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Substance abuse treatment centers and local crime☆

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ABSTRACT

In this paper we estimate the effects of expanding access to substance-abuse treatment on local crime. We do so using an identification strategy that leverages variation driven by substance-abuse-treatment facility openings and closings measured at the county level. The results indicate that substance-abuse-treatment facilities reduce both violent and financially motivated crimes in an area, and that the effects are particularly pronounced for relatively serious crimes. The effects on homicides are documented in two sources of homicide data and are concentrated in highly populated areas.

1. Introduction

Drug-induced deaths in the United States have increased 280% since 1999 and now represent the largest major category of external causes of death by a wide margin: there were 47,055 deaths due to drug overdoses in 2014 compared to 32,675 due to motor vehicle accidents.[1] These facts underscore a growing need to understand how to reduce drug-related harms. Towards this end, a large body of work has shown that policies targeting the supply of illicit drugs are rarely effective.[2] In contrast, recent work indicates that expanding access to substance-abuse-treatment (SAT) facilities significantly reduces severe drug abuse, as measured by drug-induced mortality (Swensen, 2015). While this evidence highlights that investments in SAT can improve outcomes for some individuals, it does not necessarily reflect a broad-based benefit for communities that might be considering making such investments. In this paper we fill this important gap in the literature by estimating the effects of SAT facilities on homicide rates, which are especially high in urban areas, other violent crimes, and property crimes.[3]

There are several mechanisms through which SAT facilities may affect local crime. As outlined in Goldstein's (1985) influential tripartite conceptual framework for the drugs-violence nexus, drugs may affect violence through psychopharmacological effects, economically compulsive effects, and systemic effects. In these terms, SAT could be expected to reduce violence by: (i) reducing the use of drugs that lead to aggressive behavior (though there may be some offsetting effects caused by withdrawal), (ii) by reducing conflicts associated with financially motivated crimes committed by addicts seeking funds to buy drugs, and (iii) by reducing violence among and against those associated with the drug trade.[4] Moreover, drug-abuse treatment may reduce gun carrying through all three of these mechanisms, which could serve to reduce the amount—and intensity—of violence in communities. It is also important to keep in mind that a relatively large share of drug users have mental health problems that contribute to their addiction and to violent behaviors (Lavine, 1997; Hoaken and Stewart, 2003). As such, we could expect SAT to reduce violence because it can itself include—or can direct patients towards—treatment for underlying mental health problems that contribute to violence (Lavine, 1997; Marcotte and Markowitz, 2011). Finally, SAT treatment may reduce criminal activity through positive spillover effects on friends and family members of those receiving treatment.

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[1] See Rudd et al. (2016) and NCSA (2015).

[2] See for instance DiNardo (1993); Yuan and Caulkins (1998); Miron (2003); Cunningham and Liu (2003); Kuziemko and Levitt (2004); Dobkin and Nicosia (2009); Cunningham and Finlay (2013), and Dobkin et al. (2014).

[3] In 2012, the homicide rate was 7.4 per 100,000 in central metropolitan counties compared to 4.1 per 100,000 in other counties. These statistics are based on the Uniform Crime Reports data described in detail in Section 3.

[4] Prior studies have documented causal effects of drug activity on community violence by exploiting variation in drug use induced by price shocks (Markowitz, 2001; 2005) and by exploiting variation in the timing with which specific drugs became available across different cities (Evans et al., 2012; Fryer et al., 2013).

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Although these mechanisms highlight how SAT facilities can reduce crime through their effect on drug abuse, there are other mechanisms through which we might expect SAT facilities to *increase* local crime. Featuring prominently in not-in-my-backyard arguments against SAT facilities is the notion that such facilities pose risks by drawing into the area individuals who have relatively high rates of crime perpetration (drug users). Going beyond the idea of shifting crime perpetration from one place to another, SAT facilities could increase crime by altering the social and environmental context faced by drug users. That is, by altering the types of people and places that they encounter and with which they interact.

In this study we contribute to this policy debate by quantifying the effects of SAT facilities on crime. Specifically, we use annual county-level data on the number of SAT facilities to evaluate the degree to which crime rates change when SAT facilities open and close. We consider various crime outcomes measured over time at the county and law-enforcement agency level, based on data from the National Center for Health Statistics and the FBI's Uniform Crime Reporting Program. These panel data allow us to include a rich set of fixed effects (county/agency and state-by-year) and control variables (demographics, various measures of economic conditions, and law enforcement presence) in our models, so the estimates are identified based on plausibly exogenous variation. Several ancillary analyses support the validity of this research design, including analyses that demonstrate that outcomes in an area change after but not before the number of facilities change.

Our approach shifts the focus from the effects of SAT on those who receive treatment to the effects of SAT facilities on the communities they serve. This allows us to make several contributions. First, we consider outcomes that tend to be beyond the scope of randomized control trials (RCTs), which are limited by small samples, short follow-up periods, and the potential for false reporting. In particular, our approach allows us to consider severe-but-infrequent outcomes (e.g., homicide) and behaviors that individuals are likely to conceal (e.g., sexual assault). Second, our estimates reflect the effects of SAT on patients and the spillover effects onto the broader community, inclusive of any spillover effects on nearby friends and family and on the market for illegal drugs. In so doing, our estimates will allow for more comprehensive cost-benefit considerations. Third, whereas the nature of RCTs tends to require the use of small localized samples, which may have limited external validity, our use of administrative data allows us to obtain estimates that reflect the effects of SAT facilities across the United States.

Our analysis reveals significant and robust evidence that expanding access to SAT through additional treatment facilities reduces local crime. The effects appear to be particularly pronounced for relatively serious violent and financially motivated crimes: homicides, aggravated assaults, robbery, and motor vehicle theft. We do not find significant effects on more frequent but less serious crimes (simple assault, burglary, and larceny), nor do we find a significant effect on sexual assault. We show that the estimated effects on homicides are present across two different sources of homicide data and that they are concentrated in highly populated areas.[5]

Despite the various contributions of our research described above, there are some limitations that bear noting. First, our empirical approach, which focuses on county- and law-enforcement-agency-level aggregates, implies that we cannot separate the effects of SAT facilities on those who receive treatment from the effects of SAT facilities on the broader community. Our use of aggregate data also implies that we cannot separately identify effects for areas in a county that are nearer versus farther from a SAT facility. That said, we view these as a reasonable tradeoffs in order to be able to speak to the effects on the community as a whole. Second, while there is significant variation across SAT facilities in the types of treatment that they offer and in the number of patients they can treat, our estimates will reflect an average of the effects of these facilities. Finally, openings and closings of SAT facilities are not random. While this has the potential to compromise our ability to identify causal effects, our ancillary analyses, which are discussed in detail in subsequent sections, demonstrate that it is unlikely in light of our empirical strategy.

2. Background

2.1. Substance abuse and treatment

According to the National Survey of Drug Use and Health over 21.5 million people in the U.S. are classified as having a substance-use disorder (CBHSQ, 2015).[6] A high incidence of substance abuse is also apparent in crime perpetration, with 40% of convicted violent criminals being under the influence of alcohol and nearly 60% of all arrestees testing positive for some illicit substance at the time of arrest.[7] The annual societal costs of drug abuse solely in terms of drug-related crime are estimated at over 56 billion dollars.[8]

Though substance-abuse treatment is a promising avenue to reduce these costs, treatment rates for those in need remain very low. In 2014, 85% of those abusing or dependent on an illicit substance did not receive treatment and 91% of those abusing or dependent upon alcohol did not receive treatment. Moreover, despite the prevalence of alcohol and drugs among arrestees, 70% of arrestees have never been in any form of drug or alcohol treatment (ONDCP, 2014). Notably, recent changes brought about by the Affordable Care Act are expected to increase coverage and take-up of treatment (Buck, 2011; Beronio et al., 2014).

In this context, the number of substance-abuse treatment facilities may be a particularly relevant policy parameter. In the United States, over 14,500 stand-alone treatment facilities are the primary setting for delivery of substance-abuse treatment, offering a wide range of drug-treatment programs and related services (SAMHSA, 2014). Local treatment centers most commonly offer outpatient care to deliver treatment programs such as detoxification, methadone maintenance, regular outpatient, adolescent outpatient, and drug-court programs (SAMHSA, 2014). For more serious substance-abuse problems, facilities provide residential treatment in which clients temporarily live at the treatment site (e.g. inpatient detoxification, chemical dependency programs, therapeutic communities). While treatment programs vary substantially and often target particular demographic groups or specific drug addictions, all treatment approaches share similar goals to mitigate the consequences of drug abuse and encourage healthier lifestyles. According to the National Survey of Drug Use and Health (2015), 62% of individuals undergoing treatment reported receiving treatment for alcohol, 21% reported receiving treatment for marijuana, 18% reported receiving treatment for pain relievers, 14% reported receiving treatment for cocaine, 13% reported receiving treatment for heroin, and 11% reported receiving treatment for stimulants such as methamphetamines.

More broadly, the substance-abuse treatment industry includes profit, non-profit, and public providers, the bulk of which (87%) are privately-owned facilities.[9] Though the objective functions of facilities may differ somewhat by ownership status and treatment focus, the

[5] In an earlier version of this study (Bondurant et al., 2016), we updated Swensen's (2015) analysis and showed that the impacts on drug abuse—as measured by drug-induced mortality—are readily apparent in an analysis that uses the same years of data as our analysis of crime. These results indicate a 0.50% decline in drug-induced mortality rates associated with an additional SAT facility in a county, a bit larger than the estimated effect of 0.42% reported in Swensen (2015).

[6] Based on criteria specified in the Diagnostic and Statistical Manual of Mental Disorders, 4th edition (DSM-IV).

[7] See https://ncadd.org/about-addiction/alcohol-drugs-and-crime.

[8] Estimates based on the 2011 National Drug Threat Assessment conducted by the National Drug Intelligence Center.

[9] According to the 2013 National Survey of Substance Abuse Treatment Services, 60% of facilities are nonprofit, 30% are for profit, and 10% are public.

decision to open or close a treatment facility likely depends crucially on (i) a perceived need for treatment providers or opportunities to improve upon currently offered treatment services and (ii) the ability to secure funding for treatment services from either public or private third-party payers (SAMHSA, 2011). Given the high need for addiction treatment and existing evidence of binding treatment capacity constraints and long wait lists, the availability of funds is particularly relevant when considering the predictors of facility openings and closings.[10]

Unlike general health care, which relies on funding through insurance mechanisms, substance-abuse treatment relies primarily on public funding in the form of federal block grants and state subsidies. That said, recent mental health parity legislation and the rise of managed-care contracts have increased the importance of public and private insurance revenue to providers (Horgan and Merrick, 2001; Olmstead et al., 2004). Assuming these sources of financing generally increase with drug abuse and related problems, analyses of the effect of treatment provision on drug-related outcomes may understate the actual effect of treatment.

2.2. Related literature on SAT and crime

An extensive literature has evaluated the relationship between substance-abuse treatment programs, drug related outcomes, and criminal activities, including some that use “the gold standard” for empirical research, randomized control trials (RCTs). In a widely-cited meta analysis, Prendergast et al. (2002) reviewed 78 studies of SAT, 60% of which used random or quasi-random assignment to treatment and 25 of which examined crime outcomes. They report that “drug abuse treatment has both a statistically significant and a clinically meaningful effect in reducing drug use and crime, and that these effects are unlikely to be due to publication bias.” The estimates indicate an average 13% decline in criminal involvement as a result of treatment.[11] More recent reviews of specific treatment approaches provide consistent evidence that criminal involvement declines during treatment and mixed evidence when considering longer-run crime outcomes (Amato et al., 2005; Holloway et al., 2006; Egli et al., 2009).

