ARE SYRINGE EXCHANGE PROGRAMS HELPFUL OR HARMFUL? NEW EVIDENCE IN THE WAKE OF THE OPIOID EPIDEMIC

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Abstract

The increase in the use of prescription and illegal opioid drugs in the United States since the early 2000s has raised concern about the spread of bloodborne illnesses through syringe sharing. In response, many public health entities have called for an expansion in syringe exchange programs (SEPs), which provide access to sterile syringes and facilitate safe disposal of used needles for injection drug users. This paper investigates the effects of recent SEP openings on HIV diagnoses and drug-related mortality in the wake of the opioid crisis using a difference-in-differences approach that compares the changes in health outcomes in counties that introduced SEPs to changes in other US counties with existing SEPs. I find that SEP openings decrease HIV diagnoses by 11.3-30.0 percent, corresponding to 30 fewer HIV cases per county per year, on average. However, I present new evidence that SEPs increase rates of opioid-induced mortality and opioid-related hospital admissions, especially in rural and high-poverty areas, suggesting that needle exchanges may be less effective than other interventions at stimulating recovery.

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1 Introduction

In 2016, over 64,000 people in the United States died of a drug overdose, with two-thirds of overdoses involving opioids. The US is in the midst of an opioid crisis, with the US Drug Enforcement Agency declaring in 2015 that drug overdose deaths had reached “epidemic levels" (U.S. Department of Justice, 2015). Consequently, the increase in injection drug use has led to greater risk of illness due to needle sharing. In recent years, acute cases of hepatitis C infections increased by 150 percent, and HIV diagnoses for white males aged 25-34 increased in 2013, reversing a decades-long trend (Centers for Disease Control and Prevention, 2015).

In light of this epidemic, many public health entities, including the CDC and some state and local health departments, have called for an expansion in syringe exchange programs (SEPs), which provide access to sterile syringes and facilitate safe disposal of used needles for injection drug users. Given that HIV and hepatitis C are both spread via shared needles, and that over one-third of injection drug users report having shared a needle in the past year, proponents of SEPs argue that there is scope for such programs to reduce the spread of bloodborne illness and create new opportunities for drug counseling (Centers for Disease Control and Prevention, 2016).

From an economic standpoint, SEPs have the potential to create large positive externalities by reducing the stock of used needles on the streets and preventing the spread of disease. However, by providing clean needles to drug users, reducing the stigma of using drugs and/or creating a safe environment for networking with other users, SEPs may also generate moral hazard. In particular, lowering the cost of obtaining needles and other supplies incentivizes drug users to inject more frequently, potentially exacerbating rates of opioid misuse and abuse.

In this paper, I test the causal relationships between SEP openings and drug-related health and crime outcomes. Because no official national directory of SEPs exist, I construct a handcollected dataset on program locations and opening dates to identify areas exposed to SEPs within the last ten years. In particular, I compare rates of HIV, opioid-related deaths, opioid-related overdoses, and drug-related crimes in counties with SEP openings to other counties with existing programs before and after the initial year of implementation. I find that SEPs decrease the number of HIV cases by approximately 30 cases per county per year, on average, and these effects are concentrated in urban and high-poverty areas. Moreover, I find that SEP openings increase drug-related mortality. Most notably, I estimate an increase in opioid-related deaths by 13.5 percent, and and provide some evidence that SEPs lead to a higher rate of emergency room visits and in-patient stays for drug-related complications. I also find that drug-related arrests increase after an opening, suggesting that SEPs may lead to more drug use. Effects are largest in rural and high-poverty areas, suggesting that those with larger geographic or financial obstacles to substance abuse treatment are most affected by such programs.

These findings contribute to a recent and growing literature on policies targeting opioid availability and abuse. In the past two decades, a number of state-level policies have been implemented with varying degrees of effectiveness. In particular, recent work has documented that prescription drug monitoring programs (PDMPs), which electronically record patients receiving opioids into a state-wide registry, decrease the number of oxycodone shipments, opioid abuse among young adults, and misuse for Medicare Part D patients (Buchmueller and Carey, 2018; Dave, Grecu, and Saffer, 2017; Mallatt, 2017). However, there is some evidence that PDMPs cause patients to substitute towards illicit opioids, like heroin and fentanyl when prescription pills become unavailable (Mallatt, 2017).

In an effort to prevent misdoses from leading to death, all states have recently legalized civilian access to naloxone, a drug that can reverse the symptoms of opioid use when administered during an overdose. While Rees, Sabia, Argys, Latshaw, and Dave (2017) finds that Naloxone Access Laws, which allow lay people to administer naloxone, lead to significant reductions in opioid-related deaths, Doleac and Mukherjee (2018) finds that naloxone access leads to more opioid-related ER visits and increases in drug-related crimes, with no average effects on mortality. Other state-level legal restrictions, including prescription limits, patient ID laws, doctor shopping restrictions, and pain clinic regulations have been ineffective at preventing opioid use (Meara, Horwitz, Powell, McClelland, Zhou, O'Malley, and Morden, 2016; Bao, Pan, Taylor, Radakrishnan, Luo, Pincus, and Schackman, 2016).[1] Despite this limited success, new studies show that policies aimed at physician training may be a promising way to prevent overprescribing; for example, Schnell and Currie (2017) documents that physician education plays a role in prescribing behavior, suggesting that better medical training can lead to fewer opioid prescriptions.[2]

While much of the current literature has focused on the availability of opioids and other supply-side restrictions, this is one of the first papers to analyze the causal effects of demand-side policies and estimate to what extent providing sterile needles, hygiene kits, and referral options for drug counseling in a non-judgmental setting can address the spread bloodborne illness and opioid misuse. More specifically, this paper builds on existing studies by using a unique dataset on SEP openings to measure the effects of SEPs on HIV diagnoses, opioid-related mortality, drug-related hospitalizations, and crime in the wake of the opioid crisis.

Previous research on SEPs is largely correlational, and focuses on syringe sharing during the AIDS crisis

[1] For a comprehensive review of earlier studies analyzing the effectiveness of state policies, see Haegerich, Paulozzi, Manns, and Jones (2014). Less than 10% of the studies evaluating the effects of PDMPs, clinical guidelines, naloxone distribution programs, use experimental or quasi-experimental approaches to evaluate provider or patient behavior. Findings on the effects of PDMPs on are mixed, and there is little evidence that insurance interventions, drug take-back events, pill mill legislation, clinical guidelines, or education campaigns affect prescribing behavior or drug use.

[2] Moreover, there is some evidence to suggest that marijuana legalization can reduce opioid prescriptions for Medicare patients (Bradford, Bradford, Abraham, and Adams, 2018).

in the 1980s and 1990s. These studies generally find that the programs are associated with reductions in the spread of HIV, reduced syringe sharing behavior, and are not correlated with an increase in the amount of drugs used by current drug users or an increase in new drug users (General Accounting Office, 1993; World Health Organization, 2004). Moreover, some studies report that SEPs result in fewer discarded contaminated syringes, indicating that the benefits of preventing the spread of HIV through contaminated syringes are not limited to people who inject drugs (General Accounting Office, 1993; DeSimone, 2005).[3] However, SEPs are documented to be less effective in reducing hepatitis C, implying that existing conclusions regarding SEP cost-effectiveness to date may be overstated (Pollack, 2001).

Importantly, the data from the complied studies include small sample sizes (often looking at data from only one syringe exchange clinic) and self-reported data regarding individuals' drug use rates, and do not typically consider spillover effects or externalities on those not directly treated. Additionally, many studies use data from other countries, such as Canada, Sweden, or New Zealand to serve as a comparison group for drug rates in the US. These findings are problematic for addressing causality, given that other developed countries have differing policies on the operations of SEPs. For example, New Zealand's syringe services are fee-based while Australia provides syringes free-of-charge and even has syringe vending machines that allow injection-drug users to obtain clean syringes at any time of the day (Sean Cahill and Nathan Schaefer, 2009). And many countries, including Canada, provide free substance abuse treatment to injection drug users, obscuring the true effects of needle exchange.

Moreover, the recent opioid crisis differs from the AIDS crisis in many ways, provoking a need for the reexamination of the effectiveness of harm reduction policies. For example, the opioid epidemic is dramatically escalating; among men aged 25-44, opioid-related mortality more than doubled every year from 2013-2016, representing a sharper increase than any one year during the AIDS epidemic. The effects of the opioid epidemic are larger-reaching and are more broadly distributed among US counties than the AIDS epidemic, affecting not only cities, but rural and suburban areas as well. And increases in opioid-related mortality have been exacerbated by the influx of illicit fentanyl in the US, a drug that is 80 to 100 times stronger than morphine (Waitemata District Health Borad, 2014).

Lastly, although many studies attribute SEPs with reductions in bloodborne illness over time, since HIV rates have been falling significantly throughout the United States during the last two decades, other factors likely also contributed to the decline in disease. The goal of this paper is to separate out the effects of a

[3]Specifically, General Accounting Office (1993) includes information on various compilations of medical studies that had been performed throughout the 1990s in the United States, Australia, Canada, the Netherlands, Sweden, and the United Kingdom. DeSimone (2005) uses survey data from 1989-1995 to analyze the causal effect of the introduction of needle exchange programs (NEPs) in 9 large US cities. Using a probit regression analysis, he finds that the presence of a NEP is associated with a 13 percent reduction in drug injection, and argues that these estimates may be reflective of broader public health interventions occurring concurrently with the introduction of NEPs, while also noting that contact with NEPs can serve as a gateway to drug counseling and/or substance abuse treatment.

SEP opening from the effects of these other factors to better understand the way in which SEPs can affect health. To do so, I use administrative data on health and crime outcomes from 2008–2016 to estimate the effects of SEPs on HIV diagnoses, opioid mortality, opioid-related hospital visits, and drug-related crimes using a difference-in-differences design that compares changes in counties with a SEP opening to changes observed in other US counties with SEPs. The results of these analyses indicate that SEPs do lead to significant reductions in HIV rates. Estimates indicate an effect of 11.3-30.0 percent across 3 years, with effects concentrated in high-poverty areas. However, I also find that SEPs lead to increases in drug mortality and drug use. Estimates indicate that a SEP opening corresponds to a 8.9 percent increase in opioid-related emergency department visits and a 13.5 percent increase in opioid-related mortality, with larger effects in rural areas. These findings indicate that while SEPs are successful at reducing bloodborne illness, they may unintentionally encourage more opioid use by lowering the physical or networking costs of injecting drugs.

The remainder of this paper is organized as follows. In the next section I provide background information on the history and daily operations of syringe exchange programs. Next I describe the empirical approach I use to estimate the effects of SEPs on HIV cases and drug-related overdoses. I then discuss the results of my analysis before providing some concluding thoughts.

2 Background on Syringe Exchange Programs (SEPs)

SEPs, also known as syringe services programs, are community-based public health programs that provide harm reduction services and supplies such as sterile needles, syringes, and other injection and disposal equipment and safe needle disposal. Comprehensive programs also offer HIV counseling, testing, and education, as well as referrals to substance treatment facilities or other medical and mental health services.

In the US, clients are not required to provide proof of income, health insurance, or drug usage to receive supplies, and nearly all programs allow clients to receive more syringes than deposited. About 82 percent of SEP budgets are from public funding sources, through provisions from city, county, or state governments (Jarlais, Guardino, Nugent, and Solberg, 2014). While the federal government has the ability to prohibit federal funding to support SEPs, states have authority to determine regulations for the existence, operation, and local funding of SEPs. Currently, SEPs are legal in 26 states and the District of Columbia, permitted in 9 states, and illegal in 15 states (LawAtlas, 2017).[4]

Since the early 2000s, more communities have opened SEPs in an effort to curtail the spread of HIV and hepatitis C. Figure 1 shows how the number of SEPs has changed over time, according to data from the

[4]States with permitted programs include those states where local units have interpreted state laws to allow syringe access services or where no law explicitly prohibits syringe exchange. States where SEPs remain illegal include Alabama, Arkansas, Florida, Georgia, Idaho, Iowa, Kansas, Missouri, Mississippi, Nebraska, Oklahoma, South Carolina, South Dakota, Texas, and Wyoming (LawAtlas, 2017).

North American Syringe Exchange Network. In 1998, there were 131 SEPs, but by the end of 2013, there were 204 known SEPs. This trend is mirrored in new locales; in 1998 only 77 cities had an SEP, but by 2013, 116 did, suggesting that over half of SEPs opened in areas that previously had no program (Jarlais, Guardino, Nugent, and Solberg, 2014).

To further demonstrate how the usage of SEPs has changed over time, Figure 1 also shows the number of syringes exchanged, in millions. Since 1998, the number of needles exchanged has increased by 155%, with the largest increases occurring from 2005-2013. That both the number of SEPs and syringes exchanged has increased dramatically over the course of the last twenty years has important implications for the effects on drug use and spread of bloodborne illness. Most obviously, one would expect that the exchanges reduce the proportion and/or number of used syringes improperly disposed. However, if the cost of obtaining needles is substantially lower, drug use could increase as a result, potentially leading to more misdoses and more needle sharing. Given that both the number of opioid-related deaths have been increasing steadily over time and that the number of new HIV cases has in recent years reversed a decades-long downwards trend for some groups (see Figure 2), it is important to disentangle outside factors simultaneously contributing to these trends to determine how much these health outcomes would be affected in the *absence* of SEPs.

Because there is no national reporting system for SEPs or their clients, I cannot track how a SEP opening affects the number or composition of patients at each center. Nonetheless, in an attempt to speak to the daily activities of SEPs and visitor characteristics, in Table A1, I present 2018 visit-level data on client attributes and equipment and services received for a rural, Midwest SEP located in Portsmouth, Ohio. I note that these data are not representative of the entire US, but may shed light on program-level operations in an area of the country that has been largely affected by the opioid epidemic.

Overall, client characteristics mirror those of the population of Portsmouth, with the SEP assisting almost all White clients and client age averaging 37.8 years old.[5] Notably, over 22 percent of clients report having previously sought addiction treatment, with approximately one-third of patients reporting having previously overdosed.

Like many other SEPs, the Portsmouth program is open only one day of the week (Fridays). Although a majority of clients are from the city of Portsmouth, nearly one-third travel from other areas.[6] According to self-reported survey data, most users inject heroin, although those reporting having injected fentanyl has been increasing over time, which reflect trends in the general US population. Of those visiting the SEP, one-fifth have been diagnosed with hepatitis C, and 1 percent have been diagnosed with HIV. Despite the

[5]The median age in Portsmouth is 36.8, and the city is 90.0 percent White (United States Census Bureau, 2016).

[6]See Figure A1 for a map of client zip codes. Almost all clients that disclose their zip code during a SEP visit report living in Portsmouth or West Portsmouth. However, some visitors travel from nearby zip codes in Ohio and Kentucky. The largest travel distance recorded is 250 miles, with nearly 200 records of visitors traveling over 100 miles to the SEP.

fact that SEPs offer drug counseling and referrals to substance treatment facilities, only 1 percent of clients in Portsmouth accepted a referral during the sample period, suggesting that clients are either not interested in treating their addiction, have little resources to afford medical care, and/or that referrals are not the main focus of SEP facilities.

3 Empirical Approach

In this section I provide a detailed description of the data used in my analysis as well as strategies for estimating the causal effects of SEPs.

3.1 Data

Data on SEP locations as of 2017 is from the North American Syringe Exchange Network (NASEN), a non-profit organization that previously maintained a directory of SEPs by state as a public health information resource.[7] In particular, these data contain the name and address of the program, as well as contact information, when available. To gather data on the timing of SEP openings, I used these listings to hand collect information on program dates by recording open dates listed on the NASEN website, researching the history of individual programs when they provided a website, contacting listed representatives for programs, and comparing yearly coverage maps of United States syringe service programs provided by the Foundation for AIDS Research (AMFAR).[8] I then geocoded each clinic location to identify which counties were offering SEP programs before 2008, and those that experienced openings in the following 8 years, which serve as the treatment group for this analysis. In doing so, I identified 95 SEP openings in 91 counties between 2008–2016. Figure A2 depicts US counties identified as having, versus not having, SEPs by 2016 using this approach.

One shortcoming of these data is that if a clinic opened and closed before 2017, and is not uniquely identifiable from the city-level AMFAR maps, I do not observe that location in the data.[9] Moreover, since there are programs that have not authorized NASEN to release their records due to differences in state law that may provide partial funding to SEPs, allow their operation, or prohibit them altogether, my data do not capture any programs that are working without an address, undetected, and/or in defiance with state law.[10] I note that if I am not able to observe other SEP openings, this limitation in the data will bias the

[7] These data were available only for a 2–3 years before they were removed from the website and NASEN does not release these data upon request. Therefore, previous and more current directories are unavailable.

