Journal of Health Economics 88 (2023) 102728

Contents lists available at ScienceDirect

Journal of Health Economics

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

Marijuana legalization and opioid deaths☆

Neil K. Mathur [a], Christopher J. Ruhm [b,*]

[a] Department of Economics, University of Virginia, United States
[b] Frank Batten School of Leadership & Public Policy, University of Virginia and National Bureau of Economic Research, 235 McCormick Road, Charlottesville, VA 22903-4893, United States

ARTICLE INFO

Keywords:
Marijuana
Cannabis
Marijuana legalization
Opioid deaths
Medical marijuana
Recreational marijuana

ABSTRACT

Many states have legalized marijuana over the last two decades, initially for medical purposes and more recently for recreational consumption. Despite prior research, it remains unclear how these policies are related to rates of opioid-involved overdose deaths, which have trended rapidly upwards over time. We examine this question in two ways. First, we replicate and extend previous investigations to show that the prior empirical results are frequently fragile to the choice of specifications and time periods, and probably provide an overly optimistic assessment of the effects of marijuana legalization on opioid deaths. Second, we present new estimates suggesting that legal medical marijuana, particularly when available through retail dispensaries, is associated with higher opioid mortality. The results for recreational marijuana, while less reliable, also indicate that retail sales may be correlated with greater death rates relative to the counterfactual of no legal cannabis. A likely mechanism for these effects is the emergence of illicit fentanyl, which has increased the riskiness of even small positive effects of cannabis legalization on the consumption of opioids.

More than 930,000 Americans died of drug overdoses from 1999 to 2020 (Hedegaard et al., 2021). A large majority of these involved opioids. In response, there have been multiple federal, state, and local efforts to reduce opioid deaths and related problems. These include: better tracking of prescribing through drug monitoring programs; improved access to non-opioid pain care, naloxone, and medications treating opioid use disorder; assistance to high-risk persons following release from incarceration; physician and prescriber education programs; improved data surveillance; Good Samaritan laws that reduce barriers to calling for help during opioid emergencies; and multiple federal grant programs that provide states and local governments with assistance in funding these and related endeavors (Purington 2019; Harris and Mukkamala 2020; Katcher and Ruhm 2021).

☆ We thank Arnold Ventures for financial support, the authors of some papers we replicate for providing us with data, Ashley Bradford, David Bradford and Rosalie Pacula for advice and guidance on marijuana legalization policies, and Bradley Katcher for research assistance. We also thank participants of the 5th Annual Caribbean Health Economics Symposium, the 2022 American Economic Association annual meeting, and workshop participants at George Mason University, Monash University, the University of Sydney, and the University of Wisconsin for helpful comments on earlier versions of this paper. Ruhm has served as a plaintiff's consultant and Mathur has provided support to defendants in ongoing opioid litigation. Neither role was related to any aspect of this study. The views expressed herein are those of the authors only.

* Corresponding author.
E-mail address: ruhm@virginia.edu (C.J. Ruhm).

https://doi.org/10.1016/j.jhealeco.2023.102728
Received 13 September 2022; Received in revised form 2 January 2023; Accepted 4 January 2023
Available online 6 January 2023

At the same time, policies not directly related to opioid use or deaths may affect these outcomes. An important potential example are state laws legalizing the consumption and retail sale of medical or recreational marijuana.[1] Prior to 1999, the first year analyzed below, just three states (California, Oregon, and Washington) had legalized medical marijuana and none permitted sales through retail dispensaries. By the end of 2019, the last year studied, 33 states allowed medical cannabis, 29 with dispensaries in place, 11 states permitted recreational marijuana, and eight of these had operating retail dispensaries.[2]

A rapidly growing body of scholarship examines the relationship between marijuana legalization and various aspects of public health. In their review, Anderson and Rees (Forthcoming) state that four such articles were published in major medical, public health, and economics journals in 2013 but that by 2020 the total exceeded 140. These analyses cover a wide variety of topics including effects on the consumption of marijuana itself (particularly among youths), alcohol use, traffic fatalities, and crime. There has been more limited study of the consequences for opioid-related outcomes such as prescribing behavior (Bradford and Bradford 2016; Bradford et al., 2018; Wen and Hockenberry 2018; McMichael et al., 2020) and admissions to substance abuse treatment programs, emergency departments, or hospitals (Chu 2015; Powell et al., 2018; Conyers and Ayres 2020; Jayawardhana and Fernandez 2021).[3]

A few researchers have examined how marijuana legalization is related to opioid deaths. These studies, some of which are summarized in the next section, have been influential. Particularly prominent is the Bachhuber et al. (2014) conclusion that "medical cannabis laws are associated with significantly lower state-level opioid overdose mortality rates." This article has been widely cited,[4] and played an important role in arguments that led some states to approve medical marijuana as a treatment for opioid use disorder (Shover et al., 2020).[5] However, as discussed below, these findings turn out not to be robust to changes in the analysis period, with subsequent research yielding ambiguous or deleterious associations.

Our study provides more-definitive information on the relationship between marijuana legalization and opioid deaths. We first show that past empirical results are frequently fragile: in most cases, the inclusion of more comprehensive controls, longer analysis periods, and more correctly defined treatment variables results in less beneficial or more deleterious predicted effects of legal cannabis. We then present new estimates, from generalized difference-in-differences (DiD) and event study (ES) models, that incorporate more-recent data and a variety of other improvements and extensions to previous research. These results suggest that legal medical marijuana, particularly when available through retail dispensaries, is associated with *higher* opioid death rates. The estimates for recreational cannabis while less reliable – likely because most such policies have been only recently enacted and in fewer states than for medical marijuana – also hint that retail sales through dispensaries could be associated with greater opioid mortality, relative to the counterfactual of no legal cannabis.

There also is likely to be some heterogeneity across demographic groups, with evidence of stronger deleterious recreational marijuana effects for males, nonwhites, and young adults than for their counterparts. We further show that relatively favorable findings observed when analyzing deaths from 1999 to 2010 may reflect idiosyncratic and unreliable results obtained when considering short time periods rather than, as suggested by some researchers, changes over time in the stringency of the regulatory approaches. Instead, we provide evidence that cannabis legalization leads to relatively large increases in deaths involving synthetic opioids, particularly during the current era of widespread illicit fentanyl.

1. Estimated legalization effects from prior research are ambiguous

It is theoretically ambiguous whether cannabis legalization will raise or lower deaths involving opioids. If marijuana and opioids are substitutes, legalization of the former will reduce consumption of the latter, while the reverse will be true if they are complements. However, even if legal marijuana decreases overall opioid use, fatalities involving opioids could rise. For instance, this might occur if opioid consumption falls among those at low risk of death while it rises for those with higher risk. A plausible scenario for this would be if marijuana substitutes for legal opioids but complements the use of those obtained through illicit markets. The effects could also vary with the drug environment. For example, in recent years, fentanyl has become dominant in illicit drug markets, dramatically increasing the dangers of using such products since fentanyl is so potent and its dosage in street drugs is variable.[6] Given this, the relationship between legal marijuana and opioid-involved deaths is an empirical question.

Bachhuber et al. (2014) used public-use *National Vital Statistics System* (*NVSS*) data from 1999 to 2010 to examine the relationship between medical marijuana legalization (MML) and opioid deaths.[7] Their estimates suggest that MML reduced age-adjusted opioid analgesic mortality by almost 25% and a broader measure of opioid deaths by 23%, in models with state and year fixed effects, and

[1] We use the term "marijuana" to refer to all types of cannabis products, although we sometimes equivalently refer to cannabis. Marijuana remains an illegal Schedule 1 drug under the federal Controlled Substances Act, placing it in the same category as heroin, LSD, and ecstasy (drugs with no medical value and high potential for abuse).

[2] States covered by these policies contained 66.0%, 58.0%, 24.5% and 20.9% of the U.S. population in 2019 respectively.

[3] These studies, which are examples of the larger related literature, generally suggest that marijuana legalization reduces opioid prescribing, but with mixed effects for other outcomes.

[4] As of December 2022, Bachhuber et al. (2014) had been cited more than 800 times according to Google Scholar.

[5] Recreational marijuana is only legal for adults. However, medical marijuana is permitted, and its use is rapidly growing, among children (Aran and Cayam-Rand 2020), with parent groups sometimes advocating medical marijuana as a treatment for pediatric health issues (Swyter, Talamo, and Kelley 2015).

[6] Fentanyl is up to 50 times stronger than heroin and 100 times more powerful than morphine.

[7] The starting year of analysis is 1999 in this and most investigations because the ICD-9 cause of death coding system used before 1999 is not fully comparable to the ICD-10 codes employed beginning in that year.

with attenuation of the prescription opioid effect to 18% when state time trends were also controlled for. However, this result is sensitive to the analysis period. Shover et al. (2019) replicated Bachhuber's analysis and obtain a similar 21% reduction over the 1999–2010 timespan, but they demonstrate that the relationship reverses when extending the investigation through 2017, with medical cannabis legalization predicting a 23% *increase* in prescription opioid deaths over this longer period.

The innovation of Powell et al. (2018) was to distinguish between the legalization of medical marijuana and the availability of retail sales through operating medical marijuana dispensaries (MMD). Using restricted *NVSS* data, they confirmed Bachhuber's (2014) negative relationship between legalization of medical marijuana and opioid deaths from 1999 to 2010 but, consistent with Shover et al. (2019), showed that the effects weaken and become statistically insignificant when extending the period through 2013. However, their main finding was that the availability of medical marijuana sales through retail dispensaries is associated with a 28% reduction in deaths involving prescription opioids or heroin, relative to states with legal medical cannabis but without dispensaries.

Using similar methods and data for 1999–2017, Chan et al. (2020) added controls for the legalization of *recreational* marijuana (RML) as well as corresponding operating dispensaries (RMD). In their preferred specification, which limits analysis to 28 states, the coefficient on recreational marijuana dispensaries is −0.23 and significant at the 10 percent level, which they interpreted to imply a 21% decrease in opioid death rates.[8] However, this conclusion depends critically on the counterfactual comparison. Specifically, the corresponding RML coefficient is 0.19, indicating that while RMD reduces predicted opioid mortality rates by 21% compared to an otherwise equivalent state that legalized recreational marijuana but without retail sales, the decrease is just 4% relative to one not allowing any type of recreational cannabis.[9]

In recent work, Sabia et al. (2021) used data from 2000 to 2019 to examine how the legalization of recreational marijuana is related to a variety of outcomes, including opioid mortality rates. They provide suggestive evidence of beneficial effects, but the estimates are attenuated and frequently became statistically insignificant or reverse sign with the inclusion of more comprehensive controls or if recreational marijuana sales, rather than legalization, are used as the treatment variable.

The aforementioned studies examined *annual state-level* data and estimated difference-in-differences models. By contrast, Smith (2020) collected *monthly county-level* data on marijuana dispensaries, from 1999 to 2014, and estimated that their availability reduces opioid-related mortality by 11%. This analysis did not distinguish between medical and recreational marijuana dispensaries, nor did it separately consider legalization without retail sales. Another study using county-level data (Hsu and Kovács 2021) found that increased numbers of (medical or recreational) marijuana dispensaries predict lower opioid deaths. However, this investigation covered only a short time period (2014–2018) and its log-linear models may be poorly suited for county-level data, since a large fraction of such locations have zero deaths in at least some years.

2. Methods

Following previous research, marijuana legalization effects are initially estimated from difference-in-differences (DiD) models of the form:

$$\boldsymbol{D}_{it} = \alpha + \boldsymbol{M}_{it}\boldsymbol{\beta} + \boldsymbol{X}_{it}\boldsymbol{\gamma} + \boldsymbol{S}_i + \boldsymbol{Y}_t + \epsilon_{it}, \tag{1}$$

where D_{it} is a measure of opioid deaths in state i and year t, M_{it} indicates one or more cannabis policies, X_{it} are supplementary covariates, S_i and Y_t are vectors of state and year fixed effects, ϵ_{it} is the regression error term, and $\widehat{\beta}$ provides the predicted impact of marijuana legalization. Prior studies have estimated log-linear models, so that D_{it} indicates the natural log of the opioid death rate in our replication efforts. However, since the dependent variable represents a count process, our later estimates come from Poisson models where D_{it} refers to the number of opioid deaths in each state and year, with robust standard errors clustered at the state-level.[10] Poisson estimates, with robust standard errors, are preferable in this context because log-linear specifications cannot handle zero values of the dependent variable and since small values may be overly influential (Wooldridge 2010).[11] However, we also confirm that these estimates are insensitive to this choice of functional form.

