The Effect of Overdose Prevention Centers on Overdose Mortality: Evidence from New York City

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Abstract

The surge in overdose deaths in the U.S. presents a pressing public health crisis. Despite a recent slowdown, overdose deaths remain significantly high, surpassing other leading causes of mortality. I employ a synthetic difference-in-differences strategy to assess the impact of two Overdose Prevention Centers (OPCs) in New York City on overdose mortality. I find a 14 percent increase in overdose mortality; although the point estimates are stable across specifications, they are not statistically significant. Event study estimates further reveal an increase in drug overdose mortality post-OPC opening, with a seemingly escalating impact over time. I also find that OPCs significantly increased entry into substance use disorder treatment by 19 percent, primarily driven by a higher rate of seeking rehabilitation among current drug users. These findings contribute to harm reduction policy discussions, informing policymakers grappling with effective strategies against drug abuse and overdose.

Key Words: Opioid crisis, overdose deaths, harm reduction, natural experiment.
JEL Classification: H0, I1, I18, K0, K42.

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1 Introduction

A little over 1 million people have died since 1999 from a drug overdose in the United States.[1] There is an increasing trend in drug overdose deaths where the numbers have jumped from below 20,000 per year in 1999 to close to 110,000 in 2021 while the death rate per 100,000 population is more than four times higher.[2] Most of this jump has been observed in the last years as the prevalence of more potent synthetic opioids like fentanyl has increased. According to CDC reports, the age-adjusted rate of overdose deaths increased by 31% from 2019 (21.6 per 100,000) to 2020 (28.3 per 100,000) and 15% from 2020 to 2021 (32.4 per 100,000). The dramatic increase in 2020 was a part of the shadow epidemic of addiction obscured by the COVID-19 crisis. Many experiencing addiction had difficulties finding treatment and support during the pandemic. Drug treatment experts say the pandemic also increased stress and isolation for many individuals, triggering a surge in drug use. Compared to 2020 numbers, drug overdose deaths in the U.S. seem to be slowing, however, the annual drug overdose deaths are still 25% higher than they were two years earlier and more than 50% higher than they were five years earlier. Based on the latest National Vital Statistics System Mortality Data, overdoses kill more Americans than car accidents or gun violence.

One fairly new strategy to combat drug abuse is overdose prevention centers (OPCs). OPCs are safe spaces for people to consume pre-obtained drugs in controlled settings under the supervision of trained staff and with access to sterile consumption equipment (i.e., syringes) as well as antagonist medications (i.e., naloxone, naltrexone), counseling services, and referrals to substance use disorder treatment and other services. In spite of the federal law 21 U.S. Code § 856 - Maintaining drug-involved premises, commonly known as the "crack house" statute, two overdose prevention centers (OPC) opened in New York City in

[1] 1999 thru 2021 final totals: CDC WONDER; Estimated Jan. 2022 - 2023 totals: Calculations based on National Vital Statistics System Provisional Drug Overdose Death Counts, CDC WONDER

[2] Centers for Disease Control and Prevention, National Center for Health Statistics. Multiple Cause of Death 1999-2021 on CDC WONDER online database, released 1/2023.

the neighborhoods of East Harlem and Washington Heights in November 2021.[3]

The first overdose prevention center opened in Bern, Switzerland, in 1986. Currently, there are more than 200 OPCs in more than 60 jurisdictions around the world.[4] Based on the records of the Drug Policy Alliance, a group working for decriminalization and safe drug use policies, no person has died of an overdose at any OPC in more than 30 years of operations. Despite being a proven public health intervention to prevent overdose deaths and provide health services to people who use drugs (see Background), there are critics of the moral implications of sanctioning illegal drug use. Other critics point to the legal issues of sanctioned drug use. Section 856 of the Controlled Substances Act, makes it illegal to operate, own or rent a location for the purpose of using illegal substances.[5] The ongoing debate over OPCs prevents the federal government from authorizing supervised injection sites.

Prior studies are often descriptive and mainly concerned with other countries. Kilmer et al., 2018 provide a review of 22 studies of overdose prevention sites among the 65 outcome-related articles. Most of the empirical literature concerns with Insite Supervised Injection Facility in Vancouver or the Medically Supervised Injection Centre (MSIC) in Sydney. There are no randomized control trials and only nine studies with a quasi-experimental design with control groups. These nine studies focus on OPCs in three cities: Sydney (five studies), Vancouver (two studies), and Barcelona (two studies). Overall, they find no change in crime trends where OPCs are located. If anything, they present evidence for lower crime overall over time, reductions in the number of overdoses and emergency service calls within

[3]Mays JC, Newman A. Nation's first supervised drug-injection sites open in New York. The New York Times. November 30, 2021. Accessed July 6, 2023. https://www.nytimes.com/2021/11/30/nyregion/supervised-injection-sites-nyc.html

[4]Sites operate in 14 countries: Switzerland, Germany, the Netherlands, Spain, Australia, Norway, Luxembourg, Denmark, Portugal, Belgium, France, Iceland, Canada, and the U.S. Source: Transform Drug Policy Foundation.

[5]"It is unlawful to knowingly lease, rent, use, or maintain any place, whether permanently or temporarily, for the purpose of manufacturing, distributing, or using any controlled substance." 21 U.S.C. § 856, "Maintaining Drug-Involved Premises."

treatment areas, and fewer discarded syringes.

The relevance of international studies to the current situation in the U.S. is not clear. Even small differences in the details of how OPCs in other countries are established can affect the outcomes in other countries in certain ways that make them less relevant as comparisons to what might happen in New York City. There is one U.S. study, Kral et al., 2020, that finds that there had been no overdose deaths at an unsanctioned supervised injection site in the U.S.(location not disclosed) from 2014 to 2019 and that 33 opioid-involved overdoses at the site were reversed by the staff using naloxone. However, the study is only descriptive and, hence, does not allow for making causal inferences. Harocopos et al., 2022 describe the first 2 months of operation and use at the NYC OPCs. Between November 30, 2021, and January 31, 2022, 613 individuals used OPC services 5975 times across 2 sites. Among all participants, 75.9% reported that they would have used their drugs in a public or semi-public location if OPC services had not been available. Trained staff responded 125 times to mitigate overdose risk. More than half of individuals using OPC services (52.5%) received additional support during their visit.

This study is the first quasi-experimental study to evaluate overdose prevention centers and their effect on overdose mortality and other health outcomes based on the first two publicly recognized U.S. OPCs. I estimate standard difference-in-difference models comparing state-level monthly drug overdose mortality in New York City to a synthetic control unit formed by the rest of the U.S. The design exploits within-state variation in mortality and controls flexibly for common shocks caused by, for instance, the Covid-19 pandemic. I find that OPCs increased the overdose mortality in New York City by 0.30 per 100,000 population or by 14 percent. The effect is statistically insignificant, regardless of the model choice, possibly due to the moderate power of the natural experiment. I do, however, identify a positive and statistically significant effect of OPCs on entry to drug abuse treatment. OPCs led to an increase of 64 per 100,000 population, or 19 percent, in the number of people admitted

to substance use disorder treatment. This increase comes from the higher rate of admissions into rehabilitation by already-existing drug users rather than higher drug use prevalence.

The results have important policy implications for coping with drug abuse, overdose mortality, and other health concerns as well as neighborhood-level outcomes, including public drug use, improperly discarded syringes, and drug-related crime. The findings are also very relevant to the current public policy agenda as on May 8, 2023, the U.S. government announced that for the first time, it will fund a large study based on the two sites in New York City and one opening this year in Providence, Rhode Island, measuring whether overdoses can be prevented by OPCs.[6]

The paper is organized as follows. Section 2 reviews harm reduction policies and describes the current outlook on overdose prevention centers. Section 3 elaborates on the data sources and details the sample. Section 4 lays out the empirical strategy and power calculations. Section 5 provides empirical results. Section 6 offers concluding remarks and policy considerations.

2 Background

There are four main different strategies to reduce drug use and its negative consequences for individuals and communities: Prevention, Treatment, Enforcement, and Harm reduction. OPCs fall under the "harm reduction" category. Harm reduction is a set of practical strategies and ideas that prioritize reduction in the negative consequences of a person's substance use over complete abstinence.

Harm reduction policies were first popularized in the 1980s in Europe as a response to the HIV epidemic. They have become increasingly relevant with the rising opioid crisis that

[6]Johnson K. US backs study of safe injection sites, overdose prevention. The Associated Press. Updated May 8, 2023. Accessed July 8, 2023. https://apnews.com/article/safe-injection-sites-opioids-overdose-addiction-d9bcca2500044bfc28f54330bb719ffd

has caused a significant increase in overdose deaths - 75% of overdose deaths involved opioids (NCHS, 2022). Harm reduction has historically been "controversial" because it enables those addicted to drugs to continue using and thus, is fundamentally different than an abstinence-only approach to recovery. Harm reduction focuses on educating people about safer substance use when an individual is not ready to commit to abstinence. One distinct feature of harm reduction is that it accepts the existence of substance use and the nature of drug use and addiction rather than punishing or condemning people who use drugs. Abstaining from drugs may not be a realistic expectation for every drug user; harm reduction acknowledges that and meets people who use drugs "where they are at" (National Harm Reduction Coalition). The first step in harm reduction is controlling the situation when drugs are being used and the second step is focusing on decreasing the amount and frequency of drug consumption.

The arguments for OPCs are that they 1) reduce overdose deaths (Gostin et al., 2019; Levengood et al., 2021), 2) provide clean needles to prevent the transmission of infectious diseases like HIV or Hepatitis C (Kral et al., 2020), 3) act as an entry into substance use treatment and deliver medical and social services (Visconti et al., 2019), 4) reduce public drug use and syringe and/or other drug litter (Potier et al., 2014) and 5) lower public spending due to a reduction in disease, overdose deaths, and need for emergency medical services (Irwin et al., 2017; Behrends et al., 2019).

