The Impact of California's Proposition 47 on Drug Abuse Outcomes

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Abstract

I analyze the impact of California's Proposition 47, which reduced most drug possession charges from felonies to misdemeanors in November 2014, on the drug overdose death rate, the rate of illegal drug use other than marijuana, the rate of unmet drug treatment need, and the incarceration rate using the synthetic control method. I also develop a theoretical model demonstrating the moral hazard argument against drug decriminalization. Since I am using state-year level data, my statistical power is limited, and I am unable to demonstrate that the results are statistically significant. The predicted reduction in the overdose death rate is significant in magnitude (suggesting a 14% reduction in drug overdose deaths from 2015 to 2019), and I demonstrate this result is robust to choice of predictors and omission of control units. I also run difference-in-differences regressions as another robustness test, some of which reach the 5% threshold for statistical significance and are negative. The estimated causal impact of Proposition 47 on the rate of illegal drug use was marginally negative, and the estimated causal impacts on the other outcomes were negligible. This suggests drug decriminalization may reduce overdose deaths without substantially increasing drug use.

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1. Introduction

The opioid crisis is one of the biggest problems the United States is facing today. From 2000 to 2014, the drug overdose death rate increased 137%, with a total of 47,055 drug overdose deaths in 2014 alone (Rudd et al. 2016). Drug overdose deaths related to opioids increased 200% in the same time frame, indicating that opioids are a primary driver of the increase in overdose deaths. Policymakers are split over how to approach this issue. Some policymakers believe in a tough-on-crime approach, which focuses on reducing the rate of drug use itself (Donohue III et al. 2011) through criminalization of drugs and the harsh punishment of drug offenders, which they hope will deter others from drug use. Other policymakers believe in a harm-reduction approach, which focuses on reducing negative externalities associated with drug use (primarily overdose deaths) rather than reducing drug use itself ("Harm Reduction" Substance Abuse and Mental Health Services Administration). The logic behind harm reduction is that the tough-on-crime approach fails to help addicts, and many people have continued to use drugs regardless of their legality. Many believe that harm reduction can reduce the overdose death rate because funds used to imprison drug offenders can be redirected to drug treatment and rehabilitation services, addicts will be less afraid to seek help, and punitive measures which act as obstacles to recovering addicts (like criminal records) will no longer pose barriers.

Many people view drug decriminalization as the next step of harm reduction. Decriminalization encompasses any measure that reduces legal punishment for drug offenders or focuses on rehabilitating drug offenders, however, people commonly use "decriminalization" as a euphemism for legalization, in which there are no punishments for using or possessing drugs that are currently illegal. However, many worry that harm reduction will result in more drug use. The argument against drug legalization/decriminalization is essentially a theory of moral hazard.

Moral hazard occurs when two parties enter into an agreement that motivates one party to behave more recklessly because the agreement prevents them from bearing the full cost of their actions, and these costs are then borne by other parties (either the other party who entered into an agreement and/or people outside of the contract) (Marshall 1976). Moral hazard usually comes up in insurance contexts, to describe how insurance motivates the insured to behave more recklessly than they would otherwise, because many costs of this recklessness will be borne by the insurer (reducing cost to the insured). Similarly, people worry that decriminalization will do more harm than good because it would supposedly decrease the perceived cost of drug use, thus increasing the rate of drug use (which could, thereby, increase the rate of overdose deaths).

One drug decriminalization policy is California's Proposition 47, a referendum approved by Californian voters on November 4, 2014, that took effect on the following day (Crodelle et al. 2021). It reduced drug possession charges of many illegal drugs for the purpose of use and theft of various forms below $950 in value from felonies to misdemeanors and allowed individuals already sentenced for these crimes to petition to be resentenced, with a few exceptions (some incarcerated on more serious charges were ineligible for resentencing). California's Proposition 47 was intended to reduce overcrowding in state prisons and money spent incarcerating non-violent offenders. Some of the projected savings were set aside in a fund for various causes, including increased funding for drug treatment facilities. While there has been limited research on the effects of California's Proposition 47, current research focuses on its effect on reported crime rates themselves, rather than drug abuse outcomes. Current research suggests that there was an increase in reported crime rates of crimes changed to misdemeanors under Proposition 47, but a decrease in the rates of other crimes (Bird et al. 2018; Crodelle et al. 2021). I was unable to find any study examining Proposition 47's impact on drug abuse outcomes.

There is some historical support for harm-reduction techniques, including drug decriminalization. One study found that the Netherlands' decision to allow opioid agonist therapy (allowing opioid addicts to receive methadone, an opioid, to wane them off more dangerous opioids) and needle exchange programs in the 1970s and 1980s reduced the risk of getting HIV, Hepatitis B, and Hepatitis C substantially (van Santen et al. 2021). Another study found that Portugal's 2001 drug decriminalization reduced drug overdose deaths by almost 50% from 2002 to 2005 (Greenwald 2009). Another study estimated that Good Samaritan Laws (allowing people who used drugs to call for help without fear of legal punishment) and Naloxone (an overdose-reversing medication) Access Laws each reduced drug overdose death rates by around 10% (Rees et al. 2019).

In this study, I examine the impact of California's Proposition 47 on four outcomes of interest: the overdose death rate, the rate of illegal drug use other than marijuana, the rate of unmet drug treatment need (operationally defined in the data section), and the incarceration rate. Using data from the Center for Disease Control, the Substance Abuse and Mental Health Services Administration, the Bureau of Justice Statistics, the United States Census Bureau, and other sources, I estimate the treatment effect of California's Proposition 47 on my outcomes of interest with the Synthetic Control Method, and validate my findings by demonstrating robustness to omission of control units and covariates (with alternative specifications of the synthetic control method) and running difference-in-differences regressions as an additional robustness check. I also develop a theoretical model demonstrating the economic theory underlying the moral hazard argument against drug decriminalization.

I find substantial evidence that California's Proposition 47 did not result in a large increase the rate of illegal drug use other than marijuana or the overdose death rate, suggesting moral hazard is not a major policy concern for drug decriminalization. My models suggest Proposition 47 has a negative effect of substantial magnitude on the overdose death rate and a marginal (insignificant in magnitude) or null effect on the rate of illegal drug use other than marijuana. My models find no evidence of a substantial treatment effect on the rate of unmet drug treatment need and the incarceration rate. The robustness checks support my main results.

2. Theory

Suppose someone is contemplating whether to use drugs. Let D represent the event that they decide to use drugs, N represent the event that they decide not to use drugs, π represent the payoff function, and $E[\pi(X)]$ represent the expected payoff of event X.

In theory, someone will decide to use drugs if $E[\pi(D)] > E[\pi(N)]$. Since these utilities are arbitrary, let $E[\pi(N)] = 0$. Let O represent the event that a drug overdose occurs, I represent the event that one is incarcerated, and H represent the "high" one receives from drug use. Assume other potential consequences have negligible effects on drug use. In this case,

$$E[\pi(D)] = \pi(H) + \pi(O)P(O) + \pi(I)P(I),$$

$$E[\pi(D)] > E[\pi(N)] \Rightarrow$$

$$\pi(H) + \pi(O)P(O) + \pi(I)P(I) > 0 \Rightarrow$$

$$\pi(H) > -[\pi(O)P(O) + \pi(I)P(I)]$$

If drugs are decriminalized, then there is no possibility of imprisonment associated with drug use ($P(I) = 0$), then the equation is reduced to:

$$\pi(H) > -\pi(O)P(O).$$

Let's assume that the utilities of experiencing incarceration, overdose, and the "high" are unaffected by the legal status of drugs. Assume overdose and incarceration are undesired outcomes, and thus, $\pi(O), \pi(I) \in (-\infty, 0)$. Assume the "high" itself is a desired outcome, and thus, $\pi(H) \in (0, \infty)$. Assume that, when drugs are not decriminalized, there is a non-zero probability of being incarcerated for drug use, and thus, $P(I) \in (0,1)$ when not decriminalized. Assume that, when drugs are decriminalized, $P(I) = 0$. Let $legal$ denote the relevant parameter when drug use is decriminalized and let $illegal$ denote the relevant parameter when drug use is illegal.

The minimum threshold value of the high (for a consumer to decide to use the drug) when the drug is illegal is:

$$\pi_{threshold-illegal}(H) = -\left[\pi(O)p_{illegal-overdose} + \pi(I)p_{illegal-incarc}\right].$$

The minimum threshold value of the high (for a consumer to decide to use the drug) when the drug is legal is:

$$\pi_{threshold-legal}(H) = -\pi(O)p_{legal-overdose}.$$

Since there are no additional consequences to seeking help for an overdose when the drug is legal, one should assume $p_{legal-overdose} \leq p_{illegal-overdose}$. The ratio of the illegal "high" threshold to the legal "high" threshold is

$$\frac{\pi_{threshold-illegal}(H)}{\pi_{threshold-legal}(H)} =$$

$$\frac{-\left[\pi(O)p_{illegal-overdose} + \pi(I)p_{illegal-incarc}\right]}{-\pi(O)p_{legal-overdose}} =$$

$$\frac{\pi(O)p_{illegal-overdose} + \pi(I)p_{illegal-incarc}}{\pi(O)p_{legal-overdose}} =$$

$$\frac{\pi(O)p_{illegal-overdose}}{\pi(O)p_{legal-overdose}} + \frac{\pi(I)p_{illegal-incarc}}{\pi(O)p_{legal-overdose}} =$$

$$\frac{p_{illegal-overdose}}{p_{legal-overdose}} + \frac{\pi(I)p_{illegal-incarc}}{\pi(O)p_{legal-overdose}}.$$

Under my prior assumptions, $\frac{p_{illegal-overdose}}{p_{legal-overdose}} \geq 1$ and $\frac{\pi(I)p_{illegal-incarc}}{\pi(O)p_{legal-overdose}} \in (0, \infty)$, implying $\frac{p_{illegal-overdose}}{p_{legal-overdose}} + \frac{\pi(I)p_{illegal-incarc}}{\pi(O)p_{legal-overdose}} > 1$. Qualitatively, this means decriminalizing a drug should increase drug use, and the magnitude of this increase will depend on the magnitude of the decrease in the probability of a drug overdose associated with drug decriminalization, and incarceration's strength as a deterrent to drug use relative to the chance of an overdose death.

Assume, for the moment, that the probability of overdosing on drugs (and the "high" given by a drug) is not affected by the legal status of drugs. The model in the previous section suggests that the threshold for illegal drug use is:

$$\pi_{threshold}(H) = -\left[\pi(O)p_{illegal-overdose} + \pi(I)p_{illegal-incarc}\right] \Longrightarrow$$

$$\pi_{threshold}(H) = -\pi(O)p_{illegal-overdose} - \pi(I)p_{illegal-incarc}.$$

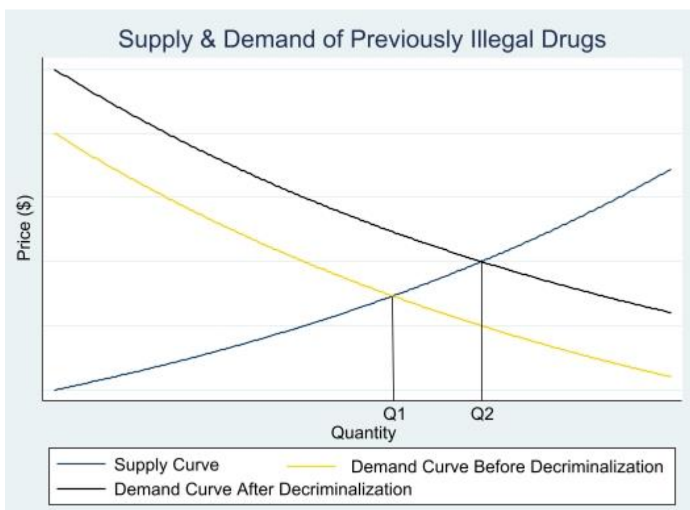
Observe

$$\frac{\partial \pi_{threshold}(H)}{\partial p_{illegal-incarc}} = -\pi(I) > 0.$$

This suggests that increasing the probability of illegal incarceration to 1, conditional on drug use, is associated with an increase in the threshold high required to incentivize drug use by a factor of the degree to which incarceration is an effective deterrent of drug use. Conversely, this suggests that decreasing the probability of illegal incarceration to 0, conditional on drug use, is associated with a decrease in the threshold high required to incentivize drug use by a factor of the degree to which jail is an effective deterrent for drug use.

