The Socioeconomic Consequences of Misdemeanor Conviction*

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Abstract

This paper combines rich administrative and survey data with two complementary research designs to provide new evidence on how misdemeanor prosecution in the United States affects the socioeconomic outcomes of defendants and their families. First, event study estimates from 11 states document a persistent labor market penalty associated with misdemeanor conviction, above and beyond a criminal charge, which varies substantially across states. Second, using a regression discontinuity design, we estimate the causal impact of a misdemeanor conviction by combining local government reforms that sharply reduced conviction rates in seven counties across five states. We find precise null average effects of these reforms on future socioeconomic outcomes, largely since the reforms were not targeted to defendants who would benefit the most. We use cross-reform variation in characteristics of the marginal defendant to identify observable features associated with adverse impacts of a misdemeanor conviction. We find that the adverse impact of a conviction is increasing in the share of marginal defendants that are young or do not have a criminal history. Finally, we use our causal estimates to calculate the welfare effects of misdemeanor convictions under a range of assumptions on statistical precision and general equilibrium deterrence effects. Our current estimates suggest that misdemeanor conviction has a negative MVPF unless it deters substantial serious crime.

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1 Introduction

In the United States, around 13 million criminal misdemeanor charges (e.g., shoplifting, drug possession, disorderly conduct) are filed per year, representing 75% of the annual criminal legal system caseload (Stevenson and Mayson 2018). Recognizing the overwhelming scale of the misdemeanor system and stigma often associated with criminal records, District Attorneys have recently scaled back prosecution of certain non-violent offenses (e.g., Agan et al. 2025; Amaral, Ouss, and Ozier 2025), prompting debates about the value of such policies (e.g., Cramer 2018; Tanenbaum 2020; Walters 2021). Understanding the net benefit of such reforms requires quantifying the impacts on defendants and their families and whether estimates differ across people. However, we have limited causal evidence on the impacts of the criminal misdemeanor system beyond recidivism (Agan, Doleac, and Harvey 2023; Kamat, Norris, and Pecenco 2023).

The effects of misdemeanor prosecution could theoretically impact the lives of defendants and their families along multiple dimensions. On one hand, prosecution might deter defendants from future criminal offending (Becker 1968), connect them with appropriate services, and eventually improve their long-term well-being.[1] On the other hand, criminal records might reduce household resources and increase social safety net reliance. For instance, criminal records can disqualify individuals from certain occupations (e.g., Rodriguez and Avery 2016) and audit studies suggest employers are reluctant to hire workers with even minor criminal records (Leasure 2019). As with other aspects of the criminal legal system, criminal records could directly alter the environments surrounding children and other family members in ways that are adverse (e.g., Finlay, Mueller-Smith, and Street 2023; Mueller-Smith et al. 2024) or beneficial (e.g., Norris, Pecenco, and Weaver 2021; Arteaga 2023). Moreover, local governments differ in how misdemeanor offenses are prioritized, often due to leadership preferences and fiscal constraints.[2] Given these differences, the impacts of misdemeanor convictions likely mirror the heterogeneity seen in responses to other interventions.[3]

In this paper, we provide descriptive and causal evidence of the impacts of misdemeanor prosecution on a broad swath of socioeconomic outcomes of misdemeanor defendants and their families, including criminal recidivism, labor market outcomes, social program usage, and mental well-being. We use our causal estimates to conduct expansive welfare analyses of misdemeanor conviction. Both sets of analyses use linked datasets covering criminal records, earnings from employer-employee matched data, population-level socioeconomic surveys, and birth certificate records. Our main samples include defendants charged with violent (e.g., simple assault, domestic violence) as well as non-violent (e.g., drug possession, disorderly conduct) misdemeanor offenses.

First, we estimate quarterly event studies to document that being charged with (i.e., having a

[1] Evidence from Norway finds that rehabilitation-focused criminal legal systems can improve employment and mental well-being (e.g., Bhuller et al. 2020; Bhuller, Khoury, and Løken 2025).

[2] 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.

[3] 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, in large part due to differences in social norms and the structure of public institutions.

case filed) and being convicted of a misdemeanor are both associated with meaningful labor market penalties for over 3 million defendants across 11 states. In the five years after being charged with a misdemeanor, average quarterly employment and earnings fall by 4–5 p.p. (8–9% of the baseline mean) and $210–270 (6–7%) respectively, increasing poverty status by 2–2.5 p.p. (3–4%) as a result. Relative to the penalty of being charged, being convicted worsens labor market outcomes by 7–30% more. The average penalties we estimate mask considerable heterogeneity across states, especially for the additional penalty of a conviction above and beyond a charge. The cross-state standard deviation of this additional penalty is 10% to 220% *larger* than the mean, depending on outcome. While these labor market penalties do not necessarily reflect causal estimates (e.g., adverse shocks may simultaneously induce job loss and an arrest in the same quarter), they provide suggestive evidence that being convicted of a misdemeanor may compound adverse impacts of a misdemeanor charge and that such impacts might vary across jurisdictions.

Second, we estimate the *causal* impact of a misdemeanor conviction (relative to being charged) using a regression discontinuity design (RDD) generated by local reforms that occurred between 2008 and 2016. These local reforms take the form of unanticipated budget cuts and changes in prosecutorial priority that sharply reduced misdemeanor conviction rates in seven jurisdictions across five geographically diverse states. The reforms affected counties and cities in Arizona (Pima), Florida (Orange), North Dakota (Williams), Virginia (Newport News City), and Wisconsin (Dane, Milwaukee, and Outagamie). Most of the reforms involved budget cuts to misdemeanor units at District Attorney offices or the criminal court system, either by reducing prosecutor resources or administrative staff. In contrast, prosecutors in Williams County and Newport News City reallocated resources toward prosecuting serious drug offenses after joining a federal effort to combat drug trafficking, a deviation which diminished residual capacity to pursue misdemeanor cases.

We use the sharp nature of these policy reforms in an RDD framework, comparing the outcomes of defendants whose cases were disposed just before versus just after the reforms were announced or implemented.[4] Since the RDD compares outcomes for individuals whose cases are disposed within a very small window, the individuals being compared face the same outcome 'regime'. As a result, the RDD nets out any impact these reforms may have had on the behavior of other institutions that were not directly targeted. For example, if police responded to the new policy environment by making fewer arrests that they felt were unlikely to result in a conviction, this behavior would be equally reflected in the outcomes for individuals on either side of the cut-off.

While the reform sharply reduced the likelihood of conviction by 10 p.p. (12% of the pre-reform mean), we do not find evidence that the reforms generated lasting changes in criminal recidivism or labor market outcomes over the following three years. Our estimates are relatively precise, ruling out changes greater than –9% to 1.7% in the cumulative likelihood of recidivism, –1.8 and 3.1% in annual employment rates, and –3.2 and 7.5% in average annual earnings. We find some evidence of short-run improvements, but these are only transitory. Specifically, defendants whose cases were

[4] When considering the impacts on defendants' families, we compare outcomes for family members of defendants whose cases were disposed just before versus just after the cut-off date.

disposed just after the cut-off were 1.7 p.p. ($p = 0.033$) less likely to be charged with a new offense six months later and 1.5 p.p. ($p = 0.11$) more likely to be employed one year later. However, these effects become smaller and less precise over longer time horizons.[5]

A potential reason for these muted impacts is that the reforms were not targeted towards individuals who were most likely to be impacted by a misdemeanor conviction. Faced with large and sudden changes in resources, decision-makers likely dismissed cases that they no longer had the resources to prosecute. As a result, they may not have had the capacity to target dismissal toward individuals who would benefit the most from avoiding a misdemeanor conviction. We assess this candidate explanation by comparing characteristics of compliers, defendants shifted from conviction to dismissal by the reforms, to the average defendant in our sample.[6]

The selection patterns in our setting suggest that decision-makers targeted individuals with low potential recidivism and formal labor market outcomes, but not large treatment effects. Compliers are less likely to have a criminal history and are more likely to be older than 40, ages at which individuals are typically less criminally-active and less likely to benefit from avoiding the criminal legal system (e.g., Mueller-Smith and Schnepel 2021; Augustine et al. 2022). Compared to the average defendant, compliers are more likely to be charged with minor crimes (e.g., traffic, disorder) than violent or property offenses, crimes that surveys of employers suggest are most likely to harm employment prospects (e.g., Holzer, Raphael, and Stoll 2007; Agan et al. 2024*b*). Finally, compliers have low enough potential employment and earnings levels that a misdemeanor conviction may not reduce these outcomes by much more. We see such patterns 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).

Since the policy reforms in each setting occurred due to different reasons, each reform likely targeted different marginal defendants. We use cross-experiment variation in targeting quality to identify observable 'tags' (Akerlof 1978) for people most likely to be affected by a misdemeanor conviction. We do this by correlating the experiment-specific local average treatment effects (LATE) and complier characteristics. Our results provide suggestive evidence that the impacts of a misdemeanor conviction are more adverse when defendants have low potential recidivism or greater potential labor market earnings. Replacing potential outcomes with complier characteristic shares in these exercises reveals similar conclusions for reforms that dismissed more young and first-time defendants. Taken together, our results suggest that younger ages and no prior criminal history are useful 'tags' for targeting policies of non-conviction.[7]

Finally, to understand the welfare consequences of misdemeanor conviction, we cast our es-

[5] Linking our sample to ACS survey responses, we find suggestive evidence that the reforms improved labor market outcomes, reduced safety net reliance, and improved mental well-being, although these estimates are imprecise due to the reduced sample size.

