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Do Recreational Marijuana Laws Reduce Racial Disparities? Evidence from Criminal Arrests, Psychological Health, and Mortality*

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I. ABSTRACT

Proponents of recreational marijuana laws (RMLs) argue that ending marijuana prohibition will reduce racial disparities in arrests and health. Using data from four national datasets (the Uniform Crime Reports, the National Survey on Drug Use and Health, the Behavioral Risk Factor Surveillance System, and the National Vital Statistics System Mortality Files) and a difference-in-differences approach, this study presents new evidence on the effects of RMLs on racial disparities in drug-related arrests, psychological health, and mortality. First, we find that RML adoption is associated with a 2.1 per 1,000-person greater reduction in marijuana-related arrests among Black as compared to White adults. However, this differential race-specific arrest rate reduction is entirely a reflection of pre-treatment racial differences in marijuana arrests. In percentage terms (relative to pre-treatment arrest rates), RMLs did little to narrow racial disparities in arrests. Second, RML adoption — particularly when accompanied by open and legal recreational marijuana dispensaries — is associated with an increase in violent crime arrests among Blacks relative to Whites. This result could suggest a racially targeted reallocation of policing resources to detect violent crime. Finally, while RMLs do not appear to have a racially disparate impact on adult psychological health, we find stronger evidence that RMLs reduced drug-involved suicides and opioid-related mortality among non-Hispanic Whites relative to racial/ethnic minorities. We conclude that RMLs largely failed to reduce health- and crime-related disparities between Whites and racial/ethnic minorities.

I. MOTIVATION

> *"[W]hile white and Black and brown people use marijuana at similar rates, Black and brown people have been arrested, prosecuted, and convicted at disproportionate rates."*
>
> *-- U.S. President Joseph R. Biden, October 6, 2022*[1]

The causes of racial disparities in arrests are hotly debated among U.S. policymakers (Lloyd 2020; Santhanam 2020). While race-specific differences in human capital acquisition and labor market opportunities may play a role (Gould et al. 2002; Lochner and Moretti 2004), racial bias in prosecutorial decisions (Arnold et al. 2018) and policing practices (Goncalves and Mello 2021) have taken center stage in policy debates. Among the most high-profile policing reforms include increased monitoring of interactions between suspects and police (Ariel et al. 2015; Henstock and Ariel 2017; Lum et al. 2019; Braga et al. 2020, 2022; Demir et al. 2020; Zamoff et al. 2022, Ferrazares 2023), greater racial diversity in police hiring (Ba et al. 2021), and increased investments in diversity training (State of New Jersey 2020). In addition, criminal justice reformers have pursued the decriminalization (or depenalization) of non-violent criminal offenses (Woods 2021; Brown 2022) with historically large racial disparities in arrests (ACLU 2013; 2020). One of the most prominent such reforms has been the legalization of recreational marijuana.

Between January 2012 and December 2022, 21 states and the District of Columbia (D.C.) adopted recreational marijuana laws (RMLs).[2] RMLs legalize possession, cultivation, and consumption of a limited amount of marijuana (e.g., one or two ounces) by anyone 21 years of age or older for any reason, including recreational purposes. Purchases of marijuana can be made at recreational dispensaries simply

[1] See 'Statement from President Biden on Marijuana Reform" at: https://www.whitehouse.gov/briefing-room/statements-releases/2022/10/06/statement-from-president-biden-on-marijuana-reform/

[2] Maryland (included in the count of RML states above) passed an RML via voter referendum in November 2022 with the law to become effective July 1, 2023.

by showing proof of age. Unlike most medical marijuana laws (MMLs) — which have been adopted by 37 states and D.C.[3] — RMLs do not require registration as part of a state registry nor do they require a doctor's recommendation to treat an "allowable medical condition."[4] Nearly all states that have legalized recreational marijuana permit home cultivation, that is, marijuana to be grown at home.[5]

Proponents of RMLs point out that ending the prohibition on recreational marijuana can generate substantial social cost savings, as the annual costs associated with enforcing prohibition — including policing, court, and prison costs (Beckett and Herbert 2009; French et al. 2022) — total $3.6 billion annually (ACLU 2013). Moreover, because the majority of marijuana-related arrests are for non-violent offenses (ACLU n.d.) — such as street-level drug dealing and transport of small quantities of drugs for transactions (i.e., couriers) (Fellner and Vinck 2008) — the crime-related social benefits of marijuana prohibition are likely small, especially given scant evidence of gateway effects to harder drug use (Sabia et al. 2021). Less than a third of such arrests are related to higher-level offenses such as the distribution of large quantities of drugs to dealers (King and Mauer 2006).

In addition to efficiency gains, proponents of marijuana legalization argue that RML adoption can also serve important racial equity-related ends given substantial racial disparities in enforcement of marijuana prohibition.[6] Despite comparable marijuana usage rates, Blacks are 3.6 times more likely than Whites to be arrested for marijuana-related offenses (ACLU 2020). Blacks are also substantially more likely than Whites to enter prison due to a marijuana-related conviction (BJS 2022). In addition, Hispanics are overrepresented — both relative to their population share and their relative prevalence of marijuana usage — in marijuana-related arrests (California NORML 2020).[7]

The disparate impact of marijuana prohibition enforcement on racial/ethnic minorities may be an important contributor to longer-run economic inequality.[8] There is evidence that having a criminal record generates substantial labor market penalties (Pager 2003; Agan and Starr 2018; Doleac and Hansen 2020; Dobbie et al. 2018; Mueller-Smith and Schnepel 2021), which could exacerbate racial/ethnic disparities in employment and earnings, and the risk of recidivism. Thus, the adoption of RMLs could generate important economic benefits, particularly for those who have been disproportionately harmed by prohibition. Underlining this point, in October 2022, President Joseph R. Biden announced a pardon for those who had been convicted of a federal offense of simple marijuana possession. The White House stated that there:

> *"are thousands of people who have prior Federal convictions for marijuana possession, who may be denied employment, housing, or educational opportunities as a result" (The White House 2022).*[9]

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[3] The vast majority of states that do not have RMLs or MMLs allow some legal access to medicinal cannabinoid oils.

[4] Such conditions may include chronic pain, nausea, seizures, epilepsy, and cancer (NCSL 2022b).

[5] As of December 2022, Illinois, New Jersey, and Washington do not allow for home cultivation under their RMLs (although legislation to allow for home cultivation is pending in all three states).

[6] The history of marijuana prohibition clearly has had racial overtones since its onset. Harry Anslinger, who became the first commissioner of the Federal Bureau of Narcotics in 1930, was quoted as saying: "Most [marijuana smokers in the U.S.] are Negroes, Hispanics, Filipinos, and entertainers. Their Satanic music, jazz and swing, results from marijuana use. This marijuana causes white women to seek sexual relations with Negroes, entertainers, and others." (Solomon 2020)

[7] For instance, in 2019, Hispanic individuals comprised 42 percent of all of California's felony marijuana arrests (California NORML 2020).

[8] Such disparate impacts in criminal records may be not only through racial bias in policing, but also through the court system, including prosecutorial decisions, judges, and juries (Kovera 2019).

[9] The President also urged Governors to follow his lead and pardon those with prior criminal records for violations of state law regarding simple marijuana possession. The Governors in Oregon and Connecticut have followed suit, who plan to expunge marijuana-related convictions for their residents (see: https://norml.org/blog/2022/11/21/oregon-governor-announces-mass-pardons-for-those-with-low-level-cannabis-convictions/; https://norml.org/blog/2022/12/06/connecticut-governor-announces-intent-to-expunge-40000-marijuana-related-convictions/).

This study provides new estimates of the impact of RML adoption on racial disparities in arrests, psychiatric well-being, and mortality. We document four key findings. First, using data from the 2000-2019 Uniform Crime Reports (UCR) and a two-way fixed effects (TWFE) approach, we document that RML adoption is associated with a 122.1 per 100,000 population decline in adult marijuana arrests. This average marginal effect masks important race-specific heterogeneity: arrests of Black adults fall by 325.2 per 100,000 population, whereas arrests of Whites fall by 115.2 per 100,000 population. The Black-White arrest decline differential (2.1 per 1,000-persons) is both statistically significant and economically meaningful and suggests that Black adults gained in absolute arrest declines relative to their White adult counterparts.

However, we also find that this absolute gain was entirely explained by pre-treatment race differentials in marijuana-related arrests. After adjusting for pre-treatment arrest differentials, we find that the percentage decline in marijuana arrests in response to RMLs was comparable (statistically and economically) for Whites and Blacks. To the extent that there were similar race-specific marijuana consumption (and possession) responses to RMLs among young adults (Martins et al. 2021), who are responsible for a large share of arrests, these findings are consistent with the hypothesis of "race blind" repeal of marijuana prohibition. However, the findings also show that RMLs did little to reduce the relative gap (in percentage terms) in marijuana-related arrests between Whites and Blacks. A causal interpretation of our findings is supported by the robustness of results to the inclusion of controls for spatial heterogeneity as well as event study analyses based on the Callaway and Sant'Anna (2021) estimator, which is designed to expunge bias in TWFE estimates caused by heterogeneous and dynamic RML effects.

Secondly, we find little evidence that RML adoption increased "harder" drug-related arrests among Whites or Blacks. This finding is consistent with evidence from Sabia et al. (2021). Moreover, we find that RMLs are associated with reductions in narcotics-involved arrests among Whites and Blacks, consistent with the (demand-side) hypothesis that opioids and marijuana are substitutes. We find that neither non-narcotic drug arrests nor property crime arrests were significantly affected by RML adoption, either among White or Black arrestees. For violent crime arrests, however, there is evidence that RML adoption — particularly when accompanied by legal open recreational dispensaries — is associated with an increase in violent crime arrests among Blacks. This finding could suggest that the reallocation of policing resources (away from enforcing marijuana prohibition) to fight violent crime were carried out in a racially disparate manner.

Thirdly, given evidence that psychological health problems may be linked to future crime (Cuellar et al. 2006; Anderson et al. 2015), we explore disparities in the effects of RML adoption on mental health. Analyses of the National Survey on Drug Use and Health (NSDUH) and Behavioral Risk Factor Surveillance System (BRFSS) suggest that RML adoption was associated with adverse mental health effects among all adults, including a 0.9 percentage-point increase in the prevalence of psychiatric illness and a 0.4 percentage-point increase in the prevalence of suicide ideation. Our analysis of race/ethnicity-specific data in the BRFSS suggest adverse psychiatric effects of RMLs for whites under age 21, but little consistent evidence of racial disparities in the adult mental health effects of RMLs.

Finally, turning to mortality, we find that RML adoption accompanied by open, legal recreational dispensaries is associated with a decline in drug-involved suicides and opioid-related mortality among non-Hispanic Whites. Specifically, we find that RML adoption is associated with a decline in the opioid-related mortality among non-Hispanic Whites of 5.8 deaths per 100,000 adults. For Blacks and Hispanics, the declines in mortality are not as pronounced and appear to pre-date RML enactment. Together, the findings of our study provide little support for the hypothesis that ending the prohibition on recreational marijuana narrowed pre-treatment racial/ethnic disparities in arrests, psychological health, or mortality.

II. BACKGROUND

A. Motivation

The social costs of marijuana prohibition — including, but not limited to, police and court costs (ACLU 2013), labor market costs to arrestees from having a criminal record (Pager 2003), public health costs from diminished access to a less dangerous substitute for opioids (Bachhuber et al. 2014; Powell et al. 2018) and tobacco (Choi et al. 2019; Dave et al. 2022), and lost utility from a recreational "high" (Sabia et al. 2021) — are substantial relative to the social benefits (Dills et al. 2021; Anderson, Rees, and Sabia 2015). Moreover, marijuana arrestees are disproportionately racial and ethnic minorities (ACLU 2020; NORML 2023),[10] which could suggest the presence of racial bias in law enforcement (i.e., from police, prosecutors, defense attorneys, judges, or juries). Indeed, former U.S. Surgeon General Joycelyn Elders argued that "the unjust prohibition of marijuana has done more damage to public health than has marijuana itself" (Nathan et al. 2017).[11]

While a handful of local jurisdictions — for example, Baltimore, Maryland (Battaglia 2021) and King County, Washington (Jouvenal 2019) — have experimented with prosecutorial reforms that would cease charging individuals for possession of "harder" drugs (i.e., cocaine, methamphetamine, heroin), legalization of recreational marijuana has emerged as the most popular statewide drug reform. Gallup polling shows that 68 percent of Americans support legalizing marijuana (Gallup 2021).

B. The Path to Recreational Marijuana Legalization

The legal status of marijuana in the American colonies and the United States has changed often during its history (Anderson, Hansen, and Rees 2013; Anderson and Rees, Forthcoming). Marijuana was introduced in the early 1600s by Jamestown settlers who used marijuana plants in hemp production. Hemp cultivation remained a prominent industry until the mid-1850s (Deitch 2003) and marijuana was commonly recommended by physicians and pharmacists to treat a variety of health ailments (Pacula et al. 2002). In 1913, California passed the first marijuana prohibition law aimed at curbing recreational use (Gieringer 1999) and by 1936, all U.S. states had banned recreational use of marijuana (Eddy 2010). The passage of the federal Marihuana Tax Act in 1937 effectively ended the use of marijuana for medicinal purposes (Bilz 1992).

As part of the "War on Drugs," launched during the Nixon Administration, marijuana was classified as a Schedule I drug in the 1970 Controlled Substances Act. This classification reflected the government's view that marijuana has a "high potential for abuse," "no currently accepted medical use," and a "lack of accepted safety for use under medical supervision." The Controlled Substances Act guides federal criminal law on marijuana. Under federal criminal law, possession and sale of marijuana is illegal, as is transport of marijuana across state lines.

Beginning in the 1970s and continuing through the next several decades, states began to experiment with state marijuana law reforms. Marijuana decriminalization laws remove criminal status of marijuana possession for small quantities (~1 oz) of marijuana under state law. This type of reform reclassifies marijuana-related criminal offenses as civil or local infractions, which is the lowest misdemeanor. Along similar lines, some states began to adopt marijuana depenalization laws, which retains the criminal status

[10] See also Camplain et al. 2020; Mitchell and Caudy 2015; Kakade et al. 2012; Donohue and Levitt 2001.

[11] In addition, Fellner and Vinck (2008) wrote in their publication *Targeting Blacks*:

"Ostensibly color-blind, the US drug war has been and continues to be waged overwhelmingly against black Americans. Although white Americans constitute the large majority of drug offenders, African American communities continue as the principal "fronts" in this unjust effort."

of marijuana possession, but reduces the severity of criminal penalties. A typical depenalization law reduces or removes jail time as a criminal sanction for marijuana possession. As of December 2022, 27 states and the District of Columbia had a marijuana decriminalization or depenalization law in effect (NCSL 2022a).

In 1996, California adopted the Compassionate Use Act, the first state medical marijuana law (MML). Medical marijuana laws legalize the possession, sale, and cultivation of marijuana for allowable medical conditions (e.g., pain, anxiety, nausea, glaucoma, and movement disorders). The typical supply channels through which one can obtain medicinal marijuana are (i) medical marijuana dispensaries and (ii) home cultivation of marijuana (growing marijuana plants). As of December 2022, 37 states and the District of Columbia had adopted an MML (ProCon.org 2022). The remaining states allow some limited access to CBD oils for medical purposes.

There is substantial heterogeneity in state statutes surrounding MMLs. The laxest state MMLs require patient registration and a doctor's recommendation to obtain medical marijuana. The strictest state MMLs further require verifiable medical conditions and distribute medicinal marijuana only through state-licensed dispensaries. There is also evidence that MMLs generated spillovers to the recreational market by lowering the street price (illicit market) of high-quality marijuana (Anderson et al. 2013).

In 2012, Colorado and Washington became the first states to adopt RMLs. As of December 2022, 21 states and D.C. have adopted such a statute. RMLs legalize the possession, sale, and consumption of small quantities of marijuana (typically less than 1 or 1.5 ounces) for recreational purposes for adults ages 21-and-older. All states that have adopted an RML had previously adopted an MML. Over the sample period analyzed in this study (2000-2019), the average length of time between MML-adoption and RML-adoption is approximately 12 years. As with MMLs, the typical supply channels through which RMLs deliver marijuana to consumers are (i) recreational marijuana dispensaries, which legal recreational sales typically follow RML adoption with a 0-to-2-year lag,[12] and (ii) homegrown marijuana plants (home cultivation), which is permitted by all but three RML-adopting states (Illinois, New Jersey, and Washington).

In addition, all states that have legalized recreational marijuana tax its sale (Marijuana Policy Project 2021). States follow a variety of marijuana taxation approaches; some states impose a weight-based excise tax while others use a price-based ad valorem tax. Some states also apply different tax rates for different marijuana products (or differing forms of marijuana), including marijuana products that differ by their THC content. There is some concern that high levels of marijuana taxation may have expanded the illicit market for marijuana (Sykes 2023).

C. Marijuana Legalization and Marijuana Consumption

Several recent studies have found that RML adoption is associated with an increase marijuana consumption among adults ages 18-and-older. Using data from the National Survey on Drug Use and Health (NSDUH) and difference-in-differences approaches (both TWFE and Callaway and Sant'Anna estimates), Sabia et al. (2021) and Dave et al. (2022) find that RMLs are associated with a 2-to-4 percentage-point increase in adult marijuana use. Note that this local average treatment effect (LATE) represents the effect of an RML above and beyond an MML that had been previously enacted, which were on the order of about 1-to-2-percentage-points.[13] Hollingsworth et al. (2022) also find evidence of large

[12] A handful of states outside our sample window (2000-2019), including Maine and Vermont, each had a lag of approximately four years between RML adoption and permitting recreational dispensaries to operate in the state.

[13] The spillover effects of RMLs on minors under age 19 is less clear. Using data from the Youth Risk Behavior Surveys (YRBS), Anderson et al. (2019; 2021) find that RML adoption does not lead to increases in marijuana consumption among U.S. high school students. Moreover, they also find evidence of a (lagged) decline in youth marijuana use, which could suggest that illicit markets for underage purchasers shrink following RML adoption.

increases in adult marijuana use following RML adoption.

Moreover, Dave et al. (2022) explore policy heterogeneity in two ways. First, they explore whether RMLs accompanied by the opening of recreational marijuana dispensaries had differential effects on consumption. Their findings provide some support for the hypothesis that dispensaries are an important supply channel through which marijuana consumption occurs following the passage of RMLs. However, there remains a boost in use even in state-years when dispensaries are not (yet) open, suggesting that (i) home cultivation of marijuana may be an important supply channel, and/or (ii) spillovers to the illicit market (via reductions in the street price of marijuana) may play a role in increases in marijuana consumption.

Second, the authors more flexibility explore heterogeneity in the effects of RMLs on marijuana consumption through a state-by-state synthetic control analysis, focusing on early adopting states for which there will be the longest post-treatment period. The results provide support for the hypothesis that RML enactment boosted consumption in nearly all early-adopting states.

Consistent with the findings of Sabia et al. (2021), Dave et al. (2022) use individual-level longitudinal data from the Population Assessment of Tobacco and Health (PATH) to study the effect of RML adoption on marijuana use. They find that RML adoption is associated with an increase in marijuana use among those ages 18-and-older. Disaggregating RML effects for those under and over the marijuana minimum legal purchase age (age 21), the authors find consumption effects for both those ages 18-to-20 and ages 21-and-older. In addition, Dave et al. (2022) study (i) the margins of marijuana use affected by RMLs and (ii) dynamics in consumption effects. Using discrete-time hazard models, they find that RML adoption increases the likelihood that marijuana abstainers initiate marijuana use as well as decrease the likelihood that marijuana users quit. In addition, the authors also find some evidence that RMLs increase the intensity of marijuana consumption (i.e., days of consumption) among users.[14]

Only one study of which we are aware has explored race-specific differences in the relationship between RML enactment and marijuana use. Using data from the NSDUH and a difference-in-differences approach, Martins et al. (2021) find that RML enactment is associated with an increase in the odds of prior-year marijuana use for non-Hispanic Whites and Hispanics. This effect appears strongest for adults ages 21-and-older. While they find no statistically significant association between RML adoption and marijuana consumption among Blacks, the effect size for those ages 21-to-30 (odds ratio = 1.31) is comparable to that observed for non-Hispanic Whites (odds ratio = 1.28) but is simply less precisely estimated.[15] Together, a reasonable interpretation of these findings suggest that at least for young adults with the highest propensities for drug arrests (those under age 30), RMLs increase marijuana use for non-Hispanic Whites, Blacks, and Hispanics by comparable magnitudes.

