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THE EFFECTS OF MEDICAL DEBT RELIEF: EVIDENCE FROM TWO RANDOMIZED EXPERIMENTS

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ABSTRACT

Two in five Americans have medical debt, nearly half of whom owe at least $2,500. Concerned by this burden, governments and private donors have undertaken large, high-profile efforts to relieve medical debt. We partnered with RIP Medical Debt to conduct two randomized experiments that relieved medical debt with a face value of $169 million for 83,401 people between 2018 and 2020. We track outcomes using credit reports, collections account data, and a multimodal survey. There are three sets of results. First, we find no impact of debt relief on credit access, utilization, and financial distress on average. Second, we estimate that debt relief causes a moderate but statistically significant reduction in payment of existing medical bills. Third, we find no effect of medical debt relief on mental health on average, with detrimental effects for some groups in pre-registered heterogeneity analysis.

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A randomized controlled trials registry entry is available at
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1 Introduction

Two in five Americans have medical debt, nearly half of whom owe at least $2,500. Medical debt is more prevalent among uninsured, low-income, Black, and Hispanic households. Due to increasing patient cost-sharing, medical debt is common even among households with health insurance. Among households with medical debt, 63% report reducing expenditures on food and clothing and 48% report using up all or most of their savings because of medical debt (Kaiser Family Foundation, 2022).[1]

Concerned by this burden, policymakers are increasingly turning to medical debt relief. As of March 8, 2024, 15 state or local governments have passed programs to fund roughly $8 billion in medical debt relief, and five other state or local governments were considering programs that would raise this total to nearly $13 billion, with nearly all of these governments working with our research partner RIP Medical Debt (see Appendix Table A1). Private donors are also generously supporting debt relief, with RIP Medical Debt raising private funding to buy and relieve more than $10 billion in medical debt to date. In announcing government purchases, politicians have lauded medical debt relief as "transformational for our most vulnerable residents" (City of New Orleans Office of the Mayor, 2023).

There are reasons to be optimistic about the benefits of medical debt relief. Debt relief in non-medical contexts – including student loans (Di Maggio et al., 2020), credit cards (Dobbie and Song, 2020), mortgages (Ganong and Noel, 2020), and bankruptcy (Dobbie and Song 2015; Dobbie et al. 2017) – has been shown to reduce financial distress, increase earnings, and improve mental health. In a survey of experts that we conducted, most respondents predicted that medical debt relief would significantly improve mental health, financial well-being, and healthcare access.

Yet there are also reasons for caution. Medical debt can be purchased for pennies on the dollar. Although proponents of medical debt relief tout the low cost as feature – the $13 billion of planned relief would cost taxpayers around $137 million – the price reflects low recovery rates, which suggests the financial impacts on households may be a small fraction of the face value of the debt relieved. This contrasts with student loans and other forms of consumer debt, where recovery

[1] All of the above statistics are taken from the same nationally representative 2,375-person Kaiser Family Foundation (2022) survey.

rates are higher. Medical debt has a strong association with negative health and financial outcomes (e.g., Banegas et al., 2019; Han et al., 2024; Himmelstein et al., 2022; Priscilla et al., 2020), but it is unclear whether this association implies a causal effect of debt relief, especially in the presence of confounding factors like persistent health conditions and reduced ability to work.

This paper studies the impact of medical debt relief on financial outcomes, health, and health-care utilization using two randomized experiments conducted in partnership with RIP Medical Debt (RIP), a non-profit organization that works with government and private donors to purchase and abolish medical debt, and has been involved in most high-profile medical debt relief to date. In total, these experiments provided relief of medical debt with a face value of $169 million to 83,401 patients.

The first *hospital debt* experiment targeted younger debt and was designed to test the effects of relieving debts before patients are exposed to third-party debt collection. We expected larger benefits from this experiment and focused on this sample for more intensive data collection. For this experiment, RIP purchased a random subset of debt at the juncture when the hospital would otherwise sell accounts to the debt collector (roughly one year after the date of the medical service) in 18 waves between August 2018 and October 2020 at a price of 5.5 cents per dollar of debt (more than 5 times RIP's typical purchase price).[2] The treatment group, which had their debt relieved, consisted of 14,377 people who received $19 million in face-value debt relief, for an average of $1,321 per person. The 61,496-person control group did not receive debt relief, and the debt collector pursued repayment following their normal protocol. Recipients of debt relief were sent two letters notifying them that their debt had been canceled.

The impact of debt relief can operate through reduced collections activity or knowledge of the charitable intervention. To test how awareness and salience of the intervention mediated the effects of debt relief, RIP conducted an additional sub-experiment in which a random subset of the treatment group was informed of debt relief via phone calls from trained RIP employees.

The second *collector debt* experiment targeted older debt, which reflects the majority of the debt relief provided by RIP Medical Debt to date and where it is possible to achieve large-scale

[2]Between 2018 and 2022, RIP Medical Debt provided $8.48 billion in medical debt relief at a price of $35.0 million or 0.42 cents per dollar of debt relief (IRS Form 990s). Since 2021, most of their purchases have been at a price of less than 1 cent per dollar relieved.

debt relief at a lower cost. For this experiment, RIP purchased a random selection of debt that had been under collection in the secondary market for several years in two waves (conducted in March and October 2018) at a price of less than 1 cent on the dollar. The treatment group consisted of 69,024 people and \$150 million in face-value debt relief, for an average of \$2,167 per person. The 68,014-person control group retained their debt and continued to be pursued for repayment by the debt collector. Together, the two experiments were designed to shine light on the cost-effectiveness of relief at different stages in the collection process.

We study the impact of debt relief using three data sources. First, we obtained collections account data from the debt collector for the entire study sample, which provides baseline information on the accounts in collections. For the hospital debt experiment, we track future accounts sent to collections, and thereby analyze impacts on the subsequent repayment of medical bills. Second, we linked our entire study sample with fully depersonalized quarterly credit-report data from TransUnion, which allows us to track financial distress, credit access, and credit utilization for at least one year before and after treatment assignment. Third, 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 consisted of five mailings, twice-weekly email invitations, paper survey instruments sent via certified mail, and telephone interviews conducted by a trained US call center, and resulted in a survey sample of 2,888 individuals.

We pre-specified our empirical specification, primary and secondary outcomes, and heterogeneity analyses (AEA RCT Registry #0003332, #0003664, and #0007426). We adjust our inference for multiple hypothesis testing as pre-specified.

We find no average effects of medical debt relief on the financial outcomes in credit bureau data in either of our experiments. We estimate a precise null effect on the number of accounts past due, our primary outcome for the credit report analysis. In the hospital debt experiment, a 95% confidence interval allows us to reject a decrease in accounts past due of more than 0.04 (relative to a control mean of 1.20 accounts). We similarly estimate economically small and statistically insignificant effects on other measures of financial distress, credit access, and credit utilization. These null effects are robust to alternative specifications, and our heterogeneity analysis does not reveal noteworthy effects for any pre-registered subgroup.

We find that debt relief causes a statistically significant and economically meaningful *reduction* in payment of existing medical bills. We define unpaid medical bills as those sent to collections after treatment in the collections account data. Using the hospital debt experiment, we find that debt relief increases unpaid bills sent to collections by \$14, or 7.2% of the control mean of \$199. This increase is almost entirely explained by lower repayment of bills for medical services received prior to treatment assignment rather than changes in healthcare utilization resulting from the treatment. Moreover, the estimated effect is larger (in levels and proportionally) for those with greater indebtedness at baseline. The repayment response is consistent with treated persons raising their expectations of future debt relief or targeting a certain level of indebtedness (as in Dobkin et al. (2018)). The result rejects the theory that debt relief could increase repayment via an income effect or by leaving more resources in a mental account to pay medical bills (as in Katz (2023)).

We estimate statistically insignificant average effects of medical debt relief on measures of mental and physical health, healthcare utilization, and financial wellness, with "opposite-signed" point estimates for the mental health outcomes relative to our prior. The outcomes were measured with our multimodal survey of the hospital debt experiment sample. Following the Oregon Health Insurance Experiment (Baicker et al., 2013), our primary survey outcome is at least moderate depression on the 8-question Patient Health Questionnaire (PHQ-8). The median expert survey respondent predicted a 7.0 percentage-point improvement in this measure. We estimate that debt relief causes a statistically insignificant average 3.2 percentage-point worsening of depression (p-value of 0.097) relative to a control mean of 45.0%. A 95% confidence interval rules out an improvement of more than 0.6 percentage points. We estimate similarly statistically insignificant, opposite-signed average effects on other measures of mental and physical well-being, including anxiety (as measured by the 7-question Generalized Anxiety Disorder (GAD-7) screen), stress, general health, and subjective well-being. We do not detect any meaningful effects on healthcare utilization or financial wellness.

The negative effects on mental health are concentrated among those with the greatest baseline medical debt (defined as medical debt eligible for randomized relief), which is a pre-specified dimension of heterogeneity analysis. Specifically, persons in the top quartile of indebtedness experience a 12.4 percentage-point increase in depression (p-value of 0.002), along with a worsening of anxiety, stress, general health, and subjective well-being. Since we did not experimentally manipulate the amount of debt relieved, these results should be interpreted as the causal effects of debt relief for groups with different baseline debt amounts, not the causal effect of different amounts of debt relief. We do not recover noticeable effects on other outcomes beyond mental health in this heterogeneity analysis or an analysis exploring other pre-specified dimensions of heterogeneity.

In addition to being concentrated among those with the highest baseline medical debt, we also estimate detrimental effects for those who were randomly assigned to receive phone calls to increase their awareness of the treatment. Specifically, we find statistically significant increases in depression for those assigned to the sub-treatment (p-value of 0.029), but not for those who were not assigned to calls (although the p-value for the difference in effects between these treatment arms is only 0.093).

We did not expect the negative effect on mental health for those with the greatest baseline debt and did not design the experiment or survey to investigate its cause. However, the result echoes Jaroszewicz et al. (2023), who find that randomized one-time unconditional cash transfers caused significant reductions in psychological well-being among low-income US households. To explain this result, they suggest a mechanism in which receiving transfers that are insufficient to address financial needs raise the salience of financial deprivation and feelings of distress. A similar mechanism could explain the negative mental health effects among those who were assigned to phone calls (which increased awareness of the treatment) and those with the largest amount of debt relief (who had higher baseline financial distress and thus may have experienced the medical debt relief as particularly insufficient). The negative mental health effects could have also worsened mental health through the stigma of receiving charity, which may be relevant given recipients did not request relief.

The removal of medical debt from credit reports has historically been cited as a primary benefit of debt relief, given the visibility of medical debt to lenders, landlords, and employers. However, like much of the industry, the debt collector we worked with shifted away from reporting due to liability risk around the time of our experiment (CFPB, 2023d). We are able to estimate the effects of the removal of medical debt from credit reports by studying a subsample of 2,768 persons where the intervention occurred before the debt collector stopped reporting. We find that debt relief immediately raises credit scores by an economically small 3.6 points on average (p-value of 0.016), with a 13.4-point increase (p-value of 0.010) for persons with no other debts in collections. This immediate increase is accompanied by a gradual increase in credit limits of \$342 on average (p-value of 0.010; 15.4% of the control mean of \$2,227), with larger effects for persons with no other debts in collections. We detect no effects on measures of borrowing or financial distress.

Medical debt on credit reports has further declined in recent years with an agreement by the three major credit bureaus to stop displaying many types of medical debt (CFPB, 2023c). As of August 2023, only 5% of credit reports had medical debt, down from 16% in 2018 (Blavin et al., 2023). The credit access effects of future medical debt relief will be limited to persons with continued credit reporting. However, these findings are broadly relevant for assessing the effect of prior medical debt relief and the impact of the CFPB's current agreement and potential rule to limit the reporting of medical debt (CFPB, 2023a).

Our paper contributes to the literature on the financial burden of the US healthcare system. Correlational studies find negative relationships between medical debt and mental health (Han et al., 2024; Himmelstein et al., 2022; Jenkins et al., 2008; Meltzer et al., 2013), healthcare utilization (O'Toole et al., 2004), and financial distress (Himmelstein et al., 2019). However, the presence of health or financial shocks that occur at the same time as medical debt accrual warrants caution against a causal interpretation of these associations. Experimental and quasi-experimental research has shown beneficial effects of *upstream* policies that address healthcare costs before bills are sent for collection (Gross and Notowidigdo, 2011; Baicker et al., 2013; Adams et al., 2022; Goldsmith-Pinkham et al., 2023). The hospital financial assistance program studied by Adams et al. (2022), which bundled medical debt relief with temporary reductions in cost-sharing, substantially increased high-value healthcare utilization. The Oregon Health Insurance Experiment (Baicker et al., 2013) found that Medicaid also reduced depression by 9 percentage points among a population of low-income uninsured adults.

More broadly, our paper contributes to research on the impact of debt relief and cash transfer programs on financially vulnerable Americans. Debt relief through bankruptcy (Dobbie and Song, 2015; Dobbie et al., 2017) and student loan forgiveness (Di Maggio et al., 2020) has been shown to cause substantial improvements in financial well-being and earnings.[3,4] The contrasting null effects we estimate may stem from the smaller balance sheet effects of medical debt relief. Our study population also has much higher rates of financial distress than student loan borrowers and may experience smaller benefits from medical debt relief on the margin. Further afield, experimental studies on unconditional cash transfers – mostly based in developing countries – generally demonstrate significant positive effects on financial and mental health (Bastagli et al., 2016; Banerjee et al., 2019; Dwyer et al., 2022). The negative mental health effects documented in Jaroszewicz et al. (2023) are a notable exception, potentially driven by the smaller size of the transfer as a share of income in the US context.

The findings from this literature underpinned optimism about the potential impacts of medical debt relief (as evidenced by our expert survey), financial outlays by private donors and local governments, and broader policy proposals (e.g., Sanders, 2022). Indeed, even if the benefits of medical debt relief were smaller than those in other contexts, the low cost of medical debt raised the prospect of a cost-effective intervention. Nonetheless, our results are sobering; they demonstrate no improvements in financial well-being or mental health from medical debt relief, reduced repayment of medical bills, and, if anything, a perverse worsening of mental health. Moreover, other than modest impacts on credit access for those whose medical debt is reported, we are unable to identify ways to target relief to subpopulations who stand to experience meaningful benefits. There is still potential that medical debt relief could have beneficial impacts on outcomes we did not measure or for populations we did not analyze. The literature suggests that upstream debt relief before patients have gone through the hospital collections process could be more effective; further research will be needed to explore such potential benefits.

The rest of the paper is structured as follows. Section 2 provides background on our setting and

[3]Relative to research on debt relief, research on debt modifications has shown more mixed, context-dependent effects. Ganong and Noel (2020) find that reducing liquidity requirements is more important than principal reductions in the context of mortgage modifications during the Great Recession. Dobbie and Song (2020) find no effect of credit card debt payment reductions on financial and labor market outcomes, but find that interest write-downs significantly improve these outcomes despite not affecting payments for several years. Dinerstein et al. (2023) show that the 2020 student debt moratorium reduced delinquencies and increased credit scores, with the additional liquidity increasing demand for credit cards and auto loans.

