The impact of state level opioid prescription limiting policies on overdose deaths in the US[†]

M2 EEE Thesis

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Abstract

The opioid epidemic is a public health crisis unique to the US. Using differences-in-differences (DiD) with staggered treatment timing to construct a dynamic event study, I estimate the average effect of state opioid prescription limiting laws on drug overdose deaths over the period 2015-2023 for 38 treatment states compared to 13 control states. No statistically significant effect is found, but effects range from -3.2% to -9.9% years after enactment. Heterogeneous analysis by age group reveals significant reductions in opioid deaths by 63.3% and 43.2% for age groups 15-24 and 25-34 respectively.

[†]I would like to thank my supervisor Professor Catarina Goulão immensely for guiding me over the course of this thesis. All remaining errors are mine.

Contents

1 Introduction

In light of the current wave of the opioid epidemic in the USA that is leading to increasing deaths of despair – deaths due to suicide, alcohol, and drugs (Case, 2015; Deaton & Case, 2020), it is ever important to study the economy of addictive drugs. It is well documented that the origin of the opioid crisis in the 1990s was due to supply side factors (Case & Deaton, 2022), namely aggressive marketing campaigns and relaxing of restrictions by healthcare providers and the pharmaceutical industry that led to higher prescriptions of opioids such as oxycodone (Maclean, Mallatt, Ruhm, & Simon, 2020). In later years, the illicit opioid market flourished in parallel with heroin becoming popular in the 2010s. With rising heroin prices (Case, 2015; Maclean et al., 2020), there was a surge in synthetic opioids such as fentanyl which is about 50 times more potent than heroin. More recently, adulterated products are gaining popularity, notably with the incorporation of powerful veterinary tranquilizer compound xylazine (Johnson, Pizzicato, Johnson, & Viner, 2021). The opioid crisis has come with a huge cost to the US economy. In just 2017, the economic burden for the lives lost to opioid overdose was estimated around $480.7 billion (Florence, Luo, & Rice, 2021).

With the established supply side factors, attempts to regulate prescription opioids has the potential to push users to the illicit market (D. Cutler, Deaton, & Lleras-Muney, 2006; Miron, Sollenberger, & Nicolae, 2019). There is evidence supporting this – opioid prescriptions have been falling since 2011, but there has been no correlated reduction in deaths (D. M. Cutler & Glaeser, 2021). Therefore, the motivation behind this study is an interest in looking at what component of mortality reduction, if any, is explained by statutory laws since these were an important factor in the origin of the crisis. Even today there is no federal law regulating prescriptions, with most state policies being enacted only around 2017 (Davis, Lieberman, Hernandez-Delgado, & Suba, 2019). Although there is some evidence of prescriber guideline mandates being effective in curbing excessive misuse (Grecu, Dave, & Saffer, 2019), there is little research on the effect of actual supply limiting policies. Also, while such policies have been shown to reduce prescriptions and dosage, studies on ultimate health outcomes such as mortality is limited. This thesis aims to contribute towards the discussion about the efficacy of supply limiting policies, and will produce evidence towards the hypothesis whether these kind of policies drive substitution towards other illicit and dangerous substances. Furthermore, a current research gap according to the US Department of Health and Human Services (2020) is that studies need outcome measures before the intervention, which is what I aim to do thus contributing methodologically also.

Using difference-in-differences (DiD), this study estimates the effect of a staggered policy treatment on overdose deaths. The main results are that these laws show no effect on overdose deaths; estimates of reductions range from 3.2-9.9% but are not statistically significant. There are heterogeneous effects by age group – overdose deaths decrease by 63.3% and 43.2% for age groups 15-24 and 25-34 respectively, and effects are decreasing in age. Importantly, these effects are delayed in response to exposure to the policy intervention. The rest of this paper is structured as follows: Section 2 details the roll out of opioid prescription limiting laws in select US states, Section 3 and Section 4 present the data and methodology used respectively. Section 5 tests the plausibility of assumptions related to DiD and highlights trends in the outcome. Section 6 presents the results obtained with a study of heterogeneous effects. Section 7 concludes.

2 Statutory Opioid Prescription Limits

2.1 Overview

Drugs are federally controlled substances in the US as per the Controlled Substances Act enacted in 1970 (United States Congress, 1970). This act functions as a comprehensive regulatory framework for drugs in the country, encompassing their manufacture, use, possession, and distribution. It consolidates prior drug laws and establishes a system of classification for controlled substances. Drugs are placed into one of five schedules based on criteria including potential and extent for abuse, addiction, health effects, current research, risk to public health, etc. Schedule I drugs have high potential for abuse, have no medical uses, and a notable lack of safety standards for their use. Heroin is the only opioid in this schedule among non-opioid drugs such as marijuana and various psychedelics. Schedule II drugs also have a high potential for abuse and addiction with accepted but restricted medical use. Most strong opioids fall in this category i.e., those that are used for treatment of severe pain – codeine, fentanyl, morphine, oxycodone, oxymorphone, methdanoe, levorphanol, hydrocodone, hydromorphone, etc. Schedule III drugs have lower risk of potential abuse and addiction, and wider accepted medical use. Opioids in this schedule include buprenorphine, an opioid antagonist to treat addiction, and dihydrocodeine; they can include lower doses of Schedule II opioids. Schedule IV drugs have low potential for abuse, and have wide medical use such as tramadol for moderate pain. Lastly, Schedule V drugs have lowest potential for abuse and currently accepted medical use. These include drugs with small amounts of opioids such as codeine or diphenoxylate for treatment of diarrhoea.

According to the Department of Health and Human Services (2020), a prescribing limit on a drug is a regulation that checks its dosage quantity or number of days supplied, and if exceeding on either front requires a mandated action in the form of prior authorisation, drug testing, etc. Such prescribing limits have been recommended since the early 2000s by the Department of Veterans Affairs and Department of Defence, and since 2016 by the Centers for Disease Control and Prevention (CDC). Following this, individual states enacted prescription limiting laws between 2016 and 2019.

2.2 Timeline and Evolution of Regulations

Since the 1990s, manufacturing and prescription of opioids has increased by several magnitudes owing several different factors, including lack of caution in prescription of addictive pain medicines coupled with aggressive marketing of new opioids. The use of drugs such as methadone and oxycodone in everyday outpatient settings, albeit more expensive, became popular during this time due to lack of evidence for opioid misuse and addiction. A reason for this was agitation from practitioners and advocacy groups that pain treatment was insufficient in the country. Citation of a now infamous 1980 letter in the *New England Journal of Medicine* was used as evidence that opioid addiction among patients in hospitals was non prevalent (Porter & Jick, 1980; Compton & Jones, 2019). Subsequently, as prescription culture evolved to increasingly prescribe opioids for longer durations and higher doses, prescriptions for Schedule II opioids such as hydrocodone and oxycodone (infamously introduced by Purdue Pharma in the market in 1996) products increased from approximately 76 million in 1991 to nearly 207 million in 2013 (Volkow et al., 2014). Furthermore, global consumption of opioid analgesics, mainly morphine, fentanyl and oxycodone increased more than two and a half times in the period 1998-2008 predominantly driven by Europe and the US (International Narcotics Control Board, 2009).

