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

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Online Appendix

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

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

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

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

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

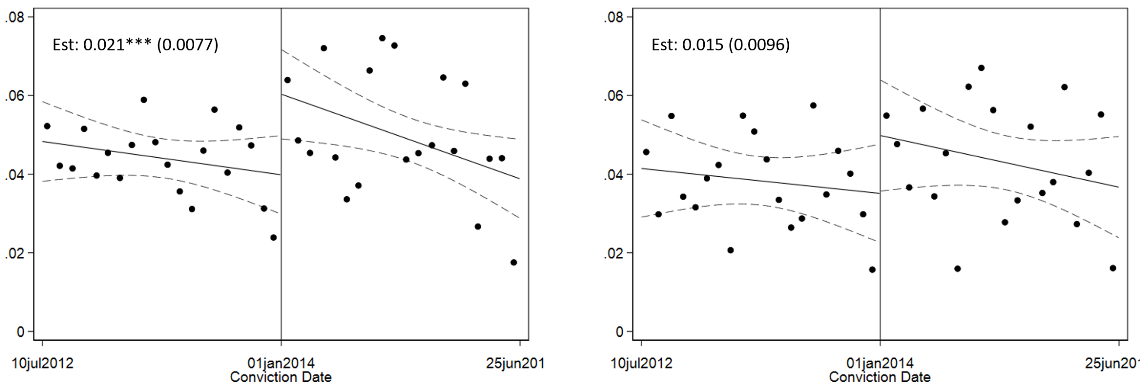
In Figure A1, we plot two graphs showing the the 1 year recidivism outcomes, defined as likelihood of new felony conviction, for misdemeanor defendants in Milwaukee, WI.[13] In the first graph, we replicate Giles (2021)'s findings through including all misdemeanor convictions in the running variable, 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

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

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

Panel A: All misdemeanor convictions, (Replication of (Giles 2021)) *Panel B: First misdemeanor convictions only*



Note: This figure presents the sharp RDD estimates for the effects of the $200 DNA fee surcharge enactment in Milwaukee County, Wisconsin. Panel A consists of all misdemeanor convictions within a 540 day window surrounding the policy reform date; panel B is restricted to first convictions that occurred within a 540 day window surrounding the policy reform date, which is our preferred specification. RDD point estimates with robust standard errors in parentheses are included in the top left of each graph.
RDD estimates are generated using Eq. 1. The black, vertical line denotes the policy reform date. Linear fitted lines from the binned averages are shown in solid, black with 95% confidence intervals in dashed black. Scatter points are binned using 27-day windows with the size of the circle denoting the number of observations within each bin.

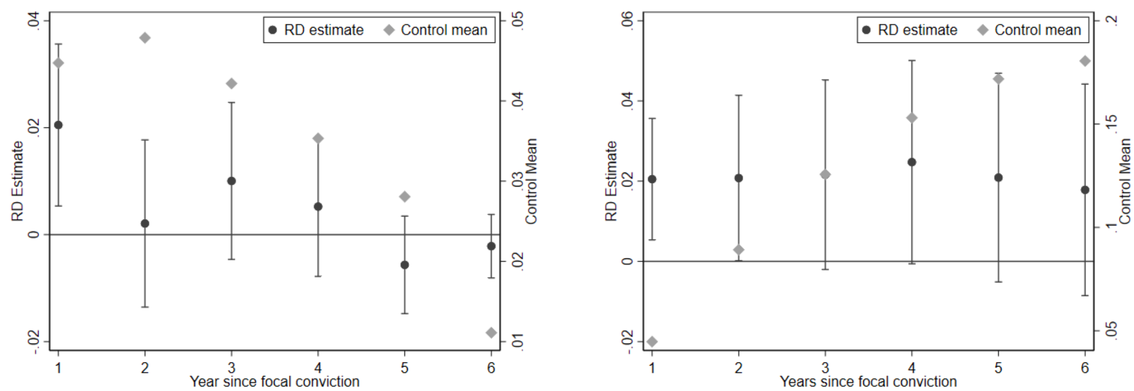
point estimate of the jump at the discontinuity that is highly statistically significant (p-value $<$ 0.01). The second panel is much less certain. The point estimate shrinks from 0.021 to 0.015, loses statistical significance for traditional threshold levels, and from a visual perspective becomes less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

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

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

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

Panel A: Contemporaneous likelihood of new felony conviction — *Panel B: Cumulative likelihood of felony conviction*



Note: This figure presents the sharp RDD estimates for the effects of the $200 DNA fee surcharge enactment in Milwaukee County, Wisconsin. Panels A and B show the RDD point estimates (black circles) with 95% confidence intervals using robust standard errors along with sample means for individuals disposed prior to the Wisconsin reform (gray diamonds).
RDD estimates are generated using Eq. 1. Estimating sample follows Giles (2021), which is restricted to individuals who have a misdemeanor conviction in Milwaukee County, Wisconsin in the 540 days surrounding the Wisconsin policy reform.

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

Online Appendix B Institutional details regarding fine increases

B.1 Florida Court Surcharge

In 1998, Florida amended its state constitution so that the state would pay a greater share of trial court funding by July 1, 2004 (Blankenship 2004). In order to fulfill the mandate, Governor, Jeb Bush signed Senate Bill 2962 into law on May 21, 2004, which allowed counties to impose $65 fees upon conviction. Florida's largest counties (Broward County, Duval County, Miami-Dade County, Escambia County, Hillsborough County, Leon County, and Orange County) immediately amended their local ordinances to implement the new fees.[14] These fines would apply to anyone convicted after June 30, 2004.

B.2 Michigan Driver Responsibility Fees

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

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

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

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

B.3 Michigan Minimum Cost

In 2003, Michigan faced a $1 billion deficit in the 2004 budget along with the highest unemployment rate in the nation (Holahan et al. 2004). In order to raise revenue, the state passed an array of bills focused on generating or diverting revenue from the criminal justice system to other branches of the government.[15] One of these bills, House Bill 4732 established a minimum cost upon conviction, ranging from $40–$45 for misdemeanor convictions to $60 for felony convictions. The minimum costs, however, absorbed other pre-existing court costs so that the marginal increase was less than $60 for those charged with a felony.

Michigan also passed Public Act 165 or the Driver Responsibility Fee (DRF), which applied only to traffic offenses.[16] We exclude all traffic offenses from this sub-experiment to prevent the confounding of our results from the DRF passage.

B.4 North Carolina Court Surcharge

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

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

[15] 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.

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

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

B.5 Texas Driver Responsibility Fees

In an effort to promote safer driving and increase state revenue, Texas passed House Bill 3588, or the Texas Driver Responsibility Program, on June 2, 2003. The law, which became effective on September 1, 2003, mandated new fines to defendants who were convicted of certain driving crimes. The Texas Department of Public Safety (DPS), which oversees Texas' Highway patrol, would enforce the fines and receive 1% of revenue (Price 2008). The remaining revenue was evenly split between the state's trauma system and the Texas Mobility Fund.[17] At the time, Texas' trauma system was seriously underfunded and overstretched with only 15.83 emergency departments and 8.14 trauma centers per one million people (Price 2008). Similar to other states' version of the DRF, the fines would be classified as administrative fines, rather than criminal penalties.

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

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

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

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

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

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

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

B.6 Texas Fine Consolidation

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

Because the effects of the cost consolidation were heterogeneous across counties depending on the pre-existing cost structure, we focus our analysis on six out of the eight most populous counties in Texas (Bexar, Dallas, Harris, Hidalgo, Tarrant, and Travis). These counties represent approximately 33% of the total Texas population. The inclusion of counties is restricted to confirm existing cost structures, which are not publicly archived, as well as our ability to measure fines and fees in criminal charge data in CJARS.[18]

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

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

B.7 Wisconsin DNA Surcharge

In the summer of 2013, then Wisconsin Governor Scott Walker proposed expanded DNA collection for individuals convicted of any crime as part of his 2014 state budget proposal. Prior to his proposal, DNA collection was only taken from people convicted of felonies with a $250 surcharge along with select misdemeanors. Anticipating the increase in DNA testing demand for state labs, the proposal also included a new $200 surcharge for all misdemeanor convictions that would go into effect at the same time. Specifically, individuals convicted of misdemeanor offenses after January 1, 2014 now faced an additional surcharge of $200 to help fund the expanded DNA testing.[19]

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

Online Appendix C Driver responsibility programs in the United States

In the early 2000s, many states were facing high rates of DUI fatalities and budget shortfalls. Thus, in an effort to solve both of these issues, states such as Michigan, New York, Texas, and Virginia passed driver responsibility fee programs modeled after New Jersey's 1983 Merit Rating Plan Surcharges (Price 2008; Wild 2008; Adair 2013). These programs assigned sizable financial penalties to drivers that either exceeded a threshold of traffic infractions or were convicted of certain traffic offenses. By 2008, over 44 million drivers, or 21% of all licensed drivers, in the United States were at risk of receiving a DRF penalty (*Highway Statistics Series* 2008).[20]

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

In general, driver's license suspension is a commonly utilized form of punishment in the criminal justice system in the United States. Driver's license suspension can also be triggered by drug conviction, failure to comply with a court order, failure to pay civil infractions such as traffic tickets,[21] failing to maintain auto insurance, and failure to pay child support. The high use of driver's license suspension is not unusual when compared to states that did not adopt the DRF program. According to a 2017 report by the Legal Aid Justice Center, 43 states suspend driver's licenses due to unpaid court debt with suspension only lifted upon payment; 18 out of the 43, including Michigan and Texas, suspend licenses automatically after the payment deadline (Salas and Ciolfi 2017). Similar to Michigan and Texas, most states do not require considering ability to pay prior to driver's license suspension. The Fines and Fees Justice Center estimates 11 million individuals in the United States have their license suspended due to unpaid court debt (Keneally 2019).

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

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

Online Appendix D Data Appendix

D.1 State specific data restrictions

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

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

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

D.2 Measuring assessed sanctions

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

D.3 Outcome measurement

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

We define recidivism using the time length between the focal disposition and future offense date or filing date.[22] 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.[23] We also break out recidivism measures based on offense type and offense grade using the offense classification system developed by Choi et al. (2023). This helps further unify our analysis since the offense types, such as property, violent, are defined under a harmonized system.

To measure labor market outcomes such as earnings and employment, we use the IRS W-2 information returns from 2005–2020.[24] Measuring income using W-2s is advantageous since they cover all formal employment, regardless of the length of employment spell. Thus, the number of W-2 tax returns filed per year on behalf of the individual can be a measure for the number of jobs an individual worked. Furthermore, W-2s are filed by the employer, not the employee, and so are not affected by endogenous tax filing behavior such as in IRS 1040 individual tax returns. Because of this, we use W-2 information returns as our main measure of annual earnings even though we have 1040 tax filings beginning in 1998. We do include 1040 tax filings as a measure of household earnings.

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

To alleviate the first issue of informal employment, we also use self-reported total income on the 1-year American Community Surveys from 2005–2020. This total income measure encompasses income earned from wages, self-employment, Social Security Income, and others.[25] 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.

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

[23]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.