[8] Excellent research assistance, provided by Katherine Wells, was highly valuable in this venture.

[9] Although data on closures is unavailable, the net increase in SEPs and client caseloads over time combined with a web search of news articles of existing SEPs suggests that very few programs shuttered from 2008–2016.

[10] There is no official national registry of SEPs in the United States, and some do not disclose their operational to NASEN publicly to avoid shut down. Seventeen states prohibit syringe distribution (AL, AZ, AR, FL, GA, ID, KS, MS, MO, NE, ND,

estimates towards zero.

I use data on HIV cases and drug overdose deaths from two main administrative datasets. To measure the effect of a SEP opening on county-level HIV diagnoses, I use data from the Center for Disease Control and Prevention's NCHHSTP Atlas, which is the only comprehensive source of annual, county-level sexually transmitted infection data to date. The data include counts of HIV diagnoses per county of residence and are available only for 2008–2016.[11] The primary advantage of these data is that I am able to observe new HIV diagnoses instead of existing cases to analyze how SEPs change the spread of disease over time.

One limitation of the CDC Atlas is that HIV data for counties with less than 5 HIV cases or populations less than 100 are censored to ensure confidentiality of personally identifiable information. Because HIV is a relatively rare event, this results in suppression for approximately 75% of county-year observations. To improve the quality of the data and reduce the number of censored cells, I additionally include available data from each state's HIV Surveillance Program separately. Of the 50 states in which I requested data, 36 states provided uncensored data.[12] Combining these data with CDC population data, I construct HIV rates (the number of HIV cases per 100,000 individuals) for the analysis. Notably, county-level data on HIV diagnoses does not contain information on transmission. According to the CDC, transmission via injection drug use comprises nearly 10 percent of total HIV cases. Therefore, my estimates may understate the true effects of SEP on HIV spread through this method.

Data on drug- and opioid-related overdoses is from restricted-use CDC mortality files. These individual-level data contain information on county of residence, cause of death, as well as age, race, ethnicity, and education levels. To identify opioid-related deaths, I use the following ICD-10 multiple cause-of-death codes: X40-X44, X60-X64, X85, and Y11-Y14. For a more broad definition of drug-related deaths, I additionally include X45-X49, X66-X69, X85-X90, and Y15-Y19.[13]

While nearly all of the analysis focuses on HIV cases and drug-related deaths, I also consider effects on

OH, OK, PA, SD, TX, WV) with some exceptions for local laws (LawAtlas, 2017).

[11] HIV cases are classified as those with confirmed diagnoses of infection or infection classified as stage 3 (AIDS) in a given year.

[12] These states include Alabama, Arizona, California, Colorado, Connecticut, Florida, Hawaii, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maryland, Massachusetts, Michigan, Missouri, Montana, Minnesota, Mississippi, Nevada, New Hampshire, New York, North Carolina, Ohio, Oklahoma, Oregon, Pennsylvania, South Carolina, Tennessee, Texas, Utah, Vermont, Virginia, Washington, and Wisconsin. In instances where a state did not provide data, and the observation is censored, I assign the number of new counts to be zero, although I note that the results are not sensitive to this choice.

[13] In particular, codes X40-X44 represent accidental poisoning by and exposure to analgesics, antipyretics, and antirheumatics, antiepileptic, sedative-hypnotic, antiparkinsonism, and psychotropic drugs, narcotics and psychodysleptics [hallucinogens], and other unspecified drugs not elsewhere classified, X60-X64 accounts for intentional self-poisoning by and exposure to analgesics, antipyretics and antirheumatics, antiepileptic, sedative-hypnotic, antiparkinsonism and psychotropic drugs, narcotics and psychodysleptics [hallucinogens], and other and unspecified drugs not elsewhere classified, X85 is assault by drugs, medicaments and biological substances, and Y11-Y14 represent poisoning by and exposure to sedative-hypnotic, antiepileptic, antiparkinsonism psychotropic drugs, psychodysleptics [hallucinogens], narcotics, and other and unspecified drugs not elsewhere classified, undetermined intent. For a full description of ICD-10 codes, see `www.cdc.gov/nchs/data/dvs/im9_2002.pdf`. As suggested by Ruhm (2017), I adjust these data to include corrected counts of any opioid and heroin/synthetic opioid-involved drug death using T-codes 40.0-40.4 and 40.6 for opioids and T-codes 40.1, specifically, for heroin, although my results are not sensitive to this change.

opioid-related emergency room visits, in-patient stays, and drug-related crimes, to estimate more comprehensive effects of SEPs. Annual, state-level hospitalization data is from the Healthcare Cost and Utilization Program (HCUP) State Emergency Department Databases (SEDD) and State Inpatient Databases (SID). The SEDD contain discharge information on all emergency department visits that do not result in admission, while the SID contain information on patients initially seen in the emergency room then admitted to the hospital. HCUP data is not available for every state, and I therefore drop some states in these analyses. Although I am unable to compare hospitalizations across counties, these data allow me to investigate the effects of SEPs on drug-related hospitalizations, which likely serve as a proxy for drug use. In doing so, I am able to observe more comprehensive effects of SEPs in the event that programs increase drug use but do not result in death.

Drug-related crime data is from the FBI Uniform Crime Reports (UCR), which represent an annual compilation of crime statistics reported by local law-enforcement agencies that cover 95 percent of the US population. I focus on the UCR county-level dataset, comprised only of reported crimes that ended in arrest. In particular, I limit my analysis to drug arrests, including “sale or manufacturing of opium, coke and their derivatives," possession of drugs, and theft. Although theft is not denoted as a drug-related crime in these data, I include this outcome in my analysis due to the fact that recent research suggests that theft becomes more common when opioid use increases (Doleac and Mukherjee, 2018).

Using data in conjunction with population counts from the National Cancer Institute’s Surveillance, Epidemiology, and End Results Program (SEER), I construct rates of all drug-related outcome variables for my analysis. I additionally include population counts SEER to construct county-level measures of demographics, including the fraction of the county population that are black and the fraction Hispanic. To control for economic conditions over time, I use data from the Bureau of Labor Statistics on county-level unemployment rates and poverty rates. Finally, I construct several policy indicator variables using data from Meara, Horwitz, Powell, McClelland, Zhou, O’Malley, and Morden (2016) to help capture the broader policy environment surrounding opioid access in a given state and year. Specifically, these policy controls are state-by-year indicator variables that account for good Samaritan laws, opioid prescription limits, prescription drug monitoring programs, and other requirements to prevent illicit opioid-seeking behavior including tamper resistant prescription forms, ID requirements, pharmacist verification, and laws to prevent “doctor shopping". I also control for paraphernalia laws, in which a state bans drug paraphernalia with no exceptions related to syringes or SEPs, using data from the LawAtlas Policy Surveillance Program, as well as Naloxone Access Laws, using information on state policy changes from Doleac and Mukherjee (2018).

Summary statistics for variables used in the county-level analysis are shown in Table 1. In Column 1, I show means for counties that experienced SEP openings from 2008–2016, while in Column 2 I show means for counties with existing SEPs. Means for all HIV cases, opioid-related deaths and hospitalization are all larger for the treatment counties; however drug-related crime is lower in these areas, suggesting that SEPs are more likely to exist in areas with more drug use *and* more legal leniency.

3.2 Identification Strategies

My primary approach for estimating the effects of SEPs is a difference-in-differences design that compares counties with a SEP opening from 2008–2016 to other US counties that contain a SEP but did not experience an opening, although below I provide evidence that my results are robust to other various comparison groups. The identifying assumption underlying this approach is that the proportional changes in health and crime outcomes in the comparison counties provide a good counterfactual for the proportional changes that would have been observed in the treated counties in the absence of the SEP.

In particular, the main results are based on OLS models of the following form:

$$y_{ct} = \alpha_c + \alpha_t + \beta X_{ct} + \sum_{k=0}^{3+} \theta_k SEP_{c,t-k} + u_{ct} \tag{1}$$

where y_{ct} is the HIV rate, drug- or opioid-related mortality rate, or crime rate in a county c in year t, $SEP_{c,t-k}$ is an indicator variable that takes a value of one for counties with a SEP opening from 2009–2016 k years after the first SEP opening during the sample period and zero otherwise, where 3+ includes all years greater than or equal to three years after the opening, α_c are county fixed effects to control for any systematic differences across counties, α_t are year fixed effects to control for shocks to health and crime outcomes that are common to all counties in a year, and X_{ct} can include time-varying county control variables, county-level demographic controls, and state-level policy controls.

Importantly, I include only counties that have an opening between 2009-2016 in the treatment group, as a way to ensure that all treated counties contain at least one year of pre-period data to test for diverging pre-trends. In some specifications, I additionally show results from OLS models that include indicator variables for treated counties prior to the SEP opening to verify that mortality rates and HIV rates did not deviate from expected levels relative to other US counties with SEPs in the years before the clinic opening, which would otherwise cast doubt on the notion that the latter provide a good comparison group. All analyses allow errors to be correlated within counties over time when constructing standard-error estimates.

Some outcome variables, such as opioid-related emergency room visits and in-patient stays, are only available at the state level. Therefore, when analyzing these outcomes, I estimate analogues to Equation 1, collapsing my dataset to a state-by-year panel. In doing so, I define the treatment year to be the year in which the first SEP opens during the sample period, and compare changes in health outcomes in states with a SEP opening to changes in health outcomes in other US states.

Given the discrete nature of HIV cases and overdose deaths, and because I sometimes have county-year cells with zeroes, I additionally show estimates from a Poisson model, as well as models that convert outcome variables using an Inverse Hyperbolic Sine transformation.[14] I also present weighted-least squares estimates, noting that this model is comparable to a Poisson model when estimating the natural log of y_{ct} while controlling for the county-level population and restricting its coefficient to be equal to one. Moreover, I show results from models that partial out pre-treatment trends from the full panel, in an effort to construct outcome variables that are robust to county-specific linear trends.

I allow the estimated effects to vary across years with a set of indicator variables rather than considering the coefficient on a single "post-treatment" indicator for two main reasons. First, the nature of disease and addiction would suggest that any effect on the spread of HIV and/or mortality could appear some time after the program's implementation. Second, I may expect that as a SEP gains clients and notoriety over time, effects accumulate as drug users continue to use SEPs and share fewer needles, leading to both a lower propensity for disease to spread and fewer individuals that have contracted the disease. Lagged indicator variables therefore will allow us to trace the dynamic pattern of the spread of illness and drug use as a result of the opening of a SEP. Nevertheless, I also present average estimates for all specifications as well as joint significance tests for these lagged estimates to get a more general sense of the post-period effects.

4 Main Results

4.1 HIV Diagnoses

To show the effects of SEP openings on HIV rates, I first present graphical analyses that correspond to the preferred difference-in-differences identification strategy. Figure 3 plots the difference-in-differences coefficient estimates and their corresponding 95% confidence intervals from Equation 1, comparing HIV rates in counties with a SEP opening to other US counties with an existing SEP. Since every treated county has at least one year of data before the SEP opening in my sample, I estimate effects relative to the year before treatment, $t = -1$. Points left of the vertical line indicate the differences in treatment and control counties prior to the introduction of a SEP. Notably, these estimates are statistically indistinguishable from zero, providing some evidence to support the notion that trends in HIV rates were not diverging in the years before treatment, i.e. that counties with existing SEPs provide a good counterfactual for counties with SEP openings. Figure 3 also provides initial evidence that the HIV rate in counties with SEP openings decreased

[14] This transformation takes on the form $sinh_z^{-1} = ln(z + \sqrt{1 + z^2})$

relative to other counties following an opening, indicating that SEPs achieve their intended goal of reducing bloodborne illness.[15]

In Table 2, I provide model-based estimates from Equation 1 as well as the mean HIV rate to inform the magnitude of the results. Column 1 shows the estimated effects from a baseline model which controls for year and county fixed effects. Estimates indicate that the introduction of a SEP reduced HIV diagnoses rates by 25.0 percent. In Column 2, I present estimates after adding demographic and economic controls. Estimates are statistically similar to the ones in Column 1, and indicate a reduction of 25.5 percent. Column 3 addresses the fact that other state-level policies affecting access to opioid prescriptions and the legal climate of drug paraphernalia changed during the sample period, 2008–2016, which could bias the results. To account for these changes, I control for time-varying indicator variables for states with prescription limits, tamper resistant prescription forms, ID requirements, prescription drug monitoring programs, good Samaritan laws, paraphernalia laws, and other physician requirements, including required verification, and exams. These estimates are smaller than those in Column 1, implying that states with higher HIV rates are more likely to implement opioid-related policies. Nonetheless, estimates indicate a reduction in HIV diagnoses of 24.5 percent, corresponding to 30 fewer HIV cases in each county per year.[16]

To get a sense of how these estimates change over time, in Columns 4–6 I show estimates from a set of lagged indicator variables. Additionally, I calculate the average effects of these lagged estimates, and provide a joint test of significance in each column. I find no evidence that SEPs reduce HIV rates in their first year. However, estimates across years 2–4 imply that an SEP opening led to a reduction of HIV diagnoses by 24.8 percent in its second year and 34.3 percent in its third year. Effects in years 4–8 are similarly negative and relatively larger in magnitude, but suffer from a lack of precision. Overall, these findings motivate the idea that the benefits of SEPs, as seen by reductions in HIV rates, accumulate over time, and imply that both drug users and other individuals that come into contact with needles on the street or elsewhere benefit from providing sterile supplies and safe disposal of used needles.

Importantly, in my above analysis, I provide estimates for HIV diagnoses based on CDC data and data from state and local governments. One main limitation of these data is that while drug overdoses have been increasing and HIV cases related to injection drug use has been decreasing (i.e. Figure 2), the county-level data available for HIV diagnoses does not distinguish by cause of infection. Because of this, the causal effect of the opening of an SEP on the rate of new HIV cases may be difficult to isolate in areas where sexual transmission of HIV accounts for the majority of new cases. Moreover, the opening of a SEP may result

[15]For a figure of difference-in-differences estimates using a longer panel of pre-period data, comparing counties with SEP openings from 2008–2016 to those with existing SEPs as of 2008, see Figure A3. I note that since HIV data from the CDC and many state agencies is not available prior to 2008, these estimates should be taken with caution.

[16]This is based on the fact that the county-level average of new HIV cases per year in counties with a SEP is approximately 24.39, and the average population is 505,847.

in more individuals getting tested for HIV. This would imply that the estimates will understate the true reduction in HIV rates as a result of the SEP.

4.2 Drug-Related Mortality

The evidence presented above indicates that SEPs achieve their intended goal of reducing bloodborne illness. If SEPs provide also drug counseling and resources for injection drug users to seek treatment, such programs could discourage drug use and facilitate recovery. However, three arguments support the notion that SEPs simultaneously create adverse effects, leading to higher death rates from overdose. First, programs distribute free supplies, including needles, sharps containers, and personal hygiene items, which lowers the expected cost of using injection drugs. Second, SEPs provide a safe space to interact with other users, increasing networking opportunities and reducing stigma. Third, communities that build a SEP may attract nearby drug users and/or signal that they also support more police leniency for drug users, lowering the legal risk of using opioids. Below, I test to what extent opening a SEP affects drug- and opioid-related mortality and present evidence consistent with the presence of moral hazard.

I first present a graphical analysis of the effects of SEPs over time. Figure 3 plots the difference-in-differences coefficient estimates from Equation 1, comparing changes in mortality in counties with a SEP opening to changes in mortality in counties with an existing SEP. Prior to the introduction of a SEP, estimates are all statistically similar to zero, indicating that mortality trends in each group were not diverging prior to the program opening. In the first two years of the SEP, estimates indicate an increase in opioid-related mortality of 14.8–15.6 percent, corresponding to approximately 13 additional deaths per county per year, or 24,402 nationwide.

In Table 3, I display point estimates from Equation 1 and show both average and lagged effects over all post years after the opening of a SEP. Due to the fact that opioid-related mortality may be difficult to classify than a more generic diagnoses of drug poisoning, and/or may be more likely to be detected over time, I additionally show effects for all drug-related mortality. Similar to Figure 3, estimates in Columns 1–3 that opioid-related mortality increased by 13.5–15.3 percent following the introduction of a SEP, with effects concentrated in the first two years of the program's implementation. Specifically, opioid-related mortality increases by 15.9 in the first year and 17.2 in the second year. Effects in later years are statistically insignificant, implying that SEPs increase drug use at the intensive margin for the most immediate clients, which may be those with a higher probability of overdose.

