The Socioeconomic Consequences of Misdemeanor Conviction*

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Abstract

This paper provides evidence on the impacts of misdemeanor prosecution in the United States 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. We combine a rich linkage of administrative and survey data with two complementary research designs. First, we estimate event studies to document how employment, earnings, and poverty status evolve around misdemeanor charges and convictions for defendants in 11 states. Second, we estimate the causal impact of a misdemeanor conviction (relative to being charged) by combining natural experiments from seven counties across five states. Each natural experiment yields a regression discontinuity design generated by unanticipated budget cuts or a changes in prosecutorial priorities that reduced misdemeanor conviction rates. We use variation in the estimated characteristics of the marginal defendant across counties to identify observable characteristics associated with adverse impacts of a misdemeanor conviction. Finally, we use our causal estimates to calculate the welfare effects of misdemeanor convictions under a range of assumptions.

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1 Introduction

In the United States, around 13 million misdemeanor charges (e.g., shoplifting, drug possession, disorderly conduct) are filed per year, representing 75% of the annual criminal legal system caseload and by far the most common form of police contact (Stevenson and Mayson 2018). Differences in leadership preferences and fiscal constraints across local governments result in enormous variation in conviction rates and the types of misdemeanor offenses that are prioritized across jurisdictions.[1] Recognizing the overwhelming scale of the misdemeanor system and stigma often associated with criminal records, District Attorneys in the United States have recently scaled back prosecution of certain non-violent offenses (Agan et al. 2025; Amaral, Ouss, and Ozier 2025), prompting debates about the net benefit of such policies (Cramer 2018; Tanenbaum 2020; Walters 2021). While a key input to understanding the social value of misdemeanor prosecution policies is quantifying how it impacts the lives of defendants and the people around them along multiple dimensions, we have limited causal evidence on its effects beyond recidivism (Agan, Doleac, and Harvey 2023).

There is good reason to think that the effects of misdemeanor prosecution could extend beyond the criminal legal system and beyond the defendants themselves. 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.[2] 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, any direct impacts of misdemeanor prosecution could alter the environments surrounding children and other family members in ways that are potentially 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). Given differences in the criminal legal systems and government institutions across states and counties, these effects might vary by setting, mirroring 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 safety net usage, mortality, and mental well-being. We use a rich linkage of administrative and survey data that combine records from the Criminal Justice Administrative Records System (CJARS), the Longitudinal Employer-Household Dynamics (LEHD), the American Community Survey, Decennial Censuses, and the Census Household Composition Key (CHCK). First, following Rose (2021) and Agan et al. (2024*b*), we estimate

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

[2] See Doleac (2023) for a review of the evidence on potentially helpful interventions, including for individuals charged with minor offenses.

[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 vary widely by country (Bertheau et al. 2023), in large part due to difference in social norms and the structure of public institutions.

event studies to document how employment, earnings, and poverty status evolve around misdemeanor charges and convictions for defendants in 11 states in the United States.[4] We then estimate the causal impact of a misdemeanor conviction (relative to being charged) using a regression discontinuity design generated by a variety of local reforms that reduced conviction rates in seven counties across five geographically diverse states. We then use this causal evidence to conduct expansive welfare analyses of misdemeanor prosecution. Our main samples consist of defendants charged with any type of misdemeanor offense.

The local reforms that provide the identifying variation for our causal analysis are unanticipated budget cuts and changes in prosecutorial priority, which sharply reduced misdemeanor conviction rates in seven counties and cities between 2008 and 2016. These reforms affected jurisdictions in Arizona (Pima County), Florida (Orange County), North Dakota (Williams County), Virginia (Newport News City), and Wisconsin (Dane, Milwaukee, and Outagamie Counties). Most of the reforms involved budget cuts to misdemeanor units, either by reducing prosecutor resources and administrative staff at the District Attorney's office or by reducing funding for administrative staff at courts that hear misdemeanor cases. In contrast, prosecutors at Williams County and Newport News City shifted their priorities toward prosecuting serious drug offenses after joining a federal effort to combat drug trafficking, a deviation which diminished residual capacity for pursuing misdemeanor cases.

We use the sharp nature of these policy reforms in a regression discontinuity design (RDD) framework, comparing the outcomes of defendants whose cases were disposed just before versus just after the reform was announced or implemented.[5] 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, even though they 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 conviction, this behavior would be equally reflected in the outcomes for individuals on either side of the cut-off.

While the reform reduced the likelihood of conviction by 10 p.p. (12% of the pre-reform mean), we do not find evidence that the reforms changed criminal recidivism or labor market outcomes over three years. Our estimates are relatively precise, ruling out changes greater than –9% to 1.7% in the likelihood of recidivism, –1.8 and 3.1% in quarterly employment rates, and –3.2 and 7.5% in average quarterly earnings. We find some evidence that outcomes improve in the short run but the improvements do not persist. Defendants whose cases were 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. These effects dissipate quickly, with smaller and less precise point estimates for longer horizons.

A potential reason for the muted impacts is that the types of individuals shifted out of convic-

[4]Note that the event study estimates are currently undergoing disclosure avoidance review.

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

tion due to the reform are less likely to be impacted by a misdemeanor conviction. Compliers in our setting, defendants shifted from conviction to dismissal, have lower potential recidivism than the average defendant. While the average defendant in our sample has relatively low employment and earnings, compliers have even lower levels of formal labor market attachment. These margins of selection have opposite implications for the effect of a misdemeanor conviction. On one hand, interactions with the criminal legal system tend to harm defendants with low baseline levels of criminal activity and shorter criminal histories (e.g., Agan, Doleac, and Harvey 2023; Kamat, Norris, and Pecenco 2023). On the other hand, individuals with lower labor market attachment may be less likely to experience adverse labor market impacts of criminal legal interventions (e.g., Garin et al. 2023).

In results currently undergoing disclosure review, we use the variation in characteristics of the marginal defendant across the seven natural experiments that we study to identify observable tags for individuals who might be likely to benefit the most from avoiding a misdemeanor conviction. Using those analyses as a guide, we re-estimate the RDDs for subgroups of our main sample and see if the effect of a misdemeanor conviction is larger for some types of defendants than others.

