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

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

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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 interest write-downs to target long-run debt constraints. We identify the separate effects of the payment reductions and interest write-downs using both the experiment and cross-sectional variation in treatment intensity. We find that the interest 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.

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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, Schneider 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 seeking debt relief, 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 of interest

[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, Liu and Souleles 2007; Agarwal et al. 2015; Gross, Notowidigdo and Wang forthcoming), and recent work shows large changes in financial distress and consumption just after anticipated reductions in mortgage interest rates (e.g., Di Maggio et al. 2017; Fuster and Willen 2017). 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, Repetto and Tobacman 2007). 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 2019), entrepreneurial activity (Adelino, Schoar and Severino 2015), and home investment (Melzer 2017).

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 of 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 interest write-downs meant to address longer-run debt overhang. The additional debt relief provided by the experiment was substantial: the maximum payment reductions in the treatment group were \$92 (21.0 percent) per month larger than those in the status quo program, while the maximum interest write-downs in the treatment group were \$4,302 (100.0 percent) larger than those in the status quo program.

We estimate the separate impact of the interest write-downs and minimum payment reductions using variation in potential treatment intensity across individuals. The variation in treatment intensity comes from two sources. First, each of the credit card issuers participating in the randomized trial offered a different combination of interest write-downs and minimum payment reductions to treated borrowers. Second, 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 interest write-downs and minimum payment reductions offered to the treatment group. For example, treated borrowers at the 75th percentile of the interest 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. We compare the effects of the randomized treatment eligibility across individuals with these higher and lower treatment intensities to identify the separate effects of the interest write-downs and minimum payment reductions.

The critical identification assumption underlying our research design is that the causal effects of the interest write-downs and payment reductions are uncorrelated with treatment intensity. This is a strong assumption that could plausibly be violated for several reasons. For example, individuals who borrow from credit card issuers offering more generous interest write-downs and payment reductions in the treatment group may be more responsive to debt relief than individuals who borrow from issuers offering less generous interest write-downs in the treatment group. To account for such Roy (1951) –type selection concerns, our preferred specifications compare individuals with the same set of credit cards, but different proportions of debt on each credit card. These specifications weaken the identifying assumption by only requiring that the proportion of debt with each card issuer is as-good-as-randomly assigned with respect to treatment effect heterogeneity, but not the initial choice of which credit cards to hold. Consistent with this weaker version of the identifying assumption, we find that treatment intensity is uncorrelated with observable borrower characteristics within these "creditor risk sets" and that our estimates are robust to a set of sharp overidentification tests once we include an exhaustive set of creditor risk set fixed effects.[2]

We begin our empirical analysis by estimating the effect of treatment eligibility on repayment, bankruptcy, collections debt, credit scores, and labor market outcomes. These intent-to-treat estimates measure the combined impact of both the interest write-downs and minimum payment reductions and, as a result, are only informative of the net impact of targeted debt relief in our sample. We find that treatment eligibility increased the probability of starting and finishing the repayment program, and decreased the probability of filing for bankruptcy. There were no detectable effects of treatment eligibility on collections debt, credit scores, or labor market outcomes, although large standard errors mean that we cannot rule out economically significant effects in both directions.

We then estimate the separate impact of the minimum payment reductions and interest write-downs using the interaction of treatment eligibility and treatment intensity. We find that the interest 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. For these high-debt borrowers, we find that the maximum interest write-down in the treatment group increased the probability of finishing a repayment program by 4.4 percentage points (30.8 percent) and decreased the probability of filing for bankruptcy by 3.5 percentage points (33.3 percent). The probability of having collections debt also decreased by 1.2 percentage points (3.1 percent) for these high-debt borrowers, while the probability of being employed increased by 4.2 percentage points (5.1 percent). The estimated effects of the interest write-downs for credit scores and earnings are smaller and not statistically significant for all borrowers. Taken together, however, our results indicate that there are significant benefits of debt relief targeting long-run debt overhang.

In sharp contrast, we find no positive effects of the more immediate payment reductions targeting short-run liquidity constraints. The maximum payment reduction in the treatment group only increased the probability of finishing a repayment program by a statistically insignificant 0.3 percentage points (2.1 percent) in the full sample, with similar null effects among the highest-debt borrowers. The maximum payment reduction in the treatment group also <u>increased</u> the probability of filing for bankruptcy in this sample by a statistically significant 2.3 percentage points (21.9 percent) and <u>decreased</u> credit scores by a statistically insignificant 1.9 points in our preferred specifications. There are also no detectable positive effects of the payment reductions on collections debt, employment, or earnings. In sum, there is no evidence that borrowers in our sample benefited from the payment reductions, and even some evidence that borrowers seem to have been hurt by

[2] Our approach builds on recent work identifying the causal effect of a single treatment relative to multiple fallback options (e.g., Kline and Walters 2016; Kirkebøen, Leuven and Mogstad 2016; Hull 2018) and potential mediators in multi-site experiments (e.g., Reardon and Raudenbush 2013). Our approach is also related to identification strategies commonly used in studies of local labor markets, immigration, and trade, which exploits the combination of geographic variation in treatment intensity and national 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 variation in treatment status determined by random assignment and individual-level variation in treatment intensity. As a result, our research design is robust to many of the potential concerns that typically arise from these types of "shift-share" instruments (e.g., Goldsmith-Pinkham, Sorkin and Swift 2018).

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 payment 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 with the vast literature documenting liquidity constraints in a variety of settings (e.g., Gross and Souleles 2002; Johnson, Parker and Souleles 2006; Agarwal, Liu and Souleles 2007; Parker et al. 2013; Agarwal et al. 2015; Gross, Notowidigdo and Wang forthcoming), 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, Keys and Mahoney 2017). There is also evidence that the availability of consumer bankruptcy as an outside option provides implicit health (Gross and Notowidigdo 2011; Mahoney 2015), consumption (Auclert, Dobbie and Goldsmith-Pinkham 2019), 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 Home Affordable Modification Program (HAMP) 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. 2017). More recent work suggests that the principal write-downs made through HAMP had no impact on underwater borrowers (Ganong and Noel 2018), while both cross-sectional regressions and theoretical work suggest that principal forgiveness may be effective for borrowers who are not underwater (Eberly and Krishnamurthy 2014; Haughwout, Okah and Tracy 2016).[3] 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, data, and experimental design. Section II describes our empirical design. Section III presents our main results, Section IV explores potential mechanisms, and Section V concludes. The Online Appendix provides additional results and econometric proofs.

[3]Related work shows that anticipated mortgage interest rate reductions decrease mortgage defaults and increase non-durable consumption (e.g., Di Maggio et al. 2017; Fuster and Willen 2017), 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 designed and implemented 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 their 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.[4] 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 $440, and the average length of the repayment program is 52 months.

Creditors will usually allow borrowers to resume the repayment program if they miss just one or two payments. However, if a borrower misses multiple 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

[4] 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. 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 paid by the credit card issuers. 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).[5] In our data, nearly all potential clients are approved during the screening process, with approved clients having only slightly better labor market and financial outcomes than rejected clients (see Appendix Table A1). As discussed in greater detail below, our sample of potential clients is also broadly similar to other financially distressed populations in the United States, suggesting that the screening process does not substantially impact our experimental population.

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 33.7 percent of individuals report that they learned about MMI from an internet search, 19.8 percent from a family member or friend, 20.0 percent from a paid advertisement, and 15.5 percent from a card issuer.

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 interest write-downs to a subset of borrowers interested in a structured

[5] 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.

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.

The resulting nationwide 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. We also drop the small number of individuals who were rejected during the screening process described above. In total, the estimation sample includes 78,438 borrowers assigned to 1,099 different counselors. The sample includes individuals from all 50 states and the District of Columbia, with a similar proportion of individuals across most states (see Appendix Figure A1).

MMI worked with the participating credit card issuers to design the experiment, but none of the card issuers were directly involved with the day-to-day management of the experiment. The participating credit card issuers were, however, given regular updates during the experiment, including the number of individuals enrolling in and completing a repayment program in both the treatment and control groups. The participating credit card issuers were also given a final report following the completion of the experiment, although those results were not shared publicly until the publication of this paper. Following the experiment, many of the participating credit card issuers began offering a combination of more generous minimum payment reductions and interest write-downs to the highest-debt borrowers interested in a DMP. However, it is not clear if this new policy was due to the experiment itself or the onset of the financial crisis.

The research team was not involved in the design, implementation, or original analysis of the experiment. Information on the experimental design comes from MMI records documenting the experimental procedures and results and conversations with both the card issuers and the MMI administrator tasked with implementing the experiment. We were also provided with administrative data from MMI (described in detail below) that allowed us to verify the most important details of the experimental design, including the random assignment of clients to credit counselors, the rotation of credit counselors between the treatment and control groups at two-week intervals, the accuracy of the algorithm used to calculate the individual-specific terms of the repayment program, and the effective random assignment of clients to the treatment and control groups during the experiment.

Sequence of the Experiment: First, each prospective client was randomly assigned to a credit counselor conditional on the contact date, state, and reference channel (i.e. web versus phone). For each counselor, the MMI computer system would automatically switch the counselor 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 staggered across counselors so that, on any given day, approximately 50 percent of prospective clients were assigned to the treatment group with the remaining 50 percent 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. In practice, the automated rotation procedure also meant that the counselors were largely unaware of when they were in the control group and when they were in the treatment group. There was also no publicly-available information available regarding the experiment or the credit card issuers concessions during or immediately after the experiment.[6]

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. Second, individuals were told that they could file for bankruptcy protection, 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 you enroll in a debt management plan, your payments will drop to $301, you will repay all of your credit cards in 56 months, and you will 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

[6]These institutional features mean that the experiment was effectively double-blind, making it unlikely that our results are driven by "Hawthorne" or "observer" effects, where treated individuals change their behavior only because they are aware of being studied. We can also directly test for Hawthorne effects by estimating the "effect" of treatment eligibility for borrowers who 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. We find statistically and economically insignificant effects of treatment eligibility for these borrowers, inconsistent with the existence of Hawthorne effects in our setting (see Panel B of Appendix Table A2).

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 as if he or she was assigned to the control group, as if he or she was assigned to the treatment group and only eligible for an interest write-down, and as if he or she was assigned to the treatment group and only eligible for a minimum payment reduction. We use the control mean for credit card debt acquired before the experiment ($18,470) throughout. We first calculate the DMP terms for this hypothetical borrower as if he or she had been assigned to the control group using the control mean for the minimum payment requirement (2.38 percent of initial debt) and an interest rate of 9.9 percent. For this hypothetical borrower, the control repayment program requires making minimum payments of $439.59 for 51.80 months, with $4,302 in financing fees.

In the second row of Table 1, we recalculate the DMP terms for this hypothetical borrower using the maximum possible interest write-down in the treatment group (9.9 percentage points), holding the minimum payment constant. The maximum interest write-down would decrease the financing fees for this hypothetical borrower by $4,302, or 100.0 percent, by dropping the last nine to ten payments of the repayment program. However, the interest write-down does not affect the borrower's minimum payment amount. Thus, the interest 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.

In the last row of Table 1, we recalculate the DMP terms using the maximum payment reduction in the treatment group (0.5 percentage points), holding the interest write-down constant. The maximum minimum payment reduction in the treatment group would decrease the hypothetical borrower's minimum payment by $92.35, or 21.0 percent, by adding an additional 18 months to the repayment program. The longer repayment period would also increase the financing fees for this borrower by $1,645, or 38.2 percent. Thus, the payment reductions may decrease liquidity-based defaults at the beginning of the repayment program by lowering the minimum payment amount, but may increase defaults at the end of the repayment program by increasing the exposure to default risk.

Treatment Costs: To understand the relative magnitudes of these interest write-downs and minimum payment reductions in the treatment group, columns 6-8 of Table 1 and Appendix Figure A2 present estimates of the net present costs of providing each treatment for a range of discount rates. For each discount rate, we calculate the net present difference between the total payments made by our hypothetical borrower if assigned to the control group and if assigned to the treatment group. For the control group calculation, we assume an interest rate of 9.9 percent and the control means for debt ($18,470), minimum payment (2.38 percent of debt), and monthly default rate (1.12 percent), which we include to account for the mechanical change in default associated with a shorter or longer repayment program.

The net present cost of providing the interest write-downs in the treatment group largely comes from the discounted value of the financing fees that are written down at the end of the repayment program, a clear economic cost that is decreasing in the discount rate. In contrast, the net present cost of providing the payment reductions in the treatment group largely comes from shifting debt payments from the present to the future at the prevailing interest rate, an unclear economic cost that is increasing in the discount rate. In practice, we find that the net present costs of providing the interest write-downs and payment reductions are identical when the discount rate is equal to 16.4 percent (see Appendix Figure A2). The costs of the interest write-downs in the treatment group are \$918 higher than the costs of the payment reductions in the treatment group when the discount rate is around 10 percent (\$2,175 vs. \$1,257), but \$381 lower when the discount rate is 20 percent (\$1,255 vs. \$1,636). We interpret these calculations as suggesting that the experiment provides a reasonably similar comparison of these two different types of debt relief.

C. Data and Experiment Validity

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 and provides tests of experimental validity.

Data Sources and Sample Construction: The counseling data provided by MMI include information on all prospective clients eligible for the randomized trial. The data include information on each individual's total unsecured debts, assets, liabilities, monthly income, monthly expenses, homeownership status, number of dependents, treatment status, enrollment in a repayment program, the repayment program characteristics, and the amount of debt repaid through the repayment program.The data also include information on the date of first contact, state of residence, who referred the individual to MMI, and the assigned counselor. Finally, the data include detailed information on the amount of unsecured debt with the nineteen largest creditors in the sample, including all eleven of the credit card issuers participating in the experiment. We top-code all continuous variables at the 99th percentile of our estimation sample.

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 Chapter 7 and Chapter 13 filings in the 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 therefore explicitly control for any potential selection bias due to the incomplete nature of the bankruptcy data. In addition, we allow individuals to be matched to multiple bankruptcy filings to account for the fact that many individuals file multiple

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

times during our sample period. We find nearly identical results if we limit the sample to borrowers living in states with PACER data coverage or if we only match to the first bankruptcy filing observed for each individual in the PACER data.

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 collections 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 86.7 percent of the credit counseling data to the credit bureau data, with a small number of observations matched but without credit scores. The probability of being matched to the credit report data is not significantly related to treatment status (see Table 2). No personally identifiable information ("PII") were provided to us by TransUnion.

Information on labor market outcomes and 401k contributions comes from administrative tax records from the SSA. The SSA data are available from 1978 to 2013 for every individual who has ever acquired an SSN, including those who are institutionalized. The SSA data include information on all formal sector earnings and 401k contributions from annual W-2s and self-employment earnings from annual 1040s at the IRS. Individuals with no W-2 or self-employment earnings in any particular year are assumed to have had no formal sector earnings in that year. Individuals with no W-2 are also assumed to have had no 401k contributions in that year. The 401k variable includes all conventional, pre-tax contributions, but does not include contributions to Roth accounts. Individuals with zero earnings or zero 401k contributions are included in all regressions throughout the paper.[8] We match the credit counseling data to the tax data using the full social security number. We were 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 Table 2).

We make three sample restrictions to the estimation sample. 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. Third, we drop the small number of individuals who were rejected during the MMI screening process. The resulting estimation sample consists of 39,855 individuals in the control group and 38,583 individuals in the treatment group. Our sample for the labor market and 401k outcomes is further restricted to 74,738 individuals matched to the SSA data, while our sample for the collections debt and credit score

[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.

outcomes is restricted to the 68,000 individuals matched to the TU data.

Descriptive Statistics: Table 2 presents descriptive statistics. Column 1 presents means for the control group. The average individual in the control group is just over 40 years old with 2.18 dependents. Thirty-seven percent of individuals in the control group are men, 64.0 percent are white, 16.8 percent are black, and 9.0 percent are Hispanic. Forty-two percent are homeowners, 43.5 percent are renters, and the remainder live with either a family member or friend. Individuals in our sample are highly indebted before contacting MMI, with the typical individual in the control group holding $18,470 in unsecured debt, with 44.5 percent of that debt being held by a credit card issuer participating in the randomized experiment. Not surprisingly, individuals in our sample are also severely financially distressed before contacting MMI. Baseline credit scores in the control group are about 587 points, with 24.8 percent of individuals in the control group having nonzero collections debt. Eighty-five percent of individuals in the control group have nonzero earnings in the SSA data, with average annual earnings of approximately $23,700 (including 0s). Baseline bankruptcy rates are very low in the control group, however, at 0.3 percent, likely because individuals are unlikely to contact a credit counselor if they have already filed for bankruptcy protection.

II. Empirical Strategy

Overview: Consider a model that relates outcomes such as debt repayment to interest write-downs $WriteDown_i$ and minimum payment reductions $Payment_i$:

$$y_{it} = \beta_0 + \beta_1 WriteDown_i + \beta_2 Payment_i + \beta_3 \mathbf{X}_i + \varepsilon_{it} \tag{1}$$

where y_{it} is the outcome of interest for individual i in year t, $\mathbf{X}_i$ is a vector of individual-level controls, and ε_{it} is an error term. The key problem for inference is that OLS estimates of Equation (1) in non-experimental populations may be biased if the interest write-downs and minimum payment reductions are correlated with the unobservable determinants of later outcomes. For example, individuals borrowing from the credit card issuers offering more generous interest write-downs and payment reductions may be unobservably different than individuals borrowing from the credit card issuers offering less generous repayment terms.