D. Marijuana Legalization and Arrests

Sabia et al. (2021) study the impact of RMLs on drug-related arrests as well as for arrest for part I offenses (property and violent crime). Using data from the Uniform Crime Reports and difference-in-differences approaches, the authors find that RML adoption was associated with a 1.3 per 1,000-adult persons reduction in marijuana possession arrests and a 1.6 per 1,000-adult persons reduction in marijuana sales arrests. Turning to arrests for possession and sales of harder drugs (heroin/cocaine, truly addictive synthetic narcotics, other dangerous non-narcotics), Sabia et al. (2021) find no consistent evidence of changes in heroin/cocaine arrests, but they do find that RML adoption is associated with a reduction in

[14] On the other hand, findings from Martins et al. (2021) from the NSDUH finds little evidence that RML adoption is significantly associated with the probability of daily marijuana use among marijuana users. However, this analysis was based on repeated cross-sectional data, which raises the possibility of sample selection bias if RMLs change the composition of marijuana users over time.

[15] The 95% confidence interval around the estimated odds ratio for non-Hispanic blacks ages 21-to-30 is (0.96, 1.80) while for non-Hispanic Whites is (1.13, 1.45).

narcotic-related arrests, consistent with the hypothesis that marijuana and opioids are substitutes (Bachhuber et al. 2014; Bradford and Bradford 2018; Powell et al. 2018; Chu 2015; Sabia et al. 2021).

The marijuana arrest results obtained by Sabia et al. (2021) differs sharply from those of Chu (2015), who studied the effect of MMLs on illicit marijuana use. Chu (2015) finds that MML adoption increased marijuana-related arrests by approximately 10 to 15 percent, which he interprets as evidence that MML adoption likely had important spillover effects to the illicit recreational market.[16] RMLs would be expected to have a decidedly more negative effect on marijuana-related arrests as long as they do not (i) induce marijuana possession above RML-prescribed thresholds, (ii) induce large spillovers (perhaps due to marijuana taxation, which often accompanies RMLs) to the black market that increase illicit sales, or (iii) change policing strategies such that more resources are targeted to detecting illicit marijuana activity and arresting its perpetrators. Finally, Chu (2015) finds evidence that MML adoption is negatively related to heroin-related arrests, consistent with the hypothesis that opioids and marijuana are substitutes.

Two studies in the medical literature have explored the impact of cannabis decriminalization laws (i.e., those that reduce criminal penalties or remove criminal penalties and replacing with more modest civil penalties) on racial disparities in marijuana possession arrests (Gunadi and Shi 2022; Sheehan et al. 2021). Each study finds that decriminalization laws are associated with a reduction in racial disparities in marijuana possession arrests. These studies raise the possibility that RML adoption could have important impacts on racial disparities in marijuana arrests.[17] Neither study, however, explored the effects of policies liberalizing access to marijuana on non-marijuana drug-related arrests.

In addition to drug offenses, Sabia et al. (2021) also explore spillovers to arrests for Part I offenses. Increased marijuana use (or spillovers that increase harder drug use, which has not, as yet, been detected) could lead to (i) addiction, for which property crime may result for income-generating purposes, and/or (ii) drug-related psychological effects that reduce inhibitions to violence (through temporary increases in discount rates, i.e., "Saturday night fever") — both of which could be potentially important externalities associated with recreational marijuana legalization. On the other hand, as Anderson and Rees (Forthcoming) note:

> *"Legalizing marijuana could shrink the black market and reduce its attendant violence or free up police resources, allowing law enforcement officials to reallocate their efforts toward reducing non-drug crime (Miron and Zwiebel 1995; Adda et al. 2014). Also, if legalization lowers the price of marijuana-- and demand is sufficiently inelastic-- then crimes committed to finance marijuana consumption could fall. On the other hand, increased marijuana use could lead to more violent behavior directly through a psychopharmacological effect or indirectly through a "gateway" effect."*

Two studies of which we are aware have studied the impact of RMLs on non-drug crime.[18] First, Dragone et al. (2019) find that Washington's RML led to a significant reduction in arrests for rapes and property

[16] This interpretation is confirmed by additional analysis in the Treatment Episode Dataset, where Chu (2015) finds that RML adoption is associated with an increase in marijuana-related drug treatment admissions.

[17] Sheehan et al. (2021) attempted to study the effects of marijuana legalization on marijuana-related arrests, but (in a specification that did not include controls for the quantity or quality of arrest reporting or other substance use policies), concluded that RML adoption was associated with "anticipatory effects" (leads) on arrests, something not detected by Sabia et al. (2021) in their study of the effect of RML adoption on arrests. This may be due to (1) the choice of controls employed in the event study generated with two-way fixed effects models, and (2) the use of alternate Callaway and Sant'Anna (2021) estimates by Sabia et al. (2021).

[18] Several prior studies had explored the impact of MML adoption on non-drug crime. The majority of studies found that MMLs had no effect on property or violent offense arrests (Morris et al. 2014; Huber III et al. 2016; Chu and Townsend 2019). However, Gavrilova et al. (2019) found that states bordering Mexico that adopted an MML experienced a 13 percent reduction in violent crime. The authors attribute this result to increased competition in the international marijuana market, which reduced "black market" power of Mexican drug trafficking cartels (Miron and Zwiebel 1995; Gavrilova et al. 2019).

crime during the period 2013 to 2014. In contrast, Sabia et al. (2021) find little evidence that RML enactment affects arrests for property crime offenses (larceny, motor vehicle theft, burglary, arson) or violent crime offenses (assault, robbery, and murder). Estimated treatment effects were economically small and not statistically distinguishable from zero at conventional levels. Neither study explored heterogeneity in effects of RMLs on non-drug-related arrests by race.

E. Marijuana Legalization and Psychological Health

While public health researchers have long documented that marijuana use and psychological problems are positively correlated (Urits et al. 2020), this association may be explained by many non-causal channels, including (i) those in poorer mental health seeking out marijuana (i.e., for medical reasons to treat anxiety or for self-medication) and (ii) difficult to measure personal or family characteristics associated with both poorer psychological health and marijuana use. The most convincing causal evidence on the relationship between marijuana access and psychological health come from studies of MMLs and, more recently, RMLs.

Liberalizing access to medical marijuana has been found to improve psychiatric health of adults (Sabia et al. 2017) and reduce suicides (Anderson et al. 2014; Bartos et al. 2020; Kalbfuß et al. 2018). Among the mechanisms hypothesized to explain this relationship include marijuana-induced improvements in anxiety and psychological health and greater access to marijuana causing substitution away from alcohol (DiNardo and Lemieux 2001; Crost and Guerrero 2012; Anderson et al. 2013; Kelly and Rasul 2014; Sabia et al. 2017; Baggio et al. 2020; Miller and Seo 2021) and opioids (Bachhuber et al. 2014; Bradford and Bradford 2018; Powell et al. 2018), each of which is linked to suicide behaviors and mortality. On the other hand, Grucza et al. (2015) suggest that evidence for MML-induced improvements in psychological health may be explained, at least in part, by omitted state-level covariates.

There is much less evidence on the effects of RML adoption on psychological health or suicides. One of the few studies that does, Sabia et al. (2021), finds no evidence that RMLs are significantly or economically related to adult suicide rates whether they are accompanied by recreational dispensary openings. However, these authors do not explore heterogeneity in suicide effects by race or ethnicity. Moreover, as Anderson and Rees (Forthcoming) write:

> *"the next step in this literature is to estimate the effect of RML adoption on measures of mental health aside from suicides."*

F. Contributions

This study makes several important contributions to the existing literature. First, this study provides new evidence on the effects of RMLs on racial disparities in marijuana arrests. In addition, this study is also the first to explore the effects of RMLs on racial disparities of non-marijuana-related drug arrests and non-drug arrests for Part I offenses. The findings from such analyses (1) help to inform whether policing resources may have been reallocated to detecting other non-violent crimes, which could have alleviated or exacerbated racial disparities, and (2) uncover whether there were any important race-specific differential spillovers on arrests for violent offenses. To our knowledge, no nationally representative study has examined the impact of RMLs on racial disparities in property or violent crime arrests.

Third, this study is the first to estimate the impact of RMLs on psychiatric health, including major depressive episodes, poor mental health days, suicide ideation, and completed suicides. In addition to being important from a health perspective, the link between poor psychological health and crime (Frank and McGuire 2010; Deza et al. 2022; Anderson, Cesur, and Tekin 2015) suggests that understanding the

psychiatric effects of recreational marijuana legalization — either through direct mental health effects of marijuana use or indirect effects through changes in consumption of complements/substitutes such as alcohol or harder drugs — may have important effects on crime.

Finally, this study examines whether potential benefits of RMLs in curbing opioid-related mortality (Sabia et al. 2021) are race-specific; that is, whether they are concentrated among non-Hispanic white males, a population at the center of "deaths of despair" in the U.S. opioid epidemic (Case and Deaton 2015).

III. DATA

Our analysis of the effects of RMLs on racial disparities in arrests, psychiatric well-being, and mortality makes use of four nationally representative datasets, described below.

A. Criminal Arrests

To explore the impact of recreational marijuana laws on arrests, we draw data from the 2000-2019 Uniform Crime Reports (UCR), compiled by the U.S. Federal Bureau of Investigation (FBI) (Kaplan 2021). The UCR data measure arrests in all 50 states and the District of Columbia, covering 98 percent of the U.S. population. Arrest data are collected via voluntary reports from more than 16,000 city, county, and state agencies. While these data may understate the true levels of crime because not every crime is reported to law enforcement (Gould et al. 2002), there is a high degree of correlation between arrest reports from the UCR and actual crimes committed when the latter are measurable (Lochner and Moretti 2004). Additionally, given the research question under study, we are interested in how RMLs may affect race-specific policing responses. Thus, the reduced form effect of RMLs on arrests will capture the joint mechanisms of changes in marijuana consumption/possession (and any spillovers to other related substances) as well as changes in policing practices.

We compile state-by-year arrest counts for adults ages 18-and-older by race using the two race categories provided in the UCR: White and Black.[19] We then calculate state-by-year arrest rates per 100,000 race-specific adult state population, using data available from the National Cancer Institute's Survey of Epidemiology and End Results (SEER).

A number of scholars have raised concerns about UCR data quality, particularly due to changes in frequency of reporting by law enforcement agencies (LEAs) or poor reporting quality by agencies serving smaller communities (Chu 2015; Chu and Townsend 2019). This issue could conflate changes in actual arrests with changes in reporting. We undertake a number of strategies to ensure that our findings are robust to data quality checks, including: (1) controlling for the number of agencies reporting arrests in each state-year and (2) exploring the robustness of findings to a city-level (i.e., LEA-level) analysis that limits the sample to agencies that report arrests in at least six months of every year (or in December only) in the UCR and that serve communities of at least 50,000 individuals, following Chu (2015).[20]

We examine several drug arrest-related outcomes. Our primary variable of interest is marijuana-related arrests, which we also disaggregate into arrests for marijuana possession and sales. Over the full sample period (2000-2019), we find that the overall marijuana arrest rate for adults (ages 18-and-older) is 230.7 per 100,000 individuals. When we examine arrests for possession and sales, the mean arrest rates are

[19] The UCR does not provide consistent reporting of arrests by ethnicity of arrestees (i.e., Hispanic origin) nor are we able to disaggregate adult arrestees by age and race (other than juvenile and adult).

[20] In our main state-year panel, Wisconsin and Washington D.C. are dropped in 2000 as they have no agencies reporting arrests. Florida is dropped from the panel entirely, due to having no agencies reporting arrests over the 2000-2016 period and having minimal reporting over 2017-2019.

204.3 and 26.4, respectively. Finally, when we disaggregate by race, we find the mean arrest rate for White adults is 182.1 per 100,000 individuals for marijuana arrests, 163.0 for marijuana possession, and 19.1 for marijuana sales. For Black adults, the corresponding arrest rates are over three times larger, 623.1 for all marijuana arrests (541.0 for possession and 82.0 for sales).

In addition, we explore spillovers to other drugs, which could be impacted by both demand-side factors, such as whether these "harder" drugs are complements or substitutes for marijuana, or supply-side factors, such as how suppliers/producers of these drugs respond to a change in the availability (and price) of marijuana caused by RML adoption. These drugs are categorized in the UCR as (1) powder cocaine, crack cocaine, heroin, and other opium derivatives, (2) truly addicting synthetic narcotics, and (3) other dangerous non-narcotic drugs. As with marijuana, we explore heterogeneity in RML effects across both the possession and sales margins. The means of these outcomes, overall and by race, appear in Appendix Table 1.

We also examine the effects of RMLs on race-specific arrests for Part I offenses. These include property crime offenses (larceny, burglary, and motor vehicle theft) and violent crime offenses (homicide, rape, robbery, and aggravated assault).[21] For White adults, the property and violent crime arrest rates are 382.0 and 135.3, respectively. For Blacks, these rates are 1,032.7 and 531.8, respectively.

Finally, we explore arrests for other minor offenses, delinquency-related offenses, including vandalism, liquor law violations, drunkenness, driving under the influence (DUI), and disorderly conduct. The means of these outcomes are shown in Appendix Table 1, with mean arrest rates 1.2 to 3.1 times higher for Black adults than White adults. Arrests for DUI is the only offense category for which the arrest rate is higher for Whites relative to Blacks.

B. Psychiatric Health

We measure psychological health among adults ages 18-and-older using data from two national surveys: (1) the 2008-2019 National Survey of Drug Use and Health (NSDUH), provided by the Substance Abuse and Mental Health Services Administration (SAMSHA), and (2) the 2000-2019 Behavioral Risk Factor Surveillance System (BRFSS), made available by the Centers for Disease Control and Prevention. The NSDUH is a household survey representative of the U.S. non-institutionalized population. Information on adult respondents' health is collected via an individual audio computer-assisted self-administered interview to increase privacy and the likelihood of a truthful response.

The publicly available NSDUH data is provided for all adults ages 18-and-older (all adults), as well as disaggregated by younger adults ages 18-to-25 (younger adults) and older adults ages 26-and-older (older adults). These data are provided as two-year averages of state-level prevalence rates. We measure three main outcomes for our NSDUH-based analysis. *Any Mental Illness (AMI)* is defined as having a diagnosable mental, behavioral, or emotional disorder, other than a developmental or substance use disorder, as assessed by the Mental Health Surveillance Study (MHSS). *Serious Mental Illness* (SMI) is defined as one or more diagnosable mental, behavioral, or emotional disorders, other than a developmental or substance use disorder, resulting in serious functional impairment, which substantially interferes with or limits one or more major life activities.

To measure suicidal ideation, respondents to the NSDUH were asked,

> *"At any time in the past 12 months, did you seriously think about trying to kill yourself?"*

[21] Arson is included as part of overall property crime arrests, but we do not examine it separately due to reporting issues with arson arrests.

If respondents answered in the affirmative, they were coded as having serious thoughts of suicide in the past year. Mean prevalence rates available in the 2008-2019 NSDUH are shown in Appendix Table 1. For adults ages 18-and-older, the mean rate of any episodes of mental problems was 19.3 percent, for serious mental illness was 4.5 percent, and for serious suicidal thoughts was 4.3 percent. The rates of each of these outcomes was higher for 18-to-25-year-olds as compared to those ages 26-and-older.

An important limitation of the NSDUH data is that we cannot explore race- or ethnicity-specific differences in mental health effects of RMLs in the publicly available data. For this task, we turn to the BRFSS. We focus on the period 2000-2019 and our analysis sample consists of 7.4 million adults (those ages 18 and older) who were sampled over this period. The BRFSS is a telephone survey, which until 2011 was conducted exclusively with landlines. However, for the period 2011-2019, the survey began sampling individuals using cellular phones. When weighted, the sample represents non-institutionalized adults ages 18-and-older in the U.S.

An important limitation of the BRFSS is the paucity of data on mental health outcomes. These data allow us to measure the number of poor mental health days. Respondents are asked:

> *"Now thinking about your mental health, which includes stress, depression, and problems with emotions, for how many days during the past 30 days was your mental health not good?"*

From this measure, we construct three measures of mental health: *Any Poor Mental Health Days*, an indicator set equal to 1 if a respondent reports a positive number of days of poor mental health in the last month, and 0 otherwise; *Poor Mental Health ≥ 15 Days,* an indicator set equal to 1 if a respondent reports that at least half of her/his days in the past month were spent in poor mental health, and 0 otherwise; and *Days of Poor Mental Health*, the number of poor mental health days in the last month. We are able to measure these outcomes by race and ethnicity for adults surveyed in the BRFSS. Specifically, we examine heterogeneity in the psychiatric effects of RMLs by whether the respondent reports being a non-Hispanic White, Black, or Hispanic.

As shown in Appendix Table 1, 30.8 percent of non-Hispanic Whites ages 18-and-older report a positive number of poor mental health days in the past month, and among those who had, the average number of poor mental health days was 3.3. For Blacks, these numbers were 33.1 percent and 3.9 days, respectively, while for Hispanics these numbers were 33.6 percent and 3.8 days, respectively.

C. Suicide and Drug-Involved Mortality

Our fourth and final dataset is collected from the National Center for Health Statistics (NCHS) National Vital Statistics System (NVSS) multiple cause-of-death mortality files from 2000 through 2019. We collect state-by-year-by-age-by-race/ethnicity-specific mortality counts using CDC Wonder. First, we measure age-and-race-specific suicide rates. As shown in Appendix Table 1, the overall adult suicide rate is 12.8 per 100,000 individuals. Disaggregated by race and ethnicity, we find the highest suicide rate among non-Hispanic Whites (20.139 per 100,000 persons), followed by Hispanics (8.7 per 100,000 persons), and then Blacks (10.066 per 100,000 persons). Suicide rates are higher for younger individuals under age 21 as compared to those ages over age 21.

Given recent evidence that RML adoption may generate spillovers to other drug use (Sabia et al. 2021; Hollingsworth et al. 2022), we also explore drug-involved suicide. Our focus is on drugs previously examined by Sabia et al. (2021) using the following International Classification of Disease, Tenth Revision (ICD-10) multiple cause-of-death codes: T40.1 (heroin), T40.2 (natural opioid analgesics, including morphine, codeine, and semisynthetic opioids, including oxycodone, hydrocodone, hydromorphone, and oxymorphone), T40.3 (methadone), T40.4 (synthetic opioid analgesics other than methadone, including

fentanyl and tramadol), T40.5 (cocaine), and T43.6 (methamphetamine). We find the highest rate of drug-involved suicides among non-Hispanic Whites (2.58 per 100,000 persons) as compared to Hispanics (0.77 per 100,000 persons) and Blacks (0.89 per 100,000 persons).

Finally, we also examine a broader definition of drug-related deaths, including both deaths classified as suicides as well as those not classified as such. Moreover, given evidence that opioid-involved mortality may be sensitive to increased access to recreational marijuana (Sabia et al. 2021; Mathur and Ruhm 2022), we also specifically examine the effect of RML adoption on race/ethnicity-specific opioid-involved deaths. Consistent with much of the opioid literature, we find the highest rate of opioid-involved overdoses over the sample period (2000-2019) among non-Hispanic Whites (11.3 per 100,000 persons) as compared to Hispanics (3.4 per 100,000 persons) and Blacks (5.5 per 100,000 persons).

IV. EMPIRICAL METHODS

A. Econometric Design

We begin by using a two-way fixed effects (TWFE) model to examine the relationship between RML adoption and race-specific arrest rates using the following estimating equation:

$$Arrest_{jst} = \beta_{j0} + \beta_{j1} RML_{st} + \beta_{j2} MML_{st} + \beta_{j3} MDL_{st} + \beta_{j4} LEA_{st} + \boldsymbol{X}_{st}'\boldsymbol{\alpha_j} + \tau_{jt} + \delta_{js} + \varepsilon_{jst}, \quad (1)$$

where $Arrest_{jst}$ is the adult arrest rate for race group *j* (White vs. Black) in state *s* in year *t*. The primary independent variable of interest, RML_{st}, is an indicator for whether a recreational marijuana law (RML) has been adopted. In all specifications, we also control for whether a medical marijuana law (MML_{st}) has been adopted, whether a marijuana decriminalization or depenalization law (MDL_{st}) has been adopted, and the number of law enforcement agencies reporting arrests (LEA_{st}) (as a means of addressing concerns that variation in measured arrests are capturing changes in reporting).