Currently, at the federal level in the US there are no laws that enforce prescription limits on opioids, but some guidelines have existed prior to the enactment of the state policies. The first prescription guideline was published in 2003 by the Department of Veterans Affairs and Department of Defence for chronic pain treatment by general clinicians. This was a time when opioid sales had surged by several magnitudes in the past decade due to lobbying in the pharmaceutical industry by companies such as Purdue Pharma, the erstwhile manufacturer of oxycodone which was one of the most popular prescription opioids along with hydrocodone (Department of Veterans Affairs and Department of Defense, 2003). The guidelines preach general caution and comprehensive assessment in the course of pain treatment, providing a set of algorithms for prescribers to follow. It does not recommend any

particular limit to dosage or prescription. As the opioid epidemic took centre stage in the following years, in 2016 the CDC guidelines were fashioned and subsequently updated in 2022. These guidelines were far more comprehensive, and served as the benchmark for state policies to follow. Instead of only focusing on chronic pain, it addressed treatment of short term pain as well, referred to as acute pain (duration of less than one month) and sub-acute pain (duration of one to three months). Among other recommendations, it stresses that non-opioid therapies are equally valid in treating most kinds of acute pain, immediate release opioids and low dosages are ideal. Also, clinicians should discuss the advantages and dangers of the treatment with patients within one to four weeks of beginning opioid therapy for sub-acute or chronic pain, or after increasing dosage (Dowell, Ragan, Jones, Baldwin, & Chou, 2022).

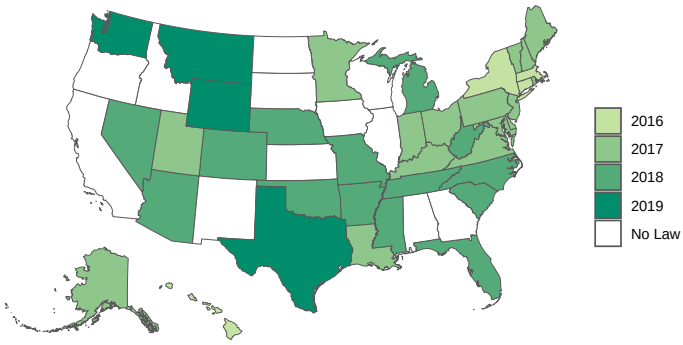

Figure 1: *Enactment of statutory laws limiting opioid prescriptions, during 2016-2019.*

Concurrently with the CDC guidelines, Massachusetts in 2016 became the first state to enact a prescription limit with the force of law. In a staggered fashion, by 2020, 38 states had implemented similar laws focused on the duration of acute pain. There is large variation in the types of opioids these laws pertain to. For the most part, they explicitly refer to Schedule II opioids. Sometimes, there is no strict mention of any particular classification. The laws exist with respect to initial days supply or minimum dosage but with several caveats. For example, there are differentials in relation to patient characteristics – whether they are a minor or an adult, a surgery patient, those who were prescribed opioids in the past 12 months, etc., or type of prescriber – optometrist, dentist, etc.

The most common provision of these laws among all 38 states is a seven day limit on a prescription,

either initial or at a given instance. The range is from three days to 30 days. Only two states, Maine and Washington, include provisions for sub-acute and chronic pain within 30 days. Furthermore, 14 of the states include provisions for limits on daily dosage, ranging from 30 to 350 morphine milligram equivalents (MME), a scale used to measure the potency of an opioid dose relative to morphine which is a very strong pain reliever. The potency of morphine is well established from historical observation of severe pain treatment. Although states continuously update the provisions of their policies, as of the available research for this study, no new opioid prescription limit laws enacted after 2019 have been identified.

Figure 1 maps the implementation of the laws within the study period by their respective year. Note that the map does not include states that passed a law prior to the study period; there is only one state that fits this bill – an Illinois law in 2012 set a 30 day limit on prescriptions for all Schedule II drugs at any one time. Discounting Illinois (which is included in the control group of thirteen states), four states implemented a law in 2016, sixteen states in 2017, fourteen states in 2018, and four states in 2019. The rest of the thirteen states with no laws constitute the control group.

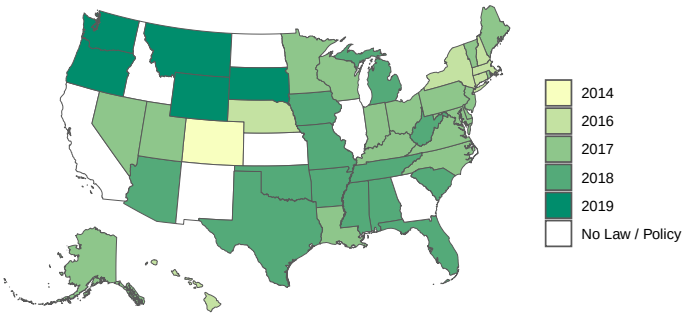

Figure 2: *Enactment of the earliest law or Medicaid policy limiting opioid prescriptions.*

To establish a general pattern, Figure 2 shows the first enactment of any kind of opioid prescription limiting guideline even if it was not a law. These non-law policies are Medicaid guidelines for prescribing opioid analgesics, whose consideration is salient in assessing state-wise policy patterns as those insured under it are vulnerable to high risk prescribing (Tormohlen et al., 2022). It can be observed that the enactment of Medicaid guidelines seem to shortly precede the laws in the respective

states. While these prescription limiting regulations directly aim at reducing high risk opioid prescribing and thereby offset the effects of the epidemic, another set of policies aim at fostering higher levels of precaution among prescribers. Several states have historically implemented prescription drug monitoring programmes (PDMP) beginning in the early 20th century, even before electronic records were available. A PDMP is essentially an information database that physicians are recommended to consult before issuing prescriptions.

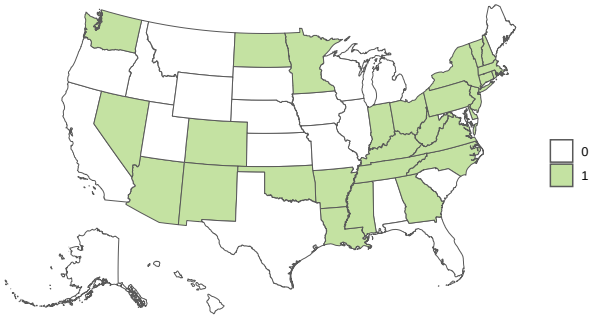

Figure 3: *Existing PDMP mandates prior to study period.*

Figure 3 shows the states with an existing PDMP mandate prior to 2016, where prescribers are required by law to consult or update an online database to track patient prescriptions. States with an existing PDMP but no mandate are not shown, as there is significant heterogeneity in the elements and implementation of these policies (Finley et al., 2017). Results of supplementary analysis using timing of Medicaid guideline implementation as predictor and heterogeneous analysis by presence of PDMP mandate is presented in the Appendix and discussed later. Overall, the laws and regulations also aim to deter behaviours that enable opioid misuse such as doctor shopping behaviour. Empirical evidence supports this assertion (see Grecu et al. (2019)), as such behavior has been identified as a crucial factor to the opioid crisis and resulting deaths (Compton & Jones, 2019).

2.3 Literature on Health and Economic Outcomes

The literature on examining health and economic outcomes can be broadly divided into two groups. Coming chronologically first, one group is concerned with deriving novel insights from past data on mortality, wages, prescriptions, etc. and therefore contextualising the opioid crisis. Case, Deaton, and Stone (2020) disentangle age and cohort effects by regressing survey reported pain in the US

and globally on year of birth and age indicators. Consequently, they find empirical support that age effects follow a universal trend but cohort effects are exacerbated in the US among those without a bachelor's degree. On the origins of the crisis in the 1990s, addiction and overdose among the less educated was almost in its entirety driven by pharmaceutical companies in an economy declining since the 1970s. Indeed there was no fundamental cause to the crisis. These companies targeted areas with higher amounts of pain (due to reasons such as job loss in the manufacturing sector primarily, declining unionisation, and rising healthcare cost) thus spurring a domino effect of addiction among the less educated. Empirically, the crisis was worse in areas with more pain and poorer mental health and higher opioid shipments (D. M. Cutler & Glaeser, 2021). At present, there is evidence for rising morbidity as well (Compton & Jones, 2019).

The second strand of literature is concerned with studying the causal effect of policy interventions, emphasising the role of supply side side origin of the crisis. So far, there seems to be only moderate effects of supply side policies in most studies of PDMPs, cap laws, insurance policies, clinical guidelines, prescriber education, and health system interventions (Haegerich, Jones, Cote, Robinson, & Ross, 2019). Since PDMPs were one of the earliest interventions, there is extensive research about their efficacy. Fink et al. (2018) review multiple studies on the same and find that there is small and inconclusive evidence that PDMPs reduce fatal overdoses. This is largely driven by studies focusing either on voluntary PDMP policies or mandates, the latter providing better evidence. Furthermore, analysis by age group suggests that effects are concentrated among younger to middle age groups (Grecu et al., 2019). Using DiD, Earlywine, Hadland, and Raifman (2020) also find that PDMP mandates reduce injection drug use among adolescents by a minuscule amount four or more years after implementation. This is encouraging given that these groups are driving much of the mortality reversal seen in recent times as studied by Case and Deaton (2015).

DiD is a popular method to evaluate opioid policy effects. Lowenstein et al. (2020) compare the frequency of new prescriptions after a prescription limit law implemented in New Jersey compared to Pennsylvania in a two year study period, and pick up small decreases in opioid dosage per prescription but no change in refill rates or patient encounters within 30 days post-intervention. The study of Tormohlen et al. (2022) mirrors this study the closest since they apply the same methodology as here. Comparing 32 treatment states to 17 control states, they find no effect on the proportion of people receiving prescriptions, dose or duration of prescriptions, and proportion of prescriptions for

overdose. Among dentists in particular, Chua, Nguyen, Waljee, Nalliah, and Brummett (2023) over a six year study period and 42 states do not find reductions in durations of prescriptions by dentists since median prescription duration amoing dentists were already lower than the statutes provision (three compared to a limit of seven).

There is a common critique across these DiD studies – small study periods are not conducive to picking up dynamic effects i.e., any delayed response in the outcome after long exposure to treatment. In Tormohlen et al. (2022), there is only one year of post law data, so barely any delayed effect is picked up which may explain their null result. However, even studies using other estimation methods do not find any effect; McGinty et al. (2022) use augmented synthetic control analyses to estimated cap laws' impact on outcomes.

Two key comments can be made. First, there is no uniformity in the structure of these causal studies. The time period and number of states considered in each study varies, inducing bias. The same type of state law may have a different effect on outcomes in one state versus another due to differing provisions within the laws or differential implementation. Second, many states implemented multiple opioid prescribing laws at or around the same time, making it difficult to isolate the effects of the laws of interest on outcomes. This paper contributes to the literature in the following ways – in terms of method, it looks at a much larger region of states than previously studied (all states and District of Columbia except Puerto Rico), and a larger time period to tease out delayed treatment effects. Also, the fraction of literature studying prescription cap laws is low (Haegerich et al., 2019), and therefore this study adds to this body. In addition, I look at overdose deaths, a rarely studied outcome variable. Most studies focus on the effect on prescriptions. Accounting for changes in opioid prescribing rates, I look at deaths in the heterogeneous analysis of reductions in the intensive margin (the length of prescriptions (Maclean et al., 2020)), which is the ultimate health outcome that can be studied thereby informing future policy measures.

3 Data

3.1 Data Access

I use publicly available data from the National Center for Health Statistics (NCHS) on drug overdose deaths. The data on deaths include different drug categories but are predominantly opioids which

comprise historically abused drugs such as heroin, oxycodone, and fentanyl. It also includes deaths due to psycho-stimulant drugs. Drug overdose deaths were identified from the raw file by International Classification of Disease, Tenth Revision, (ICD-10) codes ranging from T40.1 to T40.4 for opioid related deaths and T40.5 and T43.6 for cocaine and methamphetamine respectively which unfortunately cannot be disentangled from the opioid data. In a subsequent section on heterogeneous effects by age group I constrict the analysis only to opioid deaths using a different data source which shows that relative trends are unaffected. The NCHS data is aggregated at state-year-month level in the period 2015 to 2023, and I will further aggregate it to state-year level. The dates for state opioid prescription laws and regulations are obtained from US Department of Health and Human Services (US Department of Health and Human Services, 2020) and state government reports respectively (Davis et al., 2019). This includes general statutory limitations and exceptions for opioid prescriptions relating to pain treatment, as well as interventions implemented by state Medicaid agencies. Covariate data is obtained from public sources (see the Appendix for a description of data procurement).

The raw NCHS dataset contains overdose deaths given as a '12-month ending period' value, meaning that each record is the number of deaths occurring in the the 12-month range ending in the indicated month, it would not be possible to perform the estimation at the year-month level. A potential solution to this would be to interpolate the data points for each month by looking at the trend in the data, but this would further add noise to the estimation. Therefore, the analysis is presented at year level aggregation.

3.2 Summary Statistics

The final dataset has 459 observations (51 states over 9 years, from 2015 to 2023). Table 1 presents summary statistics. 'State_ID' is a numeric ID for each state. 'LogDeaths', the outcome of interest, is the log of the number of overdose deaths in the given year. 'LawDate' refers to the date of enactment or update of a state statutory limit on opioid prescriptions. 38 out of 51 states passed such a law in the period 2015-2023. For the analysis, the timing of the enactments are year cohort aggregated – laws passed in the latter half of a given year are considered to be enacted at the beginning of the subsequent year (Tormohlen et al., 2022).

The set of controls to reduce standard errors in estimation are as follows. 'ExistingPolicy' and 'ExistingPDMP' are binary variables indicating whether there was any extant policy regarding prescrip-

tion limits or the presence of a PDMP in the state prior to the study period respectively (Department of Health and Human Services, 2020). 'MedicaidPolicy' is an indicator if the implementation of a Medicaid quantity limit occurred during the study period (The Pew Charitable Trusts, 2016). 'PhysicianDensity' is the average number of active physicians in patient care per 10,000 resident population in the state. 'OpioidPrescribingRate' is the average fraction of Medicaid overall claims for opioid prescriptions across the study period, to account for trends in opioid prescriptions. 'Population' is the average state population. Since these control variables are not time varying (they are averages across the study period), they can be used for looking at heterogeneous effects also by splitting the dataset according to quantiles.

Variable	Mean	Std. Dev.	Min	Max
State_ID	-	-	1	51
Year	-	-	2015	2023
Deaths	1,572.100	1,706.800	57.000	11,899.000
LogDeaths	6.791	1.164	4.043	9.384
ExistingPolicy	0.118	0.323	0	1
ExistingPDMP	0.627	0.484	0	1
MedicaidPolicy	0.431	0.496	0	1
LawDate	-	-	2016	2020
PhysicianDensity	28.821	8.547	19.050	73.450
OpioidPrescribingRate	4.401	1.298	2.101	9.021
Population	6,362,631.000	7,150,630.000	573,948.000	38,607,181.000

Table 1: *Descriptive statistics [N = 459].*

4 Methodology – DiD in Multiple Time Periods

The canonical static two way fixed effects model is specified as

$$Y_{it} = \alpha_i + \alpha_t + \beta PostTreatment_{it} + \varepsilon_{it} \tag{1}$$

Where Y_{it} is the outcome for state i in year t, α_i and α_t are state and year fixed effects, $PostTreatment_{it}$ is an indicator equalling 1 for all years after the policy in treated states. ε_{it} are state level clustered standard errors. However, to estimate average effects by length of exposure to treatment an event study design is more appropriate. Analogous to Equation 1 the dynamic regression is specified as

$$Y_{it} = \alpha_i + \alpha_t + \sum_{k=-4}^{k=-1} \mu_k + \sum_{k=1}^{k=6} \mu_k + X_{it} + \varepsilon_{it} \tag{2}$$

Where μ_k is an indicator for the time period relative to the policy intervention period, X_{it} is a set of controls. It is assumed $\mu_{-1} = 0$ so that all coefficients represent differences in outcomes relative to the period before the policy. In presence of a staggered treatment adoption i.e. treatment in multiple time periods, Callaway and Sant'Anna (2021) develop an approach to estimate average treatment effects per year. The aggregated causal parameter, average treatment effect on the treated (ATT), is non-parametrically point identified. Asymptotically valid inference is done using a bootstrap procedure to obtain confidence intervals and standard errors.

The main motivation for using this kind of method is to see how the outcome varies with length of exposure to the policy treatment i.e. to see if there are any delayed effects. There are some identifying assumptions. Identification hinges on the implementation of prescription limiting policies in an as-if randomised setting (Colonnelli & Prem, 2022; Roth & Sant'Anna, 2023). The corollary of this is limited treatment anticipation where units cannot expect treatment with prior knowledge. A qualitative assessment of parallel trends is conducted in viewing averages of outcomes overdose deaths and proportion of deaths between treatment and control, and verifying that the average effect pre-treatment is around zero in the event studies.

5 Identification – Effect of Prescription Limits

To study the effect of the prescription laws on overdose deaths, it is imperative to see how deaths evolved during the study period. Figure 4 shows the trend in state average deaths and proportion of deaths for the study period.

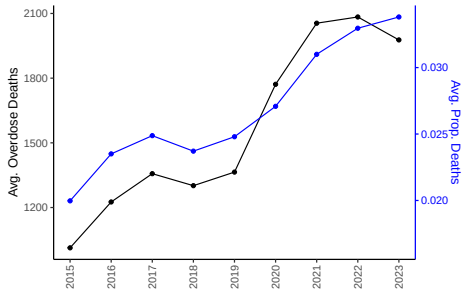

Figure 4: *State average outcomes by year.*

There are two notable dips in the increasing trend, the first in 2018 and the second in 2023 for average deaths. The former may be an effect of the major Centers for Disease Control and Prevention

(CDC) opioid guidelines in 2016. For the latter, recalling that for the thirty eight treatment states the prescription laws were enacted from 2016 to 2019, there is overall no clear demarcation of decrease in deaths after the laws. However, there are the beginnings of a decrease in the slope of both deaths and proportion of deaths beginning after 2020.

From Figure 5 it can be observed that while both control and treated states followed a similar trend in outcomes, treated states had higher recorded deaths on average.

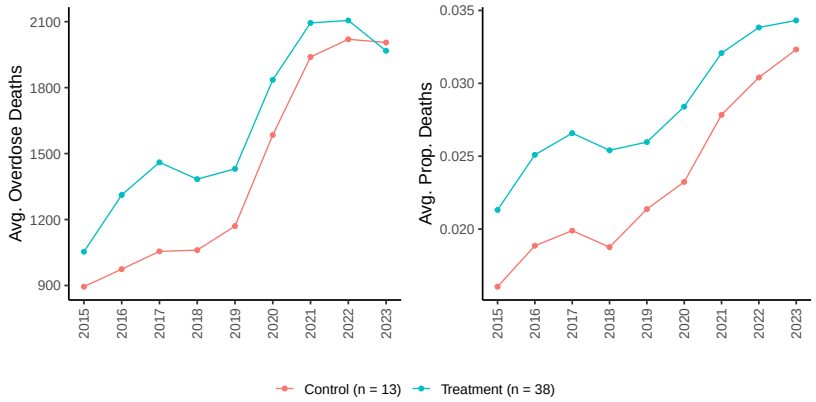

Figure 5: *Average outcomes by year in treatment and control states.*

Also since laws passed in a state are a function of their respective political leaning, we can study how democrat or republican states fare in the present context to assess whether to control for this. Aside from absolute levels, there does not seem to be differential trends.

