

The Impact of Financial Sanctions in the U.S. Justice System: Regression Discontinuity Evidence from Michigan's Driver Responsibility Program*

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Abstract

We estimate the first regression discontinuity evidence on the impact of financial sanctions in the U.S. criminal justice system on recidivism, labor market, and household spillover outcomes. We exploit the abrupt introduction of Michigan's driver responsibility fees (DRF) on October 1st, 2003, and find null to modestly positive long-term impacts of reduced recidivism and increased earnings as well as evidence of earning spillovers to romantic partners for a subset of the caseload. Overall, without any evidence of a general or specific deterrence effect and modest success with debt collection, the DRFs do not appear to have been welfare improving.

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

JEL classification codes: H72, J24, K42

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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 Rebekah 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 (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 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.

Further causal evidence though remains quite limited given the lack of data availability and exogenous variation. Dusek and Traxler (2021) finds drivers who just exceeded an excessive speeding threshold and consequently received greater fines in Prague reduce their future speeding offenses. In contrast, Mello (2021) and Kessler (2020), which both use difference-in-differences and event-study research designs, find more negative impacts of LFOs on individual outcomes. Mello (2021) finds that driver fees in Florida are associated with short-term financial distress. Similarly, Kessler (2020) finds that the adoption of aggressive traffic and parking fine debt collection in Chicago increased bankruptcy filing rates.

In this paper, we produce the first regression discontinuity evidence on this topic, exploiting a discrete policy change in the state of Michigan that substantially increased the amount of financial sanctions faced by defendants. In 2003, Michigan passed Public Act 165, known as the driver responsibility fee (DRF) program, which mandated new fines to criminal defendants who

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

were convicted of certain traffic-related offenses. The goal of the act was to raise revenue for the government while improving driving safety.² The amount of the fines varied based on the severity of the offense, ranging from \$300 to \$2,000 and averaging \$1,200 per eligible offense. Failure to pay these fines would lead to driver's license suspension, which itself could lead to new DRF-qualifying offenses. In the first two years after the law was enacted, the state levied over \$260 million in driver responsibility fines to nearly 750,000 drivers; the annual number of citations for driving with a suspended license more than doubled with over 120,000 citations in 2006 alone (Wild 2008).

We leverage this source of exogenous variation to study the causal effect of financial sanctions on recidivism, labor market, and household spillover outcomes over the short and long-run. We use the Criminal Justice Administrative Records System (CJARS), which contains longitudinal, harmonized criminal histories allowing us to observe each individual's repeated interactions with the justice system. 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.

To estimate the long-term causal effect of the financial sanctions, we exploit the fact that the DRF program in Michigan applied only to individuals convicted of a DRF-eligible offense on or after October 1, 2003, a context well-suited for regression discontinuity analysis. Two key mechanisms potentially link DRF sanctions to behavioral responses: (1) an income effect generated from the fine itself, and (2) a driver's license revocation in the event of fine non-payment. License suspension may interfere with employment outcomes if an individual has no other transportation options available, or escalate their risk of future offenses if they choose to continue driving illegally without a license.

A challenge in studying this program is the relatively high rate of regular DRF-related offending behavior within this caseload, meaning that those to the left of the cutoff who originally avoided the DRF sanction go on to receive a DRF sanction when they reoffend in a year or two. Ignoring this issue might underestimate the total impact of the policy since the license revocation channel may be neutralized as control group members also experience license revocations over the follow-up period.

To address this issue, we develop a prediction model based on observable demographic information, criminal history, and pre-existing earnings profiles to distinguish between those likely and unlikely to commit a new DRF-related offense in the short-run.³ Among those with a low likeli-

²This program is not unique to Michigan as New Jersey, New York, Texas, and Virginia have all had similar programs.

³There is no imbalance across the discontinuity in being categorized as either being part of the high or low risk samples.

hood of DRF recidivism, the integrity of the experimental variation induced by the discontinuity is maintained over roughly the entire 10-year follow-up period. For the high contamination sample, the change in likelihood of getting one or more DRF sanctions across the discontinuity declines by over 50 percent within five years, severely curtailing our ability to capture the long-term impact of the license revocation mechanism for this subpopulation.

Compared to the majority of the literature, our results show more modest and less harmful behavioral responses to financial sanctions. For the *low contamination sample*, DRFs have no economically meaningful or statistically significant short or long-term impact on employment, earnings, or recidivism. For the *high contamination sample*, we observe a short-run increase in earnings. The evidence suggests that the income effect of legal debt immediately increases labor supply, which is then carried forward for a number of years, potentially as a result of path dependence. By ten years out, however, estimated impacts contract and lose statistical significance, which may be due to either experimental contamination over time in this subgroup or a true waning of the underlying treatment effect. There is no short or long-run impact on recidivism in this subgroup, which both aligns and extends findings from Pager et al. (2022).

We exploit the variation in fine levels by offense type introduced by the DRF program to explore how treatment effects vary by fee amount. The gains in earnings and reductions in recidivism are almost entirely driven by the Level 1 offenses which have the lowest associated financial penalty. As fee amounts increase, the positive impacts shrink towards zero, a pattern consistent with two countervailing forces at play: (1) the income effect stimulating labor supply, and (2) the driver's license suspension due to fee nonpayment contracting labor supply.

As a final analysis, we examine potential household spillovers of the DRF program. Paralleling the findings of our main analysis, we find that romantic partners of the high contamination group increase their labor supply, while the romantic partners of the low contamination group do not respond. Specifically, we find a 10% increase in the cumulative earnings of partners from 2005 to 2015 relative to a mean of \$202,900, suggesting other household members may be stepping in to help cover the cost of the LFO.

This paper makes several important contributions to the literature. First, we provide robust, causal estimates on the effects of financial sanctions on labor market and recidivism outcomes. Second, our unique data allows us to estimate these effects over a substantially longer time frame and test how they might generate spillovers to other household members, which helps better characterize the full impact of the policy. Third, we develop a methodological strategy to address bias arising from high recidivism rates in the context of research designs relying on policy discontinuities in time (see, for example, Doleac (2017), Tuttle (2019), Fishbane, Ouss, and Shah (2020), and Mueller-Smith and Schnepel (2021)).

While our findings are less pessimistic than 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 a general or specific deterrence response. 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 commiserate change in income to compensate for the negative financial shocks. Even more concerning, the positive labor market response of romantic partners indicate potential spillover effects within the household. Specifically, household members who did not commit the DRF-eligible offense may have borne the monetary burden of the fines.

The remainder of the paper is as follows: Section 2 describes the policy change and judicial system of the state of Michigan; 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 Michigan's driver responsibility fees law

In an effort to promote safer driving and increase state revenue, the governor of Michigan signed Public Act 165 into law on August 11, 2003. The legislation, which became effective on October 1, 2003, mandated new fines to defendants who were convicted of certain driving crimes.⁴ The DRF would be enforced by the Michigan State Treasurer as its revenue would be directed toward the state's General Fund. As a result, the DRFs were classified as administrative fines, rather than criminal penalties (Wild 2008).

The act created two categories of fees. Category 1 was for drivers who accrued seven or more driving or traffic violations in two years. Category 2, which is the focus of this research, fined drivers for specific violations ranging from driving without a driver's license to driving under the influence. 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 \$1000 fee for two consecutive years (Wild 2008). Table 1 shows a detailed list of the Category 2 type of offenses associated with each fee level.

One particular criticism of the DRF policy argued that its impact on poor defendants was especially onerous (Hausman 2013). Failure to pay the fees after 60 days led to the suspension of one's driver's license. Driving with a suspended license was itself a DRF-qualifying offense, so lower

⁴The law was modeled on similar legislation in New Jersey. Since the passage of Michigan's law, Texas, New York, and Virginia have each instituted their own version of DRFs.

income recipients may have been at risk of accruing multiple DRFs and substantial legal debt. In 2007, over 137,000 drivers were assessed a DRF for driving with a suspended license, an increase of 44% compared to 2005 (Wild 2008), indicating many individuals fell into this self-perpetuating cycle of legal debt. All outstanding DRFs along with an additional \$125 fee had to be paid in order to reinstate a license. By the time that the law was repealed in 2018, an estimated 317,000 drivers had had their driver's licenses suspended for failure to pay DRFs (Carrasco 2018).

The bill was also criticized for failing to meet the planned collection rate or to improve driver safety. 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 driver responsibility fee legislation and canceled all remaining debt owed under the law.⁵⁶ At the time of nullification, the state forgave approximately \$630 million in outstanding driver responsibility payments (Carrasco 2018).

3 Data

We leverage several sources of rich population-level data, including criminal records from the Criminal Justice Administrative Record System (CJARS), 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.

Michigan DRF-eligible offenses that form the basis of our sample are identified from the criminal court filings contained in CJARS. We use the offense at charge to identify all court cases from April 1, 2001 to March 31, 2006 that could trigger a DRF as defined in Michigan Public Act 165. We restrict our sample to the first observed DRF-eligible conviction per individual to avoid having defendants show up multiple times in the analysis.⁷

We identify all future convictions observed in CJARS, which includes Michigan as well as a number of additional states, to measure criminal behavior outcomes. Convictions can be broken out by offense level (misdemeanor versus felony), and offense type (e.g., drug, property, violent,

⁵Since Michigan's repeal, Texas and New Jersey have also repealed their own versions of the DRF law. Virginia repealed its law in 2009.

⁶This repeal only covered the Category 2 fees, which is the focus of this study. Category 1 fees were repealed in 2011.

⁷We use conviction date as our running variable since the DRF law affected only cases convicted on or after October 1, 2003. For individuals who were convicted of two or more DRF eligible offenses in the same day, we retain the record associated with the highest potential DRF level.

etc). Outcome are defined by time since the original DRF-qualifying offense.

We use IRS W-2 information returns from 2005 to 2015 to measure employment and earnings activity.⁸ We define earnings as the sum of inflation-adjusted wages across all W-2 filings in a given period.⁹ 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 behavior inherent in IRS 1040 individual tax returns.¹⁰ 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.¹¹ Romantic relationships of interest in our sample are married romantic, unmarried romantic, and unclassified romantic partnerships.¹² Once we have linked an individual with a DRF-qualifying offense to a romantic partner whose relationship inception predates the DRF-eligible 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.¹³

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.¹⁴ 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 Cen-

⁸Unfortunately, available W2 data does not extend by to 2003 or earlier.

⁹All earnings are inflated to 2017 dollars using the Consumer Price Index for All Urban Consumers (CPI-All Urban). Fines and fees generated from DRF-eligible offenses are not adjusted.

¹⁰Employers are required to file W-2 returns if an employee earns at least \$600 in a tax year.

¹¹See Finlay, Mueller-Smith, and Street (2021) for more details on how these links were identified.

¹²Unclassified romantic is defined as pairs of individuals who are associated as co-parents in any of our government data without further relationship information available.

¹³A 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.

¹⁴Hispanic ethnicity is especially underreported in criminal justice administrative records (Eppler-Epstein, Gurvis, and King 2016; Ford 2015).

sus Numident and the 2000 and 2010 Decennial Censuses.

