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

R. Vincent Pohl[†]

November 2019

Abstract

Opioid overdose mortality has been increasing rapidly in the U.S., with some states experiencing much larger mortality growth 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.

---

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 2016, West Virginia had the highest opioid overdose mortality rate with 41.8 per 100,000 population whereas Nebraska had the lowest with 2.5 per 100,000. Mortality growth is even more heterogeneous. Since 1999, mortality rates have risen between 15% in California and over 2,200% in West Virginia and almost 4,600% 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 initiate the use of opioids recreationally, access to cannabis may also provide an alternative. While cannabis consumption is prohibited by federal law, 29 states and the District of Columbia have passed medical cannabis laws (MCL). 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. Shover et al. (2019) question the negative relationship between MCL and opioid mortality, however, and show that this relationship became positive in recent years.

In this paper, I estimate the effect of medical cannabis legalization on opioid-related mortality using mortality data from 1999 to 2016. I employ a difference-in-differences (DD) framework that accounts for heterogeneity in mortality trends across states. Estimates from a "canonical" DD model that only includes 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

---

[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.

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 necessary for a DD regression to identify the causal effect of a policy appears to be violated in this setting. States with slower growth in opioid-related mortality are possibly more likely to implement an MCL because attitudes towards cannabis legalization may be correlated with the 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 much lower opioid-related mortality rates than West Virginia and some midwestern states, which are hit hardest by the opioid epidemic and 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 58% between 1999 and 2012 whereas they rose 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. Not including time trends yields negative and statistically significant coefficients that imply a reduction in mortality rates due to active and legal dispensaries of about 22%. In contrast, these effects become mostly indistinguishable from zero with state-specific time trends. Standard errors increase slightly when I include time trends, but, more importantly, the point estimates are closer to zero than in regressions without trends or even positive. Specifically, I can rule out negative effects below −8% with 95% confidence. The results exhibit heterogeneity across types of opioids and over time, but overall the same pattern holds.

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 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 (2019) 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 (2019) 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 (2019) 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 (2019) 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 (2019), Meinhofer (2018), 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 (2018) and Evans, Lieber, and Power (2019) 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 employe 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 2017 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 (2019). In addition, I use information on state laws that require the use of PDMP from Meinhofer (2018), 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, aged 18 to 64, and aged 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, 2019; 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}. That is, in a departure from the "classic" DD regression, there are two binary treatment variables. 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 18 to 64, and aged 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. In all regressions, 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 rates. To assess if medical cannabis legalization has an effect on opioid overdose mortality overall, I test the joint hypothesis

$$H_0 : \theta_1^n = \theta_2^n = 0 \quad \text{against} \quad H_1 : \theta_1^n \neq 0 \text{ or } \theta_2^n \neq 0$$

and report the p-value from the corresponding F-test.

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

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

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. $u_{st}^n = \mu_s^\ell t + u_{st}^\ell$. 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 $\mu_s^\ell t$. 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. (I revisit the role and consequences of state-specific time trends in Section 5.) 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 $\nu_s^q t^2$:

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

To assess whether mortality trends differ across states and therefore make the inclusion of state-specific time trends necessary, I test the null hypothesis that time trends do not differ on average between states that have and do not have an MCL. After estimating regression (2), I test the following hypothesis:

$$H_0 : \frac{1}{|\mathcal{S}_1|} \sum_{s \in \mathcal{S}_1} \mu_s^\ell = \frac{1}{|\mathcal{S}_0|} \sum_{s' \in \mathcal{S}_0} \mu_{s'}^\ell \quad \text{against} \quad H_1 : \frac{1}{|\mathcal{S}_1|} \sum_{s \in \mathcal{S}_1} \mu_s^\ell \neq \frac{1}{|\mathcal{S}_0|} \sum_{s' \in \mathcal{S}_0} \mu_{s'}^\ell, \qquad (4)$$

where $\mathcal{S}_1 = \{$states with an MCL during the sample period$\}$ and $\mathcal{S}_0 = \{$states without an MCL during the sample period$\}$. After estimating regression (3), I test hypothesis (4) (with superscript $^\ell$ replaced by ^{q}) and the following hypothesis:

$$H_0 : \frac{1}{|\mathcal{S}_1|} \sum_{s \in \mathcal{S}_1} \nu_s^q = \frac{1}{|\mathcal{S}_0|} \sum_{s' \in \mathcal{S}_0} \nu_{s'}^q \quad \text{against} \quad H_1 : \frac{1}{|\mathcal{S}_1|} \sum_{s \in \mathcal{S}_1} \nu_s^q \neq \frac{1}{|\mathcal{S}_0|} \sum_{s' \in \mathcal{S}_0} \nu_{s'}^q. \qquad (5)$$

In addition, I test hypotheses (4) and (5) for states' dispensary status, i.e. $\mathcal{S}_1 = \{$states with active and legal dispensaries during the sample period$\}$ and $\mathcal{S}_0 = \{$states without active and legal dispensaries during the sample period$\}$. In the regression tables below, I report the p-values from F-tests of the respective hypotheses. Rejecting the null hypothesis (4) or (5) implies that states with and without an MCL or active and legal dispensaries exhibit different trends in opioid-related mortality. Including state-specific time trends would therefore be warranted in order to obtain unbiased estimates of the effect of medical cannabis legalization.

---

[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.

4 Results

4.1 Assessing the Common Trends Assumption

Before discussing the regression results, I compare the opioid overdose mortality growth after 1999 in states that implemented an MCL in a given year or had active and legal dispensaries to states that either never had an MCL or implemented an MCL at a later date. 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, declined by 2% between 1999 and 2000 on average. During the same time, states that adopted an MCL after 2001 experienced an increase in mortality rates by 6% and in state that never had an MCL by 2015 mortality rates grew by 31%. The same pattern holds for most of 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 32% on average in Arizona, the District of Columbia, and New Jersey between 1999 and 2010, for example, whereas it grew by 162% in never-MCL states during the same time period.

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 or started to have active and legal dispensaries at a later date. Between 1999 and 2013, for example, mortality rates rose by 89% 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 148% 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 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.

Tables 2 and 3 show unconditional mean mortality rates and their changes that do not control 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 provide further evidence against the common trends assumption conditional on state and year fixed effects and covariates.

