Debt Relief or Debt Restructuring? Evidence from an Experiment with Distressed Credit Card Borrowers*

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Abstract

This paper reports results from a randomized field experiment that offered distressed credit card borrowers more than $50 million in debt forgiveness and over 27,500 additional months to repay their debts. The experimental variation effectively randomized interest rates and repayment periods for debts held by 11 large credit card issuers. Merging information from the experiment to administrative tax and bankruptcy records, we find that lowering a borrower's interest rates increased debt repayment and decreased bankruptcy filing in the five years following the experiment. Lower interest rates also increased the probability of being employed for the most financially distressed borrowers. In contrast, we find little impact of restructuring debts to have a longer repayment period on debt repayment, bankruptcy, or employment. We use an economic model to show that this null result can be explained by the presence of two offsetting mechanisms: a positive short-run effect of increased borrower liquidity and a negative long-run effect of exposing borrowers to more default risk.

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During the 2007-2009 financial crisis, U.S. policymakers implemented a series of reforms that encouraged lenders to forgive or restructure distressed mortgage debt in an attempt to stimulate the economy.[1] The largest credit card issuers also began reducing financing fees and lengthening repayment periods for distressed credit card borrowers over this time period, lobbying government officials for permission to extend even more generous concessions to these borrowers.[2] In theory, forgiving or restructuring distressed debts can increase aggregate consumption and employment during economic downturns (e.g. Hall 2011, Eggertson and Krugman 2012, Mian, Rao, and Sufi 2013, Mian and Sufi 2014). Debt modifications can also be ex-post efficient even in regular economic conditions if debt contracts are incomplete (Bolton and Rosenthal 2002, Bolton and Scharfstein 2006) or there are negative spillovers from loan default (e.g. Campbell et al. 2010, Mian, Sufi, and Trebbi 2011, Guiso, Sapienza and Zingales 2011).[3] To date, however, there is little empirical evidence on how forgiving or restructuring distressed debt affects borrowers.

This paper provides new evidence of the impact of debt relief and debt restructuring using information from a large randomized field experiment matched to administrative tax and bankruptcy records. The experiment was designed and implemented by the largest non-profit credit counseling organization in the United States. Eleven large credit card issuers agreed to offer lower interest rates and longer repayment periods to approximately 40,000 distressed credit card borrowers that contacted the non-profit organization between January 2005 and August 2006. For the typical borrower in our sample, the median interest rate reduction of 3.69 percentage points decreased financing charges by $1,712, a 50.83 percent reduction. The median repayment period increase of

[1] These reforms included the Hope Now initiative that asked lenders to prevent adjustable-rate mortgages from increasing to higher rates at the first mortgage rate reset, the Home Affordable Refinancing Program (HARP) that provided federal guarantees on refinances of eligible mortgages, and the Home Affordable Modification Program (HAMP) that provided financial incentives to modify distressed mortgages. See Agarwal et al. (2012) for estimates of the ex-post effects of HAMP, and Mayer et al. (2014) for estimates of the ex-ante effects of announcing a similar type of mortgage modification program by Countrywide Financial Corporation.

[2] In the U.S., credit card issuers are not allowed to simultaneously reduce a borrower's original principal and significantly lengthen his or her repayment period. In cases where the original principal can be reduced, credit card borrowers are normally required to pay off the remaining debt in just a few months. The Financial Services Roundtable, which represents more than 100 large financial companies, and the Consumer Federation of America, a large consumer rights advocate, proposed amending these regulations to allow issuers to pilot a program that would have forgiven up to 40 percent of a credit card borrower's original principal, restructured the remaining principal to be repaid over a number of years, and deferred any income taxes owed on the forgiven principal until after the full debt was paid off. Reports indicated that many of the largest credit card issuers were interested in participating in the proposed pilot program. For example, see `http://www.huffingtonpost.com/2008/10/30/banks-asking-for-credit-c_n_139478.html`

[3] There may also be important ex-ante effects of allowing ex-post loan modifications. See Bolton and Rosenthal (2002) for a discussion of the ex-ante and ex-post efficiency of debt relief when debt contracts are incomplete, and Mayer et al. (2014) for estimates of the ex-ante response to the announcement of a mortgage modification program.

four months reduced the typical borrower's minimum payment by $26.68, a 6.15 percent reduction. In total, treated borrowers were offered more than $50 million in debt forgiveness and over 27,500 additional months to repay their debts as a part of the experiment.

We separately identify the effects of lower interest rates and longer repayment periods using two unique features of the experiment. First, each of the 11 credit card issuers participating in the experiment offered a different combination of interest rate and monthly payment reductions. Second, individual borrowers in our sample owed different amounts to the participating issuers. As a result, otherwise similar borrowers received very different interest rate and repayment period offers. This sizable cross-borrower variation allows us to isolate the effects of each debt modification by comparing the impact of the randomized experiment across borrowers that differed in their "potential treatment intensity," that is, the interest rate and minimum payment offers that they actually received if treated, or the offers that they would have received if not 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 administrative records from the credit counseling organization. Financial distress is measured using court bankruptcy records. Earnings, employment, and 401k contributions are measured using tax data from the Social Security Administration (SSA). The matched dataset allows us to estimate the medium-run effects of the lower interest rates and longer repayment periods across a range of important outcomes.

We begin our empirical analysis by documenting the reduced form effects of lower interest rates and longer repayment periods on subsequent borrower outcomes. We find that lower interest rates had significant ex-post benefits for both lenders and borrowers, particularly when given to the most financially distressed borrowers. Among these most distressed borrowers, the median interest rate reduction increased the probability of full debt repayment by 2.50 percentage points, a 16.92 percent increase from the control group mean, with back-of-the-envelope calculations suggesting that lender profits increased by at least $23 per borrower. We also find that lower interest rates decreased the probability that the most distressed borrowers filed for bankruptcy protection in the next five years by 1.36 percentage points, an 11.98 decrease, and increased the probability of being employed over the same time period by 1.69 percentage points, a 2.17 percent increase. The estimated effects of lower interest rates on both earnings and 401k contributions are small

and not statistically significant for all borrowers except those unemployed in the year prior to the experiment. For these unemployed borrowers, lower interest rates decreased earnings by $2,077 and decreased 401k contributions by $60.14, consistent with the idea that debt forgiveness can decrease the incentive to work for borrowers closet to the margin of working or not working.

In sharp contrast, we find that there are no economically significant benefits of a longer repayment period for either borrowers or lenders. Lengthening the repayment period via lower minimum payments had little impact on debt repayment, with the 95 percent confidence interval ruling out treatment effects larger than 0.15 percentage points. The median payment reduction also *increased* the probability of filing for bankruptcy protection in the next five years by a statistically insignificant 0.70 percentage points, a 6.75 percent increase from the control group mean. There were also no detectable effects of lower minimum payments on employment, earnings, or 401k contributions. Taken at face value, these reduced form results suggest that either liquidity constraints are not an important driver of borrower behavior in our setting, or that a longer repayment period is an ineffective way to alleviate liquidity constraints.

In the second part of the paper, we attempt to disentangle the channels driving these surprising reduced form effects using a simple economic framework that includes both (1) strategic default risk from debt overhang and (2) non-strategic default risk from potentially binding liquidity constraints. We show that lower interest rates increase repayment by decreasing borrowers' incentive to strategically default at the beginning of the repayment program through increased solvency, and by decreasing borrowers' exposure to non-strategic default risk at the end of the repayment program through a shorter repayment period. In contrast, lower minimum payments have an ambiguous impact on repayment rates largely due to two opposing channels: a decrease in non-strategic default at the beginning of the repayment program through the lower required payments, and an increase in exposure to non-strategic default risk at the end of the repayment program through a longer repayment period.[4]

The key insight of the model is that it is possible to bound the importance of these competing channels for explaining the reduced form repayment results using estimates from different points

[4]The model also implies that lower minimum payments change the option value of repayment, and hence the incentive to default strategically. The direction of this strategic effect is ambiguous as lower payments both increase future flexibility, increasing the option value of repayment, and transfer a portion of the debt burden into the future, decreasing the option value of repayment. We assume throughout that the liquidity effect net of these indirect strategic channels is positive. See Section IV for additional details.

during the repayment program. We find that at least 85.2 percent of the reduced form interest rate effect is due to fewer strategic defaults, with less than 14.8 percent of the reduced form effect explained by the decreased exposure to non-strategic default risk at the end of the repayment program. In contrast, we find that both liquidity and exposure effects are important drivers of the reduced form minimum payment effect. Consistent with the model, we find that lower minimum payments had decreased liquidity-based defaults early in the repayment program. However, this positive liquidity effect was nearly exactly offset by the increased exposure to non-strategic default risk at the end of the experiment.

Our results highlight the potential unintended consequences of restructuring debts to have a longer repayment period when borrowers face significant default risk, and help to reconcile our reduced form estimates with the large literature documenting the importance of 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, Agarwal et al. 2015). In the papers most related to this study, there is evidence that the probability of mortgage default falls and non-durable consumption increases just after anticipated reductions in mortgage interest rates (Di Maggio, Kermani, and Ramcharan 2014, Keys et al. 2014, Fuster and Willen 2015). The lack of any anticipatory response before these mortgage rate resets suggests that liquidity constrains severely constrain borrower behavior. However, the prior literature has been largely unable to identify specific programs or policies that can alleviate these types of liquidity constraints. Our estimates suggest that a longer repayment period is likely an ineffective way to address borrowers' liquidity constraints, even in a setting where liquidity constraints appear to be an important barrier to debt repayment.

This paper is also related to an emerging literature estimating 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 defaults on non-mortgage debt, although it is unclear whether the effects were driven by lower interest rates, principal reductions, or extensions to the repayment period (Agarwal et al. 2012). There is also evidence from cross-sectional comparisons that suggests that principal forgiveness is more effective than other types of mortgage modifications (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 debt relief (Eberly and Krishnamurthy 2014). We view our results are

being broadly consistent with the findings from this literature, although in a very different context.

Finally, this paper is related to recent work estimating the effects of consumer bankruptcy protection, which combines elements of both debt relief and debt restructuring. There is evidence that bankruptcy protection decreases recipients' financial distress (Dobbie, Goldsmith-Pinkham, and Yang 2015) and increases recipients' earnings and employment (Dobbie and Song 2015). There is also evidence that the consumer bankruptcy system provides implicit health insurance (Gross and Notowidigdo 2011, Mahoney 2015) and can generate positive spillovers on aggregate consumption and employment during a financial crisis (Dobbie and Goldsmith-Pinkham 2014). However, none of these papers are able to identify the mechanisms through which bankruptcy protection benefits debtors, or whether lenders might also benefit from ex-post debt relief or debt restructuring.

The remainder of this paper is structured as follows. Section I describes the institutional setting and experimental design. Section II details our data and empirical design. Section III presents our reduced form results. Section IV provides a simple economic model to interpret our estimates. Section V concludes.

I. Background and Experimental Design

A. Background

The randomized trial described in this paper 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 sessions.

One of the most important products offered by MMI 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. The remaining administrative costs are 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 reduce the borrower's monthly payments, lower or

eliminate interest payments and late fees, and stop recording the debt as delinquent on the borrower's credit report. The repayment program usually takes about three to five years to complete, with the exact length depending on the terms offered by creditors and the amount of debt to be repaid. The minimum payment to each creditor typically ranges from two to three percent of the initial debt each month. While in theory borrowers can choose to contribute more than the minimum amount, in practice the vast majority of borrowers use the minimum payment amount (see Appendix Figure 1). In our sample, the average monthly payment for the control group is 2.38 percent of initial debt holdings, or about $437 per month.

