

Criminalizing Poverty: The Consequences of Court Fees in a Randomized Experiment

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Abstract

Court-related fines and fees are widely levied on criminal defendants who are frequently poor and have little capacity to pay. Such financial obligations may produce a criminalization of poverty, where later court involvement results not from crime but from an inability to meet the financial burdens of the legal process. We test this hypothesis using a randomized controlled trial of court-related fee relief for misdemeanor defendants in Oklahoma County, Oklahoma. We find that relief from fees does not affect new criminal charges, convictions, or jail bookings after 12 months. However, control respondents were subject to debt collection efforts at significantly higher rates that involved new warrants, additional court debt, tax refund garnishment, and referral to a private debt collector. Despite significant efforts at debt collection among those in the control group, payments to the court totaled less than 5 percent of outstanding debt. The evidence indicates that court debt charged to indigent defendants neither caused nor deterred new crime, and the government obtained little financial benefit. Yet, fines and fees contributed to a criminalization of low-income defendants, placing them at risk of ongoing court involvement through new warrants and debt collection.

Keywords

criminalization, poverty, misdemeanors, fines and fees, randomized experiment

U.S. courts and criminal justice agencies levy a wide array of fines and fees on people charged with criminal offenses. Fines and fees are often imposed for prosecution, incarceration, and other procedures and interventions that accompany criminal processing. Proliferating through an anti-tax movement that aimed to shift criminal justice costs from taxpayers to defendants (Bannon, Nagrecha, and Diller 2010; Harris 2016; Harris, Evans, and Beckett 2010; U.S. Department of Justice 2015), fines and fees have been found to undermine trust and obscure transparency in the criminal justice system (Pattillo and Kirk 2020; Shannon et al. 2020), expand the conditions of parole supervision (Link 2019), diminish

voting rights (Sebastian, Lang, and Short 2020), and add to financial hardship (Harris 2016; Harris et al. 2017). Like incarceration, fines and fees are part of a criminalized socioeconomic inequality in which criminal justice surveillance and control is ubiquitous in the social life of low-income communities, particularly in low-income communities of color.

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Fines and fees are empirically and theoretically significant for understanding the connection between the criminal justice system and inequality. Empirically, fines and fees are regularly (but not only) imposed for minor criminal offenses, greatly extending the reach of the criminal justice system beyond prison incarceration (see Kohler-Hausmann 2018; Turney and Wakefield 2019). Theoretically, fines and fees are part of an institutional dynamic that connects socioeconomic disadvantage to enduring state surveillance and punishment (Friedman and Pattillo 2019; Slavinski and Pettit 2021). Low-income people have little capacity to pay fines and fees, and legal financial obligations and court involvement escalate with nonpayment. Going even further, some researchers argue that fines and fees not only expand state control of the poor, but are a type of “state predation” in which government authorities extract economic resources from low-income communities (Katzenstein and Waller 2015; Page and Soss 2017, 2021). In short, fines and fees may create a criminalization of poverty, where criminal justice control becomes difficult to escape for people who are poor and economically insecure (Edelman 2019; Herring, Yarbrough, and Alatorre 2020).

The central implication of the criminalization of poverty is that fines and fees create long-lasting criminal justice involvement among economically disadvantaged people. Two main mechanisms may cause continuing involvement with courts and the correctional system. First, financial hardship associated with fines and fees may cause stress and anxiety that ultimately lead to crime and rearrest. Second, unpaid fines and fees set in motion an array of court actions that surveil and encumber court-involved people. Both mechanisms, crime and court action, create what sociologists have called “permanent punishment” (Harris 2016) and “layaway freedom” (Pattillo and Kirk 2021), in which court oversight and the risk of incarceration is prolonged, often well beyond the term of the original sentence. Fines and fees may, proportionally, have the largest sentence-extending

effect for low-level, misdemeanor offenses. Convictions in these cases—including drug possession, trespass, petty thefts, simple assaults, and disorderly conduct—may carry incarceration sentences of a few weeks or months, but court involvement may continue for years.

If fines and fees criminalize poverty, what happens to criminal justice involvement when criminal defendants are relieved of their legal debt? We address this question by conducting a randomized experiment in a misdemeanor court in Oklahoma County, Oklahoma. A misdemeanor conviction in Oklahoma generates over a thousand dollars in court-related fees, often adding to outstanding court debt from earlier cases. The current experiment randomly assigns study participants to a treatment group that was relieved of all current and past fines and court costs in the county as well as probation and prosecution fees for the current case. We followed study participants for a year and measured all new criminal charges, court actions, and court payments. Because of randomization, the experiment yields valid inferences about the causal effect of the relief of fees on later criminal justice involvement. If fines and fees cause continuing contact with the criminal justice system, we would expect the relief of financial obligations to reduce police contact, court monitoring, and incarceration. We find the relief of fees only reduces new criminal charges for several months, but debt-related court actions are significantly lower for treatment respondents even 12 months after fee relief.

FINES, FEES, AND THE CRIMINALIZATION OF POVERTY

In the four decades since the early 1980s, police, prisons, and the courts have expanded their influence in poor communities in response to social problems linked to poverty and racial inequality (Garland 2001; Thompson 2010; Wacquant 2009). The growth in imprisonment rates from the early 1970s

to 2008 was perhaps the most vivid indicator, and a large research literature studied the effects of incarceration on poverty and inequality (Travis, Western, and Redburn 2014). Researchers have recently looked beyond imprisonment, examining the much larger reach of the criminal justice system, which includes misdemeanor court processing and jail incarceration (Kohler-Hausmann 2018; Turney and Wakefield 2019).

Fines and fees form part of this expansive criminal justice footprint that extends beyond imprisonment and pervades poor communities. Fines and fees include the punitive charges often imposed for low-level offenses, the user fees charged for court costs, incarceration, and drug tests, and the surcharges and penalties that can accumulate when legal debt goes unpaid. National surveys show fines and fees were used more frequently in the past several decades, and are now widely imposed for felonies and misdemeanors. For example, 24 percent of a national sample of people in state prison were subject to fines and fees in 1991 compared to 66 percent by 2004 (Harris et al. 2010). Harris (2016), in her survey of state felony laws, found fees are imposed in nearly all states. Nonpayment of fees can result in incarceration in 47 out of 50 states. Studies of the magnitude of criminal justice fines and fees show these costs regularly run into the thousands of dollars, even for relatively minor offenses (Harris 2016; Martin et al. 2018).

A key consequence of the proliferation of fines and fees is the widening net of criminalization. The term “criminalization” acknowledges that “crimes” are not self-evident types of human behavior but are instead the active products of state-sponsored efforts at enforcement, surveillance, labeling, and punishment (Jenness 2004). Recent analysis of the “criminalization of poverty” describes how public policies over the past few decades—in the areas of policing (Beckett and Herbert 2010; Stuart 2016), the courts (Harris 2016; Kohler-Hausmann 2018), and incarceration (Wacquant 2009)—have functioned to corral poor people to make them less intrusive in public space and render a kind

of personal accountability enforced through the threat of punishment. Some researchers view criminalization as a style of poverty governance that is part of a larger political economy in which state agencies are “predatory,” extracting “resources from poor communities of color and deliver[ing] them up to municipal coffers” (Page and Soss 2017:141; see also Katzenstein and Waller 2015; Page and Soss 2021).

Court-ordered fines and fees imposed on individuals with little capacity to pay create a direct connection between criminalization and poverty. Existing research suggests two distinct mechanisms behind this connection. First, qualitative studies show fines and fees create financial hardship, stress, and anxiety that may lead to crime, arrest, and incarceration. Shannon and colleagues (2020:275), in an eight-state study that included interviews with people who owed money to the courts, report that “stress was a common refrain among participants, and many participants had to make choices as to what to pay, as they struggled to pay for their homes, buy needed medications, and support their families.” Harris (2016:70), drawing on qualitative interviews in Washington State, similarly observes that the burden of court debt “can lead to a profound sense of despair.” The strain of legal financial obligations also spills over into family relationships, undermining bonds that could otherwise provide financial and emotional support (Harris 2016:64–65). The stress, feelings of despair, depleted family support, and financial hardship induced by fines and fees may contribute to social strain that can lead to crime (Agnew and Brezina 2019).

Legal financial obligations may also deepen economic disadvantage for defendants who are already poor (Harris et al. 2010), and thereby sharpen the economic motivation for crime (Freeman 1999). When overwhelmed by financial distress and feelings of despair, petty thefts, drug sales, robberies, and relapse to substance use may become compelling survival strategies. Under these conditions, fines and fees may lead to new criminal charges and re-incarceration.

