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THE IMPACT OF CRIMINAL FINANCIAL SANCTIONS: A MULTI-STATE ANALYSIS OF SURVEY AND ADMINISTRATIVE DATA

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The Impact of Criminal Financial Sanctions: A Multi-State Analysis of Survey and Administrative Data
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ABSTRACT

We estimate the impact of financial sanctions in the U.S. criminal justice system using nine distinct natural experiments across five states. These regression discontinuity designs capture a range of enforcement levels ($17–$6,000) and institutional environments, providing robust causal evidence and external validity. We leverage survey and administrative data to consider a variety of short and long-term outcomes including employment, recidivism, household expenditures, spousal spillovers, and other self-reported measures of well-being. We find consistent, robust evidence of precise null effects on the population, including ruling out long-run impacts larger than -$347–$168 in annual earnings and -0.002–0.01 in annual convictions.

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1 Introduction

Recent decades have seen a steady expansion in the use and magnitude of legal financial obligations (LFO) owed by criminal defendants in the United States (Bannon, Mitali, and Diller 2010; Harris, Pattillo, and Sykes 2022). These justice-related fees and financial sanctions range from minor traffic tickets to more substantial restitution and correctional supervision fees. LFOs are comprised of three main categories: (1) payments from convicted defendants to victims in the form of restitution, (2) sanction-oriented fines to discourage further criminal activity and encourage court appearances (Emanuel and Ho 2022), and (3) service-based fees to cover the cost of trials or punishment and supervision. Many state and local governments have come to rely on the revenue generated from these fines and fees to fund courts and other government services (Makowsky 2019; Maciag 2020). According to the Survey of Inmates in State and Federal Correctional Facilities, the share of inmates with LFOs increased from 25% in 1991 to 66% in 2007 (Harris, Evans, and Beckett 2010); the Fines and Fees Justice Center estimates that recent national court debt exceeded $27 billion (Hammons 2021).

From 2003 to 2014, Florida, Michigan, North Carolina, Texas, and Wisconsin each passed laws sharply increasing the financial obligations imposed upon conviction. We leverage these different jurisdiction-specific policy changes (nine in total) in a regression discontinuity design (RDD) framework to measure the causal impact of the increased monetary sanctions on individual criminal and labor market trajectories along with spillovers on the household. The marginal increase in legal financial obligations across our considered natural experiments range from $17 to $6,000, providing important exogenous variation across the distribution of potential LFO levels. This breadth of settings also strengthens both the precision and external validity of our results; our final sample draws from five states that together represent 24% of the U.S. population. Finally, the variation in institutional rules and contexts across states allows for a rich exploration of important policy mechanisms (e.g., consequences of non-payment, considerations for ability to pay, etc.).[1]

In each of the natural experiments, fine increases were strictly applied based on whether the event date, typically the case disposition, was after the policy implementation date. We utilize these institutional features to present both policy-specific and combined-sample regression discontinuity findings that together explore the possibility for treatment effect heterogeneity across jurisdictional

[1] To the best of our knowledge, eight out of the nine natural experiments studied in this paper have not been previously explored in the literature. We overlap with Giles (2021), who studies the impact on recidivism of Wisconsin's universal $200 DNA fee introduced in 2015 for misdemeanor cases (see Section 3 for details). While we extend the analysis of this natural experiment to non-recidivism outcomes and longer follow-up windows, we also come to different substantive conclusions regarding the effect of the DNA fees on recidivism compared to Giles (2021). Our reasoning is discussed in detail in Online Appendix A.

boundaries and strengthen precision in pooled regressions. When pooling sub-experiments in regression and graphical exercises, we apply a strategy to reweight observations to ensure that each policy experiment contributes equally to the combined estimates to avoid overemphasizing natural experiments with more sample observations.

We find that criminal financial sanctions have no economically meaningful impact on short- or long-term recidivism or labor market outcomes. Given the size of our research sample, these are precise null effects. Specifically, we can rule out effects greater than -2.3 percent to 1.1 percent with 95% confidence on total earnings reported on W-2 tax returns for the first 10 years after the cutoff (-\$347–\$168 per year) and effects larger than -0.9 percent to 4.8 percent on total recidivism (-.001–.01 number of convictions per year) 10 years after the focal event. A parallel analysis arrives at the same conclusions regarding potential spillovers of fines and fees onto the romantic partners of our focal defendants.

The impacts of fines and fees, however, might manifest in ways missed in traditional administrative data sources (e.g., informal labor supply, consumption, stress, or expenditures). In addition, many criminal fines use the threat of driver's license suspension to enforce payment, which could disrupt commuting patterns even if employment remains unchanged. To examine these issues, we estimate the impacts of the LFOs on self-reported earnings, housing costs, commuting patterns, and mental well-being using individual responses to the American Community Survey (ACS). Consistent with our administrative data results, we do not find any changes in self-reported total income, which rules out potential responses in either informal income or earnings from self-employment that would be missed in our administrative data. We also do not find any significant changes in monthly housing costs, rent, or mortgages. Specifically, we can rule out effects larger than -3 to 2 percent (-7 to 1 percent) with 95% confidence on monthly housing costs (self-reported monthly income). Similarly, we find no significant impact on whether the respondent commutes to work by car or their self-reported likelihood of difficulty concentrating or remembering due to a mental, physical, or emotional condition, indicating no perceptible change in driving behavior or in mental health.

Although the sub-experiments in our study represent diverse institutional and demographic contexts, our long-run results are largely consistent. This suggests our null findings are not simply the consequence of the LFOs being too high, too low, or relying on a specific punishment mechanism that might not generalize to other contexts.

Our findings contrast with descriptive research, which has largely found strong correlational evidence linking fines and fees with financial instability, criminal recidivism, and poor labor market outcomes (Harris 2016; Pleggenkuhle 2018; Menendez and Eisen 2019).[2] Some quasi-experimental analyses have found results consistent with this body of work (see Mello (2021), Kessler (2020), Luh (2020), and Giles (2022)), while others have failed to replicate these findings (see Goncalves and Mello (2017), Dusek and Traxler (2022), and Alexeev and Weatherburn (2022)). Quasi-experimental evidence in line with the descriptive research mainly focuses on non-criminal traffic infractions, which given our findings suggests that perhaps financial sanctions can harm individuals but that the marginal treatment effect of fines goes to zero conditional on the documented negative ramifications of a criminal record (Pager 2003; Mueller-Smith and Schnepel 2021; Agan, Doleac, and Harvey 2023).

Pager et al. (2022) present the highest quality causal evidence to date using a randomized controlled trial of debt relief totaling $3,000 on average from court-related LFO's for misdemeanor defendants in Oklahoma County, Oklahoma. Their findings fail to reject the null hypothesis of no impact of debt relief on future criminal behavior within one year after the intervention, although they do find evidence of increased incidences of debt collection and ongoing court supervision resulting from unpaid fines. Because of the experiment's relatively modest sample size, the confidence interval on treated defendants' outcomes encompasses a wide range. For example, the 95% confidence interval on the effect size for the impact to future likelihood of conviction ranges from -36.5% to 35.2%, which includes economically meaningful values.

Our paper makes several important contributions to the literature. First, we analyze nine distinct natural experiments and provide consistent, robust, causal estimates on the null effects of criminal financial sanctions.[3] Second, we are able to study a diverse set of outcomes (recidivism, employment, expenditures, mental well-being, household spillovers) to fully capture the potential effects of LFOs. Third, we provide evidence on the impacts to both short and long-term behavior, substantially expanding the follow-up window compared to prior work for up to 10 years. Finally, our significantly larger sample sizes permit us to meaningfully narrow confidence intervals associated with null findings.

While our findings are less pessimistic than some prior research, we still conclude that LFOs are a regressive form of funding for the government with limited benefits in terms of discouraging criminal activity. We observe no change in the offending rate for targeted crimes in the general population and no fall in recidivism in our study sample, suggesting no evidence of general or specific deterrence responses. Given the low income of our sample, the impacts of the policy were

[2] See Martin et al. (2018) or Fernandes et al. (2019) for recent reviews of the literature on financial sanctions.

[3] As discussed in Abadie (2020), failures to reject the null hypothesis when data are large (as is the case in our setting) can often be highly informative and should not be dismissed as less *scientifically significant* compared to point null rejections.

concentrated on those less likely to pay the fees, placing them at higher risk for additional fines, arrest warrants, and potential driver's license suspensions. It is perhaps unsurprising that several of the policies we study were eventually suspended by local jurisdictions due to their lack of success and large-scale debt forgiveness programs totalling up to \$3.1 billion in foregone revenue were adopted.

2 Legal financial obligations in the U.S. criminal justice system

Driven by the rising costs of the criminal justice system, the use and amount of financial sanctions in the criminal justice system has exploded in recent years (O'Neill Hayes 2020). The Hamilton Project estimates that the total annual revenue from these sanctions collected by state and local governments exceeds \$15 billion (Liu, Nunn, and Shambaugh 2019). This estimate does not encompass unpaid fines and fees, which is at least another \$27.6 billion, according to the Fines and Fees Justice Center (Hammons 2021).

In response to these growing costs, many states have adopted additional fines and fees along with increasing existing ones. For example, from 1996 to 2007, Florida added 20 new types of LFOs (Diller 2010). Prior to the COVID-19 pandemic, North Carolina collected more than 50 separate fees in its justice system (Crozier, Garrett, and Maher 2020). Funding the justice system from convicted individuals rather than from tax payers more broadly may be politically appealing since it could enable governments to raise revenue without increasing taxes (Harris, Pattillo, and Sykes 2022).

Currently, most states require consideration for ability to pay when assigning some, but not all, financial sanctions. According to the National Criminal Justice Debt Initiative, 35 states require determining an individual's ability to pay (indigence) *before* assigning a select subset of financial sanctions, but the number of mandatory sanctions far outweighs this subset. The definition of *ability to pay* also varies across courts. While some states have strict definitions (e.g., income 200% above the poverty level in Kentucky disqualifies an individual from indigent status), others such as Illinois and Michigan leave it to the judge's discretion for determining ability to pay, leading to arbitrary and inconsistent waivers across cases (Bannan, Nagrecha, and Diller 2010; Gross 2013).

In practice, ability to pay is often ignored, and the remedies may also not be affordable. Some states, such as Michigan, require an upfront flat payment to enter into a payment plan or require that the debt be repaid within an unreasonable time period (Bannan, Nagrecha, and Diller 2010).

While community service is an option to pay off debt, courts oftentimes do not offer this option to indigent individuals, and community service might only apply to certain fees (Bannan, Nagrecha, and Diller 2010; Diller 2010). As we will show later, the fines in our study were rarely waived, nor were they substituted with community service.

Consequences for failure to pay can vary depending on the type of LFO and state laws. In all of the states we study except for Wisconsin, payment of criminal debt is a condition of probation. Many argue that this condition unfairly harms low income individuals who are less able to pay their debt on time. Failure to make payments also leads to future court hearings where judges can sentence individuals to incarceration, further prolonging their contact with the justice system.

Aside from the additional criminal justice involvement, many have also pointed out the high monetary cost of the debt on the rest of society (Harris, Evans, and Beckett 2010). Furthermore, actual revenue gains are often significantly lower than the total assessed amounts. According to a report on Florida's criminal justice debt, court clerks expect to collect only 9% of financial sanctions assigned upon felony conviction, leading to significant differences between the assigned and actual revenue (Diller 2010). Court systems oftentimes contract debt collection to private companies, adding more costs upon the system. Furthermore, extended probation from debt non-payment imposes further expenditures such as increased personnel costs for probation officers.

2.1 Nine natural experiments with exogenously varying fine amounts

In this section, we outline the nine specific abrupt fine changes exploited in this paper along with the corresponding policy-specific institutional details. For the sake of brevity, we limit the background discussion for each natural experiment to just the core concepts, but Online Appendix B provides a detailed accounting of the implementation and policy variation across each of the natural experiments. A summary of this information is outlined in Table 1, highlighting the similarities and differences in the state specific samples and data availability in addition to institutional details. Although the type of monetary sanction (e.g., fine, fee, surcharge) altered by legislative changes varied by state, which we describe below, for simplicity we refer to the sanctions as fines and the policy change as the "fine increase" in the remaining sections of the paper.

Eight of out the nine fine increases were applied based on conviction date falling on or after an implementation date threshold; for the consolidation of fines for misdemeanor defendants in Texas, the offense date was alternatively used. Since conviction date is potentially manipulable, which could violate the assumptions of our research design, we provide evidence of sample balance in Section 4.

Four out of the nine policies we study raised fines by less than \$100, three between \$200 to \$750, and two by \$1,000 or more. The maximum penalty we observe is \$6,000, which defendants must pay out in even installments over the course of three years. Together, these fine increases span a wide range of dosage amounts, providing valuable exogenous variation to help identify non-linear responses to fine levels.

Seven out of the nine policies were adopted between 2003 and 2004. These early experiments provide an ample follow-up window to assess long-run behavioral changes, which we define as ten years post-disposition. Two were adopted in later years (2011 and 2014), providing useful temporal variation in adoption timing. In these two cases, when ten years of follow-up data is not always feasible, we include as much observed information about the defendants' future outcomes as possible.

Four of the nine sub-experiments were associated with Driver Responsibility Fees (DRF) adopted in Texas and Michigan.[4] These fine increases only targeted defendants convicted of criminal traffic offenses, but varied dramatically in the amount of imposed fine based on the specific type of offense. Driving under the influence (DUI) defendants faced fees in the high range of \$2,000 to \$6,000 while other traffic offenses (non-DUI) were only subjected to \$300 to \$1,000 penalties. Because of the stark variation in fine amounts that we can cleanly differentiate based on the defendant's offense type, we split these two statewide DRF policy changes into four sub-experiments in our analyses: MI DRF non-DUI, MI DRF DUI, TX non-DUI, and TX DUI.

All of the policy changes we study apply late fees for fine non-payment, although only a subset rely on private collection agencies for debt collection purposes. Most jurisdictions we consider also utilize driver's license suspensions as an additional enforcement mechanism, although some restrict that to only defendants convicted of criminal traffic offenses. Finally, some jurisdictions threaten defendants with jail time or new misdemeanor charges for non-payment. When plotting sub-experiment specific point estimates in Section 5, we visually highlight these different enforcement strategies to draw attention to potential treatment effect heterogeneity.

3 Data

We combine several sources of administrative and survey data to measure the potential impacts of fines: criminal records from CJARS (Finlay and Mueller-Smith 2020), longitudinal earnings data from IRS W-2 tax forms, and the American Community Survey from 2005–2020. The data

[4]Online Appendix C provides a brief overview and history of the use of DRF programs in the United States.

are linked together at the person-level using a Protected Identifier Key (PIK) and analyzed in the secure environment of a Federal Statistical Research Data Center. As a result of the PIK linkage, outcomes are limited to individuals in the Social Security Administration data and to individuals with an Individual Taxpayer Identification Number (ITIN) (Brown et al. 2018). Thus, we cannot link individuals who are more likely to be undocumented.[5]

One major advantage of the CJARS data is the harmonization and aggregation of local agency records into an integrated data system making multi-state analysis significantly easier (Finlay, Mueller-Smith, and Papp 2022). Criminal justice data, especially adjudication data, is disaggregated across agencies, leading to discrepancies in data availability and offense classification (Choi et al. 2023) along with other variable definitions. Using the CJARS data, the criminal records are already harmonized across state making state-wide and multi-state analysis significantly easier. The CJARS data also covers multiple stages of the justice system allowing us to link the focal disposition to probation spell to measure the impacts of sanctions on probation length. For a subset of states, we also have payment and assessment data which allows us to measure changes in total payments.

In order to avoid repeated entry into the analysis sample, we restrict the focal sample to the first charge observed for each individual within each of our experiment subsamples.[6] While we restrict to first qualifying charge by state, some of the fine increases applied to just felonies or misdemeanors meaning that some of our sample may have a prior criminal record. Panel B of Table 1 shows the sample restrictions, data restrictions, and the choice of running variable defined by each legislated fine increase.

See Online Appendix D for more details on state-specific sample restrictions and measurement of outcomes.

4 Empirical Strategy

For our identification strategy, we exploit the discontinuous increase in the fines in each of our natural experiments. Since the fines only applied based on the legislation's effective date, we can classify individuals convicted of targeted offenses before the cutoff date as untreated and those convicted after as treated. For example, an individual convicted of a misdemeanor offense in

[5]We do not have PIK linkages for about 9% of the final sample. This percentage was calculated by comparing the disclosed focal sample size to the CJARS' estimated sample size. Where feasible (e.g., recidivism results), we have confirmed that restricting to the PIKed subsample does not meaningfully alter our findings. See Table F7.

[6]For example, in Wisconsin, we focus on the first misdemeanor charge in Milwaukee County, Wisconsin.

Florida on June 30, 2004 would not face the additional \$65 fee while another individual convicted one day later would. Since the states in our sample follow a similar policy design, we utilize a sharp regression discontinuity which compares the outcomes of individuals disposed right before the fine increase to those disposed right after. We evaluate the assumptions for the validity of the RDD later in this section.

We measure the impact of each specific fine increase using a sharp regression discontinuity design:

$$Y_{i,e} = \beta_0^e + \beta_1^e Post_{i,e} + \beta_2^e RunningDate_{i,e} + \beta_3^e (Post_{i,e} \times RunningDate_{i,e}) + \delta^e X_{i,e} + \varepsilon_{i,e} \qquad (1)$$

where $Y_{i,e}$ is the outcome of interest for individual i in natural experiment e. $Post_{i,e}$ is an indicator variable equal to 1 if individual i's disposition/offense date occurred after fine increase e's effective date; $RunningDate_{i,e}$ is the running variable. $X_{i,e}$ is a vector of covariates included to increase the efficiency of our design. These include age, whether the individual has any prior convictions, race/ethnicity, pre-conviction average income measured using 1040 tax filings 1-3 years prior to the focal event, and sex. β_1^e is the coefficient of interest and captures impact of the fine increase on a given outcome in e.