[6] We also test this explanation by comparing the local average treatment effects (i.e., impacts for defendants affected by the reforms) to estimated bounds for the average treatment effects (i.e., impacts for all defendants in our sample) of conviction. These results are currently undergoing disclosure review.

[7] Estimates from classic subgroup analyses are consistent with these patterns, particularly for recidivism. We find suggestive but imprecise evidence that the reforms improved labor market outcomes for defendants whom we predict are above the median in terms of future earnings, but not for other subgroups.

timates into a Marginal Value of Public Funds (MVPF) framework (Hendren and Sprung-Keyser 2020; Finkelstein and Hendren 2020). Using our LATE estimates of misdemeanor conviction that incorporate information on recidivism, earnings, self-employment, and social safety net usage, we estimate an MVPF of –3.8, chiefly driven by both increased recidivism and worsened labor market outcomes.[8] Future versions will augment these calculations with estimates on Medicaid enrollment and household-level spillovers.

While the direct impacts of misdemeanor conviction represent welfare losses, misdemeanor conviction may nonetheless be socially valuable if it meaningfully deters others from engaging in criminal activity, which the calculation above does not include. We construct bounds on the magnitude of such "general deterrence" effects required to offset the government expenditures on misdemeanor conviction, first by taking our LATE estimates as given and second by assuming that they are all zero. We find that the MVPF of misdemeanor conviction would be 1 if the marginal conviction deters crime that would cost society between \$2,663–\$43,656, yielding a substantial required general deterrence effect. Given that the average violent and property crimes cost \$70,000 and \$900, the marginal misdemeanor conviction would need to successfully deter individuals from committing at least one violent crime or between approximately three and 50 property crimes to offset net government expenditures.

While there is a large literature studying the causal effects of criminal records, most prior work has focused on felonies, which are more severe than misdemeanors. Evidence from audit studies suggests that employers are reluctant to hire applicants with felony records (e.g., Pager 2003; Pager, Western, and Sugie 2009). These employer preferences translate into labor market penalties that typically manifest early, generating lasting scarring effects that persist even after the felony record is later removed (Agan et al. 2024*a*). Indeed, avoiding a felony conviction through diversion greatly reduces recidivism and improves employment outcomes up to 20 years later for low-risk, first-time felony defendants (Mueller-Smith and Schnepel 2021). Thus, the scarring effects of a criminal record can be a key obstacle to economic stability for individuals at the margin of the criminal legal system.

Despite how common interactions with the misdemeanor system are, we know surprisingly little about their impacts. Consistent with evidence that employers are less willing to hire individuals with misdemeanor records (Uggen et al. 2014), especially those with convictions rather than dismissals, misdemeanor convictions are associated with greater labor market penalties than charges in Maryland, Pennsylvania, and Texas (Agan et al. 2024*b*). Recent work in Massachusetts finds that avoiding prosecution for a non-violent misdemeanor reduces future interactions with the criminal legal system, regardless of whether one would have been convicted (Agan, Doleac, and Harvey 2023). Evidence from Washington State finds that budget reforms that reduced the likelihood of misdemeanor prosecution and conviction generated large reductions in criminal legal interactions (Rao and Reeves 2026). In Ohio, conditional on being charged with a serious misdemeanor, con-

[8] Accordingly, the MVPF of non-conviction, which represents both a welfare gain and contraction of government expenditures, has an implied MVPF of infinity.

viction increases future criminal interactions for those without a prior felony conviction (Kamat, Norris, and Pecenco 2023). While this growing literature provides evidence on the impacts of misdemeanor convictions on crime outcomes, we have no causal evidence on how such convictions impact the lives of defendants and their families in other aspects.

This paper contributes to the literature in several ways. First, we provide causal evidence on how a misdemeanor conviction affects defendants and their families over numerous socioeconomic outcomes beyond criminal recidivism. By combining administrative and survey data with policy reforms from five states representing 14% of the U.S. population, we conduct more expansive welfare analyses than would have been possible in prior work. Failing to capture impacts on non-recidivism outcomes and family members may misrepresent the welfare effects of misdemeanor conviction in either direction. Our estimates suggest that the reforms that reduced conviction rates were not targeted based on treatment effects. As a result, these reforms had muted impacts on the average misdemeanor defendant. Despite this, our estimates of the MVPF of misdemeanor conviction suggest welfare losses unless the marginal misdemeanor conviction deters substantial serious crime.

Second, we use the variation in local government constraints and preferences across seven natural experiments to provide policy guidance on how to target reductions in misdemeanor convictions. Using the cross-experiment variation in the characteristics of the marginal defendant, we identify the lack of criminal history and young defendants as observable 'tags' (Akerlof 1978) for individuals whose socioeconomic trajectories might be the most responsive to a misdemeanor conviction.

Finally, we build on existing work documenting how misdemeanor adjudication is associated with persistent labor market penalties in a few states by providing evidence from 11 states that cover 28% of the U.S. population (Rose 2021; Agan et al. 2024*b*). The 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 states.

2 Institutional Background and Settings

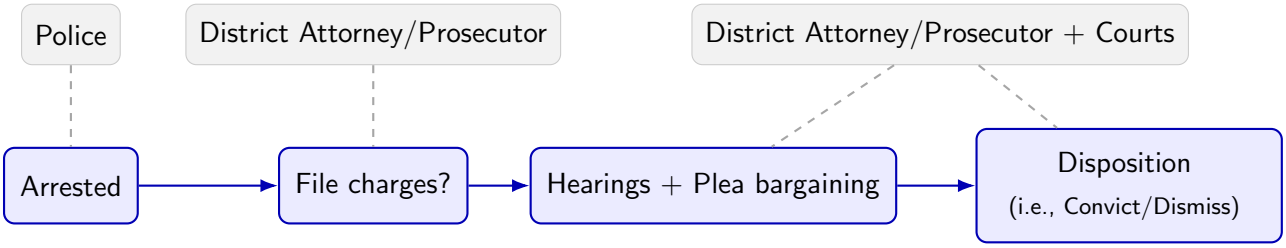
In this section, we provide more detail on the misdemeanor segment of the U.S. criminal court system. We then describe the policy reforms that yield the natural experiments that we use to understand the causal impacts of misdemeanor convictions.

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 are eligible for 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 [Undergoing disclosure avoidance review.] percent of adults born between [Undergoing disclosure avoidance review.] will have a misdemeanor charge by age 25 and [Undergoing disclosure avoidance review.] 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 widespread nature, conviction rates for misdemeanor offenses vary widely across jurisdictions. In the United States, the mean misdemeanor conviction rate between 2008 to 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.[9] 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 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 exacerbated by occupational licensing restrictions (Rodriguez and Avery 2016) and can

[9] 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.

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, suggesting that policies which reduce these ramifications may be welfare-improving.

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.

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 lose 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.

Moving to 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 Attorney's 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 shortfall (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).[10] 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 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 potentially laying off 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.

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

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 that 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.[11] While the CJARS data currently cover 39 U.S. states that represent approximately 80% of the U.S. population, CJARS contains court records for 20 states. We use data from 11 of 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, while we have LEHD data from all the states that the natural experiments occur in, 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.[12] All income measures are adjusted to be in 2017

[11] 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.

[12] 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).

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 on to 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.

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 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) and Agan et al. (2024*b*). We use this empirical approach to document how employment, earnings, and poverty status evolve over time for individuals whose cases were ultimately dismissed versus those who were convicted of their charges. Together, these estimates provide suggestive evidence on the marginal impact of a misdemeanor conviction relative to a charge. We also assess the extent to which changes in these outcomes vary across states.

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 dispositions (see Table B1 for the complete list of states and time periods). For each individual, we include only their first misdemeanor disposition across all the states we use to ensure that our sample includes

only one disposition event per person.[13] For computational tractability, 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.

We trace the labor market trajectories using Equation 1, estimated separately for individuals who were: a) dismissed of all charges and b) 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 provides suggestive 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.

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

Each of the β_k coefficients estimates the average difference in the outcome relative to the quarter before the case is filed.[14] This normalization is designed to capture any changes in labor market activity while charges are pending.[15] 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 at least suggestive but precise evidence on the marginal effect of a misdemeanor conviction relative to a misdemeanor charge and how these effects vary with geography.

[13] 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.

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

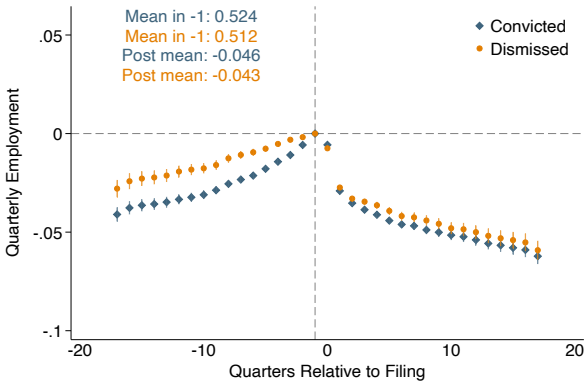
[15] Pending charges are reported on employer background checks in many states (Agan et al. 2024*b*), although samples of job applications suggest employers often do not ask about such charges (Vuolo, Lageson, and Uggen 2017).

