

# Links between COVID-19 lockdowns and drug overdose deaths, evidence from panel data\*

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## Abstract

This paper studies the impacts of divergent state government responses to the COVID-19 pandemic on drug overdose deaths. Using Difference-in-Differences method, I exploit the cross-state variation in the timing of lockdown policies to examine how COVID-19 lockdowns affected drug overdose deaths. To the author's knowledge, this is the first study that comprehensively examines the causal links between COVID-19 lockdowns and drug overdose deaths in the US. By emphasizing the impacts of lockdown "mandates" vs. "recommendations," I find credible evidence supporting that the implementation of lockdown mandates led to more drug overdose deaths. My baseline estimates indicate a 15% to 20% increase in drug overdose deaths. To make my results generalizable, I examine three potential mechanisms, including labor market outcomes, mental health, and isolation. I find that isolation induced by the COVID-19 lockdown mandates likely contributes to the increase in drug overdose deaths, while there is only weak evidence that labor market outcomes and mental health serve as plausible channels.

**Keywords:** Opioid, COVID-19, Overdose, Substance Abuse, Mortality

**JEL Codes:** I18, J08, R10

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# 1 Introduction

Illicit drug use has become a major public health issue. Previous policy reports have documented a sharp increase in the number of drug overdose deaths in recent years. Hedegaard et al. (2021) estimate a 31% (from 21.6 per 100,000 to 28.3 per 100,000) increase in drug overdose death rates from 2019 to 2020. This uprising trend continued in 2021 (from 28.3 per 100,000 to 32.4 per 100,000) and 2022 (from 32.4 per 100,000 to 32.6 per 100,000) (Spencer et al., 2022, 2023). Additionally, Hedegaard et al. (2021) estimate a 56% (from 11.4 per 100,000 to 17.8 per 100,000) increase in drug overdose death rates associated with at least one type of synthetic opioid (fentanyl) excluding methadone from 2019 to 2020. Similarly, Friedman et al. (2022) estimate that the mortality rates of drug use increased by 29% (from 21.52 per 100,000 to 27.86 per 100,000) in 2020 and 11% (from 27.86 per 100,000 to 31.06 per 100,000) in 2021. Lui et al. (2022) estimate that the “years of potential life lost” and the loss of “value of statistical life” from drug overdose deaths both increased nearly 30%, reaching 3,429,140 years and \$825.31 billion. However, these studies only examine the increase in drug overdose deaths before and after the COVID-19 lockdown while neglecting the geographic variations in lockdown policies. I contribute to the literature as the first paper to examine how variations in lockdown policies across states have impacted drug overdose deaths.

As one of the most severe public health crises in decades (Beach et al., 2022), the COVID-19 pandemic resulted in thousands of extra deaths (Lee et al., 2022; Wang et al., 2022). To curb the spread, the federal and state governments implemented various health measures including stay-at-home orders. This sparks copious policy debates. Advocates of COVID-19 lockdown policies usually highlight public health concerns (Joffe, 2021). At the same time, COVID-19 lockdowns give rise to other health issues like deterioration of mental health and an increase in suicides (Joffe, 2021).

The impacts of COVID-19 lockdowns on drug overdose remain understudied. I focus on examining the causal effects of COVID-19 lockdown “mandates” vs. “recommendations” on drug overdose deaths. Hale et al. (2021) provide one of the most comprehensive records of government policies implemented in response to COVID-19. In which, they record stay-at-home orders that prohibit individuals from leaving home, with minimal exceptions like “daily exercise”, “grocery shopping”, and “‘essential’ trips”. We exploit the variations of government policies at the state level to elicit the impacts of COVID lockdowns on drug overdose deaths.

I first use Difference-in-Differences (DiD) models to elicit the average treatment effects of stay-at-home mandates on drug overdose deaths. Using TWFE models, I find that COVID-19 lockdowns increased drug overdose deaths by 15% to 20%. Upon recent development, I use methods suggested by Callaway and Sant’Anna (2021); Wooldridge (2021, 2023) and estimate a 12% to 21% increase in drug overdose deaths.

With concerns that relying on a single lockdown criterion might contain bias, I further supplement my results using another widely-used lockdown criterion by Moreland et al. (2020) in Appendix A.1. I find estimates with consistent signs, although they are not statistically significant. However, the results are generally consistent with my baseline results after excluding states with ambiguous policies. This implies that the effects are likely underestimated by the “borderline” states that are ambiguous on lockdown mandates.

To make a causal claim, the parallel trends assumption must hold. I check the parallel trends assumption using traditional TWFE models and event study models suggested by Sun and Abraham (2021). I find some negative pre-treatment effects using both OLS and Poisson regression models , while I still observe positive treatment effects under the assumption that pre-treatment patterns continue (see Rambachan and Roth (2023)). I further supplement my results using generalized synthetic control methods by Xu (2017), which allows me to relax my parallel trends assumption. I find a 19% to 20% increase in drug overdose deaths using Xu’s (2017) method, consistent with my previous findings.

I examine three potential channels, including labor market outcomes, mental health, and isolation, through which COVID-19 lockdown mandates could potentially affect drug overdose deaths. I use the unemployment rate as a proxy for labor market outcomes. Using self-reported mental health from BRFSS, I examine whether changes in mental health outcomes contribute to drug overdose deaths. Additionally, using Google Community Mobility Reports data, I examine whether isolation/loneliness serves as a plausible channel. My results indicate that the COVID-19 lockdown mandates lead to fewer visits to public places and increase the amount of time spent in residential areas, although the latter effect is not statistically significant. This suggests that the lockdown induced isolation likely serves as a channel that affects drug overdose deaths. At the same time, the channels through labor market outcomes and mental health is weak. I further assess the potential mediating effects of these channels, as detailed in Appendix A.2.

To the author’s knowledge, there has been no research that examines how cross-state differences in policy responses to the COVID-19 pandemic impacted drug overdose deaths. My research contributes to two growing strands of literature. First, my research extends existing literature on the collateral consequences of COVID-19 lockdowns (Bullinger et al., 2021; Aknin et al., 2022; García-Prado et al., 2022; Ravindran and Shah, 2023; Marquez-Padilla and Saavedra, 2022; Hoehn-Velasco et al., 2021; Dang and Trinh, 2021; Barnes et al., 2022; Sung et al., 2024). Second, my research adopts similar empirical strategies and further examines the effects of public policies on drug overdose (Doleac and Mukherjee, 2022; Packham, 2022; Kim, 2021; Averett et al., 2019; Rees et al., 2019; Mathur and Ruhm, 2023; Popovici et al., 2018; Dowd, 2023).

The paper is organized as follows. Section 2 describes findings from related studies. Section 3 describes the data. Section 4 discusses the empirical strategies. Section 5 presents the regression results. Section 6 presents robustness checks. Section 7 presents potential mechanisms. Section 8 concludes my paper.

## 2 Literature Review

The federal and state governments implemented policies including stay-at-home orders, and income support, along with health measures like mandating the use of masks and social distancing. While previous research generally finds that policies related to mandating social distancing curbed the transmission of the virus (Murphy et al., 2023), the COVID-19 lockdown has led to unintended public health consequences. Bullinger et al. (2021) find that the COVID-19 lockdowns led to more domestic violence and less police enforcement. Sung et al. (2024) find that the COVID-19 lockdowns led to higher Body Mass Index (BMI). Aknin et al. (2022); García-Prado et al. (2022) find that stringent lockdowns were associated with a series of mental health issues. Ravindran and Shah (2023) find that the COVID-19 lockdown caused an increase in domestic violence and cyber-crime complaints in India, while the closure of public spaces following the lockdowns was associated with fewer sexual and rape complaints. Marquez-Padilla and Saavedra (2022) suggest that the COVID-19 lockdowns led to fewer abortions and abortion related government “hot-line” calls in Mexico. Hoehn-Velasco et al. (2021) find that crimes against women first decreased and then bounced back to pre-pandemic levels in Mexico. Hoehn-Velasco et al. (2021) identify some pathways, including bans on alcohol sales and risks of COVID-19 infection, that might lead to the reduction of crimes. COVID-19 lockdowns have also led to better global air quality (Dang and Trinh, 2021), and fewer traffic accidents (Barnes et al., 2022).

Previous research identifies two drivers of social distancing during the COVID-19 pandemic: (1) individuals' voluntary actions to reduce the risks of virus infection; and (2) governments' lockdown policies to reduce social activities (Goolsbee and Syverson, 2021; Durante et al., 2021; Gupta et al., 2021). Voluntary social distancing is found to be positively associated with individuals' pro-social behaviors (Durante et al., 2021; Barrios et al., 2021; Campos-Mercade et al., 2021). Using mobile device data provided by SafeGraph, Brzezinski et al. (2020) find that voluntary social distancing is determined by social characteristics like trust in science, education, and income. Similarly, using “smartphone location data,” Besley and Dray (2022) find that social distancing is determined by social characteristics like partisan leanings, household incomes, health insurance coverage rates, etc. Allcott et al. (2020) find that, in Democratic-leaning areas, people tended to engage in social distancing more than those in Republican-leaning areas.

Some regional studies find that COVID-19 lockdowns were associated with higher numbers of drug overdose deaths in Milwaukee County, Wisconsin (Ghose et al., 2022), Cook County, Illinois (Mason, 2021; Delcher et al., 2022), San Francisco, California (Appa et al., 2021), and the state of Louisiana (Leonhardt et al., 2023). Root et al. (2021); Rosenbaum et al. (2021) find that the pandemic has heterogeneous effects on the number of cases of Emergency Department visits. However, it is unclear how the variations of

COVID-19 response policies, especially those differing across states, impacted drug overdose deaths.

There are several channels through which COVID-19 lockdowns could have affected drug overdose deaths. First, the prevalence of drug use is generally found to be counter-cyclical (Nagelhout et al., 2017). Ruhm (2019) finds weak evidence that drug overdose deaths are correlated with counties' economic downturns. Additionally, Hollingsworth et al. (2017) find a relatively robust relationship between unemployment rates and drug overdose deaths and Emergency Department visits. Carpenter et al. (2017) find evidence that economic recessions led to more illicit drug use. Baek et al. (2021) find that COVID-19 stay-at-home orders lead to more unemployment insurance claims. The COVID-19 lockdown mandates likely harm the economy in general (see Bairoliya and İmrohoroglu (2023); Kok (2020)) and have an impact on drug overdose deaths (Hollingsworth et al., 2017; Carpenter et al., 2017). Furthermore, (Pieh et al., 2021) find mental health deteriorates during COVID-19 lockdown. These suggests that mental health might impact drug overdose deaths (Aknin et al., 2022; García-Prado et al., 2022).

