A Bittersweet Jubilee: Effects of Randomized Medical Debt Relief*

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Abstract

One in six Americans has medical debt in collections amounting to more than $140 billion nationally. Concerned by the burden of medical debt, non-profits and governments have undertaken large, high-profile efforts to relieve medical debt. We analyze two randomized control trials that relieved medical debt with a face value of $172 million across 84,970 people. We track outcomes using credit reports, a multimodal survey, and debt collector data. There are three sets of results. First, we estimate precise null effects of debt relief on financial distress, and no statistically significant effects on credit access or utilization. Second, we find that debt relief causes a modest but statistically significant increase in future collections due to decreased repayment of existing medical bills, as measured in our debt collector data. Third, we find no impact of medical debt relief on mental health on average, but fairly large detrimental mental health effects for those with the highest baseline medical debt. The negative effects on mental health are larger for a random subset of the treatment group who received additional outreach that raised awareness of the debt relief. We discuss explanations for these results and implications for stakeholders and policymakers.

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1 Introduction

Consumer financial distress is common in the United States (Keys et al., 2023) and medical debt is particularly widespread. Analysis of credit report data shows that roughly one in six adults has medical debt in collections. At $140 billion, aggregate medical debt in collections exceeds debt in collections from all other sources combined (Kluender et al., 2021).

Debt relief has been shown to have large benefits outside of the medical context, with evidence indicating that it can improve financial well-being, increase earnings, and even reduce mortality (Dobbie and Song, 2015; Dobbie et al., 2017; Di Maggio et al., 2019). In the medical context, *upstream* interventions provide suggestive evidence that debt relief may be similarly beneficial. In the Oregon Health Insurance Experiment (Finkelstein et al., 2012), random assignment to Medicaid improved mental health. Since the effect appeared before insurance had kicked in, the result was attributed to the peace of mind from anticipated financial protection. Financial assistance programs, which relieve debt shortly after a hospitalization, have been shown to increase health care utilization, including refills of medication for treatment-sensitive conditions (Adams et al., 2022). Moving downstream, a correlational literature has shown strong associations between indebtedness and adverse health outcomes (Jenkins et al. 2008; Meltzer et al. 2013)

This prior evidence underpins a common view that downstream interventions that relieve medical debt months or years after the precipitating health event would be beneficial for patients. We conducted a survey of academic and policy experts and respondents frequently predicted meaningful benefits from such an intervention. In recent years, widespread beliefs in the benefits of debt forgiveness has fueled millions of dollars in donations, and spending by local governments to buy and relieve medical debt.

Yet despite this evidence and enthusiasm, there are reasons to be cautious about extrapolating the benefits of upstream interventions and debt relief in other contexts to medical debt. Medical debt often sells for pennies on the dollar, reflecting low recovery rates and implying the balance sheet effects of debt forgiveness may be only a fraction of the face value of the debt. Many states have strong consumer protection regulations for medical debt, which may reduce the consequences of owing medical debt relative to debt from other sources. Moreover, correlations between medical

debt and detrimental health outcomes may reflect the impacts of precipitating medical or financial circumstances, rather than the causal impacts of medical debt.

This paper studies the impact of two randomized control trials (RCTs) of medical debt relief conducted by RIP Medical Debt (RIP), a non-profit organization that raises funding from donors and uses these resources to purchase and abolish medical debt. In total, these experiments provided relief of medical debt with a face value of $172 million for 84,970 patients.

In the first, "hospital debt" experiment, RIP coordinated with a large hospital system and a debt collector to purchase a random subset of the debt portfolio as it was sold to the debt collector, which occurred between 6 and 24 months after the date of the medical service. The purchases took place in 18 waves between August 2018 and October 2020 at a price of 5.5 cents per dollar of debt. The treatment group, which had their debt relieved, consisted of 14,377 people and $19 million in debt relief at face value, for an average forgiveness of $1,352 per person. The hospital sold control group debt to the collector, which pursued repayment following in their usual course of business.

In the second, "collector debt" experiment, RIP purchased a random selection of debt that had been under collection in the secondary market for several years. These purchases took place in three waves, two in March 2018 and one in October 2018. Debt that has remained unpaid despite collections efforts has lower recovery rates, and RIP was able to purchase this debt at a price of 1 cent per dollar or less. The treatment group consisted of 70,593 people and $153 million in face value debt relief, for an average of $2,169 per person.

We study the impact of debt relief using three data sources. We obtained collections account data which provides baseline information for the entire study sample. We can also use the occurrence of medical debt in future waves to provide a window into the effects of debt relief on downstream repayment behavior. We link our entire study sample with quarterly credit-report data from TransUnion, which allows us to track credit market outcomes for at least one year before and after treatment assignment. For a subset of subjects in the hospital debt experiment, we conducted a multimodal survey to collect information on mental and physical health, healthcare utilization, and financial wellness. The intensive survey protocol, which included up to five mailings, twice-weekly email invitations, paper survey instruments sent via certified mail, and telephone interviews conducted by a trained U.S. call center, resulted in a survey sample of 2,900 individuals.

We pre-specified our empirical specification, primary and secondary outcomes, and heterogeneity analyses. Because of the randomized nature of the debt relief intervention, we are able to recover the causal impacts of debt relief with a parsimonious regression specification that controls for wave (and where appropriate, strata and survey wave) fixed effects. We adjust our inference for multiple hypothesis testing as we pre-specified.

We find no effect of medical debt relief on financial outcomes measured in credit bureau data. We estimate a precise zero effect on the number of delinquent accounts, our primary outcome. We estimate economically small and statistically insignificant effects on other measures of financial distress, credit access, and utilization. Notably, the debt collector did not report medical debt to credit bureaus, which would have mechanically generated differences in debt in collections and credit scores between the treatment and control group. Hence, we interpret these results as null effects on behavioral margins as measured in credit bureau data.[1]

Although we find no financial impacts in credit bureau data, we find that debt relief causes a statistically significant and economically modest increase in future medical debt sent to the collections agency. We estimate that debt relief increases the amount of future debt in collections by \$14 or 7.2% of the control mean of \$199. The increase in debt in collections is almost entirely explained by lower repayment of existing bills, rather than changes in healthcare utilization that occurred after treatment assignment.

Consistent with the lack of impacts on financial outcomes measured in credit bureau data, we do not detect statistically significant effects on survey measures of financial wellness. In addition, we do not detect statistically significant effects on self-reported healthcare utilization. Our pre-specified heterogeneity analysis do not indicate any statistically or economically meaningful patterns for these outcomes.

Our survey results point to statistically significant negative mental health effects of medical debt relief for some groups and null effects for others. Following the Oregon Health Insurance Experiment, our primary survey outcome is moderately severe depression on the 8-question Patient Health Questionnaire (PHQ-8), for which we are powered to detect modest effects. We estimate

[1] Recent evidence from the CFPB indicates that non-reporting of medical debt has been the norm in the industry, lending external validity to our findings.

that debt relief causes a statistically insignificant 2.8 percentage point increase in depression (p-value of 0.11) on average. Pre-specified heterogeneity analysis shows that this mean is composed of null effects for those below the 75^{th} percentile of baseline medical debt (and equivalently, of medical debt relieved), and a large and statistically significant 8.9 percentage point effect (p-value of 0.02) for those in the top quartile of baseline medical debt relief.

Results for other measures of mental well-being — including a 7-question screen for generalized anxiety disorder (GAD-7), and measures of stress, health, and subjective well-being — follow a similar pattern, with statistically insignificant detrimental effects on average, but with larger and more statistically significant detrimental effects for those in the top quartile of potential debt relieved.

We also implemented a sub-experiment to test whether the effects on mental health was mediated by awareness of the debt relief among the treatment group. For a randomized subset of our hospital-debt treatment group, we administered phone call outreach that reinforced the standard informational letters sent by RIP to recipients of medical debt relief. We confirm that the calls increased awareness of the treatment and find that the detrimental effects on depression and anxiety are concentrated among those assigned to receive this additional informational outreach.

A central question raised by our findings is what explains the negative impacts on mental health, in particular for those who had the greatest amount of debt relief. Our results are consistent with mechanisms where, over time, recipients realize that the debt relief is insufficient. At first, receiving one-time debt relief may have created the false expectation of additional medical debt relief, consistent with the negative repayment effects and (marginally significant) increases in self-reported difficulty paying other bills (p-value of 0.10). Mental health may have suffered as subjects learned over time that the expected additional debt relief would not arrive or if the debt relief drew salience to the recipient's broader financial vulnerability that the relief was insufficient to address.

In this regard, our results echo the impacts of randomized cash transfers in Jaroszewicz et al. (2022), who find that modest cash transfers worsen psychological well-being, potentially by increasing the salience of the recipients' financial deprivation. Indeed, individuals in the highest quartile of baseline debt in collections (who received the largest debt relief and experience the largest deleterious effects on mental health), were by all measures in the credit bureau data in a

substantially worse financial situation than the rest of the sample, and thus may find debt relief particularly insufficient.

More broadly, our results relate to XYZ literature.

The remainder of this paper is structured as follows. Section 2 provides background on our setting and describes the experiment. We describe our data sources in Section 3 and our empirical framework in Section 4. Section 5 presents and discusses our results. Section 6 concludes.

2 Background and Experiment

2.1 Background

Patients incur medical debt when they fail to pay bills associated with utilization of health care services. When a patient has not paid their bill by its due date, providers will typically engage in direct outreach to patients to attempt to collect payment. After a prolonged period of non-payment, providers may place the delinquent bill with a debt collector. Patients with delinquent medical debt face a wide range of actions intended to induce payment: collectors may contact patients at home or at their place of employment; the debt may be reported to credit bureaus where it will be made visible to potential lenders, employers, and landlords through credit reports; and patients can be sued for their delinquent bills and face resulting judgments that can entail wage garnishment and even jail sentences for failure to appear in court (Presser, 2019). Delinquent medical debts that are purchased from hospitals by collections agencies can be subsequently sold on the secondary market to other agencies that will continue attempting to collect payment.

In addition to simply evading communication efforts from collectors, debtors can benefit from a variety of legal limits on collections efforts. Debtors can discharge their medical debts in bankruptcy, and medical debt is subject to a statute of limitations of about six years in most states. Medical debt is also subject to a 180-day waiting period before it can be reported to credit bureaus. Moreover, providers are legally obligated to provide emergency care to patients regardless of existing debts.

Possibly as a result of these limitations, medical debt is both highly prevalent and priced in a way that reflects low rates of repayment. As of 2020, about 18% of Americans had delinquent medical debt listed on their credit report (Kluender et al., 2021). This figure represents a lower bound on

the prevalence of medical debt as many medical debts are not reported to the credit bureaus. The amount of medical debt reported to the credit bureaus has declined since the implementation of the Affordable Care Act (Kluender et al., 2021; Guttman-Kenney et al., 2022), and reporting of medical debts to collections is poised to fall further as the credit bureaus have ceased reporting medical collection accounts under $500, paid medical collection accounts, or medical collection accounts less than a year old (Consumer Financial Protection Bureau, 2023b) and the CFPB considers further rulemaking to remove medical bills from credit reports (Consumer Financial Protection Bureau, 2023a).

Medical debts facing active collection efforts from debt collectors without reporting to the credit bureaus, as reflected in the majority of our experimental sample, is increasingly the norm. While it is becoming more difficult to measure, medical debt is nevertheless remarkably prevalent: Lopes et al. (2022) find in a nationally representative sample that 41% of Americans have some form of medical debt and that an additional 16% have had medical debt within the past five years. In our setting, the price of medical debt purchased from hospitals, and the price of medical debt in collections, was 5.5 cents and 1 cent per dollar of face value or less, respectively, which suggests debt collectors are unlikely to recover payment for the vast majority of accounts and that recovery rates continue to fall over time.

2.2 Experiment

We study medical debt relief provided by RIP Medical Debt, a 501(c)3 non-profit organization that raises funding from donors and uses these resources to purchase and abolish medical debt. We separately examine instances in which RIP randomized the forgiveness of (1) *hospital debt* acquired directly from hospitals before the debt was sent to a collection agency and (2) *collector debt* acquired from a collection agency on the secondary market after collection attempts were made. See Figure 1 for a graphical illustration of these experiments.

Hospital Debt Experiment The hospital debt stems from medical care provided by a large hospital system. 97% of the sample comes from five different states: Arizona (13%), Arkansas (5%), Louisiana (6%), Texas (50%), and Utah (24%). After a patient receives care, this hospital

system attempts to recover payment from the patient's insurance, other payors, and the patient. After 6 to 24 months, the hospital system assembles a portfolio of debt, which they would normally sell to a debt collector.

RIP coordinated with the hospital system and debt collector to purchase medical debt immediately prior to the debt being sent to collections. These purchases occurred in 18 waves basis between August 2018 and October 2020. Each wave contained the latest set of unpaid bills that were to be sent to collections, listing amount owed and a limited set of information on debtors. Within each wave, unpaid bills were grouped at the individual level, and individuals were stratified by the amount of debt, state of residence, insurance status, and a collections score. Within each of these strata, individuals were randomly assigned individuals to treatment or control. In a typical wave, 20% of individuals were assigned to treatment, although the treated percentage varied in some waves depending on the amount of donor funding available. See Appendix Table A1 for wave-by-wave statistics.

For treated individuals, RIP purchased all debt present in a given wave at a price of 5.5 cents per dollar and abolished it, eliminating any obligation to pay the debt. At approximately three weeks after debt abolishment, RIP mailed treated individuals a notice informing them of the abolishment of their debts (Appendix Figure A1 shows an example letter). A second letter containing the same information was sent out three weeks after the first. For purchase waves 6 through 14, a subset of treatment individuals were randomly selected to receive phone outreach.

For control individuals, the debt collector purchased all debts present in a given wave and collected on those debts following their standard protocols. The collector's stated protocol is as follows. For the first 24 months, each account goes through a series of six collection tiers, with each tier lasting approximately four months. At the beginning of each tier, the account is placed with a third party that takes responsibility for outreach to the debtor. Outreach includes frequent mail and telephone communication. At the end of each tier, the account is recalled from the third party, and the cycle begins with a new party responsible for outreach at the next tier. If an account has remained unpaid after the first three tiers (i.e., after about one year of collections efforts), some accounts are identified for potential litigation. Debtors with sufficiently high-value accounts and with sufficient resources (e.g., homeowners) are subject to litigation, although in practice this

comprises only a small minority of accounts.[2] Accounts not selected for litigation are placed into the following tier. During outreach, the agency may offer settlements to debtors that allow them to fulfill their obligation by paying a discounted amount. The nature of the settlement depends on the likelihood of repayment. For example, accounts in later tiers are typically offered more generous settlements because these debts are less likely to be paid.[3] These protocols are typical in the debt collection industry, in which outreach efforts are subject to both state and federal regulations, including the The Fair Debt Collection Practices Act. As is increasingly typical in the medical debt collections industry, these debts were not reported to credit bureaus.

Column 1 of Table 1 provides summary statistics on the hospital debt sample (data used for these statistics is described in more detail below) and Appendix Table A1 provides more detail on the 18 waves that comprise this experiment. The total sample consists of 75,873 persons owing $103 million of medical debt at face value. Medical debt relief was provided to 14,377 people (18.9% of the sample), amounting to $19 million in relief at face value. On average, individuals in this sample owed $1,352 of medical debt at face value (interquartile range of $235 to $1,475), and incurred debt 5.1 quarters after the date of the medical service (interquartile range of 4.7 to 5.4 quarters).

Collector Debt Experiment The collector debt was purchased from the collections agency, and consisted of debt that had already been owned by collectors in the secondary market for several years. As a result, these debts had been subject to collections efforts throughout this period of time. These prior collections efforts largely mirrored those of the hospital debt and largely consisted of mail and telephone outreach. 46% of the debtors live in three states: Texas (21%), Florida (15%), and Massachusetts (10%).

RIP coordinated with the debt collector to purchase a random subset of debt in three waves: two in March 2018 and one in October 2018. For each purchase, RIP was provided with a portfolio of unpaid accounts. Similarly to the hospital debt sample, each account listed the amount owed and identifying characteristics of each debtor. Accounts were grouped together for each individual

[2]The process of screening accounts for litigation takes about 60 days, during which the collector will use updated credit bureau data of debtors to attempt to infer their financial positions.