4.2 Impact of Medical Cannabis Legalization on Opioid- and Heroin-Related Mortality

For each mortality outcome, I estimate regressions for the time periods 1999 to 2010, 1999 to 2013, and 1999 to 2017. 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, and heroin combined. Table 4 shows the regression results. Starting with the 1999 to 2010 period and no time trends, column (1) shows a coefficient of -0.170, which corresponds to a decline in opioid-related mortality by 16% when an MCL is in effect. The coefficient is not statistically significant at conventional levels. This effect is similar to the corresponding estimate in Powell, Pacula, and Jacobson (2018). In addition, active and legal dispensaries reduce opioid mortality rates by an additional 19%, and this estimate is significant at the 5% 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 lowers the point estimate for the effect of MCL, see column (2). When adding quadratic state trends in column (3), the effect of MCL on opioid-related mortality rates equals -0.5% 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 I include linear or quadratic state trends. The point estimate is reduced to about half 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 0.47. 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 medical cannabis legalization that operates through active and legal dispensaries.

Next, I revisit the role of state-specific time trends when the sample period is extended to 2013 or 2017. 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 effect of MCL is attenuated and the effect of active and legal dispensaries is slightly larger in absolute value compared to the 1999 to 2010 sample, see column (4) in Table 4. When testing the joint effect of MCL and active and legal dispensaries, the resulting p-value equals 0.03. Once I add linear or quadratic state trends, MCL and dispensary status have no statistically significant negative effect on opioid-related mortality and the effect of active and legal dispensaries even becomes positive and significant at the 10% level. The p-values for testing the joint effect of MCL and active and legal dispensaries in the regressions in column (5) and (6) equal 0.20 and 0.95.

When further extending the sample to 2017, the estimated coefficients are similar to the shorter sample periods. Active and legal dispensaries only have a significantly negative effect when I do not include state-specific time trends. Moreover, with linear or quadratic time trends, MCL and dispensary status are not jointly significant with p-values of 0.28, and 0.32 in the regressions in columns (8) and (9). For almost all sample periods and specifications with time trends, I reject the null hypotheses that linear and quadratic trends are identical across states with and without MCL or active and legal dispensaries, see the last four 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- and heroin-related mortality overall, 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 2017, 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). For the 1999 to 2017 sample, MCL and active and legal dispensaries have no statistically significant effects regardless of whether I time trends. The last four 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 with and without MCL or active and legal dispensaries.

Next, I consider the effects of MCL and dispensaries on mortality due to heroin overdoses. 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 1% level. When I add linear or quadratic state-specific time trends in the regressions in columns (2) and (3), the effects become smaller in absolute value. The effects of MCL are now only significant at the 10% level and dispensary effects are insignificant at conventional levels. 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 relatively 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. MCL and dispensary status are jointly significant only in specifications without time trends.

Heroin overdoses played a more important role after 2010, so considering the 1999 to 2013/2017 samples is particularly relevant here. The mortality-reducing effect of MCL becomes smaller in these samples compared to the 1999 to 2010 period. For the 1999 to 2013

sample, the effect equals -54% and is significant at the 5% level, see column (4) in Table 6, but for the 1999 to 2017 sample it becomes insignificant, see column (7). 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 2017 sample, by 38%, 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. As in the 1999 to 2010 sample, MCL and dispensary status are jointly significant only in specifications without time trends.

Testing the null hypotheses of equal linear and quadratic time trends across states with and without MCL or active and legal dispensaries shows that including linear state-specific time trends is likely sufficient in the case of heroin-related mortality, see the last four rows in Table 6. Specifically, I cannot reject the null hypotheses that quadratic and linear terms are equal in the specifications with quadratic time trends. Moreover, the linear terms are only significantly different in the 1999 to 2013 and for dispensary status in the 1999 to 2017 sample.

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 2017 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 that linear time trends differ across states with and without MCL and active and legal dispensaries in the 1999 to 2013 sample and should therefore be included in regressions of overdose mortality related to synthetic opioids (see the last four rows in Table 7). For the 1999 to 2010 and the 1999 to 2017 samples, the evidence is more mixed.

16

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]

Required PDMD have no statistically significant effect on mortality due to opioids and heroin combined and due to prescription or synthetic opioids, regardless of the sample period and whether I include time trends, see Tables 4, 5, and 7. These programs have a strong negative effect on heroin-related mortality, however, at least for the 1999 to 2010 sample independent of the inclusion of time trends. Specifically, mortality rates decline 57% to 62%, and these effects are significant at the 1% level. These effects disappear in the sample periods that include post-2010 years.

Naloxone Access Laws reduce opioid-related mortality rates by 33% in the 1999 to 2010 sample when no time trends are included, see column (1) in Table 4, but the estimate becomes positive when I include linear or quadratic state-specific 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 29% 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 (+56%, see column (2)) and loses its statistical significance with quadratic time trends, column (3). When I extend the sample period to 2013 or 2017, Good Samaritan Laws lower opioid-related mortality in the specification with quadratic state trends, but these effects are not statistically significant, see columns (6) and (9). For

---

[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.

prescription opioid mortality in Table 5, the effects of Good Samaritan Laws are similar. 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 and heroin, 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 -20% and -48%. The effect on heroin-related mortality equals $+51\%$ and is statistically significant, see column (1) in Table 6. In specifications with linear or quadratic trends and when the sample period is extended to 2013 or 2017, the impact of Pill Mill Bills loses its statistical significance with some point estimates being 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 medical cannabis legalization 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, without additional covariates, and with $Y_{st} = \log(MR_{st})$:

$$Y_{st} = \theta MCL_{st} + \alpha_s + \gamma_t + u_{st}. \tag{6}$$

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 (6) has mean zero (see, e.g., Angrist and Pischke, 2009). 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 (Goodman-Bacon, 2018).

Instead of regression (6), it is plausible that the true relationship between MCL and opioid-related mortality is given by

$$Y_{st} = \theta MCL_{st} + \alpha_s + \gamma_t + \mu_s t + \epsilon_{st}, \tag{7}$$

where $\mu_s t$ is a linear state-specific time trend and $E[\epsilon_{st}|s,t] = 0$. In contrast to regression (6), which assumes that the time effects γ_t are identical across states, regression (7) 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 (7) is a better representation of the underlying data generating process than regression (6).

When state-specific time trends are present in the true relationship between MCL and opioid overdose mortality but one estimates regression (6), 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 mortality grows more slowly in the state that implemented an MCL, 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 (6) although the true relationship between MCL and mortality rates is given by equation (7) 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.

Put differently, if regression (6) reflects the true relationship between MCL and opioid mortality rates, the error term in regression (6) has mean zero conditional on state and year: $E[u_{st}|s,t] = 0$. In contrast, if regression (7) reflects the true relationship and one estimates regression (6), 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 (6) or (7) denotes the true relationship between MCL and opioid mortality rates to implement an informal and visual specification test as follows. After estimating regression (6), the residuals

$$\hat{u}_{st}^n = Y_{st} - \hat{\theta}^n MLC_{st} - \hat{\alpha}_s^n - \hat{\gamma}_t^n \tag{8}$$

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. The same applies for states with and without active and legal dispensaries. On the contrary, if regression (7) 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.

---

[10]Note that the DD regression (7) 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.

To assess the common trends assumption and to determine whether regression (6) or (7) more likely represents the true relationship between MCL and opioid mortality, I plot the residuals in equation (8) 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 (6) 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

$$\hat{u}_{st}^{\ell} = Y_{st} - \hat{\theta}^{\ell} MCL_{st} - \hat{\alpha}_s^{\ell} - \hat{\gamma}_t^{\ell} - \hat{\mu}_s^{\ell} t, \tag{9}$$

and

$$\hat{u}_{st}^{q} = Y_{st} - \hat{\theta}^{q} MCL_{st} - \hat{\alpha}_s^{q} - \hat{\gamma}_t^{q} - \hat{\mu}_s^{q} t - \hat{\nu}_s^{q} t^2, \tag{10}$$

a previously visible trend should disappear if regression (7) 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.

Comparing pre-trends of the outcome variable conditional on group and time between treatment and control group—i.e. of the residuals—is more informative in this context than the ubiquitous plot of unconditional pre-trends to check whether the common trends assumption is satisfied. After all, the common trends assumption underlying DD regressions is about the conditional mean of the error term $E[u_{st}|s,t] = 0$ and not the unconditional mean $E[u_{st}] = 0$.

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 and heroin combined, Figures 1a, 1d, and 1g plot the residuals in equation (8), Figures 1b, 1e, and 1h plot the residuals in equation (9), and Figures

1c, 1f, and 1i plot the residuals in equation (10) 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 ○) 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 and heroin related mortality rates (see Table 4) for the 1999 to 2010 sample period in the top row of Figure 1. 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 1a. 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 1b and 1c.

These residual plots highlight the importance of accounting for potential differences in pre-existing opioid and heroin 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- and heroin-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 1b and 1c. 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 increase (i.e. become smaller in absolute value) when state trends are added. The residual plots in Figure 1 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 1d and 1g show that the residuals from opioid-related mortality regressions without state-specific time trends also exhibit a downward trend for

---

[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 2016, thereby adding more states with active and legal dispensaries (see below).

states with active and legal dispensaries when the sample period is extended to 2013 or 2016. In contrast, states with an MCL that did not have active and legal dispensaries exhibit an upward trend in opioid and heroin mortality residuals in the 1999 to 2016 sample. Once linear or quadratic state trends are included in Figures 1e, 1f, 1h, and 1i, however, these trends mostly disappear and the residuals are centered around zero. differences in the point estimates in columns (4) to (9) of Table 4 are consistent with these 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). Once this bias is removed, the impact of MCL and active and legal dispensaries on opioid- and heroin-related mortality increases towards zero or becomes positive.

For regressions of prescription opioid mortality, Figure 2a, 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 2b and 2c. The residual plots in the bottom two rows of Figure 2 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 2d and 2g. In contrast, when I add linear or quadratic state trends, the residuals have no trend and are centered around zero, see Figures 2e, 2f, 2h, and 2i.

For regressions of heroin-related mortality, the residual plots in the top row of Figure 3 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 3a, 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 3b and 3c, 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 3d and 3g. Moreover, the

23

residuals for states with and MCL but without active and legal dispensaries trend upward in the 1999 to 2016 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 3g, 3h, and 3i 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 4 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 4a and 4d. 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 1 to 4 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. Moreover, the results in this paper can reconcile the contradicting findings in Bachhuber et al. (2014), Powell, Pacula, and Jacobson (2018), and Shover et al. (2019). 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.

References

Alpert, Abby, David Powell, and Rosalie Liccardo Pacula. 2018. "Supply-Side Drug Policy in the Presence of Substitutes: Evidence from the Introduction of Abuse-Deterrent Opioids." *American Economic Journal: Economic Policy* 10 (4):1–35.

Anderson, D. Mark, Benjamin Hansen, and Daniel I. Rees. 2013. "Medical Marijuana Laws, Traffic Fatalities, and Alcohol Consumption." *The Journal of Law and Economics* 56 (2):333–369.

Angrist, Joshua D. and Jorn-Steffen Pischke. 2009. *Mostly Harmless Econometrics*. Princeton University Press.

Bachhuber, Marcus A., Brendan Saloner, Chinazo O. Cunningham, and Colleen L. Barry. 2014. "Medical Cannabis Laws and Opioid Analgesic Overdose Mortality in the United States, 1999-2010." *JAMA Internal Medicine* 174 (10):1668–1673.

Bradford, Ashley C. and W. David Bradford. 2016. "Medical Marijuana Laws Reduce Prescription Medication Use In Medicare Part D." *Health Affairs* 35 (7):1230–1236.

———. 2017. "Medical Marijuana Laws May Be Associated With A Decline In The Number Of Prescriptions For Medicaid Enrollees." *Health Affairs* 36 (5):945–951.

Bradford, Ashley C., W. David Bradford, Amanda Abraham, and Grace Bagwell Adams. 2018. "Association Between US State Medical Cannabis Laws and Opioid Prescribing in the Medicare Part D Population." *JAMA Internal Medicine* 178 (5):667–672.

Buchmueller, Thomas C. and Colleen Carey. 2018. "The Effect of Prescription Drug Monitoring Programs on Opioid Utilization in Medicare." *American Economic Journal: Economic Policy* 10 (1):77–112.

Card, David and Alan B. Krueger. 1994. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania." *American Economic Review* 84 (4):772–793.

Chapman, Susan A., Joanne Spetz, Jessica Lin, Krista Chan, and Laura A. Schmidt. 2016. "Capturing Heterogeneity in Medical Marijuana Policies: A Taxonomy of Regulatory Regimes Across the United States." *Substance Use & Misuse* 51 (9):1174–1184.

Chriqui, Jamie F., Rosalie L. Pacula, Duane C. McBride, Deborah A. Reichmann, Curtis J. Vanderwaal, and Yvonne M. Terry-McElrath. 2002. *Illicit Drug Policies: Selected Laws from the 50 States.* Berrien Springs, MI: Andrews University.

Chu, Yu-Wei Luke. 2015. "Do Medical Marijuana Laws Increase Hard-Drug Use?" *The Journal of Law and Economics* 58 (2):481–517.

Dave, Dhaval M., Anca M. Grecu, and Henry Saffer. 2019. "Mandatory Access Prescription Drug Monitoring Programs and Prescription Drug Abuse." *Journal of Policy Analysis and Management* 38 (1):181–209.

Doleac, Jennifer L. and Anita Mukherjee. 2018. "The Moral Hazard of Lifesaving Innovations: Naloxone Access, Opioid Abuse, and Crime."

Evans, Williams N., Ethan Lieber, and Patrick Power. 2019. "How the Reformulation of OxyContin Ignited the Heroin Epidemic." *Review of Economics and Statistics* 101 (1):1–15.

Garin, Julio, R. Vincent Pohl, and Rhet A. Smith. 2018. "The Effect of Medical Cannabis Dispensaries on Opioid and Heroin Overdose Mortality."

Goodman-Bacon, Andrew. 2018. "Difference-in-Differences with Variation in Treatment Timing." NBER Working Paper 25018.

Hollingsworth, Alex, Christopher J. Ruhm, and Kosali Simon. 2017. "Macroeconomic Conditions and Opioid Abuse." *Journal of Health Economics* 56:222–233.

Li, Guohua, Joanne E. Brady, Barbara H. Lang, James Giglio, Hannah Wunsch, and Charles DiMaggio. 2014. "Prescription Drug Monitoring and Drug Overdose Mortality." *Injury Epidemiology* 1 (9):1–8.

Mallatt, Justine. 2017. "The Effect of Prescription Drug Monitoring Programs on Opioid Prescriptions and Heroin Crime Rates."

Meinhofer, Anglica. 2018. "Prescription Drug Monitoring Programs: The Role of Asymmetric Information on Drug Availability and Abuse." *American Journal of Health Economics* 4 (4):504–526.

Pacula, Rosalie L., Anne E. Boustead, and Priscilla Hunt. 2014. "Words Can Be Deceiving: A Review of Variation among Legally Effective Medical Marijuana Laws in the United States." *Journal of Drug Policy Analysis* 7 (1):1–19.

Pacula, Rosalie L., Jamie F. Chriqui, Deborah A. Reichmann, and Yvonne M. Terry-McElrath. 2002. "State Medical Marijuana Laws: Understanding the Laws and Their Limitations." *Journal of Public Health Policy* 23 (4):413–439.

Pacula, Rosalie L., David Powell, Paul Heaton, and Eric L. Sevigny. 2015. "Assessing the Effects of Medical Marijuana Laws on Marijuana Use: The Devil Is in the Details." *Journal of Policy Analysis and Management* 34 (1):7–31.

Pardo, Bryce. 2017. "Do More Robust Prescription Drug Monitoring Programs Reduce Prescription Opioid Overdose?: Prescription Drug Monitoring and Opioid Overdoses." *Addiction* 112 (10):1773–1783.

Patrick, Stephen W., Carrie E. Fry, Timothy F. Jones, and Melinda B. Buntin. 2016. "Implementation Of Prescription Drug Monitoring Programs Associated With Reductions In Opioid-Related Death Rates." *Health Affairs* 35 (7):1324–1332.

Paulozzi, Leonard J., Edwin M. Kilbourne, and Hema A. Desai. 2011. "Prescription Drug Monitoring Programs and Death Rates from Drug Overdose." *Pain Medicine* 12 (5):747–754.

Powell, David, Rosalie Liccardo Pacula, and Mireille Jacobson. 2018. "Do Medical Marijuana Laws Reduce Addictions and Deaths Related to Pain Killers?" *Journal of Health Economics* 58:29–42.

Quinones, Sam. 2015. *Dreamland: The True Tale of America's Opiate Epidemic.* New York: Bloomsbury.

Rees, Daniel I., Joseph J. Sabia, Laura M. Argys, Joshua Latshaw, and Dhaval Dave. 2017. "With a Little Help from My Friends: The Effects of Naloxone Access and Good Samaritan Laws on Opioid-Related Deaths." NBER Working Paper 23171.

Ruhm, Christopher J. 2018. "Corrected US Opioid-Involved Drug Poisoning Deaths and Mortality Rates, 1999-2015: Corrected Opioid-Involved Mortality Rates." *Addiction* 113 (7):1339–1344.

Shover, Chelsea L., Corey S. Davis, Sanford C. Gordon, and Keith Humphreys. 2019. "Association between Medical Cannabis Laws and Opioid Overdose Mortality Has Reversed over Time." *Proceedings of the National Academy of Sciences* 116 (26):12624–12626.

Smith, Rhet A. 2019. "The Effects of Medical Marijuana Dispensaries on Adverse Opioid Outcomes." *Economic Inquiry* .

Wen, Hefei and Jason M. Hockenberry. 2018. "Association of Medical and Adult-Use Marijuana Laws With Opioid Prescribing for Medicaid Enrollees." *JAMA Internal Medicine* 178 (5):673–679.
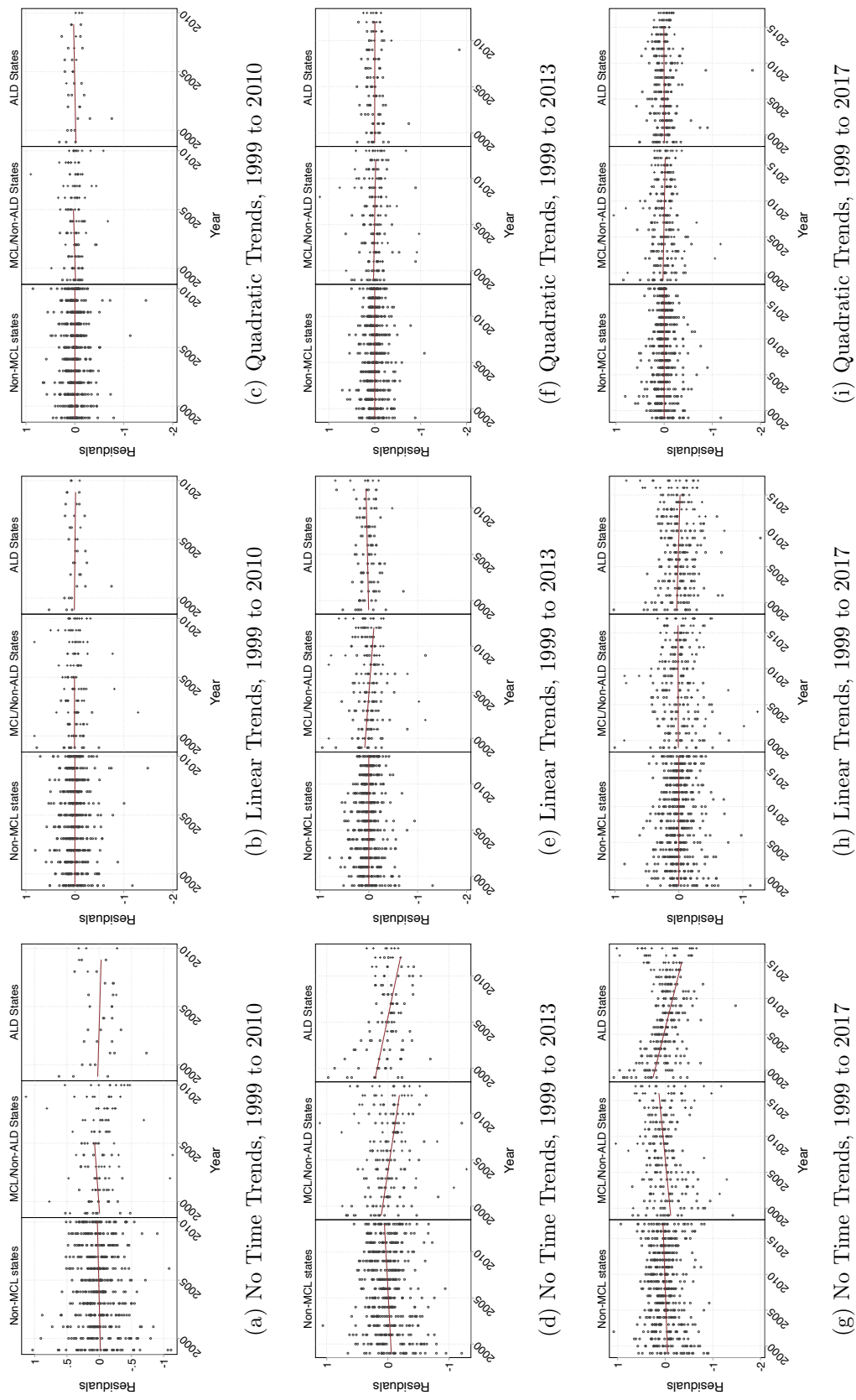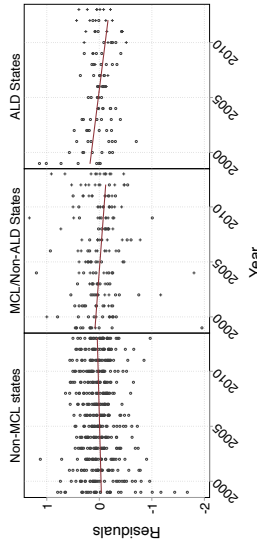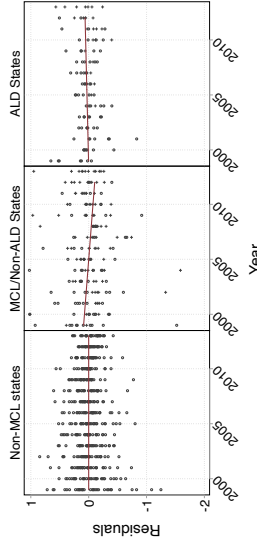
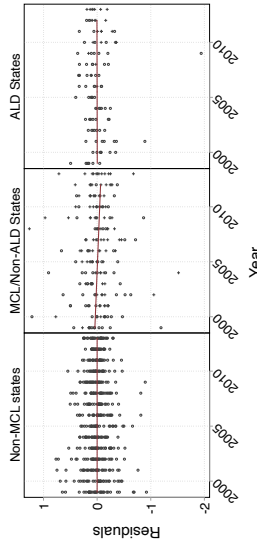(a) No Time Trends, 1999 to 2010   (b) Linear Trends, 1999 to 2010   (c) Quadratic Trends, 1999 to 2010
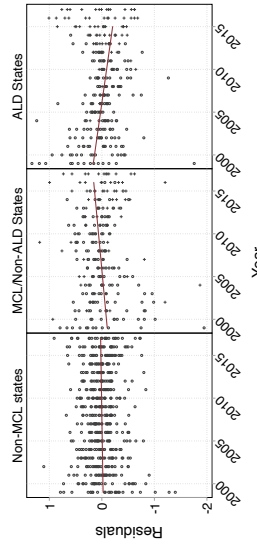
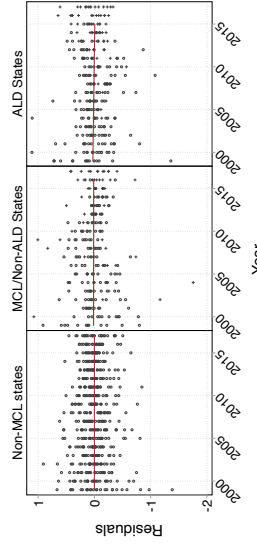(d) No Time Trends, 1999 to 2013   (e) Linear Trends, 1999 to 2013   (f) Quadratic Trends, 1999 to 2013

(g) No Time Trends, 1999 to 2017   (h) Linear Trends, 1999 to 2017   (i) Quadratic Trends, 1999 to 2017

Figure 1: Residual Plots for Opioids and Heroin

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 (8), (9), and (10) 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.

29

(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

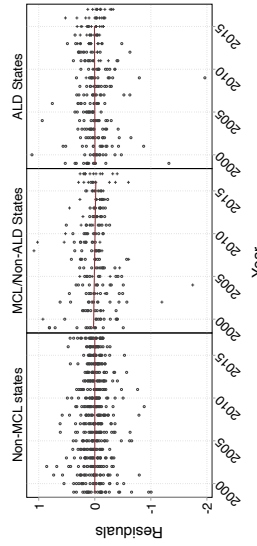(e) Linear Trends, 1999 to 2013

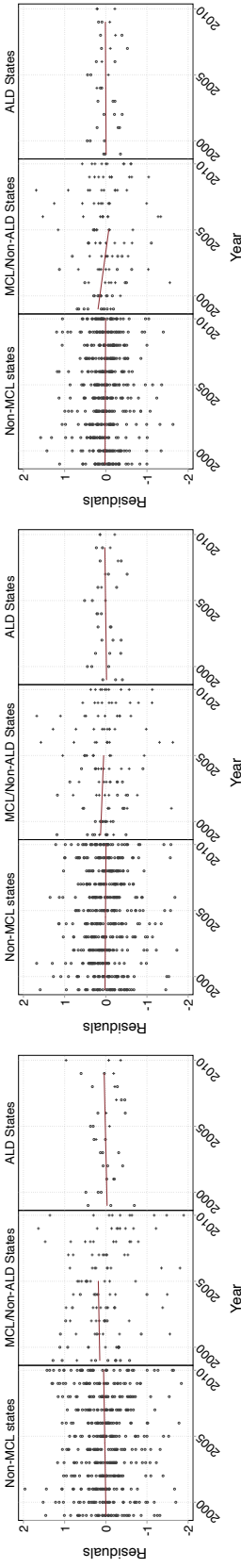(f) Quadratic Trends, 1999 to 2013

(g) No Time Trends, 1999 to 2017

(h) Linear Trends, 1999 to 2017

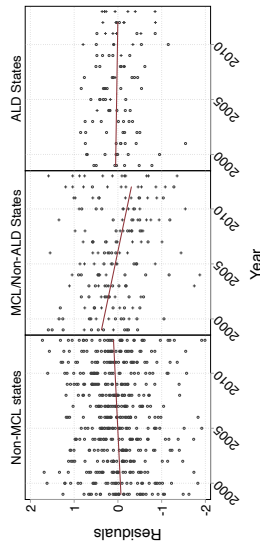(i) Quadratic Trends, 1999 to 2017

Figure 2: Residual Plots for Prescription Opioids

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 (8), (9), and (10) 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.
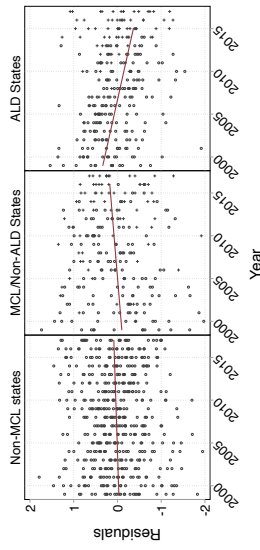
(a) No Time Trends, 1999 to 2010

(b) Linear Trends, 1999 to 2010

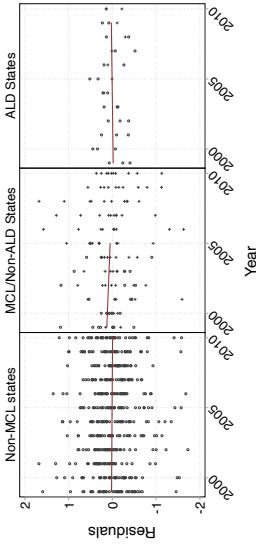(c) Quadratic Trends, 1999 to 2010

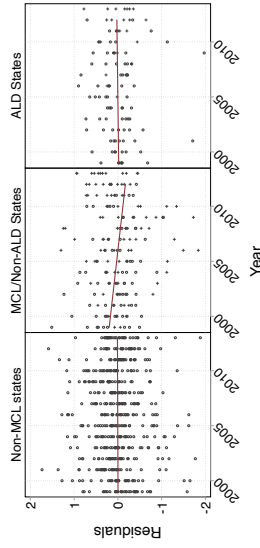(d) No Time Trends, 1999 to 2013

(e) Linear Trends, 1999 to 2013

(f) Quadratic Trends, 1999 to 2013

(g) No Time Trends, 1999 to 2017

(h) Linear Trends, 1999 to 2017

(i) Quadratic Trends, 1999 to 2017

Figure 3: Residual Plots for Heroin

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 (8), (9), and (10) 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.
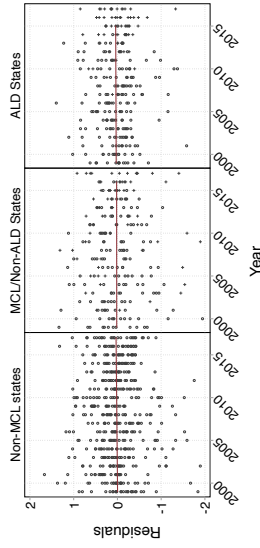
(a) No Time Trends, 1999 to 2010

(b) Linear Trends, 1999 to 2010

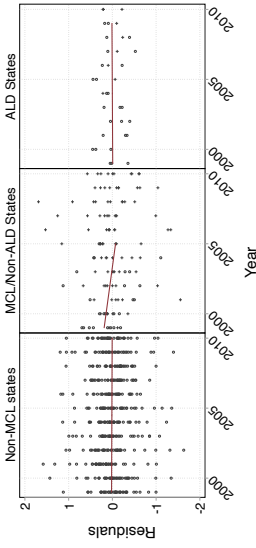(c) Quadratic Trends, 1999 to 2010

(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 2017

(h) Linear Trends, 1999 to 2017

(i) Quadratic Trends, 1999 to 2017

Figure 4: Residual Plots for Synthetic Opioids

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 (8), (9), and (10) 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.

Table 1: Summary Statistics by Medical Cannabis Legalization Status

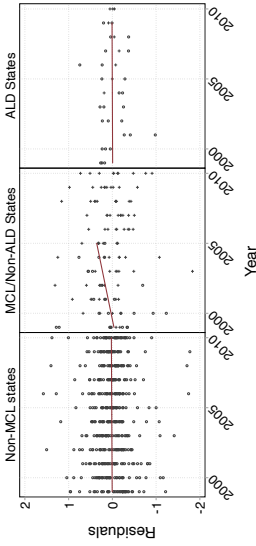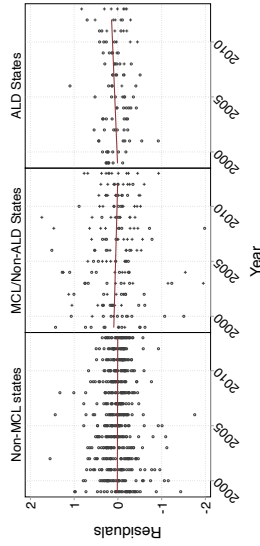|	(1) No MCL/ALD	(2) MCL	(3) ALD
Opioid and Heroin Mortality Rate	5.896 (5.250)	9.106 (6.064)	12.34 (7.018)
Prescription Opioid Mortality Rate	3.018 (2.993)	3.989 (2.351)	4.878 (2.194)
Heroin Mortality Rate	1.229 (1.775)	2.190 (2.492)	4.547 (3.642)
Synthetic Opioid Mortality Rate	1.351 (2.674)	2.516 (5.088)	4.519 (6.023)
Required PDMP	0.0435 (0.204)	0.0899 (0.287)	0.0256 (0.159)
Naloxone Access Law	0.0757 (0.265)	0.163 (0.370)	0.705 (0.459)
Good Samaritan Law	0.0393 (0.194)	0.174 (0.380)	0.474 (0.503)
Pill Mill Bill	0.0729 (0.260)	0.00562 (0.0750)	0 (0)
Beer Tax Rate	0.256 (0.206)	0.340 (0.329)	0.205 (0.102)
Unemployment Rate	5.434 (1.851)	6.164 (2.238)	6.290 (2.083)
Fraction Male	0.492 (0.00705)	0.498 (0.0104)	0.493 (0.00639)
Fraction White	0.821 (0.118)	0.783 (0.201)	0.817 (0.109)
Fraction Aged 20 to 64	0.608 (0.0158)	0.621 (0.0159)	0.615 (0.0213)
Fraction Aged 65 and Over	0.134 (0.0180)	0.133 (0.0273)	0.148 (0.0228)
Observations	713	178	78