When measuring effects on drug-related mortality more broadly, estimates likewise indicate an increase of 12.2–15.0 percent. These effects correspond to 13 additional drug-related deaths per county over the two years following a SEP opening, providing support for two stark conclusions: (i) SEPs lead to greater risk of opioid misuse and overdose and/or (ii) the increase in mortality rate among injection drug users simultaneously reduces the probability that these users will spread HIV through needle sharing in the future. In Section 5 I present evidence that effects on drug-related mortality are concentrated in different areas than the effects on HIV rates, suggesting that the former is more likely to be driving the results than the latter.

4.3 Emergency Room Visits and In-Patient Stays

Despite the fact that data on drug-related mortality is able to capture one measure of how much SEPs can affect opioid overdose, the above effects may not be picking up drug *usage* if users are injecting more frequently but not at fatal doses. To explore the more comprehensive effects of SEPs on drug use, I use data on drug-related emergency room visits and in-patient stays from the HCUP dataset. One limitation of these publicly available data are that they are only available at the state level, which does not allow for a more granular, county-level analysis.[17]

Using the difference-in-differences approach described above, in this state-level analysis I assign the year of treatment to the first SEP clinic opening year between 2009–2016 in a given state. In Figure 4 and Table 4, I provide estimates showing the effects of the opening of a SEP in a state on the rate of opioid-related emergency department (ED) admissions and in-patient stays. ED visits are likely to serve as a proxy for drug use, as these data pick up drug-related overdoses that are easily reversed and result in less than a 24-hour stay. In-patient data, on the other hand, reflect more high-risk cases requiring the care of a doctor, and may more clearly track patients with a longer history of drug abuse.

Across Table 4 Columns 1–3 estimates mirror those of Figure 4 and indicate that the introduction of a SEP increses drug-related emergency room admission and in-patient stays by approximately 8.9 percent and 3.4 percent, respectively, although estimates in Column 3 are statistically insignificant at conventional levels. Estimates in Columns 4–6 largely reinforce these findings; I estimate that SEPs increase emergency room admissions by 18.8 percent, on average, with effects driven by the second through fourth years, corresponding to 1,569 additional ED visits per year. Unlike effects for mortality, which fade after two years, effects for ED visits grow over time, from 4.5 percent in first year to 35.6 percent over three years later.

Although estimates for lagged indicator variables are positive and statistically significant for in-patient stays in Columns 4 and 5, indicating an increase in opioid-related hospital stays ranging from 4.6–13.3 percent, estimates in Column 6 are positive and statistically insignificant. Taken together, these findings suggest that while SEPs are successful in reducing disease, lowering the cost of obtaining clean needles and

[17]HCUP does not contain data on every state. In particular, I drop Colorado, Louisiana, Michigan, New Mexico, Oregon, Pennsylvania, Texas, Washington, and West Virginia for this analysis.

other supplies unintentionally encourages more drug use, leading to more opioid-related overdoses. These effects become more pronounced over time, indicating that any future cost-benefit analyses of SEPs should consider effects at least 2–4 years after the introduction of the program.

The results from this analysis provide evidence for two arguments. First, unlike effects for mortality, which seem to fade after two years, effects for ED visits grow over time, suggesting that SEPs are unsuccessful in discouraging drug use. Second, to the extent that SEPs connect users to life-saving technology, such as naloxone, or introduce ways to recognize overdose and encourage calling for help, then any increase in emergency room visits may represent a reduction in opioid-related deaths that would have occurred otherwise. Therefore, my results provide some evidence that SEPs help the marginal client from fatal overdose, but are unable to reverse addiction. Importantly, the primary goal of SEPs is to provide clean supplies to injection drug users in a safe environment with the intent of reducing needle sharing, while drug counseling and treatment referral are secondary services. Given the aims of harm reduction services, it is perhaps unsurprising that SEPs are more effective at reducing bloodborne illness than reducing opioid dependence.

5 Subgroup Analysis

Given the abundance of anecdotal and empirical evidence that the opioid crisis has largely affected younger White males in rural and low-income areas, one would reasonably expect the effects of SEPs to be largest in counties with small, mostly White populations and those with a relatively large share of low-income individuals. In the following section, I explore how SEP openings differentially affected groups across counties to provide clarity on the heterogenous effects of SEPs.

5.1 Effects Across Counties

In Table 5, I consider to what extent SEPs affect various county subgroups. In Column 1, I replicate estimates from my preferred specification for a baseline comparison. In Columns 2–5 I split the sample by urbanicity and poverty levels.

When analyzing effects by urban and rural counties with SEPs in Table 5 Columns 2–3, I find a striking result–effects for HIV rates are largely driven by urban counties, while effects for opioid-related mortality are concentrated in rural counties. One reason for this may be that bloodborne illness is able to more rapidly spread in cities, due to high population density. Moreover, SEPs in urban areas may be able to provide more references to treatment facilities and/or counseling, leading to fewer instances of drug overdose. Rural areas, on the other hand, which are well-known to be differentially affected by the recent opioid crisis, have relatively little substance abuse treatment available.[18] Although it is potentially more difficult in these areas to contract HIV through needle sharing, due to the lack of conviently located partners, distance to a hospital and/or other facilities that can revive individuals when overdosing may be a major barrier in preventing fatal overdoses.[19] To get a better sense of which counties may be driving this effect, I revisit and expand on this finding in the next section.

In Columns 4 and 5 I separately estimate effects on low- and high-poverty counties. I define high-poverty counties as those having more than the county-level median poverty rate and define low-poverty counties as those having rates below this median.[20] Estimates for both HIV rates and opioid-related mortality rates are statistically significant only for high-poverty areas, and indicate that SEPs decrease HIV rates by 28.8 percent and increase opioid-related mortality rates by 19.2 percent, implying that SEPs are more likely to affect those that have financial barriers to treatment.

5.2 Effects Across US Region

Due to the fact that the opioid crisis has not affected all states and US regions uniformly, I now turn to a discussion on how SEPs affect health outcomes by region.[21] In Table 6 I separately analyze effects of SEPs on HIV rates and opioid-related mortality rates by US Census regions.[22] Additionally, I show estimates from a separate, non-mutually exclusive region, which includes the top ten states most affected by the crisis, as defined by the CDC.[23]

Across all columns, the sign of the estimates mirror the findings in Tables 2 and 3. Reductions in HIV rates are most concentrated in Western counties, while increases in mortality are largest in Southern counties as well as those counties in "top 10" states in Appalachia and the eastern seaboard most affected by the crisis. I note that although I do not estimate statistically significant effects, on average, for Midwest counties, when estimating effects separately by year, I find statistically significant and large reductions in HIV rates ranging from 41.4–59.7 percent 1–2 years after the introduction of a SEP.

Taken with the results in Table 5, these findings imply that SEPs have differential effects that largely depend on the size of the population and area of the country in which they are located. Importantly, these results have singificant policy implications given that state and local policies may play a role in shaping the

[18] The average number of substance abuse treatment facilities in large urban, medium/small urban and rural counties is 122 and 20, respectively. By comparison, rural areas have only 2 facilities, on average.

[19] That being said, estimates for HIV rates in rural counties still indicate reductions in illness ($p = 0.12$), though the estimate is much smaller than the corresponding estimate in Column 2.

[20] Specifically, the median poverty rate across counties with SEPs is 16.1.

[21] See Ruhm (2017) for a more thorough discussion on geographical variation in drug- and opioid-related mortality.

[22] Notably, for this analysis, I have 23 treated counties in the Midwest, 39 in the South, 21 in the Northeast, and 30 in the West. See `https://www.census.gov/geo/reference/webatlas/regions.html` for a list of states in each region.

[23] In order of severity, these states are West Virginia, Ohio, Pennsylvania, District of Columbia, New Hampshire, Kentucky, Maine, Connecticut, Delaware, and Massachusetts.

effectiveness of SEPs.

5.3 Effects Across Age, Race, and Gender

Thus far I have shown that the introduction of a SEP reduces cases of HIV while generating moral hazard effects, on average. However, I note that average effects may be masking effects that differ by demographics, as motivated by Figure 2. Below, I explore effects by age, race, and gender in an attempt to understand who is most likely to be affected by SEPs.

In Table 7, I present estimates on the effects of SEPs on opioid-related mortality for individuals in their 20s, 30s, 40s, and 50s, respectively. Estimates are driven by individuals aged 30–49 and range from 3.8–7.0 percent. I find no evidence that SEPs increase mortality for those in their 20s, potentially due to the fact that the average SEP client is 38 years old, and/or that younger individuals are less likely to die from a drug overdose.

Table 8 additionally shows effects separate by race. Effects are entirely concentrated among White individuals, which is well-known to be a group most affected by the opioid crisis (Hollingsworth, Ruhm, and Simon, 2017; Case and Deaton, 2017). Similar to my main results, estimates indicate an increase in White opioid-related mortality rates by 3.1–10.7 percent, which corresponds to 1,175 additional deaths per year across the US. Effects are concentrated in the first two years of the program and become statistically indistinguishable from zero in the third year.

In Table 9 I also display state-level estimates for hospital visits by gender and age subgroups. Estimates for males are statistically similar to the average effect, and indicate that the results are not solely driven by male risky behavior. When observing differences across age, effects are 2–3 times larger for individuals aged 25–44, with no changes in ED visits for those aged 45–64.

5.4 Drug-Related Crime

If SEPs unintentionally encourage drug usage, as suggested by the aforementioned findings, then one would expect drug-related crime to also increase. However, if the introduction of a SEP also signifies increasing attitudes of legal leniency towards opioid use, we would expect drug-related crimes to decrease after a SEP opening. In this section, I analyze effects on drug arrests including possession, sale, and theft to get a sense of how drug use and/or local attitudes towards drug use respond to the opening of a SEP.

In Figure 5 and Table 10, I show estimates for various drug-related crimes. "Total Drug Abuse Crimes" refers to a more comprehensive measure of drug crime including possession, sale, and/or manufacturing of drugs of any kind. Across Columns 1–6, I present evidence that the introduction of a SEP increases total drug crime arrests by 15.6–16.4 percent, on average, including opioid-related possession and theft. These estimates correspond to approximately 650 more crimes per county per year. Estimates are largest after the initial year of the program and increase over time, following trends in drug use reported in the previous section. Notably, I find no effects on arrests for drug sales, which could indicate that as drug use increases, police target users rather than sellers due to time and/or resource constraints.

Finally, I analyze effects on theft, following recent research suggesting that as opioid use increases, abusers may become criminally active to fund their addiction. (Doleac and Mukherjee, 2018). I find modest evidence that theft increases by 23.8 percent after the opening of a SEP, and these effects are driven by increases one year after the program opening.

These findings suggest that SEPs do not result in greater leniency for drug users. While it is possible that SEPs subsequently increase police monitoring due to heightened saliency of local public health issues, my findings on drug-related mortality and hospital admissions indicate that opioid use increases after a SEP opening, and the proportion of drug-related arrests also rises. Below, I show additional evidence that this increase in drug-related crime is not mirrored by an increase in other criminal or risk-taking behavior, implying that these effects are not simply a byproduct of compositional changes in population.

6 Robustness

In this section I present a set of sensitivity checks to assess the validity of my estimates and provide additional support for the identification assumption. First, I present statistical evidence that trends in health and crime outcomes were not diverging in the year prior to treatment. In Table 11 I display lagged estimates from Equation 1 for HIV rates, opioid-related mortality rates, emergency room admissions, and in-patient stays. In Columns 2, 4, 6, and 8, I include a one-year leading indicator variable to directly test the divergence in trends in counties with a SEP opening to other counties with a SEP prior to the introduction of the program.[24] Columns 1, 3, 5, and 7 replicate the baseline results from my preferred specification for comparison. Across Columns 1–6, estimates remain similar when including the lead term and do not indicate that trends were diverging in the year prior to treatment. In Column 8, however, estimates indicate in-patient stays for opioid-related overdose were increasing prior to the introduction of a SEP ($p = 0.096$). This implies that my difference-in-differences model may be misspecified in this context, and any interpretation of this state-level analysis should be taken with caution.

Second, I test how robust my analyses are to functional form. To do so, I provide weighted least squares

[24]Similarly, I have considered models which additionally control for county-specific linear trends. Estimates are statistically similar to the main results, albeit larger and slightly less precise, and indicate a 38.3 percent reduction in HIV rates and a 14.9 percent increase in opioid-related mortality.

and Poisson estimates in Table 12. Notably, these alternative estimates are less precise and smaller in magnitude than the OLS estimates.[25] As described in Solon, Haider, and Wooldridge (2015), this pattern can reflect circumstances in which there are relatively large effects for counties which maintain small populations. Given that I show large effects for opioid-related mortality in rural counties in Table 5, these estimates remain consistent with my previous findings. However, I note that the WLS estimates for HIV diagnoses are smaller and more imprecise than the OLS estimates, despite the fact that I find larger effects in urban areas.[26]

I further explore this heterogeneity in population size directly Table 13, which displays estimates for a more binary measure of population size—counties with less/more than 200,000 people, and in Figure A4, which presents estimates by a more continuous measure of county-level population.[27] Mirroring findings in Columns 5 and 6 of Table 5, I note that these estimates do provide some suggestive evidence that estimates for mortality are comparatively large for less-populated counties, which may demonstrate why the OLS estimates are relatively large in magnitude as compared to the WLS estimates. Specifically, Figure A4 shows that estimates for opioid-related mortality rates are positive and statistically significant across counties with populations ranging from 0–2,000,000, while effects for HIV rates are similar across county size, but are larger and more precise when estimating effects for larger counties with over 150,000 individuals.

Additionally, since the opening of a SEP is not random, in my preferred specifications I account for a number of demographic, economic, and policy variables that are likely correlated with a county's decision to initiate a needle exchange program. However, if the addition of these time-varying determinants of HIV rates and mortality rates meaningfully change the main findings, this would suggest that health outcomes in the treatment and comparison groups might have diverged even in the absence of a SEP. To show that these factors are orthogonal to the within-county variation in SEP openings, in Table A3 I estimate the effects of SEP openings on HIV rates and opioid-related mortality rates, adding in controls for relevant state legal restrictions and drug-related laws in separate specifications. Importantly, estimates are similar across Columns 2–10, suggesting that no particular policy changes are driving the main results.[28]

Third, I consider estimates based on alternative difference-in-differences identification strategies to explore the extent to which comparing effects across counties with SEPs yields larger (or smaller) estimates. Table

[25]I also show estimates from an OLS model which estimates effects on the inverse hyperbolic sine transformation of HIV diagnoses and opioid-related deaths in Table A2. Estimates for HIV rates are similar to those in Table 2, and indicate an reduction in HIV cases by 4.3 percent. Estimates for opioid-related mortality are positive, but statistically insignificant across all columns, which supports the idea that counties with large increases in mortality may be driving the main results.

[26]I hypothesize that this could be due to the introduction of more noise, and note that estimates are similarly negative, albeit imprecise.

[27]I choose this cutoff for two reasons. First, the median population is 191,972 and second, in Table 5 I find larger effects for mortality in rural areas.

[28]Data on state policy variables, including laws governing patients, prescribers, or dispensing pharmacists that involve quantitative prescription limits, patient identification requirements, requirements with respect to physician examination or pharmacist verification, doctor-shopping restrictions, PDMPs, requirements related to tamper-resistant prescription forms, and pain-clinic regulations is from Meara, Horwitz, Powell, McClelland, Zhou, O'Malley, and Morden (2016). Data on Naloxone access laws is from Doleac and Mukherjee (2018).

14 presents estimates based on models using various comparison groups. In Column 1, I provide my main results shown in Table 2 Column 3 for comparison. In Columns 2–5 I compare counties with SEP openings in the last 10 years to other US counties without an existing SEP, all other US counties, all counties in states that permit SEPs, and counties bordering these treated counties, respectively. Indeed, the coefficient estimates across all columns are similar to the baseline results, although I note that estimates for HIV rates are less precise, likely due to the fact that many US counties have few to no new diagnoses of HIV in any given year. Moreover, estimates for mortality rates are similarly positive, albeit larger then more conservative estimates from the preferred specification.

Because different types of counties may have adopted SEPs at different times, I similarly test the extent to which earlier adopters differ from later adopters and present these estimates in Table A7. Column 1 displays the baseline results, while Columns 2 and 3 separately show effects for counties with SEP openings between 2008–2012 and 2013–2016 separately. Splitting the sample yields less precise estimates; however, for both HIV rates and opioid-related mortality rates, effects are largest for the later adopters. This suggests that counties that opened SEPs at the height of the opioid crisis may have had either more clients and/or clients using injection drugs at higher frequencies.

