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

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THIS IS AN EARLY VERSION AND RESULTS ARE PRELIMINARY.

Racial disparities in law enforcement of drug prohibition are widespread and longstanding, with Black communities being disproportionately affected. We studied the effect of cannabis legalization on racial disparities in the criminal justice continuum using a difference-in-differences framework. Preliminary findings suggest that legalization led to sizable reductions in arrest rates for cannabis possession and sales across all racial groups, resulting in declines in the rate ratio and rate difference for Black relative to White populations. While arrest rates for possession of other drugs did not change significantly, arrests for sales of other drugs decreased across all racial groups. However, declines in drug arrests were accompanied by offsetting increases in arrests for disorderly conduct and simple assault, which are less serious offenses that often reflect discretionary police behavior. There were no significant changes in incarceration rates. Moreover, there were no increases in street violence following legalization and in some cases, we documented declines among Black and Hispanic populations. Although cannabis legalization may be a policy lever for addressing historic racial disparities, additional provisions in the law may be crucial for reducing inequalities.

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 often considered one of the most costly and destructive aspects of America's failed War on Drugs. The toll comprises street violence from the creation of an illegal drug market, years of life lost behind bars, children growing up without a parent, criminal records crippling access to jobs, loans, housing and government benefits, and billions of dollars spent on law enforcement (Shultz and Aspe, 2017; Hudak, 2021; Earp et al., 2021). In 2018, police officers made about 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. In 2019, drug possession or trafficking was the most serious offense for serving time among 46% of sentenced federal prisoners and among 14% of sentenced state prisoners (Carson, 2020).

Racial and ethnic disparities in law enforcement of cannabis and other drugs are widespread and longstanding, with Black communities disproportionately affected. Even though White and Black persons use cannabis at roughly the same rate, 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 13.4% 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). Additionally, 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).

The legalization of cannabis may be an effective step toward correcting the damage of drug prohibition on racial disparities in the criminal justice system and other consequences related to street violence. As of 2022, 20 states have passed recreational cannabis laws (RCLs), allowing individuals ages 21+ to possess, use, and supply limited amounts of cannabis for recreational purposes (ProCon, 2022). Supporters of cannabis legalization espouse that RCLs will create hundreds of thousands of jobs, generate tax revenue, take business away from illegal markets, lower street crime and violence, reduce law enforcement costs, and close racial and ethnic disparities in criminal justice outcomes (Gettman and Kennedy, 2014).

Previous studies on the impacts of RCLs are scarce and focus primarily on measures of cannabis utilization in the overall population and by age group, documenting increases for adults and mixed evidence for teenagers (Martins et al., 2021; Cerdá et al., 2017; Aydelotte et al., 2019; Cerdá et al., 2020; Hansen et al., 2020; Meinhofer et al., 2021). One recent RCL study considered self-reported cannabis use across racial and ethnic groups, finding statistically significant increases among adults who were Hispanic, Non-Hispanic White, and non-Hispanic Other Race, but no statistically significant changes for Non-Hispanic Black adults (Martins et al., 2021). Several RCL studies have considered criminal justice outcomes in the general population, including property and violent crimes, arrests, and drug seizures, with mixed results ranging from no important changes (Lu et al., 2021; Stohr et al., 2020) to reductions (Dragone et al., 2019; Brinkman and Mok-Lamme, 2019; Wu et al., 2020; Meinhofer and Rubli, 2021). There is consensus, however, that cannabis legalization did reduce cannabis possession arrests among adults in RCL states compared to non-RCL states (Plunk et al., 2019; Stohr et al., 2020). There is also evidence that cannabis legalization reduced law enforcement seizures of cannabis and other drugs, and potentially reduced the size of the illegal drug market (Stohr et al., 2020; Meinhofer and Rubli, 2021).

The majority of previous RCL studies have not considered the role of legalization on racial disparities in the criminal legal system; the handful 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). One study documented reductions in police traffic stops resulting in searches among Black, White, and Hispanic persons (Pierson et al., 2020). These studies, however, were either descriptive or based on pre-post analyses (Edwards et al., 2020; Firth et al., 2019; Pierson et al., 2020), used data from a single state or few states (Firth et al., 2019; Pierson et al., 2020), generated separate estimates for Black and White groups but did not formally test for changes in relative and absolute disparities (Edwards et al., 2020; Pierson et al., 2020; Sheehan et al., 2021); and/or did not consider other racial or ethnic groups (Edwards et al., 2020; Firth et al., 2019; Sheehan et al., 2021). Except for Pierson et al. (2020), these studies analyzed cannabis possession arrests exclusively (Edwards et al., 2020; Firth et al., 2019; Sheehan et al., 2021), an outcome that may only reflect partial equilibrium effects. 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 and ethnic disparities in overall arrests or other aggregate criminal justice outcomes. Understanding these general equilibrium 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). Moreover, 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 street violence from drug-related crimes and associated criminal justice outcomes.

This study addressed these gaps in the literature and generated the most comprehensive estimates to date of the effects of RCLs on racial and ethnic disparities in the criminal justice continuum. The outcomes of interest included measures of law enforcement and street violence associated with illegal drug markets, both of which reflect important consequences of drug prohibition. Specific measures included the rate of arrests, prisoners, homicides, assaults, and gun injuries per 10,000 persons, overall and by racial and ethnic groups. To measure disparities, we calculated rate ratios and rate differences relative to the White group, which represents the largest proportion of the U.S. population. Rate ratios and rate differences measure relative and absolute disparities, respectively. Although both correlate perfectly in the cross-section, they may provide contradictory results when considering changes over time and space. As such, both measures provide distinct information and are important for understanding the nature of changes in disparities (Keppel et al., 2005). We leveraged a variety of administrative datasets spanning 2007-2019, each providing complementary strengths and allowing for a validation check of findings, which is necessary given the inherent challenges in measuring criminal justice outcomes. We exploited the staggered timing of RCL implementation across states, using effective dates in a difference-in-differences (DID) framework. We showed event study plots and static DID estimates, and verified that main results were robust to recent advances in the DID literature.

Preliminary findings suggest that RCLs led to significant declines in arrest rates for cannabis possession and sales across all racial groups, resulting in declines in the rate ratio and rate difference of cannabis arrests for Black populations relative to White populations.

We further documented declines in arrest rates for sales of other drugs across all racial groups, but not for possession. Declines in arrests for drug offenses were offset by increases in arrests for non-drug offenses across all races, although estimates were largest and only statistically significant among Black populations. These increases were driven by arrests for less serious Part 2 offenses and were associated with increases in absolute and relative disparities among Black populations. There were no statistically significant changes in incarceration rates across racial groups. Moreover, there were no increases in measures street violence following RCL implementation and in some cases, we documented declines among Black and Hispanic populations.

Elucidating the impact of RCLs on racial and ethnic disparities in the criminal legal system is important for designing successful regulation that works in reparative ways. This timely study is of critical importance as it can inform the cannabis legalization debate, guide federal and state governments regulating cannabis possession and distribution, identify unintended consequences, and generate knowledge that can guide policy approaches for implementing cannabis legalization policies that seek to repair unequal criminal justice outcomes caused by the War on Drugs.

The rest of 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 findings. Section 6 discusses policy implications. Section 7 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, 20 states 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). There is state variation in RCL provisions. Some RCL states legalize the use of cannabis in public, create licenses to legally sell cannabis, and permit home cultivation (ProCon, 2022). Allowed possession amounts in RCL states range from 1-3 ounces in public and up to 10 ounces in a private residence (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). 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).

While decriminalization may offer some relief from mass incarceration, it preserves many of the punitive features and consequences of the criminal misdemeanor experience (Natapoff, 2015). 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). These consequences are likely to have distributive implications, affecting poor, drug-dependent, and otherwise disadvantaged defendants, the majority of which are persons of color, while permitting well-resourced offenders to exit the process quickly and relatively unscathed (Smart and Kleiman, 2019).

2.2 Previous Literature

Previous studies of the impact of cannabis liberalization on criminal justice outcomes primarily focus on MCLs and its 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, 2021). None of these studies have found evidence indicating that MCLs increased crime; if anything, most of the evidence suggests that MCL implementation was followed by crime reductions. CDL studies of criminal justice outcomes in the general population have documented reductions in drug-related arrests (Grucza et al., 2018; Plunk et al., 2019). Several 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 (Lu et al., 2021; Stohr et al., 2020) or reductions (Dragone et al., 2019; Brinkman and Mok-Lamme, 2019; Wu et al., 2020) in crime. There is consensus, however, that cannabis legalization did reduce cannabis possession arrests among adults in RCL states compared to non-RCL states (Plunk et al., 2019; Stohr et al., 2020). Indeed, a study found that cannabis possession arrests dropped by 168.5 per 100,000 persons following RCL implementation in four states (Plunk et al., 2019). There is also consensus that cannabis legalization reduced law enforcement seizures of cannabis (Stohr et al., 2020) and other drugs, and possibly reduced the size of the illegal drug market (Meinhofer and Rubli, 2021).

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

The handful of previous RCL studies that considered racial and ethnic disparities in the criminal legal system were descriptive or based on single-state data (Edwards et al., 2020; Firth et al., 2019, 2020). An exception is Sheehan et al. (2021), that employed an event study approach and documented reductions in cannabis possession arrests for Black and White adults in the first three years following RCL implementation. However, the parallel trends assumption necessary for identification did not hold for most of these estimates, and it was unclear if and which reference period was excluded or if the authors controlled for lags and leads beyond those reported in the figures. Moreover, this study did not account for the well-documented variation across states and over time in the number of reporting agencies (Kaplan, 2021). A descriptive report by the American Civil Liberties Union documented 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 (Edwards et al., 2020). However, it was not clear that these lower racial disparities resulted from legalization as RCL states also had lower racial disparities in the years prior to legalization. Lastly, a single-state study focusing on Washington found that cannabis possession arrest rates decreased significantly among both Black and White adults but relative disparities grew from 2.5 to 5 following RCL implementation (Firth et al., 2019).

As previously noted, these studies analyzed cannabis possession arrests exclusively, an outcome that only reflects partial equilibrium effects of policing and crime production. Elucidating the full impact of RCLs on racial and ethnic disparities in the criminal legal system requires examination of non-cannabis arrests and other outcomes to assess potential spillovers and general equilibrium effects.

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 criminal activity, cannabis, and other drugs.

Cannabis and Crime. Most directly, cannabis prohibition implies that cannabis is defined as a crime; it is a crime to use, possess, manufacture, or distribute cannabis. Cannabis may also cause crime through at least three other pathways. First, the psychoactive effects of cannabis may influence user behavior, leading to criminal activity in some individuals. While studies have generally shown that cannabis use temporarily inhibits aggression and violence, there is evidence of violent behavior in some populations (i.e. adolescents) (Pacula and Kilmer, 2003). The psychoactive effects of cannabis may also cause other non-violent crimes such as traffic offenses (i.e. driving under the influence), public nuisances such as disorderly conduct, or property crimes. Second, users may engage in criminal activity to finance their cannabis use, which may cause increases in property crimes. Third, cannabis may cause crime by generating street violence and other criminal activity in connection with drug trafficking in illegal drug markets. Illegal drug markets are associated with increased street violence because of turf wars among suppliers, particularly since illegal producers do not have access to non-violent conflict resolution mechanisms, such as the legal court system (Miron, 1999; Levitt and Venkatesh, 2000; Adda et al., 2014).

RCLs and Reductions in Crime. The legalization of cannabis may reduce criminal activity and associated law enforcement efforts through various pathways. Decreases in cannabis arrests and other law enforcement efforts targeting cannabis should be mostly mechanical. 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. Assuming cannabis arrests are single-offense incidents, we would expect to see a decline not only in cannabis arrests but also in total arrests following RCL implementation. However, to the extent that cannabis arrests were pretextual and/or accompanied other non-cannabis offenses within the same incident, we may observe declines in cannabis arrests with limited or no declines in total arrests.

Cannabis legalization may also reduce criminal activity and associated law enforcement efforts through its effects on illegal drug markets. In general, the creation of a legal market should reduce the illegal market for cannabis, decreasing systemic violence and other criminal activity in connection with cannabis trafficking (Dragone et al., 2019). Indeed, previous studies suggest that the street price of cannabis declined following RCL implementation (Meinhofer and Rubli, 2021), which is consistent with reductions in the size of the illegal market. Consumers and producers of cannabis may also be consumers and producers of other illegal drugs. To the extent RCLs reduce the illegal market for other drugs or the use of other drugs that are substitutes of cannabis, legalization may also reduce criminal activity and associated law enforcement efforts involving the possession and sales of other drugs. As a sedative drug, RCL induced increases in cannabis use may lead to reductions in violent crime, especially if greater cannabis use leads to substitution away from potential violence-inducing substances such as alcohol (Dragone et al., 2019).

Law enforcement agencies may reduce resources allocated to drug prohibition following RCL implementation, further reducing arrests and incarcerations associated with other drugs, regardless of changes in the possession and sales 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. Previous research has found that law enforcement seizures of cannabis and other illegal drugs declined following RCL implementation (Meinhofer and Rubli, 2021). Lastly, cannabis legalization may reduce criminal activity through 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 and access from legalization should increase the opportunity cost of participating in illegal markets and reduce the number of drug crimes committed (Ihlanfeldt, 2007).

RCLs and Increases in Crime. In contrast, the legalization of cannabis may increase criminal activity and associated law enforcement efforts through various pathways. In particular, RCLs may increase aggregate demand for cannabis, which may lead to increases in crimes attributable to the psychoactive effects of cannabis or crimes committed by individuals to finance their cannabis consumption. Moreover, if greater cannabis use increases consumption of complement drugs, legalization could lead to more drug-related offenses. Increased competition from the creation of a legal market may increase street violence associated with drug trafficking. Lastly, law enforcement may reduce enforcement related to drug markets more broadly. The reduction in police presence, especially in socio-economically disadvantaged neighborhoods, should decrease the probability of apprehension and increase criminal activity. Moreover, law enforcement may decide to 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).

It is important to note that any changes in arrests will potentially translate to changes in prisoner populations. However, changes in prisoner populations will also depend on whether RCLs affect the court system and its considerations for prosecuting and sentencing. For instance, district attorneys', judges', and juries' perceptions may be influenced after RCL implementation: perhaps other drug offenses are not seen as important or maybe there is the opposite reaction with an increase in prosecution and sentencing.

Heterogenous Effects of RCLs by Race and Ethnicity. Given the expected net effects of cannabis legalization on criminal activity and associated law enforcement outcomes are ambiguous; the same should be true for its net effects across racial and ethnic groups. Subsequent effects on absolute and relative disparities are also ambiguous, and will depend on whether treatment effects are differential across racial and ethnic 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 and ethnic differences in the rates of some crimes and law enforcement outcomes.[1] To the extent that treatment effects change disproportionately across race and ethnicity, RCLs may also affect relative disparities.

