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

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Racial disparities in enforcement of drug prohibition are longstanding, with Black communities disproportionately affected. We study the effect of cannabis legalization on racial disparities in criminal justice outcomes using a difference-in-differences framework. Legalization led to significant declines in cannabis arrests for White and Black populations, but did not eliminate racial disparities. Total arrests were unchanged due to offsetting increases in arrests for less serious quality-of-life offenses, particularly among Black populations. Incarceration rates for drug offenses only declined for White populations. Lastly, we do not find evidence of increased criminal activity among Black populations, suggesting this mechanism cannot explain arrest increases.

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

JEL codes: I18, I14, H75.

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1 Introduction

The prohibition of cannabis is considered one of the most costly and destructive aspects of America's War on Drugs. The toll comprises years of life lost behind bars, criminal records crippling access to jobs, loans, and housing, billions of dollars spent on law enforcement, children growing up without a parent, and systemic violence from the creation of an illegal drug market (Hudak, 2021; Earp et al., 2021). In 2018, police officers made 663,000 cannabis arrests, 92% for possession and 8% for sales, accounting for 40% of all drug arrests and exceeding arrests for all violent crimes combined (Gramlich, 2020). Incarceration statistics are also striking. Drug possession or trafficking was the most serious offense for serving time among 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. Between 1980 and 2010, the percentage of the Black population that was justice-involved more than tripled and racial disparities in incarceration rose (Shannon et al., 2017). Mass incarceration was partly driven by the War on Drugs and its punitive approach to drug prohibition enforcement, targeting open-air drug markets in minority communities (Alexander, 2010; Beckett et al., 2006; Cox and Cunningham, 2021; Neal and Rick, 2016).[1] This exacerbated racial differences in police contact and increased the incidence of Black and Hispanic persons who are stopped and frisked for contraband (MacDonald and Fagan, 2019), involved in traffic stops (Horrace and Rohlin, 2016; Pierson et al., 2020; Feigenberg and Miller, 2022), and arrested.

Racial disparities in law enforcement of drug prohibition persist in the post War on Drugs era. Even though White and Black persons use cannabis at roughly similar rates, 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 than White persons for drug-related offenses. Despite representing 12.5% of the total U.S. population, Black persons account for 28% of state and 33% of federal prisoners with a sentence of more than one year for a drug-related offense (Carson, 2021; Motivans, 2020). Black communities

[1] According to the Sentencing Project, the number of individuals incarcerated for drug offenses increased from 57,975 in 1983 to 353,564 in 1993; and during the height of the War on Drugs, Black and Hispanic persons constituted nearly 90 percent of sentenced state prisoners for drug possession (Mauer et al., 1995).

are also disproportionately affected by systemic violence. Over 50% of homicide victims are Black (Federal Bureau of Investigation, 2018), and the firearm death rate among Black persons is nearly three times that of White persons (Kaiser Family Foundation, 2022).

We study the effect of drug prohibition reform on racial disparities in the criminal legal system, focusing on the legalization of cannabis, the most commonly used illegal drug. As of 2022, 21 states and the District of Columbia have passed recreational cannabis laws (RCLs), allowing individuals ages 21+ to possess, use, and supply cannabis for recreational purposes (ProCon, 2022). Supporters of legalization espouse that RCLs will create legal jobs, generate tax revenue, reduce illegal markets, lower crime and violence, reduce law enforcement costs, and narrow racial disparities in criminal justice outcomes (Gettman and Kennedy, 2014).

Previous RCL studies primarily focus on measures of cannabis use, documenting increases for adults and mixed evidence for teenagers (Cerdá et al., 2020; Hansen et al., 2020). One study found significant increases in self-reported cannabis use among Hispanic and White adults, but not for Black adults (Martins et al., 2021). Several studies have considered criminal justice outcomes in the general population, documenting mixed findings (Lu et al., 2021; Sabia et al., 2021; Stohr et al., 2020; Dragone et al., 2019; Brinkman and Mok-Lamme, 2019; Wu et al., 2020; Meinhofer and Rubli, 2021; Plunk et al., 2019). There is consensus, however, that RCLs reduced cannabis possession arrests. Evidence also points at reductions in law enforcement seizures of cannabis and other drugs.

The majority of RCL studies have not considered racial disparities in the criminal legal system. The few that have, documented large reductions in cannabis possession arrests for Black and White adults (Edwards et al., 2020; Firth et al., 2019; Sheehan et al., 2021). Previous studies, however, are either descriptive, lack comparison groups, use data from few states, or analyze cannabis possession arrests exclusively.[2] Researchers have highlighted that it may be equally or more important to evaluate other cannabis offenses, such as sales and public intoxication, as well as other drug and non-drug offenses (Smart and Kleiman, 2019). Importantly, no studies have elucidated the net effects of RCLs, that is, whether legalization narrowed longstanding racial disparities in overall arrests or other criminal justice outcomes.

[2] Focusing on an outcome related to policing, Pierson et al. (2020) found reductions in traffic stops resulting in police searches among Black, White, and Hispanic persons in two states.

Understanding these net effects is crucial because RCLs may lead to spillovers such as police reallocating resources away from cannabis possession arrests to the prevention of other crimes (Makin et al., 2019). RCLs may also lead to greater cannabis use, which in turn may increase economic crime to finance cannabis consumption or crime associated with the psychoactive effects of cannabis use. Additionally, RCLs may lead to changes in the size and nature of the illegal drug market more broadly (Stohr et al., 2020; Meinhofer and Rubli, 2021), thus, affecting systemic violence.

We address these gaps and generate the most comprehensive estimates to date of the effect of RCLs on racial disparities in criminal justice outcomes. We start by presenting estimates for arrests, the first point of contact between law enforcement and civilians. We then present estimates for the downstream outcome of incarceration. These criminal justice outcomes are a function of both law enforcement efforts and criminal activity. We therefore explore potential mechanisms by presenting estimates of criminal activity, which may reflect the psychoactive effects of cannabis use, economic crime associated with cannabis use, or systemic violence from cannabis trafficking. We analyze all outcomes by race (i.e., White and Black) and calculate the Black-White rate ratios and rate differences. We use administrative data from 2007-2019 and a difference-in-differences (DID) framework that exploits the staggered implementation of RCL in 11 states during this period. Throughout, we show static DID estimates and event study plots, and test the robustness of findings to the recent advances in the DID literature.

We find that RCLs led to significant declines in arrest rates for cannabis possession and sales among White (59%) and Black (50%) persons, reducing but not eliminating absolute disparities and generating null effects on relative disparities. Reductions in cannabis arrests were accompanied by offsetting increases in arrests for non-drug offenses, particularly low-level, quality-of-life offenses, which often involve more discretion from law enforcement (i.e., disorderly conduct, vagrancy, and vandalism). Although both White (5%) and Black (9%) persons displayed this pattern, effects were larger and only statistically significant for Black persons. Together, RCLs yielded null net effects on total arrests for both racial groups. Likewise, RCLs yielded null net effects on total prisoner admissions and total prisoners at yearend for both racial groups. However, we find significant declines in prisoner admissions for drug offenses among White persons only. These findings suggest that RCLs did not reduce longstanding racial disparities in the criminal legal system.

We next explore potential mechanisms for offsetting increases in non-drug arrests. Given that arrests are a function of crime and policing, we gauge whether arrest increases can be explained by greater crime production following legalization. While we find increases in outcomes related to the psychoactive effects of cannabis use across racial groups, we do not find increases in outcomes related to criminal activity. On the contrary, we mostly document declines in crime, especially among Black persons (e.g., declines in reported crimes and calls for service in predominantly Black neighborhoods). We also find suggestive evidence of declines in homicides for Black persons but null effects for White persons. These findings suggest that greater crime production is an unlikely driver of the increases in non-drug arrests, nor differential arrest increases for Black persons.

Together, while legalization may reduce absolute disparities in cannabis arrests, it is not a silver bullet for reducing or eliminating overall racial disparities in the criminal legal system. It is possible that longstanding law enforcement incentives and behaviors largely influence these effects. If policymakers seek to reduce and amend these disparities, further steps beyond cannabis legalization will be necessary.

The paper is structured as follows. Section 2 describes cannabis liberalization policies, previous literature, and the conceptual framework. Section 3 describes the data. Section 4 lays out the identification strategy. Section 5 presents the main findings, and Section 6 explores potential mechanisms. Section 7 discusses policy implications and concludes.

2 Background

2.1 Cannabis Liberalization

The U.S. Federal government classifies cannabis as a controlled substance in Schedule I. Drugs in this schedule have no accepted medical use, a lack of accepted safety, and a high potential for abuse (Drug Enforcement Administration, 2019). At the state level, however, 21 states and the District of Columbia have enacted recreational cannabis laws as of 2022, legalizing cannabis sales, distribution, possession, and use among adults aged 21 or older, subject to amount limits and other restrictions (ProCon, 2022). All RCLs were predated by medical cannabis laws (MCLs) and some by cannabis decriminalization laws (CDLs). MCLs allow authorized physicians to recommend cannabis use for patients with eligible health conditions. CDLs remove criminal sanctions for small cannabis possession offenses with no protection for cannabis supply offenses. Instead, the penalties for possession can range from no penalties, civil fines, drug education, or drug treatment (Svrakic et al., 2012). Decriminalization may offer some relief from mass incarceration, but it still preserves many of the punitive features and consequences of the criminal misdemeanor experience (Natapoff, 2015).[3] These consequences are likely to affect poor and otherwise disadvantaged defendants, most of which are persons of color (Smart and Kleiman, 2019).

2.2 Previous Literature

2.2.1 Cannabis Liberalization Policies and Criminal Justice Outcomes

Previous studies of the impact of cannabis liberalization policies on criminal justice outcomes primarily focus on MCLs and their effects in the general population (Morris et al., 2014; Huber III et al., 2016; Chu and Townsend, 2019; Gavrilova et al., 2019; Dragone et al., 2019; Anderson and Rees, 2023; Zakrzewski Jr et al., 2020). None of these studies found that MCLs increased crime; if anything, crime declined. Studies of the impact of CDLs on criminal justice outcomes in the general population document reductions in drug-related arrests (Grucza et al., 2018; Plunk et al., 2019). RCL studies have also considered criminal justice outcomes in the general population, including property and violent crimes, arrests, and drug seizures. There are somewhat mixed results regarding the impact of RCLs on crime, with studies documenting no changes in crime (Lu et al., 2021; Stohr et al., 2020) or reductions (Dragone et al., 2019; Brinkman and Mok-Lamme, 2019; Wu et al., 2020). There is consensus, however, that RCLs did reduce cannabis possession arrests among adults, as

[3] In particular, it makes it easier to impose fines and supervision on populations that will often face punitive consequences when they cannot afford these fines or comply with stringent supervisory conditions. An unpaid penalty can turn into a court judgment and an arrest warrant in some states, and that judgment can follow the individual for years after the penalty, when applying for a driver's license, registering an automobile, or establishing credit (Smart and Kleiman, 2019).

well as law enforcement seizures of cannabis and other drugs (Meinhofer and Rubli, 2021; Plunk et al., 2019; Stohr et al., 2020).

Previous studies of the impact of cannabis liberalization policies on criminal justice outcomes have also considered racial disparities. Sheehan et al. (2021) analyzes the impact of CDLs on cannabis possession arrests among Black and White youth and adults using an event study approach, finding reductions across the board. Gunadi and Shi (2022) finds similar results: decriminalization was associated with a 17% decrease in racial disparity in cannabis possession arrests rates between Black and White adults.

As noted above, the handful of previous RCL studies analyzing racial disparities in criminal justice outcomes are either descriptive or based on pre-post analyses, and use data from a single state or few states (Edwards et al., 2020; Firth et al., 2019, 2020; Pierson et al., 2020). An exception is Sheehan et al. (2021), which employs an event study approach and documents reductions in cannabis possession arrests for Black and White adults in the first three years following RCL implementation. In a descriptive report, Edwards et al. (2020) documents that RCL states had lower racial disparities in cannabis possession arrests in 2018 than states where cannabis remained fully illegal, as well as states that decriminalized. Firth et al. (2019), a single-state study focusing on Washington, also finds that cannabis possession arrests decreased significantly among both Black and White adults but that relative disparities grew from 2.5 to 5 following legalization. Lastly, in related work, Pierson et al. (2020) documents reductions in police traffic stops resulting in searches among Black, White, and Hispanic persons in Washington and Colorado.

In addition to methodological limitations, these RCL studies analyzed racial disparities in cannabis possession arrests almost exclusively, an outcome that only reflects partial effects. Elucidating the full impact of RCLs on racial disparities in the criminal legal system requires examination not only of arrests for cannabis possession, but also for cannabis sales, for other drug possession and sales, and for non-drug offenses, as well as other law enforcement and systemic violence outcomes that reflect the toll of drug prohibition. This way, we can assess potential spillovers and net effects associated with RCLs.

2.3 Conceptual Framework

The net effect of cannabis legalization on overall criminal activity and associated law enforcement efforts is theoretically ambiguous, and will be largely influenced by the effect of RCLs on the consumption and production of cannabis and other drugs, and the relationship between law enforcement, criminal activity, cannabis, and other drugs.

2.3.1 Cannabis and Crime

Cannabis and crime are plausibly linked through at least four pathways: (1) cannabis-defined offenses, (2) systemic violence, (3) psychoactive effects, and (4) economic crime (Pacula and Kilmer, 2003). Cannabis *prohibition* is the driver of pathways (1) and (2), while cannabis *use* is the driver of pathways (3) and (4). First, cannabis *prohibition* implies that cannabis is defined as a crime; it is a crime to use, possess, manufacture, or distribute cannabis. Second, cannabis *prohibition* may cause crime by generating systemic violence in illegal drug markets. Illegal drug markets are associated with increased systemic violence because of turf wars among suppliers, unpaid drug debts, and other issues, particularly since illegal producers do not have access to legal conflict resolution mechanisms (Miron, 1999; Levitt and Venkatesh, 2000; Adda et al., 2014). While illegal cannabis sales are generally conducted indoors and illegal cannabis markets are less violent than other illegal drug markets (Caulkins and Pacula, 2006; Pacula and Kilmer, 2003), illegal cannabis markets generate systemic violence (Charns, 2023; Drug Enforcement Administration, 2021, 2013; Dale and Izaguirre, 2017; Archibold, 2009). Third, the psychoactive effects of cannabis *use* may influence user behavior, leading to criminal activity in some individuals. While studies have generally shown that cannabis use temporarily inhibits violence, there is evidence of violent behavior in some populations (i.e. adolescents, high frequency users). Moreover, toxicology reports and self-reported data shows that the prevalence of cannabis and other substances among homicide victims and offenders significantly exceeds population prevalence (Darke et al., 2009; Fendrich et al., 1995).[4] The psychoactive effects of cannabis *use* may also cause

[4]Likewise, our analysis of inpatient data used in this study shows that the prevalence of cannabis use disorder is about six times higher for hospitalizations involving victims of gun injury or assault, relative to other hospitalizations.

other non-violent crimes such as traffic offenses (i.e. driving under the influence), or public nuisances such as disorderly conduct. Fourth, cannabis *use* may induce economic crime among users to finance their consumption. Lastly, there may be a fifth pathway: (5) the correlation between the production and consumption of cannabis and that of other drugs.[5]

2.3.2 RCLs and Reductions in Crime

When cannabis is no longer defined as a crime, police cannot make arrests for cannabis use, possession, manufacture, or distribution that abides to RCL provisions. Therefore, cannabis legalization should reduce arrests for cannabis-defined offenses. Assuming these are single-offense incidents, we would expect to see a decline not only in cannabis arrests but also in total arrests. However, to the extent that cannabis arrests are pretextual or accompanied by other offenses within the same incident, we may observe declines in cannabis arrests with limited or no declines in total arrests.

The creation of a legal cannabis market should reduce the size of the illegal cannabis market, decreasing crime from systemic violence (Dragone et al., 2019). A legal cannabis market may also decrease crime through legal job growth and expungement of cannabis conviction records. The cannabis industry is one of the fastest-growing industries in the country, creating jobs in agriculture, professional services, and hospitality (Kavousi et al., 2022). Job creation from legalization may increase the opportunity cost of participating in illegal markets and reduce drug crimes committed (Ihlanfeldt, 2007).

To the extent that RCLs reduce the production or consumption of other drugs that are substitutes of cannabis, legalization may also reduce adverse criminal justice outcomes for other drugs. RCLs may further reduce violence if greater cannabis use, a sedative drug, leads to substitution away from violence-inducing substances such as alcohol.

[5] Consumers and producers of cannabis may also be consumers and producers of other illegal drugs. The vast cannabis market size suggests potential impacts on other drug markets. Data from the 2015-2018 National Survey on Drug Use and Health indicates that, given past-month cannabis use, prevalence rates for past-month heroin, methamphetamine, and cocaine use were 1%, 2.1%, and 5.4%, respectively. Notably, past-month cannabis use was 60%, 62%, and 69% for those using heroin, methamphetamine, and cocaine, respectively. Evidence also suggests involvement of illegal drug producers and distributors in multiple markets. The majority of foreign heroin, methamphetamine, and cannabis originates from Mexico, where transnational criminal organizations, such as the Sinaloa cartel, play a significant role in production and distribution within the U.S. (Drug Enforcement Administration, 2018; Beittel, 2022).

