Criminal court fees, earnings, and expenditures: A multi-state RD analysis of survey and administrative data*

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Abstract

Millions in the United States face financial sanctions in the criminal court system each year, totaling over $27 billion in overall criminal debt. In this study, we leverage five distinct natural experiments across multiple states using regression discontinuity designs to evaluate the causal impact of these sanctions. We consider a range of long-term outcomes including employment, recidivism, household expenditures, and other self-reported measures of well-being, measured using a combination of administrative records on earnings and employment, the Criminal Justice Administrative Records System, and household surveys. We find consistent evidence across the range of natural experiments and subgroup analyses of precise null effects on the population, ruling out long-run impacts larger than +/-3.6% on total earnings and +/-4.7% on total recidivism. These results are inconsistent with theories of criminal debt poverty traps but also do not justify using financial sanctions for local revenue or as a crime control tool.

Keywords: criminal justice, fines, deterrence, recidivism, labor market outcomes

JEL classification codes: H72, J24, K42

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1 Introduction

In the United States, the growth of the criminal justice system has prompted an increase in the use and amount of criminal financial sanctions (Harris, Pattillo, and Sykes 2022). The Fines & Fees Justice Center estimates that national court debt exceeds $27 billion (Hammons 2021). Many have linked these monetary penalties, which include fees, fines, and restitution, to negative outcomes such as financial instability, future criminal activity, decreased access to credit, and poor labor market outcomes (Harris 2016; Menendez and Eisen 2019). Given the massive size of the criminal justice population in the United States, this suggests spillover impacts of criminal financial sanctions beyond just the criminal justice involved population.

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 multiple state-specific policy changes to measure the causal impact of the increased monetary sanctions, ranging from $17 to $200, on individuals' criminal and labor market outcomes over the short and long-run using a sharp regression discontinuity design. We measure these outcomes by merging administrative earning records from the IRS with administrative criminal records from the Criminal Justice Administrative Records System (CJARS). While the magnitude of the fines and fees are significantly more modest than prior research has examined, their reach is court system-wide, providing evidence for a population more representative of justice-involved individuals as a whole. In addition, through linking our study sample to individual-level survey responses from the American Community Survey (ACS), we are able provide the first evidence of the impacts of fines and fees on a substantially broader range of outcomes, including housing expenditures, household size, and measures of household circumstance (e.g., household size). We also supplement the data with additional administrative financial sanctions assessment and payment data to track changes in total sanctions assigned at the time of disposition along with total payments.[1]

This breadth of settings, which builds on recent causal evidence in Finlay et al. (2022) and Pager et al. (2022), offers several novel contributions. First, the ability to examine several policy changes across multiple states and counties in a unified setting greatly increases our sample size and the external validity of our findings. Our final sample represents approximately 11% of the U.S. population.[2] Furthermore, the variation of institutional rules and contexts across each state allows for a rich exploration of important policy mechanisms (e.g., consequences of non-payment,

[1]Payment data is limited to a subset of courts.

[2]This statistic is calculated by comparing the total population of Florida (Duval County, Hillsborough County, Leon County, Miami-Dade County, and Orange County only), Michigan, North Carolina, Texas (Bexar County, Dallas County, Hidalgo County, and Tarrant County), and Wisconsin (Milwaukee County only) to the total U.S. population.

considerations for ability to pay, etc.). Lastly, the wide range of fine amounts allow us to capture potential heterogeneous responses due to differences in affordability.

In each of the natural experiments, fine increases were strictly defined by the event date (disposition date for all of our states, except for Texas, which used offense date). We utilize these institutional features to present both state-specific and combined-sample regression discontinuity findings that together explore the possibility for treatment effect heterogeneity across jurisdictional boundaries and maximize precision in pooled regressions.

Our results align with the findings of Finlay et al. (2022): criminal financial sanctions have no meaningful impact on long-term recidivism or labor market outcomes. Given the size of our focal sample, these are precise null effects. Specifically, we can rule out effects greater than +/-3.6 percent with 95% confidence on total earnings reported on W-2 tax returns for the first 10 years after the cutoff and effects greater than +/-4.7 percent on total recidivism (-0.005–0.1 total convictions) 10 years after the focal event. Although the states in our research represent diverse institutional and demographic contexts, our long-run results are largely consistent across each state.

The impacts of fines and fees, however, might be manifested in ways missed in traditional administrative data sources (e.g., consumption, stress, or expenditures). We extend the work of Finlay et al. (2022) and estimate the impacts of the fines on expenditures, informal earnings, and plausible measures of mental well-being, using individual responses to the ACS. Using self-reported total income, which includes informal income and earnings from self-employment, we do not find any significant changes in total income. We also do not find any significant changes in monthly housing costs, rent, or mortgages. Specifically, we can rule out effects +/-4.4 (+/-3.7) percent with 95% confidence on monthly housing costs (total income). Similarly, we find no significant impact on self-reported likelihood of difficulty concentrating or remembering due to a mental, physical, or emotional problem, indicating no change on mental health. Overall, our findings show consistent evidence that the increase in monetary sanctions did not impact individuals' labor market outcomes or consumption.

We also measure the impacts of the fines on total payments to date and time spent on probationary supervision, an observable punishment for non-payment of criminal debt, following the focal event to understand the potential mechanisms contributing to our estimated null effects. On the one hand, if payments increase in response to additional fines, our reduced form results would suggest the cost of fines are minimal relative to overall budget constraints. Alternatively, if payments do not respond, our null findings could suggest that punishments (e.g., extension of probation, additional fines) are ineffective. We find that neither payments nor punishments responds to the increases in fines, suggesting a third potential interpretation of the natural experiments. Unless other unobserved punishments like discretionary sanctions imposed by judges respond to the increase in fines without corresponding payment increases, it is likely that defendants were already underwater in criminal debt and would have exhausted potential punishment options even without the imposition of the additional fees we study, which has important policy implications.

To the best of our knowledge, four out of the five 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 Appendix Online Appendix C:.

Other past causal evidence on the impacts of financial sanctions on recidivism and financial outcomes is limited to studying vehicular infractions, which is an important distinction as these financial sanctions are not accompanied with criminal convictions. This body of work documents a negative relationship between financial sanctions and financial stability along with a specific deterrent effect (Dušek and Traxler 2022; Gehrsitz 2017; Goncalves and Mello 2017; Kessler 2020; Mello 2021; Traxler, Westermaier, and Wohlschlegel 2018). But, it is unclear whether these results apply to sanctions bundled with a criminal conviction, especially as past work (e.g. Mueller-Smith and Schnepel (2021)) find negative impacts of convictions on future employment and higher rates of recidivism. Thus, conditional on having a conviction, the marginal treatment effect of certain sanctions like fines and fees may converge to zero. Furthermore, infractions fall under the jurisdiction of lower level courts (e.g., Justice of the Peace in Texas) and are not subject to other treatments imposed in criminal courts (e.g., requiring court appearances, bond, bail). Thus, these results may not extend to the broader justice involved population, especially to individuals charged with criminal offenses.

The exception to this is Hansen (2015), which jointly tests both the increase in fines and a higher rate of incarceration arising from crossing a severity threshold in blood alcohol level when convicted of driving under the influence. While Hansen (2015) finds a specific deterrent effect, it is unclear if the impact is driven by the increased fines or the increased incidence of incarceration.

Our work makes several important contributions to the literature. First, we focus on the broader justice involved population, focusing on financial sanctions stemming from misdemeanor and felony offenses rather than just traffic offenses or vehicular infractions. Second, we offer major improvements in external validity, presenting the multi-state evidence of the impact of a broad set of financial sanctions and provide consistent, robust evidence on the effects of these sanctions. Third, we are the first to include an analysis of household expenditures and other measures of well-being, extending the analysis of the impact of financial sanctions onto measures of individual welfare beyond what can be measured with administrative data. Lastly, we leverage financial sanction payment data and link the focal disposition events to probation spells to identify potential important policy mechanisms.

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

Driven by the rising costs, the use and amount of financial sanctions in the criminal justice system has exploded in recent years. 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 legal financial obligations (LFO) (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 considerations 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., 200% above the poverty level in Kentucky disqualifies an individual from indigence 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. Furthermore, actual revenue gains are often significantly lowered 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 difference 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.

We now outline the specific fine changes we exploit in this paper along with state-specific institutional details. A summary of this information is outlined in Table 1, highlighting the similarity 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 are detailed 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.

2.1 Florida

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.[3] These fines would apply to anyone convicted after June 30, 2004.

2.2 Michigan

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.[4] One of these bills, House Bill 4732 established a minimum cost upon con-

[3]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.

[4]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.

viction, 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.[5] We exclude traffic offenses from our study to prevent the confounding of our results from the DRF passage.

2.3 North Carolina

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 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.

2.4 Texas

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

[5]See Finlay et al. (2022) for details of this policy and estimates of the causal impact of DRF programs in Michigan and Texas.

existing cost structures, which are not publicly archived, as well as our ability to measure fines and fees in criminal charge data in CJARS.[6]

Similar to Michigan, Texas also passed its version of the DRF in the fall of 2003, which assigned fines if driver's 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.

2.5 Wisconsin

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.[7]

3 Data

We combine several sources of administrative records to measure criminal and employment outcomes: criminal records from CJARS (see Finlay, Mueller-Smith, and Papp (2022)), longitudinal earnings data from IRS W-2 tax forms, and the American Community Survey from 2005–2020. The data 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

[6]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.

[7]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).

individuals with an Individual Taxpayer Identification Number (ITIN) (Brown et al. 2018). Thus, we cannot link individuals who are more likely to be undocumented.[8]

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. Criminal justice data, especially adjudication data, is disaggregated across agencies, leading to discrepancies in data availability and offense classification (see (Choi et al. 2022)) 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.

We identify the focal sample from conviction information contained in CJARS. We restrict the sample to the first charge observed for each individual within each of our state subsamples.[9] While we restrict to first qualifying charge by state, some of the states are restricted to just felonies or misdemeanors. Thus, if we restrict to first felony charge and the individual has a prior misdemeanor, we consider the first felony charge the focal event. Table 1 shows the sample restrictions, data restrictions, and the choice of running variable defined by each state's policy change. For a subset of states, we also have payment and assessment data. For these states, Michigan, Wisconsin, and Florida (Hillsborough County and Miami-Dade County only), we can measure changes in total payment.

We also restrict a subset of states in our sample by offense grade due to historical adjudication data availability and policy restrictions (e.g., DNA fee in Wisconsin impacting only misdemeanor charges). Specifically, we focus on misdemeanor charges only in Texas and Wisconsin and felony charges in North Carolina and Michigan. In North Carolina, we have felony convictions but not felony charges preceding January 2011. Thus, we truncate our timeline to the 200 days preceding the cutoff in that state only. In Florida, the additional court cost applied to both misdemeanors and felonies and we have detailed adjudication and court assessment data beginning in 1999 for all counties included. Thus, we keep both misdemeanor and felony grade offenses for the Florida portion of our research sample.

In order to avoid repeated entry into the analysis sample, we restrict our focal sample to the first observed qualifying charge in the CJARS data set. For individuals disposed of multiple charges on the same day, we define their offense grade based on the most severe offense. Thus, if an individual has any felony charges along with multiple misdemeanor charges disposed within the same day,

[8]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.

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

we classify them as having a felony charge, not a misdemeanor charge. See Appendix Online Appendix B: 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 states. Since the fines only applied based on the legislation's effective date, we can classify individuals disposed before the cutoff date as untreated and those disposed after as treated. For example, an individual convicted of a misdemeanor offense in 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. In order for the results to have a causal interpretation, the fine increase must be the only variable correlated with the outcomes to shift. We test the validity of this identifying assumption in Section 4.1.