4 Research design and methodology

We exploit the discontinuous implementation of the DRF policy on October 1, 2003 to overcome potential endogeneity concerns. The statute only applied to individuals convicted of a DRF eligible offense on or after October 1, 2003. Therefore, individuals convicted of the same offense prior to October 1, 2003 would not be subject to the additional fine. 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.

For the formal regression discontinuity estimates, we follow the method proposed by Calonico, Cattaneo, and Titiunik (2014) and implemented in the Stata command *rdrobust* (Calonico et al. 2017). The point estimates $\hat{\tau}$ are estimated using the following framework:

$$\tau = \mu_+ - \mu_-,$$

where μ is the estimating equation of the outcome variable and

$$\mu_+ = \lim_{x \rightarrow d^+} \mu(x), \quad \mu_- = \lim_{x \rightarrow d^-} \mu(x), \quad \mu(x) = E[Y_i | X_i = x].$$

In this model, the average outcome function, $E[Y_i]$ is estimated on either side of the threshold where the DRF conviction date (X_i) is equal to the date of the policy change, $X = d$. The causal effect of the driver responsibility fees, τ , is thought of as the jump in the estimating equation moving from the left (d^-) to the right side (d^+) of the conviction date threshold. Instead of taking a simple average of the outcome variable, Calonico, Cattaneo, and Titiunik (2014) propose a parameterization of the estimating equation using first-order local polynomials on each side of the discontinuity.¹⁵

Throughout the analysis, we use the sharp RD design defined above with initial DRF conviction date as the running variable. We also include sample averages of the outcomes to contextualize estimate effect sizes.

¹⁵We use a uniform kernel which equally weights all observations within the bandwidth and a data-driven bandwidth selector that chooses two mean squared error optimal bandwidths, one for each side of the discontinuity. This corresponds to the *msetwo* bandwidth selector within the *rdrobust* command. We estimate first-order linear polynomials with second-order bias correction and implement heteroskedasticity-robust plug-in residuals variance estimators with HC_2 weights.

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 October 1, 2003 (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 cases 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-eligible 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 caseload density for all DRF-related offenses between April 2001 and March 2006. On average, there were over 14,000 DRF-related offenses per month, 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).¹⁶

Even if the caseload size remained unchanged, it might be possible that the caseload characteristics changes across the discontinuity. Panels B–D of Figure 1 plot three summary indices that together capture potential movements in caseload composition for our analysis sample.¹⁷ These include predicted cumulative W-2 earnings from 2005–2015, predicted cumulative number of convictions over 10 years, and the share of the caseload predicted to be in the high contamination sample (described below).¹⁸ None of these summary indices, which are generated from observable characteristics, indicate a statistically significant or economically meaningful change in caseload traits at the discontinuity, providing evidence in support of the identifying assumption of our re-

¹⁶We replicate the caseload exercise presented in Figure 1 panel A for our first observed DRF-eligible conviction analysis sample in Appendix Figure A.1. There is a natural downward slope in the caseload size, resulting from our sample inclusion criteria that only first observed offense in the 5 year window be retained for the analysis sample. However, there still is no imbalance across discontinuity in the caseload density.

¹⁷Due 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.

¹⁸To generate predicted values, we use a fully interacted regression model with the following variables: age at conviction for DRF-eligible offense, gender, race/ethnicity, average annual 1040 income, average 1040 form filing rates 1–3 years prior to conviction, fixed effects for the number of previous convictions, and the full interaction of fixed effects for the DRF offense level with fixed effects for the county of adjudication.

search design.

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 changes in the following: increase in the likelihood of being black, decrease in the likelihood of being white, and decrease in proportion convicted of a DRF level 1 offense. These estimates are relatively small when compared to the overall sample averages. All other estimates are statistically indistinguishable from zero. We also show that the estimated probability of linking to a romantic partner in the year of DRF conviction is unchanged at the discontinuity. This last fact is used to justify our empirical analysis of the effects of the fees on partner labor supply in Section 5.2.

To further test our identification assumptions, we calculate predicted income using cumulative income reported on annual IRS W-2 information returns from 2005 to 2015, predicted recidivism using total future convictions 10 years after DRF conviction, and likelihood of having higher than median predicted risk of DRF recidivism. 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. To generate both predicted variables, we use a fully interacted regression model with the following variables: age at conviction for DRF offense, gender, race/ethnicity, average annual 1040 income, average 1040 form filing rates 1–3 years prior to conviction, fixed effects for the number of previous convictions, and the full interaction of fixed effects for the DRF offense level with fixed effects for the county of adjudication.

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 where the coefficients are small, relative to the sample averages, and statistically insignificant.¹⁹ A visualization of these balance tests are shown in panels B–D of Figure 1. Each of the figures are smooth around the discontinuity, providing further evidence of the balance of the covariates.

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 qualitatively unchanged with or without the inclusion of control variables for observable characteristics, suggesting any potential biases introduced from sample imbalance is

¹⁹Due to limits on the number of results that can be disclosed by the Census Bureau, we do not include smoothed local polynomial figures for all covariates. As a compromise, we report the caseload density, predicted W-2, and predicted recidivism (which aggregate the individual covariates into two indices).

minimal.

4.2 First-stage relationship

We quantify the first-stage relationship in Figure 2 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-qualifying offense for individuals in our sample over time in panels A and B respectively. Unsurprisingly, we find a sharp jump of 100% in the likelihood of being assigned a DRF after the law goes into effect. Similarly, on the intensive margin, the average DRF amount increased from \$0 to over \$1,000 after the DRF implementation date.

For comparison purposes, the figures also include the likelihood of being assigned any non-DRF fines or fees in panel A and total non-DRF fines and fees in panel B.²⁰ Table 3 also shows the estimates from the regression discontinuity. We find that non-DRF fines and fees were infrequently assigned at less than 20% of DRF-eligible offense convictions and were small in monetary size when compared to DRFs, with an average of less than \$100. Thus, the DRFs would represent a significant change in likelihood of having any fines or fees and the monetary size of the non-DRF financial obligations the defendant would now be responsible for if convicted of a DRF-eligible offense.

4.3 Traffic offense recidivism and experimental contamination

One challenge to this research design is that individuals convicted before the discontinuity often go on to eventually be convicted of new DRF-eligible offenses after the effective date, thereby exposing the control group to DRF sanctions. If we fail to reject the null hypothesis that LFOs have no impact on future outcomes, it 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 identify drivers at high and low risk of ongoing DRF offenses in the years following their first conviction. Risk levels are predicted using a linear probability model based on the full interaction of: age at conviction for DRF offense, gender, race/ethnicity, average 1040 income, and filing rates 1–3 years prior to conviction. In addition, we include fixed effects for number of previous convictions and the full interaction of controls for the DRF offense level with fixed effects for the county of adjudication.

We evenly split our sample into “high” and “low” contamination groups based on the median predicted risk of DRF recidivism. The high contamination group is more likely to repeatedly commit DRF-eligible offenses, generating the DRF sanction spillovers in the control group that we

²⁰We do not measure non-sanction fines and fees (ex. court fees). Thus, the non-DRF fines and fees used in the figure represent a lower bound of all fines and fees assigned to the defendant.

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.

To illustrate this phenomenon, panels A and B in Figure 3 document the year-by-year evolution of the cumulative impact of the discontinuity on ever receiving a DRF sanction. The estimates from Year 0 are equivalent to the first-stage estimates from Figure 2, with each additional year expanding the follow-up period by a year.

The impact on ever receiving a DRF sanction erodes substantially over time in the high contamination group, with estimates falling almost 40 percent within two years of the original DRF-eligible offense. In contrast, the corresponding estimates for the low contamination group are relatively stable, with the impact of DRF sanction receipt falling only by 5 percent within four years of the initial conviction.

Individuals in the control group may be at risk to lose their driver's license because of their future DRF sanctions. As a result, our sharp RD estimates of the impact of DRF sanctions on labor market and recidivism outcomes may be biased towards zero since one of the two potential mechanisms may also be present in the control group, especially for the high contamination sample. Instead, our estimates may instead more strongly reflect the influence of the income effect channel, since we observe a stable discontinuity in total accumulated fines (see panels C and D of Figure 3).

We do not scale the sharp discontinuity estimates over time by the diminishing impact on ever receiving a DRF sanction because the financial sanctions may have persistent, lagged effects on economic behavior. Scaling the reduced form impacts by the estimates in panels A and B of Figure 3 would impose the assumption that our mechanisms operate immediately and contemporaneously, which we have no evidence to support. In order to take seriously the concern about our sharp RD estimates being biased towards zero, we offer two plausible solutions: (1) present short-run impacts for the high contamination group before control group spillovers become too substantial, and (2) present short and long-run impacts for the low contamination group whose control group does not have a high likelihood of another DRF-eligible offense.²¹

Since the analysis is stratified by contamination group, we consider how these groups differ along observable characteristics. Figure 4 plots the average demographic characteristics across 5 percentage point intervals of the distribution of the predicted DRF recidivism. We include individuals within 15 percentiles on either side of the central point. For example, at the 20th percentile, we include individuals from the 5th to 35th percentile. Statistics near the ends of the distribution will have fewer observations as they are bounded by 0 and 100. We also mark where the sample

²¹For completeness, we also present long-run impacts for the high contamination group. Although we believe these estimates may be at risk for being biased towards zero, given the underlying socio-economic differences between the high and low contamination samples we feel it is important to share this evidence given the potential for treatment effect heterogeneity.

splits between the low contamination group (0–50) and the high contamination group (50–100).

Taken together, these figures show that the low contamination group is on average older and is composed of more females and fewer Blacks relative to the high contamination group. We find that predicted income is lower in the high contamination group. We also find a sharp contrast in criminal histories between the low and high contamination groups. Specifically, individuals with any prior criminal convictions one to three years prior to DRF conviction are more likely to have an above median predicted likelihood of DRF recidivism and thereby be in the high contamination group.

To ensure that splitting the sample by predicted risk of DRF recidivism does not violate the identifying assumptions for a sharp RD, we graph the local polynomial estimates of the likelihood of being in the high contamination group on either side of the discontinuity in panel D of Figure 1. We also show the regression discontinuity estimate in Table 2. Both the figure and the estimates show smoothness across the discontinuity.

Table 3 shows the discontinuity estimates of the first stage on the extensive and intensive margin for the low and high contamination group and for the full sample respectively. Given the sample design, we observe a 100% likelihood of being assigned a DRF upon conviction after the cutoff of October 1st, 2003 across all of our samples of interest. On the intensive margin, we find that the average DRF assigned increases discontinuously at over \$1,000 for all groups with the largest increase for the low contamination sample.

We also include the likelihood of being assigned any non-DRF LFO. Similar to our first stage graphs using the full model, we find that the likelihood of being assigned a non-DRF LFO was rare with only 16% and 15% of cases assigned any LFO for the high and low contamination sample respectively. These LFOs were also small relative to the DRFs with the average financial obligation assigned for each case at less than \$90 for the high contamination group and \$121 for the low contamination group. While we find a small but significant increase of \$12 in the total non-DRF LFO's for the high contamination sample, the increase is quite modest relative to the magnitude of the DRF sanctions. We find no significant change in the total non-DRF LFO's assigned for individuals in the low contamination sample.