When there are multiple marijuana treatment variables, different counterfactuals can be considered. For instance, with four policies – medical marijuana legalization (MML), medical marijuana dispensaries (MMD), recreational marijuana legalization (RML) and recreational marijuana dispensaries (RMD) – we can detail the vector of treatment variables in (1) as:

$$\boldsymbol{M}_{it}\boldsymbol{\beta} = \beta_1 \mathrm{MML}_{it} + \beta_2 \mathrm{MMD}_{it} + \beta_3 \mathrm{RML}_{it} + \beta_4 \mathrm{RMD}_{it}. \tag{2}$$

Empirically, there is a hierarchy of policy implementation since dispensaries are never allowed without more general legalization and recreational marijuana is never permitted without medical marijuana being both legal and available for retail sale. This implies

[8] Percentage effects for these log-linear models are equal to $\exp(\widehat{\beta}) - 1 \times 100\%$, where $\widehat{\beta}$ is the relevant policy coefficient.

[9] This was calculated as $\exp(0.19 - 0.23) - 1 \times 100\% = -4\%$. Estimating effects versus the counterfactual of no legal marijuana of any kind also requires incorporating the coefficient estimates on MML and MMD.

[10] Population is included as an exposure variable so that the marginal effects are interpreted as effects on log mortality rates. The standard Poisson model assumes that the variance of the logged dependent variable is proportional to its mean, but this assumption is relaxed when using robust standard errors.

[11] Zero values are never observed in our main models, but they do occur when examining population groups stratified by age or race-ethnicity and when looking at more-finely defined opioid categories.

that the coefficient estimates $\widehat{\beta}_1$ through $\widehat{\beta}_4$ show *incremental* effects of policies versus the next lesser form of legalization. For instance, $\widehat{\beta}_4$ indicates the predicted impact of recreational marijuana dispensaries above and beyond that of legal medical marijuana, medical marijuana dispensaries, and recreational marijuana. Although prior studies have usually emphasized such incremental effects, the most interesting counterfactual, in our view, often compares effects of the specified policy versus when all forms of cannabis are illegal. In the example just provided, this is estimated as: $\widehat{\beta}_1 + \widehat{\beta}_2 + \widehat{\beta}_3 + \widehat{\beta}_4$. When replicating prior studies, it is possible to directly compare point estimates for this counterfactual, but frequently not the standard errors, since the information needed to do so was often not supplied.

This investigation extends beyond previous related research in other ways. Correctly measuring the effects of marijuana legalization on the mortality outcomes requires accurate data on the presence and timing of implementation of these key treatment variables. Obtaining this information has been difficult, particularly for early research where consolidated databases did not exist. One result is that these policy measures frequently differ across studies. To address this, we use carefully cross-checked and verified information on the timing of marijuana legalization policies that has lately been made available and also used in some other recent research.

Second, we adjust for the incomplete reporting of the specific types of drugs involved in overdose deaths. Correcting for incomplete reporting is potentially important because the drug categories involved in overdose deaths are not identified in 6% to 25% of cases (depending on the year).[12] The adjustment procedures, which have been detailed earlier (Ruhm 2017, 2018), impute drug involvement in these cases by estimating year-specific probit models for the sample of overdose deaths where at least one drug category is identified and then using the results to predict the probability of involvement where only unspecified drugs are mentioned. The explanatory variables in the probit models include demographic characteristics, and some interactions between them, day-of-the week indicators, location of death, and several county characteristics.

Third, in our preferred specifications, we weight observations by state-year populations. Weighting likely improves the estimates of the average treatment effects, given heterogeneity in both the marijuana legalization policies and in the size of state populations. For instance, both Florida and North Dakota legalized medical marijuana in 2017. Without weighting the two states would be treated equally, even though Florida had 28 times the population of North Dakota (21.478 vs. 0.762 million in 2019).[13] However, weighting also has possible drawbacks. In addition to potential statistical issues,[14] states are coequal, regardless of population size, in their ability to change laws regarding cannabis, and weighting treats these independent legal decisions differently. For these reasons, we frequently follow Solon et al. (2015) advice to show unweighted, as well as weighted, estimates so that the reader can see how this choice affects the results.

In addition to the DiD estimates, we estimate event study (ES) models of the form:

$$D_{it} = \alpha + \sum_{k=-j}^{j} \boldsymbol{M}_{it+k}\boldsymbol{\beta} + \boldsymbol{X}_{it}\boldsymbol{\gamma} + \boldsymbol{S}_i + Y_t + \epsilon_{it}, \tag{4}$$

where $\boldsymbol{M}_{it+k}$ is a vector of lag, contemporaneous, and lead policy variables, with negative values of k showing potential pre-trends and, assuming these are absent, positive values indicating DiD effects k years after policy implementation. For medical marijuana (MML and MMD), we use event windows $k = [-8, 8]$ and for recreational marijuana (RML and RMD), $k = [-5, 5]$. We use shorter windows for recreational marijuana since there are fewer years of post-treatment data available. The policy indicators are binned at the "endpoints", defined as last open-ended lag or lead (e.g., for periods equal to or greater than eight years before or after medical marijuana legalization). When estimating the event studies, $t-1$ is the (excluded) reference period. Policy implementation for partial years is accounted for by incorporating fractional values of the event time variables.[15] We compute the ES estimates with controls for lags and leads of only one cannabis variable at a time, but with contemporaneous values of the other three policy treatments included.[16] Note that the event study estimates are *incremental*, in that they show the estimated effects of adding the specified policy on top of previously implemented treatments. For example, the medical marijuana dispensary estimates indicate the estimated impact of retail sales beyond having legal medical cannabis without operating dispensaries.

We implement a variety of other estimates that are described in detail below. These include: stacked regression and other procedures to address potential problems recently recognized with standard DiD estimates when policy implementation is staggered; examination of how the predicted policy effects vary with the choice of time periods analyzed; and potential heterogeneity in the estimated effects for different types of opioid deaths and population subgroups stratified by sex, race/ethnicity, and age.

3. Data

We use *National Vital Statistics System* data from the *Multiple Cause of Death* (*MCOD*) files for the universe of U.S. deaths from 1999 to

[12] Drug poisoning fatalities are defined as those where the ICD-10 underlying cause-of-death codes are: X40-X44, X60-X64, X85, or Y10-X14.
[13] Source: https://www.census.gov/data/tables/time-series/demo/popest/2010s-state-total.html.
[14] Implicitly, our argument that weighting is appropriate rests on the assumption that there is unmodeled heterogeneity in the effects of the marijuana policies. This seems likely given that they are multi-dimensional in ways we do not capture.
[15] Thus, if a policy went into effect on July 1, 2010, M_{it} takes the value of 0.5 in 2010 and 1 in 2011, and M_{it+1} equals 0.5 in 2011 and 1 in 2012.
[16] For example, in the medical marijuana dispensary event studies, contemporaneous controls for MML, RML, and RMD are also included.

2019. The *MCOD* provide information on a single underlying cause of death, up to 20 additional contributory causes, and demographic variables (Centers for Disease Control and Prevention 2022). Data are utilized here on four-digit International Classification of Diseases, Tenth Revision (ICD-10) mortality codes, state and county of residence, age, race/ethnicity, gender, education, and year. Special permission was obtained to use the geographic information, which is not provided in the public use files. Problems with some death classifications in 2009 have been identified (Kochanek et al., 2011; NIDA 2020) leading us to exclude observations in that year for three states (the District of Columbia, New Jersey, and West Virginia), resulting in a sample size of 1068.[17] For our main analysis, our measure of opioid deaths covers ICD-10 T-codes 40.0–40.4 and 40.6. Earlier studies have used narrower definitions (T-codes 40.1–40.4 or 40.2–40.4), as detailed in the Appendix.[18] Our definition is consistent with that used by the Centers for Disease Control and Prevention (Ahmad et al., 2021), but we also examine whether the results are sensitive to this choice.

Our information on the timing and implementation of marijuana policies was compiled and verified by the RAND Corporation, as part of their Opioid Tools and Information Center Resources (RAND-OPTIC).[19] In creating these data sets, "OPTIC investigators consulted with public health lawyers in synthesizing data sets and corroborating key elements of each policy that are important for influencing opioid outcomes based on theory and evidence."[20] For states legalizing medical marijuana we also examined whether home and group cultivation was allowed using data obtained from Ali et al. (2021) and Marijuana Policy Project (2022).

When cannabis policies are in place for only part of the year, we create fractional variables indicating the number of months they were active.[21] We dated the marijuana dispensary treatment variables according to when the first legally-protected dispensary operated in the state. These procedures largely follow those used in the RAND-OPTIC data.[22] Effective dates for the four policies are summarized in Fig. 1A, with details provided in Appendix Table A.1. Fig. 1B shows the percentage of the national population covered by the policies in each year.

Prior analyses frequently used relatively sparse sets of supplementary covariates, raising the possibility of omitted variables bias in the estimated treatment effects. We therefore augment the original specifications with a common set of controls for state population shares of non-Hispanic whites, males, and four age groups (18–24, 25–44, 45–64, and $\geq$65 year-olds); two measures of economic conditions (state unemployment rates and median household incomes); and beer tax rates (since alcohol and drug consumption may be complements or substitutes). In addition, we control for two policies designed to reduce problem opioid use – must access prescription drug monitoring programs (PDMPs) and pill mill/pain management clinic laws – and two others designed to reduce the risk of death from drug overdoses – Good Samaritan and Naloxone Laws.[23] Policy estimates could be attenuated or biased if some of these controls are inappropriate for inclusion, a possibility we also examine below. Mean values, both weighted and unweighted, of the supplementary covariates are provided in Appendix Table A.2.

4. Previous findings are not robust to specification changes

We begin our empirical analysis by replicating and extending the results of four studies described above that use annual state-level data and DiD methods (Bachhuber et al., 2014; Powell et al., 2018; Shover et al., 2019; Chan et al., 2020). We first employ specifications and data similar or identical to those originally used. Where possible, we obtained data directly from the papers, online supplements, or the authors. When this was not feasible, either because the information was restricted (as with some mortality data) or the authors did not provide their original data, we acquired the information from other sources. Since the earlier papers frequently

[17] The three state-year observations are erroneous because the numbers of opioid fatalities reported are dramatically lower in 2009 than in either the preceding or following year. Specifically, the number of reported opioid deaths in 2008, 2009 and 2010 are as follows: Washington DC – 31, 13, and 34; New Jersey – 331, 55, and 373; West Virginia – 371, 184, and 451.

[18] The added categories include T40.0, deaths involving opium (which are extremely rare), and T40.6, deaths involving unspecified narcotics (which are mainly opioids).

[19] In the RAND-OPTIC data, MML (RML) refer to the first date that medical (recreational) cannabis was legally protected in the state and MMD (RMD) to the date that the first legally protected medical (recreational) marijuana dispensary was open for business in the state.

[20] General information on the RAND-OPTIC data can be obtained from: https://www.rand.org/health-care/centers/optic.html. Specific information on the marijuana policy and other RAND-OPTIC data sets used in this analysis is available at: https://www.rand.org/health-care/centers/optic/resources/datasets.html. An earlier version of this data set was used for related analysis by Powell, Pacula, and Jacobson (2018).

[21] Policies were treated as implemented for the month if in place by the 3rd of the month, except for January, where the cutoff date was the 7th.

[22] In three cases we deviated from RAND-OPTIC and altered the timing of the policy variables. First, while legal protections for possession and use of marijuana in Maryland were technically provided in October of 2003, no supply source was identified and we follow other researchers (Powell, Pacula, and Jacobson 2018; Bradford et al. 2018) in not recognizing this law and treating medical marijuana as being legal there starting in June of 2014. Second, when Alaska legalized recreational marijuana dispensaries on October 1, 2016, medical cannabis dispensaries had not yet been technically authorized. However, since they were effectively allowed at that point, we treat this as the date for medical marijuana dispensaries as well. Third, since the first medical cannabis in Louisiana was legally dispensed on August 6, 2019, without medical marijuana having been previously authorized; we treat this as the effective date for both MML and MMD.

