Time Trends Matter: The Case of Medical Cannabis Laws and Opioid Overdose Mortality

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Abstract

Mortality due to opioid overdoses has been growing rapidly in the U.S., with some states experiencing much steeper increases than others. Legalizing medical cannabis could reduce opioid-related mortality if potential opioid users substitute towards cannabis as a safer alternative. I show, however, that a substantial reduction in opioid-related mortality associated with the implementation of medical cannabis laws can be explained by selection bias. States that legalized medical cannabis exhibit lower pre-existing mortality trends. Accordingly, the mitigating effect of medical cannabis laws on opioid-related mortality vanishes when I include state-specific time trends in state-year-level difference-in-differences regressions.

Keywords: medical cannabis laws, opioid overdose mortality, difference-in-differences, group-specific time trends.

JEL codes: C23, I12, I18, K32.

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1 Introduction

The number of opioid overdose deaths in the United States increased from about 9,000 in 2000 to over 42,000 in 2016. Both the level of and the rise in mortality vary substantially across states. In 2015, the opioid overdose mortality rate was highest in West Virginia with 36 per 100,000 population whereas Nebraska had the lowest mortality rate with 3 per 100,000. Mortality growth is even more heterogeneous. Since 1999, mortality rates have risen between 14% in California and 1,800% in West Virginia and almost 3,000% in North Dakota. The continued increase of opioid-related mortality also shows that existing policy responses have not been successful in containing this so-called opioid epidemic.

A potential reduction in opioid overdose mortality may come from the legalization of medical cannabis.[1] Many opioid users initially obtain the drug as a prescription to treat pain, for which medical cannabis can be a safer and less addictive substitute. For individuals who are likely to use opioids, including heroin and synthetic opioids, recreationally, access to cannabis may also provide an alternative, especially when it is legally available before an individual initiates opioid consumption. While cannabis consumption is prohibited by federal law, 29 states and the District of Columbia have passed medical cannabis laws (MCL) to date. Many of these laws regulate access to medical cannabis through dispensaries, where certified individuals can purchase the drug. Bachhuber et al. (2014) and Powell, Pacula, and Jacobson (2018) find that medical cannabis legalization is associated with a 20% to 25% reduction in mortality rates. Moreover, Powell, Pacula, and Jacobson (2018) show that this reduction mostly operates through active and legal medical cannabis dispensaries.

In this paper, I estimate the effect of medical cannabis legalization on opioid-related mortality using a difference-in-differences (DD) framework that accounts for heterogeneity in mortality trends across states. Estimates from a "canonical" DD model with state and year fixed effects imply that MCL and active and legal dispensaries significantly reduce mortality rates due to opioid overdoses. I find, however, that this negative effect of medical cannabis legalization on opioid-related mortality becomes much smaller in absolute value—or even turns positive—and is mostly statistically insignificant when I include linear or quadratic state-specific time trends in the DD regressions.

These two sets of estimates indicate that the states that have legalized medical cannabis exhibit lower pre-existing opioid mortality trends. The standard textbook DD regression that only includes state and year fixed effects cannot account for such differential trends and therefore leads to biased estimates. Put differently, the common trends assumption that is

[1] Throughout this paper, I refer to the psychoactive drug derived from the cannabis plant as cannabis instead of marijuana due to the racial implications of the latter term.

necessary for a DD regression to identify the causal effect of a policy appears to be violated in this setting. It is possible that states with slower growth in opioid-related mortality are more likely to implement an MCL because attitudes towards cannabis legalization may be correlated with economic conditions, health issues, and supply side factors that have given rise to the opioid epidemic (see Hollingsworth, Ruhm, and Simon, 2017; Ruhm, 2018). For example, states that have legalized medical cannabis, such as California and Washington, have lower opioid-related mortality rates than West Virginia and some midwestern states, which are hit hardest by the opioid epidemic and mostly have no MCL. In a DD regression, state fixed effects can only account for such differences in levels.

In addition, MCL-states also experienced slower growth in opioid-related mortality before they implemented these laws than non-MCL-states during the same time period. For example, in states that introduced an MCL in 2013 (Connecticut and Massachusetts), opioid mortality rates increased by 58% between 1999 and 2012 whereas they rose by 491% in states that have not implemented an MCL so far (see Table 2). Importantly, a combination of state and year fixed effects cannot account for this pattern in a DD framework. Instead, it is necessary to include state-specific time trends to obtain unbiased estimates of the effect of medical cannabis legalization on opioid-related mortality.

After showing that the common trends assumption is likely violated in this case, I compare the estimated effects of MCL and medical cannabis dispensaries on opioid-related mortality based on DD regressions without and with linear and quadratic state-specific time trends. Whereas not including time trends yields negative and statistically significant coefficients, these effects become mostly indistinguishable from zero with state-specific time trends. While standard errors increase slightly when I include time trends, the point estimates are also closer to zero than in regressions without trends. The results exhibit heterogeneity across types of opioids and over time.

Moreover, I find that the mitigating impact of MCL on opioid overdose mortality diminishes over time. Specifically, if I include post-2013 years in the sample period, the effect of MCL becomes statistically insignificant regardless of the inclusion of state-specific time trends. The sample periods used by Bachhuber et al. (2014) and Powell, Pacula, and Jacobson (2018) end in 2010 and 2013. This finding shows that policies that may have been effective in the early years of the opioid epidemic are no longer successful in reducing the number of overdose deaths. Mortality rates have not only grown faster in recent years than in the 2000s, but the type of opioids most commonly associated with overdose deaths has shifted from prescription opioids to heroin and more recently to synthetic opioids such as fentanyl. In this evolving public health crisis, it is therefore crucial to re-evaluate policy responses.

Including state-specific time trends fundamentally alters the implications of this policy evaluation. Specifically, MCL do not seem to provide a successful avenue to lower opioid overdose mortality.[2] More generally, this finding highlights the importance of assessing the common trends assumption and—if it is violated—modeling group-specific trends in policy evaluations that rely on DD regressions.

In addition to MCL, I also consider the effect of other policies, including Prescription Drug Monitoring Programs (PDMP), Naloxone Access Laws, Good Samaritan Laws, and Pill Mill Bills, on opioid-related mortality. For most of these policies, I do not find significant effects when accounting for state trends.

After showing that time trends matter in analyzing the effect of MCL on opioid-related mortality, I provide a more general discussion of the role of group-specific time trends in DD regressions. Specifically, I show how policy effects can be biased in the presence of group-specific time trends when they are not included in a DD regression. To assess the need for state trends, I then plot residuals from opioid mortality regressions by medical cannabis's legal status. This simple visual tool allows me to provide further evidence against the common trends assumption in this context.

The remainder of this paper proceeds as follows. In the next Section, I provide institutional background on the opioid crisis, medical cannabis legalization, and other related policies and discuss the relevant literature. Section 3 describes the mortality data, policy variables, and the empirical framework. Section 4 assesses the common trends assumption and contains the regression results. I discuss the importance of group-specific time trends in DD regressions and the use of residual plots in Section 5. Section 6 concludes.

2 Background and Related Literature

In this section, I provide institutional background on the opioid epidemic, MCL, and other policies designed to reduce opioid-related mortality. I also discuss the existing literature on these laws and policies.

[2]Medical cannabis legalization has other benefits, however. Anderson, Hansen, and Rees (2013) show that MCL lower traffic fatalities, for instance, and several studies find a decline in opioid prescriptions due to MCL (see Section 2.2 for details).

2.1 Opioid Overdose Mortality

Addiction to opioids and the resulting rising trend in overdose mortality have become a major public health crisis in the United States. This so-called opioid epidemic, which claimed over 42,000 lives in 2016, has proceeded in three waves.[3] Initially, the increase in opioid deaths was linked to the abuse of prescription opioid analgesics such as OxyContin (oxycodone), often obtained from so-called pill mills, i.e. clinics that prescribe large quantities of opioids without medical need, or on the black market. As prescription opioids became more difficult to obtain, partially as a result of the implementation of PDMP (see below), many users switched to heroin, especially in the form of black tar heroin smuggled from Mexico. (See Quinones (2015) for an account of the diffusion of both prescription opioids and black tar heroin.) As a result, heroin overdose mortality started to rise steeply in 2010. More recently, synthetic opioids such as fentanyl have become the most common drug involved in overdose deaths. While fentanyl also plays a medical role as a powerful analgesic, it is increasingly manufactured and distributed illicitly, which has led to a sharp increase in overdose deaths starting in 2013. Since the nature and source of the involved drugs differ across these three waves, there is likely no single policy response that can contain this epidemic. This implies that policies that have reduced opioid-related mortality in the 2000s and early 2010s may not be effective any more in this still-evolving crisis. When evaluating existing policies, it is therefore important to consider the sample period, on which empirical results are based. I come back to this point when discussing my results below. Specifically, I estimate opioid mortality regressions for samples starting in 1999 and ending in 2010, 2013, and 2015.

2.2 Medical Cannabis Laws

While still classified as a Schedule I drug, i.e. a drug that has a high potential for abuse and no accepted role for medical treatment, on the federal level, 29 states and the District of Columbia have legalized the medical use of cannabis to date. The first state to do so was California in 1996, and this state was also the first to regulate activities of medical cannabis dispensaries. Thirteen states had legalized medical cannabis by 2009, but access to the drug was difficult in practice due to a lack of distributors who feared prosecution under federal law. This changed with the 2009 Ogden Memorandum establishing that no federal funds were to be used to prosecute individuals who complied with state laws. Since then, most states with MCL have permitted the operation of medical cannabis dispensaries. (See Smith (2017) for a more detailed account of medical cannabis legalization and especially the role of

[3]See https://www.cdc.gov/drugoverdose/epidemic/index.html.

dispensaries.) Legalizing medical cannabis may affect the consumption of other drugs. For example, Anderson, Hansen, and Rees (2013) estimate the effect of MCL on traffic fatalities, implying that there is a substitution from alcohol to cannabis consumption when the latter becomes legal. This effect does not change in magnitude when including state trends but it loses its statistical significance.

In the context of the opioid epidemic, Bradford and Bradford (2016), Bradford and Bradford (2017), Bradford et al. (2018), and Wen and Hockenberry (2018) show that legalization of medical cannabis reduces the number of prescriptions filled for opioid analgesics among Medicare and Medicaid enrollees. It is plausible that individuals who use opioids to treat pain may switch to medical cannabis when the latter becomes legally available since cannabis has analgesic properties. Chu (2015) finds that treatment admissions for heroin addiction are 20% lower in states with MCL. This result is not robust to the inclusion of quadratic time trends, whether they are state-specific or not. Smith (2017) estimates a similarly sized decline in opioid-related treatment admissions in core-based statistical areas (CBSA) with medical cannabis dispensaries.

Two studies have shown that MCL may lower opioid-related mortality. Bachhuber et al. (2014) use data from 1999 to 2010 and estimate a 25% reduction mortality rates in states with legalized medical cannabis. Powell, Pacula, and Jacobson (2018) extend their sample period to 2013 and find that an MCL alone lowers mortality rates by 8% to 21% depending on whether they include states that adopted an MCL after 2010. Importantly, Powell, Pacula, and Jacobson (2018) show that it is not the MCL per se that is associated with lower mortality rates, but rather whether a state legally protects medical cannabis dispensaries and dispensaries are actually operating in the state. Specifically, they estimate that active and legal dispensaries lower opioid mortality rates by 40% to 43% in the 1999 to 2010 time period and by about 23% in the 1999 to 2013 time period. They obtain similar results when adding heroin-related mortality. In contrast, the effect of MCL is not statistically significant when both MCL and active and legal dispensaries are included in the regression. The estimated patterns for opioid-related treatment admissions are similar. In determining the effects of MCL on opioid-related outcomes it is therefore not simply the legal status that matters but more so whether potential users can access medical cannabis legally through dispensaries.[4] Bachhuber et al. (2014) and Powell, Pacula, and Jacobson (2018) employ state-level DD regressions without time trends.

[4] Pacula et al. (2015) and Chapman et al. (2016) highlight the importance of accounting for differences in MCL.

Smith (2017) and Garin, Pohl, and Smith (2018) use a more granular approach that emphasizes the role of dispensaries. Garin, Pohl, and Smith (2018) consider states that eventually legalize medical cannabis and show that counties in these states that had operating dispensaries experience lower opioid and heroin-related mortality than counties without dispensaries. Both Smith (2017) and Garin, Pohl, and Smith (2018) include state-specific time trends in their CBSA- and county-level regressions.

2.3 Other Policies

Most states have addressed the opioid crisis by implementing measures that are aimed at restricting access to opioids or at alleviating the consequences of their use. Prescription Drug Monitoring Programs (PDMP) are the primary policy in the first category. Under a PDMP, physicians and/or pharmacists obtain information on their patients' previous use of controlled drugs from a central statewide database before writing or filling a new prescription. Evidence on the effectiveness of PDMP is mixed. Paulozzi, Kilbourne, and Desai (2011) and Li et al. (2014) do not find that these programs lower mortality. In contrast, Patrick et al. (2016), Buchmueller and Carey (2018), Dave, Grecu, and Saffer (2017), Meinhofer (2017), and Pardo (2017) show that the effectiveness of PDMP depends on their characteristics, and more robust programs can be successful in reducing opioid-related mortality and drug abuse. In particular, policies that require providers to use PDMP lower the number of opioid prescriptions and related mortality.

Another supply-side intervention is the reformulation of OxyContin to make it harder to abuse. Alpert, Powell, and Pacula (2017) and Evans, Lieber, and Power (2018) show that the OxyContin reformulation led to lower rates of misuse of this drug but also increased heroin overdose mortality as OxyContin users substituted towards heroin. In addition, a few states have passed Pill Mill Bills to reduce access to prescription drugs outside of legitimate medical providers. Mallatt (2017) finds that these laws can reduce the number of oxycodone prescriptions.

On the demand side, states have passed Naloxone Access Laws and Good Samaritan Laws to alleviate the fatal consequences of opioid use. Naloxone Access Laws allow unqualified individual to administer Naloxone, which counteracts the effects of an opioid overdose. Good Samaritan Laws give individuals immunity for crimes related to drug possession if they seek medical care in case of an overdose. Both of these types of laws are therefore designed to prevent fatal outcomes in the event of a drug overdose. Rees et al. (2017) find that Naloxone Access Laws and Good Samaritan Laws lowered opioid overdose mortality by about 10%. These laws do not necessarily reduce consumption of opioids. On the contrary, they could increase consumption because they lower the risk of death from overdoses and therefore implicitly make opioids safer to consume. Accordingly, Doleac and Mukherjee (2018) show that Naloxone Access Laws led to an increase in opioid-related emergency room visits and more larger numbers of thefts to finance opioid addiction. Although Naloxone Access Laws do not affect mortality overall, they find an increase in opioid overdose deaths associated with Naloxone Access Laws in the Midwest by 14%. Both studies employ DD regressions, but Rees et al. (2017) do not include time trends whereas Doleac and Mukherjee (2018) control for linear state-specific time trends. Hence, in the case of Naloxone Access Laws, controlling for state-specific time trends can change conclusions about the effectiveness of the policy.

3 Data and Methods

3.1 Data Sources

Using the universe of death records from 1999 to 2015 from the CDC's National Vital Statistics System (NVSS) and population data from the Surveillance, Epidemiology, and End Results (SEER) Program, I construct mortality rates per 100,000 population on the state and year level. The NVSS mortality data contain multiple causes of death, which allows me to identify the type of drug that was involved in a given death. I define opioid overdoses as deaths due to "Accidental poisoning" (ICD-10 codes X40 to X44), "Intentional self-poisoning" (X60 to X69), "Assault by drugs, medicaments and biological substances" (X85), or "Poisoning with undetermined intent" (Y10 to Y14) where heroin (T40.1), other opioids (T40.2), methadone (T40.3), or other synthetic opioids (T40.4) are listed as a contributing cause of death. "Other opioids" include natural and semisynthetic opioids and in particular prescription opioids. Since it is often not observable by medical examiners which specific opioid caused a death and whether it was prescribed or obtained illegally, I combine the codes T40.2 to T40.4 in the main results and refer to these drugs simply as "opioids."

I obtain information on relevant state and year level policies from various existing studies that have collected policy information from original sources.[5] To measure legal access to medical cannabis, I follow the approach proposed by Powell, Pacula, and Jacobson (2018) and differentiate between MCL and whether a state legally protects dispensaries and dispensaries are actually operating in the state (active and legal dispensaries). I rely on the policy data collected by these authors for the 1999 to 2013 period (see also Chriqui et al., 2002; Pacula

[5]I am grateful to these authors for publishing their policy variables.

et al., 2002; Pacula, Boustead, and Hunt, 2014). For the post-2013 period, I use information on MCL and dispensary status from Smith (2017). In addition, I use information on state laws that require the use of PDMP from Meinhofer (2017), on Naloxone Access Laws and Good Samaritan Laws from Rees et al. (2017), and on Pill Mill Bills from Mallatt (2017).

Control variables includes states' demographic composition (fraction male, fraction white, and fractions aged 15 to 19, 20 to 64, and 65 and over), which I obtain from population data from the SEER Program.[6] I also control for state-level unemployment rates derived from the Bureau of Labor Statistics's Local Area Unemployment Statistics and states' beer tax rates obtained from the Beer Institute's Brewers Almanac.[7]

3.2 Summary Statistics

Table 1 shows state-year-level summary statistics for opioid-related mortality rates, policy variables, and demographic covariates by medical cannabis legalization status. For opioids overall as well as for individual types of opioids and heroin, mortality rates are higher when an MCL is implemented and even higher when there are active and legal dispensaries. These differences are unconditional and do not imply a positive effects of medical cannabis legalization on opioid-related mortality. Policies that are intended to alleviate the opioid crisis are positively correlated with MCL and active and legal dispensaries. This relationship is partly due to the fact that more states legalized medical cannabis over the sample period and increasingly implemented PDMP, Naloxone Access Laws, and Good Samaritan Laws. It also highlights the importance of controlling for the presence of other policies when estimating the effect of MCL. In the regressions below, I also control for the beer tax rate, the unemployment rates, and demographics. These covariates do not differ substantially by medical cannabis legalization status.

3.3 Empirical Strategy

To estimate the effect of legalizing medical cannabis on opioid-related mortality, I follow Powell, Pacula, and Jacobson (2018) and differentiate between MCL per se and whether a state has active and legal medical cannabis dispensaries. Since actual access to medical cannabis may be more important than its legal status, it is important to distinguish between these two levels of medical cannabis's status in a given state (see also Smith, 2017; Garin,

[6]See https://seer.cancer.gov/popdata/.

[7]See https://www.bls.gov/lau/home.htm and http://www.beerinstitute.org/multimedia/brewers-almanac/.

Pohl, and Smith, 2018). The baseline DD regression for state-year-level opioid overdose mortality rates is

$$\log(MR_{st}) = \theta_1^n MCL_{st} + \theta_2^n ALD_{st} + X_{st}'\beta^n + \alpha_s^n + \gamma_t^n + u_{st}^n, \tag{1}$$

where MR_{st} is the mortality rate per 100,000 population in state s and year t. I consider four specific mortality rates: due to opioids overall (excluding heroin), prescription opioids, heroin, and synthetic opioids. The main policy variables are MCL_{st} and ALD_{st}. MCL_{st} is a dummy variable that equals one if state s has legalized medical cannabis in year t and zero otherwise. ALD_{st} is an indicator variable that turns on if state s has active and legal dispensaries in year t. The vector X_{st} includes indicators for whether the other policies mentioned above (required PDMP, Naloxone Access Laws, Good Samaritan Laws, and Pill Mill Bills) are in place as well as additional control variables (the beer tax rate, unemployment rate, and fraction white, male, aged 15 to 19, 20 to 64, and 65 and over). Finally, α_s^n and γ_t^n are state and year fixed effects. The superscript ^{n} stands for "no time trends" to distinguish the coefficients in regression (1) from the regressions with state-specific time trends below. Standard errors are clustered on the state level.

The coefficients of interest are θ_1^n and θ_2^n. They measure the approximate percentage effect of the implementation of an MCL and of whether a state has active and legal dispensaries on opioid-related mortality. To assess if medical cannabis legalization has an effect on opioid overdose mortality overall, I test the joint hypothesis $H_0 : \theta_1^n = \theta_2^n = 0$ and report the corresponding p-value.

To allow for different trends in opioid-related mortality across states, I add linear state-specific time trends $\mu_s^l t$ to regression (1):

$$\log(MR_{st}) = \theta_1^l MCL_{st} + \theta_2^l ALD_{st} + X_{st}'\beta^l + \alpha_s^l + \gamma_t^l + \mu_s^l t + u_{st}^l. \tag{2}$$

If the true relationship between medical cannabis legalization and opioid-related mortality is given by regression (2) instead of regression (1), the time trends would be part of the error term in regression (1), i.e. $u_{st}^n = \mu_s^l t + u_{st}^l$. Omitting time trends would therefore lead to biased estimates $\hat{\theta}_1^n$ and $\hat{\theta}_2^n$ if the implementation of MCL or active and legal dispensaries is correlated with state-specific time trends $\mu_s^l t$. This would occur, for instance, if states where opioid mortality rates grow relatively slowly are more likely to legalize medical cannabis. If states do not exhibit differential mortality trends, however, including state-specific time trends would introduce additional noise, thereby leading to inefficient estimates. The assumption of linear time trends may be too restrictive since opioid mortality rates have started to grow at a faster than constant rate in recent years.[8] I therefore allow for quadratic time trends in addition to the linear time trends in regression (2) by adding the term $\nu_s^q t^2$:

$$\log(MR_{st}) = \theta_1^q MCL_{st} + \theta_2^q ALD_{st} + X_{st}'\beta^q + \alpha_s^q + \gamma_t^q + \mu_s^q t + \nu_s^q t^2 + u_{st}^q. \tag{3}$$

To assess whether mortality trends differ across states and therefore make the inclusion of state-specific time trends necessary, I test the hypothesis $H_0 : \mu_s^l = \mu_{s'}^l, \forall s, s'$ after estimating regression (2) and the hypotheses $H_0 : \mu_s^q = \mu_{s'}^q, \forall s, s'$ and $H_0 : \nu_s^q = \nu_{s'}^q, \forall s, s'$ after regression (3). Rejecting these hypotheses implies that states exhibit different trends in opioid-related mortality rates over time, but not necessarily that these time trends are correlated with the implementation of MCL or active and legal dispensaries. I revisit the role of state-specific time trends in Section 5.

4 Results

4.1 Assessing the Common Trends Assumption

Before discussing the regression results, I provide descriptive evidence that shows whether the common trends assumption is violated. In Figure 1, I plot mean mortality rates per 100,000 population for deaths due to opioids, i.e. prescription opioids, methadone, and synthetic opioids combined, and heroin. I calculate mean mortality rates in each year for three groups of states: those that never implemented an MCL by 2015, those that had an MCL in place in a given year, and those that did not have an MCL in a given year but implemented it by 2015, see Figures 1a and 1c. I also plot the analogue mean mortality rates by whether states had active and legal dispensaries, implemented them at a later point in time, or never had them, see Figures 1b and 1d. To assess the common trends assumption, the relevant comparison is between the states that never implemented an MCL or never had active and legal dispensaries and between states that did so at a later year (MCL-states before MCL and ALD-states before ALD on Figure 1's labels).

Figure 1a shows that mortality rates in states without an MCL and MCL-states before the law's implementation trended similarly between 1999 and 2006 but diverged afterwards. Specifically, the growth in opioid overdose mortality slowed in MCL-states before they implemented the law. The pattern by dispensary status in Figure 1b is similar. Up to 2006, mortality trends are parallel with states that later implemented their MCL having higher

[8]The outcome variable is log-mortality rates, which makes it more likely that linear time trends are sufficient. However, I can test whether quadratic time trends should be included, see below.

rates. Starting in 2006, however, the mortality trends diverge. Since the majority of MCL were implemented after 2006, these diverging mortality trends imply that the common trends assumption is violated. Put differently, states that introduced an MCL after 2006 (the treatment group) experienced different pre-treatment trends in the outcome of interest than states that did not have an MCL (the control group). In particular, the introduction of MCL is associated with lower mortality, which likely introduces selection bias. Controlling for state and year fixed effects is unlikely to solve this issue because there is not simply a parallel difference in trends that these two sets of fixed effects would control for. Rather, it is likely that state-specific time trends are necessary to reduce selection bias.

The trends in heroin-related mortality rates, shown in Figures 1c and 1d, do not exhibit the same extent of divergence between states that never had MCL or active and legal dispensaries and states with a later MCL or active and legal dispensaries start date. There is some evidence that heroin mortality rates grow faster in the treatment group at the end of the sample period, however, especially when classifying states by their dispensary status.

Next, I compare the opioid overdose mortality growth after 1999 in states that implemented an MCL or had active and legal dispensaries to states that did not. Specifically, I calculate the percentage change in combined opioid and heroin mortality rates per 100,000 population between 1999 and the year before the first full year that a state had an MCL or active and legal dispensaries. Table 2 shows the resulting percentage changes by MCL status. For example, the table indicates that mortality rates in Colorado and Hawaii, which implemented an MCL in 2001, decline by 1% between 1999 and 2000 on average. During the same time, states that had never had an MCL by 2015 experienced an increase in mortality rates by 49%. The same pattern holds for all the remaining years: states that implemented an MCL saw a lower rise on opioid-related mortality in the years before the MCL came into effect than states without an MCL during the sample period. The difference can be substantial. Mortality increased by 44% on average in Arizona, the District of Columbia, and New Jersey between 1999 and 2010, for example, whereas it grew by 492% in never-MCL states.