4.2 Descriptive evidence on labor market impacts of misdemeanor convictions

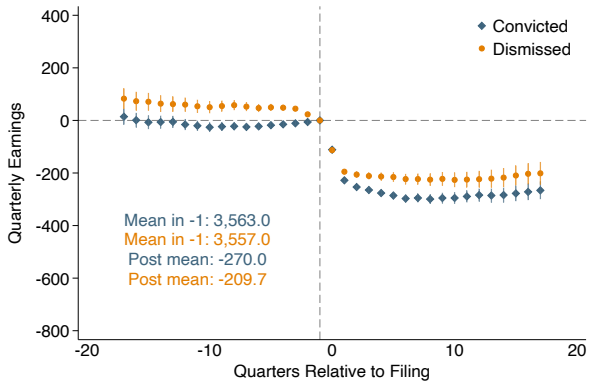
In Figure 2, we examine how labor market outcomes evolve around a misdemeanor charge, separately for charges that were dismissed versus convicted. Panel a) documents how formal employment evolves for these two groups. In the years leading up to a misdemeanor charge, quarterly employment rates increase 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 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.[16]

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

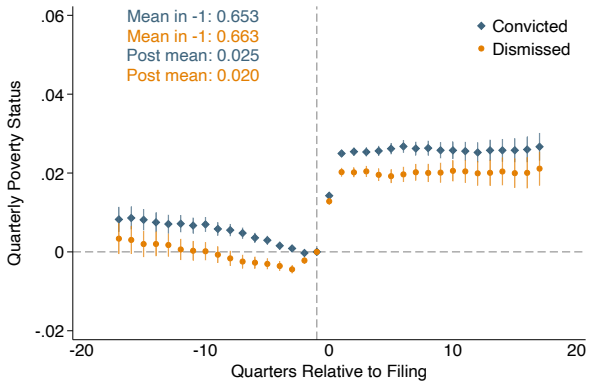
(a) Any Employment



(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.

[16] 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.

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.[17] Panel c) of Figure 2 documents how these earnings reductions translate to time spent 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.

Columns 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.[18] For both groups of defendants, the standard deviation

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

[18] The post-period means reported in Table 2 are different from those displayed in Figure 2 because the former

of the post-period penalties are 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 all of the outcomes, the standard deviation in the labor market penalty is 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 the *causal* effect of a misdemeanor conviction. Specifically, we use the sharp nature of the budget cuts and priority changes discussed in Section 2 to study the impact of a misdemeanor conviction relative to a dismissal in a regression discontinuity (RDD) framework. 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).

allows the coefficients on the control variables to vary by state while the latter estimates a single coefficient per control variable.

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.

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

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 a number of assumptions, which we empirically probe in Section 5.2. For β to represent an ITT of the reforms we study, we need to satisfy an independence assumption. Essentially, defendant characteristics must be smooth across the discontinuity, ruling out defendants of certain types sorting to one particular side of the discontinuity. Additionally, since the RDD compares defendants whose cases are disposed just before the cut-off date to those whose cases are disposed just after, both sets of defendants face the same outcome 'regime'. As a result, the RDD design nets out any impact the budget cuts and priority changes we study have on the behavior of other institutions. For instance, say police responded to the new policy environment by not making arrests that they felt were unlikely to result in conviction. This behavior would be equally reflected in the outcomes for individuals on both sides of the cut-off.

If we want to use the ITT to understand the causal impact of a misdemeanor conviction, i.e., the local average treatment effect (LATE), we need to satisfy additional assumptions. First, (relevance), the policy reforms need to actually reduce misdemeanor conviction rates. Second (monotonicity), the policy reforms must weakly make defendants less likely to be convicted. For example, this assumption would be violated if the reforms decreased the likelihood of conviction for some defendants and increased it for others. 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 other interventions sharply changed at the same time as the reform, e.g., the provision of court-mandated drug treatments.

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. 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.[19] 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.

Our main outcomes of interest 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. In our main analysis, we focus on outcomes 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.

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, this strategy compares outcomes for individuals whose family member'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 reforms. We find that the number of cases disposed is smooth around the cut-off, suggesting limited scope for this type of sorting behavior.

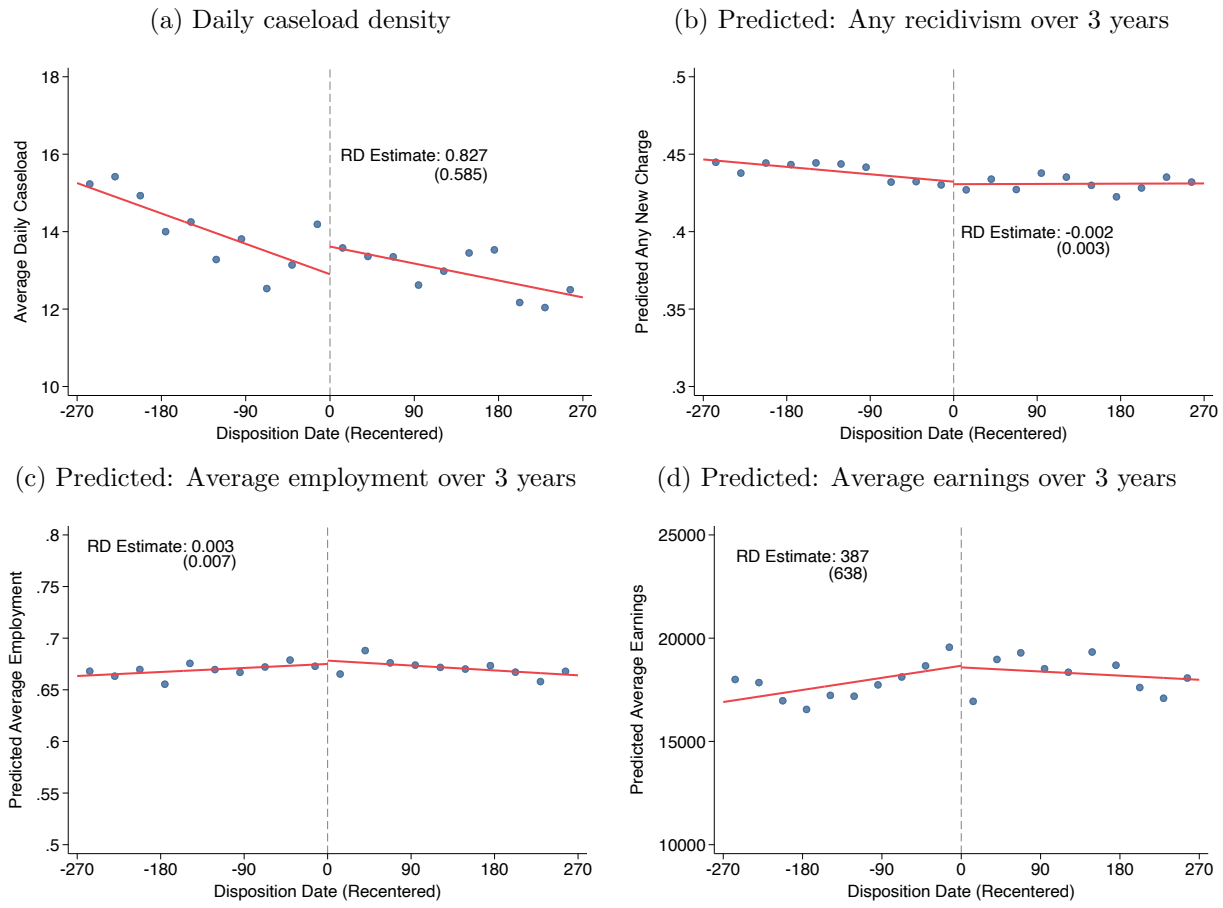
Panels b)–d) present summary evidence that the baseline observable characteristics of defendants are smooth around the cut-off. We first predict cumulative recidivism as well as average annual employment and earnings three years after disposition using two-way interactions of the covariates we include in our main analysis.[20] We find no evidence of discontinuities in any of these summary measures. Moreover, Figure A1 finds no discontinuities in alterative 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

[19] 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.

[20] 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.

Figure 3: Validating natural experiments



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, CBDRB-FY26-P2957-R13065.

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 (22%). However, the absolute number of violent offenses in our sample is generally 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. Overall, these results suggest that the natural experiments we discuss here provide valid quasi-experimental variation.

Moving on to contextualizing 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%), Non-Hispanic White (44%), and are around 34 years old on average. 47% of defendants are either Non-Hispanic Black or Hispanic, with the remaining defendants missing race

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			
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. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890, CBDRB-FY26-P2957-R13065.

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 3.8 times and has 2.6 prior convictions. Most defendants in our sample (71%) had at least one quarter of employment in the year before disposition, earning around $16,000 in the prior year.

Moving to case characteristics and outcomes, defendants on average faced [Undergoing disclosure avoidance review.] 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).