[3]During the COVID-19 pandemic, collections rates increased, consistent with overall declines in regular spending (Chetty et al., 2020) and medical indebtedness overall (Guttman-Kenney et al., 2022). According to the collector, more aggressive settlements were offered during this time period in order to try to capture some of the increase in household liquidity.

and individuals were stratified by location, debt age, individual age, and debt amount. Within each stratum, individuals were assigned to treatment or control. The share of treated individuals depended on donor funds available for purchase. Because donors typically prioritized debt forgiveness in particular locations, the treated share varied by strata.

Medical bills that have remained unpaid for several years despite ongoing collections efforts are less likely to be paid than bills that have newly been sent to collections. Accordingly, RIP was able to purchase the debt at a price of less than 1 cent per dollar, or roughly one-fifth the price of the hospital debt. RIP purchased and abolished the available medical debt of individuals in the treatment group. These treated individuals were notified with a letter (Appendix Figure A1). Control individuals continued to be subject to the debt collector's normal debt collection efforts.

Column 4 of Table 1 provides summary statistics on the collector debt sample and Appendix Table A1 provides more detail. Debt relief was provided to 70,593 treated individuals, amounting to 50.4% of 140,176 individuals in the collector debt sample. The total face value of debt relief was $153 million, an average of $2,169 per person. Debt was relieved on 28.3 quarters after the medical service provision on average (interquartile range of 22.7 to 28.8 quarters).

2.3 Expert Survey

We surveyed academic experts and policymakers to elicit their predictions of the results of the experiment, using results from the Oregon Health Insurance Experiment (Finkelstein et al., 2012) as a benchmark. Future drafts will incorporate more details on this survey and discuss how our results compare to these predictions.

3 Data

3.1 Collections Account Data

We define our combined study sample as the set of individuals who were potentially subject to randomized debt relief in either the hospital debt or collector debt experiments. For each of these individuals, the debt collector provided us with a dataset that included identifying information (i.e., name, date of birth, social security number, address, and phone number), the amount owed,

and the date and facility where the medical service was provided. For individuals in the hospital debt experiment, we also received information on health insurance status.

The hospital debt experiment consists of 18 waves. We use the first wave where a person appears to define their baseline characteristics. To measure the effect on future medical debt, we construct an outcome variable defined as the sum of medical debt appearing after the initial wave. The average individual is observed in XX subsequent waves at XX% appear in at least one additional wave. We also construct separate measures of future debt by whether the associated medical service occurred before or after initial treatment assignment. This enables us to distinguish effects on future debt accrual that are driven by changes in utilization versus changes in debt repayment.

3.2 Credit Bureau Data

We link individuals in the study sample to credit bureau records from TransUnion, one of the three nationwide credit reporting agencies. TransUnion collects information from lenders, debt collectors, and public records on nearly all consumer debts. We purchased quarterly credit records for our study sample for the period spanning March 2017 to December 2021. This time period allows us to measure outcomes at least four quarters before and after treatment assignment for all waves in our study sample. We also received a nationally representative random sample of 94,592 credit reports, which we use to contextualize our study population.

TransUnion linked individuals to their credit reports using names, addresses, dates of birth, phone numbers, and social security numbers. We were unable to consistently match XX% of the individuals in the study sample, and we accordingly excluded these individuals from the analysis of credit bureau data.[4]

We analyze credit-report outcomes across six pre-registered domains which include measures of financial distress (accounts and balances past due and in default), debt in collections (accounts and balances of debt in collections), bankruptcy (whether an individual filed within the prior 12 months), access to credit (whether an individual has a credit score, credit score, and the total borrowing limit

[4]Of the unmatched XX people in the combined study sample, XX are in the hospital debt experiment (XX% of the study sample) and YY are in the collector debt experiment (ZZ% of the study sample). The unmatched rates are virtually identical in the treatment and control groups within the hospital debt sample (XX% vs. YY%) and collector debt sample (XX% vs. YY%)

across credit cards), unsecured and borrowing (credit card accounts and outstanding balances), and secured borrowing (auto loan tradelines and outstanding balances).[5]

3.3 Survey Data

We contracted with the National Opinion Research Center (NORC) to conduct a multimodal survey of the hospital debt sample to collect information on mental and physical health, healthcare utilization, and financial wellness. We provide a brief overview of the survey methodology and survey instrument here; more detail is available in Appendix A.

The surveys were sent to a subset of our hospital debt sample who entered the study after September 2019 (waves 6 through 18) and owed at least $500 in medical bills to the collections agency in their initial wave. We imposed these restrictions because we thought that reducing the lag between debt relief and the survey and excluding those with modest debt relief would increase the likelihood of detecting effects. Of this sample, we randomly selected 15,001 individuals to receive the survey protocol. This sample size was chosen because it exhausted our budget and achieved modest minimum detectable effects for our primary outcome (discussed below). The survey protocol was conducted in two rounds, the first from November 2020 to February 2021, and the second from June to September 2021.

To construct our survey protocol, we started with the protocol in Baicker et al. (2013), which asked a similar set of questions to a demographically similar study population. We then modified our protocol based on discussions with NORC survey experts and two pilot surveys (with outreach to 1,000 and 3,000 subjects) where we tested survey modalities and experimentally varied the amount of the upfront payment and completion payments.

Our final survey protocol included up to five mailings, twice weekly email invitations, and telephone interviews conducted by a trained call center staff. Respondents could complete the survey through the personalized web survey link included in the mail and e-mail invitations, over the phone, or using a paper survey instrument. NORC ran all addresses on file through the UPS address validator tool and TransUnion's TLO service to verify and update addresses, as well as

[5] We split out unsecured and secured borrowing into different domains, though they were preregistered together. We also omit or modify outcomes that we expected to receive in the credit report data but which were either not included or unusable.

obtain phone numbers and up to five email addresses per respondent, once at before commencing the survey protocol and again before sending the paper version of the survey. Individuals were offered a $50 incentive for completing the survey.

The survey protocol spanned 13 weeks. Survey subjects were first contacted via postal mail and e-mail, both of which included a personalized web link to the survey and simple instructions for accessing the survey via any device. The mailed invitation was sent in a colored 6"-by-9" envelope and included a $2 up-front payment to attract attention. Throughout the protocol, individuals received twice weekly e-mail reminders (cycling through available email addresses) and reminder postcards every other week via postal mail. In the fourth week, individuals received a follow-up mailer via postal mail. In the fifth week following the initial invitation, individuals were mailed the full survey instrument along with a prepaid return envelope and a $5 up-front payment via FedEx certified mail. Between the sixth and twelfth weeks, trained call center workers contacted individuals by telephone and gave individuals the opportunity to complete the survey verbally. If individuals were not interested in completing the survey over the phone, they were invited to provide their email address, asked for their consent to contact them with survey invitations via text message, and offered a new paper copy of the survey to be sent via mail. Individuals received a final "last-chance" mailer via regular mail in the eleventh week before the survey closed.

The survey instrument includes questions about the respondent's medical bills (including their origins, likelihood of repayment, and consequences for their finances), whether they have had any of their medical debt forgiven, whether they received all needed care, and assessments of their mental and physical health. We measure depression and generalized anxiety disorder using the clinically validated PHQ-8 and GAD-7 screeners, respectively, and the PHQ-8 was our primary preregistered outcome.

On average, respondents completed the surveys 13 months after receiving randomized debt relief, and 28 months after receiving the care that incurred the debt. The survey received a 19.4% response rate among the 14,974 individuals selected to be contacted. Of these, 68% responded via web survey, 10% responded via telephone interview, and 23% responded via mail survey.

3.4 Summary Statistics

Panel B of Table 1 presents summary statistics for different samples. Credit scores for our study samples are low, a natural result of selection on medical indebtedness. Over 70% of our study sample has medical debt reported to the credit bureaus compared to 11.9% of the nationally representative sample. The average credit score for the hospital debt sample is 574, roughly the 15^{th} percentile of the unconditional national credit distribution. The hospital debt sample also owes more in medical and total collections, and holds markedly less total debt, than the national credit sample, likely due to financial challenges and higher cost of credit faced by the consumers in collections.

Survey outreach was restricted to individuals in the hospital debt sample with hospital debt (i.e., unpaid bills from the hospital system) greater than $500. Naturally, this sample had slightly lower credit scores and owed more in collections than the full hospital debt sample. Respondents tended to be in slightly better financial circumstances than the outreach sample, with slightly higher average credit scores (574 versus 568) and less owed in total collections ($2,196 versus $2,777). Survey respondents generally share similar financial circumstances with the national credit sample with medical collections, exhibiting similar average credit scores (574 versus 578), amounts of medical collections ($2,196 versus $2,176), and total collections reported in the credit data ($3,358 versus $3,016).

Compared to the hospital debt and conditional national credit sample, the collector debt sample owed more on average to the collection agency than the hospital debt sample ($2,169 versus $1,352), owed more in collections reported to the credit bureau ($4,405 versus $3,348), and held less total debt ($25,212 versus $30,294), likely due to the selection into the collector debt sample of individuals who still owed an old medical bill (on average 7.1 years old). The difference in amount owed to the collection agency likely stems from the fact that each hospital debt wave pertained to the unpaid bills sent in a given month from the hospital to the debt collector, whereas each collector debt wave represented a set of unpaid bills potentially accumulated over a period of many months.

4 Empirical Framework

4.1 Regression Specification

We estimate the average treatment effect of debt relief on outcome y with the ordinary least squares regressions of the form:

$$y_i = \beta T_i + \alpha_w + \varepsilon_i \tag{1}$$

where i indexes individuals and T_i is an indicator for whether the individual was assigned to the treatment of having their debt abolished. Since the probability of treatment assignment varies across waves, we control for wave fixed effects α_w to fully isolate the experimental variation.[6] For the analysis of survey data, the probability of surveying also varies across survey waves, so we control for the full interaction of treatment wave and survey wave fixed effects $\alpha_{w,v}$. For the collector debt sample, the probability of treatment varies across strata, so we control for the interaction of treatment wave and stratum, $\alpha_{w,s}$ when analyzing outcomes in the credit bureau data (the only data source where we observe outcomes for the collector debt sample). The coefficient of interest is β and captures the average causal effect of debt abolishment on outcome y.

For our analysis of secondary outcomes, we adjust our standard errors to account for multiple testing within each pre-specified domain of variables. Specifically, we report p-values that adjust for multiple outcomes using the free step-down resampling method of Westfall and Young (1993), along with standard unadjusted p-values for reference. See Anderson (2008) for details on this approach and Finkelstein et al. (2012) for an application.

We examine treatment effect heterogeneity across three pre-registered baseline characteristics for the hospital sample: the baseline amount of medical debt in the collections data, the age of the individual, and the timespan between medical service and debt relief. To do so, we assign individuals to quartiles of each characteristic and interact those quartiles with the treatment indicator, T_i.

We also analyze a sub-experiment within the hospital debt sample, which randomized a subset of debt relief recipients to receive phone calls to inform them of their debt relief, to test whether

[6] In the hospital debt experiment comprised of 18 experimental waves, an individual whose debt was assigned to treatment or control in a given wave may have debt that appears in a future wave. For the purposes of estimating Equation 1, we assign individuals the treatment status assigned to their debt in the first wave in which they were observed. This approach recovers the causal impact of being treated with debt forgiveness at one point in time, and abstracts from multiple treatments, which occur for a small share of our sample.

debt relief has different effects when it is made more salient. To test this hypothesis, we include separate treatment effect dummies in Equation 1 for those who were randomly assigned to be called and those who were not.

4.2 Balance

Table 2 examines the balance of baseline individual characteristics for each of our analysis samples. We analyze balance across individual demographics (from the debt collector and survey datasets), credit bureau outcomes recorded four quarters prior to treatment assignment, and collection account characteristics reported immediately prior to treatment assignment.

Results in Table 2 confirm random assignment within the hospital debt, survey outreach, survey respondent, and collector debt samples. Assessing baseline characteristics across these samples, p-values indicate that only one of the 21 variable-sample combinations exhibits a difference that is statistically distinguishable from zero at the 5% level, consistent with chance deviations from equality.

5 Results

This section presents results organized by dataset – the credit bureau data, collections account data, and survey data.

5.1 Credit Bureau Data

Table 3 reports the average effects of debt forgiveness on credit bureau outcomes. or brevity, we focus on a subset of our pre-specified credit bureau outcomes; the full set is shown in Appendix Tables X and X. The results are based on our baseline specification that compares outcomes for treated and control individuals at 4 quarters after treatment.

The first row of Panel A reports the effect on the number of delinquent accounts (at least 30 days past due), our pre-specified primary outcome from credit bureau data. Debt relief has a null 0.00 effect on the average number of delinquent accounts (relative to a control mean of 1.10 accounts), and we can reject effects outside of of -0.04 to 0.04 range with a 95% confidence interval.

Table 3 also reports effects on alternative measures of financial distress. Consistent with the null effects on delinquency, we estimate precise null effects on the number of accounts in default (≥90 days past due, second row of Panel A), the dollar value of balances past due and in default (remainder of Panel A), the number and dollar value of debts sent to collections (Panel B), and whether the individual filed for bankruptcy in the prior 12 months (Panel C).[7]

The remaining panels report the effects of debt relief on credit access and utilization. Panel D shows no effect on credit access, measured as whether the individual has a credit score, their credit score conditional on having one, and their combined credit card limit. Panels E and F show no effect on credit card and auto borrowing. The estimates are statistically insignificant and economically small. For example, a 95% confidence interval rejects an effect on credit card borrowing outside of -$45 to $97 (relative to a mean of $1,465), and rejects an effect on auto loan balances outside of -$314 to $224 (relative to a mean of $7,980).

Columns 4-6 report treatment effects for the collector debt sample. Treated individuals in the market debt sample received relief for medical debts that were typically 7.1 years old, as compared to 1.3 years old for the hospital debt sample. Consistent with the findings for the hospital debt sample, we find similarly null effects for debt collector debt sample.

5.2 Collections Account Data

Table 4 reports the effect of medical debt relief on the accrual of future medical debt. We conduct this analysis using the 18 waves of the hospital debt experiment and define future debt sent to collections based on whether an individual has additional accounts sent to collections in any wave after they first appear.

Panel A shows that medical debt relief caused a 1.0 percentage point or 6.6% increase in the probability of having a delinquent medical account held by the debt collection agency relative to a control mean of 15.5%. The amount of debt increased by $14 or 7.2% of the control mean of $199. Both outcomes are statistically significant.

[7] Recall from Section 2 that the collections agency did not report medical debts it held to credit agencies. Effects on debt in collections reported in Panel C therefore will not reflect "first stage" effects of the debt relief intervention. More generally, results of the study will not capture the effects of *mechanical* increases in credit scores (and thereby access to credit) triggered by the removal of a medical collection. Rather, the study isolates demand-initiated responses to the debt relief on credit supply, borrowing, repayment, and distress.

The increase in debt sent to collections could result from either increased utilization of medical care or decreased repayment for services already received. We construct separate measures of future medical debt based on whether the associated medical service occurred before or after the date of initial debt relief. Panel B shows that vast majority of the increased debt accumulation is associated with pre-relief medical services. This implies that the estimated increases in agency-held debt in collections in response to debt relief is largely driven by lower repayment of *existing* bills.

Panel C shows that debt relief causes small and statistically insignificant increases in medical collections associated with post-relief medical services. Conceptually, the experience of debt relief could lower the perceived "price" of medical care, increasing healthcare utilization and medical debt. While our point estimates on post-relief medical services are small, we do not have the precision to rule out economically significant proportional effects on utilization. Our collections account data do not extend sufficiently far beyond the date of debt relief, with only 10% of medical collection balances occurring after assignment to treatment.

We examine potential heterogeneity in the debt accrual effect by baseline amount of collector debt. In principle, the effect of debt relief may be larger for individuals with larger amounts of baseline debt sent to collections (also equal to the amount relieved for the treatment group). As shown in Figure 2 and Appendix Table A5, debt relief caused increases in the probability and the amount of future debt accrual for all quartiles, with monotonically larger effects for larger debt quartiles. For quartile four, debt relief of $3890 on average led to an increase in the probability of future debt accrual by 1.65pp (12.03%), and a $34.29 (12.84%) increase in future collector debt.[8]

We also examine potential heterogeneity in the debt accrual effect by age of debt, and by age of the debt relief beneficiary. We hypothesized larger effects for younger debt given the higher propensity of baseline repayment implied by the generally higher cost of younger debt on the secondary market; and hypothesized ambiguous effects by age of beneficiary. Appendix Figures A2 and A3 show no notable differential treatment effects across these subgroups. It is possible that the range of age of debt in the sample (from 10.5 months on average for quartile one to 20.65 months

[8]Because our experimental design did not randomize across the amount of debt relief, cross-quartile differences in treatment effects reflect differential effects by amount of debt relief, and by selection into different amounts of baseline debt. Financial conditions of individuals across quartiles also differed, with individuals in the higher quartiles exhibiting monotonically weaker financial conditions in terms of baseline collectors debt, medical and total debt in collections reported to credit bureaus.

for quartile four) did not include debt that was sufficiently fresh to generate the stronger impacts. The price of the collector debt was 5.5 cents per face value dollar for the entire hospital sample, potentially indicating small differences in expected repayment by debt age within the sample.