There is, however, still minimal evidence that OPCs can provide a net benefit to society. Critics claim that OPCs may encourage drug use and increase crime rates. The economic theory of moral hazard argues that drug users who use an OPC might use more drugs and be less cautious knowing that if they overdose medical intervention is immediately available. Although OPCs may prevent death from overdose from occurring on-site, increased drug use could increase overdoses outside of the OPC with the effect of a net increase in total overdose deaths. It is another concern that the sites are usually where low-income communities and communities of color live and that they may create centers for illicit drug

sales by attracting drug dealers. One can argue that more proof is needed to support the opening of overdose prevention centers, although the existing research shows that most of these worries are unfounded. Andresen and Boyd (2010) reports an average benefit-cost ratio of 5.12:1 as a conservative estimate for the OPC in Vancouver, Canada. Levengood et al. (2021) finds an abrupt, persistent decrease in crime after the opening of the same supervised injection site in Vancouver. While it is true that OPCs often concentrate in more "economically disadvantaged" locations, these communities are those who are hit the hardest by the overdose epidemic. New York City Department of Health and Mental Hygiene in a 2019 report on "New York's response to drug use and the overdose crisis" states that the "War on Drugs and other zero-tolerance policies decreased economic opportunity, exacerbated poverty, and limited the availability of treatment and social services", especially for low-income people and people of color.

About 6,000 individuals in New York State died from an overdose in 2021 —almost half of them in New York City—, according to the state Comptroller's Office. The City's Health Commissioner Dr. Dave Chokshi, also an advocate for opening OPCs in the city, stated that someone dies of a drug overdose every four hours in New York City. The U.S. Department of Justice has been against authorizing the operation of OPCs because of legal considerations, however, the Biden administration is willing to try harm-reduction strategies if it means saving lives.[7] Data for Progress, a data science think tank and polling firm, reported in 2022 that among all Democrat, Republican, and third-party voters, 64 percent (with a +33-point net margin) support opening overdose prevention centers. Rhode Island has become the first U.S. state to pass legislation to authorize state-regulated supervised drug-use facilities. On Feb 1, 2024, the Providence City Council agreed to open a safe injection site later this

[7]Mann B. Overdose deaths are so high that the Biden team is embracing ideas once seen as taboo. NPR. Updated October 27, 2021. Accessed July 8, 2023. https://www.npr.org/2021/10/27/1049245787/biden-hhs-tackle-drug-overdose-deaths

year. [8,9] There have been discussions of allowing OPCs in the states of Nevada, New Mexico, Massachusetts, and Washington. On the other hand, the governors of California and Vermont vetoed safe injection site bills last year. Pennsylvania's Senate voted for a ban on them in May 2023 after a very public legal battle started in 2019 between the Trump administration and Safehouse, the nonprofit group advocating opening an overdose prevention center in Philadelphia. Another opposing legislative decision had come from Colorado 6 days before the decision in Pennsylvania when lawmakers killed a bill in committee that would have authorized the sites in the state. [10,11]

Right now the two OPCs in New York run by the nonprofit OnPoint NYC are the only government-approved safe consumption program. The supervised injection sites are privately owned and operated without a state license or other oversight. This may soon change. Members of New York's Assembly Health Committee narrowly advanced legislation that would pave the way for the state to license community-based organizations to operate safe injection sites in March 2023.[12] New York would become the second after Rhode Island if the legislation passes both houses of the legislature.

Initial data from New York City's two OPCs appear encouraging. Health professionals have used the medicine Naloxone to reverse more than 670 overdoses at the two OPCs over the span of 13 months (December 2020 through December 2022; New York City Department

[8]Lisa K. Overdose expert: Allow supervised injection sites. Spectrum News. Updated March 24, 2023. Accessed July 8, 2023. https://spectrumlocalnews.com/nys/central-ny/politics/2023/03/24/overdose-expert--allow-supervised-injection-sites-

[9]Betts A. Providence Officials Approve Overdose Prevention Center. The New York Times. Updated Feb. 4, 2024. Accessed April 11, 2024. https://www.nytimes.com/2024/02/04/us/ri-opiod-safe-injection-site.html?searchResultPosition=1

[10]Pennsylvania Senate votes to ban safe injection sites. The Associated Press. Updated May 1, 2023. Accessed July 8, 2023. https://apnews.com/article/opioid-heroin-safe-injection-sites-pennsylvania-drugs-3c94134829464b858e02e3799ac3ae2b

[11]Ovalle D. Overdose prevention centers are tough sell in U.S. despite successes. The Washington Post. Updated May 8, 2023. Accessed July 8, 2023. https://www.washingtonpost.com/health/2023/05/08/overdose-prevention-centers/

[12]A.B. A338A, New York's Assembly, 2023-2024 Legislative Session (NY 2023). https://www.nysenate.gov/legislation/bills/2023/A338/amendment/A

of Health and Mental Hygiene).[13] Along with health concerns, an increase in drug abuse impacts healthcare costs, criminal justice spending, and lost productivity. When these broader costs are taken into account, the impact of substance abuse can be staggering. The CDC estimated the total cost of opioid abuse to the U.S. as $1.02 trillion in 2017. Adapting CDC's methodology, the U.S. Congress Joint Economic Committee (JEC) estimates the opioid epidemic cost $1.04 trillion in 2018, $985 billion in 2019, and nearly $1.5 trillion in 2020.[14] Given opioids are the main driver of drug overdose deaths — there are other drug categories — and the rising trend in drug overdoses suggests that the total cost is likely to continue to increase. A Health Department feasibility study found that opening four OPCs in NYC would save up to 130 lives and $7 million in direct health care costs per year.[15]

Despite the obvious gains in preventing substance abuse addiction, the use of overdose prevention centers as one of numerous strategies is not obvious. There is a gap in the literature where causal evidence for these centers is vital for the OPC debate in the U.S. Descriptive studies are insufficient to make a concrete case for (or against) them while some causal studies from other countries are not comparable to the U.S. context. There is an urgent need for effective policies to address drug abuse and its consequences. Causal research provides reliable insights to achieve such policies. This study aims to contribute to this agenda.

[13] https://www.nyc.gov/site/doh/about/press/pr2023/overdose-rates-reach-unprecedented-levels.page

[14] Joint Economic Committee Democrats. Cost of Opioids Issue Brief. (2022, September). The Economic Toll of the Opioid Crisis Reached Nearly $1.5 Trillion in 2020. https://www.jec.senate.gov/public/index.cfm/democrats/2022/9/the-economic-toll-of-the-opioid-crisis-reached-nearly-1-5-trillion-in-2020

[15] New York City Department of Health and Mental Hygiene. Overdose prevention in New York City: supervised injection as a strategy to reduce opioid overdose and public injection. 2018. https://www.nyc.gov/assets/doh/downloads/pdf/public/supervised-injection-report.pdf

3 Data & Sample

I use the National Vital Statistics System (NVSS) multiple cause-of-death mortality files by jurisdiction of residence and monthly drug overdose mortality data which reports provisional drug overdose death counts for all states, DC and NYC.[16] The data is available between January 2015 and August 2023.[17] In the paper, I exclude July and August 2023 data as it is underreported for most states. It should be noted that the numbers reported for the state of New York by NVSS exclude New York City. Mortality statistics and cause-of-death classifications follow from the World Health Organization's International Statistical Classification of Diseases and Related Health Problems (ICD). Drug overdose deaths in the NVSS data are identified using underlying cause-of-death codes from the Tenth Revision of ICD (ICD–10): X40–X44 (unintentional), X60–X64 (suicide), X85 (homicide), and Y10–Y14 (undetermined). Figure 1 shows the number of overdose deaths per 100,000 population in the U.S. and New York City between January 2015 and June 2023 by month. The latest data shows that the monthly overdose death rate was 2.55 (32.37/year) in the overall U.S. and 3.24 (35.51/year) in New York City. In both the U.S. and New York City, there is a rising trend in overdose death rates. The increase is even starker after 2020 as a result of the Covid-19 pandemic. It is also striking that the overdose mortality in New York City surpassed the national average in the most recent years.

[16]From NVSS Technical Notes: *Provisional data are based on available records that meet specific data quality criteria at the time of analysis and may not include all deaths that occurred during a given time period. Therefore, they should not be considered comparable with the final data and are subject to change.* In this paper, 2015-2021 data are final whereas 2022 and 2023 numbers are provisional.

[17]NVSS releases estimates for the United States and jurisdictions with a time lag. The time between when the death occurred and when the data are available for analysis is usually longer for overdose deaths (Spencer & Ahmad 2016). Since February 2022, the previous 6-month lag was shortened to 4 months. NVSS Technical Notes states that estimates produced with the 6-month and 4-month lags had a negligible difference for both national and state-level predicted drug overdose death counts.

