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

Will Dobbie
Princeton University and NBER

Jae Song
Social Security Administration

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Abstract

This paper uses a randomized experiment and administrative tax and bankruptcy records to estimate the impact of loan modifications on subsequent outcomes. 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 protection. Lower interest rates also increased the probability of being employed for the most heavily indebted borrowers. In contrast, there was little impact of a longer repayment period on debt repayment, bankruptcy, or employment.

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American consumers default on more than $200 billion in non-mortgage debt each year, and more than 14 percent of consumers have at least one debt in collections (Federal Reserve Bank of New York 2014). There are significant deadweight losses for both borrowers and lenders of loan default and collection, particularly when debt contracts are incomplete (e.g. Bolton and Rosenthal 2002).[1] Lenders must either pay to collect the debt directly or sell the debt to a third-party debt collector for a fraction of the face value. In turn, borrowers have an incentive to avoid these collection efforts through potentially costly strategies, such as leaving the formal banking system to avoid asset seizures or leaving the formal labor market to avoid wage garnishment. In these kinds of scenarios, ex-post loan modifications can theoretically benefit both borrowers and lenders by completing debt contracts and preventing loan default. To date, however, there is little empirical evidence on whether loan modifications provide any benefits, and if so, what types of loan modifications are most effective increasing repayment and decreasing financial distress.

This paper uses a randomized experiment and administrative tax and bankruptcy records to estimate the impact of different loan modifications on repayment, bankruptcy, and labor supply. The experiment was designed and implemented by the largest non-profit credit counseling organization in the United States. Eleven large 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. For a typical borrower in our sample, the median interest rate reduction of 3.69 percentage points shortened the repayment period by about four months and decreased the total amount repaid by $1,712. The median monthly payment reduction of 0.14 percent of initial debt holdings, or about $26.68, lengthened the repayment period by four months and increased the total amount repaid by $289.

We identify the effects of lower interest rates and longer repayment periods using two 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, and second, borrowers in our sample owed different amounts to each of these creditors. As a result of these features, otherwise similar borrowers received very different interest rate and monthly payment re-

[1] Loan default may also generate negative externalities through price effects (e.g. Campbell, Giglio, and Pathak 2011, Mian, Sufi, and Trebbi forthcoming) or the distortion of household consumption and investment decisions (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).

ductions when treated. 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. This sizable cross-borrower variation allows us to isolate the effects of each modification by comparing the effect 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.

Treatment effects are measured using three administrative datasets matched for the purposes of this study. Debt repayment is measured using data from the credit counseling organization, which record enrollment and completion of the debt repayment program linked to the experiment. Financial distress is measured using court bankruptcy records. Labor supply and 401k contributions are measured using administrative 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.

We find compelling evidence that lower interest rates (i.e. a shorter repayment period and lower repayment costs) had significant benefits for both borrowers and lenders. 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. Over the first five post-experiment years, borrowers offered lower interest rates were also 1.07 percentage points less likely to file for bankruptcy, a 10.35 percent decrease from the control group mean. 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 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.

In sharp contrast, we find that lower monthly payments (i.e. a longer repayment period and higher repayment costs) did not appear to benefit either borrowers or lenders. The lower monthly payments had no impact on debt repayment, and, if anything, increased bankruptcy filing. Over the first five years, the median monthly payment reduction increased the probability that a borrower

filed for bankruptcy 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 decreased by 1.68 percentage points, a 2.14 percent change from the control group mean.

These results suggest that there may be significant ex-post benefits of debt forgiveness. Back-of-the-envelope calculations suggest that the median interest rate reduction increased lender profits by about $150 per borrower. Borrowers also appear to benefit from lower interest rates, as indicated by the lower bankruptcy rates and higher employment rates.[2] Conversely, we find no discernible benefits of a longer repayment period alone. This finding suggests that liquidity constraints are not likely to be an important driver of borrower behavior in our data.

These findings are broadly consistent with recent work showing that mortgage rate resets and renegotiations can decrease mortgage default and increase non-durable consumption (Agarwal et al. 2012, Di Maggio, Kermani, and Ramcharan 2014, Keys et al. 2014). There is also 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 forthcoming, Dobbie, Goldsmith-Pinkham, and Yang 2015), and that the consumer bankruptcy system as a whole can provide implicit health insurance (Mahoney 2015) and generate positive spillovers during a financial crisis (Dobbie and Goldsmith-Pinkham 2014). Our results are also consistent with recent theoretical work suggesting that mortgage payment deferrals are likely to increase the probability of default unless paired with some sort of debt forgiveness (Eberly and Krishnamurthy 2014).

