State recreational cannabis laws and racial disparities in the criminal legal system*

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We estimate the direct and spillover effects of cannabis legalization on longstanding racial disparities in criminal justice outcomes. We find that legalization reduces cannabis possession and sales arrests for White and Black populations, narrowing but not eliminating disparities. We also find spillover increases in hospitalizations involving cannabis and other illegal drugs. However, spillovers on arrests, incarcerations, and crimes involving serious violent or property offenses are insignificant or even decrease. Other illegal drug sales arrests decrease across populations, while illegal drug incarcerations decrease only among White populations. Spillovers on other low-level offenses are insignificant for White but mixed for Black populations.

Key words: cannabis legalization; racial disparities; arrests; crime; incarceration; homicides; violence.

JEL codes: I18, I14, H75.

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Cannabis prohibition is one of the most costly and destructive aspects of America's War on Drugs, resulting in years of life lost behind bars, criminal records hindering access to jobs, loans, and housing, billions spent on law enforcement, and systemic violence from illegal drug markets. In 2018, cannabis possession and sales were the most serious offenses in over 660,000 arrests, accounting for 40% of all drug arrests and exceeding the number of arrests for all violent crimes combined (Gramlich, 2020). Incarcerations for drug possession and sales are also prevalent, representing the most serious offenses for 43% of federal, 13% of state, and 25% of jail prisoners (Sawyer and Wagner, 2023).

Racial disparities in law enforcement of drug prohibition are widespread and longstanding, with Black communities disproportionately affected. Despite having similar rates of cannabis use as White persons, Black persons are 3.6 times more likely to be arrested for cannabis possession (Edwards et al., 2020). Black persons are also incarcerated at dramatically higher rates for drug offenses. Although Black persons represent 12.5% of the U.S. population, they account for 28% of state and 33% of federal prisoners with sentences over one year for drug offenses (Carson, 2021). Black persons also face disproportionate systemic violence, representing over 50% and 29% of homicide and firearm death victims (Federal Bureau of Investigation, 2018; Kaiser, 2022).

We study the effect of drug prohibition reform on racial disparities in the criminal legal system, focusing on the legalization of cannabis. As of 2024, 24 states and the District of Columbia passed recreational cannabis laws (RCLs), allowing adults 21+ to use and supply cannabis for recreational purposes. Critics claim that RCLs will incite cannabis and other drug use, harm public health, diminish traffic safety, and increase crime. Supporters claim that RCLs will generate tax revenue and legal jobs, reduce illegal drug markets and systemic violence, cut law enforcement costs, and narrow racial disparities in criminal justice outcomes (Gettman and Kennedy, 2014; ProCon, 2023).

The literature on the effects of RCLs on criminal justice outcomes predominantly focuses on the general population. Studies report declines in cannabis possession arrests and law enforcement seizures, increases in police clearance rates, and declines or null effects on violent, property, and public order crimes (Harper and Jorgensen, 2023; Lu et al., 2021; Sabia et al., 2024; Stohr et al., 2020; Dragone et al., 2019; Brinkman and Mok-Lamme, 2019; Wu et al., 2020; Plunk et al., 2019; Meinhofer and Rubli, 2021; Makin et al., 2019). Fewer RCL studies examine racial disparities, with nearly all focusing on cannabis possession arrests and documenting reductions for Black and White adults. One study focuses on the proportion of police traffic stops resulting in search in two RCL states, documenting reductions for Black, White, and Hispanic persons (Edwards et al., 2020; Firth et al., 2019; Sheehan et al., 2021; Fone et al., 2023; Pierson et al., 2020).[1] The RCL literature lacks systematic exploration into potential spillovers on racial disparities within the criminal legal system, leaving gaps in understanding the broader implications of legalization.

We provide comprehensive estimates of the direct and spillover effects of RCLs on racial disparities in criminal justice outcomes, focusing on arrests and incarcerations for cannabis and other drug-defined offenses (possession and sales), serious Part 1 offenses (violent and property crimes), and low-level Part 2 offenses. As these outcomes are a function of both criminal activity and law enforcement efforts, we also examine measures of crime and law enforcement resources. Lastly, we examine potential pathways linking RCLs to crime, including cannabis use, other illegal drug use, and illegal drug markets.

[1] A related literature studies the impact of other cannabis liberalization policies on criminal justice outcomes. For example, Gunadi and Shi (2022) analyzes the impact of cannabis decriminalization on arrests, finding reduced racial disparities for White and Black adults.

We analyze outcomes by race and calculate Black-White rate ratios and rate differences, which capture relative and absolute disparities. We use administrative data from 2007-2019 and a difference-in-differences (DID) framework that exploits the staggered implementation of RCL in 11 states.

We make several contributions to the RCL literature on criminal justice outcomes. First, we focus on racial disparities, an under-researched but important issue frequently cited in ongoing debates about state and federal cannabis legalization, cannabis record expungement, and social equity policies. Second, we analyze a wide range of criminal justice outcomes beyond direct effects on cannabis possession arrests, providing the first estimates of spillover effects and associated pathways. Elucidating spillovers is crucial because RCLs could affect arrests, incarcerations, and crime, not only for cannabis-defined offenses but also for other offenses by influencing cannabis use, other illegal drug use, illegal drug markets, and law enforcement efforts. We also analyze direct effects on cannabis sales arrests, a relevant dimension of the supply side of legalization beyond cannabis possession. Third, we use nationally representative data, a larger number of treated states, and verify the robustness of findings. Overall, our study uncovers potential benefits and adverse effects of legalization, highlighting areas where these effects are and are not significant, offering valuable insights for the ongoing cannabis legalization debate.

We find that legalization leads to significant declines in arrests for cannabis-defined offenses, consistent with the direct effects of RCLs. Among White and Black persons, cannabis possession arrests decline by 62% and 51%, while cannabis sales arrests decline by 44% and 49%. These declines reduce but do not eliminate racial disparities. We then examine spillovers on other offense categories. We find that arrests and incarcerations for serious violent and property crimes do not change across racial groups. Arrests for other illegal drug possession do not change, but sales arrests decrease by 22% and 17% for White and Black persons. Prison admissions for drug-defined offenses decrease by 34% for White persons, with null effects for Black persons. Lastly, low-level Part 2 arrests increase by 11% for Black persons only. However, the identifying assumption for this outcome is less robust, warranting caution in drawing strong inferences.

Motivated by these findings, we explore spillovers on criminal activity and associated pathways. We find that hospitalizations involving cannabis use disorder and poisoning increase across racial groups, while those involving other illegal drugs increase for Black persons only. However, reported violent and property crimes, which impose the highest societal cost, do not increase or even decrease differentially in predominantly Black neighborhoods following legalization. Reported drug-defined offenses also decrease differentially in Black neighborhoods, suggesting a disruption of illegal drug markets. Reported Part 2 offenses do not change differentially in Black neighborhoods, although treatment effects are heterogenous for select Part 2 offenses. Lastly, the number of police officers, particularly civil officers, increases after legalization.

Overall, racial disparities in arrests and incarcerations for drug-defined offenses persist post-legalization, but RCLs significantly reduce absolute disparities in arrests for cannabis possession, sales, and other illegal drug sales, without meaningful impacts on crime. Policymakers seeking to reduce these racial disparities will need to take additional steps beyond legalization.

1 Background

1.1 Cannabis Liberalization Policies

The U.S. federal government classifies cannabis as a Schedule I controlled substance, indicating no accepted medical use, a lack of safety, and high abuse potential. From 2012 to 2024, however, 24 states and DC enacted RCLs, legalizing cannabis sales, distribution, possession, and use among adults 21+ (ProCon, 2022). RCL provisions are subject to limits (e.g., quantity, products, private use) and therefore, cannabis arrests may still occur. All RCLs were preceded by medical cannabis laws (MCLs) and some by cannabis decriminalization laws (CDLs). MCLs allow physicians to recommend cannabis for treating eligible health conditions. CDLs remove criminal sanctions for small possession offenses with no protection for supply offenses. Instead, the penalties for possession can range from no penalties, civil fines, or drug treatment. Decriminalization may offer some relief from mass incarceration, but it still preserves many of the punitive consequences of the criminal misdemeanor experience which are likely to affect poor and disadvantaged populations, most of which are persons of color (Smart and Kleiman, 2019).

1.2 Conceptual Framework

Cannabis and Criminal Justice Outcomes. We estimate the direct and spillover effects of RCLs on racial disparities in arrests and incarcerations, each a function of criminal activity and law enforcement efforts. Arrests and incarcerations are strongly correlated with cannabis. While cannabis-defined offenses (possession and sales) are the most serious offenses in only about 6.4% of total arrests, an estimated 34-59% of booked adult male arrestees test positive for cannabis at the time of arrest and 64% of incarcerated persons report regular cannabis use (Bureau of Justice Statistics, 2020; Bronson et al., 2017; Office of National Drug Control Policy, 2014). These estimates far exceed the 11% prevalence of past month cannabis use in the general population.

Criminal activity and cannabis are plausibly linked through several pathways: (1) cannabis-defined offenses, (2) systemic violence, (3) psychoactive effects, and (4) economic crime. Cannabis *prohibition* drives pathways (1) and (2), while cannabis *use* drives pathways (3) and (4). First, drug prohibition defines cannabis possession and sales as a crime. Second, drug prohibition can incite systemic violence in illegal cannabis markets, arising because of turf wars among suppliers, unpaid debts, and other conflicts that cannot be resolved through the legal justice system.[2] Third, the psychoactive effects of cannabis use may influence behavior, leading to criminal activity in some individuals. While studies show cannabis use generally inhibits violence, evidence indicates violent behavior in adolescents and high-frequency users (Dellazizzo et al., 2020). Toxicology and self-reports show that the prevalence of cannabis and other drugs among homicide victims and offenders exceeds population prevalence (Darke, 2010). The psychoactive effects of cannabis use may also incite non-violent crimes, such as public disorder and driving under the influence (DUI). Fourth, cannabis use may induce economic crime among users to finance their consumption (Pacula and Kilmer, 2003). Among persons incarcerated for property and violent crimes, 39% and 14% said they committed the crime to obtain drugs or money for drugs (Bronson et al., 2017). Given the interdependence in the production and consumption of cannabis and other illegal drugs, a fifth pathway (5) involves criminal activity in other illegal drug

[2] Note that many illegal cannabis sales are conducted indoors and, thus, illegal cannabis markets are less violent than other illegal drug markets (Caulkins and Pacula, 2006).

markets.[3] As criminal justice outcomes are also a function of law enforcement efforts, a sixth pathway (6) involves law enforcement priorities, incentives, and resources for targeting criminal activity from pathways (1)-(5).

RCLs and Criminal Justice Outcomes. The net effects of RCLs on criminal justice outcomes are theoretically ambiguous, and will depend on whether RCLs influence the previous pathways. We hypothesize that legalization may reduce some criminal justice outcomes through pathways (1) and (2), related to cannabis *prohibition*. With legalization, cannabis possession and sales that abide to RCL provisions are no longer defined as a crime. Moreover, the creation of a legal cannabis market may help reduce the size of the illegal cannabis market, decreasing systemic violence and other crimes related to drug trafficking. However, if illegal cannabis market suppliers are unable or unwilling to obtain legal jobs, some crimes could increase. We hypothesize that RCLs may increase criminal justice outcomes through pathways (3) and (4), related to cannabis *use*. RCLs increase cannabis use (Cerdá et al., 2020; Hollingsworth et al., 2022), and may therefore influence crimes attributable to the psychoactive effects of cannabis use as well as economic crimes. The effects of RCLs on pathway (5) will depend on whether other illegal drugs act as substitutes or complements to cannabis in production or consumption. As for pathway (6), law enforcement may de-prioritize overall drug prohibition efforts, leading to fewer arrests for both cannabis- and other drug-defined offenses, regardless of actual changes in production or consumption. Conversely, law enforcement

[3] Consumers and producers of cannabis may also be consumers and producers of other illegal drugs. Among consumers of heroin, methamphetamine, and cocaine, past-month cannabis use was 60%, 62%, and 69%, respectively. Evidence also suggests overlap in production. Most foreign heroin, methamphetamine, and cannabis originates in Mexico, where transnational criminal organizations (e.g. Sinaloa cartel) play a large role in production and distribution within the U.S. (Drug Enforcement Administration, 2018; Beittel, 2022).

may shift resources towards other drug-defined offenses or non-drug offenses, resulting in increased arrests and incarcerations for these other crimes.

RCLs and Criminal Justice Outcomes by Race. The ambiguous net effects of RCLs on criminal justice outcomes make their racial group impacts uncertain. If treatment effects are proportional across racial groups, relative disparities would remain unchanged, while absolute disparities would shift due to baseline differences. However, disproportionate effects, particularly on Black populations, could alter relative disparities, influenced by long-standing factors such as racial discrimination by police (Ba et al., 2021), over-policing in minority communities (Chen et al., 2023), and greater drug activity in public among minorities (Beckett et al., 2006).

2 Data

Arrests. We obtain arrest data from the Federal Bureau of Investigation's (FBI) 2007-2019 Uniform Crime Reporting Program: Arrests by Age, Sex, and Race (UCR). Data capture monthly arrests for each reporting agency, disaggregated by offense type, race, age, and sex. UCR data report arrests, not the number of arrested individuals. Each record reflects the highest charge during a police interaction, per the FBI hierarchy (Kaplan, 2021).[4] Police officers record offenders' race based on their perceptions. Most agencies did not report Hispanic counts during this period, so we focus on White, Black,

[4]Serious crimes (e.g., murder) follow a consistent hierarchy, but less serious crimes like drug offenses vary by agency (Kaplan, 2021). Using data with all offenses per incident, Hendrix and Martin (2019) found about two-thirds of drug offenses are single-incident events. In multiple-offense incidents, drug arrests often co-occur with other drug offenses and, less frequently, public order violations. Appendix Table S3 analyzes the 2018 National Incident-Based Reporting System, corroborating these findings.

and the total adult population.

We analyzed arrests for cannabis-defined offenses (possession and sales), other drug-defined offenses (heroin/cocaine, synthetic narcotics, and other drugs), non-drug offenses, and their total. For non-drug offenses, we distinguish between Part 1 crimes, which are more serious and subdivided into violent (aggravated assault, manslaughter, murder, rape, robbery) and property crime (arson, burglary, motor vehicle theft, larceny), and Part 2 offenses, which are less serious. We exclude "uncategorized" arrests, corresponding to offenses reported by agencies but not required by the FBI.[5]

To account for differences in when agencies reported to the FBI and to incorporate data revisions made by agencies, we aggregate arrests up to the county-year level, overall and by race.[6] A notable limitation of UCR is the variation in the number of agencies that voluntarily decide to report (Kaplan, 2021). To address this, we use the coverage indicator sample criterion (Freedman and Owens, 2011) and control for the number of reporting agencies. We construct a county-level index based on the share of reporting months and the fraction of the population covered by reporting agencies, restricting to a coverage threshold of at least 65%, and show robustness to stricter values. This effectively excludes data from Florida, Illinois, and DC, as in Sheehan et al. (2021). Crucially, we rely on assuming that reporting issues are uncorrelated with the timing of RCLs (Figure S25).

Incarcerations. We analyze prison admissions data from the 2007-2019 National Corrections Reporting Program (NCRP), capturing offender-level infor-

[5]Kaplan (2021) notes significant variation in these additional offenses reported to the FBI across agencies and over time.

[6]In UCR data, there are a very small number of negative arrest counts (less than 0.0002% of the data). These negative counts are data revisions made by agencies (Kaplan, 2021). As is standard, we aggregate to yearly counts to obtain the actual number of yearly arrests.

mation on prisoners aged 18+ and admitted while under the physical custody of state correctional authorities. An individual may have more than one record if they were admitted on multiple occasions. Demographic information, admission type, and most serious offense are collected from individual prisoner records. Admission type may include new court commitments, parole return or revocation, and other (i.e., unsentenced). We exclude admissions for parole return or revocation. We then generate prisoner admission counts at the state-year level, by race and offense. Offense categories include drug-defined offenses and other offenses (i.e., violent, property, public order, etc.).

NCRP has limitations. Participation is voluntary and not all states submit every year. Race is poorly reported in some states, and reporting practices vary. For example, some states only report admissions to state prison, while others with unified prison and jail systems report admissions to both. We drop states with significant non-reporting or missing race information.[7] We also exclude state-year cells with at least 25% missing race or offense and impute missing observations with averages from consecutive state-year cells.

We also analyze yearend prisoner data from the 2009-2019 National Prisoner Statistics Program (NPS), which provides state-year counts by race of prisoners under federal or state jurisdiction on December 31. Counts include inmates in public or private prisons, those held in jails in or out of state, and inmates temporarily out to court or in transit. NPS has limitations, including reporting inconsistencies. We exclude states or state-years with such inconsistencies and make imputations to correct for obvious reporting errors.

[7]CA is dropped from our incarceration analyses due to their implementation of major prison reform in 2011 (Public Safety Realignment Act), where selected offenders now serve their terms in local county jails rather than state prisons. DC did not submit NPS data during our study period.

Criminal Activity. We analyze incident data on calls for service and reported crimes, which may proxy for the level or perception of criminal activity and other incidents in a defined area and timeframe. Calls for service to police involve callers, call-takers, dispatchers, and responders (i.e., police, fire department, EMS) and are often generated through a call or text to 911 or a non-emergency line. Calls may be initiated by civilians or police. Call-takers receive and input calls into a system that identifies the caller's location and categorize incidents into types. Dispatchers use this information to assign responders to the incident. Crime data reflects incidents of crime reported to police, primarily Part 1 offenses, and in limited cases, select Part 2 offenses. Thus, call data are better suited for analyzing Part 2 offenses.

We obtain call and crime data from publicly available datahubs of select RCL cities (Portland, Seattle, Los Angeles, Sacramento, Denver, Boston, Burlington, Detroit, and DC) that published data for at least one year before and after their RCL effective date, and reported incident latitude and longitude coordinates (Appendix F.2.1). Each city's data varies in sample period, incident reporting and categorization, and racial composition of its population. We harmonize sample selection and incident measures where possible and report pooled estimates, but differences in data reporting require separate analyses for each city (Bennett, 2018; Neusteter et al., 2019; Lum et al., 2022). When possible, we drop police-initiated calls to minimize police influence and because these are typically reported inconsistently. We measure drug-defined, Part 1, and Part 2 offenses using text describing incident type (Appendix F.2.2). We drop incidents of domestic violence and child maltreatment because coordinates are missing in many cities to protect victims.

We match incidents to Census tracts using latitude and longitude coordinates, aggregate incidents at the tract-quarter level, and link them to American

Community Survey 5-Year tract population estimates using the initial sample year to fix assignment. We calculate the proportion of Black persons (Hispanic and non-Hispanic) in each tract. We exclude tracts with fewer than 10 average total incidents per quarter, a total population of less than 500, or with a proportion of Hispanic persons of 60% or more.[8] We also drop outcomes with fewer than 0.5 average total incidents per tract-quarter.

Call and crime data do not report race. We therefore compare incidents in minority neighborhoods to those in non-minority neighborhoods within the same city, before and after RCL implementation. Since all cities analyzed are in RCL states, minority and non-minority neighborhoods are both exposed to RCL implementation. Therefore, our estimates reflect differential RCL effects by neighborhood racial composition. We classify Census tracts as minority neighborhoods using a tailored approach that accounts for the wide variation in racial composition across cities (Appendix F.2.3). In cities with high Black populations, we set the threshold to a proportion of Black persons of at least 80%. In cities with relatively low Black populations, we set a threshold near the 90th percentile based on the proportion of Black persons, adjusting for natural breaks in the data.[9] We then exclude tracts with a proportion of Black persons within 5 percentage points below the threshold. Recognizing the sensitivity of this tailored classification, we also test the robustness of our results against various alternative classifications (Appendix F.2.4).

[8] This primarily excludes tracts in LA, with minimal impact on other cities.

[9] For example, in Burlington (median Black population: 2%), two tracts fall around the 90th percentile in the ordered sample, either of which could be assigned as the threshold. Because the proportion of Black persons in both of these tracts is comparable (9.9% versus 10%) and substantively different than in the preceding tract (3.9%), we classified tracts as minority neighborhoods starting with the tract with proportion 9.9% rather than the tract with proportion 10%. In Boston (median Black population: 11%), we initially set the threshold at 68.8% (the 90th percentile) but adjusted it to 60% to better align with a natural break in the data while still capturing tracts with a high proportion of Black persons in absolute value.

Deaths. We obtain deaths from restricted 2007-2019 National Vital Statistics System (NVSS) Multiple Cause of Death Files. These microdata are based on information abstracted from death certificates and provide underlying and multiple cause of death for nearly all U.S. deaths. We select persons aged 18+. We identify total homicides and homicides involving gun injury using standard International Classification of Diseases, Tenth Revision (ICD-10) codes (CDC, 2002). We also use a data field identifying the manner of death. We aggregate outcomes at the state of occurrence-year-quarter level, overall and by race.

Hospitalizations. We obtain hospital discharges from restricted 2007-2019 Healthcare Cost and Utilization Project State Inpatient Databases (HCUP-SID), the largest collection of all-payer U.S. hospital data. HCUP-SID reports patient demographics and healthcare information for a near census of inpatient discharge records in participating states. Select non-participating states directly shared their discharge records or generated counts through a request process. We combine HCUP-SID with discharge data directly shared by other states, for a total of 33 states including 10 switching RCL states. Our panel is unbalanced since we could not obtain all years for some states. We identify hospitalizations of persons aged 18+ with at least one admitting, principal, or secondary ICD diagnostic code indicating assault and assault involving gun injury (Smart et al., 2022; CDC, 2021). We also identify hospitalizations with diagnostic codes indicating substance use disorder and poisoning for cannabis and other illegal drugs (cocaine, methamphetamines, opioids). ICD-9 transitioned to ICD-10 in 2015Q4, improving diagnostic code specificity but also creating mapping challenges. We mitigate this issue by using ICD conversion machines, codes in previous studies, and year-quarter fixed effects.[10] We aggregate outcomes at the state-year-quarter level, overall and by race.

Police Officers. We obtain police officer counts from the Law Enforcement Officers Killed or Assaulted (LEOKA), which reports the annual number of civilian and sworn police officers as of October 31st for each reporting agency. As with UCR data, not all agencies report employee counts. We analyze data from local police departments, excluding small agencies (serving populations under 10,000) and those with overlapping jurisdictions. Agencies reporting zeros are treated as missing data, and we drop agencies missing officer counts for three or more years. Outliers at the agency level are also excluded. As before, we aggregate to the county level. Following our methodology with the UCR data, we adjust for the annual (rather than monthly) reporting frequency by constructing an index of the fraction of the county population covered by reporting agencies in LEOKA. We restrict the analytical sample to counties with at least 65% coverage and control for the number of reporting agencies in our estimations.

3 Empirical Strategy

Main Specifications. We exploit variation in the staggered implementation of RCLs in 11 states using the effective dates in Appendix Table S1. We estimate separate two-way fixed effects (TWFE) difference-in-differences (DID) regressions for the overall population and for each racial subpopulation:

[10] Cannabis poisonings are grouped with other hallucinogens in ICD-9 but are identified separately in ICD-10. To harmonize cannabis poisonings over time, our measure uses ICD-10 codes for both cannabis and other hallucinogens. Although this introduces measurement error, ICD-10 data show that other hallucinogens only represent 11% and were flat.

$$Y_{r,j,t} = \beta RCL_{j,t} + \gamma X_{j,t} + \alpha_j + \eta_t + \varepsilon_{r,j,t} \tag{1}$$

$Y_{r,j,t}$ is an outcome for population group r, in jurisdiction j (state or county), and in time period t (quarter or year). Outcomes are measured in rates per 10,000 persons by dividing counts by Census population estimates of adults 18+ corresponding to the same group-jurisdiction-period. For racial disparities, we generate the Black-White rate ratio by dividing the rate for Black persons by the rate for White persons, and the Black-White rate difference by subtracting the rate for White persons from the rate for Black persons. Rate differences measure absolute disparities while rate ratios measure relative disparities, both of which provide necessary information for understanding changes in disparities (Keppel et al., 2005).