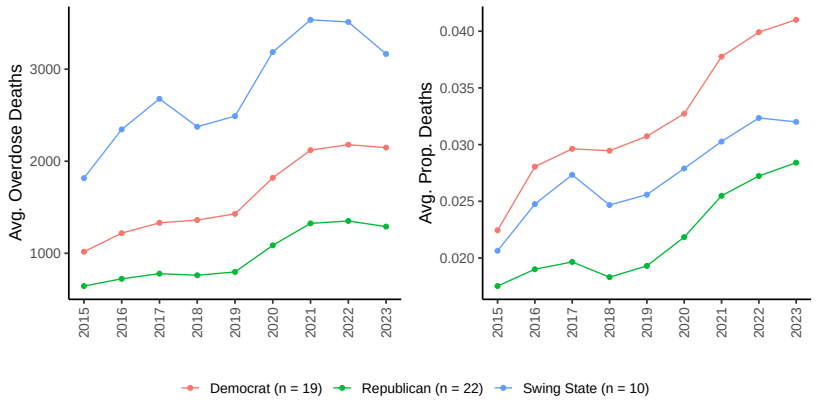

Figure 6: *Outcomes by political leaning.*

Table 2 shows the number of states that enacted prescription laws based on their political orienta-

tion. We cannot really say there is a significant difference in implementation based on states' political orientation.

Political Leaning	Law	No. of States
Democrat	0	5
Democrat	1	14
Republican	0	6
Republican	1	16
Swing State	0	2
Swing State	1	8

Table 2: *Law implementation by state orientation.*

From all these plots, especially Figure 5 with respect to treatment and control, we can infer that trends remain similar for various groups around and prior to the implementation of prescription limits providing evidence for parallel trends on average. In addition, the enactment of laws does not appear to depend on any particular variable for example with the political atmosphere of the state, and thus it can be argued that treatment is as if random which is sufficient for point identification of the ATT (Roth & Sant'Anna, 2023).

Overdose deaths in the NCHS data are based on the information on the cause of death such as unintentional, suicide, homicide, and undetermined (National Center for Health Statistics, 2023). The drug categories for multiple cause of death included are natural opioids and synthetic analgesics which include opioids from all the aforementioned drug schedules.

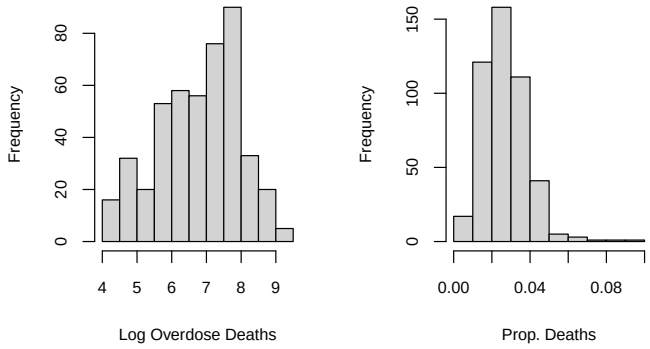

Figure 7: *Distribution of outcomes.*

The analysis is conducted for two outcome variables – the log of overdose deaths (*LogDeaths*), and

the share of overdose deaths in total state mortality (*PropDeaths*). *LogDeaths* focuses on percentage changes in deaths as estimated by DiD, while *PropDeaths* focuses on the fraction of the population affected. Figure 7 plots the distributions of the two outcomes respectively.

The following model specifications are studied with incremental addition of control variables – beginning with policy relevant variables: The presence of an existing prescription limiting policy in the state prior to the study period, the presence of an existing PDMP mandate, and the enactment of a Medicaid policy during the study. Then, healthcare and medical environment variables: physician density and opioid prescribing rates. Fourth, demographic and socioeconomic factors: age adjusted death rate (per 100,000) and state political leaning. Finally, the inclusion of environmental factors: average temperature in the state across the study period to account for differences across states.

6 Results and Discussion

Table 3 presents the results of the models. Evidence for parallel trends is not unambiguous, since the pre-treatment average effects are not distinctly near zero but the confidence intervals include it. There appears to be a noticeable change in the post-treatment effects which are mostly negative. For *LogDeaths* it can be observed that the addition of controls reduces the effect size in general. With the exception of temperature, which seems to blow up the ATT but the effect is non-significant. The effect of temperature may be obscured in the analysis because the data is not granular enough to pick up yearly trends (and covariates are time invariant averages across the study period). It appears that controlling for healthcare and demographic variables is most appropriate, the latter eliminating the statistically significant result. From specifications (1) to (4) it can be inferred that overdose deaths decreased on average by 3.2-9.9% in states that enacted prescription limiting laws compared to the control states.

	(1)	(2)	(3)	(4)	(5)
LogDeaths	-0.081 (0.044)	-0.099* (0.042)	-0.086* (0.041)	-0.032 (0.093)	-0.396 (0.237)
PropDeaths	-0.0018 (0.0019)	-0.0023 (0.0021)	-0.0018 (0.0015)	0.0006 (0.0024)	-0.0018 (0.0031)
Policy controls	N	Y	Y	Y	Y
Healthcare controls	N	N	Y	Y	Y
Demographic controls	N	N	N	Y	Y
Environmental control(s)	N	N	N	N	Y

* 95 % CI outside zero.

Table 3: *Effect on LogDeaths and PropDeaths [ATT (Bootstrap standard error)].*

The picture is similar for *PropDeaths*. The effect sizes are minuscule, ranging from a decrease in proportion of overdose deaths by 0.0018 to an increase of 0.0006. Figure A.1 and Figure A.2 plot the dynamic effect (event studies) from the respective specifications for both outcomes. The reason behind the inclusion of temperature as a covariate leading to the event study showing only three years of pre-policy estimates instead of four could not be established. Evidently, the results in general are driven by the effects more than two or three years after the laws were implemented, suggesting a delay in response not observed by other studies due to shorter time frames. The duration of the delay might not be indicative of any underlying phenomenon but simply underscores reality in the sense that initial implementation of policy witnesses results in due time. As an exercise, the timing of Medicaid policies (which coincide with the timing of the limit laws) is used as predictor (Figure A.3). There is no effect, and the confidence interval is inconclusive.

6.1 Heterogeneous Effects – Analysis by Age Group

Here the analysis is presented for overdose deaths by age category, with data obtained from the same source, National Center for Health Statistics (NCHS). However, data is relatively limited at age category level compared to the aggregated data used for the previous estimates. There are 8 time periods, 2015-2022. There is only one data point for ages below 15 and insufficient data for ages above 75. Consequently, the model is estimated for six age categories where the bulk of observations lie. Beginning with data in age group 15-24: 47 states (35 treated, 12 control), 25-34: 51 states (38 treated, 13 control), 35-44: 51 states (38 treated, 13 control), 45-54: 49 states (38 treated, 11 control), 55-64: 48 states (37 treated, 11 control), 65-74: 43 states (33 treated, 10 control), 75+: 10 states (8 treated, 2 control).

Figure 8 presents the average deaths for each age category. It is interesting to note that average deaths are significantly lower for extreme age groups 1-4, 5-14, 65-74, and 75+. The bulk of deaths can be attributed to the age groups 25-34, 35-44, and 45-54. Evidently the youth factor is not a big contributor to deaths, seeing the large disparity in deaths between age groups 15-24 and 25-34.

Table 4 presents the average effect by age group, compared to not yet treated states. As *LogDeaths* produces very similar estimates to *PropDeaths*, only the estimates for the former are presented. Results are presented for specifications with and without covariates. Figure A.4 and Figure A.5 shows the corresponding event studies of the models with no controls and all controls respectively. Presence of

heterogeneous effects are evident – for age groups 15-24 and 25-34 overdose deaths reduced by 63.3 % and 43.2% respectively in the specifications with no controls. The effect of the treatment is decreasing across the age groups, with plausibly null effects for senior age groups.

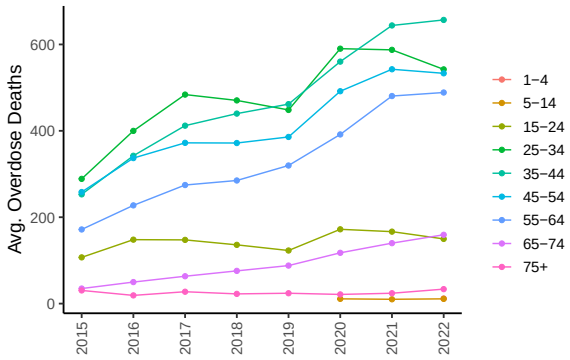

Figure 8: *Overdose deaths by age group.*

	15-24	25-34	35-44	45-54	55-64	65-74
No controls	-0.633* (0.286)	-0.432* (0.183)	-0.011 (0.127)	0.028 (0.102)	-0.058 (0.137)	0.034 (0.194)
All controls	-0.113 (0.348)	-0.368 (0.204)	0.017 (0.183)	0.018 (0.135)	-0.074 (0.177)	-0.542 (0.333)

* 95 % CI outside zero.

Table 4: *Effect on log deaths [ATT (Bootstrap Std. Err.)].*

It is interesting to look at other heterogeneous groups – Figure A.6 and Figure A.7 look at the effect of the laws on states with and without PDMP mandates as of 2015 and above and below mean prescription day supply states. There does not appear to be any differential effect in each case.

6.2 Comment on Covariates

While the results for the models with and without covariates differ significantly, this is not entirely unreasonable. As with the DiD estimation itself controlling for differences in treatment and control states before and after the treatment by design (Huntington-Klein, 2022), the addition of covariates can lead to incorrect estimates and large standard errors for a few reasons. First, there may be presence of multicollinearity which leads to higher standard errors. Second and more interestingly, there may be post treatment bias due to inclusion of covariates possibly influenced by the treatment – opioid prescribing rate for example could have likely been affected directly by the policies. So *prima facie* the models without covariates may be expected to be more accurate, and with only the inclusion

of covariates not affected by treatment, like state average temperature and pre-treatment political leaning, the results do not vary much.

7 Conclusion and Limitations

This study aims to find the effect of state opioid prescription limiting laws on overdose deaths in the US. The evidence found shows a small but non-significant effect on overall deaths. The estimated effect is driven by younger age groups and is reducing in age. Furthermore, longer exposure to the policy enables the identification of delayed effects, a result missing in earlier literature. Additionally, this study is unprecedented in that it uses a novel method to deal with studying the effect of a staggered policy treatment.

Some study limitations can be addressed that may influence the estimated effects. There is no insight into the geographic element of the opioid epidemic even though it has been documented (D. M. Cutler & Glaeser, 2021). Second, I do not account for confounding laws passed in the same periods, such as new PDMP mandates or even updated cap laws. Third, sex and education differences represent an early result of the literature (Deaton & Case, 2020), which are both not explored here. Another dimension addressed briefly is the choice of outcome. A common outcome is the frequency of prescriptions, and this is the first study to look solely at the effect on mortality. Due to lack of prescription data, trends in the frequency of prescriptions have not been assessed (the extensive margin from Maclean et al. (2020)). Finally, Lowenstein et al. (2020) hypothesise spillover effects of policies into other states or into illicit market procurement of opioids (Miron et al., 2019; Tormohlen et al., 2022) other than the fact that a null effect or small effect means the laws aren't working and people buy drugs from illegal dealers. I do not account for migration of people across states (Grecu et al., 2019).

