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 (now Undue 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. Our experiments focused on downstream medical debt that had been sold to debt collectors, and one of our experiments straddled an industry-wide pullback in the reporting of medical debt to the credit bureaus, allowing us to estimate the effects of debt relief with and without counterfactual reporting. We track outcomes using credit reports, collections account data, and a multimodal survey. There are three sets of results. First, we find a modest improvement in credit access when there is counterfactual credit reporting, but no impact on credit report outcomes when there is not. Second, we estimate that debt relief causes a moderate but statistically significant reduction in payments of existing medical bills. Third, we find no effects on survey measures of mental and physical health, healthcare utilization, and financial wellness. Taken together, our results indicate that the strong correlations documented in prior research do not translate into causal effects for downstream medical debt relief.

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I Introduction

Two in five Americans have medical debt, broadly defined, and nearly one in five 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.[1]

Concerned by this burden, policymakers are increasingly turning to medical debt relief, primarily focusing on medical debt held by debt collectors. As of August 30, 2024, 22 state or local governments have passed programs to fund roughly $10.2 billion in medical debt relief, and two more are considering programs that would raise this total to over $14.6 billion (see Appendix Table A1). Nearly all of these governments are working with our research partner, RIP Medical Debt (now Undue Medical Debt).[2] Private donors are also generously supporting debt relief and RIP Medical Debt has used private funding to buy and relieve more than $10 billion in medical debt to date.

Proponents of medical debt relief point to a literature that documents strong associations between medical debt and negative financial and health outcomes (e.g., Kale and Carroll 2016; Zafar 2016; Banegas et al. 2019; Priscilla, Ali, and Sanmartin 2020; Himmelstein et al. 2022; Han et al. 2024), and suggest a number of mechanisms through which medical debt relief could have salutary causal effects. On the financial side, medical debt relief could benefit households directly through reduced payments or indirectly by improving credit scores. On the health side, debt relief could improve mental health by alleviating the stress of debt collections and the psychological burden of debt, and improve healthcare access if patients were avoiding healthcare out of fear of accruing more debt.

Yet, there are reasons for caution. By the time medical debt is sent to collections, it can be purchased for pennies on the dollar. Although proponents of medical debt relief tout the low cost as a feature – the $14.6 billion of planned relief would cost taxpayers around $150 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

[1] All of the above statistics are from the same nationally representative 2,375-person KFF (2022) survey, which defines medical debt broadly as any debt arising from a health event, including debt that is past-due, unpaid, being paid over time, owed to friends or family, charged to a credit card, or owed to a collection agency.

[2] After we released the study, RIP Medical Debt changed their name to Undue Medical Debt. We refer to them as RIP Medical Debt throughout the paper because that was the name used during the intervention.

debt relieved. There are also reasons to be particularly cautious about interpreting the association between medical debt and adverse outcomes as reflecting a causal effect of medical debt. Medical debt arises from a health shock (generating the medical bill) and limited financial resources (preventing payment), so the correlation may reflect causal forces that operate in the opposite direction.

This paper studies the impact of medical debt relief on financial outcomes, health, and healthcare 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 forgive medical debt and has been involved in most high-profile medical debt relief to date. Our interventions focused on downstream medical debt (i.e., after the initial billing process associated with a health event) that had been or was about to be sent to collections by the healthcare provider. One of our experiments straddled an industry-wide pullback in the reporting medical debt to the credit bureaus, allowing us to separately examine the effects of credit reporting. 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 medical debt and was designed to test the effects of relieving debt before the patient is exposed to third-party debt 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 five times RIP's typical purchase price).[3] The treatment group consisted of 14,377 people who received $19 million in face-value debt relief, for an average of $1,321 per person. Recipients were sent two letters notifying them that their debt had been canceled. The 61,496-person control group did not receive debt relief, and the debt collector pursued repayment following their normal protocol. We expected larger benefits from this experiment and focused on this sample for survey outreach.

The second *collector debt* experiment targeted older debt, which reflects the majority of the debt relief provided by RIP to date and allows for large-scale 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

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

treatment group consisted of 69,024 people and $150 million in face-value debt relief, for an average of $2,167 per person. Recipients were notified of the debt relief once by letter. The 68,014-person control group retained their debt and continued to be pursued for repayment by the debt collector.

We study a third *credit reporting* sub-experiment, which allows us to estimate the impact of debt relief when accounts would have been counterfactually reported to the credit bureaus. Partway into our collector debt experiment, the debt collector ceased reporting medical debt to the credit bureaus, reflecting a broader industry trend driven initially by heightened regulatory enforcement (CFPB 2023) and later by a credit bureau agreement to cease reporting certain types of debt. We isolate a subset of accounts with credit bureau reporting prior to treatment assignment and use this subset to estimate the effects of medical debt relief when accounts would otherwise have been reported.

Together, the experiments provide a rich picture of the effects of medical debt relief. The hospital and collector debt experiments were designed to shed light on the cost-effectiveness of relief at different stages in the collection process. The credit reporting sub-experiment, when combined with the collector debt experiment, allows us to examine the effects of debt relief with and without counterfactual credit bureau reporting.

We study the impact of debt relief using three data sources. First, we linked the hospital and collector debt experiments with fully depersonalized quarterly credit-report data from TransUnion, which allows us to track financial distress, credit access, and credit utilization from at least one year before to one year after treatment assignment. Second, for the hospital debt sample, we tracked accounts sent to collections post-intervention, allowing us to analyze the "spillover effects" of debt relief on the repayment of other medical bills. 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, resulting 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 our

hospital and collector debt experiments, which do not isolate accounts with counterfactual credit reporting. 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 preregistered subgroup.

In the credit reporting sub-experiment, where control group accounts are reported, we find that debt relief immediately raises credit scores by an economically small 3.4 points on average (p-value of 0.021), with a 13.8-point increase (p-value of 0.008) for persons with no other debt in collections. This immediate increase is accompanied by a gradual increase in credit limits of $340 on average ($p$-value of 0.010; 15.3% of the post-reporting control mean of $2,231), with larger effects for persons with no other debts in collections. We detect no effects on measures of borrowing or financial distress.

We find that medical debt relief causes a statistically significant and economically meaningful *reduction* in the payment of existing medical bills. Using the hospital debt experiment, we find that debt relief increases the probability of having another unpaid bill sent to collections by 1.1 percentage points, or 6.6% of the control mean of 16.2%. The effect is almost entirely explained by lower repayment of existing medical bills, and is consistent with treated persons raising their expectations of future debt relief, targeting a certain level of indebtedness (as in Dobkin et al. [2018]), or experiencing confusion about the extent of relief. The findings reject 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 do not detect any average effects of medical debt relief on mental and physical health, healthcare utilization, and financial wellness as measured in our multimodal survey of the hospital debt experiment sample. We estimate a statistically insignificant 3.2 percentage-point average worsening of depression (p-value of 0.097), our primary survey outcome (as measured by the 8-question Patient Health Questionnaire or PHQ-8). A 95% confidence interval rules out an improvement of more than 0.6 percentage points, well below the 7.0 percentage-point improvement predicted by the median respondent in our expert survey. We estimate similarly statistically insignificant average effects on other measures of mental and physical well-being, including anxiety (as measured by the 7-question Generalized Anxiety Disorder screen or GAD-7),

stress, general health, and subjective well-being. We do not detect any meaningful effects on healthcare utilization or financial wellness.

Our paper contributes to the literature on the financial burden of the US healthcare system. 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; Hu et al. 2018; Brevoort, Grodzicki, and Hackmann 2020; Miller et al. 2021; Adams et al. 2022; Bornstein and Indarte 2023; Goldsmith-Pinkham, Pinkovskiy, and Wallace 2023). For instance, 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 reduced depression by 9 percentage points among a population of low-income uninsured adults.

More broadly, our paper also contributes to research on the impact of non-medical debt relief programs. Debt relief through bankruptcy (Dobbie and Song 2015; Dobbie, Goldsmith-Pinkham, and Yang 2017) and student loan forgiveness (Di Maggio, Kalda, and Yao 2024) have been shown to cause substantial improvements in financial well-being and earnings. In the context of mortgage modifications, Ganong and Noel (2020) find that reducing liquidity requirements is more important than principal reductions in reducing borrower default and increasing consumption. Dinerstein, Yannelis, and Chen (2024) similarly find that additional liquidity from student loan forbearance increases demand for credit cards and auto loans. In contrast, 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. To the extent that medical debt relief does not generate immediate liquidity gains or changes to expected repayment, the null results we estimate are consistent with these findings.

Our results echo the dispiriting evidence on debt relief in the development economics literature, where Kanz (2016) finds debt relief has no effect on consumption, savings, or investment but does reduce concern over future default, and Karlan, Mullainathan, and Roth (2019) find that most recipients of debt relief return to indebtedness within six weeks. Our study population has high rates of financial distress and the medical debt relief we provide may be too marginal to improve their overall well-being.

Most directly, our findings reject a causal interpretation of the correlations between medical debt and

negative health and financial outcomes documented by the prior correlational literature, which motivated financial outlays by private donors and local governments and broader policy proposals (e.g., Zhang 2022). We find a modest improvement in credit access in the earlier period when there is counterfactual reporting, but no impact in the current (non-reporting) environment. We estimate a moderate reduction in repayments of existing bills, and no effects on mental or physical health, healthcare utilization, or financial wellness. Simply put, for the downstream medical debt relief we study, most of the correlations documented in the literature do not translate into causal effects.

Our results do not imply that others forms of medical debt relief will be ineffective. Debt relief could have impacts on outcomes we did not measure and pairing debt relief with other interventions could generate meaningful benefits. Most promising, given the prior literature, is upstream medical debt relief, which occurs closer to the precipitating medical event. Further research will be needed to explore such potential benefits.

The rest of the paper is structured as follows. Section II provides background on our setting and Section III describes the experiment. We describe our data sources in Section IV and our empirical framework in Section V. Results are presented in Section VI and discussed in Section VII. Section VIII concludes.

II Background

II.A Setting

Our study focuses on medical debt in collections (hereafter "medical debt"), defined as medical bills that had been or were about to be sent to debt collectors by the healthcare provider. Alternative definitions of medical debt may be appropriate in different contexts. For example, KFF (2022) defines medical debt expansively, including unpaid medical bills sent to collections and bills owed to a hospital or other medical provider, which the patient may be paying off over time, and medical bills paid with credit cards or other loans. Our study focuses on medical debt in collections because most medical debt relief efforts target this category of debt and because it is not possible to comprehensively observe some of the types of debt that are included in more expansive definitions. For instance, when an unpaid bill is held by the hospital, it is difficult to determine whether the bill will ultimately be resolved by the provider (e.g., because of a billing mistake or charity care), paid by health insurance or a third party (e.g., Worker's Compensation), or owed

by the patient as medical debt.

In recent years, the prevalence of medical debt has been shaped by divergent trends in insurance coverage and insurance generosity. Due to coverage expansions under the Affordable Care Act, the uninsured rate fell from 16% in 2010 to 8% in 2022 (Peterson Center on Healthcare and KFF 2024). At the same time, insured patients are increasingly exposed to large out-of-pocket costs (e.g., the share of insured workers with a deductible over $1,000 rose from 12% to 50% between 2010 and 2022). On net, annual out-of-pocket spending per capita grew to $1,425 in 2022, a 14% real increase since 2010 (Peterson Center on Healthcare and KFF 2024).

Hospital financial assistance programs are designed to assist patients who are unable to pay their out-of-pocket bills, but in practice provide limited protection against medical indebtedness. Non-profit hospitals are required to offer low-income patients financial assistance in exchange for their tax-exempt status, and for-profit and government hospitals also commonly offer such programs (Adams et al. 2022).[4] The IRS rarely penalizes hospitals for non-compliance with its regulations (Lucas-Judy 2023) and investigative reporting has documented significant, widespread barriers to the take-up of hospital financial assistance programs.[5]

To recover payments for medical bills not covered by insurance or financial assistance, providers first conduct direct patient outreach for 8 to 24 months. Many providers sell unpaid debts to a third-party debt collector in bulk at a discounted price. Debt collectors, who are typically residual claimants on recoveries, pursue repayment by 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., Presser 2019; Cooper, Han, and Mahoney 2021).[6] In addition, debt collectors can sell medical debt on the secondary market to other debt collection agencies, who can continue collection attempts.

Collectors' ability to enforce and collect medical bills is limited by state and federal consumer protections. The Fair Debt Collection Practices Act (FDCPA) prohibits collectors from using deceptive or abusive

[4]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 to collections, denying care, or suing patients (IRS 2024). Nineteen states impose more generous requirements for hospital financial assistance.

[5]For instance, many nonprofit hospitals do not pre-qualify low-income patients for charity care, often pursuing payments before checking eligibility, and do not mention financial assistance when discussing payment options (Matthews, Fuller, and Evans 2022; Silver-Greenberg and Thomas 2022).

[6]An investigation of 528 hospital collection practices found more than half engage in legal actions such as lawsuits or wage garnishment and nearly one in five will further deny non-emergency medical care (Levey 2022).

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 collectors to bring lawsuits to about six years on average, although there is substantial variation across states (Locklear 2023). Some states either prohibit hospitals from selling debt to collectors or require hospitals to oversee collectors. A few states prohibit wage garnishment or home liens for medical bills entirely, while a larger number of states prohibit wage garnishment for certain populations or in cases of demonstrated financial need.[7]

II.B Credit Bureau Reporting

Historically, debt collectors voluntarily reported medical debt to the credit bureaus to increase the salience of the debt and to serve as a repayment incentive, since collectors can offer to cease reporting in exchange for repayment. The Fair Credit Reporting Act (FCRA) governs the treatment of medical debt on consumer credit reports and requires that credit bureaus accurately report information and investigate any disputed information. Starting in 2018, concerns about data integrity and the associated legal risks from inaccurate reporting contributed to a substantial drop in the reporting of medical debt information by debt collectors (CFPB 2023). In a series of changes phased in between July 2022 and April 2023, the credit bureaus voluntarily agreed to exclude medical debt from credit reports if the debt is less than $500, less than one year old, or has already been paid (CFPB 2023).

These changes led to substantial reductions in the prevalence of medical debt visible on credit reports. Subsequent analysis of credit bureau data shows that the share of credit reports with medical debt in collections declined from 16% in August 2018 to 12% in August 2022, as debt collectors curtailed reporting, then fell further to 5% in August 2023, after credit bureaus ceased to include the aforementioned categories of medical debt (Blavin, Braga, and Karpman 2023). Note that the reduction in medical debt on credit reports does not imply any corresponding decrease in underlying medical debt or collections activity. Even before these changes, an analysis of bankruptcy filings by Argyle et al. (2021) found significant amounts of medical debt that were not reported to the credit bureaus.

[7] See Kona and Raimugia (2023) for a comprehensive list of policies by state. 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. Cheng, Severino, and Townsend (2021) analyze consumers facing civil collection lawsuits and find that 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.

II.C Consequences of Medical Debt

The prior literature in medicine and health services research documents a strong association between medical debt and negative financial and health outcomes (e.g., Kale and Carroll 2016; Zafar 2016; Banegas et al. 2019; Priscilla, Ali, and Sanmartin 2020; Himmelstein et al. 2022; Han et al. 2024). In Appendix Section B, we conducted a comprehensive analysis of the cross-sectional correlation between medical debt and financial and health outcomes in nationally representative datasets, analyzing a national credit bureau sample from TransUnion and publicly available survey data from the Panel Survey of Income Dynamics (PSID), the Survey of Income and Program Participation (SIPP), and the Medical Expenditure Panel Survey (MEPS).

Consistent with the literature, we find that persons with medical debt (compared to those without) have more than twice as much debt past due, are about three times as likely to have trouble paying their mortgage or rent, and are almost twice as likely to be depressed (see Appendix Tables A4-A6 and Appendix Figures A2-A4 for more). The survey measures of medical debt, finances, and health are not identical to the measures in our study, so they do not allow for an apples-to-apples comparison with our experimental estimates. Still, they provide a useful benchmark for our causal estimates.

Proponents of medical debt relief suggest a number of potential mechanisms through which medical debt could causally affect financial and non-financial outcomes:

First, by removing the debt from household balance sheets, medical debt relief generates a direct financial benefit. We do not observe the debt collector's recovery rates. In a competitive market, the recovery rate is the sum of the price of medical debt and the collections costs. The low price of medical debt (5.5 cents per dollar in the hospital debt experiment, less than a penny per dollar in the collector debt experiment) suggests that the direct financial benefits are typically modest, assuming the recovery costs are not excessive. However, respondents to our survey, who expect to pay 54% of their outstanding medical debt and think it is fair to pay 37%, may experience consequences from medical debt if the perceived obligation to repay distorts other financial decisions.[8]

Second, the prospect of debt collectors placing liens on assets and garnishing wages may impose a

[8]In a national survey of 2,663 US adults, Perry Undem (2023) find that 60% of respondents blame companies and institutions rather than the individual for medical debt, while this figure is less than 40% for student debt, mortgage debt, auto debt, and credit card debt. This disparity suggests that respondents believe medical debt is less fair to pay than other forms of debt, and that they may expect to pay less of it.

financial burden on households. Litigation is a realistic concern for patients owing medical debt: three in five hospitals regularly file medical debt lawsuits against patients (Levey 2022) and 1.5 in every 1,000 Wisconsin residents face lawsuits for medical debts (Cooper, Han, and Mahoney 2021). Our debt collector identifies a small subset of accounts to target for litigation, although we cannot observe lawsuits in the credit bureau data due to a 2017 settlement between the credit bureaus and the FDIC.[9]

Third, medical debt has historically impacted finances through its presence on credit reports. For instance, Brevoort, Grodzicki, and Hackmann (2020) document a sharp drop in credit scores following the arrival of the first medical debt in collections. The removal of medical debt from credit reports is cited as a primary benefit of debt relief, given the visibility of these debts to lenders, landlords, and employers. Debt relief could improve finances by increasing credit scores, thereby reducing the cost of borrowing, improving access to credit, and making it easier to secure stable housing and employment. Recent decisions to remove most medical debt from credit reports mean that debt relief will need to be targeted to the remaining persons with credit reporting for this channel to be relevant.

Fourth, medical debt may impose a non-financial burden through the stress of the collections process and the psychological burden of debt. In announcing medical debt relief initiatives, politicians highlight how the stress of medical debt harms physical and mental health.[10] In surveys, media reports, and government complaints, persons with medical debt cite the stress and hassle of frequent phone calls and other contacts by debt collectors (CFPB 2017; Bryan 2018; PBS News Desk 2022; US Senate Committee on Banking, Housing, and Urban Affairs 2022).

Fifth, patients with medical debt may forgo seeking follow-up healthcare. Two in five indebted patients report delaying care to avoid accruing further debt, and one in five report avoiding the provider where they owe money due to concerns about being refused care (Perry Undem 2023). As mentioned above, Adams et al. (2022) find that patients who received hospital financial assistance substantially increased the use of high-value healthcare, including for treatment-sensitive conditions like diabetes, suggesting that medical debt is an impediment to healthcare access.

[9]The settlement required the removal of tax liens and civil judgments if the information is incomplete (FDIC 2018). In practice, we observe an almost complete removal of this information from our credit bureau data after this settlement came into effect.

[10]Cook County Board President Toni Preckwinkle highlighted that "Medical debt is a social determinant of health that can undermine people's physical and mental well-being by creating stress and preventing necessary follow-up visits" (Cook County Government 2023). Similarly, New Orleans' Mayor LaToya Cantrell stated that "medical debt...is directly tied to poor health outcomes, as individuals often do not seek further care if they are saddled with huge bills they can't pay" (City of New Orleans Office of the Mayor 2023).

However, correlation does not imply causation. Medical debt originates from a health shock and limited financial resources, so the documented correlations may reflect the persistent effects of precipitating health events and existing financial distress. Health shocks also cause persistent lost earnings (Dobkin et al. 2018), which could drive both medical debt and other negative financial and health outcomes.

II.C.1 Expert Survey

We conducted an expert survey of academics, non-profit staff, hospital revenue cycle management and debt collection practitioners, and policymakers to assess prevailing beliefs on the impact of our hospital debt experiment. The survey was administered between April 19, 2022 and May 22, 2022, after we completed the intervention but before we released any results. We presented experts with a description of the intervention, including the face value of debt relief, the purchase price of the debt, and the notification letter. We asked experts to predict the impact of debt relief on several outcomes, providing them with the control group mean and, as a benchmark, the effects of Medicaid coverage estimated in the Oregon Health Insurance Experiment (Baicker et al. 2013). Experts were not explicitly told the dates of our experiment or that there was limited credit reporting, and we did not assess whether they were aware of recent trends in the industry.[11] See Appendix Section A.5 for details and Appendix Section F for the survey instrument.

Experts predicted meaningful reductions in rates of depression, borrowing, and cutting back on spending, as well as increased healthcare access. Appendix Figure A5 shows the box plots of these expert predictions. Notably, the median expert predicted a 7.0 percentage-point reduction in depression (8.0 percentage points if we weigh by confidence in their answers) and a 10.2 percentage-point reduction in borrowing (13.7 percentage points when weighted by confidence). Taken together, 75.6% of respondents predict 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 A6.

[11] Debt collectors began pulling back on reporting medical debt in 2018, well before we fielded our expert survey. This change was not widely reported but may have been known to well-informed experts. We do not know whether our experts were aware of the trend, and we encourage the reader to consider this context when comparing the expert predictions against our estimated treatment effects.

III 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 forgive medical debt. We separately examine instances in which RIP used private funds to randomize the forgiveness 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 hospitals attempted to collect. We also examine (iii) a *credit reporting* sub-experiment in which a subset of accounts in the collector debt experiment were reported to the credit bureaus, and where debt relief eliminated the reporting of these debts. The experiments were conducted between March 2018 and October 2020. See Figure I for a flowchart summarizing these experiments.

III.A Hospital Debt Experiment

The hospital debt stems from medical care provided by a large for-profit hospital system, with facilities spread over 8 states in the South and Mountain West.[12] 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 unpaid medical bills, which it sells to a third-party debt collector.

RIP coordinated with this debt collector to purchase and relieve a random subset of the medical debt accounts at the juncture when the hospital system would typically sell them to collections. There was no scope for selection of accounts into the sample by the hospital providing the accounts (e.g., by selling the least collectible accounts) given they were unaware of the intervention. 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 predicting the likelihood of repayment. 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

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

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 A7 for wave-by-wave statistics.

For treated individuals, RIP purchased the debt at a price of 5.5 cents per dollar and forgave it, eliminating any obligation to pay the debt. Approximately three weeks later, RIP mailed treated individuals a letter informing them of the debt relief (see Appendix Figure A7 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 remains 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.[13] Our conversations with executives at other debt collection agencies 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

[13] 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.

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 the initial assignment and using the initial assignment as an instrument for cumulative assignment in a two-stage least-squares design.

Column 1 of Table I 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).

III.A.1 Awareness Sub-Experiment

The impact of debt relief can operate through reduced collections activity and knowledge of the charitable intervention. To increase awareness and salience of the intervention, RIP conducted additional phone outreach to a randomly selected subset of treated individuals in waves 6 through 14 of the hospital debt experiment. Of the 8,160 treated individuals in these waves, they randomly selected 4,232 (or 52%) to receive phone outreach. The outreach protocol consisted of a scripted message acquainting subjects with RIP and informing them of their debt relief. 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. For more details on the sub-experiment, see Appendix Section A.2.

III.B Collector Debt Experiment

The collector debt was purchased from the collections agency and consisted of debt that had been subject to collections efforts for a number of years. The sample was geographically diverse, covering 45 states spread across the South (52%), West (21%), Northeast (18%), and Midwest (9%). Compared to the hospital debt, the collector debt is more representative of RIP's existing medical debt relief programs to date.

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 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 individuals treated depended on donor funds available for purchase. Because donors typically prioritized debt relief in particular locations, the share varied by stratum. See Appendix Section A.1 for more information and Appendix Table A7 for statistics.

Medical bills that remain unpaid for several years despite ongoing collections efforts are less likely to be paid than bills that are newly 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 forgiven and were notified by letter (Appendix Figure A7). Control persons continued to be subject to normal collection efforts. 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 I provides summary statistics on the collector debt sample in the initial wave in which persons appear, and Appendix Table A7 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).

III.C Credit Reporting Sub-Experiment

The debt collector historically reported medical debt information to the credit bureaus and intended to report for the accounts in our experiments. However, like many others in the industry, they became concerned about liability risk and largely ceased reporting before we implemented our first intervention in March 2018. The exception was a subset of accounts in the collector debt experiment for which the debt collector stopped reporting in 2019 Q1, three quarters after the first wave of the experiment and one quarter after the second wave. For this subset, treatment group accounts remained on credit reports until the intervention date and control group accounts remained on credit reports until 2019 Q1.

We identify accounts that were reported by matching the dollar amounts of medical debt in the collections account data to those in the credit bureau tradeline-level data in the four quarters prior to the intervention (see Appendix Section C.3 for more details). We match 2,761 accounts (6.8%) in wave 1 of the

collector debt experiment data, with virtually identical match rates for treatment and control.[14] 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 A8).[15]

IV Data

IV.A Collections Account Data

The debt collector provided us with a dataset that includes 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 account in each wave of the hospital and collector debt experiments. For persons in the hospital debt sample, we also observe health insurance status.

We measure the effect on future medical debt accrual in the hospital debt sample using the wave structure of this experiment.[16] We construct a "future medical debt" measure, defined as the sum of medical debt appearing in the collections account data in waves subsequent to initial treatment assignment (i.e., the first wave in which the debtor appears). Due to the wave structure of the data, this measure incorporates more post-periods for persons who initially appear in earlier waves. We also construct separate future medical debt measures by whether the associated medical service occurred before or after initial treatment assignment, which allows us to distinguish whether future debt accrual reflects changes in debt repayment versus changes in healthcare utilization.

IV.B Credit Bureau Data

We linked persons in the hospital and collector debt experiments to credit bureau records from TransUnion, one of the three nationwide credit reporting agencies. The linking was conducted by TransUnion and returned as a fully depersonalized dataset with no means to link back to the original sample. We purchased

[14] As noted above, 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.

[15] We obtain a similar match rate for wave 2 of the collections account data, but control group reporting only continues for a single quarter after the intervention (see Panel B of Appendix Figure A8). Therefore, we focus on wave 1 here but show results for wave 2 in the appendix for completeness.

[16] We cannot measure future debt accrual for participants in the collector debt experiment since the two waves are not drawn from a consistent underlying population.

quarterly credit records for our study sample for the period spanning March 2017 to December 2021, which captures at least four quarters before to four quarters after treatment assignment. We also purchased a nationally representative random sample of credit reports 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 excluded these persons from the analysis of credit bureau data.[17]

TransUnion collects information from lenders, debt collectors, and public records on consumer debts. We analyze credit report outcomes across six preregistered domains including financial distress, debt in collections, bankruptcy, access to credit, and unsecured and secured borrowing. Appendix Section A.4 provides more detail on the construction of these variables.

IV.C 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 instrument is provided in Appendix Section G.

The surveys were sent to a subset of the hospital debt sample who entered the study after September 2019 (waves 6 through 18) and owed at least $500 in medical bills to the collection 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 outreach to 1,000 and 3,000 subjects), where we tested survey modalities and experimentally varied the amount of upfront

[17] Of the 13,189 unmatched people in the combined study sample, 7,222 are in the hospital debt sample (9.5% of that sample) and 5,967 are in the collector debt sample (4.4% of that 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%).

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 A9 and A10) was sent in a colored 6"-by-9" envelope and included a $2 upfront 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 upfront 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 allowed 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 measured depression and anxiety using the clinically validated PHQ-8 and GAD-7 screens, and the PHQ-8 was our primary preregistered outcome.

On average, respondents completed the survey 13 months after treatment assignment (interquartile range of 10 to 17 months) – and the commencement of control group debt collection activities – and 29 months after receiving the care that incurred the debt (interquartile range of 24 to 34 months). 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 in Deshpande and Dizon-Ross (2023) likely reflect a broader trend of declining survey response rates over time.[18] They likely also reflect differences in study populations (e.g., individuals with unpaid medical bills may more likely to ignore mail and phone calls and be less likely to respond to surveys). In Section VI, we conduct several checks of external validity and find no evidence of differential effects for persons less likely to respond to the survey.

IV.D Summary Statistics

Columns 1 through 4 of Table I present summary statistics for the hospital and collector debt samples, the survey outreach subsample, and survey respondents. Columns 5 and 6 present statistics for a nationally representative sample from TransUnion, unconditionally and conditional on having medical debt in collections. 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 A8 compares the 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, Pinkovskiy, and Wallace 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 of Table I) falls at the 20th percentile of the national distribution (column 5) but only the 60th percentile of the national distribution of persons with medical debt (column 6). 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 study samples also have roughly an order of magnitude more medical debt in collections and total debt in collections than the nationally representative sample. Our study samples have less total debt (including mortgage, credit card,

[18] 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 (2018) 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).

and auto-loan balances, as well as other tradelines), primarily because they are less likely to have a mortgage.

As mentioned above, survey outreach was restricted to persons in the hospital debt sample that owed more than $500 in medical debt to the collection agency (and who were first observed in waves 6 to 18). Accordingly, the survey outreach sample (column 2) has worse credit bureau outcomes than 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 are more negatively selected than those with younger medical debt.

V Empirical Framework

V.A Baseline Regression Specification

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

$$y_{i,t} = \beta T_{i,t} + \gamma_i + \alpha_{r(i),t} + \varepsilon_{i,t} \tag{1}$$

where i indexes persons, t indexes calendar quarter, $T_{i,t}$ is an indicator that turns on for persons randomly assigned to debt relief in the post-treatment period (and is otherwise zero), and γ_i are person fixed effects. Since the probability of treatment assignment is not uniform across waves and strata, we additionally control for randomization-group-by-time-period fixed effects, $\alpha_{r(i),t}$, to isolate the experimental variation.[19] We restrict the sample to include four pre-treatment quarters and the fourth quarter after treatment assignment so the coefficient of interest, β, captures the average effect of debt relief on the outcome four quarters after

[19] For the hospital debt experiment analysis of collections account and credit bureau data, we control for fixed effects for the full interaction of the 18 experimental waves and time period. 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, survey wave, and time period. For the collector debt experiment, the probability of treatment varies across waves and strata, so we control for the full interaction of experiment wave, stratum, and time period.

treatment.[20] We cluster the standard errors at the person level.

For analysis of the collections account and survey outcomes, where we have a single outcome period, we estimate specifications that exclude individual fixed effects and include a randomization group fixed effect, $\alpha_{r(i)}$, without the time-period interaction. Across all of our datasets, we estimate alternative specifications where we control for demographics and baseline financial characteristics from the collections account and credit bureau data (and exclude individual fixed effects). These specifications are outlined in Appendix Section C.1.

For our analysis of secondary outcomes, we adjust our p-values 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 preregistered baseline characteristics: the amount of medical debt eligible for relief, the age of the person, the age of the debt (the time span between the medical service and the intervention), and the amount of other debt in collections on the person's 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, $T_{i,t}$, and randomization group fixed effects, $\alpha_{r(i),t}$.[21] This analysis is discussed in detail in Appendix Section C.2.

To analyze the awareness sub-experiment, we replace the single treatment indicator in Equation 1 with separate indicators for treated persons who were randomly assigned to be called and those who were not.

V.B Credit Reporting Specification

For a subset of accounts in the collector debt experiment, we observed credit reporting prior to the intervention for the treatment and control group, and for three quarters post-intervention for control group accounts that were not relieved (see Section III.C for details). Using this sample, we estimate the impact of debt relief

[20] We exclude quarters [0,3] relative to treatment to avoid averaging pre-treatment periods for some of the outcome variables which include 12-month lookback periods.

[21] 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.

using regressions of the form:

$$y_{i,t} = \beta_1 T_{i,t}^{reporting} + \beta_2 T_{i,t}^{no_reporting} + \gamma_i + \alpha_{r(i),t} + \varepsilon_{i,t} \tag{2}$$

where $T_{i,t}^{reporting}$ and $T_{i,t}^{no_reporting}$ are separate treatment indicators for periods when medical debt is visible or no longer visible on control group credit reports, respectively. As above, γ_i are person fixed effects, and $\alpha_{r(i),t}$ are fixed effects at the level of randomization group fully interacted with calendar quarter.

To examine time trends in the credit reporting effects, we separately estimate event study specifications, which allow the treatment effect to vary flexibly by quarter but are otherwise identical to the above specification:

$$y_{i,t} = \sum_{t \neq -1} \beta_t T_i + \gamma_i + \alpha_{r(i),t} + \varepsilon_{i,t} \tag{3}$$

For our credit reporting analysis, we restrict the sample to the period that spans from four quarters before the intervention (2017 Q2) to four quarters after the cessation of control group reporting (2019 Q4).

V.C Balance

Tables II and III examine the balance of baseline characteristics for each of our experimental 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. In Table II, we analyze balance on demographics and collections account outcomes in the first wave we observe the person. Table III shows the balance on the credit bureau outcomes measured in the quarter before treatment assignment. We additionally show the balance on covariates within each heterogeneity split in Appendix Tables A9 to A32.

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 both survey outreach and response rates. There is no evidence of differential selection into response based on observable characteristics, with none of the p-values below 0.05 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), it motivates sensitivity analysis of whether differential selection into survey response might affect our results. We discuss this analysis after presenting our main results.

VI Results

VI.A Credit Bureau Outcomes: Hospital and Collector Debt Experiments

Table IV reports the average effects of debt relief on credit bureau outcomes for our hospital and collector debt experiments, estimated via our baseline specification (Equation 1). For brevity, we exclude several pre-specified outcomes from the table; these are shown in Appendix Table A33.

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 analysis. Debt relief has a statistically insignificant -0.01 average effect on the number of accounts past due (relative to a control mean of 1.20 accounts). In cross-sectional analysis, we show that persons with no medical debt have 0.5 fewer accounts past due than those with medical debt (Appendix Table A6). We can reject can effects outside of a -0.04 to 0.02 range with a 95% confidence interval.

Table IV 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 ($\geq$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 balances outside of -$42 to $47 (relative to a mean of $1,481) and rejects an effect on auto loan balances outside of -$235 to $148 (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 hospital debt sample, we find null effects for this sample.

We estimate similarly precise null effects in two alternative specifications that exclude the person fixed effects, one of which simply excludes them and the other of which replaces them with a rich set of controls (see Appendix Tables A34 and A35). We examine potential heterogeneity by quartiles of medical debt eligible for relief, age of debt, age of the person, and amount of debt in collections on credit reports, and find no meaningful effects for the subgroups defined by these variables (see Appendix Tables A36 through A43). Appendix Sections C.1 and C.2 provide a comprehensive discussion of the sensitivity and heterogeneity analyses.

VI.B Credit Bureau Outcomes: Credit Reporting Sub-experiment

Table V shows the effect of debt relief for our credit reporting sub-experiment, where control group accounts were reported for three quarters following treatment assignment but then removed. The effects are estimated separately for the three quarters with control group reporting and the four subsequent quarters with no reporting (Equation 2). Figure II shows corresponding event study figures that allow the treatment effect to vary flexibly over time (Equation 3).

Panel A of Table V shows effects for the full credit reporting subsample. During the period with control group reporting, medical debt relief reduces the count of medical debts in collections by 1.00 (p-value $<$ 0.001) and the dollar amount of medical debt in collections by \$1,215 (p-value $<$ 0.001; 29% of control mean of \$4,147). When there is no longer control group reporting, the effects return to zero. These patterns are clearly seen in the event study plots shown in Figure II.

When there is counterfactual reporting, debt relief *reduces* the share of persons with a credit score by 4.2 percentage points (p-value $<$ 0.001) relative to a control mean of 98.1% (Table V, Panel A). Medical debt relief raises credit scores by an economically small 3.4 points (p-value of 0.021) among persons in the balanced panel who have credit scores in all periods. Both effects drop to zero once control group reporting ends, as shown in Figure II. These results indicate that the reporting of medical debt allows the credit bureaus to "score" persons who would otherwise have too little information for their scoring algorithms and modestly raises credit scores for those who would always be scored.

The on-impact increase in credit scores is accompanied by a gradual increase in credit limits, illustrated in Panel E of Figure II. During the three quarters with control group reporting, credit limits increase by $155 ($p$-value of 0.038; 8% of the control mean of $1,953). This increase grows to $340 ($p$-value of 0.010; 15.3% of the post-reporting control mean of $2,231) in the four subsequent quarters. The event study coefficients show that the effect grows approximately linearly over the five quarters post-intervention before leveling out, consistent with control group credit limits starting to grow three quarters after the intervention, when the debt collector ceased control group reporting.

The effects on having a credit score and credit scores conditional on having one are concentrated among those with no other debt in collections. Panels B and C of Table V show results split by whether the person had other debt in collections in the quarter prior to the intervention. During the period with control group reporting, the improvement in credit scores is 13.8 points (p-value of 0.008) for those with no other debt in collections versus 1.2 points (p-value of 0.440) for those with other debt in collections. For persons with no other debt in collections, the subsequent increase in credit limits is a fairly large, but somewhat imprecise, $922 (23% of the control mean, p-value of 0.070). For persons with other debt in collections, this effect is a smaller $177 increase (10% of the control mean, p-value of 0.123). The event study plots shown in Appendix Figures A11 and A12 illustrate this heterogeneity.

In Appendix Section C.3, and corresponding Appendix Tables A44 through A46, we examine the impact of debt relief on the other main credit bureau outcomes, including measures of borrowing and financial distress, for the credit reporting subsample. We do not find any effect on these outcomes, either for the full subsample or when we split the sample by whether the person had other debt in collections.

Taken together, these results indicate that medical debt relief has a modest positive impact on credit access in the presence of reporting to the credit bureaus, with larger effects for those with no other debt in collections. However, these 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 2023).

VI.C Collections Account Outcomes

Table VI reports the effect of medical debt relief on the accrual of future medical debt at the hospital system our debt collector partnered with for our experiment. We conduct this analysis using the first 17 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 preregister this analysis.

Panel A shows that medical debt relief caused a $15 increase in the amount of debt sent to collections (7.2% of the control mean of $208) and a 1.1 percentage-point increase in the probability of having an unpaid medical bill sent to collections (6.6% of the control mean of 16.2%). Both outcomes are statistically significant at the 5% level.

The increase in future medical debt could result from reduced payments for services already received or from greater utilization of future medical care. Panel B shows that the vast majority of the increased debt accumulation is associated with pre-relief medical services (which can only result from a change in repayment behavior). Panel C shows statistically insignificant increases in future medical debt associated with post-relief medical services (which reflect a combination of changes in healthcare use and repayment). Since the control means are small, we cannot rule out meaningful proportional effects on medical debt associated with post-relief services. The results imply that reduced payment of existing bills is responsible for the increase in debt sent to collections that we observe, and we cannot rule in or rule out effects on healthcare utilization.

Appendix Table A48 reports effects on future medical debt by quartile of medical debt eligible for relief. The effects generally increase with the amount of eligible medical debt, both in levels and in proportion to the control group mean. Medical debt relief increases future debt accrual by $36 (13.0% of the $280 control mean) for those in the top quartile of baseline collector debt versus $5 (3.7% of the $147 control mean) for those in the bottom quartile. As in the baseline analysis, the effects are almost entirely driven by pre-relief medical services.

In Appendix Sections C.1 and C.2, we examine the sensitivity of our findings to controlling for baseline characteristics and testing for heterogeneity by the age of the debt, the age of the debtor, and baseline debt in collections reported to the credit bureaus (see corresponding Appendix Tables A49 through A52). The

results are robust to controls, and none of the heterogeneity analyses yield notable results.

The reduced payment of existing medical bills is consistent with an expectations mechanism where beneficiaries reduce payments because they anticipate greater future debt relief. This effect is also consistent with a confusion mechanism where patients incorrectly believe the debt relief applied to non-relieved bills. Such confusion seems plausible, as a patient needed to check the account number and date of service on their debt relief letter (see Appendix Figure A7) to determine which bills were relieved. Alternatively, or in addition, this effect could arise if patients target a certain level of indebtedness, as modeled in Dobkin et al. (2018). In this framework, patients whose debt is relieved have more "room" in their debt budgets and reduce their repayment of existing bills. Each of these mechanisms is consistent with the heterogeneous effects we document.

VI.D Survey Outcomes

Table VII 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 cross-sectional analysis of the 2022 MEPS, persons without medical debt have an 8.9 percentage-point lower rate of depression (as measured by the PHQ-2) than persons with medical debt (see Appendix Section B). 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 VII shows no detectable effect on depression. 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.