$RCL_{j,t}$ is an indicator for whether an RCL was in effect in jurisdiction j at time t. We include jurisdiction fixed effects α_j to account for time-invariant differences across jurisdictions, effectively identifying our coefficient of interest from within-jurisdiction variation over time. We also include time period fixed effects η_t to control for any common shocks affecting outcomes. $X_{j,t}$ is a vector of control variables, including an indicator for CDLs.[11] For arrest data, we also control for the number of reporting agencies in a given county-year. All regressions are weighted by Census population estimates for that group-jurisdiction-period. Standard errors are clustered by state, which is the level at which the treatment varies.

The DID coefficient β reflects the static treatment effect of RCLs on out-

[11] While there is some variation across studies regarding what should constitute a CDL, we defined CDLs as state policies that reclassified the possession of small amounts of cannabis from a criminal offense to a civil offense, regardless of first-offender status (Grucza et al., 2018; Pacula et al., 2003; Gunadi and Shi, 2022).

comes. The main DID assumption for identifying a causal effect is that, in the absence of an RCL, outcomes would have evolved similarly between RCL and non-RCL states during the post-RCL period (i.e., parallel trends). To evaluate the parallel trends assumption and whether treatment effects are dynamic over time, we present event study plots based on the following regression:

$$Y_{r,j,t} = \sum_{\tau=-L}^{L} \beta_\tau \mathbb{1}_{[t-E_j^{RCL}=\tau]} + \gamma X_{j,t} + \alpha_j + \eta_t + \varepsilon_{r,j,t} \tag{2}$$

where E_j^{RCL} indicates the time period in which jurisdiction j implemented an RCL, $\mathbb{1}_{[\cdot]}$ is the indicator function, $L > 0$ defines an arbitrary number of leads and lags, and everything else is as defined above. We also include an indicator for all periods prior to $-L$ and an indicator for all periods after L. The reference group is $\tau = 0$, the period right before RCL implementation. We set $L = 3$, hence identifying leads and lags from the full variation across RCL states (except for the third lag, which is identified from pre-2018 RCLs given that the data ends in 2019).

Robustness Checks. We conduct various robustness checks to address potential concerns. First, due to the small number of switching RCL states in our sample, standard statistical methods may over-reject the null. Thus, we calculate wild cluster bootstrapped confidence intervals (Roodman et al., 2019). Second, the TWFE DID estimator may be biased if treatment effects are heterogenous across states and over time (Goodman-Bacon, 2021). We assess this issue by calculating the share and sum of negative weights in DID comparisons (De Chaisemartin and d'Haultfoeuille, 2020). We also report results using heterogeneity robust DID estimators from De Chaisemartin and d'Haultfoeuille (2024), Sun and Abraham (2021), Borusyak et al. (2023), and Wooldridge

(2021). Third, we test the robustness of findings by adjusting control variables. We drop baseline controls and progressively include MCLs, cannabis expungement laws, and state-level unemployment rates to account for other cannabis policies and economic conditions. We also add spatial controls to account for potential spillover effects to neighboring jurisdictions without RCL implementation. Fourth, since the date of cannabis legalization alone may inadequately measure cannabis access, we replace the RCL indicator with an indicator for when recreational cannabis dispensaries first opened in the state. Lastly, for the arrest data, we impose stricter thresholds for the reporting agency coverage indicator and exclude outliers. We also drop each RCL state one at a time to check if effects are driven by an outlier state.

4 Criminal Justice Outcomes

4.1 Arrests

Raw Data Plots. Figure 1 plots raw arrest rates for cannabis-defined offenses and total offenses, before and after legalization. We normalize time periods so that zero is the year right before RCL implementation. Following legalization, cannabis arrests decline sharply for both racial groups, but do not disappear completely (in period 3, there are 2 and 5.8 arrests per 10,000 White and Black persons, respectively) as individuals can still be arrested for violating RCL provisions (e.g. possession limits). Total arrests, however, are mostly unchanged. Appendix B includes raw plots for other study outcomes.

Arrests for Cannabis-Defined Offenses. We first examine the direct effects of RCLs with arrests for cannabis-defined offenses (possession and sales).

Event study plots in Figure 2 show significant declines in cannabis arrest rates after legalization across groups, with larger estimates for Black persons. Pre-RCL coefficients are all small and insignificant, favoring a causal interpretation.

Columns (1) and (2) in Table 1 report corresponding TWFE DID estimates. Estimated declines in cannabis arrest rates are statistically significant and large across groups, albeit much greater for Black persons (possession arrests decline by 7.3 for White vs 18.9 for Black, and sales arrests decline by 1.2 for White vs 6.6 for Black). Compared to the mean in RCL states prior to legalization, these estimates imply a reduction of 62% for White and 51% for Black persons in possession arrest rates, and 44% and 49% in sales arrest rates.[12] Additionally, we obtain significant and sizable declines in the rate difference for both cannabis possession (54%) and sales (49%), but small and insignificant coefficients for the rate ratio.

In sum, legalization leads to substantial declines in arrests for cannabis-defined offenses among Black and White persons. These direct effects are expected and consistent with RCL provisions, which legalize cannabis possession and sales. Legalization also greatly reduces absolute disparities but has little effect on relative disparities since documented declines in cannabis arrest rates are proportional across racial groups. If legalization is race-neutral, we can expect proportional reductions in arrests across racial groups. Our study aligns with previous RCL studies documenting reductions in cannabis possession arrests (Edwards et al., 2020; Firth et al., 2019; Sheehan et al., 2021; Fone et al., 2023), and is the first to document reductions in cannabis sales arrests.

Arrests for Other Drug-Defined Offenses. We next explore whether RCLs generate spillovers on arrests for other drug-defined offenses. Columns

[12] All corresponding event study plots are in Appendix C.

(3) and (4) in Table 1 show small and insignificant estimates on arrest rates for possession of other drugs across racial groups. We can reject increases above 4.4 and 4.6 arrests per 10,000 persons for White and Black persons. However, we find significant declines in arrests for sales of other drugs, with a tripling of effect sizes between White and Black persons (1.4 vs 4.5). Relative to the baseline mean, estimates suggest a 22% decline for White and 17% for Black persons. The rate difference in sales arrests also declined significantly (18%), although we do not find significant effects for the rate ratio.

Together, results indicate that RCLs generate spillovers affecting arrests for sales but not for possession of other illegal drugs. These spillovers may result from illegal drug market responses affecting the production of other illegal drugs, or from law enforcement responses affecting drug prohibition efforts, regardless of changes in the production of other illegal drugs. While we cannot isolate these pathways, documented reductions in arrests for sales of other drugs rule out a shift in efforts towards greater law enforcement of other illegal drugs. Our findings are consistent with research showing that RCLs reduced law enforcement seizures of both cannabis and other illegal drugs (Meinhofer and Rubli, 2021).

Arrests for Other Offenses. We next investigate whether RCLs generate spillovers on arrests for offenses other than drug possession and sales, including serious Part 1 offenses (violent and property crime) and low-level Part 2 offenses. As discussed in Section 1.2, RCLs may influence criminal activity and law enforcement efforts related to other offense categories. For instance, if RCLs lead to an increase in economic crime to fund higher cannabis use, property crime arrests might increase.

Column (5) in Table 1 presents small and insignificant estimates for violent crime arrests across groups. We can reject increases of over 0.7 and 7.6 arrests per 10,000 for White and Black persons, respectively. Column (6) presents insignificant effects for property crime arrests across groups. We can reject increments above 4 and 11.7 arrests per 10,000 for White and Black persons, respectively. We cannot reject that effect sizes are similar across groups.

Column (7) reports positive coefficients for Part 2 arrests across all groups. The effect is statistically insignificant and relatively small for White persons (4% of the baseline mean). For Black persons, the estimate is significant and large, at 32.5 more arrests per 10,000 or about 11% of the baseline mean. However, event study plots in Figure S17 raise concerns that the increase in Part 2 arrests could be driven by an increasing pre-RCL trend. Extrapolating a linear trend would suggest that there were no changes in Part 2 arrests after legalization. Figure S18 shows event study plots leaving out one RCL state at a time from the estimation. This exercise uncovers that the increasing pre-trend is driven by California, and that estimated growth in Part 2 arrests for Black persons is sustained even after excluding this state. Since we cannot claim to have a clean estimate of Part 2 arrests, we caution against making strong inferences on this particular outcome, although the positive coefficients for Black persons invite further research into disentangling potential increases. For completeness, Figure S42 shows a condensed version of event study estimates for each of the non-drug offense categories, by race.

In sum, we find that RCLs generate limited spillovers on arrests for non-drug offenses. Arrests for serious violent and property crimes did not increase on average, although treatment effects may vary by local area, state, or specific offense. This suggests RCLs might not be strongly tied to major increases in arrests for offenses with the highest societal cost. Further investigation of potential increases in arrests for Part 2 offenses is needed, especially given the mixed evidence for Black persons.

Total Arrests. Lastly, we report the net effects of RCLs using total arrests in Column (8) of Table 1 and Figure S10. We obtain small and insignificant point estimates across racial groups. Confidence intervals do not allow ruling out declines of up to 14 and 49 arrests per 10,000 for White and Black persons, consistent with estimated reductions in arrests for drug-defined offenses.

The lack of significant net effects in total arrests may stem from cannabis-defined offenses comprising a small share of total arrests (5% for White and 8% for Black persons at baseline in RCL states). Additionally, minimal spillover effects on arrests for other drug-defined offenses as well as for other offense categories further limit any significant net impact.

Prior Cannabis Decriminalization Law. All RCL states in our sample first implemented MCLs, but only three (CA, MA, VT) had already decriminalized possession of small cannabis amounts prior to RCL implementation. To gauge whether treatment effects are heterogeneous in RCL states with and without prior CDLs, we interact the RCL indicator with a time invariant indicator for the presence (CDL_{pre}) or absence ($1 - \text{CDL}_{\text{pre}}$) of a CDL prior to RCL implementation. Table 2 shows that declines in cannabis possession arrests are steeper in RCL states that had not yet decriminalized cannabis possession, though there remained some scope for policy effects in states with pre-existing CDLs. For cannabis sales arrests, RCL effects are similar across states, consistent with CDL provisions. For other arrest categories, we generally cannot reject similar effect sizes across RCL states, regardless of prior CDLs. Notably, while we find significantly larger increases in Part 2 arrest rates for Black persons in states without prior CDLs, we interpret these results cautiously given the earlier discussion of pre-trends in this outcome. Overall, results suggest that RCLs may still influence arrests despite prior decriminalization, which has implications for states with existing CDLs but no RCLs.

Robustness Checks on Arrest Estimates. We perform a series of robustness checks in the Appendix. First, we calculate p-values that consider multiple hypothesis testing in Table S4. Our findings on White and Black arrest rates survive after adjusting for false discovery rates: arrests for cannabis possession and sales, as well as other drug sales, decline, with no spillovers into violent or property crime.

Second, we consider changes to our main specification and show TWFE DID estimates from each of these modifications in Figures S26 through S31. Specifically, we add region-by-year FE, calculate wild cluster bootstrap standard errors over 999 repetitions, drop and add policy control variables, restrict to more stringent coverage indicator thresholds, drop outliers, and change the RCL variable for an indicator for when recreational dispensaries became available. We also explore specifications excluding one RCL state at a time in Figures S32 through S37. Overall, we obtain very similar results.

Third, Table S5 shows that results are robust to controlling for potential spillovers across jurisdictions. For non-RCL states, we consider an indicator for whether an RCL had been implemented within 100 miles of the county, then add an indicator for an RCL within 100-200 miles, and lastly, include the inverse distance to the nearest county with an RCL. We also simply drop all RCL border states from the estimation. In all cases, we obtain similar results.

Fourth, we address potential bias from treatment effect heterogeneity in staggered DID designs recently identified in the literature. We first show the share of negative weights in these estimations and the sum of negative weights in Table S2. Reassuringly, we find that only a small fraction of the average treatment on the treated effects are negatively weighted in the TWFE regressions.[13] Moreover, the sum of negative weights is very small. Second, we calculate heterogeneity robust DID estimators in Figures S26 through S31. Lastly, we present the equivalent dynamic Sun and Abraham (2021) estimator in Figures S38 and S39. Results provide additional reassurance that the effects hold when accounting for heterogeneous treatment effects.

Finally, we note that our arrest data captures only the most serious offense per arrest incident. One possible interpretation of the estimated decline in cannabis arrests is that individuals were still arrested after RCLs, but the arrests were recorded under different offenses. However, most drug-related incidents involve only drug offenses.[14] If our results were driven by post-legalization changes in how arrests are tallied, we would expect to see an increase in arrests for other drug-defined offenses, which we do not observe. Thus, while the data structure does not capture all arrests in multiple offense cases, the decline in cannabis arrests cannot be merely attributed to changes in how these cases are tallied.

4.2 Incarcerations

We next examine the downstream outcome of incarceration, which could be influenced by documented decreases in arrests for drug-defined offenses or potential increases in arrests for Part 2 offenses. We analyze the flow of prisoners with prison admission rates and the stock with yearend rates.

[13] TWFE will more likely assign negative weights to periods with a large fraction of treated states and to states treated for many periods (De Chaisemartin and d'Haultfoeuille, 2020).

[14] Appendix Table S3 shows that 64% of drug-related incidents involve a single offense, 31% involve two offenses, and 5% involve three or more. In cases with two offenses, 75% are solely for drug violations.

Column (1) in Table 3 reports prison admissions for drug-defined offenses, including cannabis and other illegal drugs. We find a significant reduction of 0.71 admissions per 10,000 for White persons, or 34% of the baseline mean. We find no significant effects for Black persons. Columns (2)-(4) show prison admissions for other offenses, with small and insignificant point estimates across groups and offense categories. Net effects in Column (5) suggest insignificant changes in total prison admissions across groups, although the point estimate for White persons would imply a decline of 9.6%. Net effects in Column (6) are also insignificant for the total stock of prisoners at yearend. Figures 3 and S20 show event study plots for prison admissions, providing reassurance on our identification assumption and echoing DID results. We test the robustness of estimates in Figure S40 and consistently find that admissions for drug-defined offenses declined for White but not for Black persons.

Together, we find that only White persons benefited from reductions in prison admissions for drug-defined offenses following RCLs. These reductions align with documented declines in arrests for cannabis-defined offenses and for other illegal drug sales among White persons. While cannabis possession rarely leads to federal or state imprisonment, illegal drug sales can. Our data cannot elucidate why Black persons do not experience similar declines. Research suggests that racial differences in criminal histories might partially explain these findings. White persons are more often placed in drug treatment diversion programs instead of prison in part because they are less likely to have a criminal history (Nicosia et al., 2013). As for spillovers, null effects in prison admissions for violent and property crimes align with null effects in Part 1 arrests. While we document suggestive evidence of increases in arrests for low-level Part 2 offenses among Black persons, prison admissions for Part 2 offenses do not change. Since Part 2 offenses rarely lead to imprisonment of over a year, any potential increases in arrests may not appear in prison data. However, potential contact with the criminal legal system, even for minor offenses, generates criminal records and disrupts labor market ties, increasing racial disparities (Dobbie et al., 2018; Agan et al., 2022).

5 Criminal Activity and Pathways

We documented sizable declines in arrests for cannabis-defined offenses, reflecting the direct effects of RCLs through changes in the prohibition of cannabis possession and sales. We also documented declines in arrests for other illegal drug sales along with suggestive increases in Part 2 arrests, reflecting the spillover effects of RCLs. Spillover effects on arrests could stem from changes in: (1) criminal activity, through pathways related to cannabis use, other illegal drug use, and illegal drug markets (i.e., systemic violence); and (2) law enforcement efforts, through pathways related to police resources, priorities, and incentives. This section examines the effect of RCLs on measures of criminal activity and associated pathways, as well as on law enforcement resources.

5.1 Criminal Activity

We analyze incident data on calls for service and crimes *within* select RCL cities, comparing minority neighborhoods to non-minority neighborhoods in the same city before and after legalization. Figure 4 plots the average number of total incidents in a tract-quarter-year by city. We find that the number of incidents does not change or even decreases in minority neighborhoods following legalization, with similar trends in non-minority neighborhoods.[15]

[15] Figures S7, S8, and S9 stratify incidents into violent, property, and Part 2 offenses.

Table 4 reports city-specific and pooled DID estimates.[16] Since all cities analyzed are in RCL states, minority and non-minority neighborhoods are both exposed to RCL implementation. Therefore, coefficients have a different interpretation that in previous analyses, reflecting the differential effect of RCLs on criminal activity in minority relative to non-minority neighborhoods. Column (1) shows that incidents involving drug-defined offenses decline differentially in minority neighborhoods across many cities. Pooled estimates imply a differential reduction of 1.41 incidents, a 45% decline relative to the baseline mean. Columns (2)-(4) show that violent, property, and Part 2 offenses do not increase differentially in minority neighborhoods of nearly all cities. Pooled estimates imply a differential decline of 8% for property crimes, and small and insignificant effects for violent crimes and Part 2 offenses. Table S7 shows that DID estimates are robust to changes in the classification of minority and non-minority neighborhoods.

We stratify Part 2 offenses in Table S8 and find heterogeneity across offenses and cities. Pooled estimates show differential declines in financial crimes (17%) and simple assault (7%), while vandalism (5%) and DUI (10%) increase differentially.[17] We further explore the latter with data on DUI arrests and DUI traffic fatalities (Appendix F.3). Figures S19 and S44 show insignificant effects in DUI arrests and traffic fatalities involving drugs across races.

In sum, RCLs appear to have modest differential effects on criminal activity as measured through calls for service and reported crimes. Because estimates

[16] We present city-specific results due to heterogeneity in the collection, coding, and reporting of call and crime data across cities (Bennett, 2018; Neusteter et al., 2019; Lum et al., 2022). There is no standardized system for recording calls. Each city has its own classification methods for calls and crime reporting, reflecting local practices in managing the call system. Even when using standard criminal offense categories, such as disorderly conduct, the assignment of incidents to these categories can differ from one city to another.

[17] Financial crimes include fraud, embezzlement, forgery, money laundering, etc.

reflect relative differences between minority and non-minority neighborhoods, we cannot rule out the possibility of absolute increases in criminal activity within either group. However, descriptive evidence from the raw plots does not support absolute increases in criminal activity across the cities analyzed. For specific offense categories, drug-defined offenses decline differentially following legalization, consistent with a disruption of illegal drug markets in minority neighborhoods. Part 1 and Part 2 offenses do not increase differentially in minority neighborhoods following legalization, and even decrease differentially for property crimes, financial crimes, and simple assault. While we observe small differential increases in certain Part 2 offenses potentially linked to the psychoactive effects of cannabis use (e.g., DUI), these do not appear to impact more severe outcomes like DUI-related fatalities or arrests.

Comparing the differential effects on criminal activity in Table 4 with the Black-White arrest rate difference in Table 1, we observe alignment for drug-defined offenses and violent crimes, though not for property crimes and Part 2 offenses. Several factors may help explain why patterns differ in these categories. First, the call and crime data are drawn from a subset of RCL cities and may not generalize nor fully reflect offense definitions and distributions in the nationwide UCR arrest data. Second, the pre-trend in Part 2 arrest estimates—particularly for Black persons—in our main specification raises the possibility that the true effect may be null, which would be consistent with criminal activity estimates. Third, arrests are not only a function of criminal activity but also of law enforcement efforts. The null differential effects on criminal activity for Part 2 offenses appear inconsistent with an explanation that increased Part 2 arrests among Black persons stem from heightened low-level offending. Therefore it is possible that instead, the observed increase in Part 2 arrests may reflect shifts in law enforcement behavior rather than changes in underlying criminal activity. We next explore additional pathways to better understand these effects.

5.2 Pathways

Cannabis Use. Columns (1)-(2) in Table 5 examine hospitalizations involving cannabis. We find significant increases in cannabis use disorder and poisoning hospitalization rates for White (20% and 32%) and Black (21% and 78%) persons. Event study plots in Figure 5 support a causal interpretation.

Results suggest spillover effects on healthcare utilization associated with cannabis use, although we cannot rule out greater cannabis reporting following RCLs. Greater cannabis use, particularly patterns indicative of abuse, dependence, or poisoning, may influence criminal activity related to economic crime and the psychoactive effects of use. While our analyses of criminal activity in Table 4 do not support the economic crime pathway, the psychoactive effects of cannabis may help explain observed increases in certain Part 2 offenses (e.g. DUI). Our results align with previous RCL studies documenting increases in self-reported cannabis use, cannabis use disorder, and cannabis poisoning in the general population (Hollingsworth et al., 2022; Cerdá et al., 2020; Allaf et al., 2023).

Other Illegal Drug Use. Columns (3)-(4) in Table 5 examine hospitalizations involving other illegal drugs (opioids, methamphetamines, cocaine), which may change depending on whether these are complements or substitutes of cannabis. We find significant increases in illegal drug use disorder (25%) and poisoning (19%) hospitalization rates for Black persons, and positive but insignificant effects for White persons. Event study plots are in Figure S22.

Results suggest spillovers on other illegal drug use, which may influence criminal activity related to other drug-defined offenses, psychoactive effects, and economic crime. Alternatively, results could be related to the emergence of highly potent illicit fentanyl, therefore reflecting changes in the potency rather than in the demand of other illegal drugs. Our findings align with previous RCL studies documenting increases in illegal drug use and mortality (Liu et al., 2025; Mathur and Ruhm, 2023).

Systemic Violence. Legalization may reduce illicit cannabis distribution and associated systemic violence, but could also trigger turf wars among remaining illegal suppliers. We examine this in Columns (5)-(8) of Table 5 with total assault hospitalizations and homicides per 10,000 persons, and those involving gun violence. We find small and insignificant effects in assault hospitalizations for White and Black persons. We also find insignificant effects in homicides for White persons, but significant declines of 16% for Black persons that are entirely driven by gun violence. Figures S23 and S24 show event study plots. Robustness checks in Figure S41 show consistently negative estimates for Black persons across specifications, although some lose significance. We also find that homicide declines are driven by CA and MA, suggestive of treatment effect heterogeneity in this outcome.

Together, RCLs did not increase and possibly decreased systemic violence in some areas, particularly for Black persons. These findings coincide with documented null differential effects on criminal activity involving of violent crime in Table 4. Results provide suggestive evidence that illegal cannabis markets may not be significant drivers of violence, and are thus unlikely to be key mechanisms behind our findings.

5.3 Police Officers

Cannabis legalization may prompt shifts in policing priorities, while increased tax revenues from legal cannabis may bolster law enforcement resources. Although we do not observe specific policing tactics, we examine the effect of RCLs on police officers per 10,000 persons in Figure 6, with corresponding DID estimates in Table S6. We find a significant increase in total police officers (about 7% relative to the mean) driven largely by a 16% rise in civil police officers. While the DID estimate for sworn officers is not statistically significant, it suggests a 5% increase.

The estimated increase in police officers may reflect an operational response to legalization, potentially tied to shifting enforcement priorities or the availability of new resources. This is evident by the rise in civil officers, who enhance support and offer greater flexibility in task assignments (Forst, 2000). Civilian officers can provide specialized expertise in areas where sworn officers may have limited training, such as computing or stenography, and can assist directly with police-based diversion initiatives focused on mental health and substance abuse (Forst, 2000; McCarty and Skogan, 2013; Alderden and G. Skogan, 2014; Osher, 2018), thereby supporting potential new enforcement priorities associated with legalization. Our findings suggest that increased law enforcement resources, reflected in a rise in the number of officers, and a reduced need to police cannabis-defined offenses likely allow for a reallocation of efforts to other activities, even though we cannot directly observe changes in police behavior.

