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

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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. *JEL codes:* D18, G51, H75, I13, L31.

* We are grateful to seminar and conference participants at Harvard Business School, Northwestern Kellogg Strategy, University of Chicago Health, Stanford Institute for Economic Policy Research, University of Duisburg-Essen, University of Wisconsin–Madison Economics, Chicago Booth Finance, Waseda University Economics, NBER Economics of Health, NBER Household Finance, the AEA Health Economics Research Organization Session, BYU Finance, Wharton Health Care Management, CEPR Household Finance, the CFPB Research Conference, Urban Institute, ASHEcon Conference, NBER Summer Institute Public Economics, Yale Law School, MIT Economics, and the FDIC Consumer Research Division for helpful comments. We thank Constantine Yannelis, Tal Gross, Steffen Andersen, Michael Murto, and Martin Hackmann for thoughtful discussions

The Quarterly Journal of Economics (2025), 1187–1241. https://doi.org/10.1093/qje/qjae045.
Advance Access publication on December 26, 2024.

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 2 more are considering programs that would raise this total to over $14.6 billion (see Online 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

of the article and Will Dobbie, Zack Cooper, Amy Finkelstein, Paul Goldsmith-Pinkham, and Matthew Notowidigdo for thoughtful comments. The experiments reported in this study are listed in the AEA RCT Registry (nos. 0003332, 0003664, and 0007426) and were approved by Stanford IRB (no. 57138). We gratefully acknowledge J-PAL North America, the National Institutes of Health (R01 AG066890-01A1), and the National Institute on Aging (T32-AG000186) for financial support and RIP Medical Debt for their partnership on the study. We thank Julie Gasparac, Laurie Imhof, and Nithya Rajendran at NORC at the University of Chicago for survey implementation, and Jinglin Wang, Bruno Mauricio Escobar Izquierdo, Zahra Thabet, and Eleanor Jenke for superb research assistance.

1. All of these statistics are from the same nationally representative 2,375-person Lopes et al. (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 article because that was the name used during the intervention.

negative financial and health outcomes (e.g., Kale and Carroll 2016; Zafar 2016; Banegas et al. 2019; Novak, 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 effects on households may be a small fraction of the face value of the 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 article 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 nonprofit 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 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 $1 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 protocols. 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 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* subexperiment, which allows us to estimate the effect 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 2023b) and later by a credit bureau agreement to stop 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.

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 $1 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 $1 relieved.

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 subexperiment, 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 effect 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 phone interviews conducted by a trained U.S. call center, resulting in a survey sample of 2,888 individuals.

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

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 subexperiment, 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 .021), with a 13.8-point increase (p-value of .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 .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 .097), our primary survey outcome (as measured by the eight-question Patient Health Questionnaire, 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 seven-question Generalized Anxiety Disorder screen, GAD-7), stress, general health, and subjective well-being. We do not detect any meaningful effects on healthcare utilization or financial wellness.

Our article contributes to the literature on the financial burden of the U.S. 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, this study also contributes to research on the effects of nonmedical 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 2019) 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 that 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 effect in the current (nonreporting) 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 effects 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 article 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 a healthcare provider. Alternative definitions of medical debt may be appropriate in different contexts. For example, Lopes et al. (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 help patients who are unable to pay their out-of-pocket bills, but in practice provide limited protection against medical indebtedness. Nonprofit 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 Internal Revenue Service rarely penalizes hospitals for noncompliance 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

4. Internal Revenue Service (IRS) regulations codified in Section 501(r) require nonprofit 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 prequalify 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).

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 practices to induce payment, such as threatening arrest or calling more than seven times a 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, and 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 stop 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 2023b). In a series of changes phased in between July 2022 and April 2023, the credit bureaus voluntarily agreed to exclude

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 nonemergency medical care (Levey 2022).

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 nonpecuniary considerations, such as avoiding the stigma of wage garnishment.

medical debt from credit reports if the debt is less than \$500, less than one year old, or has already been paid (CFPB 2023a).

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 stopped including 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.