The effects of debt relief on related mental health, subjective well-being and general health mirror those for depression. The second and third rows of Panel A of Table VII 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 measure, 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. Panels B and C of Table VII 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").

We do not detect meaningful impacts on healthcare utilization (Table VII, 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 a 95% confidence interval.

We find no systematic evidence of impacts on financial distress (Table VII, 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 C.5 and C.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 A53 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 A54 indicates that neither exercise provides any evidence to suggest that our main findings are not externally valid. As another test, Appendix Tables A55 and A56 compare the credit bureau effects for the hospital debt experiment sample to those for the survey outreach and survey respondent samples, respectively. We find similar treatment effects across these groups.

We pre-specified heterogeneity analyses by medical debt eligible for relief, age of debt, age of the debtor, and amount of debt in collections on their credit report. Shown in Appendix Tables A57 through A60, we estimate null effects for each heterogeneity split except the effects on mental health outcomes for the top quartile of medical debt eligible for relief. For this quartile, 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%, and similar patterns for anxiety, stress, subjective well-being, and general health.

VI.D.1 Awareness

The impact of medical debt relief can be thought of as operating through two channels: (i) the removal of medical debt, which eliminates any associated collections activity, credit reporting, and debt repayment; and (ii) the knowledge of the charitable intervention.

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 so, how much medical debt was relieved. The questions did not mention RIP to avoid priming survey respondents. Appendix Table A61 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 three times more debt forgiven than the control group.

To explore the role that awareness and salience of the intervention play in mediating the treatment effects, we randomly selected a subset of treated persons in the hospital debt experiment to receive telephone calls in addition to the notification letters (described in Section III). Appendix Table A61 indicates that persons assigned to follow-up calls were 18.0 percentage points (p-value < 0.001) more likely than control persons to report receiving debt forgiveness. Appendix Table A62 shows no statistically significant differences in treatment effects for those who were assigned to receive phone calls versus those who were not. However, given the incomplete phone call contact rates, we caution against drawing strong conclusions from these results.

VII Discussion

There are three key sets of results: (i) a modest improvement in credit access for persons whose debt would have otherwise been reported to the credit bureaus, but no credit market effects for all others; (ii) a reduction

in payments of other existing medical bills; and (iii) no average effects on survey measures of mental and physical health, healthcare utilization, and financial wellness.

The direction of the credit score effects for persons with counterfactual credit reporting is not surprising, but the magnitude of the score increases and knock-on effects on credit access and financial distress were *a priori* uncertain. The small average effects on both credit scores and credit limits, with larger effects among those with no other medical debt in collections, indicate that medical debt relief can meaningfully improve credit access when targeted at persons with otherwise clean credit reports. However, the null effects on other outcomes suggest that medical debt relief is unlikely to ameliorate other forms of financial distress.

In recent years, medical debt has become significantly less visible on credit reports as debt collectors reduced their reporting and the credit bureaus stopped reporting some debts. The results from the credit reporting sub-experiment speak to the prior regime, while the overall null results are relevant for the current regime with limited reporting. The results from the credit reporting sub-experiment also point to the (partial-equilibrium) effects of the credit bureaus' decision to cease reporting many types of medical debt on credit reports (CFPB 2023).

In theory, medical debt relief could increase or decrease payment of other medical bills. The reduced payments are consistent with an expectations mechanism in which people anticipate additional debt relief in the future, a targeting mechanism in which patients tolerate a certain level of indebtedness (as modeled in Dobkin et al. [2018]), or a confusion mechanism in which recipients of debt relief inaccurately believe that other medical bills were forgiven.[22] The results reject the view that relief could increase payments through an income effect or by leaving additional resources in a mental account for medical bills (a "flypaper" effect Katz [2023]).

The null effects on the survey measures of mental and physical health, healthcare utilization, and financial wellness contrast with the predictions from our expert survey. They also contrast with the expectations of proponents of medical debt relief, who have pointed to the strong associations between medical debt and negative outcomes in the prior literature to support their efforts.

Why are the causal effects we estimate so much smaller than experts and proponents expected? There are several plausible explanations. Medical debt might not impose a substantial burden on the average person

[22] We note that the notice letter (Appendix Figure A7) 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.

targeted by these policies, implying limited benefits from relieving it. The amount of debt relief might be too little relative to recipients' overall financial situation to have a detectable effect. The debt relief may have occurred too late after the precipitating medical event, outside of the window when there is high demand for follow up healthcare and after people have become habituated to the stress of debt collections.

The evidence from Adams et al. (2022) showing that hospital financial assistance yields substantial benefits suggests that medical debt does impose a burden that can be addressed by immediate relief. We designed our hospital debt experiment to relieve debt at the moment it is sent to collections (15 months after the medical service on average), much closer to the time of origination than RIP's historical debt relief activity and the bulk of the publicly funded debt relief proposals. It is possible that an earlier intervention may have been more effective.

As noted in Section VI.D, we found a statistically significant detrimental effect on depression for persons in the fourth quartile of debt eligible for relief. We did not find statistically significant effects on depression for any of the other 15 groups we examined in pre-specified heterogeneity analysis, so this result may be a statistical fluke. Notwithstanding this explanation, the result is reminiscent of Jaroszewicz et al. (2023), who document significant reductions in psychological well-being among recipients of unconditional cash transfers; they propose a mechanism where the cash raises the salience of recipients' financial deprivation without addressing their needs.[23] Alternatively, the increase in depression could be driven by the stigma of receiving charity (Moffitt 1983; Atkinson 1987), particularly given the recipients of debt relief in our experiment did not request assistance. While this result warrants follow-up study, we do not think it should be given undue weight in an assessment of our findings.

VIII Conclusion

Concern about the burden of medical debt has prompted private donors and local governments to spend over a hundred million dollars buying and relieving billions of dollars of medical debt. We analyze two randomized experiments that relieved medical debt with a face value of $169 million across 83,401 people, focusing on downstream medical debt that had been sent to collections by the healthcare provider. We

[23] 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 A36 shows that control group persons in the top quartile of relief-eligible medical debt have $5,636 of debt in collections, on average, versus $2,977 for those in the bottom quartile. Though mostly insignificant, Magnuson et al. (2024) find a similar increase in parental psychological distress in an unconditional-cash-transfer experiment with low-income mothers.

arrive at three key sets of results. First, debt relief has no average effect on financial outcomes but modestly increases credit access for persons whose medical debt would have been counterfactually reported to the credit bureaus. Second, debt relief reduces repayment of existing medical bills. Third, debt relief has no average impact on survey measures of mental and physical health, healthcare utilization, and financial well-being.

Our findings contrast with the expectations of proponents of medical debt relief, who have pointed to the strong associations between medical debt and negative outcomes in the prior literature to support their efforts. They are also add odds with the views of experts with survey and the 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. These 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 causes both medical debt and worse financial and health outcomes.

The disappointing results from this intervention should not detract 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 remains 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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IX Supplementary Material

An Online Appendix for this article can be found at QJE Online (qje.oxfordjournals.org).

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Table I. 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 ($)						
Mean	1,352	2,260	2,105	2,167	.	.
25th percentile	235	815	794	300	.	.
50th percentile	620	1,340	1,276	820	.	.
75th percentile	1,475	2,426	2,276	2,073	.	.
Medical Debt Age (Quarters)						
Mean	5.1	5.1	5.2	28.2	.	.
25th percentile	4.7	4.7	4.7	22.7	.	.
50th percentile	5.0	5.0	5.0	24.3	.	.
75th percentile	5.4	5.4	5.5	28.6	.	.
Panel B. Baseline Characteristics						
Demographics						
Age (years)	43.1	41.3	41.7	46.0	.	.
Male (%)	45.2	46.7	38.8	43.3	.	.
Race and Ethnicity						
Black (%)	.	.	18.8	.	.	.
Non-Hispanic white (%)	.	.	43.7	.	.	.
Hispanic (any race) (%)	.	.	30.9	.	.	.
Credit Bureau Data						
Credit score (never missing)	575.3	569.2	576.2	572.1	693.8	579.1
Medical debt in collections (%)	58.8	60.5	57.6	65.1	17.6	100.0
Medical debt in collections ($)	2,303	2,667	2,233	2,875	276	1,567
Debts 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 summary statistics for the hospital debt and collector debt experiments with nationally representative sample from 2018 Q3 for comparison. 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 full collector debt sample. Columns (5) and (6) report credit bureau outcomes for the nationally representative sample from TransUnion and the subset of this sample with strictly positive medical debt in collections, respectively. Aggregate medical debt is defined as the sum of all medical debt eligible for relief. Credit bureau variables are measured in the quarter prior to treatment assignment.

Table II. Balance on Baseline Demographics and Collections Account 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	-0.01	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	-0.01	5.1	0.003	5.2	-0.01	28.2	-0.03
		[0.298]		[0.919]		[0.887]		[0.330]
Insured (%)	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)††		[0.902]		[0.498]		[0.395]		[0.593]

Notes: Table presents the balance of baseline demographics and medical debt eligible for relief within the hospital debt, collector debt, survey outreach, and survey respondent samples. Odd-numbered columns present the control group means. Even-numbered columns present the difference between the control and treatment group means as outlined in Section V.A. 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 III. Balance on Baseline Credit Bureau 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. Distress								
Number of accounts past due	1.3	-0.001	1.3	-0.02	1.5	-0.1	1.0	0.02
		[0.964]		[0.501]		[0.403]		[0.066]
Number of accounts in default	1.2	0.002	1.2	-0.02	1.4	-0.04	0.9	0.01
		[0.933]		[0.541]		[0.585]		[0.148]
Debt past due ($)	5,623.0	-88.2	5,594.1	55.7	5,967.0	-558.9	5,142.2	87.6
		[0.538]		[0.824]		[0.307]		[0.278]
Balances in default ($)	4,112.2	-124.2	4,281.6	37.6	4,311.5	-274.3	3,924.8	31.1
		[0.208]		[0.836]		[0.469]		[0.608]
Panel B. Debt in Collections								
Number of debts in collections	4.1	0.01	4.4	-0.04	4.1	-0.1	4.3	0.04
		[0.857]		[0.716]		[0.809]		[0.185]
Debts in collections ($)	3,478.4	-18.9	3,959.6	-143.7	3,529.6	-66.6	3,898.3	49.4
		[0.757]		[0.216]		[0.778]		[0.192]
Panel C. Bankruptcy								
Bankruptcy in last 12 months (%)	0.7	0.1	0.6	0.2	0.9	0.5	0.3	-0.003
		[0.488]		[0.277]		[0.232]		[0.933]
Panel D. Access to Credit								
Has credit score (%)	96.4	-0.03	96.4	0.2	97.8	0.2	93.8	0.2
		[0.853]		[0.565]		[0.789]		[0.181]
Credit score (never missing)	575.3	0.2	568.9	0.8	575.4	1.6	572.3	-0.4
		[0.822]		[0.560]		[0.628]		[0.334]
Credit card limit ($)	2,511.8	28.0	2,101.0	201.4	3,019.4	381.1	2,175.1	67.6
		[0.702]		[0.105]		[0.260]		[0.070]
Panel E. Borrowing								
Number of credit cards	0.7	-0.02	0.6	0.02	0.9	0.1	0.7	0.01
		[0.232]		[0.390]		[0.225]		[0.199]
Credit card balance ($)	1,526.4	14.8	1,320.8	97.7	1,857.4	17.1	1,124.6	11.1
		[0.698]		[0.138]		[0.917]		[0.510]
Number of auto loans	0.4	-0.001	0.4	0.01	0.5	-0.02	0.3	0.005
		[0.922]		[0.515]		[0.431]		[0.117]
Auto loan balance ($)	7,903.5	-34.9	7,502.8	327.5	9,109.4	-390.8	5,064.6	71.9
		[0.797]		[0.184]		[0.498]		[0.191]
Panel F. Sample Size								
Observations†	55,653	12,998	9,179	5,060	1,751	1,055	65,030	66,041
F statistic (p-value)††		[0.699]		[0.635]		[0.899]		[0.312]

Notes: Table presents the balance of baseline credit bureau characteristics within the hospital debt, collector debt, survey outreach, and survey respondent samples, as outlined in Section V.A. Odd-numbered columns present the control group means. Even-numbered columns present the difference between the control and treatment group means. 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 IV. 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	[0.374]	1.02	-0.002	[0.838]
		(0.02)	.		(0.01)	.
Number of accounts in default	1.08	-0.02	[0.290]	0.92	-0.001	[0.946]
		(0.01)	{0.553}		(0.01)	{0.995}
Debt past due ($)	4,908	4	[0.973]	4,815	6	[0.930]
		(117)	{0.976}		(68)	{0.995}
Balances in default ($)	3,741	27	[0.716]	3,705	28	[0.570]
		(75)	{0.901}		(50)	{0.879}
Panel B. Debt in Collections						
Number of debts in collections	4.66	-0.02	[0.688]	3.55	-0.02	[0.367]
		(0.04)	{0.674}		(0.02)	{0.557}
Debts in collections ($)	4,119	-32	[0.488]	3,112	-1	[0.963]
		(47)	{0.671}		(28)	{0.962}
Panel C. Bankruptcy						
Bankruptcy in last 12 months (%)	1.30	-0.12	[0.361]	0.65	-0.05	[0.338]
		(0.13)	.		(0.05)	.
Panel D. Access to Credit						
Has credit score (%)	97.22	0.004	[0.981]	90.73	-0.06	[0.640]
		(0.17)	{0.997}		(0.13)	{0.867}
Credit score (never missing)	582.29	0.04	[0.930]	577.60	-0.03	[0.908]
		(0.51)	{0.997}		(0.29)	{0.903}
Credit card limit ($)	2,654	40	[0.263]	2,640	24	[0.231]
		(36)	{0.585}		(20)	{0.532}
Panel E. Borrowing						
Number of credit cards	0.81	0.02	[0.025]	0.78	0.003	[0.551]
		(0.01)	{0.088}		(0.01)	{0.812}
Credit card balance ($)	1,481	2	[0.914]	1,306	24	[0.042]
		(23)	{0.930}		(12)	{0.135}
Number of auto loans	0.39	0.01	[0.203]	0.30	-0.0001	[0.975]
		(0.005)	{0.479}		(0.002)	{0.980}
Auto loan balance ($)	8,020	-43	[0.658]	5,417	-37	[0.367]
		(98)	{0.899}		(41)	{0.733}
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, as estimated in Equation 1. 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 measured in the fourth quarter post-treatment. Standard errors clustered at the person level 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 free step-down resampling method of Westfall and Young (1993) 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 V. 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					
Number of medical debts in collections	4.72	-1.00	[0.000]	-0.22	[0.175]
		(0.10)		(0.16)	
Medical debt in collections ($)	4,147	-1,215	[0.000]	-116	[0.576]
		(145)		(208)	
Has credit score (%)	98.08	-4.18	[0.000]	0.49	[0.634]
		(0.65)		(1.03)	
Credit score (never missing)	570.64	3.39	[0.021]	0.24	[0.902]
		(1.47)		(1.97)	
Credit card limit ($)	1,953	155	[0.038]	340	[0.010]
		(75)		(133)	
Observations†	1,338	1,423			
Panel B. No Other Debt in Collections					
Number of medical debts in collections	1.11	-0.65	[0.000]	-0.04	[0.710]
		(0.09)		(0.10)	
Medical debt in collections ($)	1,027	-1,007	[0.000]	-359	[0.211]
		(284)		(287)	
Has credit score (%)	93.07	-15.15	[0.000]	3.21	[0.415]
		(3.10)		(3.94)	
Credit score (never missing)	609.48	13.82	[0.008]	9.06	[0.162]
		(5.19)		(6.45)	
Credit card limit ($)	3,726	312	[0.287]	922	[0.070]
		(293)		(507)	
Observations†	231	234			
Panel C. Other Debt in Collections					
Number of medical debts in collections	5.57	-1.04	[0.000]	-0.21	[0.277]
		(0.12)		(0.19)	
Medical debt in collections ($)	4,901	-1,239	[0.000]	-22	[0.931]
		(174)		(253)	
Has credit score (%)	99.35	-1.79	[0.000]	0.02	[0.986]
		(0.44)		(0.91)	
Credit score (never missing)	563.99	1.20	[0.440]	-1.27	[0.549]
		(1.56)		(2.11)	
Credit card limit ($)	1,514	116	[0.099]	177	[0.123]
		(70)		(115)	
Observations†	1,077	1,160			

Notes: Table reports the effects of medical debt relief on credit bureau outcomes for the credit reporting subsample of the first wave of the collector debt experiment, before and after medical the debt collector stopped reporting all medical debt in collections to TransUnion (as specified in Equation 2). This analysis includes observations from four quarters before the intervention (2017 Q2) to four quarters after the end of the control group reporting period (2019 Q4). 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 clustered at the person level are in parentheses below the treatment effect estimates.
†: Sample sizes for control and treatment groups reported in columns (1) and (2), respectively.

Table VI. 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 ($)	207.55	15.02	[0.038]
		(7.23)	
At least some debt (%)	16.21	1.07	[0.003]
		(0.36)	
Panel B. Pre-Relief Medical Services			
Amount of debt ($)	189.17	13.65	[0.046]
		(6.85)	
At least some debt (%)	15.27	1.03	[0.003]
		(0.35)	
Panel C. Post-Relief Medical Services			
Amount of debt ($)	20.37	2.22	[0.342]
		(2.34)	
At least some debt (%)	1.82	0.09	[0.505]
		(0.13)	
Panel D. Sample Size			
Observations†	58,875	13,740	

Notes: Table presents the effects of medical debt relief on "future medical debt" as measured by (1) the probability of having medical bills sent to collections after initial treatment assignment and (2) the balances of future medical debt for the hospital debt experiment. Column (1) reports the control means, column (2) reports the treatment effects with robust standard errors reported below in parentheses, and column (3) contains *p*-values in brackets. Panel A presents effects for any future medical debt; Panel B presents effects for future medical debt with a service date prior to the wave of initial treatment assignment; Panel C presents effects for future medical debt with a service date after the wave of initial treatment assignment. Treatment effects are estimated as outlined in Section V.A.

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

Table VII. Effects of Debt Relief on Survey Outcomes

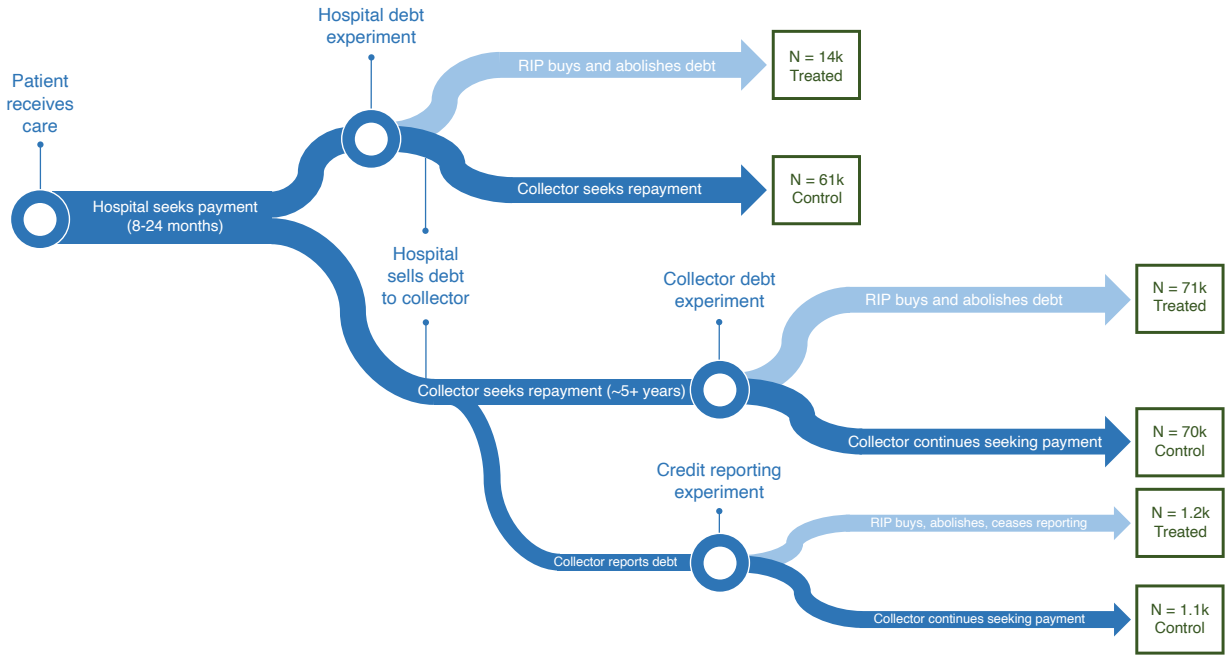
	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)
Panel A. Mental Health			
At least moderate depression (%)$^{+}$	44.95	3.23	[0.097]
		(1.94)	.
At least moderate anxiety (%)	40.07	1.63	[0.395]
		(1.92)	{0.392}
At least sometimes stressed (%)	76.53	2.72	[0.093]
		(1.62)	{0.158}
Panel B. Subjective 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. Healthcare 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	-0.0003	[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 survey respondent 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 robust standard errors reported below 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 6.

+: Primary pre-specified outcome.

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

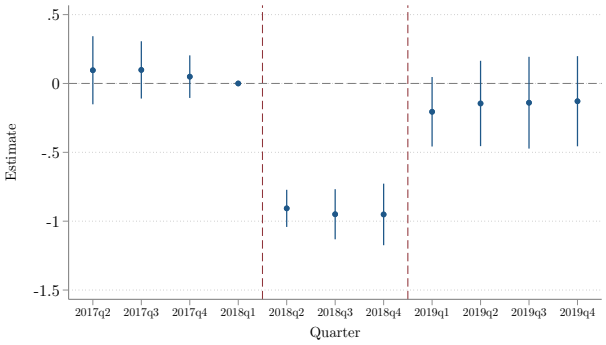
Figure I. Experiment Design



Notes: This flow chart illustrates the two primary debt relief experiments and the credit reporting sub-experiment. After the patient receives care, the hospital seeks payment for a period of 8-24 months before selling the debt to a collection agency. Our hospital debt experiment involves purchasing and relieving debt at this stage. Our collector debt experiment involves purchasing and relieving debt after it has been pursued by collectors for several years. The credit reporting sub-experiment represents a subset of the collector debt experiment, in which control group accounts continued to be reported to credit bureaus for three quarters after the intervention before they were also removed from credit reports.

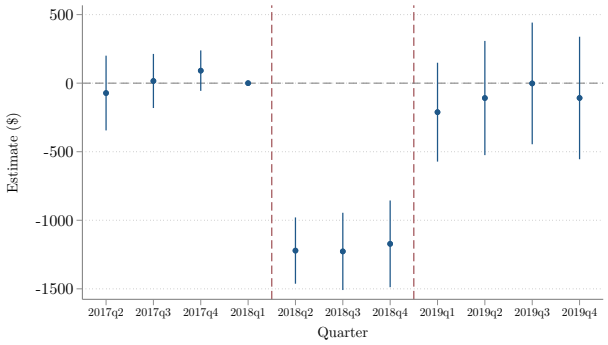
Figure II. Effects of Debt Relief in Credit Reporting Subsample

(a) Number of medical debts in collections



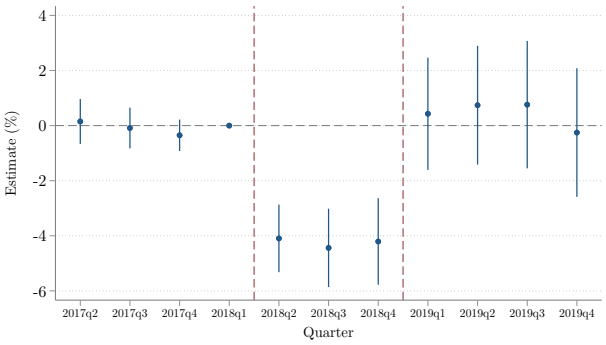
Quarter 2018q2 control mean: 4.95
Treatment Sample Size: 1,423
Control Sample Size: 1,338

(b) Medical debt in collections ($)



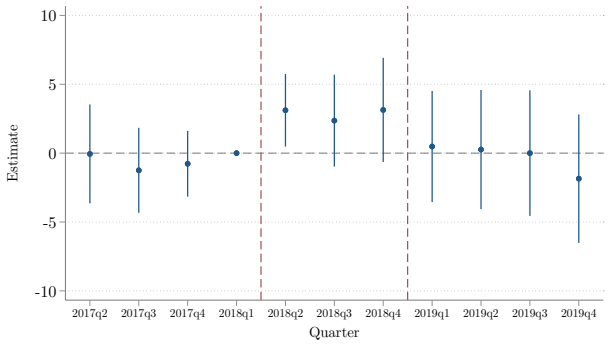
Quarter 2018q2 control mean ($): 4,370
Treatment Sample Size: 1,423
Control Sample Size: 1,338

(c) Has credit score (%)



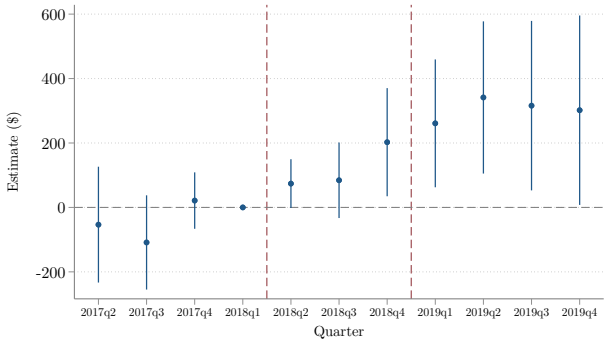
Quarter 2018q2 control mean (%): 98.51
Treatment Sample Size: 1,423
Control Sample Size: 1,338

(d) Credit score (Never Missing)



Quarter 2018q2 control mean: 569.98
Treatment Sample Size: 1,160
Control Sample Size: 1,237

(e) Credit card limit ($)



Quarter 2018q2 control mean ($): 1,898
Treatment Sample Size: 1,423
Control Sample Size: 1,338

Notes: Figure reports an event study of the effect of medical debt relief on credit bureau outcomes for the credit reporting sub-experiment, estimated using Equation 3 and observations from four quarters before the intervention (2017 Q2) to four quarters after the end of the control group reporting period (2019 Q4). The first dashed red line denotes the intervention date and the second dashed red line denotes the end of control group reporting. Blue markers represent point estimates and the blue bars represent 95% confidence intervals. Control means are estimated using data from 2018 Q2.

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

ONLINE APPENDIX

RAYMOND KLUENDER
NEALE MAHONEY
FRANCIS WONG
WESLEY YIN

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A Methodological Appendix

This section provides additional details on the randomization of debt relief, the script for the awareness sub-experiment, 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 A7 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), amount of debt, person's age, and date of service. In waves 1 and 2, the share of individuals treated 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 Sub-Experiment Outreach Protocol

For the awareness sub-experiment, RIP Medical Debt called a randomly selected subset of the hospital debt 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. RIP focused phone outreach on persons in waves 6 to 14 of the hospital debt experiment to overlap with our survey sample.

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: 95% reacted in a positive or neutral manner and 5% expressed disbelief. Only 19% reported having received the initial RIP letter.

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 strings 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 a charitable gift, with no strings 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 USPS (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 A9 and A10 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 G 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 mail outreach. The remaining weeks of the protocol entailed additional electronic and telephone outreach.

A.4 Outcome Variables

A.4.1 Collections Account Data

Within the collections account data, each debtor can have more than one account of medical debt within a wave. To construct individual-level variables, we aggregate the information associated with each account to the individual level. Age, sex, and insurance status are computed as the modal value in the first wave in

which an individual is observed. Medical debt age is the mean debt age weighted by the amount of debt of each account. Individual-level medical debt 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).

A.4.2 Credit Bureau Data

All credit bureau balance and count variables are winsorized at the 99th percentile. Our main pre-specified outcome, the number of accounts past due, is measured as the count of accounts verified in the past 12 months with a non-current Manner of Payment (MOP) code (codes 02, 03, 04, 05, 06, 07, 08, 8A, 8P, 09, 9B, 9P, UR). Number of accounts in default is measured similarly as the count of accounts verified in the past 12 months that are non-current and at least 90 days past due (codes 04, 05, 06, 07, 08, 8A, 8P, 09, 9B, 9P, UR). Balances past due and balances in default are the sums of the balances of the above qualifying accounts winsorized at the 99th percentile. These four variables (number of accounts past due, number of accounts in default, balances past due, and balances in default) are constructed from the tradeline file. All other outcome variables are constructed from the flat file.

Total balances of any type of non-collection trade (auto, credit card, home equity loans, mortgage, non-mortgage, or retail) originating from the flat file are constructed as the total balance of that type verified in the past 12 months. If this variable has an error code denoting that there are no trades of this type, no open trades of this type, or no trades on file for an individual, we code them as having a balance of zero dollars. If this balance has not been verified in the past 12 months or cannot be calculated, then we use the balance of open trades of this type. If none of these criteria are met or the open trade variable is coded as an error code, then this variable is set to zero.

Medical debt in collections is the total balance of third-party collections verified in the past 12 months less the total balance of non-medical third-party collections verified in the past 12 months. Debt in collections is the total balance of third-party collections verified in the past 12 months. Due to variable construction, count versions of these variables are not required to be verified in the past 12 months.

The credit card limit is defined as the total credit line of open credit card trades verified in the past 12 months. This variable is set to zero in the presence of error codes or missing values. Total debt, referred to as total loans in Appendix Table A33, does not have a total balance of all trades of that type defined in the flat file and is constructed as the total balance of open trades.

Bankruptcy in the past 12 months is defined as 1 if the most recent tradeline bankruptcy (excluding medical public records) occurred in the last 12 months or if the most recent public record bankruptcy occurred in the past 12 months.

We use VantageScore 4.0 as a measure of a person's credit score.

A.4.3 Survey Data

The previous section A.3 provides details on the survey protocol.

Within the survey, respondents were asked to complete the eight-item Patient Health Questionnaire (PHQ-8), a clinically validated survey comprised of 8 questions assessing symptoms of depression. Each question asks if a respondent has experienced a specific symptom over the past two weeks and is scored on

a scale of 0 (not at all), 1 (several days), 2 (more than half the days), and 3 (nearly every day). These scores are summed across the 8 questions, with the sum corresponding to the severity of depression symptoms on a scale of 0-4 (no or minimal symptoms), 5-9 (mild symptoms), 10-14 (moderate symptoms), 15-20 (moderately severe symptoms), 20-24 (severe symptoms). Our main pre-specified outcome is defined as a respondent having a PHQ-8 score of 10 or higher, which corresponds to symptoms of at least moderate depression. This is the same cutoff as Baicker et al. (2013).

Likewise, we use the Generalized Anxiety Disorder 7 (GAD-7) to assess symptoms of generalized anxiety disorder (GAD). Each question pertains to the respondent's experience within the last two weeks and is graded on the same scale as the PHQ-8 questions. The sum of the scores corresponds to the severity of anxiety symptoms on a scale of 0-4 (no or minimal anxiety), 5-9 (mild anxiety), 10-14 (moderate anxiety), and 15-21 (severe anxiety). We use the cutoff of 10 or higher to construct the outcome of whether a respondent has at least moderate anxiety.

Stress and happiness were evaluated with one question, each with a general time period of the respondent's level of experiencing either stress or happiness "these days." Stress is evaluated on the scale of experiencing stress never, rarely, sometimes, often, and always with the corresponding outcome variable of at least sometimes stress. Happiness is asked on the scale of if the respondent is very happy, sometimes happy, or not too happy with the corresponding outcome variable being if the respondent is at least sometimes happy. Likewise, participants were asked if their health was, in general, poor, fair, good, very good, or excellent.

Outcomes of having all needed prescriptions and all needed healthcare are evaluated on whether respondents had all needed access over the past year. Likewise, the outcome of having trouble paying other (non-medical) bills is evaluated over the past year.

The measures 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 in the past 18 months, and if the forgiveness had an impact on the respondent and their family. If a respondent indicated that they did not have medical debt forgiven, the amount of debt forgiven is set to \$0 and the impact of the forgiveness is set to none. If a respondent skipped the question asking if their debt was forgiven but otherwise indicated that they had a positive amount of debt forgiven or debt forgiveness had an impact on their life, we turn on the indicator for having debt forgiven.

Respondents are also asked how much they expect to pay off their personal medical debt and how much they feel is fair to pay. Respondents are limited to responding to the upper limit of their personal medical debt. Due to the survey's multimodality, some respondents indicated that they expect to pay or believe it's fair to pay more than their personal medical debt. These responses are downscaled to their personal medical

debt.

Some respondents did not respond to all questions in the survey, responded with "I don't know," or responded with multiple answers for questions where that is not appropriate. We code the latter two response types as missing and impute all missing outcome variables via hotdeck imputation. Hotdecking takes place within age cohort $\times$ treatment groups. Age cohorts are constructed as 18-29, 30-39, 40-49, 50-59, and 60+. The following variables are imputed simultaneously: all the component questions for each of the outcomes listed above (PHQ-8 questions, GAD-7 questions, the three questions pertaining to cutting spending, and the five questions pertaining to increasing debt), all other outcomes listed above (stress, happiness, general health, receiving all prescription, all healthcare, trouble paying other bills, debt forgiveness, the impact of forgiveness, amount of debt forgiven, how much a respondent expects to pay, how much is fair to pay), as well as the amount of debt of each individual, household income, and if an individual is single or not.

PHQ-8 and GAD-7 summations occur after hotdecking, as does the inverse standard deviation weighting on decreased spending and increased borrowing outcomes.

In the event that a respondent does not indicate their race, all race variables are coded as no, and a variable denoting unknown race is coded positively. Likewise, a respondent is assumed to be not Hispanic if they do not indicate if they are Hispanic or not.

A.5 Expert Survey

We conducted an expert survey to assess prevailing beliefs on the impact of our hospital debt experiment. Survey respondents were first provided with 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 F.

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 staff 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.

Appendix Figure A5 shows box plots of expert predictions for the impacts of medical debt relief. For our primary outcome of depression, we provided respondents with the control group mean and, as a benchmark, the 9 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 member predicting a larger 8.1 percentage-point reduction.

Appendix Figure A5 also shows that expert survey respondents 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 A6.

B Cross-Sectional Relationships with Medical Debt in Nationally Representative Datasets

B.1 Data Selection

We estimate the cross-sectional relationship between medical debt and several key outcome variables using nationally representative administrative and survey data. For the credit bureau outcomes, we use the nationally representative random sample of credit reports provided by TransUnion. Specifically, we analyze data from Q3 2018 (aligning with the start of the hospital debt experiment) and Q4 2021 (the most recent quarter available); these samples cover periods before and after the industry-wide pullback in credit reporting. To capture additional financial and health outcomes, we reviewed the variables available in the Panel Study of Income Dynamics (PSID), the Survey of Consumer Finances (SCF), the Survey of Income and Program Participation (SIPP), the Medical Expenditure Panel Survey (MEPS), and the Health and Retirement Study (HRS). We excluded the SCF and HRS because they do not directly measure medical debt (both surveys group medical debt with other categories of debt) and focus on the PSID, SIPP, and MEPS for our analysis. To match the timing of the credit bureau samples, we use the 2019 and 2022 waves of the SIPP, the 2019 and 2021 waves of the PSID, and the 2019 and 2022 waves of the MEPS. The reference period for most survey questions is the prior calendar year.

B.2 Methodology

For the credit reports analysis, we measure medical debt using the medical debt in collections variable and construct the same outcomes we examined in the experimental analysis (see Appendix Section A.4). We exclude "bankruptcy in the last 12 months" as an outcome for this analysis because persons who have filed are likely to have discharged their medical debt through the process.

For the surveys, our variable construction is outlined in Appendix Table A2. The PSID, SIPP, and MEPS include indicators for whether respondents have medical debt and the PSID and SIPP also measure medical debt balances. In all three surveys, medical debt includes any unpaid medical bills or medical bills that are being paid off over time.

The PSID and SIPP include measures of mortgage, credit card, and auto loan balances. The PSID and SIPP also ask whether respondents are behind on mortgage and/or rent payments, which we use as proxies for delinquency and default. All three surveys contain the same question on perceived general health as our survey, but none include the PHQ-8 or GAD-7, which we use to measure depression and anxiety. The MEPS includes the PHQ-2, an abbreviated 2-question version of the PHQ-8, and the PSID and MEPS include the 6-question Kessler Psychological Distress Scale (K6), which screens for general psychological distress, providing as an alternate mental health outcome. Our remaining outcome variables do not have comparable survey measures.

The PSID and SIPP interview all household members aged 18 or older and identify a household "reference person," who is typically the household member that leads financial decision-making. The MEPS designates a "household respondent," who reports data for all household members. Survey questions may be asked of each person in the household, the reference person only, the aggregate household, or the aggregate

family. The ways we handle these distinctions are noted in Appendix Table A2.

The share of respondents with medical debt is 18% in the TransUnion sample, 9% in the PSID, 11% in the SIPP, and 19% in the MEPS.

Let i denote the individual or survey respondent and y_i denotes the outcome variable. We analyze the correlation of our outcomes with medical debt along the intensive and extensive margins with regressions of the form:

$$y_i = \alpha_1 + \beta_1 \mathbb{1}(Debt_i > 0) + \varepsilon_i \tag{4}$$

$$y_i = \alpha_2 + \beta_2 \ln Debt_i + \varepsilon_i \quad \text{if} \quad Debt_i > 0 \tag{5}$$

where $Debt_i$ is the nominal value of medical debt balances, and $\mathbb{1}(Debt_i > 0)$ is an indicator variable that equals one if the individual has medical debt. To account for survey design elements in these analyses, we incorporate the sampling weights and strata provided by each survey.

The extensive margin estimates from Equation 4 are presented in Appendix Tables A3-A6. These tables include an alternate specification that controls for age, gender (indicator for female), education (indicators for high school and college), health insurance (indicator for any coverage), and annual household income. The intensive margin estimates for the credit bureau outcomes and the survey health outcomes are presented as binned scatterplots in Appendix Figures A1-A4.

C SUPPLEMENTAL ANALYSES

C.1 Credit Bureau Outcomes: Alternative Specifications

For our analysis of credit bureau data, we estimate two alternative specifications without person fixed effects. While controlling for person-level characteristics is not necessary for a causal interpretation of β, we include fixed effects in our baseline model to control for any incidental differences between the treatment and control groups. We did not expect and do not find any noticeable impact on the point estimates from this analysis.

The first alternative specification drops the person fixed effects from the baseline regression model and estimates results using a single post-period observation four quarters after treatment assignment. This model is shown below:

$$y_{it} = \beta T_i \times POST_{it} + \alpha_{r(i),t} + \varepsilon_{it} \tag{6}$$

The second alternative specification also uses a single period for estimation but additionally saturates the specification with controls defined prior to treatment assignment. Using the collections account data, which is observed prior to treatment assignment, we control for gender, insurance status (in the hospital debt experiment), age, state, 25-point bins of the collections score used by the debt collector, 25-point bins of credit score (Vantage 4.0), an indicator for whether the person has an open mortgage, log non-mortgage debt, log non-medical debt in collections, and log medical debt in collections. We set log variables to zero when the underlying variable is zero, and for each of the log outcomes include an indicator that takes a value of one when there is zero underlying balance. For all other outcomes besides the indicator for mortgage (which is never missing), we include a variable that is set to one when its corresponding variable is missing and zero otherwise. In these cases, we then set the missing outcome to zero.

Appendix Tables A34 and A35 present the results from the no-fixed-effects and saturated regressions, laid out in the same format as the baseline results in Table IV. As expected, the point estimates are virtually indistinguishable from the baseline estimates, and the standard errors are somewhat larger.

C.2 Heterogeneous Treatment Effects

To estimate heterogeneous treatment effects, we assign persons to quartiles of a given dimension of heterogeneity (e.g., amount of debt eligible for relief) 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:

$$y_{i,t} = \beta_h T_{i,t} + \gamma_i + \alpha_{r(i),h(i),t} + \varepsilon_{i,t} \tag{7}$$

where i indexes persons, $T_{i,h,t}$ is an indicator that turns on for persons in quartile h randomly assigned to debt relief in the post-treatment period (and is otherwise zero), and γ_i are person fixed effects. The h subscripts indicate that the treatment effects β_h vary by quartile h. We include randomization-group-by-quartile-by-time-period fixed effects $\alpha_{r(i),h(i),t}$ to isolate the experimental variation (see Section V for details across

experiments and outcomes). We cluster the standard errors at the person level.