6 Conclusion

There is a pervasive and enduring pattern of racial disparities in the enforcement of drug prohibition, impacting Black communities disproportionately. This study provides the most comprehensive evidence to date of the direct and spillover effects of cannabis legalization on racial disparities in the criminal legal system. Although our results are generally robust, there are limitations and open questions. First, there are not many available measures of criminal activity by race. Moreover, calls for service and reported crime data are not systematically available nor uniform across a wide range of cities. Second, we cannot directly observe how police resources are allocated by law enforcement agencies, their policing strategies, nor the incentives that they face. Third, we do not observe prosecutorial decisions after arrests are made. Lastly, since RCLs have only been adopted by 11 states as of 2019 (the last year in our data), our estimates may not generalize for future RCL states or in the long-term. Notably, most RCL states in our sample are liberal and have a low proportion of Black persons.

Cannabis legalization is an important step toward addressing the overenforcement of drug prohibition and related racial disparities. However, additional policies for effective oversight mechanisms and provisions that consider racial disparities and address law enforcement incentives, particularly in Black communities, are needed. This may involve not tying funding to low-level offense arrests, granting clemency and expunging records, and ensuring minority communities benefit economically from legalization. Moreover, the racial disparities observed within the criminal legal system stem from long-standing barriers, such as segregation and poverty, which contribute to both violence and inequality (Ananat, 2011; Cox et al., 2022), as well as to targeted policing in Black neighborhoods (Beckett et al., 2006; Goncalves and Mello, 2021; Feigenberg and Miller, 2022). Therefore, to effectively mitigate disparities in arrests, it is crucial to concurrently address racial inequities in economic outcomes, thereby promoting broader social equity in conjunction with legalization efforts.

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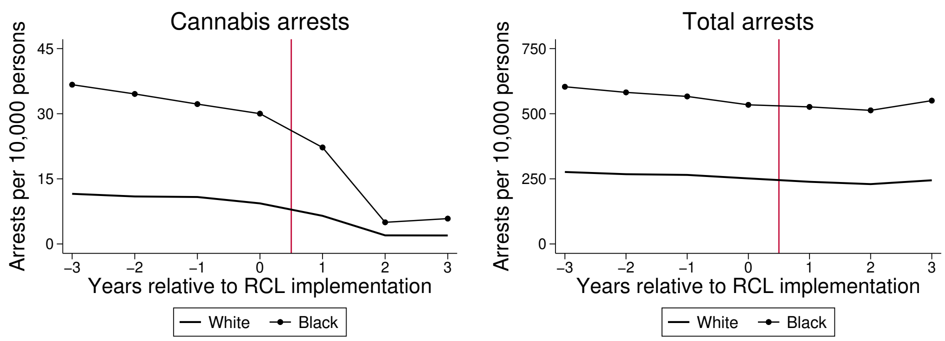

Figure 1: Arrest rates, raw plots

Notes: Arrests are from the 2007-19 Uniform Crime Reports Arrests by Age, Sex, and Race. Arrests for cannabis-defined offenses include both possession and sales. County-year counts for a given race are divided by county-year population estimates corresponding to that race, and multiplied by 10,000. Sample restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Race-specific population weighted averages calculated for periods relative to RCL implementation. The time $t = 0$ is the period immediately before RCL implementation. RCL=Recreational cannabis laws.

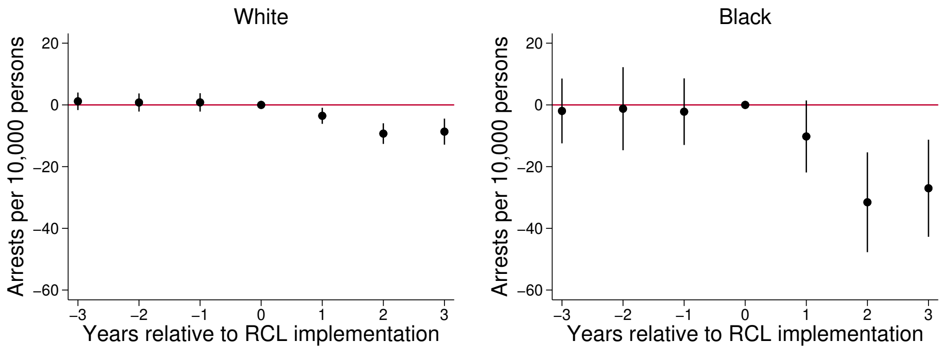

Figure 2: Cannabis arrests rates, event study

Notes: Arrests are from the 2007-19 Uniform Crime Reports Arrests by Age, Sex, and Race. Arrests for cannabis-defined offenses include both possession and sales. County-year counts for a given race are divided by county-year population estimates corresponding to that race, and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq$ 65%. Coefficients and state-level clustered 95% confidence intervals are based on an event study approach (Equation 2). Regressions are weighted by race-specific population estimates. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

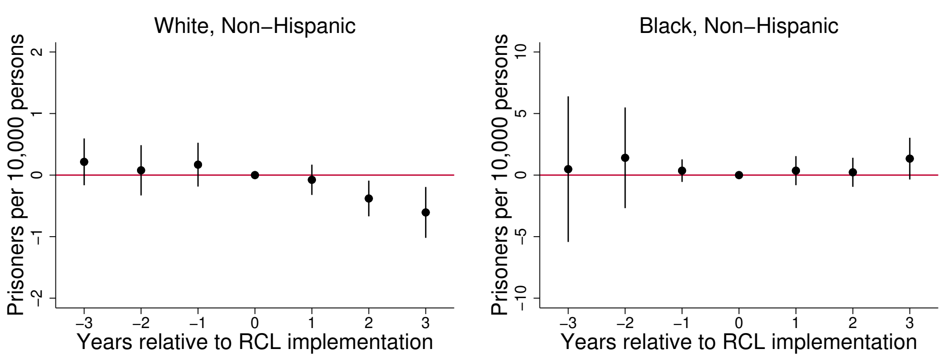

Figure 3: Prison admissions for drug-defined offenses, event study

Notes: Prison admissions are from the 2007-2019 National Corrections Reporting Program. State-year counts for a given race are divided by state-year population estimates corresponding to that race, and multiplied by 10,000. Coefficients and state-level clustered 95% confidence intervals are based on an event study approach (Equation 2). Regressions are weighted by race-specific population estimates. Controls include cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

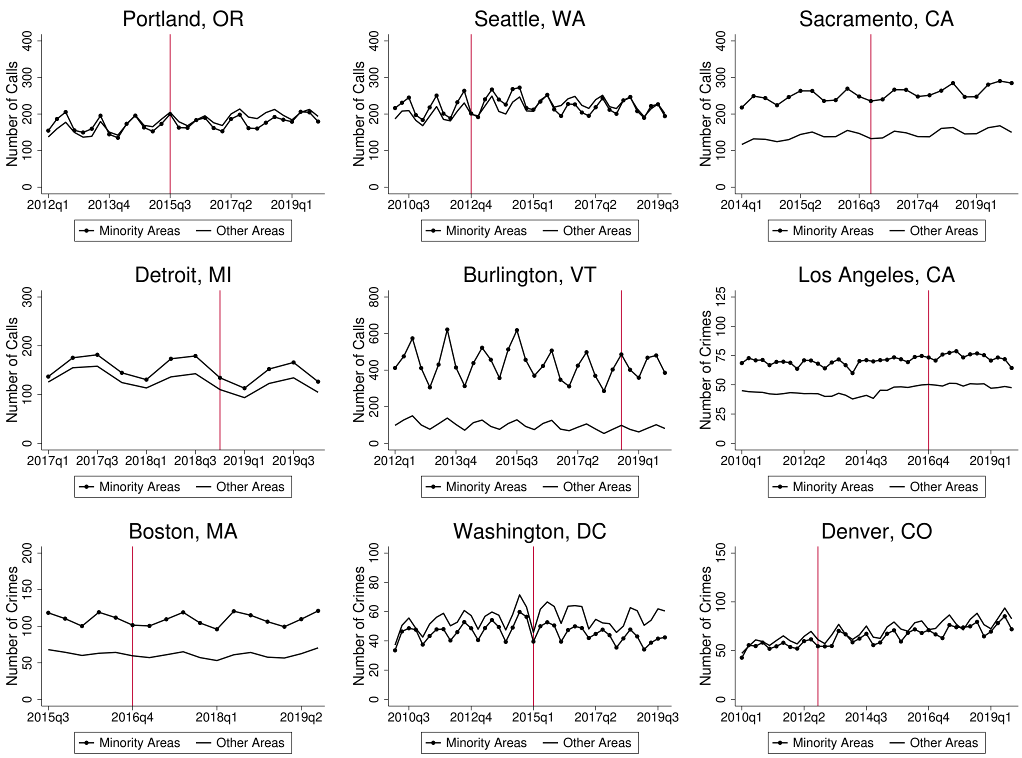

Figure 4: Total criminal activity, raw plots

Notes: Criminal activity for Part 1 and Part 2 offenses are drawn from calls for service and reported crime data in select RCL cities. Portland (C, E, NE), Seattle (CI, E, NE), Burlington (CI, E, NE), Detroit (CI, E), and Sacramento (CI, PI, E, NE) reflect calls for service. Los Angeles, Denver, District of Columbia, and Boston reflect reported crimes. Outcomes reflect total incident counts in a tract-year-quarter, stratified by minority tracts. Due to differences in collection and reporting of incidents, Part 1 and Part 2 measurement can be inconsistent across cities. See Sections 2 and F.2 for details. The vertical red line indicates the year-quarter of RCL implementation. CI=Civilian initiated calls for service. PI=Police initiated calls for service. E=Emergency calls for service. NE=Non-emergency calls for service.

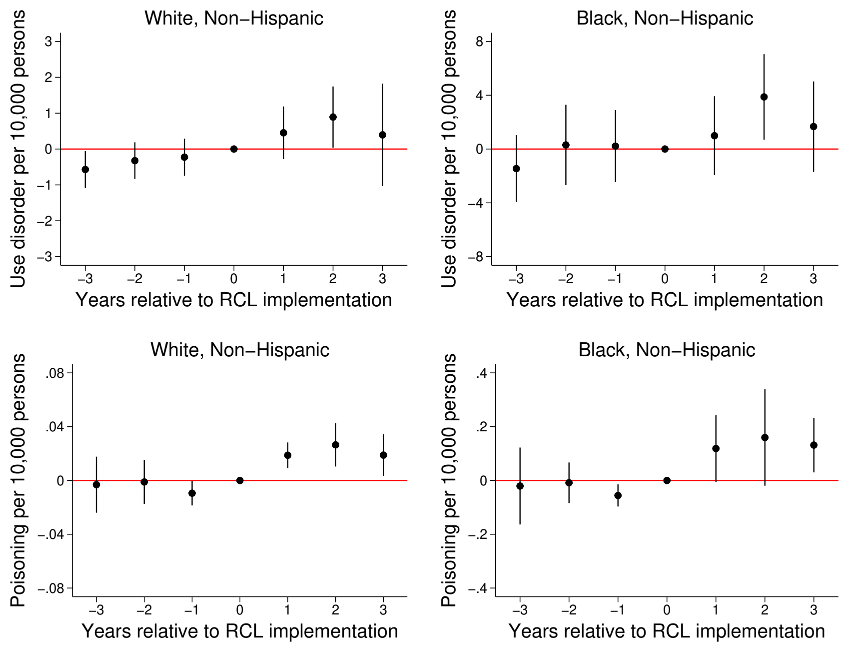

Figure 5: Cannabis hospitalizations, event study

Notes: Inpatient discharges involving diagnoses of cannabis use disorder and poisoning are from the 2007-2019 HCUP State Inpatient Databases. State-year-quarter counts for a given race are divided by state-year population estimates corresponding to that race, and multiplied by 10,000. Coefficients and state-level clustered 95% confidence intervals are based on an event study approach (Equation 2). Regressions are weighted by race-specific population estimates. Controls include cannabis decriminalization laws. The reference year is $t = 0$, the year (four quarters) immediately before RCL implementation. RCL=Recreational cannabis laws.

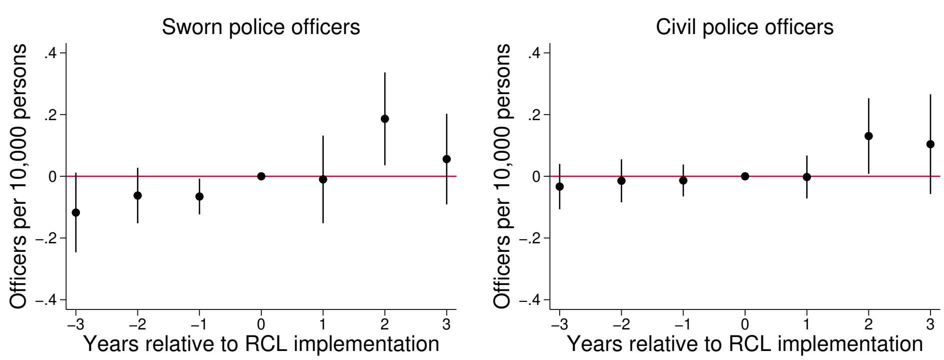

Figure 6: Police officers, event study

Notes: Police officer counts are from the 2007-2019 LEOKA Databases. County-year counts are divided by county-year population estimates, and multiplied by 10,000. Coefficients and state-level clustered 95% confidence intervals are based on an event study approach (Equation 2). Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Regressions are weighted by total population estimates. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

Table 1: Effect of recreational cannabis laws on arrests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Drug-defined offenses				Other offenses			Net effects
	Cannabis		Other illegal drugs		Part 1			
	Possession	Sales	Possession	Sales	Violent	Property	Part 2	Total
Population	-7.52***	-1.34***	0.53	-1.04***	0.11	2.05	9.66**	2.45
	(2.76)	(0.28)	(1.47)	(0.32)	(0.61)	(1.65)	(4.78)	(6.31)
Mean	12.63	3.41	37.18	7.20	27.06	42.83	159.50	289.81
N	30539	30539	30539	30539	30539	30539	30539	30539
White	-7.31***	-1.24***	0.55	-1.38***	-0.11	0.86	5.86	-2.77
	(2.44)	(0.27)	(1.96)	(0.25)	(0.43)	(1.62)	(4.72)	(5.59)
Mean	11.76	2.79	38.30	6.28	24.13	41.14	163.09	287.50
N	30539	30539	30539	30539	30539	30539	30539	30539
Black	-18.86**	-6.64***	-0.66	-4.47***	0.50	3.26	32.46**	5.58
	(8.54)	(1.15)	(2.70)	(1.48)	(3.62)	(4.30)	(14.72)	(22.00)
Mean	36.92	13.50	72.47	25.69	88.00	107.19	295.60	639.36
N	30539	30539	30539	30539	30539	30539	30539	30539
Rate Diff	-14.10**	-5.25***	-2.11	-3.50***	0.64	3.80	26.22***	5.70
	(6.72)	(0.91)	(3.24)	(1.26)	(2.99)	(2.99)	(8.44)	(14.87)
Mean	26.16	10.73	36.76	19.47	63.92	68.29	150.26	375.59
N	30539	30539	30539	30539	30539	30539	30539	30539
Rate Ratio	0.14	-0.75	0.26**	-0.12	0.02	0.10	0.07	0.00
	(0.18)	(0.51)	(0.13)	(0.22)	(0.08)	(0.08)	(0.06)	(0.06)
Mean	3.14	5.12	2.39	5.21	4.34	2.91	2.11	2.55
N	28962	22515	28887	25203	29271	29862	30465	30470

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. Effect of recreational cannabis laws on rates, rate differences, and rate ratios by race. County-year counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Rate differences and rate ratios are relative to the White group. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Each coefficient is based on a separate two-way fixed effects regression (Equation 1). Regressions are weighted by race-specific population. All regressions include county and year fixed effects. Control variables include the number of reporting agencies and cannabis decriminalization laws. Standard errors clustered by state are in parentheses. The pre-policy outcome mean is reported for RCL states. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: Effect of recreational cannabis laws on arrests, by prior presence of cannabis decriminalization law

	Drug-defined offenses				Non-drug offenses		Net effects
	Cannabis		Other illegal drugs				
	Possession	Sales	Possession	Sales	Part 1	Part 2	Total
Population							
RCL × CDL_{pre}	-2.563**	-1.415***	1.005	-1.047***	1.212	6.423	3.615
	(1.196)	(0.324)	(1.903)	(0.335)	(1.817)	(4.251)	(5.334)
RCL × $(1 - CDL_{pre})$	-15.174***	-1.231***	-0.205	-1.021**	3.609	14.667**	0.646
	(1.785)	(0.408)	(1.866)	(0.496)	(3.590)	(6.367)	(11.370)
Coefficient test	0.00	0.70	0.65	0.96	0.52	0.20	0.79
White							
RCL × CDL_{pre}	-2.761***	-1.386***	2.061	-1.287***	-0.124	2.097	-1.401
	(1.029)	(0.307)	(2.488)	(0.183)	(1.381)	(4.718)	(4.458)
RCL × $(1 - CDL_{pre})$	-13.563***	-1.046***	-1.517	-1.498***	1.934	11.036**	-4.653
	(1.797)	(0.321)	(1.785)	(0.493)	(3.447)	(5.425)	(10.36)
Coefficient test	0.00	0.40	0.25	0.67	0.56	0.16	0.75
Black							
RCL × CDL_{pre}	-6.819	-7.161***	-3.518	-5.734***	-1.164	17.728*	-6.668
	(4.449)	(1.481)	(2.582)	(1.578)	(7.031)	(9.582)	(20.322)
RCL × $(1 - CDL_{pre})$	-40.784***	-5.700***	4.543	-2.175	12.725	59.278***	27.888
	(11.545)	(1.203)	(4.940)	(1.838)	(9.921)	(18.623)	(35.856)
Coefficient test	0.01	0.40	0.14	0.08	0.22	0.02	0.34

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Effect of recreational cannabis laws on arrest rates, by race. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Each pair of coefficients is based on separate two-way fixed effects regressions (see Equation 1). The RCL treatment indicator is interacted with an indicator for the presence (CDL_{pre}) or absence ($1 - CDL_{pre}$) of cannabis decriminalization laws (CDL) prior to RCL implementation. The p-value of a test of equality of coefficients is shown. All regressions include county and year fixed-effects. Control variables include the number of reporting agencies and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.

Table 3: Effect of recreational cannabis laws on incarcerations

	(1)	(2)	(3)	(4)	(5)	(6)
	Drug-defined offenses	Other offenses			Net effects	
		Part 1				
		Violent	Property	Part 2	Total	Total
Population	0.15	0.04	0.53*	-0.82	-0.35	0.97
	(0.38)	(0.31)	(0.29)	(2.19)	(2.89)	(1.85)
Mean	3.20	5.32	3.57	8.98	21.35	45.06
N	490	490	490	490	490	513
White, NH	-0.71***	-0.12	0.17	-0.61	-1.44	-1.38
	(0.23)	(0.20)	(0.17)	(1.54)	(1.96)	(1.10)
Mean	2.06	3.42	2.99	6.38	15.06	29.03
N	490	490	490	490	490	513
Black, NH	0.77	0.03	1.14	-0.56	0.35	-2.60
	(2.34)	(1.01)	(1.12)	(2.88)	(5.65)	(9.30)
Mean	13.39	17.98	9.75	19.21	61.28	195.73
N	490	490	490	490	490	513
Rate Difference	1.32	0.01	0.84	-0.32	0.99	-1.73
	(2.37)	(0.94)	(0.97)	(1.89)	(4.81)	(8.82)
Mean	11.79	15.66	7.41	14.52	50.14	166.19
N	490	490	490	490	490	513
Rate Ratio	-25.42	-0.54	-5.22	1.86	-3.41	-0.11
	(23.34)	(0.74)	(5.01)	(1.93)	(3.27)	(0.19)
Mean	40.14	14.52	11.36	15.29	16.25	6.85
N	490	490	488	489	490	513

Notes: Prison admissions in Columns (1)-(5) are from the 2007-2019 National Corrections Reporting Program. Prisoners at yearend in Column (6) are from the 2009-2019 National Prisoner Statistics. Effect of recreational cannabis laws on rates, rate ratios, and rate differences by race. State-year counts for a given race are divided by state-year population estimates corresponding to that race, and multiplied by 10,000. Rate ratios and rate differences are relative to the Non-Hispanic White group. Each coefficient is based on separate two-way fixed effects regressions (Equation 1). Regressions are weighted by race-specific population. All regressions include state and year fixed effects. Control variables include cannabis decriminalization laws. Standard errors clustered by state are in parentheses. The pre-policy outcome mean is reported for RCL states. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Effect of recreational cannabis laws on criminal activity

	(1)	(2)	(3)	(4)	(5)
	Drug-defined offenses	Other offenses			Net effects
		Part 1			
		Violent	Property	Part 2	Total
Pooled	-1.40***	-0.58	-2.51***	-1.83	-4.81**
	(0.29)	(0.36)	(0.81)	(1.84)	(2.06)
Mean	3.12	15.72	31.32	78.57	107.20
Portland, OR	-2.32***	-1.36	-1.20	-16.42**	-18.98*
	(0.61)	(1.04)	(2.38)	(7.47)	(10.10)
Mean	5.06	12.25	40.72	114.52	167.49
Seattle, WA	-2.93***	-3.95	-8.13**	-8.14	-23.15
	(1.12)	(3.12)	(3.56)	(10.55)	(15.44)
Mean	6.55	33.95	73.21	106.97	220.68
Burlington, VT	-2.39***	1.04*	-16.76	30.29	12.18
	(0.65)	(0.56)	(13.24)	(28.97)	(16.68)
Mean	9.98	3.52	75.21	344.44	433.15
Detroit, MI	0.12	-1.58	1.20	1.89	1.63
	(0.28)	(1.08)	(1.24)	(5.76)	(7.83)
Mean	1.98	22.92	22.98	112.29	160.16
Sacramento, CA	-0.18	-1.47*	-1.17	7.15	4.33
	(0.36)	(0.87)	(2.30)	(12.10)	(13.94)
Mean	4.02	20.64	40.72	180.03	245.41
Washington, DC	n.a.	-0.99***	-4.31**	n.a.	-5.30***
	n.a.	(0.36)	(1.95)	n.a.	(2.00)
Mean	n.a.	12.90	34.02	n.a.	46.92
Los Angeles, CA	n.a.	0.81	-1.85	-1.07	-2.11
	n.a.	(0.64)	(1.13)	(1.35)	(2.85)
Mean	n.a.	11.34	29.88	28.41	69.63
Denver, CO	-1.87***	0.29	-0.78	0.37	-1.99
	(0.67)	(0.42)	(5.39)	(2.62)	(8.09)
Mean	2.11	4.47	32.26	17.14	54.80
Boston, MA	-1.13	-0.57	0.38	1.34	0.08
	(0.73)	(0.39)	(0.97)	(1.48)	(2.16)
Mean	6.83	10.95	24.91	74.28	111.97

Notes: Calls for service and reported crime data in select RCL cities. Portland (C, E, NE), Seattle (CI, E, NE), Burlington (CI, E, NE), Detroit (CI, E), and Sacramento (CI, PI, E, NE) reflect calls for service. Los Angeles, Denver, District of Columbia, and Boston reflect reported crimes. Tract-year-quarter incident counts are stratified by minority neighborhoods. Standard errors clustered at the tract level are in parentheses. Due to differences in collection and reporting of incidents, measurement of offense categories can be inconsistent across cities. See Sections 2 and F.2 for details. CI=Civilian initiated calls for service. PI=Police initiated calls for service. E=Emergency calls for service. NE=Non-emergency calls for service. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Effect of recreational cannabis laws on pathways

