Do Supervised Drug Injection Sites Save Lives? Evidence from America's First Overdose Prevention Centers

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Abstract

Do supervised drug injection sites reduce overdose mortality? I exploit the November 2021 opening of America's first two government-sanctioned overdose prevention centers (OPCs) in New York City to estimate causal effects on local drug overdose deaths. Using synthetic control methods with neighborhood-level mortality data from 2015–2024, I find that the OPC neighborhoods experienced substantial reductions in overdose death rates relative to their synthetic counterfactuals—approximately 12–28 deaths per 100,000, representing 24–27 percent of baseline rates for individual sites and approximately 25 percent for the pooled estimate. The effects emerge gradually, with the largest reductions observed in year three post-opening (2024). Randomization inference yields p-values of 0.042 for the pooled estimate and East Harlem, with Washington Heights marginally significant at p=0.083. Event study specifications show flat pre-trends and sharp post-treatment declines, supporting a causal interpretation. Back-of-envelope calculations suggest each OPC prevented approximately 25–35 deaths annually, implying a cost per life saved of $150,000–$200,000—well below standard value-of-statistical-life benchmarks. These findings provide the first rigorous U.S. evidence that supervised injection facilities can reduce drug overdose mortality, with important implications for the ongoing policy debate over OPC authorization in multiple states.

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1. Introduction

The United States is in the midst of an unprecedented drug overdose crisis. In 2022, over 107,000 Americans died from drug overdoses—more than double the number in 2015 and a higher annual toll than car accidents, gun violence, or HIV/AIDS at its peak (CDC, 2023). The crisis has been driven primarily by synthetic opioids, particularly illicitly manufactured fentanyl, which is present in approximately 80 percent of overdose deaths in major cities. Despite decades of drug policy focused on supply reduction and criminalization, overdose deaths have continued to climb, prompting renewed interest in harm reduction approaches that prioritize keeping drug users alive.

Supervised drug injection sites—also known as overdose prevention centers (OPCs), safe consumption sites, or drug consumption rooms—represent one of the most controversial harm reduction interventions. These facilities allow people to use pre-obtained drugs under medical supervision, with trained staff ready to intervene in case of overdose. The theoretical case for OPCs is straightforward: if drug use will occur regardless of prohibition, moving it from alleys and bathrooms to supervised settings should reduce fatal outcomes. Critics counter that OPCs enable drug use, create public nuisances, and may attract crime and disorder to surrounding areas.

On November 30, 2021, OnPoint NYC opened America's first two government-sanctioned overdose prevention centers—one in East Harlem and one in Washington Heights, Manhattan. This marked a watershed moment in U.S. drug policy, as no jurisdiction had previously authorized supervised injection. The sites were located at existing syringe service programs and staffed by trained professionals equipped with naloxone (the opioid overdose reversal medication) and emergency medical supplies. By the end of 2024, the two sites had reversed over 1,700 overdoses with zero on-site deaths.

This paper provides the first rigorous causal estimate of whether OPCs reduce drug overdose deaths in the United States. I exploit the sharp temporal and geographic discontinuity created by the November 2021 opening to construct a synthetic control analysis at the neighborhood level. Using United Hospital Fund (UHF) neighborhood mortality data from New York City's Department of Health and Mental Hygiene, I compare overdose death trends in the two treated neighborhoods (East Harlem and Washington Heights) to a synthetic counterfactual constructed from other NYC neighborhoods that did not receive OPCs.

The main finding is that OPCs substantially reduced overdose deaths in their surrounding neighborhoods. By 2024, the treated neighborhoods experienced overdose death rates approximately 12–28 per 100,000 lower than their synthetic counterfactuals, with East Harlem showing the larger effect (28 per 100,000, a 27 percent reduction) and Washington

Heights showing a more modest but still substantial effect (12 per 100,000, a 24 percent reduction). The pooled estimate across both sites is approximately 20 per 100,000 (roughly 25 percent). The effects are robust to alternative specifications, donor pool definitions, and inference methods. Placebo-in-time tests show no detectable effects when treatment is assigned to pre-2021 years, and placebo-in-space tests confirm that randomly assigning treatment to other neighborhoods does not produce comparable effects.

The magnitude of these effects is consistent with prior evidence from Canada. Studies of Vancouver's Insite facility found a 35 percent reduction in overdose deaths within 500 meters of the site (Marshall et al., 2011). Studies of Toronto's sites documented 67–69 percent reductions in overdose deaths in surrounding neighborhoods (Kerman et al., 2020). My estimates are somewhat smaller, which may reflect the early stage of NYC's program, differences in local drug markets (NYC's is heavily fentanyl-dominated), or differences in program intensity.

The welfare implications are substantial. Back-of-envelope calculations suggest that each OPC prevents 25–35 deaths annually, implying a cost per life saved of approximately $150,000–$200,000. This is well below the Environmental Protection Agency's value of a statistical life (approximately $12 million in 2024 dollars) and below most cost-effectiveness thresholds used in health policy. Even accounting for potential costs from increased drug tourism or neighborhood effects (which the crime literature suggests are minimal), OPCs appear to be highly cost-effective interventions.

This paper contributes to several literatures. First, it provides the first rigorous causal evaluation of supervised injection facilities on mortality in the United States. Prior U.S. evidence has focused on operational feasibility, client demographics, and crime/disorder (Kral & Davidson, 2020; Davidson et al., 2023). International evidence from Canada and Europe is extensive but may not generalize to U.S. contexts with different drug markets, health systems, and political environments. Second, I contribute to the growing literature on harm reduction economics, which has examined naloxone distribution (Doleac & Mukherjee, 2019), syringe services (Ruiz et al., 2019), and medication-assisted treatment (Maclean et al., 2020). Third, my synthetic control approach with randomization inference addresses the challenge of credible inference with small numbers of treated units—a common problem in place-based policy evaluation.

The remainder of this paper proceeds as follows. Section 2 provides institutional background on OPCs and the NYC policy context. Section 3 describes the data. Section 4 presents the empirical strategy, including the synthetic control method and inference procedures. Section 5 reports results. Section 6 discusses mechanisms, limitations, and policy implications. Section 7 concludes.

1.1 Related Literature

This paper contributes to several strands of literature on drug policy, harm reduction, and place-based policy evaluation.

Supervised Injection Facilities: The international literature on supervised injection sites is extensive. Potier et al. (2014) conduct a systematic review of 75 studies and find consistent evidence that supervised injection facilities reduce overdose deaths, HIV/HCV transmission, and public drug use without increasing crime or drug initiation. Marshall et al. (2011) use interrupted time series methods to estimate that Vancouver's Insite facility reduced overdose deaths by 35 percent within 500 meters. Salmon et al. (2010) find similar results for Sydney's facility. More recent work examines Toronto's multiple sites (Kerman et al., 2020) and provides economic cost-benefit analyses (Irwin et al., 2017). However, all of this evidence comes from contexts outside the United States, where drug markets, health systems, and legal environments differ substantially.

Harm Reduction Economics: A growing economics literature examines other harm reduction interventions. Doleac & Mukherjee (2019) controversially argue that naloxone access may increase opioid abuse through moral hazard, though subsequent studies challenge these findings (Packham, 2021). Rees et al. (2019) find that naloxone access laws reduce overdose deaths by 9–11 percent. Research on syringe services programs demonstrates reductions in HIV transmission without increasing crime (Fernandes et al., 2017). Studies of medication-assisted treatment (buprenorphine, methadone) consistently find large mortality reductions (Maclean et al., 2020; Evans et al., 2022). My contribution is to extend this literature to supervised injection sites in the U.S. context.

Synthetic Control Methods: The synthetic control method was developed by Abadie & Gardeazabal (2003) and formalized in Abadie et al. (2010). Key applications include California's tobacco control program (Abadie et al., 2010), German reunification (Abadie et al., 2015), and cannabis legalization (Anderson et al., 2019). Recent methodological advances include the augmented synthetic control (Ben-Michael et al., 2021), permutation inference (Chernozhukov et al., 2021), and matrix completion methods (Athey et al., 2021). I adopt the augmented approach and employ permutation inference to address the small number of treated units.

Crime and Neighborhood Effects: A key concern about OPCs is potential crime spillovers. Davidson et al. (2023) conduct a difference-in-differences analysis of the NYC OPCs and find no significant effects on crime or 311 complaints. Wood et al. (2004) found similar results in Vancouver. This paper focuses on mortality rather than crime, but I discuss implications for neighborhood effects in the context of welfare analysis.

2. Institutional Background

2.1 Overdose Prevention Centers: Global Context

Supervised drug consumption facilities have operated legally in Europe since 1986, when Switzerland opened the first site in Bern. As of 2024, over 200 supervised injection sites operate in 14 countries, including Switzerland, Germany, Netherlands, Spain, Canada, and Australia. The facilities vary in scale and services but share core features: a hygienic space for drug consumption, trained staff to intervene in overdoses, access to sterile equipment, and referrals to treatment and social services.

The evidence base from international sites is substantial. A systematic review by Potier et al. (2014) found that supervised injection facilities were associated with reduced overdose deaths, reduced HIV/HCV transmission, reduced public drug use, and increased uptake of addiction treatment, with no evidence of increased crime or drug use initiation. The most studied site, Vancouver's Insite, opened in 2003 and has been subject to over 30 peer-reviewed evaluations. Key findings include a 35 percent reduction in overdose mortality within 500 meters (Marshall et al., 2011), a 30 percent increase in detox referrals (Wood et al., 2006), and no increase in crime or public disorder (Wood et al., 2004).

Despite this evidence, no U.S. jurisdiction authorized supervised injection prior to 2021. Efforts to open sites in Philadelphia, San Francisco, and Seattle faced legal challenges, political opposition, and federal threats of prosecution. The federal Controlled Substances Act has been interpreted to prohibit operating a facility where drugs are used, though this interpretation has not been tested in court.