Using a sharp regression discontinuity design, we measure the overall impact of the fine change with:

$$Y_{i,s} = \beta_0 + \beta_1 Post_{i,s} + \beta_2 RunningDate_{i,s} + \beta_3(Post_{i,s} \times RunningDate_{i,s}) + \gamma_s + X_{i,s}\delta + \varepsilon_{i,s} \quad (1)$$

where $Y_{i,s}$ is the outcome of interest for individual i in state s. $Post_{i,s}$ is an indicator variable equal to 1 if individual i's disposition/offense date occurred after state s's policy effect date; $RunningDate_{i,s}$ is the running variable, usually the disposition date except for in Texas (offense date). $X_{i,s}$ 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. We also include state fixed effects, γ_s. The coefficient of interest is β_1, which describes the impact of the increased fine on outcome $Y_{i,s}$.

To unify the states in our sample in our combined analysis, we center each state's running variable relative to the cutoff (i.e., centered on 0) to create a common support. As a complement to this pooled analysis, we also estimate the above equation separately by state to explore potential state-specific heterogeneity.

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 A6, excluding covariates, and using a non-parametric analysis, shown in Table A1.[10] Our findings are robust across each of these different specifications.

4.1 Identifying Assumption

The key identifying assumption for uncovering the causal impacts of the monetary sanctions on the outcomes of interest is that an individual's treatment assignment is unrelated to the fine increase. Hence, for the validity of the RDD, we should not observe any discontinuous changes in any other variable aside from the assigned fine amounts.

There are many potential reasons why this assumption might not hold in theory. For example, the increased fines could discourage individuals from committing crimes, generating a general deterrent effect. On the other hand, judicial agents might be encouraged to charge or convict more people in order to increase revenue for the local government. Individuals may also respond differently based on underlying characteristics (e.g., attorney affordability).

In order to test for these possible threats to identification, we run a battery of tests to ensure that all of our estimated impacts are driven by the increased fines. To address the concerns around sorting, we check the 30-day caseload density across the discontinuity. If there is a general deterrent response or increase in convictions due to the fines, we should observe a discontinuous change in caseload density across the discontinuity.

We present the balance checks by showing the combined regression-discontinuity graphs in Figure 1.[11] Each point is the average within a 51-day bin with the size of the point indicating the number of observations within each bin.[12]

Figure 1 panel A shows the daily caseload density graphs for the combined sample. Over 317 cases were disposed per day. We find an insignificant decrease of over 20 cases per day across the discontinuity, indicating a minimal deterrent effect.

To address the last concern of differing responses due to case characteristics, we check the balance in individual covariates across the discontinuity. If individuals have a differential response to the fines due to underlying characteristics, we could observe a discontinuous change in baseline characteristics across the cutoff. Table 2 shows the balance estimates for each state and the

[10]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.

[11]Appendix B Figures A1–A3 shows each of the exercises individually by state, confirming the independent validity of each of the different natural experiments that we consider.

[12]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.

combined sample. Overall, we find balance in individual characteristics across the discontinuity. We do find some imbalances in some characteristics (e.g., likelihood of being male or black and total prior convictions) in North Carolina and Texas, but these estimates are small given the control mean.

We also find some sample differences across the different states. Florida represents the bulk of the caseload, with 171 daily cases on average, while Wisconsin represents the smallest, with 10 misdemeanor cases processed per day. We also observe some differences in predicted earnings, ranging from $28,000 to $79,000, and in racial composition across each of our state samples.

To further prove we fulfill the identifying assumptions, we generate summary indices by predicting certain outcomes using a set of covariates describing the individual at the time of the focal event. If any of the balance characteristics change in an economically meaningful way at the treatment cutoff, we should observe a discontinuous change in the predicted indices. We generate these predicted 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.[13]

Panels B and C of Figure 1 show the regression discontinuity graphs of the predicted indices. Reassuringly, we observe smoothness across the discontinuity in all of the samples. As shown in Table 2, we observe an insignificant 0.001 decrease in predicted recidivism and an insignificant $60 decrease in predicted income. Furthermore, the estimated discontinuities are small relative to the mean indicating no discontinuous change in any of the individual characteristics.

5 Results

5.1 Changes in fines, payments, and probation length resulting from policy variation

We present our first stage estimates for each state and the combined sample in Figure 2 and Table 3, which show the change in total court costs and total assessed at time of conviction in panel A. Due to differences in data availability, all of the states except for North Carolina show the total amount assessed at disposition.[14] For North Carolina, we show just the court costs 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

[13]As mentioned in Section 3, the follow-up periods are different due to measurement periodicity and data availability.

[14]Total assessed includes fines and total costs, but excludes restitution.

sanctions. Thus, the first stage estimates will be lower than the actual increase, which only applies upon guilty convictions.

Focusing on the graphical analysis, panel A of Figure 2 shows the change in financial sanctions assigned upon disposition for the combined sample (circles, red); panels B–F shows the equivalent for the state-specific sub-samples along with the total payments to date (diamonds, blue). On average, we find that total sanctions increased by approximately $22.44, a 20% increase relative to an average total sanctions of $113. This is largely driven by Florida, which makes up more than half of the sample and also increases by $25, a 24% increase relative to a mean of $106. Michigan has the smallest increase of around $17, but a proportionally larger increase of 24% (relative to a mean of $72). North Carolina and Texas have similarly sized increases of approximately $32 (41% and 17%, respectively). The proportional change in North Carolina is likely an upper bound as there may be other sanctions assigned (e.g., fines, restitution) that we do not observe. We find the largest monetary increase of $157 in Wisconsin. Compared to the total sanctions assigned of $782, this represents a 20% increase. This is likely a lower bound as the total sanctions assigned includes later sanctions (e.g., interest accrued, late payment fee).

While several of the fine increases we study are quite modest (e.g., ↑$17 in Michigan), research suggests that even modest amounts of debt may be unaffordable for justice-involved individuals (Stevenson 2018). The consequences of non-payment also vary across jurisdictions in our study. For example, individuals can be held for civil contempt for failure to pay, have their driver's license suspended, and have their wages or taxes garnished to pay back the debt (Diller 2010; Hammons 2021). Interpreting our evidence depends critically on whether fine increases yielded increases in payments (thereby directly impacting household finances) or triggered punishments, which themselves might impact future outcomes even if there was no direct impact to household finances.

An added issue is that even without the policy changes, punishments might already be triggered within this study population for non-payment of other fines that pre-dated the policy introduction. In this case, the increase in criminal debt would neither impact household finances nor non-pecuniary sanctions that individuals face, an important caveat when interpreting the results in this paper.

To understand this, we measure the impact of the increased fines on total payments using the payment data we have available (Michigan, Wisconsin, and Hillsborough County and Miami-Dade County in Florida). We find no significant impact of fine increases on total payments for the subset of individuals assigned sanctions related to the focal event.[15] While we observed a significant increase in assigned sanctions at time disposition, we do not observe an equal increase on payments. Notably, payments are smooth across the cutoff and actually decrease significantly in Florida and Michigan.

[15] Point estimates are shown in Table 3 panel B.

Taken together, individuals were unable to increase their payments in response to the higher sanctions, as shown by the lack of increased payments after the policy change. Given that payments did not change and in fact decreased after the increase, individuals at the margin of fine affordability in our focal sample would be at greater risk for the consequences for non-payment enumerated earlier.

To test this, we next examine the causal impact of the fine increases on the individual's first probation spell after the focal event as criminal debt is a condition for fulfilling probation in all of the states, with the exception of Wisconsin. Thus, individuals on the margin would now be subjected to increased probation lengths due to not being able to pay off their financial sanctions. While individuals are subject to a range of consequences for non-payment, probation is a measureable and observable outcome already in the data. Figure A4, with corresponding point estimates in Table 3 panel C, shows the impact of the fine increases on total number of days in probation in the 10 years following the focal event. Overall, we do not find any significant changes in probation length indicating no change in punishment despite the higher rates of non-payment.[16] This result is not surprising since the average payments were lower than average assigned sanctions in all of the subsamples, suggesting that individuals could not pay the full amount of sanctions even before the increase in fines.[17]

Although individuals did not increase their payments nor face increased probation terms associated with the focal event, despite the increase in criminal debt, this does not mean that the policy change had no impact on individual outcomes. As noted earlier, individuals face other consequences, aside from probation, if they are unable to pay their financial sanctions. For example, judges could issue contempt of court if they deem an individual is willfully not paying their fines or fees, individual's driver's licenses could be suspended, along with many other consequences that we cannot observe in the data.[18] These unobserved consequences could have important impacts on individual outcomes, which we study in the next section.[19]

[16]As shown by the sample size in the probation estimates for Florida, this is a very specific subsample of cases which may not be representative of actual probation lengths in Florida. From the CJARS documentation, statewide probation coverage doesn't begin until 2016, outside the 10 year window we study probation outcomes. Instead, we only have Miami-Dade probationary data, which starts in 1973 (Finlay, Mueller-Smith, and Street 2022).

[17]This should be interpreted with caution as the samples differ between panel A and B. Furthermore, for some of the states (e.g. Wisconsin, Florida), we cannot separate sanctions assigned at dispositions from future sanctions.

[18]See (Diller 2010; Liu, Nunn, and Shambaugh 2019; Bannan, Nagrecha, and Diller 2010; Hammons 2021; Harris 2016).

[19]Since the U.S. justice system does not require collection of financial sanction data, individual

5.2 Impacts on outcomes measured with administrative records

In this section, we present our findings on the direct impacts of the increased fines on individuals' cumulative labor market and recidivism outcomes. For both of these sets of analyses, we utilize administrative data, which has the benefits of population-wide coverage, which both increases sample size and eliminates concern about mismeasurement associated with survey data collection efforts (e.g., social desirability bias, non-response bias). We measure employment and earnings using 2005–2020 W-2 tax returns and 2004–2020 1040 tax filings. We measure recidivism outcomes using all data available in the CJARS data up to 10 years following the focal event, and labor market outcomes up to 10 years after the cutoff.

As shown in Figure 3 panel A and Table 4, we find no impact of the increased fines on future recidivism. Total convictions increases by a total of 0.05 convictions in the 10 years following the focal event. Relative to the control mean of 2.129 convictions, this represents a 2% increase. While marginally statistically significant, the increase is not economically meaningful. This is highlighted by the smoothness in total convictions across the cutoff. Additional recidivism outcomes are shown in Figure 3 panel B, which also fail to depict statistically and economically meaningful impacts.

For total earnings, shown in panel C of the same figure, we find similar null effects with a decrease in cumulative earnings reported on W-2 tax returns of $2,451 in the 10 years following the cutoff, a 2% decrease relative to a mean of $142,000 in total earnings. While also marginally statistically significant, the decrease again is not economically meaningful. Additional employment outcomes are depicted in Panel D, which also fail to show statistically and economically meaningful impacts.

To assess changes throughout the outcome period, we also provide evidence on the impacts of the fine increases over different time periods. We show our results varying the outcomes by one year increments in Figure A5. Panels A and C show the cumulative estimates over time; panels B and D show the contemporaneous estimates. These results confirm our prior findings. Overall, we do not observe any meaningful impacts over any of the time periods. While some estimates are significant, they represent small percent changes when compared to the mean.

