Journal of Public Economics 234 (2024) 105075

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

The effects of recreational marijuana laws on drug use and crime☆

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ARTICLE INFO

JEL codes:
I12
I18
K14

Keywords:
Recreational marijuana laws
Drug use
Crime
Substance use
Treatment admissions
Opioids

ABSTRACT

Recreational marijuana laws (RMLs), which legalize the sale and possession of small quantities of marijuana for recreational use, have been adopted by 24 states and the District of Columbia. Using a generalized difference-in-differences approach and data for the period 2000–2019 from a variety of sources (the National Survey of Drug Use and Health, the Uniform Crime Reports, the Treatment Episode Data Set, and the National Vital Statistics Mortality files), this study comprehensively examines the effects of legalizing recreational marijuana on drug use, crime, and admissions to substance use treatment facilities. Our analyses show that RML adoption increases the use of marijuana by adults and reduces marijuana-related arrests. However, we find little evidence that RMLs increase the use of harder drugs, admissions to substance use treatment facilities, or property and violent crime. In fact, our results are consistent with the hypothesis that marijuana and opioids are substitutes.

1. Motivation

Recreational marijuana laws (RMLs) legalize the possession, sale, and consumption of small quantities of marijuana for recreational purposes for those 21 years of age and older. As of January 2024, 24 states and the District of Columbia had adopted RMLs (Anderson and Rees, 2023; NORML, 2024). At the federal level, President Biden has pardoned individuals convicted of federal marijuana possession offenses and ordered the Secretary of Health and Human Services and the Attorney General to study changing the status of marijuana as a Schedule I drug under the Controlled Substances Act (Biden, 2022; Fandos, 2021).

Proponents of legalizing marijuana argue that RMLs will generate social benefits by eliminating enforcement costs (American Civil Liberties Union, 2013), increasing labor market opportunities for those who will no longer have drug-related criminal records (Agan and Starr, 2017), and improving health through the consumption of marijuana itself or through substitution away from more harmful substances (Anderson and Rees, 2023). Opponents argue that RML adoption will cause marijuana-related respiratory health problems (National Academies of Science, Engineering and Medicine, 2017), serve as a gateway to harder drugs, and generate external costs associated with addiction and crime (Bleyer and Barnes, 2018; Hunt et al., 2018; Wong and Lin, 2019; Olfson et al., 2018).[1] Some of these concerns center around impacts of legalization on underage youth, particularly in light of the potential harms of marijuana use on the developing brain. The prefrontal cortex, which regulates responses to emotional stimuli, is not fully developed

☆ Dr. Sabia acknowledges support from the Center for Health Economics & Policy Studies (CHEPS), which has received grant funding from the Charles Koch Foundation and Troesh Family Foundation. Dr. Sabia also acknowledges research support received from the James Hervey Johnson Charitable Educational Trust to support this project. Dr. Rees gratefully acknowledges support from MICIN/AEI/10.13039/501100011033, grant CEX2021-001181-M; and Comunidad de Madrid, grant EPUC3M11 (V PRICIT). We thank Samuel Safford, Drew McNichols, Rebecca Margolit, and Abdullah Alswelh for outstanding research assistance.

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[1] Some addiction costs may be internalized with initial marijuana consumption decisions as part of rationally addictive choices (Becker and Murphy, 1988). On the other hand, if individuals have time inconsistent preferences, future addiction costs associated with current consumption decisions may not be internalized (Gruber and Köszegi, 2001).

https://doi.org/10.1016/j.jpubeco.2024.105075
Received 8 December 2021; Received in revised form 15 January 2024; Accepted 22 January 2024
Available online 18 April 2024

until about 25 years of age (Arain et al., 2013); hence adolescents and young adults may be more likely to engage in "irrational" and impulsive behavior, under-weighting the future costs of early onset use of marijuana (and other addictive substances).[2,3] To inform this important public policy debate, we provide new evidence on the effects of RML adoption on the use of hard drugs such as cocaine and heroin, admissions to substance use treatment facilities, and arrests for property and violent crimes.

RML adoption is hypothesized to increase marijuana consumption by removing criminal penalties for recreational use and through supply-driven reductions in the price of marijuana caused by the opening of dispensaries and home cultivation.[4] Whether RML adoption leads to increases in marijuana-related drug treatment admissions depends upon the extent to which RMLs affect heavier, problematic marijuana use. The spillover effects of RMLs on illicit drug use other than marijuana — including the non-medical use of prescription pain relievers and the use of harder drugs such as cocaine, methamphetamine, and heroin — could, in theory, be affected through both demand- and supply-side channels. On the demand side, RMLs could, for example, generate a "gateway effect" whereby consumers initially respond to RMLs by increasing marijuana use, but, over time, seek out harder drugs to satisfy their need for a more intense euphoric feeling. On the supply side, dealers could more aggressively push other, still illicit, drugs because of increased competition from legal marijuana. Alternatively, dealers could shift their focus to the production and sale of marijuana, making the reduced-form effect of RML adoption on harder drug consumption (and drug-treatment admissions for harder drugs) theoretically ambiguous.

Many of the effects of RMLs on crime and arrests are also, *a priori*, difficult to sign. RML adoption is expected to reduce marijuana-related arrests by removing legal penalties for the possession of marijuana.[5] However, the effect of RMLs on non-marijuana drug crime will depend on the demand- and supply-side factors referred to above, while their effects on arrests related to drugs other than marijuana could also depend on whether and how police respond. For instance, RMLs could encourage police to substitute their patrolling time away from marijuana detection and towards heroin-related offenses. Alternatively, police could respond by focusing on violent or property crimes, reducing the time and effort they spend on fighting drug-related crime in general (Adda et al., 2014).

This study provides some of the most comprehensive evidence to date on the broader impacts of RML adoption on a wide set of outcomes, including harder drug use, criminal incidents and arrests, admissions for drug addiction-related treatment, and mortality. We draw upon survey-based and administrative information, spanning the period 2000–2019, across five national data sets: National Survey of Drug Use and Health, Uniform Crime Reports, National Incident Based Reporting System, Treatment Episode Data Set, and the National Vital Statistics multiple-cause-of-death mortality data. The main analyses rely on a generalized difference-in-differences approach, augmented with newly developed event-study estimators that account for dynamic heterogeneous treatment effects within our (cross-state) staggered treatment adoption setting (Callaway and Sant'Anna, 2021).

We document several key findings. First, RML adoption increases marijuana use among adults by as much as 47 percent and reduces arrests for marijuana possession among adults by approximately 80 percent. Second, we find little evidence that RML adoption increases the consumption of harder drugs or drug-related treatment admissions. Third, on the contrary, we document that RML adoption is associated with reductions in the non-medical use of prescription pain relievers, reductions in synthetic narcotics arrests, and reductions in opioid-related treatment admissions. Auxiliary analyses show that RML adoption is also negatively related to deaths involving opioids. These latter findings are consistent with the hypothesis that marijuana and opioids are substitutes. Fourth, with respect to any positive spillovers into the sale and trafficking of other drugs, there is no evidence to suggest this to be the case, and some evidence to the contrary that RMLs are associated with a decrease in arrests for the sale of certain harder drugs – a finding that is consistent with suppliers possibly substituting away from illicit harder drug markets due to the legalization. Finally, we find scant evidence of any spillover effects from legalizing marijuana to violent and property crime, and are able to largely rule out that observed effects on crime are driven by shifts in policing resources. A causal interpretation of these estimates is supported by event study analyses. Overall, our findings do not point to any evidence of substantial adverse spillovers from legalizing marijuana into other illicit drug use or crime thus far.

This paper is organized as follows. Section 2 provides background on recreational marijuana legalization and the relevant literature, in conjunction with a fuller accounting of the pathways through which legalization can impact drug markets and criminal activity. Data are outlined in Section 3, and Section 4 describes our methods. Our main results, robustness checks, and extensions are reported in Section 5. Finally, Section 6 concludes by offering further context as we review the findings.

2. Background

2.1. Recreational marijuana laws

In 2012, Colorado and Washington became the first states to legalize recreational marijuana. Just over a decade later, researchers are gaining a better understanding of how legalizing recreational marijuana affects outcomes of interest to policymakers and the public. Unlike medical marijuana laws (MMLs), RMLs require neither a doctor's recommendation nor registration with the state; anyone 21 years of age or older can possess limited amounts of marijuana (typically one to three ounces), and purchases of marijuana are typically made at recreational dispensaries. All but three RML states (Delaware, Illinois and New Jersey) allow the cultivation of marijuana plants at home.[6] According to polling from Gallup, marijuana legalization was supported by 70 percent of the American public in 2023, approximately double the rate of support two decades before (Saad, 2023). However, there is a relative paucity of evidence regarding the indirect, downstream effects of RMLs.

Proponents of marijuana legalization argue that light-to-moderate marijuana consumption generates few adverse health effects (National Academies of Sciences, Engineering and Medicine, 2017), while legalization might improve public health as users of alcohol, tobacco, and

[2] The Substance Use and Mental Health Services Administration (SAMHSA) labeled marijuana use among youths "a major public health concern. (SAMHSA, 2020) linking early use to 1) diminished neuro-psychological and neuro-developmental function (U.S. Office of the Surgeon General, 2019), 2) increased risk of adult psychotic disorders, 3) increased risk of suicide behaviors, and 4) lower academic achievement.

[3] Evidence on how marijuana legalization impacts consumption among adolescents is decidedly mixed, and an important area for future inquiry given these disparate findings. Hollingsworth et al. (2022), for instance, find significant increases in marijuana use among youth (on the order of about 10 percent) following legalization of recreational use, while Anderson et al. (2021) find no such impact. An earlier study by Cerdá et al. (2017), in assessing effects among adolescents in the two earliest legalizing states, found a significant increase for Washington but not for Colorado.

[4] The effects of marijuana taxation that often accompanies recreational legalization may mitigate some of these effects, as discussed below.

[5] This effect could be somewhat offset if increases in marijuana use caused by RMLs lead to some consumers possessing marijuana over the legal limits prescribed in statutes (typically one to two ounces).

[6] In Washington D.C., home cultivation is allowed, but its RML prohibits the exchange of money, goods, or services for marijuana; transfers of up to an ounce of marijuana, are, however, legal.

opioids substitute towards marijuana (Anderson et al., 2013; Bachhuber et al., 2014; Dave et al., 2023). Moreover, proponents note that legalization will generate (1) substantial cost savings from reduced policing, court, and prison/jail costs (ACLU 2013), (2) a reduction in gang-involved (or cartel-related) violence used to protect illicit local drug monopolies, and (3) important labor market benefits for those who will avoid a marijuana-related criminal record (Agan and Starr, 2017).[7] Finally, legalization may be an important tool to reduce racial disparities in the enforcement of marijuana prohibition (Fone et al., 2023). Despite comparable marijuana usage rates, Blacks are 3.6 times more likely than Whites to be arrested for marijuana-related offenses (Federal Bureau of Investigation, 2020; Edwards et al., 2020).[8]

Critics, by contrast, have expressed several concerns regarding legalization. Smoking marijuana on a regular basis may lead to chronic cough, excess phlegm production, and more frequent chronic bronchitis episodes (National Academies of Sciences, Engineering and Medicine, 2017). Moreover, if marijuana and harder drugs are complements — perhaps because marijuana users seek out a more intense drug-related high — RMLs could encourage the use of and addiction to cocaine, opioids, and methamphetamine (Bleyer and Barnes, 2018; Hunt et al., 2018; Wong and Lin, 2019; Olfson et al., 2018). Indeed, fears about a "gateway effect" of marijuana have been voiced by presidents, vice presidents, drug czars, senators, congressmen, and state and local officials, including state attorneys general (see DeAngelo and Redford, 2015).[9] Opponents note that RMLs could also have unexpected supply-side spillover effects in adjacent drug markets (i.e., illicit sellers of opioids may increase effort selling their product), and spillover effects in neighboring jurisdictions without RMLs. Finally, opponents worry that RMLs will increase marijuana use among underage youth, which can have long-lasting negative effects on their cognitive development (NIDA, 2020).

2.2. Medical marijuana laws, harder drug use, and crime

Much of the existing evidence on the effects of liberalizing access to marijuana on drug use and crime comes from studies of medical marijuana laws (MMLs). MMLs legalize, for allowable medical purposes, the possession, sale, and consumption of marijuana. As of January 2024, these laws had been enacted by 38 states and the District of Columbia (NORML, 2024). There is strong evidence that MML adoption increases marijuana consumption among adults on the order of 1 to 2 percentage points (Chu, 2014; Wen et al., 2015; Sabia and Nguyen, 2018; Choi et al., 2019; Anderson and Rees, 2023), with generally larger longer-run effects, likely due to the opening of medical marijuana dispensaries.

With respect to harder drug use, there is little support for the hypothesis that MMLs significantly affect cocaine use (Wen et al., 2015) or substance abuse treatment admissions for cocaine (Chu, 2015).[10] There is, however, stronger evidence that MML adoption is associated with a reduction in opioid prescribing (Bradford and Bradford, 2016, 2017, 2018; Bradford et al., 2018; Wen and Hockenberry, 2018; McMichael et al., 2020) and deaths involving opioids (Bachhuber et al., 2014), particularly when medical marijuana dispensaries are legally protected (Shover et al., 2019; Smith, 2020; Powell et al., 2018).[11] Moreover, there is some evidence that MML adoption increases marijuana-related admissions to substance use treatment facilities (Chu, 2014), but reduces admissions involving heroin (Chu, 2015).