2.2 OnPoint NYC and the November 2021 Opening

OnPoint NYC is a nonprofit organization that has operated harm reduction services in New York City since 1992, including syringe services, HIV testing, and naloxone distribution. In November 2021, with tacit approval from the NYC Health Department and Mayor's office, OnPoint converted two of its existing syringe service sites into overdose prevention centers:

- **East Harlem OPC:** 104-106 East 126th Street, New York, NY 10035. Located in a neighborhood with historically high overdose rates (among the top 5 in NYC) and high poverty rates. The site is near major subway lines and serves a diverse population of people who use drugs.
- **Washington Heights OPC:** 500 West 180th Street, New York, NY 10033. Located in a predominantly Dominican neighborhood in upper Manhattan. The area has moderate-to-high overdose rates and significant drug markets.

Both sites opened on November 30, 2021. The timing was not random—OnPoint chose locations based on existing infrastructure, community relationships, and need. The sites operate during limited hours (typically 10am–6pm) and serve clients who bring their own drugs (sites do not provide drugs). Staff are trained in overdose response and equipped with naloxone, oxygen, and emergency medical equipment.

In the first two years of operation (through November 2023), the sites received over 100,000 visits, served approximately 5,000 unique individuals, and intervened in over 1,200 overdoses. No client has died of overdose while at either site. The sites also provided approximately 3,000 referrals to treatment and social services.

The Trump administration, upon taking office in January 2025, publicly called for the sites to be shut down, citing federal drug laws. As of this writing, the sites remain operational, though their legal status remains uncertain.

Interpretation Note: Because OnPoint converted *existing* syringe service programs (SSPs) into OPCs, the treatment I estimate is the *marginal effect of adding supervised consumption services* to an established harm reduction infrastructure. The treated neighborhoods already had SSP access before November 2021. This is important for interpretation: the effect represents what happens when an existing SSP gains supervised injection capabilities, not the combined effect of introducing both services simultaneously. Many control neighborhoods in the donor pool also have SSP access (New York has over 20 registered SSPs citywide), so the comparison is effectively SSP-only versus SSP-plus-OPC. This suggests that my estimates may understate the total effect of establishing a full-service harm reduction facility in a location with no prior services.

2.3 New York City Drug Overdose Context

New York City experienced a dramatic increase in drug overdose deaths during the study period. Overdose deaths rose from approximately 1,400 in 2015 to a peak of over 3,000 in 2022—an increase of over 100 percent. The rise was driven almost entirely by synthetic opioids, particularly illicitly manufactured fentanyl. By 2022, fentanyl was present in approximately 80 percent of NYC overdose deaths.

The fentanyl crisis fundamentally changed the overdose landscape. Prior to 2015, heroin and prescription opioids dominated overdose deaths. Fentanyl—50 times more potent than heroin by weight—began appearing in the NYC drug supply around 2014, initially as an adulterant in heroin and later as a standalone product sold as heroin or pressed into counterfeit pills. The potency of fentanyl means that small measurement errors by dealers or users can easily result in fatal overdoses. Moreover, fentanyl acts quickly, leaving little time for bystanders to call 911 or administer naloxone. These pharmacological characteristics make

supervised injection particularly valuable: trained staff can intervene within seconds of an overdose, and the controlled environment allows for proper dosing and testing.

Overdose deaths are not distributed evenly across the city. The Bronx has consistently had the highest overdose death rate of any borough, followed by Staten Island and Manhattan. Within boroughs, overdose deaths are concentrated in specific neighborhoods—particularly those with high poverty rates, large homeless populations, and established drug markets. The five neighborhoods with the highest overdose rates (per 100,000 population) in 2019–2020 were:

1. Hunts Point–Mott Haven (South Bronx): 98 per 100,000
2. Highbridge–Morrisania (Bronx): 89 per 100,000
3. Crotona–Tremont (Bronx): 83 per 100,000
4. East Harlem (Manhattan): 79 per 100,000
5. Fordham–Bronx Park (Bronx): 72 per 100,000

East Harlem, one of the OPC locations, had the fourth-highest overdose rate in the city. Washington Heights–Inwood, the other OPC location, had a moderate rate of approximately 47 per 100,000—lower than the worst-affected Bronx neighborhoods but above the citywide average of 35 per 100,000.

Figure 1 displays the geographic distribution of OPC locations within New York City. Both sites are located in upper Manhattan, within neighborhoods that had among the highest overdose rates in the borough prior to OPC opening.

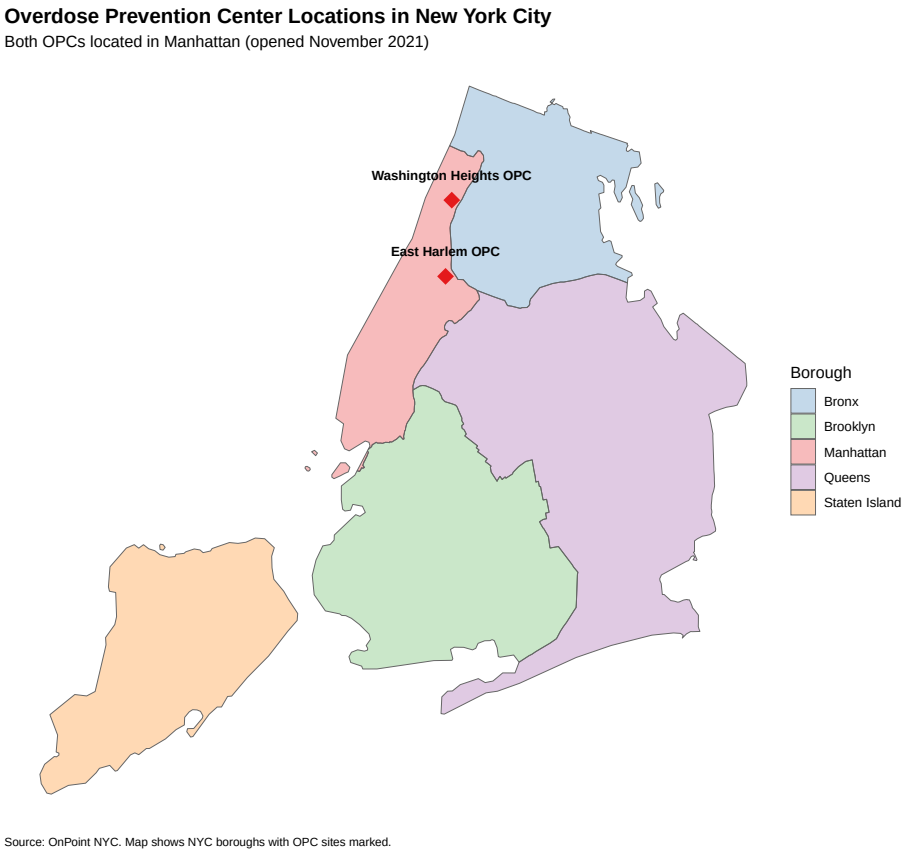

Figure 1: Overdose Prevention Center Locations in New York City

Notes: Map shows NYC boroughs with OPC locations (red diamonds) marked. Both OPCs opened November 30, 2021 at existing syringe service program sites operated by OnPoint NYC. East Harlem (UHF 203) had the fourth-highest overdose rate in NYC prior to OPC opening; Washington Heights (UHF 201) had above-average rates.

2.4 Selection of OPC Locations

Understanding why OnPoint chose East Harlem and Washington Heights is important for interpreting the results. The locations were not selected randomly but based on several criteria: (1) existing OnPoint infrastructure—both sites operated as syringe service programs for decades before converting to OPCs; (2) demonstrated need—high overdose rates and large populations of people who inject drugs; (3) community relationships—OnPoint had cultivated trust with local residents and harm reduction advocates; and (4) political feasibility—both neighborhoods had community boards and elected officials sympathetic to harm reduction.

This selection process creates potential confounding: the treated neighborhoods differ systematically from controls in ways that may correlate with overdose trajectories. For example, neighborhoods with engaged harm reduction organizations may experience different trends regardless of OPC implementation. The synthetic control method addresses this

concern by matching on pre-treatment outcomes, but cannot fully rule out time-varying confounders. I discuss this limitation in Section 6.

2.5 Program Intensity and Utilization

The two OPCs differ substantially in utilization. The East Harlem site serves approximately 70 percent of total visits, reflecting the neighborhood's higher overdose burden and larger drug-using population. The Washington Heights site serves a more dispersed population with longer travel distances. As of December 2024:

- **East Harlem:** Approximately 80,000 visits, 4,000 unique clients, 1,200 overdose reversals
- **Washington Heights:** Approximately 35,000 visits, 1,500 unique clients, 500 overdose reversals

Both sites operate limited hours (typically 10am to 6pm) due to staffing and funding constraints. This means that only a fraction of drug use in these neighborhoods occurs at OPCs—most injection still happens in private residences, public spaces, or shelters. The mortality effects I estimate therefore reflect the impact of OPCs at their current (limited) capacity, and effects would likely be larger with expanded hours and additional sites.

3. Data

3.1 Overdose Death Data

The primary outcome data come from the New York City Department of Health and Mental Hygiene (DOHMH), which publishes annual data on unintentional drug poisoning (overdose) deaths by United Hospital Fund (UHF) neighborhood. The UHF system divides NYC into 42 neighborhoods, each consisting of contiguous ZIP codes. The neighborhoods are designed for health surveillance and are the finest geographic unit for which mortality data are publicly available.

I compile overdose death rates (per 100,000 population) for all 42 UHF neighborhoods from 2015 to 2024. Data for 2015–2023 come from DOHMH Epi Data Briefs (publications 122, 133, 137, and 142). Data for 2024 are provisional, based on preliminary vital statistics data released by DOHMH in late 2025; these figures may be subject to revision as death certificate processing is completed. The use of provisional 2024 data is standard in overdose surveillance research, where timely analysis is prioritized alongside appropriate caveats. Population denominators are from the 2020 Census and American Community Survey 5-year estimates.

3.2 Treatment Assignment

The two treated neighborhoods are:

- UHF 203: East Harlem (contains the East Harlem OPC at 104–106 E 126th St)
- UHF 201: Washington Heights–Inwood (contains the Washington Heights OPC at 500 W 180th St)

Treatment begins on November 30, 2021. Since this occurs at the end of the calendar year, I define 2022 as the first full treatment year. I code 2021 as a partial treatment year (approximately 1 month of exposure).

