Targeted Debt Relief and the Origins of Financial Distress: Experimental Evidence from Distressed Credit Card Borrowers*

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June 2017

Abstract

We study the drivers of financial distress using a large-scale field experiment that offered randomly selected borrowers a combination of (i) immediate payment reductions to target short-run liquidity constraints and (ii) delayed debt write-downs to target long-run debt constraints. We identify the separate effects of the payment reductions and debt write-downs using variation from both the experiment and cross-sectional differences in treatment intensity. We find that the debt write-downs significantly improved both financial and labor market outcomes despite not taking effect for three to five years. In sharp contrast, there were no positive effects of the more immediate payment reductions. These results run counter to the widespread view that financial distress is largely the result of short-run constraints.

*A previous version of this paper was circulated under the title "Debt Relief or Debt Restructuring? Evidence from an Experiment with Distressed Credit Card Borrowers." We are extremely grateful to Ann Woods and Robert Kaplan at Money Management International, David Jones at the Association of Independent Consumer Credit Counseling Agencies, Ed Falco at Auriemma Consulting Group, Jennifer Werkley at TransUnion, and Gerald Ray and David Foster at the Social Security Administration for their help and support. We thank Tal Gross, Matthew Notowidigdo, and Jialan Wang for providing the bankruptcy data used in this analysis. We also thank Leah Platt Boustan, Hank Farber, James Feigenbaum, Paul Goldsmith-Pinkham, Tal Gross, Larry Katz, Ben Keys, Patrick Kline, Ilyana Kuziemko, Alex Mas, Jesse Shapiro, Andrei Shleifer, Crystal Yang, Jonathan Zinman, Eric Zwick, and numerous seminar participants for helpful comments and suggestions. Kevin DeLuca, Daniel Herbst, Disa Hynsjo, Samsun Knight, Kevin Tang, Daniel Van Deusen, Amy Wickett, and Yining Zhu provided excellent research assistance. Financial support from the Washington Center for Equitable Growth is gratefully acknowledged. Correspondence can be addressed to the authors by e-mail: wdobbie@princeton.edu [Dobbie] or jae.song@ssa.gov [Song]. Any opinions expressed herein are those of the authors and not those of the Social Security Administration.

Financial distress is extraordinarily common in the United States. Over one-third of Americans have a debt in collections, and more than one in ten will file for bankruptcy at some point during their lives. Americans are also severely liquidity constrained, with approximately one-quarter of households unable to come up with $2,000 to cope with an unexpected need (Lusardi, Schneiderm, and Tufano 2011).[1] As a result, there is a widespread view that liquidity constraints are the most important driver of financial distress and that debt relief will be most effective when it targets these short-run constraints. This view has important implications for understanding both the growing level of financial distress in the United States and the optimal design of debt relief programs such as consumer bankruptcy. In this paper, however, we show that this view significantly overstates the benefits of debt relief targeting short-run liquidity constraints, while significantly understating the benefits of debt relief targeting longer-run financial constraints, such as the distortionary effects of excessive debt (so called "debt overhang").

Estimating the effects of targeted debt relief is challenging because most debt relief programs are designed to address both short- and long-run financial constraints at the same time. For example, consumer bankruptcy protection offers both lower minimum payments (to address short-run liquidity constraints) and generous debt write-downs (to address longer-run debt overhang). As a result, standard "black box" estimates of consumer bankruptcy cannot be used to predict the effects of specific types of targeted debt relief or to understand the relative importance of addressing short- or long-run financial constraints alone. An added complication is that most debt relief recipients are negatively selected, biasing cross-sectional comparisons, and many of the most proximate causes of debt relief receipt, such as job loss and expense shocks, also impact later outcomes, biasing within-individual comparisons.

In this paper, we overcome these challenges using information from a randomized field experiment matched to administrative tax, bankruptcy, and credit records. The experiment was designed and implemented by a large non-profit credit counseling organization in the context of an important but under-studied debt relief program called the Debt Management Plan (DMP). The DMP is a structured repayment program that allows distressed borrowers to simultaneously repay all of their outstanding credit card debt over a three to five year period. In exchange for enrolling in a DMP, credit card issuers will lower the minimum payment amount at the beginning of the repayment program (to address short-run liquidity constraints) and provide a partial write-down

[1] An additional 19 percent of households could only come up with $2,000 by pawning or selling possessions or taking out a payday loan (Lusardi, Schneider, and Tufano 2011). There is also evidence that many households have a high marginal propensity to consume out of both transitory income shocks (e.g., Johnson, Parker, and Souleles 2006, Parker et al. 2013) and new liquidity (e.g., Gross and Souleles 2002, Agarwal, Souleles, and Liu 2007, Agarwal et al. 2015, Gross, Notowidigdo, and Wang 2016), and recent work shows large changes in financial distress and consumption just after anticipated reductions in mortgage interest rates (e.g., Di Maggio, Kermani, and Ramcharan 2014, Keys et al. 2014, Fuster and Willen 2015). There is also an important literature showing that present-biased preferences can potentially explain both low levels of liquidity and the use of high-cost credit (e.g., Laibson 1997, Heidhues and Kőszegi 2010, Meier and Sprenger 2010, Laibson et al. 2017). See DellaVigna (2009) and Zinman (2015) for reviews of the literature on present-biased preferences and liquidity constraints, respectively. Evidence on longer-run problems such as debt overhang is more limited, although recent work shows that debt overhang can affect a household's labor supply (Bernstein 2016), entrepreneurial activity (Adelino, Schoar, and Severino 2013), and home investment (Melzer forthcoming).

of interest payments and late fees at the end of the repayment program (to address longer-run debt overhang). Each year, more than 600,000 individuals repay between $1.5 and $2.5 billion credit card debt through these repayment programs (Wilshusen 2011).

During the experiment, borrowers in both the treatment and control groups were offered a repayment program. While control borrowers were offered the status quo repayment program that had been offered to all borrowers prior to the randomized trial, treated borrowers were offered a much more generous repayment program that included a combination of two different types of targeted debt relief: (i) immediate minimum payment reductions meant to address short-run liquidity constraints and (ii) delayed debt write-downs meant to address longer-run debt overhang. The additional debt relief provided by the experiment was substantial: the typical minimum payment reduction for the treatment group was just over $26 (6.15 percent) per month larger than those in the status quo program, while the typical debt write-down in the treatment group was $1,712 (49.17 percent) larger than those in the status quo program. The economic magnitudes of the payment reductions and debt write-downs in the treatment group were also relatively similar, at least as measured by the net present costs of providing the debt relief (approximately $440 for the typical borrower).

We identify the separate impact of the debt write-downs and minimum payment reductions using variation from both the randomized experiment and cross-sectional differences in treatment intensity. Each of the credit card issuers participating in the randomized trial offered a different combination of debt write-downs and minimum payment reductions to treated borrowers, and individual borrowers made different decisions about how much to borrow from each of these credit card issuers before the experiment began. These decisions translated into economically significant differences in the debt write-downs and minimum payment reductions offered to the treatment group. For example, treated borrowers at the 75th percentile of the debt write-down distribution received write-downs that were $1,521 larger than treated borrowers at the 25th percentile of the distribution. Similarly, treated borrowers at the 75th percentile of the minimum payment distribution received payment reductions that were $33 per month larger than treated borrowers at the 25th percentile of the distribution. The interaction of the randomized experiment and these cross-sectional differences in treatment intensity allows us to isolate the effects of the payment reductions and debt write-downs in the treatment group.

To see the intuition for our approach, imagine a group of borrowers with a low debt write-down intensity and a low minimum payment intensity, and a second group of borrowers with a high debt write-down intensity but the same low minimum payment intensity. In this scenario, we can isolate the impact of a larger debt write-down at the margin by comparing the effect of the randomized treatment eligibility for the low debt write-down intensity borrowers to the effect of treatment eligibility for the high write-down intensity borrowers. We can similarly isolate the causal impact of the minimum payment reductions at the margin by comparing the effects of treatment eligibility for borrowers with different minimum payment intensities but identical debt write-down intensities. Our approach builds on identification strategies commonly used in studies of local labor markets, immigration, and trade, which exploits the combination of state- or city-level variation in potential treatment intensity and national-level variation in treatment status (e.g., Bartik 1991, Blanchard and Katz 1992, Card 2001, Autor, Dorn, and Hanson 2013). In contrast to these earlier studies, however, we use individual-level differences in treatment status determined by random assignment, and individual-level differences in potential treatment intensity determined by decisions made without knowledge of the experiment. As a result, our research design is robust to many of the potential concerns that typically arise from these types of instruments (e.g., Goldsmith-Pinkham, Sorkin, and Swift 2017).

We begin our analysis by estimating the effect of treatment eligibility on repayment, bankruptcy, collections debt, credit scores, employment, and savings. These intent-to-treat effects measure the impact of both the debt write-downs and minimum payment reductions. We find that treatment eligibility increased the probability of finishing the repayment program and decreased the probability of filing for bankruptcy, particularly for the highest-debt borrowers. We also find that treatment eligibility decreased the probability of having collections debt for high-debt borrowers. There were no detectable effects of treatment eligibility on labor market outcomes or 401k contributions for either high- or low-debt borrowers, although large standard errors mean that we cannot rule out modest treatment effects in either direction.

Next, we estimate the separate impact of the minimum payment reductions and debt write-downs. We find that the debt write-downs significantly improved both financial and labor market outcomes despite not taking effect until three to five years after the experiment. For the highest-debt borrowers, the median debt write-down in the treatment group increased the probability of finishing a repayment program by 1.62 percentage points (11.89 percent) and decreased the probability of filing for bankruptcy by 1.33 percentage points (9.36 percent). The probability of having collections debt also decreased by 1.25 percentage points (3.19 percent) for these high-debt borrowers, while the probability of being employed increased by 1.66 percentage points (2.12 percent). The estimated effects of the debt write-downs for credit scores, earnings, and 401k contributions are smaller and not statistically significant. Taken together, however, our results indicate that there are significant benefits of debt relief targeting long-run debt overhang in our setting.

In sharp contrast, we find no positive effects of the minimum payment reductions targeting short-run liquidity constraints. There was no discernible effect of the payment reductions on completing the repayment program, with the 95 percent confidence interval ruling out treatment effects larger than 0.15 percentage points in the pooled sample. The median payment reduction in the treatment group also increased the probability of filing for bankruptcy in this sample by a statistically insignificant 0.70 percentage points (6.76 percent) and increased the probability of having collections debt by a statistically significant 1.40 percentage points (3.56 percent). There are also no detectable positive effects of the payment reductions on credit scores, employment, earnings, or 401k contributions. In sum, there is no evidence that borrowers in our sample benefited from the minimum payment reductions, and even some evidence that borrowers seem to have been hurt by these reductions.

We show that these null results can be explained by the unintended, negative effect of increasing the number of months a borrower remains in the repayment program. The payments reductions increased the length of the repayment program in the treatment group by an average of four months and, as a result, increased the number of months where a treated borrower could be hit by an adverse shock that causes default (e.g., job loss). We find that the positive effects of increased liquidity in the treatment group were nearly exactly offset by the negative effects of this increased exposure to default risk. These results help to reconcile our findings the vast literature documenting liquidity constraints in a variety of settings (e.g., Gross and Souleles 2002, Johnson, Parker, and Souleles 2006, Agarwal, Souleles, and Liu 2007, Parker et al. 2013, Agarwal et al. 2015, Gross, Notowidigdo, and Wang 2016), while indicating that the potential benefits of targeting these short-run constraints may have been significantly overstated, at least in our setting.

Our results contribute to an emerging literature estimating the "black box" effects of consumer bankruptcy protection, which, as mentioned above, addresses both short- and long-run financial constraints at the same time. Consistent with our findings, bankruptcy protection increases post-filing earnings and decreases both post-filing mortality and financial distress (Dobbie and Song 2015, Dobbie, Goldsmith-Pinkham, and Yang forthcoming). There is also evidence that the availability of consumer bankruptcy as an outside option provides implicit health (Gross and Notowidigdo 2011, Mahoney 2015), consumption (Dobbie and Goldsmith-Pinkham 2014), and mortgage insurance (Li, White, and Zhu 2011). However, none of these papers are able to identify the effects of targeting either short-run liquidity constraints or long-run debt overhang alone.

This paper is also related to recent work estimating the effects of debt relief in the mortgage market. Mortgage modifications made through the HAMP program modestly decreased both mortgage and non-mortgage defaults, although it is unclear whether the effects were driven by lower minimum payments or lower debt burdens (Agarwal et al. 2012). More recent work suggests that the principal write-downs made through HAMP had no impact on underwater borrowers (Ganong and Noel 2017), while both cross-sectional regressions and theoretical work suggest that principal forgiveness may be effective for non-underwater borrowers (Haughwout, Okah, and Tracy 2010, Eberly and Krishnamurthy 2014).[2] While our results are broadly consistent with this literature, we caution against generalizing our results to the mortgage market. It is possible, for example, that liquidity constraints may be more important in the mortgage market, where delinquent borrowers often have fewer outside options than otherwise similar credit card borrowers.

The remainder of this paper is structured as follows. Section I describes the institutional setting and experimental design. Section II provides a simple conceptual framework for interpreting the experimental results. Section III describes our data and empirical design. Section IV presents our main results of how the randomized experiment impacted repayment, bankruptcy, financial health, employment, and savings. Section V explores potential mechanisms, and Section VI concludes.

[2]Related work shows that anticipated mortgage interest rate reductions decrease mortgage defaults and increase non-durable consumption (e.g., Di Maggio, Kermani, and Ramcharan 2014, Keys et al. 2014, Fuster and Willen 2015), although it is unclear whether these effects are driven by a lower minimum payment or a lower debt burden.

I. Background and Experimental Design

A. Background

The randomized experiment described in this paper was implemented and designed by Money Management International (MMI), the largest non-profit credit counseling agency in the United States. In the early 1950s, the first non-profit credit counseling organizations were established to increase credit card repayment rates and decrease the number of new bankruptcy filings. Today, non-profit credit counseling organizations such as MMI provide a wide range of services to its clients via phone and in-person sessions, including credit counseling, bankruptcy counseling, and foreclosure counseling.

One of the most important products offered by non-profit credit counselors is the debt management plan (DMP), a structured repayment program that simultaneously repays all of a borrower's outstanding credit card debt over three to five years.[3] Under the DMP, the credit counseling agency negotiates directly with each of the borrower's credit card issuers to lower the minimum payment amount (to address short-run liquidity constraints) and partially write-down interest payments and late fees (to address longer-run debt overhang). In most cases, credit card issuers will also agree to stop recording the debt as delinquent on the borrower's credit report. Compared to making only the minimum payment on a credit card, enrolling in a DMP will reduce the average borrower's monthly payments by about 10 to 15 percent and reduce the total cost of repayment by about 20 to 40 percent. Following the negotiations with the credit card issuers, the borrower makes one monthly payment to the credit counseling agency that is disbursed to his or her creditors according to the terms of the restructured agreements. The minimum monthly payment for each credit card account is typically about two to three percent of the original balance, although borrowers can make additional payments to reduce the length of the repayment program. In our sample, the average minimum monthly payment for the control group is 2.38 percent of the original balance, or about $437, and the average length of the repayment program is 52.7 months.

Creditors will usually allow borrowers to resume the repayment program if they miss just one or two payments. However, if a borrower misses too many payments or withdraws from the program, the remaining credit card debt is usually sent to collections. At this point, either the original credit card issuer or a third-party debt collector will use a combination of collection letters, phone calls, wage garnishment orders, and asset seizure orders to collect the remaining debt. 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. Most borrowers also have the option of discharging the remaining credit card debt through the consumer

[3] Under current regulatory guidelines, the term length for a DMP cannot exceed five years. If borrowers cannot fully repay their credit card debts within this five-year limit, they cannot participate in a DMP unless the creditor is willing to write off a portion of the original balance and recognize the loan as impaired. To date, however, creditors have typically been unwilling to do this (Wilshusen 2011).

bankruptcy system. In all of these scenarios, however, borrowers' credit scores are likely to be adversely affected, at least in the short run.

To help ensure that creditors benefit from their participation in the repayment program, the counseling agency screens potential clients to assess whether the borrower has a sufficient cash flow to repay his or her debts over the three to five year period of the repayment program, but not enough to reasonably repay his or her debts without the repayment program. In practice, potential clients who pass this screening process have similar credit scores and financial outcomes as bankruptcy filers, but more adverse outcomes than the typical credit user in the United States (e.g., Dobbie et al. forthcoming). Historically, credit card issuers have given credit counseling agencies the incentive to effectively screen potential clients through a combination of monitoring and the fair share payments discussed above. To strengthen the counseling agencies' incentive to effectively screen clients, many credit card issuers also condition their payments to the counseling agency on the borrower's completion of the repayment program (Wilshusen 2011).[4]

The participation of the credit card issuers in a DMP is voluntary, and card issuers may choose to participate in only a subset of the DMPs proposed by the credit counseling agencies. In principle, a credit card issuer will only participate in a repayment program if doing so increases the expected repayment rate, presumably because the borrower is less likely to default or file for bankruptcy (Wilshusen 2011). Consistent with this view, 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) than observably similar individuals who are not enrolled in a DMP. Credit card issuers can also directly refer borrowers to a credit counseling agency if the risk of default or bankruptcy is particularly high. In our sample, approximately 15.5 percent of individuals report that they learned about MMI from a card issuer. In comparison, 33.7 percent of individuals in our sample report that they learned about MMI from an internet search, 19.8 percent from a family member or friend, and 20.0 percent from a paid advertisement.

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 credit card issuers between $1.5 and $2.5 billion each year (Hunt 2005, Wilshusen 2011).

B. Experimental Design

Overview: In 2003, MMI and eleven large credit card issuers agreed to offer more generous minimum payment reductions and debt write-downs to a subset of borrowers interested in a structured repayment program. The purpose of the experiment was to evaluate the effect of more generous debt relief on repayment rates, particularly for the most financially distressed borrowers.

[4]The costs of administering the DMP are covered by a small administrative fee of about $10 to $50 paid by the borrower and these larger "fair share" payments paid by the credit card issuers. Fair share payments have become somewhat less generous over time, falling from an average of twelve to fifteen percent of the recovered debt in the 1990s to about five to ten percent of the recovered debt today (Wilshusen 2011). To the best of our knowledge, both the fair share payments and administrative fees remained relatively constant throughout the experiment.

The resulting randomized experiment was conducted between January 2005 and August 2006. The experimental population consisted of the near universe of prospective clients that contacted MMI during this time period. There were two main restrictions to the experimental sample. First, the experiment was restricted to individuals contacting MMI for the first time during this time period; individuals who had already enrolled in a DMP before January 2005 were excluded from the randomized trial. Second, the experiment was restricted to individuals assigned to counselors with more than six months of experience. In total, the experimental sample included 79,739 borrowers assigned to 709 different counselors.

Sequence of the Experiment: First, each prospective client was randomly assigned to a credit counselor conditional on the contact date, the individual's state of residence, and the reference channel (i.e. web versus phone). For each counselor, the MMI computer system would automatically switch from the control group repayment program to the treatment group repayment program every two weeks. This automated rotation procedure was meant to ensure that experimental protocols were followed by the counselors and that any counselor-specific effects would not bias the experiment. The rotation procedure was also staggered across counselors so that, on any given day, approximately 50 percent of individuals were assigned to the treatment group and approximately 50 percent were assigned to the control group. Counselors were strictly instructed not to inform prospective clients of the experiment, and a senior credit counselor conducted frequent audits of the counselors to ensure that the experimental protocols were followed and that the treatment and control populations remained of relatively similar sizes during the experiment. MMI worked with the participating credit card issuers to design the automated rotation procedure, but none of the card issuers were directly involved with the implementation of the experiment or the auditing process.