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; Novak, Ali, and Sanmartin 2020; Himmelstein et al. 2022; Han et al. 2024). In Online Appendix B, we conducted a comprehensive analysis of the cross-sectional correlation between medical debt and financial and health outcomes in nationally representative data sets, 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 people 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 Online Appendix Tables A3–A6 and Online Appendix Figures A1–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 nonfinancial 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 $1 in the hospital debt experiment, less than 1 cent per $1 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 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 Federal Deposit Insurance Corporation.[9]

Third, medical debt has historically affected finances through its presence on credit reports. For example, Brevoort, Grodzicki, and Hackmann (2020) document a sharp drop in credit scores after 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,

8. In a national survey of 2,663 U.S. 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.

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.

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 nonfinancial 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; U.S. 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, 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.

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.

1. *Expert Survey.* We conducted an expert survey of academics, nonprofit staff, hospital revenue cycle management and

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's 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).

debt collection practitioners, and policymakers to assess prevailing beliefs on the impact of our hospital debt experiment. The survey was administered between April 19 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 effect 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 Online Appendix A.5 for details and Online Appendix 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. Online 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 nonprofit staff) and 51.1% think it is very valuable or extremely valuable (31.2% of academics and 69.6% of nonprofit staff), as shown in Online Appendix Figure A6.

III. Experiment

We study medical debt relief provided by RIP Medical Debt, a 501(c)(3) nonprofit 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

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.

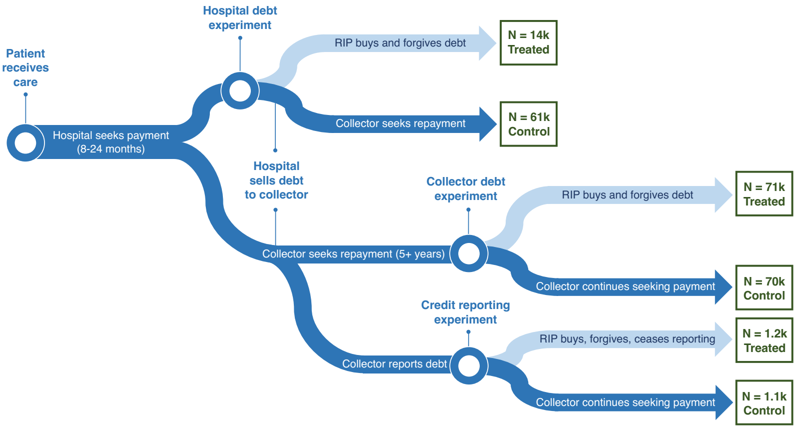

FIGURE I

Experiment Design

This flow chart illustrates the two primary debt relief experiments and the credit reporting subexperiment. 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 subexperiment 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.

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* subexperiment 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 eight states in the South and Mountain West.[12] After a patient receives

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

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 strata, persons were randomly assigned to treatment or control. The process by which portfolios were made available for randomization did not permit carrying forward treatment assignment across waves. In a typical wave, 20% of persons were assigned to treatment, although the exact treated percentage varied depending on the size of the wave and the amount of donor funding available. See Online Appendix A.1 for more detail about the stratified randomization and Online Appendix Table A7 for wave-by-wave statistics.

For treated individuals, RIP purchased the debt at a price of 5.5 cents per $1 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 Online 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 4 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; in practice, this makes up 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 effects. The average person in the hospital debt experiment appears in 0.23 subsequent waves, and 16% appear in at least 1 additional wave. However, because roughly 20% of people are assigned to treatment in each wave, those who are treated in the initial wave are on average treated 1.05 times overall, and those 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.

Table I, column (1) provides summary statistics on the hospital debt sample in the initial wave in which persons appear (data are described in more detail below). The total sample consists of 75,873 people 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

13. During the COVID-19 pandemic, collections rates increased, consistent with overall declines in regular spending (Chetty et al. 2024) 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.

