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 across the criminal justice continuum using a difference-in-differences framework. Legalization led to sizable reductions in arrest rates for cannabis possession and sales across all racial groups, resulting in declines in relative and absolute disparities for Black compared 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. These offsetting effects cannot be explained by greater state-level police capacity, nor by greater systemic violence. Although cannabis legalization may be a policy lever for addressing racial disparities, systemic and other factors may offset these equity gains.

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 years of life lost behind bars, criminal records crippling access to jobs, loans, housing and government benefits, billions of dollars spent on law enforcement, systemic violence from the creation of an illegal drug market, and children growing up without a parent (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 disparities in law enforcement of drug prohibition are widespread and longstanding, with Black communities being disproportionately affected (Vitiello, 2019). Even though White and Black persons use cannabis at roughly similar rates, Black persons are 3.6 times more likely to be arrested for cannabis possession (Edwards et al., 2020). Black persons are also incarcerated at dramatically higher rates than White persons for drug-related offenses. Despite representing 12.5% of the total U.S. population, Black persons account for 28% of state and 33% of federal prisoners with a sentence of more than one year for a drug-related offense (Carson, 2021; Motivans, 2020). Black communities are also disproportionately affected by systemic violence. Over 50% of homicide victims are Black (Federal Bureau of Investigation, 2018), and the firearm death rate among Black persons is nearly three times that of White persons (Kaiser Family Foundation, 2022).

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 systemic violence. As of 2022, 21 states and the District of Columbia have passed recreational cannabis laws (RCLs), allowing individuals ages 21+ to possess, use, and supply 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 systemic 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 systemic 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 systemic violence associated with illegal drug markets, both of which reflect important consequences of drug prohibition. Specific measures included the rate of arrests, prisoners, homicides, and hospitalizations involving assault 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 in 11 states, using effective dates in a difference-in-differences (DID) framework. We showed static DID estimates and event study plots, and tested the robustness of main results to recent advances in the DID literature.

Findings suggest that RCLs led to significant declines in arrest rates for cannabis possession and sales across all racial groups, resulting in declines in relative and absolute disparities in 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 of other drugs. 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 systemic 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, 21 states and the District of Columbia have enacted recreational cannabis laws as of 2022, legalizing cannabis sales, distribution, possession, and use among adults aged 21 or older, subject to amount limits and other restrictions (ProCon, 2022). 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 (i.e. pain, HIV, multiple sclerosis, cancer, etc.). 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

2.2.1 Cannabis Liberalization Policies and Criminal Justice Outcomes

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

As noted in Section 1, the handful of previous RCL studies analyzing racial and ethnic disparities in criminal justice outcomes are either descriptive or based on pre-post analyses, and use data from a single state or few states (Edwards et al., 2020; Firth et al., 2019, 2020; Pierson et al., 2020). An exception is Sheehan et al. (2021), which 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 consider bias from treatment effect heterogeneity with novel DID estimators, and did not account for the well-documented variation across states and over time in the number of reporting agencies (Kaplan, 2021). Edwards et al. (2020), 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. 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. Firth et al. (2019), a single-state study focusing on Washington, found that cannabis possession arrests decreased significantly among both Black and White adults but that relative disparities grew from 2.5 to 5 following RCL implementation. Pierson et al. (2020) documented reductions in police traffic stops resulting in searches among Black, White, and Hispanic persons in Washington and Colorado.

In addition to methodological limitations, these RCL studies analyzed racial disparities in cannabis possession arrests almost exclusively,[1] an outcome that only reflects partial equilibrium effects of policing and crime production. Elucidating the full impact of RCLs on racial disparities in the criminal justice system requires examination not only of arrests for cannabis possession, for cannabis sales, for other drug possession and sales, and for non-drug offenses, as well as other law enforcement and systemic violence outcomes that reflect the toll of drug prohibition. This way, we can assess potential spillovers and general equilibrium effects associated with RCLs.

[1] Pierson et al. (2020) is the only exception as their outcome is police traffic stops.

2.2.2 Racial Disparities in Criminal Justice Outcomes

Researchers have proposed various explanations for these longstanding disparities in law enforcement of drug prohibition. These include racial discrimination by law enforcement officials, greater police presence in minority communities, and that minorities are more likely to use or sell drugs in public spaces (Vitiello, 2019; King and Mauer, 2006; Wilson, 2021; Pierson et al., 2020; Chen et al., 2021; Beckett et al., 2006). Racial discrimination may arise from taste-based discrimination (i.e., police officers would rather arrest a Black person than a White person for a drug offense) or from statistical discrimination (i.e., police officers believe that criminal behavior is more predominant among Black persons and allocate more resources to those communities). Pierson et al. (2020) estimates that the bar for stopping and searching Black and Hispanic drivers is lower than that for White drivers, and that after sunset—when detecting a driver's race becomes more difficult—Black persons are much less likely to be stopped by police. Previous studies suggest that law enforcement over-target communities of color (Beckett et al., 2006; Chen et al., 2021). Over-policing in minority communities might follow from taste-based discrimination, from statistical discrimination, or from lower policing costs of minority communities. Chen et al. (2021) uses cellphone data to document that police officers spend a disproportionate amount of time in Black neighborhoods, even after controlling for drivers of crime and demand for policing, estimating that around half of the observed racial disparity in arrests can be traced to this bias in policing. Furthermore, Jurado (2022) argues that the large number of Black males killed by law enforcement is due to this disproportionate exposure of Black communities to police interactions, and not to excessive use of police force conditional on an interaction. Stashko (2023) derives testable implications from a model to show that the empirical evidence from cities in the U.S. is consistent with police officers maximizing arrests, and not with minimizing crime.

2.3 Conceptual Framework

The net effect of cannabis legalization on overall criminal activity and associated law enforcement efforts is theoretically ambiguous, and will be largely influenced by the effect of

RCLs on the consumption and production of cannabis and other drugs, and the relationship between law enforcement, criminal activity, cannabis, and other drugs.

2.3.1 Cannabis and Crime

Cannabis and crime are plausibly linked through at least four pathways: (1) cannabis-defined offenses, (2) systemic violence, (3) psychoactive effects, and (4) economic crime (Pacula and Kilmer, 2003). Cannabis *prohibition* is the driver of pathways (1) and (2), while cannabis *use* is the driver of pathways (3) and (4). First, cannabis *prohibition* implies that cannabis is defined as a crime; it is a crime to use, possess, manufacture, or distribute cannabis. Second, cannabis *prohibition* may cause crime by generating systemic violence and other criminal activity in connection with drug trafficking in illegal drug markets. Illegal drug markets are associated with increased systemic violence because of turf wars among suppliers, unpaid drug debts, and other transaction issues, 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). While illegal cannabis sales are generally conducted indoors and illegal cannabis markets are less violent than other illegal drug markets (Caulkins and Pacula, 2006; Pacula and Kilmer, 2003), illegal cannabis markets generate systemic violence (Charns, 2023; Drug Enforcement Administration, 2021, 2013; Dale and Izaguirre, 2017; Archibold, 2009). Third, the psychoactive effects of cannabis *use* may influence user behavior, leading to criminal activity in some individuals. While studies have generally shown that cannabis use temporarily inhibits aggression and violence, there is evidence of violent behavior in some populations (i.e. adolescents, high frequency users) (Pacula and Kilmer, 2003). Moreover, toxicology reports and self-reported data shows that the prevalence of cannabis and other substances among homicide victims and homicide offenders significantly exceeds population prevalence (Darke et al., 2009; Fendrich et al., 1995). Likewise, our analysis of the inpatient data used in this study shows that the prevalence of cannabis use disorder is about six times higher for hospitalizations involving victims of gun injury or assault, relative to other hospitalizations. The psychoactive effects of cannabis *use* 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. Fourth, cannabis *use* may induce economic crime among users to finance their cannabis consumption.

2.3.2 Cannabis and Other Drugs

Cannabis and crime are plausibly linked through a fifth pathway: (5) The correlation between the production and consumption of cannabis with the production and consumption of other drugs. Given the vast size of the cannabis market relative to the size of the market for heroin, methamphetamine, or cocaine (based on use prevalence), it is possible that the market for other illicit drugs is affected by changes in the market for cannabis. For instance, the 2015-2018 National Survey on Drug Use and Health shows that conditional on past month cannabis use, the prevalence of past month heroin, methamphetamine, and cocaine use was 1.00%, 2.10%, and 5.40%, respectively. However, the prevalence of past month cannabis use was 60%, 62%, and 69% conditional on past month use of heroin, methamphetamine, and cocaine, respectively. There is also evidence that illegal drug producers and distributers are involved in more than one illegal drug market. The majority of foreign heroin, methamphetamine, and cannabis comes from Mexico, with Mexican transnational criminal organizations exerting a great influence over their production and distribution within the U.S. (Drug Enforcement Administration, 2018; Beittel, 2022). For example, the Sinaloa cartel produces heroin and cannabis, both of which it illegally distributes throughout the U.S. (Drug Enforcement Administration, 2018). This is true for other cartels. Distribution, as documented in other settings, tends to follow a hierarchical structure, with dealers buying from higher-level distributers and re-selling in smaller quantities at the next lower market level, until reaching the retail or direct to consumer level (Caulkins et al., 2016).

The prohibition and use of other drugs, in turn, are plausibly linked to crime through at least some of the four pathways described above. Depending on the degree to which other drugs are complements or substitutes of cannabis in production or consumption, and the nature of the link between other drugs and crime, changes in cannabis markets may induce subsequent changes in other drug markets, and thus, in crime associated with not only cannabis, but also other drugs.

2.3.3 RCLs and Reductions in Crime

The legalization of cannabis may reduce criminal activity and associated law enforcement efforts through various pathways. First, decreases in cannabis-defined offenses 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 by other non-cannabis offenses within the same incident, we may observe declines in cannabis arrests with limited or no declines in total arrests.

Second, the creation of a legal cannabis market should reduce the size of the illegal cannabis market, decreasing systemic violence and other criminal activity in connection with cannabis trafficking (Dragone et al., 2019). Indeed, previous studies suggest that the street prices of cannabis declined following RCL implementation (Meinhofer and Rubli, 2021), which is consistent with reductions in the size of the illegal cannabis market. Third, consumers and producers of cannabis may also be consumers and producers of other illegal drugs. To the extent RCLs reduce the production or consumption of other drugs that are substitutes of cannabis, legalization may also reduce criminal activity and associated law enforcement efforts involving other drugs. RCL induced increases in cannabis use, a sedative drug, may lead to reductions in violent crime in some cases, 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 for other drug-defined offenses, regardless of changes in the actual production or consumption of other drugs. Previous research has found that law enforcement seizures of both cannabis and other illegal drugs declined following RCL implementation (Meinhofer and Rubli, 2021). Furthermore, state and local governments could use the additional tax revenue from cannabis legalization to support local law enforcement efforts to deter crime. 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).

2.3.4 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 use or crimes committed by individuals to finance their cannabis use. Moreover, if greater cannabis use increases consumption of complement drugs, legalization could lead to more drug-related offenses through any of the four pathways previously discussed. Greater competition from the creation of a legal cannabis market may increase systemic violence associated with drug trafficking and other illegal actvities. 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.

2.3.5 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.[2] To the extent that treatment effects change disproportionately across race and ethnicity, RCLs may also affect relative disparities.

2.3.6 RCLs and Reductions in Racial Disparities

There are several pathways through which the legalization of cannabis may decrease racial disparities in law enforcement and criminal activity. For instance, to the extent that illegal drug markets are most prevalent in minority communities of color, RCL implementation should reduce illegal drug markets and therefore, 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). Legalization and shrinking drug markets could have the largest effects on communities of color if police presence and contact decrease. 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. 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

[2] 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).

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.

2.3.7 RCLs and Increases in Racial Disparities

However, the legalization of cannabis may also exacerbate 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).[3] 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. Pierson et al. (2020) analyzes changes in police stops by race before and after the RCLs in Colorado and Washington, relative to time trends in other states that did not legalize cannabis. Although results indicate a significant decline in police stops across all groups, the racial disparities in both stops and search remained after the policy. Additionally, 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).

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

2.3.8 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 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

3.1 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.[4] For each arrest, police officers register the offender's

[4] 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.

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.

The UCR data do not allow us to observe the different number of individuals that were arrested in a given time period.[5] 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

[5] 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.

below, we rely on assuming that missing counts or reporting issues are uncorrelated with the timing of RCLs.

3.2 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 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.

3.3 Homicide Deaths

Homicide deaths were obtained from restricted 2007-2019 National Vital Statistics System (NVSS) Multiple Cause of Death Files. These microdata are based on information abstracted from death certificates and provide underlying cause of death and multiple cause of death for nearly all deaths 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, Y35, Y87.1) previously established (Center for Disease Control and Prevention, 2007). We also relied on a data field identifying homicide as the manner of death. We generated two outcome measures: total homicides and homicides involving gun injury. We aggregated each outcome at the state of occurrence-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.

3.4 Summary Statistics

Tables 1 through **??** report summary statistics for states with and without RCLs during the study period. For RCL states, we distinguished between the periods before the policy (baseline period) and after the policy.

Table 1 presents 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 about three times higher than for White persons, while total arrests were more than twice as large. 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 assault with gun injury were much larger for Non-Hispanic Black persons than for Non-Hispanic White persons, as seen in Table **??**.

4 Empirical Strategy

4.1 Main Specification

Our identification strategy exploits variation in the staggered implementation of recreational cannabis laws (RCLs) in 11 states using the effective dates in Appendix Table S1. Equation 1 represents the baseline two-way fixed effects (TWFE) difference-in-differences (DID) regression model. Separate TWFE DID models were estimated for the overall population and for each racial and ethnic group r:

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

$Y_{r,j,t}$ denotes an outcome for racial and ethnic group r, in jurisdiction j (which may be a state or a county), and in time period t (which may be a quarter or year). We generated three disparity measures for each outcome; rates,[6] rate ratios,[7] and rate differences.[8] First, we generated rates per 10,000 persons by dividing outcome counts for each racial and ethnic group r in jurisdiction j and period t by U.S. Census population estimates corresponding to the same racial and ethnic group r, jurisdiction j, and year y. Second, we generated the rate

[6] $Rate_{r,j,t} = (Y_{r,j,t}/Population_{r,j,y}) \times 10,000$

[7] $RateRatio_{r,j,t} = Rate_{r,j,t}/Rate_{White,j,t}$

[8] $RateDifference_{r,j,t} = Rate_{r,j,t} - Rate_{White,j,t}$

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. We included jurisdiction fixed effects, denoted by α_j, to account for any time-invariant differences across jurisdictions that may affect law enforcement and systemic 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. $X_{j,t}$ represents a vector of control variables, which include an indicator of cannabis decriminalization laws. When using arrest data, we also controlled for the number of reporting agencies in a given county-year. Lastly, $\varepsilon_{r,j,t}$ is the idiosyncratic error term. All regressions for racial and ethnic group r are weighted by U.S. Census population estimates for racial and ethnic group r. Standard errors are clustered by state, which is the level at which the treatment varies (Abadie et al., 2023). This accounts for within-state serial correlation in the error term.