Lastly, law enforcement agencies may decrease resources allocated to drug prohibition efforts following RCL implementation, further reducing arrests for other drug-defined offenses, regardless of changes in the actual production or consumption of other drugs. Furthermore, state and local governments could use the additional tax revenue from cannabis legalization to support local law enforcement efforts to deter crime.

If RCLs decrease arrests, these could also provide the added benefit of decarceration. However, changes in prisoners will also depend on whether RCLs affect the court system and its considerations for prosecution and sentencing.

2.3.3 RCLs and Increases in Crime

RCLs likely increase aggregate demand for cannabis, which may lead to increases in crimes attributable to the psychoactive effects of cannabis use or economic crimes committed by individuals to finance their consumption. Moreover, if greater cannabis use increases consumption of complement drugs, legalization could lead to more drug-related offenses through any of the pathways previously discussed. The creation of a legal cannabis market may limit profits and job opportunities among illegal drug market producers and distributors, increasing competition. Greater competition may increase systemic violence. The creation of a legal cannabis market may exacerbate criminal activity more generally if criminal records limit illegal drug market sellers' ability to enter the legal market. Lastly, law enforcement may reallocate resources towards pursuing other non-cannabis offenses, increasing arrests and incarcerations for other offenses, even in the absence of changes in the incidence of these other offenses (Makin et al., 2019).

2.3.4 Heterogenous Effects of RCLs by Race

Given the expected net effects of cannabis legalization on criminal justice outcomes are ambiguous, the same should hold for its net effects across racial groups. Subsequent effects on absolute and relative disparities are also ambiguous, and will depend on whether treatment effects are differential across racial groups. If treatment effects are proportional across groups, we would expect no changes in relative disparities but would expect changes in absolute disparities given observed baseline racial differences in the rates of some crimes and law enforcement outcomes. To the extent that treatment effects are heterogenous or change disproportionately across race groups, RCLs may also affect relative disparities.

Treatment effects may be heterogenous for Black populations due to longstanding factors influencing racial disparities in drug prohibition enforcement, including racial discrimination by police (Ba et al., 2021; Rivera, 2022; Wilson, 2021),[6] that police over-target minority communities (Chen et al., 2021; Weisburst, 2019; Jurado, 2022; Feigenberg and Miller, 2022), police incentives to maximize arrests (Stashko, 2023; King and Mauer, 2006), and that minorities are more likely to use or sell drugs in public spaces (Beckett et al., 2006; Fagan et al., 2012). These factors may mitigate or exacerbate the effects for Black populations. For instance, RCL implementation could have a stronger effect on reducing the number of illegal drug markets in communities of color or the number of minorities participating in these markets. RCLs may disproportionately result in job loss for persons of color previously employed by illegal drug markets. Prior criminal records may disproportionally limit legal job opportunities for Black men (Agan and Starr, 2018; Doleac and Hansen, 2020). Moreover, law enforcement may reallocate resources to pursue more costly criminals. If the apprehension costs are higher for White persons (Beckett et al., 2006), law enforcement efforts to engage in more costly policing could increase arrests in under-policed communities (Cox and Cunningham, 2021), further decreasing racial disparities. Also, law enforcement may continue to target minority communities and simply redirect efforts to deter non-cannabis offenses in these communities, increasing arrests for non-cannabis crimes.

3 Data

3.1 Arrests

We obtained arrest data from the Federal Bureau of Investigation's (FBI) 2007-2019 Uniform Crime Reporting Program: Arrests by Age, Sex, and Race (UCR-ASR). Data capture monthly arrest counts for each agency reporting to the UCR Program, disaggregated by

[6] Racial discrimination may arise from taste-based discrimination (i.e., police officers would rather arrest a Black person than a White person for a drug offense) or from statistical discrimination (i.e., police officers believe that crime is more predominant among minorities and allocate more resources to those communities).

offense type, race, age, and sex. UCR data reflect the number of arrests rather than the number of arrested individuals in a given time period.[7] All arrest records reflect the highest charge, according to an FBI hierarchy (Kaplan, 2021). For instance, any cannabis arrest reflects an incident in which cannabis possession or sale was the highest charge for which an individual was arrested during a police interaction.[8] Police officers register the offender's race, based on their own perceptions, as either White, Black, Asian, or American Indian. We restrict our analysis to the first two and to the full population counts. Although ethnicity is also technically reported, the vast majority of agencies did not include Hispanic counts during most of our sample period. Moreover, the data did not allow us to observe the interaction between race categories and sex. We excluded U.S. territories and focused on adults since RCLs only legalized cannabis for individuals ages 21 and over.

Our main analysis considers cannabis arrests (both possession and sales) and total arrests. We then analyze other drug arrests (heroin/cocaine, synthetic narcotics, and other drugs) and non-drug arrests. For the latter, we follow the FBI's classification to distinguish between Part 1 and Part 2 offenses. Part 1 denotes more serious crime, including violent crime (aggravated assault, manslaughter, murder, rape, and robbery) and property crime (arson, burglary, motor vehicle theft, and other theft). Part 2 offenses include less serious crime. Following Chalfin et al. (2022), we break down this category by quality-of-life arrests and all other crimes. Quality-of-life crimes are low-level offenses with a degree of moral judgement. This category includes drunkenness, liquor violations, disorderly conduct, gambling, suspicious behavior, vandalism, vagrancy, and uncategorized arrests.[9] The remaining Part 2 offenses are driving under the influence, embezzlement, family offenses, forgery, fraud, prostitution, other sex offenses, simple assault, stolen property, and weapons violations.

[7]For instance, two drug arrests could correspond to two separate individuals arrested for drug offenses on two separate incidents, or could be the same person arrested on two separate occasions.

[8]The hierarchy for serious offenses (e.g., murder) is common across all agencies. However, for less serious crimes, like drug offenses, each agency decides which crime is the most serious (Kaplan, 2021). This implies heterogeneity in the rule both across agencies and within agencies over time. Comparing data that contain all offenses per incident with UCR, Hendrix and Martin (2019) shows that around 2/3 of drug offenses correspond to single-incident events, and that among multiple-offense incidents, drug arrests are most commonly associated with other drug offenses and, to a lesser extent, public order violations. Appendix Table S3 presents our own analysis of the 2018 National Incident-Based Reporting System, mostly echoing the findings in Hendrix and Martin (2019).

[9]Results are robust to excluding uncategorized arrests from quality-of-life offenses.

Finally, to account for differences in when agencies reported to the FBI, we aggregate arrests up to the county-year level, overall and for each race category. A notable limitation of UCR-ASR is that reporting is voluntary, and some counties have a low number of reporting agencies (Kaplan, 2021). We address this limitation by using the coverage indicator sample criterion, which has been used in previous studies (Freedman and Owens, 2011), and by controlling in our specifications for the number of reporting agencies. Specifically, we construct a county-level index of the share of reporting months each year multiplied by the fraction of the total county population that is covered by reporting agencies in that county. We restrict to an agency reporting coverage threshold of at least 65% and show robustness to stricter values. This effectively drops all data for Florida, Illinois, and Washington, DC, consistent with previous studies (Sheehan et al., 2021). Crucially, for our empirical strategy below, we rely on assuming that missing counts or reporting issues are uncorrelated with the timing of RCLs. We show support for this assumption in Appendix Figure S9.

3.2 Prisoners

We obtained prisoner data from the Bureau of Justice Statistics' 2009-2019 National Prisoner Statistics Program (NPS), which provides a state-year-level enumeration of prisoners under jurisdiction of federal or state correctional authorities on December 31 of the reporting year. Counts include all state and federal inmates held in a public or private prison (custody), those held in jails either physically located inside or outside of the state of legal responsibility, and other inmates who may be temporarily out to court or in transit from the jurisdiction of legal authority to the custody of a confinement facility outside that jurisdiction. We generated yearend prisoner counts at the state-year level, overall and by racial group.

NPS data are subject to limitations. Since 2001, the District of Columbia no longer operates a prison system, thus, we drop it from our sample. A reporting change occurred in California in October of 2011, when there was a realignment of the prison system which shifted the management of lower-level felons from state prisons and parole systems to county jails and probation systems. As this mechanically reduced the number of state prisoners in the data, we also drop California. We exclude New Hampshire due to considerable inconsistencies between total counts and counts by race in various years. Lastly, we drop Michigan for non-reporting of ethnicity in 2019, its one full post-RCL year. In addition, we made some imputations to correct for obvious reporting errors in some states.[10]

We supplemented NPS data with the 2007-2019 National Corrections Reporting Program (NCRP) admissions data from the Bureau of Justice Statistics, capturing offender-level information on prisoners 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, most serious sentenced offense, and other information are collected from individual prisoner records. Admission type may include new court commitments, parole return or revocation, and other admissions (i.e., unsentenced). We exclude admissions for parole return or revocation. We then generate prisoner admission counts at the state-year level, by offense and by race. Offense categories include drug offenses and non-drug offenses (i.e., violent, property, public order, other).

NCRP data are also subject to limitations. First, participation in NCRP is voluntary, therefore not all states submit data each year. Second, race is not adequately reported in some states. Third, state reporting may vary by correctional facility. For instance, some states may only report admissions to state prisons, while others may report admissions to state prisons and jails if they have a unified jail-prison system. We attempt to address some of these issues by dropping states with considerable non-reporting or missing race information for most years (AL, CA, DC, HI, ID, LA, MD, MT, MI, OK, SD, VT). We then excluded a handful of remaining state-year cells with missing race information for at least 20% of the observations. Lastly, we imputed missing observations in a given state-year cell with the average admission count in consecutive state-year cells.

3.3 Criminal Activity

We analyzed data on calls for service and reported crimes, which may proxy for the level of criminal activity, crime perceptions, and other incidents in a defined area during a specific timeframe. Calls for service involve callers, call-takers, dispatchers, and responders,

[10] For example, in 2013 Alaska tallied zero prisoners for each racial group, but reported a jurisdiction total of 5,081 prisoners. To correct for this error, we calculate an average incarceration count using 2012 and 2014 counts to replace the zero value for each corresponding racial group in 2013.

if any (i.e., police, fire department, EMS). Calls may be initiated by civilians or by law enforcement. Civilian-initiated calls are often generated through a call or text to 911 or a non-emergency line. Police-initiated calls are often generated in-person or scheduled in advance to proactively address a community issue. Call-takers receive and input calls into a system that identifies the caller's location and categorize calls into types. Call types include incidents that can be assigned as Part 1 or Part 2 offenses, traffic incidents, health emergencies, welfare checks, and police activity, among others. Dispatchers use this information to assign responders to the incident. Crime data reflects incidents of crime reported to police. Crime types primarily comprise Part 1 offenses (index crimes), and in some cases, a limited number of Part 2 offenses. We obtained call and crime data from publicly available datahubs, where select local city governments publish their data. We selected RCL cities (Portland, Seattle, Burlington, Los Angeles, Sacramento, Denver, DC, Detroit) that published data for at least one year before their state's RCL effective date, reported latitude and longitude associated with the incident, and included incidents for which police responded. Each city varied widely in its sample period, callers and responders captured, incident reporting and categorization, and racial composition of its population. While we attempted to harmonize our sample selection criteria and measures as much as possible, a cookie-cutter approach was not always possible or appropriate. Due to these differences, we analyzed each city separately, comparing the number of incidents in minority neighborhoods relative to those in other neighborhoods within the city, before and after RCL implementation. We analyzed data through 2019. Since Vermont and Michigan implemented RCLs late in 2018, we expanded the sample period of Burlington and Detroit beyond 2019 as to capture the first three years following RCL implementation. When possible, we focused on civilian initiated calls for service for two reasons: first, to reflect changes in criminal activity with limited influence from police incentives or behaviors; and second, police initiated calls for service are not reported for all cities, and when reported, quality appeared questionable in nearly all cases. We used latitude and longitude along with TIGER/Line Shapefiles to match each incident to a Census tract, and collapsed incidents at the tract-quarter level. Then we linked these data with American Community Survey 5-Year population estimates at the tract level. We defined minority neighborhoods as tracts with a high proportion of Black persons, using population estimates corresponding to the first year of incident data reported as to fix treatment and control tracts. We dropped a small number of tracts with less than 100 average incidents per year or with a total population of 1,000 persons or less. Lastly, we dropped tracts with a high proportion of Hispanic persons, as these are also minority neighborhoods but with a low proportion of Black persons.[11] Details are in Section E of the Appendix.

3.4 Deaths

We obtained homicide and motor vehicle traffic 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 cause of death and multiple cause of death for nearly all deaths. We select homicide deaths and motor vehicle traffic deaths from persons aged 12 years or older at the time of death. Cause of death was identified using standard International Classification of Diseases (ICD), Tenth Revision codes (CDC, 2007; CDC, 2002). We also rely on a data field identifying homicide as the manner of death. We distinguish between total homicides and those involving gun injury. We aggregate outcomes at the state of occurrence-year-quarter level, overall and by race.

3.5 Assault Hospitalizations

We analyze hospital discharges from the Healthcare Cost and Utilization Project (HCUP). HCUP is the largest collection of longitudinal hospital data in the US, with all-payer, encounter-level information. We rely on the 2007-2019 HCUP State Inpatient Databases (HCUP-SID) for select states, although our panel is unbalanced since we could not obtain all years for some states. HCUP-SID contains a near census of inpatient care discharge records in participating states, and provides demographic and healthcare information for patients. Some states directly share access to their inpatient discharge records for a lower fee, or can directly generate counts through a request process. We combine HCUP-SID with hospital discharge data directly shared by other states, for a total of 30 states including 10 switching RCL states. We select assault hospitalizations from persons aged 12 years or older

[11] We use 70% as our threshold for this last criteria. This primarily drops tracts in Los Angeles, which has a large Hispanic population, and little to zero tracts in all other cities.

using standard ICD-9 and ICD-10 codes (Smart et al., 2022; CDC, 2021) to identify total assaults and assaults involving gun injury. We aggregate outcomes at the state-year-quarter level, overall and by race.

4 Empirical Strategy

4.1 Main Specification

Our identification strategy exploits 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) regression for the overall population and for each racial group:

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

$Y_{r,j,t}$ denotes an outcome for racial group r, in jurisdiction j (state or county), and in time period t (quarter or year). All outcomes are measured in rates per 10,000 persons by dividing outcome counts by U.S. Census population estimates corresponding to the same racial group r, jurisdiction j, and year t. To get direct measures of 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. In general, rate differences measure absolute disparities while rate ratios measure relative disparities, both of which provide necessary information for understanding changes in disparities across states and over time (Keppel et al., 2005).

$RCL_{j,t}$ is defined as an indicator equal to one if an RCL was effective in jurisdiction j at time period t and zero otherwise. We include jurisdiction fixed effects, denoted by α_j, to account for any time-invariant differences across jurisdictions that may affect outcomes. This implies that we effectively identify our coefficient of interest from within-jurisdiction variation in the outcomes over time. We also include time period fixed effects η_t to control for any jurisdiction-invariant nationwide shocks affecting outcomes. $X_{j,t}$ represents a vector of control variables, which includes an indicator of cannabis decriminalization laws.[12] When using arrest data, we also control for the number of reporting agencies in a given county-year. Lastly, $\varepsilon_{r,j,t}$ represents the idiosyncratic error term. All regressions for group r are weighted by U.S. Census population estimates for that group-jurisdiction-period. Standard errors are clustered by state, which is the level at which the treatment varies (Abadie et al., 2023). This accounts for within-state serial correlation in the error term.

The coefficient of interest is denoted by β, which reflects the static treatment effect of RCLs on outcomes. The main assumption for identifying a causal effect is that, in the absence of an RCL, outcomes would have evolved similarly across jurisdictions during the post-period. This can be partially tested by inspecting trends in outcomes between RCL and non-RCL states prior to implementation (i.e., verifying that differences are constant over time, or parallel trends). Given that we include jurisdiction and time period fixed effects, the only remaining source of potential bias is time-varying unobserved factors at the jurisdiction level. Our controls address some of these potential confounders, which we discuss below.

To provide supporting evidence on the validity of our DID strategy, we present event study estimates from the following equation:

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

where E_j^{RCL} indicates the time period in which jurisdiction j implemented an RCL, $\mathbb{1}_{[\cdot]}$ denotes 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. Plotting the coefficients on the leads and lags β_τ allows us to visually inspect the parallel trends assumption necessary for causal identification and whether treatment effects were dynamic.

[12] 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).

4.2 Robustness Checks

We conduct various robustness checks to address potential concerns. First, given the relatively small number of switching RCL states in our sample, standard methods of statistical inference may over-reject the null (Roodman et al., 2019; Cameron and Miller, 2015; Conley and Taber, 2011). Therefore, we calculate wild cluster bootstrapped confidence intervals (Roodman et al., 2019; Meinhofer et al., 2021). Second, the TWFE DID estimator may be biased if treatment effects are heterogeneous across states and over time (Goodman-Bacon, 2021). We verify the magnitude of this potential issue with the diagnostic test described in De Chaisemartin and d'Haultfoeuille (2020), which calculates the share of DID comparisons that have a negative weight and the sum of all negative weights. We also report results using the robust DID estimators in De Chaisemartin and D'Haultfoeuille (2022), Sun and Abraham (2021), Borusyak et al. (2023), and Wooldridge (2021). Third, we test the robustness of our main findings to the exclusion or inclusion of additional control variables. In particular, we drop baseline controls (i.e., CDLs), and progressively control for MCLs and cannabis expungement laws to account for other cannabis liberalization policies, as well as for state-level unemployment rates to account for economic conditions. Since the effects of cannabis liberalization, particularly on the demand side, may spill over to neighboring jurisdictions that have not yet implemented an RCL, we also add spatial controls that consider these potential effects. Fourth, prior research suggests that the date of cannabis legalization by itself may be an inadequate measure of cannabis access, and that dispensaries are more correlated with increases in cannabis use (Pacula et al., 2015). To address this, we estimate treatment effects replacing the RCL indicator with an indicator of the time when recreational cannabis dispensaries first opened in the state. Lastly, for the arrest data, we consider alternative sample restrictions by imposing stricter thresholds for the reporting agency coverage indicator and by excluding outliers. We also show robustness to dropping each RCL state one at a time to see that the effects are not driven by an outlier state.