To evaluate the average impact across the set of natural experiments, we consider the following:

$$\bar{\beta}_1 = \frac{\sum_{e=1}^{9} \beta_1^e}{9} \qquad (2)$$

We estimate this using seemingly unrelated regression, but in practice it is equivalent to a pooled regression with experiment-specific coefficients and fixed effects with inverse probability weights applied to each observation that scale according to the relative sample size (i.e. $\omega^e = 1/\frac{N^e}{\sum_e N^e}$). To generate corresponding scatterplot visualizations to graphically evaluate the combined RD evidence, we apply the following routine:

- Step 1: Group individual observations i from experiment e into 51-day bins to create 10 equally spaced data points on either side of the cutoff
- Step 2: Average outcomes per bin per experiment
- Step 3: Average binned averages across all experiments

In a similar spirit to our regression estimates, this form of graphical evidence will equally weigh each specific experiment. While this approach to integrating across the various fine increases is less statistically efficient, we believe it is a worthwhile trade-off considering that each natural experiment is identifying a distinct treatment effect given the variation in dosage levels previously discussed. Failure to do this would put the most emphasis on the sub-experiment with the largest sample size, which in our case would be Florida with a fine increase of just \$65.

For our main estimates, we use a 510-day bandwidth surrounding the cutoff. To check the robustness of our results, we re-estimate these results using a multitude of specifications: varying bandwidths ranging from 330 days to 690 days in 30 day increments (shown in Figure F1), excluding covariates (shown in Table F3), and using a non-parametric analysis (also shown in Table F3).[7] Our findings are robust across each of these different specifications.

4.1 Identifying Assumptions

In addition to the relevance and monotonicity assumptions, which are satisfied by nature of the sharp fine increases we study, we also require the independence and exclusion restriction assumptions to hold. In our setting, independence concerns the potential sorting of defendants with respect to the implementation threshold. The exclusion restriction relates to whether other potential interventions (e.g., adjusting other criminal sanctions like incarceration) also changed at the implementation threshold.

There are many potential reasons why the independence assumption might not hold. For example, the increased fines could discourage individuals from committing crimes, generating a general deterrent effect and imbalance in the caseload characteristics across the treatment threshold. Similarly, well-resourced individuals might find ways to avoid fines through expediting their scheduled disposition date or hiring better legal representation to avoid a conviction altogether. On the other hand, law enforcement or judicial agents might be more proactive to arrest, charge, or convict more defendants in order to maximize local government revenue.

To test for possible threats to this identifying assumption, we run a battery of tests to assess whether we observe any evidence of systematic sorting across the implementation threshold. These include both evaluating smoothness in daily caseload density as well as caseload characteristics. In two of our sub-experiments (both DRF policies in Texas), we find clear evidence of short-term sorting, wherein higher-income, white defendants were able to reschedule their disposition hearings prior to the fine increase implementation threshold (see evidence and discussion in Online Appendix E). Because of the limited temporal window over which such sorting can take place, the bias can be fully remediated through applying a modest 60-day donut to the two Texas DRF fine increases, which we condition on in all of our of main results.

We present our balance checks in Figure 1, which includes combined RD plots as well as co-

[7] We use the Stata program “rdrobust" (Calonico, Cattaneo, and Titiunik 2014) using a triangular kernel; bandwidth is chosen using the mean-squared-error-optimal bandwidth selectors. We include the same set of covariates used in our main specification.

efficient plots showing the estimated discontinuity for each sub-experiment.[8] Each point is the average within a 51-day bin with the size of the point indicating the number of observations within each bin.[9] We do not find any evidence of systematic sorting, whether evaluated in terms of daily caseload density, individual defendant characteristics, summary indices (predicted recidivism, predicted earnings), or likelihood of identifying a romantic partner at baseline.[10] These conclusions are true on average across the sub-experiments and hold individually within each sub-experiment.[11]

To interpret our discontinuity estimates as the causal effects of criminal fines, we also need to verify that other interventions across the implementation threshold do not also change. Outside of the justice system, specific conviction dates are typically not salient information limiting the scope for potential violations of the exclusion restriction that are non-justice related. Within the justice system, one may worry that prosecutors or judges could adjust other sanction margins to either amplify or weaken the legislated fine increase. We explore this empirically in Figure F4, where we confirm that overall and within each sub-experiment, it does not appear that conviction rates, incarceration sentences, or probation sentences were consistently adjusted in response to the fine increases in ways that would systematically bias our findings.

4.2 First stage relationships

The fine increases we study were applied universally within the subsets of the criminal caseload that form our analysis sample. Such policy changes provide clean variation to study the causal effect of criminal fines.

Figure 2 documents how each sub-experiment varied the amount of total fines assessed at disposition. On average, fines increased by \$492 dollars or 109% on average when pooled across our nine sub-experiments. The increase is quite stark both visually and statistically. Averaging, however, masks significant variation in dosage amount across sub-experiments. At the lowest end, the

[8] We show complete sub-experiment specific RD plots evaluating the balance separately by each of the nine fine increases in Figures F2–F3 in Online Appendix F.

[9] Due to limits in the amount of statistical output we can disclose using data from the U.S. Census Bureau, we do not show regression discontinuity graphs for all of our results.

[10] We generate the summary indices using the following specification: the two-way interactions of sex, race, and age, controls for likelihood of filing a 1040 tax return in the 1-3 years prior to the focal event, and prior criminal recidivism in the 1-3 years prior to the focal event. We predict total recidivism 1–5 years after the focal event and cumulative W-2 income 1–5 years after the cutoff.

[11] Table F1 shows the balance estimates and sample means for the combined sample and each sub-experiment. In the combined caseload density plot, you may notice what appears to be a visual break in the average number of cases per day; this is not statistically significant. While we do observe some minor imbalance on specific traits in the Texas fine consolidation, North Carolina court surcharge, and Michigan non-DUI DRF policy, each of the estimates is quite small given the pre-policy means.

Michigan minimum costs legislation increased total fines by $17, while the Texas DUI-specific DRF policy increased fines by $6,000.

In response to these fine increases, we do not see a corresponding increase in payments, perhaps indicative that increased fines and fees are not affordable within the justice-involved population. If anything, we see evidence that fine payments declined, although this should be taken with caution since we only have the relevant payment information for three out of the nine sub-experiments. Regardless, many fines in our sample go partially or entirely unpaid, further elevating the importance of considering treatment effect heterogeneity based on employed enforcement mechanisms across jurisdictions (e.g., driver's license suspensions, jail time, etc.).

5 Impacts of criminal fines

5.1 Direct impacts to justice-involved individuals

Figure 3 presents summary findings on long-term 10-year outcomes for our focal defendants, drawing on both administrative records (CJARS and IRS W-2 tax filings) and survey data (ACS).[12] On the left side of each panel, we document the pooled RD plot, which averages across the nine sub-experiments, and on the right side we present the discontinuity estimates for each sub-experiment plotted against the effective fine increase associated with a given policy change. To further highlight other dimensions along which there might be potential treatment effect heterogeneity, we note jurisdictions that did utilize driver's license suspensions (dark blue) or did not (light blue) and those that did threaten jail time or further misdemeanor charges (□) or did not (△) as enforcement mechanisms in their caseloads.

We find no statistically significant nor economically meaningful changes in either annual convictions, annual earnings, or average monthly housing costs when pooling across all sub-experiments. Together, we find an average response to fine increases of 0.004 additional crimes per year ($\uparrow$ 1.9%), $89 lower earnings per year ($\downarrow$ 0.6%), and -$6 in housing costs ($\downarrow$ 0.6%), all of which are statistically indistinguishable from zero. Even more so, given our large sample size and strong identifying variation, we have narrow confidence intervals around these findings. With 95% certainty, we can reject changes larger than -0.002 to 0.01 annual convictions, -$347 to $168 in annual earnings, and -$35 to $22 in monthly housing costs. Such null results are robust to alternative functional forms (Table F3) and different bandwidths (Figure F1), reproduced using a range of

[12]We show complete sub-experiment specific RD plots on the impact of the increased fines on annual earnings and number of convictions separately by each of the nine sub-experiments in Figure F5–F6.

follow-up windows from 1 to 10 years (Figure F7), and consistent with a range of other considered administrative and survey outcomes (Tables F4 and F5), including: extensive margins justice involvement outcomes, number of convictions disaggregated by offense type, employment rate, total employers, 1040-reported household earnings, self-reported income, self-reported measures of stress and mental well-being, self-reported mortgages and rent, self-reported commuting by car, and self-reported household size.[13]

We do not observe meaningful heterogeneity across sub-experiments. This is true when evaluating the relationship between measured discontinuities and their associated legislated fine increases, whether the jurisdiction suspended driver's licenses in response to non-payment, or whether the jurisdiction threatens jail time or further misdemeanor charges as an enforcement mechanism. Such heterogeneity would be difficult to observe given that in almost every measured discontinuity in outcomes in each individual sub-experiment we fail to reject the null hypothesis of no impact of the fine increases.

It is possible that the average response at the discontinuity masks important systematic subgroup heterogeneity within each jurisdiction. For instance, those with low earning capacity may be impacted differently from those with higher earning capacity. Figure F8 shows our heterogeneity analyses, which reevaluate the discontinuity for different subsets of the caseload, including by: sex, race, age, criminal history, and predicted income quartiles. We fail to reject the null hypothesis (with narrow confidence intervals) for each considered subgroup when looking at long-term annual earnings outcomes. For two subgroups (those over age 30 years old; lowest quartile of predicted income), we do measure statistically significant point estimates on recidivism: 0.012 convictions per year ($\uparrow$ 5.9%) and 0.007 convictions per year ($\uparrow$ 4.7%). While significant from a statistical standpoint, we believe the magnitude of the results remains quite modest, and well below how some literatures have described the relationship between fines and recidivism.

5.2 Measuring the spillover effects of the increased fines on romantic partners' outcomes and relationship outcomes

While we generally find that the increased fines have small or null effects on labor market and recidivism outcomes of the recipients, these fines may generate social spillovers within the household. For example, a large fine may trigger a change in a romantic partner's labor supply if he or she is the primary earner or in a better position to adjust hours worked; this is a highly possible

[13] Since we cannot link a portion of the individuals in our sample due to the Census PIK process discussed in Section 3, we re-estimate the main recidivism results using the CJARS data which is not restricted by the PIK process. We find similarly null results, shown in Table F7.

hypothesis since research documents that justice-involved individuals have marginal formal labor market attachment (Finlay and Mueller-Smith 2021). Primary driving responsibility may also shift onto the romantic partner due to the license suspension from unpaid criminal debt, exposing them to greater risk of getting charged with a traffic-related offense themselves. To measure these partner spillovers, we use the household crosswalk from Finlay, Mueller-Smith, and Street (2022) that synthesizes information from a variety of data from the Census Bureau, IRS, and other federal programs. This crosswalk allows us to link individuals assigned LFOs to their partners in the year of the focal event.

We show our main results in Figure 4 with corresponding estimates in tabular format shown in Table F8: annual number of convictions, total earnings, number of years observed together, and likelihood the pair are still romantically involved at the end of the follow-up window. Overall, these results echo our findings in Figure 3. Not only are estimates insignificant, but the estimates and standard errors are also relatively small when compared to the pre-policy mean. Largely all of the estimates at the sub-experiment level are also insignificant. Furthermore, we can rule out effects greater than $-273–$242 (-0.002–0.002) in annual earnings (annual number of convictions). We find similarly precisely estimated null estimates for relationship outcomes as well, ruling out effects larger than a -0.7 to 1.7 percentage point change in likelihood of remaining together after 10 years and -0.03 to 0.12 years of relationship duration.

6 Conclusion

This paper measures the impact of nine legislated fine increases spread across five major U.S. jurisdictions (Florida, Michigan, North Carolina, Texas, and Wisconsin). We leverage the abrupt introduction of fine increases assigned upon conviction, with new financial sanctions ranging from $17 to $6,000. Although the sub-experiments employ different enforcement mechanisms and cover distinct jurisdictions, demographic contexts, and time periods, we find consistent evidence of short and long-run null impacts on recidivism, labor market outcomes, and self-reported measures of expenditures, mental/emotional health, and commuting behavior. Similarly, we find no impacts to household composition or spillovers onto romantic partners.

Our results largely echo recent work on the causal impact of criminal legal debt (Pager et al. 2022). We improve on this important research by: (1) examining a broader and more generally representative set of U.S. jurisdictions, (2) extending follow-up periods to consider 10-year long-term outcomes, (3) incorporating labor market outcomes and other survey-based measures of well-being into our analyses, (4) measuring potential household spillovers and changes to family structure, and (5) gathering a sufficiently large sample size to significantly narrow confidence intervals.

While we find no evidence of significant harm being generated from fine increases, we also do not observe any meaningful benefits to justify their use. We do not find evidence of any general or specific deterrence effects of the policies, and from public reports, the increases did not meaningfully generate more revenue either. The failure of these policies to achieve their aims was perhaps already self-evident to policy makers and residents in these jurisdictions; after more than a decade of implementation, several of these policies were eventually discontinued with large scale debt forgiveness programs adopted.

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Figures and Tables

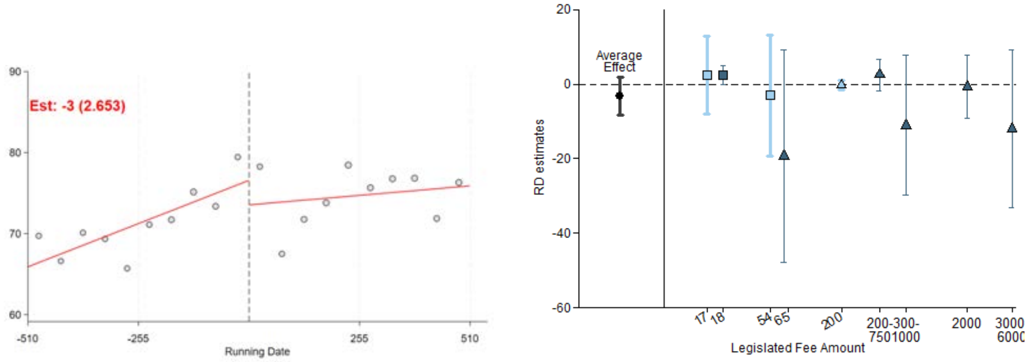
Table 1: Details regarding legislated fine increases and sample restrictions, by sub-experiment

Experiment →	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Panel A: Summary of policy and legislative details									
Offense Grade	Felony charges	Misd. charges	Felony charges	All charges	Misd. charges	Non-DUI traffic offenses	Non-DUI traffic offenses	DUI traffic offenses	DUI traffic offenses
Fine increase	17^*	18^*	$54	$65	$200	$300–$750	$300–$1,000	$2,000	$3,000–$6,000
Statue	HB 4732	HB 2424	HB 200	SB 2962	AB 40	HB 3588	SB509	SB509	HB 3588
Adoption date (m/d/y)	8/13/2003	6/16/2003	6/15/2011	5/8/2004	5/2/2014	6/22/2003	7/15/2003	7/15/2003	6/22/2003
Effective Date (m/d/y)	10/1/2003	1/1/2004	7/1/2011	7/1/2004	1/1/2014	9/1/2003	10/1/2003	10/1/2003	9/1/2003
Enforcement mechanism(s)	Jail for up to 93 days	Jail; Driv lic. susp. (DLS); collect-ions; FTC	Failure to comply (FTC); DLS†; collect-ions	Collect-ions; DLS	Collect-ions	DLS	DLS	DLS	DLS
Late Fees?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Consider ability to pay?	No	No	No	No	No	No	No	No	No
Running variable	Conviction Date	Offense Date	Conviction Date	Conviction Date	Conviction Date	Conviction Date	Conviction Date	Conviction Date	Conviction Date
Panel B: Sample restrictions									
Sample exclusions	No traff. eligible offenses	No traff. eligible offenses	None	None	None	None	None	None	None
Counties included	All	Bexar, Dallas, Hidalgo, Tarrant	All	Duval, Leon, Hillsborough, Miami-Dade, Orange	Milwau-kee only	All	All	All	All
Payment data	Yes	No	No	Yes+	Yes	No	No	No	No

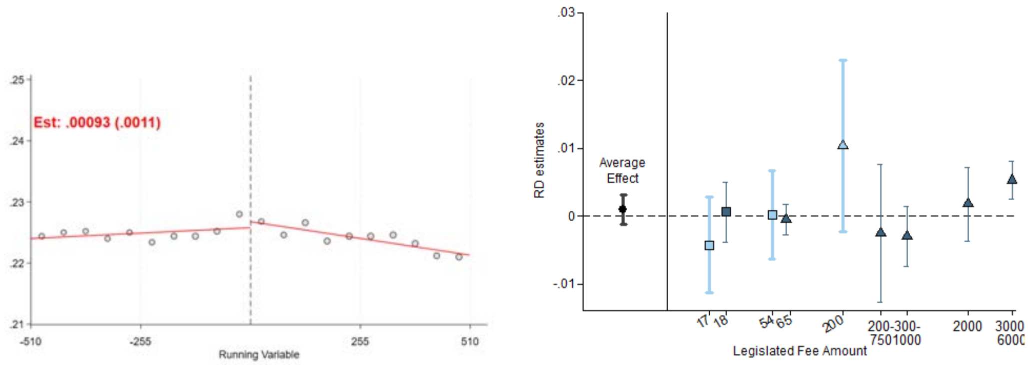
Note: * indicates that the legislative change involved consolidating certain costs; thus the actual increase was lower than the listed amount. For all included charges, we include the first eligible charge for each individual only in the focal sample. + indicates partial payment data (Hillsborough County and Miami-Dade County only). †indicates that DLS was only assigned to traffic offenses. DRF stands for driver responsibility fee. We exclude DRF-eligible offenses in MI minimum costs and TX fine consolidation.

