The Impact of Financial Sanctions: Regression Discontinuity Evidence from Driver Responsibility Fee Programs in Michigan and Texas[*]

Keith Finlay
U.S. Census Bureau

Matthew Gross
University of Michigan

Elizabeth Luh
University of Michigan

Michael Mueller-Smith[†]
University of Michigan

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Abstract

We estimate the causal impact of financial sanctions in the U.S. criminal justice system. We utilize a regression discontinuity design and exploit two distinct natural experiments: the abrupt introduction of driver responsibility fees (DRF) in Michigan and Texas. These discontinuously imposed additional surcharges ($300–$6,000) for criminal traffic offenses. Although the policies generated almost $3 billion of debt, we find consistent evidence that the DRFs had no impact on recidivism, earnings, or romantic partners' outcomes over the next decade. Without evidence of a general or specific deterrence effect and modest success with debt collection, we find little justification for these policies.

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

JEL classification codes: H72, J24, K42

1 Introduction

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

Descriptive research has found strong correlational evidence linking fines and fees with financial instability, criminal recidivism, and poor labor market outcomes (Harris, Evans, and Beckett 2010; Pleggenkuhle 2018).[1] Given the high incidence of criminal convictions in the United States and the growing use of LFOs, such evidence would suggest wide-ranging impacts on not only the most disadvantaged criminal defendants but also on the economy at large. Pager et al. (2022) presents the highest quality causal evidence to date in the field using a randomized controlled trial of debt relief totaling $3,000 on average from court-related LFO's for misdemeanor defendants in Oklahoma County, Oklahoma. Their findings show no impact of debt relief on future criminal behavior within one year after the intervention, although they find increased incidences of debt collection and ongoing court supervision resulting from unpaid fines.

In this paper, we extend the findings of Pager et al. (2022) to produce the first regression discontinuity evidence on this topic.[2] In the fall of 2003, Michigan and Texas each passed laws, the Driver Responsibility Fee (DRF) and the Driver Responsibility Program (DRP) respectively, that mandated new fines to criminal defendants convicted of certain traffic related programs.[3,4] The general goal was to raise revenue for the government while discouraging unsafe driving. We exploit the discrete adoption of these policies that increased fine amounts by $200–$6,000 for specific criminal traffic offenses to study the causal effect of these sanctions on recidivism, labor market, and household spillover outcomes over the short and long-run.

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

[2] In ongoing work, Giles (2022) also leverages a RD design to study the impact of misdemeanor fines on recidivism behavior in Milwaukee County, Wisconsin.

[3] This program is not unique to either state as New Jersey, New York, and Virginia have all adopted similar programs at different points in time.

[4] For simplicity, we refer to both programs as the Driver Responsibility Fee (DRF).

Taking advantage of the commonalities across the programs, we present parallel analysis using the same regression discontinuity research design to analyze the impact of both DRF programs. We exploit the fact that the DRF program applied only to individuals convicted on or after the effective date (October 1, 2003 in Michigan; September 1, 2003 in Texas), a context well-suited for regression discontinuity analysis. Thus, individuals convicted before the DRF-effective date were not responsible for the fees while those convicted after faced significantly higher LFOs.

We use the Criminal Justice Administrative Records System (CJARS), which contains multi-jurisdictional, longitudinal, harmonized criminal histories allowing us to observe each individual's repeated interactions with the justice system for residents in multiple states (Finlay, Mueller-Smith, and Papp 2022). Thus, we can tie fines and fees assigned to a particular offense to future recidivism behavior. Further, we merge CJARS with federal socio-economic data, linking individual's criminal justice outcomes to an extensive array of economic and social outcomes held by the U.S. Census Bureau, including IRS tax filings and Decennial Census survey responses.

While Michigan and Texas represent diverse institutional contexts, we find consistent, well-identified null estimates of the impact of the DRFs in both states, assuaging concerns about potential violations of the exclusion restrictions and strengthening the external validity of our findings. Our results align and extend the short-run findings from Pager et al. (2022). We find no significant evidence of short or long-run impacts of the DRFs on overall recidivism. These are true null effects; we can rule out effects greater than +/-1.8 (+/-2.3) percent with 95% confidence for likelihood of recidivism 10 years after DRF assignment in Michigan (Texas). We also find no sustained impact on long-term labor market outcomes. Similar to our recidivism outcomes, we can rule out effects greater than +/-0.7 (+/-4.4) percent with 95% confidence for cumulative likelihood of receiving a W-2 tax return from 2005–2015 in Michigan (Texas). We also find no evidence of spillover impacts of the DRF program onto the romantic partner. Paralleling the findings of our main analysis, we find that romantic partners do not change their labor supply or their future recidivism behavior.

While our findings align with the results of Pager et al. (2022), our evidence contrasts with other papers in the literature. Unlike Hansen (2015) and Dusek and Traxler (2022), we do not find that greater fines deter future convictions, which may be due to differences in research designs[5] or the institutional context being studied.[6] We also do not find evidence consistent with research finding negative impacts of LFOs on individual financial outcomes (Kessler 2020; Luh

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[5]The research design employed in Hansen (2015) relies on the discontinuous change in punishment arising from exceeding a blood alcohol threshold that both alters the size of the financial penalties and the risk of incarceration. As a result, it is unclear whether the reduction in future crime can be attributed to the financial sanctions.

[6]Dusek and Traxler (2022) studies discontinuous changes in fine amounts from speeding in Prague, a context with lower fines ranging ($39 and $82) and different subgroup characteristics. Further, the only punishment for failure to pay speeding tickets in Prague is another fine ($62-$200).

2020; Mello 2021). Whether this is due to different types of fines (DRFs versus parking tickets and traffic tickets), institutional differences across jurisdictions (Michigan and Texas versus Chicago, Illinois and Florida) or research designs (regression discontinuity versus difference-in-differences and event study) is a topic for future research.

This paper makes several important contributions to the literature. First, we analyze two distinct state-level natural experiments and provide consistent, robust, causal estimates on the effects of financial sanctions. Second, we are able to study a diverse set of outcomes (recidivism, employment, earnings) to fully capture the potential effects of LFOs. Third, our use of a regression discontinuity design permits us to study treated and non-treated individuals over the same follow-up period, reducing potential violations of the exclusion restriction that exist for other identification strategies. Fourth, we provide evidence on the impacts to both short and long-term behavior, including following individuals for up to 10 years. Fifth, our unique data allows us to test for potential spillovers to other household members for the first time, which is important since justice-involved individuals are economically vulnerable, which may lead them to rely on others to pay off financial sanctions. Measuring potential spillovers onto household members helps better characterize the full impact of the policy.

While our findings are less pessimistic than some prior research, we still conclude that DRFs are a regressive form of funding for the government with limited benefits in terms of labor market outcomes or criminal convictions. We observe no change in the rate of DRF-related offending in the general population and no fall in recidivism in our study sample, suggesting no evidence of general or specific deterrence responses. Given the low income of our sample, the impacts of the policy were concentrated on those less likely to pay the fees, placing them at higher risk for driver's license suspension. While unmeasured, it is possible that consumption declined in response to the fines without a commensurate change in income to compensate for the negative financial shocks.[7]

The remainder of the paper is as follows: Section 2 describes the policy change and judicial system in Michigan and Texas; Section 3 describes the data used in this analysis; Section 4 describes the empirical methodology and provides evidence to support the identification strategy; Section 5 presents the results; and we conclude in Section 6.

2 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

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[7]It is perhaps unsurprising that both of these state policies were eventually discontinued in Texas and Michigan due to their lack of success and unpopularity.

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).[8]

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,[9] 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).

2.1 Institutional details of the DRF program in Michigan and Texas

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

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

[9]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

split between the state's trauma system and the Texas Mobility Fund.[10] 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.

Coincidentally, the governor of Michigan signed its own version of the Driver Responsibility Program, or Public Act 165, into law on August 11, 2003, with an effective date of October 1, 2003. Unlike Texas, the DRF would be enforced by the Michigan State Treasurer as its revenue would be directed toward the state's General Fund. We explore whether the differences in administrative responsibilities led to different responses to the DRFs in Appendix B.

In Michigan, this fee 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). Texas' version of the DRFs was similar except the fees were applied over three consecutive years and had a fourth tier of $2,000 for repeat DUI convictions (Price 2008; Adair 2013).

In order to unify the two programs in our study, we classify the Category 2 type of offenses as non-DUI and DUI offenses. Table 1 shows a detailed list of the offenses and the proportion of DRF-eligible convictions within each state. Category 1 fees are not a part of the study so our estimates of total DRFs assigned to individuals represent lower bounds for the actual DRFs.

While certain details of the DRF varied across state, both Michigan and Texas used the same punishment for failure to pay the fines. Failure to pay the fees after 30 days in Texas or 60 days in Michigan 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 in both Michigan and Texas 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 in both states. In Texas, the state saw a significant jump in the number of drivers with suspended license 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).

For Michigan, over 137,000 drivers 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 fell into this self-perpetuating cycle of legal debt. By the time that the law was repealed in 2018,

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[10]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.

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). In the same report, Texas was ranked first with 1.8 million suspended licenses (Salas and Ciolfi 2017).

The DRF was also criticized in both states for failing to meet the planned collection rate or to improve driver 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). Similarly, in Michigan, 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.[11] At the time of nullification, the state forgave approximately $630 million in outstanding driver responsibility payments (Carrasco 2018). Texas followed suit and repealed its own version of the law on September 1, 2019. 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.

3   Data

We leverage several sources of rich population-level data, including criminal records from the Criminal Justice Administrative Record System (CJARS, Finlay and Mueller-Smith (2022)), longitudinal earnings data from IRS W-2 information returns, and romantic partner linkages compiled from a combination of survey and administrative data held by the U.S. Census Bureau. All of these data were analyzed within the restricted environment of the Federal Statistical Research Data Center system, where data can be linked at the person level using the anonymous Protected Identification Key (PIK) identifier.

One significant advantage of using CJARS criminal justice data is the multi-jurisdictional data integration. Adjudication data is oftentimes disaggregated across different agencies (ex. county v. district courts) within a state, which makes harmonization of criminal records difficult. For example, some agencies may only record offense description text fields while others may record the local ordinance number as the offense description. With the data from CJARS, statewide criminal

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[11]The repeal in Michigan only covered the Category 2 fees, which is the focus of this study. Category 1 fees were repealed in 2011.

justice data is already harmonized across the multiple local agencies and states making statewide analyses of criminal justice data significantly easier, especially when studying reforms across multiple states.

Michigan and Texas' DRF-related offenses that form the basis of our sample are identified from the criminal court filings contained in CJARS. Because cases may evolve endogenously over the course of prosecution (including initial charging decision), we include all DRF-related charges, which we define to include public intoxication, disorderly conduct, driving without a license on person, running a red light or stop sign, excessive speeding, and highway obstruction.[12]

We observe some variation in the level of criminal justice data from the state agencies, which contributes to differences in the distribution of DRF-related offenses within each state. Michigan's data consists of a significant amount of data from local agencies (ex: municipalities). Thus, we observe a higher proportion of low level of offenses in Michigan (i.e driving without a license) compared to Texas, where a majority of data came from state-level agencies, and carry a higher proportion of more severe offenses (ex: DUI). We discuss other agency and institutional differences in greater detail in Section 4.

To avoid having defendants show up multiple times in the analysis, we restrict our sample to the first observed DRF-related conviction per individual.[13] We identify all future convictions observed in CJARS, which includes Michigan and Texas as well as a number of additional states, to measure recidivism outcomes. Convictions are broken out by offense level (misdemeanor versus felony) and offense type (e.g., drug, property, violent, etc). We define recidivism using the time length between offense date or filing date of the new conviction to the original DRF-qualifying offense conviction date.[14] For example, if an individual convicted on September 1, 2005 re-offends on June 1, 2007 and is convicted of that new offense on December 1, 2007, we would consider that as recidivism within 2-years of the original conviction.

We use IRS W-2 information returns from 2005 to 2015 to measure employment and earnings activity.[15] We define earnings as the sum of inflation-adjusted wages across all W-2 filings in a given period.[16] One major benefit of using W-2s is that they cover all formal employment regardless of the duration of the employment. As such, they are not affected by endogenous tax filing

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[12]We discuss this charging behavior in further detail along with potential manipulation of the cutoff in Appendix B.

[13]We use conviction date as our running variable since the DRF law affected only cases convicted on or after the DRF effective date. For individuals who were convicted of two or more DRF related offenses in the same day, we retain the record associated with the highest potential DRF level.