## 3 Data

### 3.1 Drug Overdose Data

The overdose death data is retrieved from CDC WONDER (CDC, 2024). The CDC WONDER database provides aggregate data on a diverse selection of public health and epidemic-related issues. Specifically, this paper focuses on drug overdose deaths. I obtain the numbers of deaths by state from 2019 to 2022 with drug overdose as the underlying cause of death. CDC suppressed data entries with fewer than 10 cases. For example, if a state records fewer than 10 drug overdose deaths in a given month, the CDC does not report the exact number of deaths. I impute the suppressed monthly overdose deaths with 5 cases.<sup>1</sup> To provide a clearer picture of the data, I present the average drug overdose death rates in treated and non-treated states in Figure 1.

### 3.2 Lockdown Criteria

I use the criterion of "mandates" vs. lockdown "recommendations" by Hale et al. (2021) for my baseline analysis. Hale et al. (2021) publish the "Oxford COVID-19 Government Response Tracker (OxCGRT)," which documented government policies at different levels of government in 183 countries from January

<sup>1</sup>I also adopt alternative imputation methods to cross-check the robustness of my models. Firstly, I impute the suppressed monthly overdose deaths with 1 case. Secondly, I impute the suppressed monthly overdose deaths with 9 cases. Thirdly, I impute the suppressed monthly overdose deaths using a discrete uniform distribution. The results are presented in Appendix A.3. At least one month data was suppressed in South Dakota, Wyoming, North Dakota, Montana, Nebraska, Alaska, and Vermont during my analysis period.

2020 to December 2022. Hale et al. (2021) track the implementations of 20 individual policies in ordinal scales. The policy index of my interests is the “stay-at-home requirements” index, as this index is more likely to capture the impacts on health. Hale et al. (2021) code the “stay-at-home requirements” index into 4 levels from the most stringent to the most lenient. The most stringent level prohibits individuals from going out with very limited exceptions. There were no occasions in the US that any state implemented stay-at-home orders of this level. The second highest stringent level prohibits individuals from going out with some exceptions such as “daily exercise,” “grocery shopping,” and “‘essential’ trips” (Hale et al., 2021). The third highest stringent level only recommends individuals not going out, but shelter-in-place is not required. The fourth stringent level indicates that there are no policies regarding stay-at-home orders at all. The states that adopted the second highest “stay-at-home requirements”<sup>2</sup> at any point of the pandemic are flagged as the treatment group. And the states that did not adopt the second highest “stay-at-home requirements” are treated as the control group. According to Hale et al. (2021), 9 states, which include Arkansas, Connecticut, Iowa, Massachusetts, New Mexico, North Dakota, Utah, South Dakota, and Wyoming have never rolled out the stay-at-home mandates; 2 states (Mississippi and Nevada) rolled out their first stay-at-home mandates in April of 2020 instead of March; 1 state (Nebraska) rolled out the stay-at-home mandates in December 2020.<sup>3</sup> The timing of the stay-at-home orders is provided in Table 1.<sup>4</sup>

### 3.3 Summary Statistics

My control variables include the annual median age data from the Census Bureau, annual proportion of the population being non-Hispanic White from the Census Bureau, annual proportion of the population living below the poverty level from the Census Bureau, and annual proportion of the population with lower than high school degrees from Census Bureau. These variables are observed at the state level. The summary statistics from 2019 to 2022 are provided in Table 2.

## 4 Empirical Strategies

I first use DiD models to estimate the lockdown mandates’ effects on drug overdose deaths. Next, I estimate the effects of staggered adoptions of stay-at-home mandates on drug overdose deaths. I then utilize traditional event study models and the methods suggested by Callaway and Sant’Anna (2021), Wooldridge (2021, 2023), and Sun and Abraham (2021). After that, I plot dynamic effects to show evidence of paral-

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<sup>2</sup>As Hale et al. (2021) noted, this level of “stay-at-home requirements” imposed mandates instead of recommendations.

<sup>3</sup>As there is only one state (Nebraska) that initiated its stay-at-home order in December 2020, I drop Nebraska from my baseline analysis. I include the results with Nebraska in Appendix A.6.

<sup>4</sup>As a general assumption, I assume there is no reversal of treatment for my baseline analysis. I examine the case that treatment turns off after the states lifted the stay-at-home orders, as presented in Appendix A.4.

lel trends and further confirm my results using synthetic control methods. Lastly, I perform a drop-one analysis to test the robustness of my models.

## 4.1 DiD Methods

I proceed using DiD models to estimate the effects of stay-at-home mandates on drug overdose deaths. Past literature demonstrates that the impacts associated with economic policies or events might remain after decades (Chen and Zhou, 2007; Dore and Wurapa, 2024). These might partially explain why the number of drug overdoses remained high after the policies related to the COVID-19 pandemic were lifted. A unique advantage of DiD models is that it compares the post-treatment outcome and pre-treatment outcome, which allows me to capture the pertaining effects of COVID-19 lockdowns. To run DiD models, I need adequate observations in pre-treatment periods to ensure that the treatment group and control group follow similar trends. I select the periods from January 2019 to December 2022. I first use monthly data to conduct my analysis and further use quarterly data to probe the robustness in Appendix A.5.

The selection of functional forms requires careful consideration. I propose two strategies for conducting the analysis using an OLS model and a Poisson regression model. For the OLS model, I use log-transformed drug overdose death rates per 10,000 as my dependent variable, allowing us to interpret the marginal effects as the percentage change in drug overdose deaths. For the Poisson regression model, I include the state's population as an offset variable, which transforms the marginal effects in percentage terms. I find support for these strategies in recently published studies. For instance, Doleac and Mukherjee (2022); Packham (2022); Kim (2021); Averett et al. (2019); Rees et al. (2019) adopt drug mortality rates as their dependent variables, and examine the impacts of naloxone access laws (Doleac and Mukherjee, 2022), syringe exchange programs (Packham, 2022), prescription drug monitoring programs (PDMPs) (Kim, 2021), Medicaid expansion laws (Averett et al., 2019), and Good Samaritan laws (Rees et al., 2019) on drug-related mortality rates. On the other hand, some previous research utilize Poisson regressions with raw counts of deaths as dependent variables and population as exposure variable to examine the effects of marijuana legalization (Mathur and Ruhm, 2023), "pain management clinic and doctor shopping laws" (Popovici et al., 2018), Good Samaritan laws (Rees et al., 2019), and "untargeted naloxone distribution" (Dowd, 2023) on drug overdose deaths. Marquez-Padilla and Saavedra (2022) examine the effects of the COVID-19 lockdowns on abortions using 4 types of dependent variable transformations, including natural logs, quadratic roots, inverse hyperbolic transformation, and rates per 1,000 population. Previous studies often face issues when the outcome variables contain a large number of zero observations. For instance, Packham (2022) deals with zero observations and applies inverse hyperbolic sine transformation for the outcome variables. However,

this is not a concern for my paper, as it is unlikely that any state reports zero cases of drug overdose deaths in a given month. Additionally, I use imputation methods that do not generate zero values. I adopt two baseline strategies for the following analysis. I estimate the effects of the COVID-19 lockdown on drug overdose deaths using OLS methods in Equation (1) and Poisson regression in Equation (2).

I proceed with the following DiD models,

$$\log(y_{it}) = \beta DiD_{it} + \zeta Z_{it} + \gamma_i + \delta_t + \epsilon_{it} \quad (1)$$

$$Y_{it} = P_{it} \exp(\beta DiD_{it} + \zeta Z_{it} + \gamma_i + \delta_t + \epsilon_{it}) \quad (2)$$

where  $\log(y_{it})$  indicates the natural log of drug overdose deaths per 10,000 population in state  $i$  and time  $t$ .  $Y_{it}$  indicates the raw count of drug overdose deaths.  $\beta$  is my estimator of lockdown mandates' effects on drug overdose deaths.  $DiD$  is a dummy variable that is set to 1 starting the first treatment periods in state  $i$ .<sup>5</sup>  $P_{it}$  is the population in state  $i$  and time  $t$ .  $Z_{it}$  is a vector of control variables including median age, proportion of the population being non-Hispanic White, proportion of the population below the poverty level, and proportion of the population with lower than high school degree.  $\zeta$  contains coefficients associated with the controls.  $\gamma_i$  and  $\delta_t$  are state fixed-effects and time fixed-effects respectively to control unchanging aspects of the states and time trend.

## 4.2 Event Studies

I estimate the dynamic effects of stay-at-home orders on drug overdose deaths. Following previous research, I proceed estimating the dynamic effects using OLS method in Equation (3) and Poisson regression in Equation (4),

$$\log(y_{it}) = \sum_{k \neq -1} \beta_k D_{it}^k + \zeta Z_{it} + \gamma_i + \delta_t + \epsilon_{it} \quad (3)$$

$$Y_{it} = P_{it} \exp\left(\sum_{k \neq -1} \beta_k D_{it}^k + \zeta Z_{it} + \gamma_i + \delta_t + \epsilon_{it}\right) \quad (4)$$

where  $\beta_k$  is the event study coefficient with  $\beta_{-1}$  removed due to collinearity.  $k$  is the relative time that equals the current period minus the time of its first treatment. Thus, the event study coefficients are centered at the first treatment of that state.  $D_{it}^k$  is a dummy variable where  $D = 1$  when the real time period  $t$  is  $k$  period away from its first treatment in state  $i$ . Also, I run the Poisson regression model using raw counts of drug

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<sup>5</sup>This term is equivalent to the interaction of the treatment group and post-treatment periods. I set the post-treatment periods to 1 starting the states' first treatment. I assume the treatment cannot be reversed.

overdose deaths  $Y_{it}$  as the dependent variable, and state population  $P_{it}$  as the offset variable.

As Baker et al. (2022); Borusyak et al. (2024); Callaway and Sant'Anna (2021); Goodman-Bacon (2021) point out, the traditional TWFE estimates could be biased while heterogeneous treatment effects are present. To address these concerns, I use methods suggested by Callaway and Sant'Anna (2021) and Wooldridge (2021, 2023). I report the Average Treatment Effects on the Treated (ATT) using these two methods. Furthermore, I adopt a drop-one analysis to ensure that my results are not driven by any single state.