For analysis of the credit bureau outcomes, we restrict the sample to include four pre-treatment quarters and the fourth quarter after treatment. For analysis of the collections account and survey account outcomes, where we have a single outcome period, we exclude individual fixed effects and include randomization-group-by-quartile fixed effects $\alpha_{r(i),h(i)}$ without the time period interaction.

We pre-specified exploring heterogeneity by the amount of debt eligible for relief, age of debt eligible for relief, 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 A36 through A43 for the credit bureau outcomes, Appendix Tables A48 through A51 for the collections account outcomes, and Appendix Tables A57 through A60 for the survey outcomes. Notable results are discussed in the main body of the paper.

C.3 Credit Bureau Reporting Subsample

As previously discussed, the debt collector 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 until 2019 Q1, allowing us to estimate the impact of debt relief on credit bureau outcomes without reporting of medical debt relative to a counterfactual with reporting.

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 register a match if the difference is less than \$0.50, allowing for rounding to the nearest integer.

Appendix Figure A8 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 2019 Q1 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.

Panel B of Appendix Figure A8 shows similar patterns for wave 2. Given that the debt collector ceases reporting in 2019 Q1, only one quarter after the wave 2 intervention, there is counterfactual reporting for only a single quarter and we focus our analysis on wave 1. We show results for wave 2 for completeness.

As discussed in Section VI.B, in wave 1 of the credit reporting subsample, debt relief raised credit scores by an economically small 3.4 points (p-value of 0.021), with a 13.8-point effect (p-value of 0.008) for persons with no other debt in collections (Table V). We also estimate a credit limit effect that phased in over time, with an average \$340 increase (p-value of 0.010) for the full subsample and a less precise \$922 increase (p-value of 0.070) for persons with no other debt in collections.

We also conduct this analysis for accounts in wave 2, in which control group accounts were reported for only one quarter after intervention. Appendix Table A47 presents results for wave 2. The results are qualitatively similar but smaller in magnitude for credit scores, with a 1.6-point average effect (p-value of 0.021) and a 6.0-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 A44 shows effects on the remaining main credit bureau outcomes using the wave 1 sample. Appendix Tables A45 and A46 examine these effects separately by whether the person had another debt in collections. This analysis naturally shows an effect on the measures of debt in collections, which combine both medical and non-medical debts. 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).

C.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 in Appendix Section C.3). 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 A52, are aligned with our expectations.

C.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 in Appendix Section C.3).

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 A53 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.

C.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 using the survey outreach sample. 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 debt in collections, and log of medical debt in collections. Time-varying regressors 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. (2022) 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 A54 present results for our main survey outcomes, separately for those with above- versus below-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 above- versus below-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.

D Appendix Tables

Table A1. Use of Public Funds for Medical Debt Relief as of August 30, 2024

	Date Announced	Source of Funds	Funds ($, Millions)	Debt Relief ($, Millions)	Source
	(1)	(2)	(3)	(4)	(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
Cincinnati, OH	July, 2023	Local Taxes	1.5	150	City of Cincinnati
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, MN	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
Los Angeles, CA	June, 2024	Local Taxes	5.0	500	New York Times
Illinois	July, 2024	State Taxes	10.0	1,000	Office of the Governor JB Pritzker
St. Louis, MO	July, 2024	ARPA	0.8	80	KSDK
New Jersey	August, 2024	ARPA	0.6	100	Gothamist
Orange County, FL	August, 2024	ARPA	4.5	450	Orlando Weekly
Washington, DC	August, 2024	Local Taxes	0.2	42	NBC Washington
Total Passed			**106.8**	**10,162.0**	
Panel B. Programs Under Consideration					
Pennsylvania	February, 2024	State Taxes	4.0	400	Spotlight PA
North Carolina	July, 2024	State Taxes	40.0	4,000	NC Health News
Total Under Consideration			**44.0**	**4,400.0**	
Total			**150.8**	**14,562.0**	

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 August 30, 2024. All governments are partnering with RIP or indicated their intention to do so, except Columbus, OH, which is working directly with local hospitals. 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 Milwaukee, WI have proposed similar programs that did not move forward.

Table A2. Measuring Financial and Health Outcomes in Nationally Representative Surveys

Survey	Obs. Level	Survey Language
Panel A: Has Medical Debt[1]		
PSID	Family	Do you (or anyone in your family living there) currently have ... medical bills?
SIPP	Person	Whether any money was owed for medical bill not paid in full during the reference period.
MEPS	Person	Does anyone in your family currently have any medical bills that are being paid off over time?
Panel B: Medical Debt[1,2]		
PSID	Family	If you added up all medical bills for all of your family living there, about how much would they amount to right now?
SIPP	Person	The amount owed for medical bills not paid in full during the reference period.
Panel C: Mortgage Debt[2]		
PSID	Family	Remaining principal on mortgages, land contracts, or loans against family's primary residence.
SIPP	Person	Person-level sum of debt against primary residence in which the person is an owner of the residence. The home's debt is divided equally among its total number of owners.
Panel D: Credit Card Debt[2]		
PSID	Family	If you added up all credit card and store card debts for all of your family living there, about how much would they amount to right now?
SIPP	Person	Person-level sum of amount owed on credit card debt and store bills.
Panel E: Auto Debt[2]		
SIPP	Person	Person-level sum of debt against all vehicles in which the person owns a share. Each vehicle's debt value is divided equally among its total number of owners.
Panel F: Past-Due Mortgage or Rent[3]		
PSID	Family	Are you (or anyone in your family living there) currently behind on your (mortgage/loan) payments [against primary residence]?
SIPP	Household	Was [reference person] unable to pay rent or mortgage [in previous calendar year]?
Panel G: At Least Good Health		
PSID	Family	Would you say [reference person's] health is excellent, very good, good, fair, or poor? [Excellent, very good, or good]
SIPP	Person	What is [your] health status? [Excellent, very good, or good]
MEPS	Person	[Reference person's assessment of whether person's] health is excellent, very good, good, fair, or poor? [Excellent, very good, or good]
Panel H: At Least Moderate Distress		
PSID	Family	[Reference person's] score on the K-6 Non-Specific Psychological Distress Scale (0-24) [13 or higher]
MEPS	Person	Person's score on the K-6 Non-Specific Psychological Distress Scale (0-24) [13 or higher]
Panel I: Any Depressive Disorder		
MEPS	Person	Person's score on the Patient Health Questionnaire-2 (PHQ-2) (0-6) [2 or higher]

Notes: Table presents the variables measured in the PSID, SIPP, and MEPS for the analysis in Appendix Section B (Agency for Healthcare Research and Quality 2024; Institute for Social Research 2024; United States Census Bureau 2024). Each panel corresponds to an outcome used in the paper, and within each panel the survey-specific definition is listed. The "Obs. Level" column indicates the level of observation at which the survey variable is collected. The "Survey Language" column includes the relevant survey language.
1: In the SIPP, 7% of users who indicated they have medical debt reported a balance of $0. We adjust the medical debt indicator to zero for these users for consistency.
2: We winsorize the conditional distribution of debt balances at the 99^{th} percentile.
3: In the PSID, this question is asked only of families with mortgages, which account for 34% of the sample. We code the indicator as missing if a family has no mortgage.

Table A3. Extensive Margin Relationships with Medical Debt in Nationally Representative Surveys (2019)

	SIPP			PSID			MEPS		
	No Medical	Estimated Coefficient		No Medical	Estimated Coefficient		No Medical	Estimated Coefficient	
	Debt Mean (1)	No Controls (2)	Controls (3)	Debt Mean (4)	No Controls (5)	Controls (6)	Debt Mean (7)	No Controls (8)	Controls (9)
Panel A: Distress									
Trouble paying mortgage or rent (%)	4.11	8.26	7.33	1.33	2.53	2.02	.	.	.
		(0.76)	(0.78)		(1.59)	(2.02)		.	.
		[0.000]	[0.000]		[0.115]	[0.320]		.	.
Panel B: Borrowing									
Mortgage debt ($)	32,934	-7,073	2,877	58,158	-19,729	-4,852	.	.	.
		(1,292)	(1,239)		(4,512)	(5,642)		.	.
		[0.000]	[0.021]		[0.000]	[0.393]		.	.
Credit card debt ($)	1,660	1,340	1,564	2,800	795	2,093	.	.	.
		(140)	(142)		(387)	(758)		.	.
		[0.000]	[0.000]		[0.043]	[0.007]		.	.
Auto debt ($)	3,212	257	707	.	.	.	.	.	.
		(139)	(138)		.	.		.	.
		[0.066]	[0.000]		.	.		.	.
Panel C: General Health									
At least good health (%)	85.09	-19.65	-17.78	78.99	-13.99	-14.10	87.83	-6.33	-5.42
		(0.90)	(0.89)		(2.75)	(4.24)		(0.92)	(0.88)
		[0.000]	[0.000]		[0.000]	[0.001]		[0.000]	[0.000]
Panel D: Mental Health									
At least mod. distress (K6) (%)	.	.	.	3.86	6.20	6.63	2.83	3.76	3.25
		.	.		(1.57)	(1.84)		(0.56)	(0.58)
		.	.		[0.000]	[0.001]		[0.000]	[0.000]
Any depressive disorder (PHQ-2) (%)	.	.	.	.	.	.	15.70	8.87	7.34
		.	.		.	.		(1.03)	(1.03)
		.	.		.	.		[0.000]	[0.000]
Panel E: Sample Size									
Observations†	35,109	4,138		8,747	822		17,585	4,046	

Notes: Table shows the extensive margin regression estimates outlined in Equation 4 in Appendix Section B, using data from the SIPP, PSID, and MEPS (Agency for Healthcare Research and Quality 2024; Institute for Social Research 2024; United States Census Bureau 2024). We use the 2019 wave of each survey to most closely align with the start of the hospital debt experiment (2018 Q3). Regressions incorporate sample weights. Weighted means for the control group (i.e., respondents with no medical debt) are shown in columns (1), (4), and (7). The estimated coefficients with no controls are presented in columns (2), (5), and (8). The estimated coefficients with controls for age, gender, education, insurance coverage, and household income are shown in columns (3), (6), and (9). Standard errors and *p*-values are reported below the coefficients in parentheses and square brackets, respectively. †: Sample sizes for the group with no medical debt in collections and with a positive amount of medical debt in collections reported in Panel E below the control means and estimated effects, respectively. The sample sizes for select regressions are lower due to non-response in the outcome variables.

Table A4. Extensive Margin Relationships with Medical Debt in Nationally Representative Surveys (2021/2022)

	SIPP			PSID			MEPS		
	No Medical	Estimated Coefficient		No Medical	Estimated Coefficient		No Medical	Estimated Coefficient	
	Debt Mean	No Controls	Controls	Debt Mean	No Controls	Controls	Debt Mean	No Controls	Controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Distress									
Trouble paying mortgage or rent (%)	4.21	8.95	7.82	2.57	7.54	2.96	.	.	.
		(1.00)	(0.98)		(3.66)	(3.10)		.	.
		[0.000]	[0.000]		[0.043]	[0.344]		.	.
Panel B: Borrowing									
Mortgage debt ($)	38,457	-11,350	-554	76,715	-26,732	-13,797	.	.	.
		(1,528)	(1,521)		(5,225)	(6,787)		.	.
		[0.000]	[0.716]		[0.000]	[0.046]		.	.
Credit card debt ($)	1,421	1,600	1,713	2,958	936	1,408	.	.	.
		(159)	(160)		(390)	(588)		.	.
		[0.000]	[0.000]		[0.019]	[0.020]		.	.
Auto debt ($)	3,363	679	1,018	.	.	.	.	.	.
		(238)	(235)		.	.		.	.
		[0.005]	[0.000]		.	.		.	.
Panel C: General Health									
At least good health (%)	84.59	-17.77	-15.86	83.28	-11.99	-11.94	88.07	-8.66	-8.13
		(1.08)	(1.05)		(2.33)	(1.87)		(1.13)	(1.09)
		[0.000]	[0.000]		[0.000]	[0.000]		[0.000]	[0.000]
Panel D: Mental Health									
At least mod. distress (K6) (%)	.	.	.	3.95	7.24	5.77	3.76	2.63	2.11
		.	.		(1.76)	(2.20)		(1.03)	(1.06)
		.	.		[0.000]	[0.011]		[0.011]	[0.047]
Any depressive disorder (PHQ-2) (%)	.	.	.	.	.	.	17.55	8.92	8.57
		.	.		.	.		(2.03)	(2.15)
		.	.		.	.		[0.000]	[0.000]
Panel E: Sample Size									
Observations†	29,774	2,777		22,135	2,534		15,317	2,454	

Notes: Table shows the extensive margin regression estimates outlined in Equation 4 in Appendix Section B, using data from the SIPP, PSID, and MEPS (Agency for Healthcare Research and Quality 2024; Institute for Social Research 2024; United States Census Bureau 2024). We use the 2022 SIPP, the 2022 MEPS, and the 2021 PSID, reflecting the current policy regime with limited credit reporting of medical debt. Regressions incorporate sample weights. Weighted means for the control group (i.e., respondents with no medical debt) are shown in columns (1), (4), and (7). The estimated coefficients with no controls are presented in columns (2), (5), and (8). The estimated coefficients with controls for age, gender, education, insurance coverage, and household income are shown in columns (3), (6), and (9). Standard errors and p-values are reported below the coefficients in parentheses and square brackets, respectively. †: Sample sizes for the group with no medical debt in collections and with a positive amount of medical debt in collections reported in Panel E below the control means and estimated effects, respectively. The sample sizes for select regressions are lower due to non-response in the outcome variables.

Table A5. Extensive Margin Relationships with Medical Debt in Nationally Representative Credit Bureau Sample (2018 Q3)

	No Medical Debt Mean	**No Controls** Est Coef.	*p*-value	**Controls** Est. Coef.	*p*-value
	(1)	(2)	(3)	(4)	(5)
Panel A. Distress					
Number of accounts past due	0.37	0.70	[0.000]	0.64	[0.000]
		(0.02)		(0.02)	
Number of accounts in default	0.31	0.64	[0.000]	0.59	[0.000]
		(0.02)		(0.02)	
Debt past due ($)	2,456	3,440	[0.000]	3,097	[0.000]
		(225)		(227)	
Balances in default ($)	1,479	2,690	[0.000]	2,482	[0.000]
		(160)		(161)	
Panel B. Debt in Collections					
Number of non-medical debts in collections	0.24	0.68	[0.000]	0.64	[0.000]
		(0.01)		(0.01)	
Non-medical debts in collections ($)	254	602	[0.000]	562	[0.000]
		(22)		(22)	
Panel C. Access to Credit					
Has credit score (%)	99.86	0.14	[0.000]	0.15	[0.000]
		(0.02)		(0.02)	
Credit score (never missing)	718.35	-139.27	[0.000]	-132.69	[0.000]
		(0.82)		(0.83)	
Credit card limit ($)	22,957	-19,936	[0.000]	-19,248	[0.000]
		(163)		(172)	
Panel D. Borrowing					
Number of credit cards	2.35	-1.61	[0.000]	-1.62	[0.000]
		(0.02)		(0.02)	
Credit card balance ($)	4,837	-3,194	[0.000]	-3,409	[0.000]
		(60)		(62)	
Number of auto loans	0.45	-0.12	[0.000]	-0.16	[0.000]
		(0.01)		(0.01)	
Auto loan balance ($)	6,995	-1,590	[0.000]	-2,297	[0.000]
		(122)		(125)	
Panel E. Sample Size					
Observations†	48,333	10,335			

Notes: Table shows the extensive margin regression estimates outlined in Equation 4 in Appendix Section B, using data from a nationally representative sample from TransUnion. We use data from 2018 Q3, aligning with the start of the hospital debt experiment. Means for the sample with no medical debt in collections are shown in column (1). The estimated coefficients with no controls are presented in columns (2), with standard errors reported below in parentheses. The estimated coefficients with controls for age are shown in column (4). *p*-values are reported in columns (3) and (5).
†: Sample sizes for the group with no medical debt in collections and with a positive amount of medical debt in collections reported in columns (1) and (2) respectively.

Table A6. Extensive Margin Relationships with Medical Debt in Nationally Representative Credit Bureau Sample (2021 Q4)

	No Medical Debt Mean	**No Controls** Est Coef.	*p*-value	**Controls** Est. Coef.	*p*-value
	(1)	(2)	(3)	(4)	(5)
Panel A. Distress					
Number of accounts past due	0.30	0.53	[0.000]	0.49	[0.000]
		(0.02)		(0.02)	
Number of accounts in default	0.25	0.47	[0.000]	0.44	[0.000]
		(0.02)		(0.02)	
Debt past due ($)	1,628	2,565	[0.000]	2,376	[0.000]
		(186)		(185)	
Balances in default ($)	1,048	2,062	[0.000]	1,939	[0.000]
		(135)		(134)	
Panel B. Debt in Collections					
Number of non-medical debts in collections	0.20	0.46	[0.000]	0.44	[0.000]
		(0.01)		(0.01)	
Non-medical debts in collections ($)	228	497	[0.000]	472	[0.000]
		(22)		(22)	
Panel C. Access to Credit					
Has credit score (%)	99.84	0.16	[0.000]	0.17	[0.000]
		(0.02)		(0.02)	
Credit score (never missing)	723.35	-131.32	[0.000]	-127.02	[0.000]
		(0.85)		(0.85)	
Credit card limit ($)	23,549	-19,646	[0.000]	-19,383	[0.000]
		(176)		(185)	
Panel D. Borrowing					
Number of credit cards	2.43	-1.46	[0.000]	-1.50	[0.000]
		(0.02)		(0.02)	
Credit card balance ($)	4,614	-2,680	[0.000]	-2,882	[0.000]
		(64)		(65)	
Number of auto loans	0.42	-0.10	[0.000]	-0.13	[0.000]
		(0.01)		(0.01)	
Auto loan balance ($)	7,445	-1,321	[0.000]	-2,055	[0.000]
		(146)		(147)	
Panel E. Sample Size					
Observations†	52,101	8,909			

Notes: Table shows the extensive margin regression estimates outlined in Equation 4 in Appendix Section B, using data from a nationally representative sample from TransUnion. We use data from 2021 Q4, reflecting the current policy regime with limited credit reporting of medical debt. Means for the sample with no medical debt in collections are shown in column (1). The estimated coefficients with no controls are presented in columns (2), with standard errors reported below in parentheses. The estimated coefficients with controls for age are shown in column (4). *p*-values are reported in columns (3) and (5).
†: Sample sizes for the group with no medical debt in collections and with a positive amount of medical debt in collections reported in columns (1) and (2) respectively.

Table A7. 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 for each wave of debt relief in 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 sample sizes, respectively. Columns (5), (6), and (7) report the total face value of medical debt eligible for relief for the full sample, treatment group (relieved), and control group (not relieved), respectively. Panel A reports statistics for each wave of the hospital debt experiment, Panel B reports statistics for each wave of the collector debt experiment, and Panel C reports aggregate statistics across all waves in each experiment.

Table A8. 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
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	19.8	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	7.1	8.5	8.6
Panel E. Household Income (%)			
$0 to $30,000	48.2	23.0	23.9
$30,001 to $55,000	23.2	21.2	20.3
$55,001 to $80,000	15.9	19.2	16.1
$80,001 to $100,000	12.1	11.0	9.3
$100,001+	0.7	25.6	30.3

Notes: Table presents pre-treatment summary statistics for the survey respondent 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) (National Center for Health Statistics 2024; United States Census Bureau 2024). CPS ASEC respondents under age 18 are dropped. NHIS and CPS ASEC statistics use population weights representative of the US adult population.

Table A9. Hospital Debt Experiment: Demographic and Collector Variable Balance by Beneficiary Age

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	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)	25.57	-0.04	35.91	0.07	47.56	0.02	65.33	-0.12
		[0.446]		[0.254]		[0.745]		[0.417]
Male (%)	42.34	-2.12	43.05	2.12	45.51	0.32	50.06	0.24
		[0.020]		[0.019]		[0.739]		[0.800]
Panel B. Collector Data								
Medical debt ($)	1,337.73	-13.29	1,444.05	7.19	1,495.59	2.27	1,168.99	13.16
		[0.739]		[0.861]		[0.964]		[0.771]
Medical debt age (quarters)	4.93	-0.001	5.00	-0.04	5.13	0.02	5.56	-0.05
		[0.973]		[0.128]		[0.576]		[0.099]
Insured (%)	46.88	-0.10	52.16	-0.83	63.50	-0.68	82.51	0.01
		[0.910]		[0.341]		[0.449]		[0.993]
Panel C. Sample Size								
Observations†	15,532	3,642	16,395	3,790	14,349	3,376	14,763	3,458

Notes: Table reports balance on demographics and collections account variables for each quartile of debtor age in the hospital debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.
Quartile cutoffs in years are as follows: Q1: [18, 30], Q2: [31, 41], Q3: [42, 54], Q4: [55, 89].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A10. Hospital Debt Experiment: Credit Bureau Variable Balance by Beneficiary Age

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Distress								
Number of accounts past due	1.19	-0.03	1.57	0.01	1.48	0.01	1.09	0.02
		[0.359]		[0.811]		[0.842]		[0.674]
Number of accounts in default	1.08	-0.03	1.42	0.01	1.33	0.01	0.97	0.02
		[0.431]		[0.802]		[0.759]		[0.649]
Debt past due ($)	3,876	-393	7,017	-335	7,301	91	4,391	287
		[0.034]		[0.291]		[0.795]		[0.286]
Balances in default ($)	3,069	-347	5,252	-179	5,050	55	3,137	-13
		[0.011]		[0.429]		[0.816]		[0.940]
Panel B. Debt in Collections								
Number of debts in collections	3.66	0.03	4.89	-0.03	4.57	0.15	3.31	-0.11
		[0.772]		[0.824]		[0.220]		[0.267]
Debts in collections ($)	3,248	-74	4,279	-112	3,889	173	2,533	-60
		[0.469]		[0.421]		[0.198]		[0.576]
Panel C. Bankruptcy								
Bankruptcy in last 12 months (%)	0.41	-0.01	0.85	0.24	1.03	0.03	0.61	0.01
		[0.967]		[0.227]		[0.890]		[0.965]
Panel D. Access to Credit								
Has credit score (%)	95.36	-0.20	97.53	0.17	97.29	-0.19	95.49	0.24
		[0.616]		[0.571]		[0.554]		[0.549]
Credit score (never missing)	559.21	-0.41	566.30	-0.32	575.92	1.14	600.19	0.17
		[0.772]		[0.832]		[0.450]		[0.918]
Credit card limit ($)	1,306	-95	2,382	52	2,992	250	3,447	-121
		[0.281]		[0.722]		[0.129]		[0.490]
Panel E. Borrowing								
Number of credit cards	0.51	-0.02	0.75	0.01	0.89	0.01	0.84	-0.06
		[0.311]		[0.840]		[0.859]		[0.044]
Credit card balance ($)	894	-76	1,550	26	1,889	118	1,815	-24
		[0.104]		[0.740]		[0.181]		[0.777]
Number of auto loans	0.34	0.003	0.42	0.01	0.48	-0.01	0.36	-0.002
		[0.813]		[0.476]		[0.346]		[0.898]
Auto loan balance ($)	6,447	-131	8,795	278	9,649	-295	6,838	31
		[0.561]		[0.350]		[0.319]		[0.904]
Panel F. Sample Size								
Observations†	13,002	3,043	14,959	3,448	13,378	3,161	13,986	3,272

Notes: Table reports balance on credit bureau variables (measured in the quarter before treatment assignment) for each quartile of debtor age in the hospital debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets. Quartile cutoffs in years are as follows: Q1: [18, 31], Q2: [32, 41], Q3: [42, 55], Q4: [56, 89].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A11. Hospital Debt Experiment: Demographic and Collector Variable Balance by Medical Debt Eligible for Relief

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	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)	48.70	-0.15	41.01	-0.01	41.75	-0.28	41.12	0.43
		[0.649]		[0.961]		[0.312]		[0.096]
Male (%)	42.37	-0.18	44.39	-0.29	45.37	-0.53	48.27	1.83
		[0.843]		[0.757]		[0.568]		[0.053]
Panel B. Collector Data								
Medical debt ($)	123.87	-0.31	398.29	0.87	994.46	4.28	3,903.46	15.39
		[0.772]		[0.674]		[0.360]		[0.825]
Medical debt age (quarters)	5.37	-0.02	5.06	-0.001	5.07	-0.002	5.11	-0.03
		[0.406]		[0.978]		[0.945]		[0.282]
Insured (%)	85.75	-0.38	60.22	0.31	57.05	-0.75	40.83	-0.82
		[0.560]		[0.717]		[0.395]		[0.365]
Panel C. Sample Size								
Observations†	15,374	3,595	15,371	3,598	15,300	3,675	15,451	3,509

Notes: Table reports balance on demographics and collections account variables for each quartile of debt eligible for relief (measured in the wave of treatment assignment) in the hospital debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.
Quartile cutoffs are as follows: Q1: [$25, $235], Q2: [$235, $620], Q3: [$620, $1,475], Q4: [$1,475, $60,452].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A12. Hospital Debt Experiment: Credit Bureau Variable Balance by Medical Debt Eligible for Relief

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Distress								
Number of accounts past due	1.33	0.01	1.36	0.04	1.34	-0.04	1.30	-0.02
		[0.734]		[0.305]		[0.321]		[0.624]
Number of accounts in default	1.19	0.02	1.22	0.04	1.21	-0.04	1.17	-0.02
		[0.617]		[0.314]		[0.339]		[0.686]
Debt past due ($)	5,289	-91	5,678	-57	5,802	-246	5,726	59
		[0.742]		[0.841]		[0.401]		[0.839]
Balances in default ($)	3,727	-33	4,197	-80	4,197	-339	4,330	-29
		[0.863]		[0.686]		[0.080]		[0.889]
Panel B. Debt in Collections								
Number of debts in collections	3.69	0.03	3.98	-0.04	4.06	-0.10	4.64	0.15
		[0.774]		[0.695]		[0.314]		[0.240]
Debts in collections ($)	2,852	67	3,385	-116	3,446	-148	4,232	114
		[0.545]		[0.311]		[0.190]		[0.427]
Panel C. Bankruptcy								
Bankruptcy in last 12 months (%)	0.87	-0.06	0.77	0.20	0.65	0.12	0.60	-0.02
		[0.747]		[0.278]		[0.492]		[0.886]
Panel D. Access to Credit								
Has credit score (%)	96.84	0.11	96.80	-0.36	96.56	-0.21	95.30	0.36
		[0.744]		[0.323]		[0.553]		[0.365]
Credit score (never missing)	584.00	1.74	573.34	-1.75	573.35	1.16	570.19	-0.39
		[0.271]		[0.260]		[0.446]		[0.798]
Credit card limit ($)	3,016	-0.2	2,510	-74	2,405	150	2,113	39
		[0.999]		[0.606]		[0.302]		[0.779]
Panel E. Borrowing								
Number of credit cards	0.84	-0.02	0.75	-0.03	0.73	0.003	0.65	-0.01
		[0.471]		[0.211]		[0.904]		[0.629]
Credit card balance ($)	1,733	39	1,515	-16	1,506	52	1,351	-16
		[0.629]		[0.828]		[0.490]		[0.828]
Number of auto loans	0.41	0.01	0.42	-0.03	0.41	0.01	0.36	0.003
		[0.386]		[0.044]		[0.517]		[0.823]
Auto loan balance ($)	7,947	138	8,228	-390	8,037	-5	7,405	108
		[0.604]		[0.152]		[0.986]		[0.695]
Panel F. Sample Size								
Observations†	14,392	3,353	13,883	3,245	13,772	3,268	13,606	3,132

Notes: Table reports balance on credit bureau variables (measured in the quarter before treatment assignment) for each quartile of debt eligible for relief (measured in the wave of treatment assignment) in the hospital debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.

Quartile cutoffs are as follows: Q1: [$25, $226], Q2: [$226, $600], Q3: [$600, $1,440], Q4: [$1,440, $60,452].

†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A13. Hospital Debt Experiment: Demographic and Collector Variable Balance by Medical Debt Age

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	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)	39.23	0.06	42.46	0.34	43.99	-0.08	46.81	-0.34
		[0.803]		[0.206]		[0.776]		[0.297]
Male (%)	47.44	0.64	44.89	-0.56	45.39	0.34	42.71	0.22
		[0.489]		[0.537]		[0.714]		[0.822]
Panel B. Collector Data								
Medical debt ($)	1,494.06	22.26	1,139.15	-29.23	1,279.18	-40.22	1,517.93	48.44
		[0.624]		[0.361]		[0.296]		[0.399]
Medical debt age (quarters)	3.47	0.01	4.86	-0.002	5.20	0.01	7.04	-0.06
		[0.331]		[0.172]		[0.005]		[0.072]
Insured (%)	26.85	-0.47	61.44	-0.11	70.55	-0.47	84.59	0.25
		[0.496]		[0.884]		[0.555]		[0.699]
Panel C. Sample Size								
Observations†	15,352	3,748	15,078	3,758	15,554	3,572	15,511	3,298

Notes: Table reports balance on demographics and collections account variables for each quartile of the age of debt eligible for relief (measured in the wave of treatment assignment) in the hospital debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.
Quartile cutoffs in days are as follows: Q1: [130, 426], Q2: [426, 457], Q3: [457, 495], Q4: [495, 2,177].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A14. Hospital Debt Experiment: Credit Bureau Variable Balance by Medical Debt Age

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Distress								
Number of accounts past due	1.30	0.03	1.38	-0.07	1.38	0.03	1.27	0.01
		[0.374]		[0.053]		[0.408]		[0.844]
Number of accounts in default	1.18	0.04	1.25	-0.07	1.23	0.04	1.13	0.003
		[0.295]		[0.069]		[0.372]		[0.938]
Debt past due ($)	5,153	190	5,890	-503	6,052	315	5,396	-352
		[0.470]		[0.072]		[0.324]		[0.217]
Balances in default ($)	3,927	180	4,379	-679	4,343	229	3,801	-209
		[0.356]		[0.000]		[0.296]		[0.279]
Panel B. Debt in Collections								
Number of debts in collections	4.83	0.08	4.23	-0.20	3.72	0.15	3.60	0.002
		[0.494]		[0.039]		[0.155]		[0.986]
Debts in collections ($)	4,467	42	3,517	-227	3,036	134	2,903	-47
		[0.766]		[0.038]		[0.235]		[0.679]
Panel C. Bankruptcy								
Bankruptcy in last 12 months (%)	0.91	-0.01	0.53	0.24	0.74	-0.08	0.71	0.05
		[0.945]		[0.127]		[0.639]		[0.763]
Panel D. Access to Credit								
Has credit score (%)	97.12	-0.22	97.01	-0.33	95.59	0.39	95.79	0.08
		[0.502]		[0.333]		[0.320]		[0.842]
Credit score (never missing)	563.76	-1.54	571.38	0.97	576.20	-0.62	589.72	1.93
		[0.273]		[0.501]		[0.689]		[0.259]
Credit card limit ($)	1,807	-60	2,340	74	2,503	63	3,386	36
		[0.619]		[0.593]		[0.676]		[0.837]
Panel E. Borrowing								
Number of credit cards	0.57	-0.04	0.71	0.02	0.77	-0.03	0.90	-0.01
		[0.111]		[0.569]		[0.229]		[0.774]
Credit card balance ($)	1,159	-23	1,472	68	1,582	46	1,888	-32
		[0.725]		[0.357]		[0.568]		[0.710]
Number of auto loans	0.35	-0.01	0.41	0.001	0.41	-0.001	0.43	0.01
		[0.375]		[0.919]		[0.923]		[0.410]
Auto loan balance ($)	6,953	-339	8,118	3	8,067	101	8,466	171
		[0.154]		[0.990]		[0.718]		[0.560]
Panel F. Sample Size								
Observations†	13,039	3,194	13,734	3,428	14,251	3,263	14,629	3,112

Notes: Table reports balance on credit bureau variables (measured in the quarter before treatment assignment) for each quartile of the age of debt eligible for relief (measured in the wave of treatment assignment) in the hospital debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.
Quartile cutoffs in days are as follows: Q1: [130, 428], Q2: [428, 459], Q3: [459, 498], Q4: [498, 2,177].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A15. Hospital Debt Experiment: Demographic and Collector Variable Balance by Debt in Collections

	No Debt in Collections		**Tercile 1**		**Tercile 2**		**Tercile 3**	
	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)	45.16	0.11	45.38	0.11	43.11	-0.31	41.70	0.08
		[0.757]		[0.733]		[0.281]		[0.754]
Male (%)	47.00	-0.26	44.50	0.69	43.08	-0.75	42.74	1.71
		[0.797]		[0.479]		[0.442]		[0.078]
Panel B. Collector Data								
Medical debt ($)	1,232.67	-40.45	1,176.82	26.57	1,320.00	38.97	1,578.06	18.06
		[0.346]		[0.531]		[0.402]		[0.722]
Medical debt age (quarters)	5.39	0.02	5.30	-0.03	5.16	-0.02	5.00	-0.04
		[0.511]		[0.246]		[0.521]		[0.209]
Insured (%)	72.94	-0.15	70.52	-0.52	61.82	-0.84	51.94	-0.44
		[0.866]		[0.546]		[0.360]		[0.636]
Panel C. Sample Size								
Observations†	13,465	3,210	14,041	3,289	14,105	3,217	14,042	3,282

Notes: Table reports balance on demographics and collections account variables for each quartile of debt in collections in the hospital debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.
Quartile cutoffs are as follows: Q1: [$0, $0], Q2: [$1, $1,166], Q3: [$1,167, $3,900], Q4: [$3,901, $938,774].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A16. Hospital Debt Experiment: Credit Bureau Variable Balance by Debt in Collections

	No Debt in Collections		Tercile 1		Tercile 2		Tercile 3	
	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Distress								
Number of accounts past due	0.77	0.01	1.16	-0.02	1.55	-0.01	1.82	0.02
		[0.753]		[0.489]		[0.754]		[0.638]
Number of accounts in default	0.64	0.01	1.01	-0.01	1.41	-0.02	1.70	0.02
		[0.793]		[0.786]		[0.701]		[0.636]
Debt past due ($)	3,475	-217	5,208	-211	6,265	-108	7,452	165
		[0.366]		[0.464]		[0.718]		[0.591]
Balances in default ($)	2,251	-296	3,448	-169	4,623	-241	6,047	194
		[0.038]		[0.342]		[0.233]		[0.419]
Panel B. Debt in Collections								
Number of debts in collections	0.00	-0.00	1.85	-0.02	4.05	-0.04	10.31	0.04
		[.]		[.]		[.]		[.]
Debts in collections ($)	0	-0	518	-6	2,300	2	10,957	-185
		[.]		[.]		[.]		[.]
Panel C. Bankruptcy								
Bankruptcy in last 12 months (%)	1.36	-0.11	0.76	0.07	0.48	0.13	0.31	0.16
		[0.626]		[0.706]		[0.405]		[0.209]
Panel D. Access to Credit								
Has credit score (%)	85.02	-0.22	100.00	-0.00	100.00	0.00	100.00	0.00
		[.]		[.]		[.]		[.]
Credit score (never missing)	642.73	1.29	577.77	-0.01	553.32	0.53	539.77	-1.25
		[0.475]		[0.993]		[0.661]		[0.238]
Credit card limit ($)	6,006	73	2,367	-57	1,194	89	630	-37
		[0.749]		[0.662]		[0.330]		[0.513]
Panel E. Borrowing								
Number of credit cards	1.32	-0.002	0.79	-0.03	0.54	-0.01	0.34	-0.02
		[0.966]		[0.215]		[0.558]		[0.189]
Credit card balance ($)	2,737	-9	1,548	-64	1,011	104	861	16
		[0.932]		[0.373]		[0.087]		[0.765]
Number of auto loans	0.50	-0.005	0.43	-0.01	0.37	0.02	0.29	-0.00
		[0.744]		[0.344]		[0.171]		[0.781]
Auto loan balance ($)	9,771	41	8,178	-234	7,479	157	6,264	-108
		[0.899]		[0.395]		[0.536]		[0.625]
Panel F. Sample Size								
Observations†	13,465	3,210	14,041	3,289	14,105	3,217	14,042	3,282

Notes: Table reports balance on credit bureau variables (measured in the quarter before treatment assignment) for each quartile of debt in collections in the hospital debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets. Quartile cutoffs are as follows: Q1: [$0, $0], Q2: [$1, $1,166], Q3: [$1,167, $3,900], Q4: [$3,901, $938,774].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A17. Survey Respondent Sample: Demographic and Collector Variable Balance by Beneficiary Age

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	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)	25.63	-0.07	35.36	0.32	46.48	-0.44	61.56	0.04
		[0.757]		[0.165]		[0.104]		[0.937]
Male (%)	34.79	-6.99	37.72	-4.00	38.32	0.31	49.51	-3.86
		[0.043]		[0.295]		[0.939]		[0.338]
Panel B. Race and Ethnicity								
Black (%)	18.16	-3.48	15.79	4.83	16.84	3.99	22.57	-1.17
		[0.215]		[0.115]		[0.204]		[0.727]
Non-Hispanic white (%)	35.89	-0.57	43.42	-4.22	48.00	1.14	48.79	0.74
		[0.874]		[0.285]		[0.775]		[0.856]
Hispanic (any race) (%)	36.54	5.45	34.21	-1.36	29.89	-4.88	24.03	-0.04
		[0.137]		[0.707]		[0.168]		[0.990]
Panel C. Collector Data								
Medical debt ($)	1,852.90	37.05	2,135.19	-159.98	2,324.25	-330.72	2,424.56	-407.65
		[0.818]		[0.423]		[0.079]		[0.140]
Medical debt age (quarters)	5.07	0.01	5.08	-0.08	5.12	0.03	5.76	-0.02
		[0.930]		[0.492]		[0.759]		[0.874]
Insured (%)	43.33	3.82	53.85	-5.11	61.47	-5.94	73.06	-0.28
		[0.312]		[0.190]		[0.135]		[0.937]
Panel D. Other								
Response rate (%)	17.60	2.58	18.27	0.22	19.49	1.61	19.96	0.75
		[0.053]		[0.873]		[0.260]		[0.605]
Panel E. Sample Size								
Observations†	457	300	456	275	475	253	412	258

Notes: Table reports balance on demographics and collections account variables for each quartile of debtor age in the survey respondent sample, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.
Quartile cutoffs in years are as follows: Q1: [19, 30], Q2: [31, 40], Q3: [41, 52], Q4: [53, 89].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A18. Survey Respondent Sample: Credit Bureau Variable Balance by Beneficiary Age

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Distress								
Number of accounts past due	1.04	0.20	1.85	-0.29	1.77	-0.09	1.42	-0.04
		[0.141]		[0.095]		[0.648]		[0.834]
Number of accounts in default	0.95	0.18	1.62	-0.21	1.61	-0.03	1.25	-0.05
		[0.158]		[0.195]		[0.877]		[0.776]
Debt past due ($)	2,816	146	7,434	-1,398	7,459	-841	5,985	596
		[0.775]		[0.243]		[0.480]		[0.657]
Balances in default ($)	2,409	117	5,565	-1,321	5,629	136	3,428	675
		[0.792]		[0.105]		[0.885]		[0.397]
Panel B. Debt in Collections								
Number of debts in collections	3.61	0.31	4.94	-0.84	4.41	0.56	3.22	-0.09
		[0.457]		[0.059]		[0.247]		[0.778]
Debts in collections ($)	3,297	133	4,401	-959	3,624	665	2,710	69
		[0.769]		[0.044]		[0.230]		[0.877]
Panel C. Bankruptcy								
Bankruptcy in last 12 months (%)	0.23	0.36	0.90	0.30	1.49	0.18	1.00	1.26
		[0.542]		[0.706]		[0.864]		[0.239]
Panel D. Access to Credit								
Has credit score (%)	96.52	-1.18	97.98	1.05	98.94	0.02	97.51	1.59
		[0.457]		[0.328]		[0.981]		[0.108]
Credit score (never missing)	561.80	-2.49	561.22	4.96	574.09	1.09	606.99	0.15
		[0.665]		[0.415]		[0.860]		[0.984]
Credit card limit ($)	1,427	13	3,085	61	3,037	576	4,639	530
		[0.972]		[0.925]		[0.448]		[0.560]
Panel E. Borrowing								
Number of credit cards	0.61	-0.13	0.87	0.11	0.90	0.18	1.15	0.10
		[0.109]		[0.424]		[0.227]		[0.501]
Credit card balance ($)	933	-52	2,146	165	2,076	-212	2,276	156
		[0.730]		[0.675]		[0.537]		[0.686]
Number of auto loans	0.36	-0.002	0.45	0.04	0.56	-0.11	0.51	-0.03
		[0.958]		[0.453]		[0.061]		[0.659]
Auto loan balance ($)	6,728	0.1	9,186	1,304	10,979	-2,315	9,410	-458
		[1.000]		[0.292]		[0.063]		[0.713]
Panel F. Sample Size								
Observations†	431	280	446	268	471	252	402	255

Notes: Table reports balance on credit bureau variables (measured in the quarter before treatment assignment) for each quartile of debtor age in the survey respondent sample, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets. Quartile cutoffs in years are as follows: Q1: [18, 35], Q2: [36, 44], Q3: [45, 56], Q4: [57, 89].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A19. Survey Respondent Sample: Demographic and Collector Variable Balance by Medical Debt Eligible for Relief

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	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)	41.62	0.07	41.44	-0.05	42.85	-1.41	41.45	0.32
		[0.949]		[0.966]		[0.234]		[0.752]
Male (%)	36.53	4.83	34.86	-6.00	44.86	-9.30	43.35	-2.92
		[0.209]		[0.109]		[0.015]		[0.458]
Panel B. Race and Ethnicity								
Black (%)	22.49	-5.18	17.21	5.34	16.36	6.07	16.74	-1.64
		[0.091]		[0.102]		[0.052]		[0.578]
Non-Hispanic white (%)	44.99	2.41	44.23	-5.89	44.16	-4.17	42.49	4.30
		[0.544]		[0.132]		[0.277]		[0.280]
Hispanic (any race) (%)	26.50	2.19	30.28	2.06	33.41	-4.16	35.19	-2.42
		[0.542]		[0.578]		[0.248]		[0.517]
Panel C. Collector Data								
Medical debt ($)	637.65	8.43	1,009.61	-0.63	1,696.35	-39.79	5,256.49	-467.46
		[0.214]		[0.956]		[0.071]		[0.148]
Medical debt age (quarters)	5.28	0.06	5.24	0.11	5.27	-0.23	5.20	0.06
		[0.672]		[0.444]		[0.053]		[0.604]
Insured (%)	65.40	-4.51	62.09	2.19	59.58	-3.61	43.78	-0.76
		[0.246]		[0.571]		[0.340]		[0.848]
Panel D. Other								
Response rate (%)	20.11	1.19	19.22	1.40	18.00	2.25	17.64	0.42
		[0.407]		[0.308]		[0.112]		[0.755]
Panel E. Sample Size								
Observations†	449	273	459	263	428	294	466	256

Notes: Table reports balance on demographics and collections account variables for each quartile of debt eligible for relief (measured in the wave of treatment assignment) in the survey respondent sample, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.