5 Main Results

5.1 Arrests

We start by plotting raw data trends in RCL states before and after the policy, by racial group. We normalize time periods so that time zero corresponds to the period right before RCL implementation. Figure 1 plots cannabis arrest rates (both possession and sales) and total arrest rates. Following RCL implementation, there is a sharp decline in the levels of cannabis arrest rates for both racial groups. There is a slight downward trend prior to the policy that levels off in the post-policy period. However, total arrests appear unchanged or slightly higher post-policy, especially for Black persons.

5.1.1 Cannabis Arrests

Figure 2 presents event study plots of the effect of RCLs on county-level cannabis arrests–both possession and sales–per 10,000 persons separately for the White and Black groups. In what follows, all event study plots consider time zero to be the year prior to RCL implementation and all estimates are calculated relative to this year. We observe significant declines in cannabis arrest rates after RCL implementation across both groups. Reassuringly, we find small and insignificant coefficients for the pre-policy period. This indicates that trends in cannabis arrest rates were similar across jurisdictions prior to RCLs, which favors the interpretation of a causal effect.

We show the corresponding parametrized estimates of Equation 1 in Table 1. Each column corresponds to a different outcome: rates for the full population, for each race group, and the Black-White disparity measures. Each panel shows a different arrest category. For ease of interpretation, we calculate the mean of the outcome in RCL states prior to policy implementation. Panel A shows that the estimated declines in cannabis arrest rates are significant for all population groups. Relative to the average rates in RCL states prior to liberalization, the estimated effect amounts to a reduction of 55% in cannabis arrest rates for the full population, 59% for White, and 50% for Black persons.

The last two columns show rate ratios and rate differences of Black persons relative to the White group. We obtain significant and sizable declines in absolute disparities of around 51%

of the baseline absolute disparity. However, due to the larger effect sizes for White persons relative to Black, the point estimate for the rate ratio is small, positive, and insignificant, indicating that relative disparities between Black and White populations did not narrow after RCL implementation.

To further understand the composition of these declines, Panels B and C distinguish between possession and sales, respectively. We obtain significant declines after RCL implementation in both types of cannabis arrests. For the full population, this amounts to a 59% and 39% average reduction for possession and sales, respectively. A similar pattern holds for the White populations (62% and 44% declines), with larger declines in possession relative to sales. However, effect sizes are similar for both categories among the Black population (50% and 49% declines). Moreover, we obtain significant declines in absolute disparities between the White and Black groups, but insignificant effects of RCLs on relative disparities. Taken together, these results indicate that RCLs indeed led to declines in cannabis arrests, but effect sizes did not conclusively close the Black-White disparities.

5.1.2 Total Arrests

We next verify whether this decline in cannabis arrest rates translated into a decline in total arrest rates. Figure 3 shows the event study plots. Point estimates suggest a slight increase in arrests for Black but not for White persons, although the very large standard errors do not reject that effect sizes are equal between the groups (and equal to zero). We also do not observe any significant pre-trends.

Panel D in Table 1 shows the corresponding TWFE DID results for this category. Effects are imprecisely estimated, as evidenced by the standard errors that are quite large. However, the magnitude of the point estimate for White persons is small, implying an increase of 2%, while the point estimate for Black persons is somewhat larger, indicating an increase of 5% in the total arrest rate. This difference leads to an estimated increase in disparities between these groups of between 1 and 6%, although these are also insignificant at conventional levels.

These results suggest that RCLs were not associated with declines in total arrest rates. For the full population, a test of coefficients rejects that the effects of RCLs on cannabis and total arrests were equal at the 95% confidence level. However, this is driven by a significant difference for the Black population, while we cannot reject similar effect sizes for White persons at conventional levels. This suggests that the large reductions in cannabis arrests were offset by an increase in other arrest categories, particularly for Black populations (although we observe similar patterns for White people). This finding thus motivates our analysis of additional arrest groups.

5.1.3 Other Drug Arrests

Due to the reporting structure of the arrest data and the evidence that the majority of multiple-offense incidents involving drug violations only included drug offenses,[13] we next analyze the effect of RCLs on arrest rates for heroin, cocaine, synthetic narcotics, and other drugs. If multiple-incident arrests involving cannabis are mechanically tallied as other drugs after RCL implementation, then we should observe increases in this category after the policy.

Results in Panel A of Table 2 show negative and statistically insignificant effects of RCLs on arrest rates for other drugs across groups. Point estimates correspond to a small decline of 2% for White persons and a larger reduction of 6% for Black persons relative to the baseline mean. Importantly, the 95% confidence intervals allow us to reject increases of more than 3.1 and 2.4 for White and Black populations, respectively. Hence, the fact that the large declines in cannabis arrests did not translate into declines in total arrests cannot be explained by an increase in other drug arrests.

We further explore differences between possession and sales of other drugs in Panels B and C of Table 2. Panel B shows insignificant and small effects on arrest rates for possession of other drugs across groups. However, Panel C presents significant declines in arrest rates for sales of other drugs across all populations. Relative to the baseline mean, effect sizes correspond to a 14% decline in the full population, a 22% decline for White persons, and a reduction of 17% for Black persons. Event study plots are in Appendix Figure S2.

Taken together, these results indicate that arrest rates for other drug offenses did not increase after RCL implementation. On the contrary, we observe important reductions in arrest rates for sales of other drugs. This further suggests that arrests were not simply

[13] Table S3 shows that 64% of incidents involving drugs are single-offense incidents, 31% involve two offenses, and 5% have three or more offenses. Within two-offense incidents, 3/4 are for drug violations only.

tallied differently in the data before and after liberalization. We conclude that the decline in cannabis arrests were not offset by an increase in arrests for other drug offenses.

5.1.4 Non-Drug Arrests

We next turn our attention to all other arrests that are unrelated to drugs, with TWFE DID estimates shown in Table 3. Panel A groups all of these arrest categories together. We obtain a significant increase in non-drug arrest rates for the full population that is driven by a significant increase in these arrests for Black persons. Relative to the pre-policy mean in RCL states, we obtain an 8% increase for the full population and a 9% increase for Black persons. The effect for the White population is not statistically significant, although the point estimates suggest a 5% increase. We cannot reject that effect sizes for Black and White persons are statistically the same. We also obtain positive estimates for our measures of Black-White disparities, with an insignificant 4% increase in the rate ratio and a significant 12% increase in the rate difference. Appendix Figure S3 shows event study plots.

To shed light on these effects, we follow the FBI's classification of crimes for non-drug arrests, distinguishing between serious (Part 1) and less-serious (Part 2) offenses. We further stratify Part 2 crimes into quality-of-life offenses and all other Part 2 offenses (Chalfin et al., 2022). Quality-of-life offenses include public intoxication, liquor violations, disorderly conduct, gambling, suspicious behavior, vandalism, and vagrancy. Our TWFE DID estimates for these categories are shown in Panels B through E in Table 3.

Panel B presents insignificant effects on Part 1 arrest rates, while Panel C shows positive and significant estimates for Part 2 crimes for the full population and the Black group. This indicates that our previous findings on non-drug arrests are driven by these less serious crime categories. Relative to the baseline mean, these results suggest a 10% increase for the full population and a 12% increase for Black persons. Although insignificant, point estimates for the White group amount to 6% of the baseline mean.

Panels D and E further break down Part 2 arrests into two separate categories. We find large and significant increases in arrest rates for quality-of-life crimes, corresponding to a 15% increase for Black persons. For the White group, the point estimate suggests a 9% increase, but is statistically insignificant. Estimates for the other Part 2 crime categories are also positive, but indicate effect sizes of 6% for Black persons and 2% for White persons. However, these effects are statistically insignificant at conventional levels. Event study plots are shown in Appendix Figures S4 through S6.

To further illustrate the drivers of these effects, Appendix Figure S18 shows TWFE DID estimates for each of the non-drug crime categories, distinguishing between White and Black persons. The estimates confirm that Part 1 crime was mostly unchanged after RCL implementation. For Part 2 offenses, we obtain positive and significant increases in arrest rates for White persons for disorderly conduct, fraud, and simple assault, with effect sizes of 45%, 51%, and 10% of the baseline mean, respectively. Note that disorderly conduct is the only quality-of-life arrest that is significant for White persons. For Black persons, we also estimate significant increases in arrest rates for disorderly conduct, fraud, and simple assault (effect sizes of 80%, 38%, and 18%, respectively). However, we find further significant increases in other quality-of-life arrest rates, including gambling, vagrancy, and vandalism.

Overall, these estimates indicate that the declines in cannabis arrests were offset by increases in arrest rates of Part 2 crimes. Our results suggest that both White and Black populations saw increases in arrests for these less serious crime categories, even if the total effect was only significant (and larger) for Black persons. Moreover, the arrest categories for which we estimate significant increases are those that involve more discretionary behavior from police officers, and potentially lead to more long-lasting negative impacts.

5.1.5 Robustness Checks on Arrest Data

We perform a series of robustness checks on our main results for the arrest data in the Appendix. First, we consider a series of changes to our main specification and show TWFE DID estimates from each of these modifications in Figures S10, S11, S12, and S13. 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. Overall, we obtain very similar results.

Second, we show that the estimates are similar when excluding one RCL state from the sample at a time in Table S4. This implies that our findings are not driven by one particular RCL state, although we do obtain estimates that are somewhat larger when dropping California from the sample, suggesting a potentially more muted response in that state.

Third, we show that results are robust to controlling for potential spillover effects across jurisdictions in Table S5. 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 to 200 miles, and lastly, include the inverse distance to the nearest county with an RCL. In all cases, we obtain similar results.

Fourth, we address the potential issues with staggered timing DID recently identified in the literature in three ways. 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. This is likely because RCLs are more recent and the share of treated states in our sample is low.[14] Moreover, the sum of negative weights is very small. Second, we calculate the DID estimators proposed by De Chaisemartin and D'Haultfoeuille (2022), Sun and Abraham (2021), Borusyak et al. (2023), and Wooldridge (2021) in Figures S10, S11, S12, and S13. Lastly, we present the equivalent dynamic Sun and Abraham (2021) estimator in Figures S14 and S15. Results provide additional reassurance that the effects hold when accounting for heterogeneous average treatment effects.

Finally, we verify that our results are not driven by previous CDLs, by estimating differential impacts of RCLs by whether the state had already implemented a CDL prior to passing the RCL. Appendix Table S6 shows that across all groups, declines in cannabis arrest rates are much starker when the state had not yet decriminalized cannabis. For the other arrest categories, we mostly cannot reject similar effect sizes across RCL states that had and had not decriminalized cannabis. These findings reveal–as expected–larger declines in cannabis arrests in states that had not yet decriminalized, although there was still scope for policy effects in states that had already decriminalized cannabis.

[14] TWFE DID is more likely to 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).

5.2 Prisoners

Having analyzed the effect of RCLs on the initial interactions between law enforcement and civilians with arrest data, we turn to the most downstream law enforcement outcome – incarceration. We analyze the flow of prisoners with admission rates and the stock with yearend rates per 10,000 persons. Figure 4 plots the rate of prisoners at yearend and the rate of prisoner admissions in RCL states in relative time. There is a downward trend in the rate of Non-Hispanic Black prisoners throughout this period, but not much movement following RCL implementation.

Table 4 reports the TWFE DID estimates. Panel A shows results for prisoner admissions for drug offenses (this includes cannabis and other drugs). We find a significant decline in admissions for White persons, that represents a 37% reduction relative to the baseline mean in RCL states. We do not find significant effects for Black prisoner admissions. The 95% confidence interval allows us to reject declines of 8% or larger. We estimate a significant 35% increase in absolute disparities in admissions for drug offenses for Black relative to White populations. Figure 5 shows event study plots for drug-related prisoner admissions, providing reassurance on our identification assumption and echoing the results in Table 4. Panel B complements these results by reporting prisoner admissions for non-drug offenses, with much smaller point estimates and without any significant effects for any group. Panel C estimates suggest that net prisoner admissions were insignificant for both groups. We next turn to the stock of prisoners at yearend. Coefficients for White and Black populations in Panel D are negative but statistically insignificant. Moreover, the implied magnitudes from the point estimates are small across measures.

Overall, estimates suggest that only White persons may have benefited from RCLs in terms of this downstream outcome, although we exercise caution in the interpretation of these results given the data limitations outlined previously. We also recognize that there are many other interactions with the criminal legal system between the time of arrest and the backend result of incarceration. We do not observe changes in prosecution (i.e., taking an arrestee to trial) or sentencing. Furthermore, Part 2 offenses are less likely to result in imprisonment greater than one year. Therefore, even with differential changes in arrests for less serious crimes, it is unlikely that we would observe the effects in prison statistics. Nevertheless, contact with the criminal legal system, even for less serious offenses, may disrupt human capital accumulation and labor market attachment, and therefore, increase racial disparities in economic outcomes (Dobbie et al., 2018).

5.2.1 Robustness Checks on Prisoner Data

We test the robustness of estimates for prisoner admissions for drug offenses in Appendix Figure S16. For White persons, the coefficients are all negative and statistically significant. For Black admissions, all estimates are positive but statistically insignificant. These checks provide reassurance that prisoner admissions for drug offenses indeed declined for White persons but not for Black.

6 Potential Mechanisms

Our main findings documented that RCLs led to significant declines in cannabis arrests that were offset by increases in arrests for less serious, non-drug Part 2 offenses, yielding null net effects in total arrests. We observed this pattern for both racial groups, although effects were statistically significant for Black populations only. Given that arrests are a function of both criminal activity and policing efforts, in this section we explore whether arrest increases can be explained by greater criminal activity following RCL implementation. We first explore outcomes related to the psychoactive effects of cannabis use. We next proxy for criminal activity and perceptions by analyzing calls for service and reported crimes. Lastly, we investigate homicides and hospitalizations as a proxy for systemic violence.

6.1 Psychoactive Effects

Increased cannabis use might contribute to the rise in Part 2 offenses, as the psychoactive effects of the drug could influence behavior related to criminal activity. We explore this mechanism with two measures: the number of hospitalizations involving cannabis use disorder per 10,000 persons, and the number of motor vehicle traffic deaths per 10,000 persons.[15]

[15] About 55.8% of injured or killed roadway users test positive for alcohol or drugs.

Table 5 reports TWFE DID estimates. Panel A shows significant increases of 21% and 22% in the rate of cannabis hospitalizations for White and Black persons, respectively. Panel B shows a significant 12% increase in motor vehicle deaths for White persons but an insignificant and smaller increase for Black persons at 5%. Corresponding event study plots are in Appendix Figures S7 and S8.

6.2 Criminal Activity and Perceptions

We next turn to criminal activity and perceptions using incident data on calls for service and reported crimes for select RCL cities, comparing the average number of incidents in minority neighborhoods relative to those in other neighborhoods within the city, before and after RCL implementation. Figure 6 plots total incidents (either calls or crimes) in a tract-quarter, distinguishing between minority and other neighborhoods. We document decreases in incidents in minority neighborhoods following RCL implementation in most cities. Notable exceptions include Detroit, which displays increases starting in 2020q2, and Sacramento, which does not appear to display a trend or level break following RCL implementation.

Table 6 reports TWFE DID estimates.[16] RCL implementation led to sizable declines in total incidents in minority neighborhoods relative to other neighborhoods across most cities. Declines in reported crimes in DC, Los Angeles, and Denver were, on average, 10% of the baseline mean. Declines in calls for service in Portland and Seattle were, on average, 17% of the baseline mean. Declines in Burlington were 3% of the baseline mean. Consistent with Figure 6, Detroit and Sacramento were notable exceptions.

The second and third columns of Table 6 report results for Part 1 and Part 2 offenses. We documented reductions in both classifications for minority neighborhoods across many cities, particularly for Part 1 offenses. Again, Detroit and Sacramento were notable exceptions.

[16] Some incidents included in "Total incidents" do not fall into neither Part 1 or Part 2 offenses (i.e. mental health crisis, wellness check, traffic incidents, illegal dumping, hazards). Moreover, due to differences in collection and reporting of incidents across cities, the types of Total, Part 1, or Part 2 incidents in a given city need not align with those in another city. Nevertheless, we did our best effort to categorize incidents in the most consistent way possible, and highlight notable differences in collection and reporting in Section E of the Appendix and in Table 6.

Appendix Table S7 considers select Part 2 offenses, and Table S8 shows that crime reductions in minority neighborhoods in LA were driven by Black persons.[17]

Overall, we interpret these results as indicative of general declines in calls for service and reported crimes in minority neighborhoods of the RCL cities analyzed. Two potential explanations for documented increases in Detroit starting in 2020q2 include social unrest fueled by the Covid-19 pandemic lockdowns in March of 2020 and the murder of George Floyd in May of 2020. As for Sacramento, this was the only city for which we were unable to separately identify civilian initiated calls from police initiated calls. Therefore, estimates reflect incidents reported by both types of callers.