There are at least two important caveats of our analysis. First, 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

[2]Of course, loan modification programs may also have important ex-ante effects on borrower behavior that we cannot measure using our empirical design. There is a large literature documenting the ex-ante effects of debt forgiveness programs 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 botyh interest rates and approval rates.

as loan delinquency and the amount of debt in collections. To partially address these concerns, we are in the process of adding individual-level credit bureau data to our analysis. These data contain extensive records on liens, judgments, collections, and most forms of secured and unsecured debt.

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, community-wide educational programs, 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 financially distressed borrowers is a debt management plan (DMP), a structured repayment program that simultaneously repays all of an individual's unsecured creditors. In exchange for voluntarily enrolling in the repayment program, creditors will reduce a borrower's monthly payments, lower or eliminate interest payments and late fees, and stop recording the debt as delinquent. 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 repaid debt. 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 also 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 participat-

ing 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, the credit counselor screens borrowers on behalf of all of the participating creditors, eliminating the need for each creditor to conduct their own screens.

If either party decides not to participate in the repayment program or a borrower fails to make all of the required payments, creditors have a number of options to collect the unpaid debt. 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 forthcoming). Debtors can make these collection efforts more difficult by ignoring collection letters and calls, changing their telephone number, or moving without leaving a forwarding address. Debtors 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, debtors can discharge unsecured debts through the consumer bankruptcy system.[3]

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 between $1.5 and $2.5 billion each year (Hunt 2005, Wilshusen 2011). In comparison, approximately 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 DMP participants. The purpose of the experiment was to evaluate the effect of more debtor-friendly loan terms on repayment rates. The new loan terms were meant to increase the number of new clients repaying their debt enough to more than offset any reduction in the amount recovered from clients who would have repaid their debts, even without the new terms. The eleven participating creditors are among the largest and most well-known unsecured creditors in the United States, owning just over 50 percent of the unsecured debt held by borrowers in our data. The resulting randomized experiment was conducted between January 2005 and August 2006, before being discontinued due to the financial crisis.

[3]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.

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 group, and date of contact. In two week intervals, each credit counselor alternated between offering every assigned client to the control or treatment groups. 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 in the treatment group were offered a repayment program that included lower interest rates and a lower minimum monthly payment. The interest rate treatment decreased the total cost of the repayment program by shortening the repayment period. However, the lower interest rates did not affect minimum monthly payments, as payment amounts were calculated using the initial balance of debt, not the total cost of repaying that debt. Conversely, the monthly payment treatment reduced these minimum monthly payments by lengthening the repayment period. The lower monthly payments also modestly increased the total cost of the repayment program due to the longer repayment period. As a result, it is possible that the monthly payment treatment could have exacerbated distress among borrowers with excessive amounts of debt.

The median treated borrower was offered an interest rate that was 3.69 percentage points lower, a 43.5 percent decrease from the control group mean of 8.50 percent, and a monthly payment that was 0.14 percent of initial debt lower, a 5.8 percent decrease from the control group mean of 2.38 percent of initial debt. Appendix Table 1 describes the effect of these treatments on 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

zero to 0.5 percent of the initial debt (see Appendix Table 2 for details). Because borrowers owed different amounts to each of the participating creditors, otherwise similar borrowers received very different interest rate and monthly payment reductions when treated. Appendix Figure 1 plots the distribution of potential treatment intensities for borrowers in our sample. 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.

The observed differences in potential treatment intensity are the result of borrowers endogenously choosing different creditors before the experiment began. Appendix Table 3 describes the correlates of the potential change in interest rate if treated, and the potential change in monthly payment if treated. We 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.

C. When Should Loan Modifications Matter?

We expect that lower interest rates and lower monthly payments to have different effects on repayment, financial distress, and labor supply, depending on the types of constraints that borrowers face.

Interest Rates: The primary channel through which lower interest rates are likely to affect repayment decisions is 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. This lower repayment burden may increase both the incentive and ability of borrowers to repay their debts. Effects are likely to be largest for borrowers who were more financially distressed. Conversely, effects are likely to be smallest for borrowers with binding liquidity constraints or with short planning horizons, as the interest rate treatment does not change minimum monthly payment amounts.

Lower interest rates may also affect financial distress through this increased repayment and decreased total debt burden. Higher repayment rates mechanically lower default rates, which is likely to increase credit scores and credit access through a cleaner credit history. Lower default rates will also decrease debt collection, which decrease borrowers' incentive to avoid collection through strategies with significant deadweight costs, such as leaving the formal banking system to avoid seizure of assets or leaving the formal labor market to avoid wage garnishment. Moreover, the decreased debt burden may increase borrowers' ability to repay debts not included in the repayment plan, such as a mortgage or auto loan. The decreased debt burden may also help prevent any sharp drops in consumption that have important long-term consequences, such as becoming sick due to the lack of medical care or losing one's home due to eviction or foreclosure.