Assume the effects of drug decriminalization on the supply curve associated with the drugs being decriminalized is negligible. Also assume that the market for these drugs is a case of perfect competition. The decrease in risk associated with drug decriminalization can be modeled as an exogenous shock to the demand curve, shifting the demand curve upwards in the supply-demand graph. Since the implicit cost of being incarcerated disappears when drugs are decriminalized, more (or at least the same number of) potential drug users are willing to purchase the drug at any given monetary price. This exogenous shock to demand is shown in Figure 1.

Figure 1: Demand Shift in Drugs Due to Decriminalization



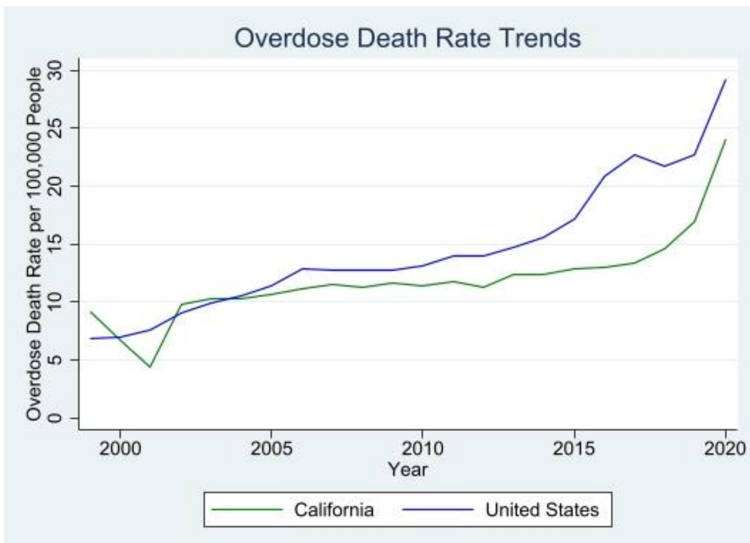
Graph created through Stata by me.

Q_1 (represented as “Q1” in the above figure) represents the equilibrium amount of drugs sold before the relevant drugs are decriminalized, while Q_2 (represented as “Q2” in the above figure) represents the equilibrium amount of drugs sold after the relevant drugs are decriminalized. In my theoretical model, the price and quantity of drugs sold at equilibrium increase when drugs are decriminalized. Though this suggests drug use will increase because of drug decriminalization, this does not necessarily imply that the overdose death rate will increase, as there are a variety of channels through which the overdose death rate may decrease with decriminalization. If drug use is not very responsive to the perceived cost of drug use, but the chance of a drug overdose death occurring conditional on drug use is sufficiently responsive to decriminalization, then the impact of a decrease in the probability of a drug overdose death conditional on drug use on the overdose death rate could outweigh the impact of an increase in drug use on the overdose death rate. Formally, if $0 < \frac{\partial u}{\partial c} < x$, and $\frac{\partial p_{overdose}}{\partial D} < 0$, then $\frac{\partial o}{\partial d} < 0$, where u represents the proportion of people deciding to use drugs, c represents the perceived cost of drug use, x represents the threshold past which the increase in drug use associated with decriminalization results in an increase in the overdose death rate, $p_{overdose}$ represents the probability of a drug overdose death occurring when using drugs, o represents the number of overdose deaths, and D represents the event that drugs are decriminalized.

3. Data

I use data from a variety of sources, most of them affiliated with a government institution. I obtained overdose death rates from the Center for Disease Control's online WONDER system (Center for Disease Control 2021), which contains data from many public health datasets. The drug overdose death rates are originally from the CDC's National Vital Statistics Systems, which contains mortality data for many different causes of death by race, gender, age, location, and other factors. I obtained data at the state-year level, which gave me the overall drug overdose death rate per 100,000 people in a given state and year. This overdose death rate is calculated based on the number of death certificates indicating "drug-induced causes" (Center for Disease Control 2021) as the single, underlying cause of death. Every death certificate can only indicate one underlying cause of death, even if the cause of death was multifaceted. Overdose death rates are available by state and year going as far back as 1999, but my analysis only uses data from 2005 onward. Overdose death rates in California and the nation have been steadily increasing since 1999. 2019 is the last year I analyze of the post-treatment period due to the potential effects of COVID-19 on my outcomes of interest.

Figure 2: Overdose Trends

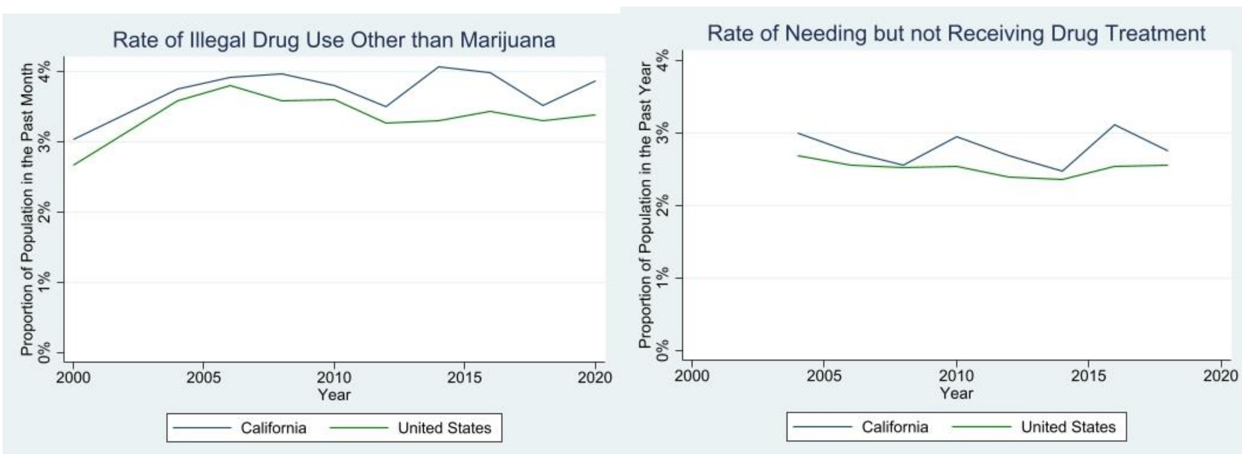


Source: Center for Disease Control (2021)

Two more outcomes I analyze, the rate of illegal drug use other than marijuana and the rate of unmet drug treatment need come from the National Survey on Drug Use and Health Small Area Estimation Dataset (Substance Use and Mental Health Services Administration 2021), which provides pooled estimates (by pair of years) of average rates of illegal drug use other than marijuana, unmet drug treatment need, and many other variables of interest at the state-year level, based on survey data. This data is from the Substance Abuse and Mental Health Services Administration, part of the United States Department of Health and Human Services. The data are pooled such that there is one estimate for the average value of each variable from 1999 to 2000, one estimate for average value from 2000 to 2001, et cetera. For the purposes of analyzing this pooled data alongside my state-year level data, I use a somewhat lagged variable for rate of illegal drug use other than marijuana and rate of unmet drug treatment need, with the rate of illegal drug use in 2007 (in my state-year level dataset) being the estimated average over 2006 and 2007, and every other pair of years is used to avoid having any year's worth of data being used twice. Meaning, in my analysis, I use the estimates of illegal drug use other than marijuana and average rate of unmet drug treatment need over 2005 and 2006, 2007 and 2008 (not 2006 and 2007), 2009 and 2010, 2010 and 2011, 2012 and 2013, etc. To reiterate, every year's average estimate is used, but every other pair of years is omitted such that each year's average estimate is used only once.

The rate of illegal drug use other than marijuana in state s and year t is equivalent to the proportion of people who used an illegal drug other than marijuana in the past month (which, for the National Survey on Drug Use and Mental Health Small Area Estimation dataset, is defined as the past 30 days). For each illegal drug, respondents were asked "How long has it been since you last used [drug]?" If the answer for any illegal drug other than marijuana was within 30 days, then the respondent is marked as having used illegal drugs in the past month. People who refused to answer or did not answer were marked as not having used illegal drugs in the past month. The proportion who used illegal drugs other than marijuana over the past year was not included in the dataset. To measure the rate of unmet drug treatment need, respondents were asked whether they were dependent on illegal drugs in the past year, had abused illegal drugs in the past year, and had been in a treatment facility for illegal drugs in the past year. If they answered in the affirmative to any of those three questions, they were marked as having needed drug treatment. If they answered in the negative to being in a drug facility for illegal drugs in the past year, but in the affirmative to either abusing or being dependent on an illegal drug in the past year, they were marked as having unmet drug treatment need. If they did not answer or refused to answer, they were marked as not having an unmet drug treatment need. Data are available going back as far as 1999, but only data from 2005 onwards are considered. The rates of illegal drug use other than marijuana and unmet drug treatment need appear to be slightly higher in California than the country average (shown in the figure below), despite California appearing to have a slightly lower drug overdose death rate than the country average.

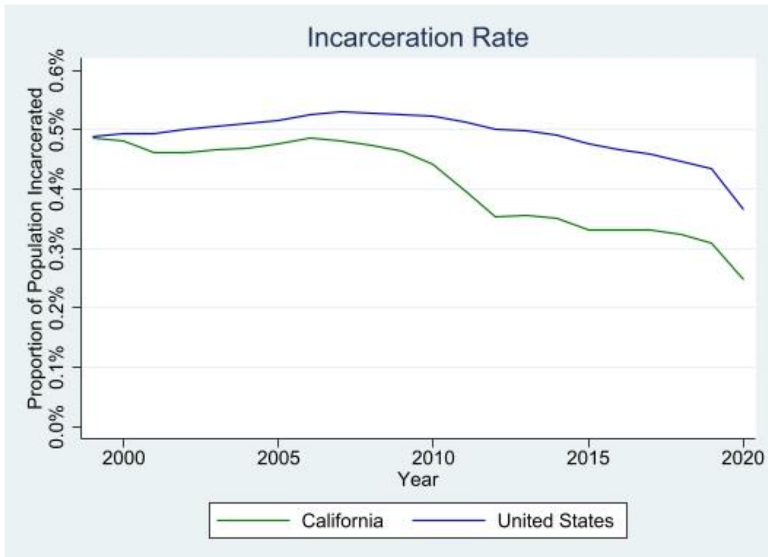
Figures 3 and 4: Trends in Drug Use and Unmet Drug Treatment Need



Source: National Survey on Drug Use and Health (Substance Abuse and Mental Health Services Administration 2021)

The incarceration rate is from the National Prisoner Statistics Program (Bureau of Justice Statistics 2021). This is a state-year level dataset, containing one observation for each state and year. The data is survey data, with one central respondent in each state's department of corrections (for the District of Columbia after 2000, the Federal Bureau of Prisons) receiving the survey, answering the questions, and mailing it back. Central respondents were asked how many men and women were incarcerated (in both private prisons and public prisons) in their state at the time of the survey. The incarceration rate was calculated by adding the total number of men and women that were incarcerated in a state at the time of the survey and dividing by the state-year population estimates given by the United States Census Bureau (detailed later in this section). The incarceration rate has declined since 2009 in both the United States and California, but has declined quicker in California, as shown in the figure below.