Throughout the analysis below of labor market and recidivism outcomes, all outcomes include controls for the driver's demographic characteristics (age, gender, race/ethnicity, total convictions 1–3 years before DRF conviction, income 1–3 years before DRF conviction as reported on 1040 tax filings, the likelihood of filing a 1040 tax return 1–3 years before DRF conviction), and fixed effects for the DRF level of offense.

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 2015.²² Future criminal convictions are measured using all DRF and non-DRF CJARS court filings up to 10 years following the original DRF-eligible conviction. The follow-up periods differ slightly across outcomes due to differences in measurement periodicity and the time frame of data availability.

Table 4 reports the estimated causal impact of DRFs on labor market outcomes. The first two columns show the short and long-run impacts for the high contamination sample; the third and fourth columns show corresponding estimates for the low contamination sample. The final column shows the pooled long-run impacts for all individuals. Complementary graphical plots are shown in Figure 5.^{23,24}

For individuals in the high contamination sample, we observe an increase of \$2,185 in cumulative W-2 earnings between 2005 and 2007. This earnings response exceeds the average amount of DRF assigned (\$1,148), suggesting a potential immediate reaction to the income shock sustained over time through path dependence. We observe no change, however, in the employment rate as measured by the likelihood of any W-2 return or the average number of W-2 returns filed, indicating that employed individuals increased their labor supply without taking on secondary or tertiary employment to pay off the fees.

For the low contamination group, we observe a modest decrease in the number of W-2 returns received per year (-0.03 returns relative to a sample-wide average of 1.18 returns). Because the data is collected on an annual basis, this reduction could reflect a contraction in secondary employment or a decreased likelihood of securing several short-term non-overlapping positions across multiple employers throughout the year. There is no statistically significant or economically meaningful

²²Data limitations unfortunately preclude measuring W-2 returns in 2004 or earlier.

²³Federal disclosure restrictions preclude generating visual graphs for all of our considered outcome variables.

²⁴The sample restriction allowing only the first observed DRF-eligible conviction to appear in the running variable alters the composition of the analysis sample across the 5 year support of the x-axis. This is most clearly observed in the recidivism patterns (panels C and D) where those who faces repeated charges over time are most likely to appear on the left-hand side of the graph, which could otherwise give the false impression of declining recidivism over time. For this reason, program impacts should only be evaluated in the neighborhood of the discontinuity.

impact on having any W-2's filed or on reported earnings, suggesting the reduction in total W-2 filings is driven by low-paying, non-primary jobs. It is possible that individuals consolidated their labor supply to one primary employer in response to potential travel disruptions arising from driver's license revocations, but there is no data to explicitly test this channel.

For both low and high contamination groups, measured impacts attenuate when we increase the follow-up window to 2005–2015. We find no impact of DRF conviction on either intensive or extensive labor market outcomes in the long-run, regardless of contamination subgroup. Overall, contrary to public concern, DRFs do not appear to create significant barriers to employment. We observe no evidence of an economically meaningful decrease in employment or earnings; instead, DRFs appear to stimulate a positive short-run earnings responses within a subset of the caseload.

In Table 5, we shift our focus to criminal justice outcomes, measured using future convictions overall and separated by crime type and offense level. We find that the DRFs had no meaningful impact either in levels or percent changes on nearly all measures of future contact with the justice system in both the high and low contamination sample. In particular, we observe no change in the likelihood of engaging in future DRF-eligible offenses, which means that there was neither a *general* or *specific* deterrent effect of the policy. While we find a statistically significant reduction in the short-run likelihood of any felony conviction (-0.004 total convictions relative to a control mean of 0.01) in the low contamination sample, this impact is not sustained in the long run.

While control group contamination motivated our split sample analysis strategy, pooling the observations together may help improve precision to measure small impacts in the overall population. In spite of likely attenuation bias, we find modest, positive effects in the full sample on labor market behavior and future convictions. We find increased 11-year cumulative household earnings of \$11,430 (3.8%) measured using income reported on 1040 tax filings and a slight decrease of -0.07 total convictions (3.2%). We do not observe a similar, significant increase in individual earnings reported using W-2 returns. Since income on 1040 tax filings is self-reported and filed by the household rather than the individual, the increased earnings may reflect changes in non-wage income (e.g. tips) or potential spillover effects onto household members, which we explore in Section 5.2.

To better understand these findings, we expand the initial analysis along several dimensions: (1) the timing of impacts over the follow-up period (Figure 6), and (2) heterogeneous treatment effects by DRF sanction level and socio-economic characteristics (Figure 7).

In Figure 6, we explore the cumulative year-by-year evolution of the impact of DRFs on W-2 earnings and total convictions. In general, these estimates largely confirm our prior analysis. The short-run impact on cumulative earnings in the high contamination sample continues to grow at a steady rate, peaking in 2010 at \$4,922 (6.3 percent). However, soon after the estimated impact contracts rapidly, perhaps indicating that the Great Recession broke the path dependence of the

stronger labor market attachment initially triggered by the DRF debt. Otherwise, we largely see null results across the outcomes and follow-up periods, consistent with our main analysis.

In Figure 7, we plot subgroup treatment effect estimates for long-run W-2 earnings and total convictions. The first set of coefficients examines heterogeneity by DRF offense level.²⁵ Michigan Public Act 165 introduced three tiers of DRF sanctions for criminal offenses, with financial penalties ranging from \$300 (Level 1) to \$2,000 (Level 3) depending on the exact nature of the traffic offense. The earnings gains and reduction in recidivism appears driven almost entirely by DRF Level 1. For this group, we observe an increase in long-run earnings of \$21,010 (10 percent) and a decrease in total convictions of 0.12 (5.7 percent). Level 2 observes similarly signed, although substantially more muted, point estimates. By Level 3, there is a sign reversal in the impact on earnings and essentially no effect on recidivism.

Two factors may explain this pattern of evidence. First, as DRF sanction amounts grow, individual ability to pay may shrink, increasing the likelihood of driver's license suspensions.²⁶ While larger fees motivate a stronger income effect (e.g. greater earnings), this ultimately may be undone by the license suspension channel. Second, the changing demographic composition of each DRF level may contribute to measuring treatment effect heterogeneity. Each level is largely dominated by a single offense type (Level 1 – 100% Driving with an expired license, Level 2 – 85% Driving with a suspended or revoked license, and Level 3 – 85% Driving while intoxicated), and exhibit observably distinct caseload traits.

The second half of Figure 7 conducts subgroup analysis by baseline socio-demographic traits (race, sex, criminal history) and predicted income levels to probe whether the DRF level treatment effect variation is driven by differences in caseload composition or fee affordability.²⁷ It appears there is evidence of both mechanisms. The largest gains in earnings are driven by individuals who are white, female, have no prior convictions, or have above median predicted income. The largest declines in recidivism are observed for individuals who are white, male, have a prior criminal conviction, and have below median predicted income.

Although we cannot statistically reject equality between the subgroup differences,²⁸ qualitatively the results provide support for both interpretations of the measured differences in treatment effects across DRF levels. For instance, the largest earnings gains are observed for those with above median predicted income, which is consistent with the idea that more affordable fines increase labor force participation while less affordable fines discourage work (perhaps through driver's license suspensions).²⁹ However, some of the starkest differences in outcomes, especially for recidivism,

²⁵See Table A1 for the estimates presented in tabular format.

²⁶In addition, (Dusek, Pardo, and Traxler 2020) finds that larger payment obligations in Prague significantly reduce timely compliance with legal financial obligations, independent of ability to pay.

²⁷See Table A2 for the estimates presented in tabular format.

²⁸The Black-White difference in total convictions is an exception to this statement.

²⁹We also separate analysis across the predicted income distribution for the high and low contamination group

are observed between socio-demographic traits like race or criminal history which might indicate that individuals respond differently to LFOs based on their background characteristics.

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, it is also important to determine whether DRFs 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. To measure 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-eligible offenses to their partners in the year of their initial DRF conviction.

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. Similar to our main analysis in Section 5, we separate our results by contamination group and with the full sample. Panel A of Table 6 shows 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—for individuals in either the low or high contamination group. For consistency, we also repeat the estimate for likelihood of being linked to a romantic partner for the full sample shown in Table 2.

Panel B of Table 6 shows a variety of outcomes of interest for romantic partners: the likelihood of remaining in a relationship, the length of the relations, cumulative W-2 earnings, and the total number of convictions. We show results over both the short and long-run, by contamination group, and for the full sample.³⁰

For the low contamination group, we find no evidence of spillover effects 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 quite small, which indicate precise null effects. These results suggest that earlier findings of null direct impacts of DRFs on labor market and recidivism outcomes among the low contamination group were not confounded by secondary impacts on partnership length, partner's labor supply, or partner's criminal charges.

For the high contamination group, we find a statistically significant increase in partner long-term cumulative earnings, but no change in the number of criminal convictions or relationship stability. That is, for individuals at a higher risk of DRF recidivism, their romantic partners in-

separately in Figure A.2. We do not observe any significant pattern across the predicted income distribution for either subgroup.

³⁰Relationship status is measured in 2007 and 2015. Labor market outcomes are measured from 2005–2007 and 2005–2015. Criminal justice outcomes are measured two and ten years after the initial DRF conviction).

creased labor supply as a result of the DRF conviction. Consequently, a portion of the net financial burden of DRFs was potentially borne by someone other than the original convicted defendant.

Pooling the high and low contamination sample together attenuates the results. Our full sample estimates yield no significant impacts on any of the long-term outcomes. We find statistically insignificant increase in income and future convictions and statistically insignificant decrease on likelihood of relationship survival and years together.

6 Conclusion

We examine the impact of financial sanctions in the state of Michigan, leveraging the abrupt introduction of sizeable fines associated with the Driver Responsibility Fees (DRF) program. Extensive data linkage allows us to track longitudinal labor market and recidivism outcomes as well as household spillovers. To the best of our knowledge, our research is the first to account for secondary impacts on romantic partners, examine the impacts of financial sanctions on such a large sample, and follow outcomes over such an extended time period.

Despite widespread criticism of the law, especially related to driver's license suspensions, we find relatively muted impacts of DRFs on individuals and their romantic partners over the long-run. We find that the fees had a small positive effect on short and medium-run W-2 income for individuals at high risk of committing another DRF offense. An initial increase in labor supply to address the DRF debt, continued by path dependence, is most consistent with the pattern of evidence. The effect attenuates, however, quickly after 2010 perhaps due to labor market disruptions resulting from the Great Recession. We also find long-term increased earnings for their romantic partners, which we interpret as suggestive evidence that these individuals may be shifting part of the financial burden onto their romantic partners. We find no meaningful impact of the DRFs on short or long-term recidivism outcomes for this subgroup. At the same time, we find null effects for criminal, labor market, and spillover outcomes, in the short and long-run, for individuals at low risk of DRF recidivism.

Our subgroup analysis suggests potential treatment effect heterogeneity. The largest gains in earnings and reductions in recidivism are observed for those facing the smallest fine levels. As the sanction amounts increase, which is driven by variation in offense type, estimates converge towards zero and even change signs in the case of long-run earnings. While DRF debt might stimulate labor supply, driver's license suspensions resulting from fee nonpayment likely work in the opposite direction. As a result, the improvements observed at low fine levels are eroded as fee amounts expand.