Consistent with the hypothesis that marijuana and alcohol are substitutes (DiNardo and Lemieux, 2001), there is strong evidence that legalizing marijuana for medicinal purposes reduces the consumption of alcohol (Anderson and Rees, 2023). Both survey-based studies (Anderson et al., 2013; Wen et al., 2015; Sabia et al., 2017; Johnson et al., 2017; Andreyeva and Ukert, 2019; Hollingsworth et al., 2022) and sales-based studies (Anderson et al., 2013; Baggio et al., 2020; Veligati et al., 2020) find that the enactment of MMLs reduces alcohol consumption among adults. In addition, MMLs appear to have important negative effects on alcohol-related traffic fatalities (Anderson et al., 2013; Chen and French, 2023).[12]

With respect to criminal activity, the evidence suggests that the legalization of medical marijuana increased marijuana possession arrests (Chu, 2014), but has no spillovers into property or violent offenses in most jurisdictions (Morris et al., 2014; Chu and Townsend, 2019). If anything, legalization of medicinal marijuana may be associated with decreases in non-drug crime. In particular, some locales that had previously experienced cartel-involved violence appear to have experienced reductions in violent crime following MML adoption. Gavrilova et al. (2017) found that states bordering Mexico that adopted an MML experienced a 13 percent reduction in violent crime. The authors attributed this result to increased competition in the international marijuana market, which reduced "black market" power of Mexican drug trafficking organizations and their incentives to invest in violence to deter illicit market entry (Miron and Zwiebel, 1995; Gavrilova et al., 2017). Finally, Chu (2015) found that legalizing medical marijuana was associated with a 10–15 percent increase in marijuana arrests and a 0–15 percent reduction in possession arrests for cocaine and heroin.[13]

Perhaps out of necessity given the paucity of evidence on RMLs, policymakers have often used the findings described above about MMLs to forecast the effects of RML adoption on harder drug use and crime. However, the effects of RMLs may differ from MMLs for several reasons. First, the population directly targeted by MMLs — registered patients in need, who stand to benefit from cannabis use upon a physician's recommendation — is a relatively small pool. Registered patients

[7] For broader references of how criminal records may affect labor market prospects, also see: Pager, 2003; Agan and Starr, 2018; Dobbie et al., 2018; Mueller-Smith and Schnepel, 2021; and Agan et al., 2023.

[8] Among other arguments for lifting the prohibition on marijuana include (i) generating a safer consumer product through increased market competition and government regulation of a legitimate market (Fernandez, 2019), (ii) reducing violence by limiting market power of and profits to illegal criminal cartels (Gavrilova et al., 2017), and (iii) increasing tax revenue to state coffers (Marijuana Policy Project, 2021a).

[9] In 2010, then-Vice President Joe Biden stated, "I still believe [marijuana] is a gateway drug. I've spent a lot of my life as chairman of the Judiciary Committee dealing with this. I think it would be a mistake to legalize."While, as a 2020 presidential candidate, Mr. Biden's position on the question of legalization evolved, many politicians, particularly in the House Republican caucus, still hold this view. In 2021, Rep. Andy Harris (R-MD) stated, "In the midst of an increase in opioid addiction deaths during the coronavirus pandemic, it seems strange…to fully legalize marijuana, a known gateway drug to opioid addiction."

[10] In addition, living near an MML dispensary was also essentially unrelated to cocaine-involved emergency department visits (Conyers and Ayres, 2020).

[11] Moreover, Bradford et al. (2018) find that MML adoption was associated with reductions in anxiety- and sleep-related prescriptions.

[12] On the other hand, Conyers and Ayres (2020) show that emergency department visits involving alcohol among Arizonans were essentially unrelated to the opening of dispensaries to purchase medical marijuana.

[13] Evidence on the labor market effects of MMLs is also generally consistent with either (i) net health benefits, or (ii) no adverse health effects of legalization of medicinal marijuana. Ghimire and Maclean (2020) show that the enactment of MMLs is associated with a reduction in the likelihood of workers' compensation claims and the income received from such claims. They interpret these results as evidence that medicinal marijuana availability generates health-related benefits via workers being able to better treat medical conditions. Along the same lines, Nicolas and Maclean (2019) find evidence that older adults' labor supply increases with increased access to medical marijuana. Finally, Sabia and Nguyen (2018) find little support for the hypothesis that MMLs had important effects on employment or wages of working-age individuals (and only a small negative effect on wages for young adult males).

comprise only about two percent of the total population in states that have legalized medical marijuana (Marijuana Policy Project 2021b). Although there was substantial diversion of medical marijuana to the recreational market in states with lax MMLs such as California, Colorado, Oregon and Washington (Anderson et al., 2013), restrictions on access to medicinal marijuana inherently constrains the "first-stage" effects of MMLs on marijuana use. For instance, even the laxest MMLs require a doctor's recommendation, and the strictest MMLs further require verifiable medical conditions and distribution only through state-licensed dispensaries. Consequently, MMLs have been found to raise marijuana use by a limited amount – on the order of 1–2 percentage points (Anderson and Rees, 2011; Wen et al., 2015; Sabia and Nguyen, 2018; Choi et al., 2019).[14] By contrast, RMLs have few restrictions beyond the age verification, and apply to a much broader population of current and potential recreational users.

Second, the limited first-stage response of the MMLs further limits some (although not all) downstream effects on harder drug use and crime. RMLs, on the other hand, by reaching a much larger population and having a stronger first-stage impact on marijuana consumption, have the capacity to have more substantial spillover effects. The one exception to the limited potential of MMLs for downstream effects relates to opioids, where the sub-population targeted by MMLs (i.e., patients suffering from medical conditions such as chronic pain, who may benefit from cannabis use) is also the one that would be predicted to have an intrinsically higher demand for opioid painkillers. Thus, the availability of medical marijuana for this population appears to have reduced their need for opioids. Here too, however, RMLs may have a very different impact on opioid use, because these policies are reaching a broader population beyond just patients with medical conditions by commercializing the marijuana market and expanding availability. As the opioid epidemic shifted from Rx opioids to heroin and synthetic opioids, several states adopted RMLs, making the impact of these laws on Rx versus illicit opioids difficult to gauge.

Finally, it is important to note that every state that has adopted an RML already had an MML in place. Expanding the legal marijuana market to include recreational as well as medicinal users could, in theory, lead to very different local average treatment effects (LATEs) than those documented by previous researchers for strict MMLs. Lax MMLs, such as those adopted by California, Colorado, Oregon and Washington, arguably led to the de facto legalization of marijuana for recreational purposes (Anderson and Rees, 2023). Whether RML adoption affected the use of hard drugs and crime in states with lax MMLs already in place is an open question.

2.3. Recreational marijuana laws, hard drug use, and crime

In theory, the relationship between RML adoption and the consumption of marijuana is not difficult to sign. Legalization should increase both the supply and demand of marijuana, leading to an increase in consumption (Anderson and Rees, 2023). By comparison, predicting the effects of RML adoption on our second-stage outcomes is not as straightforward. RMLs could have countervailing supply- and demand-side effects on the market for harder drugs, which in turn could have complicated, difficult-to-predict effects on use of other illicit drugs, criminal activity, and admissions to drug treatment centers.

For instance, if marijuana and prescription opioids are treated by consumers as substitutes, then we might expect RML adoption to reduce the abuse of prescription opioids such as OxyContin, Percocet, and Vicodin. Legalization could also impact consumers through spillover supply-side responses in adjacent illicit drug markets. By increasing competition from legal retailers in the marijuana market, street dealers could be diverted into selling other drugs such as opioids, increasing competition and/or lowering prices and search costs for consumers of opioids. Suppliers of opioids could also respond to RML adoption by more aggressively pushing their products, mitigating, or even reversing, any demand-side substitution effect from marijuana to opioid use. On net, the reduced-form effect of RML adoption on prescription opioid abuse is, *a priori*, difficult if not impossible to sign.

With respect to arrests, RML adoption is expected to affect this outcome through (1) criminal behavior, and (2) changes in policing/enforcement. We expect RMLs to reduce arrests for marijuana possession given that possessing small quantities of marijuana is explicitly allowed. This effect could be offset by increases in possession over the legal limit or spillovers to the illicit market driven by the imposition of marijuana taxes that accompany legalization (see Section 3 below). The reduced-form effect of RML adoption on arrests for harder drugs will depend on demand-side factors (such as whether the harder drug is a complement or substitute for marijuana), supply-side factors (such as shocks to the supply for harder drugs), and reallocation of policing resources (such as whether policing resources are reallocated from enforcing marijuana prohibition to enforcing the prohibition on harder drugs).

Broadly, addictive substance use can impact criminal activity through an economic effect, whereby drug users may rely on income-generating crime to finance their drug use, a pharmacological effect, whereby drug use can induce aggression or violent tendencies, and/or a systemic effect due to the high degree of violence and conflict inherent in sales and distribution networks in illicit markets.[15] These channels point to important ways in which RML adoption could potentially increase both violent and property crime, in line with concerns voiced by opponents of legalization. For instance, if marijuana legalization spills over into increased use of other illicit drugs, it could lead to more property crime to fund these drug habits, and greater violent crime due to the psychotropic effects of drug use.[16] In addition, if RMLs generate supply-side responses that affect adjacent illicit drug markets, violence by gangs or cartels could be affected because violence is often used as a barrier to illicit market entry. Finally, arrests for offenses could be influenced by any reallocation of policing resources from enforcing the prohibition on marijuana toward detecting property and violent crime.

Credible studies assessing the reduced-form effects of RMLs are few in number, probably because legalizing recreational marijuana is a relatively recent phenomenon and enough post-legalization data are only now becoming available. A handful of studies have examined the relationship between RMLs and marijuana use (Anderson et al., 2019; Hollingsworth et al., 2022; Ali et al., 2023), while others have explored whether RMLs are related to the use of other substances that could be complements or substitutes for marijuana. There is evidence that

[14] Choi et al. (2019), for instance, find that the enactment of an MML resulted in an 0.8 percentage-point increase in the rate of marijuana use, or approximately 12 percent relative to the mean prevalence rate.

[15] Effects of substance use on criminal activity are well-studied in other contexts. Grogger and Willis (2000) study the emergence of crack cocaine across metropolitan areas in the U.S. and find that its entry led to increases in both violent crime, notably aggravated assaults, as well as property crime. They attribute the effects on violent crime to an expansion of the black market (the "systemic effect" noted above), and the effects on property crime as being consistent with the need to finance the increased demand for the drug ("economic effect"). Miron (1999) studied enforcement spending for alcohol and drug prohibition in the U.S., over a long time span, and finds that higher levels of enforcement are actually associated with more homicides. He attributes this finding to the "systemic effect" wherein greater prohibition feeds black markets, and that black market participants are more likely to resort to violence to resolve disputes. More recently, Dave et al. (2021) study the effects of prescription drug monitoring programs, which restrict access to prescription opioids in an effort to limit their misuse and diversion into the black market, and find that well-deployed programs lead to significant reductions in both violent and property crime. They attribute these effects to all three channels.

[16] Evidence on whether use of marijuana leads to violent or aggressive behavior is unclear and largely inconclusive (Myerscough and Taylor, 1985; Moore and Stuart, 2005; Ostrowsky, 2011; Testa and Brown, 2015).

legalizing recreational marijuana leads to less opioid prescribing (Wen and Hockenberry, 2018) and fewer deaths involving prescription opioids (Chan et al., 2020). Evidence regarding the effects of RMLs on traffic fatalities is mixed (Lane and Hall, 2019; Hansen et al., 2020a; Santaella-Tenorio et al., 2020; Chen and French, 2023).

Studies on the relationship between RMLs and alcohol use have largely been case studies of particular states or, in one case, a study of college students. Using data from the National Survey on Drug Use and Health (NSDUH) — and a regression discontinuity design (exploiting distance to a non-RML state border) — Dragone et al. (2019) found that adoption of an RML led to a 20 percent decline in binge drinking. Focusing on Washington state, Miller and Seo (2021) found that the adoption of the state's RML was associated with a 5 percent decline in alcohol sales. Finally, Alley et al. (2020) used data from the National College Health Assessment-II and a difference-in-differences estimation strategy to find that RML enactment led to a 6 percent decline in binge drinking among young adults enrolled in college.[17,18]

Only one published study has examined the association between RMLs and crime. Dragone et al. (2019) found that Washington's RML led to a significant reduction in rapes and property crime during the period 2013–2014. Because this study had only two years of post-legalization data from a single state, Washington, it is not clear that its results extend to other years and states.[19] Whether other recreational marijuana states will experience reductions in crime, and whether the reduction in Washington is temporary or permanent, are open questions.[20]

This study contributes to the above body of literature by being the first to use nationally representative data across all 50 states and the District of Columbia to comprehensively examine the broader impacts of RMLs on (1) harder drug use, (2) drug treatment admissions, and (3) criminal arrests. In addition, we provide supplementary analyses on drug-involved deaths. We bring evidence from both survey-based data sources as well as administrative data sources (with more objective measures of drug use) and, where possible, distinguish among mechanisms through which RMLs are likely impacting the outcome under study (i.e., policing vs. criminal behavior as a channel to explain arrests). Together, the findings from this study can inform key components of the ongoing policy debate over the efficacy of legalizing access to recreational marijuana.

3. Data

3.1. National survey of drug use and health (NSDUH)

We begin by using data on adult drug use from the National Survey of Drug Use and Health (NSDUH). The NSDUH data are collected by the Substance Abuse and Mental Health Services Administration and publicly available state-level prevalence data are provided in two-year overlapping averages over the 2002–2019 period.[21] Marijuana use is measured using responses to the following survey item:

> "During the past 30 days, on how many days did you use marijuana or hashish?"