[4]Effects of debt relief in the development economics literature have been less encouraging. Kanz (2016) finds debt relief has no effect on consumption, savings, or investment but does reduce concern over future default, while Karlan et al. (2019) find that most recipients of debt relief return to indebtedness within six weeks. These findings are consistent with the increased debt accumulation through reduced debt repayment we document.

describes the experiment. We describe our data sources in Section 3 and our empirical framework in Section 4. Results are presented in Section 5 and discussed in Section 6. Section 7 concludes.

2 Setting and Experiment

2.1 Setting

Patients incur medical debt if they fail to pay the out-of-pocket or self-pay component of their medical bills. When a bill is overdue, medical providers typically engage in direct outreach to the patient. After a period of non-payment, medical providers may place the unpaid bill with a debt collector. Debt collectors attempt to recover payment by various means, including contacting patients at their home or place of employment; reporting medical debt to credit bureaus where it is visible to potential lenders, employers, and landlords; and suing patients, which can result in judgments that allow for wage garnishment and liens on patients' homes (see, e.g., Cooper et al., 2021; Presser, 2019). Debt collectors can sell medical debt on the secondary market to other debt collectors, who can continue collection attempts.

Federal laws aim to protect some patients from incurring medical debt and impose limitations on debt collection practices. Internal Revenue Service (IRS) regulations codified in Section 501(r) require non-profit hospitals to establish financial assistance policies and make "reasonable efforts" to assess eligibility before taking extraordinary collection actions, such as selling medical debt into collections, denying care, or suing patients (IRS, 2023). These requirements do not apply to for-profit hospitals, and, in practice, the IRS has rarely revoked a hospital's non-profit status (GAO, 2023). Once medical debt is in collections, the Fair Debt Collection Practices Act (FDCPA) prohibits debt collectors from using deceptive or abusive practices to induce payment, such as threatening arrest or calling more than seven times per week. State statute-of-limitation laws restrict the time horizon for debt collectors to bring lawsuits to about six years on average, although there is substantial variation across states (Locklear, 2023). Roughly half of states also have laws that reinforce and extend federal requirements on hospital financial assistance and debt collection practices.[5,6]

Debt collectors have historically voluntarily reported medical debt to the credit bureaus to increase the salience of the debt and so that they could offer to cease reporting in exchange for repayment. The Fair Credit Reporting Act (FCRA) requires that any medical debt information reported to the credit bureaus be complete and accurate. In recent years, concerns about data integrity and the associated legal risks from inaccurate reporting have contributed to a substantial drop in the reporting of medical debt information by debt collectors (CFPB, 2023d).[7] The credit bureaus also voluntarily agreed to stop displaying medical debt if the amount is less than $500, less than one year old, or has already been paid (CFPB, 2023c), phasing in these changes between July 2022 to April 2023. Urban Institute analysis of credit bureau data shows that the share of adults with medical debt in collections declined from 16% in 2018 to 13% in February 2023 and then fell to 5% in August 2023 as credit bureaus halted reporting small dollar, young, and already paid debt (Blavin et al., 2023). Even before these changes, there were significant amounts of unreported medical debt (Argyle et al., 2021).[8]

The debt collector we worked with had reporting protocols that were reflective of trends in the industry. They historically reported medical debt information to the credit bureaus and intended to report for our experimental study. However, like many others in the industry, they became concerned about liability risk and largely ceased reporting before we implemented our experiment. The exception was a small number of accounts in the collector debt experiment (described below), where the intervention occurred before the debt collector stopped reporting and where reporting continued for control group accounts for three additional quarters.

[5]Nineteen states impose more generous requirements for hospital financial assistance. A few states either prohibit hospitals from selling debt to collection agencies or require hospitals to oversee debt collectors. A small number of states prohibit wage garnishment or home liens, and a broader set of states prohibit wage garnishment for certain populations or when there is demonstrated financial need. See Kona and Raimugia (2023) for a comprehensive list of policies by state.

[6]There is a small literature on debt collection that relates to this overview. Cheng et al. (2021) analyze consumers facing civil collection lawsuits and find consumers overestimate how much they would pay through the court system and are motivated to settle by non-pecuniary considerations, such as avoiding the stigma of wage garnishment. Fedaseyeu (2020) and Fonseca (2023) find that stricter state debt collection regulations reduce both third-party debt collection activity and the supply of traditional credit.

[7]The concerns about data integrity are substantiated by consumer complaints data and associated CFPB reports. The CFPB received over 75,000 consumer complaints relating to medical debt collections between 2014 and 2023, three-quarters of which cited collections of debts not owed or insufficient information to verify the debts as the reason for the complaint (CFPB, 2023b, 2024).

[8]Argyle et al. (2021) measure "shadow debt" – debt not reported to the credit bureaus – using liability-level data from bankruptcy filings and find that medical debt and "unknown" debts are the largest categories of shadow debt.

Despite the reduced credit bureau reporting, survey evidence indicates that medical debt remains highly prevalent. A recent Kaiser Family Foundation survey (Kaiser Family Foundation, 2022) found that 41% of adults have medical or dental debt, with 24% having bills that are past due or they are unable to pay, and 21% having bills they are paying off directly to a provider. This survey found that medical debt is more common among uninsured persons (62% of currently uninsured adults under 65 have medical debt) but still notably high for those with insurance due to high deductibles and other cost-sharing for many health plans (44% of insured adults under 65 have medical debt). Medical debt is more common among low-income households (57% of households earning less than $40,000 per year have medical debt) and among Black (56%) or Hispanic (50%) households than other groups; about half of persons with medical or dental debt owe more than $2,500 (Kaiser Family Foundation, 2022).

2.2 Experiment

We study medical debt relief provided by RIP Medical Debt, a 501(c)(3) non-profit organization that raises funding from governments and private donors to purchase and abolish medical debt. We separately examine instances in which RIP used private funds to randomize the abolishment of (i) *hospital debt* acquired at the point when hospitals would normally sell the debt to a collection agency and (ii) *collector debt* acquired from a collection agency on the secondary market after collection attempts were made. These experiments were conducted between March 2018 and October 2020. See Figure 1 for a flowchart summarizing these experiments.

Hospital Debt Experiment The hospital debt stems from medical care provided by a large for-profit hospital system, with 18 facilities spread over 8 states in the South and Mountain West.[9] After a patient receives care, this hospital system attempts to recover payment from the patient's health insurance, other payors, and the patient. After about a year, the hospital system assembles a portfolio of debt, which they would normally sell to a debt collector.

RIP coordinated with a debt collector to purchase and relieve a random subset of the medical debt accounts at the juncture when they would typically be sent to collections by the hospital

[9] The vast majority of the sample had an address in Arizona (13%), Arkansas (5%), Louisiana (6%), Texas (50%), and Utah (24%).

system. These purchases occurred in 18 waves between August 2018 and October 2020. For each wave, RIP received a data file of unpaid bills listing the amount owed and information on the debtor. Within each wave, RIP grouped unpaid bills at the person level and stratified persons by the amount of debt, state of residence, insurance status, and a collections score. Within each of these strata, persons were randomly assigned to treatment or control. The process by which portfolios were made available for randomization did not permit carrying forward treatment assignment across waves. In a typical wave, 20% of persons were assigned to treatment, although the exact treated percentage varied depending on the size of the wave and the amount of donor funding available. See Appendix Section A.1 for more detail about the stratified randomization and Appendix Table A2 for wave-by-wave statistics.

For treated individuals, RIP purchased the debt 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 letter informing them of debt abolishment (see Appendix Figure A1 for an example). A second letter containing the same information was sent out three weeks after the first.

For control individuals, the debt collector purchased all debts and collected on them following their standard protocol. The collector's stated protocol is as follows. For the first 24 months, each account goes through a series of six collection stages, with each stage lasting approximately four months. At the beginning of each stage, the account is placed with a third party that takes responsibility for outreach to the debtor. The primary methods of outreach are mail and telephone communication but can include text messaging in some states and email communication with debtors who reach out using that channel. At the end of each stage, the account is recalled from the third party, and the cycle begins with a new party responsible for outreach in the next stage. If an account has remained unpaid after the first three stages (i.e., after about one year of collections efforts), it may be evaluated for litigation. Debtors with sufficiently high-value accounts and resources (e.g., homeowners, borrowers with recent auto loan originations) are subject to litigation, although, in practice, this comprises only a small minority of accounts. Accounts not selected for litigation continue to the fourth stage. 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 stages are typically offered more generous settlements because these debts are less likely to be paid.[10] Our conversations with executives at other debt collectors suggest this protocol is standard in the industry.

We define a person's treatment status by their treatment assignment in the first wave in which they appear. We focus on the initial wave for analytical convenience and because incorporating information from subsequent waves has negligible quantitative impacts. The average person in the hospital debt experiment appears in 0.23 subsequent waves and 16% appear in at least one additional wave. However, because roughly 20% of persons are assigned to treatment in each wave, persons who are treated in the initial wave are on average treated 1.05 times overall, and persons who are initially assigned to control are treated 0.04 times overall. Thus, there is little quantitative difference between focusing on initial assignment and using initial assignment as an instrument for cumulative assignment in a two-stage least-squares design.

Column 1 of Table 1 provides summary statistics on the hospital debt sample in the initial wave in which persons appear (data is described in more detail below). 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 and $1.0 million in purchase costs. On average, persons in this sample owed $1,352 of medical debt at face value (interquartile range of $235 to $1,475) and were exposed to the debt relief intervention at 5.1 quarters after the date of the medical service (interquartile range of 4.7 to 5.4 quarters).

Awareness Subexperiment The impact of debt relief can operate through reduced collections activity and knowledge of the charitable intervention. To increase awareness and salience of the debt relief intervention, RIP conducted additional phone outreach to a randomly selected subset of the treatment group. Specifically, they focused phone outreach on persons in waves 6 to 14 of the hospital debt experiment to overlap with our survey sample (described below). Of the 8,160 treated individuals in these waves, they randomly selected 4,232 (or 52%) to receive phone outreach. The

[10] During the COVID-19 pandemic, collections rates increased, consistent with overall declines in regular spending (Chetty et al., 2023) and medical indebtedness (Guttman-Kenney et al., 2022). According to the collector, more aggressive settlements were offered during this time period in an attempt to capture some of the increase in household liquidity.

outreach protocol consisted of a scripted message acquainting subjects with RIP and informing them of their debt relief. The script is provided in Appendix Section A.2.

The callers made three attempts to reach the subjects. If the callers reached voicemail, they left an abbreviated scripted message about RIP and their debt relief. Callers recorded the outcome of the call attempt. If they made contact, they noted the respondent's reaction to the news of the debt relief and whether they reported receiving the initial RIP letter. Of the 4,232 persons randomly selected for this intervention, callers spoke to 739 (17%) persons and left voicemails for an additional 1,717 (41%) persons. 95% reacted in a positive or neutral manner and 5% expressed disbelief. Only 19% reported having received the initial RIP letter.

Collector Debt Experiment The collector debt was purchased from the collections agency and consisted of debt that had been subject to collections efforts by the debt collector for a number of years. These prior collections efforts followed industry standards and largely consisted of mail and telephone outreach. The sample was geographically diverse, covering 45 states spread across the South (52%), West (21%), Northeast (18%), and Midwest (9%) regions of the country.

RIP coordinated with the debt collector to purchase a random subset of debt in two waves: one in March 2018 and one in October 2018. For each purchase, RIP was provided with a portfolio of unpaid accounts listing the amount owed and information on the debtor. Accounts were grouped by person and stratified by location, debt age, individual age, and debt amount. Within each stratum, persons were randomly assigned to treatment or control. The share of treated individuals depended on donor funds available for purchase. Because donors typically prioritized debt relief in particular locations, the treated share varied by stratum. See Appendix Section A.1 for more information and Appendix Table A2 for statistics.

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 one cent per dollar, or roughly one-sixth the price of the hospital debt. Treated persons had their debt abolished and were notified by letter (Appendix Figure A1). Control persons continued to be subject to normal collection efforts.[11]

[11] As before, we define a person's treatment status by their treatment assignment in the first wave in which they appear. Only 0.14% of persons appear in both waves.

Column 4 of Table 1 provides summary statistics on the collector debt sample in the initial wave in which persons appear, and Appendix Table A2 provides wave-by-wave detail. Debt relief was provided to 69,024 treated persons, amounting to 50.4% of 137,038 persons in the collector debt sample. The total face value of debt relief was $150 million, an average of $2,167 per person. Persons in this sample were exposed to the debt relief intervention on average 28.2 quarters after the provision of medical service (interquartile range of 22.7 to 28.6 quarters).

2.3 Expert Survey

We conducted an expert survey to assess prevailing beliefs on the impact of our hospital debt experiment. Survey respondents were first provided a description of the intervention, including the face value of debt relief, the purchase price of the debt, and the letter from RIP. We then asked respondents to predict the impact of debt relief on several outcomes and to provide their general view on the value of medical debt relief as a use of charity resources. Respondents were paid $25 for completed surveys and were told that the five respondents with the most accurate predictions would receive an additional $75 gift card. The full survey protocol is shown in Appendix Section E.

We sent our survey to academics who studied medical debt and related topics in consumer finance and healthcare, staff at non-profits that focused on medical debt, persons with private-sector experience in hospital revenue cycle management and debt collections, and staffers who worked for Congresspeople with relevant committee assignments and had relevant fields of expertise listed in their profiles. We received 45 responses, with 16 from academics, 23 from non-profit staff, and 6 from the private sector or government.

Figure 2 shows box plots of expert predictions for the impact on our primary outcome of depression, defined as the share of persons with at least moderate depression on the PHQ-8 (described in more detail below). We provided respondents with the control group mean and, as a benchmark, the 9.2 percentage-point reduction in depression from Medicaid coverage estimated in the Oregon Health Insurance Experiment (Baicker et al., 2013). The median expert predicted a 7.0 percentage-point reduction in depression (8.0 percentage points if we weigh by confidence in their answers). There is heterogeneity across respondents, with the median academic predicting a more modest 3.5

percentage-point reduction and the median non-profit staff predicting a larger 8.1 percentage-point reduction.

Appendix Figure A2 shows that expert survey respondents similarly predict increased healthcare access, reduced borrowing, and less cutting back on spending. Taken together, 75.6% of respondents report that medical debt is at least a moderately valuable use of charity resources (68.8% of academics and 78.3% of non-profit staff) and 51.1% think it is very valuable or extremely valuable (31.2% of academics and 69.6% of non-profit staff) as shown in Appendix Figure A3.