RCLs and Reductions in Racial Disparities in Police Contact. There are several pathways through which the legalization of cannabis may decrease racial disparities in criminal activity and arrests. For instance, to the extent that illegal drug markets are in minority communities of color, RCL implementation should reduce illegal drug markets and therefore, the the number of illegal drug markets in communities of color or the number of minorities participating in illegal drug markets. Previous studies suggest that law enforcement over-target communities of color (Beckett et al., 2006). Over-policing in minority communities might follow from taste-based discrimination (i.e., police officers would rather arrest a Black person than a White person for a drug offense), from statistical discrimination (i.e., police officers believe that criminal behavior is more predominant among Black persons and allocate more resources to those communities), or there may be lower policing costs of minority communities. Nonetheless, legalization and shrinking drug markets could have the largest effects on communities of color as police presence and contact decrease, resulting from a decline in arrests for cannabis possession and sales. Likewise, young Black males are more likely to be apprehended for drug-related offenses (Fielding-Miller et al., 2020) and less likely to be attached to the labor market (Mincy et al., 2006). RCL implementation and job creation have the potential to increase labor market opportunities and decrease criminal activity in minority communities, therefore possibly having the greatest effect on Black men.

[1] Prior to RCLs there were higher arrest rates for Black persons for cannabis possession, despite no differential incidence of cannabis consumption across race groups (Carson, 2021; Motivans, 2020).

Lastly, law enforcement may reallocate resources to pursue more costly criminals. If the apprehension costs are higher for White persons (Beckett et al., 2006), it is possible that law enforcement could increase arrests for non-cannabis crimes in this group with resources previously used to disrupt drug markets in minority communities (for example, a shift from cannabis to illegal opioid or methamphetamine markets). Law enforcement efforts to engage in more costly policing could increase arrests in under-policed communities (Cox and Cunningham, 2021), further decreasing racial disparities in arrests.

RCLs and Increases in Racial Disparities in Police Contact. However, the legalization of cannabis may increase racial disparities in police contact. Law enforcement may continue targeting communities of color and simply redirect efforts to deter non-cannabis offenses, increasing arrests for non-cannabis offenses. Additional resources to deter crime are made available from previous drug prohibition enforcement efforts and prior research has linked increases in police resources to higher levels of police contact reflected by an increase in "quality-of-life" arrests (Chalfin et al., 2022).[2] Elevated levels of quality-of-life arrests are a byproduct of a history of over-policing in minority communities (Hinton, 2017), and ex-ante, it is unclear that RCL will change policing strategies in these communities. In addition, RCL implementation may increase competition in non-cannabis drug markets. If non-cannabis drug markets are disproportionately in communities of color, the violence that typically accompanies territorial drug disputes will be elevated and increase racial disparities in victimization, while heightened police presence will likely increase racial disparities in arrests. Lastly, it is unclear how previous police contact may limit access to newly created jobs associated with cannabis legalization. It is possible that prior criminal records limit the job perspectives of Black men, even when states implement reforms, such as ban the box, to reduce the stigma associated with criminal histories (Agan and Starr, 2018; Doleac and Hansen, 2020).

RCLs and Racial Disparities in Incarceration. Lastly, the number of individuals confined to state or local prisons has declined steadily since 2010. A less punitive approach to drug offenses is the main contributor to the decline in incarceration. If RCL implementation decreases drug arrests, it could also provide the added benefit of contributing to overall

[2] Quality-of-life arrests include disorderly conduct, liquor violations, and loitering.

decarceration. Moreover, if RCL reduces racial disparities in police contact, then it is likely there will be a reduction in racial disparities in incarceration. This would be the case even if there is an increase in quality-of-life arrests, as these crimes typically do not result in imprisonment (Chalfin et al., 2022). However, an increase in quality-of-life arrests may have criminogenic effects. Misdemeanor convictions have been linked to a higher likelihood of re-offending and showing up on background checks, which may limit labor market opportunities (Agan et al., 2021).

3 Data

Arrests. We obtained arrest data from the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting Program: Arrests by Age, Sex, and Race (UCR-ASR). Data captured monthly arrest counts for each agency that reported to the UCR Program, disaggregated by offense type, race, age, and sex. We retrieved arrest data dating back from 2007 to 2019 and excluded U.S. territories (American Samoa, Canal Zone, Guam, Puerto Rico and Virgin Islands) from our analysis.

All arrest records corresponded to the highest charge, according to an FBI hierarchy, for each individual arrested (Kaplan, 2021). For instance, any cannabis arrest recorded was one in which cannabis possession/sale was the highest charge for which that individual was arrested during that police interaction.[3] For each arrest, police officers register the offender's race, based on their own perceptions, as either White, Black, Asian, or American Indian. We aggregated the last two into an "Other Race" category. Although ethnicity is also technically reported, the vast majority of agencies did not include Hispanic counts during most of our sample period, which is why we do not consider this dimension.

[3] The hierarchy for serious offenses (e.g., murder) is common across all agencies. However, for less serious crimes, like drug offenses, each agency must decide 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 public order violations.

The UCR data do not allow us to observe the different number of individuals that were arrested in a given time period.[4] As such, arrest rates are not exactly equal to the number of different people arrested divided by population counts. Comparing our measures across race groups should proxy for disparities as long as the rate at which individuals in different race groups get rearrested are the same or unchanging over time.

We analyzed "Cannabis arrests" and "Other drug arrests" (heroin/cocaine, synthetic narcotics, and other drugs), each of which combined arrests for possession and sales. We also aggregated all "Non-drug arrests" and "Total drug arrests". Lastly, to explore potential spillover effects, we followed the FBI's classification to aggregate arrests into Part 1 (property crimes corresponding to arson, burglary, motor vehicle theft, and theft, and violent crimes comprising aggravated assault, manslaughter, murder, rape, and robbery) and Part 2 offenses (everything else, excluding drug offenses).

Finally, to account for differences in when agencies report to the FBI, we aggregated 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 addressed 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 constructed 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. In our main analysis, we restricted to an agency reporting coverage threshold of at least 65%. This effectively completely dropped 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.

Prisoners. Prisoner data was obtained from the Bureau of Justice Statistics' 2009-2019 National Prisoner Statistics Program (NPS). NPS provides an enumeration of persons in state and federal prisons on December 31 of the reporting year, by race and ethnicity. An

[4] 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.

individual person may have more than one record if they stayed in prison longer than one year. Counts are at the state-year level and include all state and federal inmates held in a prison (custody) and those held in jail facilities either physically located inside or outside of the state of legal responsibility, and other inmates who may not be physically located in a facility at year-end but are under a state's jurisdiction.

NPS data are subject to limitations. Since 2001, the District of Columbia no longer operated a prison system, thus, we dropped it from our analysis of NPS data. No other states were dropped, but some imputations were done to correct for obvious reporting errors in some states. For example, in 2013 Alaska reported zero prisoners for each racial and ethnic group, but reported a jurisdiction total of 5,081 prisoners. To correct for this reporting error, an average incarceration count was calculated using 2012 and 2014 counts to replace the zero value for each corresponding racial and ethnic groups in 2013. Another reporting change occurred in California in October of 2011, where 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 observed in the data (now placed in county jails), we dropped observations corresponding to California in 2009-2011. Another limitation included the under-representation of Hispanic inmates due to differences in reporting of ethnicity across states. For this reason, when analyzing outcomes for Hispanic subgroups, we dropped entire states (AL, FL, ME, MD, MT, MI, NH) or select state-years (GA 2009-2010, VT 2009-2012) with poor reporting. We generated year-end prisoner counts at the state-year level, overall and by race and ethnicity. Racial and ethnic groups included Non-Hispanic White, Non-Hispanic Black, Non-Hispanic Other Race (Asian, American Indian/Alaska Native, Native Hawaiian/ other Pacific Islander), and Hispanic.

Homicides. Homicides were obtained from restricted 2007-2019 Multiple Cause of Death files from the National Vital Statistics System (NVSS) of the National Center for Health Statistics. 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 occurring within the United States. We selected homicide deaths from persons aged 12 years or older at the time of death. Homicides as the underlying cause of death were identified using International Classification of Diseases (ICD), Tenth Revision codes (U01, U02, X85-X99, Y00-Y09, Y87.1) previously established (Center for Disease Control and Prevention, 2007). We also relied on a data variable identifying homicide as the manner of death. We aggregated homicides at the state-year-quarter level, overall and by race and ethnicity. Racial and ethnic groups included Non-Hispanic White, Non-Hispanic Black, Non-Hispanic Other Race (Asian, American Indian/Alaska Native, Pacific Islander), and Hispanic.

Gun Injuries and Assault. Hospitalizations involving gun injuries and assault were drawn 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 specifically relied on the 2007-2019 HCUP State Inpatient Databases (HCUP-SID) for select states, although our panel of states was unbalanced since we could not obtain all data years for some states. HCUP-SID contains a near census of inpatient care discharge records in participating states, and provide demographic and healthcare information for patients. Demographic information included age, race and ethnicity, sex, and geographic area. Healthcare information includes primary expected payer (Medicaid, private insurance, other) and ICD-9/ICD-10 diagnostic and procedure codes associated with the discharge. We identified hospitalizations involving gun injuries and assault using standard ICD-9/ICD-10 codes previously established (Smart et al., 2022; Center for Disease Control and Prevention, 2021).

Some states participating or not participating in HCUP directly provide researchers with access to their inpatient discharge records for a lower fee, or can directly generate assault and gun injury state-year counts. We combined HCUP-SID with hospital discharge data directly shared by other states, for a total of 31 states including 10 switching RCL states. We selected hospitalizations from persons aged 12 years or older. We aggregated hospitalizations at the state-year level, overall and by race and ethnicity. Racial and ethnic groups included Non-Hispanic White, Non-Hispanic Black, Non-Hispanic Other Race (Asian, American Indian/Alaska Native, Pacific Islander), and Hispanic.

3.1 Summary statistics

Tables 1 through 4 reported summary statistics for states with and without RCLs during the study period. For RCL states, we distinguished between the periods before and after the policy.

Table 1 presented descriptives for arrest data. Arrest rates were highest for Black persons and lowest for Other Race populations across all offense categories. For instance, in the full data, cannabis arrests for Black persons were over three times higher than for White persons, while total arrests were more than twice as large. As noted above, due to how the data were compiled, this may reflect both a larger number of Black persons arrested but also a higher probability of rearrest for each individual. These simple averages also showed sharp declines in cannabis arrests across all groups in RCL states post-policy.

Table 2 presented descriptives for prisoner data. Non-Hispanic Black and Hispanic persons had higher incarceration rates across all states and periods. In the full data, the rate of prisoners was more than five times larger for Non-Hispanic Black persons than for Non-Hispanic White persons. Comparing the RCL states before and after the policy, the simple averages show very little differences over time for these jurisdictions.

Table 3 showed much higher homicide rates for Non-Hispanic Black persons. The homicide rate for Hispanic populations was similar to the full population average, while rates were lower than average for Non-Hispanic White and Non-Hispanic Other Race groups. Inspecting these averages for RCL states, post-policy we documented a sizable decline in homicide rates for Non-Hispanic Black persons only. Lastly, the rate of hospitalizations for assault and gun injuries were much larger for Non-Hispanic Black persons than for Non-Hispanic White persons, as seen in Table 4.

4 Empirical strategy

Our identification strategy exploited variation in the staggered implementation of recreational cannabis laws across states and time using the effective dates in Online Appendix S1. Equation 1 represents the baseline two-way fixed effects (TWFE) difference-in-differences

(DID) regression model. Separate DID models were estimated for the overall population and for each racial and ethnic group r:

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

$Y_{r,j,t}$ denotes an outcome for racial and ethnic group r in jurisdiction j (which may be a state or a county) in time period t (quarter or year). We generated three disparity measures for each outcome; rates, rate ratios and rate differences. First, we generated rates per 10,000 persons by dividing outcome counts for each racial/ethnic group r by U.S. Census population estimates corresponding to the same racial/ethnic group r. Second, we generated the rate ratio relative to the White group by dividing the rate for each non-White group by the rate for the White group. Lastly, we generated the rate difference relative to the White group by subtracting the rate for the White group from the rate for each non-White group. Rate differences measure absolute disparities while rate ratios measure relative disparities, both of which provide distinct and necessary information for understanding the nature of changes in disparities across geographic areas and over time (Keppel et al., 2005).

$RCL_{j,t}$ is an indicator equal to one if an RCL was effective in jurisdiction j at time period t and zero otherwise. $X_{r,j,t}$ is a vector of control variables that include a MCL indicator, a CDL indicator, and state-level unemployment rates to account for differences in economic conditions. When using arrest data, we also controlled for the number of reporting agencies in a given county-year. As a robustness check, we presented specifications excluding these controls. We included jurisdiction fixed effects, denoted by α_j, to account for any time-invariant differences across jurisdictions that may affect law enforcement and street violence outcomes. This implies that we effectively identify our coefficient of interest off of within-jurisdiction variation in the outcomes over time. We also included time period fixed effects η_t to control for any state-invariant nationwide shocks affecting outcomes. Lastly, $\varepsilon_{r,j,t}$ is the idiosyncratic error term. All regressions for racial/ethnic group r are weighted by U.S. Census population estimates for racial/ethnic group r. Standard errors are clustered by state, which is the level at which the treatment varies (Abadie et al., 2017). 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, law enforcement and street violence 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 included jurisdiction and time period fixed effects, the only remaining source of potential bias is time-varying unobserved factors at the jurisdiction level. Our vector of controls addresses some of these potential confounders.

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

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

where E_j^{RCL} indicates the time period in which jurisdiction j implemented an RCL, $\mathbb{1}_{[\cdot]}$ is the indicator function, $L > 0$ defines an arbitrary number of leads and lags, and everything else is as defined above. We also included 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.

The TWFE estimator in Equation 1 is a weighted average of all 2×2 DID comparisons between groups of jurisdictions (Goodman-Bacon, 2021). This implies that jurisdictions that passed an RCL early on in our sample period are part of the comparison group of those that implemented an RCL later in this time span. If average treatment effects are heterogeneous across jurisdictions and over time, this will bias the identification of the treatment effect. Furthermore, the TWFE estimator is a weighted average of all jurisdiction-specific treatment effects, where weights may be negative and non-convex due to heterogeneous average treatment effects (De Chaisemartin and d'Haultfoeuille, 2020). Hence, the estimator in equation 1 may be biased. We addressed this potential issue in two ways. First, in Appendix Table S2 we estimated the share of comparisons that have a negative weight and the total sum of all negative weights in TWFE regressions, following De Chaisemartin and d'Haultfoeuille (2020). Second, we showed results using the estimator in (De Chaisemartin and D'Haultfoeuille, 2022) that is robust to heterogeneous treatment effects and allows for the estimation of dynamic effects.

We presented additional checks on our main results. First, we calculated p-values using a wild cluster bootstrap approach, which improves statistical inference when the number of treated clusters is small (Roodman et al., 2019; Meinhofer et al., 2021). This addressed the potential concern that the number of switching RCL states is not sufficiently large, in which case standard methods of statistical inference may over-reject the null (Roodman et al., 2019; Cameron and Miller, 2015; Conley and Taber, 2011). Second, we showed robustness of the effects to dropping each RCL state one at a time, which indicated that estimates are not driven by a single outlier RCL state. Lastly, for the arrests data, we considered alternative sample restrictions by imposing stricter thresholds for the reporting agency coverage indicator and by excluding outliers from the estimation.

5 Results

5.1 Descriptive patterns

We started by plotting raw data trends in RCL states before and after the policy, by race and ethnicity. We normalized time periods so that time zero corresponds to the period right before RCL implementation. We calculated population-weighted averages across all RCL states.