The coefficient of interest is denoted by β, which reflects the static treatment effect of RCLs on outcomes. The main assumption for identifying a causal effect is that, in the absence of an RCL, law enforcement and systemic 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 were 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:

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

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.

4.2 Robustness Checks and Heterogeneity Analyses

We extend our analysis to examine the heterogeneity and robustness of main estimates in several ways. First, we calculated confidence intervals 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 in our sample (11 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 employed two novel DID estimators that are robust to potential bias from treatment effect heterogeneity. The baseline TWFE DID 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 or 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. 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 DID regressions, following De Chaisemartin and d'Haultfoeuille (2020). We found that the percentage of negative weights attached to $RCL_{j,t}$ is zero or minimal, likely because RCLs are more recent and the share of treated states in our sample is low.[9] This validates increases confidence in our TWFE DID estimator. Nevertheless, we also showed results using the DID estimators in De Chaisemartin and D'Haultfoeuille (2022) and Sun and Abraham (2021), which are robust to heterogeneous treatment effects.

Third, we tested the robustness of main findings to the exclusion or inclusion of control variables. In particular, we dropped baseline controls (i.e. cannabis decriminalization laws). We also progressively controlled for medical cannabis laws and cannabis expungement laws to account for other cannabis liberalization policies, as well as for state-level unemployment rates to account for economic conditions.

Fourth, we estimated treatment effects using other definitions of the RCL indicator. In particular, we replaced the RCL indicator with an indicator of the time when recreational cannabis dispensaries first opened in the state (RCL Dispensary). Prior research suggests that the date of cannabis legalization by itself may be an inadequate measure of access to cannabis, and that dispensaries are more correlated with increases in cannabis use (Pacula et al., 2015). Given that many of our outcomes (i.e. drug-defined offenses, measures of systemic violence) should be more affected by changes in cannabis prohibition (and less affected by changes in cannabis use), we expect the RCL Dispensary indicator to be less relevant in our context but nevertheless explore its relevance.

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. Moreover, we accounted for potential spillovers across states. We also 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.

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

5 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.

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 following RCL implementation. 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 ?? 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.

5.2 Law Enforcement

5.2.1 Cannabis Arrests

Figure 4 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 and all estimates are calculated relative to this year. 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 4. 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 30% in cannabis arrest rates for the full population. By race, this corresponds to a 32% reduction for White persons, 35% for Black persons, and 15% 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 11% and 42% of the baseline disparity in RCL states during the pre-policy period. For Other Race populations, we observe a significant narrowing of disparities.[10]

Table 5 breaks down the effects by cannabis arrest rates for possession (first column) and for sales (third column), with event studies in appendix Figures S2 and S3. We find significant declines after RCL implementation in both types of crimes. For the full population, this amounts to a 30% and 32% average reduction for possession and sales, respectively. We cannot reject that these effect sizes are equal. This pattern holds for the White population. For Black persons, we obtain an average decline of 32% in arrests for cannabis possession but 48% for sales. This difference in magnitudes is statistically significant. For the Other Race group, the magnitude is around a 16% decline of the baseline mean for cannabis possession and 10% for sales. These results suggest differences across race groups of the relative magnitudes of declines in cannabis arrests by possession versus sales.

[10] The baseline rate ratio is 0.23, indicating fewer arrests for the Other Race group relative to the White population. A positive coefficient here means the ratio becomes closer to one. For the rate difference, the baseline mean is -10.15, so that a positive coefficient also narrows the gap between these groups.

5.2.2 Other Drug Arrests

We complement our previous results with TWFE DID estimates of the effect of RCLs on other drug arrests and present the corresponding event studies in appendix Figure S4. The fourth column in Table 4 shows positive, small, and statistically insignificant effects of RCLs on arrest rates for other drugs for the full population and White persons. Point estimates for Black and Other Race groups are negative and more sizable, though only the latter is statistically significant at the 90% level. 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. In contrast, the point estimate for the White group would imply a 1% increase. For our measures of disparities, there are no significant impacts among Black persons, though we find a significant increase in absolute disparities for the Other Race group.

Unlike the findings for cannabis arrests, the second and fourth column in Table 5 show strong significant declines in arrest rates for sales of other drugs but smaller, sometimes positive, and insignificant effects for possession of other drugs. From the magnitude alone, we would estimate a 6% increase in possession arrest rates for White persons and a 1% increase for Black populations. For sales, we find a 23% decline relative to the baseline mean for White persons, but only a 15% decline for Black persons.

5.2.3 Non-Drug Arrests

We now turn our attention to arrests that are unrelated to drugs. Figure 5 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 4. 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 6% increase for the full population and 8% increase for Black persons. Effects for White and Other Race populations are not statistically significant, although the point estimates suggest a 3 and 4% 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 11% 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 5 and present event study plots in appendix Figures S5, S6, and S7. We find insignificant results for all Part 1 offenses. For both violent and property crimes, point estimates for White and Black persons would imply effects of less than 2% of the baseline mean in RCL states prior to the policy.

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 a 7% average increase in arrest rates for this category for the full population, and a 9% 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 S16 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 among Black persons. For the White population, we obtain a small but significant increase in motor vehicle theft arrests and a more sizable decline in burglary arrests.

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 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 vagrancy, vandalism, and weapons offenses. We only find a significant decline in Black 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. Moreover, the arrest categories for which we estimate significant increases are those that are perhaps more likely to involve discretionary behavior from police officers.

5.2.4 Total Arrests

Putting together our previous estimates, the last three columns in Table 4 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 1%, while the point estimate for Black persons is larger, indicating an increase of 5% in the total arrest rate. This difference leads to an estimated increase in disparities between these groups of between 3 and 8%, although only the rate difference is significant at the 90% level. Online Appendix Figure S8 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).

5.2.5 Robustness Checks for Arrest Data

We present a battery of robustness checks on our main results for the arrests data in the Online Appendix. First, Table S3 shows that the effects hold when we include additional policy controls for MCLs and for the unemployment rate, as well as when we exclude all controls. Second, Table S4 considers alternative sample restrictions by imposing a higher threshold for the coverage indicator or by excluding outliers, defined as arrest counts that are larger than the county-level mean plus two times its standard deviation. Third, we show that the estimates are similar when we exclude one RCL state from the sample at a time in Table S5. This implies that our findings are not driven by one particular RCL state. Fourth, we show that results are robust to controlling for potential spillover effects across jurisdictions in Table S6. Here, for non-RCL states, we consider an indicator for whether an RCL has been implemented within 100 miles of the county, then add an indicator for an RCL within 100 to 200 miles, and lastly we consider the inverse distance to the nearest county with an RCL. Lastly, Figures S9, S10, and S11 show TWFE DID estimates for a variety of specifications of the impact of RCLs on cannabis, non-drug and total arrest rates by race groups. In addition to the specifications outlined above, we also include our baseline results with 95% confidence intervals calculated from wild cluster bootstrapped standard errors over 999 repetitions to account for the fact that we may have a small number of treated clusters. We also show specifications that control for criminal record expungement laws, and a specification that estimates the impact of recreational cannabis dispensary laws instead of RCLs. 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) and by Sun and Abraham (2021) in Figures S12, S13, and S14, contrasting with the TWFE estimators. 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.

5.2.6 Heterogeneity Analysis for Arrest Data

Before moving on to our other outcomes, we discuss some heterogeneity analysis for the arrests data that is presented in the Online Appendix. First, we focus on differences in the effects of RCLs related to the presence of other drug policies. In Table S7 we estimate differential impacts of RCLs by whether the state had already implemented a cannabis decriminalization law prior to passing the RCL. Across all population groups, we find that the declines in cannabis are much starker when the state has not yet decriminalized cannabis. For non-drug and total arrests, we find generally larger increases after RCLs in states that have not yet decriminalized cannabis, although effect sizes relative to the other RCL states are only significantly different for the Black population. In Table S8 we show heterogeneity by whether the RCL is accompanied by recreational dispensary laws or not. With the exception of Nevada and Oregon, where both laws were passed in the same year, recreational dispensaries are all preceded by RCLs. Hence, we are estimating the average impact of an RCL prior to dispensaries being allowed and the impact once dispensaries are allowed. For cannabis arrests, we find significantly larger impacts once dispensary laws are passed for all groups except Other Race persons. For non-drug and total arrests, we generally find larger increases once dispensary laws are implemented, with significant differences for the White and Black groups.

Our second set of heterogeneity analysis considers differences across counties in racial composition, using baseline measures from the 2010 census (prior to any RCL). In Table S9, we stratify counties by the median of the county-level Black share of the population. We find that the decline in cannabis arrests is significantly larger in counties with a low share of Black persons. However, this difference is small and not statistically significant for Black persons. For non-drug and total arrests, we do not find any significant differences between counties that had a low share of Black persons and those that had a high share, although point estimates tend to be larger in the former. In Table S10, we repeat this exercise using the 2010 racial dissimilarity index, which quantifies the share of a racial group's population that would have to move in order to equalize population shares across counties. This exercise yields significantly larger declines in cannabis arrests for the White group in places that are more homogeneous, and no significant differences for the other population groups. For non-drug and total arrest rates, we do not find any statistically significant differences across counties for all race groups.

5.2.7 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 6 shows event study plots for each group. We do not observe any striking patterns after RCL implementation in any of the groups.

Table 6 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.

5.3 Systemic Violence

5.3.1 Homicides

Table 7 reports TWFE DID estimates of the effect of RCLs on the rates, rate ratios, and rate differences of total homicides and homicides involving gun injury, by race and ethnicity. For Non-Hispanic White, Non-Hispanic Other Race, and Hispanic groups, we obtain small and statistically insignificant estimates across disparity measures for both outcomes. The only significant impact is a decline in the rate of total homicides (-0.105) and homicides involving gun injury (-0.109) among Non-Hispanic Black persons. Relative to the baseline mean in RCL states, this implies a sizable decline of 17% (-0.105/0.62) in the rate of total homicides and of 21% (-0.109/0.51) in the rate of homicides involving gun injury. As for measures of disparities, we estimate a sizable and statistically significant decline in rate differences between the Non-Hispanic Black and White groups (-0.104), but null effects for the rate ratio.

Figure 7 shows corresponding event study plots. Consistent with TWFE DID estimates, estimates for total homicides are generally close to zero and statistically insignificant for Non-Hispanic White, Non-Hispanic Other Race, and Hispanic persons. For Non-Hispanic Black, total homicides estimates are negative and significant post-policy.

5.3.2 Robustness Checks for Homicide Data

We test the robustness of baseline TWFE DID estimates for total homicide rates in Appendix Figure S15. In particular, we use the wild cluster bootstrap for statistical inference with a small number of treated clusters, drop baseline controls (i.e. cannabis decriminalization laws), progressively add new controls (i.e. medical cannabis laws, cannabis record expungement laws, unemployment rate), replace the RCL indicator with an indicator of the time when recreational cannabis dispensaries first opened in the state, and employed two novel DID estimators that are robust to potential bias from treatment effect heterogeneity. These included the multiperiod DID estimator (DIDCD) described in De Chaisemartin and D'Haultfoeuille (2022) and the interaction weighted DID estimator (DIDAS) described in Sun and Abraham (2021).

For Non-Hispanic White, Non-Hispanic Other Race, and Hispanic groups, the coefficients are small and statistically insignificant regardless of our estimation approach, coinciding with baseline TWFE DID estimates. As for Non-Hispanic Black, estimates are no longer significant when using the wild cluster bootstrap for statistical inference. Moreover, while still negative, the size of the coefficients shrinks when using DID estimators that are robust to treatment effect heterogeneity bias (DIDCD=-0.066 and DIDAS=-0.061). The coefficient is not statistically significant when using the multiperiod DID estimator (DIDCD). The magnitude and significance of DID estimates for Non-Hispanic Black is otherwise robust. Given these somewhat mixed findings, we interpret documented declines in Non-Hispanic Black homicides as suggestive, but not conclusive evidence of the effect of RCLs on this group.

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 reducing cannabis-defined offenses. Cannabis arrests did not disappear entirely, likely due to provisions of state RCLs still 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 systemic 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. [11] Back-of-the-envelope calculations would then suggest that around 0.6 and 1.8 cannabis arrests per 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.[12] 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).

[11] 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.

[12] 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.

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 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 suggestive evidence of reductions in violence, as measured by declines in total homicides and homicides 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, 2023). Declines in total homicides and homicides involving 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. 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.

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 systemic 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, 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 systemic 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 systemic 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 suggestive evidence of 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

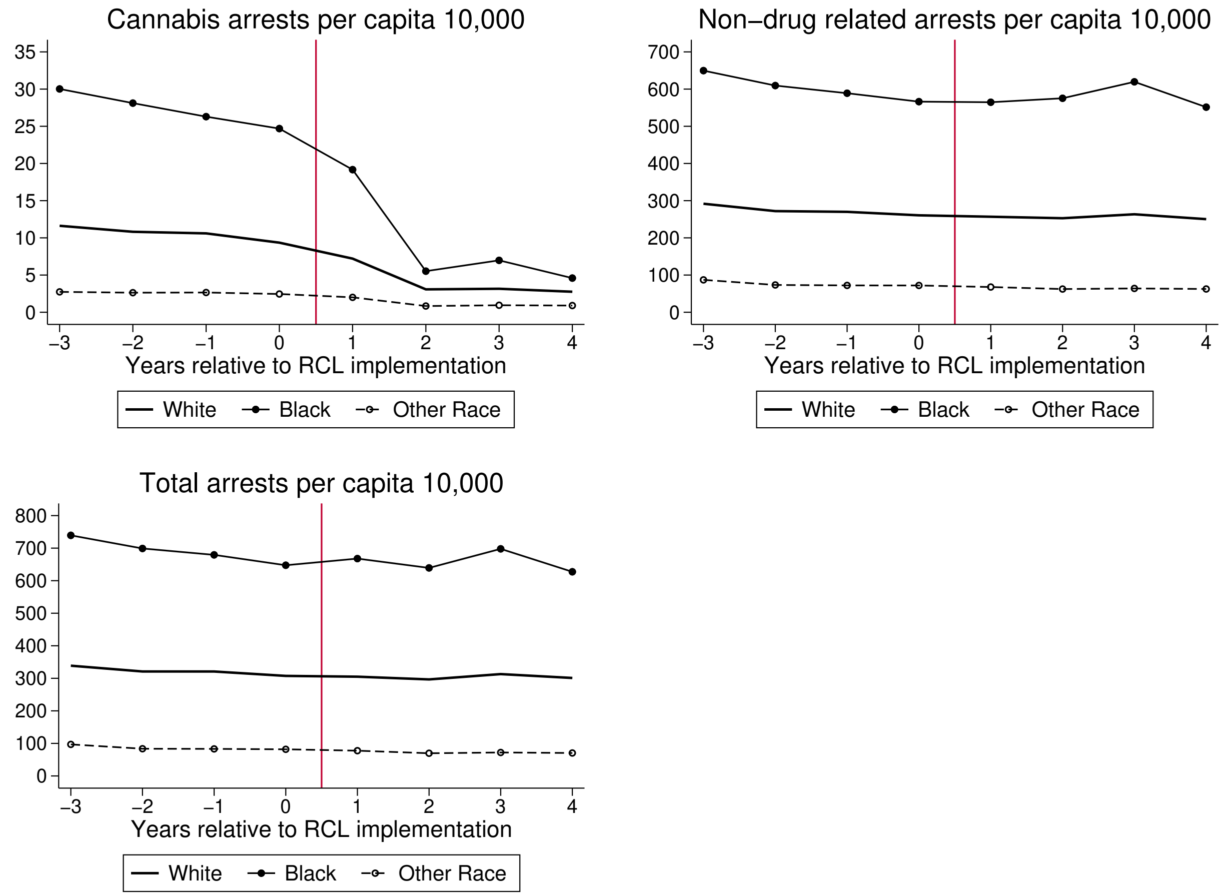


Figure 1: Arrest rates, by time since RCL implementation

Notes: Arrest 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 time periods relative to RCL implementation. The time $t = 0$ corresponds to the period immediately before RCL implementation. RCL=Recreational cannabis laws.