5.3 Impacts on outcomes measured with self-reported survey data

While administrative data is advantageous for the breadth of data coverage, it may fail to capture more nuanced responses to the imposition of additional fines and fees. The inclusion of survey responses is important because the fines could impact outcomes aside from formal employment and

agency have discretion for if and how they store the information, leading to a lack of financial sanction data, data on financial sanction payment, and data on the consequences associated with criminal debt (Bannan, Nagrecha, and Diller 2010; Harris, Pattillo, and Sykes 2022; Diller 2010).

recidivism.[20] For example, individuals may respond to the increased legal financial obligations through changing consumption, expenditures, stress, or other measures of well-being and household circumstances. Individuals may also respond by selecting housing with lower monthly costs either through reduced housing costs or increased cohabitation to split costs in order to reduce household expenditures. On the extensive margin, individuals may choose to rent rather than own a house due to the increase in financial burden. The fines may also reduce credit access leading to further reductions in mortgage access.

Currently, there are no reliable ways to measure such outcomes population-wide using administrative data; instead, we turn to the ACS, a nationally representative survey, that is linked at the individual-level to our analysis sample. While this creates the opportunity to study a range of new outcomes not previously considered in the literature, it does substantially contract our sample size since the ACS sample frame in a given year is only 1% of the U.S. population.[21]

To understand the full impact of these sanctions on individuals, we use the ACS responses to measure total income, including informal income and earnings from self-employment, along with other measures of household circumstances and individual well-being. Because 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.

Figure 4 in panels A–B and Table 4 show our results using the ACS outcomes. Panel A focuses on total monthly income and various measures of housing expenditures. Total monthly income encompasses all forms of income an individual receives, allowing us to measure changes in self-employment and informal earnings. For housing, we measure housing expenditures using monthly housing costs, monthly mortgage, and monthly rent. Our results show no significant changes across any of these measures. Notably, we observe null impacts across all of the outcomes in panel A. We observe a 2% decline in monthly earnings relative to mean of $1,852 and a 0.2% increase in monthly housing costs relative to a mean of $1,088. Again these effects are precise nulls;

[20]As past research has documented, criminal justice involved individuals have weak attachment to the labor market (Bushway, Stoll, and Weiman 2007; Fairlie 1999; Holzer, Raphael, and Stoll 2003; Schmitt and Warner 2011).

[21]Given U.S.Census Bureau concerns and policy regarding data privacy and confidentiality, this precludes our ability to present traditional RD graphical plots as well as subgroup heterogeneity analysis.

we can rule out effects greater than +/-4%-5% on monthly housing costs, rent, and mortgages. Furthermore, the lack of significant or sizeable change in monthly mortgages may imply no change in credit access, another measure of financial stability.

In panel B, we switch our attention to other survey measures of well-being and household circumstances. As noted earlier, these fines could lead to higher rates of driver's license suspension due to non-payment of financial sanctions, or increased cohabitation rates to reduce housing costs, or exacerbate mental health issues due to existing disabilities.[22] We measure the impact of the fines on these outcomes using the self-reported responses of whether an individual commutes by car, has difficulty concentrating, remembering, or making decisions due to a condition,[23] and household size. These results echo our previous findings. We find no significant evidence that the fines impacted these measures of individual well being. Our results show precise null impacts of the fines, and we can rule out changes greater than +/-4% on household size and likelihood an individual commutes to work via car. Our estimates on the impacts of difficulty concentrating, making decisions, or remembering are noisier, but still insignificant.

5.4 Subgroup heterogeneity analysis across each of the natural experiments and across individual characteristics

The fines may have heterogeneous impacts across states and subgroups of the population that are not present in the estimates using the overall sample. This is possible given the different sample criteria and demographic and institutional contexts across the states in our research sample (e.g., misdemeanor charges only in Wisconsin versus felony charges in Michigan). Due to the small sample size in our survey outcomes, our heterogeneity analysis is restricted to using the administrative data, which can provide intuition for subgroup heterogeneity in the survey outcomes. Since our results using the full sample are consistent across both the administrative data and the survey data, it's unlikely that the subgroup heterogeneity analysis will vary significantly across the different data sources.

We show the state specific estimates in Figure A7. Here, we rank the states by the amount of fine increase. Overall, we find no substantial difference in impacts of the fines by state on their future recidivism or total earnings. While we do find some significant estimates in total convictions in Florida, the estimate is not significantly different than the other state estimates. When we stratify by offense type, shown in Figure A8, this increase is largely driven by significant increases in total

[22] See Ryu and Fan (2022) and Schwabe et al. (2012) for more research on the links between financial stress and mental health.

[23] The full question is: "Because of a physical, mental, or emotional condition, does this person have serious difficulty concentrating, remembering, or making decisions?"

drug and property convictions. Again, these estimates, while significant, are not significantly different than the other state-specific estimates.

We also stratify the state-specific estimates of other labor outcomes in Figure A9. While we observe a significant decrease in likelihood of receiving a W-2 in Florida and a significant increase in Michigan, these estimates are not significantly different from the combined sample estimate nor from the other states.

We show the subgroup treatment effects on long-run total earnings and total convictions in Figure A10. We define our subgroups based on socio-economic traits (race, sex, and age), criminal history, and quartiles of predicted income levels. Most of the estimates, with the exception of the impact on earnings for those over the age of 30, are insignificant indicating no heterogeneous treatment effect.

6 Conclusion

This paper measures the impact of financial sanctions across multiple states (Florida, Michigan, North Carolina, Texas, and Wisconsin) by leveraging the abrupt monetary increase of total court costs assigned upon conviction, with increases ranging from \$17 to \$160. Although each of the states covers distinct jurisdictions, demographic contexts, and time periods, we find consistent evidence of null impacts on recidivism and labor market outcomes.

To address potential shortcomings in administrative data outcomes, we also link individuals in our sample to their ACS responses from 2005 to 2020. Consistent with our prior analysis, we find no impact of the increased sanctions on self-reported measures of income, housing expenditures, and other measures of well-being, which includes likelihood of commuting by car, household size, and difficulty remembering, concentrating, or making decisions due to having a physical, health, or mental condition. This is the first paper to integrate broad household survey into a quasi-experimental analysis of financial sanctions; further research is required to confirm the robustness of these findings. That said, our results are inconsistent with the hypotheses that the fines negatively impacted more malleable outcomes like consumption, expenditures, or stress.

Our results largely echo recent work on the causal impact of financial sanctions, notably Pager et al. (2022) and Finlay et al. (2022). We improve on the existing literature by examining a broad setting: incorporating additional states, studying a set of monetary sanctions that apply to the wider justice-involved population, and expanding the outcome set with self-reported measures of individual well-being. Similar to past work, we find no significant harm on the entire set of individual outcomes, which include recidivism, labor market, self-reported earnings, self-reported housing spending, and others. But we also do not observe any meaningful benefits to justify the use of increased sanctions. We do not find any specific deterrent effects of the policy, and from public reports, the increases did not generate more revenue either. Thus, the continued taxation of the criminal justice involved population through the use of financial sanctions is inefficient.

Tying in administrative financial sanction payment data and probation data associated with the focal event, our analysis on criminal debt repayment and probation length may shed light on the potential mechanisms contributing to our null estimates. Notably, payments and probation length both did not significantly increase as a consequence of the increased debt. These results, in addition to our null results on individual outcomes, are suggestive evidence that total fines were unaffordable even prior to the fine increases. Thus, it is possible that individuals do not respond to increased fines due to already being overtaxed by pre-existing levels of financial sanctions in the justice system.

Overall, our results show that the increased fines in our study neither raised revenue nor generated any deterrent response to future crime but also didn't cause significant harm on individual outcomes. While some policymakers and advocates argue for the waiver of certain debts and considerations for ability to pay, Pager et al. (2022) show that even erasing criminal debt does not impact recidivism rates in the 2 years following debt forgiveness. Although significant past qualitative work documents an extensive relationship between criminal court debt and negative outcomes,[24] perhaps what is at work are the negative ramifications of justice-involvement and convictions overall (Craigie et al. 2020; Dobbie, Goldin, and Yang 2018; Leslie and Pope 2017; Rose 2021; Stevenson 2018) rather than the impositions of fines and fees specifically.

[24] See Crozier, Garrett, and Maher (2020), Diller (2010), Harris, Evans, and Beckett (2010), and Menendez and Eisen (2019)

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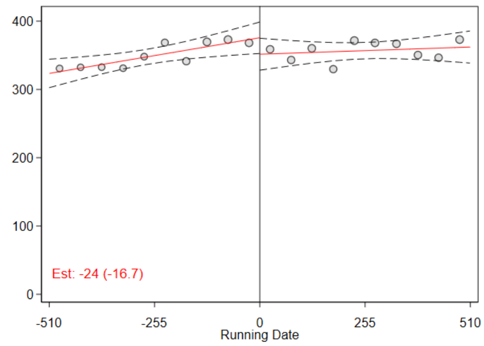
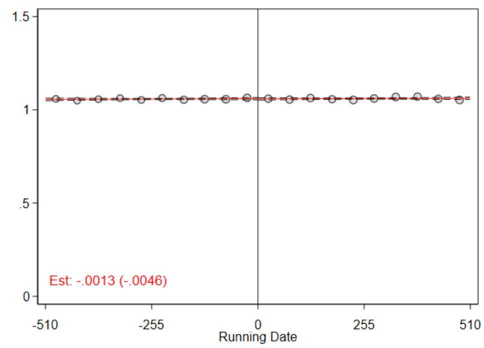
Figures

Figure 1: Balance in Caseload Density and Predicted Indices in the combined sample