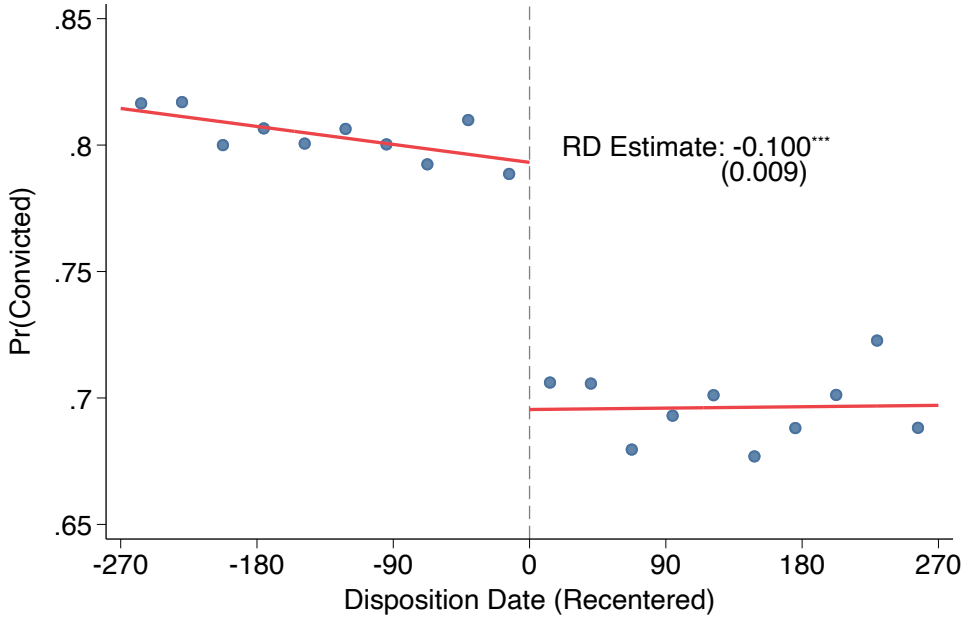
While the reforms reduced the likelihood of conviction, Figure 5 reveals that having a case disposed just after the reforms had muted impacts on recidivism, employment, earnings, and poverty status three years after disposition. While each of these estimates are insignificant, the confidence intervals are generally tight. The estimates in Panels a) and b) rule out reform-induced changes larger than –9% and 1.7% in three-year recidivism and –1.8% and 3.1% in average annual employment over three years.[21] Similarly, the estimates in Panels c) and d) can rule out changes in average annual earnings greater than –3.2% and 7.5% and changes in average annual poverty status between –3.5% and 1.9%.[22]

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

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

[22] The local average treatment effects (LATE) of conviction implied by the estimates in Figure 5 are larger than the ITTs (as a proportion of average outcome for compliers if dismissed, see Table A1 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 different from zero.

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.

counties where we only have three years of follow-up data for a particular outcome.[23] 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 –1.7 p.p. ($p = 0.033$) in the first six months to –1.3 p.p. ($p = 0.23$) after four years (see Table A2 for the underlying estimates).[24] 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 A3 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 ($p = 0.51$) to \$1153 ($p = 0.51$) over the four 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 0.99 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.

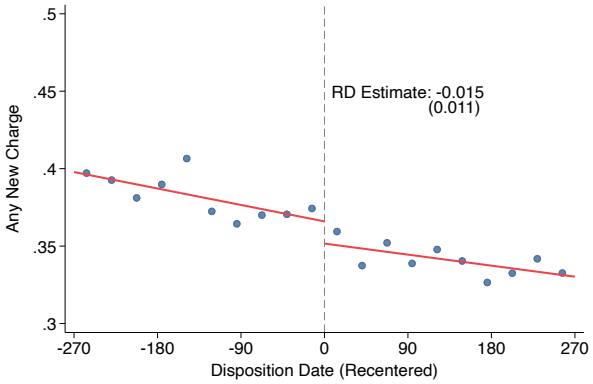
Next, we use self-reported survey responses from the 2008-2019 waves of the American Community Survey (ACS) to understand if the reforms affected defendants on dimensions that are not

[23] 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.

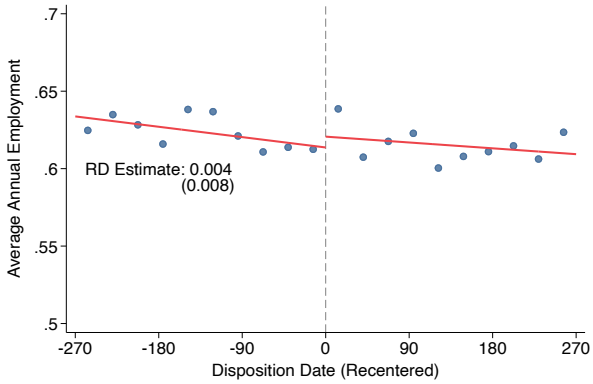
[24] The number of new charges filed falls by 0.039 after six months ($p = 0.076$) and falls by 0.053 ($p = 0.46$) after four years, although the latter is much less precise (see Panel b) of Figure A2).

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

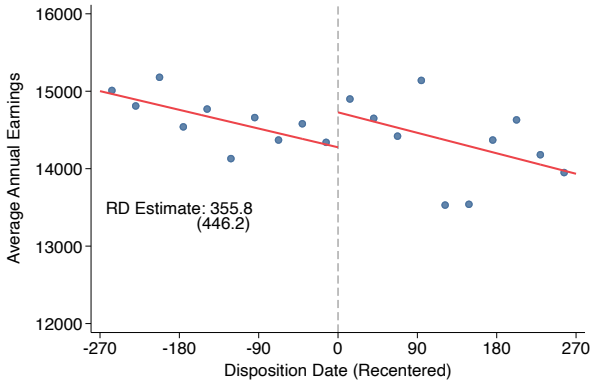
(a) Any New Charge (3 years)



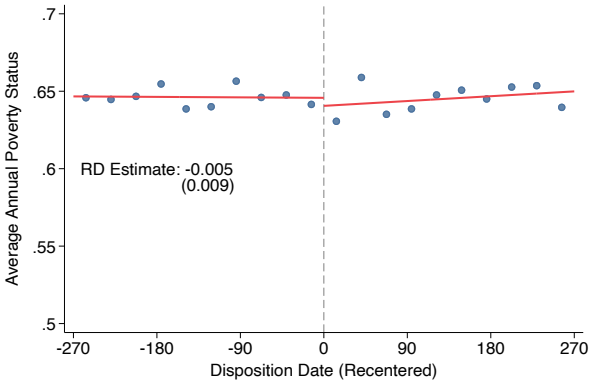
(b) Average Annual Employment (3 years)



(c) Average Annual Earnings (3 years)



(d) Average Annual Likelihood of Being Below the Federal Poverty Line (3 years)



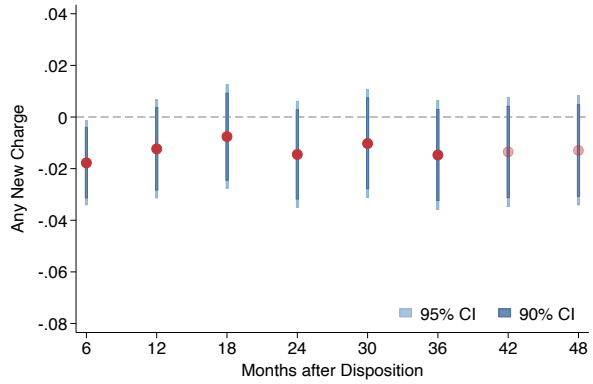
Note: Each Panel presents RDD estimates using the stacked sample and Equation 2. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890.

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.

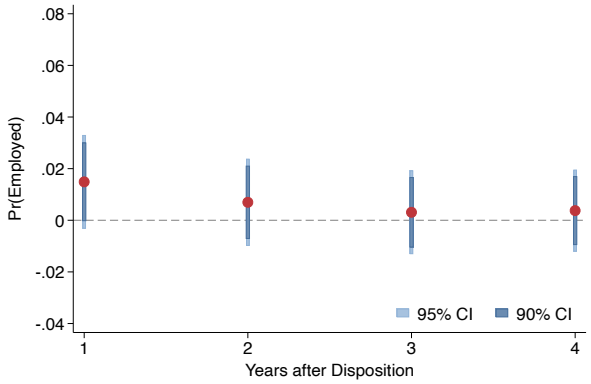
Since the ACS population weights are not designed to be representative for individuals involved in the criminal legal system, we follow the reweighting strategy used in Finlay et al. (2024). We first use covariates to predict the likelihood that individuals in our data are also in the ACS using the same prediction specification used for the predicted outcomes discussed in Figure 3. We then weight observations in our ACS analysis sample by the interaction between these predicted weights and the inverse of 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

Figure 6: Impact dynamics (Cumulative): Recidivism, Employment, Earnings, Poverty Status

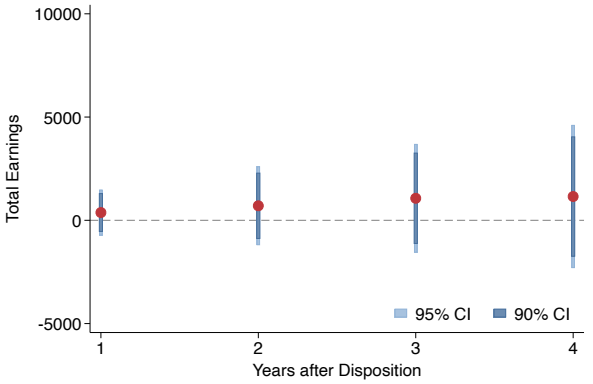
(a) Any New Charge (6–48 months)



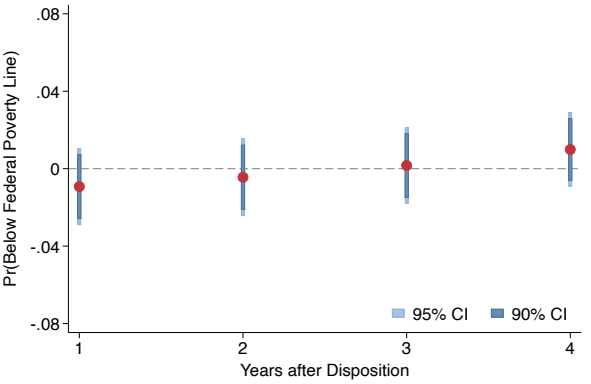
(b) Employment (1–4 years)



(c) Earnings (1–4 years)



(d) Likelihood of Being Below the Federal Poverty Line (1–4 years)



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.

Table A4 presents estimates from applying Equation 2 to the predicted ACS-CJARS weights we construct. We find no evidence of a discontinuity, suggesting that defendants on either side of the cut-off are comparable even in the ACS sample.