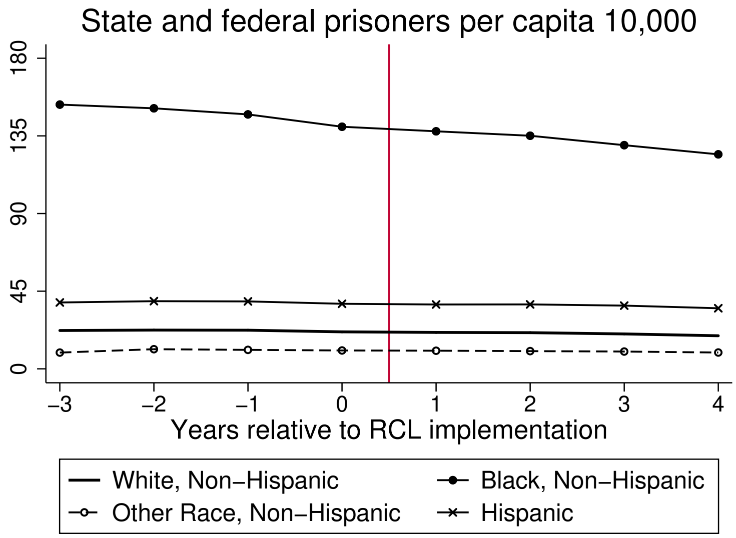


Figure 2: Prisoner rates, by time since RCL implementation

Notes: Prisoner data are from the 2009-2019 National Prisoner Statistics. 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. Race-specific population weighted averages calculated for time periods relative to RCL implementation. The time $t = 0$ corresponds to the period immediately before RCL implementation. RCL=Recreational cannabis laws.

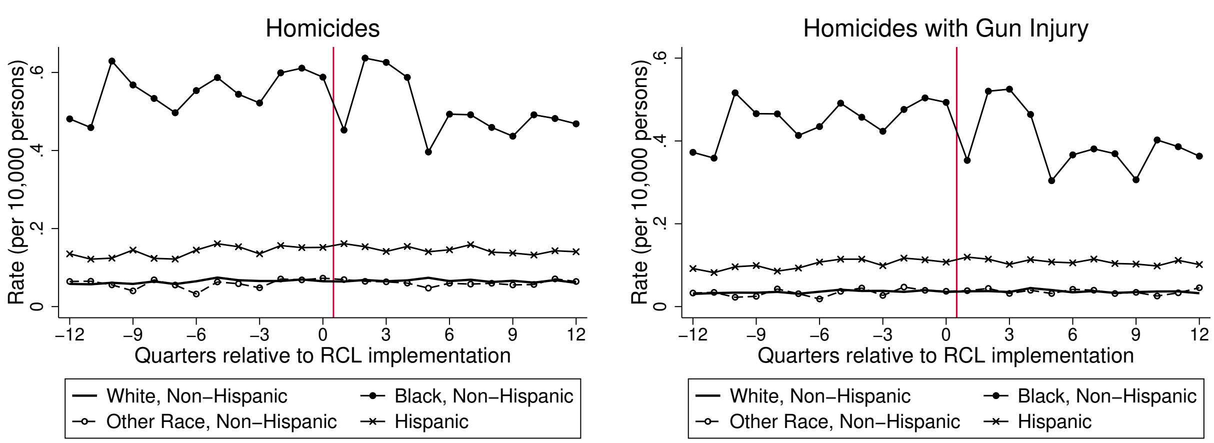


Figure 3: Homicide rates, by time since RCL implementation

Notes: Death data are from the 2007-2019 NVSS Mortality Files. The unit of analysis is a state-year-quarter. Death 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. Race-specific population weighted averages calculated for time periods relative to RCL implementation. The time $t = 0$ corresponds to the period immediately before RCL implementation. RCL=Recreational cannabis laws.

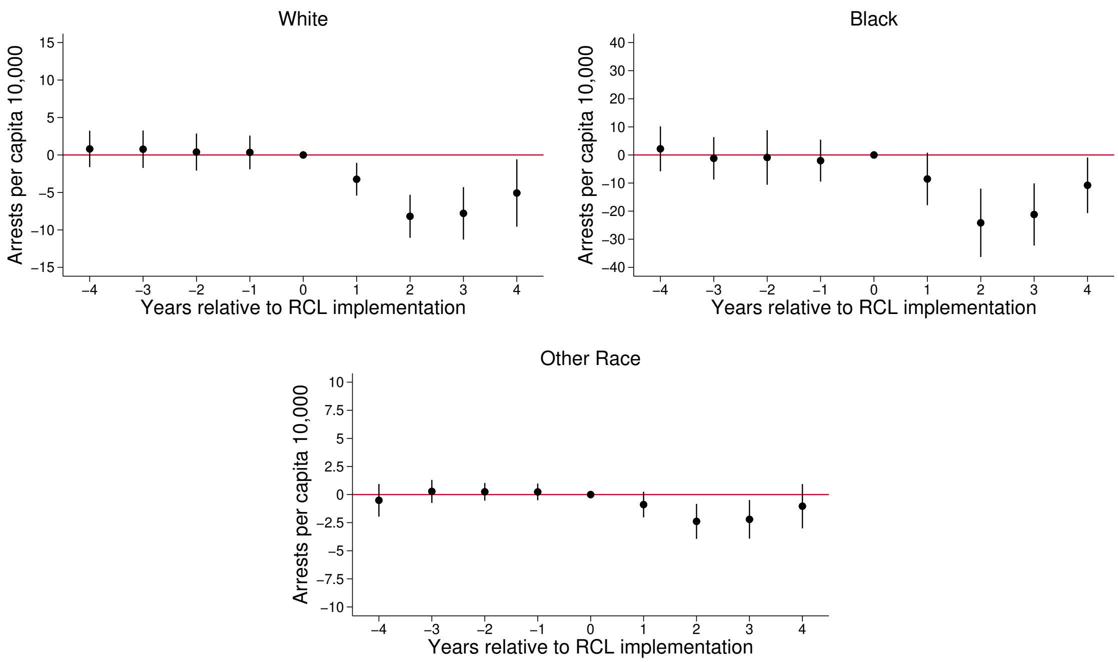


Figure 4: Cannabis arrest rates, event study

Notes: Arrest 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). Regressions are weighted by race-specific population estimates. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

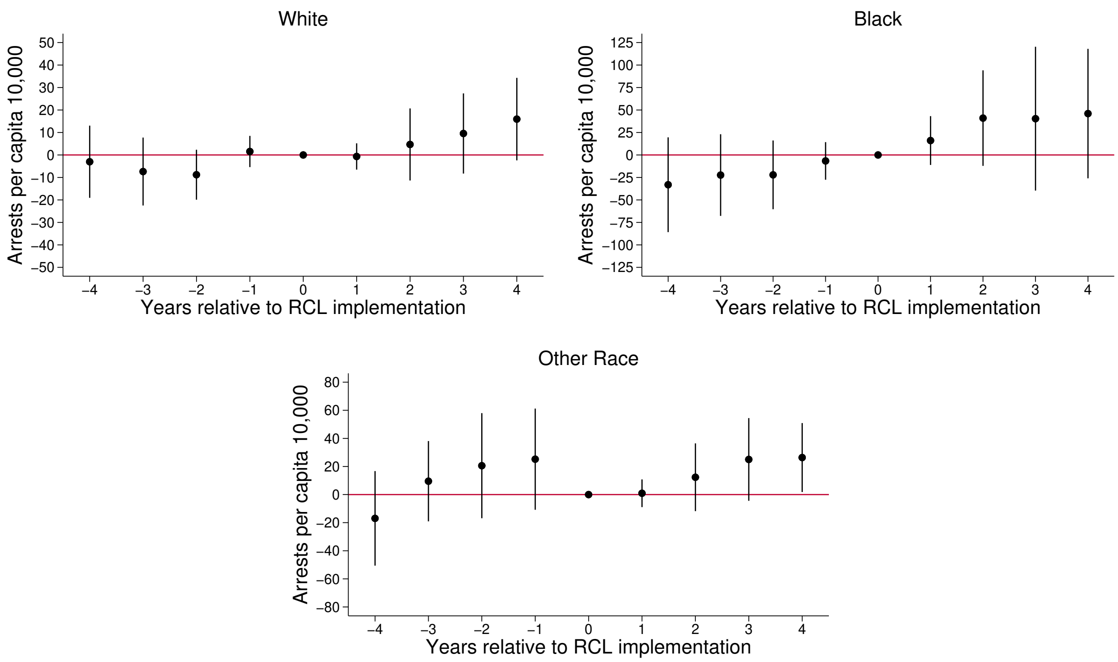


Figure 5: Non-drug arrest rates, event study

Notes: Arrest 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). Regressions are weighted by race-specific population. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

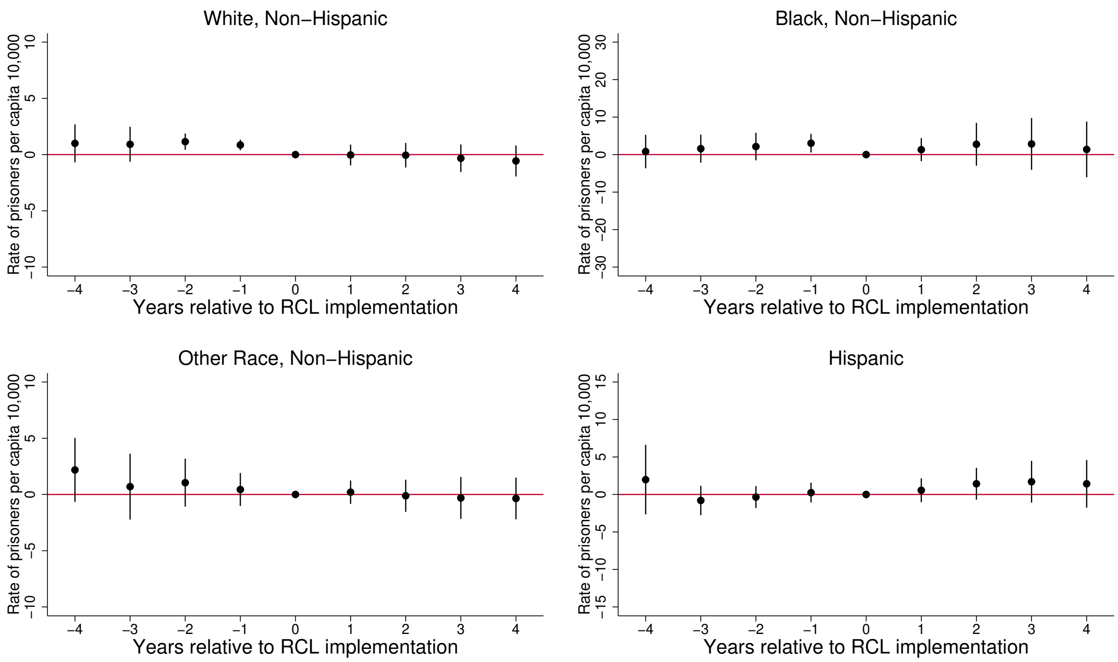


Figure 6: State and federal prisoner rates, event study

Notes: Prisoner data are from the 2009-2019 National Prisoner Statistics. The unit of analysis is a state-year. Counts for a given racial or ethnic group are divided by state-year population estimates corresponding to that racial or 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). Regressions are weighted by race or ethnicity-specific population. Controls include cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

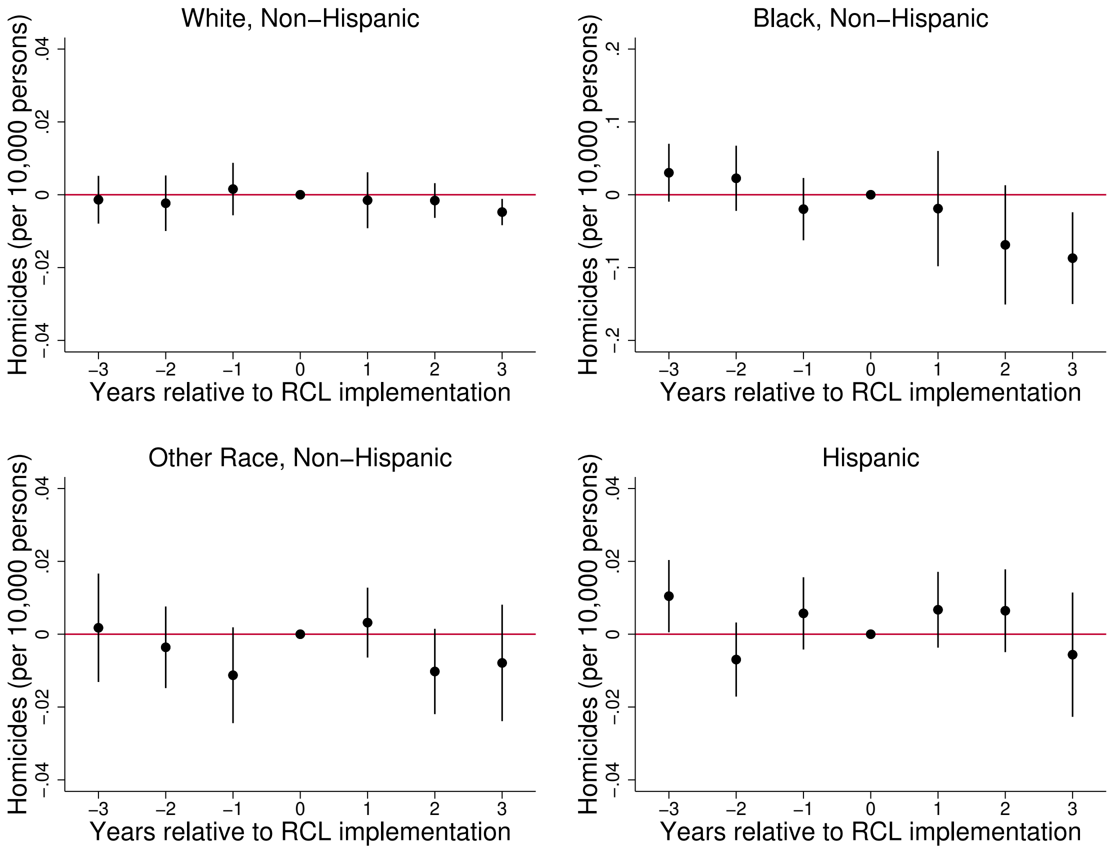


Figure 7: Homicide rates, event study

Notes: Homicide data are from the 2007-2019 NVSS Mortality Files. The unit of analysis is a state-year-quarter. Counts for a given racial or ethnic group are divided by state-year population estimates corresponding to that racial or 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). Regressions are weighted by race or ethnicity-specific population. Controls include cannabis decriminalization laws. The reference year is $t = 0$, the year (four quarters) immediately before RCL implementation. RCL=Recreational cannabis laws.