Finally, to understand the welfare consequences of budget cuts and priority changes that scale back misdemeanor conviction, we cast the estimates we have currently disclosed in a Marginal Value of Public Funds (MVPF) framework (Hendren and Sprung-Keyser 2020; Finkelstein and Hendren 2020). Based on the limited set of estimates we have disclosed, our point estimates taken literally suggest that the policy reforms we study yield an MVPF of infinity, due to both positive social benefits and net negative government costs of non-conviction. As an alternative estimate, we also calculate that if the reforms truly did not shift any of the outcomes we measure, the marginal misdemeanor conviction must deter enough criminal activity to avoid at least $3,250 worth of victim costs to achieve an MVPF of one.[6]

While there is a large literature studying the causal effects of interacting with the criminal legal system, most prior work has focused on the impact of felony records and incarceration. 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*). Evidence on the impacts of incarceration are more mixed. Prior work across multiple U.S. states has typically found limited impacts beyond the effects of incapacitation (e.g., Kling 2006; Garin et al. 2023), although the incarceration penalties seem to vary based on the characteristics of the marginal defendant (Mueller-Smith 2015). There is evidence that even criminal records for minor felonies have adverse impacts. Mueller-Smith and Schnepel (2021) find that avoiding a conviction through diversion greatly reduces recidivism and improves employment outcomes up to 20 years later for low-risk, first-time felony defendants. Thus,

[6]These calculations do not include impacts on outcomes captured in surveys but not our administrative data (e.g., social safety net usage, informal employment, mental well-being) or spillovers onto family members. We anticipate that the MVPFs we calculate and the implications may substantially change as we incorporate these additional estimates, in addition to quantifying heterogeneity in the social value of non-conviction across natural experiments.

avoiding the scarring effects of a criminal record is a key driver for preserving economic stability for individuals on the margin of the criminal legal system.

Even though misdemeanors are relatively minor offenses, surveys of hiring professionals suggest that employers are still concerned about hiring workers with misdemeanor records, albeit to a lesser extent than their concern about felony records (Uggen et al. 2014; Agan et al. 2024*b*). There is a small but growing literature seeking to understand how much of a barrier misdemeanor records pose. Recent work in Massachusetts finds that misdemeanor prosecution can increase future interactions with the criminal legal system, consistent with criminal records generating adverse impacts (Agan, Doleac, and Harvey 2023). Evidence from Ohio finds that conditional on being charged with a misdemeanor, conviction increases future criminal interactions especially for those with no felony record, suggesting that the severity of a record also negatively affects individuals (Kamat, Norris, and Pecenco 2023). Using an event study framework, Agan et al. (2024*b*) find that misdemeanor charges and conviction are associated with persistent labor market penalties in Maryland, Pennsylvania, and Texas, while Rose (2021) documents similar patterns for individuals convicted of serious misdemeanors in Washington State using a similar strategy.

This paper contributes to the literature in several ways. First, we build on existing work documenting how misdemeanor adjudication is associated with large labor market penalties by providing evidence from a broader range of geographies (Rose 2021; Agan et al. 2024*b*). Our sample of misdemeanor defendants from 11 states that cover 28% of the U.S. population lets us precisely quantify how much the labor market penalties of misdemeanor prosecution vary across states.

Second, we provide causal evidence on how a misdemeanor conviction affects defendants and their families in terms of a broad range of socioeconomic outcomes beyond criminal recidivism. Our combination of administrative and survey data, along with our sample of individuals from five states representing 14% of the U.S. population, lets us conduct more expansive welfare analyses than would have been possible in prior work. Failing to capture effects of misdemeanor conviction on non-recidivism outcomes and family members may misrepresent the welfare effects of misdemeanor conviction in either direction. Our current criminal recidivism and labor market estimates suggest that a conviction has limited impacts on the average misdemeanor defendant. While this finding might be consistent with the charging decision having a larger impact than conviction, it is plausible that conviction might still be harmful for certain defendants, e.g., those without a prior criminal record or with higher levels of labor market attachment.

Third, 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. We use the cross-experiment variation in the characteristics of the marginal defendant generated by differences across contexts to identify observable tags for the types of individuals whose socioeconomic trajectories might be the most responsive to a misdemeanor conviction.

2 Institutional Background and Settings

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 offences like homicide, robbery, burglary, and drug distribution. Sanctions for these felony convictions often include lengthy prison sentences in state correctional facilities. On the other hand, misdemeanor charges represent generally minor offences, 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 35 and [Undergoing disclosure avoidance review.] percent will have a conviction.

In addition to their widespread nature, conviction rates for misdemeanor offenses vary widely across jurisdictions. In the United States from 2008 to 2019, the mean misdemeanor conviction rate at the jurisdiction-year level is 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.[7] 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 that misdemeanor convictions lead to 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 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 can be welfare-improving.

[7] 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.

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 offences. 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	Change in practice	Prosecutor
Newport News City	Virginia	October 10, 2014	Change in practice	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 prevent 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, Erica 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 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). District Attorneys were pessimistic about their requests and likely 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).[8] 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 'High Intensity Drug Trafficking' (HIDTA) program (Grand Forks Herald 2013; White House 2013; Federal Register 2014). HIDTA is a federal program that 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 offences. We use the announcement of the HIDTA designations as the effective dates.

3 Data Infrastructure

We link numerous individual-level administrative and survey datasets, covering multiple U.S. states, available through the Federal Statistical Research Data Center (FSRDC) system. These data

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

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.[9] 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. The focal sample we use to understand the causal impact of a misdemeanor are 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) dataset. 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. Figure A3 lists the states whose LEHD data we use in this paper as well as which analyses we use them for.

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 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.[10]

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 in-

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

[10] 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).

dividuals 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, 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 associated with interacting with the misdemeanor system across 11 states, following Agan et al. (2024*b*) and Rose (2021). We use this strategy 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. We also assess the extent to which changes in these outcomes vary across states.

4.1 Event study design

We examine how labor market outcome evolve around misdemeanor charges and convictions following the event study approach used by Agan et al. (2024*b*) and Rose (2021). We focus 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 (for the complete list of states and time periods see Table A1). For each individual, we only include their first misdemeanor disposition across all the states we use to ensure that our sample includes only one disposition event per person.[11]

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 of misdemeanor adjudication vary

[11] This restriction does not 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.

between individuals who were charged with an offense and those who were charged and convicted. Y_{ist} represents our main outcomes: employment, earnings, and whether earnings were below 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.