Labor supply decisions may also be impacted by the increased repayment and decreased debt burden. A lower debt burden increases borrowers' wealth, potentially decreasing the incentive to work. However, increased repayment rates help protect future wages from wage garnishment, potentially increasing the incentive to work.[4] Thus, the interest rate treatment will increase labor supply if the substitution effect associated with a lower implicit tax rate dominates any potential wealth effect. If borrowers are not subject to wage garnishment or the wealth effect dominates, however, the interest rate treatment is likely to decrease labor supply.

Monthly Payments: There are at least two channels through which lower monthly payment obligations may affect repayment decisions. First, lower monthly payments may help relax liquidity constraints. It is possible that distressed borrowers are no longer able to borrow or that the cost of additional borrowing is prohibitively costly. If these liquidity constrained borrowers experience an

[4] 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, therefore increasing the marginal return to work.

unexpected earnings or expense shock, they may have to involuntarily default on their debts. In this scenario, lower monthly payments can increase repayment rates by helping borrowers smooth consumption. Conversely, lower monthly payments may decrease repayment rates by increasing the total cost of the repayment program. Recall that the median payment reduction increases the total cost of the repayment program by about 1.33 percent. This higher repayment burden may decrease both the incentive and ability of borrowers to repay their debts.

Financial distress and labor supply decisions are most likely to be affected by lower monthly payment obligations through any potential changes in repayment. 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, lower payments are likely to increase financial distress and decrease labor supply if repayment rates either 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 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, dependents, treatment status, enrollment in a repayment program, and completion of the 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 repayment. 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. 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.

We also use the MMI data to calculate potential treatment intensity for each individual in our sample. 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. Next, we 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 follow MMI's suggestion and 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. For debt held by participating creditors, we use the concessions detailed in Appendix Table 2. Finally, for each individual, we take the difference between the control interest rate and the treatment interest rate, and the control monthly payment and treatment monthly payment. 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 2012 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.[5] All specifications control for state fixed effects to account for the fact that we do not observe filings in all areas. 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.

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

[5]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.

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 mortality outcomes consists of the 76,008 individuals in the matched dataset.

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 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 are 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 before contacting MMI. 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 4 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.[6] 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.

Equation (1) isolates the effect of each loan modification by comparing the effect of the randomized experiment across borrowers that differed in their potential treatment intensities. We therefore interpret any treatment effect differences across these borrowers as the causal effect of the different treatment intensities.[7] 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

[6] We do not control for the direct effect of treatment eligibility when estimating equation (1). This approach is consistent with the experimental design discussed in Section IIB. 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.

[7] Our empirical strategy is similar in spirit to earlier work that 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 studies, our treatment and control groups are determined by random assignment, not state or federal law changes.

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.

To partially test the validity of this identifying assumption, Appendix Table 5 presents subsample results by predicted treatment intensity. We use the descriptive results from columns 2 and 4 from Appendix Table 3 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. Taken together, these results suggest that our main results may be modestly biased towards zero. Most importantly, it appears unlikely that our estimates are biased upwards. Nevertheless, our estimates should be interpreted with these potential issues in mind.

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. a shorter repayment period 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 percent.

Conversely, we find little impact of lower minimum payments (i.e. a longer repayment period 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. This suggests that liquidity constraints were not a primary reason that borrowers were not repaying their debts, at least in our data.

Table 5 presents estimates interacted with an indicator for having an above or below median baseline debt-to-income ratio, a proxy for financial distress. Lower interest rates and lower minimum payments are likely to have larger effects on more financially distressed borrowers for at least two reasons. 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. 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 6-8 present additional subsample results by gender, ethnicity, and homeownership. 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.

We conclude this section with a back-of-the-envelope calculation of the expected value of a lower interest rate from a lender's perspective. To simplify the calculation, we assume homogeneous treatment effects and no partial repayment. We also assume that the lender is risk neutral and does not discount future payments. Under these assumptions, the average borrower in the control group repays an average of about $2,586. Using our point estimate from Table 2, the average repayment amount increases to about $2,736 if that borrower is offered the median interest rate

cut. Thus, lenders gain approximately $150 for each borrower offered the median interest rate reduction. For borrowers with above median debt-to-income levels, lenders gain approximately $647 for each borrower offered the median interest rate reduction.