3 Data

3.1 Collections Account Data

The debt collector provided us with a dataset that included the amount owed, information on the debtor (name, date of birth, Social Security number, address, and phone number), and limited information on the underlying medical service (date and name of medical facility) for each person in each wave of the hospital and collector debt experiments. For persons in the hospital debt sample, we also received information on health insurance status.

To measure the effect on future medical debt, we constructed an outcome variable defined as the sum of medical debt appearing after the initial wave. We also constructed separate future debt measures by whether the associated medical service occurred before or after initial treatment assignment. This enabled us to distinguish effects on future debt accrual that are driven by changes in healthcare utilization versus changes in debt repayment.

3.2 Credit Bureau Data

We linked persons in the collections account data to credit bureau records from TransUnion, one of the three nationwide credit reporting agencies. The linking was conducted by TransUnion and returned as fully depersonalized with no means to link back to the original sample. We purchased quarterly credit records for our study sample for the period spanning September 2017 to December 2021. This time period allows us to measure outcomes from at least four quarters before to four quarters after treatment assignment. We also purchased a nationally representative random sample of 58,669 credit reports, which we use to contextualize our study sample.

TransUnion linked persons to their credit reports using names, addresses, dates of birth, phone numbers, and Social Security numbers. We were unable to consistently match 6.2% of persons in the study sample, and we excluded these persons from the analysis of credit bureau data.[12]

TransUnion collects information from lenders, debt collectors, and public records on consumer debts. We analyze credit report outcomes across six pre-registered domains, which include measures of financial distress, debt in collections, bankruptcy, access to credit, and unsecured and secured borrowing.

3.3 Survey Data

We contracted NORC at the University of Chicago (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 Section A.3. The full survey protocol is shown in Appendix Section F.

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 expected that reducing the lag between debt relief and the survey and prioritizing those with larger debt amounts would increase the likelihood of detecting effects. Of this sample, we randomly selected 14,922 individuals to receive the survey protocol. This sample size was chosen because it exhausted our budget. 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 develop our survey protocol, we started with the intensive 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

[12] Of the unmatched 13,189 people in the combined study sample, 7,222 are in the hospital debt sample (9.5% of the study sample) and 5,967 are in the collector debt sample (4.4% of the study sample). The unmatched rates are virtually identical in the treatment and control groups within the hospital debt sample (9.6% vs. 9.5%) and collector debt sample (4.3% vs. 4.4%).

outreach to 1,000 and 3,000 subjects), where we tested survey modalities and experimentally varied the amount of upfront and completion payments. NORC ran all addresses on file through the USPS address validator tool and TransUnion's TLOxp service to verify and update addresses, as well as obtain phone numbers and up to five email addresses per respondent. Contact information was updated using these tools once before commencing the survey protocol and again before sending the paper version of the survey. In all communications, persons were told they would receive a $50 incentive for completing the survey.

The final survey protocol spanned 13 weeks. Survey subjects were first contacted via postal mail and email, both of which included a personalized web link to the survey and simple instructions for accessing the survey via any device. The mailed invitation (see Appendix Figures A4 and A5) 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 email 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, 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 US-based call center workers contacted individuals by telephone and gave individuals the opportunity to complete the survey verbally. If subjects were not interested in completing the survey over the phone, they were invited to provide their email address, asked for consent to receive survey invitations via text message, and offered a new paper copy of the survey to be sent via mail. Subjects received a final "last-chance" mailer via mail in the eleventh week before the survey closed.

The survey instrument was titled "Health and Financial Wellness Study" and made no reference to RIP Medical Debt to avoid priming subjects about medical debt. It included questions that allow us to measure the respondent's financial situation (including medical bills and any medical debt relief), healthcare utilization, mental and physical health, and demographics. We measure depression and anxiety using the clinically validated PHQ-8 and GAD-7 screens, and the PHQ-8 was our primary pre-registered outcome.

On average, respondents completed the survey 13 months after treatment assignment (and the commencement of control group debt collection activities) and 29 months after receiving the care that incurred the debt. The survey received a 19.4% response rate among the 14,922 individuals selected to be contacted. Of these, 68% responded via web survey, 10% responded via telephone interview, and 23% responded via mail survey.

Our response rate is similar to the 18% response rate in Deshpande and Dizon-Ross (2023), which used a protocol with several mailings and a follow-up phone call to survey households with children receiving Social Security Income in 2022, but lower than the 50% effective response rate in Finkelstein et al. (2012), which used a protocol similar to ours to survey potential Medicaid recipients in 2009. The lower response rates in our study and Deshpande and Dizon-Ross (2023) likely reflect a broader trend of declining survey response rates over time.[13] It likely also reflects differences in study populations (e.g., individuals with unpaid medical bills may be less likely to respond to surveys). In Section 5, we conduct several checks of external validity and find no evidence of differential effects for persons less likely to respond to the survey.

3.4 Summary Statistics

Panel B of Table 1 presents summary statistics for our study samples (columns 1-4) and a nationally representative sample, unconditionally and conditional on having medical debt in collections (columns 5-6). The average person in our study samples is in their early forties and more likely to be female than male. Among survey respondents, 43.7% are non-Hispanic white, 30.9% are Hispanic (any race), and 18.8% are Black. Appendix Table A3 compares demographics of our survey respondents to the national population. Our respondents are more likely to be female, non-white, and low-income than the national population. They are also less likely to be elderly, consistent with financial protection from Medicare (Goldsmith-Pinkham et al., 2023).

Credit scores for our study samples are low, a natural result of selection on medical indebtedness. For instance, the average credit score of 575 for the hospital debt sample (column 1) falls at the 20$^{\text{th}}$ percentile of the national distribution. Approximately 62.9% of our study sample has medical debt reported to the credit bureaus compared to 17.6% of the nationally representative sample. The

[13] Gallup and Pew have seen telephone survey response rates decline from roughly 30% in the late 1990s to less than 10% more recently (Marken, 2018; Kennedy and Hartig, 2019). Williams and Brick (2017) documented fairly large declines in response rates in face-to-face surveys, despite offsetting increases in survey effort. Mathematica has documented declines in the response rates of 7 surveys sponsored by the Department of Health and Human Services (Czajka and Beyler, 2016).

study samples also have roughly an order of magnitude more medical debt in collection and total debt in collection than the nationally representative sample. Our study samples have less total debt (including mortgage, credit card, and auto-loan balances, as well as other tradelines) than the nationally representative sample, primarily because they are less likely to have a mortgage.

Survey outreach was restricted to persons in the hospital debt sample with greater than $500 in collector debt (and who were first observed in waves 6 to 18). As we would expect given this restriction, the survey outreach sample (column 2) has worse credit bureau outcomes relative to the full hospital debt sample (column 1), although the differences are small relative to the differences between the study sample and the nationally representative sample. Relative to the survey outreach sample, survey respondents (column 3) have slightly better credit bureau outcomes, although these differences are similarly small in magnitude. Still, the differences between the survey outreach and respondent samples motivate sensitivity analysis to probe the external validity of our findings.

The collector debt sample (column 4) has moderately worse credit bureau outcomes than the hospital debt sample (column 1), likely because persons with older medical debt in collection are more negatively selected than those with younger medical debt.

4 Empirical Framework

4.1 Regression Specification

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

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

where i indexes persons and T_i is an indicator for whether the person was randomly assigned to the debt-relief treatment. Since the probability of treatment assignment is not uniform across wave and strata, we control for fixed effects at the level of randomization α_r to isolate the experimental variation.[14] The coefficient of interest, β, captures the average effect of debt relief on the outcome.

[14]For the hospital debt experiment analysis of collection account and credit bureau data, we control for fixed effects for the 18 experimental waves. For the hospital debt experiment analysis of survey data, the probability of surveying also varies across survey waves, so we control for the full interaction of experiment wave and survey wave

While controlling for person-level characteristics is not necessary for causal interpretation of β, we conduct sensitivity analysis with person-level controls to increase the precision of the estimates and to probe robustness to any incidental differences in person-level characteristics between treatment and control groups. For analysis of the credit bureau data, where we have panel data, we estimate specifications that control for person fixed effects to isolate the within-person variation in outcomes over time. Across all of our datasets, we estimate alternative specifications where we control for demographic and baseline financial characteristics from the collections account and credit bureau data. These specifications are outlined in Appendix Section B.1.

For our analysis of secondary outcomes, we adjust our standard errors to account for multiple testing within each pre-specified domain of outcome variables. Specifically, we report p-values that adjust for multiple testing 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 four pre-registered baseline characteristics: the amount of medical debt eligible for relief, the age of the person, the time span between medical service and the intervention, and the amount of other debt in collections on their baseline credit report. To do so, we assign persons to quartiles of each characteristic and fully interact indicators for those quartiles with the treatment indicator, T_i, and randomization group fixed effects, α_r.[15] This analysis is discussed in detail in Appendix Section B.2.

We also analyze a sub-experiment within the hospital debt sample, in which a subset of treated persons were randomly assigned to receive phone calls informing them of their debt relief. To conduct this analysis, we replace the single treatment indicator in Equation 1 with separate indicators for treated persons who were assigned to be called and those who were not.

fixed effects. For the collector debt experiment, the probability of treatment varies across waves and strata, so we control for the full interaction of experiment wave and stratum.

[15] The treatment effects from the fully interacted specification are identical to the treatment effects from estimating the main specification separately for each quartile. We estimate the effects jointly so we can test for differences across quartiles. For heterogeneity by other debt in collections, we split the sample into those with no other debt in collections and terciles conditional on positive other debt in collections.

4.2 Balance

Table 2 examines the balance of baseline characteristics for each of our study samples. For each outcome, we report the control group mean and the difference between the control and treatment group means, recovered by estimating Equation 1. We analyze balance on demographics and outcomes taken from the collector data in the first wave we observe the person.

The results confirm random assignment within the hospital debt, survey outreach, and collector debt samples (columns 1-2, 3-4, 7-8); all p-values are greater than 0.05, and the F-tests fail to reject the null that the differences are jointly zero.

The survey response sample (columns 5-6) reflects balance in survey outreach and balance in response rates. There is no evidence of differential selection into response based on observable characteristics, with none of the p-values below 5% and an insignificant F-test. We observe a 1.3 percentage-point higher response rate for the treatment group than the control group (second to last row). While this difference is not statistically significant at conventional levels (p-value of 0.056) and survey outreach was completely independent of the treatment communications, it motivates sensitivity analysis of whether differential selection into survey response might affect our results. We discuss this analysis after presenting our main results.

5 Results

5.1 Credit Bureau Outcomes

Table 3 reports the average effects of debt relief on credit bureau outcomes. For brevity, we exclude several pre-specified outcomes from the table; these are shown in Appendix Table A4. The results are based on our baseline specification that compares outcomes for treated and control individuals four quarters after treatment assignment.

Columns 1 through 3 report treatment effects for the hospital debt sample. The first row of Panel A reports the effect on the number of accounts past due ($\geq$30 days past due), our pre-specified primary outcome for the credit bureau data. Debt relief has a -0.01 average effect on the number of accounts past due (relative to a control mean of 1.20 accounts), and we can reject effects outside of a -0.04 to 0.03 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 fairly 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).

The remaining panels report the effects of debt relief on credit access and utilization. Panel D shows no effect on credit access, measured by whether the person has a credit score, their credit score conditional on having one, and their combined credit card limit. Panel E shows no effect on credit card and auto loan borrowing. The estimates are statistically insignificant and economically small. For example, a 95% confidence interval rejects an effect on credit card borrowing outside of -\$46 to \$99 (relative to a mean of \$1,481) and rejects an effect on auto loan balances outside of -\$320 to \$226 (relative to a mean of \$8,020).

Columns 4 through 6 report treatment effects for the collector debt sample. Treated individuals in the collector debt sample received relief for medical debts that were typically 7.0 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 null effects for this sample.

We estimate null effects in alternative specifications that exploit the panel structure of the credit report data and control for person fixed effects. Indeed, the person-level controls increase the precision of the null effects. We examine potential heterogeneity by quartiles of medical debt eligible for relief, age of debt, age of the person, and debt in collections on their credit report, and find no meaningful effects for the subgroups defined by these variables. Appendix Sections B.1 and B.2 provide a comprehensive discussion of the sensitivity and heterogeneity analyses.

5.2 Credit Bureau Reporting Subsample

The removal of medical debt from credit reports has historically been cited as a primary benefit of debt relief, given the visibility of these debts to lenders, landlords, and employers. Shortly after the start of the study, and unrelated to the study, the collections agency discontinued reporting medical debt to the credit bureaus, in line with broader industry trends. However, for a small number of accounts in the collector debt experiment, the intervention occurred before the debt collector stopped reporting, allowing us to estimate the impact of debt relief relative to a counterfactual with reporting of medical debt.

We identify accounts that were reported to the bureaus by matching the dollar amounts of medical debt in the collections account data to those in the credit bureau data in the four quarters prior to the intervention (see Appendix Section B.3 for more details). We match 2,768 accounts (6.8%) in wave 1 of the collector debt experiment data, with virtually identical match rates for treatment and control. After the intervention, the treatment accounts no longer appear on credit reports, with the control group following three quarters later when the debt collector ceased reporting (see Panel A of Appendix Figure A6).[16] As noted in Section 2, the debt collector placed debt with several third parties that take responsibility for outreach and collections, and the partial reporting could be explained by selective reporting by some of these third parties.

We estimate the impact of debt relief for the credit reporting subsample using the credit bureau panel data and controlling for person fixed effects. We restrict the sample to a period from 4 quarters before the intervention to 4 quarters after the cessation of control group reporting. We then estimate regressions of the form:

$$
\begin{aligned}
y_{it} = {} & \beta_1 T_i \times CONTROL\_REPORTING_t \\
& + \beta_2 T_i \times CONTROL\_POST\_REPORTING_t \\
& + \gamma_i + \alpha_{r,t} + \varepsilon_{it} \qquad (2)
\end{aligned}
$$

where T_i is an indicator for assignment to the debt-relief treatment, $CONTROL\_REPORTING_t$ is an indicator for quarters after initial treatment assignment when there was control group reporting, $CONTROL\_POST\_REPORTING_t$ is an indicator for quarters after initial treatment assignment when control group had ceased reporting, γ_i are person fixed effects, and $\alpha_{r,t}$ are fixed effects at the level of randomization fully interacted with calendar quarter. The specification does not include a non-interacted T_i indicator because it is collinear with the person fixed effects and does not include non-interacted $CONTROL\_REPORTING_t$ and $CONTROL\_POST\_REPORTING_t$ indicators

[16] We obtain a similar match rate for wave 2 of the collection data but control group reporting only continues for a single quarter after the intervention (see Panel B of Appendix Figure A6). We therefore focus on wave 1 here but show results for wave 2 in the appendix for completeness.

because they are collinear with the $\alpha_{r,t}$ fixed effects. To examine time trends in the effects, we separately estimate event study specifications, which allow the treatment effect to vary flexibly by quarter but are otherwise identical to our baseline specification.

