# Cannabis Access and Opioid Harm in the Southwest U.S.

An Analysis of Medical Cannabis Legalization and the Opioid Abuse Environment of the Southwestern United States


*Author*

Mason Natale

*Student Number*

466407

*Supervisor*

Prof. dr. Anne Gielen

*Second Assessor*

Prof. dr. Dinand Webbink


A thesis presented for the degree of

*Master of Policy Economics*



School of Economics

Erasmus University Rotterdam

The Netherlands


January 19th, 2021

## Abstract

The United States has been suffering from an opioid epidemic for the last two decades, with dramatic rises in opioid mortalities, abuse, and addiction incurring significant burden and trauma for American society. This paper investigates how increased access to cannabis affects this opioid epidemic, particularly as a potential policy intervention to reduce opioid harm. Using US administrative public-access datasets, six Southwestern states (Arizona, Colorado, Nevada, New Mexico, Oklahoma, and Utah) are analyzed for changes in yearly opioid overdoses, opioid-related admissions to substance abuse treatment facilities, and prescription opioid distribution for the period of 1999 to 2015. Within this period, four of the six states legalized cannabis for medical purposes and saw the active operation of legally-protected cannabis dispensaries, allowing for a staggered difference-in-difference design focusing on these two changes to access of cannabis within a state. The analyses find no evidence that the two cannabis access changes are associated with decreased state opioid overdose rates, substance abuse admission rates, and prescription opioid distribution rates within the six Southwestern states between 1999 to 2015. These results stand in contrast with some previous research in to the relationship between cannabis access and opioid harm. While these findings could be indicative of a separate relationship between cannabis access and opioid harm for the six Southwestern states, issues with sample selection and the usage and limitations of the datasets could also have led to these disparate results.

## Acknowledgements

I am grateful for those who supported me during this thesis writing process. Dr. Anne Gielen provided me with pivotal advice, guidance, and encouragement throughout the many phases of this thesis, and was an excellent supervisor and mentor. I thank my friends and colleagues Arnstein Årdal and Benjamin Pico for the professional assistance they've given during this writing process. I thank my family and friends for the continued support they've granted me as I've worked on this thesis, especially the time my grandparents Judy and Joe Natale spent helping me proofread my writing.

*Keywords*

Medical Cannabis, Medical Marijuana, Opioids, Abuse, Overdose, Mortality, Policy Reform

*Definitions*

"MCL" = Medical Cannabis Law, "PDMP" = Prescription Drug Monitoring Program, "Opioids" = Painkillers, pain medication, and related legal or illegal drugs used to treat moderate to severe pain, "Southwestern States" = States of Arizona, Colorado, Nevada, New Mexico, Oklahoma, and Utah.

# 1 Introduction

The United States is currently in the grip of a drug epidemic - one that is persistent, destructive, and dynamic over time. Within the year of 2018, drug overdoses resulted in 67,367 deaths, a mortality count four times that of drug overdose deaths in 1999. Of these 67,367 overdoses, close to 70% were found to have involved opioids - a type of drug classification for painkillers, pain medication, and related legal or illegal drugs used to treat moderate to severe pain.[1] Since 1999, the United States has seen a dramatic rise of substance abuse and overdose deaths related to opioids, with total overdose deaths attributable to opioids having grown to almost 450,000 by 2018 (CDC, 2020). Estimates of societal costs have been substantial: Prescription opioid abuse and overdoses alone in 2007 are attributed to a $55.7 billion public burden, or 3.9% of the United States' 2007 GDP, including health care burdens from excess medical care and prescriptions, workplace costs stemming from lost earnings from death or opioid-driven unemployment, and justice system costs from increased criminal burden stemming from opioid usage (Birnbaum et al., 2011). Another estimate for 2013 finds a $78.5 billion public burden, 4.7% of the United States' 2013 GDP, and is similarly composed of health care burdens, criminal justice costs, and productivity losses (Florence et al., 2016). This public health crisis is hence referred to as the "US Opioid Crisis".

The beginning of this US Opioid Crisis in 1999 was initially marked with an explosive growth in abuse of prescription opioid drugs, such as oxycodone, hydrocodone, codeine, and more. This growth was attributed to a growing emphasis in the 90s on the aggressive treatment of pain, along with the diffusion of views amongst medical providers that prescription opioids held little addictive potential (Jones et al., 2018; Commission, 2017). Eventually, this upwards trajectory of prescription opioid abuse would flatten approaching 2010. However, from 2010 onwards non-prescription opioids, such as heroin and synthetic opioids like fentanyl, would see a rapid growth in abuse unlike that seen in the previous decade in which such "illicit opioids" showed only modest growth from 1999 to 2006 (Jones et al., 2018; Ruhm, 2018). The growth in harm attributable to these illicit opioids has not been uniform. Heroin overdoses saw substantial growth in per-capita rates from 2010 onwards, but much like what was observed with prescription opioids it has seen a flattened trajectory from 2016 to 2018. The current wave of opioid harm is being dominated by synthetic opioids, with overdoses per capita skyrocketing from 2013 onwards and still being observed on an upward trajectory as of 2018 (CDC, 2020). As such, the US Opioid Crisis is not only substantially harmful but also a dynamic societal burden, introducing extra complication to policy interventions.

Research in to what policy interventions may be effective in ameliorating this Opioid Crisis is thus prudent, including how medical cannabis legislation can factor in to policy responses. The discussion of how cannabis legislation can affect opioid harm outcomes is a relatively new one. Other policy responses, such as the establishment of prescription drug monitoring programs (PDMPs) have historically received more attention. However, a surge of research in to the effects of cannabis legislation on opioid outcomes

[1] This drug type is distinct from the drug type known as "opiates", which are drugs derived from the opium poppy plant and include opioids like heroin, morphine, and codeine. While both types refer to drugs used for pain relief, "opioids" encompass both opiates and a broader spectrum of non-opium poppy-derived pain relief drugs, such as fentanyl, hydrocodone, and oxycodone. More information can be found at https://www.medicinenet.com/script/main/art.asp?articlekey=11771.

has taken place within the last decade (Chan et al., 2020; McMichael et al., 2019; Powell et al., 2018; Shi, 2017; Bradford and Bradford, 2016), highlighting both the range of benefits such legislation can provide for addressing the Opioid Crisis and the potential fruit further research can bear for understanding the consequences of such a policy response.

The suggested mechanism underlying medical cannabis's potential to reduce societal outcomes of opioid harm is that of a substitute for usage of opioids for medicinal purposes. Cannabis has been found to be effective for treating chronic pain (Hill, 2015) and is also noted for being sought after for treating severe pain (Bowles, 2012; Nunberg et al., 2011). As such, legislation to make cannabis accessible for those in need of pain relief effectively lowers the 'costs' of acquisition, making it a more attractive option whenever an individual seeking pain relief considers using opioids. In pursuing such an option, they thus avoid the risk of entering into a destructive cycle of opioid addiction, as the predominant danger of opioid usage lies in their potent addictive potential when consuming them, spiraling into more extreme consequences of abuse and even overdose.

Cannabis has seen a remarkable shift in its role in US society throughout the last twenty years, becoming more acceptable for use over time. At the beginning of 1999, only three states out of fifty had legalized cannabis for medical usage. However, by the end of 2015 the number of states with MCLs, or Medical Cannabis laws, had increased to twenty-four, five (including the District of Columbia) of which had gone a step further and legalized cannabis for recreational usage (Chan et al., 2020; MPP, 2020). The number of states with legalized medical and recreational cannabis has only increased since then, encapsulating the majority of the United States. As such, further research on the influence of cannabis legislation upon the still occurring US Opioid Crisis can be informative upon a wide range of policy situations currently in place throughout the United States.

Given the multidimensional relevancy of this line of research, this paper will seek to test the following hypotheses through its design:

**Hypothesis 1: The passage of medical cannabis legislation is negatively associated with state rates of general opioid overdose, opioid abuse treatment, prescription opioid overdose, and prescription opioid distribution.**

**Hypothesis 2: The beginning of active operation of legally-protected dispensaries is negatively associated with state rates of general opioid overdose, opioid abuse treatment, prescription opioid overdose, and prescription opioid distribution.**

In doing so, I perform a case analysis of six "Southwestern" states, which includes Arizona, Colorado, Nevada, New Mexico, Oklahoma, and Utah, utilizing a staggered Difference-In-Difference design to compare their outcomes. These Southwestern states were selected for geographic proximity and relatively similar opioid harm outcomes, cannabis reforms, and observable characteristics. Of these six states, Arizona, Colorado, Nevada, and New Mexico see increased cannabis access with adoption of medical cannabis legislation and active operation of legalized dispensaries within the broadest sample time frame

of 1999 to 2015. These four states comprise the main "treatment group"[2] that all end up adopting the treatments at separate points of time within the sample period, whereas Oklahoma and Utah are incorporated as the "control group" with no passage of medical cannabis reforms.

The passage of medical cannabis legislation is one of two reforms increasing cannabis access in my design, the other being when the first legally protected dispensary of a state enters operation, an event which previous research has shown to increase access to medical cannabis and be impactful upon opioid harm outcomes (Pacula et al., 2015). As such, from this point forward when this paper refers to dispensaries and dispensary effects it refers to the status of a state having actively operated and legally protected dispensaries. The inclusion of two separate changes in cannabis access is done to highlight how different levels of accessibility to cannabis for consumption may affect measures of opioid harm. Within the subset of Southwestern states, I measure changes in opioid harm outcomes of interest, including overdoses and admissions for medical treatment related to opioid abuse. While most harm outcomes are investigated with regards to all opioids, I also investigate changes in the sub-category of prescription opioids for certain metrics such as overdoses. The dynamic composition of opioid types that have been involved in increasing outcomes of opioid harm leads to this distinction being both informative and presently relevant, especially with regards to the efficacy of any policy intervention. In addition, I will perform further investigation in to how these two treatments have affected the legal flow of prescription opioids from distributors to retailers and consumers.

The main results obtained from the design do not support any of the hypotheses, with no significant effects found for medical cannabis legislation or the active operation of legal dispensaries upon overall opioid overdoses, prescription opioid overdoses, opioid abuse treatment admissions, and prescription opioid distribution. In general, I do not find any analytical evidence that increased cannabis access has improved the opioid abuse environment of the Southwestern states. This contradicts findings of other research in to cannabis access as an effective policy intervention against state outcomes of opioid harm (Powell et al., 2018; Chan et al., 2020; Chu, 2015; Shi, 2017), but at least in the case of one of the six Southwestern states this lack of positive validation for cannabis policy efficacy is not unprecedented (Conyers and Ayres, 2020). Issues of multicollinearity in the design and shortcomings in the process of data acquisition may have affected the results, and a possibility of sample selection with the Southwestern states could also explain the divergence from previous works, if not necessarily invalidate it.

The paper is hence organized as follows. Section 2 introduces an underlying theoretical framework for considering the results. Section 3 provides a literature review related to legalized cannabis, opioid harm outcomes, their documented relationship, and other potentially comparable addictive substances. Section 4 discusses the institutional context revolving around the US Opioid Crisis, especially with regards to the Southwestern states. Section 5 describes the data utilized. Section 6 discusses the methodology and empirical strategy used. Section 7 discusses the results of the design. Section 8 provides sensitivity analyses for additional context of the main results. Section 9 discusses the limitations and implications of the results. Section 10 concludes.

[2]Some specifications see Nevada dropped from the treatment group and sample due to concerns of bias, this is discussed further in Section 7: Results.

## 2 Theoretical Framework

In discussing the findings of the main analysis, this paper will use the rational choice model of addiction from Becker and Murphy (1988), specifically the baseline specification noted in the appendix of Vale (2010), to provide economic intuition as to what mechanisms are driving changes in opioid outcomes. The first part of this model below is an individual's lifetime consumption utility function:

$$U(0) = \int_{t=0}^{T} e^{-\sigma} u[y(t), c(t), S(t)]dt, \tag{1}$$

$$u_y > 0, u_c > 0, u_S < 0, u_{yy} < 0, u_{yS} = 0, u_{cc} < 0, u_{cS} > 0, u_{SS} < 0$$

Subject to the following budget constraint:

$$\int_{t=0}^{T} e^{rt}[q(t) - p_y(t)y(t) - p_c(t)c(t)]dt = A(t), \tag{2}$$

$$\lim A(t)_{t \to T} = 0$$

Where a rational individual consumer at $t = 0$ maximizes their lifetime utility over lifespan $T$ via their concave consumption functions for a non-addictive representative good and an addictive good in $y(t)$ and $c(t)$, respectively. The choice of how much of either type of good to consume each period is affected by the non-addictive good price $p_y(t)$ and the addictive good price $p_c(t)$, and is constrained by current period income $q(t)$, along with the total amount of disposable income at period $t$, which is captured by $A(t)$. Savings are subject to a constant interest rate $r$. The consumer has a constant rate of current time preference $\sigma$, where an increase in current time preference leads to a discounting of future time periods. In this lifetime consumption model, addictive goods (such as opioids) in $c(t)$ have complex effects on welfare, including positive utility effects upon consumption in the immediate period, but indirectly negative effects on future period utility via accumulation of stock of "addiction", captured by $S(t)$, in the following period. As such, their utility function is also subject to investment equation (3), shown below:

$$S(t) = c(t) - \delta S_{t-1} + z \tag{3}$$

Where addiction $S(t)$ is increased by consumption of an addictive good in $c(t)$, the occurrence of an exogenous traumatizing event captured by $z$, and is decreased by the depreciation of previously accumulated addiction $S_{t-1}$ by a factor of $\delta$. This presents a flow formulation for how consumption of an addictive good or the occurrence of a traumatic event fosters addiction, which will dissipate over time. Period welfare is directly lowered by increasing values of addiction $S(t)$, representing the adverse consequences of addiction upon the health of an addictive consumer, but also increases the marginal utility of consumption of the addictive good, which incentivizes further consumption of the good - and further generation of more addiction. Consumption of a given level of the addictive good leads to a relative reduction of welfare when there has been greater consumption in the previous period. These two relationship dynamics between addiction and addictive consumption represent two keystone concepts of addiction: reinforcement and tolerance, respectively.

Equation 3 shows how consumption of an addictive good builds addiction, which then incentivizes consumption of the addictive good in Equation 1, thus formulating a cycle of addiction. The model also

includes an impetus for consumption of an addictive good outside of past consumption itself - an external shock or traumatic event, captured by $z$. This means that even without an internal motivator to cause an initial consumption of an addictive good and thus spur future consumption via stock accumulation, a rational, disinterested individual can still be pushed into a cycle of addiction. Of final note, the original model also allows for endogenous expenditures to reduce addiction growth - in other words, costly action that can be taken to avoid increasing future addiction. This channel of avoiding addiction was not included in Equation 4, as this paper intends to discuss an adjustment of the channel in order to theoretically formulate a role for medical cannabis in reducing opioid addiction.

This model and the way it formulates addiction is suitable for the paper's analysis of opioid harm outcomes. It offers a framework through which a rational, forward-looking individual can consider the benefits and drawbacks of addictive opioid consumption. Principally, it also offers a way to incorporate the motivator of pain via its formulating of the effect of external shocks $z$ on addiction - in other words, how individuals seeking pain relief can turn to opioid usage which thus puts them at risk of entering a cycle of addiction. At this juncture is where the role of medical cannabis, and thus medical cannabis policy, can make its entrance in to the model as a channel through which endogenous expenditures can reduce addiction growth. An exogenous event which normally causes addiction to grow in our model, our scenario being a shock of pain causing the individual to turn to opioids to ameliorate the pain, now has a varying effect on addiction growth, as instead the individual can substitute cannabis consumption for opioid consumption, and thus avoid the cycle of opioid addiction simulated in the model. This is shown by slight adjustment of equation (3) below:

$$S(t) = c(t) - \delta S_{t-1} + Z(t) \tag{4}$$

Where:

$$Z(t) = z + b(t), \tag{5}$$

$$z \geq Z(t) \geq 0; Z_b < 0, Z_{bb} > 0$$

Where $Z(t)$ formulates how the impetus of traumatic event $z$ can be reduced by additional consumption of cannabis in $b(t)$ with decreasing marginal returns. This is a slight adjustment to the Becker-Murphy model as it relegates the role of the endogenous expenditure on addiction growth reduction to a bounded, concave effect reducing the magnitude of the effect of an exogenous event of pain. This differs from Becker and Murphy (1988)'s implementation of endogenous expenditures as they do not have such constraints to reducing addiction growth.

Further discussion of the implications of this model upon interpreting the paper's results must be done with the many limitations of the model in mind. For one, the way addiction is formulated is highly stylized. The model of addiction stock accumulation as a representation of addiction has seen criticism as being too simplistic and not capturing the complexities of how addiction to substances is acquired (Vale, 2010; Skog, 1999). Previous literature, such as Skog (1999) amends the Becker-Murphy model's variable of addiction to depend on the quantity, frequency, and other objective features of an addictive good's consumption pattern, while also making a point that the consequences of addictive consumption

are more varied in the time frame and manner which they emerge. Another concern of the model comes with the economic concept of a constant discount rate (in this case present time preference), and how it interacts with addictive good consumption, a process which an individual with adequate information will know to be significantly detrimental in future periods for certain addictive goods. It's been argued that a model describing addiction should consider the discount rate as variable by certain criteria. Vale (2010), adding power to the significance of a traumatic event $z$ in pushing individuals to addiction, suggests an amendment to the rate of present time preference factor to be a function of both the trauma factor and age, as shown below:

$$\sigma = \sigma(z, A)$$

Where $\sigma$ is the present time preference factor, $A$ is the age of the individual, and $z$ is a variable quantifying the effect of the traumatic event on addiction $S(t)$. This amendment was made to both include evidence of time preference variation between age groups and to open up the time preference factor to being malleable by significantly traumatizing events. However, this proposed revision to the time preference factor in order to better explain how individuals rationally engage in consumption of addictive goods with potentially long-lasting consequences strikes upon another key issue of the model: individuals are assumed to have perfect information regarding the future consequences of consuming addictive goods. Other works have called in to question this assumption of perfect information for individuals considering consuming addictive goods, with Orphanides and Zervos (1999) proposing an updated model with a learning process for the addiction potential of a good, along with hetoregenous potential for addiction among individuals paired with subjective beliefs of one's potential as an addict. In more evocative words, such amendments have been called giving rational individuals the ability to regret choosing to consume addictive goods - something the original Becker-Murphy model lacks. The culminating criticism of the model that all the other limitations have led up to is that the model does not explain well how rational individuals suffering no traumatic events end up entering into a cycle of addiction - a significant issue which has been noted and addressed by later works involving the model (Vale, 2010; Orphanides and Zervos, 1999).

To close on this paper's usage of the Becker-Murphy model, it is prudent to note how its scenario of analysis - the opioid harm environments of Southwestern states before and after the passage of MCLs - allows me to avoid certain drawbacks of the model, while also acknowledging other limitations of its usage specific to our analysis. One way the paper's scenario provides a route to avoid a theoretical drawback of the model lies in the role medical cannabis legislation serves in my analysis. Medical cannabis is only approved for legal consumption via prescription for certain medical reasons, many of which fall under the scope in which addictive opioids may be prescribed or consumed. The medical circumstances leading to the prescribing of medical cannabis or prescription opioids serves as appropriate events leading to $z > 0$. For example, it is likely that the same chronic pain or post-operation pain which doctors would treat with medical cannabis or prescription opioids would also be circumstances in which an afflicted individual would be at risk of engaging in consuming said substances. Thus, I can circumvent the issue of explaining how an individual in our model would initially be motivated to enter in to a cycle of addiction. However, while the context surrounding an individual's option to consume medical cannabis does dodge one limitation of the Becker-Murphy model, it imposes another theoretical concern. As the paper intends

to include medical cannabis as a channel through which endogenous expenditures are spent to avoid or reduce addiction, I do not sufficiently factor in medical cannabis as a substitute good in its own right, with its own effects upon utility and preferences for the good defined for the individual. Additionally, I also do not consider cannabis to be an addictive good in the model, not generating its own addiction or deleterious effects upon the individual user in later periods - a rather controversial consideration in the United States, given its current classification by the federal government as a drug with high risk of abuse.[3] To put in to context how this would affect our interpretation of the analysis's results through the model, while currently the model would attribute all drops in opioid consumption, *ceteris paribus*, to individuals taking advantage of easier access to medical cannabis reducing the cost of endogenous expenditures to reduce addiction growth in $S(t)$, this interpretation would ignore the ulterior motive of individuals seeking to consume medical cannabis for pleasure or compulsion, thus overestimating the responsiveness of individuals to reductions in cost of means to avoid opioid addiction. With these considerations in mind, however, I argue for the explanatory power the Becker-Murphy model can provide for evaluating the effects of medical cannabis legislation on opioid addiction.

## 3 Literature Review

There is a wealth of research available on the potential and estimated effects of cannabis legislation on a number of social outcomes, such as impaired driving, hospitalizations, and more. A substantial portion of recent research is dedicated to the relationship between cannabis legalization and opioid harm. A consideration of this spread of literature will thus put the results of our paper in context of previous research in to the relationship between cannabis legislation and opioid outcomes, as well as the broader picture of the benefits and harms of such legislation.

The prospect of cannabis legislation providing relief to the damage of the US Opioid Crisis has sparked a considerable amount of research to determine what the true effects of such legislation are or could be. A sizeable portion of such work has found that cannabis legislation is associated with positive changes in opioid harm outcomes. Bradford and Bradford (2017) reports that from 2007 to 2014 states with MCLs were associated with lower usage of prescription drugs, including an average 11% reduction in pain drug usage, in fee-for-service Medicaid. It claims that states with MCLs saved on average $19.825 million on fee-for-service Medicaid spending in 2014, and also claims that if the entire Medicaid program faced the same associated decline in prescriptions, the average state cost savings on Medicaid could've approached $76.25 million in 2014. Bradford and Bradford (2016) found similar associations in prescription reductions and cost savings related to Medicare Part D between 2010 to 2013.

Moving away from Medicaid-specific data, Shi (2017), using a linear time-series regression, finds that from 1997 to 2014 all states on average faced a 300% increase in cannabis and opioid-related hospitalizations. However, states with MCLs were on average associated with lower growth in opioid abuse/dependence hospitalizations and opioid overdose hospitalizations by 23 and 13 percentage points, respectively. Powell et al. (2018), using an event study methodology, estimates that states with MCLs

[3] See https://www.dea.gov/drug-scheduling.

coupled with provisions for legal dispensaries saw drops in prescription opioid overdose rates of about 27%, an estimate that holds when including heroin, an illicit opioid, within the types of overdose deaths analyzed. This article also finds that state opioid-related treatment admissions for addiction and pain are found to be lowered by MCLs, a conclusion shared by Shi (2017) that can also be found in earlier research on MCLs (Chu, 2015). Finally, the article also concludes that more liberal regulation of dispensaries within states would accentuate these drops in opioid mortality and abuse further, based on a comparison between stricter, more recent dispensary provisions relative to older and less regulated provisions. Broader access to dispensaries leading to greater effects on opioid outcomes seems to be a common conclusion in research on RCLs, or recreational cannabis laws. For instance, McMichael et al. (2019) finds that while both RCLs and MCLs lead to estimated reductions in legal flows of prescription opioids from 2011 to 2018, RCLs were estimated to be more than twice as impactful for reducing prescription opioid flows relative to MCLs. This finding is striking when considering that the previously mentioned Powell et al. (2018) does not find that MCLs reduce prescription drug flows to enacting states.

Not all of the estimated effects for cannabis legislation upon opioid harm outcomes have indicated a beneficial relationship. Conyers and Ayres (2020), in its robust analysis of a close-to 'randomized experiment' of dispensary license allocation in Arizona, found no evidence of a decrease in opioid-related discharges from hospital emergency rooms, a potent finding given the strength of the analysis design.

An additional complication to this story of beneficial cannabis legalization is the finding of heterogeneous effects upon opioid harm outcomes. For one, there seems to be a notable disparity between how urban and rural regions interact with cannabis legislation. For instance, McMichael et al. (2019) finds that cannabis legislation is generally more impactful in reducing opioid prescriptions in urban counties relative to rural counties. Meanwhile, Chan et al. (2020) notes that whites and women tend to see the greatest reductions in opioid mortality following recreational cannabis legalization, though this responsiveness is also semi-consistent with a styilized pattern of whites being especially susceptible to the epidemic (Ruhm, 2018).

Other literature to consider before interpreting the results of this paper revolves around the myriad of other societal benefits and drawbacks cannabis legislation can entail. Changes in drugged driving represent one societal outcome of interest: some research points towards states with MCLs having estimated reductions in fatal car accidents where opioid influence was found (Kim et al., 2016), while other works, such as Reed (2016) on Colorado's legalized recreational and medical cannabis environment, note that there have been increases in traffic fatalities where cannabis influence was involved. A similar juxtaposition of drug effects can be identified throughout the literature. Chu (2015) finds that states with MCLs see reduced arrests for possession of heroin and cocaine, up to 15% combined. On the other hand, Conyers and Ayres (2020) finds a significant increase in Arizonan cannabis-related emergency room discharges, estimated to be above a 45% increase for zip codes with dispensaries, indicating a negative societal outcome resultant from cannabis legislation. Another societal welfare theory of interest called 'deaths of despair' - or suicides spurred by deteriorating medium-run economic conditions - has also seen research as to how cannabis legislation may factor in to its occurrence, though such research has reached conclusions indicating the irrelevancy of 'deaths of despair' to the US Opioid Crisis (Ruhm, 2018) and the

lack of evidence for the efficacy of MCLs to affect the 'deaths of despair' phenomenon (Dow et al., 2019).

Lastly, some discussion of research in to addictive products besides opioids, such as alcohol and cigarettes, could provide some insight as to how cannabis access legislation would affect the consumption of opioids. To start, Grossman et al. (1995)'s panel study of Americans 17 to 27 years old found that past and future consumption of alcohol increases consumption of alcohol in the present. This assertion was made using the Becker and Murphy (1988) rational addiction model to empirically test such a temporal relationship, in which past consumption and future consumption both raise the marginal benefit of current consumption of alcohol. Classifying alcohol as an addictive good, Grossman et al. (1995)'s findings provides adjacent evidence on what to expect of opioids, this paper's addictive good of interest, with regards to how opioid addiction is fueled by past consumption and how it can be understood using the rational addiction model. Other research illustrates more complex nuances of addiction and consumption. One source, a clinical trial of the effect of substituting snus for cigarettes on measures of cigarette consumption, made a point that indications of substitution away from cigarettes were stronger when halting cigarette consumption was directed, not voluntary (Meier et al., 2019). This could imply that inducing voluntary substitution from opioids to cannabis might not be the most effective channel of reducing opioid consumption, and therefore opioid harm, as addiction could require degrees of motivation beyond access to alternatives to be overcome. A literature review on alcohol and alcohol policy interventions discusses its complementarity or substitutability with other addictive goods, finding that a significant minority of alcohol consumers would substitute or complement alcohol with other intoxicating or addictive goods (Moore, 2010). The implications of this finding for the relationship between opioids and cannabis is twofold. First, it may be extrapolated that a subset of addictive good consumers, including those that consume opioids, will always be at risk of consuming addictive goods regardless of intervention. Second, if cannabis is considered an intoxicating good by opioid consumers, it could be possible that cannabis consumption is considered complementary to opioid consumption by this previous subset of addictive good consumers - in which increased access to cannabis would actually have an increasing effect upon opioid consumption for this subset. Some more support to this possibility may be provided by Subbaraman (2016), a literature review of the relationship between alcohol and cannabis consumption. She finds that the relationship varies between complementary, substitutional, or neither depending on the subset of the population analyzed and the setting in which they are analyzed. It is once more indicated that alcohol consumption holds a variable relationship with other addictive goods based upon the subset of the population. Such heterogeneity could possibly be extrapolated to the relationship between opioids and cannabis when considering once more that its similarly a relationship between addictive goods. If the relationship between cannabis and opioids were to have the same variance posited for the relationship between cannabis and alcohol, this would further increase the uncertainty regarding if increased cannabis access would demonstrate a decreasing effect upon opioid consumption and opioid harm. As a sum, the research discussed in to tobacco and alcohol consumption indicates the complexities of addiction, and how such complexities may potentially affect a policy intervention against an addictive good such as opioids.

# 4 Institutional Context

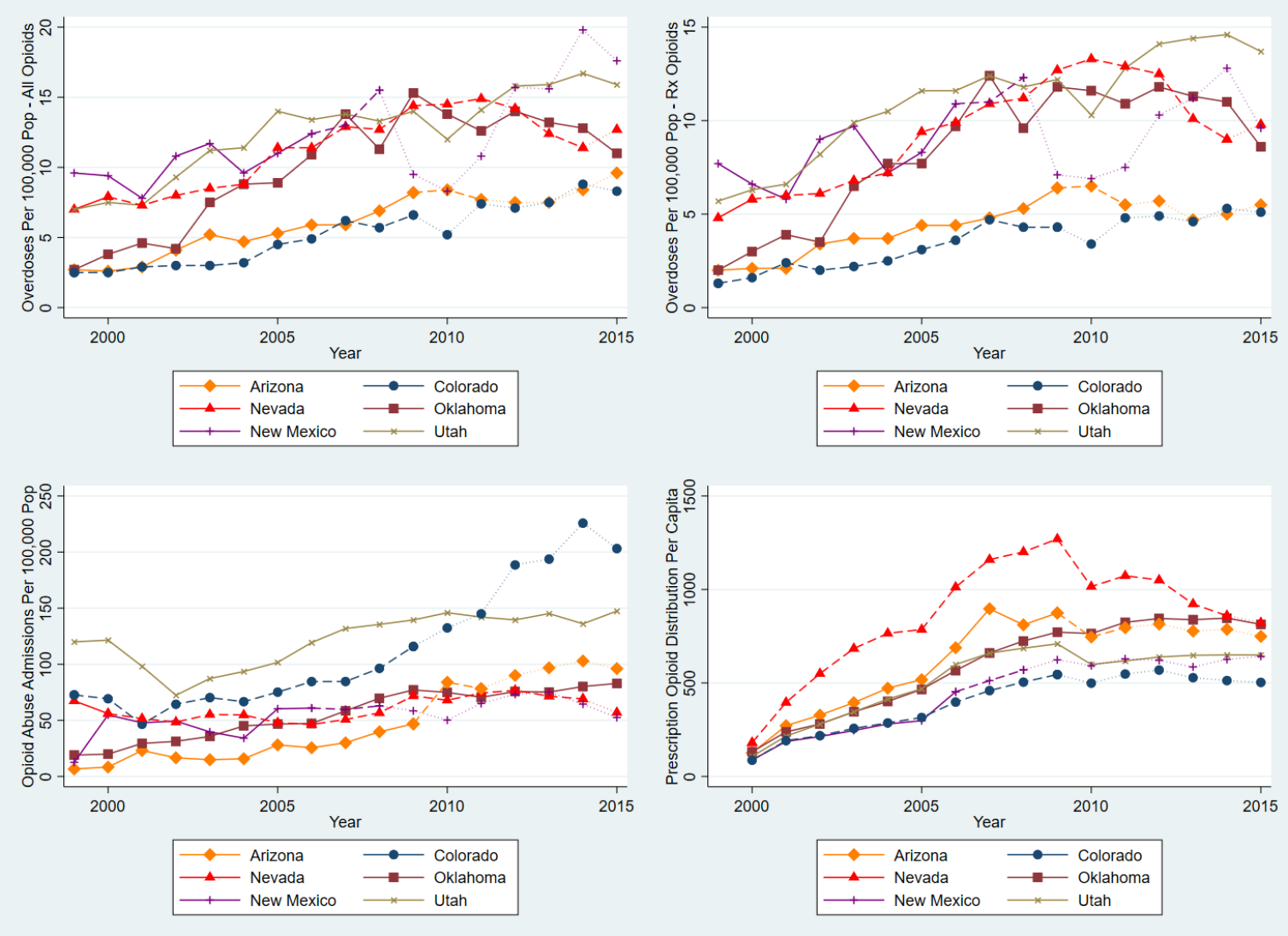
## 4.1 The Opioid Crisis

From 1999 to the present in 2021, the current US Opioid Crisis grew to grip the nation with rising rates in opioid abuse and healthcare costs related to opioid abuse treatment. The epidemic was recognized to be a national threat, including recently by the elected administration of President Donald Trump in Commission (2017). However, the impact of the opioid epidemic is highly heterogenous across states and demographics, as the policies to address the epidemic share the same heterogeneity across states.

As the historical experiences of the Southwestern states with rising opioid abuse are discussed, it is prudent to point out that many of the trends of opioid harm these states struggled with were reflected throughout the rest of the United States. The US Opioid Crisis has historically been dynamic in how it spread harm throughout the American community and in which types of opioids grew in prominence with regards to being abused. As previously mentioned, three separate waves of opioid harm have been observed to take place within the broad scope of the Opioid Crisis, that being the rise of prescription opioid abuse from the 1990s to 2010, followed by the increasing growth of heroin abuse from 2010 to 2016, and most recently the growth in abuse of fentanyl and other synthetic opioids from 2013 onwards to the most recent collection of opioid outcome data in 2018 (Ruhm, 2018). These compositional changes in the broad US Opioid Crisis consistently hold when looking at many individual and groups of states, regardless of the severity of opioid abuse within these individual environments. The Southwestern states also largely reflect the national evolution in opioid abuse and the dynamics of the Crisis; however, the region's opioid abuse situation is more severe than the national average.

### 4.1.1 Southwestern States

Figure 1: Southwestern State Opioid Trends




*Sources:* CDC Multiple Cause of Death (MCoD) data set, 1999-2018; SAMHDA Treatment Episode Data Set: Admissions, 1999-2017; ARCOS Retail Drug Summary Reports (2000-2015)

Notes: "Pop" = Population; "Rx" = Prescription; "Prescription Opioid Distribution Per Capita" is measured in Morphine Milligram Equivalents (MMEs) to standardize differing types of prescription opioids based off of strength and quantity, see Equation 6 and Appendix Table A1. Solid line patterns indicate years without an active MCL for a state, dashed line patterns indicate years with an active MCL for a state, dotted line patterns indicate years with both an active MCL and the active operation of legally protected dispensaries for a state - see Table 1 for relevant timeline of MCL reforms.

Figure 1 presents the growth trends of the opioid outcomes of interest for the six Southwestern states. Throughout the Opioid Crisis, most of the Southwestern states have seen their opioid harm metrics be in excess of the national average. By 2008, New Mexico's opioid overdose death rates grew to 15.5 deaths per 100,000 population,[4] a death rate in excess of three times the national average rate of

[4] Age-adjusted rate, taken from a CDC Multiple Cause of Death (MCOD) database analysis of all opioid-related overdoses, including in relation to heroin, methadone, opioid analgesics, and synthetic opioids, related to underlying cause of death

opioid-related overdoses. Other Southwestern states such as Nevada and Utah exhibited overdose trends in similar severity, especially the latter state with regards to prescription opioids as it often experienced the highest prescription opioid overdose rates of the Southwestern states. Interestingly, a notable break in the aggregate trend in overall opioid overdose rate growth can be visually seen occurring for New Mexico from 2008 to 2011, corresponding with a similar dip in prescription opioid overdose rates for New Mexico in the same time period. Research in to opioid environment changes for New Mexico during this period did not find any conspicuous phenomena to explain this drop in overdoses except that the state saw its first legally protected medical cannabis dispensary become operational in July of 2009. This also corresponds with relatively lower overdose counts for New Mexico after this cannabis environment change took place, with 71 overdoses occurring in the second half of 2009, compared to 117 overdoses in the first half.[5] Though this dip in overdoses quickly reverses by 2012, its timing with increased cannabis access in New Mexico heightens interest in analyzing the cannabis environment change. Rates of opioid abuse treatment admissions saw a less dramatic rise for the most part in the Southwestern states, though there was a quite incredible rise in Colorado's treatment admission rates to 226 admissions per 100,000 Coloradans, more than four times the state's 2001 levels. This punctuates the heterogeneous damage of the Crisis being observed for different states amongst differing opioid metrics. Despite increasing attention turning to the role of prescription drug providers in generating and exacerbating the epidemic in the 2000s, prescription opioid distribution steadily and consistently grew throughout the decade for all states including the Southwestern states (Jones et al., 2018). This trend would be observed to largely flatten from 2010 onwards for most of the United States, but would not end up decreasing back to 1990s levels, something which is also observed for the Southwestern states. Even as prescription opioid distribution rates have flattened towards 2010 onwards, the fact that most of the Southwestern states experienced a rise in rates of overall opioid overdoses and abuse treatment admissions is particularly striking in how it coincides with the growth in heroin abuse and later synthetic opioid abuse observed in the United States as a whole. Evidently, the data offers visual validation of the US Opioid Crisis affecting the Southwestern states, as well as reason to suspect that the same evolution in opioid types driving the Crisis nationally is taking place in these states.

Other sources provide additional context as to the struggles of the Southwestern states in dealing with the Opioid Crisis. The situation of some of these states suffering more than the national average is often noted, such as acknowledgement of Utah's position as the top seventh state leading in drug poisoning deaths out of the entire United States (VIPP, 2020). Costs of the Opioid Crisis for these states are especially illuminating, with quite a few estimates being made that range from direct hospitalization costs to estimated losses of state real output. A lawsuit in Nevada, for instance, alleges that the state's opioid-related hospitalizations cost Nevada $72.94 million in 2010 (Sadler, 2019). Arizona's own health administration found that Arizona's opioid-related hospital encounters increased from 20,365 in 2009 to 41,434 in 2015, while the costs of said encounters increased from $151 million in 2009 to $341 million in 2015 - both measures being effectively doubled (ADHS, 2016). Estimates obtained by New Mexico for

(UCD) specifications defined in the data description of the CDC Multiple Cause of Death database.

[5] Also taken from a CDC Multiple Cause of Death (MCOD) database analysis of all opioid-related overdoses.

the costs of prescription opioid abuse were much more broadly defined, estimating that such costs in 2007 were $890 million, manifested in medical and prescription costs, lost earnings from premature deaths, and costs of correctional facilities and police expenditures (OotNMAG, 2020). Meanwhile, some of the more comprehensive cost estimates of the Opioid Crisis for these states focused on losses in real output, such as Gitis (2018)'s effort to estimate labor force effects of the Crisis both nationally and by individual state. By estimating numbers of individuals lost from the labor force, multiplying by average annual hours of work for each lost worker, then multiplying each hour lost by average real output per hour, the project finds that Colorado and Oklahoma lost $21 billion and $25.5 billion in real output to opioid dependency over the cumulative period of 1999 to 2015.

#### 4.1.2 Reforms Targeting the Opioid Environment

As the 2000s came to a close, more and more attention turned to the US Opioid Crisis, including the exceptional growth in prescription opioid flows throughout the nation. As the role of prescription opioid suppliers became more clear in how they were worsening the US Opioid Crisis, a noteworthy response was undertaken with the reformulation of OxyContin, a commonly abused brand of oxycodone (Cicero et al., 2005), in to a more difficult-to-abuse opioid. This was understood to have actually created a nationwide supply disruption in abuse-worthy prescription opioids, but while it was found to reduce prescription opioid abuse, it also induced increases in heroin abuse and may have helped spur the heroin epidemic in the years following 2010 (Alpert et al., 2018). The effects of this nationwide OxyContin reformulation are thus expected to have been reflected within the opioid environments of the Southwestern states, and is noted in Table 2 along with other significant environmental changes affecting opioid outcomes within the Southwestern states. Other responses were undertaken by states, such as implementing prescription drug monitoring programs (PDMPs), to address the growth in prescription opioid abuse rates (Buchemueller and Carey, 2018). These PDMPs, by collecting data on prescriptions given to patients for controlled substances and then authorizing providers, such as pharmacists or prescribing doctors, to view the records of previous prescriptions, sought to provide means through which providers can identify patients who are misusing or otherwise diverting prescribed controlled substances. This would then place the weight of action on the provider to avoid prescribing controlled drugs to risky patients. Previous research has found that not all PDMPs are equally effective. Buchemueller and Carey (2018) highlights the importance of PDMPs being drafted with 'must access' conditions, or legal requirements for providers to make use of patient prescription records, in order to reduce misuse of prescribed drugs. Two Southwestern states, New Mexico and Nevada, implemented PDMPs with included 'must access' conditions before the end of 2015. They are considered important changes to their respective opioid environments, and are thus included in Table 2.

### 4.2 Medical Cannabis Legislation

A timeline of cannabis environment changes in the Southwestern states is presented by Table 1. Much like the vast majority of the United States, the laws of these states held cannabis as criminalized for medical or recreational purposes for the majority of the 20th century. The first Southwestern state

that started shifting towards cannabis legalization for medical purposes was Colorado with the December of 2000 enactment of Amendment 20. In the following year, a similar legalization of medical cannabis was enacted by ballot, or vote, in Nevada with the approval of Question 9. New Mexico would enact medical cannabis legislation next in July of 2000 with the Lynn and Erin Compassionate Use Act, but with a key difference from Colorado and Nevada's earlier legislation in that legal protection of cannabis dispensaries was also included within the legislation. New Mexico also became the first state to have actively operated and legally protected dispensaries within the Southwestern states on July 2009. The second state to have actively operated and legally protected dispensaries would be Colorado with the passage of the Colorado Medical Marijuana Code in June 2010, which provided legal protection to Coloradan dispensaries that were already in place. The next medical cannabis legislation enacted would be the Arizona Medical Marijuana Initiative, passed by ballot in November 2010. It legalized medical cannabis and provided dispensary protections, which were then taken advantage of in November 2012 when the first legally protected Arizonan dispensary became active. Nevada would become the last state by the end of 2015 to see the active operation of legally protected dispensaries, as the first Arizonan medical cannabis dispensary was certified by the June 2013 Senate Bill 374 in March of 2015 (Powell et al., 2018; Chan et al., 2020).

Table 1: Timeline of Medical Cannabis Legislation in Southwestern States

| Date | Reform | Details |
|---|---|---|
| December 2000 | Colorado: Amendment 20 | Approved by ballot, legalizes medical cannabis. |
| October 2001 | Nevada: Question 9 | Approved by ballot, legalizes medical cannabis. |
| July 2007 | New Mexico: Lynn and Erin Compassionate Use Act | Approved by House and Senate, legalizes medical cannabis and protects operation of dispensaries. |
| July 2009 | New Mexico: Lynn and Erin Compassionate Use Act | First legally protected dispensary becomes operational, qualifying conditions for medical cannabis usage expanded by this point. |
| June 2010 | Colorado: Medical Marijuana Code | Enacted by legislature, protects operation of dispensaries, with active legal dispensaries operating within this year. |
| November 2010 | Arizona: Medical Marijuana Initiative | Approved by ballot, legalizes medical cannabis and protects operation of dispensaries. |
| November 2012 | Colorado: Marijuana Legalization Initiative | Approved by ballot, legalizes cannabis for recreational consumption. |
| December 2012 | Arizona: Medical Marijuana Initiative | First legally protected dispensary becomes operational. |
| January 2014 | Colorado: Marijuana Legalization Initiative | First dispensaries with legal recreational licenses in operation. |
| July 2014 | Utah: Plant Extract Amendments | Enacted by legislature, legalizes medical cannabis products with low THC and high CBD, fundamentally different from medical cannabis legislation in other states. |
| March 2015 | Nevada: Senate Bill 374 | First dispensary active, protected by the legislation Senate Bill 374 which was enacted in June 2013. |

*Sources:* Powell et al. (2018); Chan et al. (2020)

A couple of complicating factors are present within an analysis of changes in the Southwestern states' medical cannabis environments. First, Colorado would go further in broadening cannabis access in November of 2012 with the enactment of Amendment 64, a law that legalizes recreational cannabis, and would later see the opening of recreational cannabis dispensaries by January of 2014. This indicates that cannabis access in Colorado is broader than that of a typical medical cannabis state, and may impact the overall analysis of the effects of medical cannabis environment changes. Another potential issue lies with one of the two states considered to have no medical cannabis legislation by the end of 2015, Utah. Utah would actually see the implementation of a different type of cannabis legislation, legalizing cannabis products with low-tetrahydrocannabinol (THC) and high-cannabidiol (CBD) concentration for medical use with the Plant Extract Amendments in July of 2014. Fundamental differences between the type of cannabis products legalized in Utah compared to other Southwestern states do not lend to Utah being grouped with the other MCL states, and the research in to medical cannabis as a policy solution to the damages of the Opioid Crisis does not give much attention to any role such a law may have. There is some evidence that cannabidiol can be used for treating opioid use disorder (Wiese and Wilson-Poe,

2018), and as such high-CBD products may have a role in reducing opioid harm. If increasing access to such products would lead to reduced state metrics of opioid harm, the inclusion of Utah's outcomes for 2014 and 2015 could prove biasing for the analysis.

### 4.2.1 State Law vs. National Law

Table 2: Timeline of Other Reforms and Environmental Changes

| Date | Reform/Environmental Change | Details |
|---|---|---|
| October 2009 | Communication of the Ogden Memo | Federal prosecution of state law-compliant medical cannabis users and suppliers was deprioritized - increased state regulation of dispensary laws from 2010 onwards an attributed reaction to Ogden Memo. |
| 2010 | Reformulation of OxyContin | OxyContin, a brand of the prescription opioid oxycodone, was reformulated in to an abuse-deterrent form, disrupting supply of abuse-worthy prescription opioids and lowering prescription abuse, but increasing heroin abuse. |
| September 2012 | New Mexico PDMP in effect | 'Must access' prescription drug monitoring program (PDMP) comes in to effect during second half of year in New Mexico. |
| October 2015 | Nevada PDMP in effect | 'Must access' prescription drug monitoring program (PDMP) comes in to effect during second half of year in Nevada. |

*Sources:* Ogden (2009); Powell et al. (2018); Buchemueller and Carey (2018); PDAPS (2020); Alpert et al. (2018)

The passage of legalized cannabis legislation in any state in the United States as of January 2021 stands in direct violation of the federal, or national, law in the United States. Cannabis, or marihuana as classified by the Drug Enforcement Agency (DEA), is considered a Schedule 1 drug, officially held to have no accepted medical use and high risk of abuse.[6] As such, possession, usage, production, and distribution of the substance is considered a criminal act under federal law with individuals engaging in such activities subject to federal law enforcement, regardless of state laws. This creates a risk factor for actors engaging in state medical cannabis markets within the Southwestern states.

While federal law enforcement have the capacity to prosecute state cannabis markets, there have been notable shifts in their stated willingness to do so, most notably occurring with the Ogden Memo. In 2009, David W. Ogden, the US Deputy Attorney General at the time, issued an official clarification to federal prosecutors deprioritizing prosecution of medical cannabis users and suppliers in clear and unambiguous compliance of state laws (Ogden, 2009). Powell et al. (2018) argues that this memo was met with a change in how dispensary legislation was formulated throughout individual legalized-cannabis states from 2010 onwards. Previous to the memo, dispensary legislation was observed to be relatively lenient in regulation of dispensaries. However, after the issuance of the Ogden Memo, it was observed that state dispensary laws were formulated to be more stringent in their regulation of dispensaries, perhaps as an effort to offer congruency to the new stated aim of federal law enforcement. As such, the

[6]Further explanation of the DEA's system of drug scheduling is available at https://www.dea.gov/drug-scheduling.

effects of dispensary allowances within the Southwestern states may have been subject to change in the years following the Ogden Memo, and there may be the possibility of heterogeneity between dispensary allowances made before and after the issuance of the Ogden Memo.

# 5 Data Description

In conducting my analysis of the relationship between medical cannabis and opioid harm, I employ three main data sources obtained from public US government records. Additionally, I use a number of secondary data sources to obtain information on state implementation of medical cannabis laws, operation of dispensaries within the states, and other characteristics of the states for use as covariates.

## 5.1 Main Data Sources

The first of these main sources is the Multiple Cause of Death (MCoD) 1999 to 2018 data set, available from the CDC WONDER database. Using this data set, I am able to reliably obtain annual counts of overdose deaths resultant from or connected to opioid abuse at the state level. This national data set provides mortality information of US residents, coded with ICD-10 codes for underlying cause of death and additional subordinate causes of death. I code deaths with the selection of specific underlying cause of death codes (X40-X44, X60-X64, X85, and Y10-Y14) and drug identification codes (T40.1-T40.4).[7]

The Multiple Cause of Death data set has a couple of limitations. First, while MCoD's parent dataset, the National Vital Statistics System (NVSS), has mortality data extended prior to 1999, data before this year was incorporated using ICD-9 codes, as opposed to the current data set's usage of ICD-10 codes. Using ICD-9 codes to identify our desired information, being causes of death related to opioid usage, is a difficult prospect and offers relatively little insight compared to the full breadth of data encoded within the ICD-10 system. Second, the data set utilized is a public-use variant, with restrictions in place to protect the anonymity of mortalities within the system. As such, mortality counts extracted for any specific time period, location, or grouping are suppressed if below a threshold of 10. For example, extracted prescription opioid overdoses for African American individuals within New Mexico are suppressed for individual years within 1999 to 2018 due to low yearly counts, while the overall mortality count of 52 throughout the 19 year period was made available. This presents a notable limitation with usage of the MCoD data set, in which analysis of various subgroups or demographics is difficult to execute.

The second of these main sources is the Treatment Episode Data Set: Admissions (TEDS-A) from 1999 to 2017. This data set is taken from the Substance Abuse & Mental Health Data Archive (SAMHDA), maintained by the SAMHSA, a branch of the US Department of Health & Human Services. I use this data set in order to obtain a proxy measure for opioid abuse and opioid dependence, harm outcomes that do not end with mortality. This national data set compiles admission records to publicly-funded substance abuse facilities, with some privately administered facilities with public funding included as well.

[7] Code T40.0, the identifier for opium connected to the underlying cause of death, was excluded due to low counts of usage and the uncertainty of which the opioid is acquired - it is unclear what share of opium used is acquired from prescriptions or through the illicit drug market.

Each admission, or individual case of acceptance of someone for treatment, is tracked along with reasons for admission, substances found at admission, and other information of the individuals admitted.

The TEDS-A data provides an imperfect proxy for identifying harm caused by opioid abuse and dependence. It would be preferable to directly analyze the negative changes to a user's lifestyle, utility, and health; however, for the purposes of determining the potential social benefits of a policy intervention against opioid abuse, the data set is informative, especially in that its collection of data stems from treatment facilities that in some way received public funding. As such, admissions to these facilities represent the public burden of opioid abuse. As previous research has shown that nationally the US public sector has held a share of 75% of all substance abuse treatment expenditures, there is evidence that the TEDS-A data set is informative when pursuing the analysis of shifts in state-level trends of abuse (Mark et al., 2011). Additionally, compilation of admissions within the dataset is not a perfect process. There are missing admission compilations for certain states in certain years, without discernible pattern or explanation. Luckily, this issue does not affect my analysis of the TEDS-A dataset, as upon inspection the Southwestern states included in the analysis do not suffer from missing admission compilations.

The last main data resource that will be utilized is the Automation of Reports and Consolidated Orders System (ARCOS), a resource provided by the US Drug Enforcement Administration (DEA). This resource provides information on flows of controlled substances, including prescription opioids, from manufacturers to distributors. This includes hospitals, pharmacies, and other typical channels of legal substance distribution. This allows the data in this set to serve as a proxy for measuring changes in prescribing patterns by health professionals as both the Opioid Crisis unfolds and medical cannabis legislation within certain Southwestern states come in to effect. I utilize the yearly retail drug summary reports of ARCOS to obtain counts of prescription opioids that have been distributed to the states included in the analysis. These counts of prescription opioids are demarcated based upon the type of prescription opioid. I take these separated counts of prescription opioids and convert them in to standardized Morphine Milligram Equivalent (MME) units in order aggregate all of the differing types of opioids based upon their relative strengths.[8] In doing so, I use the following equation:

$$\text{MME} = \frac{(\text{Strength}) * (\text{Quantity}) * (\text{Conversion Factor})}{\text{Days Supply}} \tag{6}$$

Where strength multiplied by quantity equals the milligram count of an opioid, and "Days Supply" is safely ignored.[9] By multiplying the milligram count of an opioid type by its MME conversion factor, all prescription opioids of significance are thus measured in terms of a single unit type representing strength of the opioid. This is informative for this paper's purpose of analyzing changes in uniform prescription opioid supply, and in line with previous research in to prescription opioid supply changes (Powell et al., 2018; Ruhm, 2018; McMichael et al., 2019). A more detailed breakdown of which opioid drugs are included

[8] This is a procedure used by the Center of Disease Control (CDC) and the Centers for Medicare and Medicaid Services (CMS) in order to calculate safe dosages for patients using opioids. More information can be found at https://www.cdc.gov/drugoverdose/pdf/calculating_total_daily_dose-a.pdf and https://www.cms.gov/Medicare/Prescription-Drug-Coverage/PrescriptionDrugCovContra/Downloads/Opioid-Morphine-EQ-Conversion-Factors-March-2015.pdf.

[9] "Days Supply" is used as a divisor in the MME formula in order to help calculate an individual's safe daily dosage of a prescription opioid. This is not relevant with regards to this analysis, as I simply wish to measure how much standardized units of prescription opioids are legally distributed within a state.

in my analysis of ARCOS, along with their respective MME conversion factors, is listed in the appendix under Table A1.

The ARCOS summary reports have their own limitations. For instance, the types of drugs included throughout the yearly summary reports have seen changes, with most of the opioids included in my analysis being included in the reports from 2001 onwards. No explanation has been found as to the reasons why they were omitted before. Prescription opioids with significant shares in distribution missing from the 2000 summary report include codeine, hydromorphone, meperidine, morphine, and fentanyl. As this earlier year is associated with few states in the sample having medical cannabis legislation and none having dispensaries, lower early MME counts resultant from missing data on distribution of these drug types may lead to systematically lower treatment estimates for both cannabis environment changes. An additional limitation, or more aptly put complication, of the ARCOS summary reports is that they are not available in data formats readily accessible by analysis software such as Excel or Stata, and instead must be compiled by hand from the summary report documents. This presents the possibility, over the course of transcribing annual distribution counts of multiple prescription opioid types by state between 2000 to 2015, of user error in compilation of an analysis-ready distribution dataset. This complication stems from the lack of accessibility of ARCOS for public examination, with the only public access to their database being published by the Washington Post for 2006 to 2014[10] after being obtained by court order.[11]

Descriptive statistics on these opioid outcomes of interest for the differing sample groups and states are included in Table 3 below. A few observations can be made with what is presented. To start, all states saw higher average rates of prescription overdoses, overall overdoses, substance abuse treatment admissions, and prescription distribution in the later sample period of 2010-2015. However, states that implemented medical cannabis at some point before the end of 2015 had on average lower rates of overall overdoses and prescription overdoses relative to non-MCL states throughout both 1999-2009 and 2010-2015. They also suffered lower average rates of substance abuse treatment admissions relative to non-MCL states within 1999-2009, but would see such a level of growth in treatment admissions that this difference was largely eliminated within the 2010-2015 period. The opposite trend was observed to occur with regards to prescription distribution, where MCL states saw relatively higher rates of prescription opioid distribution to their non-MCL counterparts in 1999-2009, but had relatively equalized rates of distribution to their counterparts in 2010-2015. Turning to the individual states, New Mexico and Utah experienced the highest rates of overall overdoses in either period for MCL and non-MCL states, respectively. Meanwhile, within the MCL state group, Nevada experienced the highest rate of prescription opioid distribution out of all states in both periods. Colorado also experienced a growth in substance abuse admission rates by a factor of three when comparing the two periods, holding the highest rate of substance abuse admissions out of all of states in 2010-2015. In general, the overall trend

[10] This dataset can be downloaded at https://www.washingtonpost.com/national/2019/07/18/how-download-use-dea-pain-pills-database/.

[11] See https://www.washingtonpost.com/health/how-an-epic-legal-battle-brought-a-secret-drug-database-to-light/2019/08/02/3bc594ce-b3d4-11e9-951e-de024209545d_story.html, as well as https://www.washingtonpost.com/graphics/2019/investigations/dea-pain-pill-database/.

of opioid harm growth over time is reflective of previous research in to the evolution of the US Opioid Crisis. However, whether there is a separate trend of harm or prescription growth between the MCL and non-MCL states is not immediately hinted at with these descriptive statistics.

Table 3: Descriptive Statistics - State Opioid Outcomes

| | | Medical Cannabis States | | | | | Illegal Cannabis States | | | Sample |
|---|---|---|---|---|---|---|---|---|---|---|
| | | AZ | CO | NV | NM | Avg | OK | UT | Avg | Avg |
| 1999-2009 | All ODs | 4.95 | 4.09 | 10.03 | 10.94 | 7.5 | 8.35 | 11.11 | 9.73 | 8.24 |
| | Rx ODs | 3.85 | 2.91 | 8.26 | 8.69 | 5.93 | 7.07 | 9.71 | 8.39 | 6.75 |
| | Admissions | 23.28 | 76.93 | 55.20 | 49.18 | 51.14 | 43.61 | 110.98 | 77.29 | 59.86 |
| | Distribution | 53,807 | 32,621 | 80,053 | 34,728 | 50,303 | 45,863 | 44,842 | 45,352 | 48,652 |
| 2010-2015 | All ODs | 8.18 | 7.38 | 13.35 | 14.63 | 10.89 | 12.9 | 15.07 | 13.98 | 11.92 |
| | Rx ODs | 5.48 | 4.68 | 11.27 | 9.72 | 7.79 | 10.87 | 13.32 | 12.09 | 9.22 |
| | Admissions | 91.38 | 181.42 | 69.46 | 63.48 | 101.43 | 76.57 | 142.69 | 109.63 | 104.17 |
| | Distribution | 77,841 | 52,662 | 95,731 | 61,613 | 71,962 | 82,188 | 63,388 | 72,788 | 72,237 |
| 1999-2015 | All ODs | 6.09 | 5.25 | 11.2 | 12.24 | 8.70 | 9.95 | 12.51 | 11.23 | 9.54 |
| | Rx ODs | 4.42 | 3.54 | 9.32 | 9.05 | 6.58 | 8.41 | 10.98 | 9.70 | 7.62 |
| | Admissions | 47.31 | 113.81 | 60.23 | 54.22 | 68.89 | 55.24 | 122.17 | 88.71 | 75.50 |
| | Distribution | 62,820 | 40,136 | 85,932 | 44,810 | 58,425 | 59,485 | 51,797 | 55,641 | 57,497 |

*Sources:* CDC Multiple Cause of Death (MCoD) data set, 1999-2018; SAMHDA Treatment Episode Data Set: Admissions, 1999-2017; ARCOS Retail Drug Summary Reports (2000-2015)

Notes: "AZ" = Arizona; "CO" = Colorado; "NV" = Nevada; "OK" = Oklahoma; "UT" = Utah; "Avg" = Average of outcomes within a respective sample or subsample and time period; "All ODs" = Opioid overdoses per 100,000 population for any type of opioid excluding opium, age-adjusted; "Rx ODs" = Opioid overdoses per 100,000 population for prescription opioid types, age-adjusted; "Admissions" = Opioid substance abuse treatment admissions per 100,000 population for any type of opioid; "Distribution" = Legal prescription opioid flows per 100,000 population, measured in kilograms of the standardized unit Morphine Milligram Equivalent (MME), see Equation 6 and Appendix Table A1.

## 5.2 Other Data Sources

Coding for states that implemented medical cannabis legislation as well as protected dispensaries involves diving into the laws of each state, as well as other sources tracking the status of state cannabis access. This paper cross-references between two previous works that compiled information on cannabis legality status within states, Chan et al. (2020) and Powell et al. (2018). The former source provided compilation of state medical cannabis legislation and dispensary protection up to the end of 2017, drawing from online resources such as the Marijuana Policy Project and cross-referencing with state legal resources. Meanwhile, the latter source provided the same desired vein of information up until 2014 while also including when a state first had active and legal dispensaries. They acquired this information searching through LexisNexis, a provider of a public and legal records database,[12] as well as searching through state legal resources manually to determine if the relevant statutes fulfilled the criteria of medical cannabis legislation and dispensary protection. I also cross-reference the Marijuana Policy Project website used by Chan et al. (2020) to certify the sources findings with the up-to-date resource (MPP, 2020).[13] By drawing

[12]See https://www.lexisnexis.com/en-us/about-us/about-us.page for more details.

[13]For comparison, Chan et al. (2020) accessed the source on September 18th, 2018.

from these two well-sourced previous works for information on state cannabis laws and dispensary status, with certification by cross-examination of MPP (2020), essential information to conduct this paper's analysis is thus acquired.

Demographic information for comparison of states within the sample and usage as covariates is acquired from population estimates made by the US Census Bureau. Specifically, this paper utilizes the estimates released by the National Center of Health Statistics (NCHS)[14] which are utilized in the NVSS database, including the MCoD dataset utilized by this paper. These population estimates include demographic information, such as gender, age, race (sorted in to four categories), and Hispanic origin - all of which are used to build population shares of these characteristics for the purpose of regression covariates. This information is available down to the county-level, which makes it suited for inclusion in a state-level outcome analysis. There is some heterogeneity in how these population estimates were obtained for different years. The United States undertakes a Census[15] every 10 years, with population estimates for 2000 and 2010 being directly derived from the 2000 Census and 2010 Census, respectively. When obtaining population estimates for a non-Census year, the NCHS relies on intercensal estimates released by the Census Bureau for years between two past Census years, and postcensal estimates also released by the Census Bureau for years following the latest Census year. As such, population estimates for 1999 and 2001-2009 are derived from intercensal estimates which are based off the 1990/2000 Census and 2000/2010 Census, respectively. Meanwhile, population estimates for 2011-2015 are derived from postcensal estimates based off the 2010 Census. These intercensal and postcensal estimates differ mainly in that they are based primarily on projections and involve relatively less data collection compared to Census year estimates. This is not expected to affect building the demographic covariates in a manner which introduces bias.

Turning to other economic and legal factors that may affect opioid outcomes, I obtain unemployment rate data from the Local Area Unemployment Statistics (LAUS) program of the Bureau of Labor Statistics. Deriving information on the American labor force from the US Census Bureau, the LAUS compiles a set of datasets which estimate the labor force status of the civilian non-institutional population aged 16 years and older for different intervals, including monthly and yearly, with state-level information included within the datasets.[16] Among the counts of employed and unemployed persons provided, the datasets also provide the estimated unemployment rates for the desired interval. Thus, I utilize the annual unemployment rates for the different states compiled within the dataset for my own analysis. Meanwhile, data on state beer excise tax rates was obtained from TPC (2020), which compiles annual tax rates of dollars per gallon on separate types of alcohol from 1982 to January 1st, 2020. These tax rates are compiled from a spread of different sources including the Federation of Tax Administrators, the Tax Foundation, the Council of State Governments, the Advisory Commission on Intergovernmental Relations, the Alcohol Policy Information System, and the Distilled Spirits Council of the United States. I directly incorporate the beer excise tax rates from the dataset in to my design, readily applying it as a covariate. Lastly, information on when and which states enacted "Must Access" Prescription Drug

[14] See https://www.cdc.gov/nchs/nvss/bridged_race.htm for more information and access to the datasets.
[15] See https://www.census.gov/programs-surveys/decennial-census/about.html for more information.
[16] See https://www.bls.gov/lau/rdscnp16.htm for more information and access to the datasets.

Monitoring Programs (PDMPs) was provided by PDAPS (2020). This dataset, providing information on relevant PDMP laws from January 1998 to July 2016, not only tracks when a state implements a PDMP, but also if the law requires prescribers or dispensers to monitor the PDMP, which is an important condition for the effectiveness of a PDMP in reducing prescription drug misuse, as previously noted by Buchemueller and Carey (2018). This provides a central dataset from which I can code a dummy variable for if a state has a "Must Access" PDMP within a certain year which differentiates if the PDMP has been active for the second half or the entire year.[17]

The descriptive statistics for the state characteristics used as covariates are displayed in Table 4.[18] There are a few general trends for both sample groups that can be observed. For one, unemployment rates go up for all states, coinciding with the financial crisis of 2007 which states would still be recovering from through the first half of the 2010s. Notably, medical cannabis states seem to have suffered on average from greater growth in unemployment rates between periods. Two important state characteristics, population shares of whites and those 35 and older, do experience general changes for both groups, with populations shares of non-Hispanic whites, on average, falling and population shares of 35 and older on average rising. Both groups experience the same average change, but its notable that for both periods non-MCL states are observed to on average have higher population shares of non-Hispanic whites. This is noteworthy as it has been indicated that opioid abuse disproportionately affects white populations, especially prescription opioid abuse (Alexander et al., 2018), which could also mean that non-MCL states could have higher rates of opioid harm as a result. The same association between older ages and prescription opioid abuse (Campbell et al., 2010) may also impact state metrics of opioid harm, with significant differences between MCL and non-MCL states' 35 to 64 and 65 and older population shares highlighting the importance of controlling for them in the difference-in-difference design. Other characteristics of importance, beer taxes and population shares of males, do not consistently vary with significance between sample groups and sample periods. Further information on tests for significant differences between the treatment and control groups can be found in Appendix Table A3.

[17] More specifically, a state with a "Must Access" PDMP has a dummy variable coded as "2" if the PDMP is active for both halves of the year. If the PDMP was only active in the second half of the year, such as New Mexico's "Must Access" PDMP coming into effect on September 28th, 2012, then the dummy variable is coded as "1". This was done to distinguish between PDMPs which were and were not active for the majority of the year, as it is expected they differed in effect upon opioid outcomes.

[18] Other state characteristics obtained from the data and not included as covariates can be found in Appendix Table A2.

Table 4: Descriptive Statistics - State Covariate Characteristics

| | | Medical Cannabis States | | | | | Illegal Cannabis States | | | Sample |
|---|---|---|---|---|---|---|---|---|---|---|
| | | AZ | CO | NV | NM | Avg | OK | UT | Avg | Avg |
| 1999-2009 | Male* | 49.83% | 50.24% | 50.74% | 49.29% | 50.02% | 49.33% | 50.17% | 49.75% | 49.93% |
| | White* | 62.18% | 73.58% | 61.80% | 43.81% | 60.34% | 74.52% | 84.29% | 79.40% | 66.70% |
| | 15-34* | 28.32% | 29.09% | 27.91% | 27.57% | 28.22% | 27.97% | 33.79% | 30.88% | 29.11% |
| | 35-64* | 36.68% | 40.07% | 39.68% | 38.42% | 38.71% | 37.99% | 31.09% | 34.54% | 37.32% |
| | 65+ | 13.05% | 9.99% | 11.21% | 12.22% | 11.62% | 13.22% | 8.64% | 10.93% | 11.39% |
| | Unemployed* | 5.35% | 4.71% | 5.36% | 5.22% | 5.16% | 4.36% | 4.38% | 4.37% | 4.90% |
| | Beer Tax | 0.16 | 0.08 | 0.13 | 0.41 | 0.20 | 0.40 | 0.13 | 0.27 | 0.22 |
| 2010-2015 | Male | 49.70% | 50.21% | 50.37% | 49.51% | 49.95% | 49.52% | 50.27% | 49.90% | 49.93% |
| | White* | 57.79% | 70.50% | 53.95% | 40.28% | 55.63% | 70.60% | 80.86% | 75.73% | 62.33% |
| | 15-34* | 27.43% | 28.35% | 27.40% | 27.29% | 27.62% | 27.83% | 31,59% | 29.71% | 28.32% |
| | 35-64* | 37.00% | 39.83% | 39.40% | 37.82% | 38.51% | 37.43% | 32.51% | 34.97% | 37.33% |
| | 65+* | 15.09% | 12.01% | 13.33% | 14.46% | 13.72% | 14.11% | 9.64% | 11.87% | 13.10% |
| | Unemployed* | 8.13% | 6.8% | 10.32% | 7.13% | 8.10% | 5.35% | 5.32% | 5.33% | 7.18% |
| | Beer Tax | 0.16 | 0.08 | 0.16 | 0.41 | 0.20 | 0.40 | 0.00 | 0.20 | 0.20 |
| 1999-2015 | Male | 49.78% | 50.23% | 50.61% | 49.37% | 50.00% | 49.40% | 50.21% | 49.80% | 49.93% |
| | White* | 60.63% | 72.49% | 59.03% | 42.56% | 58.68% | 73.13% | 83.08% | 78.11% | 65.15% |
| | 15-34* | 28.01% | 28.83% | 27.73% | 27.47% | 28.00% | 27.92% | 33.01% | 30.47% | 28.83% |
| | 35-64* | 36.79% | 39.98% | 39.58% | 38.20% | 38.64% | 37.80% | 31.59% | 34.69% | 37.32% |
| | 65+* | 13.77% | 10.70% | 11.96% | 13.01% | 12.36% | 13.53% | 8.99% | 11.26% | 11.99% |
| | Unemployed* | 6.34% | 5.45% | 7.11% | 5.89% | 6.20% | 4.71% | 4.71% | 4.71% | 5.70% |
| | Beer Tax | 0.16 | 0.08 | 0.14 | 0.41 | 0.20 | 0.40 | 0.09 | 0.24 | 0.21 |

*Sources:* US Census Bureau intercensal series, 1999 and 2001-2009; US Census, 2000 and 2010; Vintage postcensal series, 2011-2015; Local Area Unemployment Statistics (LAUS), Employment Status of the Civilian Noninstitutional Population 1976-2019; Tax Policy Center State Alcohol Excise Taxes, 1982-2017

Notes: "AZ" = Arizona; "CO" = Colorado; "NV" = Nevada; "OK" = Oklahoma; "UT" = Utah; "Avg" = Average of covariates within a respective sample or subsample and time period; "Male", "White", "15-34", "35-64", and "65+" = Population shares of males, non-Hispanic whites, individuals of age 15 to 34, individuals of age 35 to 64, and individuals of age 65 and older, respectively; "Unemployed" = Unemployment rate; "Beer Tax" = Beer excise tax rate. Symbol * indicates the state characteristic differs between treatment and control groups at the 5% or 1% significance level for the given period - see Appendix Table A3 for two-sample t-test results.

# 6 Research Methodology

The main empirical analysis of this paper will be conducted with the usage of the difference-in-difference methodology with staggered treatment implementation. The sample measured will be a subset of states within the United States that implemented similar reforms at different points of time (or not at all) throughout the sample period, and also shared relatively similar outcomes and trends of opioid harm.

## 6.1 Difference-In-Difference Methodology

The difference-in-difference methodology is a methodological framework for estimation of causal effects of a treatment via comparison of two similar groups, one of which undergoes the treatment while

the other remains untreated as a control group. By selection of groups with similar enough characteristics an argument is made for a common trend assumption, in that the outcomes of the untreated group represent the unobserved counterfactual of the treatment group had the latter not undergone treatment, and that the differencing out of the control and treated group's outcomes allows the isolation of the causal effect of a treatment. This is captured by the following specification including year fixed effects for the effect of medical cannabis legislation on opioid outcomes:

$$y_{st} = \alpha_t + X_{st}^{'}\beta + T_s^{'}\gamma + (T_s^{'} * ACT_{st}^{'})\delta + \eta_{st} \tag{7}$$

Where a selected opioid outcome in state $s$ during year $t$ is represented by $y_{st}$. Fixed effects for particular years are captured by $\alpha_t$, while controls for time and state-varying covariates are accounted for by $X_{st}^{'}\beta$. $T_s^{'}$ indicates that a state enacted one of the two treatments of interest - medical cannabis legislation, or active and legalized dispensaries - whenever equal to 1. Finally, the interaction term $(T_s^{'} * ACT_{st}^{'})$ captures whenever a state that enacts one of the two treatments during the time period has said treatment active during the year observed ($ACT_{st}^{'} = 1$). Thus, the causal effect of the treatment is identified by $\delta$ of the expression, and is my main estimator of interest throughout the analysis.

The main premise of using a Difference-In-Difference design to identify a causal effect of a treatment is the assumption of a "common trend" between the treated group and untreated group. As such, if a common trend exists (or is assumed to exist), then by comparing the outcomes of the two separate groups while one undergoes treatment, the common trend is effectively 'differenced' out between the two groups, leaving only the causal effect of the treatment to be captured by the design. This is a design that attempts to approximate the counterfactual of the treated group had it not undergone treatment by selection of an appropriate control group for comparison of post-treatment outcomes between groups.

### 6.1.1 Identification of Paper's Design

In my analysis, I focus upon a group of states in the Southwest of the United States, with the states of Arizona, Colorado, Nevada, and New Mexico serving as the group receiving the (staggered) treatment throughout the sample period. The states of Oklahoma and Utah are included as part of the untreated sample group. The passage of medical cannabis legislation and operation of dispensaries are the two treatments separately estimated for effects relative to not having an MCL. The analysis will be conducted over a range of years extending from 1999 or 2000 to 2013 or 2015, depending on the opioid outcome of interest. While I include yearly fixed effects to control for aggregate shocks to the opioid environment across all states, I do not include state fixed effects due to collinearity problems with the treatment variables. Controls to be established to further isolate the causal effect of the policies will include demographic controls for state population shares of differing age groups, whites, and males, as well as controls for state unemployment rates, alcohol taxes, and laws enforcing "Must Access" Prescription Drug Monitoring Programs (PDMPs).

The observable characteristics of the sample states and groups are provided in Table 4, as well as Appendix Table A2. The selection of these Southwestern states for the sample groups were done to provide a group of states with close geographic proximity, population sizes and densities, and similar

demographics in an effort to arrive at similar unobservable characteristics between groups. This was not perfectly achieved between sample groups, as shown by Appendix Table A3 that MCL states on average have significantly higher population shares of Hispanics, lower population shares of non-Hispanic whites, and higher 2010-2015 unemployment rates relative to non-MCL states, among other differences. However, controls for some of the more important differences assist in creating comparable treatment and control groups. While four out of the six sample states implemented an MCL and had dispensaries during the sample time period at different times, the MCLs of the separate states were considered similar in type and effect by previous research (Powell et al., 2018). This lends to a legitimacy in their inclusion in a staggered Difference-In-Difference design.

Of paramount importance to the difference-in-difference design is whether the common trend assumption holds between the treatment states and the control states. While in a difference-in-difference design with staggered intervention it is not possible to directly compare the pre-intervention outcomes of the treatment and control groups, this paper follows in the vein of Gipper et al. (2019) and Gormley and Matsa (2011), and conducts an event study analysis of the relative time period surrounding implementation of MCLs amongst the treatment group. By lagging the years before and after the implementation of an MCL and estimating for significant effects of the lags, differences in pre-treatment trends can be identified by the presence of significant lagged effects of the MCL before the year of implementation. Such an analysis is conducted and displayed in Table 5, with the event study graphically displayed in Figure 2. With attention primarily to significance of the lags before the treatment, some concern for violation of the common trend assumption amongst the treatment group can be observed for the overall overdose rate and abuse admission rate regressions, where the lag for three years before a state's MCL enactment is found to have significant effects at the 10% and 5% levels, respectively. While this finding of significance is not consistently found amongst other lags for the regressions, it certainly requires consideration. The event study's results also only provides insight in to the common trend assumption within the treatment group, not between the treatment group and control group, leaving open more possibility for common trend assumption violations. The strength of the difference-in-difference design's identifying assumption will be discussed more in context with the results in Section 9.

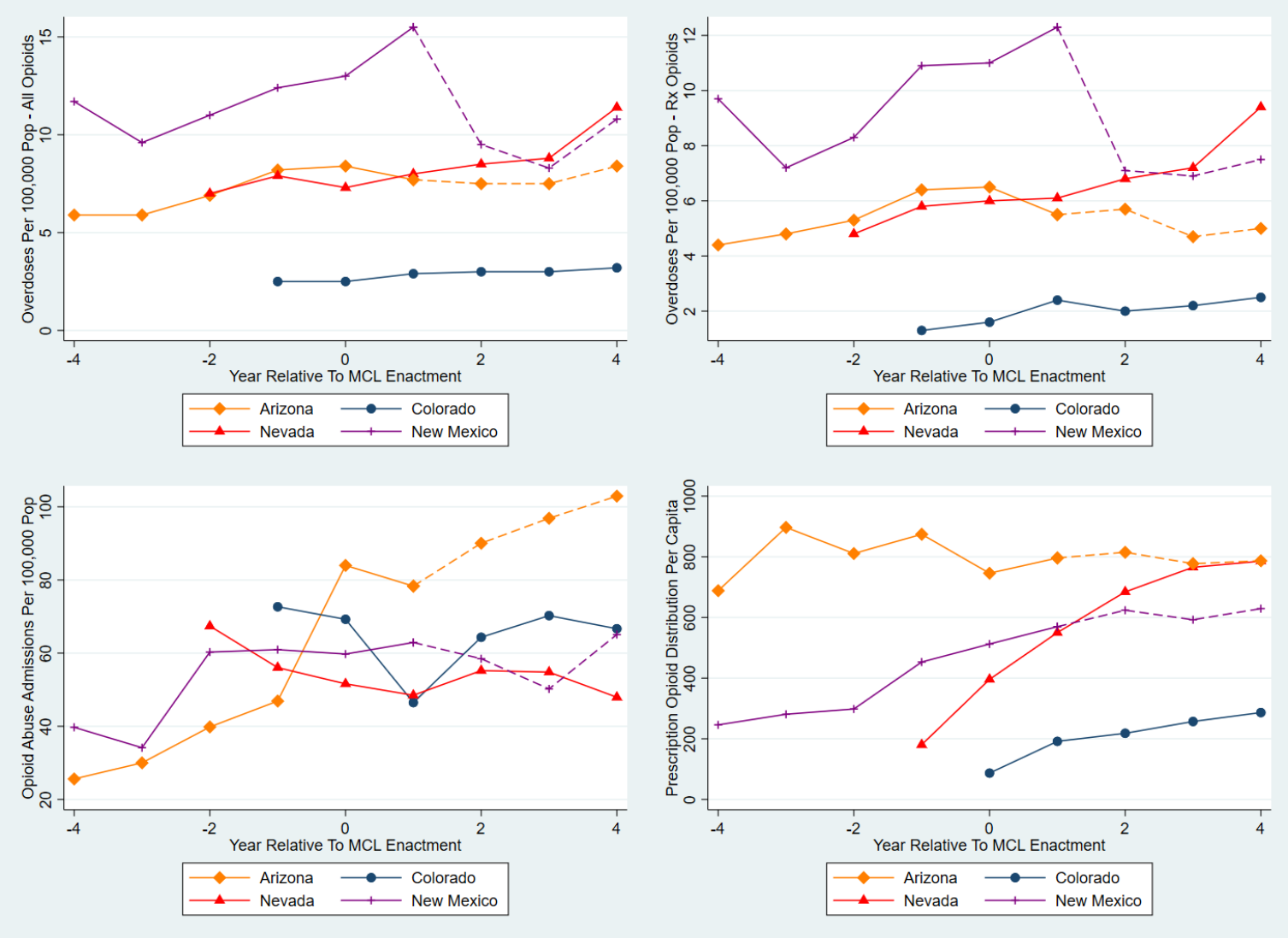
Table 5: Event Study: Medical Cannabis Legislation Lagged Effects Relative To Enactment

| 1999-2015: MCL Effects | Logged State Opioid Outcomes (Rates) | | | |
|---|---|---|---|---|
| | Any Overdose | Rx† Overdose | Abuse Admission | Distribution |
| Four Years Before Enactment | -0.011 | -0.008 | -0.759 | 0.035 |
| | (0.160) | (0.138) | (0.364) | (0.536) |
| Three Years Before Enactment | -0.160* | -0.165 | -0.754** | 0.197 |
| | (0.054) | (0.117) | (0.208) | (0.628) |
| Two Years Before Enactment | -0.039 | -0.102 | -0.170 | 0.063 |
| | (0.065) | (0.067) | (0.359) | (0.486) |
| One Year Before Enactment | 0.054 | -0.007 | -0.113 | -0.077 |
| | (0.036) | (0.049) | (0.204) | (0.417) |
| One Year After Enactment | 0.020 | 0.021 | -0.118 | 0.235 |
| | (0.108) | (0.174) | (0.119) | (0.261) |
| Two Years After Enactment | 0.133 | 0.175 | -0.038 | 0.738*** |
| | (0.053) | (0.080) | (0.106) | (0.055) |
| Three Years After Enactment | 0.062 | 0.111 | -0.046 | 0.706** |
| | (0.055) | (0.087) | (0.084) | (0.137) |
| Four Years After Enactment | 0.151 | 0.159 | -0.018 | 0.660** |
| | (0.068) | (0.108) | (0.134) | (0.208) |
| R-Squared | 0.979 | 0.960 | 0.684 | 0.7913 |
| Observations | 31 | 31 | 31 | 29 |

Notes: Values not in parentheses indicate estimated coefficients. Values in parentheses indicate standard errors clustered at the state level. Symbols *, **, and *** present next to a coefficient indicate significance at the 10%, 5%, and 1% level, respectively. Estimates were obtained in regressions including state controls for active and legal dispensaries, unemployment rates, population shares of whites, individuals 65 years and older, and males.

†: "Rx" is an abbreviation for "Prescription".

Figure 2: Opioid Trends Relative To Medical Cannabis Legalization - Event Study




*Sources:* CDC Multiple Cause of Death (MCoD) data set, 1999-2018; SAMHDA Treatment Episode Data Set: Admissions, 1999-2017; ARCOS Retail Drug Summary Reports (2000-2015)

Notes: "Pop" = Population; "Rx" = Prescription; "Prescription Opioid Distribution Per Capita" is measured in Morphine Milligram Equivalents (MMEs) to standardize differing types of prescription opioids based off of strength and quantity, see Equation 6 and Appendix Table A1. Dashed line patterns indicate years with both an active MCL and active dispensaries for a state. Colorado (MCL: 2000) and Nevada (MCL: 2001) are missing lags before MCL enactment due to dataset time period limitations - see Table 1 for relevant timeline of MCL reforms.

### 6.1.2 Difference-In-Difference Internal Validity

Beyond satisfying the common trend assumption, there are a few fundamental concerns with drawing valid conclusions from Difference-In-Difference designs, especially with concern to the treatment. For one, a treatment or policy program should be implemented for reasons exogenous to factors that affect the outcome of interest. For example, if in this design's case the medical cannabis legislation were implemented by certain states in response to rising opioid harm outcomes, there would be a challenge to the internal validity of the DiD design, as there would now be correlation with states that implemented

the treatment and higher metrics of opioid harm. To my knowledge, discussion of medical cannabis legislation as a policy solution to the Opioid Crisis was not prevalent during the sample time period of 1999 to 2015. Powell et al. (2015) cites Bachhuber et al. (2014) as one of the earliest known studies on medical cannabis legislation and opioid harm, years after Arizona became the last state in my sample to legalize medical cannabis. The notion of medical cannabis legislation providing relief from the Opioid Crisis could certainly have arisen outside of the sphere of policy research before 2014; however, the lack of glaring evidence, to my knowledge, indicating such an idea influenced the policies of my sample states lends confidence to the exogenous implementation of the principal treatment.

Additionally, anticipation of the treatment can weaken the internal validity of the DiD design, as strategic behavior pre-treatment by relevant agents in the setting can produce distortions in the measured effects of the treatment. An example that theoretically could occur in my design is that potential consumers of opioids for pain relief may hold off on consumption of opioids, in expectation of an increase in access to other pain-relief methods after a certain time period, which could lead to a drop in outcomes related to opioid harm in the periods leading up to the passage of medical cannabis legislation and thus a weakening of the measured effect of the MCL. With regards to medical cannabis legislation, three of the four treatment states within the sample announced/approved and enacted their MCLs within separate months; however, investigation shows that all MCLs were announced within the same year as enactment, and as such any possible anticipatory effects for the reforms would have taken place within the same year as the reform. Due to the focus on a state's yearly outcomes within the design, this rules out the possibility of anticipation effects distorting estimation of the effects of a state's MCL. However, this does not rule out such anticipation effects for dispensary operation, as these could occur with the passage of the MCL itself, the issuance of dispensary licenses before operation begins, or the amendment of an MCL to provide protection for medical cannabis dispensaries. To the extent anticipation could affect the estimates of dispensary effects is unclear, especially considering that some anticipation is part of the design, as laws that legalize medical cannabis are required for legal dispensaries to operate and are likely expected to be followed with dispensary operation at some later point. Overall, the features of the design and manner which cannabis environment changes take place instill confidence that anticipation effects are not expected to influence the estimates.

Lastly, changes in the composition of the treatment and control group can lead to issues for the internal validity of a DiD design. As the treatments in the design are implemented at the state level and the observations are of state outcomes themselves, the only discernible manner this could take effect in the design is if a state that implemented an MCL later repealed it, or if a state that had dispensaries later saw the dispensaries closed. This is not observed within the sample and as such this internal validity issue is not a concern for this paper's DiD design.

With a well-extended period of years before and after the passage of MCL policies and operation of dispensaries, as well as the proper selection of controls and control group states, this difference-in-difference specification can provide a robust methodology to arrive at a set of informative estimates.

# 7 Results

The results for the staggered Difference-In-Difference design analyzing changes in state opioid harm outcomes are discussed below. Two changes in the policy and health environments of the Southwestern states, the passage of medical cannabis legislation and the operation of dispensaries, are separately analyzed for estimated effects upon a state's opioid harm outcome. Four separate sets of controls are cumulatively added to compare estimates and more accurately capture the causal effect of the treatments. These four sets of sequentially added controls include yearly fixed effects, demographic covariates such as state population shares of whites, males, and individuals aged 15-34, 35-64, and 65+ years old, state economic covariates such as unemployment rates and beer excise taxes, and provider "Must Access" PDMP regulations. Estimates were obtained for the period of 1999 to 2013 and the period of 1999 to 2015 for all outcomes, with the exception of prescription opioid distribution, which lacks data for 1999 and is thus shortened to start at 2000 for both sample periods. One last distinction between specifications is the dropping of the state of Nevada from the 1999/2000 to 2013 specifications focused on dispensary effects. As a state can only have legal dispensaries after the passage of medical cannabis legislation, there are some concerns for collinearity between the two treatments masking the true causal effect of dispensaries. For specifications focusing on dispensary effects, this concern is alleviated if all states within the sample that adopt MCLs also have dispensaries, as the unobserved effect of medical cannabis legislation is uniformly confined to the treatment group and focus can be given to the difference in outcomes between states without MCLs and states with dispensaries. This does lead to the exclusion of one state, Nevada, from the dispensary specifications for the 1999/2000 to 2013 sample period, as Nevada did not have legal dispensaries until 2015. Whether the benefits from equalizing the unobservable effects of MCLs among the treatment group outweighs the loss of information provided by observations of Nevada's outcomes is uncertain; as such, 1999/2000-2013 dispensary specifications including Nevada as a control state are included as a set of sensitivity analyses.

The opioid outcomes analyzed have in the models are transformed in to logarithmic metrics in response to issues of non-normality within their respective distributions. All estimates obtained and displayed within the analyses tables reflect this transformation. As such, when interpreting estimated effects with significance, this paper converts the logarithmic values in to percentage changes with the following formula:

$$\triangle y\% = (e^{\text{coef}_y} - 1) * 100$$

Where a percentage change in opioid outcome $y$ is equal to the natural exponential value of the coefficient obtained $\text{coef}_y$, reduced by one and multiplied by 100. All models utilize standard errors clustered at the state level to minimize issues of heteroskedasticity and autocorrelation, and are tested for multicollinearity with the usage of variance inflation factors (VIFs). Variance inflation factors test for collinearity between each individual predictor with all other predictors in the model at once. This can be done by taking predictor $j$ and regressing it against all the other predictors at the same time, obtaining the model's respective $R_j^2$ value, then using said value to compute the predictor's VIF score with:

$$\text{VIF}_j = \frac{1}{1-R_j^2}$$

Where a VIF score higher than the minimum of 1 indicates inflated variance and standard errors for the predictor's baseline model coefficient. A $\text{VIF}_j > 1$ finding for a predictor does not necessarily indicate a significant multicollinearity issue unless the score is considered high enough. Scores considered the "upper limit" for a non-problematic predictor VIF range from 2.5 to 10, increasing in generosity as the upper limit is raised (Glen, 2015). This paper sets a relatively generous upper limit for predictor VIF scores at 10, where a finding of VIF scores in an individual model equal to or above the limit is determined to be an indication of problematic multicollinearity.

## 7.1 State Opioid Mortality Rate Analyses

Table 6: Difference-In-Difference Results: All Opioids Mortality Rates

| Log of Opioid Mortality Rate | 1999-2013 | | 1999-2015 | |
|---|---|---|---|---|
| | MCL | Dispensary | MCL | Dispensary |
| *A*: No Controls | 0.239 | 0.416*** | 0.303 | 0.322 |
| | (0.287) | (0.084) | (0.260) | (0.154) |
| R-Squared | 0.112 | 0.224 | 0.124 | 0.124 |
| *B*: Year Fixed Effects Only | -0.141 | -0.016 | -0.128 | -0.150 |
| | (0.432) | (0.113) | (0.416) | (0.166) |
| R-Squared | 0.467 | 0.541 | 0.486 | 0.486 |
| *C*: *B* + Demographic Covariates | -0.150 | -0.143‡ | -0.081 | 0.012 |
| | (0.164) | (0.204) | (0.164) | (0.210) |
| R-Squared | 0.892 | 0.910 | 0.884 | 0.882 |
| *D*: *C* + State Economic Covariates | -0.186 | -0.201‡ | -0.121 | 0.025 |
| | (0.146) | (0.197) | (0.150) | (0.172) |
| R-Squared | 0.903 | 0.923 | 0.892 | 0.888 |
| *E*: *D* + Provider Regulations | -0.231 | -0.209‡ | -0.189 | -0.088 |
| | (0.151) | (0.229) | (0.162) | (0.210) |
| R-Squared | 0.912 | 0.923 | 0.906 | 0.899 |
| Observations | 90 | 75† | 102 | 102 |

Notes: Values not in parentheses indicate estimated coefficients. Values in parentheses indicate standard errors clustered at the state level. Symbols *, **, and *** present next to a coefficient indicate significance at the 10%, 5%, and 1% level, respectively.

†: 1999-2013 dispensary effects specifications omit observations from Nevada.

‡: Covariates displayed high degrees of multicollinearity as estimated by variance inflation factor (VIF) scores.

The results obtained for estimating how changes to the state policy environment affected opioid mortality rates are displayed in Table 6 above. The results of the differing specifications do not seem to indicate a negative effect of the policies upon total opioid mortality rates. For nearly all specifications within the two separate sample time periods estimating the effects of the passage of medical cannabis

legislation, negative but insignificant coefficients were found. The only notable exception is specification *A* finding positive but insignificant effects of state medical cannabis legislation, which quickly changes signs once subsequent specifications add controls. Estimates of dispensary effects also found mostly negative and insignificant results for both periods, though with some deviations. The estimates obtained for specifications *C* and *D* of the 1999 to 2015 sample are both marginally positive, though still found to be insignificant. This sample period is notable for examining total opioid mortality rates as it provides more information on state outcomes coming under increased influence from an opioid environment changed by growth in heroin and synthetic opioid usage, along with more information on changes in state outcomes in general. The estimates of positive effects of these two specifications are slightly notable in being the only positive sign results found with use of controls in the analyses, but beyond that do not have any obvious implications for the effects of dispensaries on state opioid mortality rates. The dispensary specifications do find a highly significant estimated increase of state opioid mortality rates by 51.6% in specification *A* of the 1999 to 2013 sample period. This deviation from the pattern observed for MCL estimates quickly evaporates in subsequent dispensary specifications of the 1999-2013 sample, but it may indicate sensitivity of the sample to dropping observations of Nevada's outcomes. This same set of 1999-2013 dispensary specifications suffers from multicollinearity issues when including male shares of state populations as a control, indicated by high variance inflation factor (VIF) scores estimated in specifications *C*, *D*, and *E* for added covariates. This multicollinearity issue may also indicate sensitivity to dropping observations of Nevada from the sample, and may have affected the estimates obtained from the aforementioned specifications in unobservable ways. Overall, the estimates do not point towards a significant negative effect of medical cannabis legislation or dispensaries on opioid mortality rates, and highlight a certain degree of sensitivity to differing specifications.

In Table 7, a similar analysis is conducted of changes in prescription opioid mortality rates, focusing on an important and distinctive aspect of the US opioid environment policy-wise. The same general trend of negative but insignificant estimated effects was found throughout the differing treatments, specifications, and sample time periods. Few positive results were found, confined to specification *A* of the differing treatments and sample periods, though one result of significance was found within this group. Specification *A* of the 1999 to 2013 sample estimates dispensaries increased state prescription opioid mortality rates by 39.4% at the 1% significance level. However, in a similar pattern to the results pertaining to total opioid mortality rates, this estimated effect changes signs and drops in significance once subsequent specifications added controls. As the same model hints at in Table 6, there may be some sensitivity of the analyses to dropping Nevada from observations of state outcomes. Also in similar vein to Table 6, multicollinearity issues arise in specifications *C*, *D*, and *E* from the inclusion of male shares of state populations as a control for the 1999 to 2013 dispensary effect estimates. Overall, the results once again provide little support for the hypotheses that medical cannabis legislation or medical cannabis dispensaries have decreasing effects on opioid overdose mortality rates, more specifically prescription opioid mortality rates.

Table 7: Difference-In-Difference Results: Prescription Opioid Mortality Rates

| Log of Prescription Opioid | 1999-2013 | | 1999-2015 | |
|---|---|---|---|---|
| Mortality Rate | MCL | Dispensary | MCL | Dispensary |
| *A*: No Controls | 0.239 | 0.332*** | 0.272 | .165 |
| | (0.313) | (0.336) | (0.280) | (0.167) |
| R-Squared | 0.150 | 0.265 | 0.165 | 0.143 |
| *B*: Year Fixed Effects Only | -0.164 | -0.162 | -0.333 | -0.310 |
| | (0.466) | (0.105) | (0.448) | (0.202) |
| R-Squared | 0.468 | 0.558 | 0.476 | 0.489 |
| *C*: *B* + Demographic Covariates | -0.158 | -0.228‡ | -0.131 | -0.122 |
| | (0.165) | (0.190) | (0.155) | (0.217) |
| R-Squared | 0.903 | 0.923 | 0.901 | 0.900 |
| *D*: *C* + State Economic Covariates | -0.201 | -0.291‡ | -0.177 | -0.096 |
| | (0.138) | (0.182) | (0.132) | (0.176) |
| R-Squared | 0.920 | 0.936 | 0.914 | 0.909 |
| *E*: *D* + Provider Regulations | -0.231 | -0.287‡ | -0.219 | -0.178 |
| | (0.136) | (0.192) | (0.134) | (0.181) |
| R-Squared | 0.923 | 0.936 | 0.918 | 0.913 |
| Observations | 90 | 75† | 102 | 102 |

Notes: Values not in parentheses indicate estimated coefficients. Values in parentheses indicate standard errors clustered at the state level. Symbols *, **, and *** present next to a coefficient indicate significance at the 10%, 5%, and 1% level, respectively.

†: 1999-2013 dispensary effects specifications omit observations from Nevada.

‡: Covariates displayed high degrees of multicollinearity as estimated by variance inflation factor (VIF) scores.

## 7.2 State Opioid Abuse Treatment Admissions Analyses

Turning our focus towards understanding the effects of medical cannabis policies upon opioid abuse, the results of analyzing logged admissions to treatment facilities per capita are displayed in Table 8 below. The estimates do not provide evidence favorable towards the posited decreasing effect of the treatments towards opioid abuse metrics. Not only are no negative effects with significance estimated in the models, there are no negative estimates to be found in the models at all. For the 1999 to 2013 estimates of MCL and dispensary effects, specification *A* finds significantly positive effects for both treatments. For medical cannabis legislation, it is estimated it leads to a 141.3% increase in state treatment admissions per capita at the 5% significance level. Meanwhile, the same specification estimates the effect of dispensaries to be a 151.7% increase in state treatment admissions per capita at the 5% significance level. The subsequent specifications of *B* through *E* found insignificant, if still positive estimates of decreasing magnitude for both treatment effect estimates, highlighting the sensitivity of the estimates to including additional covariates. Turning to the sample period of 1999 to 2015, no significant estimates were obtained for the

effects of medical cannabis legislation, though all remained positive much like the shorter sample period. Almost all estimates of dispensary effects upon state treatment admissions per capita were found to be positive but insignificant, with the exception of specification $A$. The specification estimates a 105.7% increase in admissions per capita at the 10% significance level resultant from such a policy environment change. In similar vein to other models with differing sample periods, policy environment treatments, and even opioid harm outcomes, this finding of significant effect quickly disappears with the addition of other covariates in the subsequent specifications. Undoubtedly, the estimates throughout the models in Table 8 do not support the hypotheses expecting a drop in treatment admissions per capita from the treatments, as not one estimated effect was even predicted to decrease the opioid harm outcome. Other observations of note from the results include a continuing pattern of reduced significance for the policy environment changes when controls are included, as well as a continuing pattern of multicollinearity observed from the inclusion of male shares of a state's population as a control in 1999-2013 dispensary specifications $C$ and onwards.