We are also unable to identify the causal effects of the interest write-downs and minimum payment reductions using standard intent-to-treat estimates in our experimental sample, as treated borrowers were offered a repayment program that included a combination of both the more generous interest write-downs and the more generous minimum payment reductions. As a result, standard intent-to-treat estimates will measure the combined effect of both forms of debt relief, not the separate impact of the interest write-downs and minimum payment reductions.[9]

[9] A second complicating factor is that over 25 percent of borrowers in our sample also 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

To address these issues, we estimate the separate impact of the interest write-downs and minimum payment reductions using variation from both the randomized experiment and cross-sectional differences in treatment intensity. The critical identification assumption underlying our approach is that the causal effects of the interest write-downs and payment reductions are uncorrelated with treatment intensity. This is a strong assumption that could plausibly be violated for several reasons. For example, individuals who borrow from credit card issuers offering more generous interest write-downs in the treatment group may be more responsive to debt relief than individuals who borrow from issuers offering less generous interest write-downs in the treatment group. To account for such Roy (1951)–type selection concerns, our preferred specifications compare individuals with the same set of credit cards, but different proportions of debt on each credit card. These specifications weaken the identifying assumption by only requiring that the proportion of debt with each card issuer be as-good-as-randomly assigned with respect to treatment effect heterogeneity, not the initial choice of which credit cards to hold.

In the remainder of this section, we discuss our treatment intensity measures, our identifying assumptions, and our regression and matching estimators that compare individuals with the same set of credit cards but different proportions of debt on each card. Additional details and econometric proofs are in the Online Appendix.

Treatment Intensity Calculation: We construct our potential interest write-down and payment reduction measures using the difference between hypothetical treatment and hypothetical control repayment program offers for each individual in our sample. That is, we first calculate the hypothetical interest write-downs and minimum payments for all individuals in our sample as if they had been assigned to the control group, using exactly the same calculation and information that MMI uses to calculate the terms of the structured repayment program in the control group. We then calculate the hypothetical write-downs and minimum payments for those same individuals as if they had been assigned to the treatment group, now using the exact same calculation and information that MMI uses in the treatment group.[10] Finally, we calculate the difference between these hypothetical control and treatment write-downs and hypothetical control and treatment payment reductions for each individual in our sample, dividing each measure by the maximum possible

with a non-participating issuer. The fact that these borrowers were only partially treated by the experiment means that standard intent-to-treat estimates will understate the true impact of targeted debt relief.

[10]Our data contain 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. To confirm the accuracy of our calculations, Appendix Figure A3 plots predicted DMP characteristics against actual DMP characteristics for the control and treatment groups. Actual monthly payments are the maximum of the required minimum payment and the borrower's preferred minimum payment, and are available for all borrowers. Actual plan length is only available for borrowers completing the repayment program, and is a function of the actual minimum payment, the actual interest rate, and any extra payments made by the borrower to shorten the repayment period. In other words, the actual plan length should be weakly shorter than the predicted plan length. Actual interest rates are not recorded in the MMI data and are not included in Appendix Figure A3. There is nearly a one-to-one relationship between predicted payments and actual payments in both the control and treatment groups. There is a similarly tight relationship between predicted plan length and actual plan length for shorter programs, with a weaker relationship for the longer programs where borrowers are more likely to make extra payments.

change in the treatment group (9.9 percentage points for the interest write-downs and 0.5 percentage points for the minimum payment reductions). Following this renormalization, we can interpret our estimates as the effect of the maximum possible interest write-down in the treatment group and the maximum possible payment reduction in the treatment group.

Treatment Intensity Variation: Appendix Figure A4 plots the distribution of potential interest write-downs and payment reductions in our estimation sample. As discussed above, the variation in potential treatment intensity comes from the fact that each of the credit card issuers participating in the experiment offered a different combination of interest write-downs and minimum payment reductions to treated borrowers, and individual borrowers made different decisions about how much to borrow from each of these card issuers before the experiment. These decisions translated into approximately 50,000 different unique combinations of potential interest write-downs and payment reductions in our sample, with considerable support over the entire range of possible treatment intensities.[11,12]

The variation in potential treatment intensity is also large in economic terms. The difference between the 25th percentile and 75th percentile interest write-downs within the treatment group, for example, 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).

As discussed above, the critical identification assumption underlying our research design is that the causal effects of the interest write-downs and payment reductions are uncorrelated with this variation in potential treatment intensity. To better understand what this identifying assumption entails, Table 3 reports results from an OLS regression of potential treatment intensity on all baseline characteristics and outcomes. We begin by controlling only for date-by-state-by-reference group "randomization strata" fixed effects that account for the level at which individuals are randomly

[11] Appendix Table A3 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 interest 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 interest write-down (9.9 percentage points) and no minimum payment reduction to treated borrowers, while another offered the largest minimum payment reduction (0.5 percentage points) and the smallest interest write-down (4.0 percentage points). 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 card issuer. Consistent with this explanation, there are no systematic patterns between the generosity of the interest write-downs and minimum payment reductions offered before the experiment and the generosity of the treatments during the experiment.

[12] There is considerable bunching at the origin in Appendix Figure A4, as approximately 25 percent of borrowers in our sample had no credit card debt with the card issuers participating in the experiment and, as discussed above, were offered the "control" repayment program even when they were assigned to the treatment group. There are also four higher density "lines" that trace out the potential treatment intensities for individuals who have a mix of debt with one participating card issuer and one or more non-participating card issuers. For example, the vertical line running from the origin to the upper left corner of Appendix Figure A4 consists of individuals holding debt with one or more of the card issuers offering a 9.9 percentage point write-down and 0.0 percentage point payment reduction and one or more non-participating card issuers. The greater the proportion of debt with the participating card issuer, the larger the hypothetical interest write-down the individual would receive if treated.

assigned to counselors. Borrowers with larger potential interest write-downs have less debt in repayment, have higher baseline earnings, and are somewhat more likely to be matched to the TU credit report data (column 2). Borrowers with larger potential minimum payment reductions are also older and less likely to own a home (column 4). The proportion of debt with credit card issuers participating in the experiment is also (mechanically) correlated with both the potential interest write-downs and potential minimum payment reductions. The p-value from an F-test of the joint significance of all of the variables listed is 0.000 (column 2) and 0.002 (column 4) for the interest write-downs and payment reductions, respectively, even when omitting the proportion of debt with participating card issuers. These results suggest that our identifying assumption is extremely strong when we control only for the randomization strata fixed effects, requiring that potential treatment intensity is as-good-as-randomly assigned with respect to treatment effect heterogeneity, even though it is not as-good-as-randomly assigned with respect to observable characteristics such as baseline earnings or homeownership.[13]

Estimation and Identifying Assumptions: We therefore develop two complementary estimators that compare individuals with the exact same set of credit cards, but different proportions of debt on each credit card. Both estimators weaken the key identifying assumption by only requiring that the proportion of debt with each card issuer be as-good-as-randomly with respect to treatment effect heterogeneity, not the initial choice of which credit cards to hold. We do this by controlling for the number and identity of each individual's credit card issuers, or what we call the "creditor risk set." For each individual, we define the creditor risk set as the list of all credit cards that an individual holds from participating card issuers, as well as an indicator for holding at least one credit card from a non-participating card issuer. With eleven participating card issuers and one aggregate non-participating card issuer, there are $2^{12} = 4096$ possible creditor risk sets, although only 436 creditor risk sets include at least one treatment observation and at least one control observation in our estimation sample.

Our first estimator uses the standard regression framework to identify the causal effects of the interest write-downs and minimum payment reductions by directly controlling for the creditor risk set. Our regression estimator allows us to estimate a weighted average of the risk set-specific treatment effects by simply adding creditor risk set fixed effects to Equation (1), where the weights are proportional to the variation in $WriteDown_i$ and $Payment_i$ in each risk set. The regression estimator is simple to implement, the standard errors can be calculated using conventional statistical packages, and it is straightforward to examine treatment intensities are correlated with baseline covariates and outcomes after conditioning on the creditor risk set fixed effects. But, the weighting scheme underlying the regression estimator may not be economically relevant, complicating the interpretation of these estimates. In addition, the weighting scheme used for the interest write-downs and minimum payment reductions estimates may not be identical, as the relative variation

[13]Appendix Table A4 reports results from OLS regressions of an indicator for having any debt with each card issuer on all baseline characteristics and outcomes, i.e. the extensive margin version of Table 3. The results largely follow those reported in Table 3.

in $WriteDown_i$ and $Payment_i$ may differ across the creditor risk sets.

In contrast, our second estimator builds on the matching framework developed by Angrist (1998) and Abadie and Imbens (2002), among many others, by estimating Equation (1) separately within each creditor risk set and then imposing our own weighting scheme. In practice, we use the number of treated borrowers in each risk set as weights, yielding estimates with a clear economic interpretation and identical weights for the interest write-downs and payment reductions estimates. We calculate standard errors using a Bayesian bootstrap procedure that adjusts for first-step error in the estimation of the risk-set-specific estimates (Rubin 1981).[14] While the matching estimator allows us to impose identical and economically relevant weights, it may be infeasible when there are many small creditor risk sets. Testing whether the treatment intensities are correlated with baseline covariates and outcomes within each creditor risk set is particularly challenging in our setting, for example, as these tests require a relatively large number of observations in each risk set. We therefore view the regression and matching estimators as complementary and present estimates from both throughout much of the paper. See the Online Appendix for additional details on these estimators and econometric proofs.

Our regression and matching estimators identify the causal effects of interest write-downs and minimum payment reductions if the following conditions hold within the creditor risk sets: (1) treatment eligibility only impacts outcomes through the change in interest write-downs and minimum payment reductions, (2) the causal effects of the write-downs and payment reductions are linear and additively separable, and (3) the causal effects of the write-downs and payment reductions are uncorrelated with treatment intensity.[15] We now consider whether each of these conditions holds in our data.

Exclusion Restriction: Table 2 verifies that treatment eligibility is randomly assigned in the full sample after we condition on date-by-state-by-reference group fixed effects that account for the level at which individuals are randomly assigned to counselors. Column 2 of Table 2 reports results from an OLS regression of treatment eligibility on all baseline characteristics and these randomization strata fixed effects. Standard errors are clustered at the counselor level. The means of all of the baseline variables are similar in the treatment and control groups and the p-value from an F-test of the joint significance of all of the variables listed is 0.422, suggesting that the randomization was successful. We find similar results if we add the creditor risk set fixed effects, while Appendix Table A5 verifies that the randomization was also successful within narrowly defined treatment intensity bins.

[14] The Bayesian bootstrap smooths bootstrap samples by reweighting rather than resampling observations, preventing the omission of small randomization strata that would occasionally be dropped in a standard non-parametric bootstrap. The Bayesian bootstrap used here is implemented by drawing vectors of Dirichlet(1,...,1) weights, re-estimating Equation (1) for all risk sets using the Dirichlet weights, and then aggregating the risk-set-specific estimates using the number of treated borrowers as weights. We repeat this procedure 500 times and report the standard deviation of the bootstrap estimates.

[15] The regression estimator also relies on a functional form assumption that ensures that all baseline controls enter linearly. Both estimators also require that the correlation between potential outcomes and the potential treatment intensity measures is linear and additively separable. See the Online Appendix for additional details.

Linear and Additively Separable Treatment Effects: The second condition needed for our research design is that the causal effects of the write-downs and payment reductions are linear and additively separable. To partially test this assumption, Appendix Table A6 presents non-parametric estimates of the interest write-downs and minimum payment reductions in our experiment. We estimate these non-parametric treatment effects by grouping our treatment intensity measures into equally sized bins for both the interest write-downs and minimum payment reductions. We report the interaction of treatment eligibility and each treatment intensity bin, controlling for the treatment intensity bins and the randomization strata fixed effects described above. The results are consistent with linear and additively separable treatment effects, although large standard errors mean that we cannot rule out modest non-linearities or interaction effects. We present additional evidence in support of linear treatment effects in our robustness checks.

Conditional Independence: The final condition needed to interpret our estimates as the causal effects of the interest write-downs and payment reductions is that these treatment effects are uncorrelated with treatment intensity within each creditor risk set. Columns 3 and 5 of Table 3 provide a partial test of this assumption, reporting results from an OLS regression of potential treatment intensity on the baseline characteristics and outcomes from Table 2, the randomization strata fixed effects, and the creditor risk set fixed effects. Controlling for these creditor risk sets fixed effects, there is no discernible relationship between potential treatment intensity and the baseline controls and the p-value from an F-test of the joint significance of all of the variables listed (omitting the proportion of debt with participating issuers) is 0.880 for the interest write-downs (column 3) and 0.895 for the payment reductions (column 5). These results suggest that our identifying assumption is more likely to hold after we account for the creditor risk sets. In robustness checks, we show that our results are also robust to a set of overidentification tests once we condition on the creditor risk set fixed effects.[16]

III. Results

In this section, we examine the effects of targeted debt relief using the empirical strategy described above. We first analyze the effects of targeted debt relief on debt repayment, before turning to its effects on bankruptcy, financial outcomes, and labor market outcomes.

A. Debt Repayment

Table 4 presents estimates of the impact of being offered more generous interest write-downs and minimum payment reductions on starting and completing a structured repayment program over about the next five years. Columns 1 and 5 report intent-to-treat estimates of the impact of treatment eligibility. Columns 2 and 6 report our baseline estimates of treatment eligibility interacted

[16] We are unable to use our matching estimator to test whether the treatment intensities are correlated with baseline covariates and outcomes, as these tests require a relatively large number of observations in each risk set. We therefore use our regression estimator to implement these baseline tests.

with the potential interest write-down and treatment eligibility interacted with the potential minimum payment reduction. The potential interest write-down and payment reduction variables are scaled such that our estimates can be interpreted as the causal effect of being offered the maximum interest write-down in the treatment group and maximum payment reduction in the treatment group. Columns 3 and 7 report estimates from our preferred regression specification that adds the creditor risk set fixed effects described above. Columns 4 and 8 report estimates from our matching specification that uses the number of treated individuals in each creditor risk set cell as weights. All specifications control for potential treatment intensity, the baseline controls in Table 2, and the date-by-state-by-reference group randomization strata fixed effects that account for the level at which individuals are randomly assigned to counselors. The ITT, baseline, and preferred regression specifications report standard errors clustered at the counselor level, and the matching specifications report standard errors from the Bayesian bootstrap procedure described above.

The intent-to-treat estimates show that 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 1.9 percentage points (column 1), a 5.8 percent increase from the control mean of 32.8 percent. The probability of finishing a repayment program also increased by 1.0 percentage points (column 4), a 7.0 percent increase from the control mean of 14.3 percent. These results confirm that more generous debt relief increases debt repayment at the margin but, as discussed above, do not distinguish between the effects of the interest write-downs and minimum payment reductions.[17]

The reduced form estimates in columns 2-4 and 6-8 improve upon our intent-to-treat estimates by exploiting cross-sectional variation in treatment intensity to identify the separate impact of the interest write-downs and minimum payment reductions. We find that the interest write-downs significantly increased debt repayment in both the short- and long-run despite not taking effect until three to five years after the experiment. In our preferred regression specification controlling for creditor risk set fixed effects, for example, we find that the maximum interest write-down in the treatment group (9.90 percentage points) increased the probability of starting a structured repayment program by 3.9 percentage points (column 3), an 11.9 percent increase from the control mean.The probability of finishing the program also increased by 2.7 percentage points (column 6), an 18.9 percent increase from the control mean. Our matching specification yields similar results, implying that the maximum interest write-down in the treatment group increased the probability of starting a structured repayment program by 5.1 percentage points (column 4), and of finishing the repayment program by 2.9 percentage points (column 8). Taken together, the results from Table 4 suggest that there may be significant benefits of debt relief targeting longer-run financial

[17]Appendix Table A7 presents "first stage" estimates of the impact of treatment eligibility on interest rates and monthly minimum payment amounts. Treatment eligibility decreases the offered interest rate by 2.7 percentage points, a 31.7 percent change from the control mean of 8.5 percent, and decreases the monthly payment amount by 0.1 percentage points (times the total debt amount), a 3.8 percent change from the control mean of 2.6 percent. Conditional on having any debt with a participating card issuer, treatment eligibility decreases the interest rate by 3.5 percentage points, a 40.2 percent change, and decreases the monthly payment amount by a slightly higher 0.1 percentage points, a 4.8 percent change.

constraints such as debt overhang.

In sharp contrast, we find no positive effects of the minimum payment reductions targeting short-run liquidity constraints. In our preferred regression specification with creditor risk set fixed effects, we find that the maximum monthly payment reduction in the treatment group (0.5 percentage points) increased the probability of completing a structured repayment program by only 0.3 percentage points (column 6). The effect on starting a repayment program is slightly larger at 1.3 percentage points (column 3), but still statistically insignificant. While the 95 percent confidence intervals include modest effects (e.g., a 3.1 percent increase in program completion), all of the minimum payment estimates are statistically differentiable from the interest write-down estimates at the 1 percent level. Our matching specification again yields similar results, implying that the maximum payment reduction actually decreased the probability of starting a structured repayment program by a statistically insignificant 0.9 percentage points (column 4), and of finishing the repayment program by a statistically insignificant 0.8 percentage points (column 8). 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.

To better understand these effects, Figure 1 plots the control mean and the treatment group means implied by estimated treatment effects at each percentile of debt repayment. In other words, we estimate treatment effects for each percentile of debt repayment, adding the estimated effect of the interest write-downs and minimum payment reductions to the control mean at each percentile. We focus on our preferred regression specification with creditor risk set fixed effects. Consistent with the results from Table 4, the effect of the interest write-downs are economically and statistically significant at each point in the distribution. The immediate increase in debt repayment estimated in Table 4 remains roughly constant throughout the repayment program, with only a modest fade out of the effects in the last 25 percent of the distribution. In contrast, the small (but not statistically significant) effect of the maximum payment reduction in the short run fades out relatively quickly, with no discernible difference between the treatment and control groups after about the 25th percentile. It is also worth noting that both treatment and control borrowers exit the repayment program at high rates, with only 14.3 percent of the control group completing the repayment program. In Section IV, we will discuss what mechanisms are most consistent with these patterns.