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 A4 suggest that the reforms increased employment and earnings from self-employment.[25] Panel C of Table A4 reports the ITT estimates on safety net usage and mental well-being, suggesting that defendants are less likely to receive any welfare benefits or Supplemental Nutrition Assistance Program benefits, consistent with improvements in their overall labor market prospects.[26] Finally, our estimates suggest that the reforms reduced the likelihood of

[25] Reassuringly, the direction of the ITT on employment 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.

[26] The estimated impact on total welfare income increases, but is quite imprecise.

reporting a mental disability, a proxy for mental-well being, although this is quite imprecise.[27]

Impacts on family members

Moving beyond the focal defendants, we also estimate the impacts that the reforms had on defendants' spouses, romantic partners, and children. Panel a) of Table A5 presents the estimated effect of the reforms on the three year recidivism, average annual employment, average annual earnings, and Medicaid receipt (as measured in the ACS) of spouses and partners of the defendants in our sample, finding that [Undergoing disclosure avoidance review.] . We focus on the outcomes measured in the ACS for the children of defendants, since most of them are not old enough to appear in our administrative crime or labor market data. Panel b) of Table A5 displays the reforms' impact on school enrollment and mental well-being of defendants' children.

Robustness

Our results are robust to different specification choices. Figure A3 presents how the estimates for the first stage and main outcomes for 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. Our estimates are similar across all specifications except the estimates are much larger, significant, and sometimes have the opposite sign in the final specification. We interpret this as evidence that the final specification is mis-specified due to a 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 enough specification to yield misleading evidence of a discontinuous change where there truly is none. Finally, Figure A4 presents RDD estimates using alternative bandwidths from 120–360 days in increments of 30 days. We see that [Undergoing disclosure avoidance review.] .

Interpreting the impacts of the reforms

Taken together, our estimates suggest that the budget reforms and changes in DA 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 outcomes 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 potential reason 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

[27] 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.

they are less willing to hire workers with convicted rather than dismissed misdemeanor charges (Agan et al. 2024*b*) and an audit study found that employers almost never asked about non-convictions in a sample of job applications (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. 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. 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.

A more plausible reason for the estimates we find is that the policy reforms that we study were not targeted towards 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.[28] As a result, they may not have had capacity to target dismissal toward individuals who might benefit the most. In contrast, decision-makers in the contexts of diversion or non-prosecution may have intentionally targeted treatment to particular types of individuals.

5.4 Compliers, heterogeneity, and targeting

Below, we investigate how these reforms were targeted by first characterizing the compliers of the reform. We then examine whether the reforms had larger impacts for subgroups that would be particularly likely to benefit from avoiding the criminal legal system. Finally, we use the geographical and contextual variation across the reforms to understand the association between characteristics of the marginal defendant and treatment effects. 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.[29]

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

[28] Indeed, Table A1 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.

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

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 that of the average defendant, which quantifies how compliers are selected relative to the general sample.

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
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)	1.271	0.593	46.6
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.

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 who is well past the peak of 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, 60% of compliers were employed, earning $12,440, which represents a 15% 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 50–65% more likely to be charged with criminal traffic and public disorder 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 disorder conviction is unlikely to generate 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. 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 65% 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.

While the reforms chiefly affected defendants who are unlikely to benefit from avoiding a misdemeanor conviction, we investigate whether we see larger impacts on subgroups that prior evidence suggests should be responsive. Figure 7 presents impacts of the reform on conviction rates as well as our main outcomes for subgroups defined based on criminal history, age, and their complements: first-time defendants, defendants younger than 31 (median age), and defendants with above median predicted earnings.[30, 31]

In Panel a), we see that the reform-induced drop in conviction rates is precise and large across these subgroups. Consistent with the fact from Table 4 that older defendants are over-represented among the compliers, the first-stage is nearly 50% greater for defendants aged 32 or older than for younger defendants (11.9 p.p. vs 8.2 p.p.). Panel b) shows that the impacts of the reform on recidivism 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.[32]

While the impacts on labor market outcomes by subgroup are generally imprecise, Panels c) through e) of Figure 7 provide suggestive evidence that the reforms improved outcomes for individuals with better labor market prospects. For defendants with above median predicted earnings, the reforms increase average annual employment by 1.2 p.p. (1.4% of the baseline mean), earnings

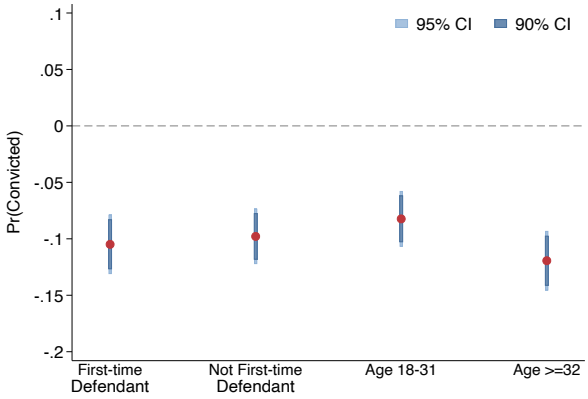
[30] We split defendants based on the median value of the predicted earnings measure discussed in the context of Figure 3.

[31] Table A6 and Table A7 present RDD estimates for other subgroups defined by gender and interactions between first-time defendant and being above or below the median age.

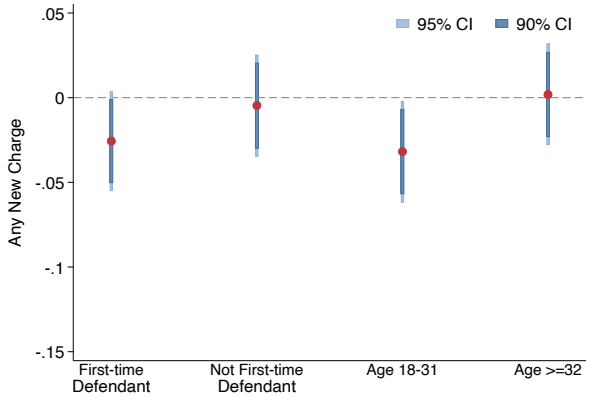
[32] Table A6 shows that these recidivism impacts are largest for first-time defendants who are also young—for this subgroup, the reforms reduce the likelihood of a new charge within three years by 4 p.p. (12%).

Figure 7: Impact of policy reforms by subgroup

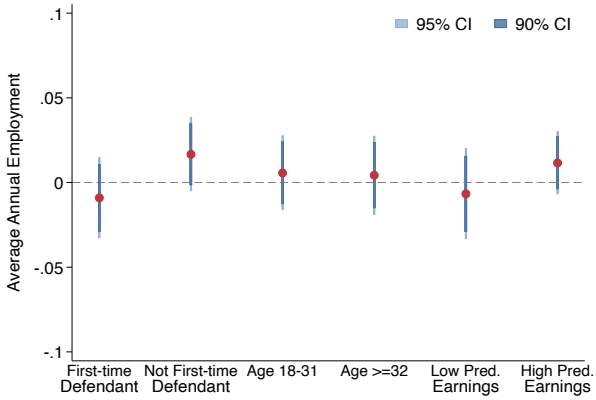
(a) Probability of Conviction



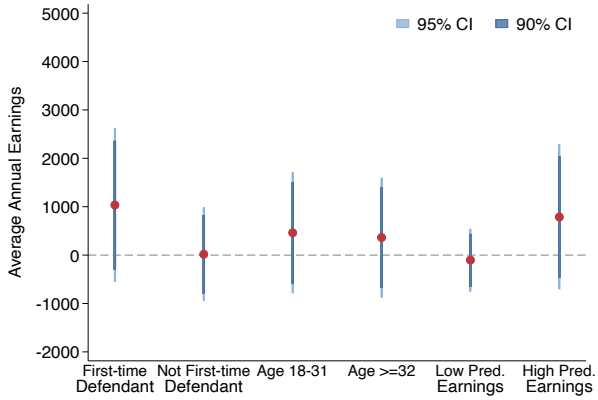
(b) Any New Charge (3 years)



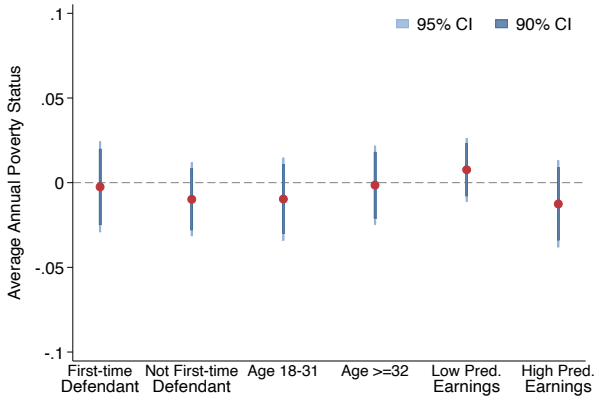
(c) Average Annual Employment (3 years)



(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. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R13065.

by $787 (2.9%), and reduce the average annual poverty status by 1.3 p.p. (3.3%). In contrast, for defendants with below median predicted earnings, the ITTs suggest *reductions* in average annual employment and earnings by 0.7 (1.7%) and $102 (2.7%) respectively, and an *increase* in average annual poverty status by 0.8 p.p. (0.9%). The labor market outcome ITTs do not display consistent patterns across the other subgroups.

Using cross-reform variation to understand targeting

Our results thus far document that the reforms were not well-targeted 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). 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.

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, and the local average treatment effect (LATE) of conviction.[33] 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 also assess the characteristics highlighted by our discussion of Figure 7: the share of compliers that are first-time defendants and 31 years old or younger.

