The Socioeconomic Consequences of Misdemeanor Conviction*

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Abstract

Misdemeanors account for three-quarters of criminal cases filed annually in the United States, yet causal evidence on the impacts of misdemeanor conviction is scarce. We combine administrative and survey data to provide novel evidence of how misdemeanor conviction affects the socioeconomic outcomes of defendants and their families. Using data on nearly seven million defendants from 11 states, we characterize the types of people involved with the misdemeanor system. We estimate causal impacts of a conviction relative to a charge using seven county reforms that sharply reduced conviction rates. Regression discontinuity design estimates yield precise null average effects of the reforms on criminal and labor market outcomes. Defendants dismissed by the reform are unlikely to re-offend if dismissed, but also unlikely to be harmed by conviction—marginal defendants are older and more likely to have been charged with minor crimes than the average defendant. Heterogeneity analyses suggest that conviction harms young and first-time defendants, implying that interventions targeting dismissal toward these groups would be beneficial. Finally, our causal estimates imply a negative Marginal Value of Public Funds of misdemeanor conviction, even under plausible estimates of general deterrence effects.

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1 Introduction

Misdemeanor offenses (e.g., shoplifting, drug possession, DUI) represent 75% of the annual number of criminal cases filed in the United States (Stevenson and Mayson 2018). Despite their prevalence, there is little causal evidence on the impact of misdemeanor conviction on the labor market outcomes and well-being of defendants and their families. The net socioeconomic effects are theoretically ambiguous. On one hand, sanctions associated with conviction might deter future criminal offending (Becker 1968). On the other hand, employer stigma against applicants with even minor criminal records might reduce employment rates, undermine economic self-sufficiency, and adversely impact children and other family members. While analyses of single counties or states find that prosecution and conviction harm defendants by increasing future criminal interactions (Agan, Doleac, and Harvey 2023; Kamat, Norris, and Pecenco 2023), the impacts of the misdemeanor system likely vary by geography, given differences in political preferences and fiscal constraints between local governments.[1] Despite the lack of evidence beyond criminal outcomes, there are unsettled debates about the net benefits of reducing the scope of the misdemeanor system. Proponents focus on the harms that criminal records can have on defendants, while opponents argue that reducing the system's scope would encourage criminal activity as an unintended consequence.

In this paper, we provide novel descriptive and causal evidence of how the misdemeanor system in the United States affects a broad swath of socioeconomic outcomes of defendants and their families. We begin by describing the demographic and labor market characteristics of individuals involved with the misdemeanor system and then estimate the causal impact of conviction for defendants charged with misdemeanor crimes. Leveraging our causal estimates on criminal recidivism, earnings, and social program usage, we calculate the Marginal Value of Public Funds (MVPF) of misdemeanor conviction, incorporating the idea that conviction might discourage crime beyond the focal defendants. Both analyses use linked datasets covering criminal records, earnings from employer-employee matched data, population-level socioeconomic surveys, and birth certificate records. While most of the defendants in our samples are charged with non-violent offenses, roughly 10% are charged with violent misdemeanors (e.g., simple assault, domestic violence).

First, we describe the characteristics of nearly seven million defendants engaged with the misdemeanor system across 11 states and document that being convicted of a misdemeanor is associated with a larger labor market penalty than being charged. Our data suggest that misdemeanor records are quite common—14% of adults have a misdemeanor charge by age 25. Three-quarters of these defendants have not previously been charged with a crime. Defendants are typically at the lower end of the income distribution, earning around $13,000 in the year before their charge. Despite low baseline earnings, average annual earnings fall by roughly $1,100 for convicted individuals and by around $800 for those whose charges are dismissed. This gap persists for at least five years. However, these estimates do not necessarily reflect causal effects of conviction since adverse shocks may simultaneously induce job loss and an arrest in the same quarter.

[1] Between 2008 and 2019, average misdemeanor conviction rates in the United States ranged from 17.3 to 98.0 percent across jurisdictions, with an interquartile range of nearly 22 percentage points.

Second, we estimate *causal* impacts of a misdemeanor conviction (relative to being charged) for the marginal defendant using a regression discontinuity design (RDD) generated by local reforms that occurred between 2008 and 2016. These reforms, which represent the most common way that the misdemeanor system contracts, sharply reduced misdemeanor conviction rates in seven counties and cities across five geographically diverse states: Arizona, Florida, North Dakota, Virginia, and Wisconsin. Most reforms involved budget cuts to misdemeanor units at District Attorney offices or the criminal court system, reducing either prosecutor resources or administrative staff. In the other reforms, prosecutors reallocated resources to tackle serious drug trafficking, resulting in less capacity to pursue misdemeanor cases. A key advantage of the RDD is that we compare outcomes measured over essentially the same time period for defendants whose cases were decided (i.e., disposed) just before versus just after the reforms took effect. As a result, the design nets out any impact these reforms may have had on institutions that were not directly targeted. For example, if police responded to the new policy environment by making fewer arrests that they felt would not result in a conviction, this behavior would be equally reflected in the outcomes for individuals on either side of the cut-off. Using the RDD, we find that the reforms sharply reduced the likelihood of conviction by 10 percentage points (p.p.), or 12% of the pre-reform mean.[2]

We do not find evidence that these reforms generated lasting changes in defendants' criminal recidivism or labor market outcomes over the following three years. 'Intent-to-treat' (ITT) estimates of the reforms are directionally consistent with improved outcomes and rule out small adverse impacts. 95% confidence intervals around the ITTs reject changes outside the ranges of –9.6% to 1.7% in the cumulative likelihood of recidivism, –1.8 to 3.1% in annual employment rates, and –3.2 to 7.5% in average annual earnings. Crucially, the confidence intervals for the recidivism ITT indicate that marginally reducing the scope of the misdemeanor system is unlikely to encourage defendants to re-offend, dampening a key concern of misdemeanor reform opponents.[3] We find some evidence of short-run improvements—defendants whose cases were disposed just after the cut-off were 1.8 p.p. ($p = 0.02$) less likely to be charged with a new offense within six months of disposition and 1.5 p.p. ($p = 0.11$) more likely to be employed one year later, though neither of these effects persist over longer time horizons.[4] Unsurprisingly, given the muted impacts of the reforms on defendants, we find null ITTs on their families.[5]

Even though background checks can display charges that are not convicted, prior surveys and our descriptive estimates both suggest that the causal impact of a conviction once a charge

[2]We find no discontinuous impact of the reforms on the number of cases filed, suggesting that if any sharp changes in police arrest or prosecutor charging decisions occurred, they must have exactly offset each other. Regardless, any changes in upstream decisions should not affect our estimates since cases disposed near the cut-off were filed an average of 8 months before the reforms. Moreover, our estimates are robust to reducing the influence of cases filed after the reform by using a smaller RDD bandwidth or dropping all cases filed after the reform.

[3]The MVPF analyses incorporate potential increases in criminal activity among those not in our RDD sample.

[4]Linking our sample to ACS survey responses, we find suggestive evidence that the reforms improved self-reported labor market outcomes, increased health insurance access, reduced safety net reliance, and improved mental well-being, although these estimates are imprecise due to the reduced sample size ($N \approx 1{,}200$).

[5]There is a strong first-stage on conviction likelihood for the 37% of our focal sample with an identifiable partner. However, most children, though we can identify them, are too young to be observed in our administrative datasets.

is observable is likely not zero. Employers report in surveys that they are less willing to hire applicants with convicted rather than dismissed misdemeanor charges (Agan et al. 2024*b*) and analyses of job applications find that applicants are rarely asked to report any criminal charges that they were not convicted of (Vuolo, Lageson, and Uggen 2017). Together, these features suggest that a misdemeanor conviction should affect labor market prospects for individuals at some firms.[6]

Rather, the selection patterns suggest that the muted impacts of the reform are driven by decision-makers dismissing cases for individuals i) unlikely to engage in criminal activity if dismissed, but ii) who would benefit little from avoiding conviction. Compared to the average defendant, compliers are more likely to be first-time defendants, older than 40, and charged with the most minor crimes (traffic and public order)—factors all associated with low recidivism risk. As we show below, while avoiding a conviction *can* benefit defendants with no criminal history, this is less true for defendants along the other two margins, resulting in smaller average effects.[7] Compliers' potential employment and earnings levels are also low enough that avoiding a misdemeanor conviction may not improve formal labor market outcomes by much.[8] Finally, we compare the local average treatment effect (LATE) of misdemeanor conviction for compliers to the average treatment effect (ATE), which we bound under the assumption that average potential outcomes are weakly monotonic in the likelihood of conviction (Mogstad, Santos, and Torgovitsky 2018; Kowalski 2023). The LATE is consistently at the lower end of the ATE bounds in terms of magnitude, providing suggestive evidence that the treatment effect of misdemeanor conviction is smaller for our compliers than for the average defendant.

Although the LATEs of conviction for our set of compliers are likely zero, numerous exercises suggest non-zero causal impacts for certain types of individuals, consistent with the descriptive facts laid out earlier. Correlating the reform-specific LATEs for conviction and complier characteristics, we find that low potential recidivism and high potential earnings among compliers are associated with greater adverse impacts. Misdemeanor convictions are also associated with worse impacts when compliers are young or first-time defendants. Estimates from subgroup analyses corroborate these findings, with the strongest patterns for criminal recidivism. Indeed, our findings suggest that a key driver of the difference between our estimates and the large recidivism impacts of misdemeanor prosecution for marginal defendants in leniency designs is due to complier composition—compliers in those settings are much more likely to be young and first-time defendants with low potential recidivism (Agan, Doleac, and Harvey 2023). Taken together, our results suggest that youth and lack of criminal history are observable 'tags' (Akerlof 1978) for targeting policies of non-conviction.

Finally, we calculate the MVPF of the marginal misdemeanor conviction, using our estimated

[6] We also find a descriptive labor market penalty in the counties we use in the RD analyses. In these counties, the post-filing drop in average annual earnings is 120% larger for convicted rather than dismissed charges.

[7] In the context of felonies, the benefits of avoiding a conviction are much larger for younger defendants (Mueller-Smith and Schnepel 2021; Augustine et al. 2022). In general, surveys find that employers are most wary of convictions for violent and property crimes, suggesting that more minor convictions are unlikely to harm employment prospects (e.g., Holzer, Raphael, and Stoll 2007).

[8] These patterns are consistent with other stages of the criminal legal system—a key reason for the muted treatment effects of incarceration on formal employment and earnings is that incarcerated individuals tend to be detached from the formal labor market to begin with (Garin et al. 2023).

LATEs for focal defendants (Hendren and Sprung-Keyser 2020; Finkelstein and Hendren 2020). We calculate an MVPF of –3.8, chiefly driven by increased recidivism and reduced earnings. While this MVPF suggests that the direct impacts of misdemeanor conviction represent welfare losses, it does not incorporate the possibility that the marginal conviction might deter others from engaging in criminal activity. We quantify plausible "general deterrence" effects using 1) changes in the number of cases filed due to our reforms and 2) estimates of how electing progressive prosecutors affects reported crime from Agan et al. (2025).[9] At best, the marginal conviction prevents the equivalent of 3–5 property crimes or 10–15 traffic crimes, values at which the MVPF is still negative, and far below the 23 property crimes or 70 traffic crimes needed to offset welfare losses and government expenditures. If we alternatively assume our LATEs are truly zero, the MVPF is positive and around 2 given these values of general deterrence. While the marginal conviction is welfare-improving under these conditions, competing policies offer more efficient uses of government funds.

This paper contributes to the literature in several ways. First, we provide novel evidence of the causal impacts of a misdemeanor conviction on defendants and their families across numerous dimensions. We combine administrative and survey data with policy reforms from five states, which provides critical variation to examine treatment effect heterogeneity. Most prior causal evidence has focused on defendants' criminal recidivism in single counties or states due to data limitations (Agan, Doleac, and Harvey 2023; Kamat, Norris, and Pecenco 2023; Rao and Reeves 2026). While audit studies find that employers call back applicants with misdemeanor records at lower rates (Uggen et al. 2014; Leasure 2019), this evidence is silent on how such employer behavior affects real-world labor market outcomes, safety net usage, and well-being.[10] Our results provide further evidence that targeting is crucial to ensure that well-intentioned criminal legal policies improve socioeconomic outcomes (e.g., Rose 2021*b*), mirroring the importance of targeting for optimal design of other social programs (e.g., Deshpande and Li 2019; Finkelstein and Notowidigdo 2019).

Second, we provide new estimates of the MVPF of misdemeanor conviction that incorporate the possibility that the marginal conviction might deter crime beyond the directly affected defendant. Recent policy discussions have debated whether such general deterrence effects outweigh personal costs to defendants, with scant empirical evidence. From our calculations, the marginal conviction reduces net social welfare, and even the most optimistic estimates of general deterrence are not large enough to offset the welfare losses of conviction.

Finally, we provide new evidence characterizing the lives of people who interact with the criminal misdemeanor system across 11 states that cover 28% of the U.S. population. In doing so, we build on existing work that documents how misdemeanor adjudication is associated with persistent labor market penalties in only a few states (Rose 2021*a*; Agan et al. 2024*b*). The average penalties we estimate using our broad range of geographies are smaller than (but directionally consistent with) estimates from prior work, largely due to considerable variation across contexts.

[9] While we find no significant impact of the reforms on the number of cases filed, we use the upper end of the 95 percent confidence interval as a best case estimate of general deterrence for this exercise.

[10] While their focus is not on minor crimes, Hjalmarsson et al. (2025) find that minor charges modestly reduce employment and earnings in Sweden. They do not examine the impacts on other outcomes or the impact of conviction.

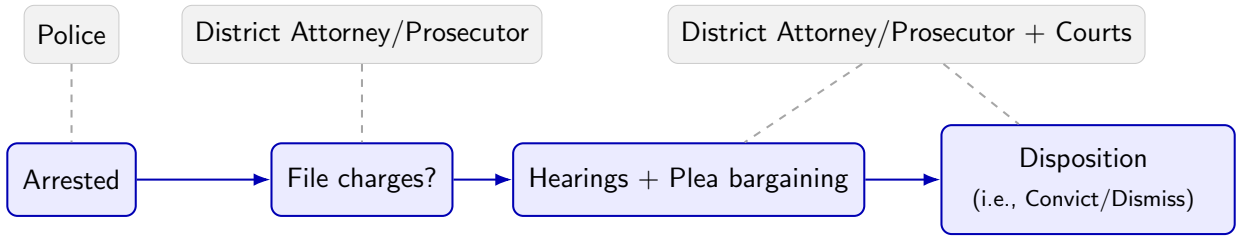
2 Institutional Background and Settings

In this section, we provide more detail on the misdemeanor segment of the U.S. criminal court system. We then describe the policy reforms that yield the natural experiments that we use to understand the causal impacts of misdemeanor convictions.

2.1 The Misdemeanor System

In the criminal court system, there are typically two types of charges—felonies and misdemeanors. Felony-level charges are the more severe of the two and include, but are not limited to, offenses like homicide, robbery, burglary, and drug distribution. Sanctions for felony convictions often include lengthy prison sentences in state correctional facilities. On the other hand, misdemeanor charges typically represent less severe offenses, including marijuana possession, petty theft, and criminal damage. Accordingly, sanctions for misdemeanor convictions commonly include fines, probation sentences, or community service and at most a one-year spell in a local jail facility. Together, misdemeanor charges represent around three-quarters of all criminal charges and are the most common form of contact with the criminal court system (Stevenson and Mayson 2018). Indeed, in our data which we discuss below, we find that 14 percent of adults born between 1980 and 1998 will have a misdemeanor charge by age 25 and 10 percent will have a conviction.

Figure 1: The misdemeanor adjudication process



Note: This figure reports a simplified overview of the misdemeanor adjudication process in the United States. Criminal legal system actors who make decisions are listed in the top row and the action at each step in the case proceedings is listed in the second row.

Figure 1 provides a high-level description of the misdemeanor adjudication process. The process begins with police, who arrest defendants suspected of misdemeanor offenses and then submit a subset of these cases to the District Attorney. Prosecutors at the District Attorney's Office will then choose whether to file charges and pursue a case. Prosecuted defendants may then appear in court for hearings. Most misdemeanor cases are resolved through plea bargaining rather than jury trials, a multi-stage process involving interactions between prosecutors and defense attorneys. As a result of this process, many defendants may plead guilty to certain charges rather than being found guilty by a jury. The final resolution of the case is called its disposition. Note that even after prosecutors decide to file charges, they can decide to ultimately dismiss charges against individuals.

In addition to their prevalence, the rates at which misdemeanor offenses are convicted vary

widely across jurisdictions. In the United States, the mean misdemeanor conviction rate between 2008 and 2019 at the jurisdiction-year level was 54.9 percent, with felonies following a similar level at 51.5 percent. However, within jurisdiction, misdemeanor conviction rates are around 17% more variable, based on within-jurisdiction standard deviations.[11] The greater variability in the misdemeanor system may reflect a combination of expansive caseloads and challenging circumstances for prosecutors (e.g., Frederick and Stemen 2012; Gershowitz and Killinger 2015), limited resources (e.g., Rao and Reeves 2026), or changes in office-wide priorities.

Despite representing relatively minor infractions, convictions for misdemeanor offenses still have the potential to harm both short- and long-run socioeconomic outcomes. Prior research has shown how misdemeanor convictions in a few states are associated with persistent declines in earnings and employment (Agan et al. 2024*b*). Moreover, this hampered labor market potential due to convictions is further compounded by occupational licensing restrictions (Rodriguez and Avery 2016), which might subsequently increase reliance on the social safety net and foster housing instability. Misdemeanor prosecution also strengthens the degree of contact with the criminal legal system, increasing the probability of future arrests (Agan, Doleac, and Harvey 2023). Taken together, the socioeconomic consequences of misdemeanor convictions are potentially wide-ranging and life-altering. If misdemeanor convictions are harmful, policies which reduce these ramifications may be welfare-improving. Additionally, given the variation in misdemeanor conviction rates across jurisdictions, it is plausible that the impacts of the misdemeanor system are heterogeneous, as are responses to other interventions. For example, the labor market penalty for women after having a child (Kleven, Landais, and Leite-Mariante 2025) and the scarring effects of job loss (Bertheau et al. 2023) vary widely by country. This variation is largely due to differences in social norms and the structure of public institutions, which are similarly heterogeneous across the United States.

2.2 Natural Experiments that Shift Misdemeanor Conviction Rates

We now discuss the seven natural experiments that we combine in our main empirical strategy. These are generated by sharp policy changes that occur in five geographically diverse states: Arizona, Florida, North Dakota, Virginia, and Wisconsin. Policy changes in these jurisdictions made misdemeanor prosecution more difficult in different ways, e.g., by sharply reducing resources to prosecute misdemeanors, or by shifting priorities to more serious offenses. The policy changes that we study fall into two broad categories: 1) large cuts to the budgets of district attorneys or courts that primarily work on criminal misdemeanor cases and 2) alterations in the political environment that change district attorney priorities. We refer to the first category of natural experiments as 'budget cuts' and the second category as 'priority changes'. We use the announcement of the budget cut or policy change as the effective date, unless noted otherwise. The reforms are summarized below in Table 1 and we describe each of the natural experiments in additional detail below.

[11] Authors' calculations using data from Finlay, Mueller-Smith, and the CJARS team (2024). We calculate the within-jurisdiction standard deviation of conviction rates, separately for felonies and misdemeanors using a common sample and then compute the percent difference in the average standard deviation.

Table 1: Details on natural experiments

Experiment	State	Effective Date	Type	Affected Institution
Pima County	Arizona	October 1, 2008	Budget cut	Prosecutor
Orange County	Florida	March 1, 2010	Budget cut	Clerk
Williams County	North Dakota	November 14, 2013	Priority change	Prosecutor
Newport News City	Virginia	October 10, 2014	Priority change	Prosecutor
Dane County	Wisconsin	September 15, 2016	Budget cut	Prosecutor
Milwaukee County	Wisconsin	October 3, 2016	Budget cut	Clerk
Outagamie County	Wisconsin	September 15, 2016	Budget cut	Prosecutor

Budget cuts

Five of our seven natural experiments are generated by large budget cuts to either the local equivalent of the prosecutors' office (e.g., Office of the District Attorney or County Attorney) or court clerks. Clerks play a crucial role in maintaining the administrative non-judicial functions of the court system—they often help manage case documents, file cases, and schedule hearings. When either of these institutions unexpectedly loses funding, it can reduce the number of cases that prosecutors can successfully prosecute, even when cuts are focused on administrative staff. In fact, surveys of prosecutors suggest that a dearth of investigative, support, and clerical staff prevents prosecutors from devoting adequate time to their cases and can affect case outcomes (Frederick and Stemen 2012). When budget shortfalls are unexpected and large, it is plausible that prosecutors prioritize cases and dismiss certain cases that they no longer have the resources to process.

In October 2008, the Pima County Attorney lost federal grant money that funded five full-time employees in their drug crime unit (Meltzer 2008). In addition to the loss of this funding, the County Attorney had lost some of their most experienced attorneys (Flick 2008; Smith 2008), which would exacerbate the effects of losing federal funding. We use the start of the following federal fiscal year as the effective date here since that is when the grant funding would have lapsed.

In Wisconsin, a new state law in 2016 required agencies to submit budget proposals that cut their funding by 5%. There was widespread media coverage of how most district attorney offices in Wisconsin were poorly funded and understaffed (Litke 2016; Schneider and Litke 2016). In this environment, District Attorneys' offices submitted their budget proposals in September 2016. Certain offices noted that they actually needed to hire more prosecutors rather than cut them, with Dane and Outagamie facing the largest shortfalls (needing 9 and 11 new prosecutors respectively). Reporting suggests that District Attorneys were pessimistic about their requests and viewed these proposals as de facto 5% cuts, especially given that the Wisconsin governor vetoed all prosecutor requests in the previous budget cycle (Beck and DeFour 2016).[12] Given their pessimism, we use the announcement of their budget proposal as the effective date.

While Milwaukee County's District Attorney did not face the same state-imposed budget cuts that other Wisconsin counties did, the county budget reduced the resources available to the

[12]Their pessimism was warranted—the Governor once again rejected all District Attorney requests and also imposed additional cuts (Walker 2017).

county court system. The approved county budget, announced in October 2016, reduced court funding by 20%, which represented 17 employees (Terrell 2016; Abele 2017). Key district attorney priorities were also set to lose funding through federal grants expiring in 2016 and 2017 (Abele 2017). Anticipation of this additional loss of funding may have factored into prosecutors' decisions when the county budget was announced, which we use as the effective date.