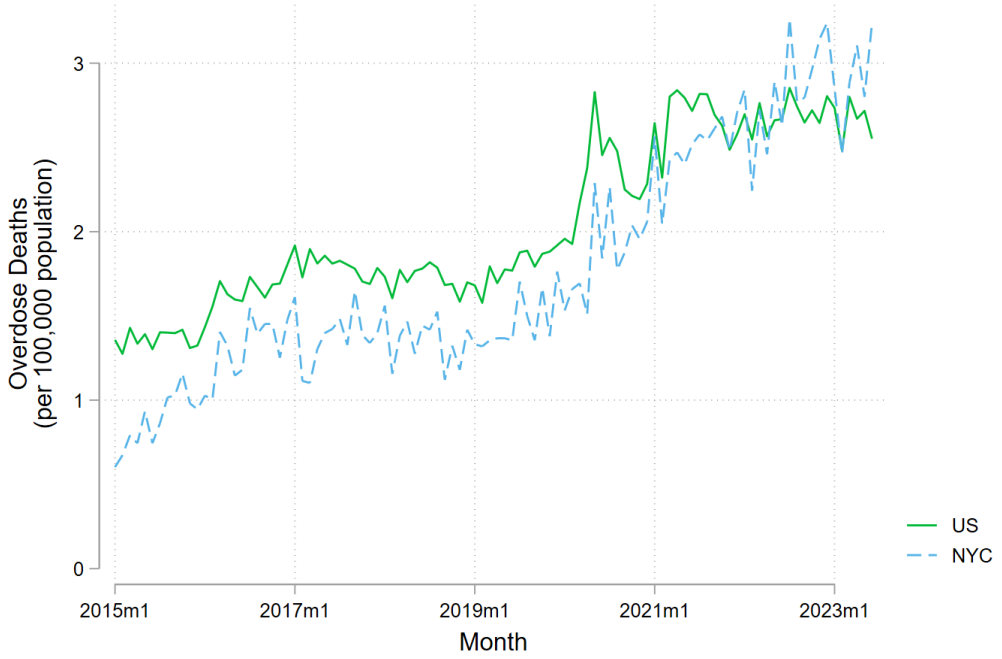


Figure 1: Drug Overdose deaths per 100,000 Population, Jan 2015 - June 2023

The additional health outcome I consider is the number of admissions to the treatment facilities that provide care for people with a substance use disorder. Overdose prevention centers not only aim to reduce deaths from drug overdoses but also create opportunities to engage with and educate people who use drugs on the risks of drug abuse. It is argued that OPCs increase referrals to and uptake of drug treatment and detoxification and, over time, are associated with drug use cessation (Kimber et al., 2008; Wood et al., 2006; DeBeck et al., 2011). I hypothesize that the OPCs in New York City may have increased treatment seeking among those who have utilized these facilities. I use The Treatment Episode Data Set (TEDS) conducted by the Substance Abuse and Mental Health Services Administration (SAMHSA). TEDS is a national census of annual admissions to substance abuse treatment facilities of individuals aged 12 years and older. I use the data for admissions from 2015 to 2022 to coincide with the mortality data.

I exploit multiple data sources to include controls in my research design. The variables considered are as follows: population size, cigarette and beer taxes, marijuana laws, per

capita income, and the unemployment rate. The literature documents that drug abuse is correlated with the use of other substances such as alcohol and tobacco (Catalano et al., 2011; Fiellin et al., 2013; Bachhuber et al., 2014). There is some evidence that substance use increases as a result of economic crises and recessions linking overdose deaths to lower GDP and unemployment (Carpenter et al., 2017; Dom et al., 2016; E. Brown and Wehby, 2019). There is also evidence that some sub-populations within society may be more vulnerable than others; African Americans were found to be more likely than whites to use heroin and cocaine and to have more severe alcohol problems (Wu et al., 2010). Previous studies also associate policing efforts with lower drug use (Cooper et al., 2005; Davis et al., 2005; Corsaro et al., 2013). Demographics (percentage of males, percentage of Black or African-American, percentage of Hispanic, and percentage of college graduates) and the number of police officers per capita, however, only vary at the state-year level and do not provide additional variation. Thus, they are collected for descriptive purposes but not used in the analysis.[18] I obtain state-year level population totals and population by characteristics such as age, sex, and race/ethnicity from the U.S. Census Bureau. I use the population estimates as of July 1st of every year. Data on educational attainment by state and year is obtained from the American Community Survey (1-year estimates). Cigarette taxes are from CDC's State Tobacco Activities Tracking and Evaluation (STATE) System and marijuana laws are hand-collected. Beer tax data comes from the National Institute on Alcohol Abuse and Alcoholism and vary by state. The number of police per capita at the state-year level is obtained from the Bureau of Justice Statistics based on the FBI's Uniform Crime Reporting (UCR) database. For each state, quarterly per-capita income data is obtained from the Bureau of Economic Analysis, and monthly unemployment data from the Bureau of Labor Statistics. The numbers for New York City are calculated separately and subtracted from the NY state totals to be consistent with the NVSS data.

[18] For further details on the exclusion of these variables, see Section 4.

The main analysis in this paper considers New York City as the treatment group where OPCs exist and the rest of the U.S. as the control group without any (government-approved) OPCs. Table 1 shows the descriptive statistics for relevant variables for both New York City and the rest of the U.S., before and after the launch of OPCs in November 2021. The table depicts how the population composition and location characteristics of New York City compare to the national average, excluding New York City. For my identification strategy (see Section 4), it is important to identify the confounding variables as well as the trends in the factors that may potentially affect the outcomes I am interested in. One can observe that New York City is different than the rest of the U.S. taken as a whole. The percentage of Black or African-Americans and Hispanics and college graduates is higher in New York City than those of the U.S. Real GDP per capita (chained 2012 dollars) is almost twice the size of that for the rest of the country while the unemployment rate is also above the national average. The excise tax for cigarettes is higher but lower for beer. It is also important to note the different trends in these variables between New York City and the rest of the U.S. Compositional changes in demographics across years seem similar in both groups. The number of police increases by 31% in the rest of the country on average versus virtually no change in New York. Real GDP per capita growth is larger in New York City (11.9% vs. 5.7%), however, the unemployment rate decreases more (29.0% vs. 11.1%) in the rest of the U.S. Not presented in Table 1 is the information on marijuana laws. It is argued that marijuana use is likely to precede the use of other licit and illicit substances (Secades-Villa et al., 2015) and the development of addiction to other substances, potentially leading to more drug overdoses. As of February 2023, 28 out of 51 "states"[19] have decriminalized marijuana use, 38 have legalized medical marijuana and 21 have fully legalized recreational marijuana (The Council of State Governments, 2023). The state of New York, and New York City, have allowed medical marijuana use since January 2015. Marijuana use was decriminalized

[19]Including District of Columbia.

in September 2019 and more recently recreational marijuana became legal in April 2021[20], making it the 15th state to legalize marijuana.

Table 1: Characteristics of the Study Population

	Pre-OPC		Post-OPC	
	Rest of the U.S.	New York City	Rest of the U.S.	New York City
Covariates†				
Real GDP per capita (US$)	55,129	95,998	58,267	107,429
Unemployment rate	4.79	6.50	3.40	5.78
Cigarette tax (US$)	1.74	4.35	1.92	4.35
Beer tax (US$)	0.318	0.140	0.319	0.140
Additional variables‡				
% Male	49.5	47.8	49.8	48.1
% Black or African-American	11.7	27.7	11.9	27.6
% Hispanic	11.9	29.1	12.7	29.1
% College graduates	29.6	36.5	32.6	39.6
Number of police (per 100,000 population)	314	612	412	614

Notes: State-year level population totals and population by characteristics are from the U.S. Census Bureau. The population estimates are as of July 1st of every year. Data on educational attainment by state and year is obtained from the American Community Survey (1-year estimates). Cigarette taxes are from CDC's State Tobacco Activities Tracking and Evaluation (STATE) System and marijuana laws are hand-collected. Beer tax data comes from the National Institute on Alcohol Abuse and Alcoholism and vary by state. The number of police per capita at the state-year level is obtained from the Bureau of Justice Statistics based on the FBI's Uniform Crime Reporting (UCR) database. Quarterly per-capita income data is obtained from the Bureau of Economic Analysis, and monthly unemployment data from the Bureau of Labor Statistics.
New York City represents the exposed "treatment" group where OPCs exist and the rest of the U.S. represent the donor pool for the comparison group without any (government-approved) OPCs.
Pre- and posdt-OPC refer to the period before and after the launch of OPCs in November 2021, respectively.
† Covariates are the main control variables that are included in the estimation model.
‡ Additional variables are provided for descriptive purposes only. For further details on the exclusion of these variables, see Section 4.

[20] "It is now legal for adults 21 and older to possess up to three ounces of cannabis and up to 24 grams of concentrated cannabis for personal use in New York.", New York City Department of Health.

4 Research Design

4.1 Empirical Methodology

Recent literature focuses on two methodologies to evaluate the effects of policy changes using panel or repeated cross-sectional data: Difference in Differences (DID) methods (Ashenfelter and Card, 1985; Bertrand et al., 2004; Angrist and Pischke, 2008) and Synthetic Control (SC) methods (Abadie and Gardeazabal, 2003; Abadie et al., 2010; Abadie et al., 2015; Abadie and L'hour, 2021). The traditional DID model assumes a $2x2$ design such that there is one pre- and one post-treatment period with a single unit exposed to treatment whereas another unit is unexposed. The policy effect is simply estimated by taking the difference of the values of the outcome of interest between the two periods for the two units, separately, and then taking the difference of those two differences calculated before —difference-in-differences. A generalization of the DID model, and more common in the literature, is the two-way fixed-effects models where one has multiple groups and time periods and the causal effect is the weighted average of all two-group/two-period DID estimators described above. DID methods compare the experiences of the exposed and unexposed comparison groups, all weighted equally, before and after the policy change under the assumption that in the absence of treatment, the difference between the exposed and unexposed comparison groups is constant over time — the parallel trends assumption. The parallel trend assumption asserts that outcomes among exposed and unexposed groups have similar trends prior to the intervention and these trends would continue in parallel after the intervention had the intervention not occurred conditional on additive unit and time fixed effects. One drawback of DID methods is that inference depends on asymptotic properties that are plausible under multiple treatment units exposed to the policy (**buch (mueller2011effect**; Donald and Lang, 2007) and the estimates are only valid if the parallel trends assumption is satisfied. SC methods offer an alternative for cases where there is only one, or a small number of,

treatment units by introducing non-unique weights to DID framework. SC methods re-weight unexposed units, often referred to as the "donor pool"), such that the pre-policy trends for the treatment unit are matched with those for a weighted "synthetic" unit, removing the need for parallel trends assumption. Further, unlike the DID methods, SC methods allow the effects of observed and unobserved predictors of the outcome to change over time, while assuming that pre-intervention covariates have a linear relationship with outcomes post-treatment (Kreif et al., 2016).