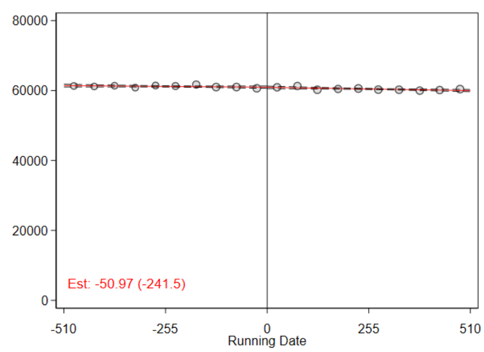
Panel A: Daily caseload density



Panel B: Predicted total recidivism



Panel C: Predicted cumulative earnings

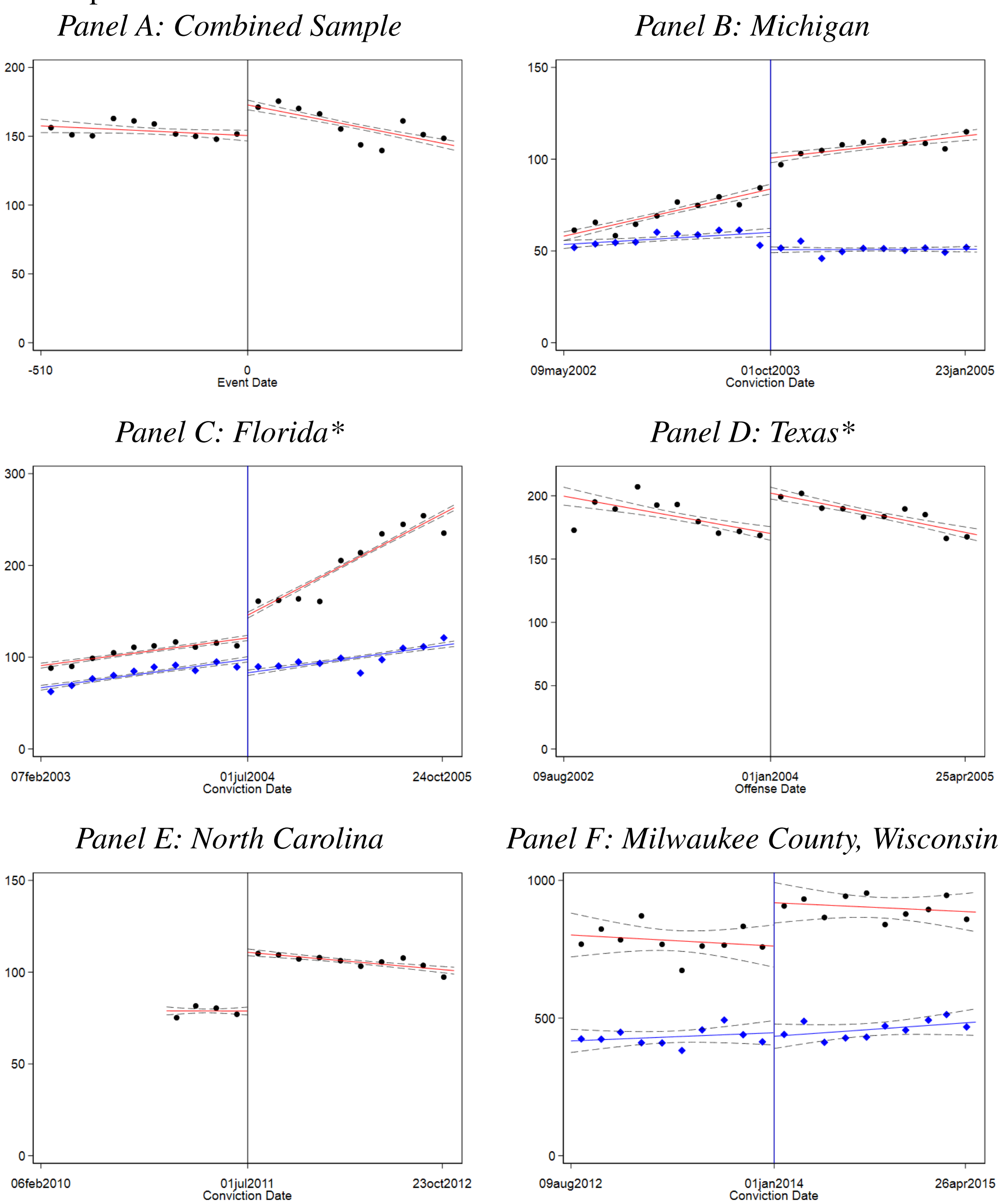


Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice 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.

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 2 for results in tabular format.

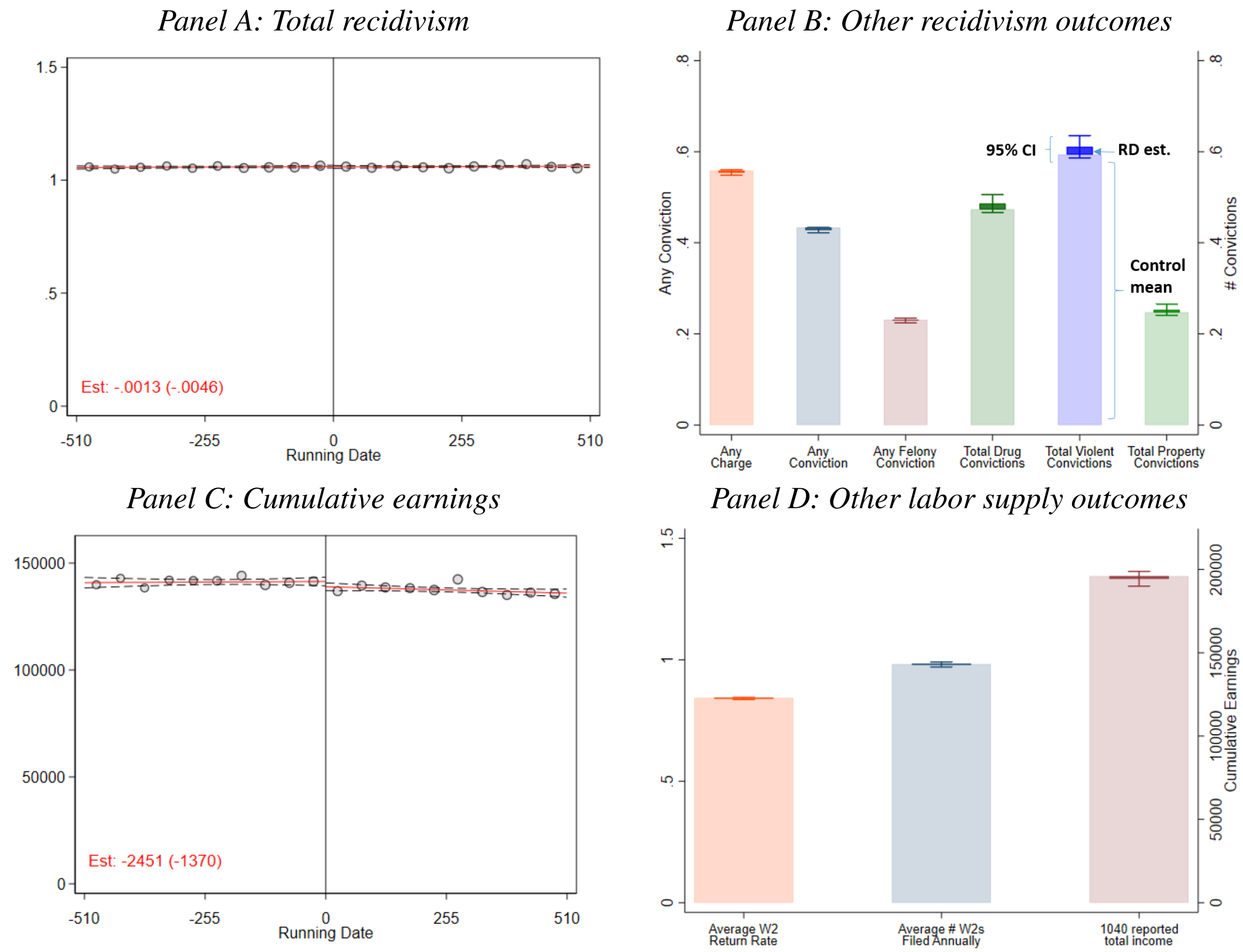
RD Figure Notes: Scatter points are binned using 51-day windows with the size of the circle denoting the number of observations within each bin. The black, solid vertical line denotes the cutoff. Predicted fit lines are generated using a sharp, linear RDD where event date is the running variable. Sharp RDD estimated fit lines are in solid pattern and red color with 95% confidence intervals in dashed pattern and black color.RD specification choices are described in Section 4.1. The estimation sample for each state is described in Section 3 and Table 1.

Figure 2: Total financial sanctions assigned upon disposition and total future payments to date for the focal sample relative to the effective date



Source: Authors' calculations using the criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin in the CJARS 2022Q2 vintage.
Note: These figures show the visual representation of the sharp RDD estimates (solid, red) and 95% confidence intervals (black, dashed) of the impact of the increased sanctions on total sanctions assigned at disposition and on total paid to date. See Table 3 for the results in tabular form.
* indicates that it's a state subsample. See Appendix Online Appendix B: for more details.
RD specification choices are described in Section 4.1. The estimation sample for each state is described in Section 3 and Table 1. *RD Figure Notes* from Figure 1 apply.

Figure 3: 10 years Total Convictions and Total Earnings outcomes relative to the fine increases, combined sample

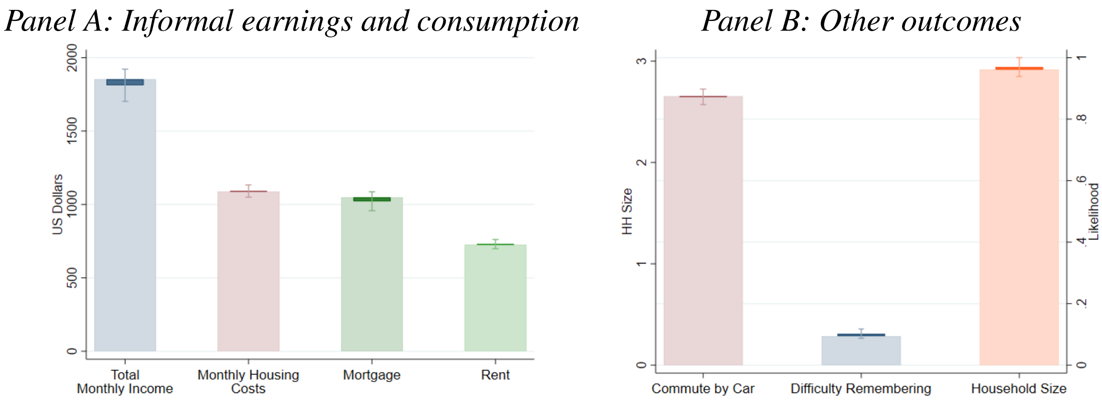


Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice 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.

Note: Panels A and B show the visual representation of the sharp RDD estimates (solid, red) and 95% confidence intervals (black, dashed) of the effect of the fine increases on the cumulative recidivism 10 years after the focal event (panel B) and total earnings measured using W-2 tax returns 10 years after the cutoff (panel A, adjusted to 2017 dollars using the CPI-All Urban). Outcome variables are residualized (with the mean from observations used in the RD estimate added back) using covariates described in Section 4. Outcomes in panels C and D are recidivism behavior 10 years after the focal event and labor outcomes measured using cumulative W-2 earnings 10 years after the cutoff. 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 Table 4 for the results in tabular format.

RD specification choices are described in Section 4.1. The estimation sample for each state is described in Section 3 and Table 1. *RD Figure Notes* from Figure 1 apply.

Figure 4: Causal impact of increased sanctions on informal earnings, consumption, and mental health - 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.

Note: This figure presents the sharp RDD estimates measuring the effects of the increased sanctions on self-reported monthly housing expenditures (housing costs, rent, mortgage), total income, likelihood of commuting by car, difficulty remembering due to cognitive disability, and household size. All outcomes are measured using the 2005–2020 ACS. Spending and income are CPI adjusted to 2017 dollars using the CPI-All Urban. See Section 5 for details on generating the population weights. The RD estimates (darker shade) are plotted on top of the control means (lighter shade) along with the 95% confidence intervals (vertical line on estimate). See Table 4 for the results in tabular format.

RD specification choices are described in Section 4.1. The estimation sample for each state is described in Section 3 and Table 1.

Tables

Table 1: Sample restrictions and fine changes

	Florida	Michigan	North Carolina	Texas	Wisconsin
Offense Grade	All charges	Felony charges	Felony charges	Misd. charges	Misd. charges
Sample exclusions	None	No DRF eligible offenses	None	No DRF eligible offenses	None
Counties included	Duval, Leon, Hillsborough, Miami-Dade, Orange	All	All	Bexar, Dallas, Hidalgo, Tarrant	Milwaukee only
Fine increase	$65	$60*	$54	$83*	$200
Statue	SB 2962	HB 4732	HB 200	HB 2424	AB 40
Effective Date (m/d/y)	7/1/2004	10/1/2003	7/1/2011	1/1/2004	1/1/2014
Running variable	Conviction Date	Conviction Date	Conviction Date	Offense Date	Conviction Date
Payment data	Yes+	Yes	No	No	Yes

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). DRF stands for driver responsibility fee. We exclude DRF-eligible offenses in Michigan and Texas because they include additional sanctions ranging from $300 to $6,000 which were introduced around a similar time (Finlay et al. 2022).