A budget cut to the court system also affected Orange County, Florida in 2010. The Clerks of Court across Florida were already reeling from large cuts in 2009 that diverted revenue streams usually intended for the clerks to other statewide agencies (SB 2108, 2009; Chapter 2009-204; Gonzalez 2009). Additional budget cuts were introduced in bills in March 2010, which amounted to a 10% cut to the Orange County clerk's budget and a potential layoff of 100 individuals (HB 1513, 2010; HB 5401, 2010; Willoughby 2010). While only some of the bills passed, we use the filing date of the first 2010 bill as the effective date for this natural experiment.

Priority changes

The final two natural experiments take the form of changes to prosecutorial priorities. We use the announcements that Williams County, North Dakota and Newport News City, Virginia would participate in the federal 'High Intensity Drug Trafficking' (HIDTA) program (Grand Forks Herald 2013; White House 2013; Federal Register 2014). The HIDTA program increases coordination between federal and local law enforcement agencies to reduce drug trafficking and drug use in the chosen regions. Areas that are part of this program prioritize the prosecution of serious drug offenses. We use the announcement of the HIDTA designations as the effective dates.

3 Data Infrastructure

We link numerous individual-level administrative and survey datasets, covering multiple U.S. states, available through the Federal Statistical Research Data Center (FSRDC) system. These data linkages let us capture recidivism, labor market activity, and other socioeconomic outcomes for our core sample of misdemeanor defendants. We also use these data to measure pre-existing household structure, which we use to understand how misdemeanor convictions may indirectly affect other family members. Crucially, this data infrastructure lets us measure outcomes that occur in multiple jurisdictions, not just in the counties in which we first observe individuals.

Sample and Crime Outcomes: We define our core samples of misdemeanor defendants using criminal court records from the Criminal Justice Administrative Records System 2025:Q1 vintage (CJARS) (Finlay and Mueller-Smith 2024). We also use these data to measure crime-related outcomes (e.g., the final disposition of a case, future recidivism) and to understand how the features of the criminal legal systems vary across our settings.[13] While the CJARS data currently cover 39 U.S. states that represent approximately 80% of the U.S. population, the court records, which we use to define our samples and measure crime outcomes, are available for 20 states. We use data from 11 of

[13]The CJARS data let us capture recidivism that occurs anywhere else within the state an individual's case was disposed, rather than only recidivism within the same county.

the 20 states for our event study analyses tracing out the labor market impacts of interactions with the misdemeanor system—Arizona, Indiana, Maryland, North Dakota, Oklahoma, Oregon, Pennsylvania, South Carolina, Texas, Virginia, and Wisconsin (see Table B1 for the time coverage we use for each of these states). The focal sample we use to understand the causal impact of a misdemeanor conviction is limited to the five states in which the natural experiments we study occur—Arizona, Florida, North Dakota, Virginia, and Wisconsin.

Labor Market Outcomes and Socioeconomic Surveys: We measure labor market outcomes (e.g., employment, earnings) using the Longitudinal Employer-Household Dynamics (LEHD) datasets. These data are sourced from each state's unemployment insurance records and provide quarterly information on individuals' earnings as well as characteristics of their employers. The map in Figure B1 displays the 29 states whose LEHD data we use in this paper.

We have LEHD data for all the states in our event study sample and all states in which the natural experiments occur, except for Florida. Hence, we omit the Florida natural experiment from all analyses of LEHD labor market outcomes, but show robustness to measuring labor market outcomes using representative population-level surveys that include Florida. We construct labor market outcomes for individuals in the rest of the sample by combining their LEHD records across all of the states whose LEHD data we use. This lets us track labor market outcomes even for people who move from the state we initially observe them in to another state that we have LEHD data for. We measure self-employment income, social safety net usage (e.g., SNAP, SSI, Medicaid), and mental well-being using the 2008–2019 American Community Surveys (ACS), which we also link to the CJARS records.[14] All income measures are adjusted to be in 2017 dollars.

Identifying Other Household Members: While our core sample consists of individuals who are directly involved with the criminal misdemeanor system, we also explore spillovers onto other family members (e.g., children, spouses, romantic partners). We construct detailed linkages between individuals and their family members by combining the Census Household Composition Key, which is sourced from birth records (Genadek, Sanders, and Stevenson 2022), with family structure and co-residency information from the Decennial Censuses, as has been done in prior work (e.g., Finlay, Mueller-Smith, and Street 2023; Joung and Reeves 2025). We measure outcomes for people indirectly affected by the criminal legal system using the same data infrastructure described above. Given the timing of the reforms discussed in Section 2, most of the children of individuals in our sample will be too young to be captured in the CJARS or LEHD records. As a result, we measure outcomes for children of these individuals using survey records, which greatly reduces the size of the sample used to assess these spillovers.

Baseline Demographics: Finally, we measure demographic characteristics using the Numident and the Census Best Race File provided by the Social Security Administration. These datasets provide information on baseline characteristics (e.g., age, race/ethnicity, gender) of individuals in our

[14] We exclude the 2020 ACS from our analysis to avoid issues with response rates during the onset of the COVID-19 pandemic (US Census Bureau 2021).

analysis samples, which we use to assess the key identifying assumptions of our empirical strategy, improve precision of our estimates, and assess treatment effect heterogeneity.

Individuals in this data infrastructure can be linked across datasets by a unique identifier called a Personal Identification Key (PIK). We limit all of our analyses to individuals who have a PIK so that everyone in our sample can be linked across datasets.

4 Evidence on the Labor Market Effects Associated with Misdemeanor Records

In this section, we first describe the event study strategy we use to characterize the labor market effects of interacting with the misdemeanor system across 11 states, following Rose (2021*a*) and Agan et al. (2024*b*). We use this empirical approach to document that convicted individuals experience persistent reductions in employment and earnings and increases in poverty status that are greater than those for individuals whose cases were ultimately dismissed. We also show that these labor market penalties vary across states, motivating the heterogeneity analyses we conduct when estimating the causal impacts of misdemeanor conviction.

4.1 Event Study Design

We examine how labor market outcomes evolve around misdemeanor charges and convictions, focusing on individuals with a misdemeanor case disposed in 11 states between the mid-1990s and mid-2010s. Combined with the temporal LEHD coverage, we construct a balanced sample of individuals with labor market outcomes for a period of five years before and after their cases were filed (see Table B1 for the complete list of states and time periods). For each individual, we include only their first misdemeanor case across all the states we use to ensure that our sample includes only one event per person, leaving us with almost seven million defendants.[15] Of the defendants in the resulting sample, 75% are male, 57% are White, 20% are Black, and 14% are Hispanic (see Table A1). 76% of defendants have no prior criminal history and 54% of them are between the ages of 18 and 31 at the time of case filing. In the year before case filing, approximately 65% of defendants were employed, earning around $13,000 per year (67% of defendants earned less than the federal poverty line). Non-violent charges make up 88% of all charges, with public order (26% of all charges), criminal traffic (21%), and property (16%) offenses as the top three broad categories.

We trace the labor market trajectories using Equation 1, estimated separately for individuals who: a) had all charges dismissed and b) were convicted of at least one charge. This separate estimation lets us understand how the labor market effects associated with misdemeanor adjudication vary between individuals who were charged with an offense and those who were charged and convicted. Differences in the labor market trajectories for these two groups provide suggestive

[15] This restriction does not necessarily limit to individuals with no prior criminal record. For instance, individuals in this sample may have prior misdemeanor charges from before our labor market data begin or may have prior felony charges.

evidence on the effect that a misdemeanor conviction has relative to being charged. In Equation 1, Y_{ist} represents our main outcomes: employment, earnings, and whether earnings were below the quarterly equivalent of the federal poverty line in quarter t. We include individual fixed effects (α_i) to account for heterogeneity across individuals. We also include fixed effects for individuals' ages in each time period ($\alpha_{i,t}$) and time (α_t) to account for changes in labor market activity over the life cycle and secular trends in the economy. The separate estimation approach allows these non-parametric terms to flexibly vary across convicted and charged groups. To make these analyses computationally tractable, we use a 50% random sample of defendants, stratified based on conviction status and state of disposition. This leaves us with a sample of 2.2 million convicted defendants and 1.2 million dismissed defendants.

$$Y_{i(s)t} = \sum_{k \neq -1} \beta_k \mathbf{1}(t = k) + \alpha_i + \alpha_{i,t} + \alpha_t + \varepsilon_{it} \tag{1}$$

Each of the β_k coefficients estimates the average difference in the outcome relative to the quarter before the case is filed.[16] This normalization is designed to capture any changes in labor market activity while charges are pending.[17] Finally, we reweight observations in this analysis using the relative sample size of defendants in each state, ensuring that each state is weighted equally. As a result, our estimates are not disproportionately weighted towards either larger states or areas with more pervasive criminal legal systems.

While event studies are quite feasible to implement, this strategy may not necessarily reflect causal estimates of misdemeanor adjudication on labor market outcomes. For example, an adverse contemporaneous shock (e.g., mental health challenges) could induce job loss and lead to an arrest that resulted in a criminal event filed in the same quarter. However, this event study design lets us use a large sample of defendants to provide precise but suggestive evidence on the marginal effect of a misdemeanor conviction relative to a misdemeanor charge and how these effects vary with geography.

4.2 Descriptive Evidence on Labor Market Impacts of Misdemeanor Convictions

In Figure 2, we examine how labor market outcomes evolve around a misdemeanor charge, separately for charges that were dismissed versus convicted. Panel a) documents how formal employment evolves for these two groups. In the years leading up to a misdemeanor charge, quarterly employment rates increase for defendants in both groups and around half of each group was employed at baseline (quarter before the case was filed). Quarterly employment rates fall after the charge is filed, dropping by around 6 p.p. at the end of five years. Over the five-year period after a charge is

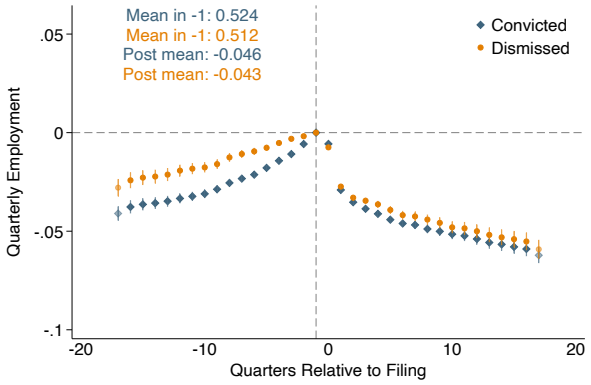
[16] We bin labor market outcomes 17 to 20 quarters before or after case filing together, which lets us separately identify the time-varying age controls as well as the coefficients on the relative time indicators.

[17] Pending charges are reported on employer background checks in many states (Agan et al. 2024*b*), although samples of job applications suggest employers often do not ask about such charges (Vuolo, Lageson, and Uggen 2017).

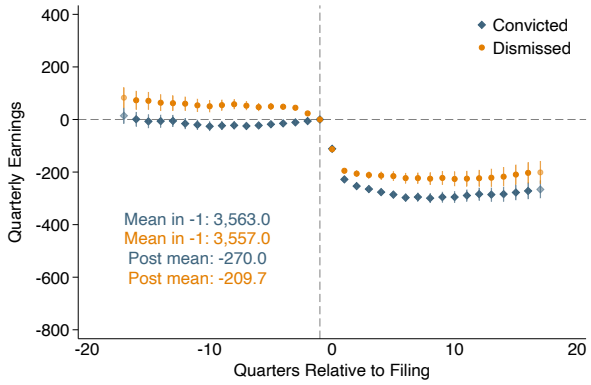
filed, average quarterly employment falls by 4.6 p.p. (9% of baseline mean) and 4.3 p.p. (8%) for individuals with convicted and dismissed charges respectively.[18]

Figure 2: Event study: labor market trajectories around the time of misdemeanor charge filing

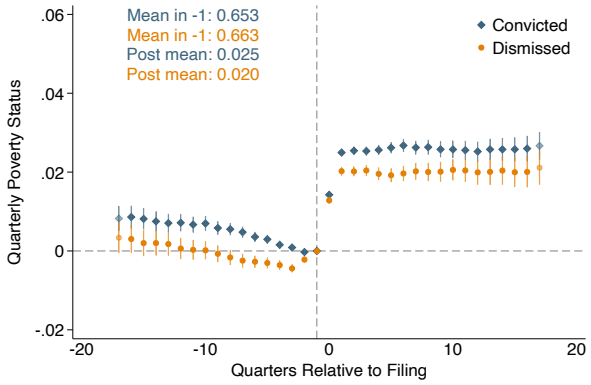
(a) Any Employment



(b) Quarterly Earnings



(c) Likelihood of Quarterly Earnings Below Federal Poverty Line



Note: This figure presents event study estimates of quarterly labor market outcomes around the time of misdemeanor charges being filed against the individual. The estimate in the quarter before the case was filed is normalized to zero. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13065.

Moving to earnings, Panel b) shows that while defendants with convicted and dismissed charges have similar quarterly earnings at baseline (around $3,600), the quarterly earnings penalty over five years for those convicted is approximately 30% greater ($270 for convictions versus $210 for dismissals). The earnings penalties for both of these groups are modest but represent meaningful amounts considering the relatively low baseline level of quarterly earnings. Over the five years after the initial misdemeanor charge, quarterly earnings fall by an average of 7% of the baseline mean for convicted defendants and by 6% of the baseline mean for dismissed defendants.[19] Panel c) of

[18] Using court records from Maryland and Texas (Bexar County) and tax data, Agan et al. (2024*b*) find that initial misdemeanor non-convictions and convictions are associated with employment reductions of around 5–8 p.p. six years after disposition, with slightly larger reductions for convictions.

[19] The penalties we estimate are smaller than those estimated in Rose (2021*a*), who finds that convictions for

Figure 2 documents how these earnings reductions translate to time spent with earnings below the federal poverty line. On average, a misdemeanor conviction is associated with a 2.5 p.p. (4% of baseline mean) increase in poverty status while a charge that is dismissed is associated with a 2 p.p. (3%) increase.

Taken at face value, these event study estimates suggest that the penalty associated with a misdemeanor conviction is larger than the penalty associated with a charge that is dismissed, albeit by a modest amount. These penalties, and the differences between them, may be influenced by local labor market characteristics and employer preferences, which vary across different parts of the United States. Next, to understand how much the labor market penalties vary across geography, we estimate versions of Equation 1 for each state in our data, again separately for convictions and dismissals.

Table 2: Variation in labor market penalties across states

Outcome	Convicted	Dismissed	Gap ((1)–(2))
	(1)	(2)	(3)
Quarterly Employment			
Mean	−0.054	−0.050	−0.004
SD	0.010	0.015	0.013
Quarterly Earnings			
Mean	−328	−254	−75
SD	79	110	84
Quarterly Poverty Status			
Mean	0.031	0.024	0.007
SD	0.006	0.010	0.010

Note: This table presents results from estimating Equation 1 by state. We calculate the average penalty within each state by taking the mean of the post-period coefficients and then compute the cross-state mean and standard deviation of these penalties. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13065.

Column 1 of Table 2 presents the mean and standard deviation of the average post-period change for each outcome across states for convicted defendants. Column 2 presents the analogous set of estimates for dismissed defendants.[20] For both groups of defendants, the standard deviations of the post-period penalties are roughly 20–40% as large as the mean penalties, with earnings penalties displaying the greatest relative variability, suggesting that there is considerable variation in the labor market penalties across states.

We see an even greater degree of cross-state variability in the marginal effect of a misdemeanor conviction beyond a charge. Column 3 presents the mean and standard deviation of the difference in post-period outcomes for defendants who were convicted and those who were dismissed. Across

serious misdemeanors which result in prison spells reduce average quarterly earnings by at least $800 in Washington State. In contrast, our sample includes all misdemeanors, including relatively minor offenses that do not lead to carceral sanctions.

[20]The post-period means reported in Table 2 are different from those displayed in Figure 2 because the former allows the coefficients on the control variables to vary by state while the latter estimates a single coefficient per control variable.

all of the outcomes, the standard deviation in the labor market penalty is roughly between 10% and 220% *larger* than the mean penalty. While these conviction–dismissal gaps in labor market outcomes may not reflect the causal effect of a misdemeanor conviction relative to a charge, they likely provide an upper bound on its magnitude, given the direction of common selection stories. Additionally, these estimates are suggestive evidence that the impacts of a misdemeanor conviction might vary considerably across jurisdictions.

5 Assessing Policy Reforms and the Causal Effects of Misdemeanor Conviction

In this section, we use a regression discontinuity design (RDD) to estimate the impact of the policy reforms on defendant outcomes and examine the *causal* impact of being convicted of a misdemeanor offense relative to being charged. We first outline the RDD, which is generated by the seven natural experiments discussed in Section 2, and demonstrate its validity. We then use the RDDs to discuss the overall impacts of the policy reforms on defendant outcomes and the implications for the causal impact of a misdemeanor conviction. Finally, we use the variation in local government constraints and preferences across the seven natural experiments we study to provide policy guidance on where misdemeanor convictions might be more or less harmful.

5.1 Regression Discontinuity Design

We build on the descriptive analysis described in Section 4 and develop an empirical strategy to recover *causal* impacts of a misdemeanor conviction. Specifically, we use the sharp nature of the budget cuts and priority changes discussed in Section 2 and an RDD to study the impact of a misdemeanor conviction relative to a dismissal for the marginal defendants affected by the reforms. The RDD effectively compares the outcomes of individuals whose cases were disposed just before versus just after a policy reform went into effect. If the policy reforms are unanticipated, cases disposed just before versus after a reform should otherwise be similar except for the sharp change in the likelihood of conviction. For example, an individual whose case is disposed before a large, unanticipated budget cut might be more likely to be convicted than an individual whose case is disposed just after the cut was announced. Such date-based RDDs have previously been used to study policy reforms in the criminal legal system (e.g., Tuttle 2019; Finlay et al. 2024).

We pool all of the natural experiments and formally estimate RDDs using Equation 2. The running variable $Date_i$ is the disposition date for individual i whose case was disposed in experiment e. $Post_i$ is an indicator for cases disposed after the policy reform, Y_{ie} represents the outcome of interest, and X_i is a vector of baseline covariates (offense type, criminal history, age, gender, race/ethnicity, employment and earnings in previous year, indicators for missing race/ethnicity). We flexibly allow the trends on either side of the discontinuity and the baseline covariates to vary with natural experiment. Thus, β represents the average 'intent-to-treat' (ITT) effect across all

the natural experiments of being disposed just after versus just before the policy reform. Our main analyses report standard errors clustered at the individual level.

$$Y_{ie} = \alpha_e + \beta Post_i + \gamma_e Date_i + \delta_e(Post_i \times Date_i) + X_i'\theta_e + \varepsilon_{ie} \tag{2}$$

Similar to the event study analyses, our main RDD results re-weight observations so that each natural experiment is weighted equally. As Finlay et al. (2024) note, this reweighting strategy is less statistically efficient but ensures that β reflects the average of treatment effects across settings that potentially have very different marginal defendants.

To interpret β as a causal impact, we need to satisfy several assumptions, which we empirically probe in Section 5.2. For β to represent an ITT of the reforms we study, the reforms must satisfy an independence assumption, requiring that defendants on either side of the policy discontinuity are comparable to one another. We assess this by conducting a battery of tests to ensure that defendant characteristics are smooth across the discontinuity, ruling out systematic sorting in the decision to dispose cases on one particular side of the policy date cut-offs. We also test for discontinuous changes in the number of cases filed, to assess whether the reform sharply affected police arrest or prosecutor charging decisions.

Critically, the RDD design we use also addresses concerns that the budget cuts and priority changes may have impacted other institutions in the affected counties. Since the design compares defendants whose cases are disposed just before the cut-off date to those whose cases are disposed just after, their outcomes are measured over essentially the same period of time for both sets of defendants. As a result, the RDD design nets out any impact the reforms had on the behavior of other institutions. For instance, say police responded to the new policy environment by making fewer arrests for minor offenses. This behavior would be equally reflected in the outcomes for individuals on both sides of the cut-off.

Moving beyond the ITT, we need to satisfy additional assumptions to use the reforms to understand the causal impact of a misdemeanor conviction, i.e., the local average treatment effect (LATE). First (relevance), the policy reforms need to actually reduce misdemeanor conviction rates, which we document in Section 5.2 by providing evidence on the 'first-stage'. Second (monotonicity), the policy reforms must weakly make defendants less likely to be convicted, ruling out 'defiers'. For example, this assumption would be violated if the reforms decreased the likelihood of conviction for some defendants and increased it for others. We provide suggestive evidence for this by documenting the stability of the first-stage estimates across different subsamples. Third (exclusion), it must be the case that the only way that the reform affected defendant outcomes is by changing their conviction status. This assumption would be violated if whether a defendant's case was disposed on either side of the cut-off sharply affected their eligibility for other services or interventions, e.g., court-mandated drug treatment or diversion. In Appendix C.1 we outline the conditions under which we can sign the resulting bias from such violations.

Our main RDD sample uses a window of 270 days on either side of each policy cut-off, although

we explore robustness to re-estimating our main results with varying bandwidths below.[21] We restrict the sample to only include each person's first disposition event to avoid multiple appearances in our sample, which could be a function of disposition outcomes.[22] For two of the natural experiments (Pima County and Milwaukee County), we exclude cases disposed six weeks before and after the policy announcement since we observe mild sorting behavior.

The main outcomes we focus on are whether any charge associated with a case was convicted, recidivism, employment, earnings, and being below the federal poverty line, using the CJARS and LEHD data. We also study the impacts on self-employment income, safety net usage, and mental well-being using ACS responses. Our baseline analyses estimate impacts up to three years after a defendant's case disposition since that is the longest follow up horizon available for all outcomes across all natural experiments.[23]

While all of the coefficients we present are estimated using versions of Equation 2 on individual-level data, the points in the scatter plots that visualize the RDD present binned means of the data. We compute the means by first residualizing the individual-level data on covariates (except for the validation exercises). We then collapse the residualized variables into ten equally spaced bins on either side of the cut-off. The lines of best fit in such scatter plots are estimated using ordinary least squares on the binned data.

Our main analyses focus on defendants whose cases are disposed around the policy cut-offs. When we move to assessing spillovers on family members of this focal sample, we follow a strategy similar to Equation 2, except the outcome is measured for the family members while the remaining terms stay the same. That is, we compare the outcomes of family members for whom the focal defendant's case was disposed just before versus just after a policy reform.

5.2 Assessing the Validity of the Regression Discontinuity Design

We now show that the budget cuts and priority changes discussed in Section 2 yield valid regression discontinuity designs (RDD). We test for violations of the identifying assumptions outlined in Section 5.1 by conducting a battery of validation exercises.