In this paper, I follow Arkhangelsky et al. (2021) and employ the "synthetic difference in differences (SDID)" methodology which combines DID and SC models. As in SC methods, SDID re-weights and matches pre-treatment trends in outcome variables not just across units but also across time periods. This allows the researcher to identify units that on average are similar in terms of their past to the treated unit by assigning them more weight as well as the periods that are on average similar to the post-treatment periods. The weighted units are then used in a regular two-way fixed effects regression (DID) to estimate the causal effect of the policy. Unit and time weights provide comparable units and time periods making DID estimates more robust.

I prefer the SDID methodology to take advantage of re-weighting and matching both the units and the time periods as I deal with only one treatment unit (New York City) and a relatively shorter post-treatment period (20 months). After computing unit and time weights[21], I employ a basic difference-in-differences estimation methodology on the weighted data to estimate the effects of OPCs on health outcomes. I compare the experiences of the treatment (New York City) and control groups (weighted "synthetic" unit based on all states excluding New York City) before and after November 2021 when the OPCs opened in New York City.

[21] See Arkhangelsky et al. (2021) for the details of the optimization problem that solves for SDID weights.

I estimate the following model:

$$Y_{it} = \alpha + \beta NYC \times Nov2021 + X_t'\Gamma + f_i + f_t + \varepsilon_{it} \quad (1)$$

where $Y_{i,t}$ is the health outcome for state i in month t, $Nov2021$ is a dummy variable for pre- and post-treatment periods taking 0 for the months before November 2021 and 1 otherwise, NYC is a dummy variable indicating that the "state" is New York City. The main variable of interest, $NYC \times Nov2021$, is the interaction of New York City and the post-treatment dummy. The main parameter of interest, β, measures the change in the health outcome for the treatment group due to the policy change compared to the control group. The vector $X_{i,t}$ is a set of control variables, namely, cigarette and beer excise taxes, whether marijuana is legalized or not, per capita income, and the unemployment rate.[22] f_i donates state fixed effects, f_t is a set of month fixed effects and $\varepsilon_{i,t}$ is the error term. Other potentially relevant variables reported in Table 1 are not included in the model to avoid over-fitting (Borusyak et al., 2021). As these variables only vary at the state-year level, the majority of their variation is captured by the state and month fixed effects with too little residual variation remaining. The standard errors are clustered at the state level to account for the correlation between time periods for each state. The model assumes the "placebo method" standard error estimator where only the untreated units are sampled without replacement to estimate the variance.[23] One should note that the model treats NYC as a separate "state" as the data for the state of New York excludes that for New York City.

The primary outcome variable is the number of overdose deaths per 100,000 population. The coefficient β estimates how the OPCs affected the overdose mortality rate in New York City relative to the rest of the U.S. (synthetic NYC). A negative sign on β would indicate

[22]The real GDP per capita is available quarterly; excise tax data, marijuana laws, and the unemployment rate are monthly.

[23]See Bottmer et al. (2023) for a discussion of the properties of such placebo variance estimators in small samples.

that OPCs caused a decrease in the overdose mortality rate in New York City compared to the control states and positive sign the opposite.

The SDID methodology "guarantees" parallel trends through weighting and allows for causal interpretation of estimates. To support this claim, Figure 2 depicts the mean of overdose mortality for the treatment (red) and control (blue) groups over time. The vertical red line shows the month when OPCs began operating in New York City. SDID seems to provide a good match between treatment and control groups as the pre-treatment trends in overdose deaths are very similar and track each other closely. Another thing to note is that the number of overdose deaths in New York City seems to have increased after November 2021, after an initial decrease, closing the pre-treatment gap between the treatment and control groups. Section 5 quantifies this observation and provides some robustness checks where I re-estimate the model using different identification strategies and an event study analysis to map out the dynamic impact of the intervention.

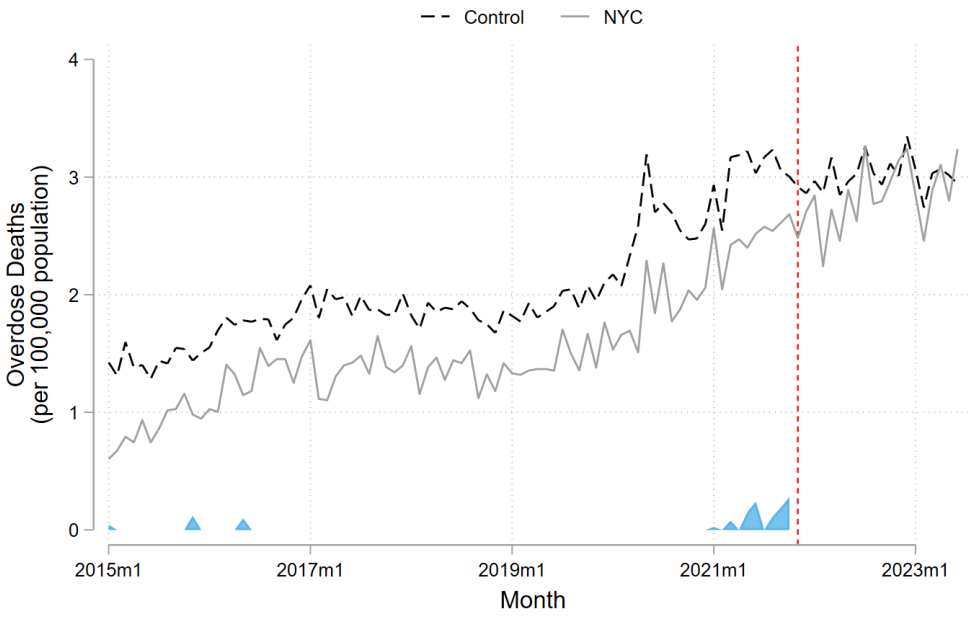


Figure 2: Common Trend Check

4.2 Power Calculations

Statistical power measures an experiment's ability to detect differences between the treatment and the control groups given a set sample size and other parameters. In the case of null results, it can be difficult to distinguish between lack of effect and lack of power. Power calculations can be done in two ways: determining the sample size needed to detect the minimum detectable effect (MDE) given some parameters or determining the effect size that can be detected given a fixed sample size and other parameters. Through power calculations, researchers can determine the smallest effect that, if true, has a certain chance of producing an impact estimate that is statistically significant at a certain level ex-ante or report their actual results along with the MDE ex-post. This approach provides an intuitive metric to distinguish tightly identified null results from those that fail to reach significance but for which we cannot rule out treatment effects with confidence. In this section, I compute the minimum detectable effect size, that is, the effect that would have been detectable with 80% power at the two-tailed 5% significance level in multiples of the standard deviation of the outcome (Duflo et al., 2007).

To estimate the minimum detectable effect of a proposed experimental design, one must estimate the standard error of its impact estimate and take an appropriate multiple based on the significance level, power value, and one- or two-sided hypothesis test. For example, the minimum detectable effect for a two-sided hypothesis test at the 0.05 level, which I use in my calculations, is 2.80 times the standard error at 80% power. For the standard error of the impact estimate, I use the following formula from Bloom, 1995:

$$\sigma_C = \sqrt{\frac{\sigma^2 \left(1 - R^2\right)}{T(1-T)n}} \tag{2}$$

where σ_C is the standard error of the impact estimator for a continuous outcome, σ is the standard deviation of the continuous outcome, T is the proportion of the study sample

that is randomly assigned to the treatment group, n is the size of the study sample and R^2 is the explanatory power of the impact regression.

There are 5,304 observations in this study where 20 are assigned to treatment. Assuming a conservative R^2 of 0.5, the standard error of the impact estimator is 0.158 times the standard deviation of the outcome. Multiplying it by 2.80, the minimum detectable effect size is around 0.44 times the standard deviation of the outcome. Cohen (1988) proposes that an effect of 0.2 standard deviation is "small", 0.5 is "medium" and 0.8 is "large". Based on this benchmark, I conclude that the natural experiment I evaluate is sensitive enough to capture sizeable treatment effects. It may, however, fail to detect smaller differences between control and treatment groups. When evaluating the results of the study, one should keep in mind that the null effects presented may be "imprecise zeros" and a result of a lack of power.

5 Results

5.1 Baseline Estimates

Table 2 shows the synthetic difference-in-differences regression estimates for overdose mortality. Columns (1) - (5) report the estimates from models without any controls, with covariates using the "optimized" method, with covariates using the "projected" method, without any controls while omitting the rest of New York from the donor pool, with covariates using the "projected" method while omitting the rest of New York from the donor pool. I only report the coefficient of interest which is the differential treatment effect on overdose mortality in New York City. In each model, standard errors are clustered at the state level.