[23] Must access state prescription drug monitoring programs (PDMPs) require prescribers to consult the PDMP before prescribing. Good Samaritan laws provide protection from criminal sanctions to overdose victims or witnesses who seek emergency services. Naloxone laws authorize third-party prescribing and lay administration of naloxone, the standard antidote to opioid overdose. Pill mill/pain clinic laws subject pain management clinics and physicians working in them to extra regulation.

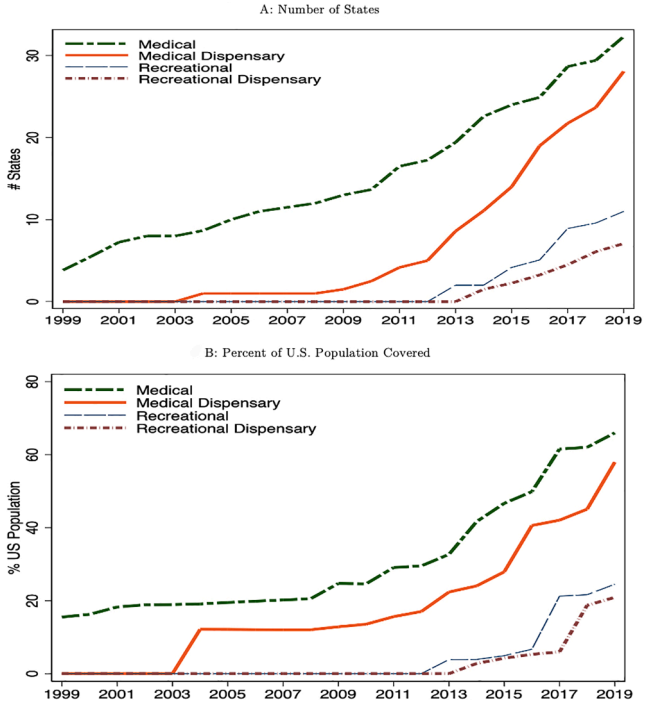

Fig. 1. Number of States and Percent of U.S. Population Covered by Specified Marijuana Policy, by Year.
Note: Figure shows the number of states and percentage of U.S. population with the specified policy in each year. Policies implemented during a given year are weighted fractionally, to the nearest month.

estimated multiple models, we focus here on the primary specifications used, provided that the sample included data from all states.[24] Appendix Table A.3 supplies information on these studies, the definition of opioid mortality and covariates used, as well as sources of the data employed in our replication efforts. Following the original replication, we extend the models by: adjusting for the incomplete reporting of the types of drugs involved in overdose deaths, using carefully cross-checked and verified information on the timing of marijuana legalization policies, controlling for the "preferred" (generally more comprehensive) set of supplementary covariates and, sometimes, weighting the data by state-year populations.

The first column of Table 1 summarizes original findings of the four studies and the second shows our replication efforts. Column (3) displays the estimates for specifications that are identical except that they adjusted for incomplete reporting of opioid mortality rates on death certificates. Column (4) uses our updated information on the timing of the marijuana legalization policies, and includes the preferred set of covariates. In this and subsequent models we use the full sample, including state-year observations suppressed from the public-use datasets due to small numbers of deaths. Column (5) weights observations by state-year populations. As mentioned, when multiple cannabis policy variables are examined, estimates are relative to the counterfactual of no marijuana legalization.

We generally replicate the original study results well. We obtain nearly identical estimates for two of them (Powell et al., 2018; Shover et al., 2019), with somewhat larger differences in the other two (Bachhuber et al., 2014; Chan et al., 2020), as might be expected because we did not acquire the underlying data directly from the authors in these later cases but rather use corresponding information from another study or compiled the data ourselves.[25] The qualitative findings are the same using either the original study findings or our replications of them. Specifically, if correct, they suggest that: 1) legal medical marijuana was associated with lower opioid death rates during the first decade of the 21st century but that this reversed when extending the analysis through 2017; 2) the effects of medical marijuana dispensaries are ambiguous, with evidence of mortality reductions from 1999 to 2013 but with neutral or deleterious predicted impacts when extending the analysis through 2017 and controlling for recreational cannabis legalization; 3) legalization of recreational marijuana raised deaths when retail sales were not permitted, but with little effect or possibly small reductions, when dispensaries operated.

However, we should have little confidence in these conclusions because the estimates are quite sensitive to the choice of specifications. Column (3) shows that adjusting for incomplete reporting of drug involvement on death certificates generally has modest

[24] Chan, Burkhardt, and Flyr (2020) highlight a preferred model that limits analysis to 28 states. In this case, we replicate and extend their national estimates.

[25] Specifically, we obtained data from Shover et al. (2019), used in their replication of Bachhuber et al. (2014). Our results for 1999-2010 are similar to those they estimate. For Chan, Burkhardt, and Flyr (2004), we acquired all of the data ourselves and so it is unsurprising that our estimates are somewhat different from the original study.

Table 1
Replication and Extensions of Prior Studies Estimating Marijuana Legalization on Opioid Mortality.

Study/Period	*Original Study*	*Replication/Extension*			
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Bachhuber et al. (2014): 1999–2010					
Medical	−0.285**	−0.237**	−0.210**	−0.148*	−0.060
	(0.094)	(0.113)	(0.087)	(0.078)	(0.091)
Observations	575	575	575	612	612
Shover et al. (2019): 1999–2017					
Medical	0.205**	0.205**	0.156*	0.283***	0.384***
	(0.094)	(0.099)	(0.084)	(0.091)	(0.073)
Observations	908	908	908	969	969
Powell et al. (2018): 1999–2013					
Medical	−0.072	−0.066	−0.040	−0.037	−0.006
	(0.107)	(0.100)	(0.085)	(0.086)	(0.081)
Medical + Dispensary	−0.333	−0.339**	−0.168	−0.229*	−0.174
		(0.164)	(0.117)	(0.118)	(0.144)
Observations	765	765	765	765	765
Chan et al. (2020): 1999–2017					
Medical	0.25***	0.186**	0.214***	0.227***	0.334***
	(0.09)	(0.076)	(0.073)	(0.083)	(0.075)
Medical + Dispensary	0.15	0.154	0.234**	0.250**	0.331***
		(0.118)	(0.103)	(0.108)	(0.096)
Recreational	0.60	0.527**	0.568**	0.588**	0.275**
		(0.259)	(0.242)	(0.228)	(0.157)
Recreational + Dispensary	−0.13	−0.117	−0.065	0.112	0.160
		(0.183)	(0.145)	(0.161)	(0.174)
Observations	969	969	969	969	969
Adjusted Mortality	No	No	Yes	Yes	Yes
Preferred Treatments/Controls	No	No	No	Yes	Yes
Population Weights	No	No	No	No	Yes

Notes: Standard errors in parentheses are robust and clustered at the state-level.
All models include state and year fixed effects. The outcomes in the Bachhuber et al. (2014) and Shover et al. (2019) are log age-standardized prescription opioid mortality rates. (T-Codes T40.2-T40.4). Those in Powell et al. (2018) and Chan et al. (2019) are the log "all" opioid mortality rates (T-Codes T40.1-T40.4). Since Bachhuber et al. and Shover et al. use the public-access CDC WONDER data, our replications in columns (4) and (5), using contain more observation since they use non-suppressed restricted-access data. The coefficients on medical dispensaries and the recreational marijuana variables show predicted effects relative to no legalization of marijuana. Models which do not include our preferred controls include the original authors' set of controls.
*$p<0.1$, **$p<0.05$, ***$p<0.01$.

consequences, with the most important exception being the reduction by half in the Powell et al. (2018) estimate of the beneficial effects of medical marijuana dispensaries. However, as shown in columns (4) and (5), less favorable or more harmful results are generally obtained when using more accurate policy data, more comprehensive controls, and population weights. Comparing the estimates in columns (2) and (5), the Bachhuber et al. (2014) finding of beneficial effects of legal medical marijuana from 1999 to 2010 attenuates by three-quarters and the advantageous estimated impact of medical marijuana dispensaries from 1999 to 2013, identified by Powell et al. (2018), is reduced by 49%. While Shover et al. (2019) previously demonstrated that the beneficial effect of medical marijuana does not survive when the analysis period is extended through 2017, we find that the consequences are almost twice as harmful as those they obtain. Similarly, Chan et al. (2020) found damaging effects of both MML and MMD in their main specifications but the harms appear to be substantially understated: by 80% and 115% when moving from column (2) to column (5).

The estimated consequences of legalizing recreational marijuana also vary substantially. RML is predicted to *raise* mortality rates in all specifications, but by much smaller amounts when using weighted data, and confidence intervals are large. Conversely, recreational marijuana dispensaries are correlated with relatively small and imprecisely estimated changes in opioid death rates, albeit with worse estimated effects when moving from model (2) to models (4) or (5).

The main conclusion from this exercise is that prior results are sensitive to the choice of time periods and specifications, with models likely to be preferred usually obtaining less beneficial or more harmful estimated effects. A consistent finding between the original studies and our extensions is that mortality reductions associated with legalizing medical cannabis do not persist when extending the analysis period beyond 2013. However, evidence from Powell et al. (2018) of the beneficial effects of medical marijuana dispensaries is also fragile, which should not be surprising since, during most of their study period just one state (California) had

working dispensaries and even by 2010 dispensaries operated in only two additional states (Colorado and New Mexico). Finally, imprecision of the results makes it hard to rule out large recreational marijuana effects, in either direction, probably reflecting the recency of these policies.[26]

5. Legal retail marijuana sales raise opioid mortality

We next go beyond the extensions of previous studies just examined to provide an in-depth examination of the effects of cannabis legalization on opioid deaths over the 1999–2019 period, using the data and methods detailed above.

5.1. Difference-in-differences results

Table 2 summarizes the primary DiD estimates. Its format resembles the replication analysis in Table 1, but we now use the full 1999–2019 period, a broader definition of opioid deaths, and estimate Poisson rather than log-linear models. Each panel and column shows results of separate regressions. In the upper panel, only medical marijuana legalization (MML) is controlled for. The middle panel adds medical marijuana dispensaries (MMD) to the set of treatment variables, and the lower panel incorporates both recreational marijuana (RML) and corresponding dispensaries (RMD). Estimates with all four policy variables will generally be the most informative, although the models with less complete controls are useful for comparison with earlier work and for some of the sensitivity testing below.[27]

A key finding is that the legalization of medical cannabis, and its sale through retail dispensaries, is associated with *higher* opioid death rates. For our preferred model, in column (3) of the bottom panel – which adjusts mortality rates for incomplete reporting of drug involvement, controls for the supplementary covariates, weights the data by population, and includes all four treatment variables – MML is associated with a 17.7% increase in opioid death rates, and MMD with a 28.1% rise.[28]

The results for recreational marijuana are more ambiguous. The RML coefficient is strongly positive when using unweighted data, implying 31% higher opioid death rates; however, after weighting, the increase is less than half as large (14%) and no longer significant. Similarly, the estimated RMD is positive and substantial (25%) in the preferred specification (column 3), but is sensitive to the choice of whether or not to weight the data and the confidence intervals are sizeable. As discussed, these predicted effects are relative to the counterfactual of no legal cannabis. If we instead compare them to the situation where the state is adding recreational marijuana to already legal medical cannabis (i.e., comparing the RML and RMD entries on the table to those for MMD), the results using weighted data suggest that recreational marijuana without retail sales reverses some of the deleterious effects of medical cannabis with dispensaries, but that the availability of retail recreational marijuana sales undoes these gains.

5.2. Event study estimates

Fig. 2 summarizes event study results for the four treatment variables using the preferred specification – with adjusted mortality rates, supplementary covariates, and weighted data. Recalling that these estimates are incremental, indicating differences relative to a lower level of legalization rather than to the counterfactual of no legal cannabis, the findings are generally consistent with the DiD findings and provide useful additional details.

Legalization of medical marijuana is associated with sharply higher opioid death rates, particularly five or more years after implementation. However, the data suggest a positive pre-trend, particularly from $t-4$ through $t-1$, which may explain a portion of this increase. For retail sales of medical cannabis, the story is different. Here, there are flat pre-trends and the estimated MMD effects grow in the years after implementation, as might be expected if access to dispensaries increases over time.