The vector $\boldsymbol{X_{st}}$ includes demographic controls (share of the state population that is African American and Hispanic); economic controls (state unemployment rate and log per capita income), policing controls (log law enforcement employees per 1,000 population); substance use policy controls (prescription drug monitoring program, naloxone access laws, Good Samaritan laws, and log beer taxes); and state-level social welfare policies (refundable EITC, Affordable Care Act-related Medicaid expansion, the natural log of the state or federal minimum wage, and whether the governor is a Democrat). In addition, τ_t is a year fixed effect, which accounts for unmeasured shocks that commonly affect states over time, and δ_s is a state fixed effect, which controls for time-invariant state-level heterogeneity.[22]

For our state-level NSDUH and NVSS-based analyses, we estimate similar models to that described in equation (1):

$$Y_{jst} = \pi_{j0} + \pi_{j1} RML_{st} + \pi_{j2} MML_{st} + \pi_{j3} MDL_{st} + \boldsymbol{X}_{st}'\boldsymbol{\sigma_j} + \tau_{jt} + \delta_{js} + \mu_{jst}, \quad (2)$$

[22] In the pooled regressions (with separate state-year observations for Black and White), all variables on the right-hand-side also include an interaction with an indicator for Black.

where Y_{jst} is, alternately, (1) the psychiatric illness prevalence rate, (2) the suicide rate, and (3) the drug-related mortality rate, for race- and ethnicity-specific demographic group *j* (non-Hispanic Whites vs Blacks of Hispanics) in state *s* in year *t*.

For our individual-level BRFSS analysis, we estimate:

$$MH_{ijst} = \gamma_{j0} + \gamma_{j1} RML_{st} + \gamma_{j2} MML_{st} + \gamma_{j3} MDL_{st} + \boldsymbol{X}_{st}'\boldsymbol{\theta_j} + \boldsymbol{Z}_{it}'\boldsymbol{\eta_j} + \tau_{jt} + \delta_{js} + \nu_{ijst}, \quad (3)$$

where MH_{ijst} measures one of the BRFSS-based outcomes for individual *i* who identifies with race-/ethnicity-group *j* in state *s* in month-by-year *t.*

The key parameters of interest in the above equations, β_{j1}, π_{j1}, and γ_{j1}, respectively, are the race- or ethnicity-specific (where available) effect of RML adoption on the outcomes under study. The treatment effect is identified from geographic and temporal variation in RML adoption, as described in Appendix Table 2, which shows effective dates of RMLs (Anderson and Rees Forthcoming). In some specifications, we replace RML_{st} with the mutually exclusive policy indicators, *RML with Dispensaries*$_{st}$ and *RML without Dispensaries*$_{st}$, which is a time-varying measure of whether the state RML is accompanied by recreational sales. The dates when recreational sales became permissible is noted in the third column of Appendix Table 2.

B. Identification Assumption

For our estimates of β_{j1}, π_{j1}, and γ_{j1} to produce unbiased estimates of the effects of RML adoption on the outcomes under study, policy endogeneity must not be present (whereby arrests, drug use, psychological health, and mortality drives RML adoption) and state-specific time-varying unobservables must not be correlated with the enactment of RMLs and with our outcomes. We undertake a number of descriptive strategies to ensure that this is the case. First, we explore the sensitivity of our estimates to the inclusion of additional controls for state-specific linear time trends and region-specific year fixed effects. These controls have the advantage of controlling for unmeasured time shocks that could be correlated with RML adoption and our outcomes, which could reduce omitted variable bias. On the other hand, the inclusion of such controls could introduce bias. For instance, the inclusion of state-specific time trends could obscure dynamics in the estimated treatment effect (Meer and West 2016). Moreover, forcing "close controls" (i.e., requiring treatment and control states to be within the same census region) may force a restriction of control states to observably worse counterfactuals (Neumark et al. 2014; Burkhauser, McNichols, and Sabia 2022). Thus, we exercise care in interpreting the findings from these specifications.

Second, we generate event studies in which we decompose the treatment effect over time. That is, we replace *RML*$_{st}$ with mutually exclusive indicators for leads and lags of *RML*$_{st}$. This allows for a test of parallel pre-treatment trends; that is, whether the outcomes were trending similarly in treatment and control states prior to the adoption of an RML. This is an indirect, descriptive test of policy endogeneity (i.e., whether changes in the outcomes drove the adoption of an RML) and the parallel trends assumption. In addition, the event-study analysis allows us to estimate lagged RML effects to explore the longer-run effects of RMLs.

Third, TWFE estimates may be biased if there are spillover effects of RMLs to control states. For instance, Hansen et al. (2020) found that those living in non-RML states, but near the border of states that have adopted RMLs may see consumption increases through cross-border travel and smuggling. Such cross-

border travel could bias estimated treatment effects.[23] To test this possibility, we model border state policies by adding the *Border State RML*$_{st}$, indicator variable for whether a border state has adopted an RML to the right-hand side of the above estimating equations. This allows us to explicitly test whether a border state RML policy affects the outcomes of a neighboring state. Additionally, to the extent that border RMLs are affecting outcomes in the control states (a violation of the stable unit treatment value assumption), we estimate models that restrict the analysis sample to RML states and never-adopting RML states that do not border an RML state to probe whether our results are robust to these potential cross-state RML spillovers.

C. Heterogeneous and Dynamic Treatment Effects

An important critique of the TWFE estimator is that in the presence of heterogeneous and dynamic treatment effects, such estimates may be biased (Goodman-Bacon 2021; Callaway and Sant'Anna 2021). We take several approaches to address this concern. First, we employ an alternate difference-in-differences estimator developed by Callaway and Sant'Anna (2021). To do so, we restrict the control group to states that never adopted an RML during the sample period and then estimate event-study coefficients using the Callaway-Sant'Anna approach.[24]

Second, we estimate synthetic control models (Abadie et al. 2010) for the six earliest adopting RML states following Sabia et al. (2021): Colorado, Washington, Alaska, Oregon, Massachusetts, and California.[25] For this analysis (which we focus on using arrest outcomes), we restrict the donor pool to states that (1) had never enacted an RML or MML, or (2) had never enacted an RML and had enacted an MML at least five years prior to the adoption of an RML by the treated unit (which allows a significant post-treatment period for which the dynamic effects of MMLs to have evolved). We then generate synthetic weights from a linear combination of donor states that had the most similar annual pre-treatment levels (and trends) in the outcomes under study.[26] This approach allows us to explore longer-run effects of RMLs on arrests for states that were earlier adopters of RMLs and had a post-treatment period of at least three (3) years.

V. RESULTS

Our main findings appear in Tables 1 through 15 and Figures 1 through 7. All regressions are weighted by the relevant population of U.S. adults and standard errors are clustered at the state level (Bertrand et al. 2004).

A. Marijuana Arrest Results

Panel I of Table 1 presents estimates of the relationship between RML adoption and adult marijuana arrests. Controlling for state and year fixed effects, the number of agencies reporting arrests, and MML and MDL adoption, we find that RMLs are associated with a 121.6 per 100,000 person decline in

[23] For instance, cross-border travel could bias effects of RMLs on consumption toward zero. For arrests, it is possible that having a border state with an RML could lead to an increase in arrests in a nearby state, thus biasing estimates upward in absolute magnitude (e.g., toward larger negative effects of RMLs).

[24] For this approach, we control for MML adoption, MDL adoption, the state unemployment rate, log per capita income, and in the case of arrests, the number of LEAs reporting arrests.

[25] We do not report synthetic control estimates for Washington D.C. (a 2015 RML adopter). This is for two reasons: (i) poor pre-treatment fit between D.C. and synthetic D.C. and (ii) we do not observe D.C. for the entirety of the sample period in the UCR.

[26] We also attempted a few other synthetic control approaches, including limiting the donor pool to only MML adopting non-RML states and matching on odd-only or even-only years on the outcome under study in the pre-treatment period, all which yielded a qualitatively similar set of results.

marijuana arrests (column 1). This represents a 71.4 percent decline relative to the pre-treatment mean arrest rate in RML-adopting states. The inclusion of controls for socioeconomic, demographic, and policing variables (column 2), substance use policies (column 3), and social welfare policies (column 4) has very little impact on the estimated treatment effect.

In the remaining panels, we present estimates for Whites (panel II) and Blacks (panel III). Our results show consistent evidence that RMLs reduced marijuana arrests for both races. However, the magnitude of the estimated treatment effect is nearly three times larger in absolute magnitude for Blacks (-325.2, panel III, column 4) as compared to Whites (-115.2, panel II, column 4). These findings suggest that RMLs reduced the absolute marijuana arrest rate more for Black adults than White adults.

The findings in panel I of Table 2 suggest that the difference in the effect of RMLs on marijuana arrests among Black versus White adults is significantly different. With respect to arrest rates (column 1), we find that the 209.9 arrests per 100,000 differential between Whites and Blacks is statistically significantly different. Moreover, when we disaggregate all marijuana arrests into marijuana arrests for possession (column 2) and sales (column 3), we continue to find that the differential arrest rate reduction is significantly greater for Blacks than Whites. This finding suggests that, in absolute arrest reductions, Blacks gain more than Whites from RMLs.

In Appendix Table 3, we show the sensitivity of the above finding to the inclusion of controls for spatial heterogeneity. The results show that controlling for state-specific linear time trends and census region-specific year fixed effects does not change our main result.

Event-study analyses, presented in Figure 1, show coefficients generated using TWFE (panels a, c, and e) and Callaway and Sant'Anna (panels b, d, and f) estimates, and suggest, in the main, common pre-treatment trends with a decline in marijuana arrests following RML adoption. In most cases, the drop in marijuana arrests is largest one or more years following RML adoption and the effect is generally larger for Blacks as compared to Whites. This finding is consistent with the hypothesis that the absolute gains (in terms of arrest reductions) to Blacks from RML adoption are greater than Whites.

However, a more nuanced story emerges when we compare the magnitudes of these marginal effects to pre-treatment means of marijuana-related arrests among Blacks and Whites. While the treatment effect is nearly three times larger (in absolute magnitude) for Blacks than Whites, so is the rate of pre-treatment marijuana arrests. Adjusting for pre-treatment differences in mean arrest rates, we find that RML adoption is associated with a 72.8 percent (-115.2/158.3) decline in arrests for Whites and a 66.0 percent decline ([-115.2 - 209.9]/492.3) in arrests for Blacks.

We also demonstrate this finding by changing the functional form of the regression. In panel II, we use the log of the marijuana arrest rate as our dependent variable rather than the level, which means that the estimated treatment effects can now be interpreted as the (approximate) percentage change in marijuana arrests. The results show that the percent decline in marijuana arrests following RML adoption is statistically equivalent for Whites and Blacks (about 80 percent).

Given that RMLs increased marijuana consumption among Black and non-Hispanic White young adults comparably (Martins et al. 2021), and the majority of arrestees for marijuana-related offenses are for those under age 35, one interpretation of these findings is that the lifting of marijuana prohibition through the adoption of RMLs led to a "race-blind" policing response.[27]

Collectively, the findings in Tables 1 and 2 suggest a nuanced interpretation of the effects of RMLs on racial disparities in arrests. On the one hand, Black adults saw absolute gains in arrest rate reductions

[27] This conclusion rests on the hypothesis that the consumption effects are a reasonable proxy for how RMLs affected race-specific arrestable (i.e.., greater than 1.0 or 1.5 ounces) rates of possessing/carrying marijuana.

relative to Whites, suggesting that RMLs benefited those who have faced disproportionately higher marijuana arrest rates. Moreover, the comparable declines (in terms of percent changes in marijuana arrests) suggests that the repeal of recreational marijuana prohibition was implemented in a way that appeared to be race neutral (at least with respect to marijuana), with Whites not disproportionately benefiting from racially biased policing practices, as they may have gained from in the past. On the other hand, RML adoption did not appear to break any pre-RML patterns in the likelihood of a marijuana arrest between Black and White adults.[28]

Lastly, in Appendix Figure 1, we turn to an alternate approach to estimate treatment effects, the synthetic control method (SCM), and focus on the six earliest-adopting RML states that allow for a longer examination of post-RML outcomes (Colorado, Washington, Alaska, Oregon, California, and Massachusetts). Synthetic units for each treated unit are constructed by matching on all pre-treatment year observations of the outcome and inference is performed via the permutation test described in Abadie et al. (2010).[29]

For each early adopter in Appendix Figure 1, we plot the synthetic control estimates for White and Black on the same figure and report the estimated RML treatment effects and the p-values obtained from the permutation tests. Across early adopters, we generally find sizable declines in arrests post-RML adoption (and larger in magnitude for Blacks than Whites). Additionally, the pre-treatment trends in the outcome between the treated and synthetic units track closely for the most part (aside from California and Massachusetts, which exhibit some divergence in pre-treatment arrest levels). While our synthetic control estimates are imprecisely estimated,[30] they yield a similar finding to that obtained using our difference-in-differences analysis, that is, RMLs are associated with reductions in marijuana arrests that are larger in absolute, but not relative terms for Blacks than Whites. Effects on other arrest outcomes largely suggest no impacts for Blacks or Whites.

B. Non-Marijuana Drug Arrests

In Tables 3 and 4, we examine non-marijuana drug arrests. Such arrests could be affected by changes in policing; for instance, by (1) police redistributing effort to detecting cocaine/heroin, narcotics, or other dangerous non-narcotic possession and sales, (2) individuals substituting toward or away from drugs other than marijuana, or (3) supply-side (producer/supplier) responses in non-marijuana drug markets.

Our findings in Table 3 provide little support for the hypothesis that RML adoption significantly increases cocaine or heroin arrests among White or Black adults. However, it is notable that the point estimate for Blacks is substantially larger (in both absolute and percentage terms) for Black as compared to White adults (15.8 percent versus 1.1 percent). This difference is not statistically different from zero at conventional levels, as shown in panel I of Table 4. However, the magnitude of the effect could suggest police reallocating resources to detecting cocaine and heroin in a way that disproportionately harms

[28] Moreover, in making "one-to-one" comparisons of RML-marijuana consumption effects to RML-arrest effects to judge the racial equity implications of RMLs in terms of arrests, we are implicitly assuming that Blacks and Whites are re-arrested at the same rate for marijuana offenses. The UCR measures race-specific totals for arrests, not the unique number of people arrested for a given offense. For example, in panel I of Table 2 we find that there are 209.9 fewer arrests per 100,000 people for Blacks than Whites in the wake of RMLs. This could reflect fewer Blacks being arrested for marijuana, Blacks being *re-arrested* at a lower rate, or a combination of both.

[29] For states that adopt an RML after July 1 of year *t*, we set their treated year to year *t*+1. Regarding the donor pool, we first drop states that ever adopt an RML during the sample period and next drop states that adopt an MML within six years of the treated unit's RML adoption. For example, the treated year for Colorado is 2013 (RML adopted December 2012), so we drop all other ever-RML adopters and the states that adopted an MML in 2008 or later from the donor pool.

[30] From an inference standpoint, our stringent donor pool criteria, in which the largest number of donor units available is 23, the ratio of the post-to-pre-root mean square prediction error (RMSPE) for the treated unit must be ranked 1 or 2 in order to achieve statistical significance at conventional levels.

Blacks, or greater substitution toward possessing or selling these “harder” drugs among Black adults in response to RML adoption.

With respect to addicting synthetic narcotics, our findings show that RML adoption is associated with a 20-to-24 per 100,000-person arrest reduction for addicting synthetic narcotics for both White and Black adults. Relative to pre-treatment means, these effect sizes are quantitatively similar (panel II of Table 3). Event-study analysis using the Callaway and Sant’Anna (2021) estimator, shown in panel (b) of Figure 2, provides stronger evidence of a decline in synthetic narcotic arrests in response to RML adoption among Whites. These results add to a growing body of evidence that marijuana and opioids may be substitutes in consumption (Abouk et al. Forthcoming; Sabia et al. 2021). Turning to dangerous non-narcotic arrests (panel III of Tables 3 and 4), our findings provide little support for the hypothesis that RML adoption affects arrests for these substances.

C. Arrests for Part I and Delinquency-Related Offenses

In Tables 5A and 5B, we explore spillover effects of RMLs on racial disparities in property and violent crime arrests. Our results in column (1) of Table 5A provide no evidence that RML adoption affects property crime arrests for either Whites or Blacks. Similarly, event-study analyses in panels (a) and (b) of Figure 3 provide little evidence that property crime arrests change in response to RMLs for either Blacks or Whites. When we disaggregate property crime arrests by specific offense type, we find no evidence that RML adoption affects White or Black adult arrests for larceny (column 2), burglary (column 3), or motor vehicle theft (column 4). However, we note that the effect for Blacks is generally more positive than for Whites.

When we explore violent crime arrests in Table 5B, we find little evidence that RMLs affect violent crime arrests overall (column 1) or arrests for assaults (column 2), robberies (column 3), or homicides (column 4) among Whites. However, for Blacks, we do detect a larger increase in assault-related arrests, though this interactive effect is not statistically distinguishable from zero at conventional levels. Event-study analyses in panels (c) and (d) of Figure 3 provide little support for the hypothesis that White violent crime arrests changed following RML adoption, but for Blacks, we see some suggestive evidence of a lagged increase in violent crime arrests. This finding could suggest a differential demand-side effect of RML adoption for Blacks (i.e., whereby the psychotropic or addictive effects of marijuana use lead to increased violent crime). However, this seems at least somewhat less likely given that the impacts on drug consumption are not statistically different for Black and White young adults (Martins et al. 2021). Another interpretation is that policing resources are reallocated to arresting violent crime offenders in a racially disparate manner. This latter interpretation could suggest an unintended inequitable policing impact of RMLs.

Consistent with Dragone et al. (2019), we also find some evidence that RML adoption was associated with a decline in arrests for rape (column 5). This finding could suggest that RML adoption induced substitution effects away from some harder drugs or alcohol, which have been found to be associated with sexual assault (Abbey et al. 2001). The finding could also be explained by policing resources formerly used to enforce marijuana laws being substituted toward sex crime detection (thereby raising the opportunity cost of such crime).

In Table 6, we explore whether RMLs affected delinquency-related offenses: vandalism (column 1), liquor law violations (column 2), driving under the influence (column 3), and drunkenness or disorderly conduct (column 4). Our results provide little support for the hypothesis that RML adoption affects delinquency-related for either Whites or Blacks.

D. Robustness Checks

In Appendix Tables 4 and 5, we undertake a series of sensitivity checks. In panel I of Appendix Table 4, models are estimated at the agency-year level and the sample is restricted to agencies that report arrests at least six months of the year (or report all arrests in December) and have populations of at least 50,000 during the sample period (and at no point less than 25,000). These analysis sample criteria follow Chu (2015) and Chu and Townsend (2019), with the objective of limiting the influence that poor agency arrest reporting may have on the estimates. The findings in our level arrest regressions (panel I) are comparable to those obtained in the state-level analyses above.

Panel II shows Poisson estimates on the same set of agencies as explored in panel I, where the dependent variable is defined as the count of arrests rather than the rate and the exposure variable is set to the estimated agency-specific adult population served by the local law enforcement agencies. The coefficients can, therefore, be interpreted as (approximate) percent changes in the arrest rate. The results provide consistent evidence that RML adoption is associated with a reduction in marijuana and non-marijuana related drug arrests, but that the percentage reduction was not statistically different for Whites and Blacks (similar to the log-level estimates in panel II of Table 2). We continue to find no evidence that the RML adoption was associated with a significant change in the arrest rates for property crime.

In panel III of Appendix Table 4, we produce unweighted as compared to weighted estimates. Unweighted estimates weight each state in the sample equally without regard to population size. The findings are qualitatively similar to the weighted model estimates (our preferred specification).

Finally, we undertake one additional strategy to disentangle drug arrest effects from changes in arrest reporting by law enforcement agencies. Following Chu (2015), we use drug arrest ratios as the dependent variable – the ratio of drug-specific arrests to the overall number of arrests (for all part I, part II, and drug offense arrests). The findings in Appendix Table 5 are consistent with our main results using the arrest rate.

One concern with the estimates reported in Tables 1 through 6 is that they could be biased by failing to control for whether a border state had adopted an RML (i.e., failing to account for possible cross-state RML spillovers). Panel I of Table 7 reports estimates from our baseline TWFE model. When we control for whether a border state has adopted an RML (panel II), the estimated effect of RMLs on marijuana arrests (columns 1 and 2), non-marijuana drug arrests (columns 3 and 4), property crime arrests (columns 5 and 6), and violent crime arrests (columns 7 and 8), changes little from the estimated RML effects in panel I. Moreover, there is no evidence of important changes in the estimated RML effect if we drop non-RML states that border a state that had adopted an RML (panel III). This provides some support for the hypothesis that our arrest estimates are not contaminated by border state policies.