The inclusion of covariates in the calculation of the synthetic counterfactual in SDID identification strategy is adjusted in two ways: "optimized" in which case it follows the

method proposed by Arkhangelsky et al., or "projected", in which case it follows the procedure proposed by Kranz (2022). In the "optimized" method, SDID is applied to the residuals of all units after regression adjustment. This procedure is different than the logic of synthetic controls proposed by Abadie et al. (2010). In Abadie et al. (2010), the synthetic control unit is created in a way that the included covariates are as closely matched as possible between treated and synthetic control units. In SDID, on the other hand, the covariate adjustment is done pre-matching, removing the impact of changes in covariates from the outcome variable before obtaining the synthetic control. Kranz (2022) argues that the residual approach may be problematic when time-varying exogenous covariates are present. The paper shows that the SDID estimate is still inconsistent after Arkhangelsky et al.'s adjustment if a covariate in the model is correlated with the time period and group and/or if it has high dispersion. Their suggested workaround consists of conducting regression adjustment based on parameters estimated only in untreated groups to avoid confounding. In columns (2) and (3), I present both estimates. As the coefficients are qualitatively similar (and both statistically insignificant), the estimates presented later in the paper assume "projected" covariates due to faster computation.

In addition to different adjustments for covariates, I show the estimates from regressions where I omit the rest of New York State from untreated units in columns (4) and (5). As noted, deaths are measured by place of residence. This measurement can be problematic for the identification strategy. Someone, for instance, who resides in the state of New York, but not in New York City, might still use OPCs in New York City and/or die from a drug overdose in New York City due to proximity. Based on CDC's measurement, this death counts towards the deaths in the control group although it occurs in New York City where OPCs, the treatment, are. These spatial spillover effects, if they exist, might bias the difference in differences estimates due to two reasons. First, the control group no longer identifies the counterfactual trend since their outcomes are affected by the treatment. Second,

the estimates do not entirely capture the treatment effect on treated units' outcomes since some "close" units are assumed to be in the control group but potentially exposed to the treatment. The estimates from the two models are similar to their counterparts where the "rest of NY" is included in the analysis (columns (1) and (3)), assuring that the spatial spillover effects are not a threat to the identification strategy.

All specifications return positive but statistically insignificant effects. I find that, on average, overdose prevention centers caused an increase of 0.30 overdose deaths per 100,000 population (or $\sim$ 3 in 1,000,000) in New York City between November 2021 and June 2023. Given the control mean of 2.12 per 100,000, this effect accounts for a 14% increase. Although quantitatively small and statistically insignificant, the estimate is counter to what advocates of OPCs would have hoped for. The 14%, however, is a point estimate. I cannot rule out a 13% decrease ($\sim$ -0.27 overdose deaths per 100,000 population) or a 38% increase ($\sim$ 0.81 overdose deaths per 100,000 population) given the 95% confidence interval. Nevertheless, there were 2,103 overdose deaths in New York City in 2020[24] (the last pre-treatment year). A 14% increase, approximately 300 additional deaths, is noteworthy.

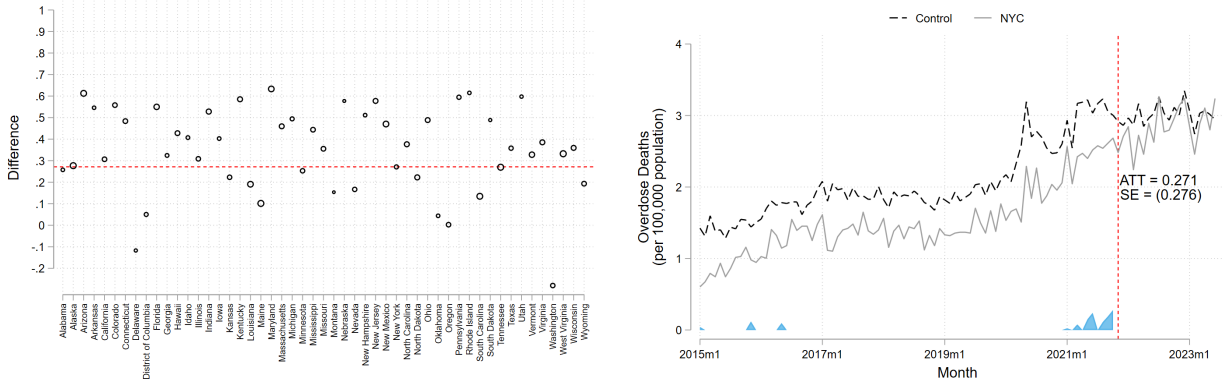
Table 2: Baseline SDID Estimates - Effect of OPCs on Overdose Mortality

	(1)	(2)	(3)	(4)	(5)
NYC X Post	0.344	0.359	0.271	0.346	0.268
	(0.272)	(0.271)	(0.276)	(0.277)	(0.283)
Covariates	No	Yes	Yes	No	Yes
Covariate adjustment	-	Optimized	Projected	-	Projected
Rest of NY omitted	No	No	No	Yes	Yes
N	5,304	5,304	5,304	5,202	5,202

Notes: The estimates in all columns are based on Equation 1. The variable *NYC X Post* is the main treatment variable taking the value of 1 for observations in New York City in and after November 2021. The full specification in Column (3) controls for cigarette and beer taxes, marijuana laws, per capita income, and the unemployment rate. Inference is based on a placebo procedure. The standard errors are clustered by state, and follow Algorithm 4 laid out in Arkhangelsky et al. (2021).
Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[24]NYC Office of Chief Medical Examiner and NYC DOHMH Bureau of Vital Statistics, 2023. Provisional data.

Based on my preferred model in Column (3), Figure 3 demonstrates the unit-specific weights (Panel (a)) as well as the outcome trends and time-specific weights (Panel (b)). Table A1 in Appendix further provides the unit- and time-specific weights.



(a) Unit-Specific Weights (b) Outcome Trends and Time-Specific Weights

Figure 3: Synthetic Difference in Difference Output for the Effect of OPCs on Overdose Mortality in NYC

5.2 Robustness Checks

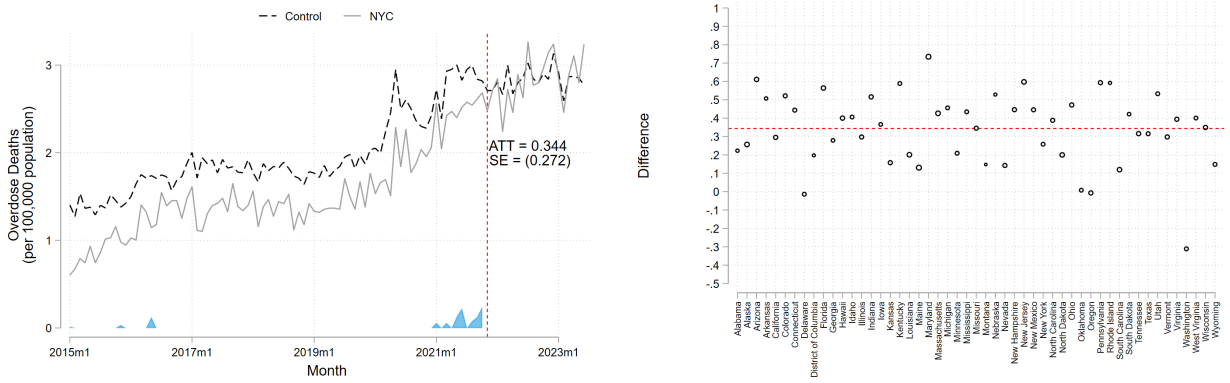
Alternative Identification Strategies

It is a useful exercise to compare the output of SDID estimation procedures with those of standard synthetic control methods of Abadie et al. (2010) (SC), and unweighted standard difference in differences estimates (DID). Figure 4 illustrates this comparison. In each case, the left column shows the outcome trends for matched treated and control/synthetic control units while the right column presents the weights received by each unit and time period (in the case of SDID). All models are estimated without covariates for easier interpretation.

Consistent with Arkhangelsky et al. (2021)'s claim, one observes that (1) the DID estimator overestimates the treatment effect, 0.461 vs. 0.344, and (2) the standard error obtained for SDID is smaller than that for DID provided by a better "local" fit due to weighting,

comparing similar units in similar time periods. At 0.502, the standard error in the DID procedure is almost twice the size of the standard error of 0.272 the SDID model returns. It is also apparent that, in DID, each control unit receives an identical weight, and each time period is weighted equally. The point estimate for the treatment effect is very close in SDID, 0.344, and SC, 0.366, while the former is more precise (standard error of 0.272 vs. 0.365) after the inclusion of time weights in addition to unit weights in SC. The outcome trends for SC differ from the other two methods as the values for treated and synthetic control units overlap by design. Panel (f) also shows that SC results in more sparse unit-specific weights with more weight given to fewer control units. Looking at the absolute weight values, SDID provides a more balanced weighting scheme compared to both DID and SC as it does not create "high influence" control units by assigning large weights. The lack of concentrated weights prevents superfluous variation in the estimator.

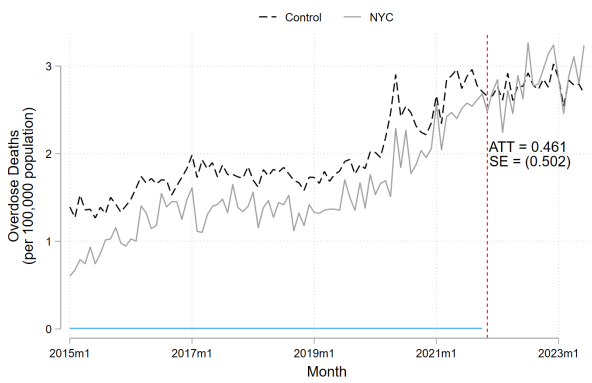
Despite the differences in the trends and weight structure, it is encouraging that the point estimates obtained by the three identification strategies are rather comparable, both direction-wise and size-wise. I conclude that the treatment effect is robust to different specifications, positive but statistically insignificant.