B. Bankruptcy

Table 5 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 10.5 percent of the control group files for bankruptcy in the first five years following the experiment. In our setting, we interpret bankruptcy as an alternative and potentially more costly form of debt forgiveness and debt restructuring.[18]

The intent-to-treat estimates in Table 5 show that treatment eligibility decreased the probability of filing for bankruptcy protection by 0.60 percentage points over the first five years following the experiment in the pooled sample (column 1), a 5.7 percent decrease from the control mean of 10.5 percent. The effects of treatment eligibility on bankruptcy filing are again driven by the interest write-downs. Over the first five years following the experiment, we find that the maximum interest write-down in the treatment group decreased the probability of filing for bankruptcy by 1.3 to 3.0 percentage points in the pooled sample (columns 3 and 4), a 12.4 to 28.6 percent decrease from the control mean. Conversely, the maximum payment reduction in the treatment group actually increased the probability of filing for bankruptcy over the first five years following the experiment by 1.3 to 2.3 percentage points (columns 3 and 4), a 12.4 to 21.9 percent increase.

In Appendix Table A8, we find that the effects of the interest write-downs on bankruptcy filing are largest in the first two to three years following the experiment, likely because this is the time period when the control group is most likely to file for bankruptcy. Appendix Table A9 also reveals that there are modestly larger effects of the interest write-downs for individuals contacting MMI prior to the 2005 Bankruptcy Reform that increased the financial and administrative costs of filing for bankruptcy protection (column 3), likely because it became more difficult for the control group to file for bankruptcy following the reform. In contrast, the estimated effects of the minimum payment reductions are statistically similar both across years and before and after the 2005 Bankruptcy Reform, in part because we lack the statistical power to detect modest differences in the estimated effects.

C. Collections Debt and Credit Score

Table 6 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.

There are no statistically or economically significant effects of treatment eligibility on collections debt or credit scores over the first five years following the experiment (columns 1 and 5). The maximum interest write-down in the treatment group decreased the probability of having nonzero collections debt by a statistically insignificant 0.6 to 2.7 percentage points (columns 3 and 4), a 1.5 to 6.9 percent change, and increased average credit scores by an insignificant 1.9 to 3.5 points (columns 7 and 8). The maximum payment reduction had little to no impact on collections debt (columns 3 and 4) and a statistically insignificant negative effect on credit scores (columns 7 and 8).

[18]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 2016) and new employment opportunities (Bos, Breza and Liberman 2018; 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, Keys and Mahoney 2017).

Thus, while underpowered, these results are broadly consistent with our earlier results showing that the interest write-downs modestly improved borrowers' financial health while the lower minimum payments had little positive effect on borrowers.

D. Labor Market Outcomes

Table 7 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 2019; Bos, Breza and Liberman 2018; Herkenhoff, Phillips and Cohen-Cole 2016; Dobbie et al. 2016) or productivity (e.g., Mullainathan and Shafir 2013).

There are no statistically or economically significant effects of treatment eligibility on either employment or earnings over the first five years following the experiment (columns 1 and 5). The estimated effects of both the interest write-downs and minimum payment reductions are also small and imprecisely estimated, with opposite signs on the employment and earnings estimates in all specifications. For our preferred regression specification with creditor risk set fixed effects, the 95 percent confidence interval for the employment estimate ranges from -0.8 to 2.0 percentage points (column 3), while the 95 percent confidence interval for the earnings estimate ranges from -\$1,666 to \$168 (column 6). For the maximum payment reduction, the 95 percent confidence interval ranges from -2.5 to 1.1 percentage points for the employment estimate (column 3) and -\$707 to \$1,759 for the earnings estimate (column 6). None of the estimates suggest economically meaningful effects on labor market outcomes in the pooled sample.

To better understand these results, Panel A of Appendix Table A10 presents results separately for borrowers who were and were not employed in the year prior to the experiment. We again focus on our preferred regression specification with creditor risk set fixed effects. We find that the maximum interest write-down decreased annual earnings by \$2,250 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 interest write-downs may decrease labor supply for borrowers most on the margin of any work.[19]

[19] 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 maximum write-down in the treatment group forgives about 25.84 percent of unsecured debt. In addition, Chapter 13 bankruptcy protects future wages from garnishment, while our experiment did not.

E. Subsample Results

Table 8 presents subsample estimates for our preferred regression specification by baseline debt-to-income and baseline credit scores, which we interpret as proxies for baseline financial distress and baseline liquidity, respectively. The split by baseline debt-to-income is motivated by the fact that only the most financially distressed borrowers were meant to be eligible for treatment in the original experimental design and, following the experiment, many credit card issuers began offering more borrower-friendly terms to financially distressed borrowers. The split by baseline credit scores is motivated by the idea that individuals who are not liquidity constrained are unlikely to benefit from the minimum payment reductions.

The effects of the interest write-downs are consistently larger for individuals with above median debt-to-income, although the differences are not statistically significant for all outcomes. For example, the maximum write-down increases the probability of starting and completing a repayment program by 6.2 percentage points (18.9 percent) and 4.4 percentage points (30.8 percent), respectively, for individuals with above median debt-to-income. In comparison, the maximum write-down only increases the probability of starting and completing a repayment program by 1.2 percentage points (3.7 percent) and 0.7 percentage points (4.9 percent) for those with below median debt-to-income. For those with above median debt-to-income, the maximum write-down also decreases the probability of having any collections debt by 1.2 percentage points (3.1 percent) and increases the probability of being employed by 4.2 percentage points (5.1 percent), compared to small and statistically insignificant effects for those with below median debt-to-income. In contrast, the effects of the minimum payment reductions are small and either of the wrong sign or statistically insignificant for both groups.

There is a similar, if less stark, pattern by baseline credit scores. The maximum interest write-down increases the probability of completing a repayment program, for example, by 5.8 percentage points (40.6 percent) for individuals with above median credit scores, compared to only 0.9 percentage points (6.3 percent) for those with below median credit scores. The effects are more comparable for starting a repayment plan, however, suggesting that liquidity may be particularly important for successfully making all of the required payments. The effects on other outcomes are also larger for the high credit score group, but none of the differences are statistically significant. We also find similar effects of the payment reductions among high and low credit score borrowers, although the effects are generally more positive for low credit score borrowers, consistent with the idea that the payment reductions are most important for liquidity constrained borrowers. However, none of the estimates suggest economically significant benefits of the payment reductions for these liquidity constrained borrowers.

Appendix Table A10 presents additional subsample estimates for our preferred regression specification by gender, ethnicity, and baseline homeownership. For each of these subgroups, there are no clear theoretical predictions as to which group will benefit most from the experiment. The interest write-downs have somewhat larger effects for women compared to men, but there are no systematic patterns by either ethnicity or baseline homeownership. Moreover, all of these results should be interpreted with some caution given that we are likely to find a number of statistically significant estimates purely by chance when performing multiple hypothesis tests. We therefore interpret these results as suggesting relatively similar effects of targeted debt relief across these groups.

F. Robustness and External Validity

In this section, we discuss robustness checks and how the details of the experimental design may affect the external validity of our results.

Overidentification Tests: Appendix Table A11 presents results from a set of overidentification tests of our main results. Panel A replicates our preferred regression estimates controlling for creditor risk set fixed effects. Panel B adds controls for treatment eligibility interacted with indicator variables for gender, race, baseline homeownership, baseline credit scores, baseline earnings, and baseline debt-to-income. Panel C instead adds controls for treatment eligibility interacted with credit card issuer fixed effects. Panel D adds both the treatment eligibility x baseline demographic variables and treatment eligibility x credit card issuer variables. Consistent with our identifying assumption, our main results are generally robust to the inclusion of treatment eligibility x baseline demographic effects, treatment eligibility x credit card issuer effects, and both treatment eligibility x baseline demographic and treatment eligibility x issuer 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 generally not statistically significant. Taken together, we interpret these results as indicating that our identifying assumption is likely to hold after we account for the creditor risk sets described above.

Permutation Test: Appendix Table A12 presents a second set of robustness checks where the p-values from our preferred regression specification are calculated using a non-parametric permutation test that accounts for the fact that we have run regressions with a number of outcomes and subsamples. That is, 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. We find that our main results are robust to this alternative method of calculating p-values. If anything, we obtain smaller p-values from the non-parametric permutation procedure than implied by conventional standard errors.

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 interest 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 an interest 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 existing credit counseling organizations. 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.[20]

Non-Linear Treatment Effects: Another potential concern is that we estimate the impact of interest 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 an interest write-down or the first dollar of a payment reduction using our experimental data. We also do not observe the kind of extremely large 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 interest write-downs or the minimum payment reductions. In addition, we assume linear treatment effects when extrapolating the effects of maximum treatment intensities in our main results. Appendix Table A6 provides some evidence of linear effects, but these "binned" estimates are too imprecise to conclusively rule out non-linear treatment effects.

To provide additional evidence on this issue, Appendix Figure A5 presents non-parametric estimates of the interest write-downs and minimum payment reductions in our experiment. We estimate these non-parametric treatment effects by grouping our treatment intensity measures into equally sized bins for both the interest write-downs and minimum payment reductions, but, unlike Appendix Table A6, do not allow for interactions between the interest write-downs and minimum payment reductions. We report the interaction of treatment eligibility and each treatment intensity bin, controlling for potential treatment intensity, the randomization strata fixed effects, and the creditor risk set fixed effects. We also plot the OLS best-fit line weighted by the standard error for each point estimate. The results in Appendix Figure A5 are consistent with linear treatment effects over the range of treatment intensities observed in our data, as well as the results from Appendix Table A6 discussed earlier. None of our results suggest the kind of non-linear treatment effects that would bias our estimates or impact out-of-sample predictions based on our experimental estimates. Of course, we cannot test whether there are non-linear effects for treatment intensities that we do not observe in the data and all of our results should be interpreted with this caveat in mind.

[20] There is a rapidly expanding literature estimating framing effects in financial settings. Bertrand and Morse (2011) find that payday borrowers are more responsive to information on fees compared to information on interest rates, perhaps because payday borrowers tend to have low levels of financial literacy (e.g., Lusardi and Scheresberg 2013). Conversely, Agarwal et al. (2015) and Keys and Wang (2019) find relatively modest effects of the behavioral nudges introduced during the 2009 CARD Act on credit card repayment behavior, although the precise effects of the nudges are difficult to determine due to the other regulatory changes introduced as a part of the Act. Outside of the United States, Bertrand et al. (2010) find large effects of seemingly irrelevant "frames" and "cues" on consumer loan demand. See DellaVigna (2009) for a review of the broader literature on framing.

Representativeness of the Sample: A final concern is that we estimate the impact of targeted debt relief within the sample of individuals who pass the screening procedure described above. Recall that credit counseling agencies screen potential clients to assess whether the individual 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. It is possible that potential clients who pass this screening process are, for example, less liquidity constrained or more forward-looking than the broader sample of individuals seeking targeted debt relief.

To provide some evidence on the types of individuals entering our experimental sample, Appendix Table A13 provides descriptive statistics for our experimental sample, a random sample of all credit users, a random sample of credit users with a serious delinquency occurring in the next calendar year, and a sample of credit users with a bankruptcy flag in the next calendar year.[21] Information on all baseline outcomes comes from the TransUnion credit records described above. Appendix Table A13 reveals that while our experimental sample is much more financially distressed than the typical credit user, it is broadly similar to other financially distressed populations in the United States, at least on observables. In our experimental sample, for example, the average credit score in the year before contacting MMI is 586.4, compared to a credit score of 572.3 in the delinquency sample and 580.8 in the bankruptcy sample. Credit card balances are somewhat higher in the experimental sample compared to the delinquency and bankruptcy samples, while credit card utilization and delinquencies are both somewhat lower in the experimental sample. For both auto and mortgage loans, the experimental sample falls in between the delinquency and bankruptcy samples. In sum, the experimental sample appears approximately representative of the financially distressed population in the United States.

IV. Mechanisms

In this section, we investigate the potential mechanisms that can explain our interest write-down and minimum payment results.

A. Overview

In theory, the interest write-downs can impact debt repayment through two distinct effects. The first is 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. The second is 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. We can test the relative importance of these competing channels using treatment

[21] Information on all credit users comes from column 1 of Table 1 in Dobbie et al. (2016), information on credit users with a serious delinquency comes from unreported results available from Dobbie, Keys and Mahoney (2017), and information on bankruptcy filers comes from columns 3-4 of Table 1 in Dobbie et al. (2016).

effects at the beginning and end of the repayment program. The interest write-downs do not affect the minimum payment requirements early in the repayment program, leaving forward-looking behavior as the only explanation for any interest write-down effects early in the program. We can therefore test for these forward-looking effects using interest write-down treatment effects at the end of the repayment program for the interest write-down group (but not the control group). Then, because the total interest write-down estimate includes the effects of both channels, we can estimate the exposure effect alone using the difference between the total interest write-down estimate and the forward-looking estimate.[22]

The minimum payment reductions can similarly impact debt repayment through two distinct effects. The first is a liquidity effect that, in general, 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. The second is another 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. Following the same logic as above, we can test for liquidity effects using payment reduction treatment effects at the end of the repayment program for the control group (but not the payment reduction group), as the only difference between the treatment and control groups to this point is the lower minimum payment. Then, because the total payment reduction estimate includes the effects of both channels, we can estimate the exposure effect alone using the difference between the total payment reduction estimate and the liquidity estimate.[23]

[22]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 not contaminated by dynamic selection over time. Dynamic selection can, however, bias our estimates of the exposure effect because we are comparing treatment effects at different points in time. For example, it is plausible that the interest 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 A14 examines the characteristics of control and treatment borrowers completing the repayment program (i.e., "compliers"). Column 1 presents the mean for control compliers. Column 2 reports results from a regression of each baseline variable on treatment eligibility within the complier population. Columns 3-4 report analogous results from a regression of each baseline variable on treatment eligibility interacted with potential treatment intensity. All specifications control for strata fixed effects and cluster standard errors at the counselor level. There is no evidence that the experiment significantly altered the composition of borrowers completing the repayment program, either overall or through the individual effects of the interest write-downs and minimum payment reductions. Given these results, it appears unlikely that our estimates of the exposure effect will be significantly biased by dynamic selection.

[23]Our estimates of the forward-looking and liquidity effects are lower and upper bounds of the true effects, respectively. This is because the control group can still make forward-looking default decisions after the end of the repayment program for the interest write-down group, while the payment reduction group can still make liquidity-based default decisions during their remaining time in the program. For the same reasons, our estimate of the mechanical exposure effect is an upper bound for the interest write-downs and a lower-bound for the payment reductions.

B. Estimation

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 interest 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 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 the treatment plan length for individuals with the shorter treatment plans (i.e. relatively larger write-downs) and payment at the control program length for individuals with the longer treatment plans (i.e. relatively larger minimum payment reductions). Third, we estimate treatment effects using this new indicator variable as the dependent variable. These reduced form estimates measure the effect of write-downs at the treatment plan length and the effect of lower minimum payments at the control plan length. Fourth, we take the difference between the reduced form treatment effects for full repayment estimated in Table 4 and the new reduced form treatment effects estimated at the shorter of the treatment and control plan lengths. 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. We control for the baseline controls listed in Table 2, randomization strata fixed effects, and creditor risk set fixed effects throughout.

C. Results

Table 9 presents estimates of the forward-looking, liquidity, and exposure effects for both the interest write-downs and minimum payment reductions. Column 1 replicates our regression estimates with creditor fixed effects from column 7 of Table 4, 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.

We find that the positive effects of the interest write-downs can be 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. Our estimates suggest that at least 85.2 percent of the interest write-down effect is due to the decrease in forward-looking defaults at the beginning of the repayment program (column 2). Decreased exposure to risk at the end of repayment can explain a maximum of 14.8 percent of the write-down effect (column 4), with the 95 percent confidence interval including estimates of up to 43.9 percent of the total reduced form effect.

Figure 1 provides additional evidence in favor of forward-looking effects. First, there is an immediate impact of the interest write-downs on repayment, indicating forward-looking behavior at program sign up. Second, the effects of the interest 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 interest write-down results, such as borrowers being "tricked" into signing up for the repayment program by some feature of the experimental design.

We also find that the null effect of the minimum payment reductions can be explained by the unintended, negative effect of increasing the number of months a borrower remains in the repayment program. Our estimates suggest that debt repayment increases by about 0.09 percentage points due to the liquidity effect (column 3), with the 95 percent confidence interval including effects as large as 3.6 percentage points. However, this positive liquidity effect is nearly exactly offset by the negative exposure effect (column 4). 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 precise zero effect of the payment reductions in the long run.

The results from Table 9 help to reconcile our findings with 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.

V. 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 interest write-downs targeting longer-run debt overhang. We find that the interest 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 interest write-downs, at least in part due to the perceived unimportance of longer-run constraints such as debt overhang.[24,25] During the financial crisis, a group of credit card issuers asked for these regulations to be relaxed so that 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 open important question is whether the increased repayment rates documented in our analysis are, on net, larger than the costs of the interest write-downs. While a comprehensive cost-benefit analysis is beyond the scope of this paper, we consider a partial back-of-the-envelope calculation that takes into account the ex-post impact of the interest write-down treatment and the average repayment rate in the control group. To simplify the calculation, we assume that the lender is risk neutral and does not discount future payments. Based on these tentative calculations, we estimate that lenders recoup $3,195 from the typical borrower assigned to the control group, compared to only $3,072 from the typical borrower assigned to the treatment group.[26] In other words, lenders lose just over $100 in expectation when giving an interest write-down. Our calculations therefore suggest that lenders have little reason to provide more generous write-downs to all borrowers. Consistent with this finding, many of the credit card issuers in our sample began offering more borrower-friendly terms only to the highest-debt borrowers following the experiment, with the terms for most borrowers remaining the same.

There are three important caveats to our analysis. First, we are not able to estimate the impact of targeted debt relief on ex-ante borrower behavior or ex-ante borrowing costs or borrowing limits. Our analysis will therefore overstate the benefits of more generous interest write-downs if the ex-ante availability of debt relief distorts borrower behavior in such a way that lenders must increase interest rates or decrease credit supply. Second, there may 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. Given these concerns, we are unable to determine the full welfare consequences of targeted debt relief using our research design.