Figure 1 plotted arrest rates for cannabis possession and sales, for non-drug arrests, and for total arrests. Following RCL implementation, there was a sharp decline in the levels of cannabis arrest rates for all groups. There was a slight downward trend prior to the policy that leveled in the post-policy period. For non-drug arrests, we observed an increase for Black persons after RCL implementation. Lastly, total arrests appeared unchanged or slightly higher post-policy, especially for Black persons. Figure 2 presented a similar graph for prisoner rates. There was a slight downward trend in the rate of Non-Hispanic Black prisoners. Figure 3 plotted homicide rates using quarterly data. While rates appeared unaffected by the policy for Non-Hispanic White, Non-Hispanic Other Race, and Hispanic populations, there is a gradual decline in homicide rates of Non-Hispanic Black persons after RCL implementation. Lastly, Figure 4 displayed similar trends for hospitalization rates. Assault and gun injury hospitalizations declined for Non-Hispanic Black persons, but remained fairly constant for the other three population groups.

Overall, the raw data trends suggested four things: (1) a decline in cannabis arrests across all races, (2) an increase in non-drug arrests, especially for Black persons, (3) no changes in imprisonment, and (4) reductions in outcomes related to violence for Non-Hispanic Black persons only. We presented results from our empirical strategy below.

5.2 Law enforcement

Cannabis arrests. Figure 5 presents event study plots of the effect of RCLs on county-level cannabis arrests–both possession and sales–per 10,000 persons for each available racial group. All event study plots consider time zero to be the year prior to RCL implementation. We observe significant declines in cannabis arrest rates post RCL implementation across all racial 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 5. The first column corresponds to cannabis arrest rates by population group and for the full population. The estimated declines in cannabis arrest rates are significant for all groups. Relative to the average cannabis arrest rate in RCL states prior to implementation, the estimated effect corresponds to a reduction of 45% in cannabis arrest rates for the full population. By race, this corresponds to a 48% reduction for White persons, 44% for Black persons, and 83% for Other Race.

The next two columns show rate ratios and rate differences relative to the White group. We obtain significant and sizable declines in relative and absolute disparities among Black persons, of around 9% and 48% of the baseline disparity in RCL states during the pre-policy period. For Other Race populations, we observe a significant increase in disparities, consistent with the smaller declines in cannabis arrest rates. However, these populations had a baseline cannabis arrest rate half the size of the White group and one fifth of the Black group, suggesting perhaps limited scope for the policy to induce large reductions.

Table 6 breaks down the effects by cannabis arrest rates for possession (first column) and for sales (third column). We find significant declines after RCL implementation in both types of crimes. For the full population, this amounts to a 48% and 34% average reduction for possession and sales, respectively. We cannot reject that these effect sizes are equal. This pattern holds for each of the race groups and effect sizes echo the previous findings that grouped possession and sales in a single category.

Other drug arrests. We complement our previous results with TWFE DID estimates of the effect of RCLs on other drug arrests. The fourth column in Table 5 shows negative and statistically insignificant effects of RCLs on arrest rates for other drugs. The largest point estimate is for Black persons, indicating an average reduction of 4% relative to the pre-policy mean for the other drug arrest rate for Black persons in RCL states. The point estimate for the White group would imply a similar effect of 4%. For our measures of disparities, there are no significant impacts. Moreover, we obtain a positive coefficient for relative disparities and a negative one for absolute disparities.

Unlike the findings for cannabis arrests, the second and fourth column in Table 6 show strong significant declines in arrest rates for sales of other drugs but smaller and insignificant effects for possession of other drugs. From the magnitude alone, we would estimate a 0.8% decline in possession arrest rates for White persons but a 0.5% decline for Black populations. For sales, we also find similar magnitudes across the White and Black groups (on average, a 23.5 and 21.9% decline relative to the pre-policy mean in RCL states, respectively).

Non-drug arrests. We now turn our attention to arrests that are unrelated to drugs. Figure 6 shows event studies for each race group for arrests per 10,000 persons, grouping all non-drug categories together (i.e., all arrests except possession and sales of any drug). Although point estimates are noisily estimated, we observe a striking increase in non-drug arrest rates for Black persons. Although there is a similar pattern for White persons, the magnitudes are not as large. For the Other Race category, we see no evident increase or decrease in non-drug arrest rates.

The corresponding TWFE DID estimates are shown in the seventh column of Table 5. We obtain a significant increase in non-drug arrest rates for the full population that is driven by the significant increase in these arrests for Black persons. Relative to the pre-policy mean in RCL states, these effects correspond to a 7% increase for the full population and 9% increase for Black persons. Effects for White and Other Race populations are not statistically significant, although the point estimates suggest a 3 and 2% increase for each group.

The next two columns consider disparities in non-drug arrest rates. We find positive, large, and significant estimates for both the rate ratio and rate difference of the Black group relative to White persons. Relative to the pre-policy mean in RCL states, we estimate that disparities between Black and White populations increased by 4 to 13% in RCL states after cannabis liberalization. Effects for the Other Race group are much smaller and statistically insignificant.

To further understand these effects, we follow the FBI's classification of crimes for non-drug arrests. We distinguish between Part 1 and Part 2 offenses, with the former being more serious than the latter. Part 1 offenses are further broken down into violent and property crime. We show TWFE DID estimates for these categories in the last three columns of Table 6. We find insignificant results for Part 1 offenses. For violent crimes, point estimates are negative for White persons but positive for Black, although the magnitudes would suggest effects of less than 1% in absolute value. However, for property crimes, although statistically insignificant, the negative coefficient for White persons would imply a decline of less than 1% in property crime arrests, while the positive one for Black persons would be a 5% increase, although it is imprecisely estimated.

The last column shows estimates for less serious crimes. We find that the positive effects for the full and Black populations found for all non-drug arrests are driven precisely by these Part 2 offenses. We estimate an 8% average increase in arrest rates for this category for the full population, and a 12% significant increase for Black persons. While not statistically significant, the point estimate for White persons suggests a smaller increase of 4% in Part 2 offense arrest rates.

Online Appendix Figure S2 shows TWFE DID estimates for each of the crime categories that make up Part 1 and Part 2 offenses, distinguishing between White and Black persons. The estimates confirm that violent crime is unchanged after RCL implementation. For property crime, no single category is statistically significant. However, the negative coefficient for White persons shown in Table 6 is driven by negative estimates in burglary, while the positive coefficient for Black persons is driven by the positive estimate in the other theft category.

For Part 2 offenses, we obtain positive and significant increases in arrest rates for White persons for disorderly conduct, fraud, and simple assault. This is potentially offset by negative and significant estimates for curfew infractions/loitering (for minors only) and other sex offenses. For Black persons, we also estimate significant increases in arrest rates for disorderly conduct, fraud, and simple assault. However, we further find significant increases in arrest rates for vandalism and weapons offenses. We only find a significant decline in arrest rates for prostitution. Taken together, these results suggest that both White and Black populations are seeing increases in arrest rates for certain less serious crime categories, even if the total effect is only significant (and larger) for Black persons.

Total arrests. Putting together our previous estimates, the last three columns in Table 5 show results for total arrests, regardless of crime. Estimates for the arrest rates are all imprecisely estimated, as evidenced by the standard errors that are quite large. However, the point estimate for White persons is small, implying an increase of 0.4%, while the point estimate for Black persons is larger, indicating an increase of 6% in the total arrest rate. This difference leads to an estimated increase in disparities between these groups of between 3 and 10%. However, only the rate difference is significant at the 90% level. Online Appendix Figure S3 shows the corresponding event study plots. Overall, point estimates suggest an 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 groups (and equal to zero).

Prisoners. Our last set of law enforcement outcomes are the number of state and federal prisoners per 10,000 persons by race and ethnicity. These data allow us to distinguish between Hispanic and Non-Hispanic populations. Figure 7 shows event study plots for each group. We do not observe any striking patterns after RCL implementation in any of the groups.

Table 7 shows the parametrized TWFE DID estimates. The first column shows prisoner rates by population groups. None of the estimates are statistically significant. The point estimates for Non-Hispanic White and Non-Hispanic Other Race would imply a 4.5% and 4.8% decline relative to the baseline mean, respectively. The point estimate for Non-Hispanic Black is much smaller, especially relative to the high incarceration rates for this group. For Hispanic persons, the point estimate corresponds to a 3% increase in incarceration rates. For disparity measures, all point estimates are positive but mostly statistically insignificant. Overall, we do not identify significant RCL effects on prisoners, both statistically or in magnitude.

Robustness checks. We present a battery of robustness checks on our main results in the Online Appendix. First, Table S3 shows that the effects hold even when we do not include our vector of policy controls. Second, specifically for the arrests data, Table S4 considers alternative sample restrictions by imposing a higher threshold for the coverage indicator and by excluding outliers, defined as arrest counts that are larger than the county-level mean plus two times its standard deviation. Third, since we may be facing a small treated clusters problem when estimating our standard errors, Table S5 presents p-values calculated from wild cluster bootstrapped standard errors over 999 repetitions. Fourth, we show that the estimates are similar when we exclude one RCL state from the sample at a time in Table S6. This implies that our findings are not driven by one particular RCL state. Taken together, these tests provide reassurance that our main results hold under various alternative specifications and further support a causal interpretation.

To address the issues with staggered timing DID recently identified in the literature, we 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 (less than 3% for White and Black populations, and less than 5% for Other Race). Moreover, the sum of negative weights is very small. We also show the dynamic estimators proposed by De Chaisemartin and D'Haultfoeuille (2022) in Figures S4, S5, and S6. Results provide additional reassurance that the effects hold when accounting for heterogeneous average treatment effects. Overall, these exercises suggest that our estimates are not driven by negative weighting in the standard DID estimation.

5.3 Street violence

Homicides. Figure 8 shows event study plots for the effect of RCLs on homicide rates by race and ethnicity. For Non-Hispanic White, Non-Hispanic Other Race, and Hispanic persons, estimates are generally close to zero and statistically insignificant, suggesting a null impact of RCLs on homicides. For Non-Hispanic Black, we obtain negative and significant estimates post-policy, although there may have been a decreasing trend in some years prior to RCL implementation.

The first column in Table 8 shows the corresponding TWFE DID estimates. As shown in the plots, we obtain an almost zero estimate for the Non-Hispanic White group and a very small and insignificant estimate for Non-Hispanic Other Race. The estimated impact is negative but small and statistically insignificant for Hispanic persons. The only significant impact is a decline for Non-Hispanic Black homicides. Given the baseline mean in RCL states pre-policy, this implies a sizable decline of around 21%. The second and third columns show our measures of disparities. As expected, we estimate a significant decline in relative and absolute disparities between the Non-Hispanic White and Black groups. Relative to the baseline mean in RCL states, this represents a respective decline of 13.7% and 24%. For the other two population groups, implied effect sizes are smaller and estimates are not significant.

Hospitalizations. Figure 9 shows event study plots for hospitalizations due to assault by race and ethnicity. For Non-Hispanic White and Black persons, we do not find any discernible pattern post-implementation. For Hispanic hospitalizations, there is a slight decline starting in the second post-policy year, although the estimates are not statistically significant. Lastly, we observe an increase in Other Race hospitalizations after RCL implementation. However, most estimates are not significant and there seems to be a slight upward trend in this outcome during some of the pre-policy years.

The fourth column in Table 8 shows TWFE DID estimates of the effect of RCLs on hospitalization rates due to assault for different population groups. Only the estimate for Non-Hispanic Other Race is statistically significant, but these estimates should be interpreted with caution given the increase in pre-policy trends among Other Race in Figure 9. In contrast, estimates for all other groups are negative, with implied magnitudes of -2%, -3.7%, and 7%, for White, Black, and Hispanic persons, respectively. Except for Other Race Non-Hispanic, we find no significant effect for disparities relative to the Non-Hispanic White group. Furthermore, the implied magnitudes for the estimates for Black Non-Hispanic and Hispanic are small.

Event study plots for hospitalizations involving gun injury are shown in Figure 10. All point estimates are close to zero and insignificant for Non-Hispanic White and Non-Hispanic Other Race groups. However, we find significant declines post-implementation for both Non-Hispanic Black and Hispanic persons. In both cases, leads of the policy suggest that the parallel trends assumption holds. The seventh column in Table 8 presents parametrized estimates for these results. We find significant declines in the rate of hospitalizations involving gun injury for the Non-Hispanic Black and Hispanic groups, with average reductions of 12% and 13.8% relative to the baseline mean in RCL states, respectively. Furthermore, the last two columns show significant reductions in relative and absolute disparities for these two groups relative to Non-Hispanic White persons, ranging between 14.6% and 13.7% for Non-Hispanic Black persons, and 6 and 20.7% for Hispanic persons.

6 Discussion and policy implications

There are five key takeaways from our preceding analysis. First, RCL implementation was associated with substantial declines in arrest rates for cannabis sales and possession across all racial groups. These findings are consistent with the legalization of cannabis. Cannabis arrests did not disappear entirely, likely due to provisions of state RCLs restricting possession and sale limits (e.g., some types of cannabis products, possession over a threshold limit, and certain sales may still be illegal). We also documented sizable declines in relative and absolute disparities in cannabis arrest rates for Black populations relative to White populations, but these disparities did not disappear completely with cannabis legalization. These findings echo previous results for the general population, as well as findings by racial group in descriptive studies (Plunk et al., 2019; Edwards et al., 2020; Firth et al., 2019; Sheehan et al., 2021).

Second, we found significant declines in arrests for sales of other drugs, but small and insignificant reductions in arrests for possession of other drugs. This pattern was observed across all racial groups, and 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. Previous RCL research has documented declines in law enforcement seizures of illegal drugs, along with increases in the street prices of opioids and cocaine (Meinhofer and Rubli, 2021).

Third, RCL implementation was associated with statistically significant increases in arrests for non-drug crimes among Black populations. We also found positive but statistically insignificant estimates for White and Other Race populations. Complementary analyses showed that these effects were driven by (less serious) Part 2 offenses, and particularly, by disorderly conduct and simple assault for both White and Black persons. There are at least three potential explanations for these findings. One is a mechanical effect due to the UCR data recording only the highest offense for each incident. Therefore, if cannabis arrests involve multiple offenses within a same incident, the increase in non-drug arrests may simply be driven by how this incident is tallied in the data after RCL implementation. According to Hendrix and Martin (2019), 39% of drug violations are multiple-offense incidents. Around two thirds of these correspond to additional drug violations, while 19% of them involve public order, fraud, and other violent offenses. As such, only about 13% of incidents involving a drug offense also involved a non-drug offense and 7% involved less serious offenses. [5] Back-of-the-envelope calculations would then suggest that around 0.6 and 1.8 cannabis arrests per

[5] Hendrix and Martin (2019) compares the UCR data with NIBRS (the FBI's National Incident-Based Reporting System), grouping offenses into broader categories. Their definition of public order offenses includes prostitution, gambling, weapons violations, and disorderly conduct, while other violent offenses include simple assault, intimidation, and kidnapping. They also do not distinguish between cannabis and other drug violations.

10,000 persons would be reclassified as disorderly conduct, fraud, or simple assault for White and Black populations, respectively. However, we found an increase of around 7.7 and 36.1 disorderly conduct, fraud, or simple assault arrests per 10,000 for these groups, suggesting that very little of these shifts can be explained as purely mechanical.[6] Moreover, as outlined above, we found *declines* in other drug arrests, which is inconsistent with the mechanical tallying argument.