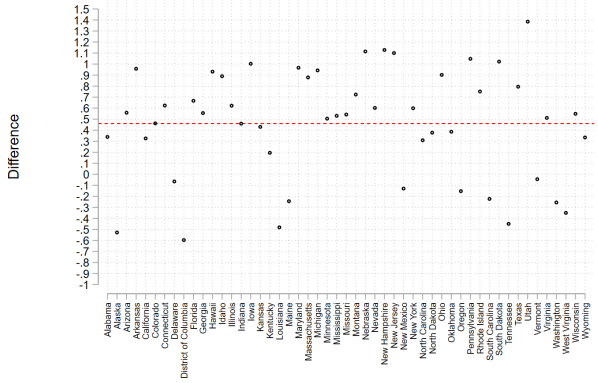
(a) SDID: Outcome Trends

(b) SDID: Unit-Specific Weights

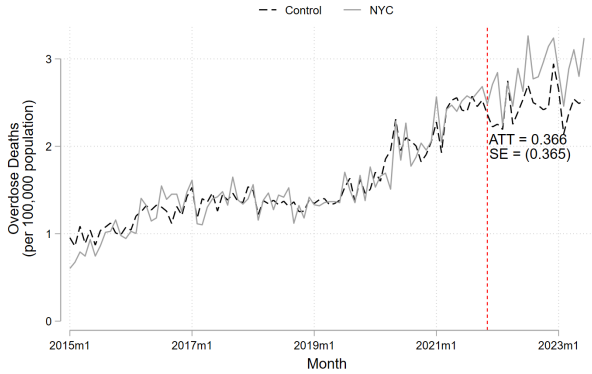
Figure 4: Comparison of Estimators



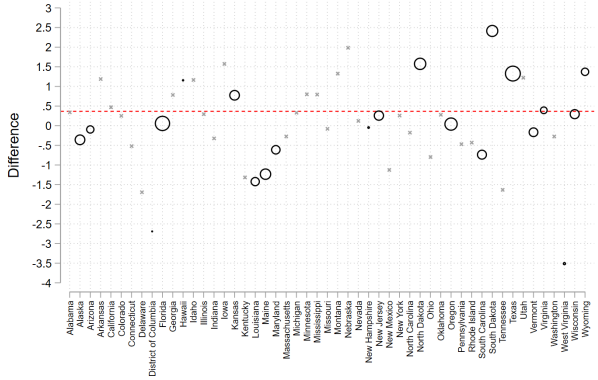
(c) DID: Outcome Trends



(d) DID: Unit-Specific Weights



(e) SC: Outcome Trends



(f) SC: Unit-Specific Weights

Figure 4: Comparison of Estimators (cont.)

Event Study Analysis

The estimates presented so far rely on the assumption that the opening of the two OPCs in New York City was exogenous and was not correlated with any unobserved variable. Given that I look at the monthly data that extends for more than 12 months, the dataset allows me to perform an event study that provides an additional examination of parallel trend assumption as well as mapping out the evolution of the treatment effect over time. Here, I apply the SDID implementation of an event study by Clarke et al. (2023) and follow their notation. An event study shows how differences between treated and control units in each

period t compare to baseline differences. Mathematically, I compute

$$\left(\bar{Y}_t^{Tr} - \bar{Y}_t^{Co}\right) - \left(\bar{Y}_{\text{baseline}}^{Tr} - \bar{Y}_{\text{baseline}}^{Co}\right), \tag{3}$$

where $\bar{Y}_{\text{baseline}}^{Tr}$ and $\bar{Y}_{\text{baseline}}^{Co}$ give the baseline or pre-treatment means for treated and control units, respectively. In standard event study analysis, this difference is calculated based on a chosen baseline period which is usually the period right before the treatment where the difference is normalized to zero. In SDID, however, the existence of time weights in the pre-period leads to a different calculation for baseline quantities as follows.

$$\bar{Y}_{\text{baseline}}^{Tr} = \sum_{t=1}^{T_{pre}} \widehat{\lambda}_t^{\text{sdid}} \, \bar{Y}_t^{Tr} \quad \bar{Y}_{\text{baseline}}^{\text{Co}} = \sum_{t=1}^{T_{\text{pre}}} \widehat{\lambda}_t^{\text{sdid}} \, \bar{Y}_t^{\text{Co}}, \tag{4}$$

where $\widehat{\lambda}_t^{\text{sdid}}$ refers to the pre-treatment weight in time t. $\bar{Y}_t^{Tr}$ and $\bar{Y}_t^{Co}$ are the outcome values for treated and synthetic control units in time t obtained by running the SDID model.

While Equation 3 returns the treatment effect in each period, the analysis also requires the estimation of a confidence interval for this effect. Clarke et al. (2023) suggest that one can re-sample and calculate the quantity in Equation 3 over and over for each t to get a variance measure based on these block-bootstrap samples which will then be used to estimate the confidence interval.[25]

Different than the specification in Equation 1, I apply the following changes when obtaining the event study analysis.

- I drop the months between March 2020 - August 2020 which was the first 6 months of the Covid-19 pandemic as extraordinary events can distort the individual estimates in the periods they were present. Since Covid-19 was an unprecedented global public health crisis that caused abrupt changes in everyone's lives, I avoid using the initial

[25] See Clarke et al. (2023) for the details of the procedure.

months of the pandemic in the analysis to limit possible contamination of the results from unusual trends in the outcome of interest and other explanatory variables.

- I drop the years before 2018 and use a shorter pre-event period to capture recent/relevant time trends and avoid inaccuracies that may result from extending the estimation window (El Ghoul et al., 2023). In this case, t ranges from January 2018 to June 2023. The analysis includes 40 pre-treatment and 20 post-treatment periods.

Neither of these changes makes a substantial difference for the average treatment effect since averaging across periods eliminates the noise in the data but they matter when estimating individual impacts for each period. Table A3 in Appendix presents the ATT estimates under these changes.

Figure 5 shows the impacts of OPCs on overdose mortality over time. Each blue dot gives the percentage impact at event time t along with the 95% confidence interval. One can observe that the pre-treatment trends between New York City and the synthetic control unit are matched closely given the fact that most confidence intervals prior to treatment include zero, except for few outliers. The event study suggests that during the 20 months OPCs were open, they increased the number of overdose deaths (per 100,000 population) in New York City by 0.33, on average.[26] In contrast to pre-treatment, the treatment effect is statistically significant for almost all post-treatment periods. The impact also seems to gradually increase over time with larger coefficients and confidence intervals moving further away from the zero line. One should note the wide confidence intervals signaling a certain degree of uncertainty and less precise impact estimates. This may result from a small sample size which also speaks to the power issues explained in Subsection 4.2. It also explains the lack of statistical significance in Table 2. Although the individual point estimate in almost every post-period is statistically significant, the average "overall" effect is insignificant due to low precision—there

[26]This is the simple average of post-treatment event study estimates presented in Table A2.

is a substantial overlap of confidence intervals pre- and post-treatment such that one cannot rule out a "null" effect. Nonetheless, the event study analysis offers suggestive evidence for a large positive effect of OPCs on overdose mortality that has increased over time. The coefficient estimates for the analysis are presented in Table A2 in Appendix.

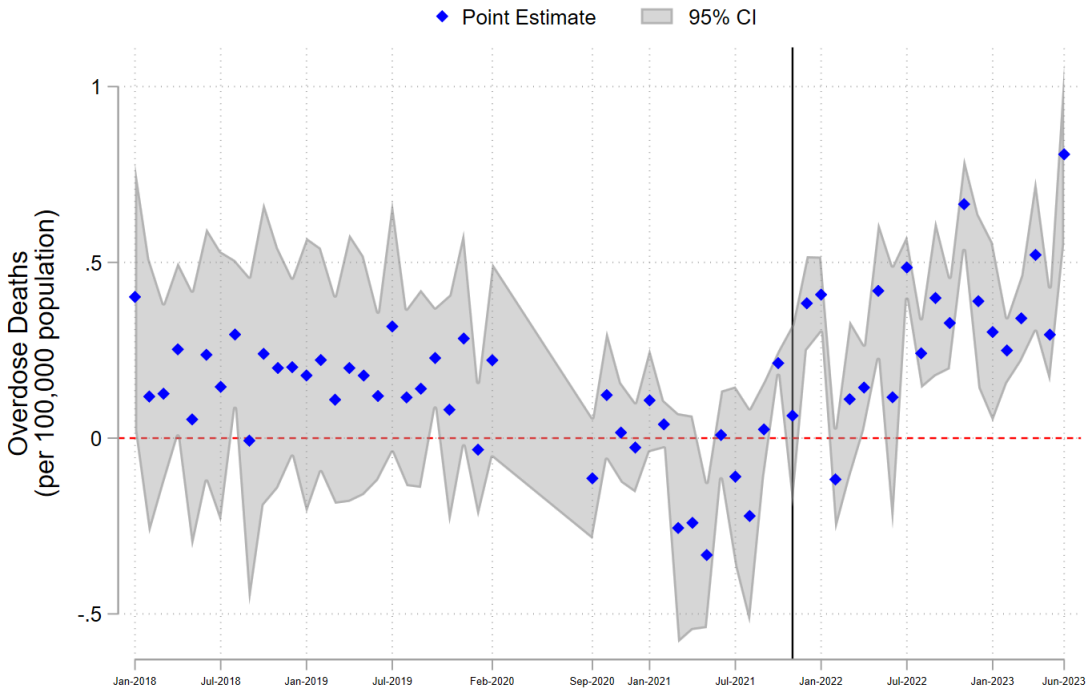


Figure 5: Event Study Style Estimate of the Impact of OPCs on Overdose Mortality

Estimates Based on Overdose Deaths by Place of Occurrence

One can argue that the number of deaths might differ when it is based on place of residence vs. place of occurrence and the latter might be a better measure to identify the treatment effect. As mentioned in Section 4, deaths measured by place of residence may not include those that occurred in the treatment state, if the place of residence is different than the place of occurrence, leading to an underestimation of the treatment effect. To test this hypothesis, I rerun the main specification in Section 4 this time using the number of deaths reported by

place of occurrence instead of that reported by place of residence. CDC discloses deaths by place of occurrence only for recent vintages starting from 2018. Thus, the analysis in this section is based on the data between January 2018 and June 2023.