## 5 Main Results

### 5.1 Difference-in-Differences

In Table 3 Panel A, I present my OLS estimates of stay-at-home mandates' effects on drug overdose deaths. Column (1) reports my OLS estimates without control variables. I find that the COVID-19 lockdowns led to a 19% increase in drug overdose deaths. Column (2) reports my OLS estimates with control variables included. I find that the COVID-19 lockdowns led to a 19% increase in drug overdose deaths. Similar to my previous specification, I present estimates of Poisson regression Panel B. I find that the COVID-19 lockdowns led to a 20% ( $\exp(0.182) - 1$ )<sup>6</sup> increase in drug overdose deaths. I present the estimates of Poisson regression with control variables included in Column (4). I find that the COVID-19 lockdowns led to a 15% ( $\exp(0.142) - 1$ ) increase in drug overdose deaths. All four estimates of ATT are significant at the 95% level.

### 5.2 Event Studies and Parallel Trends Assumption

To make a causal claim, the parallel trends assumption must hold. While the parallel trends assumption is not testable, it is common to find validity in pre-treatment periods. As seen in Figure 2, I plot the dynamic effects of the stay-at-home mandates on drug overdose deaths. I use both the traditional TWFE approach and the approach suggested by Sun and Abraham (2021). I plot the OLS estimates of stay-at-home mandates on drug overdose deaths in the top panel. Although not significant, there are some concerns about the negative trends before  $t=-5$  in Figure 2, which could potentially violate the parallel-trends assumption. I plot the effects of stay-at-home mandates on drug overdose deaths using the Poisson regression model in the bottom panel. Similarly, I find some negative pre-treatment effects prior to  $t=-5$ . I further address this concern by using a generalized synthetic control method by Xu (2017). Additionally, I observe an obvious uprising trend in treatment effects after treatment. The treatment effect is most pronounced at

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<sup>6</sup>See Wooldridge (2009).

approximately 3 months following the lockdown, suggesting a relatively immediate impact.

## 6 Robustness Checks

In Table 4, I present the ATTs using Callaway and Sant’Anna (2021) and Wooldridge (2021, 2023) methods. I present the ATTs using Callaway and Sant’Anna (2021) without covariates in Column (1) and with covariates in Column (2). I find that using Callaway and Sant’Anna (2021), COVID-19 lockdowns lead to a 12% to 15%<sup>7</sup> increase in drug overdose deaths.

I present the ATTs using Wooldridge (2021, 2023) without covariates in the Middle Panel of Table 4. Without including control variables in my model, I find a 19% increase in drug overdose deaths. I find a 21% increase in drug overdose deaths using Wooldridge (2021, 2023) with control variables included in my model.

I further check the robustness of my models using the synthetic control method. Popularized by Abadie and Gardeazabal (2003); Abadie et al. (2010, 2015), synthetic control methods have been widely adopted in causal analysis. One advantage of synthetic control is that it allows us to loosen the parallel trends assumption. Given the staggered nature of treatments, I adopt a generalized synthetic control method suggested by Xu (2017). In Table 4 Bottom Panel, I present the ATTs using the generalized synthetic control method suggested by Xu (2017), where I find a 19% to 20% increase in drug overdose deaths. These results are generally consistent with my baseline analysis, suggesting robustness of my models.

I further adopt a drop-one analysis to test the robustness of my models to ensure my result is not driven by a single state. I drop the states from the sample one by one and re-estimate using my baseline models. I present my results in Figure 3. For the most part, my estimates and confidence levels are similar to what I find in the baseline analysis.

Internal migration could potentially be a confounding factor if states that implemented lockdown mandates are linked to those with higher out-migration or those receiving more migrants. If internal migration serves as a confounder, the demographic composition would likely change. I examine whether COVID-19 lockdown mandates are related to the proportion of the population without a high school degree, as education level is an important social determinant of drug overdose deaths (Powell, 2023). I present the results in Appendix A.7. My results suggest no evidence that endogenous migration would bias my estimates.

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<sup>7</sup>For ease of explaining, I interpret the effects on the log-transformed outcome variable as the percentage increases.

## 7 Mechanisms

This section assesses the potential pathways through which the COVID-19 lockdown mandates could affect drug overdose deaths. I focus on three potential mechanisms, including labor market outcomes, mental distress, and isolation.<sup>8</sup> Firstly, using the unemployment rate as a proxy, I examine whether the COVID-19 lockdown mandates led to worse labor market outcomes. Nagelhout et al. (2017); Ruhm (2019); Hollingsworth et al. (2017); Baek et al. (2021) find that drug overdose deaths are linked to economic downturns and labor market instability. Secondly, using a rich dataset from the Behavioral Risk Factor Surveillance System (BRFSS), I examine whether the COVID-19 lockdown mandates have an impact on mental health conditions. Skogen et al. (2014); Sullivan et al. (2006) find close links between mental health conditions and substance use. Thirdly, using the Google COVID-19 Community Mobility Reports Data, I examine whether the COVID-19 lockdown mandates affect mobility in public and residential areas, as Ingram et al. (2020); Roe et al. (2021) relate loneliness and isolation to worsening drug use. Additionally, I examine the mediating effects by comparing treated states that experienced larger versus smaller changes in the potential channels. The results of these mediating effects are presented in Appendix A.2.

Table 5 presents the effects of COVID-19 lockdown mandates on unemployment rates. I find some evidence indicating that the lockdown mandates increased unemployment rates, as the coefficients are positive. However, these results are indicative rather than statistically significant. Table 6 presents the effects of COVID-19 lockdown mandates on the number of days suffering from mental hardship. I find only weak evidence that COVID-19 lockdown mandates improved mental health, as they are negatively but not significantly associated with the number of days individuals experienced mental hardship. Table 7 presents the effects of COVID-19 lockdown mandates on community mobility in different places. Columns (1) to (5) report the effects of lockdown mandates on visits to public spaces, including “retail & recreation,” “grocery & pharmacy,” “parks,” “transit stations,” and “workplaces”. Column (6) reports the effects of lockdown mandates on the duration of time spent in residential areas. I find consistent signs suggesting that COVID-19 lockdown mandates decrease the number of visits to public areas according to the first five columns. On the other hand, while not statistically significant, I find an increase amount of time spent in residential areas following the stay-at-home orders.

In summary, these results point out that the COVID-19 lockdown mandates induced isolation likely contributes to the increase in drug overdose deaths. On the other hand, I find only weak evidence suggesting that the lockdown mandates increase drug overdose deaths through worsening unemployment rates. Conversely, I find some evidence that the COVID-19 lockdown mandates are associated with improved

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<sup>8</sup>Covid-19 mortality may also influence drug overdose deaths. I assess the plausibility of this relationship in Appendix A.8.

mental health. Combining with previous research linking the deterioration of mental health to drug overdose deaths (Skogen et al., 2014; Sullivan et al., 2006), it is unlikely that mental health serves as a channel leading to increased drug overdose deaths.

## 8 Discussion

The COVID-19 pandemic caused the most severe public health crisis in decades (Beach et al., 2022). In response, the federal and state governments enacted various policies to curb the spread of the virus. The effects of the COVID-19 lockdowns on drug overdose deaths remain understudied. Using state-by-month panel data, I focus on the effects of lockdown “mandates” compared to lockdown “recommendations” on drug overdose deaths. To the author’s knowledge, this is the first study that exploits the cross-state differences in the timing of COVID-19 government responses to investigate the effects on drug overdose deaths. Using Difference-in-Differences estimation, I find credible evidence that COVID-19 lockdown mandates increased drug overdose deaths. I examine potential channels through which COVID-19 lockdown mandates could affect drug overdose deaths. I find evidence suggesting that the increase in isolation/loneliness likely contributes to the rise of drug overdose deaths.

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## Tables

Table 1: Timing of Treatments

	State	(1) Hale et al. (2021)	(2) Moreland et al. (2020)	(3) Both Criteria
1	Alabama	March 2020	April 2020	X
2	Alaska	March 2020	March 2020	March 2020
3	Arizona	March 2020	March 2020	March 2020
4	Arkansas			
5	California	March 2020	March 2020	March 2020
6	Colorado	March 2020	March 2020	March 2020
7	Connecticut			
8	Delaware	March 2020	March 2020	March 2020
9	Florida	March 2020	April 2020	X
10	Georgia	March 2020	March 2020	March 2020
11	Hawaii	March 2020	March 2020	March 2020
12	Idaho	March 2020	March 2020	March 2020
13	Illinois	March 2020	March 2020	March 2020
14	Indiana	March 2020	March 2020	March 2020
15	Iowa			
16	Kansas	March 2020	March 2020	March 2020
17	Kentucky	March 2020		X
18	Louisiana	March 2020	March 2020	March 2020
19	Maine	March 2020	April 2020	X
20	Maryland	March 2020	March 2020	March 2020
21	Massachusetts			
22	Michigan	March 2020	March 2020	March 2020
23	Minnesota	March 2020	March 2020	March 2020
24	Mississippi	April 2020	April 2020	April 2020
25	Missouri	March 2020	April 2020	X
26	Montana	March 2020	March 2020	March 2020
27	Nebraska	December 2020		X
28	Nevada	April 2020	March 2020	X
29	New Hampshire	March 2020	March 2020	March 2020
30	New Jersey	March 2020	March 2020	March 2020
31	New Mexico			
32	New York	March 2020	March 2020	March 2020
33	North Carolina	March 2020	March 2020	March 2020
34	North Dakota			
35	Ohio	March 2020	March 2020	March 2020
36	Oklahoma	March 2020	March 2020	March 2020
37	Oregon	March 2020	March 2020	March 2020
38	Pennsylvania	March 2020	March 2020	March 2020
39	Rhode Island	March 2020	March 2020	March 2020
40	South Carolina	March 2020	April 2020	X
41	South Dakota		April 2020	X
42	Tennessee	March 2020	April 2020	X
43	Texas	March 2020		X
44	Utah			
45	Vermont	March 2020	March 2020	March 2020
46	Virginia	March 2020	March 2020	March 2020
47	Washington	March 2020	March 2020	March 2020
48	West Virginia	March 2020	March 2020	March 2020
49	Wisconsin	March 2020	March 2020	March 2020
50	Wyoming			

Note: This table presents the time of treatment in each state according to Hale et al. (2021) and Moreland et al. (2020). Column (1) presents the time of treatment by Hale et al. (2021). Column (2) presents the time of treatment by Moreland et al. (2020). Column (3) presents the time of treatment after excluding any state that Hale et al. (2021) and Moreland et al. (2020) disagree, denoted by X. Blank cells indicate that the state is never treated according to the criterion.

Table 2: Summary Statistics for treatment and non-treatment groups

Statistics	Non-treatment Group		Treatment Group	
	Mean	St. Dev.	Mean	St. Dev.
<b><i>Pre-treatment periods:</i></b>				
Overdose Death Rate per 10,000	0.170	0.082	0.174	0.078
Unemployment Rate	3.282	0.913	3.646	0.878
Median Age	37.271	2.745	38.772	2.146
Proportion of Population White	0.735	0.143	0.676	0.156
Poverty Rate	0.119	0.030	0.125	0.026
Proportion without Highschool Degree	0.062	0.018	0.073	0.018
N	<b>126</b>		<b>574</b>	
<b><i>Post-treatment periods:</i></b>				
Overdose Death Rate per 10,000	0.219	0.103	0.258	0.109
Unemployment Rate	4.641	2.478	5.396	3.113
Median Age	37.527	2.661	39.033	2.058
Proportion of Population White	0.722	0.144	0.665	0.156
Poverty Rate	0.115	0.029	0.120	0.024
Proportion without Highschool Degree	0.058	0.017	0.068	0.017
N	<b>306</b>		<b>1394</b>	

Note: The data is from 2019 to 2022. The classification of the treatment group and the non-treatment group is based on Hale et al. (2021). I classify the states that rolled out a stay-at-home mandate at any point as the treatment group. For simplicity, I define the post-treatment periods as those starting in March 2020. The monthly unemployment rate data is from BLS. The data on median age, proportion of the population being non-Hispanic White, proportion of the population living below the poverty line, proportion of the population with lower than high school degrees are from the Census Bureau. The data on drug overdose death rate is from the CDC WONDER.

Table 3: Effects of lockdown mandates on drug overdose deaths

	Drug Overdose Deaths	
	(1)	(2)
OLS	0.187*** (0.061)	0.185*** (0.059)
Poisson	0.182*** (0.064)	0.142** (0.060)
$e^\beta - 1$	19.96%	15.26%
N	2352	2352
Covariates	No	Yes

\*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$

Note: This table presents the OLS and Poisson regression results. Column (1) presents estimates without covariates. Column (2) presents estimates with covariates. Both state and time fixed-effects are included across all 4 columns. All states except Nebraska are included.

Table 4: Effects of lockdown mandates on drug overdose deaths

	Drug Overdose Deaths	
	(1)	(2)
Callaway and Sant'Anna	0.145** (0.069)	0.120** (0.061)
Wooldridge	0.188*** (0.0628)	0.211*** (0.0632)
Generalized Synthetic Control	0.188*** (0.062)	0.200*** (0.072)
N	2352	2352
Covariates	No	Yes

\*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$

Note: his table presents the average treatment effects on the treated using Callaway and Sant'Anna (2021) and Wooldridge (2021, 2023). Column (1) presents estimates without covariates. Column (2) presents estimates with covariates. With the concern of small sample size, I select methods using “not yet treated group” as control groups while estimating with control variables in Callaway and Sant'Anna (2021). All states except Nebraska are included.

Table 5: Mechanism—Unemployment

	(1)	(2)
ATT	0.408 (0.344)	0.481 (0.323)
Num.Obs.	2352	2352
Covariates	X	

\* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01

Note: This table presents the average treatment effects of the lockdown mandates on unemployment rates from BLS. I adopt a TWFE model specified as  $y_{it} = \beta(Post_t \times Treatment_i) + \zeta Z_{it} + \gamma_i + \delta_t + \epsilon_{it}$ , where the dependent variable is the unemployment rate multiplied by 100 in state  $i$  during month  $t$ .  $Z_{it}$  is a matrix of state-level controls, including median age, proportion of the population being White, poverty rate, and proportion of the population without high school degrees. Standard errors clustered by state are presented in parenthesis.

Table 6: Mechanism—Mental Health

	(1)	(2)
ATT	-0.034 (0.028)	-0.036 (0.025)
Num.Obs.	1 586 309	1 240 479
Covariates	X	

\* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01

Note: This table presents the average treatment effects of the COVID-19 lockdown mandates on mental health using repeated cross-sectional individual level data from BRFSS. I adopt a TWFE model specified as

$\log(y_{it} + 0.1) = \beta(Post_t \times Treatment_i) + \zeta Z_{it} + \gamma_i + \delta_t + \epsilon_{it}$ , where the dependent variable is the log-transformed number of days feeling mental distress in the past thirty days. State fixed-effects and quarter fixed-effects are included in all columns.  $Z_{it}$  is a matrix of individual-level controls, including gender, ethnicity, income level, and education level. Standard errors clustered by state are presented in parenthesis.

Table 7: Mechanism—Google Community Mobility Reports

	Retail & Recreation (1)	Grocery & Pharmacy (2)	Parks (3)	Transit Stations (4)	Workplaces (5)	Residential (6)
ATT	-3.441** (1.645)	-3.007* (1.515)	-35.158*** (11.186)	-7.347 (5.202)	-1.052 (1.324)	0.605 (0.583)
Num.Obs.	15 729	15 729	15 304	15 594	15 729	15 729

\* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01

Note: This table presents the average treatment effects of the lockdown mandates on community mobility using state-level Google Community Mobility data in 2020. I adopt a TWFE model specified as  $y_{it} = \beta(Post_t \times Treatment_i) + \gamma_i + \delta_t + \epsilon_{it}$ , where the dependent variable is a list of indices measuring mobility in “retail and recreation,” “grocery and pharmacy,” “parks,” “transit stations,” “workplaces,” and “residential” in state  $i$  during month  $t$  compared to pre-pandemic baseline (Google, 2020). According to Google (2020), the indices for “retail and recreation,” “grocery and pharmacy,” “parks,” “transit stations,” and “workplaces” measure the number of visitors in the respective areas, while the “residential” index captures the duration of time spent in that area. Standard errors clustered by state are presented in parenthesis.

## Figures

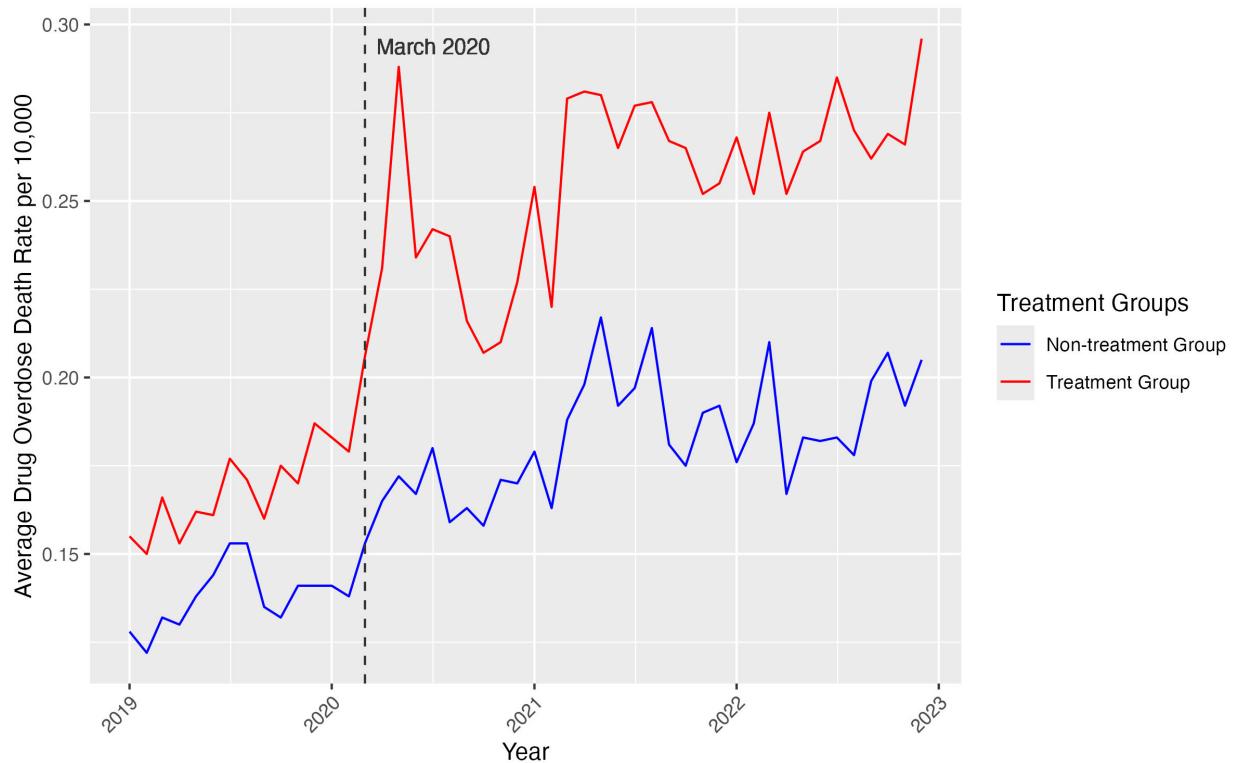


Figure 1: General Trends for Treatment Group and Non-treatment Group

Note: This figure illustrates the trends in the drug overdose death rate per 10,000 population for both the treatment and non-treatment groups. I use Hale et al.'s (2021) criterion for this figure. The treatment group includes the states that have received treatment at any point in time. The non-treatment group includes the states that never received treatment. The values presented are the average drug overdose death rates by group.

