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The long-term impact of debt relief for indigent defendants in a misdemeanor court

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US courts regularly assess fines, fees, and costs against criminal defendants. Court-related debt can cause continuing court involvement and incarceration, not because of new crimes, but because of unpaid financial obligations. We conducted an experiment with 606 people found guilty of misdemeanors in Oklahoma County, Oklahoma. Study participants were randomly selected to receive relief from all current and prior fines and fees assessed for criminal charges in the county. Fee relief reduced jail bookings 21 mo after randomization and the effect persisted over 44 mo of follow-up. Although fee relief reduced incarceration, financial sanctions had no effect on indicators of lawbreaking. Instead, the control group (who obtained no relief from fines and fees) were rearrested at significantly higher rates because of open arrest warrants for nonpayment. These results indicate the long-term and criminalizing effects of legal debt, supporting claims that financial sanctions disproportionately harm low-income defendants while contributing little to public safety.

fines and fees | jail | poverty | randomized experiment

Courts in the United States regularly assess a variety of fines and fees against criminal defendants. Fines and fees proliferated in the 2000s as lawmakers across the country sought new revenue sources to fund criminal justice activities (1, 2). Whereas fines are financial punishments imposed by a judge upon conviction for a criminal offense, fees are assessed to cover costs of prosecution, indigent defense, drug testing, community supervision, victim compensation funds, myriad other court actions, and state and local criminal justice funds. The economic burden of fines and fees regularly total hundreds and even thousands of dollars. For example, court payments due at sentencing for driving without a license were estimated to vary from \$60 and \$3,500 across nine states in a 2016 study (3). Costs associated with a misdemeanor case, the focus of the current study, were found to be at least \$175 in New York compared to an average of \$1,000 in Florida in 2020 (4, 5).

Unlike incarceration, monetary sanctions have been defended as socially efficient, involving a direct transfer from individuals to the state with no additional costs to taxpayers (6). Critics counter that fines and fees disproportionately harm the poor. Significant economic disadvantage, and racial disparity, is well documented in incarcerated populations (7), and among misdemeanor defendants who make up the largest group in criminal courts subject to fines and fees (8). Low-income defendants contending with fines and fees may be unable to meet their legal financial obligations, resulting in further criminal justice involvement (9–11). Instead of being socially efficient, fines and fees may thus be socially costly by increasing court involvement and incarceration among those who are too poor to pay.

The criminalizing effect of fines and fees—indicated by further court involvement and incarceration—encompasses several distinct processes. Court-ordered payments may cause additional legal jeopardy when, in pursuit of debt, judges assess financial penalties, order court appearances, issue arrest warrants for nonpayment, incarcerate debtors, and extend and revoke community supervision (12). In these cases, the burdens of criminal court involvement are increased, not as the result of new criminal activity, but because of the court's debt collection efforts.

Besides court actions, fines and fees may also cause economic hardship that itself contributes to crime and criminal legal involvement. If paying fines and fees reduces economic well-being, defendants may turn to crime to make up for lost income (1, 13). The civil mechanisms courts use to enforce payment—such as assessing penalties for nonpayment, garnishing state income tax returns, and referring debt to a private collections agency—may also undermine economic security and similarly contribute to lawbreaking and criminalization (14). In contrast to their criminalizing effect, fines and fees are sometimes defended for enforcing accountability for wrongdoing. Fines and fees may also deter crime by increasing the cost of offending (6, 15).

Significance

Fines and fees, assessed on criminal defendants for a wide variety of legal operations, are common in US courts. A randomized experiment in a misdemeanor court shows relief from fines and fees reduced incarceration, arrest warrants, and debt collection activity over 44 mo of follow-up. Instead of ensuring accountability or deterring crime, fines and fees prolonged court involvement for those who are often too poor to pay.

