NBER WORKING PAPER SERIES

MARIJUANA LEGALIZATION AND OPIOID DEATHS

Neil K. Mathur
Christopher J. Ruhm

Working Paper 29802
http://www.nber.org/papers/w29802

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2022

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 and the 2022 American Economic Association annual meeting for helpful comments on an earlier version 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. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Marijuana Legalization and Opioid Deaths
Neil K. Mathur and Christopher J. Ruhm
NBER Working Paper No. 29802
February 2022
JEL No. I10,I12,I18

ABSTRACT

Over the last two decades there has been considerable movement at the state-level to legalize marijuana, initially for medical purposes and more recently for recreational consumption. Despite prior research, it is unclear how, if at all, these policies are related to rates of opioid-involved overdose deaths, which have trended rapidly upwards over time and represent a major public health problem. We provide two types of new information on this question. First, we replicate and extend upon previous investigations and show that the empirical results of those studies are frequently fragile and that, in most cases, the inclusion of more comprehensive controls, longer analysis periods and more correctly defined dependent variables results in less favorable estimates, often including predicted increases in opioid deaths. Second, we present new estimates from generalized differences-in-differences and event study models that incorporate more recent data and improvements developed in our replication and extension of early research. These results indicate 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 suggest that retail sales through dispensaries are associated with greater death rates relative to the counterfactual of no legal cannabis.

Neil K. Mathur
University of Virginia
nkm7da@virginia.edu

Christopher J. Ruhm
Frank Batten School of
Leadership and Public Policy
University of Virginia
235 McCormick Rd.
P.O. Box 400893
Charlottesville, VA 22904-4893
and NBER
ruhm@virginia.edu

More than 930,000 Americans died of drug overdoses from 1999-2020 (Hedegaard et al. 2021). A large majority of these involved opioids, and both all drug mortality and deaths implicating opioids accelerated markedly during the first year of the COVID-19 pandemic. In response to these alarming trends, there have been multiple federal, state and local efforts to reduce opioid deaths and related problems including: 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 other endeavors (Purington 2019; Harris and Mukkamala 2020; Katcher and Ruhm 2021).

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

A rapidly growing body of scholarship examines the relationship between marijuana legalization and various aspects of public health. In their review, Anderson and Rees

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

(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 its effects on opioid-related outcomes such as prescribing behavior (Bradford and Bradford 2016; Bradford et al. 2018; Wen and Hockenberry 2018; McMichael, Van Horn, and Viscusi 2020) and admissions to substance abuse treatment programs, emergency departments, or hospitals (Chu 2015; Powell, Pacula, and Jacobson 2018; Conyers and Ayres 2020; Jayawardhana and Fernandez 2021).[2]

Finally, researchers have examined how marijuana legalization is related to opioid deaths. These studies, some of which are summarized in the next section, while not voluminous, have been influential. Particularly prominent is Bachhuber et al.'s (2014) conclusion that "medical cannabis laws are associated with significantly lower state-level opioid overdose mortality rates" (p. 1668). This study has been widely cited (over 760 Google Scholar citations as of February 2022) and has 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).[3] However, as discussed below, these findings turn out not to be robust to changes in the analysis period, with subsequent research yielding ambiguous findings.

The current study provides more definitive information on the relationship between marijuana legalization and opioid deaths. We first show that prior empirical results are

[2] The studies cited above, which are just examples of the larger related literature, generally suggest that marijuana legalization reduces opioid prescribing, but with more mixed effects on admissions to treatment programs, emergency departments, or hospitals.

[3] 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 also sometimes advocating medical marijuana as a treatment for pediatric health issues (Swyter, Talamo, and Kelley 2015).

frequently fragile and that, in most cases, the inclusion of more comprehensive controls, longer analysis periods and more correctly defined treatment variables results in less favorable estimates or deleterious predicted effects of legal cannabis. We then present new estimates, from generalized differences-in-differences (DiD) and event study (ES) models, that incorporate more recent data and the improvements developed in our replication and extension of previous research.

These results indicate that legal medical marijuana, particularly when available through retail dispensaries, is associated with higher opioid death rates. The estimates for recreational marijuana while less reliable – probably because most such policies have been only recently enacted and in a lower number of states than for medical marijuana – also suggest that retail sales through dispensaries are associated with greater opioid mortality, relative to the counterfactual of no legal cannabis. There is also suggestive evidence of heterogeneity across demographic groups, with stronger deleterious recreational marijuana effects for males, nonwhites, and relatively young adults than for their counterparts. Retail cannabis sales also likely increase deaths involving non-opioid drugs such as stimulants and sedatives. Finally, we indicate that more favorable findings previously observed when analyzing deaths from 1999-2010 may reflect idiosyncratic and unreliable findings when considering short time periods rather than, as suggested by some researchers, changes over time in the stringency of the regulatory approaches.

1. *Estimated Legalization Effects from Prior Research are Ambiguous*

Bachhuber et al. (2014), mentioned above, used public-use *National Vital Statistics System* (*NVSS*) data from 1999-2010 to examine the relationship between medical marijuana legalization (MML) and opioid deaths.[4] Their estimates suggest that MML reduced age-

[4] The starting year of analysis is 1999 in this and most other studies 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.

adjusted opioid analgesic mortality by almost 25% and a broader measure of opioid deaths by 23%, in models with state and year fixed effects, although with some attenuation 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 also 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.

Powell, Pacula, and Jacobson's (2018) innovation is to distinguish between the legalization of medical marijuana and the availability of retail sales to qualified patients through authorized medical marijuana dispensaries (MMD). Using non-public *NVSS* data, they confirm Bachhuber's (2014) negative relationship between legalization of medical marijuana and opioid deaths from 1999-2010 but, consistent with Shover et al. (2019), show that the effects weaken and become statistically insignificant when extending the period through 2013. However, their key finding is 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 without legal cannabis.

Using similar methods and data for 1999-2017, Chan, Burkhardt and Flyer (2020), add controls for the legalization of *recreational* marijuana (RML), as well as corresponding 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 interpret to imply a 21% decrease in opioid death rates.[5] However, this conclusion depends critically on the counterfactual comparison. Specifically, the corresponding RML coefficient is 0.19, implying that while RMD reduces predicted opioid

---

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

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.[6]

In recent work, Sabia et al. (2021) uses data from 2000-2019 to examine how the legalization of recreational marijuana relates to a variety of outcomes, including mortality rates. They provide suggestive evidence of beneficial effects, but the estimates attenuate and frequently become statistically insignificant or detrimental with the inclusion of more comprehensive controls or if recreational marijuana sales, rather than legalization, is used as the treatment variable. They also do not control for the legalization of medical marijuana in any of their models, so that the counterfactual combines states without legal marijuana and those allowing medical cannabis.

The aforementioned studies use *annual state-level* data and estimate differences-in-differences models that include state fixed effects, general year effects, and a variety of supplementary covariates. By contrast, Smith (2020) collected *monthly county-level* data on marijuana dispensaries, from 1999-2014, and estimates that their availability reduces opioid-related mortality by 11%. This analysis does not distinguish between medical and recreational marijuana dispensaries, nor does it separately consider legalization without retail sales. Another study using county-level data (Hsu and Kovács 2021) finds that increased counts of (medical or recreational) marijuana dispensaries predict lower opioid deaths. However, this investigation covers a short period (2014-2018) and its log-linear models may be poorly suited for the county-level data used, since a large fraction of locations had zero deaths in at least some years.

[6] 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.

There are also alternative ways of classifying cannabis legalization policies. For instance, rather than using retail dispensaries, Anderson and Rees (Forthcoming) advocate for dividing locations by whether or not they permit the collective cultivation (i.e. group-growing) of marijuana. To our knowledge, no current studies of marijuana legalization and opioid deaths have used this categorization.

2. Previous Findings are Not Robust to Specification Changes

We begin our empirical analysis by replicating and extending results of four studies described above that use annual state-level data and DiD methods (Bachhuber et al. 2014; Powell, Pacula, and Jacobson 2018; Shover et al. 2019; Chan, Burkhardt, and Flyr 2020). We first employ specifications and data similar or identical to those originally used and are largely successful in recreating the results when doing so. Where possible, we obtained data directly from the papers, online supplements, or from authors of the studies. When this was not feasible, either because the information was restricted (as with some mortality data) or the authors did not provide us with their original data, we obtained the information from other sources. Since the earlier papers frequently estimated multiple models, we focus here on the primary specifications used, provided that the sample included data from all states.[7] Table 1 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. After these replications, we extend the analyses in a variety of ways, as described below.

2.1. Methods

Following the prior studies, the marijuana legalization effects are estimated from differences-in-differences (DiD) models of the form:

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

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

where D_{it} is the death rate in state i and year t, M_{it} indicates one or more marijuana legalization policies, X_{it} are supplementary covariates, S_i and Y_t are vectors of state and year fixed effects, and ϵ_{it} is the regression error term. $\hat{\beta}$ estimates the impact of cannabis legalization.

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 where legal dispensaries never exist without more general legalization and recreational marijuana is never legalized without medical marijuana being both legal and available for retail sale. This implies that the coefficient estimates $\hat{\beta}_1$ through $\hat{\beta}_4$ show *incremental* effects of policies versus the next lower form of legalization. For instance, $\hat{\beta}_4$ indicates the predicted impact of recreational marijuana dispensaries above and beyond that of legal medical marijuana, medical marijuana dispensaries, and recreational marijuana. However, we focus on the effects of the specified legalization policy versus the case where all forms of cannabis are illegal. In the example just provided, this is estimated as: $\hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3 + \hat{\beta}_4$.[8] When comparing with prior studies, it is possible to directly compare the point estimates but not the standard errors, since these were not supplied in the earlier analyses.

---

[8] Alternatively, these effects can be obtained directly from the regressions if the treatment variables are redefined to be mutually exclusive (so that MML equals one if medical marijuana is legal but dispensaries or recreational marijuana is not, MMD equals one if medical marijuana dispensaries are legal but recreational marijuana is not, etc.).

Following this replication effort, we extend the original models in four ways. First, we adjust for the incomplete reporting of the specific types of drugs involved in overdose deaths and use carefully cross-checked and verified information on the timing of marijuana legalization policies. Correcting for incomplete reporting is potentially important because the drug categories involved in overdose deaths are not identified in 15% to 25% of cases (depending on the year).[9] The adjustment procedures, which have been detailed in earlier research (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 these estimates 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.

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. However, obtaining this information is difficult, and was particularly so for early research where consolidated databases did not exist. One result is that these policy measures frequently differ across studies. For this analysis, we use data compiled and verified by the RAND Corporation, as part of their Opioid Tools and Information Center Resources (RAND-OPTIC). 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".[10]

[9] 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.

[10] 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 (described below) is available at: https://www.rand.org/health-care/centers/optic/resources/datasets.html.

When cannabis policies are in place for only part of the year, we create fractional variables indicating the number of months they were active.[11] We also date the marijuana dispensary treatment variables according to when the first legally-protected dispensary operates in the state, rather than when policies permitting dispensaries are legislatively enacted. These procedures follow those used in the RAND-OPTIC data.[12]

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 for non-Hispanic whites, males, and four age groups (18-24, 25-44, 45-64 and ≥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.[13]

---

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

[12] In three cases we 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 effective 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 legalized; we treat this as the effective date for both MML and MMD.

[13] 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.

Next, we sometimes weight observations by state-year populations. Weighting likely improves the estimate of the average treatment effects, given the heterogeneity in both the marijuana legalization policies themselves 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 in 2019 (21.478 million vs. 0.762 million).[14]

Our final model includes all the previously described supplementary covariates, uses population weights, and adds a set of state-specific linear time trends. Their inclusion may be appropriate given that the sharp, but somewhat heterogeneous, trend increases in opioid deaths may reflect factors difficult to fully control for even with the comprehensive covariates previously described. On the other hand, there is a risk of "overcontrolling", particularly when the treatment effects are dynamic (Wolfers 2006). We will not attempt to resolve whether it is best to include state trends at this juncture, but the event studies presented later in the paper may help to address this issue.

2.2 Replication/Extension Results

The first column of Table 2 summarizes original findings of the four studies. The second shows our replication of these results, using the author's original data and programs if available or recreating them as closely as possible if not. Column (3) displays the estimates after adjusting for incomplete reporting of opioid mortality rates on death certificates and using our updated information on the timing of the marijuana legalization policies. In this and all subsequent models we use the full sample, including state-year observations suppressed from the public-use datasets due to small numbers of deaths. Column (4) adds our preferred (more comprehensive) controls, which also may increase sample sizes due to missing data for some supplementary covariates in the original studies. Column (5) weights

[14] Source: https://www.census.gov/data/tables/time-series/demo/popest/2010s-state-total.html.

observations by state population and column (6) adds controls for state-specific linear time trends. As mentioned, when multiple cannabis policy variables are examined, the table shows estimated effects 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, Pacula, and Jacobson 2018; Shover et al. 2019), with somewhat larger differences in the other two (Bachhuber et al. 2014; Chan, Burkhardt, and Flyr 2020), as might be expected because we did not acquire the underlying data directly from the authors in those cases but rather use corresponding information from another study or had to compile it ourselves.[15] The conclusions are the same using either the original study findings or our replications of them. Specifically, if correct, these suggest that: 1) legalization of medical marijuana was associated with lower opioid death rates during the first decade of the 21st century but that this reverses when extending the analysis through 2017; 2) the effects of medical marijuana dispensaries are ambiguous, with strong evidence of beneficial mortality effects from 1999-2013 but with attenuated or reversed impacts when extending the analysis through 2017 and controlling for recreational cannabis legalization; 3) legalization of recreational marijuana either has no effect on or raised deaths when retail sales are not permitted, but with little effect or possibly a small benefit, when dispensaries operate.

However, we should have little confidence in these conclusions because the estimates are quite sensitive to the choice of specifications, as shown in columns (3) through (6). Less favorable results are often obtained when using more accurate policy data and more comprehensive controls. The Bachhuber et al. (2014) finding of beneficial effects of legal

[15] Specifically, we obtained data from Shover et al. (2019), which they use in their replication of Bachhuber et al. (2014). Our results for 1999-2010 are like 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.

medical marijuana from 1999-2010 attenuates by more than 40% when moving from column (2) through (5) and over 80% in column (6). Similarly, the advantageous estimated impact of medical marijuana dispensaries, identified by Powell et al. (2018), is cut more than 50% when moving from column (2) to column (5) and reverses sign (while not quite reaching statistical significance) in column (6). The recreational marijuana legalization variables also vary substantially. RML is predicted to *raise* mortality rates in all specifications, but by much smaller amounts using weighted data and without time trends than otherwise, and with most estimates being imprecise. Conversely, recreational marijuana dispensaries are correlated with lower opioid death rates, but the reductions are often small and never precisely measured; even large harmful effects cannot be ruled out.

The main conclusion from this replication and extension exercise is that prior results are sensitive to the choice of time periods, specifications and other variables controlled for. Perhaps the most consistent finding is that legal medical cannabis without the availability of retail sales raises opioid death rates when the analysis period extends beyond 2010. Powell et al.'s (2018) evidence of the beneficial effects of medical marijuana dispensaries also do not appear to persist when including more recent data and controlling for recreational cannabis legalization. This should not be surprising given that during most of the period they study, 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. This again probably reflects the recency of these policies. Chan et al.'s (2020) analysis period stopped in 2017, at which point just two states (Colorado and Washington) had legal recreational marijuana in place for at least two years. These ambiguities highlight the importance of additional analyses, using more recent data to provide a longer time horizon after the implementation of some policies, with more comprehensive testing of the key DiD assumptions, and a variety of other methodological improvements.

3. Legalization of Retail Marijuana Sales Raises Opioid Mortality

Next, we provide an in-depth examination of the effects of cannabis legalization on opioid deaths over the 1999-2019 period. Since the data and most methods are similar to those detailed above, we describe them briefly, highlighting important differences, extensions and providing additional information on our treatment variables. This is followed by presentation of our main results, an extensive series of robustness checks, a brief examination of differences in the estimated policy effects across types of drug deaths and population subgroups, and an investigation of the sensitivity of the results to the choice of analysis periods.

3.1 Data and Methods

We use National Vital Statistics System data from the *Multiple Cause of Death* (*MCOD*) files for the universe of U.S. deaths from 1999-2019. 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 2021). 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, not provided on 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 1,068.[16]

---

[16] 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

Construction of the marijuana policy treatment variables and supplementary covariates was discussed in Section 2.1. Effective dates for the four policies are summarized in Figure 1, with details provided in Appendix Table A.1; blank cells indicate no enactment of the policy through the end of 2019. Mean values, both weighted and unweighted, and sources of these variables and the supplementary covariates are contained in Appendix Table A.2.

The primary differences-in-differences estimation model is:

$$D_{it} = \alpha + \boldsymbol{M}_{it}\boldsymbol{\beta} + \boldsymbol{X}_{it}\boldsymbol{\gamma} + \boldsymbol{S}_i + Y_t + \epsilon_{it}, \tag{3}$$

where D_{it} is the number of opioid deaths in state i and year t. This model differs from equation (1), used previously for our replication analysis, in two important ways. First, the dependent variable is a count and so is estimated as a Poisson, rather than a log-linear, model with robust standard errors clustered at the state-level. 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).[17] Second, we use a more comprehensive definition of opioid deaths that covers ICD-10 T-codes 40.0-40.4 and 40.6, rather than the narrower T40.1-40.4 or T40.2-40.4 codes used in the studies discussed above. Our definition is consistent with that used by the Centers for Disease Control and Prevention (Ahmad, Rossen, and Sutton 2021); however, we also examine the sensitivity of our estimates to this choice.[18] $\boldsymbol{X}_{it}$, in (3), refers to the vector of "preferred controls", described previously, that account for state demographic characteristics and policy variables. The data are again weighted by state-year

---

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.

[17] 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 if relaxed when using robust standard errors.

[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).

populations in our preferred models and state-specific time trends are sometimes incorporated.

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 the shorter window for recreational marijuana since there are fewer years of post-treatment data available. In all cases, the longest lag (lead) is set to one when the policy was implemented before (after) that amount of time. For instance, for MML, M_{it+8} is set to one for all periods eight or more years after medical marijuana was legalized. 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. For example, if a policy went into effect on July 1, 2010, the time M_{it} takes the value of 0.5 in 2010 and 0.5 in 2011, and M_{it+1} equals 0.5 in 2011 and 0.5 in 2012. We compute the ES estimates with controls for lags and leads of only one policy variable at a time, but with contemporaneous values of the other three policy variables included.[19] Finally, 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

[19] For example, in the ES models examining medical marijuana dispensaries, the models also include contemporaneous controls for MML, RML, and RMD.