Figure 1: Evaluating potential sample imbalance and sorting at implementation thresholds

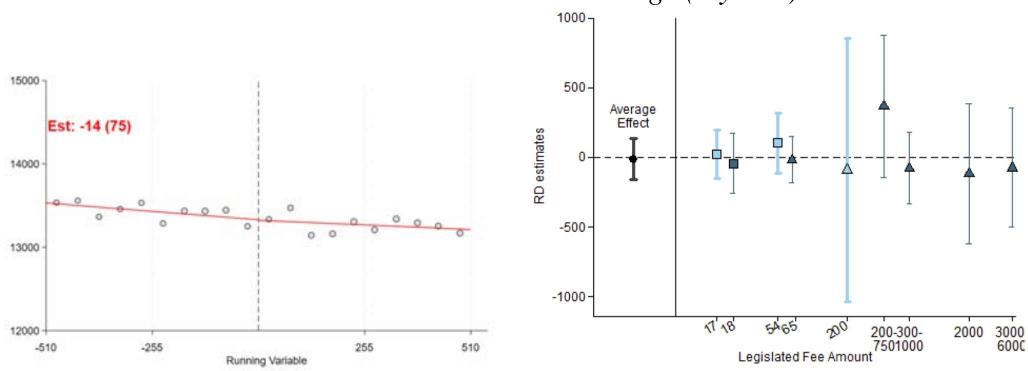
Panel A: Daily caseload density



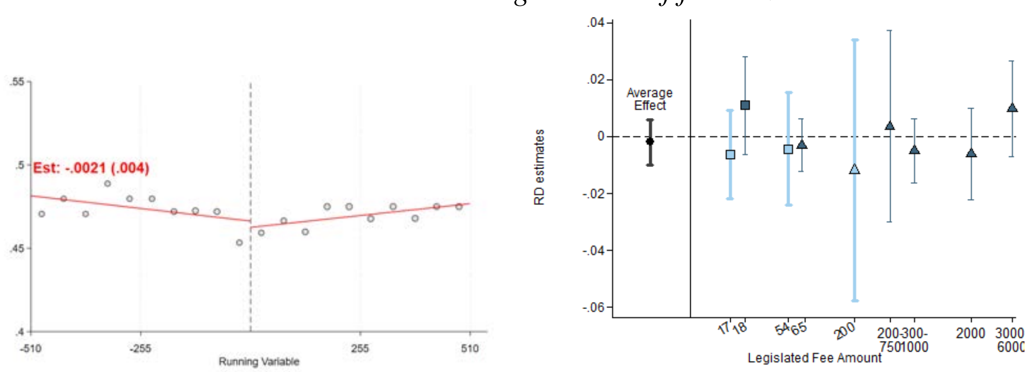
Panel B: Predicted annual recidivism (5 years)



Panel C: Predicted annual earnings (5 years)



Panel D: Romantic linkage at time of focal event



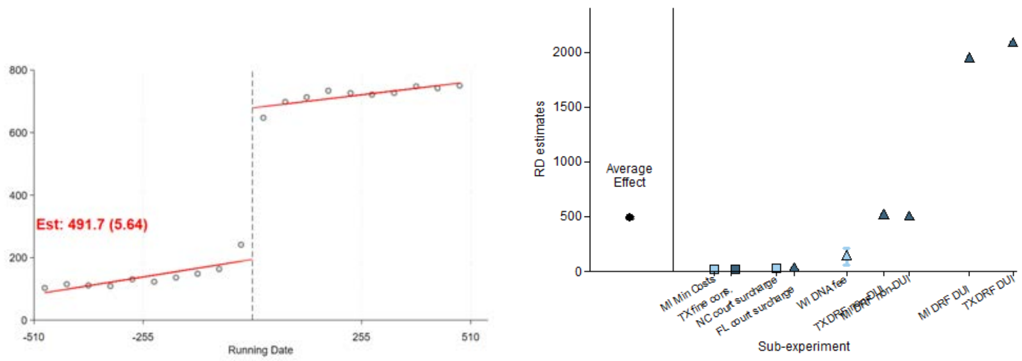
Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.

Note: This figure presents the sharp RDD estimates for the effects of the fine increase on total caseload density (panel A), predicted total recidivism 1–5 years after the focal event (panel B), and predicted total earnings 1–5 years after the cutoff measured using W-2 tax returns (panel C, adjusted to 2017 dollars using the CPI-All Urban). See Section 4.1 for description of the creation of predicted indices. See Table F1 for results in tabular format.

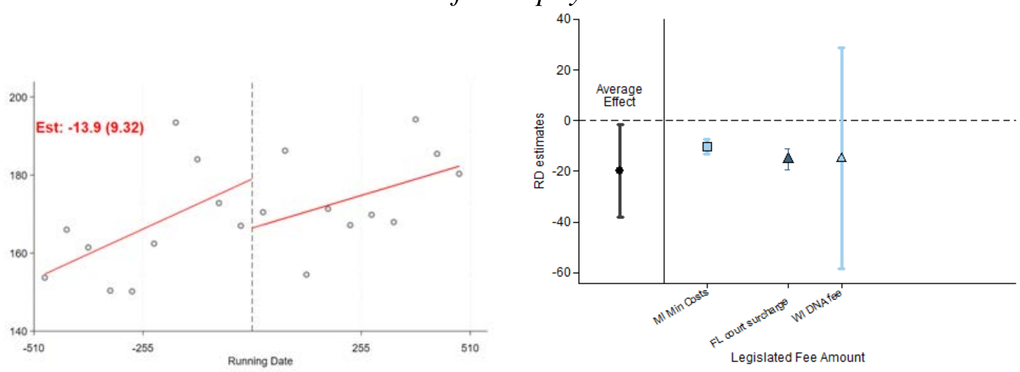
RD Figure Notes: Scatter points are binned using 51-day windows with the size of the circle denoting the number of observations within each bin. The black, dashed vertical line denotes the cutoff. Predicted fit lines are generated using a sharp, linear RDD where event date is the running variable. Sharp RDD estimated fit lines are in solid pattern and red color. Left-hand side graphs provide visual depiction of the RD for the pooled sample. Right-hand side graphs display the RD estimate for each of the sub-experiments. Dark blue (light blue) indicates jurisdictions that did (did not) utilize driver's license suspensions; □ (△) did (did not) threaten jail time or further misdemeanor charges as enforcement mechanisms in their caseloads. RD specification choices are described in Section 4.1. The estimation sample for each sub-experiment is described in Online Appendix D and Table 1.

Figure 2: Change in assessed fines and total payments at implementation thresholds

Panel A: Total legal financial obligations assessed at disposition



Panel B: Total future payments to date



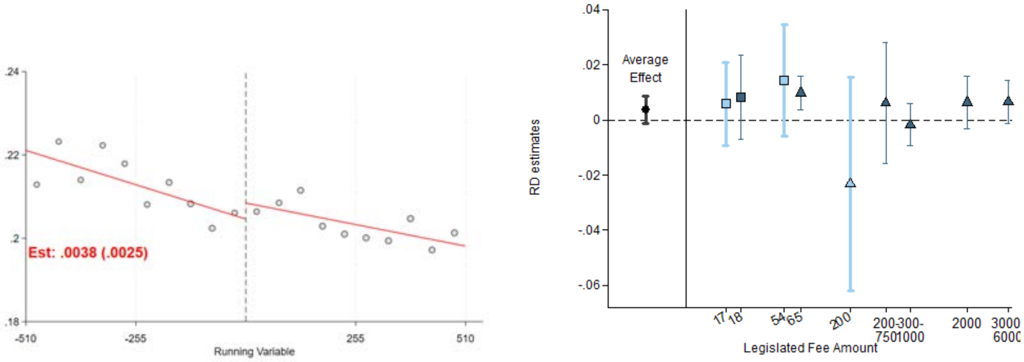
Source: Authors' calculations using the criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin in the CJARS 2022Q4 vintage.

Note: These figures show the visual representation of the sharp RDD estimates (solid, red) of the impact of the increased sanctions on total sanctions assigned at disposition and on total paid to date. See Table F2 for the results in tabular form.

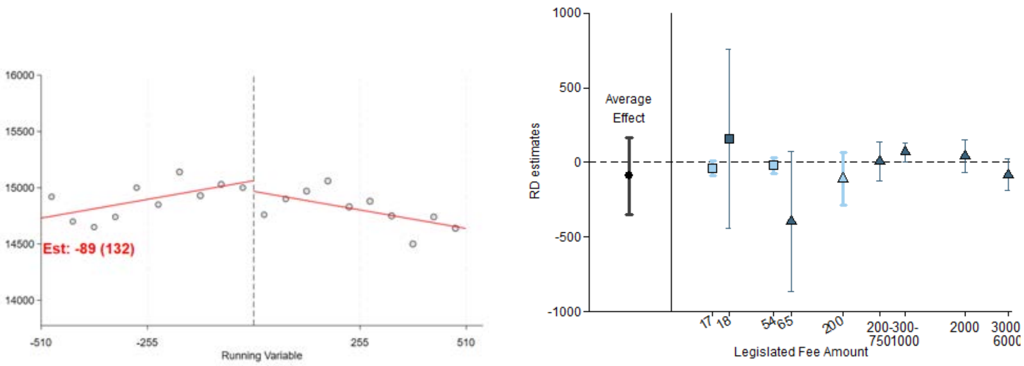
RD Figure Notes: Scatter points are binned using 51-day windows with the size of the circle denoting the number of observations within each bin. The black, dashed vertical line denotes the cutoff. Predicted fit lines are generated using a sharp, linear RDD where event date is the running variable. Sharp RDD estimated fit lines are in solid pattern and red color. Left-hand side graphs provide visual depiction of the RD for the pooled sample. Right-hand side graphs display the RD estimate for each of the sub-experiments. Dark blue (light blue) indicates jurisdictions that did (did not) utilize driver's license suspensions; □ (△) did (did not) threaten jail time or further misdemeanor charges as enforcement mechanisms in their caseloads. RD specification choices are described in Section 4.1. The estimation sample for each sub-experiment is described in Online Appendix D and Table 1.

Figure 3: Impact of increased fines on long-run convictions, earnings, and monthly housing costs

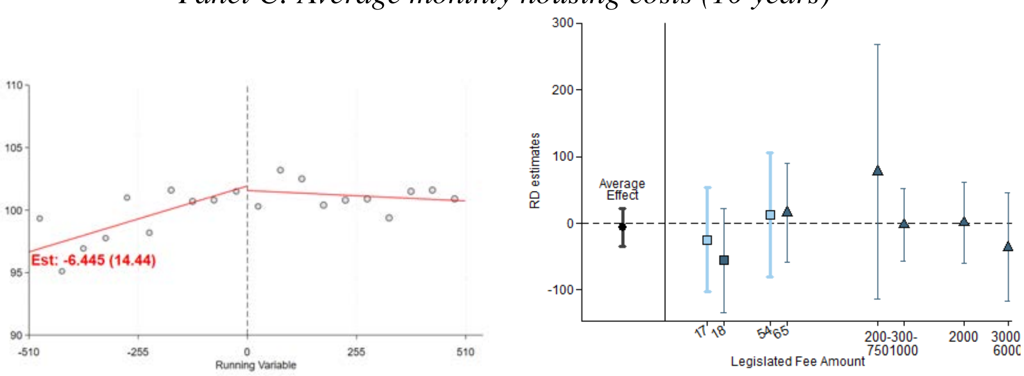
Panel A: Annual number of convictions (10 years)



Panel B: Annual earnings (10 years)



Panel C: Average monthly housing costs (10 years)



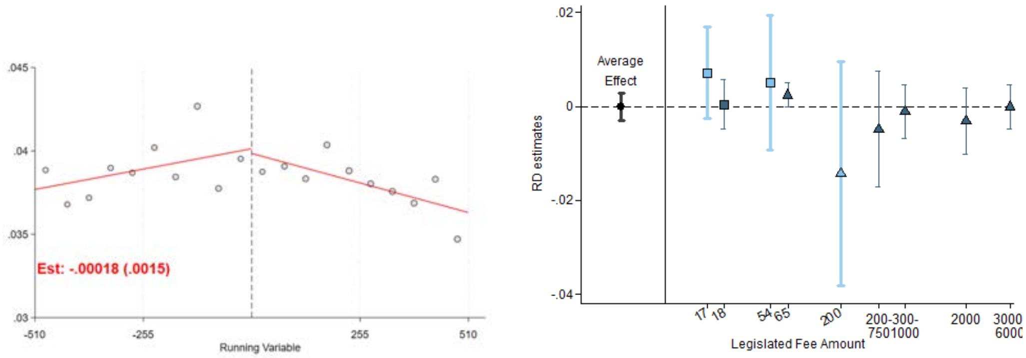
Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.

Note: This figure presents the sharp RDD estimates for the effects of the fine increase on annual number of convictions 1–10 years after the focal event (panel A), total annual earnings 1–10 years after the cutoff measured using W-2 tax returns (panel B, adjusted to 2017 dollars using the CPI-All Urban), and average monthly housing costs reported in the ACS in the 1–10 years following the focal event date. See Table F4 for results in tabular format.

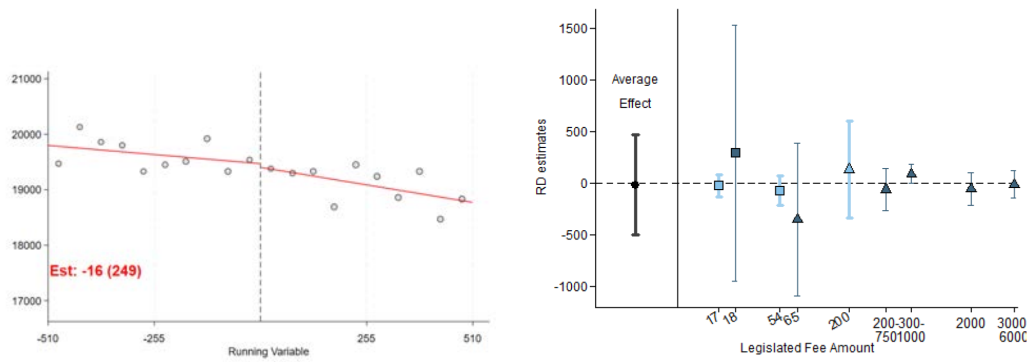
RD Figure Notes: Scatter points are binned using 51-day windows with the size of the circle denoting the number of observations within each bin. The black, dashed vertical line denotes the cutoff. Predicted fit lines are generated using a sharp, linear RDD where event date is the running variable. Sharp RDD estimated fit lines are in solid pattern and red color. Left-hand side graphs provide visual depiction of the RD for the pooled sample. Right-hand side graphs display the RD estimate for each of the sub-experiments. Dark blue (light blue) indicates jurisdictions that did (did not) utilize driver's license suspensions; □ (△) did (did not) threaten jail time or further misdemeanor charges as enforcement mechanisms in their caseloads. RD specification choices are described in Section 4.1. The estimation sample for each sub-experiment is described in Online Appendix D and Table 1.

Figure 4: Impact of increased fines on long-run convictions, earnings, and monthly housing costs for romantic partners

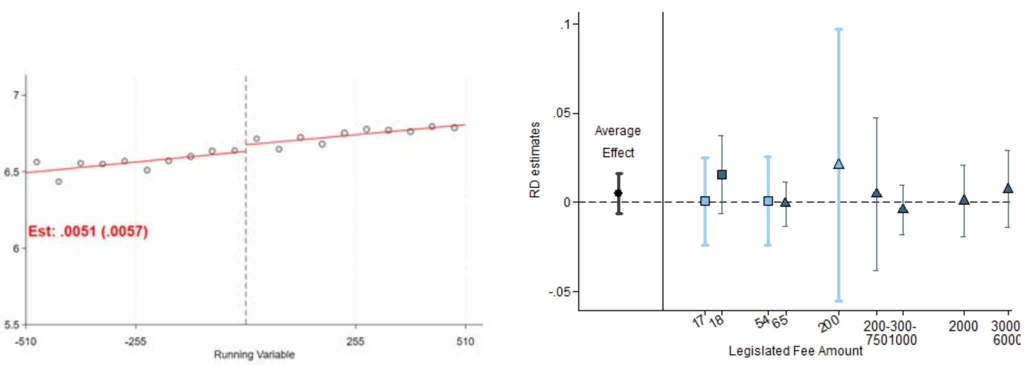
Panel A: Partner's annual number of convictions (10 years)



Panel B: Partner's annual earnings (10 years)



Panel C: Number of years observed romantically linked (10 years)



Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.

Note: This figure presents the sharp RDD estimates for the effects of the fine increase on the labor and recidivism outcomes of the individual' romantic partner at the time of disposition. Romantic partners and relationship outcomes are identified and measured using the crosswalk developed by Finlay, Mueller-Smith, and Street (2022). Outcomes of interest are annual number of convictions 1–10 years after the focal event (panel A), total annual earnings 1–10 years after the cutoff measured using W-2 tax returns (panel B, adjusted to 2017 dollars using the CPI-All Urban), and total number of years observed together in the 10 years following the cutoff. See Section 4.1 for description of the creation of predicted indices. See Table F1 for results in tabular format.

RD Figure Notes: Scatter points are binned using 51-day windows with the size of the circle denoting the number of observations within each bin. The black, dashed vertical line denotes the cutoff. Predicted fit lines are generated using a sharp, linear RDD where event date is the running variable. Sharp RDD estimated fit lines are in solid pattern and red color. Left-hand side graphs provide visual depiction of the RD for the pooled sample. Right-hand side graphs display the RD estimate for each of the sub-experiments. Dark blue (light blue) indicates jurisdictions that did (did not) utilize driver's license suspensions; □ (△) did (did not) threaten jail time or further misdemeanor charges as enforcement mechanisms in their caseloads. RD specification choices are described in Section 4.1. The estimation sample for each sub-experiment is described in Online Appendix D and Table 1.

Online Appendix A Comparison of study findings with Giles (2021)

One of the nine natural experiments we consider in this paper has been previously studied in the literature. Giles (2021) examines the discontinuous introduction on January 1, 2014 of the $200 DNA fee universally applied to criminal court cases in Milwaukee County, Wisconsin. This paper concludes that the expansion of criminal fees through this policy increased recidivism in the affected caseload.

Working with similar data extracts from the state of Wisconsin, we do replicate the findings of Giles (2021), but ultimately come to different conclusions regarding the recidivism impact of the policy. We attribute this divergence in opinions to two main issues described below.

Sample restrictions. The first discrepancy comes down to a matter of sample restrictions. In our analysis, we focus on the subset of defendants who are facing their first misdemeanor charge in Milwaukee, WI. In contrast, Giles (2021) includes all misdemeanor convictions.