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

Each of the β_k coefficients estimate the average difference in the outcome relative to the quarter before the case is filed. We construct our coefficients to be relative to the quarter of filing to capture any changes in labor market activity while charges are pending.[12] We bin labor market outcomes 17 to 20 quarters before or after a disposition event together, which lets us identify the time-varying age controls as well as the coefficients on the relative time indicators. Finally, following our approach below, we reweight observations in this analysis using the relative sample size of defendants in each state, such 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.

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 precise evidence on the marginal effect of a misdemeanor conviction relative to a misdemeanor charge. We then compare the estimates from these event studies to causal estimates of a misdemeanor conviction on a broad array of outcomes using a regression discontinuity design.

4.2 Descriptive evidence on labor market impacts of misdemeanor convictions

Here, we provide descriptive evidence highlighting the potential for sharp and lasting consequences of contact with the misdemeanor criminal legal system. Using data from 11 states, we estimate how labor market trajectories of individuals charged with and convicted of misdemeanor offenses evolve around their contact with the misdemeanor system. We also use this analysis to understand whether misdemeanor convictions result in an additional labor market penalty compared to charges and how much this varies across the United States.

In Figure 1, we plot how labor market outcomes evolve around a misdemeanor charge, separately for charges that were dismissed versus convicted. [Undergoing disclosure avoidance review.]

Consistent with prior work, we find that [Undergoing disclosure avoidance review.]

[12] Pending charges are typically reported on employer background checks, although there are some notable exceptions, e.g., California and New York (Agan et al. 2024*b*).

Figure 1: Event study: labor market trajectories around the time of misdemeanor charge filing

(a) Any Employment

[Undergoing disclosure avoidance review.]

(b) Quarterly Earnings

[Undergoing disclosure avoidance review.]

(c) Likelihood of Quarterly Earnings Above Federal Poverty Line

[Undergoing disclosure avoidance review.]

Note: This figure presents event study estimates of quarterly labor market outcomes around the time of misdemeanor charges being filed against the individual. Standard errors are clustered at the individual level.

Next, to understand how much the changes in labor market outcomes and the additional impact of misdemeanor conviction relative to a charge varies across geographies, we estimate versions of Equation 1 for each state in our data, again separately for convictions and dismissals. Columns 1 and 2 of Table 2 present the mean and variance of the average post-period change for each outcome across states. We find that [Undergoing disclosure avoidance review.] . Column 3 describes the mean and variance of the difference in post-period outcomes for defendants who were convicted and those who were dismissed. We see that [Undergoing disclosure avoidance review.] . These conviction–dismissal gaps in outcomes do not necessarily reflect the causal estimates of the effect of a misdemeanor conviction relative to a charge. Nevertheless, these estimates likely provide an upper bound on the magnitude of the causal effect and suggestive evidence on how much we might expect the causal impacts to vary across jurisdictions.

Table 2: Variation in event-study estimates across states

[Undergoing disclosure avoidance review.]

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 above 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).

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. $Post_i$ 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). Since all of the natural experiments we study involve a sharp change at a particular point in time, we pool the defendants in each natural experiment e. 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, the main RDD results that we discuss 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 sorting to one particular side of the discontinuity. 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 side 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, which we discuss further 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.[13] For two of the natural experiments (Pima County and Milwaukee County), we exclude cases disposed 6 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.

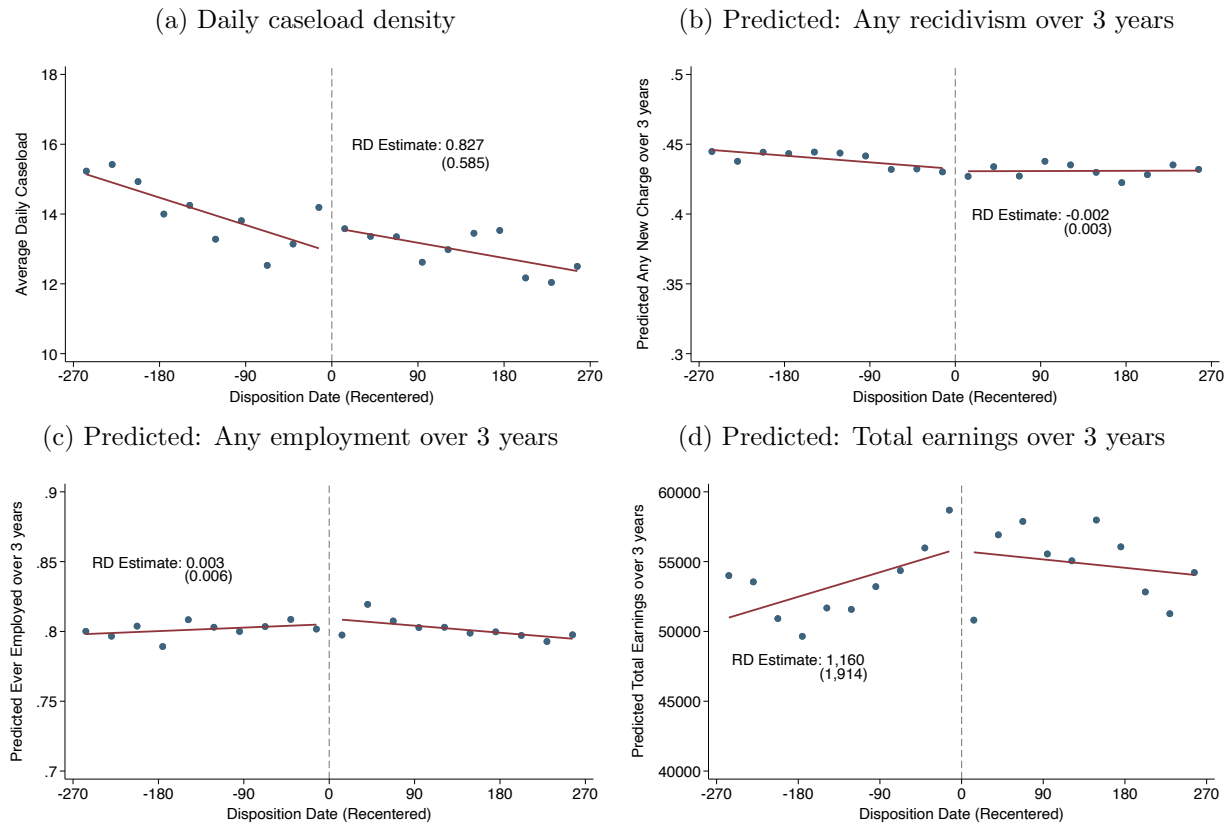
Our main analyses focus on defendants whose cases are disposed around the policy cut-offs. When we move to assessing spillovers on family members of this focal sample, we follow a strategy similar to Equation 2, except the outcome is measured for the family members while the remaining terms stay the 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 2 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 decided to sharply stop filing charges for certain types of offenses. We find that the number of cases disposed is smooth around the cut-off, suggesting limited scope for this type of sorting behavior.