Following the assignment of an individual to a credit counselor, the assigned counselor collected information on the prospective client's unsecured debts, assets, liabilities, monthly income, monthly expenses, homeownership status, number of dependents, and so on. Identical information was collected from both the treatment and control groups, and there was no indication of treatment status communicated to individuals. Using the information collected by the counselor, the MMI computer system would then calculate the individual-specific terms of the repayment program, including the minimum payment amount, the length of the program, and the total financing fees. These terms depended on the amount of debt with each credit card issuer and whether the individual was assigned to the treatment or control group.

Next, the credit counselor would explain the individual's options for repaying his or her debts. The details of this process closely followed MMI's usual procedures and were identical for the treatment and control groups. In most cases, the repayment options were explained in the following way. First, individuals were told that they could liquidate their assets and repay their debts immediately, although relatively few individuals in our sample had enough assets to make this a viable option. Next, individuals were told that they could file for Chapter 7 bankruptcy, which would allow them to discharge their unsecured debts and avoid debt collection in exchange for any non-exempt assets and the required court fees. Third, individuals were told what would happen if they continued paying only the minimum payment on their credit cards. In a representative call provided to the research team, the MMI counselor explained 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." Finally, individuals were told about the benefits of enrolling in a structured repayment program. In the same representative call, the MMI counselor explained that if the individual enrolled in a DMP, her payments would "drop to $301, 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."

Finally, the individual would indicate whether he or she wished to enroll in the offered repayment program following the counselor's explanation of the repayment options. Individuals could also call back at a later date to enroll in the repayment program under the same terms.

Treatment Intensity: Table 1 illustrates how the experiment impacted the typical borrower's repayment program. Each row presents DMP terms for a hypothetical borrower with the control mean for credit card debt acquired before the experiment ($18,212). We first calculate the DMP terms for this hypothetical borrower as if he or she had been assigned to the control group, i.e. using the control means for the both minimum payment requirement (2.38 percent of initial debt) and the implied interest rate (8.50 percent). For this hypothetical borrower, the control repayment program requires making minimum payments of $433.45 for 50.05 months, with $3,482 in financing fees.

Next, we recalculate the DMP terms for this hypothetical borrower using the median debt write-down in the treatment group (a 3.69 percentage point decrease in the implied interest rate), holding the minimum payment percentage constant. The median debt write-down in the treatment group decreases these financing fees by $1,712, or 49.17 percent, by dropping the last four payments of the borrower's repayment program. However, the debt write-down does not affect the borrower's minimum payment amount. As a result, the debt write-down will only increase enrollment in the repayment program if borrowers value debt forgiveness at the end of the repayment program, about three to five years in the future.

Finally, we recalculate the DMP terms using the median minimum payment reduction in the treatment group (a 0.14 percentage point decrease in the minimum payment percentage), holding fixed the debt write-down amount. The median minimum payment reduction in the treatment group decreases the typical borrower's minimum payment by $26.68, or 6.15 percent, by adding an additional four months to the repayment program. The longer repayment period also increases the financing fees by $289, or 8.30 percent. Thus, the minimum payment reductions may decrease liquidity-based defaults at the beginning of the repayment program by lowering the minimum payment amount and increase defaults at the end of the repayment program by mechanically increasing the exposure to default risk.

Variation in Treatment Intensity: As discussed above, an important feature of the experiment is the significant cross-sectional variation in potential treatment intensity (see Appendix Figure 1). To illustrate the economic significance of this variation, we recalculate the DMP terms using debt write-downs and minimum payment reductions at different points in the treatment intensity distribution. The difference between the 25th percentile and 75th percentile debt write-downs within the treatment group is roughly equivalent to the difference between the median control group write-down and the median treatment group write-down ($1,521 versus $1,712). Similarly, the difference between the 25th percentile and 75th percentile minimum payment reductions within the treatment group is slightly larger than the difference between the median control group reduction and the median treatment group reduction ($33 per month versus $26 per month).

These cross-sectional differences in treatment intensity are driven, in part, by each of the credit card issuers offering a different combination of debt write-downs and minimum payment reductions to treated borrowers. Appendix Table 1 lists the treatment and control group offers for each of the eleven credit card issuers participating in the experiment. There were seven different combinations of the debt write-downs and minimum payment reductions offered to treated borrowers, with considerable variation in the approaches taken by each credit card issuer. For example, one of the credit card issuers offered the largest debt write-down (a 9.9 percentage point decrease in the implied interest rate) and no minimum payment reduction to treated borrowers, while another offered the largest minimum payment reduction (a 0.5 percentage point decrease in the minimum payment percentage) and the smallest debt write-down (a 4.0 percentage point decrease in the implied interest rate). While there are no records explaining why the credit card issuers offered the combinations of treatments that they did, MMI believes that these decisions were driven by the idiosyncratic views of individual employees at each credit card issuer. Consistent with this explanation, there are no systematic patterns between the generosity of the debt write-downs and minimum payment reductions offered before the experiment and the generosity of the treatments during the experiment.

The cross-sectional differences in treatment intensity are also driven by individual borrowers making different decisions about how much to borrow from each of the credit card issuers before the experiment began. Importantly, we do not assume that these borrowing decisions are random. As will be discussed below, the key identifying assumption for our approach is that potential treatment intensity is not correlated with the potential benefits of the debt write-downs and minimum payment reductions. We view this assumption as reasonable given that there was no way for individuals to know which credit card issuers would offer which debt write-down and minimum payment treatments, and therefore no reason to believe that the differences in potential treatment intensity will be correlated with the unobserved benefit of the experimental treatments. We will also provide direct support for our identifying assumption below.

Treatment Costs: Table 1 also provides cost estimates for the median debt write-downs and minimum payment reductions in the treatment group. We use the control mean for the monthly default rate during the repayment program (1.12 percent) to capture the mechanical default risk associated with a shorter or longer repayment program. As the costs of the debt write-downs and minimum payment reductions in the treatment group are realized at different points in the repayment program (i.e. the end of the repayment program versus throughout the entire repayment program), we present estimates using discount rates of 0.0 percent, 8.5 percent (the control mean interest rate), and 20 percent (a typical interest rate in the credit card market).

The discounted costs of the median debt write-down and median minimum payment reduction in the treatment group are nearly identical (\$440 versus \$444) with a 20 percent discount rate. Under an 8.5 percent discount rate, however, the cost of the median debt write-down in the treatment group is over double the cost of the median minimum payment reduction in the treatment group (\$802 versus \$332), with even larger differences at lower discount rates. As discussed above, this is because the costs of the debt write-downs and minimum payment reductions in the treatment group are realized at different points in the repayment program. Nevertheless, we interpret these calculations as suggesting that the experiment provides a reasonably "fair" comparison of the two different types of debt relief.

C. External Validity

In this section, we discuss how the details of the experimental design may affect the externality validity of our results.

Framing Effects: As discussed above, MMI emphasized the monthly payment amount, time to repayment, and financing fees when explaining the repayment program to both the treatment and control groups during the experiment. While the internal validity of the experiment is not affected by these details of the experimental design, it is possible that the effects of the debt write-downs and minimum payment reductions are mediated by these institutional details. For example, it is possible that emphasizing the monthly payment amount increases the perceived value of a minimum payment reduction. It is also possible that emphasizing financing fees, rather than the total amount of debt repaid, either increases or decreases the perceived value of a debt write-down. Importantly, however, these experimental procedures closely followed both MMI's usual procedures and the way in which the write-downs and payment reductions would be implemented at scale through a typical DMP. Our estimates therefore measure the impact of targeted debt relief in one of the most policy-relevant contexts. Nevertheless, all of our results should be interpreted with these potential framing effects in mind.

Non-Linear Treatment Effects: Another potential concern is that we estimate the impact of debt write-downs and minimum payment reductions at the margin of an existing debt relief program, making it impossible to estimate the impact of the first dollar of a debt write-down or the first dollar of a payment reduction using our experimental data. We also do not observe the kind of extremely large debt write-downs or minimum payment reductions needed to estimate, for example, a nearly complete write-down of the original balance. As a result, out-of-sample predictions based on our experimental estimates will be biased if there is a non-linear effect of either the debt write-downs or the minimum payment reductions.

To shed some light on this issue, Appendix Figure 2 presents non-parametric estimates of the debt write-downs and minimum payment reductions in our experiment. We estimate these non-parametric treatment effects by grouping our treatment intensity measure into equally-sized bins for both the debt write-downs and minimum payment reductions (see Section III.C for details of the empirical specification and treatment intensity measure). We report the interaction of treatment eligibility and each treatment intensity bin, controlling for both treatment intensity and the state by reference group by date fixed effects that account for the stratification used in the randomization of individuals to counselors. We also plot the OLS best-fit line weighted by the standard error for each point estimate. The results are consistent with linear treatment effects over the range of treatment intensities observed in our data. Of course, we cannot test whether there are non-linear effects for treatment intensities that we do not observe in the data.

II. Conceptual Framework

In this section, we develop a stylized model to motivate our empirical analysis and to clarify how the reduced form parameters we estimate should be interpreted. We focus exclusively on the broad role of short-run liquidity constraints and longer-run debt overhang, abstracting from other drivers of financial distress such as job loss or health shocks.[5] Using the model, we show that back-loaded debt write-downs have a positive impact on repayment due to a decrease in forward-looking defaults at the beginning of the experiment and a decrease in exposure-related defaults at the end of the experiment. In contrast, more immediate minimum payment reductions have an ambiguous impact due to offsetting effects on liquidity-based defaults at the beginning of the experiment and exposure-related defaults at the end of the experiment.

A. Model Setup

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. Following the structure of the repayment program we study, debt payments begin at $t = 0$ and are set at a constant level d for 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

[5]We also do not attempt to model every possible mechanism through which liquidity constraints and debt overhang affect financial distress. The conclusions we draw in this section should be interpreted with these modeling choices in mind. Our model is related to a large literature examining the causes and consequences of individual default using quantitative models of the credit market. For example, see Chatterjee et al. (2007) for a general model of consumer default and Benjamin and Mateos-Planas (2014) for a model that distinguishes between formal and informal consumer default. Our model is also related an emerging literature that estimates the separate impact of different forms of hidden information and hidden action. See Adams, Einav, and Levin (2009) and Karlan and Zinman (2009) for examples of these approaches using observational and experimental data, respectively.

defaults on the remaining payments in period t for any reason, she loses her current income draw y_t and receives a constant amount x in period t and all future time periods. To capture the idea of a potentially binding liquidity or credit constraint, we assume that individuals automatically default if net income $y_t - d_t$ falls below threshold $\underline{v}$, regardless of the value of future cash flows.

Let $V^q(t, y)$ denote the continuation value of making repayment decision q in period t given income draw y. For periods $0 \leq t < P$, the continuation value of default $V^d(t, y)$ is equal to the discounted value of receiving x in both the current period and all future periods:

$$V^d(t, y) = \frac{x}{1-\beta} \tag{1}$$

The continuation value of repayment $V^r(t, y)$ consists of the contemporaneous value of repayment $y - d$ and the option value of being able to either repay or default in future periods:

$$V^r(t, y) = y - d + \beta \left[\int_{\underline{v}+d}^{\infty} \max \left\{ V^r\left(t+1, y^{'}\right), V^d(t, y) \right\} dF\left(y^{'}\right) + F\left(\underline{v} + d\right) V^d(t, y) \right] \tag{2}$$

The contemporaneous value of repayment $y - d$ is unaffected by the time period t, while the option value of continuing repayment, and hence the total value of continuing repayment, is weakly increasing in t for $t < P$. This is because the option value of repayment increases as individuals become closer to the "risk-free" time periods after the completion of the repayment program.

Repayment and default behavior is described by a path of cutoff values ϕ_t, where an individual defaults if $y_t < \phi_t$. The default cutoff ϕ_t combines the optimal strategic response of liquid individuals to low income draws and the non-strategic response of illiquid individuals based on $\underline{v}$ that may or may not be optimal. Following the above logic, the strategic default cutoff is weakly decreasing over time, reflecting the decreased incentive to default as individuals' remaining loan balances shrink. Appendix A provides additional details on the above results.

B. Model Predictions

Motivated by the experiment, we consider the comparative statics of debt write-downs and minimum payment reductions on repayment rates.

Debt Write-Down Prediction: In the model, back-loaded debt write-downs increase repayment rates through two complementary effects: (1) a forward-looking debt overhang effect that decreases the treatment group's incentive to strategically default while both treatment and control groups are enrolled in the repayment program and (2) a mechanical exposure effect that decreases the treatment group's exposure to default risk while the control group is still enrolled in the repayment program and the treatment group is not.
Proof – See Appendix A.

To see the intuition for this result, recall that the debt write-downs forgive treated borrowers' monthly payments at the end of the repayment program. As a result, the debt write-downs will increase repayment rates through a forward-looking debt overhang effect if borrowers value debt forgiveness three to five years in the future. The mechanical exposure effect is driven by the fact that, conditional on enrolling in the repayment program, the debt write-downs make it impossible for treated borrowers to default when their payments have been forgiven.

Formally, let d^{WD} and P^{WD} denote the monthly payment amount d and repayment period P for the debt write-down group WD, and d^C and P^C denote the monthly payment amount and repayment period for the control group C. We model the debt write-downs as reducing the overall cost of the debt by shortening the repayment period for the treatment group, $P^{WD} < P^C$, without changing the monthly payments $d^{WD} = d^C = d$. In this context, the forward-looking debt overhang channel is driven by the fact that for $0 \leq t \leq P^{WD}$, shortening the length of the repayment period brings individuals in any given period $P^C - P^{WD}$ 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 strategic, forward-looking default cutoff for liquid individuals during this time period. However, disposable income for $0 \leq t \leq P^{WD}$ remains the same, so there is no difference in the probability that an individual defaults due to the liquidity constraint $\underline{v}$ during this time period. In other words, there will only be an increase in repayment for $0 \leq t \leq P^{WD}$ if the forward-looking default cutoff is the relevant margin for at least some individuals.

The mechanical exposure effect is driven by the fact that, for $P^{WD} < t \leq P^C$, default rates mechanically drop to zero for the treatment group as they have completed the repayment program. However, the control group can still default on their debt if either the liquidity-based or forward-looking cutoffs bind over this time period. The debt write-downs can therefore increase repayment rates even if individuals never strategically default (i.e. if individuals only default due to a binding liquidity constraint) if there is sufficient default risk at the end of the repayment program.

Minimum Payment Prediction: The minimum payment reductions have an ambiguous impact on repayment rates in the model due to three different effects: (1) a liquidity effect that decreases the treatment group's probability of non-strategic or liquidity-based default while both the treatment and control groups are enrolled in the repayment program, (2) a second liquidity effect that ambiguously changes the treatment group's incentive to strategically default while both the treatment and control groups are enrolled in the repayment program, and (3) a mechanical exposure effect that increases the treatment group's exposure to default risk while the treatment group is still enrolled in the repayment program and control group is not.
Proof – See Appendix A.

To see the intuition for this result, recall that the minimum payment reductions reduce treated borrowers' minimum payment by increasing the length of the repayment program. In the model, the minimum payment reductions therefore decrease liquidity-based defaults at the beginning of the repayment program through the lower required payments, but increase defaults at the end of the repayment program through the increased exposure to all forms of default risk. The minimum payment reductions also change the option value of repayment, and hence the incentive to strategically default. The direction of this “indirect” liquidity effect is ambiguous, as the minimum payment reductions both increase future flexibility and transfer a portion of the debt burden into the future.

Formally, let d^{MP} and P^{MP} denote the monthly payment d and repayment period P for the minimum payment group MP. We model the minimum payment reductions as a lengthening of the repayment period from P^C to $P^{MP} > P^C$ that keeps the total sum of the monthly payments the same $\sum_{t=0}^{P^C} d_t = \sum_{t=0}^{P^{MP}} d_t$. The first liquidity effect is driven by the fact that the minimum payment reductions decrease the probability that the non-strategic cutoff binds for illiquid individuals for $0 \leq t \leq P^C$, increasing repayment rates over this time period if the liquidity-based default cutoff is the relevant margin for at least some individuals.

The second liquidity effect is due to the indirect effect of the minimum payment reductions on the incentive to strategically default for $0 \leq t \leq P^C$. The direction of this indirect effect is ambiguous, as the minimum payment reductions both decrease per-period repayment costs, increasing the option value of repayment, and increase the number of periods to repay, decreasing the option value of repayment. These opposing effects on the option value of repayment are not unique to minimum payment reductions; other policies that target liquidity constraints such as payment deferrals or higher credit limits will also exhibit these kinds of opposing effects. We therefore think of the liquidity effect as including both the direct effects on liquidity-based defaults discussed above and the indirect effects on the option value of repayment discussed here. We assume throughout that the liquidity effect net of these two channels is positive, although our results do not rely on this assumption.

Following the discussion for the debt write-down prediction, the mechanical exposure effect is driven by the fact that, for $P^C < t \leq P^{MP}$, default rates mechanically drop to zero for the control group, while the treatment group can still default on their debt if either the liquidity-based or strategic cutoffs bind over this time period. This exposure effect allows for the possibility that the minimum payment reductions will have a negative effect on repayment rates.

III. Data and Empirical Design

A. Data Sources and Sample Construction

To estimate the impact of the randomized experiment, we match counseling data from MMI to administrative bankruptcy, credit, and tax 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 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 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 debt write-downs and minimum payment reductions as a result of the participating issuers offering different concessions to treated borrowers. To measure this variation in treatment intensity, we first calculate the write-downs and minimum payments for all individuals as if they had been assigned to the control group and as if they had been assigned to the treatment group. In this step, we use the exact calculation that MMI uses, repeating this calculation under both the control and treatment scenarios. We then calculate the difference between the control write-downs and the treatment write-downs (in terms of the implied interest rate) for each individual, and the control minimum payment and treatment minimum payment (in terms of percent of the original balance) for each individual. These write-down and minimum payment differences are our individual-level measures of potential treatment intensity. Importantly, we observe virtually all of the same information that MMI uses to calculate the terms of the structured repayment program.[6]

Information on bankruptcy filings comes from individual-level PACER bankruptcy records. The bankruptcy records are available from 2000 to 2011 for the 81 (out of 94) federal bankruptcy courts that allow full electronic access to their dockets. These data represent approximately 87 percent of all bankruptcy filings during our sample period.[7] 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 individuals 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. We also pool Chapter 7 and Chapter 13 filings throughout the analysis. Results are similar if we limit the sample to borrowers living in states with PACER data coverage.

Information on collections debt and credit scores come from individual-level credit reports from TransUnion (TU). The TU data are derived from public records, collections agencies, and trade lines data from lending institutions. The collections data contain information on any unpaid bills that have been sent to collection agencies, including the date of collections and the current amount owed. The credit score we use is calculated by TU to predict the probability that a consumer will become delinquent on a new loan within the next 24 months. Since credit scores are used in the vast majority of lending decisions, improvements in credit scores should directly translate into increased credit availability, lower interest rates, or both (e.g., Dobbie et al. 2016). TransUnion was able to successfully match 89.7 percent of the credit counseling data to the credit bureau data. The probability of being matched to the credit report data is not significantly related to treatment

[6]Specifically, we have information on interest rates and minimum payments for the nineteen largest creditors in the sample, including all eleven of the credit card issuers participating in the experiment. For the 16.7 percent of debt held by smaller creditors not participating in the experiment, 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. Our results are robust to a wide range of alternative assumptions.