3.2 Differences-in-Differences Results

Table 3 summarizes the primary DiD estimates. Its format resembles the replication analysis in Table 2, but we now use the full 1999-2019 period, the 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 also includes medical marijuana dispensaries (MMD) in the set of treatment variables, and the lower panel additionally incorporates recreational marijuana (RML) and corresponding dispensaries (RMD). Generally, estimates with all four policy variables are most informative, although the models with less complete controls are useful for comparison with earlier work or for some of the sensitivity testing discussed below.[20]

A key finding is that the legalization of medical marijuana cannabis, and its sale through retail dispensaries, is associated with *higher* opioid death rates. For example, in model (5) – which adjusts mortality rates for incomplete reporting, includes the preferred set of supplementary covariates, and weights the data by population – MML is associated with a 16% to 21% increase in opioid death rates, and the combination of MML and MMD with a rise of around 28%.[21] These estimates attenuate substantially, to a statistically insignificant 6% to 8% for MML, when adding state-specific linear trends to the model, but they remain strongly positive (an estimated 15% increase) where legalization is accompanied by dispensaries.

The results for recreational marijuana are more ambiguous. The RML coefficients are strongly positive in models with the supplementary covariates and using unweighted

[20] As mentioned, the counterfactual for these estimates is no marijuana legalization of any kind. In Appendix Table A.3, 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).

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

data, implying 27% to 34% higher opioid death rates (see model 3 and 4). However, the weighted estimates are considerably smaller and no longer significant (model 5) and there is no predicted effect when state time trends are also included (model 6). By contrast, the estimated RMD effect remains positive and substantial for all models that include supplementary controls and adjustments for incomplete reporting of opioid deaths, but the estimates are frequently imprecise. Again, it is important to recognize that 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 (columns 5 and 6) suggest that recreational marijuana without retail sales reverses the deleterious effects of medical cannabis with dispensaries, but that the availability of retail recreational marijuana sales undoes these benefits.

3.3 Event Study Estimates

Figure 2 summarizes event study estimates for the four treatment variables using the most comprehensive specification – with adjusted mortality rates, supplementary covariates, weighted data, and state-specific time trends. Appendix Figures A.1 through A.4 provide corresponding results for each of the four marijuana policies and when using alternative model specifications.

Recalling that the event study estimates are incremental, indicating differences relative to a lower level of legalization rather than the counterfactual of no legal cannabis, the findings are generally consistent with the DiD estimates and provide useful additional details. Legalization of medical marijuana is associated with sharply higher opioid death rates in models that exclude state trends, particularly several years after implementation (Appendix Figure A.1). However, the data suggest a positive pre-trend in these specifications, particularly from $t-4$ through $t-1$, which probably explains a portion of this increase. Inclusion of controls for state trends eliminates the pre-trend and decreases the estimated treatment effects, but also reduces precision of the still fairly strong positive estimates at $t+5$ and beyond.

For retail sales of medical cannabis, the story is different. Here, none of the specifications indicate substantial pre-trends and the estimated MMD effects grow in the years after implementation. This increase over event time becomes considerably larger, albeit with some loss of precision, when including state trends in the model (Appendix Figure A.2).

The ES estimates for RML and RMD 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 effect for RMD. There are also erratic patterns of estimated effects in periods after policy implementation and, for recreational marijuana dispensaries, evidence of negative pre-trends, suggesting 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.

3.4 Robustness Checks

The first column of Appendix Table A.4 shows that estimates from the most fully specified models (equivalent to columns 5 and 6 in Table 3) are little changed when removing from the sample the three states that legalized medical marijuana prior to the 1999 start of the analysis (see column 1). Similar results are also obtained when limiting the analysis to the 20 largest states and using unweighted data (column 2), as expected since these contain most of the country's population. The findings are also substantially similar when the dependent variable is the narrower definition of opioid deaths (T-Codes: 40.1-40.4) used in some earlier research, albeit with a somewhat stronger (weaker) rise in deaths associated with marijuana legalization in models without (with) controls for state trends (columns 3 and 4).

The last two columns of the table show falsification tests, where the outcomes are mortality from heart disease and cancer, the two leading causes of death. There is no reason to expect cannabis legalization to affect either of these and the estimates confirm zero effects in the models that include state-specific time trends, although with modest reductions in cancer mortality when they are excluded.

We next estimated a series of "leave-one-out" (LOO) models to check whether the results are being driven by policy changes in any single state. We again focus on the specification with weighted data and state-specific linear time trends, which provide the most conservative treatment effects among the models that also adjust mortality rates for incomplete reporting and include the supplementary covariates. The procedure here involves estimating the model multiple times with one state instituting the specified policy change being successively deleted from the analysis sample.

Appendix Figures A.5 through A.7 summarize the results of this procedure and demonstrate that the qualitative findings of small and statistically insignificant MML and RML predicted effects combined with large and statistically significant (insignificant) MMD (RMD) estimates hold up throughout. Figure A.5 provides histograms of the estimated LOO coefficients for the four policies. Those for medical marijuana are small and positive (i.e. indicating higher deaths) in all cases, while the predicted treatment effects for medical marijuana dispensaries are always positive and usually considerably larger.[22] The LOO estimates vary considerably more for recreational marijuana, which is not surprising since there are fewer of these policy changes and they were more recently instituted. This fits with the much larger standard errors for recreational than medical marijuana in our full sample DiD estimates above. The predicted RMD effect is nevertheless always positive, although only of modest size when California is removed from the sample. Conversely, RML estimates are sensitive to the exclusion of single treatment states. Appendix Figures A.6 and A.7 show point estimates and corresponding 95% confidence intervals for the leave-one-out samples. Particularly relevant is that there is never a case where the LOO point estimate is outside the 95% confidence interval of either the full sample coefficient or of any of the other leave-one-out estimates.

3.5 Potential Problems with Differences-in-Differences Estimates

A recent literature identifies possible problems with estimates obtained using standard two-way fixed effects 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 problematic issues are: 1) the standard estimation procedure gives more weight to changes occurring in the middle of the analysis period than to those taking place near the beginning or end of it; and 2) the estimates may be biased if the treatment effects vary with time since policy implementation. 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

---

[22] The one exception is that when Florida is removed from the sample, the estimated MMD effect declines from 0.143 with the full sample to an imprecisely measured 0.093.

those treated later, but this is inappropriate if treatment effects of the prior adopters are growing or shrinking over time. Similar problems are created for locations enacting policies prior to the start of the analysis period, unless sufficient time has passed that these treatment effects have reached a steady-state.

Multiple methods have recently been proposed for dealing with these issues. Some are not ideally suited for this application because they: 1) have been designed and tested in a linear regression framework, rather than for Poisson models that are more appropriate in this context; 2) consider only a single treatment, whereas we evaluate four policies (MML, MMD, RML and RMD); 3) do not deal well with time-varying covariates, some of which may be critical to control for in our analysis.[23]

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 between 2004 and 2014, which we refer to hereafter 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.[24]

For these estimates, we first create a series of balanced panels, one for each cohort from 2004-2014, containing data for that period and the five years preceding and following it. Each cohort sample was then restricted to states legalizing marijuana in the cohort year,

---

[23] 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.

[24] The potential bias associated with multiple treatment variables is discussed in de Chaisemartin and D'Haultfoeuille (2022). They also present a method of correctly 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 this procedure.

or who had not legalized it 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 same models as above are estimated. However, since the stacked sample contains multiple observations from both the never treated controlled states and for given calendar years, we also estimate "saturated" models that include both cohort-by-state and cohort-by-year fixed effects.[25]

The stacked regression models provide no evidence of bias in our main DiD estimates. This is demonstrated in Table 4. The first row repeats the estimated MML coefficients from corresponding specifications in the first row of Table 3, covering the full 1999-2019 period and without stacking. The second panel restricts analysis to states legalizing medical cannabis from 2004-2014, 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 saturated models in the bottom panel suggest slightly larger increases in opioid mortality from legalizing medical marijuana than in the other specifications. Appendix Figure A.8 displays corresponding event study estimates from the stacked regression models. These again indicate substantial and statistically significantly higher

[25] The cohort-by-state fixed effects absorb all the variation in state-specific time trends, so the latter are excluded from these specifications.

death rates in the post-treatment periods, although sometimes with modest (and statistically insignificant) upwards pre-trends.

As a further test, we examine legalization effects using the procedures developed by Callaway and Sant'Anna (2021), which highlight potentially disparate treatment effects across policy cohorts and event time since policy implementation. For our purposes, these methods have potential complications, including the aforementioned issues of not dealing well with time-varying covariates or non-linear (e.g. Poisson) models. When using this approach, we therefore estimate log-linear, rather than Poisson, models, exclude the supplementary covariates and state time trends, and again limit analysis to MML. Given these limitations, we view these as additional checks for potential problems with our main DiD estimates. As above, we restrict the sample to states legalizing medical marijuana from 2004–2014 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 again, there is no indication of bias in our primary specifications. The 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 average treatment effect is 0.224, with a standard error of 0.084.[26] The event estimates, displayed in Appendix Table A.9, confirm the absence of pre-trends as well as the positive and generally statistically significant treatment effects following implementation of MML.

3.6 Population Groups and Types of Drug Mortality

Next, we estimated DiD models for population subgroups stratified by sex, race/ethnicity, and age. Table 5 summarizes the results for specifications using weighted

[26] However, consistent with earlier research, there is evidence of relatively more favorable legalization effects for policies enacted prior to 2008 than for those put in place later.

data, adjusted for incomplete reporting of drug involvement, and including supplementary covariates, with state trends also controlled for in the bottom panel. We frequently do not obtain strong evidence of differences in the impact of marijuana legalization across groups or types of death, and caution against overinterpreting those we do observe, given the large number of specifications estimated and treatment coefficients reported. That said, the evidence suggests that retail sales of recreational marijuana 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 declining death rates for seniors. Interestingly, medical marijuana dispensaries also raise opioid deaths in almost all cases, but these increases seem relatively uniform across the groups.

We also separately considered various types of opioid deaths including those from opioid analgesics (i.e., prescription opioids such as oxycodone or hydrocodone), heroin, and synthetic opioids (like fentanyl). And we extended this analysis to consider all drug fatalities and those involving cocaine, stimulants and sedatives. Marijuana legalization could directly affect these types of mortality through changes in their consumption or could have indirect effects when these drugs are co-prescribed with opioids (as is common for sedatives such as benzodiazepines) or if they have been adulterated with fentanyl or other opioids.

Table 6 displays the findings. The results for opioid analgesics are sensitive to the inclusion of state-specific trends. However, corresponding event studies reveal flat pre-trends in models without state trends but strong downward pre-trends for both MML and MMD when they are incorporated (Appendix Figure A.10), indicating that the findings in the top panel (absent trends) are almost certainly preferable. These reveal strong deleterious effects of retail marijuana sales. Conversely, the event studies for fatal heroin overdoses show that specifications with state trends are favored (Appendix Figure A.11); these indicate that marijuana legalization and sales also increase heroin mortality, although generally by less than for deaths involving opioid analgesics. Neither model completely eliminates pre-trends for synthetic opioid mortality (Appendix Figure A.12) but the extremely strong positive pre-trends for medical marijuana legalization and dispensaries in the absence of state time trends implies that the detrimental estimated effects in these models are almost certainly erroneous. Generally, the estimates for synthetic opioid deaths should be viewed with considerable caution.

The last four columns in the table show results for all drug deaths and important types of non-opioid drugs. The findings are mostly consistent with those obtained for all opioids, with retail medical marijuana sales generally predicting higher deaths rates, and with detrimental but less precisely estimated predicted effects also frequently obtained for recreational cannabis dispensaries.

3.7 Changes Over Time in Estimated Medical Marijuana Legalization Effects

Prior research documents the sensitivity of the estimated cannabis legalization effects to the period analyzed. Specifically, the consequences appear to have been more beneficial or less harmful when analyzing data from 1999-2010 than when extending the sample to include later years. The reasons for this are not clear. Some the prior researchers (Smart 2015; Pacula and Smart 2017; Powell, Pacula, and Jacobson 2018) suggest that the Ogden memo, which was released in 2009 and deprioritized the federal prosecution of medical marijuana users and suppliers in clear and unambiguous compliance with existing state laws, played an important role. Specifically, they argue that states legalizing medical marijuana after that date adopted stricter regulatory approaches than those that did so earlier, and that this explains the differences in effects.

The logic of why this greater stringency would result in less favorable or more harmful legalization effects is not apparent, particularly since the overall trends have been towards greater availability of legal marijuana through both the operation of dispensaries and permitting recreational cannabis. In any case, this or similar explanations hypothesize that the heterogeneous effects are caused by differences in the regulatory or legal structure of the policies. However, there are at least two alternative possibilities. First, the impacts of a given set of laws may have changed over time as predominant sources of opioid deaths have switched from opioid analgesics to heroin and synthetic opioids. Second, policy effects may be inaccurately or imprecisely measured over short periods of time and the results from 1999-2010 may coincidentally provide relatively favorable (but unstable) effects.

To examine these possibilities, we re-estimate our primary models (i.e. those in columns 5 and 6 of Table 3) but for varying analysis periods. Specifically, we first use 1999 as the starting year and vary the ending analysis year from 2004 through 2019. Second, we use 2019 as the final analysis year and vary the first year between 1999 and 2014.[27] Recreational marijuana policies are included in the group of controls, but we focus on medical marijuana because this is where the instability of estimates has been previously observed and since legal recreational cannabis has been relatively rare until recently. We emphasize the models without state trends, since their inclusion of trends is likely to problematic for the shorter analysis periods (as few as 6 years; however, we also briefly mention the results when trends are included.

Figure 3 summarizes the main findings, with additional details provided in Appendix Table A.5. The figure 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* estimated effects are obtained for analysis periods beginning in 1999 and ending in 2009 or 2010. Specifically, medical marijuana legalization is predicted to raise opioid deaths by a statistically insignificant 4 to 5 percent for these analysis periods and

[27] We choose these ranges of analysis periods to insure that we have a minimum of six years in the sample.

retail sales through dispensaries to increase them by an insignificant 12 to 14 percent (see the top two sub-figures). However, the estimated increases are considerably larger when using either shorter or longer analysis periods. For instance, using data from 1999-2006, legal medical marijuana and dispensaries raise predicted opioid fatalities by an imprecisely estimated 13 and 26 percent, respectively, with even larger and more precisely estimated opioid mortality growth for longer analyses periods (e.g. those starting in 1999 and ending in 2016 or later). There is less instability when ending the analysis in 2019, while varying the starting date, but shorter analysis windows again result in both less precise estimates and less harmful predicted treatment effects (see the lower two sub-figures). As mentioned, the inclusion of state-specific trends is likely to be problematic in models with short analysis windows; the main findings they are incorporated indicate less harmful effects in these cases than when the estimating over longer periods (see Table A.6 and Figure A.13).[28]

Our replications of prior research indicate that even when favorable predicted impacts of medical marijuana legalization were observed from 1999-2010 in these studies, the benefits were eliminated or sharply attenuated when using specifications likely to provide more accurate results. In our original analysis we demonstrate that even those attenuated estimates were probably too optimistic because the 1999-2010 period happens to be one yielding among the most beneficial predicted effects. An overall conclusion is that the differences in estimated legalization effects across varying time periods need not reflect heterogeneity in the cannabis policies adopted during them but instead are likely to reflect resulted in either differences in the impacts of given policies across time or the imprecision of estimated effects when using relatively short analysis periods.

[28] One other notable finding is that the detrimental estimated medical marijuana dispensary effects are much larger when ending the analysis between 2016 and 2018, rather than in 2019.

As a final test, we estimated a series of "Keep-One-In" (KOI) estimates. For these models each sample consists of one treatment state, legalizing medical marijuana or medical marijuana dispensaries, with the control group consisting of states that had not legalized any type of cannabis through the end of 2019. We limited the treatment states to those that first legalized medical cannabis from 2002-2016 and, for the MMD models, those that had also put operating dispensaries in place over the same period.[29] Our models again correspond to those in columns (5) and (6) of Table 3.

Appendix Figure A.14 summarizes the results, with states ordered on the X-axis from earliest to latest implementation dates of the policy. The estimates are often imprecise, as expected since only a single (often small) treatment state is used in each regression. However, the key point is that there is little evidence of consistent patterns of estimated effects as a function of the timing of the policies. In the specifications without trends, 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 the association effectively disappears when state trends are included in the model. There is never any evidence of a relationship between the timing of medical marijuana dispensaries and opioid mortality rates.

4. 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 operating dispensaries permit retail

[29] This restriction was used to insure 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 exclusion criteria of MML implementation no sooner than 2002 and RML enactment before 2017, and none that meet the corresponding criteria for RMD.

sales (Powell, Pacula, and Jacobson 2018), or that it is sales of recreational marijuana that have these benefits (Chan, Burkhardt, and Flyr 2020). However, none of these estimates are robust to changes in models or time periods. It has previously been demonstrated that extending the analysis to include more recent years reversed the mortality reductions observed by Bachhuber et al. (Shover et al. 2019), and that using a slightly longer timespan and adding controls for recreational cannabis legalization did so for medical marijuana dispensaries (Chan, Burkhardt, and Flyr 2020). Our replication analysis shows that, even when using the original study periods, adjusting for incomplete reporting of drug involvement on death certificates, incorporating more comprehensive controls for supplementary covariates, and more accurate timing of policy implementation attenuates or eliminates the estimated relationships, particularly when the data are weighted by state populations.

These conclusions are reinforced by our analysis of data covering 1999-2019, that includes the aforementioned modeling characteristics, and estimates Poisson rather than log-linear models. Our differences-in-differences estimates provide evidence that the availability of retail medical cannabis dispensaries raises opioid death rates by 15% to 32%, with similar but less precisely predicted increases for states with dispensaries selling recreational marijuana. Legalization of medical marijuana, without retail dispensaries, generally has more modest effects, while still usually being associated with some growth in opioid deaths. We also show that key results are sensitive to the choice of analysis periods. In particularly, the 1999-2010 timespan used in influential earlier research happens to provide more favorable results than when ending the analysis in either earlier or later years.