[24] 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.

[25] 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

[26] Following our calculations in Table 1, we use a baseline interest rate of 9.90 percent and the control means for debt ($18,470), minimum payment (2.38 percent of debt), and monthly default rate during the repayment program (1.12 percent). The mean completion rate for the control group is 14.3 percent while the mean completion rate for borrowers with the maximum interest write-down is 17.0 percent (column 6 of Table 4). We find nearly identical results using the percent of debt repaid results from Appendix Table A15.

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Table 1: Examples of the Randomized Treatments

Treatments		Program Characteristics			Discounted Cost to Lender		
Interest Write-Down	Payment Reduction	Minimum Payment	Financing Fees	Total Months	0% Disc. Rate	9.9% Disc. Rate	20% Disc. Rate
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
–	–	\$439.59	\$4,302	51.80	–	–	–
9.90%	–	\$439.59	\$0	42.00	\$3,621	\$2,175	\$1,255
–	0.50%	\$347.24	\$5,947	70.32	\$461	\$1,257	\$1,636

Notes: This table illustrates how treatment eligibility impacts repayment program characteristics and lender costs. The first row reports program characteristics for the baseline case in the control group. The second row reports program characteristics applying the maximum interest rate write-down in the treatment group. The third row reports program characteristics applying the maximum minimum payment reduction in the treatment group. Columns 1-2 describe the interest write-down and payment reduction received if in the treatment group. Column 3 presents the minimum required payment in dollars. Column 4 presents the total cost of all interest rate payments and late fees assuming no early payments. Column 5 presents the total number of months before the program is complete assuming no early payments. Column 6-8 present the net present cost of providing each treatment relative to the baseline case, using the control mean for the monthly default rate during the repayment program (1.12%). All program characteristics and lender costs are calculated using the control means for debt (\$18,470) and minimum payment (2.38% of debt), and a baseline interest rate of 9.90%.

Table 2: Descriptive Statistics and Balance Tests

	Control Mean	Treatment vs. Control
Baseline Characteristics	(1)	(2)
Age	40.827	-0.0004
		(0.0002)
Male	0.365	-0.0026
		(0.0045)
White	0.640	-0.0066
		(0.0076)
Black	0.168	-0.0089
		(0.0089)
Hispanic	0.090	-0.0203
		(0.0105)
Number of Dependents	2.179	-0.0016
		(0.0017)
Homeowner	0.419	0.0056
		(0.0068)
Renter	0.435	0.0017
		(0.0062)
Monthly Income (1,000s)	2.498	-0.0001
		(0.0018)
Debt in Repayment (1,000s)	18.470	0.0003
		(0.0001)
Percent with Exp. Creditors	0.445	0.0010
		(0.0076)
Baseline Outcomes		
Bankruptcy	0.003	-0.0045
		(0.0359)
Nonzero Collections Debt	0.248	0.0007
		(0.0057)
Credit Score	586.665	-0.0000
		(0.0000)
Employment	0.848	0.0034
		(0.0074)
Earnings (1,000s)	23.702	-0.0001
		(0.0001)
Data Quality		
Matched to SSA data	0.952	-0.0787
		(0.1832)
Matched to TU Data	0.869	-0.0052
		(0.0225)
p-value from joint F-test	–	[0.4215]
Number of Observations	39,855	78,438

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. Column 1 reports the mean for the control group. Column 2 reports the difference between the treatment and control groups controlling for randomization strata fixed effects and clustering standard errors at the counselor level. The p-value is from an F-test of the joint significance of the variables listed.

Table 3: Correlates of Potential Treatment Intensity

	Control Mean	Max Interest Write-Down x 100		Max Payment Reduction x 100	
Baseline Characteristics	(1)	(2)	(3)	(4)	(5)
Age	40.827	-0.0155	-0.0092	0.0441	0.0012
		(0.0177)	(0.0140)	(0.0210)	(0.0150)
Male	0.365	-0.2777	-0.0026	0.6668	-0.0338
		(0.4948)	(0.3832)	(0.4889)	(0.3502)
White	0.640	0.1346	0.1671	0.5885	0.0425
		(0.8282)	(0.6002)	(0.9558)	(0.6777)
Black	0.168	0.1433	-0.2747	0.4299	0.1025
		(0.9857)	(0.7208)	(0.2591)	(0.7089)
Hispanic	0.090	0.1414	0.2718	-0.3346	0.0623
		(1.0137)	(0.7663)	(1.0847)	(0.8168)
Number of Dependents	2.179	-0.1782	-0.1062	-0.0452	-0.0981
		(0.1693)	(0.1223)	(0.1830)	(0.1253)
Homeowner	0.419	-0.5708	-0.2717	-1.2060	0.0762
		(0.7538)	(0.5546)	(0.7574)	(0.5527)
Renter	0.435	0.1596	-0.0286	0.7964	0.6429
		(0.6417)	(0.4702)	(0.6885)	(0.4819)
Monthly Income (1,000s)	2.498	0.1538	-0.0950	0.0762	0.0669
		(0.1833)	(0.1429)	(0.1956)	(0.1486)
Debt in Repayment (1,000s)	18.470	-0.0675	-0.0171	-0.0313	0.0021
		(0.0153)	(0.0124)	(0.0151)	(0.0152)
Percent with Exp. Creditors	0.445	61.2246	64.1221	41.5915	39.9184
		(0.9304)	(1.0954)	(1.0743)	(1.1760)
Baseline Outcomes					
Bankruptcy	0.003	0.8269	2.4481	0.4035	-0.2327
		(2.9804)	(2.2521)	(2.4277)	(2.0392)
Nonzero Collections Debt	0.248	-0.2509	-0.0113	0.0047	0.5707
		(0.5425)	(0.4062)	(0.5635)	(0.4192)
Credit Score	586.665	-0.0118	-0.0007	0.0059	0.0044
		(0.0042)	(0.0029)	(0.0037)	(0.0028)
Employment	0.848	-0.1695	-0.3153	-0.4177	-0.3814
		(0.7514)	(0.5952)	(0.9123)	(0.6257)
Earnings (1,000s)	23.702	0.0110	0.0063	-0.0137	-0.0002
		(0.0124)	(0.0090)	(0.0135)	(0.0097)
Data Quality					
Matched to SSA data	0.952	-10.7645	16.3510	-16.3291	-11.7238
		(24.7010)	(19.1893)	(19.7504)	(15.0040)
Matched to TU Data	0.869	7.2316	0.6201	-2.3265	-2.2481
		(2.5278)	(1.8077)	(2.2688)	(1.7471)
p-value from joint F-test	–	[0.0000]	[0.8802]	[0.0022]	[0.8947]
Creditor Risk Set FE	–	No	Yes	No	Yes
Number of Observations	39,855	78,438	78,438	78,438	78,438

Notes: This table describes correlates of potential treatment intensity with and without controls for the creditor risk set. The dependent variable for columns 2-3 is the maximum potential change in interest rates x 100. The dependent variable for columns 4-5 is the maximum potential change in minimum payments x 100. All regressions control for randomization strata fixed effects and cluster standard errors at the counselor level. Columns 3 and 5 also control for creditor risk set fixed effects. The p-value is from an F-test of the joint significance of all the variables listed except the percent of debt with experimental creditors.

Table 4: Targeted Debt Relief and Repayment

	Start Repayment				Finish Repayment			
	ITT	Baseline	Regression	Matching	ITT	Baseline	Regression	Matching
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Eligibility	0.019				0.010			
	(0.004)				(0.003)			
Treat. x Max Interest Write-Down		0.044	0.039	0.051		0.029	0.027	0.029
		(0.017)	(0.014)	(0.027)		(0.012)	(0.011)	(0.017)
Treat. x Max Payment Reduction		0.022	0.013	-0.009		0.005	0.003	-0.008
		(0.020)	(0.018)	(0.029)		(0.015)	(0.014)	(0.023)
Control Group Mean	0.328	0.328	0.328	0.328	0.143	0.143	0.143	0.143
Number of Observations	78,438	78,438	78,438	78,438	78,438	78,438	78,438	78,438

Notes: This table reports reduced form estimates of the impact of targeted debt relief on repayment. Information on repayment comes from administrative records at the credit counseling organization. Columns 1 and 5 report intent-to-treat estimates. Columns 2 and 6 report our baseline regression estimates. Columns 3 and 7 report regression estimates with creditor risk set fixed effects. Columns 4 and 8 report matching estimates run at the creditor-risk-set-level as described in the text. All specifications control for potential treatment intensity, the baseline controls in Table 2, and randomization strata fixed effects. ITT, baseline, and regression specifications report standard errors clustered at the counselor level and matching specifications report standard errors from the Bayesian bootstrap procedure described in the text.

Table 5: Targeted Debt Relief and Bankruptcy Filing

	Bankruptcy Filing			
	ITT	Baseline	Regression	Matching
	(1)	(2)	(3)	(4)
Treatment Eligibility	-0.006			
	(0.002)			
Treat. x Max Interest Write-Down		-0.031	-0.030	-0.013
		(0.009)	(0.009)	(0.016)
Treat. x Max Payment Reduction		0.023	0.023	0.013
		(0.011)	(0.011)	(0.019)
Control Group Mean	0.105	0.105	0.105	0.105
Number of Observations	78,438	78,438	78,438	78,438

Notes: This table reports reduced form estimates of the impact of targeted debt relief on bankruptcy. Information on bankruptcy comes from court records. Column 1 reports intent-to-treat estimates. Column 2 reports our baseline regression estimates. Column 3 reports regression estimates with creditor risk set fixed effects. Column 4 reports matching estimates run at the creditor-risk-set-level as described in the text. All specifications control for potential treatment intensity, the baseline controls in Table 2, and randomization strata fixed effects. ITT, baseline, and regression specifications report standard errors clustered at the counselor level and matching specifications report standard errors from the Bayesian bootstrap procedure described in the text.

Table 6: Targeted Debt Relief and Financial Outcomes

	Nonzero Collections Debt				Credit Score			
	ITT	Baseline	Regression	Matching	ITT	Baseline	Regression	Matching
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Eligibility	0.000				-0.324			
	(0.003)				(0.530)			
Treat. x Max Interest Write-Down		-0.006	-0.006	-0.027		2.339	1.898	3.550
		(0.012)	(0.012)	(0.020)		(2.274)	(2.272)	(3.341)
Treat. x Max Payment Reduction		0.002	0.002	0.019		-2.092	-1.913	-4.124
		(0.016)	(0.016)	(0.025)		(2.716)	(2.766)	(4.261)
Control Group Mean	0.389	0.389	0.389	0.389	604.099	604.099	604.099	604.099
Number of Observations	68,000	68,000	68,000	68,000	67,705	67,705	67,705	67,705

Notes: This table reports reduced form estimates of the impact of targeted debt relief on collections debt and credit score. Information on outcomes comes from credit records at TransUnion. Columns 1 and 5 report intent-to-treat estimates. Columns 2 and 6 report our baseline regression estimates. Columns 3 and 7 report regression estimates with creditor risk set fixed effects. Columns 4 and 8 report matching estimates run at the creditor-risk-set-level as described in the text. All specifications control for potential treatment intensity, the baseline controls in Table 2, and randomization strata fixed effects. ITT, baseline, and regression specifications report standard errors clustered at the counselor level and matching specifications report standard errors from the Bayesian bootstrap procedure described in the text.

Table 7: Targeted Debt Relief and Labor Market Outcomes

	Employment				Earnings (1,000s)			
	ITT	Baseline	Regression	Matching	ITT	Baseline	Regression	Matching
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Eligibility	-0.002				-0.074			
	(0.002)				(0.112)			
Treat. x Max Interest Write-Down		0.005	0.006	0.016		-0.668	-0.749	-1.010
		(0.007)	(0.007)	(0.012)		(0.464)	(0.468)	(0.717)
Treat. x Max Payment Reduction		-0.008	-0.007	-0.003		0.413	0.526	0.605
		(0.009)	(0.009)	(0.015)		(0.620)	(0.629)	(0.919)
Control Group Mean	0.821	0.821	0.821	0.821	27.148	27.148	27.148	27.148
Number of Observations	74,738	74,738	74,738	74,738	74,738	74,738	74,738	74,738

Notes: This table reports reduced form estimates of the impact of targeted debt relief on employment and earnings. Information on outcomes comes from records at the Social Security Administration. Columns 1 and 5 report intent-to-treat estimates. Columns 2 and 6 report our baseline regression estimates. Columns 3 and 7 report regression estimates with creditor risk set fixed effects. Columns 4 and 8 report matching estimates run at the creditor-risk-set-level as described in the text. All specifications control for potential treatment intensity, the baseline controls in Table 2, and randomization strata fixed effects. ITT, baseline, and regression specifications report standard errors clustered at the counselor level and matching specifications report standard errors from the Bayesian bootstrap procedure described in the text.

Table 8: Subsample Regression Estimates

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Baseline Debt-to-Income							
Treat. x Max Interest	0.062	0.044	-0.035	-0.012	2.374	0.042	-0.816
x High DTI	(0.017)	(0.014)	(0.012)	(0.014)	(2.740)	(0.019)	(0.521)
Treat. x Max Interest	0.012	0.007	-0.023	0.003	1.321	0.008	-0.671
x Low DTI	(0.019)	(0.016)	(0.011)	(0.017)	(2.860)	(0.010)	(0.674)
p-value on difference	[0.024]	[0.065]	[0.385]	[0.407]	[0.747]	[0.826]	[0.845]
Treat. x Max Payment	0.015	-0.002	0.022	0.014	-4.124	-0.005	0.574
x High DTI	(0.020)	(0.018)	(0.014)	(0.018)	(3.353)	(0.013)	(0.710)
Treat. x Max Payment	0.007	0.005	0.024	-0.012	1.062	-0.011	0.473
x Low DTI	(0.025)	(0.019)	(0.013)	(0.019)	(3.522)	(0.011)	(0.873)
p-value on difference	[0.778]	[0.770]	[0.902]	[0.161]	[0.205]	[0.703]	[0.915]
Panel B: Baseline Credit Score							
Treat. x Max Interest	0.051	0.058	-0.027	-0.016	2.825	0.024	-0.479
x High Score	(0.020)	(0.017)	(0.013)	(0.015)	(2.882)	(0.010)	(0.639)
Treat. x Max Interest	0.034	0.009	-0.032	0.006	0.964	0.010	-0.928
x Low Score	(0.017)	(0.014)	(0.011)	(0.015)	(2.860)	(0.008)	(0.540)
p-value on difference	[0.445]	[0.018]	[0.746]	[0.189]	[0.604]	[0.531]	[0.527]
Treat. x Max Payment	0.015	-0.015	0.018	0.031	-4.055	-0.024	0.275
x High Score	(0.021)	(0.019)	(0.014)	(0.018)	(3.477)	(0.013)	(0.822)
Treat. x Max Payment	0.009	0.013	0.027	-0.026	0.273	0.009	0.726
x Low Score	(0.023)	(0.017)	(0.014)	(0.020)	(3.323)	(0.011)	(0.705)
p-value on difference	[0.806]	[0.204]	[0.594]	[0.008]	[0.280]	[0.024]	[0.600]

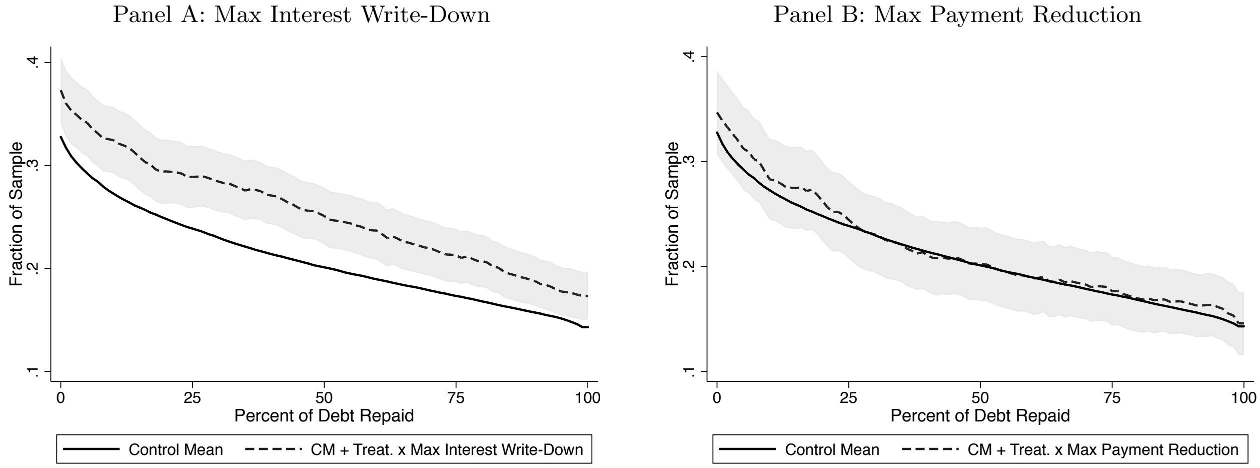
Notes: This table reports subsample regression estimates. Panel A reports estimates for individuals with above and below median debt-to-income. Panel B reports estimates for individuals with prime and subprime credit scores. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Table 9: Forward-Looking, Liquidity, and Exposure Effects

	Finish Repayment Program			
	Total Effect	Forward Looking	Liquidity Effect	Exposure Effect
	(1)	(2)	(3)	(4)
Treat. x Max Interest Write-Down	0.027	0.023		0.004
	(0.011)	(0.012)		(0.004)
Treat. x Max Payment Reduction	0.003		0.009	-0.006
	(0.014)		(0.014)	(0.004)

Notes: This table reports the forward-looking, liquidity, and exposure effects of each treatment on finishing the repayment program. Column 1 reports the total effect of each treatment on finishing the repayment program. Columns 2-3 reports estimates for being enrolled in the repayment program at the minimum of the treatment program length or the control program length. Column 4 reports the difference between Column 1 and Columns 2-3. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and the creditor risk sets described in the text. Standard errors for column 4 are calculated using the bootstrap procedure described in the text. See the text for additional details.