Criminogenic effects of fines and fees contrast with a common justification among policymakers that fines and fees enforce accountability and deter future crime (Council of Economic Advisors 2015; Ruback and Bergstrom 2006). Qualitative research has found that study respondents with court debt sometimes felt fines were justly retributive (Harris 2016:57; Pattillo and Kirk 2020). Subjective feelings of the fairness of the penalty, combined with its financial cost, may deter criminal activity. Thus, evidence for deterrence has been reported for fines for driving offenses (Yu 1994), restitution payments (Ruback et al. 2018), and financial sanctions for probationers (Gordon and Glaser 1991). Whether fines and fees cause or deter criminalized behavior is a key empirical question for our analysis.

A second possible mechanism for criminalization involves the court's response to individuals who fail to pay. People who owe legal debt regularly struggle to make payments, and courts initiate new actions for debt collection in response. Poverty, homelessness, untreated mental illness, and substance use disorders may all interfere with regular payment of court debt, and significant arrears in court payments have been found in quantitative and qualitative studies (Harris et al. 2010; Link 2019). When payments are missed and legal debt accrues, courts issue warrants, set up payment plans, and call in private debt collectors (Adamson 2020). All these efforts carry the threat of further entanglement in the system through arrest and incarceration for nonpayment. Pattillo and Kirk (2021) have described the measures taken by courts to monitor defendants and obtain payment as "coercive financialization." The imposition of legal financial obligations is just the starting point of a relationship that is simultaneously financial and controlling, in which courts act to maintain oversight, extract payment, and escalate contact in cases of nonpayment. Coercive financialization prolongs criminal justice oversight, creating a type of shadow control that surpasses the original sentence (Beckett and Murakawa 2012). The effects of

coercive financialization are proportionately large for low-level, misdemeanor offenses, where sentences are relatively brief but court monitoring of legal debt can continue for years.

Whether criminalization results from illegal activity rooted in strain and hardship or from court efforts to extract payment, renewed criminal justice contact is not related to the immediate imposition of fines or fees, but to the burden of carrying legal debt. Prior research suggests two hypotheses regarding the relief of legal debts. First, if legal debt relief could sufficiently alleviate social strain and material hardship, we would expect to see fewer arrests and incarcerations associated with new offenses. However, a deterrent effect would show the opposite pattern: greater recidivism among individuals whose debts were relieved. Second, if legal debt were relieved, coercive financialization would be alleviated, and new warrants, calls from debt collectors, and other efforts at revenue extraction would be curtailed by the courts. The efforts taken by courts to recover legal debt are fundamentally an empirical issue, and such efforts vary across jurisdictions. Estimating the reduction in debt recovery efforts associated with debt relief indicates how energetically courts pursue unpaid fines and fees. The hypothesis also illuminates whether a policy of debt forgiveness would shrink the presence of the criminal justice system in people's lives.

Theories of the criminalization of poverty also raise a third hypothesis regarding the level of payments provided by individuals owing fines and fees to the criminal justice system. Does a regime of fines and fees amount to "state predation" (Page and Soss 2017, 2021) or "economic seizure" (Katzenstein and Waller 2015), in which financial resources are extracted from poor people under the threat of punishment? Quantitative evidence suggests payment rates vary greatly. State and county restitution units in Vermont and Pennsylvania, for example, report collection rates between 24 and 74 percent (Ruback 2015:1797). Qualitative interview respondents report that repayment can be

“unrealistic” for those experiencing severe economic insecurity and facing thousands of dollars in debt (Harris 2016:62). The actual level of debt repayment is important for our understanding of the process of criminalization. A high level of fee payment provides evidence of predation, in which the state extracts economic resources from poor people. A low level of fee payment provides evidence against state predation for debt assessed for criminal convictions.

In summary, prior research suggests three main hypotheses in answer to the question, what happens when criminal legal debt is relieved? First, the relief of debt may reduce financial and emotional strain and thereby reduce the likelihood of new arrests and reincarceration. Second, the relief of debt may prevent a variety of court efforts at debt collection, including issuing new warrants for nonpayment and enlisting private debt collectors. Third, criminal legal debt may result in an economic transfer from a pool of mostly poor people to the state, and the relief of debt prevents such a transfer.

Quantitative studies have tried to estimate the effects of fines by regressing criminal justice outcomes, such as a new arrest or incarceration, on measures of assessed fines and fees (Iratzoqui and Metcalfe 2017). To estimate a causal effect, such studies must assume defendants with higher levels of court debt are no different from defendants with lower levels of debt in their chances of future criminal justice contact, except for their level of debt. For example, Gordon and Glaser (1991) estimated a regression of probation revocation on monetary sanctions, controlling for demographics, criminal history, and offense characteristics. The covariates are assumed to capture all the differences between defendants related to the risk of criminal justice contact, thus isolating the causal effect of monetary sanctions.

A limitation of observational studies like these is that the level of court debt may be related to unobserved variables that also affect criminal justice contact. Variables such as poverty, homelessness, untreated addiction,

and adverse neighborhood environments may contribute to the accumulation of court debt and also raise the risk of criminal justice contact (Harris 2016; Shannon et al. 2020). Quantitative studies thus find associations between court debt and criminal justice contact, but this may result from the confounding effects of unobserved socioeconomic disadvantage. In addition, most quantitative studies only analyze discretionary fines and payments, and are thus uninformative about non-discretionary fees imposed on all defendants (see the studies reviewed in Iratzoqui and Metcalfe 2017:374–75).

FIELD SITE, RESEARCH DESIGN, AND DATA

To estimate a causal effect of debt relief, a strong research design is needed to control for omitted variables related to fines and fees and later criminal justice outcomes. We addressed the problem of causal inference by fielding a preregistered randomized experiment in the misdemeanor court in Oklahoma County.¹ Our experimental results represent one of the first efforts to estimate the causal effect of the relief of fines and fees, including non-discretionary fees for court costs, prosecution, and probation supervision.

Fines and Fees in Oklahoma County

Beginning in the early 2000s, the Oklahoma state legislature began to cut funding for court operations but allowed the imposition of fees to cover the lost revenue. Fines and fees became a growing concern for criminal justice reform advocates and for court actors who faced shrinking state budgets (Gatewood 2018). We chose our field site, Oklahoma County, because of its extensive use of court fees intended to fund basic court functions like prosecution and indigent defense, and because key decision-makers, including the Presiding Judge, the District Attorney (DA), and the Public Defender were willing to host the study and provide letters of support as part of the IRB approval.

A misdemeanor case in Oklahoma County generates a complex web of fines and fees. Most financial obligations incurred by a misdemeanor defendant fall into one of three categories: court costs, supervision fees, and prosecution fees. Court costs begin to accrue when an arrest warrant is issued (at a charge of \$75) and continue through a case's resolution. Fees are earmarked by state law to pay for courthouse security, trauma care, law enforcement training, victim compensation funds, awareness initiatives, education programs, and many other state uses, which are not necessarily related to an individual's case. For example, drug possession convictions with no apparent victim are still assessed a fee for the state's victim compensation fund.

People convicted of misdemeanor offenses are directed to the court clerk's office to pay the costs on the case or set up a payment plan. By setting up a payment plan, the defendant defers payment for one month; defendants usually elect to pay the minimum allowed \$25 per month until all costs are paid. If scheduled payments are missed, judges can issue failure-to-pay warrants and orders to intercept state tax refunds, called "tax intercepts." They may also turn over debt from late payments to a private collection agency that applies its own 30 percent surcharge as a fee for its services. A payment plan sets a court appearance date in two years' time on the "cost docket" or in "cost court." If the defendant has not paid the outstanding balance by the time of their date in cost court, the judge will issue a warrant for failure to pay. As a matter of local policy, the Oklahoma County Sheriff's office directs individuals with failure-to-pay warrants to cost court rather than arresting them.

A misdemeanor conviction in Oklahoma County can result in a sentence to probation that includes charges for supervision fees. During our field period, probation supervision was provided by a private vendor that worked closely with the DA's office. The vendor charged \$40 per month; on average, study respondents who were sentenced to probation owed \$511 to the vendor over the course of their sentence. A

small number of cases were sentenced to be supervised by the DA rather than the private vendor. DA supervision also costs \$40 per month. Debt from probation fees leaves people vulnerable to incarceration. If the supervision fee has not been paid, the DA's office can request accelerated sentencing for defendants on probation whose sentences were deferred, or the DA's office can seek a revocation of probation, which results in incarceration.