Fourth, I investigate how much the effects of SEPs vary with state-level policies that affect access to opioid prescriptions and drug treatment facilities. In Table A4, I separately estimate the effects for states that did and did not expand Medicaid eligibility by 2015, according to Simon, Soni, and Cawley (2017). It is unclear whether Medicaid would help to reduce opioid misuse or exacerbate it. For example, expansions Medicaid eligibility increase access to drug treatment facilities for low-income individuals by eliminating financial barriers, which we would expect to reduce opioid-related overdoses. However, Medicaid could also increase opioid-related mortality via increased access to low-cost prescription opioids. In Columns 2–3 of Table A4, I find support for the latter argument. Notably, effects are larger in Medicaid expansion states for both HIV rates and opioid-related mortality, although I also note that nearly all treated counties (83%) are located in Medicaid expansion states.

Fifth, because one may be concerned that the effects I report for opioid-related mortality may be a result of population composition changes due to a SEP opening, in Figure 6 I investigate whether the treatment counties simultaneously experience increases in mortality rates from other causes. In particular, I analyze whether the introduction of a SEP in a county affects vehicle deaths and alcohol deaths.[29] I find no significant effects of SEP openings on either of these other mortality rates, implying that SEPs more directly affect

[29]I acknowledge that both could be affected by increases in injection drug use. However, since these outcomes rank in the top 10 reasons for death in the United States, and are more likely to pick up compositional changes for adults aged 18–60, or changes in risky behavior.

outcomes related to drug use.[30]

In this same vein, I have also considered that my findings may simply be driven by overall improvements in healthy living which coincide with the opening of a SEP. To test this hypothesis, in Table A5 I additionally analyze whether SEPs affect two other sexually transmitted infections (STIs) that are not contracted through needle sharing: chlamydia and gonorrhea. I find no that rates of other STIs decrease after the introduction of a SEP, although effects are relatively imprecise. These findings provide weak evidence that SEPs do not result in individuals engaging in more risky sexual behavior.[31]

Finally, in Table A6 I show estimates from a model analogous to Equation 1 that partials out pre-treatment trends, as suggested by Goodman-Bacon (2018). To do so, I calculate residuals from a regression of demeaned variables for all counties and all years and then estimate Equation 1 using these residualized variables to avoid any bias resulting from estimating group specific trends off the full set of data. Estimates for HIV rates are statistically insignificant across all columns and are relatively imprecise. Since controlling for pre-treatment trends increases the weight on units treated near the middle of the sample period, these findings may be reflective of the fact that HIV rates take at least two years to respond a SEP opening. Alternatively, controlling for pre-treatment trends yields larger estimates for opioid-related mortality, suggesting that trends in counties with new SEPs followed a different trajectory after the introduction of a program.

7 Conclusion

In this paper, I document the effects of expanding access to clean needles and opioid-related counseling through syringe exchange programs. Using county-level data on HIV cases, drug overdoses, hospitalizations, and drug-related crimes, I compare health outcomes in counties that experienced a SEP opening from 2008–2016 to counties that had an existing program. Consistent with the previous literature, I find that syringe exchange programs reduce HIV diagnoses by 11.3–30.0 percent, or nearly 30 fewer cases of HIV per year. However, I present new evidence that the SEPs produce unintended consequences. In particular, I find that a SEP opening corresponds to an average increase in opioid-related mortality by 13.5 percent, and opioid-related ED visits by 8.9 percent, and that these effects grow over time. Overall, these estimates correspond to 26 more drug-related deaths per county, or 5,500 additional deaths and 1,050 opioid-related ED visits over two years. Effects are concentrated in high poverty areas, suggesting that low-income individuals living in

[30] Similarly, I find no statistically significant average or lagged effects of SEP openings on the total mortality rate reported in a given county.

[31] Ideally, this analysis would also be able to speak to how SEPs affect rates of hepatitis C, another bloodborne illness that can be contracted through needle sharing. However, county-level data for hepatitis C are unavailable. When I estimate how SEPs affect state-level diagnoses of hepatitis C, estimates on the effects of SEPs on hepatitis C rates are small and close to zero, and I can rule out reductions greater than 0.03 percent. This is consistent with previous work suggesting that SEPs mostly address the spread of disease through the channel of reducing HIV and are ineffective at reducing hepatitis C (Pollack, 2001).

areas with fewer health care resources may face larger hurdles in obtaining drug counseling and/or substance abuse treatment.

I note that my findings imply that SEPs do little to reduce drug overdoses, and may even exacerbate opioid abuse and misuse. However, the results do not suggest that SEPs are ineffective at curbing addiction for all clients. Furthermore, the stated goal of SEPs is to provide counseling and other resources for injection drug users while ensuring the safe disposal of used needles in an effort to reduce HIV. That being said, SEPs are successful in achieving this goal, which increases total social welfare. On the other hand, if SEPs perpetuate crime and drug use, the introduction of such programs generate large negative externalities that reduce total social welfare. Since HIV has increasingly become a manageable disease with available treatment, these effects imply the increases in opioid misuse and mortality largely outweigh the reduction in bloodborne illness in terms of societal costs.

Given the well-documented benefits of substance abuse treatment facilities, my findings suggest that providing funding for these clinics over SEPs may be a more fruitful avenue for reducing drug-related mortality and financially motivated crimes (Swensen, 2015; Bondurant, Lindo, and Swensen, 2018). Moreover, prescription drugs, such as Buprenorphine that reduce symptoms of opiate addiction and withdrawal, or other opiate antagonists, which work in the brain to prevent opiate effects and decreases the desire to take opiate, could be one way for SEPs to mitigate clients' opioid dependence in the future. Policymakers and the public health community more broadly should be careful to consider all costs and benefits of SEPs, including long-run effects generated by lowering the costs of consuming injection drugs. In the wake of increased drug-related deaths and state policies to curb this epidemic, my estimates shed new light on how local policies can affect syringe sharing, drug overdose, and drug-related crime. Thus, it will become increasingly important for future research to determine the extent and scope of how expanding (or reducing) access to SEPs affects bloodborne illness and drug use more broadly.

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Table 1: Summary Statistics

	Treated Counties	Comparison Counties
Outcome Variables		
HIV Diagnoses Rate	35.56	21.00
Drug-Related Mortality Rate	21.49	17.23
Opioid-Related Mortality Rate	19.78	15.33
Opioid-Related Emergency Department Admissions Rate	177.93	172.45
Opioid-Related In-Patient Hospital Visit Rate	250.07	229.91
Total Drug Crimes Arrest Rate	317.48	421.26
Opioid-Related Drug Sale Arrest Rate	23.77	30.02
Opioid-Related Drug Possession Arrest Rate	0.56	0.96
Theft Rate	23.76	27.91
County Characteristics		
Rural	0.42	0.43
Population	491853.38	777877.86
Percent Hispanic	0.11	0.13
Percent Black	0.11	0.09
Unemployment Rate	7.86	7.72
Percent Poverty Rate	17.57	14.58
State-Level Policy Variables		
Prescription Limit	0.99	0.99
Tamper Resistant Prescription Form	0.76	0.61
ID Requirement	0.31	0.33
Doctor Shopping Prohibition	0.23	0.27
Physician Exam Requirements	0.84	0.82
Pharmacist Verification	0.19	0.23
Prescription Drug Monitoring Program	0.88	0.70
Pain Clinic	0.06	0.01
Paraphernalia Laws	0.30	0.09
Good Samaritan Laws	0.31	0.37

Notes: Data on HIV diagnoses is from the CDC NCHHSTP Atlas and 36 state agencies. Drug-related deaths are based on the National Center for Health Statistics (NCHS), Division of Vital Statistics Mortality Files. Unemployment rates are from the BLS. Information on state-level policy changes is fromMeara, Horwitz, Powell, McClelland, Zhou, O'Malley, and Morden (2016). Column 1 shows the means for treated counties in the sample, i.e., counties with a syringe exchange program opening from 2009–2016. Column 2 displays the means for the comparison counties, i.e., other US counties with a syringe exchange program. Rates are calculated as cases per 100,000 individuals.

Table 2: The Effect of a Syringe Exchange Program on HIV Diagnoses Rates, Difference-in-Differences Estimates using Counties with Existing SEPs for Comparison

	(1)	(2)	(3)	(4)	(5)	(6)
Average Effect of SEP	-6.095**	-6.219**	-5.970**			
	(2.842)	(2.890)	(2.761)			
Effect of SEP in First Year				-2.899	-3.244	-2.578
				(2.296)	(2.475)	(2.436)
Effect of SEP in Second Year				-6.353**	-6.604**	-6.057**
				(2.848)	(3.036)	(3.027)
Effect of SEP in Third Year				-9.004**	-8.770**	-8.361**
				(4.001)	(3.965)	(3.902)
Effect of SEP in Fourth+ Year				-12.900	-12.288	-13.523
				(10.733)	(10.069)	(10.242)
Average Lagged Effect				-7.79	-7.73	-7.63
P-value (test average effect = 0)				0.05	0.04	0.04
Mean	24.39	24.39	24.39	24.39	24.39	24.39
Observations	1899	1899	1899	1899	1899	1899
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	No	Yes	Yes	No	Yes	Yes
State-Level Policy Controls	No	No	Yes	No	No	Yes

Notes: Estimates are based CDC and state agency data on HIV diagnoses counts by county for the entire United States from 2008–2016. Rates are calculated as cases per 100,000 individuals. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the county level.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 3: The Effect of a Syringe Exchange Program on Drug-Related Mortality Rates, Difference-in-Differences Estimates using Counties with Existing SEPs for Comparison

	(1)	(2)	(3)	(4)	(5)	(6)
Opioid-Related Mortality						
Average Effect of SEP	2.889**	2.872**	2.541**			
	(1.352)	(1.311)	(1.245)			
Effect of SEP in First Year				3.327**	3.316**	2.995**
				(1.602)	(1.556)	(1.489)
Effect of SEP in Second Year				3.639*	3.611*	3.231*
				(1.948)	(1.912)	(1.846)
Effect of SEP in Third Year				2.753	2.835	2.495
				(1.724)	(1.717)	(1.691)
Effect of SEP in Fourth+ Year				0.060	-0.039	-0.067
				(1.483)	(1.504)	(1.455)
Average Lagged Effect				2.44	2.43	2.16
P-value (test average effect = 0)				0.06	0.06	0.08
Mean	18.82	18.82	18.82	18.82	18.82	18.82
Observations	1899	1899	1899	1899	1899	1899
All Drug-Related Mortality						
Average Effect of SEP	2.926**	2.918**	2.570**			
	(1.370)	(1.329)	(1.264)			
Effect of SEP in First Year				3.430**	3.431**	3.094**
				(1.609)	(1.564)	(1.487)
Effect of SEP in Second Year				3.567*	3.553*	3.146*
				(1.973)	(1.937)	(1.876)
Effect of SEP in Third Year				2.394	2.471	2.104
				(1.754)	(1.748)	(1.723)
Effect of SEP in Fourth+ Year				0.559	0.479	0.426
				(1.531)	(1.537)	(1.488)
Average Lagged Effect				2.49	2.48	2.19
P-value (test average effect = 0)				0.06	0.06	0.08
Mean	20.99	20.99	20.99	20.99	20.99	20.99
Observations	1899	1899	1899	1899	1899	1899
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	No	Yes	Yes	No	Yes	Yes
State-Level Policy Controls	No	No	Yes	No	No	Yes

Notes: Estimates are based on NCHS restricted mortality files by county for the entire United States from 2008–2016. Rates are calculated as cases per 100,000 individuals. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the county level.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 4: The Effect of a Syringe Exchange Program on Opioid-Related Hospital Visits, Difference-in-Differences Estimates using States with Existing SEPs for Comparison

	(1)	(2)	(3)	(4)	(5)	(6)
Emergency Room Admission Rate						
Average Effect of SEP	23.822**	20.780**	13.486			
	(10.194)	(9.231)	(9.296)			
Effect of SEP in First Year				14.395*	11.362	6.825
				(8.054)	(7.644)	(7.910)
Effect of SEP in Second Year				31.636**	30.537***	21.865**
				(11.703)	(10.731)	(9.786)
Effect of SEP in Third Year				41.068**	38.058**	30.863**
				(16.497)	(15.984)	(14.327)
Effect of SEP in Fourth+ Year				65.822**	62.014**	53.646**
				(24.484)	(24.041)	(20.952)
Average Lagged Effect				38.23	35.49	28.30
P-value (test average effect = 0)				0.01	0.01	0.02
Mean	150.75	150.75	150.75	150.75	150.75	150.75
Observations	258	258	258	258	258	258
In-Patient Stay Rate						
Average Effect of SEP	15.053**	12.503*	7.022			
	(7.321)	(6.749)	(7.185)			
Effect of SEP in First Year				11.885**	9.676*	5.385
				(5.864)	(5.361)	(5.625)
Effect of SEP in Second Year				16.983*	14.612*	7.746
				(8.472)	(8.144)	(8.507)
Effect of SEP in Third Year				17.506*	14.684	9.679
				(10.221)	(9.735)	(10.022)
Effect of SEP in Fourth+ Year				27.871**	24.966**	18.597
				(13.051)	(11.971)	(11.807)
Average Lagged Effect				18.56	15.98	10.35
P-value (test average effect = 0)				0.02	0.04	0.20
Mean	209.26	209.26	209.26	209.26	209.26	209.26
Observations	375	375	375	375	375	375
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	No	Yes	Yes	No	Yes	Yes
State-Level Policy Controls	No	No	Yes	No	No	Yes

Notes: Estimates are based on state-level opioid-related emergency room visits from the Healthcare Cost Utilization Project for 2008-2016. Rates are calculated as cases per 100,000 individuals. Economic control variables include the state-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the state level.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 5: The Effect of a Syringe Exchange Program on HIV Diagnoses Rates and Opioid-Related Mortality Rates by Subgroup

	Counties W/ SEPs	**Urban Counties**	**Rural Counties**	**Low Pov. Counties**	**High Pov. Counties**
	(1)	(2)	(3)	(4)	(5)
HIV Rate					
Average Effect of SEP	-5.970**	-10.271**	-0.030	-2.304	-10.120*
	(2.761)	(4.474)	(0.019)	(1.755)	(5.222)
Mean	24.39	45.39	0.17	15.96	35.08
Observations	1899	1017	882	1062	837
Opioid-Related Mortality Rate					
Average Effect of SEP	2.541**	1.125	4.759**	-0.001	4.362*
	(1.245)	(1.624)	(2.006)	(0.995)	(2.268)
Mean	18.82	18.50	19.19	15.76	22.70
Observations	1899	1017	882	1062	837
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	Yes	Yes	Yes	Yes	Yes
State-Level Policy Controls	Yes	Yes	Yes	Yes	Yes

Notes: Estimates are based on NCHS restricted mortality files and CDC NCHHSTP Atlas and state agency HIV diagnoses counts by county for the entire United States from 2008–2016. Rates are calculated as cases per 100,000 individuals. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Data on urbanicity is from the USDA. "Counties W/ SEPs" represents the baseline sample, comparing counties with SEP openings to those with existing programs. "Urban" counties include metropolitan areas, while "Rural" counties include micropolitan areas, small towns, and rural areas. "High Pov." counties are defined as counties with poverty rates above the 2016 median poverty rate. "Low Pov." counties are those with poverty rates at or below this median. All specifications limit the sample to include counties with new or existing SEPs. Standard errors are clustered at the county level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 6: The Effect of a Syringe Exchange Program on HIV Diagnoses Rates and Opioid-Related Mortality by Region

	(1) Midwest	(2) South	(3) Northeast	(4) West	(5) "Top 10"
HIV Rate					
Average Effect of SEP	-3.060	1.212	-6.568	-5.924**	-1.058
	(3.657)	(0.854)	(8.455)	(2.783)	(2.370)
Average Lagged Effect					
P-value (test average effect = 0)					
Mean	19.50	13.36	58.71	15.84	15.69
Observations	387	405	369	738	522
Opioid-Related Mortality Rate					
Average Effect of SEP	0.543	10.670**	-0.832	0.103	8.689**
	(1.910)	(4.702)	(1.694)	(1.029)	(3.514)
Average Lagged Effect					
P-value (test average effect = 0)					
Mean	17.03	25.01	15.92	17.80	24.62
Observations	387	405	369	738	522
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	Yes	Yes	Yes	Yes	Yes
State-Level Policy Controls	Yes	Yes	Yes	Yes	Yes

Notes: Estimates are based on NCHS restricted mortality files and CDC NCHHSTP Atlas and state agency HIV diagnoses counts by county for the entire United States from 2008–2016. Rates are calculated as cases per 100,000 individuals. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. For US Census Bureau definitions of region, see `https://www.census.gov/geo/reference/webatlas/regions.html`. "Top 10" states include those most affected by the opioid epidemic, according to the CDC. "Top 10" states are West Virginia, Ohio, Pennsylvania, District of Columbia, New Hampshire, Kentucky, Maine, Connecticut, Delaware, Massachusetts. Standard errors are clustered at the county level.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 7: The Effect of a Syringe Exchange Program on Opioid-Related Mortality Rates by Age