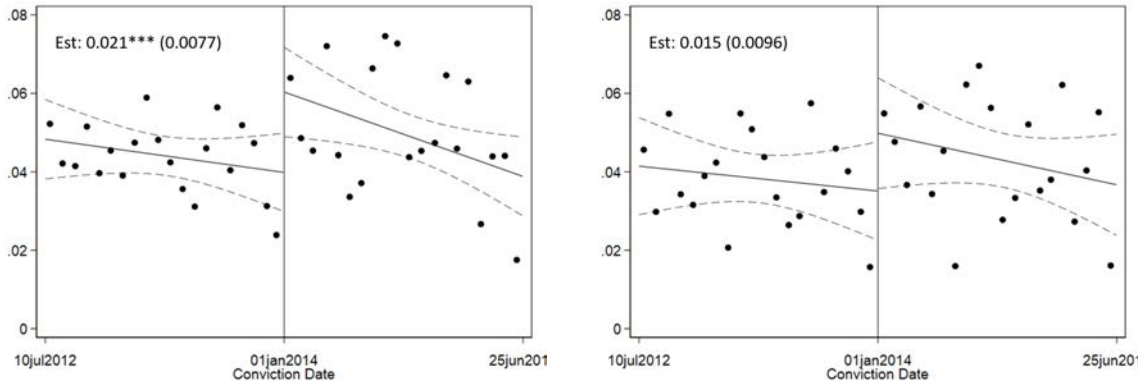
There are good motivations for including all defendants in the regression. In particular, the external validity of the findings for the entire caseload (including those who already hold a criminal history) is strengthened when incorporating the full caseload. At the same time, this choice potentially includes a degree of bias since individuals potentially endogenously show up in the analysis sample multiple times. In fact, the finding of an immediate short-run recidivism impact by definition means that the post-period in the Giles analysis should disproportionately include a higher share of repeat offenders since the found impact itself defines future inclusion in the research sample. This is what we observe in the data. Prior to the discontinuity, the ratio of convictions to unique individuals is 1.04:1; after the discontinuity, the ratio increases to 1.14:1.

In Figure A1, we plot two graphs showing the the 1 year recidivism outcomes, defined as likelihood of new felony conviction, for misdemeanor defendants in Milwaukee, WI.[14] In the first graph, we replicate Giles (2021)'s findings through including all misdemeanor convictions in the running variable, and in the second panel, we replicate our original results (restricting to just convictions rather than all charges). We also disaggregate the number of bins compared to Giles (2021) to better reflect the underlying raw data.

The results in the first panel show strong visual evidence of a discontinuity, with a corresponding

[14] We choose to replicate results on felony recidivism as this is one of Giles (2021) main results.

Figure A1: Comparing differences in one-year felony recidivism findings based on differing sample inclusion criteria



i) All misdemeanor convictions, (Replication of Giles (2021)) *ii) First misdemeanor convictions only*

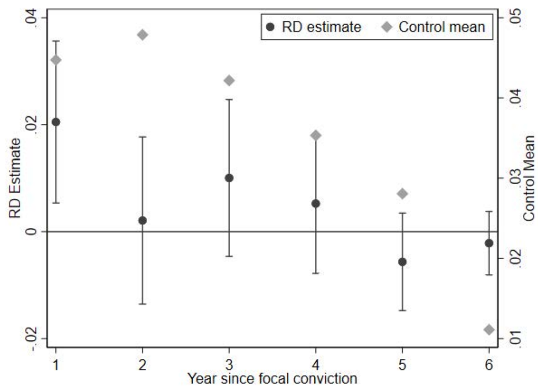
Source: Authors' calculations using criminal justice histories from Wisconsin in the CJARS 2022Q2 vintage.
Note: This figure presents the sharp RDD estimates for the effects of the $200 DNA fee surcharge enactment in Milwaukee County, Wisconsin. Panel (i) consists of all misdemeanor convictions (ii) is restricted to first convictions, which is our preferred specification. RD point estimates with standard errors in parentheses are included in the top left of each graph.
RD specification choices are described in Section 4.

point estimate of the jump at the discontinuity that is highly statistically significant (p-value $<$ 0.01). The second panel is much less certain. The point estimate shrinks from 0.021 to 0.015, loses statistical significance for traditional threshold levels, and from a visual perspective becomes less certain. Overall, the data series shows ebbs and flows over the running variable; without the inclusion of the vertical line to identify the policy implementation data, it is not clear the naked eye could identify when the policy change occurred based on this figure alone.

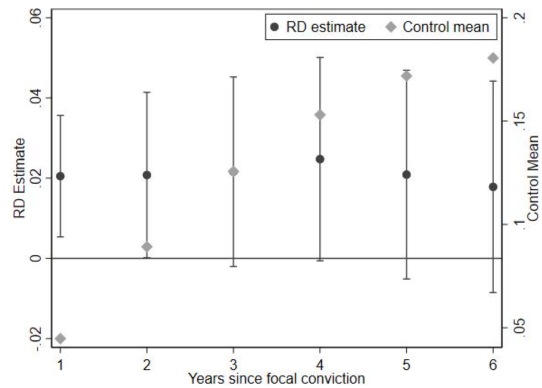
Follow-up period. Many individuals cycle through the justice system. Consequently, it would be reasonable for a researcher to argue that the tradeoff between external and internal validity is warranted in this context. Even for those taking this perspective, we believe that caution should taken with concluding that the introduction of the DNA fee in Wisconsin increased recidivism for misdemeanor defendants.

In Figure A2, we plot how the contemporaneous and cumulative impacts on recidivism evolve over different follow-up periods using the original (broader) sample inclusion criteria proposed in Giles (2021). We include impacts from 1 to 6 years, which exhausts potential follow-up available in current data. From this evidence, we can see that the recidivism impacts are acutely concentrated in just the first year. From year 2 onwards, there appears to be no impact on contemporaneous recidivism. As a product, cumulative effects become weaker as more follow-up years are included in the analysis, losing statistical precision and failing to meet traditional thresholds of statistical significance after 2 years. The point estimates do remain stable, however, relative to the control mean, the proportional effect contracts over time.

Figure A2: Impacts over time on recidivism using Giles (2021) sample including criteria



i) Contemporaneous likelihood of new felony conviction



ii) Cumulative likelihood of felony conviction

Source: Authors' calculations using criminal justice histories from Wisconsin in the CJARS 2022Q2 vintage.
Note: This figure presents the sharp RDD estimates for the effects of the $200 DNA fee surcharge enactment in Milwaukee County, Wisconsin. Panel (i) and (ii) show the RD point estimates with 95% confidence intervals.
RD specification choices are described in Section 4.

For the reasons laid out above, we conclude that the introduction of the $200 DNA fee in Wisconsin had minimal to no impact on the recidivism trajectories of misdemeanor defendants. While this conclusion depends somewhat on subjective decisions in the research process, we do believe that we have executed our study in a manner that minimizes potential contamination bias. And, when viewed in conjunction with the range of other natural experiments we study that also show evidence of precise null effects and other socio-economic outcomes that also show precise null findings, we think it is unlikely that the DNA fee increased felony recidivism.

Online Appendix B Institutional details regarding fine increases

B.1 Florida Court Surcharge

In 1998, Florida amended its state constitution so that the state would pay a greater share of trial court funding by July 1, 2004 (Blankenship 2004). In order to fulfill the mandate, Governor, Jeb Bush signed Senate Bill 2962 into law on May 21, 2004, which allowed counties to impose $65 fees upon conviction. Florida's largest counties (Broward County, Duval County, Miami-Dade County, Escambia County, Hillsborough County, Leon County, and Orange County) immediately amended their local ordinances to implement the new fees.[15] These fines would apply to anyone convicted after June 30, 2004.

B.2 Michigan Driver Responsibility Fees

The governor of Michigan signed the Driver Responsibility Program, or Public Act 165, into law on August 11, 2003, with an effective date of October 1, 2003. In Michigan, the DRF would be enforced by the Michigan State Treasurer as its revenue would be directed toward the state's General Fund. The fee amount was determined using three distinct tiers of driving violations, where the lowest level defendants were forced to pay a $150 or $200 dollar fee for two consecutive years, the middle level defendants were forced to pay a $500 dollar fee for two consecutive years, and the highest level defendants were forced to pay a $1,000 fee for two consecutive years (Wild 2008). For the purpose of our analyses, the lowest and middle tier are combined and referred to as our "non-DUI" sample, and the highest tier is the "DUI" sample. Failure to pay the fees after 60 days led to the suspension of one's driver's license. All outstanding DRFs along with any associated fees had to be paid in order to reinstate a license.

Over 137,000 drivers in Michigan were assessed a DRF for driving with a suspended license in 2007, an increase of 44% compared to 2005 (Wild 2008), indicating that many individuals may have fallen into a self-perpetuating cycle of legal debt. By the time that the law was repealed in 2018, an estimated 317,000 drivers had had their driver's licenses suspended for failure to pay DRFs (Carrasco 2018). In the year before repeal, Michigan, ranked 10th in population in the U.S., was ranked the 4th highest state for number of suspended licenses (Salas and Ciolfi 2017).

[15] Other counties also increased their fines immediately but are not included due to either lack of historical data, insufficient court assessment data, or we were unable to locate the specific county ordinance showing the immediate adoption of these fines.

The initial collection rate, from 2003 to 2009, of 48% was lower than the state's 60% projections (Wild 2008). Alcohol-related driving crimes increased by 21% after the bill went into effect, which many interpreted as evidence that the deterrent aims of the policy had failed to materialize (Johnson 2009). In 2018, the state of Michigan repealed the DRF legislation and canceled all remaining debt owed under the law. At the time of nullification, the state forgave approximately $630 million in outstanding driver responsibility payments (Carrasco 2018).

B.3 Michigan Minimum Cost

In 2003, Michigan faced a $1 billion deficit in the 2004 budget along with the highest unemployment rate in the nation (Holahan et al. 2004). In order to raise revenue, the state passed an array of bills focused on generating or diverting revenue from the criminal justice system to other branches of the government.[16] One of these bills, House Bill 4732 established a minimum cost upon conviction, ranging from $40–$45 for misdemeanor convictions to $60 for felony convictions. The minimum costs, however, absorbed other pre-existing court costs so that the marginal increase was less than $60 for those charged with a felony.

Michigan also passed Public Act 165 or the Driver Responsibility Fee (DRF), which applied only to traffic offenses.[17] We exclude all traffic offenses from this sub-experiment to prevent the confounding of our results from the DRF passage.

B.4 North Carolina Court Surcharge

In 2011, North Carolina, like many other states, was facing a budget shortfall in the midst of the Great Recession. In response to the anticipated decline in state revenue, the North Carolina General Assembly passed House Bill 200, a massive bill that overhauled the state government budget. Included in this bill was an increase in court costs from $95.90 ($102.50) to $129.50 ($154.50) for individuals convicted of a misdemeanor (felony) (Sutton 2019). The new costs would go into effect on July 1, 2011, affecting all individuals convicted on or after that date.

At the same time, the state also passed the Justice Reinvestment Act (JRA), which made major changes in community supervision and probation revocation. Most of the provisions of the law

[16]The simultaneity of the passage of multiple bills does not confound our identification strategy as the other bills focused on juvenile fines and fees (not the focus of this paper) or budget re-allocation.

[17]See Section Online Appendix C for details of this policy and estimates of the causal impact of DRF programs in Michigan and Texas.

went into effect on December 1, 2011, five months after the court cost increase went into effect (Oglesby-Neal et al. 2021) and impacted probation spells for a subset of felony offenses. Thus, the timing of the JRA passage should not undermine our empirical strategy.

B.5 Texas Driver Responsibility Fees

In an effort to promote safer driving and increase state revenue, Texas passed House Bill 3588, or the Texas Driver Responsibility Program, on June 2, 2003. The law, which became effective on September 1, 2003, mandated new fines to defendants who were convicted of certain driving crimes. The Texas Department of Public Safety (DPS), which oversees Texas' Highway patrol, would enforce the fines and receive 1% of revenue (Price 2008). The remaining revenue was evenly split between the state's trauma system and the Texas Mobility Fund.[18] At the time, Texas' trauma system was seriously underfunded and overstretched with only 15.83 emergency departments and 8.14 trauma centers per one million people (Price 2008). Similar to other states' version of the DRF, the fines would be classified as administrative fines, rather than criminal penalties.

Texas' version of the DRFs had four tiers of sanctions that would be applied over three consecutive years (Price 2008; Adair 2013). The first two tiers cover non-DUI offenses, including driving with an expired/invalid license and driving on a suspended/revoked license, with fees ranging from \$100–\$250 between per year. The second two tiers cover DUI offenses, with distinctions made for first-time and repeat offenders and fine levels ranging from \$1,000–\$2,000 per year.

Failure to pay the fees after 30 days led to the suspension of one's driver's license. All outstanding DRFs along with any associated fees had to be paid in order to reinstate a license.

Policymakers were concerned that the high monetary burden of DRFs would disproportionately impact those with low income. This concern was borne out in the years following the enactment of the DRF. For instance, the state saw a significant jump in the number of drivers with suspended licenses in the years following the implementation of the DRF. By 2013, the DPS estimated that over 1.3 million Texas drivers had invalid driver's licenses due to unpaid DRF charges. Furthermore, most of the surcharges did not originate from DUI-related cases, the intended target of the bill (Adair 2013). In 2017, Texas was ranked first in the nation with 1.8 million suspended licenses (Salas and Ciolfi 2017).

The DRF was also criticized for failing to meet the planned collection rate or to improve driver

[18] Texas Mobility Fund authorizes grants and loans of money and issuance of obligations for financing the construction, reconstruction, acquisition, operation, and expansion of state highways, turnpikes, toll roads, toll bridges, and other mobility projects.

safety. Texas only collected 40% of assessed surcharges by 2012, which was significantly lower than the state's projection of 66%. In the same time period, the percentage of traffic fatalities involving alcohol also increased from 27% to 34% (Adair 2013).

In 2019, Texas repealed its DRF law. At the time of repeal, out of the 1.6 million Texan drivers with suspended licenses, 630,000 were qualified to get their licenses immediately reinstated. The Texas Fair Defense Project estimated that total debt waived due to the repeal was close to $2.5 billion.

B.6 Texas Fine Consolidation

Prior to 2003, Texas District and County Courts set their own court costs. The passage of House Bill 2424 in 2003 required that all courts consolidate their court costs to a single, uniform court cost of $83 for misdemeanor conviction. Thus, rather than a multitude of county-defined court costs (e.g., compensation of victims fee, special services state court cost, judicial education, etc.) the costs would consolidate to a single cost of $83. Depending on the county's prior cost structure, the bill could either increase or decrease the court cost faced by those who offended after the effective date of January 1, 2004. Unlike the other states in our sample, Texas' fine determination was based on offense date, rather than conviction date.

Because the effects of the cost consolidation were heterogeneous across counties depending on the pre-existing cost structure, we focus our analysis on six out of the eight most populous counties in Texas (Bexar, Dallas, Harris, Hidalgo, Tarrant, and Travis). These counties represent approximately 33% of the total Texas population. The inclusion of counties is restricted to confirm existing cost structures, which are not publicly archived, as well as our ability to measure fines and fees in criminal charge data in CJARS.[19]

Similar to Michigan, Texas also passed its version of the DRF in the fall of 2003, which assigned fines if drivers exceeded a threshold of traffic infractions or upon conviction of certain criminal traffic offenses. To prevent the DRFs from confounding the impacts of the fines of interest, we also drop any criminal traffic related offenses.

[19]While we reached out to each of the County Clerk's office to know the exact court costs and fines used in 2002 and 2003, we only received information from Harris and Travis County. Both Harris and Travis County had already adopted the consolidated fines and fees, which we confirmed in the data. Financial sanctions data is reported county by county and varies by data collection procedures at the county level.

B.7 Wisconsin DNA Surcharge

In the summer of 2013, then Wisconsin Governor Scott Walker proposed expanded DNA collection for individuals convicted of any crime as part of his 2014 state budget proposal. Prior to his proposal, DNA collection was only taken from people convicted of felonies with a $250 surcharge along with select misdemeanors. Anticipating the increase in DNA testing demand for state labs, the proposal also included a new $200 surcharge for all misdemeanor convictions that would go into effect at the same time. Specifically, individuals convicted of misdemeanor offenses after January 1, 2014 now faced an additional surcharge of $200 to help fund the expanded DNA testing.[20]

[20] The mandatory DNA collection would not begin until April 1, 2015 (Marley 2013). The delay in actual DNA collection until after the fee enactment was controversial. When legally contested, the court of appeals declared the DNA surcharge collection without DNA collection as unconstitutional (Marley 2013). Despite the ruling, the state did not refund the surcharge; in response, some charged with the surcharge without the DNA collection filed a federal lawsuit in 2018 (Richmond 2018).

Online Appendix C Driver responsibility programs in the United States

In the early 2000s, many states were facing high rates of DUI fatalities and budget shortfalls. Thus, in an effort to solve both of these issues, states such as Michigan, New York, Texas, and Virginia passed driver responsibility fee programs modeled after New Jersey's 1983 Merit Rating Plan Surcharges (Price 2008; Wild 2008; Adair 2013). These programs assigned sizable financial penalties to drivers that either exceeded a threshold of traffic infractions or were convicted of certain traffic offenses. By 2008, over 44 million drivers, or 21% of all licensed drivers, in the United States were at risk of receiving a DRF penalty (*Highway Statistics Series* 2008).[21]

Each state's surcharge program followed the same broad structure: a point system for traffic infractions as well as surcharges for specific violations ranging from severe traffic infractions such as driving without a driver's license to more serious criminal traffic misdemeanors and felonies such as driving under the influence or driving with a suspended or revoked license. These fees ranged from $25 for every point to significantly higher amounts such as $6,000 for a DUI conviction (Wild 2008; Price 2008). If a driver was unable to pay the DRFs, then the state would suspend his or her license until all outstanding fines were repaid. In all versions of the program, driving with a suspended license was itself a DRF triggering offense, thereby placing lower income drivers at higher risk of accruing multiple DRFs and substantial legal debt. This particular aspect of the DRF policy was criticized for its potentially disparate impact on lower income drivers (Hausman 2013; Henson 2009; Carnegie 2006).

In general, driver's license suspension is a commonly utilized form of punishment in the criminal justice system in the United States. Driver's license suspension can also be triggered by drug conviction, failure to comply with a court order, failure to pay civil infractions such as traffic tickets,[22] failing to maintain auto insurance, and failure to pay child support. The high use of driver's license suspension is not unusual when compared to states that did not adopt the DRF program. According to a 2017 report by the Legal Aid Justice Center, 43 states suspend driver's licenses due to unpaid court debt with suspension only lifted upon payment; 18 out of the 43, including Michigan and Texas, suspend licenses automatically after the payment deadline (Salas and Ciolfi 2017). Similar to Michigan and Texas, most states do not require considering ability to pay prior to driver's license suspension. The Fines and Fees Justice Center estimates 11 million individuals in the United States have their license suspended due to unpaid court debt (Keneally 2019).

[21] Virginia, the last state to pass its version of the DRF program, enacted its program in 2008.