Quartile cutoffs are as follows: Q1: [$500, $794], Q2: [$794, $1,275], Q3: [$1,276, $2,275], Q4: [$2,277, $33,627].

†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A20. Survey Respondent Sample: Credit Bureau Variable Balance by Medical Debt Eligible for Relief

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Distress								
Number of accounts past due	1.58	-0.02	1.51	-0.03	1.47	-0.08	1.56	-0.11
		[0.895]		[0.878]		[0.631]		[0.533]
Number of accounts in default	1.39	-0.01	1.36	-0.06	1.33	-0.03	1.39	-0.09
		[0.965]		[0.691]		[0.863]		[0.620]
Debt past due ($)	6,527	-1,043	5,223	534	5,351	438	6,736	-1,575
		[0.307]		[0.632]		[0.697]		[0.160]
Balances in default ($)	4,535	-211	3,807	35	3,794	648	5,077	-1,389
		[0.789]		[0.963]		[0.350]		[0.093]
Panel B. Debt in Collections								
Number of debts in collections	3.74	-0.23	4.12	-0.74	3.95	0.42	4.47	0.39
		[0.567]		[0.054]		[0.307]		[0.426]
Debts in collections ($)	3,258	-351	3,452	-620	3,285	271	4,096	418
		[0.446]		[0.168]		[0.520]		[0.473]
Panel C. Bankruptcy								
Bankruptcy in last 12 months (%)	1.83	0.90	0.89	0.62	0.00	0.38	0.89	0.44
		[0.456]		[0.454]		[0.317]		[0.584]
Panel D. Access to Credit								
Has credit score (%)	97.25	0.43	99.11	-0.03	97.60	-0.06	97.11	0.35
		[0.756]		[0.974]		[0.960]		[0.794]
Credit score (never missing)	577.96	-2.77	571.87	15.42	578.50	-9.28	573.57	-0.56
		[0.691]		[0.021]		[0.148]		[0.931]
Credit card limit ($)	3,193	1,335	2,947	807	3,151	-503	2,801	-285
		[0.097]		[0.258]		[0.428]		[0.639]
Panel E. Borrowing								
Number of credit cards	0.85	0.31	0.93	0.11	0.94	-0.13	0.79	0.01
		[0.029]		[0.422]		[0.249]		[0.934]
Credit card balance ($)	2,062	285	2,082	-147	1,626	-306	1,649	154
		[0.450]		[0.678]		[0.258]		[0.635]
Number of auto loans	0.52	-0.06	0.53	-0.02	0.42	-0.06	0.42	0.09
		[0.335]		[0.787]		[0.202]		[0.114]
Auto loan balance ($)	9,526	-1,415	10,644	-477	8,418	-558	7,820	1,788
		[0.229]		[0.703]		[0.604]		[0.144]
Panel F. Sample Size								
Observations†	433	264	447	253	418	287	453	251

Notes: Table reports balance on credit bureau variables (measured in the quarter before treatment assignment) for each quartile of debt eligible for relief (measured in the wave of treatment assignment) in the survey respondent sample, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.
Quartile cutoffs are as follows: Q1: [$500, $798], Q2: [$798, $1,278], Q3: [$1,279, $2,283], Q4: [$2,285, $33,627].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A21. Survey Respondent Sample: Demographic and Collector Variable Balance by Medical Debt Age

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	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)	39.07	-0.87	40.67	1.00	42.29	-1.00	45.43	-0.44
		[0.371]		[0.336]		[0.370]		[0.719]
Male (%)	41.45	-0.02	42.35	-7.05	37.44	0.39	37.98	-5.69
		[0.996]		[0.071]		[0.917]		[0.133]
Panel B. Race and Ethnicity								
Black (%)	14.96	0.15	19.51	3.23	20.09	-0.70	18.43	1.53
		[0.957]		[0.320]		[0.826]		[0.620]
Non-Hispanic white (%)	44.87	0.82	37.92	2.56	47.03	-6.75	46.07	0.91
		[0.834]		[0.510]		[0.081]		[0.818]
Hispanic (any race) (%)	32.05	-0.91	36.81	-5.20	26.48	6.13	29.89	-2.02
		[0.802]		[0.170]		[0.085]		[0.570]
Panel C. Collector Data								
Medical debt ($)	2,135.10	-149.65	2,092.63	-264.36	1,890.59	25.48	2,593.69	-330.65
		[0.412]		[0.139]		[0.884]		[0.244]
Medical debt age (quarters)	3.60	0.04	4.89	0.003	5.23	0.01	7.35	-0.22
		[0.505]		[0.653]		[0.177]		[0.113]
Insured (%)	22.65	-2.99	52.99	3.29	71.62	-3.95	85.17	-2.19
		[0.323]		[0.394]		[0.265]		[0.437]
Panel D. Other								
Response rate (%)	16.85	0.06	18.75	1.22	18.48	2.05	20.98	1.87
		[0.961]		[0.381]		[0.146]		[0.205]
Panel E. Sample Size								
Observations†	468	276	451	258	438	275	445	277

Notes: Table reports balance on demographics and collections account variables for each quartile of the age of debt eligible for relief (measured in the wave of treatment assignment) in the survey respondent sample, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.

Quartile cutoffs in days are as follows: Q1: [198, 429], Q2: [429, 459], Q3: [459, 503], Q4: [503, 1,567].

†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A22. Survey Respondent Sample: Credit Bureau Variable Balance by Medical Debt Age

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Distress								
Number of accounts past due	1.38	0.08	1.74	-0.33	1.59	-0.10	1.42	0.16
		[0.642]		[0.046]		[0.593]		[0.384]
Number of accounts in default	1.25	0.07	1.55	-0.26	1.40	-0.06	1.27	0.15
		[0.664]		[0.097]		[0.724]		[0.383]
Debt past due ($)	4,808	29	6,142	-1,755	6,569	688	6,405	-1,082
		[0.974]		[0.077]		[0.634]		[0.326]
Balances in default ($)	3,830	195	4,620	-886	4,201	366	4,617	-483
		[0.792]		[0.245]		[0.647]		[0.582]
Panel B. Debt in Collections								
Number of debts in collections	4.92	-0.04	4.22	0.25	3.56	-0.28	3.55	0.12
		[0.929]		[0.573]		[0.441]		[0.773]
Debts in collections ($)	4,709	-451	3,467	500	2,948	-528	2,938	435
		[0.404]		[0.320]		[0.173]		[0.371]
Panel C. Bankruptcy								
Bankruptcy in last 12 months (%)	0.66	0.82	0.69	-0.08	1.15	0.63	1.16	0.65
		[0.381]		[0.905]		[0.525]		[0.492]
Panel D. Access to Credit								
Has credit score (%)	98.01	0.10	97.93	1.03	97.46	-0.65	97.68	0.88
		[0.926]		[0.333]		[0.654]		[0.461]
Credit score (never missing)	561.29	-4.87	564.74	4.34	582.08	-1.31	594.30	4.87
		[0.405]		[0.486]		[0.838]		[0.482]
Credit card limit ($)	1,954	412	2,455	942	3,293	546	4,432	-143
		[0.443]		[0.130]		[0.461]		[0.845]
Panel E. Borrowing								
Number of credit cards	0.59	0.16	0.82	0.15	1.03	-0.12	1.08	0.13
		[0.168]		[0.224]		[0.344]		[0.368]
Credit card balance ($)	1,149	419	1,700	143	2,213	-401	2,403	35
		[0.126]		[0.664]		[0.240]		[0.921]
Number of auto loans	0.34	-0.01	0.50	-0.06	0.57	-0.12	0.47	0.07
		[0.821]		[0.274]		[0.047]		[0.210]
Auto loan balance ($)	6,631	319	9,468	-687	10,673	-1,530	9,782	-163
		[0.739]		[0.570]		[0.221]		[0.893]
Panel F. Sample Size								
Observations†	453	266	434	255	428	261	436	273

Notes: Table reports balance on credit bureau variables (measured in the quarter before treatment assignment) for each quartile of the age of debt eligible for relief (measured in the wave of treatment assignment) in the survey respondent sample, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.
Quartile cutoffs in days are as follows: Q1: [198, 429], Q2: [429, 459], Q3: [459, 504], Q4: [504, 1,567].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A23. Survey Respondent Sample: Demographic and Collector Variable Balance by Debt in Collections

	No Debt in Collections		Tercile 1		Tercile 2		Tercile 3	
	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)	43.25	-0.40	42.23	0.50	42.26	-1.46	40.18	0.78
		[0.754]		[0.672]		[0.188]		[0.404]
Male (%)	38.81	-2.23	41.69	-4.53	38.78	-3.50	38.00	-0.70
		[0.567]		[0.250]		[0.372]		[0.853]
Panel B. Race and Ethnicity								
Black (%)	15.53	-1.51	20.62	2.10	19.95	3.90	16.15	2.99
		[0.596]		[0.525]		[0.241]		[0.323]
Non-Hispanic white (%)	48.40	-2.09	44.35	-3.54	42.86	-4.92	40.86	4.69
		[0.597]		[0.374]		[0.221]		[0.225]
Hispanic (any race) (%)	30.37	3.93	28.82	-0.84	31.07	0.81	35.15	-6.73
		[0.292]		[0.819]		[0.825]		[0.061]
Panel C. Collector Data								
Medical debt ($)	2,168.28	-226.72	1,974.92	-2.61	2,202.55	-87.10	2,411.86	-385.01
		[0.337]		[0.990]		[0.703]		[0.058]
Medical debt age (quarters)	5.52	-0.01	5.30	0.16	5.21	-0.15	4.97	-0.02
		[0.966]		[0.227]		[0.256]		[0.823]
Insured (%)	70.94	-2.98	63.86	1.00	53.29	-10.85	43.94	2.07
		[0.415]		[0.794]		[0.007]		[0.597]
Panel D. Other								
Response rate (%)	21.94	2.02	19.73	0.04	18.78	0.68	16.30	2.95
		[0.214]		[0.977]		[0.626]		[0.028]
Panel E. Sample Size								
Observations†	438	268	451	249	441	259	421	279

Notes: Table reports balance on demographics and collections account variables for each quartile of debt in collections in the survey respondent sample, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.
Quartile cutoffs are as follows: Q1: [$0, $0], Q2: [$7, $1,225], Q3: [$1,232, $4,105], Q4: [$4,109, $128,503].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A24. Survey Respondent Sample: Credit Bureau Variable Balance by Debt in Collections

	No Debt in Collections		Tercile 1		Tercile 2		Tercile 3	
	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Distress								
Number of accounts past due	0.86	0.06	1.39	-0.18	1.86	-0.15	2.04	-0.02
		[0.638]		[0.266]		[0.422]		[0.901]
Number of accounts in default	0.70	0.07	1.18	-0.08	1.71	-0.17	1.90	-0.02
		[0.566]		[0.589]		[0.345]		[0.931]
Debt past due ($)	3,590	-313	5,648	-364	7,590	-1,540	7,083	-131
		[0.732]		[0.761]		[0.202]		[0.908]
Balances in default ($)	2,093	230	3,735	-124	5,598	-762	5,889	-423
		[0.657]		[0.868]		[0.411]		[0.613]
Panel B. Debt in Collections								
Number of debts in collections	0.00	-0.00	1.84	0.09	4.20	-0.23	10.56	-0.89
		[.]		[0.433]		[0.229]		[0.086]
Debts in collections ($)	0	0	561	-29	2,469	-48	11,492	-1,236
		[.]		[0.314]		[0.443]		[0.050]
Panel C. Bankruptcy								
Bankruptcy in last 12 months (%)	1.14	2.35	1.33	-1.00	0.68	0.13	0.48	1.14
		[0.070]		[0.105]		[0.881]		[0.167]
Panel D. Access to Credit								
Has credit score (%)	91.10	0.67	100.00	0.00	100.00	0.00	100.00	-0.00
		[.]		[.]		[.]		[.]
Credit score (never missing)	641.51	5.17	572.99	3.72	554.12	-2.16	537.55	-0.27
		[0.476]		[0.533]		[0.660]		[0.951]
Credit card limit ($)	7,574	942	2,656	115	1,207	205	569	440
		[0.381]		[0.829]		[0.605]		[0.106]
Panel E. Borrowing								
Number of credit cards	1.64	0.18	0.93	0.11	0.56	-0.004	0.36	0.06
		[0.292]		[0.415]		[0.969]		[0.521]
Credit card balance ($)	3,696	86	1,810	-495	1,043	97	848	256
		[0.861]		[0.060]		[0.670]		[0.255]
Number of auto loans	0.55	0.05	0.52	-0.06	0.46	-0.09	0.35	-0.003
		[0.389]		[0.325]		[0.106]		[0.945]
Auto loan balance ($)	10,798	460	9,414	-534	8,903	-1,866	7,243	39
		[0.744]		[0.656]		[0.094]		[0.967]
Panel F. Sample Size								
Observations†	438	268	451	249	441	259	421	279

Notes: Table reports balance on credit bureau variables (measured in the quarter before treatment assignment) for each quartile of debt in collections in the survey respondent sample, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets. Quartile cutoffs are as follows: Q1: [$0, $0], Q2: [$7, $1,225], Q3: [$1,232, $4,105], Q4: [$4,109, $128,503].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A25. Collector Debt Experiment: Demographic and Collector Variable Balance by Beneficiary Age

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	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)	30.86	-0.01	39.77	0.07	50.37	-0.06	64.97	0.01
		[0.754]		[0.022]		[0.044]		[0.896]
Male (%)	37.34	-1.49	41.76	1.00	46.12	-0.09	48.61	0.58
		[0.264]		[0.554]		[0.959]		[0.715]
Panel B. Collector Data								
Medical debt ($)	1,934.98	25.39	2,268.92	-46.20	2,469.47	34.83	2,739.03	-10.91
		[0.449]		[0.301]		[0.519]		[0.867]
Medical debt age (quarters)	23.73	-0.004	28.82	0.03	28.53	-0.05	28.73	-0.11
		[0.926]		[0.703]		[0.416]		[0.108]
Panel C. Sample Size								
Observations†	15,742	16,167	14,818	15,044	15,236	15,536	14,028	14,116

Notes: Table reports balance on demographics and collections account variables for each quartile of debtor age in the collector debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.
Quartile cutoffs in years are as follows: Q1: [18, 35], Q2: [36, 44], Q3: [45, 56], Q4: [57, 89].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A26. Collector Debt Experiment: Credit Bureau Variable Balance by Beneficiary Age

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Distress								
Number of accounts past due	1.18	0.01	1.21	0.03	1.02	0.002	0.69	0.01
		[0.598]		[0.140]		[0.921]		[0.499]
Number of accounts in default	1.10	0.001	1.10	0.03	0.91	0.001	0.61	0.003
		[0.945]		[0.123]		[0.946]		[0.851]
Debt past due ($)	4,482	-6	6,536	-33	6,087	89	3,548	252
		[0.962]		[0.862]		[0.649]		[0.114]
Balances in default ($)	3,805	-17	5,240	-43	4,547	-31	2,404	189
		[0.875]		[0.774]		[0.831]		[0.086]
Panel B. Debt in Collections								
Number of debts in collections	4.89	-0.04	4.95	0.10	4.82	0.08	3.65	0.01
		[0.492]		[0.153]		[0.257]		[0.865]
Debts in collections ($)	4,404	-66	4,493	143	4,435	99	3,245	-3
		[0.386]		[0.102]		[0.262]		[0.971]
Panel C. Bankruptcy								
Bankruptcy in last 12 months (%)	0.28	-0.10	0.33	0.05	0.43	-0.07	0.31	0.03
		[0.074]		[0.443]		[0.330]		[0.621]
Panel D. Access to Credit								
Has credit score (%)	95.83	0.42	95.31	-0.11	95.00	-0.40	91.70	0.63
		[0.058]		[0.666]		[0.120]		[0.052]
Credit score (never missing)	552.10	-0.66	560.96	-0.34	571.44	-0.30	594.07	-0.43
		[0.370]		[0.672]		[0.709]		[0.642]
Credit card limit ($)	989	99	1,762	44	2,135	19	2,688	63
		[0.033]		[0.519]		[0.809]		[0.495]
Panel E. Borrowing								
Number of credit cards	0.44	0.02	0.62	0.004	0.70	-0.01	0.71	0.01
		[0.158]		[0.776]		[0.582]		[0.471]
Credit card balance ($)	595	45	1,041	16	1,213	-60	1,236	40
		[0.044]		[0.640]		[0.100]		[0.315]
Number of auto loans	0.26	0.004	0.30	-0.01	0.31	-0.003	0.24	0.01
		[0.466]		[0.362]		[0.969]		[0.031]
Auto loan balance ($)	4,659	139	5,548	-90	5,576	-46	4,068	228
		[0.181]		[0.457]		[0.709]		[0.042]
Panel F. Sample Size								
Observations†	15,338	15,727	14,456	14,708	14,822	15,145	13,671	13,793

Notes: Table reports balance on credit bureau variables (measured in the quarter before treatment assignment) for each quartile of debtor age in the collector debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets. Quartile cutoffs in years are as follows: Q1: [18, 35], Q2: [36, 44], Q3: [45, 56], Q4: [57, 89].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A27. Collector Debt Experiment: Demographic and Collector Variable Balance by Medical Debt Eligible for Relief

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	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)	48.00	-0.05	45.05	0.02	45.17	-0.10	46.24	-0.06
		[0.570]		[0.851]		[0.179]		[0.445]
Male (%)	42.41	-1.51	43.12	-0.22	43.31	-0.39	44.72	1.56
		[0.340]		[0.884]		[0.800]		[0.315]
Panel B. Collector Data								
Medical debt ($)	161.71	-0.38	528.99	2.50	1,332.90	-7.89	6,642.83	3.30
		[0.608]		[0.116]		[0.007]		[0.968]
Medical debt age (quarters)	29.67	-0.01	27.60	-0.07	27.96	-0.09	27.55	0.05
		[0.910]		[0.244]		[0.111]		[0.394]
Panel C. Sample Size								
Observations†	17,072	17,361	16,852	17,234	17,093	17,168	16,997	17,261

Notes: Table reports balance on demographics and collections account variables for each quartile of debt eligible for relief (measured in the wave of treatment assignment) in the collector debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated p-values reported below in brackets.

Quartile cutoffs are as follows: Q1: [$5, $300], Q2: [$300, $820], Q3: [$820, $2,073], Q4: [$2,073, $156,988].

†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A28. Collector Debt Experiment: Credit Bureau Variable Balance by Medical Debt Eligible for Relief

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Distress								
Number of accounts past due	1.08	0.03	1.08	-0.002	0.99	0.03	0.92	0.01
		[0.122]		[0.924]		[0.102]		[0.667]
Number of accounts in default	0.95	0.02	0.98	-0.005	0.91	0.02	0.84	0.01
		[0.216]		[0.811]		[0.218]		[0.594]
Debt past due ($)	5,637	246	5,405	-2	4,892	123	4,627	-18
		[0.177]		[0.991]		[0.418]		[0.903]
Balances in default ($)	3,869	150	4,139	-57	3,931	-4	3,752	31
		[0.232]		[0.643]		[0.972]		[0.793]
Panel B. Debt in Collections								
Number of debts in collections	3.08	0.13	3.87	0.02	4.45	-0.04	5.94	0.02
		[0.004]		[0.699]		[0.530]		[0.754]
Debts in collections ($)	2,303	97	3,167	16	3,902	-42	6,219	116
		[0.067]		[0.804]		[0.560]		[0.256]
Panel C. Bankruptcy								
Bankruptcy in last 12 months (%)	0.39	-0.01	0.41	0.06	0.31	-0.11	0.26	0.05
		[0.888]		[0.412]		[0.053]		[0.438]
Panel D. Access to Credit								
Has credit score (%)	93.89	0.44	93.71	0.19	93.73	-0.07	93.97	0.12
		[0.090]		[0.465]		[0.778]		[0.656]
Credit score (never missing)	589.93	-0.96	572.43	1.08	565.87	-1.90	561.02	0.07
		[0.286]		[0.189]		[0.012]		[0.926]
Credit card limit ($)	3,681	20	2,293	189	1,607	9	1,119	52
		[0.836]		[0.016]		[0.884]		[0.316]
Panel E. Borrowing								
Number of credit cards	0.97	0.02	0.69	0.02	0.55	-0.004	0.43	0.01
		[0.329]		[0.280]		[0.787]		[0.542]
Credit card balance ($)	1,766	30	1,187	44	889	-15	656	-16
		[0.477]		[0.211]		[0.615]		[0.530]
Number of auto loans	0.38	0.01	0.30	0.005	0.26	-0.003	0.21	0.003
		[0.055]		[0.436]		[0.578]		[0.606]
Auto loan balance ($)	6,557	122	5,326	124	4,606	-49	3,778	69
		[0.323]		[0.273]		[0.639]		[0.477]
Panel F. Sample Size								
Observations†	16,052	16,317	16,010	16,388	16,453	16,548	16,515	16,788

Notes: Table reports balance on credit bureau variables (measured in the quarter before treatment assignment) for each quartile of debt eligible for relief (measured in the wave of treatment assignment) in the collector debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.

Quartile cutoffs are as follows: Q1: [$5, $305], Q2: [$305, $837], Q3: [$837, $2,110], Q4: [$2,110, $156,988].

†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A29. Collector Debt Experiment: Demographic and Collector Variable Balance by Medical Debt Age

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	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)	45.10	-0.09	44.81	0.01	46.13	-0.13	48.94	0.04
		[0.288]		[0.918]		[0.083]		[0.660]
Male (%)	42.84	-0.31	43.28	-0.63	49.04	0.51	56.99	3.98
		[0.715]		[0.776]		[0.873]		[0.556]
Panel B. Collector Data								
Medical debt ($)	1,916.51	0.48	2,195.25	-36.00	2,834.81	103.01	1,710.47	-41.16
		[0.988]		[0.436]		[0.065]		[0.275]
Medical debt age (quarters)	18.76	-0.02	23.58	-0.002	25.51	0.01	44.92	-0.10
		[0.540]		[0.622]		[0.176]		[0.234]
Panel C. Sample Size								
Observations†	16,984	17,275	17,023	17,352	16,972	17,183	17,034	17,213

Notes: Table reports balance on demographics and collections account variables for each quartile of the age of debt eligible for relief (measured in the wave of treatment assignment) in the collector debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.

Quartile cutoffs in days are as follows: Q1: [987, 2,063], Q2: [2,063, 2,212], Q3: [2,212, 2,599], Q4: [2,599, 8,554].

†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A30. Collector Debt Experiment: Credit Bureau Variable Balance by Medical Debt Age

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Distress								
Number of accounts past due	1.15	-0.0003	0.99	0.05	0.96	0.01	0.96	0.02
		[0.990]		[0.019]		[0.776]		[0.254]
Number of accounts in default	1.05	-0.002	0.91	0.04	0.87	-0.001	0.85	0.02
		[0.924]		[0.039]		[0.962]		[0.272]
Debt past due ($)	5,644	-161	5,025	280	5,073	33	4,821	238
		[0.328]		[0.071]		[0.836]		[0.151]
Balances in default ($)	4,239	-132	3,995	222	3,888	-15	3,570	79
		[0.282]		[0.067]		[0.900]		[0.515]
Panel B. Debt in Collections								
Number of debts in collections	4.53	-0.01	5.14	0.07	4.71	0.04	2.97	0.07
		[0.867]		[0.276]		[0.575]		[0.197]
Debts in collections ($)	4,169	20	4,722	45	4,349	19	2,344	108
		[0.793]		[0.581]		[0.814]		[0.070]
Panel C. Bankruptcy								
Bankruptcy in last 12 months (%)	0.53	-0.05	0.27	-0.05	0.30	0.03	0.27	0.04
		[0.524]		[0.401]		[0.587]		[0.457]
Panel D. Access to Credit								
Has credit score (%)	94.96	0.29	95.56	0.02	94.60	0.28	90.11	0.14
		[0.223]		[0.932]		[0.255]		[0.672]
Credit score (never missing)	569.22	-0.07	562.70	-0.42	570.78	-0.30	587.32	-0.87
		[0.934]		[0.554]		[0.687]		[0.344]
Credit card limit ($)	2,062	154	1,329	68	1,802	136	3,506	-91
		[0.038]		[0.231]		[0.040]		[0.342]
Panel E. Borrowing								
Number of credit cards	0.61	0.02	0.50	0.01	0.61	0.02	0.92	-0.02
		[0.095]		[0.347]		[0.175]		[0.269]
Credit card balance ($)	1,088	48	758	15	994	12	1,658	-38
		[0.152]		[0.581]		[0.702]		[0.362]
Number of auto loans	0.30	0.01	0.26	0.00	0.28	0.01	0.32	-0.003
		[0.195]		[0.549]		[0.134]		[0.623]
Auto loan balance ($)	5,593	-14	4,589	81	4,777	239	5,298	-36
		[0.903]		[0.444]		[0.025]		[0.751]
Panel F. Sample Size								
Observations†	16,623	16,901	16,716	17,055	16,410	16,651	15,280	15,434

Notes: Table reports balance on credit bureau variables (measured in the quarter before treatment assignment) for each quartile of the age of debt eligible for relief (measured in the wave of treatment assignment) in the collector debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.
Quartile cutoffs in days are as follows: Q1: [987, 2,057], Q2: [2,057, 2,207], Q3: [2,207, 2,520], Q4: [2,520, 8,554].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A31. Collector Debt Experiment: Demographic and Collector Variable Balance by Debt in Collections

	No Debt in Collections		Tercile 1		Tercile 2		Tercile 3	
	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)	49.14	-0.06	47.29	0.07	44.88	-0.12	43.92	-0.08
		[0.568]		[0.406]		[0.104]		[0.249]
Male (%)	49.76	-2.78	42.02	1.04	40.88	0.98	41.82	-1.43
		[0.140]		[0.496]		[0.520]		[0.327]
Panel B. Collector Data								
Medical debt ($)	1,594.42	19.22	1,594.85	19.24	2,092.95	-33.65	3,405.87	22.09
		[0.619]		[0.575]		[0.381]		[0.725]
Medical debt age (quarters)	31.50	-0.05	28.11	-0.05	26.91	-0.07	25.60	0.03
		[0.453]		[0.387]		[0.214]		[0.607]
Panel C. Sample Size								
Observations†	14,055	14,277	16,974	17,283	16,962	17,275	17,039	17,206

Notes: Table reports balance on demographics and collections account variables for each quartile of debt in collections in the collector debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets.
Quartile cutoffs are as follows: Q1: [$0, $0], Q2: [$1, $1,252], Q3: [$1,253, $4,047], Q4: [$4,048, $2,079,212].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A32. Collector Debt Experiment: Credit Bureau Variable Balance by Debt in Collections

	No Debt in Collections		Tercile 1		Tercile 2		Tercile 3	
	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference	Control Mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Distress								
Number of accounts past due	0.57	0.01	0.89	0.02	1.19	0.01	1.33	0.04
		[0.735]		[0.222]		[0.770]		[0.057]
Number of accounts in default	0.49	-0.002	0.78	0.02	1.08	0.001	1.25	0.04
		[0.915]		[0.333]		[0.974]		[0.049]
Debt past due ($)	3,059	-30	4,838	182	5,872	-92	6,439	283
		[0.839]		[0.262]		[0.572]		[0.091]
Balances in default ($)	2,067	11	3,396	70	4,500	-107	5,411	154
		[0.915]		[0.542]		[0.386]		[0.257]
Panel B. Debt in Collections								
Number of debts in collections	0.02	0.01	1.87	0.003	4.16	0.04	10.53	0.08
		[0.033]		[0.833]		[0.159]		[0.345]
Debts in collections ($)	0	0	581	3	2,399	11	11,916	142
		[.]		[0.428]		[0.202]		[0.188]
Panel C. Bankruptcy								
Bankruptcy in last 12 months (%)	0.72	0.24	0.37	-0.09	0.17	-0.05	0.18	-0.05
		[0.029]		[0.129]		[0.193]		[0.256]
Panel D. Access to Credit								
Has credit score (%)	71.36	0.57	100.00	0.00	100.00	-0.00	100.00	-0.00
		[.]		[.]		[.]		[.]
Credit score (never missing)	647.21	0.003	577.13	-0.45	553.32	-0.62	542.36	-0.55
		[0.998]		[0.509]		[0.316]		[0.331]
Credit card limit ($)	5,490	166	2,256	68	993	44	535	18
		[0.198]		[0.340]		[0.310]		[0.563]
Panel E. Borrowing								
Number of credit cards	1.25	0.02	0.76	0.02	0.47	0.01	0.27	-0.004
		[0.483]		[0.215]		[0.274]		[0.674]
Credit card balance ($)	2,187	-30	1,239	41	740	24	517	7
		[0.567]		[0.231]		[0.328]		[0.736]
Number of auto loans	0.34	0.02	0.33	0.002	0.28	0.005	0.22	-0.003
		[0.026]		[0.695]		[0.401]		[0.557]
Auto loan balance ($)	5,711	293	5,503	-48	4,980	105	4,183	-41
		[0.029]		[0.671]		[0.319]		[0.657]
Panel F. Sample Size								
Observations†	14,055	14,277	16,974	17,283	16,962	17,275	17,039	17,206

Notes: Table reports balance on credit bureau variables (measured in the quarter before treatment assignment) for each quartile of debt in collections in the collector debt experiment, as specified in Equation 7. Control means are reported in odd columns, and difference estimates are reported in even columns with associated *p*-values reported below in brackets. Quartile cutoffs are as follows: Q1: [$0, $0], Q2: [$1, $1,252], Q3: [$1,253, $4,047], Q4: [$4,048, $2,079,212].
†: Sample size for the control and treatment groups reported in odd and even columns respectively.

Table A33. Effects of Debt Relief on Other Pre-Registered 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						
At least one debt past due (%)	48.04	-0.19	[0.650]	39.94	0.02	[0.917]
		(0.41)			(0.23)	
At least one debt in default (%)	44.83	-0.09	[0.827]	37.21	0.01	[0.977]
		(0.40)			(0.23)	
Panel B. Debt in Collections						
At least one debt in collections (%)	79.82	-0.22	[0.561]	70.79	-0.02	[0.912]
		(0.39)			(0.22)	
Panel C. Borrowing						
Count of loans	2.89	0.03	[0.144]	2.51	0.01	[0.214]
		(0.02)			(0.01)	
Total loan balance ($)	34,089	-272	[0.442]	27,834	-24	[0.892]
		(353)			(180)	
At least one credit card (%)	34.33	0.24	[0.481]	30.50	-0.23	[0.194]
		(0.34)			(0.18)	
Count of mortgages	0.11	-0.001	[0.581]	0.09	-0.0005	[0.584]
		(0.002)			(0.001)	
Mortgage balances ($)	15,105	-213	[0.485]	12,267	-74	[0.639]
		(306)			(158)	
Panel D. Sample Size						
Observations†	55,653	12,998		64,947	65,968	

Notes: Table presents the effects of medical debt relief on other pre-registered credit bureau outcomes, estimated using the baseline specification in Equation 1. Columns (1) and (4) report the control means for the hospital debt and collector debt experiments, respectively, calculated in the fourth quarter post-treatment. Columns (2) and (5) report the treatment effects measured in the fourth quarter post-treatment, with standard errors clustered at the person level 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.

Table A34. Effects of Debt Relief on Credit Bureau Outcomes (No Person Fixed Effects)

	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 ($)	4,119	-60	[0.350]	3,112	41	[0.226]
		(64)	{0.483}		(34)	{0.327}
Panel C. Bankruptcy						
Bankruptcy in last 12 months (%)	1.30	-0.05	[0.670]	0.65	-0.05	[0.287]
		(0.11)	.		(0.04)	.
Panel D. Access to Credit						
Has credit score (%)	97.22	-0.11	[0.492]	90.73	0.10	[0.544]
		(0.16)	{0.786}		(0.16)	{0.660}
Credit score (never missing)	582.16	-0.10	[0.890]	577.60	-0.35	[0.416]
		(0.76)	{0.894}		(0.42)	{0.660}
Credit card limit ($)	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	0.005	[0.737]	0.78	0.01	[0.185]
		(0.01)	{0.926}		(0.01)	{0.363}
Credit card balance ($)	1,481	27	[0.469]	1,306	35	[0.057]
		(37)	{0.868}		(18)	{0.171}
Number of auto loans	0.39	0.003	[0.603]	0.30	0.004	[0.167]
		(0.01)	{0.904}		(0.003)	{0.363}
Auto loan balance ($)	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 presents the effects of medical debt relief on credit bureau outcomes as estimated in Equation 6, which drops the person fixed effects from the baseline model (see Appendix Section C.1 for details). Columns (1) and (4) report the control means for the hospital debt and collector debt experiments, respectively, calculated in the fourth quarter post-treatment. Columns (2) and (5) report the treatment effects, with robust 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.

Table A35. Effects of Debt Relief on Credit Bureau Outcomes (No Person Fixed Effects, Saturated with Controls)

	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	-0.004	[0.788]	1.02	0.002	[0.831]
		(0.02)			(0.01)	
Number of accounts in default	1.08	-0.01	[0.693]	0.92	0.002	[0.840]
		(0.01)			(0.01)	
Debt past due ($)	4,908	58	[0.627]	4,815	17	[0.812]
		(119)			(70)	
Balances in default ($)	3,741	6	[0.944]	3,705	-6	[0.903]
		(85)			(52)	
Panel B. Debt in Collections						
Number of debts in collections	4.66	-0.02	[0.606]	3.55	-0.02	[0.397]
		(0.04)			(0.02)	
Debts in collections ($)	4,119	-70	[0.166]	3,112	19	[0.506]
		(50)			(28)	
Panel C. Bankruptcy						
Bankruptcy in last 12 months (%)	1.30	-0.05	[0.616]	0.65	-0.05	[0.245]
		(0.11)			(0.04)	
Panel D. Access to Credit						
Has credit score (%)	97.22	-0.10	[0.465]	90.73	-0.12	[0.332]
		(0.14)			(0.12)	
Credit score (never missing)	582.16	-0.20	[0.662]	577.60	-0.18	[0.506]
		(0.46)			(0.28)	
Credit card limit ($)	2,654	58	[0.318]	2,640	55	[0.078]
		(58)			(31)	
Panel E. Borrowing						
Number of credit cards	0.81	0.01	[0.605]	0.78	0.01	[0.342]
		(0.01)			(0.01)	
Credit card balance ($)	1,481	27	[0.406]	1,306	27	[0.087]
		(32)			(16)	
Number of auto loans	0.39	0.003	[0.540]	0.30	0.002	[0.479]
		(0.01)			(0.003)	
Auto loan balance ($)	8,020	-53	[0.649]	5,417	-6	[0.902]
		(116)			(49)	
Panel F. Sample Size						
Observations†	55,653	12,998		64,947	65,968	

Notes: Table presents the effects of medical debt relief on credit bureau outcomes as estimated in Equation 6. This specification drops the person fixed effects from the baseline model and saturates the model with controls observed prior to treatment (see Appendix Section C.1 for detail). Controls include gender, insurance status (in the hospital debt experiment), state, age, 25-point credit score bins, 25-point collections score bins, indicator for an open mortgage, log non-mortgage debt, log non-medical debt in collections, and log medical debt in collections. Treatment effects are estimated by comparing outcomes 12 months after treatment to pre-treatment levels. Columns (1) and (4) report the control means for the hospital debt and collector debt experiments, respectively, calculated in the fourth quarter post-treatment. Columns (2) and (5) report treatment effects, with robust standard errors below in parentheses. Columns (3) and (6) report unadjusted *p*-values.
†: Sample sizes for control and treatment groups reported in the control mean and treatment effect columns, respectively.