	(1)	(2)	(3)	(4)	(5)	(6)
20-29 Year Olds						
Average Effect of SEP	0.332	0.329	0.277			
	(0.295)	(0.297)	(0.287)			
Effect of SEP in First Year				0.466	0.467	0.436
				(0.347)	(0.351)	(0.343)
Effect of SEP in Second Year				0.459	0.457	0.402
				(0.454)	(0.458)	(0.447)
Effect of SEP in Third Year				0.330	0.318	0.262
				(0.373)	(0.372)	(0.357)
Effect of SEP in Fourth+ Year				-0.378	-0.376	-0.418
				(0.339)	(0.341)	(0.349)
Average Lagged Effect				0.22	0.22	0.17
P-value (test average effect = 0)				0.45	0.46	0.54
Mean	13.29	13.29	13.29	13.29	13.29	13.29
Observations	1899	1899	1899	1899	1899	1899
30-39 Year Olds						
Average Effect of SEP	0.807*	0.818*	0.658			
	(0.448)	(0.448)	(0.420)			
Effect of SEP in First Year				0.625	0.644	0.475
				(0.516)	(0.515)	(0.486)
Effect of SEP in Second Year				1.385**	1.394**	1.193*
				(0.690)	(0.691)	(0.668)
Effect of SEP in Third Year				0.842*	0.869*	0.708
				(0.448)	(0.446)	(0.432)
Effect of SEP in Fourth+ Year				0.214	0.190	0.188
				(0.469)	(0.471)	(0.459)
Average Lagged Effect				0.77	0.77	0.64
P-value (test average effect = 0)				0.07	0.06	0.11
Mean	16.89	16.89	16.89	16.89	16.89	16.89
Observations	1899	1899	1899	1899	1899	1899
40-49 Year Olds						
Average Effect of SEP	0.866**	0.882**	0.802*			
	(0.436)	(0.436)	(0.429)			
Effect of SEP in First Year				0.804*	0.833*	0.740
				(0.461)	(0.464)	(0.457)
Effect of SEP in Second Year				1.042	1.055	0.962
				(0.736)	(0.737)	(0.723)
Effect of SEP in Third Year				1.127	1.159	1.100
				(0.797)	(0.796)	(0.801)
Effect of SEP in Fourth+ Year				0.407	0.373	0.376
				(0.572)	(0.566)	(0.555)
Average Lagged Effect				0.85	0.85	0.79
P-value (test average effect = 0)				0.08	0.07	0.09
Mean	20.09	20.09	20.09	20.09	20.09	20.09
Observations	1899	1899	1899	1899	1899	1899
50-59 Year Olds						
Average Effect of SEP	0.581	0.590	0.560			
	(0.441)	(0.441)	(0.426)			
Effect of SEP in First Year				0.970	0.987	0.975
				(0.697)	(0.700)	(0.686)
Effect of SEP in Second Year				0.595	0.603	0.580
				(0.430)	(0.428)	(0.420)
Effect of SEP in Third Year				0.301	0.321	0.273
				(0.489)	(0.490)	(0.483)
Effect of SEP in Fourth+ Year				-0.425	-0.445	-0.432
				(0.491)	(0.489)	(0.475)
Average Lagged Effect				0.36	0.37	0.35
P-value (test average effect = 0)				0.35	0.34	0.35
Mean	20.16	20.16	20.16	20.16	20.16	20.16
Observations	1899	1899	1899	1899	1899	1899
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	No	Yes	Yes	No	Yes	Yes
State-Level Policy Controls	No	No	Yes	No	No	Yes

Notes: Estimates are based on NCHS restricted mortality files by county for each of the listed age groups for the entire United States from 2008–2016. Rates are calculated as cases per 100,000 individuals. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription

Table 8: The Effect of a Syringe Exchange Program on Drug-Related Mortality Rates by Race

	(1)	(2)	(3)	(4)	(5)	(6)
White Overdose Rate						
Average Effect of SEP	3.009**	3.038**	2.684**			
	(1.249)	(1.248)	(1.186)			
Effect of SEP in First Year				3.518**	3.572**	3.216**
				(1.530)	(1.531)	(1.462)
Effect of SEP in Second Year				3.837**	3.860**	3.462*
				(1.842)	(1.842)	(1.778)
Effect of SEP in Third Year				2.215	2.277	1.940
				(1.516)	(1.516)	(1.502)
Effect of SEP in Fourth+ Year				0.567	0.502	0.458
				(1.266)	(1.264)	(1.239)
Average Lagged Effect				2.53	2.55	2.27
P-value (test average effect = 0)				0.03	0.03	0.05
Mean	74.27	74.27	74.27	74.27	74.27	74.27
Observations	1899	1899	1899	1899	1899	1899
Black Overdose Rate						
Average Effect of SEP	-0.028	-0.025	-0.060			
	(0.267)	(0.268)	(0.261)			
Effect of SEP in First Year				-0.007	-0.001	-0.027
				(0.261)	(0.262)	(0.254)
Effect of SEP in Second Year				0.092	0.094	0.052
				(0.283)	(0.284)	(0.272)
Effect of SEP in Third Year				-0.139	-0.133	-0.173
				(0.262)	(0.263)	(0.257)
Effect of SEP in Fourth+ Year				-0.212	-0.219	-0.240
				(0.423)	(0.422)	(0.417)
Average Lagged Effect				-0.07	-0.06	-0.10
P-value (test average effect = 0)				0.82	0.82	0.73
Mean	11.80	11.80	11.80	11.80	11.80	11.80
Observations	1899	1899	1899	1899	1899	1899
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	No	Yes	Yes	No	Yes	Yes
State-Level Policy Controls	No	No	Yes	No	No	Yes

Notes: Estimates are based on restricted mortality files by county for the listed racial groups for the entire United States from 2008–2016. Rates are calculated as cases per 100,000 individuals. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the county level.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 9: The Effect of a Syringe Exchange Program on Opioid-Related Hospital Visits by Subgroup

	Emergency Room Visits			In-Patient Stays		
	(1)	(2)	(3)	(4)	(5)	(6)
Total						
Average Effect of SEP	23.822**	20.780**	13.486	15.053**	12.503*	7.022
	(10.194)	(9.231)	(9.296)	(7.321)	(6.749)	(7.185)
Mean	150.75	150.75	150.75	209.26	209.26	209.26
Observations	258	258	258	375	375	375
Males						
Average Effect of SEP	27.733*	24.136*	15.232	15.156*	12.728*	7.359
	(13.896)	(12.851)	(12.140)	(7.590)	(6.962)	(7.224)
Mean	166.04	166.04	166.04	204.17	204.17	204.17
Observations	258	258	258	375	375	375
Age 25-44						
Average Effect of SEP	77.771***	70.378**	47.162*	34.215**	29.719**	15.137
	(27.820)	(27.183)	(26.670)	(14.150)	(13.933)	(13.753)
Mean	288.19	288.19	288.19	299.00	299.00	299.00
Observations	258	258	258	375	375	375
Age 45-64						
Average Effect of SEP	7.256	5.701	2.539	18.146*	15.259*	10.209
	(10.199)	(8.632)	(9.161)	(9.644)	(8.977)	(9.473)
Mean	149.10	149.10	149.10	285.53	285.53	285.53
Observations	258	258	258	375	375	375
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	No	Yes	Yes	No	Yes	Yes
State-Level Policy Controls	No	No	Yes	No	No	Yes

Notes: Estimates are based on state-level opioid-related emergency room visits and in-patient stays from the Healthcare Cost Utilization Project 2008-2016. Rates are calculated as cases per 100,000 individuals. Economic control variables include the state-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the state level.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 10: The Effect of a Syringe Exchange Program on Drug Crimes and Theft, Difference-in-Differences Estimates using Counties with Existing SEPs for Comparison

	(1)	(2)	(3)	(4)	(5)	(6)
Total Drug Crime Rate						
Average Effect of SEP	50.134**	49.235**	51.588**			
	(21.848)	(21.935)	(22.310)			
Effect of SEP in First Year				29.221	27.734	30.805
				(20.793)	(20.611)	(20.000)
Effect of SEP in Second Year				49.955**	48.666**	51.330**
				(23.925)	(23.611)	(24.191)
Effect of SEP in Third Year				71.211**	71.708**	70.536**
				(32.717)	(33.164)	(34.018)
Effect of SEP in Fourth+ Year				95.998***	94.994**	94.481**
				(35.885)	(37.035)	(37.273)
Average Lagged Effect				61.60	60.78	61.79
P-value (test average effect = 0)				0.01	0.01	0.01
Mean	314.64	314.64	314.64	314.64	314.64	314.64
Observations	1899	1899	1899	1899	1899	1899
Opioid-Related Drug Sale Rate						
Average Effect of SEP	-1.102	-1.479	-0.202			
	(3.251)	(3.322)	(3.145)			
Effect of SEP in First Year				-1.309	-2.071	-0.551
				(3.115)	(3.129)	(2.758)
Effect of SEP in Second Year				-0.619	-1.120	0.270
				(3.298)	(3.271)	(3.037)
Effect of SEP in Third Year				-1.515	-1.173	-0.476
				(4.100)	(4.366)	(4.209)
Effect of SEP in Fourth+ Year				-0.888	-0.592	0.303
				(5.647)	(5.911)	(6.177)
Average Lagged Effect				-1.08	-1.24	-0.11
P-value (test average effect = 0)				0.76	0.74	0.97
Mean	20.98	20.98	20.98	20.98	20.98	20.98
Observations	1899	1899	1899	1899	1899	1899
Opioid-Related Drug Possession Rate						
Average Effect of SEP	0.134*	0.130	0.136*			
	(0.079)	(0.080)	(0.082)			
Effect of SEP in First Year				0.138**	0.126*	0.139**
				(0.068)	(0.067)	(0.069)
Effect of SEP in Second Year				0.108	0.102	0.112
				(0.085)	(0.083)	(0.085)
Effect of SEP in Third Year				0.094	0.108	0.114
				(0.106)	(0.110)	(0.110)
Effect of SEP in Fourth+ Year				0.219*	0.226*	0.195
				(0.118)	(0.129)	(0.129)
Average Lagged Effect				0.14	0.14	0.14
P-value (test average effect = 0)				0.11	0.12	0.13
Mean	0.55	0.55	0.55	0.55	0.55	0.55
Observations	1899	1899	1899	1899	1899	1899
Theft Rate						
Average Effect of SEP	4.304	4.444	5.027*			
	(2.731)	(2.705)	(2.669)			
Effect of SEP in First Year				3.004	3.417	4.154
				(2.505)	(2.671)	(2.564)
Effect of SEP in Second Year				5.247	5.563*	6.408*
				(3.202)	(3.278)	(3.365)
Effect of SEP in Third Year				4.781	4.745	5.303
				(4.858)	(4.585)	(4.605)
Effect of SEP in Fourth+ Year				6.225	5.260	4.844
				(4.454)	(3.946)	(3.956)
Average Lagged Effect				4.81	4.75	5.18
P-value (test average effect = 0)				0.13	0.12	0.08
Mean	21.15	21.15	21.15	21.15	21.15	21.15
Observations	1899	1899	1899	1899	1899	1899
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	No	Yes	Yes	No	Yes	Yes
State-Level Policy Controls	No	No	Yes	No	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates are based on FBI Uniform Crime Reports by county for the entire United States from 2008–2016. Rates are calculated as cases per 100,000 individuals. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs,

Table 11: OLS Estimates of Lead Terms in Difference-in-Differences Model, Using Counties with Existing SEPs for Comparison

	HIV Rate		**Opioid-Related Mortality Rate**		**Opioid-Related ED Rate**		**Opioid-Related IP Rate**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect of SEP in First Year	-2.578	-3.100	2.995**	3.062*	6.825	8.346	5.385	9.986
	(2.436)	(3.574)	(1.489)	(1.599)	(7.910)	(10.857)	(5.625)	(7.922)
Effect of SEP in Second Year	-6.057**	-6.629	3.231*	3.305*	21.865**	23.519*	7.746	12.976
	(3.027)	(4.135)	(1.846)	(1.948)	(9.786)	(12.680)	(8.507)	(10.690)
Effect of SEP in Third Year	-8.361**	-8.989*	2.495	2.576	30.863**	32.705*	9.679	15.498
	(3.902)	(4.629)	(1.691)	(1.796)	(14.327)	(16.767)	(10.022)	(12.303)
Effect of SEP in Fourth+ Year	-13.523	-14.288	-0.067	0.032	53.646**	56.106**	18.597	25.667*
	(10.242)	(9.903)	(1.455)	(1.601)	(20.952)	(22.948)	(11.807)	(13.639)
One-Year Lead		-1.452		0.188		2.981		9.681*
		(3.352)		(1.013)		(6.714)		(5.734)
Average Lagged Effect	-7.63	-8.25	2.16	2.24	28.30	30.17	10.35	16.03
P-value (test average effect = 0)	0.04	0.05	0.08	0.10	0.02	0.04	0.20	0.13
Mean	24.39	24.39	18.82	18.82	150.75	150.75	209.26	209.26
Observations	1899	1899	1899	1899	258	258	375	375
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Level Policy Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates In Columns 1 and 2 are based on NCHS restricted mortality files and CDC and state agency HIV diagnoses counts by county for the entire United States from 2008–2016. Rates are calculated as cases per 100,000 individuals. Estimates in Columns 3 and 4 are based on state-level opioid-related emergency room visits and in-patient stays from the Healthcare Cost Utilization Project 2008-2016. Economic control variables include the state-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the county level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 12: OLS, WLS, and Poisson Estimates from a
Difference-in-Differences Model using Counties with Existing SEPs for Comparison

	OLS			WLS			Poisson		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
HIV Diagnoses									
Effect of SEP in First Year	-2.899	-2.578	-3.100	-0.003	0.072	0.129	0.088	0.057	0.062
	(2.296)	(2.436)	(3.574)	(0.106)	(0.112)	(0.135)	(0.077)	(0.085)	(0.103)
Effect of SEP in Second Year	-6.353**	-6.057**	-6.629	-0.085	-0.024	0.040	0.032	-0.008	-0.002
	(2.848)	(3.027)	(4.135)	(0.131)	(0.139)	(0.163)	(0.100)	(0.113)	(0.134)
Effect of SEP in Third Year	-9.004**	-8.361**	-8.989*	-0.221	-0.180	-0.111	-0.187**	-0.170*	-0.164
	(4.001)	(3.902)	(4.629)	(0.136)	(0.126)	(0.146)	(0.090)	(0.103)	(0.116)
Effect of SEP in Fourth+ Year	-12.900	-13.523	-14.288	-0.236	-0.285	-0.207	-0.239	-0.230	-0.222
	(10.733)	(10.242)	(9.903)	(0.145)	(0.178)	(0.197)	(0.158)	(0.146)	(0.160)
One-Year Lead			-1.452			0.181*			0.012
			(3.352)			(0.096)			(0.063)
Average Lagged Effect	-7.79	-7.63	-8.25	-0.14	-0.10	-0.04	-0.08	-0.09	-0.08
P-value (test average effect = 0)	0.05	0.04	0.05	0.25	0.39	0.80	0.35	0.33	0.46
Mean	24.39	24.39	24.39	3.04	3.04	3.04	106.10	106.10	106.10
Observations	1899	1899	1899	1899	1899	1899	1764	1764	1764
Opioid-Related Deaths									
Effect of SEP in First Year	3.327**	2.995**	3.062*	-0.034	-0.057	-0.062	0.010	-0.004	-0.012
	(1.602)	(1.489)	(1.599)	(0.048)	(0.047)	(0.053)	(0.040)	(0.035)	(0.042)
Effect of SEP in Second Year	3.639*	3.231*	3.305*	0.016	-0.010	-0.016	0.039	0.026	0.017
	(1.948)	(1.846)	(1.948)	(0.049)	(0.048)	(0.054)	(0.056)	(0.050)	(0.056)
Effect of SEP in Third Year	2.753	2.495	2.576	0.016	-0.028	-0.035	-0.029	-0.055	-0.064
	(1.724)	(1.691)	(1.796)	(0.061)	(0.050)	(0.056)	(0.066)	(0.054)	(0.059)
Effect of SEP in Fourth+ Year	0.060	-0.067	0.032	0.020	-0.008	-0.015	-0.078	-0.088	-0.099
	(1.483)	(1.455)	(1.601)	(0.091)	(0.079)	(0.085)	(0.094)	(0.079)	(0.084)
One-Year Lead			0.188			-0.016			-0.024
			(1.013)			(0.032)			(0.036)
Average Lagged Effect	2.44	2.16	2.24	0.00	-0.03	-0.03	-0.01	-0.03	-0.04
P-value (test average effect = 0)	0.06	0.08	0.10	0.93	0.55	0.53	0.80	0.52	0.46
Mean	18.82	18.82	18.82	2.84	2.84	2.84	80.75	80.75	80.75
Observations	1899	1899	1899	1899	1899	1899	1899	1899	1899
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
State-Level Policy Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Notes: Estimates are based on NCHS restricted mortality files and CDC HIV diagnoses counts by county for the entire United States from 2008–2016. Columns 1–6 display the effects of SEPs on HIV and opioid-related mortality rates, while Columns 7–9 show estimates for HIV cases and opioid-related deaths. Rates are calculated as cases per 100,000 individuals. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions,

