University of Toronto
Department of Economics

Working Paper 745

Does decriminalization cause more drug overdose deaths? Evidence from Oregon Measure 110

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November 29, 2022

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November 24, 2022

Abstract

This paper evaluates the causal effect of drug decriminalization on drug overdose deaths in a context where decriminalization was not accompanied by substantial public health investments. Using the synthetic control method, I find that when Oregon decriminalized small amounts of drugs in February 2021, it caused 181 additional drug overdose deaths during the remainder of 2021. This represents a 23% increase over the number of drug overdose deaths predicted if Oregon had not decriminalized drugs. My estimates suggest that decriminalization had similar effects on drug overdose deaths among men and women and among white and non-white people.

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†I am very grateful to William Arbour for his helpful comments on this paper.

JEL classification: D04, I18, K42

Keywords: drug decriminalization, illicit drugs, synthetic control method

1 Introduction

There is debate in many jurisdictions as to whether or not drugs should be decriminalized. Proponents of drug decriminalization argue that it will reduce the stigma associated with drug use so that people using drugs are encouraged to seek treatment, remove barriers to harm reduction practices, and reduce government expenditure in the justice system (Drug Policy Alliance). Skeptics fear that liberalization would dramatically increase the number of people using drugs by removing the threat of significant punishment (Burke, 2016). Other commentators believe that in order to reduce substance abuse and associated harms, decriminalization is a necessary, but not sufficient step. They argue that the legal remedy of decriminalization must be paired with public health investments in order to achieve positive results (Greer and Shane, 2017; University of Guelph, 2022). Establishing whether decriminalization increases substance abuse is a subject of significant economic importance given the large economic costs of substance abuse and of criminalizing substance abuse.[1]

Despite the importance of the topic, there has been relatively little causal research establishing the impact of decriminalization on drug abuse. This is perhaps unsurprising given that drugs other than cannabis remain illegal in most jurisdictions worldwide. My research contributes to the existing literature by providing evidence from a natural experiment on the effect of drug decriminalization on drug overdose deaths.

In particular, I study the impact of a policy change in Oregon in February 2021 that decriminalized small amounts of drugs. Measure 110, which came into effect on February 1, 2021, reduced penalties for drug possession and made Oregon the first US state to decriminalize the personal possession of illegal drugs. Measure 110 reduced the penalty for possession of small amounts of controlled substances[2] from a criminal misdemeanor to a violation punishable by a $100 fine. In lieu of a fine, a person charged with a violation could instead complete a health assessment at an addiction recovery center (Oregon Legislative Policy and Research Office, 2020). Subsequent to this policy change, drug arrests in Oregon dropped significantly; while US drug possession arrests dropped by about 30% between 2020 and 2021, Oregon drug possession arrests dropped by almost 60%.[3] (In this paper, I do not focus on estimating the causal impact of Measure 110 on drug possession arrests, but do provide some imprecise estimates in Appendix B suggesting that decriminalization indeed had a large negative effect on drug possession arrests.)

I use the synthetic control method developed in Abadie and Gardeazabal (2003) and Abadie et al. (2010) to study the impact of Measure 110 on drug overdose deaths in Oregon. I construct a "synthetic" Oregon using drug overdose death data from other US states. I obtain monthly drug overdose death data for 2018-2021 from the US Centers for Disease Control and Prevention's (CDC's) Provisional Multiple Cause of Death database (PMCDD).[4] I estimate that Oregon's decriminalization resulted in 181 additional drug overdose deaths from February 2021 to December 2021. Given that the "synthetic" Oregon had 796 drug overdose deaths in this period, I conclude that decriminalization caused a 23% increase in drug overdose deaths. I look for heterogeneous effects between men and women and between white people and non-white people, but do not find evidence of substantial differences.

I evaluate the significance of my estimated treatment effects using a placebo test. Following Abadie et al. (2010), I ask: How often would I obtain results of this magnitude if I had chosen a state at random for the study instead of Oregon? I simulate the policy experiment in each US state (including the District of Columbia) except Washington[5], collect each pre-Measure 110 and post-Measure 110 mean squared prediction error (MSPE), and find that Oregon's post/pre-Measure 110 MSPE ratio is the largest of any state. That is, if one were to assign the intervention at random in the data, the probability of obtaining a post/pre-Measure 110 MSPE ratio as large as Oregon's is $1/50 = 0.02$. Motivated by Abadie et al. (2015), I additionally conduct a placebo test where I pretend that Measure 110 took effect in August 2019 rather than February 2021[6] and find that the fictional policy had a much smaller and statistically insignificant effect on drug overdose deaths.

I conduct several robustness checks to support my results. I use the "leave-one-out" robustness test demonstrated in Abadie et al. (2015) to show that my results are not driven by any particular control unit. I change the treatment month to November 2020, which is when Measure 110 actually passed, to account for possible anticipation effects and find similar results. I discuss other policy changes in Oregon in early 2021 and rule out the possibility that they are obscuring my inference. Finally, I note that Washington liberalized their drug possession laws in 2021, evaluate the impact of liberalization there, and find that liberalization had a positive effect on drug overdose deaths. (I include this analysis as a robustness check rather than as part of my main analysis since Washington decriminalized drugs in February 2021 and then re-criminalized them in May 2021, albeit with much less severe punishments, which makes inference from post-treatment trends less clear.)

This paper is one of the first attempts to estimate the causal effect of comprehensive drug decriminalization on drug overdose deaths. There has been research on the impacts of cannabis decriminalization, but, to the best of my knowledge, the only other paper that has studied the causal impacts of broader drug decriminalization is Félix et al. (2017). In this paper, the authors find that Portugal's 2001 drug decriminalization law decreased drug-related deaths. However, it is important to note here that Portugal did not *just* decriminalize drugs in 2001. Rather, decriminalization was one of 13 strategies listed in Portugal's 1999 *National Strategy for the Fight against Drugs*. Redirecting focus towards primary prevention, improving the quality and response capacity of the healthcare network, extending harm reduction, and doubling public investment in addiction prevention and treatment over five years were some of the other pillars of the approach. It is thus difficult to say whether Félix et al. (2017) are only capturing the effects of decriminalization in Portugal or whether they are instead capturing the effects of Portugal's broader drug strategy.[7]

This paper evaluates decriminalization in a context where there is minimal additional investment in public health programs. Measure 110 was supposed to divert about $300 million in tax revenue to pay for drug and alcohol treatment and recovery services every two years (Green, 2022a). However, due to bureaucratic hold-ups, only $40 million had actually been disbursed by June 2022 (Selsky, 2022). Given that Oregon was spending about $236 million annually on substance abuse prevention and treatment services before decriminalization (Fitzgerald and Schmidt, 2019), this additional $40 million did not represent a major increase in funding. As of April 2022, no new treatment beds had been funded in Oregon (Crombie, 2022). It is thus likely that my research more closely isolates the effect of decriminalization than the study of Portugal.