5.3 Survey Data

Table 5 shows the average effects of debt forgiveness on the pre-specified survey outcomes.

Depression Previous experimental findings point to improvements in mental health as one of the key benefits of increased access to healthcare (Finkelstein et al., 2012). Accordingly, our pre-specified primary outcome for the survey data analysis is whether an individual has at least moderately severe depression on the 8-question Patient Health Questionnaire.

Table 5, Panel A reveals no detectable improvements in depression. We estimate a statistically insignificant 2.8 percentage point increase in this outcome (p-value of 0.11) relative to a mean of 26.6%. The confidence intervals allow us to reject reductions in depression rates of more than 0.6 percentage points (2%).

Our pre-analysis plan also specified heterogeneity analysis of effects on depression by quartiles of baseline collector debt, which we present in Panel C of Figure 2. We estimate null effects in quartiles one through three. For quartile four, we estimate a fairly large and statistically significant 8.9 percentile point effect (p-value of 0.02) relative to a control mean of 28.6%.

We expected debt relief would result in improvements in mental health. For instance, the Oregon Health Insurance Experiment found that provision of health insurance (which led to large reductions in medical debt) reduced moderately severe depression by 9.15 percentage points (Baicker et al., 2013). The Oregon experiment administered the same PHQ measure with a similar survey instrument to a comparable population so it seemed like a natural benchmark. Scaling the income-value of the subsidized Medicaid benefit, we had conducted power analysis that showed we could detect meaningful reductions in depression. Moreover, the amount of medical debt owed is strongly correlated with worse mental health outcomes, a pattern previously established in Jenkins et al. (2008) and Meltzer et al. (2013) and also present in our study sample (see Appendix Figure A4). We discuss potential reasons for the sharp contrast between these prior findings and the experimental

impacts of debt relief in Section 5.

Other Health Measures Analyzing additional measures of mental well-being results in findings that are consistent with the estimated negative impacts on depression. The second and third rows of Table 5, Panel A show the average effects on whether the individual had at least moderately severe anxiety on the 7-question screener for Generalized Anxiety Disorder (GAD-7) and whether they report being sometimes stressed. Similar to the depression measures, we estimate statistically insignificant increases of 2.3 percentage points (p-value of 0.17) for anxiety and 2.8 percentage points (p-value of 0.08) for stress. As before, the heterogeneity analysis shown in Figure 2 and Appendix Table A8 indicates effects for those in the quartile 4 of baseline effect, with a 8.5 percentage point effect (p-value of 0.02) for anxiety and a 6.0 percentage point effect (p-value of 0.07) for stress.

This pattern is also borne out in the effects on those reporting at least good health in a self-assessment measure of general health and at least pretty happy on a measure of subjective well-being. Table 5, Panels B and C show statistically insignificant worsening of 2.7 percentage points (p-value of 0.17) for subjective well-bring and 2.4 percentage points for general health (p-value of 0.22). For these measures, too, heterogeneity analysis shown in Figure 2 and Appendix Table A8 indicates effects for those in the quartile four of baseline amount of debt sent to collections, with a -7.7 percentage point effect (p-value of 0.052) for subjective well-being and a -7.9 percentage point effect (p-value of 0.049) for at least good health.

Healthcare Utilization and Financial Wellness We do not detect evidence of meaningful impacts on healthcare utilization (Table 5, Panel D). Debt relief causes statistically insignificant 2.4 percentage point and 2.0 percentage point reductions in the probability of receiving all needed healthcare and prescription medicines over the past 12 months, respectively. The 95-percent confidence interval contains meaningfully positive and negative effect sizes (-6.2pp to 1.4pp from a base of 56.8pp). Heterogeneity analysis indicate monotonically larger negative effects for higher baseline collector debt quartiles, but unfortunately imprecision in the estimates do not permit further inference.

Consistent with our analysis of the credit bureau data, we find no systematic evidence of impacts

on financial distress (Table 5, Panel E). Debt relief causes a marginally statistically significant increase of 3.9 percentage points (off of a base of 60.9 percentage points) in the share of individuals having trouble paying bills (p-value of 0.042, rising to 0.10 after multiple-inference adjustment). We also find more precisely estimated zero effects for cutting back on spending or increased borrowing. Moreover, we find no effects for any quartile subgroup, by baseline collector debt, debt age, or age of beneficiary. Nor do we find any systematic pattern across those quartiles. Although we find an increase in unpaid bills in the collector data (a measure of financial distress that is not captured in the credit bureau data), these effects are small, consisting of a average increase in future accrual of collector debt of $14 as reported in 5.2. We find minimal effects of medical debt relief on financial distress, consistent with the impacts measured in both the credit bureau and the debt collector data.

5.4 Awareness

A natural channel through which the effects might operate is awareness of medical debt relief via the letters from RIP Medical Debt (see Appendix Figure A1). In the survey, we asked whether the individual had medical debt forgiven in the prior 18 months and, if yes, how much debt they had forgiven (we also asked for their total household medical debt to estimate the share relieved). Table 6 shows that treated individuals are 15.7 percentage points more likely to report debt forgiveness (p-value less than 0.001) relative to a control mean of 5.7%. Treated individuals also report having nearly three times more debt forgiven. Moreover, the share of respondents who report that medical debt forgiveness had at least some impact is nearly the same share that reports having medical debt forgiven, with treated individuals 13.5 percentage points more likely to report an impact of the medical debt forgiveness (p-value less than 0.001). This suggests that recipients of debt relief *perceive* that they experienced an impact from the debt relief, presumably a positive one.

To explore the role the salience of the debt relief plays in mediating the treatment effects, we conducted a randomized telephone follow-up to reinforce knowledge of the intervention. Of the 2,738 treatment individuals assigned to receive the caller intervention, 32.25% were contacted or left pre-scripted voicemails. Table 6 indicates that individuals assigned to receive the follow-up call were 5.2 percentage points (p-value of 0.127) more likely than other debt intervention treatment

individuals, and 14 percentage points (p-value of 0.000) more likely than control individuals, to report debt forgiveness relative to a control mean of 5.7%.

Analysis of the randomized caller intervention indicates that awareness of debt relief mediates the overall negative effects on depression. Table 7 presents heterogeneous treatment effects of debt relief by random caller assignment among the waves eligible for caller assignment (waves 6–14, comprising 55% of the hospital debt sample). The negative effect of debt relief on depression are highly concentrated among those who were assigned telephone follow-up, with the called and treated individuals 6.5 and 6.4 percentage points more likely to report at least moderate anxiety than the uncalled treated group and the control group, respectively(p-value for called treated individuals of 0.02). The effect for anxiety is also marginally more negative for the group that were assigned callers, with the called and treated individuals 4.8 percentage points more likely to report at least moderate anxiety than the uncalled treated group (p-value for called treated individuals of 0.12). For all of the other outcomes, we cannot reject the null that the point estimates are the same.[9]

5.5 Discussion

A central question raised by our findings is what explains the pattern of results we observe, in particular the following three facts: (1) Debt relief reduces repayment of future medical bills; (2) Those who receive the largest debt debt relief are the most likely to report worsened mental health and have the largest reductions in repayment; and, (3) Those assigned to receive phone calls increasing the salience of the debt relief report worse mental health while those not assigned do not. We did not expect these results, and did not design the experiment or survey to disentangle their underlying mechanisms, but we can bring some evidence to bear to reconcile them.

Our results are consistent with mechanisms whereby recipients experience the debt relief as insufficient. While the notice letter (Appendix Figure A1) explicitly states "The forgiveness is for this outstanding bill only. We have not forgiven any other medical debt you might owe.", receiving the debt relief may have created the false expectation of additional medical debt relief to follow. Such an expectation would have generated the negative repayment effects and (marginally

[9] Unfortunately, the caller intervention was implemented after most of the forward waves of collector debt, so were are unable to test whether assignment to the caller intervention differentially impacted future accrual of collector debt.

significant) increases in self-reported difficulty paying other bills (p-value of 0.10). Mental health may have suffered due to this difficulty, or in response to learning that additional debt relief would not arrive as expected. Moreover, individuals in the highest quartile of baseline medical collections accrue the most future medical collections (as observed in the control group). Expectations about future debt relief, if applied to these larger amounts of future debt, would predict the larger impacts on repayment and mental health

The debt relief may have also worsened mental health by drawing attention to individuals' financial deprivation and the insufficiency of the debt relief, a finding consistent with recent evidence of negative effects on wellbeing of small cash transfers Jaroszewicz et al. (2022). In our context, individuals in the highest quartile of medical collections are in the worst financial circumstances. Their debt relief, while larger than the relief given to the rest of treated population, represents only a small fraction of their observed indebtedness, and a smaller fraction than compared to the rest of the treated sample.[10] Hence, individuals in this group may find the debt relief particularly insufficient. While difficult to reconcile with standard economic models, there is precedent for positive but modest transfers worsening mental health. Jaroszewicz et al. (2022) find that recipients of randomized one-time modest unconditional cash transfers of $500 and $2,000 experienced significant reductions in their psychological well-being, with larger effects for the $2,000 treatment group. They argue that, by receiving a transfer insufficient to close the gap between their resources and their needs, the transfer increased the salience of recipients' financial distress. By contrast, studies have consistently found positive mental health benefits of large and sustained unconditional cash transfers .

The caller sub-experiment was designed to test the importance of the salience in mediating the effects of debt relief. We find that the negative mental health effects are concentrated in those who received this sub-treatment: those assigned to calls were 24% more likely to suffer from at least moderately severe depression than *both* the treated group that was not assigned to calls and the control group. This was despite the vast majority of call recipients having neutral to positive

[10] As reported in Appendix Table A2, this group on average owed $5,592 in debt in collections reported to credit bureaus, and $3,890 in collector debt, but had an average credit limit of only $2,410, indicating a considerable shortfall between financial obligations and their means. Debt relief for this group averaged $3,890. By contrast individuals in the lowest quartile owed $2,965 in reported collections, and $XYZ in collector debt, with a credit limit of $3,517. These individuals received on average $XYZ in debt relief.

reactions to being informed of the debt relief.[11] Given low levels of awareness of medical debt, notification of debt relief may have been the first recipients learned of the bill and served to draw attention to their financial deprivation.

Collectively, our results highlight the importance of estimating the causal impact of debt relief. Medical bills always arrive alongside a negative health shock, often rendering individuals unable to work and in need of follow-on care involving further costs. We find that one-time medical debt relief is insufficient to materially change the economic circumstances of recipients and may serve to draw attention to the financial deprivation individuals are experiencing. This stands in stark contrast to more substantial forms of debt relief and upstream interventions including health insurance and financial assistance programs, all of which have demonstrated meaningful and significant benefits for recipients (Dobbie and Song, 2015; Dobbie et al., 2017; Finkelstein et al., 2012; Baicker et al., 2013; Adams et al., 2022).

6 Conclusion

This draft is preliminary and incomplete, but future versions will include a conclusion.

[11] The scripted call followed closely the information contained in the RIP letter. In addition to recording whether recipients were reached, whether they had received the letter, the callers noted the content and tone of recipient responses. 63% expressed gratitude for the debt relief, 30% expressed neutral acknowledgement, and 6% expressed disbelief.

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Table 1. Summary Statistics

	Hospital Debt Experiment			Collector Debt Experiment	Nationally Represenative Sample	
	All	Outreach	Respondents		All	> $0 medical debt in collection
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment Overview						
Observations						
Total	75,873	15,001	2,900	140,176	94,592	11,251
Treated	14,377	5,332	1,089	70,593	.	.
Control	61,496	9,669	1,811	69,583	.	.
Aggregate medical debt ($)						
Total	102,546,054	33,898,464	6,097,328	303,987,695	.	.
Treated	18,992,055	11,848,058	2,154,878	153,203,628	.	.
Control	83,553,999	22,050,407	3,942,449	150,784,068	.	.
Person-level medical debt ($)						
Mean	1,352	2,260	2,103	2,169	.	.
25th percentile	235	815	794	300	.	.
50th percentile	620	1,340	1,275	823	.	.
75th percentile	1,475	2,426	2,273	2,077	.	.
Mean medical debt age (quarters)						
Mean	5.1	5.1	5.2	28.3	.	.
25th percentile	4.7	4.7	4.7	22.7	.	.
50th percentile	5.0	5.0	5.0	24.3	.	.
75th percentile	5.4	5.4	5.5	28.8	.	.
Panel B: Summary Statistcs						
Demographics						
Age (years)	43.1	41.5	41.7	46.0	.	.
Male (%)	45.2	46.7	38.8	43.1	.	.
Race (%)						
Black	.	.	18.8	.	.	.
White	.	.	62.2	.	.	.
Credit Bureau Data						
Credit score	574.3	568.1	574.2	571.3	682.8	577.9
Medical debt in collection (%)	68.8	76.0	74.3	72.6	11.9	100.0
Medical debt in collection ($)	1,958.0	2,356.9	1,982.1	2,798.6	165.0	1,387.0
Debt in collection ($)	3,003.3	3,522.0	3,143.4	3,794.1	399.5	2,294.6
Total debt ($)	29,195	27,131	36,676	24,275	47,680	28,623

Notes: Table reports pre-treatment summary statistics for the collector and hospital debt experiments with a comparative national representative sample from 2018Q3. Column (1) is the sample with debt bought directly from a hospital system. Columns (2) and (3) respectively report the results for a subset of the hospital debt sample who were contacted and responded to a survey on mental and financial health outcomes. Column (4) reports summary statistics for the sample with debt bought from a debt collection agency. The credit bureau summary statistics for a nationally representative sample of individuals are reported in column (5). Column (6) reports the results for a subset of these individuals who have more than $0 in medical debt in collections observed in the credit bureau data. Aggregate medical debt reports the sum of all first-wave medical debt. Credit bureau variables averaged across the four pre-treatment quarters.

Table 2. Balance on Baseline Characteristics

	Hospital Debt Experiment						Collector Debt	
	Hospital Debt		Survey Outreach		Respondents			
	Control Mean (1)	Difference (2)	Control Mean (3)	Difference (4)	Control Mean (5)	Difference (6)	Control Mean (7)	Difference (8)
Demographics								
Age (years)	43.1	-0.0	41.5	-0.0	41.9	-0.4	46.1	-0.1
		[0.929]		[0.845]		[0.504]		[0.260]
Male (%)	45.2	0.2	46.5	0.2	39.8	-3.3	43.2	-0.2
		[0.691]		[0.817]		[0.086]		[0.791]
Race (%)								
Black	.	.	.	.	18.1	1.6	.	.
						[0.285]		
White	.	.	.	.	62.8	-1.0	.	.
						[0.597]		
Collector Data								
Person-level medical debt ($)	1,359	2	2,281	-61	2,177	-176	2,167	5
		[0.916]		[0.231]		[0.085]		[0.849]
Mean medical debt age (quarters)	5.2	-0.0	5.1	0.0	5.2	-0.0	28.3	-0.1
		[0.298]		[0.923]		[0.872]		[0.017]
Has health insurance (%)	60.9	-0.4	48.1	-0.2	57.6	-1.7	.	.
		[0.352]		[0.790]		[0.372]		
Other								
Response rate (%)	.	.	.	.	18.7	1.3	.	.
						[0.058]		
Sample size †	61,496	14,377	9,669	5,332	1,811	1,089	69,583	70,593

Notes: Table presents the balance of pre-determined characteristics within the collector debt, hospital debt, and survey samples. In the first column of each panel, we present the debt relief control group mean. In the second column, we present the treatment group's average difference after controlling for the first wave fixed effects as well as the p-value for each difference (reported in square brackets). For the collector and hospital debt samples, we include fixed effect for first wave of debt relief of the beneficiary. For the survey outreach and respondents sample, we interact first wave of debt relief with fixed effects for NORC survey wave assigned to the beneficiary.
†: Sample size for control and treatment groups reported in odd- and even-numbered columns respectively.