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

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

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

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

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

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

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

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

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

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

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

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

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

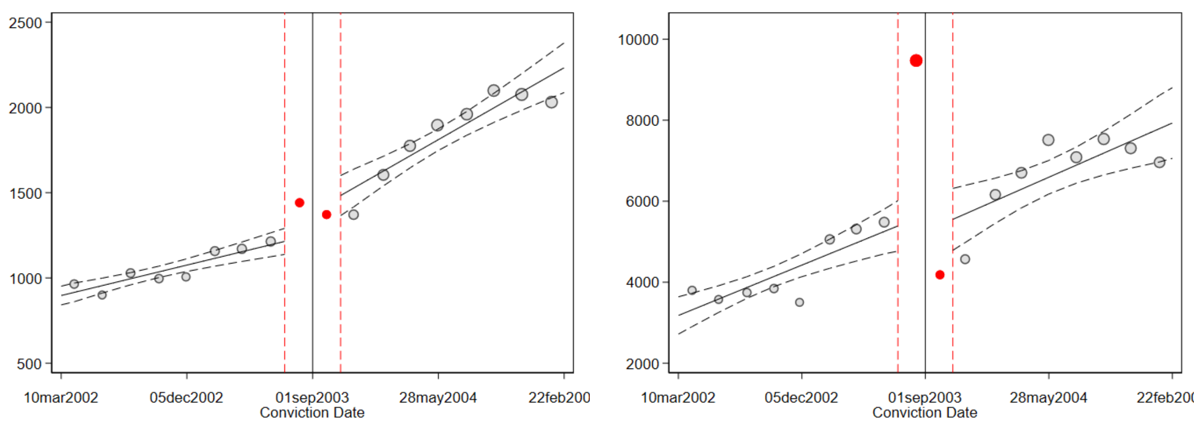
In either case, these figures imply disparate treatment in race and gender in the justice system, an interpretation supported by Doleac (2017), Abrams, Bertrand, and Mullainathan (2012), Arnold, Dobbie, and Yang (2018), Alesina and La Ferrara (2014), Arnold, Dobbie, and Yang (2018), and Depew, Eren,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

Interestingly, we do not observe the same type of behavior in Michigan, as shown in Figure G2 panels H and I. While this is not causal evidence, one institutional difference between Michigan and Texas is the administration of the DRFs. Specifically, in Texas, DPS oversaw the program and received a portion of the revenue, suggesting incentives to over-charge driver's with DRF-eligible offenses (Makowsky, Stratmann, and Tabarrok 2019; Price 2008). DPS could further increase their revenue by charging the driver's least able to contest these charges, a hypothesis supported by Makowsky and Stratmann (2009, 2011). We observe modest evidence of this with increasing caseload density in Figure E1 panels A and B. We do not observe a similar rise in Michigan.

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

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

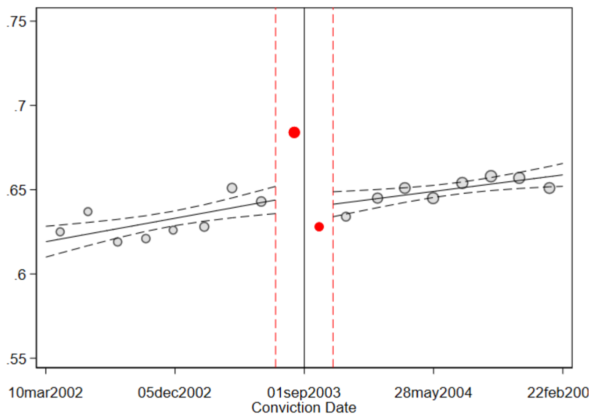
Panel A: Average 60-day non-DUI caseload *Panel B: Average 60-day DUI caseload*



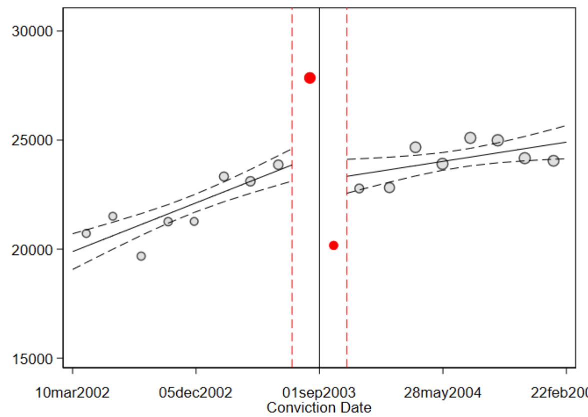
Note: These figures present the sharp RDD estimates of the effects of the DRFs on the daily caseload for non-DUI DRF-related offenses (panel A) and for DUI DRF related offenses (panel B) in Texas. The sample is restricted to individuals whose first DRF-qualifying conviction occurred within the 540 days surrounding the policy reform date excluding the 60 days surrounding the policy reform date.
RDD Figure Notes: Scatter points are binned using 60-day windows with the size of the circle denoting the number of observations within each bin. Linear fitted lines from the binned averages are shown in solid pattern with 95% confidence intervals using robust standard errors in dashed pattern. The black, solid vertical line denotes the policy reform date. The red, dashed, vertical line denotes the donut (60-day window surrounding the policy reform date; Texas only). Red data points (Texas only) reflect excluded observations within the donut and are provided for completeness even though they do not contribute to RDD estimates.

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

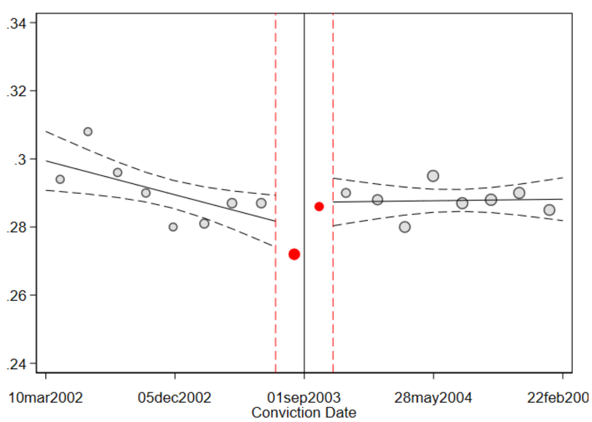
Panel A: Proportion White



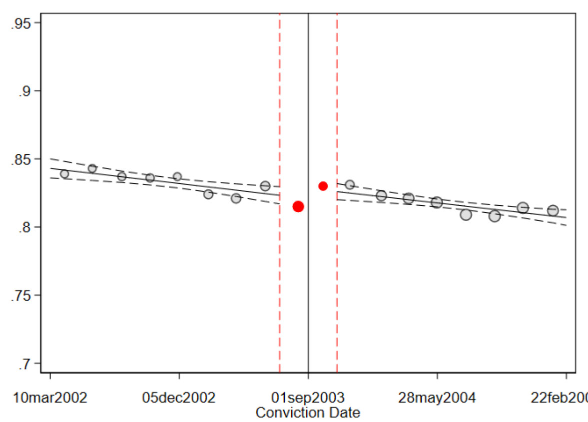
Panel B: Pre-disposition average 1040 household income



Panel C: Any prior convictions



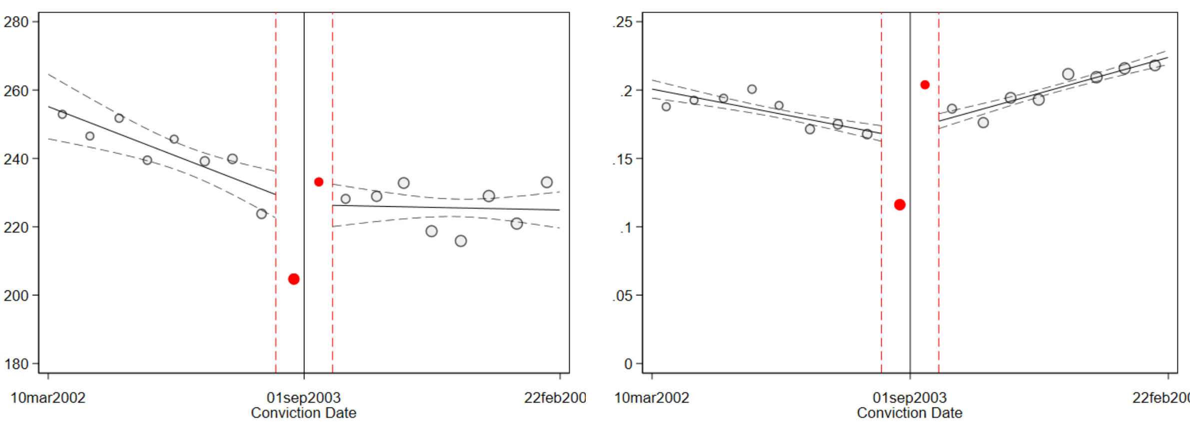
Panel D: Proportion Male



Note: These figures present the sharp RDD estimates of the subgroup characteristics denoted in the panel title describing the individual at the time of conviction. Prior convictions and pre-disposition household income are measured using criminal history and earnings history in the three years preceding the focal event. The sample is restricted to individuals whose first DRF-qualifying conviction occurred within the 540 days surrounding the policy reform date and excluding the 60 days surrounding the policy reform date.
RDD notes from Figure E1 apply.

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

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



Note: These figures present the sharp RDD estimates of the effect of the DRFs on the number of days between offense date and disposition date and the likelihood of being convicted for a non-DRF eligible but DRF-related charge. The sample is restricted to individuals whose first DRF-qualifying conviction occurred within the 540 days surrounding the policy reform date and excluding the 60 days surrounding the policy reform date. The sample is not restricted to individuals with linked PIKs.
RDD notes from Figure E1 apply.

Online Appendix F Measuring the spillover effects of the increased fines on romantic partners' outcomes and relationship outcomes

While we generally find that the increased fines have small or null effects on labor market and recidivism outcomes of the recipients, these fines may generate social spillovers within the household. For example, a large fine may trigger a change in a romantic partner's labor supply if he or she is the primary earner or in a better position to adjust hours worked; this is a highly possible hypothesis since research documents that justice-involved individuals have marginal formal labor market attachment (Finlay and Mueller-Smith 2021). Primary driving responsibility may also shift onto the romantic partner due to the license suspension from unpaid criminal debt, exposing them to greater risk of getting charged with a traffic-related offense themselves. To measure these partner spillovers, we use the household crosswalk from Finlay, Mueller-Smith, and Street (2023) that synthesizes information from a variety of data from the Census Bureau, IRS, and other federal programs. This crosswalk allows us to link individuals assigned LFOs to their partners in the year of the focal event.

We show our main results in Figure F1 with corresponding estimates in tabular format shown in Table F1: annual number of convictions, total earnings, number of years observed together, and likelihood the pair are still romantically involved at the end of the follow-up window. Overall, these results echo our findings in Figure 3. Not only are estimates insignificant, but the estimates and standard errors are also relatively small when compared to the pre-policy mean. Largely all of the estimates at the sub-experiment level are also insignificant. Furthermore, we can rule out effects greater than \$-546.72–\$450.92 (-0.003–0.003) in annual earnings (annual number of convictions). We find similarly precisely estimated null estimates for relationship outcomes as well, ruling out effects larger than a -1.3 to 3.2 percentage point change in likelihood of remaining together after 10 years and -0.04 to 0.11 years of relationship duration.