2021 Oregon also differs from 2001 Portugal in (at least) two other potentially important ways. First, the prevalence of powerful synthetic opioids like fentanyl means that drugs themselves are deadlier today than they were two decades ago.[8] Second, Oregon is a subnational jurisdiction, so it faces a greater risk of people crossing state lines to use drugs without fear of imprisonment. A possible third difference is the pre-decriminalization state of substance abuse treatment and prevention in each jurisdiction. It is difficult to compare the status of addiction care in the two locations over time, but I will note that Oregon ranked last in the US in 2020 for its access to drug and alcohol treatment according to data from the US Substance Abuse and Mental Health Services Administration (Green, 2022b).[9] Thus, my paper is unique in that it estimates how decriminalization affects drug overdose deaths in a subnational jurisdiction with relatively poor access to substance abuse treatment.

My paper contributes more broadly to three literatures. First, it contributes to a literature studying how a product's legality influences its demand. Dills and Miron (2004) find that constitutional alcohol prohibition in the United States reduced cirrhosis, a proxy for alcohol consumption, by 10-20%. Williams and Bretteville-Jensen (2014) find that recreational cannabis decriminalization causes a small increase in the prevalence of cannabis use in the first five years following decriminalization. My results generally align with these two studies in finding that a product's criminalization acts as a deterrent on a proxy for its consumption.

Second, it contributes to literature studying the impacts of reducing the severity of punishments for criminal offences. Within this literature, Collins et al. (2017) find that participants in jail diversion program for persons suspected of low-level drug and prostitution offenses are much less likely to be arrested again in the future. Arora and Bencsik (2021) find that a narcotics arrest diversion program increases substance use treatment uptake and reduces subsequent arrests. Agan et al. (2021) find that nonprosecution of a nonviolent misdemeanor offense leads to large reductions in future criminality. Mueller-Smith and T. Schnepel (2021) find that diversion in the criminal justice system cuts reoffending rates in half and grows quarterly employment rates by nearly 50% over 10 years. My results differ somewhat from those found in this literature in that I find harmful impacts of punishment severity reduction.

Third, my paper contributes to literature studying drivers of drug overdose deaths and the importance of substance use treatment. Maclean et al.

(2020) review studies researching the causes of the American opioid crsis and the effectiveness of various policy responses. They note that poor labour market conditions are associated with increased opioid use and also that healthcare providers and the pharmaceutical industry played a large role in creating the crisis. They also discuss literature showing that stricter enforcement of illicit drug prohibitions and more liberal naloxone access laws have been found to have mixed impacts on substance misuse. Regarding the importance of substance use treatment, Swensen (2015) finds that a 10% increase in substance use treatment facilities lowers a county's drug-induced mortality rate by 2%, while Bondurant et al. (2018) find that the presence of substance use treatment facilities in an area lowers crime in that area. My findings generally align with this literature in suggesting the important role of substance use treatment facilities.

A limitation of my paper is its inability to empirically pin down a mechanism explaining precisely *why* decriminalization caused more drug overdose deaths in Oregon. Decriminalization could conceivably affect drug overdoses through a few channels. First, by removing the threat of punishment, decriminalization lowers the perceived cost of drug use and thus may increase drug use at both the extensive and intensive margins. Second, decriminalization may affect drug users' treatment utilization, which research suggests is effective in decreasing drug use (Swensen, 2015) (Arora and Bencsik, 2021). The effect of decriminalization on treatment utilization is theoretically ambiguous. On one hand, decriminalization may make drug users feel more comfortable seeking out treatment. On the other hand, decriminalization may reduce treatment uptake given that a) the threat of jail time may encourage drug users to seek treatment; and b) law enforcement officers connect many more individuals to substance abuse treatment services than other government actors (Arora and Bencsik, 2021). The latter argument has been advanced by the executive director of an Oregon addiction recovery non-profit who has been critical of Oregon's decriminalization approach (Beaumont, 2022):

> *"At least through the criminal justice system, someone had a pathway to recovery, as flawed as that was, and as stigmatising as that was. There's a whole bunch of people who are no longer given a pathway to recovery if they choose it."*

My paper is, unfortunately, not able to evaluate which of these mechanisms are contributing to my main result. However, mechanism evaluation may be possible by late 2023, once the U.S.'s Substance Abuse and Mental Health Services Administration releases 2021 versions of their NSDUH, TEDS-A, and N-SUMHSS datasets. These datasets will provide the information necessary to assess how Oregon's decriminalization affected substance use rates, substance abuse treatment utilization, and substance abuse treatment availability. This will in turn provide evidence to assess the various hypotheses described above.

2 Empirical Strategy

I use the synthetic control method developed in Abadie and Gardeazabal (2003) and Abadie et al. (2010) to study the impact of Measure 110 on drug overdose deaths (per 100,000 residents) in Oregon. I construct a "synthetic" Oregon using drug overdose deaths per 100,000 data from 48 other states[10] and the District of Columbia. I then estimate the treatment effect of Measure 110 by finding the difference between post-treatment drug overdose deaths per 100,000 in Oregon and in "synthetic Oregon".

I implement the standard synthetic control method in Stata with the "synth2" command created by Yan and Chen (2022). Specifically, I declare a panel dataset with state as the panel variable and month as the time variable. I use drug overdose deaths per 100,000 as my main dependent variable. I use drug overdose deaths per 100,000 in each month in my sample before decriminalization as my independent variables.

My empirical strategy makes three important assumptions. First, I assume that decriminalization does not impact drug overdose deaths before decriminalization is implemented. This assumption may be questionable in my application due to anticipation effects, since Measure 110 became effective on February 1, 2021, but was passed on November 3, 2020. As a robustness check in Section 6.2, I redefine the intervention period to be November 2020, the first period in which the outcome could possibly react to the intervention, and find that this modification does not meaningfully affect my results. Second, I make the "no interference between units" assumption discussed by Abadie et al. (2010); that is, I assume that drug overdose deaths in states that did not decriminalize drugs are not affected by Oregon's decriminalization. I discuss the validity of this assumption further in Section 4. Third, I assume that the implementation of Measure 101 is the only change in Oregon in early 2021 that affected drug overdose deaths. I discuss the validity of this assumption further in Section 6.3.

3 Data

To estimate the model, I use state-level monthly drug overdose death data from 2018-2021 from the CDC's publicly-available PMCDD. The PMCDD contains mortality counts for all U.S. states. Data are based on death certificates for U.S. residents. Each death certificate contains a single underlying cause of death, up to twenty additional multiple causes, and demographic data. I extract monthly death counts where the underlying cause of death was listed as "Drug poisonings (overdose) Unintentional". I convert these monthly death counts to monthly death rates per 100,000 state residents using annual state population data from the same PMCDD dataset.

Though some data from 2022 was available at the time of my analysis, I excluded this data because the CDC notes that fully accurate drug overdose death data are unavailable for a 6 month lag in order to account for delays in death certificate completion. Note that the CDC suppresses death counts for state-month pairs that have 9 or fewer drug overdose deaths due to confidentiality concerns; in the few instances where this occurs in my data, I randomly impute an integer between 0 and 9. This is only an issue in a few instances in the data on total drug overdose deaths per 100,000, but does come up more frequently in the data that breaks down these death rates by sex or race. I thus present my estimates without imputed data in the Appendix A. The results do not change meaningfully once states with imputed data are removed.

4 Results

Figure 1 displays monthly drug overdose deaths per 100,000 for Oregon and its synthetic counterpart from 2018-2021. Drug overdose deaths per 100,000 in the synthetic Oregon reasonably approximate the trajectory of this variable in Oregon over the pre-Measure 110 period. This suggests that the synthetic Oregon provides a sensible approximation for the number of drug overdose deaths per 100,000 that would have occurred in Oregon from February 2021 to December 2021 in the absence of Measure 110.

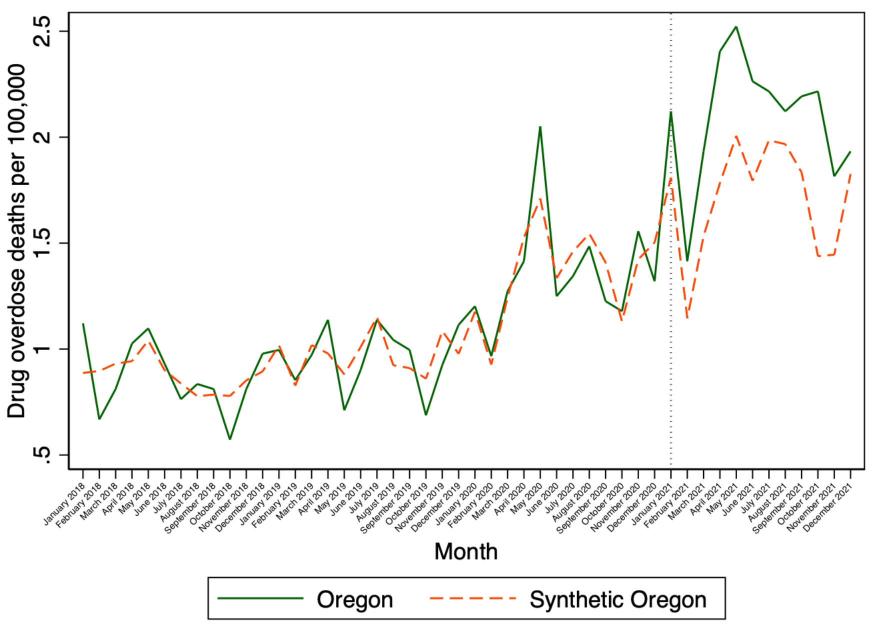
My estimate of the effect of Measure 110 on drug overdose deaths per 100,000 in Oregon is the difference between drug overdose deaths per 100,000 in Oregon and in "synthetic Oregon" after the passage of Measure 110. Figure 1 shows that while drug overdose deaths per 100,000 in the synthetic Oregon levelled off after February 2021, they sharply increased in the real Oregon. The discrepancy between the two lines suggests that Measure 110 had a large positive effect on drug overdose deaths. Indeed, Oregon had 977 drug overdose deaths between February 2021 and December 2021, whereas I predict that synthetic Oregon would have only had 796 drug overdose deaths in that same period. Thus, Measure 110 caused 181 additional drug overdose deaths, an increase of 23% over what would have otherwise been expected.

I investigate whether this increase had a disproportionate impact on men vs. women or on white people vs. non-white people using the same approach described above. I do not find evidence of substantial heterogeneity. Females make up approximately 30% of the overdose deaths in both the real and synthetic Oregon post-decriminalization, while non-white people make up approximately 5% of overdose deaths in both cases. I discuss these results in more detail in Appendix A.

This analysis crucially relies on the "no interference" assumption, the assumption that drug overdose deaths outside of Oregon are not affected by Oregon's decriminalization. The most obvious way this assumption could be violated is if people travel from other states into Oregon in order to consume drugs without fear of severe legal consequence. If this were the case, drug overdose deaths in donor states would decrease as a result of Measure 110. To test whether this phenomenon is occurring, I investigate whether decriminalization increased drug overdose deaths occurring in Oregon by much more than decriminalization increased drug overdose deaths among residents of Oregon. Whereas decriminalization caused 181 additional deaths among Oregon residents, it caused 196 additional deaths occurring in Oregon; that is, I find little evidence to support the hypothesis that a large number of people who would have used drugs elsewhere came to Oregon to do so. Furthermore, the only states used to construct synthetic

Oregon that are geographically close to Oregon are Colorado, Montana, South Dakota, Alaska, and Wyoming[11] and I show in Section 6.1 that the results are impervious to the exclusion of each of these states from the donor pool.

Figure 1: Trends in drug overdose deaths per 100,000 – Oregon vs. synthetic Oregon



5 Inference

5.1 Placebo Studies in Space

To evaluate the significance of my estimates, I follow Abadie et al. (2010) and check whether my results could be driven entirely by chance. Specifically, I run 50 iterations of the synthetic control method where, in each iteration, I pretend that a different state decriminalized drugs in February 2021. I then compute the estimated treatment effect associated with each iteration. Intuitively, if I find that the placebo decriminalization also "caused" large increases in drug overdose deaths in other states, I will not be able to conclude whether the real decriminalization in Oregon had any effect.