Table A36. Heterogeneous Effects of Debt Relief on Credit Bureau Outcomes in the Hospital Debt Experiment, by Medical Debt Eligible for Relief

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Distress												
Number of accounts past due	1.20	-0.02	[0.456]	1.25	-0.07	[0.020]	1.19	0.03	[0.324]	1.17	0.01	[0.691]
		(0.03)	.		(0.03)	.		(0.03)	.		(0.03)	.
Number of accounts in default	1.07	-0.02	[0.506]	1.12	-0.06	[0.043]	1.08	0.02	[0.515]	1.06	-0.001	[0.969]
		(0.03)	{0.538}		(0.03)	{0.121}		(0.03)	{0.763}		(0.03)	{0.999}
Debt past due ($)	4,650	470	[0.035]	4,976	-444	[0.060]	5,027	-48	[0.845]	4,983	21	[0.929]
		(223)	{0.094}		(236)	{0.121}		(242)	{0.846}		(230)	{0.999}
Balances in default ($)	3,413	239	[0.103]	3,797	-140	[0.354]	3,873	-121	[0.416]	3,883	124	[0.413]
		(147)	{0.205}		(151)	{0.358}		(149)	{0.729}		(152)	{0.731}
Panel B. Debt in Collections												
Number of debts in collections	3.86	-0.05	[0.431]	4.43	0.07	[0.369]	4.67	-0.06	[0.451]	5.68	-0.04	[0.650]
		(0.06)	{0.445}		(0.07)	{0.529}		(0.07)	{0.614}		(0.09)	{0.657}
Debts in collections ($)	2,977	-132	[0.069]	3,761	78	[0.378]	4,106	12	[0.893]	5,636	-116	[0.322]
		(72)	{0.114}		(89)	{0.529}		(88)	{0.900}		(117)	{0.468}
Panel C. Bankruptcy												
Bankruptcy in last 12 months (%)	1.36	-0.11	[0.699]	1.44	-0.20	[0.467]	1.35	-0.22	[0.386]	1.03	0.05	[0.825]
		(0.28)	.		(0.28)	.		(0.26)	.		(0.23)	.
Panel D. Access to Credit												
Has credit score (%)	96.64	-0.71	[0.039]	97.58	0.27	[0.408]	97.35	0.32	[0.339]	97.32	0.12	[0.735]
		(0.35)	{0.112}		(0.32)	{0.387}		(0.34)	{0.664}		(0.36)	{0.983}
Credit score (never missing)	591.51	-0.42	[0.672]	580.59	1.84	[0.070]	580.38	-0.89	[0.378]	576.67	-0.26	[0.798]
		(1.00)	{0.682}		(1.01)	{0.199}		(1.01)	{0.664}		(1.02)	{0.983}
Credit card limit ($)	3,147	118	[0.135]	2,694	112	[0.129]	2,556	-72	[0.325]	2,218	12	[0.849]
		(79)	{0.247}		(74)	{0.227}		(73)	{0.664}		(61)	{0.983}
Panel E. Borrowing												
Number of credit cards	0.90	0.03	[0.060]	0.84	0.02	[0.389]	0.80	0.01	[0.637]	0.70	0.02	[0.170]
		(0.02)	{0.208}		(0.02)	{0.842}		(0.02)	{0.953}		(0.02)	{0.500}
Credit card balance ($)	1,673	-1	[0.987]	1,484	-13	[0.769]	1,454	-14	[0.754]	1,312	41	[0.335]
		(51)	{0.982}		(45)	{0.985}		(44)	{0.953}		(43)	{0.572}
Number of auto loans	0.40	-0.002	[0.827]	0.42	0.002	[0.794]	0.40	0.01	[0.236]	0.35	0.01	[0.186]
		(0.01)	{0.967}		(0.01)	{0.985}		(0.01)	{0.613}		(0.01)	{0.500}
Auto loan balance ($)	7,910	-110	[0.555]	8,479	4	[0.983]	8,167	96	[0.638]	7,529	-153	[0.425]
		(187)	{0.896}		(199)	{0.985}		(204)	{0.953}		(192)	{0.572}
Panel F. Sample Size												
Observations†	14,004	3,257		13,829	3,236		13,877	3,297		13,943	3,208	

Notes: Table presents the heterogeneous effects of medical debt relief on credit outcomes by quartile of medical debt eligible for relief (measured in the wave of treatment assignment) in the hospital debt experiment, as specified in Equation 7. The first column of each quartile reports the control means for observations in that quartile, calculated in the fourth quarter post-treatment. The second column reports the treatment effects for that quartile, with standard errors clustered at the person level below in parentheses. The third column reports unadjusted *p*-values 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 7.
Quartile cutoffs are as follows: Q1: [$25, $226], Q2: [$226, $600], Q3: [$600, $1,440], Q4: [$1,440, $60,452].
†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A37. Heterogeneous Effects of Debt Relief on Credit Bureau Outcomes in the Hospital Debt Experiment, by Medical Debt Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Distress												
Number of accounts past due	1.20	-0.01	[0.796]	1.26	0.01	[0.853]	1.25	-0.06	[0.034]	1.11	0.02	[0.604]
		(0.03)	.		(0.03)	.		(0.03)	.		(0.03)	.
Number of accounts in default	1.08	-0.02	[0.505]	1.13	0.02	[0.574]	1.12	-0.06	[0.035]	1.00	0.004	[0.883]
		(0.03)	{0.844}		(0.03)	{0.637}		(0.03)	{0.075}		(0.03)	{0.995}
Debt past due ($)	4,732	125	[0.567]	5,246	191	[0.392]	5,049	-339	[0.182]	4,607	49	[0.836]
		(218)	{0.844}		(223)	{0.637}		(254)	{0.278}		(239)	{0.995}
Balances in default ($)	3,736	-76	[0.602]	3,955	311	[0.030]	3,866	-148	[0.350]	3,409	28	[0.855]
		(146)	{0.844}		(143)	{0.084}		(158)	{0.359}		(151)	{0.995}
Panel B. Debt in Collections												
Number of debts in collections	5.62	-0.04	[0.612]	4.73	0.03	[0.668]	4.43	-0.02	[0.807]	3.87	-0.04	[0.561]
		(0.08)	{0.752}		(0.07)	{0.759}		(0.07)	{0.802}		(0.07)	{0.540}
Debts in collections ($)	5,489	-59	[0.576]	4,090	52	[0.568]	3,711	-57	[0.519]	3,203	-93	[0.262]
		(106)	{0.752}		(90)	{0.759}		(89)	{0.691}		(83)	{0.387}
Panel C. Bankruptcy												
Bankruptcy in last 12 months (%)	1.43	-0.25	[0.341]	1.21	-0.07	[0.780]	1.20	0.09	[0.743]	1.34	-0.24	[0.376]
		(0.26)	.		(0.26)	.		(0.26)	.		(0.27)	.
Panel D. Access to Credit												
Has credit score (%)	97.90	-0.26	[0.441]	97.85	0.24	[0.478]	97.08	-0.15	[0.671]	96.07	0.15	[0.665]
		(0.33)	{0.829}		(0.34)	{0.870}		(0.35)	{0.881}		(0.35)	{0.884}
Credit score (never missing)	569.72	0.08	[0.938]	578.95	-0.18	[0.852]	583.43	0.68	[0.505]	597.09	-0.26	[0.807]
		(1.00)	{0.938}		(0.98)	{0.870}		(1.02)	{0.881}		(1.05)	{0.884}
Credit card limit ($)	1,958	19	[0.759]	2,492	35	[0.607]	2,658	45	[0.542]	3,498	61	[0.478]
		(61)	{0.935}		(68)	{0.870}		(73)	{0.881}		(86)	{0.847}
Panel E. Borrowing												
Number of credit cards	0.64	0.01	[0.599]	0.79	0.01	[0.428]	0.85	0.02	[0.190]	0.96	0.04	[0.080]
		(0.02)	{0.941}		(0.02)	{0.879}		(0.02)	{0.456}		(0.02)	{0.265}
Credit card balance ($)	1,176	-16	[0.666]	1,438	29	[0.496]	1,522	-37	[0.457]	1,784	27	[0.609]
		(38)	{0.941}		(43)	{0.879}		(50)	{0.704}		(53)	{0.922}
Number of auto loans	0.35	0.002	[0.859]	0.40	0.003	[0.723]	0.41	0.01	[0.144]	0.41	0.004	[0.693]
		(0.01)	{0.941}		(0.01)	{0.879}		(0.01)	{0.448}		(0.01)	{0.922}
Auto loan balance ($)	7,146	-109	[0.536]	8,288	-111	[0.559]	8,199	74	[0.712]	8,439	-41	[0.850]
		(177)	{0.941}		(191)	{0.879}		(201)	{0.707}		(215)	{0.922}
Panel F. Sample Size												
Observations†	13,816	3,402		13,952	3,443		13,864	3,146		14,021	3,006	

Notes: Table presents the heterogeneous effects of medical debt relief on credit outcomes by quartile of the age of medical debt eligible for relief (measured in the wave of treatment assignment) in the hospital debt experiment, as estimated in Equation 7. We restrict the sample to quarters $[-4,4]$ relative to treatment assignment and drop quarters $[0,3]$, so treatment effects are estimated by comparing outcomes 12 months after treatment to pre-treatment levels. The first column of each quartile reports the control means for observations in that quartile, calculated in the fourth quarter post-treatment. The second column reports the treatment effects for that quartile, with standard errors clustered at the person level below in parentheses. The third column reports unadjusted *p*-values 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.

Quartile cutoffs in days are as follows: Q1: [130, 428], Q2: [428, 459], Q3: [459, 498], Q4: [498, 2,177].

†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A38. Heterogeneous Effects of Debt Relief on Credit Bureau Outcomes in the Hospital Debt Experiment, by Beneficiary Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Distress												
Number of accounts past due	1.12	-0.01	[0.763]	1.38	0.03	[0.447]	1.33	-0.05	[0.150]	0.99	-0.03	[0.365]
		(0.03)	.		(0.03)	.		(0.03)	.		(0.03)	.
Number of accounts in default	1.02	-0.01	[0.728]	1.25	0.01	[0.749]	1.19	-0.04	[0.176]	0.89	-0.02	[0.411]
		(0.02)	{0.969}		(0.03)	{0.773}		(0.03)	{0.277}		(0.03)	{0.651}
Debt past due ($)	3,710	-14	[0.924]	6,000	517	[0.049]	6,295	-542	[0.055]	3,694	33	[0.880]
		(152)	{0.979}		(263)	{0.132}		(282)	{0.105}		(220)	{0.895}
Balances in default ($)	2,982	18	[0.870]	4,683	162	[0.340]	4,481	-223	[0.208]	2,869	134	[0.328]
		(107)	{0.979}		(170)	{0.534}		(177)	{0.277}		(137)	{0.651}
Panel B. Debt in Collections												
Number of debts in collections	4.51	0.005	[0.949]	5.39	-0.13	[0.126]	5.12	0.04	[0.638]	3.65	0.03	[0.616]
		(0.07)	{0.953}		(0.09)	{0.192}		(0.08)	{0.823}		(0.07)	{0.845}
Debts in collections ($)	4,156	-46	[0.585]	4,920	-53	[0.609]	4,549	-32	[0.755]	2,879	-2	[0.984]
		(84)	{0.774}		(103)	{0.613}		(102)	{0.823}		(84)	{0.988}
Panel C. Bankruptcy												
Bankruptcy in last 12 months (%)	1.04	-0.19	[0.350]	1.47	-0.10	[0.746]	1.80	-0.16	[0.580]	0.89	-0.06	[0.817]
		(0.21)	.		(0.30)	.		(0.29)	.		(0.24)	.
Panel D. Access to Credit												
Has credit score (%)	97.88	0.002	[0.995]	98.41	-0.36	[0.199]	98.17	0.37	[0.189]	94.63	0.05	[0.897]
		(0.37)	{0.997}		(0.28)	{0.494}		(0.28)	{0.318}		(0.41)	{0.968}
Credit score (never missing)	568.19	-0.40	[0.702]	574.65	-1.11	[0.294]	583.02	1.89	[0.049]	604.34	-0.23	[0.815]
		(1.05)	{0.911}		(1.06)	{0.499}		(0.96)	{0.143}		(1.00)	{0.968}
Credit card limit ($)	1,553	51	[0.359]	2,627	-30	[0.716]	3,184	32	[0.640]	3,335	104	[0.200]
		(56)	{0.754}		(82)	{0.743}		(69)	{0.624}		(82)	{0.492}
Panel E. Borrowing												
Number of credit cards	0.60	0.003	[0.860]	0.85	0.01	[0.649]	0.97	0.05	[0.010]	0.83	0.02	[0.220]
		(0.02)	{0.885}		(0.02)	{0.863}		(0.02)	{0.042}		(0.02)	{0.450}
Credit card balance ($)	926	-13	[0.693]	1,538	84	[0.103]	1,866	-112	[0.023]	1,630	67	[0.176]
		(32)	{0.885}		(51)	{0.317}		(49)	{0.067}		(50)	{0.450}
Number of auto loans	0.36	-0.01	[0.259]	0.42	0.01	[0.536]	0.47	0.01	[0.402]	0.33	0.02	[0.015]
		(0.01)	{0.565}		(0.01)	{0.863}		(0.01)	{0.557}		(0.01)	{0.057}
Auto loan balance ($)	7,123	-286	[0.101]	9,028	137	[0.542]	9,666	-46	[0.826]	6,331	52	[0.766]
		(174)	{0.313}		(225)	{0.863}		(207)	{0.818}		(175)	{0.779}
Panel F. Sample Size												
Observations†	14,460	3,366		13,501	3,125		14,172	3,351		13,192	3,082	

Notes: Table presents the heterogeneous effects of medical debt relief on credit outcomes by quartile of debtor age (measured in the wave of treatment assignment) in the hospital debt experiment, as estimated in Equation 7. We restrict the sample to quarters $[-4,4]$ relative to treatment assignment and drop quarters $[0,3]$, so treatment effects are estimated by comparing outcomes 12 months after treatment to pre-treatment levels. The first column of each quartile reports the control means for observations in that quartile, calculated in the fourth quarter post-treatment. The second column reports the treatment effects for that quartile, with standard errors clustered at the person level below in parentheses. Lastly, the third column reports unadjusted *p*-values 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.

Quartile cutoffs in years are as follows: Q1: [18, 31], Q2: [32, 41], Q3: [42, 55], Q4: [56, 89].

†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A39. Heterogeneous Effects of Debt Relief on Credit Bureau Outcomes in the Hospital Debt Experiment, by Debt in Collections

	No Debt in Collections			Tercile 1			Tercile 2			Tercile 3		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Distress												
Number of accounts past due	0.81	-0.03	[0.254]	1.07	-0.01	[0.751]	1.35	0.01	[0.853]	1.56	-0.02	[0.519]
		(0.03)	.		(0.03)	.		(0.03)	.		(0.03)	.
Number of accounts in default	0.69	-0.03	[0.330]	0.95	-0.02	[0.476]	1.23	0.01	[0.845]	1.45	-0.02	[0.447]
		(0.03)	{0.616}		(0.03)	{0.732}		(0.03)	{0.942}		(0.03)	{0.693}
Debt past due ($)	3,481	-42	[0.860]	4,535	73	[0.762]	5,233	100	[0.661]	6,325	-124	[0.578]
		(238)	{0.971}		(242)	{0.769}		(228)	{0.942}		(224)	{0.693}
Balances in default ($)	2,361	8	[0.956]	3,218	216	[0.135]	4,098	72	[0.626]	5,228	-193	[0.229]
		(143)	{0.971}		(145)	{0.293}		(148)	{0.942}		(161)	{0.446}
Panel B. Debt in Collections												
Number of debts in collections	1.25	-0.04	[0.340]	2.81	-0.01	[0.903]	4.64	0.11	[0.128]	9.78	-0.13	[0.260]
		(0.04)	{0.334}		(0.05)	{0.912}		(0.07)	{0.184}		(0.11)	{0.398}
Debts in collections ($)	1,157	-101	[0.049]	1,809	33	[0.578]	3,392	62	[0.432]	10,001	-124	[0.399]
		(51)	{0.075}		(60)	{0.785}		(80)	{0.409}		(147)	{0.401}
Panel C. Bankruptcy												
Bankruptcy in last 12 months (%)	0.99	-0.16	[0.530]	1.02	-0.06	[0.801]	1.26	0.11	[0.665]	1.90	-0.36	[0.198]
		(0.26)	.		(0.25)	.		(0.26)	.		(0.28)	.
Panel D. Access to Credit												
Has credit score (%)	92.60	-0.21	[0.681]	97.98	0.11	[0.733]	98.72	0.18	[0.459]	99.39	-0.06	[0.736]
		(0.51)	{0.916}		(0.33)	{0.960}		(0.25)	{0.698}		(0.19)	{0.977}
Credit score (never missing)	634.69	-0.39	[0.735]	587.21	0.39	[0.692]	564.36	0.11	[0.913]	550.49	-0.03	[0.974]
		(1.15)	{0.916}		(1.00)	{0.960}		(0.98)	{0.911}		(0.93)	{0.977}
Credit card limit ($)	6,245	90	[0.392]	2,492	28	[0.698]	1,304	53	[0.346]	730	-15	[0.705]
		(106)	{0.787}		(72)	{0.960}		(56)	{0.698}		(40)	{0.977}
Panel E. Borrowing												
Number of credit cards	1.36	0.05	[0.012]	0.85	0.003	[0.877]	0.63	0.01	[0.421]	0.43	0.01	[0.343]
		(0.02)	{0.051}		(0.02)	{0.880}		(0.02)	{0.765}		(0.02)	{0.716}
Credit card balance ($)	2,737	22	[0.728]	1,527	-25	[0.557]	946	29	[0.446]	767	-16	[0.617]
		(65)	{0.732}		(43)	{0.795}		(37)	{0.765}		(32)	{0.866}
Number of auto loans	0.50	0.01	[0.153]	0.42	0.01	[0.421]	0.37	-0.01	[0.340]	0.29	0.01	[0.175]
		(0.01)	{0.358}		(0.01)	{0.794}		(0.01)	{0.765}		(0.01)	{0.522}
Auto loan balance ($)	10,030	-179	[0.437]	8,277	213	[0.286]	7,498	-164	[0.371]	6,359	-46	[0.779]
		(230)	{0.699}		(200)	{0.693}		(184)	{0.765}		(163)	{0.866}
Panel F. Sample Size												
Observations†	13,465	3,210		14,041	3,289		14,105	3,217		14,042	3,282	

Notes: Table presents the heterogeneous effects of medical debt relief on credit outcomes by (1) individuals who have no debt in collections and (2) tercile of debt in collections (measured in the quarter before treatment), as estimated in Equation 7. Results are reported for individuals in the hospital debt experiment. We restrict the sample to quarters $[-4,4]$ relative to treatment assignment and drop quarters $[0,3]$, so treatment effects are estimated by comparing outcomes 12 months after treatment to pre-treatment levels. The first column of each quartile reports the control means for observations in that bin, calculated in the fourth quarter post-treatment. The second column reports the treatment effects for that bin, with standard errors clustered at the person level below in parentheses. The third column reports unadjusted *p*-values 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 7. Tercile cutoffs are as follows: T1: [$1, $1,166], T2: [$1,167, $3,900], T3: [$3,901, $938,774].

†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A40. Heterogeneous Effects of Debt Relief on Credit Bureau Outcomes in the Collector Debt Experiment, by Medical Debt Eligible for Relief

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Distress												
Number of accounts past due	1.11	-0.02	[0.356]	1.08	0.01	[0.617]	1.00	0.01	[0.516]	0.91	-0.01	[0.744]
		(0.02)	.		(0.02)	.		(0.02)	.		(0.02)	.
Number of accounts in default	0.99	-0.02	[0.389]	0.97	0.01	[0.526]	0.91	0.01	[0.584]	0.82	-0.002	[0.916]
		(0.02)	{0.610}		(0.02)	{0.838}		(0.02)	{0.677}		(0.02)	{0.920}
Debt past due ($)	5,446	-166	[0.291]	5,115	-67	[0.638]	4,521	99	[0.427]	4,167	165	[0.168]
		(158)	{0.578}		(143)	{0.838}		(125)	{0.677}		(119)	{0.321}
Balances in default ($)	3,845	-77	[0.474]	3,912	-25	[0.809]	3,662	96	[0.321]	3,386	139	[0.135]
		(107)	{0.610}		(103)	{0.838}		(97)	{0.624}		(93)	{0.304}
Panel B. Debt in Collections												
Number of debts in collections	2.60	-0.05	[0.150]	3.21	0.04	[0.289]	3.66	0.01	[0.890]	4.73	-0.07	[0.159]
		(0.03)	{0.221}		(0.04)	{0.462}		(0.04)	{0.895}		(0.05)	{0.260}
Debts in collections ($)	2,036	-1	[0.984]	2,730	19	[0.698]	3,228	25	[0.647]	4,452	-53	[0.479]
		(42)	{0.985}		(50)	{0.719}		(54)	{0.869}		(74)	{0.469}
Panel C. Bankruptcy												
Bankruptcy in last 12 months (%)	0.80	-0.09	[0.422]	0.71	-0.10	[0.341]	0.58	-0.001	[0.991]	0.51	-0.02	[0.801]
		(0.11)	.		(0.10)	.		(0.09)	.		(0.09)	.
Panel D. Access to Credit												
Has credit score (%)	91.84	0.003	[0.988]	90.67	-0.13	[0.608]	90.40	0.04	[0.886]	90.05	-0.13	[0.626]
		(0.22)	{0.987}		(0.25)	{0.825}		(0.26)	{0.951}		(0.27)	{0.729}
Credit score (never missing)	595.85	0.51	[0.391]	578.23	-0.94	[0.115]	570.93	-0.21	[0.724]	565.19	0.40	[0.479]
		(0.60)	{0.793}		(0.60)	{0.295}		(0.59)	{0.951}		(0.57)	{0.729}
Credit card limit ($)	4,320	36	[0.467]	2,807	12	[0.769]	2,035	18	[0.641]	1,401	32	[0.321]
		(50)	{0.793}		(41)	{0.825}		(38)	{0.951}		(32)	{0.668}
Panel E. Borrowing												
Number of credit cards	1.10	0.0005	[0.969]	0.82	0.004	[0.727]	0.66	-0.01	[0.589]	0.51	0.01	[0.147]
		(0.01)	{0.977}		(0.01)	{0.911}		(0.01)	{0.907}		(0.01)	{0.431}
Credit card balance ($)	1,991	27	[0.343]	1,392	47	[0.052]	1,054	25	[0.236]	789	0.5	[0.980]
		(29)	{0.712}		(24)	{0.153}		(21)	{0.602}		(18)	{0.977}
Number of auto loans	0.38	0.01	[0.290]	0.32	0.001	[0.830]	0.27	-0.002	[0.645]	0.22	-0.004	[0.345]
		(0.005)	{0.712}		(0.005)	{0.911}		(0.004)	{0.907}		(0.004)	{0.682}
Auto loan balance ($)	6,724	-22	[0.805]	5,775	-126	[0.144]	5,012	40	[0.617]	4,167	-42	[0.576]
		(91)	{0.959}		(86)	{0.318}		(80)	{0.907}		(74)	{0.828}
Panel F. Sample Size												
Observations†	16,210	16,504		16,156	16,537		16,317	16,373		16,205	16,482	

Notes: Table presents the heterogeneous effects of medical debt relief on credit outcomes by quartile of medical debt eligible for relief (measured in the wave of treatment assignment) in the collector debt experiment, as estimated in Equation 7. We restrict the sample to quarters $[-4, 4]$ relative to treatment assignment and drop quarters $[0, 3]$, so treatment effects are estimated by comparing outcomes 12 months after treatment to pre-treatment levels. The first column of each quartile reports the control means for observations in that quartile, calculated in the fourth quarter post-treatment. The second column reports the treatment effects for that quartile, with standard errors clustered at the person level below in parentheses. The third column reports unadjusted *p*-values 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.

Quartile cutoffs are as follows: Q1: [$5, $305], Q2: [$305, $837], Q3: [$837, $2,110], Q4: [$2,110, $156,988].

†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A41. Heterogeneous Effects of Debt Relief on Credit Bureau Outcomes in the Collector Debt Experiment, by Medical Debt Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Distress												
Number of accounts past due	1.15	0.002	[0.923]	1.03	-0.02	[0.288]	0.96	-0.002	[0.923]	0.95	0.01	[0.610]
		(0.02)	.		(0.02)	.		(0.02)	.		(0.02)	.
Number of accounts in default	1.04	0.002	[0.900]	0.94	-0.01	[0.407]	0.86	0.003	[0.866]	0.86	0.01	[0.756]
		(0.02)	{0.912}		(0.02)	{0.599}		(0.02)	{0.972}		(0.02)	{0.775}
Debt past due ($)	5,280	92	[0.515]	4,809	-160	[0.211]	4,650	-46	[0.730]	4,521	141	[0.330]
		(141)	{0.749}		(128)	{0.406}		(135)	{0.966}		(145)	{0.631}
Balances in default ($)	3,983	158	[0.118]	3,817	-80	[0.407]	3,612	-21	[0.831]	3,407	65	[0.526]
		(101)	{0.289}		(96)	{0.599}		(100)	{0.972}		(103)	{0.775}
Panel B. Debt in Collections												
Number of debts in collections	4.11	-0.004	[0.922]	3.93	-0.01	[0.908]	3.59	-0.04	[0.362]	2.57	-0.03	[0.401]
		(0.04)	{0.915}		(0.04)	{0.919}		(0.04)	{0.565}		(0.03)	{0.585}
Debts in collections ($)	3,527	-38	[0.469]	3,478	69	[0.268]	3,255	-4	[0.945]	2,180	-23	[0.598]
		(53)	{0.696}		(62)	{0.412}		(64)	{0.951}		(44)	{0.628}
Panel C. Bankruptcy												
Bankruptcy in last 12 months (%)	0.91	-0.07	[0.521]	0.54	-0.003	[0.970]	0.64	-0.04	[0.664]	0.50	-0.06	[0.511]
		(0.11)	.		(0.09)	.		(0.10)	.		(0.09)	.
Panel D. Access to Credit												
Has credit score (%)	93.48	0.38	[0.063]	90.87	-0.16	[0.560]	90.32	-0.02	[0.952]	88.24	-0.44	[0.083]
		(0.21)	{0.194}		(0.28)	{0.829}		(0.27)	{0.946}		(0.25)	{0.190}
Credit score (never missing)	572.79	0.66	[0.253]	568.24	0.24	[0.672]	576.77	-0.49	[0.407]	593.27	-0.57	[0.359]
		(0.58)	{0.453}		(0.57)	{0.829}		(0.59)	{0.777}		(0.62)	{0.562}
Credit card limit ($)	2,474	39	[0.318]	1,658	56	[0.101]	2,302	11	[0.777]	4,124	-20	[0.692]
		(40)	{0.453}		(34)	{0.278}		(38)	{0.943}		(50)	{0.670}
Panel E. Borrowing												
Number of credit cards	0.72	0.01	[0.411]	0.60	0.01	[0.200]	0.75	-0.01	[0.364]	1.02	-0.001	[0.936]
		(0.01)	{0.865}		(0.01)	{0.532}		(0.01)	{0.685}		(0.01)	{0.930}
Credit card balance ($)	1,253	5	[0.844]	913	4	[0.833]	1,204	33	[0.134]	1,854	52	[0.063]
		(23)	{0.984}		(20)	{0.823}		(22)	{0.378}		(28)	{0.204}
Number of auto loans	0.31	0.0002	[0.968]	0.27	0.004	[0.365]	0.29	0.002	[0.600]	0.33	-0.01	[0.169]
		(0.005)	{0.984}		(0.004)	{0.652}		(0.004)	{0.806}		(0.005)	{0.371}
Auto loan balance ($)	5,892	-41	[0.637]	4,951	-76	[0.339]	5,142	10	[0.906]	5,677	-35	[0.681]
		(88)	{0.949}		(79)	{0.652}		(81)	{0.908}		(84)	{0.898}
Panel F. Sample Size												
Observations†	16,224	16,518		16,170	16,513		16,248	16,458		16,246	16,423	

Notes: Table presents the heterogeneous effects of medical debt relief on credit outcomes by quartile of the age of medical debt eligible for relief (measured in the wave of treatment assignment) in the collector debt experiment, as estimated in Equation 7. We restrict the sample to quarters $[-4, 4]$ relative to treatment assignment and drop quarters $[0, 3]$, so treatment effects are estimated by comparing outcomes 12 months after treatment to pre-treatment levels. The first column of each quartile reports the control means for observations in that quartile, calculated in the fourth quarter post-treatment. The second column reports the treatment effects for that quartile, with standard errors clustered at the person level below in parentheses. The third column reports unadjusted *p*-values 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.
Quartile cutoffs in days are as follows: Q1: [987, 2,057], Q2: [2,057, 2,207], Q3: [2,207, 2,520], Q4: [2,520, 8,554].
†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A42. Heterogeneous Effects of Debt Relief on Credit Bureau Outcomes in the Collector Debt Experiment, by Beneficiary Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Distress												
Number of accounts past due	1.20	-0.02	[0.336]	1.21	-0.01	[0.794]	1.01	-0.01	[0.649]	0.68	0.03	[0.050]
		(0.02)	.		(0.02)	.		(0.02)	.		(0.02)	.
Number of accounts in default	1.11	-0.01	[0.443]	1.10	-0.01	[0.659]	0.90	-0.002	[0.926]	0.60	0.03	[0.029]
		(0.02)	{0.753}		(0.02)	{0.928}		(0.02)	{0.929}		(0.02)	{0.067}
Debt past due ($)	4,597	-61	[0.578]	6,208	-25	[0.872]	5,318	140	[0.389]	3,113	72	[0.600]
		(109)	{0.758}		(154)	{0.928}		(163)	{0.621}		(137)	{0.781}
Balances in default ($)	3,842	-38	[0.657]	4,988	41	[0.733]	3,934	183	[0.118]	2,205	37	[0.699]
		(86)	{0.758}		(120)	{0.928}		(117)	{0.269}		(94)	{0.781}
Panel B. Debt in Collections												
Number of debts in collections	4.04	-0.02	[0.684]	4.11	-0.03	[0.458]	3.97	-0.09	[0.065]	2.85	0.04	[0.357]
		(0.04)	{0.872}		(0.05)	{0.652}		(0.05)	{0.115}		(0.04)	{0.576}
Debts in collections ($)	3,577	-7	[0.904]	3,684	-15	[0.812]	3,551	-9	[0.890]	2,376	40	[0.511]
		(57)	{0.880}		(64)	{0.806}		(65)	{0.884}		(60)	{0.576}
Panel C. Bankruptcy												
Bankruptcy in last 12 months (%)	0.59	0.03	[0.708]	0.70	-0.11	[0.328]	0.84	-0.10	[0.377]	0.46	0.04	[0.663]
		(0.09)	.		(0.11)	.		(0.11)	.		(0.10)	.
Panel D. Access to Credit												
Has credit score (%)	93.11	0.15	[0.543]	92.42	0.20	[0.430]	92.03	-0.05	[0.846]	87.45	-0.30	[0.342]
		(0.24)	{0.554}		(0.26)	{0.847}		(0.26)	{0.979}		(0.32)	{0.716}
Credit score (never missing)	557.66	0.85	[0.167]	566.47	-0.14	[0.826]	576.83	-0.29	[0.630]	598.98	-0.11	[0.863]
		(0.61)	{0.310}		(0.63)	{0.863}		(0.60)	{0.948}		(0.61)	{0.861}
Credit card limit ($)	1,404	62	[0.079]	2,237	21	[0.630]	2,623	3	[0.938]	3,066	-25	[0.570]
		(35)	{0.220}		(43)	{0.863}		(42)	{0.979}		(44)	{0.802}
Panel E. Borrowing												
Number of credit cards	0.57	0.01	[0.255]	0.75	0.003	[0.817]	0.82	0.005	[0.677]	0.78	-0.01	[0.561]
		(0.01)	{0.639}		(0.01)	{0.981}		(0.01)	{0.866}		(0.01)	{0.806}
Credit card balance ($)	800	21	[0.265]	1,231	23	[0.351]	1,410	-1	[0.968]	1,345	-3	[0.914]
		(19)	{0.639}		(24)	{0.777}		(25)	{0.962}		(25)	{0.921}
Number of auto loans	0.28	0.001	[0.864]	0.31	0.002	[0.739]	0.32	-0.003	[0.466]	0.24	0.01	[0.128]
		(0.005)	{0.926}		(0.005)	{0.981}		(0.005)	{0.840}		(0.004)	{0.370}
Auto loan balance ($)	5,189	-23	[0.789]	5,968	21	[0.819]	5,972	-130	[0.141]	4,100	78	[0.328]
		(85)	{0.926}		(93)	{0.981}		(89)	{0.429}		(80)	{0.661}
Panel F. Sample Size												
Observations†	15,291	15,693		14,441	14,691		14,809	15,128		13,664	13,782	

Notes: Table presents the heterogeneous effects of medical debt relief on credit outcomes by quartile of debtor age (measured in the wave of treatment assignment) in the collector debt experiment, as estimated in Equation 7. We restrict the sample to quarters $[-4,4]$ relative to treatment assignment and drop quarters $[0,3]$, so treatment effects are estimated by comparing outcomes 12 months after treatment to pre-treatment levels. The first column of each quartile reports the control means for observations in that quartile, calculated in the fourth quarter post-treatment. The second column reports the treatment effects for that quartile, with standard errors clustered at the person level below in parentheses. The third column reports unadjusted *p*-values 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.

Quartile cutoffs in years are as follows: Q1: [18, 35], Q2: [36, 44], Q3: [45, 56], Q4: [57, 89].

†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A43. Heterogeneous Effects of Debt Relief on Credit Bureau Outcomes in the Collector Debt Experiment, by Debt in Collections

	No Debt in Collections			Tercile 1			Tercile 2			Tercile 3		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Distress												
Number of accounts past due	0.69	0.002	[0.899]	0.96	-0.01	[0.749]	1.14	0.02	[0.242]	1.25	-0.02	[0.253]
		(0.02)	.		(0.02)	.		(0.02)	.		(0.02)	.
Number of accounts in default	0.60	0.003	[0.841]	0.84	-0.01	[0.671]	1.04	0.02	[0.248]	1.16	-0.01	[0.423]
		(0.02)	{0.969}		(0.02)	{0.956}		(0.02)	{0.434}		(0.02)	{0.718}
Debt past due ($)	3,326	-17	[0.906]	4,565	43	[0.761]	5,270	91	[0.496]	5,841	-68	[0.603]
		(146)	{0.969}		(141)	{0.956}		(133)	{0.501}		(130)	{0.761}
Balances in default ($)	2,172	55	[0.570]	3,336	-28	[0.779]	4,131	126	[0.208]	4,909	-12	[0.904]
		(97)	{0.880}		(99)	{0.956}		(100)	{0.434}		(103)	{0.899}
Panel B. Debt in Collections												
Number of debts in collections	0.42	0.02	[0.211]	1.78	0.003	[0.894]	3.37	-0.01	[0.777]	8.07	-0.07	[0.247]
		(0.02)	{0.344}		(0.02)	{0.982}		(0.03)	{0.772}		(0.06)	{0.397}
Debts in collections ($)	374	24	[0.284]	999	5	[0.867]	2,286	19	[0.603]	8,301	-38	[0.675]
		(23)	{0.344}		(27)	{0.982}		(37)	{0.768}		(92)	{0.681}
Panel C. Bankruptcy												
Bankruptcy in last 12 months (%)	0.55	-0.19	[0.103]	0.57	0.02	[0.861]	0.70	-0.12	[0.183]	0.76	0.07	[0.509]
		(0.11)	.		(0.09)	.		(0.09)	.		(0.10)	.
Panel D. Access to Credit												
Has credit score (%)	75.32	-0.58	[0.073]	91.38	0.30	[0.307]	95.56	-0.22	[0.321]	97.98	0.15	[0.313]
		(0.32)	{0.200}		(0.30)	{0.504}		(0.22)	{0.596}		(0.15)	{0.650}
Credit score (never missing)	640.25	-0.84	[0.290]	585.07	-0.05	[0.925]	561.14	-0.07	[0.896]	548.25	0.49	[0.329]
		(0.80)	{0.489}		(0.58)	{0.922}		(0.55)	{0.904}		(0.51)	{0.650}
Credit card limit ($)	6,510	-37	[0.582]	2,719	83	[0.043]	1,287	34	[0.246]	716	-1	[0.951]
		(66)	{0.570}		(41)	{0.131}		(29)	{0.596}		(23)	{0.948}
Panel E. Borrowing												
Number of credit cards	1.37	0.01	[0.513]	0.87	0.01	[0.547]	0.59	0.003	[0.781]	0.37	-0.005	[0.529]
		(0.01)	{0.910}		(0.01)	{0.784}		(0.01)	{0.944}		(0.01)	{0.770}
Credit card balance ($)	2,626	26	[0.475]	1,447	36	[0.115]	838	13	[0.440]	544	18	[0.237]
		(37)	{0.910}		(23)	{0.339}		(17)	{0.789}		(15)	{0.606}
Number of auto loans	0.37	-0.002	[0.625]	0.33	-0.002	[0.683]	0.29	0.01	[0.227]	0.22	-0.002	[0.552]
		(0.005)	{0.910}		(0.004)	{0.784}		(0.005)	{0.596}		(0.004)	{0.770}
Auto loan balance ($)	6,236	-19	[0.846]	5,872	-67	[0.422]	5,298	9	[0.910]	4,408	-65	[0.363]
		(100)	{0.910}		(84)	{0.771}		(80)	{0.944}		(71)	{0.696}
Panel F. Sample Size												
Observations†	14,017	14,242		16,938	17,248		16,918	17,237		17,004	17,162	

Notes: Table presents the heterogeneous effects of medical debt relief on credit outcomes by (1) individuals who have no debt in collections and (2) tercile of debt in collections (measured in the quarter before treatment), as estimated in Equation 7. Results are reported for individuals in the collector debt experiment. We restrict the sample to quarters $[-4,4]$ relative to treatment assignment and drop quarters $[0,3]$, so treatment effects are estimated by comparing outcomes 12 months after treatment to pre-treatment levels. The first column of each bin reports the control means for observations in that bin, calculated in the fourth quarter post-treatment. The second column reports the treatment effects for that bin, with standard errors clustered at the person level below in parentheses. The third column reports unadjusted *p*-values 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.
Tercile cutoffs are as follows: T1: [$1, $1,252], T2: [$1,253, $4,047], T3: [$4,048, $2,079,212].
†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A44. Effects of Debt Relief in the 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. Distress					
Number of accounts past due	0.93	-0.001	[0.979]	0.03	[0.558]
		(0.04)		(0.06)	
Number of accounts in default	0.84	-0.01	[0.876]	0.02	[0.712]
		(0.04)		(0.05)	
Debt past due ($)	5,478.61	-295.47	[0.405]	-603.48	[0.189]
		(354.67)		(459.71)	
Balances in default ($)	4,137.64	-282.05	[0.300]	-594.70	[0.081]
		(272.28)		(340.23)	
Panel B. Debt in Collections					
Number of debts in collections	5.80	-1.02	[0.000]	-0.25	[0.137]
		(0.11)		(0.17)	
Debts in collections ($)	5,163.49	-1,211.53	[0.000]	-114.15	[0.612]
		(158.24)		(225.05)	
Panel C. Bankruptcy					
Bankruptcy in last 12 months (%)	0.25	0.64	[0.010]	0.11	[0.750]
		(0.25)		(0.34)	
Panel D. Borrowing					
Number of credit cards	0.66	0.01	[0.724]	0.02	[0.586]
		(0.02)		(0.03)	
Credit card balance ($)	1,064.36	58.24	[0.220]	103.43	[0.165]
		(47.47)		(74.40)	
Number of auto loans	0.25	0.01	[0.612]	0.01	[0.573]
		(0.01)		(0.02)	
Auto loan balance ($)	4,467.11	149.94	[0.412]	9.14	[0.973]
		(182.56)		(274.24)	
Panel E. Sample Size					
Observations†	1,338	1,423			

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 credit bureaus (as estimated with Equation 2). This analysis includes observations from four quarters before the intervention (2017 Q2) to four quarters after the end of the control group reporting period (2019 Q4). Column (1) reports the control means during the control group reporting period. Column (2) reports the treatment effect in this period, with standard errors below in parentheses, and column (3) reports the corresponding *p*-values in brackets. Column (4) reports the treatment effects during the post-reporting period, with standard errors clustered at the person level below in parentheses, and column (5) reports the corresponding *p*-value in brackets.
†: Sample sizes for control and treatment groups reported in the control mean and treatment effect columns, respectively.

Table A45. Effects of Debt Relief in the Credit Reporting Subsample with No Other Debt in Collections

	Control Reporting			Post Control Reporting	
	Control Mean	Treatment Effect	*p*-value	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)
Panel A. Distress					
Number of accounts past due	0.66	0.11	[0.254]	0.26	[0.099]
		(0.09)		(0.16)	
Number of accounts in default	0.56	0.09	[0.238]	0.25	[0.096]
		(0.08)		(0.15)	
Debt past due ($)	4,492.67	-23.72	[0.977]	-149.94	[0.897]
		(810.96)		(1158.43)	
Balances in default ($)	3,018.84	-281.77	[0.559]	-521.76	[0.493]
		(481.44)		(760.42)	
Panel B. Debt in Collections					
Number of debts in collections	1.32	-0.63	[0.000]	0.03	[0.815]
		(0.10)		(0.13)	
Debts in collections ($)	1,172.97	-819.98	[0.011]	-174.84	[0.604]
		(322.43)		(336.68)	
Panel C. Bankruptcy					
Bankruptcy in last 12 months (%)	0.00	0.30	[0.350]	-0.81	[0.340]
		(0.32)		(0.84)	
Panel D. Borrowing					
Number of credit cards	0.97	0.07	[0.282]	0.06	[0.540]
		(0.06)		(0.10)	
Credit card balance ($)	1,657.93	59.19	[0.727]	165.25	[0.515]
		(169.53)		(253.52)	
Number of auto loans	0.29	-0.01	[0.756]	-0.001	[0.973]
		(0.02)		(0.04)	
Auto loan balance ($)	4,880.05	-423.74	[0.322]	-481.00	[0.445]
		(427.05)		(629.23)	
Panel E. Sample Size					
Observations†	231	234			

Notes: Table reports the effects of medical debt relief on credit bureau outcomes for the subset of the wave 1 credit reporting subsample with no other debt in collections, before and after medical debt collections ceased being reported to credit bureaus (as estimated with Equation 2). Includes observations from four quarters before the intervention (2017 Q2) to four quarters after the end of the control group reporting period (2019 Q4). Column (1) reports the control means during the control group reporting period. Column (2) reports the treatment effects in this period, with standard errors below in parentheses, and column (3) reports the corresponding *p*-values in brackets. Column (4) reports the treatment effects during the post-reporting period, with standard errors clustered at the person level below in parentheses, and column (5) reports the corresponding *p*-values in brackets.
†: Sample sizes for control and treatment groups reported in the control mean and treatment effect columns, respectively.