[22] Due to a change in law in Texas in October 2021, driver's license suspension was lifted if they were issued for failure to pay tickets/court fines or failing to appear for some violations

Online Appendix D Data Appendix

D.1 State specific data restrictions

For a subset of the states, the treatment varied by county due to the wording of law or because the fines had already been implemented prior to the effect date. When possible, we rely on news articles or county ordinance adoption to confirm the counties that were impacted by the law change. Thus, in Florida, we only include Duval County, Hillsborough County, Leon County, Miami-Dade County, and Orange County, which all passed county ordinances immediately after the state law was passed and along with sufficient CJARS data.

In Wisconsin, we focus on Milwaukee County because we have sufficient historical adjudication data and because the DNA surcharge was treated as a mandate (see Giles (2021)). Milwaukee county is the largest county in the state at close to 1 million in population with the next largest county (Dane) having only half a million.

As discussed in detail in Section 2, we focus our analysis on Bexar County, Dallas County, Hidalgo County, and Tarrant County as treated counties. These four counties represent approximately 33% of the state population.

D.2 Measuring assessed sanctions

We define total assessed fines as the total fines and costs assigned upon conviction. There are some exceptions to this. Due to differences in data availability, we only show the court costs in North Carolina as we do not have data on other sanctions assigned. Wisconsin's sanctions are significantly higher as the data on total sanctions assigned includes later assigned debts and restitution. Since our focal sample is at the charge level, individuals who are disposed but not convicted are not assigned any financial sanctions. Thus, the first stage estimates will be lower than the actual increase, which only applies upon guilty convictions.

D.3 Outcome measurement

We measure recidivism by identifying future convictions using the criminal record data from CJARS, which includes states and counties beyond the ones included in our focal sample (see Finlay and Mueller-Smith (2022) for full geographic, temporal, and procedural coverage of CJARS).

We define recidivism using the time length between the focal disposition and future offense date or filing date.[23] Thus, if an individual was disposed of their first charge on June 1, 2003, re-offended on May 1, 2005, and was convicted for that new offense on July 2, 2005, we would consider that as recidivism within 2 years of the focal event.[24] We also break out recidivism measures based on offense type and offense grade using the offense classification system developed by Choi et al. (2023). This helps further unify our analysis since the offense types, such as property, violent, are defined under a harmonized system.

To measure labor market outcomes such as earnings and employment, we use the IRS W-2 information returns from 2005–2020.[25] Measuring income using W-2s is advantageous since they cover all formal employment, regardless of the length of employment spell. Thus, the number of W-2 tax returns filed per year on behalf of the individual can be a measure for the number of jobs an individual worked. Furthermore, W-2s are filed by the employer, not the employee, and so are not affected by endogenous tax filing behavior such as in IRS 1040 individual tax returns. Because of this, we use W-2 information returns as our main measure of annual earnings even though we have 1040 tax filings beginning in 1998. We do include 1040 tax filings as a measure of household earnings.

There are some limitations to using administrative tax returns as the measure of employment. First, we are limited to formal employment, and will not observe informal work or work done as a contractor. Second, since tax returns are filed on an annual basis, we measure the W-2 tax returns relative to the cutoff date rather than measuring it relative to the focal event. Since we do not have W-2 tax returns prior to 2005, we will not be able to observe employment outcomes in the year following the cutoff for individuals in our Michigan sample. Lastly, as noted by past research, the criminal justice-involved population is weakly attached to the labor market and may not have a labor market response to the financial sanctions.

To alleviate the first issue of informal employment, we also use self-reported total income on the 1-year American Community Surveys from 2005–2020. This total income measure encompasses income earned from wages, self-employment, Social Security Income, and others.[26] There are some drawbacks to using the ACS. First, although the ACS allows us to measure other outcomes (e.g., informal income, commuting method), the likelihood of of housing unit selection to the survey is low; for example, in 2018, the ACS sampled approximately 3.5 million addresses (U.S.

[23]When offense date is unknown, we use filing date.

[24]The exception is Texas, where we define the focal event based on the original offense date. This is because the legislation implementation was based on offense date.

[25]We do not have W-2 data before 2005.

[26]See the 2021 ACS (U.S. Census Bureau 2021a) for the full list of income measures collected in the survey.

Census Bureau 2021b). Thus, the sample size for our ACS regressions is significantly smaller than in our main specification. Second, the ACS does not provides population weights specific to the criminal justice involved population.

In order to circumvent this, we employ the following re-weighting strategy. Since we treat the CJARS data as the canonical sample of interest while the ACS is a random sample, we reweight the data so that key moments of the ACS respondent population match those of the CJARS sample. These weights are generated from predicting likelihood of being in the ACS data using the same set of covariates to generate our balance predicted indices. Using these weights, we show that the total weight does not discontinuously change across the outcome; in other words, the survey population density is constant across the discontinuity. This balance across the cutoff is crucial for interpreting our estimates using the survey responses as causal impacts of the fine increases.

Additionally, the ACS data are repeated cross sections rather than individual panel data since individuals are selected randomly each year. Thus, we do not cumulatively measure the ACS outcomes; instead, the estimates are treated as averages over the 10 year follow-up period. For individuals who are surveyed multiple times, we treat each response as a new individual. Since the ACS records the interview date, we measure the outcomes relative to the focal event.

We also use the ACS survey responses to measure outcomes aside from formal employment and recidivism, such as individual well-being and expenditures. Specifically, we use the ACS to measure monthly housing costs, which includes, gas, electricity, water, rent, mortgage, housing association fees, and others, self-reported difficulty making decisions, concentrating, or remembering due to having a mental, physical, or emotional condition lasting more than 6 months, likelihood of commuting by car, and number of adults in the household. We include the last two outcomes since the increased monetary burden from the fines may reduce access to cars (e.g., driver's license suspension due to fine non-payment) or lead to higher rates of cohabitating with other adults to reduce housing costs. Income measures are inflated to 2017 dollars using the Consumer Price Index for All Urban Consumers (CPI-All Urban). All together, we believe that the ACS responses provide a more holistic picture of the impact of fines on individual outcomes.

Online Appendix E Manipulation of conviction date with respect to the Texas Driver Responsibility Fee program

As discussed in the main text, there are a multitude of reasons why, and strategies for how, individuals might manipulate the functioning of the criminal justice system to benefit themselves. Using the specific example of this study, changing case characteristics such as conviction date or the specific offense that one is convicted of could be the difference between owing no DRFs and owing up to $6,000 in additional fines and fees upon conviction. The ability to act on these mechanisms, however, might vary based on preexisting characteristics in the population. For instance, income and wealth might afford better legal representation or having specific demographic traits (e.g., age, race, sex) might engender more or less sympathy from law enforcement, prosecutors, and judges. While a large literature exists examining potential discrimination and inequities in policing, this represents an additional dimension along which societal inequities might be manifested and amplified.

In Texas, there appears to be a significant degree of short-run manipulation of the running variable. As seen in Figure E1 panel B, we observe a spike in the average number of DUI cases disposed to the left of the cutoff reaching almost 10,000 cases per 60-day window, twice the regular caseload, and a corresponding drop immediately to the right of the cutoff.

What subgroups of the population are able to take advantage of this manipulation, and how do they accomplish this? Figure E2 panels A–D documents the change in caseload composition for individuals in Texas over the analysis sample, with the manipulated data points highlighted in red. The bunched set of individuals just to the left of the cutoff (i.e. those engaging in manipulation to avoid DRF penalties) are more likely to be White (panel A) and with higher earnings profile (panel B). These individuals were also less likely to have a prior conviction record (panel C) or be male (panel D).

How did this group achieve this manipulation and why is it time limited? While we cannot pinpoint the exact mechanism, the evidence here highlights two things. First, the law was implemented based on the date of conviction. Thus individuals with scheduled disposition dates right after the cutoff could conceivably avoid the DRFs by shifting their disposition dates earlier. Leading up to the cutoff, the average time to disposition was roughly 240 days, giving ample room for adjustment in order to get ones case disposed prior to the implementation date. We can see this in Figure E3 panel A. Average adjudication duration in the month prior to the cutoff dropped to 200 days, a roughly 16% reduction in average caseload time. After the cutoff, time to disposition is slightly elevated (which makes sense given that those with the fastest potential cases shifted to the left of the cutoff) and returns to the preexisting level and trend.

A second piece of evidence on this matter regards whether defendants were able to secure non-DRF convictions for DRF-eligible offenses. For example, an individual charged with a DUI could negotiate their conviction down to a lesser offense that was not DRF-eligible (e.g. public intoxication). In Figure E3 panel B shows the share of the DRF-related caseload that ultimately are convicted of non-DRF-related offenses. Immediately prior to the cutoff, there is a drop in the rate of non-DRF-related convictions; this reflects the bunching of dispositions for DRF cases prior to the elevated fees going into effect. In this period, there is no additional incentive for manipulating conviction offense associated with the DRF program. Immediately following the cutoff, the likelihood of a non-DRF conviction is elevated and remains slightly higher than preexisting levels. While this does not create sorting bias in the research design (since we include the entire caseload of DRF-related offenses in the analysis sample), it does provide evidence that a narrow slice of the population (about 2-3 percentage points) is able to avoid the DRF penalty in the steady state of the program.

We do not know how this population achieved changes to adjudication duration and/or final conviction offense. It could be the product of proactive behavior by charged individuals and their defense attorneys. It could also be the product of discretionary decisions taken by prosecutors or judges. Achieving a better understanding of these dynamics is an area for future research.

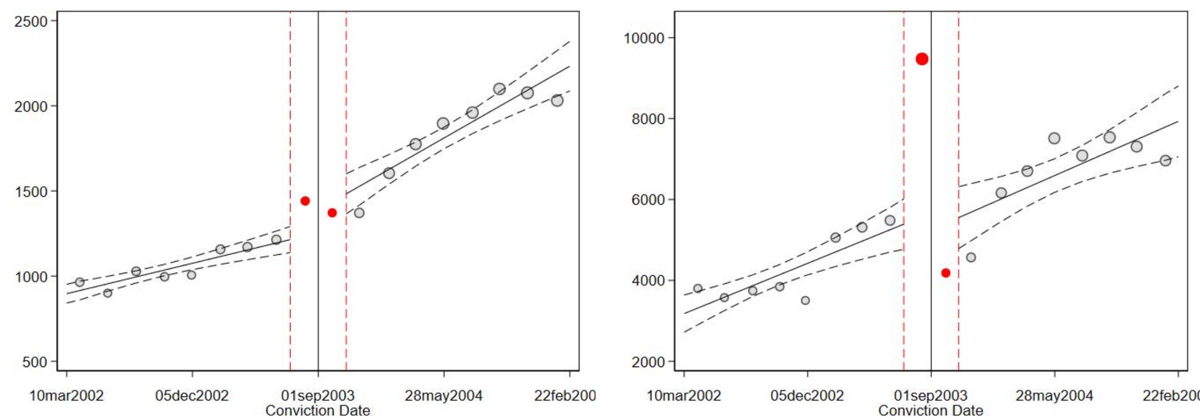
In either case, these figures imply disparate treatment in race and gender in the justice system, an interpretation supported by Doleac (2017), Abrams, Bertrand, and Mullainathan (2012), Arnold, Dobbie, and Yang (2018), Alesina and La Ferrara (2014), Arnold, Dobbie, and Yang (2018), and Depew, Eren, and Moran (2017) and that individuals with greater access to financial resources were most able to avoid the DRFs.

Interestingly, we do not observe the same type of behavior in Michigan, as shown in Figure F2 panels H and I. While this is not causal evidence, one institutional difference between Michigan and Texas is the administration of the DRFs. Specifically, in Texas, DPS oversaw the program and received a portion of the revenue, suggesting incentives to over-charge driver's with DRF-eligible offenses (Makowsky, Stratmann, and Tabarrok 2019; Price 2008). DPS could further increase their revenue by charging the driver's least able to contest these charges, a hypothesis supported by Makowsky and Stratmann (2009, 2011). We observe modest evidence of this with increasing caseload density in Figure E1 panels A and B. We do not observe a similar rise in Michigan.

Another factor that might be going on is that the penalty amounts in Texas were significantly higher than in Michigan (see Figure 2). So, even if agency behavior was similar across our two natural experiments, added incentive for individuals to pursue DRF avoidance may have generated the sorting in Texas that is not present in Michigan.

Figure E1: DRF-related caseload densities by DUI status, by conviction date relative to effective date of Texas House Bill 3588 (September 1, 2003)

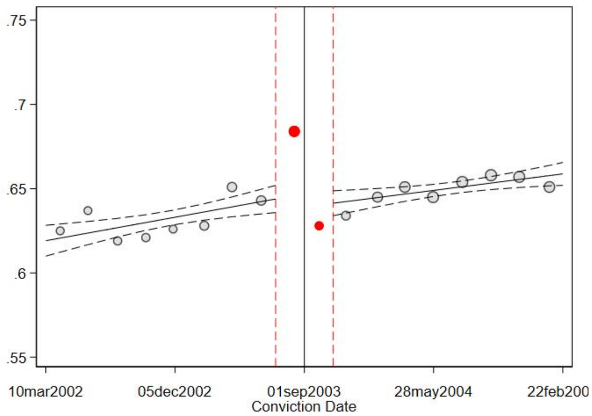
Panel A: Average 60-day non-DUI caseload *Panel B: Average 60-day DUI caseload*



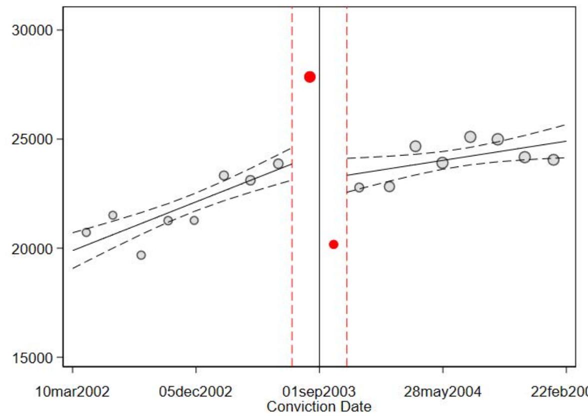
Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.
Note: These figures show the visual representation of the sharp RDD estimates (solid, black) and 95% confidence intervals (black, dashed) of the effect of the DRFs on the average DRF-eligible convictions within a 60-day window for non-DUI DRF related offenses (panel A) and the average DRF-eligible convictions within a 60-day window for DUI DRF related offenses (panel B) in Texas.
RD Figure Notes: Scatter points are binned using 60-day windows with the size of the circle denoting the number of observations within each bin. The black, solid vertical line denotes the cutoff. The red, dashed, vertical line denotes the donut (60-day window surrounding the cutoff; Texas only). Predicted fit lines are generated using a sharp, linear RDD where conviction date is the running variable. Red data points (Texas only) reflect excluded observations within the donut and are provided for completeness even though they do not contribute to RD estimates.

Figure E2: Summary characteristics, by conviction date relative to effective date of Texas House Bill 3588 (September 1, 2003)

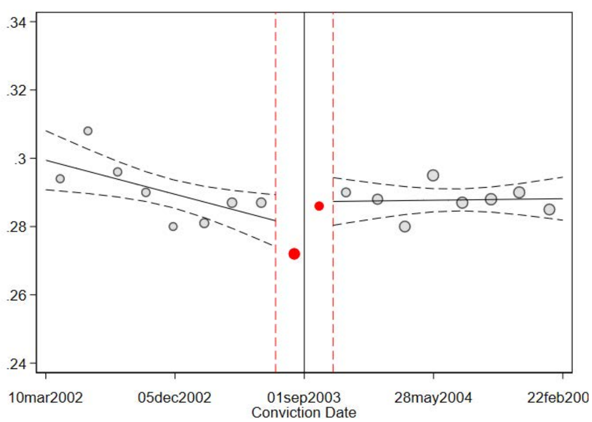
Panel A: Proportion White



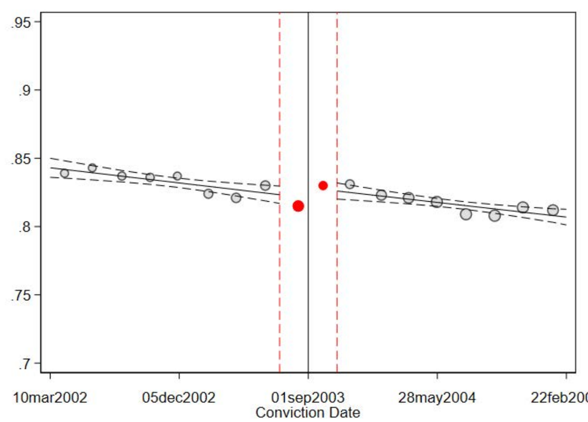
Panel B: Pre-disposition average 1040 household income



Panel C: Any prior convictions



Panel D: Proportion Male

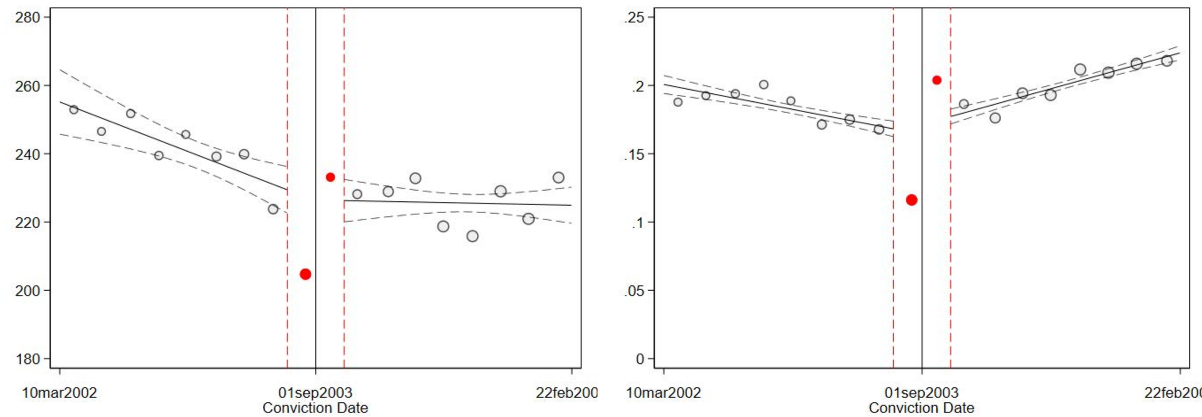


Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: These figures show the visual representation of the sharp RDD estimates (solid, black) and 95% confidence intervals (black, dashed) of proportion of subgroup characteristics denoted in the panel title in the focal sample. These subgroup characteristics are: White, male, having any prior convictions, and pre-conviction average 1040 filings. Racial identity is measuring using the Census' 'bestrace' file. Sex is measured using the Census Numident file. Any prior convictions is defined as having at least one conviction 1–3 years prior to the focal DRF-conviction. Pre-conviction average 1040 household income is measured using 1040 tax filings 1–3 years prior to the focal conviction. RD notes from Figure E1 apply.