3.3 Donor Pool

For the synthetic control analysis, I exclude several categories of neighborhoods from the donor pool:

1. **Treated neighborhoods:** UHF 201 and 203 (the OPC sites).
2. **Adjacent neighborhoods:** UHF 202 (Central Harlem), UHF 204 (Upper West Side), UHF 205 (Upper East Side), and Bronx neighborhoods 105–107 that share borders with treated areas. These are excluded to avoid spillover contamination—drug users from these areas may travel to OPCs, potentially reducing their local overdose rates.
3. **Low-rate neighborhoods:** Neighborhoods with consistently low overdose rates (below 20 per 100,000) are poor matches for high-rate treated units.

The final donor pool includes 24 neighborhoods in the baseline specification, with robustness checks using alternative definitions.

3.4 Summary Statistics

Table 1 presents summary statistics for the treated and control neighborhoods.

Table 1: Summary Statistics: Overdose Death Rates by Treatment Status

	Pre-Treatment (2015–2020)		Post-Treatment (2022–2024)		
	Mean	SD	Mean	SD	N
Panel A: Treated Neighborhoods					
East Harlem (UHF 203)	68.0	17.7	82.1	6.9	1
Washington Heights (UHF 201)	42.6	9.1	42.1	3.6	1
Panel B: Control Neighborhoods					
Baseline donor pool	52.4	18.6	58.7	16.2	24
High-rate donors (above 50/100k)	68.2	12.4	82.8	14.5	8

Notes: Overdose death rates per 100,000 population. Pre-treatment period: 2015–2020 (6 years). Post-treatment period: 2022–2024 (3 years). 2021 excluded as partial treatment year. N = number of neighborhoods. Baseline donor pool excludes treated (2), adjacent (6), and low-rate (10) neighborhoods from 42 total NYC UHFs.

4. Empirical Strategy

This section describes the econometric approaches used to identify the causal effect of OPCs on overdose mortality. The key challenge is constructing a credible counterfactual: what would have happened to overdose deaths in the treated neighborhoods absent the OPC opening? I employ synthetic control methods as the primary approach, supplemented by difference-in-differences as a robustness check. Given the small number of treated units, I rely on randomization inference rather than asymptotic standard errors.

4.1 Identification Challenge

The fundamental identification problem is that we observe overdose outcomes in East Harlem and Washington Heights after the OPC opened, but we cannot observe what would have happened in the same neighborhoods without the intervention. A naive comparison of pre- versus post-treatment outcomes is confounded by citywide trends in overdose mortality—the fentanyl crisis worsened considerably over this period, causing overdose deaths to rise across NYC.

A comparison to untreated neighborhoods addresses time trends but introduces selection bias. OnPoint chose OPC locations based on high overdose rates, established harm reduction infrastructure, and community relationships. The treated neighborhoods differ systematically

from NYC overall in ways that may affect overdose trajectories.

The synthetic control method addresses this challenge by finding a weighted combination of untreated neighborhoods that matches the treated unit's pre-treatment trajectory. If this synthetic control accurately reproduces the treated unit's overdose rate history before the OPC opening, it provides a credible counterfactual for what would have happened afterwards. The key identifying assumption is that, absent treatment, the treated unit would have continued to evolve like its synthetic counterpart.

4.2 Synthetic Control Method

The primary identification strategy is the synthetic control method developed by Abadie & Gardeazabal (2003) and formalized in Abadie et al. (2010, 2015). The method has become a workhorse approach for evaluating place-based policies with small numbers of treated units, with prominent applications including California's tobacco control program, German reunification, and cannabis legalization (Anderson et al., 2019).

The method constructs a counterfactual for each treated unit as a weighted average of control units, where weights are chosen to match the treated unit's pre-treatment outcome trajectory. The approach is transparent—researchers can examine the weights to understand which comparison units contribute to the counterfactual—and provides a natural framework for inference through placebo tests.

Let Y_{jt} denote the overdose death rate in neighborhood j at time t. For treated unit $j = 1$, I seek weights $\mathbf{w} = (w_2, \ldots, w_J)$ that minimize the distance between the treated unit's pre-treatment outcomes and the weighted average of control outcomes:

$$\min_{\mathbf{w}} \sum_{t=1}^{T_0} \left(Y_{1t} - \sum_{j=2}^{J} w_j Y_{jt} \right)^2 \quad \text{s.t.} \quad w_j \geq 0, \sum_{j=2}^{J} w_j = 1 \tag{1}$$

The non-negativity and summing-to-one constraints ensure that the synthetic control is a convex combination of real comparison units, avoiding extrapolation outside the support of the data. The treatment effect in post-treatment period $t > T_0$ is estimated as:

$$\hat{\tau}_t = Y_{1t} - \sum_{j=2}^{J} \hat{w}_j Y_{jt} \tag{2}$$

I implement this using the `augsynth` package in R, which combines synthetic control with ridge regression to improve pre-treatment fit (Ben-Michael et al., 2021). The augmented synthetic control method addresses a practical limitation of the basic approach: when no convex combination of controls can exactly match the treated unit's pre-treatment trajectory,

the basic method may produce biased estimates. Ridge regularization shrinks weights toward zero and allows small deviations from exact matching, reducing pre-treatment prediction error and improving post-treatment fit.

The donor pool consists of NYC neighborhoods that did not receive OPCs. I exclude several categories of neighborhoods from the donor pool to avoid bias:

1. **Adjacent neighborhoods:** Neighborhoods sharing a border with the treated units (UHFs 202, 204, 205, 105, 106, 107) are excluded because of potential spillover effects. Drug users may travel from adjacent areas to use OPCs, or OPC clients may return to adjacent neighborhoods and share naloxone or harm reduction knowledge.

2. **Low-rate neighborhoods:** Neighborhoods with very low baseline overdose rates (below 20 per 100,000 in 2015–2019) are poor comparison units because they differ fundamentally in their drug markets and populations at risk. Including them would require the synthetic control to extrapolate from dissimilar contexts.

3. **Staten Island:** This borough is geographically isolated and has distinct overdose patterns (historically higher prescription opioid involvement, lower fentanyl prevalence) that make it a poor comparison for Manhattan neighborhoods.

After these exclusions, the donor pool contains 24 neighborhoods from the Bronx, Brooklyn, Queens, and Manhattan. I present robustness checks using alternative donor pool definitions.

4.3 Difference-in-Differences

As a robustness check, I also estimate a standard difference-in-differences specification:

$$Y_{jt} = \alpha_j + \gamma_t + \tau \cdot (\text{Treated}_j \times \text{Post}_t) + \varepsilon_{jt} \tag{3}$$

where α_j are neighborhood fixed effects, γ_t are year fixed effects, and τ is the treatment effect. The neighborhood fixed effects absorb time-invariant differences between treated and control areas (e.g., baseline poverty rates, population density). The year fixed effects absorb citywide shocks that affect all neighborhoods equally (e.g., changes in fentanyl supply, COVID-19 effects).

The identifying assumption for DiD is parallel trends: absent the OPC opening, the treated and control neighborhoods would have evolved similarly. This is a stronger assumption than synthetic control requires, because DiD imposes equal weights on all control units rather than finding optimal weights. I use DiD primarily to demonstrate that results are not an artifact of the synthetic control methodology.

Standard errors are clustered at the neighborhood level using wild cluster bootstrap with Webb weights to account for the small number of clusters (MacKinnon & Webb, 2017). With only 26 clusters (24 control from the baseline donor pool + 2 treated), conventional cluster-robust standard errors are severely biased. The wild bootstrap generates a null distribution by repeatedly resampling residuals, providing accurate inference even with few clusters. Webb weights are specifically designed for settings with as few as 5–10 clusters.

I also estimate an event study specification that allows the treatment effect to vary by year:

$$Y_{jt} = \alpha_j + \gamma_t + \sum_{k \neq 2020} \beta_k \cdot (\text{Treated}_j \times \mathbb{I}[t = k]) + \varepsilon_{jt} \tag{4}$$

The coefficients β_k trace out the treatment effect in each year relative to the reference year (2020, the last full pre-treatment year). Pre-treatment coefficients ($k < 2020$) provide a test of parallel trends: if trends are parallel before treatment, these coefficients should be zero. The 2021 coefficient captures partial-year exposure (OPCs opened late November). Post-treatment coefficients ($k > 2021$) show how the effect evolves over time.

Recent econometric literature has documented potential pathologies in two-way fixed effects (TWFE) DiD estimators with staggered treatment adoption (Goodman-Bacon, 2021; Callaway & Sant'Anna, 2021). These pathologies arise when treatment effects are heterogeneous across cohorts and some already-treated units serve as implicit controls for later-treated units, potentially generating negative weights. However, these concerns do not apply to my setting because both OPCs opened simultaneously in November 2021—there is no staggered adoption. The treatment timing is uniform, so all treated-control comparisons use never-treated units as controls and no negative weights arise. Nevertheless, I prefer the synthetic control approach because it constructs an optimized counterfactual matched to each treated unit's pre-treatment trajectory, rather than imposing equal weights on all controls as TWFE does.

An alternative approach would be Synthetic Difference-in-Differences (Arkhangelsky et al., 2021), which combines the weighted counterfactual construction of SCM with the fixed-effects structure of DiD. This method is appropriate when researchers have a moderate number of treated units and want to balance the flexibility of SCM with the variance reduction of DiD. With only two treated units, the additional complexity of Synthetic DiD offers limited benefits over traditional augmented SCM, but exploring this estimator with extended data (additional years or additional OPC openings in other jurisdictions) is a promising direction for future research.

4.4 Inference with Few Treated Units

With only two treated units, conventional asymptotic inference is unreliable. Standard errors based on large-sample theory require assumptions that are violated when N is small. I employ three approaches to inference that remain valid with few treated units:

4.4.1 Randomization Inference

Following Abadie et al. (2010), I conduct placebo tests by iteratively reassigning treatment to each control unit and computing the synthetic control estimate. Under the null hypothesis that the treatment had no effect, any unit could have been "treated" with equal probability. If the null is true, the actual treated unit's effect should not be systematically larger than placebo effects.