Table A46. Effects of Debt Relief in the Credit Reporting Subsample with Other Debt in Collections

	Control Reporting			Post Control Reporting	
	Control Mean	Treatment Effect	*p*-value	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)
Panel A. Distress					
Number of accounts past due	0.99	-0.01	[0.860]	0.01	[0.926]
		(0.04)		(0.06)	
Number of accounts in default	0.91	-0.02	[0.620]	-0.01	[0.867]
		(0.04)		(0.06)	
Debt past due ($)	5,747.49	-227.44	[0.587]	-611.00	[0.251]
		(418.19)		(532.50)	
Balances in default ($)	4,432.67	-365.44	[0.265]	-651.62	[0.106]
		(327.73)		(402.55)	
Panel B. Debt in Collections					
Number of debts in collections	6.85	-1.05	[0.000]	-0.24	[0.230]
		(0.13)		(0.20)	
Debts in collections ($)	6,117.99	-1,263.84	[0.000]	-52.51	[0.847]
		(188.49)		(271.34)	
Panel C. Bankruptcy					
Bankruptcy in last 12 months (%)	0.31	0.69	[0.021]	0.23	[0.558]
		(0.30)		(0.39)	
Panel D. Borrowing					
Number of credit cards	0.59	-0.005	[0.837]	-0.001	[0.985]
		(0.02)		(0.04)	
Credit card balance ($)	913.57	57.89	[0.228]	70.82	[0.341]
		(48.04)		(74.29)	
Number of auto loans	0.24	0.01	[0.447]	0.01	[0.464]
		(0.01)		(0.02)	
Auto loan balance ($)	4,355.94	243.39	[0.225]	174.61	[0.571]
		(200.60)		(307.78)	
Panel E. Sample Size					
Observations†	1,077	1,160			

Notes: Table reports the effects of medical debt relief on credit bureau outcomes for the subset of the wave 1 credit reporting subsample with other debt in collections, before and after medical debt collections ceased being reported to credit bureaus (as estimated with Equation 2).Includes observations from four quarters before the intervention (2017 Q2) to four quarters after the end of the control group reporting period (2019 Q4). Column (1) reports the control means during the control group reporting period. Column (2) reports the treatment effects in this period, with standard errors below in parentheses, and column (3) reports the corresponding *p*-values in brackets. Column (4) reports the treatment effects during the post-reporting period, with standard errors clustered at the person level below in parentheses, and column (5) reports the corresponding *p*-value in brackets.
†: Sample sizes for control and treatment groups reported in the control mean and treatment effect columns, respectively.

Table A47. Effects of Debt Relief in Credit Reporting Subsample Wave 2

	Control Reporting			Post Control Reporting	
	Control Mean	Treatment Effect	*p*-value	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)
Panel A. Full Sample					
Number of medical debts in collections	5.10	-0.83	[0.000]	0.02	[0.661]
		(0.04)		(0.06)	
Medical debt in collections ($)	4,806	-1,425	[0.000]	-125	[0.151]
		(64)		(87)	
Has credit score (%)	99.15	-6.09	[0.000]	-0.08	[0.868]
		(0.32)		(0.48)	
Credit score (never missing)	557.15	1.60	[0.021]	-0.68	[0.387]
		(0.69)		(0.79)	
Credit card limit ($)	1,159	-6	[0.810]	-2	[0.949]
		(24)		(37)	
Observations†	6,138	6,148			
Panel B. No Other Debt in Collections					
Number of medical debts in collections	1.12	-0.56	[0.000]	0.10	[0.050]
		(0.04)		(0.05)	
Medical debt in collections ($)	1,220	-824	[0.000]	151	[0.130]
		(70)		(100)	
Has credit score (%)	95.97	-27.31	[0.000]	-0.19	[0.914]
		(1.36)		(1.75)	
Credit score (never missing)	603.52	5.96	[0.005]	-2.50	[0.301]
		(2.14)		(2.42)	
Credit card limit ($)	2,490	-139	[0.081]	-89	[0.394]
		(80)		(105)	
Observations†	1,217	1,242			
Panel C. Other Debt in Collections					
Number of medical debts in collections	6.10	-0.89	[0.000]	0.01	[0.922]
		(0.05)		(0.07)	
Medical debt in collections ($)	5,704	-1,572	[0.000]	-189	[0.073]
		(78)		(106)	
Has credit score (%)	99.96	-0.85	[0.000]	0.03	[0.926]
		(0.14)		(0.34)	
Credit score (never missing)	549.36	1.11	[0.127]	-0.15	[0.855]
		(0.73)		(0.83)	
Credit card limit ($)	825	24	[0.270]	20	[0.580]
		(22)		(37)	
Observations†	4,909	4,889			

Notes: Table reports the effects of medical debt relief on credit bureau outcomes for the full wave 2 credit reporting subsample, before and after medical debt collections ceased being reported to credit bureaus (as estimated with Equation 2). Includes observations from four quarters before the intervention (2017 Q4) to four quarters after the end of the control group reporting period (2019 Q4). Column (1) reports the control means during the control group reporting period. Column (2) reports the treatment effects in this period, with standard errors below in parentheses, and column (3) reports the corresponding *p*-values in brackets. Column (4) reports the treatment effects during the post-reporting period, with standard errors clustered at the person level below in parentheses, and column (5) reports the corresponding *p*-values in brackets.
†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A48. Heterogeneous Effects of Debt Relief on Future Medical Debt in the Hospital Debt Experiment, by Medical Debt Eligible for Relief

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Full Sample												
Amount of debt ($)	146.53	5.38	[0.624]	194.21	2.42	[0.852]	208.98	15.40	[0.273]	280.21	36.40	[0.051]
		(10.98)			(13.00)			(14.06)			(18.69)	
At least some debt (%)	18.88	0.64	[0.395]	16.59	0.51	[0.474]	14.97	1.42	[0.040]	14.39	1.74	[0.012]
		(0.75)			(0.71)			(0.69)			(0.70)	
Panel B. Pre-Relief Medical Services												
Amount of debt ($)	129.82	5.45	[0.591]	179.18	-5.12	[0.672]	191.30	16.09	[0.231]	256.11	37.54	[0.036]
		(10.15)			(12.08)			(13.44)			(17.93)	
At least some debt (%)	17.65	0.69	[0.346]	15.70	0.56	[0.426]	14.25	1.23	[0.069]	13.47	1.70	[0.012]
		(0.73)			(0.70)			(0.68)			(0.68)	
Panel C. Post-Relief Medical Services												
Amount of debt ($)	17.45	3.06	[0.503]	16.47	6.52	[0.147]	19.28	-0.68	[0.860]	28.21	0.29	[0.959]
		(4.56)			(4.50)			(3.86)			(5.72)	
At least some debt (%)	2.37	0.10	[0.744]	1.79	-0.10	[0.688]	1.46	0.39	[0.119]	1.64	-0.04	[0.871]
		(0.30)			(0.25)			(0.25)			(0.24)	
Panel D. Sample Size												
Observations†	14,752	3,427		14,685	3,444		14,638	3,516		14,800	3,353	

Notes: Table presents the heterogeneous effects of medical debt relief on "future medical debt" as measured by (1) the probability of having medical bills sent to collections after initial treatment assignment and (2) the balances of future medical debt for the hospital debt experiment, by quartile of medical debt eligible for relief (measured in the wave of treatment assignment) in the hospital debt experiment. Estimates are computed as outlined in Equation 7. The first column of each quartile reports the control means for observations in that quartile. The second column reports the treatment effects for that quartile, with robust standard errors below in parentheses. The third column reports the *p*-value in brackets. Panel A presents effects for any future medical debt; Panel B presents effects for future medical debt with a service date prior to the wave of initial treatment assignment; Panel C presents effects for future medical debt with a service date after this wave.

Quartile cutoffs are as follows: Q1: [$25, $232], Q2: [$232, $616] Q3: [$617,$1,469], and Q4: [$1,470, $60,452].

†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A49. Heterogeneous Effects of Debt Relief on Future Medical Debt in the Hospital Debt Experiment, by Debt Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Full Sample												
Amount of debt ($)	226.66	31.35	[0.047]	194.49	16.72	[0.212]	248.74	-10.72	[0.465]	159.61	20.87	[0.120]
		(15.77)			(13.39)			(14.68)			(13.41)	
At least some debt (%)	15.09	1.31	[0.053]	16.43	1.16	[0.104]	19.31	1.20	[0.115]	14.02	0.64	[0.353]
		(0.68)			(0.71)			(0.76)			(0.68)	
Panel B. Pre-Relief Medical Services												
Amount of debt ($)	198.14	30.39	[0.040]	171.85	12.62	[0.309]	235.36	-10.49	[0.459]	150.48	20.71	[0.110]
		(14.81)			(12.41)			(14.16)			(12.95)	
At least some debt (%)	13.67	1.25	[0.058]	15.17	1.07	[0.125]	18.77	1.07	[0.155]	13.46	0.81	[0.229]
		(0.66)			(0.69)			(0.75)			(0.68)	
Panel C. Post-Relief Medical Services												
Amount of debt ($)	30.96	4.11	[0.470]	24.81	2.63	[0.590]	15.91	0.45	[0.910]	9.91	1.30	[0.715]
		(5.69)			(4.88)			(4.01)			(3.56)	
At least some debt (%)	2.54	0.13	[0.650]	2.40	0.09	[0.750]	1.31	0.10	[0.667]	1.04	0.01	[0.948]
		(0.29)			(0.29)			(0.23)			(0.21)	
Panel D. Sample Size												
Observations†	14,944	3,673		14,220	3,534		14,864	3,379		14,846	3,153	

Notes: Table presents the heterogeneous effects of medical debt relief on "future medical debt" as measured by (1) the probability of having medical bills sent to collections after initial treatment assignment and (2) the balances of future medical debt for the hospital debt experiment, by quartile of the age of medical debt eligible for relief (measured in the wave of treatment assignment) in the hospital debt experiment. Estimates are computed as outlined in Equation 7. The first column of each quartile reports the control means for observations in that quartile. The second column reports the treatment effects for that quartile, with robust standard errors below in parentheses. The third column reports the *p*-value in brackets. Panel A presents effects for any future medical debt; Panel B presents effects for future medical debt with a service date prior to the wave of initial treatment assignment; Panel C presents effects for future medical debt with a service date after this wave.

Quartile cutoffs in days are as follows: Q1: [130, 427], Q2: [427, 457], Q3: [457, 496], Q4: [496, 2,177].

†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A50. Heterogeneous Effects of Debt Relief on Future Medical Debt in the Hospital Debt Experiment, by Beneficiary Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Full Sample												
Amount of debt ($)	195.90	13.94	[0.318]	241.31	15.00	[0.331]	225.99	6.36	[0.678]	166.17	28.45	[0.033]
		(13.98)			(15.41)			(15.33)			(13.32)	
At least some debt (%)	14.35	1.87	[0.007]	17.25	-0.01	[0.989]	16.54	0.82	[0.267]	16.73	1.80	[0.015]
		(0.69)			(0.69)			(0.74)			(0.74)	
Panel B. Pre-Relief Medical Services												
Amount of debt ($)	179.44	8.78	[0.503]	219.91	12.37	[0.397]	202.20	16.52	[0.263]	153.98	21.18	[0.091]
		(13.10)			(14.59)			(14.76)			(12.53)	
At least some debt (%)	13.62	1.76	[0.010]	16.18	0.01	[0.983]	15.54	0.94	[0.193]	15.76	1.63	[0.025]
		(0.68)			(0.68)			(0.73)			(0.73)	
Panel C. Post-Relief Medical Services												
Amount of debt ($)	17.80	5.31	[0.247]	23.47	6.67	[0.223]	26.50	-10.60	[0.015]	13.75	7.12	[0.092]
		(4.59)			(5.48)			(4.37)			(4.22)	
At least some debt (%)	1.42	0.17	[0.470]	2.02	-0.05	[0.848]	1.92	-0.23	[0.378]	1.91	0.49	[0.095]
		(0.24)			(0.26)			(0.26)			(0.29)	
Panel D. Sample Size												
Observations†	14,753	3,438		15,700	3,632		13,719	3,237		14,265	3,323	

Notes: Table presents the heterogeneous effects of medical debt relief on "future medical debt" as measured by (1) the probability of having medical bills sent to collections after initial treatment assignment and (2) the balances of future medical debt for the hospital debt experiment, by quartile of debtor age (measured in the wave of treatment assignment) in the hospital debt experiment. Estimates are computed as outlined in Equation 7. The first column of each quartile reports the control means for observations in that quartile. The second column reports the treatment effects for that quartile, with robust standard errors below in parentheses. The third column reports the *p*-value in brackets. Panel A presents effects for any future medical debt; Panel B presents effects for future medical debt with a service date prior to the wave of initial treatment assignment; Panel C presents effects for future medical debt with a service date after this wave.

Quartile cutoffs in years are as follows: Q1: [18, 30], Q2: [31, 41], Q3: [42, 54], Q4: [55, 89].

†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A51. Heterogeneous Effects of Debt Relief on Future Medical Debt in the Hospital Debt Experiment, by Debt in Collections

	No Debt in Collections			Tercile 1			Tercile 2			Tercile 3		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Full Sample												
Amount of debt ($)	147.82	9.16	[0.468]	182.44	15.35	[0.270]	219.43	52.90	[0.001]	308.38	-9.73	[0.585]
		(12.62)			(13.93)			(16.54)			(17.82)	
At least some debt (%)	12.58	0.66	[0.327]	16.38	1.33	[0.077]	18.31	1.70	[0.032]	20.35	0.90	[0.262]
		(0.68)			(0.75)			(0.79)			(0.81)	
Panel B. Pre-Relief Medical Services												
Amount of debt ($)	137.10	7.87	[0.513]	165.56	14.21	[0.280]	200.97	51.32	[0.001]	276.82	-8.90	[0.594]
		(12.03)			(13.15)			(15.94)			(16.69)	
At least some debt (%)	11.90	0.73	[0.269]	15.45	1.14	[0.121]	17.21	1.82	[0.019]	19.06	0.92	[0.243]
		(0.66)			(0.74)			(0.78)			(0.79)	
Panel C. Post-Relief Medical Services												
Amount of debt ($)	12.09	0.93	[0.806]	18.46	-0.04	[0.992]	19.94	4.78	[0.328]	34.78	2.16	[0.732]
		(3.81)			(4.47)			(4.89)			(6.31)	
At least some debt (%)	1.20	-0.20	[0.320]	1.73	0.26	[0.352]	2.17	-0.14	[0.620]	2.63	0.37	[0.274]
		(0.21)			(0.28)			(0.28)			(0.34)	
Panel D. Sample Size												
Observations†	12,965	3,089		13,458	3,155		13,522	3,076		13,474	3,130	

Notes: Table presents the heterogeneous effects of medical debt relief on "future medical debt" as measured by (1) the probability of having medical bills sent to collections after initial treatment assignment and (2) the balances of future medical debt for the hospital debt experiment, by (1) individuals who have no debt in collections and (2) tercile of debt in collections in the first quarter pre-treatment. Estimates are computed as outlined in Equation 7. Results are reported for individuals in the hospital debt experiment. The first column of each bin reports the control means for observations in that bin. The second column reports the treatment effects for that bin, with robust standard errors below in parentheses. The third column reports the *p*-value in brackets. Panel A presents effects for any future medical debt; Panel B presents effects for future medical debt with a service date prior to the wave of initial treatment assignment; Panel C presents effects for future medical debt with a service date after this wave.

Tercile cutoffs are as follows: T1: [$1, $1,164], T2: [$1,165, $3,903], T3: [$3,904, $938,774].

†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A52. Effects of Debt Relief on Future Medical Debt (Saturated with Controls)

	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)
Panel A. Full Sample			
Amount of debt ($)	215.17	16.75	[0.029]
		(7.66)	
At least some debt (%)	16.95	1.17	[0.002]
		(0.38)	
Panel B. Pre-Relief Medical Services			
Amount of debt ($)	195.68	15.90	[0.029]
		(7.28)	
At least some debt (%)	15.94	1.18	[0.001]
		(0.37)	
Panel C. Post-Relief Medical Services			
Amount of debt ($)	21.40	2.02	[0.415]
		(2.47)	
At least some debt (%)	1.94	0.07	[0.595]
		(0.14)	
Panel D. Sample Size			
Observations†	53,419	12,450	

Notes: Table presents the heterogeneous effects of medical debt relief on "future medical debt" as measured by (1) the probability of having medical bills sent to collections after initial treatment assignment and (2) the balances of future medical debt for the hospital debt experiment, as estimated in Equation 6. This specification drops the person fixed effects from the baseline model and saturates with controls observed prior to treatment (see Appendix Section C.1 for details). Controls include gender, insurance status (in the hospital debt experiment), state, age, 25-point credit score bins, 25-point collections score bins, indicator for an open mortgage, log non-mortgage debt, log non-medical debt in collections, and log medical debt in collections. Column (1) reports the control means. Column (2) reports the treatment effects with robust standard errors below in parentheses. Column (3) contains the *p*-value in brackets. Panel A presents effects for any future medical debt; Panel B presents effects for future medical debt with a service date prior to the wave of initial treatment assignment; Panel C presents effects for future medical debt with a service date after the initial wave.
†: Sample sizes for control and treatment groups reported in the control mean and treatment effect columns, respectively.

Table A53. Survey Internal Validity Estimates

	Baseline Model			Saturated Model		Last Respondents Dropped	
	Control Mean	Treatment Effect	*p*-value	Treatment Effect	*p*-value	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Mental Health							
At least moderate depression (%)	44.95	3.23	[0.097]	2.93	[0.139]	3.83	[0.057]
		(1.94)		(1.98)		(2.01)	
At least moderate anxiety (%)	40.07	1.63	[0.395]	1.88	[0.335]	2.14	[0.281]
		(1.92)		(1.95)		(1.98)	
At least sometimes stressed (%)	76.53	2.72	[0.093]	2.66	[0.105]	3.23	[0.052]
		(1.62)		(1.64)		(1.66)	
Panel B. Subjective Wellbeing							
At least pretty happy (%)	54.33	-2.72	[0.161]	-3.22	[0.103]	-3.61	[0.072]
		(1.94)		(1.97)		(2.01)	
Panel C. General Health							
At least good health (%)	53.83	-2.56	[0.188]	-2.45	[0.211]	-3.60	[0.074]
		(1.94)		(1.96)		(2.01)	
Panel D. Healthcare Utilization							
Had all needed healthcare (%)	56.66	-2.37	[0.220]	-2.64	[0.176]	-1.85	[0.352]
		(1.93)		(1.95)		(1.99)	
Had all needed RX (%)	71.92	-2.42	[0.170]	-2.95	[0.097]	-2.01	[0.270]
		(1.77)		(1.78)		(1.82)	
Panel E. Financial Distress							
Had trouble paying other bills (%)	60.82	3.53	[0.061]	3.86	[0.040]	3.99	[0.040]
		(1.88)		(1.88)		(1.94)	
Cut back spending (Z-score)	0.00	-0.0003	[0.993]	-0.001	[0.979]	-0.02	[0.700]
		(0.04)		(0.04)		(0.04)	
Increased borrowing (Z-score)	0.00	0.03	[0.381]	0.04	[0.291]	0.03	[0.526]
		(0.04)		(0.04)		(0.04)	
Panel F. Sample Size							
Observations†	1802	1086		1055		974	

Notes: Table shows the effects of medical debt relief on survey outcomes according to three specifications designed to test internal validity, as outlined in Appendix Section C.5. The first specification (columns (1)-(3)) is the baseline model presented in Table VII. The second specification (columns (4)-(5)) saturates the baseline model with controls observed prior to treatment. Controls include gender, insurance status (in the hospital debt experiment), state, age, 25-point credit score bins, 25-point collections score bins, indicator for an open mortgage, log non-mortgage debt, log non-medical debt in collections, and log medical debt in collections. The third specification (columns (6)-(7)) estimates the baseline model for a subsample where response rates are equalized across the treatment and control groups by dropping the last treated respondents to respond. Column (1) reports the control means in the baseline model. Columns (2), (4), and (6) report the treatment effects for each corresponding specification with robust standard errors reported below in parentheses. Columns (3), (5), and (7) report unadjusted *p*-values in square brackets.

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

Table A54. Survey External Validity Estimates

	Median Propensity Score					Median Time to Response				
	Below Median		Above Median		Difference	Below Median		Above Median		Difference
	Treatment Effect	*p*-value	Treatment Effect	*p*-value	(*p*-value)	Treatment Effect	*p*-value	Treatment Effect	*p*-value	(*p*-value)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Mental Health										
At least moderate depression (%)	4.66	[0.158]	2.40	[0.333]	[0.584]	4.31	[0.107]	2.36	[0.408]	[0.618]
	(3.30)	.	(2.47)	.	.	(2.67)	.	(2.85)	.	.
At least moderate anxiety (%)	3.29	[0.314]	0.42	[0.864]	[0.481]	2.75	[0.301]	0.55	[0.845]	[0.569]
	(3.27)	{0.502}	(2.43)	{0.847}	{0.565}	(2.65)	{0.300}	(2.80)	{0.839}	{0.800}
At least sometimes stressed (%)	0.58	[0.835]	3.70	[0.070]	[0.365]	3.28	[0.128]	1.37	[0.579]	[0.560]
	(2.78)	{0.846}	(2.04)	{0.116}	{0.565}	(2.15)	{0.226}	(2.47)	{0.816}	{0.800}
Panel B. Subjective Wellbeing										
At least pretty happy (%)	-8.73	[0.007]	0.95	[0.702]	[0.018]	-2.05	[0.446]	-3.26	[0.251]	[0.756]
	(3.24)	.	(2.48)	.	.	(2.69)	.	(2.84)	.	.
Panel C. General Health										
At least good health (%)	-5.37	[0.100]	-0.70	[0.778]	[0.255]	-4.47	[0.095]	-1.11	[0.696]	[0.391]
	(3.26)	.	(2.48)	.	.	(2.67)	.	(2.85)	.	.
Panel D. Healthcare Utilization										
Had all needed healthcare (%)	-2.17	[0.506]	-2.07	[0.398]	[0.980]	-0.48	[0.857]	-5.11	[0.071]	[0.234]
	(3.26)	{0.519}	(2.45)	{0.592}	{0.983}	(2.67)	{0.851}	(2.83)	{0.142}	{0.405}
Had all needed RX (%)	-4.83	[0.126]	-1.89	[0.383]	[0.441]	-3.47	[0.154]	-1.13	[0.661]	[0.509]
	(3.16)	{0.230}	(2.16)	{0.592}	{0.683}	(2.44)	{0.293}	(2.58)	{0.663}	{0.515}
Panel E. Financial Distress										
Had trouble paying other bills (%)	5.97	[0.056]	2.92	[0.229]	[0.440]	4.03	[0.118]	2.75	[0.323]	[0.738]
	(3.12)	{0.179}	(2.43)	{0.496}	{0.812}	(2.57)	{0.315}	(2.79)	{0.641}	{0.975}
Cut back spending (Z-score)	-0.01	[0.897]	-0.001	[0.989]	[0.924]	-0.004	[0.944]	-0.002	[0.975]	[0.980]
	(0.07)	{0.907}	(0.05)	{0.991}	{0.987}	(0.05)	{0.939}	(0.06)	{0.966}	{1.000}
Increased borrowing (Z-score)	0.04	[0.518]	0.03	[0.516]	[0.901]	0.04	[0.521]	0.04	[0.512]	[0.982]
	(0.07)	{0.756}	(0.05)	{0.739}	{0.987}	(0.06)	{0.748}	(0.06)	{0.716}	{1.000}
Panel F. Sample Size										
Observations†	1,038		1,768			1,515		1,373		

Notes: Table presents the effects of medical debt relief on survey outcomes, split by above- and below-median (1) propensity score and (2) time between survey invitation and response, as outlined in Appendix Section C.6. Columns (1) and (3) report the treatment effects for individuals with below- and above-median propensity scores, respectively. Corresponding robust standard errors are reported below the estimated treatment effects in parentheses. Columns (2) and (4) report the corresponding unadjusted *p*-values and multiple-inference-adjusted *p*-values in square and curly brackets, respectively. Column (5) reports the *p*-value from an F-test with the null hypothesis that individuals below- and above-median propensity scores have the same treatment effect. Columns (6)-(10) report the equivalent figures for individuals with below- and above-median response times. 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.

Table A55. Effects of Debt Relief on Credit Bureau Outcomes in the Hospital Debt Experiment versus Survey Outreach Samples

	Hospital Debt Experiment			Survey Sample (Incl. Nonrespondents)			Difference
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Distress							
Number of accounts past due	1.20	-0.01	[0.762]	1.16	0.02	[0.627]	[0.435]
		(0.02)			(0.03)		
Number of accounts in default	1.08	-0.01	[0.708]	1.05	0.003	[0.922]	[0.713]
		(0.02)			(0.03)		
Debt past due ($)	4,908	53	[0.685]	4,725	98	[0.656]	[0.811]
		(130)			(220)		
Balances in default ($)	3,741	9	[0.921]	3,802	25	[0.880]	[0.909]
		(94)			(169)		
Panel B. Debt in Collections							
Number of debts in collections	4.66	-0.02	[0.759]	5.38	-0.03	[0.751]	[0.848]
		(0.06)			(0.11)		
Debts in collections ($)	4,119	-60	[0.350]	5,214	-252	[0.048]	[0.062]
		(64)			(127)		
Panel C. Bankruptcy							
Bankruptcy in last 12 months (%)	1.30	-0.05	[0.670]	1.10	-0.10	[0.563]	[0.716]
		(0.11)			(0.18)		
Panel D. Access to Credit							
Has credit score (%)	97.22	-0.11	[0.492]	98.10	-0.02	[0.948]	[0.658]
		(0.16)			(0.24)		
Credit score (never missing)	582.16	-0.10	[0.890]	576.29	0.53	[0.681]	[0.563]
		(0.76)			(1.30)		
Credit card limit ($)	2,654	61	[0.419]	2,240	135	[0.283]	[0.490]
		(75)			(126)		
Panel E. Borrowing							
Number of credit cards	0.81	0.005	[0.737]	0.72	0.02	[0.387]	[0.426]
		(0.01)			(0.03)		
Credit card balance ($)	1,481	27	[0.469]	1,256	112	[0.075]	[0.110]
		(37)			(63)		
Number of auto loans	0.39	0.003	[0.603]	0.36	0.02	[0.065]	[0.064]
		(0.01)			(0.01)		
Auto loan balance ($)	8,020	-47	[0.735]	7,709	326	[0.203]	[0.078]
		(139)			(256)		
Panel F. Sample Size							
Observations†	55,653	12,998		9,179	5,060		
F statistic (*p*-value)††							[0.381]

Notes: Table presents the effects of medical debt relief on credit bureau outcomes for the hospital debt experiment (columns (1)-(3)) and the survey outreach sample (respondents and non-respondents) (columns (4)-(6)). Columns (1) and (4) present control means, columns (2) and (5) show treatment effects with robust standard errors in parentheses, and columns (3) and (6) show associated *p*-values. Column (7) presents the *p*-value on the difference in treatment effects for the treated individuals in the hospital debt experiment and the subset of those individuals invited to complete the survey.

†: The control and treatment group sample sizes for the hospital debt experiment as a whole are reported in columns (1) and (2) respectively. The control and treatment group sample sizes for the subset of the hospital debt experiment that were contacted for the survey reported in columns (4) and (5) respectively.

††: *p*-value on the joint null hypothesis.

Table A56. Effects of Debt Relief on Credit Bureau Outcomes in the Hospital Debt Experiment versus Survey Respondent Samples

	Hospital Debt Experiment			Survey Respondents			Difference
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Distress							
Number of accounts past due	1.20	-0.01	[0.762]	1.26	0.13	[0.038]	[0.022]
		(0.02)	.		(0.06)	.	
Number of accounts in default	1.08	-0.01	[0.708]	1.13	0.09	[0.156]	[0.106]
		(0.02)	{0.937}		(0.06)	{0.260}	
Debt past due ($)	4,908	53	[0.685]	4,385	-361	[0.332]	[0.247]
		(130)	{0.937}		(372)	{0.332}	
Balances in default ($)	3,741	9	[0.921]	3,545	-666	[0.012]	[0.008]
		(94)	{0.939}		(264)	{0.029}	
Panel B. Debt in Collections							
Number of debts in collections	4.66	-0.02	[0.759]	4.70	-0.72	[0.000]	[0.000]
		(0.06)	{0.747}		(0.18)	{0.000}	
Debts in collections ($)	4,119	-60	[0.350]	4,319	-1,034	[0.000]	[0.000]
		(64)	{0.483}		(200)	{0.000}	
Panel C. Bankruptcy							
Bankruptcy in last 12 months (%)	1.30	-0.05	[0.670]	1.71	0.78	[0.068]	[0.039]
		(0.11)	.		(0.42)	.	
Panel D. Access to Credit							
Has credit score (%)	97.22	-0.11	[0.492]	98.97	1.17	[0.000]	[0.000]
		(0.16)	{0.786}		(0.27)	{0.001}	
Credit score (never missing)	582.16	-0.10	[0.890]	585.51	9.79	[0.000]	[0.000]
		(0.76)	{0.894}		(2.51)	{0.001}	
Credit card limit ($)	2,654	61	[0.419]	3,345	1,289	[0.000]	[0.000]
		(75)	{0.786}		(272)	{0.001}	
Panel E. Borrowing							
Number of credit cards	0.81	0.005	[0.737]	1.05	0.38	[0.000]	[0.000]
		(0.01)	{0.926}		(0.05)	{0.000}	
Credit card balance ($)	1,481	27	[0.469]	1,717	685	[0.000]	[0.000]
		(37)	{0.868}		(136)	{0.000}	
Number of auto loans	0.39	0.003	[0.603]	0.47	0.11	[0.000]	[0.000]
		(0.01)	{0.904}		(0.02)	{0.000}	
Auto loan balance ($)	8,020	-47	[0.735]	9,548	1,745	[0.001]	[0.000]
		(139)	{0.926}		(510)	{0.001}	
Panel F. Sample Size							
Observations†	55,653	12,998		1,751	1,055		
F statistic (*p*-value)††							[0.083]

Notes: Table presents the effects of medical debt relief on credit bureau outcomes for the hospital debt experiment (columns (1)-(3)) and the survey respondent sample (columns (4)-(6)). Columns (1) and (4) present control means, columns (2) and (5) show treatment effects with robust standard errors in parentheses, and columns (3) and (6) show associated *p*-values. Column (7) presents the *p*-value on the difference in treatment effects for the treated individuals in the hospital debt experiment and the subset of those individuals who responded to the survey.

† : The control and treatment group sample sizes for the hospital debt experiment as a whole are reported in columns (1) and (2) respectively. The control and treatment group sample sizes for the subset of the hospital debt experiment that responded to the survey reported in columns (4) and (5) respectively.

††: *p*-value on the joint null hypothesis.

Table A57. Heterogeneous Effects of Debt Relief on Survey Outcomes in the Hospital Debt Experiment, by Medical Debt Eligible for Relief

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean (1)	Treatment Effect (2)	*p*-value (3)	Control Mean (4)	Treatment Effect (5)	*p*-value (6)	Control Mean (7)	Treatment Effect (8)	*p*-value (9)	Control Mean (10)	Treatment Effect (11)	*p*-value (12)
Panel A. Mental Health												
At least moderate depression (%)	44.54	-0.26	[0.948]	43.14	0.99	[0.804]	46.26	1.94	[0.617]	45.92	12.41	[0.002]
		(3.93)			(3.98)			(3.87)			(3.98)	
At least moderate anxiety (%)	39.42	-3.95	[0.304]	37.69	-3.52	[0.357]	42.99	4.08	[0.296]	40.34	10.62	[0.008]
		(3.84)	{0.507}		(3.83)	{0.581}		(3.90)	{0.301}		(3.98)	{0.014}
At least sometimes stressed (%)	78.84	-1.16	[0.725]	74.73	-1.24	[0.726]	75.00	7.41	[0.021]	77.47	5.75	[0.076]
		(3.30)	{0.715}		(3.53)	{0.712}		(3.21)	{0.033}		(3.24)	{0.064}
Panel B. Subjective Wellbeing												
At least pretty happy (%)	57.24	-0.53	[0.891]	54.25	-0.28	[0.944]	52.10	-3.43	[0.380]	53.65	-7.90	[0.047]
		(3.89)	.		(3.97)	.		(3.91)	.		(3.98)	.
Panel C. General Health												
At least good health (%)	56.57	-2.18	[0.577]	54.90	0.54	[0.892]	52.57	-3.47	[0.373]	51.29	-7.81	[0.051]
		(3.90)	.		(4.00)	.		(3.90)	.		(4.00)	.
Panel D. Healthcare Utilization												
Had all needed healthcare (%)	54.79	3.03	[0.439]	58.82	0.11	[0.978]	57.71	-4.55	[0.235]	55.36	-6.62	[0.094]
		(3.92)	{0.425}		(3.97)	{0.996}		(3.83)	{0.374}		(3.95)	{0.111}
Had all needed RX (%)	71.71	-4.35	[0.233]	74.51	0.20	[0.955]	68.46	2.18	[0.533]	72.75	-7.22	[0.049]
		(3.65)	{0.399}		(3.56)	{0.996}		(3.51)	{0.504}		(3.66)	{0.111}
Panel E. Financial Distress												
Had trouble paying other bills (%)	61.25	1.06	[0.781]	59.48	6.96	[0.075]	61.92	7.35	[0.047]	60.73	0.38	[0.921]
		(3.83)	{0.783}		(3.90)	{0.199}		(3.69)	{0.123}		(3.90)	{0.922}
Cut back spending (Z-score)	0.05	-0.10	[0.223]	0.01	-0.08	[0.321]	-0.01	0.05	[0.497]	-0.05	0.10	[0.220]
		(0.08)	{0.444}		(0.08)	{0.516}		(0.08)	{0.506}		(0.08)	{0.463}
Increased borrowing (Z-score)	-0.03	0.11	[0.178]	0.05	-0.07	[0.358]	-0.06	0.15	[0.055]	0.03	-0.04	[0.634]
		(0.08)	{0.444}		(0.08)	{0.516}		(0.08)	{0.123}		(0.08)	{0.849}
Panel F. Sample Size												
Observations†	449	273		459	263		428	294		466	256	

Notes: Table presents the heterogeneous effects of medical debt relief on survey outcomes by quartile of medical debt eligible for relief (measured in the wave of treatment assignment), as estimated in Equation 7. The first column of each quartile reports the control means for observations in that quartile. The second column reports the treatment effects for that quartile, with robust standard errors below in parentheses. The third column reports unadjusted *p*-values 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.
Quartile cutoffs are as follows: Q1: [$500, $794], Q2: [$794, $1,275], Q3: [$1,276, $2,275], Q4: [$2,277, $33,627].
†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A58. Heterogeneous Effects of Debt Relief on Survey Outcomes in the Hospital Debt Experiment, by Debt Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean (1)	Treatment Effect (2)	*p*-value (3)	Control Mean (4)	Treatment Effect (5)	*p*-value (6)	Control Mean (7)	Treatment Effect (8)	*p*-value (9)	Control Mean (10)	Treatment Effect (11)	*p*-value (12)
Panel A. Mental Health												
At least moderate depression (%)	52.14	-1.51	[0.697]	41.46	3.54	[0.372]	43.84	6.65	[0.092]	42.02	2.67	[0.493]
		(3.88)			(3.96)			(3.95)			(3.90)	
At least moderate anxiety (%)	44.87	-1.54	[0.692]	37.47	3.76	[0.343]	38.58	5.06	[0.193]	39.10	-2.26	[0.551]
		(3.89)	{0.891}		(3.96)	{0.356}		(3.89)	{0.310}		(3.79)	{0.656}
At least sometimes stressed (%)	79.91	0.26	[0.933]	77.38	5.08	[0.113]	76.26	3.73	[0.250]	72.36	2.77	[0.427]
		(3.12)	{0.925}		(3.20)	{0.198}		(3.24)	{0.310}		(3.48)	{0.656}
Panel B. Subjective Wellbeing												
At least pretty happy (%)	49.79	-1.49	[0.704]	53.22	-5.28	[0.185]	57.53	-2.72	[0.490]	57.08	-1.28	[0.745]
		(3.91)	.		(3.98)	.		(3.94)	.		(3.93)	.
Panel C. General Health												
At least good health (%)	49.15	-1.71	[0.664]	54.10	-3.00	[0.454]	58.45	-4.65	[0.242]	53.93	-0.84	[0.833]
		(3.94)	.		(4.00)	.		(3.97)	.		(3.98)	.
Panel D. Healthcare Utilization												
Had all needed healthcare (%)	47.01	-1.78	[0.651]	57.87	-4.60	[0.237]	60.73	-1.79	[0.648]	61.57	-0.76	[0.840]
		(3.92)	{0.651}		(3.89)	{0.366}		(3.91)	{0.836}		(3.75)	{0.837}
Had all needed RX (%)	66.03	-4.44	[0.237]	70.07	-3.14	[0.398]	75.57	1.65	[0.618]	76.40	-6.34	[0.066]
		(3.75)	{0.406}		(3.71)	{0.377}		(3.32)	{0.836}		(3.45)	{0.136}
Panel E. Financial Distress												
Had trouble paying other bills (%)	66.88	1.62	[0.660]	63.19	2.53	[0.505]	58.68	2.51	[0.514]	54.16	7.34	[0.059]
		(3.68)	{0.868}		(3.79)	{0.830}		(3.84)	{0.860}		(3.88)	{0.145}
Cut back spending (Z-score)	0.07	-0.03	[0.681]	-0.04	0.05	[0.475]	-0.04	0.03	[0.668]	0.01	-0.09	[0.277]
		(0.08)	{0.868}		(0.08)	{0.830}		(0.08)	{0.864}		(0.08)	{0.453}
Increased borrowing (Z-score)	0.07	0.06	[0.459]	-0.05	0.05	[0.544]	0.03	-0.02	[0.770]	-0.05	0.08	[0.296]
		(0.08)	{0.831}		(0.08)	{0.830}		(0.08)	{0.864}		(0.08)	{0.453}
Panel F. Sample Size												
Observations†	468	276		451	257		438	275		445	277	

Notes: Table presents the heterogeneous effects of medical debt relief on survey outcomes by quartile of the age of medical debt eligible for relief (measured in the wave of treatment assignment), as estimated in Equation 7. The first column of each quartile reports the control means for observations in that quartile. The second column reports the treatment effects for that quartile, with robust standard errors below in parentheses. The third column reports unadjusted *p*-values 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.
Quartile cutoffs in days are as follows: Q1: [198, 429], Q2: [429, 459], Q3: [459, 503], Q4: [503, 1,567].
†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A59. Heterogeneous Effects of Debt Relief on Survey Outcomes in the Hospital Debt Experiment, by Beneficiary Age

	Quartile 1			Quartile 2			Quartile 3			Quartile 4		
	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Mental Health												
At least moderate depression (%)	46.39	4.47	[0.236]	47.15	-2.76	[0.479]	46.32	7.33	[0.068]	39.56	2.75	[0.489]
		(3.77)			(3.90)			(4.01)			(3.97)	
At least moderate anxiety (%)	41.79	6.23	[0.098]	45.18	-5.27	[0.177]	42.53	1.52	[0.702]	29.85	3.79	[0.310]
		(3.77)	{0.181}		(3.90)	{0.183}		(3.97)	{0.884}		(3.73)	{0.511}
At least sometimes stressed (%)	78.34	2.00	[0.509]	76.54	7.14	[0.024]	80.63	-0.30	[0.925]	70.15	0.41	[0.911]
		(3.03)	{0.503}		(3.16)	{0.062}		(3.21)	{0.929}		(3.69)	{0.901}
Panel B. Subjective Wellbeing												
At least pretty happy (%)	54.27	-4.00	[0.288]	56.80	-2.75	[0.484]	50.74	-0.29	[0.943]	55.58	-3.31	[0.408]
		(3.77)	.		(3.93)	.		(4.04)	.		(4.00)	.
Panel C. General Health												
At least good health (%)	62.36	-5.07	[0.170]	56.36	1.91	[0.626]	49.89	-3.86	[0.331]	45.87	-2.86	[0.480]
		(3.69)	.		(3.92)	.		(3.97)	.		(4.05)	.
Panel D. Healthcare Utilization												
Had all needed healthcare (%)	54.49	-4.06	[0.287]	54.17	-0.86	[0.826]	51.16	-0.56	[0.890]	67.96	-5.34	[0.162]
		(3.81)	{0.433}		(3.92)	{0.923}		(4.04)	{0.909}		(3.82)	{0.287}
Had all needed RX (%)	70.02	-3.87	[0.269]	67.54	1.24	[0.734]	71.37	-4.50	[0.227]	79.37	-3.77	[0.269]
		(3.50)	{0.433}		(3.66)	{0.923}		(3.72)	{0.360}		(3.41)	{0.287}
Panel E. Financial Distress												
Had trouble paying other bills (%)	61.49	4.19	[0.245]	61.84	6.17	[0.107]	65.47	-1.50	[0.698]	53.64	7.18	[0.071]
		(3.60)	{0.526}		(3.82)	{0.282}		(3.86)	{0.908}		(3.97)	{0.176}
Cut back spending (Z-score)	-0.04	-0.04	[0.630]	-0.03	0.05	[0.480]	0.11	-0.01	[0.851]	-0.05	-0.03	[0.684]
		(0.08)	{0.854}		(0.08)	{0.721}		(0.08)	{0.908}		(0.08)	{0.683}
Increased borrowing (Z-score)	0.07	0.004	[0.957]	0.15	-0.04	[0.654]	-0.05	0.08	[0.308]	-0.18	0.06	[0.439]
		(0.08)	{0.955}		(0.08)	{0.721}		(0.08)	{0.642}		(0.08)	{0.669}
Panel F. Sample Size												
Observations†	457	300		456	275		475	253		412	258	

Notes: Table presents the heterogeneous effects of medical debt relief on survey outcomes by quartile of debtor age (measured in the wave of treatment assignment), as estimated in Equation 7. The first column of each quartile reports the control means for observations in that quartile. The second column reports the treatment effects for that quartile with robust standard errors below in parentheses. The third column reports unadjusted *p*-values 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.