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

status (see Panel C of Table 2). No personally identifiable information ("PII") were provided to us by TransUnion.

Information on formal sector 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 has ever acquired an SSN, including those who are institutionalized. Information on annual earnings, employment, and 401k contributions come from annual W-2s.[8] The earnings and employment variables include all formal sector earnings, but do not include earnings from the informal sector. The 401k variable includes all conventional, pre-tax contributions, but does not include contributions to Roth accounts. 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 or zero 401k contributions 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 also not significantly related to treatment status (see Panel C of Table 2).

We make two sample restrictions to the final dataset. First, we drop individuals that are not randomly assigned 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 labor market and 401k outcomes is further restricted to 76,008 individuals matched to the SSA data and our sample for the collections debt and credit score outcomes is further restricted to the 71,516 individuals matched to the TU data.

B. Descriptive Statistics and Experiment Validity

Table 2 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. Thirty-six percent of borrowers are men, 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 experiment. Monthly household incomes average about $2,450, and monthly expenses average about $2,150.

Panel B of Table 2 presents baseline outcomes for the year before contacting MMI. Not surprisingly, individuals in our sample are severely financially distressed before contacting MMI. Baseline credit scores in our sample are about 585 points, with 25.3 percent of individuals in our sample having nonzero collections debt. In comparison, the typical bankruptcy filer has a credit score of

[8] 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 for these outcomes are small and not statistically different from zero.

630 points, with 29.6 percent of filers having nonzero collections debt (Dobbie et al. forthcoming). 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 are not the sole earner in the household, that some individuals have earnings in the informal sector not captured by the SSA data, or that there is an upward bias in the self-reported earnings. Eighty-five percent of borrowers in our sample are employed in the formal sector 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 D of Table 2 presents measures of potential treatment intensity calculated using the MMI data. Specifically, we calculate the implied interest rate, the minimum payment percentage, and the program length in months for each borrower as if they had been assigned to the control group and as if they had been assigned to the treatment group (see Section III.A for details). As would be expected given the random assignment, the treatment and control groups have similar potential program characteristics. If assigned to the control group, the typical treated borrower would have had an implied interest rate of 8.5 percent, a minimum payment of 2.4 percent of the initial balance, and a program length of just over 52.6 months. Similarly, the typical control borrower actually had an implied interest rate of 8.4 percent, a minimum payment of 2.4 percent of the initial balance, and a program length of about 52.7 months. If assigned to the treatment group, those same control borrowers would have had an implied interest rate of 6.0 percent, a minimum payment of 2.3 percent of the initial balance, and a program length of 51.9 months, nearly exactly the program characteristics that the treatment group actually had.

Column 3 of Table 2 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 prospective 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 in Panels A-D 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 an F-test of the joint significance of all of the variables listed is 0.807, suggesting that the randomization was successful.

Panel E of Table 2 presents measures of the actual program characteristics offered to borrowers in the treatment and control groups (i.e. the "first stage" of the experiment). Consistent with the results from Panel D, treated borrowers have implied interest rates that are 2.6 percentage points lower than control borrowers, minimum payments that are 0.1 percentage points lower, and program lengths that are 0.8 months shorter.

C. Empirical Strategy

Intent-to-Treat Estimates: We begin our empirical analysis by estimating the impact of treatment eligibility using the following reduced form specification:

$$y_{it} = \alpha_0 + \alpha_1 Treat_i + \alpha_2 \mathbf{X}_i + \eta_{it} \quad (3)$$

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, 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 2 and cluster the standard errors at the counselor level in all specifications. Estimates without individual controls are available in Appendix Table 2.

Estimates of α_1 measure the causal impact of being offered a more generous repayment program on subsequent outcomes. However, two important issues complicate the interpretation of these intent-to-treat estimates. First, treated borrowers were offered a repayment program that included a combination of both the debt write-downs and minimum payment reductions. Thus, the intent-to-treat estimates measure the combined effect of both forms of debt relief and do not allow us to identify the separate impact of debt relief addressing short-run liquidity constraints and debt relief addressing longer-run debt overhang.

The second issue is that the intent-to-treat estimates understate the true impact of targeted debt relief because of the substantial cross-sectional variation in treatment intensity in our sample. For example, over 25 percent of borrowers in our sample had no credit card debt with the eleven credit card issuers participating in the experiment and, as a result, were offered the status quo, or "control" repayment program even when they were assigned to the treatment group. In total, nearly 90 percent of borrowers received a less intensive treatment than originally intended because they had at least some credit card debt with a non-participating issuer.

Isolating the Effects of Debt Write-Downs and Minimum Payment Reductions: We identify the separate impact of the debt write-downs and minimum payment reductions using variation from both the randomized experiment and the cross-sectional differences in treatment intensity. Recall that we can measure individual-level treatment intensities by using the detailed data from the non-profit credit counselor to calculate the difference between each borrower's hypothetical control and hypothetical treatment repayment program offers. We can then isolate the effects of the debt write-downs and minimum payment reductions by comparing the effects of treatment eligibility across borrowers with higher and lower treatment intensities.

To see the intuition for our approach, imagine a group of borrowers with a low debt write-down intensity and a low minimum payment intensity, and a second group of borrowers with a high debt write-down intensity but the same low minimum payment intensity. For the first group of borrowers, the intent-to-treat estimates from Equation (3) measure the impact of a low-intensity change in both the debt write-down and minimum payment amount. For the second group of borrowers, however, the intent-to-treat estimates measure the impact of a high-intensity change in the debt write-down and a low-intensity change in the minimum payment amount. Our approach isolates the impact of a larger debt write-down at the margin by comparing the intent-to-treat estimates for the low debt write-down intensity borrowers to the intent-to-treat estimates for the high write-down intensity borrowers. We similarly isolate the causal impact of the minimum payment reductions at the margin by comparing the effects of treatment eligibility for borrowers with different minimum payment intensities but identical debt write-down intensities.

Formally, we define the potential debt write-down and minimum payment treatment intensities as the difference between hypothetical treatment and hypothetical control repayment program offers:

$$\Delta WriteDown_i = WriteDown_i^C - WriteDown_i^T$$
$$\Delta Payment_i = Payment_i^C - Payment_i^T$$

where $\Delta WriteDown_i$ is the percentage point difference between the control interest rate $WriteDown_i^C$ and treatment interest rate $WriteDown_i^T$ for borrower i, and $\Delta Payment_i$ is the percentage point difference between the control minimum payment percentage $Payment_i^C$ and treatment minimum payment percentage $Payment_i^T$.

Using these individual-level measures of potential treatment intensity, we estimate the separate effects of the debt write-downs and minimum payment reductions using the following reduced form specification:

$$y_{it} = \beta_0 + \beta_1 Treat_i \cdot \Delta WriteDown_i + \beta_2 Treat_i \cdot \Delta Payment_i + \beta_3 \Delta WriteDown_i + \beta_4 \Delta Payment_i + \beta_5 \mathbf{X}_i + \varepsilon_{it} \quad (4)$$

We control for any independent effects of $\Delta WriteDown_i$ and $\Delta Payment_i$ because the variation in these measures may reflect unobserved borrower characteristics that have an independent impact on future outcomes y_{it}. For example, it is possible that the decision to borrow from card issuers with particularly high $\Delta WriteDown_i$ or $\Delta Payment_i$ is correlated with risk aversion or financial sophistication. As will be clear below, our approach does not assume that these treatment intensities are randomly assigned. Rather, we assume that the interaction between treatment eligibility and potential treatment intensity is conditionally random once we control for $\Delta WriteDown_i$ and $\Delta Payment_i$. Following the intent-to-treat results, we also control for the variables listed in Table

2 and cluster the standard errors at the counselor level.[9,10]

Estimates of β_1 and β_2 measure the separate effect of being offered the debt write-downs and minimum payment reductions by comparing the impact of the randomized experiment across borrowers that differed in their potential treatment intensities. Our interpretation of the estimates relies on two main assumptions.

Our first assumption is that treatment eligibility is, in fact, random. As with any non-experimental design, our estimates will be biased if treatment eligibility is correlated with unobserved determinants of future outcomes ε_{it}. However, this assumption is almost certainly satisfied in our setting, as treatment eligibility is randomly assigned by the non-profit credit counselor. To partially test this assumption, Appendix Table 4 presents summary statistics separately by treatment intensity bins and Appendix Table 5 presents results from a series of OLS regressions of each baseline variable on the interaction of treatment eligibility and potential treatment intensity. There are no statistically significant relationships between our baseline measures and the interaction of treatment eligibility and potential treatment intensity, suggesting that the randomization was successful within treatment intensity bins.[11]

Our second identifying assumption is an exclusion restriction that the interaction of treatment eligibility and potential treatment intensity only impacts borrower outcomes through an increase in actual treatment intensity. This identifying assumption would be violated if potential treatment intensity is correlated with treatment effect heterogeneity.[12] For example, our estimates would be biased if individuals with a higher local average treatment effect (LATE) to debt relief were more likely to borrow from card issuers offering the more intensive debt relief. In this scenario, our estimates would include both the true effect of the more intensive debt relief and systematic treatment eligibility x issuer "effects" from the sorting of borrowers with higher LATEs to creditors with the more intensive debt relief. Recall, however, that individuals chose their credit cards many

[9]Equation (4) implicitly assumes that there are no direct effects of treatment eligibility and that the effects of the the debt write-downs and minimum payment reductions are linear and additively separable. Consistent with the first assumption, our reduced form results are unchanged when we add an indicator for treatment eligibility. The coefficient on the indicator for treatment eligibility is also small and not statistically different from zero. To partially test the assumption of linear and additively separable treatment effects, Appendix Table 3 presents non-parametric estimates using bins of treatment intensity that do not rely on any functional form assumptions. The results are broadly consistent with linear and additively separable treatment effects, although large standard errors make a precise test of these assumptions impossible.

[10]We include all borrowers – including those with no debts with creditors participating in the experiment – when estimating Equation (4) in order to identify the strata fixed effects. Results are similar if we restrict our sample to individuals with at least one debt with a participating creditor.

[11]Appendix Table 6 describes the correlates of potential treatment intensities. Borrowers with larger potential debt write-downs are less likely to be black, more likely to be homeowners, and have higher baseline earnings. Borrowers with larger potential minimum payment reductions 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 participating issuers also have larger potential treatment intensities.

[12]Our second identifying assumption would also be violated if measurement error in the potential treatment intensity variables, $\Delta WriteDown_i$ and $\Delta Payment_i$, is correlated with unobserved determinants of future outcomes, ε_{it}. For example, our estimates would be biased upwards if we systematically overestimate the potential treatment intensity of borrowers who are most likely to repay their debts even in the absence of the treatment. Fortunately, we use a nearly identical set of information as the non-profit organization to calculate potential treatment intensity, making it unlikely that there is significant enough measurement error to bias our estimates.

years before the experiment was conducted, and there was no way for them to know which credit card issuers would offer which debt write-downs and minimum payment reductions to the treatment group during the experiment. There is therefore no reason to believe that potential treatment intensity will be correlated with the unobserved benefit of the targeted debt relief. As discussed above, we also see no systematic relationship between the payment reductions and debt write-downs offered during the experiment and the payment reductions and debt write-downs offered prior to the experiment.

To partially test the exclusion restriction, Appendix Table 7 examines the robustness of our results to the inclusion of creditor-specific and demographic-specific treatment effects. Panel A of Appendix Table 7 presents estimates of Equation (4) with additional controls for treatment eligibility interacted with an exhaustive set of credit card issuer indicator variables that are set equal to one if a borrower has nonzero debt with that credit card issuer. Panel B presents estimates that instead add controls for treatment eligibility interacted with indicator variables for gender, race, baseline homeownership, baseline credit scores, baseline employment, and baseline 401k contributions. Panel C presents estimates that include both the treatment eligibility x credit card issuer variables and treatment eligibility x baseline demographic variables. Consistent with our identifying assumption, our main results are generally robust to the inclusion of treatment eligibility x credit card issuer effects, treatment eligibility x baseline demographic effects, and both treatment eligibility x issuer and treatment eligibility x baseline demographic effects. In a series of F-tests of the joint significance of the treatment eligibility x issuer and treatment eligibility x baseline demographic effects, we also find that these interactions are, with two exceptions, not statistically significant. The exceptions are the interactions with baseline 401k and employment measures, which are individually significant in the employment and 401k outcomes regressions, leading to joint significance for those specifications. Taken together, however, we interpret these results as indicating that our exclusion restriction is likely to hold in our setting.

Subgroup Analyses: We are interested in how the effects of the experiment vary across borrower characteristics such as gender, race, and baseline homeownership, credit scores, employment, and savings. However, we are likely to find a number of statistically significant estimates purely by chance when performing multiple hypothesis tests. We were also unable to specify a pre-analysis plan, as the experiment was designed and implemented by MMI, not the research team. In our main analysis, we therefore restrict ourselves to the single subgroup analysis suggested by the experimental design: high and low levels of financial distress just prior to contacting MMI. In the original experimental design, the new debt relief was only going to be offered to the most financially distressed borrowers. Following the experiment, many of the credit card issuers also offered more borrower-friendly terms to the most financially distressed borrowers. To test how the effects of the experiment differ on this dimension, we estimate effects separately for borrowers with below and above median debt-to-income. Results are similar if we split borrowers by debt amount or by the predicted probability of default.

IV. Results

A. Debt Repayment

Table 3 presents estimates of the impact of being offered the debt write-downs and minimum payment reductions on starting and completing a structured repayment program over about the next five years. Panel A reports intent-to-treat estimates of the impact of treatment eligibility. Panel B reports the coefficient on treatment eligibility interacted with the potential percentage point change in the implied interest rate and treatment eligibility interacted with the potential percentage point change in the required minimum payment (multiplied by 100). All specifications control for potential treatment intensity, the baseline controls listed in Table 2, and strata fixed effects. Standard errors are clustered at the counselor level throughout.

Intent-to-Treat Results: There is an economically and statistically significant effect of treatment eligibility on starting and completing the repayment program. Treatment eligibility increased the probability of starting a repayment program by 1.86 percentage points, a 5.84 percent increase from the control mean of 31.85 percent. The probability of finishing a repayment program also increased by 1.31 percentage points, a 9.59 percent increase from the control mean of 13.66 percent. In total, treatment eligibility increased the amount of debt repaid by 1.54 percentage points, a 7.71 percent increase from the control mean of 19.97 percent (see Appendix Table 8).

The effects of treatment eligibility are considerably larger for borrowers with above median baseline debt-to-income, a proxy for financial distress. Treatment eligibility increased the probability of starting a repayment program by 1.42 percentage points more for borrowers with above median debt-to-income compared to borrowers with below median debt-to-income. The probability of finishing the repayment program also increased by 3.08 percentage points more for borrowers with above median debt-to-income. These results suggest that, consistent with the priors of MMI and the credit card issuers, the effects of the more generous debt relief was substantially larger for financially distressed borrowers.

Appendix Tables 9-14 present selected subsample results by gender, ethnicity, baseline homeownership, baseline employment, baseline 401k contributions, and baseline credit scores. For each of these subgroups, there are no clear theoretical predictions as to which group will benefit most from the experiment. We find larger intent-to-treat effects for borrowers with higher baseline credit scores, but similar results by gender, ethnicity, and baseline homeownership, employment, and 401k contributions.

Debt Write-Down Results: Consistent with the intent-to-treat estimates discussed above, we find that the debt write-downs had an economically large impact on repayment rates in the treatment group. The median debt write-down in the treatment group (i.e. a 3.69 percentage point implied interest rate reduction) increased the probability of starting a structured repayment program by 1.88 percentage points, a 5.88 percent increase from the control mean. The probability of finishing the program also increased by 1.62 percentage points, an 11.89 percent increase from the control mean, and the percent of debt repaid increased by 1.81 percentage points, a 9.06 percent increase.

To better understand these treatment effects, Figure 1 plots the actual control mean and the treatment group means implied by estimated treatment effects at each percentile of debt repayment. Specifically, we calculate the treatment group means by adding the reduced form effect of the median debt write-down in the treatment group to the control mean. We therefore calculate treatment group means at each percentile using control group means at each percentile and reduced form estimates from Equation (4), where the dependent variable is an indicator for repaying at least that percent of debt through the repayment program. The shaded region indicates the 95 percent confidence intervals with standard errors clustered by counselor. Figure 1 shows that effect of the debt write-downs also remains roughly constant throughout the repayment program. It is also worth noting 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. In Section V, we will discuss what mechanisms are most consistent with these patterns.

As with the intent-to-treat estimates, the effects of the debt write-downs are driven by borrowers with above median debt-to-income. For these high debt-to-income borrowers, the median write-down in the treatment group increased the probability of starting a repayment program by 2.84 percentage points, an 8.89 percent change, and increased the probability of finishing a repayment program by 2.51 percentage points, a 16.91 percent change. In comparison, there are no statistically significant effects of the debt write-downs on borrowers with below median debt-to-income.

An important question is whether the treatment effects discussed above justify the costs of the write-downs. The average borrower in the control group repays 19.97 percent of his or her debt through the structured repayment program, while the median write-down in the treatment group increases the percent of debt repaid increased by 1.81 percentage points. These results imply that lenders will benefit from offering the debt write-downs so long as repayment rates outside of the DMP are less than about 10-15 percent. Unfortunately, these outside repayment rates are not in our data. Credit card issuers participating in the experiment suggested that the average repayment rate for similar borrowers ranged from 6.5 percent to 14.5 percent during our sample period. If the marginal rates of repayment are below those average rates, this suggests that the write-downs would pass a cost-benefit calculation from the lenders' perspective.

Minimum Payment Results: In sharp contrast to the debt write-down results, we find no effect of the minimum payment reductions on repayment. The point estimates for both starting and completing a repayment program are small and not statistically different from zero, with the 95 percent confidence intervals ruling out treatment effects larger than 0.24 percentage points for starting a repayment program and 0.15 percentage points for completing a repayment program. Figure 1 shows that these results hold over every percentile of repayment. We also find no effect of lower minimum payments for borrowers with above or below median debt-to-income or among any of the other subsample groups we consider in Appendix Tables 9-14.

As discussed above, the null effect of the minimum payment reductions is surprising given a large and influential literature documenting liquidity constraints and present-biased preferences in a number of otherwise similar settings. Our reduced form results suggest that either liquidity constraints are not an important driver of borrower behavior in our data, or that a lower minimum payment is an ineffective way to alleviate these issues, at least in our setting. We return to this issue in Section V.

B. Bankruptcy

Table 4 presents results for bankruptcy filing in the first five years following the experiment, an important outside option for borrowers in our sample. MMI discusses both the costs and benefits of bankruptcy with prospective clients and 10.36 percent of the control group files for bankruptcy in the first five years following the experiment. 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 seven to ten years, potentially decreasing access to new credit (Liberman forthcoming) and new employment opportunities (Bos, Breza, and Liberman 2015, Dobbie et al. 2016). 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 et al. forthcoming). In our setting, we interpret bankruptcy as an alternative and potentially more costly form of debt forgiveness and debt restructuring.