Our event study analyses provide further compelling evidence for the negative consequences of medical marijuana dispensaries, with flat pre-trends and increases in opioid mortality rates that rise steadily over time after policy implementation in models using weighted data. The results for recreational marijuana dispensaries are less consistent, reducing our confidence in these estimates. We suspect this occurs because recreational cannabis has only been legalized recently in most states that have done so, and with an even shorter period of operating recreational marijuana dispensaries.

Notwithstanding the caveat mentioned just above, the evidence raises the possibility that recreational dispensaries raise opioid death rates more for males, blacks and Hispanics, and 15-49 year-olds than for females, non-Hispanic whites, and older individuals. The age and gender patterns are consistent with males and Millennials (born between 1980 and 1994) and, to a lesser extent, Gen X (born between 1965 and 1979) being the primary purchasers of recreational marijuana through retail outlets (Flowhub 2020; Johnson 2021). The racial breakdown of recreational cannabis consumers has been less studied, but black, American Indian/Alaska Native, and multiple race adults do have higher rates of past month marijuana use than corresponding non-Hispanic whites (Center for Behavioral Health Statistics and Quality 2020). Asians and Hispanics exhibit relatively low rates of use during the past month, but recreational cannabis has been linked to larger increases in past-year or past-month consumption for Hispanics and (non-black) other races than for non-Hispanic whites (Martins et al. 2021).

It is important to recognize limitations of quasi-experimental analyses for answering the questions posed here, and probably also for understanding marijuana legalization effects on outcomes other than mortality rates. Policies legalizing marijuana are heterogenous, as reflected by our use of four distinct treatment variables, and with considerable cross-state variation within these general policy categories. Some state policies may have had more beneficial outcomes than the average treatment effects we estimate and the impacts of given policies may have changed as the opioid crisis has evolved. This complexity also presents challenges for using some recently developed methods to estimate differences-in-differences models when policy implementation is staggered. In addition, with few exceptions, the legalization of recreational marijuana is recent, raising considerable difficulty in reliably estimating its effects.

It is critical to understand the counterfactual to which the various types of marijuana legalization are being compared. For example, Chan, Burkhardt, and Flyr (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 wide variety of outcomes, compared to a control group that includes states with no legal marijuana as well as those permitting medical but not recreational cannabis. This counterfactual may be of limited interest for policy analysis. Instead, we suspect that, for many applications, the more relevant counterfactual is no legal marijuana of any kind, and this is the one we have primarily focused upon.

Finally, while legal retail marijuana sales have probably raised opioid mortality, they are not the primary driver of the observed increases. For example, our preferred estimates suggest that operating medical marijuana dispensaries increased opioid-involved fatality rates by 15% to 29% (versus no legal cannabis). Around 58% of the country's population lived in such states in 2019, implying that this treatment effect predicts a 9% to 17% rise in national opioid mortality rates from 1999 to 2019.[30] Since that actual growth was 342% (from 3.66 to 16.17 per 100,000) over this period, other factors are responsible for over 95% of the observed increase.

[30] No states allowed medical marijuana dispensaries in 1999, so these numbers are calculated by multiplying 15% and 29% by 0.58.

References

Ahmad, FB, Rossen, LM, and Sutton, P, "Provisional Drug Overdose Death Counts," Vital Statistics Rapid Release (National Center for Health Statistics, 2021).

Anderson, Mark D., and Rees, Daniel I., "The Public Health Effects of Legalizing Marijuana," *Journal of Economic Literature*, (Forthcoming).

Aran, Adi, and Cayam-Rand, Dalit, "Medical Cannabis in Children," *Rambam Maimonides Medical Journal*, 11 (2020), e0003.

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

Baker, Andrew, Larcker, David F., and Wang, Charles C. Y., "How Much Should We Trust Staggered Difference-In-Differences Estimates?," (European Corporate Governance Institute - Finance Working Paper No. 736/2021, 2021).

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

Bradford, Ashley C., Bradford, W. David, Abraham, Amanda, and Bagwell Adams, Grace, "Association Between US State Medical Cannabis Laws and Opioid Prescribing in the Medicare Part D Population," *JAMA internal medicine*, 178 (2018), 667–672.

Callaway, Brantly, and Sant'Anna, Pedro H.C., "Difference-in-Differences with multiple time periods," *Journal of Econometrics*, 225 (2021), 200–230.

Cengiz, Doruk, Dube, Arindrajit, Lindner, Attila, and Zipperer, Ben, "The Effect of Minimum Wages on Low-Wage Jobs," *The Quarterly Journal of Economics*, 134 (2019), 1405–1454.

Center for Behavioral Health Statistics and Quality, "Results from the 2019 National Survey on Drug Use and Health: Detailed tables," (Rockville, MD, Substance Abuse and Mental Health Services Administration, 2020).

Centers for Disease Control and Prevention, "National Center for Health Statistics Mortality Data on CDC WONDER," *CDC Wonder*, <https://wonder.cdc.gov/mcd.html> (2021) (Mar. 15, 2021).

de Chaisemartin, Clément, and D'Haultfoeuille, Xavier, "Two-Way Fixed Effects and Differences-In-Differences With Heterogeneous Treatment Effects: A Survey," NBER Working Paper No. 29691, Working Paper (National Bureau of Economic Research, 2022).

de Chaisemartin, Clément, and D'Haultfœuille, Xavier, "Two-way Fixed Effects Regressions with Several Treatments," NBER Working Paper No. 29734, Working Paper (2022).

Chan, Nathan W., Burkhardt, Jesse, and Flyr, Matthew, "The Effects of Recreational Marijuana Legalization and Dispensing on Opioid Mortality," *Economic Inquiry*, 58 (2020), 589–606.

Chu, Yu-Wei Luke, "Do Medical Marijuana Laws Increase Hard-Drug Use?," *The Journal of Law and Economics*, 58 (2015), 481–517.

Conyers, Gregory, and Ayres, Ian, "A lottery test of the effect of dispensaries on emergency room visits in Arizona," *Health Economics*, 29 (2020), 854–864.

Flowhub, "Cannabis Consumer Demographics Guide," (2020).

Goodman-Bacon, Andrew, "Difference-in-differences with variation in treatment timing," *Journal of Econometrics*, 225 (2021), 254–277.

Harris, Patrice A., and Mukkamala, Bobby, "Advocacy and Action to End the Opioid Epidemic by the AMA Opioid Task Force," *AMA Journal of Ethics*, 22 (2020), 718–722 (American Medical Association).

Hedegaard, Holly, Miniño, Arialdi, Spencer, Merianne Rose, and Warner, Margaret, "Drug Overdose Deaths in the United States, 1999–2020," (National Center for Health Statistics (U.S.), 2021).

Hsu, Greta, and Kovács, Balázs, "Association between county level cannabis dispensary counts and opioid related mortality rates in the United States: panel data study," *BMJ*, (2021), m4957.

Jayawardhana, Jayani, and Fernandez, Jose M., "The associations of medical marijuana policies with opioid-related health care utilization," *Health Services Research*, 56 (2021), 299–309.

Johnson, Lee, "Cannabis Consumer Demographics: Who is Most Likely to Use Cannabis?," *CBD Oracle*, <https://cbdoracle.com/news/business/cannabis-consumer-demographics/> (2021) (Oct. 18, 2021).

Katcher, Bradley A., and Ruhm, Christopher J., "Federal Opioid Grant Funding Favors Least Populous States, Not States With Greatest Need," *Health Affairs (Project Hope)*, 40 (2021), 829–836.

Kochanek, Kenneth D., Xu, Jiaquan, Murphy, Sherry L., Miniño, Arialdi M., and Kung, Hsiang-Ching, "Deaths: final data for 2009," *National Vital Statistics Reports: From the Centers for Disease Control and Prevention, National Center for Health Statistics, National Vital Statistics System*, 60 (2011), 1–116.

Martins, Silvia S., Segura, Luis E., Levy, Natalie S., Mauro, Pia M., Mauro, Christine M., Philbin, Morgan M., and Hasin, Deborah S., "Racial and Ethnic Differences in

Cannabis Use Following Legalization in US States With Medical Cannabis Laws," *JAMA network open*, 4 (2021), e2127002.

McMichael, Benjamin J., Van Horn, R. Lawrence, and Viscusi, W. Kip, "The impact of cannabis access laws on opioid prescribing," *Journal of Health Economics*, 69 (2020), 102273.

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, and Smart, Rosanna, "Medical Marijuana and Marijuana Legalization," *Annual Review of Clinical Psychology*, 13 (2017), 397–419.

Powell, David, Pacula, Rosalie Liccardo, and Jacobson, Mireille, "Do medical marijuana laws reduce addictions and deaths related to pain killers?," *Journal of Health Economics*, 58 (2018), 29–42.

Purington, Kitty, "Tackling the Opioid Crisis: What State Strategies Are Working? – The National Academy for State Health Policy," (National Academy for State Health Policy, 2019).

Ruhm, Christopher J., "Geographic Variation in Opioid and Heroin Involved Drug Poisoning Mortality Rates," *American Journal of Preventive Medicine*, 53 (2017), 745–753.

——, "Corrected US opioid-involved drug poisoning deaths and mortality rates, 1999-2015," *Addiction (Abingdon, England)*, 113 (2018), 1339–1344.

Sabia, Joseph J., Dave, Dhaval M., Alotaibi, Fawaz, and Rees, Daniel I., "Is Recreational Marijuana a Gateway to Harder Drug Use and Crime?," NBER Working Paper No. 29038 (National Bureau of Economic Research, 2021).

Shover, Chelsea L., Davis, Corey S., Gordon, Sanford C., and Humphreys, Keith, "Association between medical cannabis laws and opioid overdose mortality has reversed over time," *Proceedings of the National Academy of Sciences of the United States of America*, 116 (2019), 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, and Humphreys, Keith, "Association of State Policies Allowing Medical Cannabis for Opioid Use Disorder With Dispensary Marketing for This Indication," *JAMA Network Open*, 3 (2020), e2010001.

Smart, Rosanna, "The Kids Aren't Alright, But Older Adults Are: How Medical Marijuana Market Growth Impacts Adult and Adolescent Substance-Related Outcomes," *SSRN Electronic Journal*, (2015).

Smith, Rhet A., "The Effects of Medical Marijuana Dispensaries on Adverse Opioid Outcomes," *Economic Inquiry*, 58 (2020), 569–588.

Sun, Liyang, and Abraham, Sarah, “Estimating dynamic treatment effects in event studies with heterogeneous treatment effects,” *Journal of Econometrics*, 225 (2021), 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).

Wen, Hefei, and Hockenberry, Jason M., “Association of Medical and Adult-Use Marijuana Laws With Opioid Prescribing for Medicaid Enrollees,” *JAMA internal medicine*, 178 (2018), 673–679.

Wolfers, Justin, “Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results,” *American Economic Review*, 96 (2006), 1802–1820.

Wooldridge, Jeffrey M., “Econometric analysis of cross section and panel data,” (Cambridge, Mass, MIT Press, 2010).

Table 1: Key Features of Replicated Studies

Study; Period (Citations)	Opioid Variable	Marijuana Policies	Supplementary Controls	Source of Replication Data
Bachhuber, et al. (2014); 1999-2010 (764 citations)	T40.2 – T40.4, age-adjusted (public-use)	Legal Medical	PDMP, Pharmacy ID law, Pain Clinic Oversight, Unemployment Rate	Shover et al. (2019)
Powell, et al. (2018); 1999-2013 (350 citations)	T40.1 – T40.4 (restricted)	Medical: Legal & Dispensaries	Population shares (male, white, age), Unemployment Rate, Alcohol Tax, Must Access PDMP, Pill Mill Law	Deaths: CDC *MCOD* files Covariates: original study
Shover, et al. (2019); 1999-2017 (148 citations)	T40.2 – T40.4, age-adjusted (public-use)	Legal Medical	PDMP, Pharmacy ID law, Pain Clinic Oversight, Unemployment rates	Original study
Chan, et al. (2020); 1999-2017 (43 citations)	T40.1 – T40.4 (restricted)	Medical: Legal & Dispensaries; Recreational: Legal & Dispensaries	Population shares (male, white, age), Median Income, Unemployment Rate, Beer Tax, PDMP, Pill Mill Law	Marijuana Policies: original study Covariates: various sources

Notes: All studies estimated log-linear differences-in-differences models with controls for state and year fixed effects, as well as the supplementary controls described above. The studies often include other specifications. Citations refer to Google Scholar citations as of 2/17/2022. The opioid dependent variable refer to ICD-10 T-codes for heroin (T40.1), natural/semisynthetic opioids (T40.2), methadone (T40.3), and synthetic opioids (T40.4). PDMP's refer to state prescription drug monitoring programs.

Table 2: Replication and Extensions of Prior Studies Estimating Marijuana Legalization on Opioid Mortality

Study/Period	*Original Study*	*Replication Estimates*				
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Bachhuber et al. (2014): 1999-2010						
Medical	-0.285**	-0.237**	-0.190*	-0.183**	-0.137*	-0.045
	(0.094)	(0.113)	(0.095)	(0.082)	(0.081)	(0.122)
Observations	575	575	600	612	612	612
Shover et al. (2019): 1999-2017						
Medical	0.205**	0.205**	0.265**	0.240***	0.328***	0.273***
	(0.094)	(0.099)	(0.115)	(0.086)	(0.068)	(0.077)
Observations	*908*	908	933	969	969	969
Powell et al. (2018): 1999-2013						
Medical	-0.072	-0.066	-0.034	-0.065	-0.058	0.201
	(0.107)	(0.100)	(0.088)	(0.089)	(0.081)	(0.188)
Medical + Dispensary	-0.333	-0.339**	-0.250**	-0.225*	-0.154	0.384
		(0.164)	(0.105)	(0.120)	(0.149)	(0.239)
Observations	*765*	765	765	765	765	765
Chan et al. (2020): 1999-2017						
Medical	0.25***	0.186**	0.182*	0.142	0.261***	0.172*
	(0.09)	(0.076)	(0.091)	(0.089)	(0.070)	(0.092)
Medical + Dispensary	0.15	0.141	0.152	0.057	0.199	0.313*
		(0.122)	(0.163)	(0.147)	(0.119)	(0.161)
Recreational	0.60	0.523*	0.534	0.447	0.154	0.282
		(0.275)	(0.330)	(0.272)	(0.187)	(0.177)
Recreational + Dispensary	-0.13	-0.120	-0.178	-0.106	-0.072	-0.006
		(0.202)	(0.223)	(0.209)	(0.223)	(0.173)
Observations	969	969	969	969	969	969
Adjusted Mortality	N	N	Y	Y	Y	Y
Preferred Treatment Definitions	N	N	Y	Y	Y	Y
Preferred Controls	N	N	N	Y	Y	Y
Population weights	N	N	N	N	Y	Y
State time trends	N	N	N	N	N	Y

Notes: Standard errors in parentheses are robust and clustered at the state-level. * p<0.1, ** p<0.05 *** p<0.01. 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 (3)-(6), using the adjusted mortality measure 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.

Table 3: Estimates of Marijuana Legalization on Opioid Mortality, 1999-2019

Legalization Status	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Medical	0.302***	0.319***	0.218***	0.225***	0.189***	0.074
	(0.085)	(0.075)	(0.057)	(0.049)	(0.043)	(0.047)
Medical	0.348***	0.338***	0.216***	0.204***	0.154***	0.064
	(0.088)	(0.079)	(0.059)	(0.047)	(0.041)	(0.048)
Medical + Dispensary	0.187*	0.268***	0.224***	0.287***	0.249***	0.136**
	(0.096)	(0.090)	(0.071)	(0.066)	(0.051)	(0.063)
Medical	0.273***	0.278***	0.207***	0.196***	0.150***	0.055
	(0.083)	(0.073)	(0.060)	(0.050)	(0.045)	(0.050)
Medical + Dispensary	0.175*	0.260***	0.213***	0.277***	0.251***	0.143**
	(0.092)	(0.087)	(0.074)	(0.068)	(0.054)	(0.067)
Recreational	0.052	0.140	0.238*	0.292**	0.153	-0.026
	(0.164)	(0.169)	(0.122)	(0.130)	(0.109)	(0.098)
Recreational + Dispensary	-0.239	-0.088	0.138	0.205	0.266**	0.162
	(0.182)	(0.159)	(0.147)	(0.137)	(0.117)	(0.105)
Adjusted Mortality	N	Y	N	Y	Y	Y
Controls	N	N	Y	Y	Y	Y
Population Weights	N	N	N	N	Y	Y
State time trends	N	N	N	N	N	Y

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 1999-2019 (n=1,068). Estimates are from Poisson models. All models include state and year fixed effects, with additional details on the regressions 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

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

Model Description	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
(a): Original estimates	0.319***	0.225***	0.189***	0.074	
	(0.075)	(0.049)	(0.043)	(0.047)	
(b): Exclude states legalizing MML before 2004 or after 2014	0.258***	0.249***	0.238***	0.130**	
	(0.089)	(0.091)	(0.083)	(0.065)	
(c): (b) plus stacked regressions with balanced samples	0.311***	0.233***	0.214***	0.198***	0.198***
	(0.069)	(0.049)	(0.043)	(0.041)	(0.039)
(d): (c) plus limit to policy changes in cohort year	0.314***	0.240**	0.207***	0.198***	0.226***
	(0.092)	(0.094)	(0.077)	(0.071)	(0.070)
Controls	N	Y	Y	Y	Y
Population Weights	N	N	Y	Y	Y
State time trends	N	N	N	Y	N
Saturated Models	N	N	N	N	Y