Following the same structure as Table 2, Table 3 reports the synthetic difference-in-differences regression estimates for overdose mortality based on the number of deaths reported by place of occurrence. I, again, only present the coefficient of interest which is the differential treatment effect on overdose mortality in New York City. In each model, standard errors are clustered at the state level. I omit the rest of New York State from untreated units in columns (4) and (5) since, similar to the arguments done for deaths by place of residence, the control group may no longer identify the counterfactual trend if their outcomes are affected by the treatment. Although the OPCs are in New York City and most likely affect predominantly New York City residents, individuals living close to the city (but not in the city) may still use the facilities "potentially contaminating the exposed units". Some of these individuals may die from a drug overdose outside of the city, i.e., where they reside. Vice versa, they may not use the OPCs in the city but may die from an overdose there. If this is the case, the estimates not only capture the treatment effect on treated units' outcomes but also the effect on "close" units. As in Table 2, the estimates from the two models are similar to their counterparts where the "rest of NY" is included in the analysis (columns (1) and (3)) such that the spatial spillover effects are not a threat to the identification strategy when using the number of deaths by place of occurrence instead of place of residence.

For every model, the treatment effect is comparable to the one where the number of drug overdose deaths is based on place of residence, although the point estimates are slightly larger. Although once again statistically insignificant, under this new measure of the number of overdose deaths, OPCs led to an increase in overdose mortality by around 0.3 deaths per 100,000 population on average. The proximity of estimates compared to Table 2 is reassuring that any bias due to the difference in measurement of deaths—place of residence vs. place

of occurrence—is likely minor.

In the interests of completeness, I also report the estimates from the standard synthetic control model and unweighted standard difference in differences model outlined in Section 5.2 for the model in Column (1) without covariates. Table 4 presents the three estimates. As in Figure 4, the DID estimator overestimates the treatment effect, and while the coefficients are closer in SDID and SC, SDID methodology provides the most precise estimate among the three. The point estimates, however, are still analogous. I conclude that using the number of deaths reported by place of occurrence instead of that reported by place of residence does not alter the findings and that the results remain stable under alternative identification strategies and thus, are robust.

Table 3: Baseline SDID Estimates - Effect of OPCs on Overdose Mortality based on Number of Deaths by Place of Occurrence

	(1)	(2)	(3)	(4)	(5)
NYC X Post	0.355	0.363	0.292	0.358	0.297
	(0.308)	(0.294)	(0.313)	(0.330)	(0.322)
Covariates	No	Yes	Yes	No	Yes
Covariate adjustment	-	Optimized	Projected	-	Projected
Rest of NY omitted	No	No	No	Yes	Yes
N	3,432	3,432	3,432	3,366	3,366

Notes: The estimates in all columns are based on Equation 1. The variable *NYC X Post* is the main treatment variable taking the value of 1 for observations in New York City in and after November 2021. The full specification in Column (3) controls for cigarette and beer taxes, marijuana laws, per capita income, and the unemployment rate. Inference is based on a placebo procedure. The standard errors are clustered by state, and follow Algorithm 4 laid out in Arkhangelsky et al. (2021).
Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Alternative Estimates - Effect of OPCs on Overdose Mortality based on Number of Deaths by Place of Occurrence

	Synthetic Difference in Difference (SDID)	Standard Difference in Difference (DID)	Synthetic Control (SC)
NYC X Post	0.355	0.499	0.277
	(0.308)	(0.430)	(0.344)
Covariates	No	No	No
Covariate adjustment	-	-	-
Rest of NY omitted	No	No	No
N	3,432	3,432	3,432

Notes: The estimates in all columns are based on Equation 1. The variable *NYC X Post* is the main treatment variable taking the value of 1 for observations in New York City in and after November 2021. All models are estimated without covariates for easier interpretation. Inference is based on a placebo procedure. The standard errors are clustered by state, and follow Algorithm 4 laid out in Arkhangelsky et al. (2021).
Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.3 OPCs and Entry to Substance Use Disorder Treatment

One argument for OPCs is that apart from creating a space for safer drug consumption, they also provide counseling services and referrals to substance use disorder (SUD) treatment. Through these services, there might be an indirect effect of OPCs on drug cessation. To test this hypothesis, I use the Treatment Episode Data Set (TEDS) by the Substance Abuse and Mental Health Services Administration (SAMHSA). TEDS is a national data system of annual admissions (TEDS-A) to and discharges (TEDS-D) from substance abuse treatment facilities in the U.S. The data collected by state agencies includes information on service setting, treatment details, substance abuse history and characteristics such as substances used, age at first use, route of use, frequency of use, and the number of previous admissions, and demographic variables such as age, sex, race/ethnicity, and employment status for each

record of admission and discharge.[27] The data is collected only for individuals 12 years old or older and is publicly available at the national and state level.

There are several limitations to the data documented by SAMHSA that concern my effort to identify the effect of OPCs on drug rehab/addiction treatment.[28] First, the data is admission-based and does not represent individuals in the sense that a person who was admitted to treatment multiple times shows up in the data multiple times as separate records. Second, there may be variations across states in the definition of admission and the referral process to treatment due to different criminal justice practices, making state comparisons challenging. Third, participating treatment facilities may vary from state to state. Publicly funded admissions have to be reported by law. While some states only collect data on publicly funded admissions to meet this requirement, others can collect privately-funded admissions as well from facilities that receive public funding. Fourth, states may target specific sub-populations such as pregnant women or adolescents to satisfy public funding constraints. Lastly, some states have no opioid treatment programs (OTPs) that provide medication-assisted therapy.

Noting these shortcomings, I follow the same specification in Equation 1 to estimate the effect of OPCs on entry to substance use disorder treatment in New York City using once again the synthetic difference-in-differences regression and TEDS-A data. The time unit is year instead of month since TEDS reports yearly admissions. I also assume that the post-treatment period only includes 2022 as SDID does not allow for a measure of "treatment intensity" in the model such that the treatment variable needs to be binary. Since the treatment was "on" only for 2 months in 2021, this is a fair assumption and not likely to alter results. I drop the states of Oregon (missing all years), Arizona, Delaware, Idaho,

[27]Please note that as of April 2024, SAMHSA has only reported annual totals without additional information for year 2022, meaning the analysis in the paper cannot speak to heterogeneous treatment effects for specific sub-populations.

[28]Please see https://www.datafiles.samhsa.gov/dataset/treatment-episode-data-set-admissions-2021-teds-2021-ds0001 for the latest information on data limitations.

Washington, and West Virginia (missing 2022 data), and North Dakota (missing 2020 data). One should note that TEDS only reports admissions for states meaning I do not have the data specifically for New York City. As a workaround, I omit the rest of New York from the analysis and treat the admission rate in the whole state as a proxy for that in New York City. In the final analysis data, there are 44 "states" (including New York City) and 8 years where 7 is pre- and 1 is post-treatment. TEDS classifies a patient's substance use problem as alcohol only, other drugs only, alcohol and other drugs, or none at the time of admission to treatment. I focus on "drugs-only" admissions as the linkage between this group and OPCs is more direct such that the individuals in this group are more likely to use OPCs compared to people with alcohol use disorder.

Table 5 presents the results. The first 3 columns show the effect of OPCs on the number of total admissions without any controls, with optimized and projected controls, respectively. Columns (4)-(6) repeat the same analysis for the total number of people using the method suggested by Batts et al. (2016). Due to multiple admissions in a year, the total number of admissions may not capture how many individuals are actually admitted to a treatment facility. The number may be exaggerated if the majority of people are admitted more than once. As a solution, Batts et al. (2016) estimate the probability of being in treatment on a given date using a direct count from linked admission/discharge records and find two-thirds (66%) of total admissions are unique. Every specification returns a statistically significant positive effect, meaning OPCs led to an increase in admissions to substance use disorder treatment. How can one interpret this increase? It is possible that OPCs provide resources to current drug users and motivate them to seek treatment, acting as an entry point to drug rehabilitation. One can also argue that OPCs may increase substance use prevalence and the positive effect may be due to a result of more people using drugs. To address the latter argument, I check the percentages of persons with SUD in New York State in 2021 (the last pre-treatment year) and 2022 (post-treatment year). According to state estimates

by SAMHSA, SUD prevalence in 2021 was 16.99%. In 2022, this number was down to 14.23% based on the data by the Office of Addiction Services and Supports.[29] The declining prevalence supports that OPCs indeed did not increase the number of drug users but instead led more already-existing users into SUD treatment. Based on the preferred specification in Column (6), OPCs increased the number of people going into substance abuse treatment by 64 per 100,000 population. Given a control mean of 335 per 100,000 population, this value represents a 19% increase. The effect is statistically significant at the 10% significance level.