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Drug Use				Systemic Violence			
	Cannabis		Other illegal drugs		Assaults		Homicides	
	Use disorder	Poisoning	Use disorder	Poisoning	Total	Gun	Total	Gun
Population	0.571	0.008	0.935	-0.024	0.030	0.001	-0.016	-0.018**
	(0.454)	(0.012)	(0.898)	(0.106)	(0.047)	(0.009)	(0.010)	(0.009)
Mean	6.156	0.085	10.725	2.624	1.152	0.195	0.16	0.11
N	1492	1492	1492	1492	1492	1492	2652	2652
White, NH	1.106**	0.024**	0.748	0.013	-0.000	0.002	0.001	-0.001
	(0.515)	(0.009)	(0.997)	(0.108)	(0.024)	(0.003)	(0.004)	(0.003)
Mean	5.671	0.074	9.071	2.516	0.665	0.041	0.07	0.04
N	1492	1492	1492	1492	1492	1492	2652	2652
Black, NH	3.922**	0.128***	6.134*	0.677***	-0.028	-0.076	-0.131**	-0.137**
	(1.856)	(0.047)	(3.136)	(0.193)	(0.211)	(0.052)	(0.058)	(0.056)
Mean	18.921	0.165	24.00	3.501	4.207	1.179	0.80	0.65
N	1492	1492	1492	1492	1492	1492	2652	2652
Rate Difference	2.575*	0.102**	4.530**	0.636**	-0.036	-0.079	-0.131**	-0.134**
	(1.33)	(0.045)	(2.154)	(0.234)	(0.187)	(0.05)	(0.056)	(0.055)
Mean	12.851	0.086	15.38	1.119	3.524	1.136	0.72	0.61
N	1492	1492	1492	1492	1492	1492	2652	2652
Rate Ratio	0.006	0.400	0.036	0.176	-0.097	-1.410	-1.051	-0.844
	(0.164)	(0.469)	(0.167)	(0.105)	(0.303)	(2.383)	(0.820)	(1.722)
Mean	3.256	1.958	3.35	1.536	6.904	33.42	11.66	18.97
N	1492	1476	1492	1492	1492	1397	2605	2501

Notes: Inpatient hospital discharges in Columns (1)-(6) are from the 2007-2019 HCUP State Inpatient Databases. Homicides in Columns (7)-(8) are from 2007-2019 NVSS Mortality Files. Effect of recreational cannabis laws on rates, rate ratios, and rate differences by race. State-year-quarter counts of adults aged 18+ of a given race are divided by state-year population estimates of adults aged 18+ corresponding to that given race, and multiplied by 10,000. Each coefficient is based on separate two-way fixed effects regressions (Equation 1). Regressions are weighted by race-specific population. All regressions control for state and year-quarter fixed effects and cannabis decriminalization laws. Standard errors clustered at the state level are in parentheses. The pre-policy outcome mean is reported for RCL states. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Supplementary Materials

A Effective Dates

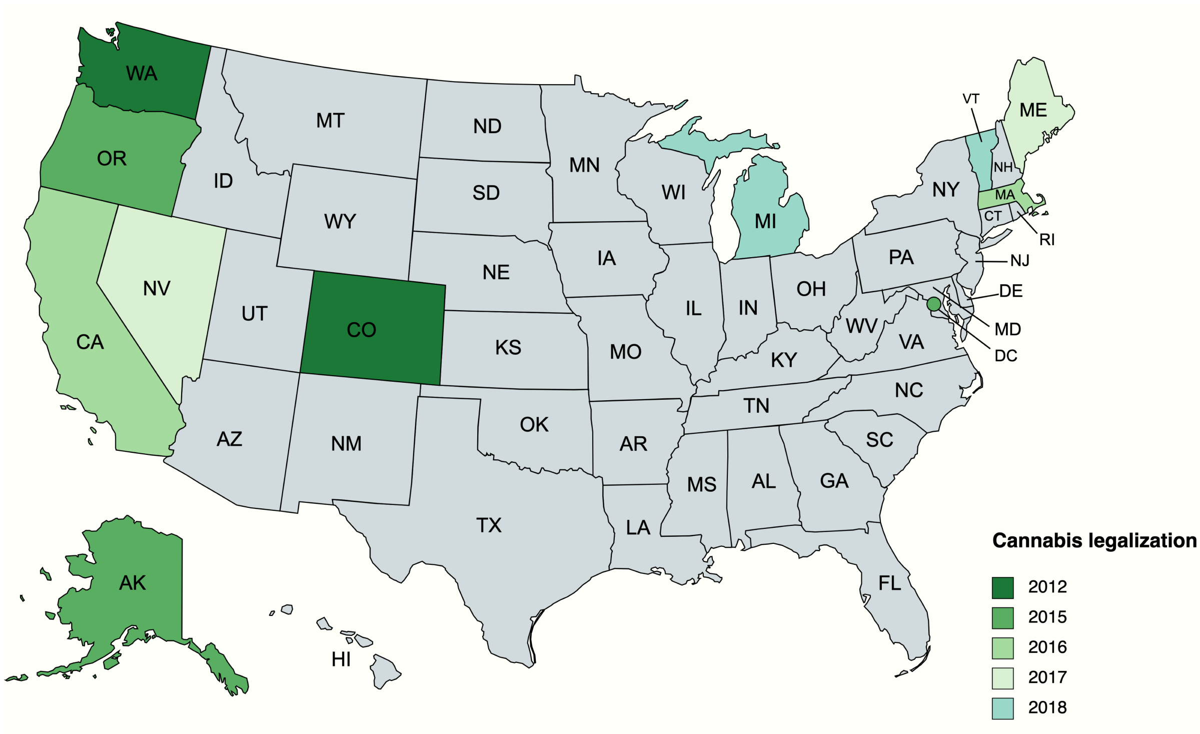

Figure S1: Implementation of recreational cannabis laws by state

Notes: The map shows the spatial roll-out of RCLs across states and over time as of 2019, using data in Table S1. RCL=Recreational cannabis laws.

Table S1: Effective dates of cannabis liberalization policies

State	MCL	CDL	RCL	RCD	CEL
AK	3/4/1999		2/24/2015	10/29/2016	
AZ	11/29/2010				
AR	11/9/2016				
CA	11/6/1996	1/1/2011	11/9/2016	1/1/2018	7/1/2019
CO	12/28/2000		12/10/2012	1/1/2014	6/6/2017
CT	10/1/2012	1/7/2011			
DE	7/1/2011	12/18/2015			8/29/2018
DC	7/27/2010		2/26/2015		
FL	1/3/2017				
HI	6/14/2000				
IL	1/1/2014	7/29/2016			
LA	5/19/2016				
ME	12/23/1999		1/30/2017		
MD	6/1/2014	1/10/2014			10/1/2017
MA	1/1/2013	1/1/2009	12/15/2016	11/20/2018	4/13/2018
MI	12/4/2008		12/6/2018	12/1/2019	
MN	5/30/2014				
MO	12/6/2018				
MT	11/2/2004				
NV	10/1/2001		1/1/2017	7/1/2017	
NH	7/23/2013	9/16/2017			
NJ	6/1/2010				
NM	7/1/2007	1/7/2019			
NY	7/5/2014	7/29/2019			8/28/2019
ND	12/8/2016	5/1/2019			7/10/2019
OH	9/8/2016				
OK	7/26/2018				
OR	12/3/1998		7/1/2015	10/1/2015	
PA	5/17/2016				
RI	1/3/2006	4/1/2013			
UT	12/3/2018				
VT	7/1/2004	1/7/2013	7/1/2018		
WA	12/3/1998		12/6/2012	7/8/2014	7/27/2019
WV	7/1/2019				

Notes: Effective dates of cannabis liberalization policies as of 2019. Information is taken from ProCon (2022); RAND (2020); Edwards et al. (2020); Grucza et al. (2018); Gunadi and Shi (2022); NORML (2022). MCL = Medical cannabis laws, RCL = Recreational cannabis laws, CDL = Cannabis decriminalization laws, RCD = Recreational cannabis dispensaries, CEL=Cannabis record expungement laws.

B Raw Data Plots

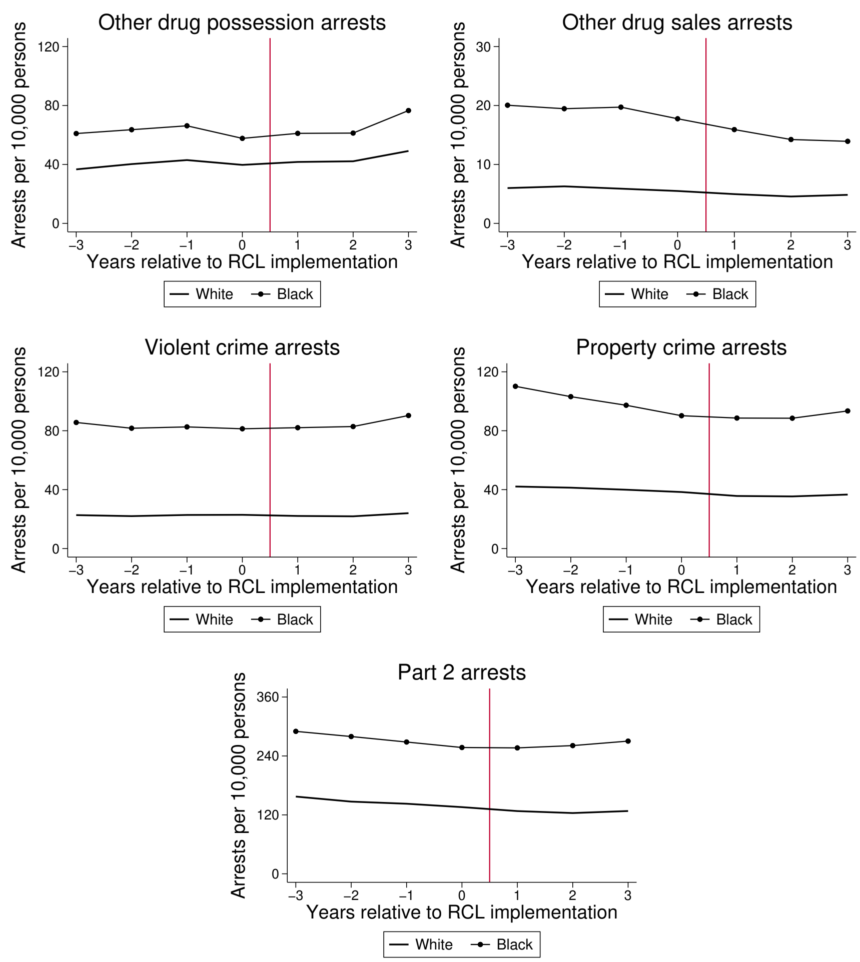

Figure S2: Arrest rates, raw plots

Notes: 2007-19 Uniform Crime Reports Arrests by Age, Sex, and Race. County-year counts for a given race are divided by county-year population estimates corresponding to that race, and multiplied by 10,000. Sample restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Race-specific population weighted averages calculated for periods relative to RCL implementation. The time $t = 0$ is the period immediately before RCL implementation.

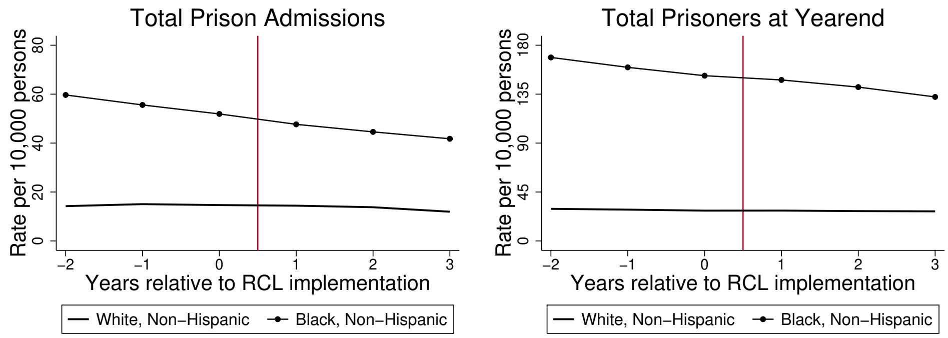

Figure S3: Incarceration rates, raw plots

Notes: Prison admissions are from the 2007-19 National Corrections Reporting Program. Prisoners at yearend are from the 2009-19 National Prisoner Statistics. State-year counts for a given race are divided by state-year population estimates corresponding to that race, and multiplied by 10,000. Race-specific population weighted averages calculated for periods relative to RCL implementation. The time $t = 0$ is the period immediately before RCL implementation.

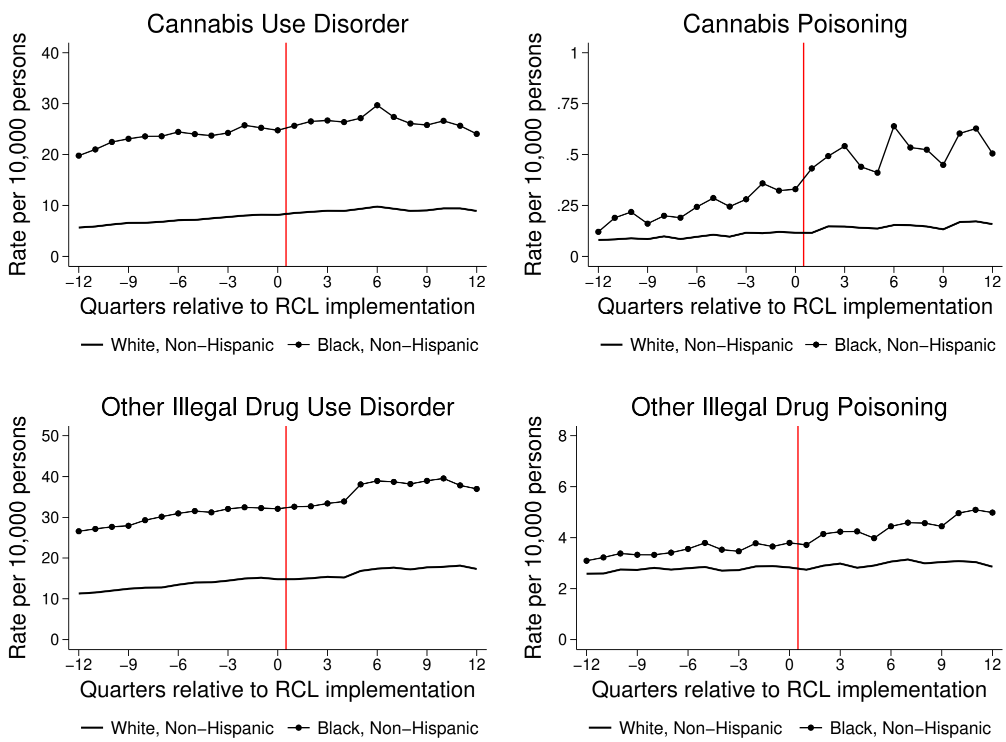

Figure S4: Drug-related hospitalization rates, raw plots

Notes: Hospital discharge data are from the 2007-19 HCUP State Inpatient Databases. The unit of analysis is a state-year-quarter. Counts for a given race are divided by state-year population estimates corresponding to that race, and multiplied by 10,000. Race-specific population weighted averages calculated for time periods relative to RCL implementation. The time $t = 0$ is the period immediately before RCL implementation.

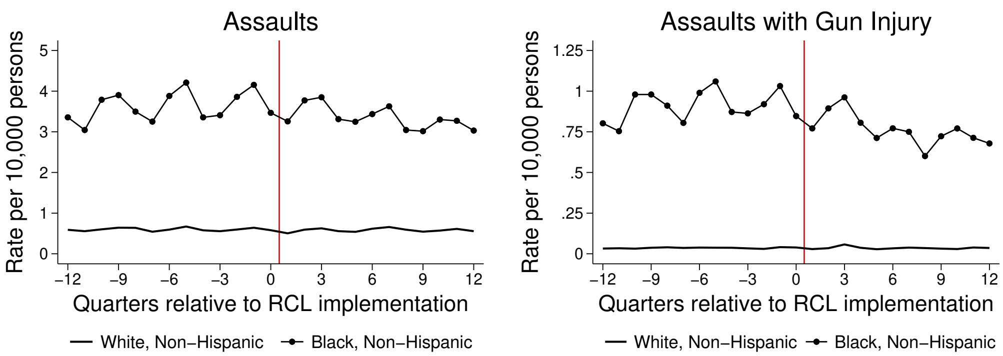

Figure S5: Assault hospitalization rates, raw plots

Notes: Hospital data are from the 2007-19 HCUP State Inpatient Databases. The unit of analysis is a state-year-quarter. Hospital discharge counts for a given race are divided by state-year population estimates corresponding to that race, and multiplied by 10,000. Race-specific population weighted averages calculated for time periods relative to RCL implementation. The time $t = 0$ is the period immediately before RCL implementation.

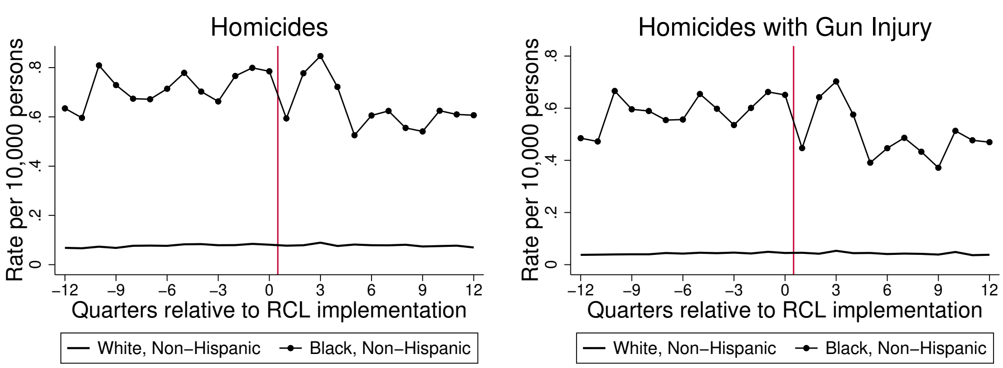

Figure S6: Homicide rates, raw plots

Notes: Death data are from the 2007-19 NVSS Mortality Files. State-year-quarter death counts for a given race are divided by state-year population estimates corresponding to that race, and multiplied by 10,000. Race-specific population weighted averages calculated for periods relative to RCL implementation. The time $t = 0$ is the period immediately before RCL implementation.

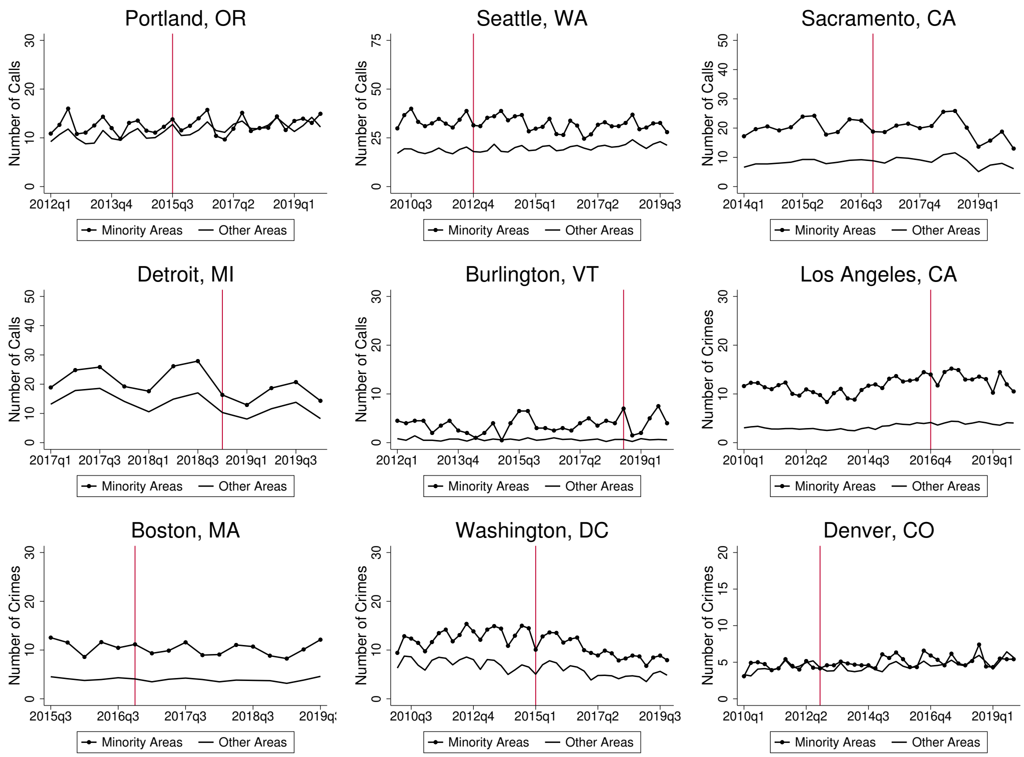

Figure S7: Violent crimes, raw plots

Notes: Calls for service and reported crime data in select RCL cities. Portland (C, E, NE), Seattle (CI, E, NE), Burlington (CI, E, NE), Detroit (CI, E), and Sacramento (CI, PI, E, NE) reflect calls for service. Los Angeles, Denver, District of Columbia, and Boston reflect reported crimes. Outcomes reflect total incident counts for Part 1 violent crimes in a tract-quarter, stratified by minority neighborhoods. Due to differences in collection and reporting of incidents, Part 1 violent crime measurement can be inconsistent across cities. See Sections 2 and F.2 for details. CI=Civilian initiated calls for service. PI=Police initiated calls for service. E=Emergency calls for service. NE=Non-emergency calls for service.

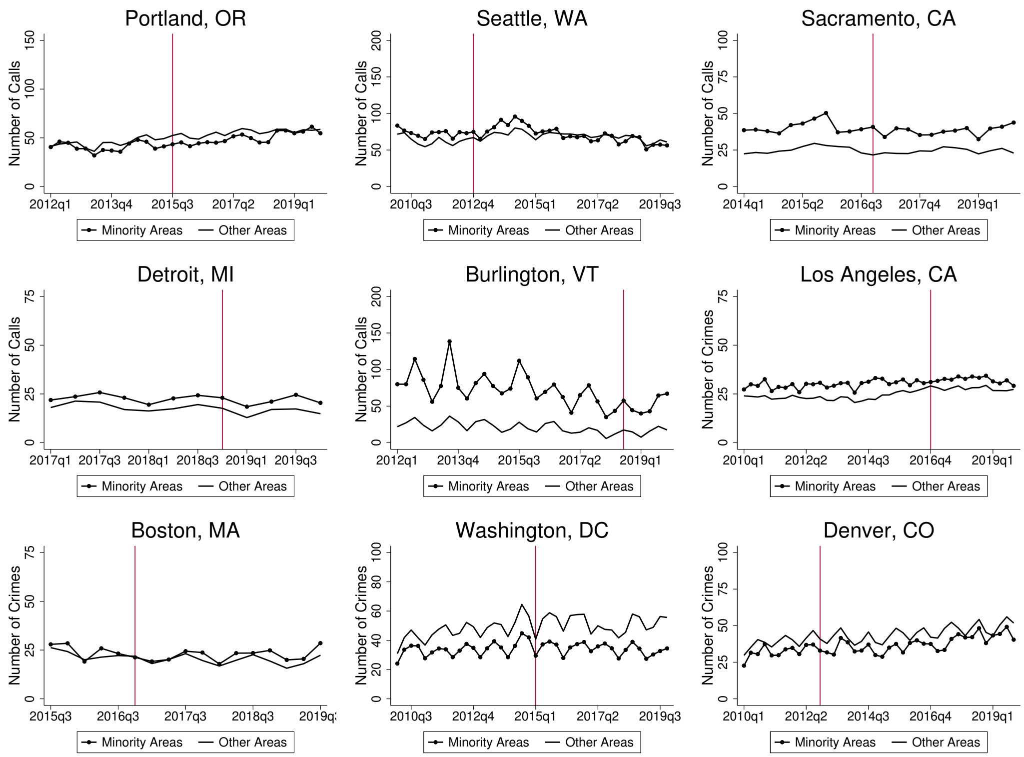

Figure S8: Property crimes, raw plots

Notes: Calls for service and reported crime data in select RCL cities. Portland (C, E, NE), Seattle (CI, E, NE), Burlington (CI, E, NE), Detroit (CI, E), and Sacramento (CI, PI, E, NE) reflect calls for service. Los Angeles, Denver, District of Columbia, and Boston reflect reported crimes. Outcomes reflect total incident counts for Part 1 property crimes in a tract-quarter, stratified by minority neighborhoods. Due to differences in collection and reporting of incidents, Part 1 property crime measurement can be inconsistent across cities. See Sections 2 and F.2 for details. CI=Civilian initiated calls for service. PI=Police initiated calls for service. E=Emergency calls for service. NE=Non-emergency calls for service.