Figure E3: Evidence of DRF avoidance behavior in Texas in response to House Bill 3588 (September 1, 2003)

Panel A: Days between offense date and disposition date *Panel B: Likelihood of non-DRF eligible traffic charge*

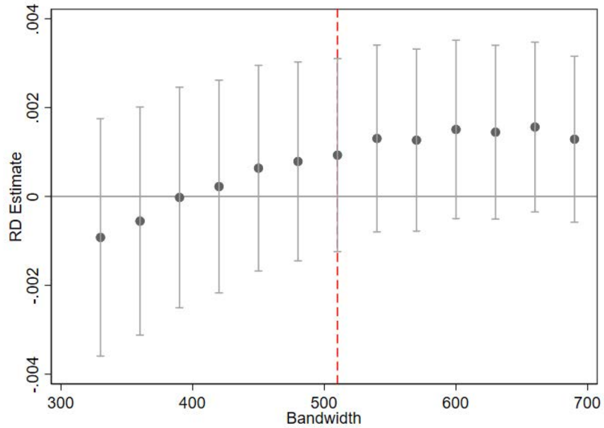


Source: Authors' calculations from Texas criminal justice histories from the CJARS 2020Q3 vintage.
Note: These figures show the visual representation of the sharp RDD estimates (solid, black) and 95% confidence intervals (black, dashed) of the effect of the DRFs on the number of days between offense date and disposition date and the likelihood of being convicted for a non-DRF eligible, but DRF-related charge. Outcome variables were residualized using data available in the CJARS data.
RD notes from Figure E1 apply.

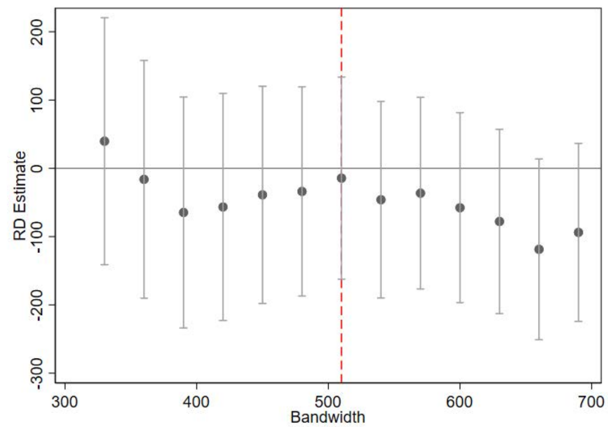
Online Appendix F Supplementary Results

Figure F1: Robustness of balance in predicted indices and main results to varied bandwidths

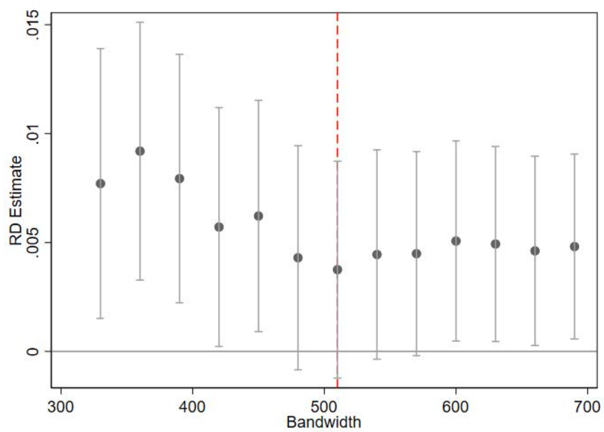
Panel A: Predicted annual number of convictions



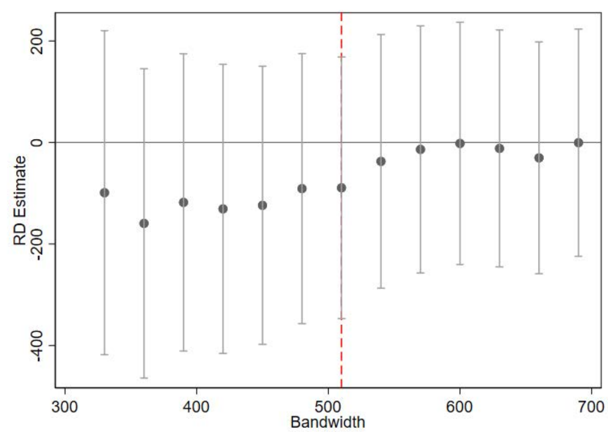
Panel B: Predicted annual earnings



Panel C: Annual number of convictions, 10 years

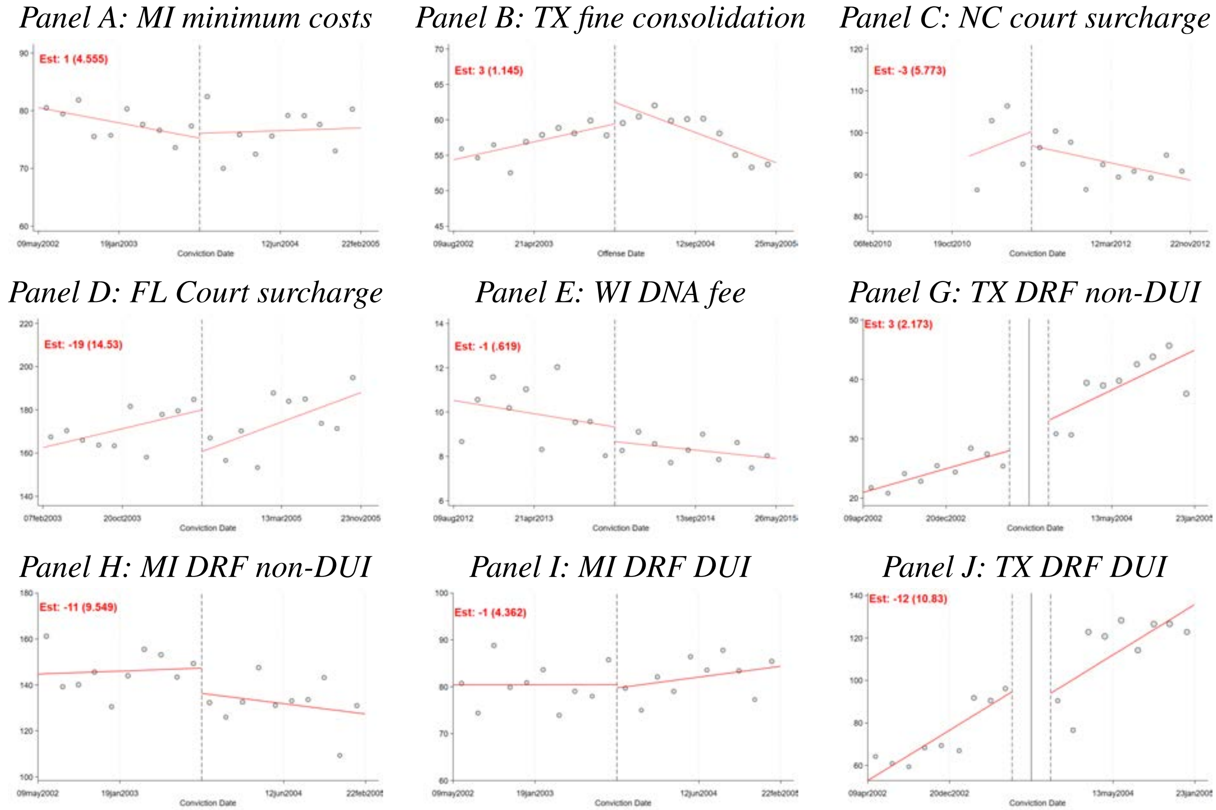


Panel D: Annual earnings, 10 years



Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.
Note: This figure plots the sharp RDD estimates measuring the effects of fine increases on predicted recidivism and predicted earnings (panels A and B) and actual total recidivism and total earnings (panels C and D) for varying bandwidths (x-axis) ranging from 330 to 690 days by 30 day intervals. Total earnings is measured using income reported on W-2 tax returns and adjusted to 2017 dollars using the CPI-All Urban.
RD specification choices are described in Section 4.1. The estimation sample for each sub-experiment is described in Online Appendix D and Table 1.

Figure F2: Robustness of balance in average daily caseload density by sub-experiment

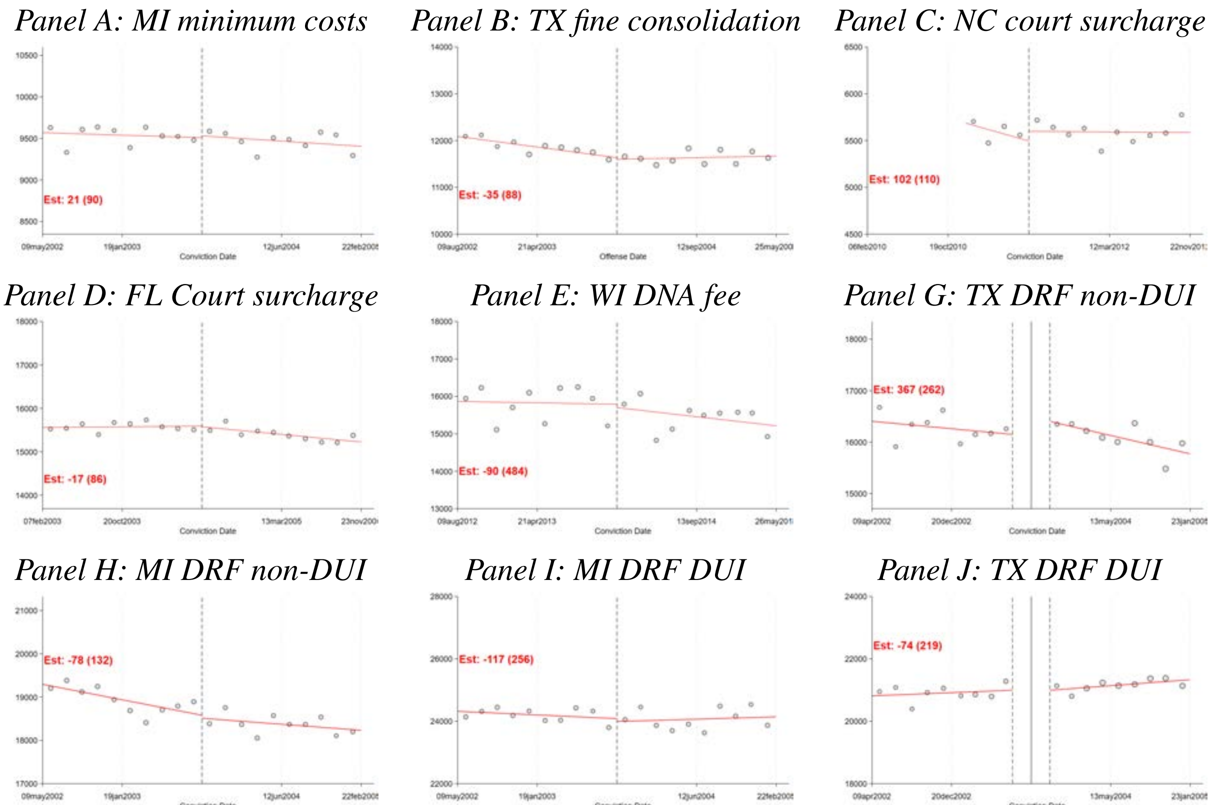


Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.

Note: This figure plots the sharp RDD estimates measuring the effects of fine increases on average daily caseload density by sub-experiment (denoted in the panel title).

RD specification choices are described in Section 4.1. The estimation sample for each sub-experiment is described in Online Appendix D and Table 1.

Figure F3: Robustness of balance in predicted earnings by sub-experiment



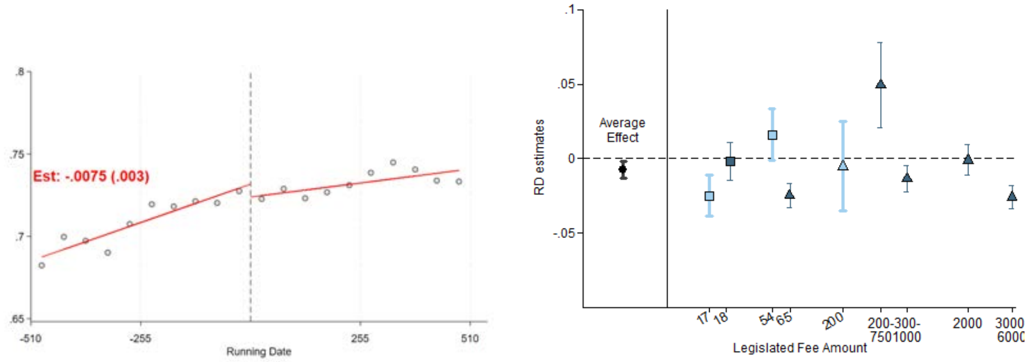
Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.

Note: This figure plots the sharp RDD estimates measuring the effects of fine increases on predicted annual earnings in the 1–5 years following the cutoff by sub-experiment (denoted in the panel title). Earnings are measured using income reported on W-2 tax returns and adjusted to 2017 dollars using the CPI-All Urban.

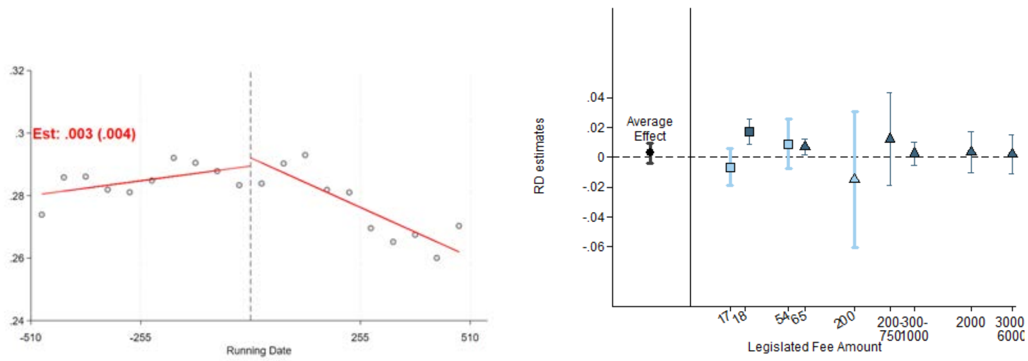
RD specification choices are described in Section 4.1. The estimation sample for each sub-experiment is described in Online Appendix D and Table 1.

Figure F4: Impact of increased fines on other criminal sanctions

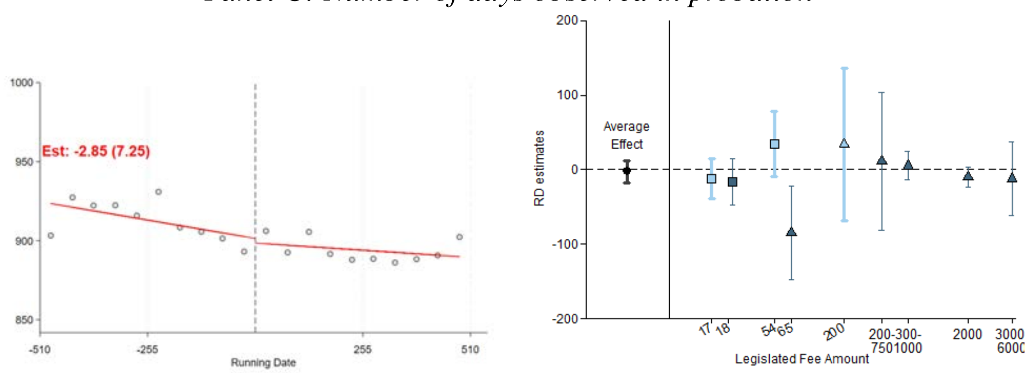
Panel A: Likelihood of conviction



Panel B: Likelihood of being sentenced to incarceration



Panel C: Number of days observed in probation

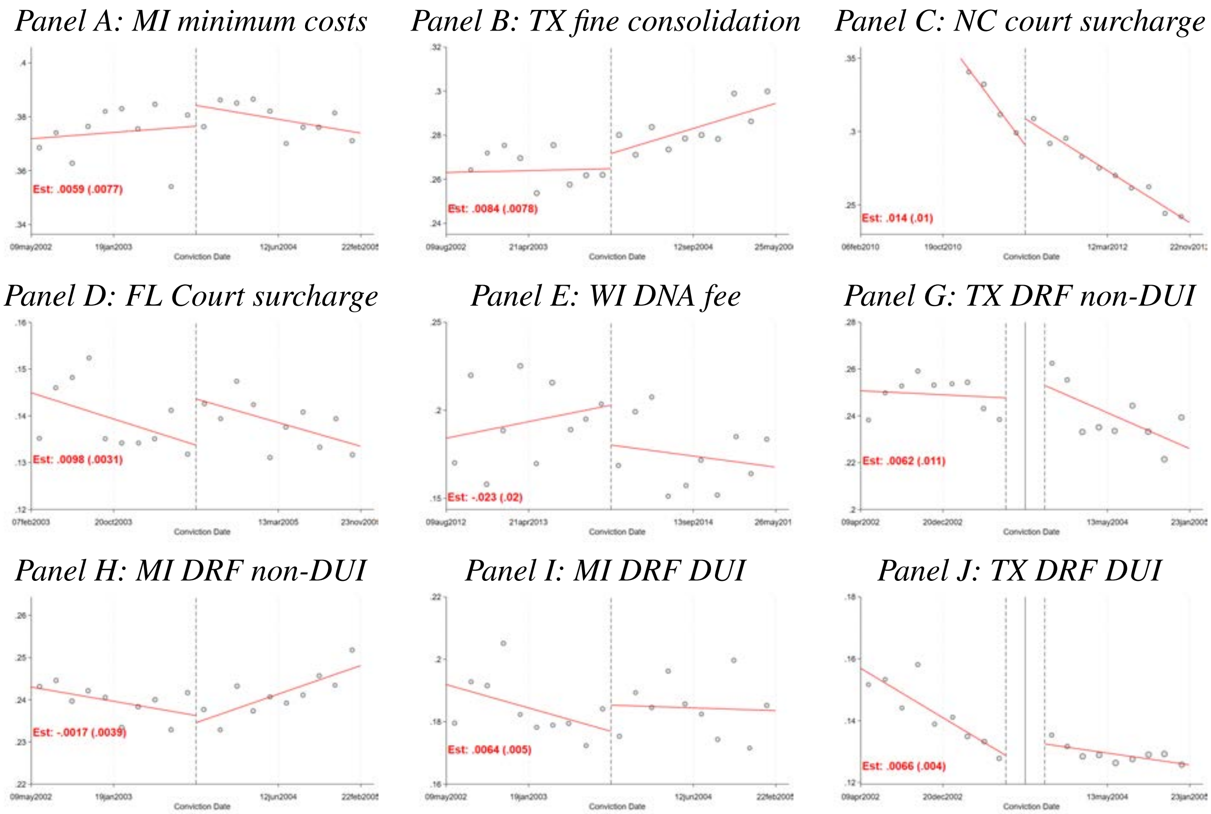


Source: Authors' calculations using the criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin in the CJARS 2022Q4 vintage.