Over the sample period, 7.4 percent of adult respondents ages 18 and older reported prior 30-day marijuana use. To measure illicit drug use other than marijuana and specific harder drugs, we use responses to the following survey items:

> "During the past 30 days, on how many days did you use an illicit drug other than marijuana? (Includes heroin, hallucinogens, inhalants, cocaine, and the nonmedical use of prescription-type pain relievers, tranquilizers, stimulants, and sedatives)?"
> "These questions are about cocaine, including all the different forms of cocaine such as powder, crack, free base, and coca paste. During the past year, on how many days did you use any form of cocaine?"
> "During the past 30 days, on how many days did you have five or more drinks (for males) or four or more drinks (for females) on the same occasion (i.e., at the same time or within a couple of hours of each other)?"
> "During the past year, did you ever use any prescription pain reliever in a way a doctor did not direct you to use it?" (Included among such pain relievers: products containing hydrocodone, oxycodone, tramadol, codeine, morphine, prescription fentanyl,31 buprenorphine, oxymorphone, and hydromorphone, as well as Demerol®, methadone, or any other prescription pain reliever."

We find that 3.4 percent reported illicit drug use other than marijuana in the prior 30 days, 2.1 percent of adults ages 18 and older reported cocaine use in the last year, 25.0 percent reported binge drinking in the last month, and 4.4 percent reported non-medical use of prescription pain relievers in the last year.[22]

The NSDUH data available to us largely measure drug use at the extensive margin. Next, we turn to outcomes related to drug-related arrest, substance use treatment, and drug-involved mortality, indirect proxies for substance use that likely capture the intensive margin of drug use (i.e., heavier drug use) as well.

[17] Some studies in this literature have explored the effect of RMLs on traffic fatalities, following the seminal work of Anderson and Rees (2023). The findings in this literature, based on Colorado and Washington, are mixed, owed both to differences in empirical strategies (synthetic controls using different matching variables vs difference-in-differences) and the state under study (Aydelotte et al., 2017; Hansen et al., 2020a; Santaella-Tenorio et al., 2020).

[18] Kelly and Rasul (2014) study the decriminalization of marijuana in a borough of London and find some evidence that reducing the penalties for possession led to a reduction in alcohol consumption.

[19] The most likely mechanism underlying this result is substitution away from alcohol as marijuana became available for recreational purposes. Previous studies provide evidence of a causal link between alcohol consumption and crime (e.g., Anderson et al., 2018).

[20] Three studies of which we are aware have explored the impact of local dispensary openings (or closings) for recreational marijuana on crime. These studies have examined the effects of recreational marijuana dispensaries in Denver, Colorado and Los Angeles, California. Using an instrumental variables (IV) approach, Brinkman and Mok-Lamme (2019) found that a dispensary opening in Denver is associated with a 19 percent decline in crime. Using a difference-in-differences approach Burkhardt and Goemans (2019) found that recreational dispensary openings in Los Angeles were associated with a decline in violent crime in geographically proximate, higher-income neighborhoods. On the other hand, Chang and Jacobson (2017) find that motor vehicle thefts near recreational dispensaries were positively related to dispensary closures.

[21] For instance, SAMHSA makes available state-specific estimates for 1999–2000, 2000–2001, and so on. We use these overlapping state panels in the analyses, and match information on RMLs based on the month and year of enactment (and in supplementary analysis, based on the month and year of the availability of retail distribution). Our indicator captures the fraction of the two-year period that the RML was in effect, ranging from 0 (for state-years that fully precede RML adoption) to 1 (for state-years that fully follow RML adoption). For adoption dates that fall within the two-year aggregates, we define the indicator to equal the fraction of the 24 months that the RML was in effect.

[22] In its administration of the NSDUH, the Substance Abuse and Mental Health Services Administration changed its asking of questions related to prescription pain reliever misuse in the 2014–15 wave, noting "Several changes were made to the prescription drug questions for the 2015 NSDUH. These changes were designed to address limitations in the survey design used in prior years. Special attention was paid to revising the modules that measure prescription drug misuse or "nonmedical" use because of public health concerns about misuse of prescription drugs, such as increases in the number of drug poisoning deaths involving opioid pain relievers like hydrocodone, oxycodone, and methadone." See: https://www.samhsa.gov/data/sites/default/files/NSDUH-RedesignChanges-2015.pdf.

3.2. Uniform crime reports (UCR)

We supplement our substance use survey data from the NSDUH with administrative data on drug and non-drug arrests from the 2000–2019 Uniform Crime Reports (UCR). The UCR data cover arrests in all 50 states and the District of Columbia, including 98 percent of the U.S. population. Arrest data are collected via voluntary reports from more than 16,000 city, county, and state agencies.[23]

We calculate state-by-year arrest counts from the UCR and then generate an offense-specific *Arrest Rate* per 1,000 (age- and gender-specific) state population using data from the Surveillance, Epidemiology, and End Results (SEER) Program. Our arrest analysis focuses on arrestees ages 18 and older and disaggregates arrests around the minimum legal purchasing age for recreational marijuana (21): (1) those ages 18–20, a population for whom possession and cultivation of marijuana under RMLs remains illegal, and (2) those ages 21 and older.

There are several well-known measurement concerns with the UCR that we address through a series of empirical checks. First, to address concerns that law enforcement agencies (LEAs) serving smaller populations may report arrests with more error (e.g., less consistently), we limit our analysis sample to larger cities serving populations with at least 50,000 persons and conduct city- rather than state-level analyses (Chu and Townsend, 2019). Second, in all state-level regressions, we control for the number of LEAs that report arrests in each state-year. Third, we experiment with an alternate arrest measure, the *Arrest Ratio*, which is the ratio of arrests for a particular offense (i.e., marijuana arrests) relative to total arrests. This approach also has the advantage of more fully controlling for overall state-trends in law enforcement effort (e.g., policing), but at the potential cost of including arrests for offenses in both the numerator and the denominator of the dependent variable that could be affected by RMLs.

In addition, treating arrests as a proxy for crime will understate true levels of criminal behavior because not every crime results in an arrest or even a report to an LEA (Gould et al., 2002). However, there is a correlation in arrest reports from UCR and actual crimes when data are available on each (Lochner and Moretti, 2004).[24]

Nonetheless, the mechanisms through which RMLs affect arrests may differ from the channels through which RMLs affect criminal behavior. For instance, RMLs may impact the probability (positively or negatively) of a non-marijuana arrest by diverting law enforcement resources away from marijuana-related policing and toward detecting other offenses. We attempt to control for this police resource allocation shift by (1) adjusting for changes in non-marijuana-related arrests in the denominator of the *Arrest Ratio* measure, and (2) turning toward a second dataset, the National Incident-Based Reporting System (NIBRS), which will allow us to measure reported criminal incidents (whether or not they led to an arrest) to minimize RML-induced changes in policing resources as a mechanism to explain our findings.

For our arrest analysis, we first measure drug-related arrests involving adult arrestees. Such arrests include those for possession and sales of four categories of drugs: (1) marijuana, (2) powder cocaine, crack cocaine, heroin, and other opium derivatives, (3) truly addicting synthetic narcotics and (4) other dangerous non-narcotic drugs (i.e., methamphetamine). Forty-nine percent of all adult drug-related arrests are attributable to marijuana arrests.

In addition, we also measure Part I offenses, including *Violent Crime Arrests* (homicide, robbery, and aggravated assault) and *Property Crime Arrests* (larceny, burglary, motor vehicle theft, and arson).[25] Over the sample period, there were 5.6 drug-related arrests per 1,000 adult population, 1.8 violent arrests per 1,000 adult population, and 4.5 property arrests per 1,000 adult population. Appendix Table 1 shows weighted means of each of our arrest measures.

3.3. Treatment Episode Data Set (TEDS)

Finally, to explore the impact of RMLs on an alternate indicator of substance use disorders related to heavy marijuana use and use of other harder drugs, we turn to admission flows into substance abuse treatment facilities from the Treatment Episode Data Set (TEDS) over the period 2000–2018. Facilities receiving public funding are required to report data on clients admitted for substance abuse treatment to their state's reporting agency; these data are then compiled by TEDS. For each admission, we observe the primary, secondary, and tertiary substance related to the treatment admission, the personal characteristics of the patient, and the source of referral. Almost half of treatment referrals originate from within the criminal justice system. Other referrals are from the patients themselves, family and peers, and health care providers. We document treatment admissions for the following drug-related reasons: marijuana, cocaine, methamphetamine or amphetamine, heroin, other opioids, and alcohol.[26] State-by-year treatment admission rates (per 1,000 population) are calculated based on the SEER data and reported in Appendix Table 1. We find that over the sample period, there were 2.04 marijuana related primary treatment admissions per 1,000 population.

As with the UCR, there are well-known reporting issues with the TEDS. To address measurement error in drug treatment admissions, we (1) control for the overall drug treatment admissions rate for all substances in each specific drug treatment admissions regression,[27] and (2) in a robustness check, use the drug treatment admissions ratio (rate of individual drug treatment admission to total drug treatment admissions) as an alternate left-hand side variable.

3.4. Supplemental analyses using National Vital Statistics System (NVSS) mortality files

To supplement our primary data analyses using the NSDUH, UCR, and TEDS, we explore auxiliary analyses on mortality using data from the National Vital Statistics System (NVSS) multiple cause-of-death mortality files. These analyses also focus on the period 2000–2019 and measure overdose deaths per 100,000 population. We model specific overdose deaths related to the following International Classification of Disease, Tenth Revision (ICD-10) multiple cause-of-death codes: T40.5 (cocaine), T43.6 (methamphetamine), T40.1 (heroin), T40.4 (fentanyl), T40.0, T40.2, T40.3, T40.6 (non-heroin-, non-fentanyl-related opioids). In addition, drawing on a possible link between legalization of access to medical marijuana and suicide (Anderson et al., 2014; Bartos et al., 2020), we also extend our mortality analysis to

[23] Florida (a non-RML state) is excluded from the UCR analyses due to lack of reporting; data are available for only three of the 20 years over our sample period. We also exclude the District of Columbia and Wisconsin in the year 2000, as law enforcement agencies did not report arrest data for this year.

[24] Moreover, measurement error is unlikely to be correlated with adoption of RMLs, which suggests that our estimated marginal effects (at least in percentage terms) will be unbiased.

[25] The UCR program divides criminal offenses into Part I and Part II offenses. Part I offenses are more severe, occur with more regularity across the U.S., and more likely to be reported to law enforcement. Part II offenses are generally less serious and reported with less frequency, and include offenses such as fraud and "white collar" crimes, prostitution and solicitation, drunk driving, vandalism, violations of liquor laws, simple assaults (where no weapon was involved and no injury resulted to the victim), and such. Drug abuse violations are categorized as Part II offenses, and we study these separately from the Part I violent and property criminal offenses.

[26] These outcomes are based on whether the specific drug is mentioned as either the primary, secondary, or tertiary substance of abuse. Alternative measures which utilize only the primary drug of mention produce a similar pattern of results.

[27] Our results and conclusions are not sensitive to omitting this control for the overall drug treatment admissions rate.

Table 1
TWFE Estimates of Effect of RMLs on Prevalence of Adult Substance Use (NSDUH, 2002–2019).

	Marijuana Use (1)	Illicit Drug Use other than Marijuana (2)	Cocaine Use (3)	Non-Medical Use Pain Relievers (4)	Binge Drinking (5)
Panel I: Baseline Controls[a]					
RML	0.0402***	0.0015	0.0035**	−0.0037***	0.0065
	(0.0077)	(0.0012)	(0.0012)	(0.0010)	(0.0058)
Panel II: Added Substance Use and Welfare Policy Controls Policy Controls[b,c]					
RML	0.0401***	0.0001	0.0018	−0.0037**	0.0037
	(0.0062)	(0.0013)	(0.0012)	(0.0010)	(0.0066)
Panel III: Added Political Environment and Crime Policy Controls[d]					
RML	0.0403 ***	0.0005	0.0021	−0.0025	0.0051
	(0.0060)	(0.0013)	(0.0013)	(0.0013)	(0.0068)
N	867	816	867	816	816
Pre-Treatment Mean Dep Var	0.085	0.038	0.023	0.045	0.257

***p < .001, **p < .01, *p < .05.
Notes: Estimates are obtained using state-by-year data on substance use prevalence rates from the 2002–2019 National Survey on Drug Use and Health (NSDUH). All models include state and year fixed effects. Regressions are weighted by adult population ages 18 and older. Standard errors are clustered at the state level.
[a] Baseline controls include a control for medical marijuana laws (MMLs), marijuana decriminalization laws (MDLs) and state macroeconomic controls (unemployment rate and per capita GDP).
[b] Substance use policy controls include naloxone access laws, Good Samaritan laws, must-access prescription drug monitoring program, and beer taxes.
[c] Welfare Policy Controls include the state EITC refundable rate, the maximum Supplemental Nutrition Assistance Program (Food Stamp) benefit level for 4-person family and an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion.
[d] Political environment and crime-related controls include whether the governor is a Democrat, the natural log of law enforcement personnel per 1,000 population, the binding state minimum wage, and the racial composition of the state.

include suicides. The means of each of these measures are included in Appendix Table 1.