Results suggest that criminal activity among Black populations (or the perception thereof) did not increase differentially following RCLs, and actually declined in many cases. This lack of increase holds for Part 1 and Part 2 offenses. Therefore, the increases in non-drug arrests, particularly for Black persons, do not appear to result from greater crime production.

6.3 Systemic Violence

We investigate homicides and assault hospitalizations, which may partially reflect systemic violence and complement previous analyses. Figure 7 plots total homicides and homicides involving gun injury. While homicide rates appeared unaffected for Non-Hispanic White persons following RCL implementation, there was a gradual decline for Non-Hispanic Black persons. Figure 8 displays similar patterns for assault hospitalizations.

Table 7 reports TWFE DID estimates of the effect of RCLs on homicides and assault hospitalizations per 10,000 persons. Panels A and B capture outcomes for homicides and homicides involving gun injury, respectively. We find a significant decline in the rate of total homicides and homicides involving gun injury among Non-Hispanic Black populations. Relative to the baseline mean in RCL states, this implies a sizable decline of 17% and of 21%, respectively. Estimates for Non-Hispanic White persons are a precisely estimated zero. Panels C and D capture total assault hospitalizations and assault hospitalizations involving gun injury, respectively. Point estimates are small and insignificant across measures and race groups. Figures 9 and 10 show event study plots for homicides and assault hospitalizations.

[17] LA is the only city that identifies the race of the victim in the data.

We test the robustness of baseline TWFE DID estimates for homicide rates in Appendix Figure S17. For Non-Hispanic White, coefficients are small and statistically insignificant regardless of our estimation approach. For Non-Hispanic Black, estimates are negative but lose statistical significance depending on the specification. Estimators that account for treatment effect heterogeneity yield a magnitude of around half the TWFE estimate. Additional inspection of the data suggests that CA and MA are largely driving homicide declines. Therefore, we exercise caution when interpreting these results.

Altogether, the estimates show that measures of systemic violence did not increase for either group following RCLs, suggesting that increases in non-drug arrests are unrelated to greater crime production. There is suggestive evidence that violence against and by Black persons declined - or at least, did not increase. This coincides with reductions in Part 1 offenses documented using calls for service and reported crimes data. [18]

7 Discussion

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 effects of cannabis legalization on racial disparities in the criminal legal system—namely, arrests and incarceration—and presents five key takeaways. First, RCL implementation led to substantial declines in arrests for cannabis possession and sales across Black and White persons. At baseline, the cannabis arrest rate of Black persons in RCL states was almost four times larger than that of White persons. With our estimated decrease after legalization, this relative disparity is maintained, although the absolute disparity declines without disappearing.

Second, declines in cannabis arrest rates did not translate into reductions in total arrests, with small and insignificant net effects in total arrests across all racial groups. For White persons, we cannot reject that the effect of RCLs on cannabis arrest rates is equal to the effect on total arrest rates. However, for Black persons the effects are significantly different.

[18] Given that the vast majority of homicides are intraracial (i.e., the perpetrator's and victim's races are the same), we interpret this as suggestive evidence that violent offenses by Black persons declined - or at least, did not increase.

Third, null net effects on total arrests cannot be explained by offsetting increases in arrests for other drug offenses. Not only do we not find increases in arrests for possession of other drugs for both groups, but we actually obtain significant *declines* in arrests for sales of other drugs. These patterns may be consistent with either declines in the size of the illegal drug market (i.e., legalization induced the market exit of illegal drug suppliers) or with reduced police monitoring of illegal drug market activities.

Fourth, null net effects on total arrests are explained by offsetting increases in non-drug arrests for both White and Black persons. Treatment effects, however, were largest and only statistically significant for Black persons. Offsetting increases were driven by less serious Part 2 offenses, which often involve more discretion from law enforcement (i.e., quality-of-life offenses). These findings suggest a reshuffling in arrests across crime categories.

Fifth, we shed light on potential mechanisms behind the offsetting increases in non-drug arrests. There are at least three mechanisms: (a) limitations in the recording of arrests in UCR data, (b) increases in criminal activity, and (c) policing behaviors and incentives. Regarding (a), increases in non-drug arrests may be mechanical due to the UCR data recording only the highest offense for each incident. Based on the 2018 National Incident-Based Reporting System (NIBRS), Appendix Table S3 shows that 36% of incidents involving a drug offense are multiple-offense incidents, consistent with Hendrix and Martin (2019). Within multiple-offense incidents, 75% only involve other drug violations. Therefore, the scope for reclassification is small. Back-of-the-envelope calculations would then suggest that at most 0.74 and 2.22 cannabis arrests per 10,000 persons would be reclassified as Part 2 offenses for White and Black populations, respectively.[19] However, we estimate an increase of 17 and 75 Part 2 arrests per 10,000 for the White and Black groups, respectively. This suggests that very little of these shifts toward low-level criminal offenses could be explained as purely mechanical in how arrests are tallied.

Regarding (b), our evidence suggests that findings are unlikely to be explained by increases in criminal activity, including differentially by race group. While we documented increases in cannabis use disorder hospitalizations and traffic fatalities, we did not docu-

[19] Appendix Table S3 shows that within incidents that involve at least one drug offense, 8.4% have at least one other offense classified as a Part 2 crime. Hence, at most this percent of the estimated declines in cannabis arrests could be reclassified as a Part 2 offense.

ment increases in criminal activity nor in systemic violence. Moreover, findings did not reflect differential increases in criminal activity for Black populations; in some instances, findings reflected differential declines for Black populations. Our findings align with previous studies of cannabis liberalization policies, most of which document declines, or at least no increases, in criminal activity in the general population (Dragone et al., 2019; Brinkman and Mok-Lamme, 2019; Morris et al., 2014; Huber III et al., 2016; Chu and Townsend, 2019; Gavrilova et al., 2019; Anderson and Rees, 2023; Zakrzewski Jr et al., 2020). Our findings suggest that Black neighborhoods are largely driving these declines. Taken together, this evidence is inconsistent with greater criminal activity, particularly among Black persons.

Lastly (c), results may be due to police incentives, such as enforcement quotas (target number of stops, tickets, or arrests per period), and police behaviors. One explanation for the increases in non-drug arrests, which were larger and stronger for Black populations, might be the continued profiling of Black persons and communities, reflecting lower policing costs, statistical discrimination, or taste-based discrimination. Targeted policing strategies, such as hot spot policing, in minority communities may result in more police contact despite a change in drug enforcement policies (Wheeler, 2020; Neil and MacDonald, 2023). RCLs are unlikely to have a large impact on long-standing policing strategies in the short-run. While our data does not allow to test for this mechanism, our findings raise the question of whether preexisting police incentives are such that RCLs lead to increases in arrests for low-level offenses, especially among Black populations. This potential explanation would align with previous studies of racial disparities in policing outcomes (West, 2018; Goncalves and Mello, 2021; Feigenberg and Miller, 2022; Chalfin et al., 2022).

Although our results are generally robust, there are limitations and open questions. First, there are not many available measures of criminal activity by race, which is why we rely on hospital and mortality data that identify race at the individual level, along with calls for service and reported crime data for select cities which identify latitude and longitude associated with an incident. Moreover, calls for service 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 may face. Third, we do not observe prosecutorial decisions after arrests are made. Lastly, since RCLs had only been adopted by 11 states up to 2019 (the last year in our data), our estimates may not generalize for states that have since implemented RCLs or other future RCLs. Notably, most states in our sample are liberal and have a low proportion of Black persons.

While cannabis legalization may be an important first step toward remediating the over-enforcement of drug prohibition and resulting racial disparities, our results illustrate that RCLs are not a silver bullet for reducing total arrests and total incarcerations across racial groups, nor for reducing longstanding racial disparities in these outcomes. Designing oversight mechanisms, policies, and provisions that are sensitive to racial disparities and that address law enforcement incentives and strategies—particularly in Black communities—may be crucial for overturning these disparities.[20] Making liberalization retroactive by granting clemency, overturning convictions, expunging cannabis arrest records, and bypassing habitual-offender laws, as well as ensuring that minority communities most affected by the War on Drugs have access to the economic benefits of cannabis legalization may be crucial for equitable RCL implementation.

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[20] For instance, not tying state and federal funding to low-level offense arrests that have predominantly targeted minority populations, and not using the raw number of arrests in a jurisdiction as a measure of policing efficacy (Owens and Ba, 2021).

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8 Figures

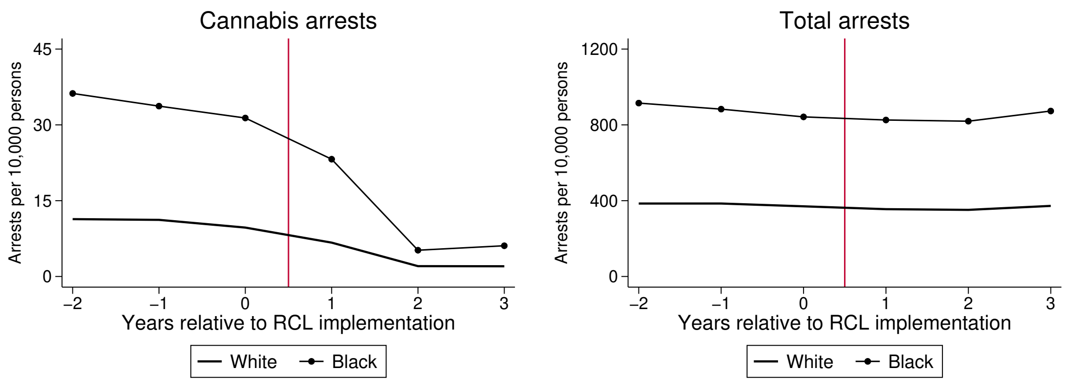

Figure 1: Arrest rates, raw plots

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 restricted to counties with an agency reporting coverage threshold above or equal to 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.

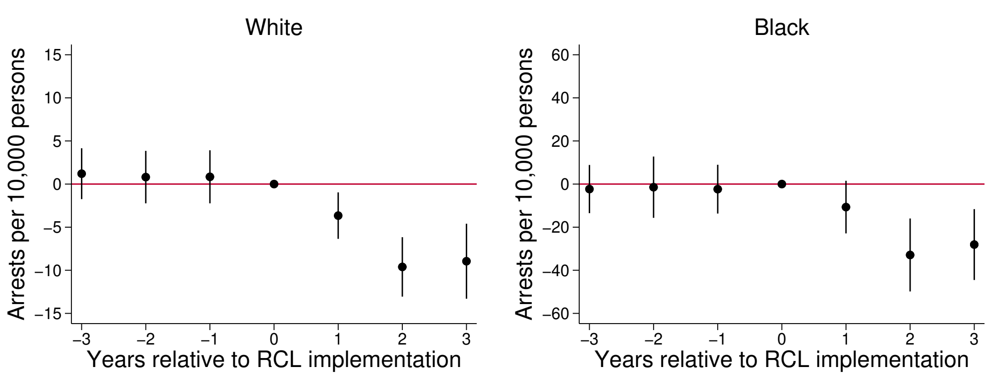

Figure 2: Cannabis arrest rates, event study plots

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 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 above or equal to 65%. 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 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.

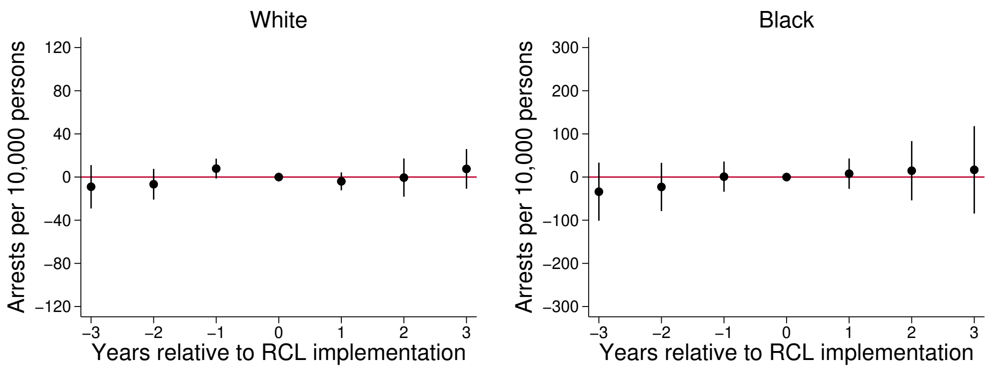

Figure 3: Total arrest rates, event study plots

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 above or equal to 65%. 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 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.

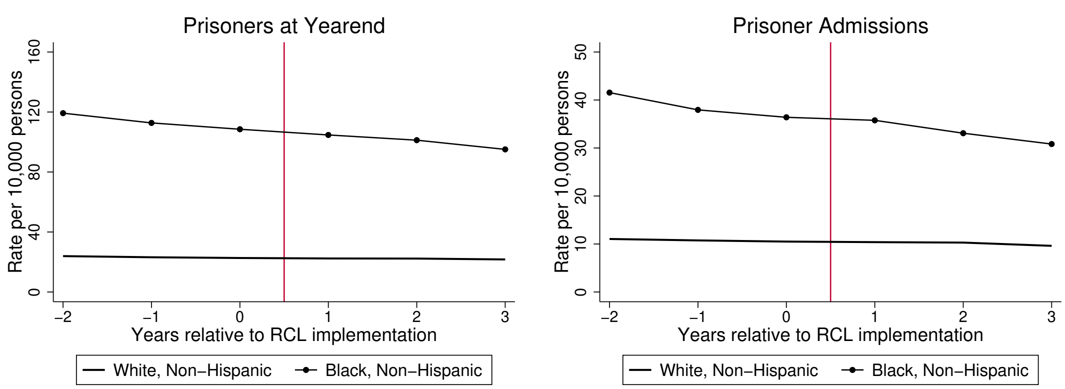

Figure 4: Prisoner rates, raw plots

Notes: Yearend prisoners are from the 2009-19 National Prisoner Statistics. Prisoner admissions are from the 2007-19 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. 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.

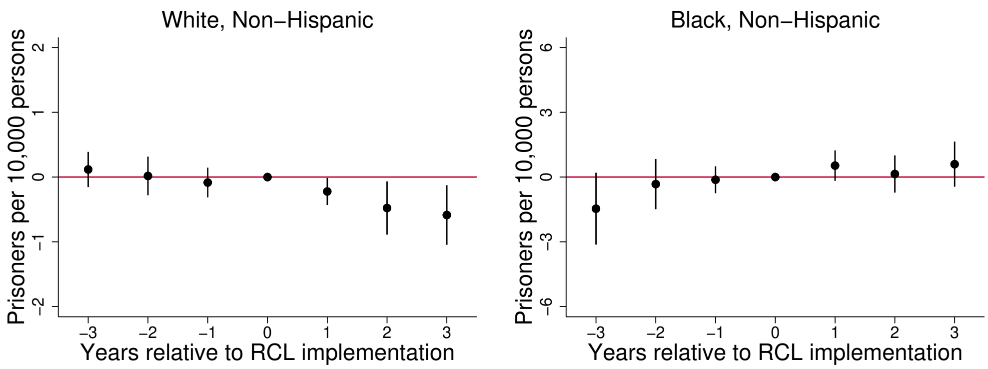

Figure 5: Prisoner admissions for drug offenses, event study plots

Notes: Prisoner admissions data 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.