Our findings starkly contrast with prior descriptive work (Harris, Evans, and Beckett 2010; Pleggenkühle 2018) and causal research (Mello 2021; Kessler 2020). We believe four factors may

contribute to the divergence in causal findings: (1) differences in research design (RD versus event study); (2) data sources to measure labor market outcomes (W2 tax filings versus credit bureau employment data, which principally comes from large employers, and consumer bankruptcy records, which only cover individuals who have declared bankruptcy); (3) different study populations; and, (4) the size of the financial sanctions. Further research is warranted to better understand how each of these differences contribute to overall discrepancy in our findings.

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.³¹ 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. Our results show that DRF convictions increased the labor supply of romantic partners, suggesting that other household members may be shouldering the monetary burden of the DRF fines. Without clear evidence of general or specific deterrence arising from the DRF policy, it remains unlikely that the DRF regime was welfare improving, even if our causal estimates are less pessimistic than prior research has found.

³¹ Average per capita, annual personal income in 2005 in Michigan was just over \$30,000, unadjusted (U.S. Bureau of Economic Analysis and Federal Reserve Bank of St. Louis 2021). From Table 4, average annual income per capita in our sample is just over \$19,000 from 2005 to 2007 using W-2 information.

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7 Tables

Table 1: Michigan driver responsibility fee amounts and eligible offenses

DRF Level	Fee amounts	Eligible offenses	% of Offenses
Level 1	\$300–400	• No proof of insurance at the time of the stop	<1%
		• Driving with an expired/invalid license	100%
Level 2	\$1,000	• Driving on a suspended/revoked/denied license	85%
		• No insurance under the insurance code	2%
		• Operating with presence of drugs	<1%
		• Operating while impaired by liquor/controlled substance	9%
		• Reckless driving	4%
Level 3	\$2,000	• Vehicular manslaughter	<1%
		• Felony with an auto	<1%
		• Unlawful/felonious driving	4%
		• Failing to stop after accident causing injury	10%
		• Operating while intoxicated or under the influence	85%
		• Fleeing or eluding an officer	1%

Source: Authors' calculations using the Michigan criminal justice histories from the CJARS 2020Q1 vintage. Offense classification by DRF level is from the Michigan Department of State

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. Most frequently violated offenses within each level are highlighted in bold. Rounded distributions of violations within each level are shown in the last column. Violations composing of less than 1% of all violations within level are excluded. All results were approved for release by the Census Bureau, authorization numbers CBDRB-FY21-ERD002-023 and CBDRB-FY22-ERD002-001.

Table 2: Evaluating balance of selected observable characteristics and predicted earnings and criminal activity in the analysis sample on the effective date of Michigan Public Act 165 (October 1, 2003)

Variable	Control mean	RD estimate (standard error)	Variable	Sample mean	RD estimate (standard error)
Caseload size:					
Average daily DRF caseload	337.3	-16 (32)	Age at conviction	32.31	0.250 (0.170)
Summary Indices:					
Predicted cumulative 2005–2015 W-2 Income	210,400	914 (2,589)	Pre-conviction average total convictions	1.092	-0.004 (0.033)
DRF Offense Level:					
Predicted 10-year total convictions	2.271	-0.011 (0.038)	DRF level 1	0.295	-0.012** (0.006)
Predicted above-median risk for 2-year DRF recidivism	0.490	-0.007 (0.007)	DRF level 2	0.367	-0.006 (0.008)
Demographic Traits:					
Male	0.739	0.009 (0.008)	DRF level 3	0.338	0.004 (0.009)
Pre-conviction 1040 information:					
Hispanic	0.028	0.002 (0.003)	Pre-conviction average 1040 filing rate	0.621	-0.001 (0.007)
Black	0.238	0.022** (0.008)	Pre-conviction average 1040 household income	23,970	-8 (723)
Pre-conviction relationship status:					
White	0.694	-0.026** (0.009)	Matched to romantic partner prior to conviction	0.189	-0.006 (0.005)
Observations	423,000		Observations	423,000	

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 criminal justice histories from the CJARS 2020Q1 vintage. Individuals are linked to their romantic partners using the universe of 1040 filings and survey responses to the Decennial (2000) and American Community Survey (ACS) (2005–2018).

Note: This table presents the sharp RD estimates for select characteristics describing the individual at the time of conviction. Measurement of predicted indices is described in Section 4.3. Wages and income are adjusted to 2017 dollars using the CPI-All Urban. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. Standard errors are enclosed in parentheses. Control mean is calculated using cases in our sample disposed from April 1, 2003 to September 30, 2003. All results were approved for release by the Census Bureau, authorization numbers CBDRB-FY21-ERD002-023 and CBDRB-FY22-ERD002-001.

RD estimate notes: Coefficients are estimated using a first-order local polynomial regression discontinuity with a uniform kernel that has been bias corrected using a second order local polynomial with robust standard errors based on a heteroskedasticity-robust plug-in residuals variance with HC2 weights. Bandwidth was selected using two (below cutoff and above cutoff) mean-squared-error-optimal bandwidth selectors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Evaluating assignment of driver responsibility fee (DRF) and non-DRF fee or fine (LFO) in the analysis sample on the effective date of Michigan Public Act 165 (October 1, 2003)

<i>First Stage Outcomes</i> Outcome	Sample→	High contamination	Low contamination	Full sample
Driver Responsibility Fees:				
Extensive Margin		1.000*** (0.000) [0]	1.000*** (0.000) [0]	1.000*** (0.000) [0]
Intensive Margin		1,148*** (7.327) [0]	1,353*** (12.79) [0]	1,260*** (8.079) [0]
Non-DRF Fines, Fees, and Restitution:				
Extensive Margin		0.007 (0.008) [0.160]	-0.000 (0.007) [0.146]	0.002 (0.006) [0.153]
Intensive Margin		12.02* (6.915) [87.99]	5.065 (8.261) [117]	10.22* (5.655) [102.8]
Observations		211,500	211,500	423,000

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 criminal justice histories from the CJARS 2020Q1 vintage.

Note: This table presents the sharp RD estimates for the likelihood of DRF assignment, total DRFs assigned, likelihood of any non-DRF fine and fee assignment, and total non-DRF fine and fee assignment at the time of conviction. By sample design, discontinuity estimate of likelihood of having any DRF assigned is equal to one after the effective date of Michigan Public Act 165. Total DRFs and total non-DRF fines or fees are measured in unadjusted dollars. We do not measure non-sanction fines and fees (ex. court fees). Thus, the non-DRF fines and fees used in the the estimate represent a lower bound of all fines and fees assigned to the defendant. Estimates are generated for individuals in the low and high contamination group and the full sample separately. Low (high) contamination is defined as having below (above) median risk for predicted DRF recidivism two years after conviction. Prediction of two-year DRF recidivism is described in Section 4.3. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. Standard errors are enclosed in parentheses and control means are enclosed in brackets. All results were approved for release by the Census Bureau, authorization numbers CBDRB-FY21-ERD002-027 and CBDRB-FY22-ERD002-001.

General RD Table Notes: Coefficients are estimated using a first-order local polynomial regression discontinuity with a uniform kernel that has been bias corrected using a second order local polynomial with robust standard errors based on a heteroskedasticity-robust plug-in residuals variance with HC2 weights. Bandwidth was selected using two (below cutoff and above cutoff) mean-squared-error-optimal bandwidth selectors. Unless otherwise noted, all regressions include covariates for individual characteristics (age at conviction, gender, race and ethnicity, average income reported on 1040 tax filings 1–3 years pre-conviction, average 1040 filing rate 1–3 years pre-conviction, total number of prior convictions 1–3 years pre-conviction), and fixed effects for the level of DRF offense. Control means are measured using convictions from April 1st, 2003 – September 30th, 2003. * p<0.1, ** p<0.05, *** p<0.01.

Table 4: Impact of driver responsibility fees on short and long-run labor market outcomes

Outcome	Sample→	High contamination		Low contamination		Full sample
	Period→	2005–2007	2005–2015	2005–2007	2005–2015	2005–2015
Employment:						
Annual average rate of 1+ W-2 returns		0.005 (0.007) [0.672]	0.010 (0.006) [0.593]	-0.011 (0.009) [0.723]	-0.000 (0.007) [0.649]	0.004 (0.005) [0.621]
Annual average number of W-2 returns		-0.001 (0.021) [1.209]	0.013 (0.014) [0.966]	-0.033* (0.020) [1.177]	-0.015 (0.014) [0.975]	-0.001 (0.010) [0.971]
Earnings:						
Total W-2 earnings		2,185** (1,055) [41,330]	2,818 (3,732) [153,800]	134 (1,922) [73,650]	9,660 (6,561) [260,000]	5,171 (3,959) [207,900]
Total 1040 household earnings		1,291 (1,422) [46,870]	4,699 (5,494) [192,600]	1,217 (3,511) [104,500]	12,980 (11,900) [396,800]	11,430* (6,522) [296,800]
Observations		211,500		211,500		423,000

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 criminal justice histories from the CJARS 2020Q1 vintage.

Note: This table presents the sharp RD estimates for the effects of DRF conviction on labor outcomes across varying time periods noted in the column names. Wages and income are CPI adjusted to 2017 dollars using the CPI-All Urban. Estimates are generated for individuals in the low and high contamination group and the full sample separately. Low (high) contamination is defined as having below (above) median risk for predicted DRF recidivism two years after conviction. Prediction of two-year DRF recidivism is described in Section 4.3. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. Standard errors are enclosed in parentheses and control means are enclosed in brackets. All results were approved for release by the Census Bureau, authorization numbers CBDRB-FY21-ERD002-023 and CBDRB-FY22-ERD002-001.

General RD Table Notes from Table 3 apply. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Impact of driver responsibility fees on short and long-run criminal outcomes

Outcome	Sample→ Period→	High contamination		Low contamination		Full sample
		2 years	10 years	2 years	10 years	10 years
Extensive Margin:						
Any conviction		0.014 (0.010) [0.564]	0.008 (0.007) [0.858]	-0.003 (0.005) [0.065]	-0.011 (0.009) [0.293]	-0.003 (0.006) [0.569]
Any felony conviction		-0.008 (0.006) [0.133]	-0.004 (0.008) [0.319]	-0.004* (0.002) [0.011]	-0.002 (0.004) [0.062]	-0.002 (0.005) [0.188]
Intensive Margin:						
Total convictions		0.040 (0.029) [1.238]	-0.070 (0.069) [3.895]	-0.007 (0.008) [0.095]	-0.031 (0.031) [0.659]	-0.071* (0.038) [2.244]
Drug convictions		-0.006 (0.011) [0.134]	-0.039 (0.024) [0.454]	-0.002 (0.003) [0.009]	0.002 (0.007) [0.063]	-0.020 (0.013) [0.255]
Property convictions		0.006 (0.011) [0.164]	-0.012 (0.026) [0.531]	-0.000 (0.002) [0.009]	-0.002 (0.009) [0.063]	-0.009 (0.014) [0.297]
Violent convictions		0.002 (0.008) [0.099]	-0.016 (0.014) [0.325]	-0.002 (0.002) [0.007]	-0.007 (0.006) [0.053]	-0.010 (0.008) [0.186]
DRF-eligible convictions		0.019 (0.016) [0.463]	-0.007 (0.035) [1.395]	-0.000 (0.000) [0.000]	-0.031 (0.014) [0.249]	-0.020 (0.020) [0.810]
Observations		211,500		211,500		423,000

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 criminal justice histories from the CJARS 2020Q1 vintage. Estimates on DRF-eligible convictions are pending disclosure.