[14]Offense date is missing for most of our data in Michigan while filing date is missing for most of our data in Texas. When both dates are available, we use the offense date.

[15]Unfortunately, available W2 data does not extend by to 2003 or earlier.

[16]All earnings and criminal fines and fees are inflated to 2017 dollars using the Consumer Price Index for All Urban Consumers (CPI-All Urban).

behavior inherent in IRS 1040 individual tax returns.[17] Because of this, W-2 reported income is our preferred measure of annual earnings despite IRS 1040 tax returns information being available beginning in 1998. Furthermore, if an individual works for multiple employers in one year, each of the employers must issue a W-2 tax return. We can use the number of W-2 returns filed in a year on behalf of an individual as a measure of the number of jobs that individual worked. The downsides of using administrative tax records to measure labor market outcomes is that we are limited to formal employment; in addition, we will not be able to observe work done as a contractor.

We link individuals to their partner or spouse using a wide array of government data including the 2000 and 2010 Decennial Censuses, IRS 1040 tax returns, housing assistance data from the Department of Housing and Urban Development, American Community Survey responses, and other survey and administrative records that identify romantic partnerships between individuals over time.[18] Romantic relationships of interest in our sample are married romantic, unmarried romantic, and unclassified romantic partnerships.[19] Once we have linked an individual with a DRF-qualifying offense to a romantic partner whose relationship inception predates the DRF-related offense, we are able to draw on the same IRS and CJARS data to identify the corresponding labor market outcomes and criminal behavior for the partner. This enables us to test how pre-existing relationships and partners' outcomes are affected by the fees.[20]

Finally, we leverage Census Bureau survey and administrative records to identify demographic characteristics so that we do not have to rely on possibly mismeasured analogues in court records.[21] We use date of birth and gender records from the 2020 Census Bureau Numident file, which is based on the Social Security Administrations Numident register. For race and ethnicity information, we use the Census Bureau 2016 Title 13 race and ethnicity file, which combines self-reported and administrative records of an individual's race/ethnicity from various sources, such as the Census Numident and the 2000 and 2010 Decennial Censuses.

4   Research design and methodology

We exploit the discontinuous implementation of the DRF policy in Texas and Michigan to overcome potential endogeneity of the assignment of financial sanctions. The statute only applied to

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[17]Employers are required to file W-2 returns if an employee earns at least $600 in a tax year.

[18]See Finlay, Mueller-Smith, and Street (2022) for more details on how these links were identified.

[19]Unclassified romantic is defined as pairs of individuals who are associated as co-parents in any of our government data without further relationship information available.

[20]A limitation of this approach is that we are less likely to observe informal relationships, such as unmarried romantic relationships that do not involve cohabitation, since they are unlikely to jointly file taxes, co-reside, or respond to household surveys together.

[21]Hispanic ethnicity is especially underreported in criminal justice administrative records (Eppler-Epstein, Gurvis, and King 2016; Ford 2015).

individuals convicted of a DRF eligible offense on or after the policy's effective date. Therefore, individuals convicted of the same offense one day prior to October 1, 2003 in Michigan or September 1, 2003 in Texas, would not be subject to the additional fine, whereas those convicted on or after the implementation dates would face a DRF penalty. Given the policy design, we utilize a sharp regression discontinuity designed to compare outcomes for individuals convicted of the same crimes right before and after the policy implementation. Under standard assumptions, the difference in outcomes can be attributed to the policy change at the discontinuity. In order to have a causal interpretation, the change in policy must be the only variable correlated with the outcomes to shift. In other words, the convictions in the neighborhood of the discontinuity are as good as randomly assigned so there should be no differences in caseload size or composition.

Throughout the analysis, we use the sharp regression discontinuity (RD) design defined above with initial DRF conviction date as the running variable. Specifically:

$$Y_i = \beta_0 + \beta_1 Post_i + \beta_2 ConvictionDate_i + \beta_3(Post_i \times ConvictionDate_i) + X_i \delta + \varepsilon_i \qquad (1)$$

where Y_i is an outcome for individual i; $Post_i$ is an indicator variable equal to one if an individual was convicted after the DRF effective date; and $ConvictionDate_i$ is the date of conviction running variable. The coefficient of interest, β_1, gives the impact of receiving the DRF after the cutoff. X_i includes covariates to increase the plausibility of our design; these covariates are all of the variables listed in Table 2, which are age at conviction, sex, pre-conviction 1040 filing rate, any prior criminal convictions, the predicted indices, and likelihood of being in a romantic relationship the year of conviction. We use a 540 day bandwidth in our main estimates. As a robustness check, we present results where we vary the bandwidth from 360 to 900 days in 30 day increments (see Figure A1), exclude covariates, and use a nonparametric analysis (see Table A1). Our results are robust across all of these different specifications.

4.1 Caseload density and balance tests

This research design relies on the identifying assumption that whether justice-involved individuals had their cases convicted just before (not subject to a DRF) versus just after the DRF effective date (subject to a DRF) is as good as random. There are many theoretical reasons why this identifying assumption, however, may not hold. For instance, if individuals change their behavior in response to the increased penalties introduced under the DRF regime, often referred to as a general deterrence effect, we would observe a drop in caseload across the discontinuity and potentially selection on observable characteristics. Even if individuals do not change their underlying behavior, other factors might introduce bias into the natural experiment. Sympathy from government agents such as police, prosecutors, or judges might reduce the number of individuals charged with a DRF-

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related offense. Conversely, more aggressive enforcement or delayed court filings in response to financial incentives (see Makowsky, Stratmann, and Tabarrok (2019)) could produce the opposite effect. Additionally, higher fees could incentivize defendants to hire private or specialized defense attorneys to negotiate the offense to a lesser charge or to avoid conviction altogether.

The common implication across these potential sources of bias is the prediction of a discontinuous change in either the caseload size or caseload composition. Figure 1 panel A documents the average caseload density within a 60-day window for all DRF-related offenses in our research window in Michigan. On average, there were over 12,000 DRF-related offenses per 60-day window, with minimal change over the analysis period, suggesting minimal influence of general deterrence. This agrees with public accounts stating the policy did not generate the desired reduction in driving offenses (Wild 2008).

For Texas, we observe short-run manipulation of the conviction dates in the 60 days before and after the cutoff that complicates our sharp RD design. The primary driver of this manipulation is defendants who were charged with a DRF-eligible offense before the cutoff who expedited their case disposition in order to avoid having their conviction fall after the implementation date.[22] Naturally, the ability to leverage this strategy was time-limited; once charge dates fell after the cutoff, no amount of expedited case processing could avoid a DRF penalty. As such, to recover causal impacts in this context, we use a "donut" RD design where we omit individuals convicted within the 60 days before and after the cutoff in Texas. After omitting these observations, we observe smoothness in the caseload density shown in Figure 1 panel B (grey dots) with estimates shown in Table 2. For the Texas portion of figures in our paper, we show the scatter points within the donut (red points) to document this manipulation, but we exclude these observations when estimating our RD models. Compared to Michigan, the caseload density of DRF-related offenses in Texas is lower with over 6,000 DRF-related cases convicted per 60-day window.

Even if the caseload size remained unchanged, it might be possible that the caseload characteristics adjust across the discontinuity. Table 2 reports regression discontinuity estimates for caseload size, the summary indices, and observable socio-economic traits at the time of conviction. Consistent with the graphical evidence, nearly all balance test estimates are statistically insignificant and close to zero supporting the causal interpretation of our research design. We estimate significant, but small, changes after Michigan's DRF effective date in the following: increase of 1 percentage point in the likelihood of being black, decrease of 1 percentage in the likelihood of being white, a decrease in likelihood of filing a 1040 tax form of 1 percentage point, and an increase of 0.3 years in age at conviction. These estimates are all small when compared to the overall sample averages. All other estimates are statistically indistinguishable from zero. For Texas, we observe no signifi-

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[22]Evidence on this sorting behavior and who was able to take advantage of this avoidance mechanism is presented in detail in Appendix B.

cant difference in observable characteristics except for a significant decline in age of 0.4 years. In addition, we also show that the estimated probability of linking to a romantic partner in the year of DRF conviction is unchanged at the discontinuity for both states. This last fact is used to justify our empirical analysis of the effects of the fees on partner labor supply in Section 5.2.

Panels A–D of Figure 2 plot two out of the three summary indices that together capture potential movements in caseload composition for our analysis sample. The two plotted summary indices are predicted cumulative W-2 earnings from 2005–2015 and predicted cumulative number of convictions over 10 years. We also predict likelihood of DRF-related recidivism within 4 years of the focal conviction.[23] We use predicted DRF recidivism in Section 5 to explore whether attenuation of our estimates is driven by contamination of the control sample.

To generate predicted values, we use the full interaction of age at conviction for DRF offense, gender, race/ethnicity, the full interaction of average annual 1040 income, average 1040 form filing rates 1–3 years prior to conviction, having any previous convictions, and also include fixed effects for the county of adjudication. The follow-up periods vary by outcome as a result of both data availability and measurement periodicity (e.g. annual tax returns versus criminal convictions with exact disposition date), but largely cover the same follow-up period.

Since we do not use the cutoff in constructing these summary indices, the identifying assumptions of the sharp RD design would imply smoothness in the predicted variables across the cutoff. This is indeed what we find across all of our predicted indices in Figure 2 where the coefficients are small, relative to the sample averages, and statistically insignificant. This provides further evidence in support of the identifying assumption of our research design.

Overall, we do not observe systematic sorting of an economically meaningful magnitude for any of our demographic variables or summary indices. Instances of statistically significant imbalance reflect the large size of our analysis sample, and not economically meaningful variation; our findings remain effectively unchanged with or without the inclusion of control variables for observable characteristics as shown in Table A1, suggesting any potential biases introduced from sample imbalance is minimal.

We quantify the first-stage relationship in Figure 3 and Table 3, which show the changing likelihood of being subject to a driver responsibility fee and the total driver responsibility fees assigned conditional on being convicted of a DRF-related offense for individuals in our sample over time in panels A and B respectively. Unsurprisingly, we find a sharp jump of 95% and 74% in the likelihood of being assigned a DRF after the law goes into effect in Michigan and Texas, respectively.[24] Similarly, on the intensive margin, the average DRF amount increased from $0

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[23]Due to limits on the number of results that can be disclosed by the Census Bureau in order to protect individual privacy, we were unable to release visual evidence for each covariate included in Table 2.

[24]Since we include some non-DRF eligible offenses (listed in Section 3, the first stage estimate is not 100%.

to over \$1,431 and \$2,504 after the DRF implementation date. As shown in Figure A2, these changes reflect substantial increases over the prevailing rate and level of non-DRF LFOs, and did not displace other types of fines and fees after implementation.

5 Results

We split our discussion of the impacts of financial sanctions into two categories: (1) direct effects on individuals convicted of DRF-qualifying offenses and (2) spillovers of DRFs onto romantic partners.

5.1 Direct impacts on labor market outcomes and recidivism

Our main analysis examines the direct effect of DRFs on individuals' own cumulative employment and recidivism outcomes. Employment and earnings are measured using IRS W-2 and 1040 information returns between 2005 and 2015.[25] Future criminal convictions are measured using all DRF and non-DRF CJARS court filings up to 10 years following the original DRF-related conviction. As noted in Section 3, the follow-up periods differ slightly across outcomes due to differences in measurement periodicity and the time frame of data availability.

Figure 4 shows a graphical plot of the long-run earnings and recidivism findings for both Michigan and Texas, with estimated treatment effects and additional results for both extensive and intensive recidivism and labor market outcomes presented in Table 4. In both natural experiments, we observe no statistically significant or economically meaningful impacts of the DRFs on long-term outcomes, including: employment rates, earnings, any future convictions, and total future convictions.

In Michigan (Texas), annual employment rates change by 0.0 (0.4) percentage points and the average number of employers per year change by -0.001 (0.011) over the 2005 to 2015 follow-up period. With regard to earnings, we see total W2 earnings between 2005 and 2015 change by \$4,480 and \$212 in Michigan and Texas, or \$407 and \$19 per year. None of these changes are statistically significant.

Likewise, there is minimal change in future recidivism. The likelihood of receiving any future convictions over the next 10 years changes by 0.2 (0.0) percentage points in Michigan (Texas), with total accumulated convictions changing by 0.005 and 0.010 convictions in Michigan and Texas, or 0.0005 and 0.001 criminal incidences per year. Further analysis of the intensive margin comes to similar conclusions regarding: total felony convictions (MI: -0.004, TX: -0.010), violent convictions (MI: -0.003; -0.001), property convictions (MI: -0.002; TX: 0.003), and drug convictions (MI: -0.001; TX 0.000).