Finally, people with misdemeanor convictions must also pay prosecution fees that are intended to help fund the operation of the DA's office. DA offices in Oklahoma can impose a \$40 per month fee on anyone sentenced to private probation supervision, even though the DA's office does not carry out the supervision itself. In practice, this means many individuals who are placed on probation owe \$80 per month—half to the DA's office in the form of prosecution fees and half to the private probation vendor. Study respondents who were charged prosecution fees owed, on average, \$513 to the DA's office over the course of their supervision sentence. As with supervision fees, failure to pay the prosecution fee can result in the DA's office filing a request for acceleration of the date of a deferred sentencing proceeding or the revocation of a suspended sentence. Sentencing or revocation often lead to the extension of supervision periods and more time for the defendant to pay, rather than the enforcement of the original sentence.

Enrollment, Randomization, and Follow-Up

Study respondents were recruited between September 2017 and January 2019 and randomized through March 2019. Researchers reviewed each week's court docket and selected individuals who were charged with a misdemeanor offense and were represented by the Oklahoma County Public Defender's Office. During our field period, the Public Defender represented 86 percent of cases in misdemeanor court. Driving-under-the-influence (DUI) and domestic violence cases

were excluded from the study at the request of the DA. DUI cases accounted for 21 percent of the misdemeanor docket and domestic violence cases for 16 percent. Rates of private representation for DUI (20 percent) and domestic violence (16 percent) were only slightly higher than the overall rate (14 percent), suggesting our sampling frame had a similar economic status as defendants in the misdemeanor court as a whole. About half the respondents were recruited to the study in court when they returned for their court date, having paid bond. The other half of respondents had not paid bond and were recruited in jail while awaiting their court date. Just over 6 percent of potentially eligible participants did not consent to the initial survey and were thus excluded from the study. We did not disclose the possibility of debt relief during enrollment so as to avoid creating undue pressure to participate in the study or influencing case dispositions.

For study recruitment, researchers approached prospective respondents either in the courtroom as they waited for their case to be called, or in jail as they waited to speak to a public defender or to be escorted back to their cells after speaking to a public defender. We administered a short interview with prospective respondents who consented to participate. If individuals were convicted and assessed fines and fees, they were assigned to either the treatment or control groups using a random number generator. In cases involving co-defendants, only one defendant was selected for study participation to minimize spillover effects between respondents. Individuals whose cases ended in acquittal or dismissal were not assessed court fees and were excluded from the study. Study recruitment yielded $N = 606$ respondents, of whom 295 were randomly assigned to treatment and 311 to the control group. With samples of this size, the experiment was powered at the 80 percent level to detect a 10-percentage-point reduction in new criminal charges if the control group recidivism rate was 30 percent.

For participants assigned to the treatment group, we filed court orders that waived all

current and prior Oklahoma County court fines and costs on felony and misdemeanor cases, as well as supervision and prosecution fees on the current case. The court's fee waiver was provided through an agreement we negotiated to pay a flat sum for costs owed to the court for each treatment-group respondent. We also paid a flat sum to the DA's office for each treatment respondent sentenced to pay prosecution or DA supervision fees. Treatment respondents ordered to probation had their fees paid by the study, but they still needed to comply with the conditions of supervision, which typically involved programming, drug testing, and avoiding new arrests. Some study participants were ordered by the court to attend classes on substance use disorders and anger management, for example. In these cases, class fees were paid by the study, and treatment-group participants were still required to attend the classes.

Because relief from any type of financial obligation is a presumptive increase in an individual's welfare, there is a potential ethical problem with randomized relief from these costs. However, we did not know the effective size of the welfare gains that relief would cause before the study, and we were constrained in how many individuals to whom we could offer relief by the limited size of the study's budget. Under these conditions, a random lottery was an ethically fair way to allocate fee and fine relief.

Table 1 lists debt from prior cases and the main categories of court-related debt for treatment and control respondents. Because data on prior case debt were lost for treatment respondents, we rely on data from the control group to describe the level of outstanding prior case debt. Over half of the control-group respondents had outstanding court costs in Oklahoma County and owed an average of \$1,779 for prior criminal cases. On the current case, nearly all treatment and control respondents were assessed court costs that averaged just over \$700. DA and probation fees were assessed for nearly 40 percent of the sample and together totaled over \$300. In addition to court costs, supervision fees, and

Table 1. Court-Ordered Costs for a Sample of Indigent Misdemeanor Defendants in Oklahoma County

	Percent Owning (%)		Mean Amount Owed (\$)		Difference in Means <i>p</i> -value
	Treatment	Control	Treatment	Control	
<i>Prior Cases</i>					
Outstanding costs and fees		53.7		1,779.19	
<i>Current Case</i>					
Court costs	99.7	100.0	714.74	737.89	.44
DA prosecution fee	39.0	37.9	201.49	192.99	.72
Private probation fee	36.9	37.6	187.97	192.99	.82
DA supervision fee	3.1	3.5	16.27	15.56	.92
Mandatory classes fee	1.0	.0	6.51	.00	.10
Fines	2.7	2.3	3.22	1.25	.30
Total current costs	100.0	100.0	1,130.20	1,140.69	.85
Sample size (<i>N</i>)	295	311	295	311	

Note: Data on outstanding court costs and fines are only available for control cases. The reported amounts owed for current and prior cases are unconditional means that are calculated to include respondents who did not owe for that cost category. The *p*-values are for *t*-tests of the differences in mean amounts owed between the treatment and control groups. The *p*-value for the joint test of significance is .39.

prosecution fees, a few respondents were also assessed fees for court-ordered classes. The average cost of a misdemeanor case for the treatment group totaled \$1,130, compared to \$1,141 for the control group. Summing prior debt and current costs, control group participants owed an average of \$2,920 at the time of their most recent misdemeanor conviction. To implement the debt relief, the court deleted from the county record system the amounts owed by treatment respondents. As expected from the randomization, we found no significant differences in the mean amounts owed by treatment and control groups.

Although the treatment paid off a wide range of fees, the experiment omitted several categories of court debt. Respondents were not relieved of any court-ordered restitution or court-related debt owed to any counties other than Oklahoma County, or to any municipal government, including Oklahoma City. If a respondent was convicted of a new offense after randomization, the new debt was not relieved.

To inform treatment respondents that their current and prior costs had been relieved by court order, we made phone calls, sent

emails, and communicated through social media accounts. We also sent letters to control respondents thanking them for their participation and providing basic information about their cases and associated costs. We conducted surveys that indicated 80 percent of treatment respondents were aware they no longer owed fines and fees in Oklahoma County. If a respondent's knowledge of their treatment status led to reduced strain, and ultimately lowered the risk of new charges and convictions, then failure to inform respondents would have tended to shrink treatment effects on charges and convictions to zero.

Nineteen cases, or 3 percent of the sample, were not in full compliance with the treatment assignment. Of these 19, three control respondents had their debt relieved by mistake. Another 16 treatment respondents had at least one case in their history that was not included in the initial debt relief treatment. Those cases were later found and associated court fines and fees were waived. Our analysis here estimates intent-to-treat effects based on a respondent's status at random assignment. In Appendix Table A1, we report the effects of the treatment on the treated

using two methods: instrumental variables with assignment status as an instrument for implemented treatment, and ordinary least squares with implemented treatment as the independent variable. Only 3 out of 611 cases were fully noncompliant, another 16 were partially noncompliant, and noncompliance was random, so the intent-to-treat estimates are similar to the instrumental variables and treatment-on-treated estimates.

Data and Descriptive Statistics

Our main data sources were a baseline survey, administrative court records, and county jail records. We surveyed study participants either in jail or in court prior to case disposition. The baseline survey included questions on demographics, sources of income, substance use, housing, whether the respondent had a driver's license, and prior debt.² We observed baseline criminal history, charges, disposition dates, and sentences from state court records. More generally, electronic records for criminal processing were decentralized and sometimes incomplete. For example, the DA collects restitution using a separate web portal from the court cost payment system. As a result, some outcomes, such as arrest and restitution payments, were not readily available.

We grouped the dependent variables for analysis into three main categories. First, to test the hypothesis that relief of fines reduces the likelihood of criminalized behavior, we recorded new criminal charges in the Oklahoma County court, new convictions, and jail bookings after the initial case. New charges, convictions, and jail incarceration are system actions, but they represent responses by police and the courts to new behavior rather than a direct response to prior court debt. Second, to test the hypothesis that relief of fines and fees reduced new court action, we measured new court action related specifically to court-related fees, including new warrants, any new court debt (that might result from warrant fees, interest charges, or penalties), tax intercepts, and referral of outstanding debt to a private debt collector. Finally, to test whether fines and fees produced an economic

transfer to the criminal justice system, we recorded whether respondents made payments to the court and the amount they paid. We did not observe payments respondents may have made to the probation agency or the DA's office.