Table 8: Difference-In-Difference Results: Opioid Abuse Treatment Admissions Per Capita

| Log of Treatment Admissions | 1999-2013 | | 1999-2015 | |
|---|---|---|---|---|
| Per Capita | MCL | Dispensary | MCL | Dispensary |
| $A$: No Controls | 0.881** | 0.923** | 0.391 | .721* |
| | (0.339) | (0.321) | (0.356) | (0.302) |
| R-Squared | 0.343 | 0.214 | 0.117 | 0.209 |
| $B$: Year Fixed Effects Only | 0.601 | 0.599 | 0.090 | 0.272 |
| | (0.380) | (0.367) | (0.391) | (0.305) |
| R-Squared | 0.464 | 0.411 | 0.391 | 0.393 |
| $C$: $B$ + Demographic Covariates | 0.180 | 0.300‡ | 0.290 | 0.448 |
| | (0.335) | (0.267) | (0.381) | (0.241) |
| R-Squared | 0.772 | 0.830 | 0.754 | 0.771 |
| $D$: $C$ + State Economic Covariates | 0.117 | 0.183‡ | 0.224 | 0.404 |
| | (0.313) | (0.194) | (0.373) | (0.242) |
| R-Squared | 0.784 | 0.857 | 0.769 | 0.784 |
| $E$: $D$ + Provider Regulations | 0.017 | 0.129‡ | 0.110 | 0.238 |
| | (0.304) | (0.314) | (0.366) | (0.309) |
| R-Squared | 0.813 | 0.858 | 0.794 | 0.799 |
| Observations | 90 | 75† | 102 | 102 |

Notes: Values not in parentheses indicate estimated coefficients. Values in parentheses indicate standard errors clustered at the state level. Symbols *, **, and *** present next to a coefficient indicate significance at the 10%, 5%, and 1% level, respectively.

†: 1999-2013 dispensary effects specifications omit observations from Nevada.

‡: Covariates displayed high degrees of multicollinearity as estimated by variance inflation factor (VIF) scores.

## 7.3 State Prescription Opioid Distribution Analyses

Table 9 below displays the analyses' estimated effects of the two state policy environmental changes upon state prescription opioid distribution per capita. In general, almost none of the estimates obtained indicated a significant effect of the separate treatments upon prescription opioid distribution rates, measured by logged morphine milligram equivalents (MMEs) per capita. The only exception is specification $A$ of 2000-2013 dispensary treatment effects, which estimates that dispensaries increase state prescription opioid distribution by 71.9% at the 1% significance level. Subsequent specifications within the same sample period lose significance and all except for specification $E$ change signs. Specifications $C$ through $E$ of the same sample period and treatment show signs of multicollinearity within the models once male shares of state populations are included as a control, similar to previous analyses with other opioid outcomes. Overall, a similar pattern to previous analyses on opioid mortalities and treatment admissions can be found. All specifications that add covariates find insignificant effects of the policy environment changes upon prescription opioid distribution per capita. Estimates are mostly negative, though they oscillate between positive and negative values with some models, offering marginal predictive value of any possible effect not isolated by the design. Without significance, these results provide little evidence that the treatments reduced prescription opioid distribution.

Table 9: Difference-In-Difference Results: Prescription Opioid Distribution Per Capita

| Log of Morphine Milligram | 2000-2013 | | 2000-2015 | |
|---|---|---|---|---|
| Equivalents (MME) Per Capita | MCL | Dispensary | MCL | Dispensary |
| *A*: No Controls | 0.533 | 0.542*** | 0.562 | .309 |
| | (0.372) | (0.131) | (0.348) | (0.211) |
| R-Squared | 0.113 | 0.118 | 0.126 | 0.038 |
| *B*: Year Fixed Effects Only | 0.062 | -0.091 | 0.053 | -0.299 |
| | (0.330) | (0.142) | (0.319) | (0.162) |
| R-Squared | 0.789 | 0.916 | 0.789 | 0.809 |
| *C*: *B* + Demographic Covariates | -0.008 | -0.054‡ | -0.046 | -0.096 |
| | (0.068) | (0.058) | (0.058) | (0.053) |
| R-Squared | 0.980 | 0.989 | 0.976 | 0.977 |
| *D*: *C* + State Economic Covariates | -0.002 | -0.035‡ | -0.039 | -0.092 |
| | (0.063) | (0.057) | (0.054) | (0.053) |
| R-Squared | 0.980 | 0.991 | 0.976 | 0.978 |
| *E*: *D* + Provider Regulations | 0.026 | 0.010‡ | -0.002 | -0.042 |
| | (0.070) | (0.041) | (0.056) | (0.034) |
| R-Squared | 0.982 | 0.992 | 0.979 | 0.980 |
| Observations | 84 | 70† | 96 | 96 |

Notes: Values not in parentheses indicate estimated coefficients. Values in parentheses indicate standard errors clustered at the state level. Symbols *, **, and *** present next to a coefficient indicate significance at the 10%, 5%, and 1% level, respectively.

†: 1999-2013 dispensary effects specifications omit observations from Nevada.

‡: Covariates displayed high degrees of multicollinearity as estimated by variance inflation factor (VIF) scores.

## 7.4 Implications of Results

Readily apparent is that the estimates of the effects of medical cannabis legislation and legal dispensary establishment do little to support the hypotheses of the design. General trends of insignificance as subsequent specifications add controls are prevalent between the different sample time periods for estimated effects of either change to the medical cannabis environment upon opioid harm outcomes. Whereas previous works find significantly negative effects of MCLs and legal dispensaries upon opioid mortality rates, opioid treatment admission rates, (Powell et al., 2018; Shi, 2017; Chan et al., 2020) and prescription opioid distribution rates (McMichael et al., 2019), the vast majority of models above estimate insignificant and sometimes positive effects of the cannabis environment changes upon opioid harm outcomes. A persistent exception between the different analyses of all of the opioid outcomes exists in specification *A* when examining the effects of dispensaries for the sample period between 1999 (or 2000 for prescription opioid distribution) to 2013 with no controls in place. These models consistently find very significant positive effects of dispensaries on all opioid outcomes. As outlined before, such results may

indicate sensitivity of the design to dropping Nevada from the data - a possibility supported by unique concerns of multicollinearity within later specifications of the same sample period and treatment models, which are consistently found between different opioid outcome analyses. As such, in consideration of these unsupportive results as well as additional possibilities to test the data, the following sensitivity analyses are all the more prudent for providing additional context to the main results.

## 8 Sensitivity Analyses

First, an additional set of analyses are conducted into changes in prescription opioid mortality rates during the period of 1999 to 2010. During this period, prescription opioid distribution, abuse, and overdoses grew at an alarming pace, before they slowed and eventually stabilized between 2010 to 2011 (Ruhm, 2018; Jones et al., 2018). As such, by shortening the sample period to 2010, a conspicuous change in the growth trend can be removed from the variation observed in state overdose outcomes, though at the cost of losing additional information granted by observations of state outcomes after 2010. The results of these models are displayed in Table 10 below.

Table 10: DiD Sensitivity Analysis: Prescription Opioid Mortality Rates (1999-2010)

| Log of Prescription Opioid Mortality Rate | 1999-2010 | |
|---|---|---|
| | MCL | Dispensary |
| *A*: No Controls | 0.152 | 0.161 |
| | (0.368) | (0.217) |
| R-Squared | 0.117 | 0.158 |
| *B*: Year Fixed Effects Only | -0.156 | -0.140 |
| | (0.498) | (0.371) |
| R-Squared | 0.434 | 0.444 |
| *C*: *B* + Demographic Covariates | -0.157 | -0.259***‡ |
| | (0.165) | (0.058) |
| R-Squared | 0.903 | 0.923 |
| *D*: *C* + State Economic Covariates | -0.176 | -0.288***‡ |
| | (0.145) | (0.031) |
| R-Squared | 0.919 | 0.936 |
| *E*: *D* + Provider Regulations | -0.159 | N/A§ |
| | (0.143) | |
| R-Squared | 0.929 | |
| Observations | 72 | 48† |

Notes: Values not in parentheses indicate estimated coefficients. Values in parentheses indicate standard errors clustered at the state level. Symbols *, **, and *** present next to a coefficient indicate significance at the 10%, 5%, and 1% level, respectively.

†: 1999-2010 dispensary effects specifications omit observations from Nevada and Arizona.

‡: Covariates displayed high degrees of multicollinearity as estimated by variance inflation factor (VIF) scores.

§: No observations in sample with "Must Access" Prescription Drug Monitoring Programs in place, specification not possible.

Patterns of MCL estimated effects reflect the coefficients found within sample periods 1999-2013 and 1999-2015 of Table 7, with an initial positive predicted effect in specification *A* turning negative as additional covariates are added. However, this shortened sample period offers little in the way of interpretable estimates of the effect of medical cannabis legislation upon prescription opioid mortalities - no specification finds any significant effect of the policy change. Before examining the results of the 1999 to 2010 specifications for dispensary effects, it is important to note that the shortened sample period leads to the exclusion of both Nevada and Arizona from the sample. Nevada and Arizona, while seeing state MCLs enacted during the sample period, do not see the establishment of legal dispensaries until 2015 and 2012, respectively, outside of the sample period. As such, observations of their outcomes are omitted from the sample to prevent unobserved variation stemming from their MCLs from influencing the control group and thereby the estimated effects of dispensaries. With this in mind, specifications *C*

and $D$ estimate that dispensaries lead to highly significant drops in state prescription opioid mortality rates of 29.6% and 33.4%, respectively, at the 1% significance level. However, much like previous analyses of dispensary effects that involved dropping a state from the sample, issues of multicollinearity with these specifications cloud the interpretability of these estimates. This again raises the possibility of sensitivity of the design to dropping certain states from the analysis.

The second analysis conducted pertains to the sensitivity of the 1999 to 2013 dispensary models that dropped the state of Nevada. These models showed some deviance from others within their respective outcome analysis groups, displaying differing patterns of significance in their estimated effects and possibly being affected by multicollinearity issues for some control specifications. As such, these same models for dispensary effects on state opioid outcomes are analyzed again without dropping observations of Nevada's outcomes. The results are displayed in Table 11 below.

Table 11: DiD Sensitivity Analysis: Active and Legal Dispensary Effects Including Nevada (1999-2013)

| 1999-2013: Dispensary Effects | Logged State Opioid Outcomes (Rates) | | | |
|---|---|---|---|---|
| | Any Overdose | Rx† Overdose | Abuse Admission | Distribution |
| $A$: No Controls | 0.416*** | 0.332*** | 0.824** | 0.542*** |
| | (0.082) | (0.073) | (0.260) | (0.128) |
| R-Squared | 0.257 | 0.296 | 0.198 | 0.143 |
| $B$: Year Fixed Effects Only | 0.008 | -0.088 | 0.352 | -0.023 |
| | (0.101) | (0.085) | (0.347) | (0.137) |
| R-Squared | 0.577 | 0.602 | 0.371 | 0.852 |
| $C$: $B$ + Demographic Covariates | -0.048 | -0.147 | 0.371 | -0.018 |
| | (0.199) | (0.190) | (0.227) | (0.054) |
| R-Squared | 0.890 | 0.912 | 0.784 | 0.985 |
| $D$: $C$ + State Economic Covariates | -0.082 | -0.176 | 0.318 | -0.017 |
| | (0.202) | (0.192) | (0.225) | (0.049) |
| R-Squared | 0.893 | 0.915 | 0.794 | 0.986 |
| $E$: $D$ + Provider Regulations | -0.197 | -0.274 | 0.114 | 0.025 |
| | (0.245) | (0.215) | (0.313) | (0.039) |
| R-Squared | 0.903 | 0.921 | 0.815 | 0.987 |
| Observations | 90 | 90 | 90 | 90 |

Notes: Values not in parentheses indicate estimated coefficients. Values in parentheses indicate standard errors clustered at the state level. Symbols *, **, and *** present next to a coefficient indicate significance at the 10%, 5%, and 1% level, respectively.

†: "Rx" is an abbreviation for "Prescription".

In general, the estimates above reflect trends of significance, magnitude, and sign observed in the original 1999 to 2013 dispensary models conducted without Nevada. All estimates of significant effects are confined to specification $A$ of the differing state opioid outcome models, something which was observed

of the original models without Nevada. This specification estimates dispensaries increase state overall opioid overdose rates by 51.6%, prescription opioid overdose rates by 39.4%, and prescription opioid distribution rates by 71.9%, all at the 1% significance level. The specification also estimates the policy environment change would increase substance abuse treatment admission rates by 128.0% at the 5% significance level. Further specifications for all of the outcomes of interest do not display any significance, much like their counterpart analyses which exclude Nevada in Section 7. While these results do little to provide additional evidence of a causal effect from the policy environment changes, they do offer additional context to consider how sensitivity to removing the state of Nevada from the sample may have manifested in the main analyses. Two observations can be made from this sensitivity analysis. First, none of the control specifications for any opioid outcome analysis displayed any conspicuous signs of multicollinearity, which is an issue that may be affecting some of their counterpart analyses without Nevada, based on VIF analyses conducted for both. Second, specification *A* of both the sensitivity analyses and main analyses of the 1999-2013 opioid outcomes universally finds significantly positive dispensary effects, with only one estimate found to have a significance level other than 1%. Given that most of the 1999-2013 dispensary analyses including Nevada find estimates of similar magnitude, sign, and significance relative to their main analyses counterparts, this may indicate that any sensitivity to dropping Nevada from the design does not strongly manifest in the treatment estimates themselves. While the specter of multicollinearity still looms over some of the main 1999-2013 dispensary analyses in Section 7, which may be resultant from excluding Nevada from their samples, it is possible the main estimates may not be have been affected extensively from this exclusion.

# 9 Discussion

Before diving into the implications of the results for the hypotheses defined earlier in the paper, discussion must be had as to how they relate to the underlying theory driving the hypotheses, the design used, and how the data may have affected the estimates in adverse ways. From there, the results can be discussed on how they compare and contrast with previous literature.

## 9.1 Theoretical Framework

The results obtained from the main analyses offer little in support to the principal idea of the hypotheses: that harm associated with opioid abuse, including overdose, is reduced by legalization and accessibility to medical cannabis. This is in opposition of the paper's particular permutation of Becker and Murphy (1988)'s rational choice model of addiction considered to drive such a relationship. The adjusted model considered medical cannabis as a channel through which a potential opioid addict could pay a cost to lessen or avoid an initial exogenous impulse to enter the cycle of opioid addiction. The two policy environment changes, passage of medical cannabis legislation and the active operation of legally protected dispensaries, were factored in to reduce the marginal cost of medical cannabis, and therefore reduce the cost of avoiding opioid consumption and addiction, which would then reduce state metrics of opioid overdoses and opioid abuse treatment, as well as prescription opioid distribution via less demand

for prescription opioids. However, the results do not support the validity of this theoretical model, as nearly all estimates within the main models obtained for either medical cannabis environment change were insignificant. There are multiple potential explanations for why the adjusted rational addiction model found little validation here. First, the role medical cannabis plays in the addiction model, which is purely as a channel through which exogenous events inducing addictive consumption are avoided, may be too limited or even incorrect. For one, cannabis is not treated as a fully functional good with its own effects on utility - or addictive potential. Accounting for these potential features complicates the theoretical relationship between cannabis consumption and opioid addiction posited by the model. A consumer may respond to drops in cannabis cost spurred by the policy environment changes differently than if cannabis was just a simple channel to reduce exogenous incentives to consume opioids. A positive utility-generating cannabis good would have ulterior motives for consumption and thus more response to cost reductions, but an addictive cannabis good would mean rational consumers would be less incentivized to consume it, indicating lower response to cost reductions. To summarize, the overly-simplified manner in which cannabis consumption is included in the paper's adjusted Becker-Murphy model may not be representative of the relationship between it and opioid consumption and addiction. A desired refinement of the model for future work on this research topic would be to reformulate the relationship in to a more realistic role. This, however, is well outside of the scope of this current paper.

On the other hand, the general Becker and Murphy model may not be appropriate for understanding the relationship between medical cannabis and opioid harm. As other discussions of the model have pointed towards (Skog, 1999; Orphanides and Zervos, 1999; Vale, 2010), there are issues with Becker and Murphy (1988)'s model in explaining why consumers would rationally enter in to a cycle of addiction by consuming an addictive good. The only rational impetus formulated in the model, beyond the condition of addiction itself, is an exogenous traumatic event occurring which spurs an initial consumption of an addictive good, thus beginning the cycle of addiction. In this paper, the exogenous traumatic event was considered a given for those affected by the medical cannabis policy and at risk of opioid consumption. Those who were in the sample states and suffered some sort of trauma, like injuries or chronic conditions, and would be spurred to rationally consume addictive opioids would also potentially be affected by state policies to reduce the costs of channels to avoid addiction, which was theorized to be accomplished by adoption of medical cannabis legislation and operation of active and legal dispensaries. This is all predicated on the idea that, whether motivated by pain treatment or otherwise, medical cannabis could provide a substitute for opioids for whatever purpose rational consumers in the model sought out the latter good. However, its possible that those at risk or in the process of abusing opioids in the sample were not motivated by traumatic events of pain, nor previous addiction, and that the criticism of vague explanation as to what motivators incite addictive consumption may ring true, true enough that medical cannabis does not in fact serve as a substitute for the purpose of opioid consumption. This is not to say the theoretical model may not be applicable for certain individuals in the sample, and that medical cannabis legislation did not offer an alternate path for rational at-risk consumers to avoid opioid addiction. However, it is indeed possible that the model could not be relevant to a large enough proportion of at-risk consumer situations for medical cannabis availability to demonstrate a significant decreasing effect on

opioid harm.

One last consideration for understanding the lack of evidence for the theoretical model's predictions is that the focus on state-level outcomes of opioid metrics may have been too imprecise for usage of the Becker and Murphy model, in that heterogeneous responses amongst individuals to the cannabis environment changes may have been washed out at the state level. This concern is at the junction of the theoretical model, its possible issues, and the design of the analyses. Sources on opioid addiction highlight how certain demographics, such as whites and older individuals, are disproportionately found to be at risk of becoming opioid addicts, but this aggregate trend belies the underlying complexity of why an individual would become an addict in the first place. Discussion of other addictive goods and their consumption in Section 3 is very cognizant of the complexity of addiction, with propositions that some individuals will always be at risk of consuming addictive substances (Moore, 2010) and that reactions to policies to reduce addictive good consumption are highly heterogenous amongst population subsets (Subbaraman, 2016). Controls for state demographics empirically afflicted with addiction can address some of this heterogeneity. However, without further analysis in to the responsiveness of demographic subsets, or with perfect information the responsiveness of subsets of individuals with differing predispositions to addiction, predictions with the Becker and Murphy model must be tested via analysis of responses aggregated at the state level. As such, the lack of evidence for the hypotheses and for the Becker and Murphy model's predictions could have been partially resultant from the relative imprecision of state-level outcomes.

These potential shortcomings of the adjusted Becker and Murphy model and its interaction with the design may offer some insight as to how the analyses could not find significant negative effects of medical cannabis legislation and dispensaries upon opioid overdose, abuse, and prescription opioid distribution. However, further consideration of the data, sample, and design offers relatively mechanical possibilities as to how the hypotheses could not be validated.