Table 1: Summary statistics for arrest rates

	RCL States Pre-Policy		RCL States Post-Policy		Non-RCL States		All States	
	Mean	N	Mean	N	Mean	N	Mean	N
Rate of cannabis arrests per 10,000 persons								
Population	15.40	1871	4.24	952	26.97	17976	22.48	20799
White	14.56	1871	4.22	952	21.03	17976	18.09	20799
Black	40.92	1871	9.24	952	62.54	17976	57.13	20799
Other Race	2.47	1871	1.32	952	7.68	17976	5.07	20799
Rate of other drug arrests per 10,000 persons								
Population	36.18	1871	36.39	952	23.45	17976	27.18	20799
White	36.95	1871	39.15	952	20.07	17976	25.25	20799
Black	72.51	1871	60.85	952	46.09	17976	50.03	20799
Other Race	5.94	1871	7.13	952	5.86	17976	6.10	20799
Rate of non-drug arrests per 10,000 persons								
Population	300.55	1871	261.68	952	334.53	17976	320.76	20799
White	297.63	1871	261.40	952	281.53	17976	282.63	20799
Black	652.77	1871	594.74	952	647.08	17976	644.98	20799
Other Race	67.28	1871	75.47	952	163.83	17976	120.56	20799
Rate of total arrests per 10,000 persons								
Population	352.12	1871	302.30	952	385.10	17976	370.53	20799
White	349.13	1871	304.77	952	322.76	17976	326.06	20799
Black	766.18	1871	670.68	952	755.99	17976	752.69	20799
Other Race	75.70	1871	83.92	952	177.39	17976	131.75	20799

Notes: Arrest 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 prisoner rates

	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	22.65	63	21.12	44	28.17	440	26.91	547
Black, Non-Hispanic	148.33	63	130.02	44	131.72	440	132.78	547
Other Race, Non-Hispanic	12.14	63	12.11	44	16.12	439	14.68	546
Hispanic	38.76	42	37	39	42.85	383	41.17	464

Notes: Prisoner 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 or ethnicity-specific population. RCL=Recreational cannabis laws.

Table 3: Summary statistics for homicide rates

	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	402	0.12	170	0.15	2080	0.14	2652
White, Non-Hispanic	0.06	402	0.07	170	0.07	2080	0.07	2652
Black, Non-Hispanic	0.62	402	0.51	170	0.52	2080	0.53	2652
Other Race, Non-Hispanic	0.06	402	0.07	170	0.08	2080	0.07	2652
Hispanic	0.16	402	0.15	170	0.13	2080	0.14	2652
Rate of homicides with gun injury per 10,000 persons								
Population	0.09	402	0.08	170	0.11	2080	0.10	2652
White, Non-Hispanic	0.03	402	0.04	170	0.04	2080	0.04	2652
Black, Non-Hispanic	0.51	402	0.41	170	0.43	2080	0.44	2652
Other Race, Non-Hispanic	0.04	402	0.04	170	0.04	2080	0.04	2652
Hispanic	0.12	402	0.11	170	0.09	2080	0.10	2652

Notes: Homicide 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 or ethnicity-specific population. RCL=Recreational cannabis laws.

Table 4: 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	-7.322***	n.a.	n.a.	0.468	n.a.	n.a.	22.003**	n.a.	n.a.	15.251	n.a.	n.a.
	(2.385)			(1.176)			(9.763)			(9.663)		
White	-7.385***	n.a.	n.a.	0.230	n.a.	n.a.	10.820	n.a.	n.a.	3.739	n.a.	n.a.
	(2.077)			(1.335)			(9.571)			(9.372)		
Black	-19.839***	-0.312***	-14.401***	-2.484	0.129	-3.339	60.363**	0.100*	50.944**	46.099	0.071	41.151*
	(6.544)	(0.103)	(5.044)	(2.949)	(0.124)	(2.660)	(28.538)	(0.058)	(19.033)	(31.138)	(0.069)	(22.563)
Other Race	-1.756*	0.110***	4.185***	-1.522*	-0.013	-3.711**	8.259	-0.006	-2.749	5.006	0.006	-2.362
	(0.875)	(0.039)	(1.280)	(0.854)	(0.032)	(1.383)	(17.348)	(0.058)	(15.453)	(18.227)	(0.051)	(15.984)

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

Table 5: Effect of recreational cannabis laws on arrests, by crime categories

	Drug Possession		Drug Sales		Part 1		Part 2
	Cannabis	Other Drugs	Cannabis	Other Drugs	Violent	Property	
Population	-6.147**	1.213	-1.175***	-0.745**	0.229	0.926	20.848**
	(2.409)	(0.998)	(0.239)	(0.323)	(0.533)	(1.564)	(9.056)
White	-6.255***	1.312	-1.130***	-1.082***	-0.115	-0.788	11.723
	(2.171)	(1.236)	(0.230)	(0.237)	(0.378)	(1.581)	(8.855)
Black	-14.641**	0.568	-5.198***	-3.052**	0.413	0.127	59.823**
	(6.562)	(1.975)	(0.794)	(1.292)	(2.819)	(4.062)	(25.251)
Other Race	-1.563*	-1.277	-0.193*	-0.245*	-0.428	2.205	6.483
	(0.818)	(0.790)	(0.112)	(0.136)	(0.957)	(1.626)	(15.560)

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. 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. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each coefficient is based on separate two-way fixed effects regression (see Equation 1). Regressions are weighted by race-specific population. All regressions include county and year fixed effects. Control variables include the number of reporting agencies and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: 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: Prisoner data are from the 2009-2019 National Prisoner Statistics. The unit of analysis is a state-year. Effect of recreational cannabis laws on prisoner rates, rate ratios, and rate differences, by race and ethnicity. 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 Non-Hispanic White group. Each coefficient is based on separate two-way fixed effects regressions (see Equation 1). Regressions are weighted by race or ethnicity-specific population. All regressions include state and year fixed effects. Control variables include cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Effect of recreational cannabis laws on homicides

	Homicides			Homicides with Gun Injury		
	Rate	Rate Ratio	Rate Difference	Rate	Rate Ratio	Rate Difference
Population	-0.014	n.a.	n.a.	-0.015**	n.a.	n.a.
	(0.009)			(0.008)		
White, Non-Hispanic	0.001	n.a.	n.a.	-0.000	n.a.	n.a.
	(0.004)			(0.003)		
Black, Non-Hispanic	-0.105**	-1.045	-0.104**	-0.109**	-1.027	-0.107**
	(0.045)	(0.738)	(0.043)	(0.044)	(1.505)	(0.043)
Other Race, Non-Hispanic	-0.000	-0.008	-0.001	-0.001	-0.063	-0.001
	(0.008)	(0.071)	(0.006)	(0.007)	(0.138)	(0.005)
Hispanic	-0.002	0.027	-0.003	-0.003	0.091	-0.003
	(0.008)	(0.079)	(0.006)	(0.008)	(0.154)	(0.006)

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

Supplementary Materials

A Effective Dates

Table S1: Effective dates of cannabis liberalization policies

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

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

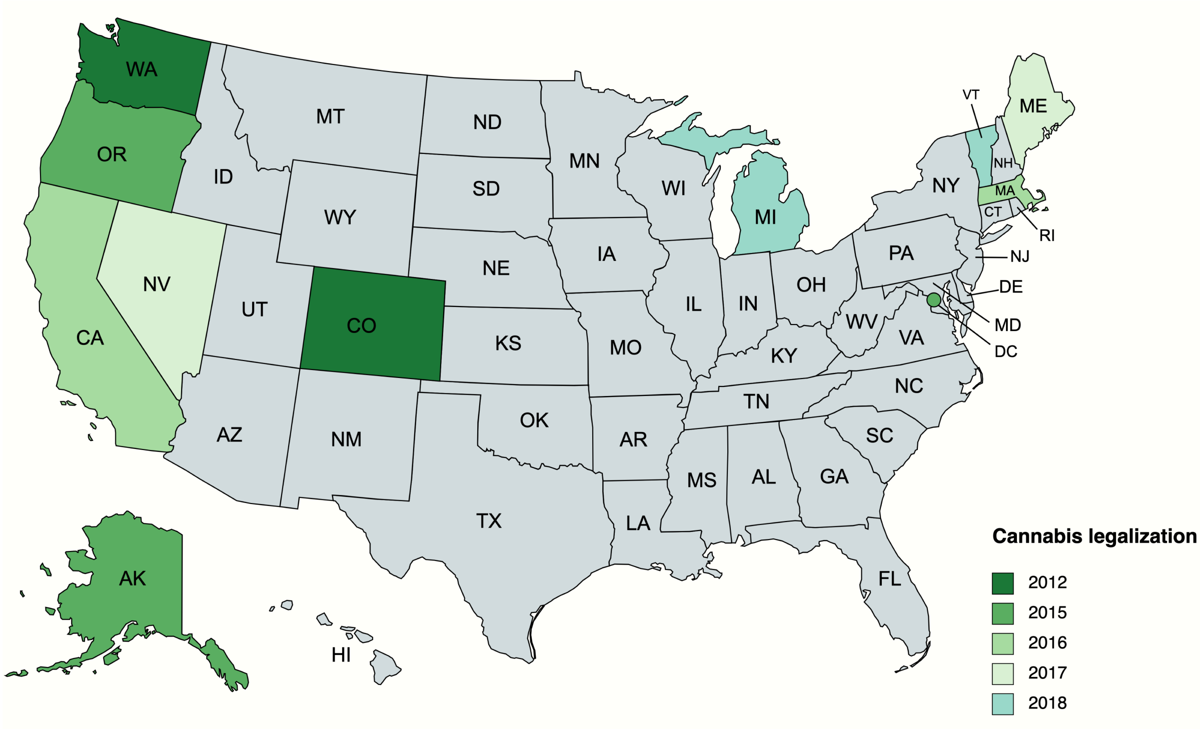


Figure S1: Implementation of recreational cannabis laws by state

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

B Event Study Regressions

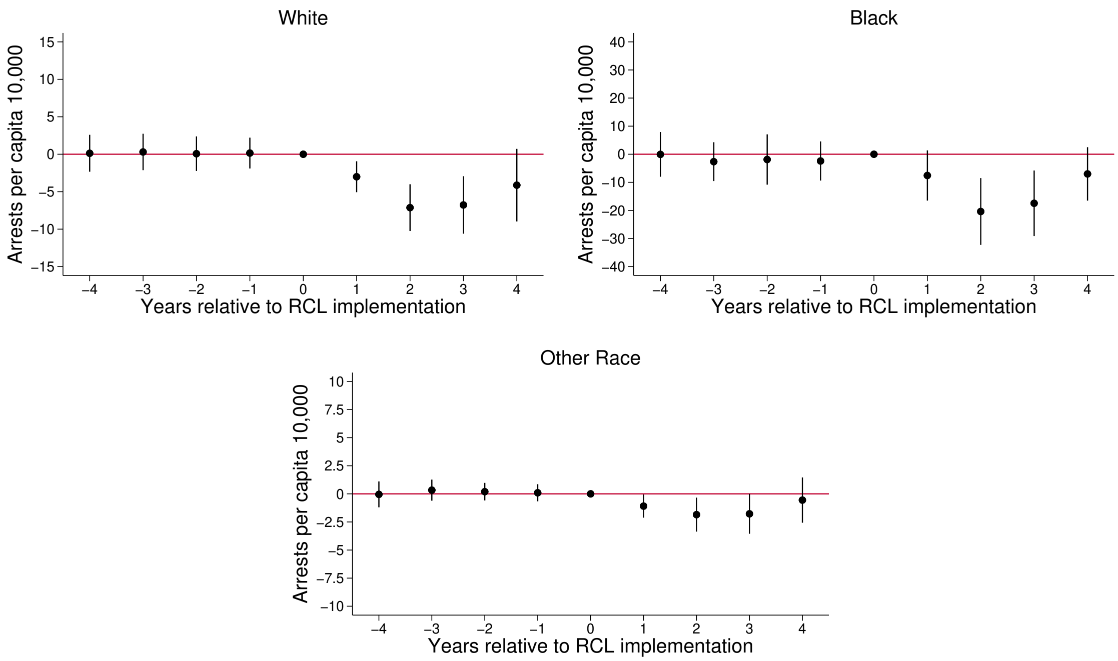


Figure S2: Cannabis possession arrest rates, event study

Notes: Arrest 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). Regressions are weighted by race-specific population. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

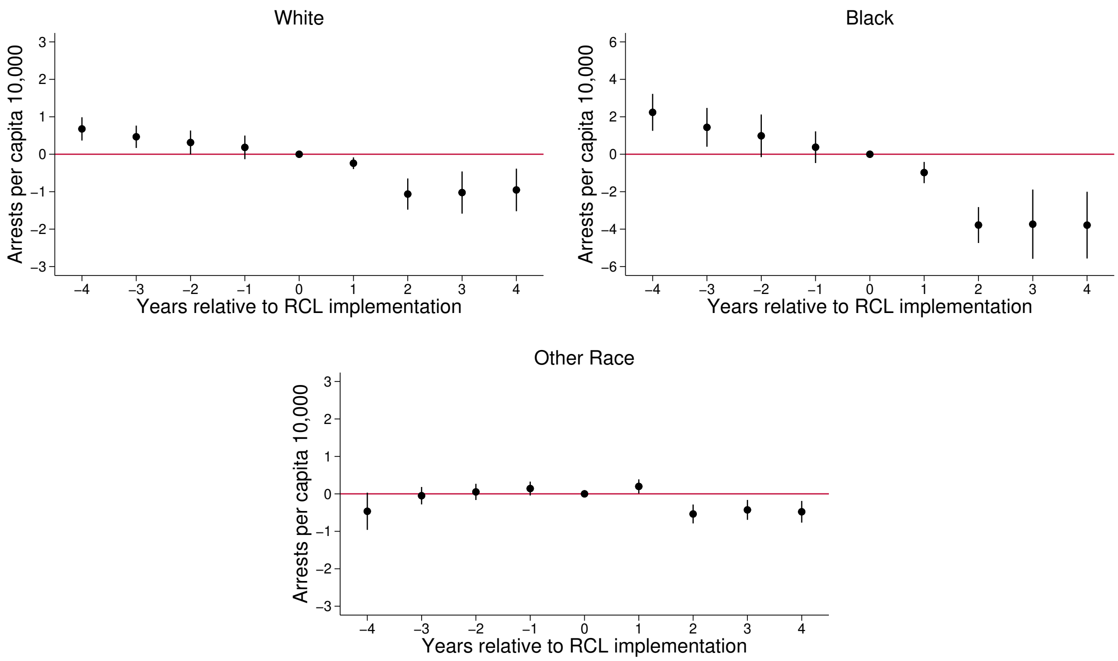


Figure S3: Cannabis sales arrest rates, event study

Notes: Arrest 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). Regressions are weighted by race-specific population. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