Independently, we do uncover some evidence that is consistent with some policing responses to RMLs (panel II, row 2). For instance, we find that a border state's adoption of an RML is associated with a reduction in the non-marijuana drug arrest rate among Whites (independent of the reduction seen from their own state RML). This result is consistent with the hypothesis that for White adults, access to legalized marijuana in a nearby jurisdiction may reduce reliance on other substances (i.e., opioids) that can lead to arrests if obtained (or sold) illegally.

In addition, we find evidence that a border state adopting an RML is associated with a 67.9 per 100,000 (7.5 percent) increase in violent crime arrests among Blacks. This result is consistent with the hypothesis that RML adoption in neighboring states may increase police resource allocation in non-RML states to patrol possible violent crime in non-race neutral ways that disproportionately impact Blacks.

Finally, in Table 8, we explore the role of open dispensaries on arrests. Our findings suggest that the

disproportionate decline in arrests that we detect is driven largely by the period immediately following RML adoption before dispensaries are open. When dispensaries open, it appears that there is no difference in the rate of marijuana arrests among Blacks and Whites (the estimated differential effect for Black is larger in absolute value and negative, but not statistically different from zero). Moreover, when dispensaries open, there is evidence that RMLs marginally increase violent crime arrests, which could be consistent with several hypotheses, including (1) dispensaries leading to larger increases in marijuana use (Dave et al. 2022), which generates more violent crime due to psychiatric effects of RMLs or, (2) police allocating greater resources to violent crime detection after recreational dispensaries open. We find evidence that RMLs accompanied by legal open dispensaries are associated with an increase in violent crime arrests of Black adults. This finding could be consistent with the hypothesis that policing resources reallocated away from enforcing marijuana prohibition are deployed to detect violent crime in a racially disparate manner.

Taken together, the above results provide support for the hypothesis that RML adoption was associated with a reduction in marijuana arrests for White and Black adults. The arrest rate fell more for Blacks than Whites, reflecting pre-treatment differentials in arrest rates for marijuana-related offenses. However, in percentage terms, the declines were similar. We find little support for the hypothesis that RML adoption led to greater policing resources being reallocated to fight other drug crime, more serious Part I offenses, or delinquency-related offenses in a manner that disproportionately harmed Blacks.

E. Psychiatric Well-Being

Next, we turn to the NSDUH and BRFSS to explore the impact of RMLs on psychiatric health of adults. Our results in Table 9 (panel I) show that RML adoption is associated with a 0.9 to 1.1 percentage-point increase in the prevalence rate of psychiatric illness of adults. This represents an approximately 5 percent increase relative to the pre-treatment mean and could suggest the presence of important adverse mental health effects of recreational legalization, in sharp contrast to earlier work on medical marijuana laws (Anderson et al. 2014; Sabia et al. 2017).[31]

When we turn to the outcomes of major depressive episodes (panel II) and suicidal thoughts (panel III), the pattern of results is similar. In our preferred specification (column 4), our findings show that RML adoption is associated with a 0.4 percentage-point increase in the prevalence rate of major depressive episodes (an approximately 10 percent increase relative to the pre-treatment mean) and a 0.21 percentage-point (5.4 percent) increase in the prevalence of suicidal thoughts. Event-study analyses of all three mental health outcomes in Figure 4 show a pattern of findings consistent with a causal impact of RML adoption on adult psychiatric health.

When we examine microdata from the BRFSS the pattern of findings suggests that the aggregate state-level findings from the NSDUH may mask important age- and race/ethnicity-specific heterogeneity. An examination of all adults ages 18-and-older in Table 10 provides little evidence that RML adoption is associated with statistically significant changes in poor mental health days among non-Hispanic Whites, Blacks, or Hispanics.[32] This pattern of findings is generally confirmed in Figure 5, where we show results from event-study analyses using TWFE and Callaway and Sant'Anna (2021) estimates.

When we further disaggregate the BRFSS data by age — those ages 18-to-20 and hence under the minimum legal purchase age for marijuana (Table 11) and those ages 21-and-older (Table 12) — we find stronger evidence that RML adoption is positively related to a greater number of poor mental health days

[31] On the other hand, Sabia and Nguyen (2018) find some evidence of MML-induced declines in wages for young adult males, which could be consistent with adverse psychiatric effects of MMLs.

[32] In Appendix Table 6, we show little evidence in support of important racial disparities in the effect of RML adoption on poor mental health days.

among those under age 21 (Table 11, panel I). Specifically, for non-Hispanic Whites, we find that the enactment of an RML is associated with a 2.8 percentage-point (22.2 percent) increase in the probability that a non-Hispanic White 18-to-20-year-old has at least 15 poor mental health days in the past month (column 2, panel I) and, on average, 0.8 more poor mental health days in the past month (17.3 percent more relative to the pre-treatment mean) (column 3, panel I). We also detect some evidence of increases in poor mental health following RML adoption for Blacks and Hispanics (panels II through IV), consistent with evidence that more frequent and heavier marijuana use following RML adoption could generate psychiatric problems for some teens (van Ours and Williams 2015). The results in Figure 6 provide inconsistent support for the hypothesis that RML adoption increases poor mental health days among young adults under age 21, with the strongest causal evidence for adverse psychiatric effects of RMLs when using TWFE as opposed to Callaway and Sant'Anna (2021) estimates.[33]

For those ages 21-and-older, there is less evidence of RML-induced changes in psychological health for either non-Hispanic Whites or racial/ethnic minorities (Table 12), though the effects for racial/ethnic minorities is decidedly more negative in the post-treatment period than for non-Hispanic Whites. Together, the findings provide little support for the hypothesis that RMLs impacted racial disparities in mental health among respondents to the BRFSS.

F. Mortality

The remaining tables of this study present estimates of the effects of RMLs on racial disparities in suicides and drug-related mortality. In Table 13, we find no evidence, among non-Hispanic White adults (panel I), that RML adoption significantly affects suicides (column 1), drug-involved suicide rates (column 2), or drug-involved deaths (column 3). However, when we turn to opioid-involved mortality (column 4), we find that RML adoption is associated with a (marginally significant) 4.2 per 100,000 persons reduction in opioid-involved mortality (column 4, panel I), representing a 37.1 percent decline relative to the pre-treatment mean. Event-study analyses in Figure 7 show that the decline in opioid-involved mortality appears to be causal in nature for non-Hispanic Whites, with opioid-involved mortality declining in treatment relative to control states after largely null pre-treatment coefficients.

Turning to Blacks and Hispanics in panel II, we find no evidence that RML adoption significantly affects drug-involved suicide rates (column 2), drug-involved deaths (column 3), or opioid-involved mortality (column 4). We do find that RML adoption is associated with an increase in suicide rates among Blacks and Hispanics (column 1, panels I and III). However, event-study analyses in Figure 7 and Appendix Figure 3 suggest that this effect is driven, at least in part, by a pre-treatment trend.[34,35] Thus, we are very cautious in interpreting this as evidence that RMLs leads to increased suicides among racial/ethnic minorities.[36]

Finally, in Table 14, we explore whether marijuana dispensaries play an important role in the race/ethnicity-specific estimates of the mortality (and mental health) effects of RMLs. We find that for non-Hispanic Whites (panel I), open recreational dispensaries play a key role in the decline of drug-involved

[33] Appendix Figure 2 provides separate event study depictions for Blacks and Hispanics. These results provide stronger support for the hypothesis that RMLs are associated with an increase in poor mental health days among younger non-Hispanic whites than racial/ethnic minorities. In either case, there is little evidence that RML adoption reduced disparities in psychological health.

[34] Appendix Table 7 provides formal statistical tests for whether the estimated suicide and opioid mortality coefficients differ by race- and ethnicity.

[35] Age-specific event-study estimates in Appendix Figures 4 and 5 suggest the strongest causal evidence for declines in opioid-involved mortality are among non-Hispanic Whites ages 21-and-older.

[36] In Appendix Table 8, we document that the decline in opioid-involved mortality for non-Hispanic whites (which we believe is causal in nature), is driven by adults ages 21-and-older. Across both non-Hispanic whites and Blacks, there is no evidence that RML adoption affects mortality among 18-to-20-year-olds.

suicides (column 3) and opioid-involved mortality (column 5). RML adoption accompanied by a legal recreational dispensary is associated with a 0.196 per 100,000 (7.6 percent) decline in drug-involved suicides and a 5.75 per 100,000 (approximately 50 percent) decline in opioid-involved mortality among non-Hispanic whites. For Black and Hispanic adults (panel II), RMLs with recreational dispensary openings are associated with an increase in suicide rates. This positive association among Black and Hispanic adults does not appear to be causal in nature, but rather driven by the pre-treatment trend we first detected in Table 14 and Figure 7.[37]

VI. CONCLUSIONS

On October 6, 2022, President Biden requested that the Secretary of Health and Human Services and the Attorney General to:

> *"initiate the administrative process to review expeditiously how marijuana is scheduled under federal law. Federal law currently classifies marijuana in Schedule I of the Controlled Substances Act, the classification meant for the most dangerous substances. This is the same schedule as for heroin and LSD, and even higher than the classification of fentanyl and methamphetamine – the drugs that are driving our overdose epidemic." (The White House 2022)*

One important impetus for this policy change was the disproportionate negative impact of marijuana prohibition on Black and Hispanic individuals. This study provides new estimates of the effects of state-level recreational marijuana laws on racial disparities in arrests, psychiatric health, and mortality.

Our findings show that the adoption of an RML leads to a decline in marijuana-related arrests among both Black and White adults. In absolute terms, the decline was greater for Black adults, but this was entirely a reflection of pre-treatment differentials in arrest rates between Blacks and Whites. We find little evidence that RML adoption affected non-marijuana drug arrests or property crime arrests among Whites or Blacks. However, there is some evidence that (lagged) arrests for violent offenses involving Black adults rose following RML adoption, particularly when open recreational dispensaries are permitted. This could suggest racial differences in violent crime responses to RML adoption. However, given that Whites and Blacks see similar marijuana arrest responses, consumption responses, and psychiatric responses to RML adoption, a more likely explanation may be that the reallocation of policing resources to fight violent crime are undertaken in a racially disparate manner.

With respect to psychological health, we find some evidence that RML adoption may have adversely affected the psychiatric health of younger white individuals but had little effect on (or possibly helped) the psychological health of Black adults. Finally, turning to mortality, we find that RML adoption is associated with a reduction in opioid-involved mortality. The finding appears largest (in absolute and statistical significance terms) for non-Hispanic Whites relative to Blacks and Hispanics.

Together our findings suggest that RMLs did little to reduce racial inequalities in arrests, psychological health, or mortality. However, the failure of RMLs to reduce racial disparities (from a relative perspective) does not imply that there are not important absolute benefits for racial/ethnic minorities from RML adoption. The costs of having an arrest record on future economic wellbeing is substantial, particularly for historically marginalized groups, including young Black and Hispanic males. The magnitudes of our

[37] Appendix Tables 9 and 10 show the sensitivity of our mental health and mortality findings to the inclusion of state-specific linear time trends and census region-specific year fixed effects. The inclusion of these controls for spatial heterogeneity does not change our main conclusions on adult psychological health, but the effects on mortality are, in the main, much smaller in magnitude. We interpret this as evidence of state-specific linear time trends obscuring important dynamics in the opioid mortality-related effects of RML adoption.

estimated marijuana arrest declines suggest that RML adoption results in approximately 3,800 fewer marijuana arrests per treatment state-year for Black adults and about 12,600 fewer arrests per treatment state-year for White adults. These arrests declines will translate to at least some decline in the number of individuals with a recent criminal record (to the extent that some marijuana arrestees do not have recent prior arrest records). Given experimental evidence that not having a drug-related criminal record is associated with a nearly 60 percent higher likelihood of receiving a job callback (Agan and Starr 2017), our findings suggest potentially important labor market gains for racial/ethnic minorities from the adoption of RMLs.

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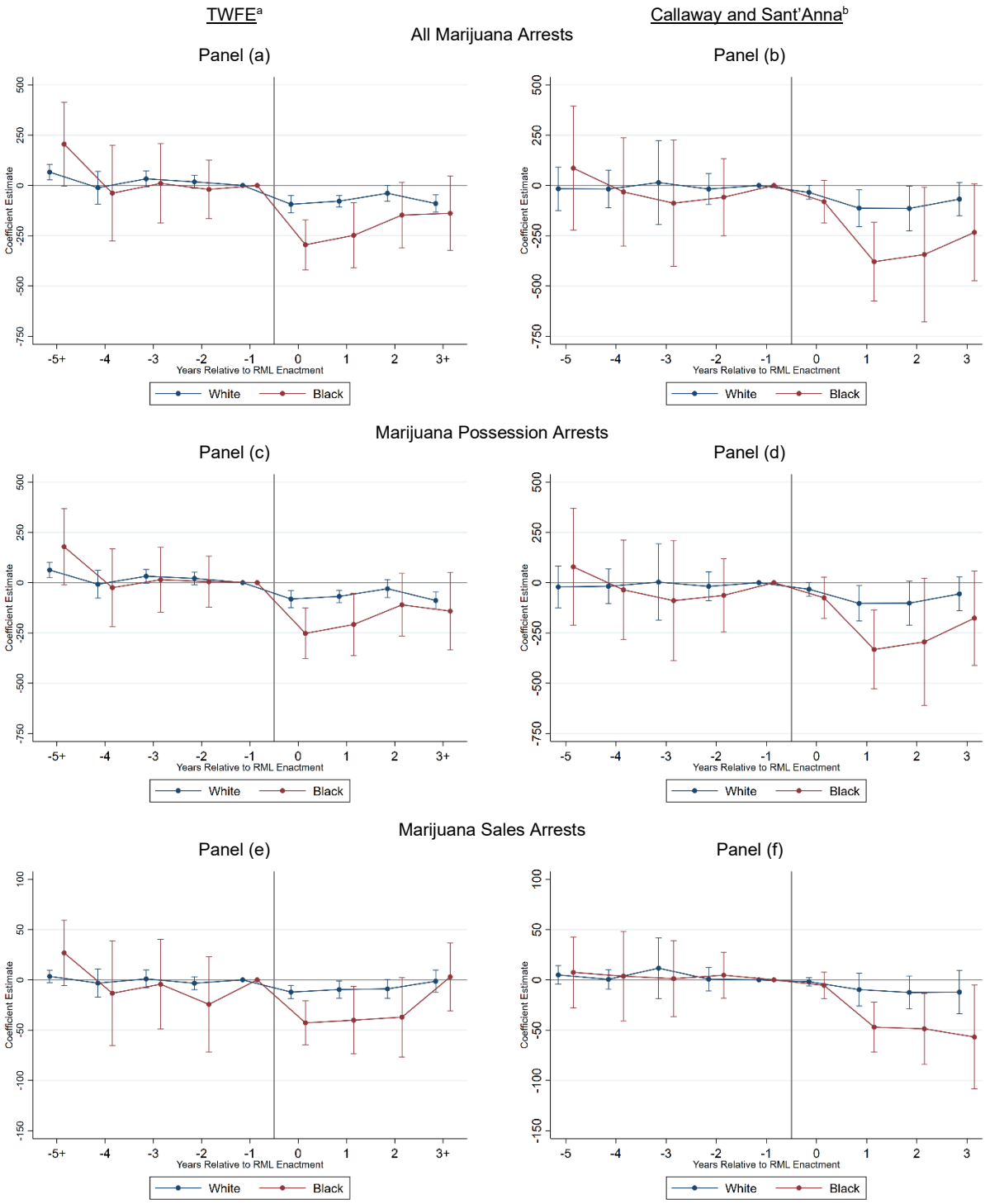
VIII. FIGURES AND TABLES

To request a fully accessible version of this section, please email depc@osu.edu.

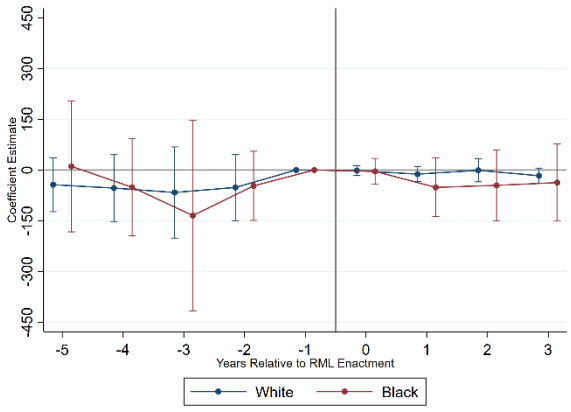
Figure 1. Race-Specific Event-Study Analyses of RMLs and Marijuana Arrests, UCR



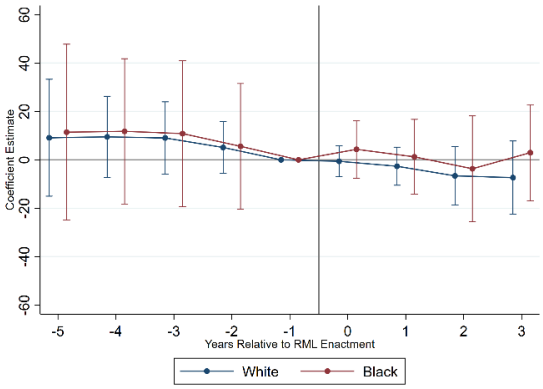
[a] All regressions include the control variables listed in the notes of Table 1. Regressions are weighted using the adult state population and standard errors are clustered at the state level. Error bars are 95 percent confidence intervals.
[b] Event study analysis conducted using the outcome regression approach of Callaway and Sant'Anna (2021). The control group is comprised of "never-treated" states. All regressions include control variables for medical marijuana laws, marijuana decriminalization laws, the state unemployment rate, per capita personal income (logged), and the number of law enforcement agencies reporting arrests. Regressions are weighted using the adult state population and bootstrapped standard errors are clustered at the state level. Error bars are 95 percent confidence intervals.

Figure 2. Race-Specific Event-Study Analyses of RMLs and Non-Marijuana Drug Arrests, UCR, Callaway and Sant'Anna Estimates

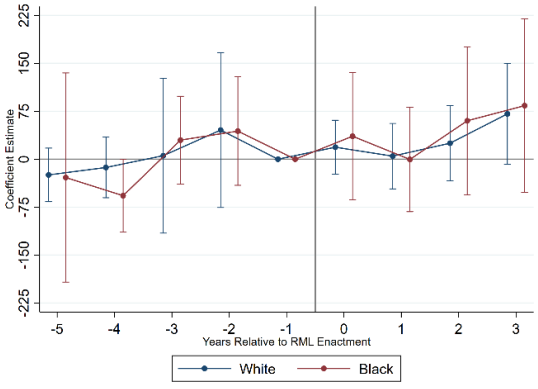
Panel (a) Cocaine-Heroin Arrests



Panel (b): Addicting Synthetic Narcotics Arrests

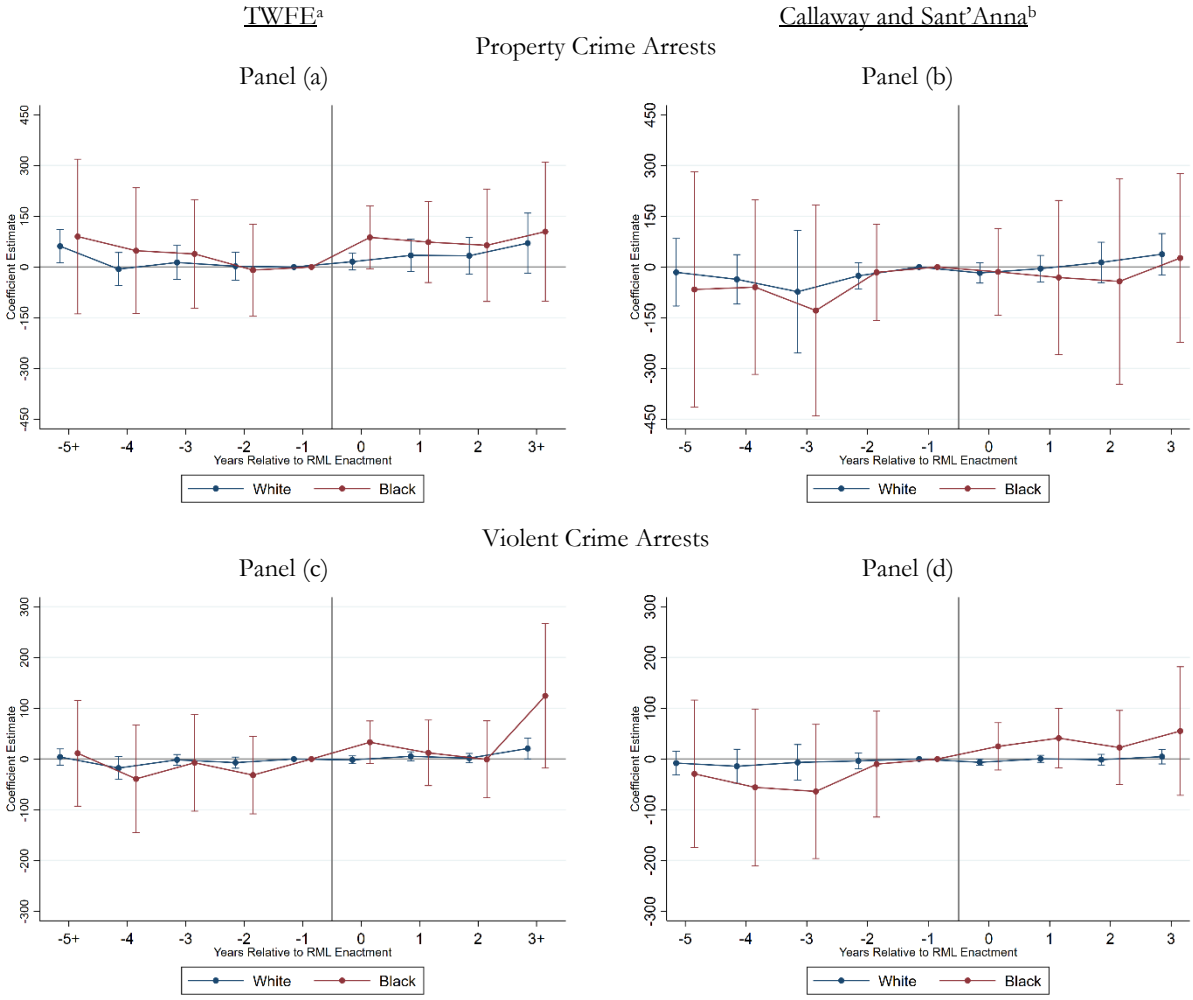


Panel (c): Dangerous Non-Narcotic Arrests



Notes: Event study analysis conducted using the outcome regression approach of Callaway and Sant'Anna (2021). The control group is comprised of "never-treated" states. All regressions include control variables for medical marijuana laws, marijuana decriminalization laws, the state unemployment rate, per capita personal income (logged), and the number of law enforcement agencies reporting arrests. Regressions are weighted using the adult state population and bootstrapped standard errors are clustered at the state level. Error bars are 95 percent confidence intervals.