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Appendix

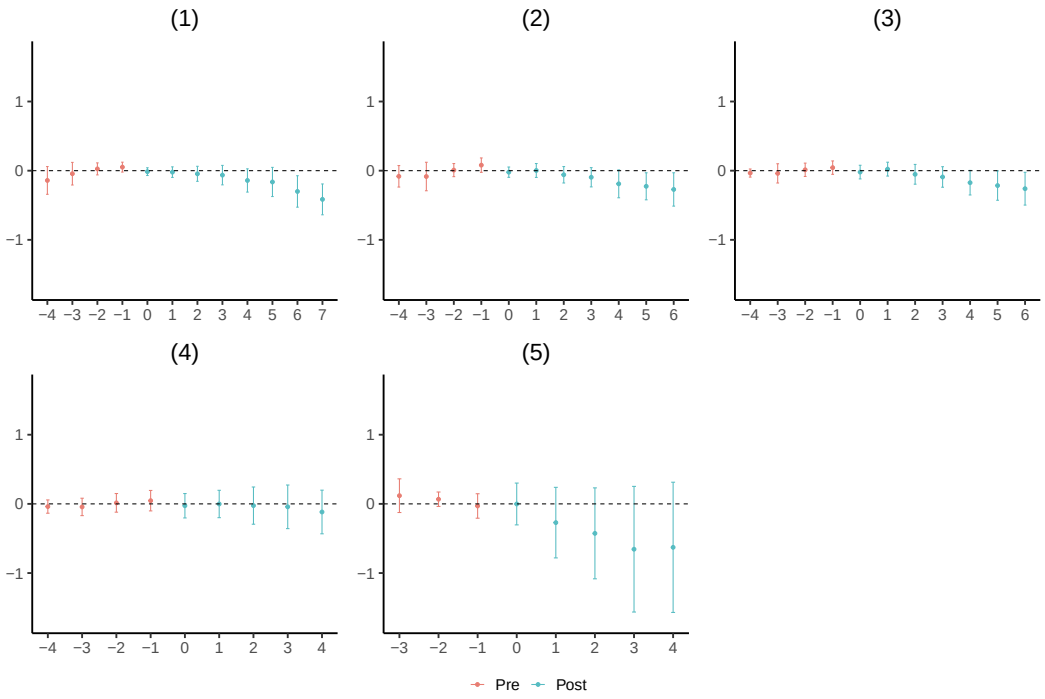

Figure A.1: *Event studies of all model specifications for LogDeaths.*

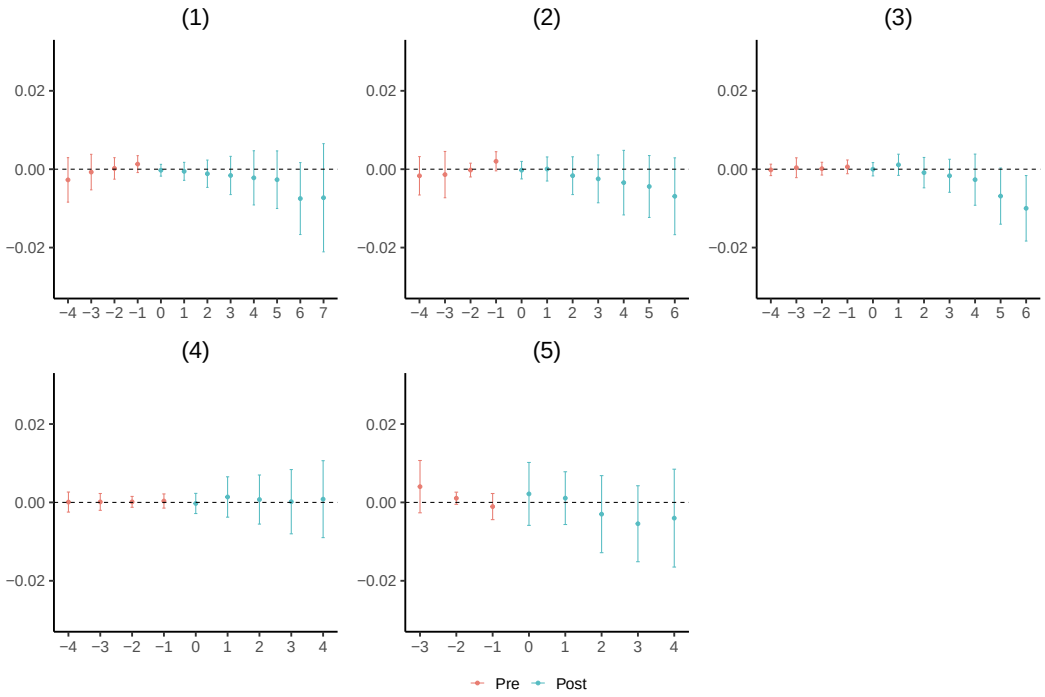

Figure A.2: *Event studies of all model specifications for PropDeaths.*

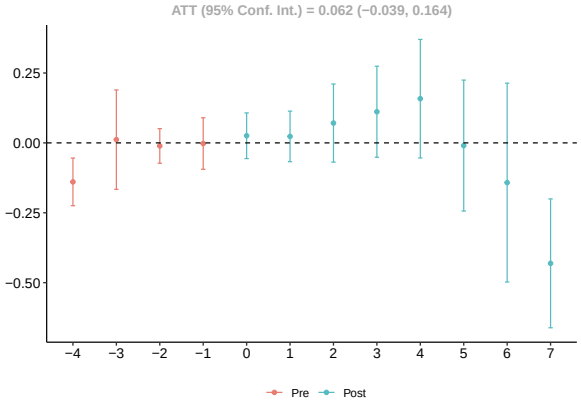

Figure A.3: *Effect of Medicaid limits on LogDeaths.*

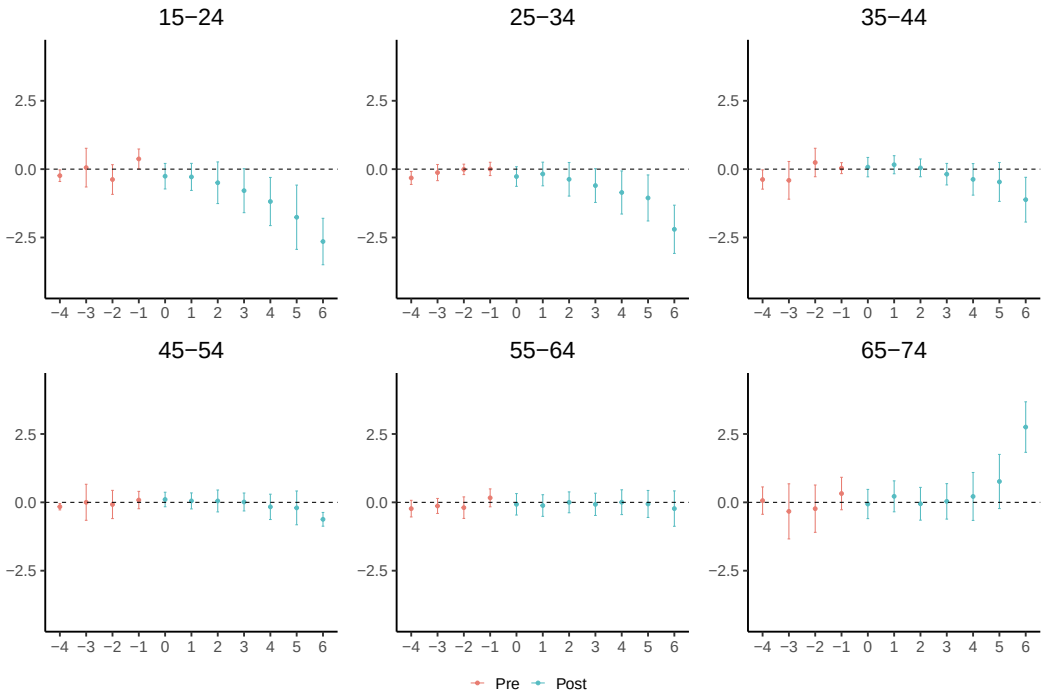

Figure A.4: *Average effect by age group on LogDeaths (no covariates).*

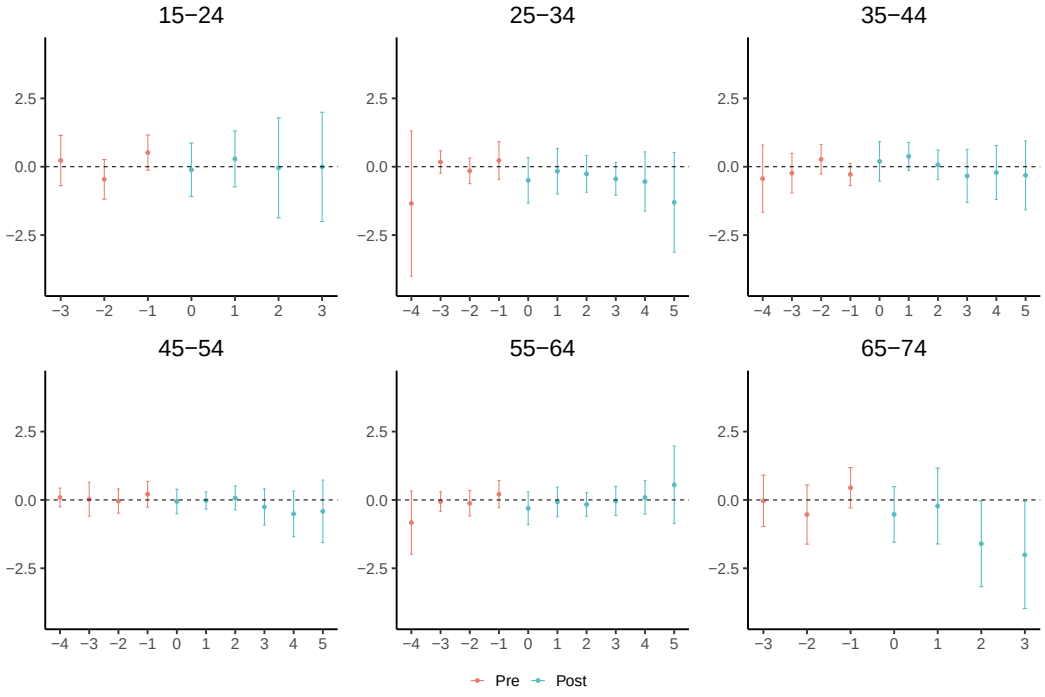

Figure A.5: *Average effect by age group on LogDeaths (all covariates).*

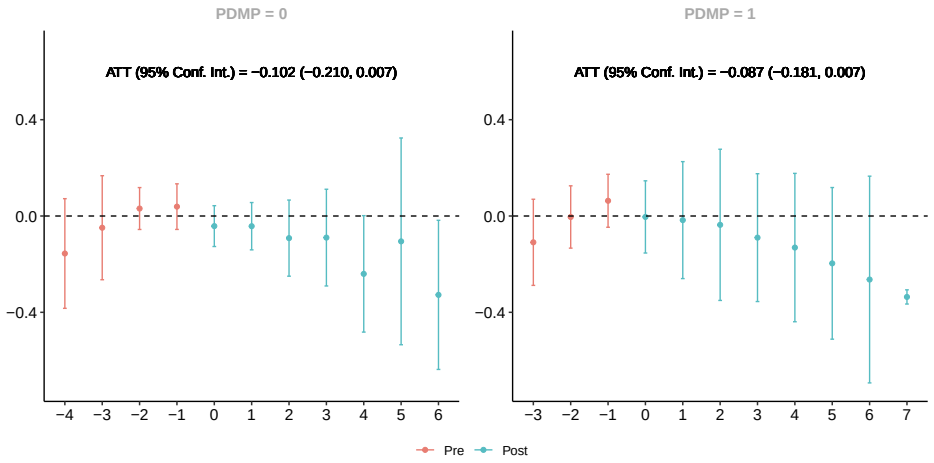

Figure A.6: *Effect on LogDeaths in states with or without PDMP mandates.*

Note: PDMP = 0 there are 23 states (13 treated, 10 control). PDMP = 1 there are 28 states (25 treated, 3 control).

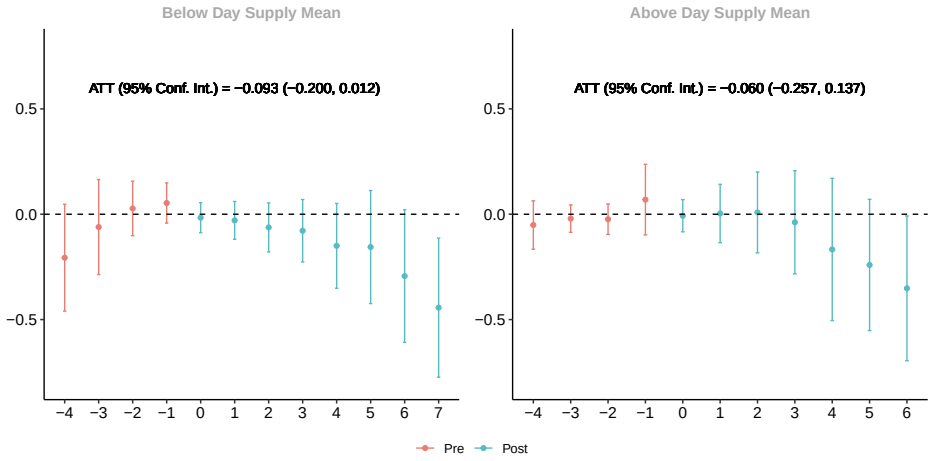

Figure A.7: *Effect in states above or below mean day supply.*

Note: The effect of the intervention is also looked at on average duration of mostly initial outpatient prescriptions as delineated by either new laws or existing medicaid guidelines or existing practices, as well as the effect in states below and above the median days' supply. Below mean day supply there are 41 treatment states and 9 control. Above mean day supply there are 10 treatment states, and 4 control. In regard to (Chua et al., 2023), my analysis with mean day supply is quite rough. First, they have prescription data whereas I have data on the implemented day supply limits. Also, I look

at mean instead of median, since the median is 7 and very few states vary from this. They do the same thing where they check among states that enforced 5 or 3 day supply or less, and among states greater than 5 or 3 day supply.

Data Sources

The cleaned datasets and code for replication of results can be found on my GitHub: `https://github.com/onirudh3/opioid`. Data were obtained from the following sources:

CDC Wonder (for age group-wise data): `https://wonder.cdc.gov/`

NCHS overdose data: `https://www.cdc.gov/nchs/nvss/vsrr/drug-overdose-data.htm`

PDMP dates: `https://www.pewtrusts.org/-/media/assets/2016/12/prescription_drug_monitoring_programs.pdf`

Physicial density: `https://www.cdc.gov/nchs/hus/topics/physicians.htm`

Opioid prescribing rate: `https://data.cms.gov/summary-statistics-on-use-and-payments/medicare-medicaid-opioid-prescribing-rates/medicaid-opioid-prescribing-rates-by-geography`

Population data: `https://data.ers.usda.gov/reports.aspx?ID=17827`

Age adjusted death rates: `https://hdpulse.nimhd.nih.gov/data-portal/mortality/`

Temperature data: `https://www.ncei.noaa.gov/access/monitoring/climate-at-a-glance/statewide/time-series`

State political leaning: `https://www.jagranjosh.com/general-knowledge/red-and-blue-states-in-us-1701677972-1`