4. Empirical methods

We use a difference-in-differences (DD) regression strategy to estimate the effects of RMLs on harder drug use and crime. Specifically, we begin by estimating the following two-way fixed effects (TWFE) regression equation:

$$Y_{st} = \beta_0 + \beta_1 RML_{st} + \boldsymbol{X}_{st}\boldsymbol{\alpha} + \tau_t + \delta_s + \varepsilon_{st}, \quad (1)$$

where Y_{st} denotes the outcome of interest in state s in year t. These include the prevalence of substance use (NSDUH), the arrest rate per 1,000 population (UCR), and the rate of treatment admissions per 1,000 population (TEDS).[28]

The primary independent variable of interest, RML_{st}, measures the share of the year for each state during which a recreational marijuana law (RML) is in effect. In alternate specifications, we also separately include mutually exclusive indicators for RML_{st} and *Recreational Sales Allowed*$_{st}$, the latter being an indicator variable for the date at which marijuana sales were permitted in the state (see Anderson and Rees, 2023, Table 1). Doing so allows us to explore the separate and any delayed treatment effect from when legalization is enacted to when recreational sales began.[29]

The vector $\boldsymbol{X}_{st}$ first includes a set of baseline marijuana policy and economic controls, including the adoption of a medical marijuana law (MML), the adoption of a marijuana decriminalization or depenalization law (MDL)[30] and macroeconomic controls (unemployment rate and per capita GDP). We then augment these baseline controls with state-level policies related to substances that may be substitutes or complements for marijuana (the real beer tax per gallon, whether the state has a must-access prescription drug monitoring program, whether the state has a naloxone access law, and whether the state has a Good Samaritan law). Next, we add controls for social welfare policies that could affect substance use (the state EITC refundable rate, the maximum Supplemental Nutrition Assistance Program benefit level for 4-person family and an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion). Finally, we include controls related to the sociopolitical environment of the state (whether the governor is a Democrat, state minimum wage, and the racial composition of the state).[31]

Common (i.e., country-wide) shocks are accounted for by including year fixed effects on the right-hand side of equation (1), τ_t, while state fixed effects, δ_s, control for time-invariant state determinants of Y_{st}. All regressions are weighted by the state population, though we confirm that our findings are not materially different across weighted and unweighted estimates (discussed below). Standard errors are clustered at the state level (Bertrand et al., 2004).

Our key parameter of interest, β_1, is the reduced form effect of RML adoption on the outcome of interest, through all reinforcing and competing pathways, *ceteris paribus*. Such pathways could manifest on

[28] While our main analyses utilize the arrest rate as the requisite outcome, our findings and conclusions are not materially altered across alternate specifications: a) log of the rate; b) Poisson regression.
[29] While dispensaries are an important feature of the (medical and) recreational marijuana landscape, given that home cultivation is permitted in all but three of the RML states, one cannot a priori dismiss the possibility that supply (and consumption) of marijuana could have increased well before recreational sales through dispensaries became possible. We therefore explore the separate effects of both the enactment and the opening of marijuana distribution at the retail level in the RML states. We also allow for border state RML policies to affect neighboring states, as well as interactive effects of own and border-state RML policies, in alternate models to assess spillovers from RML states to bordering non-RML states.
[30] A marijuana decriminalization policy imposes "no arrest, prison time, or criminal record" for first-time possession of a small quantity of marijuana (not for sales, but rather for personal consumption). Thus, under this definition of decriminalization, marijuana possession remains illegal, but the legal system would not prosecute a person for possession under a specified amount. This generally means certain small, personal-consumption amounts are a civil or local infraction, and not a state crime, or are a low-level misdemeanor with no possibility of jail time. Instead, penalties would range from no penalties at all, civil fines, drug education, or drug treatment. A depenalization law reduces the penalty for a criminal offense (i.e., no jail time for a first offense). Over the sample period under study, only Missouri adopted a depenalization law; the remainder of states adopting such a law decriminalized marijuana. See Appendix Table 3 for a summary of the states that had enacted these laws, along with their effective dates.
[31] The means of each of these control variables are listed in Appendix Table 2.

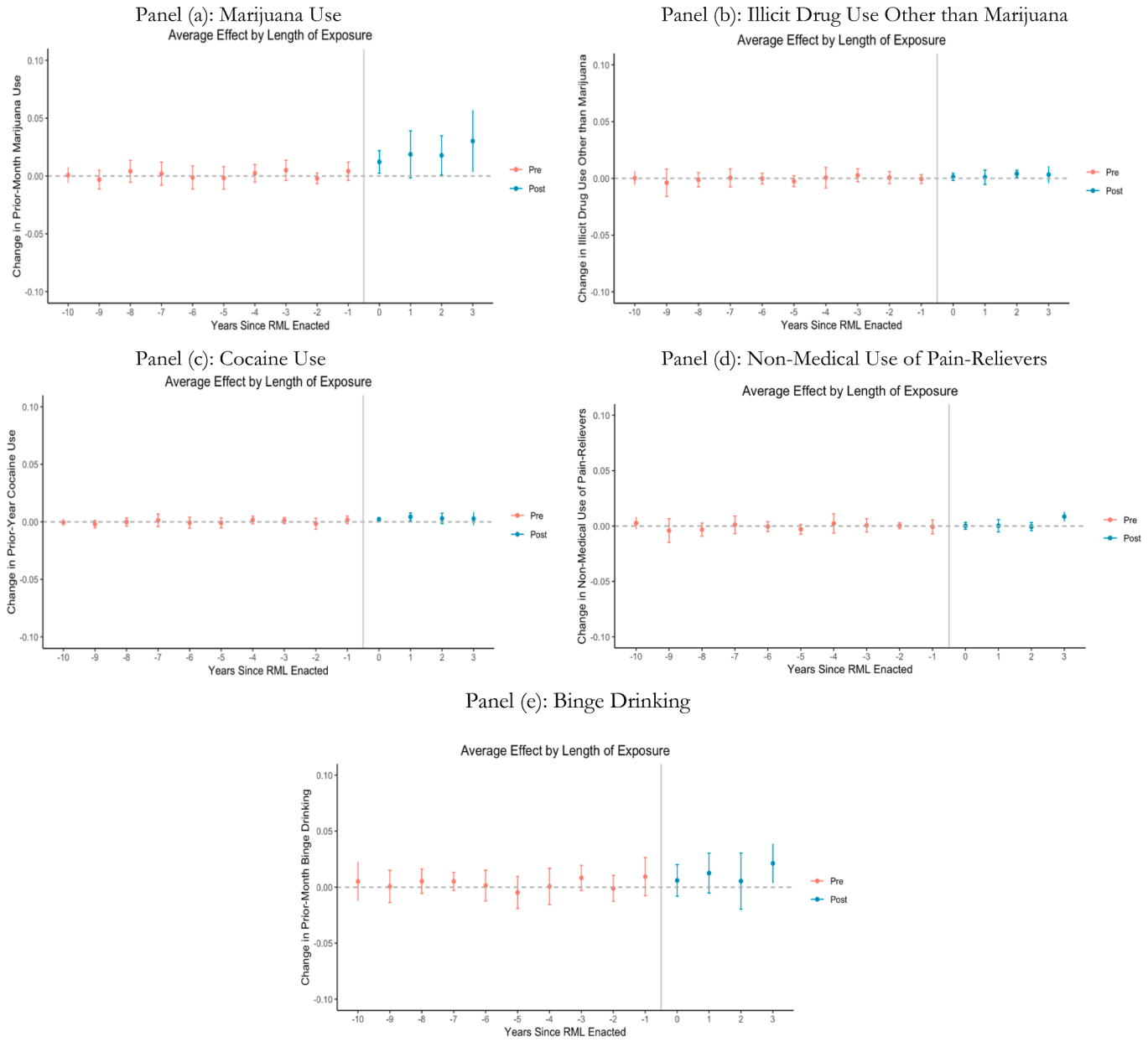

Fig. 1. Event Study Analysis of Recreational Marijuana Laws and Adult Drug Use, Using Callaway and Sant'Anna (2021) Estimates (Vertical lines show 95 % confidence intervals around Callaway and Sant'Anna (2021) estimates of the effect of an RML at each year relative to RML enactment. Estimates are weighted and include controls for state and year fixed effects, medical marijuana laws, marijuana decriminalization laws and state macroeconomic controls (unemployment rate and per capita GDP)).

either the demand- or supply-side of the recreational marijuana market or even in adjacent drug markets. With respect to spillover effects to non-marijuana-related arrests and outcomes related to harder drugs, we attempt to disentangle the effects of changes in (aggregate- and offense-specific) policing efforts in response to RML adoption. We do this through (1) our use of arrest ratios as an alternate dependent variable, (2) including observable controls for policing investments, and (3) using the NIBRS to measure Part I criminal incidents, whether or not such incidents lead to an arrest.

Our intent-to-treat estimate, β_1, is identified from within-state variation in recreational marijuana laws as outlined in Appendix Table 3. This table is restricted to states with RML effective dates spanning the period 2000–2019, which is the analysis period. Also noted in Appendix Table 3 is the date of MML adoption. Because all states that legalized recreational marijuana had previously legalized medical marijuana, the reduced form parameter β_1 captures the effect of RML adoption over and above MML adoption.[32]

We take several tacks to assess the credibility of the parallel trends assumption underlying our DD approach. First, we explore the sensitivity of our estimates to using a parsimonious set of controls versus a

[32] Without exception, RMLs allow for the sale of marijuana for recreational purposes, but there is no uniform approach to taxing these sales. Some RMLs introduce a price-based ad valorem tax while others use a weight-based excise tax. Of the states that had legalized recreational marijuana by March 31, 2021 (16), 11 follow a pure ad-valorem taxation approach, and the other five states levy an excise weight-based tax or a combination of an excise tax and an ad valorem tax. IL is the only state that levies a graduated ad valorem tax based specifically also on the marijuana product's THC content (≥35 % THC vs. <35 % THC). See https://taxfoundation.org/state-recreational-marijuana-taxes-2021/.

Table 2
TWFE Estimates of Effect of RMLs on Adult Drug Possession Arrests (UCR, 2000–2019).

	All Drugs (1)	Marijuana (2)	Heroin and Cocaine (3)	Truly Addicting Synthetic Narcotics (4)	Other Dangerous Non-Narcotics (5)
Panel I: State Arrest Rate[a]					
RML	−1.183***	−1.050***	0.3617**	−0.1809*	−0.3148*
	(0.3364)	(0.2912)	(0.1135)	(0.0875)	(0.1368)
Pre-Treatment Mean Dep Var	5.21	1.39	1.71	0.13	1.98
N	998	998	998	998	998
Panel II: State Arrest Rate[b]					
RML	−1.114***	−1.108***	0.2324	−0.1936**	−0.0455
	(0.2484)	(0.1853)	(0.1556)	(0.0594)	(0.1094)
Pre-Treatment Mean Dep Var	5.21	1.39	1.71	0.13	1.98
N	998	998	998	998	998
Panel III: State Arrest Ratio[b]					
RML	−0.0510***	−0.0433***	0.0018	−0.0050**	−0.0045
	(0.0072)	(0.0068)	(0.0029)	(0.0014)	(0.0030)
Pre-Treatment Mean Dep Var	0.121	0.032	0.040	0.003	0.045
N	998	998	998	998	998
Panel IV: Large City (≥50,000 Population) Arrest Ratio[b]					
RML	−0.0556**	−0.0787***	0.0069	−0.0084 (0.0047)	0.0119
	(0.0160)	(0.0143)	(0.0068)		(0.0086)
Pre-Treatment Mean Dep Var	0.269	0.049	0.096	0.001	0.121
N	22,495	22,495	22,495	22,495	22,495
Panel V: Large City (≥50,000 Population) Arrest Rate[b]					
RML	−0.4879***	−0.6292***	0.0354	−0.0923	0.1028
	(0.1017)	(0.0684)	(0.0625)	(0.0487)	(0.0707)
Pre-Treatment Mean Dep Var	4.68	0.745	1.75	0.019	2.16
N	22,495	22,495	22,495	22,495	22,495

***p < .001, **p < .01, *p < .05.

Notes: Estimates are obtained using state-by-year (panels I through III) or city-by-year (panels IV and V) data on drug possession arrests from the 2000–2019 Uniform Crime Reports. In panels I, II, and V, the dependent variable is the number of drug possession arrests per 1,000 population; in panels III and IV, the dependent variable is the ratio of drug possession arrests to total arrests for all offenses. All models include state and year fixed effects and the controls listed below. Regressions are weighted by adult population ages 18 and older. Standard errors are clustered at the state level.

[a] Regressions include baseline controls: the number of law enforcement agencies reporting arrests, medical marijuana laws (MMLs), marijuana decriminalization laws (MDLs), and state macroeconomic controls (unemployment rate and per capita GDP).

[b] Regressions include baseline controls plus naloxone access laws, Good Samaritan laws, must-access prescription drug monitoring program, beer taxes, welfare Policy Controls including the state EITC refundable rate, the maximum Supplemental Nutrition Assistance Program (Food Stamp) benefit level for a 4-person family, an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, whether the governor is a Democrat, the natural log of law enforcement personnel per 1,000 population, the binding state minimum wage, and the racial composition of the state.

more saturated regression model. Second, we augment the controls in equation (1) with state-specific linear time trends to account for the influence of unobservables that could be unfolding coincidentally with the adoption of RMLs and the outcomes under study:

$$Y_{st} = \beta_0 + \beta_1 RML_{st} + \boldsymbol{X}_{st}\boldsymbol{\alpha} + \tau_t + \delta_s + \delta_s{}^*t + \varepsilon_{st} \quad (2)$$

Third, developments in the DD literature suggest that in the presence of heterogeneous treatment effects over time, TWFE estimates of β_1 in a staggered adoption framework (equation (1) may be biased (Goodman-Bacon, 2021) and tests of pre-treatment trends in a TWFE-based event study framework may be unreliable (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). To assess and correct for these potential biases, we apply an alternate difference-in-differences estimator proposed by Callaway and Sant'Anna (2021). Using never-adopting states as controls for treated states — and thereby bypassing the problematic use of early adopters of RMLs as controls for later adopters — we estimate event studies using the Callaway and Sant'Anna (2021) estimates.[33] For these event studies, we evaluate the period from 10 years prior to RML adoption through 3 years after RML adoption. This allows us to test for common pre-trends, and assess potential biases in the TWFE estimates.