Table 5: SDID Estimates - Effect of OPCs on Entry to Substance Use Disorder Treatment

	(1)	(2)	(3)	(4)	(5)	(6)
	Total # of Admissions			Total # of People		
Substance when admitted						
Drug-only	106.632**	128.540**	97.346*	70.377**	84.836**	64.248*
	(51.536)	(49.917)	(57.057)	(34.014)	(32.946)	(37.658)
Covariates	No	Yes	Yes	No	Yes	Yes
Covariate adjustment	-	Optimized	Projected	-	Optimized	Projected
N	352	352	352	352	352	352

Notes: The estimates in all columns are based on Equation 1. The dependent variable is the number of total admissions in columns (1) - (3) whereas it is the total number of people admitted in columns (4) - (6). The estimates presented are for the main treatment variable *NYC X Post* taking the value of 1 for observations in New York City in and after November 2021. The full specification in Column (3) controls for cigarette and beer taxes, marijuana laws, per capita income, and the unemployment rate. Inference is based on a placebo procedure. The standard errors are clustered by state, and follow Algorithm 4 laid out in Arkhangelsky et al. (2021).
Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6 Conclusion

The U.S. has witnessed a devastating toll of over one million lives lost to drug overdoses in the last 25 years. The COVID-19 crisis in 2020 exacerbated the situation, creating challenges for those grappling with addiction to access treatment and support. A recent approach to tackle

[29]Jette G, Heck A, Burke C, Cunningham, CO. New York State Substance Use Disorder Treatment Service System. New York State Office of Addiction Services and Supports: Addiction Data Bulletin (No. 2023-01); September 2023

drug abuse involves Overdose Prevention Centers (OPCs), safe spaces where individuals can consume pre-obtained drugs under supervised conditions. While federal laws prohibit such centers, two recently opened in New York City in November 2021.

I estimate the effect of the two OPCs in New York City on overdose mortality using a synthetic difference-in-differences identification strategy. I find that OPCs increased the overdose mortality in New York City by 0.3 per 100,000 population or by 14 percent. The effect is statistically insignificant in each of my proposed model specifications with varying sets of covariates, covariate adjustments, and sample choice. The positive but insignificant results are robust to different identification strategies including the standard difference-in-differences and the synthetic control methods. I present event study style estimates to further probe the causal relationship between OPCs and increased overdose mortality rate. The event study confirms the uptick in drug overdose mortality after the opening of OPCs —with several statistically significant individual estimates and no specific pre-trend— and also reveals that the impact seems to increase over time. Finally, I investigate the effect of OPCs on entry to substance use disorder treatment. I find that OPCs increased the number of people going into substance abuse treatment by 64 per 100,000 population or by 19 percent. The effect is statistically significant and driven by the higher rate of seeking rehabilitation rather than the higher rate of drug prevalence.

My design and results come with some caveats and suggest directions for future research. First, the post-treatment period is rather short, making it more difficult to capture the effects that may present themselves if more post-treatment data were available. This difficulty becomes apparent in power calculations where the experiment only has a moderate minimum detectable effect size. Second, due to a lack of granular spatial information, I could not control for some additional variables that may influence overdose mortality. The fact that the addition of controls does not drastically alter the results mitigates the concerns regarding the set of covariates. Third, due to data availability, I could not investigate the causal

relationship between OPCs and other related health outcomes such as HIV and Hepatitis C (HCV) infections. Both HIV and HCV are known risk factors for drug users due to unsterile injection practices and unsafe sexual behaviors. According to the World Health Organization (WHO), drug use is a factor in approximately 10% of new HIV infections and an estimated 23–39% of new HCV infections globally (UNAIDS, 2020). The CDC has not published the data regarding HIV and HCV diagnoses in 2022 yet which leaves an opportunity for future research.

The study is unique as it is the first quasi-experimental study to investigate overdose prevention centers and their effect on overdose mortality and entry to drug use disorder treatment based on the first two publicly recognized U.S. OPCs. Previous studies, often descriptive and focused on other countries, indicate positive outcomes for OPCs, but the U.S. context is different and calls for a specific evaluation. The soaring number of overdose deaths in the country underscores the urgent need for comprehensive approaches to tackle drug-related fatalities and addiction. As the debate over OPCs in the U.S. is hindered by legal and moral concerns, there is a pressing need for evidence-based policymaking in addressing drug abuse and its consequences. The findings not only affect the person who uses drugs but also their families and the community. This study aims to contribute to the conversation regarding harm reduction policies. While the effect on overdose mortality is only suggestive, the strong association between OPCs and entry into drug addiction treatment reveals one of the potential mechanisms OPCs may lead to drug use cessation. These findings are important for our understanding of the function of overdose prevention centers when fighting drug abuse and how policymakers could design better and more effective policies around drug use and addiction to achieve the main goal of preventing any death due to a drug overdose.

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Appendix

Additional Tables

Table A1: Unit-Specific and Time-Specific Weights - Baseline SDID Specification

Unit weights		**Time weights**			
Alabama	0.011	Jan-15	0.031	Apr-19	0
Alaska	0.034	Feb-15	0	May-19	0
Arizona	0.032	Mar-15	0	Jun-19	0
Arkansas	0.010	Apr-15	0	Jul-19	0
California	0.019	May-15	0	Aug-19	0
Colorado	0.019	Jun-15	0	Sep-19	0
Connecticut	0.020	Jul-15	0	Oct-19	0
Delaware	0.008	Aug-15	0	Nov-19	0
District of Columbia	0.014	Sep-15	0	Dec-19	0
Florida	0.028	Oct-15	0	Jan-20	0
Georgia	0.013	Nov-15	0.090	Feb-20	0
Hawaii	0.021	Dec-15	0	Mar-20	0
Idaho	0.012	Jan-16	0	Apr-20	0
Illinois	0.018	Feb-16	0	May-20	0
Indiana	0.024	Mar-16	0	Jun-20	0
Iowa	0.012	Apr-16	0	Jul-20	0
Kansas	0.018	May-16	0.073	Aug-20	0
Kentucky	0.023	Jun-16	0	Sep-20	0
Louisiana	0.030	Jul-16	0	Oct-20	0
Maine	0.034	Aug-16	0	Nov-20	0
Maryland	0.030	Sep-16	0	Dec-20	0
Massachusetts	0.022	Oct-16	0	Jan-21	0.018
Michigan	0.014	Nov-16	0	Feb-21	0
Minnesota	0.018	Dec-16	0	Mar-21	0.058
Mississippi	0.020	Jan-17	0	Apr-21	0
Missouri	0.020	Feb-17	0	May-21	0.113
Montana	0.005	Mar-17	0	Jun-21	0.180
Nebraska	0.007	Apr-17	0	Jul-21	0
Nevada	0.017	May-17	0	Aug-21	0.084
New Hampshire	0.011	Jun-17	0	Sep-21	0.143
New Jersey	0.022	Jul-17	0	Oct-21	0.209
New Mexico	0.031	Aug-17	0	Nov-21	.
New York	0.015	Sep-17	0	Dec-21	.
North Carolina	0.023	Oct-17	0	Jan-22	.
North Dakota	0.023	Nov-17	0	Feb-22	.
Ohio	0.021	Dec-17	0	Mar-22	.
Oklahoma	0.011	Jan-18	0	Apr-22	.
Oregon	0.019	Feb-18	0	May-22	.
Pennsylvania	0.016	Mar-18	0	Jun-22	.
Rhode Island	0.010	Apr-18	0	Jul-22	.
South Carolina	0.032	May-18	0	Aug-22	.
South Dakota	0.008	Jun-18	0	Sep-22	.
Tennessee	0.034	Jul-18	0	Oct-22	.
Texas	0.016	Aug-18	0	Nov-22	.
Utah	0.009	Sep-18	0	Dec-22	.
Vermont	0.027	Oct-18	0	Jan-23	.
Virginia	0.023	Nov-18	0	Feb-23	.
Washington	0.018	Dec-18	0	Mar-23	.
West Virginia	0.035	Jan-19	0	Apr-23	.
Wisconsin	0.022	Feb-19	0	May-23	.
Wyoming	0.020	Mar-19	0	Jun-23	.

Table A2: Impact of OPCs on Overdose Mortality – Event Study Estimates

Time to/from Treatment	*NYC X Post*	Time to/from Treatment	*NYC X Post*
-46	0.402	-10	0.108
-45	0.118	-9	0.039
-44	0.126	-8	-0.255
-43	0.253	-7	-0.241
-42	0.053	-6	-0.332
-41	0.237	-5	0.009
-40	0.146	-4	-0.110
-39	0.295	-3	-0.221
-38	-0.007	-2	0.025
-37	0.240	-1	0.213
-36	0.200	0	0.064
-35	0.202	1	0.384
-34	0.178	2	0.408
-33	0.222	3	-0.117
-32	0.109	4	0.111
-31	0.200	5	0.144
-30	0.178	6	0.419
-29	0.120	7	0.116
-28	0.318	8	0.486
-27	0.116	9	0.241
-26	0.141	10	0.399
-25	0.228	11	0.328
-24	0.081	12	0.666
-23	0.283	13	0.390
-22	-0.033	14	0.302
-21	0.222	15	0.249
-14	-0.114	16	0.341
-13	0.123	17	0.521
-12	0.016	18	0.294
-11	-0.027	19	0.807

Table A3: SDID Estimates, Event-Study Specification - Effect of OPCs on Overdose Mortality

	(1)	(2)	(3)	(4)	(5)
NYC X Post	0.314	0.373	0.228	0.320	0.233
	(0.299)	(0.326)	(0.298)	(0.332)	(0.329)
Covariates	No	Yes	Yes	No	Yes
Covariate adjustment	-	Optimized	Projected	-	Projected
Rest of NY omitted	No	No	No	Yes	Yes
N	3,120	3,120	3,120	3,066	3,066

Notes: The variable *NYC X Post* is the main treatment variable taking the value of 1 for observations in New York City in and after November 2021.

All models follow the specification used in the event-study analysis such that the months between March 2020 - August 2020 and the years before 2018 are dropped and only a subset of the covariates that vary monthly or quarterly (instead of yearly variables) are included. As in the main specification, the covariates used are cigarette and beer excise taxes, a binary indicator for whether marijuana is legalized or not, per capita income, and the unemployment rate.

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$