Intent-to-Treat Results: In the pooled sample, treatment eligibility decreased the probability of filing for bankruptcy protection by a statistically insignificant 0.30 percentage points over the first five years following the experiment. Consistent with the repayment results, however, the effects are larger and more statistically significant for borrowers with above median debt-to-income. For these high debt-to-income borrowers, treatment eligibility reduced the probability of filing for bankruptcy by 1.13 percentage points, a 7.98 percent decrease from the control mean. The effects of treatment eligibility on bankruptcy are larger in the third year following the experiment (see Appendix Table 15) and prior to the 2005 Bankruptcy Reform that increased the financial and administrative costs of filing for bankruptcy protection (see Appendix Table 16), although neither difference is statistically significant due to large standard errors.

Debt Write-Down Results: The effects of treatment eligibility on bankruptcy filing are again driven by the debt write-downs. Over the first five years following the experiment, the median write-down in the treatment group decreased the probability of filing for bankruptcy by 0.99 percentage points in the pooled sample, a 9.61 percent decrease from the control mean of 10.36 percent. The decrease in bankruptcy filing is again driven by changes in the second and third post-experiment years for the pooled sample, and the point estimates are again larger prior to the 2005 Bankruptcy Reform. However, neither difference is statistically significant due to large standard errors.

Consistent with the earlier results, we also find larger effects for borrowers with above median debt-to-income levels. The median write-down in the treatment group decreased the probability of filing for bankruptcy by 1.33 percentage points for these high debt-to-income borrowers, a 9.36

percent decrease from the control mean. The effects for borrowers with below median debt-to-income are much smaller, although relatively large standard errors mean that the difference is not statistically significant (p-value = 0.152). The bankruptcy effects are also somewhat larger for female and non-white borrowers, though again neither difference is statistically significant (see Appendix Tables 9-14).

Minimum Payment Results: Over the first five years following the experiment, the median minimum payment reduction in the treatment group actually <u>increased</u> 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. In Appendix Table 15, we show that there are statistically significant increases in the probability of filing in the fifth post-experiment year, suggesting that lower minimum payments may exacerbate financial distress at the end of the repayment program, perhaps due to a longer repayment period.

C. Collections Debt and Credit Score

Table 5 presents results for average collections debt and credit scores over the first five years following the experiment, both important proxies for financial distress and access to credit. In theory, the experiment could either improve borrowers' financial health by increasing debt repayment and decreasing collections activity, or have no impact if the experiment crowds out other debt payments.

Intent-to-Treat Results: In the pooled sample, there are no statistically or economically significant effects of treatment eligibility on collections debt or credit scores. Consistent with our earlier results, however, we find statistically significant results for borrowers with above median debt-to-income. For these high debt-to-income borrowers, treatment eligibility reduced the probability of having nonzero collections debt by 0.76 percentage points, a 2.4 percent decrease from the control mean, and increases credit scores by 3.2 points. Also consistent with our earlier results, the treatment effects are similar by gender, ethnicity, and baseline homeownership, employment, 401k contributions, and credit scores.

Debt Write-Down Results: The effects of treatment eligibility on financial distress are again driven by the debt write-downs. Over the first five years following the experiment, the median write-down in the treatment group decreased the probability of having nonzero collections debt by 1.25 percentage points in the pooled sample, a 3.19 percent decrease from the control mean of 10.36 percent. The median write-down in the treatment group also increases credit scores for borrowers with both below and above median debt-to-income, although neither estimate is statistically significant.

In Appendix Table 8, we show that the median debt write-down in the treatment group also decreases the probability of a serious credit delinquency by 1.29 percentage points, a 2.60 percent decrease, and credit card utilization by 1.43 percentage points, a 3.08 percent decrease. There are no discernible effects of the write-downs on credit card balances or the probability of having an automobile or mortgage loan, however. Taken together with our collections and credit score estimates, these results suggest that the debt write-downs modestly improved borrowers' financial health.

Minimum Payment Results: Following the repayment and bankruptcy results, the median minimum payment reduction in the treatment group <u>increased</u> the probability of having nonzero collections debt by a statistically significant 1.40 percentage points, a 3.56 percent increase from the control mean, with similar point estimates for borrowers with below and above median debt-to-income. There are also modest <u>decreases</u> in credit scores, particularly for high debt-to-income borrowers. Finally, we find modest increases in the probability of a serious credit delinquency and decreases in automobile lending (see Appendix Table 8). These results are all consistent with lower minimum payments having no positive effects and perhaps even exacerbating financial distress.

D. Labor Market Outcomes

Table 6 presents results for average employment and earnings over the first five years following the experiment. The experiment could affect labor market outcomes through a number of different channels. For example, enrollment in the repayment program could increase labor supply by decreasing the frequency of wage garnishment orders that occur when an employer is compelled by a court order to withhold a portion of an employee's earnings to repay delinquent debt. The experiment could also impact labor market outcomes through its effects on credit scores (e.g., Herkenhoff 2013, Bos et al. 2015, Herkenhoff and Phillips 2015, Dobbie et al. 2016) or productivity (e.g., Mullainathan and Shafir 2013).

Intent-to-Treat Results: There are no effects of treatment eligibility on employment or earnings for either the pooled sample or the sample of borrowers with above median debt-to-income. We also find similar (null) effects among all subsamples.

Debt Write-Down Results: The estimated effect of the debt write-downs on employment and earnings is also small and imprecisely estimated in the pooled sample. The 95 percent confidence interval for the employment effect ranges from -0.61 to 0.83 percentage points, while the 95 percent confidence interval for the earnings effect ranges from -$643 to $355. The effect of the write-downs on employment is larger for borrowers with above median debt-to-income, although the effect on earnings remains small and imprecisely estimated. For these high debt-to-income borrowers, the median write-down in the treatment group increased average employment by 1.70 percentage points, a 2.17 percent increase from the control mean.

Consistent with our earlier results, we find similar labor market effects by gender, ethnicity, and homeownership. However, Appendix Table 12 reveals contrasting labor market effects by baseline employment status. In unreported results, the debt write-downs also decreased annual earnings by $2,077 for borrowers who were not employed in the year prior to the experiment, while having essentially no effect on borrowers employed at baseline. The employment effects are also negative for nonemployed borrowers, but the point estimate is not statistically significant. These subsample results suggest that the kind of debt forgiveness provided by the write-downs may decrease labor supply for borrowers most on the margin of any work.

In contrast to the relatively modest labor market effects documented here, Dobbie and Song (2015) find that Chapter 13 bankruptcy protection increases annual earnings by $5,562 and annual employment by 6.8 percentage points. These contrasting results are most likely due to differences in the intensity of the debt relief provided by consumer bankruptcy and our experiment. Chapter 13 bankruptcy, for example, provides a write-down of approximately 80 to 85 percent of the typical filer's unsecured debt. Conversely, the median write-down in the treatment group forgives about 9.63 percent of unsecured debt. In addition, Chapter 13 bankruptcy protects future wages from garnishment, while our experiment did not.

Minimum Payment Results: The estimated effect of the minimum payment reductions on labor market outcomes is small and relatively imprecisely estimated across all borrowers, including those who were not employed at baseline. In the pooled sample, the 95 percent confidence interval for the employment effect ranges from -1.38 to 0.26 percentage points, and from -$437 to $566 for the earnings effect. None of the estimates suggest economically meaningful effects of a lower minimum payment on labor market outcomes.

E. 401k Contributions

Table 7 presents results for average 401k contributions, a proxy for savings, over the first five years following the experiment. In theory, the experiment could either crowd out savings by increasing the returns of debt repayment, or increase savings by decreasing financial distress and increasing employment and earnings.

Intent-to-Treat Results: There are no effects of treatment eligibility on 401k contributions in either the pooled sample or the sample of borrowers with above median debt-to-income. We also find similar (null) effects by gender, ethnicity, and baseline homeownership credit scores.

Debt Write-Down Results: Consistent with the intent-to-treat estimates, the estimated effect of the debt write-downs on 401k contributions is small and imprecisely estimated in the pooled sample, with the 95 percent confidence interval ranging from -$49.20 to $10.09 for the median write-down in the treatment group. We find similar (null) effects by baseline financial distress, gender, ethnicity, and homeownership. Consistent with our labor market results, however, we find in unreported results that the write-downs decreased 401k contributions by $60.14 for nonemployed borrowers. We also find similar results for borrowers with zero 401k contributions at baseline. These results suggest that the debt forgiveness provided by the debt write-downs may decrease savings for borrowers most on the margin of work, and hence most on the margin of contributing to a 401k.

Minimum Payment Results: The estimated effect of the minimum payment reductions on 401k contributions is statistically zero in both the pooled and subsample results, with the 95 percent confidence interval ranging from -$12.04 to $42.84 for the median minimum payment reduction for the pooled sample.

F. Robustness Checks

We have run regressions with a number of outcomes and subsamples. The problem of multiplicity can lead one to incorrectly reject some null hypothesis purely by chance. To test the robustness of our results, we calculate an alternative set of p-values for our full sample results using a non-parametric permutation test. Specifically, we create 1,000 "placebo" samples where we randomly re-assign treatment status to individuals within the randomization strata. We then calculate the fraction of treatment effects from these 1,000 placebo samples that are larger (in absolute value) than the treatment effects from the true sample.

Appendix Table 17 presents p-values from this non-parametric permutation test. We find that our main results are robust to this alternative method of calculating standard errors. If anything, we obtain smaller p-values from the non-parametric permutation procedure than implied by conventional standard errors. Results are similar for borrowers with above median debt-to-income, our preferred subsample split.

V. Mechanisms

In this section, we consider the potential mechanisms that can explain our debt write-down and minimum payment results using the economic model developed above.

A. Overview

In theory, the debt write-downs can impact repayment through a forward-looking effect on debt overhang and a mechanical exposure effect. An important implication of our model is that we can use treatment effects at the beginning and end of the repayment program to test the relative importance of these competing channels. Specifically, the model implies that we can test for forward-looking effects using debt write-down treatment effects early in the repayment program when both the debt write-down and control groups are still making payments. This is because the debt write-downs do not affect the minimum payment requirements early in the repayment program, leaving forward-looking behavior as the only explanation for any debt write-down effects early in the program. Then, because the total debt write-down estimate includes the effects of both channels, we can estimate the exposure effect alone using the difference between the total debt write-down estimate and the forward-looking estimate.[13]

[13]Our approach is similar to the one used by Schmieder, von Wachter, and Bender (2016) to estimate the effect of nonemployment durations on wage offers, with one important exception. Nonemployment durations must be estimated relative to some intermediate time period $t > 0$, making it possible for differential selection into the sample to bias their estimates. In contrast, we are primarily interested in the forward-looking and liquidity effects of the experiment, both of which are measured relative to $t = 0$. Because we include all individuals, including both those that never enroll in a repayment program and those who enroll but later drop out, our estimates of these effects are contaminated by dynamic selection over time. Dynamic selection can, however, bias our estimates of the exposure

Formally, we test for forward-looking behavior using an estimate of repayment at P^{WD}, or the end of the repayment program for the debt write-down group (but not the control group). Again, this is a valid test of forward-looking behavior because the debt write-down and control groups have identical minimum payments for $t \leq P^{WD}$, and therefore have identical exposure to non-strategic liquidity risk during this time period. As a result, the debt write-down estimate at P^{WD} is driven by forward-looking behavior alone. The estimate at P^{WD} is likely a lower bound of the forward-looking effect, however, as the control group may make forward-looking default decisions for $P^{WD} < t \leq P^{C}$, i.e. the end of the repayment program for the control group. As a result, our estimate of the mechanical exposure effect based on the difference between the total debt write-down effect at P^{C} and the forward-looking effect at P^{WD} is likely an upper bound of the true exposure effect.

Now consider the minimum payment reductions, which can impact repayment through a liquidity effect and a similar (but oppositely signed) mechanical exposure effect. Following a similar logic, the model implies that we can test for liquidity effects using payment reduction treatment effects at P^{C}, or the end of the repayment program for the control group (but not the payment reduction group). For $t \leq P^{C}$, both the control and treatment groups are enrolled in the repayment program, but the treatment group has lower minimum payments and, as a result, increased liquidity. The payment reduction estimate at P^{C} is therefore driven by the liquidity effect alone. The payment reduction estimate at P^{C} likely measures an upper bound of the true liquidity effect, however, as the treatment group can still make forward-looking default decisions for $P^{C} < t \leq P^{MP}$. Thus, as with the debt write-downs, our estimate of the mechanical exposure effect based on the difference between the total payment reduction effect at P^{MP} and the liquidity effect at P^{C} likely reflects a lower bound of the true exposure effect. The appendix provides additional discussion of both the debt write-down and payment reduction results.

B. Empirical Implementation

We implement these empirical tests 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 debt write-downs and longer for individuals with relatively larger minimum payment reductions. For example, individuals with the largest write-downs have treatment plans that are up to 20 percent shorter than their control

effect because we are comparing treatment effects at different points in time. For example, it is plausible that the debt write-downs or minimum payment reductions will induce relatively more distressed borrowers to repay their debts, leading less distressed borrowers to drop out of the repayment program earlier on. This type of selection might lead to a different composition of treated and control borrowers later in the repayment program. In this scenario, our estimate of the exposure effect will be biased downwards. To shed some light on this issue, Appendix Table 18 compares the characteristics of control and treatment borrowers completing the repayment program. Only one of the 28 baseline differences is statistically significant at the ten percent level and the p-value from an F-test of the joint significance of all of the variables listed is 0.976, suggesting that the experiment did not significantly alter the composition of borrowers completing the repayment program. Given these results, it appears unlikely that our estimates of the exposure effect will be significantly biased by dynamic selection.

plans, while individuals with the smallest write-downs and largest minimum payment reductions 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 until the minimum of the treatment plan length and the control plan length. This indicator variable measures payment at P^{WD} for individuals with the shorter treatment plans (i.e. relatively larger write-downs) and payment at P^{C} for individuals with the longer treatment plans (i.e. relatively larger minimum payment reductions). Third, we estimate Equation (4) using this new indicator variable. These reduced form estimates measure the effect of write-downs at P^{WD} 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 3 and the new reduced form treatment effects estimated at the shorter of P^{WD} 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.

C. Results

Table 8 presents estimates of the forward-looking, liquidity, and exposure effects for both the debt write-downs and minimum payment reductions. Column 1 replicates our estimates from column 4 of Table 3, showing the net effect of all channels on completing the repayment program. Columns 2-3 report estimates for still being 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.

Debt Write-Down Results: We find that the effect of the debt write-downs on repayment is almost entirely explained by forward-looking decisions made early in the repayment program, not the mechanical reduction in default risk from a shorter repayment program. The estimates from Table 8 suggest that at least 85.1 percent of the debt write-down effect is due to the decrease in forward-looking defaults at the beginning of the repayment program. Decreased exposure to risk at the end of repayment can explain a maximum of 14.9 percent of the write-down effect, with the 95 percent confidence interval including estimates of up to 38.2 percent of the total reduced form effect.

Additional evidence in favor of forward-looking effects comes from Figure 1, which plots treatment effects at every point in the distribution. Two patterns emerge from these results. First, there is an immediate impact of the debt write-downs on repayment, indicating forward-looking behavior at program sign up. Second, the effects of the debt write-downs, if anything, grow over time relative to the control mean. These results suggest additional forward-looking behavior throughout the repayment program, not just at program sign up. Taken at face value, these two findings rule out many of the most simple "behavioral" explanations for our debt write-down results, such as borrowers being "tricked" into signing up for the repayment program by some feature of the experimental design.

Minimum Payment Results: We find that while liquidity constraints are a somewhat important driver of default in our sample, the positive short-run effect of increasing liquidity is offset by the unintended, negative effect of the longer repayment period. The minimum payment reductions have a small positive impact of about 0.03 percentage points on repayment through the liquidity effect, with the 95 percent confidence interval including effects as large as 0.16 percentage points. In all specifications, however, any positive liquidity effect is nearly exactly offset by the negative exposure effect. These estimates are also consistent with the patterns observed in Figure 1, where we see a small positive effect of the minimum payment reductions in the short run, and a small negative effect of the payment reductions in the long run.

These results help to reconcile our findings the vast literature documenting liquidity constraints in a variety of settings, while indicating that the potential benefits of targeting these liquidity constraints may have been significantly overstated, at least in our setting. Of course, the standard caveat applies that the effects of an increase in liquidity may be non-linear or context dependent. For example, it is possible the short-run benefits from a very large increase in liquidity may outweigh the long-run costs of a much longer repayment period. It is also possible that liquidity may be more important in the mortgage or student loan markets, where borrowers usually have fewer outside options compared to the credit card borrowers that we study in this paper.

VI. Conclusion

This paper uses information from a large-scale randomized experiment to estimate the effects of immediate minimum payment reductions targeting short-run liquidity constraints and delayed debt write-downs targeting longer-run debt overhang. We find that the debt write-downs significantly improved both financial and labor market outcomes, particularly for the highest-debt borrowers, despite not taking effect for three to five years. In contrast, we find no positive effects of the more immediate payment reductions on any outcome. These results stand in stark contrast to the widespread view that short-run liquidity constraints are the most important driver of borrower distress.

Our results are of particular importance in light of the ongoing debate on the relative merits of different types of debt relief. For example, current banking regulations in the United States prevent credit card issuers from offering more generous debt write-downs, at least in part due to the perceived unimportance of longer-run constraints such as debt overhang.[14,15] During the financial crisis, a group of credit card issuers asked for these regulations to be relaxed so that

[14]Specifically, U.S. banking regulations prevent credit card issuers from simultaneously reducing the original principal and lengthening the repayment period unless a debt is first classified as impaired. If the original principal is reduced without the debt being classified as impaired, borrowers are required to pay off the remaining debt in just a few months. Government regulators justify these restrictions based on concerns about when delinquent debts would be recognized on the card issuers' balance sheets.

[15]There was an analogous debate regarding targeted debt relief for mortgage borrowers during the financial crisis. For example, former Treasury Secretary Timothy Geithner wrote in his memoir that the government's "biggest debate [during the financial crisis] was whether to try to reduce overall mortgage loans or just monthly payments." See also `https://www.washingtonpost.com/business/economy/economists-obama-administration-at-odds-over-role-of-mortgage-debt-in-slow-recovery/2012/11/22/dc83f25e-2e87-11e2-89d4-040c9330702a_story.html`

they could conduct a pilot program forgiving up to 40 percent of a credit card borrower's original principal (while restructuring the remaining principal to be repaid over a number of years and deferring any income taxes owed on the forgiven principal). Our results suggest that there may be substantial benefits of considering such pilot programs.

An important limitation of our analysis is that we are not able to estimate the impact of targeted debt relief on ex-ante borrower behavior or ex-ante borrowing costs. There may also be important ex-post impacts of targeted debt relief on outcomes such as post-repayment interest rates that we are unable to measure with our data. Finally, we are unable to test whether the forward-looking decisions documented in this paper are due to rational or non-rational decision making. These questions remain important areas for future research.

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Table 1: Examples of the Randomized Treatments

Treatments		Program Characteristics			Discounted Cost to Lender		
Debt Write-Down	Payment Reduction	Minimum Payment	Financing Fees	Total Months	0% Disc. Rate	8.5% Disc. Rate	20% Disc. Rate
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
–	–	\$433.45	\$3,482	50.05	–	–	–
Δ3.69%	–	\$433.45	\$1,770	46.10	\$1,221	\$802	\$440
–	Δ0.14%	\$406.77	\$3,771	54.04	\$159	\$332	\$444

Notes: This table describes the effect of treatment eligibility on repayment program attributes and lender costs. Minimum payment is the minimum required payment of the program. Financing fees include the total cost of all interest rate payments and late fees. Total months is the total number of months before the program is complete. All program characteristics and lender costs are calculated using the control means for debt (\$18,212), minimum payment (2.38% of debt), and interest rate (8.5%). The net present value (NPV) of lender costs (relative to the baseline case) using the control mean for the monthly default rate during the repayment program (1.12%) and using discount rates of 0%, 8.5%, and 20%. The first row reports program characteristics for the baseline case in the control group. The second row reports program characteristics after the 50th percentile debt write-down in the treatment group in terms of the implied interest rate cut. The third row reports program characteristics after the 50th percentile minimum payment reduction in the treatment group in terms of the percentage point decrease in the required payment.