Figure 1: Debt Relief and Repayment Rates



Notes: These figures report control means and the implied treatment group means from our regression estimates. We calculate each treatment group mean using the control mean and the regression estimates described in Table 4. The shaded regions indicate the 95 percent confidence intervals. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix A: Additional Results

Appendix Table A1: Comparison of Recommended and Not Recommended Borrowers

	Approved Mean	Approved vs. Not Approved
Baseline Characteristics	(1)	(2)
Age	40.797	0.0001
		(0.0001)
Male	0.363	0.0008
		(0.0016)
White	0.639	-0.0050
		(0.0039)
Black	0.170	-0.0028
		(0.0046)
Hispanic	0.088	-0.0074
		(0.0041)
Number of Dependents	2.178	0.0032
		(0.0018)
Homeowner	0.419	-0.0055
		(0.0030)
Renter	0.435	-0.0016
		(0.0022)
Monthly Income (1,000s)	2.495	-0.0010
		(0.0009)
Debt in Repayment (1,000s)	18.558	0.0000
		(0.0001)
Percent with Exp. Creditors	0.446	0.0008
		(0.0032)
Baseline Outcomes		
Bankruptcy	0.003	-0.0095
		(0.0134)
Nonzero Collections Debt	0.248	-0.0013
		(0.0018)
Credit Score	586.355	0.0000
		(0.0000)
Employment	0.848	0.0047
		(0.0029)
Earnings (1,000s)	23.698	-0.0001
		(0.0000)
Data Quality		
Matched to SSA data	0.953	0.0319
		(0.0243)
Matched to TU Data	0.867	-0.0144
		(0.0096)
p-value from joint F-test		[0.0012]
Number of Observations	78,438	85,152

Notes: This table reports descriptive statistics for individuals recommended and not recommended for the repayment program. Column 1 reports the mean for the estimation sample recommended for the repayment program. Column 2 reports the difference between recommended and and not recommended individuals controlling for randomization strata fixed effects and clustering standard errors at the counselor level. The p-value is from an F-test of the joint significance of the variables listed.

Appendix Table A2: Intent-to-Treat Estimates in Different Samples

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full-Sample Estimates							
Treat. Eligibility	0.019	0.010	-0.006	0.000	-0.324	-0.002	-0.074
	(0.004)	(0.003)	(0.002)	(0.003)	(0.530)	(0.002)	(0.112)
Control Group Mean	0.328	0.143	0.105	0.389	604.099	0.821	27.148
Number of Observations	78,438	78,438	78,438	68,000	67,705	74,738	74,738
Panel B: No Debt with Exp. Creditors							
Treat. Eligibility	0.009	-0.003	-0.005	-0.000	-0.628	0.001	0.384
	(0.008)	(0.004)	(0.006)	(0.009)	(1.469)	(0.005)	(0.309)
Control Group Mean	0.158	0.042	0.084	0.558	567.859	0.829	22.808
Number of Observations	18,582	18,582	18,582	16,122	16,022	17,742	17,742
Panel C: Nonzero Debt with Exp. Creditors							
Treat. Eligibility	0.022	0.014	-0.008	0.001	-0.429	-0.003	-0.146
	(0.005)	(0.003)	(0.003)	(0.004)	(0.674)	(0.002)	(0.140)
Control Group Mean	0.381	0.175	0.111	0.336	615.425	0.818	28.512
Number of Observations	59,856	59,856	59,856	51,878	51,778	56,996	56,996
Panel D: 1%-50% Debt with Exp. Creditors							
Treat. Eligibility	0.013	0.016	-0.014	0.001	-0.014	-0.000	0.196
	(0.009)	(0.007)	(0.005)	(0.007)	(1.161)	(0.004)	(0.267)
Control Group Mean	0.354	0.129	0.124	0.414	599.096	0.822	27.264
Number of Observations	23,914	23,914	23,914	20,798	20,698	22,719	22,719
Panel E: 51%-100% Debt with Exp. Creditors							
Treat. Eligibility	0.029	0.014	-0.004	0.003	-1.048	-0.003	-0.172
	(0.007)	(0.006)	(0.004)	(0.005)	(0.894)	(0.003)	(0.203)
Control Group Mean	0.407	0.209	0.102	0.282	626.787	0.815	29.347
Number of Observations	35,216	35,216	35,216	30,429	30,424	33,599	33,599

Notes: This table reports intent-to-treat estimates of the impact of treatment eligibility in different samples. Panel A reports the full-sample estimates from Tables 4-7. Panel B restricts the sample to individuals with no experimental debt. Panel C restricts the sample to individuals with experimental debt. Panel D restricts the sample to individuals with 1%-50% experimental debt. Panel E restricts the sample to individuals with 51%-100% experimental debt. All specifications control for potential treatment intensity, the baseline controls in Table 2, and randomization strata fixed effects. Standard errors are clustered at the counselor level.

Appendix Table A3: 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 the text for additional details.

Appendix Table A4: Correlates of Creditor Holdings

	Creditor 1	Creditor 2	Creditor 3	Creditor 4	Creditor 5	Creditor 6	Creditor 7	Creditor 8	Creditor 9	Creditor 10	Creditor 11
Baseline Characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Age	-0.0004	-0.0010	0.0017	0.0000	0.0005	-0.0003	-0.0009	-0.0001	0.0022	-0.0006	-0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0001)
Male	-0.0028	0.0015	-0.0046	-0.0005	0.0022	-0.0238	0.0058	-0.0046	-0.0235	-0.0039	0.0088
	(0.0022)	(0.0032)	(0.0033)	(0.0002)	(0.0022)	(0.0040)	(0.0025)	(0.0036)	(0.0038)	(0.0034)	(0.0019)
White	0.0021	0.0067	-0.0046	0.0002	0.0015	-0.0361	0.0087	-0.0146	0.0196	-0.0187	-0.0190
	(0.0046)	(0.0064)	(0.0069)	(0.0004)	(0.0039)	(0.0083)	(0.0056)	(0.0078)	(0.0073)	(0.0068)	(0.0043)
Black	0.0061	-0.0052	-0.0023	0.0007	-0.0054	-0.0284	-0.0054	-0.0497	-0.0452	-0.0578	-0.0212
	(0.0049)	(0.0072)	(0.0076)	(0.0005)	(0.0045)	(0.0090)	(0.0056)	(0.0082)	(0.0088)	(0.0072)	(0.0048)
Hispanic	0.0045	0.0021	0.0207	-0.0002	-0.0082	0.0048	-0.0148	-0.0069	-0.0129	-0.0353	-0.0227
	(0.0062)	(0.0074)	(0.0085)	(0.0005)	(0.0047)	(0.0101)	(0.0060)	(0.0090)	(0.0094)	(0.0081)	(0.0046)
Number of Dependents	-0.0002	-0.0041	0.0020	-0.0001	0.0006	-0.0028	-0.0019	-0.0046	0.0020	-0.0057	-0.0057
	(0.0008)	(0.0015)	(0.0013)	(0.0001)	(0.0008)	(0.0015)	(0.0010)	(0.0014)	(0.0016)	(0.0012)	(0.0008)
Homeowner	0.0227	-0.0193	-0.0042	-0.0006	0.0016	0.0112	0.0113	-0.0034	-0.0135	0.0188	-0.0099
	(0.0036)	(0.0054)	(0.0055)	(0.0004)	(0.0033)	(0.0065)	(0.0043)	(0.0063)	(0.0061)	(0.0057)	(0.0029)
Renter	-0.0081	-0.0058	0.0018	-0.0004	-0.0021	-0.0097	0.0039	-0.0043	-0.0196	-0.0018	-0.0021
	(0.0030)	(0.0052)	(0.0051)	(0.0004)	(0.0028)	(0.0056)	(0.0035)	(0.0049)	(0.0056)	(0.0047)	(0.0028)
Monthly Income (1,000s)	0.0049	0.0039	0.0037	0.0002	-0.0023	0.0130	-0.0002	0.0014	0.0129	-0.0008	0.0094
	(0.0011)	(0.0015)	(0.0015)	(0.0001)	(0.0011)	(0.0018)	(0.0012)	(0.0014)	(0.0016)	(0.0015)	(0.0010)
Debt in Repayment (1,000s)	0.0009	0.0046	0.0001	0.0000	0.0018	0.0057	0.0047	0.0067	-0.0007	0.0080	0.0023
	(0.0001)	(0.0002)	(0.0001)	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Percent with Exp. Creditors	0.0923	0.3496	0.0894	0.0002	0.0889	0.3841	0.1301	0.4588	-0.1543	-0.1275	-0.0346
	(0.0047)	(0.0075)	(0.0052)	(0.0003)	(0.0043)	(0.0060)	(0.0042)	(0.0070)	(0.0080)	(0.0043)	(0.0030)
Baseline Outcomes											
Bankruptcy	0.0072	0.0073	0.0372	-0.0005	0.0029	-0.0399	-0.0345	-0.0544	-0.0637	-0.0481	-0.0241
	(0.0165)	(0.0209)	(0.0261)	(0.0005)	(0.0112)	(0.0225)	(0.0129)	(0.0230)	(0.0299)	(0.0217)	(0.0096)
Nonzero Collections Debt	-0.0179	-0.0440	-0.0277	0.0001	-0.0047	-0.0197	0.0051	-0.0590	-0.0804	-0.0517	-0.0105
	(0.0031)	(0.0040)	(0.0041)	(0.0003)	(0.0024)	(0.0047)	(0.0027)	(0.0043)	(0.0051)	(0.0042)	(0.0023)
Credit Score	-0.0001	-0.0001	-0.0005	-0.0000	0.0001	-0.0002	0.0001	-0.0001	-0.0005	0.0002	0.0001
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Employment	0.0068	0.0091	0.0077	0.0004	0.0031	0.0200	-0.0023	0.0024	0.0351	0.0154	-0.0001
	(0.0039)	(0.0055)	(0.0057)	(0.0003)	(0.0040)	(0.0059)	(0.0041)	(0.0066)	(0.0066)	(0.0049)	(0.0035)
Earnings (1,000s)	0.0002	-0.0002	0.0004	-0.0000	-0.0001	0.0001	0.0002	-0.0003	0.0002	-0.0003	0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Data Quality											
Matched to SSA data	-0.0208	-0.0754	-0.0898	0.0004	0.0123	-0.0536	-0.1240	0.3314	0.1211	0.0017	-0.0378
	(0.1354)	(0.1219)	(0.1434)	(0.0007)	(0.0692)	(0.1828)	(0.0531)	(0.1566)	(0.1731)	(0.0701)	(0.0309)
Matched to TU Data	0.0731	0.0578	0.2689	0.0010	-0.0488	0.0830	-0.0509	0.0284	0.3140	-0.1145	-0.0738
	(0.0128)	(0.0178)	(0.0181)	(0.0011)	(0.0101)	(0.0202)	(0.0128)	(0.0192)	(0.0208)	(0.0170)	(0.0108)
Control Group Mean	0.073	0.215	0.165	0.001	0.056	0.320	0.100	0.381	0.248	0.189	0.049
Number of Observations	78,438	78,438	78,438	78,438	78,438	78,438	78,438	78,438	78,438	78,438	78,438

Notes: This table reports estimates from OLS regressions of creditor holdings on baseline characteristics. See the text for additional details.

Appendix Table A5: Descriptive Statistics by Treatment Intensity

	Zero Write-Down Zero Payment		Low Write-Down Low Payment		Low Write-Down High Payment		High Write-Down Low Payment		High Write-Down High Payment	
	Control	T vs. C	Control	T vs. C	Control	T vs. C	Control	T vs. C	Control	T vs. C
Baseline Characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age	38.656	−0.0007	41.846	−0.0004	40.768	0.0001	44.416	−0.0010	41.668	0.0001
		(0.0005)		(0.0005)		(0.0008)		(0.0016)		(0.0005)
Male	0.356	−0.0087	0.359	−0.0099	0.367	0.0020	0.363	0.0295	0.383	0.0060
		(0.0089)		(0.0109)		(0.0205)		(0.0353)		(0.0135)
White	0.577	−0.0085	0.650	−0.0044	0.654	0.0012	0.721	0.0569	0.689	−0.0339
		(0.0178)		(0.0224)		(0.0370)		(0.0698)		(0.0247)
Black	0.233	−0.0017	0.151	−0.0220	0.137	−0.0277	0.121	0.0427	0.125	−0.0409
		(0.0198)		(0.0232)		(0.0456)		(0.0901)		(0.0303)
Hispanic	0.096	−0.0002	0.092	−0.0222	0.099	−0.0381	0.069	0.0346	0.080	−0.044
		(0.0236)		(0.0261)		(0.0427)		(0.0930)		(0.0309)
Number of Dependents	2.171	−0.0010	2.248	−0.0009	2.217	−0.0104	2.173	−0.0000	2.089	−0.0003
		(0.0029)		(0.0047)		(0.0079)		(0.0144)		(0.0051)
Homeowner	0.321	−0.0205	0.462	0.0164	0.476	0.0163	0.510	0.1038	0.449	0.0328
		(0.0161)		(0.0188)		(0.0358)		(0.0630)		(0.0245)
Renter	0.506	−0.0208	0.406	0.0020	0.391	0.0030	0.365	0.0558	0.411	0.0272
		(0.0122)		(0.0181)		(0.0309)		(0.0583)		(0.0226)
Monthly Income (1,000s)	2.116	−0.0004	2.678	−0.0005	2.715	−0.0008	2.729	−0.0192	2.653	0.0017
		(0.0045)		(0.0042)		(0.0074)		(0.0144)		(0.0051)
Debt in Repayment (1,000s)	11.041	0.0003	22.133	0.0001	21.561	0.0005	25.873	−0.0003	20.953	−0.0001
		(0.0004)		(0.0004)		(0.0006)		(0.0011)		(0.0005)
Percent with Exp. Creditors	0.154	−0.0366	0.349	−0.0018	0.697	−0.0554	0.591	−0.1075	0.807	−0.0429
		(0.0177)		(0.0256)		(0.0503)		(0.1054)		(0.036)
Baseline Outcomes										
Bankruptcy	0.007	−0.0399	0.003	0.0679	0.002	−0.0682	0.000	0.3807	0.001	0.0532
		(0.0533)		(0.0979)		(0.1864)		(0.3602)		(0.1459)
Nonzero Collections Debt	0.397	0.0105	0.216	0.0033	0.176	0.0057	0.128	0.0421	0.143	−0.0344
		(0.0109)		(0.0158)		(0.0292)		(0.0501)		(0.0202)
Credit Score	567.626	0.0000	583.136	−0.0000	596.860	−0.0002	609.825	0.0001	606.083	−0.0000
		(0.0001)		(0.0001)		(0.0002)		(0.0003)		(0.0001)
Employment	0.850	0.0119	0.850	0.0103	0.871	−0.0029	0.820	−0.0400	0.839	−0.0089
		(0.0163)		(0.0201)		(0.0330)		(0.0549)		(0.0224)
Earnings (1,000s)	20.586	−0.0000	25.222	−0.0002	26.215	−0.0004	24.605	−0.0003	24.828	−0.0001
		(0.0003)		(0.0003)		(0.0005)		(0.0010)		(0.0004)
Data Quality										
Matched to SSA data	0.953	−0.2159	0.948	−0.0099	0.953	0.1483	0.962	0.0687	0.953	0.0364
		(0.1893)		(0.3275)		(0.5624)		(0.1453)		(0.0541)
Matched to TU Data	0.872	−0.0272	0.870	−0.0047	0.864	0.1227	0.876	−0.0556	0.865	−0.0028
		(0.0439)		(0.0640)		(0.1053)		(0.1957)		(0.0766)
p-value from joint F-test		[0.2595]		[0.9949]		[0.7907]		[0.9067]		[0.6046]
Number of Observations	12,996	25,477	10,390	20,429	4,959	9,998	2,962	5,780	8,548	16,754

Notes: This table reports descriptive statistics and balance tests by potential treatment intensity. See the Table 2 notes for details.

Appendix Table A6: Non-Parametric Estimates

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat. x No Write-Down	0.003	-0.003	-0.001	0.007	-1.404	0.001	0.085
x No Payment Reduction	(0.006)	(0.004)	(0.004)	(0.005)	(0.894)	(0.003)	(0.198)
Treat. x Low Write-Down	0.003	0.012	-0.012	-0.002	0.543	0.001	0.023
x Low Payment Reduction	(0.007)	(0.006)	(0.005)	(0.006)	(1.073)	(0.004)	(0.223)
Treat. x Low Write-Down	-0.003	-0.001	0.002	0.010	-2.780	0.002	0.105
x High Payment Reduction	(0.011)	(0.010)	(0.009)	(0.011)	(2.129)	(0.007)	(0.444)
Treat. x High Write-Down	0.025	0.016	-0.000	-0.002	0.130	0.004	-0.680
x Low Payment Reduction	(0.010)	(0.008)	(0.007)	(0.008)	(1.377)	(0.005)	(0.322)
Treat. x High Write-Down	0.035	0.016	-0.012	-0.003	0.031	-0.011	-0.171
x High Payment Reduction	(0.007)	(0.006)	(0.006)	(0.007)	(1.219)	(0.004)	(0.271)
Control Group Mean	0.328	0.143	0.105	0.389	604.099	0.821	27.148
Number of Observations	78,438	78,438	78,438	68,000	67,705	74,738	74,738

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 potential treatment intensity bins, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix Table A7: Treatment Eligibility and Program Characteristics

	Interest Rate (pp)			Monthly Payment (pp)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat. Eligibility	-0.0269	-0.0268	-0.0350	-0.0010	-0.0010	-0.0012
	(0.0003)	(0.0003)	(0.0003)	(0.0000)	(0.0000)	(0.0000)
Control Group Mean	0.0854	0.0854	0.0869	0.0259	0.0259	0.0251
Creditor Risk Sets		X	X		X	X
Nonzero Exp. Debt			X			X
Number of Observations	78,438	78,438	59,856	78,438	78,438	59,856

Notes: This table reports the impact of treatment eligibility on repayment program characteristics. Columns 1-2 and 4-5 report estimates for the full sample. Columns 3 and 6 restrict the sample to individuals with nonzero debt with experimental creditors. All specifications control for potential treatment intensity, the baseline controls in Table 2, and randomization strata fixed effects. Standard errors are clustered at the counselor level.