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 1. All models include state and year fixed effects. Model (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). Model (c) creates "stacked regressions" from the sample in model (b) where each of the 11-years from 2004-2014 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 2006-2015). The 11 data sets were then "stacked" with analysis performed on the combined data set. Model (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: 1,068, 711, 6,696, and 2,560. State time trends in models (c) and (d) are cohort-specific (i.e., taking the value of 0 in the first year included for that cohort). "Saturated" models refer to those that include cohort-by-state and cohort-by-year fixed effects. Standard errors in parentheses are robust and clustered at the state-level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 5: 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
Excludes State Trends										
Medical	0.150***	0.144***	0.158***	0.165***	0.037	0.160***	0.130**	0.175***	0.149**	0.006
	(0.045)	(0.047)	(0.044)	(0.043)	(0.081)	(0.061)	(0.058)	(0.048)	(0.061)	(0.056)
Medical + Dispensary	0.251***	0.228***	0.279***	0.246***	0.232**	0.206***	0.190***	0.257***	0.292***	0.149*
	(0.054)	(0.054)	(0.057)	(0.049)	(0.091)	(0.067)	(0.065)	(0.059)	(0.079)	(0.080)
Recreational	0.153	0.166	0.108	0.155	0.073	0.263**	0.144	0.193	0.127	-0.017
	(0.109)	(0.111)	(0.097)	(0.118)	(0.146)	(0.125)	(0.098)	(0.128)	(0.135)	(0.107)
Recreational + Dispensary	0.266**	0.307***	0.160	0.178	0.406**	0.473***	0.303**	0.347**	0.175	-0.131
	(0.117)	(0.118)	(0.116)	(0.131)	(0.195)	(0.111)	(0.124)	(0.138)	(0.149)	(0.105)
Includes State Trends										
Medical	0.055	0.052	0.056	0.066	0.086	0.090	0.067	0.078	0.021	-0.077
	(0.050)	(0.052)	(0.045)	(0.043)	(0.000)	(0.067)	(0.051)	(0.055)	(0.053)	(0.055)
Medical + Dispensary	0.143**	0.131*	0.156***	0.148**	0.289	0.160*	0.112	0.161**	0.142**	0.019
	(0.067)	(0.070)	(0.060)	(0.064)	(0.000)	(0.091)	(0.072)	(0.070)	(0.067)	(0.078)
Recreational	-0.026	-0.018	-0.052	0.011	-0.082	0.157	-0.011	0.022	-0.087	-0.192*
	(0.098)	(0.098)	(0.093)	(0.103)	(0.000)	(0.137)	(0.089)	(0.106)	(0.102)	(0.101)
Recreational + Dispensary	0.162	0.193*	0.081	0.150	0.212	0.370***	0.226**	0.244**	0.063	-0.256**
	(0.105)	(0.107)	(0.098)	(0.097)	(0.000)	(0.102)	(0.102)	(0.109)	(0.124)	(0.118)

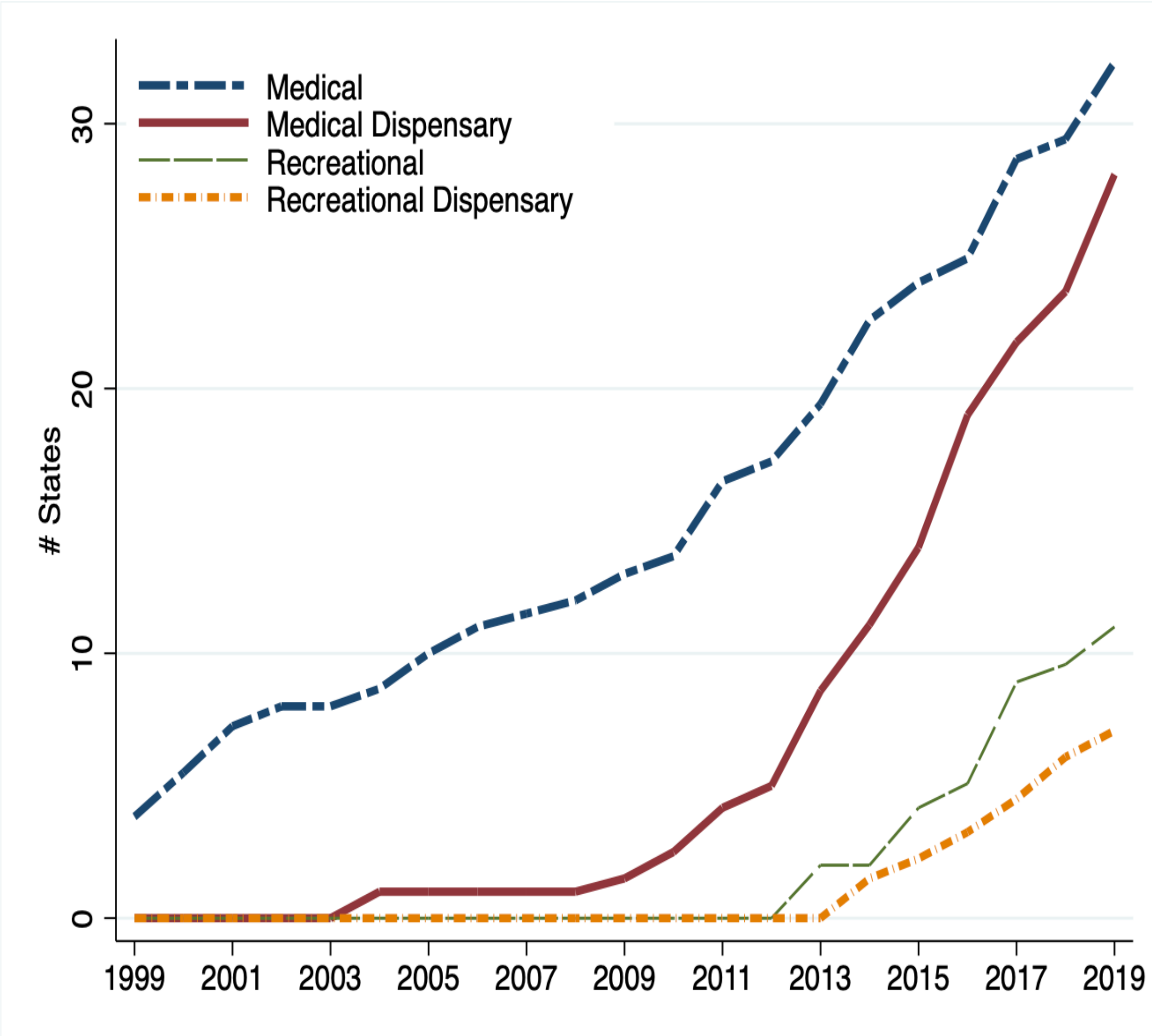
Notes: Table shows estimated effects of various types of marijuana legalization on opioid deaths (T-Codes: 40.0-40.4, 40.6) for the specified type of group using data from 1999-2019 (n=1,068). Estimates are from Poisson models that include state and year fixed effects, controls, and in the lower panel, state-specific time trends. 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. Whites refer to non-Hispanic whites and other to nonwhites and Hispanics. Observations are weighted by group population. Standard errors in parentheses are robust and clustered at the state-level. * p<0.10, ** p<0.05, *** p<0.01

Table 6: Estimates of Marijuana Legalization on Opioid Mortality by Type of Drug

	Opioids				All	Cocaine	Stim-	Seda-
	All	Analgesic	Heroin	Synthetic	Drugs		ulants	tives
Excludes State Trends								
Medical	0.150***	0.225***	0.036	0.188*	0.102***	0.077	0.223**	0.109
	(0.045)	(0.069)	(0.129)	(0.100)	(0.035)	(0.060)	(0.098)	(0.071)
Medical +	0.251***	0.206**	0.104	0.317**	0.183***	0.075	0.232**	0.240**
Dispensary	(0.054)	(0.080)	(0.166)	(0.155)	(0.037)	(0.072)	(0.106)	(0.116)
Recreational	0.153	0.126	-0.081	0.079	0.095	-0.009	0.088	0.199
	(0.109)	(0.114)	(0.253)	(0.211)	(0.072)	(0.139)	(0.150)	(0.193)
Recreational +	0.266**	0.224	0.093	0.327	0.204**	0.192	0.106	0.189
Dispensary	(0.117)	(0.149)	(0.293)	(0.237)	(0.084)	(0.151)	(0.167)	(0.187)
Includes State Trends								
Medical	0.055	-0.191*	0.093*	-0.037	0.052	0.056	0.066	0.077
	(0.050)	(0.098)	(0.049)	(0.075)	(0.052)	(0.045)	(0.043)	(0.082)
Medical +	0.143**	-0.222*	0.066	0.005	0.131*	0.156***	0.148**	0.179*
Dispensary	(0.067)	(0.115)	(0.080)	(0.122)	(0.070)	(0.060)	(0.064)	(0.104)
Recreational	-0.026	-0.300*	0.057	-0.270*	-0.018	-0.052	0.011	0.034
	(0.098)	(0.157)	(0.123)	(0.159)	(0.098)	(0.093)	(0.103)	(0.153)
Recreational +	0.162	-0.092	0.201*	0.062	0.193*	0.081	0.150	0.266*
Dispensary	(0.105)	(0.221)	(0.116)	(0.251)	(0.107)	(0.098)	(0.097)	(0.151)

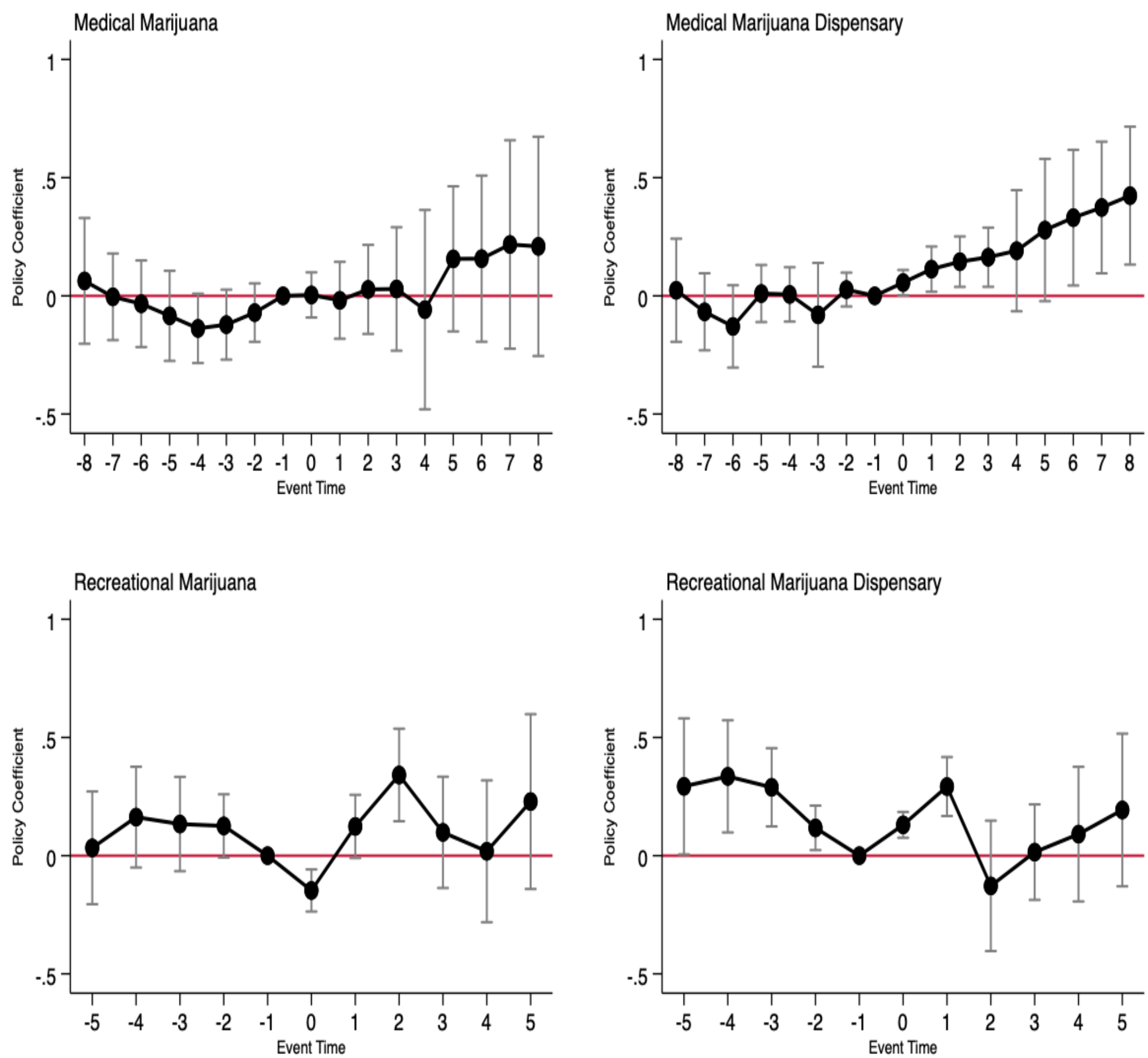
Notes: Table shows estimated effects of various types of marijuana legalization on opioid deaths for the specified type of drug using data from 1999-2019 (n=1,068). Estimates are from Poisson models that include state and year fixed effects, controls, and in the lower panel, state-specific time trends. 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. Heroin, Analgesics and Synthetics refer to T-Codes 40.1, 40.2 and 40.4, drug deaths to underlying cause of death codes: X40-44, X60-64, X85 and Y10-14, and cocaine, stimulants, and sedatives to T-Codes 40.5, 43.6 and 42.0-42.8. Observations are weighted by group population. Standard errors in parentheses are robust and clustered at the state-level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Figure 1: Number of States with Specified Marijuana Policy, by Year



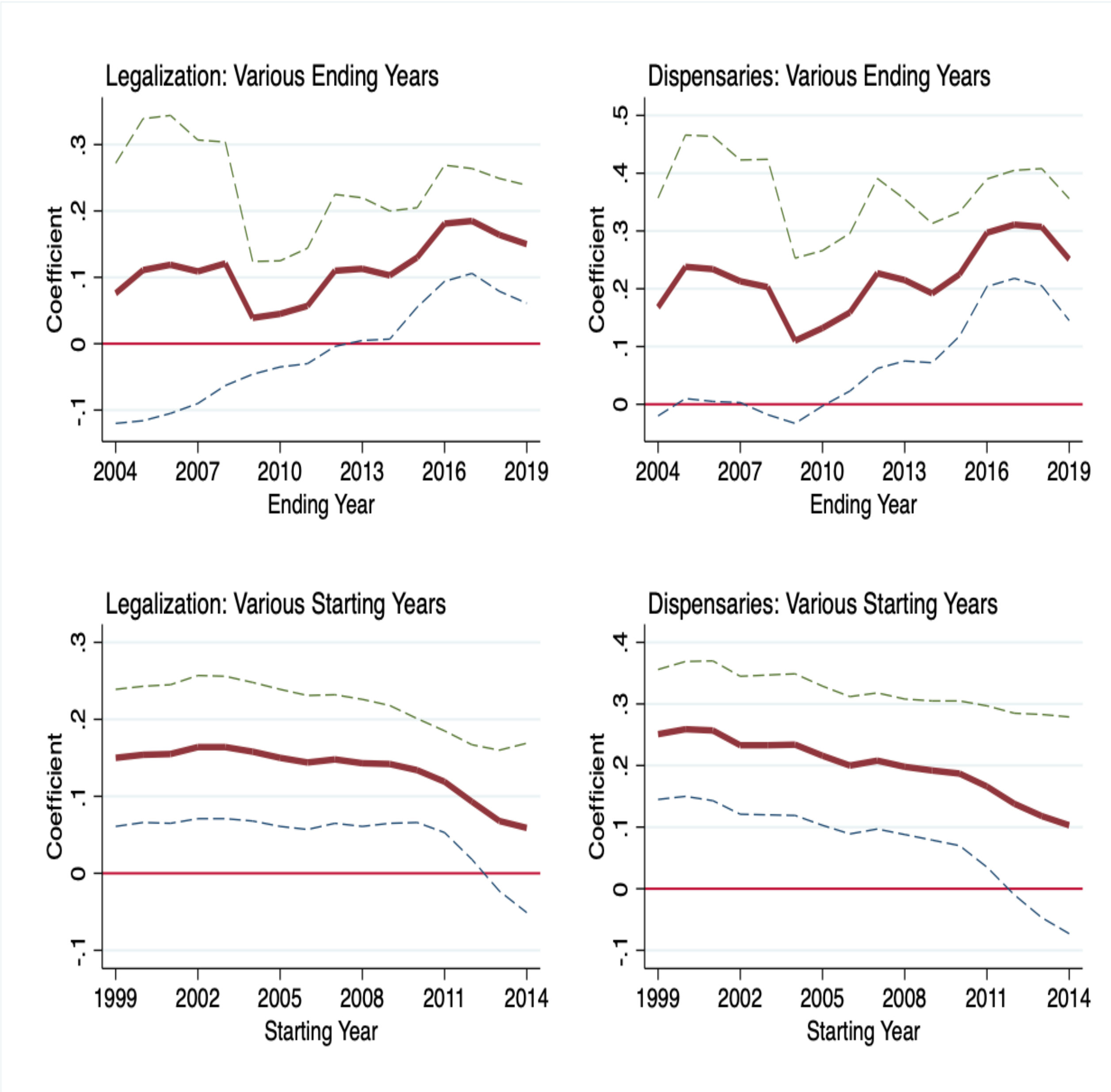
Note: Figure shows the number of states with the specified policy in each year, with policies implemented during a given year weighted fractionally, to the nearest month.

Figure 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), corrected for under-reporting, with the preferred set of controls included as well as vectors of state and year dummy variables and state time trends. 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 percent confidence intervals, based upon robust standard errors clustered at the state-level.

Figure 3: Estimated Effects of Medical Marijuana Legalization and Dispensaries for Different Analysis Periods



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. In the top row, the starting year is always 1999 and the ending year various between 2004 and 2019. In the bottom row, the ending year is always 2019 and the starting year varies between 1999 and 2014. Models correct opioid deaths for under-reporting, use the preferred set of controls and include vectors of state and year dummy variables. Recreational marijuana legalization and dispensaries are also controlled for. Dotted lines show 95 percent confidence intervals, based upon robust standard errors clustered at the state-level.

Appendix

Table A.1: Effective Dates of Marijuana Policies

	Marijuana Policy			
State	Medical	Medical Dispensary	Recreational	Recreational Dispensary
Alabama	.	.	.	.
Alaska	2-Mar-99	1-Oct-16	24-Feb-15	1-Oct-16
Arizona	14-Dec-10	6-Dec-12	.	.
Arkansas	9-Nov-16	.	.	.
California	6-Nov-96	1-Jan-04	9-Nov-16	1-Jan-18
Colorado	28-Dec-00	7-Jun-10	10-Dec-12	1-Jan-14
Connecticut	1-Oct-12	20-Aug-14	.	.
Delaware	1-Jul-11	26-Jun-15	.	.
DC	27-Jul-10	29-Jul-13	26-Feb-15	.
Florida	3-Jan-17	19-Dec-18	.	.
Georgia	.	.	.	.
Hawaii	14-Jun-00	8-Aug-17	.	.
Idaho	.	.	.	.
Illinois	1-Jan-14	9-Nov-15	.	.
Indiana	.	.	.	.
Iowa	.	.	.	.
Kansas	.	.	.	.
Kentucky	.	.	.	.
Louisiana	6-Aug-19	6-Aug-19	.	.
Maine	23-Dec-99	31-Mar-11	30-Jan-17	.
Maryland	1-Jun-14	6-Jul-17	.	.
Massachusetts	1-Jan-13	24-Jun-15	15-Dec-16	20-Nov-18
Michigan	4-Dec-08	15-Jun-18	6-Nov-18	1-Dec-19
Minnesota	30-May-14	1-Jul-15	.	.
Mississippi	.	.	.	.
Missouri	6-Dec-18	.	.	.
Montana	2-Nov-04	1-Dec-16	.	.
Nebraska	.	.	.	.
Nevada	1-Oct-01	31-Jul-15	1-Jan-17	1-Jul-17
New Hampshire	23-Jul-13	30-Apr-16	.	.
New Jersey	1-Oct-10	6-Dec-12	.	.
New Mexico	1-Jul-07	1-Jul-09	.	.
New York	5-Jul-14	7-Jan-16	.	.
North Carolina	.	.	.	.
North Dakota	18-Apr-17	1-Mar-19	.	.
Ohio	8-Sep-16	16-Jan-19	.	.
Oklahoma	25-Aug-18	26-Oct-18	.	.
Oregon	3-Dec-98	24-Mar-14	30-Jun-15	1-Oct-15
Pennsylvania	17-May-16	.	.	.

Rhode Island	3-Jan-06	19-Apr-13	.	.
South Carolina	.	.	.	.
South Dakota	.	.	.	.
Tennessee	.	.	.	.
Texas	.	.	.	.
Utah	3-Dec-18	.	.	.
Vermont	1-Jul-04	21-Jun-13	1-Jul-18	.
Virginia	.	.	.	.
Washington	3-Dec-98	22-Jul-11	6-Dec-12	1-Jul-14
West Virginia	.	.	.	.
Wisconsin	.	.	.	.
Wyoming	.	.	.	.

Note: Effective dates for medical and recreational marijuana dispensaries refer to the first date at which a dispensary was open and legally protected in the state.

Table A.2: Variable Means for Explanatory Variables

Variable	Source	Unweighted	Weighted
Marijuana Policies	RAND-OPTIC		
Medical Marijuana		0.306	0.319
Medical Marijuana Dispensary		0.135	0.188
Recreational Marijuana		0.040	0.044
Recreational Marijuana Dispensary		0.023	0.029
State Population Shares:	SEER		
White		0.723	0.653
Male		0.493	0.492
Age			
<18		0.239	0.240
18 -24		0.099	0.098
25-44		0.271	0.275
45-64		0.254	0.251
≥65		0.138	0.136
Unemployment Rate (%)	LAUS	5.44	5.83
Median Household Income ($1000's)	SAIPE	50.57	51.75
State Beer Tax ($ per gallon)	Tax Policy Center	0.287	0.272
Must Access PDMP	RAND-OPTIC*	0.101	0.112
Good Samaritan Law	RAND-OPTIC*	0.238	0.266
Naloxone Law	RAND-OPTIC	0.287	0.352
Pill Mill/Pain Clinic Law	PDAPS	0.090	0.143

Note: Data refer to 1999-2019 (n=1,068). Observations in last column are weighted by state populations. Must Access PDMP refers to states where prescribers are required to consult the state prescription drug monitoring program before prescribing. PDMP, Good Samaritan, Naloxone and Pill Mil Laws include partial year coverage, rounded to the nearest month, where laws are effective for only a portion of the year. Further information on the variable sources includes: RAND Opioid Tools Information Center (RAND-OPTIC), https://www.rand.org/health-care/centers/optic/resources/datasets.html; Surveillance, Epidemiology, and End Results Population Data (SEER), https://seer.cancer.gov/popdata/; Local Area Unemployment Statistics (LAUS), https://www.bls.gov/lau/; Small Area Income and Poverty Estimates (SAIPE), https://www.census.gov/programs-surveys/saipe/data.html; Tax Policy Center, https://www.taxpolicycenter.org/statistics/state-alcohol-excise-taxes; Prescription Drug Abuse Policy System (PDAPS), https://pdaps.org/datasets/pain-management-clinic-laws. When there is an asterisk, we updated the RAND-OPTIC data through 2019, as described in Appendix XX.

Table A.3: Preferred Estimates of Marijuana legalization on Opioid Mortality, Incremental Estimates

Legalization Status	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Medical	0.302***	0.319***	0.218***	0.225***	0.189***	0.074
	(0.085)	(0.075)	(0.057)	(0.049)	(0.043)	(0.047)
Medical	0.348***	0.338***	0.216***	0.204***	0.154***	0.064
	(0.088)	(0.079)	(0.059)	(0.047)	(0.041)	(0.048)
Medical + Dispensary	-0.161*	-0.071	0.009	0.083*	0.094**	0.072**
	(0.092)	(0.080)	(0.055)	(0.044)	(0.041)	(0.035)
Medical	0.273***	0.278***	0.207***	0.196***	0.150***	0.055
	(0.083)	(0.073)	(0.060)	(0.050)	(0.045)	(0.050)
Medical + Dispensary	-0.098	-0.018	0.006	0.081*	0.100**	0.088***
	(0.084)	(0.073)	(0.054)	(0.043)	(0.039)	(0.034)
Recreational	-0.124	-0.121	0.025	0.016	-0.097	-0.169***
	(0.134)	(0.143)	(0.085)	(0.098)	(0.073)	(0.053)
Recreational + Dispensary	-0.291	-0.228	-0.100	-0.087	0.113	0.187***
	(0.187)	(0.170)	(0.105)	(0.098)	(0.075)	(0.050)
Adjusted Mortality	N	Y	N	Y	Y	Y
Controls	N	N	Y	Y	Y	Y
Population Weights	N	N	N	N	Y	Y
State time trends	N	N	N	N	N	Y

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 1999-2019 (n=1,068). Estimates are from Poisson models. All models include state and year fixed effects, with additional details on the regressions in the bottom panel. Each panel shows the results of a different set of regressions. The coefficients show predicted effects relative to the next lower level of marijuana legalization (e.g. those on medical dispensaries show additional effect of the dispensary over medical marijuana legalization.) Standard errors in parentheses are robust and clustered at the state-level. * p<0.10, ** p<0.05, *** p<0.01

Table A.4: Supplementary Estimates and Falsification Tests

Legalization Status	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
<u>Excludes State Trends</u>						
Medical	0.150***	0.139***	0.134***	0.254***	-0.012	-0.015***
	(0.045)	(0.045)	(0.049)	(0.071)	(0.009)	(0.005)
Medical + Dispensary	0.251***	0.234***	0.269***	0.345***	-0.007	-0.012***
	(0.054)	(0.059)	(0.073)	(0.095)	(0.010)	(0.004)
Recreational	0.153	0.301**	0.254*	0.226	-0.005	-0.009
	(0.109)	(0.125)	(0.154)	(0.161)	(0.014)	(0.008)
Recreational +	0.266**	0.245	0.300	0.314*	-0.009	0.004
Dispensary	(0.117)	(0.219)	(0.189)	(0.171)	(0.019)	(0.010)
<u>Includes State Trends</u>						
Medical	0.055	0.069	0.037	0.047	0.001	0.001
	(0.050)	(0.048)	(0.055)	(0.053)	(0.008)	(0.003)
Medical + Dispensary	0.143**	0.186**	0.148*	0.081	0.009	0.003
	(0.067)	(0.081)	(0.082)	(0.068)	(0.010)	(0.003)
Recreational	-0.026	0.082	0.011	-0.061	0.003	0.000
	(0.098)	(0.117)	(0.124)	(0.092)	(0.013)	(0.006)
Recreational +	0.161	0.114	0.137	0.124	-0.009	0.004
Dispensary	(0.105)	(0.127)	(0.140)	(0.106)	(0.012)	(0.006)
Description	Main Model	Deletes "Always Legal"	20 Largest States (Unweighted)	Narrower Opioid Definition	Heart Disease	Cancer

Notes: Models (1) through (3) show estimated effects of various types of marijuana legalization on opioid deaths (T-Codes: 40.0-40.4, 40.6), using data from 1999-2019. Estimates are from Poisson models that include state and year fixed effects, controls, and in the lower panel, state-specific time trends. 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. Data are also weighted by state population, except in model (3). Model (2) excludes states that had legalized medical marijuana prior to 1999. Model (3) limits analysis 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 (4) shows estimates using the same specification as model (1), except using a narrower definition of opioid deaths (T-Codes: 40.1-40.4). In models (5) and (6) the dependent variables are deaths due to heart disease (ICD-10 Codes: I00-I09, I11, I13, I20-I51) and malignant neoplasms (ICD-10 Codes: C00-C97). Standard errors in parentheses are robust and clustered at the state-level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A.5: Estimated Effects of Medical Marijuana Legalization and Dispensaries For Different Analysis Periods

	Ending Year of Analysis															
	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019
Medical Marijuana Legalization																
Coefficient	0.076	0.111	0.119	0.109	0.121	0.039	0.045	0.057	0.110	0.113	0.103	0.130	0.181	0.185	0.164	0.150
Lower CI	-0.120	-0.116	-0.105	-0.090	-0.063	-0.046	-0.035	-0.030	-0.004	0.005	0.007	0.055	0.094	0.106	0.079	0.061
Upper CI	0.272	0.339	0.344	0.307	0.304	0.124	0.125	0.144	0.225	0.220	0.200	0.205	0.269	0.264	0.249	0.239
Medical Marijuana Dispensaries																
Coefficient	0.168	0.238	0.234	0.213	0.203	0.110	0.132	0.159	0.227	0.215	0.192	0.225	0.297	0.311	0.307	0.251
Lower CI	-0.020	0.010	0.005	0.003	-0.018	-0.033	-0.003	0.023	0.062	0.075	0.072	0.118	0.204	0.218	0.205	0.145
Upper CI	0.357	0.466	0.464	0.423	0.424	0.253	0.266	0.296	0.391	0.355	0.313	0.333	0.390	0.405	0.408	0.356

	Starting Year of Analysis															
	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
Medical Marijuana Legalization																
Coefficient	0.150	0.154	0.155	0.164	0.164	0.158	0.150	0.144	0.148	0.143	0.142	0.134	0.119	0.093	0.068	0.059
Lower CI	0.061	0.066	0.065	0.071	0.071	0.068	0.061	0.057	0.065	0.061	0.065	0.066	0.053	0.018	-0.023	-0.051
Upper CI	0.239	0.243	0.245	0.257	0.256	0.248	0.239	0.231	0.232	0.226	0.218	0.201	0.185	0.167	0.160	0.169
Medical Marijuana Dispensaries																
Coefficient	0.251	0.259	0.257	0.233	0.233	0.234	0.216	0.200	0.208	0.198	0.192	0.187	0.166	0.138	0.118	0.103
Lower CI	0.145	0.150	0.143	0.121	0.120	0.119	0.103	0.089	0.097	0.088	0.079	0.070	0.035	-0.010	-0.047	-0.073
Upper CI	0.356	0.369	0.370	0.345	0.347	0.349	0.329	0.312	0.318	0.308	0.305	0.305	0.297	0.285	0.283	0.279

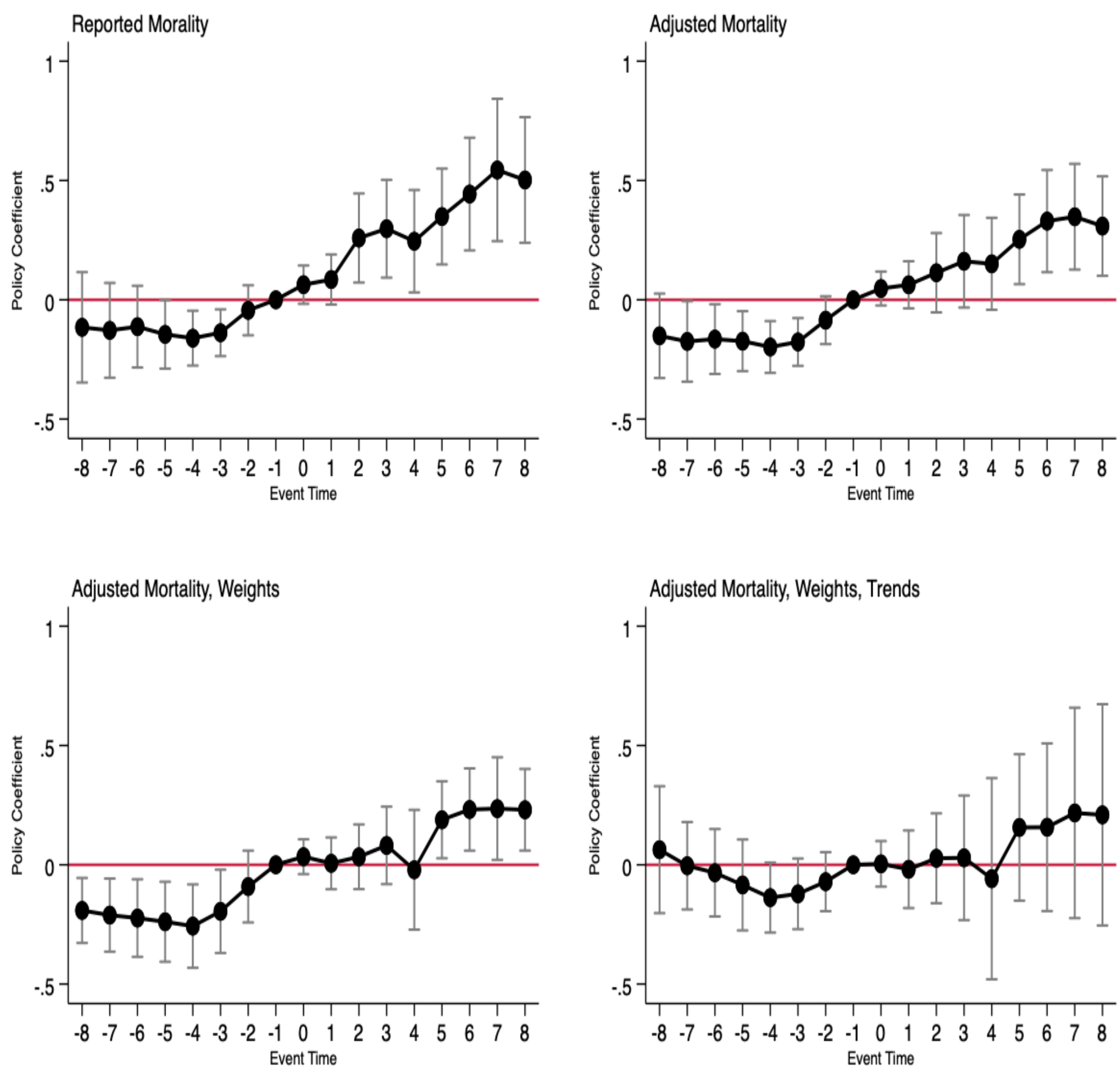
Note: Table displays predicted effect of medical marijuana legalization or operating dispensaries on all opioid deaths (T-Codes: 40.0-40.4, 40.6) compared to no legalization for differing analysis periods. In the top panel, the starting year is always 1999 and the ending year various between 2004 and 2019. In the bottom panel, the ending year is always 2019 and the starting year varies between 1999 and 2014. Models correct opioid deaths for under-reporting, use the preferred set of controls and include vectors of state and year dummy variables. Recreational marijuana legalization and dispensaries are also controlled for. Lower and Upper thresholds of 95% confidence intervals are based upon robust standard errors clustered at the state-level.

Table A.6: Estimated Effects of Medical Marijuana Legalization and Dispensaries For Different Analysis Periods With Controls for State Trends

	Ending Year of Analysis															
	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019
Medical Marijuana Legalization																
Coefficient	0.049	-0.172	-0.133	-0.198	-0.174	-0.081	-0.057	0.021	0.077	0.078	0.093	0.137	0.198	0.152	0.101	0.055
Lower CI	-0.466	-0.476	-0.438	-0.417	-0.350	-0.166	-0.128	-0.122	-0.108	-0.085	-0.025	0.040	0.083	0.061	-0.006	-0.043
Upper CI	0.564	0.132	0.172	0.021	0.003	0.004	0.013	0.163	0.262	0.242	0.210	0.234	0.314	0.243	0.207	0.153
Medical Marijuana Dispensaries																
Coefficient	0.087	-0.084	0.020	-0.063	0.033	0.122	0.117	0.184	0.240	0.206	0.187	0.244	0.345	0.306	0.258	0.143
Lower CI	-0.400	-0.383	-0.293	-0.275	-0.117	-0.055	-0.019	0.005	0.013	0.007	0.028	0.107	0.229	0.201	0.126	0.012
Upper CI	0.575	0.215	0.332	0.149	0.182	0.299	0.252	0.363	0.468	0.404	0.346	0.381	0.461	0.412	0.391	0.275
	Starting Year of Analysis															
	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
Medical Marijuana Legalization																
Coefficient	0.055	0.047	0.039	0.034	0.028	0.029	0.025	0.018	0.019	0.020	0.012	0.003	-0.019	-0.046	-0.037	-0.035
Lower CI	-0.043	-0.045	-0.048	-0.044	-0.046	-0.041	-0.044	-0.047	-0.044	-0.046	-0.067	-0.078	-0.124	-0.160	-0.170	-0.211
Upper CI	0.153	0.140	0.126	0.112	0.102	0.099	0.094	0.083	0.081	0.086	0.091	0.085	0.085	0.068	0.095	0.141
Medical Marijuana Dispensaries																
Coefficient	0.143	0.134	0.125	0.076	0.070	0.087	0.080	0.067	0.053	0.049	0.019	0.009	-0.026	-0.047	-0.022	-0.011
Lower CI	0.012	0.009	0.005	-0.044	-0.054	-0.045	-0.052	-0.059	-0.065	-0.069	-0.125	-0.140	-0.195	-0.204	-0.185	-0.213
Upper CI	0.275	0.260	0.244	0.195	0.194	0.218	0.213	0.192	0.172	0.167	0.162	0.158	0.143	0.109	0.141	0.190

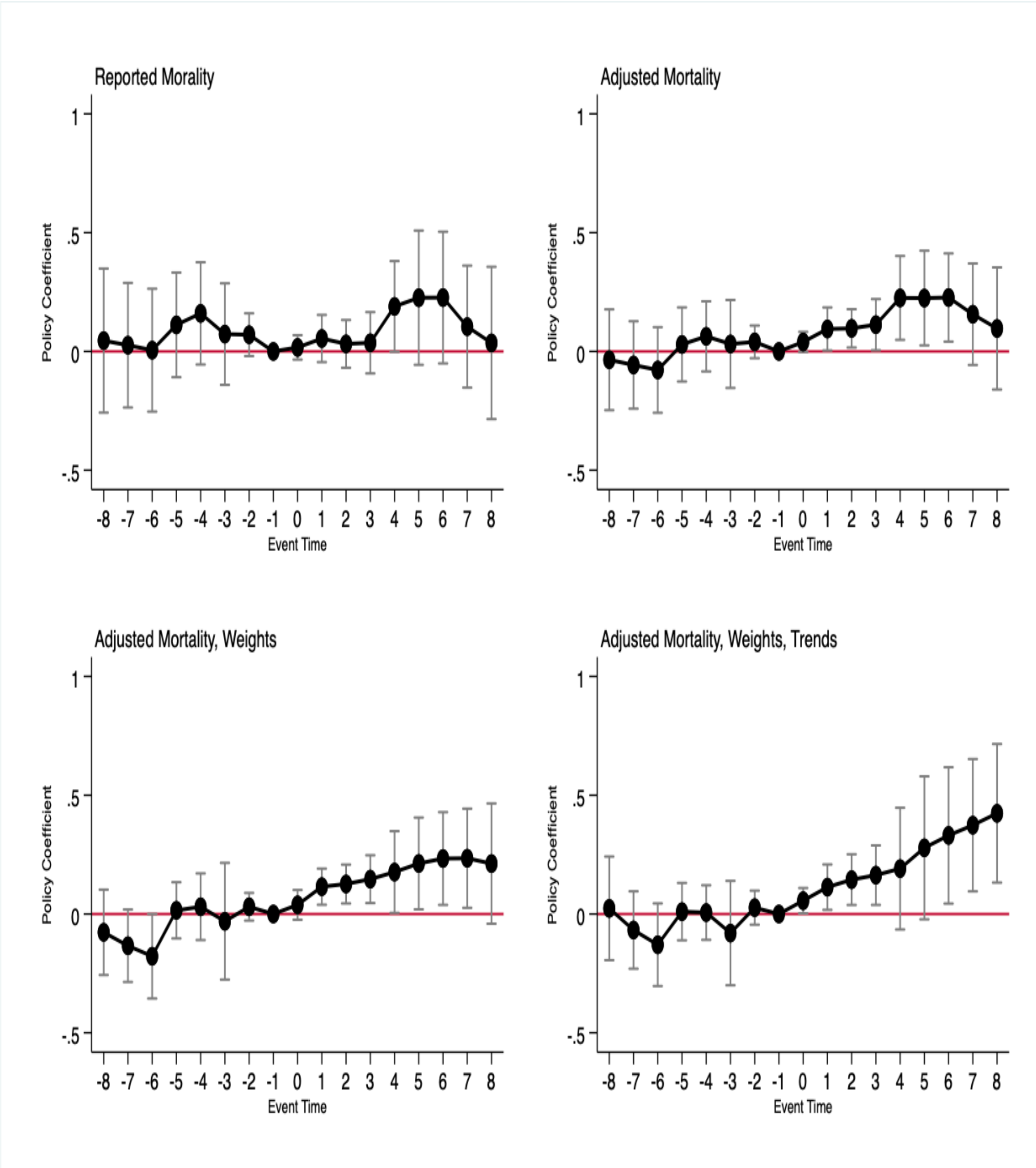
Note: Table displays predicted effect of medical marijuana legalization or operating dispensaries on all opioid deaths (T-Codes: 40.0-40.4, 40.6) compared to no legalization for differing analysis periods. In the top panel, the starting year is always 1999 and the ending year various between 2004 and 2019. In the bottom panel, the ending year is always 2019 and the starting year varies between 1999 and 2014. Models correct opioid deaths for under-reporting, use the preferred set of controls and include vectors of state and year dummy variables, and state-specific linear trends. Recreational marijuana legalization and dispensaries are also controlled for. Lower and Upper thresholds of 95% confidence intervals are based upon robust standard errors clustered at the state-level.

Figure A.1: Medical Marijuana Legalization Event Studies



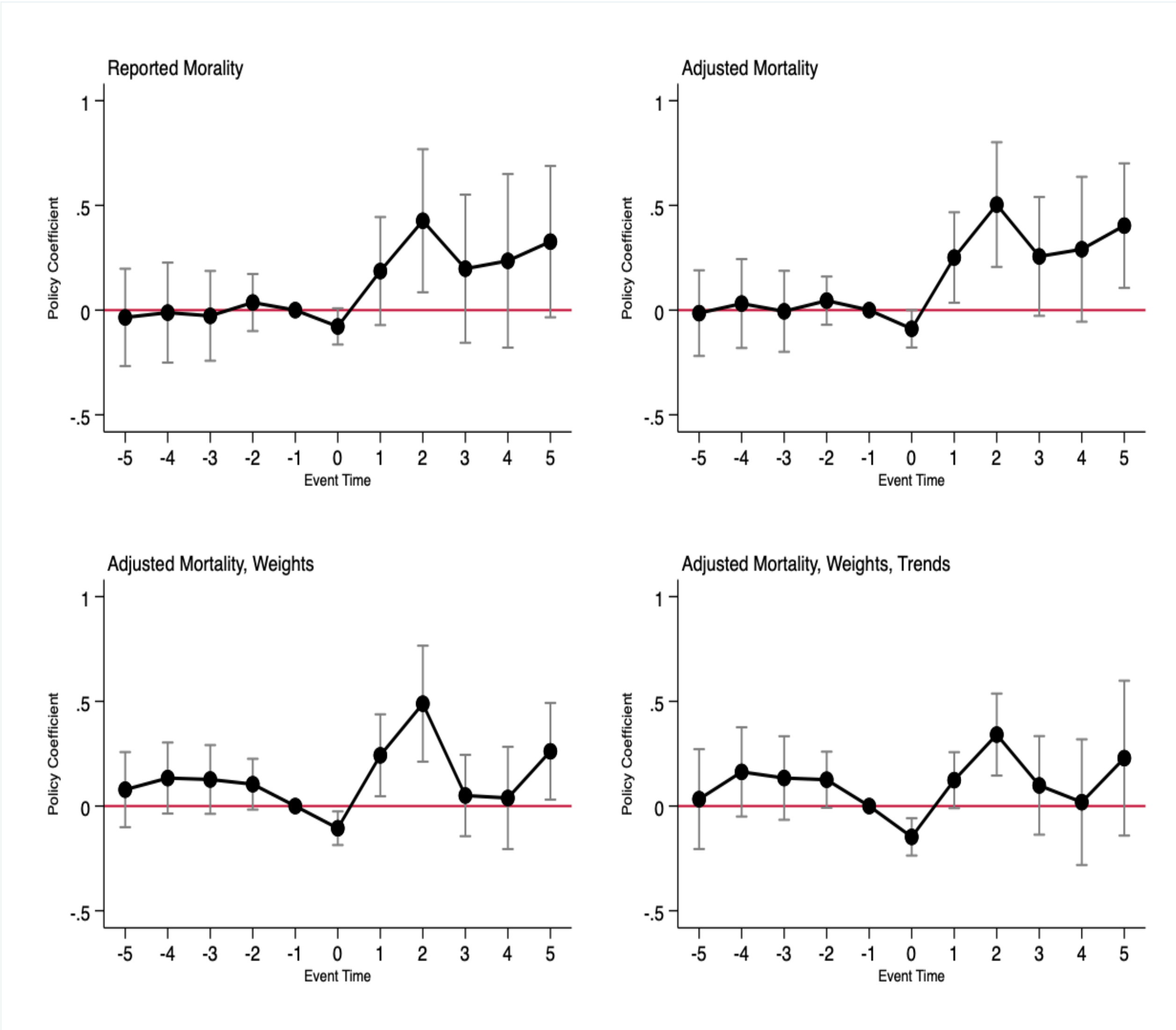
Note: See note on Figure 2. All estimates include the preferred set of controls and state and year fixed effects. Opioid mortality (T-Codes: 40.0-40.4, 40.6) is corrected for under-reporting, except in the first panel. State specific-time trends are included, and observations are weighted for state population, as described at the top of each sub-figure.

Figure A.2: Medical Marijuana Dispensary Event Studies



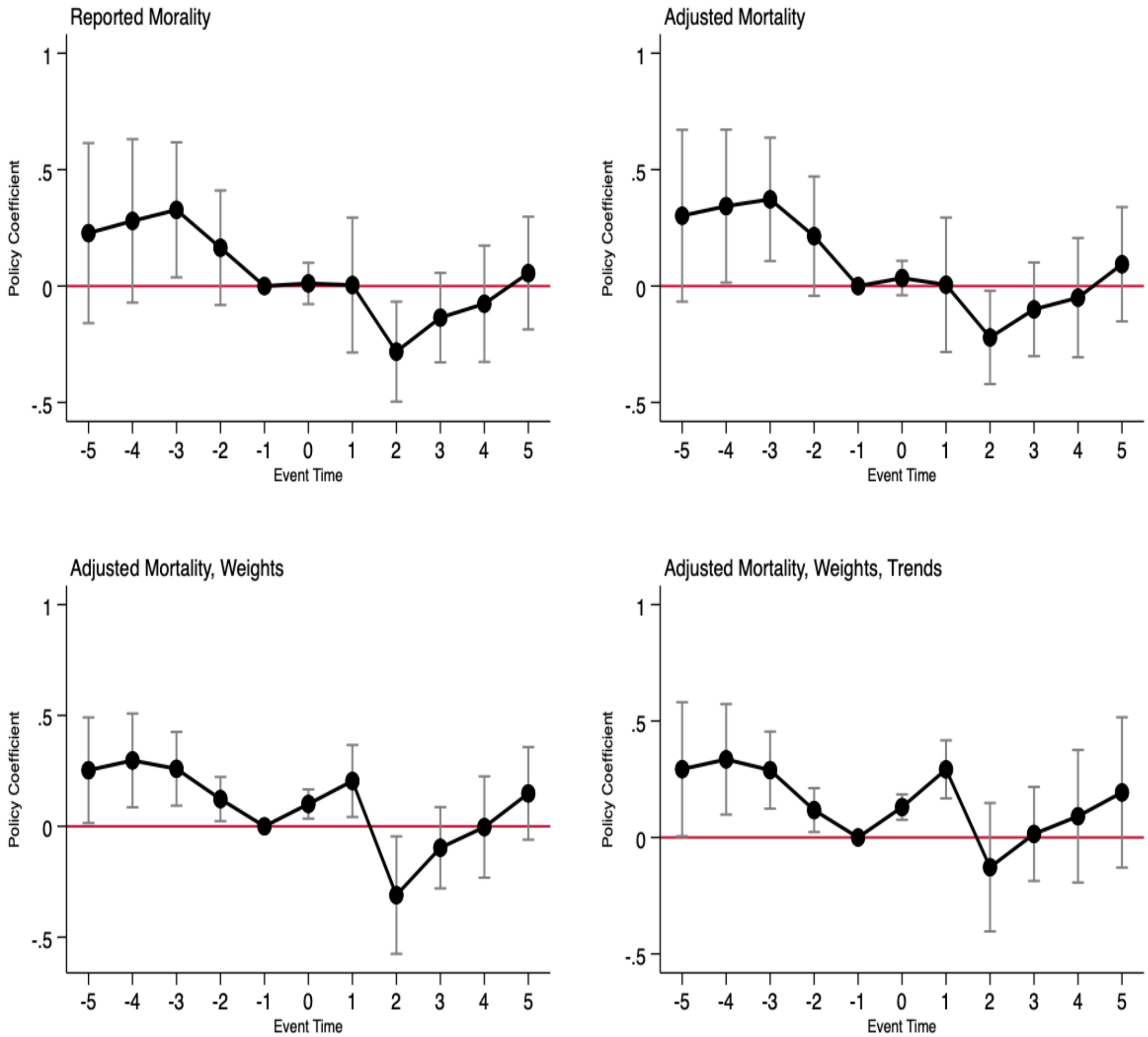
Note: See note on Figure 2. All estimates include the preferred set of controls and state and year fixed effects. Opioid mortality (T-Codes: 40.0-40.4, 40.6) is corrected for under-reporting, except in the first panel. State specific-time trends are included, and observations are weighted for state population, as described at the top of each sub-figure.

Figure A.3: Recreational Marijuana Legalization Event Studies



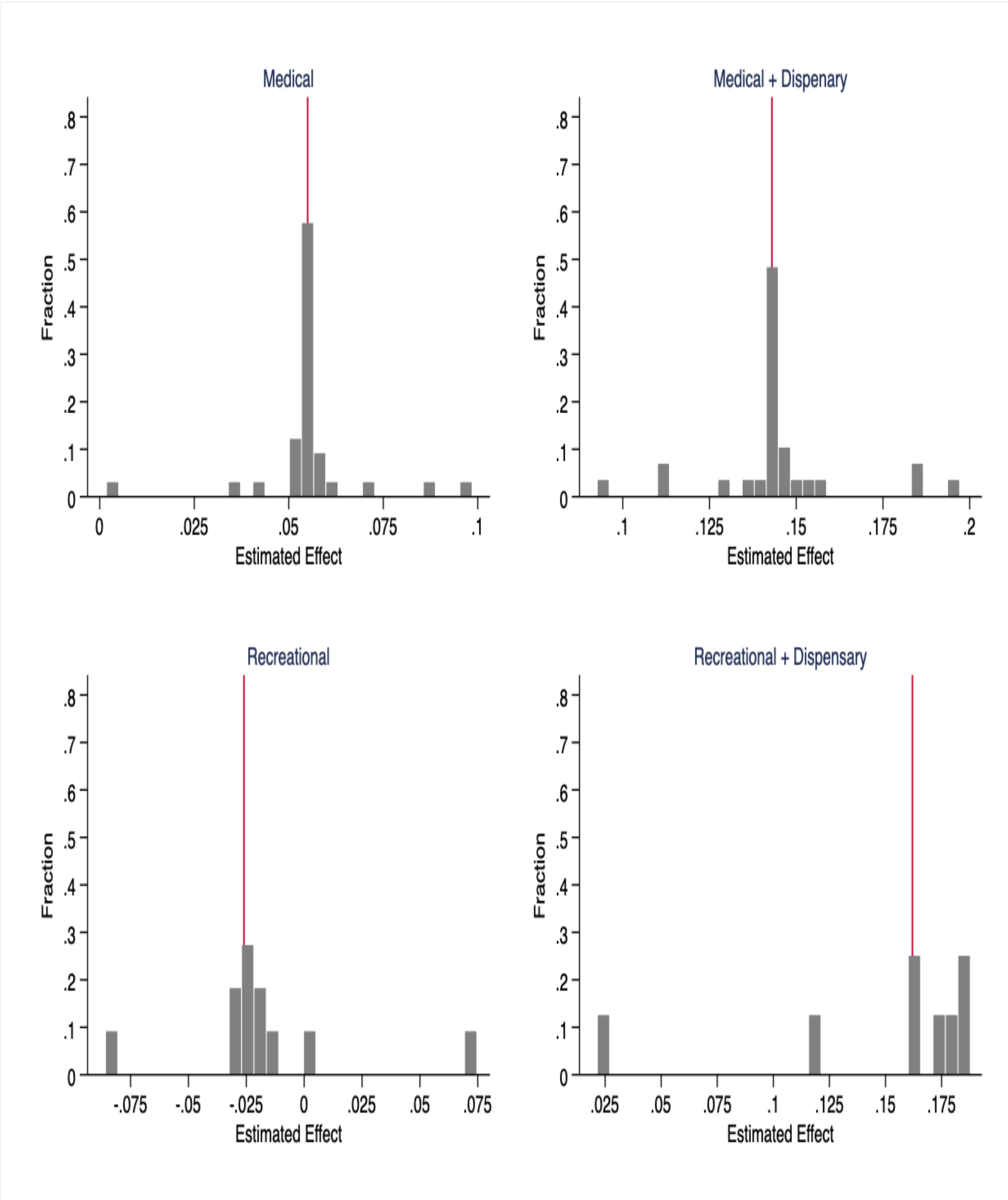
Note: See note on Figure 2. All estimates include the preferred set of controls and state and year fixed effects. Opioid mortality (T-Codes: 40.0-40.4, 40.6) is corrected for under-reporting, except in the first panel. State specific-time trends are included, and observations are weighted for state population, as described at the top of each sub-figure.