Note: This figure presents the sharp RDD estimates for the effects of the fine increase on likelihood of conviction (panel A), likelihood of being sentenced to any incarceration (panel B), and number of days assigned to probation (panel C).

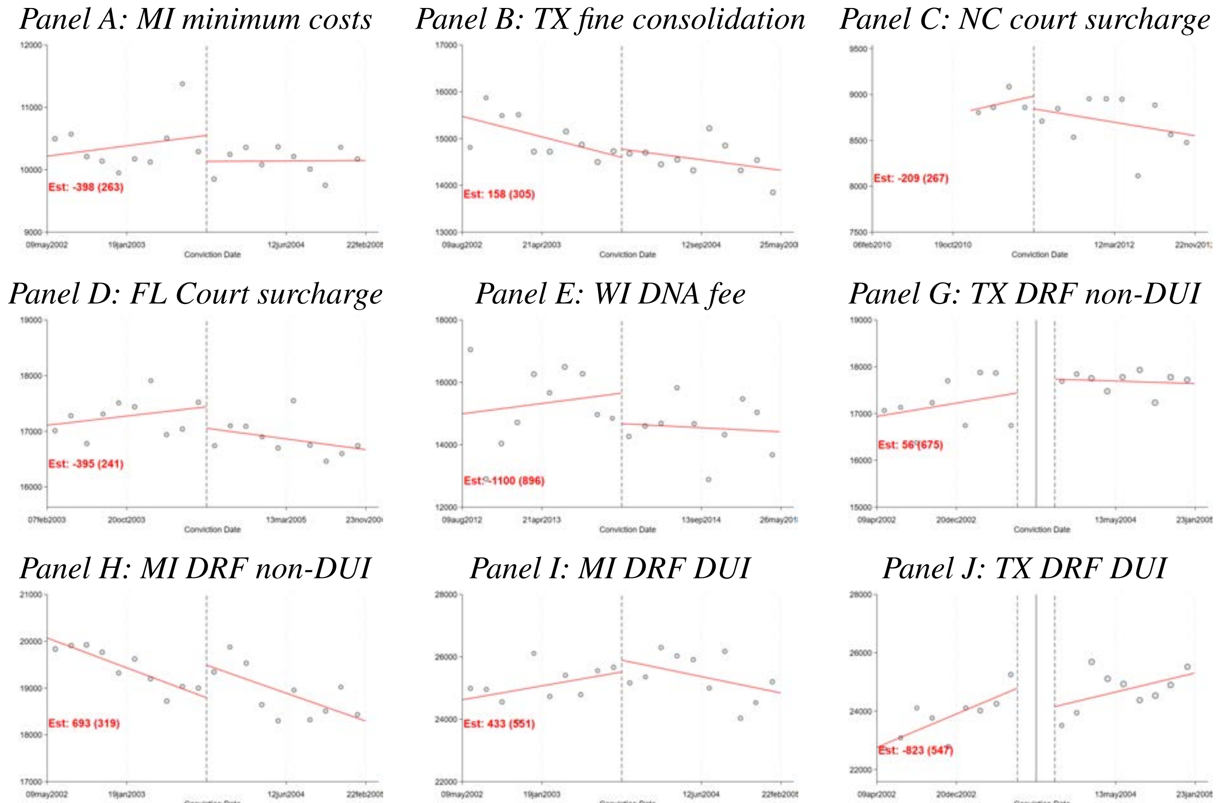
RD Figure Notes: Scatter points are binned using 51-day windows with the size of the circle denoting the number of observations within each bin. The black, dashed vertical line denotes the cutoff. Predicted fit lines are generated using a sharp, linear RDD where event date is the running variable. Sharp RDD estimated fit lines are in solid pattern and red color. Left-hand side graphs provide visual depiction of the RD for the pooled sample. Right-hand side graphs display the RD estimate for each of the sub-experiments. Dark blue (light blue) indicates jurisdictions that did (did not) utilize driver's license suspensions; □ (△) did (did not) threaten jail time or further misdemeanor charges as enforcement mechanisms in their caseloads. RD specification choices are described in Section 4.1. The estimation sample for each sub-experiment is described in Online Appendix D and Table 1.

Figure F5: Impact of increased fines on annual number of convictions by sub-experiment 10 years after the cutoff



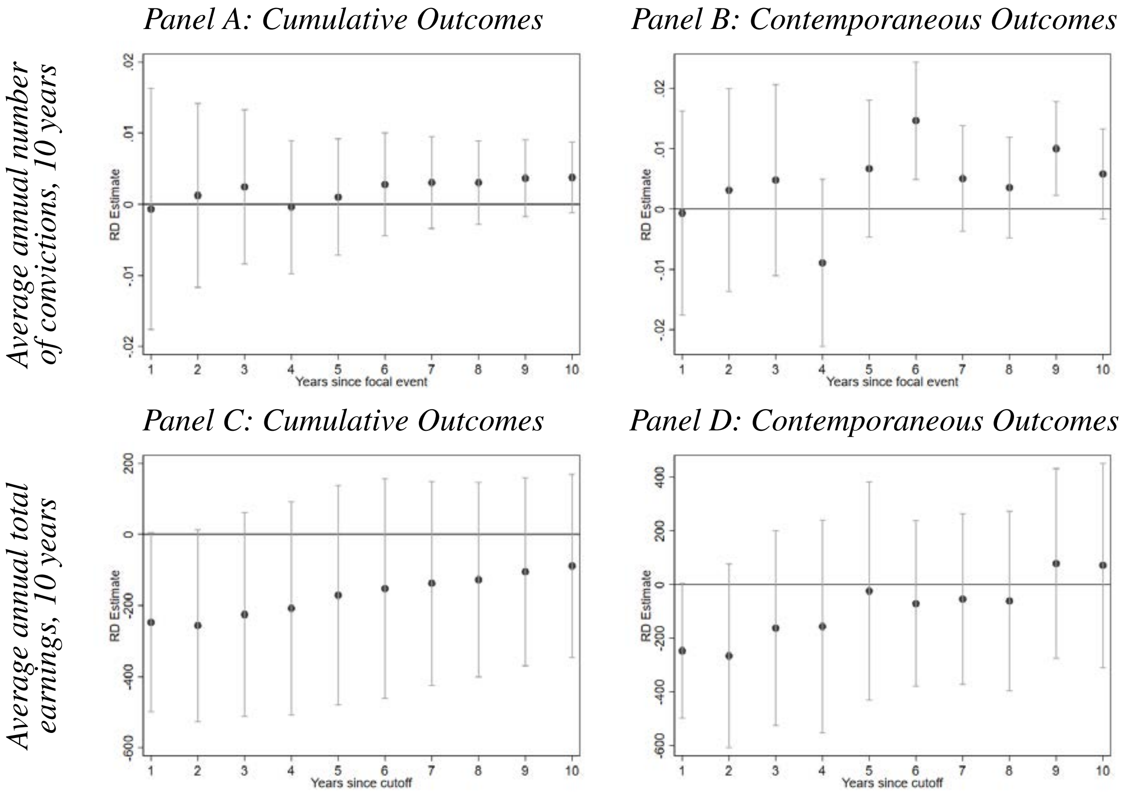
Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.
Note: This figure plots the sharp RDD estimates measuring the effects of fine increases on annual number of convictions in the 10 years following disposition by sub-experiment.
RD specification choices are described in Section 4.1. The estimation sample for each sub-experiment is described in Online Appendix D and Table 1.

Figure F6: Impact of increased fines on annual earnings by sub-experiment 10 years after the cutoff



Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.
Note: This figure plots the sharp RDD estimates measuring the effects of fine increases on annual earnings in the 10 years following the cutoff for each sub-experiment. Earnings are measured using income reported on W-2 tax returns and adjusted to 2017 dollars using the CPI-All Urban.
RD specification choices are described in Section 4.1. The estimation sample for each sub-experiment is described in Online Appendix D and Table 1.

Figure F7: Evolution of RD-based causal estimates over the 10 year followup period - Combined Sample

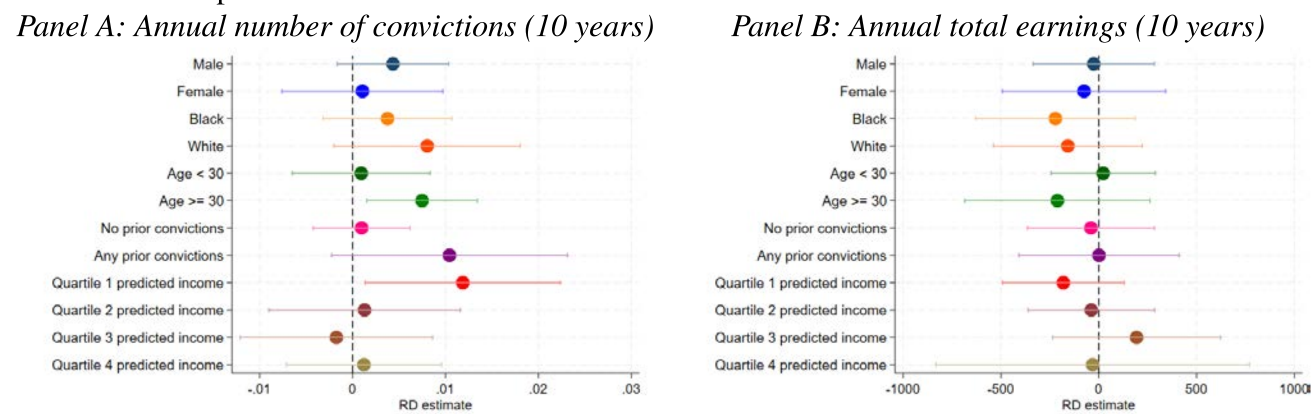


Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.

Note: This figure plots the sharp regression discontinuity design (RDD) estimates (dark grey, circles) measuring the effects of the increased sanctions on recidivism and labor market outcomes over a time period that varies by graph. For number of convictions (panels A and B) the time frame is between 1 and 10 years following disposition of first eligible offense. Annual earnings (adjusted to 2017 dollars using the CPI-All Urban) are measured using income reported on W-2 tax returns (panels A and B). The time frame covered is from 1 to 10 years following the cutoff. Outcomes in panels A and C are cumulative while outcomes in panels B and D are contemporaneous. All RD estimates are shown with 95% confidence intervals.

RD specification choices are described in Section 4.1. The estimation sample for each sub-experiment is described in Online Appendix D and Table 1.

Figure F8: Causal impact of increased sanctions on future earnings and convictions by sub-group - Combined Sample



Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.
Note: This figure plots the sharp RDD estimates measuring the effects of fine increases on annual total recidivism and annual total earnings (panels A and B) across different sub-groups (x-axis). Total earnings is measured using income reported on W-2 tax returns and adjusted to 2017 dollars using the CPI-All Urban.
RD specification choices are described in Section 4.1. The estimation sample for each sub-experiment is described in Online Appendix D and Table 1.

Table F1: Evaluating balance of observable characteristics and sample populations and predicted indices by sub-experiment

Experiment →	Average Effect	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Caseload density and weights:										
Average daily caseload	-3.213	2.41	2.395*	-3.019	-19.25	-.256	2.553	-11.03	-.6738	-11.93
	(2.653)	(5.382)	(1.317)	(8.249)	(-14.53)	(.6664)	(2.173)	(9.549)	(4.362)	(10.83)
	[71.22]	[58]	[62]	[70]	[171.3]	[6.7]	[25]	[150]	[80]	[74]
In a romantic relationship	-0.002	-0.006	0.011	-0.004	-0.003	-0.012	0.004	-0.005	-0.006	0.010
	(0.004)	(0.008)	(0.009)	(0.010)	(0.005)	(0.023)	(0.017)	(0.006)	(0.008)	(0.009)
	[0.474]	[0.384]	[0.488]	[0.400]	[0.511]	[0.315]	[0.597]	[0.535]	[0.480]	[0.539]
Sum of inverse sample weights, ACS	-15.08	7.361*	.4692	-2.092	-19.01*		.1336	-1.239	-1.04	.3327
	(17.72)	(4.051)	(2.622)	(5.213)	(10.63)		(3.636)	(5.712)	(3.925)	(9.154)
	[311.6]	[51.08]	[34.18]	[53.87]	[135.7]		[21.95]	[102.5]	[52.61]	[66.14]
Sum of inverse sample weights	-111.3									
	(236.6)									
	[5,800]									
Demographic characteristics:										
Male	-0.001	-0.010	-0.021**	0.018*	0.004	0.003	0.006	-0.003	0.002	0.011*
	(0.004)	(0.007)	(0.007)	(0.009)	(0.004)	(0.022)	(0.013)	(0.006)	(0.007)	(0.007)
	[0.752]	[0.787]	[0.724]	[0.734]	[0.710]	[0.766]	[0.834]	[0.671]	[0.764]	[0.829]
White	-0.006	-0.011	0.016**	-0.017*	0.006	-0.009	-0.010	-0.018***	-0.006	-0.006
	(0.004)	(0.008)	(0.008)	(0.010)	(0.005)	(0.025)	(0.018)	(0.005)	(0.006)	(0.008)
	[0.594]	[0.613]	[0.532]	[0.507]	[0.576]	[0.412]	[0.517]	[0.683]	[0.833]	[0.677]
Black	0.006	0.009	-0.017**	0.017*	-0.006	0.015	0.020	0.018***	0.004	-0.001
	(0.004)	(0.008)	(0.007)	(0.010)	(0.004)	(0.026)	(0.016)	(0.005)	(0.005)	(0.005)
	[0.271]	[0.317]	[0.228]	[0.411]	[0.291]	[0.465]	[0.271]	[0.245]	[0.108]	[0.086]
Hispanic	-0.002	0.000	-0.000	0.003	0.000	-0.015	-0.012	0.001	-0.000	0.011
	(0.002)	(0.003)	(0.006)	(0.004)	(0.003)	(0.013)	(0.014)	(0.002)	(0.003)	(0.007)
	[0.097]	[0.027]	[0.212]	[0.033]	[0.098]	[0.070]	[0.184]	[0.030]	[0.025]	[0.209]
Age at disposition	-0.101	-0.120	-0.286*	0.113	0.076	-0.691	-0.494	0.314**	0.289	-0.345*
	(0.096)	(0.190)	(0.173)	(0.238)	(0.117)	(0.582)	(0.308)	(0.137)	(0.215)	(0.192)
	[31]	[30]	[29]	[31]	[31]	[31]	[29]	[29]	[35]	[34]
Criminal history and prior income:										
Total prior convictions	0.024*	-0.035	0.038*	0.101**	-0.006	0.094	0.003	-0.016	0.014	0.075***
	(0.014)	(0.037)	(0.020)	(0.050)	(0.009)	(0.084)	(0.052)	(0.014)	(0.021)	(0.019)
	[0.748]	[1.510]	[0.336]	[1.530]	[0.205]	[0.550]	[0.756]	[0.628]	[0.526]	[0.506]
Average prior income	-18.38	-61.62	-216	286.3	-398.6	-354.6	562.2	422	384.4	-725.6
	(268.2)	(452.8)	(442.8)	(395.5)	(505)	(1262)	(661.8)	(560.2)	(1113)	(873.2)
	[19,000]	[14,000]	[14,000]	[9,400]	[18,000]	[15,000]	[13,000]	[22,000]	[38,000]	[26,000]
Predicted indices:										
Predicted annual number of convictions	0.001	-0.003	0.000	0.000	-0.001	0.010	-0.002	-0.002	0.001	0.004***
	(0.001)	(-0.003)	(-0.002)	(-0.003)	(-0.001)	(-0.006)	(0.004)	(0.002)	(0.002)	(0.001)
	[0.220]	[0.341]	[0.246]	[0.248]	[0.145]	[0.265]	[0.220]	[0.260]	[0.196]	[0.114]
Predicted annual earnings	-14.27	21.40	-35	102.46	-16.68	-90.32	366.5	-78.18	-116.53	-73.55
	(75.44)	(-89.98)	(-88.26)	(-110.28)	(-85.9)	(-484.2)	(262)	(131.53)	(255.75)	(219.03)
	[13,400]	[9,540]	[11,838]	[5,590]	[15,576]	[15,828]	[16,250]	[19,000]	[24,250]	[21,000]
Observations	626,000	59,500	58,500	48,000	176,000	6,300	20,000	117,000	60,000	81,000

Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.

Note: This table presents the sharp RDD estimates for select characteristics describing the individual at the time of conviction. Wages and income are adjusted to 2017 dollars using the CPI-All Urban.