We can assess the implementation of the randomization and sample characteristics by examining the means of covariates that may be associated with the outcomes of interest. Table 2 reports demographic, socioeconomic, and criminal history characteristics of the study participants. Public defender clients in the Oklahoma County misdemeanor court are mostly poor and contending with poor health and economic insecurity. The participants were mostly male, in their mid-30s, on average, and over half were non-White. Over half the sample were jobless at the baseline interview. Slightly fewer than half were enrolled in public assistance. Nearly two-thirds reported problems with drugs or alcohol, 40 percent had a diagnosis of mental illness, and 31 percent reported being homeless. Most study participants faced charges for low-level offenses (drug possession or petty thefts); only 5 percent were charged with a violent offense. Given our high rate of study participation from the eligible population and the small differences in public defender representation rates between the eligible and ineligible charges, we think the sample is generally representative of individuals represented by the public defender in the Oklahoma County misdemeanor court. Sample characteristics are also similar to those found in other studies of court-involved respondents, who tend to be poor, insecurely housed, and with a history of substance use and mental illness (Harris 2016; Shannon et al. 2020; Western et al. 2015).

Consistent with randomization, the difference in means for the treatment and control groups is small for nearly all baseline characteristics. Unemployment is higher in the treatment group, suggesting relatively more disadvantage among study participants whose fees were waived. However, the control group shows a difference in the opposite direction for self-reported drug use, suggesting random assignment was not systematically violated to

Table 2. Means of Baseline Characteristics by Treatment Status, Oklahoma County

	Full Sample	Treatment Group	Control Group	Difference in Means <i>p</i> -value
<i>Demographic Characteristics</i>				
Male	.66	.67	.64	.49
Age (years)	36.04	35.86	36.22	.68
White	.43	.40	.46	.20
Black	.31	.33	.29	.29
Hispanic	.07	.08	.06	.37
Native American	.10	.10	.09	.79
Other	.09	.09	.10	.56
<i>Socioeconomic Characteristics</i>				
Unemployed	.58	.63	.53	.01
Public assistance	.49	.49	.49	.90
Other debt over \$1,000	.40	.38	.42	.35
High school or less	.52	.53	.51	.64
Mental health condition	.40	.41	.39	.71
Homeless	.31	.30	.33	.48
Substances a problem	.61	.56	.65	.02
No valid driver's license	.74	.75	.74	.73
<i>Current Offense and Criminal History</i>				
Drug offense	.55	.53	.57	.41
Property offense	.27	.27	.26	.83
Violence offense	.05	.05	.06	.46
In jail at enrollment	.50	.49	.50	.80
Prior charge in OK County	.53	.52	.55	.44
Prior charge outside OK County	.32	.30	.34	.27
Sample size (<i>N</i>)	606	295	311	

Note: Means are reported for respondents with non-missing values. The *p*-values are for a *t*-test of the difference in means between treatment and control groups. The *p*-value for the joint test of significance for the full sample is .29. For this test, missing values are replaced with 0, except for age, which is replaced with the sample mean.

treat more or less disadvantaged study participants. A joint test of the differences between treatment and control means yields a *p*-value greater than 10 percent, indicating we cannot reject the null hypothesis that the characteristics of the two groups are equal.

Methods

For respondent *i* ($i = 1, 2, \dots, N$), we estimate treatment effects with ordinary least squares regressions of the following form:

$$Y_i = \beta_0 + \beta_1 T_i + \varepsilon_i,$$

where Y_i is the outcome of interest, T_i is a dummy variable indicating assignment to

treatment, and ε_i is a random error with zero mean. Other studies of criminal justice involvement have found larger negative effects for Black men (Pager 2003; Pager, Western, and Bonikowski 2009), so we explore racial variation by estimating treatment effects for White and non-White participants. We also examine heterogeneity by employment status and self-reported substance use because these covariates were unbalanced between the treatment and control groups.

Given random assignment of the treatment, raw differences in mean outcomes without controlling for covariates yield unbiased estimates of treatment effects (Freedman 2008; Lin 2013; Mutz, Pemantle, and Pham 2019). However, covariate controls can

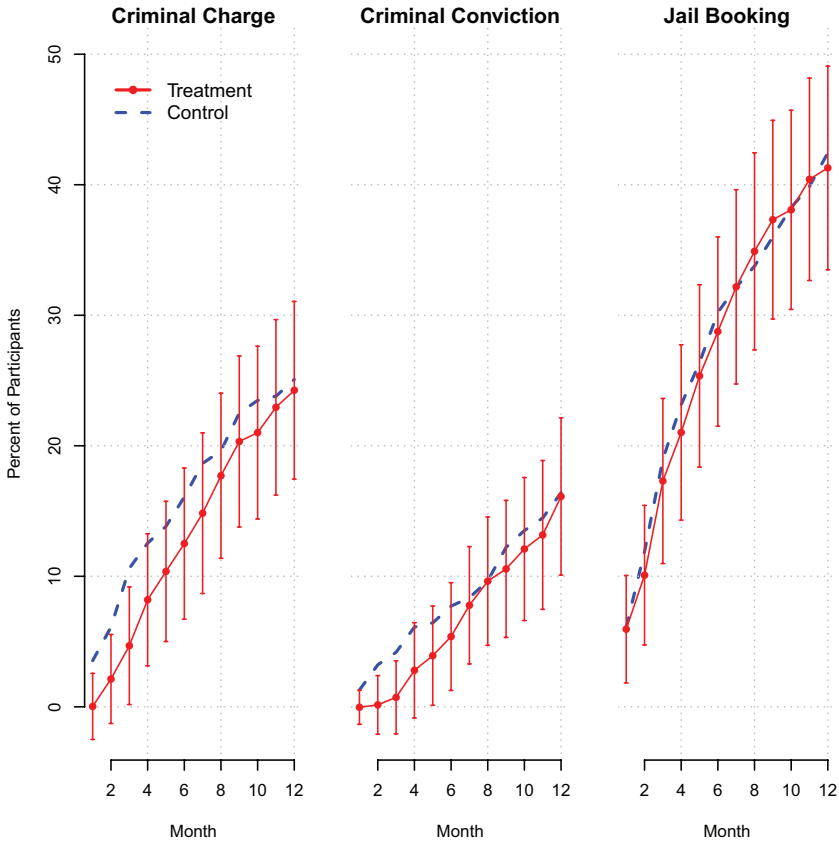


Figure 1. Mean Levels of New Criminal Justice Contact for Treatment and Control Groups in the 12 Months after Randomization, Oklahoma County
Note: Ninety-five percent confidence intervals are indicated for the treatment group.

reduce the residual variance and improve the precision of estimated treatment effects. Additional precision is gained if covariates are strongly predictive of outcomes (Lin 2013). We report covariate-adjusted treatment effects in Appendix Figures A1, A2, and A3.

RESULTS

Figure 1 reports monthly means for the treatment and control groups for the measures of new criminal justice contact. In the first three months after a misdemeanor conviction, respondents whose fines and fees were waived were significantly less likely to be charged with a new offense and also had a lower rate of new criminal convictions. We

find no difference between the treatment and control groups for new jail bookings. At one year after the initial conviction, treatment effects are close to zero for the three measures of criminal justice contact. The results suggest the relief of fees reduces crime for several months, but after a year they neither contribute to crime nor do they have any specific deterrent effect.