The ES estimates for recreational marijuana are harder to interpret. Precision is often low, and some potential inconsistencies raise concerns. For example, while the post-treatment estimates are modest and not statistically significant in most years, at event time $t+2$ there is a large and significant positive predicted RML effect on opioid deaths that is accompanied by a similarly sized negative coefficient for RMD. There are also erratic patterns of estimated effects in periods after policy implementation and, for dispensaries, the possibility of negative pre-trends suggests that the harmful consequences of this policy may be understated in the DiD models. Our suspicion is that because most recreational marijuana policies have been so recently implemented, the estimated treatment effects are unreliable, making it difficult to draw trustworthy conclusions about them.

5.3. Robustness checks and falsification tests

The first column of Table 3 repeats results from our preferred model (column 3 in Table 2) for the specification with all four marijuana policy treatments controlled for. The results for medical marijuana legalization are virtually identical and those for

[26] When Chan, Burkhardt, and Flyr's (2020) analysis ended, in 2017, just two states (Colorado and Washington) had legal recreational marijuana in place for at least two years.

[27] As mentioned, the counterfactual for these estimates is no marijuana legalization of any kind. In Appendix Table A.4, we present corresponding "incremental" estimates, where the counterfactual is the treatment effect relative to the next less comprehensive policy (e.g., the impact of medical cannabis dispensaries, above and beyond the effect of legalized medical marijuana without retail sales).

[28] Marginal effects from the Poisson model are computed as $\exp(\widehat{\beta}-1)\times 100\%$.

Table 2
Estimates of Marijuana Legalization on Opioid Mortality, 1999–2019.

Legalization Status	*(1)*	*(2)*	*(3)*
Medical	0.230***	0.228***	0.200***
	(0.054)	(0.045)	(0.038)
Medical	0.232***	0.212***	0.174***
	(0.055)	(0.043)	(0.038)
Medical + Dispensary	0.223***	0.282***	0.256***
	(0.071)	(0.062)	(0.045)
Medical	0.213***	0.197***	0.163***
	(0.057)	(0.046)	(0.044)
Medical + Dispensary	0.205***	0.268***	0.248***
	(0.074)	(0.064)	(0.051)
Recreational	0.207*	0.269**	0.134
	(0.117)	(0.120)	(0.103)
Recreational + Dispensary	0.075	0.165	0.224**
	(0.143)	(0.128)	(0.114)
Adjusted Mortality	No	Yes	Yes
Population Weights	No	No	Yes

Notes: Table shows estimated effects of various types of marijuana legalization on opioid deaths (T-Codes: 40.0–40.4, 40.6), using data from 1999 to 2019 ($n = 1068$). Estimates are from Poisson models. All models include state and year fixed effects and the preferred set of controls. Additional details on the regressions are in the bottom panel. Each panel shows the results of a different set of regressions. The coefficients on medical dispensaries and the recreational marijuana variables show predicted effects relative to no legalization of marijuana. Standard errors in parentheses are robust and clustered at the state-level.
*$p<0.10$, **$p<0.05$, ***$p<0.01$.

recreational marijuana change little when estimating log-linear, rather than Poisson, models as has been done in prior research (column 2), or when the dependent variable has been transformed using the inverse hyperbolic sine function, an alternative sometimes used when outcomes that are censored at zero (column 3). Column (4) shows that the results are also not sensitive to the use of fractional measures for cases where the policy variables are become effective part way through the year.[29] Similar or even more harmful MMD results are also obtained when limiting the analysis to the 20 largest states and using unweighted data (column 5), as expected since these states contain most of the country's population. The findings are also comparable or more deleterious when the dependent variable is the narrower definition of opioid deaths (T-Codes: 40.1–40.4) used in some earlier research (column 6).

Although the inclusion of supplementary regressors reduces potential bias due to confounding, doing so might be problematic if the policy being investigated causally influences an included mediating variable. This is sometimes referred to as the "bad controls" problem (Angrist and Pischke 2009; Cinelli et al., 2020). Column (7) investigates whether this could be an issue for our preferred estimates by estimating models with sparse supplementary covariates, limited to state demographic characteristics. Compared to the main estimates, these yield generally more detrimental predicted consequences of cannabis legalization.[30]

Column (8) adds controls for state-specific linear time trends. Although their inclusion may account for some difficult-to-identify determinants of opioid deaths, the risks of "overcontrolling" have been emphasized, particularly when the treatment effects are dynamic (Wolfers 2006; Neumark et al., 2014; Goodman-Bacon 2021). Given this, it is no surprise that their inclusion attenuates the coefficients on the legalization variables (e.g. by 42% for MMD). Nevertheless, medical marijuana dispensaries continue to predict a substantial rise in opioid deaths with similar, although less precisely determined, estimated increases for retail recreational marijuana sales.[31]

Columns (9) and (10) show falsification tests, where the outcomes are mortality from heart disease and cancer, the two leading causes of death. Cannabis legalization would not be expected to strongly affect either of these. The estimates confirm zero effects for heart disease deaths but indicate modest (1.3%) reductions in cancer mortality.[32] A possible explanation for this last finding is that some terminally-ill cancer patients substitute marijuana for opioids as a treatment for pain.

We also compared predicted effects of MML policies that allowed for home or group cultivation, versus those that did not. There

[29] For the studies we replicated, Bachhuber et al. (2014) and Shover et al. (2019) used fractional treatment variables, whereas Powell, Pacula, and Jacobson (2018) only assign the treatment variables a value of one if the policies have been in place for the full year and Chan et al. (2020) do so if they are in effect for any part of the year. In column (4), we assign the policy variables values of zero (one) if they are effective for less than (at least) half the year.

[30] Few of the covariates used here seem likely to be "bad controls". However, there could be issues, at least theoretically if, for instance, legalizing marijuana affects economic conditions or (through its impact on opioid deaths) the implementation of opioid policies.

[31] Event study estimates for the models with time trends further demonstrate the sharp increase in predicted opioid deaths from medical marijuana dispensaries several years after enactment (Appendix Figure A.1).

[32] Interestingly, some research suggests that marijuana legalization might *increase* cardiovascular deaths (Abouk and Adams 2018), but we do not find any indication of this here.

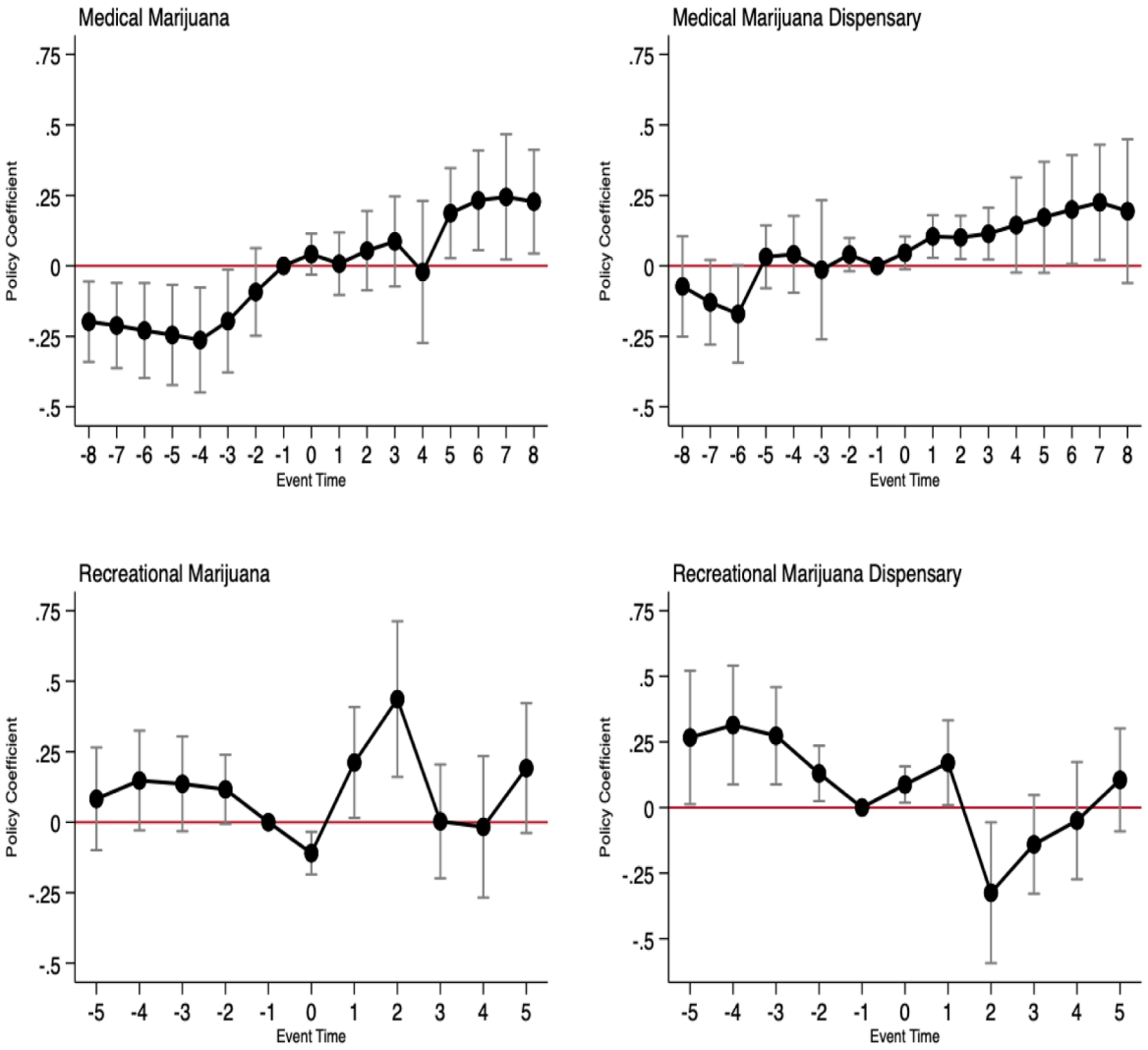

Fig. 2. Event Study Estimates for Marijuana Policies.
Note: Figure shows event study estimates for the four policy variables for all opioids (T-Codes: 40.0–40.4, 40.6) from Poisson regressions, corrected for under-reporting, with the preferred set of controls included as well as vectors of state and year dummy variables. Observations are weighted by state population. Event time refers to the number of years before or after medical or recreational legalization occurs, or the first dispensary of the specified type is active in the state. Error bars show 95% confidence intervals, based upon robust standard errors clustered at the state-level.

was no evidence of differences in the effects.[33]

Finally, we estimate a series of "leave-one-out" (LOO) models to check whether the results are being driven by policy changes in any single state. Here, we estimate the preferred specification multiple times with one state instituting the specified policy change successively deleted from the analysis sample. The results confirm that the estimated MML and MMD treatment effects are positive and significant in all cases while those for RML and RMD are also always positive but imprecisely measured and frequently not statistically significant (Appendix Figures A.2 and A.3). Consistent with this, the histograms of estimated LOO coefficients indicate a fairly narrow interval of estimates for MML and MMD but a wider range for RML and RMD (Appendix Figure A.4).

6. Potential problems with difference-in-differences estimates

Recent literature identifies possible concerns with estimates obtained using standard DiD models when the timing of treatment implementation is staggered across locations (Goodman-Bacon 2021; Sun and Abraham 2021). In our context, the two most

[33] Medical marijuana laws with no cultivation, home cultivation, and group cultivation were predicted to increase opioid deaths by 17.5%, 18.6%, and 18.5% respectively, compared to the counterfactual of no legal cannabis, with reasonably wide confidence intervals.

Table 3
Supplementary Estimates and Falsification Tests.