Appendix Table A8: Bankruptcy Regression Estimates by Year

	Year 1	Year 2	Year 3	Year 4	Year 5
	(1)	(2)	(3)	(4)	(5)
Treat. x Max Interest Write-Down	-0.014	-0.008	-0.008	0.001	-0.002
	(0.007)	(0.004)	(0.004)	(0.003)	(0.003)
Treat. x Max Payment Reduction	0.010	0.003	0.003	0.000	0.006
	(0.008)	(0.005)	(0.005)	(0.004)	(0.003)
Control Group Mean	0.058	0.018	0.014	0.009	0.006
Creditor Risk Sets	X	X	X	X	X
Number of Observations	78,438	78,438	78,438	78,438	78,438

Notes: This table reports reduced form regression estimates of the impact of targeted debt relief on bankruptcy filing by year. We report estimates for the interaction of treatment eligibility and the maximum potential interest write-down and maximum potential minimum payment reduction. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix Table A9: Regression Estimates Pre- and Post-BAPCPA

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat. x Max Interest	0.029	0.017	-0.039	-0.001	0.565	0.011	-0.648
x Pre-BAPCPA	(0.016)	(0.013)	(0.011)	(0.015)	(2.494)	(0.008)	(0.560)
Treat. x Max Interest	0.064	0.052	-0.012	-0.017	4.152	-0.001	-0.914
x Post-BAPCPA	(0.022)	(0.018)	(0.014)	(0.017)	(3.316)	(0.010)	(0.672)
p-value on difference	[0.124]	[0.082]	[0.098]	[0.429]	[0.315]	[0.260]	[0.735]
Treat. x Max Payment	0.010	0.000	0.031	0.003	0.209	0.001	0.411
x Pre-BAPCPA	(0.021)	(0.017)	(0.013)	(0.020)	(3.060)	(0.011)	(0.788)
Treat. x Max Payment	0.008	-0.004	0.010	0.007	-4.653	-0.016	0.673
x Post-BAPCPA	(0.024)	(0.020)	(0.015)	(0.018)	(3.908)	(0.012)	(0.755)
p-value on difference	[0.931]	[0.847]	[0.232]	[0.871]	[0.258]	[0.197]	[0.767]

Notes: This table reports additional subsample regression estimates. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix Table A10: Additional Subsample Estimates

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Estimates by Baseline Employment							
Treat. x Max Interest	0.051	0.032	-0.002	-0.011	7.426	-0.018	-2.250
x Nonemployed	(0.025)	(0.023)	(0.016)	(0.022)	(4.364)	(0.022)	(0.813)
Treat. x Max Interest	0.036	0.025	-0.035	-0.005	0.772	0.004	-0.494
x Employed	(0.016)	(0.012)	(0.010)	(0.013)	(2.469)	(0.007)	(0.505)
p-value on difference	[0.568]	[0.755]	[0.035]	[0.796]	[0.158]	[0.526]	[0.043]
Treat. x Max Payment	-0.005	-0.009	0.028	-0.003	-5.561	0.013	1.663
x Nonemployed	(0.030)	(0.024)	(0.018)	(0.024)	(4.571)	(0.022)	(0.899)
Treat. x Max Payment	0.019	0.006	0.019	0.005	-1.092	-0.012	0.301
x Employed	(0.020)	(0.015)	(0.012)	(0.016)	(2.964)	(0.010)	(0.678)
p-value on difference	[0.462]	[0.533]	[0.659]	[0.719]	[0.346]	[0.256]	[0.138]
Panel B: Estimates by Gender							
Treat. x Max Interest	0.014	-0.013	-0.012	0.005	0.544	0.008	-0.271
x Male	(0.021)	(0.016)	(0.016)	(0.018)	(3.334)	(0.009)	(0.649)
Treat. x Max Interest	0.049	0.047	-0.041	-0.013	2.565	0.005	-1.034
x Female	(0.017)	(0.015)	(0.010)	(0.014)	(2.604)	(0.009)	(0.531)
p-value on difference	[0.138]	[0.003]	[0.069]	[0.306]	[0.587]	[0.756]	[0.276]
Treat. x Max Payment	0.052	0.023	0.001	0.014	-4.723	-0.010	0.017
x Male	(0.025)	(0.020)	(0.018)	(0.023)	(4.090)	(0.012)	(0.829)
Treat. x Max Payment	-0.008	-0.003	0.037	-0.005	-0.184	-0.005	0.824
x Female	(0.021)	(0.018)	(0.012)	(0.018)	(3.244)	(0.010)	(0.675)
p-value on difference	[0.034]	[0.273]	[0.049]	[0.411]	[0.322]	[0.667]	[0.308]
Panel C: Estimates by Ethnicity							
Treat. x Max Interest	0.036	0.026	-0.024	-0.002	1.225	0.010	-0.856
x White	(0.017)	(0.013)	(0.012)	(0.014)	(2.662)	(0.008)	(0.534)
Treat. x Max Interest	0.039	0.021	-0.047	-0.012	2.802	-0.002	-0.517
x Non-White	(0.021)	(0.018)	(0.013)	(0.019)	(3.013)	(0.011)	(0.665)
p-value on difference	[0.887]	[0.773]	[0.116]	[0.638]	[0.651]	[0.323]	[0.643]
Treat. x Max Payment	0.017	0.002	0.014	0.006	-2.717	-0.006	0.599
x White	(0.019)	(0.016)	(0.013)	(0.017)	(3.014)	(0.011)	(0.703)
Treat. x Max Payment	0.006	0.015	0.050	-0.009	0.420	-0.009	0.375
x Non-White	(0.029)	(0.024)	(0.017)	(0.025)	(4.358)	(0.014)	(0.891)
p-value on difference	[0.692]	[0.606]	[0.051]	[0.530]	[0.476]	[0.826]	[0.812]

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel D: Estimates by Baseline Homeownership							
Treat. x Max Interest	0.033	0.018	-0.028	-0.015	2.107	0.010	-1.250
x Homeowner	(0.018)	(0.016)	(0.012)	(0.015)	(2.746)	(0.009)	(0.577)
Treat. x Max Interest	0.045	0.033	-0.031	0.003	1.614	0.003	-0.371
x Non-Owner	(0.017)	(0.014)	(0.012)	(0.014)	(2.570)	(0.009)	(0.586)
p-value on difference	[0.548]	[0.441]	[0.798]	[0.259]	[0.856]	[0.527]	[0.209]
Treat. x Max Payment	0.029	-0.000	0.016	0.015	-3.417	-0.007	0.696
x Homeowner	(0.021)	(0.019)	(0.014)	(0.018)	(3.198)	(0.011)	(0.768)
Treat. x Max Payment	-0.002	0.006	0.029	-0.009	-0.450	-0.007	0.401
x Non-Owner	(0.021)	(0.017)	(0.013)	(0.018)	(3.323)	(0.011)	(0.737)
p-value on difference	[0.178]	[0.782]	[0.444]	[0.186]	[0.393]	[0.977]	[0.722]

Notes: This table reports additional subsample regression estimates. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix Table A11: Robustness Checks

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Regression Estimates							
Treat. x Max Interest Write-Down	0.039	0.027	-0.030	-0.006	1.898	0.006	-0.749
	(0.014)	(0.011)	(0.009)	(0.012)	(2.272)	(0.007)	(0.468)
Treat. x Max Payment Reduction	0.013	0.003	0.023	0.002	-1.913	-0.007	0.526
	(0.018)	(0.014)	(0.011)	(0.016)	(2.766)	(0.009)	(0.629)
Panel B: Treatment x Demographic Effects							
Treat. x Max Interest Write-Down	0.042	0.028	-0.024	-0.013	3.871	0.010	-0.779
	(0.015)	(0.012)	(0.010)	(0.013)	(2.595)	(0.008)	(0.527)
Treat. x Max Payment Reduction	0.015	0.003	0.025	0.000	-1.382	-0.008	0.507
	(0.018)	(0.014)	(0.011)	(0.017)	(2.798)	(0.009)	(0.648)
p-value from joint F-test	[0.357]	[0.642]	[0.058]	[0.470]	[0.566]	[0.702]	[0.850]
Panel C: Treatment x Creditor Effects							
Treat. x Max Interest Write-Down	0.024	0.011	-0.022	0.008	-0.667	0.016	-1.210
	(0.018)	(0.016)	(0.014)	(0.018)	(3.191)	(0.011)	(0.671)
Treat. x Max Payment Reduction	0.017	0.008	0.014	-0.014	3.859	-0.004	0.559
	(0.022)	(0.018)	(0.015)	(0.021)	(3.460)	(0.012)	(0.757)
p-value from joint F-test	[0.207]	[0.319]	[0.977]	[0.202]	[0.075]	[0.727]	[0.417]
Panel D: Treatment x Demographic and Treatment x Creditor Effects							
Treat. x Max Interest Write-Down	0.028	0.016	-0.018	0.007	-0.189	0.017	-1.213
	(0.018)	(0.016)	(0.014)	(0.018)	(3.327)	(0.011)	(0.678)
Treat. x Max Payment Reduction	0.017	0.010	0.017	-0.014	4.051	-0.004	0.558
	(0.022)	(0.018)	(0.015)	(0.021)	(3.473)	(0.012)	(0.767)
p-value from joint F-test	[0.317]	[0.401]	[0.334]	[0.259]	[0.185]	[0.575]	[0.627]

Notes: This table reports robustness checks of our regression estimates. Panel A reports the regression estimates from Tables 4-7. Panel B adds treatment eligibility x demographic fixed effects for gender, race, homeownership, credit score, earnings, and debt-to-income. Panel C adds treatment eligibility x credit card issuer fixed effects. Panel D adds both treatment eligibility x credit card issuer and treatment eligibility 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 potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix Table A12: Estimates with p-values from Permutation Test

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat. x Max Interest Write-Down	0.039	0.026	-0.031	-0.015	2.045	0.006	-0.719
	[0.006]	[0.030]	[0.000]	[0.603]	[0.336]	[0.331]	[0.156]
Treat. x Max Payment Reduction	0.013	0.002	0.024	0.002	-1.987	-0.008	0.488
	[0.397]	[0.862]	[0.037]	[0.817]	[0.442]	[0.355]	[0.286]
Control Group Mean	0.328	0.143	0.105	0.389	604.099	0.821	27.148
Creditor Risk Sets	X	X	X	X	X	X	X
Number of Observations	78,438	78,438	78,438	68,000	67,705	74,738	74,738

Notes: This table reports reduced form regression estimates where the p-values are calculated using a non-parametric permutation test with 1,000 draws. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. See the text for additional details on the non-parametric permutation test.

Appendix Table A13: Comparison of Experimental Sample to Other Samples

	Experimental Sample	Credit User Sample	Default Sample	Bankruptcy Sample
	(1)	(2)	(3)	(4)
Age	40.85	48.55	41.87	44.84
Credit Score	586.4	739.5	572.3	580.8
Delinquency	0.323	0.148	0.634	0.678
Credit Card Balance (1,000s)	15.95	6.011	5.346	10.46
Credit Card Utilization	66.96	25.50	72.37	70.92
Any Auto Loan	0.467	0.283	0.420	0.473
Any Mortgage Loan	0.335	0.367	0.287	0.579
Number of Observations	68,000	3,308,824	61,079	56,906

Notes: This table reports descriptive statistics for different samples in the TransUnion credit data. Column 1 reports the mean for the estimation sample matched to the TransUnion data. Column 2 reports the mean for a random sample of all credit users from Dobbie et al. (2016). Column 3 reports the mean for credit users with a default in the next year from Dobbie et al. (2017). Column 4 reports the mean for credit users with a Chapter 13 bankruptcy in the next year from Dobbie et al. (2016). See the text for additional details.

Appendix Table A14: Characteristics of Borrowers Completing the Repayment Program

	Control	Independent Variable		
	Complier Mean	Treatment Eligibility	Treatment x Max Interest Write-Down	Treatment x Max Payment Reduction
Baseline Characteristics	(1)	(2)	(3)	(4)
Age	41.548	-0.0077	0.2775	-0.3839
		(0.2908)	(1.0292)	(1.3007)
Male	0.358	0.0064	-0.0069	0.0355
		(0.0080)	(0.0315)	(0.0403)
White	0.666	0.0058	0.0055	-0.0075
		(0.0090)	(0.0292)	(0.0363)
Black	0.136	0.0005	0.0114	-0.0061
		(0.0058)	(0.0191)	(0.0266)
Hispanic	0.091	-0.0084	-0.0203	0.0105
		(0.0058)	(0.0212)	(0.0256)
Number of Dependents	2.112	-0.0373	-0.0384	-0.0428
		(0.0244)	(0.0850)	(0.1129)
Homeowner	0.422	0.0017	-0.0374	0.0685
		(0.0088)	(0.0317)	(0.0420)
Renter	0.421	0.0082	0.0510	-0.0621
		(0.0090)	(0.0316)	(0.0406)
Monthly Income (1,000s)	2.691	0.0002	0.0493	-0.0729
		(0.0295)	(0.0992)	(0.1424)
Debt in Repayment (1,000s)	19.184	0.5993	1.9798	-1.2594
		(0.2841)	(0.9960)	(1.4747)
Percent with Exp. Creditors	0.532	0.0061	-0.0050	0.0005
		(0.0064)	(0.0090)	(0.0125)
Baseline Outcomes				
Bankruptcy	0.003	0.0006	0.0002	0.0010
		(0.0009)	(0.0029)	(0.0042)
Nonzero Collections Debt	0.163	0.0005	0.0234	-0.0427
		(0.0063)	(0.0221)	(0.0273)
Credit Score	596.163	-1.7605	1.1706	-6.2257
		(4.0306)	(12.9818)	(17.5044)
Employment	0.861	0.0084	-0.0003	0.0079
		(0.0067)	(0.0257)	(0.0332)
Earnings (1,000s)	25.944	0.3457	0.1835	0.6630
		(0.4001)	(1.4184)	(1.7806)
Data Quality				
Matched to SSA data	0.949	0.0025	-0.0033	0.0008
		(0.0038)	(0.0137)	(0.0170)
Matched to TU Data	0.833	-0.0031	-0.0004	-0.0094
		(0.0066)	(0.0207)	(0.0270)
Number of Observations	13,063	26,418	26,418	

Notes: This table reports descriptive statistics for control and treatment compliers based on program completion. Column 1 reports means for the the control compliers. Column 2 reports estimates from a regression of the indicated variable on treatment eligibility and randomization strata fixed effects. Columns 3-4 reports estimates from a regression of the indicated variable on treatment eligibility interacted with potential treatment intensity and randomization strata fixed effects. All specifications cluster standard errors by counselor. See the text for additional details.

Appendix Table A15: Regression Estimates for Additional Outcomes

	Percent Repaid	Serious Default	Card Balance	Card Util.	Any Auto	Any Mortgage	Nonzero 401k
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat. x Max Interest Write-Down	0.041	-0.012	0.159	-1.609	-0.014	0.016	0.003
	(0.012)	(0.013)	(0.390)	(1.219)	(0.014)	(0.012)	(0.012)
Treat. x Max Payment Reduction	0.003	0.010	0.279	1.032	0.003	-0.018	-0.005
	(0.015)	(0.017)	(0.526)	(1.393)	(0.018)	(0.016)	(0.015)
Control Group Mean	0.209	0.476	8.503	46.277	0.396	0.308	0.274
Creditor Risk Sets	X	X	X	X	X	X	X
Number of Observations	78,438	68,000	68,000	68,000	68,000	68,000	74,738

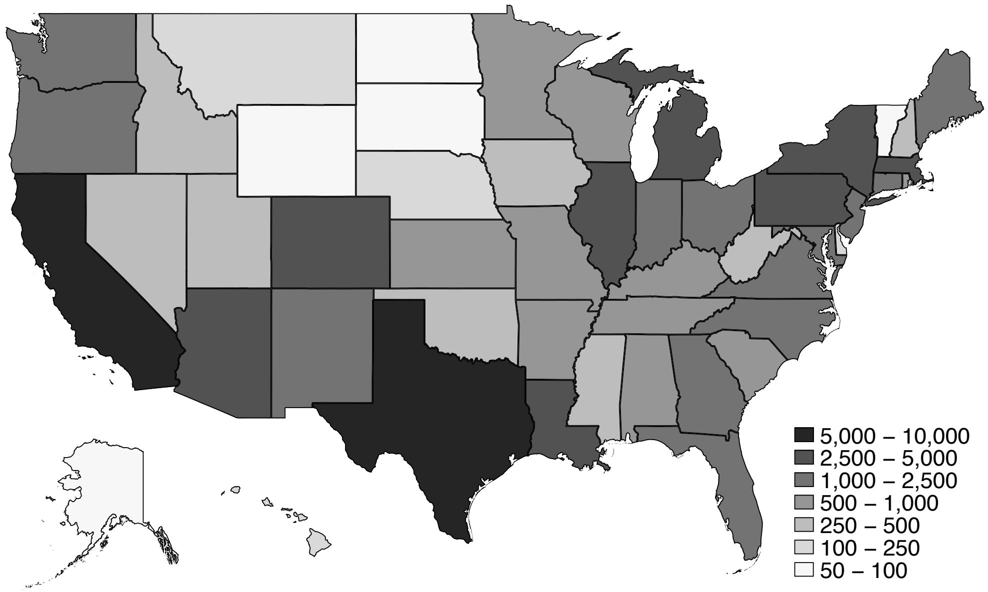
Notes: This table reports additional reduced form regression estimates of the impact of targeted debt relief. We report estimates for the interaction of treatment eligibility and the maximum potential interest write-down and maximum potential minimum payment reduction. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and the creditor risk sets described in the text. Standard errors are clustered at the counselor level.