[13] 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.

Figure 2: Validating natural experiments: Stacked analysis



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.

Panels b)–d) present summary evidence that the baseline observable characteristics of defendants are smooth around the cut-off. We first predict recidivism, employment, and total earnings three years after disposition using two-way interactions of the covariates we include in our main analysis.[14] We find no evidence of discontinuities in any of these summary measures.

Table 3 presents RDD estimates from tests of whether each individual baseline covariate is smooth around the cut-off. As shown in Column 2, we see that 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. 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. We include controls for all of these baseline characteristics in our main specification to account for any chance imbalance and improve precision.

Overall, these results suggest that the natural experiments we discuss here provide valid quasi-

[14] Versions of these balance tests for average employment and earnings over three years are currently undergoing disclosure review.

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

Baseline Characteristic	Baseline Mean	RD Estimate	Underlying N
	(1)	(2)	(3)
Demographics and Criminal History			
Age	33.88	0.17	46000
		(0.27)	
Male	0.77	-0.00	46000
		(0.01)	
White (Non-Hispanic)	0.44	-0.02**	46000
		(0.01)	
Black (Non-Hispanic)	0.30	0.02*	46000
		(0.01)	
Hispanic	0.18	-0.00	46000
		(0.01)	
Missing Ethnicity	0.04	0.00	46000
		(0.00)	
Missing Race	0.05	0.00	46000
		(0.00)	
First-time Offence	0.43	0.01	46000
		(0.01)	
Number of Previous Charges			46000
Number of Previous Convictions			46000
Case Characteristics			
Violent Charge	0.09	0.02***	46000
		(0.01)	
Drug Charge	0.13	0.00	46000
		(0.01)	
Property Charge	0.22	-0.01	46000
		(0.01)	
Criminal Traffic Charge	0.29	0.01	46000
		(0.01)	
Public Order Charge	0.39	0.03***	46000
		(0.01)	
Predicted Indices and Caseload Density			
Predicted Cumulative Recidivism (3 Year)	0.44	-0.00	46000
		(0.00)	
Predicted Cumulative Employment (3 Year)	0.80	0.00	36500
		(0.01)	
Predicted Cumulative Earnings (3 Year)	52,530.00	1,160.00	36500
		(1,914.00)	
Average Daily Caseload Density	14.08	0.83	36500
		(0.58)	

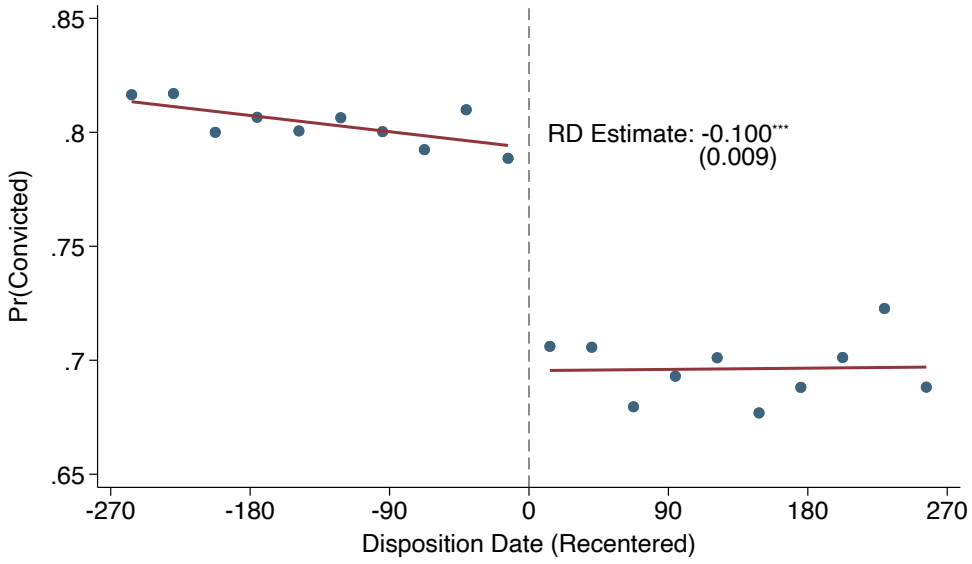
Note: Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890.

experimental variation. Next, we use this variation to understand the impacts of not being prosecuted for a misdemeanor offense.

5.3 Average impact of misdemeanor conviction

Having established that the budget cuts and priority changes that form our identifying variation represent valid RDDs, we now turn to estimating how the reform affected defendants' socioeconomic outcomes and the causal impact of misdemeanor conviction. We first verify that the policy reforms we study actually changed defendants' conviction status. As Figure 3 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).