TABLE I
SUMMARY STATISTICS

	Hospital debt experiment			Collector debt experiment	Nationally representative sample	
	All (1)	Outreach (2)	Respondents (3)	All (4)	All (5)	> \$0 Medical debt in collections (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	—	—

TABLE I

CONTINUED

	Hospital debt experiment			Collector debt experiment	Nationally representative sample	
	All (1)	Outreach (2)	Respondents (3)	All (4)	All (5)	> $0 Medical debt in collections (6)
Panel B: Baseline characteristics						
Demographics						
Age (years)	43.1	41.3	41.7	46.0	—	—
Male (%)	45.2	46.7	38.8	43.3	—	—
Race and ethnicity						
Black (%)	—	—	18.8	—	—	—
Non-Hispanic white (%)	—	—	43.7	—	—	—
Hispanic (any race) (%)	—	—	30.9	—	—	—
Credit bureau data						
Credit score (never missing)	575.3	569.2	576.2	572.1	693.8	579.1
Medical debt in collections (%)	58.8	60.5	57.6	65.1	17.6	100.0
Medical debt in collections ($)	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. The table reports summary statistics for the hospital debt and collector debt experiments with a 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.

$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).

1. *Awareness Subexperiment.* The effect 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 6–14 of the hospital debt experiment. Of the 8,160 treated individuals in these waves, they randomly selected 4,232 (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 subexperiment, see Online Appendix 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 with 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 Online Appendix A.1 for more information and Online 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 1 cent per $1, or roughly one-sixth the price of the hospital debt. Treated persons had their debt forgiven and were notified by letter (Online 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.

Table I, column (4) provides summary statistics on the collector debt sample in the initial wave in which persons appear, and Online Appendix Table A7 provides wave-by-wave detail. Debt relief was provided to 69,024 treated persons, amounting to 50.4% of 137,038 people 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 Subexperiment

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 stopped 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 trade line–level data in the four quarters prior to the intervention (see Online Appendix 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 Online Appendix Figure A8, Panel A).[15]

IV. Data

IV.A. Collections Account Data

The debt collector provided us with a data set 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 people 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 people 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

14. As noted, 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 Online Appendix Figure A8, Panel B). Therefore, we focus on wave 1 here but show results for wave 2 in the Online 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.

nationwide credit reporting agencies. The linking was conducted by TransUnion and returned as a fully depersonalized data set with no means to link back to the original sample. We purchased 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 them 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: financial distress, debt in collections, bankruptcy, access to credit, and unsecured and secured borrowing. Online Appendix A.4 provides more detail on the construction of these variables.

IV.C. Survey Data

We contracted NORC at the University of Chicago 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 Online Appendix A.3. The full survey instrument is provided in Online Appendix G.

The surveys were sent to a subset of the hospital debt sample who entered the study after September 2019 (waves 6–18) 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

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% versus 9.5%) and collector debt sample (4.3% versus 4.4%).

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 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 up-front 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 to 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 Online Appendix Figures A9 and A10) was sent in a colored 6" $\times$ 9" envelope and included a $2 up-front payment to attract attention. Throughout the protocol, individuals received twice-weekly email reminders (cycling through available email addresses) and reminder postcards every other week via postal mail. In the fourth week, individuals received a follow-up mailer via postal mail. In the fifth week, individuals were mailed the full survey instrument along with a prepaid return envelope and a $5 up-front payment via FedEx-certified mail. Between the sixth and twelfth weeks, trained U.S.-based call center workers contacted individuals by telephone and gave them 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 10–17 months)—and the commencement of control-group debt collection activities—and 29 months after receiving the care that incurred the debt (interquartile range of 24–34 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 be 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

Table I, columns (1)–(4) present summary statistics for the hospital and collector debt samples, the survey outreach subsample, and survey respondents. Columns (5) and (6) present

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 seven surveys sponsored by the Department of Health and Human Services (Czajka and Beyler 2016).

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. Online Appendix Table A8 compares the demographics of our survey respondents to the national population. Our respondents are more likely to be female, nonwhite, 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 (Table I, column (1)) 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 with 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, and auto loan balances, as well as other trade lines), primarily because they are less likely to have a mortgage.

As mentioned, survey outreach was restricted to people in the hospital debt sample that owed more than $500 in medical debt to the collection agency (and who were first observed in waves 6–18). 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 OLS regressions of the form:

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

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 posttreatment 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 also 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 pretreatment 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 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 data sets, we estimate alternative specifications where we control for demographics and baseline financial characteristics from the collections account and credit bureau data (and

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.