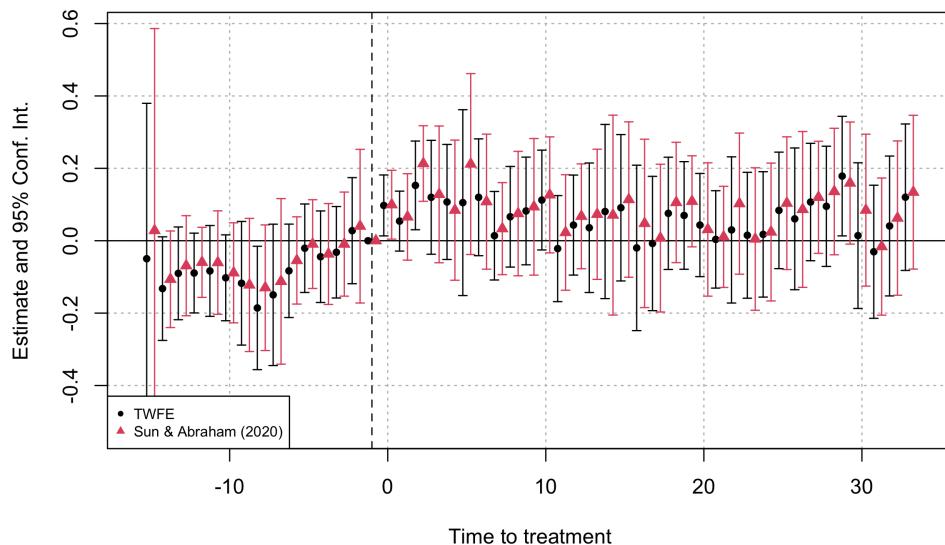
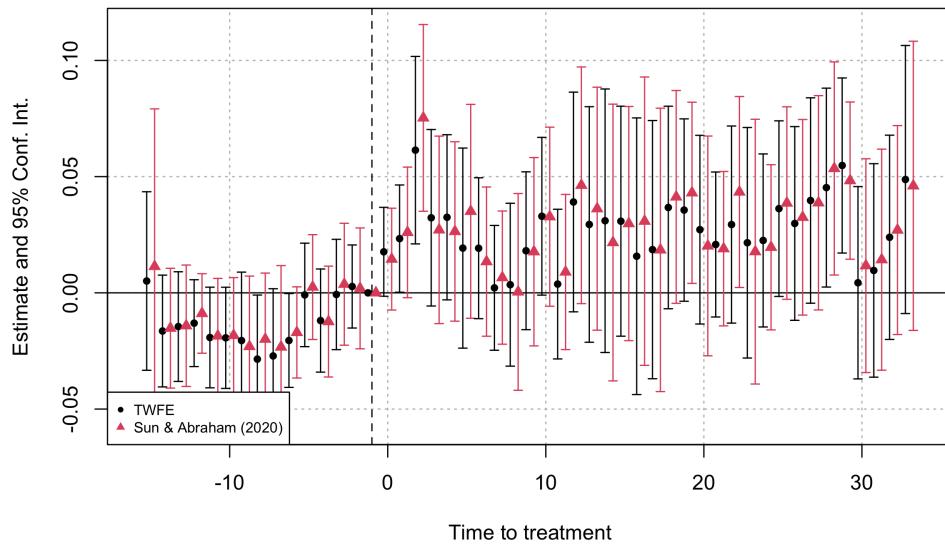
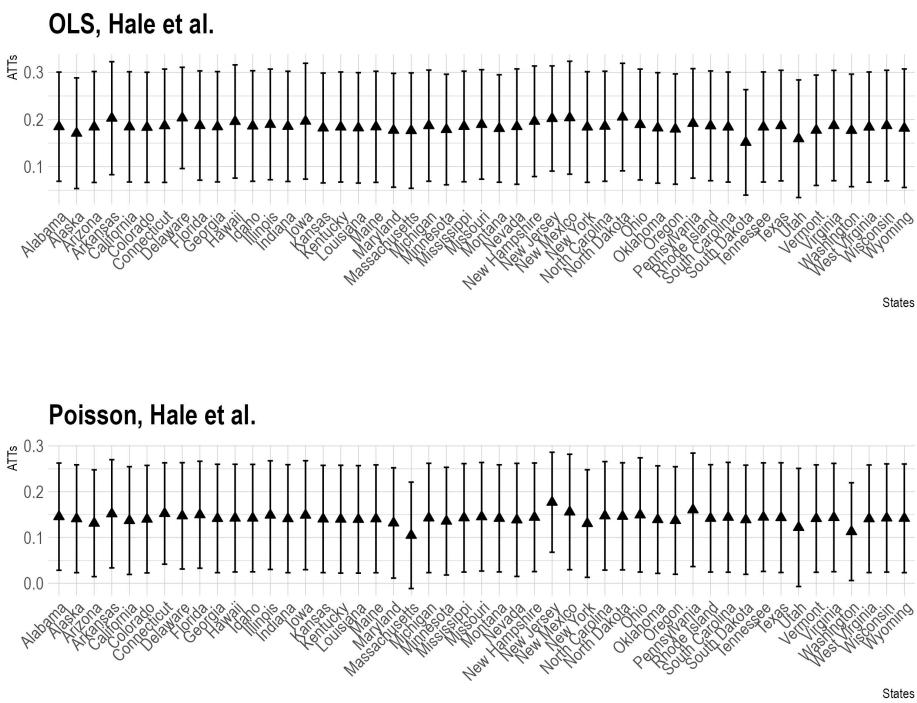


Figure 2: Event study plots

Note: This figure presents the dynamic treatment effects of COVID-19 lockdowns on drug overdose deaths using Hale et al.'s (2021) specification. The upper panel presents the OLS estimates for the baseline model using traditional TWFE and Sun and Abraham (2021). The bottom panel presents the Poisson estimates for the baseline model using traditional TWFE and Sun and Abraham (2021). All control variables are included. All states except Nebraska are included.



Note: This figure presents the average treatment effects on the treated by dropping each state from the sample. The upper panel presents estimates using OLS methods, and the bottom panel presents estimates using Poisson methods. I plot 95% confidence intervals. All controls are included. All states except Nebraska are included.

Figure 3: Drop-one analysis

## A Appendix

### A.1 Alternative Lockdown Criteria

Hale et al. (2021) likely better suits the study, as Moreland et al. (2020) documents stay-at-home orders only through May 2020, and the timing of these orders is not the primary focus of that study. However, there are still concerns that relying on a single lockdown criterion might introduce biases. I first use the criterion suggested by Moreland et al. (2020). Additionally, to address the biases that could arise from an individual criterion, I adopt a classification using states that Hale et al. (2021) and Moreland et al. (2020) agreed on. While I find smaller and statistically insignificant effects using Moreland et al. (2020), the estimated effects become similar in scale to my baseline results after excluding states that Hale et al. (2021) and Moreland et al. (2020) disagree on. As a result, I find generally consistent results using alternative lockdown criteria.

Moreland et al. (2020) adopts a different approach to categorizing the lockdown orders, dividing them into 5 categories including “mandatory for all persons,” “mandatory only for persons in certain areas of the jurisdiction,” “mandatory only for persons at increased risk in the jurisdiction,” “mandatory only for persons at increased risk in certain areas of the jurisdiction,” and “advisory or recommendation.” According to this distinction, 5 states (Arkansas, Connecticut, Nebraska, North Dakota, and Wyoming) did not implement any stay-at-home order from March 2020 to May 2020. Also, Moreland et al. (2020) identifies that 6 states including New Mexico, Massachusetts, Kentucky, Utah, Texas, and Iowa have only implemented “advisory” orders instead of the mandatory ones.<sup>9</sup> I code the states that implemented some mandatory orders for at least some of their population as the treatment group.

With further concerns that the criteria by Hale et al. (2021) and Moreland et al. (2020) have biases, I drop the states that Hale et al. (2021) and Moreland et al. (2020) disagree on. This will likely exclude states with ambiguous policies. I drop Kentucky, Nebraska, and Texas from my sample because Moreland et al. (2020) considers them as non-treated while Hale et al. (2021) consider them as treated. I drop South Dakota from the sample because Moreland et al. (2020) considers it as treated while Hale et al. (2021) consider it as non-treated. I also drop Nebraska, Alabama, Florida, Maine, Missouri, South Carolina, Tennessee, and Nevada from the sample because Hale et al. (2021) and Moreland et al. (2020) disagree on the timings of treatment. In total, applying both criteria results in a sample of 39 states, excluding Alabama, Florida, Kentucky, Maine, Missouri, Nebraska, Nevada, South Carolina, South Dakota, Tennessee, and Texas.

I present the results using Moreland et al. (2020) categorization in Table 1 Panel A. I find a 5% to 8% and non-significant increase in drug overdose deaths. I further proceed by excluding the states that Hale et al. (2021) and Moreland et al. (2020) disagree on. I present my estimates in Table 1 Panel B. I find a 14% to 16% increase in drug overdose deaths. Three out of four estimates using both criteria are significant at the 95% confidence level, while one is significant at the 90% confidence level. This suggests that the effects of COVID-19 lockdown mandates on drug overdose deaths hold well after excluding the states that Hale et al. (2021) and Moreland et al. (2020) disagree on.

Additionally, I use methods suggested by Callaway and Sant’Anna (2021) and Wooldridge (2021, 2023). I find a 9% to 14% increase in drug overdose deaths using Moreland et al. (2020) criterion, although the effect is not statistically significant at the 5% level. After excluding the states that Hale et al. (2021) and Moreland et al. (2020) disagree on, I find that lockdown mandates lead to a 13% to 21% increase in drug

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<sup>9</sup>Moreland et al. (2020) introduces that the orders with words like “shall,” ‘must,’ or ‘are directed to’ were coded as mandatory orders. On the other hand, the stay-at-home orders that included words like “should,” ‘are encouraged to,’ or ‘are urged to’ are considered non-mandatory orders. (Moreland et al., 2020)

overdose deaths.

I plot the event study plots for Moreland et al. (2020) criterion and further exclude states with ambiguous policies in Figures 1. I find no divergent trends in pre-treatment periods for OLS models, as the pre-treatment effects are not significant in any period. I find some divergent paths using Poisson models, as the pre-treatment effects are consistently negative. This suggests my models are sensitive to functional form. The violations of parallel trends in Poisson models are likely not a significant concern, as there could be only one true generation process (see Roth and Sant'Anna (2023)). I still observe an increase in drug overdose death rates by comparing average post-treatment effects to pre-treatment effects. The treatment effect remains positive under the assumption that pre-treatment patterns continue in post-treatment periods (see Rambachan and Roth (2023)).

Table 1: Effects of lockdown mandates on drug overdose deaths

	OLS		Poisson	
	(1)	(2)	(3)	(4)
<b>Panel A Moreland</b>				
ATT	0.083 (0.051)	0.084 (0.051)	0.045 (0.071)	0.053 (0.050)
$e^\beta - 1$			4.60%	5.44%
N	2400	2400	2400	2400
<b>Panel B Both Criteria</b>				
ATT	0.143** (0.061)	0.151* (0.064)	0.164** (0.072)	0.159** (0.069)
$e^\beta - 1$			17.82%	17.23%
N	1872	1872	1872	1872
Covariates	No	Yes	No	Yes

\*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$

Note: This table presents the OLS and Poisson regression results. Columns (1) and (2) report average treatment effects on the treated using OLS models. Columns (3) and (4) report Poisson regression coefficients offsetting log population at the state level. Both state and time fixed-effects are included across all 4 columns. Panel A follows the criterion and timing of treatment based on Moreland et al. (2020), Panel B excludes states where Hale et al. (2021) and Moreland et al. (2020) disagree on policies. All 50 states are included in Panel A. Panel B excludes states with ambiguous lockdown policies: Alabama, Florida, Kentucky, Maine, Missouri, Nebraska, Nevada, South Carolina, South Dakota, Tennessee, and Texas.

Standard errors clustered by state are presented in parenthesis.

Table 2: Effects of lockdown mandates on drug overdose deaths Callaway and Sant'Anna, and Wooldridge

	Callaway and Sant'Anna		Wooldridge	
	(1)	(2)	(3)	(4)
<b>Moreland</b>	0.108*	0.089	0.086	0.144
	(0.064)	(0.060)	(0.054)	(0.095)
<b>Both criteria</b>	0.128	0.128*	0.143**	0.208***
	(0.079)	(0.066)	(0.063)	(0.064)
Covariates	No	Yes	No	Yes

\*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$

Note: This table presents the average treatment effects on the treated using Callaway and Sant'Anna (2021) and Wooldridge (2021, 2023). Columns (1) and (3) report the average treatment effects on the treated without control. Columns (2) and (4) report the average treatment effects on the treated with control. With the concern of small sample size, I select methods using “not yet treated group” as control groups while estimating with control variables in Callaway and Sant'Anna (2021).

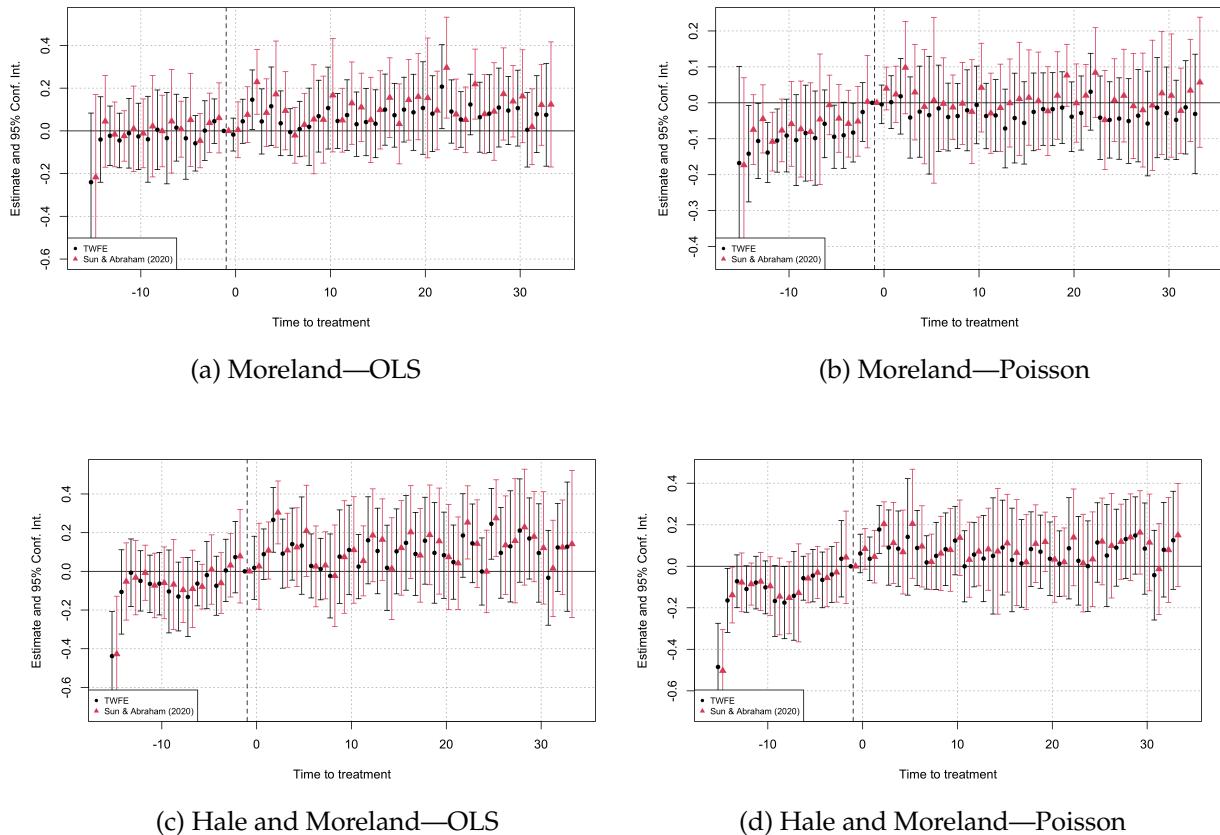


Figure 1: Event study plots—Hale et al. and Moreland

Note: This figure presents the dynamic treatment effects of COVID-19 lockdowns on drug overdose deaths using Moreland et al. (2020) specification and both Hale et al. (2021); Moreland et al. (2020) criteria. I use both traditional TWFE and Sun and Abraham (2021). The upper panel presents the OLS and Poisson estimates using Moreland et al. (2020) criterion. The bottom panel presents the OLS and Poisson estimates using states that Hale et al. (2021); Moreland et al. (2020) agree on. All control variables are included. All 50 states are included in the upper panel. States with ambiguous lockdown policies are excluded in the bottom panel: Alabama, Florida, Kentucky, Maine, Missouri, Nebraska, Nevada, South Carolina, South Dakota, Tennessee, and Texas.

## A.2 Mediating Effects of Channels

I explore the mediating effects of the potential channels in Section 7 through which lockdown mandates could have affected drug overdose deaths. Specifically, I examine two subsample. I first examine the effects of lockdown mandates on drug overdose deaths in treated states that experienced relatively larger changes in unemployment, mental health, and mobility compared to the never-treated states. Then, I examine those effects in treated states that experienced smaller changes in unemployment, mental health, and mobility compared to the never-treated states. These allow me to assess whether and to what extent the responses of specific channels serve as mediators of the overall effects.

I first present the effects of lockdown mandates on drug overdose deaths in states with high and low unemployment increases in Table 3. Surprisingly, I find that the effects are larger in magnitude in states with relatively low unemployment increase. These suggest that the mediating effects of unemployment are limited. I then present the effects comparing states with more severe and milder deterioration in mental health in Table 4. I find that the increase in drug overdose deaths are more pronounced in states with greater declines in mental health. However, combined with our earlier findings that lockdown mandates were associated with some improvements in mental health, although statistically insignificant, these results provide little evidence for a mediating role of mental health in the relationship between lockdown mandates and drug overdose deaths. Finally, I present the effects of lockdown mandates on drug overdose deaths in states with greater versus smaller increase in isolation according to each criterion in Table 5. I find mixed evidence on the mediating effects of isolation. States with greater declines in mobility at grocery markets & pharmacies and parks are associated with greater increase in drug overdose deaths. On the other hand, states with greater declines in mobility at retail & recreation areas, transit stations, and workplaces are associated with smaller increase in drug overdose deaths. According to Column (6), states that experienced larger increase in the amount of time residents spent in residential areas (greater isolation) are associated with smaller increase in drug overdose deaths.

Table 3: Mediating Effects—Unemployment

	States with High Unemployment Increase		States with Low Unemployment Increase	
	(1)	(2)	(3)	(4)
ATT	0.168** (0.071)	0.155** (0.068)	0.208*** (0.067)	0.236*** (0.063)
Num.Obs.	1392	1392	1392	1392
Covariates		X		X

\* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01

Note: This table presents the effects of lockdown mandates on drug overdose deaths in states with high increase in unemployment and states with low increase in unemployment. The sample for Columns (1) and (2) includes never-treated states and treated states that experienced relatively larger increase in unemployment. The sample for Columns (3) and (4) includes never-treated states and treated states that experienced relatively smaller increase in unemployment. Both state and time fixed-effects are included in all four columns.

Table 4: Mediating Effects—Mental Health

	States with Greater Declines in Mental Health		States with Smaller Declines in Mental Health	
	(1)	(2)	(3)	(4)
ATT	0.209*** (0.070)	0.224*** (0.059)	0.167** (0.068)	0.113 (0.076)
Num.Obs.	1392	1392	1392	1392
Covariates:	X		X	

\* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01

Note: This table presents the effects of lockdown mandates on drug overdose deaths in states with greater declines of mental health and states with smaller declines of mental health. The sample for Columns (1) and (2) includes never-treated states and treated states that experienced greater declines of mental health.

The sample for Columns (3) and (4) includes never-treated states and treated states that experienced relatively smaller declines of mental health. Both state and time fixed-effects are included in all four columns.

Table 5: Mediating Effects—Isolation

	Retail & Recreation (1)	Grocery & Pharmacy (2)	Parks (3)	Transit Stations (4)	Workplaces (5)	Residential (6)
<b>Panel A, States with Greater Increase in Isolation</b>						
ATT	0.175** (0.071)	0.190** (0.070)	0.223*** (0.063)	0.140* (0.074)	0.138* (0.070)	0.140* (0.070)
<b>Panel B, States with Smaller Increase in Isolation</b>						
ATT	0.200*** (0.067)	0.186** (0.068)	0.153** (0.073)	0.233*** (0.062)	0.235*** (0.066)	0.233*** (0.066)
Num.Obs.	1392	1392	1392	1392	1392	1392

\* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01

Note: This table presents the effects of lockdown mandates on drug overdose deaths in states with greater declines of mobility and states with smaller declines of mobility. The sample in Panel A includes never-treated states and treated states that experienced greater declines in mobility or increases in isolation, as defined by each respective criterion. The sample in Panel B includes never-treated states and treated states that experienced smaller declines in mobility or increases in isolation, as defined by each respective criterion. For example, Column (1) of Panel A shows that treated states with greater increase in isolation, as defined by Retail & Recreation, experienced 17.5% increase in drug overdose deaths compared to never treated states. Both state and time fixed-effects are included in all six columns.