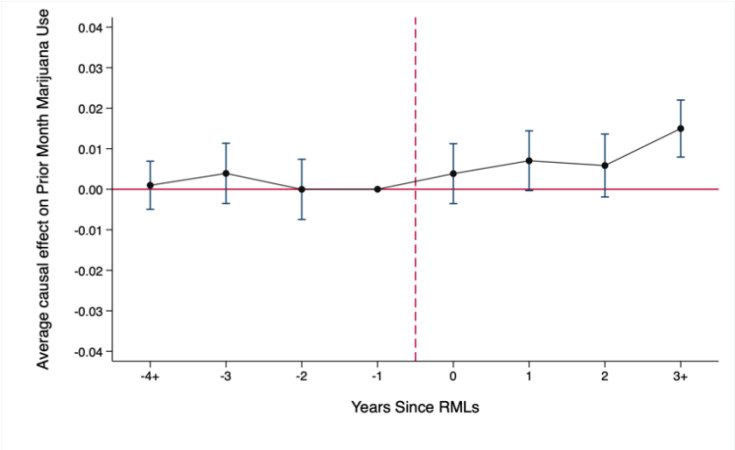
Figure 3. Event-Study Analyses of RMLs and Race-Specific Property and Violent Crime Arrests



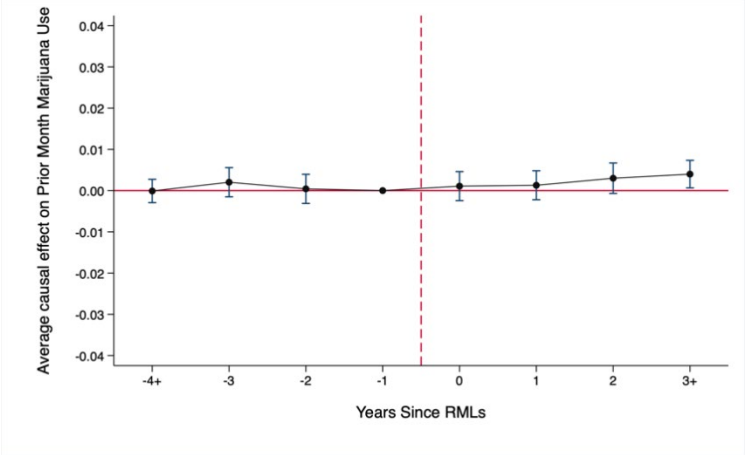
[a] All regressions include the control variables listed in the notes of Table 1. Regressions are weighted using the adult state population and standard errors are clustered at the state level. Error bars are 95 percent confidence intervals.
[b] Event study analysis conducted using the outcome regression approach of Callaway and Sant'Anna (2021). The control group is comprised of "never-treated" states. All regressions include control variables for medical marijuana laws, marijuana decriminalization laws, the state unemployment rate, per capita personal income (logged), and the number of law enforcement agencies reporting arrests. Regressions are weighted using the adult state population and bootstrapped standard errors are clustered at the state level. Error bars are 95 percent confidence intervals.

Figure 4. Event-Study Analyses of RMLs and Psychiatric Health, TWFE Estimates

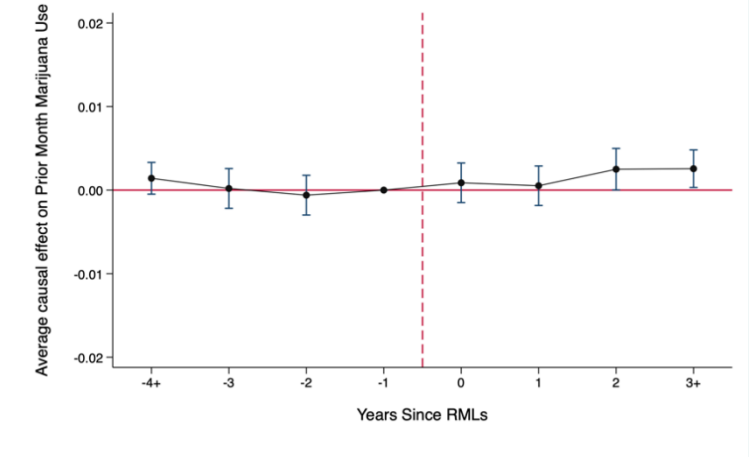
Panel (a): Any Mental Health Problem



Panel (b): Major Depressive Episodes



Panel (c): Suicidal Thoughts



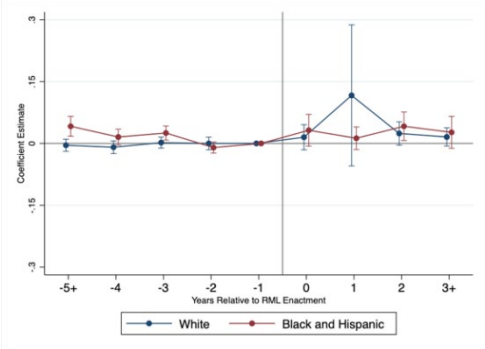
Notes: All regressions include the control variables listed in the notes of Table 1. Regressions are weighted using the adult state population and standard errors are clustered at the state level. Error bars are 95 percent confidence intervals.

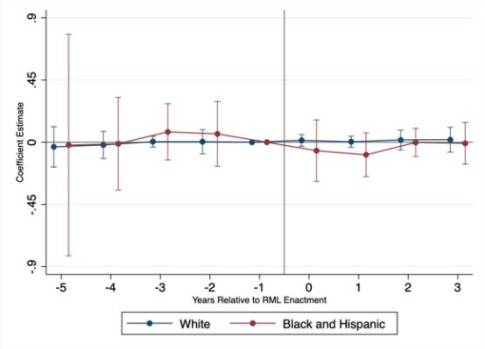
Figure 5. Event-Study Analyses of RMLs and Race-Specific Mental Health Outcomes

TWFE[a] Callaway and Sant'Anna[b]

Any Poor Mental Health Days

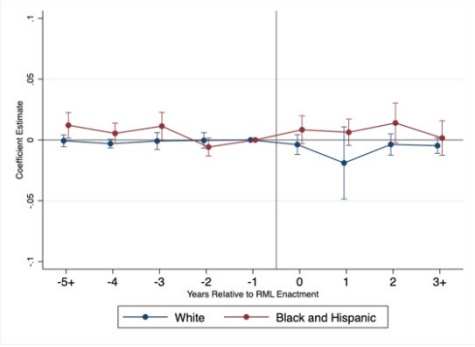
Panel (a) Panel (b)

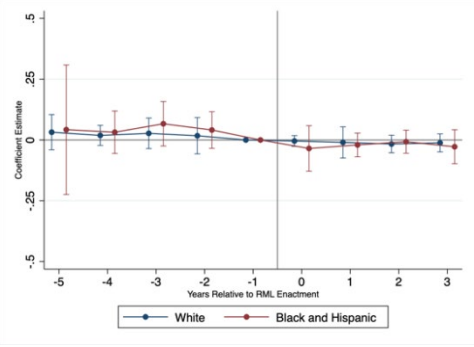




≥ 15 Poor Mental Days

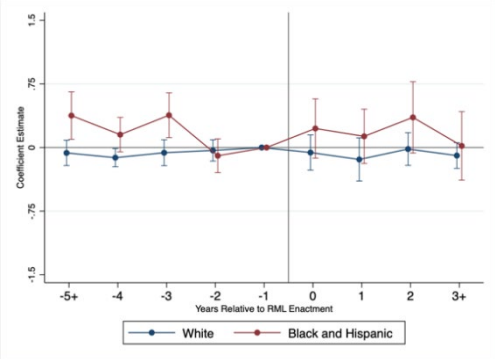
Panel (c) Panel (d)

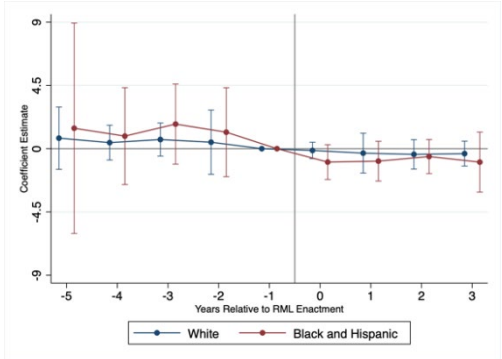




Number of Poor Mental Health Days

Panel (e) Panel (f)





[a] All regressions include the control variables listed in listed in the notes of Table 1. Regressions are weighted using BRFSS sample weights and standard errors are clustered at the state. Error bars are 95 percent confidence intervals.

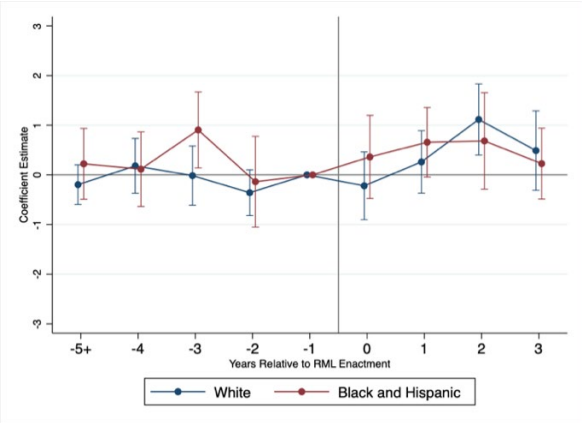
[b] Event study analysis conducted using the outcome regression approach of Callaway and Sant'Anna (2021). The control group is comprised of "never-treated" states. All regressions include control variables for medical marijuana laws, marijuana decriminalization laws, the state unemployment rate, and per capita personal income (logged). Regressions are weighted using the adult state population and bootstrapped standard errors are clustered at the state level. Error bars are 95 percent confidence intervals.

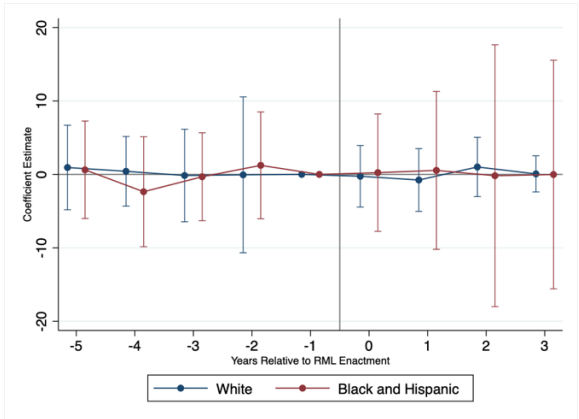
Figure 6. Event-Study Analyses of RMLs and Age- and Race-Specific Number of Poor Mental Health Days, by Age, BRFSS

TWFE[a] Callaway and Sant'Anna[b]

Ages 18-20

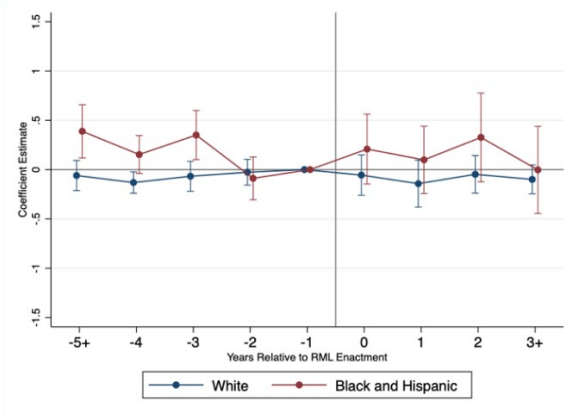
Panel (a) Panel (b)



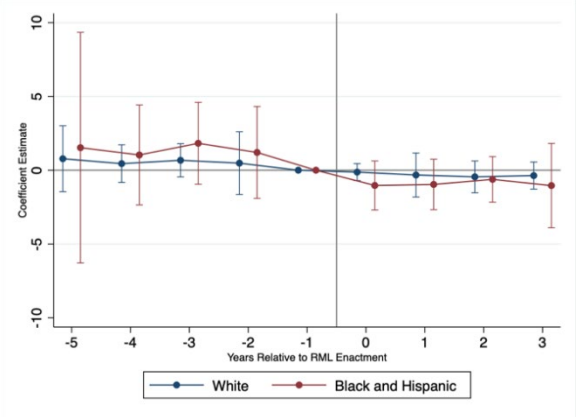


Ages 21+

Panel (c)



Panel (d)



[a] All regressions include the control variables listed in the notes of Table 1. Regressions are weighted using BRFSS sample weights and standard errors are clustered at the state. Error bars are 95 percent confidence intervals.

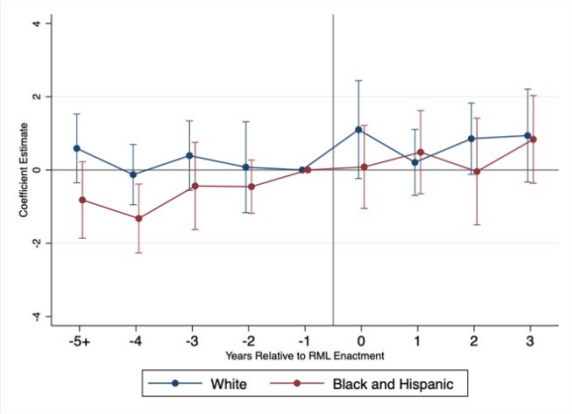
[b] Event study analysis conducted using the outcome regression approach of Callaway and Sant'Anna (2021). The control group is comprised of "never-treated" states. All regressions include control variables for medical marijuana laws, marijuana decriminalization laws, the state unemployment rate, and per capita personal income (logged). Regressions are weighted using the adult state population and bootstrapped standard errors are clustered at the state level. Error bars are 95 percent confidence intervals.

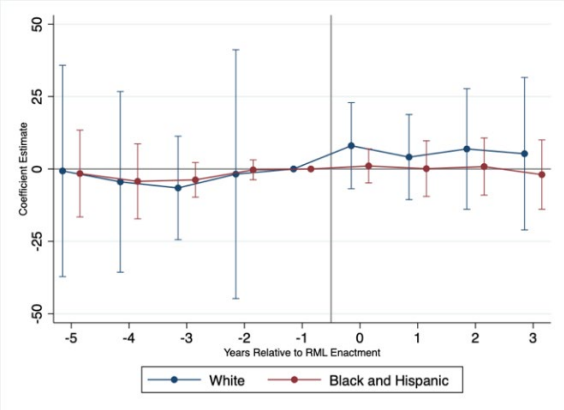
Figure 7. Event-Study Analyses of RMLs and Race-Specific Mortality Outcomes, NVSS

TWFE[a] Callaway and Sant'Anna[b]

All Suicide

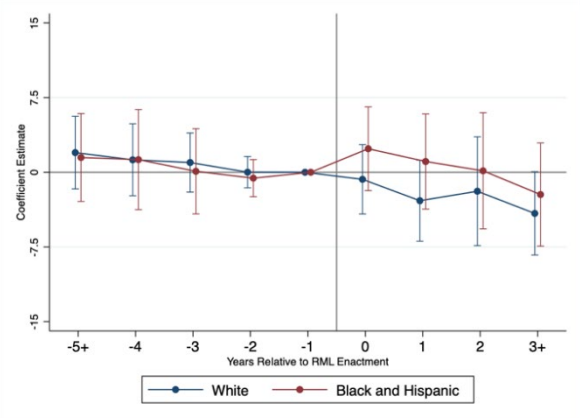
Panel (a) Panel (b)

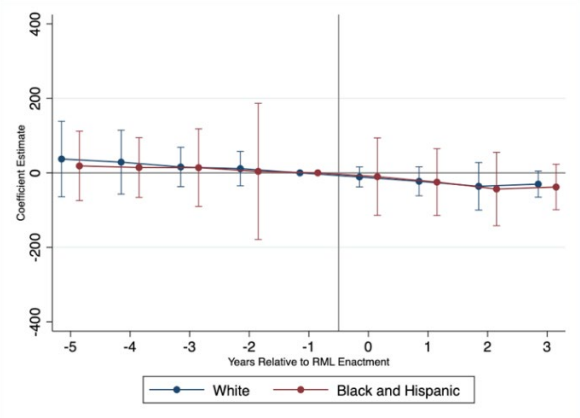




Opioid Overdose

Panel (c) Panel (d)





[a] All regressions include the control variables listed in the notes of Table 1. Regressions are weighted using the adult state population and standard errors are clustered at the state level. Error bars are 95 percent confidence intervals.

[b] Event study analysis conducted using the outcome regression approach of Callaway and Sant'Anna (2021). The control group is comprised of "never-treated" states. All regressions include control variables for medical marijuana laws, marijuana decriminalization laws, the state unemployment rate, and per capita personal income (logged). Regressions are weighted using the adult state population and bootstrapped standard errors are clustered at the state level. Error bars are 95 percent confidence intervals.