In practice, the DMP is typically presented to clients along with a number of other repayment options. The first option for most borrowers is to liquidate their assets and repay their debts immediately, although relatively few distressed borrowers have enough assets to make this a viable option.[5] A second and typically more attractive option is to file for bankruptcy protection. Chapter 7 bankruptcy allows borrowers to discharge their unsecured debts and avoid debt collection in exchange for any non-exempt assets and any court fees. In practice, repayment rates in Chapter 7 average less than one percent (Sullivan et al. 1989) and Chapter 7 court fees averaged $921 before 2005 to $1,377 after 2005 (GAO 2008). In our data, 5.78 of the control group files for bankruptcy protection in the year following their first MMI counseling session.

Borrowers who are not eligible or not interested in filing for bankruptcy protection usually have two remaining options. The first is to continue making their current payments. In a representative call provided to the research team, MMI explained to the client that "if you continue making the minimum payment of $350, it will take you 348 months to repay your credit cards and you will have to spend about $21,300 in financing charges." The second option is to enroll in a repayment program. In calls with clients, MMI explained that if the client enrolled in MMI's structured repayment program, her payments would "drop to $301, and you would repay all of your credit cards in 56 months and only have $3,800 in financing charges. That is a savings of about $17,500." In our data, 31.85 percent of the control group enrolls in a DMP, and 11.93 percent completely repay their debts through the program.

Creditor participation in the repayment program is also voluntary. From a creditor's perspective, there are at least three reasons to prefer the DMP to outside options such as insisting on full

[5]Private communication with MMI.

repayment or negotiating a bilateral workout with the borrower. First, the DMP allows participating creditors to internalize many of the externalities associated with bilateral loan modifications, including positive effects on the ability to repay debts and negative effects on the incentive to repay non-modified debts. Second, MMI eliminates the need for each creditor to conduct their own eligibility screens and negotiations by helping to match borrowers to the appropriate debt product. Finally, the DMP may increase total recovery rates through higher repayment and lower delinquency rates.[6]

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).

B. Experimental Design

In 2003, MMI and 11 large credit card issuers agreed to offer lower interest rates and longer repayment periods 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, particularly for the most financially distressed borrowers. The 11 participating credit card issuers are among the largest 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.[7]

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,

[6] Creditors have a number of options to collect unpaid debts 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. 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.

[7] Many of the credit card issuers that participated in the experiment subsequently offered more borrower-friendly loan terms to borrowers contacting MMI during the financial crisis. These improved concessions were typically limited to the most financially distressed borrowers. Unfortunately, data from this time period are not available.

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 longer repayment periods than they would have received if they had been assigned to the control group. Borrowers offered a lower interest rate had lower financing costs through a shorter repayment period, not lower monthly payments. Thus, lower interest rates are more likely to benefit borrowers by increasing their solvency, not be decreasing liquidity constraints. Conversely, borrowers offered a longer repayment period had lower minimum monthly payments but somewhat higher financing costs, and is therefore likely to benefit borrowers by decreasing the probability of a binding liquidity constraint. Of course, the effects of both treatments will be mediated through the benefits and costs of the program characteristics as perceived by borrowers in our sample, including any behavioral responses to the particular way in which program characteristics are described. For example, it is possible that borrowers will view a reduction in financing charges as being more beneficial than the equivalent interest rate reduction. It is therefore important to interpret the results with the framing of the repayment program in mind. Importantly, however, the repayment program was described in exactly the same way to borrowers in the treatment and control groups during the experiment. As a result, the internal validity of the experiment will be unaffected by this issue.

Conditional on having at least one debt with a participating bank, 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. Appendix Table 1 further details the effect of these treatments on various repayment program attributes for a representative borrower. Panel A presents 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). Panel B and C show how each program attribute changes with various interest rate and monthly payment treatments. The median interest rate reduction would shorten the repayment period by about four months,

a 8.00 percent change, and decrease the financing charges by $1,712, a 50.83 percent change. For the same representative borrower, the median monthly payment reduction would lengthen the repayment period by four months and increase the financing charges by $289, a 8.29 percent change.

Importantly, each of the 11 issuers 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 issuers. 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. For example, moving from the 25th to the 75th percentile interest rate reduction shortens the repayment period by about 3.5 months and decreases the financing charges by $1,521. Similarly, moving from the 25th to the 75th percentile monthly payment reduction increases the repayment period by just over five months, and the financing charges by $387. In Section II.C, we explain how we use this cross-borrower variation in treatment intensity to isolate the effects of each debt modification. Intuitively, our approach compares the impact of the randomized experiment across borrowers that differed in the interest rate and repayment period changes that they would have received if treated.[8]

II. Data and Empirical Design

A. Data Sources and Sample Construction

To estimate the impact of the randomized treatments, we match counseling data from MMI to administrative tax and bankruptcy records. 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

[8]There is also significant independent cross-borrower variation in the treatment intensity. While 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 and 9.9 percent received above median reductions for only monthly payment reductions. The remaining 40.0 percent of borrowers 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 issuer and Appendix Figure 2 for a graphical illustration of the potential treatment intensities for borrowers in our sample.

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, the assigned counselor, and 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 also use the data provided by MMI 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 issuers offering different concessions to treated borrowers. To measure this variation in treatment intensity, we calculate the interest rate and monthly payment for all individuals as if they had been assigned to both the control and treatment groups using the concessions detailed in Appendix Table 2. We then calculate the difference between the control interest rate and the treatment 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.[9]

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.[10] 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, and control for state fixed effects in all specifications to account for the fact that we do not observe filings in all states.

Information on labor market outcomes and 401k contributions comes from administrative tax records from the SSA. The SSA data are available from 1978 to 2013 for every individual who

[9] We have information on interest rates and minimum payments for the 19 largest creditors in the sample, including all 11 of the credit card issuers participating in the experiment. For the 16.7 percent of debt holdings held by smaller creditors, we assume an interest rate of 6.7 percent and a minimum payment of 2.25 percent. These assumptions follow MMI's internal guidelines for calculating expected DMP payments. Results are also robust to a wide range of alternative assumptions.

[10] See Gross, Notowidigdo, and Wang (2014) for additional details on the bankruptcy data used in our analysis.

has ever acquired a SSN, including those who are institutionalized. Illegal immigrants without a valid SSN are not included in the SSA data. Information on earnings, employment, and annual 401k contributions come from annual W-2s.[11] 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).

We make two sample restrictions to the final dataset. First, we drop individuals that are not randomly assign to counselors because they need specialized services 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 40,496 individuals in the control group and 39,243 individuals in the treatment group. Our sample for the employment and 401k outcomes is further restricted to 76,008 individuals matched to the SSA data.

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 credit card issuer 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 in the SSA data are approximately $23,500, slightly lower than the self-reported household earnings reported in the MMI data. These results suggest that either some individuals in our sample

[11] The SSA data also include information on mortality and Disability Insurance receipt. Very few individuals in our data die or receive Disability Insurance during our sample period, and estimates on these outcomes are small and not statistically different from zero.

are not the sole earner in the household, or that there is upward bias in the self-reported earnings. Eight-five percent of borrowers in our sample are employed at baseline according to the SSA data. Baseline bankruptcy rates are very low, 0.3 percent, likely because individuals are unlikely to contact a credit counselor if they have already received bankruptcy protection. Finally, baseline 401k contributions are $373 for borrowers in our sample.

Panel C of Table 1 presents measures of treatment intensity calculated using the MMI data. Including zeros, the mean minimum payment reduction is 0.09 percent of initial debt, a 3.78 percent change from the control group mean of 2.38 percent of initial debt. Treatment reduces interest rates by an average of 2.6 percentage points, a 31.7 percent change from the control mean of 8.50 percent. The median minimum payment and interest rate reductions are both larger than the mean reductions reported in Table 1, at 0.14 percent of initial debt and 3.69 percentage points, respectively.

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 0.723, suggesting that the randomization was successful.

Appendix Table 3 presents additional tests for balance. Following our main specification described below, we regress each baseline variable 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.

C. Empirical Strategy

We estimate the impact of lower interest rates and minimum monthly payments using the following reduced form specification:

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

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 also include the individual controls listed in Table 1 when estimating equation (1). We cluster standard errors at the counselor level in all specifications.[12]

Equation (1) isolates the effect of each treatment by comparing the impact 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. Our empirical strategy is closely related 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. However, in contrast to these earlier studies, the treatment and control groups in our setting are determined by random assignment.

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

[12] Note that equation (1) assumes that that there are no direct effects of treatment eligibility and that the impact of lower interest rates and longer repayment periods are linear and additively separable. Consistent with the first assumption, our reduced form results are unchanged when we add an indicator for treatment eligibility, and the coefficient on the indicator for treatment eligibility is small and not statistically different from zero. To partially test the second assumption, Appendix Table 4 presents non-parametric results using bins of treatment intensity 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.

with greater sensitivity to interest rate or monthly payment changes are more likely to borrow from the issuers who offered more generous debt 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 debt modification, not the greater sensitivity of the individuals who chose that bank. Conversely, our estimates would be biased downwards if these individuals with greater sensitivities are less likely to borrow from the issuers who offered more generous debt modifications.

To partially test the validity of our identifying assumption, Appendix Table 5 examines whether our potential treatment intensity variables capture all of the relevant variation in issuer specific treatment effects. Our identifying assumption would be violated if borrowers from a particular issuer systematically experience smaller or larger treatment effects than would be predicted from the potential treatment intensity variable. Appendix Table 5 reports coefficients of indicators for holding debt with each of the 11 credit card issuers participating in the experiment interacted with treatment eligibility. We also control for treatment eligibility interacted with potential treatment intensity, potential treatment intensity, the individual controls listed in Table 1, strata fixed effects, and non-interacted indicators for holding debt with each of the 11 credit card issuers. None of the credit card issuers have systematically larger or small treatment effects once we control for the direct effect of lower interest rates and lower minimum payments, and the p-values from F-tests of the joint significance of the issuer interactions range from 0.369 to 0.940. None of the results suggest that our identifying assumption is invalid in our setting.

To provide additional evidence on this issue, Appendix Table 6 presents subsample results by predicted treatment intensity.[13] We first use the baseline characteristics available from Table 1 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

[13]Appendix Table 7 describes the correlates of potential treatment intensities. 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. Not surprisingly, borrowers with more debt with issuers participating in the experiment and less debt with issuers not participating in the experiment have larger potential treatment intensities.

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. Our estimates should be interpreted with this potential issue in mind.[14]

III. Results

A. Debt Repayment

Table 2 present estimates of the impact of lower interest rates and lower minimum payments on starting and completing a structured repayment program. We report the coefficient on treatment eligibility interacted with the potential percentage point change in interest rates, and the coefficient on treatment eligibility interacted with the potential percentage point change (multiplied by 100) in the required minimum payment. All specifications control for potential treatment intensity, the baseline controls listed in Table 1, and strata fixed effects. Standard errors are clustered at the counselor level throughout. Figure 1 presents analogous results for each percentile of debt repayment.