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Evidence that fines and fees affect lawbreaking and criminalization has been mixed, however. Randomized experiments over 1- and 2-y follow-up periods found no evidence of a criminalizing or deterrent effect of restitution payments or misdemeanor court fees (12, 16). Several regression discontinuity studies examined the effects of policy change. A large-scale study of nine distinct policy changes in US states for felonies, misdemeanors, and traffic offenses found no effect of fines and fees on recidivism (17). Another study of speeding tickets in the Czech Republic found that tickets reduced future offending but that the deterrent effect of ticketing did not increase with higher fines (18). Closer to the current experiment, higher misdemeanor legal fees have been found to increase rates of rearrest in the short-run and the severity of new criminal charges in the long-run (14).

Uneven empirical evidence may reflect three significant challenges for causal inference. First, fines and fees are usually accompanied by other sanctions (such as sentences to probation or incarceration) (3). Isolating the effect of one sanction among several can be difficult where they are highly correlated and likely have similar effects. Second, fines and fees are likely to be levied against those whose with hard-to-observe health and behavioral characteristics which themselves are correlated with incarceration and other criminal-system interactions (19, 20). Unobserved selection of this kind can lead to overestimates of the impact of fines or fees. Third, although randomized experiments can yield strong causal inferences, follow-ups have been relatively short lasting only 1 or 2 y (12, 16). Where the effects of fines and fees depend on a protracted legal process, such as court-ordered warrants for nonpayment, defendants may be at risk of adverse outcomes that do not materialize for several years (11).

We resolve these challenges by estimating the effect of fines and fees on court involvement over a 44-mo follow-up period using data from a randomized experiment conducted in a misdemeanor court in Oklahoma County, Oklahoma. The experiment recruited 606 public defender clients who were found guilty of a misdemeanor. Typical of criminal court defendants nationwide, study respondents reported low levels of schooling, high rates of unemployment, homelessness, and welfare receipt (*SI Appendix*). Respondents were assessed fees for court costs and other charges that averaged over \$1,000, similar to the fees reported for misdemeanors in other jurisdictions. Respondents were randomly assigned to a treatment group ($N = 295$) that was relieved of all fines and fees assessed at the time of judgment or to a control group ($N = 311$) that received no relief from fines and fees. Treatment subjects were also relieved of all outstanding debt owed on past criminal cases in the county. After randomization, we followed respondents using detailed court docket and jail registry data. Results from the 12-mo follow-up showed no decline in the risk of reincarceration with the relief of fees, but extensive efforts at debt collection by the courts for control group respondents (12). With significantly longer follow-up in the current study, we are able to observe long-term effects of the court process.

Results

Fig. 1 reports the average treatment effect (intent-to-treat) for criminal processing outcomes in Oklahoma County by the end of the follow-up period, measured at 44 mo after randomization. After sentencing, the court may issue warrants authorizing arrest for failure to pay, failure to appear at a scheduled court appointment (such as a cost-court or revocation hearing) or failure to comply with court-ordered supervision requirements.

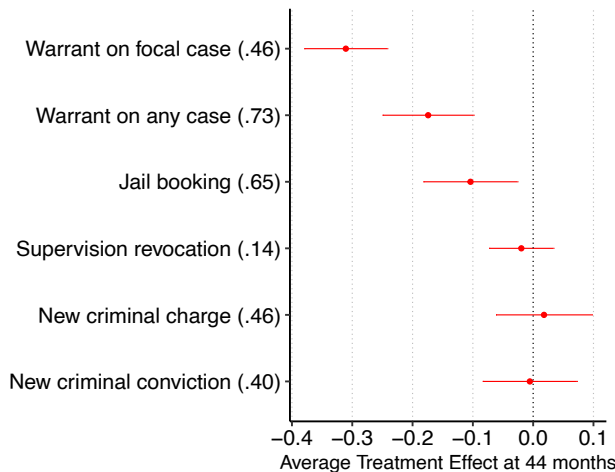


Fig. 1. Estimated treatment effect of debt relief on criminal justice outcomes at 44 mo postrandomization, with 95% CI. Control group mean reported in parentheses.