Legalization Status	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Medical	0.163***	0.161***	0.166***	0.166***	0.149***	0.244***	0.222***	0.056	−0.012	−0.013***
	(0.044)	(0.047)	(0.046)	(0.040)	(0.050)	(0.070)	(0.059)	(0.051)	(0.011)	(0.005)
Medical + Dispensary	0.248***	0.248***	0.250***	0.243***	0.274***	0.343***	0.301***	0.144**	−0.007	−0.013***
	(0.051)	(0.063)	(0.063)	(0.048)	(0.070)	(0.088)	(0.064)	(0.068)	(0.010)	(0.005)
Recreational	0.134	0.205	0.209	0.152	0.256*	0.237	0.127	−0.036	−0.005	−0.013*
	(0.103)	(0.126)	(0.125)	(0.093)	(0.146)	(0.152)	(0.136)	(0.102)	(0.013)	(0.008)
Recreational + Dispensary	0.224**	0.165	0.168	0.217**	0.270	0.351**	0.28**	0.135	−0.010	−0.004
	(0.114)	(0.117)	(0.116)	(0.108)	(0.179)	(0.162)	(0.131)	(0.118)	(0.015)	(0.009)
Description	Main Model	Log-Linear Model	IHS Model	No Fractional Treatments	20 Largest States (No Weights)	Narrow Opioid Definition	Short Covar-iates	Includes State Trends	Heart Disease	Cancer

Notes: Models (1) through (5), (7) and (8) show estimated effects of various types of marijuana legalization on opioid deaths (T-Codes: 40.0–40.4, 40.6), using data from 1999 to 2019. Model (6) uses a narrower definition of opioid deaths (T-Codes: 40.1–40.4) and the dependent variables in columns (9) and (10) are deaths due to heart disease (ICD-10 Codes: I00-I09, I11, I13, I20-I51) and malignant neoplasms (ICD-10 Codes: C00-C97). Except for models (2) and (3), estimates are from Poisson models that include state and year fixed effects, and supplementary controls. Column (2) estimates corresponding regression models where the dependent variable is the natural log of mortality rates and the dependent variable in model (3) has been transformed using the inverse hyperbolic sine (IHS) function. The coefficients on medical dispensaries and the recreational marijuana variables show predicted effects relative to no legalization of marijuana. The treatment variables take fractional values when they are implemented in the middle of the year, except in column (4), where they take the value of 0 (1) if implemented after June 30 (before July 1) of the year. Data are weighted by state-year populations, except in model (5), where analysis is limited to the 20 largest states by population (CA, TX, FL, NY, PA, IL, OH, GA, NC, MI, NJ, VA, WA, AZ, MA, TN, IN, MD, MO and WI; $n = 419$). Model (7) restricts the set of supplementary covariates to state population shares and column (8) adds controls for state-specific linear time trends to the model. Standard errors in parentheses are robust and clustered at the state-level.
*$p<0.10$, **$p<0.05$, ***$p<0.01$.

problematic issues are: 1) the usual procedure gives more weight to policy changes occurring in the middle of the analysis period than to those taking place near either end of it; and 2) the estimates may be biased if the treatment effects vary with the time since policy implementation, as seems likely based on the ES estimates above.[34]

Multiple methods have recently been proposed for dealing with these problems. Some are not well suited for this application because they: 1) have been designed and tested in a linear regression framework, rather than for Poisson models; 2) consider only a single treatment, whereas we evaluate four treatment policies (MML, MMD, RML and RMD); or 3) do not deal well with time-varying covariates, some of which may be critical to control for in our analysis.[35]

Given these difficulties, we follow Cengiz et al. (2019) in estimating "stacked regression" models to examine the robustness of our medical marijuana treatment estimates. For these, we limit analysis to policy changes occurring from 2004 to 2014, with each of these years hereafter referred to as "cohorts", to ensure that we have event windows covering at least five years before and after each cohort. We focus on MML because it is less obvious how the estimates of MMD, RML, or RMD can be operationalized in this framework since some of these policies have been put in place shortly after the implementation of "lower-level" treatments.[36]

For these estimates, we first create a series of panels – one for each cohort – containing data for that period and the five years preceding and following it. Each cohort sample is then restricted to states legalizing marijuana in the cohort year, or who had not done so by the end of 2019. This results in eleven balanced panels consisting of treatment states, those legalizing medical marijuana in cohort year t, and control states where cannabis remained illegal through the end of 2019. The balanced panels are then "stacked" (joined together) and the models are estimated. Since the stacked sample contains multiple observations from the never treated control states and for given calendar years, we also estimate "saturated" models that include both cohort-by-state and cohort-by-year fixed effects.

The stacked regression models provide no evidence of bias in our main DiD estimates, as demonstrated in Table 4. The top panel repeats the estimated MML coefficients from corresponding specifications in columns (2) and (3) of the first row of Table 2, covering

[34] The first problem occurs because weights on individual policy changes are proportional to group sizes and variance of the treatment variables, with the latter being highest for groups treated in the middle of the panel. The intuition behind the second issue is that previously treated locations act as controls for those treated later, but this is inappropriate if policy effects of the prior adopters are growing or shrinking over time. Similar problems are created for locations enacting policies before the start of the analysis period, unless sufficient time has passed that these treatment effects have reached a steady-state.

[35] Baker, Larcker and Wang (2021) and de Chaisemartin and D'Haultfoeuille (2022) provide useful summaries of the issues of estimating DiD models with staggered treatments and summarize several methods proposed to address them.

[36] The potential bias associated with multiple treatment variables is discussed in de Chaisemartin and D'Haultfoeuille (2022). They also present a method of identifying the parameter estimates for multiple treatments; however, the conditions under which these are operationalizable are quite restrictive and we were not able to obtain estimates in our application using their procedure.

the full 1999–2019 period and without stacking. The second restricts analysis to states legalizing medical cannabis from 2004 to 2014, or who had not done so by 2019, the sample from which we construct the balanced panels. The third provides corresponding coefficients from the stacked sample, but where we have not yet excluded states legalizing medical marijuana in other than the cohort year. All these estimates contain the potential issues described above and we are interested in comparing them to results in the fourth panel, where states are deleted from the stacked samples if they legalized medical marijuana outside the cohort year.

The key finding is that the MML coefficients are similar across the four panels. If anything, the models in the bottom panel suggest slightly larger increases in opioid mortality from legalizing medical marijuana, and with no attenuation in column (3) where we add controls for cohort-by-state and cohort-by-year fixed effects. Fig. 3 displays corresponding stacked regression event studies. These again indicate substantial and statistically significantly higher death rates in the post-treatment periods, although possibly with modest (and statistically insignificant) upwards pre-trends.

As further tests, we examine estimated MML effects using the procedures developed by Callaway and Sant'Anna (2021), Sun and Abraham (2021), and Borusyak et al. (2022). For our purposes, these methods have complications, including the aforementioned issues of not always dealing well with time-varying covariates or non-linear models. For these comparisons, we therefore estimate log-linear, rather than Poisson, specifications and exclude supplementary covariates. Given these limitations, we view them as additional checks for potential problems with our main DiD estimates. We again restrict the sample to states legalizing medical marijuana from 2004 to 2014 or that had not done so by the end of 2019, to ensure at least five years of data before and after policy implementation.

Once more, there is no indication of bias in our primary specifications. The standard two-way fixed-effect DiD log-linear regression model for the sample just described yields an MML coefficient of 0.241, with a clustered robust standard error of 0.125. The corresponding Callaway and Sant'Anna and Borusyak et al. average treatment effects are 0.224 and 0.222, with standard errors of 0.084 and 0.113. The event study estimates, displayed in Appendix Figure A.5, confirm the similarities of results using these alternative procedures and compared to standard DiD methods. All of them suggest positive and growing treatment effects following implementation of MML, although with some suggestion of pre-trends, as was also observed in our main estimates in Section 5.

7. Estimated medical marijuana legalization effects change over time

Estimated medical cannabis legalization effects are sensitive to the period analyzed, with more beneficial or less detrimental consequences when analyzing data from 1999 to 2010 than when extending the sample to include later years. Some prior researchers (Smart 2015; Pacula and Smart 2017; Powell et al., 2018) suggest that one cause may have been the Ogden memo, released in 2009, which deprioritized the federal prosecution of medical marijuana users and suppliers. They argue that states legalizing medical marijuana after that date adopted stricter regulatory approaches than those doing so earlier and that this may explain the different effects. However, there are at least two potential alternative explanations. First, the impacts of given laws may have changed over time, as predominant sources of opioid deaths switched from opioid analgesics to heroin and synthetic opioids. Second, policy effects may be inaccurately or imprecisely measured when using short analysis periods and the results for 1999–2010 may coincidentally yield relatively favorable (but unstable) effects.

We provide evidence for both alternatives by re-estimating models using varying analysis periods. Specifically, we use 1999 as the starting year and vary the endpoint from 2004 to 2019.[37] To provide results for specifications that correspond fairly closely to those where the original sensitivity of results was initially identified (Powell et al., 2018; Shover et al., 2019), we first show findings where MML is the only policy controlled and the data are not weighted.[38] Next, we supply estimates from our preferred specifications, with all four marijuana policy variables included and using weighted data. Here, we focus on findings for MML and MMD since the recency of recreational marijuana legalization precludes estimating their effects for analysis periods ending before 2013.

Fig. 4 summarizes the main results. The left panel, which presents estimates for specifications roughly corresponding to those in earlier studies, shows that the predicted impacts are less precisely estimated when using shorter analysis periods, as expected. However, what is particularly striking is that the *least unfavorable* MML effects are obtained for samples ending between 2010 and 2012. Specifically, medical marijuana legalization is predicted to raise opioid deaths by a statistically insignificant 3% to 5% for these periods, compared with a considerably larger increases when ending the analysis either earlier or later. For instance, the MML effect is an imprecisely estimated 13% rise when using data from 1999–2006 and a statistically significant 14% increase for 1999–2015. Moreover, the growth in mortality predicted to result from MML grows rapidly for samples ending after 2013 and reaches 24% to 30% for analyses concluding in 2016 or later.[39]

The right panel of Fig. 4 shows corresponding MML and MMD point estimates from our preferred models, with population weights and all four policy treatments controlled for.[40] Here, the least unfavorable estimates are obtained for analysis periods ending between 2009 and 2011 – where MML predicts 10% to 12% increases in opioid mortality and MMD a 13% to 15% rise – and with considerably more harmful effects for sample periods terminating either earlier or later, particularly when the timespan extends beyond 2014. For instance, legal medical marijuana is associated with 20% to 23% higher opioid mortality for analysis periods ending between 2016 and 2018, and retail medical marijuana sales with a corresponding growth of 32% to 35%.

[37] We choose 2004 as the earliest ending year to ensure that we have a minimum of six years in the sample.
[38] However, we still adjust mortality for incomplete reporting and include the preferred controls.
[39] See Appendix Table A.5 for details.
[40] Confidence intervals are shown in Appendix Figure A.6.

Table 4
Stacked Regression Estimates of the Effect of Medical Marijuana Legalization on Opioid Deaths*.

Model Description	*(1)*	*(2)*	*(3)*
(a): Original estimates	0.228***	0.200***	
	(0.045)	(0.038)	
(b): Exclude states legalizing MML before 2004 or after 2014	0.241***	0.228***	
	(0.087)	(0.082)	
(c): (b) plus stacked regressions with balanced samples	0.237***	0.220***	0.202***
	(0.048)	(0.041)	(0.037)
(d): (c) plus limit to policy changes in cohort year	0.235**	0.206***	0.219***
	(0.093)	(0.078)	(0.069)
Population Weights	No	Yes	Yes
Saturated Models	No	No	Yes

Notes: Table shows estimated effects, from Poisson models, of legalizing medical marijuana on opioid deaths (T-Codes: 40.0–40.4, 40.6), corrected for incomplete reporting of the drugs involved in overdose deaths. Model (a) repeats the estimates in the top panel of Table 2. All models include state and year fixed effects, adjusted mortality rates, and the preferred set of controls. Panel (b) limits analysis to states legalizing marijuana at some point between 2004 and 2014, or who had not legalized it by 2019 (the control group). Panel (c) creates "stacked regressions" from the sample in model (b) where each of the 11-years from 2004 to 2014 is treated as a "cohort", with separate samples created for each cohort, with a balanced panel created by limiting limited to the range of five years before and after the cohort date (e.g., the 2010 cohort includes data from 2006 to 2015). The 11 data sets were then "stacked" with analysis performed on the combined data set. Panel (d) uses the stacked data set created in (c) but then further excludes states legalizing medical marijuana outside of the cohort year. Sample sizes for models (a) through (d) are: 1068, 711, 6696, and 2560 respectively. "Saturated" models also include controls for include cohort-by-state and cohort-by-year fixed effects. Standard errors in parentheses are robust and clustered at the state-level.
*$p<0.10$, **$p<0.05$, ***$p<0.01$.