Note: This table presents the sharp RD estimates for the effects of DRF conviction on criminal outcomes across varying time periods noted in the column names. Estimates are generated for individuals in the low and high contamination group and the full sample separately. Low (high) contamination is defined as having below (above) median risk for predicted DRF recidivism two years after conviction. Prediction of two-year DRF recidivism is described in Section 4.3. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. Standard errors are enclosed in parentheses and control means are enclosed in brackets. All results were approved for release by the Census Bureau, authorization numbers CBDRB-FY21-ERD002-023 and CBDRB-FY22-ERD002-001.

General RD Table Notes from Table 3 apply. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Impact of driver responsibility fees on household composition and romantic partner’s future outcomes

<i>Panel A: Balance tests</i>						
Outcome	Sample→	High contamination	Low contamination	Full Sample		
Matched to romantic partner prior to conviction		-0.005 (0.007) [0.168]	-0.008 (0.008) [0.210]	-0.006 (0.005) [0.189]		
Observations		211,500	211,500	423,000		
<i>Panel B: Relationship and partner outcomes</i>						
Outcome	Sample→ Period→	High contamination Short-Run Long-Run	Low contamination Short-Run Long-Run	Full Sample Long-Run		
Relationship Survival and Duration:						
Survival rate		-0.020 (0.026) [0.622]	-0.002 (0.024) [0.315]	0.005 (0.022) [0.731]	-0.013 (0.023) [0.465]	-0.024 (0.018) [0.399]
Years together		-0.103 (0.068) [2.031]	-0.153 (0.204) [5.137]	0.009 (0.061) [2.313]	-0.072 (0.205) [6.365]	-0.112 (0.159) [5.831]
Partner labor market and criminal outcomes:						
Cumulative W-2 earnings		4,040 (3,601) [56,490]	18,670* (11,310) [194,500]	-1,400 (4,489) [99,270]	-3,214 (17,680) [352,100]	1,015 (11,600) [283,600]
Total convictions		0.007 (0.036) [0.277]	0.037 (0.112) [1.035]	-0.004 (0.025) [0.137]	0.022 (0.068) [0.550]	0.032 (0.061) [0.761]
Observations		36,000		44,500		80,500

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 criminal justice histories from the CJARS 2020Q1 vintage. Individuals are linked to their romantic partners using the universe of IRS 1040 returns and survey responses to the Decennial (2000) and American Community Survey (ACS) (2005–2018). Full sample results are pending disclosure.

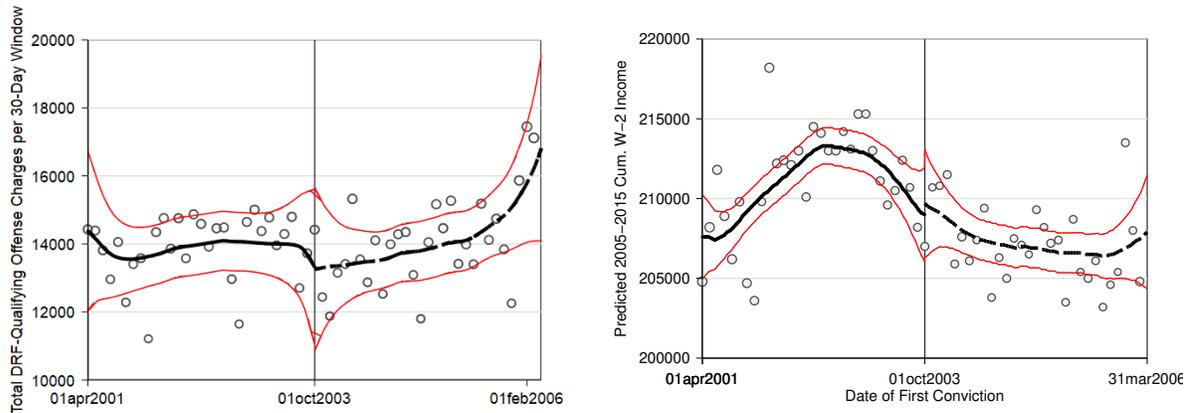
Note: This table presents the sharp RD estimates for the effects of DRF conviction on partnership and partner outcomes. Panel A is a balance test for the likelihood an individual is in a relationship in the year of conviction. Panel B presents results on partnership outcomes, cumulative W-2 earnings (adjusted to 2017 dollars using the CPI-All Urban), and total convictions for the partners of individuals conditional on observing a relationship during the year of conviction. Estimates are generated separately for individuals in the low and high contamination group. Low (high) contamination is defined as having below (above) median risk for predicted DRF recidivism two years after conviction. Prediction of two-year DRF recidivism is described in Section 4.3. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. Standard errors are enclosed in parentheses and control means are enclosed in brackets. All results were approved for release by the Census Bureau, authorization numbers CBDRB-FY21-ERD002-023 and CBDRB-FY22-ERD002-001.

General RD Table Notes from Table 3 apply. * p<0.1, ** p<0.05, *** p<0.01.

8 Figures

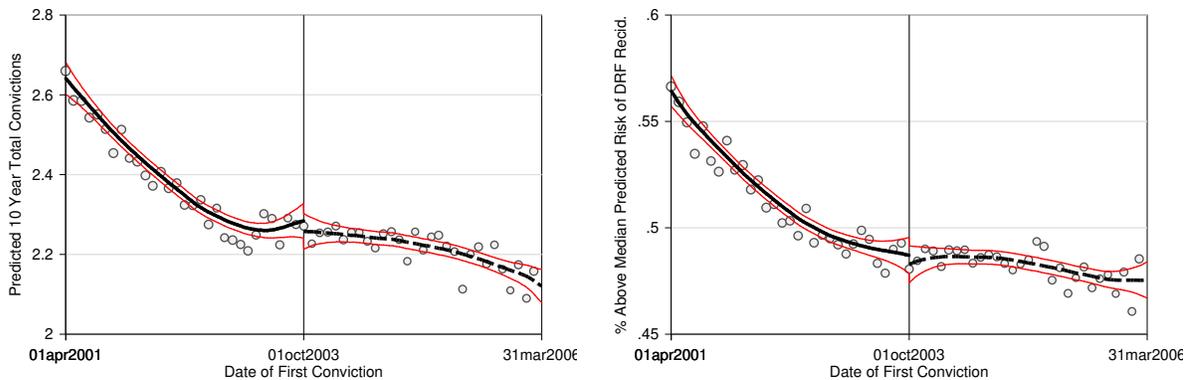
Figure 1: Summary characteristics and caseload density of analysis sample, by conviction date relative to effective date of Michigan Public Law 165 (October 1, 2003)

Panel A: Total driver responsibility fee-related offenses *Panel B: Predicted cumulative 2005–2015 W-2 income*



Panel C: Predicted total convictions 10 years after DRF conviction

Panel D: Share of caseload with high contamination likelihood



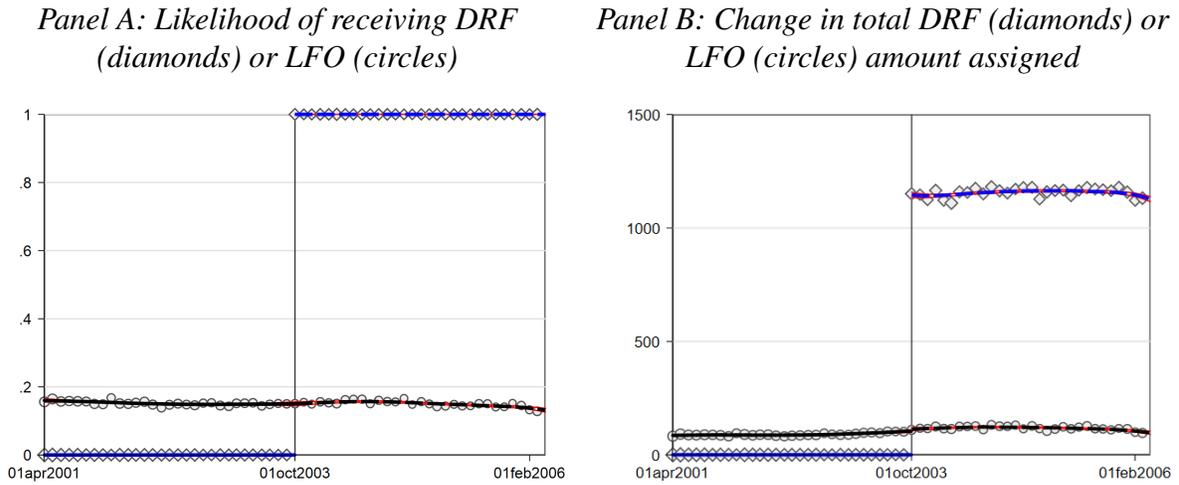
Panel A Source: Authors' calculations using the Michigan criminal justice histories from the CJARS 2020Q1 vintage. Estimates from this subfigure are generated using total DRF offenses with disposition date as the running variable within the focal time period.

Panels B–D 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 criminal justice histories from the CJARS 2020Q1 vintage.

Notes: Panel A shows the smoothed non-parametric estimates and 95% confidence intervals of the total caseload density estimated on either side of the discontinuity. The estimates are based off of a sample of all individuals charged with a driver responsibility fee qualifying offense from April 1, 2001 to March 31, 2006 in Michigan. Caseload density is measured using the total number of DRF convictions per 30 day window. High contamination is defined as having above median risk for predicted DRF recidivism two years after conviction. Measurement of predicted indices in panels B–D is described in Section 4.3. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the Census Bureau, authorization number CBDRB-FY21-ERD002-023.

General RD figure notes: Figures show smoothed, nonparametric estimates and 95% confidence intervals of the relevant outcome variable, estimated on either side of the discontinuity. In addition, the monthly average of the outcome variable is plotted as a scatter, where the size of each point is weighted by the monthly case count. The estimates are generated using a non-parametric local polynomial with a 120-day bandwidth and weighted with an Epanechnikov kernel estimated separately across both sides of the discontinuity.

Figure 2: Assignment of driver responsibility fee (DRF) and non-DRF fine or fee (LFO) in analysis sample relative to effective date of Michigan Public Law 165 (October 1, 2003)



Source: Authors' calculations using the Michigan criminal justice histories from the CJARS 2020Q1 vintage.