A second explanation is the continued profiling of Black persons and communities, which may reflect lower policing costs, statistical discrimination, or taste-based discrimination. Lastly, a third explanation is increases in the incidence of simple assaults, disorderly conduct, and other low level crimes, which may result from increases in cannabis use along with the psychoactive effects of cannabis. This potential pathway, however, is inconsistent with our null findings for arrests involving public intoxication and driving under the influence, as well as for assaults when using hospital data. While we cannot disentangle the mechanism behind these estimates, these alarming findings raise the question of whether incentives in law enforcement are such that RCLs increase disparities in arrest rates for Part 2 offenses, and suggests that additional policies that curtail this effect may be necessary. At the very least, our findings shed light on how RCLs are not a silver bullet for decreasing racial disparities in police contact and our results are aligned with previous studies on racial disparities in policing outcomes where law enforcement officers can exercise discretion such as fines, ticketing, and arrests for less serious crimes (West, 2018; Goncalves and Mello, 2021; Feigenberg and Miller, 2022; Chalfin et al., 2022).

Fourth, total arrest rates and rate differences were small and statistically insignificant for White and Other Race populations, but were positive, more sizable, and statistically significant at the 90% level for Black populations. This suggests a reshuffling in arrests across offense categories, with increases in Part 2 offenses offsetting arrest declines for cannabis possession and sales. Moreover, we did not find any effects on total incarceration for either the rates or disparities in the rates across groups. This suggests that there is no change in the rate at which arrests lead to incarceration. However, we cannot observe whether

[6] From the point estimates alone, it seems effects are not equal for White and Black persons: the total effect of RCLs on disorderly conduct, fraud, and simple assault is about 12 times larger than the potential mechanical reclassification for White persons, but 20 times larger for Black individuals.

this is driven by changes in prosecution (i.e., taking an arrestee to trial) or sentencing. It is important to note that Part 2 offenses are less likely to result in imprisonment greater than one year. Therefore, even if racial disparities in total arrests increase due to arrests for less serious crimes, it is unlikely that it would contribute to racial disparities in federal and state prison statistics. However, contact with the criminal legal system, even for less serious offenses, can disrupt human capital development and labor market attachment, and therefore, increase racial disparities in economic outcomes (Dobbie et al., 2018). At the very least, our results indicate that RCLs do not decrease racial disparities in total arrests or incarceration.

Lastly, RCL implementation was associated with some statistically significant reductions in violence, as measured by declines in homicides and hospitalizations involving gun injury. These findings align with previous studies documenting crime reductions in the overall population following the implementation of RCLs, MCLs, and CDLs (Dragone et al., 2019; Brinkman and Mok-Lamme, 2019; Wu et al., 2020; Morris et al., 2014; Huber III et al., 2016; Chu and Townsend, 2019; Gavrilova et al., 2019; Anderson and Rees, 2021). Declines in homicides and gun injury were concentrated among Black persons. About 79% of all homicides involve a gun, and this rate is nearly 90 percent for Black homicide victimization (for Gun Violence Solutions, 2022). Due to the fact that Black persons are over-represented in gun violence, it is not surprising that the decrease in violence primarily occurred in this group, with a smaller but significant decline in Hispanic communities. The reduction in violence suggests that cannabis legalization may lead to some improvements in safety, possibly because of reductions in the size of illegal drug markets, which is also consistent with declines in arrest rates for sales of cannabis and other drugs outlined above. The decline in violence is likely driven by changes in policing strategy rather than changes in labor market outcomes. The qualitative pattern of post-policy effects in Figure 10 essentially mirrors post-policy effects in Figure 5. Moreover, changes in gun violence and arrests for non-drug crimes are immediate, and labor market dynamics would likely adjust slowly due to historical differences in labor market attachment.[7]

[7]Persons of color face important barriers to opening a dispensary or cultivation business, including licensing restrictions, compliance costs, and limited access to capital (Quinton, 2021). Although legislation in certain states, such as Illinois, has explicitly included social equity provisions, access to the legal cannabis

Our findings have important implications for cannabis policy design. First, we show that cannabis liberalization is not a silver bullet for reducing disparities, particularly when considering the entire criminal legal system and not just the narrow outcome of cannabis arrests. Second, our results emphasize the need for changing incentives for law enforcement agencies, such as 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. Beyond making police profiling explicitly illegal, reshaping incentives are fundamental for reducing disparities. Third, and related to our second point, our estimates bring attention to the importance of reforming local police agencies by implementing oversight mechanisms that are sensitive to racial disparities and to the factors that may perpetuate or increase them. In particular, increased enforcement through discretionary policing exacerbates racial differences in arrests. Reducing discriminatory discretionary policing will reduce not only racial disparities in the criminal legal system but also racial disparities in police use of force incidents since disparities in police violence are driven by differences in police contact (Weisburst, 2019). Fourth, since Black persons are disproportionately arrested, prosecuted, and jailed for drug offenses, RCLs will not be enough to reduce current disparities in prison populations. Hence, legislators could make liberalization retroactive by granting clemency, overturning convictions, expunging cannabis arrest records, and bypassing habitual-offender laws that overly affect minorities. Lastly, the economic benefits of cannabis liberalization have not spilled over to marginalized groups, as evidenced by the low number of Black and Hispanic legal cannabis business owners. It may be possible that reducing barriers for persons of color to participate in legal cannabis businesses may lead to even larger declines in street violence outcomes, such as homicides and gun injuries, by further shrinking the size of the illegal market.

While our results are generally robust across specifications, a few key limitations and open questions remain. First, we observe the outcomes from policing efforts in the form of arrest rates for various categories of crime, but we cannot directly observe how resources are allocated by law enforcement agencies nor the incentives that police may face. For instance,

market still skews largely toward White persons. These restrictions suggest a slower diffusion of improved labor market opportunities to Black communities.

we do not know whether their objective is to decrease criminal activity or to comply with a fixed number of arrests per year. As such, we only estimate the impacts of RCLs on arrest rates but cannot pin down the mechanisms in terms of potential changes in policing strategies. Second, we only observe counts of incarcerated persons, but cannot observe the length of sentence, types of crime that led to conviction, or other outcomes like eligibility for parole. Hence, we might be missing important ways in which incarcerations are changing after RCL implementation. We also cannot observe prosecutorial decisions after arrests are made. Lastly, we only observe instances of street violence that led to hospitalization or death. Both are relatively extreme outcomes, such that violence may be changing with RCLs in ways unobservable in the data. Lastly, since RCLs have only been adopted by a few states in recent years, our estimates may not generalize in the long term or for future RCLs.

Overall, while the War on Drugs and the illegal status of cannabis was, in theory, a race-blind policy, decades of racially charged implementation and enforcement have led to huge disparities in criminal justice outcomes. While cannabis liberalization may be an important step toward addressing these and other disparities and injustices in the criminal legal system, designing policies and provisions that specifically address these issues, as outlined above, will be crucial for guaranteeing that RCLs do not replicate these deep-rooted injustices.

7 Conclusion

Racial and ethnic disparities in law enforcement of drug prohibition are widespread and longstanding, with Black communities being disproportionately affected. Understanding the effect of cannabis legalization on these disparities is crucial for designing successful policies that work in reparative ways. This study provides the most comprehensive evidence to date of the overall effects of RCLs on racial and ethnic disparities in the criminal legal system and other consequences related to street violence.

Our estimates suggest that RCLs led to a decrease in cannabis arrests, reducing—but not entirely eliminating—disparities between Black and White populations. We also found declines in arrests for sales of other drugs but not for possession. However, these declines were offset by similar increases in non-drug arrests, driven by less serious offenses, which led to an increase in disparities between Black and White populations. Overall, we find zero or limited effects on total arrest rates and total incarceration rates. For outcomes related to violence, we find significant declines in homicides and gun injuries for Black persons, leading to a reduction—though not fully eliminating—disparities between Non-Hispanic Black and White populations.

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Figures and Tables

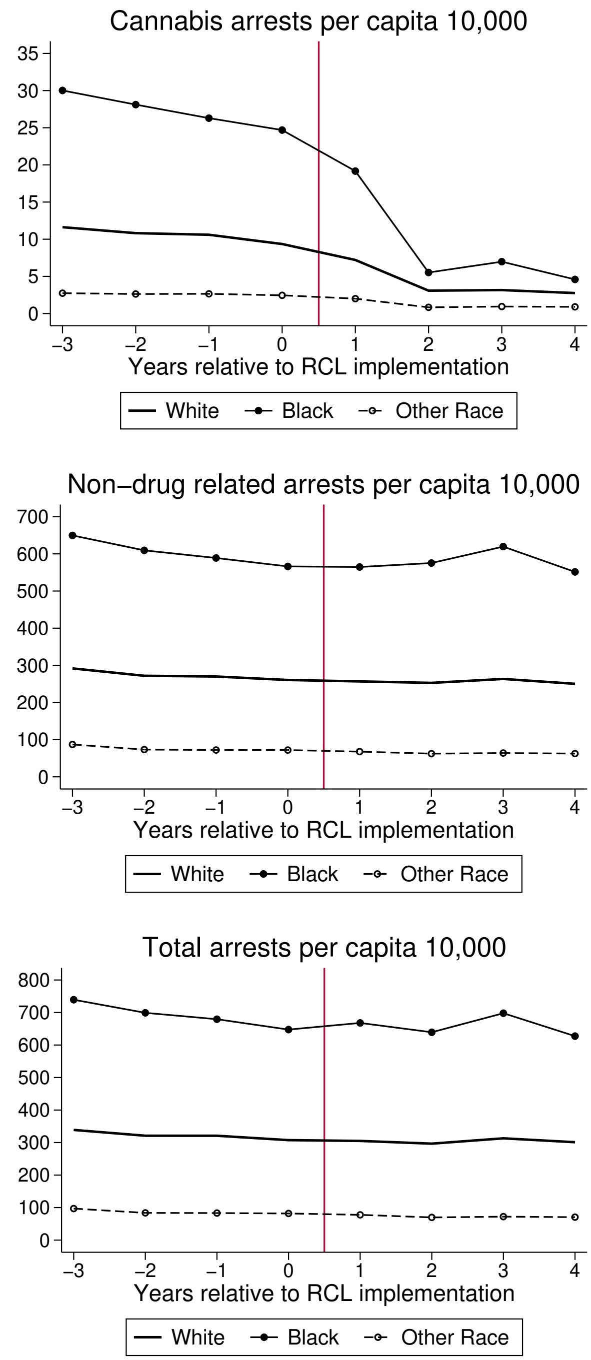


Figure 1: Arrest rates, by time since RCL implementation

Notes: Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. Counts for a given category 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 years relative to RCL implementation. The year $t = 0$ corresponds to the period immediately before RCL implementation. RCL=Recreational cannabis laws.

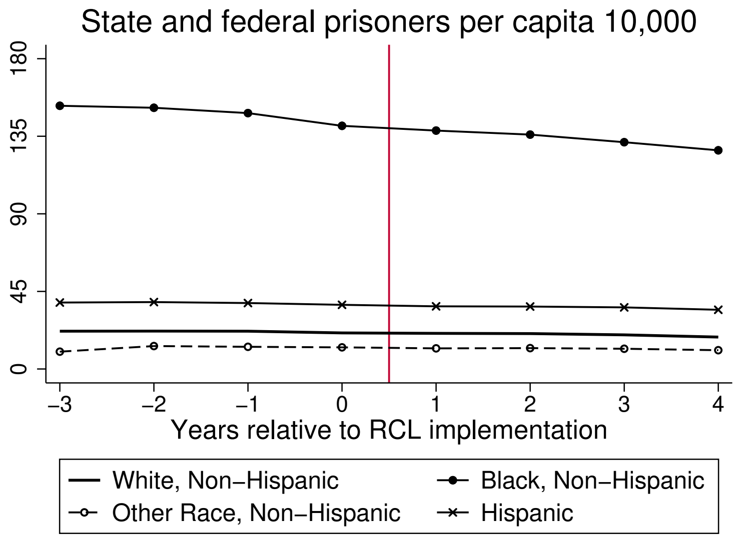


Figure 2: Prisoner rates, by time since RCL implementation

Notes: Data are from the 2009-2019 National Prisoner Statistics. The unit of analysis is a state-year. Counts for a given race/ethnicity are divided by state-year population estimates corresponding to that race/ethnicity, and multiplied by 10,000. The year $t = 0$ corresponds to the period immediately before RCL implementation. RCL=Recreational cannabis laws.

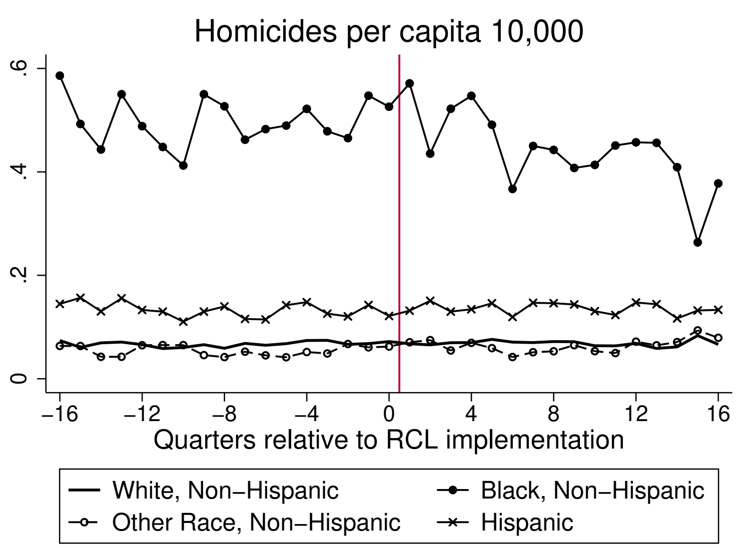


Figure 3: Homicide rates, by time since RCL implementation

Notes: Data are from the 2007-2019 NVSS Mortality Files. The unit of analysis is a state-year-quarter. Counts for a given race or ethnicity are divided by state-year population estimates corresponding to that race or ethnicity, and multiplied by 10,000. The year $t = 0$ corresponds to the period immediately before RCL implementation. RCL=Recreational cannabis laws.