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

exclude individual fixed effects). These specifications are outlined in Online Appendix C.1.

For our analysis of secondary outcomes, we adjust the p-values to account for multiple testing in each prespecified 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 Online Appendix C.2.

To analyze the awareness subexperiment, 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 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

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.

control-group credit reports, respectively. As before, γ_i are person fixed effects, and $\alpha_{r(i),t}$ are fixed effects at the level of the 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 end 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 Online Appendix Tables A9–A32.

The results confirm random assignment in the hospital debt, survey outreach, and collector debt samples. All p-values are greater than .05, and the F-tests fail to reject the null that the differences are jointly zero.

The survey response sample (columns (5) and (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 .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). Although this difference is not statistically significant at conventional levels (p-value of .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.

TABLE II

BALANCE ON BASELINE DEMOGRAPHICS AND COLLECTIONS ACCOUNT CHARACTERISTICS

	Hospital debt experiment						Collector debt experiment	
	All		Survey outreach		Respondents			
	Control mean (1)	Difference (2)	Control mean (3)	Difference (4)	Control mean (5)	Difference (6)	Control mean (7)	Difference (8)
Panel A: Demographics								
Age (years)	43.1	−0.01	41.3	−0.1	41.8	−0.3	46.0	−0.1
		[.936]		[.796]		[.574]		[.189]
Male (%)	45.1	0.2	46.5	0.1	39.8	−3.3	43.4	−0.2
		[.685]		[.884]		[.078]		[.825]
Panel B: Race and ethnicity								
Black (%)	—	—	—	—	18.2	1.5	—	—
						[.326]		
Non-Hispanic white (%)	—	—	—	—	44.0	−1.3	—	—
						[.494]		
Hispanic (any race) (%)	—	—	—	—	31.4	−0.4	—	—
						[.823]		
Panel C: Collector data								
Medical debt ($)	1,359	2	2,280	−61	2,178	−176	2,166	9
		[.916]		[.236]		[.085]		[.694]
Medical debt age (quarters)	5.2	−0.01	5.1	0.003	5.2	−0.01	28.2	−0.03
		[.298]		[.919]		[.887]		[.330]
Insured (%)	60.9	−0.4	48.2	−0.2	57.6	−1.7	—	—
		[.322]		[.847]		[.378]		

TABLE II
CONTINUED

	Hospital debt experiment						Collector debt experiment	
	All		Survey outreach		Respondents			
	Control mean (1)	Difference (2)	Control mean (3)	Difference (4)	Control mean (5)	Difference (6)	Control mean (7)	Difference (8)
Panel D: Other								
Response rate (%)	—	—	—	—	18.7	1.3	—	—
						[.056]		
Panel E: Sample Size								
Observations†	61,496	14,377	9,611	5,311	1,802	1,086	68,014	69,024
F-statistic (p-value)††		[.902]		[.498]		[.395]		[.593]

Notes. The 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.

† The sample size for control and treatment groups is reported in odd- and even-numbered columns, respectively.

†† The F-statistic p-value is 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 (1)	Difference (2)	Control mean (3)	Difference (4)	Control mean (5)	Difference (6)	Control mean (7)	Difference (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
		[.964]		[.501]		[.403]		[.066]
Number of accounts in default	1.2	0.002	1.2	−0.02	1.4	−0.04	0.9	0.01
		[.933]		[.541]		[.585]		[.148]
Debt past due ($)	5,623.0	−88.2	5,594.1	55.7	5,967.0	−558.9	5,142.2	87.6
		[.538]		[.824]		[.307]		[.278]
Balances in default ($)	4,112.2	−124.2	4,281.6	37.6	4,311.5	−274.3	3,924.8	31.1
		[.208]		[.836]		[.469]		[.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
		[.857]		[.716]		[.809]		[.185]
Debts in collections ($)	3,478.4	−18.9	3,959.6	−143.7	3,529.6	−66.6	3,898.3	49.4
		[.757]		[.216]		[.778]		[.192]
Panel C: Bankruptcy								
Bankruptcy in past 12 months (%)	0.7	0.1	0.6	0.2	0.9	0.5	0.3	−0.003
		[.488]		[.277]		[.232]		[.933]