Notes: The table shows means and standard deviations (in parentheses). Mortality Rates are per 100,000 population. "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	Percentage change in mortality between 1999 and $t-1$		
		New-MCL states	Later-MCL states	Never-MCL states
2001	CO, HI	−2	6	31
2002	NV	13	27	64
2005	MT, VT	94	43	111
2006	RI	119	47	116
2008	NM	28	64	146
2009	MI	154	62	154
2011	AZ, DC, NJ	32	81	162
2012	DE	112	85	164
2013	CT, MA	43	100	165
2014	IL, NH	99	105	172
2015	MD, MN, NY	97	136	189

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. "Later-MCL states" are states that introduced an MCL after 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, HI, ME, OR, and WA introduced an MCL before 2001 and are not included.

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 ALD Implementation

| Year t | States With New MCL | Percentage change in mortality between 1999 and $t-1$ | | |
		New-MCL states	Later-MCL states	Never-MCL states
2004	CA	−9	49	74
2010	NM	84	39	136
2011	CO	48	59	138
2012	ME	104	69	141
2013	AZ, NJ	54	68	141
2014	DC, MI, MT, NV, OR, RI, VT, WA	89	79	148
2015	CT	115	53	163

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 (ALD) for a full year for the first time in the given year. "Later-ADL states" are states that had ADL after 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 and Heroin Overdose Mortality Rates

| | 1999 to 2010 | | | 1999 to 2013 | | | 1999 to 2017 | | |
	(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	−0.163 (0.102)	−0.203 (0.141)	−0.0506 (0.0483)	−0.0870 (0.0938)	0.0980 (0.155)	0.0107 (0.129)	0.129 (0.102)	0.167 (0.103)	0.0660 (0.0974)
Active & Legal Disp.	−0.114 (0.105)	−0.0524 (0.0883)	−0.131 (0.169)	−0.147 (0.105)	0.138 (0.104)	0.0174 (0.121)	−0.169 (0.126)	0.200 (0.136)	−0.0710 (0.0538)
Required PDMP	−0.100 (0.154)	0.118 (0.121)	−0.0434 (0.0678)	0.00543 (0.115)	0.0358 (0.115)	−0.00596 (0.0811)	0.124 (0.123)	0.0345 (0.0923)	0.0724 (0.104)
Naloxone Access Law	−0.407** (0.162)	0.124 (0.0893)	0.0452 (0.0743)	−0.257* (0.145)	0.115 (0.115)	0.166 (0.0994)	0.0748 (0.104)	0.0188 (0.0961)	0.0592 (0.0758)
Good Samaritan Law	−0.373*** (0.114)	0.435** (0.198)	0.0357 (0.144)	0.0536 (0.142)	0.171* (0.0945)	−0.0885 (0.125)	0.156 (0.121)	0.209** (0.0814)	−0.0537 (0.0693)
Pill Mill Bill	−0.223*** (0.0715)	−0.0787 (0.164)	0.508 (0.429)	−0.0686 (0.111)	−0.118* (0.0704)	−0.0181 (0.121)	0.0515 (0.148)	−0.110 (0.0738)	−0.0547 (0.0957)
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	0.699	0.808	0.860	0.718	0.813	0.853	0.751	0.851	0.896
Observations	612	612	612	765	765	765	969	969	969
p: MCL and ALD	0.206	0.198	0.395	0.271	0.407	0.987	0.177	0.250	0.378
p: Linear Trend (MCL)		0.153	0.000		0.000	0.000		0.000	0.000
p: Quadratic Trend (MCL)			0.015			0.000			0.000
p: Linear Trend (ALD)		0.023	0.005		0.000	0.000		0.000	0.000
p: Quadratic Trend (ALD)			0.150			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 null hypothesis that MCL and active legal dispensaries have jointly no effect on the outcome. "p: Linear Trend" and "p: Quadratic Trend" are the p-values for the hypotheses that linear and quadratic time trends do not differ between states with and without MCL and active and legal dispensaries (ALD). See Section 3.3 for details. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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