## 9.2 Design and Data

In this paper's design, all of the analyses are conducted with six states out of all fifty states, excluding the District of Columbia and US Territories. This set of Southwestern states was selected to group together a set of states with relatively similar characteristics and geographic proximity in order to reduce unobserved differences between the control states and treated states, as well as determine how medical cannabis policy changes affected this distinct region of the United States. However, in electing to focus on such a relatively small sample of states there is a loss of variation between state outcomes that could have allowed for better isolation of the potential treatment effects of the two medical cannabis environment changes. This small set of six states, five for 1999-2013 dispensary specifications, observed for sample periods ranging from 11 to 17 years also generates a statistically low number of observations. The potential effects of this small sample size are particularly visible when considering specifications that drop the state of Nevada had been found to suffer from multicollinearity - a finding that did not hold in the sensitivity analysis which conducted these same specifications again without dropping Nevada. As such, my finding of no significant treatment effects of either type of medical cannabis change could be resultant from this narrower focus, as well as other issues related to a smaller sample size.

The usage of a staggered difference-in-difference methodology could also have had problematic implications for the overall design. While challenges to the internal validity of the design such as endogeneity of the treatments, anticipation effects, and changes in group composition are largely absent, the identifying assumption of a common trend within the sample of states is not comprehensively validated. This paper tests and does not find consistent evidence against a common trend within the treatment group, as shown in Table 5. However, the lack of evidence against the treatment group's common trend is not absolute, and the staggered implementation of the MCLs does not allow the tests to offer insight in to whether a common trend is shared between the treatment and control group. It could be erroneous to assume that the medical cannabis states and the illicit-cannabis states reasonably share a common trend of opioid outcomes, and that the difference-in-difference design is thus able to arrive at valid estimates of the treatment effects of the medical cannabis environment changes.

Within the sample states, there is a stark possibility of bias via the inclusion of two states within the analysis: Colorado and Utah. As previously noted, Colorado approved the legalization of recreational cannabis in 2012 and saw operation of recreational cannabis dispensaries in 2014, both steps of which increased cannabis access beyond the cannabis environment changes analyzed by this paper's design. If the hypotheses of cannabis access leading to reductions in opioid harm holds merit, such changes leading to recreational cannabis access would lead to reductions on Colorado's opioid harm metrics within the years following access to recreational cannabis, which at a minimum introduces bias threatening the validity of the design's common trend assumption between sample groups. Turning to the control group state of Utah, this state implemented a unique reform relative to other cannabis legislation in 2014 with the legalization of medical cannabis products with low-THC and high-CBD. While some clinical trial evidence points towards a role for high-CBD products in reducing opioid use disorder (Wiese and Wilson-Poe, 2018), there is little empirical evidence on the effects of such high-CBD cannabis legislation. If access to such products were to reduce state metrics of opioid harm, it may be expected that inclusion of Utah's 2014 and 2015 annual opioid harm outcomes could introduce noise in to the 1999/2000 to 2015 analyses, positively biasing the treatment estimates upon opioid overdoses and abuse and weakening the validity of the common trend assumption.

Looking at other state characteristics noted in Table 3 and Appendix Table A2, the two sample groups seemingly differ on average between several demographic and economic factors. In Appendix Table A3, two-sample t-tests conducted to determine if state characteristics significantly differ between the sample groups are shown, many of which find results affirming our previous observation. For the overall 1999 to 2015 period, population shares of non-Hispanic whites, individuals aged 15-34, 35-64, and at youngest 65 years old, Hispanics, and those living in non-rural metropolitan areas differ significantly on average between MCL and non-MCL states. Additionally, average unemployment rates and population sizes also significantly differ between groups for the sample period. The finding of a multitude of significant observable differences between the sample groups could indicate that the common trend assumption stands on unstable foundation, and that the difference-in-difference design is inappropriate for analyzing this sample. While some of these covariates are controlled for, with attention given to characteristics associated with the opioid environment such as age groups, population proportions of whites, and unemployment

rates, not all characteristics are included in the design, and the risk of unobservable differences between groups driving opioid outcome variation stands as a non-dismissible threat to the validity of the analysis.

Turning away from the potential implications of the sample selection, consideration of the data sources used for compiling the opioid metrics may also add context as how to understand the non-significant estimates. While all three data sources came with their own limitations, as discussed in Section 5, the data sources for opioid abuse treatment admissions and prescription drug distribution may have had flaws that actually affected the estimates.

Looking at opioid abuse admissions per-capita, a potential issue pertaining to usage of the TEDS-A dataset lies in how this paper obtained state counts for opioid abuse from the TEDS-A dataset. The dataset reports each individual observation of admission to a publicy-funded substance abuse facility, with each observation including information on substance found to be used, as well as a categorization of the reason for admission.[19] Such categorizations of admission purpose, including "Opioid Dependence" and "Opioid Abuse", were not used to identify admissions considered to be related to opioid abuse by this paper. Instead, this paper counts an admission as being related to opioid abuse treatment if the admission is found to have any type opioid in their body upon admission at the facility. This was done in order to have a broad definition of opioid abuse based on objective findings, as opposed to the relatively subjective categorization of treatment reason, which could have suffered from reporting bias. It is noted that the count of admissions with any opioid in their body, used for this paper's analysis, exceeds the count of admissions categorized as being for opioid abuse or opioid dependence. In obtaining the opioid abuse admission counts with this more liberal definition, it is possible that the treatment estimates obtained from the design for the opioid abuse treatment admission outcome may have been fundamentally different than those that would be obtained with this other method of counting opioid abuse admissions.

Turning to data obtained for prescription opioid distribution from the ARCOS data source, two quirks of the data may have had impacts on the estimates obtained for the per-capita prescription opioid distribution design. First, the numbers for per-capita prescription opioid distribution were not provided by an analyzable ARCOS dataset, but were hand-compiled from ARCOS retail drug summary reports. As such, each per capita milligram measurement for each prescription opioid drug flow had to be input by hand for each state by year. This process opens up room for user error when inputting the distribution values for each prescription opioid. While I consider it important to note the possibility of user error, my subjective assessment is that such error has not occurred to any significant degree that could have affected the analysis. Second, several prescription opioid types were not reported in the retail drug summary reports of 2000 to 2005, with the most drugs omitted in the 2000 report. This could occur without note if no drugs of the type were distributed during a report's time period; however, multiple prescription opioid types were observed to be omitted inconsistently for the differing reports from 2000 to 2005, some of which were shown to have significantly large amounts distributed during years they were included, large enough to significantly affect the final MME per capita values used by the prescription opioid distribution analyses. No documentable reason has been found as to why these impactful opioid

[19]This categorization is more specifically a diagnosis according to the Diagnostic and Statistical Manual of Mental Disorders (DSM).

types were omitted from these early summary reports, and by not being included in the reports may lead to systematically lower prescription opioid distribution outcomes between 2000 to 2005. As some medical cannabis legislation was passed later in the sample periods, and most active and legal dispensaries came into effect later in the sample periods, by systematically underreporting prescription opioid distribution before the reforms were in place, the treatment estimates may have been biased upwards, relatively more so for dispensary effect estimates. This would push the prescription opioid distribution results away from validating the hypotheses, assuming the bias also reduced significance of the treatment estimates.

## 9.3 Results In Context and Previous Literature

With this final consideration of how the underlying theoretical model may be inappropriate, how the difference-in-difference design may not have worked well with the sample selection of states, and how the data's flaws and use may have introduced bias in to the estimates, we at last turn back to the results and the sensitivity analyses. Undoubtedly, the main results do not support either of the hypotheses. There was no instance where a significant negative effect of medical cannabis legislation or dispensaries was estimated for any opioid outcome, regardless of the sample period or control specification. The only significant estimates found, referring to the 1999 to 2013 dispensary specifications excluding Nevada, pointed towards a positive effect of the policy environment changes, though these estimates were quickly washed out with the addition of controls. Alternative models and specifications, both in the main analyses and the sensitivity analyses, did not lend to the veracity of the hypotheses as well. While two alternate sample periods, 1999/2000 to 2013 and 1999/2000 to 2015, were included in the analyses for more usage of available outcome information as well as differing levels of incorporation of heroin and synthetic opioid harm trends, neither alternative presented any supportive results for any of the hypotheses. Narrowing opioid mortalities down to prescription opioid mortalities, and subsequently shortening the sample period as in the sensitivity analysis displayed in Table 10, did little to offer credible[20] support for either of the hypotheses as well. Sensitivity analyses in Table 11 conducted to address the issue of multicollinearity in the 1999/2000 to 2013 analyses of dispensary effects did not contradict the findings of insignificance in the main analyses either. As such, the main analyses point towards an unmitigated failure to reject either hypothesis 1 or 2. First, medical cannabis legislation is not found to be negatively associated with state rates of general opioid overdose, prescription opioid overdose, opioid abuse treatment admissions, and prescription opioid distribution, as posited by hypothesis 1. Second, the beginning of active operation of legally-protected dispensaries is not found to be negatively associated with state rates of general opioid overdose, prescription opioid overdose, opioid abuse treatment admissions, and prescription opioid distribution, as posited by hypothesis 2. This paper's design does not contribute to the idea that medical cannabis legalization and increased cannabis availability can provide a beneficial reduction of the harms caused by the US Opioid Crisis.

Previously in the Literature Review Section 3, a spread of earlier research was discussed that

[20] Though specifications $C$ and $D$ of the 1999-2010 prescription opioid mortality rate analysis for dispensary effects did find very significant negative effects, issues of serious multicollinearity highlighted by high VIF scores cloud the interpretability of these results.

involved the effects of medical cannabis upon the US Opioid Crisis. The majority of research reached conclusions that are in relative opposition to the findings of this paper. The most obviously comparable work to compare results with is Powell et al. (2018), as this paper shares much of the same design, data sources, and outcomes of interest. Their research's results differ in that operation of legal dispensaries was found to significantly decrease logged state opioid overdose rates, in separate specifications including only prescription opioids or prescription opioids and heroin, for two different sample periods of 1999 to 2010 and 1999 to 2013. They also find significant decreases in logged state opioid treatment admissions resultant from operation of legal dispensaries within sample period 1999 to 2010 and two separate set of specifications for prescription opioids or prescription opioids and heroin. Some differences in their design and this paper's design, and how they pulled data for analysis from the datasets both papers share, could explain this disparity. First, Powell et al. (2018) has a much broader difference-in-difference design than my own, including all fifty US states and the District of Columbia, trading less precision in isolating regional effects of medical cannabis legislation with more general applicability and variation for their analysis. Second, their difference-in-difference regression formula include state fixed effects and logged population size, both of which were found to lead to high degrees of multicollinearity in my design as captured by VIF analysis. They also included state dummy variables indicating active medical cannabis legislation and active and legal dispensaries at the same time within their specifications estimating dispensary treatment effects. This is a fundamentally different formulation from my own dispensary specifications, which only includes a dummy variable for active and legal dispensaries and not for medical cannabis legislation, and may be expected to arrive at different estimates from my own as a result. With these underlying differences between this highly comparable study and my paper in mind, it is also worth noting that some common ground is found in their results. Powell et al. (2018)'s analysis of changes to logged state prescription opioid distribution, outcomes of which are similarly gathered from the ARCOS, resultant from medical cannabis environment changes are found to be insignificant, same as the results obtained by this paper's design in Table 9. Additionally, their analysis of logged state opioid treatment admissions for sample period 1999 to 2013 estimated an insignificant association between the outcome and active, legal dispensaries, which is similar to this paper's own results for those specifications. As a whole, Powell et al. (2018) provides a valuable, if not always favorable, source of comparison for the results obtained by this paper's design.

A couple of other studies that conduct analyses comparable to this paper's design find contradicting results as well. Chan et al. (2020) focuses on state opioid mortality rates gathered from the same dataset as used by this paper with a sample period from 1999 to 2017 and a sample including all fifty US states and the District of Columbia. With difference-in-difference specifications that include indicators for active medical cannabis legislation, recreational cannabis legislation, and legal dispensaries all in one regression, they find that medical cannabis and recreational cannabis dispensaries reduce logged state opioid mortality rates, both for all types of opioids and for specifically synthetic opioids. Again, there are some notable differences in their design from this paper's design. They have a much broader scope by including all fifty states and the District of Columbia, and have a longer sample period than any of my specifications, ending in 2017 and resultantly capturing more of the growth in synthetic opioid harm

observed in these few additional years. They also use additional controls such as state fixed effects and median income, though their choices to formulate their regression with indicators for all of the differing cannabis environment changes compiled into one formula and include states with recreational cannabis laws both undoubtedly lead to fundamentally different results than my own dispensary specifications. The similarity of their results in sign, significance, design, and type of cannabis environment change to Powell et al. (2018)'s results for opioid mortality rates does strengthen these two separate studies' findings against my own, though again the differences between their studies and my own undoubtedly plays a role in this paper's dissimilar results. Meanwhile, Chu (2015), using a difference-in-difference design and the TEDS-A dataset, concludes that heroin-related treatment admissions significantly decrease relative to overall treatment admissions for the sample period of 1992 to 2011 whenever states implement medical cannabis legislation. The study uses a differing sample period, a different set of controls including state fixed effects and time trends, a sample that includes only states that pass medical cannabis legislation before 2012, and a different outcome variable to measure public burden of opioid abuse, opting to focus on state treatment ratios of heroin relative to other admissions rather than heroin admissions rates in similar vein to my own outcome. While these undoubtedly could explain some deviation in his results leading to a differing conclusion on opioid abuse treatment changes than my own, the usage of the same dataset as my own along with the similarity of his study's conclusions on opioid abuse treatment to Powell et al. (2018) does indicate that scrutiny of my differing conclusion is warranted.

Other adjacent studies previously discussed, Shi (2017) and Conyers and Ayres (2020), do not quite so cleanly call in to question this paper's results. The former study, using a time-series design for a sample with information on state-level hospitalizations throughout the United States,[21] finds that between 1997 to 2014 states with medical cannabis legislation suffered less growth on average in hospitalizations for opioid abuse/dependence and opioid overdoses than non-MCL states - though active dispensaries were not found to have any independent effect. While the outcome and data source do differ from those analyzed in this paper, the conclusion by Shi (2017) does point in a direction of the role of medical cannabis access which the other literature is in favor of, while my own results do not support. However, Conyers and Ayres (2020)'s robust difference-in-difference analysis of Arizona emergency room visits between 2010 to 2016 actually provides supportive context for my dissenting results. They estimate that Arizona zip codes that were semi-randomly allocated a cannabis dispensary license via lottery in August 2012 saw a weakly significant increase in opioid-related emergency visits. Though the scope of the study is relatively smaller than my own in that it pertains to only Arizona, and is for an outcome outside of my dataset and design, the robustness of its design in that treatment was semi-randomly allocated[22] and the fact its scope pertains to one of the six states included in my sample does offer some peripheral legitimacy to this paper's results. That being said, mixed support from this adjacent literature for my results is overshadowed by the more comparable studies previously discussed.

[21] 24 states were excluded from the sample, notably Nevada and New Mexico, two states which are the focus of this paper's design.

[22] This was compensated for in the study via the usage of "propensity scores" to differently weight zip codes with more or less likelihood to be allocated a license.

## 9.4 Future Research

With a thorough consideration of the quirks of the theoretical model, the design, and how this paper's analysis stacks up against previous research, it is clear that this line of research would benefit from further investigation. Of the previous research literature discussed and compared to, an economic theory to describe how individuals would compare consumption of opioids and cannabis could not be found. This paper attempts to adapt such a theory, Becker and Murphy (1988)'s rational addiction model with the inclusion of medical cannabis as a channel to avoid opioid addiction, but the adaption suffers from a simplistic role for medical cannabis consumption along with other criticisms of the theory made by other previous literature. Future efforts to understand the role of cannabis in combating opioid addiction, especially when framed as an economic policy problem, would greatly benefit from a more robust underlying theory, whether that be a better developed extension to the rational addiction model, or another health and choice model entirely. From a more mechanical perspective the lack of validation for this paper's hypotheses, especially when compared and contrasted with mostly contradictory results from similar or adjacent studies, warrants changes to the sample design of the paper. This means that while this paper focuses on the six Southwestern states out of all fifty states and the District of Columbia, changing or expanding the sample to include other appropriate states may incorporate valuable variation and provide robust results to support - or further refute - this paper's findings. As this paper's design may have suffered from its relatively small sample size, expanding the sample seems to be a favorable adjustment at a glance, if states with similar unobserved variation are chosen. The inclusion of more states, however, brings up the need to have access to restricted-use NVSS data, which this paper did not acquire, in order to include states with observations of low counts of opioid overdose mortalities without risks of data suppression. This would both allow for the inclusion of states with low counts of opioid overdose mortalities during observed periods, and for research to drill down in to how specific demographics were affected, whereas this paper suffered from opioid overdose mortalities of demographic subsets often being suppressed from the data due to being too low in yearly observations. Deeper analysis of demographic subsets could also contribute to a better understanding of the heterogeneity of responses to increased cannabis access, which may have been lost with the aggregation of responses to the state-level. The inclusion of more states, especially when expanding the sample period to later than 2015, also means that further research will want to incorporate an examination of recreational cannabis laws and high-CBD cannabis laws, examining their unique effects on opioid outcomes. This is especially valuable as more states continue to legalize recreational cannabis as we enter 2021. Finally, further analyses in to prescription opioid distribution in general will benefit from access to an ARCOS database, of which public access is not known of as of this time, or the acquisition of another comparable data source, due to the previously mentioned risks of hand-compiling a dataset from the ARCOS public retail drug summary reports.

## 10 Conclusion

In this paper, I have attempted to contribute to the literature on positive policy solutions to reducing the damage done by the US Opioid Crisis. The US Opioid Crisis has been a persistent health and economic public burden on the United States since the late 1990s to the present, and has been shown to be quite dynamic as types of opioids causing harm and the impact of the Crisis has shifted over time. Previous research has pointed towards a potentially helpful effect of increasing cannabis access in that it may reduce the measures of harm associated with the US Opioid Crisis, including abuse and in extreme cases overdose involving the consumption of opioids. In similar vein to such research, this paper investigates a set of hypotheses positing that increased access to cannabis, measured by two different cannabis environment changes of legalization of medical cannabis and operation of legal dispensaries, reduces state measures of opioid overdose, abuse, and prescription opioid distribution. This paper attempts to explain any possible negative association between increased cannabis access and measures of opioid overdose, abuse, and prescription opioid distribution via the Becker and Murphy (1988) rational choice model of addiction, adjusted to include a role for cannabis consumption as a channel to avoid entering into the cycle of addictive consumption of opioids.

Using a staggered difference-in-difference design focused on the yearly opioid outcomes of six Southwestern states, the analyses conducted by this paper found little evidence that medical cannabis legislation or the operation of legal dispensaries impacted measures of opioid overdose, abuse, or prescription opioid distribution within these states, and thus failed to reject the hypotheses. Given previously mentioned concerns of the inappropriateness of the theoretical model, issues with how the sample and design may have impacted the estimates in a distortionary manner, limitations and problems with the usage of publicly-available data, and the spread of countervailing evidence against the conclusions of the paper, further research is advised to continue to ascertain the role of cannabis in combating the health and economic consequences of opioid addiction. While the results of this paper's analysis warrants further investigation given its contrast with other conclusions made by previous literature, further research will be especially beneficial as both the US Opioid Crisis and the nature of cannabis access laws continue to evolve over time, and thus the potential interactions between the two are subject to change as well.