As a final test, we estimated a series of "Keep-One-In" (KOI) estimates where each sample consisted of one treatment state legalizing medical marijuana or medical marijuana dispensaries, and with the control group consisting of states that did not permit any type of cannabis through the end of 2019. We limited the treatment states to those first allowing medical cannabis between 2002 and 2016 and, for MMD, those also establishing operating dispensaries over the same period.[41]

Appendix Figure A.7 summarizes results, with states ordered on the X-axis from earliest to latest policy implementation dates. The estimates are often imprecise, as expected since only a single (often small) treatment state is used in each regression. However, there is little evidence that the estimated effects consistently vary as a function of the timing of the policies. The data hint at the possibility of more favorable effects for early MML implementers, but this appears to be unrelated to the timing of the Ogden memo and there is no indication of a relationship between the timing of MMD and opioid mortality rates.

8. Illicit fentanyl and the effects of marijuana legalization

Next, we examine the possibility that cannabis legalization has become more harmful in recent years because of the growth of fentanyl in illicit drug markets. Fentanyl and its analogues began to increase opioid mortality in 2014, with a rapid acceleration after 2015 (Hedegaard et al., 2021). This closely accords with the timing of the worsening MML and MMD effects observed in the previous section.

Table 5 shows separate estimates from our preferred models for deaths from natural/semisynthetic opioids such as oxycodone or hydrocodone (ICD-10 T-Code 40.2, often referred to as prescription opioids), heroin (T-Code 40.1), and synthetic opioids like fentanyl (T-Code 40.4).[42] The findings indicate that retail sales of medical and recreational marijuana have a particularly strong positive association with deaths from synthetic opioids: estimated to increase them by 37% and an imprecisely estimated 51%, respectively, compared to 23% and 26% for prescription opioids and a statistically insignificant 11% and 17% for heroin.

Furthermore, the predicted MML and MMD effects increase sharply when adding to the analysis data for periods after the emergence of illicit fentanyl. As shown on the right-hand side of Fig. 5, the legalization of medical cannabis, with or without dispensaries, had no impact or negative estimated effects on synthetic opioid mortality for periods starting in 1999 and ending before 2015 but the

[41] This restriction was used to ensure having at least three years of data before and after policy implementation. We do not provide corresponding estimates for recreational marijuana because there are only two states (Washington DC and Massachusetts) meeting our inclusion criteria of MML implementation no sooner than 2002 and RML enactment before 2017, and none that meet the corresponding standard for RMD.

[42] Since the death certificate often contain multiple T-codes, a drug fatality can be attributed to more than one type of opioid. For instance, both prescription and synthetic opioids can be involved in the same deaths.

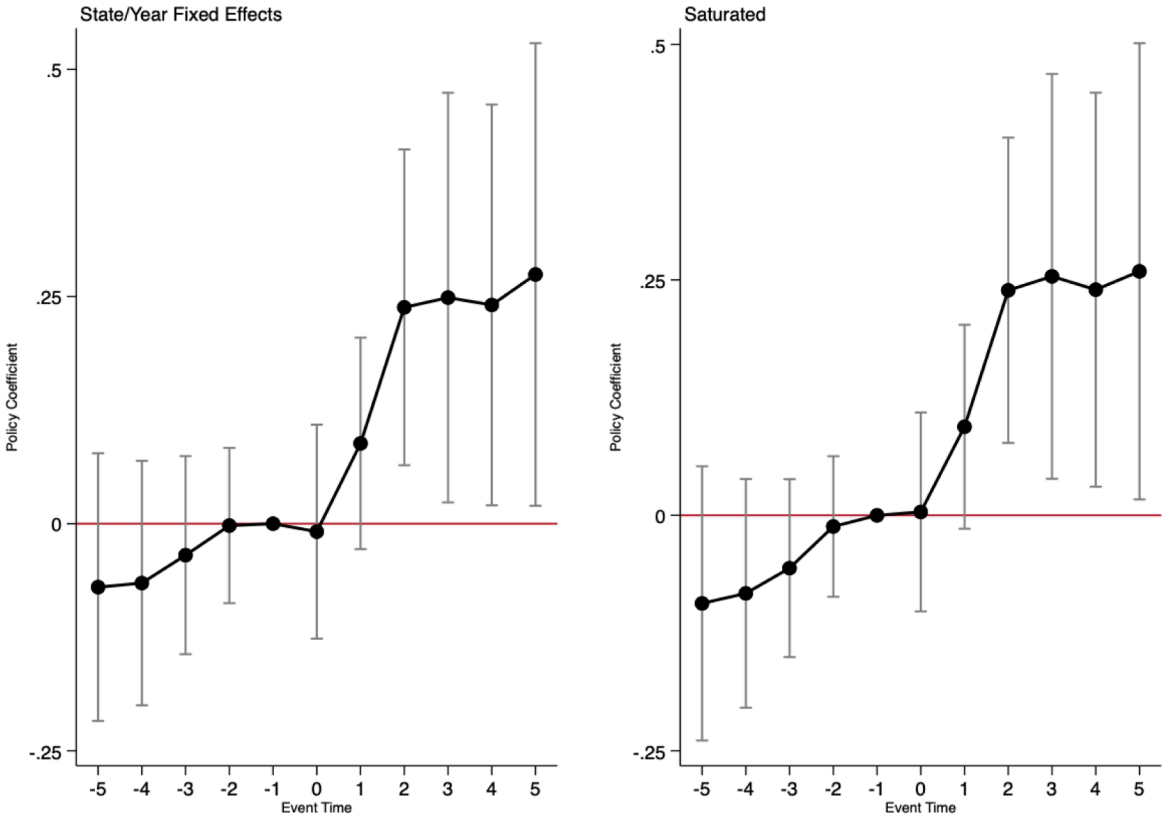

Fig. 3. Stacked Regression Event Study Estimates of Medical Marijuana Legalization Effect on Opioid Deaths.
Notes: Figure shows event study results for opioid mortality (T-Codes: 40.0–40.4, 40.6) from "stacked" Poisson regressions where the treatment states legalized medical marijuana between 2004 and 2014 and the control states had not legalized medical marijuana as of 2019. Each of these 11-years is treated as a "cohort", with separate samples created for each cohort, with data for each sample limited to the range of five years before and after the cohort date and to states that either legalized medical marijuana in the cohort year or were in the control group. All models include state and year fixed effects, the preferred set of controls, and adjust opioid mortality for incomplete reporting. Data are weighted by state populations. The "Saturated" models also include cohort-by-state and cohort-by-year fixed effects. Error bars show 95% confidence intervals, based upon robust standard errors clustered at the state-level.

coefficients turn positive and large as 2015 and subsequent years are included in the sample.[43] By contrast, the corresponding estimated increases in deaths from natural and semisynthetic opioids are much smaller, consistent with these patterns being driven by the emergence of fentanyl.[44]

9. Heterogeneity

The effects of marijuana legalization may vary across population groups, depending on how increased access to cannabis changes overall opioid use and specific types of consumption. To examine this, we estimated our preferred models for population subgroups stratified by sex, race/ethnicity, and age. Table 6 summarizes the results. We rarely obtain strong evidence of differential impacts and caution against overinterpreting those we do observe, given the large number of models and coefficients estimated. With that said, the findings suggest that retail recreational cannabis sales raise deaths more for males than females, blacks and Hispanics than non-Hispanic whites, and 15–49 year-olds than 50–64 year-olds, with the possibility of reduced death rates for seniors. Medical marijuana dispensaries also predict higher opioid deaths in almost all cases, but these increases seem relatively uniform across the groups,

[43] See Appendix Figure A.8 for the confidence intervals on these estimated effects. These issues have been addressed to some degree in prior research. For instance, Chan et al. (2020) limit their primary analysis sample to 28 states without statistically significant structural breaks in synthetic opioid mortality before and after 2013, as well as for other periods in robustness checks. Using their primary sample, they also examined predicted effects of cannabis legalization for all, prescription, and synthetic opioid deaths.

[44] The predicted changes in prescription opioid mortality are not statistically significant for samples ending prior to 2015 for MML and 2017 for MMD. An estimated increase in these deaths when the analysis is extended to include more recent years is still consistent with the emergence of fentanyl since it is increasingly being combined with natural/semisynthetic opioids (United States Drug Enforcement Administration 2022).

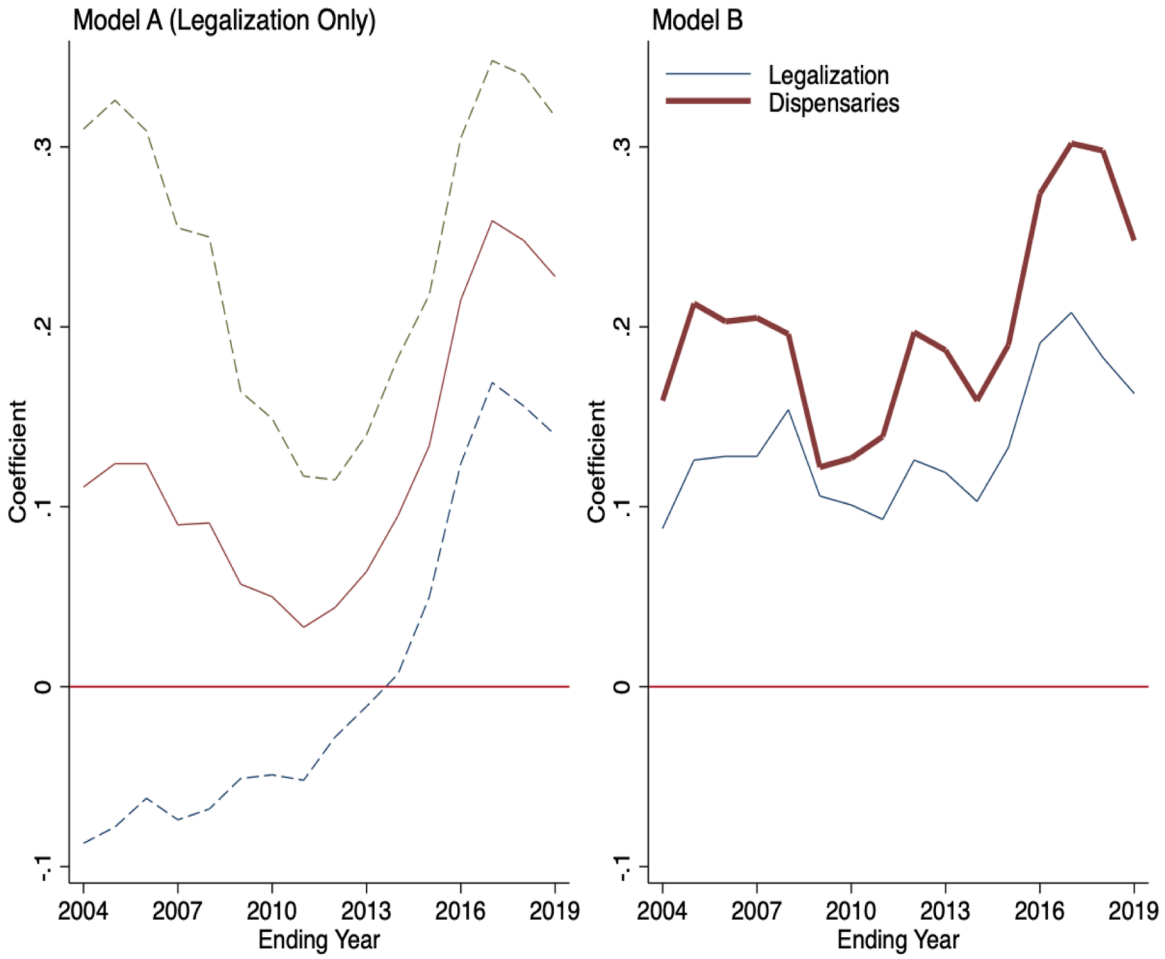

Fig. 4. Estimated Effects of Medical Marijuana Legalization and Dispensaries for Analysis Periods Starting in 1999 and Ending in Different Years. Note: Figure shows predicted effect of medical marijuana legalization or operating dispensaries on all opioid mortality (T-Codes: 40.0–40.4, 40.6) compared to no legalization for differing analysis periods. The starting year is always 1999 and the ending year various between 2004 and 2019. All estimates correct opioid deaths for under-reporting, use the preferred set of controls and include state and year dummy variables. In model A (the left panel) medical marijuana legalization is the only cannabis policy variable controlled for and the regressions do not use population weights. In this figure, dotted lines 95% confidence intervals, based upon robust standard errors clustered at the state-level. In model B, all four marijuana policy variables are controlled for (MML, MMD, RML and RMD), although only point estimates for the first two of these are shown, and state-year cells are weighted by population.

except for greater estimated growth for persons in their middle years (30–64 year-olds) than at other ages.