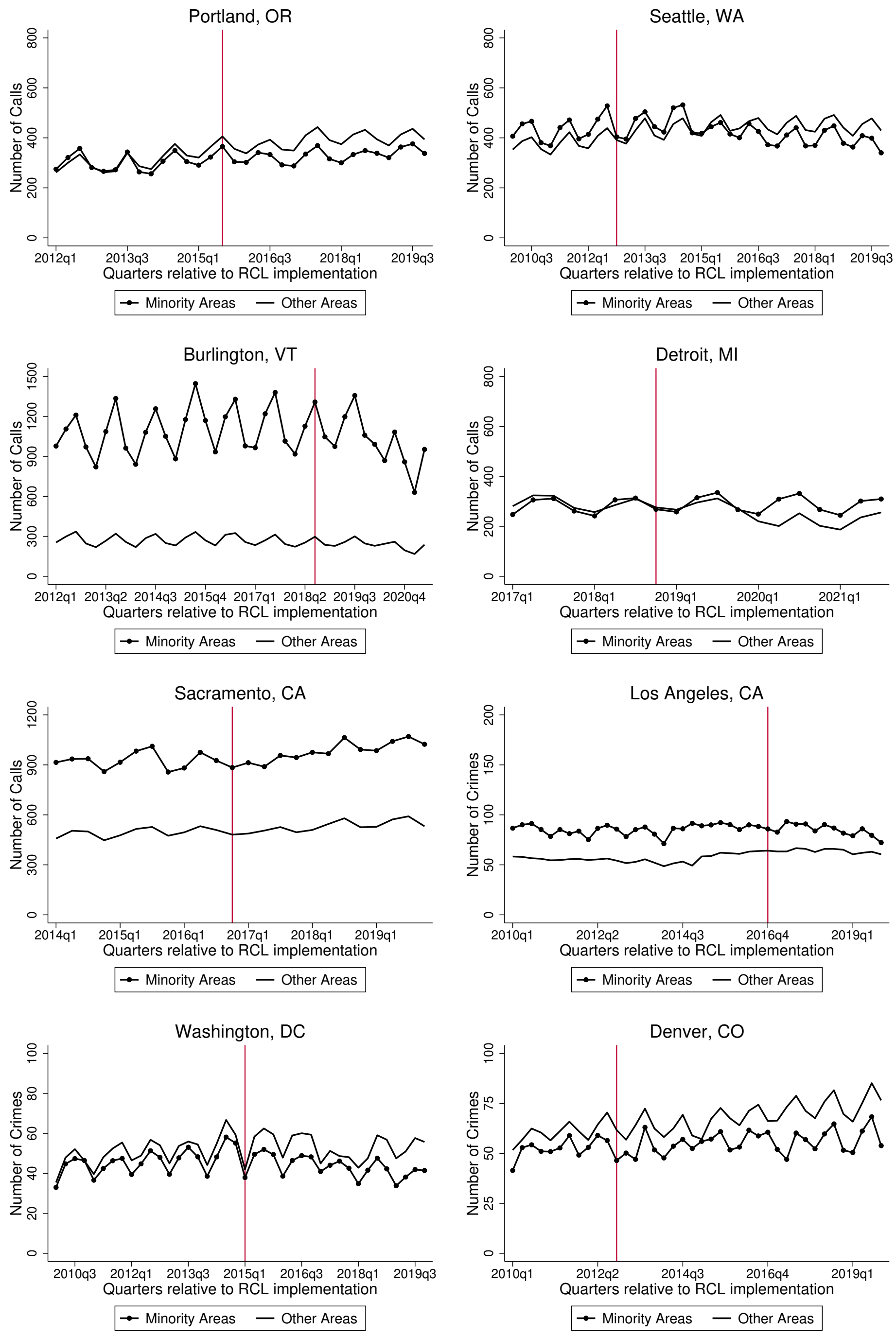

Figure 6: Criminal activity and perceptions in minority neighborhoods, raw plots

Notes: Calls for service or reported crime data in select RCL cities Portland, Seattle, Burlington, Los Angeles, Sacramento, Denver, DC, Detroit. The unit of analysis is a tract-quarter. Outcomes reflect total incident counts at the tract-quarter level. Details are in Section 3 of the manuscript and Section E of the appendix.

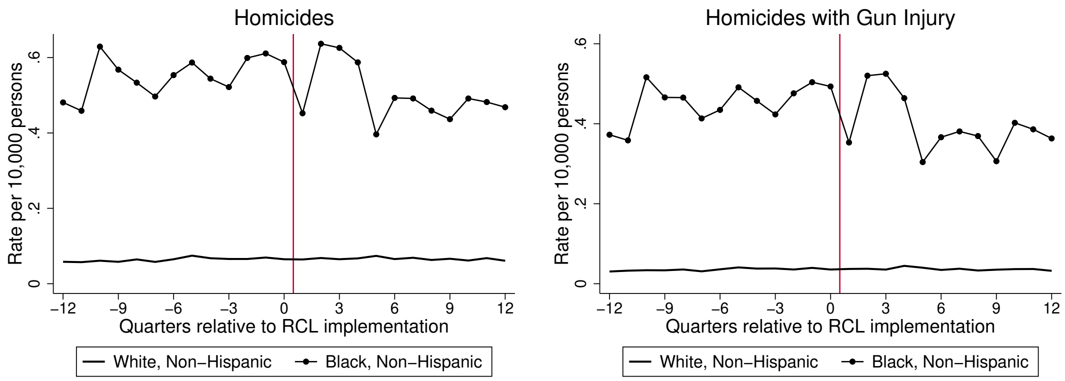

Figure 7: Homicide rates, raw plots

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. RCL=Recreational cannabis laws.

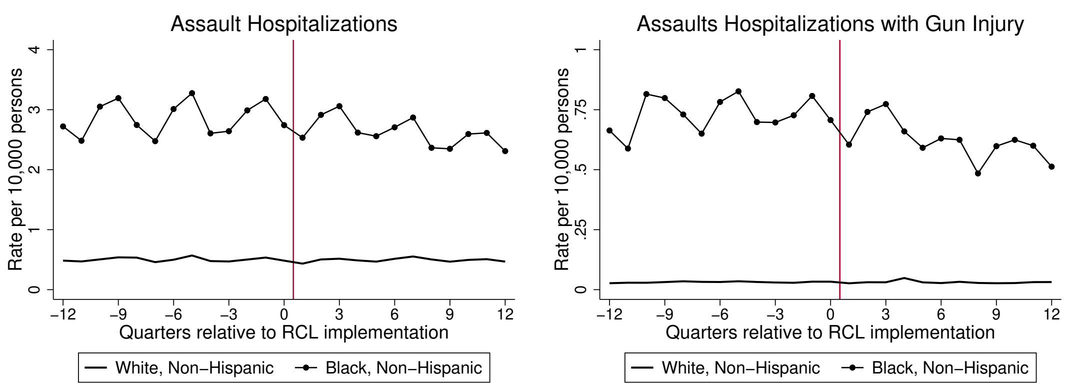

Figure 8: Hospitalization rates, raw plots

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. RCL=Recreational cannabis laws.

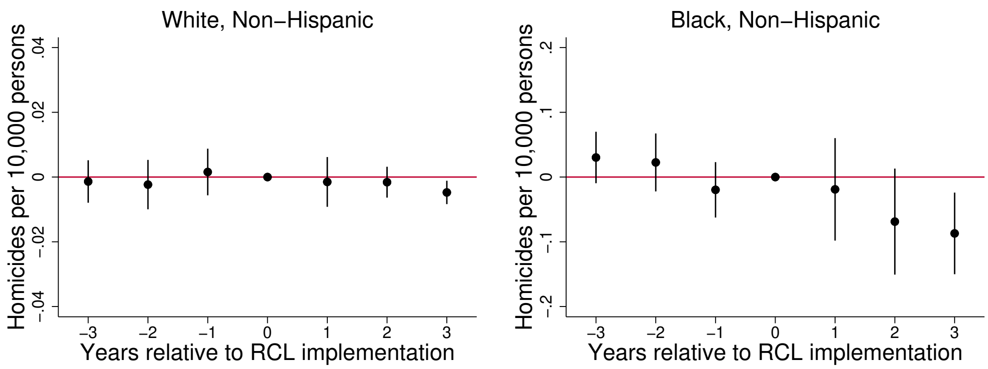

Figure 9: Homicide rates, event study plots

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.

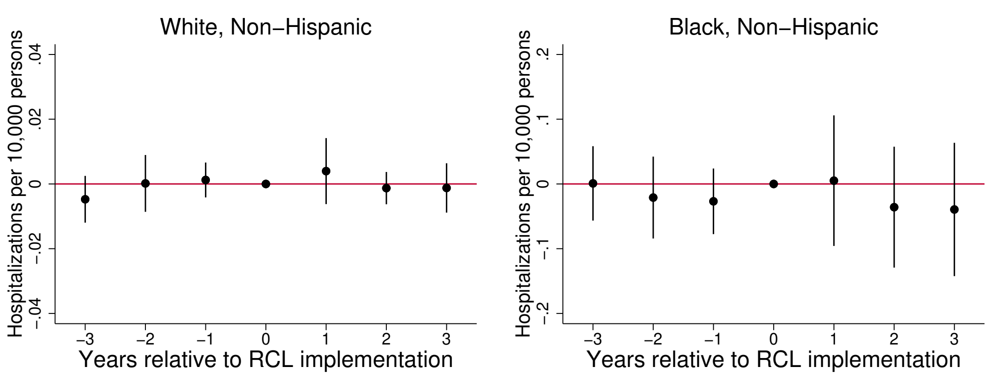

Figure 10: Assault hospitalizations with gun injury, event study plots

Notes: Hospital data are from the 2007-2019 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. Coefficients and 95% confidence intervals clustered at the state level are based on an event study approach that estimates leads and lags of the intervention and that accounts for controls (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.

9 Tables

Table 1: Effect of recreational cannabis laws on cannabis and total arrests

	Population	White	Black		
	Rate	Rate	Rate	Rate Ratio	Rate Difference
Panel A: Cannabis arrests per 10,000 persons					
RCL	-9.14***	-8.84***	-26.43***	0.04	-20.07***
	(2.84)	(2.43)	(9.18)	(0.22)	(7.32)
Mean	16.66	15.11	53.02	3.81	39.01
N	30524	30524	30524	29265	30524
Panel B: Cannabis possession arrests per 10,000 persons					
RCL	-7.75***	-7.55***	-19.46**	0.16	-14.55**
	(2.86)	(2.52)	(8.99)	(0.18)	(7.12)
Mean	13.12	12.21	38.82	3.19	27.69
N	30524	30524	30524	28942	30524
Panel C: Cannabis sales arrests per 10,000 persons					
RCL	-1.39***	-1.29***	-6.97***	-0.75	-5.52***
	(0.30)	(0.28)	(1.20)	(0.52)	(0.96)
Mean	3.54	2.90	14.19	5.19	11.32
N	30524	30524	30524	22497	30524
Panel D: Total arrests per 10,000 persons					
RCL	19.28	7.81	45.99	0.04	36.35
	(13.03)	(11.96)	(39.92)	(0.07)	(29.59)
Mean	418.12	409.00	989.70	2.78	619.08
N	30524	30524	30524	30450	30524

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 ratios, and rate differences, by race. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Rate ratios and rate differences are relative to the White group. Sample restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each coefficient is based on 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 clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: Effect of recreational cannabis laws on other drug arrests

	Population	White	Black		
	Rate	Rate	Rate	Rate Ratio	Rate Difference
Panel A: Other drug arrests per 10,000 persons					
RCL	-0.64	-0.97	-5.79	0.22	-6.15
	(1.60)	(2.02)	(4.06)	(0.17)	(4.37)
Mean	46.14	46.29	103.24	2.96	59.61
N	30524	30524	30524	29312	30524
Panel B: Other drug possession arrests per 10,000 persons					
RCL	0.44	0.46	-1.07	0.26*	-2.45
	(1.50)	(1.99)	(2.90)	(0.13)	(3.47)
Mean	38.65	39.77	76.22	2.42	39.06
N	30524	30524	30524	28879	30524
Panel C: Other drug sales arrests per 10,000 persons					
RCL	-1.08***	-1.44***	-4.72***	-0.12	-3.69***
	(0.33)	(0.26)	(1.57)	(0.23)	(1.33)
Mean	7.48	6.52	27.02	5.28	20.55
N	30524	30524	30524	25189	30524

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 ratios, and rate differences, by race. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Rate ratios and rate differences are relative to the White group. Sample restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each coefficient is based on 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 clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Effect of recreational cannabis laws on non-drug arrests

	Population	White	Black		
	Rate	Rate	Rate	Rate Ratio	Rate Difference
Panel A: Non-drug arrests per 10,000 persons					
RCL	29.05**	17.63	78.21**	0.10	62.56**
	(13.38)	(12.49)	(37.87)	(0.07)	(26.91)
Mean	355.31	347.60	833.45	2.76	520.46
N	30524	30524	30524	30449	30524
Panel B: Part 1 crime arrests per 10,000 persons					
RCL	2.14	0.64	3.33	0.11	4.21
	(2.01)	(1.81)	(6.82)	(0.09)	(5.05)
Mean	72.65	67.78	205.28	3.37	139.86
N	30524	30524	30524	30155	30524
Panel C: Part 2 crime arrests per 10,000 persons					
RCL	26.91**	16.99	74.87**	0.10	58.35**
	(12.49)	(11.70)	(34.46)	(0.07)	(24.45)
Mean	282.67	279.82	628.17	2.60	380.60
N	30524	30524	30524	30447	30524
Panel D: Quality-of-life Part 2 crime arrests per 10,000 persons					
RCL	22.84**	14.98	60.38**	0.12	45.65**
	(10.58)	(10.03)	(27.55)	(0.12)	(21.34)
Mean	165.06	161.17	390.93	2.85	252.98
N	30524	30524	30524	30406	30524
Panel E: Other Part 2 crime arrests per 10,000 persons					
RCL	4.07	2.01	14.49	0.02	12.70*
	(3.38)	(3.22)	(10.51)	(0.06)	(6.50)
Mean	117.61	118.65	237.24	2.28	127.62
N	30524	30524	30524	30436	30524

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 ratios, and rate differences, by race. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Rate ratios and rate differences are relative to the White group. Sample restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each coefficient is based on 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 clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Effect of recreational cannabis laws on prisoners

	Population	White, NH	Black, NH		
	Rate	Rate	Rate	Rate Ratio	Rate Difference
Panel A: Prisoner admissions for drug offenses per 10,000 persons					
RCL	0.07	-0.62***	1.7	0.04	2.25*
	(0.26)	(0.21)	(1.21)	(0.72)	(1.19)
Mean	2.33	1.68	8.02	6.62	6.39
N	479	477	477	477	477
Panel B: Prisoner admissions for non-drug offenses per 10,000 persons					
RCL	-0.06	-0.36	0.27	-0.04	0.58
	(1.76)	(1.23)	(2.94)	(0.26)	(2.00)
Mean	14.27	10.54	36.07	4.31	26.20
N	479	477	477	477	477
Panel C: Prisoner admissions per 10,000 persons					
RCL	0.01	-0.98	1.98	0.11	2.83
	(1.93)	(1.37)	(3.72)	(0.25)	(2.88)
Mean	16.60	12.22	44.09	4.66	32.60
N	479	477	477	477	477
Panel D: Prisoners at yearend per 10,000 persons					
RCL	0.51	-1.24	-2.41	-0.09	-1.58
	(1.37)	(0.89)	(6.22)	(0.16)	(5.81)
Mean	30.59	22.92	113.39	5.72	90.96
N	517	517	517	517	517

Notes: Yearend prisoner data are from the 2009-2019 National Prisoner Statistics. Prisoner admissions data are from the 2007-2019 National Corrections Reporting Program. The unit of analysis is a state-year. Effect of recreational cannabis laws on prisoner rates, rate ratios, and rate differences, by racial group. 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 (see 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.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Effect of recreational cannabis laws on psychoactive effects of cannabis use

	Population	White, NH	Black, NH		
	Rate	Rate	Rate	Rate Ratio	Rate Difference
Panel A: Cannabis use disorder hospitalizations per 10,000 persons					
RCL	0.465	0.963**	3.013**	0.019	1.849*
	(0.363)	(0.436)	(1.465)	(0.152)	(1.044)
Mean	4.715	4.610	13.894	2.931	8.969
N	1460	1448	1448	1448	1448
Panel B: Motor vehicle traffic deaths per 10,000 persons					
RCL	0.024***	0.029***	0.013	0.012	-0.015
	(0.007)	(0.007)	(0.027)	(0.076)	(0.023)
Mean	0.23	0.24	0.25	1.08	0.01
N	2652	2652	2652	2652	2652

Notes: Hospital data are from the 2007-2019 HCUP State Inpatient Databases. Death data are from the 2007-2019 NVSS Mortality Files. The unit of analysis is a state-year-quarter. Effect of recreational cannabis laws on the rate of outcomes that may reflect the psychoactive effects of cannabis use, by racial group. Counts for a given race are divided by state-year population estimates corresponding to that 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 include state and year-quarter fixed effects. Control variables include cannabis decriminalization laws. Standard errors clustered at the state level are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Effect of recreational cannabis laws on criminal activity and perceptions in minority neighborhoods

	Total Incidents	Part 1 Incidents	Part 2 Incidents
Panel A: Portland, OR			
Civilian initiated emergency and non-emergency calls for service			
RCL×Minority	-51.39***	-4.56	-24.95***
	(18.79)	(3.28)	(8.36)
Mean	300.81	51.92	136.09
Panel B: Seattle, WA			
Civilian initiated emergency and non-emergency calls for service			
RCL×Minority	-74.69***	-18.76***	-39.99***
	(13.56)	(14.85)	(22.76)
Mean	436.70	93.17	212.69
Panel C: Burlington, VT			
Civilian initiated emergency and non-emergency calls for service			
RCL×Minority	-36.62**	-19.16	1.05
	(68.85)	(13.22)	(38.73)
Mean	1093.46	82.23	389.35
Panel D: Detroit, MI			
Civilian initiated emergency calls for service			
RCL×Minority	49.49**	8.50*	16.93***
	(20.74)	(4.40)	(5.92)
Mean	283.79	77.93	88.40
Panel E: Sacramento, CA			
Civilian and police initiated emergency and non-emergency calls for service			
RCL×Minority	15.69	-1.48	22.02
	(64.75)	(5.32)	(39.29)
Mean	927.01	75.42	376.64
Panel F: Washington, DC			
Reported crimes			
RCL×Minority	-4.55**	-4.55**	n.a.
	(1.83)	(1.83)	n.a.
Mean	45.82	45.82	n.a.
Panel G: Los Angeles, CA			
Reported crimes			
RCL×Minority	-8.49***	-8.22***	-0.83
	(2.30)	(2.11)	(0.57)
Mean	85.63	68.57	21.26
Panel H: Denver, CO			
Reported crimes			
RCL×Minority	-5.36*	-4.51*	-1.34
	(2.98)	(2.34)	(1.06)
Mean	52.69	37.26	16.19

Notes: Calls for service or reported crime data in select RCL cities Portland, Seattle, Los Angeles, Sacramento, Denver, DC, Detroit. The unit of analysis is a tract-quarter. Outcomes reflect total incident counts at the tract-quarter level. Details are in Section 3 of the manuscript and Section E of the appendix. Standard errors clustered at the tract level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Effect of recreational cannabis laws on violence

	Population	White, NH	Black, NH		
	Rate	Rate	Rate	Rate Ratio	Rate Difference
Panel A: Homicides per 10,000 persons					
RCL	-0.014	0.001	-0.105**	-1.045	-0.104**
	(0.009)	(0.004)	(0.045)	(0.738)	(0.043)
Mean	0.13	0.06	0.62	10.90	0.56
N	2652	2652	2652	2606	2652
Panel B: Homicides involving gun injury per 10,000 persons					
RCL	-0.015**	-0.000	-0.109**	-1.027	-0.107**
	(0.008)	(0.003)	(0.044)	(1.505)	(0.043)
Mean	0.09	0.03	0.51	17.89	0.47
N	2652	2652	2652	2606	2652
Panel C: Assault hospitalizations per 10,000 persons					
RCL	0.028	0.004	0.022	-0.047	0.016
	(0.039)	(0.022)	(0.167)	(0.302)	(0.149)
Mean	0.891	0.543	3.166	6.343	2.605
N	1460	1448	1448	1448	1448
Panel D: Assault hospitalizations involving gun injury per 10,000 persons					
RCL	0.002	0.002	-0.053	-1.766	-0.055
	(0.009)	(0.003)	(0.046)	(2.495)	(0.044)
Mean	0.152	0.034	0.897	30.989	0.861
N	1460	1448	1448	1396	1448

Notes: Homicide data are from 2007-2019 NVSS Mortality files. Hospital data are from the 2007-2019 HCUP State Inpatient Databases. The unit of analysis is a state-year-quarter. Effect of recreational cannabis laws on homicide rates, rate ratios, and rate differences, by racial group. Outcomes include total homicides and homicides involving gun injuries. 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 (see Equation 1). Regressions are weighted by race-specific population. All regressions include state and year-quarter fixed effects. Control variables include cannabis decriminalization laws. Standard errors clustered at the state level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Supplementary Materials

A Effective Dates

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.