5. Results

5.1. Substance use

In column (1) of Table 1, we document the first-stage effect of RML adoption on adult marijuana use. Using a parsimonious set of controls (indicators for marijuana-related laws and a set of macroeconomic variables) in panel I, we find that RML adoption is associated with a 4.0 percentage-point increase in adult past-month marijuana use. This represents a 47 percent increase relative to the pre-treatment mean of marijuana use in RML-adopting states, which is substantially larger than has been documented for MML adoption (Chu, 2014; Wen et al., 2015; Choi et al., 2019; Hollingsworth et al., 2022; Sabia and Nguyen, 2018). The larger RML estimated effect is consistent with MMLs ostensibly targeting patients with specific qualifying conditions (e.g., those suffering from chronic pain or Alzheimer's disease).

When we move from a parsimonious to more saturated specifications (panels II and III), the estimated first-stage effect of RMLs on adult marijuana use is similar in terms of magnitude (panels II and III). Specifically, including policies related to substance use and social welfare as controls continues to yield an estimated RML effect on past-month marijuana consumption of 4.0 percentage points (panel II) as does including sociopolitical and crime policies (panel III).

[33] All Callaway and Sant'Anna (2021) estimates control for state and year fixed effects, medical marijuana laws, marijuana decriminalization laws and state macroeconomic controls (unemployment rate and per capita GDP).

When we take a dynamic difference-in-differences approach using Callaway and Sant'Anna estimators (panel a of Fig. 1), we find that our TWFE estimate does not appear to be biased due to heterogeneous and dynamic treatment effects. Furthermore, we find that adult marijuana use in soon-to-be-treated states was moving in tandem with adult use in the control states, which is consistent with the parallel trends assumption.[34]

In column (2), we explore potential spillover effects of RML adoption on illicit drug use other than marijuana. Across specifications, we find no evidence that RMLs generate net spillovers to illicit drug use other than marijuana. TWFE estimates show that RML adoption is associated with a statistically insignificant 0.01 to 0.15 percentage-point increase in illicit drug use other than marijuana. When we separately examine cocaine use (column 3), there is a small positive effect in the most parsimonious regression model, but the estimated RML effect becomes smaller in magnitude and is statistically indistinguishable from zero at conventional levels after including the full set of controls. There is no evidence that RML adoption affects binge drinking behavior among adults (column 5), with estimated treatment effects on the order of approximately 0.5 percentage point that are consistently statistically indistinguishable from zero.

With respect to non-medicinal use of prescription pain relievers (column 4), there is more consistent evidence of a decline in prescription opioid misuse following RML adoption (panels I and II). This pattern of results might suggest that RMLs are inducing demand-side substitution away from recreational opioid use. It is also consistent with illicit opioid sellers/prescribers shifting away from pushing pain relievers. However, we note that after including the full set of controls (panel III), there is little evidence that RML adoption significantly affects prescription opioid misuse.

The pattern of Callaway and Sant'Anna (2021) estimates shown in the event study analyses (Fig. 1) is consistent with parallel pre-treatment trends but provides little evidence that RML adoption significantly affected adult substance use beyond marijuana consumption.[35,36]. Drawing on the 95 percent confidence intervals from the saturated specifications (Panel III), we can rule out large increases exceeding 0.3 percentage points (8 percent) in other illicit drug use, 0.005 percentage points (0.1 percent) in non-medical use of pain relievers, and 1.8 percentage points (7 percent) in binge drinking.

One of the limitations of our NSDUH-based analyses is that they rely on self-reported survey data and could, therefore, reflect social desirability bias. Willingness to report substance use could, in fact, be correlated with RML adoption. To supplement these analyses, we next turn to administrative data on drug-related arrests, drug treatment admissions, and, in auxiliary analyses, mortality.

5.2. Drug-related arrests

In Table 2, we present estimated effects of RML adoption on arrests for drug possession, and corresponding estimates for drug sale in Table 3. It is important to note here that observed arrests for drug possession are a product of two conditions, using/possessing drugs and being apprehended. Thus, while shifts in arrests could reflect changes in underlying drug use behaviors, they are more likely reflecting changes in policing and illicit activity. This may be particularly the case for arrests related to drug sale, which are likely a more suitable proxy for black market activity.

Controlling for marijuana-related laws and a set of macroeconomic variables (panel I), we find that RML adoption is associated with a 1.18 per 1,000-person reduction in total drug possession arrests (23 percent reduction relative to the baseline mean; column 1), driven largely by a 1.10 per 1,000-person reduction in arrests for marijuana possession (column 2). Including the full set of controls (panel II) has little impact on these estimates. RML adoption is associated with a nearly 80 percent reduction in marijuana-related arrests relative to the pre-treatment mean, a result that is likely explained by changes in the legal status of possessing small quantities of marijuana.[37]

If we use the arrest ratio (i.e., the ratio of drug-specific arrests to arrests for all offenses) rather than the arrest rate (panel III), the pattern of findings is quite similar, suggesting that overall changes in policing resources cannot explain our results. We find that RML adoption is associated with a 4.3 percentage-point (134 percent) reduction in the ratio of marijuana possession arrests to total arrests (column 2).[38]

Finally, given well-known concerns about arrest reporting by smaller law enforcement agencies (LEAs), we follow Chu and Townsend (2019) by experimenting with restricting our analysis to large cities serving populations of at least 50,000. There is evidence that agencies serving populations of more than 50,000 are in more consistent communication with the FBI and more likely to report arrests each month (Lynch and Jarvis, 2008). Our results show that RML adoption is associated with a 7.9 percentage-point (161 percent) reduction in the ratio of marijuana possession arrests to total arrests (panel IV) and a 0.629 arrest per 1,000 adult population (84.5 percent) reduction in the marijuana possession arrest rate (panel V).

In columns (3) through (5) of Table 2, we explore whether there are spillover effects of RML adoption on arrests for the possession of harder drugs. First, we find some evidence that RML adoption is associated with a reduction in arrests among adults for possession of truly addicting synthetic narcotics (column 4). While less precisely estimated in large city regressions (panels IV and V) relative to state-level regressions, the pattern of results is quite similar. For instance, in our fully specified state-level arrest rate regression (panel II), we find that RML adoption is

[34] Our finding that the Callaway and Sant'Anna (2021) estimator is largely consistent with our TWFE estimator is also suggested by a formal Goodman-Bacon decomposition of the comparisons driving the estimated treatment effects in our marijuana analyses. This decomposition indicated that between 87.5 and 91.6 percent of the weight of our TWFE estimator can be attributed to the comparison of RML states versus never adopters and between 7.2 and 12.5 percent can be attributed to earlier versus later-adopting states. The potentially problematic comparison, which uses early-adopting states as a counterfactual for later-adopting states, never drives more than 1.3 percent of the identifying variation in deriving the treatment effects in our DD estimation. In the analyses that follow, we draw conclusions from the weight of the evidence across consistent patterns and credible event studies that emerge from the results, and where there is any divergence, we give preference to the Callaway-Sant'Anna estimation results.

[35] In a series of robustness checks available upon request, we have recoded the RML variable such that the treatment only turns on if (1) at least one full year of the two-year pair is treated, and (2) both years of the two-year pair are treated. The pattern of findings is qualitatively similar to those shown in our main tables of NSDUH results.

[36] In a series of robustness checks, we explore the sensitivity of estimated treatment effects and event study estimates to (1) dropping non-MML states from the control group, and (2) detrending drug use (using residuals from a fully specified model, including state-specific linear time trends). The results continue to show little evidence that RML adoption increases illicit drug use other than marijuana, non-marijuana-related arrests, arrests for Part I offenses, or drug treatment admissions.

[37] Another mechanism through which RMLs could be affecting marijuana arrests is through changes in police sentiment toward marijuana law enforcement. For instance, while marijuana sale/trafficking remains illegal, the decline in arrests related to MJ sale/trafficking could be due to a thinning illicit market (as legal sales crowd out illicit sales) and/or a shift in police sentiment toward enforcing drug-related offenses involving marijuana.

[38] Our central finding of RML-induced declines in marijuana arrests is not materially altered if we define the arrest ratio for specific drug-related arrests relative to all drug-related arrests (denominator); that is, not only are marijuana-related arrests declining overall as a result of legalization, but the share of marijuana arrests in total drug-related arrests is also significantly lower post-RML adoption.

Table 3
TWFE Estimates of Effect of RMLs on Adult Drug Sales (UCR, 2000–2019).

	All Drugs (1)	Marijuana (2)	Heroin and Cocaine (3)	Truly Addicting Synthetic Narcotics (4)	Other Dangerous Non-Narcotics (5)
Panel I: State Arrest Rate[a]					
RML	−0.2550*	−0.1304**	0.0848	−0.0544	−0.1550***
	(0.0987)	(0.0385)	(0.0429)	(0.0328)	(0.0396)
Pre-Treatment Mean Dep Var	1.10	0.32	0.41	0.04	0.34
N	998	998	998	998	998
Panel II: State Arrest Rate[b]					
RML	−0.1047	−0.1014*	0.1209	−0.0461	−0.0780**
	(0.0967)	(0.0423)	(0.0621)	(0.0234)	(0.0246)
Pre-Treatment Mean Dep Var	1.10	0.32	0.41	0.04	0.34
N	998	998	998	998	998
Panel III: State Arrest Ratio[b]					
RML	−0.0062**	−0.0033**	0.0010	−0.0014	−0.0024***
	(0.0018)	(0.0010)	(0.0007)	(0.0007)	(0.0006)
Pre-Treatment Mean Dep Var	0.026	0.007	0.010	0.0008	0.007
N	998	998	998	998	998
Panel IV: Large City (≥50,000 Population) Arrest Ratio[b]					
RML	−0.0198***	−0.0128***	−0.0016	−0.0017	−0.0045**
	(0.0047)	(0.0023)	(0.0016)	(0.0015)	(0.0015)
Pre-Treatment Mean Dep Var	0.046	0.014	0.012	0.0004	0.019
N	22,495	22,495	22,495	22,495	22,495
Panel V: Large City (≥50,000 Population) Arrest Rate[b]					
RML	−0.1966**	−0.1227***	−0.0084	−0.0244	−0.0486**
	(0.0591)	(0.0256)	(0.0247)	(0.0138)	(0.0159)
Pre-Treatment Mean Dep Var	0.789	0.229	0.215	0.004	0.339
N	22,495	22,495	22,495	22,495	22,495

***p < .001, **p < .01, *p < .05.

Notes: Estimates are obtained using state-by-year (panels I through III) or city-by-year (panels IV and V) data on drug possession arrests from the 2000–2019 Uniform Crime Reports. In panels I, II, and V, the dependent variable is the number of drug possession arrests per 1,000 population; in panels III and IV, the dependent variable is the ratio of drug possession arrests to total arrests for all offenses. All models include state and year fixed effects and the controls listed below. Regressions are weighted by adult population ages 18 and older. Standard errors are clustered at the state level.

All regressions are weighted by adult population ages 18 and older. Standard errors are clustered at the state level.

[a] Regressions include baseline controls: the number of law enforcement agencies reporting arrests, medical marijuana laws (MMLs), marijuana decriminalization laws (MDLs), and state macroeconomic controls (unemployment rate and per capita GDP).

[b] Regressions include baseline controls plus naloxone access laws, Good Samaritan laws, must-access prescription drug monitoring program, beer taxes, welfare Policy Controls including the state EITC refundable rate, the maximum Supplemental Nutrition Assistance Program (Food Stamp) benefit level for a 4-person family, an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, whether the governor is a Democrat, the natural log of law enforcement personnel per 1,000 population, the binding state minimum wage, and the racial composition of the state.

associated with a 0.194 reduction in possession arrests for synthetic narcotics per 1,000 population. Event study analyses using Callaway and Sant'Anna (2021) estimates in Fig. 2 suggest that this effect is likely causal.

The results discussed in the paragraph above are consistent with several hypotheses. First, marijuana and opioids may be substitutes, perhaps because both substances help users achieve a euphoric feeling ("high") or to treat pain or anxiety. They are also consistent with the results of studies by Bachhuber et al (2014) and Bradford and Bradford (2016, 2017, 2018). While this channel reflects a shift in underlying consumption, as noted above, shifts in arrests could also reflect other factors conditional on use. Thus, it may be that suppliers of illicit narcotics substitute away from marketing opioids, thereby reducing access to illicit opioids, and toward selling marijuana as part of the expanding cannabis market. Or, it could be that police are reducing total resources allocated to detecting all drugs. The strongest evidence against this third hypothesis is that we do not observe significant reductions in all non-marijuana related possession arrests, but only for narcotics, suggesting that the first two channels are more likely.

In our fully saturated regression specifications (Table 2, panels II through V), we find no evidence that RML adoption affects possession arrests for heroin or cocaine (column 3) or other dangerous non-narcotics (column 5).[39] While this result is consistent with evidence in Table 1 that RML adoption has little demand-side effect on consumption of illicit drugs other than marijuana, it could also suggest that any consumption effects (positive or negative) are, on net, offset by countervailing supply-side efforts. Event study analyses using Callaway and Sant'Anna (2021) estimates provide little support for the hypothesis that RML adoption significantly increases possession arrests for harder drugs and some evidence to support the hypothesis that RMLs may lead to a (modest) reduction in hard drug possession.

Turning to the arrests for drug sales (Table 3), a more salient proxy for how marijuana legalization impacted drug markets and underlying black market activity, we continue to find consistent evidence that RML adoption is associated with a significant reduction in all drug-related arrests (column 1). This is driven largely by a reduction in marijuana sales arrests among adults (column 2). RML adoption is associated with a

[39] The effect in Panel II is imprecisely estimated, and we cannot rule out relatively large increases in arrests for heroin and cocaine possession (0.5 arrests per 1,000 population, 31 percent relative to the baseline mean) with 95 percent confidence. Though, after adjusting for shifts in overall policing or limiting to agencies serving large cities (Panels III – V), the null becomes more precise, estimated confidence intervals can rule out positive spillovers into arrests for cocaine and heroin exceeding about 9–19 percent. For arrests involving other dangerous non-narcotics, confidence intervals in Panels II and III can rule out positive spillovers exceeding about 3–9 percent.