Figure 3: First stage: Impact of policy reform on probability of conviction



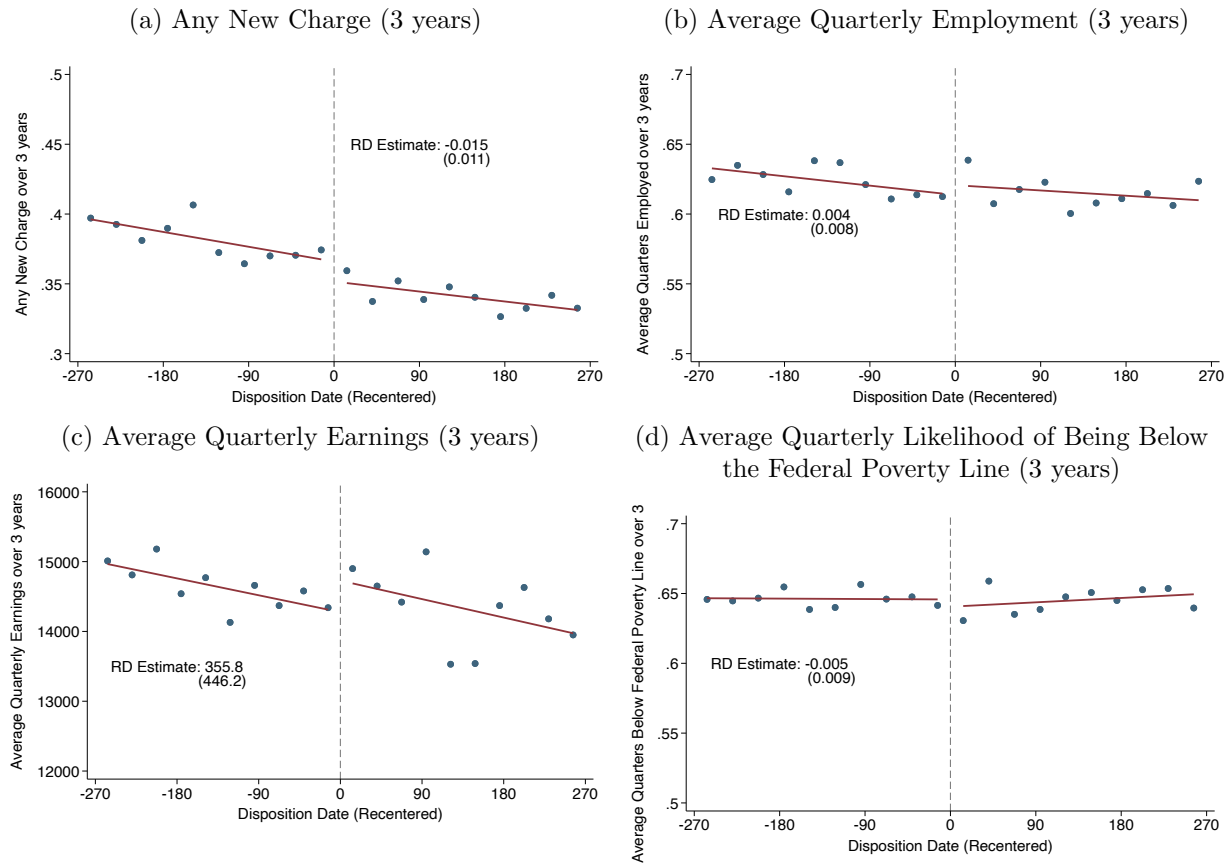
Note: This figure presents the RDD estimate for the likelihood of conviction for any charge using the stacked sample and Equation 2. Standard errors are clustered at the individual level. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890.

While the reform reduced the likelihood of conviction, Figure 4 shows muted impacts of a misdemeanor conviction 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 employment over three years. Similarly, the estimates in Panels c) and d) can rule out changes in average earnings greater than –3.2% and 7.5% and changes in average poverty status between –3.5% and 1.9%.[15]

Figure 5 presents how the impacts of the reforms on our main outcomes evolve over time. In each panel, we plot the RDD estimate for the cumulative outcome a certain amount of time after disposition. We present these estimates for up to four years after disposition, imputing zeros in year

[15]Estimates undergoing disclosure review examine the impacts of the reforms on employment, safety net usage, and mental well-being using data from the ACS. Since the ACS is not a panel, we pool ACS responses over multiple years, controlling for the response year. The resulting estimates can be interpreted as impacts on averages over the three years after disposition. We find [Undergoing disclosure avoidance review.] .

Figure 4: Impact of policy reforms on defendant crime and labor market outcomes



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

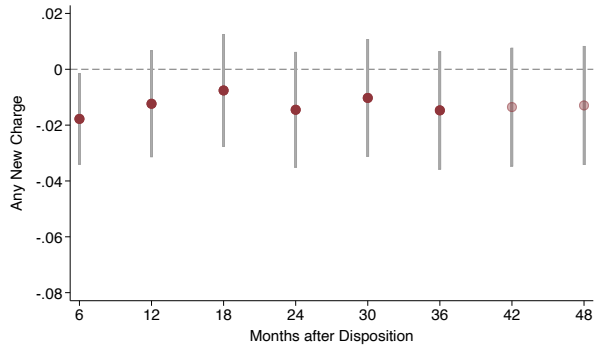
four for individuals in counties where we only have three years of follow-up data.[16] We measure recidivism outcomes in 6-month increments and labor market outcomes in annual increments. In Panel a), we find that the reforms reduce recidivism in the 6 months after disposition, but the effect is smaller and less significant as time goes on, moving from –1.7 p.p. ($p = 0.033$) in the first 6 months to –1.3 p.p. ($p = 0.23$) after four years. 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. ($p = 0.11$) shrinks to just 0.4 p.p. ($p = 0.64$) after four years.

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.

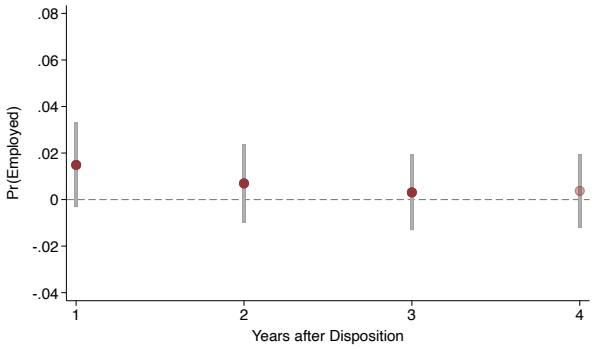
[16] Since court records for Wisconsin end in 2019, we do not observe recidivism outcomes beyond 3 years for defendants in the Wisconsin counties.

Figure 5: Impact dynamics: Recidivism, Employment, Earnings, Below Poverty Line

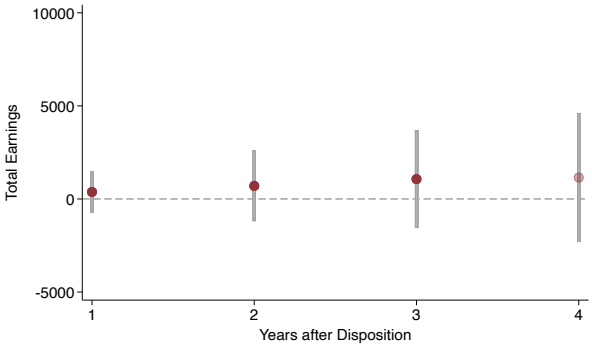
(a) Any New Charge (6–36 months)



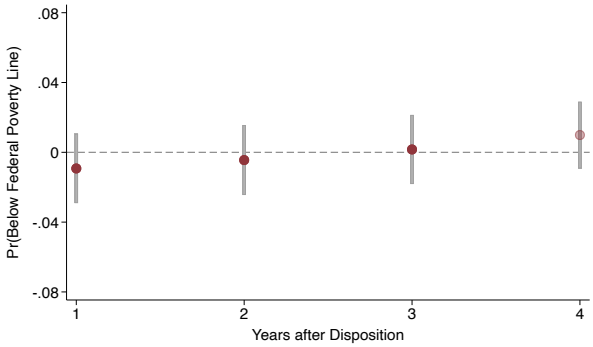
(b) Employment (1–3 years)



(c) Earnings (1–3 years)



(d) Likelihood of Being Below the Federal Poverty Line (1–3 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.