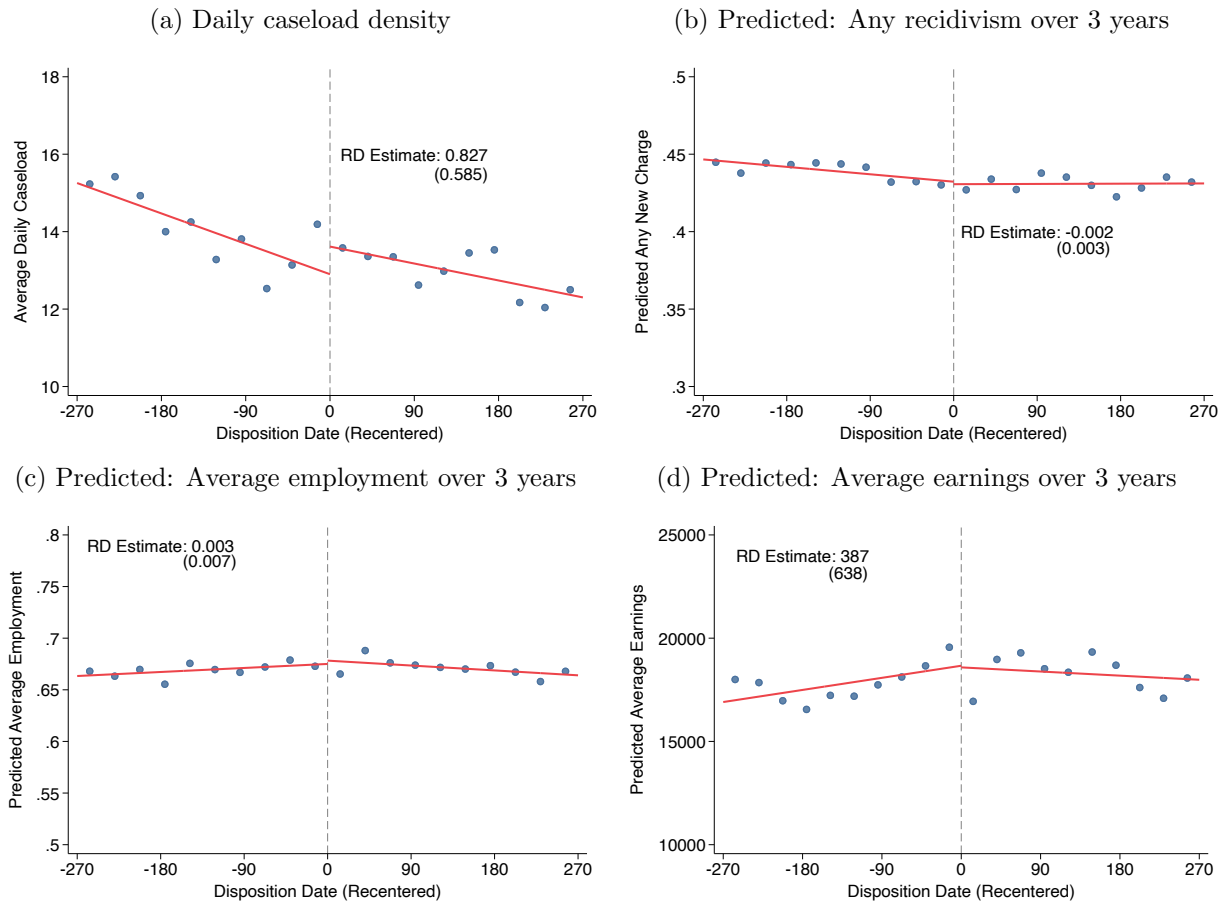
Figure 3 presents evidence that the defendant characteristics are smooth around the date cut-offs on average. Panel a) tests whether the number of cases disposed discontinuously changes around the cut-off. Any significant evidence of discontinuities here might indicate sorting behavior such that cases disposed just after the reform could be unobservably different, e.g., if prosecutors disposed cases for particular types of individuals just before the cut-off, in anticipation of the

[21] We choose a baseline bandwidth of 270 days by 1) estimating the optimal bandwidth using the `rdbwselect` package (Cattaneo, Idrobo, and Titiunik 2019) across our main outcomes and 2) taking the median bandwidth across these outcomes. We prefer to use a single bandwidth across all of the main analyses so that our estimates reflect the same underlying sample across outcomes.

[22] We apply this restriction to a larger window of four years surrounding the policy cut-off so that the designation of whether a case was someone's first appearance in our sample does not change with the RD bandwidth.

[23] Most recidivism occurs in a short period after disposition. In Suffolk County, Massachusetts, over 80% of new criminal complaints five years after disposition occur in the first three years, with nearly 60% coming in the first post-disposition year (see Appendix Table B5 of Agan, Doleac, and Harvey (2023)). The control means in our analysis of dynamics in recidivism document similar patterns (see Table A5).

Figure 3: Validating natural experiments



Note: Each Panel presents RDD estimates using a version of Equation 2 that excludes covariates. Standard errors are heteroskedasticity-robust in Panel a) and otherwise are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890, CBDRB-FY26-P2957-R13065.

reforms. We find that the number of cases disposed is smooth around the cut-off, suggesting limited scope for this type of sorting behavior.

Panels b)–d) present summary evidence that the baseline observable characteristics of defendants are smooth around the cut-off. We first predict cumulative recidivism as well as average annual employment and earnings three years after disposition using two-way interactions of the covariates we include in our main analysis.[24] We find no evidence of discontinuities in any of these summary measures. Moreover, Figure A1 finds no discontinuities in alternative summary measures that predict cumulative versions of employment and earnings.

Table 3 presents RDD estimates from tests of whether each individual baseline covariate is smooth around the cut-off as well as average characteristics of defendants in our sample. Focusing on the covariate-specific balance tests first, the estimates in Column 2 of Table 3 suggest that

[24]Specifically, we include demographics (gender, race/ethnicity, age, flags for missing demographics), offense type (violent, drug, criminal traffic), and whether a defendant has a criminal record. We also include cumulative employment and earnings in the four quarters before disposition when predicting labor market outcomes.

defendants on either side of the cut-off are similar across most observable case characteristics. However, there is some chance imbalance in terms of race and whether an individual is charged with a violent or public order offense, although these estimated discontinuities are generally a small proportion of the pre-policy means in Column 3. One exception is whether an individual is charged with a violent offense, which is a large proportion of the pre-policy mean (24%). However, the absolute number of violent offenses in our sample is small. In our main specifications, we control for these characteristics as well as the other characteristics mentioned in Section 5.1 to account for any chance imbalance and improve precision. Regardless, our estimates are robust to excluding covariates, as we discuss in Section 5.3.

Even though these patterns suggest that defendants whose cases are disposed just before versus just after the reform are comparable, we conduct additional exercises to ensure that our analysis is not affected by any reform impacts on upstream stages of the criminal legal system, e.g., prosecutors' decisions to file charges. Table A2 documents that the reforms did not sharply affect the number of cases filed. However, there are compositional shifts in the types of cases filed. Specifically, cases are more likely to be filed for offenses that typically involve high levels of police discretion or tend to generate fine revenue.

While these patterns represent interesting evidence on how the reforms affected upstream decisions, we rule out that such changes confound our findings in multiple ways. First, the average time between filing and disposition in our sample is 8 months, suggesting that cases disposed on either side of the policy cut-offs were filed before the reforms, and at similar times. Second, in robustness checks below, we show that our results are stable when shrinking the RD bandwidth, which reduces the influence of cases filed after the reform on our estimates. Third, Panel A of Table A3 repeats our validation exercises, limiting our sample to cases disposed within a small window around the reform but all filed before the reform, which entirely excludes any impact of changes in filing behavior.[25] Across all of these exercises, we find no evidence of sorting or compositional shifts in the types of defendants whose cases are disposed just before versus after the reforms. Overall, these results suggest that the natural experiments we discuss here provide valid quasi-experimental variation.

Turning to our analysis sample, Column 1 of Table 3 presents average characteristics of defendants whose cases are disposed within the RDD bandwidth. Defendants are mostly male (77%) and are around 34 years old on average. 47% of defendants are either Non-Hispanic Black or Hispanic and 44% are Non-Hispanic White, with the remaining defendants missing race and/or ethnicity information. Most defendants have a criminal history—only 45% of our sample are first-time defendants. The average defendant has been previously charged with a criminal offense 3.9 times and has 2.6 prior convictions. Most defendants in our sample (71%) had at least one quarter of

[25] We use a smaller RD bandwidth of 120 days for this exercise. This is because using our baseline 270-day bandwidth for this exercise mechanically induces a type of selection in which quick-to-resolve cases are near the cut-off and cases that typically take long to resolve are to the right of the cut-off. In fact, the standard deviation of the case duration for cases filed and disposed before the reforms is around 477 days, while the analogous standard deviation for cases filed before the reform but disposed after is 924 days.

Table 3: Descriptive statistics and tests for covariate smoothness across discontinuity

Baseline Characteristic	Overall Mean	RD Estimate	Control Mean
	(1)	(2)	(3)
Demographics			
Age	33.680	0.171	33.880
		(0.274)	
Male	0.768	−0.000	0.769
		(0.010)	
White (Non-Hispanic)	0.443	−0.025**	0.439
		(0.011)	
Black (Non-Hispanic)	0.294	0.017*	0.299
		(0.009)	
Hispanic	0.181	−0.001	0.181
		(0.008)	
Missing Ethnicity	0.040	0.003	0.040
		(0.004)	
Missing Race	0.049	0.003	0.050
		(0.004)	
Criminal and Labor Market History			
First-time Defendant	0.450	0.007	0.430
		(0.011)	
Number of Previous Charges	3.891	−0.345	4.043
		(0.290)	
Number of Previous Convictions	2.635	−0.150	2.720
		(0.159)	
Any Employment in Previous Year	0.708	−0.001	0.707
		(0.012)	
Total Earnings in Previous Year	15,590	−93	15,170
		(681)	
Case Characteristics			
Number of Charges Filed		0.033	1.531
		(0.020)	
Violent Charge	0.101	0.023***	0.094
		(0.007)	
Drug Charge	0.129	0.001	0.133
		(0.007)	
Property Charge	0.216	−0.014	0.220
		(0.010)	
Criminal Traffic Charge	0.290	0.007	0.290
		(0.010)	
Public Order Charge	0.397	0.030***	0.390
		(0.011)	
Case Outcomes + Future Criminal Involvement			
Convicted	0.769		
Incarceration | Convicted	0.631		
Probation | Convicted	0.285		
Fine | Convicted	0.333		
New Criminal Charge within 3 Years	0.356		
New Incarceration Spell within 3 Years	0.176		
Sample Sizes			
N, CJARS	46,000		
N, LEHD	36,500		

Note: First-time defendants are defendants not previously charged with any offense in the state. 'Control Mean' represents the average value of the variable to the left of the cut-off. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890, CBDRB-FY26-P2957-R13065, CBDRB-FY26-P2957-R13295.

employment in the year before disposition, earning around $16,000 in the prior year, including zeros.[26]

Moving to case characteristics and outcomes, defendants on average faced 1.53 charges in the case that is disposed within the RDD bandwidth. While approximately 32% of these cases involved violent or property charges, nearly 70% of the cases involved relatively minor criminal traffic or public order charges. Conviction rates in our sample are generally high, with 77% of cases ending up with at least one charge convicted. Conditional on conviction, sentences involve one or more of: jail spells (63%), probation (29%), or fines (33%). While jail spells are the most common in this sample, spells are usually capped at one year and are typically shorter. Recidivism rates after disposition are relatively high, with 36% of our sample ending up with a new criminal charge (either a misdemeanor or a felony) and 18% of the sample ending up in jail or prison within three years after disposition.

5.3 Impact of Reforms on Outcomes of Defendants and Their Families

Direct impacts on defendants

Having established that the budget cuts and priority changes that form our identifying variation represent valid RDDs, we now turn to estimating how the reforms affected defendants' socioeconomic outcomes and assessing the causal impact of misdemeanor conviction. We first verify that the policy reforms we study actually changed defendants' conviction status. As Figure 4 shows, the reforms had a large first-stage effect—individuals whose cases were disposed just after the cut-off date were 10 p.p. less likely to be convicted (12% of the pre-period mean).[27]

While the reforms reduced the likelihood of conviction, Figure 5 reveals that having a case disposed just after the reforms had muted impacts on recidivism, employment, earnings, and poverty status three years after disposition. While each of these estimates is insignificant, the confidence intervals are generally tight. The estimates in Panel a) rule out reform-induced changes larger than –9.6% and 1.7% in three-year recidivism, indicating that marginally reducing the scope of the misdemeanor system is unlikely to encourage defendants to re-offend, dampening a key concern that opponents of misdemeanor reform have.[28, 29] Similarly, the estimates in Panels b)–d) can rule out changes in average annual employment larger than –1.8% and 3.1%, changes in average annual earnings greater than –3.2% and 7.5%, and changes in average annual poverty status between –3.6% and 2.0%.[30]

[26] Column 3 of Table A1 compares the RD sample to the average misdemeanor defendant across the 11 states with adequate CJARS and LEHD coverage used in Section 4. The RD sample resembles the broader sample across most dimensions, except defendants in the RD sample are less likely to be White or first-time defendants. Individuals in the RD sample also have better labor market history and are more likely to have been charged with property, criminal traffic, and public order offenses.

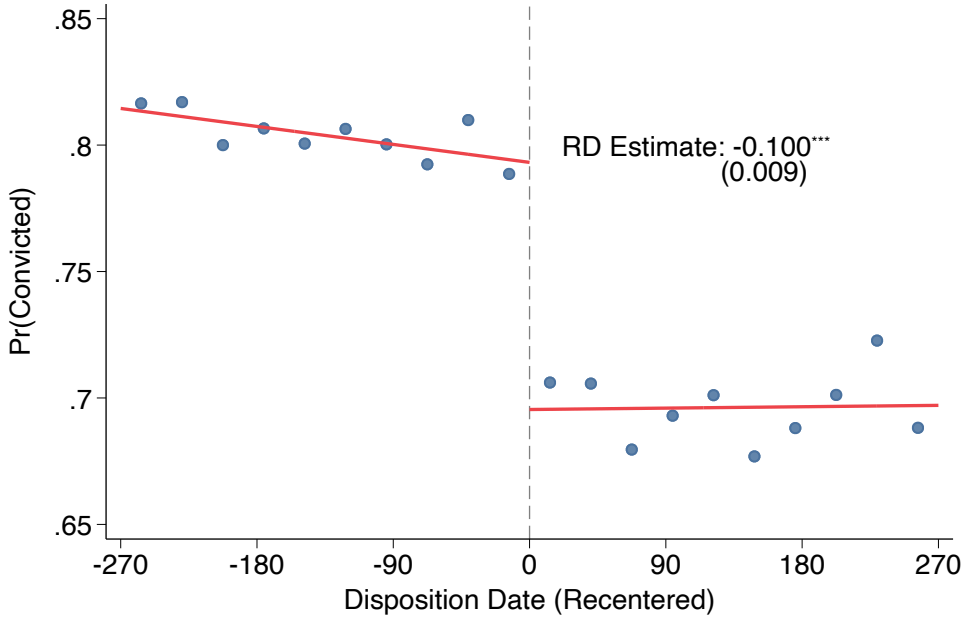
[27] Figure A9 documents reductions of similar sizes across different demographic subgroups, suggesting that defiers are unlikely to be present in our setting.

[28] Panel a) of Figure A2 shows similarly muted impacts on the number of new charges filed.

[29] We explicitly address the idea that the reforms may have encouraged criminal activity among those not in our RDD sample when discussing the MVPF.

[30] The local average treatment effects (LATE) of conviction implied by the estimates in Figure 5 are larger than

Figure 4: First stage: Impact of policy reform on probability of conviction



Note: This figure presents the RDD estimate for the likelihood of conviction for any charge using the stacked sample and Equation 2. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890.

Figure 6 illustrates how the impacts of the reforms on our main outcomes evolve over time, plotting RDD estimates for cumulative outcomes over varying time horizons. We present these estimates for up to four years after disposition, imputing zeros in year four for individuals in counties where we only have three years of follow-up data for a particular outcome.[31] We measure recidivism outcomes in six-month increments and labor market outcomes in annual increments. In Panel a), we find that the reforms reduce recidivism in the six months after disposition, but the effect is smaller and less significant as time goes on, moving from –1.8 p.p. ($p = 0.024$) in the first six months to –1.3 p.p. ($p = 0.24$) after four years (see Table A5 for the underlying estimates).[32] In Panel b) we see a similar pattern of larger (albeit insignificant) point estimates in the short run. The impact on first year employment is 1.5 p.p. ($p = 0.11$), which shrinks to just 0.4 p.p. ($p = 0.64$) after four years (see Table A6 for more details).

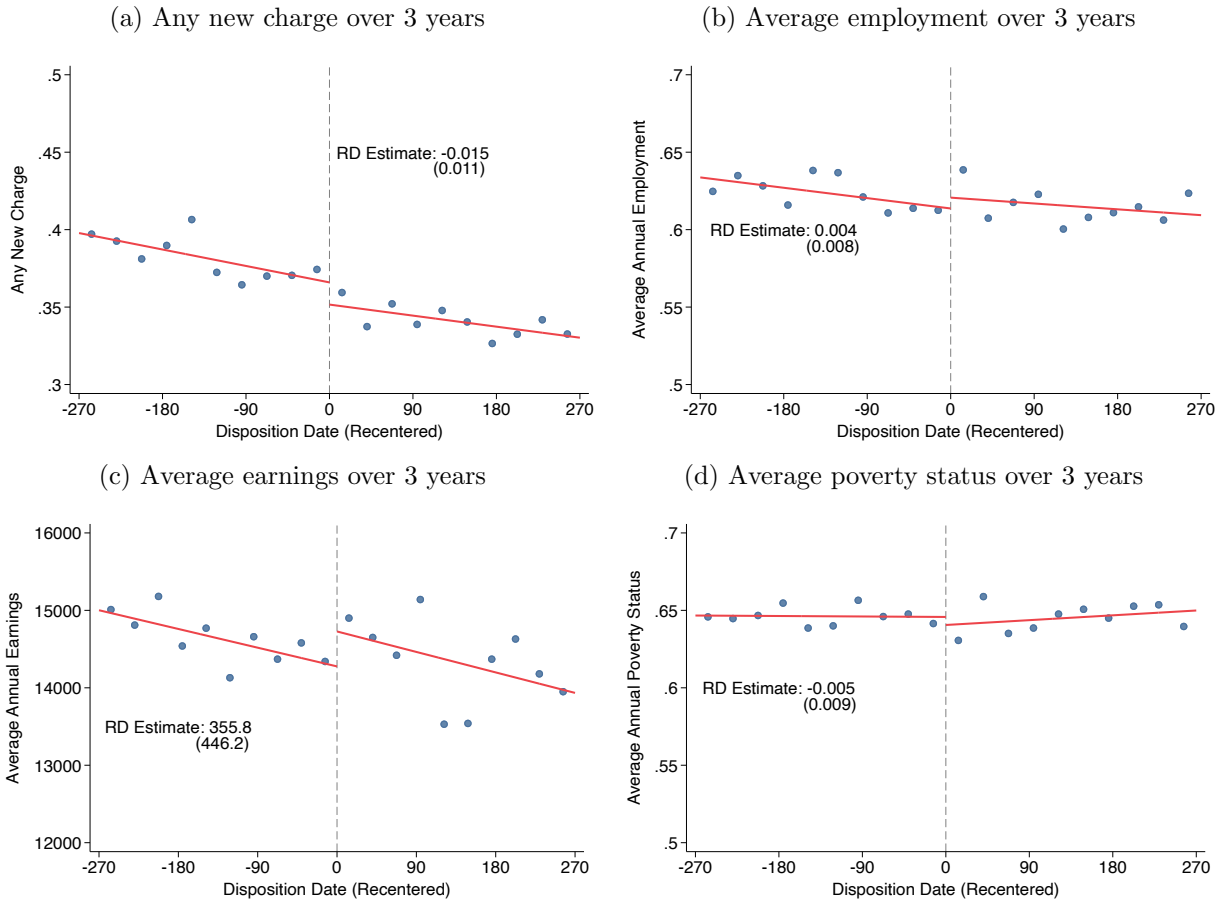
The dynamics for earnings (Panel c)) and poverty status (Panel d)) exhibit a different pattern. The impact on total earnings increases from \$377 ($p = 0.50$) to \$1,153 ($p = 0.51$) over the four

the ITTs (as a proportion of average outcome for compliers if dismissed, see Table A4 for details). Scaling up the ITTs by the magnitude of the first-stage implies that misdemeanor conviction increases the likelihood of recidivism by 60%, reduces average annual employment by 8%, reduces average annual earnings by 28%, and increases the average annual poverty rate by 7%, although we cannot reject that any of these LATEs are equal to zero.

[31] This imputation only occurs for recidivism beyond three years for defendants in the Wisconsin counties because the available court records for Wisconsin end in 2019.

[32] The number of new charges filed falls by 0.037 after six months ($p = 0.11$) and falls by 0.054 ($p = 0.48$) after four years, although the latter is much less precise (see Panel b) of Figure A2).

