment intensity, and an indicator for being white or non-white. All specifications control for an indicator for being white and the potential minimum payment and interest rate changes if treated. Standard errors are clustered at the counselor level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Table 1 notes for details on the baseline controls and sample.

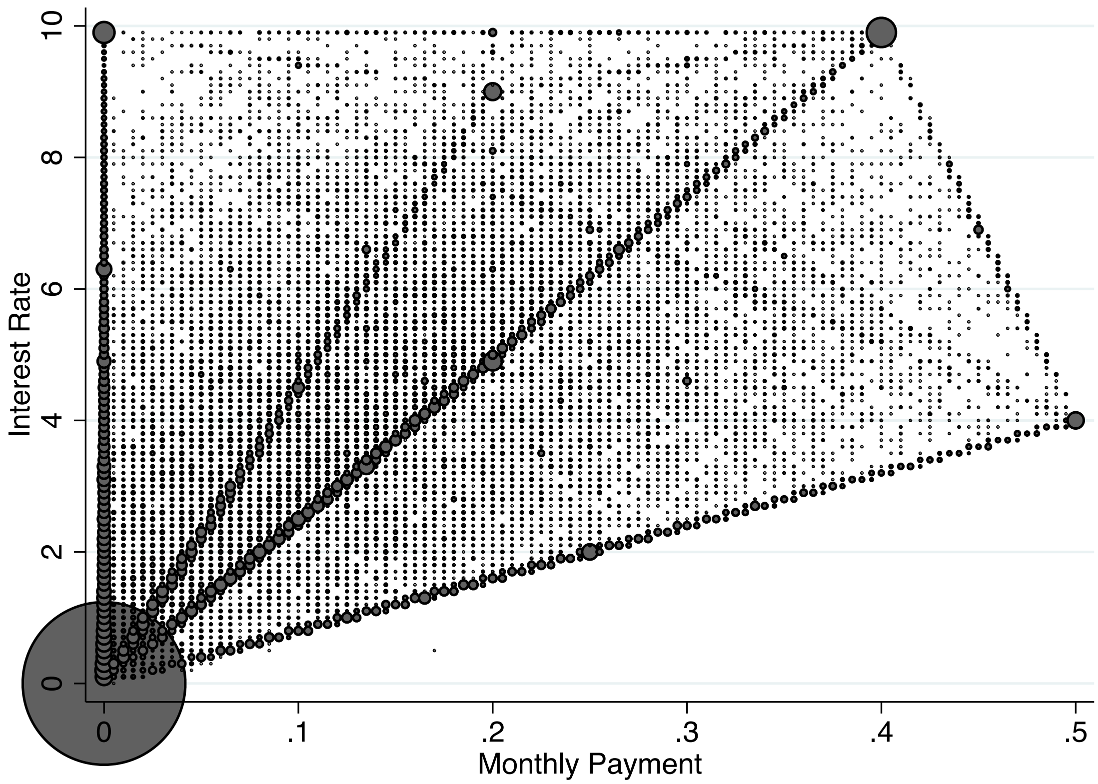
Appendix Table 9

Results by Homeownership

	Start Payment	Complete Payment	Bankrupt	Earnings	Employed	401k Cont.
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Interest x homeowner	0.0055*	0.0046**	−0.0030*	−0.0082	0.0039	−0.0034
	(0.0031)	(0.0023)	(0.0019)	(0.1883)	(0.0025)	(0.0078)
(2) Interest x non-homeowner	0.0054*	0.0060***	−0.0019	0.0670	0.0008	−0.0066
	(0.0030)	(0.0021)	(0.0017)	(0.1455)	(0.0019)	(0.0062)
P-value for (1)-(2)	[0.9658]	[0.6009]	[0.6159]	[0.7115]	[0.2710]	[0.6965]
(3) Payment x homeowner	−0.0003	−0.0002	0.0004	−0.0171	−0.0011*	0.0002
	(0.0007)	(0.0006)	(0.0004)	(0.0440)	(0.0006)	(0.0017)
(4) Payment x non-homeowner	0.0009	0.0000	0.0005	−0.0050	−0.0004	0.0011
	(0.0007)	(0.0006)	(0.0004)	(0.0373)	(0.0005)	(0.0016)
P-value for (3)-(4)	[0.1695]	[0.7502]	[0.8030]	[0.7926]	[0.2816]	[0.6513]
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No	No	No
Counselor FE	No	No	No	No	No	No
Observations	79,739	79,739	79,739	76,008	76,008	76,008
Mean if homeowner	0.3214	0.1219	0.1140	29.0246	0.7987	0.5673
Mean if non-homeowner	0.3165	0.1174	0.0963	25.3983	0.8353	0.3922

Notes: This table reports reduced form estimates by baseline homeownership. We report coefficients on the interaction of treatment, potential treatment intensity, and homeownership. All specifications control for homeownership and the potential minimum payment and interest rate changes if treated. Standard errors are clustered at the counselor level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Table 1 notes for details on the baseline controls and sample.

Appendix Figure 1
Distribution of Potential Treatment Intensity



Notes: This figure plots the distribution of potential interest rate and monthly payment changes in our estimation sample. Potential minimum payment and interest rate changes are calculated using the amount of debt held by each creditor and the rules listed in Appendix Table 1. See text for additional details.