10. Discussion

There are many reasons why it may be desirable to legalize the use and sale of medical and recreational marijuana. Decreasing opioid mortality is not one of them. Some earlier research suggests that the legalization of medical cannabis reduces these deaths (Bachhuber et al., 2014), that it does so provided that there are sales through retail dispensaries (Powell et al., 2018), or that it is recreational marijuana that has these benefits (Chan et al., 2020). However, none of these results are robust to changes in models or time periods. Extending the analysis to include more recent years reverses the mortality reductions observed by Bachhuber et al. (2014), and using a slightly longer timespan and adding controls for recreational cannabis legalization does so for medical marijuana dispensaries. Even using the original study periods, we show that adjusting for incomplete reporting of drug involvement on death certificates, incorporating more comprehensive controls for supplementary covariates, more accurately timing policy implementation, and weighting the data by state populations, attenuates or eliminates any favorable estimated effects of cannabis legalization.

These conclusions are reinforced by our new analysis of data covering 1999–2019, which includes the aforementioned modeling characteristics and estimates Poisson rather than log-linear models. Our difference-in-differences estimates indicate that the availability of retail medical cannabis dispensaries raises opioid death rates by 28% in our preferred specification, with similar but less precisely predicted increases for states with dispensaries selling recreational marijuana. Legalization of medical marijuana, without

Table 5
Estimates of Marijuana Legalization on Overdose Deaths by Type of Opioid.

Legalization Status	All	Natural/Semisynthetic	Heroin	Synthetic
Medical	0.163***	0.223***	0.015	0.159*
	(0.044)	(0.068)	(0.128)	(0.095)
Medical + Dispensary	0.248***	0.207***	0.107	0.312**
	(0.051)	(0.079)	(0.165)	(0.144)
Recreational	0.134	0.129	−0.050	0.094
	(0.103)	(0.099)	(0.269)	(0.197)
Recreational + Dispensary	0.224**	0.233**	0.160	0.409
	(0.114)	(0.113)	(0.309)	(0.249)

Notes: Table shows estimated effects of various types of marijuana legalization on opioid deaths for the specified type of drug using data from 1999 to 2019 ($n = 1068$). Estimates are from Poisson models that include state and year fixed effects, and the preferred controls. Mortality counts are adjusted for incomplete reporting of drug involvement on death certificates. The coefficients on medical dispensaries and the recreational marijuana variables show predicted effects relative to no legalization of marijuana. All opioids include ICD-10 T-Codes 40.0–40.4 and 40.6, and Natural/Semisynthetic, Heroin, and Synthetic opioids refer to T-Codes 40.2, 40.1 and 40.4 respectively. Observations are weighted by state-year population. Standard errors in parentheses are robust and clustered at the state-level.
*$p<0.10$, **, $p<0.05$, ***$p<0.01$.

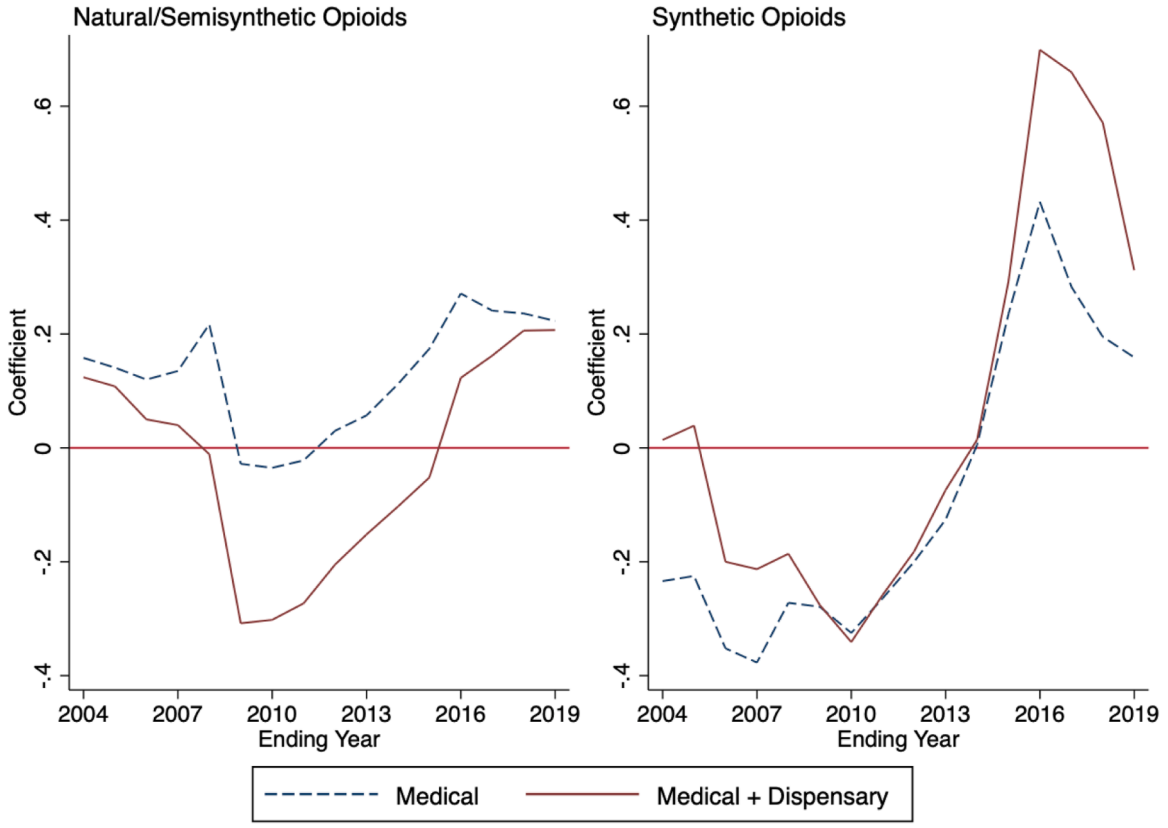

Fig. 5. Estimated Effects of Medical Marijuana Legalization and Dispensaries for Specific Causes of Opioid Deaths and Analysis Periods Starting in 1999 and Ending in Different Years.
Note: Figure shows point estimates of the predicted effect of medical marijuana legalization or operating dispensaries on mortality from natural/semisynthetic opioids (T-Code 40.2) and synthetic opioids (T-Code 40.4). The starting year is always 1999 and the ending year various between 2004 and 2019. All estimates correct opioid deaths for under-reporting, use the preferred set of controls, include state and year dummy variables, and control for all four marijuana policy variables (MML, MMD, RML and RMD). State-year cells are weighted by population.

Table 6
Estimates of Marijuana Legalization on Opioid Mortality by Population Group.

	All	Sex		Race/Ethnicity			Age (Years)			
		Male	Female	Non-Hispanic White	Non-Hispanic Black	Hispanic	15–29	30–49	50–64	≥65
Medical	0.163***	0.159***	0.165***	0.174***	0.047	0.157***	0.119**	0.205***	0.149**	0.009
	(0.044)	(0.046)	(0.042)	(0.043)	(0.080)	(0.046)	(0.056)	(0.046)	(0.059)	(0.051)
Medical + Dispensary	0.248***	0.225***	0.275***	0.244***	0.232**	0.177+	0.188***	0.255***	0.289***	0.154**
	(0.051)	(0.051)	(0.053)	(0.048)	(0.097)	(0.056)	(0.063)	(0.060)	(0.073)	(0.068)
Recreational	0.134	0.145	0.096	0.145	0.037	0.200*	0.149*	0.164	0.126	0.004
	(0.103)	(0.107)	(0.090)	(0.112)	(0.144)	(0.106)	(0.088)	(0.130)	(0.126)	(0.093)
Recreational + Dispensary	0.224**	0.258**	0.136	0.148	0.300	0.413***	0.338***	0.261*	0.179	−0.082
	(0.114)	(0.116)	(0.110)	(0.120)	(0.192)	(0.091)	(0.114)	(0.139)	(0.143)	(0.103)

Notes: Table shows estimated effects of various types of marijuana legalization on opioid deaths (T-Codes: 40.0–40.4, 40.6) for the specified type of group using data from 1999 to 2019 ($n = 1068$). Estimates are from Poisson models that include state and year fixed effects, and the preferred controls. Mortality counts are adjusted for incomplete reporting of drug involvement on death certificates. The coefficients on medical dispensaries and the recreational marijuana variables show predicted effects relative to no legalization of marijuana. Observations are weighted by group-year populations. Standard errors in parentheses are robust and clustered at the state-level.
>*$p<0.10$, **$p<0.05$, ***$p<0.01$.

retail dispensaries, generally has more modest effects, while still being associated with growth in opioid deaths. Key results are sensitive to the choice of sample periods. In particular, the 1999–2010 timespan, used in influential earlier research, happens to provide among the most favorable or least harmful estimates, compared to analyses ending either earlier or later.

Our event studies provide further evidence for the deleterious consequences of medical marijuana dispensaries, with flat pre-trends and increases in opioid mortality rates that rise with time since policy implementation. The results for recreational marijuana dispensaries are less consistent, reducing our confidence in these estimates. We suspect this occurs because recreational cannabis has been legalized only recently (if at all) in most states, and with recreational marijuana dispensaries operational over even shorter periods. Nevertheless, the results raise the possibility that recreational cannabis dispensaries may raise opioid death rates more for males, blacks, Hispanics, and 15–49 year-olds than for females, non-Hispanic whites, and older individuals.

Although we cannot be sure about mechanisms for the observed effects, it seems likely that changes in the "drug environment" have played a role. Specifically, with the emergence of fentanyl and its analogs, the use of illicit drugs has probably become increasingly toxic. Supporting this possibility, the positive association between cannabis legalization and opioid mortality strengthens when extending the analysis past 2014, precisely when synthetic opioids began to emerge as the dominant source of opioid mortality. Moreover, retail cannabis sales predict particularly large increases in synthetic opioid deaths relative to those for other opioids but these effects only materialize when extending the timespan to include periods when illicit fentanyl had penetrated deeply into the United States.

One possibility is that some users of legal marijuana transition to opioids. However, evidence on such potential "gateway" effects remains ambiguous and further study is needed.[45] Of particular note, analysis by Ali et al. (2021) of restricted use data from the National Survey of Drug Use and Health indicates that there is little relationship between the legalization of recreational marijuana and either the misuse of prescription opioids or the use of heroin.[46] Supply-side responses present a potential alternative mechanism. For example, as legal marijuana has become more widely available, profits for illicit cannabis suppliers may have fallen, causing some of them to transition into fentanyl markets, whose wider availability then increases opioid deaths.[47]

It is critical to understand the counterfactual to which the various types of marijuana legalization are being compared. For example, Chan et al. (2020) indicate that retail sales of recreational marijuana have beneficial effects, but this is relative to states with legal recreational cannabis but no dispensaries. Sabia et al. (2021) examine how the legalization of recreational marijuana influences a variety of outcomes, compared to a control group of states where medical marijuana is legal.[48] While such counterfactuals will sometimes be of interest, we suspect that for many applications the more relevant comparison is to no legal marijuana and this is what we have primarily focused upon. However, we also provide the information needed for other counterfactual comparisons – such as the impact of medical marijuana dispensaries compared to legal medical marijuana without retail sales.

[45] Several studies find that legal marijuana is associated with lower rates of opioid prescribing, although often to senior citizens who may be unlikely to use illegal drugs (Bradford and Bradford 2016; Bradford et al. 2018). The evidence is mixed for illicit drug use. Some research finds no or negative effects on consumption of these drugs (Chu 2015; Wen, Hockenberry, and Cummings 2015), while other investigations indicate that marijuana legalization increases their use (Kelly and Rasul 2014; Conyers and Ayres 2020; Hollingsworth et al., 2022).

[46] Interestingly, medical marijuana is positively associated with heroin use in their study, but the estimated effects are small and statistically insignificant.