The treatment group was about 30 percentage points less likely to receive warrants stemming from the focal case (the case that brought participants into the study). Warrants authorize police to arrest the named individual and book them into jail. Indeed, debt relief reduced the likelihood of having any warrant issued in Oklahoma County (for any case) by 18 percentage points.

The reduction in warrants caused by debt relief also resulted in a reduction in incarceration. Debt relief reduced jail bookings from 65 to 55%, a 15% reduction in incarceration. We can place the magnitude of this effect in the context of the length of stay in jail. We do not observe the duration of jail stays in the experiment but other analysis reports that the average duration of detention in Oklahoma County was 21 d (21). Assuming this length of stay for each respondent booked into jail, a 15 percent reduction in incarceration implies that fee relief eliminated roughly 620 d of incarceration among the 295 treatment respondents.

To assess the impact of fee relief on crime, we collected official records on revocations of probation supervision and new criminal charges and convictions. Although official records are only an indirect indicator of criminal behavior, more than 70% of those with new charges and convictions faced felony-level charges. Respondents sentenced to probation who violated their release conditions were typically revoked for a new arrest. The prevalence of felony charges and arrests suggests that further contact with police and the courts often involved serious offenses, rather than minor violations.

Debt relief had no effect on revocations, charges, or convictions, our indicators of new criminal behavior (Fig. 1). Treatment group respondents were revoked from probation at slightly lower rates than the control group but the difference was small and not significant. New criminal charges, filed by the prosecutor, and convictions were also unaffected by legal debt relief nearly 4 y after randomization. In short, we find large treatment effects on warrants and incarceration and no effects on revocations, charges, and convictions. The pattern of findings suggests that court penalties for nonpayment, rather than new criminal behavior, is the mechanism by which legal debt increases court involvement.

The treatment effect on warrants issued for the focal case emerged early in the follow-up period and increased substantially over time. Fig. 2 reports monthly treatment- and control-group means for warrants on the focal case. By 12 mo after randomization, debt relief had reduced issuance of warrants by

14 percentage points. Over the following 2 y, the effect of debt relief had more than doubled to 31 points. The large and growing treatment effect on warrants on the focal case illustrate how fines and fees can increase the risk of arrest years after the original case.

Analysis of the type and timing of each warrant shows that fee relief reduced warrants issued for nonpayment but did not affect compliance with supervision conditions. Differences in warrants for nonpayment account for nearly all of the treatment effect observed in the first year for warrants on the focal case. At months 11 and 12, when most respondents on supervision were nearing the end of their 1-y sentence, there was an increase in warrants issued in relation to motions to revoke supervision (or accelerate a deferral to a conviction) that was of similar magnitude for treatment and control groups. The increase in warrants for revocation among both groups suggests that relief from monthly supervision fees did not affect compliance with probation conditions.

To examine the relationship between debt relief and jail incarceration in greater detail, we compare monthly trends in jail bookings and new criminal charges for the treatment and control groups. Fig. 3 reports monthly treatment and control means for jail bookings and new criminal charges. For the first 20 mo after randomization, there was no statistically significant difference in jail bookings between the treatment and control groups. After 20 mo, however, debt relief reduced the likelihood of going to jail by 8 to 10 percentage points. The trends in jail bookings for the first 20 mo run parallel to trends in new criminal charges, suggesting that most of the jail bookings in the first 2 y were related to lawbreaking rather than unpaid court fees. After 20 mo, however, jail bookings began to rise at a faster rate for control respondents than for treatment respondents. The results suggest that people who were jailed for the first time 20 mo after the focal case faced an increased risk of incarceration because of nonpayment rather than new crimes.