Table 2: Evaluating balance of selected observable characteristics and predicted earnings and criminal activity in the aggregated and state-specific samples on the focal event

Sample →	Combined	Florida*	Michigan	North Carolina	Texas*	Wisconsin*
Caseload size:						
Average daily caseload	-24.16	-19.25	.8264	-3.367	3.049**	-.663
	(-16.73)	(-14.53)	(-4.555)	(-5.773)	(-1.145)	(-.619)
	[317.1]	[171.3]	[77.88]	[97.36]	[53.49]	[9.93]
ACS Weights	-16.24					
	(13.05)					
	[195.7]					
Summary Indicies:						
Predicted Income	-50.97	-83.41	85.58	512.3	-175	-451.6
	(-241.5)	(-429.5)	(-359.9)	(-551.4)	(-441.3)	(-2421)
	[63,150]	[77,880]	[38,160]	[27,950]	[59,190]	[79,140]
Predicted Recidivism	-0.001	-0.003	-0.017	0.001	0.002	0.052
	(0.005)	(0.006)	(0.014)	(0.017)	(0.009)	(0.032)
	[1.046]	[.723]	[1.704]	[1.242]	[1.23]	[1.327]
Demographic Traits:						
Male	-0.002	0.004	-0.010	0.018*	-0.021**	0.003
	(0.003)	(0.004)	(0.007)	(0.009)	(0.007)	(0.022)
	[0.730]	[0.710]	[0.787]	[0.734]	[0.724]	[0.766]
White, non Hispanic	0.002	0.006	-0.011	-0.017*	0.016**	-0.009
	(0.003)	(0.005)	(0.008)	(0.010)	(0.008)	(0.025)
	[0.566]	[0.576]	[0.613]	[0.507]	[0.532]	[0.412]
Black	-0.003	-0.006	0.009	0.017*	-0.017**	0.015
	(0.003)	(0.004)	(0.008)	(0.010)	(0.007)	(0.026)
	[0.299]	[0.291]	[0.317]	[0.411]	[0.228]	[0.465]
Hispanic	-0.000	0.000	0.000	0.003	-0.000	-0.015
	(0.002)	(0.003)	(0.003)	(0.004)	(0.006)	(0.013)
	[0.098]	[0.098]	[0.027]	[0.033]	[0.212]	[0.070]
Age at Focal Event	-0.051	0.076	-0.120	0.113	-0.286*	-0.691
	(0.078)	(0.117)	(0.190)	(0.238)	(0.173)	(0.582)
	[31]	[31]	[30]	[31]	[29]	[31]
Criminal History:						
Total prior convictions	0.017	-0.006	-0.035	0.101**	0.038*	0.094
	(0.011)	(0.009)	(0.037)	(0.050)	(0.020)	(0.084)
	[0.589]	[0.205]	[1.510]	[1.530]	[0.336]	[0.550]
Any prior convictions	-0.002	-0.002	-0.019**	0.006	0.002	0.031
	(0.003)	(0.003)	(0.008)	(0.010)	(0.006)	(0.022)
	[0.224]	[0.094]	[0.545]	[0.537]	[0.135]	[0.210]
Pre-conviction 1040 information:						
Pre-Disposition avg.	-227.3	-398.6	-61.62	286.3	-216	-354.6
1040 Income	(268.9)	(505)	(452.8)	(395.5)	(442.8)	(1262)
	[16,000]	[18,000]	[14,000]	[9,400]	[14,000]	[15,000]
Observations	349,000	176,000	59,500	48,000	58,500	6,300

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.

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. See Section 4.1 for creation of predicted indices and Section 5.3 for details on our ACS reweighting strategy.

* indicates that it's a state subsample. See Appendix Online Appendix B: for more details.

Table 3: Evaluating change in total fines assigned upon disposition and total payments to date in the analysis sample upon fine increase implementation

Sample →	Combined	Florida*	Michigan	North Carolina	Texas*	Wisconsin*
Panel A: Total sanctions assigned						
	22.44***	24.8***	16.93***	32***	31.9***	157.4***
	(1.30)	(2.22)	(1.82)	(1.44)	(3.63)	(54.3)
	[112.84]	[106.04]	[71.53]	[78.86]	[183]	[782.15]
Observations	395,930	207,735	68,045	62,599	50,484	7,067
Panel B: Total paid to date						
		-14.679***	-9.49***			-12.73
		(2.115)	(1.39)			(32.018)
		[82.29]	[56.84]			[431.41]
Observations		146,204	35,762			6,245
Panel C: Total days in probation for the probation spell associated with the focal disposition						
	-8.57	-80.19***	-11.71	33.97	-14.98	30.85
	(8.64)	(31.67)	(13.61)	(22.54)	(15.49)	(51.87)
	[934.02]	[650.82]	[582.59]	[2735]	[715.33]	[609.40]
Observations	79,651	9,935	24,970	14,805	28,456	1,485

Source: Authors' calculations using the criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin in the CJARS 2022Q2 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; in panel C, observations are conditional on being observed in the probation data. See Section 3 along with Table 1 for more details on sample restrictions.
* indicates that it's a state subsample. See Appendix Online Appendix B: for more details.
RD Notes from Table 2 apply. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Table 4: Impacts of increased financial sanctions on administrative and survey outcomes

Data source →	Administrative Data		Survey Responses
Recidivism:		**Housing Expenditures:**	
Any charges	-0.004	Monthly	3.058
	(0.003)	housing costs	(21.45)
	[0.559]		[1,088]
Any convictions	-0.006*	Monthly	-26.1
	(0.003)	mortgage	(32.76
	[0.434]		[1,048]
Any felony	-0.002	Monthly	3.524
	(0.003)	Rent	(16.01)
	[0.231]		[726.3]
Total convictions	0.047*	**Measure of well-being**	
	(0.027)	Serious difficulty	0.008
	[2.129]	concentrating,	(0.008)
Total drug convictions	0.013	remembering,	[0.094]
	(0.010)	or making	
	[0.473]	decisions due to	
Total property convictions	0.017	a mental, physical	
	(0.012)	or health condition	
	[0.594]		
Total violent convictions	0.006		
	(0.006)		
	[0.247]		
Employment:		**Total income**	
Total earnings	-2,451*	Total earnings	-487.3
	(1,370)		(672.8)
	[141,300]		[22,230]
Average employment rate per year	-0.002		
	(0.002)	**Household circumstances**	
	[0.843]	Commutes	-0.002
Average number of employers per year	-0.002	by car	(0.013)
	(0.005)		[0.874]
	[0.982]	Household	0.024
Total household earnings	-1,522	size	(0.048)
	(2,256)		[2.917]
	[195,900]		
Observations	349,000	Responses*	23,000

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.

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. (2022) for details on offense classification. All survey outcomes are measured using the 2005–2020 ACS.

*Number of responses varies due to differences in the number of responses across questions. See Table A3 for the response rates for each question.

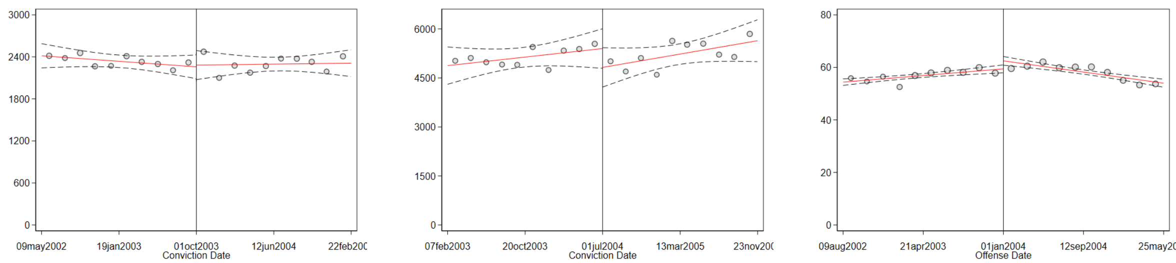
RD Notes from Table 2 apply. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Online Appendix A: Supplementary Results

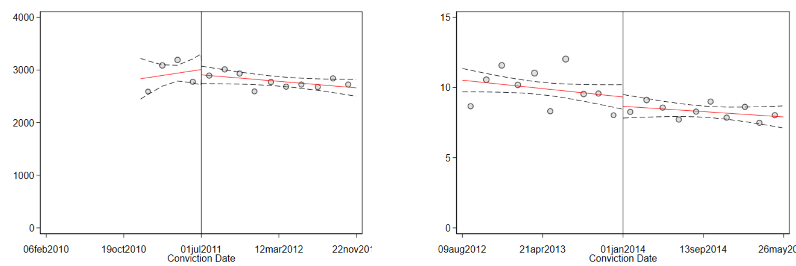
Online Appendix A:.1 Appendix Figures

Figure A1: Balance in Monthly Caseload Density

Panel A: State-specific caseload density of analysis sample relative to effective dates



i) Michigan *ii) Florida** *iii) Texas**



iv) North Carolina *v) Milwaukee County, Wisconsin*

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.

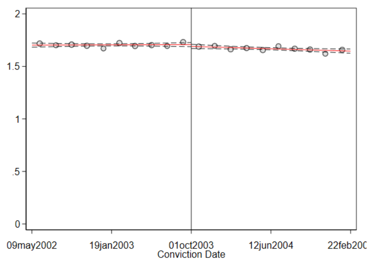
Note: This figure presents the sharp RDD estimates for the effects of the fine increase on total caseload density. See Table 2 for results in tabular format.

* indicates that it's a state subsample. See Appendix Online Appendix B: for more details.

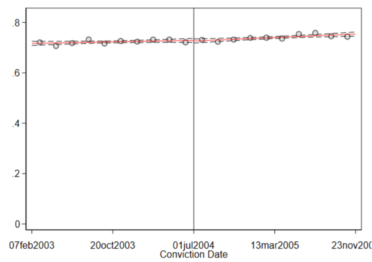
RD Figure Notes: Scatter points are binned using 51-day windows with the size of the circle denoting the number of observations within each bin. The black, solid vertical line denotes the cutoff. Predicted fit lines are generated using a sharp, linear RDD where event date is the running variable. Sharp RDD estimated fit lines are in solid pattern and red color with 95% confidence intervals in dashed pattern and black color.RD specification choices are described in Section 4.1. The estimation sample for each state is described in Section 3 and Table 1.

Figure A2: Balance in Predicted Cumulative Recidivism, 1–5 years after focal event

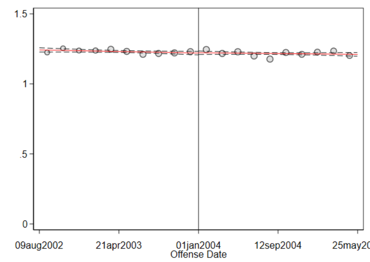
Panel A: State-specific predicted cumulative recidivism of analysis sample relative to effective dates



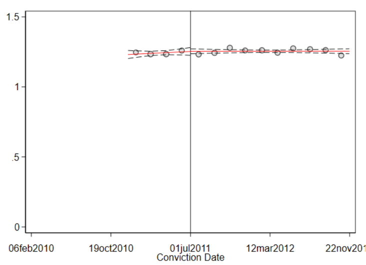
i) Michigan



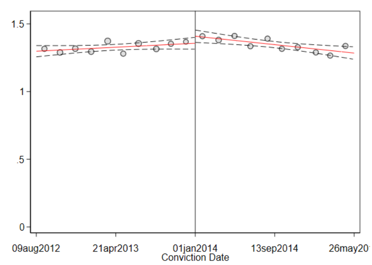
*ii) Florida**



*iii) Texas**



iv) North Carolina



v) Milwaukee County, Wisconsin

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.