Panel A of Table 4 shows the effect of debt relief in the reporting subsample. Medical debt relief reduces the count of medical debts in collections by 0.98 (p-value < 0.001) and the dollar amount of medical debt in collections by \$1,206 (p-value < 0.001; 29% of control mean of \$4,145), during the period of control group reporting. When there is no longer control group reporting, the effects return to zero. These patterns can be clearly seen in the event study plots shown in Appendix Figure A7.

During the period with control group reporting, debt relief causes a 4.2 percentage-point (p-value < 0.001) reduction in the share of persons with a credit score relative to a control mean of 98.1% (Table 4, Panel A). The result suggests that the reporting of medical debt allows the credit bureaus to "score" persons who would counterfactually have too thin of a file for their scoring algorithms. Among persons in the balanced panel who have credit scores in all periods, medical debt relief raises credit scores by an economically small 3.6 points (p-value of 0.016). The effects drop to zero when there is no longer control group reporting, as can be clearly seen in Appendix Figure A7.

The on-impact increase in credit scores is accompanied by a gradual increase in credit limits, illustrated in Appendix Figure A7. During the three quarters with reporting for the control group, limits increase by \$155 (p-value of 0.037; 8% of the control mean of 1,949). This increase grows to \$342 (p-value of 0.010; 15.4% of the control mean of 2,227) in the four subsequent quarters. The time path illustrated by the event study coefficients shows that the effect grows approximately linearly over the five quarters following the intervention before leveling out. This leveling out is consistent with credit limits for the control group starting to grow three quarters after the intervention, when the debt collector ceased reporting for the control group.

We also assess whether the effects are different between persons for whom debt relief restores a "clean" credit report with no other debts in collections and persons who have other debts in collections. Panels B and C of Table 4 show results split by whether the person had other debts in collection in the quarter prior to the intervention. The effects on having a credit score and credit scores conditional on having one are concentrated among those with no other debt in collections. For example, during the period with control group reporting, the improvement in credit scores is 13.4 points (p-value of 0.010) for those with no other debts in collections versus 1.4 points (p-value of 0.379) for those with other debts in collections. For those with no other debts in collections, the subsequent increase in credit limits is a fairly large, but somewhat imprecise, $922 (23% of the control mean, p-value of 0.069). For those with other debts in collection, this effect is a modest $179 increase (10% of the control mean, p-value of 0.117). This heterogeneity can be seen in the event study plots shown in Appendix Figures A8 and A9.

In Appendix Section B.3, and corresponding Appendix Tables A26 through A28, we examine the impact of debt relief on the other main credit bureau outcomes, including measures of borrowing and financial distress. We do not find any effect on these outcomes, either for the full sample of matched persons or when we split the sample by whether the person had other debts in collections.

Taken together, these results indicate that medical debt relief has a marginal positive impact on credit access in the presence of reporting to the credit bureaus, with larger effects for those with no other debts in collections. Overall and for all subgroups, the effects are too small to generate noticeable changes in borrowing or financial distress. These results are relevant for the effects of medical debt relief in previous periods where reporting was common. While current or future medical debt relief may not deliver these benefits (unless the relief is precisely targeted to the small share of persons with ongoing credit reporting), the results also speak to the partial equilibrium effects of the CFPB agreement with the credit bureaus to stop displaying many types of medical debt on credit reports (CFPB, 2023c).

5.3 Collections Account Outcomes

Table 5 reports the effect of medical debt relief on the accrual of future medical debt. We conduct this analysis using all 18 waves of the hospital debt experiment and define future medical debt using appearances in the collections account data subsequent to the initial wave in which a person appears. We did not consider using the data in this manner when designing the study and did not pre-register this analysis.

Panel A shows that medical debt relief caused a 1.0 percentage-point increase in the probability of having an unpaid medical bill sent to collections (6.6% of the control mean of 15.5%). The amount of debt increased by $14 (7.2% of the control mean of $199). Both outcomes are statistically significant at the 5% level.

The increase in debt sent to collections could result from reduced payments for services already received or from greater utilization of future medical care. To distinguish the two, 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 the vast majority of the increased debt accumulation is associated with pre-relief medical services, while Panel C shows small and statistically insignificant increases in medical debt collections associated with post-relief medical services. The results imply that reduced payment of existing bills, rather than additional utilization, are responsible for the increases in debt sent to collections that we observe.

Figure 3 plots the effects on future medical debt by quartile of medical debt eligible for relief, and Appendix Table A15 shows the underlying estimates. The effects are generally increasing in the amount of medical debt relieved, both in levels and in proportion to the control group mean. For instance, medical debt relief increases future debt accrual by $34 (12.8% of the $267 control mean) for those in the top quartile of baseline collector debt versus $6 (4.0% of the $142 control mean) for those in the bottom quartile. As in the baseline analysis, the effects are almost entirely driven by pre-relief medical services (see Appendix Table A15).

The effect on pre-relief services is consistent with a simple expectations mechanism where those who receive medical debt relief reduce their payments of existing bills due because they anticipate greater future debt relief.[17] Alternatively, or in addition, these effects could arise from a mechanism in which patients target a certain level of indebtedness, such as in the model in Dobkin et al. (2018). In this framework, patients who have their debt relieved have more "room" in their debt budgets and reduce their repayment of existing bills. Both mechanisms could also explain the heterogeneous effects we document.

In Appendix Sections B.1 and B.2, we examine the sensitivity of our findings to controlling

[17]Conceptually, such a mechanism could also raise healthcare utilization, which could result in greater medical debt. While our point estimates on post-relief medical services are small, our collections account data do not extend sufficiently far beyond the date of debt relief to give us the power to rule out economically significant proportional utilization effects.

for baseline characteristics and heterogeneity in the effects by the age of the debt, the age of the potential beneficiary, and baseline debt in collections reported to the credit bureaus. None of these analyses provide any notable results.

5.4 Survey Outcomes

Table 6 shows the average effects of debt relief on pre-specified survey outcomes. Our primary outcome is an indicator for at least moderate depression, as measured by the PHQ-8. In our expert survey, the median respondent predicted a 7.0 percentage-point reduction in depression (8.0 percentage points if we weigh by confidence in their answers). Panel A of Table 6 shows no detectable effect on depression. We estimate that debt relief *raises* the share with at least moderate depression by a statistically insignificant 3.2 percentage points (p-value of 0.097) relative to a mean of 45.0%. A 95% confidence interval allows us to reject a reduction in depression of more than 0.6 percentage points.

Our pre-analysis plan specified heterogeneity analysis of effects on depression by the amount of medical debt eligible for relief. Panel C of Figure 3 plots point estimates by debt-relief quartile, and Appendix Table A19 shows the underlying estimates. We estimate null effects on debt relief in quartiles one through three. For quartile four, we estimate a large and statistically significant 12.4 percentage-point increase in depression (p-value of 0.002) relative to a control mean of 45.9%. We discuss this result in more detail in Section 6 below.

The effects of debt relief on related mental health measures largely mirror those for depression. The second and third rows of Panel A of Table 6 show the average effects on whether the person had at least moderate anxiety on the GAD-7 and whether they reported being sometimes stressed. Similar to the depression measures, we estimate statistically insignificant increases of 1.6 percentage points (adjusted p-value of 0.392) for anxiety and 2.7 percentage points (adjusted p-value of 0.158) for stress.[18] Heterogeneity analysis by relief-eligible medical debt shown in Figure 3 and Appendix Table A19 indicates substantial worsening of mental health for those in the top quartile of debt relief, with a 10.6 percentage-point increase (adjusted p-value of 0.014) in anxiety and a 5.8 percentage-

[18] Recall that for our secondary outcomes, we report adjusted p-values that correct for multiple testing within each pre-specified domain using the free step-down resampling method of Westfall and Young (1993).

point increase (adjusted p-value of 0.064) in stress.

The effects on self-assessed general health and subjective well-being follow the same pattern. Panels B and C of Table 6 show statistically insignificant reductions of 2.7 percentage points (p-value of 0.161) for subjective well-being (at least "pretty happy") and 2.6 percentage points (p-value of 0.188) for general health (at least "good health"). Heterogeneity analysis shown in Figure 3 and Appendix Table A19 indicates stronger effects for those in the top quartile of relief-eligible medical debt, with a -7.9 percentage-point effect (p-value of 0.047) for subjective well-being and a -7.8 percentage-point effect (p-value of 0.051) for general health.

We do not detect evidence of meaningful impacts on healthcare utilization (Table 6, Panel D). Debt relief causes a statistically insignificant 2.4 percentage-point reduction in the probability of receiving all needed healthcare in the past 12 months (relative to a control mean of 56.7%), and we can reject an effect outside of -6.2 to 1.4 percentage points with a 95% confidence interval. We estimate a statistically insignificant 2.4 percentage-point reduction in the probability of receiving all needed prescription medicines over the past 12 months (relative to a control mean of 71.9%) and can reject an effect outside of -5.9 to 1.0 percentage points with 95% probability. Heterogeneity analyses suggest larger negative effects for those with larger amounts of medical debt relief, but the results are too imprecise to be conclusive.

We find no systematic evidence of impacts on financial distress (Table 6, Panel E), consistent with the analysis of the credit bureau data. Debt relief causes a statistically insignificant 3.5 percentage-point increase in whether individuals had trouble paying other bills (adjusted p-value of 0.150). Our survey asks multiple questions about whether the respondent cut back their spending or increased their borrowing. We construct inverse-standard deviation indices that separately combine responses to these sets of questions and estimate fairly precise null effects on these outcomes.

In Appendix Sections B.5 and B.6, we present additional analyses to probe the internal and external validity of our findings. Recall that treated persons were a statistically insignificant 1.3 percentage points more likely to respond to our survey. We examine internal validity to differential response rates with alternative specifications that (i) saturate the regression with observable controls and (ii) adjust the sample using speed to respond to the surveys (i.e., time between outreach and response) as a proxy for the unobserved propensity to respond. Appendix Table A29 shows that neither exercise has a noticeable impact on our estimates, giving us confidence in the internal validity of our findings.

To examine the external validity of our results to survey non-respondents, we test for heterogeneous effects based on (i) the predicted response propensity from a logistic regression of a response indicator on baseline characteristics and (ii) proxying for the unobservable response propensity with speed to respond to our survey. While these exercises are inherently limited in their ability to reveal differences for non-respondents, Appendix Table A30 indicates that neither exercise provides any evidence to suggest that our main findings are not externally valid.

We pre-specified heterogeneity analyses by medical debt eligible for relief, age of debt relieved, age of the person, and amount of debt in collections on their credit report. We report the results of these heterogeneity splits in the Appendix Tables A19 through A22. None of these analyses produce noteworthy results beyond those described above.

5.5 Awareness

The impact of medical debt relief can be thought of as operating through two channels: (i) the removal of medical debt, which eliminates collections activity and any debt repayment, and (ii) the knowledge of the charitable intervention via the notification letter (see Appendix Figure A1).

To measure knowledge and recall of the intervention, our survey asked subjects whether they had medical debt forgiven in the prior 18 months and, if yes, how much medical debt had been relieved. The questions did not mention RIP to avoid priming survey respondents. Appendix Table A23 shows that treated individuals are 16.1 percentage points more likely to report debt forgiveness (p-value < 0.001) relative to the control mean of 8.1%. Treated persons also report having over twice as much debt forgiven than the control group.

To explore the role that awareness and salience of the intervention plays in mediating the treatment effects, we randomly selected a subset of treated persons to receive telephone calls in addition to the notification letters (described in Section 2.2). Appendix Table A23 indicates that persons assigned to follow-up calls were 18.0 percentage points (p-value less than 0.001) more likely than control persons to report receiving debt forgiveness. They were also 3.8 percentage points more likely than other treatment persons to report debt forgiveness, but this estimate is less precise and we cannot rule out effects outside of -2.4 to 10.1 with a 95% confidence interval.

Table 7 indicates that the additional awareness or salience from the phone calls may partially mediate the negative effects of medical debt relief on depression and anxiety, with mixed results for the other health measures. Treated persons assigned to phone calls were 6.4 percentage points more likely to have at least moderate depression (p-value of 0.029) relative to the control group, while treated persons who were not assigned to phone calls showed virtually no change in depression relative to the control. The pattern is similar for anxiety, with treated persons assigned to phone calls experiencing a statistically insignificant 4.5 percentage-point increase in anxiety (adjusted p-value of 0.232) relative to the control group, compared to no effect for treated persons who were not called. For stress, subjective well-being, and general health, the pattern is less clear, and none of the estimates for treated individuals assigned to receive phone calls are statistically significant.

Taken together, we view this analysis as providing suggestive evidence that awareness or salience of the charitable intervention may have mediated some of the negative mental health effects of medical debt relief. However, given the statistical power, incomplete contact rates, and some inconsistency across the secondary outcomes, we caution against drawing strong conclusions from this analysis.

6 Discussion

There were three key statistically significant effects of our intervention: (i) small improvements in credit access for the subset of persons whose medical debt would have otherwise been reported to the credit bureaus, (ii) modest reduction in payments of future medical bills, and (iii) worsened mental health outcomes, concentrated among those who had the largest amount of debt relieved and those who received phone calls to raise awareness and salience of the intervention. In this section, we discuss potential explanations for these findings.

First, while the sign of the effects on credit scores was not surprising, we were unsure of the magnitudes and whether they would translate into effects on measures of credit access, such as credit card limits, and financial distress, such as debts past due. The small average effects on both scores and credit limits, with larger effects among those with no other medical debts in collection, indicate medical debt relief can have a meaningful effect on credit access when targeted at persons with otherwise clean credit reports, while the null effects on other outcomes suggest it is unlikely to reduce financial distress more broadly.

The results indicate that historical medical debt relief, when reporting was common, likely improved credit access. The results also point to the (partial-equilibrium) effects of the credit bureaus' decision to cease reporting many types of medical debt on credit reports (CFPB, 2023c). By the same token, this large-scale cessation of reporting indicates there may be limited opportunities for medical debt relief to improve credit access in the future. As such, our null average effects may be most appropriate for projecting the credit market effects of ongoing initiatives to relieve medical debt.

Second, the reduced payments of future medical bills are consistent with an expectations mechanism. If persons who receive debt relief expect additional debt relief in the future, they will be less likely to pay future medical bills. This finding is also consistent with a mechanism in which persons target an amount of indebtedness and debt forgiveness provides them with more room to increase their debt (Dobkin et al., 2018). Both of these mechanisms could also explain our finding that the repayment effects are increasing in the (non-experimentally manipulated) amount of debt forgiven. We note that the notice letter (Appendix Figure A1) explicitly states "[t]he forgiveness is for this outstanding bill only" and "[w]e have not forgiven any other medical debt you might owe." However, we do not know if this statement was internalized by the recipients of debt relief.