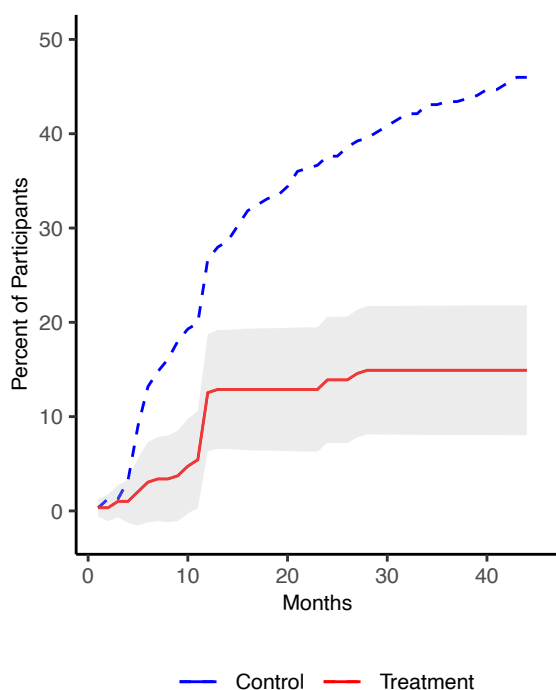


Fig. 2. Monthly treatment and control means for warrants issued on focal misdemeanor case. 95% CI are indicated by the shaded area.

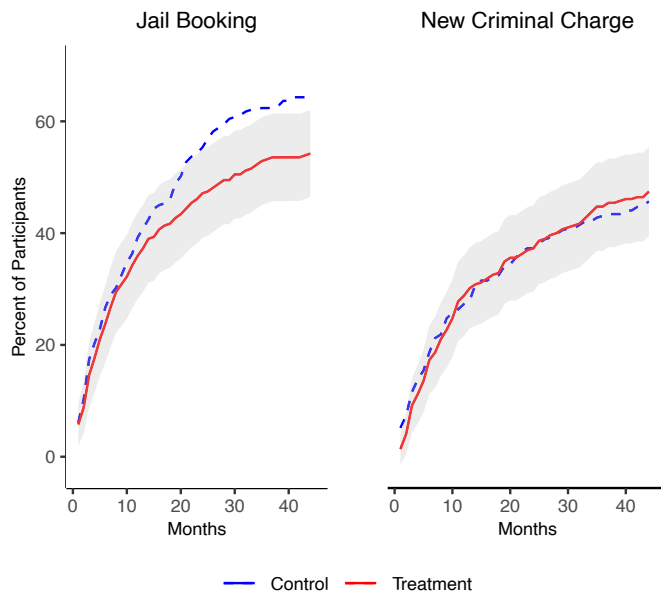


Fig. 3. Monthly treatment and control means for jail booking and new criminal charges. 95% CI are indicated by the shaded area.

Over the 44-mo follow-up, only a minority of respondents made any payments, and those who did still held considerable debt. Table 1 reports control means and treatment effects for payments made on criminal legal debt statewide and on the focal misdemeanor case. After 44 mo, 41% percent of the control group had made at least one payment toward their court debt. On average, control respondents had paid \$272 to courts statewide, \$137 of which was paid to Oklahoma County for debt from the focal misdemeanor case. Control-group respondents owed an average of \$1,141 for the focal misdemeanor case at sentencing, so most still owed substantial debt after 44 mo.

Debt relief also significantly reduced the occurrence of noncriminal penalties related to nonpayment. Fig. 4 reports the estimated treatment effect of debt relief on civil enforcement mechanisms for legal debt. Nearly 4 y after randomization, treatment respondents were 40 percentage points less likely to have a tax return interception mandate issued, 15 percentage points less likely to have new debt added to their outstanding balance, and 26 percentage points less likely to have debt sent to a private collection agency.