Figure 2 reports the monthly treatment and control means for court action. In contrast to the small treatment effects for new criminal justice contact, we find large and significant differences in court actions based on treatment assignment. Six months after the initial case, the Oklahoma County misdemeanor court was about 13 percentage

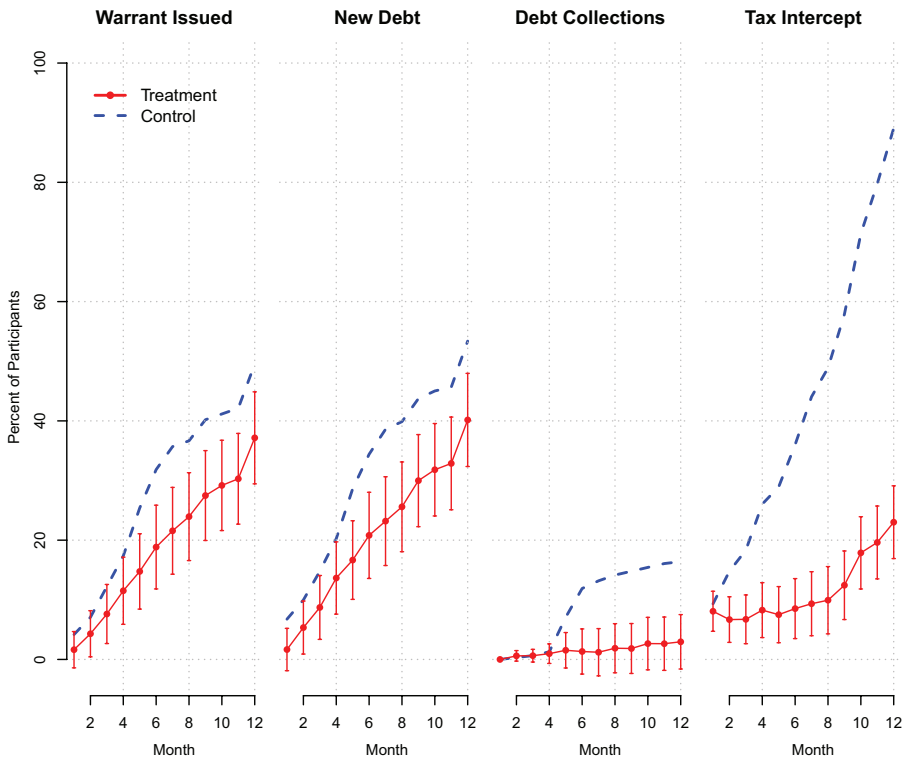


Figure 2. Mean Levels of New Court Actions for Treatment and Control Groups in the 12 Months after Randomization, Oklahoma County

Note: Ninety-five percent confidence intervals are indicated for the treatment group.

points more likely to issue a new warrant for control-group participants. Significantly more control-group participants had accumulated additional debt and had their outstanding fees sent to private collectors. We also observed a sharp growth in the proportion of control participants for whom the court ordered a new intercept for state income tax refunds in the months following treatment assignment. Additional analysis shows the long-term treatment effects on warrants resulted from the large increase in the number of failure-to-pay warrants among control-group participants (see Appendix Figures A4 and A5). In short, the Oklahoma County court took a range of steps to recover outstanding fees. Each of these steps created the possibility of continuing legal jeopardy.

Figure 3 shows the pattern of payments over time. Most control participants

who made a payment did so in the first six months; few additional participants began making payments after that point. Treatment participants, relieved of court debt that was outstanding at the time of randomization, made virtually no payments to the court in the year after their initial cases.³ Further analysis shows that, among the control-group respondents, only 11 percent paid an amount that represented 10 percent or more of the debt assessed on the current case. Only 5 percent of control respondents paid an amount equivalent to at least one year's worth of payments on a payment plan (\$300). Among the 22 percent of control participants who made at least one payment, the median amount paid was \$100. The 25th percentile total payment was \$50, and the 75th percentile was \$275. Even among control-group participants who made court payments, few made significant

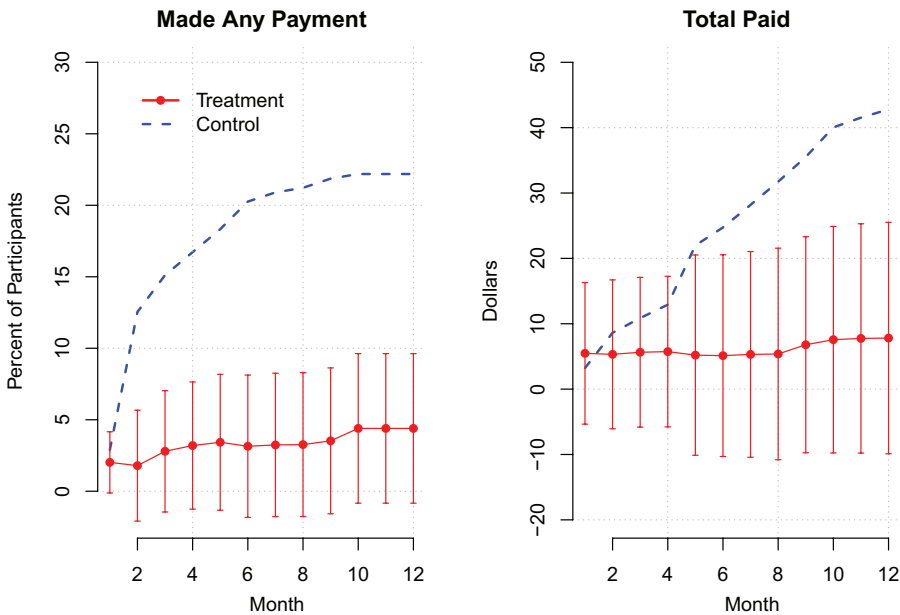


Figure 3. Mean Levels of Debt Payment for Treatment and Control Groups in the 12 Months after Randomization, Oklahoma County
Note: Ninety-five percent confidence intervals are indicated for the treatment group.

progress toward paying off debt on their new case (\$1,141, on average) or outstanding court costs (\$1,779, on average) one year after randomization.

Table 3 shows a summary of 12-month treatment effects. Results for new criminal justice contact indicate that the relief of legal debt does not affect the likelihood of new charges, convictions, or jail bookings after one year. However, relief of fines and fees significantly reduced new court action in the form of new warrants, new debt, tax intercepts, and private debt collection. Relief of fines and fees decreases the probability of new warrants and new debt by about a quarter (.13/.50 = .26 for warrants, .14/.53 = .26 for new debt). Whereas state tax refunds were ordered to be intercepted for 89 percent of control-group participants, only 22 percent of treatment-group participants were issued tax intercepts. One in six control-group respondents had their court fees sent to a private debt collector, compared to only 2 percent in the treatment group.

Table 3 also reports debt payment by the study respondents. In the year after their

initial case, control-group respondents were more likely to make a payment and had paid significantly more than treatment respondents who were relieved of court debt. Despite significant outstanding debts for control participants, nearly 80 percent made no court payments after conviction, and the average level of payments totaled \$43—less than 4 percent of the average court fees levied on the current case.

To explore treatment-effect heterogeneity by race, Table 4 reports 12-month treatment effects for White and non-White respondents. The control rates of criminal charges, convictions, warrants, and jail bookings are higher among non-White than among White participants. However, the magnitude and significance of treatment effects are similar for both race groups and similar to the full-sample treatment effects.

To assess the robustness of the results, we explore the heterogeneity of treatment effects by employment status and substance use. Results for jobless and employed respondents are similar to those obtained for the

Table 3. Control Group Mean and 12-Month Treatment Effects, Oklahoma County Misdemeanor Defendants

	Control Mean	Treatment Effect
<i>New Criminal Justice Contact</i>		
Criminal charge	.251 (.025)	-.003 (.035)
Conviction	.164 (.021)	-.001 (.030)
Any jail booking	.424 (.028)	-.014 (.040)
Number of bookings	1.019 (.110)	.082 (.179)
<i>New Court Actions</i>		
Warrant	.495 (.028)	-.129** (.040)
New debt	.534 (.028)	-.144** (.040)
Tax intercept	.891 (.018)	-.670** (.030)
Debt in collection	.164 (.021)	-.140** (.023)
<i>Debt Payment</i>		
Any debt paid	.222 (.024)	-.181** (.027)
Amount paid (\$)	42.803 (7.342)	-36.411** (8.850)
Sample size (N)	311	295

Note: Standard errors are in parentheses. Sample sizes are for control and treatment groups.
* $p < .05$; ** $p < .01$ (two-tailed tests).

full sample (see Table 5). Treatment effects for new criminal justice contact are mostly close to zero, except for jail bookings. Jobless respondents were booked into jail at higher rates than were respondents employed at baseline, but there is evidence that debt relief reduces jail bookings for the jobless. Among the employed, the treatment coefficient is large and positive, but not statistically significant. The effect of fee relief on new court action also appears stronger for jobless respondents. Employed respondents also recorded large negative treatment effects for intercepts on state tax refunds and the involvement of private debt collectors. A minority of jobless and employed respondents in the control group made any payments to the court. The average level of repayment

was low for both groups, despite differences in economic circumstances: \$39 for jobless control respondents, and \$49 for employed control respondents.