Third, the effects on mental health are more difficult to explain. Because we did not expect these results, neither the experiment nor the survey was designed to investigate their cause. The discussion of mechanisms should, therefore, be viewed as speculative.

Debt relief may have worsened mental health by raising the salience of recipients' financial deprivation without meaningfully addressing their underlying economic situations. Jaroszewicz et al. (2023) find that recipients of randomized one-time unconditional cash transfers of $500 or $2,000 experienced significant reductions in their psychological well-being. The authors' preferred interpretation is that recipients of the cash payments viewed the transfers as insufficient to close the gap between their resources and needs, raising the salience of their financial distress and harming their mental health.

In our context, medical debt relief may have similarly impacted mental health by highlighting the gap between resources and needs. This mechanism can readily explain our finding that the mental health effects are larger among those who were assigned to phone calls, which may have increased awareness and salience of the treatment. The concentrated effects among those with the largest amount of debt relief could reflect greater baseline financial distress among these persons and, thus, greater insufficiency of the debt relief. Appendix Table A7 shows that control group persons in the top quartile of relief-eligible medical debt have $5,636 of debt in collections versus $2,977 for those in the bottom quartile.

In Appendix Table A31, we separately examine the effects of debt relief on the eight components of the PHQ-8 depression screen. One particular component, "Feeling bad about self, like a failure, or let yourself or family down," maps to the salience of financial deprivation more clearly than the others. We estimate a prominent increase in this component (p-value of 0.026). Similar to our findings for the combined PHQ-8 measure, we estimate larger effects for this subquestion among those with the greatest relief-eligible debt (Appendix Table A32) and for those who received the awareness intervention (Appendix Table A33). This analysis should also be interpreted as suggestive but does provide some suggestive corroborating evidence for a financial deprivation mechanism.

Alternatively, or in addition, our charitable debt relief could have worsened mental health through the stigma of receiving charity. Moffitt (1983) and Atkinson (1987), among others, point to stigma as a factor that deters needy persons from seeking benefits for which they are eligible. In our context, persons do not need to apply for medical debt relief, but may still feel stigma from receiving the charitable intervention. Recipients are overoptimistic about the likelihood that they will pay their medical debt relative to recovery rates, with survey respondents expecting to pay 54% of their medical debt on average and reporting it they feel it is fair to pay 37% of their outstanding bills on average. A stigma mechanism could explain the concentrated negative mental health effects among those who were assigned phone calls to increase awareness since being aware of the intervention is a necessary condition for this mechanism to operate. The interpersonal interactions may have also accentuated any stigma effects.[19]

Stepping back, the negative mental health effects for some subgroups fall into the category of

[19]To explain the concentration of negative mental health effects among those with the largest amount of debt relief, the severity of stigma would need to increase in the amount of debt relief. This could occur because a greater amount of debt relief generates intrinsically more stigma or because greater debt relief is more salient.

"raising more questions than they answer." Given the similarly worrying detrimental effects in Jaroszewicz et al. (2023), we think designing studies to understand this mechanism is an important endeavor.

7 Conclusion

Concern about the burden of medical debt has prompted private donors and local governments to spend millions of dollars buying and relieving billions of dollars in medical debt. We analyzed two randomized experiments that relieved medical debt with a face value of $169 million across 83,401 people. We estimated that debt relief has no average effect on financial outcomes, but modestly increases credit access for a small subset of persons whose medical debt would have been counterfactually reported to the credit bureaus. We found that debt relief reduces repayment of existing medical bills. We estimated that debt relief had no average impact on mental health, with detrimental effects for some groups in pre-registered heterogeneity analysis.

Our findings contrast with evidence on the effects of non-medical debt relief and evidence on the benefits of upstream relief of medical bills through hospital financial assistance programs. Our results are similarly at odds with views of the experts we surveyed, pronouncements by policymakers funding medical debt relief, and self-reported assessments of recipients of medical debt relief. In a survey conducted by RIP Medical Debt (2023) of persons with medical debt, 60% of respondents reported that medical debt negatively impacted their mental health, and 42% reported it lowered their self-worth. The results underscore the importance of using randomized experiments to separate the causal impact of debt relief from correlations that arise from, for example, a negative health shock that independently causes medical debt and worse financial and health outcomes.

The disappointing results from the intervention we studied should not distract from the underlying problem we sought to address. Medical debt is pervasive, and the population we study is experiencing poor mental health and severe financial distress. While the results indicate limited benefits from downstream debt forgiveness, there is still potential that medical debt relief targeted further upstream or in different populations could yield meaningful benefits. Further research will be needed to demonstrate such effects.

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Table 1. Summary Statistics

	Hospital Debt Experiment			Collector Debt Experiment	Nationally Representative Sample	
	All	Outreach	Respondents		All	> $0 Medical Debt in Collections
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Experiment Overview						
Observations						
Total	75,873	14,922	2,888	137,038	58,669	10,336
Treated	14,377	5,311	1,086	69,024	.	.
Control	61,496	9,611	1,802	68,014	.	.
Aggregate Medical Debt ($, Millions)						
Total	102.5	33.7	6.1	296.9	.	.
Treated	19.0	11.8	2.2	149.6	.	.
Control	83.6	21.9	3.9	147.3	.	.
Medical Debt ($)						
Mean	1,352	2,260	2,105	2,167	.	.
25th percentile	235	815	794	300	.	.
50th percentile	620	1,340	1,276	820	.	.
75th percentile	1,475	2,426	2,276	2,073	.	.
Medical Debt Age (Quarters)						
Mean	5.1	5.1	5.2	28.2	.	.
25th percentile	4.7	4.7	4.7	22.7	.	.
50th percentile	5.0	5.0	5.0	24.3	.	.
75th percentile	5.4	5.4	5.5	28.6	.	.
Panel B. Baseline Characteristics						
Demographics						
Age (years)	43.1	41.3	41.7	46.0	.	.
Male (%)	45.2	46.7	38.8	43.3	.	.
Race and Ethnicity (%)						
Black	.	.	18.8	.	.	.
Non-hispanic white	.	.	43.7	.	.	.
Hispanic (any race)	.	.	30.9	.	.	.
Credit Bureau Data						
Credit score	575.3	569.2	576.2	572.1	693.8	579.1
Medical debt in collections (%)	58.8	60.5	57.6	65.1	17.6	100.0
Medical debt in collections ($)	2,303	2,667	2,233	2,875	276	1,567
Debt in collections ($)	3,468	3,906	3,485	3,916	645	2,542
Total debt ($)	32,654	28,843	38,933	25,908	77,647	31,209

Notes: Table reports pre-treatment summary statistics for the hospital debt and collector experiments with a comparative national representative sample from Q3 2018. Column (1) reports statistics for the full hospital debt sample. Columns (2) and (3) show the subsamples that were contacted for and responded to the NORC survey, respectively. Column (4) reports statistics for the collector debt sample. Columns (5) and (6) report credit bureau outcomes for a nationally representative sample and the subset of this sample with strictly positive medical debt in collections (as observed in credit bureau data), respectively. Aggregate medical debt is defined as the sum of all first-wave medical debt. Credit bureau variables are measured in the first pre-treatment quarter.

Table 2. Balance on Baseline Characteristics

	Hospital Debt Experiment						Collector Debt Experiment	
	All		Survey Outreach		Respondents			
	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Demographics								
Age (years)	43.1	-0.0	41.3	-0.1	41.8	-0.3	46.0	-0.1
		[0.936]		[0.796]		[0.574]		[0.189]
Male (%)	45.1	0.2	46.5	0.1	39.8	-3.3	43.4	-0.2
		[0.685]		[0.884]		[0.078]		[0.825]
Panel B. Race and Ethnicity								
Black (%)	.	.	.	.	18.2	1.5	.	.
						[0.326]		
Non-hispanic white (%)	.	.	.	.	44.0	-1.3	.	.
						[0.494]		
Hispanic (any race) (%)	.	.	.	.	31.4	-0.4	.	.
						[0.823]		
Panel C. Collector Data								
Medical debt ($)	1,359	2	2,280	-61	2,178	-176	2,166	9
		[0.916]		[0.236]		[0.085]		[0.694]
Medical debt age (quarters)	5.2	-0.0	5.1	0.0	5.2	-0.0	28.2	-0.0
		[0.298]		[0.919]		[0.887]		[0.330]
Has health insurance (%)	60.9	-0.4	48.2	-0.2	57.6	-1.7	.	.
		[0.322]		[0.847]		[0.378]		
Panel D. Other								
Response rate (%)	.	.	.	.	18.7	1.3	.	.
						[0.056]		
Observations†	61,496	14,377	9,611	5,311	1,802	1,086	68,014	69,024
F statistic (p-value)††		0.90		0.50		0.39		0.59

Notes: Table presents the balance of baseline characteristics within the collector debt, hospital debt, and survey samples. Odd-numbered columns present the control group means. Even-numbered columns present the difference between the control and treatment group means, recovered from estimating Equation 1. p-values for each difference are reported in square brackets.
†: Sample size for control and treatment groups reported in odd- and even-numbered columns, respectively.
††: F-statistic p-value reported for the joint null hypothesis that all of the differences for a given sample are zero.

Table 3. Effects of Debt Relief on Credit Bureau Outcomes

	Hospital Debt Experiment			**Collector Debt Experiment**		
	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^{+}	1.20	-0.01	[0.762]	1.02	0.01	[0.162]
		(0.02)	.		(0.01)	.
Number of accounts in default	1.08	-0.01	[0.708]	0.92	0.01	[0.172]
		(0.02)	{0.937}		(0.01)	{0.329}
Debt past due ($)	4,908	53	[0.685]	4,815	85	[0.269]
		(130)	{0.937}		(77)	{0.348}
Balances in default ($)	3,741	9	[0.921]	3,705	50	[0.390]
		(94)	{0.939}		(58)	{0.388}
Panel B. Debt in Collections						
Number of debts in collections	4.66	-0.02	[0.759]	3.55	0.01	[0.785]
		(0.06)	{0.747}		(0.03)	{0.792}
Debts in collections ($)	4,119	-60	[0.350]	3,112	41	[0.226]
		(64)	{0.483}		(34)	{0.327}
Panel C. Bankruptcy						
Bankruptcy in last 12 months (%)	1.30	-0.05	[0.670]	0.65	-0.05	[0.287]
		(0.11)	.		(0.04)	.
Panel D. Access to Credit						
Has credit score (%)	97.22	-0.11	[0.492]	90.73	0.10	[0.544]
		(0.16)	{0.786}		(0.16)	{0.660}
Credit score (excluding missing)	582.16	-0.10	[0.890]	577.60	-0.35	[0.416]
		(0.76)	{0.894}		(0.42)	{0.660}
Credit card limit ($)	2,654	61	[0.419]	2,640	79	[0.059]
		(75)	{0.786}		(42)	{0.164}
Panel E. Borrowing						
Number of credit cards	0.81	0.00	[0.737]	0.78	0.01	[0.185]
		(0.01)	{0.926}		(0.01)	{0.363}
Credit card balance ($)	1,481	27	[0.469]	1,306	35	[0.057]
		(37)	{0.868}		(18)	{0.171}
Number of auto loans	0.39	0.00	[0.603]	0.30	0.00	[0.167]
		(0.01)	{0.904}		(0.00)	{0.363}
Auto loan balance ($)	8,020	-47	[0.735]	5,417	41	[0.480]
		(139)	{0.926}		(58)	{0.462}
Panel F. Sample Size						
Observations†	55,653	12,998		64,947	65,968	

Notes: Table reports the effects of medical debt relief on credit bureau outcomes for the hospital debt and collector debt experiments. Columns (1) and (4) report the control means in the fourth quarter post-treatment for each experiment, and columns (2) and (5) report treatment effects as estimated in Equation 1. Standard errors are in parentheses below the treatment effect estimates. In columns (3) and (6), unadjusted and multiple-inference-adjusted p-values are in square and curly brackets, respectively. Multiple inference adjustment is performed using the Westfall and Young (1993) method by domain.
+: Primary pre-specified outcome. Indicates the number of accounts $\geq$ 30 days past due.
†: Sample sizes for control and treatment groups reported in the control mean and treatment effect columns, respectively.

Table 4. Effects of Debt Relief in Credit Reporting Subsample

	Control Reporting			Post Control Reporting	
	Control Mean	Treatment Effect	p-value	Treatment Effect	p-value
	(1)	(2)	(3)	(4)	(5)
Panel A. Full Sample of Matched Persons					
Number of medical debts in collections	4.73	-0.98	[0.000]	-0.21	[0.196]
		(0.10)		(0.16)	
Medical debts in collections ($)	4,145.31	-1,205.78	[0.000]	-124.79	[0.549]
		(144.49)		(208.39)	
Has credit score (%)	98.09	-4.16	[0.000]	0.49	[0.632]
		(0.65)		(1.03)	
Credit score (never missing)	570.72	3.55	[0.016]	0.42	[0.831]
		(1.47)		(1.97)	
Credit card limit ($)	1,949.26	155.15	[0.037]	342.17	[0.010]
		(74.50)		(132.28)	
Observations†	1,341	1,427			
Panel B. No Other Debt in Collections					
Number of medical debts in collections	1.10	-0.65	[0.000]	-0.04	[0.706]
		(0.09)		(0.10)	
Medical debts in collections ($)	1,022.57	-1,005.54	[0.000]	-358.66	[0.211]
		(283.83)		(286.59)	
Has credit score (%)	93.10	-15.14	[0.000]	3.17	[0.420]
		(3.10)		(3.93)	
Credit score (never missing)	609.78	13.44	[0.010]	8.56	[0.186]
		(5.19)		(6.46)	
Credit card limit ($)	3,715.93	312.68	[0.286]	921.90	[0.069]
		(292.65)		(506.28)	
Observations†	232	234			
Panel C. Other Debt in Collections					
Number of medical debts in collections	5.58	-1.02	[0.000]	-0.20	[0.310]
		(0.12)		(0.19)	
Medical debts in collections ($)	4,900.94	-1,227.51	[0.000]	-30.31	[0.905]
		(173.28)		(253.80)	
Has credit score (%)	99.35	-1.78	[0.000]	0.01	[0.989]
		(0.44)		(0.91)	
Credit score (never missing)	564.00	1.37	[0.379]	-1.07	[0.612]
		(1.56)		(2.11)	
Credit card limit ($)	1,511.24	116.31	[0.097]	179.42	[0.117]
		(70.07)		(114.30)	
Observations†	1,079	1,164			

Notes: Table reports the effects of medical debt relief on credit bureau outcomes for the wave 1 credit reporting subsample, before and after medical debt collections ceased being reported to a credit bureau (as specified in Equation 2). Column (1) reports the control means during the control group reporting period, column (2) reports the treatment effects in this period, and column (3) reports the corresponding p-values in brackets. Columns (4) and (5) report the treatment effects and corresponding p-values during the post-reporting period, respectively. Standard errors are in parentheses below the treatment effect estimates.
†: Sample sizes for control and treatment groups reported in the control mean and treatment effect columns, respectively.