Quartile cutoffs in years are as follows: Q1: [19, 30], Q2: [31, 40], Q3: [41, 52], Q4: [53, 89].

†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A60. Heterogeneous Effects of Debt Relief on Survey Outcomes in the Hospital Debt Experiment, by Debt in Collections

	No Debt in Collections			Tercile 1			Tercile 2			Tercile 3		
	Control Mean (1)	Treatment Effect (2)	*p*-value (3)	Control Mean (4)	Treatment Effect (5)	*p*-value (6)	Control Mean (7)	Treatment Effect (8)	*p*-value (9)	Control Mean (10)	Treatment Effect (11)	*p*-value (12)
Panel A. Mental Health												
At least moderate depression (%)	43.15	-2.04	[0.607]	40.35	7.97	[0.046]	43.99	4.63	[0.254]	53.44	1.16	[0.768]
		(3.97)			(3.99)			(4.05)			(3.92)	
At least moderate anxiety (%)	37.21	-0.58	[0.884]	33.04	4.71	[0.216]	42.40	2.87	[0.479]	48.46	-1.16	[0.768]
		(3.94)	{0.982}		(3.81)	{0.215}		(4.05)	{0.477}		(3.95)	{0.947}
At least sometimes stressed (%)	75.57	-0.01	[0.998]	74.28	5.96	[0.082]	77.32	3.82	[0.246]	79.57	0.70	[0.825]
		(3.46)	{0.999}		(3.43)	{0.159}		(3.29)	{0.392}		(3.15)	{0.947}
Panel B. Subjective Wellbeing												
At least pretty happy (%)	61.64	-0.49	[0.901]	53.44	-1.86	[0.641]	57.82	-11.41	[0.004]	44.89	0.43	[0.913]
		(3.91)	.		(3.99)	.		(3.97)	.		(3.89)	.
Panel C. General Health												
At least good health (%)	63.47	-5.21	[0.184]	51.88	-0.71	[0.861]	56.46	-9.46	[0.020]	43.47	5.59	[0.155]
		(3.93)	.		(4.05)	.		(4.08)	.		(3.93)	.
Panel D. Healthcare Utilization												
Had all needed healthcare (%)	65.07	-2.83	[0.468]	54.10	5.43	[0.171]	56.01	-8.52	[0.036]	51.07	-3.09	[0.436]
		(3.90)	{0.704}		(3.97)	{0.306}		(4.07)	{0.040}		(3.97)	{0.662}
Had all needed RX (%)	79.45	-0.90	[0.781]	74.72	-2.90	[0.419]	72.56	-8.71	[0.022]	61.52	0.80	[0.833]
		(3.25)	{0.800}		(3.58)	{0.420}		(3.81)	{0.040}		(3.81)	{0.833}
Panel E. Financial Distress												
Had trouble paying other bills (%)	51.83	5.09	[0.206]	58.98	2.56	[0.519]	63.95	6.26	[0.100]	68.17	2.54	[0.488]
		(4.02)	{0.436}		(3.97)	{0.790}		(3.81)	{0.248}		(3.67)	{0.856}
Cut back spending (Z-score)	-0.12	0.04	[0.662]	0.02	-0.03	[0.673]	0.06	-0.04	[0.622]	0.05	0.04	[0.576]
		(0.08)	{0.691}		(0.08)	{0.790}		(0.08)	{0.835}		(0.08)	{0.856}
Increased borrowing (Z-score)	-0.15	0.06	[0.471]	-0.02	0.06	[0.430]	0.05	0.04	[0.624]	0.15	-0.02	[0.780]
		(0.08)	{0.691}		(0.08)	{0.790}		(0.08)	{0.835}		(0.08)	{0.856}
Panel F. Sample Size												
Observations†	438	268		451	249		441	259		421	279	

Notes: Table presents the heterogeneous effects of medical debt relief on survey outcomes by (1) individuals who have no debt in collections and (2) tercile of debt in collections in the first quarter pre-treatment, as estimated in Equation 7. The first column of each quartile reports the control means for observations in that quartile. The second column reports the treatment effects for that quartile, with robust standard errors below in parentheses. The third column reports unadjusted *p*-values 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.
Tercile cutoffs are as follows: T1: [$7, $1,225], T2: [$1,232, $4,105], T3: [$4,109, $128,503].
†: Sample size for control and treatment groups reported in control mean and treatment effect columns respectively.

Table A61. Effect of Medical Debt Forgiveness on Self-Reported Awareness of Medical Debt Forgiveness

	Control	All Treated		Awareness Intervention				
				Treated, Not Called		Treated, Called		Difference
	Control Mean	Treatment Effect	*p*-value	Treatment Effect	*p*-value	Treatment Effect	*p*-value	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Awareness								
Had medical debt forgiven (%)	8.1	16.1	[0.000]	14.1	[0.000]	18.0	[0.000]	[0.225]
		(1.75)	{0.000}	(2.30)	{0.000}	(2.41)	{0.000}	{0.393}
Amount of medical debt forgiven ($)	147.1	289.5	[0.000]	241.3	[0.002]	336.7	[0.000]	[0.341]
		(58.03)	{0.000}	(77.24)	{0.007}	(76.05)	{0.001}	{0.393}
Medical debt forgiveness had at least some impact (%)	5.4	14.6	[0.000]	12.0	[0.000]	17.1	[0.000]	[0.088]
		(1.60)	{0.000}	(2.09)	{0.000}	(2.27)	{0.000}	{0.206}
Panel B. Sample Size								
Observations†	1,251	744		363		381		

Notes: Table presents the effects of medical debt relief and the awareness sub-experiment on self-reported awareness of medical debt forgiveness and its perceived impact, for survey respondents in waves 6-14 of the hospital debt experiment. Columns (2) and (3) use the specification from Equation 6. Columns (4) through (8), we adapt this specification by adding an interaction term between debt relief treatment and an indicator for call attempted. Column (2) reports the treatment effects of debt relief on all treated respondents, column (4) reports the treatment effects for those who were not assigned to receive a call in the awareness sub-experiment, and column (6) reports the treatment effect for those who were assigned to receive a call. Robust standard errors are reported below point estimates in parentheses. Corresponding unadjusted and adjusted *p*-values are reported in columns (3), (5), and (7) in square and curly brackets, respectively. Column (8) 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.
†: Sample sizes for control and treatment groups reported in the control mean and treatment effect columns, respectively.

Table A62. 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. Healthcare 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)	-0.001	0.001	[0.985]	-0.003	[0.956]	[0.953]
		(0.06)	{0.987}	(0.06)	{0.955}	{0.948}
Increased borrowing (Z-score)	-0.001	0.02	[0.806]	0.04	[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 sub-experiment on self-reported health and financial distress outcomes, for waves 6-14 of the hospital debt sample surveyed. We adapt the specification from Equation 6 by adding an 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 sub-experiment, and column (4) reports the treatment effects for those who were assigned to receive a call. Robust 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.

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

Table A63. Effects of Debt Relief on Debt Repayment Expectations

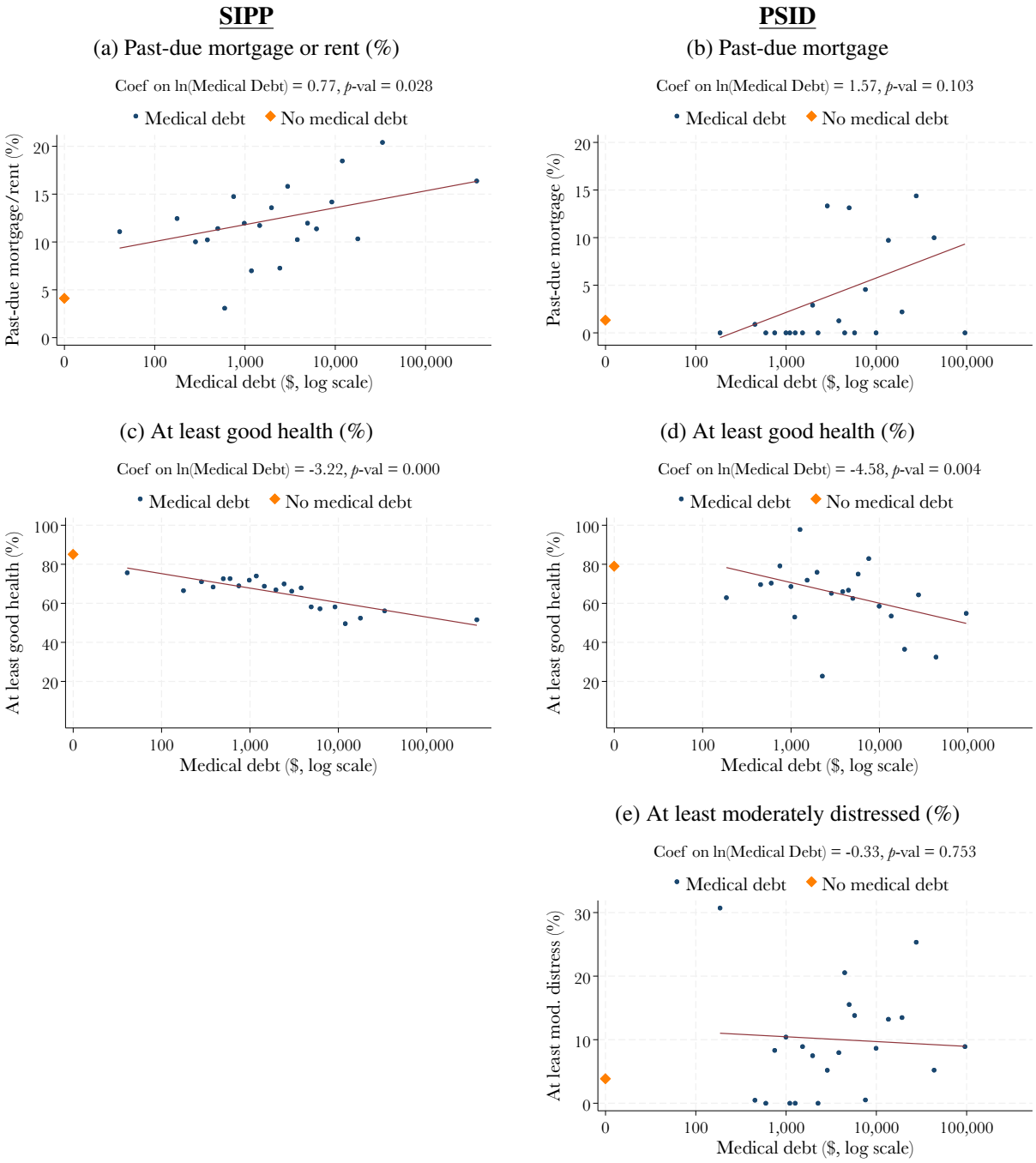
	Control Mean	Treatment Effect	*p*-value
	(1)	(2)	(3)
Panel A. Medical Debt Payment			
Amount of debt ($)	7,316.90	499.01	[0.247]
		(431.01)	{0.859}
Expected to pay ($)	3,253.52	-38.91	[0.895]
		(294.15)	{0.859}
Fair to pay ($)	2,186.88	94.67	[0.692]
		(238.95)	{0.859}
Panel B. Sample Size			
Observations†	1,197	773	

Notes: Table presents the effects of medical debt relief on self-reported medical debt, expectations of repayment, and perceived fairness of repayment within the survey respondent sample (a subset of the hospital debt sample). Column (1) reports the means for control group respondents. Column (2) reports the treatment effects, with robust standard errors below 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 6.

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

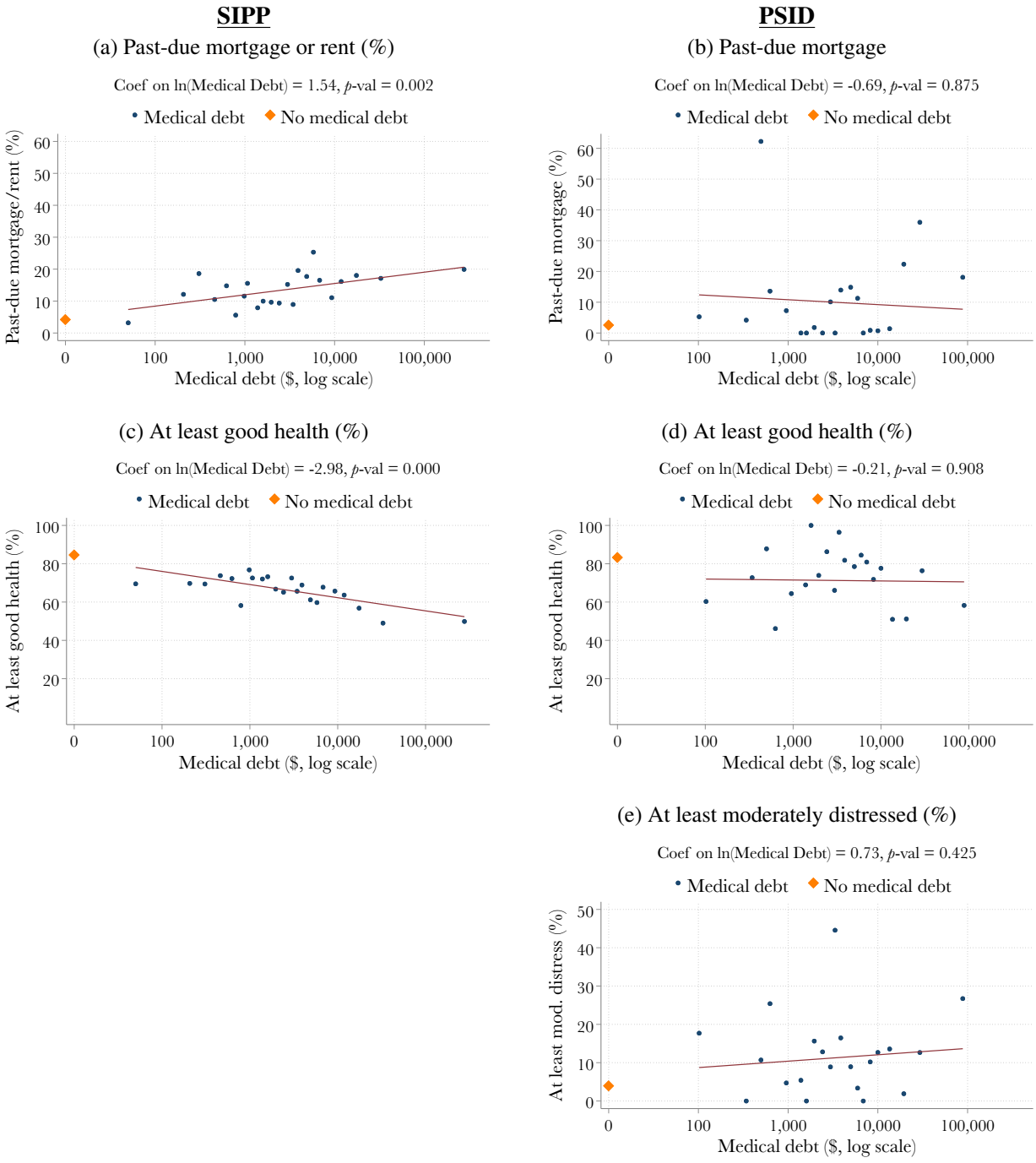
E Appendix Figures

Figure A1. Intensive Margin Relationships with Medical Debt in Nationally Representative Surveys (2019)



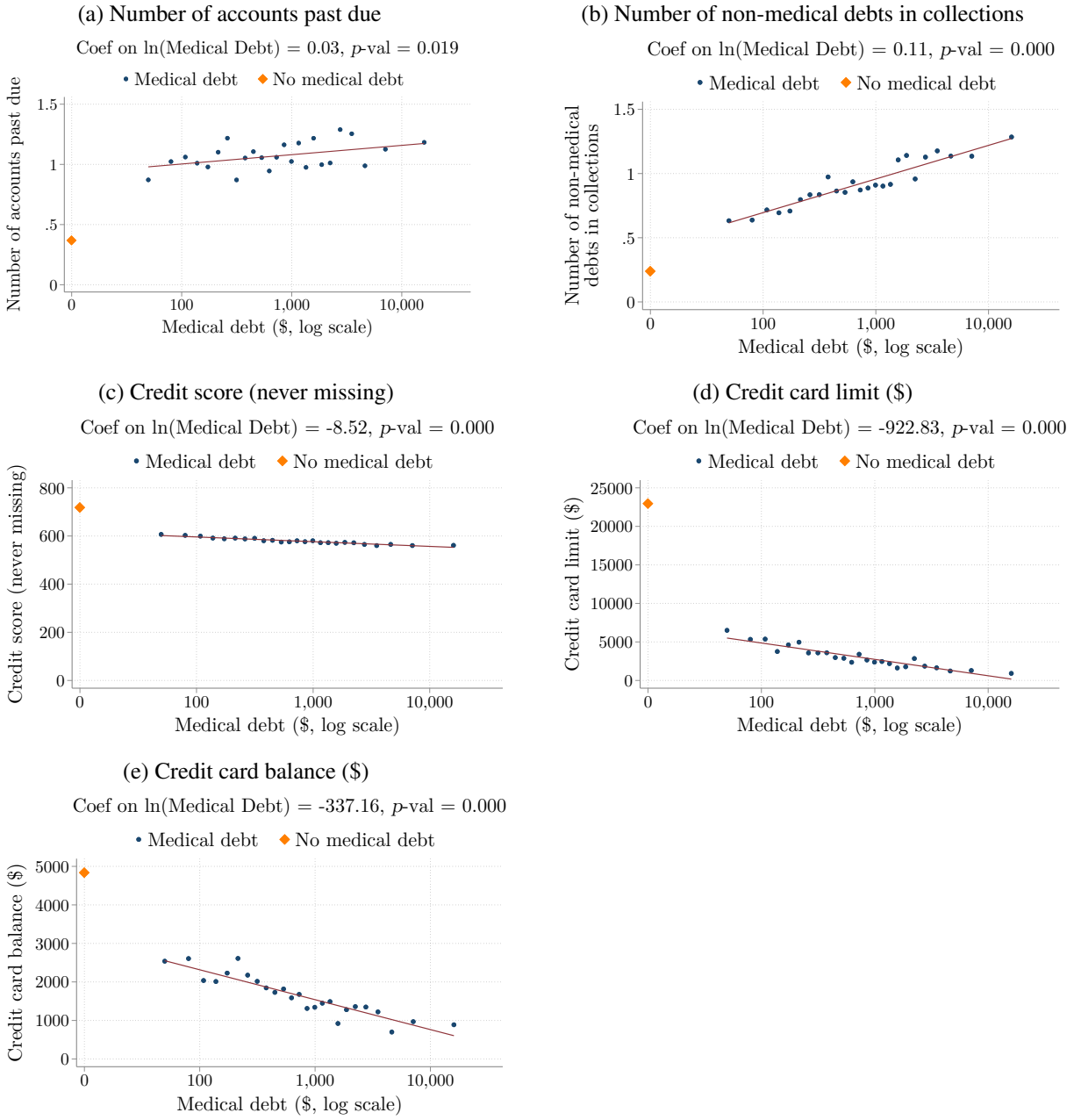
Notes: Figure presents binned scatterplots of medical debt versus the financial distress and health outcomes outlined in Appendix Table A2, using data from the SIPP and PSID (Institute for Social Research 2024; United States Census Bureau 2024). We use the 2019 wave of each survey to most closely align with the start of the hospital debt experiment (2018 Q3). The blue markers represent binned observations with medical debt, and the orange marker represents the bin of observations with no medical debt. The red lines represent the slopes from the intensive margin regression outlined in Equation 5, with coefficient and p-values listed at the top of the respective binned scatterplot. Debt balances are shown on a log scale. The conditional distribution of medical debt is winsorized at the 99th percentile.

Figure A2. Intensive Margin Relationships with Medical Debt in Nationally Representative Surveys (2021/2022)



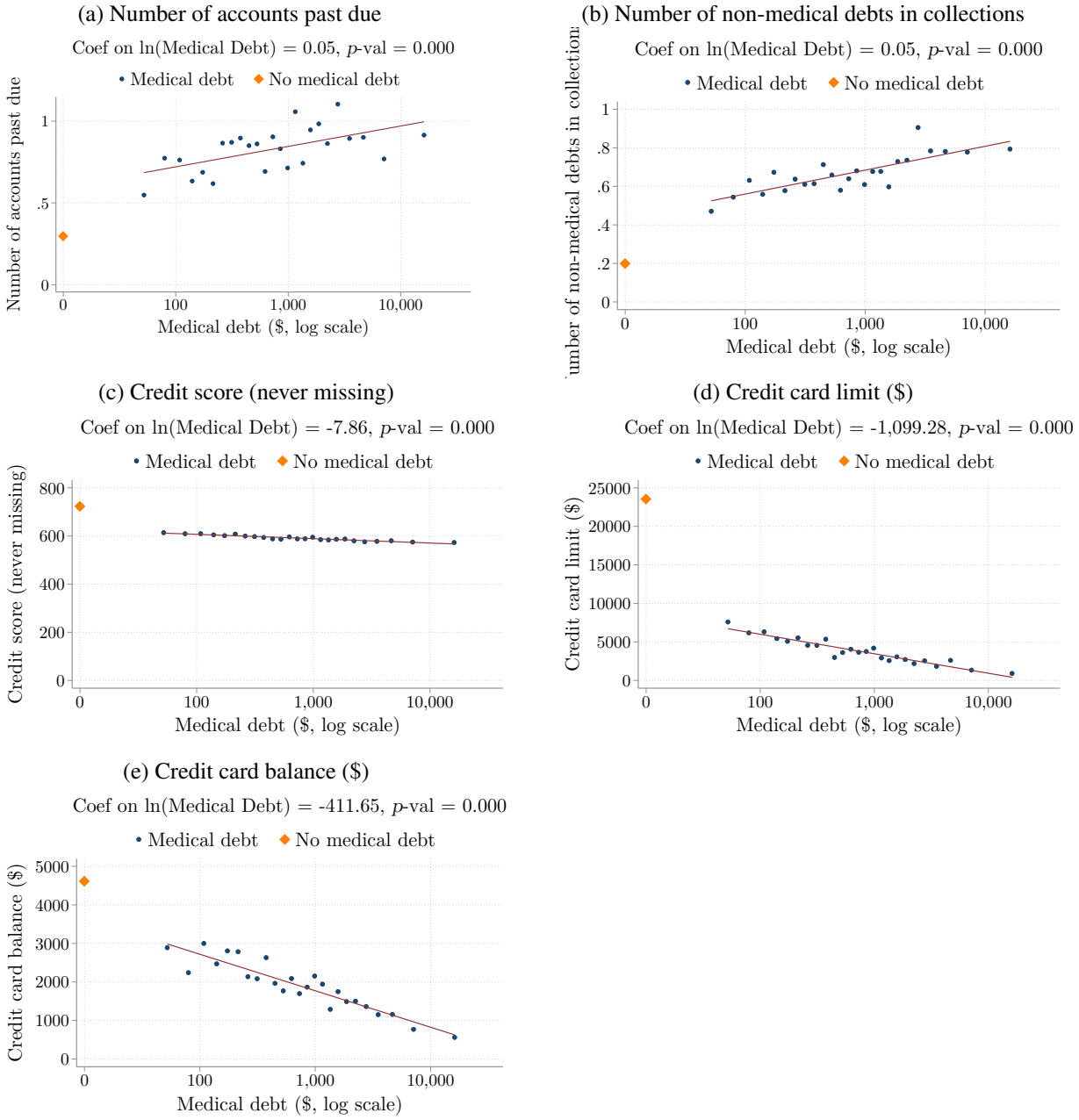
Notes: Figure presents binned scatterplots of medical debt versus the financial distress and health outcomes outlined in Appendix Table A2, using data from the SIPP and PSID (Institute for Social Research 2024; United States Census Bureau 2024). We use the 2022 SIPP and the 2021 PSID, reflecting the current policy regime with limited credit reporting of medical debt. The blue markers represent binned observations with medical debt, and the orange marker represents the bin of observations with no medical debt. The red lines represent the slopes from the intensive margin regression outlined in Equation 5, with coefficient and p-values listed at the top of the respective binned scatterplot. Debt balances are shown on a log scale. The conditional distribution of medical debt is winsorized at the 99th percentile.

Figure A3. Intensive Margin Relationships with Medical Debt in Nationally Representative Credit Bureau Sample (2018 Q3)



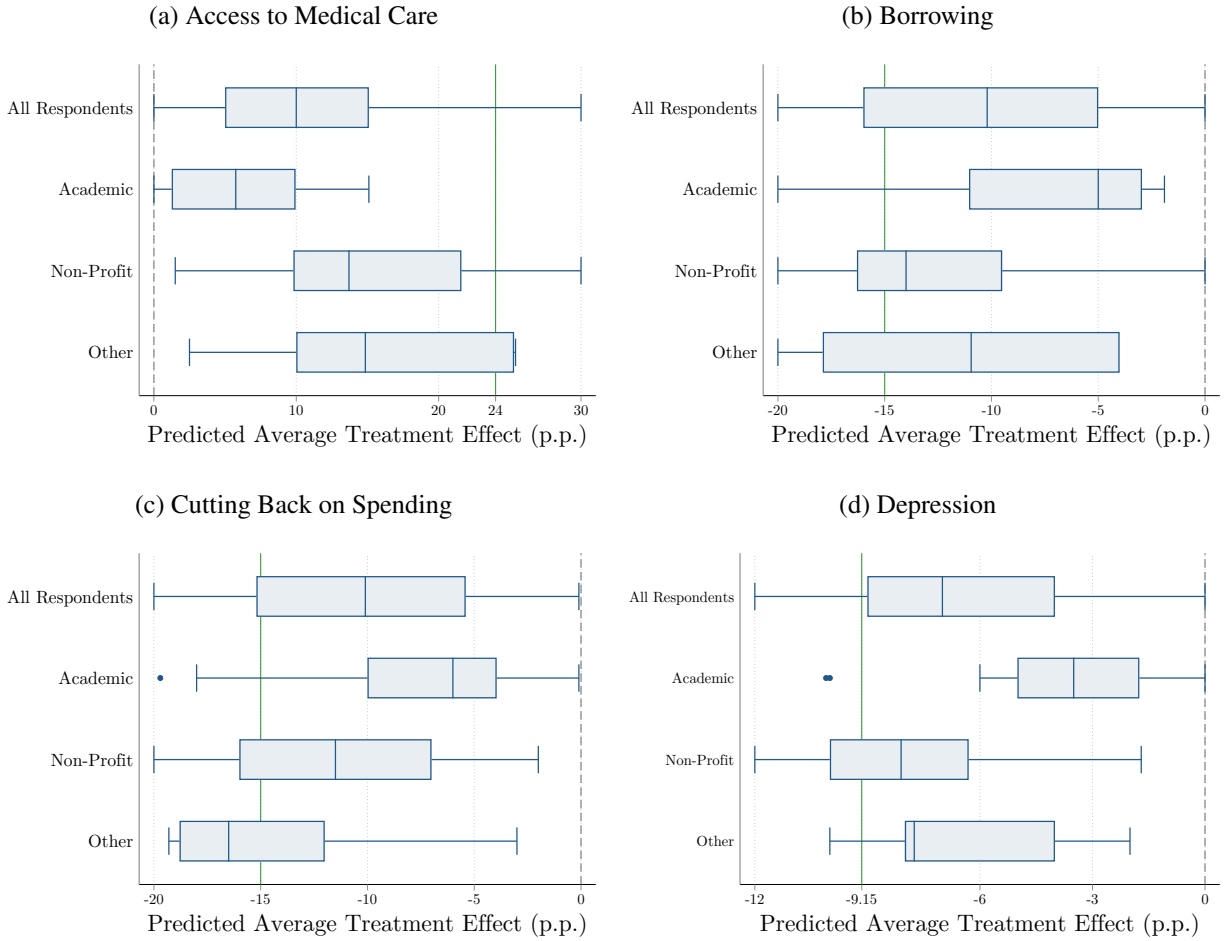
Notes: Figure presents binned scatterplots of medical debt versus the credit bureau outcomes, using data from the nationally representative credit bureau sample from TransUnion. We use data from 2018 Q3 to align with the start of the hospital debt experiment. The blue markers represent binned users with medical debt, and the orange marker represents the bin of users with zero medical debt. The red lines represent the slopes from the intensive margin regression outlined in Equation 5. In these regressions we take the log of debt balances, and in the binscatters we show debt balances using a log scale. The coefficient and p-value from each regression are listed at the top of the respective binned scatterplot. The conditional distribution of medical debt is winsorized at the 99th percentile.

Figure A4. Intensive Margin Relationships with Medical Debt in Nationally Representative Credit Bureau Sample (2021 Q4)



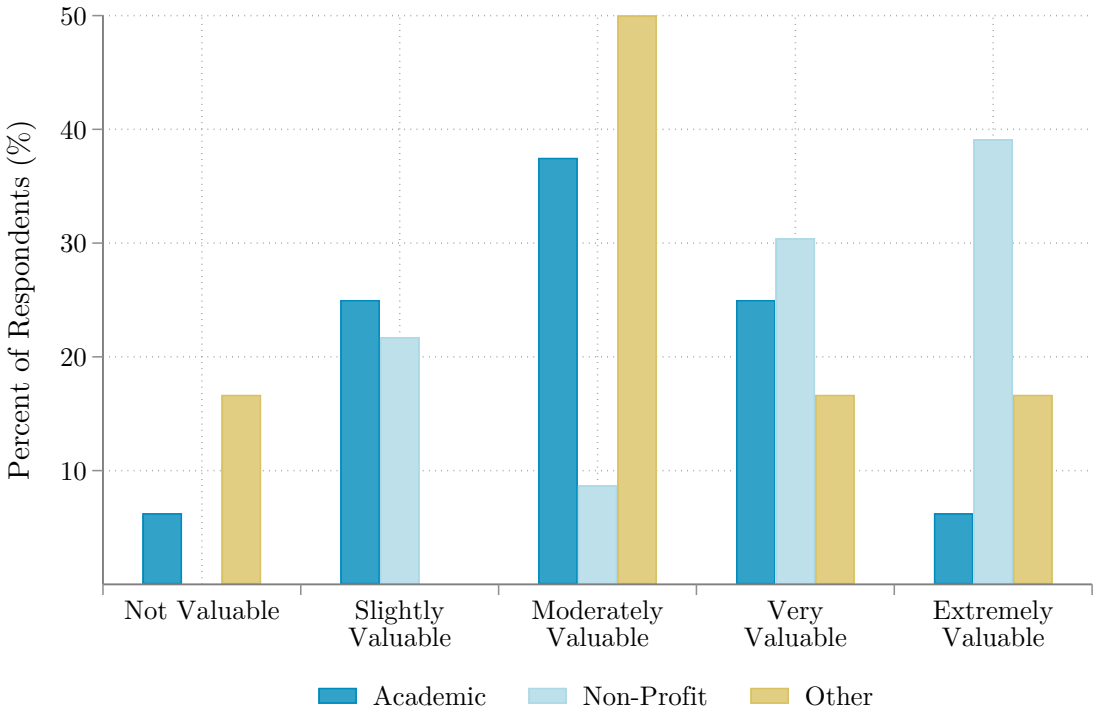
Notes: Figure presents binned scatterplots of medical debt versus the credit bureau outcomes, using data from the nationally representative credit bureau sample from TransUnion. We use data from 2021 Q4, reflecting the current policy regime with limited credit reporting of medical debt. The blue markers represent binned users with medical debt, and the orange marker represents the bin of users with zero medical debt. The red lines represent the slopes from the intensive margin regression outlined in Equation 5. In these regressions we take the log of debt balances, and in the binscatters we show debt balances using a log scale. The coefficient and p-value from each regression are listed at the top of the respective binned scatterplot. The conditional distribution of medical debt is winsorized at the 99th percentile.

Figure A5. Predicted Effect of Medical Debt Relief from Expert Survey



Notes: This figure shows box plots of expert predictions for the impact on medical debt relief on access to medical care (a), borrowing to cover medical bills (b), cutting back on spending to cover medical bills (c), and depression (d). 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 $\times$ 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 contextual treatment effects from the Oregon Health Insurance Experiment (Baicker et al. 2013) that were shown to respondents. Survey respondents were additionally provided with the statistic that 57% of the control group received all needed medical care, 87% had increased formal borrowing due to medical bills, 44% cut back on spending, and 47% had screened positive for depression.

Figure A6. Expert Survey: Value of Medical Debt Relief



Notes: This figure shows expert survey respondents' belief of the value of medical debt relief by occupation. See Appendix F for the corresponding expert survey instrument.

Figure A7. Sample Letter Sent to Treated Individual

RIP MEDICAL DEBT

THE CURE FOR MEDICAL DEBT

Name & address block

Date

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

Dear XYZ,

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

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

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

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

Regards,

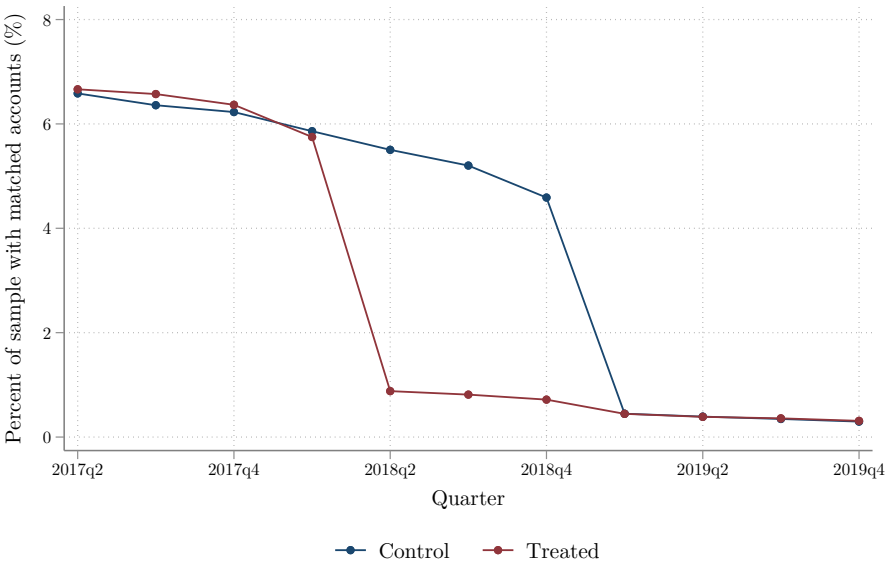
The Staff of RIP Medical Debt

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

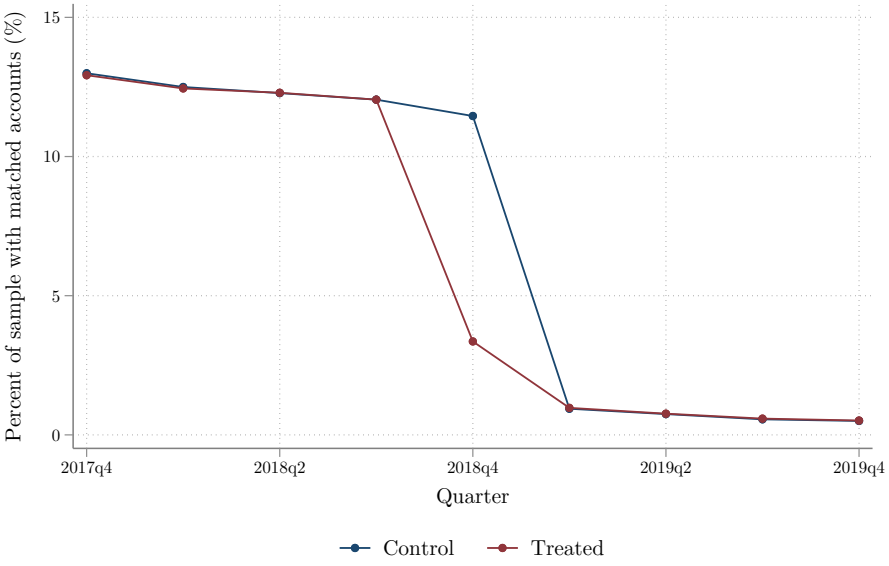
Notes: This figure presents an example of a letter sent to a recipient of debt relief. A Spanish translation was included on the reverse side.

Figure A8. Collections Account Match Rates

(a) Wave 1

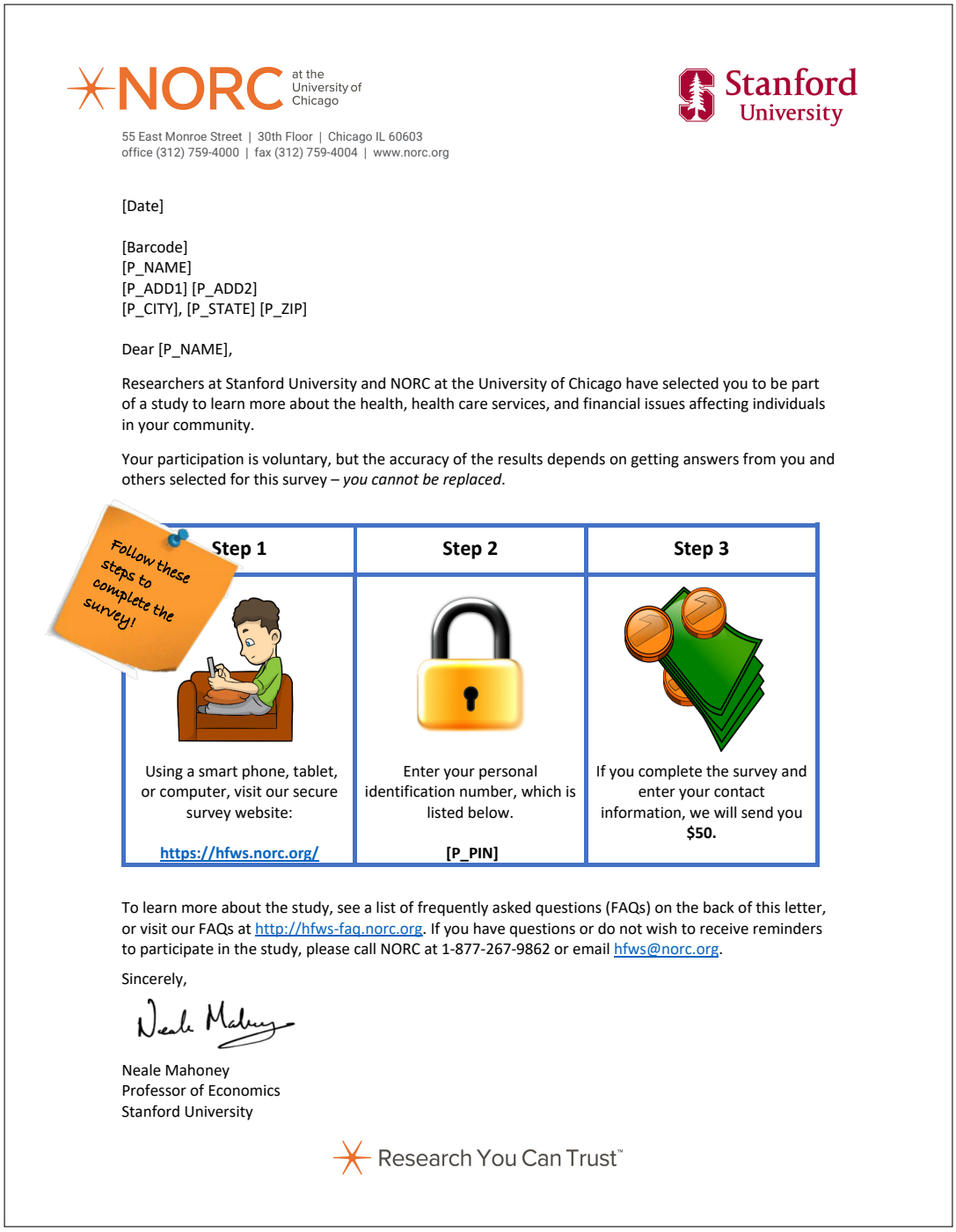


(b) Wave 2



Notes: Figures show the percent of collections account debt with matches in the credit report data, based on the dollar amount of medical debt. Match rates are shown separately for wave 1 and wave 2 of the collector debt experiment. See Appendix Section II.B for more details.