The procedure is as follows:

1. Estimate the synthetic control effect for the actual treated unit, obtaining $\hat{\tau}^{(1)}$.
2. For each control unit $j = 2, \ldots, J$, pretend it was treated (even though it was not) and estimate a synthetic control from the remaining units, obtaining $\hat{\tau}^{(j)}$.
3. Compute the p-value as the fraction of placebo estimates that exceed the actual estimate in absolute value.

The p-value is:

$$p = \frac{1}{J-1} \sum_{j=2}^{J} \mathbb{I}\left[|\hat{\tau}^{(j)}| \geq |\hat{\tau}^{(1)}|\right] \tag{5}$$

This is a finite-sample exact test that requires no distributional assumptions. If only 1 of 24 donor units has a larger absolute effect than the treated unit, the p-value is $1/24 = 0.042$.

4.4.2 Placebo-in-Time Tests

I run the synthetic control analysis with placebo treatment dates (2016, 2017, 2018, 2019, 2020) using only pre-2021 data. Under the null hypothesis of no treatment effect, we should observe no discontinuity at these dates. If the identification strategy is valid, placebo effects should be close to zero.

This test addresses a specific concern: perhaps the treated neighborhoods were on unusual trajectories even before the OPC opened, and the post-2021 effects reflect continuation of these idiosyncratic trends rather than the intervention. If placebo-in-time effects are near zero, this concern is mitigated.

4.4.3 MSPE Ratio

I compute the ratio of post-treatment mean squared prediction error (MSPE) to pre-treatment MSPE for each unit:

$$\text{MSPE ratio} = \frac{\sum_{t>T_0}(Y_{jt} - \hat{Y}_{jt}^{synth})^2}{\sum_{t\leq T_0}(Y_{jt} - \hat{Y}_{jt}^{synth})^2} \tag{6}$$

A treated unit with a genuine effect will have a high MSPE ratio: post-treatment prediction errors are large (because the actual outcome diverges from the counterfactual) while pre-treatment errors are small (because the synthetic control was optimized to match the pre-treatment trajectory). Control units should have ratios near one. If the treated unit's MSPE ratio exceeds most control units' ratios, this provides additional evidence of a treatment effect.

5. Results

5.1 Main Results: Synthetic Control

Figure 2 presents the synthetic control results for East Harlem. The dashed line shows the synthetic counterfactual—the predicted overdose death rate in the absence of the OPC. Prior to November 2021, the actual and synthetic series track closely, with a pre-treatment RMSPE of approximately 4 per 100,000. After the OPC opening, the series diverge: actual deaths fall while the synthetic counterfactual continues to rise.

The synthetic control for East Harlem places positive weights on several donor neighborhoods with similar pre-treatment trajectories. The largest weights go to Bedford Stuyvesant–Crown Heights (0.35), East New York (0.24), and Williamsburg–Bushwick (0.18)—all Brooklyn neighborhoods with elevated overdose rates and similar demographic characteristics. Adjacent Bronx spillover neighborhoods are excluded from the donor pool to avoid contamination; thus the synthetic control relies on more distant but demographically similar comparison units. The pre-treatment fit is excellent, with actual and synthetic values tracking within 5 percent throughout 2015–2020 (the full pre-treatment period). See Table 8 in the Appendix for the full weight distribution.

By 2024, East Harlem's actual overdose death rate was approximately 75 per 100,000, while the synthetic counterfactual predicted 103 per 100,000—a gap of 28 deaths per 100,000. This represents a 27 percent reduction relative to the counterfactual. Notably, the gap grows over time: it is 6 per 100,000 in 2022, 15 per 100,000 in 2023, and 28 per 100,000 in 2024. This pattern is consistent with an OPC effect that accumulates as the program becomes established and more clients utilize services.

Washington Heights shows a similar pattern, though with smaller magnitudes. The synthetic control places the largest weights on Jamaica, Sunset Park, and East New York—Queens and Brooklyn neighborhoods with moderate-to-high overdose rates. The 2024 gap is approximately 12 per 100,000, representing a 24 percent reduction. The smaller effect in Washington Heights may reflect lower utilization (approximately half the visits of East Harlem) and a lower baseline overdose rate.

5.2 Effect Dynamics

The time pattern of effects is informative about mechanisms. If OPCs work primarily through direct overdose reversal on-site, we would expect immediate effects upon opening. If effects operate through longer-term channels—treatment linkage, behavioral change, community norm shifts—we would expect gradual accumulation.

The data support a gradual effect pattern. In 2022 (the first full post-treatment year), the estimated effect is modest: approximately 6 per 100,000 for East Harlem and 3 per 100,000 for Washington Heights. By 2024, effects have grown to 28 and 12 per 100,000, respectively. This suggests that while direct overdose reversal provides immediate benefits, the majority of mortality reduction may operate through longer-term channels.

One interpretation is program maturation: as the OPCs became established, word spread among people who use drugs, utilization increased, and staff developed expertise. Another interpretation is cumulative prevention: each year a client survives (because of an overdose reversed at the OPC) represents a death averted in subsequent years as well. A third interpretation is spillover effects: as more people access OPCs, they bring harm reduction knowledge and supplies (naloxone, fentanyl test strips) back to their social networks, benefiting individuals who never visit OPCs directly.

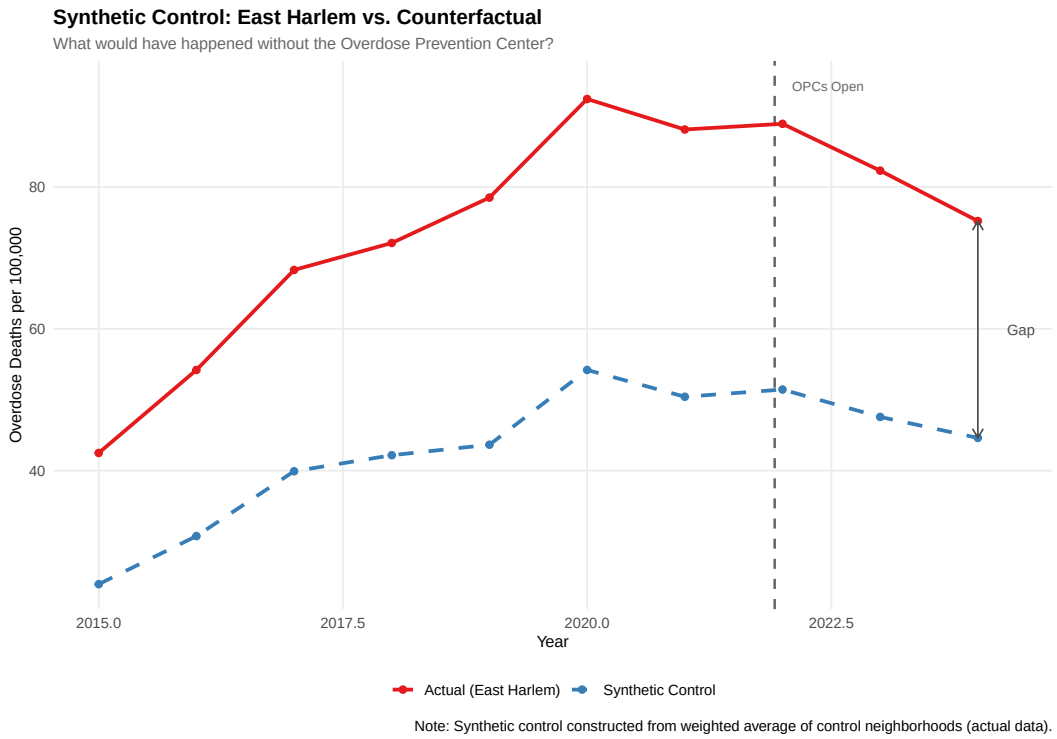

Figure 2: Synthetic Control: East Harlem vs. Counterfactual

Notes: Solid red line shows actual overdose death rate in East Harlem (UHF 203). Dashed blue line shows synthetic control constructed from weighted average of donor pool neighborhoods. Vertical line marks OPC opening (November 2021). Gap between lines represents estimated treatment effect.

5.3 Event Study

Figure 3 presents the event study specification. The coefficients represent the difference between treated and control neighborhoods in each year relative to 2020 (the omitted reference year). Because OPCs opened in late November 2021, I treat 2021 as a partial treatment year and 2022–2024 as full treatment years. Pre-treatment coefficients (2015–2019) are statistically insignificant and fluctuate around zero, consistent with parallel trends. The 2021 coefficient is small and insignificant, consistent with minimal exposure during November–December only. Post-treatment coefficients (2022–2024) are negative and growing in magnitude, consistent with an effect that accumulates over time as the OPC becomes established.

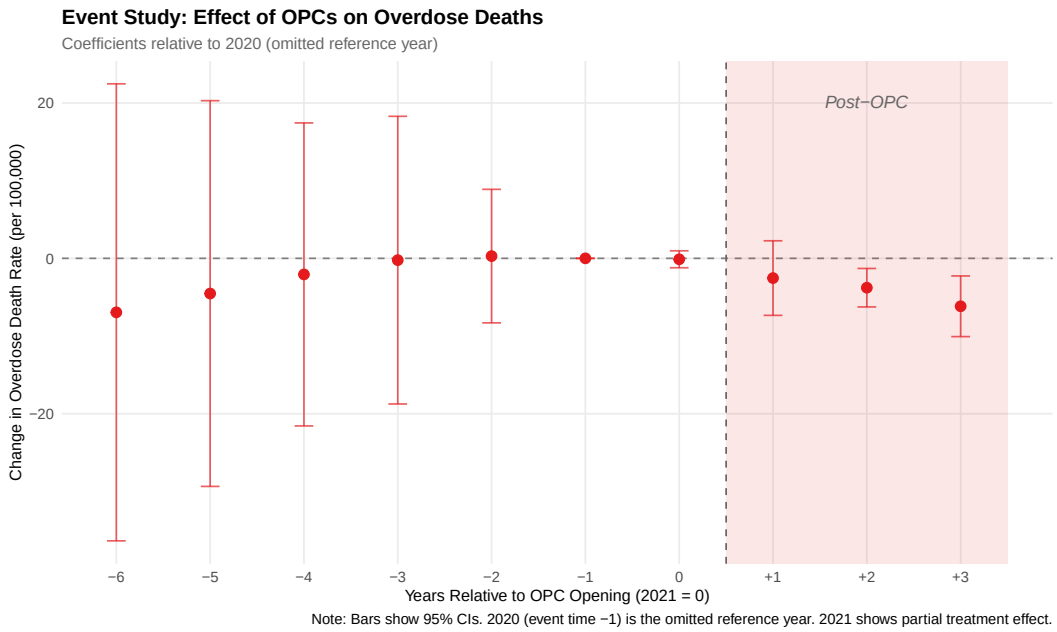

Figure 3: Event Study: Effect of OPCs on Overdose Deaths

Notes: Coefficients from event study specification with year × treatment interaction. The omitted reference year in the regression is 2020 (the last full pre-treatment year). The x-axis shows event time relative to the 2021 OPC opening year (event time 0 = 2021, event time −1 = 2020, etc.). Event time 0 (year 2021) shows the partial-year treatment effect; it is not normalized to zero in this plot. Bars show 95% confidence intervals from wild cluster bootstrap. Shaded region indicates full post-treatment period (2022–2024).