Table 5. Effects of Debt Relief on Future Medical Debt in the Hospital Debt Experiment

	Control Mean	Treatment Effect	p-value
	(1)	(2)	(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)	
Panel D. Sample Size			
Observations†	61,496	14,377	

Notes: Table presents the effects of medical debt relief on (1) the probability of having future medical debt sent to collections and (2) the balances of future medical debt in collections for the hospital debt experiment. Column (1) reports the control means, column (2) reports the treatment effects with standard errors reported below (in parentheses), and column (3) contains the p-value in brackets. Panel A presents effects for any debt purchased in waves after the first wave a person is observed in ("future debt"); Panel B presents effects for future debt that has a service date prior to this wave; Panel C presents effects for future debt whose service date is after this wave. Treatment effects are estimated from Equation 1.

†: Sample sizes for control and treatment groups reported in the control mean and treatment effect columns, respectively.

Table 6. Effects of Debt Relief on Survey Outcomes

	Control Mean	Treatment Effect	p-value
	(1)	(2)	(3)
Panel A. Mental Health			
At least moderate depression (%)+	44.95	3.23	[0.097]
		(1.94)	.
At least moderate anxiety (%)	40.07	1.63	[0.395]
		(1.92)	{0.392}
At least sometimes stressed (%)	76.53	2.72	[0.093]
		(1.62)	{0.158}
Panel B. Subjective Wellbeing			
At least pretty happy (%)	54.33	-2.72	[0.161]
		(1.94)	.
Panel C. General Health			
At least good health (%)	53.83	-2.56	[0.188]
		(1.94)	.
Panel D. Health Care Utilization			
Had all needed healthcare (%)	56.66	-2.37	[0.220]
		(1.93)	{0.310}
Had all needed RX (%)	71.92	-2.42	[0.170]
		(1.77)	{0.310}
Panel E. Financial Distress			
Had trouble paying other bills (%)	60.82	3.53	[0.061]
		(1.88)	{0.150}
Cut back spending (Z-score)	0.00	-0.00	[0.993]
		(0.04)	{0.994}
Increased borrowing (Z-score)	0.00	0.03	[0.381]
		(0.04)	{0.558}
Panel F. Sample Size			
Observations†	1,802	1,086	

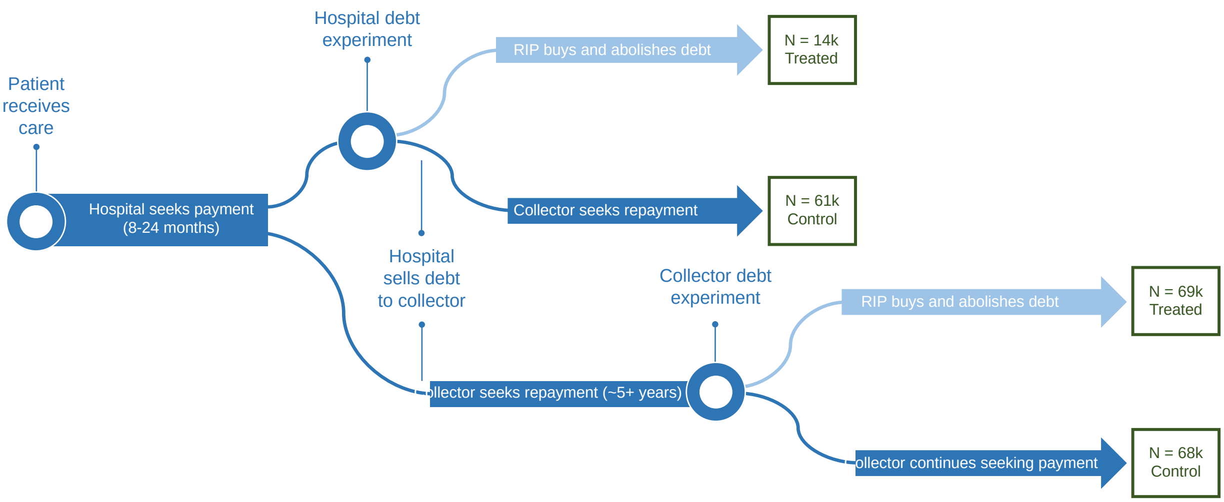
Notes: Table presents the effects of medical debt relief on self-reported health and financial distress outcomes within the NORC survey sample (a subset of the hospital debt sample). Column (1) reports the means for control group respondents. Column (2) reports the treatment effects for treatment group respondents, with standard errors reported in parentheses. Column (3) reports unadjusted and multiple-inference-adjusted p-values in square and curly brackets, respectively. Multiple inference adjustment is performed using the Westfall and Young (1993) method by domain. Estimates are computed as outlined in Equation 1.
+: Primary pre-specified outcome.
†: Sample sizes for control and treatment groups reported in the control mean and treatment effect columns, respectively.

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

	Control	**Awareness Intervention**				
		Treated, Not Called		Treated, Called		Difference
	Control Mean	Treatment Effect	p-value	Treatment Effect	p-value	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Mental Health						
At least moderate depression (%)+	44.8	0.3	[0.930]	6.4	[0.029]	[0.093]
		(2.99)	.	(2.96)	.	.
At least moderate anxiety (%)	40.4	1.1	[0.717]	4.5	[0.123]	[0.342]
		(2.95)	{0.704}	(2.95)	{0.232}	{0.544}
At least sometimes stressed (%)	76.8	4.4	[0.069]	2.7	[0.260]	[0.578]
		(2.40)	{0.120}	(2.42)	{0.287}	{0.582}
Panel B. Subjective Wellbeing						
At least pretty happy (%)	54.3	-2.9	[0.325]	-1.2	[0.686]	[0.636]
		(2.98)	.	(2.96)	.	.
Panel C. General Health						
At least good health (%)	53.6	-1.5	[0.608]	-3.1	[0.300]	[0.677]
		(2.99)	.	(2.97)	.	.
Panel D. Health Care Utilization						
Had all needed healthcare (%)	56.4	-6.9	[0.020]	-1.4	[0.629]	[0.136]
		(2.97)	{0.046}	(2.96)	{0.845}	{0.221}
Had all needed RX (%)	71.2	-3.9	[0.159]	0.5	[0.856]	[0.195]
		(2.77)	{0.131}	(2.67)	{0.849}	{0.221}
Panel E. Financial Distress						
Had trouble paying other bills (%)	61.6	5.3	[0.061]	3.3	[0.244]	[0.561]
		(2.84)	{0.196}	(2.84)	{0.508}	{0.904}
Cut back spending (Z-score)	-0.0	0.0	[0.985]	-0.0	[0.956]	[0.953]
		(0.06)	{0.987}	(0.06)	{0.955}	{0.948}
Increased borrowing (Z-score)	-0.0	0.0	[0.806]	0.0	[0.552]	[0.780]
		(0.06)	{0.969}	(0.06)	{0.763}	{0.943}
Panel F. Sample Size						
Observations†	1,251	363		381		

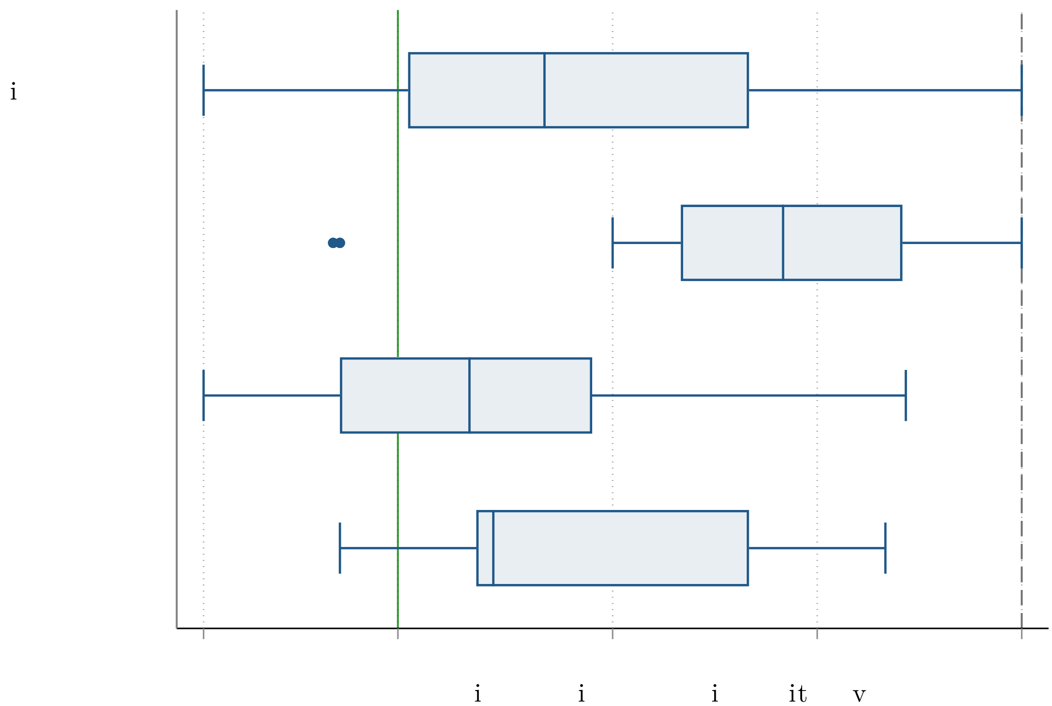
Notes: Table presents the effect of medical debt relief and the awareness subexperiment on health and financial distress outcomes, for waves 6-14 of the hospital debt sample surveyed. We adapt the specification from Equation 1 by adding an additional interaction term between debt relief treatment and call attempted. Column (1) reports the means for control group respondents. Column (2) reports the treatment effects for treated respondents who were not assigned to receive a call in the awareness subexperiment, and column (4) reports the treatment effects for those who were assigned to receive a call. Standard errors are reported below the point estimates in parentheses. Columns (3) and (5) report the corresponding unadjusted and adjusted p-values in square and curly brackets, respectively. Column (6) reports the p-value of the difference between the treatment effects on treated individuals not called and those who were called. Multiple inference adjustment is performed using the Westfall and Young (1993) method by domain.
+: Primary pre-specified outcome.
†: Sample sizes for control and treatment groups reported in the control mean and treatment effect columns, respectively.

Figure 1. Experiment Design



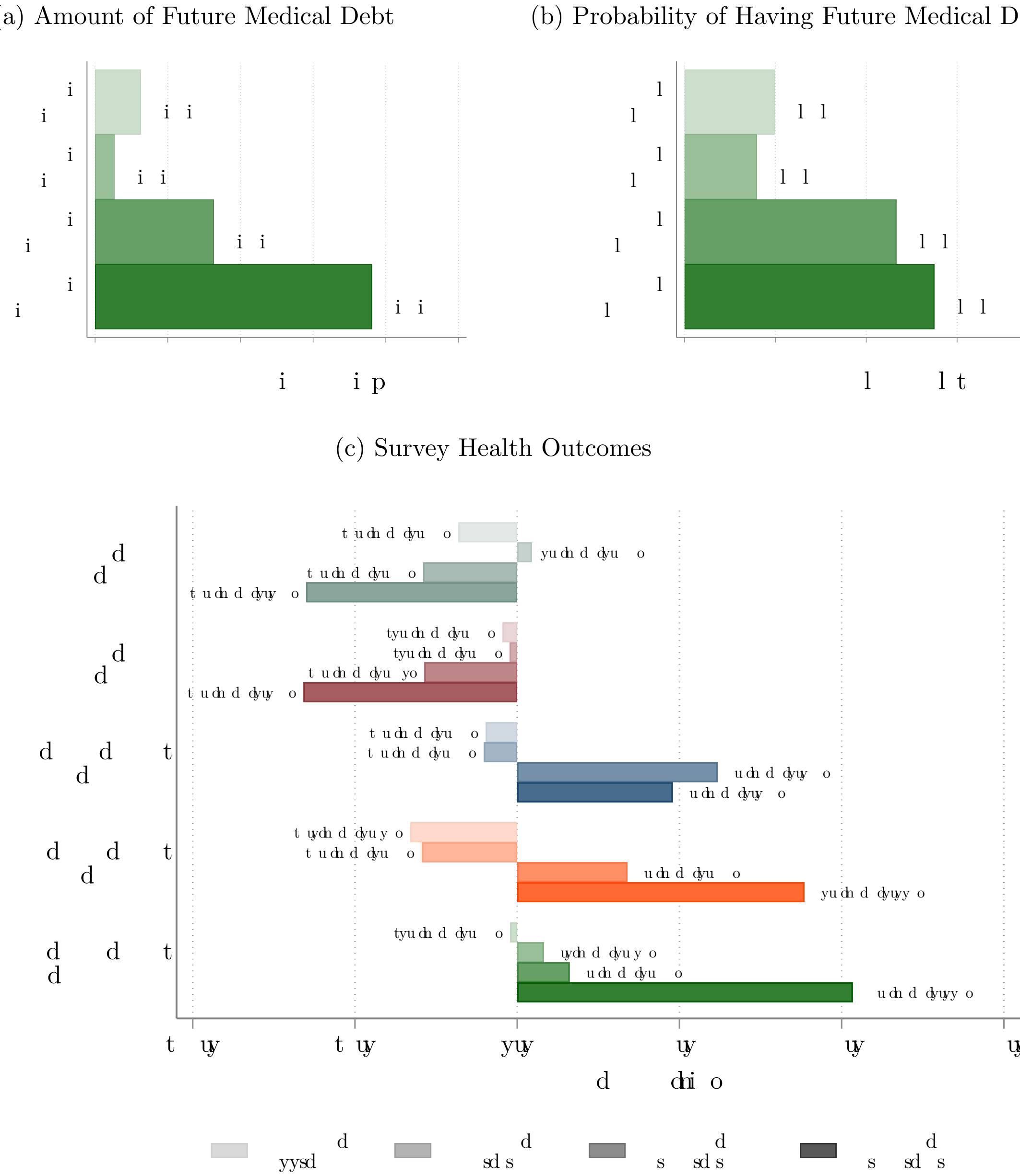
Notes: This flow chart illustrates the two debt relief experiments. After the patient receives care, the hospital seeks payment for a period of 8-24 months before selling the debt to a collections agency. Our hospital debt experiment involves purchasing and relieving debt at this stage. Under the status 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. Predicted Effect of Medical Debt Relief on Depression from Expert Survey



Notes: This figure shows box plots of expert predictions for the impact on medical debt relief on our primary outcome of depression, defined as the share of persons with at least moderate depression on the PHQ-8. The sides of the box represent the interquartile range and the line inside the box represents the median. The whiskers extend up to 1.5 × the interquartile range, unless the most outlying observation is less extreme, in which case the whisker is truncated at this point. The green vertical line shows the 9.2 percentage-point reduction in depression from Medicaid coverage estimated in the Oregon Health Insurance Experiment (Baicker et al., 2013), which we provided survey respondents as a benchmark.