Discussion

In a randomized experiment that relieved misdemeanor defendants of all their county court debt, we find that over a 44-mo follow-up that relief from fines and fees reduced incarceration and court involvement. Treatment respondents were 15% less likely to be jailed than controls, despite similar

Table 1. Control group means and treatment effects on payments of fines and fees

	Control mean	Treatment effect	SE
Any payment statewide	0.41	-0.16***	0.04
Amount paid statewide	\$271.74	-\$139.62**	\$47.00
Amount paid on focal case	\$136.58	-\$118.16***	\$22.08

*** = $P < 0.001$; ** = $P < 0.01$, * = $P < 0.05$ on two-tailed test

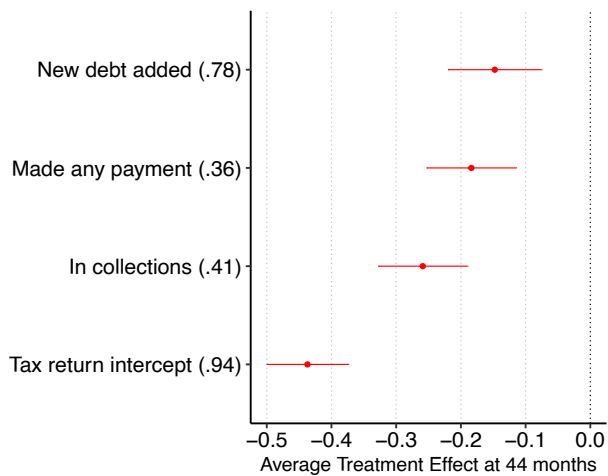


Fig. 4. Estimated treatment effects and 95% CI on financial events at 44 mo postrandomization. Control group mean reported in parentheses.

rates of new criminal charges. The findings suggest that fines and fees can induce incarceration but do not deter crime. The treatment effect on incarceration did not emerge until 21 mo after randomization but persisted for the remaining 2 y of observation. The Oklahoma County misdemeanor court provides payment plans to defendants where arrest warrants for failure-to-pay are delayed for 2 y (12). The timing of new warrants illustrates how the nonpayment of fees can, by itself, increase the risk of arrest and incarceration.

Besides the effect on incarceration, we find that fines and fees increased court involvement without increasing incidents of new criminal charges or convictions. Nearly 4 y after conviction for a misdemeanor, 73% of control subjects had been issued an arrest warrant and 67% had been booked into county jail, while less than half had been charged with a new crime. In addition to protecting against jail incarceration, relief from fines and fees reduced all arrest warrants by 25% and reduced warrants issued on the focal case by 65%. These findings suggest that court actions aimed at collecting unpaid fines and fees criminalize nonpayment and expose otherwise law-abiding people to open warrants, arrest, and jail incarceration.

We also find that fines and fees effectively enhance the typical misdemeanor punishment. Although most respondents were sentenced to a year or less of probation supervision, control respondents continued to face court actions 44 mo later. During this time, control respondents paid off just a small fraction of their total debt. Even for defendants who manage to satisfy all other conditions of their sentence, outstanding court fees leave them subject to financial penalties, debt collection, and new warrants that significantly increase the risk of additional legal jeopardy years after the original misdemeanor case.

These findings are subject to several limitations related to specific characteristics of the research design. The current estimates may be relatively large because the sample was recruited from public defender clients who are economically disadvantaged and have less capacity to pay. Despite this concern, the public defender represented around 86% of all misdemeanor defendants in Oklahoma County and other evidence shows high poverty rates among those who owe fines and fees in other jurisdictions (9, 14). This suggests that the experiences of study subjects in Oklahoma are typical of low-level defendants more generally.