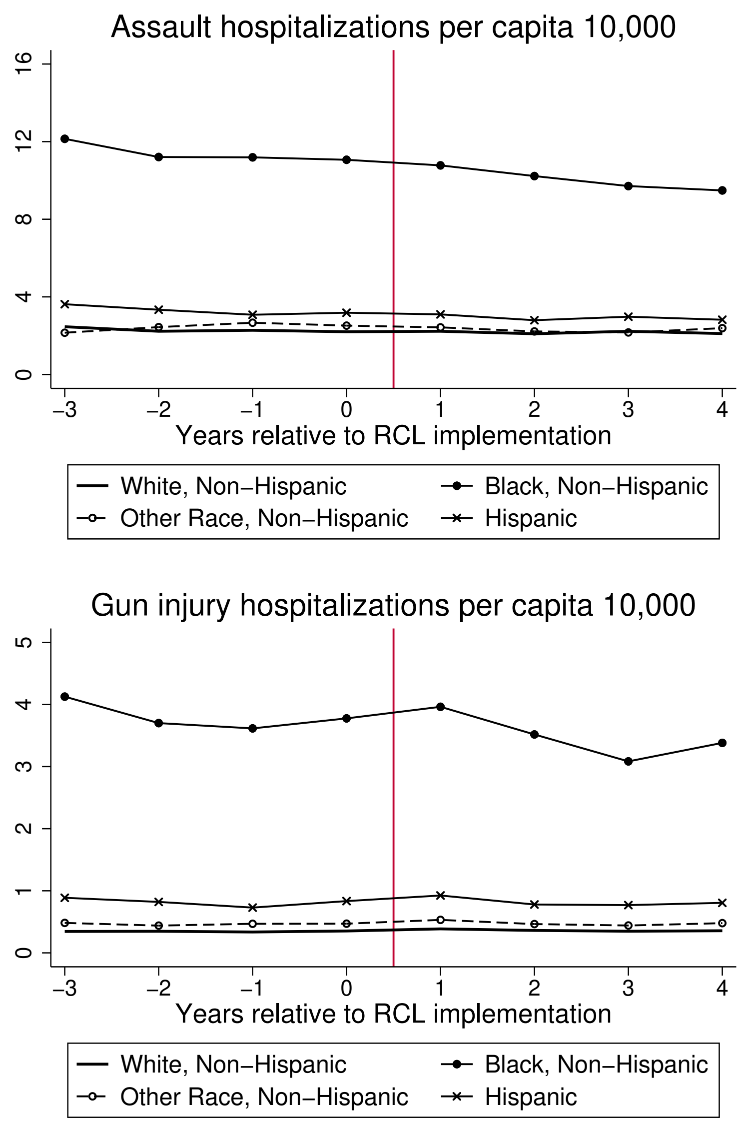


Figure 4: Hospitalization rates, by time since RCL implementation

Notes: Data are from the 2007-2019 HCUP-SID. The unit of analysis is a state-year. Counts for a given race or ethnicity are divided by state-year population estimates corresponding to that race or ethnicity, and multiplied by 10,000. The year $t = 0$ corresponds to the period immediately before RCL implementation. RCL=Recreational cannabis laws.

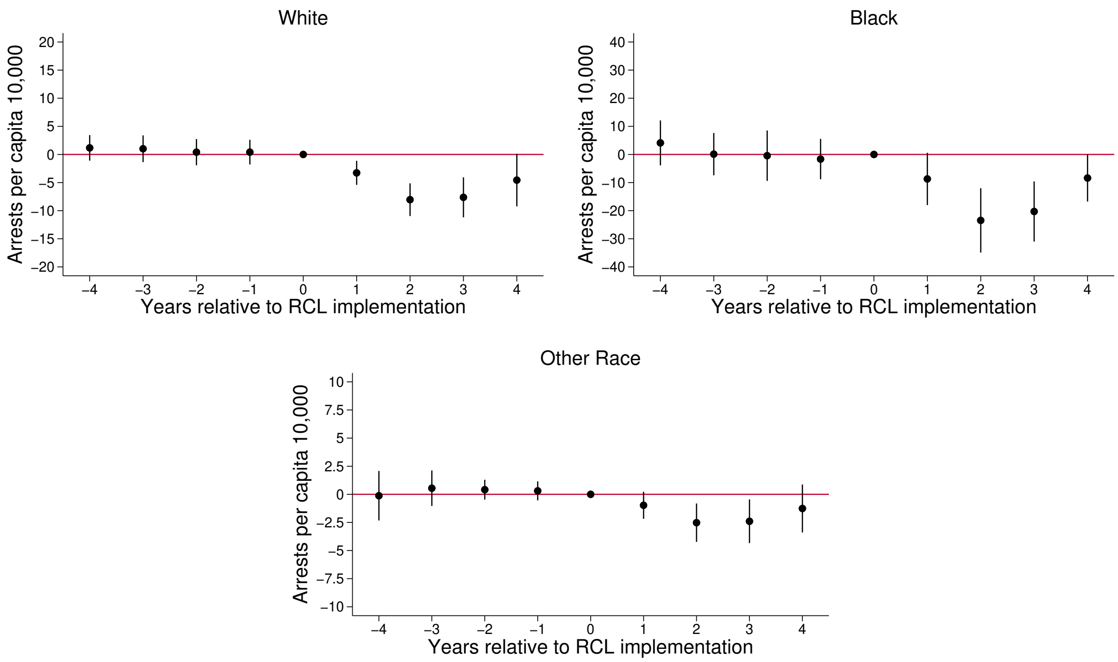


Figure 5: Effect of recreational cannabis laws on cannabis arrests, event study

Notes: Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. 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 that estimates leads and lags of the intervention and that accounts for controls (see equation 2 in the text). Regressions are weighted by race-specific population. Controls include the number of reporting agencies, unemployment rates, medical cannabis laws, and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

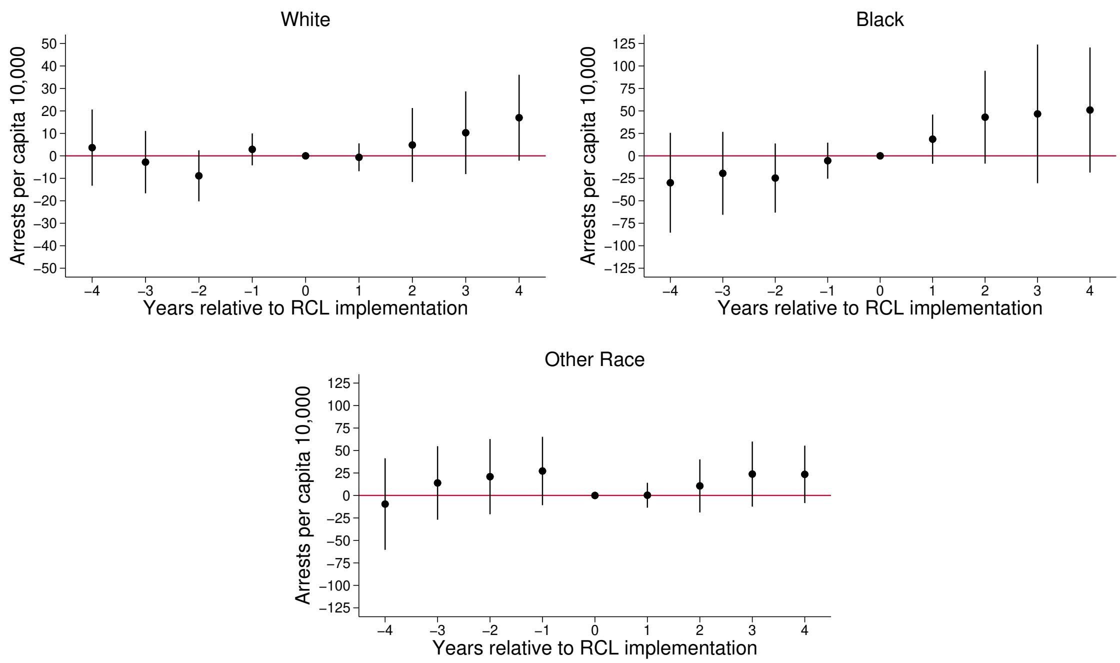


Figure 6: Effect of recreational cannabis laws on non-drug arrests, event study

Notes: Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. 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 that estimates leads and lags of the intervention and that accounts for controls (see equation 2 in the text). Regressions are weighted by race-specific population. Controls include the number of reporting agencies, unemployment rates, medical cannabis laws, and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

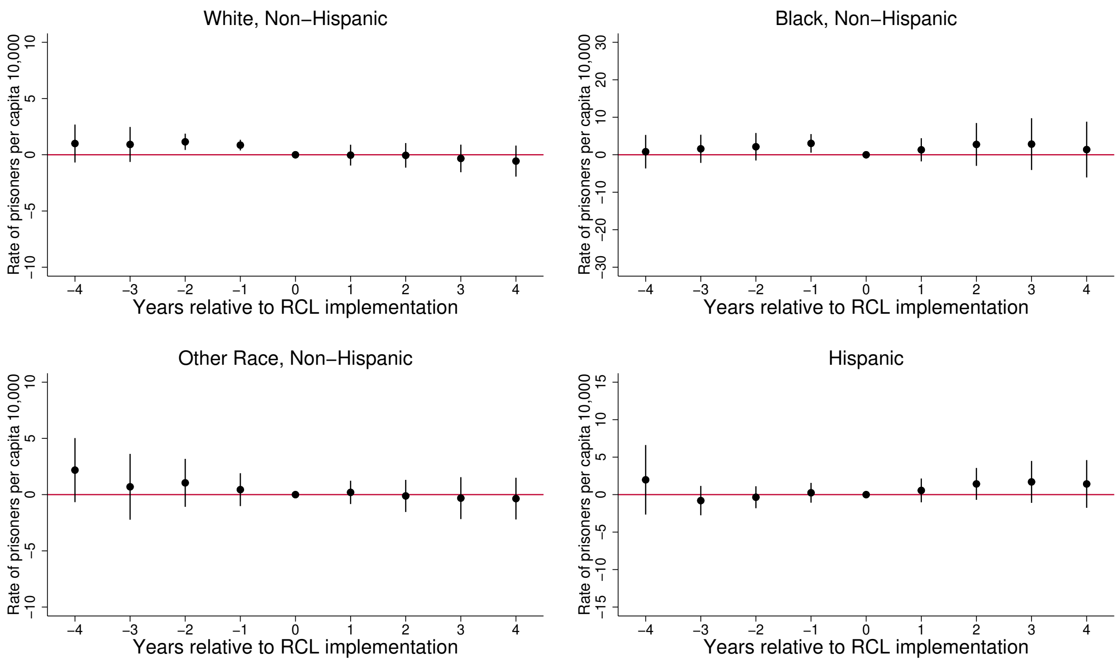


Figure 7: Effect of recreational cannabis laws on state and federal prisoners, event study

Notes: Data are from the 2009-2019 National Prisoner Statistics. The unit of analysis is a state-year. Counts for a given racial/ethnic group are divided by state-year population estimates corresponding to that racial/ethnic group 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 (see equation 2 in the text). Regressions are weighted by race/ethnicity-specific population. Controls include unemployment rates, medical cannabis laws, and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

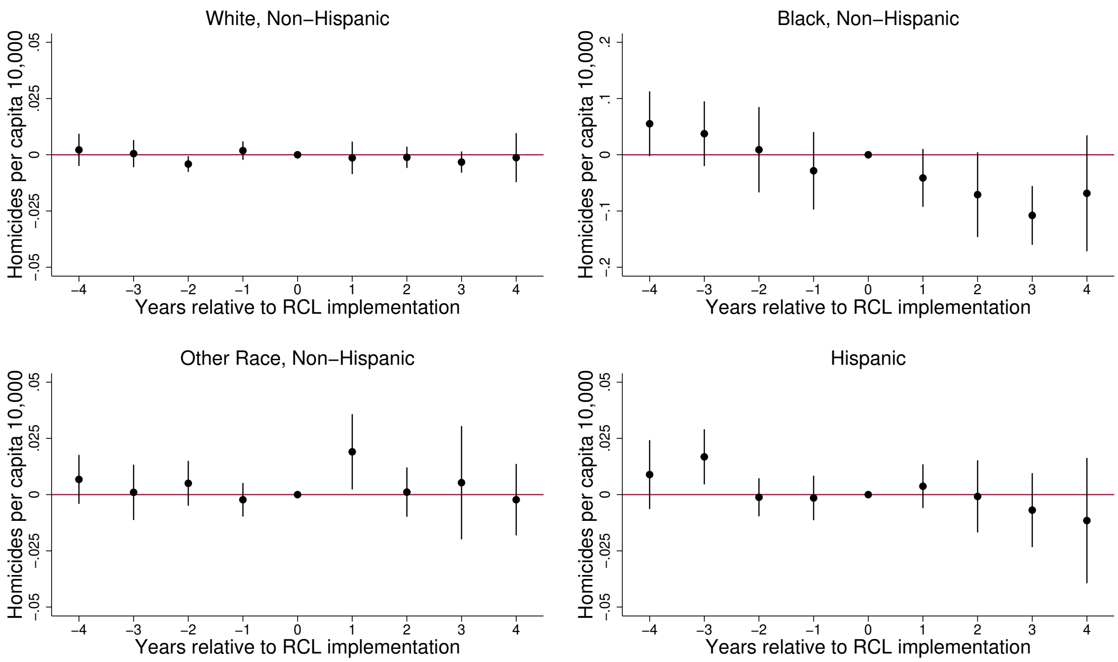


Figure 8: Effect of recreational cannabis laws on homicides, event study

Notes: Data are from the 2007-2019 NVSS Mortality Files. The unit of analysis is a state-year-quarter. Counts for a given racial/ethnic group are divided by state-year population estimates corresponding to that racial/ethnic group, 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 (see equation 2 in the text). Regressions are weighted by race-specific population. Controls include unemployment rates, medical cannabis laws, and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

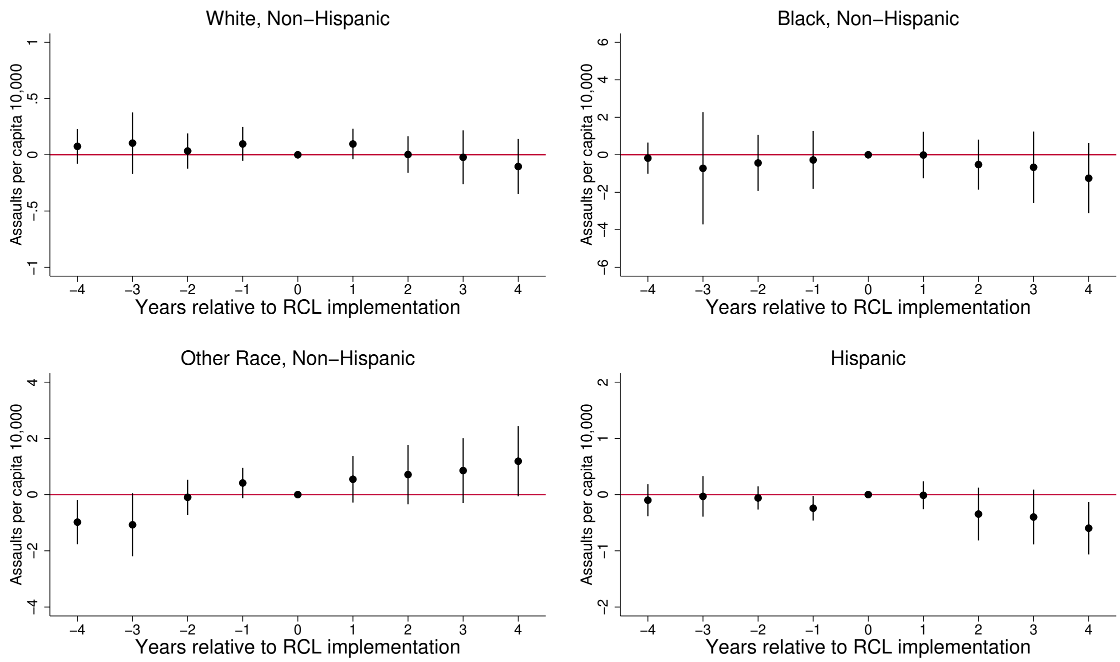


Figure 9: Effect of recreational cannabis laws on assault hospitalizations, event study

Notes: Data are from the 2007-2019 HCUP-SID. The unit of analysis is a state-year. Counts for a given racial/ethnic group are divided by state-year population estimates corresponding to that racial/ethnic group, 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 (see equation 2 in the text). Regressions are weighted by race-specific population. Controls include unemployment rates, medical cannabis laws, and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