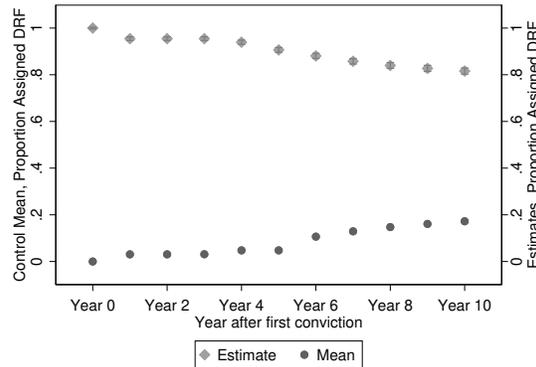
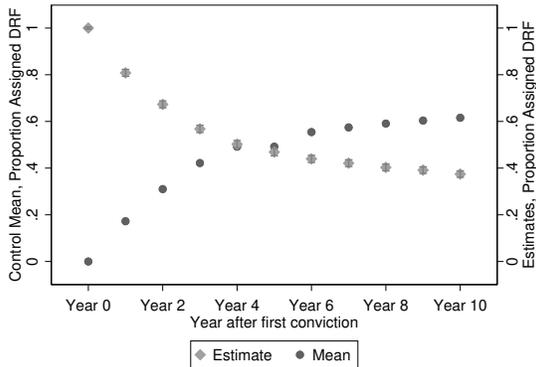
Note: This figure shows the smoothed non-parametric estimates and 95% confidence intervals of the first stage estimated on either side of the discontinuity. For Panel A, the outcomes are likelihood of being assigned a DRF (diamonds, blue) and likelihood of any non-DRF fine or fee assignment (circles, black). For Panel B, the outcomes are total DRF assigned (diamonds, blue) and total non-DRF fines and fees assigned (circles, black); both measures are in dollars, unadjusted. Measurement of fines and fees exclude non-sanction fees (ex. court fees). The estimates are based off of a sample of all individuals convicted of a driver responsibility fee qualifying offense from April 1st, 2001 to March 31st, 2006 in Michigan. The sample contains only the first DRF-qualifying offense of the individuals within the relevant time period.

General RD Figure Notes from Figure 1 apply.

Figure 3: Evolution of first stage relationship over time, accounting for recidivism that would trigger a driver responsibility fee, by predicted high and low contamination likelihood

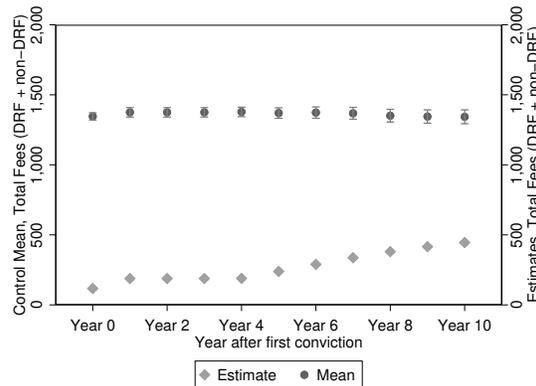
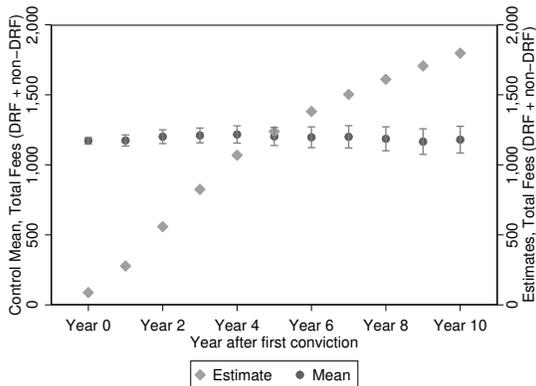
Panel A: Cumulative likelihood of DRF conviction, high contamination sample

Panel B: Cumulative likelihood of DRF conviction, low contamination sample



Panel C: Accumulated DRFs (\$), high contamination sample

Panel D: Accumulated DRFs (\$), low contamination sample

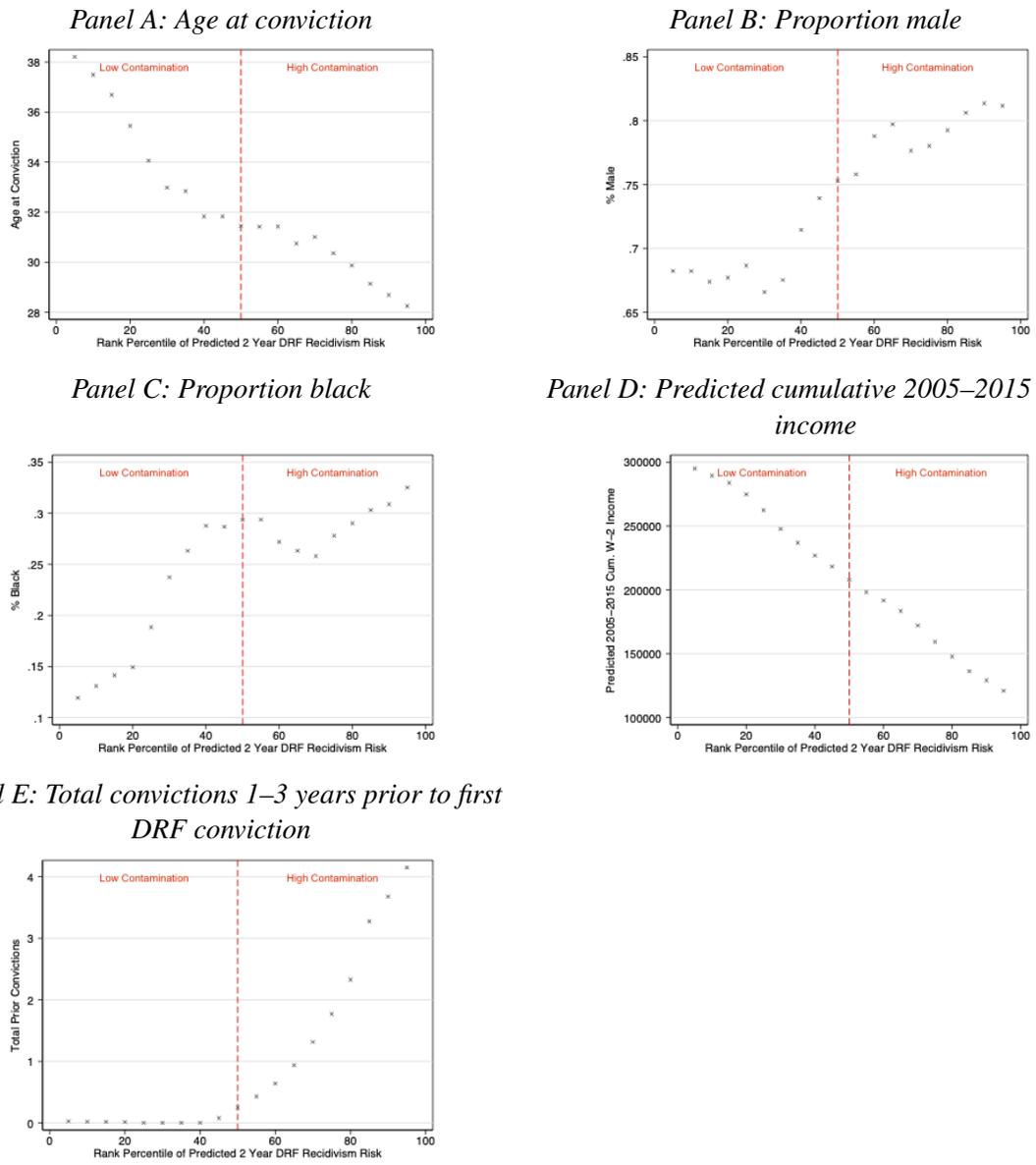


Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, and Michigan criminal justice histories from the CJARS 2020Q1 vintage.

Note: These figures plot regression discontinuity estimates measuring likelihood of being assigned a DRF (subgraphs (a) and (b)) and accumulated DRFs (subgraphs (c) and (d)) over a cumulative time period starting from the first conviction and the ten years after. The left graphs are for the subsample of individuals in the high contamination group and the right is for the low contamination group. Low (high) contamination is defined as having below (above) median risk for predicted DRF recidivism 2 years after conviction. Prediction of two-year DRF recidivism is described in Section 4.3. The control means are included for each outcome variable (dark grey circles). All estimates are shown with 95% confidence intervals. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the Census Bureau, authorization numbers CBDRB-FY21-ERD002-023, CBDRB-FY21-ERD002-027, and CBDRB-FY22-ERD002-001.

General RD Table Notes from Table 3 apply.

Figure 4: Means of selected characteristics across the distribution of predicted recidivism for driver responsibility fee (DRF) related offenses

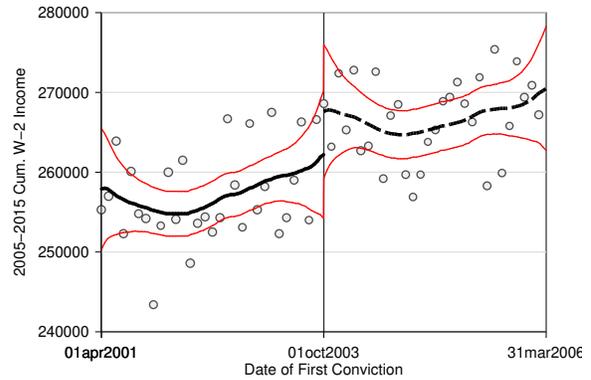
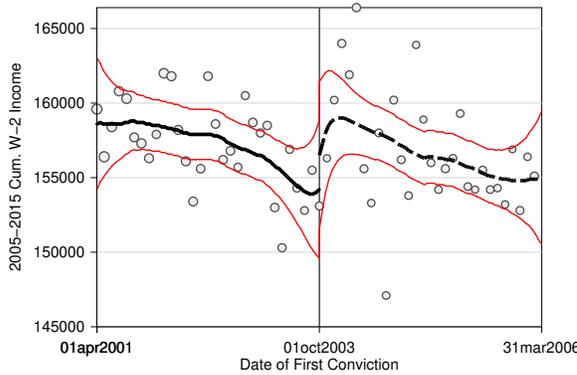


Source: Authors’ calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, and Michigan criminal justice histories from the CJARS 2020Q1 vintage.

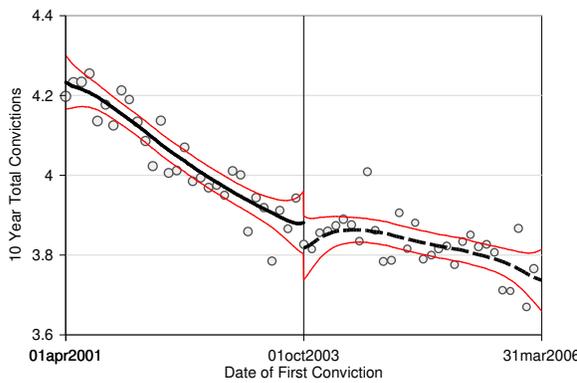
Note: This figure shows the average of select individual characteristics at different points on the distribution of predicted DRF recidivism two years after original conviction date. Prediction of two-year DRF recidivism and 2005–2015 cumulative W-2 income is described in Section 4.3. Pre-conviction criminal history is measured using total convictions 1–3 years prior to DRF conviction. Wages and income are adjusted to 2017 dollars using the CPI-All Urban. The means are calculated at every 5th percentile from the 5th to 95th percentile. To reduce noise, we include individuals in the 15 percentiles above and below the central point. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the Census Bureau, authorization number CDBDRB-FY21-ERD002-023.