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[25]Data limitations unfortunately preclude measuring W-2 returns in 2004 or earlier.

An interesting caveat to these findings is that we do find a statistically significant increase in the likelihood of individuals being convicted of driving with a suspended license in Michigan: 1.3 percentage points (↑ 5.8%). Since the punishment for failing to pay the DRFs was license suspension, individuals who were unable to pay their DRFs but also could not afford to stop driving (e.g. commuting to work) were essentially forced to regularly violate the law. Driving each day without a valid license risked criminal charges and conviction if they happened to be pulled over by law enforcement (a low but non-zero risk), which appears to be what we are capturing. We do not observe similar impacts in Texas, which could be due to differences in our ability to capture local ordinance violations in the data,[26] differences in enforcement practices, or true null effects.

Because of the size of our sample and the sharp experimental variation, we can be confident in the precision of our null result findings. Specifically, we can rule out average employment declines greater than 0.53% and individual earnings declines greater than 2.84% in both natural experiments. Likewise, we can rule out increases in recidivism greater than 2.54% (extensive margin) and 4.4% (intensive margin).[27]

Long-term, average treatment effects may obscure important nuances in the data analysis. Short-run impacts might be muddied by accumulated noise during our 10+ year follow-up windows. Similarly, treatment effect heterogeneity might be glossed over, especially if a minority subset of the population is especially vulnerable to these financial shocks. To address these concerns, we expand the analysis along several dimensions: (1) the timing of impacts over the follow-up period (Figure 5), and (2) heterogeneous treatment effects by socio-economic/subgroup characteristics (Figure 6).

In Figure 5, we explore the cumulative year-by-year evolution of the impact of DRFs on cumulative earnings and total convictions. In general, these estimates largely confirm our prior analysis. Overall, we observe null effects across all of the outcomes and follow-up periods except in Michigan where we observe growing, positive, but insignificant, impacts on earnings over time. We also explore the contemporaneous impacts of the DRFs in Appendix Figure A3 where we find no significant impact in any of the follow-up years.

In Figure 6, we plot subgroup treatment effect estimates for long-run total earnings and total convictions. We stratify the subgroup analysis by baseline socio-demographic traits (race, sex, age), criminal history, predicted income levels, and predicted likelihood of DRF recidivism to probe whether our null results are driven by heterogeneous response in caseload composition or fee affordability.[28] Overall, there is limited evidence of heterogeneous treatment effects. Most

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[26]For instance, the average 10 year likelihood of being convicted of driving with a suspended license in Michigan 22.6% whereas in Texas the same statistic is only 4.1%.

[27]Given low recidivism rates in this sample, the percent effects for future crime outcomes are not especially comparable to the full justice-involved population.

[28]See Table A2 for the estimates presented in tabular format.

estimates are not statistically distinguishable from the null hypothesis or from each other. One exception is that we observe a positive impact on earnings in Michigan for those whose background characteristics would predict them to higher than median income in our sample. That said, the estimated effect only represents a 3% change relative to the control mean ($342,100 over 2005 to 2015), and the statistical significant could purely be a product of multiple hypothesis testing.

Contamination of the control sample due to future DRF recidivism may also attenuate the estimated impacts of the DRFs. Individuals convicted before the discontinuity often go on to eventually be convicted of new DRF-related offenses after the effective date, thereby exposing the control group to DRF sanctions. Failure to reject the null hypothesis that LFOs have no impact on future outcomes could be either due to control group contamination or that the underlying treatment effects are in fact close to zero, a critical distinction to make.

To address this issue, we use the predicted likelihood of DRF recidivism from Table 2. We evenly split our sample into "high" and "low" contamination groups based on the median predicted risk of DRF recidivism in the four years following the focal conviction. The high contamination group is more likely to repeatedly commit DRF-related offenses, generating the DRF sanction spillovers in the control group that we are concerned about. The low contamination group is unlikely to exhibit such behavior, providing stronger experimental integrity over the duration of the follow-up period.

The bottom part of Figure 6 shows the RD estimates by contamination group. We find no significant impact, and in fact largely null estimates on all outcomes by contamination group. Thus, it appears that our null impacts are not driven by attenuation from contamination of the control group.

5.2 Effects of DRFs on romantic partners

While we generally find that the driver responsibility fees have small or null effects on labor market and recidivism outcomes of DRF recipients, DRFs 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 initial DRF conviction (and resulting license suspension), exposing them to greater risk of getting charged with a traffic-related offense themselves. To measure these partner spillovers, we use the household crosswalk discussed in Section 3 that synthesizes information from a variety of Census Bureau, IRS and other federal program data. This crosswalk allows us to link individuals convicted of DRF-related offenses to their partners in the year of their initial DRF conviction.

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In order to identify the causal impact of DRFs on partner outcomes, we first establish balance in the likelihood of being linked to a pre-existing partner across the DRF effective date. Returning to Table 2, we find no effect of the fines on the likelihood an individual charged with a DRF offense is linked to a romantic partner in the year of conviction.

Figure 7 shows a variety of long-term outcomes of interest for romantic partners: the likelihood of remaining in a relationship in the top panel, the length of the relations, total earnings, total number of convictions, and total number of DRF-eligible convictions in the bottom panel.

We find no evidence of spillover impacts of the DRFs on partnership rates or partner outcomes. Not only are estimates statistically insignificant, but relative to the mean, the effect sizes and standard errors are small, indicating precise, null effects. These results suggest that earlier findings of null direct impacts of labor market and recidivism outcomes in Michigan and Texas were not confounded by secondary impacts on partnership length, partner's labor supply, or partner's criminal charges. We also do not find any evidence that the DRFs placed romantic partner's at higher risk of getting a DRF criminal conviction themselves.[29]

We again stratify our results by the same set of subgroup characteristics in our direct impact analysis with our results shown in Figure 8. The grouping characteristics describe that of the individual convicted of the DRF-related offense, not the partner. Here we find largely null impacts on earnings and recidivism. We do find a large, significant increase in recidivism for the romantic partner's of Black and older (age $\geq$ 30) individuals. But, again, caution should be taken when interpreting the subgroup analysis due to multiple hypothesis testing and the potential for a spurious finding.

6 Conclusion

In this paper, we examine the impact of financial sanctions in the state of Michigan and Texas, leveraging the abrupt introduction of sizeable fines associated with the Driver Responsibility Fees (DRF) program in each of these states. While these states present diverse contexts, both institutional and demographically, the programs were nearly the same in both states. We find consistent, null impacts of the DRFs on labor market and recidivism outcomes in both states over the entire ten year follow-up period of our study. The degree of consistency across these two natural experiments is perhaps surprising given differences in fine levels implemented by the two states: in Michigan average amount of LFOs increased by only $1,431 while in Texas fines increased by $2,504 on average.

Leveraging extensive data linkage to track household spillovers, we also find no impact on the

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[29]Because we do not measure non-criminal violations resulting in DRF points (e.g. speeding), it is possible that partners experienced changes in fees that we cannot measure because they are beyond the scope of our data.

romantic partner's own labor market and recidivism outcomes nor relationship outcomes, ruling out spillover impacts of the DRFs. To the best of our knowledge, our research is the first to to account for secondary impacts on romantic partners, examine the impacts of financial sanctions on such a large sample, and follow outcomes for such an extended time period.

Further investigation of these results that disaggregate the analysis by narrower follow-up windows and heterogeneous subgroups indicate that the main findings (null long-term effects) do not obscure impacts concentrated during specific points in time or subsets of the overall population.

While our findings starkly contrast with prior descriptive work (Harris, Evans, and Beckett 2010; Pleggenkuhle 2018), they are consistent with and extend the conclusions of Pager et al. (2022) - the only randomized control trial evidence to date regarding the impact of criminal legal debt. We build on this path breaking work through (1) replicating their recidivism findings from Oklahoma County in two statewide populations (Texas and Michigan), (2) evaluation of employment and earnings outcomes, (3) the inclusion of household spillovers, and (4) increasing the outcome window of the analysis sample to 10 years.

While we find no significant harm on individuals' labor market outcomes or criminal behavior, we also find limited evidence of benefits to justify this policy. As a source of revenue generation, the DRF was an inefficient and regressive form of taxation. Funds were being raised from individuals with lower than average income.[30] It is therefore unsurprising that DRF payment rates were quite low, reducing revenue, and placing these individuals at higher risk of recidivism due to driver's license revocation from non-payment.

The failure of these policies to achieve their aims was perhaps already self-evident to policymakers and residents in these jurisdictions. Ultimately, the DRF policies in Michigan and Texas were discontinued after roughly 15 years with a total of $3.1 billion in debt forgiven. Whether these lessons from the DRF program should apply to justice-involved individuals facing non-traffic criminal charges is an important area for future research to better understand the total impact of financial sanctions within the U.S. justice system.

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[30]Average per capita, annual personal income in 2005 in Michigan (Texas) was just over $37,000 ($41,000), adjusted to 2017 dollars (U.S. Bureau of Economic Analysis and Federal Reserve Bank of St. Louis 2021). From Table 4, average annual income per capita, adjusted to 2017 dollars, in our sample is just over $20,000 ($22,000) from 2005 to 2007 using W-2 information.

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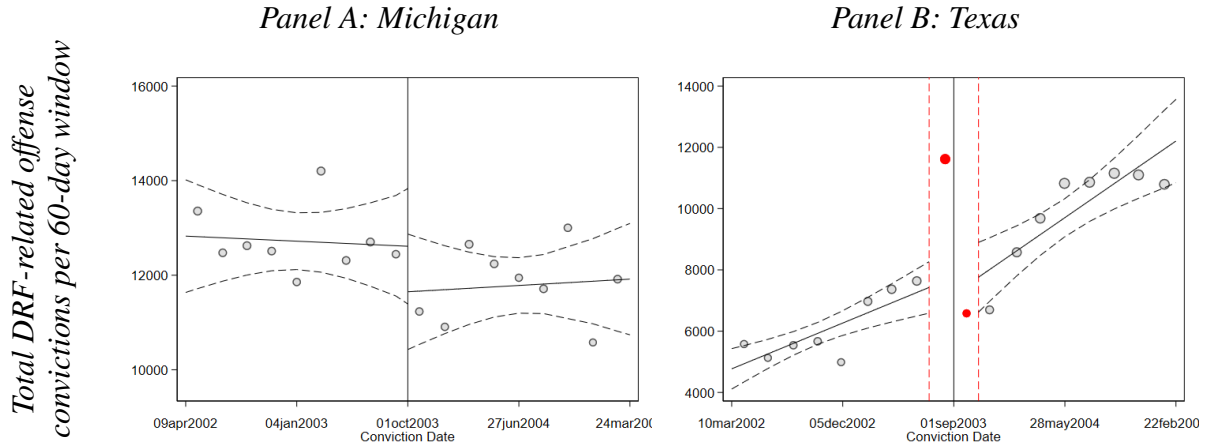
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Figures

Figure 1: Caseload density of analysis sample, by conviction date relative to effective date of Michigan Public Law 165 (October 1, 2003) and Texas House Bill 3588 (September 1, 2003)