Figure 3. Heterogeneous Effects of Debt Relief on Survey Health Outcomes and Future Medical Debt, by Medical Debt Eligible for Relief



(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 survey health outcomes and future medical debt in collections by quartile of medical debt eligible for relief (as measured in the first wave an individual is observed). Panel (A) presents the effect of medical debt relief on the amount of medical debt observed in future waves. Panel (B) presents the effect on the probability of having medical debt observed in future waves. Panel (C) shows the effects on the health outcomes measured in the NORC survey.
Quartiles are created using the medical debt balance in the first wave individuals are observed in. Quartiles are computed separately for the survey sample (Panel (C)) and the hospital debt sample (Panels (A) and (B)). In Panels (A) and (B), the quartile cutoffs are as follows Q1: [$25, $235], Q2: [$235, $620] Q3: [$620, $1,475], and Q4: [$1,475, $60,452]. In Panel (C), the quartiles are Q1: [$500, $794], Q2: [$794, $1,275], Q3: [$1,276, $2,275], Q4: [$2,277, $33,627]. For each outcome, the effects are pictured from lightest (Quartile 1) to darkest (Quartile 4) in descending order. See Appendix Table A19 and Appendix Table A15 for corresponding numerical values.

Appendix

A Methodological Appendix

This section provides additional details on the randomization of debt relief, the script for the awareness subexperiment, the survey protocol, and the construction of the outcome variables.

A.1 Debt Relief Randomization

Debt relief was randomized at the person $\times$ wave level and stratified to ensure balance. Strata were also chosen to satisfy the priorities of RIP Medical Debt's donors. For example, some donors earmarked their contributions for persons living in certain geographic areas. The specific requirements imposed on a debt purchase varied across purchase waves (see Appendix Table A2 for wave-by-wave statistics).

In the hospital debt experiment, persons in each wave were stratified by the amount of debt, state, whether or not the person had health insurance, and a collections score. The collections score is designed to predict a debtor's repayment likelihood, and its inclusion was required by the debt collector to ensure balance along this dimension. The probability of treatment was approximately 20% in most waves, but occasionally varied depending on the amount of donor funds available for debt relief. Within each wave, the probability of treatment was constant across strata.

In the collector debt experiment, persons in each wave were stratified by geographic area (e.g., city, county, or designated market area), debt amount, person's age, and date of service. In Waves 1 and 2, the share of treated individuals in each geographic area depended on the share of debt that a donor aimed to purchase, relative to the amount of debt made available by the debt collector. In Wave 2, the probability of treatment within each stratum was 50%.

A.2 Awareness Subexperiment Outreach Protocol

For the awareness subexperiment, RIP Medical Debt called a randomly selected subset of the treatment group to notify them of their debt relief. The callers were master's students of social welfare and public policy, who were selected, trained, and employed by RIP Medical Debt and the authors. The script is as follows:

[If voice mail] *Hello, my name is [Your Name]. I'm calling from the non-profit charity, RIP Medical Debt. Our charity specializes in forgiving people's medical debt. I'm calling with good news that, thanks to our donors, our charity has forgiven some of [Recipient's Name] medical debt.*

This is a no-strings-attached gift, and you no longer owe this debt. We recently mailed you a letter with information about this gift. If you'd like to learn more about this gift, our charity, please visit our website at RIPmedicaldebt.org, or call us back at (844) 637-3328. Have a wonderful day.

[If pick up] *Hello, may I speak to [Recipient's Name]?*

*[**If you're not sure it's the recipient** (avoid mentioning debt $)]: This is [Your Name], calling from the non-profit charity, RIP Medical Debt. We specialize in forgiving people's medical debt, and I'm calling with good news. Thanks to our donors, our charity has forgiven some of [Recipient's Name] medical debt.*

*Is he/she available to talk? **[IF NO]** When is a good time for me to call back? [Record time]*

***[If you have recipient on phone]** This is [Your Name], calling from the non-profit charity, RIP Medical Debt. We specialize in forgiving people's medical debt. I'm calling with good news. With the support of our donors, our charity has forgiven about [$Round down to nearest $] of your medical bills. This is a no-strings-attached gift from our charity.*

How does all this sound to you? [Pause for reaction. As appropriate, follow "deeper dive script", below. Keep it warm, understanding. Don't forget to ask about the letter.]

By the way, we previously sent you a letter in the mail with information about the specific bills we paid off. Have you received our letter? [Record response]

***[IF NO]** If you'd like, we'd be happy to resend you a letter. Previously we sent it to [Address]. Is that the best address? [Record better address, as needed]*

I won't take much more of your time. If you have any further questions, you can find us online at RIPMedicalDebt.org, or call us at (844) 637-3328. Please understand that this is a no-strings gift. You are under no obligation to do anything more than enjoy a stroke of good luck. Have a wonderful day.

[Deeper dive script]

"Which debt?"

- *We paid off [#bills from [provider(s)], for services that took place on [dates]]*
- *Mention the letter (script above). "We previously sent you a letter in the mail with information about the specific bills we paid off. Have you received our letter?...Another letter sent?"*

"Why?" or "Why me?"

- *Our charity believes medical debt is an unfair burden on families.*
- *With the support of our donors, we work with local hospitals to forgive medical debt that patients owe. You were one of the recipients of this gift.*
- *The debt forgiveness is charitable gift, with no string attached.*
- *Mention the letter (script above) "We previously sent you a letter in the mail with information about the specific bills we paid off. Have you received our letter?...Another letter sent?"*

Allay suspicion and gain credibility:

- *Our charity believes medical debt is an unfair burden on families.*
- *With the support of our donors, we work with local hospitals to pay off medical debt that patients owe. You were one of the recipients of this gift.*
- *Our charity has been featured on local news all over the US.*
- *You can find more information on our website: RIPmedicaldebt.org, and on youtube.*
- *Mention the letter (script above) "We previously sent you a letter in the mail with information about the specific bills we paid off. Have you received our letter?...Another letter sent?"*
- *If needed, share origins story: Founders are former debt collectors who believed people are unfairly burdened by medical debt. They gave up their careers, and founded the charity in 2014.*

"So I don't owe any more debt?"

- *We work with local hospitals to forgive medical bills, but only can only pay off some of the debt that patients owe them.*
- *With the support of our donors, we were able to paid off [# of bills from [provider(s)], for services that took place on [dates]]*
- *Mention letter (script above) "We previously sent you a letter in the mail with information about the specific bills we paid off. Have you received our letter?...Another letter sent?"*

Life Implications

- *The debt forgiveness is charitable gift, with no string attached*
- *A bill collector will never again contact you about this account.*
- *And the item will be removed from your credit report.*
- *And because we are a 501(c)(3) charity, the forgiven debt does not count as income. It's a charitable gift, so there are no tax consequences.*
- *Mention letter (script below)*

A.3 Survey Protocol

This subsection describes the multimodal protocol used to collect survey responses from our hospital debt sample. Before initiating contact with prospective respondents, we gathered updated address, email, and telephone information using address validators from UPS (SmartMailer) and TransUnion (TLOxp). This process yielded updated addresses and phone numbers, and up to five email addresses per respondent. Outreach efforts were rotated across email addresses. We collected our survey responses in two waves, each of which applied the same protocol. Wave 1 lasted for 15 weeks starting in November 2020. Wave 2 lasted for 17 weeks starting in June 2021.

Our outreach protocol proceeded according to three phases. Phase 1 focused on mail and email outreach. In Week 1, we mailed respondents a survey invitation letter (see Appendix Figures A4 and A5 for an example letter). The invitation letter was packaged in a large (6"-by-9") colored envelope and included a $2 pre-paid incentive, a link to the web survey, and a notification of a post-completion $50 incentive. Starting in Week 2, we sent subjects an email reminder with a link to the web survey, and we repeated this email outreach twice per week for the remainder of the 15- or 17-week protocol. In Week 3, we mailed subjects a reminder postcard, and repeated the postcard outreach three more times, each spaced out by two weeks. In Week 4, we mailed a follow-up letter that was similar in content to the initial letter.

Phase 2 of the outreach protocol focused on intensified mail outreach. Subjects received a FedEx envelope containing a $5 pre-paid incentive, a description of the survey, and a physical copy of the survey instrument that respondents could complete and return via mail. See Appendix F for the full mailed survey instrument. After receiving the mailed instrument, subjects continued to receive reminder postcards every two weeks, as well as reminder emails twice per week.

Phase 3 of the protocol entailed direct telephone outreach. Starting in Week 6, trained telephone interviewers contacted subjects using the available phone numbers. This continued through Week 14 in Wave 1 and Week 13 in Wave 2. Respondents were prompted to complete the survey in one of three ways. First, respondents were able to complete the survey verbally with the interviewer. Second, the interviewer requested an updated email address for the respondent for the purpose of sending email reminders and requested consent for text messages that contained a link to the online survey and a reminder. Third, interviewers reminded subjects about the mailed survey instrument and mailed an additional instrument if necessary. If the subject did not complete the survey over the phone, they received twice weekly email reminders and (if consent was given) text reminders using updated email addresses and phone numbers. In Week 11, respondents received a "last chance" letter in the mail, the final component of the the mail outreach. The remaining weeks of the protocol entailed additional electronic and telephone outreach.

A.4 Outcome Variables

Collections Account 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.

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.

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, (3) borrowed from friends/family, (4) used savings, or (5) 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 the impact of the forgiveness is imputed to be none.

B Supplemental Analyses

B.1 Credit Bureau Outcomes: Alternative Specifications

For our analysis of credit bureau data, we estimate two alternative specifications that are designed to increase the precision of our estimates. Because our baseline specification isolates experimental variation, we did not expect and do not find any noticeable impact on the point estimates from this analysis.

The first alternative specification uses the panel structure of the credit bureau data to estimate regression models that control for person fixed effects. As described in Section 3, we observe credit bureau outcomes on a quarterly basis for a period spanning from at least four quarters before to four quarters after treatment assignment. Letting t denote calendar quarters, we estimate regressions of the form:

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

where T_i is an indicator for assignment to the debt-relief treatment, $POST_{it}$ is an indicator for quarters after initial treatment assignment, γ_i are person fixed effects, and $\alpha_{r,t}$ are fixed effects at the level of randomization fully interacted with calendar quarter. This specification does not include a non-interacted T_i indicator because it is collinear with the person fixed effects and does not include a non-interacted $POST_{it}$ indicator because it is collinear with the $\alpha_{r,t}$ fixed effects.

For this analysis, we restrict the sample to quarters $[-4, 4]$ relative to treatment assignment, and we drop quarter 0, which contains both 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 outcomes other than those reflecting access to credit (i.e., indicator for having a credit score, level of credit score, and credit card limits), we also drop quarters $[1, 3]$, such that treatment effects are estimated by comparing outcomes 12 months after treatment to pre-treatment levels. We cluster the standard errors at the person level.

The second alternative specification maintains the non-panel structure of the baseline regression model and saturates the specification with controls defined prior to treatment assignment. Using the collection account data, which is observed prior to treatment assignment, we control for gender, insurance status, quartiles of age, state, quartiles of credit score (Vantage 4.0), and quartiles of collections score used by the debt collector. Using the credit bureau data from the quarter prior to treatment assignment, we control for 25-point credit score bins, an indicator for whether the person has an open mortgage, log non-mortgage debt, log non-medical debts in collections, and log medical debts in collections. We set log variables to zero when the underlying variable is zero, and for each of these outcomes include an indicator that takes a value of one when there is zero underlying balance.

Appendix Tables A5 and A6 present the results from the person fixed effects and saturated regressions, respectively, laid out in the exact same manner as the baseline results in Table 3. As expected, the point estimates are virtually indistinguishable from the baseline estimates, and the standard errors are somewhat smaller.

B.2 Heterogeneous Treatment Effects

To estimate heterogeneous treatment effects, we assign persons to quartiles of a given dimension of heterogeneity (e.g., medical debt balance, beneficiary age) and estimate regression models where we fully interact both the treatment indicator and the wave fixed effects with indicators for these quartiles. Specifically, letting h index these quartiles, we estimate regressions of the form:

$$y_i = \beta_h T_i + \alpha_{r,h} + \varepsilon_i \tag{4}$$

where T_i is an indicator for assignment to the debt-relief treatment, and the h subscripts indicate that the treatment effects β_h and wave fixed effects $\alpha_{r,h}$ vary by h.

We pre-specified exploring heterogeneity by the amount of debt relieved, age of debt relieved, age of person, and amount of debt in collections prior to the intervention (measured using credit bureau data). This analysis is shown in Appendix Tables A7 through A14 for the credit bureau outcomes, Appendix Tables A15 through A18 for the collections account outcomes, and Appendix Tables A19 through A22 for the survey outcomes. Notable results are discussed in the main body of the paper.

B.3 Credit Bureau Reporting Subsample

As previously discussed, the debt collector had largely ceased reporting accounts to the credit bureaus prior to the start of our experiment, in line with broader industry trends. However, for a subset of accounts in the collector debt experiment, the debt collector continued to report accounts to the credit bureaus, allowing us to estimate the impact of debt relief on credit bureau outcomes relative to a counterfactual when medical debt is reported.

We identify the set of accounts with reporting by matching the dollar amount of medical debt in the collections account data to the dollar amount of medical debt in the credit bureau data in the four quarters prior to the intervention. Recall that the collector debt experiment focused on accounts that had been in collections for 7.0 years on average; if there was reporting, the accounts would be observable in the credit bureau data prior to treatment assignment. We keep all matches that are within \$0.50 of each other, which we found performs better than an exact match because it registers a match if one of the values is rounded to the nearest integer.

Appendix Figure A6 illustrates the match by plotting the percentage of control and treatment group persons with matched medical debt in the collector account data separately for wave 1 and wave 2 of the collections account data. Panel A shows that prior to the intervention, we are able to match 6.8% percent of wave 1, with very similar match rates for treatment and control. After the intervention, match rates drop sharply for the treatment group. For the control group, match rates drop three quarters later in Q1 2019 when the debt collector ceases reporting. The debt collector placed debt with several third parties that take responsibility for outreach and collections, and the partial reporting is consistent with reporting by some third parties and not others. Match rates are slightly positive after reporting ceases due to a small number of false positives.

Panel B of Appendix Figure A6 shows similar patterns for wave 2. The main exception is that Q1 2019, when the debt collector ceases reporting, is only one quarter after the intervention. Because there is counterfactual reporting for only a single quarter and some effects phase in over time, we focus our analysis on wave 1, although we show results for wave 2 for completeness.