[47] We are not aware of any research on this possibility.

[48] More specifically, their models also include controls for medical cannabis legalization or decriminalization. As discussed, medical marijuana is already legal in all states permitting recreational cannabis.

It is important to recognize limitations of quasi-experimental analyses for answering the questions posed here, and probably also for understanding the effects of marijuana legalization on other outcomes. Cannabis policies are heterogenous, as reflected by our use of four distinct treatment variables. This presents challenges for recently developed methods estimating difference-in-differences models with staggered policy implementation and our examination of these methods is limited. Moreover, there remains considerable cross-state variation within these general policy categories. For instance, states differ in the types of health problems for which medical cannabis is permitted. A variety of other factors, such as possible changes in norms regarding the coding of opioid deaths after marijuana legalization, could also affect the results.

There are also a number of more general caveats. As shown above, legalization may sometimes have less harmful or more beneficial outcomes than the average treatment effects we estimate and the impacts of given policies may change as the opioid crisis evolves. Reasonable modifications in model specifications sometimes also affect the estimates. For instance, incorporating state-specific time trends into our preferred models attenuates the increase in opioid deaths associated with retail medical marijuana sales by more than 40% (but does not eliminate it). In addition, with few exceptions, the legalization of recreational marijuana is recent, raising considerable difficulty in reliably estimating the associated impacts. Substate variations could also be important in ways not captured by this analysis, although some research has begun to address this issue.

Given these issues, a more conservative reading of our findings is that while we cannot be completely confident exactly how marijuana legalization affects opioid mortality, there is no reason to think that it saves lives. Even assuming that retail marijuana sales do raise opioid mortality, which we believe is likely, they are not a primary driver of its observed growth. Our preferred estimates for the 1999–2019 period suggest that operating medical marijuana dispensaries increased opioid-involved fatality rates by 28% versus no legal cannabis. Around 58% of the country's population lived in such states in 2019, implying that this treatment effect predicts a 16% growth in national opioid mortality rates from 1999 to 2019.[49] Since the actual rise was 342% (from 3.66 to 16.17 per 100,000), other factors are responsible for over 95% of this increase.

CRediT authorship contribution statement

Neil K. Mathur: Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Writing – original draft, Writing – review & editing. **Christopher J. Ruhm:** Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Supervision, Writing – original draft, Writing – review & editing.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jhealeco.2023.102728.

References

Abouk, Rahi, Adams, Scott, 2018. Examining the relationship between medical cannabis laws and cardiovascular deaths in the US. Int. J. Drug Policy 53, 1–7.

Ahmad, F.B., Rossen, L.M., Sutton, P., 2021. Provisional drug overdose death counts. Vital Statistics Rapid Release. National Center for Health Statistics.

Ali, Mir M, McClellan, Chandler B, Mutter, Ryan, Rees, Daniel I, 2021. Recreational marijuana laws and the use of opioids: evidence from NSDUH microdata. NBER Working Paper No. 29087. National Bureau of Economic Research.

Anderson, Mark D., and Rees, Daniel I., "The public health effects of legalizing marijuana," *J. Econ. Lit.*, (Forthcoming).

Angrist, Joshua David, Pischke, Jörn-Steffen, 2009. Mostly Harmless Econometrics: an Empiricist's Companion. Princeton University Press, Princeton.

Aran, Adi, Cayam-Rand, Dalit, 2020. Medical cannabis in children. Rambam Maimonides Med. J. 11, e0003.

Bachhuber, Marcus A., Saloner, Brendan, Cunningham, Chinazo O., Barry, Colleen L., 2014. Medical cannabis laws and opioid analgesic overdose mortality in the United States, 1999-2010. JAMA Intern. Med. 174, 1668–1673.

Baker, Andrew, Larcker, David F., Wang, Charles C.Y., 2021. How much should we trust staggered difference-in-differences estimates? (European Corporate Governance Institute - Finance Working Paper No. 736/2021.

Borusyak, Kirill, Jaravel, Xavier, and Spiess, Jann, "Revisiting event study designs: robust and efficient estimation," arXiv preprint arXiv:2108.12419, (2022) (arXiv).

Bradford, Ashley C., Bradford, W.David, 2016. Medical Marijuana Laws Reduce Prescription Medication Use In Medicare Part D, 35. Health Affairs (Project Hope), pp. 1230–1236.

Bradford, Ashley C., Bradford, W.David, Abraham, Amanda, Bagwell Adams,, Grace, 2018. Association between US state medical cannabis laws and opioid prescribing in the medicare part D population. JAMA Intern. Med. 178, 667–672.

Callaway, Brantly, Sant'Anna, Pedro H.C., 2021. Difference-in-Differences with multiple time periods. J. Econom. 225, 200–230.

Cengiz, Doruk, Dube, Arindrajit, Lindner, Attila, Zipperer, Ben, 2019. The effect of minimum wages on low-wage jobs. Q. J. Econ. 134, 1405–1454.

Centers for Disease Control and Prevention, "National center for health statistics mortality data on CDC WONDER," *CDC Wonder*, https://wonder.cdc.gov/mcd.html (2022) (May 15, 2022).

de Chaisemartin, Clément, D'Haultfoeuille, Xavier, 2022. Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: a survey. NBER Working Paper No. 29691. National Bureau of Economic Research. Working Paper.

de Chaisemartin, Clément, D'Haultfœuille, Xavier, 2022. Two-way fixed effects regressions with several treatments. NBER Working Paper No. 29734. Working Paper.

Chan, Nathan W., Burkhardt, Jesse, Flyr, Matthew, 2020. The effects of recreational marijuana legalization and dispensing on opioid mortality. Econ. Inq 58, 589–606.

Chu, Yu-Wei Luke, 2015. Do medical marijuana laws increase hard-drug use? J. Law Econ. 58, 481–517.

Cinelli, Carlos, Forney, Andrew, Pearl, Judea, 2020. A crash course in good and bad controls. SSRN Electron. J.

Conyers, Gregory, Ayres, Ian, 2020. A lottery test of the effect of dispensaries on emergency room visits in Arizona. Health Econ. 29, 854–864.

[49] No states allowed medical marijuana dispensaries in 1999 so these numbers are calculated by multiplying 28% by 0.58.

Goodman-Bacon, Andrew, 2021. Difference-in-differences with variation in treatment timing. J. Econom. 225, 254–277.
Harris, Patrice A., Mukkamala, Bobby, 2020. Advocacy and action to end the opioid epidemic by the AMA opioid task force. AMA J. Ethics 22, 718–722 (American Medical Association).
Hedegaard, Holly, Miniño, Arialdi, Spencer, Merianne Rose, Warner, Margaret, 2021. Drug Overdose Deaths in the United States, 1999–2020. National Center for Health Statistics (U.S.).
Hsu, Greta, Kovács, Balázs, 2021. Association between county level cannabis dispensary counts and opioid related mortality rates in the United States: panel data study. BMJ m4957.
Jayawardhana, Jayani, Fernandez, Jose M., 2021. The associations of medical marijuana policies with opioid-related health care utilization. Health Serv. Res. 56, 299–309.
Katcher, Bradley A., Ruhm, Christopher J., 2021. Federal Opioid Grant Funding Favors Least Populous States, Not States With Greatest Need, 40. Health Affairs (Project Hope), pp. 829–836.
Kelly, Elaine, Rasul, Imran, 2014. Policing cannabis and drug related hospital admissions: evidence from administrative records. J. Public Econ. 112, 89–114.
Kochanek, Kenneth D., Xu, Jiaquan, Murphy, Sherry L., Miniño, Arialdi M., Kung, Hsiang-Ching, 2011. Deaths: final data for 2009. In: National Vital Statistics Reports: From the Centers for Disease Control and Prevention, 60. National Center for Health Statistics, National Vital Statistics System, pp. 1–116.
Hollingsworth, Alex, Wing, Coady, Bradford, Ashley C., 2022. Comparative effects of recreational and medical marijuana laws on drug use among adults and adolescents. J. Law Econ. 65 (3), 515–554.
Marijuana Policy Project, "Key Aspects of State and D.C. Medical Marijuana Laws," MPP, <https://www.mpp.org/issues/medical-marijuana/state-by-state-medical-marijuana-laws/key-aspects-of-state-and-d-c-medical-marijuana-laws/>(2022) (Dec. 15, 2022).
McMichael, Benjamin J., Van Horn, R.Lawrence, Viscusi, W.Kip, 2020. The impact of cannabis access laws on opioid prescribing. J. Health Econ. 69, 102273.
Neumark, David, Salas, J.M.Ian, Wascher, William, 2014. Revisiting the minimum wage—employment debate: throwing out the baby with the bathwater? ILR Rev. 67, 608–648.
NIDA, "New Jersey: opioid-involved deaths and related harms," <https://www.drugabuse.gov/drug-topics/opioids/opioid-summaries-by-state/new-jersey-opioid-involved-deaths-related-harms>(2020) (Oct. 12, 2021).
Pacula, Rosalie Liccardo, Smart, Rosanna, 2017. Medical marijuana and marijuana legalization. Annu. Rev. Clin. Psychol. 13, 397–419.
Powell, David, Pacula, Rosalie Liccardo, Jacobson, Mireille, 2018. Do medical marijuana laws reduce addictions and deaths related to pain killers? J. Health Econ. 58, 29–42.
Purington, Kitty, 2019. Tackling the Opioid Crisis: What State Strategies are Working? – The National Academy for State Health Policy. National Academy for State Health Policy.
Ruhm, Christopher J., 2017. Geographic variation in opioid and heroin involved drug poisoning mortality rates. Am. J. Prev. Med. 53, 745–753.
Corrected US opioid-involved drug poisoning deaths and mortality rates, 1999-2015. Addiction 113, 2018, 1339–1344.
Sabia, Joseph J., Dave, Dhaval M., Alotaibi, Fawaz, Rees, Daniel I., 2021. Is recreational marijuana a gateway to harder drug use and crime? NBER Working Paper No. 29038. National Bureau of Economic Research.
Shover, Chelsea L., Davis, Corey S., Gordon, Sanford C., Humphreys, Keith, 2019. Association between medical cannabis laws and opioid overdose mortality has reversed over time. Proc. Natl. Acad. Sci. U.S.A. 116, 12624–12626.
Shover, Chelsea L., Vest, Noel A., Chen, Derek, Stueber, Amanda, Falasinnu, Titilola O., Hah, Jennifer M., Kim, Jinhee, Mackey, Ian, Weber, Kenneth A., Ziadni, Maisa, Humphreys, Keith, 2020. Association of state policies allowing medical cannabis for opioid use disorder with dispensary marketing for this indication. JAMA Netw. Open 3, e2010001.
Smart, Rosanna, 2015. The kids aren't alright, but older adults are: how medical marijuana market growth impacts adult and adolescent substance-related outcomes. SSRN Electron. J.
Smith, Rhet A., 2020. The effects of medical marijuana dispensaries on adverse opioid outcomes. Econ. Inq. 58, 569–588.
Solon, Gary, Haider, Steven J., Wooldridge, Jeffrey M., 2015. What are we weighting for? J. Hum. Resour. 50, 301–316.
Sun, Liyang, Abraham, Sarah, 2021. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. J. Econom. 225, 175–199.
Swyter, Nick, Talamo, Lex, and Kelley, Calah, "Resolute parents demand marijuana treatment for children news21: America's weed rush," <https://weedrush.news21.com>(2015) (Oct. 19, 2021).
United States Drug Enforcement Administration, "One pill can kill," <https://www.dea.gov/onepill>(2022) (Aug. 11, 2022).
Wen, Hefei, Hockenberry, Jason M., 2018. Association of medical and adult-use marijuana laws with opioid prescribing for medicaid enrollees. JAMA Intern. Med. 178, 673–679.
Wen, Hefei, Hockenberry, Jason M., Cummings, Janet R., 2015. The effect of medical marijuana laws on adolescent and adult use of marijuana, alcohol, and other substances. J. Health Econ. 42, 64–80.
Wolfers, Justin, 2006. Did unilateral divorce laws raise divorce rates? A reconciliation and new results. Am. Econ. Rev. 96, 1802–1820.
Wooldridge, Jeffrey M., 2010. Econometric Analysis of Cross Section and Panel Data. MIT Press, Cambridge, Mass.