Table 13: The Effect of a Syringe Exchange Program on HIV Diagnoses Rates and Opioid-Related Mortality Rates by Population Size

	All Counties			Counties with Population > Median			Counties with Population < Median		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
HIV Rate									
Average Effect of SEP									
Effect of SEP in First Year	-2.899	-3.244	-2.578	-6.362	-6.721	-5.384	0.204	0.187	0.199
	(2.296)	(2.475)	(2.436)	(5.055)	(4.963)	(4.674)	(0.176)	(0.178)	(0.181)
Effect of SEP in Second Year	-6.353**	-6.604**	-6.057**	-12.972**	-12.979**	-12.347**	-0.019	-0.041	-0.032
	(2.848)	(3.036)	(3.027)	(5.500)	(5.424)	(5.550)	(0.044)	(0.043)	(0.038)
Effect of SEP in Third Year	-9.004**	-8.770**	-8.361**	-16.345**	-15.662**	-15.552**	-0.053	-0.075	-0.056
	(4.001)	(3.965)	(3.902)	(6.721)	(6.492)	(6.374)	(0.047)	(0.053)	(0.052)
Effect of SEP in Fourth+ Year	-12.900	-12.288	-13.523	-22.438	-21.174	-22.854	-0.086	-0.113**	-0.088*
	(10.733)	(10.069)	(10.242)	(17.336)	(16.120)	(15.580)	(0.055)	(0.054)	(0.052)
Average Lagged Effect	-7.79	-7.73	-7.63	-14.53	-14.13	-14.03	0.01	-0.01	0.01
P-value (test average effect = 0)	0.05	0.04	0.04	0.02	0.02	0.02	0.88	0.89	0.94
Mean	24.39	24.39	24.39	24.39	24.39	24.39	24.39	24.39	24.39
Observations	1899	1899	1899	949	949	949	950	950	950
Opioid-Related Mortality Rate									
Average Effect of SEP									
Effect of SEP in First Year	3.327**	3.316**	2.995**	0.427	0.477	-0.074	6.200**	6.065**	5.669**
	(1.602)	(1.556)	(1.489)	(1.181)	(1.166)	(1.084)	(2.702)	(2.619)	(2.500)
Effect of SEP in Second Year	3.639*	3.611*	3.231*	1.490	1.526	0.997	6.018	5.764	5.606
	(1.948)	(1.912)	(1.846)	(1.644)	(1.655)	(1.502)	(3.682)	(3.633)	(3.509)
Effect of SEP in Third Year	2.753	2.835	2.495	-0.681	-0.608	-1.084	7.173**	7.427**	7.058**
	(1.724)	(1.717)	(1.691)	(1.574)	(1.596)	(1.410)	(3.545)	(3.561)	(3.495)
Effect of SEP in Fourth+ Year	0.060	-0.039	-0.067	-2.342	-2.255	-2.325	2.012	1.353	1.441
	(1.483)	(1.504)	(1.455)	(1.976)	(1.939)	(1.837)	(2.228)	(2.229)	(2.294)
Average Lagged Effect	2.44	2.43	2.16	-0.28	-0.22	-0.62	5.35	5.15	4.94
P-value (test average effect = 0)	0.06	0.06	0.08	0.85	0.88	0.63	0.02	0.02	0.02
Mean	18.82	18.82	18.82	18.82	18.82	18.82	18.82	18.82	18.82
Observations	1899	1899	1899	949	949	949	950	950	950
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
State-Level Policy Controls	No	No	Yes	No	No	Yes	No	No	Yes

Notes: Estimates are based on NCHS restricted mortality files and CDC NCHHSTP Atlas and state agency HIV diagnoses counts by county for the entire United States from 2008–2016. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. The median population is 191,972. Standard errors are clustered at the county level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

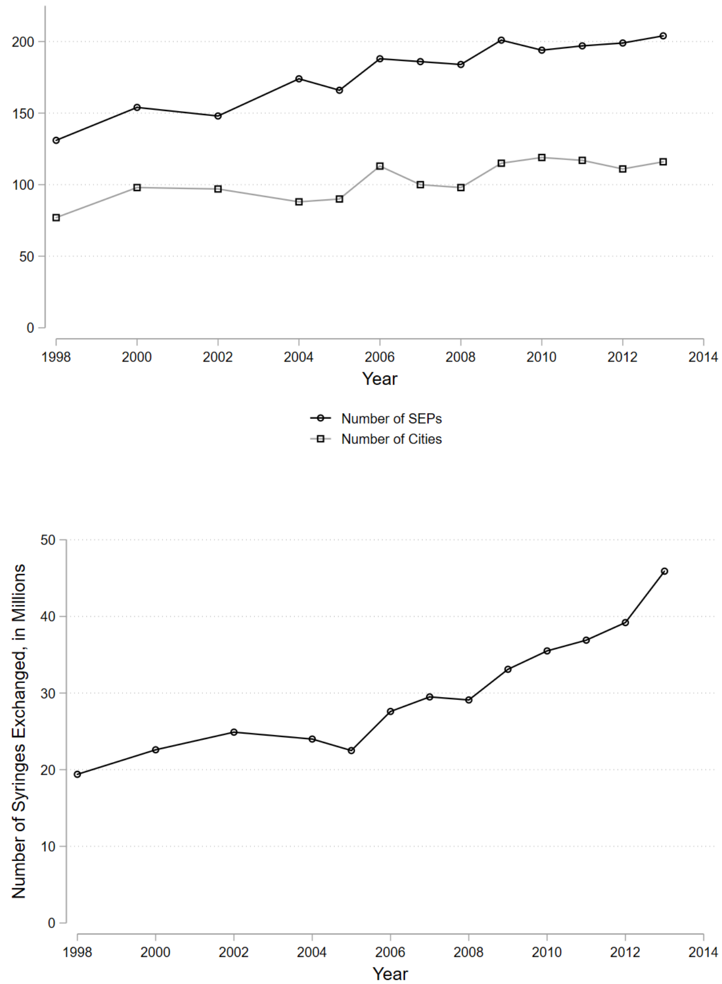
Table 14: The Effect of a Syringe Exchange Program on HIV Diagnoses Rates and Opioid-Related Mortality Rates Using Various Comparison Groups

	Counties W/ SEPs	**Counties W/O SEPs**	**All Counties**	**Counties in SEP States**	**Border Counties**
	(1)	(2)	(3)	(4)	(5)
HIV Rate					
Average Effect of SEP	-5.970**	-3.983	-4.183	-4.180	-4.380
	(2.761)	(3.239)	(3.257)	(3.248)	(3.004)
Mean	24.39	2.02	2.62	3.40	8.64
Observations	1899	27324	28206	21591	5418
Opioid-Related Mortality Rate					
Average Effect of SEP	2.541**	4.986***	4.877***	4.659***	4.087***
	(1.245)	(1.380)	(1.379)	(1.373)	(1.355)
Mean	18.82	11.68	11.86	12.60	16.42
Observations	1899	27324	28206	21591	5418
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	Yes	Yes	Yes	Yes	Yes
State-Level Policy Controls	Yes	Yes	Yes	Yes	Yes

Notes: Estimates are based on NCHS restricted mortality files and CDC NCHHSTP Atlas and state agency HIV diagnoses counts by county for the entire United States from 2008–2016. Economic control variables include the county-level poverty rate, unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. "Counties W/ SEPs" represent the baseline results comparing counties with SEP openings to those with existing SEPs, "Counties W/O SEPs" compares counties with recent SEPs openings to all other counties in the US without an existing SEP, "All Counties" represents a full sample of US counties comparing counties with recent SEP openings to all other US counties, "Counties in SEP States" represents a subsample of all counties in US states with legal access to SEPs, and "Border Counties" shows estimates from a model comparing counties with SEP openings to their respective bordering counties. Standard errors are clustered at the county level.

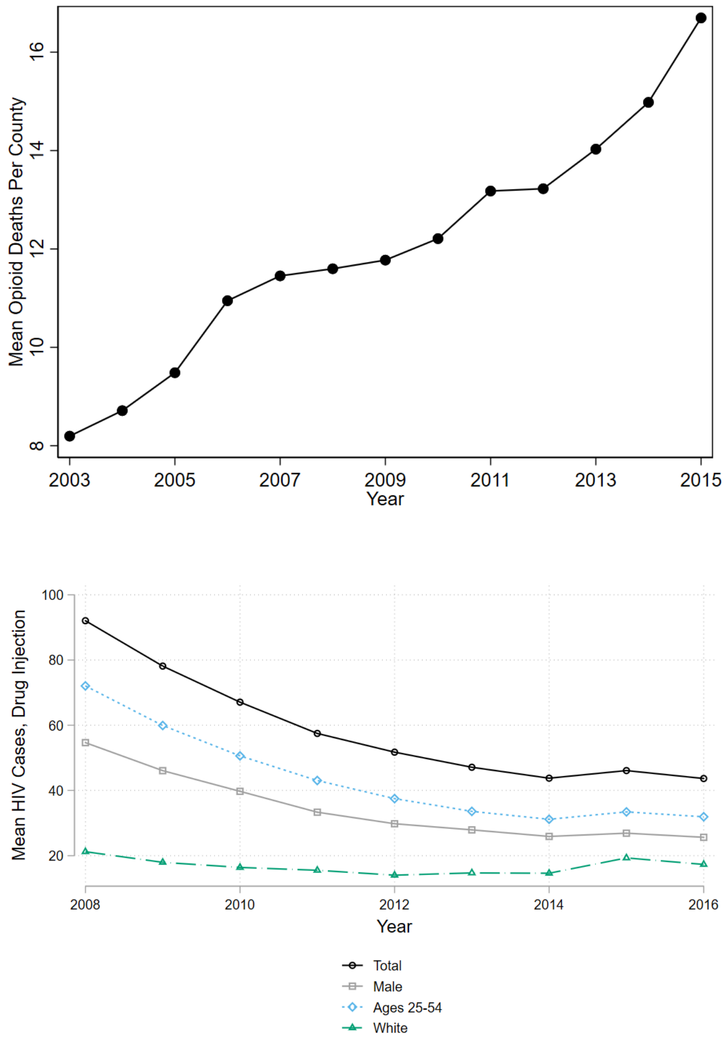
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Figure 1: Number of SEPs and Syringes Exchanged Over Time



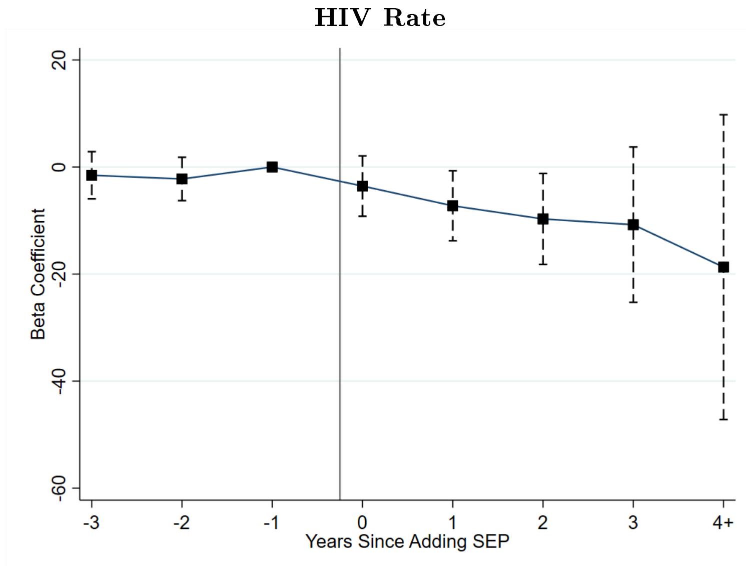
Notes: Data is from the NASEN 2014 National Survey of Syringe Exchange Programs.

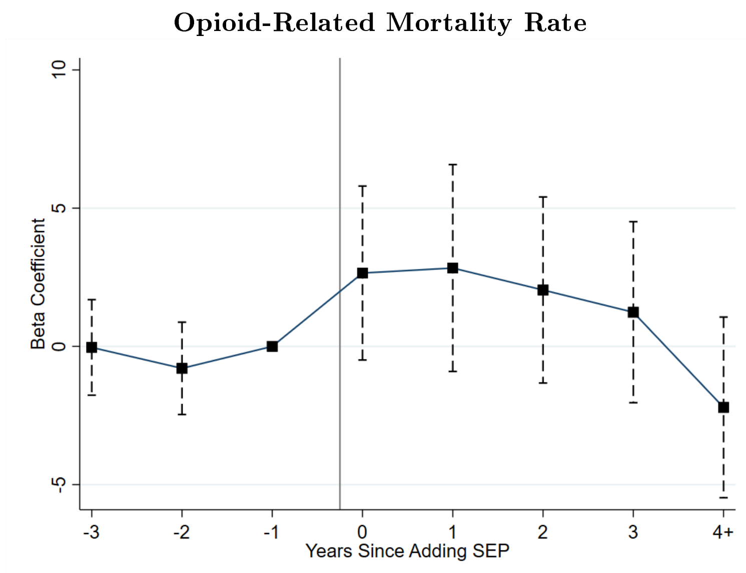
Figure 2: Opioid-Related Deaths and HIV Cases Over Time



Notes: County-level data on drug-related mortality from 2008–2016 is from the National Center for Health Statistics Mortality Files.

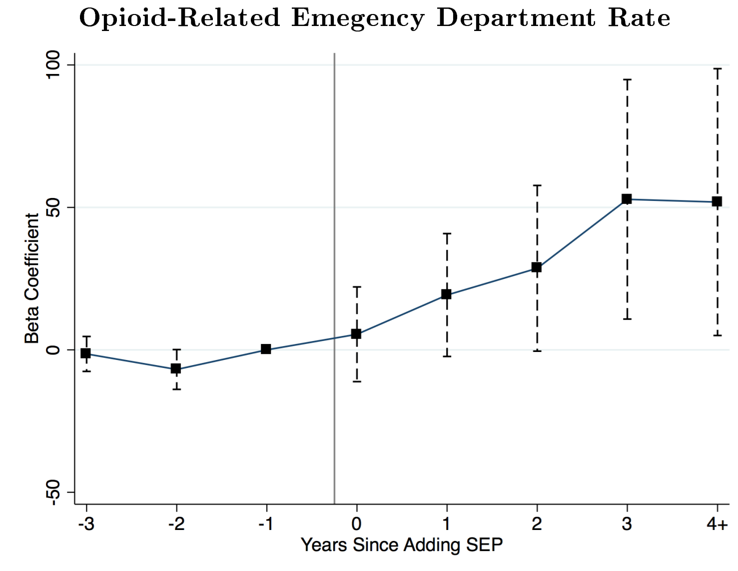
Figure 3: Difference-in-Differences Estimates, HIV and Opioid-Related Mortality Rates

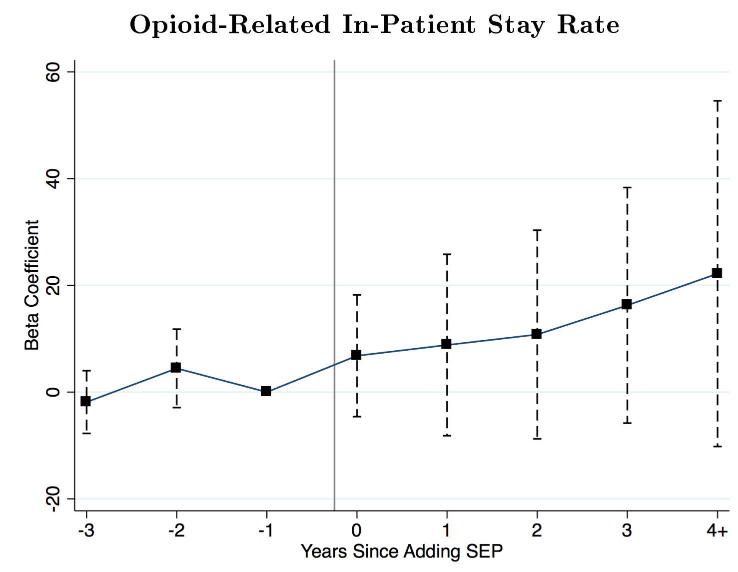




Notes: Each figure displays the coefficients and their respective 95% confidence intervals for the leading indicators and lagged treatment effects from OLS regressions, as specified in Equation 1. The vertical line represents the first year during the sample period that a county experienced a syringe exchange program opening. Estimates are based on restricted mortality files and CDC HIV diagnoses counts by county for the entire United States from 2008–2016. HIV diagnoses rates are from the Center for Disease Control and Prevention's NCHHSTP Atlas and 36 state agencies. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the county level.