Table 3 shows a similar pattern for states that had active and legal dispensaries compared to states that never allowed dispensaries to operate by 2015. Between 1999 and 2013, for example, mortality rates rose by 233% in the District of Columbia, Michigan, Montana, Nevada, Oregon, Rhode Island, Vermont, and Washington, which started having active and legal dispensaries in 2014, whereas mortality increased by 432% in states that never had active and legal dispensaries over the same time period. The smaller mortality growth in states with active and legal dispensaries also holds in all other years. These results clearly show that states with active and legal dispensaries are negatively selected on pre-existing mortality trends. Taken together, Tables 2 and 3 provide additional evidence that states with an MCL or active and legal dispensaries experienced lower mortality rates in the years leading up to an MCL implementation or start of active and legal dispensaries than states that did not legalize medical cannabis or dispensaries.

Figure 1 and Tables 2 and 3 show simple mean mortality rates and their changes without controlling for state and year fixed effects or other covariates. Nevertheless, this evidence suggests that the common trends assumption underlying a standard DD analysis is likely violated in this case. In Section 5.2, I provider further evidence against the common trends assumption conditional on state and year fixed effects and covariates.

4.2 Impact of MCL and Dispensary Status on Opioid-Related Mortality

For each mortality outcome, I estimate regressions for the time periods 1999 to 2010, 1999 to 2013, and 1999 to 2015. The first time period corresponds to the first wave of the opioid crisis that mostly involved prescription opioids, the second set of regressions adds the years during which heroin emerged as the main driver of opioid-related mortality, and the 1999 to 2015 time period also captures the recent increase in mortality due to synthetic opioids. For each of these three time periods, I estimate regression (1) without time trends, regression (2) with linear state-specific time trends, and regression (3) with quadratic time trends.

First, I consider the effect of MCL and active and legal dispensaries on the log-mortality rate per 100,000 population due to opioids, which include prescription opioids, methadone, and synthetic opioids, i.e. all types of opioids excluding heroin. Table 4 shows the regression results. Starting with the 1999 to 2010 period and no time trends, column (1) shows a coefficient of -0.186, which corresponds to a decline in opioid-related mortality by 17% when an MCL is in effect. This effect is nearly identical to the corresponding estimate found by Powell, Pacula, and Jacobson (2018) and slightly smaller than in Bachhuber et al. (2014). This effect is significant at the 10% level. In addition, active and legal dispensaries reduce opioid mortality rates by an additional 26%, and this estimate is significant at the 1% level. These estimates confirm the results in Powell, Pacula, and Jacobson (2018), i.e. it is not primarily the implementation of an MCL that lowers opioid-related mortality, but it is more important whether a state has legalized medical cannabis dispensaries and they are operational.

Adding linear state-specific time trends slightly increases the point estimate for the effect of MCL, but lowers its precision, see column (2). When adding quadratic state trends in column (3), the effect of MCL on opioid-related mortality rates equals -0.4% and is not significant at conventional levels, i.e. the negative effect of MCL on opioid overdose mortality disappears. The estimated effect of active and legal dispensaries also substantially changes when linear or quadratic state trends are included. The point estimate is reduced to about a third and the effect loses its statistical significance. When adding linear and quadratic time trends, the MCL and dispensary status indicators are not jointly statistically significant, with p-values for the associated F-tests of 0.20 and 0.94. In other words, when allowing for state-specific time trends, I cannot reject the null hypothesis that MCL and dispensary status have no effect on opioid-related mortality. This conclusion contrasts with the estimates that do not account for state trends in column (1) and which point to a strong mortality-reducing effect of MCL and especially active and legal dispensaries.

Next, I revisit the role of state-specific time trends when the sample period is extended to 2013 or 2015. Due to the changing nature of the opioid crisis, these results are important in their own right. In the 1999 to 2013 sample without time trends, the effects of MCL and active and legal dispensaries are attenuated and less precisely estimated compared to the 1999 to 2010 sample, but the overall pattern is similar, see column (4) in Table 4. When testing the joint effect of MCL and active and legal dispensaries, the resulting p-value equals 0.08, i.e. the two variables are marginally jointly significant. Once I add linear or quadratic state trends, MCL and dispensary status have no statistically significant effect on opioid-related mortality and some point estimates even become positive. The p-values for testing the joint effect of MCL and active and legal dispensaries in the regressions in column (5) and (6) equal 0.67 and 0.60.

When further extending the sample to 2015, neither MCL nor dispensary status have a statistically significant effect on opioid-related mortality regardless of whether I include state trends in the regression, with most point estimates being positive. Moreover, MCL and dispensary status are not jointly significant with p-values of 0.80, 0.75, and 0.71 in the regressions in columns (7), (8), and (9). This finding suggests that the evolving nature of the opioid crisis led to a change in the relationship between medical cannabis legalization and opioid-related mortality. While substitution from opioids to cannabis may have lowered mortality rates before 2010, this effect disappeared in more recent years as mortality due to heroin and synthetic opioid overdoses rapidly increased. However, accounting for differences in pre-existing mortality trends across states sheds doubt on the notion that MCL had a significantly negative effect on opioid-related mortality even before 2010. For all sample periods and specifications with time trends, I strongly reject the null hypotheses that linear and quadratic trends are identical across states, see the last two rows in Table 4. This suggests that accounting for state-specific time trends is indeed necessary when modeling opioid-related mortality rates.

4.3 Impact of MCL and Dispensary Status on Mortality Related to Specific Types of Opioids and Heroin

After analyzing the impact of MCL and active and legal dispensaries on opioid-related mortality overall, i.e. the sum of deaths due to prescription and synthetic opioids and methadone, I consider effects on mortality related to prescription opioids, heroin, and synthetic opioids separately in this section. The regression results for prescription opioids are shown in Table 5, which follows the same structure as Table 4. For the 1999 to 2010 period, when prescription drugs were the main driver of the opioid crisis, the overall patterns are similar to those in Table 4, but the effects of MCL and active and legal dispensaries are larger in absolute value. Without including state trends, MCL reduce mortality due to prescription opioids by 31% and active and legal dispensaries lower mortality rates by an additional 37%, see column (1). These effects are significant at the 1% level.

As with opioids overall, the significantly negative effect of active and legal dispensaries disappears in columns (2) and (3) of Table 5 when I control for state trends. The effect of MCL does not change when I add only linear trends but is cut by two thirds and loses its statistical significance when I introduce quadratic time trends. Hence, the mitigating effect of medical cannabis legalization on mortality due to prescription opioids is more robust than for opioids overall, but accounting for differential state trends nevertheless affects the results.

When I extend the sample to 2013 or 2015, the patterns are similar but attenuated compared to the 1999 to 2010 sample period. MCL and active legal dispensaries lower prescription opioid mortality by 21% and 28% during the 1999 to 2013 period when I do not include time trends, see column (4) in Table 5. These effects become closer to zero or positive and statistically insignificant when I add linear or quadratic state-specific time trends in the regressions in columns (5) and (6). Similarly, MCL and active and legal dispensaries have smaller and less precisely estimated negative effects on prescription opioid overdoses in the 1999 to 2015 sample, see column (7), and these effects become economically and statistically insignificant when I add linear or quadratic state trends, see columns (8) and (9). The last two rows in Table 5 show that state-specific time trends should be included in these regressions since I strongly reject the null hypotheses that linear and quadratic trends are the same across states.

Next, I consider the effects of MCL and dispensaries on mortality due to heroin overdoses. These deaths are not included in the opioid mortality rates studied in Section 4.2. Starting with the 1999 to 2010 period during which heroin played a less important role than prescription opioids, I find that legalizing medical cannabis and dispensaries has a strong negative effect on heroin-related mortality. Specifically, MCL reduce mortality rates by 71% and active and legal dispensaries lower it by another 49% when I do not control for time trends, see column (1) in Table 6, and these effects are statistically significant at the 5% and 1% level, respectively. When I add linear or quadratic state-specific time trends in the regressions in columns (2) and (3), the effect of MCL is smaller, with a reduction in mortality by 46% and 53%. These effects are only significant at the 10% level. Similarly, the impact of active and legal dispensaries is reduced and the estimated coefficients lose their statistical significance when I add time trends. These results suggest that MCL mitigated the part of the opioid crisis that was related to heroin in the 1999 to 2010 period and this effect is robust to the inclusion on state trends. In contrast, dispensary status does not play a significant role for heroin overdose mortality once differential trends in this outcome variable across states are accounted for.

Heroin overdoses played a more important role after 2010, so considering the 1999 to 2013/2015 samples is particularly relevant here. The mortality-reducing effect of MCL becomes smaller in these samples compared to the 1999 to 2010 period, but still ranges between −53% and −41%, see columns (4) and (7) in Table 6, and it is significant at least at the 10% level. When I add linear or quadratic time trends, the effects become smaller in absolute value. Depending on the sample period, they range between 25% and 47% and are significant at most at the 10% level, see columns (5), (6), (8), and (9). Hence, in contrast to opioids, the mitigating effects of MCL on heroin overdose mortality are more pronounced even in the longer sample periods and more robust to including state-specific time trends. Nevertheless, the point estimates increase towards zero when I account for state trends, although the differences are not statistically significant. Active and legal dispensaries only significantly reduce mortality in the 1999 to 2015 sample, by 33%, if no time trends are included. In the other specifications, dispensary status does not have a statistically significant effect. This finding suggests that MCL play a more important role in reducing heroin-related mortality than dispensary status. Testing the null hypotheses of equal linear and quadratic time trends shows that including linear state-specific time trends is likely sufficient in the case of heroin-related mortality, see the last two rows in Table 6. Specifically, I cannot reject the null hypotheses in the specifications with quadratic time trends except for the 1999 to 2015 sample period.

Finally, I estimate the effect of MCL and dispensaries on mortality due to synthetic opioids. Since this type of drug only started to play an important role in the opioid crisis in recent years, I focus on the 1999 to 2015 sample period here. Table 7 shows the regression results. In contrast to the other drugs, the effects of MCL and active and legal dispensaries on synthetic opioid overdose mortality are mostly not significantly negative with one exception, independent of the sample period and whether I include time trends. The regression results rule out any alleviating effect of medical cannabis legalization on mortality due to synthetic opioid overdoses. Similarly to the results on heroin-related mortality, I cannot reject the null hypotheses that linear and quadratic time trends are equal across states in the specification with quadratic trends in the sample periods until 2010 and 2013. There is strong evidence, however, that linear time trends differ across states and should therefore be included in regressions of overdose mortality related to synthetic opioids.

4.4 Effects of Other Policies

While the main focus of this paper is on the impact of MCL, the analysis discussed above also serves as a suitable framework to study the effect of policies that are explicitly aimed at alleviating the opioid crisis. Here, I consider the impact of PDMP that medical providers are required to use, Naloxone Access Laws, Good Samaritan Laws, and Pill Mill Bills.[9]

Table 4 provides some evidence that required PDMD increase mortality due to opioids (including prescription and synthetic opioids and methadone) by about 32% during the 1999 to 2010 sample period (see column (1)). These effects disappear, however, when linear or quadratic time trends are added. In the same time period, required PDMP lower mortality due to heroin overdoses. This impact is large (63% to 67% reduction in mortality rates) and highly statistically significant only in the 1999 to 2010 sample period and when linear or quadratic time trends are included (see columns (2) and (3) in Table 6). When I extend the sample period to 2013, I find suggestive evidence that PDMP lower mortality related to prescription opioid overdoses by about 19%, at least in the specification that includes quadratic time trends (see column (6) in Table 5). The latter result is consistent with the findings by Meinhofer (2017). I do not find any statistically significant effects when using the 1999 to 2015 sample period. Overall, required PDMP do not have a strong and systematic impact on opioid-related mortality rates.

Naloxone Access Laws reduce opioid-related mortality rates by 22% in the 1999 to 2010 sample when no time trends are included (see column (1) in Table 4), but lead to a mortality

[9]There are several papers studying the effects of these policies, see Section 2.3, so I focus on how adding state-specific time trends affects the results.

increase by 25% when linear state-specific time trends are included (see column (2)). The effect is also positive but not statistically significant with quadratic time trends. There are similar patterns for the 1999 to 2013 sample period and for mortality related to prescription opioids (Table 5) and heroin (Table 6). Generally, Naloxone Access Laws have a sizable negative effect without controlling for state trends, but once linear state trends are included, these laws are associated with a rise in mortality rates. This increase can be substantial, for example for mortality related to heroin and synthetic opioids in the 1999 to 2013 sample (see column (5) in Table 6 and column (6) in Table 7). These findings are consistent with the existing literature on Naloxone Access Laws. Rees et al. (2017) do not include state trends and estimate negative effects whereas Doleac and Mukherjee (2018) control for linear state trends and find no effect overall but an increase in opioid mortality in the Midwest. The effects of Naloxone Access Laws on opioid- and heroin-related mortality therefore provide another case for why time trends matter when evaluating state-level policies.

The effects of Good Samaritan Laws follow a similar pattern to those of Naloxone Access Laws. In particular, these laws reduce opioid mortality by about 25% in the 1999 to 2010 sample when no time trends are added (see column (1) in Table 4), but this effect becomes positive with linear state trends (+54%, see column (2)) and loses its statistical significance with quadratic time trends (column (3)). When the sample period is extended to 2013 or 2015, Good Samaritan Laws lower opioid-related mortality in the specification with quadratic state trends (columns (6) and (9)). These effects range between 10% and 26% and are statistically significant at least at the 10% level. Hence, there is some evidence that Good Samaritan Laws can lead to a reduction in opioid-related mortality even in more recent years when differences in state-specific mortality trends are accounted for. For prescription opioid mortality in Table 5, the effects of Good Samaritan Laws are similar but less precisely estimated. In addition, these laws are associated with a substantial increase in heroin-related mortality, at least in the 1999 to 2013 sample when quadratic time trends are included (see column (6) in Table 6).

Finally, Pill Mill Bills have a large negative and statistically significant effect on mortality related to opioids overall, and prescription and synthetic opioids in the 1999 to 2010 sample when no time trends are included (see column (1) in Tables 4, 5, and 7). These effects range between −30% and −48%. In specifications with linear or quadratic trends and when the sample period is extended to 2013 or 2015, the impact of Pill Mill Bills loses its statistical significance with point estimates that are mostly positive. Hence, the conclusions about the impact of these bills are similar to those regarding MCL. They seem to have alleviated the opioid crisis during its first wave until 2010, but the mortality-reducing effect disappears in later years and when accounting for differences in opioid mortality trends across states.

5 The Role of State-Specific Time Trends

In this section, I discuss the role of time trends in DD regressions in more general terms. I also propose a simple visual test for the presence of group-specific time trends and apply it in the context of MCL and opioid-related mortality.

5.1 Theory

I start with the usual DD regression for state-year-level data that countless economic studies have employed to evaluate various state-level policies. To focus on the main points, I use a simplified version of regression (1) without the indicator variable for active and legal dispensaries and without additional covariates:

$$Y_{st} = \theta MCL_{st} + \alpha_s + \gamma_t + u_{st}. \tag{4}$$

To estimate the causal effect of MCL on opioid-related mortality, θ, the usual assumption is that

$$E[Y_{st}|s,t] = \theta MCL_{st} + \alpha_s + \gamma_t,$$

which is equivalent to $E[u_{st}|s,t] = 0$. That is, conditional on state and year, the error term in regression (4) has mean zero. This assumption is known as the common trends assumption. In this context, it implies that opioid overdose mortality rates cannot follow a different trend over time in states that did and did not implement an MCL. In other words, in the absence of the implementation of MCL the mortality trends in both groups of states would have been parallel.

If the common trends assumption holds, the causal effect θ can be estimated via the DD estimator as follows. In the two states and two time periods case, for example (see Card and Krueger, 1994), if state s_1 implemented the MCL in year t_1 whereas no MCL was effective at any time in state s_0 and in year t_0 in state s_1, θ would be given by the following expression:

$$E[Y_{s_1t_1}|s_1,t_1] - E[Y_{s_1t_0}|s_1,t_0] - (E[Y_{s_0t_1}|s_0,t_1] - E[Y_{s_0t_0}|s_0,t_0]) = \theta.$$

In practice, the data contain multiple states with and without MCL, and the laws are implemented at different time periods in each state. In this case, a regression-based DD estimator averages over all state-year comparisons.

Instead of regression (4), it is plausible that the true relationship between MCL and opioid-related mortality is given by

$$Y_{st} = \theta MCL_{st} + \alpha_s + \gamma_t + \mu_s t + \epsilon_{st}, \tag{5}$$

where $\mu_s t$ is a linear state-specific time trend and $E[\epsilon_{st}|s,t] = 0$. In contrast to regression (4), which assumes that the time effects γ_t are identical across states, regression (5) allows for systematic differences between states in how opioid mortality rates evolve over time. As a consequence, if the μ_s differ across states the common trends assumption is violated. This is of particular concern if $\mu_{s_1} \neq \mu_{s_0}$, i.e. if opioid-related mortality in states with an MCL follows a different trend than in non-MCL states. Since the opioid crisis affects some states more than others (see Section 4.1), it is likely that regression (5) is a better representation of the underlying data generating process than regression (4).

When state-specific time trends are present in the true relationship between MCL and opioid overdose mortality but one estimates regression (4), the expected value of the treatment effect, would be given by

$$E[Y_{s_1t_1}|s_1,t_1] - E[Y_{s_1t_0}|s_1,t_0] - (E[Y_{s_0t_1}|s_0,t_1] - E[Y_{s_0t_0}|s_0,t_0]) = \theta + (\mu_{s_1} - \mu_{s_0})(t_1 - t_0).$$

If the time trends in both states are identical, i.e. if the common trends assumption is satisfied, this expression reduces to θ and the DD estimator recovers the true causal effect.[10] In contrast, this is not the case if the state-specific time trends differ. For example, if the state that implemented an MCL has a lower mortality trend, i.e. $\mu_{s_1} < \mu_{s_0}$,

$$\theta + (\mu_{s_1} - \mu_{s_0})(t_1 - t_0) < \theta.$$

In other words, when estimating regression (4) although the true relationship between MCL and mortality rates is given by equation (5) and opioid-related mortality in states that implemented MCL grows more slowly, the estimated coefficient of interest is downward biased. This constitutes a form of selection bias in the sense that states that implemented an MCL are selected based on lower pre-existing mortality trends.

[10]Note that the DD regression (@eq:didtrend) is not identified in the two-period case. The data in the application span at least 12 years, so this is not a practical concern in this case.

Put differently, if regression (4) reflects the true relationship between MCL and opioid mortality rates, the error term in regression (4) has mean zero conditional on state and year: $E[u_{st}|s,t] = 0$. In contrast, if regression (5) reflects the true relationship and one estimates regression (4), the error term has a conditional mean different from zero:

$$E[u_{st}|s,t] = E[\epsilon_{st} + \mu_s t|s,t] = E[Y_{st} - \theta MCL_{st} - \alpha_s - \gamma_t] = \mu_s t.$$

I use the fact that the error terms contain information on whether regression (4) or (5) denotes the true relationship between MCL and opioid mortality rates to implement an informal and visual specification test as follows. After estimating regression (4), the residuals

$$\hat{u}^n_{st} = Y_{st} - \hat{\alpha}^n_s - \hat{\gamma}^n_t \tag{6}$$

should not exhibit a differential trend for any state regardless of the states' MCL status if the assumption $E[u_{st}|s,t] = 0$ is correct. Specifically, the residuals for MCL-states before they implemented the law should not trend differently from states that never had an MCL over the same time period. On the contrary, if regression (5) reflects the true relationship, the residuals may exhibit different trends. Moreover, MCL-states would show a lower trend than non-MCL-states if $\mu_{s_1} < \mu_{s_0}$ on average, where s_1 indexes states that implemented an MCL during the sample period and s_0 those that did not. Note that the residuals in equation (6) do not include the term $\hat{\theta} MCL_{st}$ because I am interested in the residual trends in the absence of the treatment.

To assess the common trends assumption and to determine whether regression (4) or (5) more likely represents the true relationship between MCL and opioid mortality, I plot the residuals in equation (6) over time and separately for states that have not legalized medical cannabis, those that have implemented an MCL, and those with active and legal dispensaries at some point during the sample period. I then assess whether the residuals that correspond to state-year observations without an MCL or active and legal dispensaries between the three categories of states. In other words, I check whether pre-existing trends in opioid mortality residuals differ by medical cannabis's legal status. If the residuals do not show different trends, regression (4) is likely an accurate representation of the true relationship. However, if the residuals for MCL-states or states with active and legal dispensaries exhibit different pre-trends from states that did not legalize medical cannabis, the common trends assumption is possibly violated. When I include linear and quadratic state-specific time trends in the regression and plot the resulting residuals

$$\hat{u}^l_{st} = Y_{st} - \hat{\alpha}^l_s - \hat{\gamma}^l_t - \hat{\mu}^l_s t, \tag{7}$$

and

$$\hat{u}^q_{st} = Y_{st} - \hat{\alpha}^q_s - \hat{\gamma}^q_t - \hat{\mu}^q_s t - \hat{\nu}^q_s t^2, \tag{8}$$

a previously visible trend should disappear if regression (5) or a regression that also contains quadratic state-specific time trends represent the true relationship between MCL and mortality. A visual inspection of the residuals from regressions with and without (linear or quadratic) state-specific time trends can therefore be informative about (i) the potential selection bias in the regression without trends and (ii) whether adding state trends reduces this bias.

5.2 Application

Next, I illustrate the use of the residual plots in the context of the regressions discussed in Sections 4.2 and 4.3. For opioids overall, Figures 2a, 2d, and 2g plot the residuals in equation (6), Figures 2b, 2e, and 2h plot the residuals in equation (7), and Figures 2c, 2f, and 2i plot the residuals in equation (8) separately for states that never legalized medical cannabis by the end of the respective sample period, those that implemented an MCL but never had active and legal dispensaries, and finally the states that had active and legal dispensaries during the sample period. I distinguish between residuals that correspond to state-year observations without an MCL or active and legal dispensaries (denoted by $\circ$) and those when either an MCL was in place or a state had active and legal dispensaries (denoted by $+$). To aid the comparison, I indicate the best linear fit for the residuals without MCL or active and legal dispensaries since they correspond to the relevant mortality pre-trends.

I plot the residuals from regressions of opioid-related mortality rates (see Table 4) for the 1999 to 2010 sample period in the top row of Figure 2. While the residuals for non-MCL states and for states with an MCL but without dispensaries are centered around zero and do not exhibit a substantial trend, the residuals for the two states that had active and legal dispensaries by 2010 (California and New Mexico) show a downward sloping trend when no time trends are added in the underlying regression, see Figure 2a. That is, in the notation of Section 5.1, $\mu_1 < \mu_0$.[11] When linear or quadratic time trends are added, the residuals cease to exhibit a trend and are instead centered around zero, as shown in Figures 2b and 2c.

[11] Clearly, basing this conclusion on two states is a stretch, but the result is the same when expanding the sample period to 2013 and 2015, thereby adding more states with active and legal dispensaries (see below).

These residual plots highlight the importance of accounting for potential differences in pre-existing opioid-mortality trends across states when estimating the impact of MCL and dispensary status (and possibly other policies). In this case, states that allowed medical cannabis dispensaries to operate exhibited a declining trend in opioid-related mortality compared to states without active and legal dispensaries (conditional on state and year fixed effects and time-varying covariates). Therefore, not controlling for state-specific time trends biases the estimated effect of dispensary status, in this case downward. Once these differential trends are accounted for via linear or quadratic state-specific time trends, the bias vanishes as indicated by the horizontal fit lines in Figures 2b and 2c. The regression results in columns (1) to (3) in Table 4 also reflect these findings since the effects of MCL and active and legal dispensaries become larger (albeit statistically insignificant) when state trends are added. The residual plots in Figure 2 therefore provide a simple visual tool to assess differences in outcome trends across groups and to check that these differences disappear when group-specific trends are included in the regression.

The residual plots in Figures 2e and 2g show that the residuals from opioid-related mortality regressions with state-specific time trends also exhibit a downward trend for states with active and legal dispensaries when the sample period is extended to 2013 or 2015. In contrast, states with an MCL that did not have active and legal dispensaries exhibit an upward trend in opioid mortality residuals in the 1999 to 2015 sample. Once linear or quadratic state trends are included in Figures 2e, 2f, 2h, and 2i, however, these trends mostly disappear and the residuals are centered around zero for all years. Although the estimated MCL and dispensary status coefficients are not statistically significant in columns (4) to (9) of Table 4 with only one exception, the differences in point estimates are nevertheless consistent with the corresponding residual plots. In particular, the downward trend of residuals in states with active and legal dispensaries points to a downward bias in the regression coefficients in columns (4) and (7), i.e. in the regressions that do not include time trends. Indeed, the point estimates for both the dispensary status and the MCL effects are smaller in these two regressions than in the regressions in columns (5) or (6) and (8) or (9), respectively, although none of the latter is statistically significant. Once this bias is removed, the impact of MCL and active and legal dispensaries on opioid-related mortality increases towards zero or becomes positive although the effects are too imprecisely measured to allow any definitive conclusions.

For regressions of prescription opioid mortality, Figure 3a, which plots the residuals from regressions without time trends for the 1999 to 2010 sample, shows a downward trend among states that allowed dispensaries to operate by 2010, thereby pointing to possible downward bias for the dispensary status coefficient. The downward trend in the mortality residuals disappears when I add linear or quadratic state-specific time trends, see Figures 3b and 3c. The residual plots in the bottom two rows of Figure 3 confirm the notion that the MCL and dispensary effects in the regressions in columns (4) and (7) of Table 5 are downward biased since prescription opioid mortality residuals exhibit a downward trend in states with active and legal dispensaries, see Figures 3d and 3g. In contrast, when I add linear or quadratic state trends, the residuals have no trend and are centered around zero, see Figures 3e, 3f, 3h, and 3i.

For regressions of heroin-related mortality, the residual plots in the top row of Figure 4 also indicate that the estimated MCL and dispensary effects may be subject to downward bias if state trends are not included. Especially, the residuals for states with an MCL but without active and legal dispensaries exhibit a strong downward sloping trend in Figure 4a, indicating that the MCL coefficient in column (1) of Table 6 overstates (in absolute value) the true mitigating effect of MCL on heroin overdose mortality. The fact that the residuals' downward trend disappears with linear or quadratic state trends, see Figures 4b and 4c, suggests that the coefficients in columns (2) and (3) are closer to the true effect.

The residual plots for heroin are less conclusive than for opioids. Without state trends, residuals for states with an MCL but without active and legal dispensaries exhibit a downward or upward trend depending on the sample period, see Figures 4d and 4g. Moreover, the residuals for states with and MCL but without active and legal dispensaries trend upward in the 1999 to 2015 sample. When linear or quadratic time trends are added, the trends do not disappear entirely but become smaller and more similar across groups of states. Comparing Figures 4g, 4h, and 4i shows that including linear or quadratic state-specific time trends reduces differences in residual trends across treatment groups and thereby potential lowers the bias in the corresponding treatment effect estimates.

Figure 5 shows that the residuals from regressions of synthetic opioid mortality also follow slightly different patterns than for the other types of opioids. When no time trends are added, they follow an upward trend in states with MCL but without active and legal dispensaries whereas the trend is downward sloping in states with active and legal dispensaries, see Figures 5a and 5d. With linear or quadratic state trends, the residuals' trend becomes completely flat only for states without MCL. In the case of synthetic opioids, it is not clear from these residual plots how omitting state-specific time trends biases the MCL and active and legal dispensaries coefficients. However, most estimates for the effects of MCL and dispensary status in Table 7 are not statistically significant.

Overall, the residual plots in Figures 2 to 5 demonstrate how a visual inspection of residual trends can inform researchers about which of the underlying regression results is likely biased due to a violation of the common trends assumption.

6 Conclusion

I estimate the impact of MCL and active and legal medical cannabis dispensaries and find that it is sensitive to the inclusion of state-specific time trends. This finding implies that the common trends assumption that is necessary for DD regressions to deliver unbiased estimates is violated. The estimated effects also vary with the sample period. As more recent years are added, the impact of MCL generally diminishes, suggesting that they may have worked in reducing opioid-related mortality in the early years of the opioid crisis but to a lesser extent as the crisis has become more severe in recent years. Hence, MCL may not be as successful in alleviating the opioid crisis as previously found. While medical cannabis legalization may not achieve a reduction in opioid-related mortality on the state level, Garin, Pohl, and Smith (2018) find that counties where a dispensary operates experience significantly lower mortality rates compared to counties where medical cannabis is legal but no dispensary exists. In addition, my results imply that other policies intended to mitigate the opioid crisis are mostly ineffective.

These findings highlight the importance of carefully modeling group-specific time effects in DD analyses. The common assumption of equal time trends across groups that underlies the use of time fixed effects is likely violated in many settings and at a minimum should be tested, for example by using residual plots as a simple visual tool.

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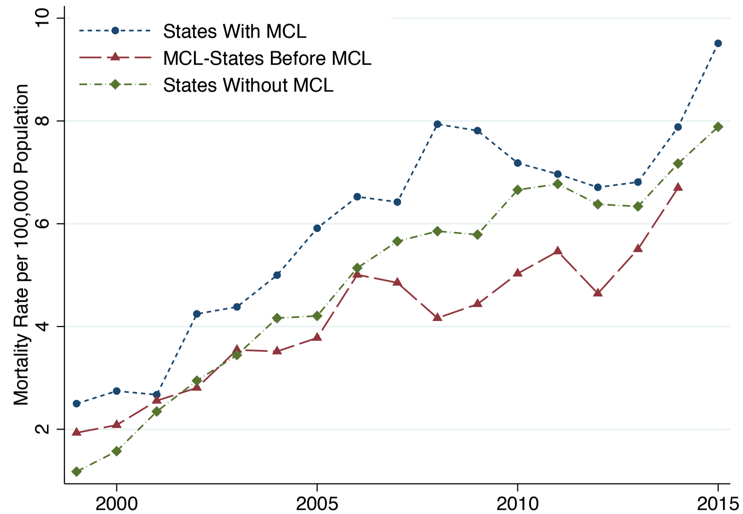
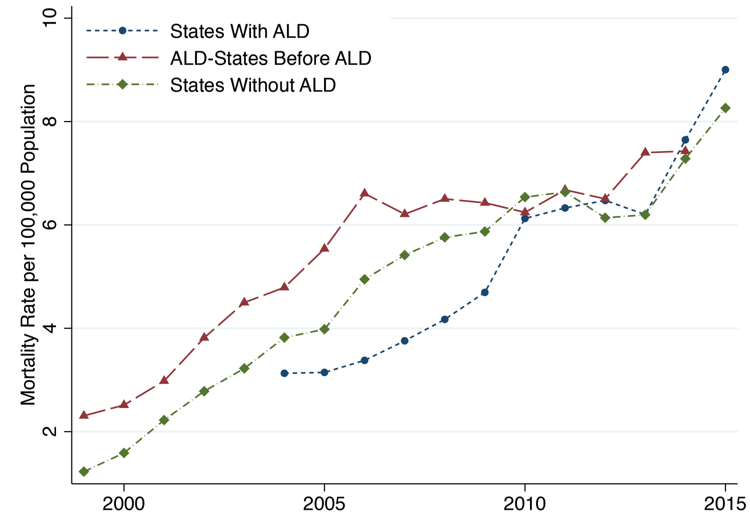
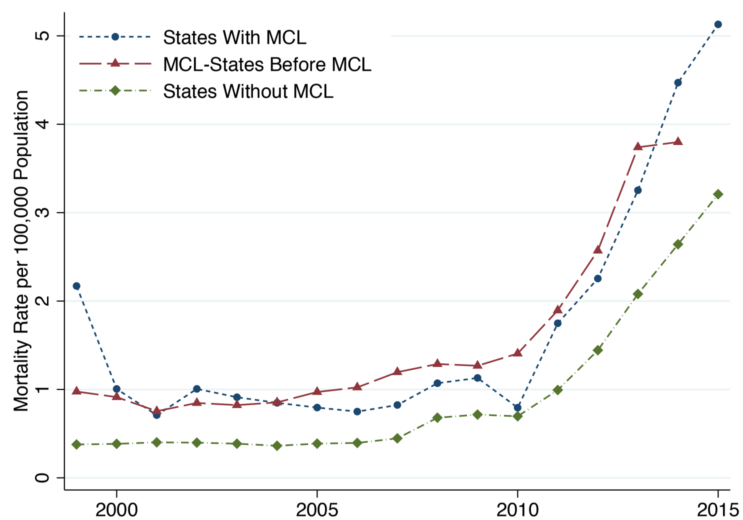


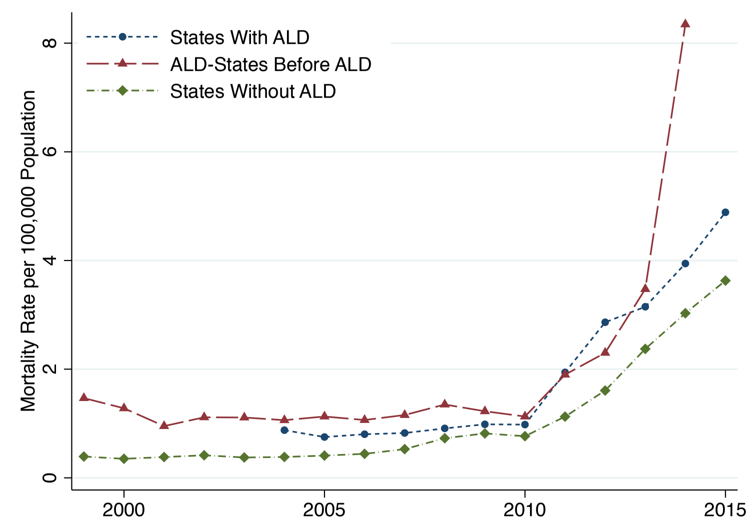
(a) Opioids, by MCL Status



(b) Opioids, by ALD Status



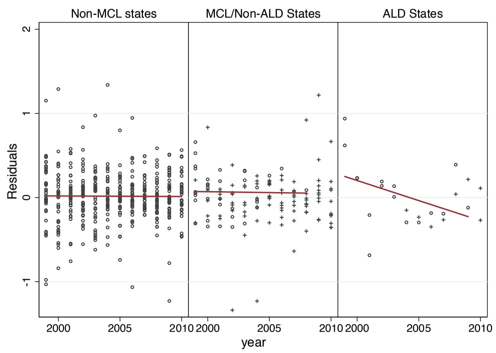
(c) Heroin, by MCL Status



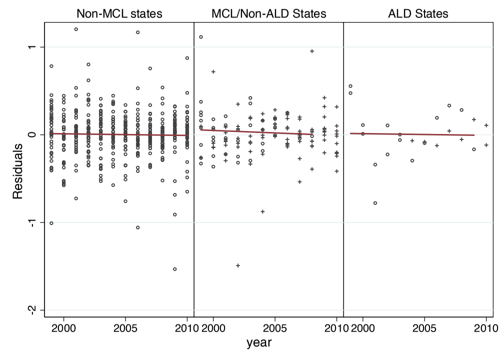
(d) Heroin, by ALD Status

Notes: Opioids include prescription opioids, methadone, and synthetic opioids. Mean mortality rates are shown for states that had a contemporaneous medical cannabis law (MCL) or active and legal dispensaries (ALD), states that implemented an MCL or ALD at a later point in time ("MCL-States Before MCL"), and states that never had an MCL or ALD by 2015.

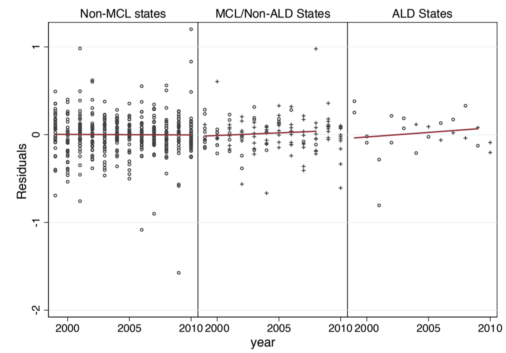
Figure 1: Mortality Rates for Opioids and Heroin by MCL and ALD Status



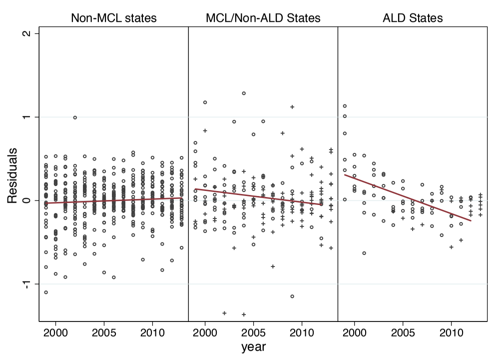
(a) No Time Trends, 1999 to 2010



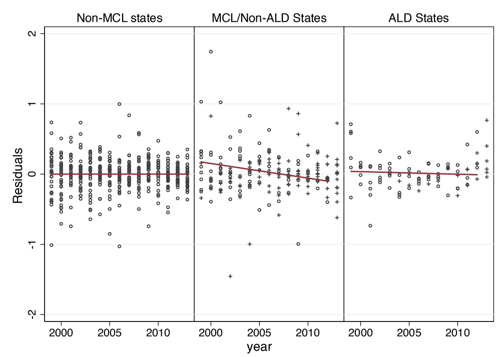
(b) Linear Trends, 1999 to 2010



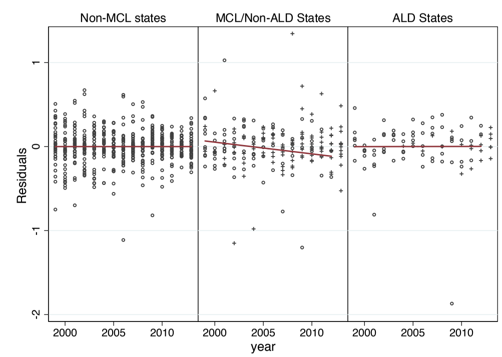
(c) Quadratic Trends, 1999 to 2010



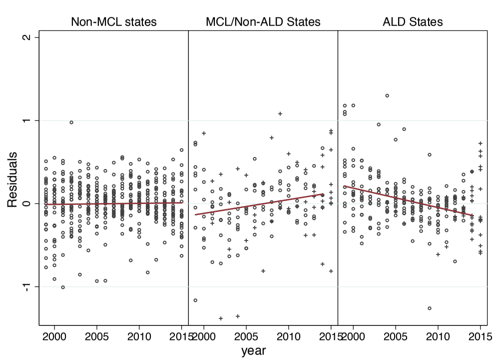
(d) No Time Trends, 1999 to 2013



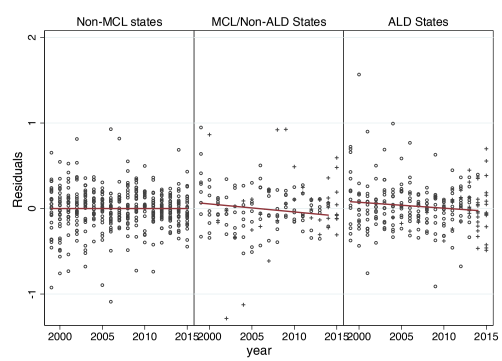
(e) Linear Trends, 1999 to 2013



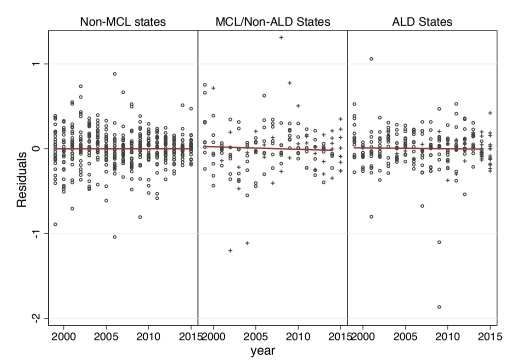
(f) Quadratic Trends, 1999 to 2013



(g) No Time Trends, 1999 to 2015



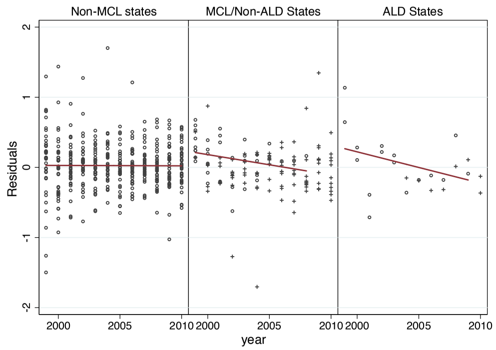
(h) Linear Trends, 1999 to 2015



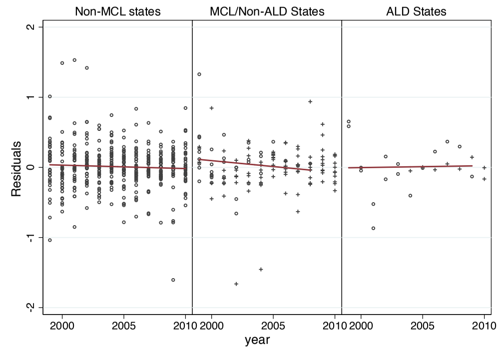
(i) Quadratic Trends, 1999 to 2015

Notes: Opioids include prescription opioids, methadone, and synthetic opioids. Residuals stem from regressions of log-mortality rates and are plotted by whether states had a medical cannabis law (MCL) and active and legal dispensaries (ALD) at any time during the indicated sample periods. Residuals without and with linear and quadratic state-specific time trends are defined in equations (6), (7), and (8) in the text. ○ denotes residuals when no MCL/ALD was in place and + denotes residuals with MCL/ALD. The lines indicate the best linear fit for no-MCL/ALD residuals.

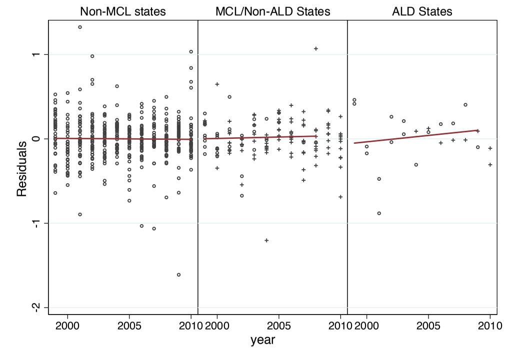
Figure 2: Residual Plots for Opioids



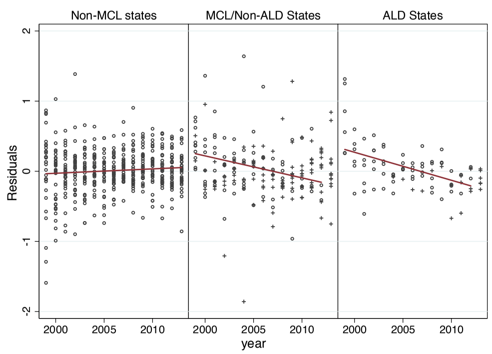
(a) No Time Trends, 1999 to 2010



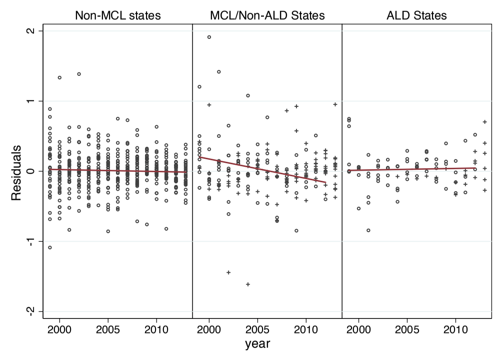
(b) Linear Trends, 1999 to 2010



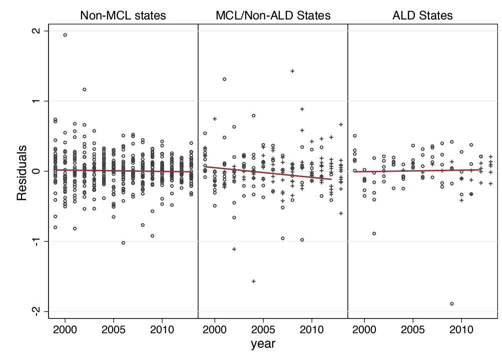
(c) Quadratic Trends, 1999 to 2010



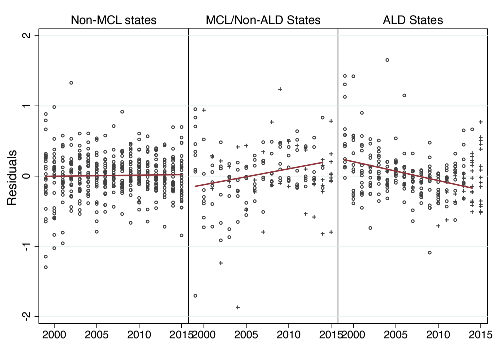
(d) No Time Trends, 1999 to 2013



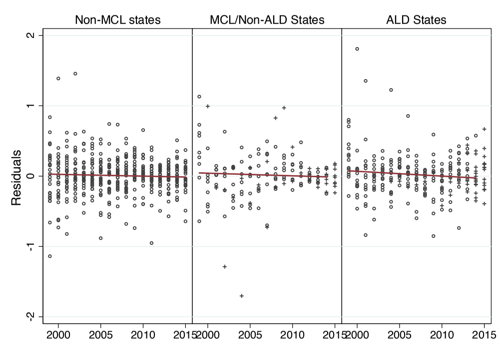
(e) Linear Trends, 1999 to 2013



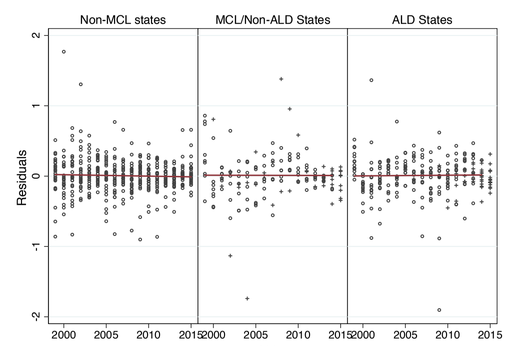
(f) Quadratic Trends, 1999 to 2013



(g) No Time Trends, 1999 to 2015



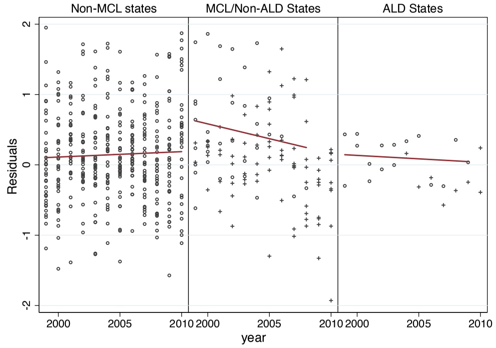
(h) Linear Trends, 1999 to 2015



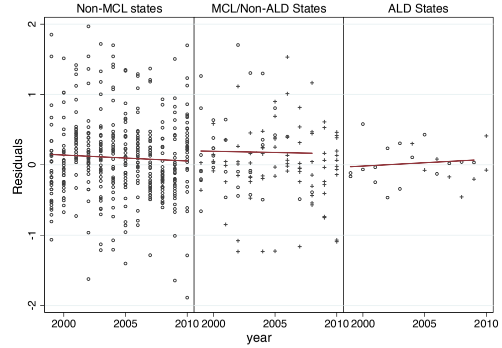
(i) Quadratic Trends, 1999 to 2015

Notes: Residuals stem from regressions of log-mortality rates and are plotted by whether states had a medical cannabis law (MCL) and active and legal dispensaries (ALD) at any time during the indicated sample periods. Residuals without and with linear and quadratic state-specific time trends are defined in equations (6), (7), and (8) in the text. ○ denotes residuals when no MCL/ALD was in place and + denotes residuals with MCL/ALD. The lines indicate the best linear fit for no-MCL/ALD residuals.

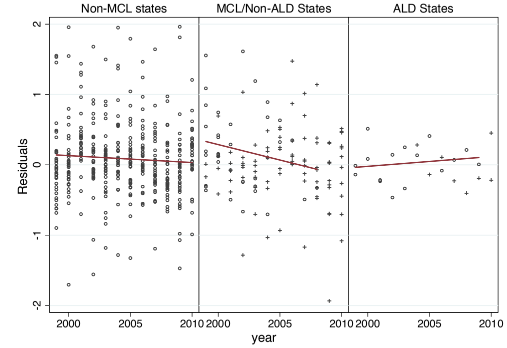
Figure 3: Residual Plots for Prescription Opioids



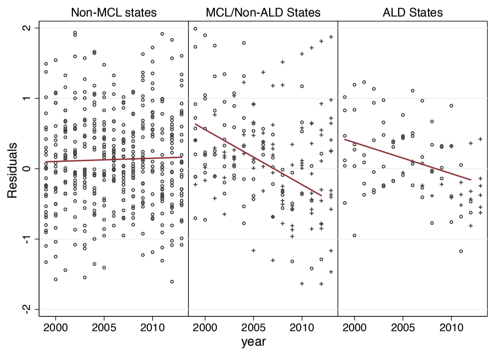
(a) No Time Trends, 1999 to 2010



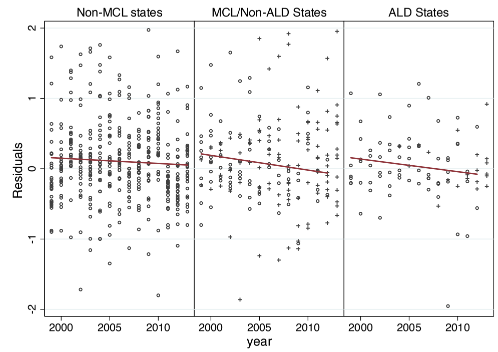
(b) Linear Trends, 1999 to 2010



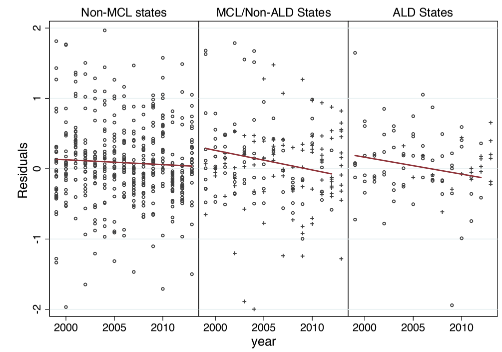
(c) Quadratic Trends, 1999 to 2010



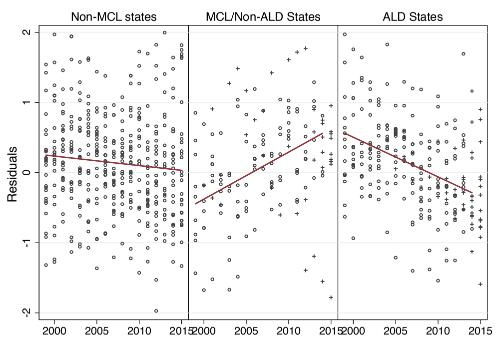
(d) No Time Trends, 1999 to 2013



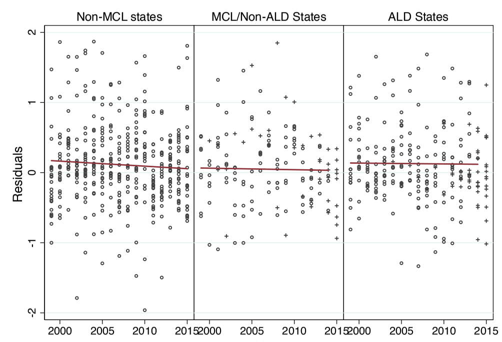
(e) Linear Trends, 1999 to 2013



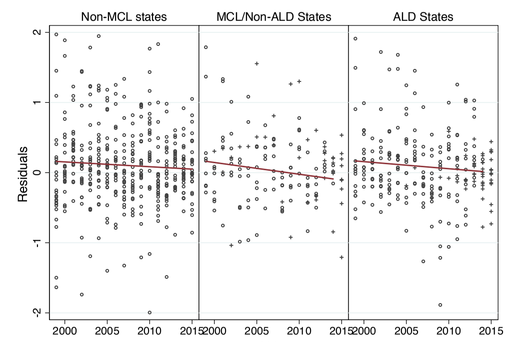
(f) Quadratic Trends, 1999 to 2013



(g) No Time Trends, 1999 to 2015



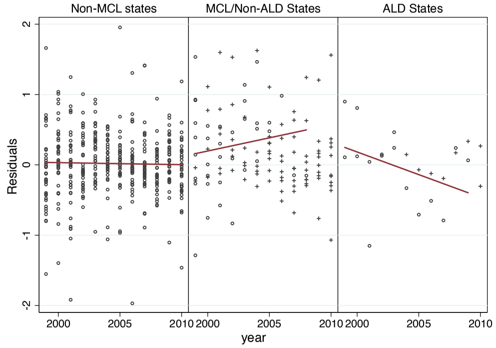
(h) Linear Trends, 1999 to 2015



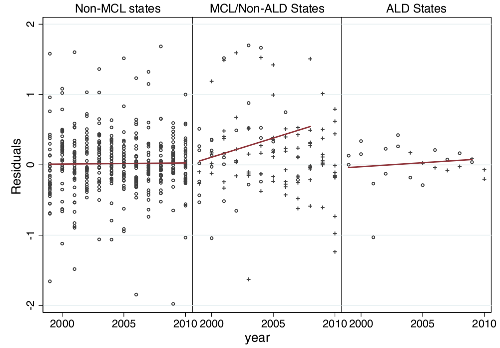
(i) Quadratic Trends, 1999 to 2015

Notes: Residuals stem from regressions of log-mortality rates and are plotted by whether states had a medical cannabis law (MCL) and active and legal dispensaries (ALD) at any time during the indicated sample periods. Residuals without and with linear and quadratic state-specific time trends are defined in equations (6), (7), and (8) in the text. ○ denotes residuals when no MCL/ALD was in place and + denotes residuals with MCL/ALD. The lines indicate the best linear fit for no-MCL/ALD residuals.

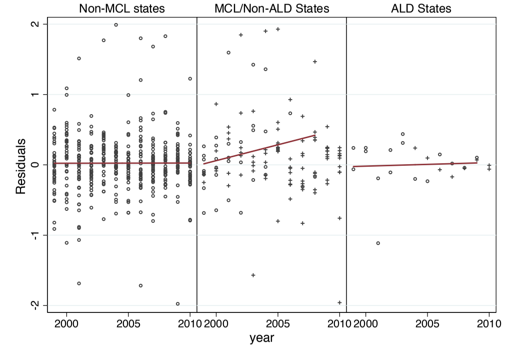
Figure 4: Residual Plots for Heroin



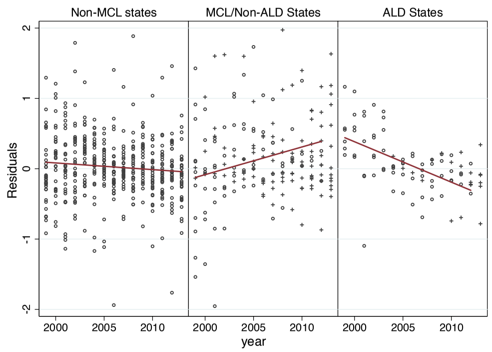
(a) No Time Trends, 1999 to 2010



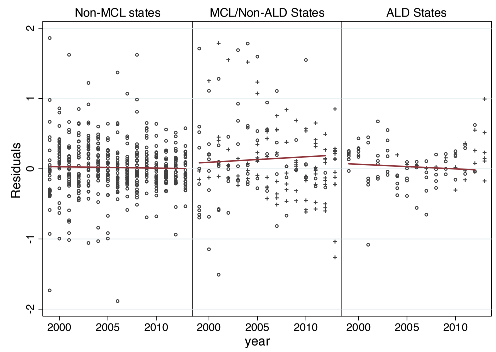
(b) Linear Trends, 1999 to 2010



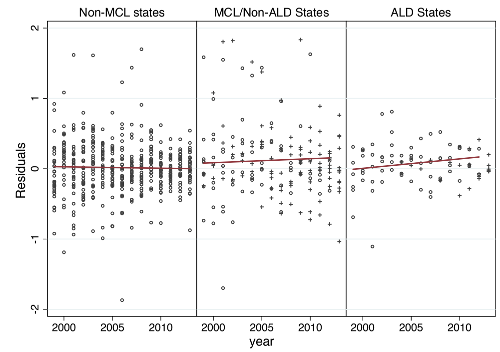
(c) Quadratic Trends, 1999 to 2010



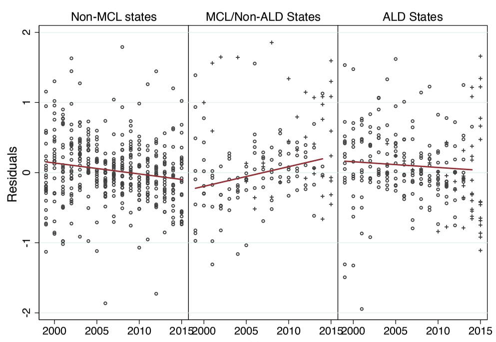
(d) No Time Trends, 1999 to 2013



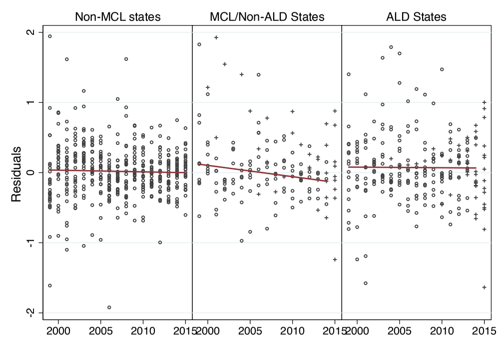
(e) Linear Trends, 1999 to 2013



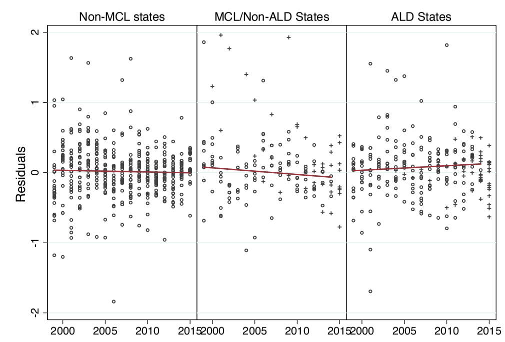
(f) Quadratic Trends, 1999 to 2013



(g) No Time Trends, 1999 to 2015



(h) Linear Trends, 1999 to 2015



(i) Quadratic Trends, 1999 to 2015

Notes: Residuals stem from regressions of log-mortality rates and are plotted by whether states had a medical cannabis law (MCL) and active and legal dispensaries (ALD) at any time during the indicated sample periods. Residuals without and with linear and quadratic state-specific time trends are defined in equations (6), (7), and (8) in the text. ○ denotes residuals when no MCL/ALD was in place and + denotes residuals with MCL/ALD. The lines indicate the best linear fit for no-MCL/ALD residuals.

Figure 5: Residual Plots for Synthetic Opioids

Table 1: Summary Statistics by Medical Cannabis Legalization Status

	(1) No MCL/ALD	(2) MCL	(3) ALD
Opioid Mortality Rate	4.531	6.568	7.294
	(3.813)	(3.654)	(3.831)
Prescription Opioid Mortality Rate	2.835	3.818	4.724
	(2.879)	(2.260)	(2.385)
Heroin Mortality Rate	1.019	1.800	3.320
	(1.414)	(1.944)	(2.460)
Synthetic Opioid Mortality Rate	0.938	1.332	2.083
	(1.053)	(2.313)	(2.494)
Required PDMP	0.0287	0.0641	0.0400
	(0.167)	(0.246)	(0.198)
Naloxone Access Law	0.0514	0.109	0.620
	(0.221)	(0.313)	(0.490)
Good Samaritan Law	0.0227	0.103	0.420
	(0.149)	(0.304)	(0.499)
Pill Mill Bill	0.0560	0	0
	(0.230)	(0)	(0)
Beer Tax Rate	0.253	0.342	0.207
	(0.205)	(0.329)	(0.106)
Unemployment Rate	5.521	6.542	6.796
	(1.879)	(2.300)	(1.852)
Fraction Male	0.491	0.498	0.495
	(0.00695)	(0.0105)	(0.00642)
Fraction White	0.821	0.789	0.822
	(0.119)	(0.203)	(0.104)
Fraction Aged 15 to 19	0.0719	0.0696	0.0670
	(0.00529)	(0.00539)	(0.00518)
Fraction Aged 20 to 64	0.594	0.609	0.602
	(0.0157)	(0.0145)	(0.0194)
Fraction Aged 65 and Over	0.132	0.128	0.142
	(0.0170)	(0.0253)	(0.0229)
Observations	661	156	50

Notes: The table shows means and standard deviations (in parentheses). Mortality Rates are per 100,000 population. Opioids (in the first row) include prescription opioids, methadone, and synthetic opioids. "No MCL/ALD" refers to state-year observations without any medical cannabis legalization and "MCL" and "ALD" refers to observations with a medical cannabis law and active and legal dispensaries, respectively.

Table 2: Change in Opioid and Heroin Overdose Mortality Rates Since 1999 for States With and Without Medical Cannbis Laws (MCL), by Year of MCL Introduction

Year t	States With New MCL	Change in Mortality Between 1999 and $t-1$	
		New-MCL States	Never-MCL States
2000	AK, ME	0%	0%
2001	CO, HI	−1%	49%
2002	NV	14%	116%
2005	MT, VT	156%	254%
2006	RI	230%	253%
2008	NM	32%	393%
2009	MI	366%	474%
2011	AZ, DC, NJ	44%	492%
2012	DE	207%	483%
2013	CT, MA	58%	491%
2014	IL, NH	182%	524%
2015	MD, MN, NY	187%	673%

Notes: Mortality rates include deaths related to all opioids and heroin. Percentage changes are calculated between 1999 and the year before the year indicated in the first column. "New-MCL States" are the states given in the second column that had an MCL for a full year for the first time in the given year. "Never-MCL States" are all remaining states: AL, AR, FL, GA, ID, IN, IA, KS, KY, LA, MS, MO, NE, NC, ND, OH, OK, PA, SC, SD, TN, TX, UT, VA, WV, WI, and WY. CA, OR, and WA introduced an MCL before 2000.

Table 3: Change in Opioid and Heroin Overdose Mortality Rates Since 1999 for States With and Without Active and Legal Dispensaries (ALD), by Year of First ALD

Year t	States With New ALD	Change in Mortality Between 1999 and $t-1$	
		New-ALD States	Never-ALD States
2004	CA	−9%	145%
2010	NM	1%	364%
2011	CO	61%	397%
2012	ME	183%	397%
2013	AZ, NJ	72%	401%
2014	DC, MI, MT, NV, OR, RI, VT, WA	233%	432%
2015	CT	217%	557%

Notes: Mortality rates include deaths related to all opioids and heroin. Percentage changes are calculated between 1999 and the year before the year indicated in the first column. "New-ALD States" are the states given in the second column that had active and legal medical cannabis dispensaries for a full year for the first time in the given year. "Never-ALD States" are all remaining states: AL, AK, AR, DE, FL, GA, HI, ID, IL, IN, IA, KS, KY, LA, MD, MA, MN, MS, MO, NE, NH, NY, NC, ND, OH, OK, PA, SC, SD, TN, TX, UT, VA, WV, WI, and WY.