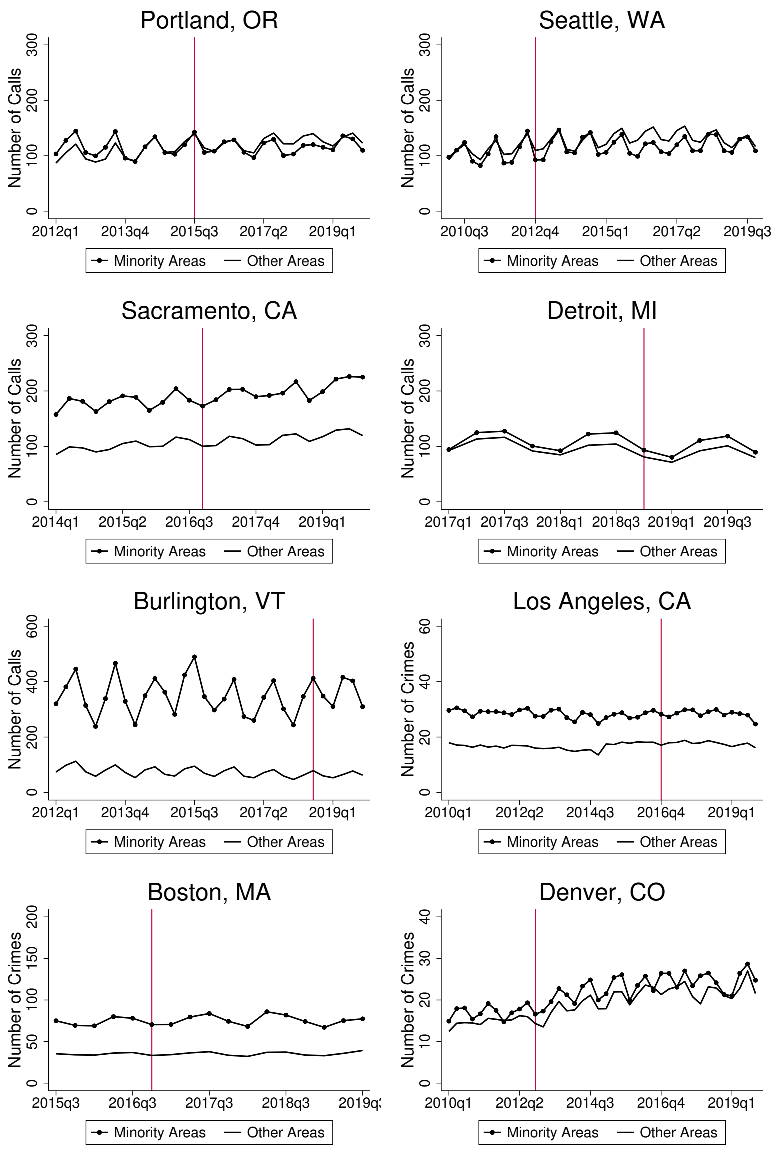

Figure S9: Criminal activity related to Part 2 offenses, raw plots

Notes: Calls for service and reported crime data in select RCL cities. Portland (C, E, NE), Seattle (CI, E, NE), Burlington (CI, E, NE), Detroit (CI, E), and Sacramento (CI, PI, E, NE) reflect calls for service. Los Angeles, Denver, District of Columbia, and Boston reflect reported crimes. Outcomes reflect total incident counts for Part 2 offenses in a tract-quarter, stratified by minority neighborhoods. Due to differences in collection and reporting of incidents, Part 2 measurement can be inconsistent across cities. See Sections 2 and F.2 for details. CI=Civilian initiated calls for service. PI=Police initiated calls for service. E=Emergency calls for service. NE=Non-emergency calls for service.

C Event Study Plots

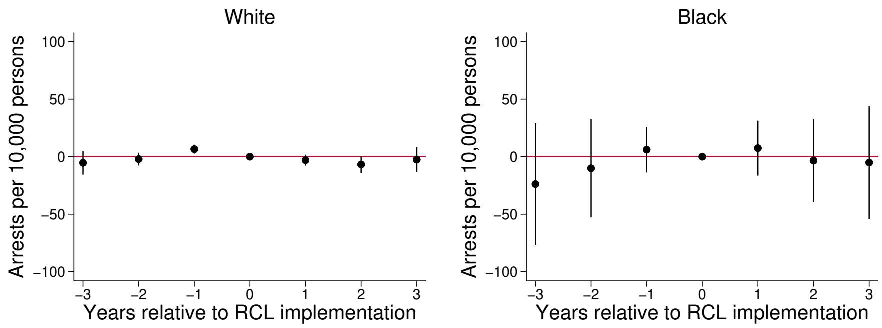

Figure S10: Total arrest rates, event study

Notes: Arrests are from the 2007-19 Uniform Crime Reports Arrests by Age, Sex, and Race. County-year counts for a given race are divided by county-year population estimates corresponding to that race, and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold ≥ 65%. Coefficients and state-level clustered 95% confidence intervals are based on an event study approach (Equation 2). Regressions are weighted by race-specific population. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

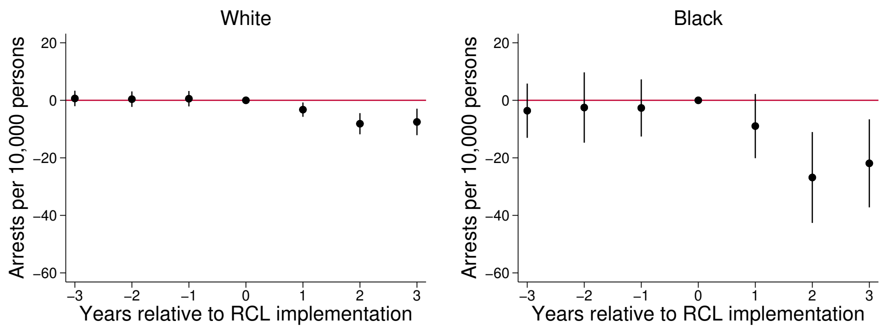

Figure S11: Cannabis possession arrest rates, event study

Notes: Arrests are from the 2007-19 Uniform Crime Reports Arrests by Age, Sex, and Race. County-year counts for a given race are divided by county-year population estimates corresponding to that race, and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Event study regressions are weighted by race-specific population estimates. Controls include the number of reporting agencies and cannabis decriminalization laws. 95% confidence intervals are clustered at the state level. The reference year is $t = 0$, the year immediately before RCL implementation.

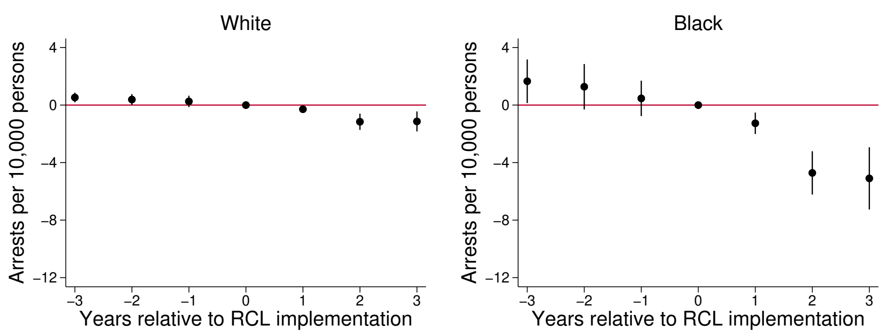

Figure S12: Cannabis sales arrest rates, event study

Notes: Arrests are from the 2007-19 Uniform Crime Reports Arrests by Age, Sex, and Race. County-year counts for a given race are divided by county-year population estimates corresponding to that race, and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Event study regressions are weighted by race-specific population estimates. Controls include the number of reporting agencies and cannabis decriminalization laws. 95% confidence intervals are clustered at the state level. The reference year is $t = 0$, the year immediately before RCL implementation.

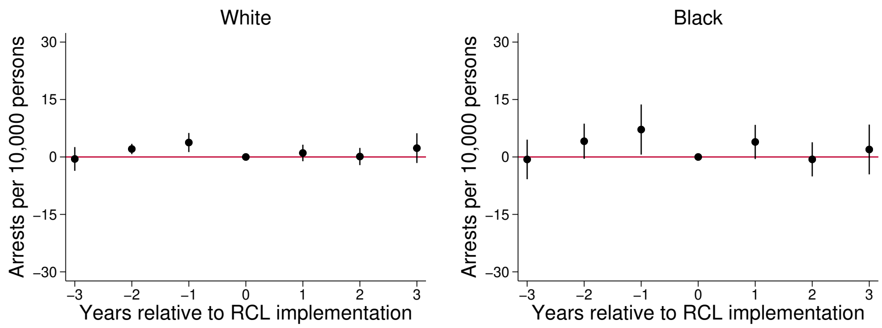

Figure S13: Other drug possession arrest rates, event study

Notes: Arrests are from the 2007-19 Uniform Crime Reports Arrests by Age, Sex, and Race. County-year counts for a given race are divided by county-year population estimates corresponding to that race, and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Event study regressions are weighted by race-specific population estimates. Controls include the number of reporting agencies and cannabis decriminalization laws. 95% confidence intervals are clustered at the state level. The reference year is $t = 0$, the year immediately before RCL implementation.

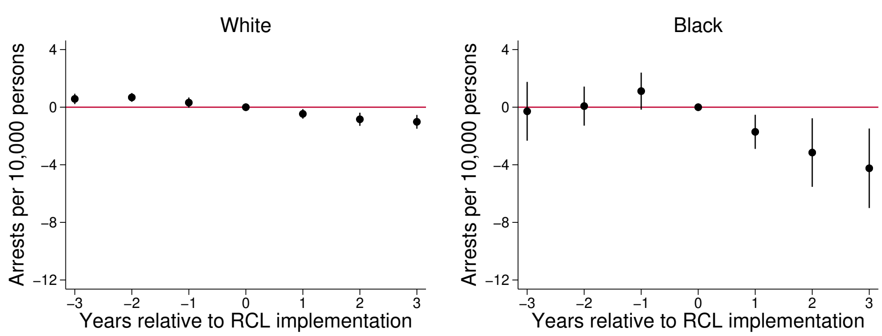

Figure S14: Other drug sales arrest rates, event study

Notes: Arrests are from the 2007-19 Uniform Crime Reports Arrests by Age, Sex, and Race. County-year counts for a given race are divided by county-year population estimates corresponding to that race, and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Event study regressions are weighted by race-specific population estimates. Controls include the number of reporting agencies and cannabis decriminalization laws. 95% confidence intervals are clustered at the state level. The reference year is $t = 0$, the year immediately before RCL implementation.

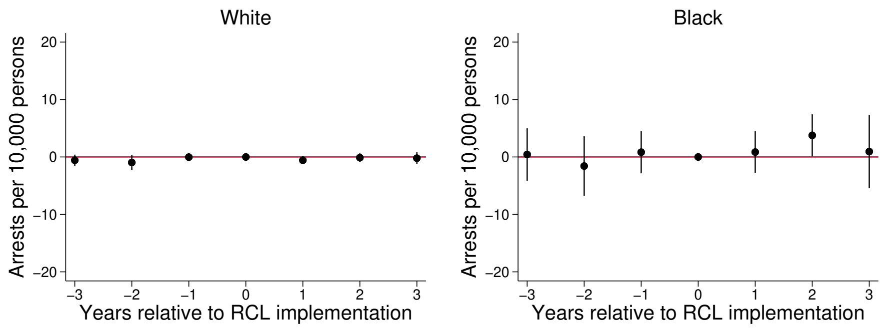

Figure S15: Violent crime arrest rates, event study

Notes: Arrests are from the 2007-19 Uniform Crime Reports Arrests by Age, Sex, and Race. County-year counts for a given race are divided by county-year population estimates corresponding to that race, and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Event study regressions are weighted by race-specific population estimates. Controls include the number of reporting agencies and cannabis decriminalization laws. 95% confidence intervals are clustered at the state level. The reference year is $t = 0$, the year immediately before RCL implementation.

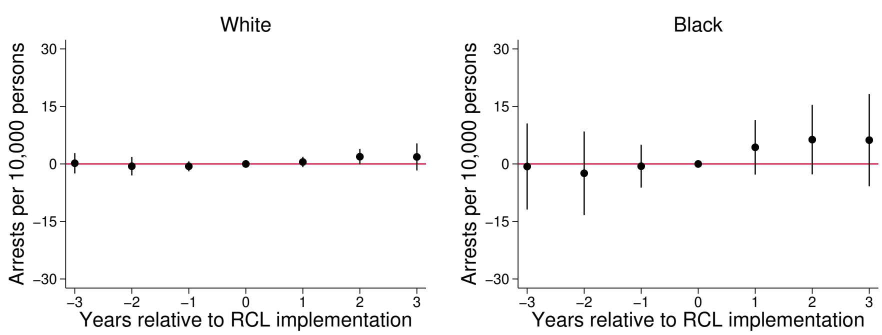

Figure S16: Property crime arrest rates, event study

Notes: Arrests are from the 2007-19 Uniform Crime Reports Arrests by Age, Sex, and Race. County-year counts for a given race are divided by county-year population estimates corresponding to that race, and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Event study regressions are weighted by race-specific population estimates. Controls include the number of reporting agencies and cannabis decriminalization laws. 95% confidence intervals are clustered at the state level. The reference year is $t = 0$, the year immediately before RCL implementation.

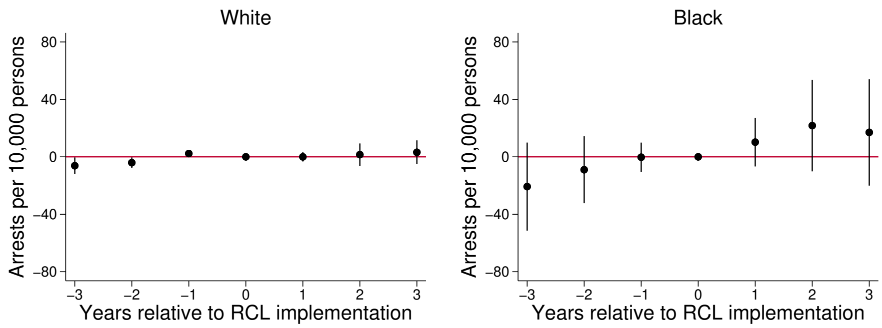

Figure S17: Part 2 arrest rates, event study

Notes: Arrests are from the 2007-19 Uniform Crime Reports Arrests by Age, Sex, and Race. County-year counts for a given race are divided by county-year population estimates corresponding to that race, and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Event study regressions are weighted by race-specific population estimates. Controls include the number of reporting agencies and cannabis decriminalization laws. 95% confidence intervals are clustered at the state level. The reference year is $t = 0$, the year immediately before RCL implementation.

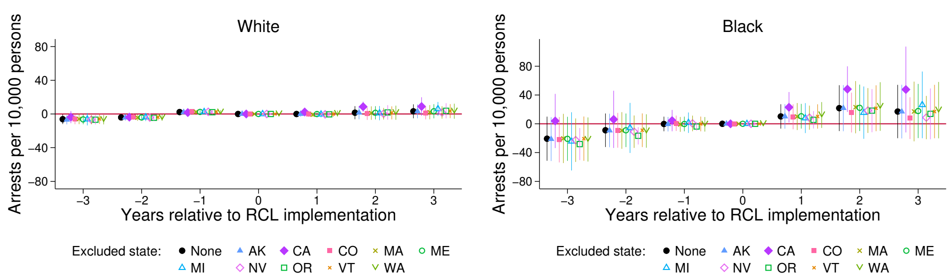

Figure S18: Part 2 arrest rates, excluding one state at a time

Notes: Arrests are from the 2007-19 Uniform Crime Reports Arrests by Age, Sex, and Race. County-year counts for a given race are divided by county-year population estimates corresponding to that race, and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Event study regressions are weighted by race-specific population estimates. Controls include the number of reporting agencies and cannabis decriminalization laws. 95% confidence intervals are clustered at the state level. The reference year is $t = 0$, the year immediately before RCL implementation. Each series of markers corresponds to a separate regression where one RCL state was excluded from the sample.

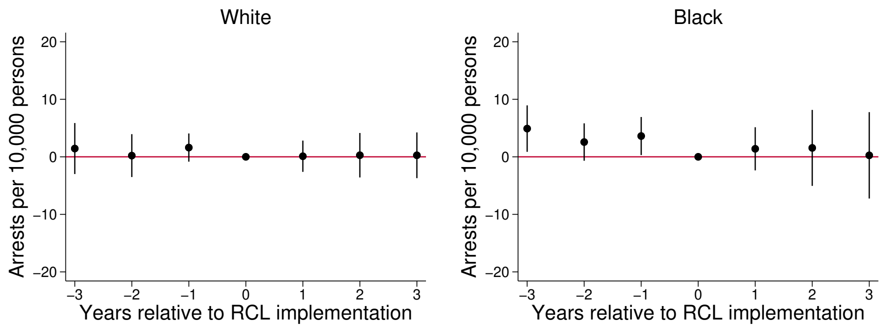

Figure S19: Driving under the influence arrest rates, event study

Notes: Arrests are from the 2007-19 Uniform Crime Reports Arrests by Age, Sex, and Race. County-year counts for a given race are divided by county-year population estimates corresponding to that race, and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Event study regressions are weighted by race-specific population estimates. Controls include the number of reporting agencies and cannabis decriminalization laws. 95% confidence intervals are clustered at the state level. The reference year is $t = 0$, the year immediately before RCL implementation.

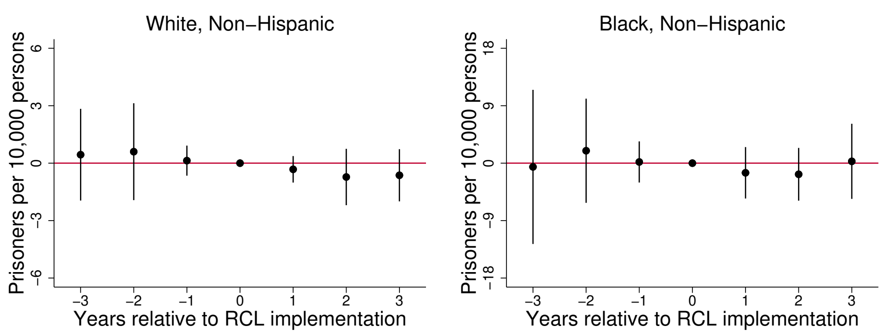

Figure S20: Total prison admissions, event study

Notes: Prison admissions are from the 2007-2019 National Corrections Reporting Program. The unit of analysis is a state-year. Counts for a given race group are divided by state-year population estimates corresponding to that race, and multiplied by 10,000. Coefficients and 95% confidence intervals clustered at the state level are based on an event study approach (Equation 2). Regressions are weighted by race-specific population estimates. Controls include cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

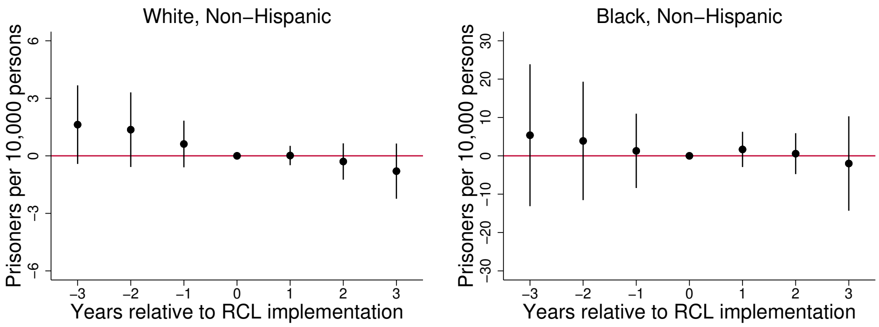

Figure S21: Total prisoners at yearend, event study

Notes: Prisoners at yearend are from the 2009-19 National Prisoner Statistics. The unit of analysis is a state-year. Counts for a given race group are divided by state-year population estimates corresponding to that race, and multiplied by 10,000. Coefficients and 95% confidence intervals clustered at the state level are based on an event study approach (Equation 2). Regressions are weighted by race-specific population estimates. Controls include cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

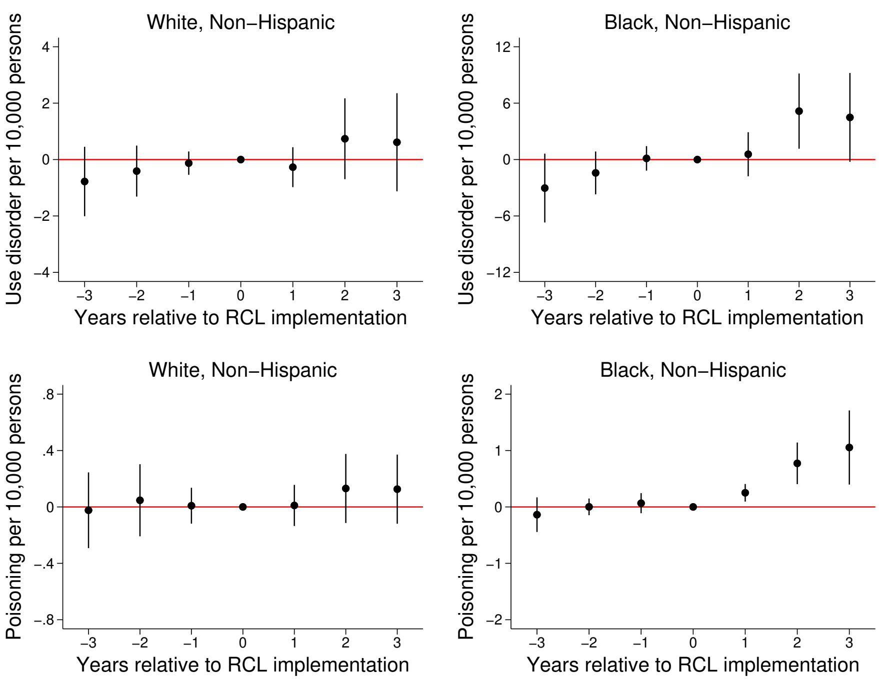

Figure S22: Other illegal drug hospitalizations, event study

Notes: Hospital discharges involving other illegal drug use disorder and poisoning diagnoses (opioids, methamphetamine, cocaine) are from the 2007-2019 HCUP State Inpatient Databases. State-year-quarter counts for a given race are divided by state-year population estimates corresponding to that race, and multiplied by 10,000. Event study regressions are weighted by race-specific population. Controls include cannabis decriminalization laws. 95% confidence intervals are clustered at the state level. The reference year is $t = 0$, the year (four quarters) immediately before RCL implementation.