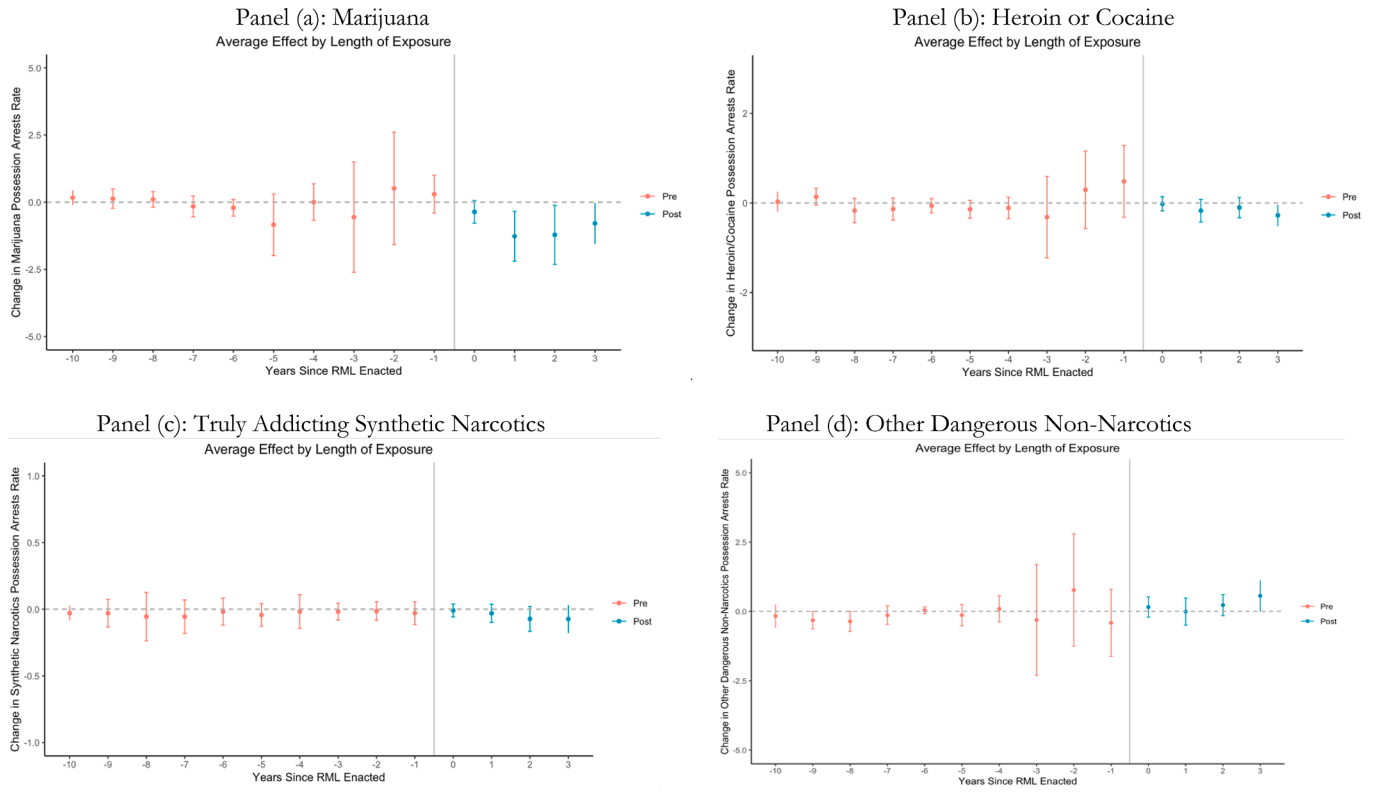

Fig. 2. Event Study Analysis of Recreational Marijuana Laws and Adult Drug Possession Arrest Rate, Using Callaway and Sant'Anna (2021) Estimates (Vertical lines show 95 % confidence intervals around Callaway and Sant'Anna (2021) estimates of the effect of an RML at each year relative to RML enactment. Estimates are weighted and include controls for state and year fixed effects, medical marijuana laws, marijuana decriminalization laws and state macroeconomic controls (unemployment rate and per capita GDP)).

0.101 per 1,000-person reduction in marijuana sales arrests, or about 33 percent relative to the pre-treatment mean. This decline in arrests related to marijuana sale/trafficking could be due to a thinning illicit market (as legal sales crowd out illicit sales) and/or a shift in police sentiment toward enforcing drug-related offenses involving marijuana.

In terms of spillover effects to sales of other drugs, we again find no evidence that RMLs lead to increases in harder drug sales arrests and some evidence that RML adoption is associated with a reduction in sales of harder drugs, including truly dangerous non-narcotics (column 5). This finding, which is also detected in panel (d) of Fig. 3, provides some support for the claim that supply may be substituting toward the licit drug and away from illicit harder drug sales.[40]

5.3. *Property and violent crime arrests*

The evidence presented thus far provides little support for the hypothesis that RML adoption leads to net increases in harder drug use or arrests for harder drug-related offenses. In Fig. 4 and Table 4, we explore the effects of RML adoption on property and violent crime arrests. Event study analyses using the Callaway and Sant'Anna estimator (2021) reported in panels (a) and (b) provide little support for the hypothesis that RML adoption leads to an increase in arrests for Part I offenses. Regression estimates reported in columns (1) and (2) of Table 4 show that RML adoption is associated with a *statistically insignificant* 0.061 to 0.080 (1.4 to 1.8 percent) increase in the property crime arrest rate (panel I) and a 0.057 to 0.104 (2.1 to 4.0 percent) increase in the violent crime arrest rate (panel II). If we instead use the ratio of adult property (violent) crime arrests to total arrests (column 3) to account for changes in overall policing resources (or changes in LEA reporting), the estimated treatment effects are actually negative and statistically indistinguishable from zero. Moreover, if we examine agencies reporting from large cities (column 4), we continue to find that RMLs have little effect on property and violent crime arrests, and estimated standard errors permit us to rule out increases in property or violent crime exceeding about 6–7 percent with 95 percent confidence.

Finally, we turn to the NIBRS to estimate the effect of RMLs on criminal incidents for property and violent offenses. These analyses capture the effects of RMLs on criminal offenses whether or not they result in an arrest, thereby netting out any potential policing effects. Our results, reported in columns (5) and (6), continue to show little evidence that RML adoption affects Part I criminal offenses.[41,42] As noted above, potential supply side responses in hard drug markets could also generate independent effects on violence and criminal activity due to greater competition over limited drug "turf"; that we do not observe any consistent or meaningful effects on these crimes is perhaps suggestive of muted supply-side responses in adjacent illicit drug markets.

[40] Appendix Table 4 presents estimates of drug-related arrest effects by gender and age with little evidence that the estimated RML effects are driven by those under or over the MLPA for marijuana or by males or females.

[41] Following most NIBRS-based studies (see, for example, Sabia et al., 2021, Fone et al., 2023), we use the law enforcement agency as the unit of observation and estimate Poisson regressions.

[42] Appendix Table 5 presents estimates of the effects of RML adoption on arrests (and marijuana versus non-marijuana drug treatment admissions) by gender, with little evidence that the estimates treatment effects are driven by one gender.

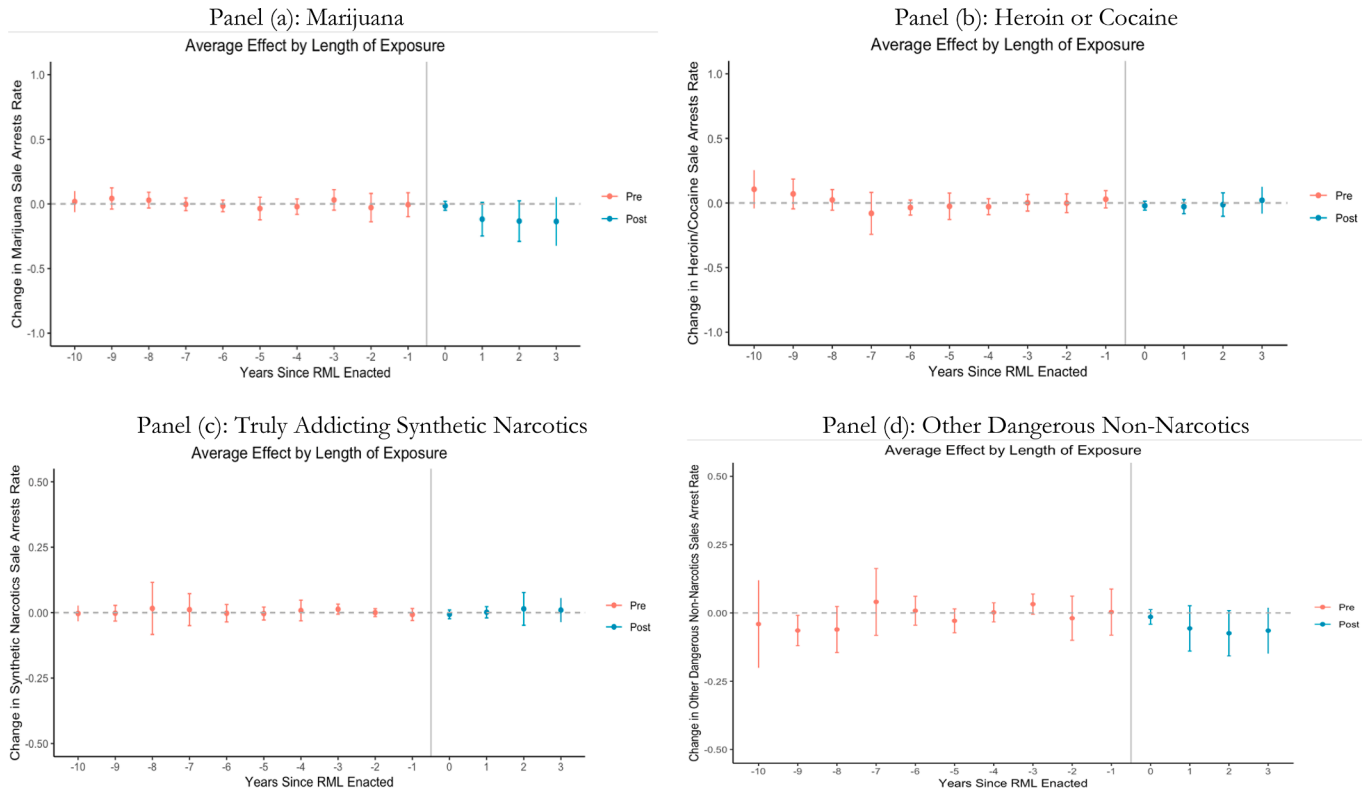

Fig. 3. Event Study Analysis of Recreational Marijuana Laws and Adult Drug Sales Arrest Rate, Using Callaway and Sant'Anna (2021) Estimates. (Vertical lines show 95 % confidence intervals around Callaway and Sant'Anna (2021) estimates of the effect of an RML at each year relative to RML enactment. Estimates are weighted and include controls for state and year fixed effects, medical marijuana laws, marijuana decriminalization laws and state macroeconomic controls (unemployment rate and per capita GDP)).

5.4. Drug treatment admissions

The evidence presented thus far provides little support for the hypothesis that that RML adoption increases hard drug use or crime. In Fig. 5 and Table 5, we explore the effect of RMLs on substance use-related treatment admissions, a proxy for substance use disorders and adverse events related to marijuana and other drug use.[43] We do not find any meaningful effects of marijuana legalization on flows into treatment facilities, associated with marijuana, cocaine, amphetamines, opioids or alcohol (Table 5). Effects are statistically insignificant and relatively small across most substances. In our fully specified regression model (panel II), we find that RML adoption is associated with a 0.464 per 1,000-person (53 percent) reduction in non-heroin-related opioid admissions, a result that is consistent with evidence that marijuana and prescription opioids may be substitutes in consumption and/or production. By contrast, event study analyses, provide little support for the hypothesis that RML adoption affects treatment admissions (Fig. 5).

5.5. Sensitivity checks

In Table 6, we explore the sensitivity of our estimated treatment effects (panel I) to the inclusion of state-specific linear time trends (panel II), to splitting the analysis sample to those under the minimum legal purchasing age for recreational marijuana (those ages 18 to 20, panel III) and those at or above the MLPA for marijuana (those ages 21 and older, panel IV), and in unweighted models (panel V).[44] For ease of presentation, when we examine drug-related outcomes, we focus on marijuana and illicit drug use other than marijuana.

We find consistent evidence that RML adoption increases adult marijuana use and reduces marijuana-related arrests (columns 1 and 3). However, across specifications and sample composition, there is little evidence that RML adoption generates statistically significant or economically important changes in hard drug use (column 2), non-marijuana-related drug arrests (column 4), Part I offense arrests (columns 5–6), or marijuana-involved treatment admissions (column 7).

In Table 7, we explore if the effects of RMLs on marijuana use, hard drug use, and arrests differ by whether a recreational dispensary was permissible and open (panel I) or whether a border state had adopted an RML (panel II). The opening of a recreational dispensary is associated with an increase in adult marijuana use that is approximately three times larger than the estimated effect of an RML without a dispensary (0.037 vs. 0.012), suggesting that dispensaries are an important licit channel through which adults obtain newly legalized recreational marijuana. However, we find no evidence that dispensary openings lead to increases in illicit drug use other than marijuana (column 2), non-marijuana drug arrests (column 4), property or violent crime arrests

[43] Treatment admissions likely also capture heavier substance use. In Appendix Table 6, we further disaggregate drug treatment admissions by primary drug, as well as by age (18-to-20 vs 21-and-older) and gender, with continued little evidence that RMLs significantly impact drug treatment admissions other than that for opioids; for opioids other than heroin, evidence points to significant declines in treatment admissions for adults above the MLPA and for both genders.