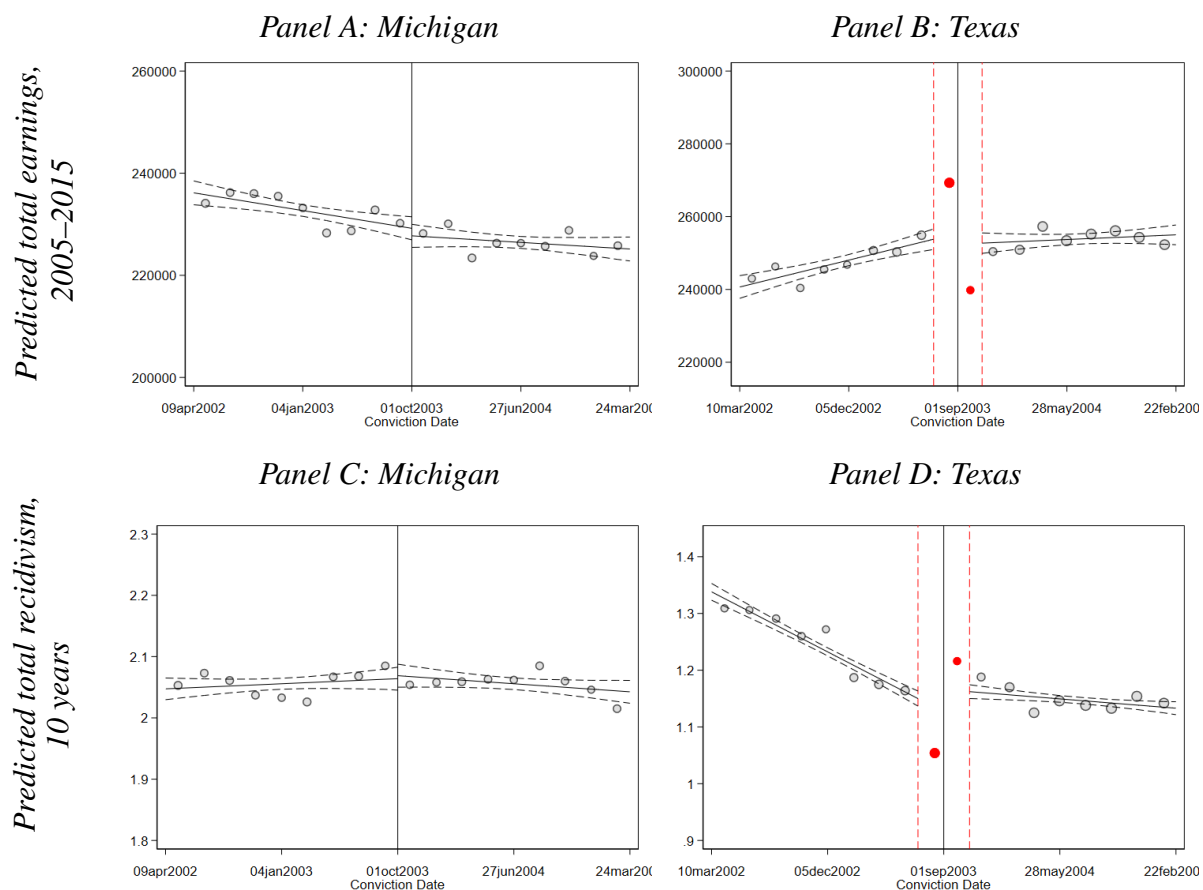
Panel A: Michigan *Panel B: Texas*

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: These figures show the visual representation of the sharp RDD estimates (solid, black) and 95% confidence intervals (black, dashed) of the effect of the DRFs on the average number of DRF-related convictions within a 60-day window in Michigan (panel A) and Texas (panel B).

RD Figure Notes: Scatter points are binned using 60-day windows with the size of the circle denoting the number of observations within each bin. The black, solid vertical line denotes the cutoff. The red, dashed, vertical line denotes the donut (60-day window surrounding the cutoff; Texas only). Predicted fit lines are generated using a sharp, linear RDD where conviction date is the running variable. Red data points (Texas only) reflect excluded observations within the donut and are provided for completeness even though they do not contribute to RD estimates. RD specification choices are described in Section 4. The estimation sample for each state is described in Section 3.

Figure 2: Summary characteristics, by conviction date relative to effective date of Michigan Public Law 165 (October 1, 2003) and Texas House Bill 3588 (September 1, 2003)

Notes: These figures show the visual representation of the sharp RDD estimates (solid, black) and 95% confidence intervals (black, dashed) of the effect of the DRFs on predicted total earnings measured using 2005–2015 W-2 tax returns (adjusted to 2017 dollars using the CPI-All Urban) and cumulative recidivism ten years after the focal conviction. See Section 4.1 for the creation of predicted indices.
RD Figure Notes from Figure 1 apply.

Figure 3: Assignment of driver responsibility fee (DRF) in analysis sample relative to effective date of Michigan Public Law 165 (October 1, 2003) and Texas House Bill 3588 (September 1, 2003)
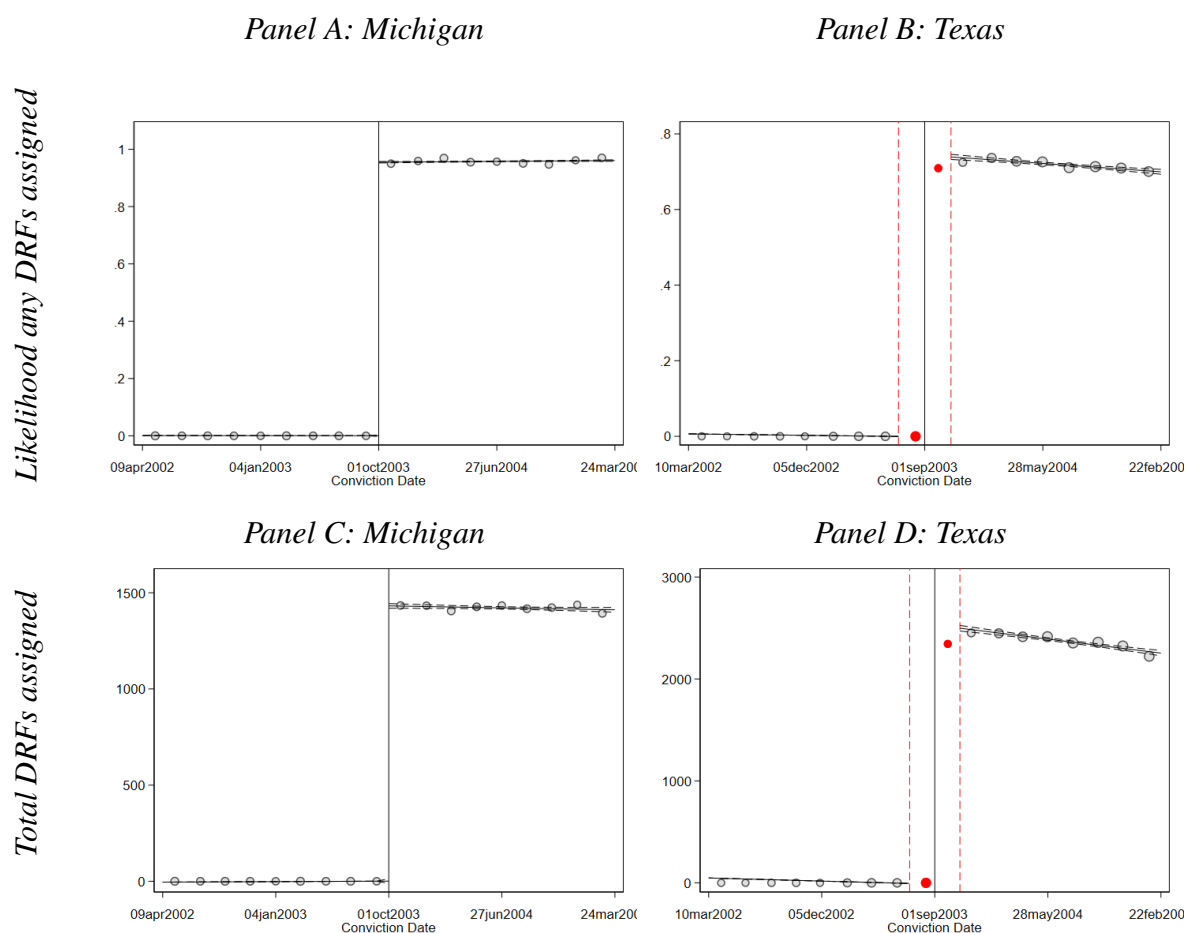

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011 and CBDRB-FY22-ERD002-015. Note: These figures show the visual representation of the sharp RDD estimates (solid, black) and 95% confidence intervals (black, dashed) of the effect of the DRFs on the likelihood of DRF assignment (panels A and B) and the total DRFs assigned (panels C and D, 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 all summary indices, demographic traits, relationship status, and pre-conviction information in Table 2.
RD Figure Notes from Figure 1 apply.

Figure 4: 10-year earnings and recidivism outcomes in analysis sample relative to effective date of Michigan Public Law 165 (October 1, 2003) and Texas House Bill 3588 (September 1, 2003)
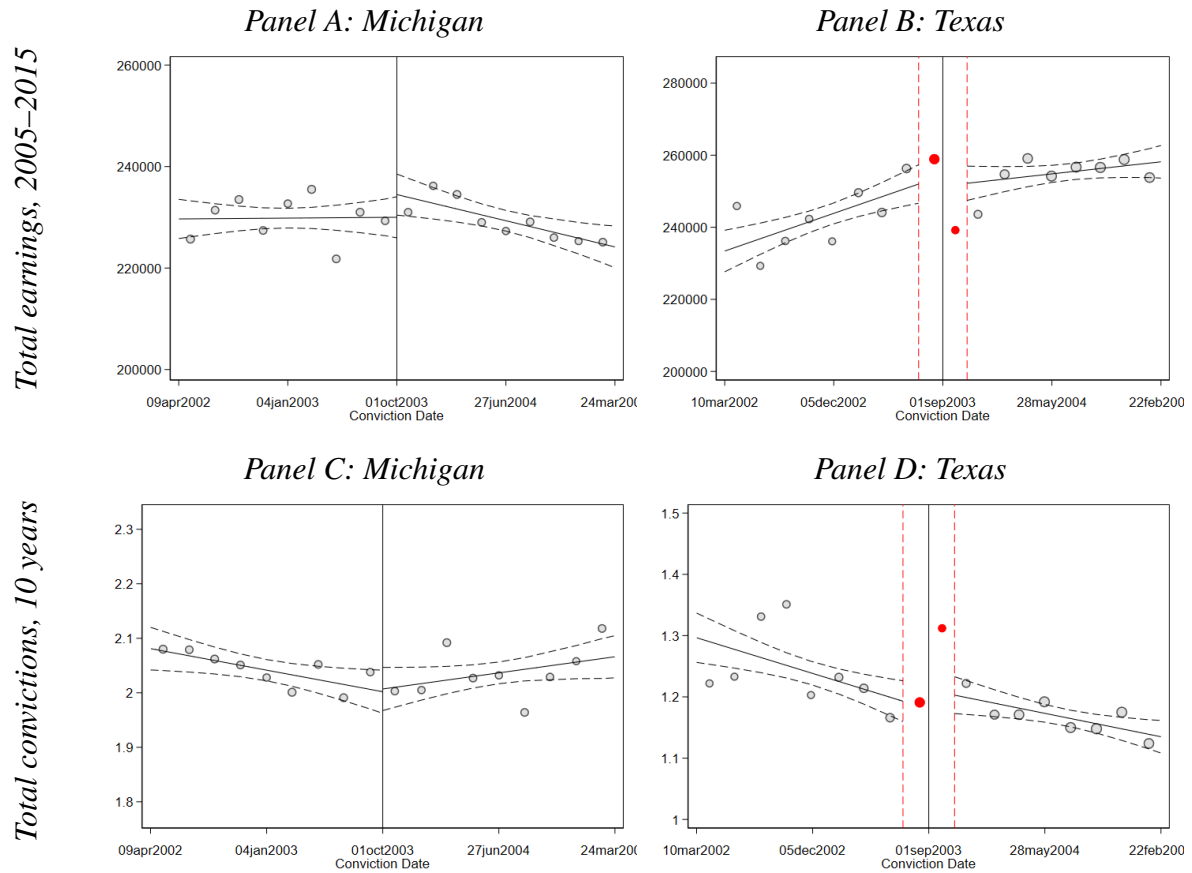