Figure A9. Survey Invitation Letter (Front)



NORC at the University of Chicago

55 East Monroe Street | 30th Floor | Chicago IL 60603
office (312) 759-4000 | fax (312) 759-4004 | www.norc.org

Stanford University

[Date]

[Barcode]
[P_NAME]
[P_ADD1] [P_ADD2]
[P_CITY], [P_STATE] [P_ZIP]

Dear [P_NAME],

Researchers at Stanford University and NORC at the University of Chicago have selected you to be part of a study to learn more about the health, health care services, and financial issues affecting individuals in your community.

Your participation is voluntary, but the accuracy of the results depends on getting answers from you and others selected for this survey – *you cannot be replaced*.

Follow these steps to complete the survey!

Step 1	Step 2	Step 3
Using a smart phone, tablet, or computer, visit our secure survey website: **https://hfws.norc.org/**	Enter your personal identification number, which is listed below. **[P_PIN]**	If you complete the survey and enter your contact information, we will send you **$50.**

To learn more about the study, see a list of frequently asked questions (FAQs) on the back of this letter, or visit our FAQs at http://hfws-faq.norc.org. If you have questions or do not wish to receive reminders to participate in the study, please call NORC at 1-877-267-9862 or email hfws@norc.org.

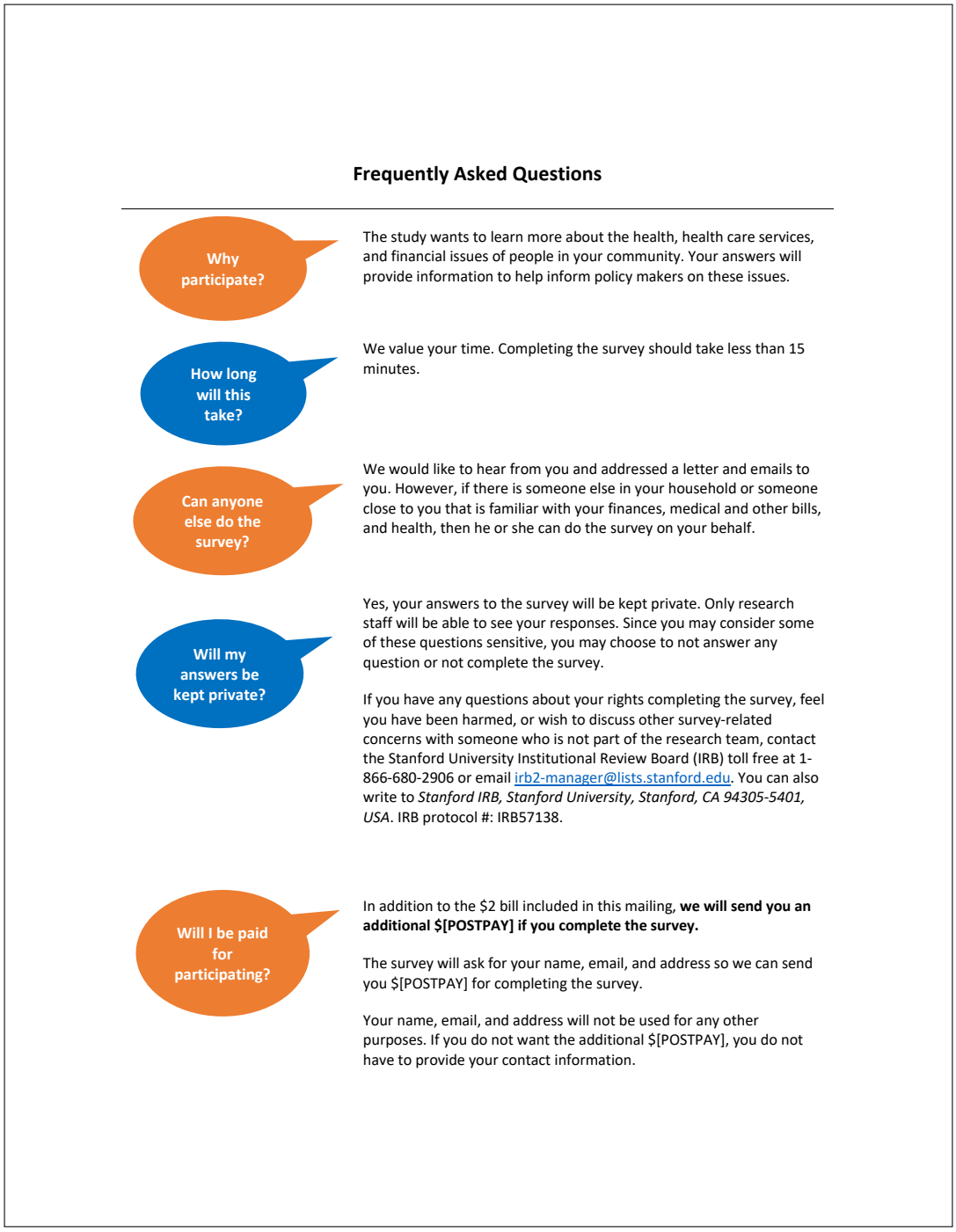
Sincerely,

Neale Mahoney
Professor of Economics
Stanford University

Research You Can Trust™

Notes: This figure presents an example of the invitation sent to our survey outreach sample. See A.3 for the corresponding protocol.

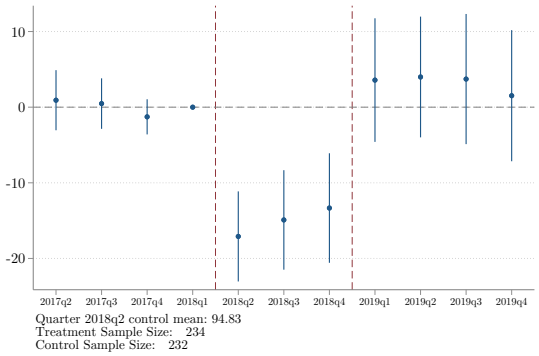
Figure A10. Survey Invitation Letter (Reverse)



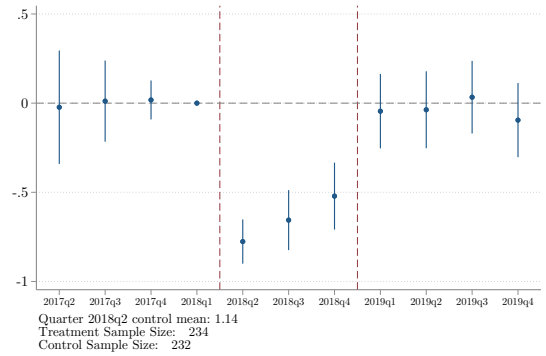
Notes: This figure presents an example of the invitation sent to our survey outreach sample. See Appendix Section A.3 for the corresponding protocol.

Figure A11. Effects of Debt Relief in Credit Reporting Subsample with No Other Debt in Collections

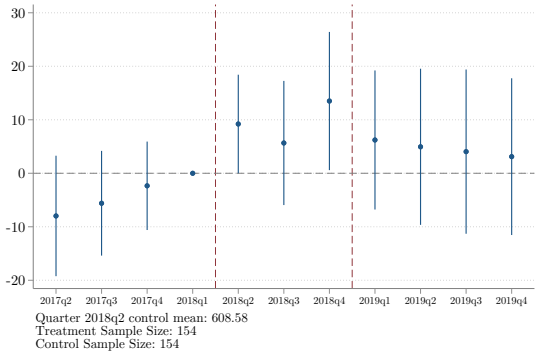
(a) Has credit score (%)



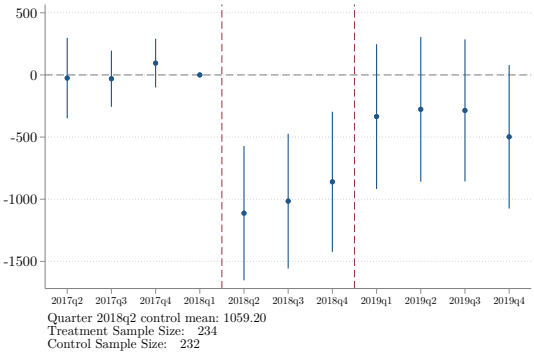
(b) Number of medical debts in collections



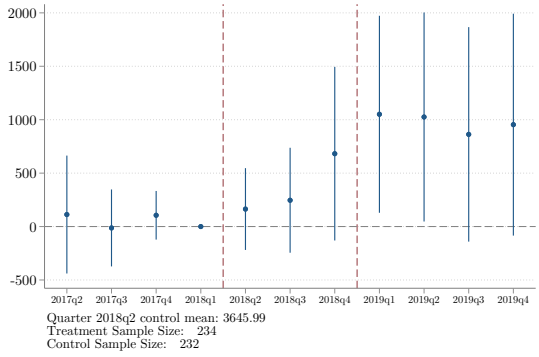
(c) Credit score (Never Missing)



(d) Medical debt in collections ($)



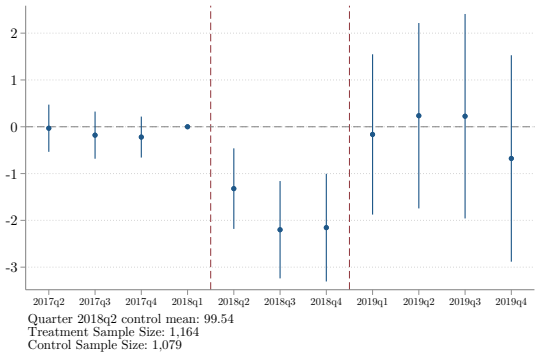
(e) Credit card limit ($)



Notes: Figure reports an event study of the effect of medical debt relief on credit access outcomes ((a), (c), and (e)) and medical debt in collections ((b) and (d)) for the subset of the wave 1 credit reporting subsample with other debt in collections. This specification, outlined in Section 3, allows the treatment effects from Table A46 to vary flexibly over time. This analysis includes observations from four quarters before the intervention (2017 Q2) to four quarters after the end of the control group reporting period (2019 Q4). The first dashed red line denotes the intervention date (i.e., the start of the control group reporting period), and the second dashed red line denotes the end of the control group reporting period. The blue markers represent point estimates, and the blue bars represent a 95% confidence interval around these estimates. At the bottom left of each figure we report the control mean from 2018 Q2 and the treatment and control group sample sizes.

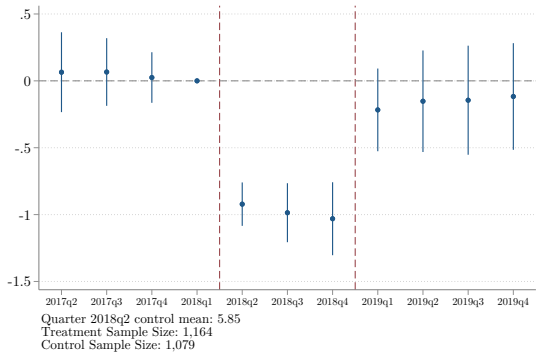
Figure A12. Effects of Debt Relief in Credit Reporting Subsample with Other Debt in Collections

(a) Has credit score (%)



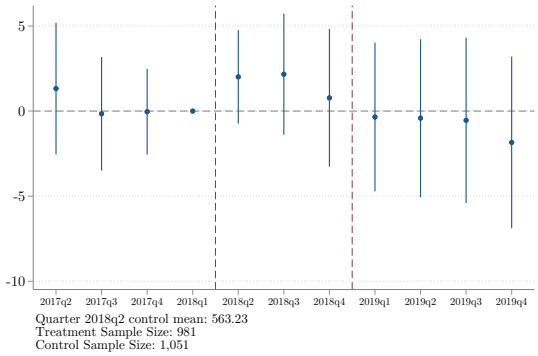
Quarter 2018q2 control mean: 99.54
Treatment Sample Size: 1,164
Control Sample Size: 1,079

(b) Number of medical debts in collections



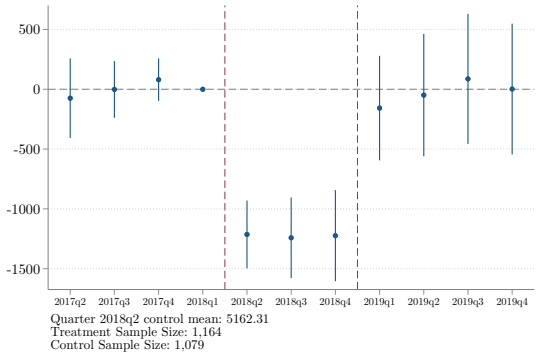
Quarter 2018q2 control mean: 5.85
Treatment Sample Size: 1,164
Control Sample Size: 1,079

(c) Credit score (Never Missing)



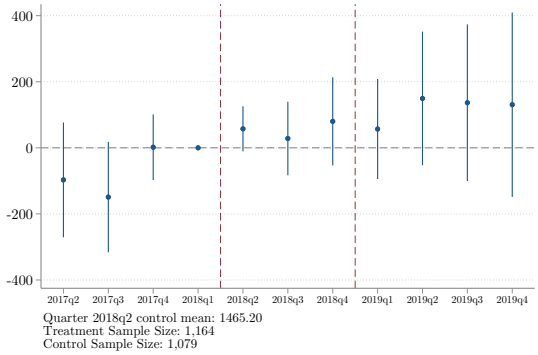
Quarter 2018q2 control mean: 563.23
Treatment Sample Size: 981
Control Sample Size: 1,051

(d) Medical debt in collections ($)



Quarter 2018q2 control mean: 5162.31
Treatment Sample Size: 1,164
Control Sample Size: 1,079

(e) Credit card limit ($)



Quarter 2018q2 control mean: 1465.20
Treatment Sample Size: 1,164
Control Sample Size: 1,079

Notes: Figure reports an event study of the effect of medical debt relief on credit access outcomes ((a), (c), and (e)) and medical debt in collections ((b) and (d)) for the subset of the wave 1 credit reporting subsample with no other debt in collections. This specification, outlined in Section 3, allows the treatment effects from Appendix Table A45 to vary flexibly over time. This analysis includes observations from four quarters before the intervention (2017 Q2) to four quarters after the end of the control group reporting period (2019 Q4). The first dashed red line denotes the intervention date (i.e., the start of the control group reporting period), and the second dashed red line denotes the end of the control group reporting period. The blue markers represent point estimates, and the blue bars represent a 95% confidence interval around these estimates. At the bottom left of each figure we report the control mean from 2018 Q2 and the treatment and control group sample sizes. Figure reports event study estimates for the credit access outcomes in Appendix Table A46. As such, the sample is restricted to individuals who have debts in collections with TransUnion beyond their medical debts.

Experts Survey

Survey Flow

Block: Introduction (2 Questions)

Branch: New Branch
If
If Consent No Is Selected

EndSurvey:

Branch: New Branch
If
If Consent Yes Is Selected

Standard: Main Survey (16 Questions)

EndSurvey:

EndSurvey: Advanced

Page Break

Start of Block: Introduction

Introduction
[Consent language is displayed here, see “Expert_Survey_Info_Sheet”]

Consent Given the information above, do you wish to participate in the survey?

- ○ Yes (1)
- ○ No (2)

End of Block: Introduction

Start of Block: Main Survey

Demo_education What is the highest degree that you have completed?

- ○ PhD (1)
- ○ Masters or Professional Degree (2)
- ○ Bachelor's or 4-year college (3)
- ○ Other (4)

Demo_employer Which of the following options best describes your primary employer?

- ○ Federal Government, Executive Branch (1)
- ○ Federal Government, Congress (2)
- ○ State Government (3)
- ○ Private company: Debt collection industry (4)
- ○ Private company: Other industry (Please specify) (5) _____
- ○ Non-profit or advocacy organization (6)
- ○ University or other academic institution (7)
- ○ Think-tank (8)
- ○ Other (9) _____

Page Break

context1 **Description of our experiment**

We studied patients at a large hospital system with unpaid bills that would typically be sent to collections. This medical debt amounted to **$1,500 on average**. We partnered with a non-profit called RIP Medical Debt to conduct a randomized controlled trial in which patients were randomly assigned to either:

Treatment group: Had this medical debt forgiven. **Control group**: Had this medical debt collected on as normal by a debt collection company.

The treatment group's debt was forgiven 15 months after the initiating medical event on average, at a cost of $0.06 per dollar of debt. The treatment group was informed of debt forgiveness in two letters sent in the mail.

Next: Your predictions of our findings

Page Break

context2 **Description of this survey**
Around one year after debt forgiveness, we surveyed patients in both the treatment group and the control group to measure the impacts of medical debt forgiveness on health, healthcare utilization, and financial well-being.

Your predictions

We would like to ask you about **your predictions** of the impacts of debt forgiveness on these outcomes.

Page Break

JS

phq8_op
Question 1/5

Our primary outcome is whether the subject screened positive for depression. To measure depression, we used the Personal Health Questionnaire for Depression Scale, or PHQ8.

In our study, 47% of the **control group** screened positive for depression. By how much do you think the average $1,500 in medical debt forgiveness reduced depression in the **treatment group** (compared to the **control group**)?

If you think the debt forgiveness had a similar impact as gaining health insurance coverage through Medicaid, your answer would be around a 9 percentage point decrease (the finding from the Oregon Health Insurance Experiment). If you think the debt forgiveness had little effect, your answer would be closer to 0.

	0 2 4 6 8 10 12
Your answer (in percentage points) ()	

phq8_op_conf How certain are you of your answer?

- ○ Not certain at all (1)
- ○ Slightly certain (2)
- ○ Moderately certain (3)
- ○ Very certain (4)
- ○ Extremely certain (5)

Page Break

JS

allneed_op
Question 2/5

We also asked subjects, “In the last 12 months, did you get all the medical care you needed?”

In our study, 57% of the **control group** reported getting all the medical care they needed. By how much do you think the average $1,500 in medical debt forgiveness increased the percentage of patients receiving all needed medical care in the **treatment group**?

If you think the debt forgiveness had a similar impact as gaining insurance through Medicaid, your answer would be around a 24 percentage point increase. If you think debt forgiveness had little effect, your answer would be closer to 0.

	0 5 10 15 20 25 30
Your answer (in percentage points) ()	

allneed_op_conf How certain are you of your answer?

- ○ Not certain at all (1)
- ○ Slightly certain (2)
- ○ Moderately certain (3)
- ○ Very certain (4)
- ○ Extremely certain (5)

Page Break

JS

bills_op

Question 3/5

To assess financial wellbeing, we asked study subjects “Due to medical bills, have you cut back on spending in the past 12 months on basic necessities?”

In our study, 44% of the **control group** reported cutting back on spending on basic necessities. By how much do you think the average $1,500 in medical debt forgiveness reduced this percentage?

If you think the debt forgiveness had a similar impact as gaining insurance through Medicaid, your answer would be around a 15 percentage point decrease. If debt forgiveness had little effect, your answer would be closer to 0.

	0 4 8 12 16 20
Your answer (in percentage points) ()	

bills_op_conf How certain are you of your answer?

- ○ Not certain at all (1)
- ○ Slightly certain (2)
- ○ Moderately certain (3)
- ○ Very certain (4)
- ○ Extremely certain (5)

Page Break

JS

health_op
Question 4/5

To further assess whether medical debt forgiveness improved financial well-being, we asked subjects whether they had increased formal borrowing (i.e. credit cards, payday loans, or other lines of credit) in the past 12 months due to medical bills.

In our study, 87% of the **control group** reported they had increased formal borrowing due to medical bills. By how much do you think the average $1,500 in medical debt forgiveness lowered the percentage of patients needing to borrow more because of medical debt?

If you think the debt forgiveness had a similar impact as gaining insurance through Medicaid, your answer would be around a 15 percentage point decrease. If you think debt forgiveness had little effect, your answer would be closer to 0.

	0 4 8 12 16 20
Your answer (in percentage points) ()	

health_op_conf How certain are you of your answer?

- ○ Not certain at all (1)
- ○ Slightly certain (2)
- ○ Moderately certain (3)
- ○ Very certain (4)
- ○ Extremely certain (5)

Page Break

value_op **Question 5/5**

Do you think that medical debt forgiveness is a valuable use of charity resources?

- ○ Not valuable at all (1)
- ○ Slightly valuable (2)
- ○ Moderately valuable (3)
- ○ Very valuable (4)
- ○ Extremely valuable (5)

---

value_text Please use this final question to explain your response to the prior question, and to expound on the predictions you made about the effects of medical debt forgiveness.

_____

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Page Break

contact_giftcard Thank you for your response!

In order to deliver your $25 gift card, we need you to enter to enter an email address where you wish to receive payment in. We will issue your payment within 14 days.

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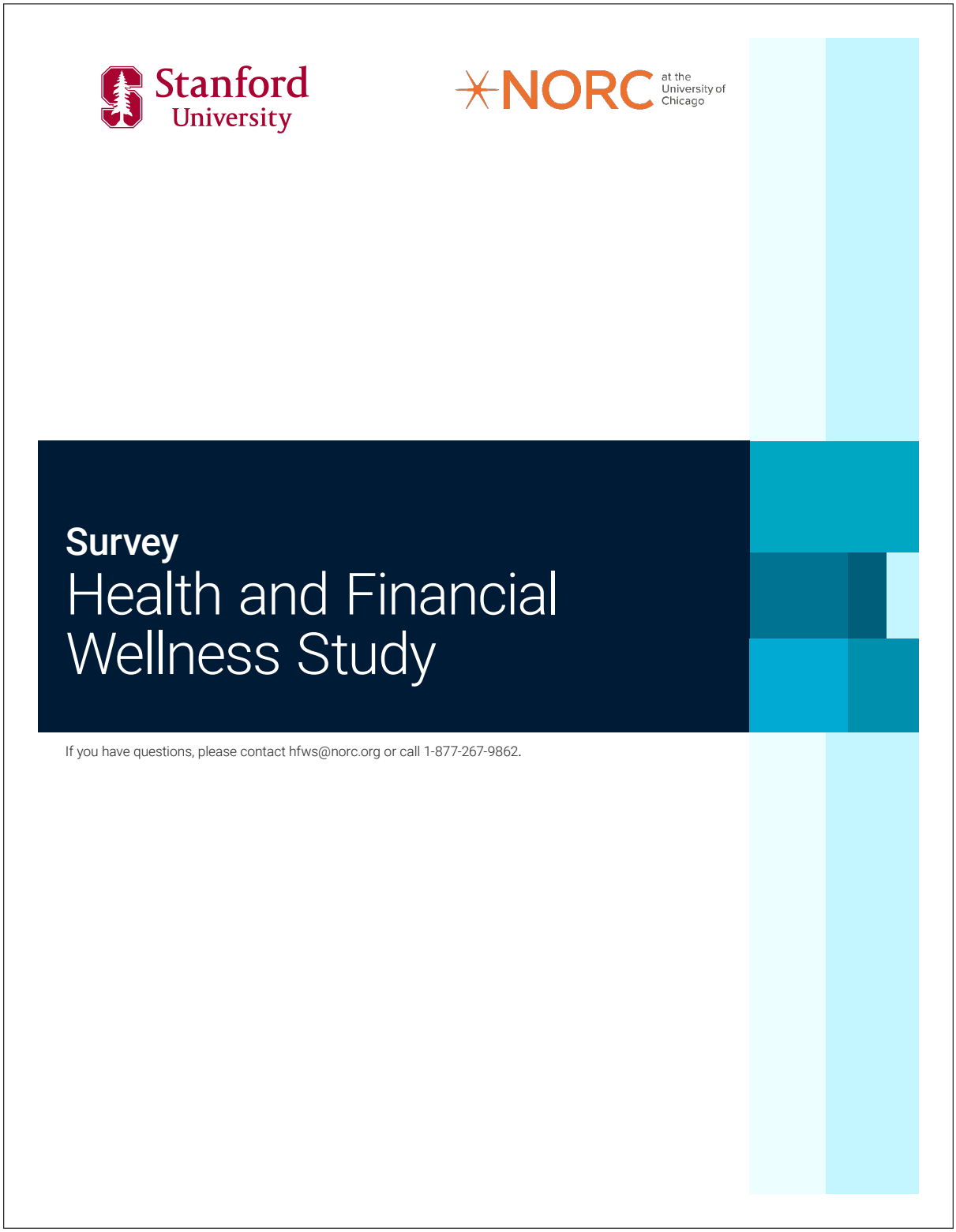
contact_email IMPORTANT: Enter a personal email address here. We cannot process your payment if you do not provide one below.

_____

End of Block: Main Survey


Stanford University
NORC at the University of Chicago
Survey
Health and Financial Wellness Study
If you have questions, please contact hfws@norc.org or call 1-877-267-9862.

Health and Financial Wellness Study

The purpose of this survey is to learn more about the health, health care services, and financial issues affecting individuals in your community. It is your choice to take part in the survey, and you can decide not to answer one or more questions. We cannot and do not guarantee or promise that you will receive any benefits from this study. All answers will remain confidential. The results of the study will be reported for large groups of people and will not contain names or other information that identifies you. We will remove any information that identifies you from the study results and information. Another investigator could use this information for future research studies once you agree to participate in this survey now.

Completing the survey should take no more than 15 minutes. To thank you for your participation, we will send you $50 for completing the survey.

If you have any questions about your rights as a participant in this research, feel you have been harmed, or wish to discuss other study-related concerns with someone who is not part of the research team, you can contact the Stanford Institutional Review Board (IRB):

phone: 1-866-680-2906

email: irb2-manager@lists.stanford.edu

IRB protocol #: IRB57138

INSTRUCTIONS FOR COMPLETING THE SURVEY

Please answer the questions in this survey using a pen with blue or black ink.

This survey contains several types of questions:

1 **For some questions, you answer the question by marking a box, like this:**

1 ☒ Yes
2 ☐ No

2 **You are sometimes told to skip over questions in this survey. When this happens, you will see an arrow with a note that tells you what question to answer next, like this:**

1 ☐ Yes → *Go to question 4*
2 ☐ No

1 **Thinking about everyone in your *household,* including you, how much does your household owe in *combined* medical bills, including to healthcare providers, credit card companies, family and friends, or anyone else?** *Your best estimate is fine.*

1 ☐ $0 → *Go to question 5*
2 ☐ $1 to $500
3 ☐ $501 to $1,000
4 ☐ $1,001 to $2,500
5 ☐ $2,501 to $5,000
6 ☐ $5,001 to $10,000
7 ☐ $10,001 to $20,000
8 ☐ $20,001 to $30,000
9 ☐ $30,001 or more
77 ☐ I don't know

2 **Now, thinking only about the medical care you've received, how much do *you* owe in total medical bills, including to healthcare providers, credit card companies, family and friends, or anyone else?** *Your best estimate is fine.*

1 ☐ $0 → *Go to question 5*
2 ☐ $1 to $500
3 ☐ $501 to $1,000
4 ☐ $1,001 to $2,500
5 ☐ $2,501 to $5,000
6 ☐ $5,001 to $10,000
7 ☐ $10,001 to $20,000
8 ☐ $20,001 to $30,000
9 ☐ $30,001 or more
77 ☐ I don't know

3 **How much of your medical bills do you *expect* to pay?** *Your best estimate is fine.*

1 ☐ $0
2 ☐ $1 to $500
3 ☐ $501 to $1,000
4 ☐ $1,001 to $2,500
5 ☐ $2,501 to $5,000
6 ☐ $5,001 to $10,000
7 ☐ $10,001 to $20,000
8 ☐ $20,001 to $30,000
9 ☐ $30,001 or more
77 ☐ I don't know

4 **How much of your medical bills do you feel it would be *fair* for you to pay?** *Your best estimate is fine.*

1 ☐ $0
2 ☐ $1 to $500
3 ☐ $501 to $1,000
4 ☐ $1,001 to $2,500
5 ☐ $2,501 to $5,000
6 ☐ $5,001 to $10,000
7 ☐ $10,001 to $20,000
8 ☐ $20,001 to $30,000
9 ☐ $30,001 or more
77 ☐ I don't know

5 **In the past 12 months, did you have problems paying or an inability to pay any medical bills, such as bills for doctors, dentists, medication, or home care?**

1 ☐ Yes
2 ☐ No → *Go to question 9*
77 ☐ I don't know

6 **What were the reasons you had trouble paying your medical bills?** *Mark all that apply.*

1 ☐ Didn't have health insurance
2 ☐ Had health insurance, but copay or deductible was too high
3 ☐ Submitted a claim to insurance company but all or part of the claim was denied
4 ☐ Other, *please specify:*

7 **Which of the following comes closer to describing the medical bills you've had problems paying?**

1 ☐ Bills for a one-time or short-term medical expense, such as a single hospital stay or treatment for an accident
2 ☐ Bills that have built up over time, such as treatment for a chronic illness like diabetes or cancer
77 ☐ I don't know

8 **In the past 12 months, how often have you been contacted by a debt collector about paying your past medical bills?**

1 ☐ Never
2 ☐ Once a month or less
3 ☐ A few times a month
4 ☐ A few times a week
5 ☐ Daily or more

9 **Besides medical bills, have you had problems paying other types of bills in the past 12 months?**

1 ☐ Yes
2 ☐ No → *Go to question 11*
77 ☐ I don't know

10 **What reasons caused you to have problems paying other types of bills?** *Mark all that apply.*

1 ☐ Had to pay medical bills
2 ☐ Lost job
3 ☐ Couldn't work as much as I'd like
4 ☐ Got divorced or separated
5 ☐ Spent too much money
6 ☐ Had to make interest payments
7 ☐ Other, *please specify:*

11 **For the last 12 months, how many months did you have some kind of health insurance?** *Your best estimate is fine.*

1 ☐ Never
2 ☐ 1 to 5 months
3 ☐ 6 to 11 months
4 ☐ The whole time (all 12 months)
77 ☐ I don't know

12 **How has the COVID-19 (Coronavirus) pandemic affected your health insurance coverage?**

1 ☐ Lost insurance coverage and have not regained coverage
2 ☐ Lost insurance coverage but have regained coverage
3 ☐ NA - Health insurance has not been affected

13 **In the past 18 months, has any of your medical debt been forgiven by a charity or non-profit organization?**

1 ☐ Yes
2 ☐ No → *Go to question 16*
77 ☐ I don't know

14 **How much of your medical debt was forgiven in the past 18 months?** *Your best estimate is fine.*

1 ☐ Less than $500
2 ☐ $501 to $1,000
3 ☐ $1,001 to $2,500
4 ☐ $2,501 to $5,000
5 ☐ $5,001 to $10,000
6 ☐ $10,001 or more
77 ☐ I don't know

15 **Overall, how much of an impact has this debt forgiveness had on you and your family?**

1 ☐ A major impact
2 ☐ A minor impact
3 ☐ No real impact
77 ☐ I don't know

16 **<u>As a result of medical bills</u> have you cut back on spending in the past 12 months on…**

	Yes	No	I don't know
a. Basic necessities like food, heat or housing, or other basic household items?	1 ☐	2 ☐	77 ☐
b. Big-ticket items like cars, furniture, or appliances?	1 ☐	2 ☐	77 ☐
c. Business investments?	1 ☐	2 ☐	77 ☐

17 **<u>As a result of medical bills</u>, in the past 12 months have you…?**

	Yes	No	I don't know
a. Increased your credit card debt, or charge card debt?	1 ☐	2 ☐	77 ☐
b. Borrowed money from a payday lender?	1 ☐	2 ☐	77 ☐
c. Borrowed from friends and family?	1 ☐	2 ☐	77 ☐
d. Used up all or most of your savings?	1 ☐	2 ☐	77 ☐
e. Increased debt on other lines of credit?	1 ☐	2 ☐	77 ☐

18 **Taken all together, how would you say things are these days – would you say that you are…?**

1 ☐ Very happy
2 ☐ Pretty happy
3 ☐ Not too happy

19 **In general, would you say your health is:**

1 ☐ Excellent
2 ☐ Very good
3 ☐ Good
4 ☐ Fair
5 ☐ Poor

20 **How has your health changed in the last 12 months? Would you say your health…**

1 ☐ Has gotten better
2 ☐ Is about the same
3 ☐ Has gotten worse

21 **Over the last 2 weeks, how often have you been bothered by any of the following problems?**

	Not at all	Several days	More than half the days	Nearly every day
a. Little interest or pleasure in doing things	1☐	2☐	3☐	4☐
b. Feeling down, depressed, or hopeless	1☐	2☐	3☐	4☐
c. Trouble falling or staying asleep, or sleeping too much	1☐	2☐	3☐	4☐
d. Feeling tired or having little energy	1☐	2☐	3☐	4☐
e. Poor appetite or overeating	1☐	2☐	3☐	4☐
f. Feeling bad about yourself or that you are a failure or have let yourself or your family down	1☐	2☐	3☐	4☐
g. Trouble concentrating on things, such as reading the newspaper or watching television	1☐	2☐	3☐	4☐
h. Moving or speaking so slowly that other people have noticed. Or the opposite—being so fidgety or restless that you have been moving around a lot more than usual	1☐	2☐	3☐	4☐

22 **Over the last 2 weeks, how often have you been bothered by any of the following problems?**

	Not at all	Several days	More than half the days	Nearly every day
a. Feeling nervous, anxious, or on edge	1☐	2☐	3☐	4☐
b. Not being able to stop or control worrying	1☐	2☐	3☐	4☐
c. Worrying too much about different things	1☐	2☐	3☐	4☐
d. Trouble relaxing	1☐	2☐	3☐	4☐
e. Being so restless that it is hard to sit still	1☐	2☐	3☐	4☐
f. Becoming easily annoyed or irritable	1☐	2☐	3☐	4☐
g. Feeling afraid as if something awful might happen	1☐	2☐	3☐	4☐

23 **Stress means a situation in which a person feels tense, restless, nervous or anxious or is unable to sleep at night because his/her mind is troubled all the time. Do you feel this kind of stress these days?**

1☐ Never
2☐ Rarely
3☐ Sometimes
4☐ Often
5☐ Always

24 **If you needed medical care in the last 12 months, did you get ALL the medical care you needed?**

1☐ Yes → *Go to question 26*
2☐ No
3☐ NA - Did not need medical care in the past 12 months → *Go to question 26*

25 **The most recent time you went without needed medical care, what were the main reasons?** *Mark all that apply.*

1☐ It cost too much
2☐ Didn't have insurance
4☐ Owed money to the care provider
8☐ Reasons related to the COVID-19 pandemic (e.g., the office was closed or was worried about getting COVID-19)
7☐ Some other reason, *please specify:*

26 **If you needed prescription medications in the last 12 months, did you get all the prescription medications you needed?**

1☐ Yes → *Go to question 28*
2☐ No
3☐ NA - Did not need prescription medications in the past 12 months → *Go to question 28*

27 **The most recent time you went without prescription medications you needed, what were the main reasons?** *Mark all that apply.*

1☐ They cost too much
2☐ Didn't have insurance
4☐ Couldn't get a prescription
5☐ Reasons related to the COVID-19 pandemic (e.g., the pharmacy was closed or was worried about getting COVID-19)
6☐ Some other reason, *please specify:*

77☐ I don't know

28 **Would you describe yourself as Spanish, Hispanic, or Latino?**

1☐ Yes
2☐ No

29 **How would you describe your race?** *Mark all that apply.*

1 ☐ White
2 ☐ Black or African-American
3 ☐ Asian
4 ☐ American Indian or Alaska Native
5 ☐ Native Hawaiian or Pacific Islander
6 ☐ Other, *please specify:*

77 ☐ I don't know

30 **What is the highest level of education you have completed?**

1 ☐ Less than high school
2 ☐ High school diploma or GED
3 ☐ Some college but no degree
4 ☐ Vocational training
5 ☐ 2-year degree
6 ☐ Bachelor's degree
7 ☐ Master's degree
8 ☐ Professional school degree
9 ☐ Doctorate degree

31 **Are you currently employed or self-employed?**

1 ☐ Yes
2 ☐ No

32 **How has the COVID-19 (Coronavirus) pandemic affected your employment?** *Mark all that apply.*

1 ☐ Permanently lost job
2 ☐ Furloughed or temporarily laid off
3 ☐ Hours and/or pay were cut
4 ☐ NA - No impact on employment

33 **How has the COVID-19 (Coronavirus) pandemic affected any other member of your household's employment?** *Mark all that apply.*

1 ☐ They permanently lost their job(s)
2 ☐ They were furloughed or temporarily laid off
3 ☐ Their hours and/or pay were cut
4 ☐ No impact on their employment
5 ☐ NA - I am single or the only household member who works

34 **What was your gross household income (before taxes and deductions taken out) for last year?** *Your best estimate is fine.*

$30,000 and below

1 ☐ Under $2,501
2 ☐ $2,501 to $5,000
3 ☐ $5,001 to $7,500
4 ☐ $7,501 to $10,000
5 ☐ $10,001 to $12,500
6 ☐ $12,501 to $15,000
7 ☐ $15,001 to $17,500
8 ☐ $17,501 to $20,000
9 ☐ $20,001 to $22,500
10 ☐ $22,501 to $25,000
11 ☐ $25,001 to $27,500
12 ☐ $27,501 to $30,000

Between $30,001 and $55,000

13 ☐ $30,001 to $32,500
14 ☐ $32,501 to $35,000
15 ☐ $35,001 to $37,500
16 ☐ $37,501 to $40,000
17 ☐ $40,001 to $42,500
18 ☐ $42,501 to $45,000
19 ☐ $45,001 to $47,500
20 ☐ $47,501 to $50,000
21 ☐ $50,001 to $52,500
22 ☐ $52,501 to $55,000

Between $55,001 and $80,000

23 ☐ $55,001 to $57,500
24 ☐ $57,501 to $60,000
25 ☐ $60,001 to $62,500
26 ☐ $62,501 to $65,000
27 ☐ $65,001 to $67,500
28 ☐ $67,501 to $70,000
29 ☐ $70,001 to $72,500
30 ☐ $72,501 to $75,000
31 ☐ $75,001 to $77,500
32 ☐ $77,501 to $80,000

Between $80,001 and $100,000+

33 ☐ $80,001 to $82,500
34 ☐ $82,501 to $85,000
35 ☐ $85,001 to $87,500
36 ☐ $87,501 to $90,000
37 ☐ $90,001 to $92,500
38 ☐ $92,501 to $95,000
39 ☐ $95,001 to $97,500
40 ☐ $97,501 to $100,000
41 ☐ $100,001 and over

THANK YOU FOR YOUR PARTICIPATION.

Your responses will provide valuable information about the health, health care services, and financial issues affecting individuals in your community.

Please provide your name, email, and address so we can send you a token of appreciation for completing this survey.

Name:

Email address:

Street address 1:

Street address 2:

City:

State:

Zip code:

Date:

Please place your completed survey in the pre-paid return envelope and mail back to:

NORC at the University of Chicago
55 East Monroe Street
Suite 1900
Chicago, IL 60603

If you have misplaced the pre-paid return envelope or have any further questions or feedback about this study, please contact the study team at **1-877-267-9862** or email **hfws@norc.org**.