TABLE III
CONTINUED

	Hospital debt experiment						Collector debt experiment	
	All		Survey outreach		Respondents			
	Control mean (1)	Difference (2)	Control mean (3)	Difference (4)	Control mean (5)	Difference (6)	Control mean (7)	Difference (8)
Panel D: Access to credit								
Has credit score (%)	96.4	−0.03	96.4	0.2	97.8	0.2	93.8	0.2
		[.853]		[.565]		[.789]		[.181]
Credit score (never missing)	575.3	0.2	568.9	0.8	575.4	1.6	572.3	−0.4
		[.822]		[.560]		[.628]		[.334]
Credit card limit ($)	2,511.8	28.0	2,101.0	201.4	3,019.4	381.1	2,175.1	67.6
		[.702]		[.105]		[.260]		[.070]
Panel E: Borrowing								
Number of credit cards	0.7	−0.02	0.6	0.02	0.9	0.1	0.7	0.01
		[.232]		[.390]		[.225]		[.199]
Credit card balance ($)	1,526.4	14.8	1,320.8	97.7	1,857.4	17.1	1,124.6	11.1
		[.698]		[.138]		[.917]		[.510]
Number of auto loans	0.4	−0.001	0.4	0.01	0.5	−0.02	0.3	0.005
		[.922]		[.515]		[.431]		[.117]
Auto loan balance ($)	7,903.5	−34.9	7,502.8	327.5	9,109.4	−390.8	5,064.6	71.9
		[.797]		[.184]		[.498]		[.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)††		[.699]		[.635]		[.899]		[.312]

Notes. The table presents the balance of baseline credit bureau characteristics in 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.

† The sample size for control and treatment groups is reported in odd- and even-numbered columns, respectively.

†† The *F*-statistic *p*-value is reported for the joint null hypothesis that all of the differences for a given sample are zero.

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 prespecified outcomes from the table; these are shown in Online Appendix Table A33.

Columns (1)–(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 prespecified 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 people with no medical debt have 0.5 fewer accounts past due than those with medical debt (Online Appendix Table A6). We can reject 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)–(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 with 1.3 years old for the hospital debt sample. Consistent with the hospital debt sample, we find null effects for this sample.

TABLE IV

EFFECTS OF DEBT RELIEF ON CREDIT BUREAU OUTCOMES

	Hospital debt experiment			Collector debt experiment		
	Control mean (1)	Treatment effect (2)	p-value (3)	Control mean (4)	Treatment effect (5)	p-value (6)
Panel A: Distress						
Number of accounts past due^{+}	1.20	−0.01	[.374]	1.02	−0.002	[.838]
		(0.02)	—		(0.01)	—
Number of accounts in default	1.08	−0.02	[.290]	0.92	−0.001	[.946]
		(0.01)	{.553}		(0.01)	{.995}
Debt past due ($)	4,908	4	[.973]	4,815	6	[.930]
		(117)	{.976}		(68)	{.995}
Balances in default ($)	3,741	27	[.716]	3,705	28	[.570]
		(75)	{.901}		(50)	{.879}
Panel B: Debt in collections						
Number of debts in collections	4.66	−0.02	[.688]	3.55	−0.02	[.367]
		(0.04)	{.674}		(0.02)	{.557}
Debts in collections ($)	4,119	−32	[.488]	3,112	−1	[.963]
		(47)	{.671}		(28)	{.962}
Panel C: Bankruptcy						
Bankruptcy in past 12 months (%)	1.30	−0.12	[.361]	0.65	−0.05	[.338]
		(0.13)	—		(0.05)	—