Figure 5: 10-year earnings and recidivism outcomes in analysis sample relative to effective date of Michigan Public Law 165 (October 1, 2003), by predicted contamination group

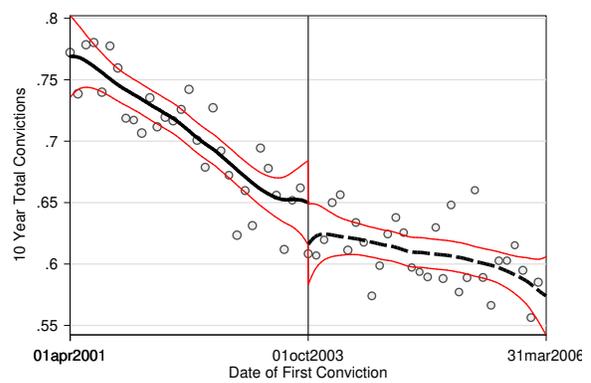
Panel A: Cumulative 2005–2015 W-2 income, high contamination, *Panel B: Cumulative 2005–2015 W-2 income, low contamination*



Panel C: 10-year total convictions, high contamination



Panel D: 10-year total convictions, low contamination



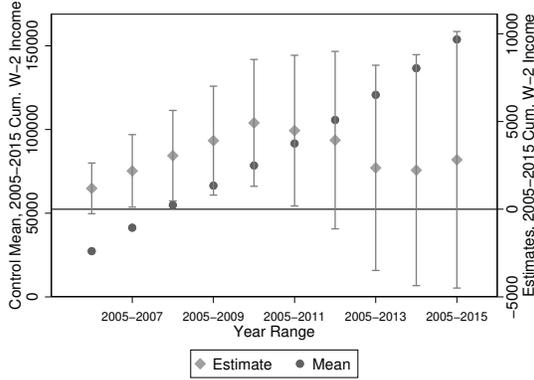
Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, and Michigan criminal justice histories from the CJARS 2020Q1 vintage.

Note: This figure shows smoothed nonparametric estimates and 95% confidence intervals of the effect of DRF conviction on total recidivism 10 years after (subgraphs (c) and (d)) and 2005–2015 cumulative W-2 income (adjusted to 2017 dollars using the CPI-All Urban) (subgraphs (a) and (b)), estimated on either side of the discontinuity for the low and high contamination groups separately. Low (high) contamination is defined as having below (above) median risk for predicted DRF recidivism 2 years after conviction. Prediction of two year DRF recidivism is described in Section 4.3. For all subfigures, the outcome variable is residualized to reduce disclosure risk and protect individual privacy using age at conviction for DRF offense fully interacted with gender and race/ethnicity, controls for average 1040 income income 1–3 years prior to conviction, average 1040 filing rate 1–3 years prior to conviction, the DRF offense level, and the number of previous convictions. The mean of the outcome variable is added back to the residuals. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the Census Bureau, authorization number CBDRB-FY21-ERD002-023.

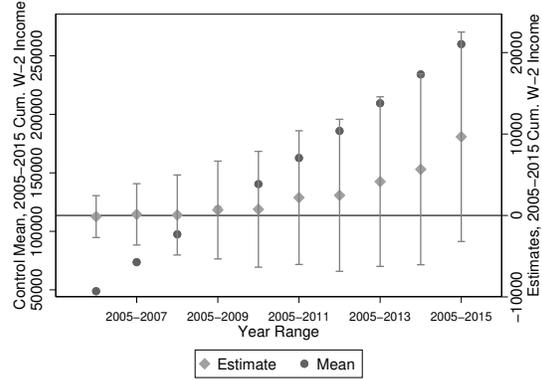
General RD Figure Notes from Figure 1 apply.

Figure 6: Evolution of RD-based causal estimates over the 10 year follow-up period, by contamination group

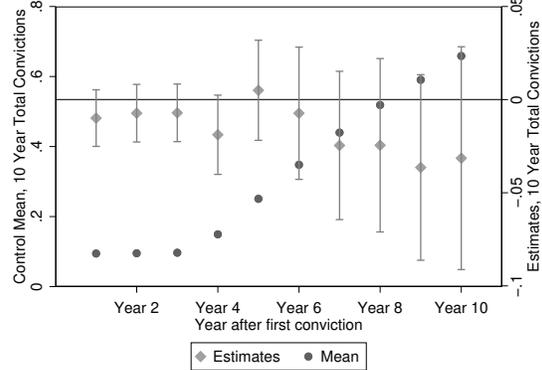
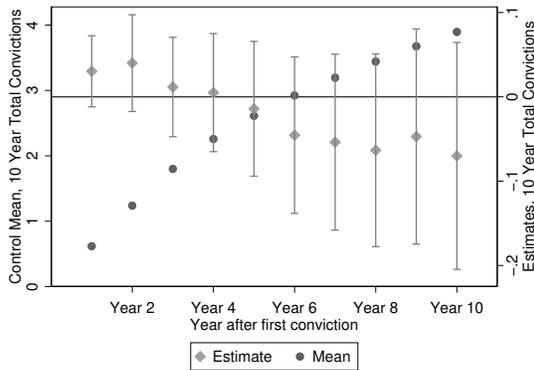
Panel A: Cumulative W-2 income, high contamination



Panel B: Cumulative W-2 income, low contamination



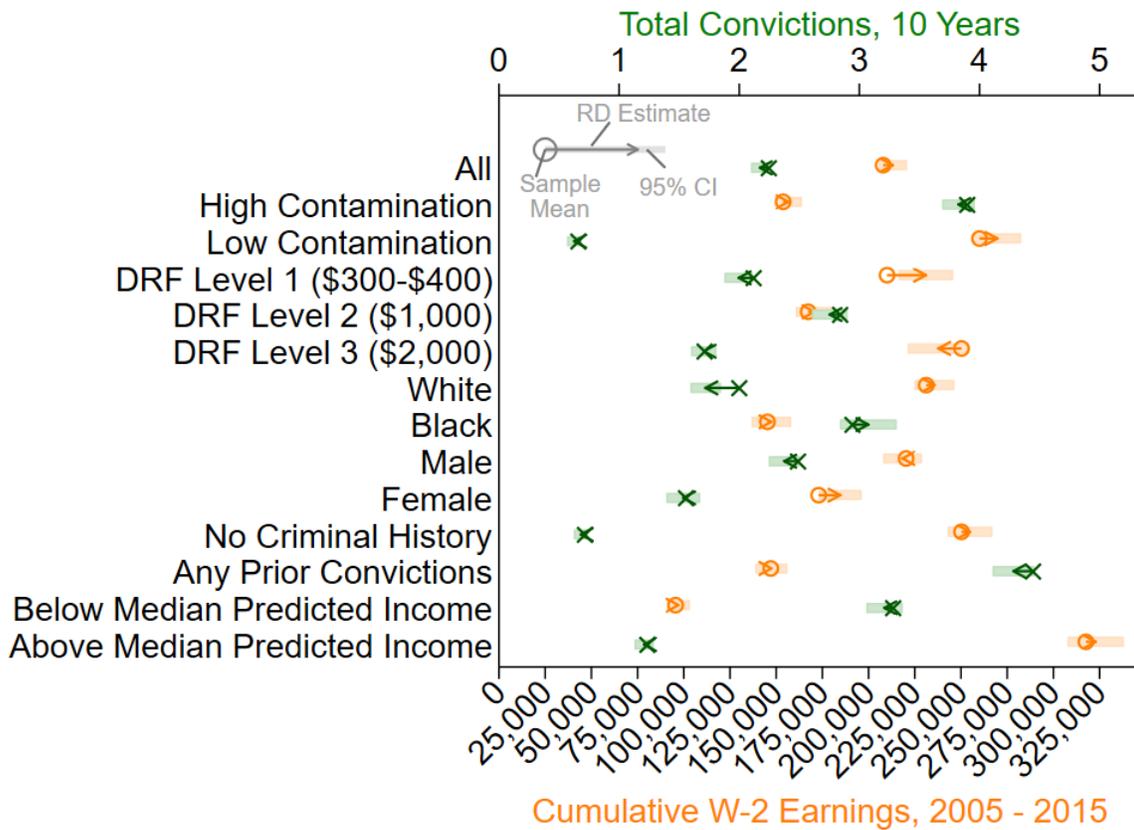
Panel C: Total convictions, high contamination Panel D: Total convictions, low contamination



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 criminal justice histories from the CJARS 2020Q1 vintage.

Note: This figure plots regression discontinuity estimates measuring the effects of DRFs on labor and recidivism outcomes over a cumulative time period that varies by graph. For the labor market outcomes (Adjusted to 2017 dollars using the CPI-All Urban) (subgraphs (a) and (c)), the time frame covered is from 2005–2006 to 2005–2015. For the recidivism outcomes (subgraphs (b) and (d)) the time frame is between 1 and 10 years following conviction of first DRF offense. The full sample control means are also included for each outcome variable (dark grey circles). All estimates are generated for individuals in the low and high contamination group separately. Low (high) contamination is defined as having below (above) median risk for predicted DRF recidivism two years after conviction. Prediction of two-year DRF recidivism is described in Section 4.3. All estimates are shown with 95% confidence intervals. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the Census Bureau, authorization numbers CBDRB-FY21-ERD002-023 and CBDRB-FY22-ERD002-001. *General RD Table Notes* from Table 3 apply.

Figure 7: Causal impact of driver responsibility fees on 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 criminal justice histories from the CJARS 2020Q1 vintage. Estimates for DRF Level 1, DRF Level 2, DRF Level 3, Male, and Female are missing and are pending disclosure.

Note: This figure presents the sharp RD estimates for the effects of DRF conviction on cumulative income (Adjusted to 2017 dollars using the CPI-All Urban, orange) measured using W-2 tax returns and total convictions (green) across various subgroups noted in the Y-axis. Estimates are plotted at the control mean of the outcome variable with the length and direction of the arrow indicating estimate size and direction; 95% confidence intervals are included in a lighter shade plotted behind the estimate. Racial identity is measured using the Census' 'betrace' 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. Low (high) contamination is defined as having below (above) median risk for predicted DRF recidivism 2 years after conviction. Measurement of predicted indices (2005–2015 cumulative W-2 income (Adjusted to 2017 dollars using the CPI-All Urban) and two-year DRF recidivism) is described in Section 4.3. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the Census Bureau, authorization numbers CBDRB-FY21-ERD002-023, CBDRB-FY21-ERD002-027, CBDRB-FY22-ERD002-001.

General RD Table Notes from Table 3 apply. Regressions for all individuals, by high and low contamination include covariates for individual characteristics (age at conviction, gender, race and ethnicity, average income reported on 1040 tax filings 1–3 years pre-conviction, average 1040 filing rate 1–3 years pre-conviction, total number of prior convictions 1–3 years pre-conviction), and fixed effects for the level of DRF offense. Regressions by DRF level of offense include the same set of covariates but excluding fixed effects for the level of DRF offense. All other subgroup regressions include controls for average income reported on 1040 tax filings 1–3 years pre-conviction, average 1040 filing rate 1–3 years pre-conviction, and fixed effects for the level of DRF offense.