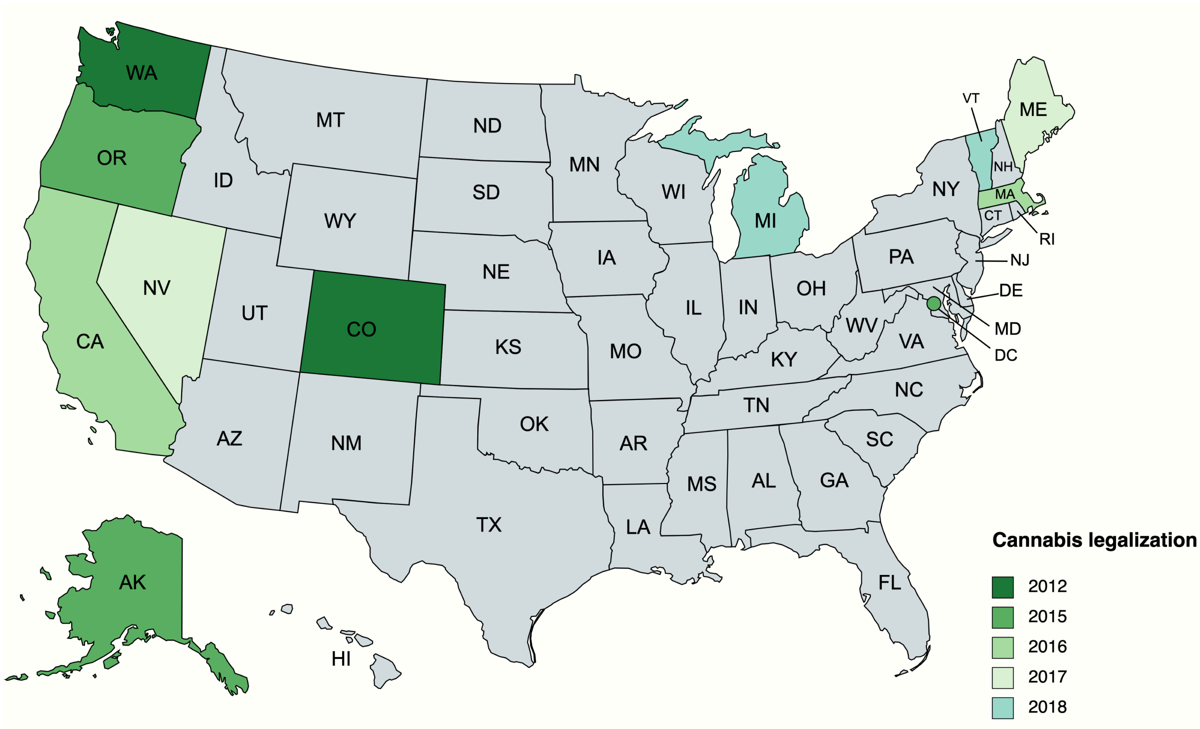

Figure S1: Implementation of recreational cannabis laws by state

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

B Event Study Plots

B.1 Law Enforcement

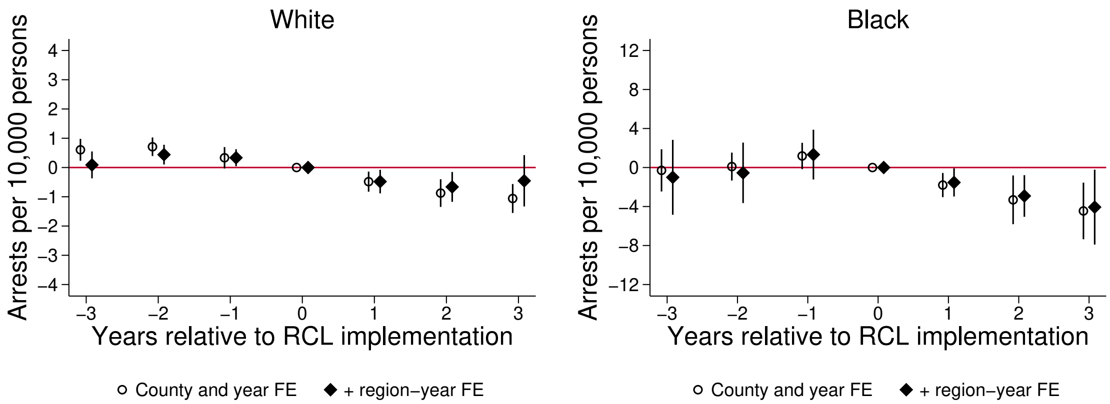

Figure S2: Other drug sales arrest rates, event study plots

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 above or equal to 65%. Coefficients and 95% confidence intervals clustered at the state level are based on an event study approach (Equation 2). Hollow markers denote the base specification; solid markers also include region-by-year fixed effects. 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.

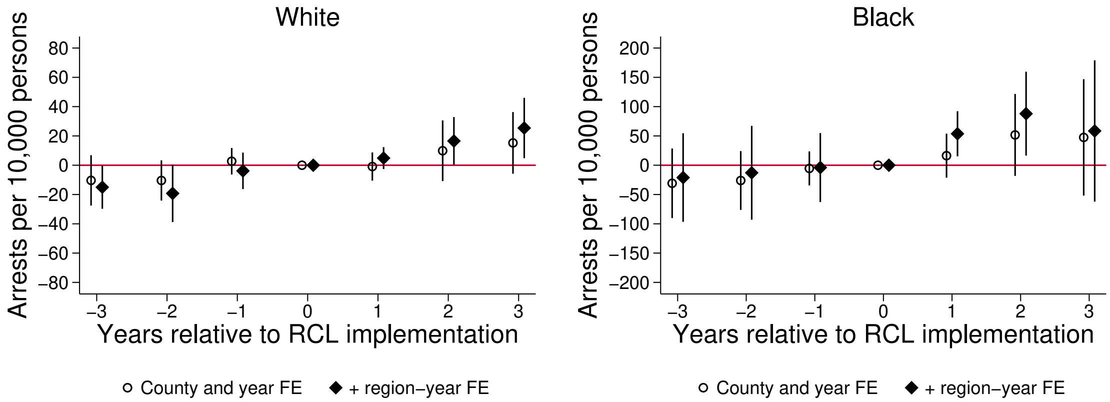

Figure S3: Non-drug arrest rates, event study plots

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 above or equal to 65%. Coefficients and 95% confidence intervals clustered at the state level are based on an event study approach (Equation 2). Hollow markers denote the base specification; solid markers also include region-by-year fixed effects. 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.

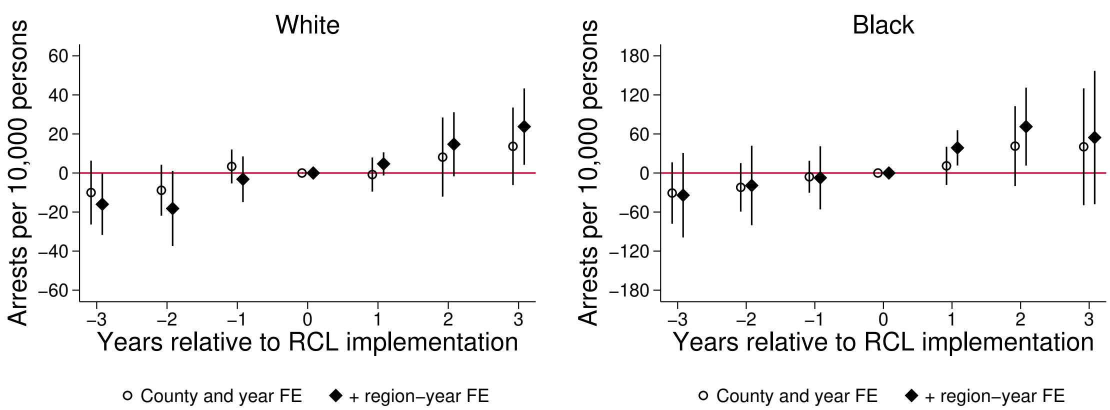

Figure S4: Part 2 crimes arrest rates, event study plots

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 above or equal to 65%. Coefficients and 95% confidence intervals clustered at the state level are based on an event study approach (Equation 2). Hollow markers denote the base specification; solid markers also include region-by-year fixed effects. 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.

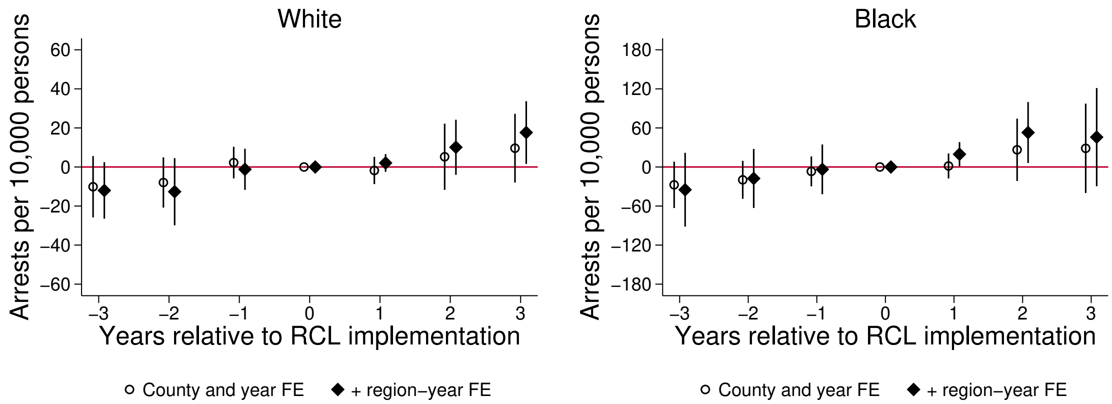

Figure S5: Quality-of-life arrest rates, event study plots

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. Quality-of-life arrest categories are all Part 2 crimes and include: drunkenness, liquor violations, disorderly conduct, gambling, suspicious behavior, vandalism, vagrancy, and uncategorized arrests. 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 above or equal to 65%. Coefficients and 95% confidence intervals clustered at the state level are based on an event study approach (Equation 2). Hollow markers denote the base specification; solid markers also include region-by-year fixed effects. 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.

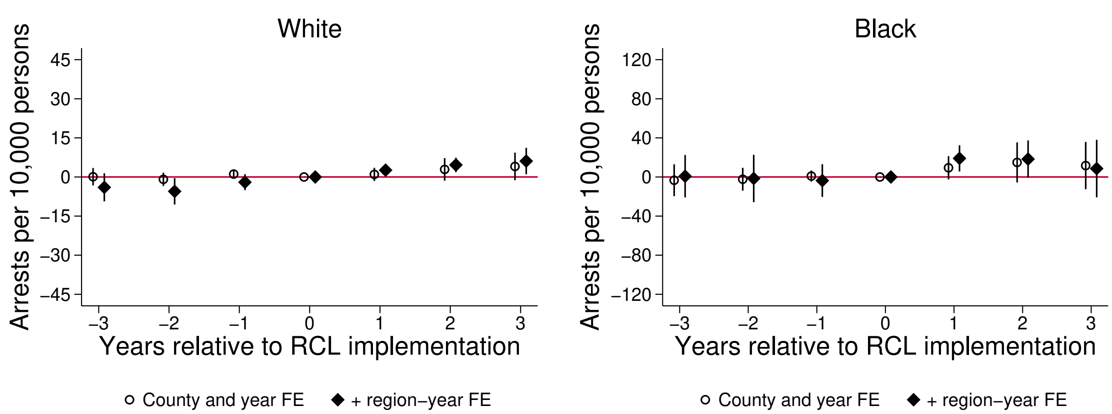

Figure S6: Other Part 2 crime arrest rates, event study plots

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, restricting to adults only. Other Part 2 crimes include: driving under the influence, embezzlement, family offenses, forgery, fraud, prostitution, other sex offenses, simple assault, stolen property, and weapons violations. 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 above or equal to 65%. Coefficients and 95% confidence intervals clustered at the state level are based on an event study approach (Equation 2). Hollow markers denote the base specification; solid markers also include region-by-year fixed effects. 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.

B.2 Psychoactive Effects

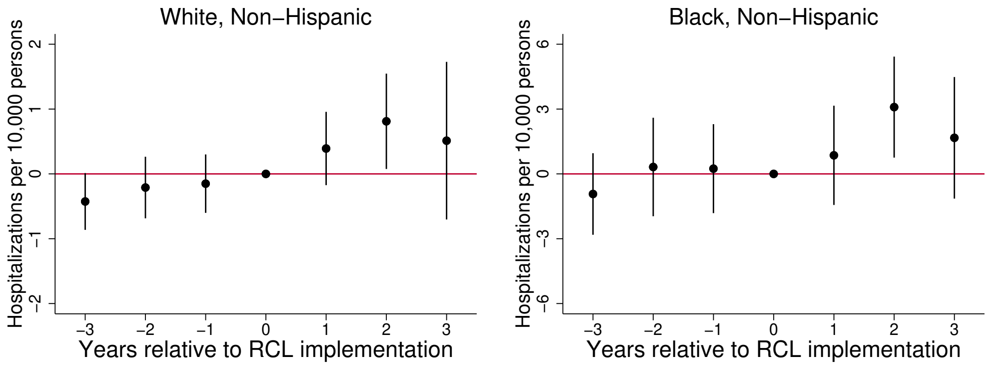

Figure S7: Cannabis use hospitalization rates, event study plots

Notes: Hospital data are from the 2007-2019 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. 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.

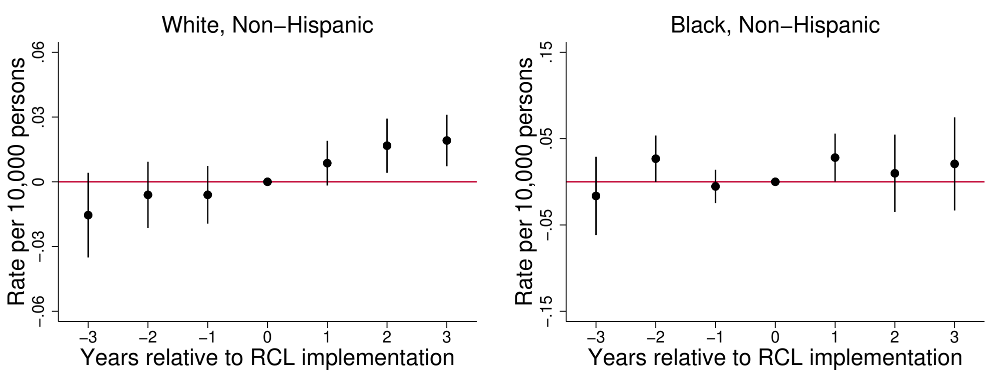

Figure S8: Motor vehicle traffic fatality rates, event study plots

Notes: Traffic fatality 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.

C Robustness Checks

Table S2: Diagnostic test of percentage and sum of negative weights

Rates per 10,000 persons	Percentage	Sum
Cannabis Arrests	1.6%	-0.003
Other drug Arrests	1.6%	-0.003
Non-drug Arrests	1.6%	-0.003
Total Arrests	1.6%	-0.003
Prisoners at Yearend	0%	0
Prisoners Admissions	0%	0
Homicide Deaths	0%	0
Assault Hospitalizations	0%	0

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 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.