Our results are robust to different specification choices. Figure A1 presents how the estimates for the first stage and main outcomes 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 and makes a strong homogeneity assumption. While each natural experiment may 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. Figure A2 presents RDD estimates using alternative bandwidths from 120–360 days in increments of 30 days. We see that [Undergoing disclosure avoidance review.] .

Overall, these 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. There are two potential reasons for this. First, we include all misdemeanor defendants in our sample. Whether a misdemeanor conviction is harmful, beneficial, or has no impact might vary by defendant characteristics, e.g., age, criminal history, prior labor market attachment.[17] The second potential reason for our muted findings is that the marginal effect of being convicted of a misdemeanor conditional on being charged may be zero. Agan, Doleac, and Harvey (2023) find that avoiding misdemeanor prosecution reduces future criminal interactions for up to six years, especially for defendants with no criminal history. They argue that the key mechanism underlying their finding is that individuals not prosecuted avoid a criminal charge and thus avoid having a criminal record show up on an employer background check. If most employers refuse to hire workers with a criminal charge regardless of conviction, then we should expect to see no impacts of a misdemeanor conviction above and beyond the effect of a charge. However, surveys of employers suggests that they less willing to hire workers with convicted rather than dismissed misdemeanor charges (Agan et al. 2024*b*). Next, we assess which of the potential explanations is responsible for the patterns above.

5.4 Understanding compliers and heterogeneity

So far, our results suggest that the budget cuts and priority changes caused large shifts in the likelihood of conviction, with limited impacts on defendant outcomes beyond the short-term. Next, we describe characteristics of the marginal defendant in our sample. We then use the variation marginal defendants' characteristics across natural experiments to identify observable tags of individuals who might be likely to benefit from avoiding a misdemeanor conviction. Finally, we use these analyses to guide subgroup analyses that investigate whether the effect of a conviction is larger for some types of defendants than others.

As discussed above, one potential reason for the muted effects that we find could be because a misdemeanor conviction is less likely to affect the types of defendants affected by the policy reforms. To gauge this potential explanation, we estimate the average potential outcomes and baseline characteristics for compliers in our sample, following Imbens and Rubin (1997). In our setting, compliers are individuals who would have been convicted if their case was disposed before the reform but would not have been convicted if their case was disposed after. In other words, compliers are defendants who are shifted from conviction to dismissal—these defendants are at the *margin* of conviction and dismissal.

From Table 4, we see that compliers are less likely interact with the criminal legal system but also have worse labor market outcomes than the average defendant in the pre-policy period. Column 1 compares average characteristics of individuals in the pre-policy period to those of compliers (Column 2). Panel A compares these groups based in terms of our main outcomes, while Panel B compares these groups in terms of baseline characteristics. From the ratios in Column 3, we see

[17] E.g., Agan, Doleac, and Harvey (2023) limit their sample to non-violent offenders (since only those cases are randomly assigned) and find the largest effects for first-time defendants.

that compliers have lower potential recidivism outcomes if dismissed—compliers are 35% less likely than the average defendant to have a new charge associated with them in the three years after disposition. Even though the average defendant in our sample has low labor market attachment (e.g., 65% employment rate and $16,300 average annual earnings), compliers are negatively selected on this dimension. Compared to the average pre-reform defendant, compliers are 25% less likely to be employed, would earn 20% less, and are 12% more likely to have earnings below the federal poverty line.

Table 4: Comparing compliers to overall sample

Variable	Control Mean (CM)	Control Complier Mean (CCM)	$100 \times \frac{CCM}{CM}(\%)$
	(1)	(2)	(3)
Panel A: Outcomes			
Any new charge (3 years)	0.38	0.25	65.5
Average employment (3 years)	0.65	0.50	76.9
Average earnings (3 years)	16,300.00	12,910.00	79.2
Average poverty status (3 years)	0.63	0.70	111.7
Panel B: Baseline Characteristics			
Age			
Male			
White (Non-Hispanic)			
Black (Non-Hispanic)			
Hispanic			
First-time defendant			
Number of Previous Charges			
Number of Previous Convictions			
Any employment in the previous year			
Total earnings in the previous year			

Note: In Panel A, 'Control Complier Mean' represents the average untreated potential outcome among compliers, where treatment is conviction. For Panel B, 'Control Complier Mean' represents the average value of the baseline characteristics among untreated compliers. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2957-R12890.

While compliers in our setting are certainly selected relative to the average defendant, how such selection drives the overall patterns discussed in Section 5.3 is ambiguous. On one hand, individuals with low levels of baseline criminal activity are likely to experience adverse impacts of interaction with the criminal legal system—Agan, Doleac, and Harvey (2023) find the largest reductions in future criminal interactions among defendants with no prior contact with the prosecutor's office. On the other hand, compliers in our context seem to be negatively selected in terms of labor market outcomes. Recent work has attributed muted labor market effects of incarceration in some settings to low baseline labor market attachment (e.g., Garin et al. 2023). Given such discussions, it is possible that this negative selection is behind why we do not find evidence of large labor market impacts of misdemeanor convictions on average.

Next, we use the geographic and contextual diversity across the counties that each natural experiment occurs in to understand how the impacts of misdemeanor conviction vary with characteristics of the marginal defendant. We do this by estimating the following for each natural experiment: the local average treatment effect (LATE), the control complier mean (CCM), and average baseline characteristics of compliers. We then estimate bivariate regressions of the LATE

for each main outcome on either the CCM for that outcome or set of key baseline complier characteristics.