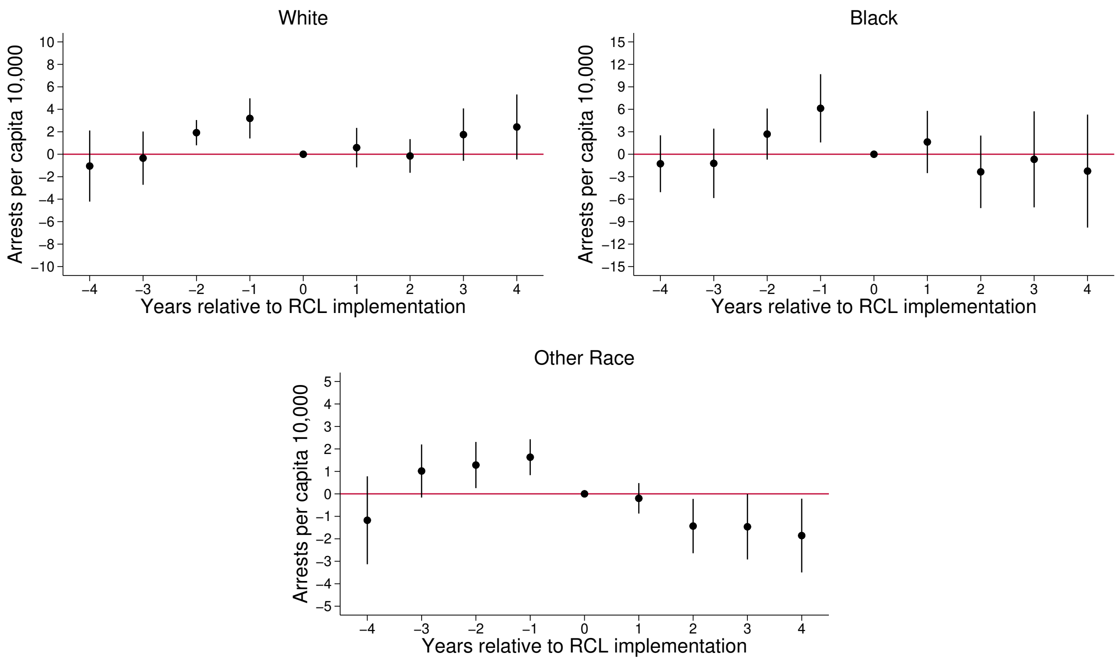


Figure S4: Other drug arrest rates, event study

Notes: Arrest 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). Regressions are weighted by race-specific population. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

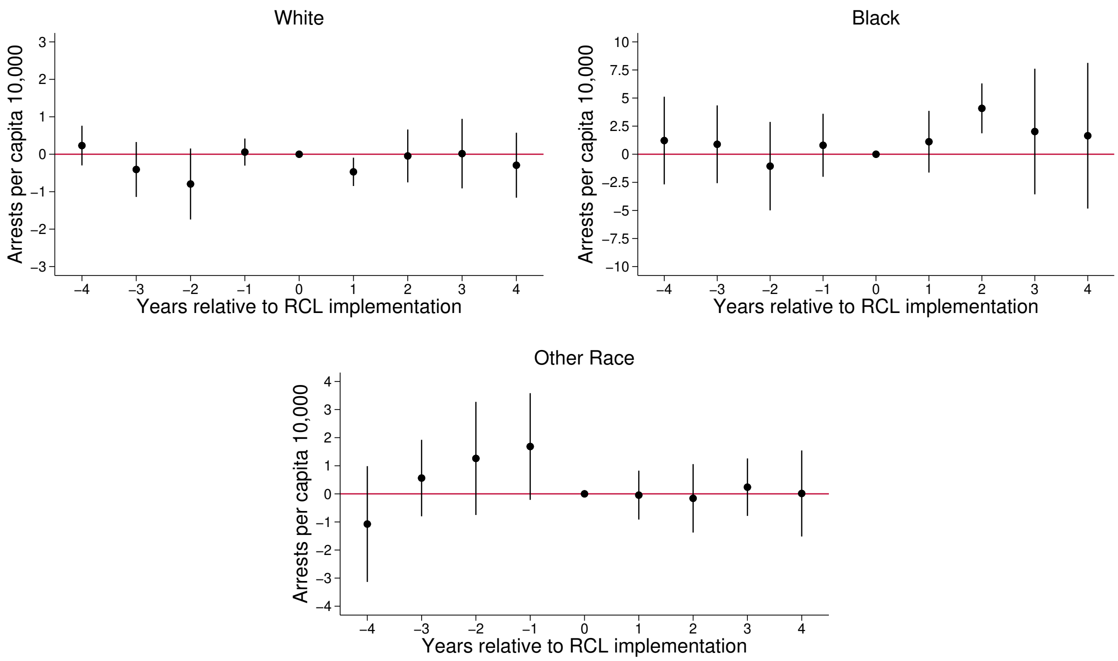


Figure S5: Violent crime arrests (Part 1), event study

Notes: Arrest 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). Regressions are weighted by race-specific population. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

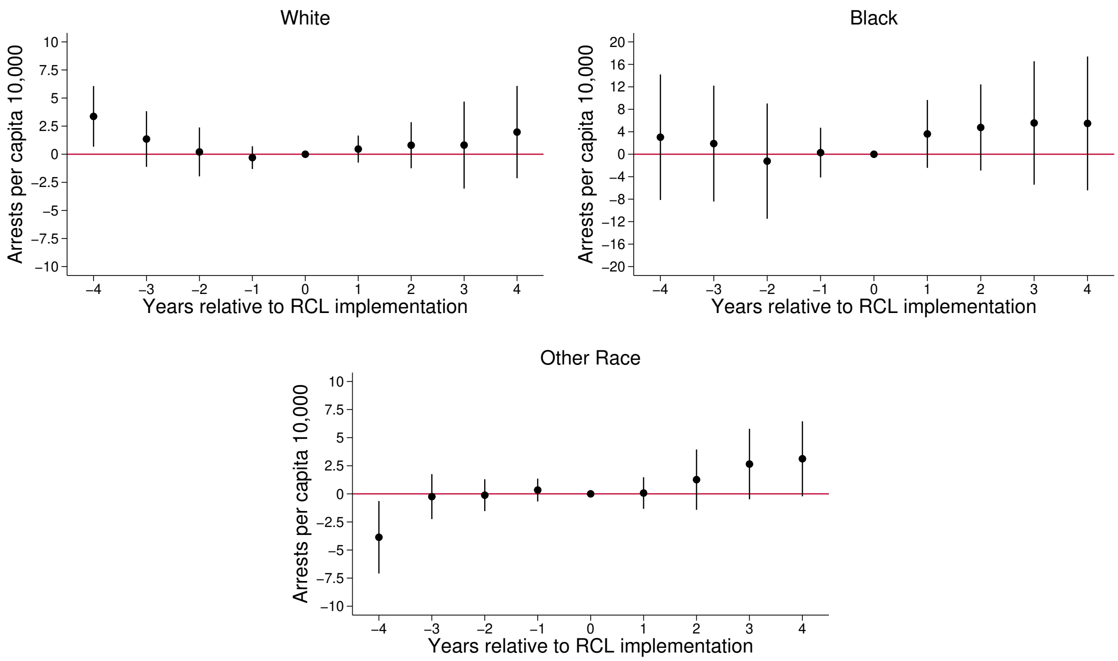


Figure S6: Property crime arrests (Part 1), event study

Notes: Arrest 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). Regressions are weighted by race-specific population. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

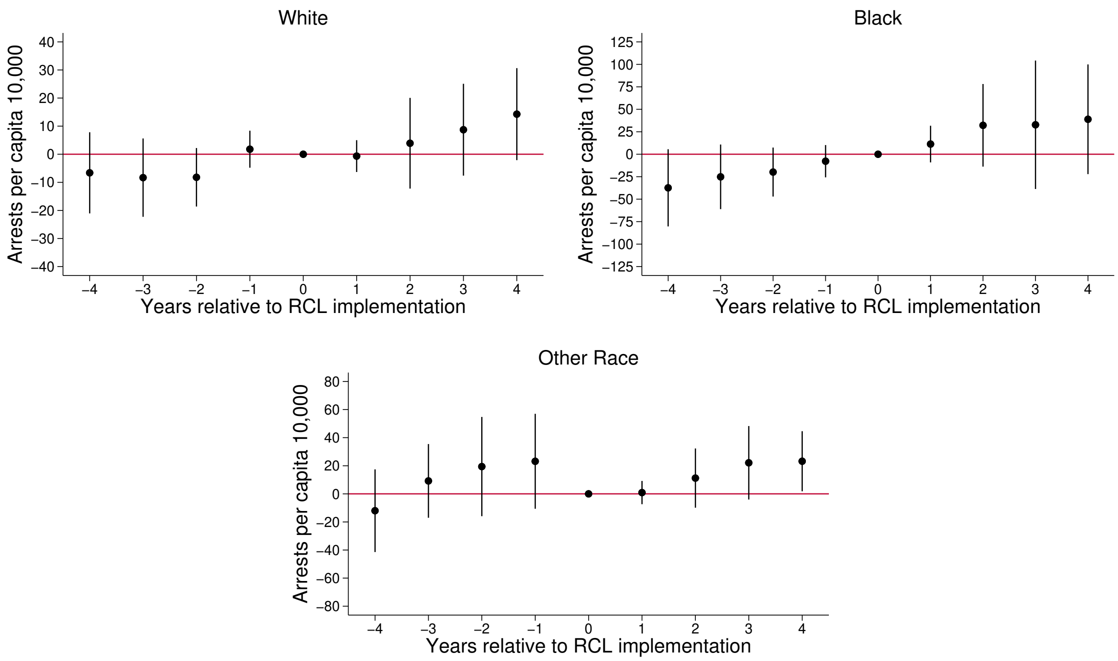


Figure S7: Part 2 crimes arrest rates, event study

Notes: Arrest 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). Regressions are weighted by race-specific population. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

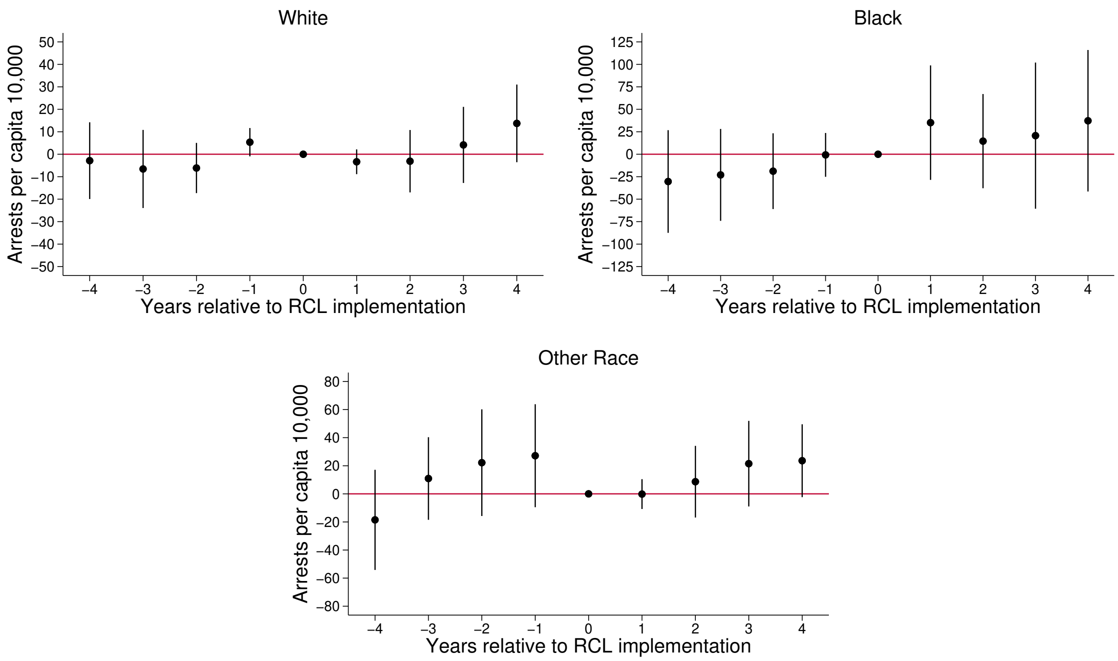


Figure S8: Total arrest rates, event study

Notes: Arrest 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). Regressions are weighted by race-specific population. Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

C Robustness Checks

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

Rate Outcomes	Percentage	Sum
Cannabis Arrests	2.5%	-0.003
Non-drug Arrests	2.5%	-0.003
Total Arrests	2.9%	-0.008
Prisoners	0%	0
Homicide Deaths	0%	0
Assault Hospitalizations	0%	0

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

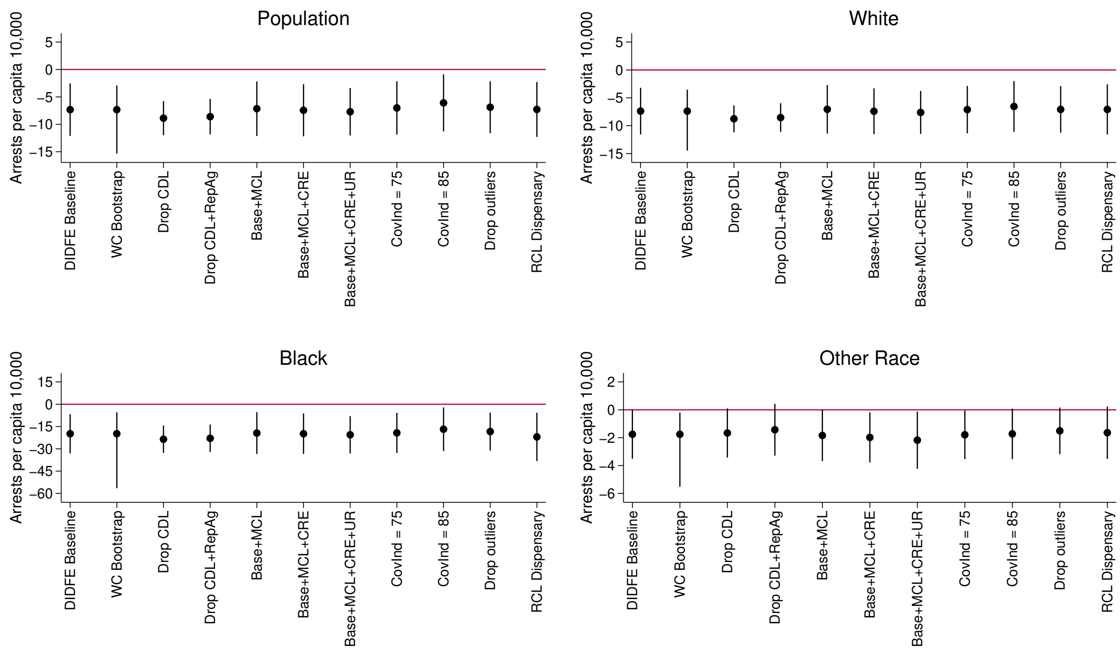


Figure S9: Cannabis arrest rates, robustness checks

Notes: Arrest 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. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. DIDFE Baseline is the main specification. WC Bootstrap calculates wild cluster bootstrap standard errors. Controls that are added or dropped include cannabis decriminalization laws (CDL), the number of reporting agencies (RepAg), medical cannabis laws (MCL), criminal record expungement laws (CRE), and the unemployment rate (UR). Sample is restricted to counties with an agency reporting coverage threshold (CovInd) above or equal to 65% unless otherwise noted. Outliers are arrest rates above 2 standard deviations from the county-level mean. The last specification replaces the RCL indicator with an indicator for recreational cannabis dispensary laws.