Figure 5: Impact of policy reforms on defendant crime and labor market outcomes



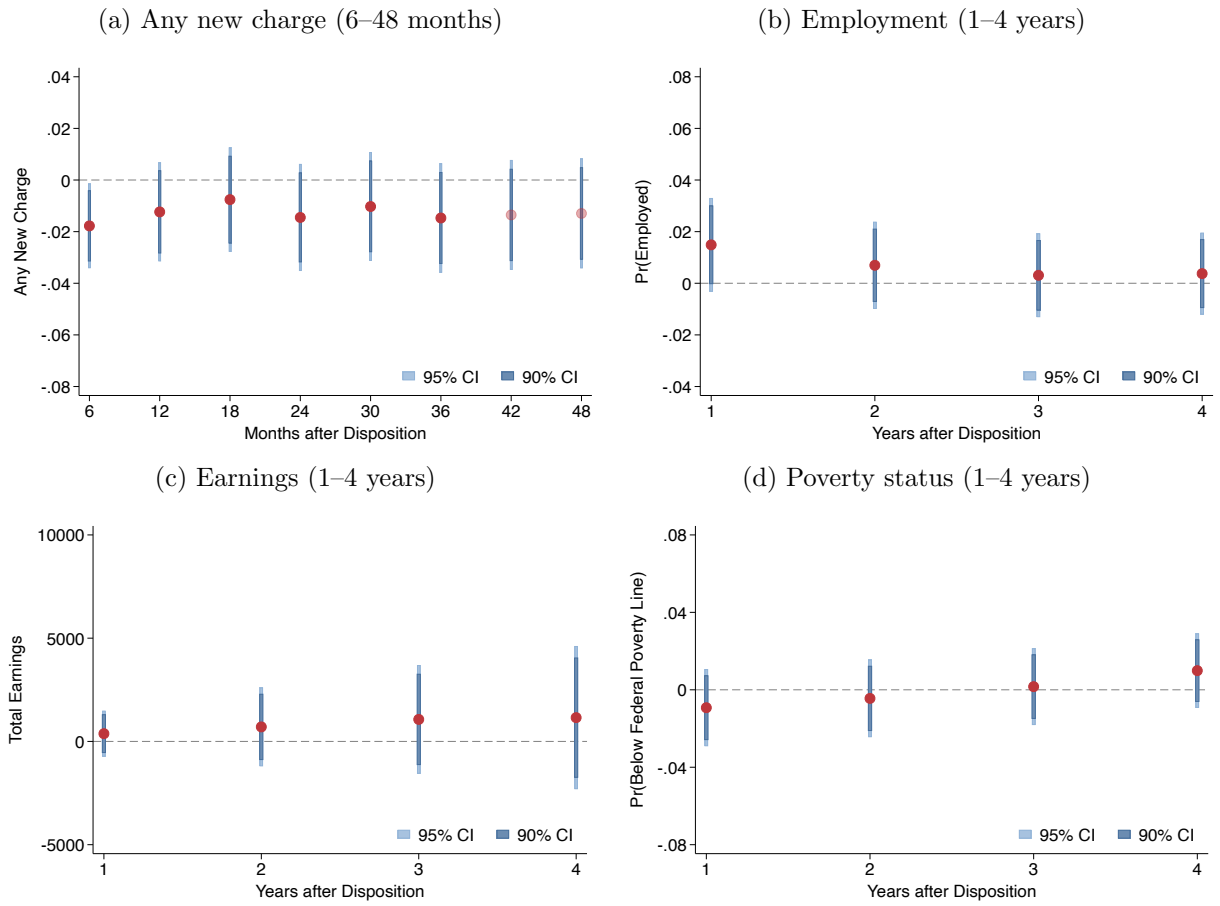
Note: Each Panel presents RDD estimates using the stacked sample and Equation 2. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890.

years, while the reform reduces the likelihood of being below the federal poverty line by 0.9 p.p. ($p = 0.36$) in the first year and *increases* it by 1.0 p.p. ($p = 0.31$) after four years. However, the cumulative estimates for earnings and poverty status are much less precise, even in the short run, than the estimates for recidivism or employment.

Next, we use self-reported survey responses from the 2008–2019 waves of the American Community Survey (ACS) to understand if the reforms affected defendants on dimensions that are not well-captured in our administrative data, e.g., safety net usage. Since the ACS is not a panel, we pool ACS responses over multiple years and estimate Equation 2, controlling for the response year. The resulting estimates can be interpreted as average impacts over the three years after disposition. Given the low response rates and imperfect overlap between the ACS sampling frame and CJARS population, the sample sizes for these analyses are much smaller than in our analyses using administrative data ($N \approx 1,200$).

Since the ACS population weights are not designed to be representative for individuals involved in the criminal legal system, we follow the reweighting strategy used in Finlay et al. (2024). We

Figure 6: Impact dynamics (cumulative): Recidivism, employment, earnings, poverty status



Note: Each Panel presents RDD estimates using the stacked sample and Equation 2. Hollow dots indicate outcomes for which missing follow up information is imputed with zeros. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890.

first use covariates to predict the likelihood that individuals in our data are also in the ACS using the same prediction specification used for the predicted outcomes discussed in Figure 3. We then weight observations in our ACS analysis sample by the inverse of the interaction between these predicted weights and the number of observations per experiment that we use in the rest of the analyses. To test whether the characteristics of our ACS sample are smooth around the cut-off, Panel A of Table A7 presents estimates from applying Equation 2 to the predicted ACS-CJARS weights we construct, $\frac{1}{Pr(\text{In ACS})}$. We find a slight discontinuous increase in the constructed weights, suggesting that defendants to the right of the cut-off are marginally less likely than those to the left of the cut-off to have responded to the ACS. Panel A also provides an estimate of the first-stage for the subset of our main sample that responded to the ACS. We find evidence of a strong first-stage here, with the likelihood of conviction falling by 17.6 p.p. (21.8%) for such defendants.

Turning to self-reported outcomes, we find imprecise but suggestive evidence that the reforms improved labor market outcomes, reduced reliance on the safety net, and improved mental well-

being. The estimates in Panel B of Table A7 suggest that the reforms increased employment and earnings (including earnings from self-employment) and reduced the likelihood of being in poverty.[33] Panel C of Table A7 reports the ITT estimates on health insurance and safety net usage, as well as mental well-being. These estimates suggest that after the reforms, defendants are more likely to have health insurance (whether publicly-provided or through an employer/union) and are less likely to receive any welfare benefits or Supplemental Nutrition Assistance Program benefits, consistent with improvements in their overall labor market prospects.[34] Finally, our estimates suggest that the reforms reduced the likelihood of reporting a mental disability, a proxy for mental well-being, although this is imprecisely estimated.[35]

Impacts on family members

Moving beyond the focal defendants, we also estimate the impacts that the reforms had on defendants' spouses and romantic partners. While we can observe outcomes for children in the ACS (e.g., school enrollment and mental well-being), the number of children whose families responded to the ACS and who have a family member in our RDD sample is quite small. Given this, we limit our current discussion of spillovers to defendants' spouses and romantic partners, but will assess impacts on children's outcomes measured in administrative data when they grow older.[36]

In Table A8, we find that we can identify a partner for 37% of our focal sample, with defendants to the right of the cut-off being marginally more likely to have a linked partner (1.9 p.p., or 5% of the baseline mean). Consistent with the full-sample first-stage, defendants with partners are 9.4 p.p. less likely to be convicted due to the reform. However, we see insignificant impacts of the reforms on partners across all of our main outcomes. The labor market outcomes for partners are in line with what we observe for the focal defendants while recidivism among partners exhibits a small increase. The ITTs on partners' labor market outcomes are relatively precise, e.g., 95% confidence intervals rule out changes in employment rates greater than –2% to 5%.

Robustness

Our results are robust to different specification choices. Figure A3 presents how the estimates for the first stage and main outcomes for focal defendants change if we: 1) do not equally-weight the natural experiments, 2) cluster standard errors at the level of the running variable, 3) do not control for baseline covariates, and 4) restrict all the natural experiments to have the same relationship between the running variable and the outcomes. Estimates are similar across the first three specifications. Imposing common slopes across natural experiments results in estimates

[33] Reassuringly, the direction of the ITTs on employment and earnings here is the same as our estimates from administrative data in Figure 5. This suggests that labor market outcomes may also have improved for individuals in the Florida natural experiment, for which we have ACS data but not administrative LEHD data.

[34] The estimate for total welfare income is positive but imprecise.

[35] Reported mental disability is measured using responses to questions about whether one had difficulty concentrating or remembering due to having a mental, physical, or emotional condition lasting more than 6 months. Prior work (e.g., Finlay, Mueller-Smith, and Street 2023) demonstrates that this measure is correlated with other markers of socioeconomic well-being.

[36] In fact, we do not find a significant first-stage for parents of this small sample. As the children of defendants grow older, their outcomes will be observable in our administrative data, yielding larger analysis samples. Future work will assess the impacts on these children's labor market and crime outcomes.

that are much larger, significant, and sometimes have the opposite sign. We interpret this as evidence that the final specification is misspecified due to this strong homogeneity assumption. While each natural experiment may or may not exhibit a discontinuous shift in outcomes due to the policy reform, they may all be following different trends. As a result, imposing a common trend across heterogeneous settings is an inflexible specification that yields misleading evidence of a discontinuous change when there truly is none.

To assess the sensitivity of our results to the choice of bandwidth, Figure A4 and Figure A5 present RDD estimates using alternative bandwidths from 120–360 days in increments of 30 days. The takeaways of our validation exercises and the sign and magnitude of our first-stage and reduced form estimates are similar across these alternative bandwidth choices. In addition, Table A3 shows that our results are robust to excluding cases that are filed after the reforms, thereby excluding any potential impacts that the reforms may have had on whether criminal offenses make it to the disposition stage.

Interpreting the impacts of the reforms

Taken together, our estimates suggest that the budget reforms and changes in District Attorney priorities led misdemeanor defendants to have fewer interactions with the criminal legal system and greater attachment to the formal labor market in the short run, but that these improvements did not persist over time. Estimates using survey measures suggest similar improvements in labor market outcomes and reduced safety net usage, but these estimates are generally quite imprecise and cannot rule out muted impacts on these socioeconomic outcomes.

Since having charges filed generates a criminal record that is observable to some employers in each of our settings, one candidate explanation for our muted estimates is that the marginal impact of a conviction is zero once a charge is observable. We think this explanation is unlikely—evidence suggests that convictions **do** matter above and beyond a charge. Surveys of employers suggest that they are less willing to hire workers with convicted rather than dismissed misdemeanor charges (Agan et al. 2024*b*) and an analysis of job applications found that employers almost never asked applicants to self-report non-convictions (Vuolo, Lageson, and Uggen 2017). This implies that a misdemeanor conviction record should affect labor market prospects for employees at/applicants to some firms. Event study evidence from Section 4 suggests that a conviction is associated with a greater labor market penalty than being charged and that this gap varies considerably across states.[37] Prior work in other contexts shows that conviction can adversely impact outcomes, especially for individuals with short criminal histories. Mueller-Smith and Schnepel (2021) find persistent reductions in the total number of future convictions and increases in employment and earnings from diversion for first-time felony defendants even ten years after disposition. Similarly, Kamat, Norris, and Pecenco (2023) find that avoiding conviction reduces the total number of future charges and convictions filed against misdemeanor defendants with no prior felony record.

[37] As Table A9 shows, a relatively larger labor market penalty associated with conviction rather than a charge is also the case in the counties used in the RD analyses. In fact, in these counties, the penalty associated with a conviction is 40–120% greater than that for a charge, a greater differential than we see in the states included in Section 4, further underscoring the substantial heterogeneity in criminal record penalties across the U.S.

A more plausible reason for the estimates we find is that the policy reforms that we study were not targeted toward individuals who were most likely to be impacted by a misdemeanor conviction. Faced with large and sudden changes in resources, decision-makers in our settings likely dismissed cases that they no longer had the resources to prosecute.[38] As a result, they may not have had the capacity to target dismissal toward individuals who might benefit the most. In contrast, decision-makers in the contexts of diversion (Mueller-Smith and Schnepel 2021) or non-prosecution (Agan, Doleac, and Harvey 2023) may have more intentionally targeted treatment to particular types of individuals.

5.4 Compliers, Heterogeneity, and Targeting

We investigate the plausibility of the targeting mechanism by first characterizing the compliers of the reform. We then leverage the geographical and contextual variation across the reforms to understand the correlation between characteristics of the marginal defendant and treatment effect magnitudes.[39] We use these analyses to identify observable 'tags' (Akerlof 1978) for individuals most responsive to a misdemeanor conviction. These analyses also let us extrapolate our estimates to other settings and understand how much any cross-setting differences in treatment effects are due to differences in complier characteristics.[40]

Characterizing compliers and heterogeneity

As discussed above, one potential reason for the muted estimates in Figure 5 is that the policy reforms may have affected the defendants least likely to be impacted by a misdemeanor conviction. To evaluate this explanation, we estimate the average potential outcomes and baseline characteristics for compliers in our sample, following Imbens and Rubin (1997), and compare these characteristics to those of the average defendant. In our setting, compliers are individuals who would have been convicted if their case was disposed before the reform but would not have been convicted if their case was disposed after. Put differently, compliers are defendants who are shifted from conviction to dismissal and are at the *margin* of conviction and dismissal. Table 4 compares the characteristics of the average defendant (Column 1) and compliers (Column 2). Column 3 displays the ratio of the characteristics of compliers to those of the average defendant, which quantifies how compliers are selected relative to the general sample.

The patterns in Table 4 suggest that compared to the average defendant, compliers are at the lower end of the recidivism 'risk' distribution but less likely to be adversely impacted by a misdemeanor conviction. In Panel A, we see that compliers are 38% more likely than the average defendant to be first-time defendants, i.e. with no criminal history. Compliers are also overrepresented among the oldest age group in the sample (43 and over), a group that is well past the peak of

[38] Indeed, Table A4 shows that the reforms increased the average case length by 41 days (17% of the pre-reform mean), suggesting that the cases dismissed due to the reform were likely old, pending cases.

[39] Our estimates of compliers and the local average treatment effects will be biased if defendants shift across multiple treatment states, e.g., diversion. Appendix C.1 discusses what our estimates recover in such situations, characterizes the resulting bias, and discusses conditions under which our estimates are still valid.

[40] This exercise is valid under a strong assumption that alternative settings share the same marginal treatment effect curve as the contexts in our RDD analysis.

Table 4: Comparing compliers to overall sample

Variable	Baseline Mean ($\bar{X}$)	Control Complier Mean (CCM)	$100 \times \frac{CCM}{\bar{X}}(\%)$
	(1)	(2)	(3)
Panel A: Demographics and Outcome History			
Male	0.768	0.723	94.1
Age: 18-24	0.263	0.197	74.9
Age: 25-31	0.265	0.213	80.1
Age: 32-42	0.243	0.235	96.8
Age: 43 and over	0.229	0.355	155.4
First-time Defendant	0.450	0.621	137.9
Any Employment in Previous Year	0.708	0.594	84.0
Total Earnings in Previous Year	15,590	12,440	79.8
Panel B: Case Characteristics			
Violent Charge	0.101	0.042	41.8
Drug Charge	0.129	0.000	0.0
Property Charge	0.216	0.053	24.5
DUI Charge	0.143	0.278	194.8
Criminal Traffic Charge	0.290	0.444	153.1
Public Order Charge	0.397	0.656	165.2
Panel C: Outcomes			
Any new charge (3 years)	0.381	0.250	65.5
Number of new charges (3 years)	1.308	0.586	44.8
Average employment (3 years)	0.647	0.497	76.9
Average earnings (3 years)	16,300	12,910	79.2
Average poverty status (3 years)	0.625	0.699	111.7

Note: First-time defendants are defendants not previously charged with any offense in the state. 'Baseline Mean' presents the mean in the sample for Panels A and B but the mean to the left of the cut-off for Panel C. In Panels A and B, 'Control Complier Mean' represents the average value of the baseline characteristic among untreated compliers. For Panel C, 'Control Complier Mean' represents the average untreated potential outcome among compliers, where treatment is conviction. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890, CBDRB-FY26-P2957-R13065, CBDRB-FY26-P2957-R13295.

the age-crime profile. While both groups typically have low 'risk' of recidivism, younger defendants tend to benefit more from avoiding a conviction record, especially in the context of felony diversion (e.g., Mueller-Smith and Schnepel 2021; Augustine et al. 2022).

Compliers in our setting are also negatively selected relative to the average defendant in terms of formal labor market attachment. In the year before disposition, nearly 60% of compliers were employed, earning $12,440, which represents a 16% lower employment rate and 20% lower annual earnings (see Panel A). Formal labor market prospects and attachment may be poor enough for these individuals such that a misdemeanor conviction may not reduce employment and earnings due to a potential 'floor' effect. Evidence of such a pattern exists in other contexts—a key reason for the muted impacts of incarceration on employment and earnings is that incarcerated individuals tend to be detached from the formal labor market to begin with (Garin et al. 2023).

Turning to charge characteristics (Panel B), compliers are approximately 50–65% more likely to be charged with criminal traffic and public order offenses, the most minor types of misdemeanor crimes, and are 60–75% less likely to be charged with violent or property offenses. Given that employers tend to be most wary of individuals with records for violent or property crimes, a marginal criminal traffic or public order conviction is unlikely to generate large negative employment consequences (e.g., Holzer, Raphael, and Stoll 2007; Agan et al. 2024*b*).

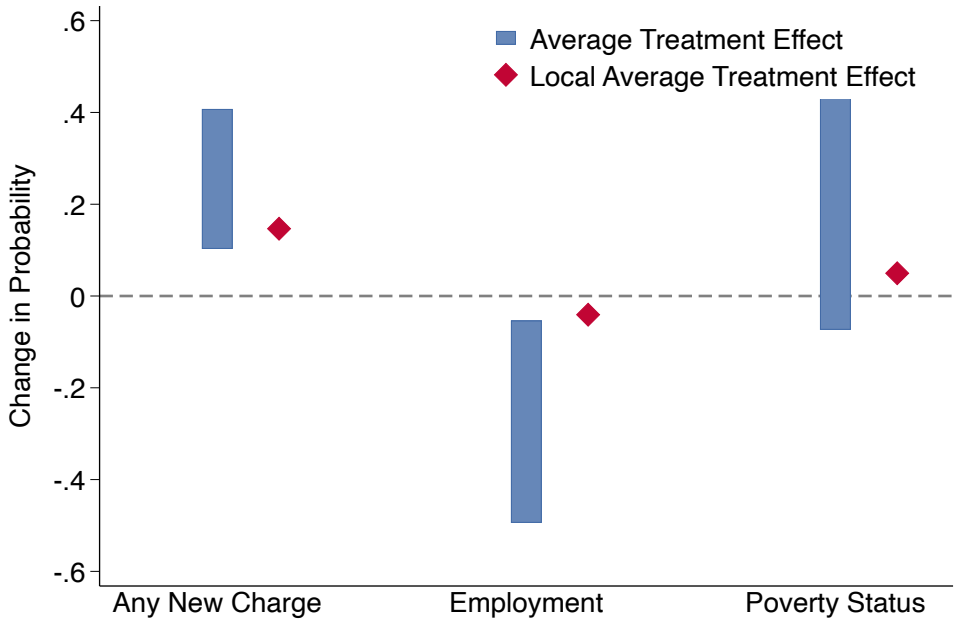
Together, these selection patterns suggest that decision-makers targeted individuals with low potential recidivism and labor market outcomes, but not necessarily large treatment effects.[41] Panel C investigates how defendant potential outcomes if not convicted ($Y_i(0)$) vary for compliers relative to the overall sample mean. Compliers are 35% less likely than the average defendant to have a new charge in the three years following disposition, if not convicted, and would have 55% fewer new charges. If not convicted, compliers would have a 23% lower employment rate, 21% lower earnings, and a 12% greater poverty rate over the same time horizon, compared to the average defendant.

Figure 7 furthers this point by comparing the local average treatment effect (LATE) for compliers affected by these reforms to the average treatment effect (ATE) across all defendants in our analysis sample. We estimate bounds for the ATE by casting the fuzzy RD design into a binary instrumental variable framework. This framework, along with an assumption that potential outcomes are weakly monotonic in the likelihood of conviction, identifies point estimates and bounds for average potential outcomes for always takers, compliers, and never takers, which we use to bound the ATE (Mogstad, Santos, and Torgovitsky 2018; Kowalski 2023).[42] Across each of these

[41] Column 5 of Table A1 highlights that RD compliers in these settings are starkly different from the broader sample of misdemeanor defendants from 11 states studied in Section 4. In particular, RD compliers are 54% more likely to be in the oldest age quartile (at least 43 years old), 18% less likely to be first-time defendants, and 74–156% more likely to be charged with a DUI, criminal traffic, or public order offense. Interestingly, complier employment and earnings in the prior year are only 6–8% lower than those of the average misdemeanor defendant.

[42] The bounding approach we use requires that the outcome variables are bounded. Hence, we focus on any new charge, average employment, and average poverty status, since these outcomes all must lie between zero and one. In contrast, there is no clear upper bound for earnings. See Figure A6 for the estimated average treated potential outcomes (for always takers and compliers), average untreated potential outcomes (for compliers and never takers) as well the range of the bounds for the unidentified averages (untreated potential outcomes for always takers and treated potential outcomes for never takers).

Figure 7: Comparing the treatment effects for compliers to the average treatment effects (ATE)



Note: This figure displays estimated treatment effects of conviction on any new charge, employment, and poverty status over three years. The diamonds represent the local average treatment effect for compliers, obtained by dividing the reduced form estimates in Figure 5 by the first-stage estimates in Figure 4. The bars in blue represent estimated bounds for the average treatment effect, estimated by extrapolating average potential convicted and dismissed outcomes beyond compliers under behavioral assumptions on selection into conviction, following Mogstad, Santos, and Torgovitsky (2018); Kowalski (2023). See Figure A6 for the components that are estimated using the data and the components that are bounded by the behavioral assumptions. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890, CBDRB-FY26-P2957-R13295.

outcomes, the LATE is consistently at the lower end or outside of the ATE bounds, providing suggestive evidence that compliers in our setting are individuals who would experience lower treatment effects than the average defendant.[43] This is in line with other interventions where outcomes do not meaningfully change when individuals are selected on untreated potential outcomes, rather than selected on gains (e.g., Bhatt et al. 2024).

Using cross-reform variation to understand targeting

Our results thus far document that the reforms were not targeted to those who would benefit the most, on average. Moving beyond the average, the natural experiments could vary in targeting quality, especially since each reform reduced misdemeanor conviction for different reasons (see Section 2). Similar to the cross-site analyses of the Moving to Opportunity program (Kling, Liebman, and Katz 2007), we use variation in complier characteristics, which provide information on whom the reforms affected, and treatment effects across natural experiments to identify observable 'tags' for people most likely to be affected by a misdemeanor conviction.

[43]The presence of defiers is unlikely to be driving the low LATEs in our setting since the first-stage reduction in the likelihood of conviction is the same sign and similar magnitude across different demographic subgroups (see Figure A9).

We do this by estimating the following for each natural experiment: the control complier mean (i.e., the average untreated potential outcome), average baseline characteristics of compliers (criminal history and age), and the local average treatment effect (LATE) of conviction.[44] We then estimate the bivariate regression in Equation 3, where ϕ_x provides a measure of how correlated a particular characteristic, e.g., criminal history, is with the impact of conviction. The idea behind including the control complier mean (CCM) in this exercise is that since settings can vary in terms of multiple dimensions, the CCM provides a summary measure of the marginal defendants' recidivism 'risk' or labor market potential. These CCMs capture many factors of defendants in a particular setting, including the local labor markets that compliers have access to. We assess the share of compliers that are first-time defendants and 31 years old or younger since our discussion of the literature above highlighted people with these characteristics as potentially responsive to criminal legal interactions.

$$\widehat{\beta}_{e,x}^{LATE} = \alpha_x + \phi_x \widehat{\text{CCM}}_{e,x} + \epsilon_{e,x} \tag{3}$$

This framework is useful for two reasons. First, the implications of each ϕ_x provide suggestive evidence on the types of people most likely to be affected by a misdemeanor conviction. Second, combining our estimates of ϕ_x with complier characteristics from settings outside of our sample provides an estimate of the impact of conviction in those settings, under strong assumptions of homogeneous marginal treatment effect functions across settings. As a result, Equation 3 can provide a guide to understand how much of cross-setting differences in treatment effects are due to differences in complier characteristics. Figure 8 uses the intercepts and slopes from Equation 3 to plot the implied linear relationship between each of these complier characteristics and the LATEs for conviction on the probability of recidivism and average annual earnings over three years.[45] In each figure, the solid vertical lines denote the mean value of the complier characteristics across our settings and the dashed lines represent values that are one standard deviation above or below this mean, indicating the variability in complier characteristics across our settings.

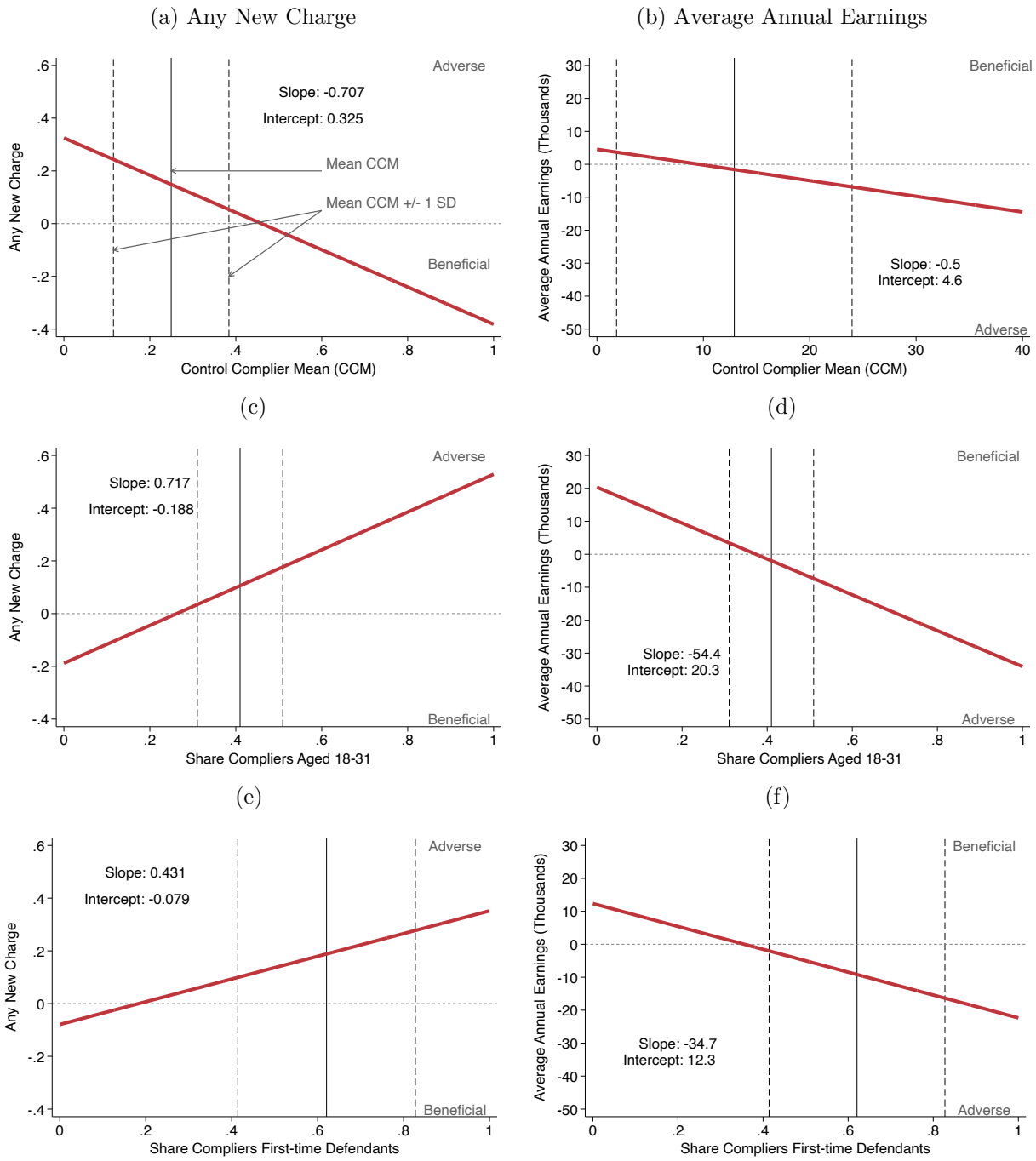
Focusing on Panels a) and b), we see suggestive evidence that the impacts of a misdemeanor conviction depend on the average potential outcomes of the marginal defendant. The adverse impact of conviction on future recidivism is decreasing in the likelihood that compliers, if not convicted, would have charges filed against them in the future (Panel a). This pattern is consistent with the large body of evidence documenting that criminal legal interactions harm defendants with shorter criminal histories. In Panel b) we see that earnings reductions due to conviction are greater when compliers would have higher potential labor market earnings if not convicted.

Moving beyond the average potential outcomes of marginal defendants, we next explore which observable characteristics predict treatment effects. Such an exercise is especially useful since policymakers in criminal misdemeanor systems can make decisions based on defendant characteristics

[44] We use the LATE rather than the ITT so that estimates of a misdemeanor conviction are comparable across settings.

[45] Table A10 presents the slope and intercept coefficients along with their standard errors, which are heteroskedasticity-robust, but do not account for noise in the estimated LATEs or complier characteristics.

Figure 8: Relationship between the local average treatment effect (LATE) of conviction and characteristics of the marginal defendant



Note: This figure plots predicted relationships between the local average treatment effect (y-axis) and complier characteristics (x-axis) in each reform, estimated using Equation 3. The solid vertical line represents the mean complier characteristic across the reforms (from Table 4) while the dashed vertical lines represent values that are one standard deviation above or below the mean. We are not able to plot the estimates from each individual natural experiment due to disclosure avoidance rules particular to the LEHD. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13065, CBDRB-FY26-P2957-R13295.

which are directly observable, while potential outcomes are not. Panels c) and d) of Figure 8 trace out the relationship between the share of compliers that are young (between 18–31 years old) and the LATE of conviction on recidivism and earnings. We see that having a greater share of young compliers is associated with harmful impacts on both dimensions. Finally, Panels e) and f) document that a larger share of compliers who are first-time defendants, i.e., have no criminal history, is associated with increased recidivism and lower labor market earnings.

Interestingly, the implied relationship between complier characteristics and LATEs crosses zero in all of these graphs, suggesting that there are scenarios in which misdemeanor convictions are weakly beneficial for defendants.[46] While we cannot rule out beneficial effects, the harmful impacts of conviction are highly pronounced for the first-time and young defendants, suggesting that alternative reforms that target non-conviction to these groups would be beneficial.

Evidence from classic subgroup analyses is largely consistent with the patterns in Figure 8. Figure A9 presents impacts of the reform on conviction rates as well as our main outcomes for first-time defendants, defendants 31 or younger (median age), and defendants with above median predicted earnings and their complements.[47] Panel a) documents that the first-stage is consistently negative across these subgroups, but larger for defendants aged 32 or older, consistent with the takeaways from Table 4. Panel b) shows that the reforms' recidivism impacts are concentrated among first-time and younger defendants. The likelihood of being charged with a new offense in the next three years significantly falls by 2.6 p.p. (9.6% of the baseline mean) for first-time defendants and by 3.2 p.p. (7.6%) for defendants between the ages of 18 and 31. In contrast, the impacts on three-year recidivism for repeat offenders and defendants older than 32 are –0.5 p.p. (1.1%) and 0.2 p.p. (0.6%) respectively.[48] Panels c) through e) of Figure A9 provide suggestive evidence that the reforms improve labor market outcomes for individuals with better labor market prospects, with mixed findings for first-time and young defendants. For instance, the reforms increased average annual employment and earnings for defendants with above median predicted earnings by 1.2 p.p. (1.4% of the baseline mean) and by $787 (2.9%) respectively. In contrast, average annual employment and earnings for defendants with below median predicted earnings fell by 0.7 p.p. (1.7%) and $102 (2.7%) respectively. We see muted labor market impacts for other subgroups.

[46] Figure A7 extrapolates what the LATEs would be in other studies examining the impact of a criminal record for low-level defendants, in particular non-violent misdemeanor defendants in Suffolk County, Massachusetts (Agan, Doleac, and Harvey 2023) and low-risk felony defendants in Harris County, Texas (Mueller-Smith and Schnepel 2021). We estimate complier characteristics in each alternative setting and use the relationships in Figure 8 to predict the LATE in that setting. Compliers in all other settings have lower potential recidivism, higher potential earnings, and are more likely to be young or first-time defendants than our compliers. This potentially rationalizes the large treatment effects from those contexts, abstracting away from treatment differences (e.g., diversion, non-prosecution). This exercise also lets us extrapolate earnings impacts for studies that did not have access to labor market outcomes. Our estimated relationships suggest that conviction may have generated substantial earnings losses for marginal misdemeanor defendants in Suffolk County.

[47] Table A11 and Table A12 present RDD estimates for other subgroups defined by gender and interactions between first-time defendant and being above or below the median age.

[48] Table A11 shows that these recidivism impacts are largest for first-time defendants who are also young—for this subgroup, the reforms reduce the likelihood of a new charge within three years by 4 p.p. (12%).

Taken together, these patterns highlight how understanding the characteristics of the marginal defendant is crucial to forecast the effects of changes in misdemeanor conviction policy. Our results suggest that misdemeanor conviction generates future criminal legal contact for defendants with relatively low 'risk' of recidivism. Having no criminal history and being a young defendant are observable 'tags' for such defendants. Our results also provide suggestive evidence that misdemeanor conviction harms earnings of such defendants as well as those with greater potential earnings.

6 MVPF and Discussion

Is misdemeanor conviction for marginal cases a socially beneficial policy? To answer this question we cast our causal estimates of misdemeanor conviction (the LATEs) into a Marginal Value of Public Funds (MVPF) framework (Hendren and Sprung-Keyser 2020; Finkelstein and Hendren 2020) which relates the benefits of the policy to the net government cost.[49] We construct our baseline MVPF estimate, which represents the average across all natural experiments, as:

$$\text{MVPF} = \frac{\text{Beneficiaries' willingness to pay}}{\text{Net cost to government}} \tag{4}$$

Our estimate of the MVPF uses our LATE estimates (re-scaling the ITT by the magnitude of the first-stage) of the effect of reforms that reduced misdemeanor conviction. We combine this with prior estimates of the social cost of crime (Miller, Cohen, and Wiersema 1996; Deshpande and Mueller-Smith 2022) and government costs of investigation and prosecution (Caulkins 2010; The Vermont Center for Justice Research 2014; Hunt, Anderson, and Saunders 2017; Hunt, Saunders, and Kilmer 2019), along with estimates of the value of public programs. We discuss each component of the MVPF in greater detail below.[50]

Willingness to Pay: Our estimate of willingness to pay includes the change in the social cost of future crime (–$17,432) adjusted for offense under-reporting, following Deshpande and Mueller-Smith (2022), and changes in defendant post-tax cumulative earnings over three years (–$9,578), assuming an income tax rate of ten percent. Additionally, we include changes to post-tax self-employment income (–$3,434), transfers from the Supplemental Nutrition Assistance Program (SNAP) ($739), estimates of non-SNAP welfare transfers (–$1,046), and lower Medicaid enrollment (–$282). Together, we estimate that the marginal misdemeanor conviction yields a willingness to pay of –$31,032. Nonetheless, misdemeanor conviction may discourage people other than the marginal defendant from additional criminal activity. Below, we assess how our MVPF estimates change as we include the value of such general deterrence effects in the willingness to pay.

[49] We abstract from spillover estimates such as romantic partner's labor supply and criminal behavior to be conservative. For administratively recorded outcomes, we convert ITTs to LATEs using the first-stage estimate from the full sample. For survey responses, we use the first-stage estimate from the subsample of individuals whom we connect to the ACS.

[50] Each of the components reported below is rounded to the nearest dollar, while the reported sums are computed on the unrounded values. As a result, the sums of the rounded components will not always align with the rounded sum.

Net Government Costs: Misdemeanor convictions generate a suite of costs for the government, including prosecution ($640) and investigative costs ($2,023).[51] There are also behavioral effects that affect the government's budget constraint due to administrative costs associated with changes in future recidivism ($4,570) and additional fiscal externalities from increases in SNAP ($739) and decreases in non-SNAP expenditures (–$1,046) and Medicaid (–$282). Similarly, because of the harmful labor market impacts of criminal legal system contact, misdemeanor convictions reduce income tax revenue from formal and self-employment sources, increasing net costs by $1,064 and $382 respectively, assuming a ten percent average income tax rate for both components. While the government may recoup some of these expenditures with additional fine revenue, we abstract from these margins given empirical evidence highlighting that many defendants do not remit these penalties to the government (Pager et al. 2022; Finlay et al. 2024). These components imply a net government cost of misdemeanor conviction of $8,090.

Estimated MVPF: Together, we calculate the MVPF of misdemeanor conviction as –3.8 when taking our point estimates as given, assuming that there are no general deterrence effects from the marginal conviction.[52,53] Given the statistical uncertainty with some of our results, we also construct an MVPF assuming all of our LATE point estimates are in fact zero. This exercise yields an MVPF of 0, implying that the marginal misdemeanor conviction incurs positive government costs for no detectable social benefit.

In Figure 9, we explore how our conclusions about the welfare costs of misdemeanor conviction vary over a range of potential general deterrence effects, where the blue circle at the bottom left denotes the MVPF estimate of –3.8 when there is no general deterrence effect. Using our baseline LATE estimates, we calculate that the marginal misdemeanor conviction would require $39,122 in general deterrence effects in order to restore the MVPF to unity. Assuming all of our LATE estimates are zero still requires $2,663 of general deterrence to offset government expenditures.[54] Put differently, this range implies that misdemeanor convictions are socially beneficial if they successfully deter individuals from committing at least one violent crime, between two and 23 property crimes, or between five and 70 criminal traffic offenses.

To gauge how plausible these deterrence magnitudes are, we take two approaches to quantify the "best case" general deterrence effect from a misdemeanor conviction. First, we estimate the impact of the reforms on the number of cases filed using our criminal court data (see Table A2, described in Section 5.2). Using the upper end of the 95 percent confidence interval, we estimate

[51] Our cost estimates account for differences in investigative and prosecution costs by crime type.

[52] Accordingly, the MVPF of *non*-conviction, which represents both a welfare gain and contraction of government expenditures, has an implied MVPF of infinity. By extension, policies which shrink the scope of the misdemeanor system also generate an infinite MVPF, assuming the potential utility losses associated with reduced employment for judicial agents are second-order.

[53] An alternative thought experiment would be to construct an MVPF of a policy that marginally reduces misdemeanor conviction for young or first-time defendants. Since the discussion in Section 5.4 suggests that adverse impacts from conviction are larger for these groups, the resulting MVPF should be more negative than our baseline MVPF estimate.

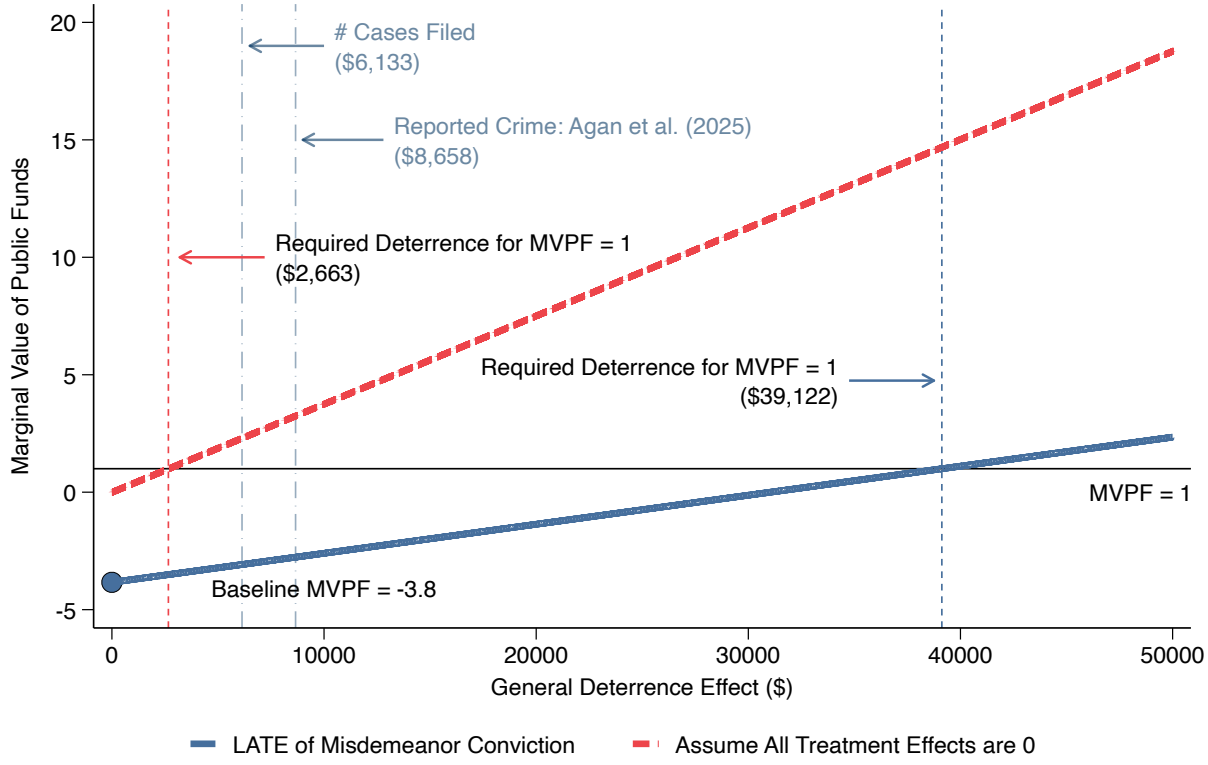
[54] Figure D1 illustrates how the required general deterrence effect changes when assuming that groups of outcomes (crime, labor market, social programs) are non-zero.

that the marginal misdemeanor conviction deters at most $6,133 of crime. As a secondary approach, we use estimates from Agan et al. (2025), who study the effect of electing progressive prosecutors (who scale back misdemeanor prosecution) on reported crime rates. Once again using the upper end of the 95 percent confidence interval, we find that the marginal misdemeanor conviction deters at most $8,658 of crime.[55] These "best case" estimates suggest that the marginal misdemeanor conviction deters at most 3–5 property or 10–15 criminal traffic offenses from occurring.

The vertical dash-dot lines in Figure 9 denote the general deterrence effect estimates from each of these exercises. They intersect the baseline MVPF curve at MVPF values that are negative, suggesting that even these generous estimates of general deterrence are not enough to offset the negative social consequences of the marginal misdemeanor conviction. If misdemeanor convictions truly have no impact on any of the outcomes we study, the MVPF will be above one ($\approx$ 2 to 3) for these values of general deterrence. Given the discussion in Section 5.4, misdemeanor conviction has adverse impacts on young and first-time defendants, who make up a majority of the general sample of misdemeanor defendants across a diverse set of 11 states (see Table A1). Given this, as well as the "best case" nature of the general deterrence estimates we calculate, we view the MVPF of misdemeanor conviction as more likely to be below rather than above unity.

[55] For both of these exercises, we use the upper bound of the 95 percent confidence interval on the reduced form effect of either the number of cases filed (which we convert to charges) or the offense-specific crime rate. We then calculate the number of charges averted per misdemeanor conviction by dividing by the first-stage effect of changes in misdemeanor prosecution, re-orienting outcomes as needed so they can be interpreted as the effect of a marginal conviction. Finally, we use offense-specific cost of crime estimates as in our base MVPF calculation to recover the social cost of crime averted per misdemeanor conviction. For the general deterrence effect implied by the filing date RD, we additionally adjust for crime underreporting, as not all reported offenses are observed in court records.

Figure 9: MVPF of misdemeanor conviction over range of general deterrence effects



Note: This figure reports estimates of the Marginal Value of Public Funds (MVPF) of misdemeanor conviction for a range of possible values of general deterrence effects. 'General Deterrence Effect' represents the total social value of the crimes that are deterred by a marginal misdemeanor conviction. The x-axis displays different potential values for such a deterrence effect. The dark blue line reports the estimated MVPF using our estimated Local Average Treatment Effects (LATE) of misdemeanor conviction and the dashed red line reports the estimated MVPF assuming that the estimated LATEs on all outcomes are zero. For each point on the line we vary the assumed level of general criminal deterrence. The point where each line intersects the horizontal black line at one is the required level of general deterrence to restore the MVPF to unity. Light dash-dot lines indicate upper bounds on potential general deterrence effects from misdemeanor conviction from Agan et al. (2025) and the filing date regression discontinuity exercise in Table A2. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890, CBDRB-FY26-P2957-R13065, CBDRB-FY26-P2957-R13295.

7 Conclusion

Even though misdemeanors represent the most common type of criminal charge in the United States, policymakers have little causal evidence on how obtaining a misdemeanor conviction affects the socioeconomic outcomes of defendants and their families. Despite this lack of evidence, there have been recent attempts to mitigate harms to defendants by scaling back the reach of the misdemeanor system. These actions have generated fierce policy debates, with opponents arguing that such reforms can encourage criminal activity. Evaluating these arguments requires quantifying the net benefit of misdemeanor conviction, which has proven challenging as the existing causal evidence is limited to defendants' recidivism outcomes.

In this paper, we provide descriptive and causal evidence of the impacts of misdemeanor prosecution on a broad swath of socioeconomic outcomes of defendants and their families, including criminal recidivism, labor market outcomes, social safety net usage, and mental well-being. We first describe the demographic and labor market characteristics of individuals involved with the misdemeanor system across 11 states and estimate the causal impact of conviction for defendants charged with misdemeanor crimes. Misdemeanor interactions are common—our data suggest that 14% of adults obtain at least a misdemeanor charge by the time they turn 25. We document that being convicted of a misdemeanor is associated with a greater worsening of labor market outcomes than just having a case filed, consistent with evidence that convictions are more salient to employers than non-convictions and with prior work using estimates from fewer jurisdictions.

We then estimate the causal impact of a misdemeanor conviction (relative to being charged) by combining natural experiments that sharply reduced conviction rates in seven counties and cities across five states. Each natural experiment, generated by unanticipated budget cuts or changes in prosecutorial priorities, yields a regression discontinuity design. While the reforms reduced the likelihood of conviction by 10 p.p. (12% of the pre-reform mean), they had muted impacts on recidivism and labor market outcomes of defendants and their families, although the directions are consistent with improvements in outcomes. We find suggestive evidence that the reforms reduced reliance on the safety net and improved mental well-being.

An important reason for these muted impacts is that the reforms were targeted towards individuals who would benefit the least from avoiding a misdemeanor conviction. Relative to the average defendant in our setting, compliers are more likely to have been charged with minor offenses, are older, and have worse labor market history and potential. As further evidence, estimates of the local average treatment effects (LATE) for compliers are typically at the lower end of estimated bounds for average treatment effects, providing suggestive evidence that defendants are not selected on gains. Through heterogeneity analyses and leveraging cross-reform variation in characteristics of the marginal defendant, we find harmful impacts of conviction for younger or first-time defendants. These analyses suggest that youth and lack of prior criminal history are 'tags' for people most likely to be harmed by misdemeanor conviction and that interventions targeting dismissal toward these groups would be beneficial.

Using our estimated LATEs for focal defendants, we calculate that the Marginal Value of Public Funds (MVPF) of the marginal misdemeanor conviction is –3.8. This estimate implies that the marginal misdemeanor conviction generates substantially less social value than the government costs incurred, unless the conviction deters other individuals from engaging in criminal activity. Given this MVPF, such "general deterrence" effects must prevent the equivalent of at least 23 property crimes or almost 70 traffic crimes from occurring to yield an MVPF of one. Our most optimistic estimates of general deterrence suggest the marginal conviction deters at most 3–5 property crimes or 10–15 traffic crimes, values at which the MVPF is still firmly negative.

While our causal estimates reflect the marginal impact of a misdemeanor conviction conditional on being charged, future work might examine how avoiding a misdemeanor charge in the first place causally affects a broad range of outcomes for defendants and families. Finally, an important caveat in interpreting our estimates is that our results only speak to the outcomes that we can measure. Misdemeanor conviction might have large and meaningful impacts on individuals' lives beyond what is recorded in administrative data or what individuals choose to report in surveys.

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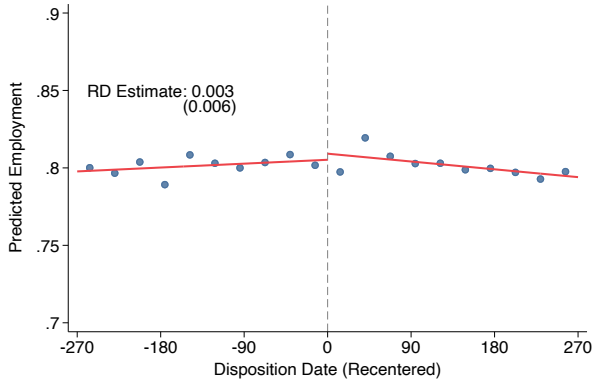
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Appendix A: Additional Results

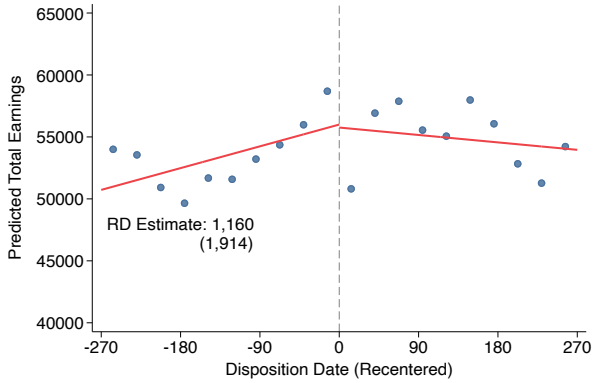
Figures

Figure A1: Validation: Predicted cumulative labor market outcomes over three years

(a) Employment



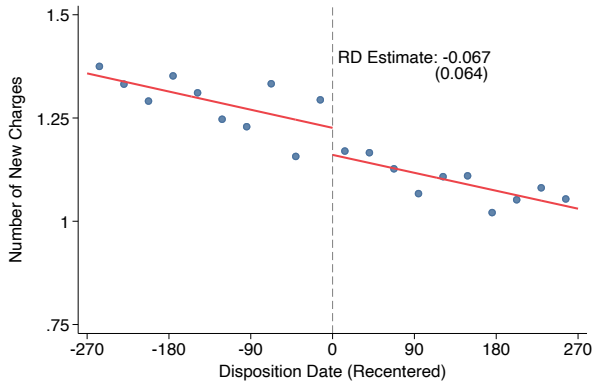
(b) Earnings



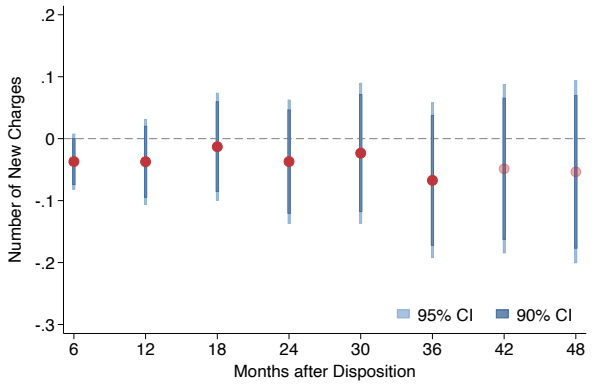
Note: Each Panel presents RDD estimates using a version of Equation 2 that excludes covariates. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890.

Figure A2: Impact of policy reforms on number of new charges

(a) Cumulative (3 years)



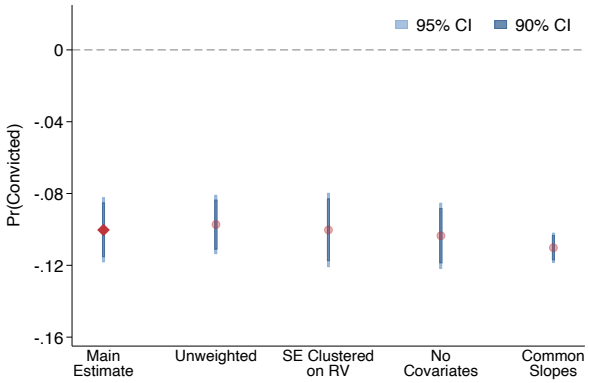
(b) Dynamics (6-48 months)



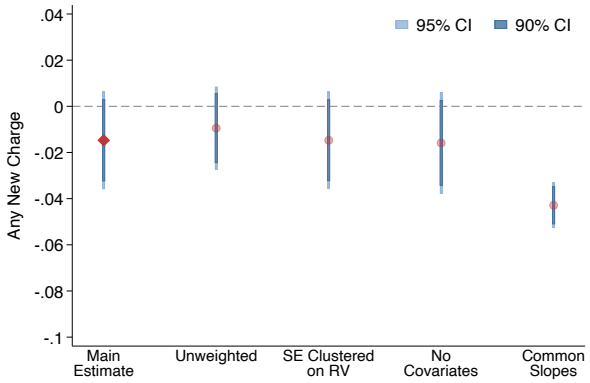
Note: Panel a) presents RDD estimates for the total number of new charges filed over three years using the stacked sample and Equation 2. Panel b) presents RDD estimates for different time horizons. Hollow dots indicate outcomes for which missing follow up information is imputed with zeros. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13295.

Figure A3: Assessing robustness to specification choices

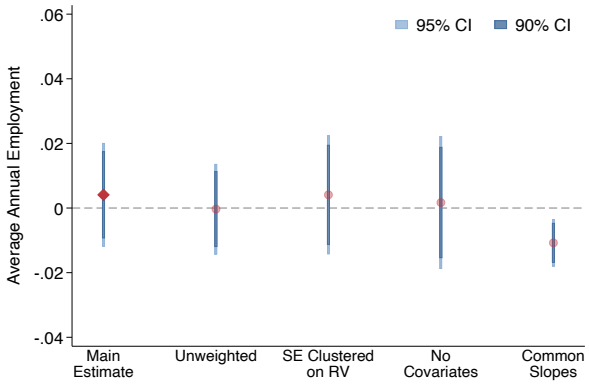
(a) Probability of Conviction



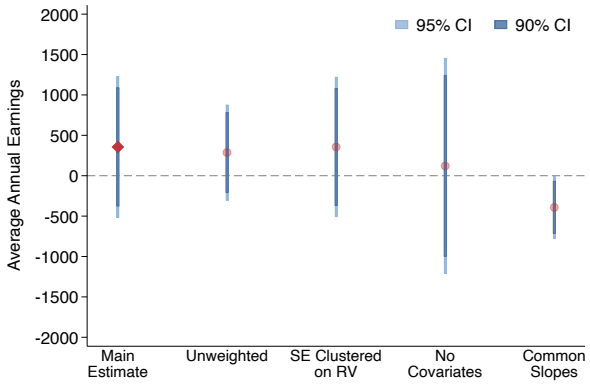
(b) Any New Charge (3 years)



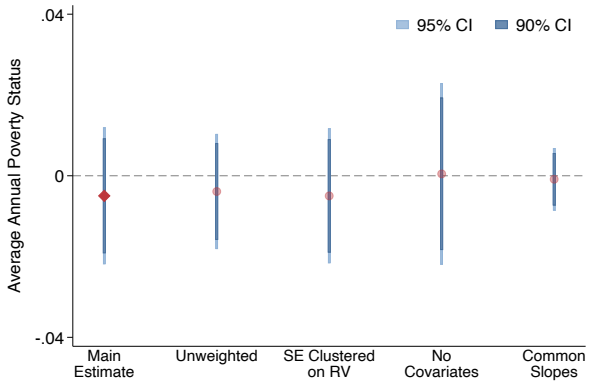
(c) Average Annual Employment (3 years)



(d) Average Annual Earnings (3 years)



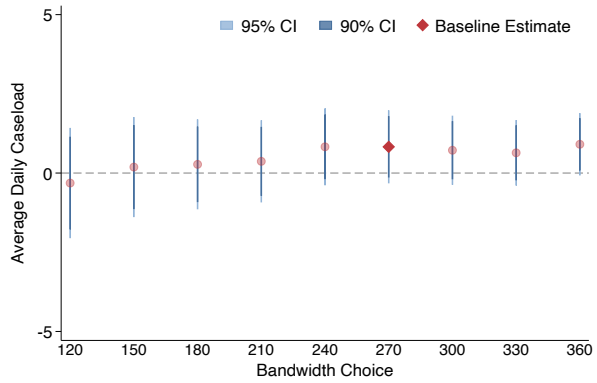
(e) Likelihood of Being Below the Federal Poverty Line (1–3 years)



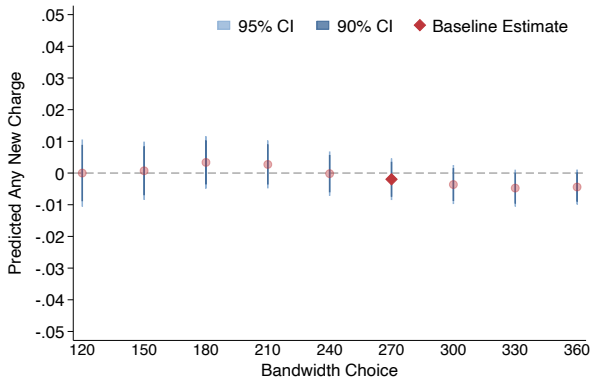
Note: Each Panel presents RDD estimates for our main outcomes using the stacked sample and variations of our baseline specification: 1) unweighted estimates, 2) following our baseline specification but clustering on the running variable, 3) not controlling for baseline covariates, and 4) forcing all natural experiments to have common slopes. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890, CBDRB-FY26-P2957-R13295.

Figure A4: Assessing robustness to choice of bandwidth: Validation exercises

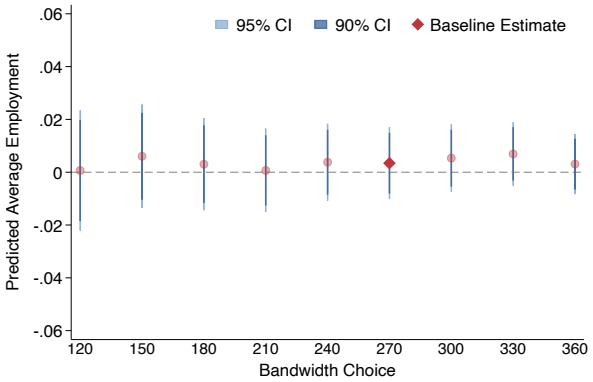
(a) Daily caseload density



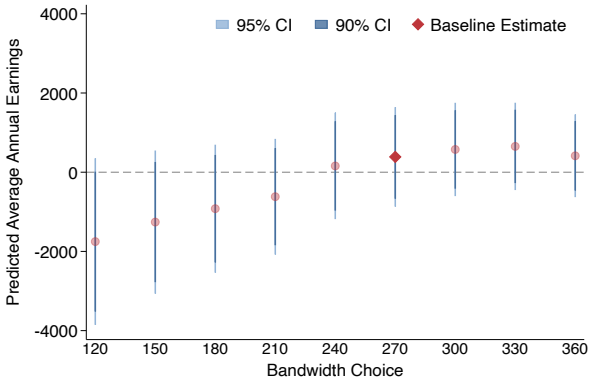
(b) Predicted: Any new charge over 3 years



(c) Predicted: Average employment over 3 years



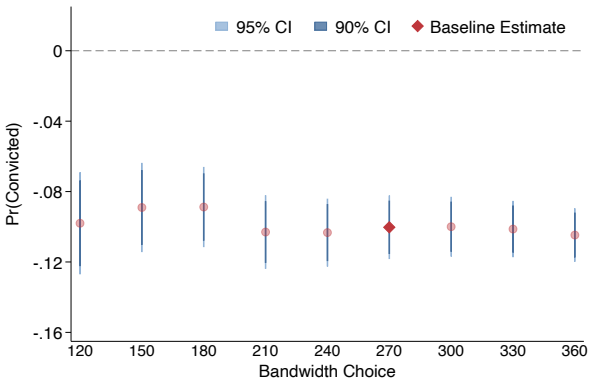
(d) Predicted: Average earnings over 3 years



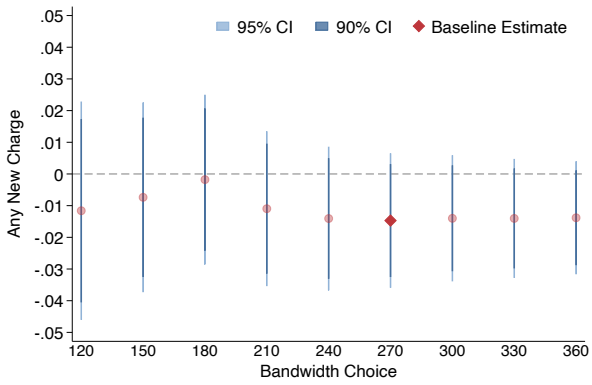
Note: Each Panel presents RDD estimates for the validation exercises using the stacked sample and specification in Equation 2 for different choices of bandwidth. Standard errors are heteroskedasticity-robust in Panel a) and otherwise are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13295.

Figure A5: Assessing robustness to choice of bandwidth: Outcomes

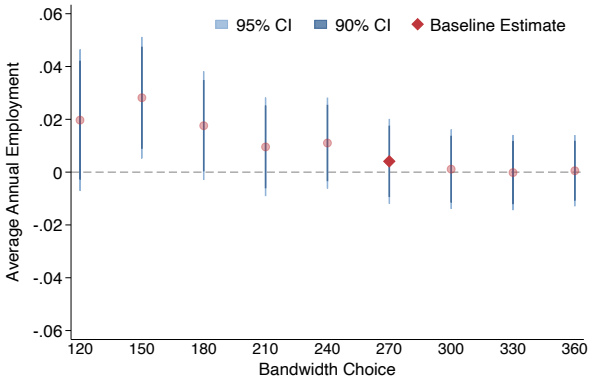
(a) Probability of Conviction



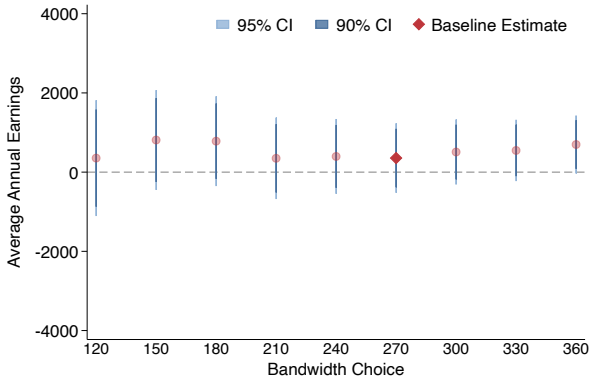
(b) Any New Charge (3 years)



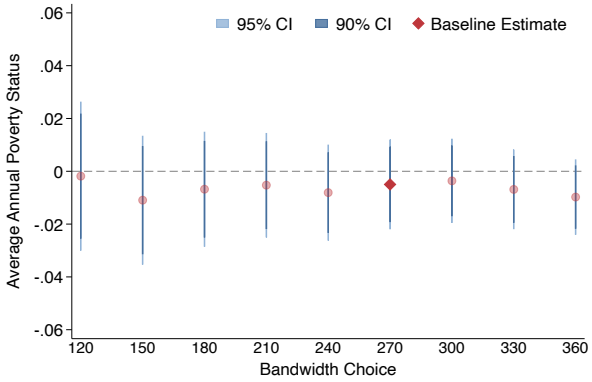
(c) Average Annual Employment (3 years)



(d) Average Annual Earnings (3 years)



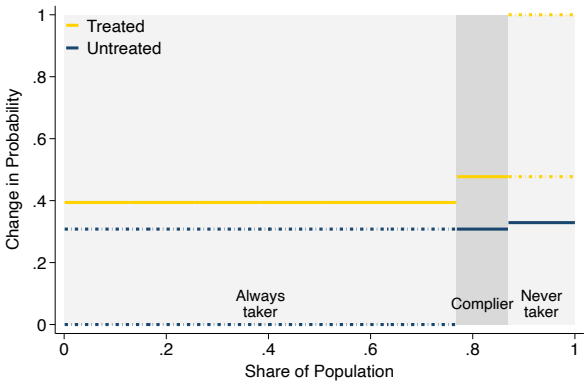
(e) Average Annual Poverty Status (3 years)



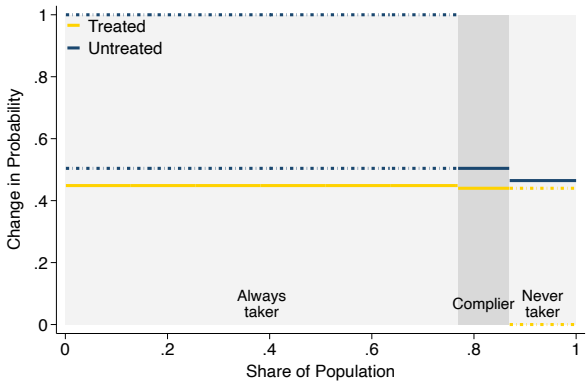
Note: Each Panel presents RDD estimates for our main outcomes using the stacked sample and specification in Equation 2 for different choices of bandwidth. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13295.

Figure A6: Marginal treatment response functions: Outcomes that lie in $[0, 1]$

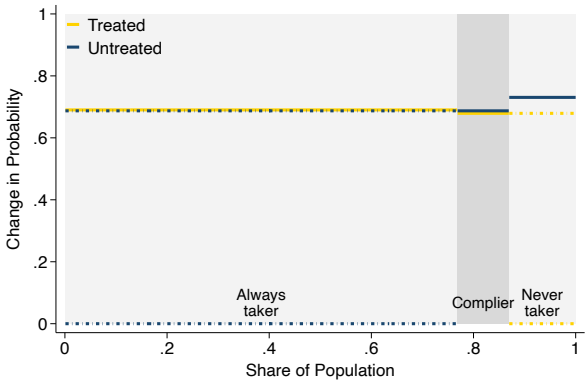
(a) Any New Charge (3 years)



(b) Average Annual Employment (3 years)

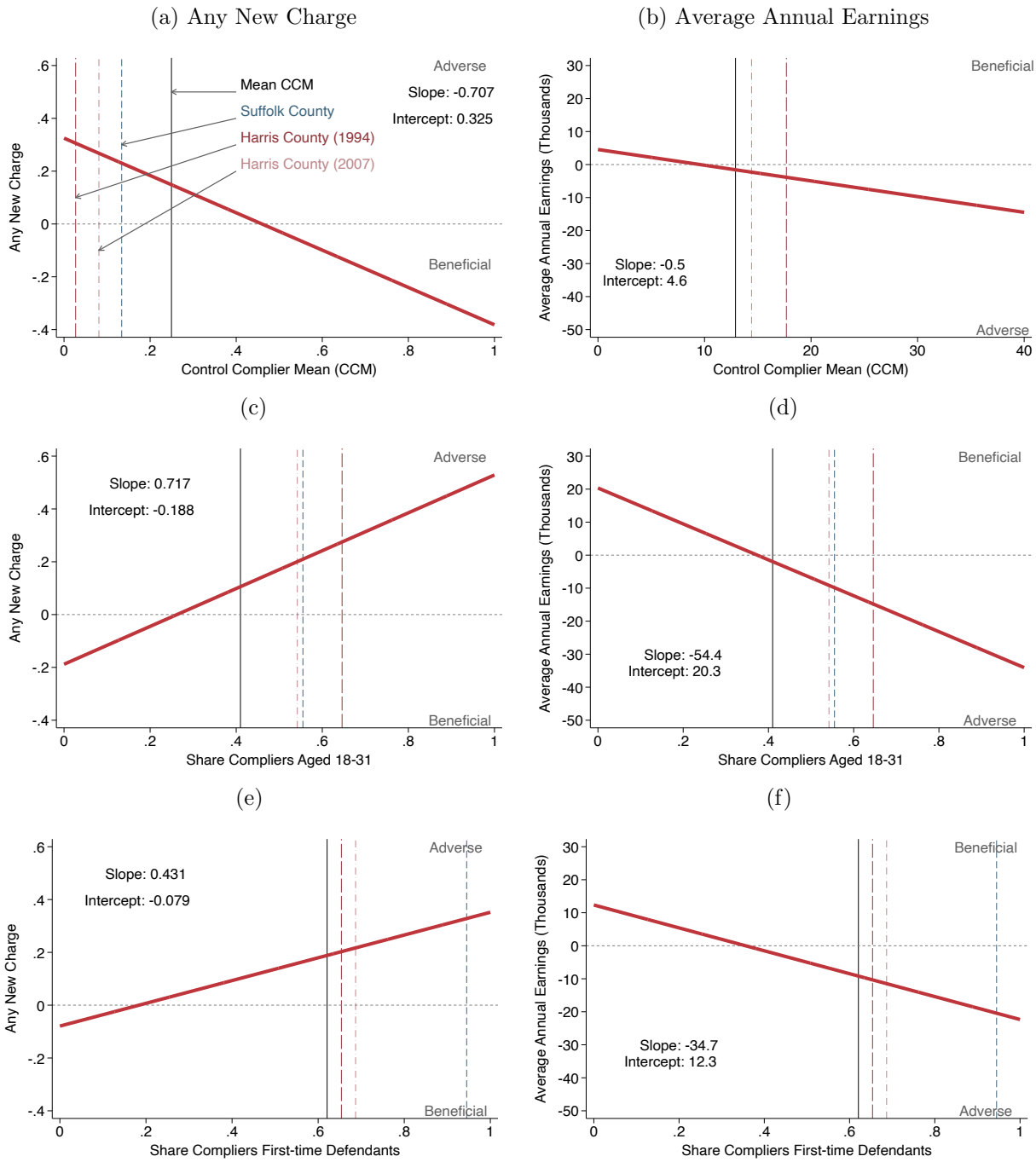


(c) Average Annual Poverty Status (3 years)