Appendix Table A16: Regression Estimates in Different Samples

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full-Sample Estimates							
Treat. x Max Interest Write-Down	0.039	0.027	-0.030	-0.006	1.898	0.006	-0.749
	(0.014)	(0.011)	(0.009)	(0.012)	(2.272)	(0.007)	(0.468)
Treat. x Max Payment Reduction	0.013	0.003	0.023	0.002	-1.913	-0.007	0.526
	(0.018)	(0.014)	(0.011)	(0.016)	(2.766)	(0.009)	(0.629)
Control Group Mean	0.328	0.143	0.105	0.389	604.099	0.821	27.148
Number of Observations	78,438	78,438	78,438	68,000	67,705	74,738	74,738
Panel B: Nonzero Debt with Exp. Creditors							
Treat. x Max Interest Write-Down	0.059	0.049	-0.115	-0.002	10.358	0.009	-0.449
	(0.055)	(0.045)	(0.039)	(0.046)	(7.116)	(0.027)	(1.620)
Treat. x Max Payment Reduction	-0.031	-0.008	0.067	0.017	-20.383	-0.003	-0.611
	(0.068)	(0.063)	(0.057)	(0.064)	(10.069)	(0.038)	(2.457)
Control Group Mean	0.381	0.175	0.111	0.336	615.425	0.818	28.512
Number of Observations	59,856	59,856	59,856	51,878	51,778	56,996	56,996
Panel C: 0%-50% of Debt with Exp. Creditors							
Treat. x Max Interest Write-Down	0.039	0.023	-0.035	-0.001	1.302	0.006	-0.657
	(0.015)	(0.012)	(0.010)	(0.013)	(2.324)	(0.007)	(0.509)
Treat. x Max Payment Reduction	0.012	0.005	0.027	-0.002	-1.352	-0.005	0.550
	(0.019)	(0.015)	(0.011)	(0.016)	(2.899)	(0.010)	(0.653)
Control Group Mean	0.354	0.129	0.124	0.414	599.096	0.822	27.264
Number of Observations	23,914	23,914	23,914	20,798	20,698	22,719	22,719
Panel D: 51%-100% of Debt with Exp. Creditors							
Treat. x Max Interest Write-Down	0.039	0.026	-0.021	-0.002	-0.681	0.000	-0.482
	(0.017)	(0.015)	(0.010)	(0.014)	(2.853)	(0.009)	(0.607)
Treat. x Max Payment Reduction	0.017	-0.003	0.017	-0.004	1.741	-0.009	0.597
	(0.021)	(0.018)	(0.013)	(0.018)	(3.570)	(0.011)	(0.734)
Control Group Mean	0.407	0.209	0.102	0.282	626.787	0.815	29.347
Number of Observations	35,216	35,216	35,216	30,429	30,424	33,599	33,599

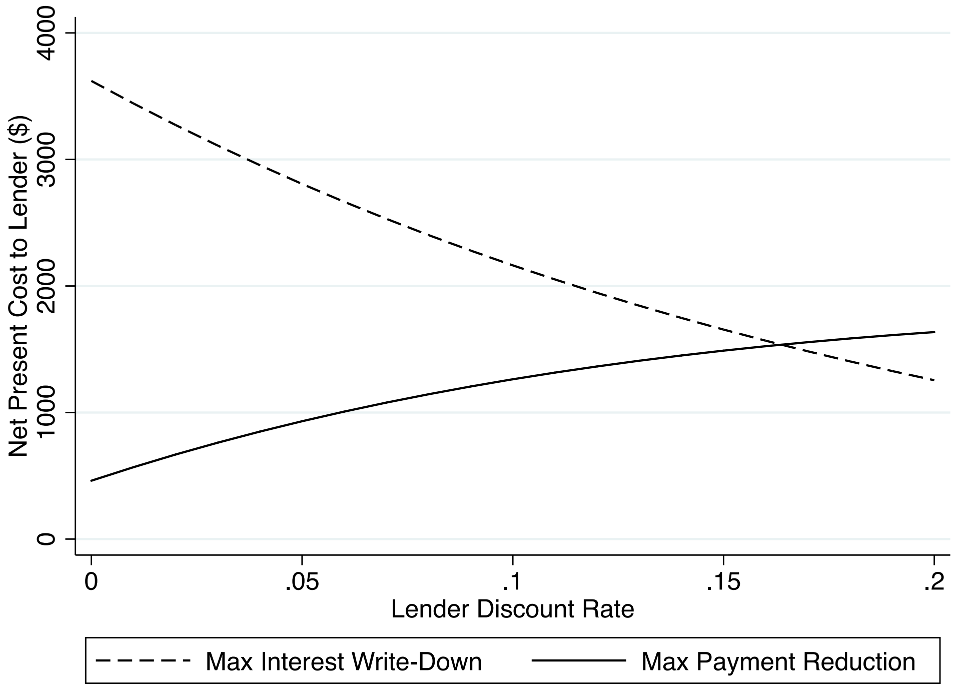
Notes: This table reports our regression estimates of targeted debt relief in different samples. Panel A reports the full-sample regression estimates from Tables 4-7. Panel B restricts the sample to individuals with experimental debt. Panel C restricts the sample to individuals with 1%-50% experimental debt. Panel D restricts the sample to individuals with 51%-100% experimental debt. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix Figure A1: Geographic Distribution of the Experimental Sample



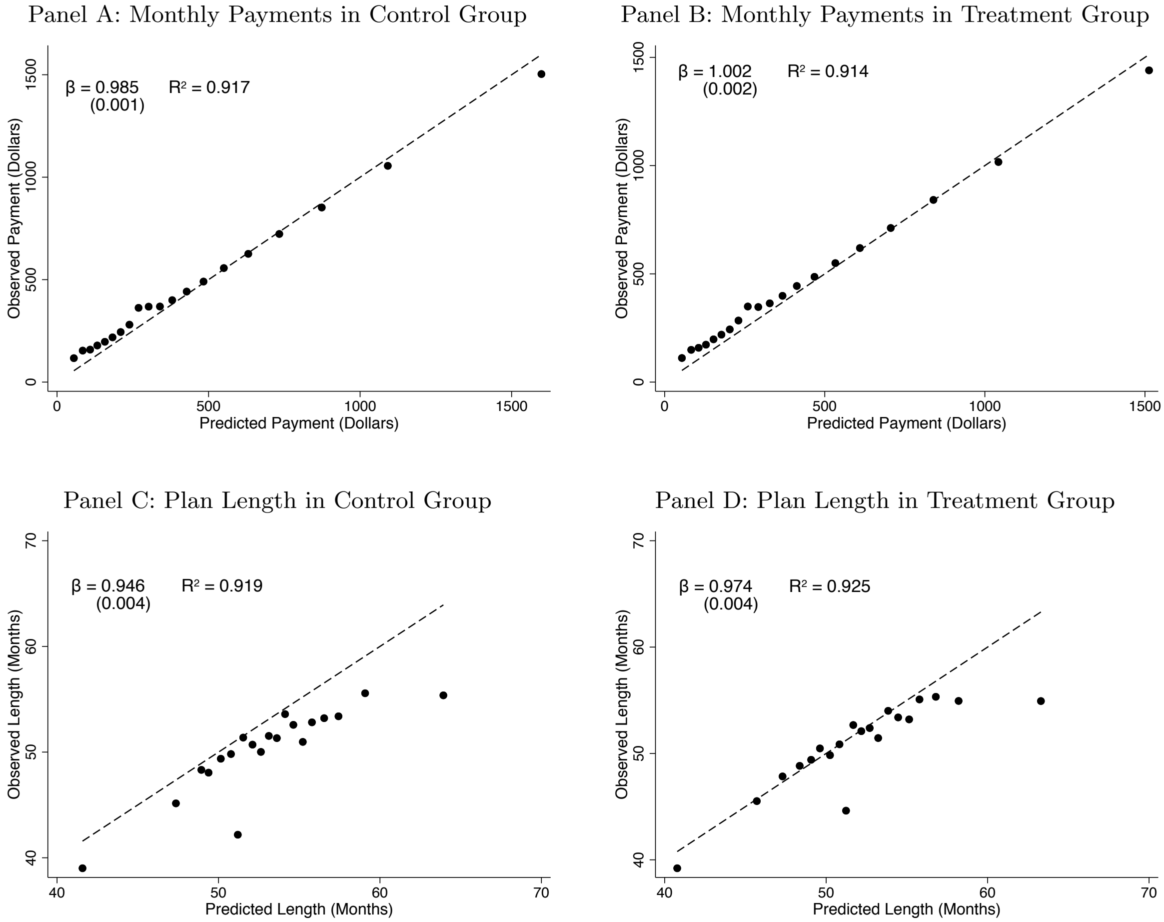
Notes: This figure plots the number of individuals in the experimental sample by state. See the text for additional details.

Appendix Figure A2: Net Present Costs to the Lender



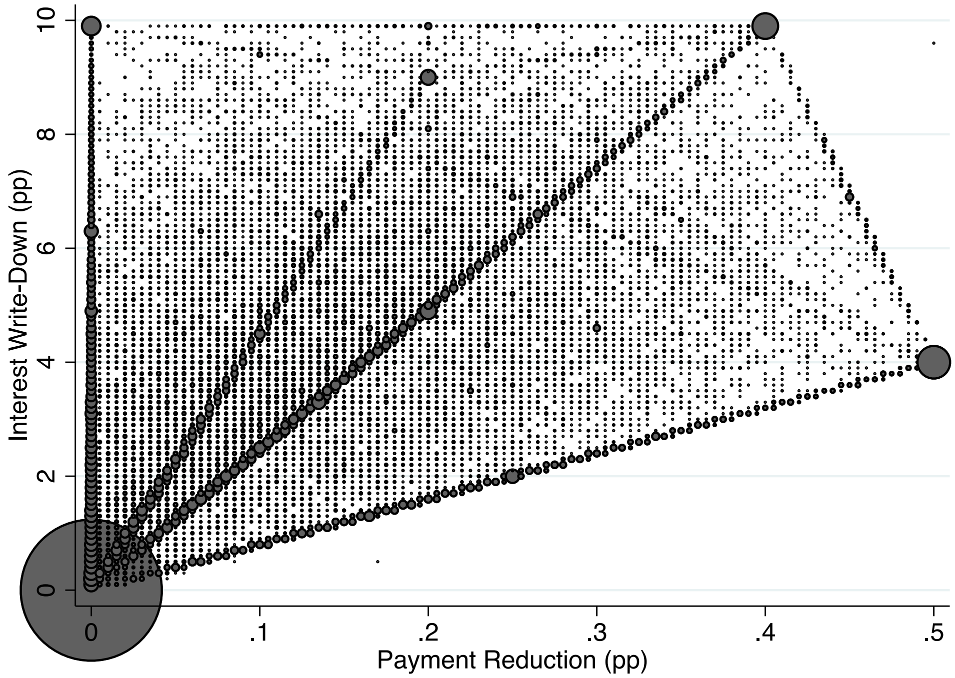
Notes: This figure plots the net present costs to the lender of providing each treatment. Lender costs (relative to the baseline case) are calculated using the control means for debt ($18,470), minimum payment (2.38% of debt), and monthly default rate during the repayment program (1.12%), and a baseline interest rate of 9.90%. The dashed line plots net present costs with the maximum 9.90 percentage point interest rate write-down. The solid line plots costs with the maximum 0.50 percentage point decrease in the required minimum payment. See the text for additional details.

Appendix Figure A3: Predicted and Actual Program Characteristics



Notes: These figures plot predicted repayment program characteristics against actual program characteristics for the control and treatment groups. Actual plan length is only observed for individuals completing the DMP and includes voluntary early repayment. The coefficient, standard error, and R^2 are estimated using OLS on the underlying micro data. The dashed line is the 45 degree line. See the text for additional details.

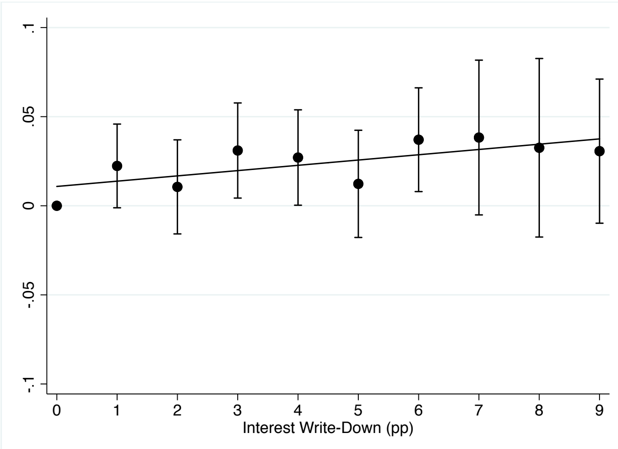
Appendix Figure A4: Distribution of Potential Treatment Intensity



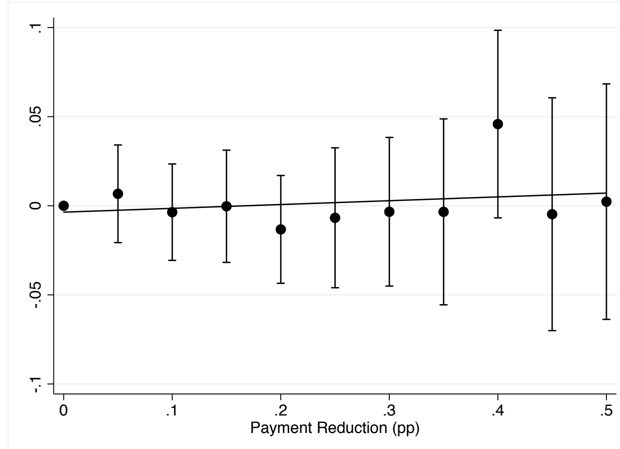
Notes: This figure plots the distribution of potential interest write-downs and potential minimum payment reductions in our estimation sample. Potential interest write-downs and potential minimum payment reductions are calculated using borrower-level data and the rules listed in Appendix Table A4. See the text for additional details.

Appendix Figure A5: Non-Parametric Treatment Effects

Panel A: Interest Write-Down



Panel B: Minimum Payment Reduction



Notes: These figures report non-parametric treatment effects and associated 95 percent confidence intervals. All specifications control for potential treatment intensity, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix B: Empirical Design Details

In this appendix, we formalize the assumptions under which we can identify the causal effects of the interest write-downs and minimum payment reductions using cross-sectional variation in potential treatment intensity.

A. Setup and Identifying Assumptions

Setup and Identifying Assumptions: We omit time subscripts and abstract away from baseline controls for notational simplicity. For each individual i, we observe a binary indicator for treatment eligibility Z_i, an outcome Y_i, and two continuously distributed variables X_{1i} and X_{2i} that determine the treatment intensity if in the treatment group. That is, individuals in the control group ($Z_i = 0$) receive no treatments, while individuals in the treatment group ($Z_i = 1$) receive X_{1i} and X_{2i}. The realized treatment variables can therefore be written as Z_iX_{1i} and Z_iX_{2i}. In our context, these realized treatment variables Z_iX_{1i} and Z_iX_{2i} correspond to the $WriteDown_i$ and $Payment_i$ variables in Equation (1), respectively, while the continuous covariate variables X_{1i} and X_{2i} correspond to the potential treatment intensity variables described in the text. Note that we observe the potential treatment intensity variables X_{1i} and X_{2i} for everyone in the sample, regardless of treatment eligibility Z_i.

Causal effects are defined in terms of potential outcomes, where $Y_i(0)$ is the outcome if i is in the control group and $Y_i(1)$ is the outcome if i is in the treatment group. These latent variables are independent across individuals, satisfying a stable unit treatment value assumption (Rubin 1980). To this we add,

A1 *Additive Separability:* Potential outcomes can be written as

$$\begin{aligned} Y_i(1) &= Y_i(0) + \beta_{1i}X_{1i} + \beta_{2i}X_{2i} \\ Y_i(0) &= \mu(X_{1i}, X_{2i}) \\ &= \beta_{3i}X_{1i} + \beta_{4i}X_{2i} + \beta_{5i} \end{aligned}$$

Realized outcomes $Y_i = Y_i(Z_i)$ can therefore be written as

$$Y_i = \beta_{1i}Z_iX_{1i} + \beta_{2i}Z_iX_{2i} + \beta_{3i}X_{1i} + \beta_{4i}X_{2i} + \beta_{5i}.$$

A2 *Independence:* $(X_{1i}, X_{2i}, \beta_{1i}, \beta_{2i}, \beta_{3i}, \beta_{4i}, \beta_{5i}) \perp Z_i$.

Assumption A1 states that potential outcomes are a function of the potential treatment intensities X_{1i} and X_{2i}, thereby allowing for selection bias in non-experimental estimates. In our context, for example, this setup allows individuals holding debt with the credit card issuers offering relatively more generous interest write-downs and payment reductions to be unobservably different compared to individuals holding debt with the credit card issuers offering relatively less generous interest write-downs and payment reductions. For this reason, OLS estimates of Y_i on Z_iX_{1i} and Z_iX_{2i} in non-experimental populations will be biased if we cannot also control for $\mu(X_{1i}, X_{2i})$, e.g., by using the randomly assigned control group.

In addition, Assumptions A1 and A2 state that Z_iX_{1i} and Z_iX_{2i} can be correlated with β_{1i} and β_{2i}, thereby allowing for Roy (1951)–type selection into the potential treatment intensities. In our context, for example, this setup allows for individuals holding debt with the credit card issuers offering relatively more generous interest write-downs and payment reductions to be more responsive to those interest write-downs and minimum payment reductions compared to individuals holding debt with the credit card issuers offering relatively less generous interest write-downs and payment reductions. For this reason, reduced form estimates of Y_i on Z_iX_{1i} and Z_iX_{2i} (also controlling for X_{1i} and X_{2i}) in our experimental population will also be biased, as variation in Z_iX_{1i} and Z_iX_{2i} is not only associated with change in the realized treatments, but also the gains to those realized treatments.