Figure 2a displays the results for this placebo test after excluding states that had a pre-Measure 110 mean squared prediction error (MSPE) of more than 2 times the MSPE of Oregon. I exclude these states from this figure because placebo runs with poor fit prior to the implementation of Measure 110 are not useful in measuring the relative rarity of estimating a large post-Measure 110 gap for a state, like Oregon, that was well fitted prior to Measure 110 (Abadie et al., 2010). The gray lines in Figure 2a represent the difference in drug overdose deaths per 100,000 between each remaining state in the donor pool and its respective synthetic version. The superimposed green line denotes this gap estimated for Oregon. Among the states remaining in the figure, the Oregon gap line is about the most consistently unusual.

To quantitatively evaluate the Oregon gap relative to the gaps obtained from the placebo runs, I look at the distribution of the post/pre-Measure

110 MSPE ratios in Figure 2b. Oregon's ratio clearly stands out as the largest among all the ratios I calculate. This indicates that if one were to assign the intervention at random in the data, the probability of obtaining a post/pre-Measure 110 MSPE ratio as large as Oregon's is 1/50 = 0.020.

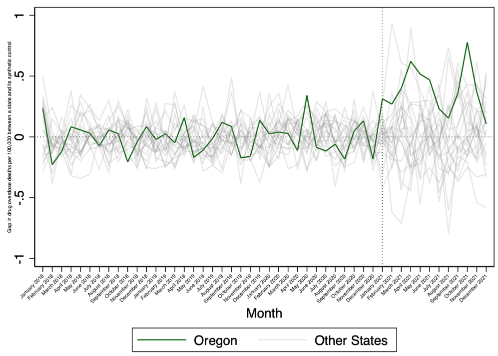
5.2 Placebo Studies in Time

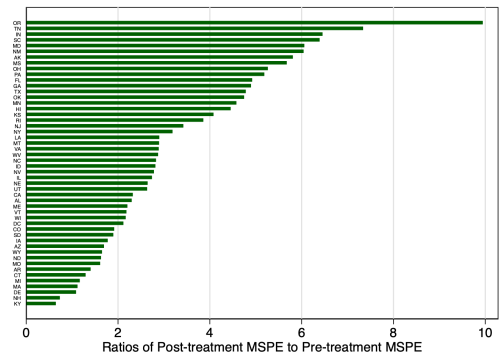
An alternative approach for evaluating the credibility of my results, as demonstrated by Abadie et al. (2015), is to conduct placebo studies where the treatment of interest is reassigned in the data to a month other than February 2021. Intuitively, if I reassign the treatment month to a month far before decriminalization was actually implemented, I should not expect to see a similarly large and positive treatment effect.

To conduct this placebo study, I reassign decriminalization to the middle of the pre-treatment period[12], August 2019, and rerun the synthetic control model used above. Figure 2c displays the results of this "in-time placebo" study. The synthetic Oregon now closely reproduces the trend in drug overdose deaths in the actual Oregon for the pre-"treatment" period *and* in the post-"treatment" period, with the exception of one deviation in May 2020 and slightly higher levels in Oregon after decriminalization was announced, but not implemented, in November 2020.[13]

In contrast to the actual February 2021 decriminalization, my August 2019 placebo decriminalization has a small and statistically insignificant effect. The estimated placebo treatment effect here is an additional 0.14 drug overdose deaths per 100,000 per month, whereas my estimated actual

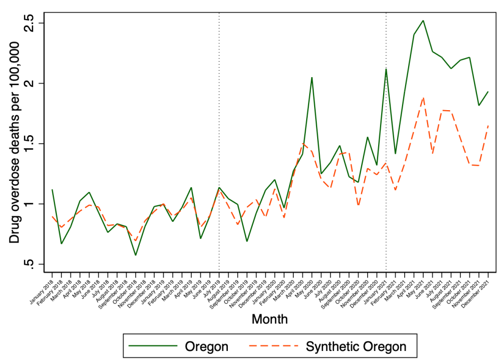
Figure 2: Placebo Studies





(a) Drug overdose deaths per 100,000 gaps in Oregon and placebo gaps in control states with pre-Measure 110 MSPE less than two times higher than Oregon's

(b) Ratios of post-treatment MSPE to pre-treatment MSPE



(c) Placebo decriminalization August 2019 – Trends in drug overdose deaths per 100,000 in Oregon vs. synthetic Oregon treatment effect was 0.39 additional drug overdose deaths per 100,000 per month. The p-value generated by the in-space placebo test here is large at 0.24. Furthermore, if I remove data from after decriminalization was announced, my estimated placebo effect drops to 0.09 additional deaths per 100,000 per month with a p-value of 0.32.

Overall, the results of this placebo test suggest that the gap estimated in Figure 1 reflects the impact of decriminalization and not a potential lack of predictive power of the synthetic control.

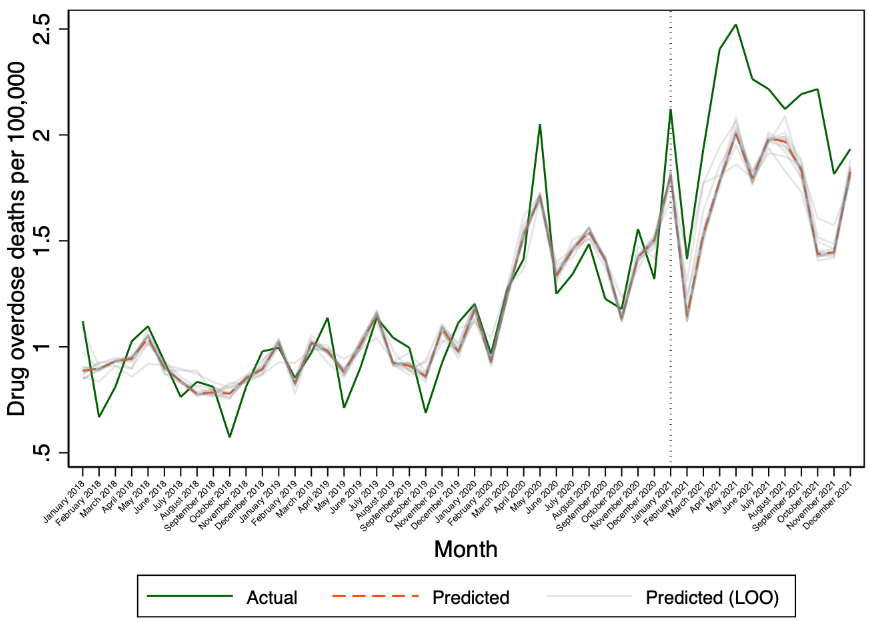
6 Robustness Checks

6.1 Leave-One-Out Robustness Test

To test the sensitivity of my main results to changes in the state weights, I run the "leave-one-out" robustness check described in Abadie et al. (2015). This robustness test involves iteratively re-estimating the baseline model to construct several different versions of the synthetic Oregon. Each version is unique in that it omits data from a different state used to generate the synthetic control in the baseline model.