Table 6 reports variation in treatment effects according to whether respondents reported having problems with drugs or alcohol. Results for the two subgroups are again similar to those obtained for the sample as a whole. Regardless of self-reported drug or alcohol problems, we find no evidence of effects of the relief of fines and fees on charges, convictions, or jail bookings. New court actions in the form of warrants, new debt, tax intercepts, and third-party debt collections are significantly more common for control respondents, both among those reporting drug or alcohol problems and those not

Table 4. Control Group Means and 12-Month Treatment Effects for White and Non-White Respondents

	White		Non-White	
	Control Mean	Treatment Effect	Control Mean	Treatment Effect
<i>New Criminal Justice Contact</i>				
Criminal charge	.194 (.033)	-.035 (.048)	.303 (.035)	.006 (.050)
Conviction	.129 (.028)	-.003 (.042)	.194 (.030)	-.005 (.043)
Any jail booking	.381 (.040)	-.037 (.060)	.473 (.038)	-.016 (.054)
Number of bookings	.763 (.129)	.027 (.243)	1.267 (.170)	.053 (.261)
<i>New Court Actions</i>				
Warrant	.424 (.041)	-.139** (.059)	.552 (.038)	-.129** (.054)
New debt	.453 (.041)	-.126* (.061)	.600 (.037)	-.166** (.054)
Tax intercept	.878 (.027)	-.718** (.044)	.909 (.022)	-.646** (.040)
Debt in collection	.165 (.031)	-.140** (.035)	.158 (.028)	-.135** (.031)
<i>Debt Payment</i>				
Any debt paid	.237 (.035)	-.179** (.042)	.212 (.031)	-.184** (.034)
Amount paid (\$)	45.699 (11.359)	-43.799** (11.678)	41.876 (9.573)	-32.393** (12.632)
Sample size (N)	145	125	159	169

Note: Standard errors are in parentheses. Sample sizes are for control and treatment groups.
* $p < .05$; ** $p < .01$ (two-tailed tests).

reporting such problems. For debt repayment, control-group participants were more likely to make a payment to the court, but they paid very low amounts compared to the amount of court fines and fees levied. In the absence of a waiver of fines and fees, respondents would have paid around \$38 more, regardless of substance use history.⁴

DISCUSSION

Legal fines and fees have emerged as a key area of interest for policymakers and researchers, but there is little quantitative evidence for their causal effects on criminal justice involvement. In a randomized controlled trial in a misdemeanor court in

Oklahoma, we estimated the effects of the relief of fines and fees on new criminal charges and convictions, new court actions, and debt payments. Randomization of treatment assignment is important for estimating causal effects because factors like poverty, housing insecurity, untreated mental illness, substance use disorders, and a lack of social support may cause defendants to accrue court debt and are also associated with increased criminal justice involvement. If confounding factors are uncontrolled, criminal justice involvement may be wrongly attributed to the causal effect of legal debt.

We find that respondents whose fines and fees were relieved were no more likely to face new criminal justice contact after one year

Table 5. Control Group Means and 12-Month Treatment Effects by Pre-arrest Employment Status

	Employed		Not Employed	
	Control Mean	Treatment Effect	Control Mean	Treatment Effect
<i>New Criminal Justice Contact</i>				
Criminal charge	.175 (.031)	.057 (.052)	.321 (.036)	-.062 (.049)
Conviction	.119 (.027)	.011 (.042)	.204 (.031)	-.020 (.043)
Any jail booking	.322 (.041)	.104 (.062)	.519 (.039)	-.119** (.053)
Number of bookings	.573 (.097)	.380 (.177)	1.395 (.181)	-.217 (.279)
<i>New Court Actions</i>				
Warrant	.448 (.041)	-.077 (.063)	.537 (.039)	-.175** (.053)
New debt	.497 (.041)	-.080 (.063)	.568 (.038)	-.195** (.053)
Tax intercept	.923 (.022)	-.701** (.046)	.864 (.027)	-.648** (.041)
Debt in collection	.175 (.031)	-.147** (.036)	.148 (.027)	-.127** (.030)
<i>Debt Payment</i>				
Any debt paid	.266 (.036)	-.238** (.040)	.185 (.030)	-.137** (.034)
Amount paid (\$)	48.703 (11.115)	-35.134* (17.090)	38.872 (9.741)	-36.601** (10.017)
Sample size (N)	143	108	162	185

Note: Standard errors are in parentheses. Sample sizes are for control and treatment groups.

* $p < .05$; ** $p < .01$ (two-tailed tests).

compared to the control group, but they were slightly less likely to incur new charges and convictions in the first three months. These results are consistent with a short-lived causal effect in which fee relief briefly reduces crime and police contact. However, relief of fines and fees greatly reduces continuing court involvement by largely eliminating court efforts to collect payment. Study participants who were relieved of legal debt were significantly less likely to receive new warrants, to be assessed new debt, to receive a new hold on state tax refunds, and to have their cases referred to a private debt collector. The control group was also more likely than the treatment group to pay off court fees, but they paid only a small fraction of the total debt owed to the court. Given the low level of payment by

control-group participants, we conclude that extensive efforts at debt collection do little to achieve their stated purposes of recovering costs or ensuring personal accountability.

These findings are subject to several limitations. First, with roughly 300 participants in each experimental group, the study only had statistical power to detect relatively large effects. Evidence for the criminogenic effects of unpaid fines and fees may have been stronger with a larger sample size. Second, we were only able to relieve fines and fees in Oklahoma County; we did not relieve any fines or fees that participants may have owed to the local municipality (Oklahoma City) or other counties, nor any restitution. Relief of legal debt outside the county may have produced larger treatment effects. Finally, we

Table 6. Control Group Means and 12-Month Treatment Effects by Pre-arrest Substance Use

	Substance Use		No Substance Use	
	Control Mean	Treatment Effect	Control Mean	Treatment Effect
<i>New Criminal Justice Contact</i>				
Criminal charge	.269 (.031)	.001 (.047)	.236 (.039)	-.002 (.055)
Conviction	.178 (.027)	.025 (.042)	.142 (.033)	-.025 (.044)
Any jail booking	.437 (.035)	.024 (.053)	.425 (.047)	-.068 (.064)
Number of bookings	1.036 (.125)	.204 (.221)	1.047 (.212)	-.094 (.321)
<i>New Court Actions</i>				
Warrant	.492 (.035)	-.100* (.052)	.491 (.047)	-.149** (.064)
New debt	.538 (.035)	-.127** (.053)	.519 (.047)	-.147** (.065)
Tax intercept	.893 (.022)	-.642** (.041)	.896 (.029)	-.710** (.045)
Debt in collection	.162 (.026)	-.132** (.030)	.151 (.034)	-.135** (.037)
<i>Debt Payment</i>				
Any debt paid	.208 (.028)	-.171** (.033)	.255 (.041)	-.216** (.046)
Amount paid (\$)	40.935 (8.770)	-38.907** (9.082)	49.034 (13.209)	-37.018* (17.387)
Sample size (N)	197	163	106	129

Note: Standard errors are in parentheses. Sample sizes are for control and treatment groups.
* $p < .05$; ** $p < .01$ (two-tailed tests).

were not able to directly observe participant behavior; we could only observe behavior indirectly for a relatively short period through the court’s administrative records. Because we focus on administrative records, where our data collection is most complete, we have not considered the effects of relief of fines and fees on, for example, economic security and well-being. A longer follow-up period, even with the available administrative data, would also show whether warrants for non-payment, for example, are associated with a higher risk of incarceration.

The results of this study likely generalize to other jurisdictions, but they may understate the effects on criminal justice involvement for more punitive court systems. Many jurisdictions impose fines and fees widely with the aim (perhaps unfulfilled) of shifting

the cost of court operations from the general public to defendants (Harris 2016; Shannon et al. 2020). Poverty and accompanying social disadvantages are broadly found in criminal courts across the country (Irwin 1985; Kohler-Hausmann 2018; Natapoff 2018; Van Cleve 2016), but Oklahoma County’s policy is not to jail those who fail to pay, so effects on criminal justice contact may be larger in jurisdictions that pursue criminal justice debt more aggressively (Menendez et al. 2019). Indeed, several study respondents reported they were more concerned about criminal justice debt in other Oklahoma counties.

The experimental results provide strong evidence for how fines and fees contribute to a criminalization of poverty, and they help specify how poor people become subject to enduring criminal justice control. In contrast

to arguments that fines and fees are a source of social strain that leads to re-arrest, we find little evidence for this pathway. For much of the study sample, criminal justice debt was just one potential source of strain amid myriad financial and health difficulties. Instead, the experimental evidence indicates that fines and fees criminalize poverty by prolonging criminal court involvement for those unable to pay. Our evidence is thus consistent with other observations of ongoing court supervision that have been described as “permanent punishment” (Harris 2016) and “layaway freedom” (Pattillo and Kirk 2021). The ongoing court obligations we observed are similar to the “procedural hassle” described by Kohler-Hausmann (2018) in the New York misdemeanor courts. Like procedural hassle, court obligations for fines and fees are less painful than incarceration, but they are intrusive and create the risk of more serious punishment.