Table 3. Effects of Debt Relief on Credit Bureau Outcomes

	Hospital Debt Sample			Debt Collector Sample		
	Control Mean	Treatment Effect	p-value	Control Mean	Treatment Effect	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Distress						
Number of accounts past due $^{+}$	1.10	-0.00	[1.000]	0.91	0.02	[0.132]
		(0.02)	.		(0.01)	.
Number of accounts in default	0.98	-0.00	[0.936]	0.81	0.01	[0.156]
		(0.02)	{0.949}		(0.01)	{0.388}
Balances in default ($)	3,228	-17	[0.818]	2,897	27	[0.518]
		(73.83)	{0.949}		(42.35)	{0.755}
Debt past due ($)	240	18	[0.189]	155	-2	[0.687]
		(13.64)	{0.433}		(4.32)	{0.755}
Panel B. Debt in Collections						
Number of debts in collections	4.92	0.07	[0.455]	3.79	0.07	[0.147]
		(0.10)	{0.515}		(0.05)	{0.243}
Debts in collections ($)	4,095	-59	[0.346]	3,094	44	[0.174]
		(62.82)	{0.515}		(32.06)	{0.243}
Panel C. Bankruptcy						
Bankruptcy in last 12 months (%)	1.30	-0.05	[0.670]	0.65	-0.04	[0.346]
		(0.11)	.		(0.04)	.
Panel D. Access to Credit						
Has credit score (%)	97.22	-0.11	[0.492]	90.80	0.07	[0.644]
		(0.16)	{0.860}		(0.16)	{0.730}
Credit score (excluding missing)	582.16	-0.10	[0.890]	577.63	-0.35	[0.410]
		(0.76)	{0.894}		(0.42)	{0.730}
Credit card limit ($)	2,939	48	[0.624]	2,987	52	[0.361]
		(97.13)	{0.860}		(56.63)	{0.730}
Panel E. Unsecured and Secured Borrowing						
Number of credit cards	0.84	0.00	[0.930]	0.81	0.01	[0.258]
		(0.02)	{0.964}		(0.01)	{0.445}
Credit card balance ($)	1,465	26	[0.466]	1,307	36	[0.048]
		(36.08)	{0.864}		(18.32)	{0.142}
Number of auto loans	0.40	0.00	[0.705]	0.31	0.00	[0.186]
		(0.01)	{0.964}		(0.00)	{0.394}
Auto loan balance ($)	7,980	-45	[0.741]	5,444	40	[0.482]
		(137.37)	{0.964}		(57.57)	{0.475}

Notes: Sample sizes are N = 68,651 for hospital sample and N = 133,965 for debt collector sample. Sample size for credit score outcome Standard errors in parenthesis, un-adjusted p-values and multiple-inference-adjusted p-values in square and curly brackets respectively. Multiple inference adjustment using the (Westfall and Young, 1993) method by domain.
Estimates are computed as outlined in Equation 1. Balance variables are winsorized at the 99$^{\text{th}}$ percentile.

Table 4. Effects of Debt Relief on Future Medical Debt

	Control Mean (1)	Treatment Effect (2)	p-value (3)
Panel A. Full Sample			
Amount of debt ($)	198.71	14.34	[0.038]
		(6.91)	
At least some debt (%)	15.52	1.02	[0.003]
		(0.34)	
Panel B: Pre-Relief Medical Services			
Amount of debt ($)	177.40	12.80	[0.042]
		(6.30)	
At least some debt (%)	14.62	0.99	[0.003]
		(0.33)	
Panel C: Post-Relief Medical Services			
Amount of debt ($)	6.40	0.20	[0.686]
		(0.50)	
At least some debt (%)	1.74	0.08	[0.505]
		(0.12)	

Notes: Table presents the repayment effect of debt relief treatment on the extensive margin (probability) and intensive margin (balances) for our hospital debt experiment. Within each panel, we include the mean of the control group in (1), report the treatment effect in (2) with standard errors reported below (in parenthesis), and the p-value (in brackets) in (3). The total sample size for control is 61,496 people and 14,377 for the treatment.
Panel A presents effects for any future debt; Panel B presents effects for future debt whose service date is before our debt relief intervention; Panel C presents effects for future debt whose service date is after our debt relief intervention. Amount of debt is winsorized at 99^{th} percentile after aggregating accounts to the person level. The treatment estimate comes from Equation 1.

Table 5. Effects of Debt Relief on Survey Outcomes

	Control Mean (1)	Treatment Effect (2)	p-value (3)
Panel A. Mental Health			
At least moderately severe depression (%)	26.62	2.81	[0.110]
		(1.76)	.
At least moderately severe anxiety (%)	23.58	2.27	[0.179]
		(1.69)	{0.17}
At least sometimes stressed (%)	76.48	2.79	[0.084]
		(1.62)	{0.15}
Panel B: Subjective Wellbeing			
At least pretty happy (%)	54.22	-2.68	[0.167]
		(1.94)	.
Panel C: General Health			
At least good health (%)	53.78	-2.39	[0.218]
		(1.94)	.
Panel D. Health Care Utilization			
Had all needed healthcare (%)	56.82	-2.39	[0.216]
		(1.93)	{0.38}
Had all needed RX (%)	71.73	-2.00	[0.255]
		(1.76)	{0.38}
Panel E: Financial distress			
Had trouble paying other bills (%)	60.87	3.85	[0.042]
		(1.90)	{0.10}
Cut back spending on spending (Z-score index)	-0.00	-0.02	[0.606]
		(0.04)	{0.61}
Increased borrowing (Z-score index)	0.00	0.05	[0.172]
		(0.04)	{0.29}

Notes: Table presents health and self-reported financial distress effects of medical debt relief within the NORC survey sample. The sample size is 2,899 with one singleton observation dropped. Column (1) reports the mean of respondents who were in the control in the hospital debt experiment, column (2) reports the treatment effect for respondents who were treated in the hospital debt experiment with standard errors reported in parenthesis, and column (3) reports unadjusted and multiple-inference-adjusted p-values for column (2). Multiple inference adjustment using the (Westfall and Young, 1993) method by domain.
Estimates are computed as outlined in Equation 1.

Table 6. Effect of Medical Debt Forgiveness on Self-Reported Awareness of Medical Debt Forgiveness

	Control Mean	Treatment Effect (full sample)	p-value	Treatment Effect (no call)	p-value	Treatment Effect (caller assigned)	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Full Sample							
Had medical debt forgiven (%)	5.7	17.2	[0.000]	.	.	.	.
		(1.81)		.		.	
Amount of medical debt forgiven ($)	139.8	313.5	[0.000]	.	.	.	.
		(61.41)		.		.	
Medical debt forgiveness had at least some impact (%)	3.1	15.4	[0.000]	.	.	.	.
		(1.62)		.		.	
Panel B. Caller Experiment							
Had medical debt forgiven (%)	5.7	.	.	14.6	[0.000]	19.8	[0.000]
		.		(2.41)		(2.54)	
Amount of medical debt forgiven ($)	139.8	.	.	302.5	[0.001]	324.1	[0.000]
		.		(90.20)		(74.88)	
Medical debt forgiveness had at least some impact (%)	3.1	.	.	13.9	[0.000]	16.9	[0.000]
		.		(2.21)		(2.30)	

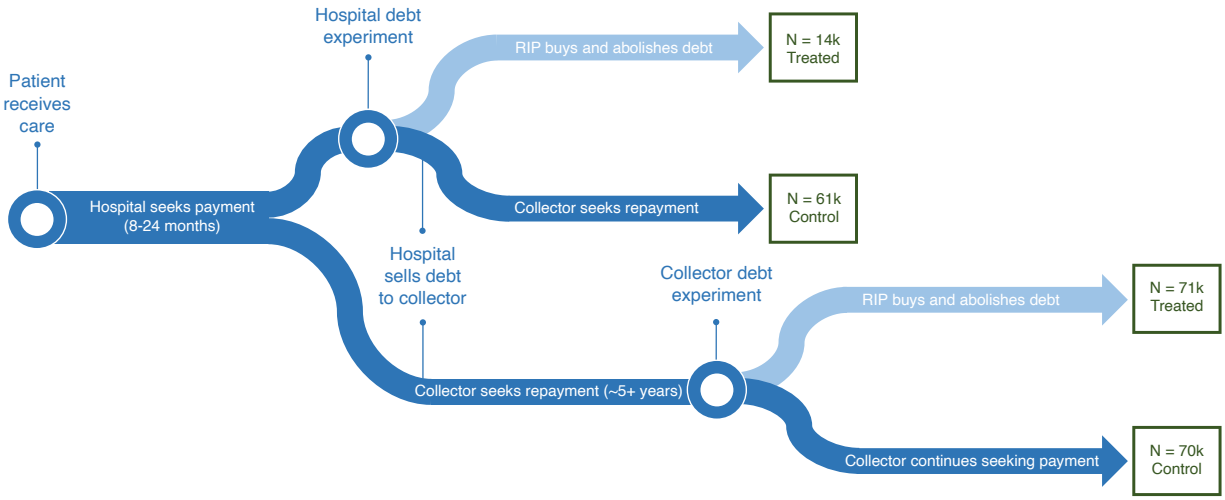
Notes: Table presents the effect of medical debt relief in addition to the caller sub-experiment effect on self-reported medical debt forgiveness and its impact. Sample size for medical debt forgiven, amount forgiven, and impact of forgiveness is 1,686, 1,699, and 1,674 respectively across both panels. Column (2) reports the treatment effect of debt relief on all treated respondents with standard errors reported in parenthesis. Corresponding p-values are reported in column (3) in square and curly brackets respectively. Column (4) reports the treatment effect of debt relief for treated respondents who were not assigned to receive a call with standard errors reported in parenthesis and unadjusted and adjusted p-values reported in column (5). Column (6) reports the treatment effect for treated respondents who were assigned to receive a call for the call experiment with standard errors reported in paranthesis; column (7) reports the corresponding unadjusted and adjusted p-values.

Table 7. Heterogeneous Treatment Effects on Survey Outcomes by Call Assigned

	Control Mean	Treatment Effect (no call)	p-value	Treatment Effect (caller assigned)	p-value
	(1)	(2)	(3)	(4)	(5)
Panel A. Mental Health					
At least moderately severe depression (%)$^{+}$	27.0	-0.1	[0.957]	6.4	[0.021]
		(2.64)	.	(2.78)	.
At least moderately severe anxiety (%)	24.6	-0.6	[0.810]	4.2	[0.116]
		(2.56)	{0.82}	(2.67)	{0.22}
At least sometimes stressed (%)	76.7	4.5	[0.059]	2.9	[0.238]
		(2.40)	{0.12}	(2.42)	{0.25}
Panel B: Subjective Wellbeing					
At least pretty happy (%)	54.1	-2.9	[0.330]	-1.1	[0.701]
		(2.98)	.	(2.95)	.
Panel C: General Health					
At least good health (%)	53.6	-1.3	[0.665]	-2.9	[0.335]
		(2.99)	.	(2.96)	.
Panel D. Health Care Utilization					
Had all needed healthcare (%)	56.6	-7.2	[0.016]	-1.3	[0.653]
		(2.97)	{0.04}	(2.95)	{0.86}
Had all needed RX (%)	70.9	-3.2	[0.240]	1.1	[0.678]
		(2.76)	{0.24}	(2.65)	{0.86}
Panel E: Financial distress					
Had trouble paying other bills (%)	61.7	5.7	[0.046]	3.8	[0.188]
		(2.86)	{0.11}	(2.86)	{0.42}
Cut back spending on spending (Z-score index)	-0.0	-0.0	[0.915]	-0.0	[0.494]
		(0.06)	{0.92}	(0.06)	{0.53}
Increased borrowing (Z-score index)	-0.0	0.1	[0.366]	0.1	[0.327]
		(0.06)	{0.58}	(0.06)	{0.53}

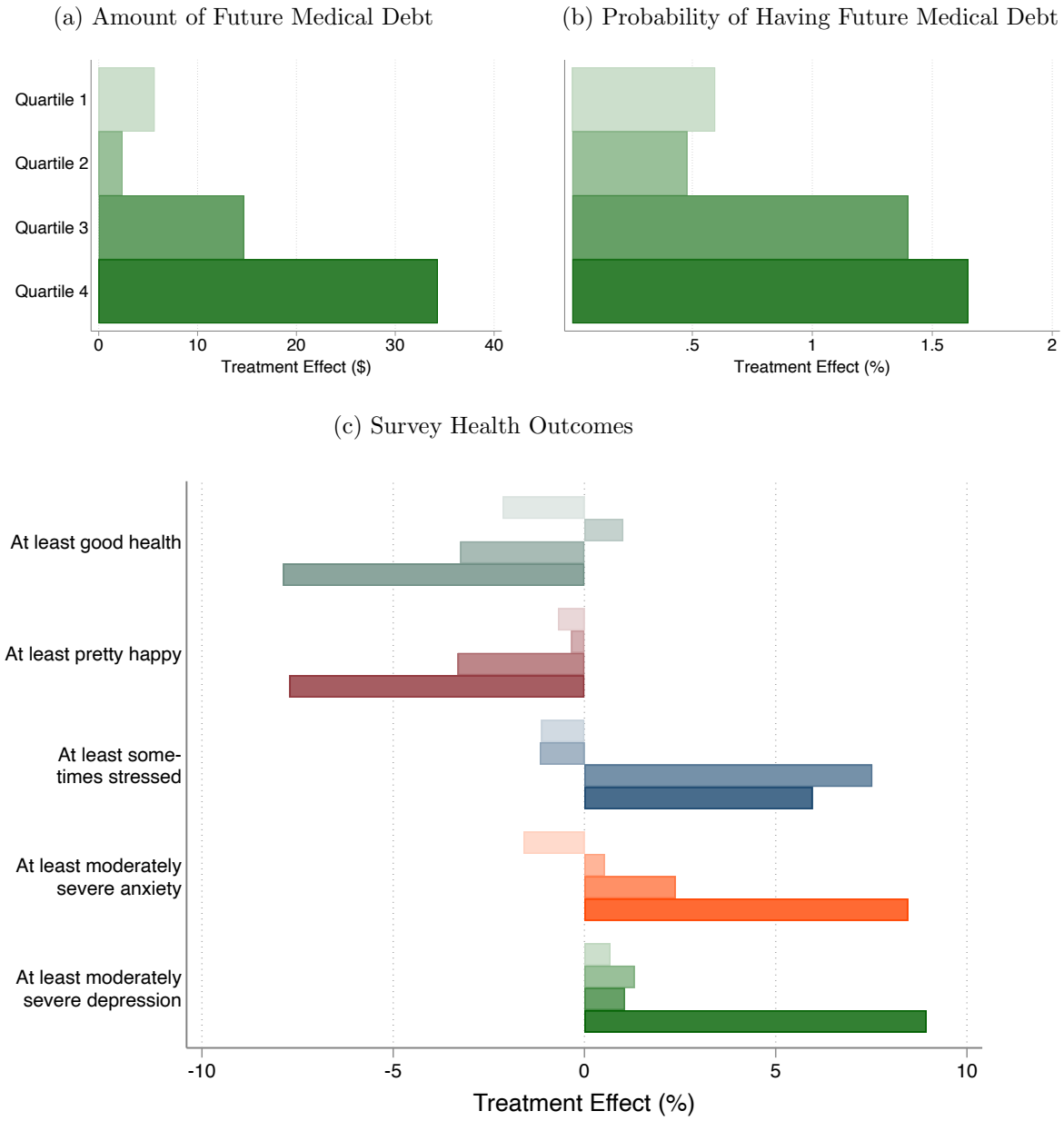
Notes: Table presents the effect of debt relief treatment on health and financial distress outcomes for waves 6-14 of the hospital debt sample surveyed. We adapt the specification from Equation 1 by adding an additional interaction term between debt relief treatment and call attempted. Sample size is 2,004.
Column (1) reports the mean of respondents in the control group. Column (2) reports the treatment effect of debt relief for treated respondents who were not assigned to receive a call with standard errors reported in parenthesis and unadjusted and adjusted p-values reported in column (3). Column (4) reports the treatment effect for treated respondents who were assigned to receive a call for the call experiment with standard errors reported in parenthesis; column (5) reports the corresponding unadjusted and adjusted p-values.