RD Notes: Coefficients are estimated using a linear, sharp regression discontinuity design where event date is the running variable. The regression includes linear controls for the event date and the interaction of event date with the treatment variable, an indicator for if the case was disposed after the state's effective date. The estimation sample for each state is described in Section 3. Standard errors are shown in parentheses; control mean, measured using individuals to the left of the cutoff, are shown in square brackets. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Table F2: Evaluating change in total fines assigned upon disposition and total payments to date in the analysis sample upon fine increase implementation

Experiment →	Average Effect	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Panel A: Total sanctions assigned:										
	491.68***	16.92***	17.57***	32.00***	24.20***	135.76***	511.80***	493.12***	1,938.56***	2,076.46***
	(5.64)	(2.07)	(3.25)	(1.44)	(2.72)	(38.04)	(7.31)	(3.28)	(14.82)	(13.70)
	[452.60]	[71.53]	[131.70]	[78.86]	[113.82]	[686.81]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	745,045	68,045	67,997	62,599	207,735	7,067	23,045	125,324	75,996	107,237
Panel A: Total paid to date:										
	-13.90	-10.25***			-15.27***	-14.92				
	(9.32)	(1.50)			(2.18)	(22.26)				
	[170.47]	[58.19]			[83.37]	[359.17]				
Observations	188,211	35,762			146,204	6,245				

Source: Authors' calculations using the criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin in the CJARS 2022Q4 vintage.

Note: This table presents the sharp RDD estimates on the change in financial sanctions assigned upon disposition after the fine increases (panel A), total sanctions paid to date (panel B) for the subsets of data that we have payment data, and total days in probation for the spell associated with the focal sentence in the 10 years following the focal event. In all panels, the sample is not conditional on being matched to a PIK leading to differences in the total number of observations compared to the PIK'ed sample; in panel B, observations are also also conditional on being in the payment data. See Section 3 along with Table 1 for more details on sample restrictions.
* indicates that it's a state subsample. See Online Appendix D for more details.
RD Notes from Table F1 apply. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Table F3: Local Polynomial and Sharp RD, with no covariates, estimates of main outcomes on the pooled sample

Outcome	Sample→	Non-parametric estimation	No Covariates
Annual W-2 earnings, 1–10 years		-312.4	-114.6
		(360.9)	(154.2)
		[16,800]	[15,070]
Annual number of convictions, 1–10 years		0.001	0.005^{*}
		(0.008)	(0.003)
		[0.198]	[0.205]

Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.

Note: This table presents the sharp RDD estimates on the impacts of the increased fines on select outcomes using alternative specifications. The outcomes of interest are annual earnings 1-–10 years after the cutoff and total recidivism 1–10 years after the focal event. RD estimates under the non-parametric column are generated using the Stata program "rdrobust" (Calonico, Cattaneo, and Titiunik 2014), using a triangular kernel; bandwidth is chosen using the mean-squared-error-optimal bandwidth. We include the same set of covariates used in our main specification. For the column 'No covariates', we use the same RDD as in our main specification but do not include any covariates. We also use the same estimation sample used in our main specification described in Section 3

$*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Table F4: Impact of fine increases on employment and recidivism using administrative data by sub-experiment

Experiment →	Average Effect	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Panel A, Recidivism, 10 years following focal event:										
Annual number of convictions	0.004	0.006	0.008	0.014	0.010**	-0.023	0.006	-0.002	0.006	0.007*
	(0.003)	(0.008)	(0.008)	(0.010)	(0.003)	(0.020)	(0.011)	(0.004)	(0.005)	(0.004)
	[0.205]	[0.337]	[0.264]	[0.320]	[0.139]	[0.195]	[0.224]	[0.216]	[0.166]	[0.127]
Any charges	0.005	-0.002	-0.005	-0.001	-0.002	0.038	0.000	-0.001	0.007	0.007
	(0.004)	(0.007)	(0.008)	(0.009)	(0.005)	(0.024)	(0.017)	(0.005)	(0.008)	(0.008)
	[0.549]	[0.703]	[0.561]	[0.677]	[0.485]	[0.393]	[0.621]	[0.616]	[0.506]	[0.457]
Any convictions	0.003	0.001	-0.007	0.001	-0.002	0.019	0.010	-0.004	0.005	0.008
	(0.004)	(0.007)	(0.008)	(0.009)	(0.004)	(0.024)	(0.017)	(0.005)	(0.008)	(0.008)
	[0.471]	[0.635]	[0.498]	[0.523]	[0.326]	[0.357]	[0.563]	[0.550]	[0.456]	[0.411]
Any felony	0.004	-0.004	-0.014*	0.004	0.007**	0.021	0.013	-0.000	0.010*	-0.003
	(0.003)	(0.007)	(0.007)	(0.009)	(0.003)	(0.018)	(0.015)	(0.004)	(0.005)	(0.007)
	[0.217]	[0.310]	[0.289]	[0.292]	[0.173]	[0.169]	[0.293]	[0.156]	[0.121]	[0.205]
Annual number of drug convictions	0.001	-0.000	-0.001	0.005	0.004**	0.002	-0.006	-0.001	0.001	0.001
	(0.001)	(0.002)	(0.003)	(0.004)	(0.001)	(0.007)	(0.004)	(0.001)	(0.001)	(0.001)
	[0.039]	[0.051]	[0.068]	[0.080]	[0.034]	[0.033]	[0.051]	[0.026]	[0.016]	[0.019]
Annual number of property convictions	0.002	0.004	0.001	0.007	0.003*	-0.004	0.006	0.000	-0.000	0.001
	(0.001)	(0.003)	(0.004)	(0.006)	(0.001)	(0.009)	(0.004)	(0.001)	(0.001)	(0.001)
	[0.047]	[0.069]	[0.073]	[0.118]	[0.042]	[0.035]	[0.047]	[0.031]	[0.021]	[0.019]
Annual number of violent convictions	0.001	-0.000	0.002	0.002	0.001	-0.001	0.001	0.000	-0.000	0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.007)	(0.002)	(0.001)	(0.001)	(0.001)
	[0.023]	[0.034]	[0.040]	[0.040]	[0.014]	[0.033]	[0.023]	[0.017]	[0.016]	[0.014]
Panel B, Employment, 10 years following cutoff:										
Annual number of earnings	-89.41	-397.7	158.4	-208.7	-394.9	-1,100	55.77	692.8**	433.3	-823
	(131.5)	(263.3)	(304.7)	(267)	(240.9)	(895.8)	(675.4)	(318.8)	(550.7)	(546.7)
	[15,070]	[10,380]	[14,970]	[8,910]	[17,270]	[15,330]	[17,240]	[19,430]	[25,040]	[23,860]
Annual employment rate per year	0.001	0.013**	-0.001	-0.005	-0.007**	0.008	-0.017*	0.005	-0.000	0.007
	(0.003)	(0.006)	(0.005)	(0.007)	(0.003)	(0.017)	(0.010)	(0.003)	(0.005)	(0.005)
	[0.860]	[0.793]	[0.874]	[0.796]	[0.863]	[0.826]	[0.884]	[0.882]	[0.854]	[0.851]
Annual number of employers per year	0.000	0.009	0.013	-0.006	-0.008	-0.011	0.015	0.002	-0.002	-0.012
	(0.008)	(0.011)	(0.016)	(0.016)	(0.007)	(0.053)	(0.029)	(0.008)	(0.010)	(0.014)
	[1.011]	[0.729]	[1.163]	[0.868]	[1.027]	[1.248]	[1.049]	[0.968]	[0.878]	[0.966]
Annual total household earnings	14.76	-492.3	213.2	-258.3	-247.1	-1,282	486.5	946.5**	866.7	-1067
	(198.3)	(340.5)	(452)	(316.9)	(415.3)	(973.7)	(709.2)	(477.6)	(956.6)	(731.4)
	[21,440]	[14,420]	[18,040]	[10,910]	[24,780]	[17,630]	[18,710]	[27,930]	[38,400]	[30,850]
Observations	626,000	59,500	58,500	48,000	176,000	6,300	20,000	117,000	60,000	81,000

Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.

Note: This table presents the sharp RDD estimates on the impacts of the increased fines on labor market and recidivism outcomes. Recidivism behavior is measured 10 years after the focal event and labor outcomes are measured using cumulative W-2 earnings 10 years after the cutoff (adjusted to 2017 dollars using the CPI-All Urban). Wages and income are CPI adjusted to 2017 dollars using the CPI-All Urban. Total earnings is measured using income reported on W-2 tax returns. Average employment rate is defined as whether an individual received a W-2 tax return in that year. Average number of employers is measured using number of W-2 tax returns received that year. Total household earnings is measured using income reported on 1040 tax filings. See Choi et al. (2023) for details on offense classification.

RD Notes from Table F1 apply. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Table F5: Impact of fine increases on employment and household circumstances using American Community Survey responses by sub-experiment

Experiment →	Average Effect	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Panel A: Earnings and costs										
Monthly income	-64.78	-87.67	-9.675	38.92	-106.1		-294.2	-36.63	-25.12	-236.6
	(48.28)	(133.6)	(110)	(114.2)	(96.5)		(347.9)	(88.42)	(120.8)	(164.4)
	[2,323]	[1,538]	[1,738]	[1,144]	[2,088]		[2,277]	[2,417]	[3,056]	[2773]
Monthly housing costs	-6.445	-24.61	-55.66	12.14	16.15		77.63	-2.078	.9786	-35.71
	(14.44)	(39.99)	(39.6)	(47.55)	(37.65)		(97.66)	(28.02)	(31.12)	(41.53)
	[1,034]	[875.4]	[948.8]	[858.6]	[1,226]		[942]	[1,052]	[1,051]	[996.3]
Reported mental disability	0.004	-0.006	0.028	0.044*	0.015		-0.020	-0.016	0.012	0.009
	(0.006)	(0.024)	(0.017)	(0.026)	(0.012)		(0.032)	(0.011)	(0.013)	(0.014)
	[0.083]	[0.130]	[0.102]	[0.125]	[0.070]		[0.060]	[0.078]	[0.066]	[0.062]
Monthly mortgage	-.9063	-2.166	-86.38	33	-30.11		96.42	16.18	19.51	-62.71
	(18.55)	(50.22)	(70.64)	(79.61)	(53.39)		(179.5)	(32.21)	(32.4)	(60.61)
	[919.9]	[726.6]	[930.5]	[827.9]	[1194]		[913.5]	[896.2]	[875.7]	[941.2]
Monthly rent	2.689	-20.01	-38.15	15.51	14.22		12.14	32.05	-4.914	.6285
	(11.55)	(29.39)	(30.74)	(41.88)	(27.15)		(67.1)	(20.6)	(32.2)	(39.14)
	[660.4]	[579.5]	[674.5]	[587.8]	[804]		[645.6]	[644.8]	[636.7]	[663.7]
Panel B: Change in household circumstances										
Household size	0.013	0.118	-0.007	0.044	0.012		0.137	0.010	-0.065	0.044
	(0.035)	(0.115)	(0.116)	(0.134)	(0.077)		(0.254)	(0.066)	(0.068)	(0.115)
	[2.960]	[2.811]	[3.163]	[2.673]	[2.897]		[3.161]	[3.159]	[2.651]	[3.008]
Commute by Car	-0.001	-0.015	-0.015	-0.048	0.008		-0.042	0.015	-0.001	-0.005
	(0.008)	(0.036)	(0.027)	(0.038)	(0.020)		(0.055)	(0.015)	(0.017)	(0.023)
	[0.901]	[0.890]	[0.902]	[0.885]	[0.867]		[0.901]	[0.901]	[0.909]	[0.903]
Observations	45,000	3,900	4,100	2,800	8,800		1,300	11,000	6,900	6,100

Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.

Note: This table presents the sharp RDD estimates on the impacts of the increased fines on survey outcomes, measured using the 2005–2020 ACS. "Reported Mental Disability" refers to the question: Due to a physical, mental, or emotional condition lasting 6 months or more, do you have difficulty learning, remembering or concentrating (US Census Bureau 2021c)?

RD Notes from Table F1 apply. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Table F6: Causal impact of the increased fines on future recidivism and earnings by subgroup

Panel A: Demographic Characteristics Outcome Sample→	Male	Female	Black	White	Age < 30	Age ≥30	No prior convictions	Any prior convictions
Annual W-2 earnings, 10 years	-25.98	-75.47	-221.8	-158.4	22.42	-211.5	-40.06	1.137
	(158.2)	(213.1)	(207.6)	(194)	(136.2)	(241.6)	(165.7)	(209.1)
	[16040]	[12030]	[16960]	[10540]	[14530]	[15800]	[16410]	[11410]
Annual number of convictions, 10 years	0.004	0.001	0.004	0.008	0.001	0.007**	0.001	0.010
	(0.003)	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.003)	(0.006)
	[0.226]	[0.144]	[0.191]	[0.244]	[0.263]	[0.127]	[0.148]	[0.324]
Observations	459,000	167,000	389,000	155,000	351,000	275,000	456,000	170,000

Panel B: Quartiles of Predicted Income Outcome Sample→	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Annual W-2 earnings, 10 years	-181.2	-38.45	193.1	-31.32
	(158.6)	(165.3)	(219.2)	(408.8)
	[6472]	[10560]	[14990]	[28250]
Annual number of convictions, 10 years	.01186**	.001292	-.001738	.001222
	(.005362)	(.005261)	(.005282)	(.004257)
	[.2516]	[.2166]	[.2159]	[.1347]
Observations	157,000	157,000	157,000	157,000

Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.

Note: This table presents the sharp RDD estimates on the impacts of the increased fines on annual number of convictions and annual earnings by subgroups of the individuals in our focal sample defined in the columns.

RD Notes from Table F1 apply. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Table F7: Causal impact of the increased fines on future recidivism, unrestricted by PIK-process

Experiment →	Average Effect	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Recidivism:										
Annual number of convictions	0.001	-0.007	0.002	0.013*	-0.001	0.004	0.007	-0.001	0.005	0.003
	(0.002)	(0.006)	(0.004)	(0.007)	(0.001)	(0.011)	(0.007)	(0.004)	(0.004)	(0.002)
	[0.141]	[0.235]	[0.128]	[0.191]	[0.190]	[0.126]	[0.184]	[0.209]	[0.158]	[0.100]
Any charges	0.004	-0.012	0.006	0.009	-0.000	0.033	0.011	0.002	0.001	0.004
	(0.004)	(0.007)	(0.006)	(0.008)	(0.002)	(0.023)	(0.012)	(0.005)	(0.007)	(0.005)
	[0.433]	[0.548]	[0.391]	[0.415]	[0.565]	[0.366]	[0.588]	[0.619]	[0.522]	[0.422]
Any convictions	0.003	-0.008	0.009	0.005	0.000	0.021	0.011	0.001	-0.001	0.005
	(0.003)	(0.007)	(0.006)	(0.008)	(0.002)	(0.022)	(0.012)	(0.005)	(0.007)	(0.005)
	[0.385]	[0.493]	[0.348]	[0.335]	[0.439]	[0.339]	[0.532]	[0.548]	[0.468]	[0.382]
Annual number of drug convictions	-0.000	-0.002	0.000	0.003	-0.001	0.003	-0.000	-0.001	-0.000	0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.000)	(0.004)	(0.003)	(0.001)	(0.001)	(0.001)
	[0.026]	[0.035]	[0.032]	[0.049]	[0.047]	[0.020]	[0.048]	[0.025]	[0.016]	[0.016]
Annual number of property convictions	0.001	-0.000	0.001	0.005	-0.000	0.001	0.005	-0.002	0.001	0.001
	(0.001)	(0.002)	(0.002)	(0.004)	(0.000)	(0.005)	(0.003)	(0.001)	(0.001)	(0.001)
	[0.031]	[0.049]	[0.037]	[0.069]	[0.057]	[0.023]	[0.044]	[0.033]	[0.024]	[0.016]
Annual number of violent convictios	0.000	-0.000	0.000	0.001	-0.000	0.004	0.000	0.000	0.000	0.001
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.004)	(0.002)	(0.001)	(0.001)	(0.001)
	[0.016]	[0.023]	[0.019]	[0.022]	[0.016]	[0.021]	[0.023]	[0.016]	[0.016]	[0.012]
Observations	745,045	68,045	67,997	62,599	207,735	7,067	23,045	125,324	75,996	107,237

Source: Authors' calculations using the criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin in the CJARS 2022Q4 vintage.
Note: This table presents the sharp RDD estimates on the impacts of the increased fines on annual number of convictions of the individuals in our focal sample using only the CJARS data.
RD Notes from Table F1 apply. ∗ p<0.1, ∗∗ p<0.05, ∗∗∗ p<0.01.

Table F8: Impact of fine increases on relationship and romantic partners' employment and recidivism outcomes by sub-experiment

Experiment →	Average Effect	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Panel A: Recidivism and earnings, 10 years										
Annual number of convictions	-0.000	0.007	0.000	0.005	0.002*	-0.014	-0.005	-0.001	-0.003	-0.000
	(0.001)	(0.005)	(0.003)	(0.007)	(0.001)	(0.012)	(0.006)	(0.003)	(0.004)	(0.002)
	[0.040]	[0.070]	[0.036]	[0.061]	[0.015]	[0.033]	[0.041]	[0.058]	[0.046]	[0.028]
Annual total earnings	-15.88	-237.1	293.3	-716.7	-352.3	1332	-580.1	892.9*	-545.3	-115.3
	(248.9)	(539.6)	(632)	(736.8)	(378.8)	(2403)	(1051)	(461.7)	(801.4)	(669.6)
	[19,000]	[17,370]	[20,960]	[17,320]	[22,810]	[26,020]	[19,580]	[23,360]	[28,800]	[24,210]
Panel B: Relationship outcomes, 10 years										
Likelihood still reported together	0.005	0.001	0.016	0.001	-0.001	0.021	0.005	-0.004	0.001	0.007
	(0.006)	(0.013)	(0.011)	(0.013)	(0.006)	(0.039)	(0.022)	(0.007)	(0.010)	(0.011)
	[0.530]	[0.566]	[0.535]	[0.202]	[0.614]	[0.221]	[0.561]	[0.708]	[0.689]	[0.600]
Total years together	0.044	0.033	0.140*	-0.172	0.036	0.404*	-0.043	-0.058	-0.029	0.035
	(0.038)	(0.093)	(0.085)	(0.114)	(0.045)	(0.233)	(0.160)	(0.046)	(0.075)	(0.084)
	[6.630]	[7.274]	[6.967]	[3.777]	[7.644]	[2.082]	[7.328]	[8.392]	[7.998]	[7.395]
Observations	310,000	23,000	29,000	19,000	89,500	2,000	12,000	62,000	29,000	44,500

Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin using the CJARS 2022Q2 vintage. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES014-006 and CBDRB-FY23-0374.

Note: This table presents the sharp RDD estimates on the impacts of the increased fines on administrative outcomes of the romantic partner of the individuals in our focal sample. Romantic partners are identified using Finlay, Mueller-Smith, and Street (2022).

RD Notes from Table F1 apply. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.