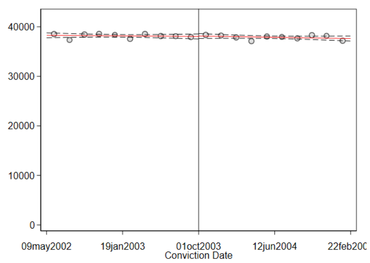
Notes: These figures show the visual representation of the sharp RDD estimates (solid, red) and 95% confidence intervals (black, dashed) of the effect of the increased fines on predicted total recidivism 1–5 years after the focal event. See Section 4.1 for the creation of predicted indices. See Table 2 for results in tabular format.

* indicates that it's a state subsample. See Appendix Online Appendix B: for more details.

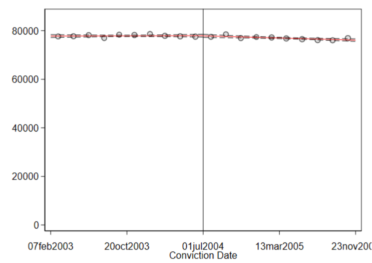
RD specification choices are described in Section 4.1. The estimation sample for each state is described in Section 3 and Table 1. *RD Figure Notes* from Figure 1 apply.

Figure A3: Balance in Predicted Cumulative W-2 income, 1–5 years after the cutoff

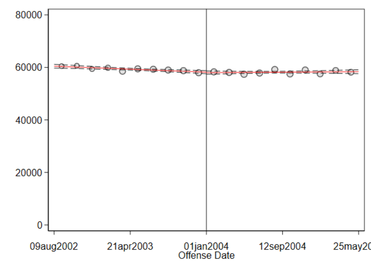
Panel A: State-specific predicted cumulative W-2 income of analysis sample relative to effective dates



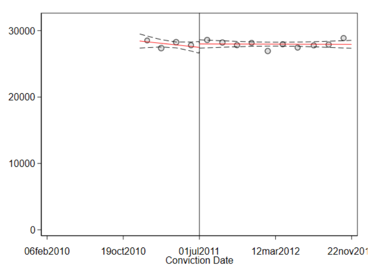
i) Michigan



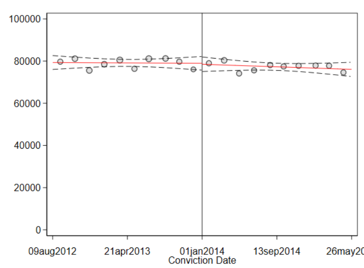
*ii) Florida**



*iii) Texas**



iv) North Carolina



v) Milwaukee County, Wisconsin

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.

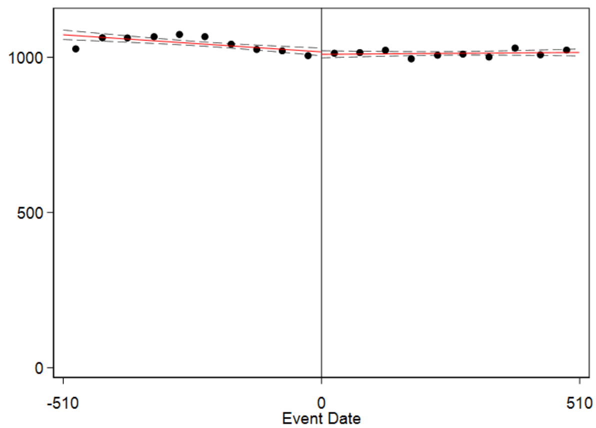
Notes: These figures show the visual representation of the sharp RDD estimates (solid, red) and 95% confidence intervals (black, dashed) of the effect of the increased fines on predicted total earnings 1–5 years after the cutoff measured using W-2 tax returns (adjusted to 2017 dollars using the CPI-All Urban). See Section 4.1 for the creation of predicted indices. See Table 2 for results in tabular format.

* indicates that it's a state subsample. See Appendix Online Appendix B: for more details.

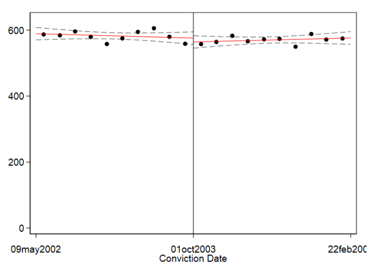
RD specification choices are described in Section 4.1. The estimation sample for each state is described in Section 3 and Table 1. *RD Figure Notes* from Figure 1 apply.

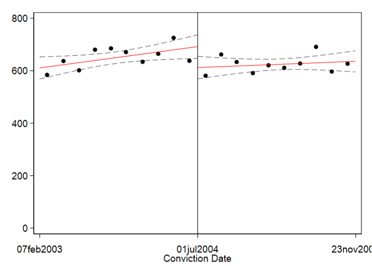
Figure A4: Sharp RD estimates of the impact of the increased monetary sanctions on number of days observed in the probation spell associated with the focal sentence in the ten years following the focal event

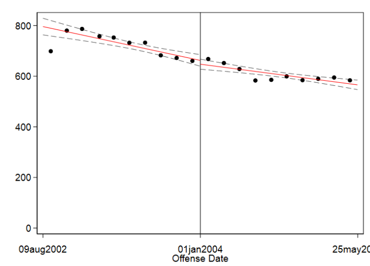
Panel A: Combined Sample



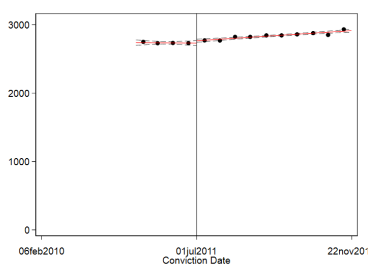
Panel B: State-specific estimates relative to effective dates

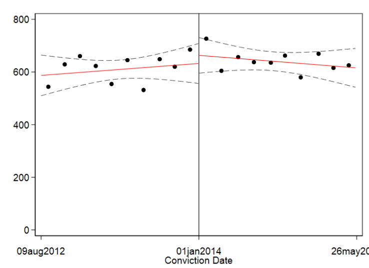






i) Michigan *ii) Florida** *iii) Texas**





iv) North Carolina *v) Milwaukee County, Wisconsin*

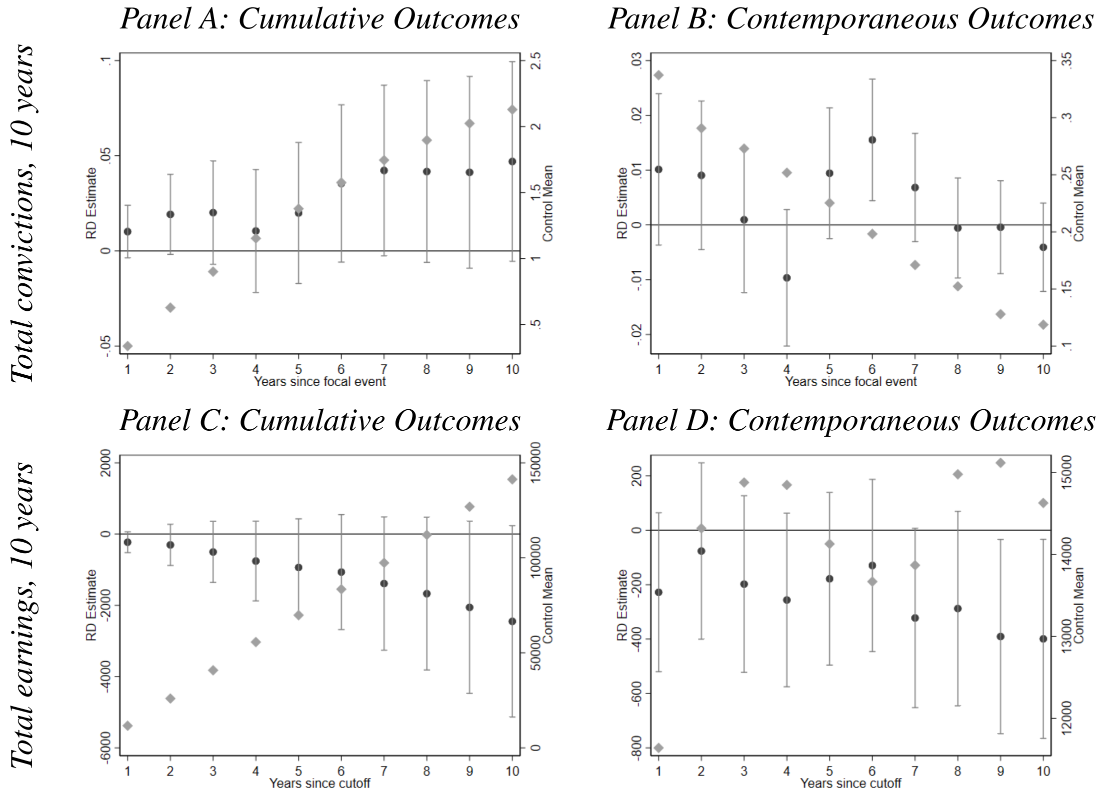
Authors' calculations using the criminal justice histories from Florida, Michigan, North Carolina, Texas, and Wisconsin in the CJARS 2022Q2 vintage.

Note: These figures show the visual representation of the sharp RDD estimates (solid, red) and 95% confidence intervals (black, dashed) for the number of days an individual is observed in probation (first spell post focal event) relative to the focal event.

* indicates that it's a state subsample. See Appendix Online Appendix B: for more details.

RD specification choices are described in Section 4.1. The estimation sample for each state is described in Section 3 and Table 1. *RD Figure Notes* from Figure 1 apply.

Figure A5: Evolution of RD-based causal estimates over the 10 year followup period - Combined Sample