The evidence indicates that criminal justice policy creates an absorbing state for poor people. The imposition of fines and fees operates as a bureaucratic adjunct to criminal processing, added as a matter of court operations without reference to the facts of any particular case. In this way, entanglement in the courts and the penal system is routinized without opportunities for challenge. In Oklahoma County, the misdemeanor court processed drug, public order, and other low-level offenses for people who experienced high levels of poverty, unemployment, homelessness, and poor health. From these defendants, the court recouped less than 5 percent of imposed fees. Intercepted tax refunds and debt collector pressure can add to the economic insecurity of poor defendants, and warrants place them at risk of arrest and incarceration. Studies show that the criminal justice system by itself can be criminalizing, in the specific sense of causing new criminal justice contact (Aizer and Doyle 2015; Dobbie, Goldin, and Yang 2018; Paternoster and Iovanni 1989). In the case of fines and fees, continuing court involvement emerges without evidence of a corresponding increase in criminal behavior. Court fees thus create a pure criminalization of poverty, in which

a misdemeanor conviction by itself, and not crime, creates ongoing involvement in the criminal justice system.

The proliferation of fines and fees has been viewed as part of a larger political economy of state predation, in which criminal justice agencies directly seize the financial assets of individuals who are subject to its control (Katzenstein and Waller 2015; Page and Soss 2017, 2021). The theory helps explain the nexus between punishment and inequality, and the directly economic character of punishment in a period of mass criminalization. However, low levels of repayment among control participants provide evidence against the predatory effects of fines and fees from criminal convictions in Oklahoma.

The Oklahoma County court tried to recover outstanding debt, but few control respondents made any court payments. The low level of repayment can be understood in the context of the low socioeconomic status of study participants, over half of whom were not working at the time of the baseline survey. At baseline, half of respondents were on public assistance, almost a third reported being homeless, and 40 percent reported having more than \$1,000 in non-criminal justice debt. These disadvantages reflect a low capacity to pay and a high burden of competing financial obligations. There is certainly strong evidence of predation for municipal violations (U.S. Department of Justice 2015), but the current results underline the fact that many people who are subject to state authority are already poor and can provide little economically to a predatory state. From this perspective, criminal justice control and predation are distinct forms of domination. Under the harsh conditions of U.S. poverty, fines and fees may contribute more to enduring disadvantage through permanent punishment and procedural hassle than through the extraction of income.

Finally, the findings have implications for policy. A number of implications can be drawn from the analysis, but three stand out. First, widespread poverty among people convicted of misdemeanors rendered court efforts to collect payment largely futile. As another analysis of fines and fees has observed,

efforts to extract payment are like “drawing blood from stones” (Harris et al. 2010). Significant efforts to recover legal debt through the use of private debt collectors and on the part of court clerks could be greatly reduced. This would reduce procedural hassle and the risks of later incarceration for a mostly poor population with misdemeanor convictions. The evidence indicates this would have little adverse effect on court finances. Second, the evidence also supports the decision in some jurisdictions (e.g., in California [CA AB-1869] and San Francisco [SF Ord. 131-18]) to forgive outstanding criminal justice fees en masse. According to our evidence, mass waiver of fines and fees would have no effect on recidivism or court finances,

but it would limit court supervision rooted in poverty. Mass waiver represents an effort to repair the negative effects of fines and fees policy. Third, our evidence suggests policy reform could go even further. Abolishing user fees in criminal courts would have no effect on crime but would eliminate a large but ineffective cost-recovery bureaucracy. Having unpaid debt accelerates criminal justice involvement, and there is little evidence that the severity of criminal sanctions is an effective lever for reducing crime in general (Chalfin and McCrary 2017; Nagin 2013). The abolition of fines and fees would cut the nexus between criminalization and poverty in at least one domain of the criminal justice system.

APPENDIX

Table A1. Instrumental Variable and Ordinary Least Squares Estimates of the Effects of the Treatment on the Treated, 3 Months and 12 Months after Randomization ($N = 606$)

	Instrumental Variables		OLS Treatment on Treated	
	3 Months	12 Months	3 Months	12 Months
<i>New Criminal Justice Contact</i>				
Criminal charge	-.046* (.023)	-.003 (.035)	-.047* (.022)	-.002 (.035)
Conviction	-.032* (.013)	-.001 (.030)	-.032* (.013)	-.005 (.030)
Any jail booking	-.010 (.032)	-.014 (.040)	-.007 (.032)	-.009 (.040)
Number of bookings	.032 (.060)	.083 (.182)	.032 (.059)	.081 (.180)
<i>New Court Actions</i>				
Warrant	-.038 (.025)	-.130** (.040)	-.033 (.025)	-.124** (.040)
New debt	-.050 (.027)	-.145** (.040)	-.045* (.027)	-.140** (.040)
Tax intercept	-.106** (.027)	-.677** (.030)	-.101** (.027)	-.668** (.030)
Debt in collection	.000 (.007)	-.142** (.023)	.000 (.007)	-.142** (.023)
<i>Debt Payment</i>				
Any debt paid	-.129** (.022)	-.183** (.026)	-.129** (.022)	-.184** (.026)
Amount paid (\$)	-5.826 (5.684)	-36.766** (8.818)	-5.927 (5.617)	-36.892** (8.781)

Note: The instrumental variable analysis uses treatment assignment as an instrument for implemented treatment. The ordinary least squares analysis uses implemented treatment instead of assigned treatment as the independent variable. Standard errors are in parentheses.

* $p < .05$; ** $p < .01$ (two-tailed tests).

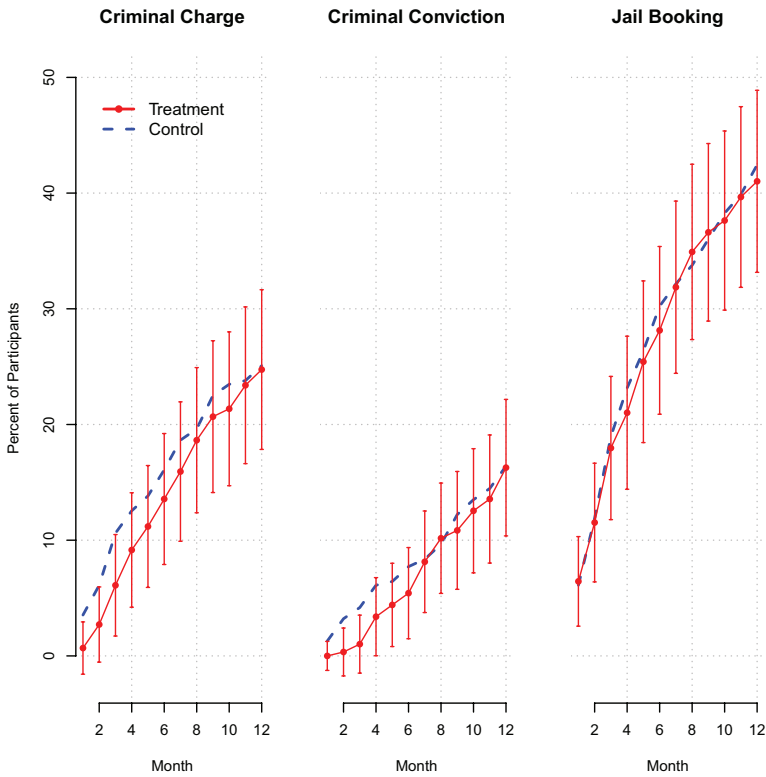


Figure A1. Covariate-Adjusted Mean Levels of New Criminal Justice Contact for Treatment and Control Groups in the 12 Months after Randomization, Oklahoma County
Note: Ninety-five percent confidence intervals are indicated for the treatment group.