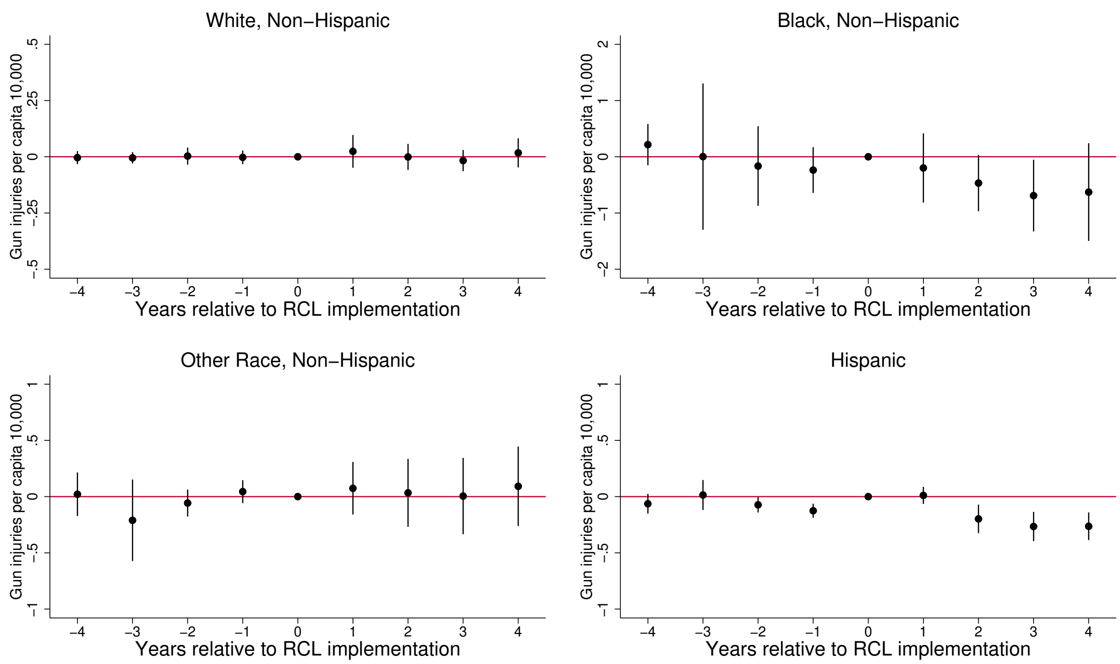


Figure 10: Effect of recreational cannabis laws on gun injury hospitalizations, event study

Notes: Data are from the 2007-2019 HCUP-SID. The unit of analysis is a state-year. Counts for a given racial/ethnic group are divided by state-year population estimates corresponding to that racial/ethnic group, 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 (see equation 2 in the text). Regressions are weighted by race-specific population. Controls include unemployment rates, medical cannabis laws, and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

Table 1: Summary statistics for arrests, 2007-2019

	RCL States							
	Pre-Policy		Post-Policy		Non-RCL States		All States	
	Mean	N	Mean	N	Mean	N	Mean	N
Rate of arrests per 10,000 persons								
Population								
Cannabis arrests	15.40	1866	4.24	950	26.97	18000	22.49	20816
Other drug arrests	36.18	1866	36.38	950	23.45	18000	27.18	20816
Total non-drug arrests	300.58	1866	261.65	950	334.53	18000	320.77	20816
Total arrests	352.16	1866	302.27	950	385.10	18000	370.53	20816
White								
Cannabis arrests	14.55	1866	4.21	950	21.00	18000	18.07	20816
Other drug arrests	36.93	1866	39.06	950	20.05	18000	25.22	20816
Total non-drug arrests	297.47	1866	260.80	950	281.22	18000	282.32	20816
Total arrests	348.95	1866	304.07	950	322.40	18000	325.69	20816
Black								
Cannabis arrests	40.95	1866	9.28	950	62.64	18000	57.22	20816
Other drug arrests	72.57	1866	61.08	950	46.15	18000	50.10	20816
Total non-drug arrests	653.32	1866	596.97	950	647.98	18000	645.92	20816
Total arrests	766.83	1866	673.20	950	757.05	18000	753.79	20816
Other Race								
Cannabis arrests	2.48	1866	1.33	950	7.76	18000	5.12	20816
Other drug arrests	5.96	1866	7.21	950	5.92	18000	6.15	20816
Total non-drug arrests	67.52	1866	76.24	950	165.63	18000	121.61	20816
Total arrests	75.96	1866	84.78	950	179.34	18000	132.89	20816
Ratio of arrest rates (relative to White)								
Black								
Cannabis arrests	3.13	1831	2.97	893	3.81	17889	3.68	20613
Other drug arrests	2.61	1834	2.03	915	2.59	17927	2.56	20670
Total non-drug arrests	2.57	1847	2.71	941	2.69	17964	2.68	20752
Total arrests	2.57	1847	2.63	941	2.72	17964	2.69	20752
Other Race								
Cannabis arrests	0.23	1831	0.56	893	0.45	17889	0.40	20613
Other drug arrests	0.16	1834	0.23	915	0.40	17927	0.30	20670
Total non-drug arrests	0.24	1847	0.34	941	0.59	17964	0.44	20752
Total arrests	0.23	1847	0.33	941	0.56	17964	0.42	20752
Differences in arrest rates (relative to White)								
Black								
Cannabis arrests	27.76	1866	5.95	950	44.90	18000	40.79	20816
Other drug arrests	39.45	1866	24.81	950	26.19	18000	27.71	20816
Total non-drug arrests	390.56	1866	366.40	950	401.21	18000	398.10	20816
Total arrests	457.76	1866	403.04	950	472.51	18000	467.07	20816
Other Race								
Cannabis arrests	-10.16	1866	-1.25	950	-10.38	18000	-8.76	20816
Other drug arrests	-37.92	1866	-36.88	950	-11.08	18000	-23.35	20816
Total non-drug arrests	-225.98	1866	-155.34	950	-78.78	18000	-135.00	20816
Total arrests	-274.05	1866	-193.47	950	-100.30	18000	-167.14	20816

Notes: Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. Averages and sample size shown. Averages are weighted by race-specific population. Sample restricted to counties with an agency reporting coverage threshold above or equal to 65%. RCL=Recreational cannabis laws.

Table 2: Summary statistics for state and federal prisoners, 2009-2019

	RCL States							
	Pre-Policy		Post-Policy		Non-RCL States		All States	
	Mean	N	Mean	N	Mean	N	Mean	N
Rate of prisoners per 10,000 persons								
Population	35.36	63	31.41	44	44.64	440	42.21	547
White, Non-Hispanic	22.36	63	20.55	44	28.76	440	27.18	547
Black, Non-Hispanic	146.77	63	130.24	44	139.69	440	139.54	547
Other Race, Non-Hispanic	13.34	63	14.34	44	12.53	439	12.81	546
Hispanic	38.77	42	35.95	39	35.77	383	36.09	464
Ratio of prisoner rates (relative to White, Non-Hispanic)								
Black, Non-Hispanic	6.89	63	6.66	44	5.67	440	5.91	547
Other Race, Non-Hispanic	0.55	63	0.63	44	0.52	439	0.53	546
Hispanic	2.13	42	1.94	39	1.57	383	1.67	464
Differences in prisoner rates (relative to White, Non-Hispanic)								
Black, Non-Hispanic	124.41	63	109.68	44	110.92	440	112.36	547
Other Race, Non-Hispanic	-9.02	63	-6.21	44	-16.24	439	-14.38	546
Hispanic	16.97	42	15.45	39	8.02	383	9.75	464

Notes: Data are from the 2009-2019 National Prisoner Statistics. The unit of analysis is a state-year. Averages and sample size shown. Averages are weighted by race/ethnicity-specific population. RCL=Recreational cannabis laws.

Table 3: Summary statistics for homicides, 2007-2019

	RCL States							
	Pre-Policy		Post-Policy		Non-RCL States		All States	
	Mean	N	Mean	N	Mean	N	Mean	N
Rate of homicides per 10,000 persons								
Population	0.13	395	0.12	177	0.15	2080	0.14	2652
White, Non-Hispanic	0.07	395	0.07	177	0.07	2080	0.07	2652
Black, Non-Hispanic	0.55	395	0.45	177	0.51	2080	0.51	2652
Other Race, Non-Hispanic	0.06	395	0.07	177	0.08	2080	0.07	2652
Hispanic	0.15	395	0.14	177	0.13	2080	0.14	2652
Ratio of homicide rates (relative to White, Non-Hispanic)								
Black, Non-Hispanic	9.33	370	7.05	167	8.60	2055	8.60	2592
Other Race, Non-Hispanic	0.98	370	0.99	167	1.17	2055	1.13	2592
Hispanic	2.60	370	2.30	167	2.20	2055	2.28	2592
Differences in homicide rates (relative to White, Non-Hispanic)								
Black, Non-Hispanic	0.48	395	0.38	177	0.44	2080	0.44	2652
Other Race, Non-Hispanic	-0.01	395	-0.00	177	0.00	2080	0.00	2652
Hispanic	0.08	395	0.07	177	0.06	2080	0.07	2652

Notes: Data are from 2007-2019 NVSS Mortality Files. The unit of analysis is a state-year-quarter. Averages and sample size shown. Averages are weighted by race/ethnicity-specific population. RCL=Recreational cannabis laws.

Table 4: Summary statistics for hospitalizations, 2007-2019

	RCL States							
	Pre-Policy		Post-Policy		Non-RCL States		All States	
	Mean	N	Mean	N	Mean	N	Mean	N
Rate of hospitalizations per 10,000 persons								
Assault								
Population	3.64	74	2.87	48	3.28	254	3.31	376
White, Non-Hispanic	2.44	76	2.04	49	1.97	246	2.08	371
Black, Non-Hispanic	12.68	57	9.35	44	8.74	246	9.61	347
Other Race, Non-Hispanic	2.38	57	2.34	44	4.66	246	3.93	347
Hispanic	3.76	57	2.75	44	2.86	246	3.03	347
Gun injuries								
Population	0.89	74	0.77	48	1.01	254	0.96	376
White, Non-Hispanic	0.36	76	0.38	49	0.49	245	0.45	370
Black, Non-Hispanic	4.38	57	3.34	44	3.58	245	3.72	346
Other Race, Non-Hispanic	0.52	57	0.53	44	1.16	245	0.96	346
Hispanic	0.94	57	0.82	44	0.71	245	0.77	346
Ratio of hospitalization rates (relative to White, Non-Hispanic)								
Assault								
Black, Non-Hispanic	5.59	57	4.57	44	4.72	246	4.88	347
Other Race, Non-Hispanic	1.13	57	1.23	44	2.48	246	2.07	347
Hispanic	1.54	57	1.34	44	1.53	246	1.51	347
Gun injuries								
Black, Non-Hispanic	13.84	57	9.81	44	10.62	245	11.18	346
Other Race, Non-Hispanic	1.73	57	1.44	44	2.90	245	2.49	346
Hispanic	2.99	57	2.55	44	1.93	245	2.22	346
Differences in hospitalization rates (relative to White, Non-Hispanic)								
Assault								
Black, Non-Hispanic	10.21	57	7.30	44	6.76	246	7.52	347
Other Race, Non-Hispanic	-0.10	57	0.29	44	2.69	246	1.85	347
Hispanic	1.29	57	0.70	44	0.88	246	0.94	347
Gun injuries								
Black, Non-Hispanic	4.02	57	2.97	44	3.10	245	3.27	346
Other Race, Non-Hispanic	0.16	57	0.15	44	0.67	245	0.51	346
Hispanic	0.58	57	0.44	44	0.23	245	0.32	346

Notes: Data are from the 2007-2019 HCUP-SID. The unit of analysis is a state-year. Averages and sample size shown. Averages are weighted by race/ethnicity-specific population. RCL=Recreational cannabis laws.

Table 5: Effect of recreational cannabis laws on arrests

	Cannabis Arrests			Other Drug Arrests			Non-Drug Arrests			Total Arrests		
	Rate	Rate Ratio	Rate Difference	Rate	Rate Ratio	Rate Difference	Rate	Rate Ratio	Rate Difference	Rate	Rate Ratio	Rate Difference
Population	-7.006***	n.a.	n.a.	-1.106	n.a.	n.a.	21.65*	n.a.	n.a.	13.79	n.a.	n.a.
	(2.27)			(0.90)			(11.28)			(11.07)		
White	-6.95***	n.a.	n.a.	-1.448	n.a.	n.a.	9.42	n.a.	n.a.	1.25	n.a.	n.a.
	(2.07)			(1.00)			(10.51)			(10.35)		
Black	-18.08***	-0.29**	-13.44***	-3.21	0.21	-2.47	61.87**	0.11**	51.49**	49.01*	0.08	43.82*
	(6.56)	(0.13)	(4.99)	(2.52)	(0.13)	(2.22)	(26.76)	(0.05)	(19.38)	(28.88)	(0.06)	(22.50)
Other Race	-2.053*	0.11***	3.03**	-0.99	0.02	-1.15	1.09	0.02	-14.19	-1.91	0.03	-12.43
	(1.04)	(0.04)	(1.24)	(0.79)	(0.03)	(0.94)	(21.87)	(0.07)	(16.44)	(23.04)	(0.065)	(17.14)

Notes: Effect of recreational cannabis laws on arrest rates, rate ratios, and rate differences, by race. Ethnicity is not available. Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, where 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. 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 regressions (see equation 1 in the text). Regressions are weighted by race-specific population. All regressions include county and year fixed effects. Control variables include the number of reporting agencies, unemployment rates, medical cannabis laws, and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Effect of recreational cannabis laws on arrests, by different crime categories

	Drug Possession		Drug Sales		Part 1		Part 2
	Cannabis	Other Drugs	Cannabis	Other Drugs	Violent	Property	
Population	-5.972**	-0.178	-1.034***	-0.927***	0.168	2.119	19.357*
	(2.265)	(0.794)	(0.198)	(0.277)	(0.596)	(1.616)	(10.671)
White	-5.877***	-0.247	-1.070***	-1.201***	-0.189	-0.246	9.855
	(2.124)	(0.872)	(0.169)	(0.274)	(0.406)	(1.619)	(9.954)
Black	-13.961**	-0.246	-4.120***	-2.964***	0.513	4.681	56.674**
	(6.407)	(1.932)	(0.618)	(0.961)	(2.937)	(4.408)	(24.008)
Other Race	-1.669*	-0.671	-0.384***	-0.315*	-0.854	1.076	0.871
	(0.969)	(0.705)	(0.124)	(0.158)	(1.084)	(1.710)	(19.972)

Notes: Effect of recreational cannabis laws on arrest rates for select crime categories, by race. See text for definitions of Part 1 and Part 2 offenses. Ethnicity is not available. Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, where 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 restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each coefficient is based on separate two-way fixed effects regressions (see equation 1 in the text). Regressions are weighted by race-specific population. All regressions include county and year fixed effects. Control variables include the number of reporting agencies, unemployment rates, medical cannabis laws, and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Effect of recreational cannabis laws on state and federal prisoners

	Rate	Rate Ratio	Rate Difference
Population	0.56	n.a.	n.a.
	(0.82)		
White, Non-Hispanic	-1.01	n.a.	n.a.
	(0.82)		
Black, Non-Hispanic	0.85	0.12	1.47
	(2.98)	(0.14)	(2.5)
Other Race, Non-Hispanic	-0.64	0.02	0.14
	(1.16)	(0.02)	(0.78)
Hispanic	1.28	0.08	2.19*
	(1.49)	(0.05)	(1.27)