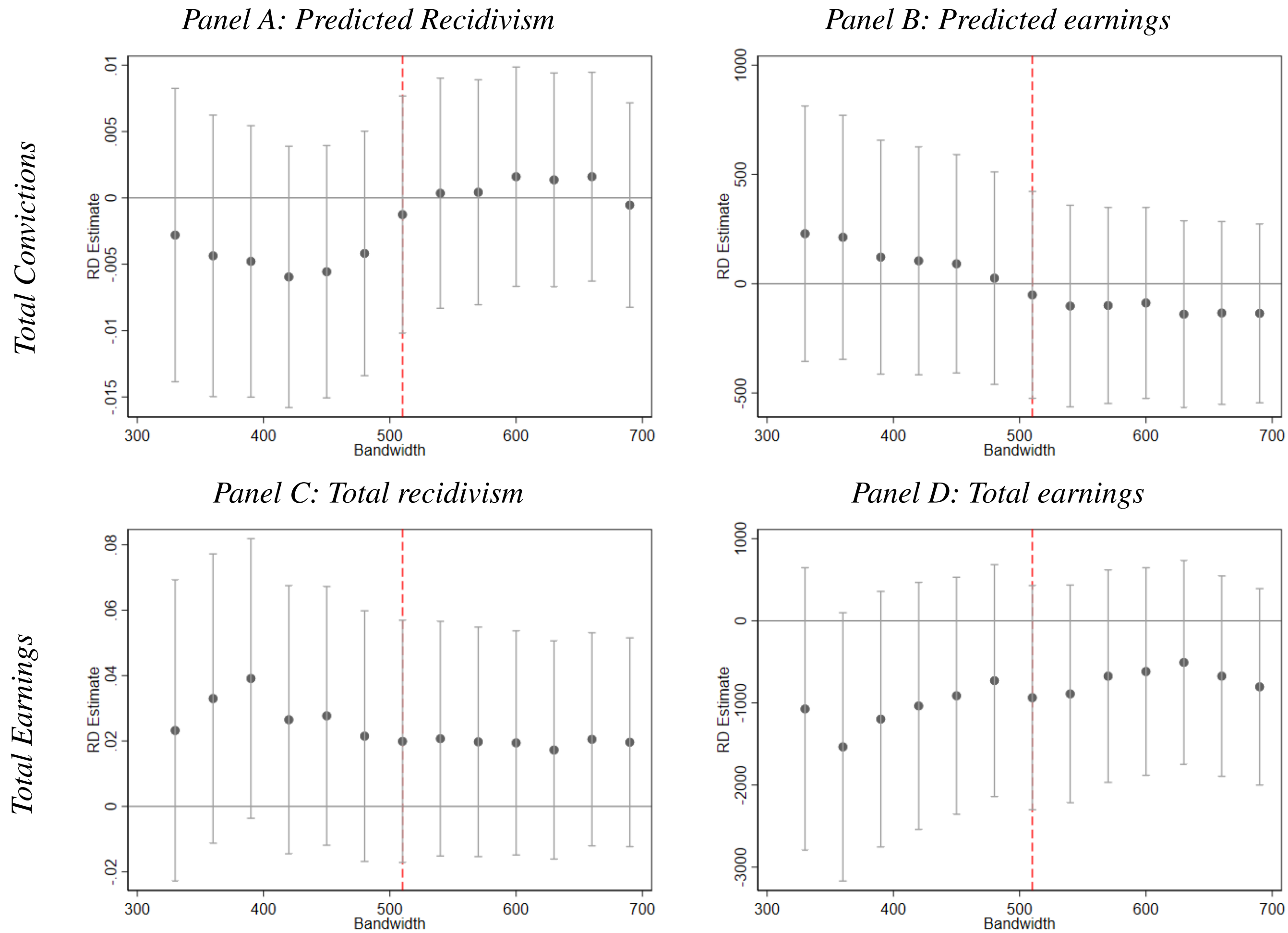


Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice 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.

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 the recidivism outcomes (panels A and B) the time frame is between 1 and 10 years following disposition of first eligible offense. Total 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. The control means are also included for each outcome variable (light grey, diamonds). All RD estimates are shown with 95% confidence intervals.

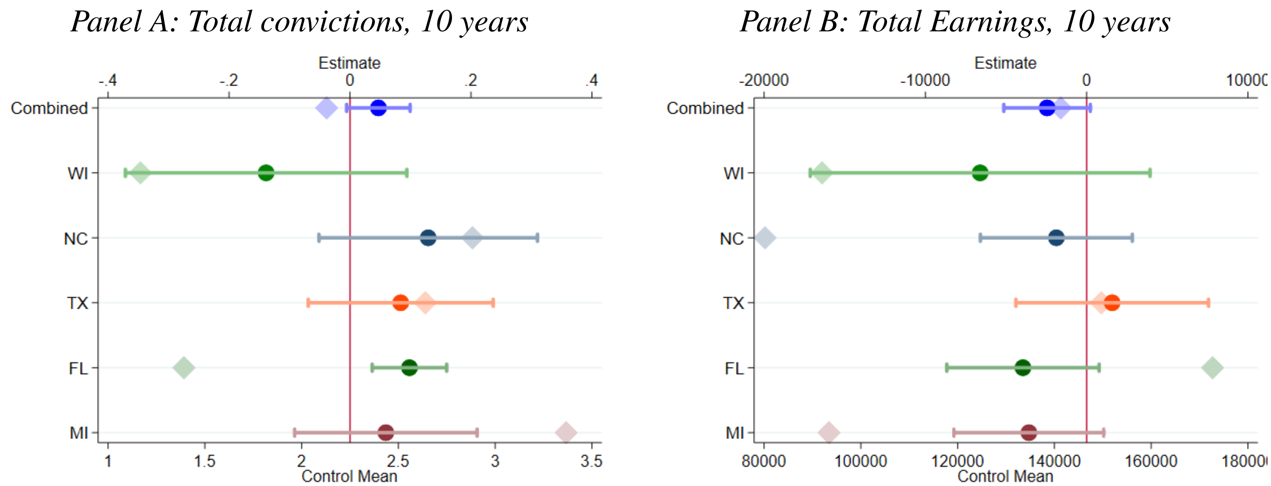
RD specification choices are described in Section 4.1. The estimation sample for each state is described in Section 3 and Table 1.

Figure A6: Robustness of balance in predicted indices and main results to varied bandwidths



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.
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 state is described in Section 3 and Table 1.

Figure A7: Sharp RD estimates of the impact of the increased monetary sanctions on total recidivism and cumulative earnings by state and combined sample

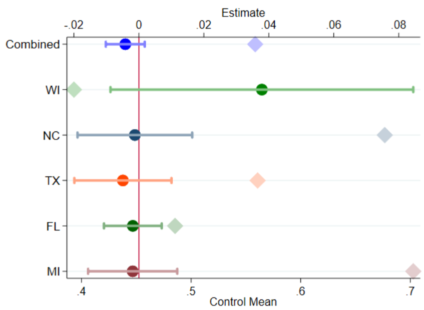


Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice 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.

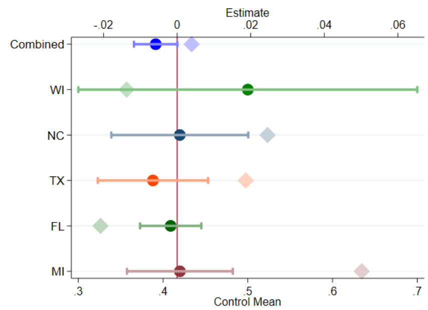
Note: This figure presents the sharp RDD estimates measuring the effects of the increased sanctions for each state specific sample and combined for recidivism and labor market outcomes measured 10 years after the focal event and cutoff, respectively. Total earnings is measured using income reported on W-2 tax returns. Each of the RD estimates (circle) are plotted with the 95% confidence intervals along with the control mean (diamonds).

RD specification choices are described in Section 4.1. The estimation sample for each state is described in Section 3 and Table 1.

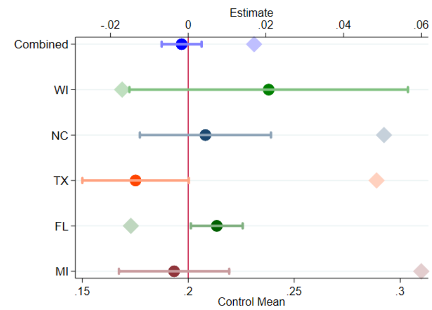
Figure A8: Sharp RD estimates of other recidivism outcomes, 1–10 years after focal event



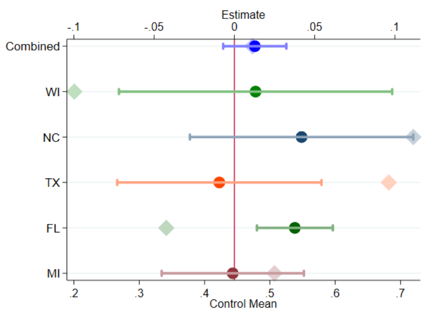
i) Likelihood of Charge



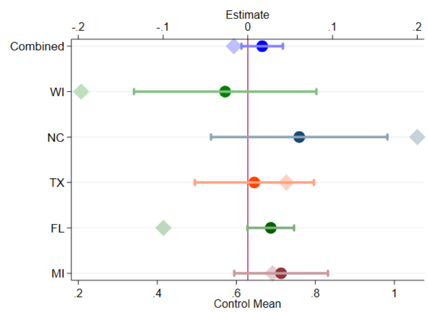
ii) Likelihood of Conviction



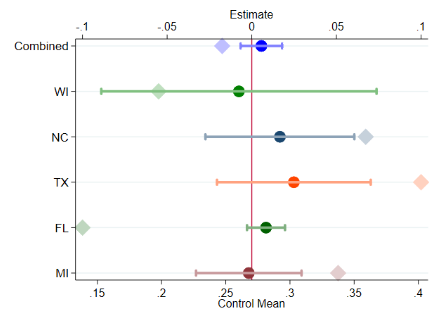
iii) Likelihood of Felony Conviction



i) Total Drug Convictions



ii) Total Property Crime Convictions



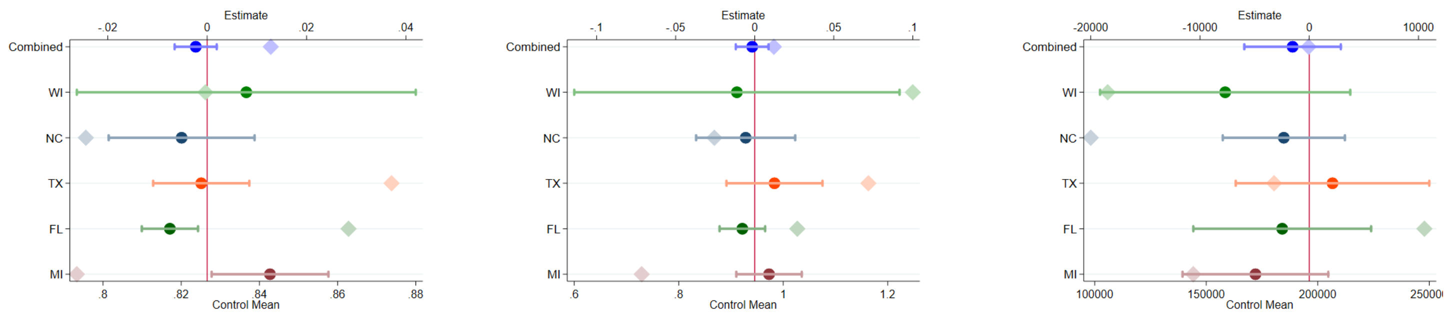
iii) Total Violent Crime Convictions

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.

Note: This figure presents the sharp RDD estimates measuring the effects of the increased sanctions for each state specific sample and combined for different recidivism outcomes measured 10 years after the focal event. See Choi et al. (2022) for description on offense classification. Each of the RD estimates (circle) are plotted with the 95% confidence intervals along with the control mean (diamonds).

RD specification choices are described in Section 4.1. The estimation sample for each state is described in Section 3 and Table 1.

Figure A9: Sharp RD estimates of other labor outcomes, 1–10 years after the cutoff



i) Average W-2 Return Rate *ii) Average # of W-2s Filed Annually* *iii) 1040 reported cumulative Income*

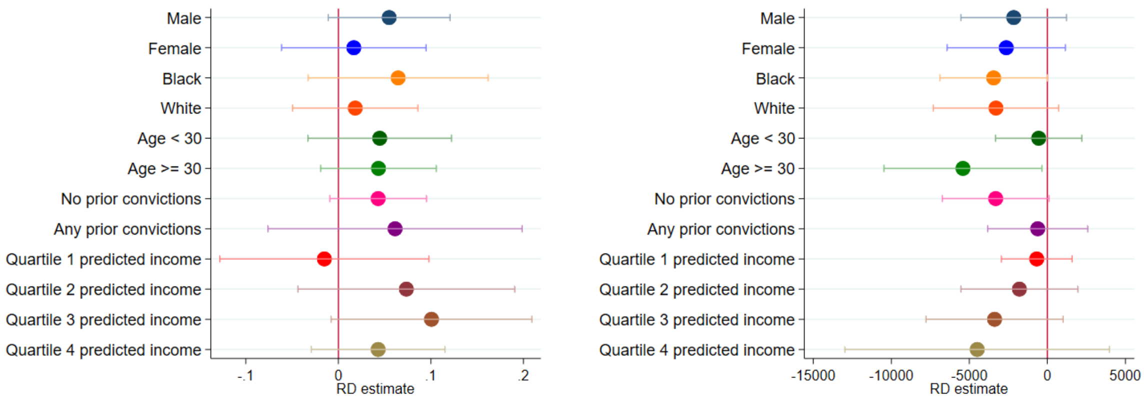
Source: Authors' calculations from 1998–2020 IRS 1040 individual tax returns, 2005–2020 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, the 2005–2020 ACS survey responses, and criminal justice 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.