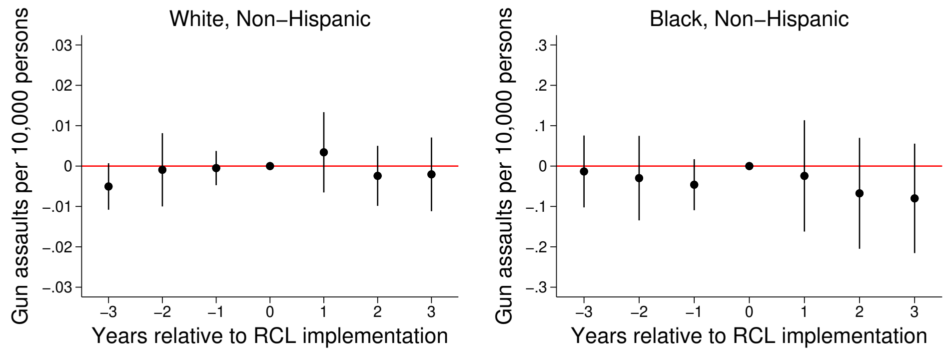

Figure S23: Assault hospitalizations with gun injury, event study

Notes: Hospital data are from the 2007-2019 HCUP State Inpatient Databases. State-year-quarter counts for a given race are divided by state-year population estimates corresponding to that race, and multiplied by 10,000. Event study regressions are weighted by race-specific population. Controls include cannabis decriminalization laws. 95% confidence intervals are clustered at the state level. The reference year is $t = 0$, the year (four quarters) immediately before RCL implementation.

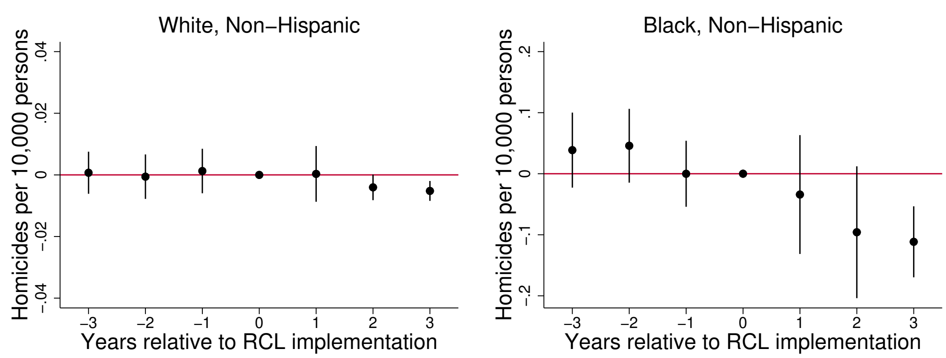

Figure S24: Homicides with gun injury, event study

Notes: Homicide data are from the 2007-2019 NVSS Mortality Files. The unit of analysis is a state-year-quarter. Counts for a given race are divided by state-year population estimates corresponding to that race, and multiplied by 10,000. Coefficients and 95% confidence intervals clustered at the state level are based on an event study approach (Equation 2). Regressions are weighted by race-specific population. Controls include cannabis decriminalization laws. The reference year is $t = 0$, the year (four quarters) immediately before RCL implementation. RCL=Recreational cannabis laws.

D Robustness Checks

Table S2: Diagnostic test of percentage and sum of negative weights in TWFE estimators

Rates per 10,000 persons	Percentage	Sum
Cannabis Arrests	1.5%	-0.003
Other drug Arrests	1.5%	-0.003
Non-drug Arrests	1.5%	-0.003
Total Arrests	1.5%	-0.003
Prisoners at Yearend	0%	0
Prisoners Admissions	0%	0
Homicide Deaths	0%	0
CUD Hospitalizations	2.6%	-0.00009
Assault Hospitalizations	2.6%	-0.00009

Notes: This table presents the percentage of all ATT estimates that have a negative weight and the sum of negative weights attached to two-way fixed effects (TWFE) DID estimators of recreational cannabis laws for each analytical sample. Diagnostic tests were performed with the *twowayfeweights* Stata command described in De Chaisemartin and d'Haultfoeuille (2020) and rate outcomes for the Black population.

Table S3: Co-occurrence of drug and non-drug offenses

Panel A: All incidents	
Single offense	88.2%
Part 1 crimes	44.7
Violent crimes	5.9
Property crimes	38.8
Part 2 crimes	43.5
Non-drug crimes	34.9
Drug violations	8.6
Drug possession	7.4
Drug sales	1.2
Multiple offenses	11.8
Involves a drug violation	4.8
Without drug violations	7.0
Total number of incidents	5,642,801
Panel B: Incidents involving drug possession or sales	
Single offense	64.1%
Drug possession	55.5
Drug sales	8.6
Incidents with two offenses	30.8
Only drug violations	22.8
Involves Part 1 crime	2.8
Involves simple assault	0.9
Involves vandalism	0.3
Involves fraud	0.4
Involves gambling	0.0
Involves other Part 2 crime	3.5
Incidents with three or more offenses	5.2
Only drug violations	0.0
Involves Part 1 crimes	2.0
Involves simple assault	0.6
Involves vandalism	0.5
Involves fraud	0.6
Involves gambling	0.0
Involves other Part 2 crimes	3.2
Total number of incidents	757,579

Notes: Incident-level data are from the 2018 National Incident-Based Reporting System (NIBRS). The table shows the share of incidents in 2018 (from reporting agencies) that fall under each categorization. Panel A considers all incidents and Panel B shows incidents involving at least one drug violation. For incidents with three or more offenses, percentages do not add up to the total since the presence of a particular crime is not mutually exclusive with the rest.

Table S4: Effect of recreational cannabis laws on arrests, multiple hypothesis testing

	Drug-defined offenses				Non-drug offenses			Net effects
	Cannabis		Other Drugs		Part 1			
	Possession	Sales	Possession	Sales	Violent	Property	Part 2	Total
Population	-7.52	-1.34	0.53	-1.04	0.11	2.05	9.66	2.45
Uncorrected p-value	0.009	0.000	0.721	0.002	0.859	0.220	0.049	0.700
Holm p-value	0.012	0.016	1.000	0.014	0.727	0.008	0.010	1.000
Romano-Wolf p-value	0.002	0.002	0.814	0.002	0.814	0.054	0.006	0.814
Mean	12.63	3.41	37.18	7.20	27.06	42.83	159.50	289.81
N	30539	30539	30539	30539	30539	30539	30539	30539
White	-7.31	-1.24	0.55	-1.38	-0.11	0.86	5.86	-2.77
Uncorrected p-value	0.004	0.000	0.778	0.000	0.791	0.599	0.220	0.622
Holm p-value	0.012	0.014	1.000	0.016	0.587	0.335	0.010	0.784
Romano-Wolf p-value	0.002	0.002	0.790	0.002	0.790	0.579	0.048	0.609
Mean	11.76	2.79	38.30	6.28	24.13	41.14	163.09	287.50
N	30539	30539	30539	30539	30539	30539	30539	30539
Black	-18.86	-6.64	-0.66	-4.47	0.50	3.26	32.46	5.58
Uncorrected p-value	0.032	0.000	0.808	0.004	0.891	0.452	0.032	0.801
Holm p-value	0.012	0.016	1.000	0.014	0.774	0.343	0.020	1.000
Romano-Wolf p-value	0.032	0.002	0.974	0.002	0.974	0.607	0.036	0.974
Mean	36.92	13.50	72.47	25.69	88.00	107.19	295.60	639.36
N	30539	30539	30539	30539	30539	30539	30539	30539

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Effect of recreational cannabis laws on arrest rates, rate differences, and rate ratios by race. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Rate differences and rate ratios are relative to the White group. Sample is restricted to counties with an agency reporting coverage threshold $\geq$ 65%. Each coefficient is based on a separate two-way fixed effects regression (see Equation 1). Regressions are weighted by race-specific population. All regressions include county and year fixed effects. Control variables include the number of reporting agencies and cannabis decriminalization laws. Standard errors are clustered by state. The mean of the outcome variables in RCL states pre-policy is shown. Different p-values that account for multiple-hypothesis testing are shown: baseline (uncorrected) p-values, applying the Holm step-down procedure (Holm, 1979), and controlling for the family-wise error rate (Romano and Wolf, 2005). We use the algorithm developed in Clarke et al. (2020) over 500 replications for our calculations.

Table S5: Effect of recreational cannabis laws on arrests, controlling for geographic spillovers

Panel A: Cannabis arrests						
Population	-8.860***	-8.380***	-7.125**	-8.143***	-7.046**	-8.192***
	(2.744)	(2.832)	(2.781)	(2.800)	(2.784)	(3.016)
White	-8.554***	-8.012***	-7.087***	-7.767***	-6.978***	-8.326***
	(2.352)	(2.433)	(2.397)	(2.416)	(2.402)	(2.566)
Black	-25.504***	-25.034***	-21.101**	-23.191**	-20.379**	-22.939**
	(8.711)	(9.395)	(9.159)	(9.252)	(9.205)	(9.770)
Panel B: Other drug sales arrests						
Population	-1.037***	-1.128***	-1.284***	-1.081***	-1.258***	-0.647**
	(0.319)	(0.329)	(0.356)	(0.339)	(0.358)	(0.307)
White	-1.376***	-1.399***	-1.527***	-1.384***	-1.510***	-1.322***
	(0.252)	(0.278)	(0.300)	(0.278)	(0.300)	(0.264)
Black	-4.472***	-4.924***	-5.452***	-4.063**	-5.048***	-2.679***
	(1.479)	(1.620)	(1.380)	(1.688)	(1.430)	(0.882)
Panel C: Part 1 arrests						
Population	2.154	2.775	3.975*	2.933	4.033*	4.252**
	(1.940)	(2.093)	(2.272)	(2.082)	(2.268)	(2.103)
White	0.743	1.072	1.487	1.338	1.629	1.841
	(1.743)	(1.878)	(2.158)	(1.900)	(2.167)	(1.947)
Black	3.761	5.276	10.407*	5.930	10.596*	6.762
	(6.428)	(6.735)	(6.189)	(6.486)	(6.168)	(6.675)
Panel D: Part 2 arrests						
Population	9.662**	11.330**	14.575***	12.795***	15.268***	11.574**
	(4.785)	(4.631)	(4.294)	(4.540)	(4.307)	(4.632)
White	5.862	7.503*	8.593**	8.675*	9.301**	5.784
	(4.717)	(4.458)	(4.246)	(4.443)	(4.268)	(4.226)
Black	32.460**	33.949**	45.553***	39.100***	47.555***	35.282***
	(14.722)	(14.307)	(12.458)	(13.798)	(12.444)	(13.631)
Panel E: Total arrests						
Population	2.449	4.551	10.029	6.514	10.859*	7.842
	(6.310)	(6.610)	(6.388)	(6.372)	(6.377)	(6.778)
White	-2.771	-0.796	1.219	0.868	2.149	-2.080
	(5.591)	(5.773)	(6.005)	(5.696)	(6.026)	(6.249)
Black	5.584	7.952	29.105	18.411	33.240*	18.256
	(21.997)	(23.284)	(18.103)	(21.543)	(17.954)	(20.062)
Controls for spillovers:						
RCL within 0-100 miles	No	Yes	Yes	No	Yes	No
RCL within 100-200 miles	No	No	Yes	No	Yes	No
Inverse distance to nearest RCL	No	No	No	Yes	Yes	No
Sample restrictions:						
Excl. RCL border states	No	No	No	No	No	Yes

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Effect of recreational cannabis laws on arrest rates, by race. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq$ 65%. Each coefficient is based on separate two-way fixed effects regression (see Equation 1). Each column adds different control variables that account for potential spillovers of RCLs: conditional on not having an RCL, an indicator for whether there is a county within 100 miles with an RCL in place, whether there is a county within 100-200 miles with an RCL, and the inverse distance to the nearest county with an RCL. The final column excludes all RCL border states from the sample. Regressions are weighted by race-specific population. All regressions include county and year fixed effects. Control variables include the number of reporting agencies and cannabis decriminalization laws. Standard errors clustered by state are in parentheses. Total observations for the main sample is 30524; total observations excluding RCL border states is 23156. States that border any of our RCL states are ID, UT, AZ, WY, NE, KS, OK, NM, WI, IN, OH, NY, CT, NH, and RI.

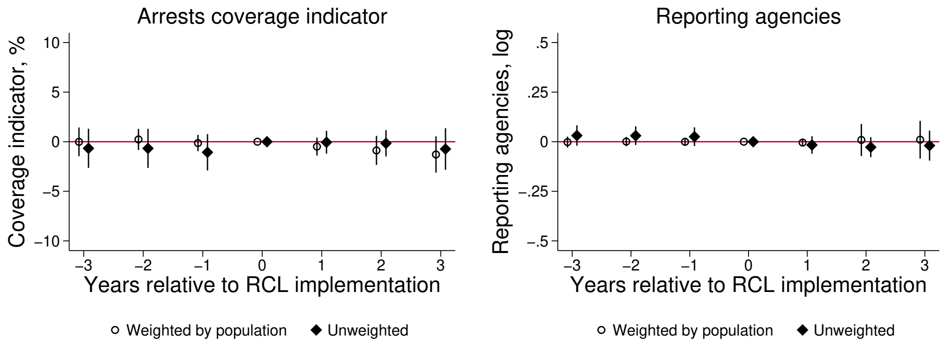

Figure S25: Agency reporting coverage indicator and reporting agencies

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Coverage indicator is a number between 0 and 100% denoting the share of arrests in a county-year that are accounted for in the data. Reporting agencies are the number of police agencies in a county-year that are reporting data to the FBI, in logs. Coefficients and 95% confidence intervals clustered at the state level are based on an event study approach (Equation 2). Hollow markers correspond to regressions weighted by total population; solid markers are unweighted. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

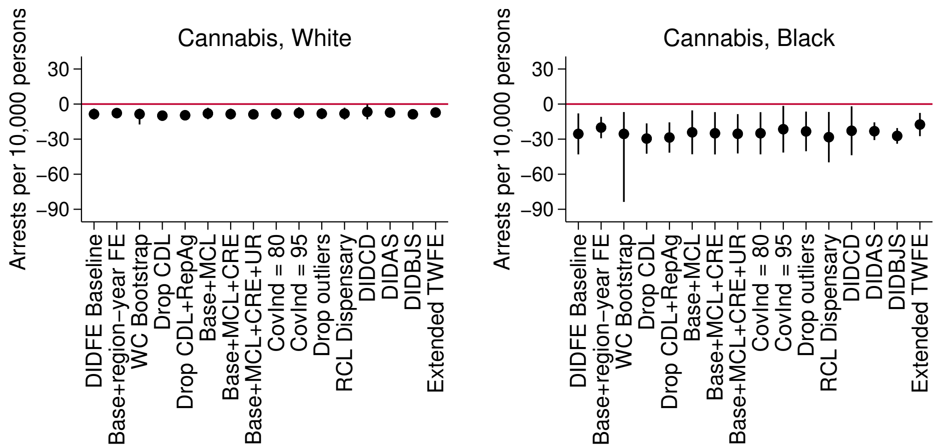

Figure S26: Cannabis arrest rates, robustness checks

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. DIDFE Baseline is the main specification. Region-year FE denote indicators for each US Census region interacted with each calendar year. WC Bootstrap calculates wild cluster bootstrap standard errors. Controls that are added or dropped include cannabis decriminalization laws (CDL), the number of reporting agencies (RepAg), medical cannabis laws (MCL), criminal record expungement laws (CRE), and the unemployment rate (UR). Sample is restricted to counties with an agency reporting coverage threshold (CovInd) $\geq$ 65% unless otherwise noted. Outliers are arrest rates above 2 standard deviations from the county-level mean. RCL dispensary replaces the RCL indicator with an indicator for recreational cannabis dispensary laws. DIDCD implements the multiperiod DID estimator described in De Chaisemartin and d'Haultfoeuille (2024) capturing the average effect in the first three years post RCLs. DIDAS shows the interaction weighted DID estimator described in Sun and Abraham (2021) capturing the average effect in the first three years post RCLs. DIDBJS shows the imputation approach of Borusyak et al. (2023). Extended DID shows the extended TWFE estimator proposed in Wooldridge (2021), using the Mundlak approach.

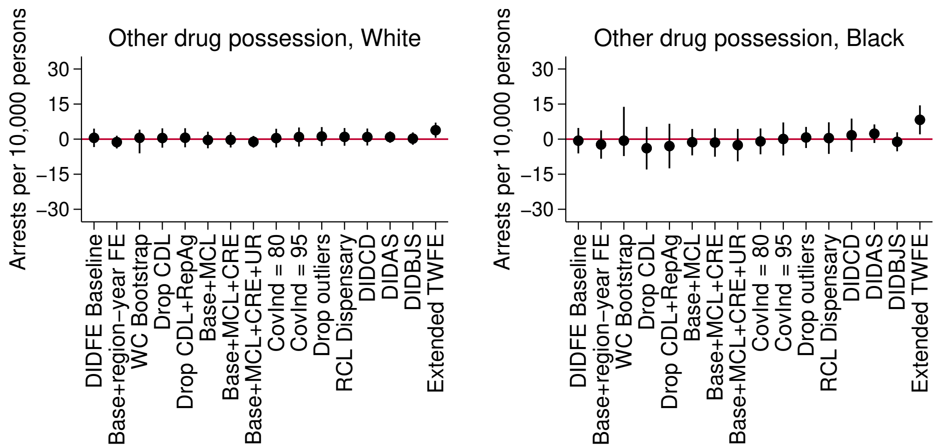

Figure S27: Other drug possession arrest rates, robustness checks

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. DIDFE Baseline is the main specification. Region-year FE denote indicators for each US Census region interacted with each calendar year. WC Bootstrap calculates wild cluster bootstrap standard errors. Controls that are added or dropped include cannabis decriminalization laws (CDL), the number of reporting agencies (RepAg), medical cannabis laws (MCL), criminal record expungement laws (CRE), and the unemployment rate (UR). Sample is restricted to counties with an agency reporting coverage threshold (CovInd) $\geq$ 65% unless otherwise noted. Outliers are arrest rates above 2 standard deviations from the county-level mean. RCL dispensary replaces the RCL indicator with an indicator for recreational cannabis dispensary laws. DIDCD implements the multiperiod DID estimator described in De Chaisemartin and d'Haultfoeuille (2024) capturing the average effect in the first three years post RCLs. DIDAS shows the interaction weighted DID estimator described in Sun and Abraham (2021) capturing the average effect in the first three years post RCLs. DIDBJS shows the imputation approach of Borusyak et al. (2023). Extended DID shows the extended TWFE estimator proposed in Wooldridge (2021), using the Mundlak approach.

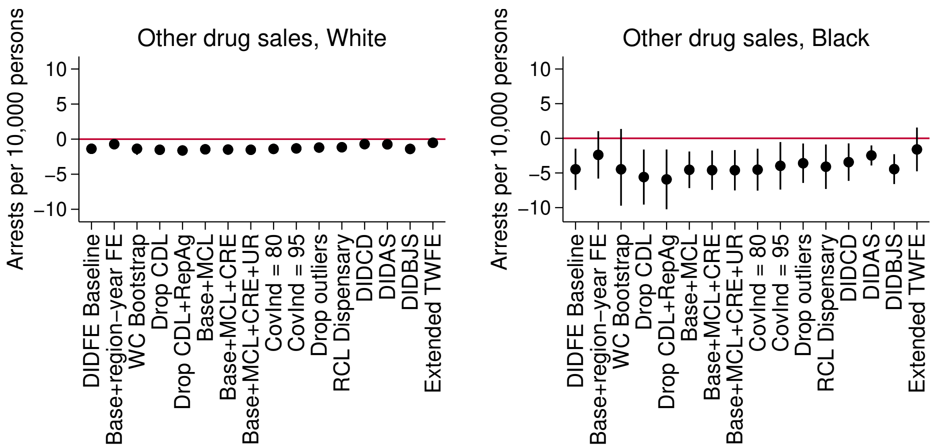

Figure S28: Other drug sales arrest rates, robustness checks

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. DIDFE Baseline is the main specification. Region-year FE denote indicators for each US Census region interacted with each calendar year. WC Bootstrap calculates wild cluster bootstrap standard errors. Controls that are added or dropped include cannabis decriminalization laws (CDL), the number of reporting agencies (RepAg), medical cannabis laws (MCL), criminal record expungement laws (CRE), and the unemployment rate (UR). Sample is restricted to counties with an agency reporting coverage threshold (CovInd) $\geq$ 65% unless otherwise noted. Outliers are arrest rates above 2 standard deviations from the county-level mean. RCL dispensary replaces the RCL indicator with an indicator for recreational cannabis dispensary laws. DIDCD implements the multiperiod DID estimator described in De Chaisemartin and d'Haultfoeuille (2024) capturing the average effect in the first three years post RCLs. DIDAS shows the interaction weighted DID estimator described in Sun and Abraham (2021) capturing the average effect in the first three years post RCLs. DIDBJS shows the imputation approach of Borusyak et al. (2023). Extended DID shows the extended TWFE estimator proposed in Wooldridge (2021), using the Mundlak approach.

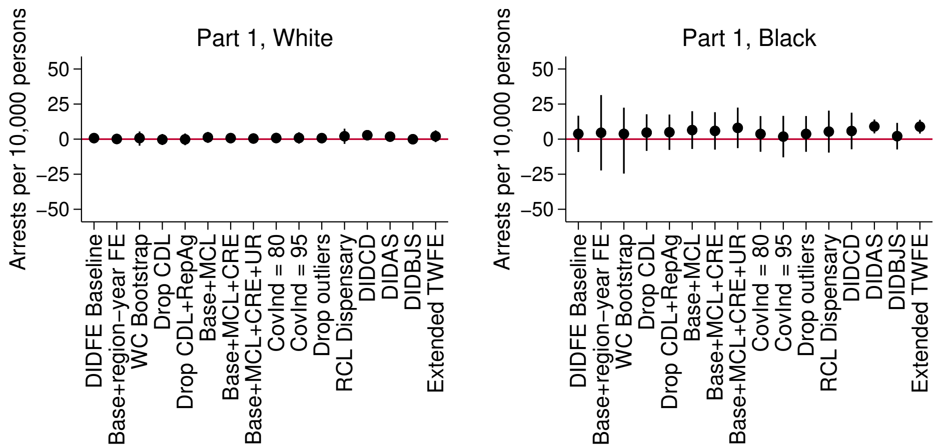

Figure S29: Part 1 arrest rates, robustness checks

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. DIDFE Baseline is the main specification. Region-year FE denote indicators for each US Census region interacted with each calendar year. WC Bootstrap calculates wild cluster bootstrap standard errors. Controls that are added or dropped include cannabis decriminalization laws (CDL), the number of reporting agencies (RepAg), medical cannabis laws (MCL), criminal record expungement laws (CRE), and the unemployment rate (UR). Sample is restricted to counties with an agency reporting coverage threshold (CovInd) $\geq$ 65% unless otherwise noted. Outliers are arrest rates above 2 standard deviations from the county-level mean. RCL dispensary replaces the RCL indicator with an indicator for recreational cannabis dispensary laws. DIDCD implements the multiperiod DID estimator described in De Chaisemartin and d'Haultfoeuille (2024) capturing the average effect in the first three years post RCLs. DIDAS shows the interaction weighted DID estimator described in Sun and Abraham (2021) capturing the average effect in the first three years post RCLs. DIDBJS shows the imputation approach of Borusyak et al. (2023). Extended DID shows the extended TWFE estimator proposed in Wooldridge (2021), using the Mundlak approach.