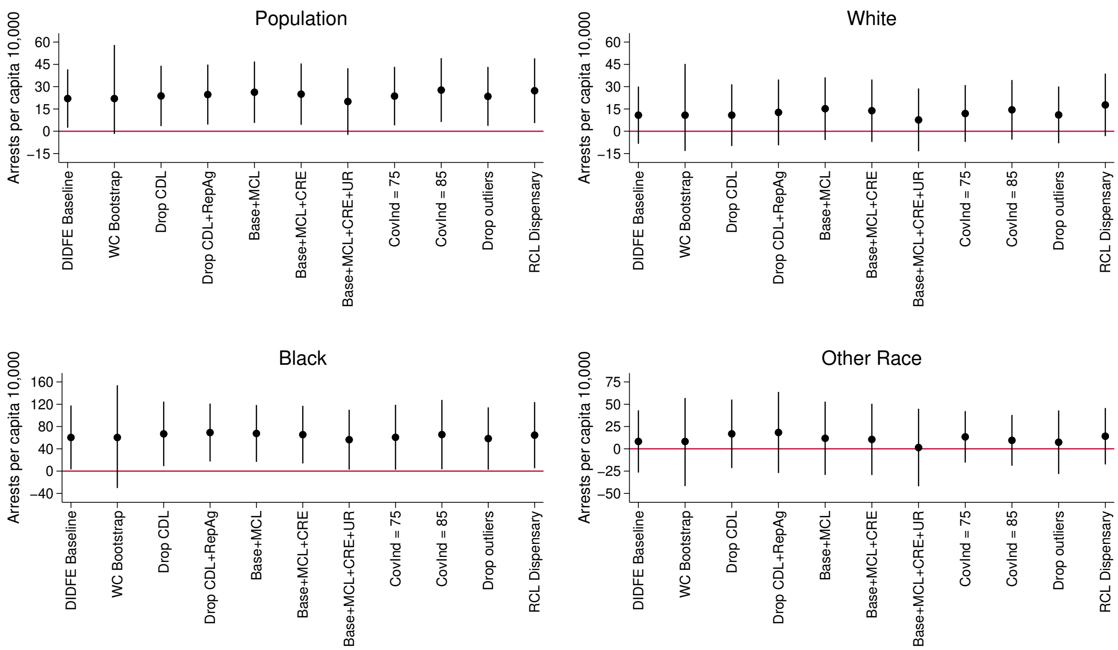


Figure S10: Non-drug arrest rates, robustness checks

Notes: Arrest 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. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. DIDFE Baseline is the main specification. WC Bootstrap calculates wild cluster bootstrap standard errors. Controls that are added or dropped include cannabis decriminalization laws (CDL), the number of reporting agencies (RepAg), medical cannabis laws (MCL), criminal record expungement laws (CRE), and the unemployment rate (UR). Sample is restricted to counties with an agency reporting coverage threshold (CovInd) above or equal to 65% unless otherwise noted. Outliers are arrest rates above 2 standard deviations from the county-level mean. The last specification replaces the RCL indicator with an indicator for recreational cannabis dispensary laws.

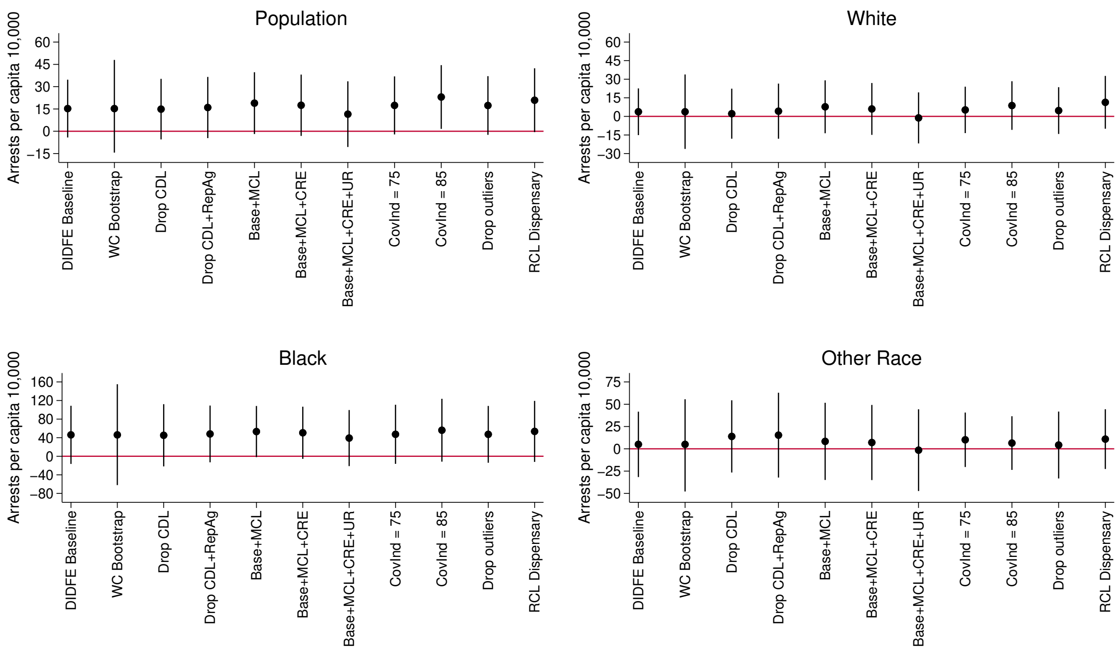


Figure S11: Total arrest rates, robustness checks

Notes: Arrest 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. Each coefficient corresponds to a separate two-way fixed effects regression (see Equation 1). Bars denote 95% confidence intervals from standard errors clustered at the state level. DIDFE Baseline is the main specification. WC Bootstrap calculates wild cluster bootstrap standard errors. Controls that are added or dropped include cannabis decriminalization laws (CDL), the number of reporting agencies (RepAg), medical cannabis laws (MCL), criminal record expungement laws (CRE), and the unemployment rate (UR). Sample is restricted to counties with an agency reporting coverage threshold (CovInd) above or equal to 65% unless otherwise noted. Outliers are arrest rates above 2 standard deviations from the county-level mean. The last specification replaces the RCL indicator with an indicator for recreational cannabis dispensary laws.

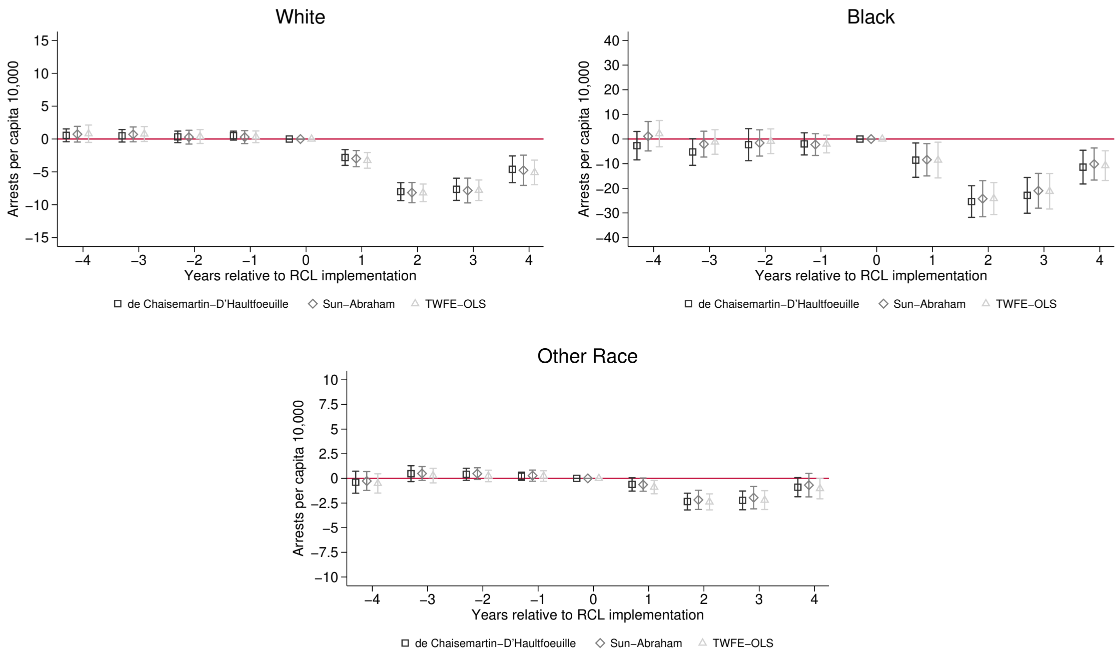


Figure S12: Cannabis arrest rates, alternative DID estimators

Notes: Arrest 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%. Each series corresponds to a different DID estimator: the Wald-TC estimator proposed in De Chaisemartin and D'Haultfoeuille (2022), the interaction-weighted estimator proposed in Sun and Abraham (2021), and the standard two-way fixed effects OLS estimator. Bars denote 95% confidence intervals from robust standard errors clustered at the county level (for the de Chaisemartin-D'Haultfoeuille estimator, errors are obtained from 50 bootstrap repetitions). Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

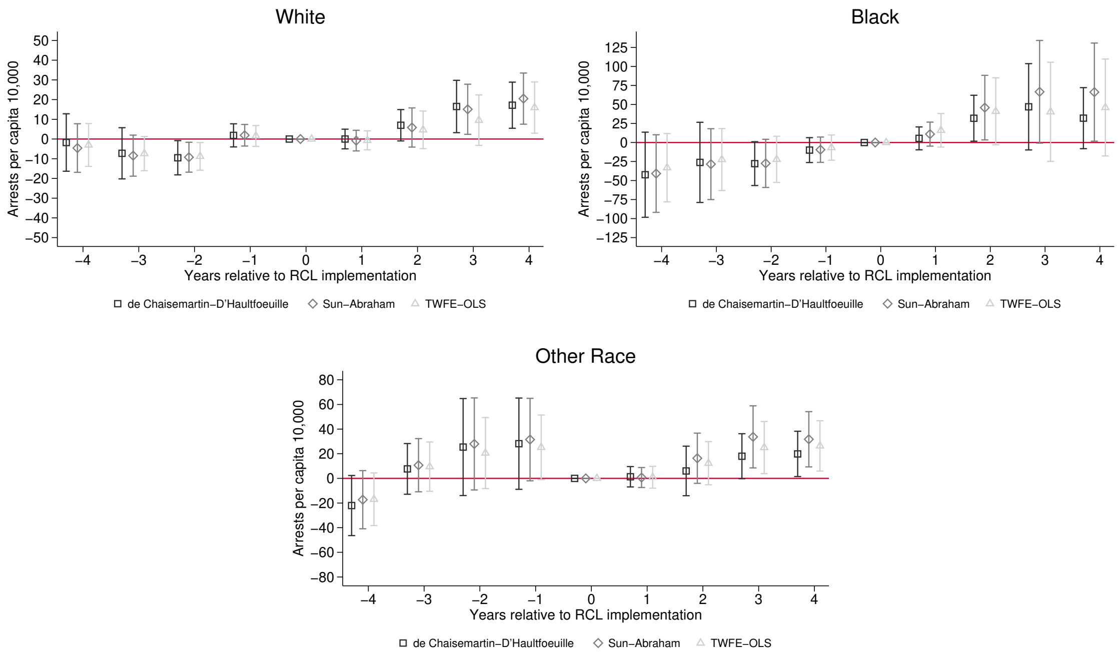


Figure S13: Non-drug arrest rates, alternative DID estimators

Notes: Arrest 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%. Each series corresponds to a different DID estimator: the Wald-TC estimator proposed in De Chaisemartin and D'Haultfoeuille (2022), the interaction-weighted estimator proposed in Sun and Abraham (2021), and the standard two-way fixed effects OLS estimator. Bars denote 95% confidence intervals from robust standard errors clustered at the county level (for the de Chaisemartin-D'Haultfoeuille estimator, errors are obtained from 50 bootstrap repetitions). Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

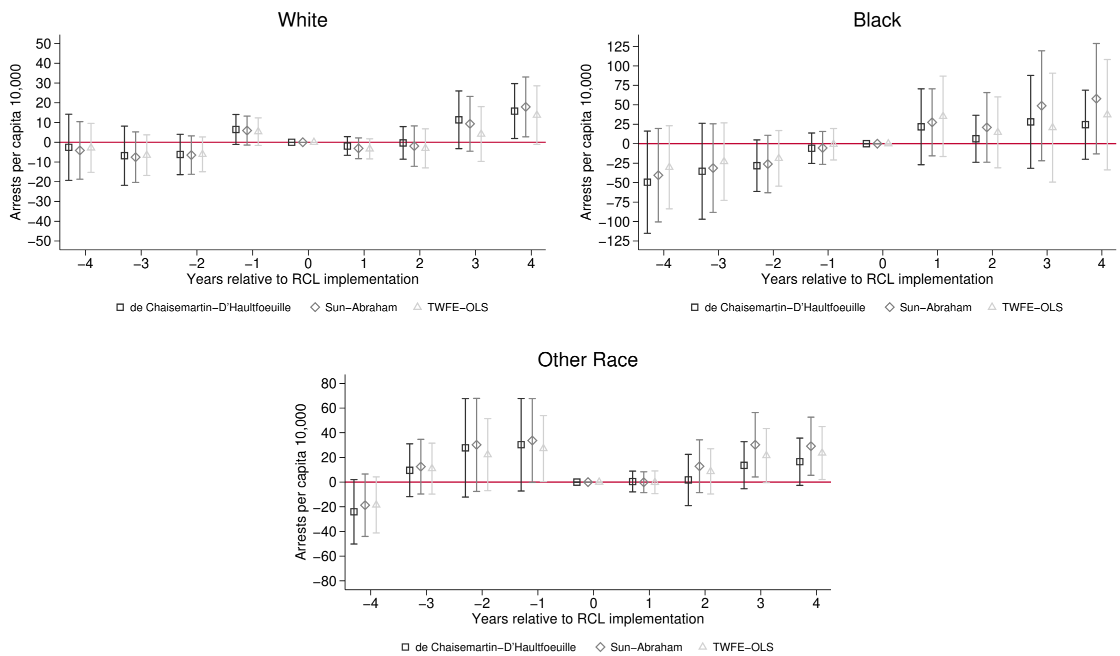


Figure S14: Total arrest rates, alternative DID estimators

Notes: Arrest 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%. Each series corresponds to a different DID estimator: the Wald-TC estimator proposed in De Chaisemartin and D'Haultfoeuille (2022), the interaction-weighted estimator proposed in Sun and Abraham (2021), and the standard two-way fixed effects OLS estimator. Bars denote 95% confidence intervals from robust standard errors clustered at the county level (for the de Chaisemartin-D'Haultfoeuille estimator, errors are obtained from 50 bootstrap repetitions). Controls include the number of reporting agencies and cannabis decriminalization laws. The reference year is $t = 0$, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