Note: This figure presents the sharp RDD estimates measuring the effects of the increased sanctions for each state specific sample and combined for different labor market outcomes measured 10 years after the cutoff. 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. Each of the RD estimates (circle) are plotted with the 95% confidence intervals along with the control mean (diamonds).

RD specification choices are described in Section 4.1. The estimation sample for each state is described in Section 3 and Table 1.

Figure A10: Causal impact of increased sanctions on future earnings and convictions by subgroup - Combined Sample

Panel A: 10 year total recidivism *Panel B: 10 year total earnings*



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.

Note: This figure presents the sharp RDD estimates for the effects of the increased financial sanctions on total convictions (panel A) and total earnings (adjusted to 2017 dollars using the CPI-All Urban, panels B) measured using W-2 tax returns across various subgroups of the individual, noted in the Y-axis. RD estimates are plotted on the graph (diamonds); 95% confidence intervals are included and plotted behind the estimate. Racial identity is measuring using the Census' 'bestrace' file. Sex and age at conviction 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 event. See Section 4.1 for the creation of predicted indices.

RD specification choices are described in Section 4.1. The estimation sample for each state is described in Section 3 and Table 1.

Online Appendix A:.2 Appendix Tables

Table A1: Local Polynomial and Sharp RD, with no covariates, estimates of main outcomes

Outcome	Identification strategy→	Non-parametric estimation	No Covariates
Total earnings, 1–5 years		-1,649	-1,314
		(1,737)	(818)
		[71,960]	[70,190]
Total convictions, 1–5 years		0.075	0.029
		(0.060)	(0.020)
		[1.337]	[1.374]

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.

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 total earnings 1–5 years after the cutoff and total recidivism 1–5 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.

RD Notes from Table 2 apply. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Table A2: Impacts of increased financial sanctions on 10 years total earnings and recidivism outcomes, state specific estimates

Sample →	Florida	Michigan	North Carolina	Texas	Wisconsin
Recidivism measure:					
Any charges	-0.002	-0.002	-0.001	-0.005	0.038
	(0.005)	(0.007)	(0.009)	(0.008)	(0.024)
	[0.485]	[0.703]	[0.677]	[0.561]	[0.393]
Any convictions	-0.002	0.001	0.001	-0.007	0.019
	(0.004)	(0.007)	(0.009)	(0.008)	(0.024)
	[0.326]	[0.635]	[0.523]	[0.498]	[0.357]
Total convictions	0.098^{**}	0.059	0.129	0.084	-0.139
	(0.031)	(0.077)	(0.092)	(0.078)	(0.119)
	[1.392]	[3.367]	[2.883]	[2.639]	[1.167]
Any drug convictions	0.038^{**}	-0.001	0.042	-0.009	0.013
	(0.012)	(0.023)	(0.036)	(0.032)	(0.043)
	[0.341]	[0.507]	[0.720]	[0.682]	[0.201]
Any property convictions	0.027^{*}	0.039	0.061	0.008	-0.027
	(0.014)	(0.028)	(0.053)	(0.036)	(0.055)
	[0.415]	[0.692]	[1.058]	[0.727]	[0.207]
Any violent convictions	0.008	-0.002	0.017	0.025	-0.008
	(0.006)	(0.016)	(0.022)	(0.023)	(0.041)
	[0.138]	[0.338]	[0.359]	[0.402]	[0.198]
Employment:					
Average employment rate per year	-0.007^{**}	0.013^{**}	-0.005	-0.001	0.008
	(0.003)	(0.006)	(0.007)	(0.005)	(0.017)
	[0.863]	[0.793]	[0.796]	[0.874]	[0.826]
Average number of employers per year	-0.008	0.009	-0.006	0.013	-0.011
	(0.007)	(0.011)	(0.016)	(0.016)	(0.053)
	[1.027]	[0.729]	[0.868]	[1.163]	[1.248]
Total earnings	-3,949	-3,579	-1,878	1,584	-6,603
	(2,409)	(2,370)	(2,403)	(3,047)	(5,375)
	[172,700]	[93,400]	[80,190]	[149,700]	[91,980]
Observations	176,000	59,500	48,000	58,500	6,300

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.

Note: This table presents the sharp RDD estimates on the impacts of the increased fines on labor market and recidivism outcomes for each state subsample. Recidivism behavior is measured 5 and 10 years after the focal event and labor outcomes are measured using cumulative W-2 earnings 5 and 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. (2022) for details on offense classification.

RD Notes from Table 2 apply. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Table A3: ACS response counts by question

ACS Question	Number of responses
Housing expenditures:	
Monthly housing costs	18,500
Monthly mortgage	7,100
Monthly rent	8,500
Measure of well-beings:	
Serious difficulty concentrating, remembering, or making decisions due to mental, physical, or health condition	23,000
Total income:	
Total earnings	23,000
Household circumstances:	
Commute by car	11,500
Household size	23,000

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.

Note: This table presents the observation counts for each of the ACS questions used in this study.

Table A4: 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
Cumulative W-2 earnings, 10 years	-2,155	-2,633	-3,441*	-3,293	-555.2	-5,409**	-3,312*	-617.2
	(1,726)	(1,933)	(1,757)	(2,050)	(1,411)	(2,583)	(1,745)	(1,636)
	[148,700]	[121,300]	[101,500]	[162,000]	[139,200]	[144,300]	[160,100]	[82,480]
Cumulative total convictions, 10 years	0.055	0.017	0.065	0.018	0.045	0.043	0.043	0.061
	(0.034)	(0.040)	(0.050)	(0.035)	(0.040)	(0.032)	(0.027)	(0.070)
	[2.402]	[1.404]	[2.570]	[1.920]	[2.714]	[1.308]	[1.527]	[4.011]
Observations	253,000	95,000	107,000	196,000	202,000	146,000	264,000	85,000

Panel B: Quartiles of Predicted Income Outcome Sample→	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Cumulative W-2 earnings, 10 years	-680.3	-1791	-3381	-4497
	(1,156)	(1,912)	(2,242)	(4,325)
	[53,860]	[96,640]	[142,200]	[263,000]
Cumulative total convictions, 10 years	-.01514	.07346	.1006*	.04296
	(.05764)	(.05977)	(.05534)	(.03684)
	[3.258]	[2.273]	[1.948]	[1.053]
Observations	96,000	72,000	86,500	94,000

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.
Note: This table presents the sharp RDD estimates on the impacts of the increased fines on labor market and recidivism outcomes by subgroups denoted in the column titles. 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. Racial identity is measuring using the Census' 'bestrace' file. Sex and age at conviction 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 event. See Section 4.1 for the creation of predicted indices.
RD Notes from Table 2 apply. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Online Appendix B: Data Appendix

Online Appendix B:.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.

Online Appendix B:.2 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.[25] 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.[26] We also break out recidivism measures based on offense type and offense grade using the offense classification system developed by Choi et al. (2022). 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.[27] Measuring income using W-2s is advantageous since they cover all formal employment, regardless of the length of employment spell. Thus, the number of

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

[26]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.

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

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.[28] 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. 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. We discuss our weighting strategy to circumvent this issue in Section 5.3.

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

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

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 C: Comparison of study findings with Giles (2021)

One of the five 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.

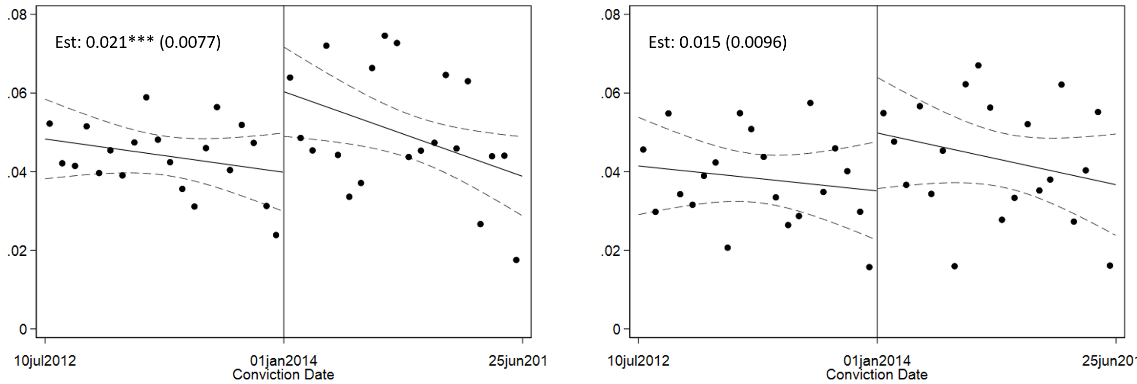
There are good motivations for including all defendants in the regression. In particular, the external validity of the findings for the entire caseload (including those who already hold a criminal history) is strengthened when incorporating the full caseload. At the same time, this choice potentially includes a degree of bias since individuals may 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 C1, we plot two graphs showing the the 1 year recidivism outcomes, defined as likelihood of new felony conviction, for misdemeanor defendants in Milwaukee, WI.[29] 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 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;

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

Figure C1: 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.

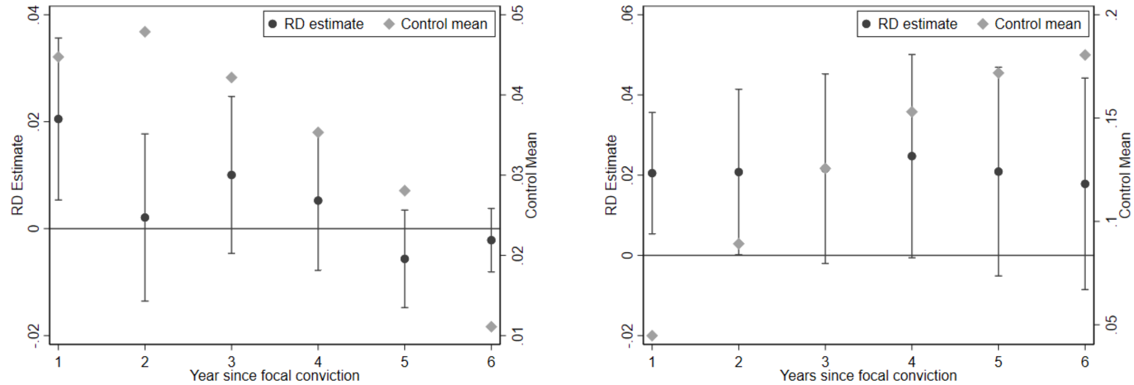
without the inclusion of the vertical line to identify the policy implementation data, it is not clear the naked eye could identify when the policy change occurred based on this figure alone.

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 C2, 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.

For the reasons laid out above, we conclude that the introduction of the $200 DNA fee in

Figure C2: 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.

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.