Figure A.4: Recreational Marijuana Dispensary Event Studies



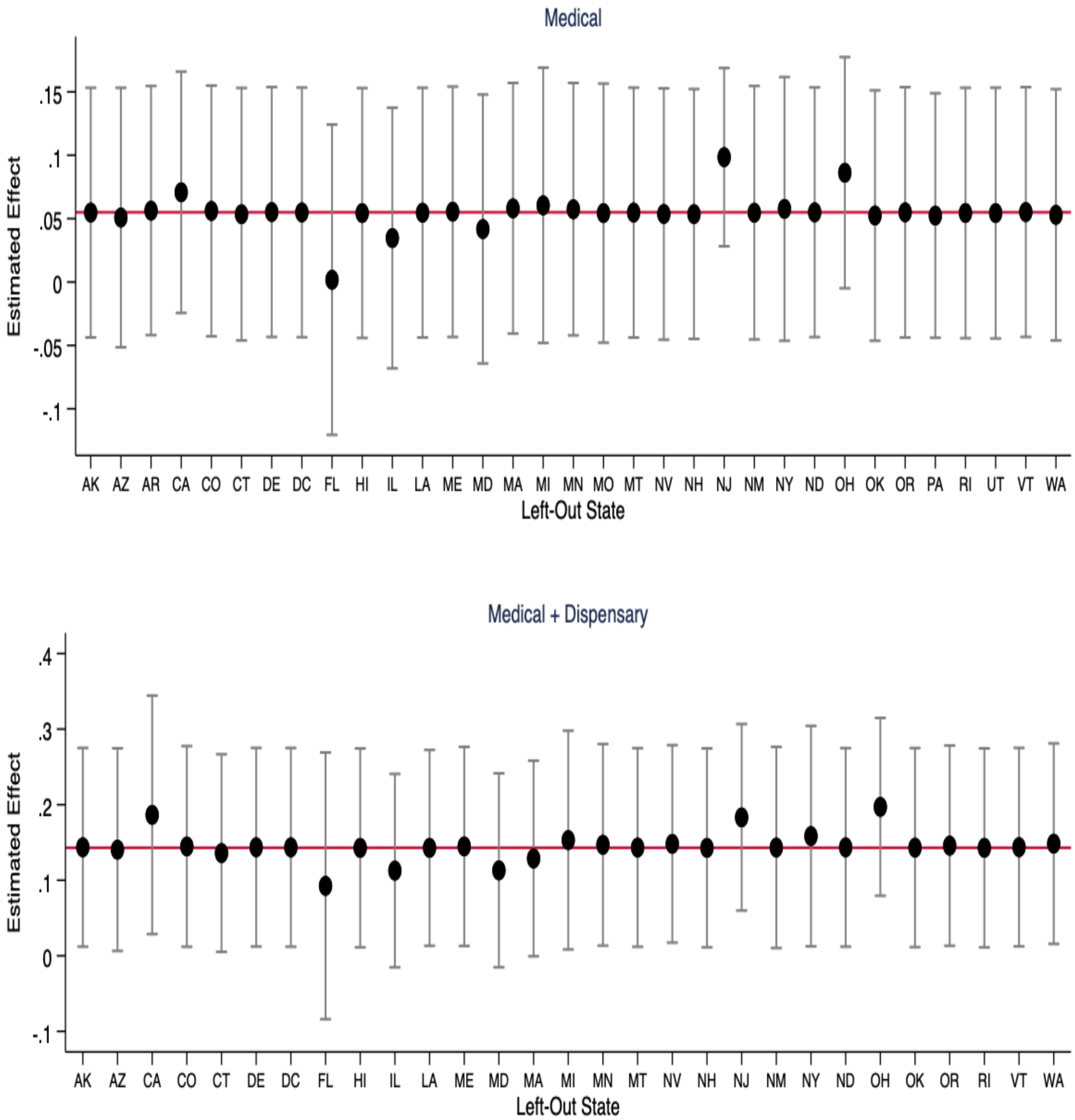
Note: See note on Figure 2. All estimates include the preferred set of controls and state and year fixed effects. Opioid mortality (T-Codes: 40.0-40.4, 40.6) is corrected for under-reporting, except in the first panel. State specific-time trends are included, and observations are weighted for state population, as described at the top of each sub-figure.

Figure A.5: Histograms of Estimated Policy Effects from Leave-One-Out Models



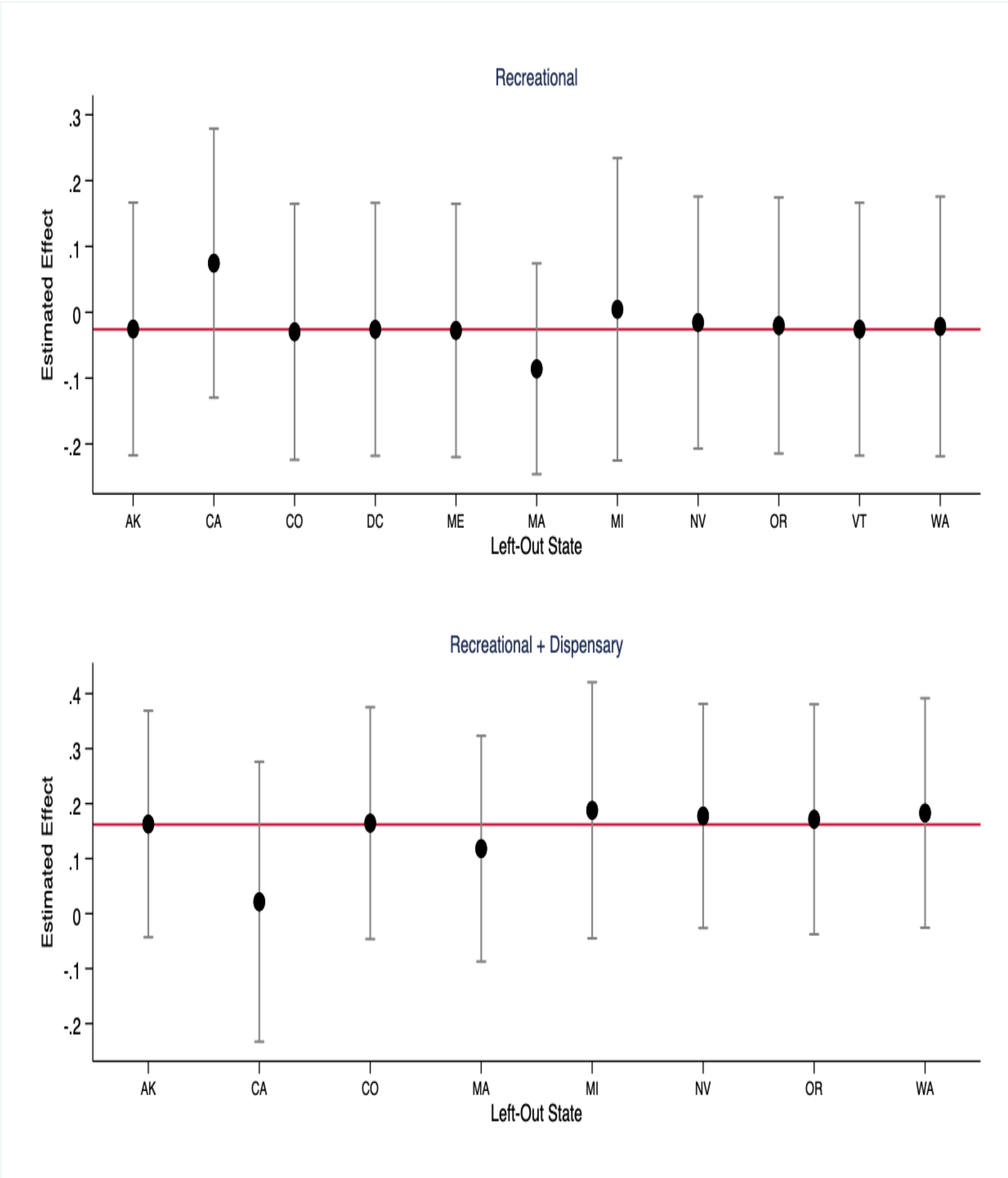
Note: Figure shows histograms of estimated policy effects on opioid mortality (T-Codes: 40.0-40.4, 40.6) from models where one state instituting the specified marijuana policy changes are successively excluded from the sample.

Figure A.6: Leave-One-Out Estimated Effects and Confidence Intervals for Medical Marijuana Legalization



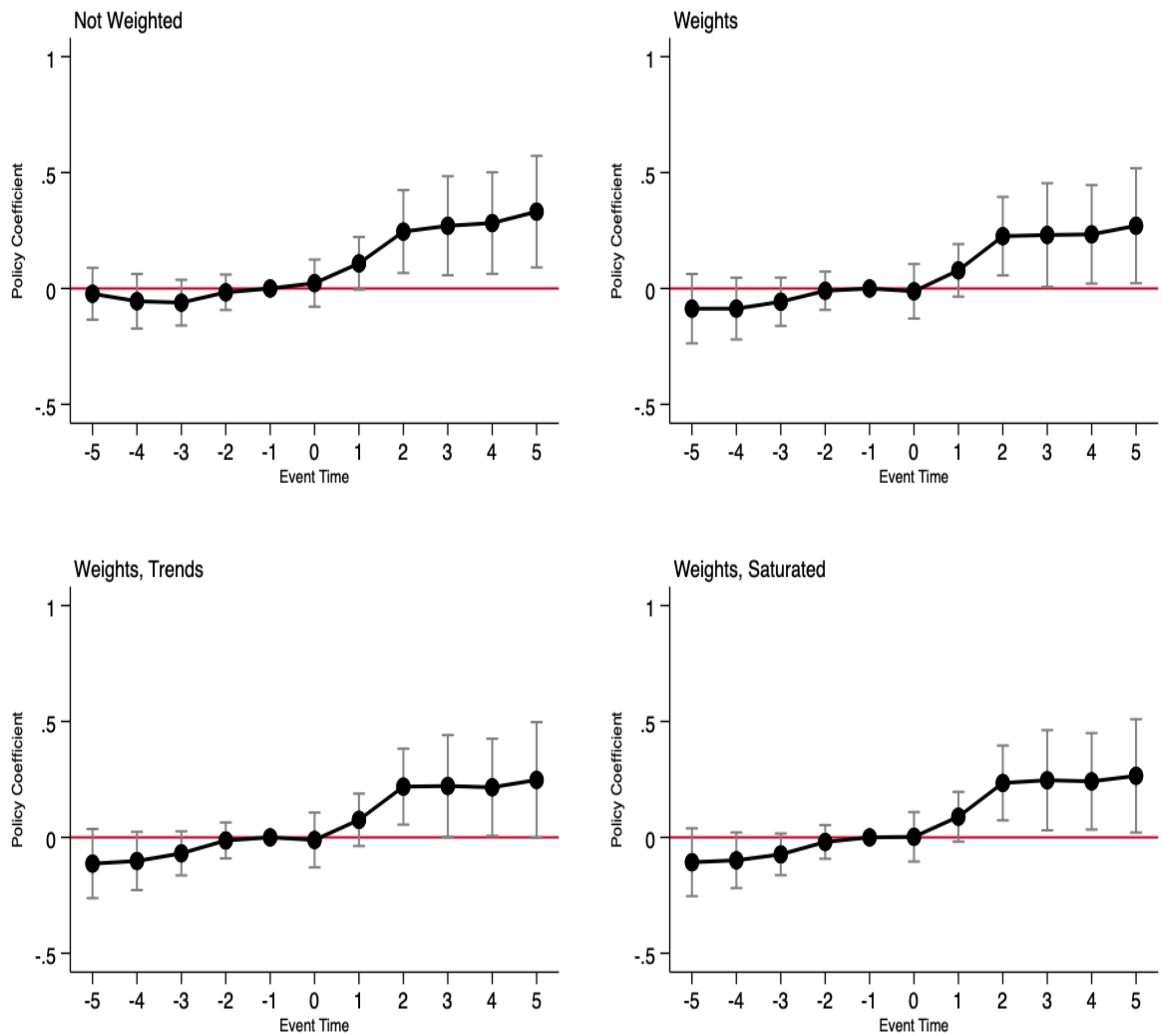
Note: Figure shows estimated medical marijuana policy effects on opioid mortality (T-Codes: 40.0-40.4, 40.6) and 95% confidence intervals from models where one state instituting the specified policy changes is successively excluded from the sample.

Figure A.7: Leave-One-Out Estimated Effects and Confidence Intervals for Recreational Marijuana Legalization



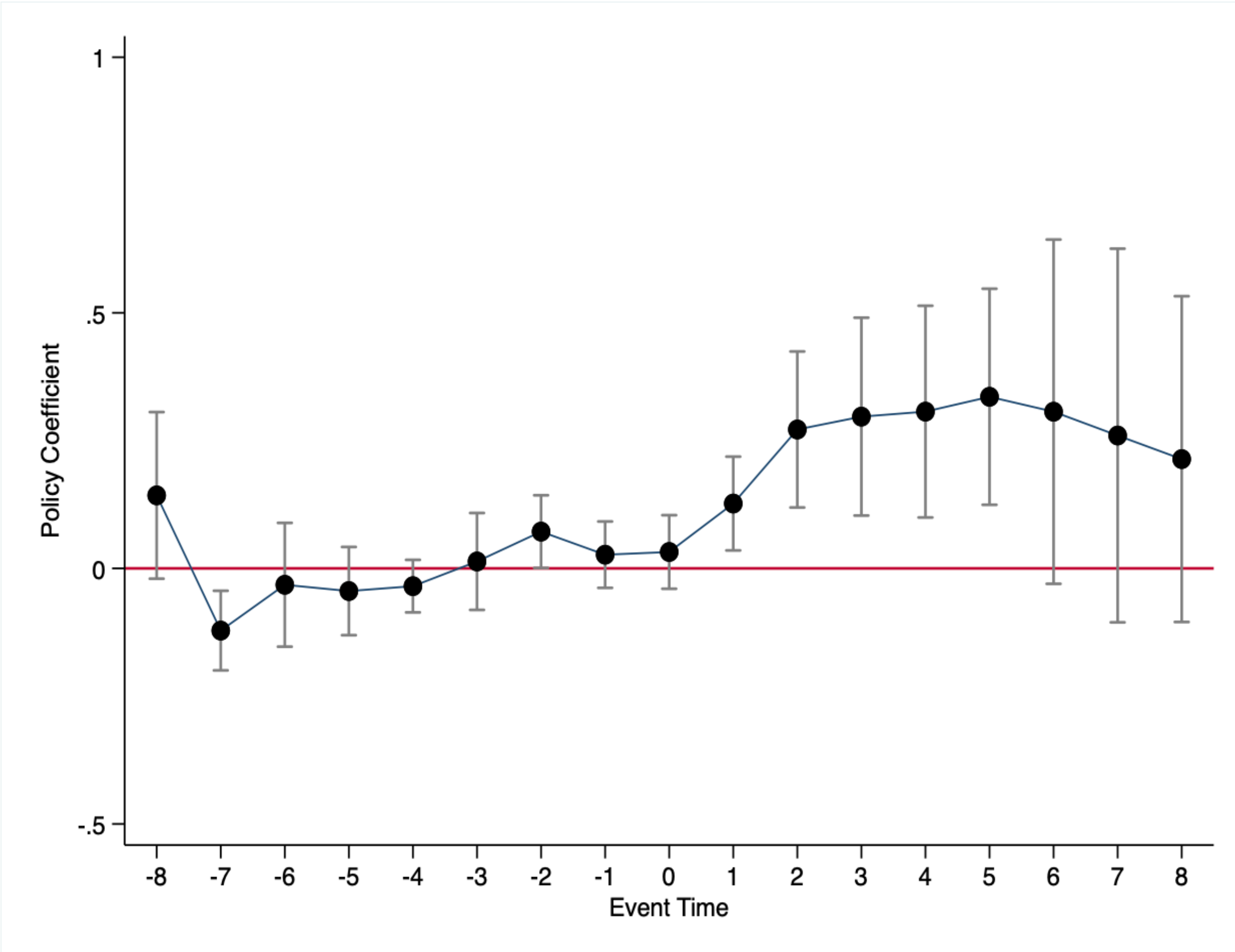
Note: Figure shows estimated recreational marijuana policy effects on opioid mortality (T-Codes: 40.0-40.4, 40.6) and 95% confidence intervals from models where one state instituting the specified policy changes is successively excluded from the sample.

Figure A.8: Stacked Regression Event Study Estimates of Medical Marijuana Legalization Effect on Opioid Deaths



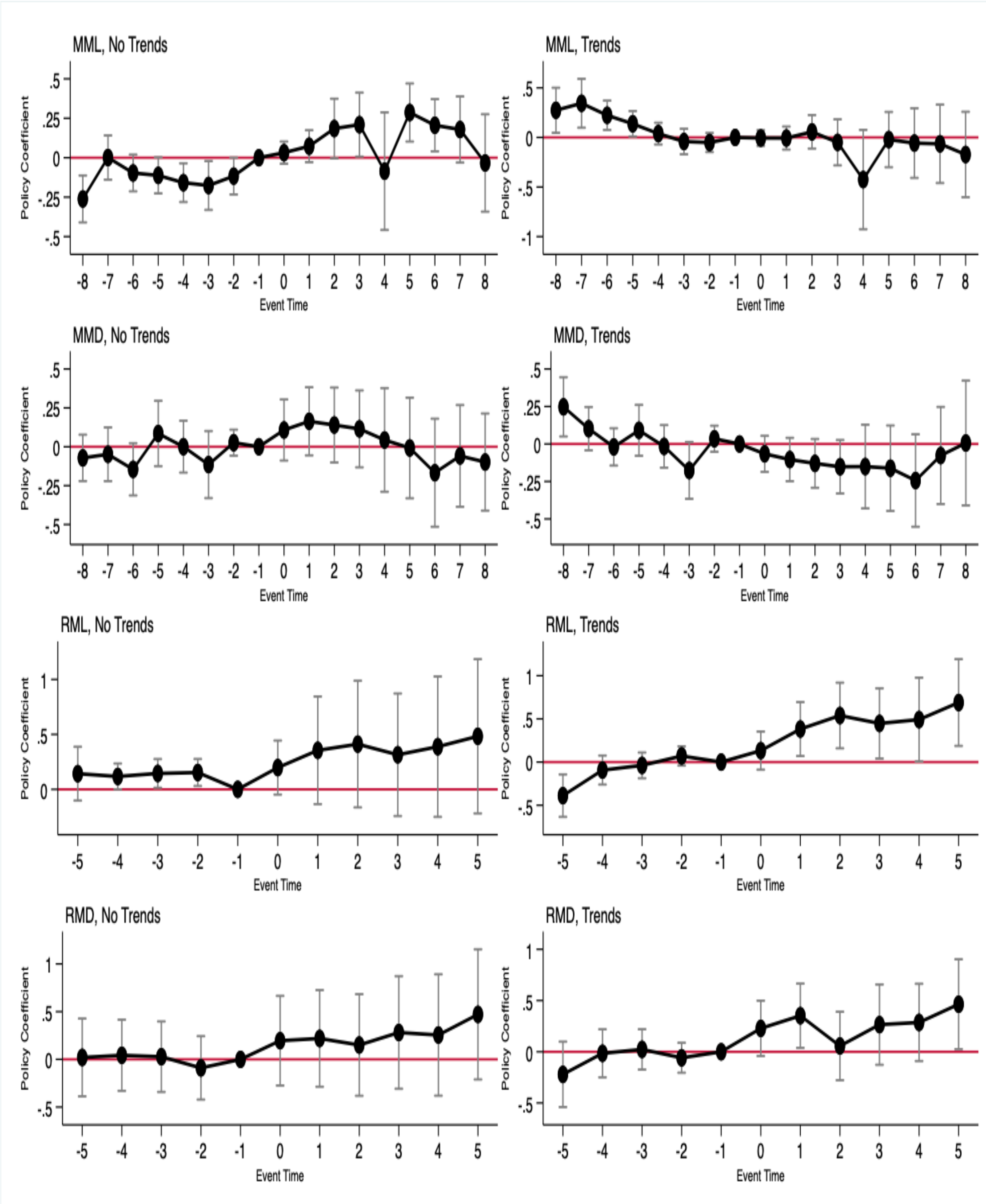
Notes: Figure shows event study results for opioid mortality (T-Codes: 40.0-40.4, 40.6) from "stacked" Poisson regressions where the treatment states legalized medical marijuana between 2004 and 2014 and the control states had not legalized medical marijuana as of 2019. Each of these 11-years is treated 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 as well as the preferred set of controls described above. Additional controls and weighted data are used as described on the figures. The "Saturated" models include cohort-by-state and cohort-by-year fixed effects.