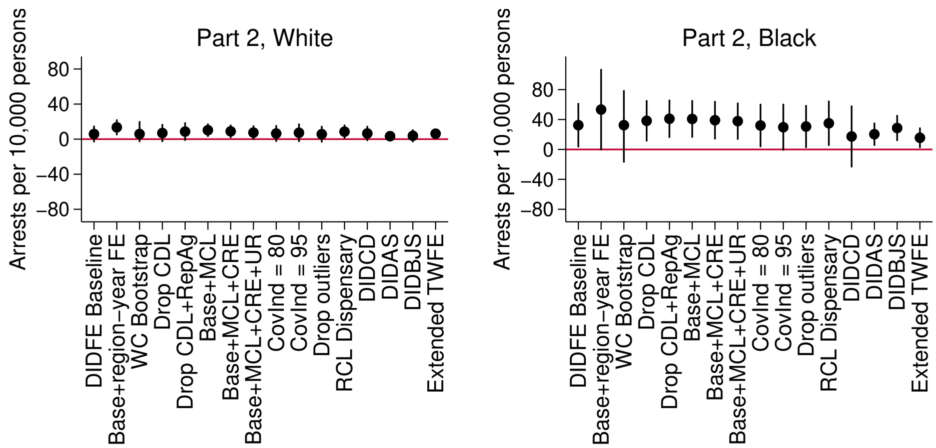

Figure S30: Part 2 arrest rates, robustness checks

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. DIDFE Baseline is the main specification. Region-year FE denote indicators for each US Census region interacted with each calendar year. WC Bootstrap calculates wild cluster bootstrap standard errors. Controls that are added or dropped include cannabis decriminalization laws (CDL), the number of reporting agencies (RepAg), medical cannabis laws (MCL), criminal record expungement laws (CRE), and the unemployment rate (UR). Sample is restricted to counties with an agency reporting coverage threshold (CovInd) $\geq$ 65% unless otherwise noted. Outliers are arrest rates above 2 standard deviations from the county-level mean. RCL dispensary replaces the RCL indicator with an indicator for recreational cannabis dispensary laws. DIDCD implements the multiperiod DID estimator described in De Chaisemartin and d'Haultfoeuille (2024) capturing the average effect in the first three years post RCLs. DIDAS shows the interaction weighted DID estimator described in Sun and Abraham (2021) capturing the average effect in the first three years post RCLs. DIDBJS shows the imputation approach of Borusyak et al. (2023). Extended DID shows the extended TWFE estimator proposed in Wooldridge (2021), using the Mundlak approach.

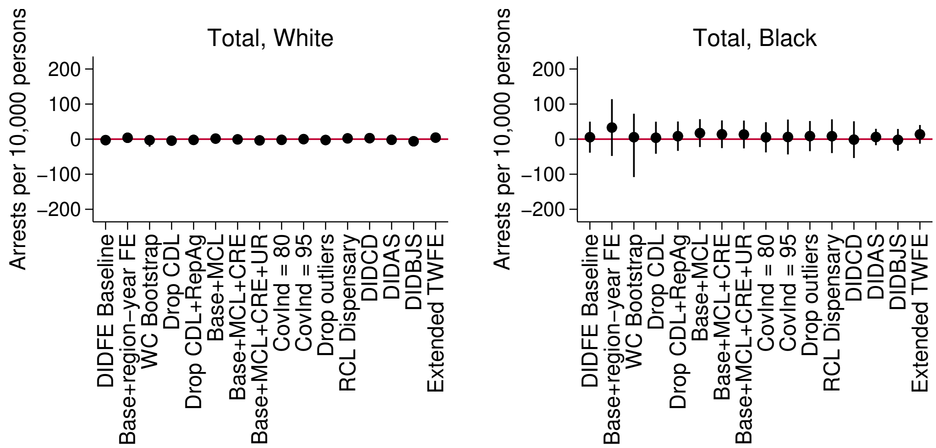

Figure S31: Total arrest rates, robustness checks

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. DIDFE Baseline is the main specification. Region-year FE denote indicators for each US Census region interacted with each calendar year. WC Bootstrap calculates wild cluster bootstrap standard errors. Controls that are added or dropped include cannabis decriminalization laws (CDL), the number of reporting agencies (RepAg), medical cannabis laws (MCL), criminal record expungement laws (CRE), and the unemployment rate (UR). Sample is restricted to counties with an agency reporting coverage threshold (CovInd) $\geq$ 65% unless otherwise noted. Outliers are arrest rates above 2 standard deviations from the county-level mean. RCL dispensary replaces the RCL indicator with an indicator for recreational cannabis dispensary laws. DIDCD implements the multiperiod DID estimator described in De Chaisemartin and d'Haultfoeuille (2024) capturing the average effect in the first three years post RCLs. DIDAS shows the interaction weighted DID estimator described in Sun and Abraham (2021) capturing the average effect in the first three years post RCLs. DIDBJS shows the imputation approach of Borusyak et al. (2023). Extended DID shows the extended TWFE estimator proposed in Wooldridge (2021), using the Mundlak approach.

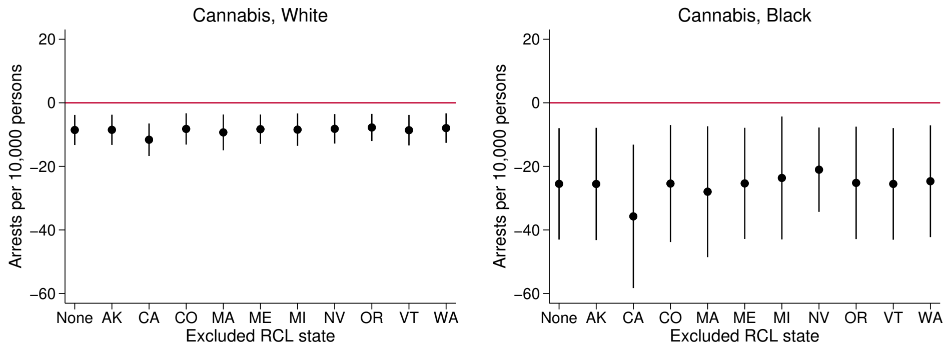

Figure S32: Cannabis arrest rates, leave-one-out robustness

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. Each regression excludes one RCL state at a time.

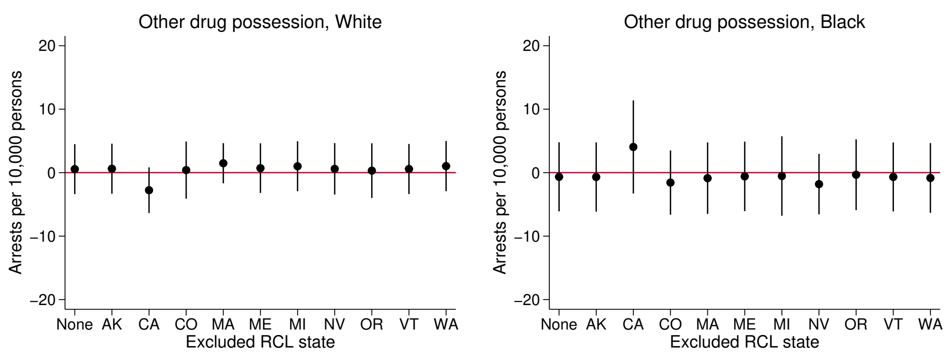

Figure S33: Other drug possession arrest rates, leave-one-out robustness

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. Each regression excludes one RCL state at a time.

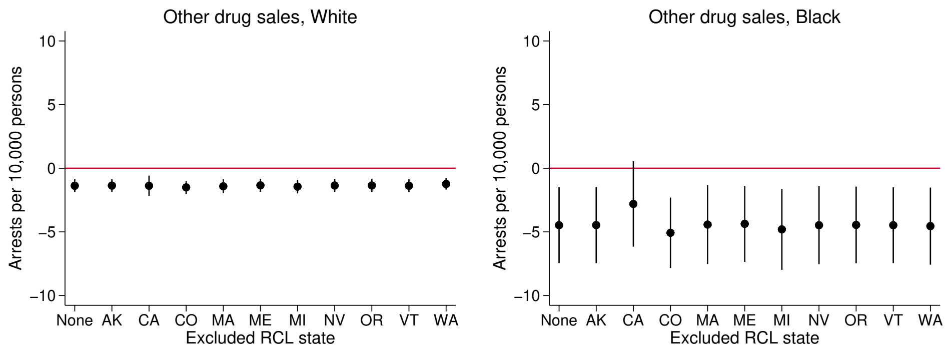

Figure S34: Other drug sales arrest rates, leave-one-out robustness

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. Each regression excludes one RCL state at a time.

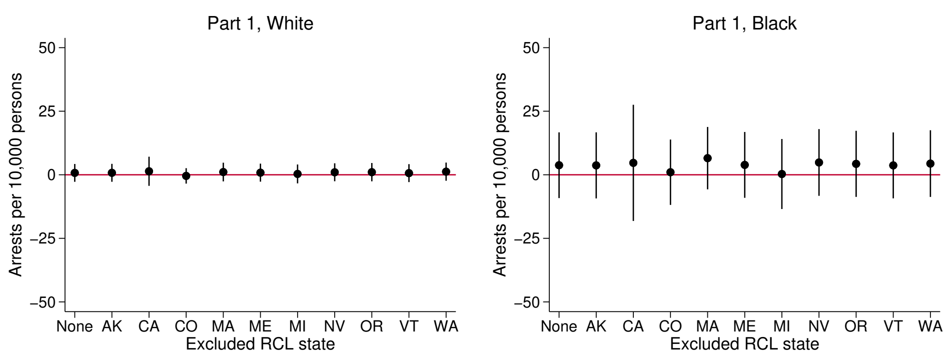

Figure S35: Part 1 arrest rates, leave-one-out robustness

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. Each regression excludes one RCL state at a time.

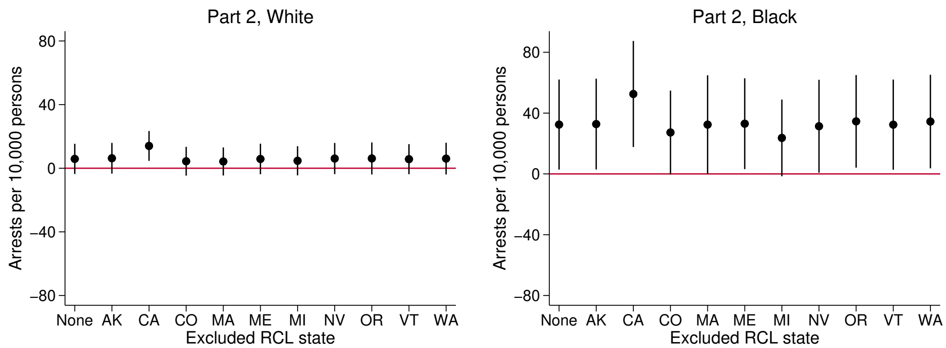

Figure S36: Part 2 arrest rates, leave-one-out robustness

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq$ 65%. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. Each regression excludes one RCL state at a time.

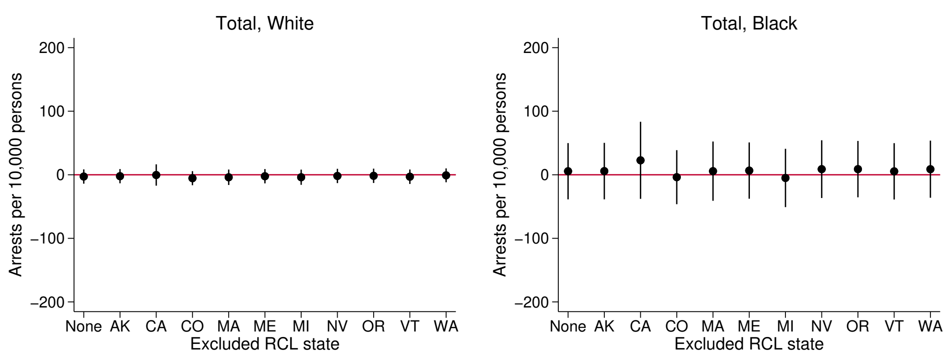

Figure S37: Total arrest rates, leave-one-out robustness

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq$ 65%. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. Each regression excludes one RCL state at a time.

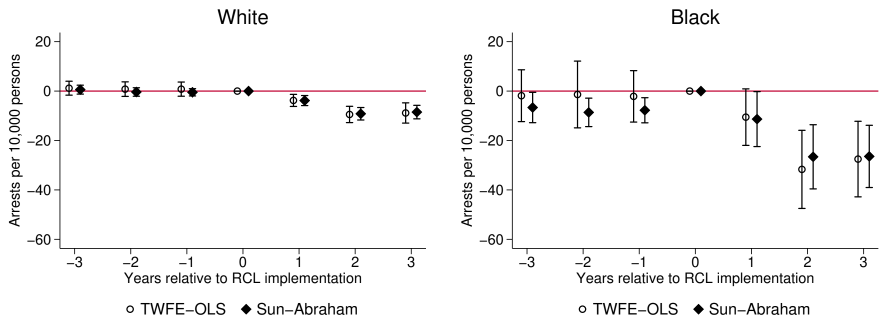

Figure S38: Cannabis arrest rates, alternative DID estimator

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Hollow markers correspond to the standard two-way fixed effects OLS estimator. Solid markers show the interaction-weighted estimator proposed in Sun and Abraham (2021). Bars denote 95% confidence intervals from robust standard errors clustered at the county level. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

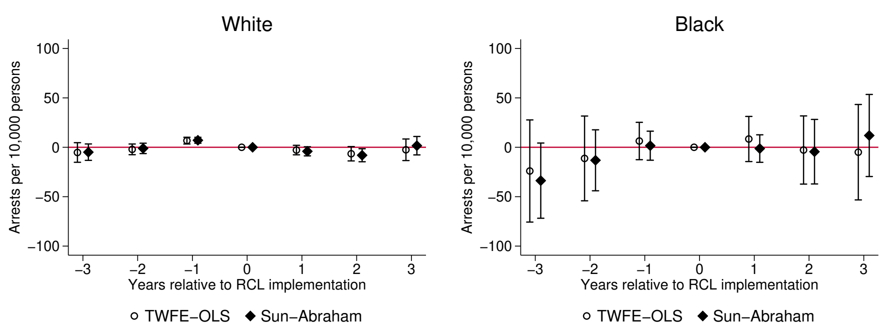

Figure S39: Total arrest rates, alternative DID estimator

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Hollow markers correspond to the standard two-way fixed effects OLS estimator. Solid markers show the interaction-weighted estimator proposed in Sun and Abraham (2021). Bars denote 95% confidence intervals from robust standard errors clustered at the county level. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

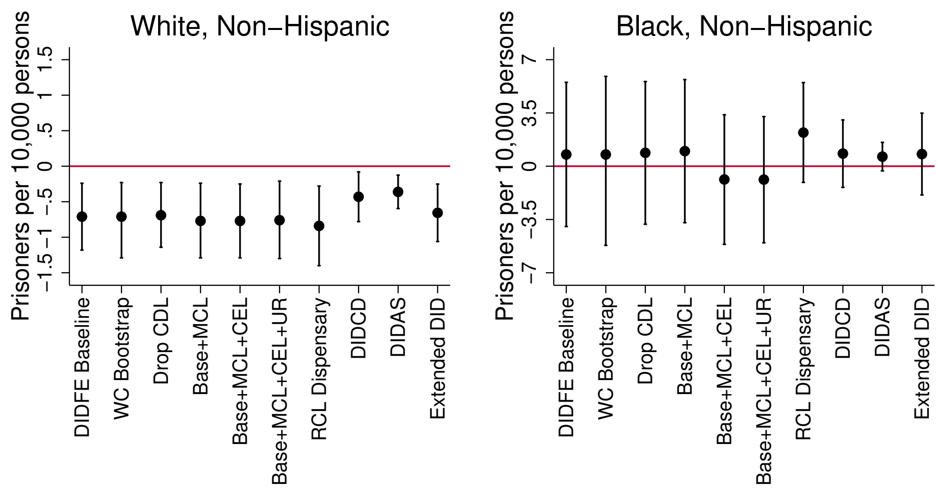

Figure S40: Prison admissions for drug-defined offenses, robustness checks

Notes: Prison admissions data are from the 2007-2019 National Corrections Reporting Program. The unit of analysis is a state-year. Counts for a given racial group are divided by state-year population estimates corresponding to that racial group, and multiplied by 10,000. Each coefficient is based on separate two-way fixed effects regressions (see Equation 1). Regressions are weighted by race-specific population. All regressions include state and year fixed effects, and control for CDLs unless stated otherwise. DIDFE=Two-way fixed effect difference-in-differences estimator. DIDCD=Multiperiod difference-in-differences estimator described in De Chaisemartin and d'Haultfoeuille (2024) capturing the average effect in the first three years post RCLs. DIDAS=Interaction weighted difference-in-differences estimator described in Sun and Abraham (2021) capturing the average effect in the first three years post RCLs. Extended DID=Extended TWFE estimator proposed in Wooldridge (2021), using the Mundlak approach. WC Bootstrap=Wild cluster bootstrap. RCL=Recreational cannabis laws. MCL=Medical cannabis laws. CDL=Cannabis decriminalization laws. CEL=Cannabis record expungement laws. UR=Unemployment rate.

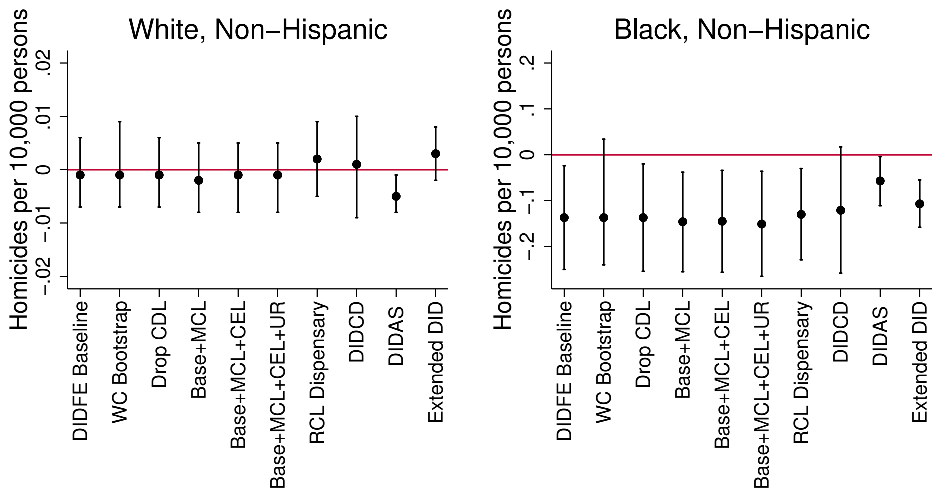

Figure S41: Homicide rates, robustness checks

Notes: Homicide data are from the 2007-2019 NVSS Mortality Files. The unit of analysis is a state-year-quarter. Counts for a given racial group are divided by state-year population estimates corresponding to that racial group, and multiplied by 10,000. Each coefficient is based on separate two-way fixed effects regressions (see Equation 1). Regressions are weighted by race-specific population. All regressions include state and year-quarter fixed effects, and control for CDLs unless stated otherwise. DIDFE=Two-way fixed effect difference-in-differences estimator. DIDCD=Multiperiod difference-in-differences estimator described in De Chaisemartin and d'Haultfoeuille (2024) capturing the average effect in the first three years post RCLs. DIDAS=Interaction weighted difference-in-differences estimator described in Sun and Abraham (2021) capturing the average effect in the first three years post RCLs. Extended DID=Extended TWFE estimator proposed in Wooldridge (2021), using the Mundlak approach. WC Bootstrap=Wild cluster bootstrap. RCL=Recreational cannabis laws. MCL=Medical cannabis laws. CDL=Cannabis decriminalization laws. CEL=Cannabis record expungement laws. UR=Unemployment rate.

E Arrests by Offense Categories

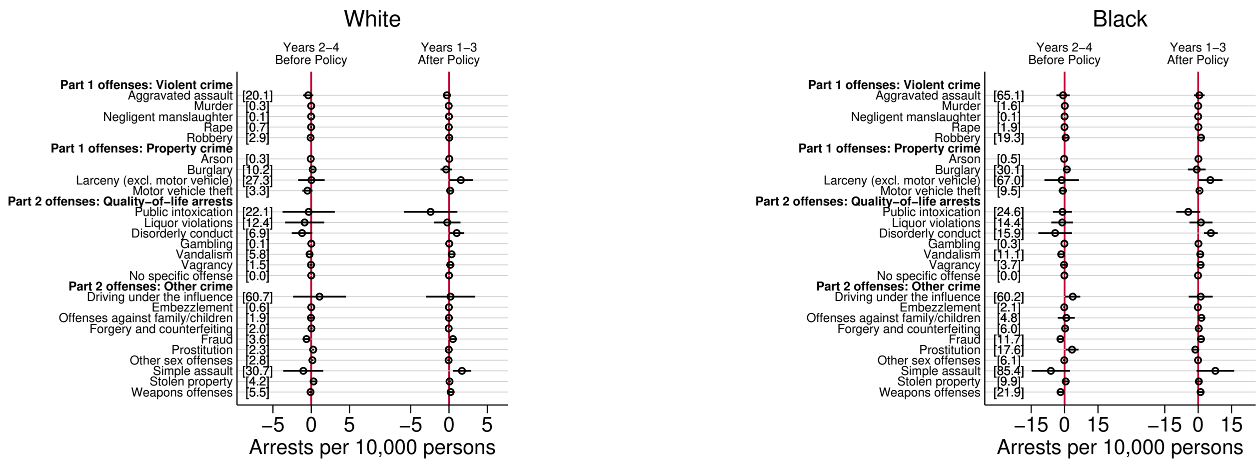

Figure S42: Arrest rates, by offense categories

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. The unit of analysis is a county-year. Effect of recreational cannabis laws on arrest rates for all crime categories, by race groups (White and Black). Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Coefficients and 95% confidence intervals shown from standard errors clustered by state. Numbers in brackets on the left show the pre-policy mean in RCL states. Each pair of coefficients (for a given offense and race group) is based on separate two-way fixed effects regression, with an indicator for years 2 to 4 prior to the policy and years 1 to 3 after the policy (the reference period is the year prior to RCL). Regressions are weighted by race-specific population. All regressions include county and year fixed effects. Control variables include the number of reporting agencies and cannabis decriminalization laws. RCL=Recreational cannabis laws.

F Potential Mechanisms

F.1 Police Officers

Table S6: Effect of recreational cannabis laws on police officers

	Sworn officers		Civil officers		Total officers	
RCL	0.18		0.15**		0.33**	
	(0.12)		(0.06)		(0.16)	
RCL Dispensary		0.15		0.16**		0.31
		(0.14)		(0.06)		(0.19)
Mean	3.63	3.60	0.93	0.92	4.56	4.52
N	4097	4097	4097	4097	4097	4097

Notes: Police officer counts are from the 2007-2019 LEOKA. County-year counts are divided by county-year population estimates, and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold $\geq 65\%$. Each coefficient is based on a separate two-way fixed effects regression (Equation 1). Regressions are weighted by total population estimates. All regressions include county and year fixed effects. Controls include the number of reporting agencies and cannabis decriminalization laws. Standard errors clustered by state are in parentheses. The pre-policy outcome mean is reported for RCL states. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

F.2 Criminal Activity

F.2.1 Data

We describe incident data on calls for service and reported crimes to police in select RCL cities. Los Angeles, Denver, District of Columbia, and Boston reflect reported crimes to police. Portland (C, E, NE), Seattle (CI, E, NE), Burlington (CI, E, NE), Detroit (CI, E), and Sacramento (CI, PI, E, NE) reflect calls for service, and include the types of calls in parentheses. CI=Civilian initiated calls for service to police. PI=Police initiated calls for service. E=Emergency calls for service. NE=Non-emergency calls for service.

Portland, OR. We analyzed 2012-2019 civilian initiated calls for service collected by the Portland Police Bureau, which included calls to the emergency 911 line or the non-emergency line (n=1,894,454).[1] The Portland Police Bureau counted all calls for service where at least one Portland police officer

[1] https://public.tableau.com/app/profile/portlandpolicebureau/viz/DispatchedCallsforService/DispatchedCalls

was dispatched. Final call type was used to categorize Part 1 and Part 2 offenses, and latitude and longitude were used to merge calls with 2012 population data at the tract level. Calls deemed sensitive due to the nature of the incident, potential suspect or offender, potential victim-offender relationship, or investigation were not included in the public data. Latitude and longitude information for sensitive incidents (i.e., domestic violence, rape, child abuse, restraining order, behavioral health) were not reported in the public data and therefore not reflected in our neighborhood analyses (8% missing lat/lon). Drug-defined offenses are included along with other vice crimes under the offense description "Vice." We therefore use this offense description to measure drug-defined offenses.