TABLE IV
CONTINUED

	Hospital debt experiment			Collector debt experiment		
	Control mean (1)	Treatment effect (2)	p-value (3)	Control mean (4)	Treatment effect (5)	p-value (6)
Panel D: Access to credit						
Has credit score (%)	97.22	0.004	[.981]	90.73	−0.06	[.640]
		(0.17)	{.997}		(0.13)	{.867}
Credit score (never missing)	582.29	0.04	[.930]	577.60	−0.03	[.908]
		(0.51)	{.997}		(0.29)	{.903}
Credit card limit ($)	2,654	40	[.263]	2,640	24	[.231]
		(36)	{.585}		(20)	{.532}
Panel E: Borrowing						
Number of credit cards	0.81	0.02	[.025]	0.78	0.003	[.551]
		(0.01)	{.088}		(0.01)	{.812}
Credit card balance ($)	1,481	2	[.914]	1,306	24	[.042]
		(23)	{.930}		(12)	{.135}
Number of auto loans	0.39	0.01	[.203]	0.30	−0.0001	[.975]
		(0.005)	{.479}		(0.002)	{.980}
Auto loan balance ($)	8,020	−43	[.658]	5,417	−37	[.367]
		(98)	{.899}		(41)	{.733}
Panel F: Sample size						
Observations†	55,653	12,998		64,947	65,968	

Notes. The 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 posttreatment for each experiment, and columns (2) and (5) report treatment effects measured in the fourth quarter posttreatment. 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 prespecified outcome. Indicates the number of accounts $\geq$ 30 days past due.

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

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 Online 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 Online Appendix Tables A36–A43). Online Appendix C.1 and C.2 provide a comprehensive discussion of the sensitivity and heterogeneity analyses.

VI.B. Credit Bureau Outcomes: Credit Reporting Subexperiment

Table V shows the effect of debt relief for our credit reporting subexperiment, 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)).

Table V, Panel A 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 $< .001$) and the dollar amount of medical debt in collections by \$1,215 (p-value $< .001$; 29% of the 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 $< .001$) 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 .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" people who would otherwise have too little information for their scoring algorithms and modestly raises credit scores for those who would always be scored.

TABLE V
EFFECTS OF DEBT RELIEF IN THE CREDIT REPORTING SUBSAMPLE

	Control reporting			Post control reporting	
	Control mean (1)	Treatment effect (2)	*p*-value (3)	Treatment effect (4)	*p*-value (5)
Panel A: Full sample					
Number of medical debts in collections	4.72	−1.00	.000	−0.22	.175
		(0.10)		(0.16)	
Medical debt in collections ($)	4,147	−1,215	.000	−116	.576
		(145)		(208)	
Has credit score (%)	98.08	−4.18	.000	0.49	.634
		(0.65)		(1.03)	
Credit score (never missing)	570.64	3.39	.021	0.24	.902
		(1.47)		(1.97)	
Credit card limit ($)	1,953	155	.038	340	.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	.000	−0.04	.710
		(0.09)		(0.10)	
Medical debt in collections ($)	1,027	−1,007	.000	−359	.211
		(284)		(287)	
Has credit score (%)	93.07	−15.15	.000	3.21	.415
		(3.10)		(3.94)	
Credit score (never missing)	609.48	13.82	.008	9.06	.162
		(5.19)		(6.45)	
Credit card limit ($)	3,726	312	.287	922	.070
		(293)		(507)	
Observations†	231	234			

TABLE V

CONTINUED

	Control reporting			Post control reporting	
	Control mean (1)	Treatment effect (2)	p-value (3)	Treatment effect (4)	p-value (5)
Panel C: Other debt in collections					
Number of medical debts in collections	5.57	−1.04	.000	−0.21	.277
		(0.12)		(0.19)	
Medical debt in collections ($)	4,901	−1,239	.000	−22	.931
		(174)		(253)	
Has credit score (%)	99.35	−1.79	.000	0.02	.986
		(0.44)		(0.91)	
Credit score (never missing)	563.99	1.20	.440	−1.27	.549
		(1.56)		(2.11)	
Credit card limit ($)	1,514	116	.099	177	.123
		(70)		(115)	
Observations†	1,077	1,160			

Notes. The 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 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. 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 are reported in columns (1) and (2), respectively.