### A.3 Alternative Imputation Methods

The CDC WONDER database suppresses data entries with fewer than 10 deaths. For my baseline analysis, I impute the suppressed data entries with 5 cases. I employ alternative imputation methods to probe the robustness of my models. Firstly, I impute the suppressed data entries with 9 cases, as shown in Panel B. Secondly, I impute data suppressed data entries with a discrete uniform distribution with outcomes ranging from 1 to 9, as shown in Panel C. I use OLS models strictly following my baseline analysis. The results show that my findings are consistent under alternative imputation methods.

Table 6: Appendix—Alternative Imputations

	(1)	(2)
<i>Panel A, Imputation with 5s:</i>		
ATT	0.187*** (0.061)	0.185*** (0.059)
<i>Panel B, Imputation with 9s:</i>		
ATT	0.212*** (0.061)	0.205*** (0.053)
<i>Panel C, Imputation with discrete uniform distribution:</i>		
ATT	0.155** (0.065)	0.150** (0.063)
Num.Obs.	2352	2352
Std.Errors	by: State	by: State
Covariates		X
FE: State	X	X
FE: period	X	X

\* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01

Note: This table presents the OLS results with alternative imputation methods. The functional form strictly follows that of Equation 1. Panel A presents my baseline results, where I impute with 5 cases. Panel B imputes them with 9 cases, and Panel C imputes them using a discrete uniform distribution. All states excluding Nebraska are included.

#### A.4 Reverse Treatment

It is common to assume no reverse treatment in DiD analysis. However, it is unclear whether the effects of COVID-19 lockdown mandates would last after the mandates are lifted. Hale et al. (2021) report the COVID-19 policies in each state by date, allowing me to examine the case where treatment can be reversed. In other words, I examine the case where treatment turns on and off according to states' policies. I summarize the timing of treatment in each state in Table 7. I present the results in Table 8. I find mixed results on the reverse treatment effects. I find null estimates using OLS models and a significant increase in drug overdose deaths using Poisson models. This suggests mixed evidence on whether the treatment effects of lockdown mandates are concurrent.

Table 7: Timing of Treatments

1	Alabama	March 2020 to June 2020
2	Alaska	March 2020 and April 2020
3	Arizona	March 2020 to May 2020
4	Arkansas	
5	California	March 2020 to May 2020, and November 2020 to January 2021, and March 2021 and April 2021
6	Colorado	March 2020 to May 2020, and January 2021 and February 2021
7	Connecticut	
8	Delaware	March 2020 to July 2020
9	Florida	March 2020 to April 2021
10	Georgia	March 2020 and April 2020
11	Hawaii	March 2020 to November 2020, and February 2021 to May 2021
12	Idaho	March 2020 and April 2020
13	Illinois	March 2020 to June 2020
14	Indiana	March 2020 and April 2020
15	Iowa	
16	Kansas	March 2020 to May 2020
17	Kentucky	March 2020 to May 2020
18	Louisiana	March 2020 to May 2020, and February 2021
19	Maine	March 2020 to May 2020
20	Maryland	March 2020 to May 2020
21	Massachusetts	
22	Michigan	March 2020 to May 2020
23	Minnesota	March 2020 to May 2020
24	Mississippi	April 2020 and May 2020
25	Missouri	March 2020 to May 2020, and November 2020 to May 2021
26	Montana	March 2020 to March 2021
27	Nebraska	December 2020
28	Nevada	April 2020 and May 2020
29	New Hampshire	March 2020 to June 2020
30	New Jersey	March 2020 to June 2020, and November 2020 and December 2020
31	New Mexico	
32	New York	March 2020 to June 2020
33	North Carolina	March 2020 to May 2020, and December 2020 to February 2021
34	North Dakota	
35	Ohio	March 2020 to May 2020, and November 2020 to February 2021
36	Oklahoma	March 2020 and April 2020
37	Oregon	March 2020 to August 2020
38	Pennsylvania	March 2020 to June 2020
39	Rhode Island	March 2020 to May 2020, and November 2020
40	South Carolina	March 2020 to May 2020
41	South Dakota	
42	Tennessee	March 2020 to May 2020
43	Texas	March 2020 to May 2020, and October 2020 and November 2020
44	Utah	
45	Vermont	March 2020 to May 2020
46	Virginia	March 2020 to May 2020, and December 2020 to February 2021
47	Washington	March 2020 to May 2020
48	West Virginia	March 2020 to May 2020
49	Wisconsin	March 2020 to May 2020
50	Wyoming	

Table 8: Appendix: Reverse Treatment

	OLS		Poisson	
	(1)	(2)	(3)	(4)
ATT	0.007 (0.031)	0.006 (0.034)	0.031** (0.012)	0.034** (0.013)
$e^\beta - 1$			3.14%	3.46%
Num.Obs.	2352	2352	2352	2352
Std.Errors	by: State	by: State	by: State	by: State
FE: State	X	X	X	X
FE: period	X	X	X	X
Covariates		X		X

\* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01

Note: This table presents the results assuming reverse treatment. The models strictly follow my baseline models of Equations 1 and 2. All states excluding Nebraska are included.

## A.5 Quarterly Analysis

I further probe the robustness of my models using quarterly aggregated data. This is relevant because the results may be sensitive to the treatment cut-off points. For example, the treatment starting in March 2020 would fall into the first quarter, and the treatment starting in April 2020 would be placed into the second quarter. I strictly follow the strategies in my baseline analysis and present the results. I present the ATTs in Table 9. The results indicate that the estimated effects are consistent in both magnitude and significance with the baseline analysis. I also present the event study plots in Figure 2. I find that the results are consistent with those in my baseline analysis.

Table 9: Effects of lockdown mandates on drug overdose deaths—Quarterly Analysis

	OLS		Poisson	
	(1)	(2)	(3)	(4)
ATT	0.221*** (0.056)	0.217*** (0.053)	0.193*** (0.063)	0.155** (0.058)
$e^\beta - 1$			21.29%	16.77%
N	784	784	784	784
Covariates	No	Yes	No	Yes

\*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$

Note: This table presents the OLS and Poisson regression results using quarterly data. Columns (1) and (2) report average treatment effects on the treated using OLS models. Columns (3) and (4) report Poisson regression coefficients offsetting log population at the state level. Both state and quarter fixed-effects are included across all 4 columns. All states excluding Nebraska are included.

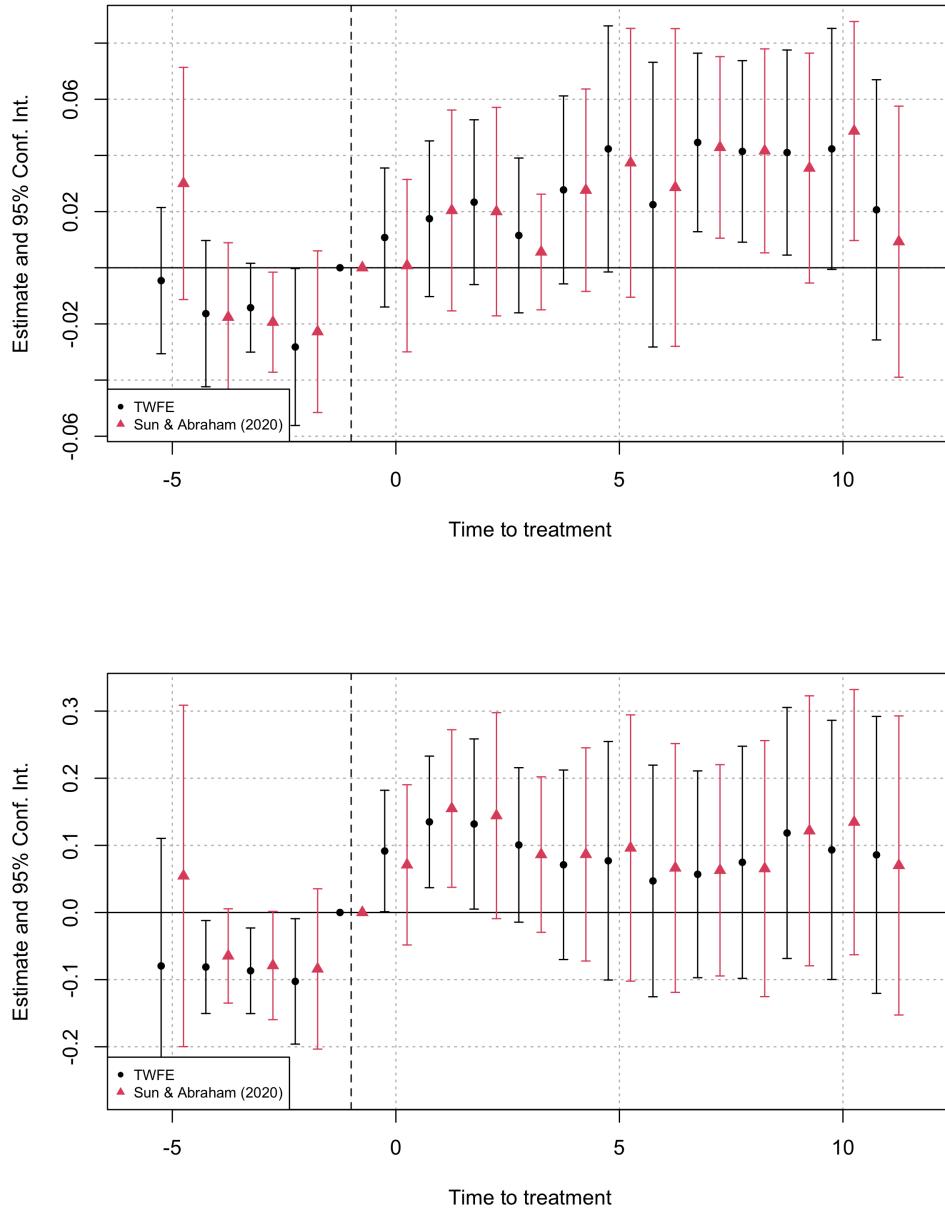


Figure 2: Event study plots—Quarterly Analysis using Hale et al. Criterion

Note: This figure presents the dynamic treatment effects of COVID-19 lockdowns on drug overdose deaths using Hale et al. (2021) specification. The upper panel presents the OLS estimates for the baseline model using traditional TWFE and Sun and Abraham (2021). The bottom panel presents the Poisson estimates for the baseline model using traditional TWFE and Sun and Abraham (2021). All control variables are included. All states excluding Nebraska are included.