As discussed in Section 5.2, in wave 1 of the credit reporting subsample, debt relief raised credit scores by an economically small 3.6 points (p-value of 0.016), with a 13.4-point effect (p-value of 0.010) for persons with no other debt in collections (Table 4). We also estimate a credit limit effect that phased in over time, with an average \$342 increase (p-value of 0.010) for the full subsample and a less-precise \$922 increase (p-value of 0.069) for persons with no over debt in collections.

We also conduct an analogous analysis for accounts in wave 2, in which control group accounts were reported for only one quarter after intervention. Appendix Table A25 presents results for wave 2. The results are qualitatively similar and smaller in magnitude for credit scores, with an average 1.6-point effect (p-value of 0.020) and a 6-point effect (p-value of 0.005) for persons with no other debt in collections. There are no detectable effects on credit limits, which is unsurprising given the finding from wave 1 that these effects phase in over time.

Appendix Table A26 shows effects on the remaining main credit bureau outcomes using the wave 1 sample. Appendix Tables A27 and A28 examine effects separately by whether the person had another debt in collections, also using wave 1. This analysis naturally shows an effect on the measures of debt in collections (combined medical and non-medical). Aside from these outcomes, the analysis shows no economically meaningful effects of medical debt relief on measures of borrowing or financial distress. None of the estimates are statistically distinguishable from zero after multiple inference adjustment. Our interpretation is that, aside from the effects on credit limits, the credit score effects are too small to generate noticeable increases in borrowing or changes in financial distress (which are of theoretically ambiguous direction).

B.4 Collections Account Outcomes: Alternative Specification

For our analysis of collection account data, we estimate an alternative regression model that saturates the specification with the same controls defined prior to treatment assignment that we included in the analysis of credit bureau data (described above). Since our baseline specification isolates experimental variation, we expected similar point estimates and smaller standard errors from this analysis. The results, shown in Appendix Table A24, are aligned with our expectations.

B.5 Survey Outcomes: Internal Validity

A potential threat to the internal validity of our findings is differential survey response rates between the treatment and control groups. To the extent that survey response rates are correlated with outcomes, different response rates can bias the estimated treatment effects. In our survey sample, treated persons were 1.3 percentage points (p-value of 0.056) more likely to respond to the survey relative to a control response rate of 18.7%. While this difference is not statistically significant at conventional levels, we conduct two exercises to probe the sensitivity of our findings to any potential bias from this source.

The first method for addressing potential bias from differential response rates is to saturate the regression with controls for observable characteristics (defined prior to treatment assignment). To the extent that observable characteristics differ between the treatment and control groups and are correlated with outcomes, controlling for them will mitigate differential response rate bias. We saturate the regression model with the same collection account and credit bureau control variables used in our other analyses (described above).

The second method for addressing potential bias is to re-estimate our regression model for a subsample of respondents with identical response rates. Recall that we conducted a multimodal survey in which subjects were contacted numerous times over a 15- to 17-week period with invitations to complete the survey. We can correct for the 1.3 percentage-point higher response rate for treatment group persons by dropping the 112 treatment group persons who were latest to respond, such that the response rates in both the treatment and control groups are identical. Under the assumption that there is a latent type (i.e., response propensity) that has a stable ordering in the treatment and control groups and is correlated with the speed to respond to our survey, this method will obtain balance between the treatment and control groups on this latent type and eliminate any bias from a correlation between the outcomes and this latent factor.

Appendix Table A29 presents the results from these exercises. Columns 1 to 3 present the baseline estimates for comparison, columns 4 to 5 present estimates from our saturated specification, and columns 6 to 7 present the estimates that achieve balance by dropping last responders. Across both of these exercises, the point estimates are virtually identical to baseline point estimates, with no statistically or economically notable differences for any of the outcome variables. Taken together, the identical results when controlling for observables and equalizing response rates on latent response propensity provide confidence in the internal validity of our findings.

B.6 Survey Outcomes: External Validity

A natural question is the extent to which our estimated treatment effects apply to persons who did not respond to our survey. We conduct two exercises to probe the external validity of our findings.

The first method is to compare treatment effects across persons who are more versus less likely to respond to the survey based on observable characteristics. We estimate a logistic model of survey response on persons who were assigned to the control group. The regressors are an indicator that the individual had insurance at the date of service, an indicator that the individual is male, their state of residence, 25-point credit score bins (Vantage 4.0), 25-point collections score bins, an indicator for whether the individual has an open mortgage, log of non-mortgage balances, log of non-medical collections balances, and log of medical collections balances. Time-varying outcomes are measured in the quarter before intervention. The logistic model is reasonably predictive of survey response, with an AUC of 0.617 and a response rate of 24.9% for persons with an above-median predicted response propensity versus 14.6% for those with a below-median predicted response propensity from the model.

The second method is to compare treatment effects across persons with different speeds to respond to our survey. Dutz et al. (2021) randomize monetary incentives for survey completion and use the resulting variation in response rates to estimate a model of survey response which they use to correct for nonresponse bias. In piloting, we were unable to generate meaningful differences in response rates using reasonable monetary incentives. Instead, our survey design, with numerous contacts over a 15- to 17-week period, lends itself to using speed to respond to the survey as a proxy for unobserved response propensity. We split the sample by above- versus below-median response time and estimate treatment effects separately for each group. If there is a latent response propensity, and speed to respond is correlated with this latent factor, then comparing treatment effects by response time will be informative about heterogeneity by response propensity and thus the external validity of our findings.

Columns 1 through 4 of Appendix Table A30 present results for our main survey outcomes, separately for those with below- versus above-median response propensity based on the logistic response model. Columns 5 through 8 present results split by response time. For almost all outcomes, treatment effects are statistically indistinguishable between the below- versus above-median groups, although our standard errors do not allow us to reject moderate differences for most outcomes. More broadly, neither group exhibits meaningful improvements in our primary outcomes, and the differences do not exhibit a consistent pattern (i.e., the direction of the difference varies by outcome).

While we are inherently limited in our ability to probe the external validity of our findings, our examination of heterogeneity based on observable characteristics and a proxy for unobservable response propensity does not reveal any evidence that our main conclusion (i.e., that debt relief has no meaningful benefits) is not externally valid.

C Appendix Tables

Table A1. Use of Public Funds for Medical Debt Relief as of March 11, 2024

	Date Announced (1)	Source of Funds (2)	Funds ($, Millions) (3)	Debt Relief ($, Millions) (4)	Source (5)
Panel A. Programs Passed					
Cook County, IL	July, 2022	ARPA	12.0	1,000	Cook County Government
Akron, OH	March, 2023	ARPA	0.5	50	Public News Service
Cleveland, OH	April, 2023	ARPA	1.9	181	City of Cleveland
New Orleans, LA	May, 2023	ARPA	1.3	130	City of New Orleans
Pittsburgh, PA	August, 2023	ARPA	1.0	115	City of Pittsburgh
Toledo, OH	October, 2023	ARPA & Local Taxes	1.6	240	Mercy Health
Oakland County, MI	October, 2023	ARPA	2.0	200	Oakland County
Columbus, OH	October, 2023	ARPA	0.5	335	City of Columbus
Kalamazoo, MI	November, 2023	ARPA	0.5	89	MLive Media Group
St. Paul, MI	December, 2023	ARPA	1.0	100	MPR News
New York, NY	January, 2024	Local Taxes	18.0	2,000	NYC.gov
Connecticut	February, 2024	ARPA	6.5	650	Becker's Hospital Review
Arizona	March, 2024	ARPA	30.0	2,000	Office of the Governor, AZ
Wayne County, MI	March, 2024	ARPA	7.0	700	Michigan Advance
Ingham County, MI	March, 2024	State & Local Taxes	0.5	50	Ingham County
Total Passed			**84.3**	**7,840**	
Panel B. Programs Under Consideration					
New Jersey	March, 2023	ARPA	10.0	1,000	NJ Spotlight News
Los Angeles, CA	October, 2023	Local Taxes	24.0	2,000	County of Los Angeles
Pennsylvania	February, 2024	State Taxes	4.0	400	Spotlight PA
Orange County, FL	February, 2024	ARPA	4.5	450	Orlando Weekly
Chicago, IL	February, 2024	ARPA	10.0	1,000	Illinois.gov
Total Under Consideration			**52.5**	**4,850**	
Total			**136.8**	**12,690**	

Notes: Table presents a list of city, county, and state governments that passed or are currently considering publicly-funded medical debt relief programs as of March 11, 2024. All governments have partnered with RIP or indicated their intention to do so, except Columbus, OH, which is working directly with local hospitals, and Los Angeles, CA, which has not yet detailed its proposal. In column (2), "ARPA" denotes federal funds from the American Rescue Plan Act. Column (3) reports the targeted amount of medical debt forgiven at the program's announcement. Additional governments including Washington, DC and Milwaukee, WI have proposed similar programs that did not move forward.

Table A2. Summary Statistics by Purchase Wave

	Date of Purchase	Sample Size			Medical Debt ($, Thousands)		
		Total	Treatment	Control	Total	Treatment	Control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Hospital Debt Experiment							
Wave 1	August 30, 2018	3,083	617	2,466	3,031	605	2,426
Wave 2	October 25, 2018	3,451	690	2,761	4,182	843	3,339
Wave 3	November 21, 2018	2,760	546	2,214	2,347	471	1,875
Wave 4	December 28, 2018	1,848	372	1,476	1,387	289	1,097
Wave 5	January 31, 2019	1,654	341	1,313	1,162	232	930
Wave 6	September 17, 2019	6,467	865	5,602	10,369	1,426	8,943
Wave 7	October 21, 2019	4,346	934	3,412	6,054	1,309	4,745
Wave 8	October 21, 2019	3,473	1,056	2,417	4,283	1,268	3,015
Wave 9	December 20, 2019	3,986	1,003	2,983	4,662	1,170	3,491
Wave 10	January 10, 2020	6,187	587	5,600	9,892	988	8,905
Wave 11	February 18, 2020	4,359	734	3,625	6,021	967	5,054
Wave 12	March 20, 2020	4,382	774	3,608	6,100	1,057	5,042
Wave 13	April 27, 2020	4,051	984	3,067	4,779	1,188	3,592
Wave 14	May 29, 2020	4,874	1,223	3,651	7,989	2,003	5,986
Wave 15	July 8, 2020	3,759	958	2,801	4,942	1,240	3,702
Wave 16	August 13, 2020	3,869	968	2,901	5,269	1,349	3,920
Wave 17	September 21, 2020	10,066	1,088	8,978	15,118	1,649	13,469
Wave 18	October 13, 2020	3,258	637	2,621	4,960	938	4,022
Panel B. Collector Debt Experiment							
Wave 1	March 9, 2018	42,181	21,599	20,582	87,118	44,079	43,039
Wave 2	October 15, 2018	94,857	47,425	47,432	209,824	105,525	104,299
Panel C. Aggregate							
Hospital Debt Experiment		75,873	14,377	61,496	102,546	18,992	83,554
Collector Debt Experiment		137,038	69,024	68,014	296,942	149,605	147,338

Notes: Table presents summary statistics across each wave of debt relief in both the hospital debt and collector debt experiments. Column (1) reports the date of each wave of relief. Columns (2), (3), and (4) report the total, treated, and control count, respectively. Columns (5), (6), and (7) report the total face value of medical debt in total, relieved, and not relieved, respectively. Panel A reports statistics for each wave in the hospital debt experiment, panel B reports the same for each wave in the debt collector experiment, and panel C reports aggregate statistics across all waves in each experiment.

Table A3. Characteristics of Survey Respondents versus Nationally Representative Samples

	Respondents	NHIS	CPS ASEC
	(1)	(2)	(3)
Panel A. Observations			
Total	2,888	31,997	132,868
Panel B. Gender (%)			
Male	38.8	48.3	48.4
Female	61.2	51.7	51.6
Panel C. Age (%)			
18-24	10.3	11.8	11.6
25-34	25.7	17.9	18.0
35-44	24.0	16.3	16.4
45-54	19.7	16.1	16.2
55-64	13.5	16.8	16.7
65+	6.7	21.1	21.1
Panel D. Race and Ethnicity (%)			
Black	18.8	11.8	11.8
Non-hispanic white	43.7	63.2	63.1
Hispanic	30.9	16.5	16.4
Other	6.6	8.5	8.6
Panel E. Household Income (%)			
$0 to $30,000	48.2	23.0	17.5
$30,001 to $55,000	23.2	21.2	18.3
$55,001 to $80,000	15.9	19.2	16.7
$80,001 to $100,000	12.1	11.0	10.4
$100,001+	0.7	25.6	37.1

Notes: Table presents pre-treatment summary statistics for the NORC survey sample in column (1) versus two nationally representative samples, the 2019 National Health Interview Survey (NHIS) in column (2) and the 2019 Current Population Survey Annual Social and Economic Supplement (CPS ASEC) in column (3) (Center for Disease Control and Prevention, 2021; United States Census Bureau, 2021). CPS ASEC respondents under age 18 are dropped. NHIS and CPS ASEC statistics use population weights representative of the US adult population.
+: Main pre-specified outcome.

Table A4. Effects of Debt Relief on Credit Bureau Outcomes (Other Pre-Registered Outcomes)

	Hospital Debt Experiment			Collector Debt Experiment		
	Control Mean	Treatment Effect	p-value	Control Mean	Treatment Effect	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Distress						
At least one debt past due (%)	48.04	0.10	[0.842]	39.94	0.56	[0.037]
		(0.49)			(0.27)	
At least one debt in default (%)	44.83	0.18	[0.712]	37.21	0.43	[0.102]
		(0.49)			(0.26)	
Panel B. Debt in Collections						
At least one debt in collections (%)	79.82	-0.66	[0.097]	70.79	0.15	[0.544]
		(0.40)			(0.24)	
Panel C. Borrowing						
Count of loans	4.72	-0.06	[0.277]	3.96	0.06	[0.045]
		(0.05)			(0.03)	
Total loan balance ($)	34,420	-964	[0.132]	28,052	435	[0.175]
		(640)			(320)	
At least one credit card (%)	34.33	-0.03	[0.948]	30.50	0.20	[0.428]
		(0.47)			(0.25)	
Count of mortgages	0.11	-0.00	[0.410]	0.09	-0.00	[0.843]
		(0.00)			(0.00)	
Mortgage balances ($)	15,105	-453	[0.368]	12,267	223	[0.372]
		(503)			(249)	
Panel D. Sample Size						
Observations†	55,653	12,998		64,947	65,968	

Notes: Table presents credit bureau outcomes as outlined in Equation 3. Columns (1) and (4) report the control means for the hospital debt and collector debt experiments, respectively. Control means are averaged across post-treatment quarters. Columns (2) and (5) report the treatment effects, with standard errors below in parentheses. Columns (3) and (6) report unadjusted and multiple-inference-adjusted p-values in square and curly brackets, respectively. Multiple inference adjustment is performed using the Westfall and Young (1993) method by domain. †: Sample sizes for control and treatment groups reported in the control mean and treatment effect columns, respectively.