Figure 5: Incarceration Trends



Source: National Prisoner Statistics (Bureau of Justice Statistics 2021) and author's own calculations

The controls I include in my analysis came from a variety of sources. The population of each state (consisting of population totals, racial breakdowns, and age breakdowns) by year is from the United States Census Bureau's Population Estimates Program (PEP), obtained through Sage Data's Population Estimates - Detail (1990 - Current) Database (United States Census Bureau 2022b). The area for each state (in square miles) comes from the Census Bureau's 2010 State Area Measurements and Internal Point Coordinates (United States Census Bureau 2010). The population density was obtained by dividing the population by the area, to give the population density in units of people per square mile. The percent of the population that is Black was obtained by dividing the number of Black residents in a state, according to the United States Census Bureau's Population Estimates Program (United States Census Bureau 2022b), by the total number of residents in a state each year, also obtained from Sage Data's Population Estimates - Detail (1990 - Current) Database. People of multiple races were marked as belonging to “two or more races”, and not grouped together with any single race for the purposes of estimating the number of Black residents. The percentage of the population between the ages of 15 to 29 was obtained through the Population Estimates Program (United States Census Bureau 2022b), also obtained from Sage Data’s Population Estimates - Detail (1990 - Current) Database, by summing the total amount of people aged 15 to 19, 20 to 24, and 25 to 29, then dividing this sum by the total population in each state by year. The poverty rate in a given state and year is calculated by the United States Census Bureau, through the Small Area Income and Poverty Estimates program (United States Census Bureau 2022c), and I obtained it through Sage Data’s Poverty Database. Estimates for the educational attainment variable (measured as the proportion of residents at least 18 years of age with a bachelor’s degree) from 2006 to 2020 are from the United States Census Bureau’s American Community Survey (ACS) (United States Census Bureau 2022a), and I obtained them through the Federal Reserve Bank of St. Louis’s FRED (Federal Reserve Economic Data) system. The 2005 estimates for the educational attainment variable (measured the same way) were obtained from a table of 2005 educational attainment estimates published on the National Center for Education’s website, indicated to be from the United States Census Bureau (United States Census Bureau 2006).

Health expenditures per capita were estimated at the state and year level by the Centers for Medicare and Medicaid Services, estimated as the amount of money (in dollars) spent on personal health expenses in the state divided by the population of the state (Centers for Medicare and Medicaid Services 2022), obtained through Sage Data’s Health Expenditures by State of Residence Database. A binary variable indicating whether a state has a good Samaritan law (GSL) in effect in a given state during a given year (Prescription Drug Abuse Policy System (PDAPS) 2021), and a binary variable indicating whether a state has a naloxone access law

(NAL) in effect in a given state and year were obtained from the Prescription Drug Abuse Policy System (Prescription Drug Abuse Policy System (PDAPS) 2022), with a value of 0 if the policy was in effect for less than half of the year and 1 if the policy was in effect for at least half of the year.

The police per capita variable is estimated by dividing the number of police employed at the state level in a given state and year by the population. The number of police employed at the state level is given by the United States Census Bureau's Annual Survey of Public Employment & Payroll (United States Census Bureau 2021) and is estimated as the number of full-time officers involved in police protection.

4. Methodology

To estimate the causal impact of California's Proposition 47, I use the Synthetic Control Method (Abadie and Gardeazabal 2003). The Synthetic Control Method is an alternative to the commonly used difference-in-differences method designed for use when there is not a single control unit that neatly meets the parallel trends assumption necessary for inference, or when there are many control units but only one unit that undergoes the treatment. It creates a "synthetic" control unit of the treatment unit, as a weighted average of different control units from the "donor pool" (the set of all potential control units), representing the counterfactual values of the outcome of interest one would expect to see in the treatment unit if the treatment had never occurred. Covariates (averaged over all pre-treatment periods) and pre-intervention outcome values are selected as predictors of the post-intervention outcome of interest for the purposes of creating the synthetic control unit, with the goal of matching their estimated values in the synthetic control unit as close as possible to the treatment unit before the treatment occurs.

The synthetic control estimator of the treatment effect of Proposition 47 on a given outcome of interest, Y_{1t}, in a given year is

$$\delta_{outcome} = Y_{1t} - \sum_{s=2}^{s_c+1} w_s^* Y_{st},$$

where w_s^* is the optimized weight chosen for state s (with state 1 being the treatment state), s_c represents the number of states in the donor pool (states from the donor pool not used in the synthetic control are considered to have a weight of 0), t represents the year, and $\sum_{s=2}^{s_c+1} w_s^* Y_{st}$ represents the counterfactual value of the outcome of interest one would expect to see had the treatment never occurred (the value of the outcome of interest in the synthetic control unit). Following standard synthetic control practices, all weights should be non-negative and sum to one.

The state weights are selected to minimize the discrepancy in values of the predictors between the synthetic control unit and the treatment unit, $\|\boldsymbol{X_1} - \boldsymbol{X_0}\boldsymbol{W}\|_{\boldsymbol{V}} = \sqrt{(\boldsymbol{X_1} - \boldsymbol{X_0}\boldsymbol{W})'\boldsymbol{V}(\boldsymbol{X_1} - \boldsymbol{X_0}\boldsymbol{W})}$, where $\boldsymbol{X_1}$ is a $(k \times 1)$ vector of k predictors of the outcome of interest in the treatment unit, $\boldsymbol{X_0}$ is a $(k \times s_c)$ matrix of k predictors of the outcome of interest across the control units in the donor pool, $\boldsymbol{W}$ is a $(s_c \times 1)$ vector of state weights for all states in the donor pool), and $\boldsymbol{V}$ is a $(k \times k)$ diagonal matrix of predictor weights chosen to minimize the mean squared prediction error of the outcome of interest between the synthetic control unit and the treatment unit in preintervention periods. While it is standard practice to limit the donor pool to a set of control units similar to the treatment unit to reduce the risk of interpolation and overfitting when there is an extremely large number of units that could act as controls (Abadie et al. 2010), the implementation of this policy occurring at the state level limits the donor pool to

50 potential units (the District of Columbia and all states except California). My statistical power is, therefore, limited. Thus, the only states I exclude from the donor pool are those for which there are missing values of covariates: District of Columbia, Hawaii, and North Dakota, leaving me with 47 potential control units in the donor pool. This is similar in practice to the selection of donor pool units in Abadie, Diamond, and Hainmueller (2010), in which the only states excluded from the donor pool are those which are undergoing similar interventions as the treatment unit (leaving 38 states left in the donor pool). No other states undergo a similar intervention during my time period of analysis.

There are conflicting opinions on the appropriate balance between covariates and pre-intervention values of the outcome of interest in estimating the synthetic control unit. Some suggest that as many pre-intervention outcomes as possible should be used to increase the robustness of the synthetic control model to specification searching (Ferman et al. 2020), while others believe that using all pre-intervention outcomes together can result in overfitting (Kaul et al. 2021). Following Mei (2022), I choose to use the pre-intervention values of the outcomes of interest for approximately every other year in each synthetic control model (for each outcome of interest). For the synthetic control models of the overdose death rate and the incarceration rate (which are not pooled), I use the overdose death rate and incarceration rate (respectively) in 2005, 2007, 2009, 2011, and 2013 as predictors of that outcome of interest in every year after 2014, and covariates are averaged over every pre-treatment year (2005 through 2014). For the two pooled outcomes of interest (rate of illegal drug use other than marijuana and rate of unmet drug treatment need), since one data point corresponds to two years of data (the outcome of interest averaged over the prior year and the current year), I must use only every other year of data to avoid using the same year of data in two data points. However, to avoid overfitting, I cannot use every other year's value of the outcome of interest, as this would result in all pre-treatment values of the outcome being used as predictors. Therefore, I use the average value of the outcome of interest over the following pairs of years as predictors: 2005-2006, 2009-2010, and 2013-2014. The post-intervention outcome values predicted are the average values of the outcomes of interest over the following pairs of years: 2015-2016 and 2017-2018. The covariates (which are not pooled) are averaged over 2005, 2007, 2009, 2011, and 2013 (I am unable to average the covariates over all pre-treatment years due to the outcomes of interest being pooled).

For consistency, I use the same nine covariates in all four synthetic control models: the percentage of the population that is Black, percentage of the population between the ages of 15 and 29 (inclusive), percentage of residents living in poverty, population density, percentage of the population with a bachelor's degree, personal health expenditures per capita, whether a Good Samaritan Law is in effect in the state in a given year, whether a Naloxone Access Law is in effect in the state in a given year, and police per capita. In Abadie (2021), he suggests there are six requirements for the synthetic control method to be an appropriate methodology to evaluate the impact of a policy: the treatment effect must be large enough to distinguish from other small shocks (it is nearly impossible to truly know if this condition is satisfied until after the synthetic control method has been used), there must exist a control group for comparison which has not been exposed to the treatment (California is the only state to undergo the treatment until 2020, so all other states are available as a control group), the effect must not have been reacted to in advance by economic agents (the policy narrowly passed and took effect immediately, so this condition is satisfied), the treatment does not spillover into control units (since the intervention occurs at the state level, one would not expect a significant spillover into another state), a combination of control units must be able to approximate the characteristics of the treatment unit before the treatment occurs (the convex hull condition), and the period of analysis must include an ample amount of time after the treatment has taken effect (my period of analysis extends five years after the policy was passed). There are also three data requirements indicated by Abadie, all of which are satisfied: there must exist aggregate data on the outcomes of interest and their predictors (I have all of these data points at the state-year level), there must be a large pre-intervention window of data (my analysis begins in 2005, nine years before the policy was passed), and there must be a large post-intervention window of data (my analysis ends in 2019, five years after the policy was passed).

The synthetic control estimates do not have standard errors or confidence intervals; instead, placebo tests are used to construct empirical p-values as measures of statistical significance (Abadie 2010). These "placebos" are created by running the synthetic control method once with each control state as a fake treatment unit, instead of the true treatment unit. The rigorous way to do statistical inference relies on the ratio of the post-treatment mean squared prediction errors to the pre-treatment mean squared prediction errors. Since the synthetic control method minimizes the mean squared prediction error in the pre-treatment period, the logic behind using this ratio relies on an assumption that post-treatment mean squared prediction error is due largely to the treatment effect, as the average treatment effect is measured as the difference between the true average value of the outcome of interest and the synthetic control's average value of the outcome of interest in the post-treatment period (i.e., the "error" of the synthetic control model's prediction of the outcome of interest in the post-treatment period). One divides the number of states (including the treatment state) with an equal or higher ratio of the post-treatment to pre-treatment mean squared prediction error by the total number of states in the calculation to obtain the empirical p-value. However, it is worth noting that a case with a very low (or null) average treatment effect that oscillates above and below the true value can have an artificially inflated statistical significance, since the mean prediction error is squared.

To test robustness, it is standard practice to run alternate synthetic control models which leave out one predictor or control unit at a time to show that the omission of a single predictor or control state does not alter the results significantly (Abadie 2021). I also run difference-in-differences regressions with the same outcome variables, the same covariates as controls, state fixed effects, and year fixed effects at the national level, among states with similarly large populations, and among similar regional states to further demonstrate robustness. The specifics of these robustness tests, including the estimating equations for the difference-in-differences models, are laid out in Section 6.

5. Results

For the results of the synthetic control method to be valid, one wants the "characteristics" (weighted average predictor values) of the synthetic control unit to be similar to the characteristics of the treatment unit. In the two tables below, I summarize the true value of the predictors in the treatment unit (California), the value of the predictors in the synthetic control unit (the weighted average of the predictors across the control units), and the unweighted average of the predictors across the control units to demonstrate that the synthetic control unit matches the characteristics of the treatment unit more closely than a combination of control units. The first table below summarizes the characteristics of the predictors for the synthetic control models with a non-pooled outcome of interest (the overdose death rate and incarceration rate), and the table beneath that summarizes the characteristics of the predictors for the two synthetic control models with a pooled outcome of interest (rate of illegal drug use other than marijuana and rate of needing but not receiving drug treatment in a specialty facility).