	1999 to 2010			1999 to 2013			1999 to 2017		
	(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	−0.299*** (0.102)	−0.326** (0.130)	−0.134 (0.104)	−0.196** (0.0942)	−0.0339 (0.147)	0.0382 (0.170)	−0.0225 (0.105)	0.00356 (0.0988)	0.0180 (0.103)
Active & Legal Disp.	−0.259 (0.163)	0.0361 (0.141)	−0.0940 (0.215)	−0.195 (0.135)	0.0846 (0.0835)	−0.0568 (0.150)	−0.102 (0.134)	0.0935 (0.114)	−0.0533 (0.0578)
Required PDMP	−0.0292 (0.145)	0.0571 (0.107)	−0.113 (0.0774)	−0.0140 (0.0888)	−0.00746 (0.0904)	−0.185 (0.128)	0.130 (0.122)	−0.0574 (0.100)	−0.0110 (0.124)
Naloxone Access Law	−0.352*** (0.0969)	0.149* (0.0745)	0.0222 (0.159)	−0.319*** (0.0877)	0.0833 (0.109)	0.105 (0.126)	0.00349 (0.0952)	−0.0191 (0.0839)	0.0207 (0.0785)
Good Samaritan Law	−0.287** (0.132)	0.479** (0.205)	−0.0210 (0.165)	0.0699 (0.148)	0.0854 (0.108)	−0.192 (0.132)	0.132 (0.123)	0.179** (0.0716)	−0.0533 (0.0638)
Pill Mill Bill	−0.360*** (0.0788)	0.215 (0.379)	0.418 (0.359)	−0.171 (0.123)	−0.0730 (0.100)	−0.0338 (0.124)	−0.0751 (0.161)	−0.163* (0.0912)	−0.0753 (0.0809)
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	0.695	0.787	0.838	0.727	0.806	0.840	0.720	0.824	0.859
Observations	612	612	612	765	765	765	969	969	969
p: MCL and ALD	0.013	0.048	0.407	0.054	0.544	0.920	0.746	0.536	0.634
p: Linear Trend (MCL)		0.012	0.000		0.000	0.000		0.000	0.000
p: Quadratic Trend (MCL)			0.001			0.000			0.000
p: Linear Trend (ALD)		0.002	0.017		0.000	0.000		0.000	0.000
p: Quadratic Trend (ALD)			0.174			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 null hypothesis that MCL and active legal dispensaries have jointly no effect on the outcome. "p: Linear Trend" and "p: Quadratic Trend" are the p-values for the hypotheses that linear and quadratic time trends do not differ between states with and without MCL and active and legal dispensaries (ALD). See Section 3.3 for details. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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

	1999 to 2010			1999 to 2013			1999 to 2017		
	(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	-0.867*** (0.331)	-0.394* (0.229)	-0.536* (0.302)	-0.545** (0.270)	-0.202 (0.195)	-0.396* (0.232)	-0.242 (0.218)	-0.283* (0.158)	-0.128 (0.150)
Active & Legal Disp.	-0.264 (0.290)	-0.119 (0.188)	-0.0658 (0.270)	-0.220 (0.217)	0.0883 (0.187)	0.338 (0.267)	-0.408** (0.201)	0.0117 (0.180)	-0.117 (0.138)
Required PDMP	-0.952*** (0.323)	-0.909*** (0.289)	-0.856*** (0.152)	-0.0259 (0.328)	-0.00355 (0.221)	-0.222 (0.203)	0.183 (0.249)	-0.228 (0.212)	-0.177 (0.134)
Naloxone Access Law	-0.374* (0.211)	0.166 (0.184)	-0.0344 (0.236)	-0.0657 (0.264)	0.571** (0.250)	0.412* (0.245)	0.0257 (0.163)	0.0351 (0.135)	0.0902 (0.167)
Good Samaritan Law	0.526 (0.418)	0.687* (0.367)	1.113*** (0.387)	0.0801 (0.211)	0.150 (0.147)	0.432* (0.233)	0.173 (0.174)	0.137 (0.144)	-0.0174 (0.131)
Pill Mill Bill	0.722** (0.333)	0.348 (0.390)	1.572 (1.120)	0.352 (0.361)	0.237 (0.200)	0.434 (0.285)	0.339 (0.362)	0.347 (0.221)	0.216 (0.187)
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	0.210	0.494	0.590	0.434	0.660	0.705	0.665	0.797	0.830
Observations	612	612	612	765	765	765	969	969	969
p: MCL and ALD	0.034	0.094	0.212	0.039	0.491	0.199	0.050	0.209	0.481
p: Linear Trend (MCL)		0.129	0.491		0.001	0.061		0.318	0.505
p: Quadratic Trend (MCL)			0.195			0.636			0.401
p: Linear Trend (ALD)		0.488	0.703		0.006	0.463		0.000	0.146
p: Quadratic Trend (ALD)			0.600			0.010			0.614

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 null hypothesis that MCL and active legal dispensaries have jointly no effect on the outcome. "p: Linear Trend" and "p: Quadratic Trend" are the p-values for the hypotheses that linear and quadratic time trends do not differ between states with and without MCL and active and legal dispensaries (ALD). See Section 3.3 for details. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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

	1999 to 2010			1999 to 2013			1999 to 2017		
	(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	−0.0338	−0.445*	0.0169	0.0123	0.0600	0.0943	0.372**	0.337*	−0.00944
	(0.225)	(0.240)	(0.272)	(0.164)	(0.213)	(0.236)	(0.157)	(0.171)	(0.152)
Active & Legal Disp.	0.0242	−0.0575	−0.0987	−0.230	0.304*	−0.0760	−0.106	0.326	0.00607
	(0.134)	(0.122)	(0.176)	(0.144)	(0.177)	(0.129)	(0.227)	(0.229)	(0.143)
Required PDMP	0.0104	0.163	0.0558	−0.0109	0.102	0.0790	0.149	0.216	0.0908
	(0.220)	(0.192)	(0.157)	(0.0978)	(0.105)	(0.112)	(0.160)	(0.138)	(0.123)
Naloxone Access Law	−0.183	−0.0224	0.0681	−0.134	0.107	0.375**	0.314**	0.150	0.246
	(0.253)	(0.0702)	(0.101)	(0.171)	(0.170)	(0.182)	(0.155)	(0.169)	(0.148)
Good Samaritan Law	−0.769***	0.820**	0.0978	0.0918	0.226*	−0.109	0.292	0.204	−0.0543
	(0.257)	(0.315)	(0.365)	(0.214)	(0.123)	(0.176)	(0.189)	(0.161)	(0.0998)
Pill Mill Bill	−0.425**	0.459	0.337	−0.160	0.0415	0.0818	0.133	0.0767	0.0749
	(0.173)	(0.379)	(0.385)	(0.122)	(0.0851)	(0.210)	(0.164)	(0.128)	(0.127)
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	0.447	0.573	0.634	0.482	0.594	0.645	0.690	0.782	0.834
Observations	612	612	612	765	765	765	969	969	969
p: MCL and ALD	0.974	0.031	0.854	0.272	0.238	0.822	0.053	0.130	0.998
p: Linear Trend (MCL)		0.013	0.246		0.205	0.079		0.607	0.340
p: Quadratic Trend (MCL)			0.115			0.001			0.011
p: Linear Trend (ALD)		0.823	0.794		0.001	0.000		0.172	0.042
p: Quadratic Trend (ALD)			0.948			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 null hypothesis that MCL and active legal dispensaries have jointly no effect on the outcome. "p: Linear Trend" and "p: Quadratic Trend" are the p-values for the hypotheses that linear and quadratic time trends do not differ between states with and without MCL and active and legal dispensaries (ALD). See Section 3.3 for details. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.