## A.6 Adding Late Treatment Group

According to Hale et al. (2021), Nebraska implemented its stay-at-home orders in December 2020, much later than other states that received treatment. With concern about the unnecessary noise, I excluded Nebraska in my baseline analysis. I present the results adding Nebraska in this section. I strictly follow models in my baseline analysis. The coefficient estimates are presented in Table 10. The event study plots are presented in Figure 3. I find that the coefficient estimates are generally consistent with previous analysis.

Table 10: Effects of lockdown mandates on drug overdose deaths—Adding Nebraska

	OLS		Poisson	
	(1)	(2)	(3)	(4)
ATT	0.166*** (0.060)	0.163*** (0.059)	0.176*** (0.064)	0.137** (0.059)
$e^\beta - 1$			19.24%	14.68%
N	2400	2400	2400	2400
Covariates	No	Yes	No	Yes

\*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$

Note: This table presents the OLS and Poisson regression results. Columns (1) and (2) report average treatment effects on the treated using OLS models. Columns (3) and (4) report Poisson regression coefficients offsetting log population at state level. Both state and time fixed-effects are included across all 4 columns. All 50 states are included. The models strictly follow the baseline analysis.

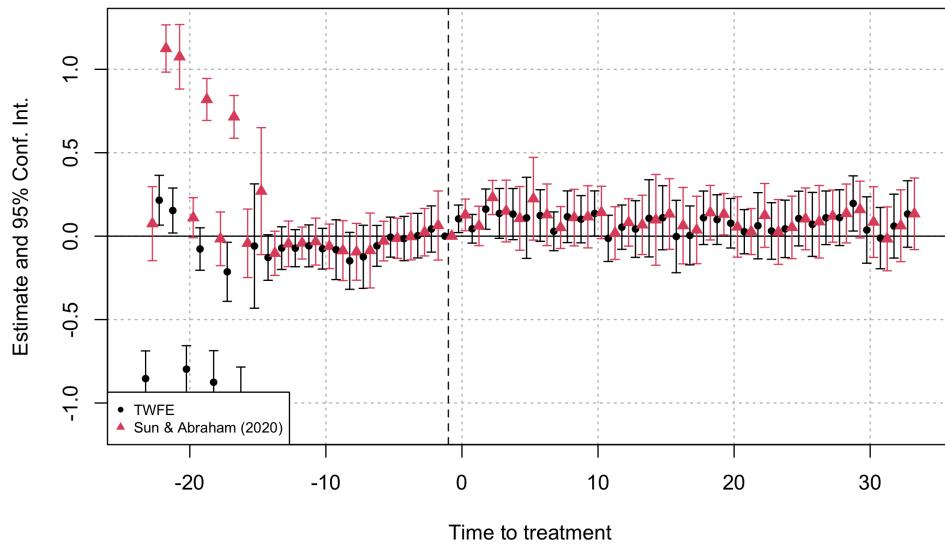
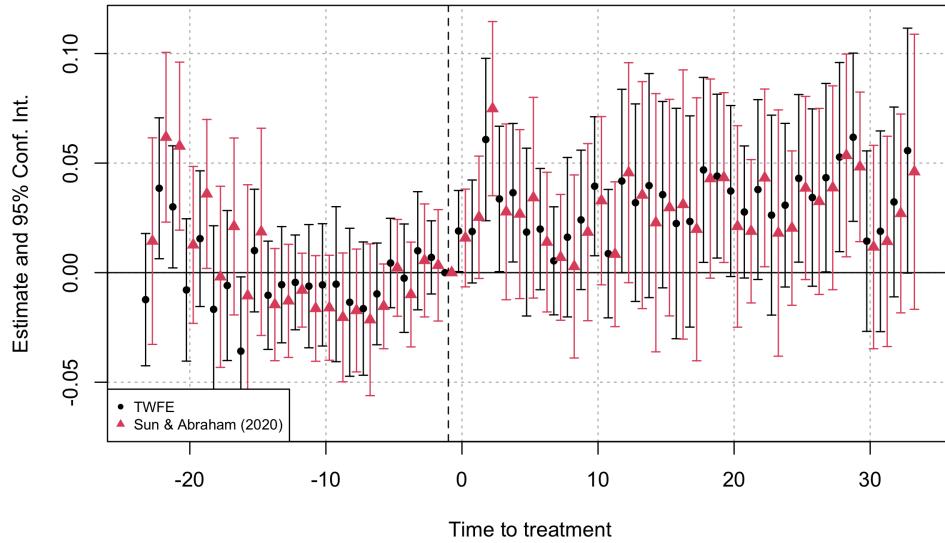


Figure 3: Event study plots—Adding Nebraska

Note: This figure presents the dynamic treatment effects of COVID-19 lockdowns on drug overdose deaths using Hale et al. (2021); Moreland et al. (2020). The upper panel presents the OLS estimates using traditional TWFE and Sun and Abraham (2021). The bottom panel presents the Poisson estimates using traditional TWFE and Sun and Abraham (2021). All control variables are included. All 50 states are included. The models strictly follow the baseline analysis.

## A.7 Endogenous Migration

Migration is a plausible confounding factor of drug overdose deaths, as the internal migration patterns might correlate with decisions of COVID-19 lockdown mandates. If this is the case, it is likely to observe changes of demographic composition. Conversely, if there is no significant demographic composition change, there is likely no endogeneity. I examine whether COVID-19 lockdown mandates affect the proportion of the population without a high school degree, as education level serves as an important social determinant of drug overdose deaths (Powell, 2023). I present the results using the three criteria in the following table. I find no significant changes in proportion of the population without a high school degree, suggesting endogenous migration is unlikely a threat to the identification.

	Population Without High School Degree (1)
ATT	−0.076 (0.063)
Num.Obs.	196

\* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01

Note: This table presents the average treatment effects of the COVID-19 lockdown mandates on the proportion of the population without a high school degree. I adopt a TWFE model specified as  $y_{it} = \beta(Post_t \times Treatment_i) + \gamma_i + \delta_t + \epsilon_{it}$ , where the dependent variable is the proportion of the population without a high school degree. State fixed-effects and year fixed-effects are included in all columns. Standard errors clustered by state are presented in parenthesis. All states excluding Nebraska are included.

## A.8 Correlation Between COVID-19 and Drug Overdose Mortality

Previous research find that COVID-19 and drug overdose are frequently comorbidities (Wang et al., 2021; Awortwe and Cascorbi, 2020). This implies that COVID-19 mortality are likely contemporaneously correlated with drug overdose deaths. In other words, if we observe an increase in drug overdose deaths, we likely also observe an increase in fatalities from COVID-19 infections. While it might alleviate the concerns using the primary cause of deaths to exclude comorbidities, the relationship between fatalities from COVID-19 infections and drug overdose may still be driven by reverse causality. I plot the correlation between mortality rates from COVID-19 infections and drug overdose as primary cause of deaths. The results show that states with higher COVID-19 death rates also tend to have higher drug overdose death rates. The interaction between fatalities from COVID infections and drug overdoses likely form reverse causality and will further complicate my results. Disentangling the causal relationship between COVID-19 infections and drug overdose deaths may require further research.

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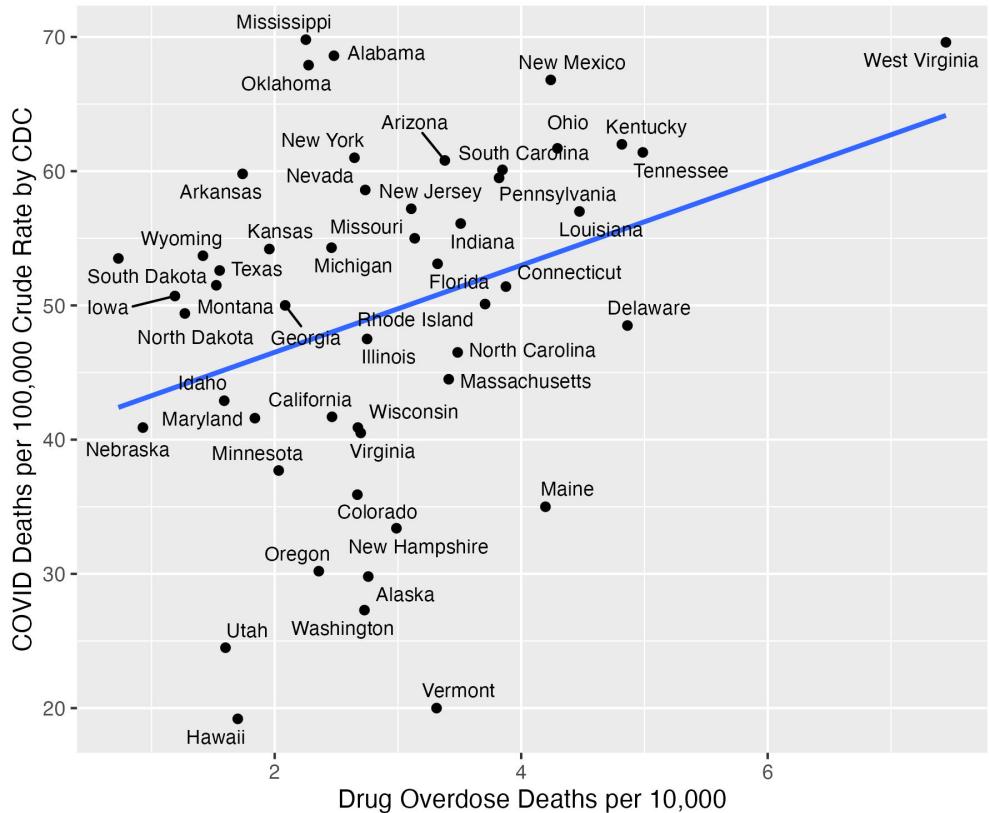


Figure 4: Correlation between primary cause deaths from COVID-19 infections and drug overdose

Howard-Williams, M. (2020). Timing of state and territorial covid-19 stay-at-home orders and changes in population movement - united states, march 1–may 31, 2020. *MMWR. Morbidity and mortality weekly report*, 69:1198–1203.

Powell, D. (2023). Educational attainment and us drug overdose deaths. In *JAMA health forum*, volume 4, pages e233274–e233274. American Medical Association.

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