Table 1. TWFE Estimates of Effect of RMLs on Adult Marijuana Arrest Rate, by Race

	(1)	(2)	(3)	(4)
	Panel I: All Marijuana Arrests			
RML	-121.56***	-127.20***	-128.46***	-122.09***
	(19.59)	(19.83)	(17.25)	(18.35)
Pre-Treat Mean	170.41	170.41	170.41	170.41
N	998	998	998	998
	Panel II: White Marijuana Arrests			
RML	-114.06***	-120.05***	-119.60***	-115.23***
	(17.54)	(17.80)	(16.44)	(17.52)
Pre-Treat Mean	158.32	158.32	158.32	158.32
N	998	998	998	998
	Panel III: Black Marijuana Arrests			
RML	-349.78***	-360.72***	-376.93***	-325.15***
	(70.62)	(72.11)	(76.42)	(103.67)
Pre-Treat Mean	492.25	492.25	492.25	492.25
N	998	998	998	998
Controls				
State & Year FE, LEAs?	Yes	Yes	Yes	Yes
MML & MDL?	Yes	Yes	Yes	Yes
Socioeconomic, Demographic, and Policing?	No	Yes	Yes	Yes
Substance Use Policy?	No	No	Yes	Yes

Social Welfare Policy Environment?	No	No	No	Yes

***p < .01; **p<.05; *p<0.10

Notes: All regressions include state fixed effects, year fixed effects, and a control for the number of law enforcement agencies reporting arrests. "MML & MDL" refer to controls for the presence of a medical marijuana law and the presence of a marijuana decriminalization law. "Sociodemographic, Demographic, and Policy" refer to controls for the state unemployment rate, per capita personal income (logged), the proportion of state population that is Black, the proportion of the state population that is Hispanic, and law enforcement personnel per 1,000 population (logged). "Substance Use Policy" refers to controls for beer tax per gallon (logged), the presence of alcohol- and drug-specific Good Samaritan laws, the presence of a naloxone access law, and the presence of a must-access prescription drug monitoring program. "Social Welfare Policy Environment" refers to controls for the state EITC refundable rate, the higher of the state of Federal minimum wage (logged), an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, and whether the governor is a Democrat. Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Table 2. TWFE Estimates of the Effect of RMLs on the Race-Specific Differential in Marijuana Arrest Rate, by Possession or Sales Arrest

	(1)	(2)	(3)
	All Marijuana Arrests	Marijuana Possession Arrests	Marijuana Sales Arrests
		Panel I: Arrest Rate	
RML	-115.23***	-105.93***	-9.30***
	(17.52)	(17.18)	(3.46)
RML*Black	-209.92**	-177.08*	-32.84**
	(98.10)	(89.34)	(15.43)
Pre-Treat Mean (White)	158.32	132.31	26.00
Pre-Treat Mean (Black)	492.25	363.44	128.81
N	1,996	1,996	1,996
		Panel II: Log (Arrest Rate)	
RML	-1.701***	-1.702***	-1.165***
	(0.255)	(0.333)	(0.259)
RML*Black	0.037	-0.028	0.151
	(0.238)	(0.270)	(0.258)
N	1,982	1,982	1,943

***p < .01; **p<.05; *p<0.10

Notes: All regressions include the control variables included in column (4) of Table 1 (for the list of variables, see notes to Table 1). All variables included on the right-hand-side of the regression include both the "level" and an interaction with "Black." Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Table 3. TWFE Estimates of Effect of RMLs on Adult Non-Marijuana Drug Arrest Rate, by Race

	(1)	(2)
	White	Black
	Panel I: Cocaine-Heroin Arrests	
RML	2.20	114.98
	(8.23)	(88.19)
Pre-Treat Mean	187.59	729.01
N	998	998
	Panel II: Addicting Synthetic Narcotics Arrests	
RML	-24.29***	-20.11**
	(8.74)	(9.80)
Pre-Treat Mean	18.07	19.99
N	998	998
	Panel III: Dangerous Non-Narcotic Arrests	
RML	-11.72	-30.22
	(11.12)	(43.98)
Pre-Treat Mean	243.22	386.90
N	998	998

***p < .01; **p<.05; *p<0.10

Notes: All regressions include the control variables included in column (4) of Table 1 (for the list of variables, see notes to Table 1). All variables included on the right-hand-side of the regression include both the "level" and an interaction with "Black." Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Table 4. TWFE Estimates of the Effect of RMLs on the Race-Specific Differential in Non-Marijuana Drug Arrest Rate, by Possession or Sales Arrest

	(1)	(2)	(3)
	All Arrests	Possession Arrests	Sales Arrests
		Panel I: Cocaine-Heroin Arrests	
RML	2.20	1.74	0.47
	(8.23)	(7.77)	(2.30)
RML*Black	112.77	68.59	44.18
	(84.38)	(58.47)	(34.72)
Pre-Treat Mean (White)	187.59	162.35	25.25
Pre-Treat Mean (Black)	729.01	476.78	252.23
N	1,996	1,996	1,996
		Panel II: Addicting Synthetic Narcotics Arrests	
RML	-24.29***	-19.77***	-4.51*
	(8.74)	(6.42)	(2.58)
RML*Black	4.18	3.64	0.54
	(4.77)	(3.62)	(1.84)
Pre-Treat Mean (White)	18.07	14.21	3.86
Pre-Treat Mean (Black)	19.99	14.79	5.20
N	1,996	1,996	1,996
		Panel III: Dangerous Non-Narcotic Arrests	
RML	-11.72	-5.15	-6.57***
	(11.12)	(10.49)	(2.01)
RML*Black	-18.50	-4.54	-13.97

	(42.08)	(36.47)	(9.70)
Pre-Treat Mean (White)	243.22	207.44	35.78
Pre-Treat Mean (Black)	386.90	328.90	58.00
N	1,996	1,996	1,996

***p < .01; **p<.05; *p<0.10

Notes: All regressions include the control variables included in column (4) of Table 1 (for the list of variables, see notes to Table 1). All variables included on the right-hand-side of the regression include both the "level" and an interaction with "Black." Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Table 5A. Racial Disparities in Effects of RMLs on Property Crime, by Offense Type

	(1)	(2)	(3)	(4)
	All Property Offenses	Larceny	Burglary	Motor Vehicle Theft
RML	5.87	1.14	2.38	1.73
	(20.97)	(16.87)	(3.59)	(3.53)
RML*Black	24.80	-5.42	12.11	17.27
	(64.41)	(50.94)	(7.60)	(15.47)
Pre-Treat Mean (White)	416.85	273.45	98.78	41.98
Pre-Treat Mean (Black)	1101.27	686.78	287.30	121.02
N	1,996	1,996	1,996	1,996

***p < .01; **p<.05; *p<0.10

Notes: All regressions include the control variables included in column (4) of Table 1 (for the list of variables, see notes to Table 1). All variables included on the right-hand-side of the regression include both the "level" and an interaction with "Black." Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Table 5B. Racial Disparities in Effects of RMLs on Violent Crime, by Offense Type

	(1)	(2)	(3)	(4)	(5)
	All Violent Offenses	Assault	Robbery	Homicide	Rape
RML	6.56	5.80	2.05	0.14	-1.42**
	(6.51)	(6.09)	(1.36)	(0.18)	(0.60)
RML*Black	31.96	22.55	9.54	0.87	-0.99
	(45.30)	(37.22)	(11.67)	(1.62)	(1.68)
Pre-Treat Mean (White)	244.71	205.93	27.83	3.56	7.39
Pre-Treat Mean (Black)	898.66	663.28	192.37	19.63	23.38
N	1,996	1,996	1,996	1,996	1,996

***p < .01; **p<.05; *p<0.10

Notes: All regressions include the control variables included in column (4) of Table 1 (for the list of variables, see notes to Table 1). All variables included on the right-hand-side of the regression include both the "level" and an interaction with "Black." Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Table 6. Racial Disparities in Effects of RMLs on Drinking and Delinquency-Related Part II Offenses

	(1)	(2)	(3)	(4)
	Vandalism	Liquor Law Violations	DUI	Drunkenness/Disorderly Conduct
RML	-0.94	-28.20	-23.96	42.16
	(3.41)	(21.60)	(35.18)	(28.98)
RML*Black	17.29	107.49	-22.09	71.78
	(14.71)	(108.74)	(33.20)	(78.11)
Pre-Treat Mean (White)	60.70	148.77	653.38	291.11
Pre-Treat Mean (Black)	111.17	178.12	591.38	412.81
N	1,996	1,996	1,996	1,996

***p < .01; **p<.05; *p<0.10

Notes: All regressions include the control variables included in column (4) of Table 1 (for the list of variables, see notes to Table 1). All variables included on the right-hand-side of the regression include both the "level" and an interaction with "Black." Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Table 7. Robustness of Arrest Findings to Spillovers from Border State RMLs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Marijuana Arrests		Non-Marijuana Drug Arrests		Property Crime Arrests		Violent Crime Arrests	
	White	Black	White	Black	White	Black	White	Black
				Panel I: Baseline				
RML	-115.23***	-325.15***	-33.80***	64.65	5.87	30.67	6.56	38.52
	(17.52)	(103.67)	(12.16)	(101.57)	(20.97)	(73.26)	(6.51)	(50.51)
Pre-Treat Mean	158.32	492.25	448.88	1135.90	416.85	1101.27	244.71	898.66
N	998	998	998	998	998	998	998	998
				Panel II: Controlling for Bordering an RML State				
RML	-114.36***	-325.40***	-32.16***	65.00	6.22	31.72	6.10	39.44
	(16.73)	(102.51)	(11.56)	(102.61)	(20.69)	(77.69)	(6.77)	(52.74)
Border State RML	-12.98	-18.21	-24.63*	25.64	-5.32	77.55	6.92	67.88*
	(20.30)	(103.27)	(13.05)	(87.18)	(16.17)	(68.22)	(5.94)	(40.38)
Pre-treat DV mean	158.32	492.25	448.88	1135.90	416.85	1101.27	244.71	898.66

N	998	998	998	998	998	998	998	998
			Panel III: Dropping Non-RML States that Border an RML State					
RML	-116.29***	-399.46***	-50.03***	-82.99	-5.44	85.68	4.89	55.13
	(18.25)	(110.61)	(18.06)	(86.98)	(25.05)	(107.18)	(6.47)	(40.59)
Pre-Treat Mean	158.32	492.25	448.88	1135.90	416.85	1101.27	244.71	898.66
N	659	659	659	659	659	659	659	659

***p < .01; **p<.05; *p<0.10

Notes: All regressions include the control variables included in column (4) of Table 1 (for the list of variables, see notes to Table 1). Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Table 8. Exploration of Heterogeneity in the Effects of RMLs on Arrests by Whether a Recreational Dispensary is Allowed

	(1)	(2)	(3)	(4)
	Marijuana Arrests	Non-Marijuana Drug Arrest	Property Crime Arrests	Violent Crime Arrests
RML without Dispensaries	-133.53***	-52.50***	-4.67	-1.99
	(18.96)	(12.15)	(14.89)	(7.39)
RML without Dispensaries*Black	-278.83***	54.75	56.23	16.02
	(81.74)	(95.22)	(57.39)	(55.43)
RML with Dispensaries	-105.83***	-23.97*	11.42	11.11*
	(18.27)	(13.53)	(27.30)	(5.94)
RML with Dispensaries*Black	-167.78	126.69	6.74	42.22
	(111.32)	(105.35)	(71.16)	(42.40)
Pre-Treat Mean (White)	158.32	448.88	416.85	244.71
Pre-Treat Mean (Black)	492.25	1135.90	1101.27	898.66
N	1,996	1,996	1,996	1,996

***p < .01; **p<.05; *p<0.10

Notes: All regressions include the control variables included in column (4) of Table 1 (for the list of variables, see notes to Table 1). All variables included on the right-hand-side of the regression include both the "level" and an interaction with "Black." Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Table 9. TWFE Estimates of Effect of RMLs on Psychiatric Health, by Race, NSDUH

	(1)	(2)	(3)	(4)
		Panel I: Mental Illness		
RML	0.00886***	0.0102***	0.0103***	0.0108***
	(0.00201)	(0.00242)	(0.0037)	(0.00282)
Pre-Treat Mean	0.180	0.180	0.180	0.180
N	612	612	612	612
		Panel II: Major Depressive Episode		
RML	0.00266**	0.00410***	0.00440***	0.00384***
	(0.000953)	(0.00114)	(0.00116)	(0.00134)
Pre-Treat Mean	0.066	0.066	0.066	0.066
N	612	612	612	612
		Panel III: Suicidal Thoughts		
RML	0.000950	0.00170*	0.00191*	0.00208*
	(0.000645)	(0.00778)	(0.000788)	(0.000913)
	612	612	612	612
Pre-Treat Mean	0.039	0.039	0.039	0.039
N	612	612	612	612
Controls				
State and Year FE?	Yes	Yes	Yes	Yes
MML and MDL?	Yes	Yes	Yes	Yes
Socioeconomic, Demographic, Policing	No	Yes	Yes	Yes
Substance Use Policy?	No	No	Yes	Yes
Social Welfare Policy Environment?	No	No	Yes	Yes

***p < .01; **p<.05; *p<0.10

Notes: All regressions include state fixed effects and year fixed effects. "MML & MDL" refer to controls for the presence of a medical marijuana law and the presence of a marijuana decriminalization law. "Sociodemographic, Demographic, and Policy" refer to controls for the state unemployment rate, per capita personal income (logged), the proportion of state population that is Black, and the proportion of the state population that is Hispanic. "Substance Use Policy" refers to controls for beer tax per gallon (logged), the presence of alcohol- and drug-specific Good Samaritan laws, the presence of a naloxone access law, and the presence of a must-access prescription drug monitoring program. "Social Welfare Policy Environment" refers to controls for the state EITC refundable rate, the higher of the state of Federal minimum wage (logged), an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, and whether the governor is a Democrat. Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Table 10. TWFE Estimates of the Effect of RMLs on Poor Mental Health Days, by Race and Ethnicity, BRFSS

	(1)	(2)	(3)
	Any Poor Mental Health Days	>= 15 Poor Mental Health Days	Number of Poor Mental Health Days
		Panel I: Non-Hispanic Whites	
RML	0.0532	-0.00729	-0.0177
	(0.0392)	(0.00606)	(0.0471)
Pre-Treat Mean	0.308	0.095	3.259
N	5,834,973	5,834,973	5,834,973
		Panel II: Blacks and Hispanics	
RML	-0.0034	-0.0002	-0.0780
	(0.0121)	(0.0039)	(0.1124)
Pre-Treat Mean	0.328	0.113	3.814
N	1,077,137	1,077,137	1,077,137
		Panel III: Blacks	
RML	-0.0197	-0.00514	-0.205
	(0.0118)	(0.00775)	(0.183)
Pre-Treat Mean	0.331	0.117	3.921
N	586,710	586,710	586,710
		Panel IV: Hispanics	
RML	0.00177	-0.00140	-0.106
	(0.0129)	(0.00474)	(0.150)
Pre-Treat Mean	0.336	0.113	3.812
N	490,427	490,427	490,427

***p < .01 **p<.05 *p<0.10

Notes: All regressions include state fixed effects, year fixed effects, month fixed effects and month by year fixed effects. Furthermore, regressions include controls for a medical marijuana law, a marijuana decriminalization law the state unemployment rate, per capita personal income (logged), the proportion of state population that is Black, the proportion of the state population that is Hispanic, law enforcement personnel per 1,000 population (logged), beer tax per gallon (logged), alcohol- and drug-specific Good Samaritan laws, a naloxone access law, a must-access prescription drug monitoring program, the state EITC refundable rate, the higher of the state of Federal minimum wage (logged), an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, and whether the governor is a Democrat. Regressions are weighted using BRFSS sample weights and standard errors are clustered at the state level.

Table 11. TWFE Estimates of the Effect of RMLs on Poor Mental Health Days, by Race and Ethnicity, Ages 18-to-20, BRFSS

	(1)	(2)	(3)
	Any Poor Mental Health Days	>= 15 Poor Mental Health Days	Number of Poor Mental Health Days
		Panel I: Non-Hispanic Whites	
RML	0.0230	0.0283***	0.800***
	(0.0204)	(0.0100)	(0.209)
Pre-Treat Mean	0.505	0.126	4.626
N	95,664	95,664	95,664
		Panel II: Blacks and Hispanics	
RML	0.0298	0.0275***	0.409*
	(0.0232)	(0.006)	(0.216)
Pre-Treat Mean	0.427	0.104	3.994
N	37,972	37,972	37,972
		Panel II: Blacks	
RML	-0.0218	0.0358	0.132
	(0.0342)	(0.0305)	(0.580)
Pre-Treat Mean	0.409	0.098	3.821
N	15,648	15,648	15,648
		Panel III: Hispanics	
RML	0.0469**	0.0202	0.448
	(0.0232)	(0.0206)	(0.353)
Pre-Treat Mean	0.436	0.108	4.094
N	22,324	22,324	22,324

***p < .01 **p<.05 *p<0.10

Notes: All regressions include state fixed effects, year fixed effects, month fixed effects and month by year fixed effects. Furthermore, regressions include controls for a medical marijuana law, a marijuana decriminalization law the state unemployment rate, per capita personal income (logged), the proportion of state population that is Black, the proportion of the state population that is Hispanic, law enforcement personnel per 1,000 population (logged), beer tax per gallon (logged), alcohol- and drug-specific Good Samaritan laws, a naloxone access law, a must-access prescription drug monitoring program, the state EITC refundable rate, the higher of the state or Federal minimum wage (logged), an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, and whether the governor is a Democrat. Regressions are weighted using BRFSS sample weights and standard errors are clustered at the state level.

Table 12. TWFE Estimates of the Effect of RMLs on Poor Mental Health Days, by Race and Ethnicity, Ages 21-and-Older, BRFSS

	(1)	(2)	(3)
	Any Poor Mental Health Days	>= 15 Poor Mental Health Days	Number of Poor Mental Health Days
		Panel I: Non-Hispanic Whites	
RML	0.0547	-0.00793	-0.0305
	(0.0401)	(0.00595)	(0.0436)
Pre-Treat Mean	0.305	0.094	3.237
N	5,739,309	5,739,309	5,739,309
		Panel II: Blacks and Hispanics	
RML	-0.0054	-0.0021	-0.112
	(0.0121)	(0.0041)	(0.121)
Pre-Treat Mean	0.325	0.113	3.808
N	1,039,165	1,039,165	1,039,165
		Panel III: Blacks	
RML	-0.0200*	-0.00681	-0.217
	(0.0115)	(0.00791)	(0.191)
Pre-Treat Mean	0.329	0.118	3.924
N	571,062	571,062	571,062
		Panel IV: Hispanics	
RML	-0.000699	-0.00295	-0.142
	(0.0133)	(0.00481)	(0.155)
Pre-Treat Mean	0.331	0.113	3.799
N	468,103	468,103	468,103

***p < .01 **p<.05 *p<0.10

Notes: All regressions include state fixed effects, year fixed effects, month fixed effects and month by year fixed effects. Furthermore, regressions include controls for a medical marijuana law, a marijuana decriminalization law the state unemployment rate, per capita personal income (logged), the proportion of state population that is Black, the proportion of the state population that is Hispanic, law enforcement personnel per 1,000 population (logged), beer tax per gallon (logged), alcohol- and drug-specific Good Samaritan laws, a naloxone access law, a must-access prescription drug monitoring program, the state EITC refundable rate, the higher of the state or Federal minimum wage (logged), an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, and whether the governor is a Democrat. Regressions are weighted using BRFSS sample weights and standard errors are clustered at the state level.