Figure 1. Experiment Design



Notes: This figure presents a flow chart that illustrates how the two debt relief experiments relate to a typical timeline of medical debt accrual. After the patient receives care, the hospital seeks payment for a period of 8-24 months before selling the debt to a collections agency. Our hospital debt experiment involves purchasing and relieving debt at this stage. Under the status quo, the collector will seek repayment of the debt until it is paid or the debt has exceeded its statute of limitations. Our collector debt experiment involves purchasing and relieving debt after it has been pursued by collectors for several years.

Figure 2. Heterogeneous Effects of Debt Relief on Survey Health Outcomes and Future Medical Debt, by Medical Debt Balance in First Wave



Notes: Figure presents the heterogeneous effects of medical debt relief on health survey outcomes and future medical debt in collections by first wave medical debt balance. Panel (A) presents effect of medical debt relief on the amount of medical debt observed in future waves. Panel (B) presents the effect of medical debt relief on the probability of having medical debt observed in future waves. Panel (C) shows the effect of medical debt relief on health outcomes as measured in the survey experiment.
Quartiles are created using the medical debt balance in the first wave individuals are observed in. Quartiles are computed separately within the survey experiment (Panel (C)) and the hospital debt experiment (Panel (A) and Panel (B)). In Panel (A) and Panel (B), the quartile cutoffs are as follows Q1: [$25, $234.7], Q2: ($234.7, $620.06] Q3: ($620.06,$1,475], and Q4: ($1,475, $60,452.21]. In Panel (C), the quartiles are Q1: [$99.53, $793.60], Q2: ($793.60, $1,274.28], Q3: ($1,274.28, $2,271.41], Q4: ($2,275.03, $33,626.89]. For each outcome, the effects are pictured from lightest (Quartile 1) to darkest (Quartile 4) in descending order. See Appendix Table A8 and Appendix Table A5 for corresponding numerical values.

Appendix

A Methodological Appendix

This section provides additional information on the methods used in our survey data collection and analysis. Additional details on the cleaning of the collected survey responses, as well as information on the debt collector and credit bureau data are presented in Appendix Section B.

Survey Protocol

Proposed Timeline and Survey Protocol

Phase 1: Email and letter invitation

- Week 1: Simultaneously mail survey invitation letter with link to web survey w/ pre-paid $2 incentive (and promise of post-completion $50 incentive)
- Week 2: Begin twice weekly email reminders with link to web survey
- Week 3: Begin every other week reminder postcard with link to web survey
- Week 4: Send follow up letter (similar to initial invitation letter) with link to web survey

Phase 2: FedEx Paper Self-Assessment Questionnaires (SAQs)

- Week 5: Distribute SAQs
  - Send via FedEx with $5 pre-incentive
- Weeks 6, 8, and 10: Bi-weekly reminder postcard with link to web survey

Phase 3: Telephone Prompting and Interviewing

- Weeks 6-12: Conduct telephone prompting and telephone interviewing. Start in week 6 to allow for SAQ delivery.
  - Offer three options for survey completion:
    - Phone interview (conduct in real time)
    - Email: Ask for updated email for reminders, and consent for text message link/reminder
    - SAQ: refer to mailed SAQ, as needed, send new SAQ via mail. Ask for updated email for reminders, and consent for text message
  - Send twice weekly email reminders and (if applicable) text reminders, with updated email and/or text consent
- Week 11: Last chance letter with link to web survey

Study Details

Locating

- Verify and update addresses and obtain email and phone numbers: Run addresses and names through UPS address validator tool (SmartMailer) and TransUnion (TLO) to:

  - Verify and update addresses.
  - Obtain updated phone number, and up to 5 email addresses per respondent, based on provided name, address, etc. and returns.

- Run subsequent locating batch prior to SAQ mailing
  - Use updated address file for FedEx SAQ mailing + $5 pre-paid incentive

Multimodal Outreach

- Use email and phone contact as major modes of contact
  - Send survey invitation and twice weekly email reminders based on locating results.
  - Send reminder postcard every other week following the survey invitation letter mailing.
  - During telephone prompting, if respondents do not want to do the survey via phone, ask respondents for their:
    * Email address (to send email reminders with clickable survey link). If we already have an email address from TransUnion, we will use this to update.
    * Consent to send text messages.

- Additional targeted outreach
  - Update cases to second phone number (where available) for disconnected numbers early in CATI dialing
  - Updated cases to second phone number (where available) for cases with no contact mid-way through CATI dialing
  - Send targeted follow up emails to respondents that have started but not completed the web survey

Enhanced survey materials

- Colored, larger 6"x9" invitation envelope to stand out among mail/envelopes
- Letters include simple language, bulleted statements, and images
- Insert with the survey invitation letter mailing explains the process of logging on to the survey (with both easy to read text and pictures).
  - This helps clarify that respondents can use a smart phone or tablet to complete the survey, not just through a laptop/computer access.

B Data Appendix

B.1 Debt Collector Data

Within the debt collector dataset, each debtor can have more than one account of medical debt within a given wave. To construct individual level variables, we aggregate the information associated with each account to an individual level. Age, sex, and insurance status are computed as the modal value in the first wave an individual is observed in. Medical debt age is calculated by averaging the account-level medical debt within a person within the first wave that they are observed weighted by the amount of debt of each account. Individual-level balance is the sum of balances across all accounts within a wave (for the first wave debt measures) or across future waves (for future debt balance variables). Debt age is the mean debt age within a wave or across future waves weighted by the balance of each account.

B.2 Credit Bureau Data

Medical debt in collections is the total balance of third-party collections verified in past 12 months less the total balance of non-medical third-party collections verified in past 12 months. Debt in collections is the total balance of third-party collections verified in the past 12 months, and total debt is the total balance of all trades verified in the past 12 months.

B.3 NORC Survey Data

Within our pre-specified outcomes, the measure of whether a respondent cut back on spending and if they increased borrowing because of medical bills are constructed as follows. Spending is weighted by the inverse standard deviation of the survey outcomes of if a respondent cut spending on 1) basic necessities, 2) big-ticket items, and 3) business investments. Increased debt is also weighted by the inverse standard deviation across 1) increased credit card debt, 2) borrowed from a payday lender, 4) borrowed from friends/family, 5) used savings, or 6) increased debt on other lines of credit. For both variables, a z-score index is computed on the control observations and applied to the whole sample.

The survey asks respondents if they have had medical debt forgiven in the past 18 months, the amount of medical debt forgiven, and if the forgiveness had an impact on the respondent and their family. For those who indicated that they did not have medical debt forgiven, the amount of debt forgiven is imputed to be $0 and a personal life impact of is imputed to be none.

C Results Appendix

This section discusses results from supplementary analyses.

C.1 Heterogeneous Treatment Effects Specification

For the purposes of estimating heterogeneous treatment effects, we assign individuals to bins according to a given dimension of heterogeneity, and interact our treatment indicator and wave fixed effects with those bins. Specifically, we estimate specifications of the following form:

$$y_i = \sum_{h \in H} \left(\beta T_i \times 1\{h(i) = h\} \right) + \alpha_{w,h(i)} + \varepsilon_i \tag{2}$$

In the above, h denotes a particular bin (e.g., highest quartile of individual age), and H denotes the set of bins.

C.2 Alternative Regression Specifications

We supplement the analysis using our baseline regression specification (i.e., Equation 1) with two alternative approaches that leverage features of our data to increase statistical precision. The first alternative specification applies individual fixed effects, while the second applies additional controls. Note that neither set of controls is necessary to recover unbiased estimates of treatment effects. Rather, they increase statistical precision by absorbing variation in outcomes that is orthogonal to the likelihood of treatment assignment.

Our first alternative approach leverages the panel structure of the credit bureau data. As described in Section 3, we have a panel of quarterly outcomes for each individual in our sample, spanning at least four quarters before and four quarters after treatment. As a result, we are able to estimate regression specifications that include individual fixed effects. In particular, we estimate specifications of the following form, where i denotes individual and t denotes calendar quarter:

$$y_{it} = \beta T_i \times POST_{it} + \alpha_{wt} + \gamma_i + \varepsilon_{it} \tag{3}$$

In the above, $POST_{it}$ is an indicator that individual i was assigned to treatment or control in or before quarter t, and γ_i denotes a set of individual fixed effects. All other objects follow the notation of our primary specification (Equation 1). Also following our primary specification, for the collector debt experiment we interact the wave-quarter fixed effects with strata fixed effects because probability of treatment varied by strata in that experiment.

In estimating Equation 3, we restrict to quarters $[-4, 4]$ relative to treatment, and drop quarter 0 (which mixes pre- and post-treatment months. In addition, some of our primary outcomes are constructed by cumulating over 12 months (e.g., any bankruptcy in the last 12 months). For these 12-month outcomes, we also drop quarters $[1, 3]$, such that treatment effects are estimated by comparing outcomes 12 months after treatment to pre-treatment levels.

The results of estimating Equation 3 are presented in Appendix Tables XX, XX, and XX. Overall, we find...

Our second alternative approach aims to increase statistical precision of estimated effects on outcomes measured in survey data, for which we do not have access to pre-treatment data (and are therefore unable to include individual fixed effects). To increase statistical precision, we enrich our baseline specification to include a vector of individual-level controls:

$$y_i = \beta T_i + X_i'\gamma + \alpha_w + \varepsilon_i \tag{4}$$

In the above, X_i denotes the vector of individual-level controls. The remaining components follow the notation of our primary specification (Equation 1).

We present the results of estimating Equation 4 in Tables XX, XX. Our baseline set of controls includes XX... Overall, we find XX.

C.3 Differential Response Rates by Treatment Status

As is standard in experimental studies using survey data, one potential threat to the internal validity of our results is the possibility of differential response rates between treated and control individuals. To the extent that the propensity to respond to a survey is correlated with outcomes of interest, differential response rates bias estimated treatment effects.

In our survey sample, treated individuals were 1.3 percentage points more likely than control individuals to respond to the survey (p-value of 0.058). While this difference is not statistically significant at conventional levels, it is nonetheless valuable to evaluate the extent to which this difference can plausibly affect the magnitudes of our estimates. Accordingly, we conduct two complementary exercises to probe the extent to which this difference generates bias in our estimates.

Our first method of evaluating robustness to differential response rates is to control for observable individual characteristics. To the extent that such observable characteristics are correlated with outcomes, controlling for these characteristics mitigates any bias in the treatment effects that is generated by imbalance between treatment and control groups. Accordingly, we estimate a version of Equation 4 that controls for XX. These estimates are presented in Appendix Tables XX, XX, and XX.

Our second method for addressing differential response rates is to reproduce our baseline estimates for a subsample of the data without differential response rates. Suppose that individuals differ based on a latent propensity to respond to the survey, and that this propensity is correlated with outcomes of interest. Moreover, suppose that whether an individual responds is a function of their propensity as well as the intensity of survey outreach efforts. If this is the case, then individuals who respond later to the survey will systematically differ from those who respond earlier; however, the extent of imbalance can be reduced by dropping the individuals in the treatment group who respond latest, such that response rates between treated and control groups are equal. Moreover, if response propensity is a monotonic function only of the duration of outreach, then this adjustment will eliminate any bias in the estimated treatment effects.

We implement this method by dropping XX individuals in the treatment group, such that response rates in both treatment and control groups are 18.7%. We then estimate Equation 1 on this restricted sample, and present results in Tables XX, XX, and XX. We find that XX...

C.4 Selection into Survey Completion

In light of our 19% survey completion rate, a natural question is the extent to which these findings represent treatment effects among individuals who did not complete the survey. To do so, we conduct two complementary exercises that build on prior approaches.

Our first approach to evaluating the external validity of our results is to re-weight our sample of respondents to match our outreach sample. We do XX propensity score matching...

Our second approach attempts to go beyond the limitations inherent in sample reweighting. In particular, reweighting on observables may not be sufficient to overcome non-response bias (Dutz

et al., 2021). Accordingly, we estiamte heterogeneous treatment effects by splitting the sample by quartile of response time since the initial survey outreach. The intuition motivating this approach is similar to that which motivates our test for differential response bias in the previous subsection. If treatment effects vary systematically by an individual's propensity to respond to a survey that is monotonically increasing in the extent of survey outreach, then comparing treatment effects by response time is informative about the extent of treatment effect heterogeneity by response propensity. Appendix Table XX implements this approach...

D Appendix Tables

Table A1. Summary statistics by purchase wave

	Purchase Date	Num. Treated	Num. Control	Debt forgiven	Debt balance	Mean debt age
A. Market purchased / Old debt						
Wave 1	03-09-2018	21837	20823	2020.487	2042.772	4213.864
Wave 2	03-29-2018	1605	1604	2245.188	2199.854	3946.930
Wave 3	10-15-2018	49454	49399	2136.230	2123.924	3109.743
B. Hospital purchased / Young debt						
Wave 1	08-30-2018	617	2467	1500.864	1404.530	1231.378
Wave 2	10-25-2018	690	2761	1695.674	1670.042	1210.031
Wave 3	11-21-2018	546	2215	1083.362	1130.731	1177.694
Wave 4	12-28-2018	372	1476	1011.389	983.264	1154.414
Wave 5	01-31-2019	341	1313	969.650	945.358	1132.062
Wave 6	09-17-2019	867	5602	2308.512	2125.266	931.600
Wave 7	10-21-2019	937	3414	1786.172	1802.553	913.375
Wave 8	10-21-2019	1059	2423	1717.952	1702.814	867.488
Wave 9	12-20-2019	1001	2991	1481.530	1513.733	870.177
Wave 10	01-10-2020	584	5620	2014.194	1905.529	824.076
Wave 11	02-18-2020	733	3641	1597.021	1641.379	797.800
Wave 12	03-20-2020	775	3615	1542.456	1615.302	745.410
Wave 13	04-27-2020	986	3075	1383.724	1367.495	669.690
Wave 14	05-29-2020	1225	3670	1765.795	1762.794	630.229
Wave 15	07-08-2020	963	2812	1505.443	1489.535	591.635
Wave 16	08-13-2020	971	2904	1490.759	1481.753	641.885
Wave 17	09-21-2020	1093	9004	1545.029	1522.708	606.439
Wave 18	10-13-2020	637	2635	1473.217	1522.823	513.571
C. Aggregates						
All market purchased waves		72896	71826	2103.957	2101.686	3453.719
All hospital purchased waves		14397	61638	1595.199	1606.599	814.484

Notes: Table presents the mean of a handful of individual-level debt balance variables by purchase wave. These variables include average debt balance for the treat (imputation for debt forgiven), average debt balance, and age of debt across the whole sample. Panel A shows the Old debt waves, which were all purchased through market sources; Panel B show Young debt waves, which were all purchased through hospital; Panel C aggregates market purchased and hospital purchased waves separately.