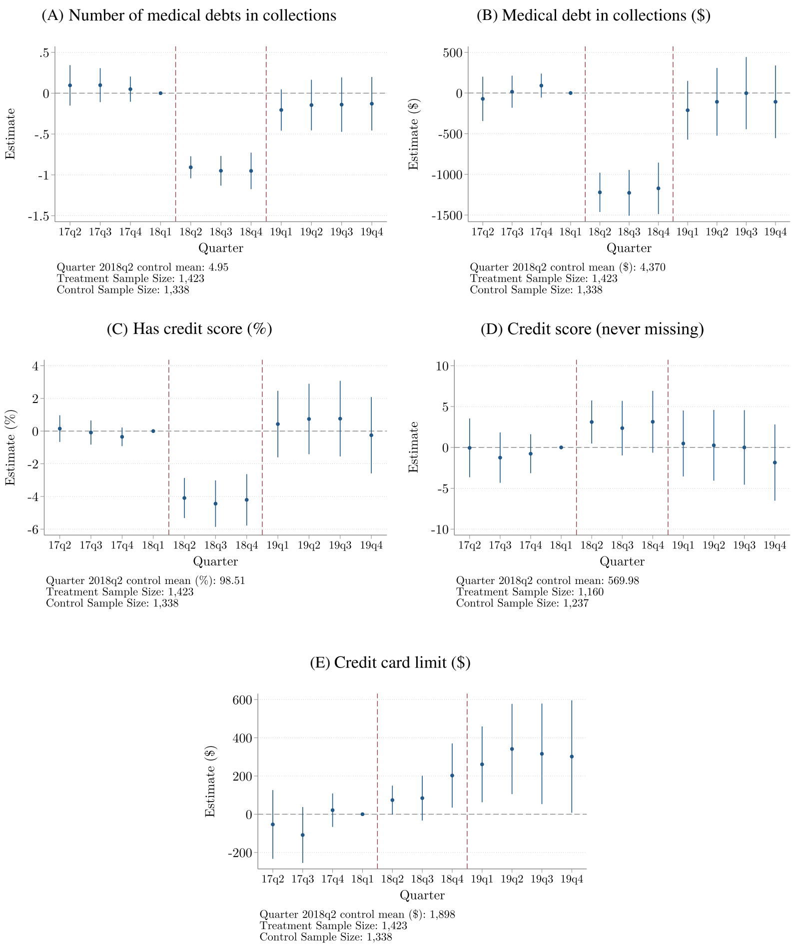

FIGURE II

Effects of Debt Relief in the Credit Reporting Subsample

The figure reports an event study of the effect of medical debt relief on credit bureau outcomes for the credit reporting subexperiment, 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 (color version available online) 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 on-impact increase in credit scores is accompanied by a gradual increase in credit limits, illustrated in Figure II, Panel E. During the three quarters with control-group reporting, credit limits increase by \$155 (p-value of .038; 8% of the control mean of \$1,953). This increase grows to \$340 (p-value of .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. Table V, Panels B and C 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 .008) for those with no other debt in collections versus 1.2 points (p-value of .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 .070). For persons with other debt in collections, this effect is a smaller \$177 increase (10% of the control mean, p-value of .123). The event-study plots shown in Online Appendix Figures A11 and A12 illustrate this heterogeneity.

In Online Appendix C.3, and corresponding Online Appendix Tables A44–A46, we examine the effect 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

TABLE VI

EFFECTS OF DEBT RELIEF ON FUTURE MEDICAL DEBT IN THE HOSPITAL DEBT EXPERIMENT

	Control mean (1)	Treatment effect (2)	*p*-value (3)
Panel A: Full sample			
Amount of debt ($)	207.55	15.02	.038
		(7.23)	
At least some debt (%)	16.21	1.07	.003
		(0.36)	
Panel B: Pre-relief medical services			
Amount of debt ($)	189.17	13.65	.046
		(6.85)	
At least some debt (%)	15.27	1.03	.003
		(0.35)	
Panel C: Post-relief medical services			
Amount of debt ($)	20.37	2.22	.342
		(2.34)	
At least some debt (%)	1.82	0.09	.505
		(0.13)	
Panel D: Sample size			
Observations†	58,875	13,740	

Notes. The table presents the effects of medical debt relief on "future medical debt" as measured by (i) the probability of having medical bills sent to collections after initial treatment assignment and (ii) 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 in parentheses, and column (3) contains *p*-values. Panel A presents effects for any future medical debt; Panel B presents effects for future medical debt with a service date before 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 are reported in the Control mean and Treatment effect columns, respectively.

(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 2023a).