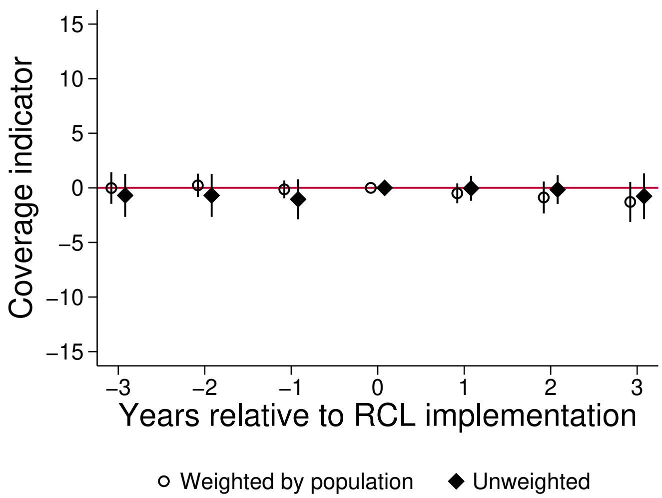

Figure S9: Arrests coverage indicator

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. 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. 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.

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/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.

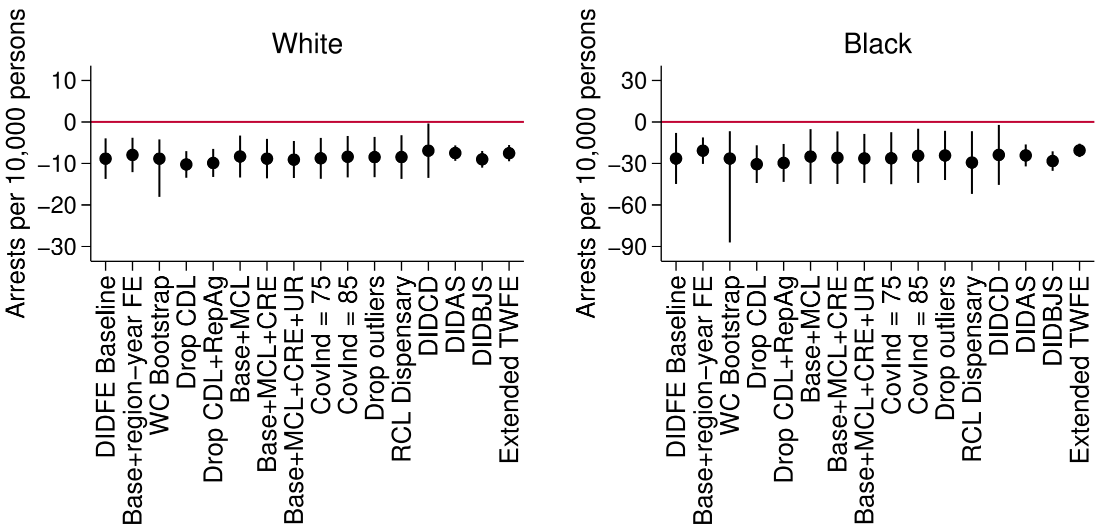

Figure S10: Cannabis arrest rates, robustness checks

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) above or equal to 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 (2022) 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.

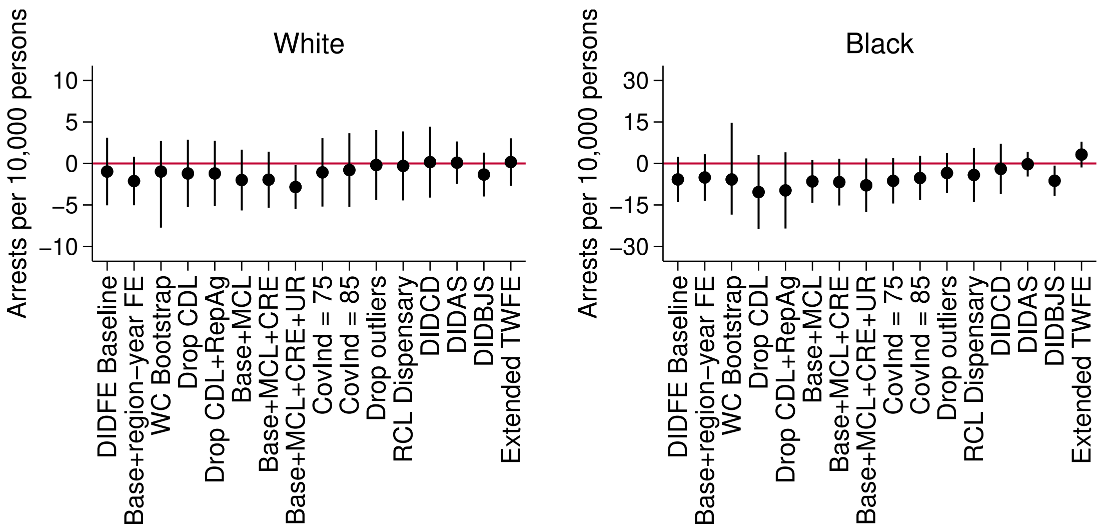

Figure S11: Other drug arrest rates, robustness checks

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) above or equal to 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 (2022) 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.

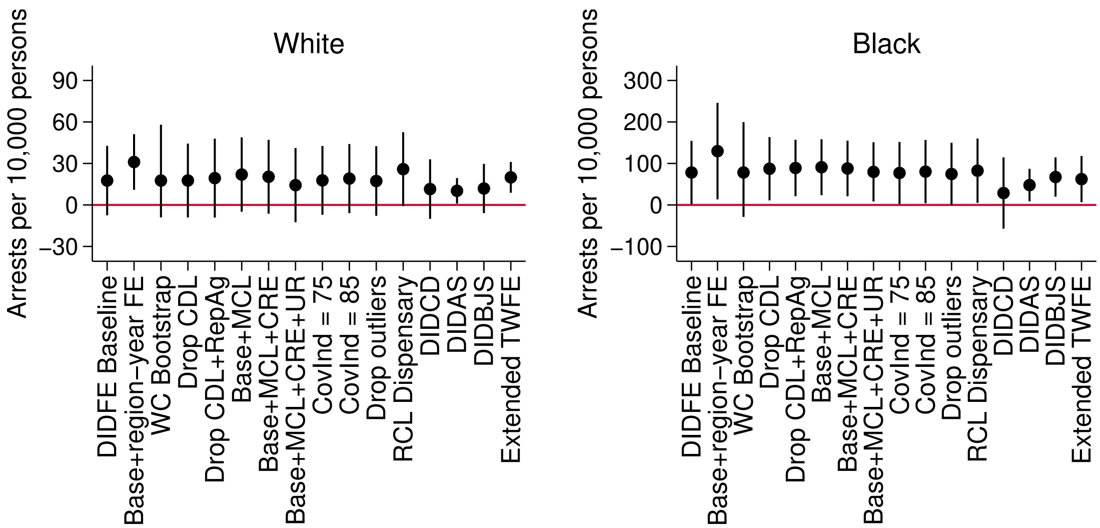

Figure S12: Non-drug arrest rates, robustness checks

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) above or equal to 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 (2022) 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.

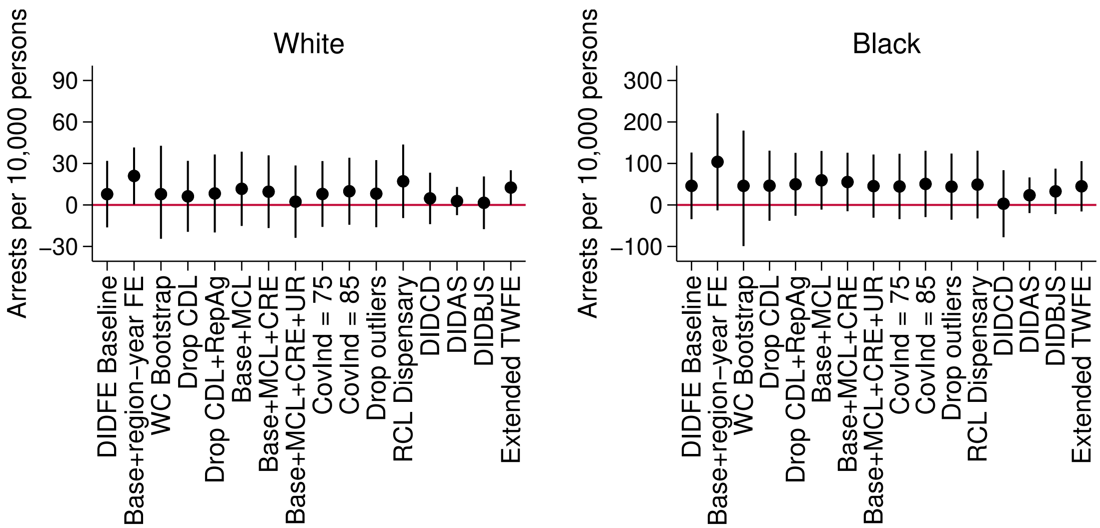

Figure S13: Total arrest rates, robustness checks

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) above or equal to 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 (2022) 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.

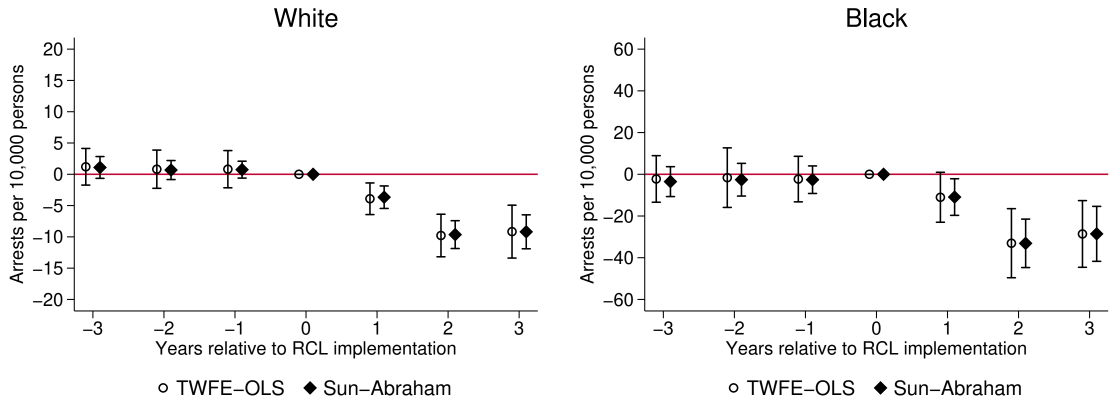

Figure S14: Cannabis arrest rates, alternative DID estimator

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 above or equal to 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.

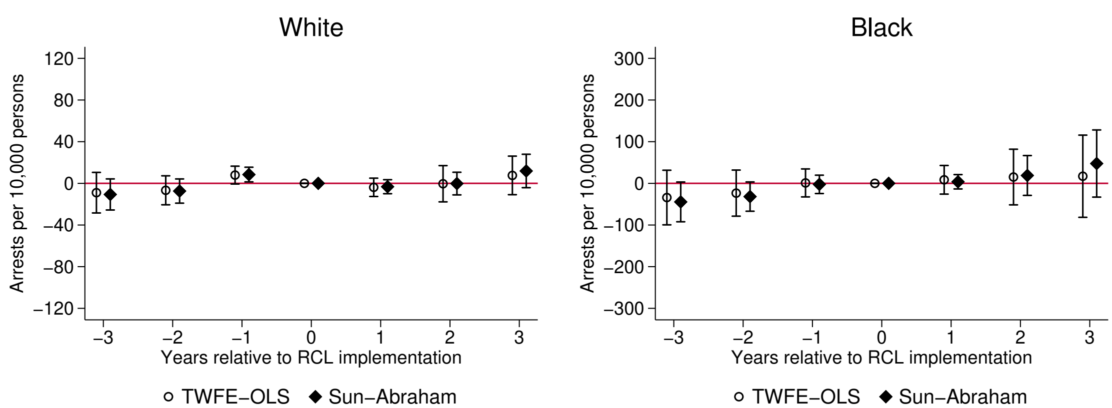

Figure S15: Total arrest rates, alternative DID estimator

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 above or equal to 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.

Table S4: Effect of recreational cannabis laws on arrests, excluding each RCL state at a time

Excluded state:	AK	CA	CO	MA	ME	MI	NV	OR	VT	WA
Panel A: Cannabis arrests per 10,000 persons										
Population	-9.069***	-13.536***	-8.842***	-9.831***	-8.911***	-8.632***	-8.477***	-8.463***	-9.185***	-8.563***
	(2.852)	(2.866)	(2.964)	(3.333)	(2.790)	(2.873)	(2.662)	(2.699)	(2.872)	(2.829)
White	-8.790***	-11.998***	-8.495***	-9.622***	-8.578***	-8.727***	-8.466***	-8.022***	-8.900***	-8.223***
	(2.441)	(2.630)	(2.529)	(2.891)	(2.373)	(2.622)	(2.382)	(2.193)	(2.462)	(2.387)
Black	-26.472***	-37.324***	-26.346***	-29.011***	-26.276***	-24.435**	-21.751***	-26.109***	-26.450***	-25.558***
	(9.249)	(11.785)	(9.639)	(10.774)	(9.155)	(10.105)	(6.967)	(9.257)	(9.193)	(9.206)
Panel B: Other drug arrests per 10,000 persons										
Population	-0.575	-2.489	-1.120	0.090	-0.505	-0.187	-0.641	-0.901	-0.622	-0.325
	(1.606)	(2.021)	(1.792)	(1.389)	(1.592)	(1.588)	(1.653)	(1.715)	(1.612)	(1.651)
White	-0.892	-4.312**	-1.276	-0.075	-0.784	-0.588	-0.903	-1.202	-0.958	-0.342
	(2.022)	(1.930)	(2.336)	(1.685)	(1.998)	(2.068)	(2.080)	(2.213)	(2.034)	(1.951)
Black	-5.814	1.220	-7.392*	-5.995	-5.600	-6.092	-7.006*	-5.416	-5.810	-6.062
	(4.071)	(4.578)	(3.690)	(4.222)	(4.091)	(4.563)	(3.855)	(4.147)	(4.048)	(4.114)
Panel C: Non-drug arrests per 10,000 persons										
Population	30.834**	45.303**	24.359*	27.930**	29.318**	28.582**	29.170**	23.783**	28.827**	33.245**
	(13.649)	(17.520)	(12.446)	(13.813)	(13.551)	(13.849)	(13.650)	(11.760)	(13.354)	(13.957)
White	19.092	32.769*	12.903	16.345	18.013	16.664	17.870	12.183	17.313	22.253*
	(12.683)	(16.516)	(11.759)	(12.991)	(12.708)	(12.907)	(12.734)	(10.922)	(12.453)	(12.951)
Black	80.005**	125.865***	69.910*	74.875*	79.400**	62.803*	79.171*	70.581*	78.002**	87.053**
	(38.287)	(40.729)	(36.888)	(39.439)	(38.130)	(36.663)	(39.595)	(36.299)	(37.871)	(39.294)
Panel D: Total arrests per 10,000 persons										
Population	21.189	29.278	14.397	18.189	19.902	19.763	20.052	14.419	19.020	24.356*
	(13.135)	(18.933)	(12.375)	(13.463)	(13.168)	(13.603)	(13.289)	(12.038)	(13.030)	(13.005)
White	9.410	16.459	3.133	6.648	8.651	7.349	8.501	2.959	7.455	13.687
	(11.994)	(17.903)	(11.611)	(12.543)	(12.102)	(12.484)	(12.146)	(11.153)	(11.964)	(11.381)
Black	47.719	89.760**	36.172	39.869	47.524	32.276	50.414	39.056	45.742	55.432
	(40.224)	(44.256)	(39.027)	(40.509)	(40.123)	(41.082)	(41.743)	(39.043)	(39.933)	(40.883)

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 above or equal to 65%. Each coefficient is based on separate two-way fixed effects regression (Equation 1). Each column excludes one state that passed a recreational cannabis law from the estimation 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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

Panel A: Cannabis arrests per 10,000 persons					
Population	-9.137***	-8.640***	-7.339**	-8.396***	-7.256**
	(2.841)	(2.933)	(2.879)	(2.899)	(2.883)
White	-8.842***	-8.279***	-7.322***	-8.027***	-7.209***
	(2.430)	(2.515)	(2.478)	(2.497)	(2.483)
Black	-26.434***	-25.944**	-21.810**	-23.981**	-21.038**
	(9.179)	(9.900)	(9.650)	(9.749)	(9.697)
Panel B: Other drug arrests per 10,000 persons					
Population	-0.635	-1.320	-1.554	-1.208	-1.552
	(1.603)	(1.459)	(1.543)	(1.492)	(1.548)
White	-0.973	-1.521	-1.951	-1.538	-1.980
	(2.025)	(1.997)	(2.070)	(2.015)	(2.076)
Black	-5.786	-6.925*	-6.428*	-3.921	-5.118
	(4.055)	(4.121)	(3.404)	(4.635)	(3.569)
Panel C: Non-drug arrests per 10,000 persons					
Population	29.047**	29.678**	32.978**	31.430**	33.704**
	(13.384)	(14.167)	(16.164)	(14.186)	(16.117)
White	17.627	17.713	16.978	19.142	17.819
	(12.492)	(13.107)	(15.134)	(13.207)	(15.102)
Black	78.208**	78.864**	100.256***	90.363**	104.673***
	(37.872)	(38.966)	(37.110)	(37.765)	(36.916)
Panel D: Total arrests per 10,000 persons					
Population	19.275	19.717	24.085	21.826	24.896
	(13.032)	(14.028)	(16.275)	(14.061)	(16.239)
White	7.812	7.913	7.706	9.578	8.631
	(11.956)	(12.924)	(15.264)	(13.033)	(15.253)
Black	45.988	45.995	72.019*	62.461	78.517**
	(39.922)	(41.592)	(37.596)	(40.198)	(37.419)
Controls for spillovers:					
RCL within 0-100 miles	No	Yes	Yes	No	Yes
RCL within 100-200 miles	No	No	Yes	No	Yes
Inv. dist. to nearest RCL	No	No	No	Yes	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 restricted to counties with an agency reporting coverage threshold above or equal to 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. 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

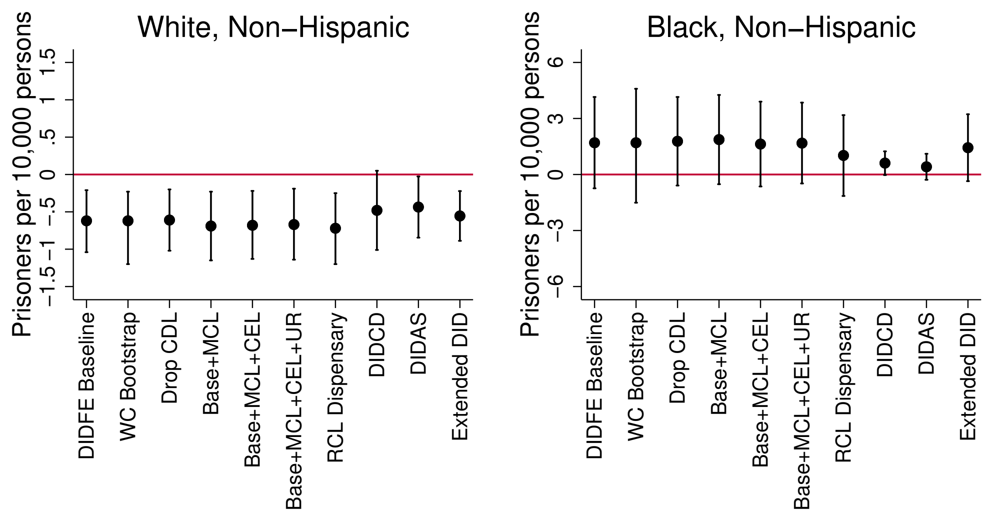

Figure S16: Prisoner admissions for drug offenses, robustness checks

Notes: Prisoner 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 (2022) 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.