Interest Rate Results: There is an economically significant impact of lower interest rates (i.e. shorter repayment periods and lower financing costs) on both starting and completing repayment. Borrowers offered the median interest rate reduction of 3.69 percentage points were 1.84 percentage points more likely to start a repayment program, a 5.79 percent increase from the control group mean of 31.85 percent. Figure 1 shows that both treatment and control borrowers exit the repayment program at high rates, with only 13.66 percent of the control group completely repaying their debts. The effect of lower interest rates remains roughly constant throughout the repayment program, with treated borrowers being 1.62 percentage points, or 11.88 percent, more likely to repay their debts compared to control borrowers.

Columns 2-3 and 5-6 of Table 2 present estimates for borrowers with above and below median baseline debt-to-income ratio, a proxy for financial distress. The more generous debt modifications

[14] A third potential test of our identifying assumption would be to compare the effects of treatment eligibility for borrowers with different creditors but identical treatment intensities (i.e. borrowers with all debts held by one of the three issuers that reduced interest rates by 9.9 percentage points and reduced monthly payments by 0.4 percentage points). Unfortunately, there are too few borrowers meeting these criteria to provide empirically informative tests of our identifying assumption.

were originally meant to be implemented only for more financially distressed borrowers, and subsequent reforms have primarily targeted these types of individuals. We find that borrowers with above median debt-to-income ratios were 3.18 percentage points more likely to start and 2.50 percentage points more likely to complete repayment if offered the median interest rate cut, a 16.92 percent increase from the control mean for this subsample. In comparison, there were no statistically significant effects of lower interest rates on borrowers with below median debt-to-income ratios.

Appendix Tables 8-10 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.

We conclude this section by considering whether lenders benefit from offering lower interest rates. To shed light on this issue, 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, repayment rates outside of the repayment program is not available in our data. Credit card issuers 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.[15] 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 about 2.0 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

[15] Private communication with MMI and anonymous credit card issuers.

$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 participating in the experiment. The ex-post benefits to creditors are also clearly larger if the lower interest rates were only given to more financially distressed borrowers.

Monthly Payment Results: In contrast to the interest rate results 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. Figure 1 shows that these results hold over every percentile of debt repayment. We also find no effect of lower minimum payments for borrowers with either above or below median debt-to-income ratios, or among any of the subsample groups we consider in Appendix Tables 8-10.

The null effect of a longer repayment period is particularly surprising given a large and influential literature documenting liquidity constraints in a variety of similar contexts (e.g. Gross and Souleles 2002, Johnson, Parker, and Souleles 2006, Agarwal et al. 2007, Parker et al. 2013). For example, recent work suggests that anticipated mortgage interest rate reductions decrease the probability of default and increase non-durable consumption, with no evidence of effects just before the rate resets (Di Maggio, Kermani, and Ramcharan 2014, Keys et al. 2014, Fuster and Willen 2015). Our 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. We return this issue in Section IV.

B. Bankruptcy

Table 3 presents estimates of the effect of debt modifications on bankruptcy filing in the five years following the experiment, a proxy for financial distress and an important outside option for borrowers in our sample. Bankruptcy allows most borrowers to discharge their unsecured debts in

exchange for either their non-exempt assets or the partial repayment of debt. Bankruptcy filings are reported on a borrower's credit report for 7 to 10 years, potentially decreasing access to new credit (Liberman forthcoming) and new employment opportunities (Bos, Breza, and Liberman 2015). However, conditional on filing, there is evidence that bankruptcy protection improves recipients' labor market outcomes, health, and financial well-being (Dobbie and Song 2015, Dobbie, Goldsmith-Pinkham, and Yang 2015). MMI discusses these costs and benefits of bankruptcy with most borrowers, and 10.36 percent of the control group files for bankruptcy in the first five years filing the experiment.

Interest Rate Results: 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 0.99 percentage points less likely to file for bankruptcy, a 9.61 percent decrease from the control mean of 10.36 percent. In Appendix Table 11, we show that the decrease in bankruptcy filing is largely driven by reductions in the second and third post-experiment years. Consistent with our repayment results, we also 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 again neither difference is statistically significant.

If lower interest rates only impact bankruptcy through increased debt repayment, we could use treatment eligibility as an instrumental variable for repayment. The resulting two-stage least squares estimates would measure the local average treatment effect of debt repayment for borrowers' induced to repay because of the experiment. These estimates would be approximately equal to our reduced form bankruptcy estimates divided by the "first stage" repayment results presented in Table 2, implying that debt repayment decreases the probability of filing for bankruptcy by about 46.23 percentage points. Of course, it is likely that lower interest rates also effect bankruptcy filing through other channels and that these calculations overstate the true effects of debt repayment on bankruptcy filing.

Monthly Payment Results: 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 Appendix Table 11, we show that there are statistically significant increases in the probability of filing in the fifth post-experiment year, suggesting that lower monthly payments may exacerbate financial distress at the end of the experiment while having no positive or negative effects at the beginning of the experiment.

C. Labor Market Outcomes

Table 4 presents estimates of the effect of debt modifications on annual employment and earnings averaged over the first five years following the experiment. Lower interest rates and longer repayment periods could theoretically effect labor market outcomes through a number of channels. For example, debt modifications could increase labor supply by protecting wages from creditor garnishments that occur when an employer is compelled by a court order to withhold a portion of an employee's earnings to repay delinquent debt. Debt modifications could also impact either labor supply or labor demand market changes in the credit score and credit access (e.g. Herkenhoff 2013, Bos, Breza, and Liberman 2015, Herkenhoff and Phillips 2015) or through the effect of financial distress on productivity (e.g. Mullainathan and Shafir 2013).

Interest Rate Results: The estimated effect of interest rates on employment and earnings is small and relatively imprecisely estimated in the full sample of borrowers. The 95 percent confidence interval for the employment effect ranges from -0.41 to 1.89 percentage points, while the 95 percent confidence interval for the earnings ranges from -$649 to $347. The effect of lower interest rates on employment is larger for borrowers with above median debt-to-income ratios, although the effect on earnings remains small and imprecisely estimated. For these heavily indebted borrowers, the median interest rate reduction increased employment rates by 1.69 percentage points over the first five post-randomization years, a 2.17 percent increase from the control mean.

Consistent with our repayment and bankruptcy results, we find similar effects by gender, ethnicity, and homeownership. However, Appendix Table 12 reveals contrasting labor market effects by baseline employment status. Lower interest rates decrease earnings by $2,077 for borrowers who

were unemployed in the year prior to the experiment, while having essentially no effect on borrowers employed at baseline. The employment effects are also negative for borrowers unemployed at baseline, but the estimates are not statistically significant. These results suggest that the debt relief provided by lower interest rates may decrease labor supply for borrowers most on the margin of work.

In contrast to the relatively modest spillover effects of lower interest rates, Dobbie and Song (2015) find that Chapter 13 bankruptcy protection increases annual earnings by $5,562 and annual employment by 6.8 percentage points. There are at least three potential explanations for these contrasting results. First, bankruptcy protection discharges approximately 80 to 85 percent of the typical bankruptcy filer's unsecured debt (Dobbie, Goldsmith-Pinkham and Yang 2015), compared to a 9.63 percent reduction of unsecured debt offered by the lower interest rate treatment in our experiment. Second, Chapter 13 bankruptcy also protects future wages from garnishment and allows filers to retain most assets. It is plausible that bankruptcy protection has a larger effect on labor market outcomes because of either the increased debt forgiveness or the independent effects of wage garnishment and asset seizures protections. Finally, it is possible that bankruptcy protection has a larger impact on labor market outcomes due to the different populations served by our experiment and the bankruptcy system. Dobbie and Song (2015) show that dismissed bankruptcy filers experienced large and permanent decreases in both earnings and employment, while borrowers in the control group of our experiment have similar employment probabilities and earnings following the experiment. These data suggest that borrowers in our sample are not subject to the same types of earnings and expense shocks as the typical Chapter 13 filer, potentially reducing the potential benefits of debt relief.

Monthly Payment Results: The estimated effect of minimum payments on labor market outcomes is also small and relatively imprecisely estimated across all types of borrowers, including those who were unemployed prior to the experiment. In the full sample, the 95 percent confidence interval for employment ranging from -1.38 to 0.26 percentage points, while for earnings the 95 percent confidence interval ranges from -$428 to $570. None of the estimates suggest economically meaningful effects of a longer repayment period on labor market outcomes.

D. 401k Contributions

Table 5 presents estimates of the effect of debt modifications on 401k contributions, a proxy for savings, averaged over the first five years following the experiment. Debt relief and debt restructuring can either crowd out savings by increasing the return to paying one's debts instead, or increase savings by decreasing financial distress.

Interest Rate Results: The estimated effect of interest rates on 401k contributions is small and relatively imprecisely estimated in the full sample, with the 95 percent confidence interval ranging from -$49.20 to $10.09 for the median interest rate reduction. We find similar effects across baseline financial distress, gender, ethnicity, and homeownership. Consistent with our labor market results, however, we find that lower interest rates decrease 401k contributions by $60.14 for borrowers who were unemployed in the year prior to the experiment. In Appendix Table 13, we find similar results for borrowers with zero 401k contributions at baseline. These results suggest that the debt relief provided by lower interest rates may decrease savings for borrowers most on the margin of work, and hence most on the margin of contributing to a 401k.

Monthly Payment Results: The estimated effect of minimum payments on 401k contributions is also small in both the full and subsample results, with the 95 percent confidence interval ranging from -$12.00 to $42.80 for the median minimum payment reduction in the full sample.

E. Robustness Checks

Appendix Table 14 presents estimates using a variety of specifications to assess the robustness of our main specification. Panel A reports intent-to-treat results with the standard baseline controls. Panel B reports estimates of equation (1) with no baseline controls. Panel C reports estimates of equation (1) with both baseline controls and counselor fixed effects. Finally, Panel D reports estimates of equation (1) where the p-values are calculated using a nonparametric permutation test. Specifically, we first create 5,000 "placebo" samples where we randomly re-assign treatment status to individuals within randomization strata. We then calculate the fraction of treatment effects from these 5,000 placebo samples that are larger (in absolute value) than the treatment effects from the true sample.

We find that our main results are robust to these alternative specification choices and methods of calculating standard errors. The results in Panel A show that the intent-to-treat estimates suggest large increases in both starting and completing the repayment program, with a large but not statistically significant decrease in the probability of filing for bankruptcy protection.[16] Panels B and C show that are our main results are essentially unchanged if we drop the baseline controls or include both the baseline controls and counselor fixed effects, and Panel D shows that, if anything, we obtain somewhat smaller p-values from the nonparametric permutation procedure.

IV. Model of Debt Repayment

This section develops a simple economic model to disentangle the mechanisms underlying the reduced form treatment effects. The model clarifies how lower interest rates increase repayment by decreasing borrowers' incentive to strategically default at the beginning of the experiment through increased solvency, and by decreasing borrowers' exposure to non-strategic default risk at the end of the experiment through a shorter repayment period. In contrast, lower minimum payments have an ambiguous impact on repayment rates due to two competing channels: a decrease in non-strategic default and an ambiguous change in strategic default at the beginning of the experiment through increased liquidity, and an increase in exposure to non-strategic default risk at the end of the experiment through a longer repayment period.