To address these issues, we experimentally compare individuals with the same set of credit cards where we might expect similar Roy (1951)–type selection, but with different proportions of debt on each credit card. Formally, let there be a set of covariates W_i with discrete support (e.g., the creditor risk set) such that the causal responses are independent from treatment intensity conditional on W_i:

A3 *Conditional Independence:* $(\beta_{1i}, \beta_{2i}, \beta_{3i}, \beta_{4i}, \beta_{5i}) \perp (X_{1i}, X_{2i})|W_i$.

In our context, Assumption A3 rules out Roy (1951)–type selection within creditor risk sets, but not across risk sets. In other words, we require that the proportion of debt with each card issuer be as-good-as-randomly assigned with respect to the gains from treatment β_{1i} and β_{2i}, but not with respect to the initial choice of which credit cards to hold. We also require the conditional independence of β_{3i}, β_{4i}, and β_{5i} so that we can estimate the causal effects conditional on $W_i = w$ through a linear model of Y_i on $(Z_iX_{1i}, Z_iX_{2i}, X_{1i}, X_{2i}, 1)$, as described in greater detail below.

Given the conditional independence of treatment effects within creditor risk sets (Assumption A3), one might wonder whether we still need to be concerned about selection bias. In our framework, selection bias can still arise from the correlation between the potential treatment intensities X_{1i} and X_{2i} and potential outcomes within risk sets W_i. That is, we allow for the possibility that $\beta_{3i} \neq 0$ and $\beta_{4i} \neq 0$ within risk sets. We only rule out Roy (1951)–type selection on gains into the potential treatment intensities within risk sets W_i, not selection bias on levels within those risk sets. Building on our above example, our setup allows for individuals holding relatively more debt with the credit card issuers within a risk set offering relatively more generous interest write-downs and payment reductions to be unobservably different on levels (but not the gains) compared to individuals in the same risk set holding relatively less debt with those credit card issuers.

B. Estimating Equations

The goal of our empirical analysis is to estimate a weighted average of the risk set-specific causal effects of the realized treatment variables, β_1^λ and β_2^λ, for some weighting scheme λ. We begin by providing additional details on our two estimators, before formally proving that each estimator yields unbiased estimates of β_1^λ and β_2^λ for two different weighting schemes.

Matching Estimator: Our first set of estimates come from a matching estimator that allows us to impose our own weights on each risk-set specific estimate, but at the cost of statistical precision and feasibility in finite samples. We estimate these matching results using a two-step procedure. First, we estimate the effects of Z_iX_{1i} and Z_iX_{2i} on Y_i separately for each risk set $W_i = w$ using the following reduced form specification:

$$Y_i = \beta_{1,w}Z_iX_{1i} + \beta_{2,w}Z_iX_{2i} + \beta_{3,w}X_{1i} + \beta_{4,w}X_{2i} + \beta_{5,w} + \psi_i \tag{B.1}$$

Note that we do not require weighting superscripts in Equation (B.1) under Assumption A3. We can also residualize the included variables using additional baseline controls such as randomization strata fixed effects as needed. In our context, we residualize for both baseline covariates and randomization strata fixed effects. Using these first-step estimates, we can then construct our matching estimates using the weighted averages of the risk-set specific estimates $\beta_{1,w}$ and $\beta_{2,w}$:

$$\beta_1^M = \sum_w \beta_{1,w} \times Pr(W_i = w|Z_i = 1) \tag{B.2}$$

$$\beta_2^M = \sum_w \beta_{2,w} \times Pr(W_i = w|Z_i = 1) \tag{B.3}$$

where the weights M are equal to the fraction of treated individuals in each risk set. We calculate standard errors for β_1^M and β_2^M using the Bayesian bootstrap procedure described in the text.

Regression Estimator: Our second set of estimates comes from a regression estimator that is statistically precise and straightforward to implement in finite samples, but at the cost that the weighting scheme underlying the estimator may not be economically relevant. We estimate these regression results using the full experimental sample and the following reduced form specification:

$$Y_i = \beta_1^R Z_iX_{1i} + \beta_2^R Z_iX_{2i} + \beta_3^R X_{1i} + \beta_4^R X_{2i} + \beta_5^R + W_i'\gamma^R + \nu_i \tag{B.4}$$

where the weights R are described in greater detail below. We can also add additional baseline controls such as randomization strata fixed effects to the estimating equation as needed. In our context, we control for both baseline covariates and randomization strata fixed effects and cluster the standard errors at the examiner level.

We will now show that both the matching and regression estimators have a causal interpretation when Assumptions A1-A3 hold. We begin by showing that our matching estimator provides unbiased estimates for the treatment effects of X_{1i} and X_{2i} within each risk set, which can then be aggregated using any researcher-imposed weighting scheme. We then show that, with an additional functional form assumption described below, our regression estimator provides unbiased estimates of a weighted average of risk set-specific treatment effects.

C. Proof of Matching Estimator

Proposition 1 Given Assumptions A1-A3, OLS estimates of Equation (B.1) provide unbiased estimates for the treatment effects of X_{1i} and X_{2i} within each risk set W_i. The constructed matching estimators in Equations (B.2) and (B.3) provide a weighted average of these risk set-specific treatment effects, where the weights are equal to the fraction of treated individuals in each risk set $Pr(W_i = w|Z_i = 1)$.

Proof of Proposition 1: Conditional on $W_i = w$, by Assumption A1 we have,

$$\begin{aligned}
E[Y_i|Z_i, X_{1i}, X_{2i}, W_i = w] &= E[\beta_{1i}Z_iX_{1i} + \beta_{2i}Z_iX_{2i} + \beta_{3i}X_{1i} + \beta_{4i}X_{2i} + \beta_{5i}|Z_i, X_{1i}, X_{2i}, W_i = w] \\
&= E[\beta_{1i}Z_iX_{1i}|Z_i, X_{1i}, X_{2i}, W_i = w] + E[\beta_{2i}Z_iX_{2i}|Z_i, X_{1i}, X_{2i}, W_i = w] \\
&+ E[\beta_{3i}X_{1i}|Z_i, X_{1i}, X_{2i}, W_i = w] + E[\beta_{4i}X_{2i}|Z_i, X_{1i}, X_{2i}, W_i = w] \\
&+ E[\beta_{5i}|Z_i, X_{1i}, X_{2i}, W_i = w]
\end{aligned}$$

By Assumptions A2 and A3,

$$\begin{aligned}
E[\beta_{1i}Z_iX_{1i}|Z_i, X_{1i}, X_{2i}, W_i = w] &= E[\beta_{1i}|Z_i, X_{1i}, X_{2i}, W_i = w]Z_iX_{1i} \\
&= E[\beta_{1i}|X_{1i}, X_{2i}, W_i = w]Z_iX_{1i} \\
&= E[\beta_{1i}|W_i = w]Z_iX_{1i}
\end{aligned}$$

Similarly, we can show

$$\begin{aligned}
E[\beta_{2i}Z_iX_{2i}|Z_i, X_{1i}, X_{2i}, W_i = w] &= E[\beta_{2i}|W_i = w]Z_iX_{2i} \\
E[\beta_{3i}X_{1i}|Z_i, X_{1i}, X_{2i}, W_i = w] &= E[\beta_{3i}|W_i = w]X_{1i} \\
E[\beta_{4i}X_{2i}|Z_i, X_{1i}, X_{2i}, W_i = w] &= E[\beta_{4i}|W_i = w]X_{2i} \\
E[\beta_{5i}|Z_i, X_{1i}, X_{2i}, W_i = w] &= E[\beta_{5i}|W_i = w]
\end{aligned}$$

Within each risk set $W_i = w$, we therefore have a linear conditional expectation model as follows:

$$E[Y_i|Z_i, X_{1i}, X_{2i}, W_i = w] = \beta_{1,w}Z_iX_{1i} + \beta_{2,w}Z_iX_{2i} + \beta_{3,w}X_{1i} + \beta_{4,w}X_{2i} + \beta_{5,w}$$

where the causal responses of interest are independent from (X_{1i}, X_{2i}) conditional on $W_i = w$:

$$\beta_{1,w} = E[\beta_{1i}|W_i = w]$$
$$\beta_{2,w} = E[\beta_{2i}|W_i = w]$$

Thus, an OLS regression of Y_i on $(Z_iX_{1i}, Z_iX_{2i}, X_{1i}, X_{2i}, 1)$ in Equation (B.1) yields unbiased estimates for $\beta_{1,w}$ and $\beta_{2,w}$. We refer to $\beta_{1,w}$ as the average causal response to X_{1i} within risk set $W_i = w$, and $\beta_{2,w}$ as the average causal response to X_{2i} within risk set $W_i = w$.

Finally, Equations (B.2) and (B.3) state that the matching estimators β_1^M and β_2^M are constructed as weighted averages of risk set-specific $\beta_{1,w}$ and $\beta_{2,w}$, respectively, where the weights are equal to the fraction of treated individuals in each risk set $W_i = w$.

D. Proof of Regression Estimator

In addition to our identifying assumptions A1-A3, we require the following functional form assumption to identify the causal effects of the treatments using our regression estimator:

A4 *Linear Relationship Between Covariates:*

$$E[Z_iX_{1i}|Z_iX_{2i}, X_{1i}, X_{2i}, W_i] = \pi_{2,1}Z_iX_{2i} + \pi_{3,1}X_{1i} + \pi_{4,1}X_{2i} + \pi_{5,1} + W_i'\Pi_1$$
$$E[Z_iX_{2i}|Z_iX_{1i}, X_{1i}, X_{2i}, W_i] = \pi_{1,2}Z_iX_{1i} + \pi_{3,2}X_{1i} + \pi_{4,2}X_{2i} + \pi_{5,2} + W_i'\Pi_2.$$

Assumption A4 ensures that the conditional expectation function $E[Z_iX_{1i}|X_{1i}, X_{2i}, Z_iX_{2i}, W_i]$ is linear in $(X_{1i}, X_{2i}, Z_iX_{2i}, 1, W_i)$ and the conditional expectation function $E[Z_iX_{2i}|X_{1i}, X_{2i}, Z_iX_{1i}, W_i]$ is linear in $(X_{1i}, X_{2i}, Z_iX_{1i}, 1, W_i)$. A4 would be violated if, for example, the correlation between the covariates X_{1i} and X_{2i} differs across risk sets W_i.

Proposition 2 Given Assumptions A1-A4, the regression estimators β_1^R and β_2^R in Equation (B.4) identify a weighted-average of the risk-set-specific effects of X_{1i} and X_{2i} on Y_i, where the weights are proportional to the variation in Z_iX_{1i} and Z_iX_{2i} in each risk set.

Proof of Proposition 2: By the Frisch-Waugh-Lovell theorem, the risk set-specific OLS estimates of Equation (B.1) are equal to

$$\beta_{1,w} = \frac{cov(Y_i, \widetilde{Z_iX_{1i}}|W_i = w)}{var(\widetilde{Z_iX_{1i}}|W_i = w)}$$
$$\beta_{2,w} = \frac{cov(Y_i, \widetilde{Z_iX_{2i}}|W_i = w)}{var(\widetilde{Z_iX_{2i}}|W_i = w)} \tag{B.5}$$

where the conditional $\widetilde{Z_iX_{1i}}|(W_i = w)$ denotes the residual of Z_iX_{1i} over the linear projection of Z_iX_{1i} on the space spanned by the covariates $X_{1i}, X_{2i}, Z_iX_{2i}$, and 1 <u>within</u> the risk set $W_i = w$, $Z_iX_{1i} - E^*[Z_iX_{1i}|X_{1i}, X_{2i}, Z_iX_{2i}, 1, W_i = w]$. $\widetilde{Z_iX_{2i}}|(W_i = w)$ similarly denotes the residual of

Z_iX_{2i} over the linear projection of Z_iX_{2i} on the space spanned by the covariates $X_{1i}, X_{2i}, Z_iX_{1i}$, and 1 within the risk set $W_i = w$, $Z_iX_{2i} - E^*[Z_iX_{2i}|X_{1i}, X_{2i}, Z_iX_{1i}, 1, W_i = w]$.

By the Frisch-Waugh-Lovell theorem, the full-sample OLS estimates of Equation (B.4) are equal to

$$\beta_1^R = \frac{cov(Y_i, \widetilde{Z_iX_{1i}})}{var(\widetilde{Z_iX_{1i}})}$$
$$\beta_2^R = \frac{cov(Y_i, \widetilde{Z_iX_{2i}})}{var(\widetilde{Z_iX_{2i}})} \tag{B.6}$$

where (with some abuse of notation) the unconditional $\widetilde{Z_iX_{1i}}$ denotes the residual of Z_iX_{1i} over the linear projection of Z_iX_{1i} on the space spanned by the covariates $X_{1i}, X_{2i}, Z_iX_{2i}, 1$, and W_i, $Z_iX_{1i} - E^*[Z_iX_{1i}|X_{1i}, X_{2i}, Z_iX_{2i}, 1, W_i]$. $\widetilde{Z_iX_{2i}}$ similarly denotes the residual of Z_iX_{2i} over the linear projection of Z_iX_{2i} on the space spanned by the covariates $X_{1i}, X_{2i}, Z_iX_{1i}, 1$, and W_i, $Z_iX_{2i} - E^*[Z_iX_{2i}|X_{1i}, X_{2i}, Z_iX_{1i}, 1, W_i]$.

Conditional on $W_i = w$, by Assumption A4, the residuals $\widetilde{Z_iX_{1i}}$ and $\widetilde{Z_iX_{2i}}$ from Equation (B.6) are equal to the residuals $\widetilde{Z_iX_{1i}}|W_i = w$ and $\widetilde{Z_iX_{2i}}|W_i = w$ from Equation (B.6) for a given risk set $W_i = w$. To see why this equivalence holds, note that the residual $\widetilde{Z_iX_{1i}}$ from Equation (B.6) can be expressed as

$$\begin{aligned}
\widetilde{Z_iX_{1i}} &= Z_iX_{1i} - E[Z_iX_{1i}|X_{1i}, X_{2i}, Z_iX_{2i}, W_i] \\
&= Z_iX_{1i} - (\pi_{2,1}Z_iX_{2i} + \pi_{3,1}X_{1i} + \pi_{4,1}X_{2i} + \pi_{5,1} + \underbrace{\Pi_{1,w}}_{\text{constant on } W_i=w}) \\
&= Z_iX_{1i} - E[Z_iX_{1i}|X_{1i}, X_{2i}, Z_iX_{2i}, 1, W_i = w] \\
&= \widetilde{Z_iX_{1i}}|(W_i = w)
\end{aligned}$$

We can similarly show that the residual $\widetilde{Z_iX_{2i}}$ from Equation (B.6) can be expressed as

$$\widetilde{Z_iX_{2i}} = \widetilde{Z_iX_{2i}}|(W_i = w)$$

Finally, by the law of iterated expectations, the regression estimator β_1^R can be written as a weighted average of risk set-specific effects of the treatment X_{1i}:

$$\begin{aligned}
\beta_1^R &= \frac{E[\widetilde{Z_i X_{1i}} Y_i]}{E[(\widetilde{Z_i X_{1i}})^2]} = \frac{E[E[\widetilde{Z_i X_{1i}} Y_i | W_i = w]]}{E[E[\widetilde{Z_i X_{1i}}^2 | W_i = w]]} \\
&= \frac{E_w[\beta_{1,w} var(\widetilde{Z_i X_{1i}} | W_i = w)]}{E_w[var(\widetilde{Z_i X_{1i}} | W_i = w)]} \\
&= E_w\left[\beta_{1,w} \times \left(\frac{var(\widetilde{Z_i X_{1i}} | W_i = w)}{E_w[var(\widetilde{Z_i X_{1i}} | W_i = w)]}\right)\right] \\
&= \sum_w \beta_{1,w} \times \underbrace{\left(\frac{var(\widetilde{Z_i X_{1i}} | W_i = w)}{E_w[var(\widetilde{Z_i X_{1i}} | W_i = w)]} \times Pr(W_i = w)\right)}_{\text{weight on } \beta_{1,w}}
\end{aligned}$$

Given the same assumptions, the regression estimator β_2^R can be similarly written as a weighted average of risk set-specific effects of the second treatment X_{2i}:

$$\beta_2^R = \sum_w \beta_{2,w} \times \underbrace{\left(\frac{var(\widetilde{Z_i X_{2i}} | W_i = w)}{E_w[var(\widetilde{Z_i X_{2i}} | W_i = w)]} \times Pr(W_i = w)\right)}_{\text{weight on } \beta_{2,w}}$$

□

Comment on the Weights for β_1^R and β_2^R: The covariate-adjusted weights underlying β_1^R and β_2^R are increasing in the variation in each treatment intensity in each risk set $W_i = w$ and the sample share of each risk set $W_i = w$. The covariate-adjusted weights underlying β_1^R and β_2^R are not necessarily the same, as the relative variation in each treatment intensity $var(\widetilde{Z_i X_{1i}} | W_i = w)/E_w[var(\widetilde{Z_i X_{1i}} | W_i = w)]$ and $var(\widetilde{Z_i X_{2i}} | W_i = w)/E_w[var(\widetilde{Z_i X_{2i}} | W_i = w)]$ may differ across risk sets W_i. This issue is problematic to the extent that the relative variation in each treatment intensity is correlated with the risk set-specific gains from treatment $\beta_{1,w}$ and $\beta_{2,w}$. In contrast, the weights underlying β_1^M and β_2^M can be chosen by the researcher to be identical, albeit at the possible cost of statistical precision in finite samples. We therefore present estimates from both our regression and matching estimators throughout.

Appendix B References

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