Figure 3 displays the results of this test. It reproduces Figure 1 (solid green and dashed orange lines) while also incorporating each of the "synthetic Oregons" created by the leave-one-out procedure (gray lines). This figure shows that the results of the previous analysis are robust to the exclusion of any particular state from my sample of control states. The minimum treatment effect generated by this test indicates that decriminalization resulted in an additional 124 drug overdose deaths in Oregon in 2021. The maximum treatment effect generated by this test indicates that decriminalization resulted in an additional 210 drug overdose deaths in Oregon in 2021.

Figure 3: Trends in drug overdose deaths – Oregon vs. baseline synthetic Oregon vs. leave-one-out synthetic Oregons



6.2 Including the Anticipatory Period

I run an additional robustness check to test whether my results are sensitive to redefining the intervention as the passage of Measure 110, rather than its implementation. (Measure 110 passed on November 3, 2020, but was not actually effective until February 1, 2021.) Using the same synthetic control approach as above, but redefining the treatment month as November 2020, I estimate an average monthly treatment effect of 0.43 additional drug overdose deaths per 100,000, which is quite similar to my estimated monthly average treatment effect of 0.39 additional drug overdose deaths per 100,000 when I do not include the anticipatory period as a period of treatment. After running the same in-space placebo test described in Section 5.1, the probability of obtaining a post/pre-Measure 110 MSPE ratio as large as Oregon's is 0.04 here. The average monthly treatment effect during the three anticipatory months is 0.05, which is much lower than the average monthly treatment effect during the post-implementation period, as would be expected.

6.3 Ruling Out Coinciding Policy Changes

Inferring that the increase in drug overdose deaths in Oregon after February 2021 was due to decriminalization requires the assumption that there was no other major policy change in Oregon around the same time that could have affected drug overdose deaths. A local news article from December 30, 2020 lists several new laws that went into effect in Oregon in early 2021 (KGW Staff, 2020). Of these laws, only a minimum wage increase and a cigarette tax increase could have conceivably had a substantial effect on drug overdose deaths. The minimum wage increase did not take effect until July 1, 2021 – five months after decriminalization. My estimated average monthly treatment effect from February 2021 through June 2021 is 0.46 additional overdose deaths, which is similar to, albeit slightly higher than, my estimate for the entire post-treatment period, so it is unlikely that there is a major issue here. Regarding the cigarette tax increase, I test in Appendix C whether similar tax increases in other states impact drug overdose deaths and find no evidence of such an effect.

6.4 Similar Results in Washington

On February 25, 2021, the Washington Supreme Court struck down the state's felony drug possession law in a "bombshell" decision that came as a "shock" to state policymakers (Mikkelsen, 2021; Decker, 2021). A local news article by Mikkelsen (2021) reported:

> *"The ruling came in the case of a Spokane woman who had received a pair of jeans from a friend that had a small bag of methamphetamine in a pocket. Five justices said in the decision Thursday that the state law was unconstitutional because it criminalized her passive, unknowing conduct, in violation of her due process protections."*

The ruling effectively decriminalized the possession of small amounts of drugs and Washington police departments stopped making arrests for simple possession immediately. A new possession law was not enacted until

May 14, 2021. The new law made drug possession a misdemeanor instead of a felony. It further required police to divert a defendant's first two offenses to treatment before the case made it to a prosecutor, who could then also choose to divert the case to treatment (La Corte and Johnson, 2021).

Given that Washington liberalized their drug policy a month after neighbouring Oregon, it would undermine my estimated treatment effects if Washington did not see an increase in drug overdose deaths in this period of less punitive drug laws. However, I find that in the 10 months following Washington's initial decriminalization, there was an additional 135 drug overdose deaths per 100,000 in Washington compared to its synthetic counterpart. Figure 4 shows these results. There were 1,811 actual drug overdose deaths in the 10 months following decriminalization in Washington compared to 1,676 drug overdose deaths in synthetic Washington, meaning that decriminalization caused an 8% increase in actual drug overdose deaths per 100,000 over what would have been otherwise predicted. I apply the "in-space" placebo test here and find that there is only a 2% chance of obtaining a post/pre-decriminalization MSPE ratio as large as Washington's. When I only consider the 2 months following Washington's decriminalization before drug possession was made a misdemeanor, the treatment effect decreases from 0.13 additional drug overdose deaths per 100,000 per month to 0.07, though this estimate is very noisy.[14] The smaller, but still positive effects observed in Washington aligns with the fact that drugs were not fully decriminalized there for an extended period of time.

Figure 4: Trends in drug overdose deaths – Washington vs. synthetic Washington

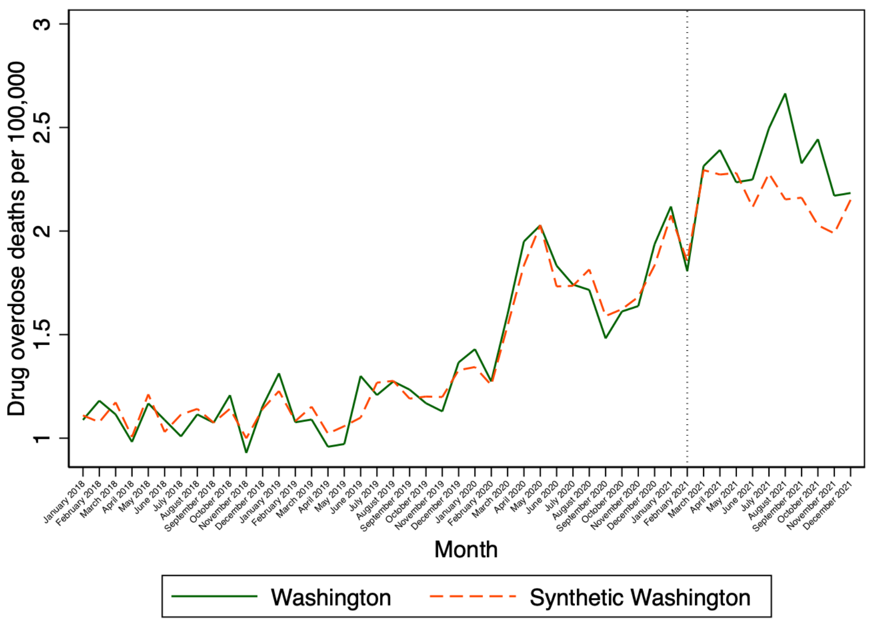


Table 1: The Impacts of Drug Policy Liberalization on Monthly Drug Overdose Deaths per 100,000