Figure A.9: Callaway-Sant'Anna Event Study Estimates of Medical Marijuana Legalization Effect on Opioid Deaths



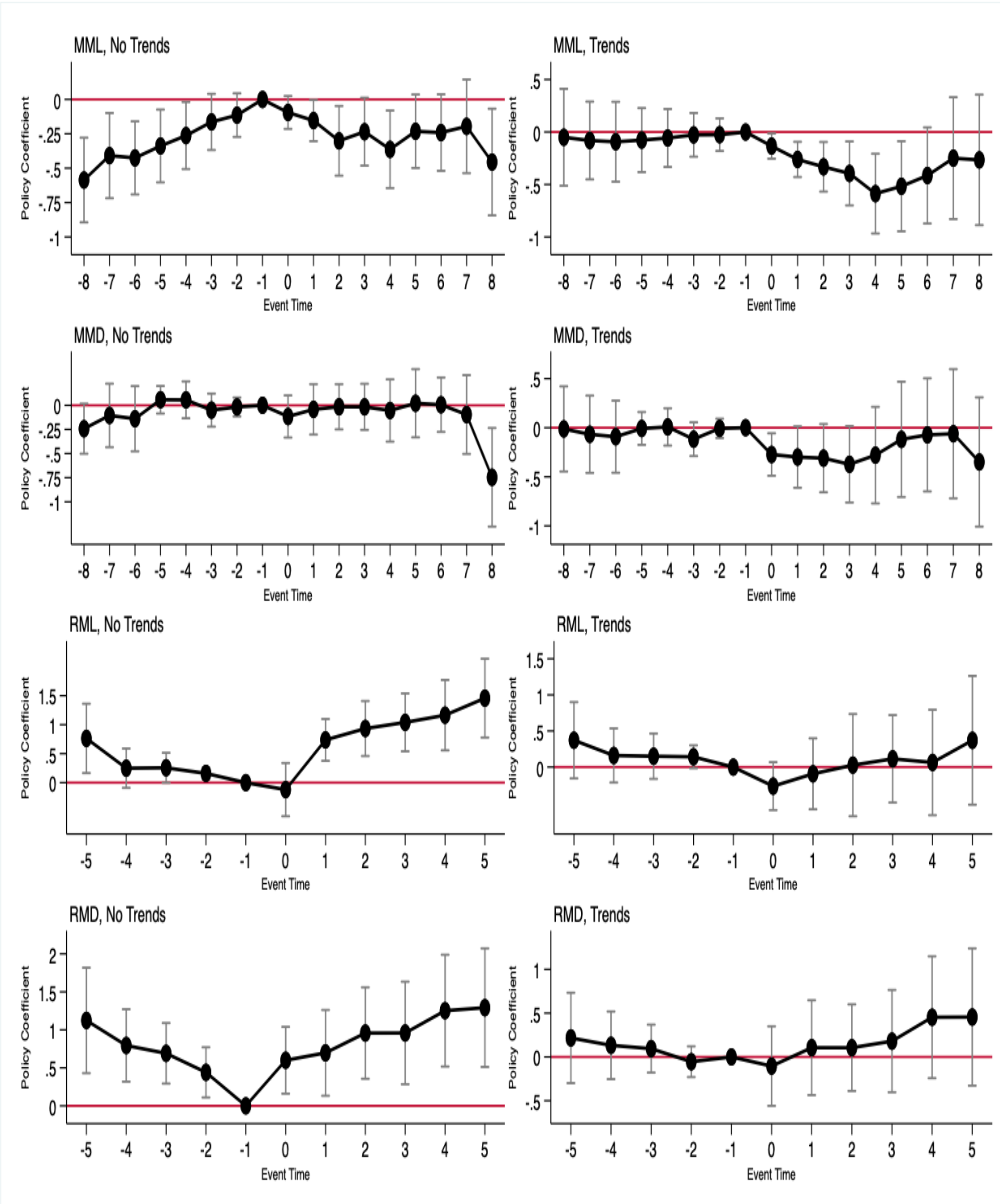
Notes: Figure shows event study using the Callaway-Sant'Anna for opioid mortality (T-Codes: 40.0-40.4, 40.6) using a sample of states that legalized medical marijuana between 2004 and 2014 or were control states that had not legalized medical marijuana as of 2019. This guarantees that there were data from at least t-5 to t+5 for legalizing states. The dependent variable was the natural log of the opioid death rate and observations were weighted by state-year populations.

Figure A.10: Event Study Estimates for Opioid Analgesic Deaths



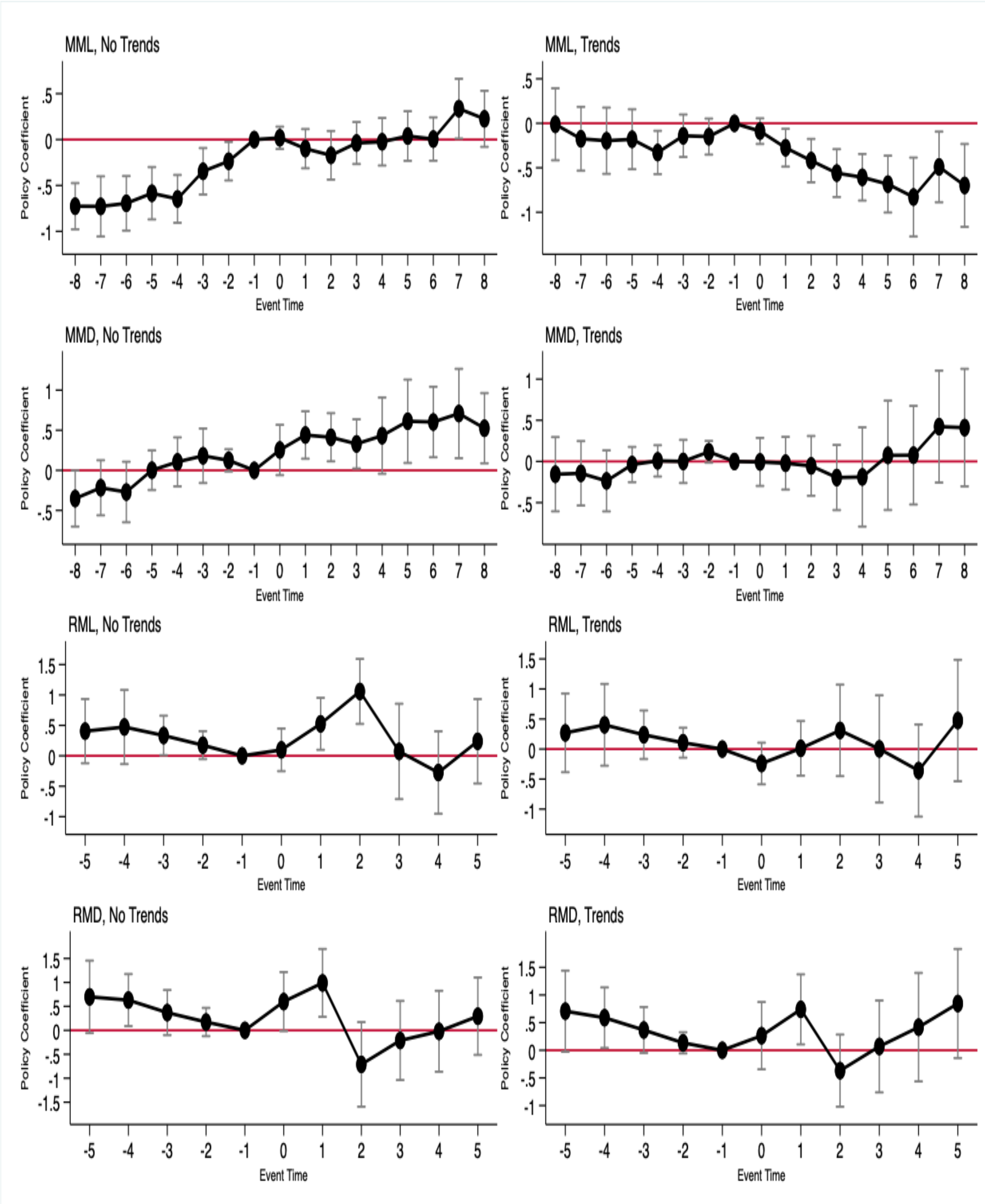
Note: The dependent variable is opioid analgesic mortality (T-Code 40.2). Estimates adjust for incomplete reporting of drug involvement, include the preferred set of supplementary covariates and weight observations by state-year populations. The left column also includes controls for state-specific time trends.

Figure A.11: Event Study Estimates for Heroin Deaths



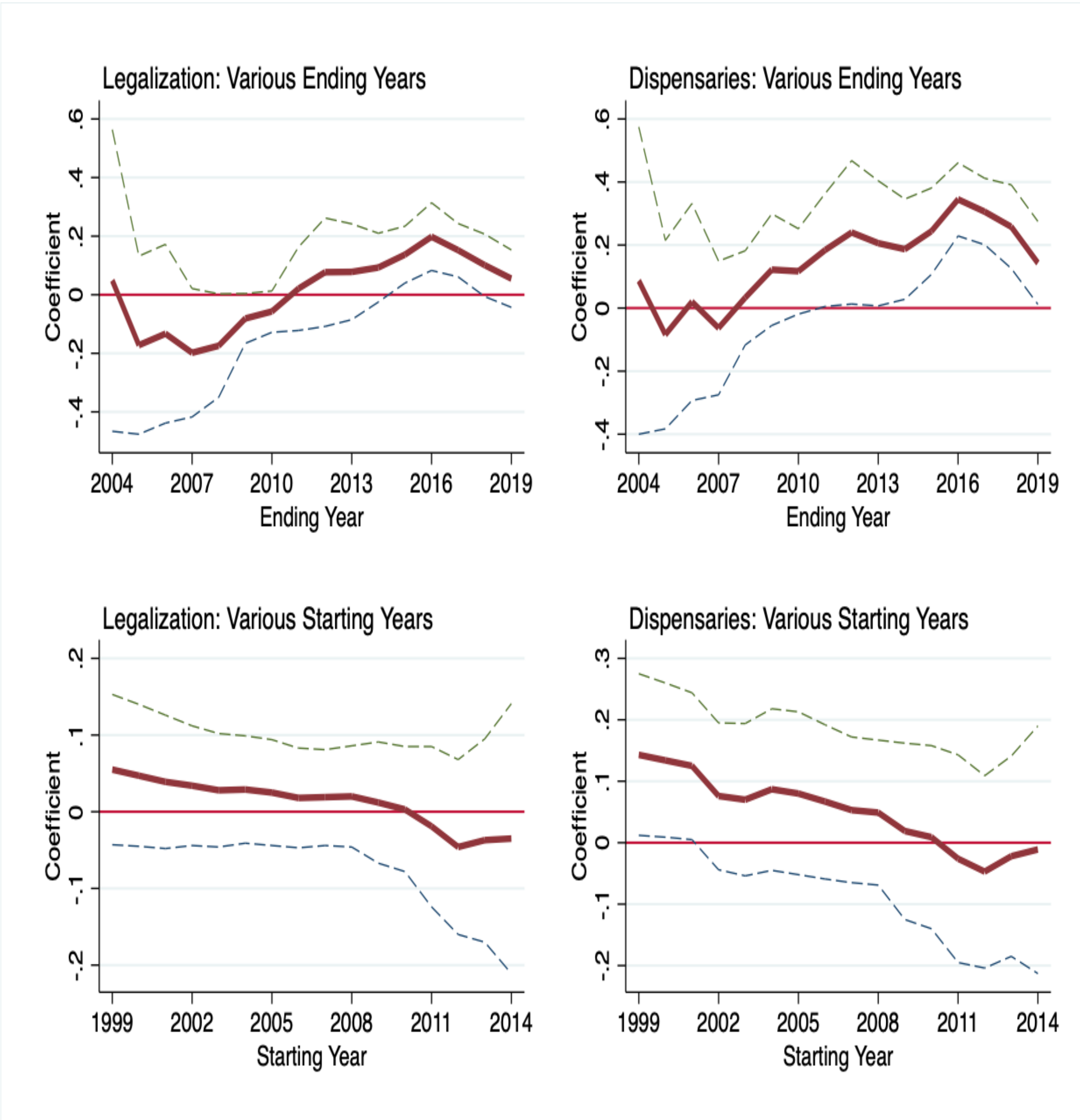
Note: The dependent variable is heroin mortality (T-Code 40.1). Estimates adjust for incomplete reporting of drug involvement, include the preferred set of supplementary covariates and weight observations by state-year populations. The left column also includes controls for state-specific time trends.

Figure A.12: Event Study Estimates for Synthetic Opioid Deaths



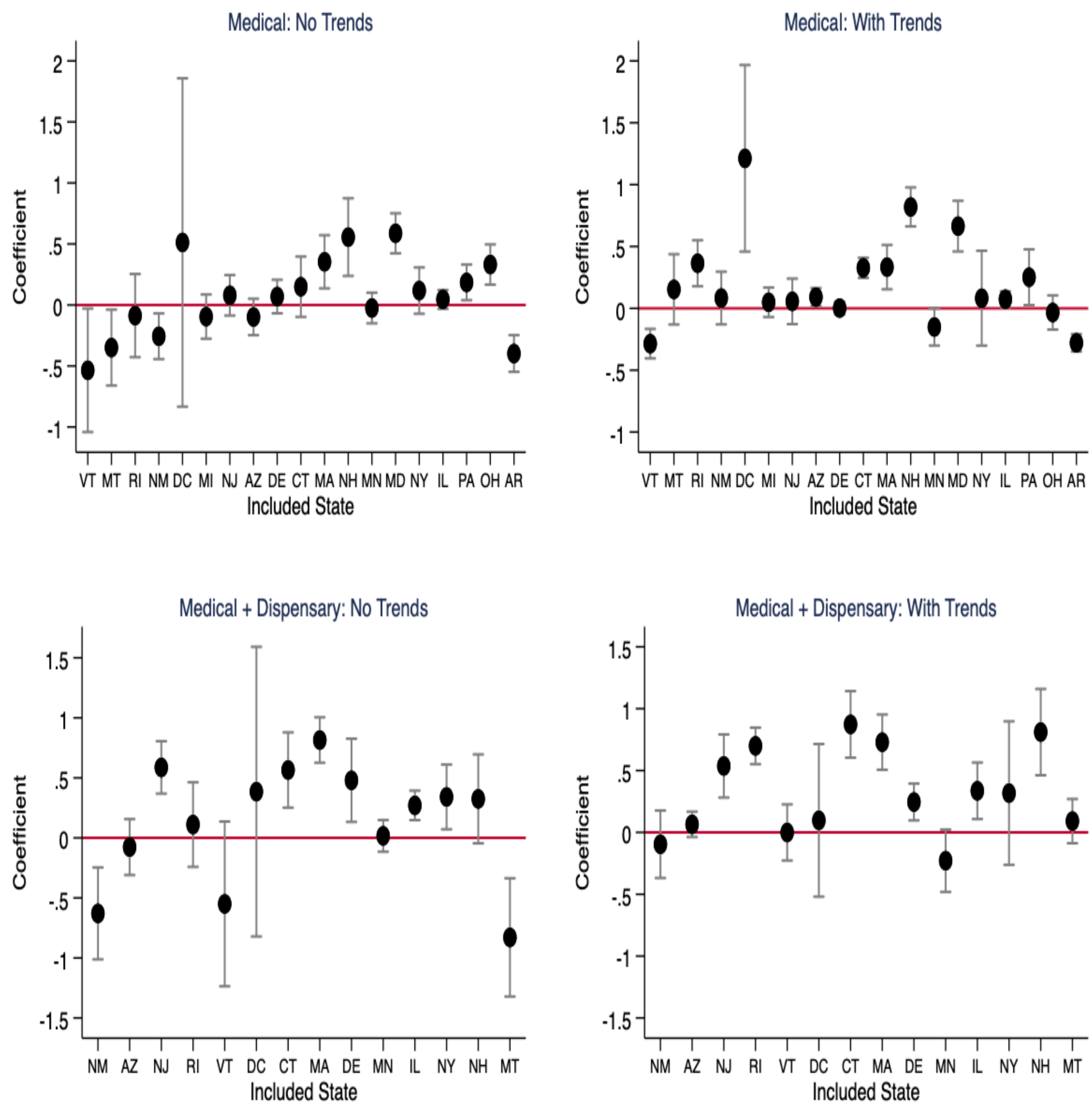
Note: The dependent variable is synthetic opioid mortality (T-Code 40.4). Estimates adjust for incomplete reporting of drug involvement, include the preferred set of supplementary covariates and weight observations by state-year populations. The left column also includes controls for state-specific time trends.

Figure A.13: Estimated Effects of Medical Marijuana Legalization and Dispensaries For Different Analysis Periods, with Controls for State-Specific Trends



Note: Figure shows predicted effect of medical marijuana legalization or operating dispensaries on opioid mortality (T-Codes: 40.0-40.4, 40.6) compared to no legalization for differing analysis periods. In the top row, the starting year is always 1999 and the ending year various between 2004 and 2019. In the bottom row, the ending year is always 2019 and the starting year varies between 1999 and 2014. Models correct opioid deaths for under-reporting, use the preferred set of controls and include vectors of state and year dummy variables, and state-specific linear trends. Recreational marijuana legalization and dispensaries are also controlled for. Dotted lines show 95 percent confidence intervals, based upon robust standard errors clustered at the state-level.

Figure A.14: Estimated Effects of Medical Marijuana Legalization and Dispensaries From "Keep One In" Regressions



Note: Figure shows predicted medical marijuana legalization or dispensary effects, relative to no legalization, on opioid mortality (T-Codes: 40.0-40.4, 40.6) from models where a single treatment state is included and the control group contains all states not legalizing medical cannabis by 2019. The treatment states are restricted to those legalizing marijuana between 2002 and 2016 (top figure) or those legalizing marijuana after 2002 and first having medical marijuana dispensaries in operation from 2002 through 2016. States are ordered on the X-axis from earliest to latest implementation of the policies.