## References

ADHS (2016). 2016 arizona opioid report. Arizona Department of Health Services. Available at https://www.azdhs.gov/documents/audiences/clinicians/clinical-guidelines-recommendations/prescribing-guidelines/arizona-opioid-report.pdf.

Alexander, M. J., Kiang, M. V., and Barbieri, M. (2018). Trends in black and white opioid mortality in the united states, 1979-2015. *Epidemiology*, 29(5), pp. 707-715.

Alpert, A., Powell, D., and Pacula, R. L. (2018). Supply-side drug policy in the presence of substitutes: Evidence from the introduction of abuse-deterrent opioids. *American Economic Journal*, 10(4), pp. 1-35.

Bachhuber, M. A., Saloner, B., Cunningham, C. O., and Barry, C. L. (2014). Medical cannabis laws and opioid analgesic overdose mortality in the united states, 1999-2010. *JAMA Internal Medicine*, 174(10), pp. 1668-1673.

Becker, G. and Murphy, K. (1988). A theory of rational addiction. *Journal of Political Economy*, 96(4), pp. 675-700.

Birnbaum, H. G., White, A. G., Schiller, M., Waldman, T., Cleveland, J. M., and Roland, C. L. (2011). Societal costs of prescription opioid abuse, dependence, and misuse in the united states. *Pain Medicine*, 12, pp. 657-667.

Bowles, D. W. (2012). Persons registered for medical marijuana in the united states. *Journal of Palliative Medicine*, 15(1). doi:10.1089/jpm.2011.0356.

Bradford, A. and Bradford, W. D. (2016). Medical marijuana laws reduce prescription medication use in medicare part d. *Health Affairs*, 35(7), pp. 1230-1236.

Bradford, A. and Bradford, W. D. (2017). Medical marijuana laws may be associated with a decline in the number of prescriptions for medicaid enrollees. *Health Affairs*, 36(5), pp. 945-951.

Buchemueller, T. C. and Carey, C. (2018). The effect of prescription drug monitoring programs on opioid utilization in medicare. *American Economic Journal: Economic Policy*, 10(1), pp. 77-112.

Campbell, C. I., Weisner, C., LeResche, L., Ray, T., Saunders, K., Sullivan, M. D., Banta-Green, C. J., Merrill, J. O., Silverberg, M. J., Boudreau, D., Satre, D. D., and Korff, M. V. (2010). Age and gender trends in long-term opioid analgesic use for noncancer pain. *American Journal of Public Health*, 100(12), pp. 2541-2547.

CDC (2020). Understanding the epidemic. Centers for Disease Control and Prevention. Retrieved September 10, 2020. Available at https://www.cdc.gov/drugoverdose/epidemic/index.html#resources.

Chan, N., Burkhardt, J., and Flyr, M. (2020). The effects of recreational marijuana legalization and dispensing on opioid mortality. *Economic Inquiry*, 58(2), pp. 589-606.

Chu, Y.-W. (2015). Do medical marijuana laws increase hard-drug use? *The Journal of Law & Economics*, 58(2), pp. 481-517.

Cicero, T. J., Inciardi, J. A., and Muñoz, A. (2005). Trends in abuse of oxycontin and other opioid analgesics in the united states:2002-2004. *Journal of Pain*, 6(10), pp. 662-672.

Commission, P. (2017). The president's commission on combating drug addiction and the opioid crisis. Available at https://www.whitehouse.gov/sites/whitehouse.gov/files/images/Final_Report_Draft_11-1-2017.pdf.

Conyers, G. and Ayres, I. (2020). A lottery test of the effect of dispensaries on emergency room visits in arizona. *Health Economics*, 29, pp. 854-864.

Dow, W., Godøy, A., Lowenstein, C., and Reich, M. (2019). Can economic policies reduce deaths of despair? *National Bureau of Economic Research*, Working Paper 25787.

Florence, C., Luo, F., Xu, L., and Zhou, C. (2016). The economic burden of prescription opioid overdose, abuse and dependence in the united states. *Med Care*, 54(10), pp. 901-906. doi:10.1097/MLR.0000000000000625.

Gipper, B., Lutz, C., and Maffett, M. G. (2019). Public oversight and reporting credibility: Evidence from the pcaob audit inspection. *European Corporate Governance Institute (ECGI)*, Finance Working Paper No. 453/2015.

Gitis, B. (2018). State-by-state: The labor force and economic effects of the opioid crisis. American Action Forum. Retrieved December 7th, 2020. Available at https://www.americanactionforum.org/project/opioid-state-summary/.

Glen, S. (2015). Variance inflation factor. Statistics How To. Retrieved December 14th, 2020. Available at https://www.statisticshowto.com/variance-inflation-factor/.

Gormley, T. A. and Matsa, D. A. (2011). Growing out of trouble? corporate responses to liability risk. *The Review of Financial Studies*, 24(8), pp. 2781-2821.

Grossman, M., Chaloupka, F. J., and Sirtalan, I. (1995). An empirical analysis of alcohol addiction: Results from the monitoring the future panels.

Hill, K. P. (2015). Medical marijuana for treatment of chronic pain and other medical and psychiatric problems: A clinical review. *JAMA*, 313(24), pp. 2474-2483. doi:10.1097/MLR.0000000000000625.

Jones, M. R., Viswanath, O., Peck, J., Kaye, A. D., Gill, J. S., and Simopoulos, T. T. (2018). A brief history of the opioid epidemic and strategies for pain medicine. *Pain and Therapy*, 7(1), pp. 13-21.

Kim, J., Santaella-Tenorio, J., Mauro, C., Wrobel, J., Cerdà, M., Keyes, K., Hasin, D., Martins, S., and Li, G. (2016). State medical marijuana laws and the prevalence of opioids detected among fatally injured drivers.

Mark, T. L., Levit, K. R., Buck, R. V.-W. J. A., and Coffey, R. M. (2011). Changes in us spending on mental health and substance abuse treatment, 1986-2005, and implications for policy. *Health Affairs*, 30(2), pp. 284-292.

McMichael, B., Horn, R. L. V., and Viscusi, W. K. (2019). The impact of cannabis access laws on opioid prescribing.

Meier, E., Lindgren, B. R., Anderson, A., Reisinger, S. A., Norton, K. J., Jensen, J., Strayer, L., Dick, L., Tang, M.-K., Chen, M., Carmella, S. G., Hecht, S. S., Murphy, S. E., Yang, J., Stepanov, I., O'Connor, R. J., Shields, P. G., and Hatsukami, D. K. (2019). A randomized clinical trial of snus examining the effect of complete versus partial cigarette substitution on smoking-related behaviors, and biomarkers of exposure. *Nicotine & Tobacco Research*, 22(4), pp. 473-481.

Moore, S. C. (2010). Substitution and complementarity in the face of alcohol-specific policy interventions. *Alcohol and Alcoholism*, 45(5), pp. 403-408.

MPP (2020). State policy. Marijuana Policy Project. Retrieved October 2nd, 2020. Available at https://www.mpp.org/states/.

Nunberg, H., Kilmer, B., Pacula, R. L., and Burgdorf, J. R. (2011). An analysis of applicants presenting to a medical marijuana specialty practice in california. *Journal of Drug Policy Analysis*, 4(1), Article 1. doi:10.2202/1941-2851.1017.

Ogden, D. W. (2009). Investigations and prosecutions in states authorizing the medical use of marijuana. Available at https://www.justice.gov/archives/opa/blog/memorandum-selected-united-state-attorneys-investigations-and-prosecutions-states.

OotNMAG (2020). Fighting the opioid crisis in new mexico. Office of the New Mexico Attorney General. Retrieved August 27, 2020. Available at https://www.nmag.gov/fighting-the-opioid-crisis-in-new-mexico.aspx.

Orphanides, A. and Zervos, D. (1999). Rational addiction with learning and regret. *Journal of Political Economy*, 103(4), pp. 739-758.

Pacula, R. L., Powell, D., Heaton, P., and Sevigny, E. L. (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), pp. 7-31.

PDAPS (2020). Pdmp reporting and authorized use. Prescription Drug Abuse Policy System. Retrieved October 1st, 2020. Available at http://pdaps.org/datasets/prescription-monitoring-program-laws-1408223416-1502818373.

Powell, D., Pacula, R. L., and Jacobsen, M. (2015). Do medical marijuana laws reduce addictions and deaths related to pain killers? *National Bureau of Economic Research*, Working Paper 21345.

Powell, D., Pacula, R. L., and Jacobsen, M. (2018). Do medical marijuana laws reduce addictions and deaths related to pain killers? *Journal of Health Economics*, 58, pp. 29-42.

Reed, J. K. (2016). Marijuana legalization in colorado: Early findings. Available at https://cdpsdocs.state.co.us/ors/docs/reports/2016-SB13-283-Rpt.pdf.

Ruhm, C. (2018). Deaths of despair or drug problems? doi:10.3386/w24188.

Sadler, J. (2019). Opioid epidemic touched thousands of nevadans, costing millions of dollars, state lawsuit alleges. Las Vegas Sun. Retrieved December 7th, 2020. Available at https://lasvegassun.com/news/2019/jun/20/opioid-epidemic-touched-thousands-of-nevadans-cost/.

Shi, Y. (2017). Medical marijuana policies and hospitalizations related to marijuana and opioid pain reliever. *Drug and Alcohol Dependence*, 173, pp. 144-150.

Skog, O.-J. (1999). Rationality, irrationality and addiction: notes on becker and murphy's theory of addiction. In *Getting Hooked: Rationality and Addiction* (ed. by J. Elster & O.-J. Skog), pp. 173-207.

Subbaraman, M. S. (2016). Substitution and complementarity of alcohol and cannabis: A review of the literature. *Substance Use & Misuse*, 51(11), pp. 1399-1414.

TPC (2020). State alcohol excise taxes. Tax Policy Center. Retrieved October 1st, 2020. Available at https://www.taxpolicycenter.org/statistics/state-alcohol-excise-taxes.

Vale, P. H. (2010). Addiction – and rational choice theory. *International Journal of Consumer Studies*, 34, pp. 38-45.

VIPP (2020). Opioid overdoses. Violence & Injury Prevention Program. Retrieved December 7th, 2020. Available at https://health.utah.gov/vipp/topics/prescription-drug-overdoses/.

Wiese, B. and Wilson-Poe, A. R. (2018). Emerging evidence for cannabis' role in opioid use disorder. *Cannabis and Cannabinoid*, 3(1), pp. 179-189.

## Appendix A

Table A1: List of Drugs Classified as Prescription Opioids

| DEA Code | Commercial Name | MME Conversion Factor |
|---|---|---|
| 9050 | Codeine | 0.15 |
| 9120 | Dihydrocodeine | 0.25 |
| 9143 | Oxycodone | 1 |
| 9150 | Hydromorphone | 4 |
| 9193 | Hydrocodone | 1 |
| 9230 | Meperidine | 0.1 |
| 9250B | Methadone† | 3 |
| 9300 | Morphine | 1 |
| 9600, 9639* | Opium | 1 |
| 9652 | Oxymorphone | 3 |
| 9780 | Tapentadol | 0.4 |
| 9801 | Fentanyl | 0.13 |

*Sources:* https://www.cms.gov/Medicare/Prescription-Drug-Coverage/PrescriptionDrugCovContra/Downloads/Opioid-Morphine-EQ-Conversion-Factors-March-2015.pdf for MME conversions.

*: Multiple classifications of differing opioid products, DEA code 9639 refers to powdered opium.

†: Methadone is not included in the analysis of prescription opioid distribution. It is a prescription opioid used for treating opioid addiction, and while it has been proven to be abused like other prescription opioids and is included in analysis of opioid overdose and abuse, the legal usage of the opioid could indicate reverse causality where higher flows of methadone distribution are a response to increased opioid abuse.

Table A2: Descriptive Statistics - Other State Characteristics

| | | Medical Cannabis States | | | | | Illegal Cannabis States | | | Sample |
|---|---|---|---|---|---|---|---|---|---|---|
| | | AZ | CO | NV | NM | Avg | OK | UT | Avg | Avg |
| | Population | 5.7 | 4.6 | 2.3 | 1.9 | 3.6 | 3.6 | 2.4 | 3.0 | 3.4 |
| 1999- | Black | 3.58% | 4.12% | 7.70% | 1.97% | 4.34% | 8.00% | 1.02% | 4.51% | 4.40% |
| 2009 | Hispanic | 27.24% | 18.76% | 22.74% | 43.86% | 28.15% | 6.73% | 10.68% | 8.70% | 21.67% |
| | Non-Rural | 94.22% | 85.83% | 89.45% | 64.79% | 83.57% | 63.41% | 88.99% | 76.20% | 81.12% |
| | Population | 6.6 | 5.2 | 2.8 | 2.1 | 4.2 | 3.8 | 2.9 | 3.4 | 3.9 |
| 2010- | Black | 4.43% | 4.41% | 8.85% | 2.15% | 4.96% | 8.30% | 1.29% | 4.79% | 4.91% |
| 2015 | Hispanic | 30.24% | 21.02% | 27.39% | 47.17% | 31.45% | 9.49% | 13.35% | 11.42% | 24.78% |
| | Non-Rural | 94.75% | 86.84% | 90.31% | 66.63% | 84.63% | 64.91% | 89.22% | 77.06% | 82.11% |
| | Population | 6.0 | 4.8 | 2.5 | 2.0 | 3.8 | 3.7 | 2.6 | 3.1 | 3.6 |
| 1999- | Black | 3.88% | 4.22% | 8.11% | 2.03% | 4.56% | 8.10% | 1.12% | 4.61% | 4.58% |
| 2015 | Hispanic | 28.30% | 19.56% | 24.38% | 45.03% | 29.32% | 7.70% | 11.63% | 9.66% | 22.77% |
| | Non-Rural | 94.41% | 86.18% | 89.76% | 65.44% | 83.95% | 63.94% | 89.07% | 76.51% | 81.47% |

*Sources:* US Census Bureau intercensal series, 1999 and 2001-2009; US Census, 2000 and 2010; Vintage postcensal series, 2011-2015.

Notes: "AZ" = Arizona; "CO" = Colorado; "NV" = Nevada; "OK" = Oklahoma; "UT" = Utah; "Avg" = Average of characteristics within a respective sample or subsample and time period; "Population" = Population of state/group in millions; "Black", "Hispanic", and "Non-Rural" = Population shares of non-Hispanic blacks, Hispanics, and individuals living in metropolitan areas according to the 2013 urbanization, respectively.

Table A3: State Characteristics - Group Mean Differences

| | | MCL States | No-MCL States | Mean | 2-Sample | Equal | F-test |
|---|---|---|---|---|---|---|---|
| | | Mean | Mean | Difference | t-score | Variance? | p-value |
| 1999-2009 | Male | 50.02% | 49.75% | -0.28% | -2.05** | Yes | .2773 |
| | White | 60.34% | 79.40% | 19.06% | 9.50*** | No | .0005*** |
| | 15-34 | 28.22% | 30.88% | 2.66% | 4.09*** | No | .0000*** |
| | 35-64 | 38.71% | 34.54% | -4.17% | -5.29*** | No | .0000*** |
| | 65+ | 11.62% | 10.93% | -0.69% | -1.29 | No | .0002*** |
| | Unemployed | 5.16% | 4.37% | -0.79% | -2.08** | Yes | .1725 |
| | Beer Tax | 0.20 | 0.27 | 0.07 | 1.60 | No | .0471** |
| | Population | 3.6 | 3.0 | -0.6 | -2.36** | No | .0000*** |
| | Black | 4.34% | 4.51% | 0.17% | 0.20 | No | .0043*** |
| | Hispanic | 28.15% | 8.70% | -19.45% | -12.43*** | No | .0000*** |
| | Non-Rural | 83.57% | 76.20% | -7.37% | -2.36** | Yes | .4319 |
| | | n=44 | n=22 | | | | |
| 2010-2015 | Male | 49.95% | 49.90% | -0.05% | -0.36 | Yes | .7456 |
| | White | 55.63% | 75.73% | 20.10% | 7.34*** | No | .0173** |
| | 15-34 | 27.62% | 29.71% | 2.09% | 3.57*** | No | .0000*** |
| | 35-64 | 38.51% | 34.97% | -3.54% | -4.44*** | No | .0031*** |
| | 65+ | 13.72% | 11.87% | -1.85% | -2.85*** | Yes | .0580* |
| | Unemployed | 8.10% | 5.33% | -2.76% | -3.93*** | Yes | .0541* |
| | Beer Tax | 0.20 | 0.20 | <0.00 | -0.04 | No | .0420** |
| | Population | 4.2 | 3.4 | -0.8 | -2.01* | No | .0001*** |
| | Black | 4.96% | 4.79% | -0.17% | -0.16 | Yes | .1148 |
| | Hispanic | 31.45% | 11.42% | -20.03% | -9.53*** | No | .0000*** |
| | Non-Rural | 84.63% | 77.06% | -7.57% | -1.85* | Yes | .5380 |
| | | n=24 | n=12 | | | | |
| 1999-2015 | Male | 50.00% | 49.80% | -0.20% | -1.98* | Yes | .3635 |
| | White | 58.68% | 78.11% | 19.43% | 11.75*** | No | .0000*** |
| | 15-34 | 28.00% | 30.47% | 2.46% | 5.18*** | No | .0000*** |
| | 35-64 | 38.64% | 34.69% | -3.95% | -6.84*** | No | .0000*** |
| | 65+ | 12.36% | 11.26% | -1.10% | -2.42** | No | .0128** |
| | Unemployed | 6.20% | 4.71% | -1.49% | -4.17*** | No | .0005*** |
| | Beer Tax | 0.20 | 0.24 | 0.04 | 1.23 | No | .0041*** |
| | Population | 3.8 | 3.1 | -0.7 | -3.07*** | No | .0000*** |
| | Black | 4.56% | 4.61% | 0.05% | 0.07 | No | .0019*** |
| | Hispanic | 29.32% | 9.66% | -19.66% | -15.36*** | No | .0000*** |
| | Non-Rural | 83.95% | 76.51% | -7.44% | -3.02*** | Yes | .3569 |
| | | n=68 | n=34 | | | | |

*Sources:* US Census Bureau intercensal series, 1999 and 2001-2009; US Census, 2000 and 2010; Vintage postcensal series, 2011-2015; Local Area Unemployment Statistics (LAUS), Employment Status of the Civilian Noninstitutional Population 1976-2019; Tax Policy Center State Alcohol Excise Taxes, 1982-2017.

Notes: "MCL States" = The states of Arizona, Colorado, Nevada, and New Mexico, whom legalized medical cannabis by the end of 2015; "No-MCL States" = States of Oklahoma and Utah, whom do not legalize medical cannabis by the end of 2015; "2-Sample t-score" = t value of a characteristic's two-sample t test; "Equal Variance?" = If the associated two-sample t test conditions for equal variances between sample groups, "No" if an associated F-test rejects the hypothesis of equal variances at the 5% or 1% significance level; "n" = Observations; "Male", "White", "15-34", "35-64", and "65+" = Population shares of males, non-Hispanic whites, individuals of age 15 to 34, individuals of age 35 to 64, and individuals of age 65 and older, respectively; "Unemployed" = Unemployment rate; "Beer Tax" = Beer excise tax rate; "Population" = Population of state/group in millions; "Black", "Hispanic", and "Non-Rural" = Population shares of non-Hispanic blacks, Hispanics, and individuals living in metropolitan areas according to the 2013 urbanization, respectively. Symbols *, **, and *** present next to a t-score or p-value indicate significance at the 10%, 5%, and 1% level, respectively.