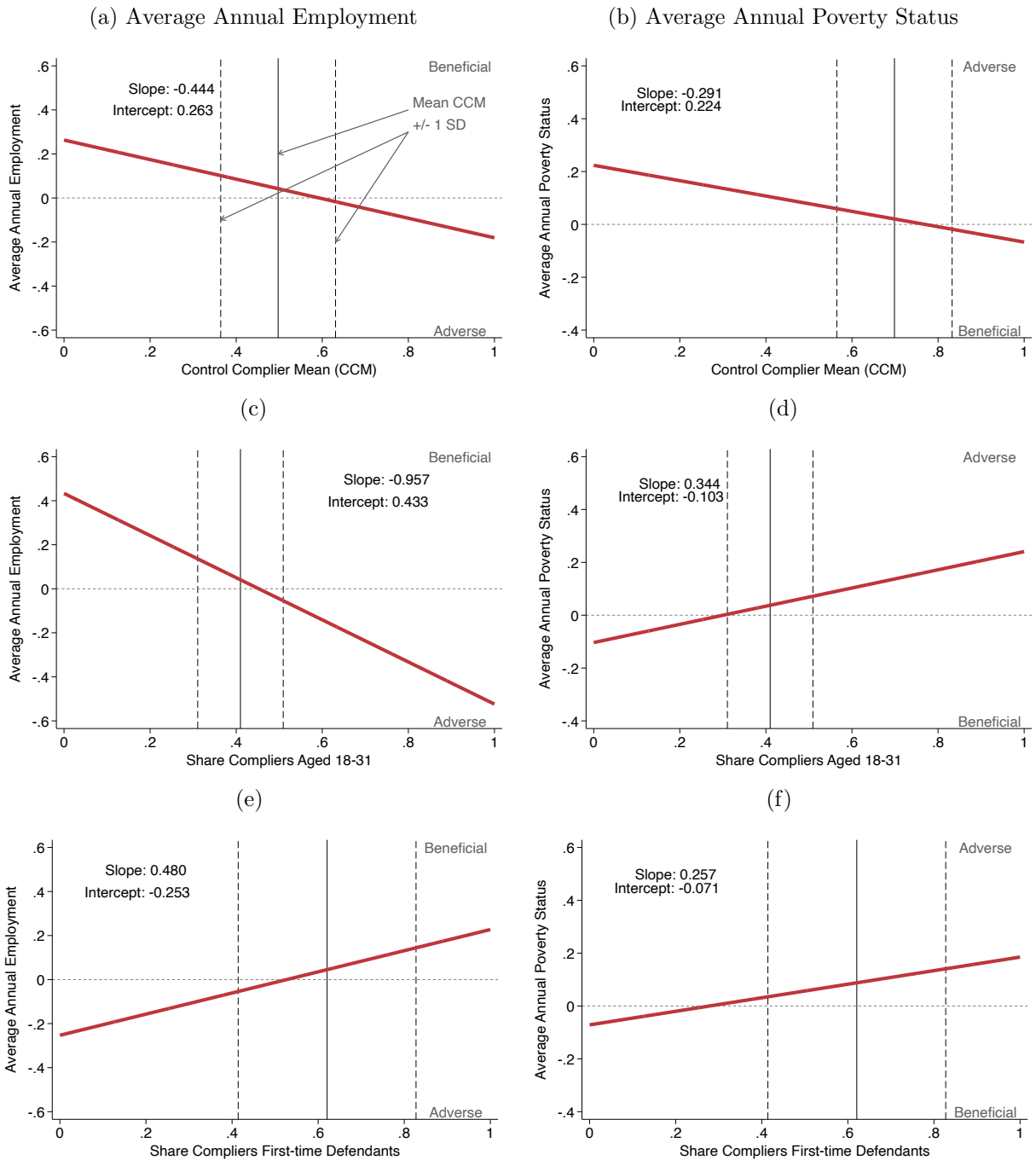
Note: Each Panel presents estimates of the average treated and untreated potential outcomes for always takers, compliers, and never takers. Treatment is defined by conviction and average potential outcomes are estimated using the RDD variation in a binary instrumental variable (IV) framework. Solid lines denote average potential outcomes identified by the IV assumption while dashed lines represent bounds implied by an additional assumption that the average potential outcome is weakly monotonic across always takers, compliers, and never takers (e.g., see Kowalski (2023)). Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890, CBDRB-FY26-P2957-R13295.

Figure A7: Extrapolating treatment effects in other settings: Main outcomes



Note: This figure plots predicted relationships between the local average treatment effect (y-axis) and complier characteristics (x-axis) in each reform, estimated using Equation 3. The solid vertical line represents the mean complier characteristic across the reforms (from Table 4). The dashed blue vertical line represents the complier mean from the study in Suffolk County, Massachusetts studied in Agan, Doleac, and Harvey (2023). The maroon lines represent complier means from Harris County, Texas studied in Mueller-Smith and Schnepel (2021). The long dashed opaque maroon line represents the 1994 penal code reform while the dash-dot translucent maroon line represents the failed jail expansion in 2007. We are not able to plot the estimates from each individual natural experiment due to disclosure avoidance rules particular to the LEHD. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13065, CBDRB-FY26-P2957-R13295.

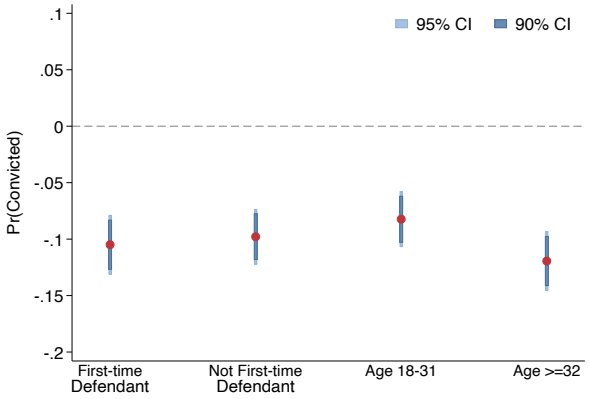
Figure A8: Relationship between the local average treatment effect (LATE) of conviction and characteristics of the marginal defendant: Additional results



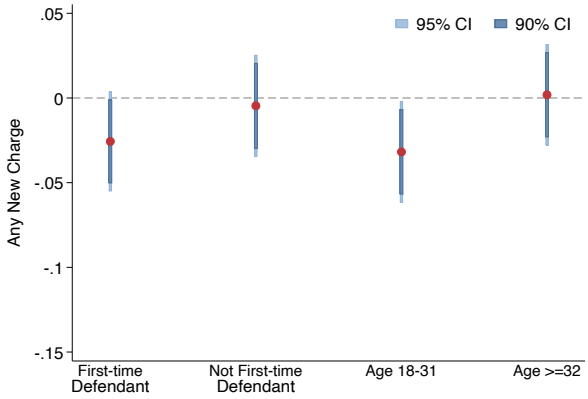
Note: This figure plots predicted relationships between the local average treatment effect (y-axis) and complier characteristics (x-axis) in each reform, estimated using Equation 3. The solid vertical line represents the mean complier characteristic across the reforms (from Table 4) while the dashed vertical lines represent values that are one standard deviation above or below the mean. We are not able to plot the estimates from each individual natural experiment due to disclosure avoidance rules particular to the LEHD. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13065, CBDRB-FY26-P2957-R13295.

Figure A9: Impact of policy reforms by subgroup

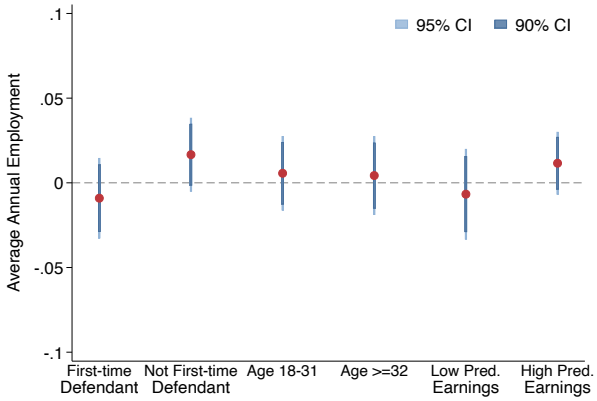
(a) Probability of Conviction



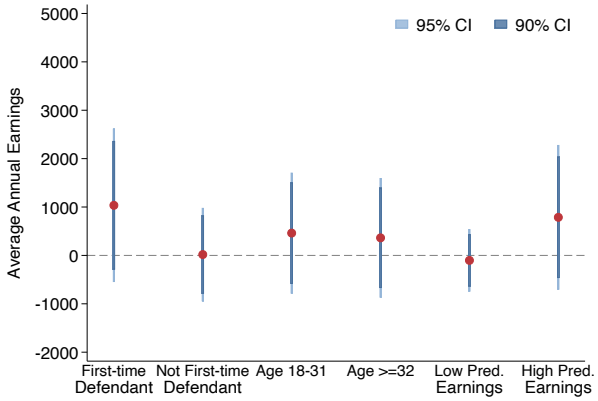
(b) Any New Charge (3 years)



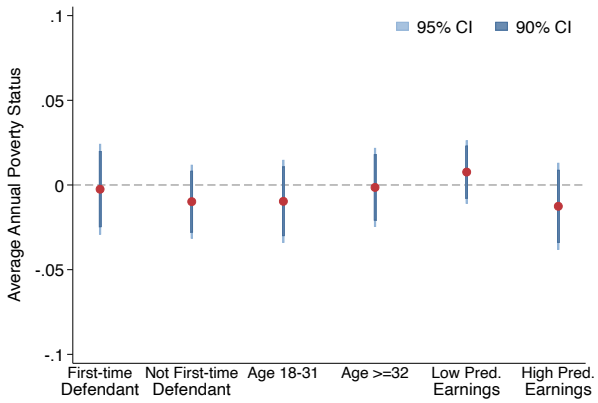
(c) Average Annual Employment (3 years)



(d) Average Annual Earnings (3 years)



(e) Average Annual Poverty Status (3 years)



Note: Each Panel presents RDD estimates using Equation 2. The sample includes individuals in a particular subgroup across all natural experiments. First-time defendants are defendants not previously charged with any offense in the state. High/low predicted earnings are defined by splitting defendants based on the median value of the predicted earnings measure discussed in the context of Figure 3. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13065.

Tables

Table A1: Descriptive statistics: Event study versus RD samples

Variable	Event Study	RD	$100 \times \frac{(2)}{(1)}(\%)$	RD Compliers	$100 \times \frac{(4)}{(1)}(\%)$
	(1)	(2)	(3)	(4)	(5)
Panel A: Demographics					
Male	0.746	0.768	103.0	0.723	97.0
White, non-Hispanic	0.569	0.443	77.7	0.281	49.4
Black, non-Hispanic	0.205	0.294	143.3	0.090	44.0
Hispanic	0.140	0.181	129.3	0.521	371.2
Age: 18-24	0.341	0.263	77.2	0.197	57.8
Age: 25-31	0.203	0.265	130.7	0.213	104.7
Age: 32-42	0.226	0.243	107.4	0.235	104.0
Age: 43 and over	0.230	0.229	99.4	0.355	154.4
First-time Defendant	0.756	0.450	59.6	0.621	82.1
Panel B: Labor Market Outcomes in Previous Year					
Any Employment	0.645	0.708	109.6	0.594	92.1
Total Earnings	13,250	15,590	117.7	12,440	93.9
Poverty Status	0.670	0.624	93.1	0.668	99.8
Panel C: Case Characteristics					
Violent Charge	0.117	0.101	86.3	0.042	36.0
Drug Charge	0.134	0.129	96.1	0.000	0.0
Property Charge	0.198	0.216	109.0	0.053	26.7
DUI Charge	0.160	0.143	89.5	0.278	174.3
Criminal Traffic Charge	0.215	0.290	134.9	0.444	206.6
Public Order Charge	0.257	0.397	154.7	0.656	255.6
Sample Sizes					
N, Event Study	6,892,000				
N, CJARS RD	46,000				
N, LEHD RD	36,500				

Note: This table compares summary statistics across the misdemeanor defendants from the 11 states used in the event study analysis in Section 4 (Column 1), the RD analysis in Section 5 (Column 2), and the untreated outcomes for the RD compliers (Column 4). Columns 3 and 5 display how over- or under-represented the overall RD sample and RD compliers are relative to the event study sample. Note that the analysis in Section 4 uses a 50% sample of defendants for computational ease, while Column 1 uses the 100% sample. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890, CBDRB-FY26-P2957-R13065, CBDRB-FY26-P2957-R13295.

Table A2: Assessing impacts of reform on cases filed

	RD Estimate	Control Mean
	(1)	(2)
Average Daily Caseload	−0.543	13.510
	(0.487)	
Characteristics of Charges Filed		
Violent Offense	−0.019***	0.099
	(0.006)	
Drug Offense	0.009	0.124
	(0.007)	
Property Offense	−0.017**	0.217
	(0.008)	
DUI Offense	0.016**	0.135
	(0.007)	
Criminal Traffic Offense	0.022**	0.294
	(0.009)	
Public Order Offense	−0.012	0.386
	(0.010)	
High Police Discretion	0.056***	0.565
	(0.010)	
High Fine Prevalence	0.033***	0.516
	(0.010)	
N	50,000	

Note: This table presents results from estimating a version of Equation 2 with the case filing date as the running variable, where each row represents the outcome of the regression. Standard errors in the first row are heteroskedasticity-robust while the rest are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13295.

Table A3: Robustness: Only cases filed before reforms

	RD Estimate	Control Mean	Control Complier Mean
	(1)	(2)	(3)
Panel A: Caseload Density and Balance			
Average Daily Caseload	−1.328	13.060	
	(0.823)		
Predicted Any New Charge	−0.001	0.433	
	(0.006)		
Predicted Average Employment	0.006	0.673	
	(0.012)		
Predicted Average Annual Earnings	643	18,740	
	(1,245)		
Panel B: Crime-related Outcomes			
Probability of Conviction	−0.090***	0.822	
	(0.015)		
Any New Charge	−0.021	0.366	0.295
	(0.019)		
Panel C: Labor Market Outcomes			
Average Annual Employment	0.008	0.645	0.433
	(0.015)		
Average Annual Earnings	141	17,090	15,500
	(869)		
Average Annual Poverty Status	0.004	0.616	0.678
	(0.015)		

Note: This table presents results from Equation 2, limiting to cases from our main analysis sample that were filed before the reforms. 'Control Complier Mean' represents the average untreated potential outcome among compliers, where treatment is conviction. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13295.

Table A4: RD estimates: Main outcomes

	RD Estimate	Control Mean	Control Complier Mean
	(1)	(2)	(3)
Panel A: Crime-related Outcomes			
Probability of Conviction	−0.100***	0.823	
	(0.009)		
Case Length (Days)	40.7***	240.9	1,269.0
	(12.8)		
Any New Charge	−0.015	0.381	0.250
	(0.011)		
Panel B: Labor Market Outcomes			
Average Annual Employment	0.004	0.647	0.497
	(0.008)		
Average Annual Earnings	356	16,300	12,910
	(446)		
Average Annual Poverty Status	−0.005	0.625	0.699
	(0.009)		

Note: 'Any New Charge' is a cumulative measure of recidivism over three years. Labor market outcomes are annual averages over three years. 'Control Mean' represents the average value of the variable to the left of the cut-off. 'Control Complier Mean' represents the average untreated potential outcome among compliers, where treatment is conviction. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890.

Table A5: Dynamics, RD estimates: Crime outcomes

Outcome Horizon (Months)	Any New Charge		Number of New Charges	
	RD Estimate	Control Mean	RD Estimate	Control Mean
	(1)	(2)	(3)	(4)
6	−0.018**	0.149	−0.037	0.308
	(0.008)		(0.023)	
	[0.076]		[0.108]	
12	−0.012	0.234	−0.037	0.570
	(0.010)		(0.035)	
	[0.135]		[0.210]	
18	−0.008	0.294	−0.013	0.811
	(0.010)		(0.044)	
	[0.179]		[0.348]	
24	−0.015	0.332	−0.037	0.996
	(0.011)		(0.051)	
	[0.207]		[0.431]	
30	−0.010	0.359	−0.023	1.158
	(0.011)		(0.058)	
	[0.229]		[0.536]	
36	−0.015	0.381	−0.067	1.308
	(0.011)		(0.064)	
	[0.250]		[0.586]	
42	−0.014	0.396	−0.049	1.421
	(0.011)		(0.070)	
	[0.268]		[0.679]	
48	−0.013	0.407	−0.054	1.515
	(0.011)		(0.075)	
	[0.280]		[0.742]	

Note: 'Control Mean' represents the average value of the variable to the left of the cut-off. Standard errors, clustered at the individual level, are in parentheses while control complier means are in brackets. 'Control Complier Mean' represents the average untreated potential outcome among compliers, where treatment is conviction. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890, CBDRB-FY26-P2957-R13295.

Table A6: Dynamics, RD estimates: Cumulative labor market outcomes

Outcome Horizon (Years)	Ever Employed		Total Annual Earnings		Ever Below Federal Poverty Line	
	RD Estimate	Control Mean	RD Estimate	Control Mean	RD Estimate	Control Mean
	(1)	(2)	(3)	(4)	(5)	(6)
1	0.015	0.699	377	19,180	−0.009	0.600
	(0.009)		(565)		(0.010)	
	[0.583]		[13, 460]		[0.663]	
2	0.007	0.758	704	34,290	−0.004	0.696
	(0.009)		(971)		(0.010)	
	[0.610]		[25, 040]		[0.753]	
3	0.003	0.791	1, 067	48,890	0.002	0.747
	(0.008)		(1, 339)		(0.010)	
	[0.635]		[38, 740]		[0.786]	
4	0.004	0.812	1, 153	63,770	0.010	0.776
	(0.008)		(1, 765)		(0.010)	
	[0.658]		[51, 810]		[0.811]	

Note: All outcomes are cumulative, representing the total or the likelihood through year 't'. 'Control Mean' represents the average value of the variable to the left of the cut-off. Standard errors, clustered at the individual level, are in parentheses while control complier means are in brackets. 'Control Complier Mean' represents the average untreated potential outcome among compliers, where treatment is conviction. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890.

Table A7: Impact of policy reforms on self-reported survey outcomes over three years

	RD Estimate	Control Mean	Control Complier Mean
	(1)	(2)	(3)
Panel A: Baseline Characteristics and First-Stage			
Predicted ACS Weight	1.463*	20.560	
	(0.809)		
Probability of Conviction	−0.176***	0.808	
	(0.064)		
Panel B: Labor Market Outcomes			
Employment	0.080	0.615	0.948
	(0.065)		
Earnings	2, 277	18, 970	19,850
	(3, 510)		
Average Poverty Status	−0.113	0.596	0.610
	(0.069)		
Self-employment Income	224	1,131	0
	(1, 785)		
Panel C: Health Insurance Provision, Safety Net Usage, and Mental Well-being			
Any Health Insurance	0.020	0.695	0.488
	(0.060)		
Publicly-provided	0.048	0.371	0.365
	(0.067)		
Through Employer/Union	0.069	0.332	0.158
	(0.068)		
Any Welfare Receipt	−0.085	0.282	0.302
	(0.059)		
Food Stamp Receipt	−0.070	0.277	0.302
	(0.058)		
Welfare Income	61	86	351
	(73)		
Reported Mental Disability	−0.018	0.079	0.067
	(0.042)		
N	1,200		

Note: 'Predicted ACS Weight' is obtained by using covariates to predict the likelihood that individuals in our sample are in the American Community Survey (ACS), following Finlay et al. (2024). We use the interaction of the predicted weights and the inverse of the number of observations in each natural experiment as analytical weights in all analyses using the ACS data. 'Reported Mental Disability' is measured using responses to questions about whether one had difficulty concentrating or remembering due to having a mental, physical, or emotional condition lasting more than 6 months. 'Control Mean' represents the average value of the variable to the left of the cut-off. 'Control Complier Mean' represents the average untreated potential outcome among compliers, where treatment is conviction. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13065, CBDRB-FY26-P2957-R13295.

Table A8: Impact of policy reforms on family members

	RD Estimate	Control Mean	Control Complier Mean
	(1)	(2)	(3)
Probability of Linked Partner	0.019*	0.373	
	(0.011)		
Probability of Conviction	−0.094***	0.830	
	(0.015)		
Any New Charge	0.006	0.107	0.065
	(0.010)		
Average Annual Employment	0.010	0.653	0.651
	(0.011)		
Average Annual Earnings	366	19,890	17,580
	(470)		
Average Annual Poverty Status	−0.013	0.533	0.477
	(0.012)		

Note: The first row estimates whether the likelihood that focal defendants in our sample are linked to their partners varies around the cut-off, using Equation 2. The subsequent rows display the first-stage and impacts on our main outcomes for partners of defendants in our focal sample. 'Control Complier Mean' represents the average untreated potential outcome among compliers, where treatment is conviction. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13295.

Table A9: Event study estimates for counties in RD sample

Outcome	Convicted	Dismissed
	(1)	(2)
Quarterly Employment		
Average after filing	−0.078	−0.056
Quarter before filing	0.560	0.486
Quarterly Earnings		
Average after filing	−589	−266
Quarter before filing	3,658	2,882
Quarterly Poverty Status		
Average after filing	0.055	0.028
Quarter before filing	0.631	0.698
N	166,000	33,500

Note: This table presents estimated event study coefficients using Equation 1 as in Figure 2, but for defendants in counties used in the RD analysis in Section 5. 'Average after filing' presents the mean five-year change in outcomes relative to the quarter before misdemeanor charges were filed against the individual. 'Quarter before filing' presents the average value of the outcome in the quarter before charges were filed. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13295.