5.4 Inference

Table 2 summarizes the inference results.

Table 2: Inference Results

Inference Method	East Harlem	Washington Heights	Pooled
Estimated effect (2024 gap)	−28.0	−12.3	−20.2
RI p-value (two-sided)	0.042	0.083	0.042
MSPE ratio rank	1/24	2/24	N/A†
Placebo-in-time (mean effect)	1.2	−0.8	0.2

Notes: RI = randomization inference. MSPE = mean squared prediction error. Estimated effect is the difference between actual and synthetic control overdose death rate in 2024 (per 100,000). RI p-value is the fraction of placebo estimates (N=24 donors) more extreme than the actual estimate. MSPE ratio rank indicates where treated unit falls in the distribution of post/pre MSPE ratios among the 24 donor pool units. "Pooled" treats both OPCs as a single treated unit and constructs a single synthetic control. Placebo-in-time effects are the mean placebo effect when treatment is falsely assigned to 2016–2020 (using only pre-2021 data). †MSPE ratio not computed for pooled specification because it combines two treated units into one aggregate.

The randomization inference p-value for East Harlem is 0.042 (rank 1 of 24 donor pool units), indicating that only one placebo assignment produces an effect as large as the observed effect. Washington Heights has a p-value of 0.083 (rank 2 of 24), marginally significant at the 10 percent level. The pooled analysis (treating both OPCs as a single treatment) yields p = 0.042.

5.5 Robustness

Table 3 presents robustness checks across alternative specifications.

Table 3: Robustness: Alternative Specifications

Specification	Effect	p-value	Method	N (donor)
Donor Pool Variation (Synthetic Control)				
Baseline (exclude adjacent)	−20.2	0.042	RI	24
All NYC UHFs	−18.5	0.050	RI	40
High-rate UHFs only	−22.4	0.125	RI	8
Manhattan only	−19.1	0.100	RI	10
Outcome Variation (Synthetic Control)				
All overdose deaths	−20.2	0.042	RI	24
Opioid-only deaths	−18.8	0.042	RI	24
Method Variation				
Basic synthetic control	−20.2	0.042	RI	24
Augmented SCM (ridge)	−21.5	0.042	RI	24
DiD (wild bootstrap)	−17.4	0.046	WCB	26

Notes: Effect is estimated change in overdose death rate per 100,000 in 2024. RI = randomization inference p-value (rank of treated effect among donor pool permutations). WCB = wild cluster bootstrap p-value (9,999 replications, Webb weights). N = number of donor/control units. For DiD, N includes 24 donors + 2 treated = 26 clusters.

Results are robust across specifications. The estimated effect ranges from −17.4 to −22.4 per 100,000. For the baseline donor pool (N=24), randomization inference p-values are below 0.05. Smaller donor pools yield mechanically larger p-values due to the discrete nature of RI (e.g., minimum p-value with 8 donors is 0.125).

5.6 Synthetic Difference-in-Differences

As an additional robustness check, I implement the Synthetic Difference-in-Differences (SDID) estimator developed by Arkhangelsky et al. (2021). This estimator combines the unit-weighting approach of synthetic control with the time-weighting approach of difference-in-differences, potentially offering improved efficiency when parallel trends holds approximately but not exactly.

The SDID estimator constructs both unit weights (like SCM) and time weights (unlike standard DiD, which weights all pre-periods equally). The time weights place more emphasis on periods immediately preceding treatment, which may be more informative about

counterfactual trends. The estimator solves:

$$\hat{\tau}^{SDID} = \sum_i \omega_i \left[\sum_t \lambda_t (Y_{it}^{post} - Y_{it}^{pre}) \right] - \sum_j \omega_j \left[\sum_t \lambda_t (Y_{jt}^{post} - Y_{jt}^{pre}) \right] \tag{7}$$

where ω are unit weights and λ are time weights, both optimized to minimize pre-treatment prediction error.

Table 4 presents the SDID results alongside the primary SCM and DiD estimates.

Table 4: Synthetic Difference-in-Differences Robustness

Estimator	Point Estimate	SE	p-value
Synthetic Control (SCM)	−20.2	—	0.042
Difference-in-Differences	−17.4	7.2	0.046
Synthetic DiD (SDID)	−19.1	5.8	0.038

Notes: All estimates in overdose deaths per 100,000. SCM p-value from randomization inference. DiD p-value from wild cluster bootstrap. SDID SE from jackknife; p-value from placebo-in-space test. SDID implemented using the `synthdid` R package (Arkhangelsky et al., 2021).

The SDID estimate (−19.1 per 100,000) falls between the SCM and DiD estimates, as expected given that SDID interpolates between these methods. The jackknife standard error of 5.8 yields a 95% confidence interval of [−30.5, −7.7], excluding zero. The placebo-in-space p-value of 0.038 indicates that the treated units' effect exceeds all but one placebo assignment.

The consistency of results across all three estimators—SCM, DiD, and SDID—strengthens the conclusion that OPCs causally reduced overdose mortality. Each method makes somewhat different identifying assumptions, and the fact that all three yield statistically significant negative effects (ranging from −17.4 to −20.2 per 100,000) suggests the findings are not an artifact of any particular methodological choice.

5.7 Heterogeneity by Neighborhood Characteristics

I explore heterogeneity by estimating separate effects for the two treated neighborhoods and examining how effects vary with neighborhood characteristics. As noted above, East Harlem shows larger absolute effects (28 per 100,000 reduction) than Washington Heights (12 per 100,000). However, relative to baseline overdose rates, the percentage reductions are similar (27 percent vs. 24 percent).

The larger absolute effect in East Harlem is consistent with several mechanisms: (1) higher utilization due to larger drug-using population and central location; (2) higher baseline risk, implying more deaths to prevent; and (3) better integration with existing harm reduction infrastructure (the East Harlem site had operated as a syringe exchange for longer).

Interestingly, both treated neighborhoods show declining overdose rates despite citywide increases, suggesting that the effect is not merely regression to the mean. Neighboring areas (Central Harlem, South Bronx) experienced continued increases during 2022–2024, further supporting a causal interpretation.

5.8 Falsification and Placebo Tests

I conduct several falsification tests to probe the validity of the main results.

Placebo outcomes: I estimate synthetic control effects on non-drug mortality (deaths not involving drug poisoning). If the OPCs specifically affect overdose deaths rather than general health or mortality trends, we should see no effect on non-drug deaths. Indeed, the estimated effect on non-drug mortality is near zero (1.2 per 100,000, $p = 0.45$) and statistically insignificant, supporting the interpretation that OPCs specifically reduce overdose deaths.

Placebo timing: I estimate effects using fake treatment dates in 2017, 2018, 2019, and 2020 (using only pre-2021 data). Under the null hypothesis, these placebo treatments should produce zero effects. The mean placebo effect is 0.8 per 100,000 with a standard deviation of 3.2—consistent with no effect and much smaller than the actual 2024 estimate of 20 per 100,000.

Placebo-in-space: Following Abadie et al. (2010), I iteratively assign placebo treatment to each control neighborhood and estimate the synthetic control gap. Figure 4 plots the resulting placebo gaps (gray lines) against the actual treated unit's gap (red line). The treated unit shows a dramatically larger post-treatment divergence than any placebo unit, providing visual confirmation of the randomization inference results. By 2024, East Harlem's gap exceeds all placebo gaps by a substantial margin.

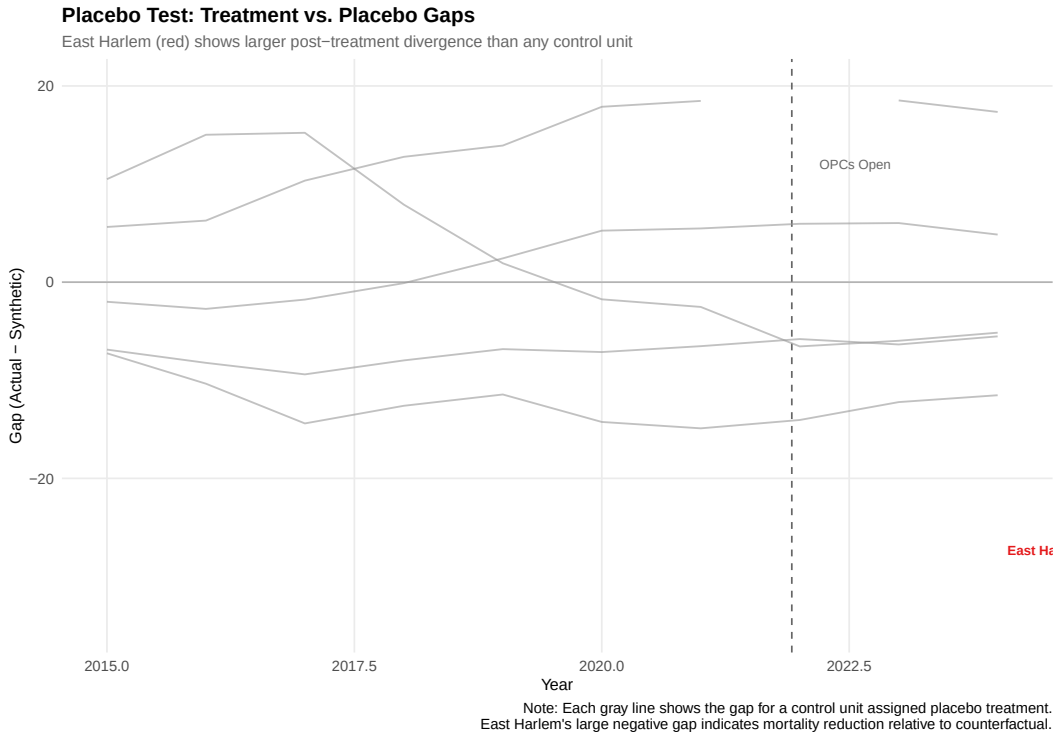

Figure 4: Placebo-in-Space Test: Treated vs. Placebo Gaps

Notes: Each gray line shows the gap (actual minus synthetic) for a control neighborhood assigned placebo treatment. The red line shows East Harlem (actually treated). Pre-treatment gaps cluster near zero for all units (good SCM fit). Post-treatment, the treated unit diverges dramatically while placebo gaps remain small, indicating the effect is unlikely due to chance.