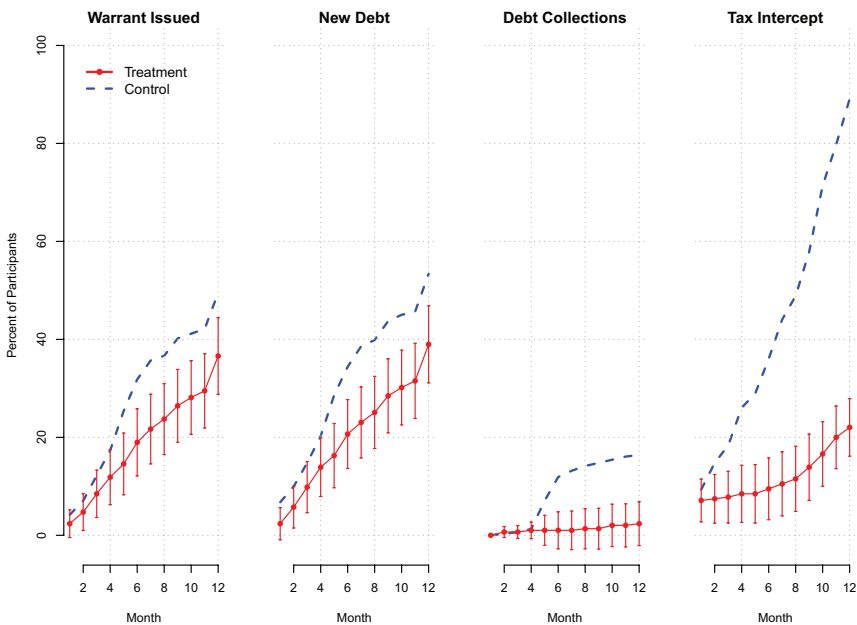


Figure A2. Covariate-Adjusted Mean Levels of New Court Actions for Treatment and Control Groups in the 12 Months after Randomization, Oklahoma County
Note: Ninety-five percent confidence intervals are indicated for the treatment group.

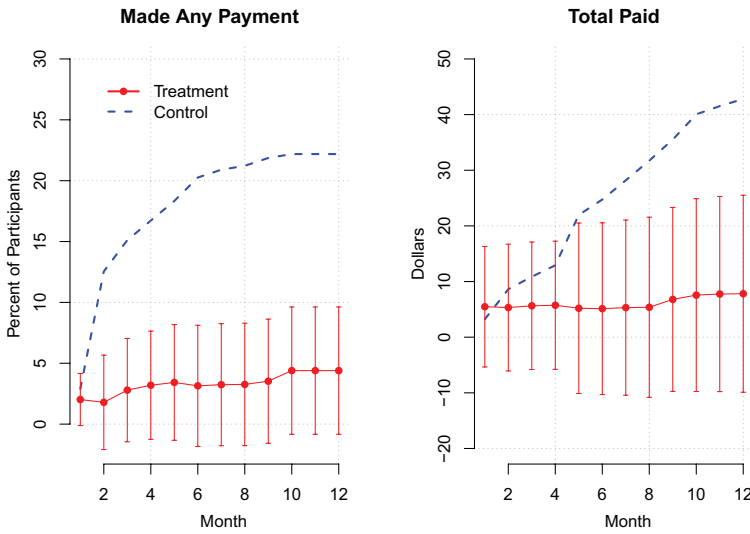


Figure A3. Covariate-Adjusted Mean Levels of Debt Payment for Treatment and Control Groups in the 12 Months after Randomization, Oklahoma County
Note: Ninety-five percent confidence intervals are indicated for the treatment group.

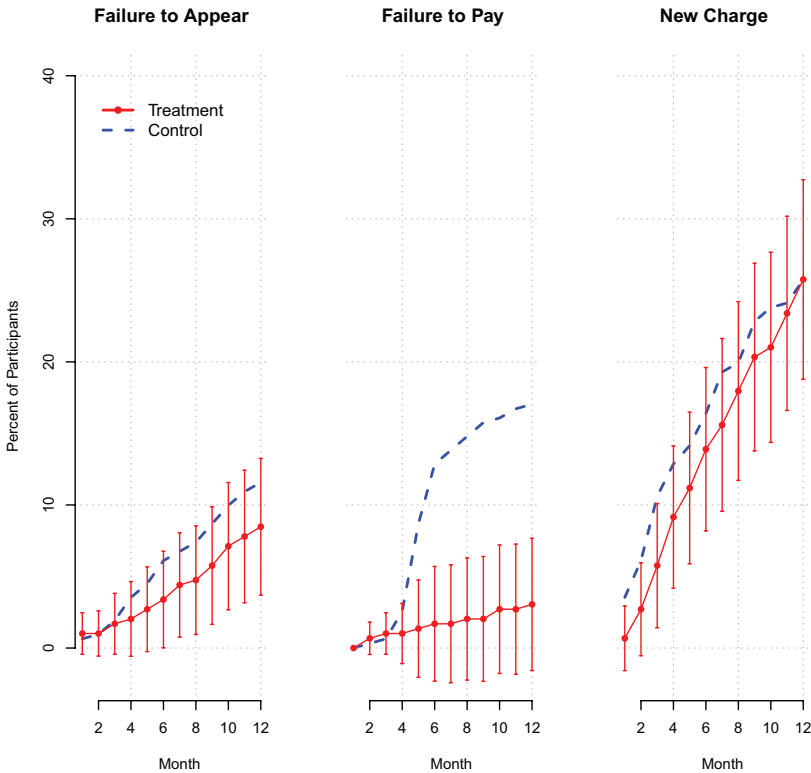


Figure A4. Mean Levels of Warrants for Failure to Appear, Failure to Appear and Pay, and for New Charges for Treatment and Control Groups in the 12 Months after Randomization, Oklahoma County
Note: Ninety-five percent confidence intervals are indicated for the treatment group.

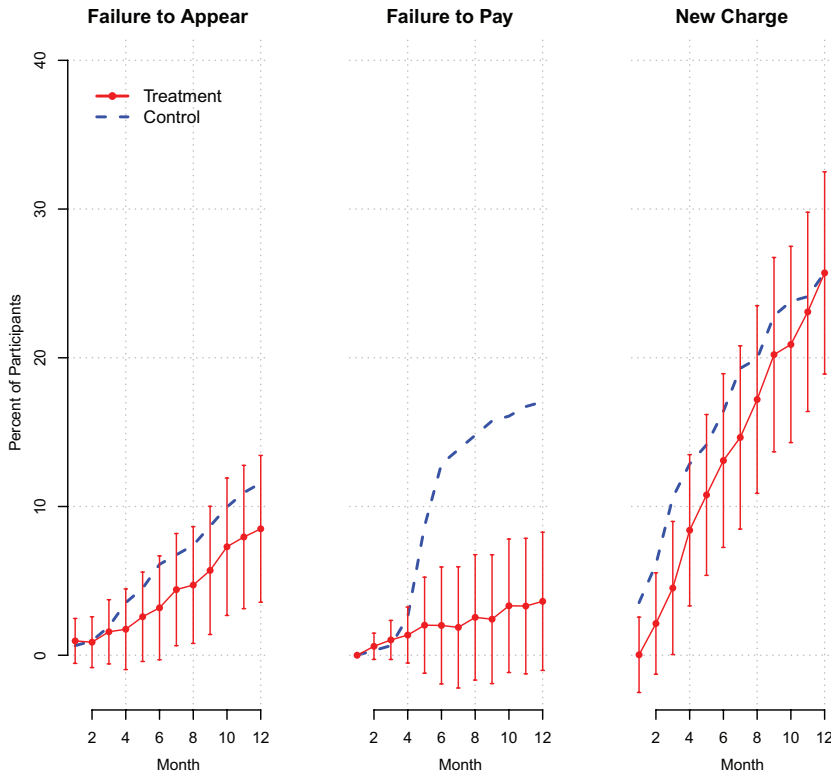


Figure A5. Covariate-Adjusted Mean Levels of Warrants for Failure to Appear, Failure to Appear and Pay, and for New Charges for Treatment and Control Groups in the 12 Months after Randomization, Oklahoma County

Note: Ninety-five percent confidence intervals are indicated for the treatment group.

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Notes

1. Preregistration details are available from the American Economic Association Randomized Controlled Trial Registry as AEARCTR-0002865.
2. An early version of the baseline survey omitted a question about gender, affecting 20 cases (3 percent). We imputed gender for these respondents.
3. Of the 12 treatment participants who made payments, two made payments for post-randomization convictions and four made payments before the court could implement the debt relief. In six cases, the court erroneously accepted payment, as the system allows court staff to accept payments even when the balance is zero.

4. We also examined the sensitivity of treatment effects to restitution status. Dividing the sample according to whether respondents were ordered to pay restitution yielded similar treatment effects to those reported. For respondents ordered to pay restitution, treatment effects for new court actions at 12 months were generally slightly smaller than for respondents not ordered to pay restitution, but we cannot draw strong conclusions because of the small sample size.

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