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 liquidity constraints are not likely

to be an important driver of borrower behavior in our data.

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 $150 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

	Interest Rate		Monthly Payment		
Creditor	Treatment	Control	Treatment	Control	Dates of Participation
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
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 4
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 5
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 6
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 7
Results by Ethnicity

	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 ethnicity. 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 8
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

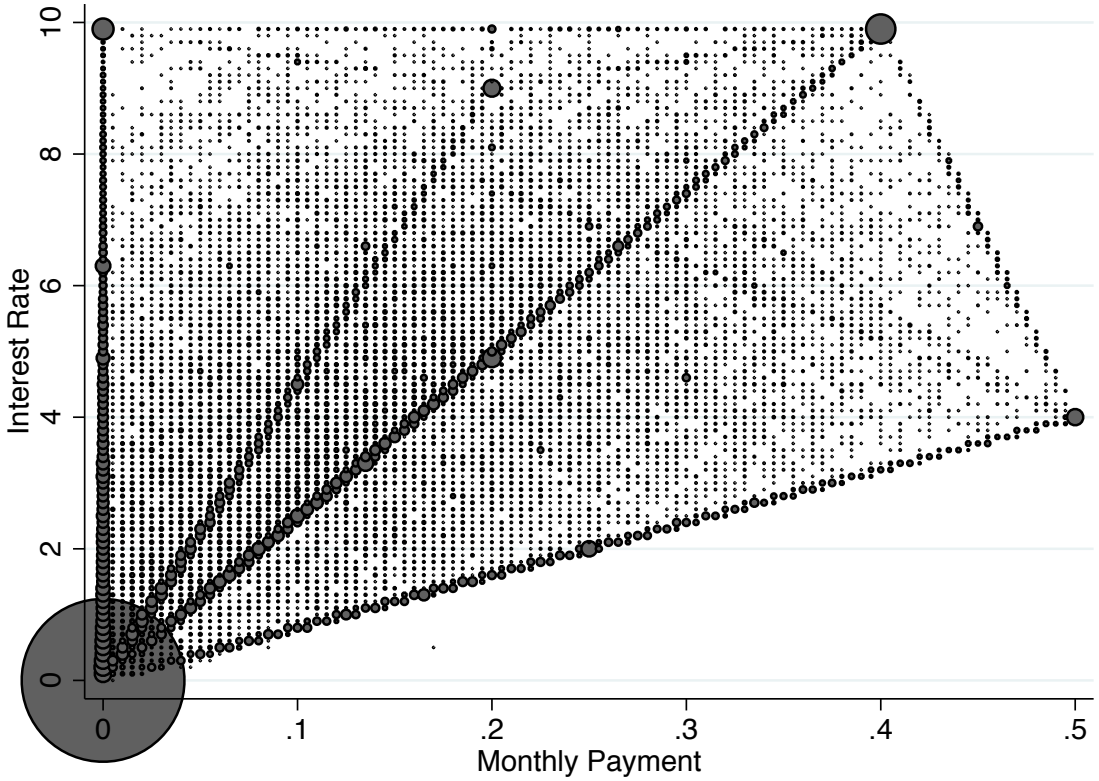
Notes: This table reports reduced form estimates by baseline homeownership. We report coefficients on the interaction of treatment, potential treatment intensity, and homeownership. All specifications control for homeownership and the potential minimum payment and interest rate changes if treated. Standard 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 Table 9
ITT Results

	Control Mean	ITT Estimates		
	(1)	(2)	(3)	(4)
Start Payment	0.3185	0.0198***	0.0196***	0.0211***
	(0.4659)	(0.0061)	(0.0058)	(0.0059)
Complete Payment	0.1193	0.0152***	0.0149***	0.0151***
	(0.3241)	(0.0046)	(0.0043)	(0.0044)
Bankruptcy	0.1036	−0.0021	−0.0031	−0.0036
	(0.3111)	(0.0036)	(0.0035)	(0.0036)
Earnings	26.8915	0.1228	0.0704	0.0633
	(22.4094)	(0.3025)	(0.1748)	(0.1811)
Employment	0.8202	0.0024	−0.0015	−0.0010
	(0.3349)	(0.0039)	(0.0025)	(0.0026)
401k contributions	0.4643	−0.0030	0.0016	0.0007
	(0.9611)	(0.0131)	(0.0102)	(0.0105)
Strata FE	–	No	Yes	Yes
Baseline Controls	–	No	Yes	Yes
Counselor FE	–	No	No	Yes
Observations	40,496	79,739	79,739	79,739

Notes: This table reports ITT results. 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.