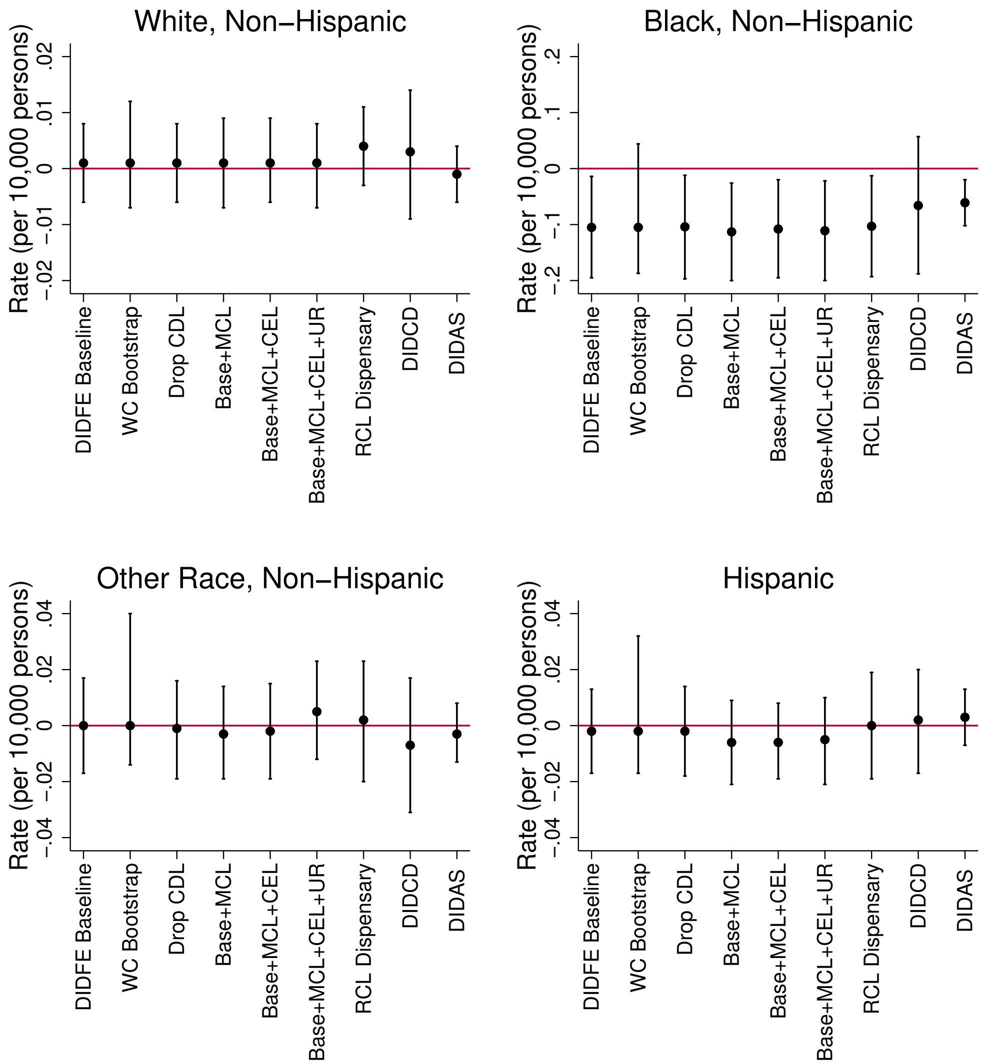


Figure S15: Homicide rates, robustness checks

Notes: Homicide data are from the 2007-2019 NVSS Mortality Files. The unit of analysis is a state-year-quarter. Counts for a given racial or ethnic group are divided by state-year population estimates corresponding to that racial or ethnic group, and multiplied by 10,000. Each coefficient is based on separate two-way fixed effects regressions (see Equation 1). Regressions are weighted by race-specific population. All regressions include state and year-quarter fixed effects, and control for CDLs unless stated otherwise. DIDFE=Two-way fixed effect difference-in-differences estimator. DIDCD=Multiperiod difference-in-differences estimator described in De Chaisemartin and D'Haultfoeuille (2022) capturing the average effect in the first three years post RCLs. DIDAS=Interaction weighted difference-in-differences estimator described in Sun and Abraham (2021) capturing the average effect in the first three years post RCLs. WC Bootstrap=Wild cluster bootstrap. RCL=Recreational cannabis laws. MCL=Medical cannabis laws. CDL=Cannabis decriminalization laws. CEL=Cannabis record expungement laws. UR=Unemployment rate.

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

	Cannabis arrests				Non-drug arrests				Total arrests			
Population	-7.322***	-7.145***	-7.399***	-8.596***	22.003**	26.252**	21.396*	24.694**	15.251	18.918*	13.053	15.973
	(2.385)	(2.476)	(2.258)	(1.607)	(9.763)	(10.269)	(11.259)	(10.025)	(9.663)	(10.334)	(11.094)	(10.234)
White	-7.385***	-7.062***	-7.264***	-8.542***	10.820	15.185	9.097	12.684	3.739	7.748	0.624	4.209
	(2.077)	(2.165)	(2.025)	(1.275)	(9.571)	(10.477)	(10.604)	(11.001)	(9.372)	(10.622)	(10.423)	(11.065)
Black	-19.839***	-19.409***	-19.966***	-22.931***	60.363**	67.593**	59.323**	69.184**	46.099	53.192*	42.793	48.056
	(6.544)	(7.014)	(6.504)	(4.596)	(28.538)	(25.375)	(26.777)	(25.826)	(31.138)	(27.357)	(29.752)	(30.376)
Other Race	-1.756*	-1.840*	-2.038*	-1.432	8.259	11.801	2.695	18.331	5.006	8.362	-0.218	15.342
	(0.875)	(0.917)	(1.033)	(0.923)	(17.348)	(20.428)	(22.109)	(22.602)	(18.227)	(21.513)	(23.280)	(23.643)
Controls:												
CDL	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No
MCL	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No
UR	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
RepAg	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. Effect of recreational cannabis laws on arrest rates, by race. Ethnicity is not available. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each coefficient is based on separate two-way fixed effects regression (see Equation 1). Each column 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 cannabis decriminalization laws (CDL), medical cannabis laws (MCL), and unemployment rates (UR). The reporting agency control (RepAg) 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

	Cannabis arrests				Non-drug arrests				Total arrests			
Population	-7.322***	-7.004***	-6.088**	-6.880***	22.003**	23.654**	27.712**	23.453**	15.251	17.393*	23.029**	17.329*
	(2.385)	(2.414)	(2.588)	(2.350)	(9.763)	(9.782)	(10.678)	(9.856)	(9.663)	(9.708)	(10.658)	(9.811)
White	-7.385***	-7.116***	-6.553***	-7.084***	10.820	11.934	14.413	11.003	3.739	5.230	8.754	4.695
	(2.077)	(2.106)	(2.260)	(2.078)	(9.571)	(9.499)	(9.976)	(9.496)	(9.372)	(9.314)	(9.753)	(9.380)
Black	-19.839***	-19.272***	-16.821**	-18.374***	60.363**	60.685**	65.577**	58.428**	46.099	47.295	56.070	47.208
	(6.544)	(6.704)	(7.297)	(6.360)	(28.538)	(28.937)	(30.904)	(27.788)	(31.138)	(31.630)	(33.643)	(30.399)
Other Race	-1.756*	-1.792**	-1.728*	-1.510*	8.259	13.423	9.523	7.384	5.006	10.140	6.474	4.270
	(0.875)	(0.867)	(0.901)	(0.831)	(17.348)	(14.373)	(14.141)	(17.711)	(18.227)	(15.176)	(14.955)	(18.649)
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: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. Effect of recreational cannabis laws on arrest rates, by race. Ethnicity is not available. 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 regression (see Equation 1). 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 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 excluding each state for the effect of recreational cannabis laws on arrests per 10,000 persons

Excluded state:	AK	CA	CO	MA	ME	MI	NV	OR	VT	WA
<u>Panel A: Cannabis arrests</u>										
Population	-7.280***	-12.069***	-7.109***	-7.445***	-7.174***	-7.036***	-6.835***	-6.758***	-7.338***	-6.891***
	(2.399)	(1.497)	(2.486)	(2.513)	(2.343)	(2.437)	(2.241)	(2.230)	(2.398)	(2.385)
White	-7.351***	-10.894***	-7.148***	-7.554***	-7.213***	-7.415***	-7.116***	-6.704***	-7.405***	-6.938***
	(2.089)	(1.559)	(2.165)	(2.224)	(2.031)	(2.259)	(2.043)	(1.836)	(2.091)	(2.061)
Black	-19.902***	-28.887***	-19.970***	-20.476***	-19.765***	-18.882**	-16.563***	-19.610***	-19.853***	-19.299***
	(6.612)	(7.321)	(6.960)	(7.078)	(6.540)	(7.414)	(4.898)	(6.610)	(6.555)	(6.615)
Other Race	-1.429*	-4.810***	-1.849*	-1.772*	-1.744*	-1.788*	-1.614*	-1.611*	-1.756*	-1.410*
	(0.730)	(1.436)	(0.944)	(0.888)	(0.873)	(0.913)	(0.843)	(0.834)	(0.876)	(0.772)
<u>Panel B: Non-drug arrests</u>										
Population	23.390**	35.239**	18.123**	22.254**	22.116**	21.639**	22.039**	18.220**	21.977**	24.984**
	(9.960)	(14.517)	(8.830)	(9.929)	(9.846)	(10.128)	(9.994)	(8.667)	(9.759)	(10.178)
White	11.931	24.565*	6.769	10.914	10.999	9.742	11.038	6.863	10.765	14.152
	(9.708)	(14.563)	(8.721)	(9.694)	(9.692)	(9.752)	(9.764)	(8.514)	(9.561)	(9.919)
Black	61.531**	99.259***	53.362*	61.144**	60.883**	48.109*	60.314**	54.907*	60.226**	66.378**
	(28.846)	(31.894)	(27.518)	(29.514)	(28.682)	(27.269)	(29.837)	(27.442)	(28.522)	(29.787)
Other Race	13.940	-5.972	6.011	8.073	8.222	7.727	8.217	8.520	8.256	10.309
	(16.455)	(30.456)	(17.743)	(17.349)	(17.363)	(17.768)	(17.257)	(17.175)	(17.364)	(18.257)
<u>Panel C: Total arrests</u>										
Population	16.748*	22.093	11.251	15.689	15.585	15.595	15.801	11.851	15.218	18.986*
	(9.697)	(15.813)	(9.163)	(9.809)	(9.723)	(10.161)	(9.897)	(9.192)	(9.674)	(9.552)
White	4.969	11.221	-0.256	3.942	4.204	2.993	4.330	0.321	3.675	8.124
	(9.365)	(15.948)	(9.029)	(9.533)	(9.445)	(9.755)	(9.519)	(9.025)	(9.385)	(8.850)
Black	47.257	88.971**	38.362	47.278	46.826	36.551	40.718	41.308	45.934	52.780
	(31.430)	(33.396)	(30.012)	(32.037)	(31.303)	(31.996)	(31.021)	(30.355)	(31.120)	(32.371)
Other Race	11.047	-12.796	2.657	4.810	4.971	4.421	5.117	5.431	5.005	7.392
	(17.185)	(31.975)	(18.703)	(18.234)	(18.244)	(18.684)	(18.143)	(18.050)	(18.244)	(19.146)

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. Effect of recreational cannabis laws on arrest rates, by race. Ethnicity is not available. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each coefficient is based on separate two-way fixed effects regression (see Equation 1). Each column excludes one state that passed a recreational cannabis law from the estimation sample. Regressions are weighted by race-specific population. All regressions include county and year fixed effects. Control variables include the number of reporting agencies and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S6: Robustness to potential spillovers for the effect of recreational cannabis laws on arrests per 10,000 persons

Panel A: Cannabis arrests					
Population	-7.322***	-6.993***	-6.042**	-6.751***	-5.954**
	(2.385)	(2.456)	(2.394)	(2.421)	(2.396)
White	-7.385***	-6.917***	-6.180***	-6.687***	-6.072***
	(2.077)	(2.150)	(2.113)	(2.129)	(2.115)
Black	-19.839***	-19.755***	-17.132**	-18.260**	-16.550**
	(6.544)	(7.063)	(6.830)	(6.941)	(6.858)
Other Race	-1.756*	-1.558*	-1.163	-1.582*	-1.178
	(0.875)	(0.885)	(0.945)	(0.906)	(0.949)
Panel B: Non-drug arrests					
Population	22.003**	22.386**	22.992**	23.926**	23.778**
	(9.763)	(9.956)	(10.899)	(10.067)	(10.910)
White	10.820	10.822	9.346	12.451	10.338
	(9.571)	(9.854)	(11.297)	(10.018)	(11.310)
Black	60.363**	60.403**	71.341**	68.361**	74.590***
	(28.538)	(28.936)	(26.780)	(28.190)	(26.705)
Other Race	8.259	8.990	14.974	10.403	15.386
	(17.348)	(19.113)	(24.998)	(20.595)	(25.466)
Panel C: Total arrests					
Population	15.251	15.372	16.532	17.253*	17.427
	(9.663)	(9.998)	(11.051)	(10.111)	(11.075)
White	3.739	3.776	2.546	5.619	3.635
	(9.372)	(9.977)	(11.669)	(10.133)	(11.700)
Black	46.099	45.383	58.740**	56.857*	63.491**
	(31.138)	(31.969)	(28.126)	(31.188)	(28.122)
Other Race	5.006	5.713	11.703	7.039	12.076
	(18.227)	(20.063)	(26.145)	(21.596)	(26.630)
Controls for spillovers:					
RCL within 0-100 miles	No	Yes	Yes	No	Yes
RCL within 100-200 miles	No	No	Yes	No	Yes
Inverse distance to nearest RCL	No	No	No	Yes	Yes