Table 13. TWFE Estimates of Effect of RMLs on Suicides and Drug-Related Mortality, by Race/Ethnicity, Ages 18-and-Older, NVSS

	(1)	(2)	(3)	(4)
	Suicides	Drug-Related Suicides	Drug Overdose Deaths	Opioid Overdose Deaths
		Panel I: Non-Hispanic Whites		
RML	0.218	-0.073	-2.531	-4.201*
	(0.366)	(0.104)	(2.269)	(2.441)
Pre-Treat Mean	20.139	2.577	15.420	11.320
N	1,020	1,020	1,020	1,020
		Panel II: Blacks and Hispanics		
RML	0.682**	0.016	0.587	-1.649
	(0.319)	(0.078)	(2.428)	(2.237)
Pre-Treat Mean	9.391	0.846	12.093	5.262
N	1,020	1,020	1,020	1,020

***p < .01 **p<.05 *p<0.10

Notes: All regressions include state fixed effects, and year fixed effects. Furthermore, regressions include controls for a medical marijuana law, a marijuana decriminalization law the state unemployment rate, per capita personal income (logged), the proportion of state population that is Black, the proportion of the state population that is Hispanic, law enforcement personnel per 1,000 population (logged), beer tax per gallon (logged), alcohol- and drug-specific Good Samaritan laws, a naloxone access law, a must-access prescription drug monitoring program, the state EITC refundable rate, the higher of the state of Federal minimum wage (logged), an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, and whether the governor is a Democrat. Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Table 14. Exploring Heterogeneity in Mental Health and Mortality Effects of RMLs by Whether Recreational Dispensary Open, BRFSS and NVSS

	(1)	(2)	(3)	(4)	(5)
	Any Poor Mental Health Days	Suicides	Drug-Related Suicides	Drug Overdose Deaths	Opioid Overdose Deaths
			Panel I: Non-Hispanic White		
RML with Dispensary	-1.165	0.270	-0.196**	-3.351	-5.754**
	(3.246)	(0.393)	(0.093)	(2.195)	(2.235)
RML without Dispensary	0.086	0.327	0.047	-0.429	-1.364
	(0.060)	(0.616)	(0.215)	(2.165)	(2.192)
Pre-Treat Mean (White)	0.308	20.139	2.577	15.420	11.320
N	5,834,973	1,020	1,020	1,020	1,020
			Panel II: Black and Hispanic		
RML with Dispensary	1.425	0.871*	0.017	0.107	-2.867
	(2.320)	(0.504)	(0.090)	(2.683)	(2.308)
RML without Dispensary	0.016	0.280	-0.017	0.705	-0.482
	(0.012)	(0.205)	(0.063)	(2.219)	(1.897)
Pre-Treat Mean	0.333	9.391	0.846	12.093	5.262
N	1,077,137	1,020	1,020	1,020	1,020

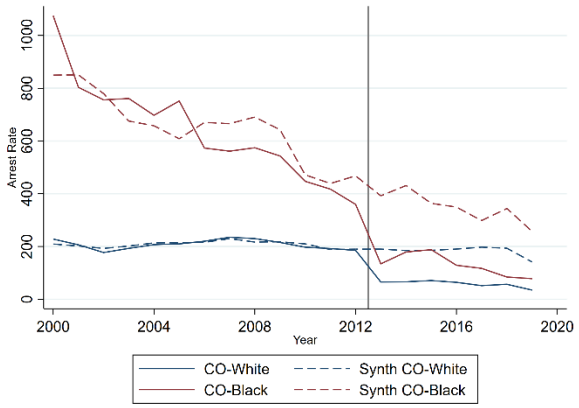
***p < .01 **p<.05 *p<0.13

Notes: All regressions include state fixed effects, and year fixed effects. Furthermore, regressions include controls for a medical marijuana law, a marijuana decriminalization law the state unemployment rate, per capita personal income (logged), the proportion of state population that is Black, the proportion of the state population that is Hispanic, law enforcement personnel per 1,000 population (logged), beer tax per gallon (logged), alcohol- and drug-specific Good Samaritan laws, a naloxone access law, a must-access prescription drug monitoring program, the state EITC refundable rate, the higher of the state of Federal minimum wage (logged), an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, and whether the governor is a Democrat. Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Appendix Figure 1. Synthetic Control Estimates, Marijuana Arrest Rate

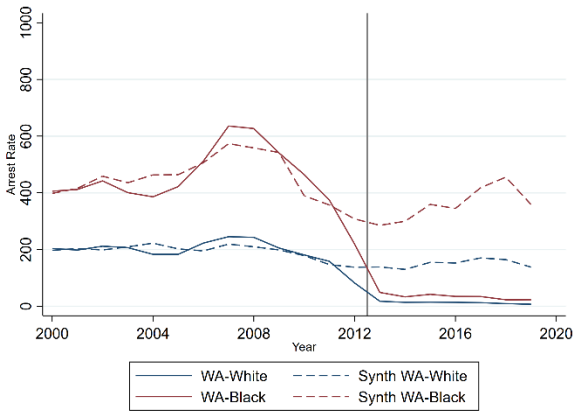
Colorado

Black RML effect: -218.09 (p-value = 0.667)
White RML effect: -124.74 (p-value = 0.286)



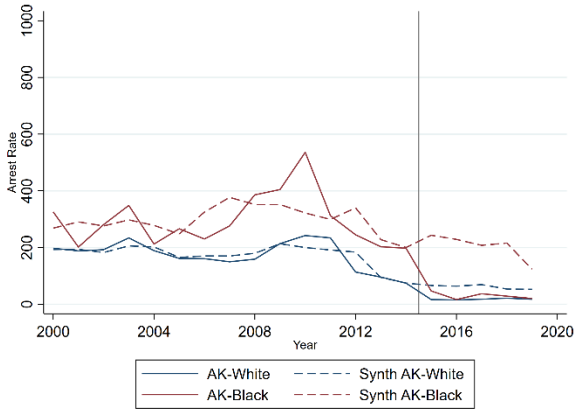
Washington

Black RML effect: -326.19 (p-value = 0.286)
White RML effect: -137.23 (p-value = 0.524)



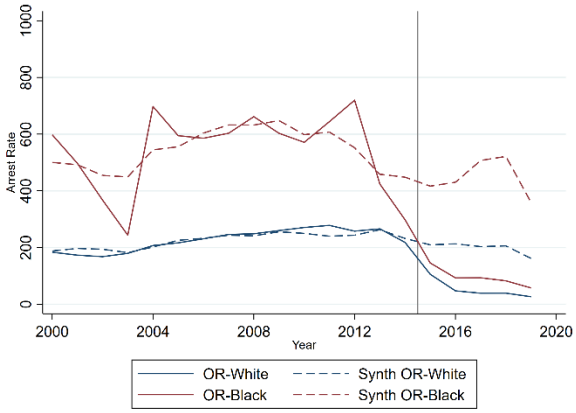
Alaska

Black RML effect: -174.01 (p-value = 0.524)
White RML effect: -43.48 (p-value = 0.524)



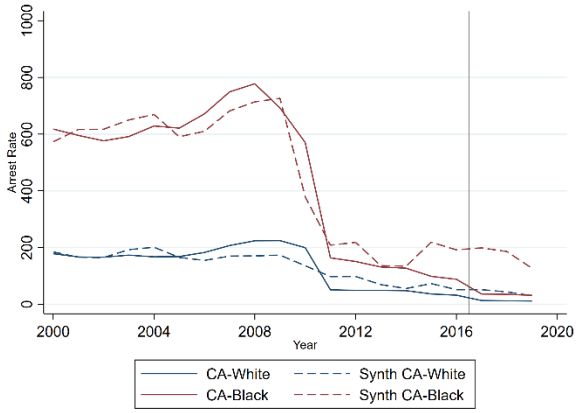
Oregon

Black RML effect: -352.55 (p-value = 0.429)
White RML effect: -147.23 (p-value = 0.048)



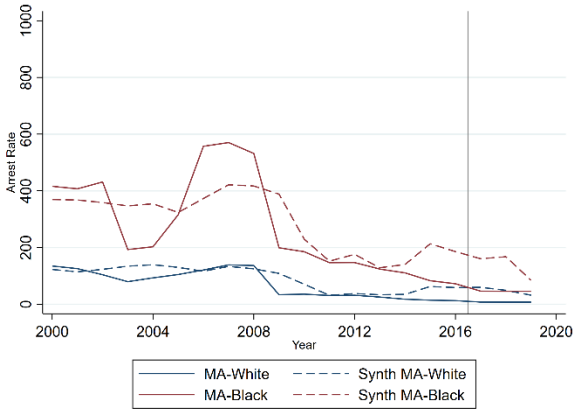
California

Black RML effect: -136.48 (p-value = 0.542)
White RML effect: -29.51 (p-value = 0.708)



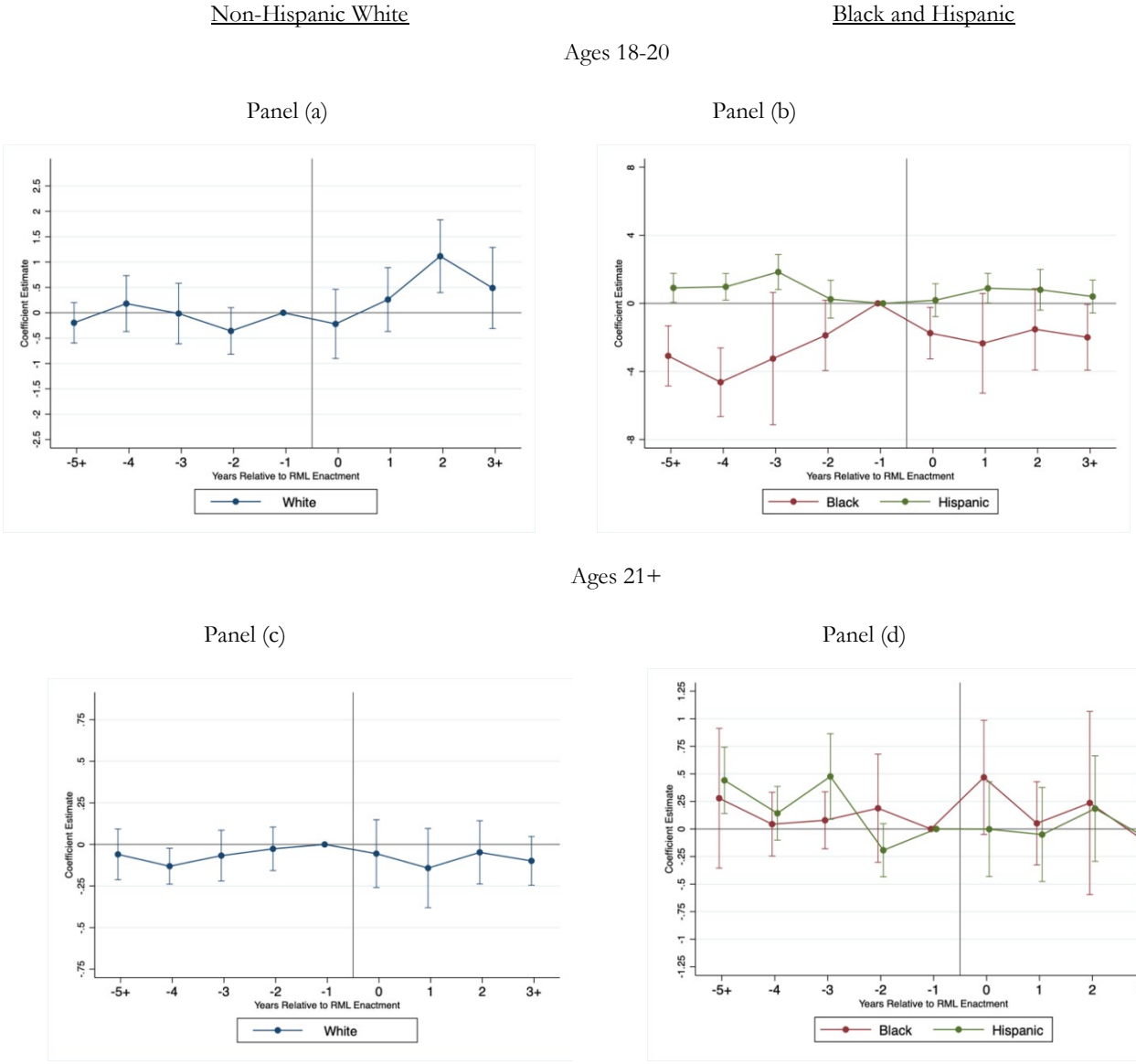
Massachusetts

Black RML effect: -91.75 (p-value = 0.792)
White RML effect: -39.55 (p-value = 0.667)



Notes: Synthetic units for each treated unit are constructed by matching on all pre-treatment year observations of the outcome and inference is performed via the permutation test described in Abadie et al. (2010). States that ever adopt an RML during the sample period and/or states that adopt an MML within six years of the treated unit's RML adoption are dropped from the donor pool.

Appendix Figure 2. Event-Study Analyses of RMLs and Age- and Race-Specific Number of Poor Mental Health Days, by Age, BRFSS, Using TWFE Estimates



All regressions include the control variables listed in listed in the notes of Table 1. Regressions are weighted using BRFSS sample weights and standard errors are clustered at the state. Error bars are 95 percent confidence intervals.

IX. APPENDIX

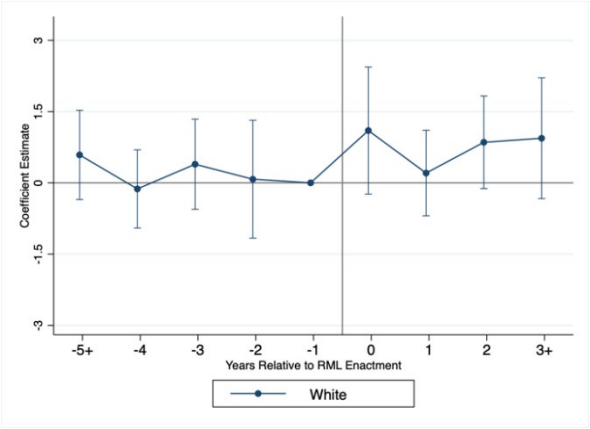
To request a fully accessible version of this section, please email depc@osu.edu.

Appendix Figure 3. Event-Study Analyses of RMLs and Race-Specific Mortality Outcomes, Using Two-Way Fixed Effects Estimates

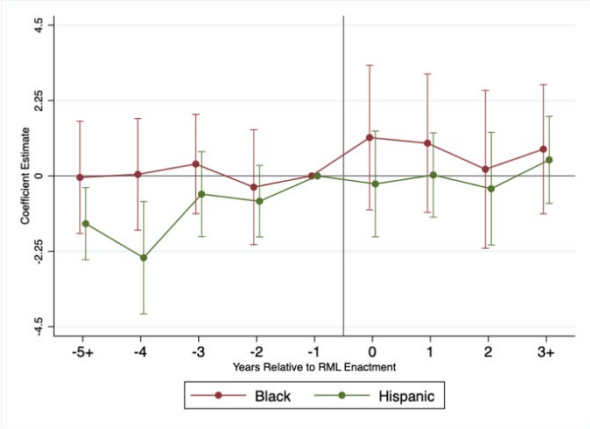
Non-Hispanic White | Black and Hispanic

All Suicide

Panel (a)

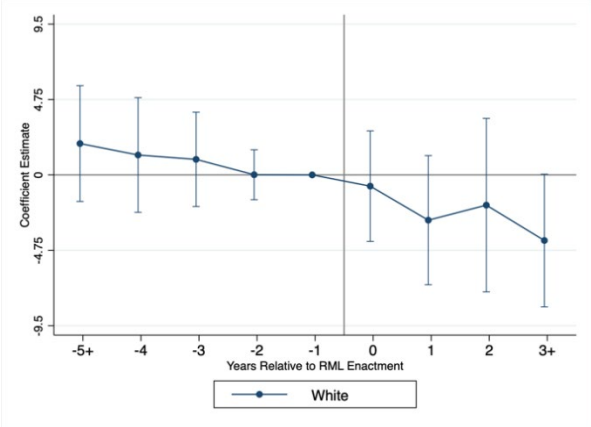


Panel (b)

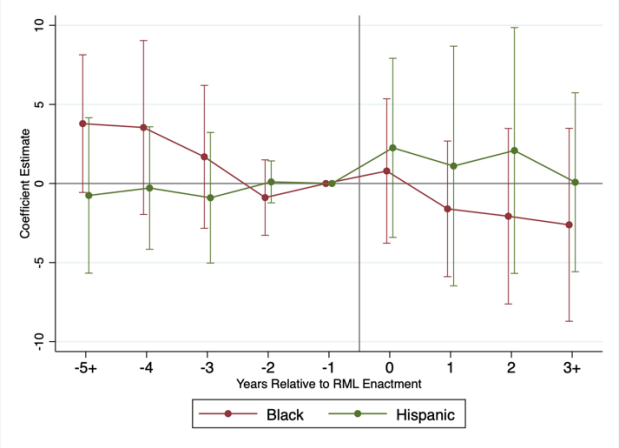


Opioid Overdose

Panel (c)



Panel (d)



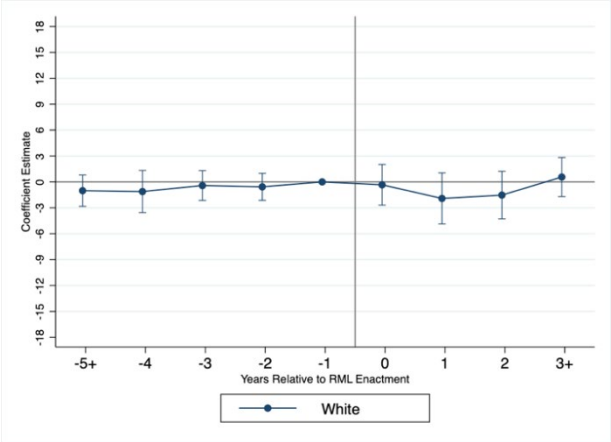
All regressions include the control variables listed in listed in the notes of Table 1. Regressions are weighted using the adult state population and standard errors are clustered at the state level. Error bars are 95 percent confidence intervals.

Appendix Figure 4. Event-Study Analyses of RMLs and Race-Specific Mortality Outcomes Ages 18-to-20, Using Two-Way Fixed Effects Estimates

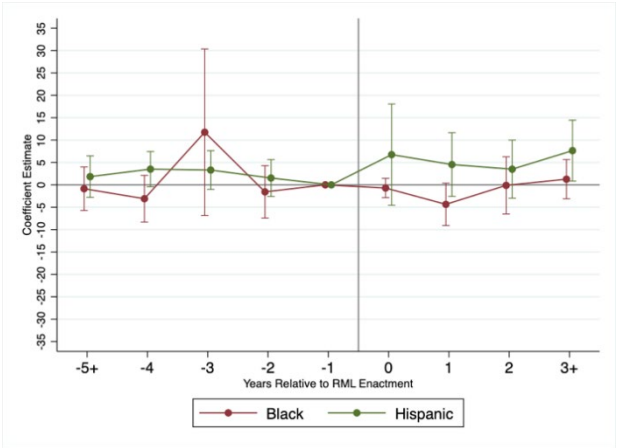
Non-Hispanic White | Black and Hispanic

All Suicide

Panel (a)

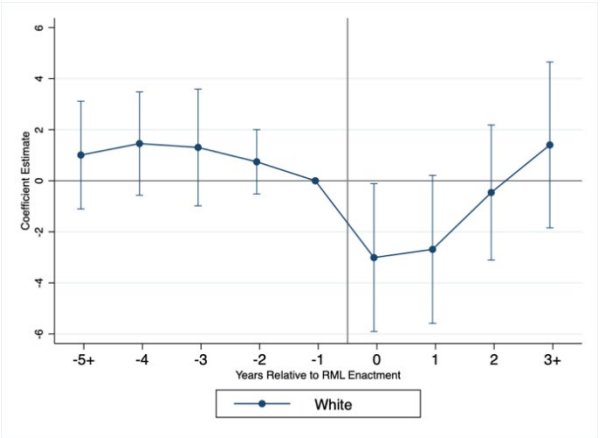


Panel (b)

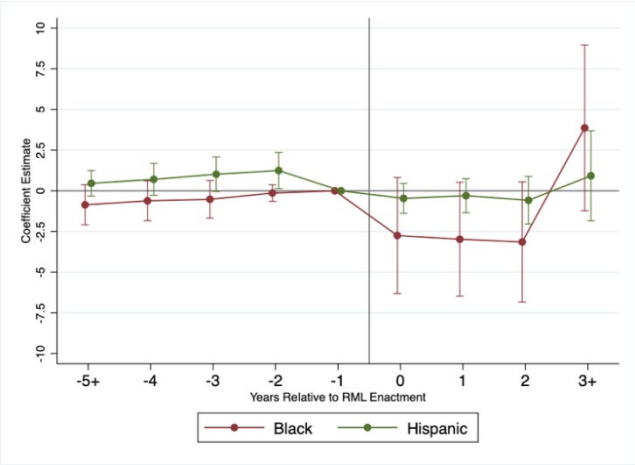


Opioid Overdose

Panel (c)



Panel (d)



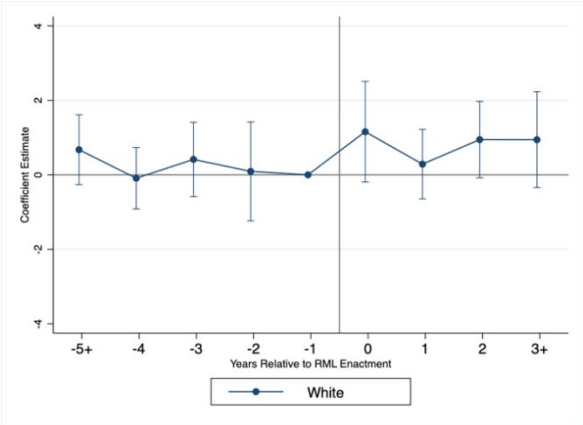
All regressions include the control variables listed in the notes of Table 1. Regressions are weighted using BRFSS sample weights and standard errors are clustered at the state. Error bars are 95 percent confidence intervals.