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

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. The extent to which these implications line up with the patterns in Figure 7 also builds confidence in our assessment of the most responsive subgroups. 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.

Table 5 displays the slope and intercept coefficients from such regressions, separately for each complier characteristic, where each LATE measures the impact on compliers of obtaining a conviction. Panel A presents estimates from regressions of the LATE for each outcome against the CCM for that outcome. Focusing on Column 1, the intercept suggests that in settings where the average

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

Table 5: 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.

complier, if not convicted, would have no charges filed against them in the future, conviction is associated with an increase in recidivism. As the likelihood of potential recidivism for compliers increases, the adverse impact of conviction decreases. This pattern is consistent with the large body of evidence documenting that criminal legal interactions harm defendants with shorter criminal histories. Moving to Column 3, the estimates suggest that conviction has no earnings impacts when the marginal defendant has zero potential earnings but that an increase in marginal defendants' potential earnings is associated with an earnings penalty from conviction. While imprecise, the patterns for employment and poverty status (Columns 2 and 4) mirror these patterns.

The patterns in Panel A of Table 5 provide suggestive evidence that the impacts of a misdemeanor conviction depend on the average potential outcomes of the marginal defendant. Even so, these patterns alone are of limited use to policy-makers—decision-makers in criminal misdemeanor systems do not observe each defendant's potential outcomes. To address this concern, Panels B and C of Table 5 investigate whether there are observable characteristics of defendants that display similar patterns. Focusing on Panel B, the estimates suggest that in settings with no young defendant compliers, conviction is associated with improved employment and earnings, but that as the share of compliers who are young increases, conviction is associated with more adverse impacts on labor market prospects. The corresponding estimates for recidivism and poverty status are consistent with this pattern, but less precise. Moving to the share of compliers who are first-time defendants, the patterns in Panel C are less consistent. The intercepts suggest that when the marginal defendants are all repeat offenders, conviction is associated with LATEs that are indistinguishable from zero for all of these outcomes. An increase in the share of first-time defendants among compliers is

associated with reductions in earnings but *increases* in employment. While imprecise, the slopes for recidivism and poverty status align with the implications of the earnings estimate.

Taken together, the patterns in Figure 7 and Table 5 suggest that understanding the characteristics of the marginal defendant is crucial to forecast the effects of changes in misdemeanor conviction policy. Our results provide suggestive evidence that misdemeanor conviction harms the labor market outcomes, especially earnings, of defendants who are between the ages of 18 to 31, have no criminal history, and have greater potential earnings. Our results also suggest that misdemeanor conviction generates future criminal legal contact for defendants with relatively low 'risk' of recidivism. The correlations in Table 5 suggest that observable 'tags' for such defendants are those with no prior criminal history and young defendants, which is directionally-consistent with the patterns in Figure 7.

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. 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 combines our LATE estimates (re-scaling the ITT by the magnitude of the first-stage) of the effect of reforms that reduced misdemeanor conviction 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). We discuss each component of the MVPF in greater detail below.

Willingness to Pay: Our estimate of willingness to pay includes change in the social cost of future crime (–$17,357) adjusted for offense under-reporting, following Deshpande and Mueller-Smith (2022), and changes in defendant cumulative earnings over three years (–$10,642). Additionally, we include changes to self-employment income (–$6,691), transfers from the Supplemental Nutrition Assistance Program (SNAP) ($1,944) and estimates of non-SNAP welfare transfers (–$1,835). Additional margins such as Medicaid participation and spillover estimates on children's educational attainment and romantic partner's labor supply and criminal behavior are [Undergoing disclosure avoidance review.] . Together, we estimate that the marginal misdemeanor conviction yields a willingness to pay of –$34,581.

Net Government Costs: Misdemeanor convictions generate a suite of costs for the government, including prosecution ($640), investigative costs ($2,023), and potential costs of housing a convicted

and sentenced individual in jail.[34] Similarly, because of the harmful labor market impacts of criminal legal system contact, misdemeanor convictions can also negatively harm the government's budget constraint due to reduced income tax revenue from formal (–$1,064) and self-employment (–$669), 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 $9,075.

Estimated MVPF: Together, we calculate the MVPF of misdemeanor conviction as –3.8 when taking our point estimates as given.[35] Respecting the statistical uncertainty with some of our results and assuming all of our LATE point estimates are in fact zero yields an MVPF of 0 (no social benefit and positive government costs). Nonetheless, the marginal misdemeanor conviction may be socially beneficial if it generates large general deterrence effects that discourage people other than the marginal defendant from additional criminal activity, which are not captured in our above calculations.

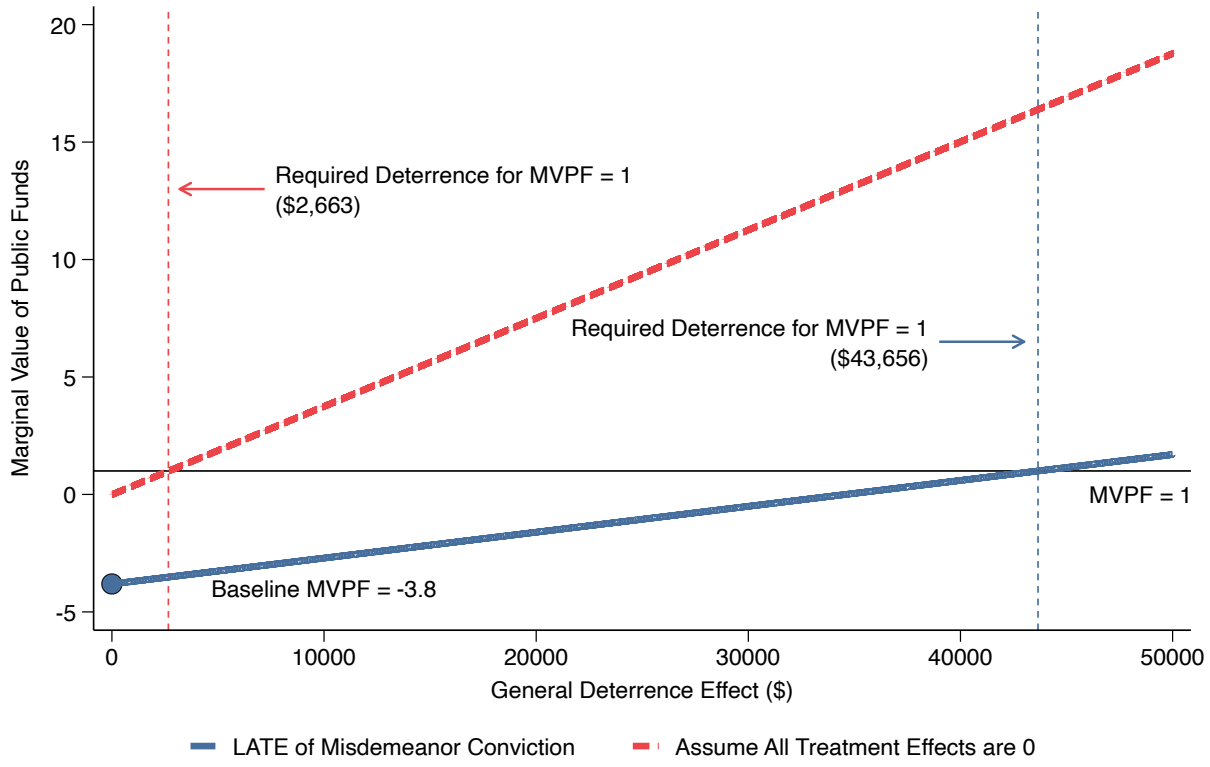
In Figure 8, we explore how our conclusions about the welfare costs of misdemeanor conviction vary over a range of potential general deterrence effects, where the blue the circle at the bottom left denotes the MVPF estimate of –3.8 when there is no general deterrence effect. Using our baseline LATE estimates, we calculate that the marginal misdemeanor conviction would require $43,656 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.[36] Put differently, this range implies that misdemeanor convictions are socially beneficial if they successfully deter individuals from committing at least one violent crime or between three and 50 property crimes.

[34] Our cost estimates account for differences in investigative and prosecution costs by crime type. 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 changes in SNAP ($1,944) and non-SNAP transfers (–$1,835).

[35] 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.

[36] Figure C1 illustrates how the required general deterrence effect changes when assuming that groups of outcomes (crime, labor market, social programs) are non-zero.

Figure 8: 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 prosecution 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. The MVPF when general deterrence effects are zero is the MVPF of misdemeanor conviction in the absence of any general deterrence effects.

7 Conclusion

In the United States, misdemeanor charges represent approximately 75% of the annual criminal legal system caseload (Stevenson and Mayson 2018). Given the overwhelming scale of the misdemeanor system and stigma often associated with criminal records, several District Attorneys in the United States have recently scaled back prosecution of certain non-violent offenses. Quantifying the net benefit of such reforms has proven challenging since the existing causal evidence on misdemeanor prosecution and conviction is limited to defendants' recidivism outcomes. However, misdemeanor records may impact defendants as well as their families along many other dimensions.

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, mortality, and mental well-being. First, we use event studies to document how labor market outcomes and poverty status evolve around misdemeanor charges and convictions for over three million defendants in 11 states. Our estimates suggest that interacting with the misdemeanor system is associated with meaningful labor market penalties that are smaller than those found in prior work using this strategy in other contexts (Rose 2021; Agan et al. 2024*b*). A key reason for this difference is that penalties vary across jurisdiction—our mean estimates mask considerable cross-state heterogeneity. Additionally, our event study estimates suggest that the misdemeanor conviction penalty is 7–30% greater than the charge penalty and that the marginal conviction penalty is what varies the most across states.