Table F1: Impact of fine increases on relationship and romantic partners' employment and recidivism outcomes by sub-experiment

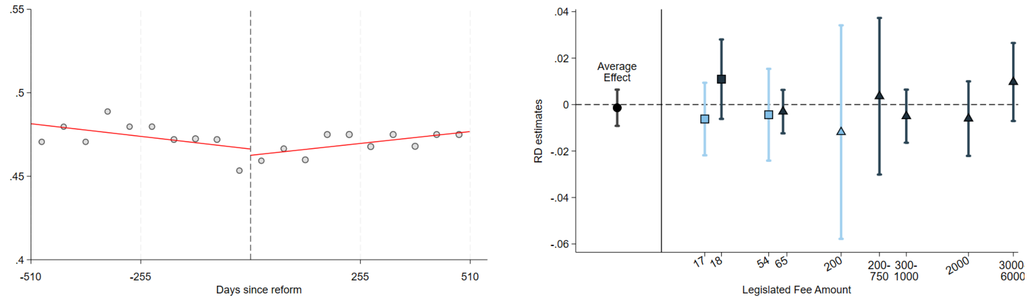
Experiment →	Average Effect	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Panel A: Recidivism and earnings, 10 years										
Annual number of convictions	-0.000	0.007	0.000	0.005	0.002*	-0.014	-0.005	-0.001	-0.003	-0.000
	(0.002)	(0.005)	(0.003)	(0.007)	(0.001)	(0.012)	(0.006)	(0.003)	(0.004)	(0.002)
	[0.041]	[0.070]	[0.036]	[0.061]	[0.015]	[0.033]	[0.041]	[0.058]	[0.046]	[0.028]
Annual total earnings	-47.9	-237.1	293.3	-716.7	-352.3	1332	-580.1	892.9*	-545.3	-115.3
	(254.5)	(539.6)	(632)	(736.8)	(378.8)	(2403)	(1051)	(461.7)	(801.4)	(669.6)
	[19,660]	[17,370]	[20,960]	[17,320]	[22,810]	[26,020]	[19,580]	[23,360]	[28,800]	[24,210]
Panel B: Relationship outcomes, 10 years										
Likelihood still reported together	0.005	0.001	0.016	0.001	-0.001	0.021	0.005	-0.004	0.001	0.007
	(0.006)	(0.013)	(0.011)	(0.013)	(0.006)	(0.039)	(0.022)	(0.007)	(0.010)	(0.011)
	[0.522]	[0.566]	[0.535]	[0.202]	[0.614]	[0.221]	[0.561]	[0.708]	[0.689]	[0.600]
Total years together	0.038	0.033	0.140*	-0.172	0.036	0.404*	-0.043	-0.058	-0.029	0.035
	(0.039)	(0.093)	(0.085)	(0.114)	(0.045)	(0.233)	(0.160)	(0.046)	(0.075)	(0.084)
	[6.540]	[7.274]	[6.967]	[3.777]	[7.644]	[2.082]	[7.328]	[8.392]	[7.998]	[7.395]
Observations	310,000	23,000	29,000	19,000	89,500	2,000	12,000	62,000	29,000	44,500

Note: This table presents the sharp RDD estimates of the effects of the increased fines on administrative outcomes of the romantic partner of the individuals in our focal sample. Romantic partners are identified using Finlay, Mueller-Smith, and Street (2023).

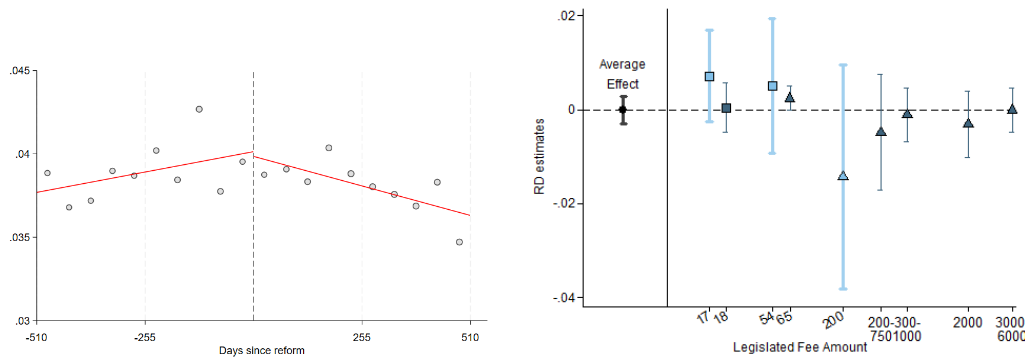
RDD Notes: Coefficients for each sub-experiment are estimated using Eq. 1. RDD estimates of the average effect across sub-experiments are calculated by estimating Eq. 2 via SUR. Robust standard errors are shown in parentheses; control means, measured using individuals whose focal event occurred before the policy reform date, are shown in square brackets. The data for each sub-experiment are described in Online Appendix D and Table 1. * p<0.1, ** p<0.05, *** p<0.01.

Figure F1: Impact of increased fines on linkage rates, long-run convictions, earnings, and observed relationship length for romantic partners

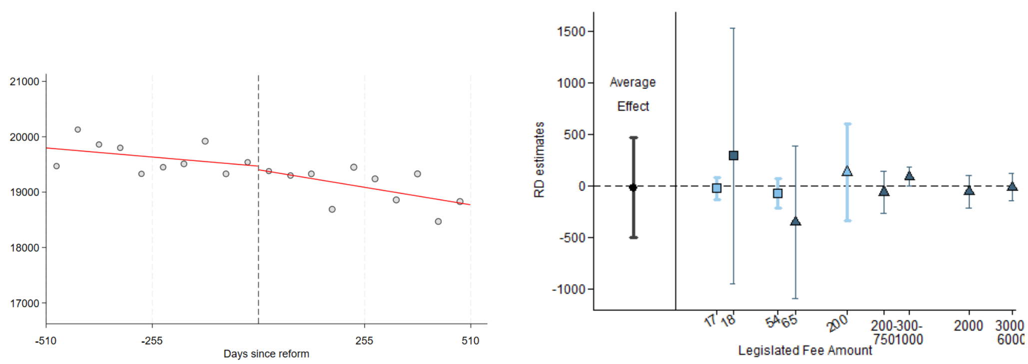
Panel A: Romantic linkage at time of focal event



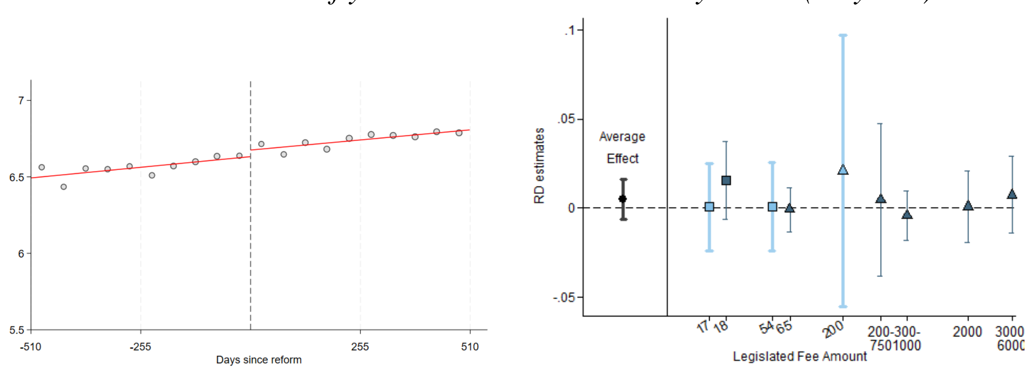
Panel B: Partner's annual number of convictions (10 years)



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



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

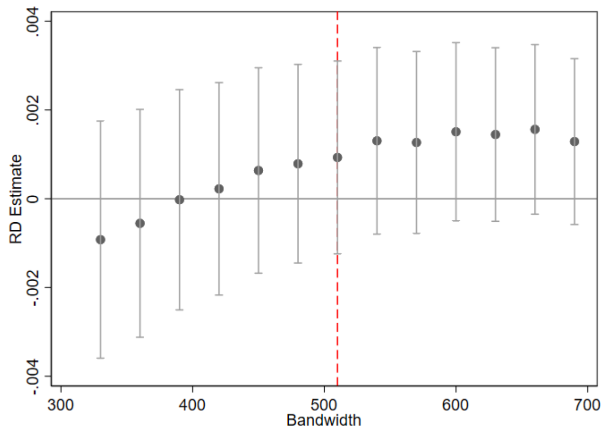


Note: This figure presents the sharp RDD estimates of the effects of the fine increase on outcomes of romantic partners described in the panel titles. Romantic partners are identified using the household crosswalk created by Finlay, Mueller-Smith, and Street (2023) and described in Online Appendix F. Partner's annual earnings are measured using W-2 tax returns and adjusted to 2017 dollars using the CPI-All Urban. The left-hand side graphs the fitted lines and raw data of the pooled experiments using the four-step procedure described in Section 4; the right-hand side graphs plot β_1^e of each sub-experiment e from Eq. 1. $\bar{\beta}_1$ is calculated by averaging each of the sub-experiment β_1^e together using Eq. 2, which was estimated using SUR. All estimates are at the individual level. See Tables F4 and G1 for results in tabular format along with underlying sample sizes. *RDD Figure Notes* from Figure 1 apply. The data for each sub-experiment are described in Online Appendix D and Table 1.

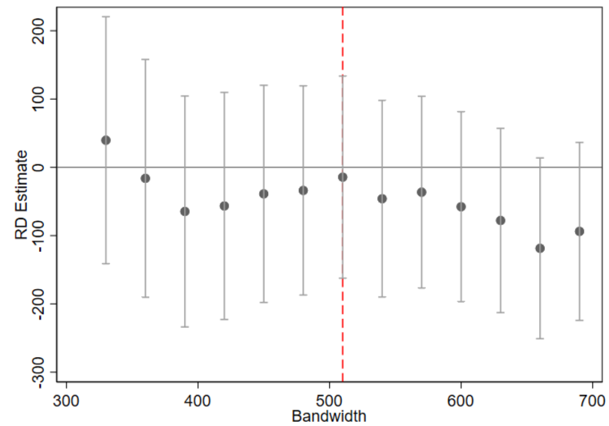
Online Appendix G Supplementary Results

Figure G1: Robustness of balance in predicted indices and main results to varied bandwidths

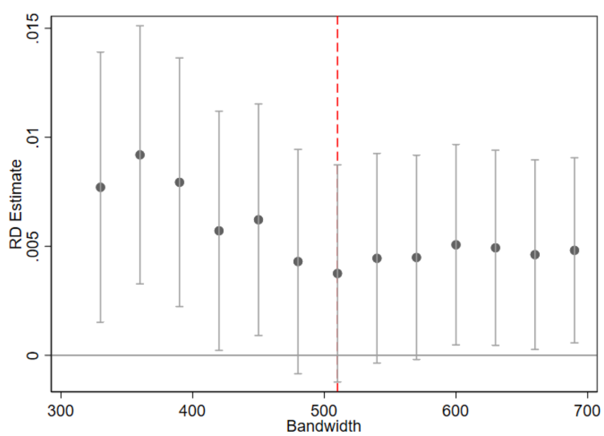
Panel A: Predicted annual number of convictions



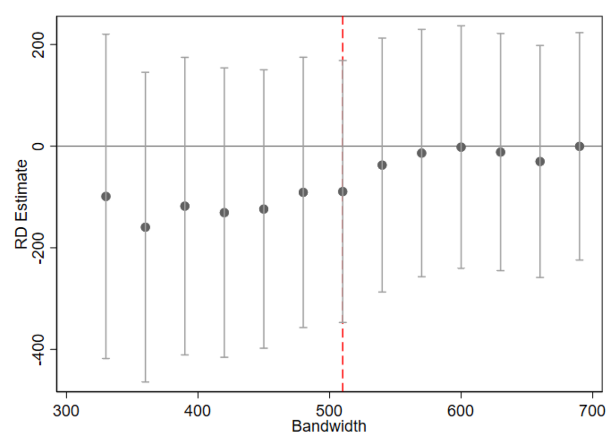
Panel B: Predicted annual earnings



Panel C: Annual number of convictions, 10 years

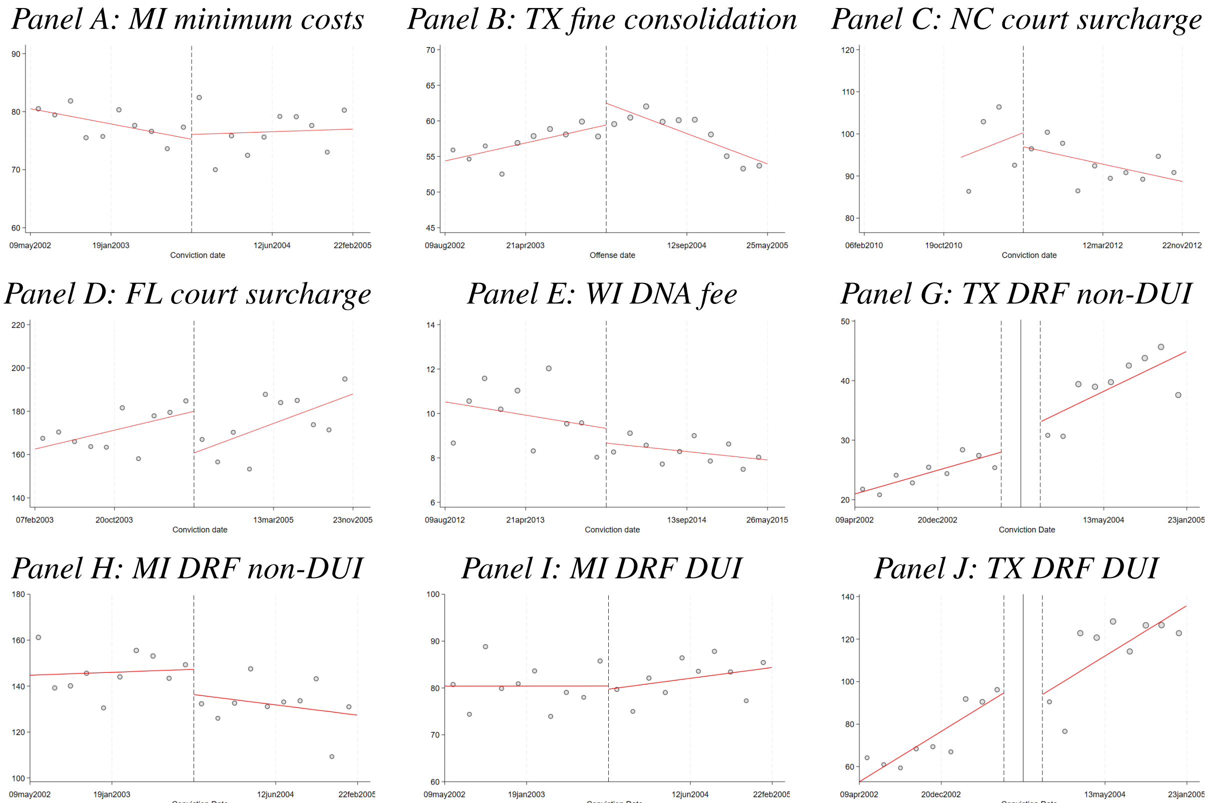


Panel D: Annual earnings, 10 years



Note: This figure plots the sharp RDD estimates from Eq. 1, excluding covariates X_i in panels A and B, pooled across the sub-experiments, with varying bandwidths (x-axis) ranging from 330 to 690 days in 30-day intervals, with 95% confidence intervals using robust standard errors. Estimates are at the individual level using Eq. 1 and each observation is weighted to ensure that each policy experiment contributes equally to the combined estimates. Outcomes are listed in the sub-titles of the figures. Total annual earnings is measured using income reported on W-2 tax returns and adjusted to 2017 dollars using the CPI-All Urban. See Table G10 for results in tabular form.
The data for each sub-experiment are described in Online Appendix D and Table 1.

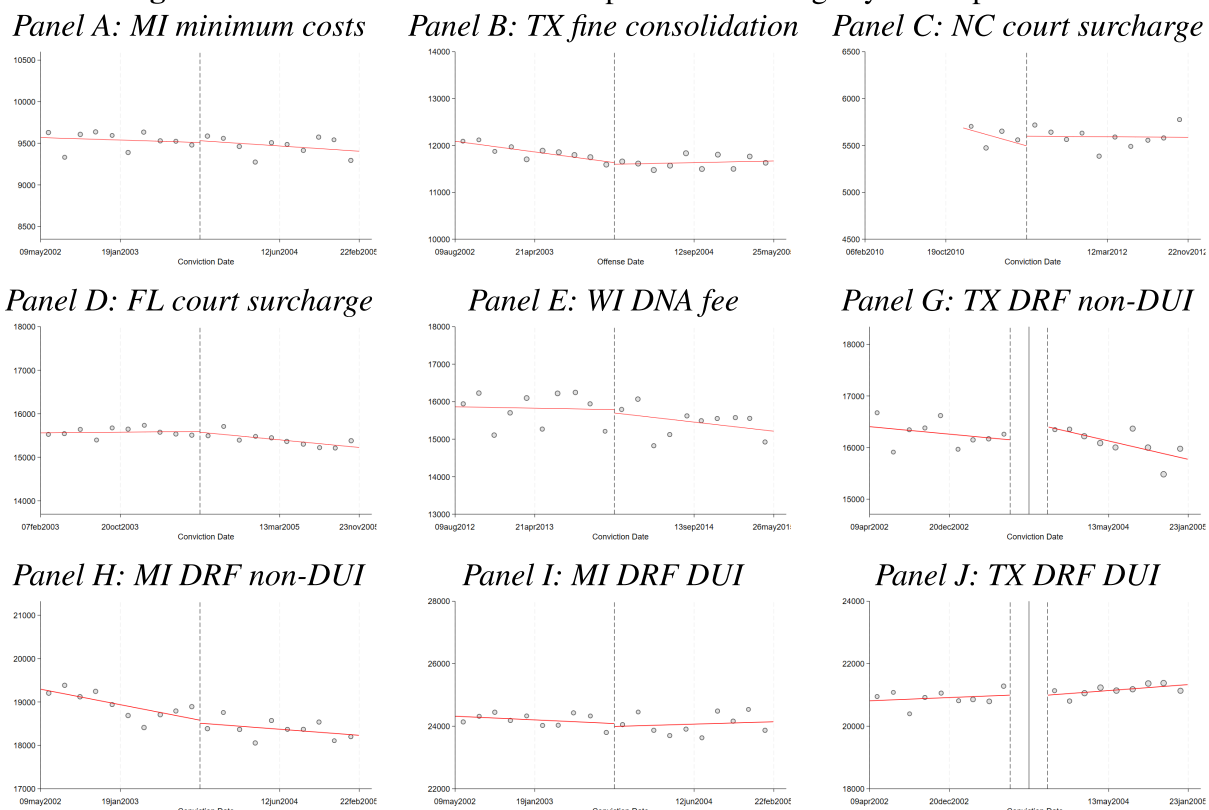
Figure G2: Robustness of balance in average daily caseload density by sub-experiment



Note: This figure presents the sharp RDD estimates of the effects of the fine increase on caseload density for each sub-experiment. It graphs the fitted lines and raw data of the sub-experiments. Estimates are at the daily level and are generated using Eq. 1, excluding covariates X_i in order to assess unconditional caseload balance across the implementation threshold. See Table G1 for results in tabular format along with underlying sample sizes.

RDD Sub-Experiment Figure Notes: Scatter points are binned using 51-day windows with the size of the circle denoting the number of observations within each bin. The black, dashed vertical line denotes the policy reform date. Linear fitted lines from the binned averages are shown in solid pattern and red color. The data for each sub-experiment are described in Online Appendix D and Table 1.

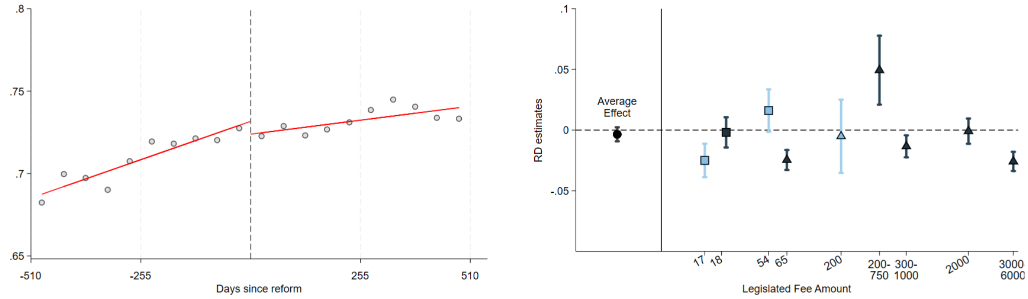
Figure G3: Robustness of balance in predicted earnings by sub-experiment



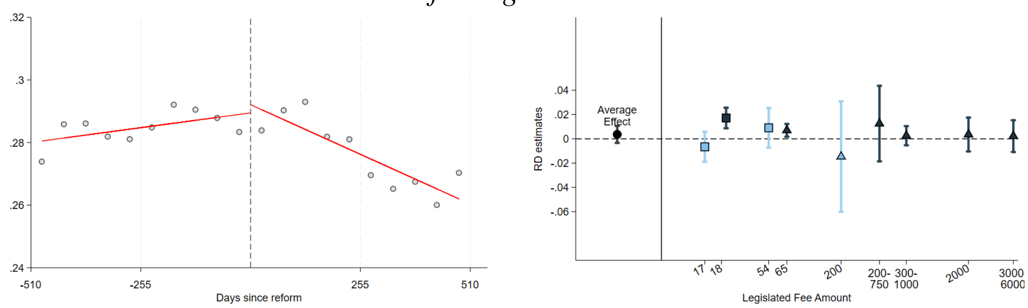
Note: This figure presents the sharp RDD estimates of the effects of the fine increase on predicted total earnings 1–5 years after the policy reform date measured using W-2 tax returns for each sub-experiment. Estimates are at the individual level and are generated using Eq. 1, excluding covariates X_i in order to assess unconditional caseload balance across the implementation threshold. See Table G1 for results in tabular format along with underlying sample sizes. *RDD Sub-Experiment Figure Notes* from Figure G2 apply. The data for each sub-experiment are described in Online Appendix D and Table 1.

Figure G4: Impact of increased fines on other criminal sanctions

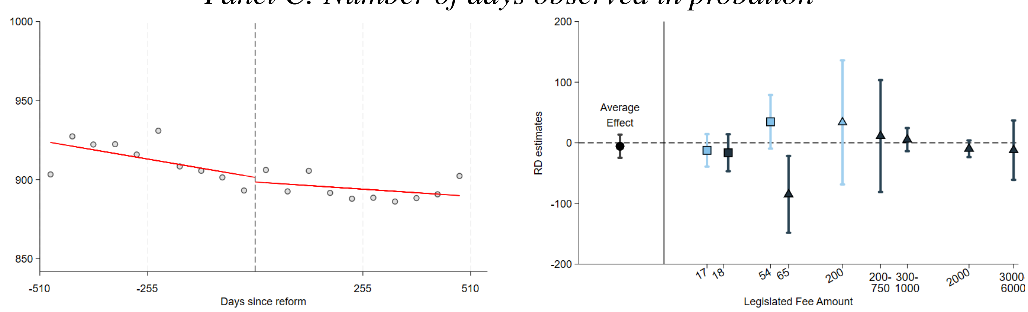
Panel A: Likelihood of conviction



Panel B: Likelihood of being sentenced to incarceration



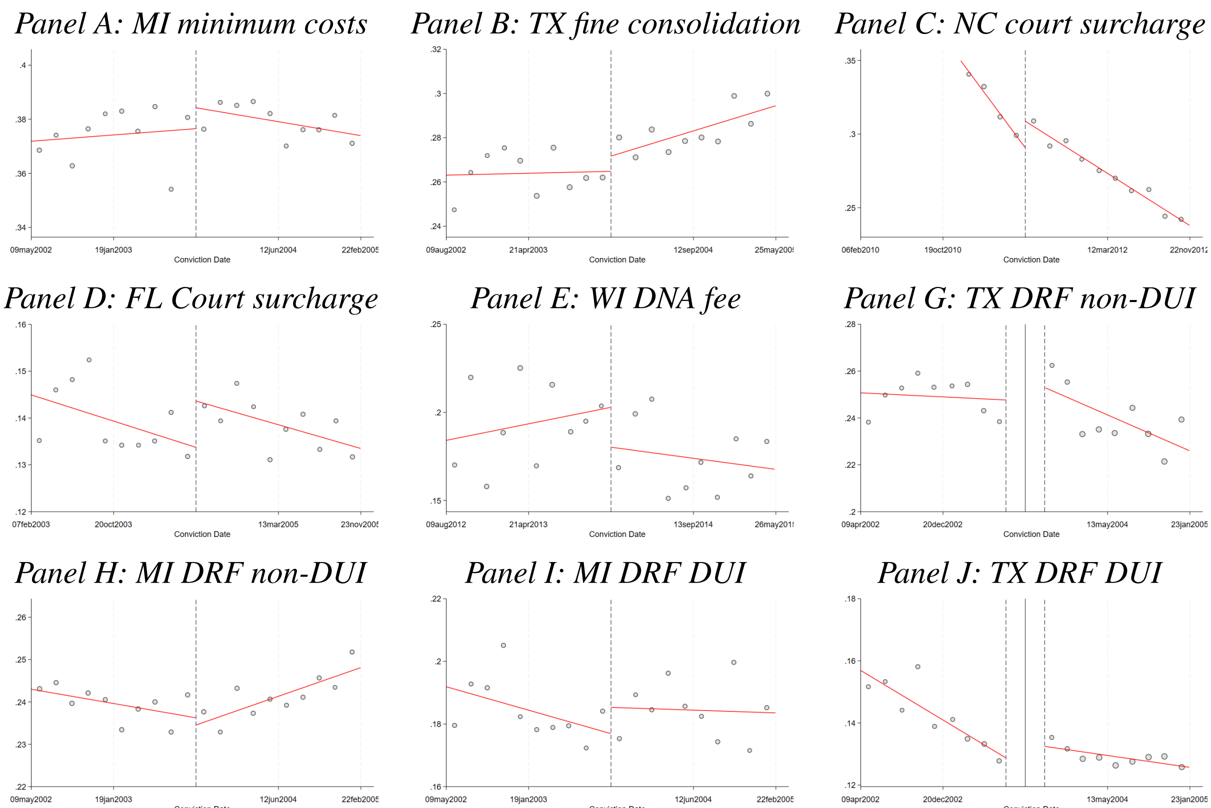
Panel C: Number of days observed in probation



Note: This figure presents the sharp RDD estimates of the effects of the fine increase on the probability of conviction for the focal event (panel A), probability of being sentenced to incarceration for the focal event (panel B), and the number of days for which an individual was subsequently observed on probation (panel C). The left-hand side graphs the fitted lines and raw data of the pooled experiments using the four-step procedure described in Section 4; the right-hand side graphs plot β_1^e of each sub-experiment e from Eq. 1 excluding covariates X_i in order to assess unconditional caseload balance across the implementation threshold. $\bar{\beta}_1$ is calculated by averaging each of the sub-experiment β_1^e together using Eq. 2, which were estimated using SUR. All estimates are at the individual level. Sample is not restricted to individuals with a linked PIK. See Table G2 for results in tabular format along with sample means.

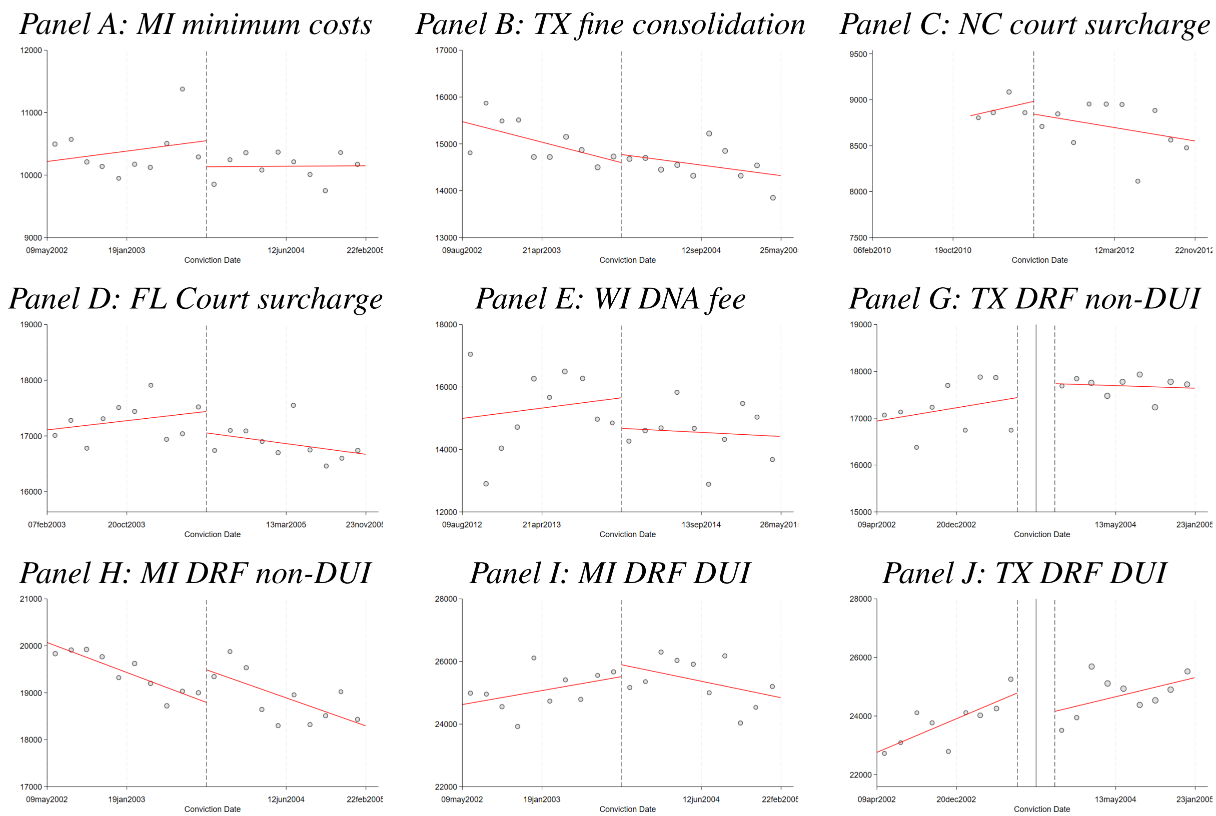
RDD Figure Notes from Figure 1 apply. The data for each sub-experiment are described in Online Appendix D and Table 1.

Figure G5: Impact of increased fines on annual number of convictions by sub-experiment 10 years after the cutoff



Note: This figure presents the sharp RDD estimates of the effects of the fine increase on annual number of convictions 1–10 years after the focal event for each sub-experiment. Estimates are at the individual level and are generated using Eq. 1. See Table G5 for results in tabular format along with underlying sample sizes.
RDD Sub-Experiment Figure Notes from Figure G2 apply. The data for each sub-experiment are described in Online Appendix D and Table 1.

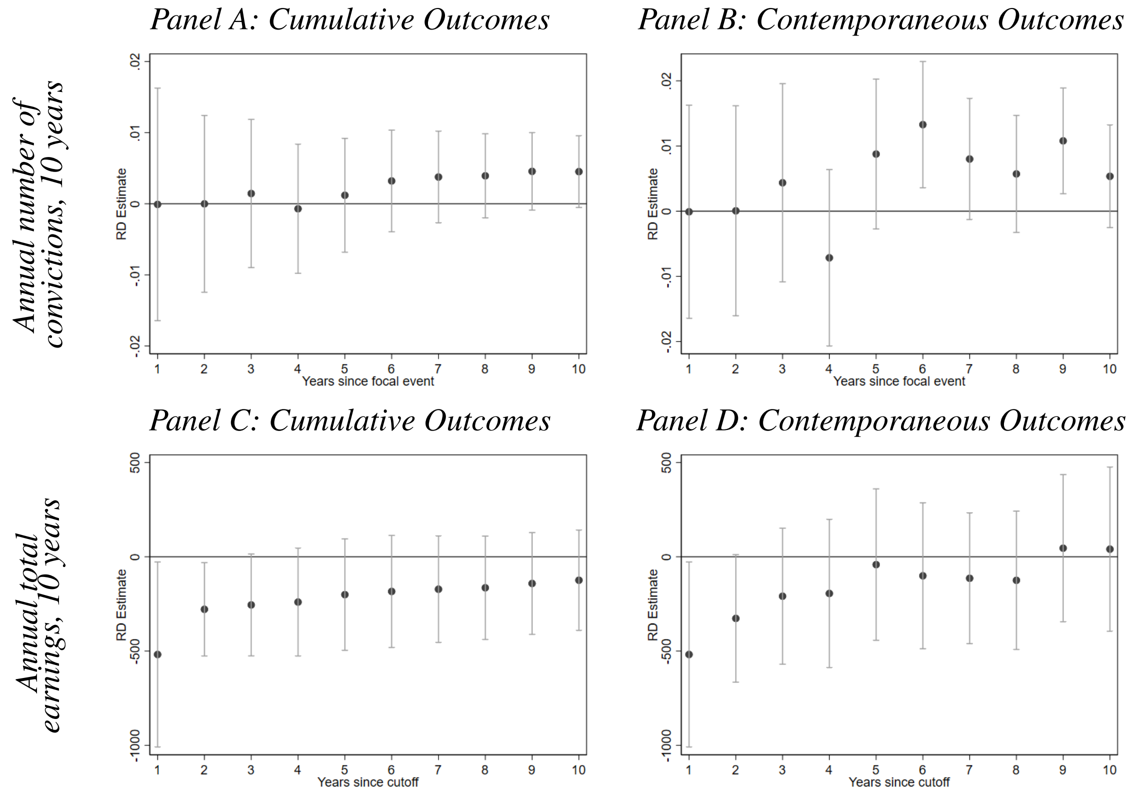
Figure G6: Impact of increased fines on annual earnings by sub-experiment 10 years after the cutoff



Note: This figure presents the sharp RDD estimates of the effects of the fine increase on total annual earnings measured using W-2 tax returns and adjusted to 2017 dollars using the CPI-All Urban 1–10 years after the policy reform date for each sub-experiment. Estimates are at the individual level and are generated using Eq. 1. See Table G5 for results in tabular format along with underlying sample sizes.

RDD Sub-Experiment Figure Notes from Figure G2 apply. The data for each sub-experiment are described in Online Appendix D and Table 1.

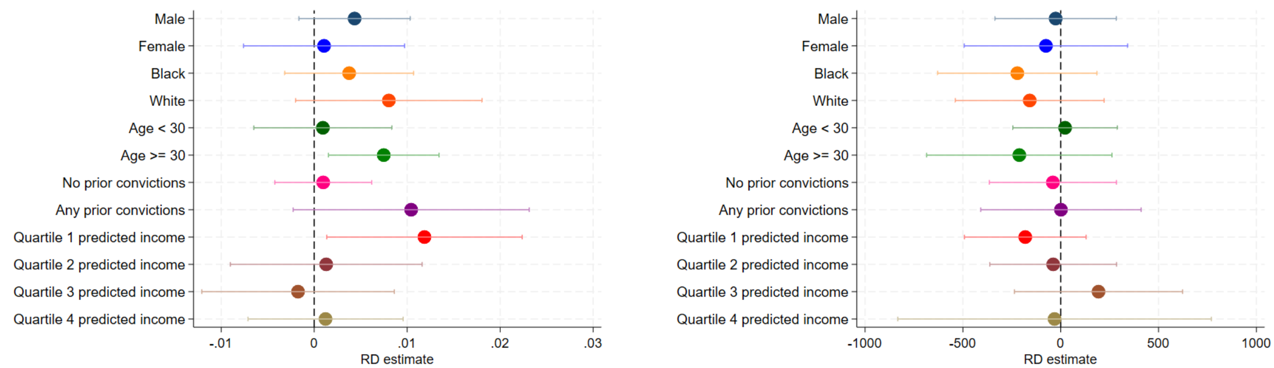
Figure G7: Evolution of RDD-based causal estimates over the 10 year followup period - Combined Sample



Note: This figure plots the sharp RDD estimates (dark grey, circles), with 95% confidence intervals using robust standard errors, measuring the effects of the increased sanctions on recidivism and labor market outcomes over a time period that varies by graph for the pooled sample. For number of convictions (panels A and B) the time frame is between 1 and 10 years following the focal event. Annual earnings (adjusted to 2017 dollars using the CPI-All Urban) are measured using income reported on W-2 tax returns (panels A and B). The time frame is from 1 to 10 years following the policy reform date. Outcomes in panels A and C are cumulative while outcomes in panels B and D are contemporaneous. All RDD estimates are shown with 95% confidence intervals. Estimates are at the individual level using Eq. 1 and are aggregated by estimating Eq. 2 via SUR. See Table G8 for results in tabular format along with sample means.
The data for each sub-experiment are described in Online Appendix D and Table 1.

Figure G8: Causal impact of increased sanctions on future earnings and convictions by sub-group - Combined Sample

Panel A: Annual number of convictions (10 years) *Panel B: Annual total earnings (10 years)*



Note: This figure plots the sharp RDD estimates, with 95% confidence intervals using robust standard errors, measuring the effects of the fine increases on annual total recidivism and annual total earnings across different sub-groups (x-axis) for the pooled sample. Total earnings is measured using income reported on W-2 tax returns and adjusted to 2017 dollars using the CPI-All Urban. Groups are defined by individual characteristics at the time of the focal event. Estimates are at the individual level using Eq. 1 and each observation is weighted to ensure that each policy experiment contributes equally to the combined estimates. See Table G7 for results in tabular formats and underlying sample sizes.
The data for each sub-experiment are described in Online Appendix D and Table 1.

Table G1: Evaluating balance of observable characteristics and sample populations and predicted indices by sub-experiment

Experiment →	Average Effect	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Caseload density and weights:										
Average daily caseload	-4.311	2.41	2.395*	-3.019	-19.25	-.256	2.553	-11.03	-.6738	-11.93
	(2.588)	(5.382)	(1.317)	(8.249)	(-14.53)	(.6664)	(2.173)	(9.549)	(4.362)	(10.83)
	[77.44]	[58]	[62]	[70]	[171.3]	[6.7]	[25]	[150]	[80]	[74]
Sum of inverse sample weights, ACS	-1.886	7.361*	.4692	-2.092	-19.01*		.1336	-1.239	-1.04	.3327
	(1.952)	(4.051)	(2.622)	(5.213)	(10.63)		(3.636)	(5.712)	(3.925)	(9.154)
	[64.75]	[51.08]	[34.18]	[53.87]	[135.7]		[21.95]	[102.5]	[52.61]	[66.14]
Sum of inverse sample weights	-111.3									
	(236.6)									
	[5800]									
Demographic characteristics:										
Male	0.001	-0.010	-0.021**	0.018*	0.004	0.003	0.006	-0.003	0.002	0.011*
	(0.003)	(0.007)	(0.007)	(0.009)	(0.004)	(0.022)	(0.013)	(0.006)	(0.007)	(0.007)
	[0.758]	[0.787]	[0.724]	[0.734]	[0.710]	[0.766]	[0.834]	[0.671]	[0.764]	[0.829]
White	-0.006	-0.011	0.016**	-0.017*	0.006	-0.009	-0.010	-0.018***	-0.006	-0.006
	(0.004)	(0.008)	(0.008)	(0.010)	(0.005)	(0.025)	(0.018)	(0.005)	(0.006)	(0.008)
	[0.594]	[0.613]	[0.532]	[0.507]	[0.576]	[0.412]	[0.517]	[0.683]	[0.833]	[0.677]
Black	0.007*	0.009	-0.017**	0.017*	-0.006	0.015	0.020	0.018***	0.004	-0.001
	(0.004)	(0.008)	(0.007)	(0.010)	(0.004)	(0.026)	(0.016)	(0.005)	(0.005)	(0.005)
	[0.269]	[0.317]	[0.228]	[0.411]	[0.291]	[0.465]	[0.271]	[0.245]	[0.108]	[0.086]
Hispanic	-0.001	0.000	-0.000	0.003	0.000	-0.015	-0.012	0.001	-0.000	0.011
	(0.002)	(0.003)	(0.006)	(0.004)	(0.003)	(0.013)	(0.014)	(0.002)	(0.003)	(0.007)
	[0.099]	[0.027]	[0.212]	[0.033]	[0.098]	[0.070]	[0.184]	[0.030]	[0.025]	[0.209]
Age at disposition	-0.127	-0.120	-0.286*	0.113	0.076	-0.691	-0.494	0.314**	0.289	-0.345*
	(0.091)	(0.190)	(0.173)	(0.238)	(0.117)	(0.582)	(0.308)	(0.137)	(0.215)	(0.192)
	[31]	[30]	[29]	[31]	[31]	[31]	[29]	[29]	[35]	[34]
In a romantic relationship	-0.001	-0.006	0.011	-0.004	-0.003	-0.012	0.004	-0.005	-0.006	0.010
	(0.004)	(0.008)	(0.009)	(0.010)	(0.005)	(0.023)	(0.017)	(0.006)	(0.008)	(0.009)
	[0.472]	[0.384]	[0.488]	[0.400]	[0.511]	[0.315]	[0.597]	[0.535]	[0.480]	[0.539]
Criminal history and prior income:										
Total prior convictions	0.030**	-0.035	0.038*	0.101**	-0.006	0.094	0.003	-0.016	0.014	0.075***
	(0.014)	(0.037)	(0.020)	(0.050)	(0.009)	(0.084)	(0.052)	(0.014)	(0.021)	(0.019)
	[0.727]	[1.510]	[0.336]	[1.530]	[0.205]	[0.550]	[0.756]	[0.628]	[0.526]	[0.506]
Average prior income	-11.28	-61.62	-216	286.3	-398.6	-354.6	562.2	422	384.4	-725.6
	(252.4)	(452.8)	(442.8)	(395.5)	(505)	(1262)	(661.8)	(560.2)	(1113)	(873.2)
	[18,820]	[14,000]	[14,000]	[9,400]	[18,000]	[15,000]	[13,000]	[22,000]	[38,000]	[26,000]
Predicted indices:										
Predicted annual number of convictions	0.001	-0.003	0.000	0.000	-0.001	0.010	-0.002	-0.002	0.001	0.004***
	(0.001)	(-0.003)	(-0.002)	(-0.003)	(-0.001)	(-0.006)	(0.004)	(0.002)	(0.002)	(0.001)
	[0.226]	[0.341]	[0.246]	[0.248]	[0.145]	[0.265]	[0.220]	[0.260]	[0.196]	[0.114]
Predicted annual income	6.242	21.395	-35	102.46	-16.682	-90.32	366.5	-78.175	-116.525	-73.55
	(69.8)	(-89.975)	(-88.26)	(-110.28)	(-85.9)	(-484.2)	(262)	(131.525)	(255.75)	(219.025)
	[13,430]	[9,540]	[11,838]	[5,590]	[15,576]	[15,828]	[16,250]	[19,000]	[24,250]	[21,000]
Observations	626,000	59,500	58,500	48,000	176,000	6,300	20,000	117,000	60,000	81,000

Note: This table presents the sharp RDD estimates for select characteristics describing the individual at the time of conviction. Prior income is measured using income reported on 1040 tax filings; predicted income is measured using income reported on W-2 tax returns. Wages and income are adjusted to 2017 dollars using the CPI-All Urban. Prior convictions and prior average income are measured using criminal history and earnings history in the three years preceding the focal event. With the exception of average daily caseload, which is at the day level, all regression estimates are at the individual level.

RDD Notes: Coefficients for each sub-experiment are estimated using Eq. 1. RDD estimates of the average effect across sub-experiments are calculated by estimating Eq. 2 via SUR. Robust standard errors are shown in parentheses; control means, measured using individuals whose focal event occurred before the policy reform date, are shown in square brackets. The data for each sub-experiment are described in Online Appendix D and Table 1. For this table only, the regressions exclude covariates X_i in order to assess unconditional caseload balance across the implementation threshold. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Table G2: Evaluating change in sentencing outcomes associated with focal event upon fine increase implementation

Experiment →	Average Effect	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Panel A: Likelihood of conviction:										
	-0.003	-0.025***	-0.002	0.016*	-0.025***	-0.005	0.049***	-0.013**	-0.001	-0.026***
	(0.003)	(0.007)	(0.006)	(0.009)	(0.004)	(0.015)	(0.015)	(0.005)	(0.005)	(0.004)
	[0.708]	[0.642]	[0.738]	[0.544]	[0.305]	[0.903]	[0.691]	[0.794]	[0.841]	[0.917]
Panel B: Likelihood of being sentenced to incarceration:										
	0.004	-0.007	0.017***	0.009	0.007**	-0.015	0.013	0.003	0.004	0.002
	(0.004)	(0.006)	(0.004)	(0.008)	(0.003)	(0.023)	(0.016)	(0.004)	(0.007)	(0.007)
	[0.289]	[0.194]	[0.098]	[0.328]	[0.099]	[0.651]	[0.405]	[0.138]	[0.375]	[0.312]
Panel C: Number of days observed in probation:										
	-5.632	-12.42	-16.33	34.69	-84.97**	33.7	11.18	5.304	-9.752	-12.09
	(9.743)	(13.68)	(15.56)	(22.56)	(32.32)	(52.19)	(47.07)	(9.722)	(7.004)	(24.98)
	[907.4]	[584]	[716]	[2735]	[649.2]	[607.9]	[963.5]	[505.9]	[426.6]	[978.1]
Observations	745,045	68,045	67,997	62,599	207,735	7,067	23,045	125,324	75,996	107,237

Note: This table presents the sharp RDD estimates on individual sentencing outcomes linked to the focal event after the policy reform with outcomes listed in the panel titles. In all panels, the sample is not conditional on being matched to a PIK.
RDD Notes from Table G1 apply. The data for each sub-experiment are described in Online Appendix D and Table 1. $*$ $p<0.1$, $**$ $p<0.05$, $***$ $p<0.01$.

Table G3: Evaluating change in total fines assigned upon focal event and total payments to date in the analysis sample upon fine increase implementation

Experiment →	Average Effect	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge+	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Panel A: Total sanctions assigned:										
	596.51***	17.73***	17.42***	31.74***	24.35***	150.10***	550.72***	492.72***	1,939.37***	2,144.41***
	(4.80)	(2.06)	(3.22)	(1.44)	(2.71)	(37.21)	(6.97)	(3.27)	(14.77)	(13.40)
	[120.30]	[71.53]	[131.70]	[78.86]	[113.82]	[686.81]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	745,045	68,045	67,997	62,599	207,735	7,067	23,045	125,324	75,996	107,237
Panel A: Total paid to date:										
	-5.05	-10.09***			-13.08***	8.02				
	(6.98)	(1.47)			(2.12)	(20.77)				
	[166.91]	[58.19]			[83.37]	[359.17]				
Observations	188,211	35,762			146,204	6,245				

Note: This table presents the sharp RDD estimates of the change in financial sanctions assigned at the focal event for an individual after the fine increases (panel A) and total sanctions paid to date (panel B) for the subsets of data for which we have payment data. In all panels, the sample is not conditional on being matched to a PIK; in panel B, observations are also conditional on being in the payment data.

RDD Notes from Table G1 apply. The data for each sub-experiment are described in Online Appendix D and Table 1. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

+ indicates that it's a subsample of the total analysis sample with available payment data (Hillsborough County and Miami-Dade County only). See Table 1 for more details.

Table G4: Local Polynomial and Sharp RDD, with no covariates, estimates of main outcomes on the pooled sample

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

Note: This table presents the sharp RDD estimates of the impacts of the fine increases on select outcomes at the individual level using alternative regression estimation strategies. The outcomes of interest are annual earnings 1-–10 years after the policy reform date and total recidivism 1–10 years after the focal event. RDD 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, X_i used in our main specification, Eq. 1. For the column ‘No covariates,’ we use Eq. 1 but do not include any covariates X_i. We also use the same estimation sample used in our main specification described in Section 3. For both specifications, each observation is weighted to ensure that each policy experiment contributes equally to the combined estimates.
Robust standard errors are shown in parentheses; control means, measured using individuals whose focal event occurred before the policy reform date, are shown in square brackets. The data for each sub-experiment are described in Online Appendix D and Table 1. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Table G5: Impact of fine increases on employment and recidivism using administrative data by sub-experiment

Experiment →	Average Effect	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Panel A, Recidivism, 10 years following focal event:										
Annual number of convictions	0.005*	0.006	0.008	0.014	0.010**	-0.023	0.006	-0.002	0.006	0.007*
	(0.003)	(0.008)	(0.008)	(0.010)	(0.003)	(0.020)	(0.011)	(0.004)	(0.005)	(0.004)
	[0.209]	[0.337]	[0.264]	[0.320]	[0.139]	[0.195]	[0.224]	[0.216]	[0.166]	[0.127]
Any charges	0.005	-0.002	-0.005	-0.001	-0.002	0.038	0.000	-0.001	0.007	0.007
	(0.004)	(0.007)	(0.008)	(0.009)	(0.005)	(0.024)	(0.017)	(0.005)	(0.008)	(0.008)
	[0.558]	[0.703]	[0.561]	[0.677]	[0.485]	[0.393]	[0.621]	[0.616]	[0.506]	[0.457]
Any convictions	0.004	0.001	-0.007	0.001	-0.002	0.019	0.010	-0.004	0.005	0.008
	(0.004)	(0.007)	(0.008)	(0.009)	(0.004)	(0.024)	(0.017)	(0.005)	(0.008)	(0.008)
	[0.480]	[0.635]	[0.498]	[0.523]	[0.326]	[0.357]	[0.563]	[0.550]	[0.456]	[0.411]
Any felony	0.004	-0.004	-0.014*	0.004	0.007**	0.021	0.013	-0.000	0.010*	-0.003
	(0.003)	(0.007)	(0.007)	(0.009)	(0.003)	(0.018)	(0.015)	(0.004)	(0.005)	(0.007)
	[0.223]	[0.310]	[0.289]	[0.292]	[0.173]	[0.169]	[0.293]	[0.156]	[0.121]	[0.205]
Annual number of drug convictions	0.000	-0.000	-0.001	0.005	0.004**	0.002	-0.006	-0.001	0.001	0.001
	(0.001)	(0.002)	(0.003)	(0.004)	(0.001)	(0.007)	(0.004)	(0.001)	(0.001)	(0.001)
	[0.040]	[0.051]	[0.068]	[0.080]	[0.034]	[0.033]	[0.051]	[0.026]	[0.016]	[0.019]
Annual number of property convictions	0.002*	0.004	0.001	0.007	0.003*	-0.004	0.006	0.000	-0.000	0.001
	(0.001)	(0.003)	(0.004)	(0.006)	(0.001)	(0.009)	(0.004)	(0.001)	(0.001)	(0.001)
	[0.047]	[0.069]	[0.073]	[0.118]	[0.042]	[0.035]	[0.047]	[0.031]	[0.021]	[0.019]
Annual number of violent convictions	0.001	-0.000	0.002	0.002	0.001	-0.001	0.001	0.000	-0.000	0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.007)	(0.002)	(0.001)	(0.001)	(0.001)
	[0.024]	[0.034]	[0.040]	[0.040]	[0.014]	[0.033]	[0.023]	[0.017]	[0.016]	[0.014]
Panel B, Employment, 10 years following cutoff:										
Annual total earnings	-124.4	-397.7	158.4	-208.7	-394.9	-1100	55.77	692.8**	433.3	-823
	(135.8)	(263.3)	(304.7)	(267)	(240.9)	(895.8)	(675.4)	(318.8)	(550.7)	(546.7)
	[15,090]	[10,380]	[14,970]	[8,910]	[17,270]	[15,330]	[17,240]	[19,430]	[25,040]	[23,860]
Annual employment rate per year	0.000	0.013**	-0.001	-0.005	-0.007**	0.008	-0.017*	0.005	-0.000	0.007
	(0.003)	(0.006)	(0.005)	(0.007)	(0.003)	(0.017)	(0.010)	(0.003)	(0.005)	(0.005)
	[0.847]	[0.793]	[0.874]	[0.796]	[0.863]	[0.826]	[0.884]	[0.882]	[0.854]	[0.851]
Annual number of employers per year	-0.000	0.009	0.013	-0.006	-0.008	-0.011	0.015	0.002	-0.002	-0.012
	(0.008)	(0.011)	(0.016)	(0.016)	(0.007)	(0.053)	(0.029)	(0.008)	(0.010)	(0.014)
	[0.988]	[0.729]	[1.163]	[0.868]	[1.027]	[1.248]	[1.049]	[0.968]	[0.878]	[0.966]
Annual total household earnings	-32.81	-492.3	213.2	-258.3	-247.1	-1282	486.5	946.5**	866.7	-1067
	(195.5)	(340.5)	(452)	(316.9)	(415.3)	(973.7)	(709.2)	(477.6)	(956.6)	(731.4)
	[21,500]	[14,420]	[18,040]	[10,910]	[24,780]	[17,630]	[18,710]	[27,930]	[38,400]	[30,850]
Observations	626,000	59,500	58,500	48,000	176,000	6,300	20,000	117,000	60,000	81,000

Note: This table presents the sharp RDD estimates of the impacts of the fine increases on labor market and recidivism outcomes at the individual level. Recidivism behavior is measured 10 years after the focal event and labor outcomes are measured using cumulative W-2 earnings 10 years after the policy reform date (adjusted to 2017 dollars using the CPI-All Urban). Average employment rate is defined as whether an individual received a W-2 tax return in that year. Average number of employers is measured using number of W-2 tax returns received that year. Total household earnings is measured using income reported on 1040 tax filings. See Choi et al. (2023) for details on offense classification. *RDD Notes* from Table G1 apply. The data for each sub-experiment are described in Online Appendix D and Table 1. ∗ p<0.1, ∗∗ p<0.05, ∗∗∗ p<0.01.

Table G6: Impact of fine increases on employment and household circumstances using American Community Survey responses by sub-experiment

Experiment →	Average Effect	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Panel A: Earnings and costs										
Monthly income	-94.63	-87.67	-9.675	38.92	-106.1		-294.2	-36.63	-25.12	-236.6
	(59.00)	(133.6)	(110)	(114.2)	(96.5)		(347.9)	(88.42)	(120.8)	(164.4)
	[2,129]	[1,538]	[1,738]	[1,144]	[2,088]		[2,277]	[2,417]	[3,056]	[2,773]
Monthly housing costs	-1.395	-24.61	-55.66	12.14	16.15		77.63	-2.078	.9786	-35.71
	(17.62)	(39.99)	(39.6)	(47.55)	(37.65)		(97.66)	(28.02)	(31.12)	(41.53)
	[993.8]	[875.4]	[948.8]	[858.6]	[1226]		[942]	[1052]	[1051]	[996.3]
Reported mental disability	0.008	-0.006	0.028	0.044*	0.015		-0.020	-0.016	0.012	0.009
	(0.007)	(0.024)	(0.017)	(0.026)	(0.012)		(0.032)	(0.011)	(0.013)	(0.014)
	[0.087]	[0.130]	[0.102]	[0.125]	[0.070]		[0.060]	[0.078]	[0.066]	[0.062]
Monthly mortgage	-2.032	-2.166	-86.38	33	-30.11		96.42	16.18	19.51	-62.71
	(29.23)	(50.22)	(70.64)	(79.61)	(53.39)		(179.5)	(32.21)	(32.4)	(60.61)
	[913.2]	[726.6]	[930.5]	[827.9]	[1194]		[913.5]	[896.2]	[875.7]	[941.2]
Monthly rent	1.434	-20.01	-38.15	15.51	14.22		12.14	32.05	-4.914	.6285
	(13.58)	(29.39)	(30.74)	(41.88)	(27.15)		(67.1)	(20.6)	(32.2)	(39.14)
	[654.6]	[579.5]	[674.5]	[587.8]	[804]		[645.6]	[644.8]	[636.7]	[663.7]
Panel B: Change in household circumstances										
Household size	0.037	0.118	-0.007	0.044	0.012		0.137	0.010	-0.065	0.044
	(0.046)	(0.115)	(0.116)	(0.134)	(0.077)		(0.254)	(0.066)	(0.068)	(0.115)
	[2.940]	[2.811]	[3.163]	[2.673]	[2.897]		[3.161]	[3.159]	[2.651]	[3.008]
Commute by car	-0.013	-0.015	-0.015	-0.048	0.008		-0.042	0.015	-0.001	-0.005
	(0.011)	(0.036)	(0.027)	(0.038)	(0.020)		(0.055)	(0.015)	(0.017)	(0.023)
	[0.895]	[0.890]	[0.902]	[0.885]	[0.867]		[0.901]	[0.901]	[0.909]	[0.903]
Observations	45,000	3,900	4,100	2,800	8,800		1,300	11,000	6,900	6,100