Notes: Effect of recreational cannabis laws on prisoner rates, rate ratios, and rate differences, by race and ethnicity. Data are from the 2009-2019 National Prisoner Statistics, where the unit of analysis is a state-year. Counts for a given race/ethnicity are divided by state-year population estimates corresponding to that race/ethnicity and multiplied by 10,000. Rate ratios and rate differences are relative to the White, Non-Hispanic group. Each coefficient is based on separate two-way fixed effects regressions (see equation 1 in the text). Regressions are weighted by race-specific population. All regressions include state and year fixed effects. Control variables include unemployment rates, medical cannabis laws, and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Effect of recreational cannabis laws on street violence

	Homicides			Assault Hospitalizations			Gun Injury Hospitalizations		
	Rate	Rate Ratio	Rate Difference	Rate	Rate Ratio	Rate Difference	Rate	Rate Ratio	Rate Difference
Population	-0.017**	n.a.	n.a.	-0.01	n.a.	n.a.	-0.06	n.a.	n.a.
	(0.008)			(0.13)			(0.04)		
White, Non-Hispanic	-0.000	n.a.	n.a.	-0.05	n.a.	n.a.	0.01	n.a.	n.a.
	(0.003)			(0.08)			(0.02)		
Black, Non-Hispanic	-0.118***	-1.282**	-0.117***	-0.48	-0.12	-0.45	-0.53*	-2.03*	-0.55**
	(0.041)	(0.556)	(0.040)	(0.56)	(0.30)	(0.50)	(0.27)	(1.15)	(0.26)
Other Race, Non-Hispanic	0.008	0.074	0.008	1.04**	0.43**	1.12**	0.09	0.20	0.09
	(0.009)	(0.062)	(0.007)	(0.50)	(0.18)	(0.46)	(0.13)	(0.29)	(0.13)
Hispanic	-0.006	-0.006	-0.009	-0.26	0.03	-0.07	-0.13**	-0.19	-0.12**
	(0.008)	(0.123)	(0.009)	(0.17)	(0.08)	(0.14)	(0.06)	(0.14)	(0.05)

Notes: Effect of recreational cannabis laws on rates, rate ratios, and rate differences, by race and ethnicity, for different outcomes related to street violence. Homicide data are from 2007-2019 NVSS Mortality files, where the unit of analysis is a state-year-quarter. Hospital discharge data are from 2007-2019 HCUP-SID, where the unit of analysis is a state-year. Counts for a given race or ethnicity are divided by state-year population estimates corresponding to that race or ethnicity, and multiplied by 10,000. Rate ratios and rate differences are relative to the White, Non-Hispanic group. Each coefficient is based on separate two-way fixed effects regressions (see Equation 1 in the text). Regressions are weighted by race-specific population. All regressions include state and time period (i.e., year-quarter in the first three columns, and year in the remaining columns) fixed effects. Control variables include unemployment rates, medical cannabis laws, and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Supplementary Materials

Table S1: Recreational cannabis laws as of 2019

State	Effective date
Alaska	2/24/2015
California	11/9/2016
Colorado	12/10/2012
District of Columbia	2/26/2015
Maine	1/30/2017
Massachusetts	12/15/2016
Michigan	12/6/2018
Nevada	1/1/2017
Oregon	7/1/2015
Vermont	7/1/2018
Washington	12/6/2012

Notes: Effective dates of implementation of recreational cannabis laws as of 2019 by state. Information taken from ProCon (2022) and RAND.

Table S2: Percentage and sum of negative weights

Data	Percentage	Sum
Arrests	2.5%	-0.003
Prisoners	0%	0
Homicides	0%	0
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 the outcome for the Black population.

Table S3: Robustness to excluding controls for the effect of recreational cannabis laws on arrests per 10,000 persons, 2007-2019

	Cannabis arrests			Non-drug arrests			Total arrests		
Population	-7.006***	-8.872***	-8.596***	21.645*	23.763**	24.694**	13.794	14.886	15.973
	(2.266)	(1.546)	(1.607)	(11.283)	(10.097)	(10.025)	(11.066)	(10.127)	(10.234)
White	-6.947***	-8.767***	-8.542***	9.420	10.827	12.684	1.247	2.170	4.209
	(2.074)	(1.203)	(1.275)	(10.510)	(10.313)	(11.001)	(10.350)	(10.038)	(11.065)
Black	-18.081***	-23.541***	-22.931***	61.867**	66.863**	69.184**	49.009*	45.145	48.056
	(6.557)	(4.576)	(4.596)	(26.760)	(28.776)	(25.826)	(28.880)	(33.306)	(30.376)
Other Race	-2.053*	-1.664*	-1.432	1.092	16.817	18.331	-1.905	13.913	15.342
	(1.039)	(0.877)	(0.923)	(21.870)	(19.105)	(22.602)	(23.043)	(20.127)	(23.643)
Policy controls	Yes	No	No	Yes	No	No	Yes	No	No
Reporting agency control	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No

Notes: Effect of recreational cannabis laws on arrest rates, by race. Ethnicity is not available. Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, where 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 restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each coefficient is based on separate two-way fixed effects regressions (see equation 1 in the text). Each column excludes a different set of controls from the estimation. The first column replicates the results in the main text. Regressions are weighted by race-specific population. All regressions include county and year fixed effects. Policy control variables include unemployment rates, medical cannabis laws, and cannabis decriminalization laws. The reporting agency control refers to the number of agencies reporting in a given county-year. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S4: Robustness to different sample restrictions for the effect of recreational cannabis laws on arrests per 10,000 persons, 2007-2019

	Cannabis arrests				Non-drug arrests				Total arrests			
Population	-7.006***	-6.717***	-6.021**	-6.638***	21.645*	23.034**	26.262**	22.721*	13.794	15.585	19.907*	15.355
	(2.266)	(2.273)	(2.390)	(2.207)	(11.283)	(11.206)	(11.810)	(11.340)	(11.066)	(10.920)	(11.425)	(11.173)
White	-6.947***	-6.675***	-6.204***	-6.749***	9.420	10.450	12.375	8.989	1.247	2.616	5.331	1.589
	(2.074)	(2.092)	(2.199)	(2.068)	(10.510)	(10.450)	(11.018)	(10.387)	(10.350)	(10.228)	(10.664)	(10.293)
Black	-18.081***	-17.505**	-15.716**	-16.594**	61.867**	62.490**	69.415**	59.160**	49.009*	50.763*	60.653**	48.256
	(6.557)	(6.695)	(7.176)	(6.321)	(26.760)	(26.918)	(27.660)	(26.315)	(28.880)	(29.092)	(29.738)	(28.898)
Other Race	-2.053*	-2.159**	-2.136*	-1.755*	1.092	6.329	1.125	-0.406	-1.905	3.174	-1.865	-3.275
	(1.039)	(1.039)	(1.078)	(0.954)	(21.870)	(18.618)	(18.408)	(21.954)	(23.043)	(19.675)	(19.480)	(23.175)
Coverage threshold	65%	75%	85%	65%	65%	75%	85%	65%	65%	75%	85%	65%
Excluding outliers	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

Notes: Effect of recreational cannabis laws on arrest rates, by race. Ethnicity is not available. Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, where 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. Each coefficient is based on separate two-way fixed effects regressions (see equation 1 in the text). Each column considers a different sample restriction based on agency reporting coverage thresholds and outliers (arrest rates above 2 standard deviations from the county-level mean). The first column replicates the results in the main text. Regressions are weighted by race-specific population. All regressions include county and year fixed effects. Control variables include the number of reporting agencies, unemployment rates, medical cannabis laws, and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S5: Robustness to wild cluster bootstrapped standard errors for the effect of recreational cannabis laws on arrests per 10,000 persons, 2007-2019

	Cannabis arrests	Non-drug arrests	Total arrests
Population	-7.006***	21.645*	13.794
	[0.001]	[0.055]	[0.223]
White	-6.947***	9.420	1.247
	[0.000]	[0.398]	[0.905]
Black	-18.081***	61.867**	49.009*
	[0.010]	[0.034]	[0.092]
Other Race	-2.053**	1.092	-1.905
	[0.049]	[0.967]	[0.951]

Notes: Effect of recreational cannabis laws on arrest rates, by race. Ethnicity is not available. Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, where 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 restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each coefficient is based on separate two-way fixed effects regressions (see equation 1 in the text). Regressions are weighted by race-specific population. All regressions include county and year fixed effects. Control variables include the number of reporting agencies, unemployment rates, medical cannabis laws, and cannabis decriminalization laws. Wild cluster bootstrap p-values over 999 iterations reported in brackets. Stars denote significance from these p-values.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S6: Robustness to excluding each state for the effect of recreational cannabis laws on arrests per 10,000 persons, 2007-2019

	Excluded state:											
	AK	CA	CO	DC	IL	ME	MA	MI	NV	OR	VT	WA
Panel A: Cannabis arrests												
Population	-6.933***	-11.817***	-6.768***	-7.006***	-7.006***	-6.838***	-7.169***	-6.672***	-6.647***	-6.397***	-7.027***	-6.456***
	(2.291)	(1.460)	(2.383)	(2.266)	(2.266)	(2.234)	(2.409)	(2.312)	(2.140)	(2.114)	(2.278)	(2.270)
White	-6.885***	-10.687***	-6.694***	-6.947***	-6.947***	-6.755***	-7.151***	-6.879***	-6.781***	-6.227***	-6.971***	-6.377***
	(2.098)	(1.507)	(2.193)	(2.074)	(2.074)	(2.040)	(2.232)	(2.258)	(2.043)	(1.847)	(2.087)	(2.071)
Black	-18.153***	-28.517***	-18.108**	-18.081***	-18.081***	-17.992***	-18.782**	-16.768**	-15.003***	-17.808***	-18.095***	-17.377**
	(6.629)	(7.132)	(6.970)	(6.557)	(6.557)	(6.555)	(7.242)	(7.316)	(4.929)	(6.613)	(6.569)	(6.609)
Other Race	-1.351*	-4.747***	-2.185*	-2.053*	-2.053*	-2.036*	-2.097*	-2.078*	-2.017*	-1.892*	-2.054*	-1.616
	(0.788)	(1.578)	(1.114)	(1.039)	(1.039)	(1.040)	(1.068)	(1.098)	(1.047)	(1.028)	(1.040)	(1.008)
Panel B: Non-drug arrests												
Population	23.494**	34.608**	16.972	21.645*	21.645*	21.716*	21.903*	23.627*	21.539*	17.874*	21.604*	24.847**
	(11.510)	(14.854)	(10.312)	(11.283)	(11.283)	(11.401)	(11.720)	(11.912)	(11.551)	(10.257)	(11.295)	(11.947)
White	10.751	22.350	4.252	9.420	9.420	9.493	9.469	9.967	9.436	5.452	9.330	12.856
	(10.624)	(14.407)	(9.434)	(10.510)	(10.510)	(10.643)	(10.866)	(11.193)	(10.728)	(9.662)	(10.519)	(10.947)
Black	63.246**	94.351***	54.059**	61.867**	61.867**	62.434**	63.138**	60.102**	61.802**	56.033**	61.716**	68.547**
	(27.178)	(31.036)	(25.224)	(26.760)	(26.760)	(26.936)	(28.447)	(28.652)	(27.845)	(25.221)	(26.739)	(28.052)
Other Race	11.305	-4.897	-2.290	1.092	1.092	0.992	0.604	0.707	0.232	1.264	1.065	2.827
	(20.189)	(32.780)	(22.330)	(21.870)	(21.870)	(21.898)	(22.189)	(23.189)	(22.057)	(21.823)	(21.894)	(23.414)
Panel C: Total arrests												
Population	15.777	21.317	8.878	13.794	13.794	14.107	14.232	16.173	13.972	10.466	13.740	17.829
	(11.208)	(16.134)	(10.181)	(11.066)	(11.066)	(11.193)	(11.470)	(11.761)	(11.377)	(10.463)	(11.081)	(11.342)
White	2.721	8.743	-4.016	1.247	1.247	1.621	1.377	1.801	1.421	-2.138	1.137	5.838
	(10.386)	(15.737)	(9.534)	(10.350)	(10.350)	(10.465)	(10.710)	(11.173)	(10.532)	(10.032)	(10.369)	(10.057)
Black	50.355*	83.792**	40.524	49.009*	49.009*	49.812*	50.972	50.413	43.273	43.921	48.826*	56.544*
	(29.331)	(33.705)	(27.213)	(28.880)	(28.880)	(29.101)	(30.598)	(32.055)	(28.232)	(27.704)	(28.855)	(30.177)
Other Race	9.226	-10.803	-5.390	-1.905	-1.905	-1.998	-2.378	-2.340	-2.739	-1.535	-1.926	0.312
	(21.122)	(34.719)	(23.577)	(23.043)	(23.043)	(23.073)	(23.392)	(24.431)	(23.265)	(23.020)	(23.069)	(24.684)

Notes: Effect of recreational cannabis laws on arrest rates, by race. Ethnicity is not available. Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, where 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 restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each coefficient is based on separate two-way fixed effects regressions (see equation 1 in the text). 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, unemployment rates, medical cannabis laws, and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