Appendix Figure 5. Event-Study Analyses of RMLs and Race-Specific Mortality Outcomes Ages 21-and-Older, Using Two-Way Fixed Effects Estimates

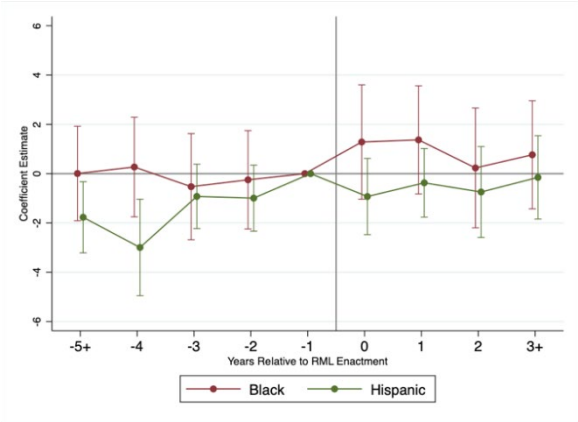
Non-Hispanic White | Black and Hispanic

All Suicide

Panel (a)

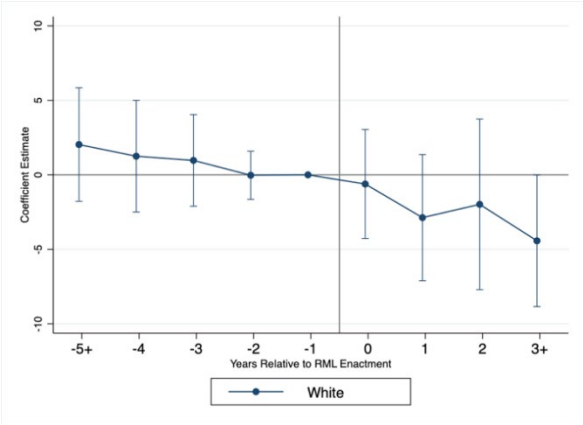


Panel (b)

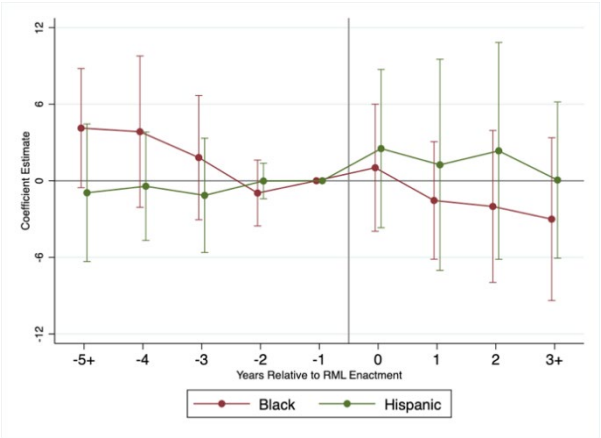


Opioid Overdose

Panel (c)



Panel (d)



All regressions include the control variables listed in the notes of Table 1. Regressions are weighted using BRFSS sample weights and standard errors are clustered at the state. Error bars are 95 percent confidence intervals.

Appendix Table 1. Descriptive Statistics

	Mean	(SD)
Data Source I: Uniform Crime Reports, 2000-2019		
Drug Arrests (per 100,000 population)		
Adult Marijuana Arrests		
All	230.7	(137.3)
Whites	182.1	(111.0)
Blacks	623.1	(336.9)
Adult Marijuana Possession Arrests		
All	204.3	(129.1)
Whites	163.0	(105.0)
Blacks	541.0	(302.2)
Cocaine/Heroin Possession Arrests		
All	112.9	(93.7)
Whites	89.0	(90.5)
Blacks	315.3	(268.8)
Addicting Synthetic Narcotic Possession Arrests		
All	21.9	(23.8)
Whites	22.6	(24.9)
Blacks	25.2	(28.5)
Dangerous Non-Narcotic Possession Arrests		
All	102.9	(117.1)
Whites	102.4	(123.3)
Blacks	133.6	(178.6)

Adult Marijuana Sales Arrests		
All	26.4	(19.3)
Whites	19.1	(16.1)
Blacks	82.0	(66.2)
Cocaine/Heroin Sales Arrests		
All	39.2	(37.5)
Whites	20.1	(16.7)
Blacks	179.6	(189.2)
Addicting Synthetic Narcotic Sales Arrests		
All	9.1	(12.7)
Whites	9.1	(13.0)
Blacks	12.7	(18.5)
Dangerous Non-Narcotic Sales Arrests		
All	20.9	(20.1)
Whites	20.2	(19.5)
Blacks	31.0	(56.6)
Part I Offense Arrests (per 100,000 population)		
Property Crime Arrests		
All	451.8	(168.7)
Whites	382.0	(149.1)
Blacks	1032.7	(460.1)
Larceny Arrests		
All	344.2	(148.0)

Whites	291.2	(125.5)
Blacks	779.6	(371.3)
Burglary Arrests		
All	75.9	(40.4)
Whites	64.9	(37.3)
Blacks	173.8	(107.0)
Motor Vehicle Theft Arrests		
All	29.0	(20.2)
Whites	23.3	(20.3)
Blacks	73.8	(68.8)
Violent Crime Arrests		
All	181.4	(99.2)
Whites	135.3	(99.8)
Blacks	531.8	(308.2)
Assault Arrests		
All	138.8	(86.9)
Whites	109.3	(88.5)
Blacks	367.2	(234.2)
Robbery Arrests		
All	30.9	(14.3)
Whites	17.5	(11.0)
Blacks	128.7	(72.8)
Homicide Arrests		
All	4.3	(2.3)

Whites	2.5	(1.5)
Blacks	17.3	(9.9)
Rape Arrests		
All	7.4	(3.1)
Whites	6.0	(2.7)
Blacks	18.6	(12.3)
Selected Part II Offense Arrests (per 100,000 population)		
Vandalism Arrests		
All	62.5	(37.9)
Whites	54.6	(33.3)
Blacks	130.3	(102.9)
Liquor Law Violation Arrests		
All	148.8	(183.8)
Whites	145.9	(161.9)
Blacks	183.0	(349.2)
Drunkenness Arrests		
All	184.7	(240.8)
Whites	184.1	(232.1)
Blacks	220.9	(365.9)
Disorderly Conduct Arrests		
All	160.6	(156.1)
Whites	128.8	(128.5)
Blacks	403.0	(425.0)
DUI Arrests		

All	467.1	(223.8)
Whites	491.4	(238.0)
Blacks	418.8	(240.9)

Data Source II: National Survey on Drug Use and Health, 2008-2019		
Any Mental Illness Ages 18-and-older	0.1928	0.0206
Any Mental Illness Ages 18-to-25	0.2260	0.0470
Any Mental Illness Ages 26-and-older	0.1875	0.0198
Serious Mental Illness Ages 18-and-older	0.0452	0.0079
Serious Mental Illness Ages 18-to-25	0.0575	0.0222
Serious Mental Illness Ages 26-and-older	0.0433	0.0069
Suicidal Thoughts Ages 18-and-older	0.0429	0.0063

Data Source III: Behavioral Risk Factor Surveillance Survey		
Mental Health Outcomes		
Number of Poor Mental Health Days		
All	3.393	(7.698)
Whites	3.259	(7.542)
Blacks	3.921	(8.264)
Hispanics	3.812	(8.081)
Probability of Any Poor Mental Health Days		
All	0.314	(0.464)
Whites	0.308	(0.462)
Blacks	0.331	(0.471)

Hispanics	0.336	(0.472)
Data Source IV: National Vital Statistics Multiple Cause of Death Mortality Files		
Mortality (per 100,000 population)		
Suicide Rate		
All	12.809	(12.059)
White	20.139	(5.756)
Black	10.066	(16.427)
Hispanic	8.733	(8.444)
Drug Related Suicide		
All	1.404	(2.750)
White	2.577	(1.115)
Black	0.888	(2.030)
Hispanic	0.769	(1.289)
Non-Drug Related Suicide		
All	11.439	(11.434)
White	17.562	(5.104)
Black	9.177	(16.382)
Hispanic	7.961	(8.112)
Drug Overdose		
All	11.603	(12.018)
White	15.420	(10.058)
Black	15.079	(15.013)
Hispanic	8.644	(10.282)

Opioid Overdose		
All	5.313	(8.615)
White	11.320	(9.267)
Black	5.473	(10.079)
Hispanic	3.436	(6.490)

Appendix Table 2. Recreational Marijuana Law Enactment Dates and Dates Recreational Sales of Marijuana Legalized, 2000-2019

State	RML Effective Date	Open Recreational Dispensary Permitted
Alaska	2/24/2015	10/29/2016
California	11/9/2016	1/1/2018
Colorado	12/10/2012	1/1/2014
Washington D. C.	2/26/2015	2/26/2015
Maine	1/31/2017	10/9/2020
Massachusetts	12/15/2016	11/20/2018
Michigan	12/6/2018	12/1/2019
Nevada	1/1/2017	7/1/2017
Oregon	7/1/2015	10/1/2015
Vermont	7/1/2018	10/1/2022
Washington	12/6/2012	7/8/2014

Notes: Policy dates sourced from Anderson and Rees (Forthcoming) and the Marijuana Policy Project.

Appendix Table 3. Sensitivity of Arrest Estimates to Inclusion of State-Specific Linear Time Trends and Census Region-Year Fixed Effects

	(1)	(2)	(3)	(4)
	Marijuana Arrest Rate		**Non-Marijuana Drug Arrest Rate**	
RML	-101.16***	-92.84***	-36.77*	-46.29***
	(23.65)	(20.07)	(18.50)	(16.67)
RML*Black	-278.70**	-301.21***	-95.49	-141.72**
	(113.50)	(95.64)	(57.41)	(53.16)
Pre-Treat Mean (White)	158.32	158.32	448.88	448.88
Pre-Treat Mean (Black)	492.25	492.25	1135.90	1135.90
N	1,996	1,996	1,996	1,996
State-specific linear time trends	Yes	Yes	Yes	Yes
Census region-year FE	No	Yes	No	Yes

***p < .01; **p<.05; *p<0.10

Notes: All regressions include the control variables included in column (4) of Table 1 (for the list of variables, see notes to Table 1). All variables included on the right-hand-side of the regression include both the "level" and an interaction with "Black." Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Appendix Table 4. Sensitivity of Estimated Arrest Effects to UCR Data Quality Checks and Using Unweighted Estimates

	(1)	(2)	(3)	(4)
	Marijuana Arrests	Non-Marijuana Drug Arrests	Property Crime Arrests	Violent Crime Arrests
		Panel I: Agency-Level Analysis for Large Cities[a]		
RML	-76.03***	-29.07	-25.35	4.48
	(13.75)	(19.02)	(34.58)	(12.07)
RML*Black	-204.39**	230.19	-51.44	-10.83
	(87.89)	(199.68)	(81.20)	(91.02)
Pre-Treat Mean (White)	139.46	526.08	499.80	299.06
Pre-Treat Mean (Black)	570.55	1480.54	1347.71	1183.68
N	28,026	28,026	28,026	28,026
		Panel II: Poisson Estimates Using Large Cities[b]		
RML	-1.520***	-0.139**	-0.009	0.020
	(0.320)	(0.056)	(0.057)	(0.058)
RML*Black	-0.295	0.097	-0.019	-0.008
	(0.315)	(0.074)	(0.033)	(0.050)
N	27,986	27,986	28,026	28,006
		Panel III: Unweighted Estimates[c]		
RML	-120.53***	-48.02**	-7.99	0.24
	(22.62)	(18.77)	(26.06)	(7.64)

RML*Black	-203.91**	-164.36	-75.91	33.67
	(100.06)	(118.15)	(77.77)	(37.31)
Pre-Treat Mean (White)	164.09	220.45	405.54	141.40
Pre-Treat Mean (Black)	528.77	987.39	1343.11	736.19
N	1,996	1,996	1,996	1,996

***p < .01; **p<.05; *p<0.10

Notes: All regressions include the control variables included in column (4) of Table 1 (for the list of variables, see notes to Table 1). All variables included on the right-hand-side of the regression include both the "level" and an interaction with "Black." Standard errors are clustered at the state level.

[a] Regressions are estimated at the agency-year level and weighted using the estimated agency-specific adult population. The sample is restricted to agencies that have a population of 50,000 or greater at any point during the sample period (and never less than 25,000) and report arrests at least six months during the year (or only report arrests in December).

[b] Regressions are estimated via Poisson at the agency-year level (using the sample described in [a]) with the count of arrests as the dependent variable. The estimated agency-specific adult population is used as the exposure variable.

[c] Regressions are estimated at the state-year level and unweighted.

Appendix Table 5. Sensitivity of Estimated Drug Arrest Effects to Use of the Drug Arrest Ratio as the Dependent Variable

	(1)	(2)	(3)	(4)
	Marijuana Arrests	Cocaine-Heroin Arrests	Addicting Synthetic Narcotics Arrests	Dangerous Non-Narcotic Arrests
			Panel I: White	
RML	-0.0451***	-0.0025	-0.0073***	-0.0085**
	(0.0076)	(0.0024)	(0.0024)	(0.0035)
Pre-Treat Mean	0.0400	0.0444	0.0046	0.0541
N	998	998	998	998
			Panel II: Black	
RML	-0.0632***	0.0029	-0.0022*	-0.0084*
	(0.0119)	(0.0052)	(0.0012)	(0.0048)
Pre-Treat Mean	0.0490	0.0694	0.0019	0.0355
N	998	998	998	998

***p < .01; **p<.05; *p<0.10

Notes: All regressions include the control variables included in column (4) of Table 1 (for the list of variables, see notes to Table 1). Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Appendix Table 6. Racial/Ethnic Disparities in the Effects of RMLs on Poor Mental Health Days, BRFSS

	(1)	(2)	(3)
	Any Poor Mental Health Days	>= 15 Poor Mental Health Days	Continuous Number of Days
RML	0.0532	-0.00729	-0.0177
	(0.0392)	(0.00606)	(0.0471)
RML*Black	-0.0730*	0.00215	-0.187
	(0.0397)	(0.00911)	(0.173)
RML*Hispanic	-0.0515	0.00589	-0.0888
	(0.0417)	(0.00678)	(0.144)
Pre-Treat Mean (White)	0.308	0.095	3.259
Pre-Treat Mean (Black)	0.331	0.117	3.921
Pre-Treat Mean (Hispanic)	0.336	0.113	3.812
N	7,376,633	7,376,633	7,376,633

***p < .01 **p<.05 *p<0.10

Notes: All regressions include state fixed effects, year fixed effects, month fixed effects and month by year fixed effects. Furthermore, regressions include controls for a medical marijuana law, a marijuana decriminalization law the state unemployment rate, per capita personal income (logged), the proportion of state population that is Black, the proportion of the state population that is Hispanic, law enforcement personnel per 1,000 population (logged), beer tax per gallon (logged), alcohol- and drug-specific Good Samaritan laws, a naloxone access law, a must-access prescription drug monitoring program, the state EITC refundable rate, the higher of the state of Federal minimum wage (logged), an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, and whether the governor is a Democrat. Regressions are weighted using BRFSS sample weights and standard errors are clustered at the state level.

Appendix Table 7. Racial Disparities in the Effects of RMLs on Suicides and Drug-Related Mortality, by Race and Ethnicity, NVSS

	(1)	(2)	(3)	(4)
	Suicides	Drug-Related Suicide	Drug Overdose Deaths	Opioid Overdose Deaths
RML	0.218	-0.073	-2.531	-4.201*
	(0.374)	(0.106)	(2.315)	(2.490)
RML*Black	0.308	0.123	4.755	1.037
	(0.553)	(0.158)	(4.657)	(4.068)
RML*Hispanic	0.597*	0.067	2.987*	3.907**
	(0.316)	(0.123)	(1.520)	(1.702)
Pre-Treat Mean (White)	20.139	2.577	15.420	11.320
Pre-Treat Mean (Black)	10.066	0.888	15.079	5.473
Pre-Treat Mean (Hispanic)	8.733	0.769	8.644	3.436
N	4,080	4,080	4,080	4,080

***p < .01 **p<.05 *p<0.10

Notes: All regressions include state fixed effects, and year fixed effects. Furthermore, regressions include controls for a medical marijuana law, a marijuana decriminalization law the state unemployment rate, per capita personal income (logged), the proportion of state population that is Black, the proportion of the state population that is Hispanic, law enforcement personnel per 1,000 population (logged), beer tax per gallon (logged), alcohol- and drug-specific Good Samaritan laws, a naloxone access law, a must-access prescription drug monitoring program, the state EITC refundable rate, the higher of the state of Federal minimum wage (logged), an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, and whether the governor is a Democrat. Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Appendix Table 8. TWFE Estimates of Effect of RMLs on Suicides and Drug-Related Mortality, by Race/Ethnicity and Age, NVSS

	(1)	(2)	(3)	(4)
	Suicides	Drug-Related Suicides	Drug Overdose Deaths	Opioid Overdose Deaths
		Panel I: Ages 18-to-20, Non-Hispanic Whites		
RML	0.194	-0.002	0.941	-0.603
	(0.825)	(0.254)	(1.180)	(1.674)
Pre-Treat Mean	14.661	0.971	7.875	3.128
N	1,020	1,020	1,020	1,020
		Panel II: Ages 21-and-Older, Non-Hispanic Whites		
RML	0.118	-0.079	-2.505	-4.434*
	(0.379)	(0.106)	(2.251)	(2.435)
Pre-Treat Mean	20.418	2.660	15.799	11.736
N	1,020	1,020	1,020	1,020
		Panel III: Ages 18-to-20, Blacks and Hispanics		
RML	0.353	0.191	-0.153	-0.164
	(0.836)	(0.187)	(1.142)	(1.098)
Pre-Treat Mean	7.132	0.247	2.179	0.142
N	1,020	1,020	1,020	1,020
		Panel IV: Ages 21-and-Older, Blacks and Hispanics		
RML	0.723**	0.008	0.660	-1.727
	(0.325)	(0.081)	(2.592)	(2.414)
Pre-Treat Mean	9.681	0.905	12.991	5.678
N	1,020	1,020	1,020	1,020

***p < .01

**p<.05 *p<0.10

Notes: All regressions include state fixed effects, and year fixed effects. Furthermore, regressions include controls for a medical marijuana law, a marijuana decriminalization law the state unemployment rate, per capita personal income (logged), the proportion of state population that is Black, the proportion of the state population that is Hispanic, law enforcement personnel per 1,000 population (logged), beer tax per gallon (logged), alcohol- and drug-specific Good Samaritan laws, a naloxone access law, a must-access prescription drug monitoring program, the state EITC refundable rate, the higher of the state of Federal minimum wage (logged), an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, and whether the governor is a Democrat. Regressions are weighted using the adult state population and standard errors are clustered at the state level.

Appendix Table 9. Sensitivity of Estimated Mental Health Effects to State-Specific Linear Time Trends and Census Region-Specific Year Fixed Effects, BRFSS

	(1)	(2)	(3)
	Any Poor Mental Health Days	>= 15 Poor Mental Health Days	Number of Poor Mental Health Days
	Panel I: Non-Hispanic White		
RML	0.0584*	-0.00519	0.0286
	(0.0333)	(0.00539)	(0.0611)
Pre-Treat Mean	0.308	0.095	3.259
N	5,834,973	5,834,973	5,834,973
	Panel II: Black and Hispanic		
RML	0.0142	0.00646**	0.139
	(0.00895)	(0.00283)	(0.0959)
Pre-Treat Mean	0.333	0.115	3.872
N	1,077,137	1,077,137	1,077,137

***p < .01 **p<.05 *p<0.10

Notes: All regressions include state fixed effects, year fixed effects, month fixed effects and month by year fixed effects. Furthermore, regressions include controls for a medical marijuana law, a marijuana decriminalization law the state unemployment rate, per capita personal income (logged), the proportion of state population that is Black, the proportion of the state population that is Hispanic, law enforcement personnel per 1,000 population (logged), beer tax per gallon (logged), alcohol- and drug-specific Good Samaritan laws, a naloxone access law, a must-access prescription drug monitoring program, the state EITC refundable rate, the higher of the state of Federal minimum wage (logged), an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, and whether the governor is a Democrat. Regressions are weighted using BRFSS sample weights and standard errors are clustered at the state level.

Appendix Table 10. Sensitivity of Estimated Mortality Effects to State-Specific Linear Time Trends and Census Region-Specific Year Fixed Effects, NVSS

	(1)	(2)	(3)	(4)
	Suicides	Drug-Related Suicides	Drug Overdose Deaths	Opioid Overdose Deaths
			Panel I: Non-Hispanic White	
RML	0.251	0.107	-1.399	-1.302
	(0.334)	(0.111)	(1.143)	(1.432)
Pre-Treat Mean	20.139	2.577	15.420	11.320
N	1,020	1,020	1,020	1,020
			Panel II: Black and Hispanic	
RML	-0.286	0.121	0.738	0.520
	(0.380)	(0.125)	(2.477)	(1.734)
Pre-Treat Mean	9.391	0.846	12.093	5.262
N	1,020	1,020	1,020	1,020

***p < .01 **p<.05 *p<0.10

Notes: All regressions include state fixed effects, and year fixed effects. Furthermore, regressions include controls for a medical marijuana law, a marijuana decriminalization law the state unemployment rate, per capita personal income (logged), the proportion of state population that is Black, the proportion of the state population that is Hispanic, law enforcement personnel per 1,000 population (logged), beer tax per gallon (logged), alcohol- and drug-specific Good Samaritan laws, a naloxone access law, a must-access prescription drug monitoring program, the state EITC refundable rate, the higher of the state of Federal minimum wage (logged), an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, and whether the governor is a Democrat. Regressions are weighted using the adult state population and standard errors are clustered at the state level.