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). Because 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.

Online 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 Online Appendix 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 Online Appendix Tables A49–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 Online 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 prespecified 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 Online Appendix 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).

Table VII, Panel A 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 .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 Table VII, Panel A 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 .392) for anxiety and 2.7 percentage points (adjusted p-value of .158) for stress. Table VII, Panels B and C show statistically insignificant reductions of 2.7 percentage points (p-value of .161) for subjective well-being (at least "pretty happy") and 2.6 percentage points (p-value of .188) for general health (at least "good health").

We do not detect meaningful effects on healthcare utilization (Table VII, Panel D). Debt relief causes a statistically

TABLE VII
EFFECTS OF DEBT RELIEF ON SURVEY OUTCOMES

	Control mean (1)	Treatment effect (2)	p-value (3)
Panel A: Mental health			
At least moderate depression (%)+	44.95	3.23	[.097]
		(1.94)	—
At least moderate anxiety (%)	40.07	1.63	[.395]
		(1.92)	{.392}
At least sometimes stressed (%)	76.53	2.72	[.093]
		(1.62)	{.158}
Panel B: Subjective well-being			
At least pretty happy (%)	54.33	−2.72	[.161]
		(1.94)	—
Panel C: General health			
At least good health (%)	53.83	−2.56	[.188]
		(1.94)	—
Panel D: Healthcare utilization			
Had all needed healthcare (%)	56.66	−2.37	[.220]
		(1.93)	{.310}
Had all needed RX (%)	71.92	−2.42	[.170]
		(1.77)	{.310}
Panel E: Financial distress			
Had trouble paying other bills (%)	60.82	3.53	[.061]
		(1.88)	{.150}
Cut back spending (z-score)	0.00	−0.0003	[.993]
		(0.04)	{.994}
Increased borrowing (z-score)	0.00	0.03	[.381]
		(0.04)	{.558}
Panel F: Sample size			
Observations†	1,802	1,086	

Notes. The table presents the effects of medical debt relief on self-reported health and financial-distress outcomes in 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 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) in the Online Appendix.

+ Primary prespecified outcome.

† The sample sizes for control and treatment groups are reported in the Control mean and Treatment effect columns, respectively.

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 effects 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 .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 Online Appendix 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. Online Appendix Table A53 shows that neither exercise has a noticeable effect on our estimates, giving us confidence in the internal validity of our findings.

To examine the external validity of our results to survey nonrespondents, 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. Although these exercises are inherently limited in their ability to reveal differences for nonrespondents, Online Appendix Table A54 indicates that neither exercise provides any evidence to suggest that our main findings are not externally valid. As another test, Online 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 prespecified 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 Online Appendix Tables A57–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 .002) relative to a control mean of 45.9%, and similar patterns for anxiety, stress, subjective wellbeing, and general health.

1. *Awareness.* The effect 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. Online Appendix Table A61 shows that treated individuals are 16.1 percentage points more likely to report debt forgiveness (p-value $<$ .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). Online Appendix Table A61 indicates that persons assigned to follow-up calls were 18.0 percentage points (p-value $<$ .001) more likely than control persons to report receiving debt forgiveness. Online 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 people 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 subexperiment 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 subexperiment also point to the (partial equilibrium) effects of the credit bureaus' decision to cease reporting many types of medical debt on credit reports (CFPB 2023a).

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.

22. We note that the notice letter (Online Appendix Figure A7) explicitly states "the forgiveness is for this outstanding bill only" and "we 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.

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 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 prespecified heterogeneity analysis, so this result may be a statistical fluke. Even so, 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 that the recipients of debt relief in our experiment did not request assistance. While this result warrants follow-up

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. Online 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.

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 $100 million 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 arrive at three key sets of results. First, debt relief has no average effect on financial outcomes but modestly increases credit access for people 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 effect 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 at odds with the views of experts and with 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 affected their mental health, and 42% reported that 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. Although 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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Supplementary Material

An Online Appendix for this article can be found at the *The Quarterly Journal of Economics* online.

Data Availability

The data underlying this article are available in the Harvard Dataverse, https://doi.org/10.7910/DVN/WY6QQO (Kluender et al. 2024).

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