Table 1: Predictor Values for Non-Pooled Outcomes of Interest

Predictors	True California (Overdose)	Synthetic California (Overdose)	Unweighted Average of Controls (Overdose)	True California (Incarceration)	Synthetic California (Incarceration)	Unweighted Average of Controls (Incarceration)
Outcome in 2005	10.66	10.6571	11.6869	0.48%	0.46%	0.44%
Outcome in 2007	11.53	11.4857	13.2217	0.48%	0.47%	0.45%
Outcome in 2009	11.61	11.5866	13.3985	0.46%	0.45%	0.44%
Outcome in 2011	11.75	11.8822	15.0304	0.40%	0.42%	0.44%
Outcome in 2013	12.38	12.41	15.8758	0.35%	0.38%	0.43%
Percentage Black	6.63%	11.6%	11.2%	6.63%	12.5%	11.2%
Percentage Age 15 to 29	21.9%	21.3%	20.8%	21.9%	20.4%	20.8%
Poverty Rate	14.8%	14.4%	14.1%	14.8%	12.4%	14.1%
Population Density	228	187	167	228	352	167
Percentage with Bachelor's Degree	30.15%	29.5%	27.5%	30.15%	33.5%	27.5%
Personal Health Expenditures per Capita	6348	6576	7047	6348	6686	7047
Good Samaritan Law	0.20	0.19	0.12	0.20	0.21	0.12
Naloxone Access Law	0.70	0.31	0.14	0.70	0.24	0.14
Police per 1,000 People	0.23	0.21	0.29	0.23	0.28	0.29

Source: All data references and author's own calculations

One sees that, in all four outcomes of interest, the synthetic control unit's relevant characteristics are substantially closer in value to that of the treatment unit than the unweighted average of the control units. This suggests the synthetic control acts as a better counterfactual for the treatment unit than an unweighted average of all potential control units.

Table 2: Predictor Values for Pooled Outcomes of Interest

Predictors	True California (Drug Use)	Synthetic California (Drug Use)	Unweighted Average of Controls (Drug Use)	True California (Treatment)	Synthetic California (Treatment)	Unweighted Average of Controls (Treatment)
Outcome in 2005-2006	3.92%	3.96%	3.77%	2.73%	2.74%	2.57%
Outcome in 2009-2010	3.80%	3.89%	3.68%	2.95%	2.90%	2.49%
Outcome in 2013-2014	4.07%	3.96%	3.20%	2.47%	2.48%	2.35%
Percentage Black	6.66%	10.6%	11.1%	6.7%	3.3%	11.1%
Percentage Age 15 to 29	22.1%	21.3%	20.9%	22.1%	21.5%	20.9%
Poverty Rate	14.6%	13.7%	13.95%	14.6%	15.2%	13.95%
Population Density	225	90.8	166	22	148	166
Percentage with Bachelor's Degree	30.05%	32.7%	27.3%	30.05%	28.6%	27.3%
Personal Health Expenditures per Capita	6212	6114	6910	6212	6334	6910
Good Samaritan Law	0.20	0.24	0.094	0.20	0.34	0.094
Naloxone Access Law	0.60	0.23	0.11	0.60	0.41	0.11
Police per 1,000 People	0.23	0.22	0.28	0.23	0.26	0.28

Source: All data references and author's own calculations

In each synthetic control unit, there are some individual control units represented that have clear similarities with California. For example, Texas, Illinois, and New York are also states with a large population, while Washington, Colorado, and New Mexico are also states in the same geographic region as California. However, many other control states represented in the synthetic control units, such as Connecticut, Utah, Mississippi, and Iowa do not share clear similarities with the treatment unit.

The synthetic control unit associated with the overdose death rate appears to match the treatment unit in the pre-treatment period well, except for a small divergence between the treatment unit and the synthetic control unit that occurs in the year leading up to the policy taking effect (2014). The average estimated treatment effect of California's Proposition 47 on the overdose death rate from 2015-2019 is -2.2987, suggesting Proposition 47 may have reduced the overdose death rate by 2.2987 deaths per $100{,}000$ people per year, on average.

Table 3: State Weights in Synthetic Control Units

State Weights				
State	**Drug Overdose**	**Drug Use**	**Treatment**	**Incarceration**
Texas	43.1%	0%	0%	0%
Washington	19.8%	0%	0%	0%
Illinois	15.2%	0%	0%	0%
Connecticut	12.3%	0%	0%	10.9%
Virginia	4.7%	0%	0%	0%
Georgia	3.2%	0%	0%	0%
Utah	1.8%	0%	1.3%	0%
Colorado	0%	64.1%	17.2%	45.1%
New York	0%	2.3%	0%	0%
Mississippi	0%	12.1%	0%	0%
New Mexico	0%	8.2%	33.8%	0%
Maryland	0%	8.8%	0%	0%
Iowa	0%	4.6%	0%	0%
Idaho	0%	0%	28.4%	0%
Rhode Island	0%	0%	13.1%	0%
Massachusetts	0%	0%	6.2%	0%
New Jersey	0%	0%	0%	22.8%
South Carolina	0%	0%	0%	21.3%

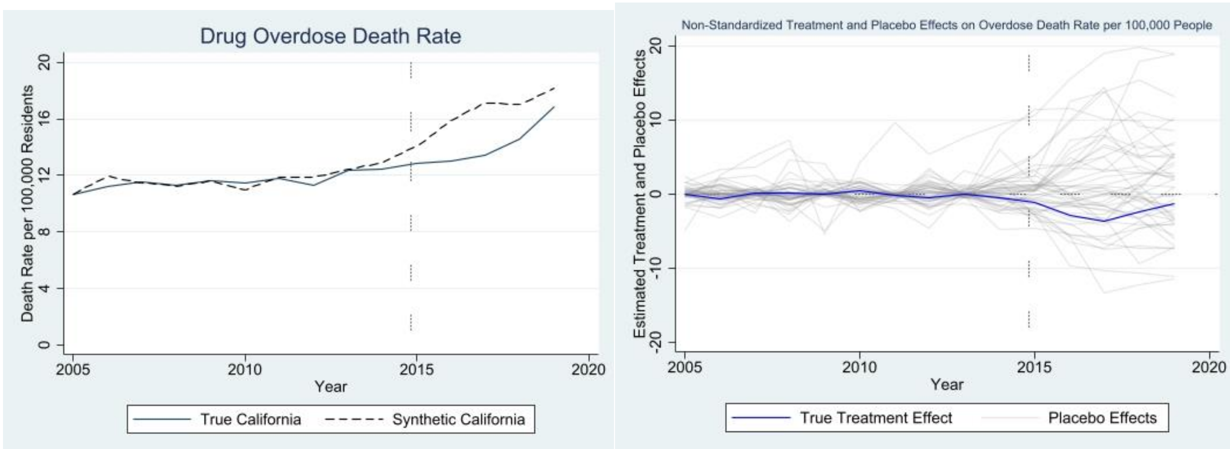
Source: All data references and author's own calculations

Table 4: Results of Synthetic Control Method for Outcomes of Interest

Outcome				
	Overdose Death Rate	**Illegal Drug Use other than Marijuana**	**Unmet Drug Treatment Need**	**Incarceration Rate**
Estimated Average Treatment Effect	-2.2987	-0.0012	0.0010	-0.000128
Pre-Treatment Root Mean Squared Error	0.36148	0.00076	0.00297	0.000218
Empirical p-value	0.2083	0.1042	0.8125	0.9792
Mean True Post-Treatment Value	14.14	0.0375	0.0293	0.00326
Mean Synthetic Post-Treatment Value	16.4387	0.0387	0.0283	0.00339
Estimated Percent Change	-14.0%	-3.1%	+3.5%	-3.8%

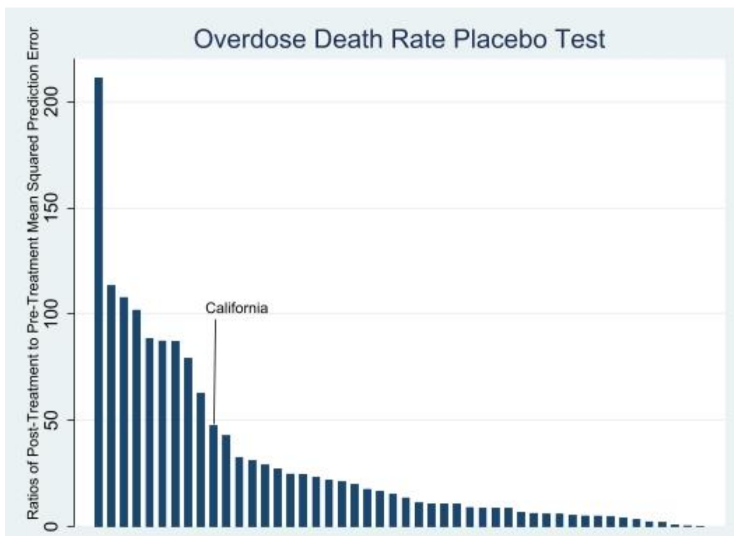
Source: All data references and author's own calculations

Figures 6 and 7: Synthetic Control Model for Drug Overdose, Estimated Treatment and Placebo Effects



Source: All data references and author's own calculations

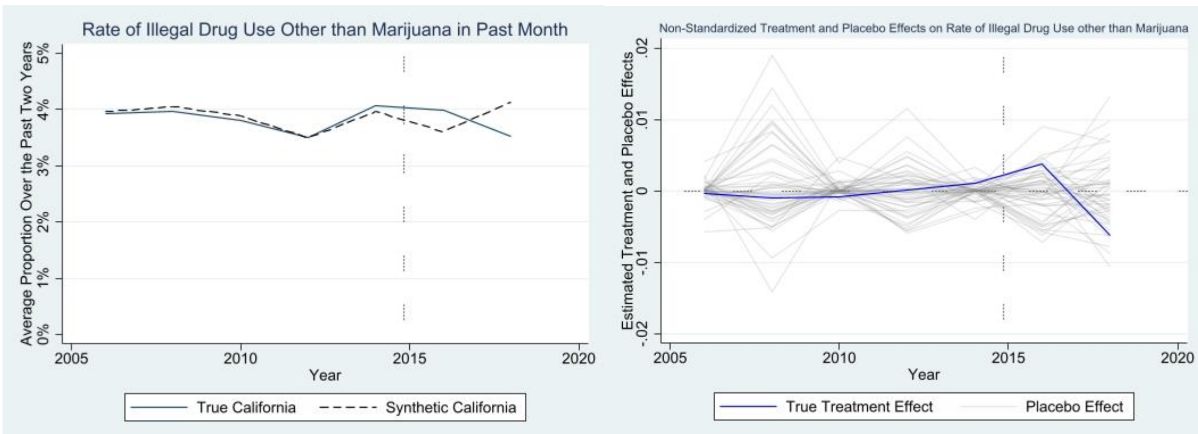
Figure 8: Placebo Test for Drug Overdose Synthetic Control Model



Source: All data references and author's own calculations

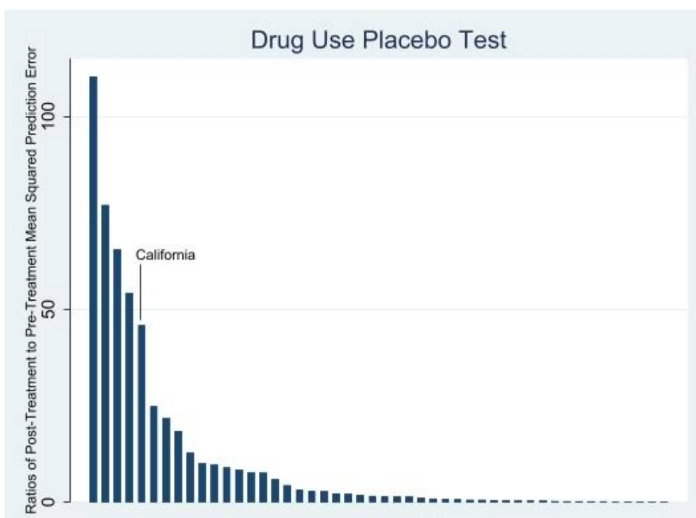
The average overdose death rate in California from 2015 to 2019 was 14.14 deaths per 100,000 people per year, while the synthetic control unit suggests the average overdose death rate would have been 16.4387 deaths per 100,000 people per year, implying that Proposition 47 may have reduced overdose deaths by roughly 14%. The placebo test shows that California's overdose rate synthetic control model has the tenth highest ratio of post-treatment to pre-treatment mean squared prediction error compared to placebo models, implying the empirical p-value associated with this synthetic control model is $p = \frac{10}{48} = 0.2083$. While this fails to reach a meaningful level of statistical significance, it is worth noting that the estimated treatment effect is substantial in magnitude (suggesting a 14% reduction in drug overdose deaths), suggesting drug decriminalization could result in a significant reduction in overdose deaths.

Figures 9 and 10: Synthetic Control Model for Drug Use, Estimated Treatment and Placebo Effects



Source: All data references and author's own calculations

Figure 11: Placebo Test for Drug Use Synthetic Control Model



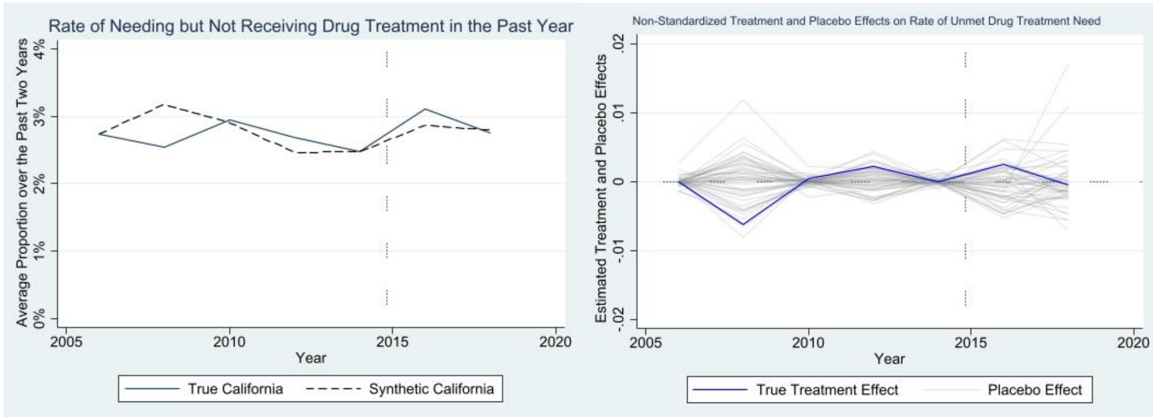
Source: All data references and author's own calculations

The synthetic control unit associated with the rate of illegal drug use other than marijuana matches the treatment unit well from 2005-2006 through 2011-2012 (since this is a pooled outcome variable) but diverges from the treatment unit in the last pre-period (2013-2014) before

Proposition 47 takes effect. The average estimated treatment effect of Proposition 47 on the proportion of California residents using illegal drugs other than marijuana from 2015 through 2018 is -0.0012, suggesting Proposition 47 may have reduced the proportion of California residents using illegal drugs other than marijuana by 0.12 percentage points per year, on average. The average proportion of illegal drug use other than marijuana in California from 2015 to 2018 was 3.75%, while the synthetic control unit suggests the average proportion of illegal drug use other than marijuana in California from 2015 to 2018 would have been 3.87% had Proposition 47 not passed, suggesting the measure may have reduced the proportion of residents using illegal drugs other than marijuana by 3%. The placebo test shows that California's drug use synthetic control model has the fifth-highest ratio of post-treatment to pre-treatment mean squared prediction error compared to the placebo models, implying the empirical p-value associated with this synthetic control model is $p = \frac{5}{48} = 0.1042$. While this is higher than the p-value associated with the synthetic control model for the overdose death rate, the magnitude of the treatment effect is much smaller (in terms of percent reduction), and the treatment effect in this case appears to be positive over 2015-2016 and negative over 2017-2018, nearly cancelling out the average treatment effect. This suggests that drug overdose may be substantially more sensitive to drug decriminalization than drug use, which could allow drug overdose deaths to be substantially reduced with either a null effect or small positive effect on the drug use rate.

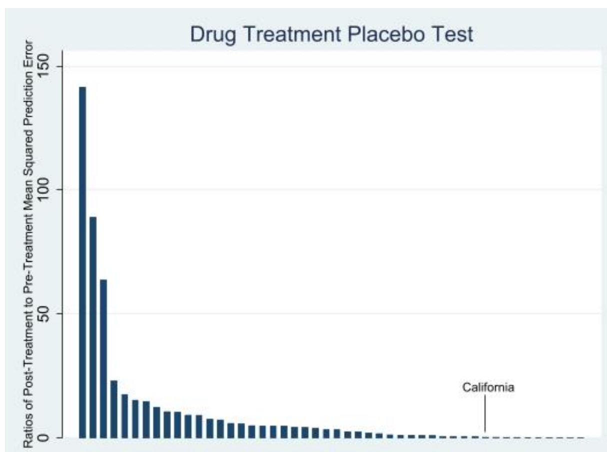
The synthetic control unit associated with unmet drug treatment need does not match the treatment unit as well in the pre-period as the synthetic control unit in the prior two models, showing substantial divergence from the treatment unit in the first two pre-periods (2005-2006 and 2007-2008), and only loosely matching the treatment unit's pre-treatment trends in the next three pre-periods (2009-2010, 2011-2012, and 2013-2014).

Figures 12 and 13: Synthetic Control Model for Unmet Drug Treatment Need, Estimated Treatment and Placebo Effects



Source: All data references and author's own calculations

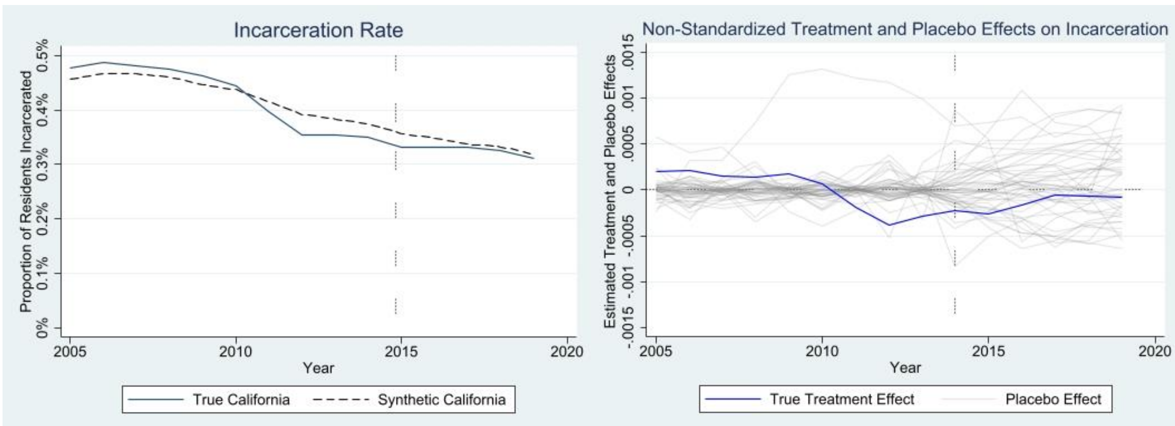
Figure 14: Placebo Test for Unmet Drug Treatment Need Synthetic Control Model



Source: All data references and author's own calculations

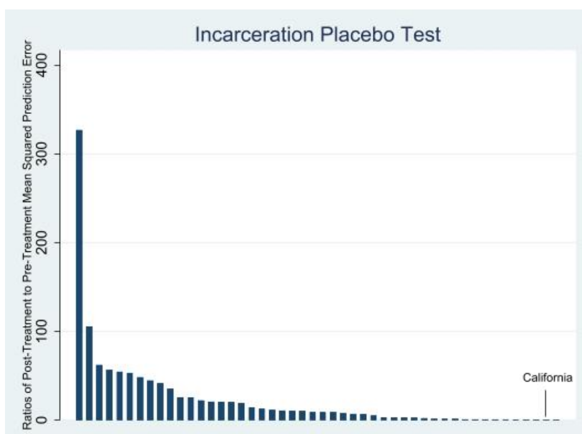
The average estimated treatment effect of Proposition 47 on the proportion of California residents who have an unmet drug treatment need is 0.0010 from 2015 to 2018, suggesting Proposition 47 may have increased the rate of unmet drug treatment by 0.10 percentage points on average. The average proportion of unmet drug treatment need over 2015 through 2018 was 2.93%, while the synthetic control unit suggests the average proportion of unmet drug treatment need over 2015 through 2018 would have been 2.83% had Proposition 47 not passed, suggesting Proposition 47 may have resulted in a 3.4% increase in the rate of unmet drug treatment need. The placebo test shows that California's unmet drug treatment need synthetic control model has the 39th highest ratio of post-treatment to pre-treatment mean squared prediction error compared to the placebo models, implying the empirical p-value associated with this synthetic control model is $p = \frac{39}{48} = 0.8125$. This does not provide any evidence of a substantial impact of California's Proposition 47 on the rate of unmet drug treatment need.

Figures 15 and 16: Synthetic Control Model for Incarceration, Estimated Treatment and Placebo Effects



Source: All data references and author's own calculations

Figure 17: Placebo Test for Incarceration Synthetic Control Model



Source: All data references and author's own calculations

The synthetic control unit associated with incarceration does not match the treatment unit as well as the models associated with drug overdose deaths and illegal drug use other than marijuana, but matches the general trends well, though I do see slightly negative pre-trends in incarceration. The average estimated treatment effect of Proposition 47 on the proportion of

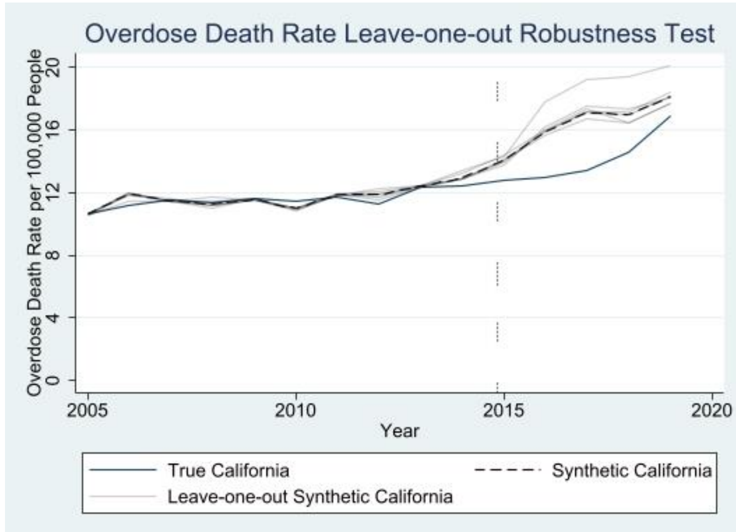
California’s residents that are incarcerated is -0.000128 from 2015 to 2019, suggesting that Proposition 47 may have reduced the incarceration rate by 0.0128 percentage points, on average. The average proportion of Californian residents in jail from 2015 to 2019 was 0.326%, while the synthetic control unit suggests that the average proportion of Californian residents in jail from 2015 to 2019 would have been 0.339% had Proposition 47 not passed, suggesting Proposition 47 may have resulted in a 3.8% decrease in the incarceration rate. The placebo test shows that California’s incarceration synthetic control model has the 47th highest ratio of post-treatment to pre-treatment mean squared prediction error compared to the placebo models, implying the empirical p-value associated with this synthetic control model is $p = \frac{47}{48} = 0.9792$. This does not provide any evidence of a substantial impact of California’s Proposition 47 on the rate of incarceration.

While the impacts of California’s Proposition 47 on the rate of unmet drug treatment need and incarceration appear to be insignificant both statistically and in magnitude, and its impact on the rate of drug use appears to be insignificant in magnitude while just falling short of the 10% statistical significance threshold, its impact on the drug overdose death rate appears substantial in magnitude, though it fails to come near the 10% threshold for statistical significance. This suggests that, despite the state-year level data limiting my statistical power, the public policy ramifications of these findings could be substantial if further data suggests the treatment effects of California’s Proposition 47 are of similar magnitudes, at lower thresholds of statistical significance. These magnitudes suggest that drug decriminalization may reduce the rate of overdose deaths without substantially increasing the rate of drug use, which would provide a strong case for drug decriminalization.

6. Robustness Checks

The results of the synthetic control method can be sensitive to choice of control units or covariates, therefore, it is recommended to run alternative specifications of the synthetic control method to test the robustness of the results (Abadie 2021). To test the robustness of the results with respect to choice of control units, it is standard practice to do a “leave-one-out” robustness test (Abadie 2021), where the synthetic control method is reran as many times as there are control units that receive a positive weight in the synthetic control unit, with one control unit excluded from the donor pool each time, to demonstrate the estimates are not overly dependent on a single control unit. To test the robustness of the results with respect to covariate selection, it is recommended to run alternative specifications of the synthetic control method with some covariates omitted (Abadie 2021). For each outcome of interest, I demonstrate whether the estimated treatment effect is robust to omission of covariates by running the synthetic control method for every combination of 8 and 7 covariates (of my original 9 covariates), and showing the distributions of the treatment effects for the combinations in addition to the original model. I also include the distributions of the empirical p-values for these models, to demonstrate that the empirical p-values for the models I include in the results section are not anomalies.

Figure 18: Leave-one-out Robustness Test for Overdose Synthetic Control Model

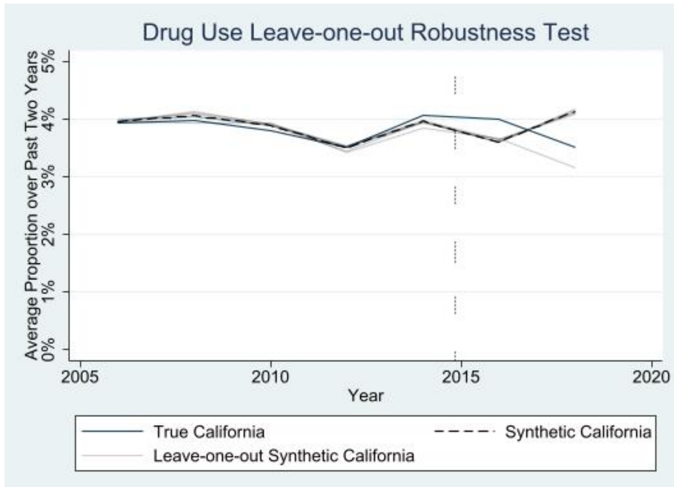


Source: All data references and author's own calculations

The above figure shows that none of the leave-one-out synthetic control models for overdose death rate display a treatment effect of substantially lower magnitude than the original synthetic control model, suggesting the estimated treatment effect I obtained is robust to omission of individual control units (even with one synthetic control model suggesting a substantially higher estimated treatment effect), as none of the leave-one-out synthetic control models suggest the estimated treatment effect of -2.30 overdose deaths per 100,000 residents is a great overestimate.

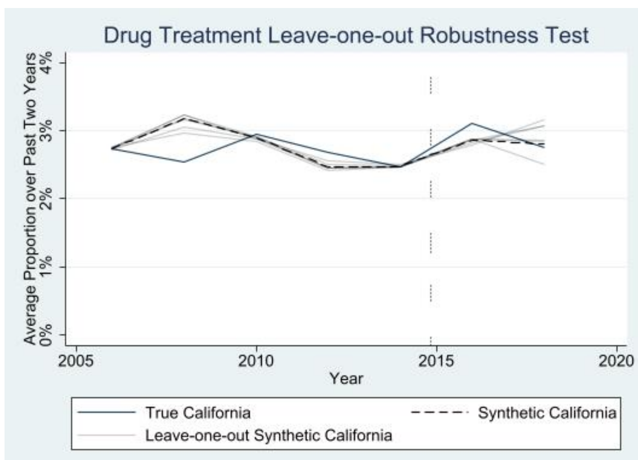
The below figure shows that the estimated treatment effect on the rate of illegal drug use other than marijuana is not fully robust to omission of individual control units, as the omission of one control unit (associated with the synthetic control model with the gray line that trends downward parallel to California's true overdose death rate over the last two years) suggests a positive treatment effect of a small, non-negligible magnitude rather than a negative treatment effect of negligible magnitude. The omitted control unit in this synthetic control model is Colorado, which (according to Table 3) comprises 64.1% of the synthetic control unit in my main drug use model. This suggests my results for the rate of illegal drug use other than marijuana are moderately sensitive to the use of Colorado as a control unit.

Figure 19: Leave-one-out Robustness Test for Drug Use Synthetic Control Model



Source: All data references and author’s own calculations

Figure 20: Leave-one-out Robustness Test for Unmet Drug Treatment Need Synthetic Control Model

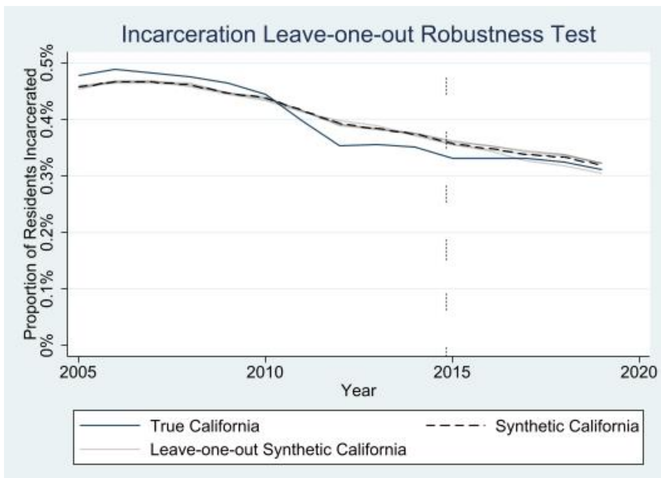


Source: All data references and author’s own calculations

The above figure shows that the leave-one-out estimates of the treatment effect on unmet drug treatment need do not deviate substantially from the main synthetic control unit’s estimate through 2016, but the 2017-2018 leave-one-out estimates deviate from the synthetic control unit to an extent, in both directions. This suggests my estimated treatment effect of California’s Proposition 47 on the rate of unmet drug treatment need may not be perfectly robust to choice of control units, but the leave-one-out estimates still suggest a roughly null effect, so this does not affect my results.

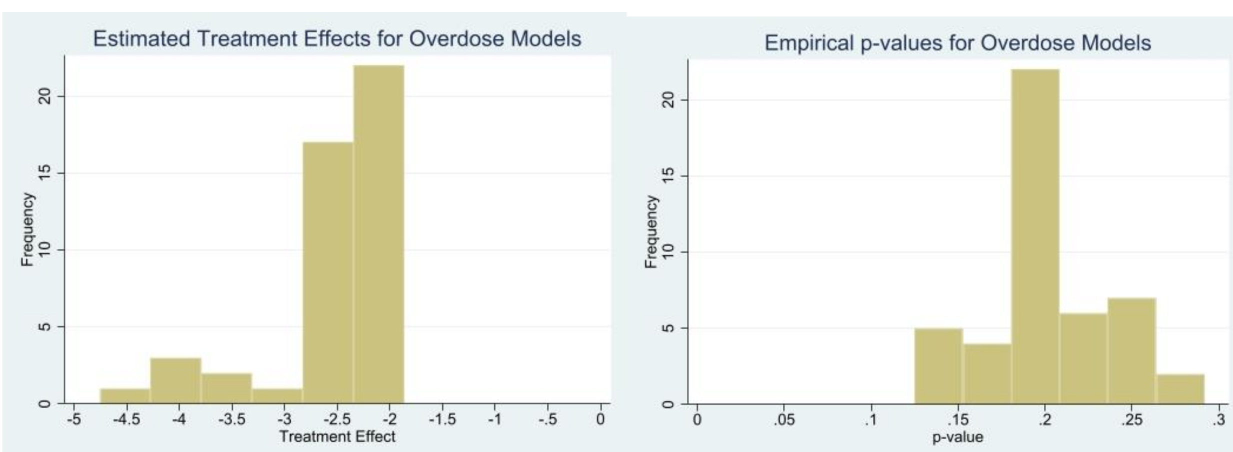
The below figure shows that all the leave-one-out synthetic control unit estimates for the incarceration rate closely match the main synthetic control unit estimates for the incarceration rate for the entire period of analysis. This suggests my estimate of the treatment effect of California's Proposition 47 on the incarceration rate is robust to choice of control units.

Figure 21: Leave-one-out Robustness Test for Incarceration Synthetic Control Model



Source: All data references and author's own calculations

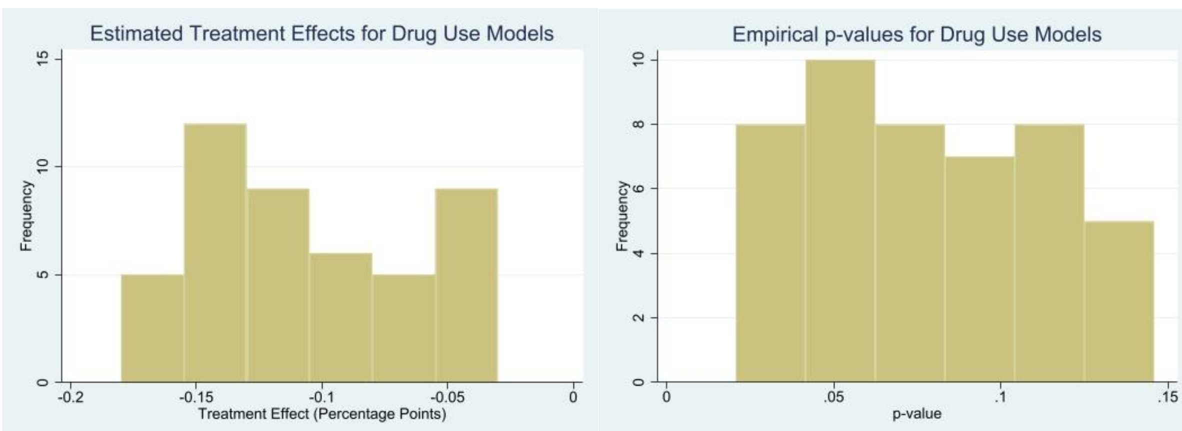
Figures 22 and 23: Estimated Treatment Effects and Empirical p-values for Overdose Models



Source: All data references and author's own calculations

The estimated treatment effect of my main synthetic control model for the overdose death rate is -2.2987 per $100{,}000$ residents, which one can see in the figure above is near the center of the distribution of estimated treatment effects for the 46 overdose models I ran. One can also see the empirical p-value of 0.2083 is also near the center of the distribution of empirical p-values of the 46 overdose models I ran. This suggests the estimated treatment effect and empirical p-value of my main overdose synthetic control model were not outliers, relative to the synthetic control models for overdose associated with other combinations of covariates.

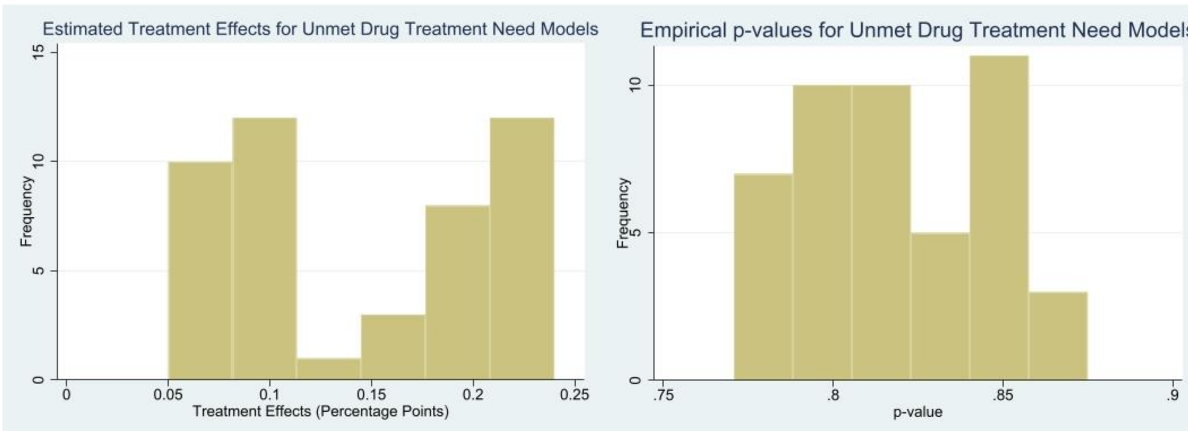
Figures 24 and 25: Estimated Treatment Effects and Empirical p-values for Drug Use Models



Source: All data references and author's own calculations

The estimated treatment effect of my main synthetic control model for the rate of illegal drug use other than marijuana is -0.12 percentage points, which one can see in the figure above is near the center of the distribution of estimated treatment effects for the 46 drug use models I ran. One can also see the empirical p-value of 0.1042 is in the upper end of the distribution of empirical p-values of the 46 drug use models I ran, but is by no means an anomaly. Many of the models are significant at the 10% level, and the treatment effects associated with the significant estimates (at the 10% level) do not seem to differ substantially from the treatment effects associated with non-significant estimates. This suggests the estimated treatment effect and empirical p-value of my main drug use synthetic control model were not outliers, relative to the synthetic control models for drug use associated with other combinations of covariates. Also, it suggests drug decriminalization may have a marginal, yet statistically significant, negative effect on the rate of illegal drug use other than marijuana.

Figures 26 and 27: Estimated Treatment Effects and Empirical p-values for Unmet Drug Treatment Need Models

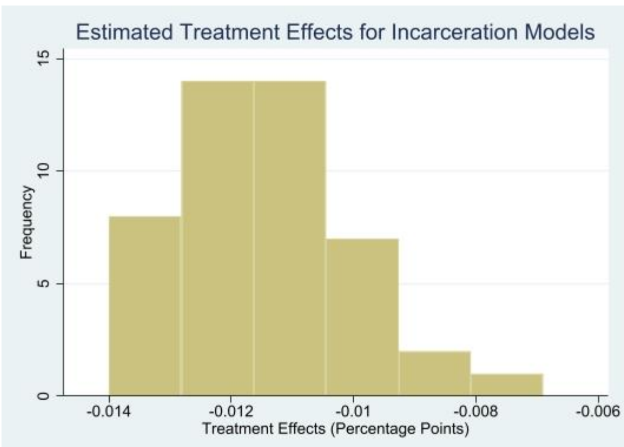


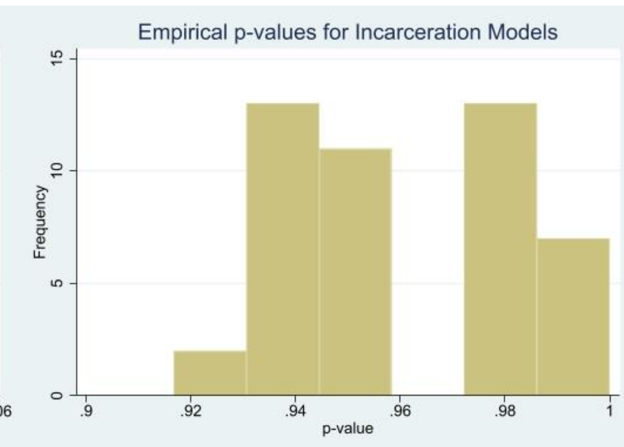
Source: All data references and author's own calculations

The estimated treatment effect of my main synthetic control model for the rate of unmet drug treatment need is 0.1 percentage points, which one can see in the figure above is near the center of the distribution of estimated treatment effects for the 46 unmet drug treatment need models I ran. One can also see the empirical p-value of 0.8125 is also near the center of the distribution of empirical p-values of the 46 unmet drug treatment need models I ran. This suggests the estimated treatment effect and empirical p-value of my main synthetic control model for unmet drug treatment need were not outliers, relative to the synthetic control models for unmet drug treatment need associated with other combinations of covariates.

The estimated treatment effect of my main synthetic control model for the incarceration rate is -0.01 percentage points, which one can see in the figure above is at the center of the distribution of estimated treatment effects for the 46 incarceration models I ran. One can also see the empirical p-value of 0.9792, while in the upper end of the distribution, is also near the center of the distribution of empirical p-values of the 46 incarceration models I ran. This suggests the estimated treatment effect and empirical p-value of my main synthetic control model for incarceration were not outliers, relative to other potential synthetic control models for incarceration associated with other combinations of covariates.

Figures 28 and 29: Estimated Treatment Effects and Empirical p-values for Incarceration Models





Source: All data references and author's own calculations

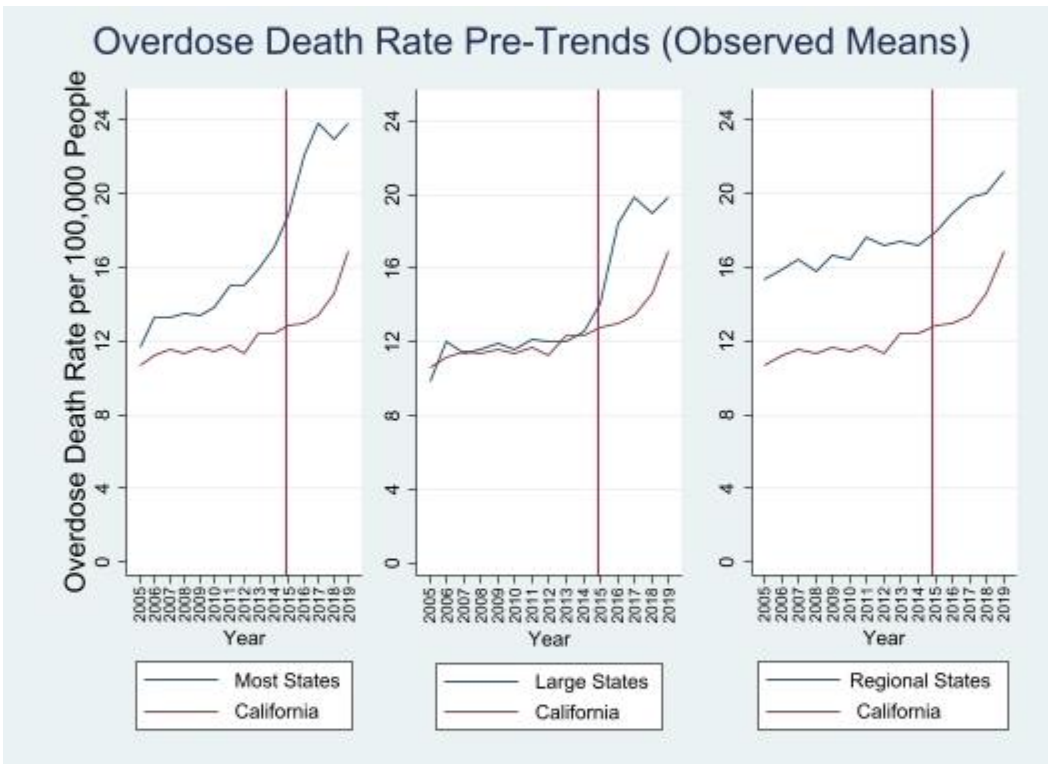
As an additional robustness check, I run three difference-in-differences regressions for each outcome of interest with the same covariates in the synthetic control method as controls in the regression, state fixed effects, year fixed effects, standard errors clustered at the state level, and three different control groups: all states (except those excluded from the donor pool in the synthetic control models), other large states, and regionally similar states. The "large states" control group consists of four states: Texas, Florida, New York, and Illinois. The "regional" control group consists of five states: Washington, Oregon, Nevada, Arizona, and Colorado. These control groups were selected before looking at results or pre-trends. The estimating equation for each outcome of interest is:

$$Y_{st} = \beta_0 + \beta_1 treat_{st} + X_{st} + \alpha_s + \gamma_t + \varepsilon_{st},$$

where Y_{st} represents the outcome of interest in state s and year t, $treat$ is a binary indicator indicating whether Proposition 47 is in effect in state s during year t (1 if California after 2014, 0 otherwise), X_{st} is a vector of controls, α_s represents state fixed effects, and γ_t represents year fixed effects. There are 9 controls, identical to the covariates used in the synthetic control models: percentage of residents that are black, percentage of residents that are between age 15 and 29, poverty rate, population density, percentage of residents with a bachelor's degree, personal health expenditures per capita, whether a good Samaritan law was in effect for at least half of the year, whether a naloxone access law was in effect for at least half of the year, and state-employed police per capita.

While the "most states" control group has nearly parallel trends in the overdose death rate to California across the pre-treatment period, one can see the mean overdose death rate among most states increases at a substantially greater rate than California in the two years leading up to the pre-treatment period, suggesting the estimated treatment effect may be an overestimate. The mean overdose death rate among the "large states" group clearly has parallel (nearly identical) trends to California's overdose death rate, with small positive pre-trends in the control group before the treatment takes effect, suggesting this difference-in-differences model is valid, but may overestimate the treatment effect. The "regional" control group has nearly perfect parallel trends in the overdose death rate to California before Proposition 47 took effect, suggesting this difference-in-differences model is valid than the other two, and suggesting the other estimated treatment effects may be overestimates. The difference-in-differences regressions on overdose death rate with the "most states" and "large states" control groups both suggest that the treatment effect of California Proposition 47 on the overdose death rate is statistically significant at the 5% level (1% level with the "most states" control group). In both cases, California's Proposition 47 is estimated to have resulted in a 31% reduction in overdose deaths, which is substantially greater in magnitude than the 14% reduction in overdose deaths suggested by my synthetic control model. difference-in-differences regression on overdose death rate with the "regional" control group suggests a null treatment effect, with a treatment effect that is positive in magnitude but insignificant both statistically and in magnitude.

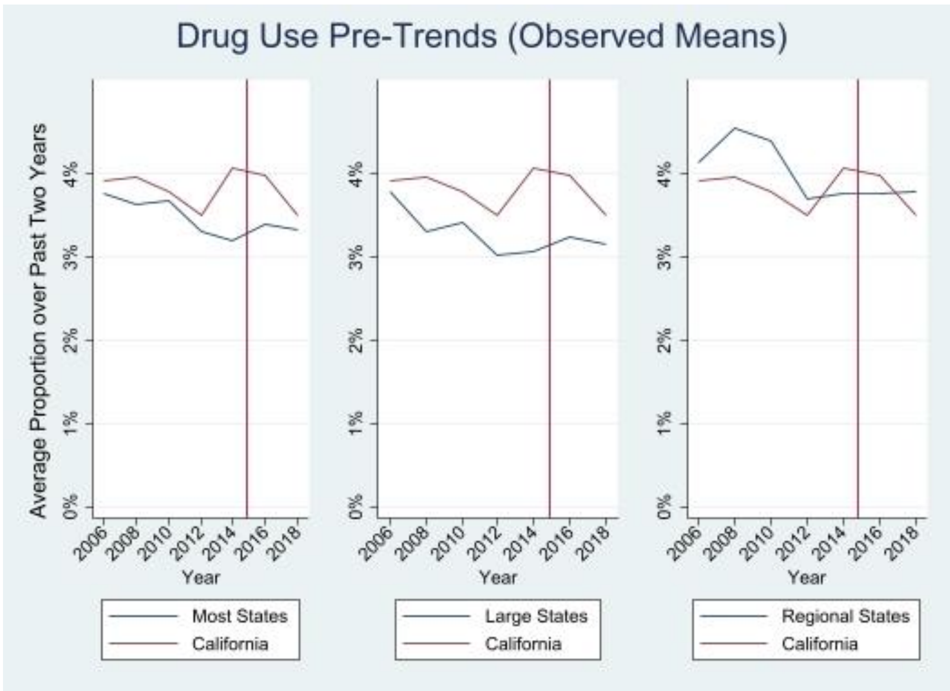
Figure 30: Pre-Trends in Drug Overdose



Source: All data references and author's own calculations

The mean rates of illegal drug use other than marijuana among the "regional" and "most states" control groups clearly did not have parallel trends to California before Proposition 47 took effect, suggesting those difference-in-differences models may not be valid. The mean rate of illegal drug use other than marijuana among the "large states" control group has some of the same general trends as California in the pre-treatment period, but has a small jump rather than a small decline from 2007-2008 to 2009-2010, and California has a much larger jump than the control group right before the treatment takes effect, suggesting the estimated treatment effect of California's Proposition 47 with the "large states" control group may be a substantial underestimate (meaning the true treatment effect may be null or positive in magnitude).