Seattle, WA. We analyzed 2010-2019 civilian initiated calls for service collected by the Seattle Police Department, which included calls to the emergency 911 line and the non-emergency line (n=2,490,720).[2] Data only contained records of police response. If a call was queued in the system but cleared before an officer could respond, it was not included. While data included police initiated calls for service, these were inadequately reported for many types of calls during the study period, and thus, we excluded them from the analyses. Final call type was used to categorize Part 1 and Part 2 offenses, and latitude and longitude were used to merge calls with 2010 population data at the tract level (1% missing lat/lon).

Burlington, VT. We analyzed 2012-2019 civilian initiated calls for service collected by the Burlington Police Department, which included calls to the emergency 911 line or the non-emergency line (n=186,257). Data also included other incidents collected through online reports, in person, or initiated by police.[3] While data included police initiated calls for service, these were inadequately reported for many types of calls during the study period, and thus, we excluded them from the analyses. Call type was used to categorize Part 1 and Part 2 offenses, and latitude and longitude were used to merge calls with 2012 population data at the tract level. Latitude and longitude information for sensitive incidents (i.e., domestic violence, juvenile problem) were not reported in the public data and therefore not reflected in our neighborhood analyses (5.75% missing lat/lon).

[2] https://data.seattle.gov/Public-Safety/Call-Data/33kz-ixgy

[3] https://data.burlingtonvt.gov/search?collection=Dataset&q=incident

Detroit, MI. We analyzed 2017-2019 civilian initiated calls for service collected by the Detroit Police Department, which included emergency calls to the 911 line but did not include non-emergency calls (n=1,015,409).[4] While data included police initiated calls for service, these were inadequately reported for many types of calls during the study period, and thus, we excluded them from the analyses. Call description was used to categorize Part 1 and Part 2 offenses, and latitude and longitude were used to merge calls with 2017 population data at the tract level (0% missing lat/lon).

Sacramento, CA. We analyzed 2014-2019 civilian and police initiated calls for service collected by the Sacramento Police Department, which included calls to the emergency 911 line or the non-emergency line (n=2,033,139).[5] The data included calls for service that were entered into the computer-aided dispatch system, regardless of whether police responded to the call. Civilian calls cannot be separately identified from police calls in the data, and thus estimates are based on the combined data. Call description was used to categorize Part 1 and Part 2 offenses, and latitude and longitude were used to merge calls with 2014 population data at the tract level. Latitude and longitude for sensitive cases (i.e. domestic violence, rape, child abuse, behavioral health) were not reported in the public data (3% missing lat/lon).

Washington, DC. We analyzed 2010-2019 crime reports collected by the District of Columbia Metropolitan Police Department (n= 349,630).[6] The dataset contains a subset of locations and attributes of incidents reported in the Analytical Services Application crime report database by the District of Columbia Metropolitan Police Department. This data is shared via an automated process where addresses are geocoded to the District's Master Address Repository and assigned to the appropriate street block. Only Part 1 offenses are collected, and thus, we were not able to analyze Part 2 offenses. Latitude and longitude were used to merge calls with 2010 population data at the tract level (0% missing lat/lon).

Los Angeles, CA. We analyzed 2010-2019 crime reports collected by the Los Angeles Police Department, which included index crimes, select Part 2 crimes, and race of victim (n=2,094,018).[7] This data was transcribed from

[4] https://data.detroitmi.gov/datasets/detroitmi::911-calls-for-service/about

[5] https://data.cityofsacramento.org

[6] https://opendata.dc.gov/datasets

[7] https://data.lacity.org/Public-Safety/Crime-Data-from-2010-to-2019/63jg-8b9z

original crime reports that were typed on paper and therefore there may be some inaccuracies within the data. Tracts primarily in two areas, Olympic and Topanga, did not report crimes in 2014. We imputed 2014 values with the average number of reported crimes in 2013 and 2015 for these tracts. Crime type was used to categorize Part 1 and Part 2 offenses, and latitude and longitude were used to merge crimes with 2010 population data at the tract level (0% missing lat/lon).

Denver, CO. We analyzed 2010-2019 crime reports collected by the Denver Police Department, which included Part 1 crimes and select Part 2 crimes (n=480,847).[8] The data is based on the National Incident Based Reporting System (NIBRS) which includes all victims of person crimes and all crimes within an incident. The data is dynamic, which allows for additions, deletions and/or modifications at any time, resulting in more accurate information in the database. Due to continuous data entry, the number of records in subsequent extractions are subject to change. In accordance with legal restrictions against identifying sexual assault and child abuse victims and juvenile perpetrators, victims, and witnesses of certain crimes, public data takes the following precautionary measures: (a) Latitude and longitude of sexual assaults are not included. (b) Child abuse cases, and other crimes which by their nature involve juveniles, or which the reports indicate involve juveniles as victims, suspects, or witnesses, are not reported at all. Crimes that are initially reported, but that are later determined not to have occurred, are called "unfounded" offenses. These incidents are excluded once they have been designated as unfounded. Most Part 2 offenses were not properly reported until 2013. Therefore, we only analyzed a small number of. Part 2 offenses that were being reported prior to 2012. Crime type was used to categorize Part 1 offenses, and latitude and longitude were used to merge calls with 2010 population data at the tract level (2% missing lat/lon).

Boston, MA. We analyzed 2015-2019 crime reports collected by the Boston Police Department, which included Part 1 crimes and select Part 2 crimes.[9] Full data reporting is available starting in Q3/2015 and document the initial details surrounding an incident to which the Boston Police Department respond. We dropped Q4/2019 due to a sharp decline in data reporting. Crime type was used to categorize Part 1 and Part 2 offenses, and latitude and longitude were used to merge crimes with 2015 population data at the tract level

[8] https://www.denvergov.org/opendata/dataset/city-and-county-of-denver-crime

[9] https://data.boston.gov/dataset/crime-incident-reports-august-2015-to-date-source-new-system

(5% missing lat/lon).

F.2.2 Outcome Measures

Calls for service and reported crime data identify incident type with names (i.e. text). Incidents reported and names used vary across cities, which creates challenges for categorizing incidents using a standard and consistent methodology. To match incident definitions in call and crime data to definitions in arrest and incarceration data as much as possible, we categorize drug-defined offenses and other offenses as follows.

Drug-defined offenses. Incidents with text indicating drug possession, drug sales, narcotic violations, or drug violations. Drug overdoses and drug intoxication were not included in this category.

Other offenses. Incidents with text indicating Part 1 and Part 2 offenses. Part 1 offenses can include violent (homicide, murder, manslaughter, rape, sexual assault, aggravated or felonious assault, and robbery) and property crimes (burglary, theft, larceny, motor vehicle theft, and arson). Part 2 offenses include financial and other white collar crimes (fraud, embezzlement, forgery, identity theft, blackmail, bribery, money laundering, scam), simple assault (simple assault, threats, harassment, fight, battery), vandalism (vandalism, graffiti, malicious destruction of property), other theft (possession of stolen property, property missing, lost or found), public intoxication (intoxication, drunkenness, drug overdose), driving under the influence, gambling, liquor violations, public disorder (disorder, disturbance, trespassing, mischief, nuisance, noise, unwanted person, annoyance, stalking, verbal dispute, panhandling, loitering), weapon offenses (weapon, armed, knife, gun, shots fired), and other sex offenses (prostitution, indecent exposure, pornography, sexual harassment, lascivious acts, etc). We drop all incidents of domestic violence and child maltreatment regardless of offense category because coordinates are often missing in most cities to protect victims.

Uncategorized. Across cities, we encounter incidents with some variation of the following names: "suspicious (person, circumstance, auto, building)" or "investigate (person, auto, building)." These incidents have high frequency in many cities, but it is unclear whether all or some can be considered either Part 2 offenses, an alternative name for incidents related to "premise checks" or "welfare checks," or some catch all general category. We therefore do not include these incidents in the definition of Part 2 offenses, but report estimates for “suspicious event” in Table S8, defined as suspicious person, circumstance, auto or investigate person or auto.

F.2.3 Minority Neighborhoods

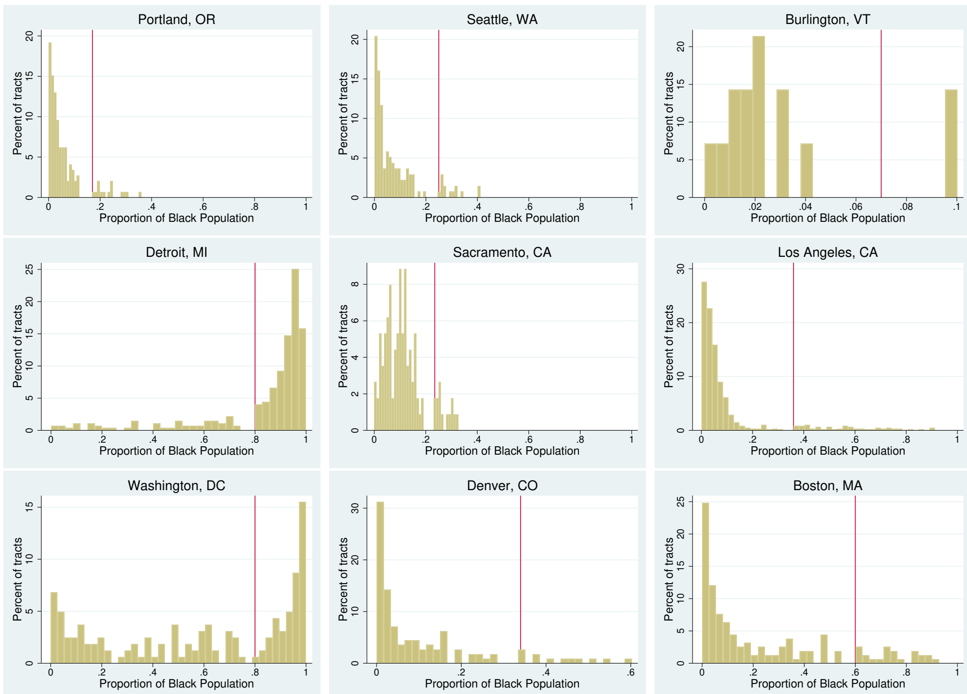

Figure S43: Distribution of minority neighborhoods

Histograms in Figure S43 depict the distribution of Census tracts based on the proportion of Hispanic and non-Hispanic Black residents b in the tract. The red line indicates the threshold m used to classify tracts as minority neighborhoods, with all tracts where the proportion of Black residents b is greater or equal than m classified as minority areas ($b \geq m$). The distribution of tracts varied widely across cities, making it challenging to apply a common threshold m for all cities. We therefore implemented a tailored approach that accounted for these differences in distribution across cities.

In cities where a majority of tracts had a low proportion of Black residents (e.g., Portland, Seattle, Sacramento, Los Angeles, Denver, Boston, Burlington), we used modified percentile-based cutoffs. In these cases, we classified tracts as minority neighborhoods if its proportion of Black residents was approximately at the 90th percentile or higher ($m \approx b_{90th}$). Specific assignment varied slightly from the 90th percentile based on histogram patterns and plausible natural breaks. In cities where a substantial number of tracts had a high proportion of Black residents (Detroit, District of Columbia), we classified tracts as minority neighborhoods if the proportion of Black persons b was at least 0.80 ($m = 0.80$), regardless of percentile. We then established a comparison group threshold n to classify tracts as non-minority neighborhoods with $b \leq n$. We set $n = m - a$, effectively excluding tracts that were adjacent but below threshold m if the proportion of Black persons b fell within $[m - a, m)$. We defined the adjacent tract threshold as $a = 0.05$. We did this to exclude tracts that would otherwise be classified as non-minority neighborhoods despite having a proportion of Black residents b close to the threshold m for minority neighborhoods.

F.2.4 Robustness Checks and Additional Findings

Robustness Checks. Table S7 examines the robustness of our baseline estimates in Table 4 to alternative approaches for classifying tracts into minority and non-minority neighborhoods. We show ten exercises. (i) We start by reporting baseline estimates, where the minority tract threshold m was generated with the tailored approach previously described in F.2.3 and S43 and the adjacent tract threshold $a = 0.05$. We then conduct various exercises, modifying thresholds for minority tracts m, non-minority tracts n, or adjacent tracts a.

In the first set of exercises, we modify the adjacent tract threshold a, while fixing m as defined at baseline. (ii) We set $a = 0$ to reintroduce the previously excluded adjacent tracts (those within 5 percentage points below the city-specific threshold m). (iii) We also set $a = 0.10$, which excludes a larger number of adjacent tracts than the baseline specification.

For the second set of exercises, we modify the treatment tract threshold m by using city-specific percentiles, while fixing $a = 0.05$ as defined at baseline. (iv-vi) We assign threshold m using a strict percentile-based approach (85th, 90th, 95th percentile) that classifies a fixed percentage of tracts as minority neighborhoods in each city ($m = b_{85th}$, $m = b_{90th}$, $m = b_{95th}$), irrespective of the shape of the distribution. (vii) We assign threshold m using an alternative tailored approach with two different conditions for m, where a tract is considered a minority neighborhood if the proportion of Black residents b is greater than the minimum between $m = b_{90th}$ or $m = 0.50$, so that cities that are more diverse rely on the 50% threshold instead of the percentile ($m = \min\{b_{90th}, 0.5\}$).

For the last set of exercises, we use a relative deviation approach, defining tracts as minority neighborhoods if its proportion of Black persons b exceeds the citywide average $\overline{b}$ by k times the citywide standard deviation $sd(b)$. Again, we hold the adjacent tracts that are excluded fixed at $a = 0.05$. (viii-ix) We classify tracts as minority neighborhoods if b is higher than the citywide average plus 1 or 2 standard deviations ($m = \overline{b} + 1sd(b)$, $m = \overline{b} + 2sd(b)$). (x) Lastly, we modify this approach by applying a similar rule for the non-minority tracts. To avoid losing too many tracts, we use half a standard deviation in our definition so that the threshold for belonging to the non-minority group is $n = \overline{b} - 0.5sd(b)$ and the threshold for belonging to the minority group is $m = \overline{b} + 0.5sd(b)$, with all other tracts in the middle excluded.

Additional Findings. We stratify Part 2 offenses in Table S8 and find heterogeneity across offenses and cities.

Table S7: Alternative classification of minority neighborhoods

	Drug-defined offenses	Other offenses			Net effects
		Part 1			
		Violent	Property	Part 2	Total
Baseline	-1.40***	-0.58	-2.51***	-1.83	-4.81**
	(0.29)	(0.36)	(0.81)	(1.84)	(2.06)
Mean	3.12	15.72	31.32	78.57	107.20
$a = 0$	-1.23***	-0.53	-2.45***	-1.43	-4.35**
	(0.29)	(0.35)	(0.79)	(1.79)	(2.01)
Mean	3.12	15.72	31.32	78.57	107.20
$a = 0.10$	-1.40***	-0.56	-2.45***	-2.01	-4.84**
	(0.30)	(0.36)	(0.83)	(1.84)	(2.10)
Mean	3.12	15.72	31.32	78.57	107.20
$m = b_{85th}$	-1.04***	0.26	-1.22	1.28	-0.11
	(0.32)	(0.39)	(0.84)	(2.50)	(2.84)
Mean	4.45	12.59	33.52	60.85	100.66
$m = b_{90th}$	-1.50***	-0.41	-2.57**	-1.59	-4.79*
	(0.32)	(0.43)	(1.00)	(1.96)	(2.66)
Mean	4.53	13.71	34.76	63.01	104.83
$m = b_{95th}$	-1.88***	-0.59	-3.47***	-2.63	-6.83**
	(0.51)	(0.59)	(1.26)	(2.50)	(3.41)
Mean	4.58	13.91	34.61	61.82	103.80
$m = \min\{b_{90th}, 0.5\}$	-1.30***	-0.41	-1.49*	-1.76	-3.48
	(0.42)	(0.38)	(0.90)	(1.93)	(2.18)
Mean	3.05	15.14	31.21	79.17	100.26
$m = \bar{b} + 1sd(b)$	-1.18***	-0.32	-2.23**	-2.43	-4.68**
	(0.41)	(0.35)	(0.90)	(1.96)	(2.22)
Mean	5.13	12.69	34.59	59.13	92.00
$m = \bar{b} + 2sd(b)$	-2.21***	-0.34	-3.58***	-2.44	-6.98*
	(0.52)	(0.75)	(1.27)	(2.57)	(3.89)
Mean	5.83	13.73	37.48	60.17	112.48
$n = \bar{b} - .5sd(b)$,	-0.89***	0.03	-0.19	1.33	0.64
$m = \bar{b} + .5sd(b)$	(0.30)	(0.33)	(0.84)	(2.43)	(2.62)
Mean	3.85	14.03	34.03	75.60	106.36

Notes: Calls for service and reported crime data in select RCL cities, pooling all nine cities. Tract-year-quarter incident counts are stratified by minority neighborhoods using different definitions. m = minority neighborhood threshold. n = non-minority neighborhood threshold. a = adjacent tract threshold. b = proportion of Black residents in tract. $\bar{b}$ = citywide average of b. $sd(b)$ = citywide standard deviation of b. Standard errors clustered at the tract level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S8: Effect of recreational cannabis laws on Part 2 offenses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Part 2 Offenses								Related Incidents
	Financial	Weapon	Simple Assault	Vandalism	Disorder	Public Intoxic.	DUI	Other Part 2	Suspicious Event
Pooled	-0.60***	-0.22	-1.41***	0.33**	-1.31	1.29	0.56**	0.32	-0.95
	(0.16)	(0.31)	(0.42)	(0.16)	(2.50)	(0.87)	(0.23)	(0.34)	(2.31)
Mean	3.52	12.41	19.08	6.84	55.66	7.97	5.71	3.12	26.88
Portland, OR	-0.01	-0.65*	-0.63	-0.68	-12.15*	n.a.	0.02	n.a.	-2.76
	(0.23)	(0.38)	(0.62)	(0.43)	(6.40)	n.a.	(0.12)	n.a.	(1.79)
Mean	1.78	4.95	14.16	6.81	80.99	n.a.	0.78	n.a.	22.83
Seattle, WA	-0.29	-0.41	-0.32	-0.01	-9.52	3.53	0.51***	2.61	-12.11***
	(0.45)	(0.35)	(0.52)	(0.37)	(7.87)	(2.81)	(0.16)	(2.86)	(2.97)
Mean	5.38	3.11	3.21	10.02	89.00	5.58	1.25	9.42	56.08
Burlington, VT	0.46	n.a.	-0.20	2.32	15.46	11.42	0.25*	0.60	14.13
	(0.53)	n.a.	(2.06)	(2.32)	(16.40)	(14.43)	(0.12)	(2.22)	(8.20)
Mean	8.83	n.a.	36.90	24.46	149.15	55.73	1.88	67.40	102.33
Detroit, MI	-0.30**	0.42	-3.65**	0.57	3.66	0.23	1.42**	0.13	2.30
	(0.14)	(0.81)	(1.54)	(0.35)	(2.76)	(0.54)	(0.72)	(0.24)	(1.47)
Mean	0.77	15.36	27.92	5.66	46.45	7.19	7.34	1.59	13.73
Sacramento, CA	n.a.	-2.13	-1.37	1.13*	10.20	-0.35	-0.02	-0.69	6.73
	n.a.	(1.43)	(1.60)	(0.63)	(10.94)	(0.77)	(0.14)	(0.61)	(13.61)
Mean	n.a.	18.42	30.12	5.49	116.48	7.27	2.08	4.91	118.45
Los Angeles, CA	-0.96***	n.a.	-1.68**	0.61**	n.a.	n.a.	n.a.	0.24	n.a.
	(0.26)	n.a.	(0.74)	(0.28)	n.a.	n.a.	n.a.	(0.21)	n.a.
Mean	5.68	n.a.	12.55	7.15	n.a.	n.a.	n.a.	1.61	n.a.
Boston, MA	-0.94**	-0.58**	0.66	-0.90	2.13***	0.51*	n.a.	0.47	2.04***
	(0.39)	(0.28)	(0.76)	(0.56)	(0.52)	(0.26)	n.a.	(0.43)	(0.69)
Mean	7.72	3.91	23.12	13.96	18.42	1.02	n.a.	8.32	12.68
Denver, CO	0.14	1.51**	-0.68	0.23	0.09	n.a.	n.a.	-0.87*	n.a.
	(0.80)	(0.68)	(0.90)	(0.19)	(0.47)	n.a.	n.a.	(0.46)	n.a.
Mean	2.58	1.83	3.05	0.94	8.64	n.a.	n.a.	0.38	n.a.

Notes: Calls for service or reported crime data for Part 2 offenses and other incidents that are not in the Part 2 definition but that might be related. Portland (C, E, NE), Seattle (CI, E, NE), Burlington (CI, E, NE), Detroit (CI, E), and Sacramento (CI, PI, E, NE) reflect calls for service. Los Angeles, Denver, DC, and Boston reflect reported crimes. DC does not report Part 2 offenses. Outcomes reflect total incident counts for Part 2 offenses in a tract-quarter, stratified by minority neighborhoods. Due to differences in collection and reporting of incidents, outcomes can be inconsistent across cities. Details in Sections 2 and F.2. CI=Civilian initiated calls for service. PI=Police initiated calls for service. E=Emergency calls for service. NE=Non-emergency calls for service. Standard errors clustered at the tract level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

F.3 DUI Fatalities

We examine the effect of RCLs on DUI fatalities per 10,000 people using data from the Fatality Analysis Reporting System (FARS), which provides a comprehensive record of fatal motor vehicle traffic crashes. This dataset includes all public road accidents where at least one person died within 30 days of the crash. While FARS data are available in 2007, changes in some variables make earlier years not directly comparable with later data. Therefore, we focus on the period from 2008 to 2019.

We aggregate deaths at the state-year level, distinguishing between White and Black individuals (excluding other races or cases with unidentified race). We identify accidents where drivers were suspected or confirmed to be under the influence of drugs (excluding alcohol and nicotine) based on police reports and/or drug tests. We convert fatalities to rates per 10,000 people, and we perform data checks to address inconsistencies, such as excluding Pennsylvania due to having zero fatalities in most years of this sample.

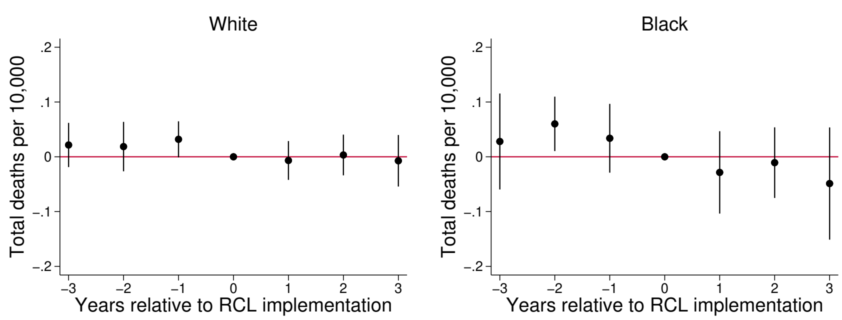

Figure S44: DUI fatality rates, event study

Notes: DUI fatality rates are from the 2008-19 Fatality Analysis Reporting System. State-year counts for a given race are divided by state-year population estimates corresponding to that race, and multiplied by 10,000. Event study regressions are weighted by race-specific population estimates. Controls include cannabis decriminalization laws. 95% confidence intervals are clustered at the state level. The reference year is $t = 0$, the year immediately before RCL implementation.

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