Table A2. Heterogeneous Effects of Debt Abolishment on Credit Outcomes, by Medical Debt Balance in First Wave

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	p-value	Control Mean	Treatment Effect	p-value	Control Mean	Treatment Effect	p-value	Control Mean	Treatment Effect	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Distress												
Number of accounts past due	1.08	0.03	[0.471]	1.15	-0.04	[0.247]	1.09	-0.00	[0.921]	1.06	0.02	[0.603]
		(0.04)	.		(0.04)	.		(0.04)	.		(0.04)	.
Number of accounts in default	0.96	0.03	[0.384]	1.02	-0.03	[0.333]	0.98	-0.01	[0.795]	0.96	0.01	[0.871]
		(0.04)	{0.574}		(0.03)	{0.644}		(0.03)	{0.799}		(0.03)	{0.875}
Balances in default ($)	2,902.48	109.55	[0.435]	3,288.15	-108.61	[0.455]	3,336.54	-192.77	[0.190]	3,385.42	134.89	[0.392]
		(140.45)	{0.574}		(145.25)	{0.674}		(147.24)	{0.454}		(157.46)	{0.599}
Balances past due ($)	176.87	36.84	[0.118]	238.96	-5.12	[0.847]	259.89	-32.08	[0.215]	285.03	71.23	[0.028]
		(23.59)	{0.290}		(26.52)	{0.858}		(25.87)	{0.454}		(32.50)	{0.087}
Panel B. Debt in Collections												
Number of debts in collection	3.98	-0.05	[0.695]	4.61	0.15	[0.428]	4.92	-0.11	[0.575]	6.16	0.27	[0.289]
		(0.13)	{0.697}		(0.19)	{0.672}		(0.19)	{0.601}		(0.25)	{0.438}
Debt in collection ($)	2,964.61	-112.90	[0.281]	3,742.98	8.27	[0.943]	4,084.20	-152.02	[0.199]	5,591.88	-12.69	[0.934]
		(104.74)	{0.411}		(116.71)	{0.951}		(118.38)	{0.315}		(152.98)	{0.925}
Panel C. Bankruptcy												
Bankruptcy in last 12 months (%)	1.36	0.06	[0.801]	1.44	-0.03	[0.898]	1.35	-0.18	[0.402]	1.03	-0.04	[0.860]
		(0.23)	.		(0.23)	.		(0.22)	.		(0.20)	.
Panel D. Access to Credit												
Has Credit Score (%)	96.64	-0.65	[0.091]	97.58	-0.12	[0.707]	97.35	0.16	[0.590]	97.32	0.17	[0.567]
		(0.38)	{0.247}		(0.31)	{0.968}		(0.31)	{0.889}		(0.30)	{0.922}
Credit Score	591.38	0.84	[0.596]	580.46	-0.61	[0.689]	580.22	0.32	[0.830]	576.56	-0.72	[0.623]
		(1.58)	{0.814}		(1.51)	{0.968}		(1.49)	{0.889}		(1.46)	{0.922}
Credit limit ($)	3,516.67	-2.50	[0.990]	3,001.45	38.28	[0.850]	2,826.84	116.64	[0.549]	2,409.77	47.89	[0.792]
		(198.40)	{0.983}		(201.96)	{0.968}		(194.60)	{0.889}		(181.17)	{0.922}
Panel E. Unsecured and Secured Borrowing												
Number of credit cards	0.94	0.00	[0.961]	0.88	-0.02	[0.610]	0.83	0.02	[0.569]	0.73	0.01	[0.869]
		(0.03)	{0.980}		(0.03)	{0.820}		(0.03)	{0.805}		(0.03)	{0.985}
Credit card balance ($)	1,654.72	37.80	[0.619]	1,467.14	-13.64	[0.848]	1,439.78	61.94	[0.387]	1,297.79	21.58	[0.757]
		(76.03)	{0.952}		(70.93)	{0.858}		(71.56)	{0.765}		(69.82)	{0.984}
Number of auto loans	0.40	-0.00	[0.872]	0.42	-0.02	[0.149]	0.40	0.02	[0.181]	0.35	0.01	[0.337]
		(0.01)	{0.980}		(0.01)	{0.415}		(0.01)	{0.483}		(0.01)	{0.724}
Auto loan balance ($)	7,876.04	-73.08	[0.781]	8,440.48	-285.95	[0.307]	8,122.60	163.99	[0.561]	7,484.09	10.59	[0.969]
		(262.97)	{0.980}		(280.13)	{0.636}		(281.97)	{0.805}		(273.78)	{0.985}

Notes: Table presents the heterogeneous effects of medical debt relief on credit outcomes by quartile of medical debt balance seen in the first wave an individual is observed. Sample size is N = 68,651. The first column of each quartile reports the control mean for observations in that quartile. The second column reports the treatment effect of medical debt relief for that quartile with standard errors are reported in parenthesis. Lastly, the third column reports un-adjusted p-values and multiple-inference-adjusted p-values in square and curly brackets respectively. Multiple inference adjustment using the (Westfall and Young, 1993) method by domain. Balance variables are winsorized at the 99^{th} percentile.
Estimates are computed as outlined in Equation 1 with the independent variables interacted with a categorical quartile variable. Quartile cutoffs are as follows: Q1: [25, 226.3], Q2: (226.3, 600.25], Q3: (600.25, 1440], Q4: (1440, 60452.21].

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Table A3. Heterogeneous Effects of Debt Abolishment on Credit Outcomes, by Medical Debt Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	p-value	Control Mean	Treatment Effect	p-value	Control Mean	Treatment Effect	p-value	Control Mean	Treatment Effect	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Distress												
Number of accounts past due	1.10	0.04	[0.276]	1.16	-0.03	[0.357]	1.13	-0.03	[0.423]	0.99	0.03	[0.408]
		(0.04)	.		(0.04)	.		(0.04)	.		(0.04)	.
Number of accounts in default	0.99	0.04	[0.321]	1.03	-0.03	[0.440]	1.01	-0.02	[0.524]	0.88	0.02	[0.660]
		(0.04)	{0.969}		(0.03)	{0.969}		(0.04)	{0.969}		(0.04)	{0.969}
Balances in default ($)	3,254.97	78.45	[0.596]	3,405.49	-177.75	[0.212]	3,330.94	112.43	[0.472]	2,921.18	-60.07	[0.675]
		(147.89)	{0.992}		(142.33)	{0.992}		(156.16)	{0.992}		(143.22)	{0.992}
Balances past due ($)	265.28	33.47	[0.254]	270.41	0.38	[0.989]	252.45	16.62	[0.551]	172.90	21.80	[0.352]
		(29.34)	{0.947}		(27.49)	{0.947}		(27.88)	{0.947}		(23.42)	{0.947}
Panel B. Debt in Collections												
Number of debts in collection	6.04	0.12	[0.606]	4.93	0.02	[0.909]	4.65	0.10	[0.496]	4.06	0.04	[0.818]
		(0.23)	{0.997}		(0.20)	{0.997}		(0.15)	{0.997}		(0.18)	{0.997}
Debt in collection ($)	5,449.01	-4.41	[0.976]	4,072.58	-135.99	[0.237]	3,691.91	37.23	[0.753]	3,183.31	-175.76	[0.116]
		(143.52)	{0.999}		(114.99)	{0.999}		(118.25)	{0.999}		(111.84)	{0.999}
Panel C. Bankruptcy												
Bankruptcy in last 12 months (%)	1.43	-0.24	[0.273]	1.21	0.06	[0.785]	1.20	0.12	[0.589]	1.34	-0.14	[0.527]
		(0.22)	.		(0.22)	.		(0.23)	.		(0.22)	.
Panel D. Access to Credit												
Has Credit Score (%)	97.90	-0.25	[0.374]	97.85	-0.10	[0.718]	97.08	0.07	[0.834]	96.07	-0.14	[0.739]
		(0.28)	{0.984}		(0.28)	{0.984}		(0.34)	{0.984}		(0.41)	{0.984}
Credit Score	569.66	-1.41	[0.303]	578.87	0.90	[0.531]	583.24	-0.38	[0.799]	596.95	0.64	[0.703]
		(1.37)	{0.984}		(1.44)	{0.984}		(1.51)	{0.984}		(1.67)	{0.984}
Credit limit ($)	2,206.49	-186.98	[0.235]	2,750.07	102.03	[0.568]	2,923.08	196.79	[0.339]	3,865.81	85.78	[0.712]
		(157.40)	{0.953}		(178.89)	{0.953}		(205.95)	{0.953}		(232.07)	{0.953}
Panel E. Unsecured and Secured Borrowing												
Number of credit cards	0.66	-0.03	[0.272]	0.82	0.03	[0.291]	0.89	-0.02	[0.602]	1.01	0.02	[0.589]
		(0.03)	{0.984}		(0.03)	{0.984}		(0.03)	{0.984}		(0.04)	{0.984}
Credit card balance ($)	1,165.13	-30.29	[0.626]	1,422.97	77.41	[0.269]	1,505.60	31.49	[0.674]	1,762.52	25.99	[0.748]
		(62.12)	{1.000}		(70.11)	{1.000}		(74.79)	{1.000}		(81.02)	{1.000}
Number of auto loans	0.35	-0.01	[0.515]	0.40	0.00	[0.936]	0.41	0.00	[0.706]	0.42	0.01	[0.298]
		(0.01)	{0.999}		(0.01)	{0.999}		(0.01)	{0.999}		(0.01)	{0.999}
Auto loan balance ($)	7,109.31	-267.28	[0.277]	8,244.43	-165.80	[0.548]	8,160.77	155.58	[0.582]	8,394.40	161.35	[0.583]
		(245.94)	{0.984}		(275.78)	{0.984}		(282.49)	{0.984}		(293.87)	{0.984}

Notes: Table presents the heterogeneous effects of medical debt relief on credit outcomes by quartile of medical debt age in the first wave an individual is observed. Sample size is N = 68,651. The first column of each quartile reports the control mean for observations in that quartile. The second column reports the treatment effect of medical debt relief for that quartile with standard errors are reported in parenthesis. Lastly, the third column reports un-adjusted p-values and multiple-inference-adjusted p-values in square and curly brackets respectively. Multiple inference adjustment using the (Westfall and Young, 1993) method by domain. Balance variables are winsorized at the 99^{th} percentile.
Estimates are computed as outlined in Equation 1 with the independent variables interacted with a categorical quartile variable. Quartile cutoffs in days are as follows: Q1: [130, 428], Q2: (428, 459], Q3: (459, 498], Q4: (498, 2,177].

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Table A4. Heterogeneous Effects of Debt Abolishment on Credit Outcomes, by Beneficiary Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Distress												
Number of accounts past due	1.04	-0.03	[0.334]	1.28	0.02	[0.701]	1.22	-0.02	[0.647]	0.86	0.04	[0.252]
		(0.03)	.		(0.04)	.		(0.04)	.		(0.03)	.
Number of accounts in default	0.93	-0.03	[0.356]	1.15	-0.00	[1.000]	1.08	-0.01	[0.848]	0.76	0.03	[0.307]
		(0.03)	{0.563}		(0.04)	{1.000}		(0.04)	{0.983}		(0.03)	{0.601}
Balances in default ($)	2,681.89	-135.91	[0.253]	4,053.36	-10.00	[0.954]	3,807.11	-58.22	[0.726]	2,409.04	139.08	[0.278]
		(118.78)	{0.521}		(171.77)	{0.999}		(166.34)	{0.983}		(128.07)	{0.601}
Balances past due ($)	193.31	7.82	[0.740]	382.51	45.97	[0.207]	285.72	4.74	[0.873]	102.51	15.59	[0.318]
		(23.51)	{0.715}		(36.43)	{0.454}		(29.64)	{0.983}		(15.60)	{0.601}
Panel B. Debt in Collections												
Number of debts in collection	4.62	-0.01	[0.943]	5.73	0.02	[0.939]	5.40	0.63	[0.016]	3.98	-0.35	[0.042]
		(0.11)	{0.948}		(0.20)	{0.954}		(0.26)	{0.042}		(0.17)	{0.072}
Debt in collection ($)	4,136.33	-97.42	[0.380]	4,887.37	-148.93	[0.287]	4,522.92	123.38	[0.373]	2,859.37	-116.42	[0.281]
		(110.93)	{0.490}		(139.91)	{0.410}		(138.63)	{0.385}		(107.92)	{0.274}
Panel C. Bankruptcy												
Bankruptcy in last 12 months (%)	1.04	-0.21	[0.258]	1.47	0.11	[0.651]	1.80	-0.12	[0.621]	0.89	0.01	[0.961]
		(0.18)	.		(0.25)	.		(0.25)	.		(0.19)	.
Panel D. Access to Credit												
Has Credit Score (%)	97.88	-0.09	[0.761]	98.41	-0.18	[0.474]	98.17	-0.11	[0.690]	94.63	0.07	[0.875]
		(0.28)	{0.749}		(0.25)	{0.735}		(0.26)	{0.703}		(0.45)	{0.989}
Credit Score	568.16	-1.50	[0.269]	574.63	-1.40	[0.355]	582.89	2.82	[0.057]	604.13	-0.41	[0.799]
		(1.36)	{0.589}		(1.51)	{0.735}		(1.48)	{0.149}		(1.63)	{0.989}
Credit limit ($)	1,629.73	-87.56	[0.430]	2,897.70	-45.76	[0.805]	3,571.11	338.80	[0.140]	3,761.01	-58.20	[0.801]
		(110.83)	{0.676}		(185.83)	{0.806}		(229.41)	{0.250}		(231.49)	{0.989}
Panel E. Unsecured and Secured Borrowing												
Number of credit cards	0.61	-0.02	[0.351]	0.88	0.02	[0.523]	1.03	0.05	[0.191]	0.88	-0.05	[0.192]
		(0.02)	{0.427}		(0.03)	{0.549}		(0.04)	{0.478}		(0.04)	{0.494}
Credit card balance ($)	921.17	-60.37	[0.197]	1,523.41	77.67	[0.306]	1,841.20	43.58	[0.595]	1,609.33	40.06	[0.614]
		(46.82)	{0.427}		(75.88)	{0.523}		(81.91)	{0.831}		(79.48)	{0.846}
Number of auto loans	0.36	-0.01	[0.259]	0.42	0.02	[0.198]	0.48	-0.00	[0.843]	0.33	0.01	[0.434]
		(0.01)	{0.427}		(0.01)	{0.523}		(0.01)	{0.857}		(0.01)	{0.777}
Auto loan balance ($)	7,099.56	-402.31	[0.084]	8,980.83	398.63	[0.198]	9,602.16	-206.94	[0.492]	6,303.51	86.74	[0.728]
		(232.55)	{0.246}		(309.91)	{0.523}		(301.04)	{0.813}		(249.15)	{0.846}

Notes: Table presents the heterogeneous effects of medical debt relief on credit outcomes by quartile of debtor age in the first wave an individual is observed. Sample size is N = 68,651. The first column of each quartile reports the control mean for observations in that quartile. The second column reports the treatment effect of medical debt relief for that quartile with standard errors are reported in parenthesis. Lastly, the third column reports un-adjusted *p*-values and multiple-inference-adjusted *p*-values in square and curly brackets respectively. Multiple inference adjustment using the (Westfall and Young, 1993) method by domain. Balance variables are winsorized at the 99^{th} percentile.

Estimates are computed as outlined in Equation 1 with the independent variables interacted with a categorical quartile variable. Quartile cutoffs in years are as follows: Q1: [18, 31], Q2: [32, 41], Q3: [42, 55], Q4: [56, 89]

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Table A5. Heterogeneous Effects of Debt Abolishment on Outcomes in Debt Collector Data, by Medical Debt Balance in First Wave

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean (1)	Treatment Effect (2)	p-value (3)	Control Mean (4)	Treatment Effect (5)	p-value (6)	Control Mean (7)	Treatment Effect (8)	p-value (9)	Control Mean (10)	Treatment Effect (11)	p-value (12)
Panel A. Full Sample												
Amount of debt ($)	141.63	5.66	[0.591]	185.24	2.40	[0.847]	200.62	14.70	[0.275]	267.01	34.29	[0.054]
		(10.52)			(12.44)			(13.46)			(17.79)	
At least some debt (%)	18.23	0.60	[0.406]	15.82	0.48	[0.483]	14.32	1.40	[0.034]	13.71	1.65	[0.013]
		(0.72)			(0.68)			(0.66)			(0.66)	
Panel B. Pre-Relief Medical Services												
Amount of debt ($)	124.11	5.23	[0.580]	168.05	-4.06	[0.717]	180.50	14.97	[0.229]	236.68	34.61	[0.034]
		(9.44)			(11.19)			(12.45)			(16.31)	
At least some debt (%)	17.04	0.61	[0.381]	14.96	0.56	[0.401]	13.63	1.22	[0.059]	12.84	1.60	[0.013]
		(0.70)			(0.67)			(0.65)			(0.65)	
Panel C. Post-Relief Medical Services												
Amount of debt ($)	7.08	-0.41	[0.677]	6.07	0.36	[0.718]	5.65	0.91	[0.348]	6.79	-0.05	[0.963]
		(0.99)			(0.99)			(0.97)			(1.04)	
At least some debt (%)	2.28	0.12	[0.669]	1.71	-0.11	[0.651]	1.40	0.34	[0.142]	1.57	-0.03	[0.888]
		(0.29)			(0.24)			(0.23)			(0.23)	

Notes: Table presents the heterogeneous effects of medical debt relief on the extensive margin (probability) and intensive margin (balances) for our hospital debt experiment by medical debt balance in the first wave an individual is observed. The first column of each quartile reports the control mean for observations in that quartile. The second column reports the treatment effect of medical debt relief for that quartile with standard errors are reported in parenthesis. Lastly, the third column reports un-adjusted p-values and multiple-inference-adjusted p-values in square and curly brackets respectively. Multiple inference adjustment using the (Westfall and Young, 1993) method by domain. Balance variables are winsorized at the 99^{th} percentile. The total sample size for control is 61,496 people and 14,377 for the treatment.

Panel A presents effects for any future debt; Panel B presents effects for future debt whose service date is before our debt relief intervention; Panel C presents effects for future debt whose service date is after our debt relief intervention.

Amount of debt is winsorized at 99^{th} percentile after aggregating accounts to the person level.

Estimates are computed as outlined in Equation 1 with the independent variables interacted with a categorical quartile variable. Quartile cutoffs are as follows: Q1: [$25, $234.7], Q2: ($234.7, $620.06] Q3: ($620.06,$1,475], and Q4: ($1,475, $60,452.21].

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Table A6. Heterogeneous Effects of Debt Abolishment on Outcomes in Debt Collector Data, by Debt Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean (1)	Treatment Effect (2)	p-value (3)	Control Mean (4)	Treatment Effect (5)	p-value (6)	Control Mean (7)	Treatment Effect (8)	p-value (9)	Control Mean (10)	Treatment Effect (11)	p-value (12)
Panel A. Full Sample												
Amount of debt ($)	215.13	27.82	[0.067]	189.03	18.79	[0.145]	234.63	-11.57	[0.402]	155.85	21.17	[0.102]
		(15.18)			(12.88)			(13.79)			(12.95)	
At least some debt (%)	14.28	1.16	[0.077]	15.92	1.21	[0.074]	18.20	1.03	[0.152]	13.67	0.71	[0.285]
		(0.65)			(0.68)			(0.72)			(0.66)	
Panel B. Pre-Relief Medical Services												
Amount of debt ($)	184.26	26.13	[0.056]	163.60	14.79	[0.202]	217.16	-10.72	[0.403]	144.18	20.12	[0.094]
		(13.68)			(11.58)			(12.81)			(12.01)	
At least some debt (%)	12.93	1.11	[0.080]	14.69	1.10	[0.096]	17.69	0.90	[0.204]	13.13	0.88	[0.179]
		(0.63)			(0.66)			(0.71)			(0.65)	
Panel C. Post-Relief Medical Services												
Amount of debt ($)	9.03	0.44	[0.705]	8.44	0.38	[0.732]	4.60	0.05	[0.951]	3.62	-0.07	[0.928]
		(1.16)			(1.10)			(0.84)			(0.79)	
At least some debt (%)	2.37	0.13	[0.647]	2.36	0.09	[0.733]	1.23	0.10	[0.646]	1.01	0.01	[0.964]
		(0.28)			(0.28)			(0.21)			(0.20)	

Notes: Table presents the heterogeneous effects of medical debt relief on the extensive margin (probability) and intensive margin (balances) for our hospital debt experiment by debtor age. The first column of each quartile reports the control mean for observations in that quartile. The second column reports the treatment effect of medical debt relief for that quartile with standard errors are reported in parenthesis. Lastly, the third column reports un-adjusted p-values and multiple-inference-adjusted p-values in square and curly brackets respectively. Multiple inference adjustment using the (Westfall and Young, 1993) method by domain. Balance variables are winsorized at the 99^{th} percentile. The total sample size for control is 61,496 people and 14,377 for the treatment.
Panel A presents effects for any future debt; Panel B presents effects for future debt whose service date is before our debt relief intervention; Panel C presents effects for future debt whose service date is after our debt relief intervention.
Amount of debt is winsorized at 99^{th} percentile after aggregating accounts to the person level.
Estimates are computed as outlined in Equation 1 with the independent variables interacted with a categorical quartile variable. Quartile cutoffs days are as follows: Q1: [130, 426], Q2: (426, 457.19], Q3: (457.19, 495], Q4: [495, 2,177]

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Table A7. Heterogeneous Effects of Debt Abolishment on Outcomes in Debt Collector Data, by Beneficiary Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean (1)	Treatment Effect (2)	p-value (3)	Control Mean (4)	Treatment Effect (5)	p-value (6)	Control Mean (7)	Treatment Effect (8)	p-value (9)	Control Mean (10)	Treatment Effect (11)	p-value (12)
Panel A. Full Sample												
Amount of debt ($)	186.09	13.14	[0.319]	231.01	14.52	[0.325]	216.55	5.82	[0.692]	160.25	27.61	[0.031]
		(13.19)			(14.76)			(14.68)			(12.81)	
At least some debt (%)	13.63	1.76	[0.007]	16.52	-0.01	[0.990]	15.83	0.78	[0.272]	16.16	1.73	[0.015]
		(0.65)			(0.66)			(0.71)			(0.71)	
Panel B. Pre-Relief Medical Services												
Amount of debt ($)	167.03	8.20	[0.492]	206.33	11.06	[0.410]	189.46	15.87	[0.244]	145.60	20.36	[0.080]
		(11.92)			(13.43)			(13.62)			(11.62)	
At least some debt (%)	12.94	1.66	[0.010]	15.50	0.02	[0.980]	14.87	0.90	[0.197]	15.22	1.58	[0.024]
		(0.64)			(0.65)			(0.69)			(0.70)	
Panel C. Post-Relief Medical Services												
Amount of debt ($)	5.59	0.81	[0.407]	7.60	-0.10	[0.927]	6.97	-0.94	[0.356]	5.43	1.05	[0.273]
		(0.98)			(1.05)			(1.02)			(0.96)	
At least some debt (%)	1.35	0.16	[0.470]	1.94	-0.05	[0.843]	1.84	-0.22	[0.377]	1.84	0.47	[0.095]
		(0.23)			(0.25)			(0.25)			(0.28)	

Notes: Table presents the heterogeneous effects of medical debt relief on the extensive margin (probability) and intensive margin (balances) for our hospital debt experiment by mean medical debt age in the first wave an individual is observed. The first column of each quartile reports the control mean for observations in that quartile. The second column reports the treatment effect of medical debt relief for that quartile with standard errors are reported in parenthesis. Lastly, the third column reports un-adjusted p-values and multiple-inference-adjusted p-values in square and curly brackets respectively. Multiple inference adjustment using the (Westfall and Young, 1993) method by domain. Balance variables are winsorized at the 99^{th} percentile. The total sample size for control is 61,496 people and 14,377 for the treatment.

Panel A presents effects for any future debt; Panel B presents effects for future debt whose service date is before our debt relief intervention; Panel C presents effects for future debt whose service date is after our debt relief intervention.

Amount of debt is winsorized at 99^{th} percentile after aggregating accounts to the person level.

Estimates are computed as outlined in Equation 1 with the independent variables interacted with a categorical quartile variable. Quartile cutoffs in years are as follows: Q1: [18, 30], Q2: [31, 41], Q3: [42, 54], Q4: [55, 89].

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Table A8. Heterogeneous Effects of Debt Abolishment on Survey Outcomes, by Medical Debt Balance in First Wave

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Mental Health												
At least moderately severe depression (%)	25.78	0.68	[0.845]	23.64	1.31	[0.700]	28.47	1.06	[0.765]	28.63	8.94	[0.019]
		(3.49)	.		(3.41)	.		(3.55)	.		(3.80)	.
At least moderately severe anxiety (%)	21.56	-1.59	[0.619]	22.13	0.53	[0.874]	27.55	2.38	[0.494]	23.29	8.46	[0.020]
		(3.20)	{0.850}		(3.37)	{0.937}		(3.49)	{0.510}		(3.65)	{0.041}
At least sometimes stressed (%)	78.89	-1.13	[0.731]	74.84	-1.17	[0.740]	74.77	7.53	[0.019]	77.35	5.97	[0.065]
		(3.28)	{0.850}		(3.52)	{0.937}		(3.21)	{0.052}		(3.23)	{0.057}
Panel B. Subjective Wellbeing												
At least pretty happy (%)	57.11	-0.69	[0.858]	54.23	-0.35	[0.931]	51.85	-3.31	[0.395]	53.63	-7.71	[0.052]
		(3.88)	.		(3.96)	.		(3.90)	.		(3.97)	.
Panel C. General Health												
At least good health (%)	56.67	-2.14	[0.582]	54.66	1.02	[0.799]	52.55	-3.25	[0.406]	51.28	-7.88	[0.049]
		(3.89)	.		(4.00)	.		(3.91)	.		(3.99)	.
Panel D. Health Care Utilization												
Had all needed healthcare (%)	55.56	2.46	[0.530]	58.79	-0.04	[0.993]	58.10	-4.94	[0.196]	54.91	-6.02	[0.126]
		(3.91)	{0.544}		(3.96)	{0.993}		(3.83)	{0.314}		(3.93)	{0.135}
Had all needed RX (%)	71.33	-3.86	[0.289]	74.62	0.42	[0.905]	68.29	2.16	[0.538]	72.44	-6.35	[0.082]
		(3.64)	{0.531}		(3.53)	{0.991}		(3.51)	{0.507}		(3.65)	{0.135}
Panel E. Financial Distress												
Had trouble paying other bills (%)	61.59	0.72	[0.853]	59.29	7.83	[0.047]	61.74	7.76	[0.037]	60.92	0.57	[0.884]
		(3.85)	{0.846}		(3.94)	{0.137}		(3.72)	{0.074}		(3.92)	{0.984}
Cut back spending on spending (Z-score index)	0.04	-0.10	[0.199]	0.01	-0.08	[0.322]	-0.03	0.04	[0.661]	-0.02	0.04	[0.627]
		(0.08)	{0.368}		(0.08)	{0.432}		(0.08)	{0.673}		(0.08)	{0.934}
Increased borrowing (Z-score index)	-0.01	0.11	[0.170]	0.06	-0.09	[0.271]	-0.07	0.18	[0.025]	0.01	0.01	[0.910]
		(0.08)	{0.368}		(0.08)	{0.432}		(0.08)	{0.074}		(0.08)	{0.984}

Notes: Table presents the heterogeneous effects of medical debt relief on survey outcomes by quartile of medical debt balance seen in the first wave an individual is observed. The sample size is 2,899 with one singleton observation dropped. The first column of each quartile reports the control mean for observations in that quartile. The second column reports the treatment effect of medical debt relief for that quartile with standard errors are reported in parenthesis. Lastly, the third column reports un-adjusted *p*-values and multiple-inference-adjusted *p*-values in square and curly brackets respectively. Multiple inference adjustment using the (Westfall and Young, 1993) method by domain.
Estimates are computed as outlined in Equation 1 with the independent variables interacted with a categorical quartile variable. Quartile cutoffs are as follows: Q1: [$99.53, $793.60], Q2: ($793.60, $1,274.28], Q3: ($1,274.28, $2,271.41], Q4: ($2,275.03, $33,626.89].

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Table A9. Heterogeneous Effects of Debt Abolishment on Survey Outcomes, by Debt Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	p-value	Control Mean	Treatment Effect	p-value	Control Mean	Treatment Effect	p-value	Control Mean	Treatment Effect	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Mental Health												
At least moderately severe depression (%)	30.21	1.29	[0.723]	24.06	8.13	[0.027]	26.76	1.27	[0.718]	25.28	-0.04	[0.990]
		(3.63)	.		(3.68)	.		(3.53)	.		(3.43)	.
At least moderately severe anxiety (%)	27.66	1.17	[0.741]	22.30	4.31	[0.212]	23.13	3.46	[0.313]	21.03	0.08	[0.980]
		(3.54)	{0.940}		(3.45)	{0.196}		(3.43)	{0.356}		(3.21)	{0.980}
At least sometimes stressed (%)	80.00	0.16	[0.958]	77.26	5.32	[0.096]	75.96	4.10	[0.207]	72.48	2.61	[0.454]
		(3.11)	{0.959}		(3.19)	{0.161}		(3.25)	{0.356}		(3.48)	{0.704}
Panel B. Subjective Wellbeing												
At least pretty happy (%)	49.57	-1.31	[0.738]	53.20	-5.71	[0.151]	57.82	-3.03	[0.440]	56.60	-0.78	[0.843]
		(3.91)	.		(3.97)	.		(3.93)	.		(3.93)	.
Panel C. General Health												
At least good health (%)	49.15	-1.68	[0.670]	54.08	-2.69	[0.501]	58.73	-4.99	[0.207]	53.47	-0.34	[0.932]
		(3.93)	.		(3.99)	.		(3.96)	.		(3.98)	.
Panel D. Health Care Utilization												
Had all needed healthcare (%)	47.02	-1.77	[0.651]	58.28	-5.08	[0.191]	61.45	-2.49	[0.522]	61.07	0.07	[0.985]
		(3.92)	{0.646}		(3.88)	{0.315}		(3.90)	{0.745}		(3.75)	{0.990}
Had all needed RX (%)	65.74	-4.20	[0.262]	69.76	-2.59	[0.485]	75.51	1.58	[0.632]	76.29	-5.58	[0.104]
		(3.75)	{0.433}		(3.71)	{0.468}		(3.30)	{0.745}		(3.43)	{0.195}
Panel E. Financial Distress												
Had trouble paying other bills (%)	67.10	1.31	[0.723]	63.43	2.91	[0.447]	58.41	2.84	[0.461]	54.18	8.41	[0.031]
		(3.70)	{0.843}		(3.83)	{0.724}		(3.86)	{0.817}		(3.90)	{0.082}
Cut back spending on spending (Z-score index)	0.06	-0.05	[0.550]	-0.02	0.02	[0.844]	-0.04	0.04	[0.616]	-0.00	-0.09	[0.236]
		(0.08)	{0.843}		(0.08)	{0.831}		(0.08)	{0.826}		(0.08)	{0.387}
Increased borrowing (Z-score index)	0.07	0.06	[0.464]	-0.05	0.07	[0.372]	0.02	0.00	[0.961]	-0.04	0.10	[0.228]
		(0.08)	{0.843}		(0.08)	{0.724}		(0.08)	{0.960}		(0.08)	{0.387}

Notes: Table presents the heterogeneous effects of medical debt relief on survey outcomes by quartile of medical debt age in the first wave an individual is observed. The sample size is 2,899 with one singleton observation dropped. The first column of each quartile reports the control mean for observations in that quartile. The second column reports the treatment effect of medical debt relief for that quartile with standard errors are reported in parenthesis. Lastly, the third column reports un-adjusted p-values and multiple-inference-adjusted p-values in square and curly brackets respectively. Multiple inference adjustment using the (Westfall and Young, 1993) method by domain.
Estimates are computed as outlined in Equation 1 with the independent variables interacted with a categorical quartile variable. Quartile cutoffs in days are as follows: Q1: [198, 429], Q2: (429, 459], Q3: (459, 503], Q4: (503, 1,567].

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Table A10. Heterogeneous Effects of Debt Abolishment on Survey Outcomes, by Beneficiary Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Mental Health												
At least moderately severe depression (%)	27.61	2.88	[0.403]	29.98	-2.33	[0.522]	27.25	6.87	[0.065]	21.10	3.92	[0.245]
		(3.44)	.		(3.64)	.		(3.72)	.		(3.37)	.
At least moderately severe anxiety (%)	25.22	4.66	[0.177]	26.91	-1.81	[0.607]	24.53	2.85	[0.413]	17.03	2.76	[0.371]
		(3.45)	{0.330}		(3.51)	{0.617}		(3.48)	{0.635}		(3.09)	{0.586}
At least sometimes stressed (%)	78.26	2.14	[0.479]	76.59	7.11	[0.024]	80.50	-0.11	[0.972]	69.78	0.73	[0.843]
		(3.02)	{0.506}		(3.16)	{0.039}		(3.21)	{0.980}		(3.69)	{0.837}
Panel B. Subjective Wellbeing												
At least pretty happy (%)	54.13	-4.05	[0.281]	56.67	-2.65	[0.501]	50.73	-0.49	[0.903]	55.64	-3.16	[0.427]
		(3.76)	.		(3.93)	.		(4.04)	.		(3.97)	.
Panel C. General Health												
At least good health (%)	62.17	-4.75	[0.197]	56.24	2.00	[0.609]	50.10	-3.85	[0.332]	46.04	-2.98	[0.461]
		(3.69)	.		(3.92)	.		(3.97)	.		(4.04)	.
Panel D. Health Care Utilization												
Had all needed healthcare (%)	54.35	-3.76	[0.323]	54.05	-0.59	[0.880]	51.36	-0.96	[0.811]	68.82	-5.80	[0.127]
		(3.80)	{0.496}		(3.92)	{0.884}		(4.04)	{0.802}		(3.80)	{0.214}
Had all needed RX (%)	69.78	-3.51	[0.316]	67.40	1.41	[0.699]	70.86	-3.90	[0.294]	79.62	-3.26	[0.332]
		(3.50)	{0.496}		(3.66)	{0.883}		(3.72)	{0.494}		(3.36)	{0.322}
Panel E. Financial Distress												
Had trouble paying other bills (%)	61.50	4.90	[0.175]	61.59	6.68	[0.082]	65.31	-0.80	[0.836]	54.21	7.05	[0.080]
		(3.61)	{0.440}		(3.84)	{0.234}		(3.89)	{0.832}		(4.03)	{0.221}
Cut back spending on spending (Z-score index)	-0.06	-0.04	[0.571]	-0.03	0.06	[0.472]	0.11	-0.06	[0.457]	-0.03	-0.03	[0.665]
		(0.08)	{0.815}		(0.08)	{0.692}		(0.08)	{0.689}		(0.08)	{0.654}
Increased borrowing (Z-score index)	0.09	0.02	[0.752]	0.16	-0.01	[0.870]	-0.04	0.09	[0.262]	-0.22	0.10	[0.179]
		(0.08)	{0.815}		(0.08)	{0.865}		(0.08)	{0.563}		(0.08)	{0.321}

Notes: Table presents the heterogeneous effects of medical debt relief on survey outcomes by quartile of debtor age in the first wave an individual is observed. The sample size is 2,899 with one singleton observation dropped. The first column of each quartile reports the control mean for observations in that quartile. The second column reports the treatment effect of medical debt relief for that quartile with standard errors are reported in parenthesis. Lastly, the third column reports un-adjusted *p*-values and multiple-inference-adjusted *p*-values in square and curly brackets respectively. Multiple inference adjustment using the (Westfall and Young, 1993) method by domain.
Estimates are computed as outlined in Equation 1 with the independent variables interacted with a categorical quartile variable. Quartile cutoffs in years are as follows: Q1: [19, 30], Q2 [31, 40], Q3 [41, 52], Q4 [53, 90].