An important insight of the model is that it is possible to bound the empirical importance of these strategic, liquidity, and exposure effects using estimates from different points during the experiment. Estimating these bounds is important for two reasons. First, it is critical to know the extent to which the interest rate effect is driven by the mechanical reduction in default through the exposure effect. These exposure effects are unique to policies that affect the repayment period, and estimates that operate through these exposure effects are unlikely to be externally valid in other settings. The second reason it is important to estimate bounds on each channel is to better understand the null result for a longer repayment period. There is extensive evidence that otherwise similar borrowers are liquidity constrained (e.g. Gross and Souleles 2002) and a number of recent

[16] As previously discussed, Appendix Table 4 shows that these intent-to-treat estimates are essentially zero for borrowers with no debt with participating creditors and larger and more precise for borrowers with debts held by creditors with relatively larger interest rate reductions.

policies explicitly target illiquid borrowers, yet our reduced form estimates suggest that either liquidity constraints are less prevalent than previously thought or that a longer repayment period is an ineffective way to address these liquidity constraints. Understanding which scenario holds will help guide future research and policy efforts.

A. Model Setup

In the following we omit individual subscripts from the model parameters to simplify notation. Individuals are risk neutral and maximize the present discounted value of disposable income at a subjective discount rate β. In each period t, individuals receive earnings $y_t = \mu + \epsilon_t$, where ϵ are i.i.d. shocks drawn from a known mean zero distribution $f(\epsilon)$ and μ is assumed to be both known and positive. Debt payments begin at $t = 0$ and are set at a constant level d for the repayment program of length P, so that $d_t = d$ for $t \leq P$ and $d_t = 0$ for $t > P$.

In each time period $0 \leq t \leq P$, individuals observe their income draw y_t and decide whether to make the required debt payment d or default on the remaining debt payments. If an individual defaults on the remaining payments in period t for any reason, she loses her current income draw y_t and receives the constant amount x in period t and all future time periods. To capture the idea of a potentially binding liquidity or credit constraint, we also assume that borrowers automatically default if net income $y_t - d_t$ falls below threshold $\underline{v}$, regardless of value of future cash flows.

Borrowers' default behavior is described by a path of cutoff values ϕ_t, so that a borrower defaults if $y_t < \phi_t$. The default cutoff ϕ_t combines the optimal strategic response of liquid borrowers to low income draws ϕ_t^* and the non-strategic response of illiquid borrowers that may or may not be optimal. The appendix provides additional details on the value functions, first order conditions, and derivations of the above results.

B. Model Predictions

Motivated by the experiment, we consider the comparative statics of lower interest rates and lower minimum payments on debt repayment.

Interest Rate Prediction: The lower interest rate treatment increase debt repayment through two complimentary effects: (1) a decrease in treated borrowers' incentive to strategically default

while both treatment and control borrowers are enrolled in the repayment program, and (2) a decrease in treated borrowers' exposure to non-strategic default risk while control borrowers are still enrolled in the repayment program and treatment borrowers are not.

Proof – See Appendix A.

The interest rate treatment I reduced overall financing charges by shortening the repayment period for treated borrowers relative to control group borrowers, $P^I < P^C$, without changing the monthly debt payments $d^I = d^C = d$. For $0 \leq t \leq P^I$, shortening the length of the repayment period brings borrowers in any given period $P^C - P^I$ periods closer to finishing the repayment program, increasing the expected value of continuing the repayment program. This increase in the expected value of repayment decreases the optimal default cutoff ϕ_t^* for liquid borrowers during this time period. However, disposable income for $0 \leq t \leq P^I$ remains the same, so there is no difference in the probability that a borrower defaults due to the liquidity constraint $\underline{v}$ during this time period. In other words, there will only be an increase in repayment for $0 \leq t \leq P^I$ if the optimal default cutoff ϕ_t^* is the relevant margin for at least some borrowers.

For $P^I < t \leq P^C$, default rates mechanically drop to zero for treated borrowers as they have completed the repayment program. However, control borrowers can still default on their debt if either the liquidity-based cutoffs bind over this time period. Lower interest rates can therefore increase debt repayment even if borrowers never strategically default (i.e., if borrowers only default due to a binding liquidity constraint) if there is sufficient liquidity-based default risk near the end of the repayment program that at least some borrowers continue to exit repayment. In what follows we refer to this as the "exposure" effect.

Monthly Payment Prediction: The lower minimum payment treatment has an ambiguous impact on repayment rates due to three effects: (1) a decrease in treated borrowers non-strategic or liquidity-based default while both treatment and control borrowers are enrolled in the repayment program, (2) an ambiguous change in treated borrowers' incentive to strategically default while both treatment and control borrowers are enrolled in the repayment program, and (3) an increase in treated borrowers' exposure to non-strategic default risk while treated borrowers are still enrolled in the repayment program and control borrowers are not.

Proof – See Appendix A.

The minimum payment treatment R lengthens the repayment P^C to $P^M > P^C$ while keeping the total debt burden the same $\sum_{t=0}^{P^C} d_t = \sum_{t=0}^{P^M} d_t$. Lower minimum payments therefore decrease the probability that the non-strategic cutoff $\underline{v}$ binds for illiquid borrowers for $0 \leq t \leq P^C$, increasing repayment rates over this time period if that liquidity-based default cutoff $\underline{v}$ is the relevant margin for at least some borrowers.

Second, the model shows that lower minimum payments can also affect the incentive to strategically default by changing the option value of repayment for $0 \leq t \leq P^C$. However, the direction of the effect is ambiguous as lower payments both increase future flexibility, increasing the option value of repayment, and transfer a portion of the debt burden into the future, decreasing the option value of repayment. These two offsetting, indirect effects are not unique to a policy of lower minimum payments; other policies targeting liquidity constraints such as payment deferrals or higher credit limits will exhibit these types of offsetting effects. For this reason, we include both these indirect effects on strategic default and the direct effects on non-strategic default discussed above in what we call the "liquidity effect."

Finally, for $P^C < t \leq P^M$, default rates mechanically drop to zero for control borrowers, while treated borrowers can still default on their debt if the liquidity-based cutoffs binds over this time period for these borrowers. This exposure effect allows for the possibility that a longer repayment period will have no effect, or even a negative effect, on repayment rates when liquidity constraints are an important driver of borrower defaults.

C. Empirical Implementation

Overview: An important insight from our model is that we can bound the relative importance of each channel using estimates from different points in the repayment program. The lower bound of the reduced form interest rate effect attributable to strategic behavior is identified by an estimate of repayment at P^I (i.e. the end of the repayment program for treated but not control borrowers). This is because treated and control borrowers have identical monthly payments for $t \leq P^I$, and therefore have identical exposure to the non-strategic liquidity risk over this time period. The estimate at P^I is therefore driven solely by forward-looking strategic behavior. However, this repayment estimate at P^I is a lower bound of the strategic effect because control borrowers can still make forward-looking default decisions for $P^I < t \leq P^C$. As the the reduced form effect

evaluated at P^C (i.e. the end of the repayment program for both treated and control borrowers) is the sum of the strategic and exposure effects, we can then calculate the upper bound of the exposure effect by taking the difference between the reduced form effect at P^C and the reduced form effect at P^I.

Following similar logic, the upper bound of the reduced form minimum payment effect attributable to increased liquidity is identified by an estimate of repayment at P^C (i.e. the end of the repayment program for control but not treated borrowers). For $t \leq P^C$, both control and treated borrowers are enrolled in the repayment program, but treated borrowers have lower monthly payments and subsequently increased liquidity on the margin. The estimate at P^C is therefore driven solely by the direct and indirect effects of increased liquidity. The reduced form estimate at P^C measures an upper bound of the liquidity effect because treatment borrowers can still make forward-looking default decisions in periods $P^C < t \leq P^M$. We can then calculate the lower bound of the exposure effect by taking the difference between the reduced form effect at P^M and the upper bound of the liquidity effect given by the reduced form effect at P^C. The appendix provides additional details on the above results.

Estimation Procedure: We estimate bounds and the associated standard errors using a five step process. First, we calculate how long the repayment plan would have been had the individual been assigned to the treatment group and how long the repayment plan would have been had the individual been assigned to the control group. The treatment plans are shorter for individuals with relatively larger interest rate reductions and longer for individuals with relatively larger minimum payment reductions. For example, individuals with the largest interest rate reductions of 9.9 percentage points and no minimum payment reduction have treatment plans are up to 20 percent shorter than their control plans, while individuals with the lowest interest rate reductions of 4.0 percentage points and the largest minimum payment reductions of 0.5 percentage points have treatment plans that are up to 100 percent longer than their control plans. Second, we create an indicator for staying enrolled in the repayment program up until the minimum of the treatment plan length and the control plan length. This indicator variable measures payment at P^I for individuals with the shorter treatment plans (i.e. relatively larger interest rate reductions) and payment at P^C for individuals with the longer treatment plans (i.e. relatively larger monthly payment reductions).

Third, we estimate equation (1) using this new indicator variable. These reduced form estimates measure the effect of lower interest rates at P^I and the effect of lower minimum payments at P^C. Fourth, we take the difference between the reduced form treatment effects for full repayment estimated in Table 2 and the new reduced form treatment effects estimated at the shorter P^I and P^C. Finally, we calculate the standard error of the difference by bootstrapping the entire procedure described above 500 times. We define the standard error of the treatment effect difference as the standard deviation of the resulting distribution of estimated differences.

D. Estimates

Table 6 presents estimates of strategic, liquidity, and exposure effects for both treatments. Column 1 replicates our estimates from column 4 of Table 2 showing the net effect of all channels on completing repayment. Columns 2-3 report estimates for enrollment in the repayment program at the minimum of the treatment program length and control program length. Column 4 reports the difference between column 1 and columns 2-3.

Interest Rate Results: We find that the positive reduced form interest rate effect is due to decreased strategic defaults, not decreased exposure to risk at the end of the repayment program. Our estimates suggest that at least 85.2 percent of the interest rate effect is due to the decrease in strategic defaults at the beginning of repayment. Decreased exposure to non-strategic risk at the end of repayment can explain a maximum of 14.8 percent of the interest rate effect, with the 95 percent confidence interval including estimates of up to 38.2 percent of the total reduced form effect.

Monthly Payment Results: We find evidence consistent with both liquidity and exposure effects being important drivers of the reduced form minimum payment effect. Longer repayment periods have a small positive effect of about 0.03 percentage points on debt repayment through the increased liquidity, with the 95 percent confidence interval including estimates as large as 0.16 percentage points. In all specifications, any positive effect from increased liquidity is nearly exactly offset by the negative effect of increased exposure to non-strategic default risk.

We view these results as being consistent with the prior literature in suggesting that liquidity constraints are likely an important to the repayment of distressed credit card debt, and that our

reduced form findings of no effect for a longer repayment period is due to the unintended negative effect of exposing borrowers to more default risk. We also note that our modest point estimates on the liquidity effect may be due to the relatively small payment reductions offered to treated borrowers. It is possible that distressed borrowers benefit disproportionately more from larger increases in liquidity, such as the mortgage rate resets examined in the prior literature (e.g. Di Maggio, Kermani, and Ramcharan 2014, Keys et al. 2014, Fuster and Willen 2015).

V. Conclusion

This paper uses a randomized experiment to estimate the ex-post impact of lower interest rates and longer repayment periods for distressed credit card borrowers. We find that lower interest rates increase debt repayment and decrease bankruptcy filing. While employment increases for the most heavily indebted borrowers, earnings and 401k contributions decrease for the borrowers unemployed at baseline. In contrast, we find little impact of a longer repayment period on debt repayment, bankruptcy, employment, or savings. We show that this null reduced form result is due to the unintended negative effect of exposing borrowers to more default risk.

Our estimates suggest that there can be important ex-post benefits of debt relief for both borrowers and lenders. In the U.S., unsecured lenders are not allowed to simultaneously reduce a borrower's original principal and significantly lengthen his or her repayment period. In cases where the original principal can be reduced, borrowers are typically required to pay off the remaining debt in just a few months. These restrictions significantly limit the ability of credit card issuers to forgive distressed debt. The findings from this paper suggest that relaxing these restrictions to allow creditors to extend more generous concessions may increase social welfare.

An important limitation of our analysis is that we are not able to estimate the impact of debt forgiveness and restructuring on ex-ante borrower behavior or borrowing costs. There may also be important ex-post impacts of debt modifications on outcomes such as post-repayment 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: Characteristics	(1)	(2)	(3)
Age	40.626	40.516	−0.271
Male	0.363	0.361	0.008
White	0.636	0.635	0.010
Black	0.171	0.174	−0.008*
Hispanic	0.090	0.088	−0.001
Total Unsecured Debt	18.212	18.368	0.299
Debt with Part. Creditors	9.568	9.615	0.163
Risk Score	-0.000	-0.003	−0.003
Homeowner	0.412	0.410	−0.003
Renter	0.440	0.442	0.003
Dependents	2.159	2.156	−0.006
Monthly Income	2.453	2.448	0.010
Monthly Expenses	2.168	2.158	0.003
Total Assets	71.635	71.545	−0.373
Total Liabilities	68.488	68.101	−0.125
Panel B: Baseline Outcomes			
Bankruptcy	0.004	0.003	−0.001
Employment	0.848	0.850	0.004
Earnings	23.447	23.518	−0.108
Nonzero 401k Cont.	0.227	0.224	−0.006
401k Contributions	0.372	0.373	−0.008
Panel C: Potential Treatment Intensity			
Interest Rate Reduction	2.641	2.650	0.034
Minimum Payment Reduction	9.513	9.371	0.081
Net Change in Program Length	0.708	0.764	0.022
Panel D: Data Quality			
Matched to SSA data	0.953	0.954	0.003
p-value from joint F-test	–	–	0.723
Observations	40,496	39,243	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 - 2013 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
Debt Modifications and Repayment

	Start Payment			Complete Payment		
	Full Sample	Low Debt	High Debt	Full Sample	Low Debt	High Debt
	(1)	(2)	(3)	(4)	(5)	(6)
Interest Rate Reduction	0.0050**	0.0021	0.0076***	0.0044**	0.0020	0.0068***
	(0.0023)	(0.0030)	(0.0028)	(0.0017)	(0.0023)	(0.0023)
Min. Payment Reduction	0.0005	0.0009	0.0001	0.0001	0.0007	−0.0006
	(0.0006)	(0.0007)	(0.0007)	(0.0005)	(0.0006)	(0.0006)
Observations	79,739	39,869	39,870	79,739	39,869	39,870
Mean in Control Group	0.3185	0.3170	0.3201	0.1366	0.1247	0.1484

Notes: This table reports reduced form estimates of the impact of debt modifications on repayment. Information on repayment comes from administrative records at the credit counseling organization. Columns 1 and 4 report results for the full sample of borrowers. Columns 2-3 and 5-6 report results for borrowers with above and below median debt-to-income ratios. We report coefficients on the interaction of treatment eligibility and potential interest rate reduction (in percentage points), and the interaction of treatment eligibility and potential monthly payment reduction (in percentage points x 100). All specifications control for potential interest rate reduction, potential monthly payment reduction, the baseline controls in Table 1, and 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 details on the baseline controls and sample.

Table 3
Debt Modifications and Bankruptcy Filing

	Bankruptcy in Years 1-5		
	Full Sample	Low Debt	High Debt
	(1)	(2)	(3)
Interest Rate Reduction	−0.0027**	−0.0016	−0.0037**
	(0.0014)	(0.0015)	(0.0019)
Min. Payment Reduction	0.0005	0.0002	0.0007
	(0.0003)	(0.0004)	(0.0004)
Observations	79,739	39,869	39,870
Mean in Control Group	0.1036	0.0658	0.1416

Notes: This table reports reduced form estimates of the impact of debt modifications on bankruptcy. Information on bankruptcy comes from court records. Column 1 reports results for the full sample of borrowers. Columns 2-3 report results for borrowers with above and below median debt-to-income ratios. We report coefficients on the interaction of treatment eligibility and potential interest rate reduction (in percentage points), and the interaction of treatment eligibility and potential monthly payment reduction (in percentage points x 100). All specifications control for potential interest rate reduction, potential monthly payment reduction, the baseline controls in Table 1, and 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 details on the baseline controls and sample.

Table 4
Debt Modifications and Labor Market Outcomes

	Employment			Earnings		
	Full Sample	Low Debt	High Debt	Full Sample	Low Debt	High Debt
	(1)	(2)	(3)	(4)	(5)	(6)
Interest Rate Reduction	0.0020	−0.0010	0.0046**	−0.0410	−0.0972	0.0235
	(0.0016)	(0.0013)	(0.0021)	(0.0689)	(0.0770)	(0.0982)
Min. Payment Reduction	−0.0004	−0.0006*	−0.0001	0.0051	0.0009	0.0098
	(0.0003)	(0.0003)	(0.0003)	(0.0182)	(0.0243)	(0.0213)
Observations	76,008	37,867	38,141	76,008	37,867	38,141
Mean in Control Group	0.8202	0.8586	0.7819	26.8915	27.6506	26.1331

Notes: This table reports reduced form estimates of the impact of debt modifications on employment and earnings. Information on outcomes comes from records at the Social Security Administration. Columns 1 and 4 report results for the full sample of borrowers. Columns 2-3 and 5-6 report results for borrowers with above and below median debt-to-income ratios. We report coefficients on the interaction of treatment eligibility and potential interest rate reduction (in percentage points), and the interaction of treatment eligibility and potential monthly payment reduction (in percentage points x 100). All specifications control for potential interest rate reduction, potential monthly payment reduction, the baseline controls in Table 1, and 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 details on the baseline controls and sample.

Table 5
Debt Modifications and 401k Contributions

	Nonzero 401k Contributions			401k Contributions		
	Full Sample	Low Debt	High Debt	Full Sample	Low Debt	High Debt
	(1)	(2)	(3)	(4)	(5)	(6)
Interest Rate Reduction	0.0003	0.0006	0.0001	−0.0053	−0.0067	−0.0039
	(0.0017)	(0.0022)	(0.0023)	(0.0041)	(0.0056)	(0.0051)
Min. Payment Reduction	−0.0001	−0.0001	−0.0001	0.0012	0.0017	0.0007
	(0.0004)	(0.0005)	(0.0005)	(0.0010)	(0.0013)	(0.0011)
Observations	76,008	37,867	38,141	76,008	37,867	38,141
Mean in Control Group	0.2723	0.2784	0.2662	0.4643	0.4413	0.4872

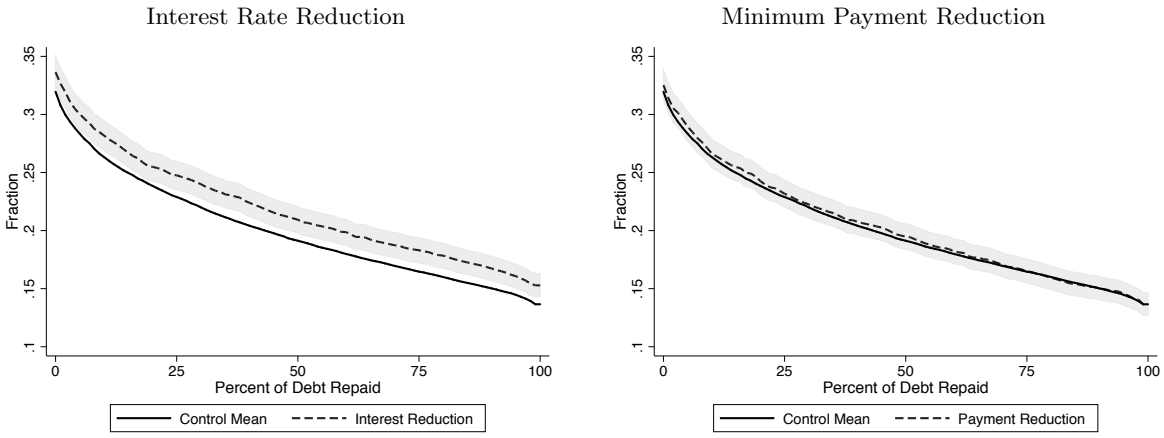
Notes: This table reports reduced form estimates of the impact of debt modifications on 401k contributions. Information on all outcomes comes from records at the Social Security Administration. Columns 1 and 4 report results for the full sample of borrowers. Columns 2-3 and 5-6 report results for borrowers with above and below median debt-to-income ratios. We report coefficients on the interaction of treatment eligibility and potential interest rate reduction (in percentage points), and the interaction of treatment eligibility and potential monthly payment reduction (in percentage points x 100). All specifications control for potential interest rate reduction, potential monthly payment reduction, the baseline controls in Table 1, and 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 details on the baseline controls and sample.

Table 6
Bounds on Strategic, Liquidity, and Exposure Effects

	Total Effect	Strategic Effect	Liquidity Effect	Exposure Effect
	(1)	(2)	(3)	(4)
Interest Rate Reduction	0.00444***	0.00378**		0.00066
	(0.00174)	(0.00184)		(0.00053)
Min. Payment Reduction	0.00001		0.00018	−0.00017
	(0.00048)		(0.00048)	(0.00011)

Notes: This table reports the implied lower bounds on the strategic and liquidity effects and the upper bounds on the exposure effects of each treatment. Column 1 reports results for fully completing debt repayment. Columns 2-3 reports results for being enrolled in the repayment program at the minimum of the treatment program length or the control program length. Row 4 reports the difference between column 1 and columns 2-3. All specifications control for potential interest rate reduction, potential monthly payment reduction, the baseline controls in Table 1, and strata fixed effects, and cluster standard errors at the counselor level. Standard errors for column 4 are calculated using the bootstrap procedure described in the text. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See the text for additional details on the estimation procedure.

Figure 1
Debt Modifications and Repayment Rates



Notes: This figure reports the control group mean and implied treatment group means for debt repayment. We calculate each treatment group mean using the control mean and the reduced form estimates described in Table 2. The shaded regions indicate the 95 percent confidence intervals. All specifications control for the potential minimum payment and interest rate changes if treated and cluster standard errors at the counselor level. See the Table 2 notes for additional details on the sample and specification.

Appendix Table 1
Randomized Treatments and Repayment Program Attributes

Treatment	Minimum Payment	Financing Costs	Total Months
Panel A: Baseline Case			
–	$433.45	$3,482	50.05
Panel B: Interest Rate Reduction			
Δ1.96%	$433.45	$2,523	47.84
Δ3.69%	$433.45	$1,770	46.10
Δ5.63%	$433.45	$1,002	44.33
Panel C: Minimum Payment Reduction			
Δ0.07%	$420.14	$3,620	51.97
Δ0.14%	$406.77	$3,771	54.04
Δ0.25%	$387.92	$4,007	57.28

Notes: This table describes the effect of treatment eligibility on repayment program attributes. Monthly payment is the minimum required payment of the program. Financing cost is the total interest charges during the program. 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 Rates		Minimum Payments		
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 11 creditors participating in the randomized trial. Minimum 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
	(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)	
Total Unsecured Debt	18.2120	0.1233	−0.0107	0.1775
	(16.9388)	(0.0761)	(0.0195)	
Debt with Part. Creditors	9.5679	0.0813	−0.0110	0.3257
	(12.6572)	(0.0566)	(0.0154)	
Risk Score	-0.0000	0.0010	−0.0007	0.8118
	(1.0000)	(0.0051)	(0.0012)	
Homeowner	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)	
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)	
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)	
Nonzero 401k Cont.	0.2272	−0.0004	−0.0001	0.8762
	(0.4190)	(0.0023)	(0.0006)	
401k Contributions	0.3717	−0.0019	−0.0002	0.7577
	(0.9688)	(0.0056)	(0.0014)	
Matched to SSA data	0.9526	0.0005	0.0001	0.5749
	(0.2124)	(0.0011)	(0.0003)	
Observations	40,496	79,739		

Notes: This table reports additional tests of random assignment. 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 all interactions are jointly equal to zero. See Table 1 notes for additional details on the sample and variable construction.

Appendix Table 4
Non-Parametric Results

	Start Payment	Complete Payment	Bankrupt	Employed	Earnings	Nonzero 401k	401k Cont.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
No Payment Red. x No Interest Red.	0.0041	−0.0033	0.0019	0.0040	0.2403	0.0012	0.0047
	(0.0089)	(0.0066)	(0.0054)	(0.0071)	(0.4954)	(0.0082)	(0.0204)
Low Payment Red. x Low Interest Red.	0.0114	0.0110	0.0009	0.0077	0.3674	0.0099	0.0229
	(0.0150)	(0.0109)	(0.0094)	(0.0101)	(0.6946)	(0.0117)	(0.0300)
High Payment Red. x Low Interest Red.	0.0135	0.0024	0.0048	−0.0159	−1.1358	−0.0205	−0.0241
	(0.0212)	(0.0181)	(0.0140)	(0.0178)	(1.3029)	(0.0192)	(0.0510)
Low Payment Red. x High Interest Red.	0.0279	0.0301	0.0111	−0.0005	−0.3478	−0.0040	−0.0156
	(0.0213)	(0.0190)	(0.0149)	(0.0151)	(1.1798)	(0.0187)	(0.0497)
High Payment Red. x High Interest Red.	0.0450***	0.0255**	−0.0110	−0.0079	0.2291	−0.0029	−0.0158
	(0.0132)	(0.0109)	(0.0080)	(0.0092)	(0.7177)	(0.0115)	(0.0303)
Observations	79,739	79,739	79,739	76,008	76,008	76,008	76,008

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 range. All specifications control for an exhaustive set of potential treatment intensity fixed effects, the baseline controls listed in Table 1, and 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.

Appendix Table 5
Over-Identification Tests

	Crd. 1	Crd. 2	Crd. 3	Crd. 4	Crd. 5	Crd. 6	Crd. 7	Crd. 8	Crd. 9	Crd. 10	Crd. 11	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Start Payment	−0.0082	−0.0011	0.0009	−0.0532	0.0005	0.0049	0.0500***	−0.0086	−0.0063	−0.0036	−0.0152	0.416
	(0.0220)	(0.0145)	(0.0153)	(0.2299)	(0.0238)	(0.0142)	(0.0162)	(0.0124)	(0.0114)	(0.0134)	(0.0258)	
Complete Payment	0.0128	−0.0067	0.0014	−0.2493	−0.0062	0.0074	0.0339***	−0.0097	0.0014	0.0080	0.0055	0.369
	(0.0184)	(0.0120)	(0.0126)	(0.2190)	(0.0173)	(0.0113)	(0.0125)	(0.0104)	(0.0095)	(0.0111)	(0.0238)	
Bankruptcy	0.0011	0.0040	−0.0069	0.0422	0.0159	−0.0038	0.0098	0.0030	0.0025	0.0055	0.0040	0.986
	(0.0149)	(0.0114)	(0.0109)	(0.1059)	(0.0163)	(0.0093)	(0.0123)	(0.0084)	(0.0083)	(0.0091)	(0.0165)	
Employment	0.0040	0.0041	0.0039	−0.0856	−0.0059	0.0059	0.0063	−0.0050	0.0047	0.0067	−0.0018	0.940
	(0.0112)	(0.0069)	(0.0081)	(0.1483)	(0.0142)	(0.0067)	(0.0100)	(0.0062)	(0.0061)	(0.0071)	(0.0123)	
Earnings	0.0913	0.6825	−0.0077	−2.0041	−0.9102	0.1850	0.9332	−0.4827	0.2154	0.4029	−0.0410	0.718
	(0.6054)	(0.4672)	(0.5065)	(9.5178)	(0.9091)	(0.3983)	(0.6737)	(0.3562)	(0.3667)	(0.4866)	(0.9303)	
Nonzero 401k Cont.	0.0155	0.0141	0.0009	−0.1251	0.0223	0.0028	0.0083	−0.0068	−0.0044	0.0002	0.0037	0.840
	(0.0163)	(0.0114)	(0.0114)	(0.1771)	(0.0188)	(0.0099)	(0.0145)	(0.0082)	(0.0090)	(0.0115)	(0.0213)	
401k Contributions	0.0551	0.0364	0.0257	0.0978	0.0393	0.0117	0.0321	−0.0244	0.0033	0.0022	0.0168	0.834
	(0.0413)	(0.0293)	(0.0256)	(0.3886)	(0.0486)	(0.0242)	(0.0366)	(0.0241)	(0.0196)	(0.0274)	(0.0561)	

Notes: This table reports over-identification tests. Columns 1-11 report the coefficient on treatment eligibility interacted with an indicator for having debt with each credit card bank. Column 12 reports the p-value from an F-test that all interactions are jointly equal to zero. All specifications control for treatment eligibility interacted with the potential interest rate reduction, treatment eligibility interacted with the potential monthly payment reduction, potential interest rate reduction, potential monthly payment reduction, the baseline controls listed in Table 1, and strata fixed effects. 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 Predicted Treatment Intensity

	Start Payment	Complete Payment	Bankrupt	Employed	Earnings	Nonzero 401k	401k Cont.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) Interest x High Pred. Reduction	0.0024	0.0032	−0.0020	0.0019	−0.0742	−0.0004	−0.0076
	(0.0027)	(0.0020)	(0.0016)	(0.0018)	(0.1433)	(0.0025)	(0.0064)
(2) Interest x Low Pred. Reduction	0.0135***	0.0091**	−0.0017	0.0030	0.3672*	0.0032	0.0010
	(0.0044)	(0.0036)	(0.0022)	(0.0031)	(0.2149)	(0.0035)	(0.0085)
p-value for (1)-(2)	[0.0210]	[0.1334]	[0.8909]	[0.7498]	[0.0651]	[0.3524]	[0.3720]
(3) Payment x High Pred. Reduction	0.0010	0.0003	0.0003	−0.0006	0.0118	−0.0001	0.0012
	(0.0006)	(0.0005)	(0.0004)	(0.0005)	(0.0367)	(0.0005)	(0.0015)
(4) Payment x Low Pred. Reduction	−0.0006	−0.0009	0.0027***	−0.0029***	−0.0426	−0.0007	0.0002
	(0.0012)	(0.0009)	(0.0009)	(0.0010)	(0.0712)	(0.0011)	(0.0028)
p-value for (3)-(4)	[0.2015]	[0.1943]	[0.0070]	[0.0172]	[0.4602]	[0.5402]	[0.7273]
Observations	79,739	79,739	79,739	76,008	76,008	76,008	76,008
Mean if High Pred. Interest Red.	0.3903	0.1919	0.1054	0.8269	29.6291	0.2946	0.5495
Mean if Low Pred. Interest Red.	0.2463	0.0808	0.1018	0.8136	24.1407	0.2500	0.3786
Mean if High Pred. Min. Payment Red.	0.4021	0.1971	0.1068	0.8087	28.4987	0.2832	0.5285
Mean if Low Pred. Min. Payment Red.	0.2356	0.0764	0.1004	0.8318	25.2864	0.2614	0.4002

Notes: This table reports results by predicted potential treatment intensity. The predicted potential interest rate and monthly payment change is calculated using the baseline variables listed in Table 1. We report coefficients on the interaction of treatment eligibility x true potential treatment intensity x an indicator for having above or below median predicted treatment intensity. All specifications control for an indicator for predicted potential treatment intensity, potential treatment intensity, the baseline variables listed in Table 1, and strata fixed effects. 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
Correlates of Potential Treatment Intensity

	Control Mean	Δ Interest	Δ Payment	p-value on joint test
	(1)	(2)	(3)	(4)
Age	40.6256	0.0281	0.0782***	0.0000
	(13.4135)	(0.0417)	(0.0108)	
Male	0.3631	0.0020	0.0009**	0.0000
	(0.4809)	(0.0015)	(0.0004)	
White	0.6363	0.0070***	0.0023***	0.0000
	(0.4811)	(0.0016)	(0.0004)	
Black	0.1712	−0.0079***	−0.0015***	0.0000
	(0.3767)	(0.0012)	(0.0003)	
Hispanic	0.0904	−0.0006	−0.0008***	0.0000
	(0.2868)	(0.0011)	(0.0003)	
Total Unsecured Debt	18.2120	0.7264***	0.0850***	0.0000
	(16.9388)	(0.0561)	(0.0133)	
Debt with Participating Creditors	9.5679	1.4554***	0.1580***	0.0000
	(12.6572)	(0.0393)	(0.0103)	
Risk Score	-0.0000	−0.0242***	−0.0090***	0.0000
	(1.0000)	(0.0030)	(0.0006)	
Homeowner	0.4123	0.0122***	0.0009***	0.0000
	(0.4923)	(0.0015)	(0.0003)	
Renter	0.4395	−0.0092***	−0.0006*	0.0000
	(0.4963)	(0.0015)	(0.0003)	
Dependents	2.1590	−0.0058	−0.0028***	0.0000
	(1.3852)	(0.0043)	(0.0010)	
Monthly Income	2.4534	0.0415***	0.0007	0.0000
	(1.4452)	(0.0045)	(0.0011)	
Monthly Expenses	2.1682	0.0272***	0.0003	0.0000
	(1.2944)	(0.0041)	(0.0010)	
Total Assets	71.6355	2.0982***	0.2910***	0.0000
	(109.8651)	(0.3519)	(0.0842)	
Total Liabilities	68.4875	2.1742***	0.1360**	0.0000
	(86.2506)	(0.2739)	(0.0656)	
Bankruptcy	0.0038	−0.0003*	−0.0000	0.0072
	(0.0614)	(0.0002)	(0.0000)	
Employment	0.8478	0.0037***	−0.0012***	0.0000
	(0.3593)	(0.0010)	(0.0002)	
Earnings	23.4466	0.5484***	−0.0204	0.0000
	(21.1752)	(0.0642)	(0.0147)	
Nonzero 401k Cont.	0.2272	0.0050***	−0.0002	0.0000
	(0.4190)	(0.0012)	(0.0002)	
401k Contributions	0.3717	0.0150***	0.0009	0.0000
	(0.9688)	(0.0029)	(0.0007)	
Matched to SSA data	0.9526	−0.0002	0.0000	0.9491
	(0.2124)	(0.0007)	(0.0002)	
Observations	40,496	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 8
Results by Gender

	Start Payment	Complete Payment	Bankrupt	Employed	Earnings	Nonzero 401k	401k Cont.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) Interest x Male	0.0016	0.0002	−0.0013	0.0031	0.0660	0.0002	−0.0072
	(0.0034)	(0.0025)	(0.0022)	(0.0025)	(0.1890)	(0.0028)	(0.0077)
(2) Interest x Female	0.0081***	0.0074***	−0.0033**	0.0017	−0.0180	0.0006	−0.0050
	(0.0030)	(0.0024)	(0.0017)	(0.0020)	(0.1305)	(0.0026)	(0.0066)
p-value for (1)-(2)	[0.0935]	[0.0271]	[0.3981]	[0.6105]	[0.6510]	[0.9073]	[0.8068]
(3) Payment x Male	0.0013	0.0004	0.0004	−0.0006	−0.0133	−0.0006	0.0003
	(0.0009)	(0.0006)	(0.0005)	(0.0007)	(0.0456)	(0.0007)	(0.0020)
(4) Payment x Female	−0.0002	−0.0001	0.0005	−0.0008	−0.0014	−0.0000	0.0012
	(0.0008)	(0.0006)	(0.0004)	(0.0005)	(0.0351)	(0.0006)	(0.0016)
p-value for (3)-(4)	[0.1486]	[0.5660]	[0.7995]	[0.8779]	[0.7997]	[0.4941]	[0.6733]
Observations	79,739	79,739	79,739	76,008	76,008	76,008	
Mean if Male	0.3121	0.1255	0.1252	0.8430	32.0416	0.2825	0.5633
Mean if Female	0.3203	0.1391	0.0993	0.8073	23.9590	0.2666	0.4079

Notes: This table reports results by gender. We report coefficients on the interaction of gender x treatment eligibility x potential treatment intensity. All specifications control for an indicator for potential treatment intensity, the baseline variables listed in Table 1, and strata fixed effects. 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 Ethnicity

	Start Payment	Complete Payment	Bankrupt	Employed	Earnings	Nonzero 401k	401k Cont.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) Interest x White	0.0039	0.0044**	−0.0015	0.0026	0.0149	0.0003	−0.0071
	(0.0030)	(0.0020)	(0.0017)	(0.0018)	(0.1416)	(0.0023)	(0.0059)
(2) Interest x Non-White	0.0093***	0.0051*	−0.0049**	0.0016	0.0523	0.0010	−0.0023
	(0.0035)	(0.0030)	(0.0020)	(0.0029)	(0.1830)	(0.0031)	(0.0081)
p-value for (1)-(2)	[0.1777]	[0.8313]	[0.1328]	[0.7408]	[0.8381]	[0.8031]	[0.5353]
(3) Payment x White	0.0002	−0.0001	0.0002	−0.0007	−0.0047	−0.0004	0.0003
	(0.0007)	(0.0005)	(0.0004)	(0.0005)	(0.0350)	(0.0005)	(0.0015)
(4) Payment x Non-White	0.0008	0.0005	0.0009*	−0.0007	−0.0132	0.0002	0.0021
	(0.0009)	(0.0007)	(0.0005)	(0.0008)	(0.0530)	(0.0007)	(0.0021)
p-value for (3)-(4)	[0.5516]	[0.4574]	[0.2264]	[0.9766]	[0.8716]	[0.3741]	[0.3938]
Observations	79,739	79,739	79,739	76,008	76,008	76,008	
Mean if White	0.3330	0.1474	0.1155	0.8187	27.1763	0.2691	0.4673
Mean if Non-White	0.2858	0.1075	0.0951	0.8234	26.3202	0.2788	0.4583

Notes: This table reports results by ethnicity. We report coefficients on the interaction of ethnicity x treatment eligibility x potential treatment intensity. All specifications control for an indicator for potential treatment intensity, the baseline variables listed in Table 1, and strata fixed effects. 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 10
Results by Homeownership

	Start Payment	Complete Payment	Bankrupt	Employed	Earnings	Nonzero 401k	401k Cont.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) Interest x Homeowner	0.0055*	0.0038*	−0.0030	0.0039	−0.0082	0.0007	−0.0034
	(0.0031)	(0.0023)	(0.0019)	(0.0025)	(0.1883)	(0.0029)	(0.0078)
(2) Interest x Non-Owner	0.0054*	0.0056**	−0.0019	0.0008	0.0670	0.0003	−0.0066
	(0.0030)	(0.0023)	(0.0017)	(0.0019)	(0.1455)	(0.0025)	(0.0062)
p-value for (1)-(2)	[0.9658]	[0.5265]	[0.6159]	[0.2710]	[0.7115]	[0.8733]	[0.6965]
(3) Payment x Homeowner	−0.0003	−0.0005	0.0004	−0.0011*	−0.0171	−0.0005	0.0002
	(0.0007)	(0.0006)	(0.0004)	(0.0006)	(0.0440)	(0.0006)	(0.0017)
(4) Payment x Non-Owner	0.0009	0.0002	0.0005	−0.0004	−0.0050	−0.0001	0.0011
	(0.0007)	(0.0006)	(0.0004)	(0.0005)	(0.0373)	(0.0006)	(0.0016)
p-value for (3)-(4)	[0.1695]	[0.3547]	[0.8030]	[0.2816]	[0.7926]	[0.5713]	[0.6513]
Observations	79,739	79,739	79,739	76,008	76,008	76,008	76,008
Mean if Homeowner	0.3214	0.1401	0.1140	0.7987	29.0246	0.2974	0.5673
Mean if Non-Owner	0.3165	0.1340	0.0963	0.8353	25.3983	0.2548	0.3922

Notes: This table reports results by baseline homeownership. We report coefficients on the interaction of homeownership x treatment eligibility x potential treatment intensity. All specifications control for an indicator for potential treatment intensity, the baseline variables listed in Table 1, and strata fixed effects. 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 11
Bankruptcy Results by Year

	Year 1	Year 2	Year 3	Year 4	Year 5
	(1)	(2)	(3)	(4)	(5)
Interest Rate Reduction	−0.0002	−0.0008	−0.0012**	−0.0001	−0.0004
	(0.0012)	(0.0006)	(0.0005)	(0.0005)	(0.0004)
Min. Payment Reduction	−0.0001	0.0001	0.0001	0.0001	0.0002**
	(0.0003)	(0.0002)	(0.0002)	(0.0001)	(0.0001)
Observations	79,739	79,739	79,739	79,739	79,739
Mean in Control Group	0.0578	0.0173	0.0133	0.0093	0.0059

Notes: This table reports reduced form estimates of the impact of debt modifications on bankruptcy. Information on bankruptcy comes from court records. We report coefficients on the interaction of treatment eligibility and potential interest rate reduction (in percentage points), and the interaction of treatment eligibility and potential monthly payment reduction (in percentage points x 100). All specifications control for potential interest rate reduction, potential monthly payment reduction, the baseline controls in Table 1, and 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 details on the baseline controls and sample.

Appendix Table 12
Results by Baseline Employment

	Start Payment	Complete Payment	Bankrupt	Employed	Earnings	Nonzero 401k	401k Cont.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) Interest x Employed	0.0053**	0.0055***	−0.0029**	0.0005	0.0262	−0.0001	−0.0059
	(0.0026)	(0.0018)	(0.0015)	(0.0012)	(0.1240)	(0.0023)	(0.0060)
(2) Interest x Unemployed	0.0055	0.0016	0.0003	−0.0008	−0.5631***	−0.0024	−0.0163**
	(0.0044)	(0.0035)	(0.0024)	(0.0037)	(0.1663)	(0.0030)	(0.0080)
p-value for (1)-(2)	[0.9629]	[0.2644]	[0.1897]	[0.7511]	[0.0006]	[0.4254]	[0.2245]
(3) Payment x Employed	0.0006	0.0000	0.0003	−0.0003	0.0076	−0.0001	0.0015
	(0.0006)	(0.0005)	(0.0004)	(0.0003)	(0.0322)	(0.0005)	(0.0014)
(4) Payment x Unemployed	−0.0005	−0.0004	0.0008	−0.0006	0.0255	−0.0001	0.0002
	(0.0009)	(0.0008)	(0.0005)	(0.0008)	(0.0359)	(0.0006)	(0.0016)
p-value for (3)-(4)	[0.2280]	[0.6026]	[0.4255]	[0.7524]	[0.6385]	[0.9683]	[0.4287]
Observations	79,739	79,739	79,739	76,008	76,008	76,008	76,008
Mean if Employed	0.3223	0.1373	0.1121	0.9244	31.0802	0.3170	0.5412
Mean if Unemployed	0.3029	0.1332	0.0680	0.2404	3.5658	0.0238	0.0358

Notes: This table reports results by baseline employment. We report coefficients on the interaction of employment x treatment eligibility x potential treatment intensity. All specifications control for an indicator for potential treatment intensity, the baseline variables listed in Table 1, and strata fixed effects. 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 13
Results by Baseline 401k Contribution

	Start Payment	Complete Payment	Bankrupt	Employed	Earnings	Nonzero 401k	401k Cont.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) Interest x 401k > 0	0.0021	0.0052*	−0.0037	−0.0007	0.0520	−0.0009	−0.0002
	(0.0039)	(0.0029)	(0.0026)	(0.0019)	(0.1747)	(0.0030)	(0.0106)
(2) Interest x 401k = 0	0.0067**	0.0048**	−0.0019	0.0033*	0.0224	0.0010	−0.0075*
	(0.0027)	(0.0020)	(0.0014)	(0.0020)	(0.1362)	(0.0016)	(0.0045)
p-value for (1)-(2)	[0.2764]	[0.9148]	[0.4868]	[0.0962]	[0.8729]	[0.5375]	[0.4739]
(3) Payment x 401k > 0	0.0014	0.0004	0.0002	0.0003	0.0537	0.0007	0.0051**
	(0.0009)	(0.0007)	(0.0006)	(0.0005)	(0.0488)	(0.0007)	(0.0026)
(4) Payment x 401k = 0	0.0000	−0.0003	0.0005	−0.0010*	−0.0190	−0.0003	0.0000
	(0.0006)	(0.0005)	(0.0004)	(0.0005)	(0.0336)	(0.0004)	(0.0010)
p-value for (3)-(4)	[0.1513]	[0.3512]	[0.5580]	[0.0540]	[0.1533]	[0.1350]	[0.0412]
Observations	79,739	79,739	79,739	76,008	76,008	76,008	76,008
Mean if 401k > 0	0.3492	0.1609	0.1209	0.9610	42.0110	0.6818	1.3318
Mean if 401k = 0	0.3096	0.1294	0.0985	0.7762	22.1573	0.1441	0.1927

Notes: This table reports results by baseline 401k contribution status. We report coefficients on the interaction of 401 contributions x treatment eligibility x potential treatment intensity. All specifications control for an indicator for potential treatment intensity, the baseline variables listed in Table 1, and strata fixed effects. 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 14
Robustness of Main Results

	Start Payment	Complete Payment	Bankrupt	Employed	Earnings	Nonzero 401k	401k Cont.
Panel A: ITT Results	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment Indicator	0.0186***	0.0133***	−0.0031	−0.0019	0.0584	0.0012	0.0009
	(0.0057)	(0.0044)	(0.0035)	(0.0025)	(0.1738)	(0.0041)	(0.0101)
Panel B: No Baseline Controls							
Interest Rate Reduction	0.0055**	0.0049***	−0.0024*	0.0022	0.0290	0.0004	−0.0056
	(0.0025)	(0.0018)	(0.0014)	(0.0017)	(0.1309)	(0.0022)	(0.0056)
Min. Payment Reduction	0.0003	−0.0001	0.0005	−0.0007	−0.0075	−0.0002	0.0008
	(0.0006)	(0.0005)	(0.0003)	(0.0005)	(0.0334)	(0.0005)	(0.0014)
Panel C: Controls for Counselor Effects							
Interest Rate Reduction	0.0048**	0.0043**	−0.0029**	0.0003	−0.0357	0.0002	−0.0054
	(0.0023)	(0.0017)	(0.0014)	(0.0010)	(0.0703)	(0.0018)	(0.0042)
Min. Payment Reduction	0.0006	0.0001	0.0005	−0.0003	0.0038	−0.0000	0.0011
	(0.0006)	(0.0005)	(0.0003)	(0.0003)	(0.0184)	(0.0004)	(0.0010)
Panel D: p-values from Permutation Test							
Interest Rate Reduction	0.0050***	0.0044***	−0.0027**	0.0020	−0.0410	0.0003	−0.0053
	[0.0049]	[0.0009]	[0.0259]	[0.2243]	[0.5926]	[0.8397]	[0.1774]
Min. Payment Reduction	0.0005	0.0001	0.0005*	−0.0004	0.0051	−0.0002	0.0012
	[0.2907]	[0.9880]	[0.0739]	[0.3062]	[0.7427]	[0.6760]	[0.2433]
Observations	79,739	79,739	79,739	76,008	76,008	76,008	76,008
Mean in Control Group	0.3185	0.1366	0.1036	0.8202	26.8915	0.2723	0.4643

Notes: This table reports robustness checks of our main results. Panel A reports intent-to-treat effects. Panel B reports reduced form results with no baseline controls. Panel C reports reduced form results with the addition of counselor fixed effects. Panel D reports reduced form results where the p-values are calculated using a nonparametric permutation test with 5,000 draws. All specifications control for potential interest rate reduction, potential monthly payment reduction, and 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 details on the baseline controls and sample and the text for additional details on the nonparametric permutation test.

Appendix Table 15
Control vs. Treatment Compliers

	Control Compliers	Treatment Compliers	Difference
Panel A: Baseline Characteristics	(1)	(2)	(3)
Age	41.399	41.259	−0.301
Male	0.357	0.355	0.021
White	0.665	0.661	0.017
Black	0.136	0.140	−0.009
Hispanic	0.091	0.088	−0.011
Total Unsecured Debt	18.702	19.036	0.882
Debt with Part. Creditors	10.965	11.331	0.921*
Risk Score	-0.151	-0.159	−0.020
Homeowner	0.416	0.423	0.010
Renter	0.426	0.426	0.002
Dependents	2.092	2.075	−0.018
Monthly Income	2.647	2.652	0.026
Monthly Expenses	2.212	2.219	0.017
Total Assets	72.498	73.232	−0.375
Total Liabilities	61.693	62.719	0.972
Panel B: Baseline Outcomes			
Bankruptcy	0.003	0.002	−0.000
Employment	0.861	0.867	−0.002
Earnings	25.797	25.832	−0.388
Nonzero 401k Cont.	0.249	0.248	−0.013
401k Contributions	0.436	0.435	−0.034
Panel C: Potential Treatment Intensity			
Interest Rate Reduction	3.438	3.516	0.118
Minimum Payment Reduction	11.763	11.875	0.600
Net Change in Program Length	1.076	1.156	−0.016
Panel D: Data Quality			
Matched to SSA data	0.949	0.948	0.007
p-value from joint F-test	–	–	0.938
Observations	12,900	13,152	26,052

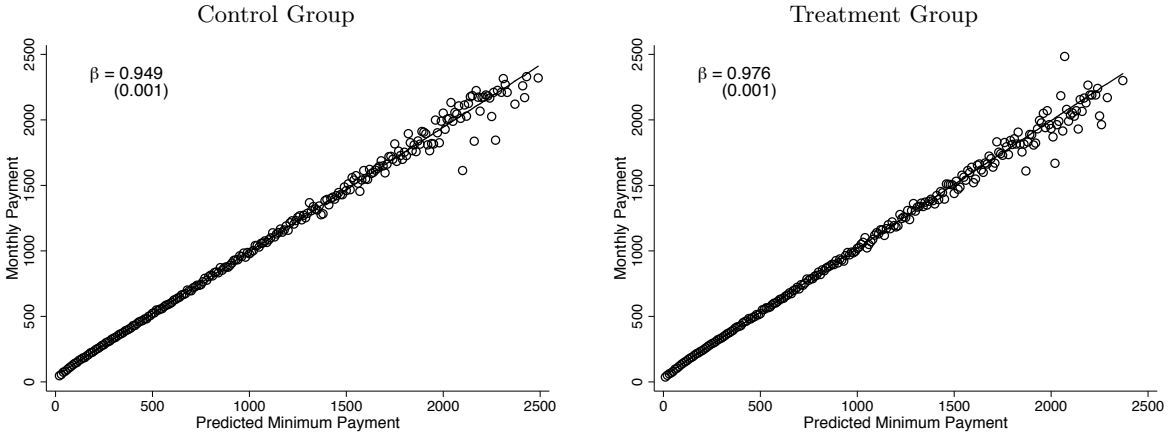
Notes: This table reports descriptive statistics for control and treatment compliers based on program enrollment. Column 3 reports the difference between the treatment and control groups, controlling for strata fixed effects and clustering standard errors at the counselor level. *** = significant at 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. See Table 1 notes for additional details on the sample and variable construction.

Appendix Table 16
Additional Tests of Control vs. Treatment Compliers

	Control Compliers	Treated x Δ Interest	Treated x Δ Payment	p-value on joint test
	(1)	(2)	(3)	(4)
Age	41.3994	−0.1551	0.0650	0.4702
	(13.5974)	(0.2128)	(0.0562)	
Male	0.3571	0.0004	0.0011	0.5610
	(0.4792)	(0.0073)	(0.0021)	
White	0.6647	0.0026	0.0002	0.7509
	(0.4721)	(0.0068)	(0.0017)	
Black	0.1362	0.0033	−0.0009	0.7165
	(0.3430)	(0.0046)	(0.0012)	
Hispanic	0.0911	−0.0045	0.0005	0.5349
	(0.2878)	(0.0047)	(0.0012)	
Total Unsecured Debt	18.7022	0.3217	−0.0349	0.2800
	(16.1538)	(0.2278)	(0.0591)	
Debt with Part. Creditors	10.9645	0.2235	−0.0179	0.3033
	(12.6128)	(0.1716)	(0.0443)	
Risk Score	-0.1514	−0.0067	−0.0012	0.2936
	(0.9305)	(0.0125)	(0.0030)	
Homeowner	0.4160	−0.0079	0.0024	0.4400
	(0.4929)	(0.0067)	(0.0019)	
Renter	0.4264	0.0081	−0.0020	0.4080
	(0.4946)	(0.0061)	(0.0018)	
Dependents	2.0922	−0.0096	0.0014	0.8560
	(1.3339)	(0.0191)	(0.0050)	
Monthly Income	2.6465	−0.0008	0.0005	0.9954
	(1.5137)	(0.0237)	(0.0063)	
Monthly Expenses	2.2121	−0.0041	0.0007	0.9762
	(1.3228)	(0.0208)	(0.0057)	
Total Assets	72.4978	−1.8962	0.4698	0.4194
	(110.0206)	(1.4450)	(0.4140)	
Total Liabilities	61.6932	−1.1192	0.2899	0.6123
	(83.9793)	(1.1396)	(0.3257)	
Bankruptcy	0.0030	0.0003	−0.0000	0.7533
	(0.0549)	(0.0006)	(0.0001)	
Employment	0.8612	0.0047	−0.0016	0.5602
	(0.3458)	(0.0055)	(0.0015)	
Earnings	25.7968	0.0721	−0.0100	0.9680
	(21.9611)	(0.3000)	(0.0800)	
Nonzero 401k Cont.	0.2490	−0.0018	0.0002	0.9482
	(0.4324)	(0.0064)	(0.0017)	
401k Contributions	0.4359	−0.0118	0.0021	0.7484
	(1.0481)	(0.0156)	(0.0043)	
Matched to SSA data	0.9488	−0.0002	0.0003	0.7929
	(0.2205)	(0.0029)	(0.0007)	
Observations	12,900	26,052		

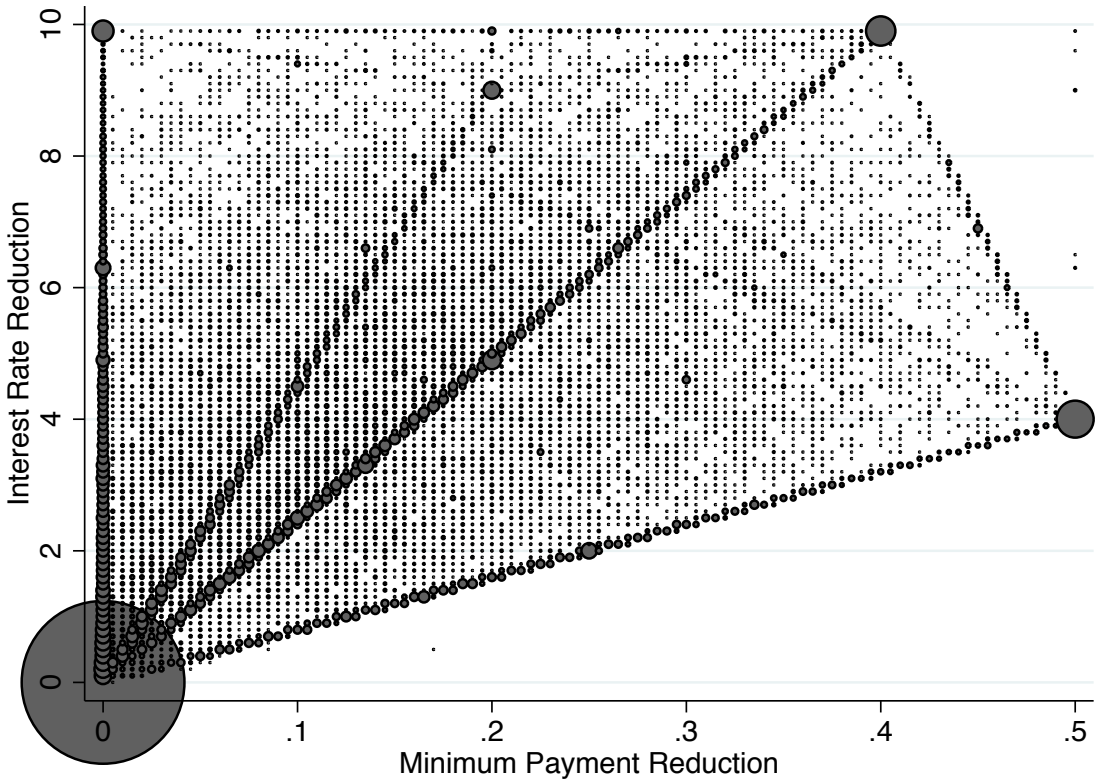
Notes: This table reports additional tests of the difference between control and treatment compliers based on program enrollment. We report coefficients on the interaction of treatment and potential treatment intensity. All regressions control for potential treatment intensity and strata fixed effects, and cluster standard errors at the counselor level. Column 4 reports the p-value from an F-test that all interactions are jointly equal to zero.

Appendix Figure 1
Actual Monthly Payments vs. Predicted Minimum Monthly Payments



Notes: This figure plots actual monthly payments in the MMI data and minimum monthly payments predicted using borrower debt holdings and treatment eligibility. See the text for additional details.

Appendix Figure 2
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.