A Appendix

A.1 Tables

Table A1: Impact of driver responsibility fees on future earnings and convictions, by fee amount

DRF Level	Fine Amount/ Predominant Offense Type	Outcome→ Period→	Cumulative W-2 earnings 2005–2015	Total Convictions 10 years
Level 1	\$300–\$400 Driving with an expired license, 100%		21,010*** (7,510) [210,000]	-0.1208** (0.06041) [2.117]
Level 2	\$1,000 Driving with a suspended or revoked license, 85%		3,396 (5,166) [167,300]	-0.08425 (0.07522) [2.838]
Level 3	\$2,000 Driving while intoxicated, 85%		-12,530 (8,450) [250,300]	-0.004545 (0.05328) [1.709]

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 criminal justice histories from the CJARS 2020Q1 vintage. All estimates for this table are pending disclosure.

Note: This table presents the sharp RD estimates for the effects of DRF conviction on cumulative income (Adjusted to 2017 dollars using the CPI-All Urban) measured using W-2 tax returns and total convictions by the fee amount across varying time periods noted in the columns. Dominant offense within each fee level and the share of the DRF level that is that offense type are included under the fee level. Estimates are generated for individuals in the low and high contamination group. Low (high) contamination is defined as having below (above) median risk for predicted DRF recidivism two years after conviction. Prediction of two-year DRF recidivism is described in Section 4.3. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. Standard errors are enclosed in parentheses and control means are enclosed in brackets. All results were approved for release by the Census Bureau, authorization number CBDRB-FY22-ERD002-001.

General RD Table Notes from Table 3 apply. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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

Sample→	Black	White	Male	Female	No Prior Convictions	Any Prior Convictions	Below Median Predicted Income	Above Median Predicted Income
<i>Panel A: Cumulative W-2 earnings 2005–2015</i>								
	2,063	4,495	-1,922	11,690***	4,522	253	1,466	5,275
	(5,476)	(5,474)	(5,330)	(5,893)	(6,159)	(4,462)	(3,177)	(7,734)
	[145,300]	[231,200]	[220,300]	[173,000]	[250,300]	[147,100]	[95,590]	[317,600]
<i>Panel B: Total Convictions, 10 years</i>								
	0.134	-0.278***	-0.112*	-0.021	-0.030	-0.160*	-0.070	-0.025
	(0.120)	(0.065)	(0.065)	(0.072)	(0.030)	(0.089)	(0.077)	(0.041)
	[2.940]	[1.998]	[2.487]	[1.555]	[0.713]	[4.443]	[3.279]	[1.233]
Observations	104,000	290,000	313,000	110,000	245,000	178,000	211,000	211,000

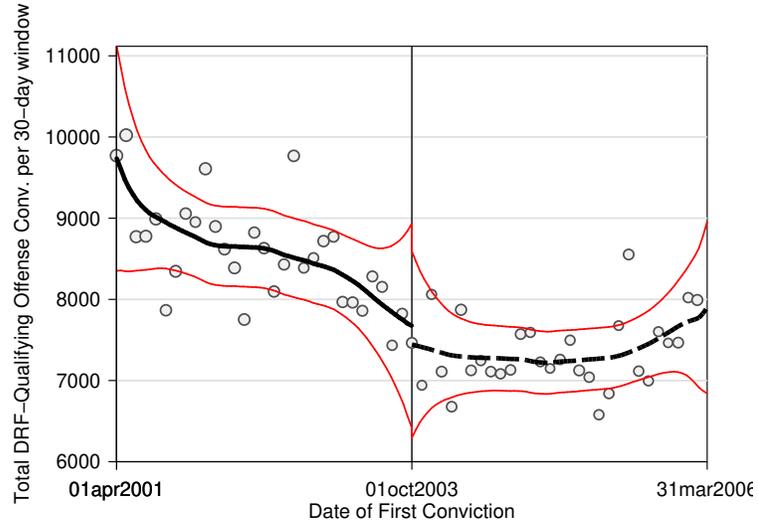
Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, and Michigan criminal justice histories from the CJARS 2020Q1 vintage. Male and female estimates are pending disclosure.

Note: This table presents the sharp RD estimates for the effects of DRF conviction on cumulative income (Adjusted to 2017 dollars using the CPI-All Urban) measured using W-2 tax returns and total convictions by the fee amount across various subgroups noted in the columns. Estimates are generated for individuals in the low and high contamination group. Racial identity is measuring using the Census' 'betrace' file. Any prior convictions is defined as having at least one conviction 1–3 years prior to initial DRF-conviction within the sample time period. Low (high) contamination is defined as having below (above) median risk for predicted DRF recidivism 2 years after conviction. Measurement of predicted indicies (2005–2015 cumulative W-2 income (Adjusted to 2017 dollars using the CPI-All Urban) and two-year DRF recidivism) is described in Section 4.3. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. Standard errors are enclosed in parentheses and control means are enclosed in brackets. All results were approved for release by the Census Bureau, authorization numbers CBDRB-FY21-ERD002-027 and CBDRB-FY22-ERD002-001.

RD estimate notes: We present estimates using a local-polynomial regression discontinuity. All regressions in this table include covariates for individual characteristics (age at conviction, average income reported on 1040 tax filings 1–3 years pre-conviction, average 1040 filing rate 1–3 years pre-conviction, and fixed effects for the level of DRF offense. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.2 Figures

Figure A.1: Total driver responsibility fee-related offenses in focal sample, by disposition date relative to effective date of Michigan Public Law 165 (October 1, 2003)



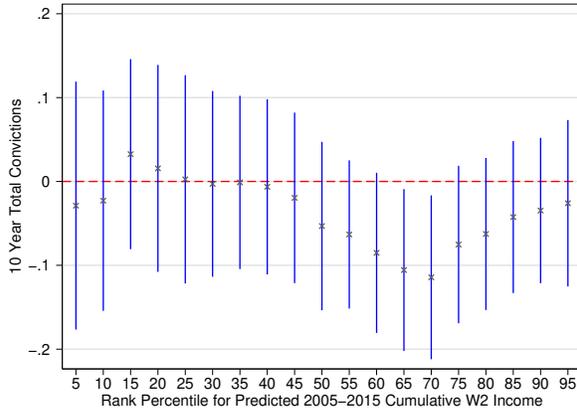
Source: Authors' calculations from 1998–2015 IRS 1040 individual tax returns, 2005–2015 IRS W-2 information returns, the 2020 Census Numident (to measure year of birth, state of birth, and gender), the 2020 Census Bureau Title 13 race/ethnicity file, and Michigan criminal justice histories from the CJARS 2020Q1 vintage.

Note: This figure shows the smoothed non-parametric estimates and 95% confidence intervals of the total caseload density estimated on either side of the discontinuity for individuals in our focal sample. This sample is restricted to an individual's first conviction of a DRF-eligible offense from April 1, 2001 to March 31, 2006. Given this restriction, the downward trend of the non-parametric estimates is from the decreasing incidences of first conviction of a DRF-eligible offense over the focal time period. All results were approved for release by the Census Bureau, authorization number CBDRB-FY21-ERD002-023.

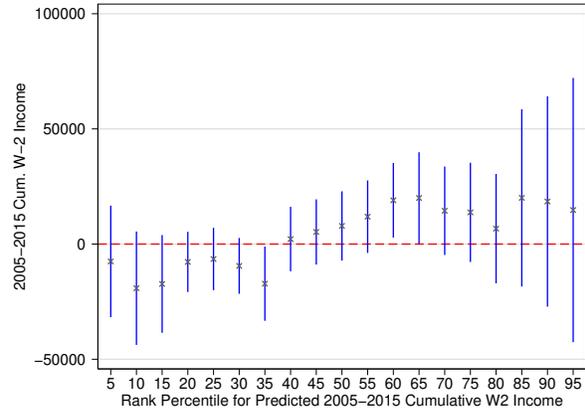
General RD Figure Notes from Figure 1 apply.

Figure A.2: Heterogeneous treatment effects of driver responsibility fees on labor market outcomes and criminal behavior, by percentile of predicted income and contamination group

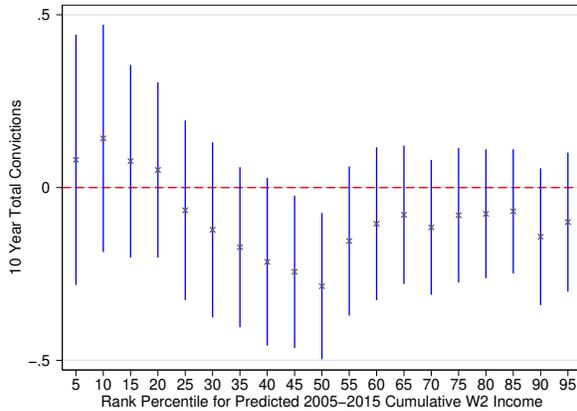
Panel A: 10-year total convictions, low contamination



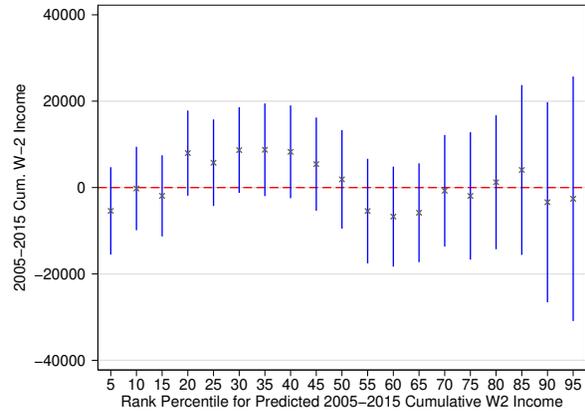
Panel B: Cumulative 2005–2015 W-2 income, low contamination



Panel C: 10-year total convictions, high contamination



Panel D: Cumulative 2005–2015 W-2 income, high contamination



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 criminal justice histories from the CJARS 2020Q1 vintage.

Notes: This figure presents the sharp RD estimates for the effect of the DRFs on labor and criminal outcomes at different points in the percentile rank of predicted 2005-2015 cumulative W-2 income distribution for individuals in the low and high contamination group separately. Low (high) contamination is defined as having below (above) median risk for predicted DRF recidivism 2 years after conviction. Measurement of predicted indices (2005–2015 cumulative W-2 income (Adjusted to 2017 dollars using the CPI-All Urban) and two-year DRF recidivism) is described in Section 4.3. Estimates are generated separately at every 5 percentiles from the 5th through 95th percentiles. To improve stability, we include individuals in the 15 percentiles above and below the central point. We also include robust 95% confidence intervals of the estimate. Outcome variable for the left panels (subgraphs (a) and (c)) is 10 year total recidivism; outcome variable for the right panels (subgraphs (b) and (d)) is 2005-2015 cumulative W-2 income. The low and high contamination groups are presented separately in the top and bottom panel respectively. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the Census Bureau, authorization number CBDRB-FY21-ERD002-023.

General RD Table Notes from Table 3 apply.