	Oregon	Washington	Both states
All drug overdose deaths	0.235***	0.191***	0.213***
	(0.069)	(0.070)	(0.071)
	{0.251}	{0.870}	
Male drug overdose deaths	0.364***	0.297***	0.331***
	(0.106)	(0.110)	(0.110)
	{0.091}	{0.817}	
Female drug overdose deaths	0.090*	0.055	0.073
	(0.046)	(0.048)	(0.050)
	{0.878}	{0.209}	
White drug overdose deaths	0.355***	0.209***	0.284***
	(0.073)	(0.074)	(0.090)
	{0.809}	{0.447}	
Non-white drug overdose deaths	-0.276**	0.181	-0.055
	(0.120)	(0.114)	(0.200)
	{0.174}	{0.710}	

[a] Table description: This table shows the results I obtain from using a difference-in-differences approach to estimate the impacts of Oregon's decriminalization, Washington's liberalization-then-defelonization, and the two policy regime changes considered together. The dependent variable of each regression is listed in the first column in monthly and per 100,000 terms.

[b] Numbers in round brackets are standard errors clustered at the state-level.

[c] * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[d] Numbers in curly braces are p-values from a parallel trends test in the pre-treatment period, where the null hypothesis is that the trends are parallel. I conduct this test using the "estat ptrends" command in Stata following regression estimation.

6.5 Difference-in-Differences Analysis

This section presents estimates of the impact of Oregon's decriminalization and Washington's liberalization on drug overdose deaths in these states obtained by estimating a difference-in-differences model. Table 1 presents the estimated impacts of drug policy liberalization on drug overdose deaths per 100,000 residents in Oregon, Washington, and both states together.

The table again shows evidence that decriminalization has large positive effects on drug overdose deaths. The estimated effects are smaller for Oregon and larger for Washington using the difference-in-differences approach compared to the synthetic control method, but the overall "flavour" of the results is the same.

7 Discussion

Comments by Oregonians working in substance abuse treatment and prevention, advocacy, and policy reinforce my empirical finding that decriminalization, without significant improvements to treatment accessibility, did not solve Oregon's substance abuse issues. In June 2022 (Benner, 2022), Oregon's Secretary of State concluded:

> *"When the voters of Oregon passed Measure 110, we did so because we wanted to change a policy in Oregon to improve the lives of people, to improve our communities ... and in the years since, we haven't seen that play out."*

Why didn't decriminalization in Oregon decrease drug overdose deaths like it did in Portugal twenty years earlier? As discussed in the introduction, one key difference between Oregon and Portugal that could have played a role is that decriminalization in Portugal was accompanied by significant investments in substance abuse treatment, prevention, and harm reduction, while the same was not true in Oregon. The hold-up of funds intended for these services and the general lack of accessible treatment for people with substance abuse issues has been an oft-cited reason for the negative effects of decriminalization in Oregon. The founder of a harm reduction provider put it bluntly (Green, 2022b):

> *"The important context is that 90% of the funds are currently tied up in bureaucracy, and we are out here on the ground, watching our friends die."*

A June 2021 NPR article reported that "many recovery leaders [in Oregon] who support ending the criminalization of addiction are deeply concerned the state basically jumped off the decriminalization cliff toward a fractured, dysfunctional and underfunded treatment system that's not at all ready to handle an influx of more people seeking treatment." (Westervelt, 2021) The director of an Oregon addiction recovery advocacy group is quoted as saying:

> *"Our big problem is our health care system doesn't want it, is not prepared for it, doesn't have the resources for it and honestly doesn't have the leadership to begin to incorporate that [expanded treatment]."*

An unnamed official involved in implementing Measure 110 voiced similar concerns:

> *"I really hope we don't spend the next 10 to 12 months with open air drug markets and nowhere to send [those seeking help]."*

This anecdotal evidence suggests that the lack of access to appropriate treatment is a potentially important factor in explaining why Oregon's decriminalization results have been so different than Portugal's. It is also worth noting here that the similar effects I observed in Washington similarly reflected a context in which decriminalization was not accompanied by significantly improved access to healthcare (as in this case, decriminalization was not even implemented intentionally). In 2020, Oregon had the highest proportion of people needing but not receiving substance abuse treatment, while Washington had the third-highest (Substance Abuse and Mental Health Services Administration, 2021).

There are other differences between 2001 Portugal and 2021 Oregon, which make it difficult to say definitively that lack of access to treatment is the primary driver of the disparity in outcomes. Very powerful and deadly synthetic opioids are far more common today than they were twenty years ago, so there is now a greater risk that a person using drugs overdoses before obtaining sufficient treatment. Another hypothesis is that some of the additional drug deaths in Oregon post-decriminalization are people coming in from other states to use drugs without fear of severe legal punishment. Portugal would have been less likely to have people entering the country to use drugs given their national, rather than subnational, boundaries. I attempt to test this hypothesis by using the synthetic control method to determine whether drug overdose deaths occurring in Oregon increased much more than drug overdose deaths among residents of Oregon due to decriminalization, but do not find a substantial difference.

Considering the research on decriminalization in Portugal, my finding that decriminalization in Oregon caused a 23% increase in drug overdose deaths highlights the importance of context in studying the impacts of drug decriminalization. As more jurisdictions decriminalize drugs, it will be crucial to assess the effects of these policies in order to better understand how best to design drug policy to minimize drug-related harms.

It will also be crucial for future research to address an important limitation of this study: its inability to pin down a precise mechanism that explains *why* decriminalization led to more drug overdose deaths in Oregon. Did decriminalization cause more people to try drugs for the first time? Did it increase use among people who already used drugs? Did it decrease the incentive to seek treatment? Did it overwhelm underfunded treatment services? Did decreased contact with law enforcement decrease awareness and/or uptake of available treatment? This paper cannot answer these questions, but they are vital for understanding how to best direct treatment and prevention efforts. Evaluating these hypotheses may be possible by late 2023, once the U.S.'s Substance Abuse and Mental Health Services Administration releases 2021 versions of their NSDUH, TEDS-A, and N-SUMHSS datasets, which provide annual state-level data on substance use rates, substance abuse treatment utilization, and substance abuse treatment availability.