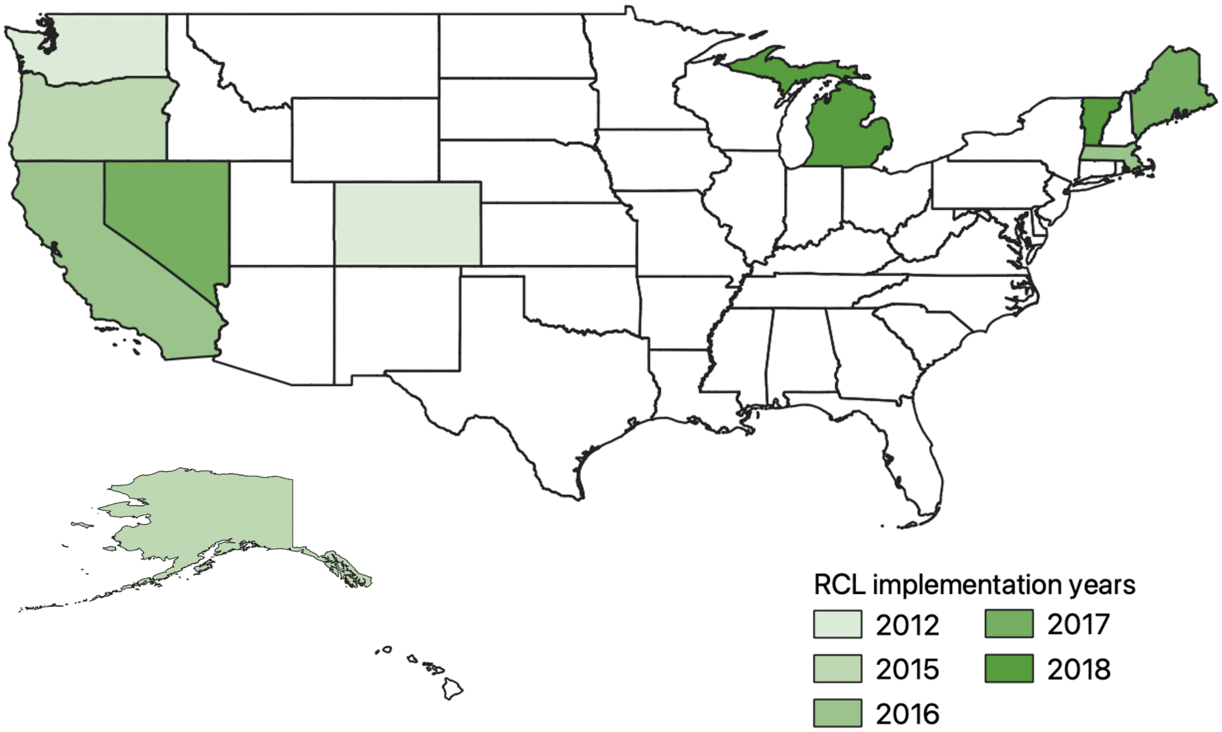
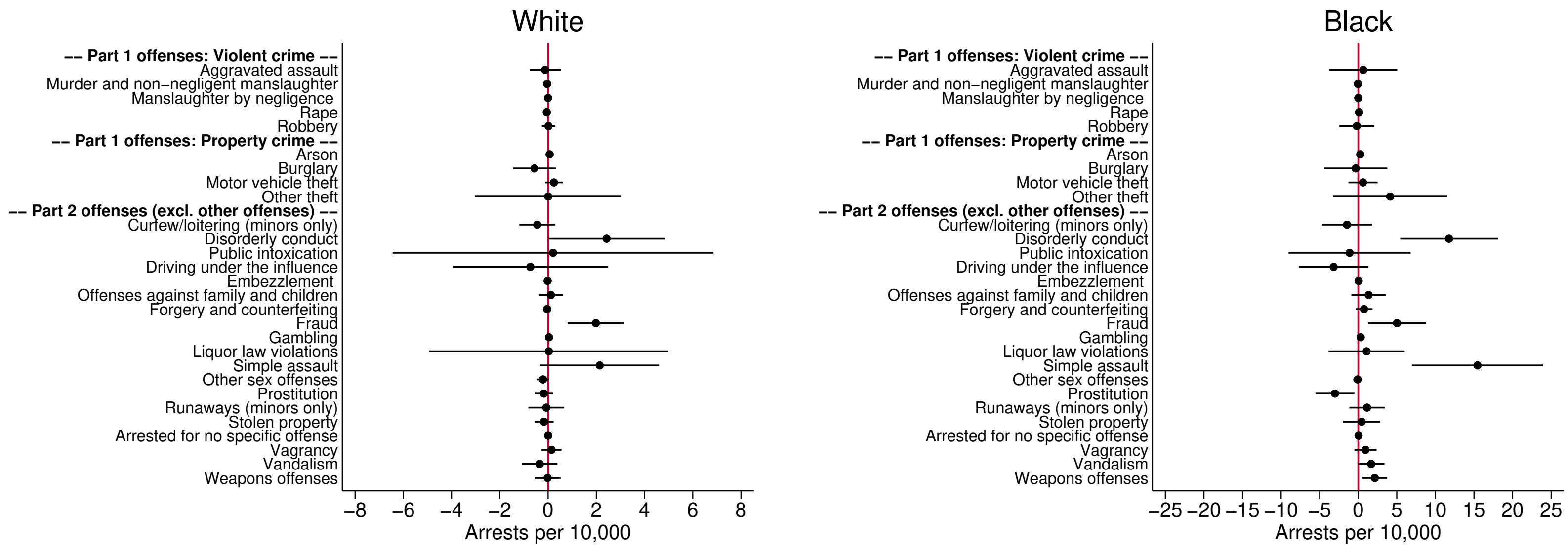


Figure S1: Implementation of recreational cannabis laws by state over time

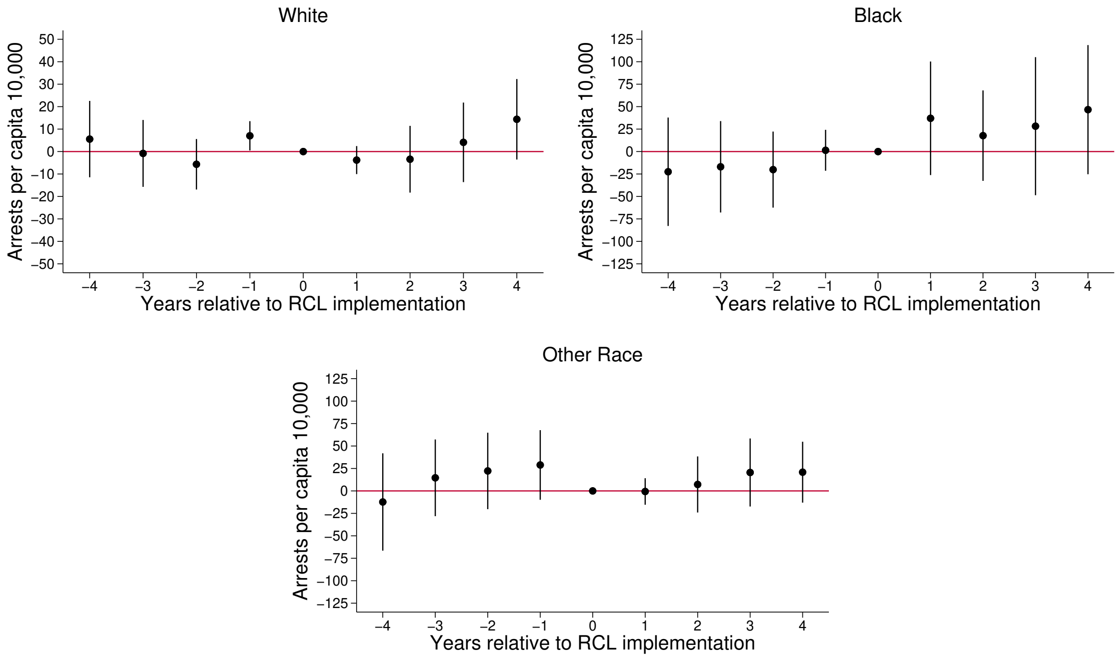
Notes: The map shows the spatial roll-out of RCLs over time, using data shown in Table S1. RCL=Recreational cannabis laws.

Figure S2: Effect of recreational cannabis laws on arrests per 10,000 persons, by crime categories, 2007-2019



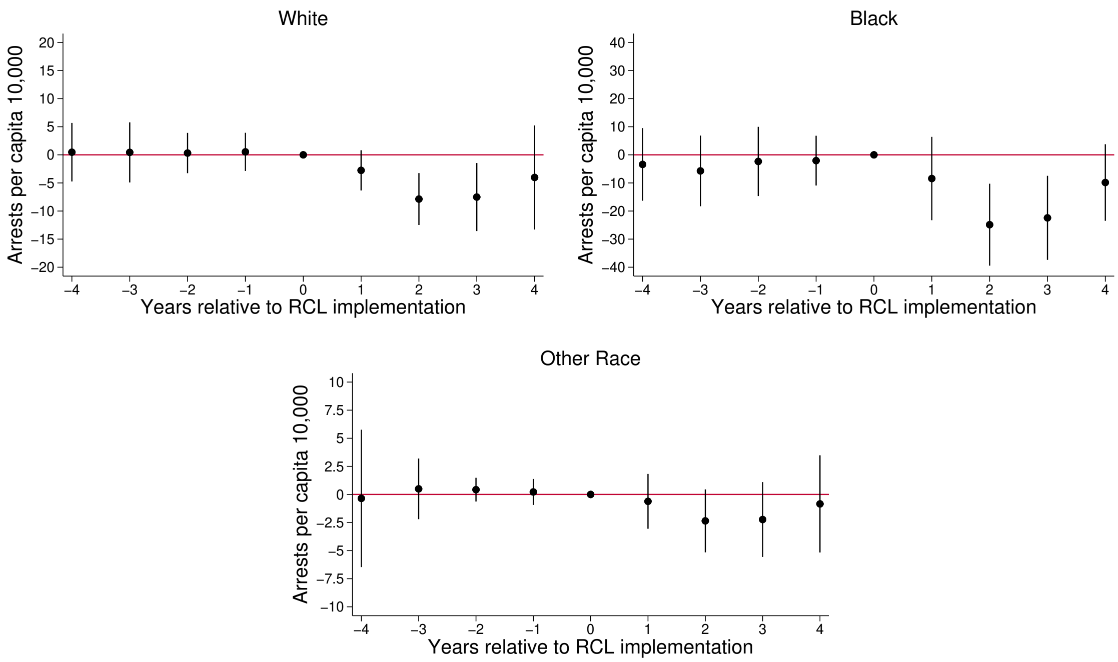
Notes: Effect of recreational cannabis laws on arrest rates for all crime categories, by race groups (White and Black). Ethnicity is not available. Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race, where the unit of analysis is a county-year. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample 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. Each coefficient is based on separate two-way fixed effects regressions (see equation 1 in the text). Regressions are weighted by race-specific population. All regressions include county and year fixed effects. Control variables include the number of reporting agencies, unemployment rates, medical cannabis laws, and cannabis decriminalization laws. RCL=Recreational cannabis laws.

Figure S3: Effect of recreational cannabis laws on total arrests per 10,000 persons, by time since RCL implementation



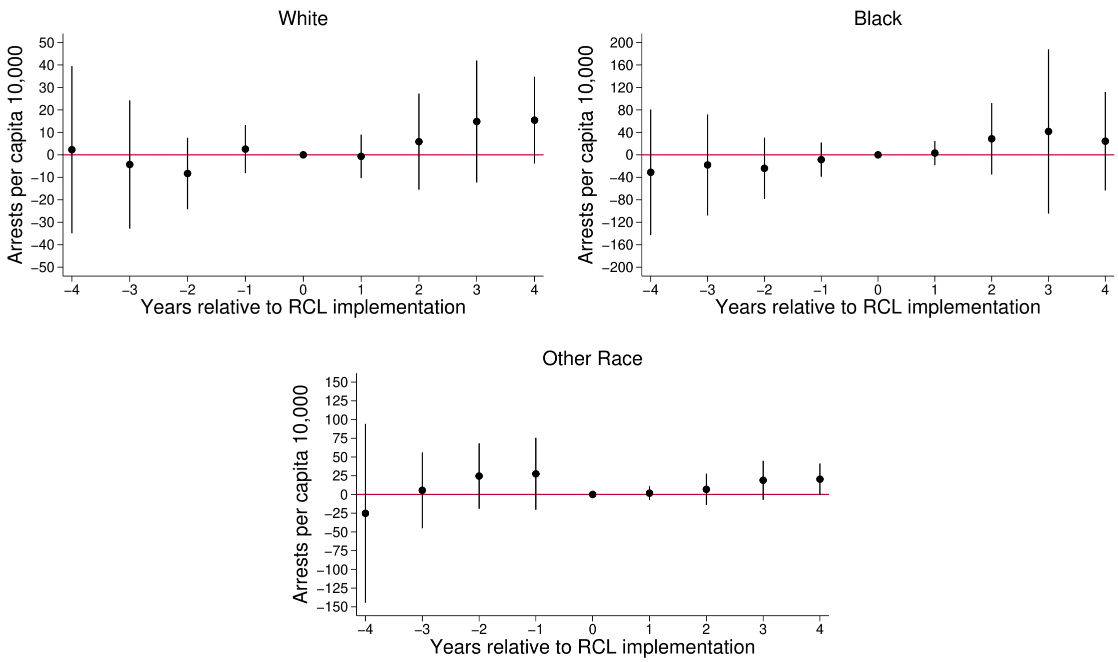
Notes: Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample is restricted to counties with an agency 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 that estimates leads and lags of the intervention and that accounts for controls (see equation 2 in the text). Regressions are weighted by race-specific population. Controls include the number of reporting agencies, unemployment rates, medical cannabis laws, and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

Figure S4: Robustness to heterogeneous effects of recreational cannabis laws on cannabis arrests per 10,000 persons, by time since RCL implementation



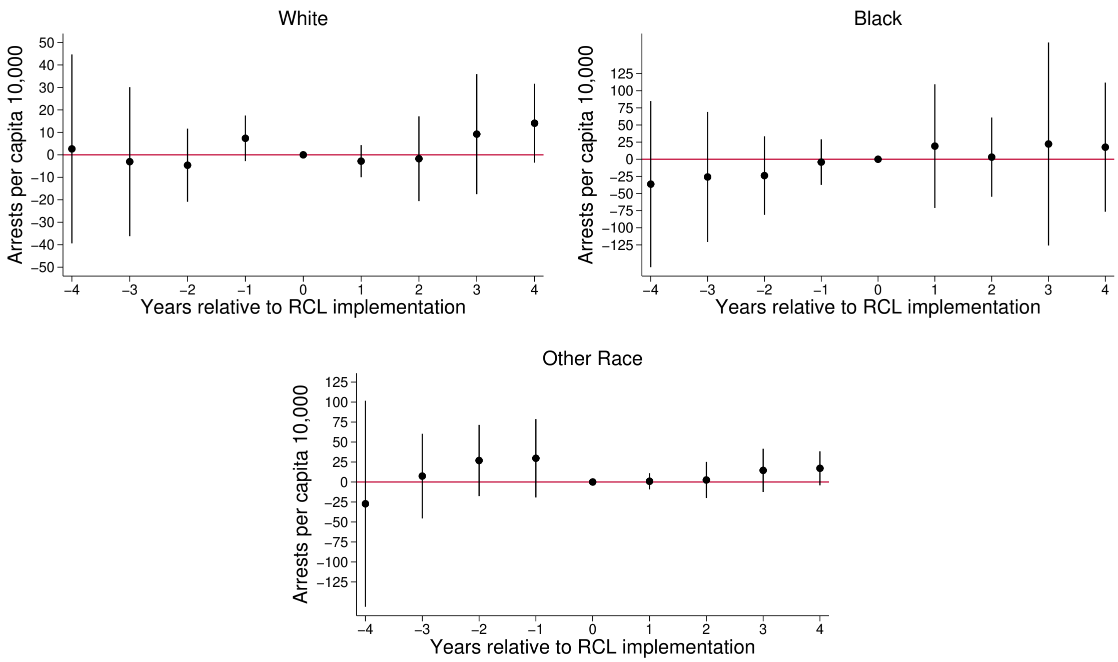
Notes: Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample is restricted to counties with an agency reporting coverage threshold above or equal to 65%. Coefficients and 95% confidence intervals (from standard errors clustered at the state level with 100 bootstrap iterations) are based on the dynamic and placebo Wald-TC estimator proposed in De Chaisemartin and D'Haultfoeuille (2022), which is robust to dynamic heterogeneous effects. Controls include the number of reporting agencies, unemployment rates, medical cannabis laws, and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

Figure S5: Robustness to heterogeneous effects of recreational cannabis laws on non-drug arrests per 10,000 persons, by time since RCL implementation



Notes: Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. 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 (from standard errors clustered at the state level with 100 bootstrap iterations) are based on the dynamic and placebo Wald-TC estimator proposed in De Chaisemartin and D'Haultfoeuille (2022), which is robust to dynamic heterogeneous effects. Controls include the number of reporting agencies, unemployment rates, medical cannabis laws, and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

Figure S6: Robustness to heterogeneous effects of recreational cannabis laws on total arrests per 10,000 persons, by time since RCL implementation



Notes: Data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. 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 (from standard errors clustered at the state level with 100 bootstrap iterations) are based on the dynamic and placebo Wald-TC estimator proposed in De Chaisemartin and D'Haultfoeuille (2022), which is robust to dynamic heterogeneous effects. Controls include the number of reporting agencies, unemployment rates, medical cannabis laws, and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.