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E Appendix Figures

Figure A1. Sample Letter Sent to Treated Individual



RIP MEDICAL DEBT

THE CURE FOR MEDICAL DEBT

Name & address block

Date

Re: Balance Abolished
Old Balance: $xx.xx
Balance Now: $0
Gift ID: 13288269
Hospital:
Account Number:
Date of service:

Dear XYZ,

We are pleased to inform you that you no longer owe the balance on the debt referenced above to the above provider. RIP Medical Debt is a not-for-profit 501(c)(3) corporation that acquires and then cancels unpaid and unpayable medical debt.

Our forgiveness of the amount you owe is a no-strings-attached gift. You no longer have any obligation to pay this debt to anyone, at any future time. Because this debt has been cancelled as a gift by a 501(c)(3) charity, you do not owe any taxes on the "cancellation of debt" income.

This forgiveness is for this outstanding bill only. We have not forgiven any other medical debt you might owe.

Your privacy is protected. Medical records remain with the physician or hospital.

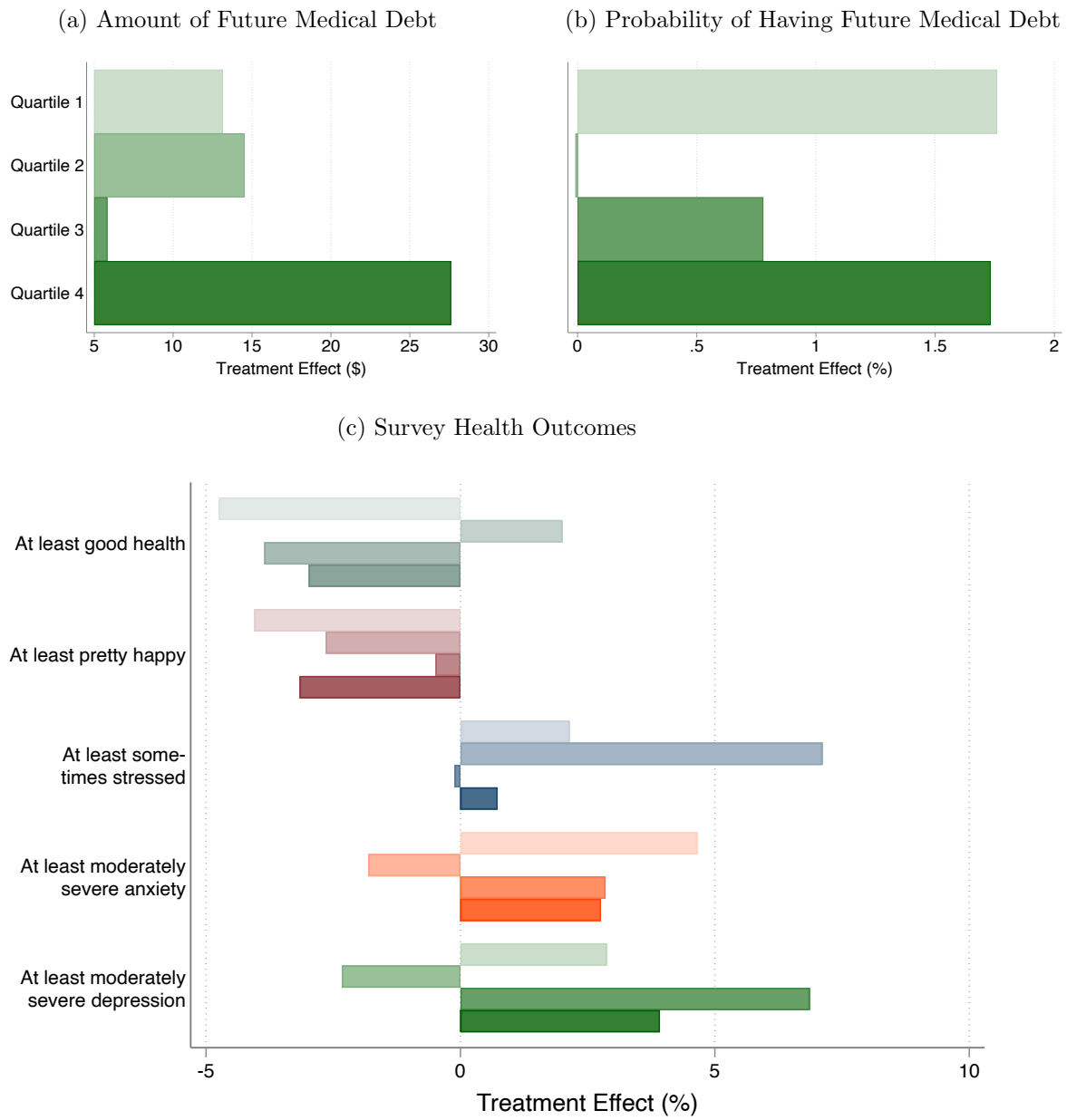
Regards,

The Staff of RIP Medical Debt

RIP Medical Debt | 80 Theodore Fremd Ave. | Rye NY | 10580-2981 | www.ripmedicaldebt.org

Notes: This figure presents an example of a letter sent to a recipient of debt relief. A Spanish translation was included on the reverse side.

Figure A2. Heterogeneous Effects of Debt Abolishment on Survey Health Outcomes, by Beneficiary Age



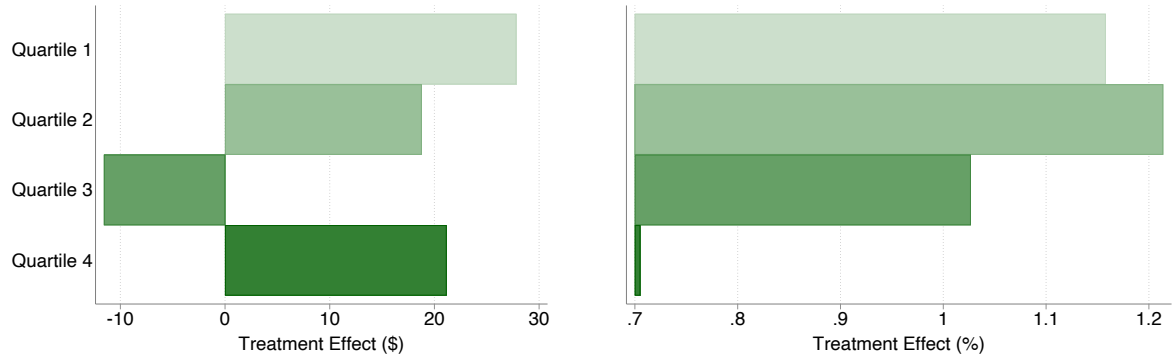
Notes: Figure presents the heterogeneous effects of medical debt relief on health survey outcomes and future medical debt in collections by age of debtor. Panel (A) presents effect of medical debt relief on the amount of medical debt observed in future waves. Panel (B) presents the effect of medical debt relief on the probability of having medical debt observed in future waves. Panel (C) shows the effect of medical debt relief on health outcomes as measured in the survey experiment.

Quartiles are created using the medical debt balance in the first wave individuals are observed in. Quartiles are computed separately within the survey experiment (Panel (C)) and the hospital debt experiment (Panel (A) and Panel (B)). In Panel (A) and Panel (B), the quartile cutoffs are as follows: Q1: [18, 30], Q2: [31, 41], Q3: [42, 54], Q4: [55, 89]. In Panel (C), the age quartiles are Q1: [19, 30], Q2 [31, 40], Q3 [41, 52], Q4 [53, 90]. For each outcome, the effects are pictured from lightest (Quartile 1) to darkest (Quartile 4) in descending order. See Appendix Table A10 and Appendix Table A7 for corresponding numerical values.

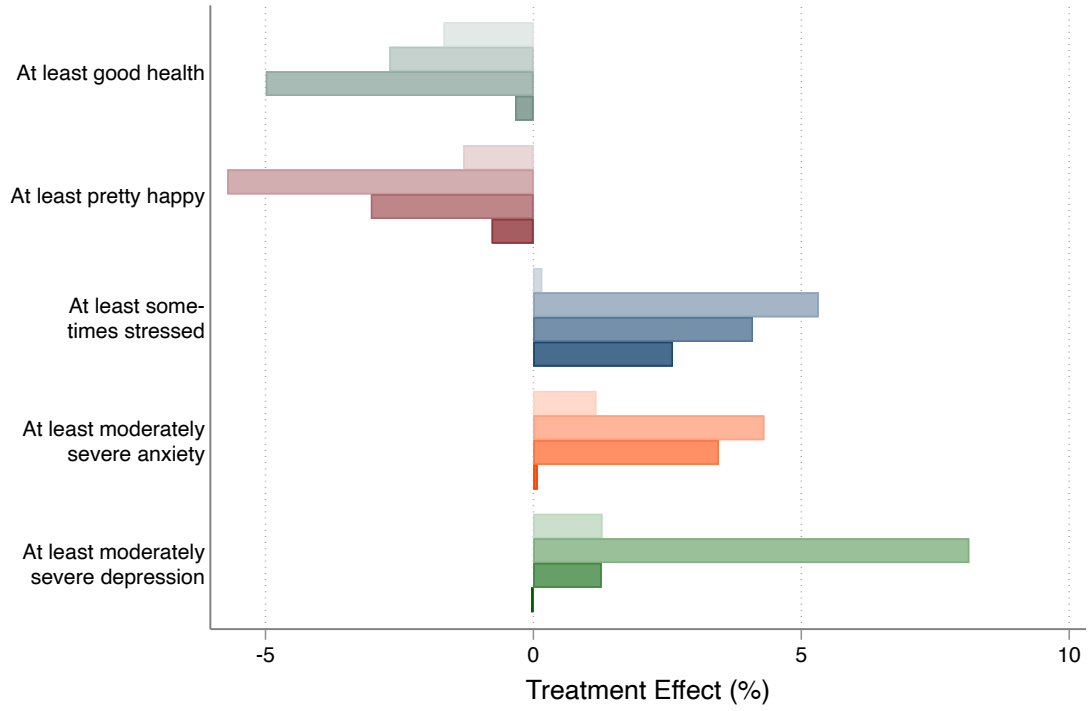
Figure A3. Heterogeneous Effects of Debt Abolishment on Survey Health Outcomes, by Debt Age

(a) Amount of Future Medical Debt

(b) Probability of Having Future Medical Debt

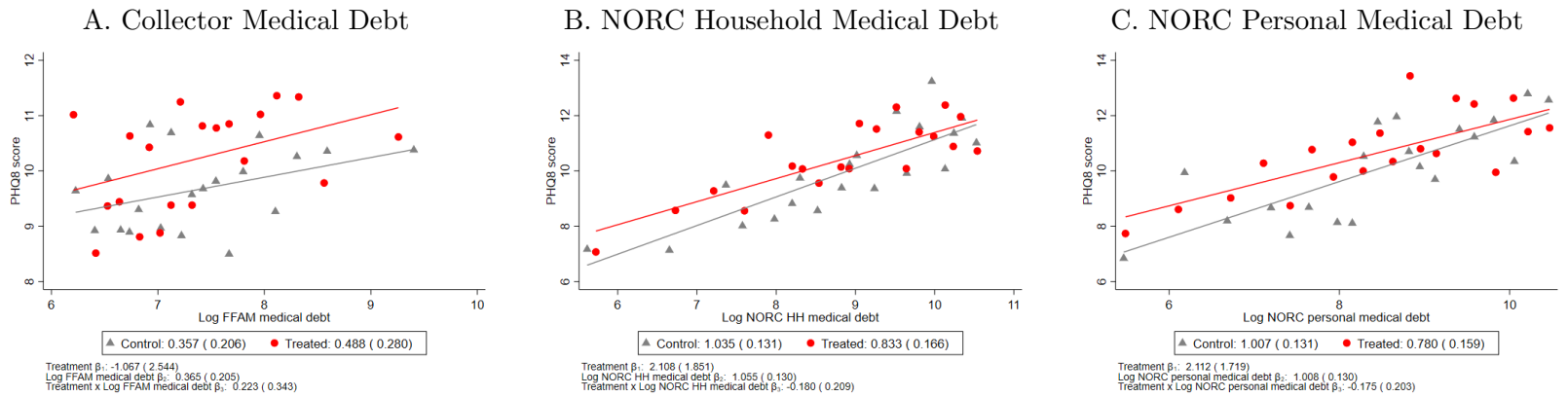


(c) Survey Health Outcomes



Notes: Figure presents the heterogeneous effects of medical debt relief on health survey outcomes and future medical debt in collections by age of first wave medical debt. Panel (A) presents effect of medical debt relief on the amount of medical debt observed in future waves. Panel (B) presents the effect of medical debt relief on the probability of having medical debt observed in future waves. Panel (C) shows the effect of medical debt relief on health outcomes as measured in the survey experiment.
Quartiles are created using the medical debt balance in the first wave individuals are observed in. Quartiles are computed separately within the survey experiment (Panel (C)) and the hospital debt experiment (Panel (A) and Panel (B)). In Panel (A) and Panel (B), the quartile cutoffs in days are as follows: Q1: [130, 426], Q2: (426, 457.19], Q3: (457.19, 495], Q4: [495, 2,177]. In Panel (C), the day quartiles are Q1: [198, 429], Q2: (429, 459], Q3: (459, 503], Q4: (503, 1,567]. For each outcome, the effects are pictured from lightest (Quartile 1) to darkest (Quartile 4) in descending order. See Appendix Table A9 and Appendix Table A6 for corresponding numerical values.

Figure A4. Effect of medical debt forgiveness on depression



Notes: The figure reflects the results from estimating following equation, where $PHQ8_i$ is the PHQ8 score imputed using random hotdecking; $Treated_i$ is an indicator for whether someone is in the treated group for debt forgiveness; $X_{w,v}$ includes self-reported annual income from NORC and a fully interacted treatment-wave-by-survey-wave FE, $DebtBal_i$ is log debt balance:

$$PHQ8_i = \beta_0 + \beta_1 Treated_i + \beta_2 DebtBal_i + \beta_3 Treated_i \times DebtBal_i + \theta X_i + \alpha_{w,v} + \epsilon_i$$

We binscatter the residuals of $PHQ8_i$ and $DebtBal_i$ from the above regression after adding back the variables' means to the residuals. This procedure is done separately for the treated (red circles) and the control (grey triangles). As a result, the slope of the grey control (red treated) line is equal to the estimate of β_2 ($\beta_2 + \beta_3$). The estimates of β_1, β_2, and β_3 are included in the footnotes of each panel.
Each panel uses a different metric for medical debt from the collector's debt in panel A, log personal and household medical debt from NORC in panels B and C, respectively.

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