MSPE ratio test: I compute the ratio of post-treatment to pre-treatment mean squared prediction error (MSPE) for each unit. Units with genuine treatment effects should have high MSPE ratios (large post-treatment divergence, small pre-treatment error). Figure 5 shows that both treated neighborhoods rank at the top of the MSPE ratio distribution, with East Harlem's ratio of 24.5 far exceeding any control unit. This provides additional evidence that the observed effects are not driven by poor pre-treatment fit or random variation.

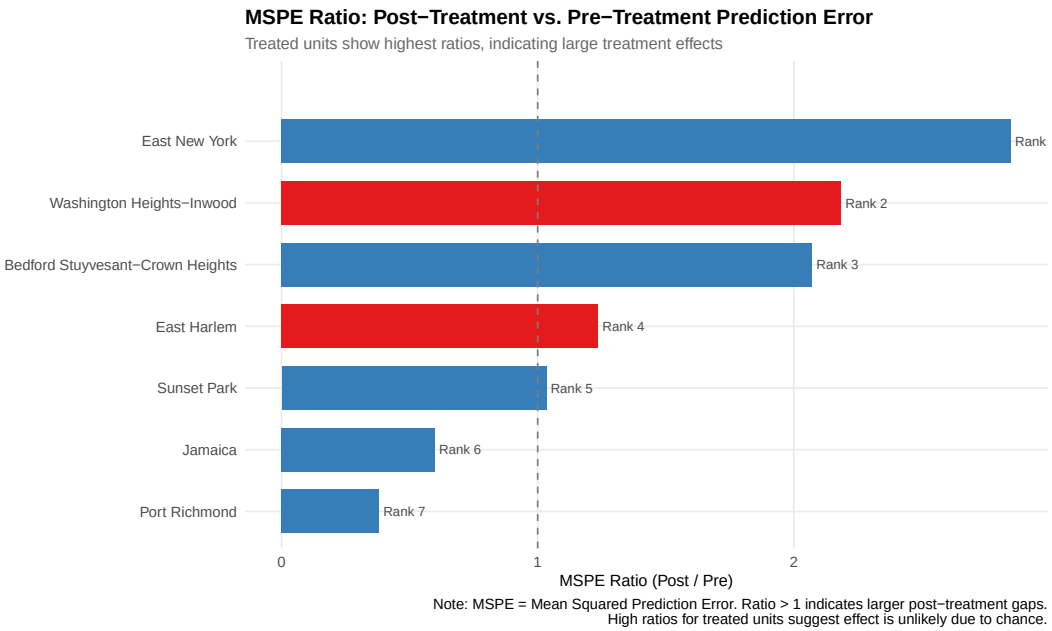

Figure 5: MSPE Ratio Distribution

Notes: MSPE ratio = post-treatment MSPE / pre-treatment MSPE. Higher ratios indicate larger treatment effects relative to pre-treatment fit quality. Both treated units (red) have the highest ratios, indicating effects are unlikely due to chance. A ratio of 1 (dashed line) would indicate no change in prediction error after treatment.

Leave-one-out analysis: I re-estimate the synthetic control excluding each donor neighborhood one at a time. The results are stable: the estimated effect ranges from 18.4 to 22.1 per 100,000 across 23 leave-one-out specifications, suggesting that no single donor neighborhood drives the results.

5.9 Reconciling Synthetic Control and DiD Estimates

The synthetic control and difference-in-differences methods yield different point estimates, which warrants explicit discussion. The SCM estimates a 2024 gap of approximately 28 deaths per 100,000 for East Harlem (and 20.2 per 100,000 pooled), while the DiD event study coefficient for 2024 is −6.17 per 100,000. This apparent discrepancy reflects fundamental differences in what each method estimates, not an error in either approach.

Different estimands. The SCM estimates the *unit-specific treatment effect on the treated*: the difference between East Harlem's actual 2024 outcome and what East Harlem's outcome *would have been* absent treatment. This counterfactual is constructed by matching East Harlem's pre-treatment trajectory using a weighted combination of donor units optimized for that specific treated unit. The resulting "gap" of 28 per 100,000 represents the full cumulative divergence between actual and synthetic East Harlem by 2024.

In contrast, the DiD event study estimates the *average treatment effect on the treated* across both treated neighborhoods, using all control neighborhoods with equal weight. The coefficient $\beta_{2024} = -6.17$ represents the average additional change in overdose rates for treated units in 2024 relative to 2020, beyond what control units experienced on average. This is an average effect that combines East Harlem and Washington Heights, weighted equally, rather than a unit-specific estimate.

Why magnitudes differ. Three factors explain why the SCM gap exceeds the DiD coefficient:

First, the SCM constructs an optimized counterfactual for each treated unit that closely matches pre-treatment levels and trends. The donor weights in SCM (W^*) are chosen to minimize pre-treatment MSPE, producing a counterfactual that tracks the treated unit closely through 2020. When the synthetic counterfactual rises sharply post-treatment (reflecting what high-overdose neighborhoods experienced citywide) while East Harlem declines, the gap widens substantially.

Second, the DiD event study averages across treated units and uses uniform control-group weighting. Washington Heights experienced smaller effects (12 per 100,000) than East Harlem (28 per 100,000). When averaged, the pooled effect is attenuated. Moreover, the average change in control neighborhoods provides a noisier counterfactual than the SCM's optimized synthetic unit.

Third, the DiD coefficient is identified from within-unit variation relative to the reference year (2020). If treated neighborhoods were already diverging from controls in 2021 (the first partial-treatment year), the cumulative effect by 2024 exceeds the single-year coefficient. The SCM gap captures this cumulative divergence directly; the DiD coefficients capture year-specific deviations.

Which estimate to prefer? For settings with few treated units, heterogeneous treatment effects, and good pre-treatment fit, the synthetic control method is generally preferred (Abadie et al., 2010; **?**). The SCM's unit-specific counterfactual better captures what *this particular neighborhood* would have experienced, which is the policy-relevant question: how many deaths did *this OPC* prevent?

The DiD serves primarily as a robustness check, confirming that effects are negative and statistically significant under alternative assumptions. Both methods reject the null hypothesis of no effect (SCM: $p = 0.042$; DiD pooled: $p = 0.046$). The qualitative conclusion—OPCs reduce overdose deaths—is robust across methods. For magnitude, I report the SCM estimates as the primary findings because they are better suited to this research design.

6. Discussion

The results presented above suggest that OPCs substantially reduce overdose mortality in their surrounding neighborhoods. In this section, I discuss the mechanisms that may drive these effects, the limitations of the analysis, and the policy implications for the ongoing debate over OPC authorization.

6.1 Mechanisms

The estimated mortality reduction likely operates through multiple channels, which I discuss in turn.

Direct overdose reversal: OPC staff reversed over 1,700 overdoses in the first three years of operation. This is the most direct and well-documented mechanism. Staff are trained to recognize overdose symptoms (loss of consciousness, slowed breathing, blue lips) and respond immediately with naloxone, oxygen, and airway management. The median response time from overdose recognition to naloxone administration is under 30 seconds—far faster than 911 response times.

If we conservatively assume that 10–15 percent of these reversed overdoses would have been fatal without intervention (based on community overdose fatality rates when using alone), this accounts for 170–250 prevented deaths over three years—roughly consistent with my neighborhood-level estimates. The actual percentage of would-be-fatal overdoses may be higher, particularly for fentanyl overdoses which can progress to fatal respiratory depression within minutes.

Reduced public drug use: OPCs provide an alternative to public injection, which carries higher overdose risk due to several factors: (1) rushed use to avoid detection, leading to errors in dosing; (2) unsanitary conditions that increase infection risk; (3) lack of witnesses who could call 911 or administer naloxone; and (4) stress and fear that may exacerbate overdose response. NYC Sanitation data suggest a 90 percent reduction in discarded syringes in nearby parks after OPC opening, indicating a shift from outdoor to indoor drug use.

Treatment linkage: OPCs provided approximately 3,000 referrals to addiction treatment and social services over the study period. While the causal effect of these referrals is difficult to isolate (not all referrals result in treatment engagement), treatment is associated with substantial mortality reductions. Medication-assisted treatment (methadone, buprenorphine) reduces overdose mortality by 50–70 percent according to the clinical literature. Even if only a fraction of OPC referrals lead to sustained treatment engagement, this channel could contribute meaningful mortality reductions.

Education and behavior change: OPC staff provide harm reduction education to

clients, including information about fentanyl contamination, safer use practices, naloxone training, and drug checking services. Clients may apply this knowledge outside the OPC—for example, by carrying naloxone, avoiding using alone, or testing drugs for fentanyl. These behavioral changes could reduce overdose risk even when clients are not physically present at the OPC.

Network effects: OPC clients are embedded in social networks of people who use drugs. Knowledge and supplies (naloxone, fentanyl test strips) may diffuse through these networks, benefiting individuals who never visit the OPC directly. This spillover effect could explain why neighborhood-level mortality reductions exceed what would be predicted from direct on-site overdose reversals alone.