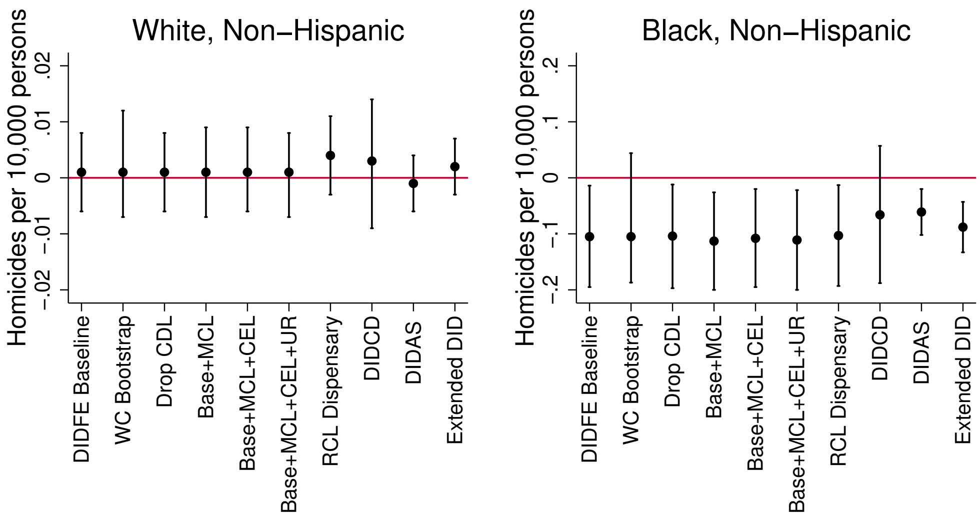

Figure S17: Homicide rates, robustness checks

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 (2022) 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.

D Heterogeneity Analysis

D.1 Cannabis Decriminalizations Laws

Table S6: Effect of recreational cannabis laws on arrests, by presence of decriminalization law

		Cannabis	Other drug	Non-drug	Total
Population:	RCL × Decriminalization	-3.403**	-0.208	20.893**	17.282
		(1.273)	(1.947)	(9.889)	(10.346)
	RCL ×(1− Decriminalization)	-16.994***	-1.286	43.876**	25.596
		(1.887)	(2.147)	(21.418)	(23.448)
	Coefficient test	0.00	0.69	0.23	0.68
White:	RCL × Decriminalization	-3.666***	0.593	11.016	7.942
		(1.199)	(2.365)	(9.530)	(9.464)
	RCL ×(1− Decriminalization)	-15.112***	-3.154	30.770	12.504
		(1.888)	(2.052)	(20.514)	(22.474)
	Coefficient test	0.00	0.23	0.29	0.82
Black:	RCL × Decriminalization	-12.583***	-8.350**	50.978**	30.046
		(4.502)	(3.793)	(24.234)	(29.450)
	RCL ×(1− Decriminalization)	-49.073***	1.903	131.777***	84.607
		(12.444)	(5.955)	(48.701)	(55.474)
	Coefficient test	0.00	0.09	0.07	0.29

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 above or equal to 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 (or absence) of cannabis decriminalization laws 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.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

D.2 Offense Categories

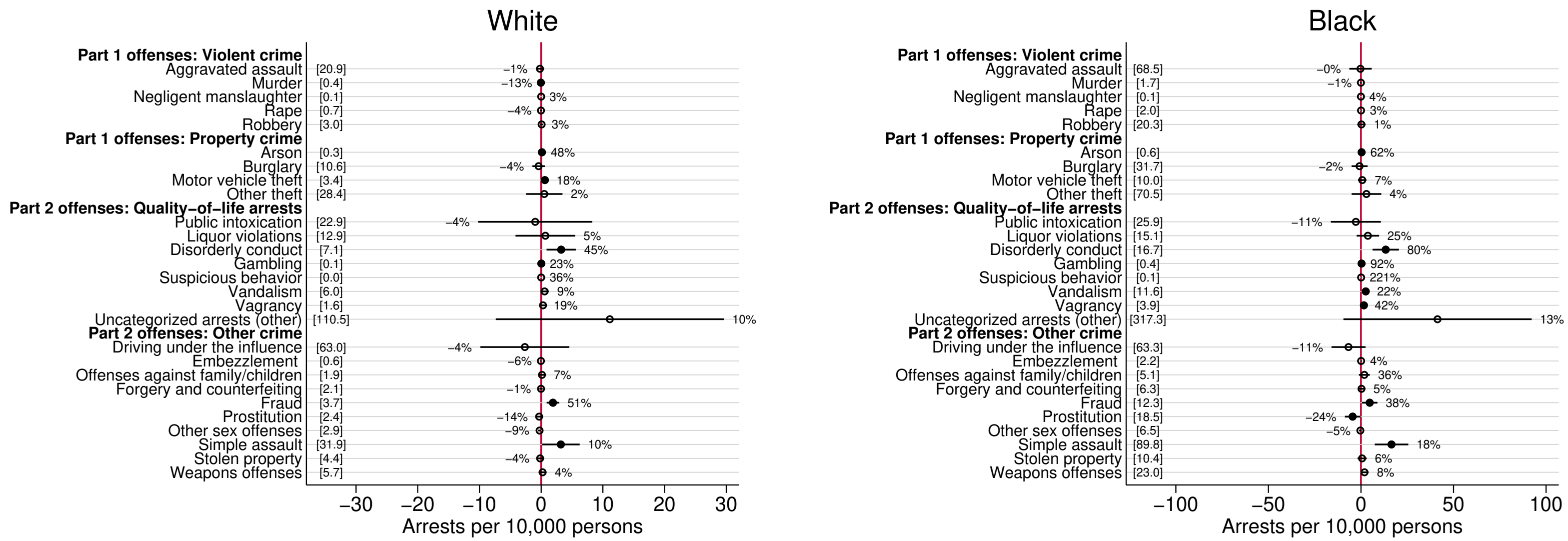

Figure S18: Arrest rates, by offense categories

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 above or equal to 65%. Coefficients and 95% confidence intervals shown from standard errors clustered by state. Solid markers denote significant effects; hollow markers are insignificant at the 95% level. Numbers next to markers denote the effect size in percentage terms of the pre-policy mean in RCL states. Numbers in brackets on the left show the pre-policy mean in RCL states. Each coefficient is based on 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. RCL=Recreational cannabis laws.

E Criminal Activity

E.1 Data

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=1,740,291).[1] The Portland Police Bureau counted all calls for service where at least one Portland police officer 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).

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=2,353,110).[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 2012-2021 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=232,303). 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. Final call type was used to categorize

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

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

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

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).

Detroit, MI - We analyzed 2017-2021 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=1,657,713).[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. Final call type 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=1,678,676).[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. We dropped various call types that could only be initiated by police (e.g., directed patrol, subject stop) so that estimates would be more comparable to those in other cities. Final call type 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= 348,638).[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

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

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

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

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=2,076,050).[7] This data was transcribed from 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 index crimes and select Part 2 crimes (n=430,282).[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 after 2013. Therefore, we only analyzed Part 2 offenses that were being reported since 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).

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

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

E.2 Minority Neighborhoods

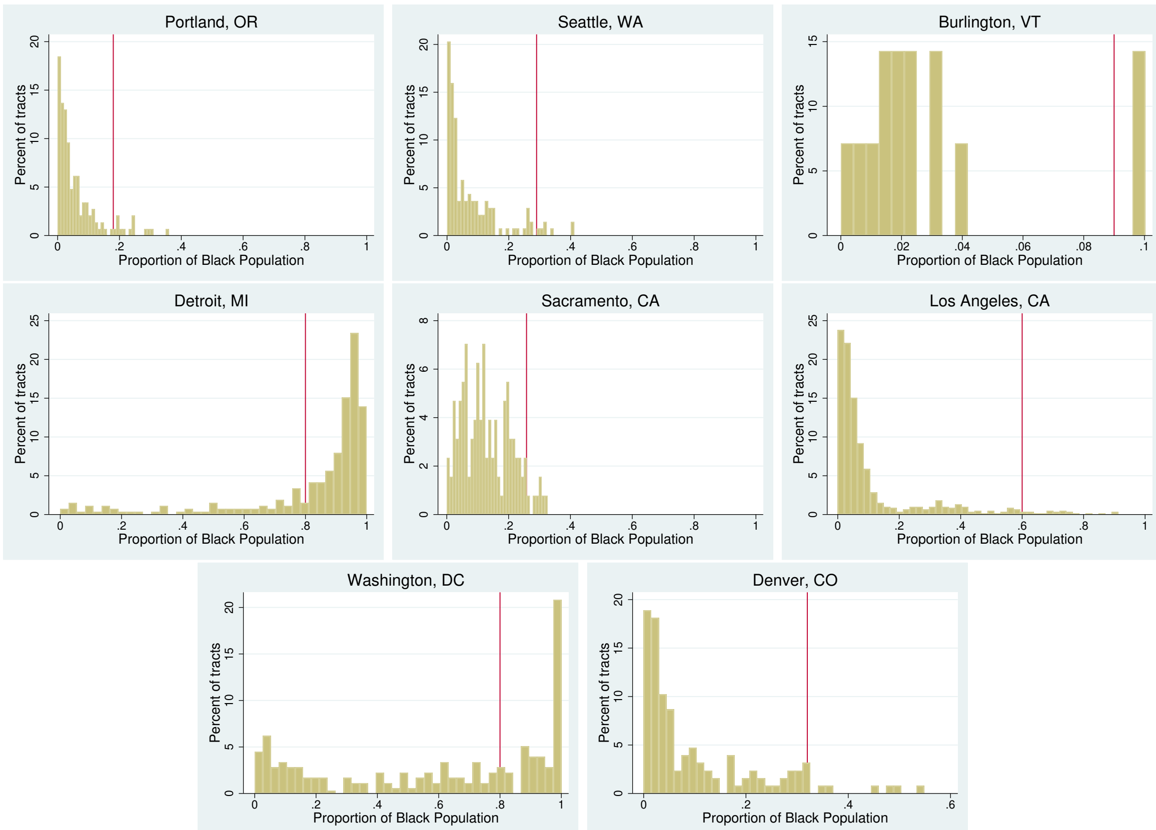

Figure S19: Distribution of minority neighborhoods

Notes: Histograms indicate the distribution of tracts based on the proportion of Black persons (Hispanic and non-Hispanic). The red line indicates the threshold used to assign tracts to the treatment group, with all tracts to the right of the line considered minority neighborhoods. The distribution of tracts varied widely across cities, making it impossible to assign tracts using a uniform rule. In cities with low Black populations (Portland, Seattle, Sacramento, Los Angeles, Denver, Burlington), we assigned tracts to the treatment group if the proportion of Black persons was at least in the top 5-15th percentile. In cities with large Black populations (Detroit, DC), we assigned tracts to the treatment group if the proportion of Black persons was at least 0.8. Since Detroit has a very small number of tracts with a large proportion of non-Black persons, we dropped tracts in the control group if the proportion of Black persons was 0.7-0.8.

Table S7: Effect of recreational cannabis laws on select Part 2 offenses in minority neighborhoods

	Fraud	Weapon	Simple Assault	Vandalism	Disturbance	Suspicious	DUI
Panel A: Portland, OR							
Civilian initiated emergency and non-emergency calls for service							
RCL×Minority	0.00	-0.73*	-1.00*	-0.89**	-15.21**	-4.60***	-0.02
	(0.24)	(0.40)	(0.60)	(0.43)	(6.38)	(1.43)	(0.12)
Mean	1.77	4.96	13.99	6.78	80.44	22.34	0.77
Panel B: Seattle, WA							
Civilian initiated emergency and non-emergency calls for service							
RCL×Minority	-1.02**	-1.09*	-4.87**	-0.48	-15.45**	-17.49***	0.55**
	(0.43)	(0.57)	(2.13)	(0.45)	(7.07)	(3.34)	(0.24)
Mean	5.87	5.36	36.47	10.68	81.14	66.81	1.22
Panel C: Burlington, VT							
Civilian initiated emergency and non-emergency calls for service							
RCL×Minority	-0.92	0.02	-0.61	3.53**	4.46	2.18	-0.51*
	(0.60)	(0.06)	(1.48)	(1.48)	(13.51)	(5.21)	(0.26)
Mean	8.83	0.08	36.90	24.46	149.15	102.33	1.88
Panel D: Detroit, MI							
Civilian initiated emergency calls for service							
RCL×Minority	0.07	3.21***	0.29**	1.07**	6.80**	5.53**	
	(0.11)	(1.16)	(0.13)	(0.43)	(3.08)	(2.53)	
Mean	0.80	21.57	0.13	5.87	48.34	14.25	
Panel E: Sacramento, CA							
Civilian and police initiated emergency and non-emergency calls for service							
RCL×Minority	-0.07	-1.14	-0.50	0.66	14.60	9.18	-1.14
	(0.10)	(1.83)	(2.46)	(0.92)	(16.65)	(23.07)	(1.14)
Mean	0.67	22.92	36.42	5.88	160.69	145.60	13.86
Panel F: Los Angeles, CA							
Reported crimes							
RCL×Minority	-0.46***	0.28	-0.55***	0.08	-0.19**		
	(0.14)	(0.41)	(0.20)	(0.35)	(0.08)		
Mean	1.19	8.27	3.16	8.11	0.52		

Notes: Calls for service or reported crime data for select Part 2 offenses. Typical text used to identify each offense type included: Fraud (fraud, forgery, bad check, embezzlement, counterfeiting, scam, extortion, identity theft); Weapon (gun, weapon, shooting, shorts fired, firearm, armed); Simple Assault (simple assault, harassment, threats, fight); Vandalism (vandalism, graffiti, malicious destruction of property, property damage -excluding from car crashes-); Disturbance (disturbance, disorder, noise, mischief, prowler, nuisance, trespassing, unwanted person, disorderly conduct); Suspicious (suspicious person, circumstances, event, or auto, loitering, panhandling); and DUI (driving under the influence). Other Part 2 offenses sometimes collected by few cities but not reflected in here included gambling, prostitution, drug possession or sales, liquor violations, other vice crimes, and public intoxication. Text used and categorizations vary widely across cities. The unit of analysis is a tract-quarter. Outcomes reflect the number of incidents at the tract-quarter level. Details are in Section 3 of the manuscript and Section E of the Appendix. 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 reported crimes in minority neighborhoods, by victim race

	White, NH	Black, NH	Hispanic
Panel A: Los Angeles, CA			
Reported crimes			
RCL×Minority	-0.30	-8.18***	-0.44
	(0.33)	(2.23)	(0.94)
Mean	3.43	55.55	15.60

Notes: 2010-2019 Crime reports collected by the Los Angeles Police Department. The unit of analysis is a tract-quarter. Outcomes reflect the number of reported crimes at the tract-quarter level. Details are in Section 3 of the manuscript and Section E of the Appendix. Standard errors clustered at the tract level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.