Table 2: 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
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 Unsecured Debt	18.212	18.368	0.299
Debt with Part. Creditors	9.568	9.615	0.163
Internal Risk Score	-0.000	-0.003	−0.003
Panel B: Baseline Outcomes			
Bankruptcy	0.004	0.003	−0.001
Nonzero Collections Debt	0.253	0.254	−0.001
Credit Score	585.661	584.991	0.182
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: Data Quality			
Matched to SSA data	0.953	0.954	0.003
Matched to TU Data	0.899	0.895	−0.001
Panel D: Potential Treatment Intensity			
Interest Rate if Control	0.085	0.084	0.001
Interest Rate if Treatment	0.059	0.060	−0.001
Min. Payment Percent if Control	0.024	0.024	−0.001
Min. Payment Percent if Treatment	0.023	0.023	−0.000
Program Length in Months if Control	52.671	52.678	0.058
Program Length in Months if Treatment	51.963	51.914	0.036
Panel E: Characteristics of Repayment Program			
Interest Rate	0.085	0.059	−0.026***
Min. Payment Percent	0.024	0.023	−0.001***
Program Length in Months	52.671	51.914	−0.806***
p-value from joint F-test of Panels A-D	–	–	0.991
p-value from joint F-test of Panel E	–	–	0.000
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 and information on collections debt and credit score are only available for individuals matched to the TU data. Each baseline outcome is for the year before the experiment. Potential minimum payment and interest rates are calculated using the amount of debt held by each creditor and the rules listed in Appendix Table 1. 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 3: Debt Relief and Repayment

	Start Payment			Complete Payment		
	Full Sample	Low Debt/Inc.	High Debt/Inc.	Full Sample	Low Debt/Inc.	High Debt/Inc.
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Eligibility	0.0186***	0.0115	0.0257***	0.0131***	−0.0022	0.0286***
	(0.0058)	(0.0074)	(0.0074)	(0.0044)	(0.0059)	(0.0056)
Panel B: Debt Write-Down and Minimum Payment Estimates						
Debt Write-Down	0.0051**	0.0022	0.0077***	0.0044**	0.0020	0.0068***
	(0.0023)	(0.0029)	(0.0027)	(0.0017)	(0.0023)	(0.0023)
Min. Payment Reduction	0.0004	0.0008	0.0001	0.0001	0.0006	−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 relief 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. Panel A reports the coefficient on treatment eligibility. Panel B reports coefficients on the interaction of treatment eligibility and potential debt write-down (in terms of the interest rate in percentage points), and the interaction of treatment eligibility and potential minimum payment reduction (in percentage points x 100). All specifications control for potential debt write-down, potential minimum payment reduction, the baseline controls in Table 2, 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 2 notes for details on the baseline controls and sample.

Table 4: Debt Relief and Bankruptcy Filing

	Bankruptcy in Years 1-5		
	Full Sample	Low Debt/Inc.	High Debt/Inc.
Panel A: ITT Estimates	(1)	(2)	(3)
Treatment Eligibility	−0.0030	0.0054	−0.0113***
	(0.0035)	(0.0050)	(0.0040)
Panel B: Debt Write-Down and Minimum Payment Estimates			
Debt Write-Down	−0.0027**	−0.0016	−0.0036**
	(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 relief 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. Panel A reports the coefficient on treatment eligibility. Panel B reports coefficients on the interaction of treatment eligibility and potential debt write-down (in terms of the interest rate in percentage points), and the interaction of treatment eligibility and potential minimum payment reduction (in percentage points x 100). All specifications control for potential debt write-down, potential minimum payment reduction, the baseline controls in Table 2, 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 2 notes for details on the baseline controls and sample.

Table 5: Debt Relief and Financial Outcomes

	Nonzero Collections Debt			Credit Score		
	Full Sample	Low Debt/Inc.	High Debt/Inc.	Full Sample	Low Debt/Inc.	High Debt/Inc.
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Eligibility	0.0007	0.0091	−0.0076*	−0.2195	−0.7046	3.2585***
	(0.0046)	(0.0057)	(0.0044)	(0.7537)	(1.0352)	(0.9855)
Panel B: Debt Write-Down and Minimum Payment						
Debt Write-Down	−0.0034*	−0.0047**	−0.0023	0.5278	0.6617	0.4602
	(0.0019)	(0.0024)	(0.0023)	(0.3422)	(0.4452)	(0.4063)
Min. Payment Reduction	0.0010**	0.0010*	0.0009*	−0.1270	−0.0297*	−0.2042**
	(0.0005)	(0.0006)	(0.0005)	(0.0862)	(0.1151)	(0.0949)
Observations	71,516	35,354	36,162	71,516	35,354	36,162
Mean in Control Group	0.3929	0.4659	0.3213	603.0766	586.9836	618.9447

Notes: This table reports reduced form estimates of the impact of debt relief on having any collections debt and credit score. Information on outcomes comes from credit records at TransUnion. 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. Panel A reports the coefficient on treatment eligibility. Panel B reports coefficients on the interaction of treatment eligibility and potential debt write-down (in terms of the interest rate in percentage points), and the interaction of treatment eligibility and potential minimum payment reduction (in percentage points x 100). All specifications control for potential debt write-down, potential minimum payment reduction, the baseline controls in Table 2, 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 2 notes for details on the baseline controls and sample.

Table 6: Debt Relief and Labor Market Outcomes

	Employment			Earnings		
	Full Sample	Low Debt/Inc.	High Debt/Inc.	Full Sample	Low Debt/Inc.	High Debt/Inc.
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Eligibility	−0.0018	−0.0009	−0.0028	0.0567	−0.0799	0.1942
	(0.0026)	(0.0034)	(0.0035)	(0.1746)	(0.2245)	(0.2191)
Panel B: Debt Write-Down and Minimum Payment Estimates						
Debt Write-Down	0.0003	0.0010	0.0045**	−0.0390	0.0264	−0.0959
	(0.0010)	(0.0013)	(0.0021)	(0.0691)	(0.0983)	(0.0774)
Min. Payment Reduction	−0.0004	−0.0006*	−0.0001	0.0046	0.0004	0.0094
	(0.0003)	(0.0003)	(0.0003)	(0.0183)	(0.0243)	(0.0215)
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 relief 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. Panel A reports the coefficient on treatment eligibility. Panel B reports coefficients on the interaction of treatment eligibility and potential debt write-down (in terms of the interest rate in percentage points), and the interaction of treatment eligibility and potential minimum payment reduction (in percentage points x 100). All specifications control for potential debt write-down, potential minimum payment reduction, the baseline controls in Table 2, 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 2 notes for details on the baseline controls and sample.

Table 7: Debt Relief and 401k Contributions

	Nonzero 401k Contributions			401k Contributions		
	Full Sample	Low Debt/Inc.	High Debt/Inc.	Full Sample	Low Debt/Inc.	High Debt/Inc.
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Eligibility	0.0012	0.0003	0.0020	0.0010	−0.0098	0.0118
	(0.0041)	(0.0058)	(0.0051)	(0.0101)	(0.0131)	(0.0117)
Panel B: Debt Write-Down and Minimum Payment						
Debt Write-Down	0.0003	0.0006	0.0001	−0.0053	−0.0067	−0.0038
	(0.0017)	(0.0022)	(0.0023)	(0.0041)	(0.0056)	(0.0051)
Min. Payment Reduction	−0.0001	−0.0001	−0.0001	0.0011	0.0017	0.0006
	(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

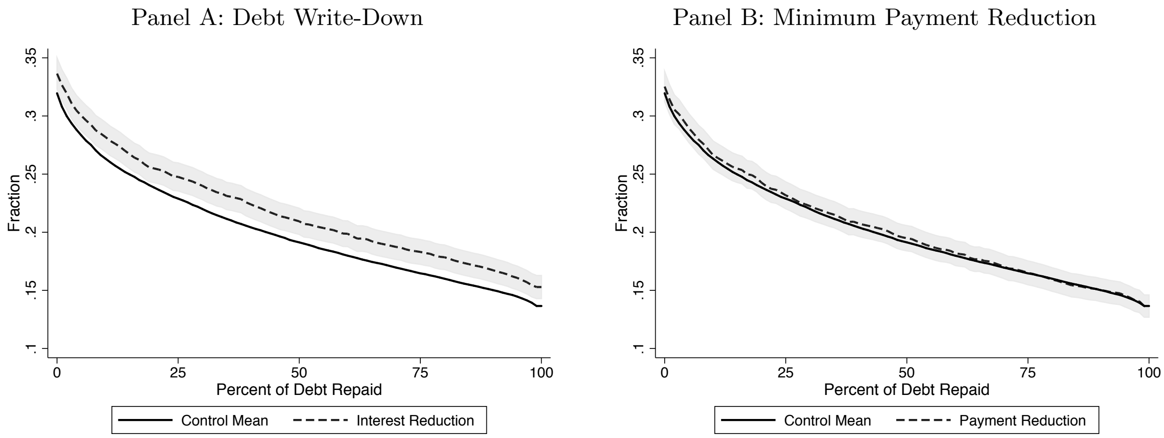
Notes: This table reports reduced form estimates of the impact of debt relief on 401k contributions. Information on all outcomes comes from records at the Social Security Administration. Columns 1 and 4 report results for the full sample of borrowers. Columns 2-3 and 5-6 report results for borrowers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

Table 8: Forward-Looking, Liquidity, and Exposure Effects

	Total Effect	Forward Looking	Liquidity Effect	Exposure Effect
	(1)	(2)	(3)	(4)
Debt Write-Down	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 forward-looking, liquidity, and 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. All specifications control for potential debt write-down, potential minimum payment reduction, the baseline controls in Table 2, and strata fixed effects. 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 Relief and Repayment Rates



Notes: These figures report the control 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 3. The shaded regions indicate the 95 percent confidence intervals. All specifications control for the potential minimum payment and write-down changes if treated and cluster standard errors at the counselor level. See the Table 3 notes for additional details on the sample and specification.

Appendix Table 1: 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 2: Results with No Baseline Controls

	Complete Payment	Bankrupt	Collections Debt	Credit Score	Employed	Nonzero 401k
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Eligibility	0.0134***	−0.0021	−0.0002	−0.0663	0.0023	0.0002
	(0.0047)	(0.0036)	(0.0054)	(0.9428)	(0.0039)	(0.0048)
Panel B: Debt Write-Down and Minimum Payment						
Debt Write-Down	0.0049***	−0.0024*	−0.0035*	0.5911	0.0022	0.0004
	(0.0018)	(0.0014)	(0.0021)	(0.3707)	(0.0017)	(0.0022)
Min. Payment Reduction	−0.0001	0.0005	0.0008*	−0.0985	−0.0007	−0.0002
	(0.0005)	(0.0003)	(0.0005)	(0.0951)	(0.0005)	(0.0005)
Observations	79,739	79,739	71,516	71,516	76,008	76,008
Mean in Control Group	0.1366	0.1036	0.3929	603.0766	0.8202	0.2723

Notes: This table reports reduced form estimates with no baseline controls. Panel A reports the coefficient on treatment eligibility. Panel B reports coefficients on the interaction of treatment eligibility and potential debt write-down (in terms of the interest rate in percentage points), and the interaction of treatment eligibility and potential minimum payment reduction (in percentage points x 100). All specifications control for potential debt write-down, potential monthly minimum 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 2 notes for details on the sample.

Appendix Table 3: Non-Parametric Results

	Complete Payment	Bankrupt	Collections Debt	Credit Score	Employed	Nonzero 401k
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment x No Reductions	−0.0027	0.0030	0.0063	−1.0877	−0.0015	0.0002
	(0.0064)	(0.0054)	(0.0072)	(1.2538)	(0.0049)	(0.0074)
Treatment x Low Write-Down x Low Payment	0.0208**	−0.0076	−0.0096	1.2971	0.0057	0.0054
	(0.0090)	(0.0075)	(0.0084)	(1.4916)	(0.0050)	(0.0079)
Treatment x Low Write-Down x High Payment	0.0282**	0.0015	−0.0152	2.1956	−0.0006	0.0050
	(0.0124)	(0.0105)	(0.0125)	(2.0919)	(0.0068)	(0.0117)
Treatment x High Write-Down x Low Payment	0.0156	0.0065	0.0233	−0.7674	−0.0004	−0.0082
	(0.0171)	(0.0142)	(0.0152)	(1.8131)	(0.0114)	(0.0159)
Treatment x High Write-Down x High Payment	0.0254**	−0.0131*	0.0085	5.5177*	−0.0136**	−0.0020
	(0.0105)	(0.0078)	(0.0106)	(3.2213)	(0.0063)	(0.0106)
Observations	79,739	79,739	71,516	71,516	76,008	76,008
Mean in Control Group	0.1366	0.1036	0.3929	603.0766	0.8202	0.2723

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 2, 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 4: Descriptive Statistics by Treatment Intensity

	Zero Write-Down x Zero Payment		Low Write-Down x Low Payment		Low Write-Down x High Payment		High Write-Down x Low Payment		High Write-Down x High Payment	
	Control	Diff.	Control	Diff.	Control	Diff.	Control	Diff.	Control	Diff.
Panel A: Characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age	38.481	−0.735	41.695	0.010	40.624	−0.724	44.310	0.307	41.477	0.227
Male	0.357	0.017	0.357	0.001	0.366	0.032	0.357	0.024	0.380	0.012
White	0.572	−0.014	0.648	0.010	0.652	0.005	0.727	0.084	0.687	0.008
Black	0.237	−0.006	0.152	−0.006	0.141	−0.018	0.120	−0.029	0.124	−0.022
Hispanic	0.096	0.014	0.093	0.002	0.098	−0.009	0.068	−0.017	0.081	−0.003
Homeowner	0.316	−0.008	0.458	0.002	0.474	−0.023	0.508	−0.018	0.443	−0.003
Renter	0.511	0.006	0.408	−0.018	0.390	0.015	0.366	0.021	0.417	0.017
Dependents	2.155	−0.008	2.227	−0.004	2.199	−0.014	2.160	−0.094	2.057	−0.034
Monthly Income	2.092	0.019	2.648	−0.014	2.677	0.026	2.695	0.019	2.585	0.052
Monthly Expenses	1.897	0.008	2.324	−0.037	2.337	0.002	2.373	0.099	2.245	0.031
Total Unsecured Debt	10.852	−0.187	21.832	0.617	21.721	1.012	26.354	0.153	20.782	0.990
Debt with Part. Creditors	2.634	−0.123	8.684	0.155	14.979	0.580	15.906	−0.442	16.602	0.869
Internal Risk Score	0.079	−0.026	0.187	0.027	-0.072	0.076	-0.219	0.149	-0.244	−0.043
Panel B: Baseline Outcomes										
Bankruptcy	0.007	−0.002	0.003	0.001	0.002	0.001	0.000	0.003	0.002	0.001
Credit Score	566.353	−0.612	582.373	1.868	596.595	−3.179	609.809	6.538	605.821	3.457
Nonzero Collections Debt	0.402	0.020	0.221	−0.001	0.176	0.032	0.127	0.053	0.142	−0.037
Employment	0.848	0.012	0.852	0.002	0.870	0.021	0.821	−0.031	0.839	−0.010
Earnings	20.398	−0.194	25.097	−0.467	26.006	1.252	24.382	0.055	24.534	0.753
Nonzero 401k Cont.	0.195	−0.005	0.243	−0.007	0.257	0.001	0.253	−0.024	0.234	0.005
401k Contributions	0.271	−0.012	0.400	−0.006	0.452	−0.063	0.460	−0.057	0.422	0.021
Panel C: Data Quality										
Matched to SSA data	0.953	−0.002	0.949	0.003	0.952	−0.005	0.962	0.013	0.953	−0.001
Matched to TU Data	0.889	−0.019**	0.902	0.008	0.901	0.001	0.913	0.036	0.903	−0.011
Panel D: Potential Treatment Intensity										
Interest Rate if Control	0.080	0.000	0.086	0.001	0.093	0.000	0.080	0.000	0.089	0.000
Interest Rate if Treatment	0.080	0.000	0.068	0.000	0.034	0.002	0.055	0.000	0.027	0.000
Min. Payment if Control	0.028	−0.000	0.025	−0.000	0.025	−0.000	0.025	−0.000	0.025	−0.000
Min. Payment if Treatment	0.028	−0.000	0.025	−0.000	0.024	−0.000	0.022	−0.000	0.022	−0.000
Program Length if Control	51.379	0.081	52.682	0.012	54.894	0.413	51.750	0.335	53.785	0.199
Program Length if Treatment	51.463	0.089	51.633	−0.029	49.136	0.449	55.386	0.352	53.644	0.109

Panel E: Characteristics of Repayment Program										
Interest Rate	0.080	0.000	0.086	−0.018***	0.093	−0.057***	0.080	−0.024***	0.089	−0.062***
Min. Payment Percent	0.028	−0.000	0.025	−0.001***	0.025	−0.001**	0.025	−0.003***	0.025	−0.003***
Program Length in Months	51.379	0.152	52.682	−1.154***	54.894	−5.347***	51.750	4.014***	53.785	−0.146
joint F-test of Panels A-D	–	0.998	–	0.999	–	1.000	–	0.988	–	0.999
joint F-test of Panel E	–	0.847	–	0.000	–	0.000	–	0.000	–	0.000
Observations	13,701	26,992	10,545	20,677	2,926	5,676	4,900	9,870	8,424	16,524

Notes: This table reports summary statistics by potential treatment intensity. See the Table 2 notes for details on the variable definitions.

Appendix Table 5: Additional Tests of Random Assignment

	Control Mean	Treated x Δ Rate	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)	
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 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)	
Internal Risk Score	-0.0000	0.0010	−0.0007	0.8118
	(1.0000)	(0.0051)	(0.0012)	
Collections Debt	0.2529	0.0022	−0.0009	0.1658
	(0.4347)	(0.0024)	(0.0007)	
Credit Score	585.6605	−0.0588	0.0588	0.7114
	(69.8287)	(0.3795)	(0.0993)	
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)	
Matched to TU Data	0.8985	0.0002	0.0000	0.9747
	(0.3019)	(0.0015)	(0.0004)	
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 2 notes for additional details on the sample and variable construction.

Appendix Table 6: Correlates of Potential Treatment Intensity

	Control Mean	Δ Rate	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)	
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 Unsecured Debt	18.2120	0.7264***	0.0850***	0.0000
	(16.9388)	(0.0561)	(0.0133)	
Debt with Part. Creditors	9.5679	1.4554***	0.1580***	0.0000
	(12.6572)	(0.0393)	(0.0103)	
Internal Risk Score	-0.0000	−0.0242***	−0.0090***	0.0000
	(1.0000)	(0.0030)	(0.0006)	
Collections Debt	0.2529	−0.0231***	−0.0030***	0.0000
	(0.4347)	(0.0014)	(0.0003)	
Credit Score	585.6605	2.6416***	0.6141***	0.0000
	(69.8287)	(0.2334)	(0.0528)	
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)	
Matched to TU Data	0.8985	0.0005	0.0005***	0.0001
	(0.3019)	(0.0009)	(0.0002)	
Observations	40,496	79,739		

Notes: This table describes correlates of potential treatment intensity. The dependent variable for column 2 is the potential change in interest rates. The dependent variable for column 3 is the potential change in minimum 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.

Appendix Table 7: Over-Identification Tests

	Complete Payment	Bankrupt	Collections Debt	Credit Score	Employed	Nonzero 401k
Panel A: Baseline Results	(1)	(2)	(3)	(4)	(5)	(6)
Debt Write-Down	0.0044**	−0.0027*	−0.0034*	0.5278	0.0003	0.0003
	(0.0017)	(0.0014)	(0.0019)	(0.3422)	(0.0010)	(0.0017)
Min. Payment Reduction	−0.0000	0.0004	0.0010**	−0.1270	−0.0004	−0.0001
	(0.0005)	(0.0003)	(0.0005)	(0.0862)	(0.0003)	(0.0004)
Panel B: Results with Treatment x Creditor Effects						
Debt Write-Down	0.0030*	−0.0026*	−0.0019	0.4565	−0.0015	−0.0009
	(0.0018)	(0.0021)	(0.0027)	(0.4723)	(0.0015)	(0.0024)
Min. Payment Reduction	0.0003	0.0002	0.0009	−0.0644	−0.0002	−0.0002
	(0.0006)	(0.0005)	(0.0006)	(0.1005)	(0.0003)	(0.0005)
Panel C: Results with Treatment x Demographic Effects						
Debt Write-Down	0.0039**	−0.0025*	−0.0037**	0.6943*	0.0001	−0.0008
	(0.0018)	(0.0015)	(0.0018)	(0.3725)	(0.0012)	(0.0019)
Min. Payment Reduction	−0.0000	0.0004	0.0010**	−0.1249	−0.0004	−0.0000
	(0.0005)	(0.0003)	(0.0005)	(0.0879)	(0.0003)	(0.0004)
Panel D: Results with Treatment x Creditor and Treatment x Demographic Effects						
Debt Write-Down	0.0028*	−0.0023	−0.0019	0.4595	−0.0012	−0.0012
	(0.0017)	(0.0021)	(0.0026)	(0.4779)	(0.0015)	(0.0024)
Min. Payment Reduction	0.0003	0.0002	0.0009	−0.0684	−0.0002	−0.0002
	(0.0006)	(0.0005)	(0.0006)	(0.1005)	(0.0003)	(0.0005)
p-value from joint F-test in Panel B	0.399	0.983	0.775	0.217	0.940	0.847
p-value from joint F-test in Panel C	0.466	0.638	0.872	0.793	0.101	0.000
p-value from joint F-test in Panel D	0.247	0.814	0.918	0.406	0.312	0.000
Observations	79,739	79,739	71,516	71,516	76,008	76,008
Mean in Control Group	0.1366	0.1036	0.3929	603.0766	0.8202	0.2723

Notes: This table reports over-identification tests. Panel A reports the baseline results from Tables 3-6. Panel B adds treatment x credit card issuer fixed effects. Panel C adds treatment x demographic fixed effects for gender, race, homeownership, credit score, employment, and 401k contributions. Panel D adds both treatment x credit card issuer and treatment x demographic fixed effects. We also report the p-value from an F-test that all of the indicated interactions are jointly equal to zero. All specifications control for treatment eligibility interacted with the potential debt write-down, treatment eligibility interacted with the potential minimum payment reduction, potential debt write-down, potential minimum payment reduction, the baseline controls listed in Table 2, 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 2 notes for details on the baseline controls and sample.

Appendix Table 8: Results for Additional Outcomes

	Percent Repaid	Serious Default	Card Balance	Card Util.	Any Auto	Any Mortgage
Panel A: ITT Results	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Eligibility	0.0154***	0.0010	−0.1244	−0.9866**	0.0008	−0.0012
	(0.0045)	(0.0046)	(0.1437)	(0.4651)	(0.0050)	(0.0043)
Panel B: Debt Write-Down and Minimum Payment						
Debt Write-Down	0.0049***	−0.0035*	−0.0169	−0.3881**	0.0031	0.0010
	(0.0019)	(0.0020)	(0.0596)	(0.1797)	(0.0022)	(0.0020)
Min. Payment Reduction	0.0002	0.0009**	0.0091	0.0636	−0.0011**	−0.0004
	(0.0005)	(0.0005)	(0.0162)	(0.0446)	(0.0006)	(0.0005)
Observations	79,739	71,516	71,516	71,516	71,516	71,516
Mean in Control Group	0.1997	0.4797	8.3507	46.2858	0.3945	0.3059

Notes: This table reports reduced form estimates for additional outcomes. Panel A reports the coefficient on treatment eligibility. Panel B reports coefficients on the interaction of treatment eligibility and potential debt write-down (in terms of the interest rate in percentage points), and the interaction of treatment eligibility and potential minimum payment reduction (in percentage points x 100). All specifications control for potential debt write-down, potential monthly minimum 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 2 notes for details on the sample.

Appendix Table 9: Results by Gender

	Complete Payment	Bankrupt	Collections Debt	Credit Score	Employed	Nonzero 401k
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
(1) Treatment x Male	0.0103	−0.0052	0.0072	−0.1729	−0.0057	0.0011
	(0.0072)	(0.0067)	(0.0076)	(1.3281)	(0.0043)	(0.0073)
(2) Treatment x Female	0.0144**	−0.0007	−0.0019	−0.1977	0.0005	0.0012
	(0.0056)	(0.0047)	(0.0064)	(1.0537)	(0.0033)	(0.0050)
p-value for (1)-(2)	[0.6381]	[0.5870]	[0.3612]	[0.9888]	[0.2724]	[0.9864]
Panel B: Debt Write-Down and Minimum Payment						
(3) Write-Down x Male	0.0002	−0.0017	−0.0005	0.1773	0.0007	0.0001
	(0.0024)	(0.0022)	(0.0025)	(0.4927)	(0.0016)	(0.0023)
(4) Write-Down x Female	0.0062***	−0.0033**	−0.0052**	0.7539*	0.0000	0.0005
	(0.0024)	(0.0016)	(0.0023)	(0.4084)	(0.0011)	(0.0020)
p-value for (3)-(4)	[0.0564]	[0.4822]	[0.0865]	[0.2829]	[0.7087]	[0.8512]
(5) Payment x Male	0.0005	0.0004	0.0008	−0.1213	−0.0005	−0.0005
	(0.0006)	(0.0005)	(0.0007)	(0.1217)	(0.0004)	(0.0006)
(6) Payment x Female	0.0001	0.0005	0.0010**	−0.1101	−0.0003	0.0001
	(0.0006)	(0.0004)	(0.0005)	(0.1017)	(0.0003)	(0.0004)
p-value for (5)-(6)	[0.6552]	[0.8857]	[0.7798]	[0.9314]	[0.7090]	[0.3279]
Observations	79,739	79,739	71,516	71,516	76,008	76,008
Mean if Male	0.1255	0.1252	0.3866	601.7084	0.8430	0.2825
Mean if Female	0.1391	0.0993	0.3988	603.9307	0.8073	0.2666

Notes: This table reports results by gender. Panel A reports coefficients on the interaction of gender x treatment eligibility. Panel B reports 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 2, 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 2 notes for details on the baseline controls and sample.

Appendix Table 10: Results by Ethnicity

	Complete Payment	Bankrupt	Collections Debt	Credit Score	Employed	Nonzero 401k
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
(1) Treatment x White	0.0091*	−0.0065	0.0033	−0.2481	−0.0046	−0.0019
	(0.0054)	(0.0053)	(0.0053)	(0.9766)	(0.0030)	(0.0051)
(2) Treatment x Non-White	0.0198**	0.0050	−0.0021	−0.0737	0.0031	0.0066
	(0.0077)	(0.0062)	(0.0083)	(1.4343)	(0.0046)	(0.0066)
p-value for (1)-(2)	[0.2519]	[0.1863]	[0.5466]	[0.9221]	[0.1629]	[0.3020]
Panel B: Debt Write-Down and Minimum Payment						
(3) Write-Down x White	0.0038*	−0.0015	−0.0043**	0.5183	0.0000	0.0002
	(0.0020)	(0.0017)	(0.0021)	(0.3766)	(0.0010)	(0.0019)
(4) Write-Down x Non-White	0.0043	−0.0055***	−0.0019	0.5786	0.0008	0.0006
	(0.0030)	(0.0020)	(0.0032)	(0.5555)	(0.0017)	(0.0025)
p-value for (3)-(4)	[0.8786]	[0.0718]	[0.4499]	[0.9140]	[0.6757]	[0.8996]
(5) Payment x White	0.0000	0.0002	0.0014***	−0.1880**	−0.0004	−0.0003
	(0.0006)	(0.0004)	(0.0005)	(0.0920)	(0.0003)	(0.0005)
(6) Payment x Non-White	0.0007	0.0009*	0.0001	0.0553	−0.0003	0.0004
	(0.0007)	(0.0005)	(0.0007)	(0.1459)	(0.0004)	(0.0006)
p-value for (5)-(6)	[0.3490]	[0.2359]	[0.0660]	[0.0923]	[0.7516]	[0.2433]
Observations	79,739	79,739	71,516	71,516	76,008	76,008
Mean if White	0.1474	0.1155	0.3454	611.9167	0.8187	0.2691
Mean if Non-White	0.1075	0.0951	0.4923	585.4953	0.8234	0.2788

Notes: This table reports results by ethnicity. Panel A reports coefficients on the interaction of ethnicity x treatment eligibility. Panel B reports 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 2, 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 2 notes for details on the baseline controls and sample.

Appendix Table 11: Results by Homeownership

	Complete Payment	Bankrupt	Collections Debt	Credit Score	Employed	Nonzero 401k
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
(1) Treatment x Homeowner	0.0093	−0.0090	0.0002	−0.9562	−0.0009	0.0015
	(0.0067)	(0.0062)	(0.0065)	(1.1462)	(0.0044)	(0.0066)
(2) Treatment x Non-Owner	0.0157***	0.0011	0.0011	0.3193	−0.0024	0.0010
	(0.0056)	(0.0046)	(0.0059)	(1.0012)	(0.0034)	(0.0050)
p-value for (1)-(2)	[0.4458]	[0.2208]	[0.9139]	[0.4031]	[0.8002]	[0.9551]
Panel B: Debt Write-Down and Minimum Payment						
(3) Write-Down x Homeowner	0.0041*	−0.0029	−0.0042*	0.4226	0.0001	−0.0001
	(0.0023)	(0.0018)	(0.0024)	(0.4269)	(0.0014)	(0.0023)
(4) Write-Down x Non-Owner	0.0045**	−0.0024	−0.0025	0.5881	0.0004	0.0006
	(0.0022)	(0.0017)	(0.0022)	(0.3878)	(0.0012)	(0.0020)
p-value for (3)-(4)	[0.8953]	[0.8238]	[0.4982]	[0.7051]	[0.8814]	[0.7667]
(5) Payment x Homeowner	−0.0006	0.0005	0.0017***	−0.1997*	−0.0004	−0.0003
	(0.0006)	(0.0004)	(0.0006)	(0.1038)	(0.0003)	(0.0005)
(6) Payment x Non-Owner	0.0005	0.0005	0.0003	−0.0584	−0.0003	0.0001
	(0.0006)	(0.0004)	(0.0005)	(0.0987)	(0.0003)	(0.0005)
p-value for (5)-(6)	[0.1262]	[0.9985]	[0.0191]	[0.1826]	[0.8431]	[0.5273]
Observations	79,739	79,739	71,516	71,516	76,008	76,008
Mean if Homeowner	0.1401	0.1140	0.3189	619.0990	0.7987	0.2974
Mean if Non-Owner	0.1340	0.0963	0.4485	591.0496	0.8353	0.2548

Notes: This table reports results by baseline homeownership. Panel A reports coefficients on the interaction of homeownership x treatment eligibility. Panel B reports 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 2, 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 2 notes for details on the baseline controls and sample.

Appendix Table 12: Results by Baseline Employment

	Complete Payment	Bankrupt	Collections Debt	Credit Score	Employed	Nonzero 401k
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
(1) Treatment x Employed	0.0149***	−0.0048	0.0023	−0.6114	−0.0026	−0.0006
	(0.0048)	(0.0038)	(0.0051)	(0.8308)	(0.0027)	(0.0046)
(2) Treatment x Unemployed	0.0056	0.0047	−0.0065	1.6267	0.0028	0.0112*
	(0.0091)	(0.0073)	(0.0104)	(1.8045)	(0.0105)	(0.0068)
p-value for (1)-(2)	[0.3419]	[0.2352]	[0.4396]	[0.2607]	[0.6289]	[0.1430]
Panel B: Debt Write-Down and Minimum Payment						
(3) Write-Down x Employed	0.0049***	−0.0033**	−0.0032	0.4349	0.0005	0.0002
	(0.0018)	(0.0014)	(0.0020)	(0.3753)	(0.0011)	(0.0018)
(4) Write-Down x Unemployed	0.0018	0.0011	−0.0046	0.9657	−0.0014	0.0018
	(0.0034)	(0.0024)	(0.0032)	(0.6022)	(0.0032)	(0.0025)
p-value for (3)-(4)	[0.3703]	[0.0640]	[0.6732]	[0.4213]	[0.5766]	[0.5246]
(5) Payment x Employed	0.0001	0.0003	0.0010**	−0.1057	−0.0004	−0.0001
	(0.0005)	(0.0004)	(0.0005)	(0.0965)	(0.0003)	(0.0005)
(6) Payment x Unemployed	−0.0002	0.0007	0.0010	−0.2117*	−0.0000	−0.0000
	(0.0008)	(0.0005)	(0.0007)	(0.1233)	(0.0006)	(0.0005)
p-value for (5)-(6)	[0.6767]	[0.5464]	[0.9590]	[0.4436]	[0.5851]	[0.8520]
Observations	79,739	79,739	71,516	71,516	76,008	76,008
Mean if Employed	0.1373	0.1121	0.3956	602.1617	0.9244	0.3170
Mean if Unemployed	0.1332	0.0680	0.3806	607.2904	0.2404	0.0238

Notes: This table reports results by baseline employment. Panel A reports coefficients on the interaction of employment x treatment eligibility. Panel B reports 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 2, 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 2 notes for details on the baseline controls and sample.

Appendix Table 13: Results by Baseline 401k Contribution

	Complete Payment	Bankrupt	Collections Debt	Credit Score	Employed	Nonzero 401k
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
(1) Treatment x Non-Zero 401k	0.0174*	−0.0097	0.0035	−1.9045	−0.0030	−0.0051
	(0.0101)	(0.0075)	(0.0087)	(1.5525)	(0.0047)	(0.0099)
(2) Treatment x Zero 401k	0.0119**	−0.0010	−0.0002	0.2985	−0.0011	0.0067*
	(0.0051)	(0.0041)	(0.0051)	(0.8331)	(0.0030)	(0.0039)
p-value for (1)-(2)	[0.6377]	[0.3228]	[0.6932]	[0.1999]	[0.7366]	[0.2595]
Panel B: Debt Write-Down and Minimum Payment						
(3) Write-Down x Non-Zero 401k	0.0051*	−0.0039	−0.0029	0.0461	0.0003	−0.0013
	(0.0028)	(0.0026)	(0.0029)	(0.5403)	(0.0014)	(0.0029)
(4) Write-Down x Zero 401k	0.0042**	−0.0022	−0.0036*	0.6931*	0.0002	0.0009
	(0.0020)	(0.0014)	(0.0020)	(0.3627)	(0.0011)	(0.0016)
p-value for (3)-(4)	[0.7735]	[0.5150]	[0.8108]	[0.2307]	[0.9430]	[0.4570]
(5) Payment x Non-Zero 401k	0.0004	0.0002	0.0014**	−0.0227	−0.0006	0.0002
	(0.0007)	(0.0006)	(0.0007)	(0.1385)	(0.0004)	(0.0007)
(6) Payment x Zero 401k	−0.0001	0.0005	0.0009*	−0.1612*	−0.0003	−0.0001
	(0.0005)	(0.0004)	(0.0005)	(0.0895)	(0.0003)	(0.0004)
p-value for (5)-(6)	[0.4703]	[0.5584]	[0.4596]	[0.3004]	[0.4145]	[0.6188]
Observations	79,739	79,739	71,516	71,516	76,008	76,008
Mean if Non-Zero 401k	0.1609	0.1209	0.3351	614.4463	0.9610	0.6818
Mean if Zero 401k	0.1294	0.0985	0.4103	599.6405	0.7762	0.1441

Notes: This table reports results by baseline 401k contribution status. Panel A reports coefficients on the interaction of 401k contributions x treatment eligibility. Panel B reports coefficients on the interaction of 401k contributions x treatment eligibility x potential treatment intensity. All specifications control for an indicator for potential treatment intensity, the baseline variables listed in Table 2, 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 2 notes for details on the baseline controls and sample.

Appendix Table 14: High Credit Score versus Low Credit Score

	Complete Payment	Bankrupt	Collections Debt	Credit Score	Employed	Nonzero 401k
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
(1) Treatment x High Score	0.0263***	−0.0055	−0.0012	0.0708	−0.0013	−0.0065
	(0.0071)	(0.0057)	(0.0062)	(1.1429)	(0.0042)	(0.0061)
(2) Treatment x Low Score	0.0040	−0.0001	0.0029	−0.5140	−0.0025	0.0069
	(0.0058)	(0.0052)	(0.0068)	(1.0477)	(0.0038)	(0.0059)
p-value for (1)-(2)	[0.0105]	[0.4951]	[0.6577]	[0.7130]	[0.8425]	[0.1108]
Panel B: Debt Write-Down and Minimum Payment						
(3) Write-Down x High Score	0.0071***	−0.0034*	−0.0028	0.2955	−0.0001	0.0016
	(0.0023)	(0.0018)	(0.0022)	(0.3869)	(0.0012)	(0.0022)
(4) Write-Down x Low Score	0.0027	−0.0022	−0.0043*	0.8636*	0.0007	−0.0014
	(0.0027)	(0.0021)	(0.0026)	(0.4735)	(0.0014)	(0.0023)
p-value for (3)-(4)	[0.1556]	[0.6256]	[0.6098]	[0.2654]	[0.5928]	[0.2684]
(5) Payment x High Score	−0.0003	0.0005	0.0009*	−0.1056	−0.0005	−0.0007
	(0.0006)	(0.0004)	(0.0005)	(0.0917)	(0.0003)	(0.0005)
(6) Payment x Low Score	0.0001	0.0003	0.0012*	−0.1565	−0.0001	0.0006
	(0.0007)	(0.0006)	(0.0006)	(0.1204)	(0.0004)	(0.0006)
p-value for (5)-(6)	[0.6064]	[0.6773]	[0.6733]	[0.6625]	[0.3976]	[0.0525]
Observations	79,739	79,739	71,516	71,516	76,008	76,008
Mean if High Score	0.1721	0.1146	0.2639	632.8460	0.8023	0.2899
Mean if Low Score	0.0942	0.0995	0.5238	572.9552	0.8353	0.2556

Notes: This table reports results by baseline credit score. Panel A reports coefficients on the interaction of an indicator for above or below median credit score x treatment eligibility. Panel B reports coefficients on the interaction of an indicator for above or below median credit score x treatment eligibility x potential treatment intensity. All specifications control for an indicator for potential treatment intensity, the baseline variables listed in Table 2, 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 2 notes for details on the baseline controls and sample.

Appendix Table 15: Bankruptcy Results by Year

	Year 1	Year 2	Year 3	Year 4	Year 5
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)
Treatment Eligibility	-0.0004	-0.0004	-0.0030^{**}	-0.0004	0.0010
	(0.0028)	(0.0014)	(0.0013)	(0.0012)	(0.0009)
Panel B: Debt Write-Down and Minimum Payment Estimates					
Debt Write-Down	-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 relief on bankruptcy. Information on bankruptcy comes from court records. We report coefficients on the interaction of treatment eligibility and potential debt write-down (in terms of the interest rate in percentage points), and the interaction of treatment eligibility and potential minimum payment reduction (in percentage points x 100). All specifications control for potential debt write-down, potential minimum payment reduction, the baseline controls in Table 2, 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 2 notes for details on the baseline controls and sample.

Appendix Table 16: Pre-BAPCPA versus Post-BAPCPA

	Complete Payment	Bankrupt	Collections Debt	Credit Score	Employed	Nonzero 401k
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
(1) Treatment x Pre-BAPCPA	0.0133**	−0.0054	0.0007	−0.3201	−0.0007	0.0000
	(0.0053)	(0.0044)	(0.0052)	(0.8558)	(0.0032)	(0.0047)
(2) Treatment x Post-BAPCPA	0.0127	0.0042	0.0008	0.1009	−0.0052	0.0047
	(0.0089)	(0.0063)	(0.0086)	(1.6241)	(0.0054)	(0.0077)
p-value for (1)-(2)	[0.9547]	[0.2386]	[0.9924]	[0.8195]	[0.5030]	[0.5959]
Panel B: Debt Write-Down and Minimum Payment						
(3) Write-Down x Pre-BAPCPA	0.0053***	−0.0035**	−0.0031	0.4038	−0.0004	0.0004
	(0.0020)	(0.0016)	(0.0021)	(0.3658)	(0.0011)	(0.0019)
(4) Write-Down x Post-BAPCPA	0.0053*	−0.0007	−0.0043	0.9504*	0.0013	0.0003
	(0.0030)	(0.0018)	(0.0027)	(0.5515)	(0.0015)	(0.0027)
p-value for (3)-(4)	[0.9817]	[0.1833]	[0.6843]	[0.3444]	[0.3312]	[0.9708]
(5) Payment x Pre-BAPCPA	−0.0011**	0.0007	0.0010**	−0.1323	−0.0001	−0.0001
	(0.0005)	(0.0004)	(0.0005)	(0.0946)	(0.0003)	(0.0005)
(6) Payment x Post-BAPCPA	0.0013*	0.0001	0.0010	−0.1489	−0.0008**	−0.0001
	(0.0007)	(0.0004)	(0.0006)	(0.1205)	(0.0004)	(0.0006)
p-value for (5)-(6)	[0.0015]	[0.2039]	[0.9300]	[0.8944]	[0.0759]	[0.9161]
Observations	79,739	79,739	71,516	71,516	76,008	76,008
Mean if Pre-BAPCPA	0.1197	0.1121	0.3904	602.9336	0.8212	0.2692
Mean if Post-BAPCPA	0.1665	0.0886	0.3975	603.3402	0.8186	0.2780

Notes: This table reports results by date of the counseling session. Panel A reports coefficients on the interaction of contacting MMI before or after the implementation of BAPCPA (October 17, 2005) x treatment eligibility. Panel B reports coefficients on the interaction of contacting MMI before or after the implementation of BAPCPA (October 17, 2005) x treatment eligibility x potential treatment intensity. All specifications control for an indicator for potential treatment intensity, the baseline variables listed in Table 2, 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 2 notes for details on the baseline controls and sample.

Appendix Table 17: Results with p-values from Permutation Test

	Complete Payment	Bankrupt	Collections Debt	Credit Score	Employed	Nonzero 401k
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Eligibility	0.0131***	−0.0030	0.0007	−0.2195	−0.0018	0.0012
	[0.0000]	[0.3276]	[0.8081]	[0.7522]	[0.4185]	[0.7312]
Panel B: Debt Write-Down and Minimum Payment						
Debt Write-Down	0.0044**	−0.0027***	−0.0034*	0.5278**	0.0003	0.0003
	[0.0000]	[0.0189]	[0.0974]	[0.0379]	[0.7442]	[0.8021]
Min. Payment Reduction	−0.0000	0.0004*	0.0010**	−0.1270***	−0.0004*	−0.0001
	[0.9700]	[0.0589]	[0.0419]	[0.0279]	[0.0719]	[0.7172]
Observations	79,739	79,739	71,516	71,516	76,008	76,008
Mean in Control Group	0.1366	0.1036	0.3929	603.0766	0.8202	0.2723

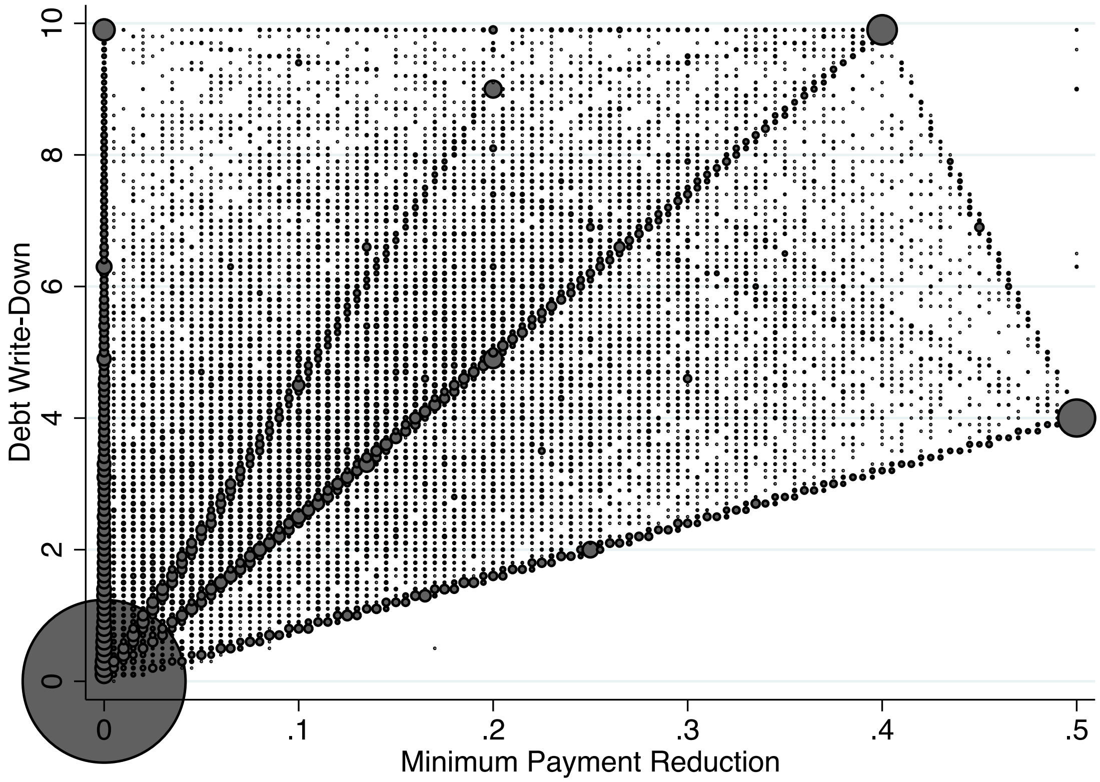
Notes: This table reports reduced form results where the p-values are calculated using a non-parametric permutation test with 1,000 draws. All specifications control for potential debt write-down, potential minimum payment reduction, and strata fixed effects. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Table 2 notes for details on the baseline controls and sample and the text for additional details on the non-parametric permutation test.

Appendix Table 18: Characteristics of Borrowers Completing the Repayment Program

	Control Compliers	Treatment Compliers	Difference
Panel A: Baseline Characteristics	(1)	(2)	(3)
Age	42.453	42.112	−0.848
Male	0.340	0.339	0.005
White	0.686	0.682	−0.000
Black	0.116	0.120	0.020
Hispanic	0.079	0.077	0.006
Homeowner	0.423	0.425	0.029
Renter	0.423	0.427	0.006
Dependents	2.022	1.975	−0.029
Monthly Income	2.740	2.719	−0.026
Monthly Expenses	2.233	2.219	−0.041
Total Unsecured Debt	20.319	20.533	0.023
Debt with Part. Creditors	13.163	13.395	0.157
Internal Risk Score	-0.375	-0.376	−0.018
Panel B: Baseline Outcomes			
Collections Debt	0.166	0.171	0.008
Credit Score	595.094	595.323	−0.841
Bankruptcy	0.002	0.001	−0.002
Employment	0.868	0.876	−0.005
Earnings	27.805	27.374	−1.984*
Nonzero 401k Cont.	0.268	0.263	−0.042
401k Contributions	0.515	0.499	−0.186
Panel C: Data Quality			
Matched to SSA data	0.936	0.937	0.015
Matched to TU Data	0.883	0.878	−0.001
Panel D: Potential Treatment Intensity			
Interest Rate if Control	0.088	0.088	0.000
Interest Rate if Treatment	0.049	0.048	−0.001
Min. Payment Percent if Control	0.025	0.025	−0.000
Min. Payment Percent if Treatment	0.024	0.024	−0.000
Program Length in Months if Control	52.637	53.017	0.276
Program Length in Months if Treatment	51.567	51.772	0.161
p-value from joint F-test	–	–	0.976
Observations	5,530	5,713	11,243

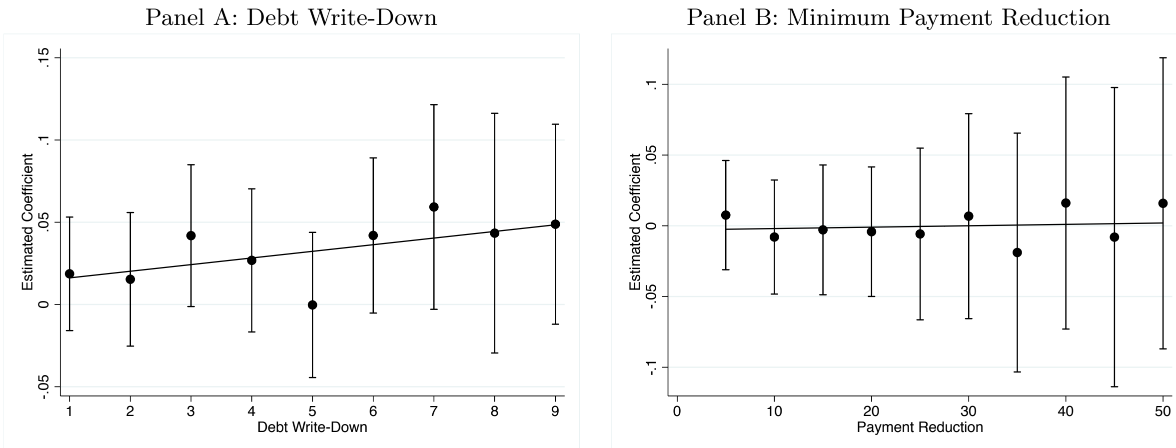
Notes: This table reports descriptive statistics for control and treatment compliers based on program completion. Column 3 reports the difference between the treatment and control groups, controlling for strata fixed effects and clustering standard errors at the counselor level. *** = significant at 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 2 notes for additional details on the sample and variable construction.

Appendix Figure 1: Distribution of Potential Treatment Intensity



Notes: This figure plots the distribution of potential debt write-downs and minimum payment reductions in our estimation sample. Potential minimum payment reductions and debt write-downs are calculated using borrower-level data and the rules listed in Appendix Table 1. See text for additional details.

Appendix Figure 2: Non-Parametric Treatment Effects



Notes: These figures report non-parametric treatment effects. All specifications control for the potential minimum payment and debt write-down changes if treated and cluster standard errors at the counselor level. See the Table 3 notes for additional details on the sample and specification.

Appendix A: Model Details

A. Additional Details from the Model Setup

Let $q_t \in \{0,1\}$ be a binary variable where $q_t = 1$ denotes default in period t, and $q_t = 0$ repayment in t. The net cash flow $v(t,q)$ associated with the default decision q in period t is:

$$\begin{aligned} v(t,1) &= x \\ v(t,0) &= y_t - d_t \end{aligned}$$

Then, the continuation value $V(t,q)$ of making decision q subject to the liquidity constraint $\underline{v}$ for any time period $0 \leq t < P$ is given by the present discounted value of the contemporaneous period cash flow of decision q and the future value of expected future cash flows associated with q:

$$V(t,q) \equiv v(t,q) + E_t\left[\max_{\{q_k\}} \sum_{k=t+1}^{\infty} \beta^{k-t} v(k,q_k)\right] \tag{5}$$

$$\text{s.t.} \quad \begin{aligned} q_t &\in \{q_{t-1}, 1\} \\ q_t &= 1 \text{ if } y_t - d_t > \underline{v} \quad \forall t \leq P \end{aligned}$$

Letting $\Gamma(q,t)$ denote the set of values $q^{'}$ which satisfy constraints for q_{t+1} given $q_t = q$, we can rewrite Equation (5) as:

$$V(t,q) \equiv v(t,q) + \beta E\left[\max_{q^{'} \in \Gamma(q,t)} \left\{V\left(t+1,q^{'}\right)\right\}\right] \tag{6}$$

where $v(t,q)$ is the individual's contemporaneous period cash flow in period t, and $\beta E\left[\max_{q^{'}\in\Gamma(q,t)}\left\{V\left(t+1,q^{'}\right)\right\}\right]$ is the individual's future value of expected future cash flows associated with default decision q in t.

The above setup implies that the value of default $V^d \equiv V(t,1)$ simplifies to the discounted value of receiving x in both the current period and all future periods:

$$V^d = \frac{x}{1-\beta} \tag{7}$$

as individuals discount the future with a common (across-time) subjective discount rate β.

Conversely, the value of repayment $V^r(t,y) \equiv V(t,0;y)$ for periods $0 \leq t < P$ consists of the contemporaneous value of repayment $y - d$ and the option value of being able to either repay or default in future periods $\beta\left[\int_{\underline{v}+d}^{\infty} \max\left\{V^r\left(t+1,y^{'}\right), V^d\right\} dF\left(y^{'}\right) + F(\underline{v}+d)V^d\right]$:

$$V^r(t,y) = y - d + \beta\left[\int_{\underline{v}+d}^{\infty} \max\left\{V^r\left(t+1,y^{'}\right), V^d\right\} dF\left(y^{'}\right) + F(\underline{v}+d)V^d\right] \tag{8}$$

Note that the contemporaneous value of repayment $v^r(y) \equiv y - d$ depends on both income y and the constant debt payment d, while the option value of being able to either repay or default in future periods $\beta\left[\int_{\underline{v}+d}^{\infty}\max\left\{V^r\left(t+1,y^{'}\right),V^d\right\}dF\left(y^{'}\right)+F\left(\underline{v}+d\right)V^d\right]$ depends on the expected value of repayment relative to default value in period $t+1$. This expected value of repayment also explicitly includes the expected possibility of involuntary default in future periods. We let this future value, or "option value," of repayment be denoted by $Q^r(t)$.

Now, note that in period $t=P$, repayment implies solvency in the next period, implying that the option value of repayment $Q^r(P)$ is:

$$\begin{aligned}Q^r(P) &= \beta E\left[\sum_{k=P+1}^{\infty} y_k\right]\\ E\left[Q^r(P)\right] &= \frac{\beta\mu}{1-\beta}\end{aligned}$$

further implying that the repayment value in period $t=P$ is:

$$\begin{aligned}V^r(P,y) &= y-d+Q^r(P)\\ &= y-d+\beta\frac{\mu}{1-\beta}\end{aligned} \tag{9}$$

The model is characterized by the value of repayment in period $t=P$ given by Equation (9) above, and the Bellman equation that gives the repayment continuation value $V^r(t,y)$ in periods $0\le t<P$. We form this Bellman equation by combining Equations (7) and (8):

$$V^r(t,y) \;=\; y-d \;+\; \beta\left[\int\max\left\{V^r\left(t+1,y^{'}\right),\tfrac{x}{1-\beta}\right\}dF\left(y^{'}\right)\right] \tag{10}$$

Equation (10) shows that while contemporaneous net income $v^r(y)=y-d$ is unaffected by t for $t<P$, the option value of continuing repayment $Q^r(t)$ is weakly increasing in t for $0\le t<P$. This is because the absence of payments or liquidity risk for solvent individuals in periods $t>P$ means the option value of repayment increases as individuals approach the end of the repayment period. As a result, the total value of repayment $V^r(t,y)$ is also weakly increasing in t for $0\le t<P$.

B. Solving for Individuals' Default Decision

The model implies that default decisions are driven by two separate forces: (1) involuntary default that occurs among individuals who are liquidity constrained, and (2) the strategic response to a low income draw among individuals who are not liquidity constrained.

To see this, first recall that the liquidity constraint implies automatic default for individuals with $y\le\underline{v}+d$. Next, note that default occurs for individuals with $y>\underline{v}+d$ if and only if $V^r(t,y)<V^d$, where the value of repayment $V^r(t,y)$ is strictly increasing in y. This implies that optimal default behavior for a liquid individual can be characterized by a path of cutoff values ϕ_t^*, defined by:

$$V^r(t,\phi_t^*)=V^d$$

where an individual defaults if $y_t < \phi_t^*$. Taken together, these two facts imply that general default behavior can be characterized by ϕ_t:

$$q(t,y) = \begin{cases} 1 & \text{if } y \le \phi_t \\ 0 & \text{if } y > \phi_t \end{cases} \tag{11}$$

$$\phi_t = \max\left(\phi_t^*, \underline{v} + d\right) \tag{12}$$

To solve for the path of ϕ^* in periods $0 \le t \le P$, we use the decision rule given by Equation (11) to write the individuals' Bellman equation (10) as:

$$V^r(t,y) = y - d + \beta\left(\int_{\phi_{t+1}}^{\infty} V^r\left(t+1, y'\right) dF\left(y'\right) + F\left(\phi_{t+1}\right) V^d\right) \tag{13}$$

Next, we use the fact that individuals are indifferent between repayment and default when the income draw is equal to the cutoff (i.e. $V^r(t,\phi_t^*) = V^d$) to show that:

$$V^r(t,\phi_t^*) = \frac{x}{1-\beta} \tag{14}$$

and

$$\begin{aligned} v^r(t,\phi_t^*) + Q^r(t) &= \frac{x}{1-\beta} \\ Q^r(t) &= \frac{x}{1-\beta} - \phi_t^* + d \end{aligned} \tag{15}$$

where $Q^r(t)$ again denotes the option value of expected future cash flows under repayment. We can then substitute Equations (14) and (15) into the Bellman equation given by (13) to solve for the path of default cutoffs ϕ_t^* for liquid individuals in periods $0 \le t \le P$:

$$\begin{aligned} V^r(t,\phi_t^*) &= \phi_t^* - d + \beta\left[\int_{\phi_{t+1}}^{\infty} V^r\left(t+1,y'\right) dF\left(y'\right) + F\left(\phi_{t+1}\right) V^d\right] \\ \frac{x}{1-\beta} &= \phi_t^* - d + \beta\left[\int_{\phi_{t+1}}^{\infty} \left[y' - d + Q^r(t+1)\right] dF\left(y'\right) + F\left(\phi_{t+1}\right)\left(\frac{x}{1-\beta}\right)\right] \\ \frac{x}{1-\beta} &= \phi_t^* - d + \beta\left[\int_{\phi_{t+1}}^{\infty} \left[y' - d + \left(\frac{x}{1-\beta} - \phi_{t+1}^* + d\right)\right] dF\left(y'\right) + F\left(\phi_{t+1}\right)\left(\frac{x}{1-\beta}\right)\right] \\ \phi_t^* &= \frac{x}{1-\beta} + d - \beta\left[\int_{\phi_{t+1}}^{\infty} \left(y' + \frac{x}{1-\beta} - \phi_{t+1}^*\right) dF\left(y'\right) + F\left(\phi_{t+1}\right)\left(\frac{x}{1-\beta}\right)\right] \end{aligned} \tag{16}$$

Following the discussion in the main text, Equation (16) implies that the optimal default cutoffs ϕ_t^* are strictly decreasing over time, reflecting the decreased incentive to default as individuals remaining loan balances shrink.

Equation (16) and the fact that the liquidity constraint implies automatic default for individuals with $y \leq \underline{v} + d$ imply that the general decision rule ϕ_t that applies to both liquid and illiquid individuals is:

$$\phi_t = d + \max\left\{\frac{x}{1-\beta} - \beta\left[\int_{\phi_{t+1}}^{\infty}\left(y^{'} + \frac{x}{1-\beta} - \phi_{t+1}^{*}\right)dF\left(y^{'}\right) + F\left(\phi_{t+1}\right)\left(\frac{x}{1-\beta}\right)\right], \underline{v}\right\} \quad (17)$$

Finally, we can fully characterize the path $\{\phi_t\}$ by using Equations (9) and (14) to find ϕ_P, the cutoff in period $t = P$:

$$\begin{aligned} V^{r}\left(P, \phi_P^{*}\right) &= V^{d} \\ \phi_P^{*} - d + \frac{\beta\mu}{1-\beta} &= \frac{x}{1-\beta} \\ \phi_P^{*} &= d + \frac{x - \beta\mu}{1-\beta} \\ \phi_P &= d + \max\left\{\frac{x-\beta\mu}{1-\beta}, \underline{v}\right\} \end{aligned} \quad (18)$$

Default cutoffs ϕ_t for $0 \leq t < P$ can be found via backward recursion using the difference equation given by Equation (17) and the explicit solution for ϕ_P given by Equation (18).

C. Default Likelihood and Repayment

To examine the impact of the experiment on repayment rates through two different channels, we must show how the default behavior described by Equation (17) affects the probability of individuals' remaining in repayment through period t. To do this, we first define an expression for risk of default among repaying individuals at period t:

$$\lambda\left(t\right) \equiv \begin{cases} F\left(\phi_t\right) & \text{if } t \leq P \\ 0 & \text{if } t > P \end{cases}$$

where we note that the hazard rate $\lambda\left(t\right)$ is weakly decreasing as individuals approach the end of repayment. This result is due to the path of optimal default cutoffs ϕ_t strictly decreasing for all $0 \leq t < P$. Also note that the hazard rate $\lambda\left(t\right)$ is strictly decreasing as individuals approach the end of repayment if $F\left(\cdot\right)$ is assumed to increase strictly for all $0 \leq t < P$.

We then decompose the hazard rate $\lambda\left(t\right)$ into *strategic* and *non-strategic* default risks:

$$\lambda\left(t\right) \equiv \rho\left(t\right) + \gamma\left(t\right) \quad (19)$$

where

$$\gamma(t) \equiv \begin{cases} F(\underline{v}+d) & \text{if } t \leq P \\ 0 & \text{if } t > P \end{cases} \tag{20}$$

$$\rho(t) \equiv \begin{cases} \int_{\underline{v}+d}^{\phi_t} dF(y) & \text{if } t \leq P \\ 0 & \text{if } t > P \end{cases} \tag{21}$$

To map the hazard rates from each channel into the repayment rates observed in the experiment, we first define the probability of remaining in repayment by period $\theta(t)$ as:

$$\theta(t) \equiv \prod_{k=0}^{t} [1-\lambda(k)] \tag{22}$$

using the fact that $\lambda(t)$ is the conditional likelihood of exiting repayment at t. Letting the probability of avoiding default throughout the repayment period be $\Theta \equiv \theta_P$, we then have that the probability of avoiding default through the entire repayment period Θ is:

$$\Theta = \prod_{t=0}^{P} [1-\lambda(t)] \tag{23}$$

Using this framework, we can now investigate the implications of the experiment on the default likelihood $\lambda(t;\xi)$ as a function of the period t and repayment plan parameter ξ (e.g., repayment period P, minimum payment d, etc.). To see this, note that Equation (19) implies:

$$\lambda(t;\xi) = \underset{\text{Strategic Risk}}{\rho(t;\xi)} + \underset{\text{Non-Strategic Risk}}{\gamma(t;\xi)}$$

which gives us the model's predicted repayment probability Θ:

$$\begin{aligned} \Theta(\xi) &= \prod_{t=0}^{t} [1-\lambda(t;\xi)] \\ &= \prod_{t=0}^{t} [1-\rho(t;\xi)-\gamma(t;\xi)] \end{aligned} \tag{24}$$

where we have the familiar result that repayment rates are driven by two factors: (1) involuntary default that occurs among individuals who are liquidity constrained, and (2) the strategic response to a low income draw among individuals who are not liquidity constrained.

D. Proofs of Model Predictions

D.1 Proof of Debt Write-Down Prediction

In this section, we expand on the discussion of the debt write-down effect from the main text before providing a formal proof of the debt write-down prediction.

Preliminaries: The write-down treatment shortens the repayment period P to $P^{WD} < P^{C}$ while keeping the monthly payments d the same $d^{WD} = d^{C}$. Our model predicts that the write-down treatment increases debt repayment through two complementary effects: (1) a decrease in treated individuals' incentive to strategically default while both treatment and control individuals are enrolled in the repayment program, and (2) a decrease in treated individuals' exposure to default risk while control individuals are still enrolled in the repayment program and treatment individuals are not.

To formally establish these predictions, we first consider how the treatment reduces the "strategic risk" of default in periods $0 \leq t \leq P^{WD}$:

$$\Delta\rho \approx \frac{\partial \rho\left(t;P\right)}{\partial P}\Delta P < 0 \tag{25}$$

The direction of the strategic channel is weakly positive because a shorter repayment period increases individuals' strategic incentive to stay in repayment, thereby reducing strategic default probability among treated individuals. This is because the lifetime of debt has been shortened $P^{C} - P^{WD}$ periods, leading treated individuals to place more value on repayment in each time period t. Formally, it can be shown that:

$$\begin{aligned} \frac{\partial \phi^{*}\left(t;P\right)}{\partial P} &= \frac{\partial E\left[V^{r}\left(t,y\right)\right]}{\partial t} \\ \frac{\partial \rho\left(t;P\right)}{\partial P} &= -\frac{\partial \rho\left(t\right)}{\partial t} \end{aligned}$$

Implying that the shorter repayment period decreases optimal default cutoffs at exactly the rate that expected continuation value increases over time. Intuitively, shortening repayment lengths brings individuals closer to the point of solvency $t = P$, making it possible for these individuals to accept lower net income in t in anticipation greater income in future time periods. As a result, there is an increase in the mean continuation value $E\left[V^{r}\left(t,y\right)\right]$ for all $0 \leq t < P^{WD}$, resulting in a lower strategic cutoff ϕ_{t}^{*}.

Note that effects through the strategic risk channel in periods $0 \leq t \leq P^{WD}$ are tempered by the presence of liquidity constraints, as changes in strategic default behavior are only realized for liquid individuals (i.e. $y_t > \underline{v} + d$). In other words, the change in default cutoffs $\Delta\phi\left(t\right) = \phi^{WD}\left(t\right) - \phi^{C}\left(t\right)$ can only be so large in magnitude because $\phi^{WD}\left(t\right)$ and $\phi^{C}\left(t\right)$ are bounded below by $\underline{v} + d$.

Next, we consider the non-strategic default likelihood in periods $0 \leq t \leq P^{WD}$:

$$\Delta\gamma \approx \frac{\partial \gamma\left(t;P\right)}{\partial P}\Delta P = 0 \tag{26}$$

In contrast to this strategic effect in periods $0 \leq t \leq P^{WD}$, the non-strategic default likelihood in periods $0 \leq t \leq P^{WD}$ is exactly zero. This is because the liquidity constraint $\underline{v} + d$ is the same for both the treatment and control groups in periods $t \leq P^{WD}$, meaning that treated individuals are no more or less liquid. There is therefore zero effect through the non-strategic channel in these time periods.

Finally, we consider both the strategic and non-strategic default probabilities in periods $P^{WD} < t \leq P^{C}$. By assumption, "repayment" in these periods is automatic for treated individuals that have not defaulted by P^{WD}. In contrast, control individuals are still exposed to both strategic and non-strategic default risk. Differences in default rates are therefore given by:

$$\begin{aligned} \Delta\lambda(t) &= 0 - \lambda^{C}(t) \\ &= -\rho^{C}(t) - \gamma^{C}(t) \end{aligned}$$

where we have the result that default risk has decreased mechanically through both strategic and non-strategic channels. Again, this is because control individuals still face the possibility of both voluntary or involuntary default, while both forms of default risk have been eliminated for treated individuals.

Proof of Debt Write-Down Prediction: Given the above insights concerning default likelihood throughout the experiment, we can now predict the effect of lower debt write-downs on the change in repayment rates $\Delta\theta$. First, consider $\theta\left(P^{WD}\right)$, the treatment effect at $t = P^{WD}$, which is given by:

$$\begin{aligned} \Delta\theta\left(P^{WD}\right) &\equiv \theta^{WD}\left(P^{WD}\right) - \theta^{C}\left(P^{WD}\right) \\ &= \prod_{t=0}^{P^{WD}}\left[1 - \lambda^{WD}(t)\right] - \prod_{t=0}^{P^{WD}}\left[1 - \lambda^{C}(t)\right] \\ &= \prod_{t=0}^{P^{WD}}\left[1 - \gamma - \rho^{WD}(t)\right] - \prod_{t=0}^{P^{WD}}\left[1 - \gamma - \rho^{C}(t)\right] \end{aligned}$$

where $\gamma = F(\underline{v} + d)$ is non-strategic risk and $\rho^{WD}(t)$, $\rho^{C}(t)$ is period-t strategic default risk for treatment and control. Importantly, since non-strategic risk γ is identical for both treatment and control individuals, the treatment effect at $\Delta\theta\left(P^{WD}\right)$ is driven entirely by differences in strategic default behavior $\phi^{*}(t)$.

That *total* treatment effect $\Delta\Theta = \Delta\theta\left(P^C\right)$ is given by:

$$\begin{aligned}
\Delta\Theta &= \Theta^{WD} - \Theta^{C} \\
&= \prod_{t=0}^{P^C}\left[1-\lambda^{WD}(t)\right] - \prod_{t=0}^{P^C}\left[1-\lambda^{C}(t)\right] \\
&= \prod_{t=0}^{P^{WD}}\left[1-\lambda^{WD}(t)\right] - \prod_{t=0}^{P^{WD}}\left[1-\lambda^{C}(t)\right]\prod_{t=P^{WD}+1}^{P^C}\left[1-\lambda^{C}(t)\right] \\
&= \theta^{WD}\left(P^{WD}\right) - \theta^{C}\left(P^{C}\right)\left(\prod_{t=P^{WD}+1}^{P^C}\left[1-\lambda^{WD}(t)\right]\right)
\end{aligned}$$

Since $\prod_{t=P^{WD}+1}^{P^C}\left[1-\lambda^{C}(t)\right] < 1$, the total treatment effect is larger than the period P^{WD} treatment effect, i.e. $\Delta\Theta \geq \Delta\theta\left(P^{WD}\right)$.

D.2 Proof of Minimum Payment Prediction

Following the proof of the debt write-down prediction, we first expand on the discussion of the minimum payment effect from the main text before providing a formal proof of the minimum payment prediction.

Preliminaries: The minimum payment treatment reduces the required minimum payment by lengthening the repayment period. In the context of our model, a lower minimum payment can therefore be thought of as lengthening the repayment period P to $P^{MP} > P^C$ while keeping the total debt burden the same $\sum_{t=0}^{P^C} d_t = \sum_{t=0}^{P^{MP}} d_t$. Our model predicts an ambiguous impact of the minimum payment treatment on repayment rates due to three opposing effects: (1) a decrease in treated individuals' non-strategic or liquidity-based default while both treatment and control individuals are enrolled in the repayment program, (2) an ambiguous change in treated individuals' incentive to strategically default while both treatment and control individuals are enrolled in the repayment program, and (3) an increase in treated individuals' exposure to default risk while treated individuals are still enrolled in the repayment program and control individuals are not.

To formally establish these predictions, we first alter the model to make monthly payment d endogenous. Specifically, we let D denote the individuals debt balance at $t = 0$ and treat repayment amounts d as function of their total debt D and repayment period length P:

$$d = d(P, D) = \frac{D}{P}$$

Modifying Equation (10), we now have:

$$V^r(t) = y - d(P, D) + \beta E\left[\max\left\{V^r\left(t+1, y'\right), V^d\left(y'\right)\right\}\right]$$

To consider the effects of an increase in P, we must investigate how strategic and non-strategic risk respond to both a greater repayment length and lower minimum payments. We first restrict our attention to periods in which neither group has reached solvency ($0 \leq t \leq P^C$). Consider treatment's effect on strategic default risk $\rho\left(t\right)$, holding liquidity constraints fixed. We have:

$$\Delta\rho \quad \approx \quad \underset{\text{Solvency Effect (+)}}{\frac{\partial\rho(t;P,d)}{\partial P}\Delta P} \quad + \quad \underset{\text{Payment Effect (-)}}{\frac{\partial\rho(t;P,d)}{\partial d}\frac{\partial d}{\partial P}\Delta P} \gtreqless 0 \tag{27}$$

The direction of the strategic channel is ambiguous because two opposing forces influence individuals' considerations of whether or not default is optimal. First, extending the number of periods in which individuals make payments will, all else equal, increase strategic risk ($\frac{\partial\rho(P,d)}{\partial P}\Delta P > 0$). This is the same as the effect from the debt write-down treatment (25), only in the opposite direction. Thus, as the end of repayment P moves farther away, the option value of repayment is lower because treated individuals anticipate a more difficult road to solvency. On the other hand, the decrease in minimum payment size will decrease default risk ($\frac{\partial\rho(P,d)}{\partial d}\frac{\partial d}{\partial P}\Delta P < 0$), as lower payments mean higher cash flows both presently and in expectation. The net direction of changes in strategic risk depends on the relative magnitude of "solvency" and "payment" effects, which vary according to the period. In $t = 0$, the payment effect must dominate and strategic concerns must have a net negative effect on default likelihood. This is due to the fact that the minimum payment treatment only lowers the *minimum* payments individuals have to pay, repayment under the terms of control individuals (i.e. higher minimum payments and shorter repayment length) is still in each treated individual's choice set. Therefore, repayment in period $t = 0$ must be at least as attractive as it would have been under control conditions. However, as each period passes, control individuals have paid an increasingly larger portion of their debt burden D relative to treated individuals. As t approaches the end of control repayment period P^C, control individuals have already repaid all but a small portion of their debt $\left(\frac{D}{P^C}\right)$, whereas treated individuals have a remaining loan balance of $\Delta P * \frac{D}{P^{MP}}$, and thus have less incentive to avoid default.

Now consider treatment's effect on non-strategic default risk $\gamma\left(t\right)$ in periods $0 \leq t \leq P^C$. We have:

$$\Delta\gamma \quad \approx \quad \frac{\partial\gamma}{\partial d}\frac{\partial d}{\partial P}\Delta P < 0 \tag{28}$$

In contrast to the strategic channel, the non-strategic channel has an unambiguously negative effect on default risk. Liquidity constraints are less likely to bind under treatment ($\frac{\partial\gamma}{\partial d}\frac{\partial d}{\partial P}\Delta P < 0$) because payments are lower and net income is higher. So, holding her strategy fixed, a treated individual is less likely to be forced into involuntary default in periods $t \leq P^C$ because repayment has been made less onerous in these periods. Note that this only affects total default probability $\lambda\left(t\right)$ if some "illiquid" control individuals would optimally repay (i.e. $\phi^*\left(t\right) < \underline{v} + d^C$ and $\rho\left(t\right) = 0$). Otherwise, differences in liquidity only occur among individuals who would default anyway.

Finally, we consider periods $P^C < t \leq P^{MP}$. Just as it was for treated individuals under the debt write-down treatment, under minimum payment treatment debt has been completely forgiven for *control* individuals in these periods. Using Equation (24), we have:

$$\begin{aligned}\Delta\lambda(t) &= \lambda^{MP}(t) - 0 \\ &= \rho^{MP}(t) + \gamma^{MP}(t)\end{aligned}$$

The difference in default probability between treatment and control for any period $P^C < t \leq P^{MP}$ is simply the sum of strategic and liquidity risk components contributing to default risk for treated individuals who are still in repayment.

Proof of Minimum Payment Prediction: We can now use these insights to predict treatment effects $\Delta\theta$. First, consider $\theta\left(P^C\right)$, the treatment effect at $t = P^C$, given by:

$$\begin{aligned}\Delta\theta\left(P^C\right) &\equiv \theta^{MP}\left(P^C\right) - \theta^C\left(P^C\right) \\ &= \prod_{t=0}^{P^C}\left[1 - \gamma^{MP}(t) - \rho^{MP}(t)\right] - \prod_{t=0}^{P^C}\left[1 - \gamma^C(t) - \rho^C(t)\right]\end{aligned}$$

where $\gamma^{MP}(t)$, $\gamma^C(t)$ is non-strategic and $\rho^{MP}(t)$, $\rho^C(t)$ is strategic default risk in period t for treatment and control groups. As we established above, lower payments implies lower non-strategic risk for treated individuals ($\gamma^{MP}(t) - \gamma^C(t) < 0$), but the difference in strategic default risk $\rho^{MP}(t) - \rho^C(t)$ is ambiguous in both size and magnitude due to the countervailing forces associated with making a lower payment for a longer time period. The lower minimum payment treatment therefore has an ambiguous impact on repayment rates at P^C.

The total treatment effect $\Theta = \theta\left(P^{MP}\right)$ is given by:

$$\begin{aligned}\Delta\Theta &= \Theta^{MP} - \Theta^C \\ &= \prod_{t=0}^{P^{MP}}\left[1 - \lambda^{MP}(t)\right] - \prod_{t=0}^{P^{MP}}\left[1 - \lambda^C(t)\right] \\ &= \prod_{t=0}^{P^{MP}}\left[1 - \lambda^{MP}(t)\right]\prod_{t=P^C+1}^{P^{MP}}\left[1 - \lambda^{MP}(t)\right] - \prod_{t=0}^{P^C}\left[1 - \lambda^C(t)\right] \\ &= \theta^{MP}\left(P^{WD}\right)\left(\prod_{t=P^{WD}+1}^{P^C}\left[1 - \lambda^{MP}(t)\right]\right) - \theta^C\left(P^C\right)\end{aligned}$$

Since $\prod_{t=P^C+1}^{P^{MP}}\left[1 - \lambda^{MP}(t)\right] < 1$, the total treatment effect is smaller than the period P^C treatment effect, i.e. $\Delta\Theta \leq \Delta\theta\left(P^C\right)$.