Table A10: Relationship between the local average treatment effect (LATE) of conviction and characteristics of the marginal defendant

Estimate	Any New Charge	Employment	Earnings	Poverty Status
	(1)	(2)	(3)	(4)
Panel A: Control Complier Mean				
Slope	-0.707^{*}	-0.444	-0.5^{**}	-0.291
	(0.306)	(0.601)	(0.1)	(0.465)
Intercept	0.325^{**}	0.263	$4,557$	0.224
	(0.107)	(0.345)	$(5,407)$	(0.246)
Panel B: Share Compliers Aged 18-31				
Slope	0.717	-0.957^{**}	$-54,400^{**}$	0.344
	(0.551)	(0.308)	$(17,680)$	(0.792)
Intercept	-0.188	0.433^{**}	$20,340^{**}$	-0.103
	(0.210)	(0.139)	$(7,794)$	(0.360)
Panel C: Share Compliers First-time Defendants				
Slope	0.431	0.480^{**}	$-34,690^{***}$	0.257
	(0.393)	(0.164)	$(7,974)$	(0.318)
Intercept	-0.079	-0.253	$12,350$	-0.071
	(0.276)	(0.140)	$(6,366)$	(0.175)

Note: Each panel presents the estimated slope and intercepts from Equation 3. 'Any New Charge' is a cumulative measure of recidivism over three years. The remaining outcomes are annual averages over three years. First-time defendants are defendants not previously charged with any offense in the state. 'Control Complier Mean' (CCM) represents the average untreated potential outcome among compliers, where treatment is conviction. Standard errors are heteroskedasticity-robust and do not account for uncertainty in the LATE or CCM estimates. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13065.

Table A11: Heterogeneity analyses: Main crime outcomes

Subgroup	Probability of Conviction		Any New Charge	
	RD Estimate	Control Mean	RD Estimate	Control Mean
	(1)	(2)	(3)	(4)
Age 18-31	−0.082***	0.813	−0.032**	0.422
	(0.013)		(0.015)	
			[0.263]	
Age 32+	−0.119***	0.833	0.002	0.336
	(0.013)		(0.015)	
			[0.234]	
Male	−0.103***	0.829	−0.023*	0.390
	(0.011)		(0.013)	
			[0.268]	
Female	−0.091***	0.801	0.011	0.350
	(0.019)		(0.021)	
			[0.199]	
First-time Defendant	−0.105***	0.807	−0.026*	0.271
	(0.013)		(0.015)	
			[0.128]	
Not First-time Defendant	−0.098***	0.834	−0.005	0.464
	(0.012)		(0.015)	
			[0.428]	
First-time Defendant, Age 18-31	−0.094***	0.798	−0.040*	0.323
	(0.017)		(0.021)	
			[0.126]	
First-time Defendant, Age 32+	−0.122***	0.820	−0.012	0.198
	(0.021)		(0.021)	
			[0.143]	
Not First-time Defendant, Age 18-31	−0.075***	0.827	−0.021	0.516
	(0.018)		(0.023)	
			[0.529]	
Not First-time Defendant, Age 32+	−0.118***	0.841	0.009	0.417
	(0.017)		(0.021)	
			[0.356]	

Note: 'Any New Charge' is a cumulative measure of recidivism over three years. First-time defendants are defendants not previously charged with any offense in the state. Standard errors, clustered at the individual level, are in parentheses while control complier means are in brackets. 'Control Mean' represents the average value of the variable to the left of the cut-off. 'Control Complier Mean' represents the average untreated potential outcome among compliers, where treatment is conviction. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13065.

Table A12: Heterogeneity analyses: Main labor market outcomes

Subgroup	Avg. Annual Employment		Avg. Annual Earnings		Avg. Poverty Status	
	RD Estimate	Control Mean	RD Estimate	Control Mean	RD Estimate	Control Mean
	(1)	(2)	(3)	(4)	(5)	(6)
Age 18-31	0.006	0.720	461	16,030	-0.010	0.611
	(0.011)		(636)		(0.012)	
	[0.665]		[14, 380]		[0.598]	
Age 32+	0.004	0.570	364	16,580	-0.001	0.641
	(0.012)		(629)		(0.012)	
	[0.366]		[11, 400]		[0.773]	
Male	0.003	0.643	559	17,770	-0.007	0.612
	(0.009)		(559)		(0.010)	
	[0.505]		[14, 490]		[0.683]	
Female	0.009	0.660	−404	11,380	-0.003	0.669
	(0.016)		(535)		(0.017)	
	[0.486]		[8, 783]		[0.731]	
Low Predicted Earnings	-0.007	0.414	−102	3,814	0.008	0.895
	(0.014)		(328)		(0.009)	
	[0.159]		[1, 505]		[0.966]	
High Predicted Earnings	0.012	0.853	787	27,350	-0.013	0.387
	(0.009)		(761)		(0.013)	
	[0.807]		[22, 910]		[0.468]	
First-time Defendant	-0.009	0.687	1, 035	20,000	-0.002	0.563
	(0.012)		(807)		(0.014)	
	[0.522]		[14, 180]		[0.666]	
Not First-time Defendant	0.017	0.619	18	13,660	-0.010	0.670
	(0.011)		(490)		(0.011)	
	[0.423]		[10, 380]		[0.764]	
First-time Defendant, Age 18-31	-0.013	0.758	1, 129	19,240	-0.014	0.549
	(0.015)		(1, 049)		(0.019)	
	[0.666]		[13, 860]		[0.574]	
First-time Defendant, Age 32+	-0.008	0.590	1, 023	21,060	0.014	0.581
	(0.019)		(1, 277)		(0.020)	
	[0.385]		[13, 770]		[0.763]	
Not First-time Defendant, Age 18-31	0.022	0.687	215	13,210	-0.008	0.665
	(0.016)		(744)		(0.017)	
	[0.627]		[13, 730]		[0.682]	
Not First-time Defendant, Age 32+	0.013	0.558	−68	14,050	-0.012	0.675
	(0.015)		(654)		(0.015)	
	[0.285]		[7, 956]		[0.820]	

Note: Outcomes are annual averages over three years. High and low predicted earnings categories are obtained by splitting defendants based on the median value of the predicted earnings measure discussed in the context of Figure 3. First-time defendants are defendants not previously charged with any offense in the state. Standard errors, clustered at the individual level, are in parentheses while control complier means are in brackets. 'Control Mean' represents the average value of the variable to the left of the cut-off. 'Control Complier Mean' represents the average untreated potential outcome among compliers, where treatment is conviction. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13065.

Appendix B: Data Details

Figure B1: Coverage of LEHD states

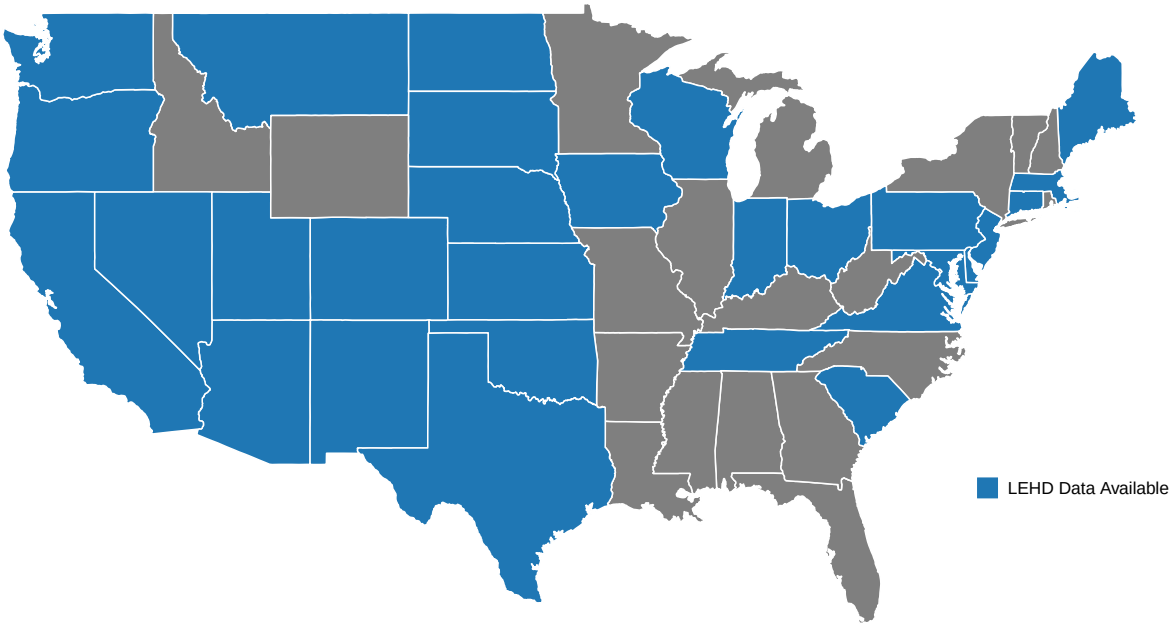


Table B1: States and time periods used in event study analysis

State	Sample Start	Sample End
Arizona	2009:Q1	2014:Q4
Indiana	2010:Q1	2014:Q4
Maryland	1995:Q1	2014:Q4
North Dakota	2003:Q1	2014:Q4
Oklahoma	2005:Q1	2014:Q4
Oregon	1996:Q1	2014:Q4
Pennsylvania	2009:Q1	2014:Q4
South Carolina	2012:Q1	2014:Q4
Texas	2000:Q1	2012:Q4
Virginia	2009:Q1	2014:Q4
Wisconsin	1999:Q1	2014:Q4

Note: This table describes the sample of misdemeanor cases we use in our event study analyses. For each individual with a case filed in this date range, we construct a panel of labor market histories and outcomes of five years before and after the case was filed. The listed start and end dates are defined by the date ranges used to construct the Census Bureau's Quarterly Workforce Indicators for a particular state (see U.S. Census Bureau (2026)). End dates are defined so that all five year post-periods end by 2019:Q4, to avoid including the start of the COVID-19 pandemic. If a state's CJARS court data availability does not match that of the LEHD for that state, we use time periods that let us observe at least five years of data before and after defendants' cases were filed.

Appendix C: Methodological Details

C.1 Treatment Effect Bias Under Multiple Treatments

Given the nature of the policy reforms that we study in Section 5, a reasonable concern is that having a case disposed just before versus just after the policy cut-off date might influence other aspects of treatment, e.g., eligibility for diversion programs, rather than just conviction. We characterize how such behavior would bias our estimates of the treatment effect of misdemeanor conviction by decomposing our estimates into margin-specific treatment effects using the logic of Humphries et al. (2023).

Define the following treatment states: conviction (c), diversion (d) and dismissal, which we refer to as non-conviction in this section for simplicity (n). Let $D_i = j$ if individual i received treatment j. Let $\omega_{p \to q}$ denote the proportion of individuals flowing in direction $p \to q$ and let $\triangle_{p \to q}^{j-k}$ denote the treatment effect between treatments j and k for people shifted along the $p \to q$ margin. For simplicity let the binary instrument be $Z = \mathbb{1}(Post_i)$, where the instrument reduces the likelihood of conviction, as in the main text.

Since our reforms predominantly involve either cuts to prosecutor and court resources or a deprioritization of misdemeanor prosecution, we assume conditional pairwise monotonicity (CPM) and permit the following set of treatment flows that occur when moving from $Z = 0$ to $Z = 1$ (Humphries et al. 2023): conviction to diversion ($c \to d$), diversion to non-conviction ($d \to n$), and conviction to non-conviction ($c \to n$). This assumption ensures that flows between treatments are weakly unidirectional and forces individuals to flow into less punitive treatment states.

Recall that the instrument is binary for simplicity. Hence, the analog of the first-stage estimate that we present in Section 5 takes the form of $\text{Convict}_i = \alpha + \pi Z_i + \varepsilon_i$ and $\pi = -(\omega_{c \to d} + \omega_{c \to n})$; the sum of the proportion of defendants shifted from conviction to diversion and conviction to non-conviction. The reduced form regression is given by $Y_i = \alpha + \beta Z_i + \epsilon_i$ and we can write β as the following weighted average of treatment effects associated with the permitted one-way flows.

$$\beta = \omega_{c \to n} \triangle_{c \to n}^{n-c} + \omega_{c \to d} \triangle_{c \to d}^{d-c} + \omega_{d \to n} \triangle_{d \to n}^{n-d}$$

The first two terms above represent a combination of the treatment effect of conviction relative to two non-conviction alternatives (diversion and non-conviction) for different sets of compliers. While this will not recover the impact of conviction relative to dismissal, $\omega_{c \to n} \triangle_{c \to n}^{n-c}$, it can still be interpreted as the impact of being convicted relative to not being convicted. However, we note that maintaining diversionary programs requires adequate funding and budget cuts represent the predominant source of our variation. As a result, defendants in our setting are more likely to be shifted out of rather than into diversion in response to binding resource constraints, suggesting that $\omega_{c \to d} \approx 0$.

The third term represents the treatment effect of non-conviction relative to diversion, which can contaminate our estimate of the impact of conviction relative to non-conviction. This term is

unproblematic if either no one is shifted from diversion to non-conviction ($\omega_{d\to n} = 0$) or if diversion has no impact relative to non-conviction ($\triangle_{(\cdot)}^{n-d} = 0$). Absent either of these conditions, the third term introduces a bias, where the sign and magnitude depend on $\omega_{d\to n}\triangle_{d\to n}^{n-d}$.

Given the large literature finding that more intense engagement with the criminal legal system worsens outcomes for defendants, this second condition of zero treatment effect is unlikely to be fully satisfied (e.g., Mueller-Smith 2015; Mueller-Smith and Schnepel 2021; Agan, Doleac, and Harvey 2023; Kamat, Norris, and Pecenco 2023; Agan et al. 2024*b*,*a*). Even though the eventual criminal record under a completed misdemeanor diversion program looks similar to that of an individual who had a case outright dismissed (i.e., a dismissed criminal charge), an individual who is shifted from diversion to non-conviction does not have to comply with numerous court-ordered conditions of their diversion agreement, which may also influence future outcomes. Since non-conviction is unconditional, it is plausible that being shifted from diversion to non-conviction weakly reduces future recidivism ($\triangle_{d\to n}^{n-d} \leq 0$) and improves labor market outcomes ($\triangle_{d\to n}^{n-d} \geq 0$). Under these conditions, using β to estimate the LATE of conviction relative to non-conviction will provide a lower bound (since non-conviction reduces recidivism relative to conviction; upper bound in absolute terms) for the impact on recidivism and an upper bound on the impact on labor market outcomes. Alternatively, if the entirety of the treatment effect between diversion and non-conviction operates through the status and visibility of an individual's criminal record, then the treatment effect moving from diversion to non-conviction is likely close to zero. In such a scenario, our estimate of β will be unbiased.

Appendix D: MVPF Calculation Details

In this appendix, we provide additional details on our approach to calculating the Marginal Value of Public Funds (MVPF), using the framework from Hendren and Sprung-Keyser (2020), which we use to quantify the welfare effects of misdemeanor convictions. Below, we discuss the relevant assumptions for both the numerator (willingness to pay) and the denominator (net government cost).

Willingness to Pay: Our estimate of willingness to pay for misdemeanor convictions stems from causal estimates on several key outcomes: criminal recidivism, labor market earnings, and transfers from social programs, all of which are measured over the three years following the disposition.

1. Criminal recidivism: We estimate the change in the social cost of crime due to misdemeanor conviction by multiplying the change in the number of charges by type for the marginal defendant by estimates of the cost of crime. Our estimates include the victim costs of crime adjusted for underreporting following Deshpande and Mueller-Smith (2022).[1] While many of these offenses will be typically categorically charged as felonies or misdemeanors, we are unable to distinguish between them in cases where the same offense may be charged as either (e.g., forms of assault). Our estimates should therefore be interpreted as taking an implicit weighted average across these two classes of charges. More generally, we use cost of crime estimates from Miller, Cohen, and Wiersema (1996). Aggregated together, this recovers the change in the social cost of crime, which we estimate at $17,432.

2. Earnings: Our estimate in the main text suggests that misdemeanor conviction decreases formal post-tax (10%) labor market earnings by $9,578 over the following three years. Using survey-based self-employment income from the American Community Survey, we estimate that misdemeanor conviction depresses post-tax self-employment earnings by $3,434.

3. Social program transfers: We also estimate the effect of misdemeanor conviction on non-SNAP welfare use and SNAP take-up. For the former, we estimate a cumulative reduction of $1,046. For SNAP, we are only able to measure the extensive margin of take-up. To translate this into a dollar value, we use SNAP Quality Control microdata to estimate the mean benefit amount for households receiving benefits in our five states and assume 8 months of receipt, the typical non-pandemic spell length (Gregory 2025). This calculation yields an estimated increase of $739. We follow a similar approach to calculating willingness to pay for Medicaid. Specifically, we use estimates of the willingness to pay for Medicaid from Finkelstein, Hendren, and Luttmer (2019), which we assume is $1 per dollar of annual Medicaid net cost ($1,450).[2]

[1] For the residual of the change in number of charges where we do not have type-specific estimates, we assume they are proportionally distributed between public order and criminal traffic. To be conservative, we assume the victim costs associated with public order offenses are $0, and for criminal traffic, are the insurance premium ($563) that would be needed to internalize the externalities from distracted driving (Karl et al. 2023).

[2] Finkelstein, Hendren, and Luttmer (2019) find that the willingness to pay is between $0.5 and $1.2 per dollar of net cost of Medicaid.

We then multiply this quantity by the average Medicaid spell length for healthy adults under 65 (8.6 months, Wagner and Solomon 2021) and the change in the extensive margin probability of receiving Medicaid, which yields –$282.[3]

Net Government Cost: Our estimate of the net government cost includes four components.

1. Mechanical change in judicial costs: Conviction of misdemeanor offenses incurs government administrative costs of prosecution and investigation that vary with offense type (Caulkins 2010; The Vermont Center for Justice Research 2014; Hunt, Anderson, and Saunders 2017; Hunt, Saunders, and Kilmer 2019). Using a similar distribution-based approach as our estimate of the social costs of crime, but instead based on the baseline charged offense, we estimate that the government spends $640 in prosecution costs and $2,023 in investigation costs per convicted misdemeanor.

2. Behavioral change in judicial costs: Because misdemeanor conviction also changes future criminal recidivism, the prosecution and investigative expenditures are affected by behavioral changes of defendants. To calculate this component, we multiply the change in future recidivism by the prosecution and investigative costs (weighted by type of crime), also adjusted for underreporting using stage-specific estimates. We calculate additional government expenditures of $4,570.

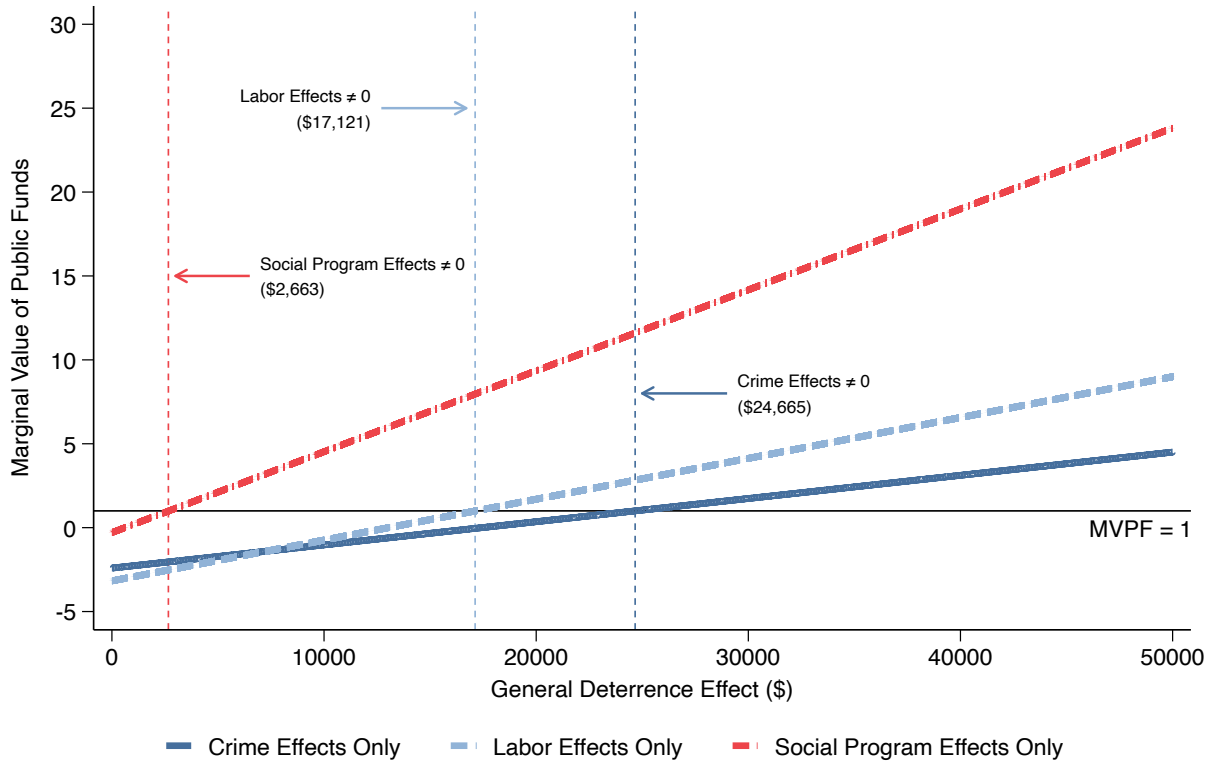
3. Change in income tax revenue: Following Deshpande and Mueller-Smith (2022), we assume that individuals in our setting remit 10% of their formal and self-employment income in taxes. Multiplying our causal estimate of the change in labor market earnings by this rate implies a decrease of $1,064 and $382 in government revenue.

4. Behavioral change in social program transfers: Because misdemeanor conviction also has effects on social program use, which represents a direct fiscal externality for the government's budget constraint, we additionally include the LATE estimates of increases in SNAP usage ($739) and decreases in non-SNAP program transfers (–$1,046) and Medicaid (–$282).

Putting these two pieces together, we estimate negative social benefits of –$31,032 and positive government costs of $8,090 per misdemeanor conviction. Taking the ratio implies a Marginal Value of Public Funds of –3.8.

[3]Our method of converting these extensive margin responses into dollar values implicitly rules out multiple spells of similar lengths within the follow-up period or one continuous spell that spans the follow-up period. Because both of these unobserved receipt profiles would imply greater benefits, our estimates of the willingness to pay will be a lower bound.

Figure D1: MVPF of misdemeanor conviction over range of general deterrence effects assuming outcome groups are non-zero



Note: This figure reports estimates of the Marginal Value of Public Funds (MVPF) of misdemeanor conviction for a range of possible values of general deterrence effects. 'General Deterrence Effect' represents the total social value of the crimes that are deterred by a marginal misdemeanor conviction. The x-axis displays different potential values for such a deterrence effect. The dark blue line reports the estimated MVPF using our estimated Local Average Treatment Effects (LATE) of misdemeanor conviction assuming that only the crime effects are non-zero, the light blue dashed line assumes that only labor market effects are non-zero, and the dashed red line assumes that only social program effects are non-zero. For each point on the line we vary the assumed level of general criminal deterrence. The point where each line intersects the horizontal black line at one is the required level of general deterrence to restore the MVPF to unity.