Table 4: Difference-in-Difference Regressions for Opioid Overdose Mortality Rates

	1999 to 2010			1999 to 2013			1999 to 2015		
	(1) No Trend	(2) Lin. Trend	(3) Quad. Trend	(4) No Trend	(5) Lin. Trend	(6) Quad. Trend	(7) No Trend	(8) Lin. Trend	(9) Quad. Trend
Medical Cannabis Law	−0.186*	−0.221	−0.00397	−0.111	−0.0137	0.153	0.0274	0.0845	0.0907
	(0.105)	(0.148)	(0.0590)	(0.0778)	(0.157)	(0.178)	(0.0876)	(0.133)	(0.121)
Active & Legal Disp.	−0.300***	−0.0813	−0.0837	−0.228***	0.165	−0.0174	−0.168*	−0.0118	−0.0380
	(0.0654)	(0.121)	(0.253)	(0.0803)	(0.143)	(0.160)	(0.0989)	(0.160)	(0.0835)
Required PDMP	0.142	0.387**	0.0696	0.0548	0.0799	−0.0992	0.119	0.0211	−0.0397
	(0.141)	(0.145)	(0.0829)	(0.0755)	(0.0974)	(0.0935)	(0.0836)	(0.0914)	(0.127)
Naloxone Access Law	−0.246	0.222*	0.113	−0.256***	0.0761	0.194	−0.0390	−0.00942	0.109
	(0.153)	(0.118)	(0.141)	(0.0918)	(0.0901)	(0.123)	(0.0784)	(0.0804)	(0.0774)
Good Samaritan Law	−0.290**	0.433*	−0.138	0.0543	0.161*	−0.308**	0.0820	0.0960	−0.143*
	(0.133)	(0.216)	(0.183)	(0.105)	(0.0922)	(0.146)	(0.0973)	(0.0671)	(0.0799)
Pill Mill Bill	−0.365***	0.726	0.396	−0.167	0.0253	0.0834	−0.116	−0.0395	0.0996
	(0.0825)	(0.658)	(0.424)	(0.113)	(0.135)	(0.135)	(0.123)	(0.100)	(0.104)
Time-Varying Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear State Trends	No	Yes	No	No	Yes	No	No	Yes	No
Quadratic State Trends	No	No	Yes	No	No	Yes	No	No	Yes
Within-R^2	0.668	0.768	0.833	0.684	0.763	0.816	0.691	0.772	0.829
Observations	612	612	612	765	765	765	867	867	867
p: MCL and ALD	0.000	0.200	0.942	0.012	0.466	0.692	0.242	0.770	0.649
p: Linear Trend		0.000	0.000		0.000	0.000		0.000	0.000
p: Quadratic Trend			0.000			0.000			0.000

Notes: The dependent variable is measured in log(deaths per 100,000 population). Covariates include the unemployment rate, the beer tax rate, fraction male, fraction white, and fractions aged 15 to 19, 20 to 64, and 65 and over. Standard errors clustered by state in parentheses. "p: MCL and ALD" is the p-value for the hypothesis $H_0 : \theta_1^n = \theta_2^n = 0$. "$p$: Linear Trend" and "$p$: Quadratic Trend" are the p-values for the hypotheses $H_0 : \mu_s = \mu_{s'}, \forall s, s'$ and $H_0 : \nu_s = \nu_{s'}, \forall s, s'$, respectively. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Difference-in-Difference Regressions for Prescription Opioid Overdose Mortality Rates

	1999 to 2010			1999 to 2013			1999 to 2015		
	(1) No Trend	(2) Lin. Trend	(3) Quad. Trend	(4) No Trend	(5) Lin. Trend	(6) Quad. Trend	(7) No Trend	(8) Lin. Trend	(9) Quad. Trend
Medical Cannabis Law	−0.373***	−0.373**	−0.0921	−0.252**	−0.0591	0.111	−0.148	−0.0266	0.0331
	(0.119)	(0.175)	(0.126)	(0.0996)	(0.166)	(0.193)	(0.104)	(0.136)	(0.123)
Active & Legal Disp.	−0.464***	−0.0509	−0.180	−0.314***	0.0721	−0.0985	−0.160	−0.00482	−0.146
	(0.0788)	(0.176)	(0.351)	(0.112)	(0.141)	(0.185)	(0.108)	(0.131)	(0.102)
Required PDMP	0.141	0.119	−0.0965	0.0615	0.0447	−0.217*	0.170*	−0.0390	−0.149
	(0.143)	(0.146)	(0.0788)	(0.0850)	(0.0993)	(0.124)	(0.0939)	(0.0953)	(0.126)
Naloxone Access Law	−0.360**	0.156	0.0271	−0.357***	0.0712	0.0977	−0.0964	−0.00825	0.00748
	(0.136)	(0.0987)	(0.163)	(0.0978)	(0.110)	(0.143)	(0.0872)	(0.0847)	(0.0893)
Good Samaritan Law	−0.158	0.606*	−0.225	0.103	0.137	−0.308*	0.0932	0.136*	−0.135
	(0.147)	(0.313)	(0.260)	(0.117)	(0.121)	(0.167)	(0.107)	(0.0807)	(0.0832)
Pill Mill Bill	−0.462***	0.627	0.423	−0.227*	−0.0176	0.0633	−0.183	−0.0860	0.0963
	(0.0926)	(0.657)	(0.414)	(0.132)	(0.160)	(0.156)	(0.144)	(0.130)	(0.138)
Time-Varying Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear State Trends	No	Yes	No	No	Yes	No	No	Yes	No
Quadratic State Trends	No	No	Yes	No	No	Yes	No	No	Yes
Within-R^2	0.616	0.727	0.797	0.662	0.747	0.802	0.671	0.764	0.811
Observations	612	612	612	765	765	765	867	867	867
p: MCL and ALD	0.000	0.088	0.672	0.001	0.772	0.781	0.124	0.978	0.327
p: Linear Trend		0.000	0.000		0.000	0.000		0.000	0.000
p: Quadratic Trend			0.000			0.000			0.000

Notes: The dependent variable is measured in log(deaths per 100,000 population). Covariates include the unemployment rate, the beer tax rate, fraction male, fraction white, and fractions aged 15 to 19, 20 to 64, and 65 and over. Standard errors clustered by state in parentheses. "p: MCL and ALD" is the p-value for the hypothesis $H_0 : \theta_1^n = \theta_2^n = 0$. "$p$: Linear Trend" and "$p$: Quadratic Trend" are the p-values for the hypotheses $H_0 : \mu_s = \mu_{s'}, \forall s, s'$ and $H_0 : \nu_s = \nu_{s'}, \forall s, s'$, respectively. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Difference-in-Difference Regressions for Heroin Overdose Mortality Rates

	1999 to 2010			1999 to 2013			1999 to 2015		
	(1) No Trend	(2) Lin. Trend	(3) Quad. Trend	(4) No Trend	(5) Lin. Trend	(6) Quad. Trend	(7) No Trend	(8) Lin. Trend	(9) Quad. Trend
Medical Cannabis Law	−1.255**	−0.609*	−0.752*	−0.762**	−0.313	−0.597*	−0.517*	−0.367*	−0.296
	(0.514)	(0.315)	(0.448)	(0.354)	(0.245)	(0.336)	(0.296)	(0.205)	(0.247)
Active & Legal Disp.	−0.667***	−0.123	−0.311	−0.343	0.141	0.303	−0.412*	−0.0862	−0.206
	(0.191)	(0.255)	(0.381)	(0.235)	(0.240)	(0.329)	(0.216)	(0.213)	(0.197)
Required PDMP	−0.641	−1.117***	−0.998***	0.0365	−0.0833	−0.262	0.134	−0.244	−0.289
	(0.406)	(0.344)	(0.242)	(0.325)	(0.296)	(0.312)	(0.296)	(0.254)	(0.216)
Naloxone Access Law	−0.614*	0.176	0.0451	−0.151	0.778**	0.573	−0.130	0.0499	0.127
	(0.311)	(0.326)	(0.369)	(0.312)	(0.368)	(0.414)	(0.207)	(0.161)	(0.257)
Good Samaritan Law	0.965	1.226*	1.011	0.0611	0.205	0.656**	0.0480	0.203	0.0364
	(0.680)	(0.616)	(0.636)	(0.248)	(0.184)	(0.316)	(0.187)	(0.150)	(0.163)
Pill Mill Bill	0.526	0.310	2.178	0.280	0.307	0.627	0.284	0.399	0.307
	(0.342)	(0.416)	(1.519)	(0.413)	(0.318)	(0.551)	(0.442)	(0.316)	(0.363)
Time-Varying Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear State Trends	No	Yes	No	No	Yes	No	No	Yes	No
Quadratic State Trends	No	No	Yes	No	No	Yes	No	No	Yes
Within-R^2	0.146	0.348	0.426	0.304	0.468	0.511	0.438	0.577	0.612
Observations	612	612	612	765	765	765	867	867	867
p: MCL and ALD	0.002	0.086	0.153	0.015	0.320	0.213	0.023	0.205	0.374
p: Linear Trend		0.000	0.970		0.000	0.277		0.000	0.015
p: Quadratic Trend			0.632			0.532			0.193

Notes: The dependent variable is measured in log(deaths per 100,000 population). Covariates include the unemployment rate, the beer tax rate, fraction male, fraction white, and fractions aged 15 to 19, 20 to 64, and 65 and over. Standard errors clustered by state in parentheses. "p: MCL and ALD" is the p-value for the hypothesis $H_0 : \theta_1^n = \theta_2^n = 0$. "$p$: Linear Trend" and "$p$: Quadratic Trend" are the p-values for the hypotheses $H_0 : \mu_s = \mu_{s'}, \forall s, s'$ and $H_0 : \nu_s = \nu_{s'}, \forall s, s'$, respectively. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Difference-in-Difference Regressions for Synthetic Opioid Overdose Mortality Rates

	1999 to 2010			1999 to 2013			1999 to 2015		
	(1) No Trend	(2) Lin. Trend	(3) Quad. Trend	(4) No Trend	(5) Lin. Trend	(6) Quad. Trend	(7) No Trend	(8) Lin. Trend	(9) Quad. Trend
Medical Cannabis Law	−0.0101	−0.595*	−0.0322	0.0702	0.00417	0.105	0.271	0.170	0.00849
	(0.386)	(0.317)	(0.346)	(0.264)	(0.264)	(0.319)	(0.229)	(0.223)	(0.228)
Active & Legal Disp.	0.0689	−0.0807	0.0481	−0.254	0.545***	0.0418	−0.154	0.131	−0.0510
	(0.158)	(0.196)	(0.202)	(0.181)	(0.179)	(0.189)	(0.191)	(0.213)	(0.163)
Required PDMP	0.282	0.101	0.191	0.0928	0.130	0.0447	0.149	0.0705	0.0754
	(0.254)	(0.270)	(0.211)	(0.112)	(0.115)	(0.147)	(0.139)	(0.127)	(0.140)
Naloxone Access Law	−0.220	0.0247	0.0263	−0.167	0.199	0.488**	0.122	0.136	0.344**
	(0.286)	(0.103)	(0.0820)	(0.189)	(0.195)	(0.223)	(0.136)	(0.146)	(0.164)
Good Samaritan Law	−0.910*	0.825*	0.490	0.0429	0.158	−0.174	0.199	0.0476	−0.156
	(0.470)	(0.422)	(0.487)	(0.246)	(0.145)	(0.225)	(0.184)	(0.148)	(0.101)
Pill Mill Bill	−0.658**	0.920	0.102	−0.275*	0.150	0.114	−0.116	0.165	0.196
	(0.273)	(0.760)	(0.593)	(0.164)	(0.149)	(0.270)	(0.160)	(0.128)	(0.193)
Time-Varying Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear State Trends	No	Yes	No	No	Yes	No	No	Yes	No
Quadratic State Trends	No	No	Yes	No	No	Yes	No	No	Yes
Within-R^2	0.310	0.440	0.507	0.357	0.479	0.521	0.462	0.572	0.618
Observations	612	612	612	765	765	765	867	867	867
p: MCL and ALD	0.888	0.114	0.971	0.378	0.013	0.853	0.342	0.602	0.952
p: Linear Trend		0.000	0.540		0.000	0.183		0.000	0.000
p: Quadratic Trend			0.435			0.574			0.004

Notes: The dependent variable is measured in log(deaths per 100,000 population). Covariates include the unemployment rate, the beer tax rate, fraction male, fraction white, and fractions aged 15 to 19, 20 to 64, and 65 and over. Standard errors clustered by state in parentheses. "p: MCL and ALD" is the p-value for the hypothesis $H_0 : \theta_1^n = \theta_2^n = 0$. "$p$: Linear Trend" and "$p$: Quadratic Trend" are the p-values for the hypotheses $H_0 : \mu_s = \mu_{s'}, \forall s, s'$ and $H_0 : \nu_s = \nu_{s'}, \forall s, s'$, respectively. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.