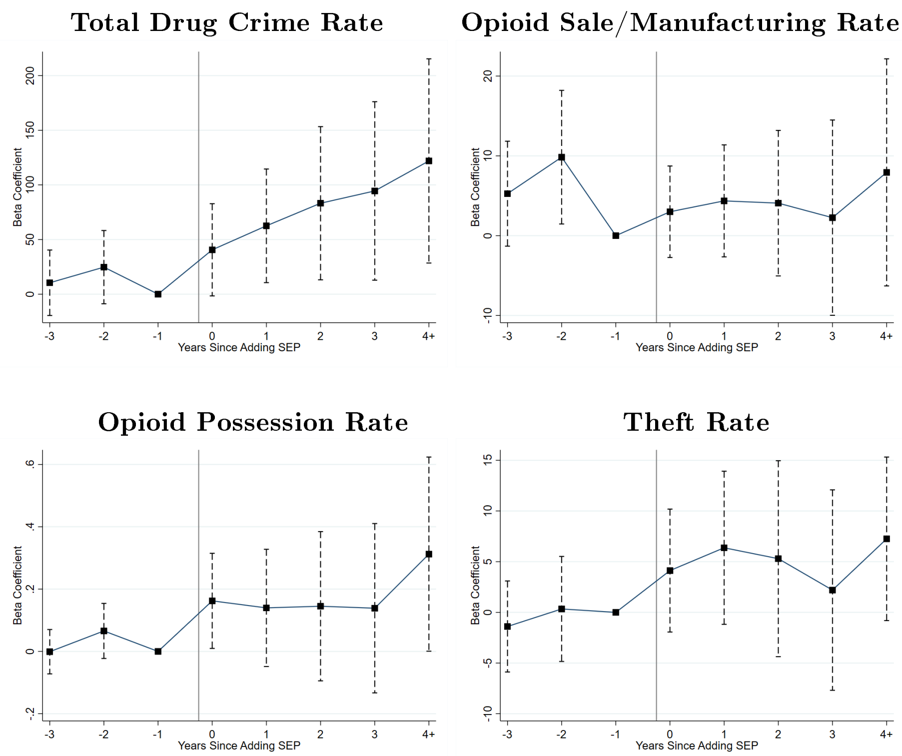
Figure 4: Difference-in-Differences Estimates, Opioid-Related Hospital Visits (State-Level)





Notes: Each figure displays the coefficients and their respective 95% confidence intervals for the leading indicators and lagged treatment effects from OLS regressions, as specified in Equation 1. The vertical line represents the first year during the sample period that a state experienced a syringe exchange program opening. Estimates are based on state-level data on emergency department (ED) visits and in-patient (IP) hospital stays from 2008–2016 from the Healthcare Cost and Utilization Project (HCUP). Economic control variables include poverty rate, unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the state level.

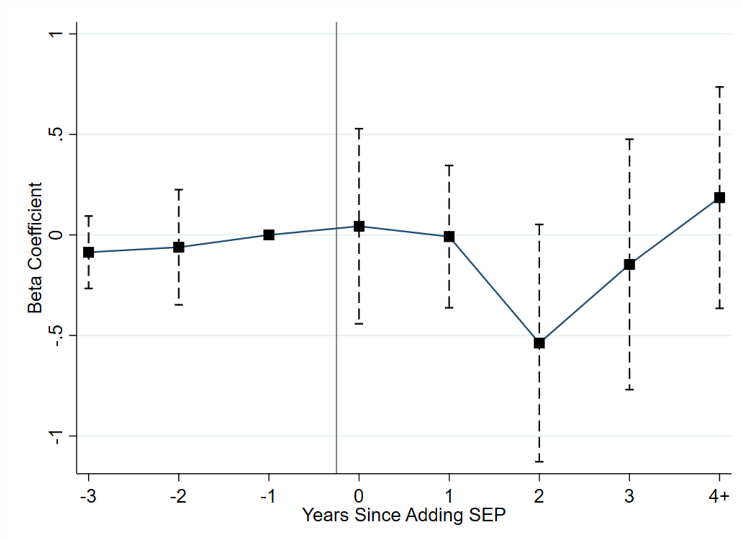
Figure 5: Difference-in-Differences Estimates, Drug-Related Crime Rates



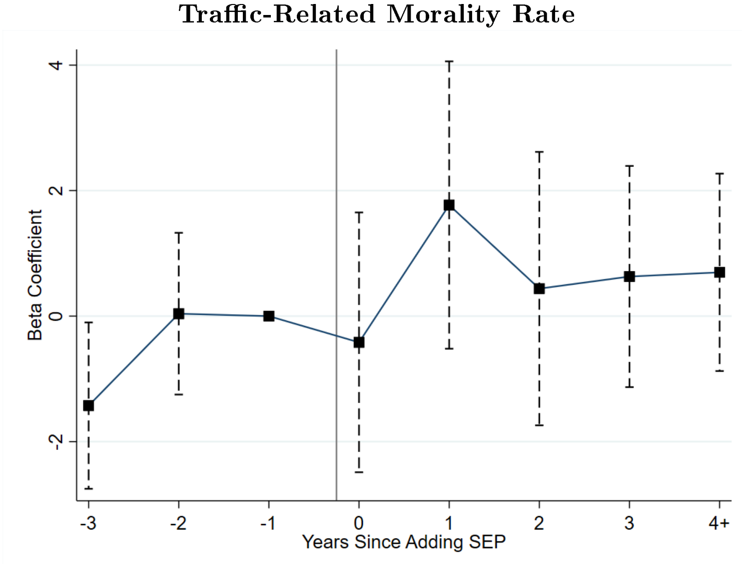
Notes: Each figure displays the coefficients and their respective 95% confidence intervals for the leading indicators and lagged treatment effects from OLS regressions, as specified in Equation 1. The vertical line represents the first year during the sample period that a county experienced a syringe exchange program opening. County-level arrest data from 2008–2016 is from the FBI Uniform Crime Reports. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the county level.

Figure 6: Difference-in-Differences Estimates, Alcohol-Related Mortality and Traffic-Related Mortality

Alcohol-Related Mortality Rate



Traffic-Related Morality Rate



Notes: Each figure displays the coefficients and their respective 95% confidence intervals for the leading indicators and lagged treatment effects from OLS regressions, as specified in Equation 1. The vertical line represents the first year during the sample period that a county experienced a syringe exchange program opening. Estimates are based on restricted mortality files by county for the entire United States from 2008–2016. HIV diagnoses rates are from the Center for Disease Control and Prevention's NCHHSTP Atlas and 36 state agencies. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the county level.

Appendix

Table A1: Summary Statistics for a Rural Midwest Syringe Exchange Program

Client Characteristics	**Mean**	**St.Dev.**
Age	37.84	10.13
Percent White	0.97	0.18
Percent Male	0.59	0.49
First Injection Age	27.14	10.25
Previously Sought Addiction Treatment	0.22	0.41
Percent Ever Overdosed	0.32	0.47
Number of Times Overdosed	3.42	4.83
Percent Injected Heroin at First Use	0.49	0.50
Percent Injected Opioid Pills at First Use	0.29	0.46
Percent Prescribed Opioid Pain Pills	0.26	0.44
Percent Carry Naloxone	0.67	0.47
Visit Characteristics		
First Exchange	0.22	0.42
Number of Syringes Exchanged	30.15	11.49
Percent Inject Heroin	0.80	0.40
Percent Inject Fentanyl	0.16	0.37
Percent Inject Opioid Pills	0.02	0.15
Percent Diagnosed with HIV	0.01	0.07
Percent Diagnosed with Hepatitis C	0.21	0.41
Percent Given a Referral	0.01	0.07
Percent Given Naloxone	0.14	0.34
Percent Received HIV Education	0.14	0.35
Number of Clients	144.59	30.27
Distance Traveled, in Miles	14.52	40.37

Notes: Data is from the Portsmouth syringe exchange program from 2018.

Table A2: The Effect of a Syringe Exchange Program on Inverse Hyperbolic Sine Transformations of HIV Diagnoses and Opioid-Related Mortality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HIV Diagnoses								
Average Effect of SEP	-0.164*	-0.153	-0.140					
	(0.097)	(0.095)	(0.093)					
Effect of SEP in First Year				-0.020	0.007	0.024	0.072	0.114
				(0.098)	(0.098)	(0.098)	(0.117)	(0.135)
Effect of SEP in Second Year				-0.195	-0.180	-0.149	-0.097	-0.050
				(0.127)	(0.123)	(0.121)	(0.134)	(0.156)
Effect of SEP in Third Year				-0.350***	-0.370***	-0.323***	-0.265**	-0.213
				(0.122)	(0.119)	(0.116)	(0.131)	(0.156)
Effect of SEP in Fourth+ Year				-0.368***	-0.370***	-0.418***	-0.348**	-0.289
				(0.126)	(0.125)	(0.141)	(0.160)	(0.182)
One-Year Lead							0.133	0.171
							(0.082)	(0.104)
Two-Year Lead								0.115
								(0.098)
Mean	3.24	3.24	3.24	3.24	3.24	3.24	3.24	3.24
Observations	1899	1899	1899	1899	1899	1899	1899	1899
Opioid-Related Deaths								
Average Effect of SEP	0.057	0.062	0.047					
	(0.045)	(0.044)	(0.042)					
Effect of SEP in First Year				0.065	0.072	0.053	0.060	0.045
				(0.052)	(0.050)	(0.050)	(0.055)	(0.061)
Effect of SEP in Second Year				0.036	0.040	0.023	0.031	0.013
				(0.060)	(0.059)	(0.056)	(0.062)	(0.066)
Effect of SEP in Third Year				0.107	0.113	0.102	0.110	0.091
				(0.077)	(0.076)	(0.075)	(0.082)	(0.084)
Effect of SEP in Fourth+ Year				0.012	0.015	0.011	0.021	-0.000
				(0.077)	(0.077)	(0.073)	(0.079)	(0.084)
One-Year Lead							0.019	0.005
							(0.046)	(0.050)
Two-Year Lead								-0.042
								(0.046)
Mean	4.06	4.06	4.06	4.06	4.06	4.06	4.06	4.06
Observations	1899	1899	1899	1899	1899	1899	1899	1899
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
State-Level Policy Controls	No	No	Yes	No	No	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: See Table 2 and Table 3. Estimates are from Equation 1, using the inverse hyperbolic sine transformation of the listed outcome variables.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A3: The Effect of a Syringe Exchange Program on HIV Diagnoses Rates and Opioid-Related Mortality Rates, Controlling for State Legal Restrictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
HIV Diagnoses										
Average Effect of SEP	-3.906	-6.095**	-6.219**	-6.163**	-6.189**	-6.191**	-6.263**	-6.199**	-5.970**	-5.960**
	(3.362)	(2.842)	(2.890)	(2.920)	(2.930)	(2.931)	(2.935)	(2.892)	(2.761)	(2.769)
Mean	24.39	24.39	24.39	24.39	24.39	24.39	24.39	24.39	24.39	24.39
Observations	1899	1899	1899	1899	1899	1899	1899	1899	1899	1899
Opioid-Related Deaths										
Average Effect of SEP	7.501***	2.889**	2.872**	2.736**	2.745**	2.743**	2.667**	2.666**	2.541**	2.557**
	(1.426)	(1.352)	(1.311)	(1.275)	(1.275)	(1.276)	(1.268)	(1.269)	(1.245)	(1.234)
Mean	18.82	18.82	18.82	18.82	18.82	18.82	18.82	18.82	18.82	18.82
Observations	1899	1899	1899	1899	1899	1899	1899	1899	1899	1899
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rx Limits, Tamper Resistant Forms, ID Laws	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Doctor Shopping Restrictions	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Physician Exam, Pharmacist Verification	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Pain Clinic Regulations	No	No	No	No	No	No	Yes	Yes	Yes	Yes
Prescription Drug Monitoring Programs	No	No	No	No	No	No	No	Yes	Yes	Yes
Paraphernalia and Good Samaritan Laws	No	No	No	No	No	No	No	No	Yes	Yes
Naloxone Laws	No	No	No	No	No	No	No	No	No	Yes

Notes: See Table 2 and Table 3. Standard errors are clustered at the county level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A4: The Effect of a Syringe Exchange Program on HIV Rates, Opioid-Related Mortality Rates, Drug-Related Mortality Rates, and Drug Arrest Rates by 2015 Medicaid Expansion Status

	HIV Rate	Drug-Related Mortality Rate	Opioid-Related Mortality Rate	Drug-Related Arrest Rate
	(1)	(2)	(3)	(4)
Medicaid Expansion				
Average Effect of SEP	-7.047**	2.922**	2.928**	34.219
	(3.441)	(1.457)	(1.458)	(24.223)
Mean	3.69	13.33	15.30	253.12
Observations	1629	1629	1629	1629
No Medicaid Expansion				
Average Effect of SEP	0.450	1.149	1.131	89.121**
	(1.077)	(2.610)	(2.756)	(35.813)
Mean	1.78	10.71	12.22	276.95
Observations	270	270	270	270
County and Year Fixed Effects	Yes	Yes	Yes	Yes
Demographic and Economic Controls	Yes	Yes	Yes	Yes
State-Level Policy Controls	Yes	Yes	Yes	Yes

Notes: See Table 2 and Table 3. Data on Medicaid expansion status is from Simon, Soni, and Cawley (2017). Expansion states include AK, AZ, AR, CA, CO, CT, DE, HI, IL, IA, IN, KY, MD, MA, MI, MN, NV, NH, NJ, NM, NY, ND, OH, OR, PA, RI, VT, WA, WV, and WI. Standard errors are clustered at the county level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A5: The Effect of a Syringe Exchange Program on Chlamydia and Gonorrhea Rates, Difference-in-Differences Estimates using Counties with Existing SEPs for Comparison

	(1)	(2)	(3)	(4)	(5)	(6)
Chlamydia Rate						
Average Effect of SEP	-8.002	-10.961	-8.627			
	(11.087)	(10.892)	(10.639)			
Effect of SEP in First Year				-1.773	-6.286	-3.637
				(9.684)	(9.155)	(9.034)
Effect of SEP in Second Year				-8.198	-12.117	-8.856
				(13.648)	(13.640)	(13.452)
Effect of SEP in Third Year				-16.521	-17.545	-14.994
				(18.297)	(18.337)	(18.026)
Effect of SEP in Fourth+ Year				-18.832	-16.552	-16.496
				(20.236)	(19.737)	(19.306)
Average Effect				-11.33	-13.13	-11.00
P-value (test average effect = 0)				0.39	0.31	0.38
Mean	426.26	426.26	426.26	426.26	426.26	426.26
Observations	1871	1871	1871	1871	1871	1871
Gonorrhea Rate						
Average Effect of SEP	5.236	7.876	9.588			
	(8.258)	(8.637)	(9.216)			
Effect of SEP in First Year				-3.322	0.076	1.743
				(4.405)	(4.662)	(5.269)
Effect of SEP in Second Year				6.019	8.157	9.786
				(8.378)	(8.534)	(8.785)
Effect of SEP in Third Year				8.074	9.352	10.919
				(10.534)	(10.816)	(10.709)
Effect of SEP in Fourth+ Year				29.558	31.688	31.769
				(25.362)	(24.889)	(24.821)
Average Effect				10.08	12.32	13.55
P-value (test average effect = 0)				0.37	0.28	0.25
Mean	92.21	92.21	92.21	92.21	92.21	92.21
Observations	1870	1870	1870	1870	1870	1870
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	No	Yes	Yes	No	Yes	Yes
State-Level Policy Controls	No	No	Yes	No	No	Yes

Notes: Estimates are based on CDC NCHHSTP Atlas data on county-level rates of chlamydia and gonorrhea for the entire United States from 2008–2016. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the county level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A6: The Effect of a Syringe Exchange Program on HIV Rates and Opioid-Related Mortality Rates, Accounting for Pre-Trends