While my research identifies the potentially harmful effects of decriminalization without increased treatment accessibility, it is important to keep in mind that decriminalization is not only intended to improve lives by reducing substance abuse. It is also intended to improve lives by reducing arrests. I present some evidence that decriminalization substantially reduced the number of Oregonians arrested for drug possession in Appendix B, although these synthetic-control-method estimates are not very precise.[15] Thus, Oregon's decriminalization may have still had a net positive impact on the many Oregonians who would have otherwise been arrested.

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A More Detailed Results

This section presents the main results referred to in the paper in greater depth and detail. Table 2 shows the estimated impacts of decriminalization on various outcome variables according to several different model or data specifications. Column 1 presents the estimated impacts of Oregon's decriminalization using imputed data in the few instances where data is suppressed. Column 2 presents the estimated impacts of Oregon's decriminalization after removing states with any suppressed data from the dataset. Column 3 presents the estimated impacts of Oregon's decriminalization when it is assumed that "treatment" started when decriminalization was announced in November 2020, rather than when it was implemented in February 2021 (as discussed in Section 6.2). Note that the dataset used for this estimate includes imputed data. Column 4 presents the estimated impacts of Washington's liberalization using imputed data in the few instances where data is suppressed. Column 5 presents the estimated impacts of Washington's liberalization after removing states with any suppressed data from the dataset. The estimates presented here are the change in monthly drug overdose deaths per 100,000 that decriminalization/liberalization caused when comparing actual outcomes to outcomes "observed" in the relevant synthetic control. The bracketed figures below these estimates are the p-values obtained by running the "in-space" placebo test described in Section 5.1. Figures similar to those shown in the paper are available for each of these estimates upon request.

In the paper, I refer to various drug overdose death counts estimated according to the estimated treatment effects shown in Table 2. For instance, I state that I estimate that decriminalization caused an additional 181 drug overdose deaths in Oregon. To arrive at this figure, I multiply the average monthly treatment effect shown in Row 1 of Column 1 (0.39) by the 11 post-treatment months I observe, multiply this number by Oregon's 2021 population (4,241,507 according to the CDC data), and divide by 100,000. Furthermore, noting that there were 977 drug overdose deaths actually observed in Oregon in 2021, I calculate that there were $977 - 181 = 796$ drug overdose deaths in the synthetic Oregon in 2021.

I use a similar approach to help understand treatment effect heterogeneity by sex and race. For men, given that there were 2,102,461 men in Oregon in 2021 and 700 drug overdose deaths among men in 2021, I calculate that the synthetic Oregon would have had 578 male drug overdose deaths. I similarly calculate that synthetic Oregon would have had 223 female drug overdose deaths. In the real Oregon, males made up 700/977 = 72% of drug overdose deaths, whereas females made up 277/977 = 28%. In the synthetic Oregon, men made up 578/796 = 72% of drug overdose deaths, whereas females made up 223/796 = 28% of drug overdose deaths. Thus, I conclude that there is no evidence of treatment effect heterogeneity by sex. (Note that the synthetic control proportions do not exactly add up to 100%. This is due to imprecision in the estimation of monthly treatment effects.) When I conduct the same analysis by race, I find that non-white people make up about 4% of drug overdose deaths in both the synthetic and actual Oregon, whereas white people make up 96% of drug overdose deaths in actual Oregon and 90%

of drug overdose deaths in synthetic Oregon. (Again, the synthetic Oregon proportions do not add up to 100% due to imprecision in the estimated monthly treatment effects.)

The final row in the table shows the impact of drug decriminalization/liberalization on the number of monthly drug overdose deaths per 100,000 that occur in a state, rather than the number of monthly drug overdose deaths per 100,000 among state residents. These estimated impacts are quite similar, suggesting that decriminalization and liberalization did not cause many out-of-state residents to cross into Oregon or Washington to use drugs with less fear of punishment.

B The Impact of Decriminalization and Liberalization on Drug Possession Arrests in Oregon and Washington

This section presents estimates of the impact of Oregon's decriminalization and Washington's liberalization on drug possession arrests in these states. To estimate this relationship, I scrape annual 2016-2021 state-level arrest data from the Federal Bureau of Investigation's Crime Data Explorer[16] and use both the synthetic control method ("SCM") and difference-in-differences ("DiD") approach described above. Table 3 presents the estimated impacts of the policy changes on annual drug possession arrests per 100,000 residents in Oregon, Washington, and the two states combined.

The evidence in Table 3 suggests that Oregon's decriminalization and Washington's liberalization may have substantially decreased drug possession arrests, but my estimates here are imprecise. Future work could use less aggregated data (i.e. county-level and/or monthly data) from the National Incident-Based Reporting System to try to obtain more precise estimates.[17]

C The Effect of Cigarette Tax Increases on Drug Overdose Deaths

On January 1, 2021, Oregon's cigarette tax increased by $2 per pack of 20 and $2.50 per pack of 25. This tax increase could, in theory, increase drug consumption if some consumers view cigarettes and drugs as substitutes. Though cigarettes are a textbook example of a price inelastic product, the $2 tax increase per pack of 20 was a sizable increase over the previous $1.33 tax per pack. Thus, I check that a cigarette tax increase would be unlikely to affect drug overdose deaths by studying the impact of cigarette tax increases in other states on drug overdose deaths, again using the synthetic control method.

I study eight other state-level cigarette tax increases that have occurred since 2018: a $0.50/pack tax increase in Kentucky in July 2018; a $1.00/pack tax increase in Oklahoma in July 2018; a $2.50/pack tax increase in DC in October 2018; a $1.00/pack tax increase in Illinois in July 2019; a $0.34/pack tax increase in New Mexico in July 2019; a $0.30/pack tax increase in Virginia in July 2020; a $1.10/pack tax increase in Colorado in January 2021; and a

$1.75/pack tax increase in Maryland in March 2021.[18] I use the synthetic control method described above, excluding all other states with cigarette tax increases from each analysis, as well as Oregon and Washington.

Table 4 shows the estimated monthly treatment effect for each of the eight trials, as well as the p-value from the associated "in-space" placebo test. The estimated treatment effects range from large and negative to large and positive and the p-values indicate that one cannot rule out that these treatment effects occurred by chance. Thus, I find no evidence that cigarette tax increases have a consistent effect on drug overdose deaths, which would otherwise obscure my inference.

Notes

[1]A 2011 report by the US Department of Justice's National Drug Intelligence Center estimated that, in 2007, illicit drug use cost the US economy $72 billion in lost productivity not due to incarceration, $48 billion in lost productivity due to incarceration, and $61 billion in criminal justice costs. Note that these estimates included costs from cannabis consumption (U.S. Department of Justice, 2011).

[2]More specifically, Oregon decriminalized possession of less than 40 user units of LSD; less than 12 grams of psilocybin and psilocin; less than 40 user units of methadone; less than 40 pills, tablets, or capsules of oxycodone; less than 1 gram of heroin; less than 1 gram of MDMA; less than 2 grams of cocaine; and less than 2 grams of methamphetamine, among other substances (Oregon Legislative Policy and Research Office, 2020).

[3]I calculate these numbers using data from the Federal Bureau of Investigation's Crime Data Explorer: https://crime-data-explorer.app.cloud.gov/pages/explorer/crime/arrest.

[4]The data is publicly available at https://wonder.cdc.gov/mcd-icd10-provisional.html

[5]I explain this exclusion in Section 6.4.

[6]I follow Abadie et al. (2015) and use the halfway point of the pre-treatment period as the fake treatment month.

[7]Anecdotally, the architect of Portugal's drug strategy, Dr. João Goulão, believes it is "very difficult to identify a causal link between decriminalization by itself and the positive tendencies [Portugal has] seen" and believes that Portugal's positive results are instead due to the "total package" of policies and programs Portugal implemented (Hawkes, 2011).

[8]The CDC claims that fentanyl can be up to 50 times stronger than heroin (Centers for Disease Control and Prevention, 2022).

[9]Specifically, Oregon had the highest percentage of people aged 12+ "needing but not receiving treatment at a specialty facility for illicit drug use in the past year" (Substance Abuse and Mental Health Services Administration, 2021).

[10]Washington also decriminalized drugs in February 2021, so it is not a valid control state. I will discuss Washington's decriminalization in greater detail in Section 6.4.

[11]The states used in the weighted average that constructs "synthetic Oregon" are Maryland (weight = 0.281), Kansas (0.214), Montana (0.176), Colorado (0.082), Iowa (0.058), North Carolina (0.046), South Dakota (0.033), District of Columbia (0.033), Alaska (0.025), Vermont (0.023), Wyoming (0.022), and Mississippi (0.008).

[12]This is the approach used by Abadie et al. (2015).

[13] I provide treatment effect estimates where this anticipatory period is included in the treatment in Section 6.2.

[14]The p-value generated from the in-space placebo test here is 0.66.

[15]While the synthetic control version of these estimates are imprecise, the difference-in-differences estimates do provide large and precise negative impacts.

[16]This data is available at https://crime-data-explorer.fr.cloud.gov/pages/explorer/crime/arrest.

[17]This data is available from https://crime-data-explorer.app.cloud.gov/pages/downloads.

[18]I compiled this list of tax increases using information from the Federation of Tax Administrators: https://www.taxadmin.org/cigarette-tax-increases.

Table 2: The Impacts of Decriminalization/Liberalization on Monthly Drug Overdose Deaths per 100,000

	(1)	(2)	(3)	(4)	(5)
Drug overdose deaths	0.39**	0.34**	0.43**	0.18**	0.19**
	(0.02)	(0.05)	(0.04)	(0.02)	(0.02)
Male drug overdose deaths	0.53**	0.56*	0.70**	0.30*	0.29
	(0.02)	(0.05)	(0.02)	(0.10)	(0.16)
Female drug overdose deaths	0.23*	0.22	0.20	0.14	0.17
	(0.10)	(0.14)	(0.30)	(0.50)	(0.65)
White drug overdose deaths	0.55**	0.53**	0.58**	0.24**	0.21*
	(0.02)	(0.03)	(0.02)	(0.02)	(0.10)
Non-white drug overdose deaths	0.12	0.17	0.28	0.14	0.10
	(0.24)	(0.25)	(0.26)	(0.40)	(0.50)
Drug overdose death occurrences	0.42**	0.38**	0.44*	0.18**	0.17
	(0.02)	(0.02)	(0.06)	(0.02)	(0.17)

[a] Table description: This table shows the results I obtain from using the synthetic control method to estimate the impacts of Oregon's decriminalization and Washington's liberalization-then-defelonization. The dependent variable of each regression is listed in the first column in monthly and per 100,000 terms. Column 1 presents the estimated impacts of Oregon's decriminalization using imputed data in the few instances where data is suppressed. Column 2 presents the estimated impacts of Oregon's decriminalization after removing states with any suppressed data from the dataset. Column 3 presents the estimated impacts of Oregon's decriminalization when it is assumed that "treatment" started when decriminalization was announced in November 2020, rather than when it was implemented in February 2021 (as discussed in Section 6.2). Note that the dataset used for this estimate includes imputed data. Column 4 presents the estimated impacts of Washington's liberalization using imputed data in the few instances where data is suppressed. Column 5 presents the estimated impacts of Washington's liberalization after removing states with any suppressed data from the dataset. The bracketed figures below these estimates are the p-values obtained by running the "in-space" placebo test described in Section 5.1.

[b] * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: The Impacts of Decriminalization/Liberalization on Annual Drug Possession Arrests per 100,000

	SCM	DiD
Oregon	-157.7	-215.9***
	(0.5)	(37.8)
Washington	-83.7	-114.0***
	(0.5)	(37.8)
Both states together		-165.0***
		(52.7)

[a] Table description: This table shows the results I obtain from using the synthetic control method and difference-in-differences method to estimate the impacts of Oregon's decriminalization and Washington's liberalization-then-defelonization on annual drug possession arrests per 100,000.

[b] * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[c] Numbers in round brackets in the SCM column are p-values from the "in-space" placebo test.

[d] Numbers in round brackets in the DiD column are standard errors clustered at the state-level.

Table 4: Impact of cigarette tax increases on monthly drug overdose deaths per 100,000

Tax Increase	Treatment Effect	p-value
Kentucky, July 2018	0.36	0.79
Oklahoma, July 2018	-0.42	0.21
District of Columbia, October 2018	1.13	0.86
Illinois, July 2019	0.05	0.50
New Mexico, July 2019	0.52	0.55
Virginia, July 2020	0.10	0.60
Colorado, January 2021	0.13	0.57
Maryland, March 2021	-0.09	0.43

Table description: This table reports the treatment effect estimated by applying the synthetic control method described in Section 2 to each of the cigarette tax increase interventions listed in the table. It also presents the probability that these treatment effects could have been driven by random chance. Specifically, I conduct the in-space placebo test outlined in Section 5.1 for each tax increase and report the probability of obtaining a post-/pre-treatment MSPE ratio as large as the ratio of the state with the tax increase.