Figure 6 plots the slope coefficients from such regressions, separately for each complier characteristic. The slopes are normalized in such a way that a positive coefficient indicates that the particular characteristic is associated with a beneficial effect of a misdemeanor conviction while a negative coefficient indicates a characteristic associated with an adverse effect of misdemeanor conviction. Panel a) presents the slope coefficients against the CCMs, providing suggestive evidence that misdemeanor conviction is particularly harmful for marginal defendants with [Undergoing disclosure avoidance review.] potential recidivism and [Undergoing disclosure avoidance review.] labor market outcomes. Panels b) and c) of Figure 6 show the relationship between the LATE for each outcome and the share of compliers who are first-time defendants and below the median age respectively. These figures exhibit similar patterns to the CCM relationships in Panel a). This is suggestive evidence that these observable characteristics are tags for individuals who might be adversely impacted by a misdemeanor conviction.

Figure 6: Relationship between the local average treatment effect (LATE) of conviction and characteristics of the marginal defendant

(a) Control complier mean

[Undergoing disclosure avoidance review.]

(b) Complier share of first-time defendants

[Undergoing disclosure avoidance review.]

(c) Complier share of young defendants

[Undergoing disclosure avoidance review.]

Note: Each Panel plots the slope coefficient from a regression of the local average treatment effect (LATE) of conviction for the outcome listed on the x-axis against a particular complier characteristic. First-time defendants are defendants not previously charged with any offence in the state. 'Young' defendants are those between 18 and 31 years old. Control complier means denote compliers' average potential outcome if not convicted. 95% confidence intervals are based on heteroskedasticity-robust standard errors.

Finally, we divide our sample into subgroups, based on criminal history, labor market characteristics, and demographics to assess whether the impacts of misdemeanor conviction are larger for certain subgroups than others, using the above analysis as a guide. Figure 7 presents RDD estimates from Equation 2 separately for each subgroup. Each panel focuses on a different outcome, with Panel a) displaying the first stage impacts on the likelihood of conviction. The remaining panels present the impacts on recidivism as well as average employment, earnings, and poverty status over three years. We present these estimates for the following subgroups and their complements: first-time defendants, below median age, male, and above median predicted earnings (only for labor market outcomes). We also present estimates for first-time offenders who are above and below the median age in our overall sample.

We find that [Undergoing disclosure avoidance review.] .

Figure 7: Impact of policy reforms by subgroup

(a) Probability of Conviction

[Undergoing disclosure avoidance review.]

(b) Any New Charge (3 years)

[Undergoing disclosure avoidance review.]

(c) Average Quarterly Employment (3 years)

[Undergoing disclosure avoidance review.]

(d) Average Quarterly Earnings (3 years)

[Undergoing disclosure avoidance review.]

(e) Average Quarterly Likelihood of Being Above the Federal Poverty Line (3 years)

[Undergoing disclosure avoidance review.]

Note: Each Panel presents RDD estimates using Equation 2. The sample includes individuals in a particular subgroup across all natural experiments. Standard errors are clustered at the individual level.

6 MVPF and discussion

Are policies that reduce the prevalence of misdemeanor conviction socially beneficial? To answer this question we cast our causal estimates of misdemeanor conviction into a Marginal Value of Public Funds (MVPF) framework (Hendren and Sprung-Keyser 2020; Finkelstein and Hendren 2020). Given the type of quasi-experimental variation we study, we orient our analysis from the perspective of agents (e.g., prosecutors, clerks) who work in the criminal court system.[18] That is, these agents experience the changes in resource allocations and directly respond by reducing the conviction rate. We construct our baseline MVPF estimate, which represents the average across all natural experiments as:

$$\text{MVPF} = \frac{\text{Beneficiaries' willingness to pay}}{\text{Net cost to government}} \tag{3}$$

Our estimate of the MVPF of combines our ITT estimates of the effect 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 (Hunt, Anderson, and Saunders 2017; Hunt, Saunders, and Kilmer 2019; Caulkins 2010; for Justice Research 2014). We discuss each component of the MVPF below.

Willingness to Pay: Our estimate of willingness to pay includes change in the social cost of future crime ($85) and changes in defendant cumulative earnings over three years ($4270). To these measures, we will add causal estimates on changes in defendant social safety net receipt, as well as spillover estimates on children's educational attainment and romantic partner's labor supply and criminal behavior. Results from these latter benefits are [Undergoing disclosure avoidance review.] Given the inherent uncertainty with quantifying the social cost of crime, we will explore how our estimates change under varying assumptions, including underreporting of crime (Deshpande and Mueller-Smith 2022).

Net Government Costs: Misdemeanor convictions generate a suite of costs for the government,

[18] For the moment, given data constraints, we abstract away from utility costs that may incur via job loss or other staffing contractions.

including prosecution ($695), investigative costs ($2554), and potential costs of housing a convicted and sentenced individual in jail. Similarly, because of the harmful labor market impacts of criminal justice system contact, misdemeanor convictions can also negatively harm the government's budget constraint due to reduced income tax revenue ($427, assuming ten percent average income tax rate) and increased reliance on the social safety net for individuals with records. However, the government may, on net, recoup some of these expenditures with additional fine revenue and may reduce future prosecution costs if conviction has positive deterrence effects on defendant behavior.

Together, our initial estimates, abstracting from statistical uncertainty, suggest that the willingness to pay term is positive and non-conviction for governments represents fiscal savings, thus yielding a MVPF of infinity, or a program that pays for itself. Notably, our MVPF estimates do not currently incorporate potential changes in deterrence, which may have first-order impacts on the conclusions we draw here. For example, if lowering the conviction rate increases crime due to reduced deterrence effects, such influences may be large enough to shift the MVPF above or below one, which has significant implications for policy efficacy.

We take a first-step in this direction by instead casting the focal policy as one of expanding prosecution, so that the net cost to the government is now positive for each marginal conviction. Assuming that all of our estimated reduced form effects are zero, misdemeanor conviction must generate at least $3,250 in general deterrence effects (the government's net cost of conviction) to achieve an MVPF of one. In future versions, we plan to assess the robustness of our MVPF estimates to a range of assumptions on the magnitude of any potential deterrence effects and to examine heterogeneity in the social value of misdemeanor non-conviction across natural experiments and contextual factors.[19]

7 Conclusion

In the United States, misdemeanor charges represent approximately 75% of the annual criminal legal system caseload and are the most common form of police contact (Stevenson and Mayson 2018). Given the overwhelming scale of the misdemeanor system and stigma often associated with criminal records, certain District Attorneys in the United States have recently scaled back prosecution of certain non-violent offenses. However, there is limited causal evidence on how misdemeanor convictions impact the lives of defendants and their families and the net benefits of such policies is ambiguous.