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011 and CBDRB-FY22-ERD002-015.
Note: These figures show the visual representation of the sharp RDD estimates (solid, black) and 95% confidence intervals (black, dashed) of the effect of the DRFs on the total earnings measured using W-2 tax returns from 2005 to 2015 (panels A and B, adjusted to 2017 dollars using the CPI-All Urban) and cumulative recidivism 10 years after the focal conviction (panels C and D). Outcome variables are residualized (with the mean from observations used in the RD estimate added back) using all summary indices, demographic traits, relationship status, and pre-conviction information in Table 2.
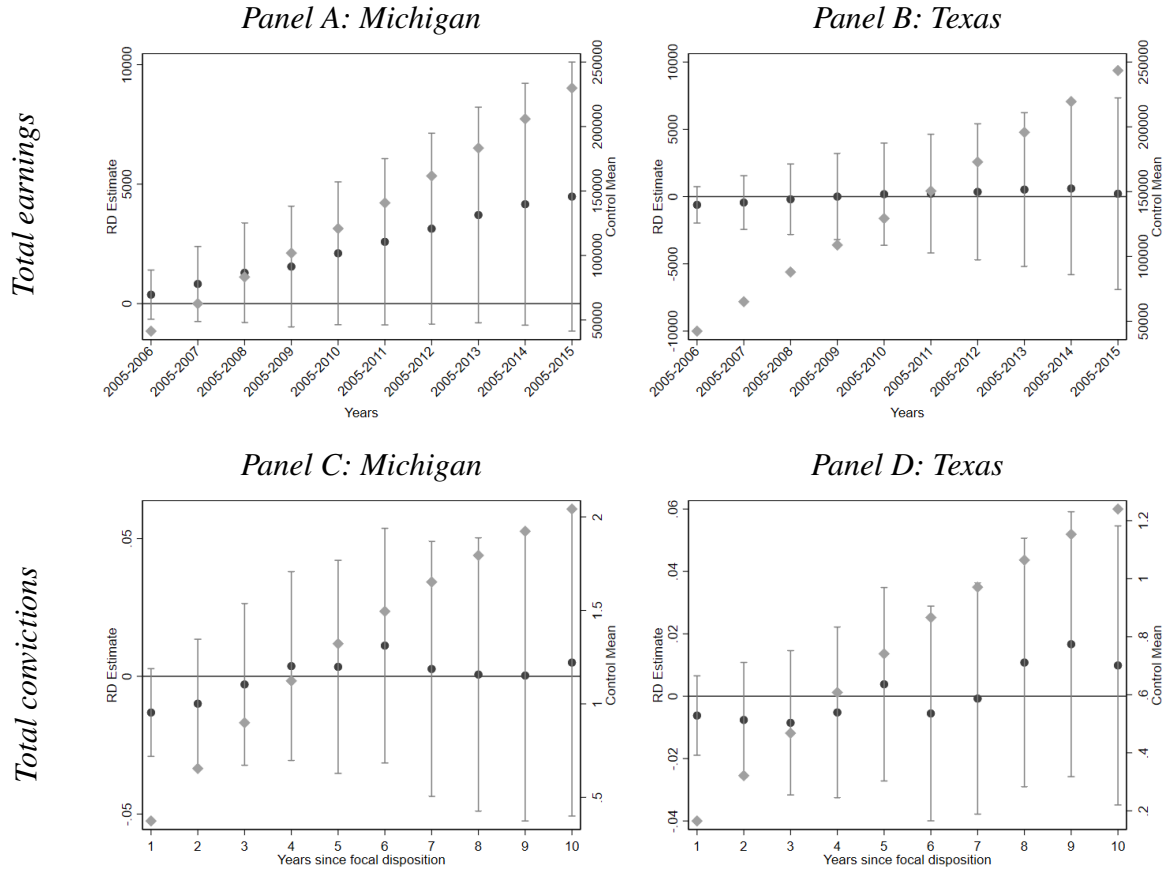*RD Figure Notes* from Figure 1 apply.

Figure 5: Evolution of RD-based causal estimates over the 10 year follow-up period, by state

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This figure plots the sharp regression discontinuity design (RDD) estimates (dark grey, circles) measuring the effects of DRFs on labor and recidivism outcomes over a cumulative time period that varies by graph. 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 2005–2006 to 2005–2015. For the recidivism outcomes (panels C and D) the time frame is between 1 and 10 years following conviction of first DRF-related offense. 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. The estimation sample for each state is described in Section 3.

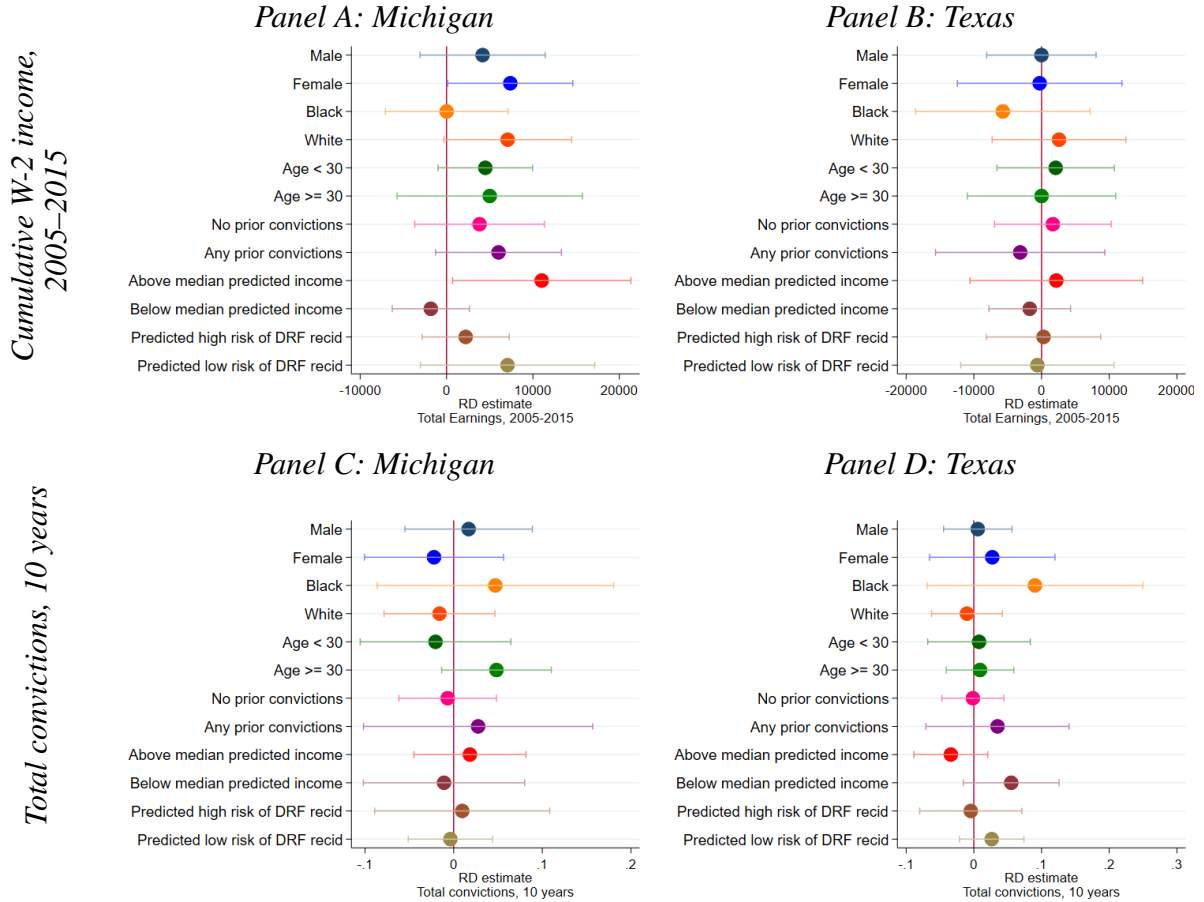**Figure 6:** Causal impact of driver responsibility fees on future earnings and convictions, by sub-group

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This figure presents the sharp RDD estimates for the effects of DRF conviction on total earnings (adjusted to 2017 dollars using the CPI-All Urban, panels A and B) measured using W-2 tax returns and total convictions (panels C and D) across various subgroups noted in the Y-axis. RD estimates are plotted on the graph (circles); 95% confidence intervals are included in a lighter shade 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 DRF-conviction. High (low) risk of DRF recidivism is defined as having above (below) median risk for predicted DRF recidivism rates 4 years after the focal conviction. See Section 4.1 for the creation of predicted indices. See Table A2 for results in tabular format. RD specification choices are described in Section 4. The estimation sample for each state is described in Section 3.

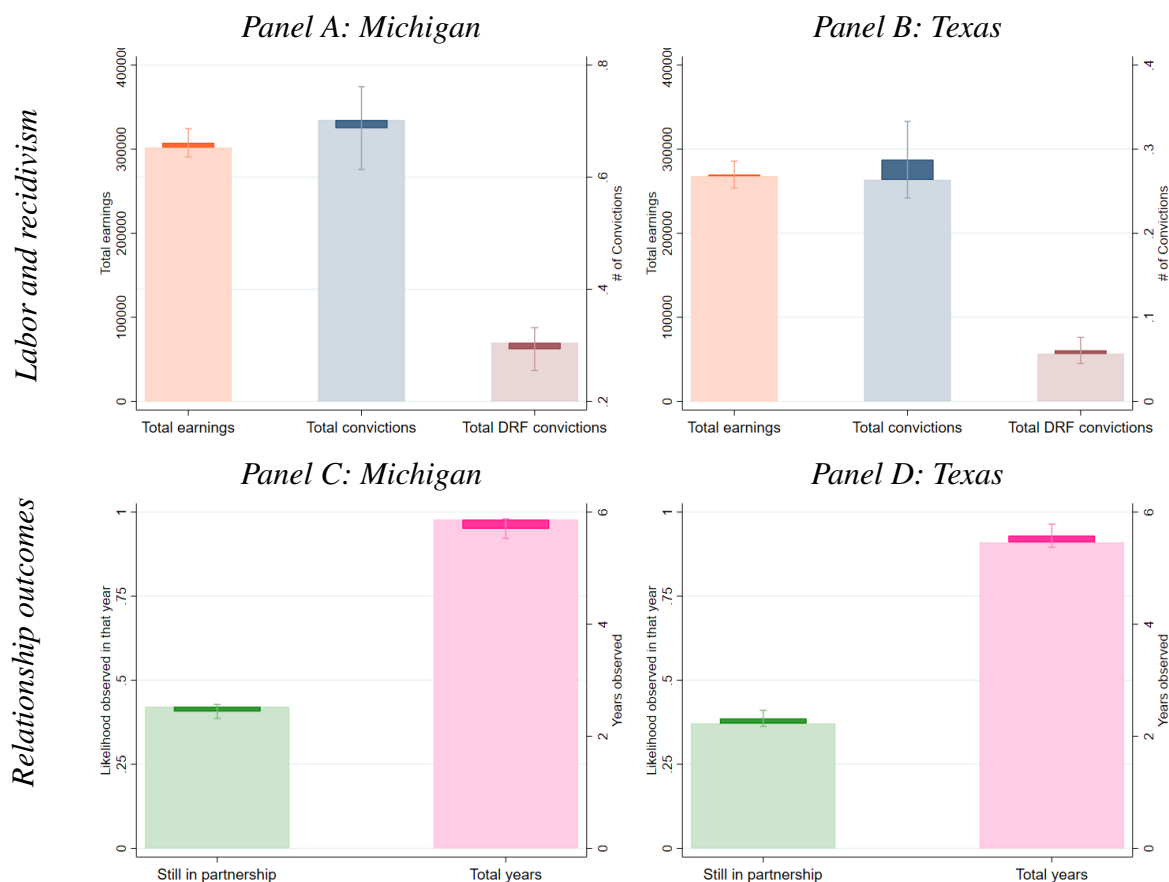**Figure 7:** Causal impact of driver responsibility fees on relationship and romantic partner's future earnings and convictions

Panel A: Michigan

Panel B: Texas

Panel C: Michigan

Panel D: Texas

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This figure presents the sharp RDD estimates measuring the effects of DRFs on labor and recidivism outcomes on the romantic partner's outcomes. Outcomes in panels A and B are total earnings, measured using cumulative W-2 earnings from 2005 to 2015 (adjusted to 2017 dollars using the CPI-All Urban), total recidivism 10 years after the focal conviction, and total DRF convictions, 10 years after the focal conviction. Outcomes in panels C and D are likelihood the individual is still in a relationship with the romantic partner in 2015 and total years observed together from 2005 to 2015. 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 A3 for results in tabular format. RD specification choices are described in Section 4. The estimation sample for each state is described in Section 3.

Figure 8: Causal impact of driver responsibility fees on romantic partner's future earnings and convictions, by subgroup
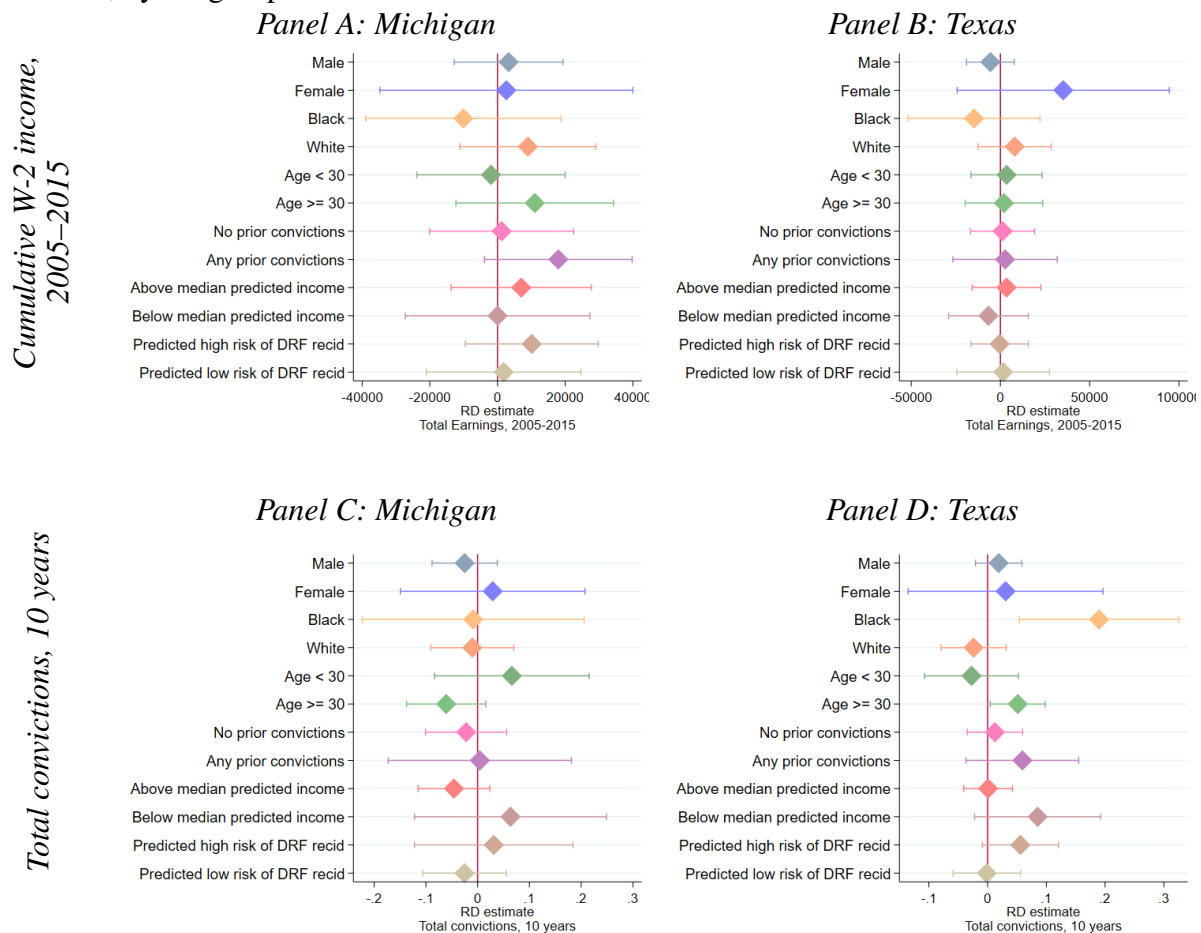

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This figure presents the sharp RDD estimates for the effects of DRF conviction on romantic partner's total earnings (adjusted to 2017 dollars using the CPI-All Urban, panels A and B) measured using W-2 tax returns and total convictions (panels C and D) across various subgroups of the individual, assigned the DRF, 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 DRF-conviction. High (low) risk of DRF recidivism is defined as having above (below) median risk for predicted DRF recidivism rates 4 years after the focal conviction. See Section 4.1 for the creation of predicted indices. See Table A4 for results in tabular format.

RD specification choices are described in Section 4. The estimation sample for each state is described in Section 3.

Tables

Table 1: Driver responsibility fee amounts and eligible offenses

DRF Type	Fee amounts	Eligible offenses	% of total DRF offenses in MI	% of total DRF offenses in TX
Non-DUI	$300-600	• Driving with an expired/invalid license • Driving on a suspended/revoked license	29% 31%	0.2% 9.1%
DUI	$1,000-6,000	• Operating while intoxicated or under the influence • Operating while intoxicated, second or subsequent conviction or BAC>0.16	29% NA	77% 13%

Source: Authors' calculations using the Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Offense classification by DRF level is from the Michigan Department of State and the Texas Department of Public Safety.

Notes: This table presents the list of offenses associated with the driver responsibility fee (DRF) assigned upon conviction. This list includes offenses enumerated under Michigan Public Act 165, Category 2, which was in effect from October 1, 2003 to October 1, 2018 and Texas House Bill 3588, Category 2, which was in effect from September 1, 2003 to September 1, 2019. Top two most frequently convicted offenses are included across each category. Rounded distributions of violations within each level are shown in the last column.

Table 2: Evaluating balance of selected observable characteristics and predicted earnings and criminal activity in the analysis sample on the DRF effective date

	Sample→	Michigan		Texas	
		Control mean	RD estimate (standard error)	Control mean	RD estimate (standard error)
Variable					
Caseload size:					
Average daily DRF caseload		212	-16.05	101.9	5.542
			(14.28)		(11.99)
Summary Indicies:					
Predicted Income		232,800	-1,467	247,800	-1,083
			(1,623)		(2,025)
Predicted Recidivism		2.056	0.005	1.236	0.012
			(0.013)		(0.009)
Predicted DRF Recidivism		0.280	0.000	0.140	0.000
			(0.001)		(0.001)
Demographic Traits:					
Male		0.709	0.005	0.832	0.003
			(0.004)		(0.004)
White, non Hispanic		0.706	-0.012**	0.633	-0.003
			(0.004)		(0.006)
Black		0.225	0.013***	0.134	0.005
			(0.004)		(0.004)
Hispanic		0.029	-0.000	0.205	-0.001
			(0.002)		(0.005)
Age at Disposition		30.94	0.314**	33.09	-0.417**
			(0.108)		(0.128)
Any prior convictions		0.317	0.000	0.290	0.006
			(0.004)		(0.005)
Pre-conviction 1040 information:					
Pre-disposition average 1040 household income		26,130	89.97 (491.8)	22,040	-521.1 (546.2)
Pre-disposition average 1040 filing rate		0.653	-0.010**	0.630	0.006
			(0.004)		(0.005)
Pre-conviction relationship status:					
% Matched to romantic partner in year of disposition		0.182	-0.005 (0.004)	0.195	-0.003 (0.005)
Observations		187,000		124,000	

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This table presents the sharp RDD estimates for select characteristics describing the individual at the time of conviction. See Wages and income are adjusted to 2017 dollars using the CPI-All Urban. Section 4.1 for creation of predicted indices.

RD Notes: Coefficients are estimated using a linear, sharp regression discontinuity design where conviction date is the running variable. The regression includes linear controls for the conviction date and the interaction of conviction date with the treatment variable, an indicator for if the case was disposed after the state's DRF effective date. The estimation sample for each state is described in Section 3. Standard errors are enclosed in parentheses and control means are enclosed in brackets. ∗ p<0.1, ∗∗ p<0.05, ∗∗∗ p<0.01.

Table 3: Evaluating assignment of driver responsibility fee (DRF) in the analysis sample on the DRF effective date

First Stage Outcomes		
Sample→	Michigan	Texas
Driver Responsibility Fees:		
Extensive Margin	0.954***	0.739***
	(0.001)	(0.003)
	[0]	[0]
Intensive Margin	1,431***	2,504***
	(6.28)	(14.13)
	[0]	[0]
Observations	187,000	124,000

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This table presents the sharp RDD estimates for the likelihood of DRF assignment and total DRFs assigned at the time of conviction. Total DRFs are adjusted to 2017 dollars using the CPI-All Urban.

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

Table 4: Impact of driver responsibility fees on long-term labor market and recidivism outcomes

Margin→	Extensive		Intensive	
Sample→	Michigan	Texas	Michigan	Texas
Employment, 2005–2015:				
Average employment rate per year	-0.001	0.011		
	(0.003)	(0.003)		
	[0.881]	[0.864]		
Average number of employers per year	-0.001	0.011		
	(0.006)	(0.010)		
	[1.014]	[1.058]		
Total earnings			4,480	212.2
			(2,873)	(3,633)
			[229,800]	[243,500]
Total household earnings			6,372	-662.7
			(4,938)	(5,560)
			[340,300]	[314,200]
Recidivism, 10 years:				
Any conviction	0.002	0.000	0.005	0.010
	(0.004)	(0.005)	(0.028)	(0.023)
	[0.539]	[0.434]	[2.042]	[1.240]
Driving with a suspended or revoked license conviction	0.013***	0.002		
	(0.004)	(0.002)		
	[0.226]	[0.041]		
Felony conviction			-0.004	-0.010
			(0.010)	(0.013)
			[0.357]	[0.542]
Violent conviction			-0.003	-0.001
			(0.005)	(0.006)
			[0.161]	[0.148]
Property conviction			-0.002	0.003
			(0.009)	(0.009)
			[0.272]	[0.225]
Drug conviction			-0.001	0.000
			(0.007)	(0.008)
			[0.223]	[0.222]
Observations	187,000	124,000	187,000	124,000

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This table presents the sharp RDD estimates for DRF conviction on labor outcomes from 2005 to 2015 and on recidivism outcomes ten years after the focal conviction. 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, Kilmer, and Muller-Smith (2022) for details on offense classification.

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

Appendices

Appendix A Supplementary Results

Figure A1: Robustness to alternative bandwidths

Panel A: Michigan   *Panel B: Texas*

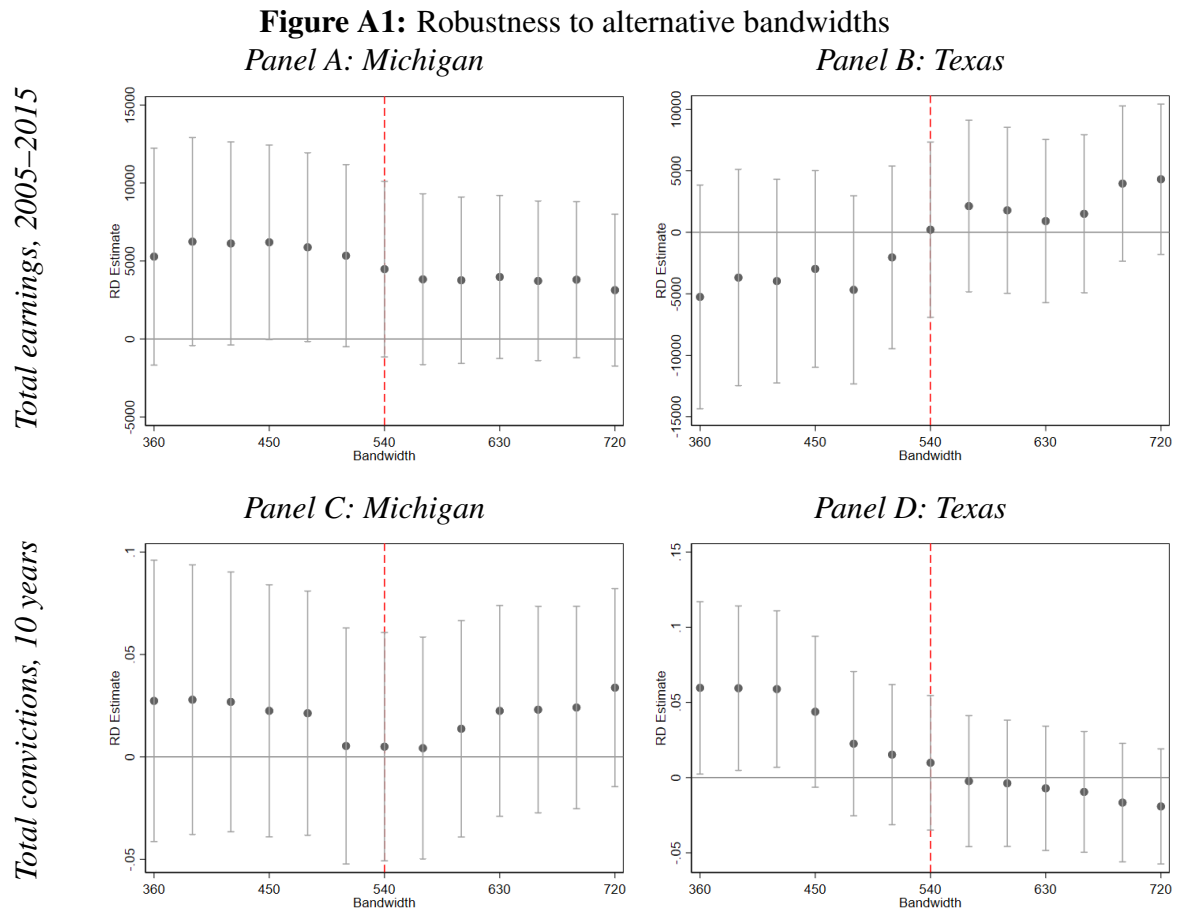*Panel C: Michigan*   *Panel D: Texas*

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This figure plots the sharp RDD estimates measuring the effects of DRFs on labor (panels A and B) and recidivism (panels C and D) outcomes for varying bandwidths (x-axis) ranging from 360–720 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. The estimation sample for each state is described in Section 3.

Figure A2: Total DRFs (circles, black) and non-DRF sanction fines (diamonds, blue)

Panel A: Michigan   *Panel B: Texas*

Source: Authors' calculations from Texas criminal justice histories from the CJARS 2020Q3 vintage.
Note: These figures show the visual representation of the sharp RDD estimates (solid, black) and 95% confidence intervals (black, dashed) of the effect of the DRFs on the total DRFs assigned (circles, black) and total non-sanction (diamonds, blue), non-DRF fines assigned upon conviction. All fines and fees have been adjusted to 2017 dollars using the CPI-All Urban. Outcome variables were residualized using data available in the CJARS data.
RD Figure Notes from Figure 1 apply.

Figure A3: Contemporaneous evolution of RD-based causal estimates over the 10 year follow-up period, by state
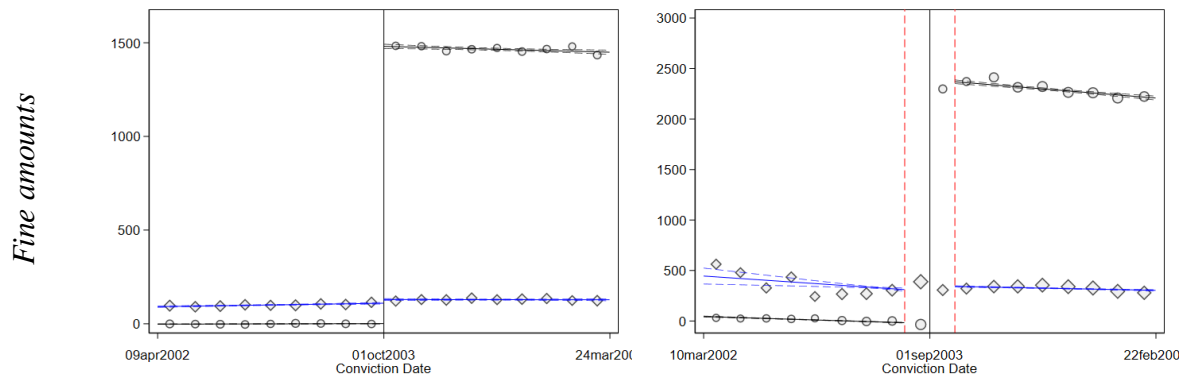

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This figure plots the sharp RDD estimates (dark grey, circles) measuring the effects of DRFs on labor and recidivism outcomes (not cumulative) for the year denoted in the x-axis. Annual earnings (adjusted to 2017 dollars using the CPI-All Urban, panels A and B) is measured using income reported on W-2 tax returns for the year denoted in the x-axis. The time frame covered is from 2005 to 2015. For the recidivism outcomes (panels C and D) the time frame is between 1 and 10 years following conviction of first DRF-related offense. 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. The estimation sample for each state is described in Section 3.

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

Sample→	Michigan		Texas	
	Non-parametric estimation	No Covariates	Non-parametric estimation	No Covariates
First Stage:				
Total DRFs Assigned	1,439***	1,432***	2,611***	2,504***
	(16.04)	(6.414)	(104.1)	(14.23)
	[0]	[0]	[0]	[0]
Employment, 2005–2015:				
Total earnings	-5,168	3,017	-510.6	-784.7
	(7,096)	(3,311)	(225,100)	(4,192)
	[235,600]	[232,800]	[237,700]	[239,400]
Average number of employers	-0.016	-0.011*	0.029	0.024**
per year	(0.014)	(0.007)	(0.068)	(0.011)
	[1.002]	[1.018]	[1.039]	[1.054]
Recidivism, 10 years:				
Total convictions	-0.032	0.011	0.300	0.023
	(0.063)	(0.031)	(0.584)	(0.025)
	[2.068]	[2.042]	[1.309]	[1.295]
Total felony convictions	-0.017	-0.001	-0.145	-0.003
	(0.021)	(0.010)	(0.225)	(0.014)
	[0.356]	[0.356]	[0.560]	[0.560]

 Note: This table presents the sharp RDD estimates for DRFs applied upon conviction for certain offenses. Total earnings is measured using income reported on W-2 tax returns. Average number of employers is measured using number of W-2 tax returns received that year. Wages and fees are adjusted to 2017 dollars using the CPI-All Urban.
 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 common coverage error rate optimal bandwidth selector. Same set of covariates in our main specification are included.
 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.

Table A2: Causal impact of driver responsibility fees on future earnings and convictions by subgroup

Panel A: Demographic Characteristics

Sample→	Male	Female	Black	White	Age < 30	Age ≥30	No prior convictions	Any prior convictions
Michigan								
Total earnings, 2005–2015	4,176	7,383**	19.49	7,086*	4,490	5,005	3,825	6,020
	(3,700)	(3,697)	(3,622)	(3,777)	(2,791)	(5,477)	(3,828)	(3718)
	[250,200]	[181,200]	[152,100]	[257,200]	[221,600]	[238,900]	[253,000]	[178,400]
Total convictions, 10 years	0.017	-0.022	0.047	-0.016	-0.020	0.048	-0.007	0.028
	(0.037)	(0.040)	(0.068)	(0.032)	(0.043)	(0.032)	(0.028)	(0.066)
	[2.305]	[1.419]	[2.715]	[1.813]	[2.671]	[1.221]	[1.476]	[3.289]
Observations	131,000	55,000	43,000	131,000	106,000	81,000	128,000	58,000

Sample→	Male	Female	Black	White	Age < 30	Age ≥30	No prior convictions	Any prior convictions
Texas								
Total earnings, 2005–2015	-13.9	-272.3	-5,720	2,584	2,091	.1855	1,672	-3,151
	(4,121)	(6,202)	(6,573)	(5,042)	(4,423)	(5,591)	(4,397)	(6,380)
	[258,400]	[177,500]	[154,800]	[266,600]	[256,700]	[230,500]	[259,900]	[202,800]
Total convictions, 10 years	0.006	0.027	0.090	-0.010	0.008	0.009	-0.001	0.035
	(0.026)	(0.047)	(0.081)	(0.027)	(0.039)	(0.025)	(0.023)	(0.054)
	[1.296]	[0.985]	[1.902]	[1.105]	[1.651]	[0.862]	[0.990]	[1.869]
Observations	102,000	22,000	16,000	80,000	59,500	64,500	88,500	36,000

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This table presents the sharp RDD estimates for total earnings, measured using cumulative W-2 earnings from 2005 to 2015 (adjusted to 2017 dollars using the CPI-All Urban) and total recidivism 10 years after the focal conviction across various subgroups noted in the column titles. 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 DRF-conviction. High (low) risk of DRF recidivism is defined as having above (below) median risk for predicted DRF recidivism rates 4 years after the focal conviction. See Section 4.1 for the creation of predicted indices.

RD Notes from Table 2 apply.

Table A2: Causal impact of driver responsibility fees on future earnings and convictions by subgroup, *cont'd*

Panel B: Predicted indices

Sample→	Above median predicted income	Below median predicted income	Predicted High Risk DRF Recidivism	Predicted Low Risk DRF Recidivism
Michigan				
Total earnings, 2005–2015	11,020**	-1,821	2,219	7,061
	(5,268)	(2,279)	(2,572)	(5,151)
	[342,100]	[117,600]	[182,200]	[277,100]
Total convictions, 10 years	0.018	-0.011	0.010	-0.004
	(0.032)	(0.046)	(0.050)	(0.024)
	[1.462]	[2.625]	[3.116]	[0.967]
Observations	93,000	93,500	94,000	92,500

Sample→	Above median predicted income	Below median predicted income	Predicted High Risk DRF Recidivism	Predicted Low Risk DRF Recidivism
Texas				
Total earnings, 2005–2015	2,183	-1,740	301.8	-613
	(6,503)	(3,080)	(4,308)	(5,777)
	[359,900]	[127,300]	[241,900]	[243,400]
Total convictions, 10 years	-0.034	0.055	-0.004	0.026
	(0.028)	(0.036)	(0.038)	(0.024)
	[0.983]	[1.497]	[1.677]	[0.807]
Observations	62,000	62,000	62,500	62,000

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This table presents the sharp RDD estimates for total earnings, measured using cumulative W-2 earnings from 2005 to 2015 (adjusted to 2017 dollars using the CPI-All Urban) and total recidivism 10 years after the focal conviction across various subgroups noted in the column titles. 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 DRF-conviction. High (low) risk of DRF recidivism is defined as having above (below) median risk for predicted DRF recidivism rates 4 years after the focal conviction. See Section 4.1 for the creation of predicted indices.

RD Notes from Table 2 apply.

Table A3: Impact of driver responsibility fees on household composition and romantic partner's future outcomes

Panel A: Balance test			
	Sample→	Michigan	Texas
% Matched to romantic partner		-0.005	-0.003
in year of disposition		(0.004)	(0.005)
		[0.182]	[0.195]
Panel B: Relationship and partner outcomes			
Relationship survival and duration:			
Still Together, 2015		-0.014	0.015
		(0.011)	(0.012)
		[0.421]	[0.371]
Total years observed together, 2005-2015		-0.163*	0.119
		(0.089)	(0.105)
		[5.866]	[5.459]
Partner labor market and criminal outcomes:			
Total earnings, 2005-2015		5,631	1,688
		(8,587)	(8,232)
		[301,900]	[267,900]
Total Convictions, 10 Years		-0.014	0.024
		(0.038)	(0.023)
		[0.702]	[0.264]
Total DRF Convictions, 10 Years		-0.011	0.004
		(0.020)	(0.008)
		[0.304]	[0.057]
Observations		33,500	25,000

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This table presents the sharp RDD estimates for likelihood an individual is observed in a documented relationship in the year of DRF conviction, likelihood the individual is still in a relationship with the romantic partner in 2015, total years observed together from 2005 to 2015. The bottom portion of panel B presents the sharp RDD estimates of the DRFs on the romantic partner's total earnings(measured using cumulative W-2 earnings from 2005 to 2015 and adjusted to 2017 dollars using the CPI-All-Urban), total recidivism overall, and total DRF convictions 10 years after the focal conviction.

RD Notes from Table 2 apply.

Table A4: Causal impact of driver responsibility fees on romantic partner's future earnings and convictions by subgroup

Panel A: Demographic Characteristics

Sample→	Male	Female	Black	White	Age < 30	Age ≥30	No prior convictions	Any prior convictions
Michigan								
Total earnings, 2005–2015	3277	2,614	-10,080	8,973	-1,928	11,010	1,221	17,960
	(8,206)	(19,070)	(14,720)	(10,250)	(11,190)	(11,880)	(10,850)	(11,130)
	[257,400]	[390,100]	[277,600]	[308,300]	[254,500]	[328,100]	[323,000]	[237,600]
Total convictions, 10 years	-0.025	0.029	-0.009	-0.010	0.066	-0.061	-0.022	0.004
	(0.032)	(0.091)	(0.109)	(0.041)	(0.076)	(0.039)	(0.040)	(0.090)
	[0.417]	[1.252]	[0.884]	[0.658]	[0.991]	[0.537]	[0.606]	[0.997]
Observations	22,000	11,500	5,200	26,000	12,500	21,000	25,500	8,300

Sample→	Male	Female	Black	White	Age < 30	Age ≥30	No prior convictions	Any prior convictions
Texas								
Total earnings, 2005–2015	-5,623	35,200	-14,760	7,981	3,455	1,971	1,236	2,629
	(6,821)	(30,320)	(18,840)	(10,440)	(10,150)	(11,090)	(9,166)	(14,910)
	[233,100]	[440,700]	[277,400]	[282,800]	[238,200]	[286,600]	[271,900]	[255,900]
Total convictions, 10 years	0.019	0.031	0.190**	-0.024	-0.027	0.051**	0.012	0.059
	(0.020)	(0.085)	(0.069)	(0.028)	(0.041)	(0.024)	(0.024)	(0.049)
	[0.193]	[0.624]	[0.289]	[0.276]	[0.356]	[0.212]	[0.245]	[0.341]
Observations	23,500	4,900	2,800	19,000	11,500	17,000	21,500	7,300

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This table presents the sharp RDD estimates for the romantic partner of the individual convicted of a DRF-related conviction. The outcomes are total earnings measured using cumulative W-2 earnings from 2005 to 2015 (adjusted to 2017 dollars using the CPI-All Urban) and total recidivism 10 years after the focal conviction. The estimates are measured separately across various subgroups noted in the column titles. 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 DRF-conviction. High (low) risk of DRF recidivism is defined as having above (below) median risk for predicted DRF recidivism rates 4 years after the focal conviction. See Section 4.1 for the creation of predicted indices. *RD Notes* from Table 2 apply.

Table A4: Causal impact of driver responsibility fees on romantic partner's future earnings and convictions by subgroup, *cont'd*

Panel B: Predicted indices

Sample→	Above median predicted income	Below median predicted income	Predicted High Risk DRF Recidivism	Predicted Low Risk DRF Recidivism
Michigan				
Total earnings, 2005–2015	7,020	22.64	10,110	1,817
	(10,570)	(13,910)	(10,000)	(11,670)
	[342,000]	[205,500]	[222,600]	[338,400]
Total convictions, 10 years	-0.046	0.063	0.031	-0.025
	(0.035)	(0.095)	(0.078)	(0.041)
	[0.496]	[1.202]	[0.953]	[0.586]
Observations	23,500	10,000	10,500	23,000

Sample→	Above median predicted income	Below median predicted income	Predicted High Risk DRF Recidivism	Predicted Low Risk DRF Recidivism
Texas				
Total earnings, 10 years	3,443	-6,702	-410.9	1,561
	(9,811)	(11,380)	(8,190)	(13,260)
	[289,200]	[215,400]	[226,600]	[306,900]
Total convictions, 10 years	0.001	0.085	0.056*	-0.001
	(0.021)	(0.055)	(0.033)	(0.029)
	[0.187]	[0.471]	[0.276]	[0.270]
Observations	20,500	8,300	14,000	14,500

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

Note: This table presents the sharp RDD estimates for the romantic partner of the individual convicted of a DRF-related conviction. The outcomes are total earnings measured using cumulative W-2 earnings from 2005 to 2015 (adjusted to 2017 dollars using the CPI-All Urban) and total recidivism 10 years after the focal conviction. The estimates are measured separately across various subgroups noted in the column titles. 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 DRF-conviction. High (low) risk of DRF recidivism is defined as having above (below) median risk for predicted DRF recidivism rates 4 years after the focal conviction. See Section 4.1 for the creation of predicted indices. *RD Notes* from Table 2 apply.

Appendix B   Manipulation of conviction date in Texas

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 1 panel B, we observe a spike in the average number of cases disposed to the left of the cutoff reaching almost 12,000 cases per 60-day window, twice the regular caseload, and a corresponding drop immediately to the right of the cutoff. This is largely driven by the more expensive DUI-related DRF offenses (Figure B1 panel B). The spike and corresponding drop are suggestive of manipulation of the conviction date, which appears to be confirmed by discernible departures from caseload-wide trends in our summary indices of background characteristics and outcome plots (see Figures 2 and 4) for these same data points in Texas.

What subgroups of the population are able to take advantage of this manipulation, and how do they accomplish this? Figure B2 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 B3 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

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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 B3 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.
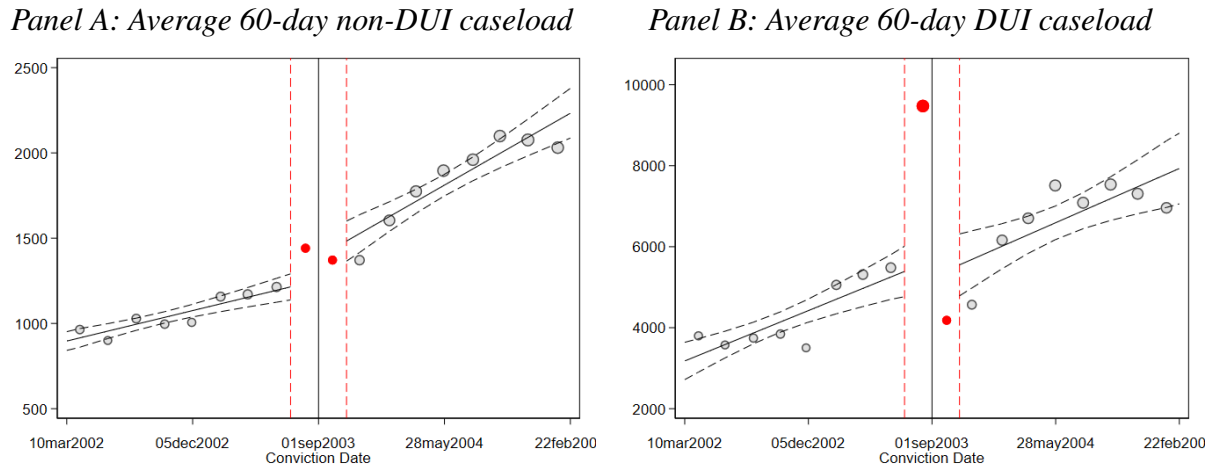
In either case, these figures imply disparate treatment in race and gender in the justice system, an interpretation supported by Doleac (2017), Abrams, Bertrand, and Mullainathan (2012), Arnold, Dobbie, and Yang (2018), Alesina and La Ferrara (2014), Arnold, Dobbie, and Yang (2018), and Depew, Eren, 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 1 panel A. 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 1 panel A and Figure B1 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 3). 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 B1: DRF-related caseload densities by DUI status, by conviction date relative to effective date of Texas House Bill 3588 (September 1, 2003)

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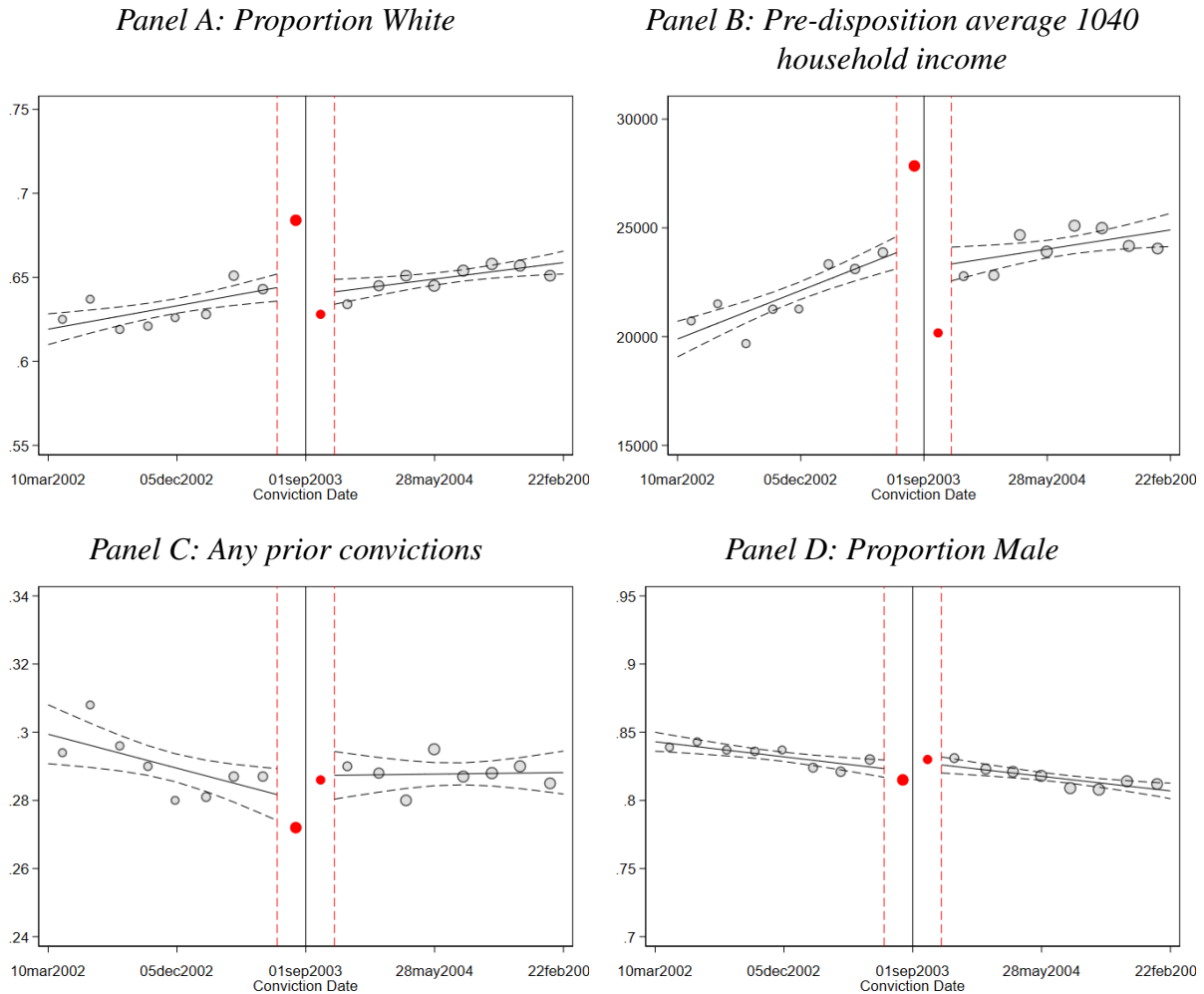

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.
Note: These figures show the visual representation of the sharp RDD estimates (solid, black) and 95% confidence intervals (black, dashed) of the effect of the DRFs on the average DRF-eligible convictions within a 60-day window for non-DUI DRF related offenses (panel A) and the average DRF-eligible convictions within a 60-day window for DUI DRF related offenses (panel B) in Texas.
RD Figure Notes from Figure 1 apply.

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Figure B2: Summary characteristics, by conviction date relative to effective date of Texas House Bill 3588 (September 1, 2003)

Panel A: Proportion White

Panel B: Pre-disposition average 1040 household income

*Panel C: Any prior convictions*

Panel D: Proportion Male

Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 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, and Michigan and Texas criminal justice histories from the CJARS 2020Q3 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial Census (2000) and American Community Survey (ACS) (2005–2018). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and CBDRB-FY22-ERD002-011.

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

Figure B3: Evidence of DRF avoidance behavior in Texas in response to House Bill 3588 (September 1, 2003)

Panel A: Days between offense date and disposition date

Panel B: Likelihood of non-DRF eligible traffic charge
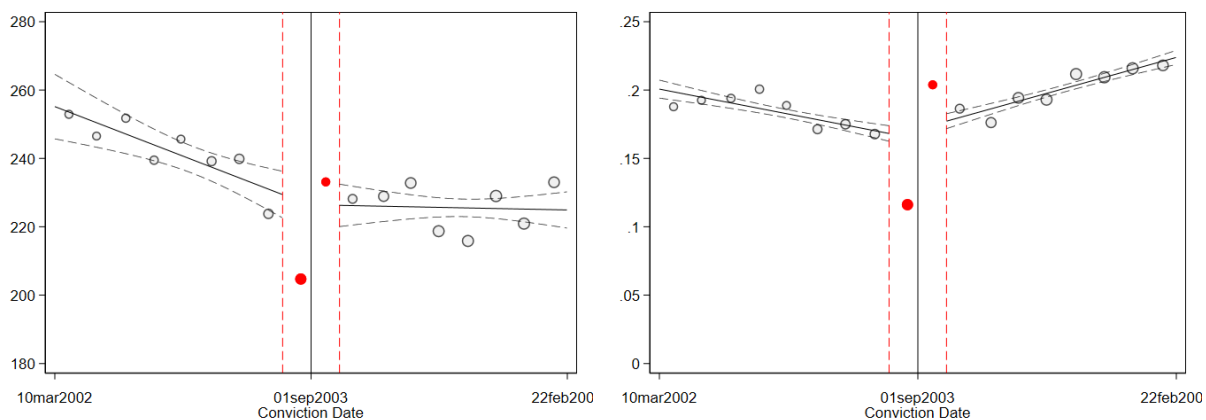
Source: Authors' calculations from Texas criminal justice histories from the CJARS 2020Q3 vintage.

Note: These figures show the visual representation of the sharp RDD estimates (solid, black) and 95% confidence intervals (black, dashed) of the effect of the DRFs on the number of days between offense date and disposition date and the likelihood of being convicted for a non-DRF eligible, but DRF-related charge. Outcome variables were residualized using data available in the CJARS data.

RD Figure Notes from Figure 1 apply.