[44] Unweighted event study analyses, based on the Callaway Sant'Anna estimator, are presented in Appendix Fig. 1.

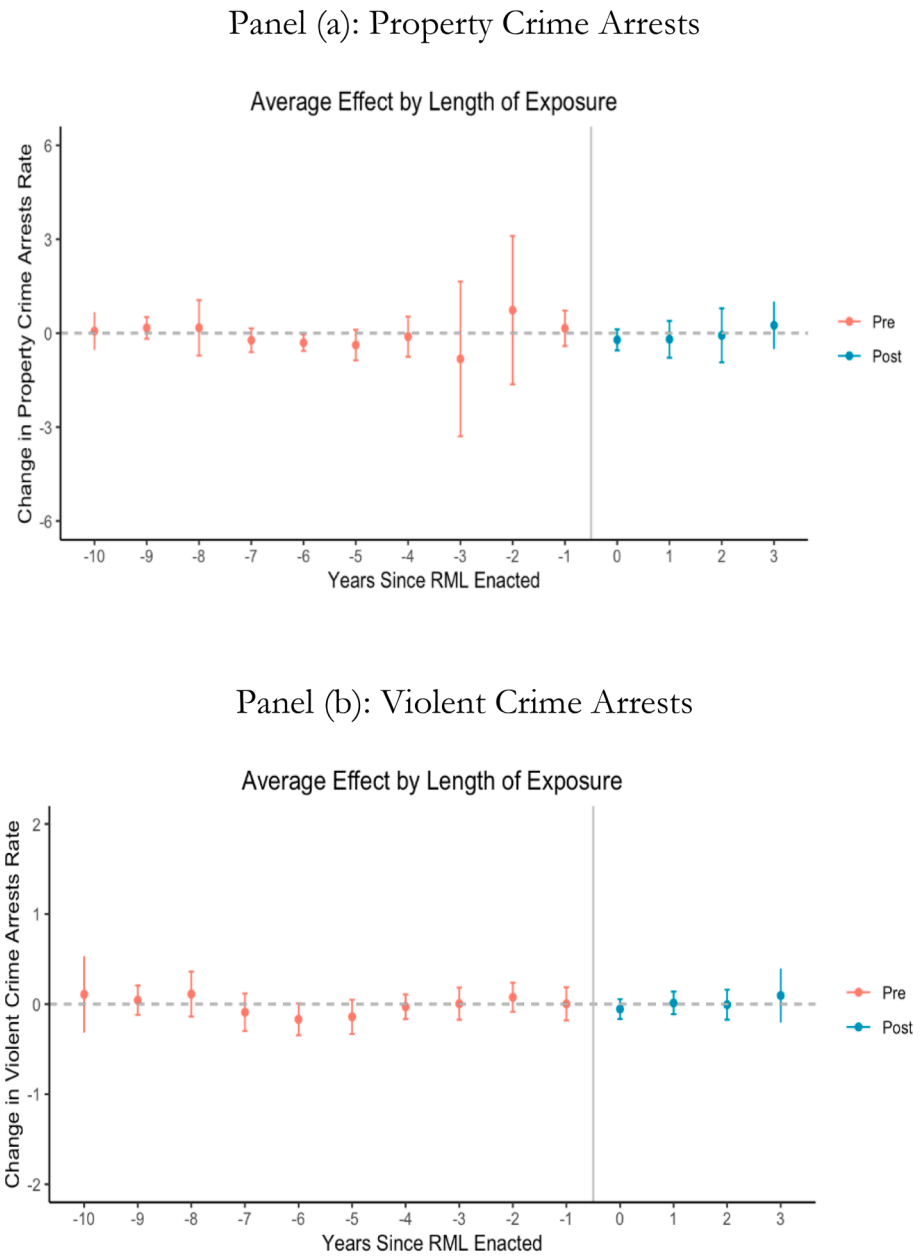

Fig. 4. Event Study Analysis of Recreational Marijuana Laws and Part I Offense Arrest Rates, Using Callaway and Sant'Anna (2021) Estimates. (Vertical lines show 95 % confidence intervals around Callaway and Sant'Anna (2021) estimates of the effect of an RML at each year relative to RML enactment. Estimates are weighted and include controls for state and year fixed effects, medical marijuana laws, marijuana decriminalization laws and state macroeconomic controls (unemployment rate and per capita GDP)).

(columns 5–6), or marijuana-involved drug treatment admissions (column 7).

Border-state RMLs have been found to be important for cross-border shopping (Hansen et al., 2020b), which could result in estimated RML effects to be biased toward zero (due to a contaminated control group). In panel II, we find some evidence that border-state RMLs reinforce the consumption effects of RML adoption. That is, the increase in adult marijuana use when a state adopts an RML and a border state adopts an RML is 3.6 percentage points higher (panel II, column 1). However, we find little evidence that border state RMLs affect hard drug use or crime (or have important interactive effects).[45]

Next, we use a synthetic control design to explore, in the most flexible way possible, whether there are heterogeneous effects of RML

[45] One exception is for violent crime arrests, where we observe that border state RMLs are significantly associated with the violent crime arrest rate in a state without an RML in place. This might reflect displacement or greater deployment in policing resources in states that border legalizing states.

Table 4
TWFE Estimates of Effect of RMLs on Adult Property and Violent Crime (UCR and NIBRS, 2000–2019).

	(1)	(2)	(3)	(4)	(5)	(6)
Panel I: Property Crime						
RML	0.0605	0.0804	−0.0035	−0.0888	0.0433	0.0741
	(0.2883)	(0.2353)	(0.0064)	(0.1426)	(0.0767)	(0.0486)
N	998	998	998	23,043	91,321	91,321
Pre-Treat Mean Dep Var	4.36	4.36	0.104	3.28	311.27	311.27
Panel II: Violent Crime						
RML	0.0570	0.1035	−0.0002	0.0659	−0.0090	0.0513
	(0.0891)	(0.0919)	(0.0020)	(0.0481)	(0.0863)	(0.0591)
N	998	998	998	22,495	91,321	91,321
Pre-Treat Mean Dep Var	2.74	2.74	0.065	2.44	80.9	80.9
Definition of Dep Var	Arrest Rate	Arrest Rate	Arrest Ratio	Arrest Rate	Incident Count	Incident Count
Source of Data	UCR	UCR	UCR	UCR	NIBRS	NIBRS
Estimation	OLS	OLS	OLS	OLS	Poisson	Poisson
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Drug & Welfare Controls	No	Yes	Yes	Yes	Yes	Yes
Police & Crime Controls	No	Yes	Yes	Yes	No	Yes
Jurisdiction-Level	State	State	State	City ≥ 50 K Pop	City/Agency	City/Agency

***p < .001, **p < .01, *p < .05.

Notes: Estimates are obtained using state-by-year (columns 1–3) or city-by-year (columns 3–6) data on property and violent crime from the Uniform Crime Reports and the National-Incident Based Reporting System. The arrest rate is calculated as the number of arrests per 1,000 adult population and the arrest ratio is calculated as the share of property or violent crime arrests to total arrests. For NIBRS-based analyses, the dependent variable is the law enforcement agency (LEA)-level count of criminal incidents and regressions control for local population served by the LEA. All models include state and year fixed effects, control for the number of law enforcement agencies reporting arrests, medical marijuana laws (MMLs), marijuana decriminalization laws (MDLs), state macroeconomic controls (unemployment rate and per capita GDP), naloxone access laws, Good Samaritan laws, must-access prescription drug monitoring program, beer taxes, welfare Policy Controls including the state EITC refundable rate, the maximum Supplemental Nutrition Assistance Program (Food Stamp) benefit level for a 4-person family, an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, whether the governor is a Democrat, the natural log of law enforcement personnel per 1,000 population, the binding state minimum wage, and the racial composition of the state. Regressions are weighted by adult population ages 18 and older. Standard errors are clustered at the state level.

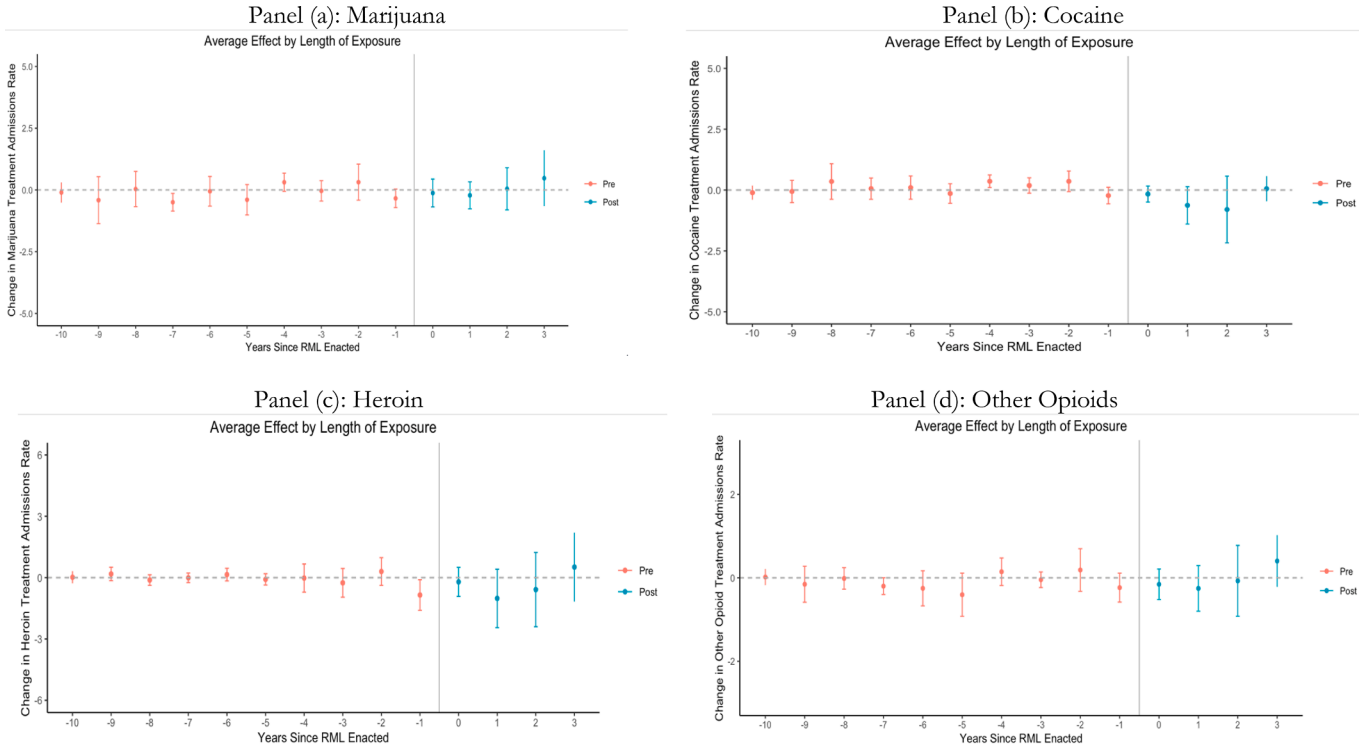

Fig. 5. Event Study Analysis of Recreational Marijuana Laws and Drug Treatment Admissions Rate, Using Callaway and Sant'Anna (2021) Estimates. (Vertical lines show 95 % confidence intervals around Callaway and Sant'Anna (2021) estimates of the effect of an RML at each year relative to RML enactment. Estimates are weighted and include controls for state and year fixed effects, medical marijuana laws, marijuana decriminalization laws and state macroeconomic controls (unemployment rate and per capita GDP)).

Table 5
TWFE Estimates of Effect of RMLs on Drug Treatment Admissions (TEDS, 2000–2018).

	Marijuana (1)	Cocaine (2)	Methamphetamine/Amphetamine (3)	Heroin (4)	Other Opioids (5)	Alcohol (6)
Panel I: Admissions Rate[a]						
RML	−0.1846	0.1000	0.2450	−0.1297	−0.3686	0.1200
	(0.1355)	(0.2337)	(0.2995)	(0.2383)	(0.2042)	(0.6165)
Pre-Treat Mean Dep Var	2.33	1.72	1.81	2.08	0.87	4.79
Panel II: Admissions Rate[b]						
RML	−0.0380	−0.0413	0.4318	−0.4462	−0.4640**	0.1432
	(0.1590)	(0.1890)	(0.2827)	(0.3350)	(0.1633)	(0.6281)
Pre-Treat Mean Dep Var	2.33	1.72	1.81	2.08	0.87	4.79
Panel III: Admissions Ratio[b]						
RML	−0.0135	0.0093	0.0033	0.007	−0.0081	−0.0177
	(0.0128)	(0.0082)	(0.0103)	(0.0125)	(0.0055)	(0.0193)
Pre-Treat Mean Dep Var	0.17	0.11	0.16	0.14	0.05	0.31
N	953	953	953	953	953	953

***p < .001, **p < .01, *p < .05.

Notes: Estimates are obtained using state-by-year data from the 2000–2018 Treatment Episode Dataset from 2000 to 2018. The dependent variable in panels I and II is the number of primary dug-specific admissions per 1,000 population. In panel III, the dependent variable is the ratio of drug specific treatment admissions to total drug admissions. All models include state and year fixed effects.

[a] Regressions include baseline controls: medical marijuana laws (MMLs), marijuana decriminalization laws (MDLs), state macroeconomic controls (unemployment rate and per capita GDP), and, in admissions rate regressions, the number of total admissions to account for changes in reporting.