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 defendants in 11 states, building on prior work

[19] For example, scaling back the misdemeanor system may be particularly beneficial in settings where the marginal defendant is low-risk.

using this strategy in other contexts (Rose 2021; Agan et al. 2024*b*).[20] We then estimate the causal impact of a misdemeanor conviction (relative to being charged) using a regression discontinuity design generated by unanticipated budget cuts and changes in prosecutorial priority that reduced conviction rates in seven counties across five states. We then use this causal evidence to conduct expansive welfare analyses of misdemeanor prosecution.

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 quarterly employment rates, and –3.2 and 7.5% in average quarterly earnings. Despite the small size of our point estimates, taken literally, they suggest that the policy reforms we study yield an MVPF of infinity. We also calculate that if the reforms truly did not shift any of the outcomes we measure, the marginal misdemeanor conviction must deter enough criminal activity to avoid at least $3,250 worth of government and victim costs to achieve an MVPF of one.

One potential reason for the muted effects is that relative to the average defendant in our setting, compliers have lower potential recidivism and lower levels of formal labor market attachment, margins of selection that have opposite implications for the treatment effect we might expect. While interactions with the criminal legal system tend to harm defendants with low baseline levels of criminal activity (e.g., Agan, Doleac, and Harvey 2023; Kamat, Norris, and Pecenco 2023) individuals with lower labor market attachment are less likely to experience adverse labor market impacts of criminal legal interventions (e.g., Garin et al. 2023).

In results that are currently undergoing disclosure review, we use the variation in characteristics of the marginal defendant across the seven natural experiments we study to identify observable tags for individuals who might be likely to benefit from avoiding a misdemeanor conviction. Using those analyses as a guide, we re-estimate the RDDs for subgroups of our main sample and see if the effect of a misdemeanor conviction is larger for some types of defendants than others. Future work also undergoing disclosure review includes understanding how a misdemeanor conviction affects outcomes captured in survey but not administrative data and spillovers on family members, which will let us expand our welfare analysis.

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.

[20] Note that the event study estimates are currently undergoing disclosure avoidance review.

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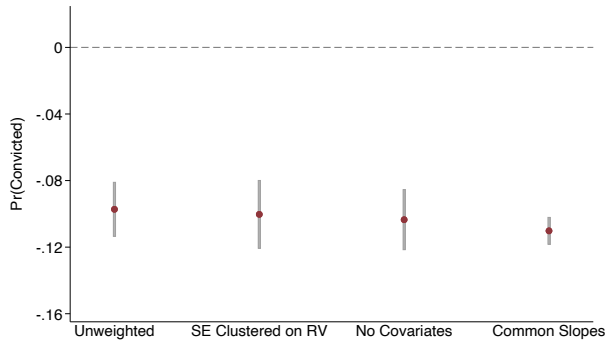
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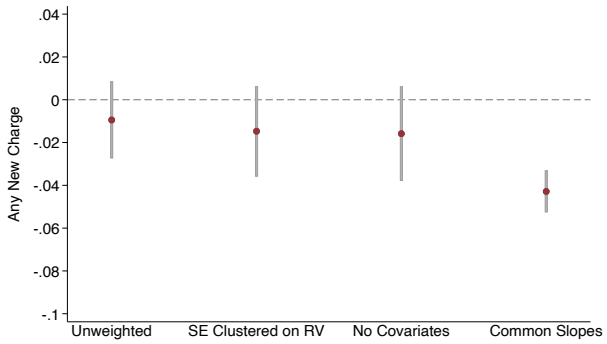
Appendix A: Additional Results

Appendix Figure A1: Assessing robustness to specification choices

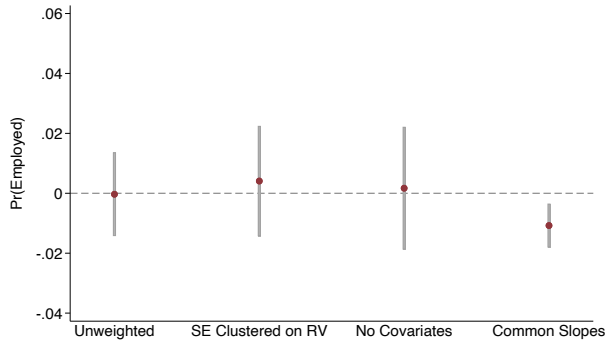
(a) Pr(Convicted)



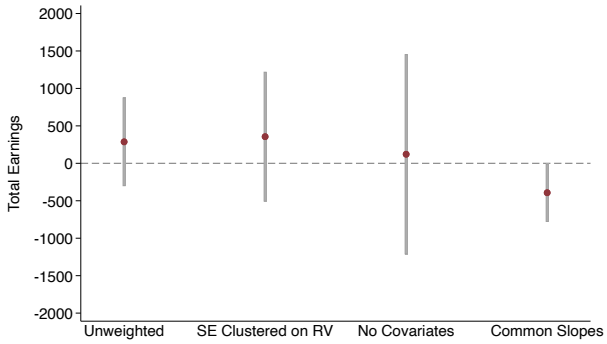
(b) Any New Charge (6–36 months)



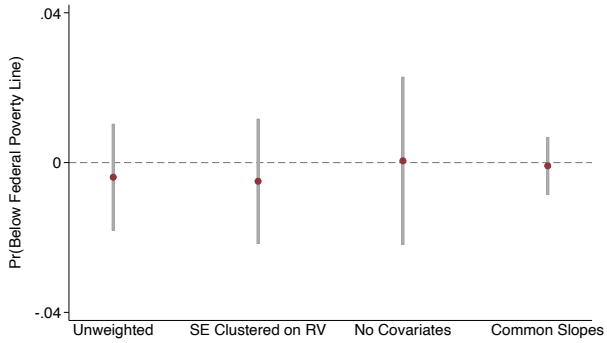
(c) Employment (1–3 years)



(d) Earnings (1–3 years)



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



Note: Each Panel presents RDD estimates for our main outcomes using the stacked sample and 5 different 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.

Appendix Figure A2: 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.

Appendix B: Data Details

Appendix Figure A3: Coverage of LEHD states

[Undergoing disclosure avoidance review.]

Note:

Appendix Table A1: States and time periods used in event study analysis

[Undergoing disclosure avoidance review.]

Note: