Did Decriminalization Increase Overdoses

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Abstract

We study the impact of drug decriminalization on overdoses. Two papers have received substantial academic, media, and political attention regarding decriminalization's impacts, reaching opposing conclusions despite using identical methods and near-identical data sources. To this end, we replicate and extend their approaches. We first document that slight differences in outcomes and pre-treatment window create different counterfactual weights, which led to different conclusions regarding the first-year impacts. We next document that with an extended time window, the differences in the methodologies between the two papers do not matter, and we find robust evidence that drug overdose, particularly after the second year, increased significantly by 0.8 per 100,000. Finally, we conduct additional analyses to investigate whether the arrival of the fentanyl crisis on the West Coast is driving the increase in overdoses.

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1 Introduction

The overdose epidemic has been one of the most devastating public health crises in recent history. The United States saw more than 100,000 overdose deaths in each year from 2021-2024. Unintentional overdose mortality is the leading cause of external mortality (Center for Disease Control and Prevention, National Center for Health Statistics, 2022) surpassing transportation accidents ($\sim 47,000$) and firearm discharge-related ($\sim 46,000$).

While the social costs of drug addiction are vast, the criminalization of drug use imposes burdens, including reduced employment opportunities for individuals with criminal records, lower lifetime earnings, and deterrence from seeking emergency care during overdose events (Agan and Starr, 2018, Holzer et al., 2006, Mueller-Smith, Byles et al., 2024). Decriminalization may mitigate these harms, but the net benefit depends on whether decriminalization can reduce or at least not increase drug abuse. If decriminalization increases access and use of drug treatment and safer use of illicit substances, it could have benefits through both reducing overdoses and reducing the consequences of contact with the criminal justice system. In this paper, we estimate the impact of decriminalizing drug possession on one such potential benefit: drug-related overdose mortality.

We study the impact of decriminalization on the fatal overdose rate in the context of Oregon's Measure 110 and Washington's Blake Fix. Enacted in February of 2021, Measure 110 was a voter ballot initiative that brought became a harm reduction policy primarily intended to reduce racial inequities within the incarceration system via decriminalizing the possession of small amounts of drugs (Anthony Johnson et al., 2019). In particular, Measure 110 reduced the penalty for small amounts of drug possession from a Class A Misdemeanor to a Class E Violation carrying a fine of $100. Similarly, Washington also experimented with a similar drug decriminalization law beginning in March 2021, motivated by a court decision.

Our main empirical specification uses the synthetic control method. Synthetic control is a reasonable approach in this context because of its ability to create a counterfactual in case studies with single one treated unit, and also due to the transparency of the design (Abadie, 2021). The weights used to create Synthetic Oregon are a direct result of implementing the model. Joshi et al. (2023) and Spencer (2023) both provide early estimates of the causal impact, and both approaches use synthetic control to form their counterfactuals.

However, despite using seemingly identical approaches and asking the same question, they vary in their conclusions, with one suggesting a statistically insignificant effect and one suggesting a significant increase. Zoorob et al. (2024) use a generalized Synthetic control with controls for fentanyl's arrival to estimate the causal impact of Measure 110 on longer-term overdose-related mortality and find little long-run effects. The goal of this paper is to replicate these findings, explain methodological differences between the two papers, and finally extend the analysis windows to produce longer-term estimates and policy conclusions about the net impacts of decriminalization on overdose mortality.

We extend the methodologies of Spencer (2023) and Joshi et al. (2023) through the end of 2023 and provide updated estimates from our preferred specification. Across all model variations, we find that the implementation of Measure 110 led to an increase of between 0.79 to 0.85 per 100,000 overdose-related deaths per month. This corresponds to approximately 1125 to 1213 additional deaths—an increase of 38 to 46% relative to the baseline mean. Measure 110 was a multifaceted policy that combined decriminalization of drug possession with investments in treatment access and support services for people who use drugs. We also consider whether the primary driver of the observed mortality increase was a rise in fentanyl supply.

We provide several robustness tests to ensure that our estimated effect is not biased from either unobservable factors or design choices in synthetic control estimation. We show results from various extensions of synthetic control, including the augmented synthetic control (Ben-Michael et al., 2021), generalized synthetic control (Xu, 2017), and synthetic difference-in-differences (Arkhangelsky et al., 2021). Results from these alternative specifications are in line with our main results. We also show results from an exercise where we switch the weights of Spencer (2023) and Joshi et al. (2023) while using their respective definitions. While this results in poor pre-treatment matches, the conclusion of an increase in overdose-related mortality remains. To test whether particular donor units are driving our results, we perform a remove-one robustness check.

A particular concern is whether our results reflect the effects of COVID-19 or are driven by broader regional trends in fentanyl supply rather than drug decriminalization. The COVID-related concern stems from potential structural breaks in the donor pool – states that served as good counterfactuals for Oregon before 2020 may no longer be valid comparisons post-pandemic. To address this, we implement a sensitivity

check by restricting the synthetic control match to data through February 2020, allowing us to isolate pre-pandemic trends and test whether the estimated effects are confounded by state-specific COVID-19 impacts on substance use. To assess the possibility of a regional effect, we construct placebo synthetic control estimates for California, Nevada, and Idaho. We evaluate the robustness of our findings across various definitions of overdose mortality – including opioid-related, non-opioid, and non-synthetic deaths – to assess whether Measure 110 disproportionately influenced synthetic opioid outcomes or affected drug use more broadly.

Finally, we replicate Zoorob et al. (2024), whose methodology differs from Spencer (2023) and Joshi et al. (2023) principally by explicitly attempting to control for fentanyl's market saturation through a proxy: the fraction of drug seizures involving fentanyl. They find there is no increase in overdoses after controlling for fentanyl's arrival. We replicate and explore the sensitivity of this approach to a variety of measures of fentanyl saturation.

This paper contributes to two areas of research. First, it adds to the literature on opioid abatement strategies. Traditional approaches often involve state-level policies such as doctor shopping restrictions, targeting pill mills (**?**), and prescription drug monitoring programs (Maclean et al., 2020). In contrast, decriminalization is intended to be a part of a broader harm reduction framework, alongside interventions like syringe exchange programs (SEPs) and expanded access to naloxone. For example, Packham (2022) finds that while SEPs effectively reduce the spread of HIV and HCV, they may unintentionally increase overdose mortality. Compared to these strategies, decriminalization has received relatively little causal analysis, particularly outside the context of Oregon's Measure 110, due to limited quasi-experimental opportunities. The primary evidence to date comes from Portugal's 2001 decriminalization, where Félix et al. (2017) estimate a significant reduction in drug-related deaths following the policy change. Beyond its policy relevance, empirical analysis of decriminalization also tests classical economic theory. Our results speak to the Economic Theory of Illegal Goods proposed by Becker et al. (2004). Becker et al. (2004) argue that public expenditure on apprehension of illegal good suppliers — such as narcotics traffickers — depends on the social value of consuming that illegal good. Under plausible estimates of demand elasticity, they show that criminal penalties for drug use may be welfare-reducing. We contribute to this theoretical framework by examining

one critical welfare-relevant outcome of decriminalization: overdose mortality.

We also contribute to ongoing efforts to verify published results in science more generally and economics more specifically (Hillary and Medaglia, 2020, Camerer et al., 2018, Chang and Li, 2015). We replicate prior work estimating the overdose mortality effects of Measure 110, synthesize the methodological differences, and provide our estimates transparently. In using the synthetic control method, we ensure reproducibility by making the donor weights used to construct our counterfactual explicit and matching on outcome only, following Ferman et al. (2020). We document this approach in both the paper and our replication package. In doing so, we underscore how synthetic control model can help align empirical research with the broader goals of transparency and replicability in applied economics, but also highlight studies with limited time horizons or marginally significant or insignificant effects can reach different conclusions.

The remainder of the paper is structured as follows. Section 2 provides context on Measure 110 as well as Washington's "Blake Fix" decision. Section 3 reviews prior literature estimating the impact of Measure 110 on overdose mortality, as well as other evaluations of the policy's broader effects. Section 4 shows the results of our replication and extensions of Spencer (2023), Joshi et al. (2023), and Zoorob et al. (2024). We conclude that section with robustness tests to ensure that potential threats to causal identification — including COVID-19 and the timing of fentanyl saturation – are not driving our findings. Section 5 concludes.

2 Background

2.1 Oregon's Measure 110

Oregon voters passed Ballot Measure 110 on November 3, 2020.[1] The bill passed by 8.46 percentage points and was enacted on February 1, 2021. The primary component of the policy was the decriminalization of all scheduled narcotics at the user-level of possession. The bill adjusts drug possession violations depending on the categorized amount of drugs an individual is found in possession of. Individuals found with "user amounts" of drugs are subject to a Class E Violation as opposed to a Class A Misdemeanor (Anthony Johnson et al., 2019).[2] This particular Class E violation entails a $100 fine which can be waived if the individual

[1] Measure 110 is also known as the Drug Addiction and Recovery Act.

[2] A class A Misdemeanor is subject to a maximum of up to 364 days of imprisonment and up to $6,250 in fines.https://oregon.public.law/statutes/ors_161.635

calls a helpline that provides information on treatment services. Individuals possessing more than the "user" threshold are subject to a Class A misdemeanor rather than a Class B or C felony [3]. Table 1 lists how the classification changed for each drug type and threshold amount.

Original provisions of the bill did not include fentanyl nor its analogues.[4] Synthetic opioids, such as fentanyl, are the primary driver of overdose deaths nationwide, and have been so since the start of 2016. On July 27, 2023, the state adjusted the violations of fentanyl possession under House Bill 2645 (HB2645). Before the passage of this bill, an individual found in possession of less than one gram of fentanyl would be charged with a Class A Misdemeanor. Fentanyl is lethal at a dose of as little as 2mg. An individual possessing 1g of fentanyl could have 500 user doses of fentanyl and be charged for a misdemeanor before HB2645.

The bill's goal was to shift legal attitudes surrounding addiction (Oregon Health Justice Recovery Alliance, 2021). Supporters of the bill, including the Drug Policy Alliance and Mark Zuckerburg Foundation advocated that substance use disorders (SUDs) should be treated as a disease rather than as a crime, thus an individual with a SUD should receive treatment rather than incarceration. This would alleviate concerns about capacity constraints in jails. Black people who use drugs (PWUD) are also more likely to be incarcerated for drug possession, and Measure 110 would reduce racial inequities within the Oregon incarceration system.

Critics of the bill included law enforcement and legislative opposition (Vote No on Measure 110, 2020). Oregon was already progressive regarding drug decriminalization; those found in possession of drugs were already given opportunities to seek treatment through the legal system via drug courts, for instance. Another concern was that the increase in treatment funding was overstated. Treatment providers were already at capacity and providing more beds required immediate funding that was not provisioned in the bill.[5]

Due to the limited timeframe in which the full decriminalization policy was active, little published research has been conducted.[6] Davis et al. (2023) find a statistically significant decrease in drug possession

[3]Consequences of a Class B (C) felony are up to 10 (5) years imprisonment and a $250,000 ($125,000) fine.

[4]Moving forward, we use "fentanyl" to encompass all fentanyl analogues as well.

[5]The bill does include provisions for funding treatment services, but the process of funding health providers would not begin until more than a year post-Measure 110 enactment.

[6]We acknowledge there is active and ongoing unpublished as-of-yet research, particularly that of researchers who attended Comagine Health's Measure 110 Research Symposium in January of 2024.

arrests following the enactment of Measure 110. They also find a significant increase in "displaced" arrests, providing evidence of a substitution pattern among law enforcement agents. Smiley-McDonald et al. (2023) find evidence of asymmetric information among law enforcement agents and the public regarding the entirety of Measure 110 provisions. Law enforcement officers (LEOs) were unaware of treatment funding provided by Measure 110. LEOs were also hesitant to interact with PWUD after M110's enactment due to perceptions that the policy lacks teeth (Henderson et al., 2023). Decreased interaction with law enforcement agents may be the reason for limited use of the statewide referral hotline (Russoniello et al., 2023). Those found in possession of user amounts of drugs were given a $100 fine or the option to call the statewide telephone hotline to have the fine removed and be referred to treatment services (Russoniello et al., 2023). By the end of 2023, only 577 people had called into the hotline (Gaitán, 2023).

After just three years since the enactment of the bill, Governor Tina Kotek signed House Bill 4002, repealing much of the progress made in the way of decriminalization (Representative Jason Kropf and Senator Kate Lieber, 2024). Advocates of recriminalization cite increasing overdose rates in the state of Oregon; the state itself declared an emergency in June of 2024 to address the fentanyl crisis in Portland (A. B. C. News, 2024). On September 1, 2024, the repeal became effective, making the possession of drugs back as a misdeamenor offense.

2.2 Washington's "Blake Fix"

On February 5, 2021 - just four days after Oregon's decriminalization went into effect - the Washington Supreme Court found the state's possession of a controlled substance statute unconstitutional.[7] In the case of *State v. Blake*, the court ruled that "convictions for Possession of Controlled Substance aka Violation of Uniform Controlled Substances Act (VUCSA), were not lawful," and made it possible for those charged to vacate the conviction (State of Washington, 2021). This ruling decriminalized the possession of scheduled substances until WA lawmakers enacted SB5476 ("Blake Fix"). On May 13, 2021 — with an effective date of 7/25/21 — Washington recriminalized possession but lowered sentencing guidelines for simple possession

[7] Washington's controlled substance statute did not require prosecution to prove that the defendant was aware they possessed such a substance.

(Manka, 2021).[8]

3 Literature Review

3.1 Two Contradicting Early Evidence of Drug Decriminalization on Drug Overdose

Two papers published in 2023 investigate the causal effect of drug decriminalization on overdoses using a synthetic control estimator (Joshi et al., 2023, Spencer, 2023). Both teams estimate the average treatment effect using monthly data on overdoses directly from the CDC multiple cause of death files. Despite nearly identical settings for the analysis, Spencer (2023) finds a statistically significant increase in the overdose rate of approximately 0.39 in the state of Oregon, whereas Joshi et al. (2023) find no statistically significant change in the rate of drug deaths (ATE of 0.268 with a p-value of 0.26). For the state of Washington, Spencer (2023) also finds a statistically significant increase in drug mortality, and Joshi et al. (2023) find an increase, which is only statistically significant at the 10 percent level.

A synthetic control is a linear combination of observations from non-treated (donor) units with weights derived by minimizing the pre-treatment prediction error. Therefore, the synthetic control model is sensitive to the weight and inclusion of donor units. Table 2 provides the resulting weights from each of the authors' analyses. Spencer (2023)'s estimate weighs more heavily Kansas and Maryland, while Joshi et al. (2023) weigh Mississippi and Alaska more heavily. Both synthetic controls rely heavily on Montana as well (0.178 for Spencer (2023), 0.272 for Joshi et al. (2023)). We document that there are several differences in methodologies that drive the difference in these weights and the estimate. Table 3 summarizes the key differences between the two papers, and we discuss these differences in the subsequent paragraphs.

[8]Simple possession is similar to personal use as defined by the Oregon law. SB5476 was effective until mid-2023. In May of 2023, WA Governor Jay Inslee signed SB5536, a bill that increased sentencing on simple possession.

3.1.1 Definitions

Users of the CDC WONDER database can filter underlying causes of death using ICD (International Cause of Death) codes.[9] Spencer (2023) filters by unintentional overdoses (X40-44) whereas Joshi et al. (2023) filter by all overdoses (X60-64, X85, Y10-14, multiple cause of death T36-50).

As categorized by the World Health Organization, unintentional overdoses are external causes of morbidity and mortality. Specifically (X40-X49) is defined as "accidental overdose of drug, wrong drug given or taken in error, and drug taken inadvertently"; "accidents in the use of drugs, medicaments and biological substances in medical and surgical procedures;" and "(self-inflicted) poisoning, when not specified whether accidental or with intent to harm." The subset Spencer (2023) uses, X40-X44, is categorized under "Accidental posioning by and exposure to noxious substances." X60-64 includes intentional self-harm from poisoning by drugs. X85 is classified as homicidal poisoning. Y10-14 covers all drug poisonings with undetermined intent. Finally, T36-50 is classified as "Poisoning by drugs, medicaments and biological substances" and would be included in a multiple cause of death download of the series.[10]

The average difference between total overdoses (nationally) is 692 per month (nationwide). The average using Joshi et al. (2023)'s definition is 7710, and the average using Spencer (2023)'s definition is 7019. The standard deviation is 1539 and 1543, respectively. In the states of Oregon and Washington, the averages using Joshi et al. (2023)'s definition are 89 and 176, respectively. For Spencer (2023)'s definition, the monthly averages are 79 in Oregon and 162 in Washington. Figure 1 plots the national time trend for these different definitions. We note that the majority of drug deaths are due to accidental overdoses (panel a), and these two outcomes follow a similar trend over time (panel b).[11]

There are reasonable arguments for using different outcome measures in each study. Drug decriminalization may affect mortality through a demand-side channel—that is, by increasing the number of users and/or the frequency or dosage of use—rather than through increases in suicide or homicide. As a result, researchers may have more statistical power when focusing specifically on accidental overdoses. However, analyzing all drug-related mortality may help mitigate potential classification or measurement errors, since sometimes the

[9] At this time, the tenth revision of these codes (ICD-10) is being used. Codes can be searched using the ICD-10-CM Browser Tool `https://www.cdc.gov/nchs/icd/icd10cm_browsertool.htm`

[10] Filtering by "multiple cause of deaths" allows the user to subset even overdoses into a specific drug used.

[11] In Appendix Figure 1, we present the same figure, but focusing on Oregon and Washington.

data can misclassify unintentional overdoses as suicides or homicides.

3.1.2 Timing

The synthetic control method requires selecting a pre-treatment period over which to match outcomes to determine the optimal synthetic weights As Abadie et al. (2010) notes: "In any research design that exploits time variation in the outcome variable to estimate the effect of an intervention, synthetic control estimators may be biased if forward-looking economic agents react in advance of the policy intervention under investigation." Thus, researchers need to use economic theory to justify their decisions on which periods to minimize the sum of squared prediction errors and determine the correct weights.

Spencer (2023) matches on 36 monthly pre-treatment periods between January 2018 and December 2020.[12] While Spencer (2023)'s pre-treatment sample window ends one month before the implementation of Measure 110, he argues that he chose this decision because he wanted consistency across outcomes. Some of his outcomes are at the annual level, meaning the synthetic control can only match until December and define treatment starting in January. Joshi et al. (2023) also begin their sample in January 2018 but utilize all 37 pre-treatment periods, including January 2021, citing they "followed the Strengthening the Reporting of Observational Studies in Epidemiology (STROBE) reporting guideline for observational studies."

Beyond ensuring consistency across outcomes, the correct specification between the two models depends on whether anticipation effects are relevant. For instance, if there was anticipation prior to implementation (e.g., in January 2021), where overdoses had already begun to change, researchers should exclude the anticipatory window for matching. While we do not statistically verify whether and when (if any) anticipation happened, in our preferred specification, we will experiment with ending our matching period in December 2021 and January 2022 for robustness.

Finally, the authors differ in the number of observations in the post-treatment period. Spencer runs their analysis through the end of 2021, while Joshi et al. (2023) run their analysis through the end of March, 2022.

[12]Spencer (2023) makes a note of the policy being implemented on February 1, 2021, in the paper, but chooses January as the treatment unit in coding and chart output. In his replication package, this is identified by unit 37 (t=37) being selected as the treatment period, which in a monthly dataset beginning (t=1) in January of 2018, corresponds to January of 2021.

3.1.3 Data Set

For reasons of security and privacy, the CDC suppresses the number of deaths for an observation if the number of deaths is below ten. Spencer (2023) uses this restricted version of the data and imputes integers from a random uniform distribution on the interval [0,9] to fill suppressed values. Joshi et al. (2023) had access to the unrestricted version of the data.

In Appendix Figure 3, we re-estimate our synthetic control 1,000 times using different imputed values and plot the distribution of possible point estimates. The findings from this exercise confirm that the point estimates reported by Spencer (2023) are reasonable and not driven by artifacts of imputed data.

In our replication and extension of each work, we utilize the restricted data.

3.2 Longer-run Effect of Drug Decriminalization

Zoorob et al. (2024) extend the time period analyzed in Spencer (2023) and Joshi et al. (2023), focusing on the second-year update of Measure 110, though their methodologies differ slightly. They aggregate their panel data into half-year periods rather than monthly, and they use a longer pre-treatment window starting in January 2008. In addition, they adopt a matrix completion approach to synthetic control. Initially, they find a statistically significant biannual increase in drug mortality of 1.83 per 100,000 (equivalent to a monthly increase of 0.3 per 100,000). However, in their preferred specification—which controls for fentanyl supply as measured by the percentage of all drug seizures involving fentanyl—they find no association between Measure 110 and drug mortality. Using a structural break test, Zoorob et al. (2024) show that fentanyl crisis arrived in Oregon at the same time as Measure 110, which explains why Measure 110 is not associated with an increase in drug mortality.

We raise two methodological concerns with the approach taken by Zoorob et al. (2024). In their preferred specification, the authors explicitly control for changes in the supply of illicitly manufactured fentanyl. If the saturation of fentanyl in the drug market coincided with the passage of Measure 110, synthetic control estimates could be biased upward—unless the donor pool units used to construct the synthetic Oregon also experienced similar supply-side shocks. To address this, the authors include a direct control for fentanyl availability, using the ratio of fentanyl-related seizures to the total number of drug seizures in Oregon.

There are two issues with this approach. One relates to the assumption underlying the inclusion of this control; the other pertains to the specific measurement strategy used. First, the authors implicitly assume that Measure 110 had no effect on the fentanyl supply chain itself. We find this assumption limiting. Consider a rational agent engaged in illicit fentanyl trafficking, as modeled in Becker's (1968) economic theory of crime (Becker, 1968). If such agents weigh the costs of apprehension and conviction, decriminalization may reduce their perceived risk and lead to increased offending. In other words, if drug-related arrests fell post-Measure 110—as they did—then the assumption that fentanyl supply remained unaffected by the policy change is hard to justify. This raises concerns about treating the supply of fentanyl as exogenous in the model.

Second, the specific control used—the ratio of fentanyl seizures to all drug seizures—may be a poor proxy for fentanyl supply. Aside from documented concerns about the accuracy of NFLIS data compared to actual illicit supply (Pitts et al., 2023), this ratio can vary either because of changes in fentanyl prevalence or because of changes in the seizure patterns of other drugs.[13] If this ratio changes due to the changes in the seizure patterns of other drugs, which is induced by the treatment, then this ratio is considered a bad control and bias the treatment effect (Caetano et al., 2024). If the goal is to capture a structural shift in fentanyl availability, then more direct measures—less affected by the broader seizure landscape—would be preferable.

A separate empirical concern involves the decision to use biannual data and to extend the pre-treatment window back to 2008. The authors aim to address potential temporal confounding between Measure 110 and the transition from the heroin wave to the fentanyl wave of the opioid epidemic. However, by extending the pre-period to 2008, they allow the synthetic control to span across multiple regime shifts—most notably, the period when overdose deaths shifted from being driven by prescription opioids to heroin following the reformulation of OxyContin (Evans et al., 2018, Ciccarone, 2019). Prior research suggests that these transitions occurred idiosyncratically across states (Ciccarone, 2019, Shover et al., 2020, West et al., 2021), raising concerns about the plausibility of achieving valid synthetic matches—not just for the fentanyl era but for each of the preceding waves as well.

[13] In Appendix Table 1, we confirm that the total number of drugs seized decreased and, as a result, the ratio of fentanyl seizures to all drug seizures increased, even though the number of fentanyl seizures did not significantly change.

4 Results

4.1 Replication & Extension of Spencer (2023) and Joshi et al. (2023)

We begin our estimation by replicating both papers using a traditional synthetic control estimate. More specifically, for each donor state (j $\in$ {2,..., J}), we find a donor weight w_j which minimizes the pre-treatment (t $\in \{1, ..., T^{pre}\}$) differences in the outcome Y_{jt} between the treatment state (j=1) and synthetic control group which is a weighted combination of all donor states. Mathematically, we find w_j that solves the following optimization problem.

$$\min_{w_s} \sum_{t=1}^{T^{pre}} (Y_{1t} - \sum_{j=2}^{J} w_j Y_{jt})^2$$

$$\text{s.t.} \sum_{j=2}^{J} w_j = 1$$

$$w_j \geq 0 \quad \forall j$$

Once we solve for w_j, we use these donor weights to construct a synthetic counterfactual during the post-treatment period, which we assume to be the outcome of interest in the treatment states had neither Measure 110 nor The Blake Fix been enacted. The average post-treatment difference between the actual treatment state and the synthetic counterfactual is the causal effect of the policy.

We conduct our inference using permutation-based p-values. For each donor state $j \in \{2, ..., J\}$, we assign treatment and re-estimate the synthetic control estimate. Then, we calculate the ratio of the pre- and post-mean squared prediction errors (MSPE), which we define as the sum of the squared differences between the actual treatment state and the synthetic counterfactual. A high MSPE implies a better pre-treatment fit as compared to the post-treatment fit (i.e., large treatment effects). In contrast, a low MSPE can be interpreted in two ways: either as a poor pre-treatment fit that carries over into the post-treatment fit, or as a tight post-treatment fit (i.e., null treatment effects). We rank the ratio of MSPE and use the relative ranking (in percentage terms) for our p-value for inference.[14]

[14] For instance, if Oregon's MSPE ratio was ranked 1 out of 50, then the MSPE will be 0.02 (1/50).

In column of panel I, and column 4 of panel II of Table 4, we successfully replicate both Joshi et al. (2023) and Spencer (2023) results for Measure 110 in Oregon. However, we note that our point estimates are slightly different than those reported by the papers. One explanation for this difference is the difference in the Stata command that we utilize.[15] Moreover, in fig. 2, we show the synthetic control weights that we used to replicate both papers. These weights are qualitatively similar to the previous studies.

In the remaining columns of Table 4, we experiment with changing the specification to be more similar to each other's specification, including changing the definitions of drug overdose (panels I and II), changing the treatment start month (columns 1 and 3 vs. 2 and 4), and sample window (columns 1 and 2 vs. 3 and 4). We document a few findings. First, ceteris paribus, our estimated effects are smaller when we focus on all drug-related mortality instead of unintentional drug overdoses. Second, our point estimate and precision are sensitive to whether we begin our treatment in January or February. For instance, when we focus on unintentional drug overdoses and end our sample in March 2022 (panel A columns 3 and 4), our estimated coefficient decreases by 0.057 percentage points. Third, the results are robust to the inclusion of three more months of post-treatment data.

We repeat the same exercise in Table 5; however, this time we focus on Washington. Similar to Oregon, we find that the differences in the two studies' conclusions stem from differences in the definitions of drug overdoses and when the treatment date began (and when the synthetic control was matched until).

In panel I of Appendix Table 2, we re-estimate Spencer (2023) and Joshi et al. (2023)'s preferred model and add 90 percent prediction intervals (Cattaneo et al., 2021). In panel II, we also calculate the 90 percent confidence intervals using t-tests following Chernozhukov et al. (2024) and using K=3 cross-fold validation. We note that our point estimates and statistical significance, reported in Appendix Table 2, are slightly different from those of our replication results. This discrepancy may be due to slight differences in methodologies and weights assigned to each donor state. Nevertheless, we note that the 90 percent confidence and prediction interval overlaps between Spencer (2023)'s preferred specification and Joshi et al. (2023)'s main model. The results from this exercise confirm that the estimated treatment effects in the two papers may not be statistically different.

[15] We use Synth package, whereas Spencer (2023) uses Synth2 and Joshi et al. (2023) use synth_runner.

In Figure 2, we extend our synthetic control estimates for Oregon to include up to November 2023. For simplicity, we report synthetic control estimates using Spencer (2023) and Joshi et al. (2023)'s specifications. We note that the gap between Oregon and synthetic Oregon becomes larger in late 2022, suggesting a potential dynamic effect that was not explored in either study. In Table 6, we report the average treatment effects and the p-values. Regardless of which specification we use, we find a statistically significant increase of 0.79 to 0.85 per 100,000 in Oregon, driven by increases in late 2022 and 2023. Relative to the synthetic counterfactual, these point estimates suggest a roughly 37 to 47 percent increase relative to the baseline (synthetic counterfactual) mean. This finding from Table 6 suggests that the difference between the two papers may also be due to imprecision problems caused by an insufficient post-treatment window.

In Figure 3, we expand our synthetic control estimate for Washington. Consistent with Oregon, we continue to find a second and third-year effect. However, this finding is surprising given that Washington only had decriminalization for five months. In Appendix Figure 4, we experiment with estimating a synthetic control model using crime and arrest data from the National Incident-Based Reporting System (NIBRS). Our results indicate that in Washington, even after the re-criminalization of drug possession, the arrest rate for drug overdose did not revert to the pre-treatment period, which is similar to that of Oregon, suggesting that the change in police enforcement may explain our results.

4.2 Robustness

We conduct several robustness tests to explore the sensitivity of the estimated impacts of drug decriminalization.(Ferman et al., 2020).

First, we experiment with other extensions of the synthetic control model. In columns (1) to (8) of Appendix Table 3, we find that our estimates continue to show a significant increase in drug overdose rate when we use augmented synthetic control (Ben-Michael et al., 2021), synthetic difference-in-differences (Arkhangelsky et al., 2021), generalized synthetic control methods (Xu, 2017), or synthetic control using multiple outcomes XXX. In columns (9) to (10), we find that our point estimates are qualitatively similar when we estimate a two-way fixed effects difference-in-differences estimator. However, our estimates are more imprecise and have larger p-values.

In the next set of estimations shown in Appendix Figure 5, we experiment with keeping the same weight variables for both outcomes. While we have poor pre-treatment matches when we force the same weights, we continue to find an uptick relative to synthetic control estimation.[16] Moreover, in Appendix Figure 6, we experiment with excluding one donor state at a time. We find that no particular control states are driving our increase in drug overdose rate. Our results from this exercise suggest that the drug overdose rate increased by at least 0.77 per 100,000 in Oregon and 0.71 per 100,000 in Washington. Finally, in Appendix Table 5, we conduct different inference procedures (Chernozhukov et al., 2024, Cattaneo et al., 2021). We continue to find a statistically significant increase in drug mortality. This finding confirms that our findings are not due to false positives that can stem from a specific inference procedure.

In Appendix Table 6, we experiment with matching our Synthetic Control only up to February 2020, allowing for our donor pool to be based on the pre-pandemic trend of drug-related fatalities. The main idea of this exercise is to ensure that our choice of the donor pool and, consequently, our results are not driven by state-specific dynamic effects of COVID-19 on substance use. Our findings remain qualitatively similar, suggesting that the COVID-19 pandemics unique affects on drug use nationwide is not adding bias.

In Appendix Table 7, we estimate placebo synthetic control for California, Nevada, and Idaho. This relates to the concerns that decriminalization coincided spuriously with the arrival of fentanyl from the East Coast to the West Coast. We find little evidence that drug overdose rates increased in these nearby states, suggesting that the coincidental arrival of fentanyl in Oregon does not drive our estimates. In addition, along the same lines, we experiment with three alternative tests to ensure fentanyl is not driving our results. First, we estimate a two-way fixed effects model using only these three states as our counterfactual. While inference is more challenging due to the small cluster problem, our estimated treatment effects suggest increases of 0.77 to 0.79 in Oregon and 0.76 to 0.78 in Washington, which are qualitatively similar to those in our main specification.

In the next subsection, we explore the timing of fentanyl's arrival in the west more fully.

[16] In Appendix Table 4, we also experiment with estimating difference-in-differences using the same synthetic control weights, which allows the outcome level to be different across the treated unit and the counterfactual. We continue to find a large and statistically significant increase of 0.77 to 1.09 per 100,000 increase in drug overdose rate in Oregon, and 0.76 to 0.83 per 100,000 increase in drug overdose rate in Washington.

4.3 Replication of Zoorob et al. (2024)

To first estimate when fentanyl first arrived in Oregon, Zoorob et al. (2024) conduct a structural break test for the ratio of fentanyl to all-drugs seized. We attempt to replicate their results by testing their identified break-date using a Chow structural-break test (Chow, 1960). We test the structural break date of the first-half of 2021 for Oregon and second-half of 2020 for Washington on three measures: the ratio used by Zoorob et al. (2024) (fentanyl to all-drugs seized), an alternative per-capita fentanyl seized ratio and per-capita non-fentanyl seizures. Figures 4, 5 and 6 show the time series being tested. F-tests of all three measures indicate a rejection of the no-break hypotheses at that date, both confirming the break found by Zoorob et al. (2024) and providing evidence of the per-capita fentanyl measure robustness. The rejection of the null hypothesis for non-fentanyl seizures provides evidence of a change in enforcement that may bias the measure used by Zoorob et al. (2024).

We next perform sup-Wald and Bai-Perron tests on each univariate time series to estimate structural breaks assuming one exists. Figure 7 shows results for the first set of tests, and figure 7 provides results for the second. For the set of sup-Wald tests, we allow a single structural break for each state by performing a Chow test on each possible break date and identifying that with the highest F-statistic. The Bai-Perron allows for multiple breaks, and we provide results for both mean and trend. The sup-Wald test results indicate a slight difference in date of break from Zoorob et al.; we find that, when allowing for a single break only, the break in mean occurs in the latter half of 2018 for both Oregon and Washington which is prior to the implementation of Measure 110. When we allow for multiple breaks in the Bai-Perron testing, we concur with the Zoorob estimate of a late-2020 break for both Oregon and Washington, though Washington's breakpoint may also include 2018. Since these breakpoints also coincide with COVID-19, a national shock to overdose (Friedman and Akre, 2021) we also test for multiple structural breaks on each univariate time-series demeaned by the national mean of the same measure. This removes common national shocks from Oregon and Washington's state-level structure. Figure 8 shows the results for a break in both mean and trend.

In Table 8, we assess the robustness of key modeling choices made by Zoorob et al. (2024): the use of a fentanyl control and the extension of the pre-period to 2008. To evaluate the robustness of the fentanyl control, we compare models without any fentanyl control to three specifications: one using the original

control employed by the authors (i.e., the share of fentanyl seizures relative to all drug seizures, denoted as "Share"), a second using fentanyl seizures per capita, and third restricting the donor pool to states hit by fentanyl later, as measured by Zoorob et al. (2024). We include the per-capita measure as an alternative proxy for fentanyl supply, as it captures overall prevalence while being independent of the frequency with which other drugs are seized. The primary justification for using the fentanyl seizure share, rather than raw counts, is that it is less susceptible to variation in overall law enforcement activity (Zibbell et al., 2023). However, while our per-capita measure is potentially influenced by changes in police behavior, we argue that such behavioral changes are themselves a consequence of Measure 110—again, our preferred specification omits any control variables due to changing behavior of those associated with the criminal justice system. Furthermore, if Zoorob et al. (2024)'s intent in controlling for fentanyl supply was to isolate shifts in law enforcement activity, then any regime switching—as previously discussed—would itself induce endogenous changes in police behavior, as enforcement efforts are likely to focus on drugs most associated with overdose mortality.

In Panel I columns (1) and (2) Table 8, we successfully replicate the point estimates for Oregon reported in Figure 3 of 8.[17] In Panel II, we use the same biannual structure but a shortened pre-period beginning in 2014 to isolate the fentanyl wave of the opioid epidemic, thereby limiting confounding from earlier regime shifts that could distort share-based measures of fentanyl supply. We document that, regardless of when the sample starts, Measure 110 has no effect on mortality when we include Zoorob et al. (2024)'s preferred control (column 2), but a positive, statistically significant effect without it (column 1). When we replace the share-based fentanyl control with an alternative proxy—fentanyl seizures per capita (column 3)—the estimated treatment effect remains positive and statistically significant. These estimates (ATTs of 2.301 and 2.924 in Panels I and II, respectively) are also much closer in magnitude to the estimates produced when no fentanyl control is used at all (1.83 and 2.37, respectively). In columns 4 and 5, we find that the estimated treatment effect is still economically and statistically significant when we restrict our donor pool to states where fentanyl were hit on 2019 onwards or 2020 onwards based on Zoorob et al. (2024)'s changepoint analysis. Notably, we find that the estimated coefficient becomes slightly larger, which is consistent with the

[17]We use the exact data and code provided by Zoorob et al. (2024), available at: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/JT4CDS

possibility that not accounting for fentanyl and the early-exposed states can downwardly bias our treatment effects if the arrival of the fentanyl crisis has positive dynamic effects on drug mortality.

Columns 6 through 10 show a similar pattern for Washington. Models that control for the share of fentanyl seizures yield statistically insignificant estimates. However, when using the per-capita fentanyl control or restricting the donor pool, we observe significant increases in overdose mortality (ATTs of 2.03 to 3.23), both significant at the 1% level.

Panel III of Table 8 presents replication estimates using the difference-in-differences (DiD) specification from Spencer (2023), following the approach outlined in Table 2 of Zoorob et al. (2024). As with the synthetic control models, controlling for the share of fentanyl seizures attenuates the estimated effect of Measure 110 to statistical insignificance. However, when we instead control for fentanyl seizures per capita, the estimated effect remains positive and statistically significant, closely aligning with results from the uncontrolled specification. These findings reinforce the conclusion that estimates of Measure 110's impact on overdose mortality are susceptible to how fentanyl supply is measured, with substantively different results depending on whether a relative (share-based) or absolute (per-capita) control is used.

Panels IV and V further shorten the pre-period while maintaining the biannual structure, replicating the estimation strategies of Spencer (2023) and Joshi et al. (2023), respectively. In both panels, we again compare specifications with no fentanyl control, a share-based fentanyl control, and a per-capita control. Notably, the attenuation observed in prior panels disappears —across all specifications, the estimated effects of Measure 110 on overdose mortality remain positive and statistically significant, regardless of the fentanyl control used. Moreover, our findings are robust to whether we restrict our donor pool to "late" fentanyl arriving states.

By further shortening the pre-period—and accepting the conclusion from the changepoint analysis in Zoorob et al. (2024) that Oregon's fentanyl saturation coincided with the implementation of Measure 110—we would expect the inclusion of a fentanyl control to substantially attenuate the estimated treatment effect. In this restricted sample, the donor pool is primarily composed of states that had already experienced full fentanyl saturation, making it difficult to separately identify the effects of Measure 110 from those of increased fentanyl exposure. Under this logic, controlling for fentanyl supply should absorb some of the

increase in overdose mortality and thus reduce the estimated impact of M110. However, the fact that the point estimate changes little when using the share-based control—and in fact increases when using a per-capita measure—raises two possibilities. Either fentanyl saturation occurred well before the policy change, leaving no substantive shift to control for, or the variables used to proxy fentanyl supply—particularly the share-based measure—are inadequate and fail to capture meaningful variation in fentanyl prevalence.

5 Conclusion

The overdose epidemic is among the most devastating public health crises in recent memory. In an effort to reduce overdose deaths, many legislators have turned to the decriminalization of drug possession as a potential solution. In February 2021, Oregon became the first state to implement such a policy through Measure 110. In this paper, we replicate and extend short-run findings on the causal impact of Measure 110 on overdose mortality (Spencer, 2023, Joshi et al., 2023), and critically examine the empirical strategy of the only longer-term evaluation to date (Zoorob et al., 2024).

Our replication results show that small differences in the implementation of the synthetic control method can lead to contradictory short-run findings regarding Measure 110's impact on overdose mortality. While there were some differences in timing specifications and data access, the primary source of divergence stems from how overdose mortality was defined—specifically, unintentional overdoses versus all-cause overdose deaths—which affects the donor weights that compose the synthetic control. We also find that the synthetic control method is relatively underpowered to detect small effect sizes, such as those estimated in Joshi et al. (2023). When extending the analysis to cover a longer horizon, we find that regardless of model specification, the long-run (2+ year) results consistently indicate a statistically significant increase in overdose mortality. The estimated average treatment effect on the treated (ATT) grows year-over-year from 2021 to 2023, suggesting a compounding effect in the second and third years post-implementation. Our estimates imply that Measure 110 is associated with a monthly increase of 0.78 to 0.85 deaths, corresponding to approximately 1125 to 1213 additional deaths during our sample window—an increase of 38 to 46% relative to the baseline mean.

While the magnitude of the estimated effect may be striking, we acknowledge several limitations in our

empirical approach. Most notably, the pre-treatment period overlaps with the COVID-19 pandemic, meaning that donor states that served as suitable counterfactuals for Oregon pre-pandemic may not have remained so afterward. To address this, we exclude the COVID period from our pre-treatment window and find that the estimated effect remains positive and statistically significant, with an ATT of 0.88 to 0.89.

Additionally, concerns have been raised that the fentanyl wave of the opioid epidemic may confound estimates of Measure 110's impact (Zoorob et al., 2024). We replicate, extend, and conduct sensitivity analyses on the findings in Zoorob et al. (2024) to assess (a) whether the saturation of fentanyl into the illicit drug market biased M110 estimates, and (b) whether their biannual specification was robust to alternative pre-treatment periods. We find that their use of fentanyl's share of all drug seizures is highly sensitive to law enforcement priorities across substances. As an alternative, we control for fentanyl seizures per capita—acknowledging that this measure is also influenced by enforcement intensity—and observe that the attenuation effect found using the fentanyl share disappears. Beyond sensitivity to the fentanyl measure itself, we also test the robustness of these results to variations in the length of the pre-treatment window. Across shorter pre-treatment periods, the estimated effect remains positive and statistically significant.

We contribute to the harm reduction and substance use disorder intervention literature by providing credible evidence of externalities associated with decriminalization policies. However, it is important to acknowledge that Measure 110 included many components beyond decriminalization—several of which are still ongoing—such as expanded funding for treatment services. We also draw attention to the so-called "fentanyl loophole," which was not closed until June 2023. While our findings suggest that Measure 110 increased Oregon's overdose mortality rate, further research is needed—and is already underway—to identify the specific mechanisms driving this increase. Finally, our analysis is necessarily limited to the unique context of Oregon's implementation of decriminalization, including its imperfections. Our results reflect the policy as it was enacted in Oregon, rather than decriminalization in the abstract.

Any evaluation of Oregon's outcomes must also consider parallel evidence from Washington's Blake Fix. Our analysis indicates that the brief period of decriminalization under the Blake Fix was likewise associated with an increase in overdose mortality. Thus, any proposed mechanism driving Oregon's outcomes must also be plausible in light of Washington's distinct implementation. Further research is needed to uncover shared

pathways through which decriminalization may contribute to overdose increases in both states. One such candidate is reduced enforcement, as proxied by declining arrest rates—a pattern we illustrate in Figure 4.

Further research is also needed to assess the broader impacts of Measure 110 beyond overdose mortality. As Netherland et al. (2022) note: "The success or failure of Measure 110 has the potential to shape drug policy in the USA for decades to come, but 'success' or 'failure' is entirely dependent on what outcomes are being measured, how the data are gathered, and whether the findings are understood within the broader context of what is happening on the ground in Oregon." A comprehensive evaluation of the policy must therefore move beyond a narrow focus on mortality and engage with the full spectrum of ways in which substance use intersects with health, criminal justice, and social systems.

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6 Tables & Figures

6.1 Figures

Figure 1: A chart of the overdoses nationwide using different definitions of overdose.

(a) By Definition



(b) Differences Between Spencer and Joshi et al. Definition

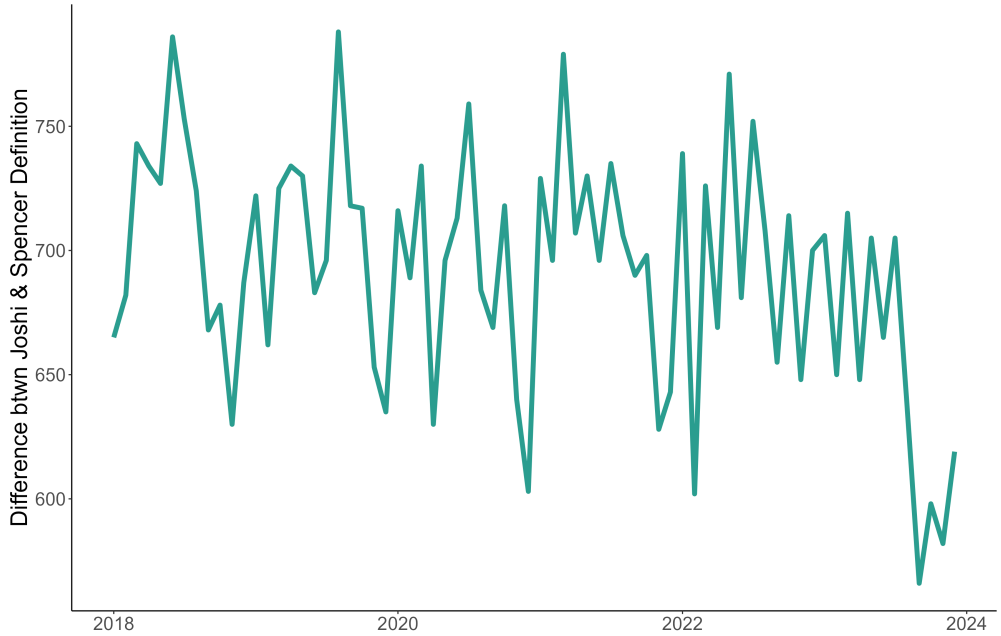
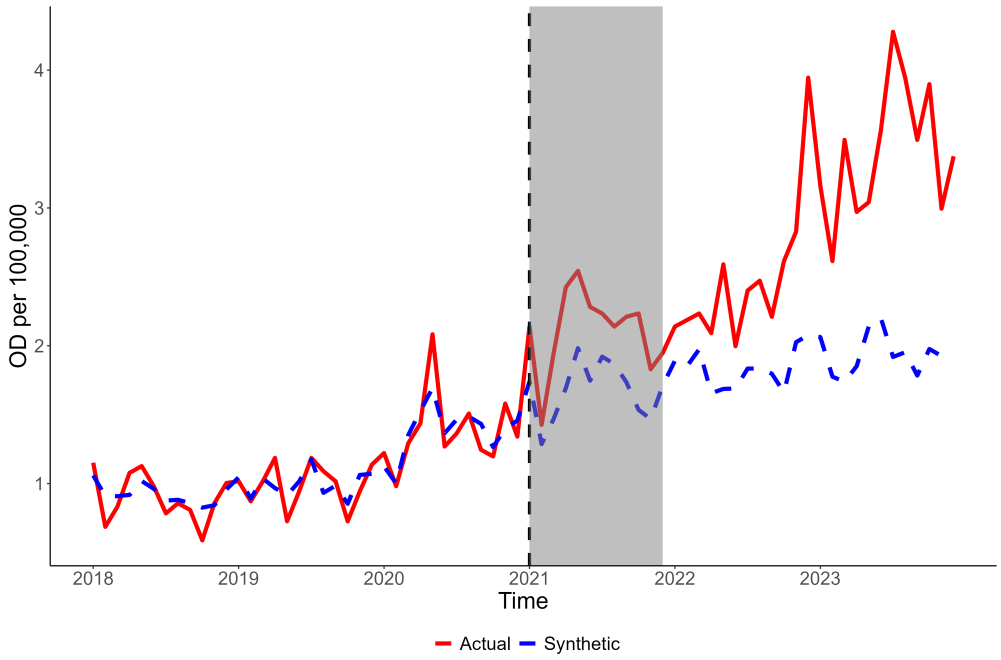
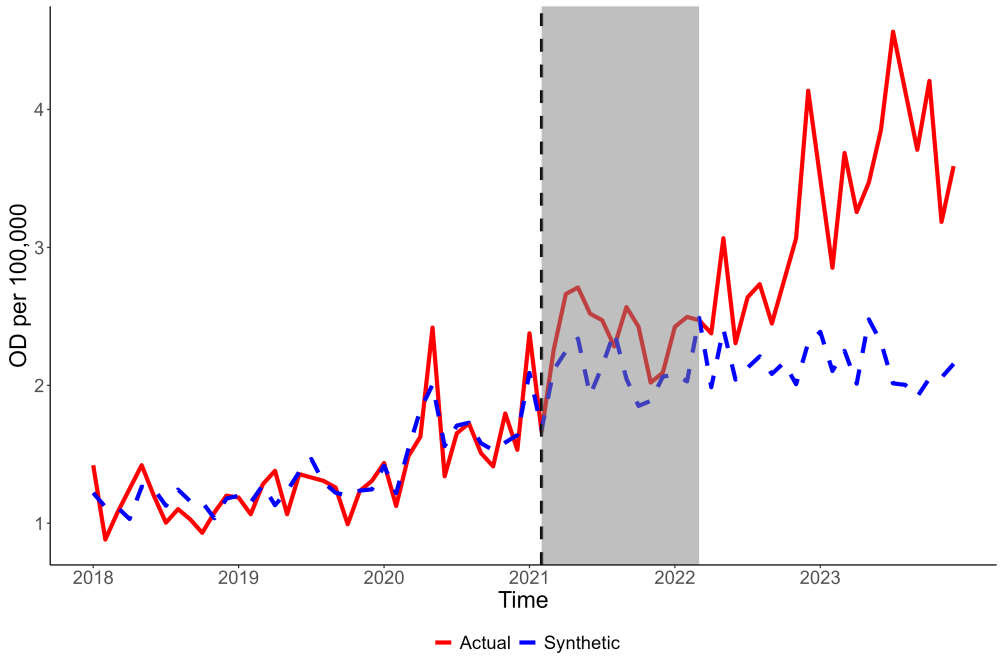


Figure 2: Oregon vs. Synthetic Oregon in Drug Overdose Rate, Longer Time Window

(a) Spencer (2023) Specification



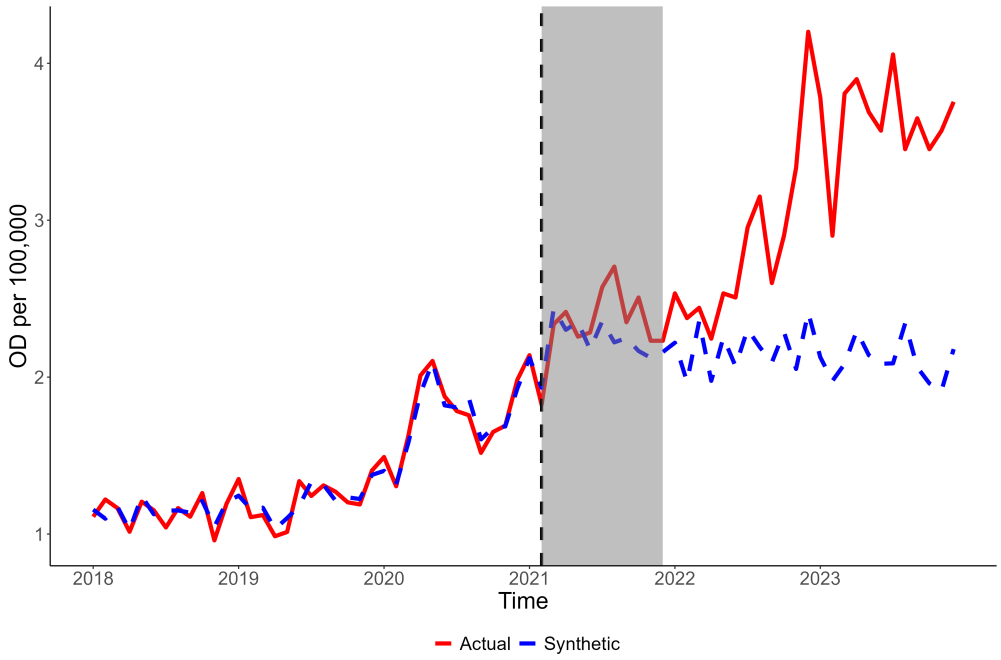
(b) Joshi et al. (2023) Specification



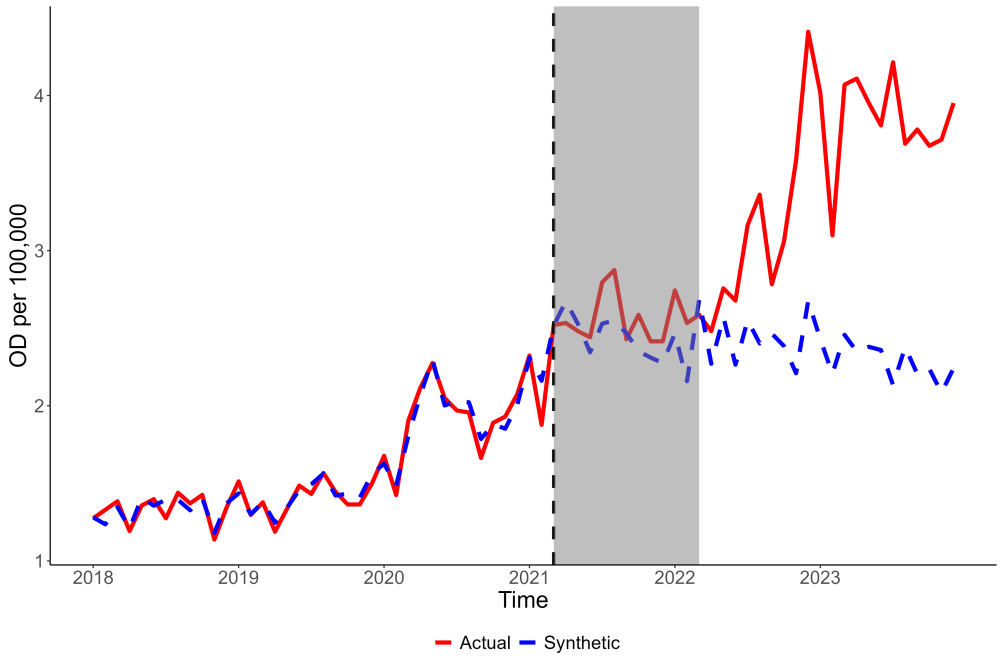
Notes: Synthetic Control Estimates using Spencer (2023)'s specification (panel a) and Joshi et al. (2023)'s specification (panel b) are shown. In panel a, the outcome is unintentional drug overdose, and the pre-treatment window is from Jan 2018 to Dec 2020. In panel b, the outcome is all drug overdose and the pre-treatment widnow is from Jan 2018 to Jan 2021. The gray shaded area represents each paper's post-treatment window.

Figure 3: Washington vs. Synthetic Washington in Drug Overdose Rate, Longer Time Window

(a) Spencer (2023) Specification

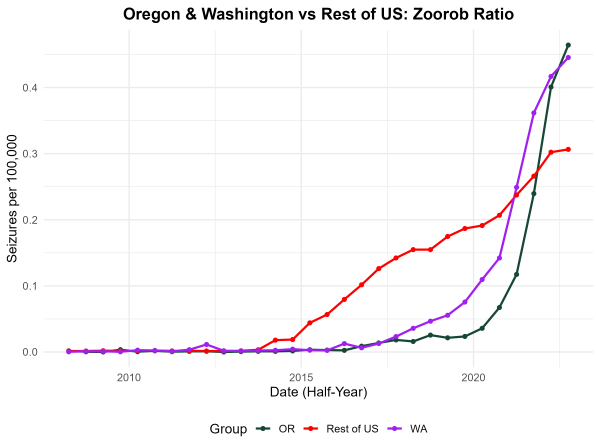


(b) Joshi et al. (2023) (2023) Specification

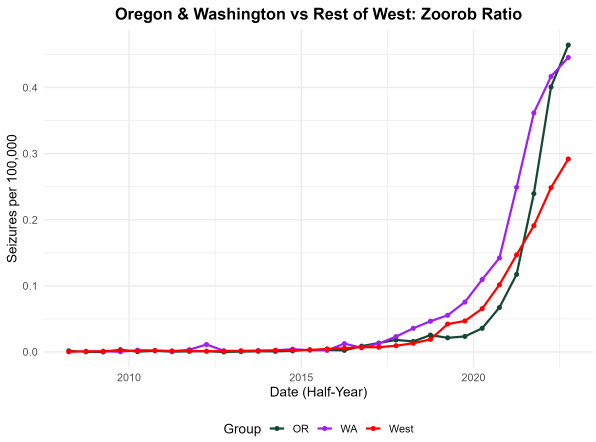


Notes: Synthetic Control Estimates using Spencer (2023)'s specification (panel a) and Joshi et al. (2023)'s specification (panel b) are shown. In panel a, the outcome is unintentional drug overdose, and the pre-treatment window is from Jan 2018 to Jan 2021. In panel b, the outcome is all drug overdose, and the pre-treatment widnow is from Jan 2018 to Feb 2021. The gray shaded area represents each paper's post-treatment window.

Figure 4: Zoorob et al. (2024) Ratio Charts



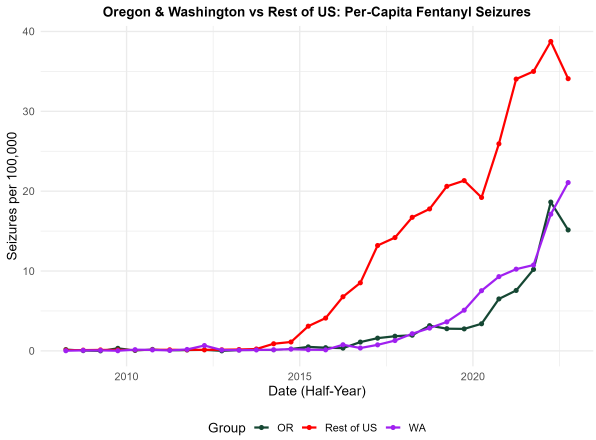
(a) Oregon & Washington vs U.S.



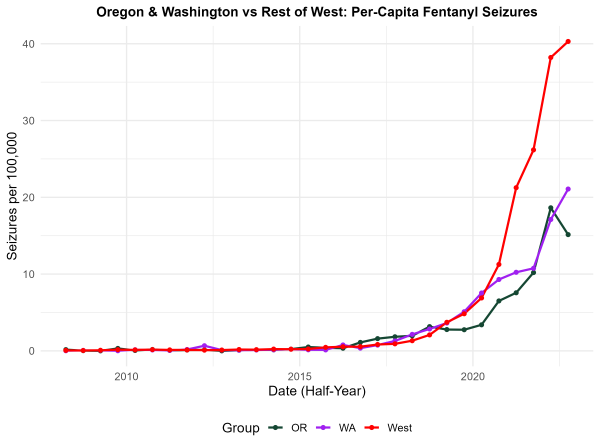
(b) Oregon & Washington vs Western States

Notes: Charts of the ratio of fentanyl to all-drug seizures in Oregon and Washington as compared to the rest of the U.S. (Panel A) and the Western states (Panel B). Comparisons are made via a mean transformation.

Figure 5: Charts of Per-Capita Fentanyl



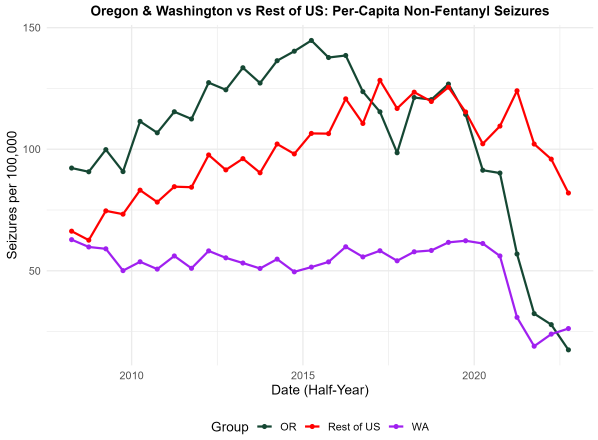
(a) Oregon & Washington vs U.S.



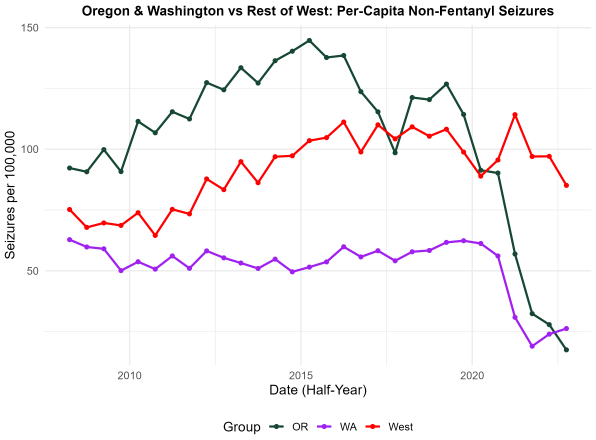
(b) Oregon & Washington vs Western States

Notes: Charts of the number of fentanyl seizures in Oregon and Washington as compared to the rest of the U.S. (Panel A) and the Western states (Panel B). Comparisons are made via a mean transformation.

Figure 6: Charts of Per-Capita Non-Fentanyl Seizures



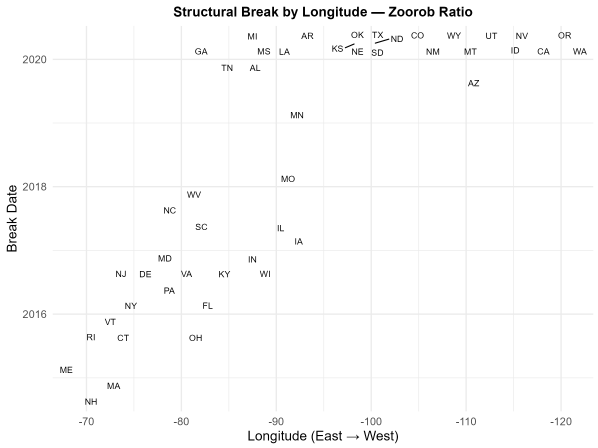
(a) Oregon & Washington vs U.S.



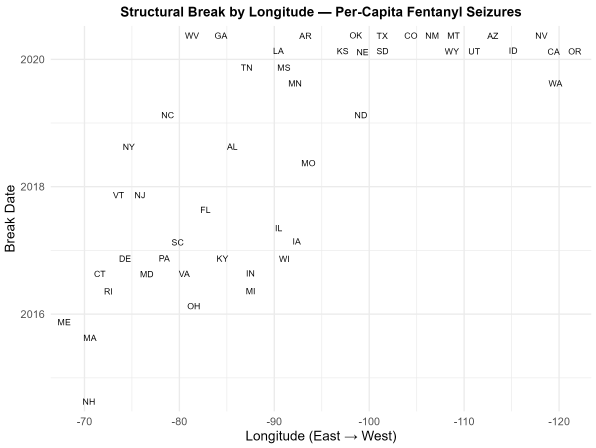
(b) Oregon & Washington vs Western States

Notes: Charts of the number of non-fentanyl seizures in Oregon and Washington as compared to the rest of the U.S. (Panel A) and the Western states (Panel B). Comparisons are made via a mean transformation.

Figure 7: Chow Single Break Tests



(a) Structural Breaks by Longitude Using the Fentanyl to All-Drugs Ratio



(b) Structural Breaks by Longitude Using the Fentanyl per-Capita Measure

Notes: Results for the sup-Wald structural break test of the ratio used by Zoorob et al. (2024) (Panel A) and the fentanyl-per-capita ratio (Panel B). Each break date is plotted by state centroid longitude.

6.2 Tables

Table 1: Changes from M110 for possession above or below "user threshold."

Drug:	Section:	Threshold Amt.:	Pre: Below	Post: Below	Pre: Above	Post: Above
LSD	11	40 u.u.	Class A Misdemeanor	Class E Violation	Class B Felony	Class A Misdemeanor
Psilocybin/Psilocin	11	12g	Class A Misdemeanor	Class E Violation	Class B Felony	Class A Misdemeanor
Methadone	12	40 u.u.	Class A Misdemeanor	Class E Violation	Class C Felony	Class A Misdemeanor
Oxycodone	13	40 tab.	Class A Misdemeanor	Class E Violation	Class C Felony	Class A Misdemeanor
Heroin	14	1g	Class A Misdemeanor	Class E Violation	Class B Felony	Class A Misdemeanor
MDMA/Ecstacy	15	1g/5 tab.	Class A Misdemeanor	Class E Violation	Class B Felony	Class A Misdemeanor
Cocaine	16	2g	Class A Misdemeanor	Class E Violation	Class C Felony	Class A Misdemeanor
Methamphetamine	17	2g	Class A Misdemeanor	Class E Violation	Class C Felony	Class A Misdemeanor

Table 2: Synthetic control weights in each authors' analysis by state (abbreviated).

	Weight	
State Abbr.	**Spencer (2023)**	**Joshi et al. (2023)**
AK	0.026	—
AL	—	0.111
CO	0.072	0.045
DC	0.036	—
IA	0.061	—
KS	0.216	0.058
MD	0.282	—
MN	—	0.027
MS	0.012	0.149
MT	0.178	0.272
NC	0.04	0.107
NE	—	0.038
OK	—	0.089
SD	0.032	—
VT	0.021	—
WV	—	0.01
WY	0.023	0.044

Table 3: An overview of the differences between the two replicated works.

Author	Joshi et al. (2023)	Spencer (2023)
ICD-10 Codes	X40-44, X60-64, X85, Y10-14, T36-50	X40-44
NVSS Access	Unrestricted	Restricted
Treatment Date	Feb. 2021	Jan. 2021

Table 4: Replication: Synthetic Control Estimate for Oregon

	Panel I: Spencer (2023) Outcome			
ATT	0.434**	0.381**	0.403**	0.346**
P-Val	{0.02}	{0.02}	{0.02}	{0.04}
	Panel II: Joshi et al. (2023) Outcome			
ATT	0.329*	0.272	0.32	0.27
P-Val	{0.08}	{0.16}	{0.16}	{0.26}
Sample	2018-21	2018-21	2018-22	2018-22
Matching Until	Dec	Jan	Dec	Jan

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: Synthetic control estimates are shown. All estimates use January 2018 to either December 2020 or January 2021 to find the synthetic weights. Panel I focuses on all unintentional drug overdoses, which is the outcome used by Spencer (2023). Panel II focuses on all drug mortality, which is the outcome used by Joshi et al. (2023). The p-values, calculated via a permutation test, are provided inside the curly brackets.

Table 5: Replication: Synthetic Control Estimate for Washington

	Panel I: Spencer (2023) Outcome			
ATT	0.113**	0.155**	0.147**	0.184**
P-Val	{0.02}	{0.02}	{0.02}	{0.02}
	Panel II: Joshi et al. (2023) Outcome			
ATT	0.06**	0.103	0.087**	0.115*
P-Val	{0.02}	{0.12}	{0.02}	{0.06}
Sample	2018-21	2018-21	2018-22	2018-22
Matching Until	Jan	Feb	Jan	Feb

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: Synthetic control estimates are shown. All estimates use January 2018 to either January 2021 or February 2021 to find the synthetic weights. Panel I focuses on all unintentional drug overdoses, which is the outcome used by Spencer (2023). Panel II focuses on all drug mortality, which is the outcome used by Joshi et al. (2023). The p-values, calculated via a permutation test, are provided inside the curly brackets.

Table 6: Extension: Synthetic Control Estimate for Oregon, Include Longer Post-treatment Period

	Spencer (2023)		Joshi et al. (2023)	
ATT Overall	0.848**	0.787**	0.796**	0.806**
P-Val	{0.02}	{0.02}	{0.02}	{0.02}
ATT 2021	0.434**	0.381**	0.329*	0.272
P-Val	{0.02}	{0.02}	{0.08}	{0.16}
ATT 2022	0.644**	0.567**	0.563**	0.582**
P-Val	{0.02}	{0.02}	{0.02}	{0.02}
ATT 2023	1.466**	1.378**	1.496**	1.521**
P-Val	{0.02}	{0.02}	{0.02}	{0.02}
Marching Until	Dec	Jan	Dec	Jan

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: Synthetic control estimates are shown. All estimates use January 2018 to either December 2020 or January 2021 to find the synthetic weights. Panel I focuses on all unintentional drug overdoses, which is the outcome used by Spencer (2023). Panel II focuses on all drug mortality, which is the outcome used by Joshi et al. (2023). The p-values, calculated via a permutation test, are provided inside the curly brackets.

Table 7: Extension: Synthetic Control Estimate for Washington, Include Longer Post-treatment Period

	Spencer (2023)		Joshi et al. (2023)	
ATT Overall	0.776**	0.79**	0.752**	0.732**
P-Val	{0.02}	{0.02}	{0.02}	{0.02}
ATT 2021	0.113**	0.155**	0.06**	0.103
P-Val	{0.02}	{0.02}	{0.02}	{0.12}
ATT 2022	0.633**	0.625**	0.586**	0.533**
P-Val	{0.02}	{0.02}	{0.02}	{0.02}
ATT 2023	1.528**	1.485**	1.551**	1.455**
P-Val	{0.02}	{0.02}	{0.02}	{0.02}
Marching Until	Jan	Feb	Jan	Feb

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: Synthetic control estimates are shown. All estimates use January 2018 to either January 2021 or February 2021 to find the synthetic weights. Panel I focuses on all unintentional drug overdoses, which is the outcome used by Spencer (2023). Panel II focuses on all drug mortality, which is the outcome used by Joshi et al. (2023). The p-values, calculated via a permutation test, are provided inside the curly brackets.

Table 8: Zoorob et al. (2024) Replication & Extension

	Oregon					Washington				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel I: Zoorob SC Replication, 2008-2022									
ATT	1.83***	-0.505	2.301***	2.499***	3.083***	1.341**	-0.868	2.035***	2.536***	3.048***
P-Val	{0}	{0.407}	{0}	{0.002}	{0}	{0.044}	{0.132}	{0.001}	{0.001}	{0}
	Panel II: Zoorob SC Replication, 2014-2022									
ATT	2.374***	0.415	2.924***	2.907***	3.306***	2.124***	-0.031	2.658***	2.817***	3.235***
P-Val	{0}	{0.473}	{0}	{0}	{0}	{0}	{0.961}	{0}	{0}	{0}
	Panel III: Zoorob DiD Replication									
ATT	0.241***	0.089	0.346***	0.29***	0.348***	0.186**	-0.129	0.292***	0.243**	0.301**
	(0.071)	(0.106)	(0.061)	(0.101)	(0.11)	(0.072)	(0.157)	(0.063)	(0.102)	(0.111)
	Panel IV: Spencer (2023) SC Estimate									
ATT	0.848**	0.725**	0.883**	1**	0.994**	0.776**	0.576**	0.358**	0.691**	0.827**
P-Val	{0.02}	{0.02}	{0.02}	{0.038}	{0.045}	{0.02}	{0.02}	{0.04}	{0.038}	{0.045}
	Panel V: Joshi et al. (2023) SC Estimate									
ATT	0.806**	0.82**	0.79**	1.004**	1.005**	0.782**	0.572**	0.615**	0.816**	0.922**
P-Val	{0.02}	{0.02}	{0.02}	{0.038}	{0.045}	{0.02}	{0.02}	{0.02}	{0.038}	{0.045}
Fentanyl Control	None	Share	PC	None	None	None	Share	PC	None	None
Sample Restricted to Late Fentanyl Arrivers	No	No	No	2019	2020	No	No	No	2019	2020

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

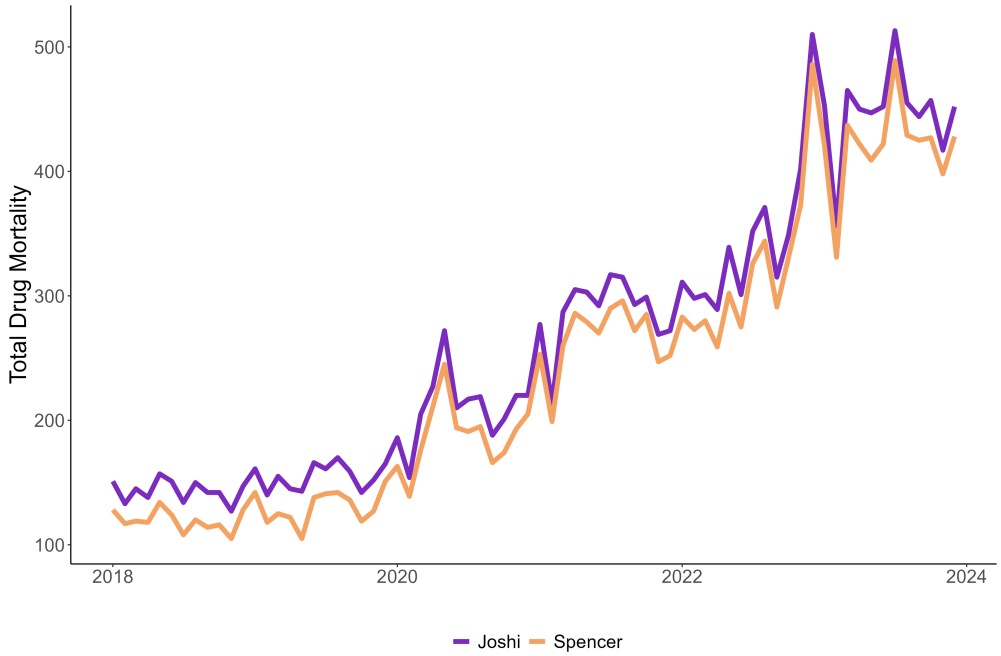
Notes: Panel I replicates and extends Zoorob et al. (2024)'s preferred synthetic control method. Panel II replicates and extends Zoorob et al. (2024)'s preferred specification using a narrower pre-treatment window. Panel III replicates and extends Zoorob et al. (2024)'s DiD replication of Spencer (2023). Panels IV and V provide robustness tests for the inclusion of fentanyl control in our preferred synthetic control estimation. In columns 2,3, 7, and 8 of panels IV and V, we use bi-annual pre-treatment rate of drug overdose rates as well as bi-annual fentanyl controls from 2018-2022 to find our synthetic weights. In columns 4 and 9, the donor pool is restricted to states where fentanyl crisis arrived in 2019 as defined in Figure 1 of Zoorob et al. (2024). In columns 5 and 10, the donor pool is restricted to states where fentanyl crisis arrived in 2020.

7 Appendix

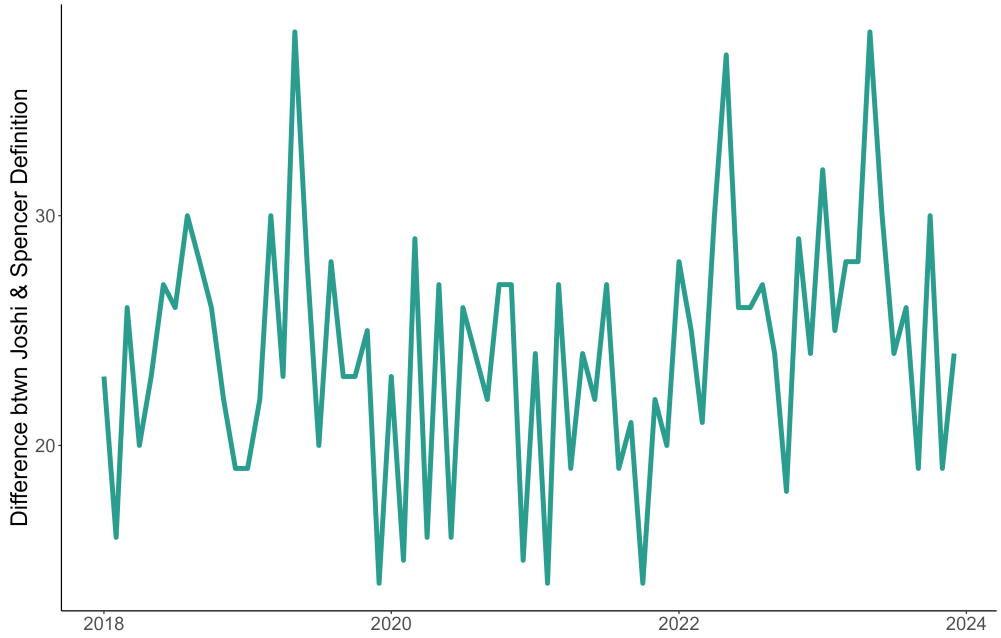
7.1 Appendix Figures

Appendix Figure 1: A chart of the overdoses in oregon and washington using different definitions of overdose.

(a) By Definition

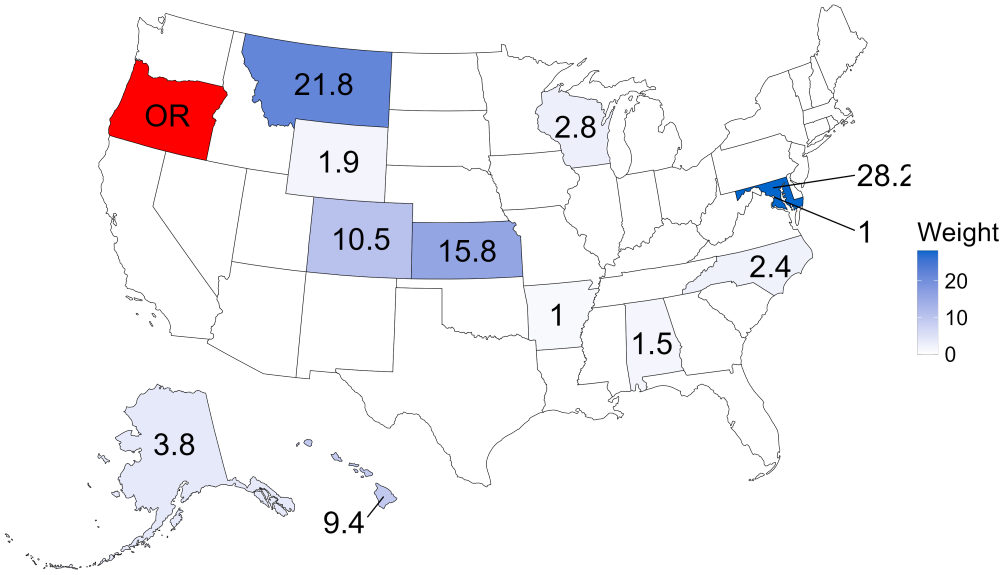


(b) Differences btwn Definition

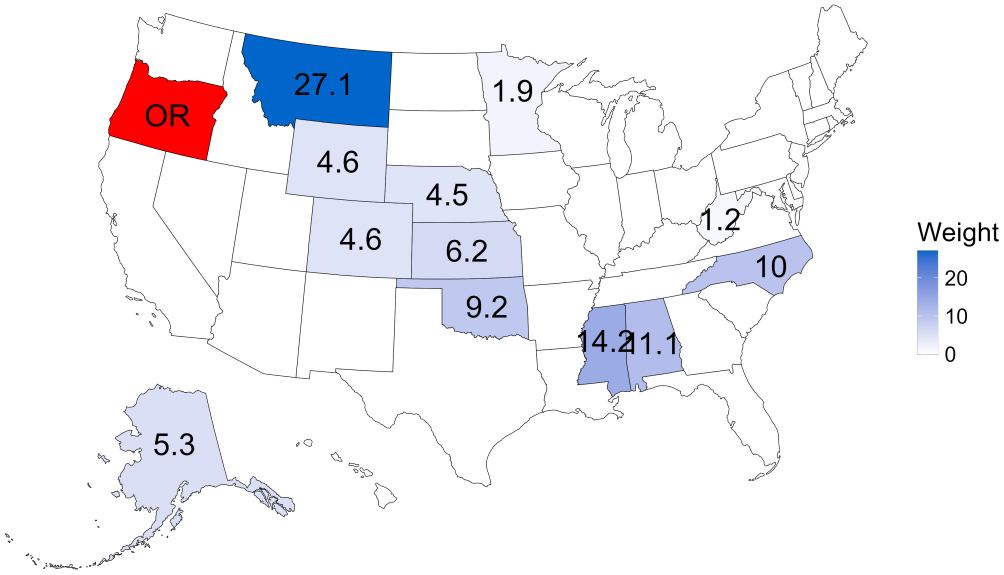


Appendix Figure 2: Maps of our Synthetic Control Weights

(a) Replication of Spencer's work.

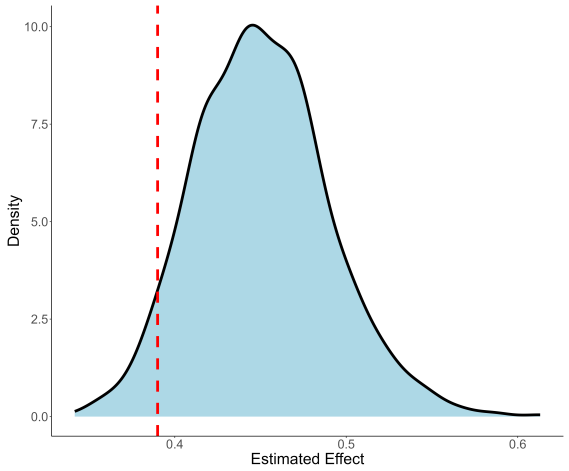


(b) Replication of Joshi et al. work.