The finding of a zero deterrent effect should also be placed in the context of the design. Treatment participants may have

understood the one-off nature of financial relief under the experiment and thus knew that offending again could result in new fines and fees, undermining a test of specific deterrence. This objection, however, likely overestimates respondents' knowledge of the experiment. Several treatment respondents stated in interviews that they believed the study was part of a rehabilitative program and 20% reported they were unaware of debt relief. The one-time intervention was likely not understood in enough detail to alter their perception of the potential consequences of future offending. Respondents' imperfect understanding of the treatment is consistent with broader evidence of a "knowledge hurdle." Behavioral research indicates that decisions to commit crime typically lack enough information about legal penalties for rational desistance, particularly for those experiencing extreme material hardship and substance use problems (22–24). Sanctions are also considered to have a "general" deterrent effect on those who have not been arrested. In this case, because the intervention was limited in time and not publicized, we would not expect any effect on general deterrence.

Limitations of the current study indicate several directions for future research. The current experiment relieved court debt just in the county jurisdiction and a larger treatment would relieve fees statewide. Recruiting study subjects represented by private attorneys would illuminate how the effects of fines and fees vary with economic status. Our current focus on misdemeanor courts overlooks other important domains including supervision fees for parole and felony probation (25), and municipal court fees (26). The legal consequences for nonpayment also vary across states. Although the procedures for enforcing fines and fees in Oklahoma County have been found in many jurisdictions (27, 28), there has also been substantial reform aiming to curtail warrants and incarceration for nonpayment (29). The effects of fee relief on reduced incarceration that we observed may depend on a relatively draconian legal environment. Estimating the effects of fee relief across jurisdictions is an urgent question for future research.

What are the implications for public policy? The results suggest that eliminating fines and fees for indigent defendants can reduce warrants and incarceration. Reduced criminal justice involvement can be obtained without increasing crime, reducing probation compliance, or greatly affecting public revenue. The effects of fee relief occurred without any new administrative or rehabilitative programs, or any additional tests for the defendant's ability to pay. Evidence shown here suggests that policymakers can shrink jail populations and save court and law enforcement resources otherwise spent on debt collection by reducing, or indeed eliminating, fines and fees for anyone who qualifies for indigent defense.

Design, Data, and Methods

Design. Study respondents were drawn from defendants who were represented by the public defender and who were found guilty of a misdemeanor offense in Oklahoma County, Oklahoma. The research team recruited respondents between September 2017 and January 2019 and randomized respondents to treatment through March 2019. Study recruitment drew from defendants who had paid bond and were waiting for trial in the misdemeanor courtroom and those who had not paid bond and appeared in court from jail. Potential participants were screened for eligibility and completed a brief interview and survey. They were not told of the possibility of debt relief to ensure they did not feel financial pressure to consent to participate in the study.

The 606 study respondents were randomly assigned to either the treatment or control groups only after their case was disposed and sentenced, ensuring that treatment status did not influence case outcomes. The study was powered at 80 percent, allowing us to detect a 10-percentage point reduction in rearrest if the control group's rearrest rate is 30 percent. Informed consent was obtained from all study participants. The study design and data collection were approved by the Harvard University's Committee on the Use of Human Subjects, and the current research was approved by Columbia University's Institutional Review Board.

After recruitment, study subjects were randomly assigned to receive relief of all fines and fees assessed in the case and all previous criminal court debt owed in Oklahoma County. For each study subject in the treatment group, the study paid a negotiated flat rate in return for a waiver of all current and prior criminal legal debt owed to the Oklahoma County District Court and to the district attorney (DA). During the study period, Oklahoma courts assessed defendants a base fee of \$93 for each misdemeanor conviction, in addition to fees for at least 16 different services and revolving funds ranging from a \$5.00 Sheriff's Service Fee on Arrests to a \$100 fee for the Trauma Care Assistance Revolving Fund. Fines, by comparison, were rare and assessed in just 3% of cases.

On average, defendants were assessed \$725 in fees at sentencing, and owed an additional \$1,780 for prior criminal convictions in the county. Study subjects who participated in a deferred adjudication program or were sentenced to probation were also required to pay \$40 per month in probation fees, which summed to an average of \$511 per defendant, and those sentenced to pay prosecution fees of \$40 per month owed an average of \$512 each. The study paid the supervision fees of treatment participants, although they were still required to complete all other conditions of supervision such as treatment program participation and regular reporting.