Note: This table presents the sharp RDD estimates of the impacts of the fine increases on survey outcomes measured using individual responses to the 2005–2020 ACS. "Reported Mental Disability" refers to the question: Due to a physical, mental, or emotional condition lasting 6 months or more, do you have difficulty learning, remembering or concentrating (US Census Bureau 2021c)? Sample sizes for the WI DNA fee sub-experiment are too small for disclosure under Census Bureau policies.
RDD Notes from Table G1 apply. The data for each sub-experiment are described in Online Appendix D and Table 1. ∗ p<0.1, ∗∗ p<0.05, ∗∗∗ p<0.01.

Table G7: Causal impact of the increased fines on future recidivism and earnings by subgroup

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

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

Note: This table presents the sharp RDD estimates for the pooled sample of the impacts of the fine increases on annual number of convictions and annual earnings by subgroups of the individuals in our focal sample defined in the columns. Subgroups are defined by individual characteristics at the time of the focal event.

RDD Notes: Coefficients for each sub-experiment are estimated using Eq. 1. RDD estimates of the average effect across sub-experiments are calculated by estimating Eq. 1 with each observation weighted so that each policy experiment contributes equally to the combined estimate. Robust standard errors are shown in parentheses; control means, measured using individuals whose focal event occurred before the policy reform date, are shown in square brackets. The data for each sub-experiment are described in Online Appendix D and Table 1. ∗ p<0.1, ∗∗ p<0.05, ∗∗∗ p<0.01.

Table G8: Evolution of RDD-based causal estimates over the 10 year followup period - Combined Sample

	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 9	Year 10
Panel A: Cumulative outcomes:										
Average annual number of convictions. 10 years										
	-.0001	-.00001	0.001	-0.001	0.001	0.003	0.004	0.004	0.005	0.005*
	(0.008)	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
	[0.350]	[0.324]	[0.307]	[0.292]	[0.277]	[0.262]	[0.247]	[0.233]	[0.221]	[0.209]
Observations	626,000	626,000	626,000	626,000	626,000	626,000	626,000	626,000	626,000	626,000
Average annual earnings, 10 years										
	-518.2**	-278.65**	-255.47*	-240.3*	-200.6	-183.83	-172.14	-164.5	-141.67	-124.4
	(250.4)	(126.25)	(137.93)	(145.88)	(150.84)	(151.62)	(144)	(139.88)	(137.67)	(135.8)
	[12,530]	[16,160]	[16,540]	[16,870]	[16,990]	[16,760]	[16,680]	[16,720]	[16,810]	[16,940]
Observations	288,000	626,000	626,000	626,000	626,000	626,000	626,000	626,000	626,000	626,000
Panel B: Contemporaneous outcomes:										
Average annual number of convictions. 10 years										
	-.0001	.0001	0.004	-0.007	0.009	0.013**	0.008*	0.006	0.011**	0.005
	(0.008)	(0.008)	(0.008)	(0.007)	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
	[0.350]	[0.298]	[0.272]	[0.246]	[0.217]	[0.188]	[0.175]	[0.157]	[0.135]	[0.130]
Observations	626,000	626,000	626,000	626,000	626,000	626,000	620,000	620,000	620,000	572,000
Average annual earnings, 10 years										
	-518.2**	-327*	-209.3	-194.6	-41.47	-100.8	-114.1	-124.7	45.67	40.39
	(250.4)	(172.6)	(184)	(200.5)	(204.8)	(197.5)	(200)	(207.3)	(215.8)	(216.3)
	[12,530]	[16,370]	[17,090]	[17,670]	[17,420]	[15,790]	[15,990]	[16,740]	[17,140]	[17,640]
Observations	288,000	626,000	626,000	626,000	626,000	626,000	620,000	620,000	620,000	572,000

Note: This table presents the sharp RDD estimates measuring the effects of the increased sanctions on recidivism and labor market outcomes over a time period that varies by graph for the pooled sample, with the time period denoted in the columns. Outcomes in panel A are cumulative while outcomes in panel B are contemporaneous. Observation counts vary due to lack of W-2 filings prior to 2005 and the shorter time length of follow-up windows for later policy changes (e.g., Wisconsin). See more details in Online Appendix D.

RDD Notes from Table G1 apply. The data for each sub-experiment are described in Online Appendix D and Table 1. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Table G9: Causal impact of the increased fines on future recidivism, unrestricted by PIK-process

Experiment →	Average Effect	MI Minimum costs	TX fine cons.	NC Court surcharge	FL Court surcharge	WI DNA fee	TX DRF non-DUI	MI DRF non-DUI	MI DRF DUI	TX DRF DUI
Recidivism:										
Annual number of convictions	0.002	-0.007	0.002	0.013*	-0.001	0.004	0.007	-0.001	0.005	0.003
	(0.002)	(0.006)	(0.004)	(0.007)	(0.001)	(0.011)	(0.007)	(0.004)	(0.004)	(0.002)
	[0.145]	[0.235]	[0.128]	[0.191]	[0.019]	[0.126]	[0.184]	[0.209]	[0.158]	[0.100]
Any charges	0.005	-0.012	0.006	0.009	-0.000	0.033	0.011	0.002	0.001	0.004
	(0.003)	(0.007)	(0.006)	(0.008)	(0.002)	(0.023)	(0.012)	(0.005)	(0.007)	(0.005)
	[0.435]	[0.548]	[0.391]	[0.415]	[0.056]	[0.366]	[0.588]	[0.619]	[0.522]	[0.422]
Any convictions	0.004	-0.008	0.009	0.005	0.000	0.021	0.011	0.001	-0.001	0.005
	(0.003)	(0.007)	(0.006)	(0.008)	(0.002)	(0.022)	(0.012)	(0.005)	(0.007)	(0.005)
	[0.386]	[0.493]	[0.348]	[0.335]	[0.044]	[0.339]	[0.532]	[0.548]	[0.468]	[0.382]
Annual number of drug convictions	0.000	-0.002	0.000	0.003	-0.001	0.003	-0.000	-0.001	-0.000	0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.000)	(0.004)	(0.003)	(0.001)	(0.001)	(0.001)
	[0.027]	[0.035]	[0.032]	[0.049]	[0.005]	[0.020]	[0.048]	[0.025]	[0.016]	[0.016]
Annual number of property convictions	0.001	-0.000	0.001	0.005	-0.000	0.001	0.005	-0.002	0.001	0.001
	(0.001)	(0.002)	(0.002)	(0.004)	(0.000)	(0.005)	(0.003)	(0.001)	(0.001)	(0.001)
	[0.032]	[0.049]	[0.037]	[0.069]	[0.006]	[0.023]	[0.044]	[0.033]	[0.024]	[0.016]
Annual number of violent convictions	0.000	-0.000	0.000	0.001	-0.000	0.004	0.000	0.000	0.000	0.001
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.004)	(0.002)	(0.001)	(0.001)	(0.001)
	[0.016]	[0.023]	[0.019]	[0.022]	[0.002]	[0.021]	[0.023]	[0.016]	[0.016]	[0.012]
Observations	745,045	68,045	67,997	62,599	207,735	7,067	23,045	125,324	75,996	107,237

Note: This table presents the sharp RDD estimates of the impacts of the increased fines on annual number of convictions 10 years after the focal event at the individual level. The regression sample is not conditional on being matched to a PIK.
RDD Notes from Table G1 apply. The data for each sub-experiment are described in Online Appendix D and Table 1. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.

Table G10: Robustness of balance in predicted indices and main results to varied bandwidths

Bandwidth:	Predicted annual number convictions	Predicted annual earnings	Annual number of convictions, 10 years	Annual earnings, 10 years
330 Days	-0.0009	39.66	0.0077**	-98.98
	(0.0014)	(92.28)	(0.0032)	(162.8)
360 Days	-0.0006	-16.16	0.0092**	-159.5
	(0.0013)	(88.84)	(0.0030)	(155.5)
390 Days	-0.0000	-64.66	0.0079**	-118.1
	(0.0013)	(86.28)	(0.0029)	(149.4)
420 Days	0.0002	-56.52	0.0057**	-130.8
	(0.0012)	(84.82)	(0.0028)	(145.3)
450 Days	0.0006	-38.86	0.0062**	-123.8
	(0.0012)	(81.14)	(0.0027)	(139.8)
480 Days	0.0008	-33.84	0.0043	-90.99
	(0.0011)	(78.14)	(0.0026)	(135.7)
510 Days	0.0009	-14.34	0.0038	-89.4
	(0.0011)	(75.5)	(0.0025)	(131.5)
540 Days	0.0013	-45.96	0.0045*	-37.26
	(0.0011)	(73.44)	(0.0025)	(127.5)
570 Days	0.0013	-36.38	0.0045*	-13.74
	(0.0010)	(71.64)	(0.0024)	(124.2)
600 Days	0.0015	-57.68	0.0051**	-1.912
	(0.0010)	(70.96)	(0.0023)	(121.8)
630 Days	0.0014	-77.86	0.0049**	-11.74
	(0.0010)	(68.84)	(0.0023)	(119.1)
660 Days	0.0016	-118.64*	0.0046**	-30.25
	(0.0010)	(67.54)	(0.0022)	(116.5)
690 Days	0.0013	-93.82	0.0048**	-0.391
	(0.0010)	(66.48)	(0.0022)	(114.2)

Note: This figure plots the sharp RDD estimates from Eq. 1, excluding covariates X_i in the columns "Predicted annual number of convictions" and "Predicted annual earnings," pooled across the sub-experiments, with varying bandwidths ranging from 330 to 690 days in 30-day intervals. Estimates are at the individual level using Eq. 1 and each observation is weighted to ensure that each policy experiment contributes equally to the combined estimates. Total annual earnings is measured using income reported on W-2 tax returns and adjusted to 2017 dollars using the CPI-All Urban. See Section 4.1 for details on creation of predicted indices.
RDD Notes from Table G7 apply. The data for each sub-experiment are described in Online Appendix D and Table 1. $*$ p<0.1, $**$ p<0.05, $***$ p<0.01.