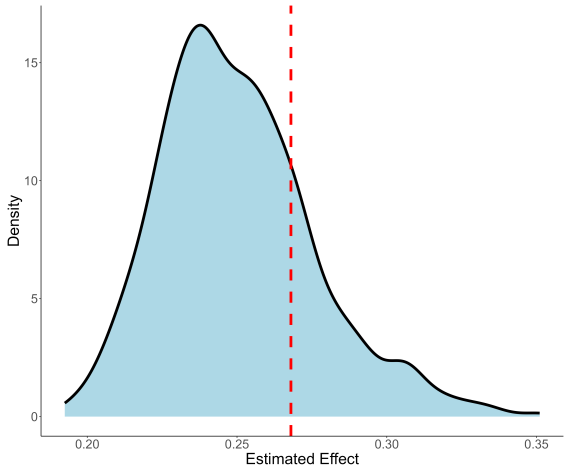


Appendix Figure 3: Simulated Effect (1000 times) of Random Imputation of Overdose

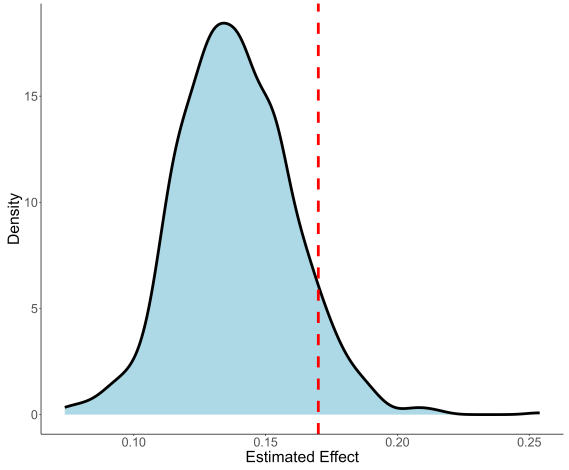
(a) Spencer Specification: Oregon



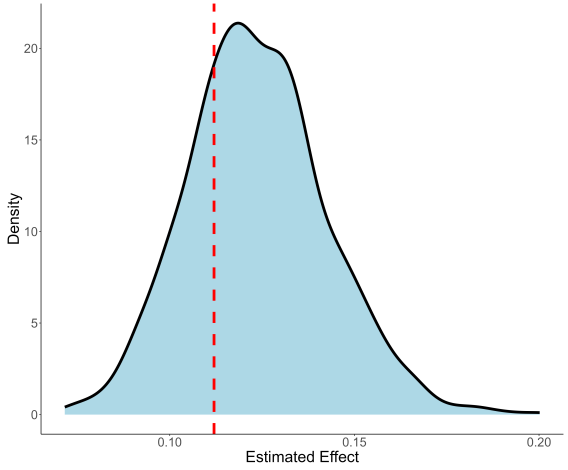
(b) Joshi et al. Specification: Oregon



(c) Spencer Specification: Washington



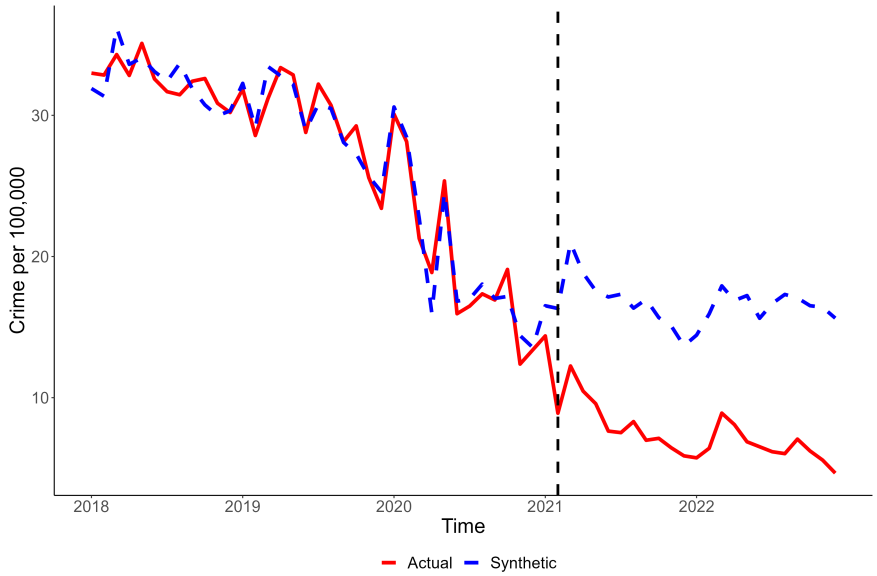
(d) Joshi et al. Specification: Washington



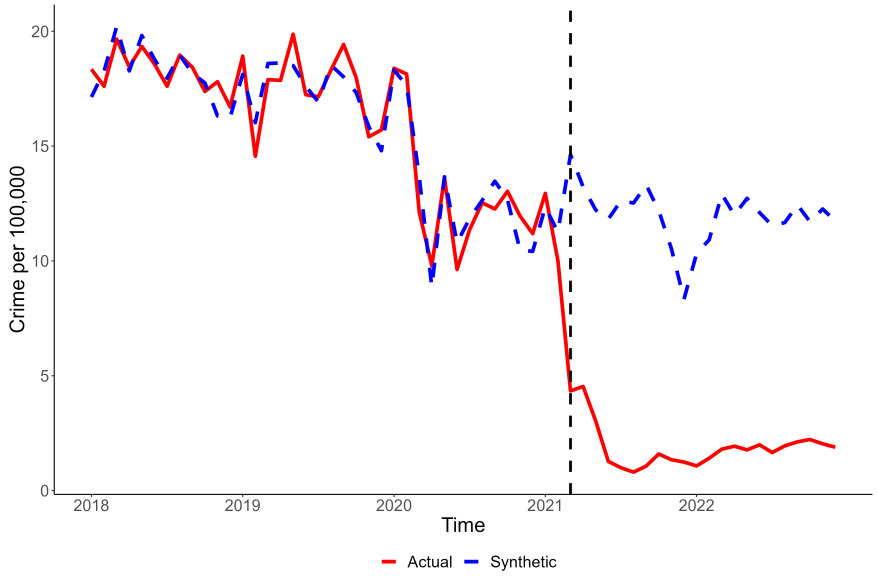
Notes: These figures show simulated ATEs for 1,000 different imputed values for state-month where fewer than 9 overdoses were reported. The red dashed line shows the average treatment effect reported by Spencer (2023) or Joshi et al. (2023).

Appendix Figure 4: NIBRS Estimate, Arrests

(a) Oregon



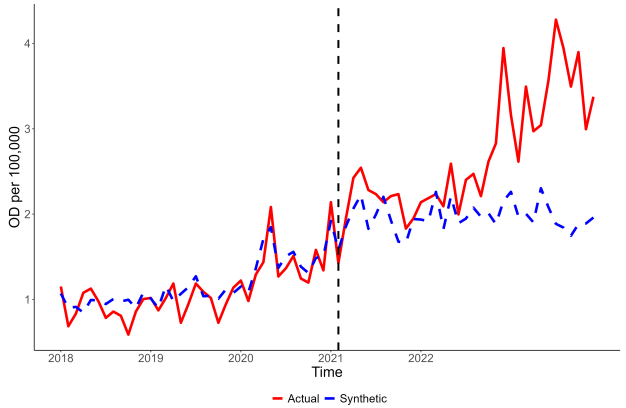
(b) Washington



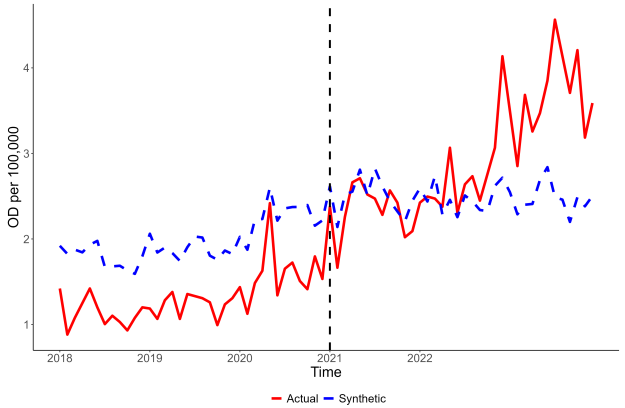
Notes: Synthetic Control Estimates for reported arrests are shown. The data is obtained from 2018-2022 NIBRS. Our sample is restricted to agencies that report all 60 months and aggregated to a state-by-month panel.

Appendix Figure 5: Synthetic Control Estimate: Forcing Same Weights

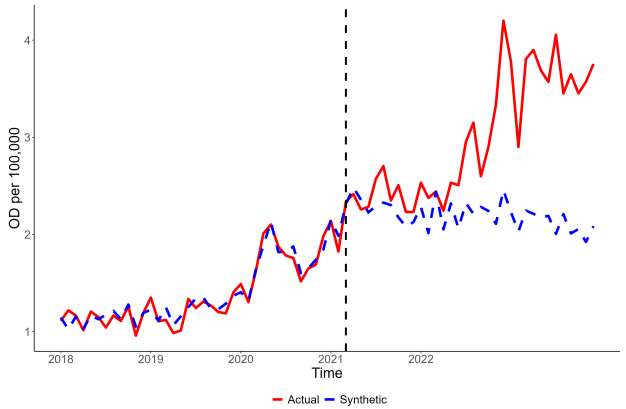
(a) Spencer Outcome Using Joshi et al. Weights: Oregon



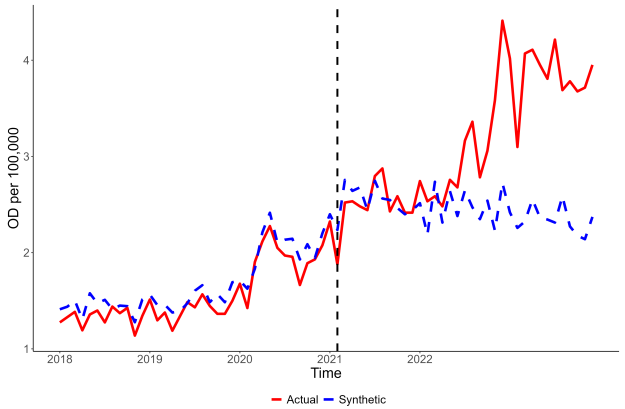
(b) Joshi et al. Outcome Using Spencer Weights: Oregon



(c) Spencer Outcome Using Joshi et al. Weights: Washington



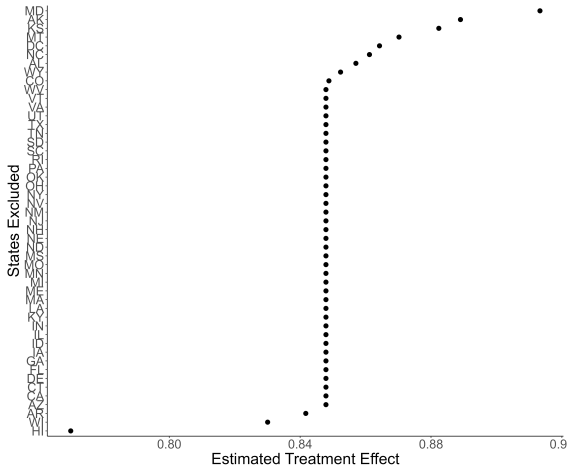
(d) Joshi et al. Outcome Using Spencer Weights: Washington



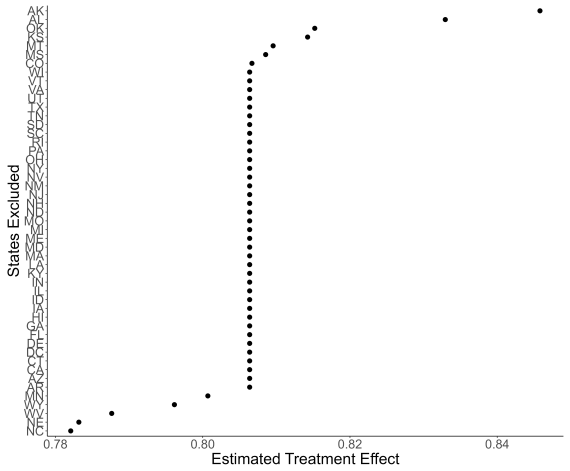
Notes: These figures are generated using synthetic control weights for the other outcome.