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HIV Rate								
Average Effect of SEP	-4.062	-2.657	-2.517					
	(3.435)	(2.994)	(3.018)					
Effect of SEP in First Year				-1.106	-0.904	-0.514	-0.377	-0.529
				(2.576)	(2.310)	(2.337)	(3.090)	(3.597)
Effect of SEP in Second Year				-4.129	-3.129	-2.809	-2.673	-2.834
				(2.775)	(2.451)	(2.436)	(3.115)	(3.576)
Effect of SEP in Third Year				-6.638*	-4.415	-4.238	-4.091	-4.270
				(3.956)	(3.440)	(3.431)	(3.904)	(4.316)
Effect of SEP in Fourth+ Year				-8.498	-5.198	-5.591	-5.424	-5.601
				(10.983)	(9.841)	(9.906)	(9.936)	(10.048)
One-Year Lead							0.500	0.356
							(2.898)	(3.357)
Two-Year Lead								-0.610
								(2.503)
Average Lagged Effect				-5.09	-3.41	-3.29	-3.14	-3.31
P-value (test average effect = 0)				0.21	0.34	0.35	0.43	0.44
Mean	24.39	24.39	24.39	24.39	24.39	24.39	24.39	24.39
Observations	1899	1899	1899	1899	1899	1899	1899	1899
Opioid-Related Mortality Rate								
Average Effect of SEP	3.848***	3.940***	3.826***					
	(1.288)	(1.295)	(1.268)					
Effect of SEP in First Year				4.191***	4.276***	4.186***	4.576***	4.643**
				(1.611)	(1.622)	(1.578)	(1.687)	(1.820)
Effect of SEP in Second Year				4.538**	4.564**	4.436**	4.823**	4.893**
				(1.921)	(1.921)	(1.871)	(1.977)	(2.083)
Effect of SEP in Third Year				4.081**	4.145**	4.136**	4.553***	4.631***
				(1.630)	(1.621)	(1.622)	(1.705)	(1.778)
Effect of SEP in Fourth+ Year				2.072*	2.224*	2.078*	2.551*	2.628*
				(1.171)	(1.195)	(1.193)	(1.314)	(1.424)
One-Year Lead							1.422	1.485
							(1.002)	(1.125)
Two-Year Lead								0.267
								(0.984)
Average Lagged Effect				3.72	3.80	3.71	4.13	4.20
P-value (test average effect = 0)				0.00	0.00	0.00	0.00	0.01
Mean	18.82	18.82	18.82	18.82	18.82	18.82	18.82	18.82
Observations	1899	1899	1899	1899	1899	1899	1899	1899
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and Economic Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
State-Level Policy Controls	No	No	Yes	No	No	Yes	Yes	Yes

Notes: See Table 2 and Table 3. Estimates are from a model analogous to Equation 1 that partials out pre-treatment trends. *, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

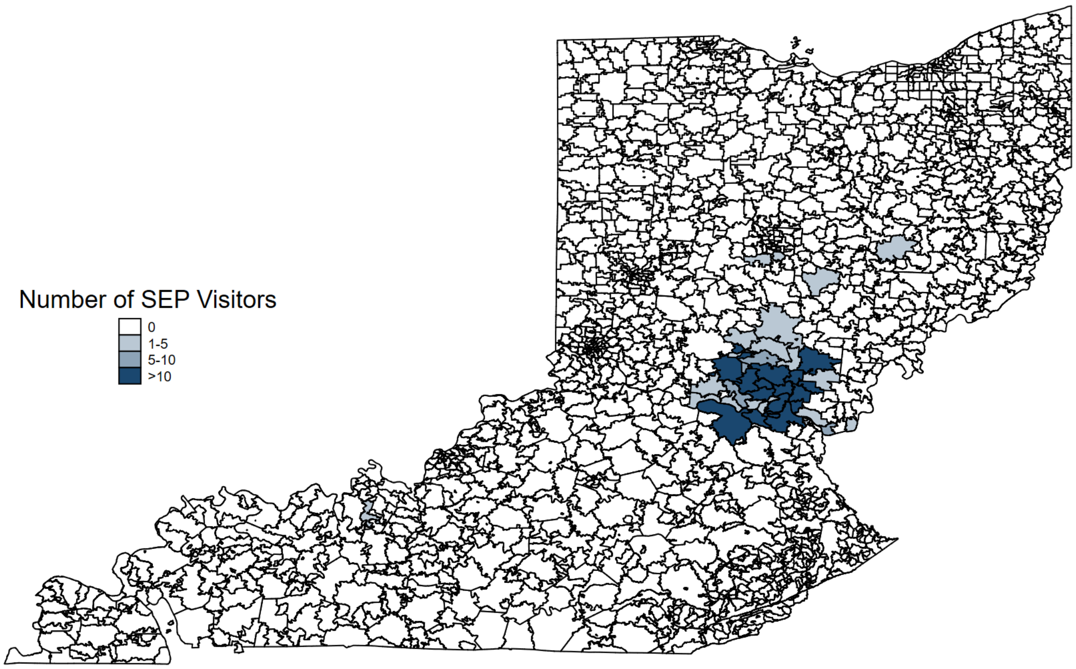
Table A7: The Effect of a Syringe Exchange Program on HIV Diagnoses Rates and Opioid-Related Mortality Rates, by Treatment Year

Treated Year (# Treated Counties)	**2009–2016** ($n = 85$)	**2009–2012** ($n = 26$)	**2013–2016** ($n = 59$)
	(1)	(2)	(3)
HIV Rate			
Effect of SEP in First Year	-2.578	-1.493	-4.498
	(2.436)	(3.390)	(2.862)
Effect of SEP in Second Year	-6.057**	-3.907	-8.822*
	(3.027)	(2.938)	(4.511)
Effect of SEP in Third Year	-8.361**	-8.587*	-9.231
	(3.902)	(4.563)	(6.826)
Effect of SEP in Fourth+ Year	-13.523	-11.888	-17.155
	(10.242)	(11.821)	(11.592)
Average Lagged Effect	-7.63	-6.47	-9.93
P-value (test average effect = 0)	0.04	0.08	0.11
Mean	24.39	24.10	21.11
Observations	1899	1368	1665
Opioid-Related Mortality Rate			
Effect of SEP in First Year	2.995**	0.493	3.685*
	(1.489)	(1.238)	(2.008)
Effect of SEP in Second Year	3.231*	-1.401	5.669*
	(1.846)	(1.313)	(2.903)
Effect of SEP in Third Year	2.495	-0.057	4.272
	(1.691)	(1.931)	(2.743)
Effect of SEP in Fourth+ Year	-0.067	-2.034	4.410
	(1.455)	(1.394)	(3.458)
Average Lagged Effect	2.16	-0.75	4.51
P-value (test average effect = 0)	0.08	0.50	0.03
Mean	18.82	17.75	19.17
Observations	1899	1368	1665
County and Year Fixed Effects	Yes	Yes	Yes
Demographic and Economic Controls	Yes	Yes	Yes
State-Level Policy Controls	Yes	Yes	Yes

Notes: Estimates are based on NCHS restricted mortality files and CDC NCHHSTP Atlas and state agency HIV diagnoses counts by county for the entire United States from 2008–2016. "Treated Year" represents the first year a county experiences a SEP opening. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the county level.

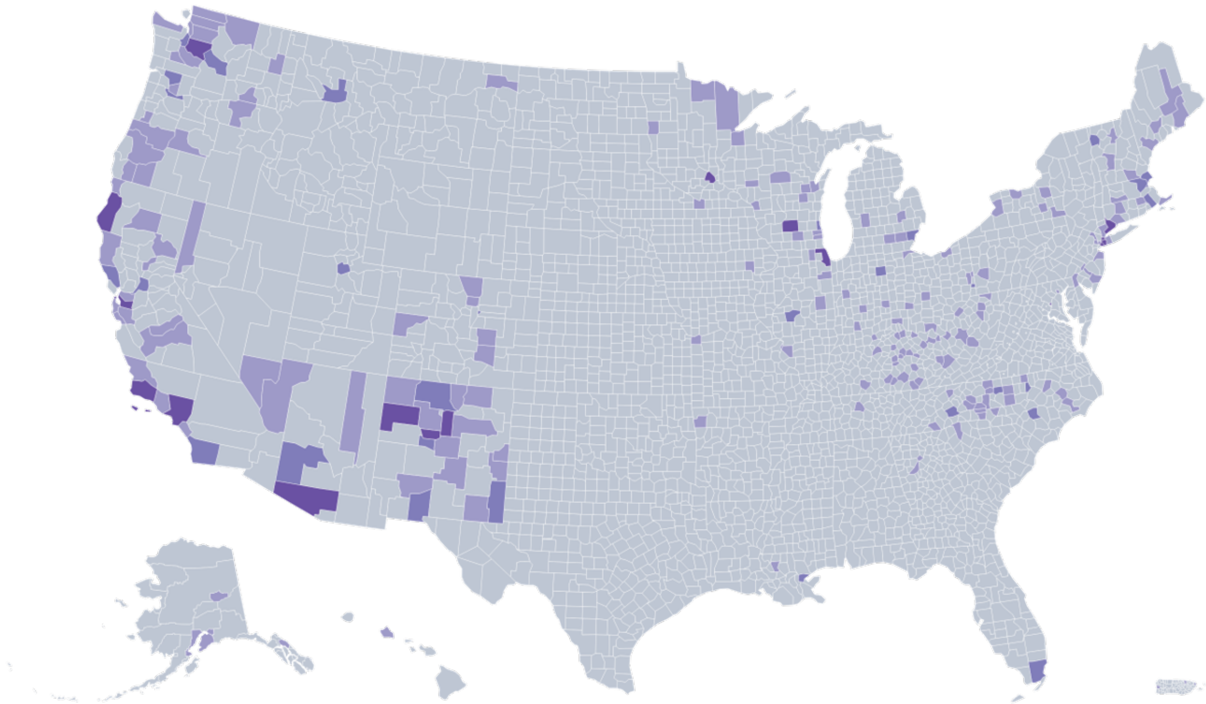
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Figure A1: Map of Portsmouth SEP Visitor Zip Codes



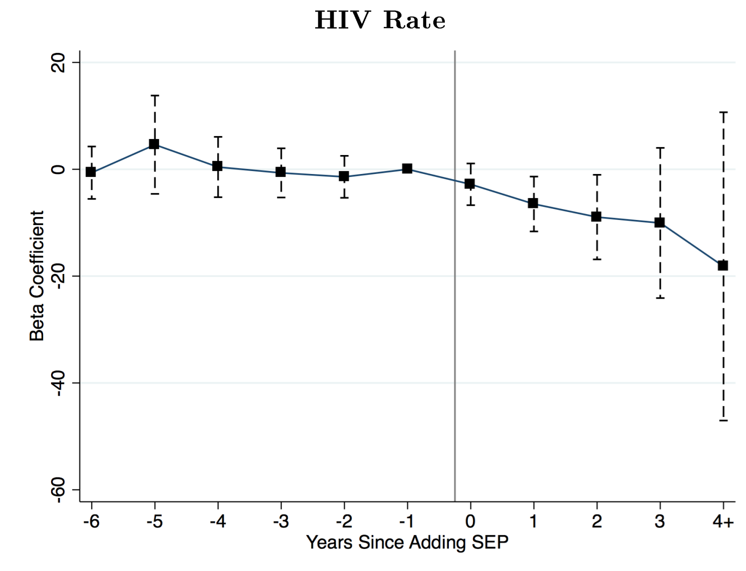
Notes: Geocoded clinic-level data on patient residence zip code is from the Portsmouth, Ohio SEP.

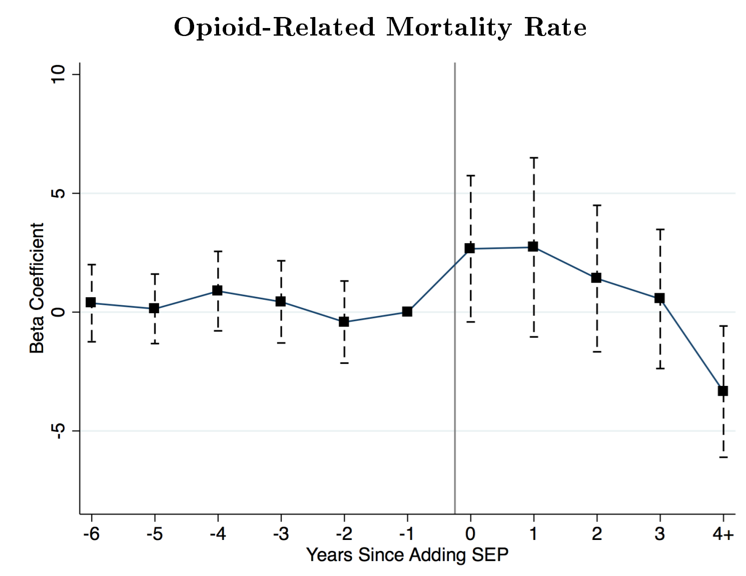
Figure A2: County-Level Locations of SEPs



Notes: Geocoded data on SEP location by county is from NASEN. Shaded counties represent those with SEPs as of 2016.

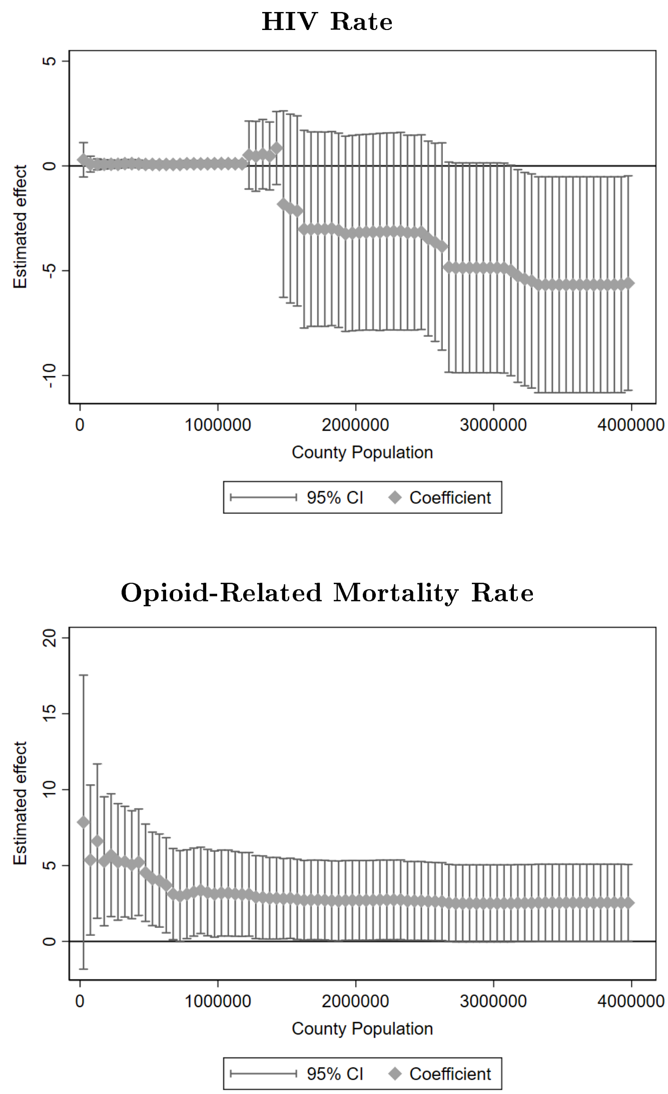
Figure A3: Difference-in-Differences Estimates, HIV and Opioid-Related Mortality, Using Data from 2003–2016





Notes: Each figure displays the coefficients and their respective 95% confidence intervals for the leading indicators and lagged treatment effects from OLS regressions, as specified in Equation 1. The vertical line represents the first year during the sample period that a county experienced a syringe exchange program opening. Estimates are based on restricted mortality files and CDC HIV diagnoses counts by county for the entire United States from 2003–2016. HIV diagnoses rates are from the Center for Disease Control and Prevention's NCHHSTP Atlas and 36 state agencies. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the county level.

Figure A4: Difference-in-Differences Estimates for HIV Rate and Opioid-Related Mortality Rate by Population Size



Notes: Each figure displays the coefficients and their respective 95% confidence intervals for the effects from OLS regressions, as specified in Equation 1, by population size. A x-axis value of "i" where $i = 25,000, 75,000, 125,000, ...4,000,000$ indicates an estimate from a difference-in-differences analysis comparing health outcomes in treated and comparison counties with less than i individuals. Estimates are based on restricted mortality files and CDC HIV diagnoses counts by county for the entire United States from 2008–2016. HIV diagnoses rates are from the Center for Disease Control and Prevention's NCHHSTP Atlas and 36 state agencies. Economic control variables include the county-level poverty rate and unemployment rate, demographic controls include percent Hispanic and percent black, and state-level policy controls include whether a state imposes quantitative prescription limit, tamper-resistant prescription forms, pain clinic regulations, patient identification requirements, doctor shopping restrictions, requirements with respect to physician examination or pharmacist verification, prescription drug monitoring programs, paraphernalia laws, and good Samaritan laws. Standard errors are clustered at the county level.