The study did not relieve treatment participants of restitution, outstanding debt owed to municipal courts or courts in other counties, or of fines and fees stemming from new criminal convictions or additional court actions occurring after the initial treatment. At the time of randomization, approximately 59% of both treatment and control participants held criminal legal debt in other counties and 25% had been sentenced to pay restitution. The presence of external court debt and restitution were balanced across treatment and control groups. At the request of the DA, defendants charged with driving under the influence of intoxicants (DUI) and domestic violence (DV) were excluded from eligibility for the study. DUI and DV cases accounted for 21 and 16 percent of the misdemeanor docket.

We contacted treatment participants to inform them that their debt had been relieved with phone calls, emails, Facebook messages, and by mail. A survey of participants indicated that 80% of the treatment group were aware of their debt relief. If a treatment participant tried to make a payment, the court clerk or probation officer also informed them that their debt had been relieved. In total, 19 cases were not in full compliance with the treatment. Of these, 3 control participants received relief by mistake, and 16 treatment participants had at least one prior criminal charge that was not included in the initial round of debt relief. Upon discovery, legal debt from these prior cases was waived. We thus present estimates of intention-to-treat effects based on random assignment. With just 3 percent of the sample partially or fully noncompliant, intention-to-treat estimates are similar to treatment-on-treated estimates.

Data. Following randomization, we tracked participants over the subsequent 44 mo through docket and case data available online through the Oklahoma State Court Network (OSCN) web site. We scraped OSCN dockets using a combination of name and date of birth to generate a list of case numbers, then used case numbers to scrape detailed case event and debt data. The dockets were scraped in November 2022 and the payment portal, which contains up-to-date information on outstanding debt, was scraped in May 2023 using the list of cases generated in November 2022. This list thus represents all outstanding court debt owed on cases filed before or during November 2022.

Descriptive statistics of baseline and demographic characteristics show no statistically significant differences between treatment and controls on any variables except for employment status and self-reported substance use problems (*SI Appendix*). The treatment group was more likely to be unemployed at baseline (0.63 vs. 0.52), indicating those receiving debt relief may be relatively more disadvantaged than the controls. However, consistent with random assignment, there were no statistically significant differences in other measures of disadvantage, including education, homelessness, and benefit receipt. The treatment group was also less likely than the control group to report a substance use problem (0.55 vs. 0.63) but we find no significant difference between treatments and controls in drug offenses (0.53 vs 0.56) or prior drug charges (0.46 vs. 0.50), suggesting that there may not be behaviorally meaningful differences in substance use.

Methods

Because treatment was randomly assigned, unbiased estimates of treatment effects are given by the difference in outcome means. With data for each respondent i ($i = 1, 2, \dots, 606$), we estimate the treatment effect on outcome Y_i by fitting the regression,

$$Y_i = \beta_0 + \beta_1 T_i + \epsilon_i$$

where T_i is a dummy variable indicating assignment to treatment, and ϵ_i is a random error with mean of zero.

In *SI Appendix*, we include covariates in the regression as a robustness check, and to improve the precision of treatment effect estimates by reducing residual variance (30–32). Because other studies show that the consequences of criminal justice contact can vary across demographic groups (33–35), we also estimated treatment effects by race, employment status, and self-reported substance use problem. We include employment status and substance problems because they were unbalanced across subgroups. Subgroup estimates are reported in *SI Appendix*.

Data, Materials, and Software Availability. Anonymized (original experimental data) data have been deposited in Open Science Framework (<https://doi.org/10.17605/OSF.IO/QB36V>) (36). Some study data are available (Variables recording the level of financial debt cannot be disclosed under the terms of the IRB protocol).

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