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. Effect of recreational cannabis laws on arrest rates, by race. Ethnicity is not available. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each coefficient is based on separate two-way fixed effects regression (see Equation 1). Each column adds different control variables that account for potential spillovers of RCLs: conditional on not having an RCL, an indicator for whether there is a county within 100 miles with an RCL in place, whether there is a county within 100-200 miles with an RCL, and the inverse distance to the nearest county with an RCL. Regressions are weighted by race-specific population. All regressions include county and year fixed effects. Control variables include the number of reporting agencies and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

D Heterogeneity Analysis

Table S7: Effect of recreational cannabis laws on arrests per 10,000 persons, heterogeneity by decriminalizations laws

		Cannabis arrests	Non-drug arrests	Total arrests
Population:	RCL × Decriminalization	-3.007***	15.087**	13.270*
		(1.077)	(6.137)	(6.986)
	RCL ×(1− Decriminalization)	-13.043***	34.413**	20.872
		(1.484)	(15.678)	(17.208)
	Coefficient test	0.00	0.16	0.62
White:	RCL × Decriminalization	-3.576***	4.834	3.209
		(1.030)	(6.950)	(7.666)
	RCL ×(1− Decriminalization)	-11.856***	22.972	9.087
		(1.557)	(15.872)	(17.473)
	Coefficient test	0.00	0.19	0.70
Black:	RCL × Decriminalization	-10.691***	35.938**	21.102
		(3.503)	(16.904)	(21.344)
	RCL ×(1− Decriminalization)	-32.863***	105.080***	94.835**
		(8.269)	(34.018)	(35.671)
	Coefficient test	0.01	0.02	0.01
Other Race:	RCL × Decriminalization	-0.423	13.909	11.674
		(0.457)	(19.053)	(19.857)
	RCL ×(1− Decriminalization)	-5.128***	-10.038	-16.998
		(1.523)	(30.875)	(32.457)
	Coefficient test	0.00	0.33	0.27

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. Effect of recreational cannabis laws on arrest rates, by race. Ethnicity is not available. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each pair of coefficients is based on separate two-way fixed effects regressions (see Equation 1). The RCL treatment indicator is interacted with an indicator for the presence (or absence) of cannabis decriminalization laws prior to RCL implementation. The p-value of a test of equality of coefficients is shown. All regressions include county and year fixed effects. Control variables include the number of reporting agencies and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S8: Effect of recreational cannabis laws on arrests per 10,000 persons, heterogeneity by recreational dispensaries

		Cannabis arrests	Non-drug arrests	Total arrests
Population:	RCL × Dispensaries	-8.357***	31.078**	23.616*
		(2.970)	(11.882)	(12.136)
	RCL ×(1− Dispensaries)	-5.067***	11.841*	6.851
		(1.726)	(6.234)	(6.803)
	Coefficient test	0.03	0.01	0.03
White:	RCL × Dispensaries	-8.315***	19.808	12.138
		(2.636)	(12.413)	(13.107)
	RCL ×(1− Dispensaries)	-5.380***	2.872	-2.675
		(1.495)	(6.636)	(6.905)
	Coefficient test	0.04	0.02	0.06
Black:	RCL × Dispensaries	-24.064***	76.327**	63.389*
		(8.774)	(30.953)	(33.910)
	RCL ×(1− Dispensaries)	-12.852***	44.272**	30.937
		(3.853)	(21.260)	(22.814)
	Coefficient test	0.05	0.06	0.10
Other Race:	RCL × Dispensaries	-1.894	14.202	10.014
		(1.148)	(23.065)	(24.181)
	RCL ×(1− Dispensaries)	-1.200*	1.011	-1.457
		(0.633)	(17.647)	(18.329)
	Coefficient test	0.26	0.19	0.27

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. Effect of recreational cannabis laws on arrest rates, by race. Ethnicity is not available. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each pair of coefficients is based on separate two-way fixed effects regressions (see Equation 1). The RCL treatment indicator is interacted with an indicator for the presence (or absence) of recreational cannabis dispensary laws. The p-value of a test of equality of coefficients is shown. All regressions include county and year fixed effects. Control variables include the number of reporting agencies and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S9: Effect of recreational cannabis laws on arrests per 10,000 persons, heterogeneity by share of Black population in 2010

		Cannabis arrests	Non-drug arrests	Total arrests
Population:	RCL × high Black share	-5.839**	19.991**	11.656
		(2.397)	(9.409)	(9.142)
	RCL × low Black share	-10.418***	31.170**	31.257*
		(2.731)	(14.676)	(17.476)
	Coefficient test	0.01	0.40	0.23
White:	RCL × high Black share	-5.439***	11.197	3.094
		(1.934)	(10.156)	(9.832)
	RCL × low Black share	-11.515***	15.806	12.893
		(2.413)	(15.241)	(18.481)
	Coefficient test	0.00	0.74	0.57
Black:	RCL × high Black share	-18.826***	60.096**	45.381
		(6.859)	(27.036)	(29.392)
	RCL × low Black share	-21.929***	92.177*	107.012*
		(7.617)	(50.570)	(61.577)
	Coefficient test	0.54	0.55	0.35
Other Race:	RCL × high Black share	-1.334	6.813	2.768
		(0.828)	(20.276)	(21.152)
	RCL × low Black share	-2.741**	14.270	14.130
		(1.346)	(22.059)	(23.211)
	Coefficient test	0.06	0.54	0.37

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. Effect of recreational cannabis laws on arrest rates, by race. Ethnicity is not available. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each pair of coefficients is based on separate two-way fixed effects regressions (see Equation 1). The RCL treatment indicator is interacted with an indicator for above the median (or below) of the 2010 Black share of the population at the county level. The p-value of a test of equality of coefficients is shown. All regressions include county and year fixed effects. Control variables include the number of reporting agencies and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S10: Effect of recreational cannabis laws on arrests per 10,000 persons, heterogeneity by racial dissimilarity index

		Cannabis arrests	Non-drug arrests	Total arrests
Population:	RCL × high dissimilarity index	-5.857***	24.812*	14.580
		(2.001)	(14.328)	(14.005)
	RCL × low dissimilarity index	-7.798**	20.714**	17.620
		(3.097)	(9.790)	(12.136)
	Coefficient test	0.29	0.78	0.86
White:	RCL × high dissimilarity index	-5.308***	17.796	8.775
		(1.600)	(14.757)	(14.350)
	RCL × low dissimilarity index	-8.375***	8.206	3.239
		(2.656)	(10.314)	(12.754)
	Coefficient test	0.04	0.52	0.76
Black:	RCL × high dissimilarity index	-17.526***	72.274*	44.123
		(4.277)	(41.128)	(43.927)
	RCL × low dissimilarity index	-21.604	43.269	56.875
		(13.671)	(31.772)	(38.059)
	Coefficient test	0.73	0.63	0.84
Other Race:	RCL × high dissimilarity index	-0.876*	13.335	9.374
		(0.476)	(19.478)	(20.290)
	RCL × low dissimilarity index	-2.204	3.272	0.406
		(1.385)	(22.336)	(23.641)
	Coefficient test	0.27	0.45	0.54

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. Effect of recreational cannabis laws on arrest rates, by race. Ethnicity is not available. Counts for a given race are divided by county-year population estimates corresponding to that race and multiplied by 10,000. Sample restricted to counties with an agency reporting coverage threshold above or equal to 65%. Each pair of coefficients is based on separate two-way fixed effects regressions (see Equation 1). The RCL treatment indicator is interacted with an indicator for above the median (or below) of the 2010 racial dissimilarity index at the county level. The p-value of a test of equality of coefficients is shown. All regressions include county and year fixed effects. Control variables include the number of reporting agencies and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

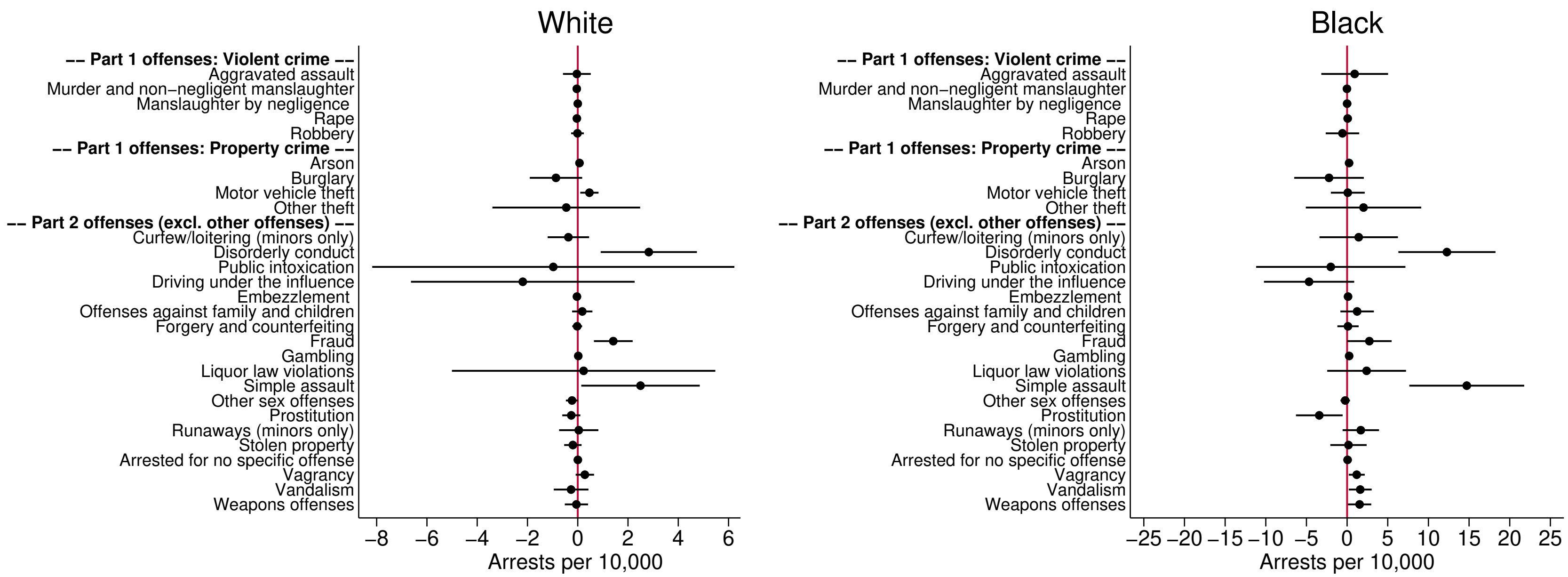
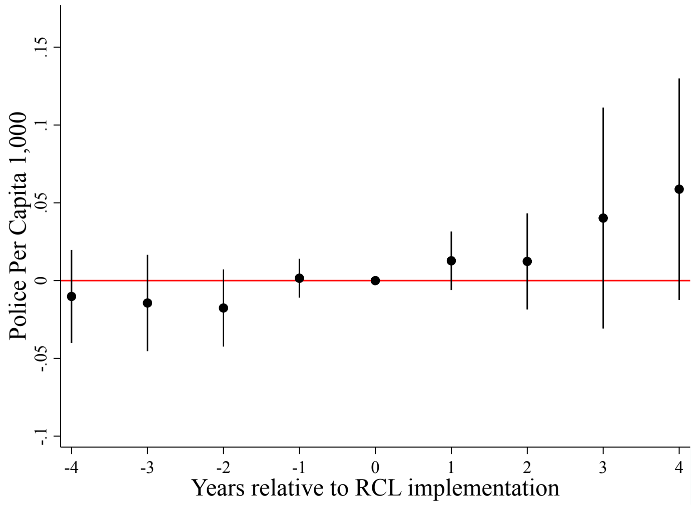


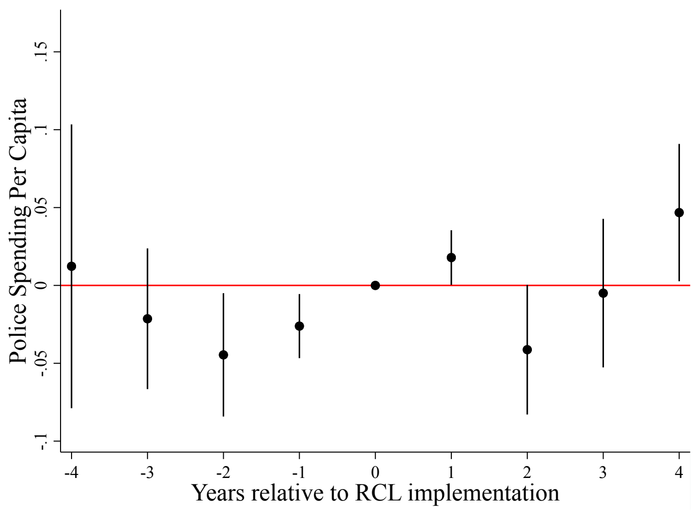
Figure S16: Arrest rates, by crime categories

Notes: Arrest data are from the 2007-2019 Uniform Crime Reports Arrests by Age, Sex, and Race. The unit of analysis is a county-year. Effect of recreational cannabis laws on arrest rates for all crime categories, by race groups (White and Black). Ethnicity is not available. 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 regression (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 and cannabis decriminalization laws. RCL=Recreational cannabis laws.

E Police Outcomes



(a) Sworn Officers Per 1,000 Person



(b) Police Spending Per Capita

Figure S17: Effect of recreational cannabis laws on police, by time since RCL implementation

Notes: Data are from the 2007-2019 Uniform Crime Reports Law Enforcement Officers Killed and Assaulted and the Annual Survey of Governments. The unit of analysis is a county-year. Counts for a given outcome are divided by county-year population and multiplied by 1,000 for the number of police and by 10 for spending. Regressions are weighted by total population. 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). Controls include county and year fixed effects, the number of reporting agencies, medical cannabis laws, and cannabis decriminalization laws. The reference year is t = 0, the year immediately before RCL implementation. RCL=Recreational cannabis laws.

Table S11: Effect of recreational cannabis laws on arrests, by demographic characteristics

	(1)	(2)	(3)	(4)	(5)
		Segregation		Black Share	
	Main	Below	Above	Below	Above
Officers	0.0327*	0.0264*	0.0313	0.0212	0.0333
	(0.0182)	(0.0132)	(0.0281)	(0.0141)	(0.0258)
Spending	0.000993	0.00244**	0.000563	0.00116	-0.00388
	(0.00198)	(0.00108)	(0.00322)	(0.00108)	(0.00350)

Notes: Effect of recreational cannabis laws on police by city demographics. Data are from the 2007-2019 Uniform Crime Reports Law Enforcement Officers Killed and Assaulted and the Annual Survey of Governments. The unit of analysis is a county-year. Regressions are weighted by total population. Each coefficient is based on separate two-way fixed effects regressions (see Equation 1). Column 2 and 3 stratify the treated sample by the level of segregation and columns 4 and 5 stratify the treatment group by share of the population black. All regressions include county and year fixed effects, the number of reporting agencies, medical cannabis laws, and cannabis decriminalization laws. Standard errors clustered by state are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.