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 across five states. Each natural experiment, generated by unanticipated budget cuts or changes in prosecutorial priorities, yields a regression discontinuity design. We find that the reforms had a large impact on conviction rates, reducing the likelihood of conviction by 10 p.p. (12% of the pre-reform mean). In contrast, the reforms had muted impacts on recidivism and labor market outcomes. Our estimates can rule out changes greater than –9% to 1.7% in the likelihood of recidivism, –1.8 and 3.1% in annual employment rates, and –3.2 and 7.5% in average annual earnings. We find suggestive evidence that the reforms reduced reliance on the safety net and improved mental well-being.

An important reason for the muted impacts is that the reforms were not precisely targeted towards individuals who would benefit the most 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. While the reforms may not have been targeted based on treatment effects, they had larger impacts for the complement of the subgroups highlighted above. We find that the reforms significantly reduced future criminal legal interactions for first-time defendants and defendants between the ages of 18 and 31, but had no significant impact on their labor market outcomes. We also find suggestive evidence that the reforms improved labor market outcomes for defendants with above median predicted future earnings.

Since the policy reforms in each setting occurred due to different reasons, we also use cross-experiment variation in targeting quality to identify observable 'tags' for people most likely to be affected by a misdemeanor conviction. We find suggestive evidence that the impacts of a misdemeanor conviction are especially adverse when defendants have low potential recidivism or greater potential labor market earnings. Additionally, greater shares of defendants between the ages of 18 to 31 or first-time defendants among compliers are associated with more adverse impacts of a misdemeanor conviction.

Finally, our MVPF calculations suggest that the marginal misdemeanor conviction generates substantially less social value than the government costs incurred, unless misdemeanor conviction generated considerable general deterrence. Incorporating the prospect of general deterrence into the MVPF calculation, the marginal misdemeanor conviction must prevent at least one violent crime or between three and 50 property crimes from occurring to yield an MVPF of one.

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 affects a broad range of outcomes for defendants and families. An important caveat is that our results only speak to the outcomes that we are able to 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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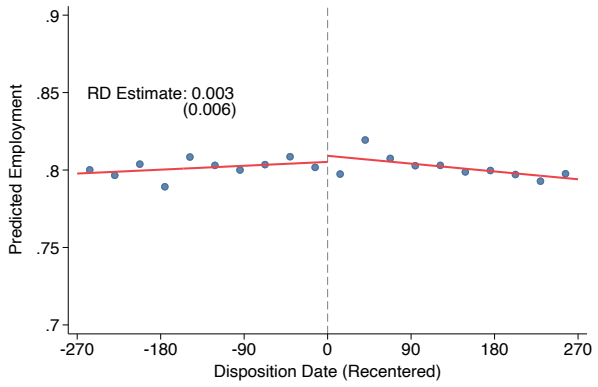
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Appendix A: Additional Results

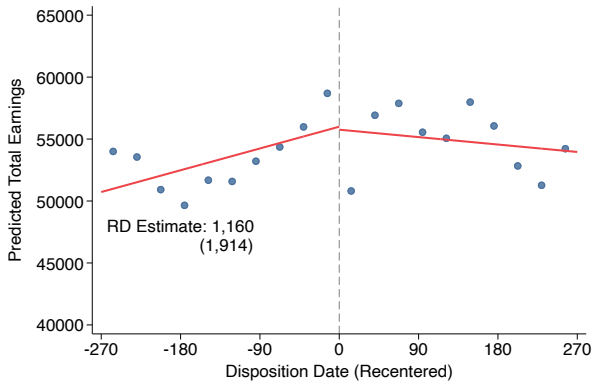
Figures

Appendix Figure A1: Validation: Predicted Cumulative Labor Market Outcomes over Three Years

(a) Employment



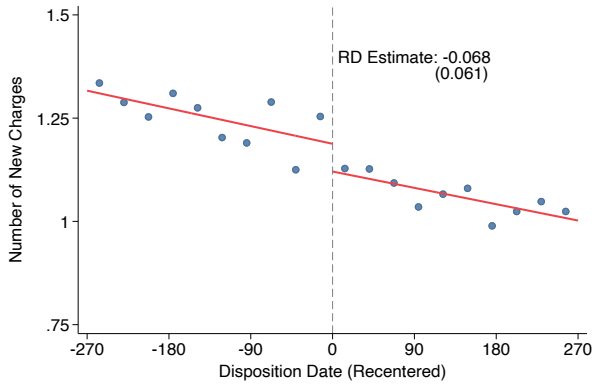
(b) Earnings



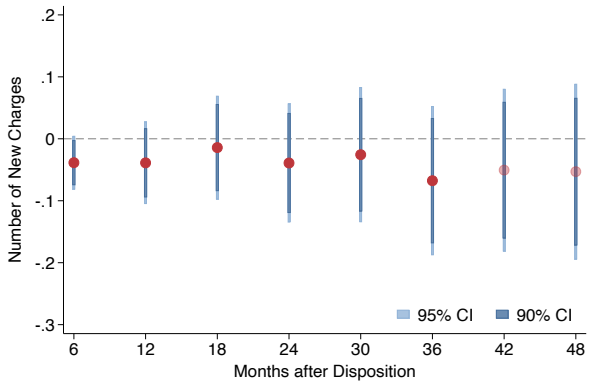
Note: Each Panel presents RDD estimates using a version of Equation 2 that excludes covariates. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890.

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

(a) Cumulative (3 years)



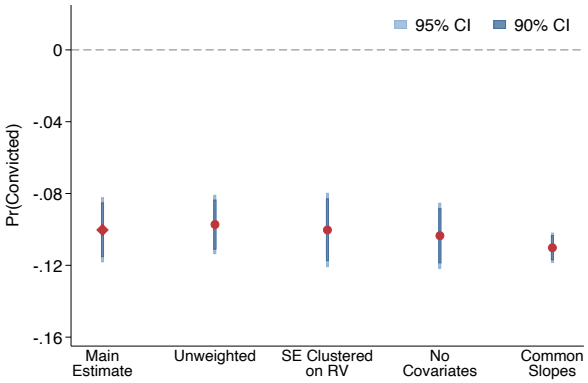
(b) Dynamics (6-48 months)



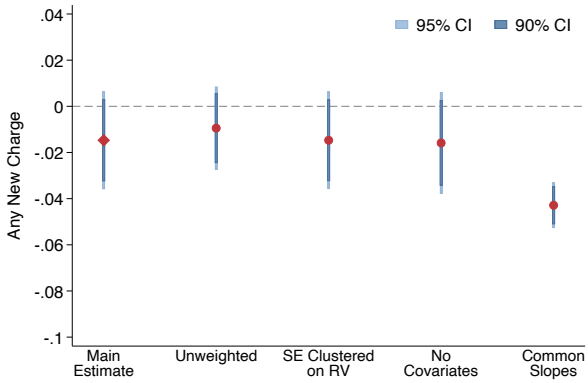
Note: Panel a) presents RDD estimates for the total number of new charges filed over three years using the stacked sample and Equation 2. Panel b) presents RDD estimates for different time horizons. Hollow dots indicate outcomes for which missing follow up information is imputed with zeros. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890.

Appendix Figure A3: Assessing robustness to specification choices

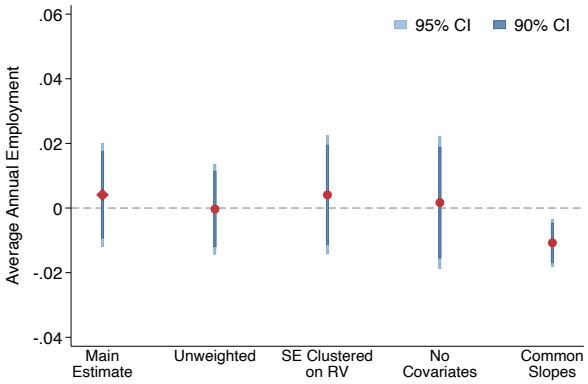
(a) Pr(Convicted)



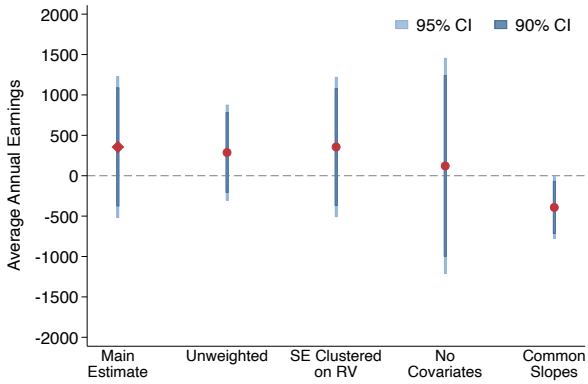
(b) Any New Charge (3 years)



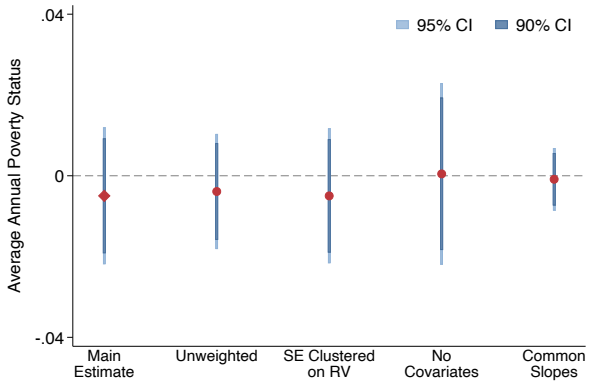
(c) Average Annual Employment (3 years)



(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-R12890.