Appendix Figure 6: Synthetic Control Estimate: Dropping One Control Units

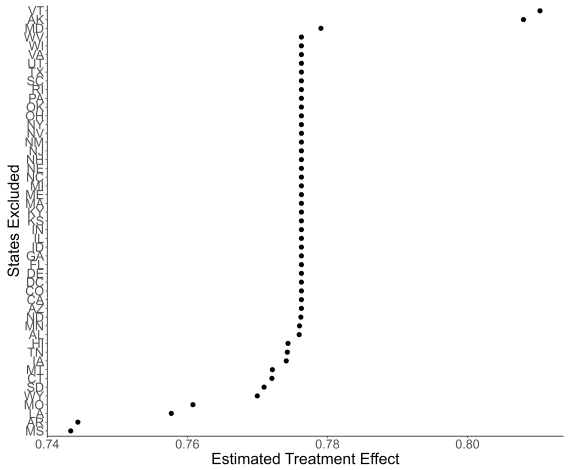
(a) Spencer Specification, Oregon



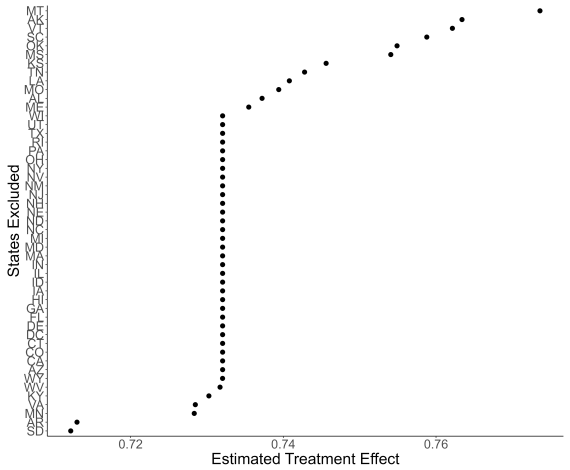
(b) Joshi et al. Specification, Oregon



(c) Spencer Specification, Washington

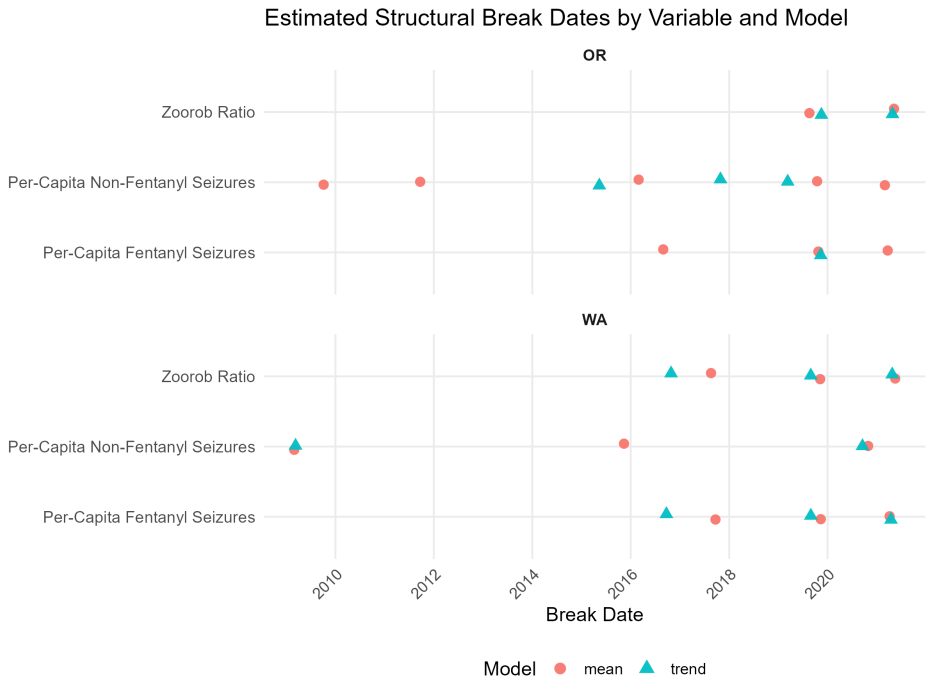


(d) Joshi et al. Specification, Washington



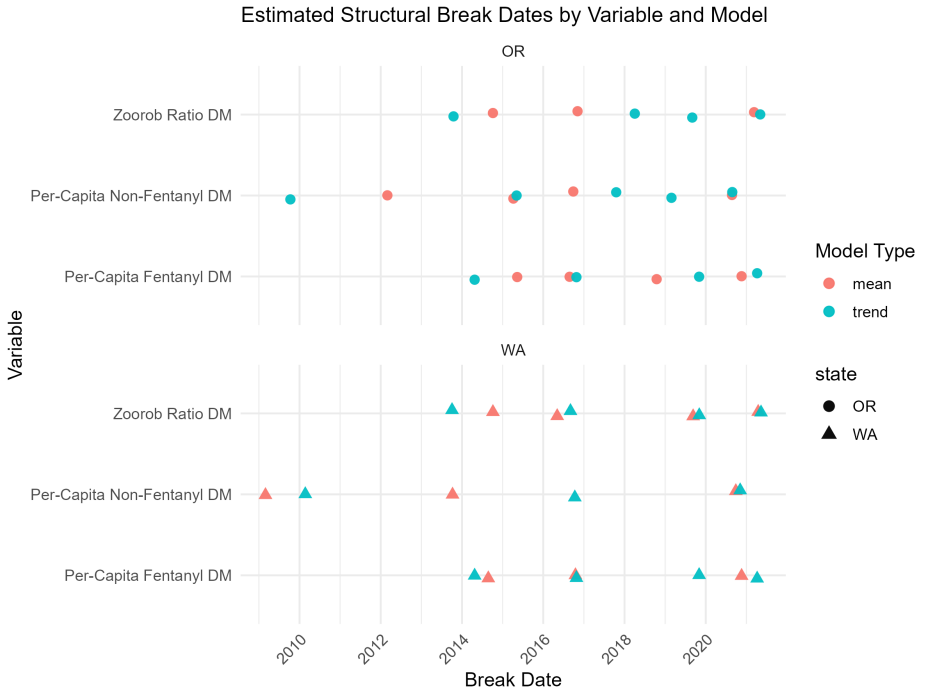
Notes: These figures provide the estimated ATTs when we drop one state from our donor pool.

Appendix Figure 7: Bai-Perron Multiple Break Tests for All Measures



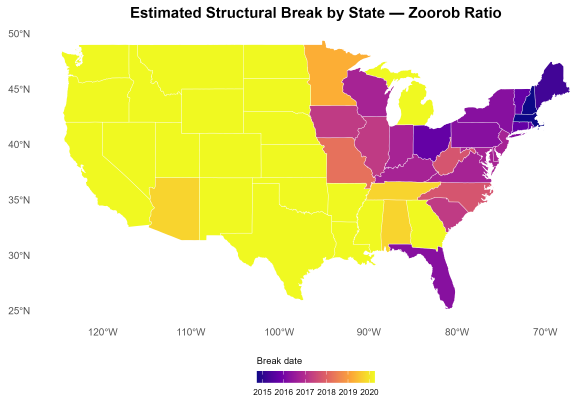
Notes: Results from the Bai-Perron structural break testing allowing for multiple breaks in mean as indicated by red circles, and trend as indicated by blue triangles. The top three models test the univariate time series for Oregon, and the bottom three test the univariate time series for Washington.

Appendix Figure 8: Bai-Perron Multiple Break Tests for Demeaned Measures

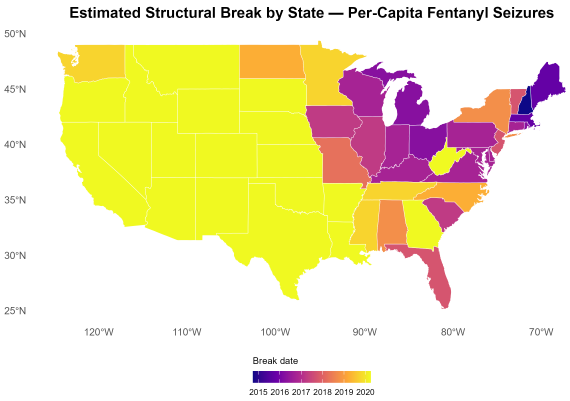


Notes: Results from the Bai-Perron structural break testing allowing for multiple breaks in mean as indicated by red circles, and trend as indicated by blue triangles. The top three models test the univariate time series for Oregon, and the bottom three test the univariate time series for Washington, both demeaned by teh average of the rest of the nation.

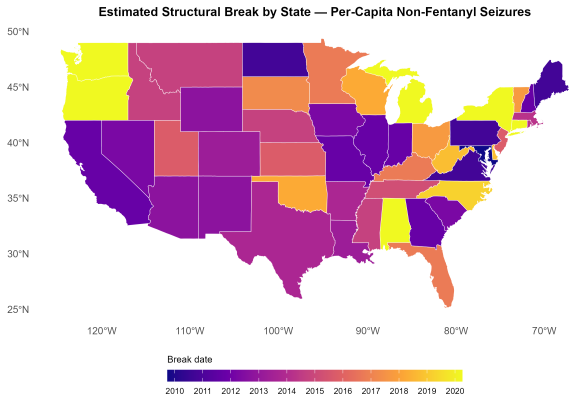
Appendix Figure 9: Estimated Structural Break Dates by State



(a) Break dates using the fentanyl-to-all-drugs ratio (Zoorob-style).



(b) Break dates using fentanyl seizures per capita.



(c) Break dates using non-fentanyl seizures per capita.

Notes: Each panel maps the estimated single structural break date by state for the indicated series. Break dates are shown using a continuous date scale. All analyses used a single structural break estimated using a sup-F test.

7.2 Appendix Tables

Appendix Table 1: Synthetic Control Estimate Drugs Seized, NFILS

	Fentanyl (1)	Non Fentanyl (2)	All Drugs (3)	Fentanyl Ratio (4)
	Panel I: Oregon			
ATT	-10.814	-55.083***	-66.846*	17.382***
P-Val	{0.18}	{0.02}	{0.06}	{0.02}
	Panel II: Washington			
ATT	-13.615	-23.151***	-17.668***	15.329*
P-Val	{0.2}	{0.02}	{0.04}	{0.08}

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: Synthetic control estimates are shown. All estimates use biannual data from 2010 to 2020 to find the synthetic weights. The p-values, calculated via a permutation test, are provided inside the curly brackets.

Appendix Table 2: Synthetic Control Estimates with Prediction & Confidence Intervals, Replication

	Oregon		Washington	
	Panel I: Prediction Intervals			
ATT	0.406**	0.264	0.107*	0.098
	[0.13,0.68]	[-0.2,1.01]	[-0.01,0.23]	[-0.08,0.27]
	Panel II: Chernozhukov et al. (2018) T-Test Procedure			
ATT	0.429*	0.331	0.175	0.192
	[0.03,0.83]	[-0.01,0.67]	[-0.01,0.36]	[-0.1,0.48]
Specification	Spencer	Joshi	Spencer	Joshi

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: In panel I, we report the 90 percent prediction intervals (Cattaneo et al., 2021). In panel II, we report the 90 percent confidence intervals calculated using the K=3-fold cross-validation technique (Chernozhukov et al., 2024). These intervals are reported inside the square brackets.

Appendix Table 3: Robustness: Other Estimator

	Aug Synth		SDiD		GSynth		Multi Outcome SC		Unweighted TWFE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					Panel A: Oregon					
ATT Overall	0.85*	0.806**	0.866**	0.81**	0.811***	0.838***	0.817*	0.866*	0.848	0.883*
P-Val	{0.09}	{0.024}	{0.015}	{0.04}	{0}	{0}	{0.06}	{0.06}	0.108	0.094
					Panel B: Washington					
ATT Overall	0.775***	0.733***	0.799**	0.859**	0.801***	0.836***	0.775***	0.756***	0.853*	0.888*
P-Val	{0.001}	{0}	{0.029}	{0.036}	{0}	{0}	{0.005}	{0.002}	0.095	0.093
Specification	Spencer	Joshi	Spencer	Joshi	Spencer	Joshi	Spencer	Joshi	Spencer	Joshi

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: Columns (1) and (2) report results from augmented synthetic control; columns (3) and (4) report results from synthetic difference-in-differences method; columns (5) and (6) report results from generalized synthetic control method; and columns (7) and (8) report results from two-way fixed effects estimator.

Appendix Table 4: Robustness: Forcing Same Weights & Estimating Difference-in-Differences

	Oregon				Washington			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT Overall	0.771*	0.777*	1.089***	1.079***	0.761*	0.792*	0.795*	0.832*
P-Val	{0.06}	{0.06}	{0.04}	{0.04}	{0.06}	{0.06}	{0.06}	{0.06}
Outcome	Spencer	Spencer	Joshi	Joshi	Spencer	Spencer	Joshi	Joshi
Weight	Joshi	Joshi	Spencer	Spencer	Joshi	Joshi	Spencer	Spencer
Treatment	Jan	Feb	Jan	Feb	Feb	Mar	Feb	Mar

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: Results from two-way fixed effects estimator are shown. Each regression is weighted via synthetic control weights. Permutation-based p-values are reported inside the curly brackets.

Appendix Table 5: Synthetic Control Confidence Intervals, Full Period

	Oregon		Washington	
	Panel I: Prediction Intervals			
ATT	0.875**	0.784**	0.759**	0.711**
	[0.6,1.15]	[0.32,1.61]	[0.62,0.9]	[0.54,0.88]
	Panel II: Chernozhukov et al. (2018) Standard Errors			
ATT	0.86**	0.878**	0.768***	0.816**
	[0.49,1.23]	[0.5,1.26]	[0.7,0.83]	[0.55,1.09]
Specification	Spencer	Joshi	Spencer	Joshi

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: In panel I, we report the 90 percent prediction intervals (Cattaneo et al., 2021). In panel II, we report the 90 percent confidence intervals calculated using the K=3-fold cross-validation technique (Chernozhukov et al., 2024). These intervals are reported inside the square brackets.

Appendix Table 6: Synthetic Control Estimate Excluding COVID-19

	Oregon		Washington	
	(1)	(2)	(3)	(4)
ATT	0.878**	0.889**	0.809**	0.954**
P-Val	{0.02}	{0.02}	{0.02}	{0.02}
Data	Spencer	Joshi	Spencer	Joshi

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: Synthetic control estimates are shown. All estimates use January 2018 to February 2020 to find the synthetic weights. Columns 1 and 3 focus on all unintentional drug overdoses, which is the outcome used by Spencer (2023). Columns 2 and 4 focus on all drug mortality, which is the outcome used by Joshi et al. (2023). The p-values, calculated via a permutation test, are provided inside the curly brackets.

Appendix Table 7: Placebo Test: West Coast State

	Spencer (2023)			Joshi et al. (2023)		
	CA	ID	NV	CA	ID	NV
ATT	0.135	-0.089	0.418	0.176	-0.132	0.255
P-Val	{0.408}	{0.796}	{0.367}	{0.327}	{0.776}	{0.429}

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01

Notes: Synthetic control estimates are shown. All estimates use biannual data from 2010 to 2020 to find the synthetic weights. The p-values, calculated via a permutation test, are provided inside the curly brackets.