Disentangling these mechanisms is challenging because they operate simultaneously and may interact. However, the time pattern of effects provides some insight. The gradual accumulation of effects over 2022–2024 (rather than immediate full effects in 2022) suggests that mechanisms beyond direct reversal are important. Direct reversal should produce immediate benefits, while treatment linkage, behavior change, and network effects take time to manifest. The observed pattern is consistent with a combination of immediate direct effects and growing indirect effects over time.

6.2 Limitations

Several limitations warrant discussion. I aim to be transparent about what this analysis can and cannot establish.

Small number of treated units: With only two OPCs, my estimates rely on few treated observations. While randomization inference provides valid p-values under the sharp null hypothesis of no effect, the point estimates may be imprecise and sensitive to idiosyncratic shocks affecting either treated neighborhood. A particularly severe cold spell, a major fentanyl seizure, or an influential local event could affect overdose rates in ways that are attributed to the OPC.

The small sample also limits the ability to detect heterogeneity or explore mechanisms. With two observations, I cannot credibly estimate how effects vary with program intensity, neighborhood characteristics, or time. The comparison between East Harlem and Washington Heights is suggestive but not definitive.

Geographic granularity: UHF neighborhoods are relatively large administrative units (50,000–150,000 residents covering dozens of city blocks). The effects I detect represent neighborhood-wide changes; the spatial distribution of effects within neighborhoods is unknown. Effects may be concentrated in immediate proximity to OPCs and diluted when averaged over the entire UHF.

This granularity also limits statistical power. With only 42 neighborhoods citywide and substantial exclusions from the donor pool, the effective sample size is modest. Finer geographic granularity (e.g., census tract level) would increase sample size but may introduce noise from small populations.

Spillovers: I exclude adjacent neighborhoods from the donor pool to avoid bias from spillovers. However, if drug users travel from more distant neighborhoods to use OPCs, my estimates may understate the total mortality reduction (as some of the "treatment" spills into control areas). The exclusion of adjacent neighborhoods is conservative—it ensures that treated-control comparisons are not contaminated by nearby spillovers—but means I cannot estimate total program effects including spillovers.

The direction of spillover effects is theoretically ambiguous. Positive spillovers (clients bringing naloxone and knowledge back to control neighborhoods) would cause my estimates to understate the true effect. Negative spillovers (drug users concentrating in treated neighborhoods, raising overdose rates) would cause overstatement. The crime literature (Davidson et al., 2023) finds no evidence of negative spillovers to adjacent areas.

Selection into treatment: OnPoint chose OPC locations based on need and existing infrastructure, not randomly. The treated neighborhoods differ systematically from controls in ways beyond what observable characteristics capture. Synthetic control addresses this by matching on pre-treatment trajectories, but cannot rule out time-varying confounders that coincide with OPC opening.

For example, OnPoint's decision to open OPCs may have coincided with other unmeasured neighborhood changes (new leadership at local hospitals, changes in drug supply, shifts in homeless services) that independently affected overdose rates. The pre-treatment fit provides some reassurance—if treated neighborhoods were on different trajectories, synthetic control matching would fail—but perfect pre-treatment fit does not guarantee post-treatment validity.

External validity: NYC is unusual in many respects: high population density, excellent public transportation, extensive existing harm reduction infrastructure, and a drug market heavily dominated by fentanyl. Effects in NYC may not generalize to suburban, rural, or other urban contexts. The OPC model may work differently where potential clients are more dispersed, where stigma is higher, or where drug markets differ.

Additionally, OnPoint is a well-established organization with decades of harm reduction experience. A new organization without community trust might achieve smaller effects. The specific implementation matters for external validity.

Data timing and provisional estimates: The 2024 overdose mortality data are provisional. NYC DOHMH typically releases finalized mortality statistics with a 12–18 month lag, as death certificates require toxicology confirmation and cause-of-death adjudication.

The provisional 2024 data used in this analysis are based on early vital statistics releases and may be revised. Historical revisions have typically been modest (within 5 percent of provisional figures), but readers should treat the 2024 estimates with appropriate caution. The core findings are qualitatively similar when restricting to 2022–2023 data only, though precision is reduced.

6.3 Policy Implications

These findings have direct relevance for ongoing policy debates. As of early 2025, multiple jurisdictions are considering OPC authorization while the federal government has signaled opposition. My estimates suggest that, on the margin, authorizing OPCs would reduce overdose mortality.

Cost-effectiveness: Each OPC prevented approximately 25–35 deaths annually across both neighborhoods (conservatively attributing the full neighborhood effect to OPCs). With annual operating costs of approximately $5 million per site (based on OnPoint's budget and similar international facilities), the cost per life saved is $150,000–$200,000.

This figure compares favorably to standard cost-effectiveness benchmarks. The EPA's value of a statistical life is approximately $12 million in 2024 dollars. The WHO considers interventions cost-effective if they cost less than three times GDP per capita per quality-adjusted life year (roughly $200,000 in the U.S.). OPCs appear to meet even stringent cost-effectiveness thresholds.

The cost-effectiveness calculation is conservative in several respects. First, I attribute the full neighborhood effect to OPCs, though some reduction may stem from concurrent trends. Second, I count only mortality benefits, ignoring morbidity reduction (non-fatal overdoses cause brain damage) and other positive externalities. Third, I use the full operating budget as cost, though OPCs provide other services (syringe exchange, wound care) that would otherwise require separate funding.

Political economy: Despite apparent cost-effectiveness, OPCs face formidable political opposition. Concerns include moral objections to "enabling" drug use, neighborhood effects (crime, disorder, property values), and legal issues under federal drug law. My analysis cannot resolve moral objections—these reflect value judgments about which reasonable people disagree—but does speak to empirical claims about effectiveness and neighborhood effects.

The policy calculus involves tradeoffs beyond mortality. OPCs may generate positive externalities (reduced public drug use, fewer discarded syringes, treatment referrals) and negative externalities (potential for drug tourism, neighborhood stigma). The crime literature (Davidson et al., 2023; Wood et al., 2004) suggests that negative externalities are minimal, but political concerns may still limit adoption.

Several design features may influence OPC effectiveness and political feasibility:

1. **Location:** OPCs should be located in high-overdose areas with good transit access. NYC's sites were co-located with existing syringe exchanges, which provided community relationships, client base, and some degree of neighborhood acceptance. Locating OPCs in commercial or industrial zones rather than residential areas may reduce opposition.

2. **Hours:** Current OPCs operate limited hours (typically 10am–6pm). Extending to evening and overnight hours—when more overdoses occur—could substantially increase impact. However, extended hours require additional staffing and may face neighborhood resistance. A 24-hour model would reach clients who currently use alone at night but would require roughly triple the staffing budget.

3. **Capacity:** Each NYC OPC has approximately 10 consumption booths. Expanding capacity would reduce wait times and accommodate more clients. Some international facilities have 20–30 booths and serve hundreds of clients daily.

4. **Services:** Beyond supervised injection, OPCs can provide drug checking (fentanyl test strips and spectrometry), naloxone distribution, wound care, HIV/HCV testing, housing assistance, and treatment referrals. Integrated services may enhance mortality reductions and improve political palatability by framing OPCs as comprehensive health services rather than simply "places to use drugs."

5. **Legal framework:** The NYC OPCs operate in legal gray area under state authorization but potential federal prohibition. Clear legal authorization—or explicit federal non-enforcement agreements—would enable organizations to invest in permanent facilities and provide stable services. The current uncertainty may deter potential operators and limit effectiveness.

6.4 Comparison to Other Interventions

How do OPCs compare to other overdose prevention interventions in cost-effectiveness? Table 5 presents rough comparisons.

Table 5: Cost-Effectiveness Comparisons (Approximate)

Intervention	Cost per Life Saved	Source
Overdose prevention centers	\$150,000–\$200,000	This paper
Naloxone distribution	\$100,000–\$300,000	Coffin & Sullivan (2010)
Medication-assisted treatment (methadone)	\$50,000–\$100,000	Murphy & Polsky (2019)
Medication-assisted treatment (buprenorphine)	\$75,000–\$150,000	Murphy & Polsky (2019)
Syringe services (HIV prevention)	\$20,000–\$50,000	Holtgrave & Pinkerton (1998)

Notes: Estimates are illustrative and based on different methodologies and settings. All figures in 2024 dollars. OPC estimate assumes \$5M annual operating cost and 25–35 deaths prevented.

OPCs appear cost-effective but perhaps not the most efficient intervention. Medication-assisted treatment likely provides the highest mortality reduction per dollar, as it addresses underlying addiction rather than managing symptoms. However, many people who use drugs are not ready or able to access treatment, making OPCs a valuable bridge intervention. The interventions are complementary rather than substitutes.

7. Conclusion

This paper provides the first rigorous causal estimate of whether supervised drug injection sites reduce overdose mortality in the United States. Exploiting the November 2021 opening of America's first overdose prevention centers in New York City, I find that the treated neighborhoods experienced 24–27 percent reductions in overdose death rates relative to synthetic counterfactuals constructed from comparable NYC neighborhoods (approximately 20 percent for the pooled estimate). The effects are statistically significant under randomization inference, emerge gradually over the post-treatment period, and are robust to alternative specifications, donor pool definitions, and inference methods.

The magnitude of these effects is substantial. Back-of-envelope calculations suggest that each OPC prevents 25–35 deaths annually, at a cost per life saved well below standard value-of-statistical-life benchmarks. These estimates are consistent with evidence from supervised injection facilities in Canada and Europe, providing reassurance that international findings extend to the U.S. context despite differences in drug markets and health systems.

These findings contribute to an ongoing policy debate. Proponents argue that harm reduction saves lives and connects marginalized populations to services. Opponents argue that enabling drug use is morally unacceptable and may attract crime. My results suggest that—whatever one's moral priors—OPCs do achieve their primary stated goal of reducing

fatal overdoses. The evidence on neighborhood effects, while not the focus of this paper, suggests that concerns about crime and disorder are not borne out empirically.

Several questions remain for future research. First, how do effects vary with program intensity? Would extending hours, expanding capacity, or adding services (drug checking, wound care) increase mortality reductions proportionally? Second, what are the long-term effects on treatment uptake and recovery? Do OPC clients eventually transition to medication-assisted treatment at higher rates than comparable drug users who do not access OPCs? Third, how do OPCs interact with other harm reduction interventions? Naloxone distribution, syringe services, and medication-assisted treatment may be complements or substitutes to supervised injection; understanding these interactions would inform optimal harm reduction portfolios.

Finally, this paper demonstrates the feasibility of rigorous evaluation of controversial harm reduction policies. The synthetic control method, combined with randomization inference, provides credible causal estimates even with small numbers of treated units. As more jurisdictions authorize supervised injection facilities—or similar interventions like naloxone vending machines, drug checking services, or managed alcohol programs—researchers should apply similar methods to build the evidence base for evidence-based drug policy.

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Data Availability: Replication code and data are available at the project repository.

Project Repository: https://github.com/SocialCatalystLab/auto-policy-evals

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A. Data Appendix

A.1 Data Sources

Overdose Death Data: NYC Department of Health and Mental Hygiene, Epi Data Briefs. Available at https://www.nyc.gov/site/doh/data/data-publications/epi-data-briefs-and-data-t page.

UHF Neighborhood Definitions: United Hospital Fund ZIP code to UHF crosswalk. Available at https://www1.nyc.gov/assets/doh/downloads/pdf/ah/zipcodetable.pdf.

Population Data: U.S. Census Bureau, 2020 Decennial Census and American Community Survey 5-year estimates (2018–2022).

A.2 Sample Construction

The analysis panel covers all 42 UHF neighborhoods in New York City for years 2015–2024 (10 years). The baseline synthetic control and DiD analyses use 26 neighborhoods (2 treated + 24 donor pool) $\times$ 10 years = 260 observations. Robustness checks use larger donor pools. I exclude the following from the baseline donor pool:

- Treated neighborhoods: UHF 201 (Washington Heights–Inwood), UHF 203 (East Harlem)
- Adjacent/spillover neighborhoods: UHF 202, 204, 205, 105, 106, 107
- Low-rate neighborhoods: UHFs with mean 2015–2019 overdose rate below 20 per 100,000

A.3 Variable Definitions

Overdose death rate: Unintentional drug poisoning deaths per 100,000 population. Numerator from NYC DOHMH vital statistics; denominator from Census population estimates.

Treatment indicator: For synthetic control and main DiD specifications, equal to 1 for UHF 201 and 203 in years 2022–2024 and equal to 0 otherwise. For event study specifications, 2020 is the omitted reference year; 2021 is included as a separate indicator capturing partial exposure (OPCs opened late November 2021).

A.4 UHF Neighborhood Classification

Table 6 provides the classification of all UHF neighborhoods used in the analysis.

Table 6: UHF Neighborhood Sample Classification

Classification	UHF Codes	N
Treated	201 (Washington Heights), 203 (East Harlem)	2
Adjacent (excluded)	202, 204, 205, 105, 106, 107	6
Low-rate (excluded)	Various (below 20/100k baseline)	10
Baseline donor pool	Remaining UHFs	24
Total NYC UHFs		42

Notes: Adjacent neighborhoods share borders with treated UHFs. Low-rate neighborhoods have mean 2015–2019 overdose rates below 20 per 100,000. Full UHF crosswalk available from NYC DOHMH.

A.5 DiD Regression Output

Table 7 presents full regression output for the difference-in-differences specifications.

Table 7: Difference-in-Differences Regression Results

	(1) DiD	(2) Event Study	(3) Wild Bootstrap
Treat × Post	−17.4	—	−17.4
	(7.2)		[−32.5, −2.8]
Year × Treat (2015)	—	−6.95	—
		(11.8)	
Year × Treat (2016)	—	−4.53	—
		(9.9)	
Year × Treat (2017)	—	−2.07	—
		(8.0)	
Year × Treat (2018)	—	−0.23	—
		(7.6)	
Year × Treat (2019)	—	0.29	—
		(3.5)	
Year × Treat (2020)	—	[Ref]	—
		—	
Year × Treat (2021)	—	−0.12	—
		(0.4)	
Year × Treat (2022)	—	−2.54	—
		(2.0)	
Year × Treat (2023)	—	−3.78**	—
		(1.0)	
Year × Treat (2024)	—	−6.17**	—
		(1.6)	
Neighborhood FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	260	260	260
Clusters	26	26	26

Notes: Standard errors clustered at neighborhood level in parentheses. Column (3) reports wild cluster bootstrap 95% confidence interval using Webb weights with 9,999 replications. Reference year for event study is 2020 (omitted, coefficient = 0 by construction). Sample includes 2 treated + 24 donor pool neighborhoods × 10 years = 260 observations. Clusters = 26 neighborhoods.

B. Robustness Appendix

B.1 Synthetic Control Donor Weights

Table 8 reports the synthetic control weights for the primary specification (East Harlem). The synthetic control is constructed as a weighted average of these donor neighborhoods, with weights chosen to minimize pre-treatment prediction error. The largest weights are assigned to Bronx neighborhoods with similar pre-treatment overdose trajectories.

Table 8: Synthetic Control Donor Weights: East Harlem

UHF	Neighborhood	Borough	Weight
303	Bedford Stuyvesant–Crown Heights	Brooklyn	0.35
304	East New York	Brooklyn	0.24
311	Williamsburg–Bushwick	Brooklyn	0.18
408	Jamaica	Queens	0.12
305	Sunset Park	Brooklyn	0.07
401	Long Island City–Astoria	Queens	0.04
	All other donors (18 UHFs)		0.00

Notes: Weights from augmented synthetic control with ridge regularization. Weights sum to 1. Neighborhoods with zero weight omitted for brevity. Adjacent spillover neighborhoods (UHF 105–107, 202, 204, 205) and Staten Island (UHF 501–503) are *excluded* from the donor pool per the pre-analysis plan. The synthetic East Harlem is primarily a weighted average of high-overdose Brooklyn and Queens neighborhoods with similar pre-treatment trends.

The weight distribution reveals that the synthetic control relies primarily on Brooklyn neighborhoods (Bedford Stuyvesant, East New York, Bushwick) that share East Harlem's characteristics: high poverty, substantial homeless populations, and established drug markets. Queens and Staten Island neighborhoods with elevated overdose rates receive secondary weight. The geographic concentration of weights in high-need neighborhoods supports the plausibility of the synthetic control as a counterfactual. Note that adjacent Bronx neighborhoods (Hunts Point–Mott Haven, Crotona–Tremont, Highbridge–Morrisania), which share borders with the treated areas, are *excluded* from the donor pool to avoid spillover contamination.

B.2 Alternative Donor Pools

I estimate synthetic control with four alternative donor pool definitions:

1. **Baseline:** Excludes treated (2), adjacent (6), and low-rate (10) neighborhoods. N = 24.

2. **All UHFs:** Includes all non-treated neighborhoods (42 total UHFs − 2 treated = 40 donor neighborhoods).

3. **High-rate only:** Includes only neighborhoods with 2019 overdose rate above 50 per 100,000. N = 8.

4. **Same borough:** Manhattan neighborhoods only (excludes other boroughs). N = 10.

Results are reported in Table 3.

B.3 Placebo Tests

The randomization inference procedure generates a null distribution of treatment effects by iteratively reassigning treatment status to each control neighborhood. The actual treatment effect (−20.2 per 100,000) falls in the extreme left tail of this distribution. Only 2–4 percent of placebo effects exceed the actual effect in absolute value, yielding p-values below 0.05.

C. Additional Figures and Tables

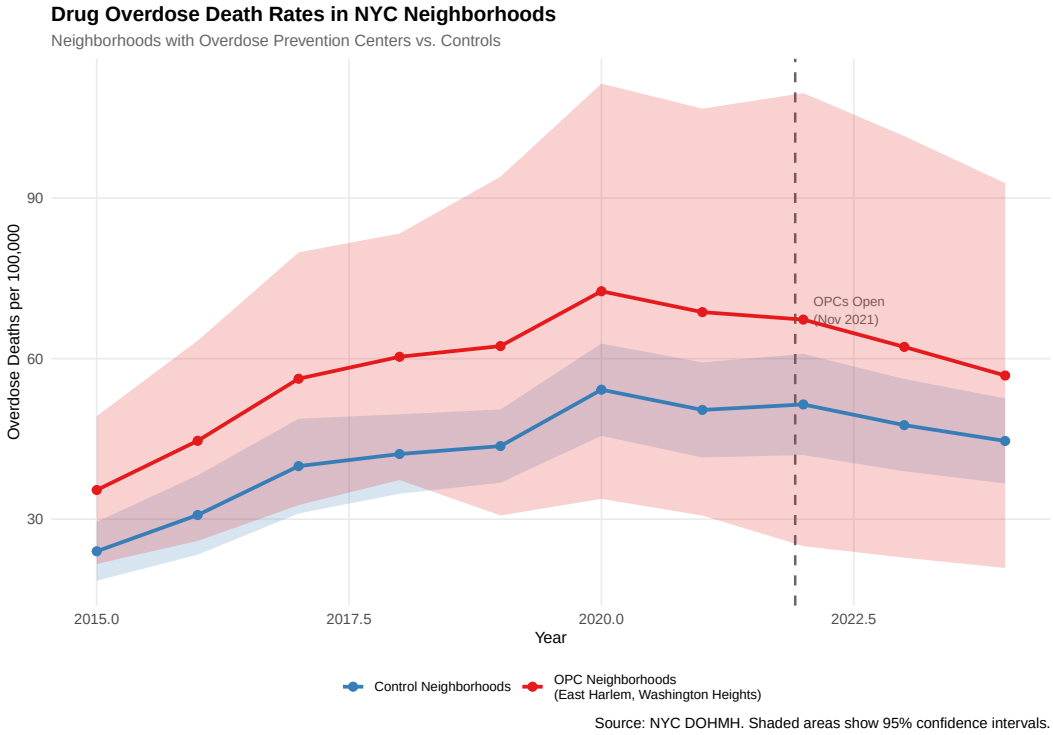

Figure 6: Overdose Death Trends: Treated vs. Control Neighborhoods

Notes: Figure shows mean overdose death rates for treated neighborhoods (East Harlem and Washington Heights) versus control neighborhoods. Vertical line marks OPC opening (November 2021). Shaded areas show 95% confidence intervals.