Appendix Figure A4: Assessing robustness to choice of bandwidth

(a) Any New Charge (6–36 months)

[Undergoing disclosure avoidance review.]

(b) Employment (1–3 years)

[Undergoing disclosure avoidance review.]

(c) Earnings (1–3 years)

[Undergoing disclosure avoidance review.]

(d) Likelihood of Being Above the Federal Poverty Line (1–3 years)

[Undergoing disclosure avoidance review.]

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.

Tables

Appendix Table A1: 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.

Appendix Table A2: 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	−0.039*	0.299
	(0.008)		(0.022)	
	[0.076]		[0.107]	
12	−0.012	0.234	−0.039	0.554
	(0.010)		(0.034)	
	[0.135]		[0.219]	
18	−0.008	0.294	−0.014	0.788
	(0.010)		(0.043)	
	[0.179]		[0.355]	
24	−0.015	0.332	−0.039	0.967
	(0.011)		(0.049)	
	[0.207]		[0.444]	
30	−0.010	0.359	−0.026	1.125
	(0.011)		(0.056)	
	[0.229]		[0.546]	
36	−0.015	0.381	−0.068	1.271
	(0.011)		(0.061)	
	[0.250]		[0.593]	
42	−0.014	0.396	−0.051	1.381
	(0.011)		(0.067)	
	[0.268]		[0.682]	
48	−0.013	0.407	−0.053	1.471
	(0.011)		(0.072)	
	[0.280]		[0.748]	

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.

Appendix Table A3: Dynamics, RD Estimates: Labor Market Outcomes

Outcome Horizon (Years)	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)
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: '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.

Appendix Table A4: 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			
Predicted ACS Weight	2.947	22.880	
	(2.385)		
Panel B: Labor Market Outcomes			
Employment	0.080	0.615	0.948
	(0.065)		
Earnings			
Average Poverty Status			
Self-employment Income	224	1,131	0
	(1,785)		
Health Insurance through Employer/Union			
Panel C: Safety Net Usage and Mental Well-being			
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)		
Publicly-provided Health Insurance			
Reported Mental Disability	−0.018	0.079	0.067
	(0.042)		

Note: 'Predicted ACS Weight' is obtained by using covariates to predict the likelihood that individuals in our sample are in the ACS, following Finlay et al. (2024). We use the interaction of the ACS population weights and the predicted 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.

Appendix Table A5: Impact of policy reforms on family members

[Undergoing disclosure avoidance review.]

Note: Results approved for release by the U.S. Census Bureau, authorization number X.

Appendix Table A6: 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.

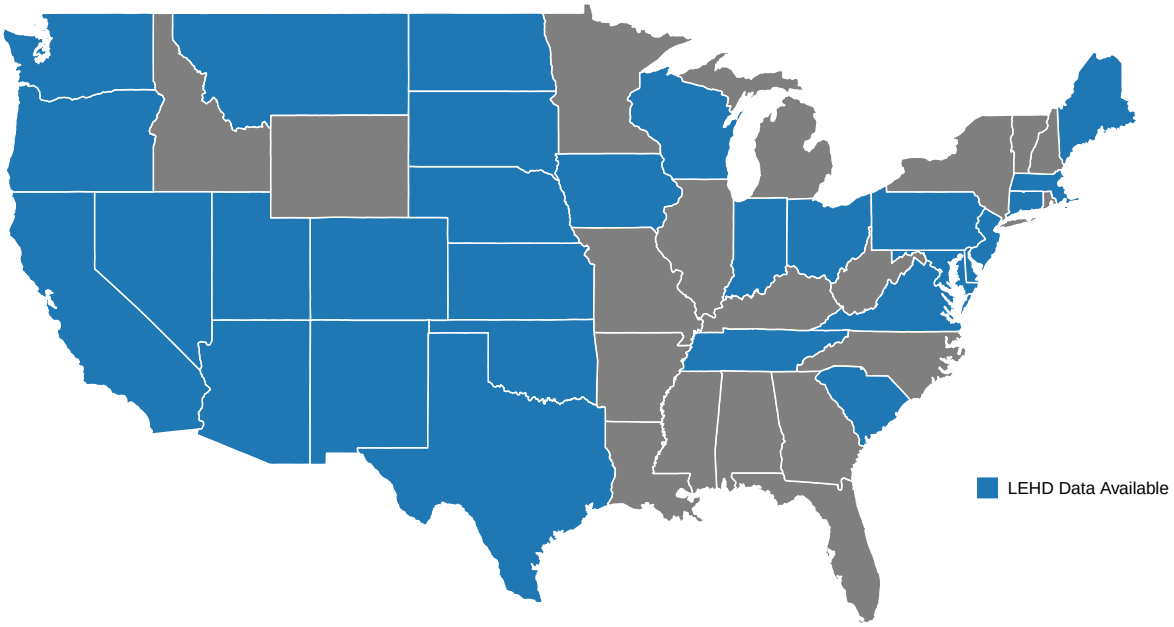
Appendix Table A7: Heterogeneity Analyses: Main Labor Market Outcomes

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

Appendix Figure B1: Coverage of LEHD states



Appendix 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: 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 current 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 under-reporting 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 $17,357.

2. Earnings: Our estimate in the main text suggests that misdemeanor conviction decreases formal labor market earnings by $10,642 over the following three years. Using survey-based self-employment income from the American Community Survey, we estimate that misdemeanor conviction depresses self-employment earnings by $6,691.

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 $1,835. 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 12 months of receipt. This calculation yields an estimated increase of $1,944.

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

1. Mechanical change in judicial costs: Conviction of misdemeanor offenses invokes government administrative costs of prosecution and investigation that vary with offense type (Caulkins

[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 each of these are zero.

2010; The Vermont Center for Justice Research 2014; Hunt, Anderson, and Saunders 2017; Hunt, Saunders, and Kilmer 2019). Using a similar distributional-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.

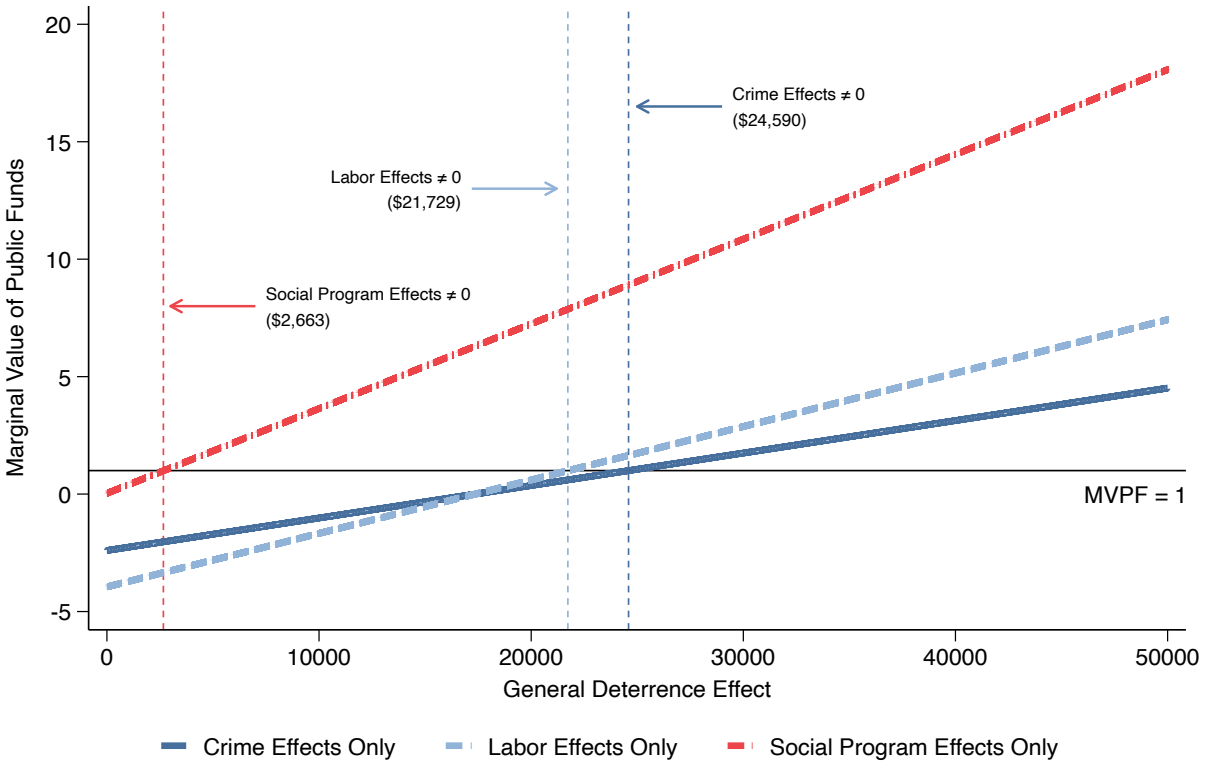
2. Behavioral change in judicial costs: Because misdemeanor conviction also changes future criminal recidivism, the prosecution and investigative expenditures are affected by behavioral changes of defendants. To calculate this component, we multiply the change in future recidivism by the type of crime-weighted prosecution and investigative costs, 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 $669 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 ($1,944) and decreases in non-SNAP program transfers ($1,834).

Putting these two pieces together, we estimate negative social benefits of –$34,581 and positive government costs of $9,075, per misdemeanor conviction. Taking the ratio implies a Marginal Value of Public Funds of –3.8.

Appendix Figure C1: 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 prosecution 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. The MVPF when general deterrence effects are zero is the MVPF of misdemeanor conviction in the absence of any general deterrence effects.