[b] Regressions include baseline controls plus naloxone access laws, Good Samaritan laws, must-access prescription drug monitoring program, beer taxes, welfare Policy Controls including the state EITC refundable rate, the maximum Supplemental Nutrition Assistance Program (Food Stamp) benefit level for a 4-person family, an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, whether the governor is a Democrat, the natural log of law enforcement personnel per 1,000 population, the binding state minimum wage, and the racial composition of the state.

adoption on our key outcomes.[46] For instance, police departments may vary in their leniency toward those who commit drug offenses, perhaps due to local sentiment and drug policies. Given large changes in sentiment toward marijuana legalization over time (Saad, 2023), this could suggest that the earliest adopting RML states (i.e., Washington, Oregon) could see larger treatment effects while later-adopting states see a smaller effect (i.e., Massachusetts), perhaps because earlier adopting states experience larger differences in pre- and post-treatment outcomes in treatment and control states. To take another example, treatment effects could vary across RML states based on the magnitude and enforcement of marijuana taxes.

Our synthetic control analyses focus on states for which we observe a post-legalization window of at least three full years (Washington, Colorado, Alaska, Oregon, California, and Massachusetts), allowing us to explore longer-term effects of legalizing recreational marijuana.[47] We report the average effect over the post-treatment window, along with permutation-based p-values generated via placebo tests (Abadie et al., 2010).

Across each of the six RML states under study (see Appendix Figs. 2–7), synthetic control estimates provide generally consistent evidence of first-order effects (i.e., increases in marijuana use and reductions in marijuana-related arrests). There is some heterogeneity in the magnitudes of these estimated effects, with the largest increases in marijuana use and the largest declines in marijuana-related arrests generally observed for the four earliest adopters with relatively longer post-policy follow-ups (4.5 years or more).[48] RMLs decreased marijuana-related arrests across all six states, but there is heterogeneity in how other arrests evolved in the post-treatment period. Arrests for other drug arrests decreased in three states (Washington, Alaska, and Massachusetts), increased modestly in Colorado, and remained essentially unchanged (relative to the synthetic control) in Oregon and California. Likewise, the results for Part I offenses were decidedly mixed, suggesting that there was heterogeneity in how states diverted their policing resources across drug-related and non-drug related crimes. Spillover effects on hard drug use and drug treatment admissions are largely null or negative.

5.6. *Auxiliary analysis of drug overdose deaths*

Finally, in Appendix Table 7, we explore the effects of RML adoption on drug overdose deaths using NVSS data at the state level. We find little support for the hypothesis that RML adoption increases overdose deaths across a wide set of substances. In fact, Poisson regressions provide evidence that suggests that RML adoption reduces opioid-related deaths (panels I and II). This pattern of results is consistent with evidence from the NSDUH, UCR, and TEDS that marijuana and opioids are substitutes in consumption.

[46] We construct the synthetic counterfactual for each treated state by matching on the outcome in all pre-treatment periods. To construct a counterfactual that is not contaminated from the effects of post-RML MML adoption by donors, we select the donor pool from within the non-RML states that also did not enact any MML in the post-treatment period. For instance, for Colorado, which enacted an RML on December 10, 2012, the donor pool consists of states that have not adopted any RML over the sample period and not adopted any MML post-2012. Results are not sensitive to using a more restrictive donor pool, where we exclude any state that enacted an MML in the three years prior to the treatment state's RML adoption or anytime later. For each RML state, to facilitate the state-year synthetic control analyses, we assign the treatment period t based on the majority of the year in which the RML became effective. Thus, if an RML is enacted in year t, in June or earlier, we denote all years starting with t and later as treated; otherwise, we denote all years starting with t+1 and later as treated. For analyses of arrests, we drop Florida from the donor pool since UCR data are missing for the state from 2000 to 2016. The number of states in the donor pool ranges from 23 to 35. Finally, we also explore synthetic control analyses where we omit border states from the donor pool to avoid contamination from cross-border spillovers (Hansen et al. (2020b). The pattern of findings is similar to those presented in the appendix figures.

[47] We exclude the District of Columba from this analysis because the constructed synthetic controls for various outcomes showed a poor pre-policy match. We also do not present estimates from the TEDS since it is an unbalanced panel.

[48] We note that the permutation-based p-values on our donor pool of 23 to 35 states impose a high standard for achieving statistical significance, with the treatment effect having to rank at the very top (or rank among the top 2 or 3) along the distribution of placebo tests to achieve 5 (or 10) percent statistical significance.

Table 6
Sensitivity of Estimates to Inclusion of State-Specific Linear Time Trends, Allowing Heterogeneous Effects by Age, and Unweighted Specifications.

	Marijuana (MJ) Use (1)	Illicit Drug Use Other than MJ (2)	MJ Arrest Rate (3)	Non-MJ Drug Arrest Rate (4)	Property Crime Arrest Rate (5)	Violent Crime Arrest Rate (6)	MJ Admission Rate (7)
Panel I: TWFE Estimates							
RML	0.0403***	0.0005	−1.2095***	−0.0100	0.0804	0.1035	−0.0380
	(0.0060)	(0.0013)	(0.1913)	(0.2059)	(0.2353)	(0.0919)	(0.1590)
N	867	816	998	998	998	998	953
Pre-Treat Mean Dep Var	0.083	0.037	1.70	4.59	4.36	2.74	2.33
Panel II: Added State-Specific Linear Time Trends							
RML	0.0204**	0.0013	−1.255***	−0.5382*	0.2706	−0.1483	0.2193
	(0.0072)	(0.0015)	(0.2227)	(0.2103)	(0.2827)	(0.1044)	(0.1153)
N	867	816	998	998	998	998	953
Pre-Treat Mean Dep Var	0.083	0.037	1.70	4.59	4.36	2.74	2.33
Panel III: Ages 18 to 20[a]							
RML	0.0427***	−0.0029	−3.750***	−0.1973	−1.136	0.1639	0.0278
	(0.0097)	(0.0034)	(1.067)	(0.5397)	(0.7066)	(0.2599)	(0.3284)
N	867	816	998	998	998	998	953
Pre-Treat Mean Dep Var	0.204	0.082	8.72	7.86	15.70	6.51	5.30
Panel IV: Ages 21 and Older[b]							
RML	0.0403***	0.0011	−1.079***	−0.0016	0.1575	0.1025	−0.0560
	(0.0057)	(0.0011)	(0.1594)	(0.1968)	(0.2338)	(0.0833)	(0.1567)
N	867	816	998	998	998	998	953
Pre-Treat Mean Dep Var	0.061	0.029	1.27	4.39	3.67	2.52	2.15
Panel V: Unweighted TWFE Estimates							
RML	0.0457***	0.0009	−1.360***	−0.3344	−0.2191	0.0435	0.1179
	(0.0055)	(0.0013)	(0.2652)	(0.2771)	(0.2812)	(0.0941)	(0.1567)
N	867	816	998	998	998	998	953
Pre-Treat Mean Dep Var	0.083	0.037	1.70	4.59	4.36	2.74	2.33

***p < .001, **p < .01, *p < .05.

Notes: Estimates are obtained using state-by-year data from the NSDUH, UCR, and TEDS from 2002–2019, 2000–2019, and 2000–2018, respectively. Admissions rates and arrest rates are calculated per 1,000 population. All models include state and year fixed effects, control for the number of law enforcement agencies reporting arrests, medical marijuana laws (MMLs), marijuana decriminalization laws (MDLs), state macroeconomic controls (unemployment rate and per capita GDP), naloxone access laws, Good Samaritan laws, must-access prescription drug monitoring program, beer taxes, welfare Policy Controls including the state EITC refundable rate, the maximum Supplemental Nutrition Assistance Program (Food Stamp) benefit level for a 4-person family, an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, whether the governor is a Democrat, the natural log of law enforcement personnel per 1,000 population, the binding state minimum wage, and the racial composition of the state. Regressions are weighted by adult population ages 18 and older. Standard errors are clustered at the state level.

[a] For publicly available NSDUH data (columns 1 and 2), younger adults for whom data are publicly available are ages 18 to 25. For all other outcomes (columns 3 through 8), outcomes are measured for those ages 18-to-20.

[b] For publicly available NSDUH data (columns 1 and 2), older adults for whom data are publicly available are ages 26-and older. For all other outcomes (columns 3 through 8), outcomes are measured for those ages 21 and older. [c]Non-Marijuana Admissions Rates includes drug treatment admissions for primary drugs other than marijuana.

Table 7
Heterogeneity in Effects of RMLs, by Recreational Dispensary Opening and Border State Policies.

	Marijuana (MJ) Use (1)	Illicit Drug Use Other than MJ (2)	MJ Arrest Rate (3)	Non-MJ Drug Arrest Rate (4)	Property Crime Arrest Rate (5)	Violent Crime Arrest Rate (6)	MJ Admission Rate (7)
Panel I: Recreational Dispensary Opening							
RML with Dispensary Opening	0.0374***	0.0002	−0.7053**	0.1666	0.0921	0.1088	0.0427
	(0.0096)	(0.0012)	(0.2629)	(0.1572)	(0.2467)	(0.0619)	(0.1500)
RML without Dispensary Opening	0.0123	0.0014	−0.6616**	−0.1277	0.0705	0.0538	−0.1064
	(0.0087)	(0.0019)	(0.2453)	(0.1782)	(0.1895)	(0.0928)	(0.1051)
N	867	816	998	998	998	998	953
Pre-Treat Mean Dep Var	0.083	0.037	1.70	4.59	4.36	2.74	2.33
Panel II: Border State RML							
RML	0.0403***	0.0005 (0.0013)	−1. 209***	−0.0100 (0.2059)	0.0804 (0.2353)	0.1035 (0.0919)	−0.0380
	(0.0060)		(0.1913)				(0.1590)
Border State RML	0.0061	−0.0009	−0.1709	−0.0217	0.1521 (0.1690)	0.1968**	−0.2437
	(0.0040)	(0.0007)	(0.2680)	(0.1620)		(0.0698)	(0.1457)
RML*Border State RML	0.0358***	−0.0010	−0.9803***	−0.0458	−0.3627 (0.2083)	−0.0162	−0.1151
	(0.0087)	(0.0013)	(0.2211)	(0.2454)		(0.0830)	(0.1554)
N	867	816	998	998	998	998	953
Pre-Treat Mean Dep Var	0.083	0.037	1.70	4.59	4.36	2.74	2.33

***p < .001, **p < .01, *p < .05.
Notes: Estimates are obtained using state-by-year data from the NSDUH, UCR, and TEDS from 2002–2019, 2000–2019, and 2000–2018, respectively. Admissions rates and arrest rates are calculated per 1,000 population. All models include state and year fixed effects, control for the number of law enforcement agencies reporting arrests, medical marijuana laws (MMLs), marijuana decriminalization laws (MDLs), state macroeconomic controls (unemployment rate and per capita GDP), naloxone access laws, Good Samaritan laws, must-access prescription drug monitoring program, beer taxes, welfare Policy Controls including the state EITC refundable rate, the maximum Supplemental Nutrition Assistance Program (Food Stamp) benefit level for a 4-person family, an indicator for whether the state has implemented an Affordable Care Act Medicaid expansion, whether the governor is a Democrat, the natural log of law enforcement personnel per 1,000 population, the binding state minimum wage, and the racial composition of the state. Regressions are weighted by adult population ages 18 and older. Standard errors are clustered at the state level.
[c]Non-Marijuana Admissions Rates includes drug treatment admissions for primary drugs other than marijuana.

6. Conclusions

In this study, we comprehensively examine the effects of RML adoption on harder drug use and crime. Across analyses of four national datasets (the NSDUH, the UCR, the NIBRS, and the TEDS), we find little evidence to suggest that RMLs increase illicit drug use, arrests for Part I offenses, or drug-related treatment admissions for addiction. By contrast, we do find evidence, albeit tentative, that RML-adoption reduces opioid misuse and arrests. In the main, a causal interpretation of our findings is supported by event study analyses, including those that account for heterogeneous policy effects over time.

Regarding limitations of our analyses, we note that our estimates represent average treatment effects on the treated over the post-treatment windows available for our RML states. On average, these windows are approximately three to four years. If gateway effects take longer to unfold, future research will be necessary to detect them. Nonetheless, our synthetic control analyses of Washington and Colorado suggest that such long-run gateway effects have not materialized in the eight years following their adoption. In addition, it is certainly possible that nuisance crimes, those not captured by Part I offenses, could themselves be impacted by RMLs, which could be a useful path for future work. Finally, an important area of scholarship in this space includes whether RMLs generate adverse effects on marijuana use to minors. While early evidence is mixed on the impact of marijuana liberalization on youth use (Hollingsworth et al., 2022; Anderson et al., 2019, 2021; Cerdá et al., 2017), this question remains open for researchers to tackle. The research design we follow in this study provides an important and useful starting point for extending these analyses as more data become available and marijuana markets in legalized states evolve and mature.[49]

The findings of our study are of clear importance to policymakers and the public. The making of sound public policy depends, in part, upon carefully weighing the explicit costs of legalizing recreational marijuana against the utility gains from consumption and any savings from, for instance, reductions in crime or non-rational addition. Our results provide important evidence on both sides of this social welfare ledger.

[49] See Clemens et al. (2021), Currie et al. (2020), and Christensen and Miguel (2018) for a discussion of the value of pre-committed research designs and transparency in quasi-experimental policy analysis.

CRediT authorship contribution statement

Joseph J. Sabia: Conceptualization, Data curation, Writing – original draft, Writing – review & editing, Investigation, Validation, Methodology, Supervision. **Dhaval Dave:** Conceptualization, Data curation, Writing – original draft, Writing – review & editing, Investigation, Validation, Methodology. **Fawaz Alotaibi:** Formal analysis, Visualization. **Daniel I. Rees:** Conceptualization, Writing – original draft, Writing – review & editing, Investigation, Validation.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Appendix A. Supplementary analyses

Supplementary analyses and auxiliary figures and tables to this article can be found online at https://doi.org/10.1016/j.jpubeco.2024.105075.

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