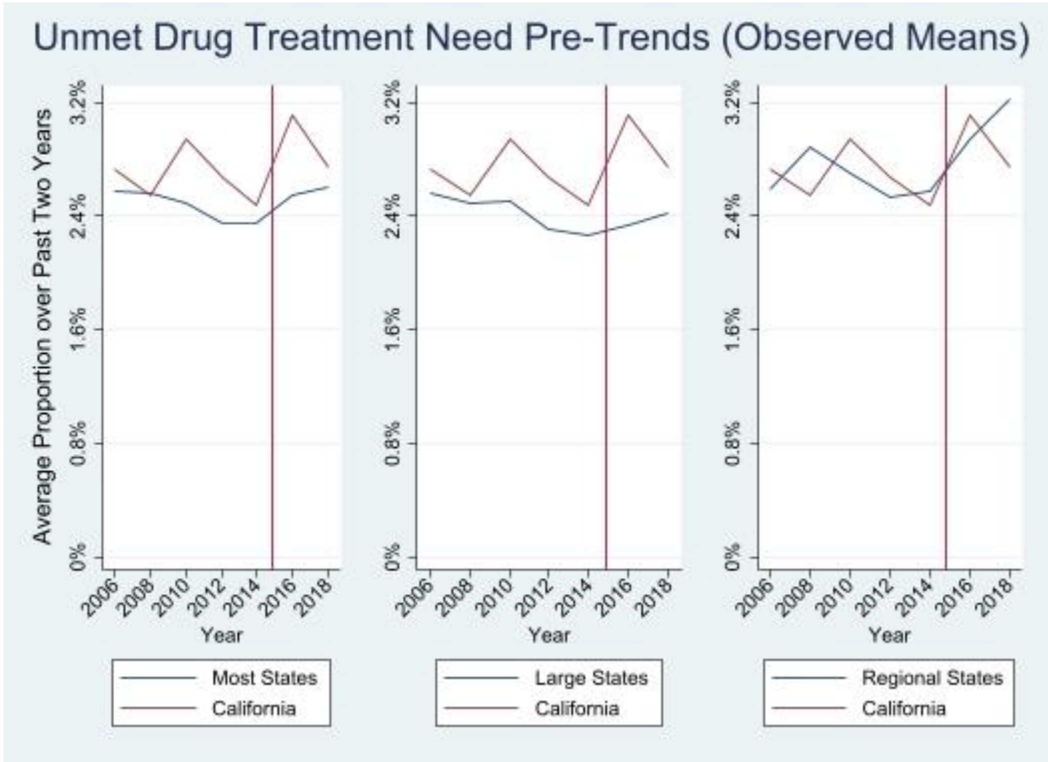
Figure 31: Pre-Trends in Drug Use



Source: All data references and author's own calculations

This suggests none of the three difference-in-differences estimates are particularly reliable. The difference-in-differences regressions on the rate of illegal drug use other than marijuana with all three control groups suggest the treatment effect of California's Proposition 47 on the rate of illegal drug use other than marijuana is not statistically significant at the 10% level.

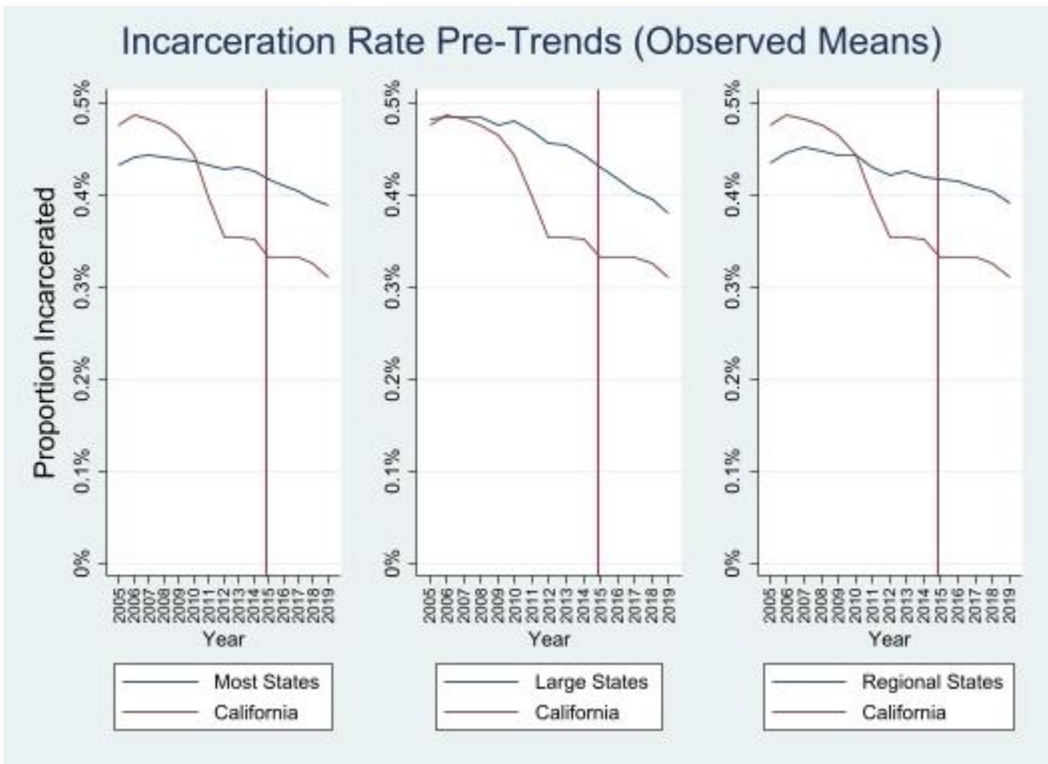
Figure 32: Pre-Trends in Unmet Drug Treatment Need



Source: All data references and author's own calculations

None of the control groups have remotely similar trends in the rate of unmet drug treatment need to California, suggesting the difference-in-differences estimates are not valid. The difference-in-differences regressions on incarceration rate with the "most states" and "regional" control groups suggest the treatment effect of California's Proposition 47 is negative and of substantial magnitude, while the difference-in-differences regression on incarceration rate with the "large states" control group suggests the treatment effect is either positive and extremely small or null, failing to meet a statistical significance level of 90%. The estimated treatment effect associated with the "most states" control group is significant at the 1% level, while the estimated treatment effect associated with the "large states" control group approaches statistical significance but falls short of the 10% significance level. While none of the control groups neatly meet the parallel trends assumption over the entire pre-treatment period, all three control groups follow the same general trend in the incarceration rate as California, generally decreasing over time. California experienced a massive dip in the incarceration rate from 2006 to 2012, and one can see that all three control groups follow parallel trends neatly from 2012 to 2014 and onwards into the post-treatment period, which would suggest a null treatment effect, contrary to the treatment effects suggested by the difference-in-differences model.

Figure 33: Pre-Trends in Incarceration



Source: All data references and author's own calculations

Two of the difference-in-differences models associated with the overdose death rate are consistent with the synthetic control model's estimation of a substantial negative treatment effect on the drug overdose death rate, and the third suggests a null treatment effect. All have good pre-trends, though the model suggesting the null treatment effect has slightly better pre-trends. All the difference-in-difference models associated with the rate of illegal drug use other than marijuana are consistent with a null treatment effect on this outcome. There is no evidence of a substantial treatment effect on unmet drug treatment need or incarceration.

Table 5: Results of Difference-in-Differences Regressions on Outcomes of Interests

	Outcome			
	Overdose Death Rate	Illegal Drug Use other than Marijuana	Unmet Drug Treatment Need	Incarceration
Panel A: Synthetic Control Results				
Treatment Effect	-2.2987	-0.0012	0.0010	-0.000128
Pre-Treatment Mean Squared Prediction Error	0.36148	0.00076	0.00297	0.000218
Empirical p-value	0.2083	0.1042	0.8125	0.9792
Estimated Percent Change	-14.0%	-3.1%	+3.5%	-3.8%
Panel B: Difference-in-Differences with "Most States" Control Group				
Treatment Effect (Robust SE)	-6.742*** (1.110)	0.0007824 (0.001227)	0.0020405*** (0.000673)	-0.0005969*** (0.0001143)
Estimated Percent Change	-31.4%	2.1%	7.5%	-15.3%
p-value	<0.001	0.527	0.004	<0.001
Adjusted R^2	0.7902	0.5843	0.5408	0.9636
N	720	336	336	720
Panel C: Difference-in-Differences with "Large States" Control Group				
Treatment Effect (Robust SE)	-6.421** (1.717)	-0.003205 (0.001721)	0.002588* (0.001158)	0.0000123 (0.0001065)
Estimated Percent Change	-31.2%	-7.9%	9.7%	0.37%
p-value	0.02	0.136	0.089	0.914
Adjusted R^2	0.8997	0.7684	0.7074	0.9841
N	75	35	35	75
Panel D: Difference-in-Differences with "Regional" Control Group				
Treatment Effect (Robust SE)	0.6483 (0.9953)	0.002576 (0.003618)	-0.0009696 (0.002955)	-0.0004388 (0.0002807)
Estimated Percent Change	4.8%	7.4%	-3.2%	-11.7%
p-value	0.544	0.508	0.756	0.179
Adjusted R^2	0.8876	0.2394	0.2345	0.9670
N	90	42	42	90

Source: All data references and author's own calculations
* p<0.1, ** p<0.05, *** p<0.01

7. Conclusion

While my results are not as robust or statistically precise as I would like, I do find substantial evidence that there could have been a substantial reduction in the overdose death rate due to California's Proposition 47, and I find no evidence that California's Proposition 47 caused a substantial increase in the overdose death rate. Since my results for the overdose death rate suggested a negative treatment effect of significant magnitude, but not of statistical significance, this warrants additional research into the potential of drug decriminalization as a policy to tackle the opioid crisis. The policy ramifications are significant, as decriminalization is estimated to save $41.3 billion per year (Miron 2018). Even if the effect is null, this indicates the money could be directed to other potentially effective methods for reducing overdose death rates or other essential services.

I also find no evidence that California's Proposition 47 had a significant impact on the rate of illegal drug use other than marijuana, as the synthetic control model and the difference-in-difference models suggest treatment effects that are of insignificant magnitude. This also has substantial policy implications in support of drug decriminalization, as one of the fears associated with the prospect of drug decriminalization is that this will greatly increase drug use and the number of addicts. If drug decriminalization has little effect on the rate of illegal drug use other than marijuana and does not result in a substantial increase in the overdose death rate, this suggests that the opioid crisis can be greatly improved through drug decriminalization. I find no evidence that California Proposition 47 had a substantial treatment effect on the rate of unmet drug treatment need or incarceration. These results have significant policy implications and lend substantial credibility to the arguments of drug decriminalization advocates, suggesting that drug criminalization does not reduce the rate of drug use or overdose deaths.

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