The existing literature also adds insight into the efficacy of specific treatment settings in reducing drug-related crime. Some of the more convincing and consistent evidence comes from studies evaluating prison-based drug treatment. This is partly due to the relative ease of employing a randomized treatment design and the ability to consider recidivism rates rather than relying on self-reported criminal activity.[12] Summarizing the literature, Mitchell et al. (2012) review 74 studies of prison-based treatment programs and conclude that substance-abuse treatment for inmates reduces recidivism by 15%. Existing evidence also suggests that court-mandated treatment programs, which account for a third of all treatment admissions, can be effective in reducing crime.[13] For instance, Wilson et al. (2006) identify and review 55 quasi-experimental and experimental evaluations of drug courts. They concluded that court-referred treatment does lower re-arrest rates though the estimated effects were notably smaller and less precise among evaluations that employed randomization. They also find consistent evidence of declines in re-offending both during and following court-referred treatment programs, however the estimated effects do decay over time.

Together, this literature provides consistent evidence that treatment programs can reduce crime. While these studies have made significant contributions to our knowledge, the merit of our study is predicated on the notion that some of the most important questions about the effects of SAT are only likely to be answered using alternative methods applied to observational data. In particular, our study shifts the focus from the effects of SAT on those who receive treatment to the effects of SAT facilities on the communities they serve and uses data that allow us to obtain estimates that reflect the effects of SAT facilities on local-area crime across the United States.

To our knowledge only one other recent working paper attempts to consider the effects of SAT on crime in such a comprehensive fashion. Wen et al. (2014) consider the effects of changes in SAT rates on property and violent crimes using data collected by the FBI that span the United States. Their instrumental variables approach relies on the assumption that state health insurance expansions (made possible through Health Insurance Flexibility and Accountability waivers) only relate to changes in crime through their impacts on SAT.[14] This assumption could be violated if, for example, expanding access to health insurance affects crime through its impact on treatment for mental health problems or through its impacts on overall health and well being. As all observational studies rely on fundamentally untestable assumptions, and as any body of evidence is more compelling when similar results are documented using approaches that rely on different assumptions, we view our work as an important contribution that complements this prior study, which reports that increases in substance-use-disorder treatment significantly reduces robbery, aggravated assault, and larceny.

3. Data

Following Swensen (2015), we identify county-level changes in the number of substance-abuse treatment facilities using data from the U.S. Census Bureau’s County Business Patterns (CBP). The CBP data reports the annual number of substance-abuse treatment clinics (a single physical location) in each U.S. county for both outpatient and residential facilities from 1999 to 2012.[15] Although classified separately in the CBP data, residential and outpatient establishments often offer both residential and outpatient treatment services with 90% of all admissions occurring in an outpatient setting (SAMHSA, 2014). Therefore, estimating the effects separately for outpatient and residential facilities would not be informative as residential and outpatient services are not distinctly identified. As such, we combine outpatient and residential classifications using the total count of establishments as an indicator for county-level provision of substance-abuse treatment.

To estimate the effect of treatment facilities on local-area crime we merge CBP data with several independent data sources for criminal activity. We use two datasets to investigate impacts on homicides, one of which we also use to investigate a wide variety of crimes. First, we use annual county-level mortality data from the National Center for Health Statistics (NCHS) Multiple Cause of Death Data to analyze homicides.[16] We combine these data with county-year population counts

[10] Evidence suggests that capacity concerns and being put on a wait list are important barriers to treatment enrollment (Appel et al., 2004; Friedmann et al., 2003; Pollini et al., 2006). Relatedly, Dave and Mukerjee (2011) analyze the effect of state legislation that reduces out-of pocket costs for mental health and substance-abuse treatment and find a relatively small effect on treatment admissions. They argue that the effect on admissions is muted, in part, because of treatment capacity constraints suggested by limited growth in the number of treatment facilities and increasing treatment waiting periods.

[11] Crime outcomes included self-reported crimes and official records on arrest, conviction and incarceration. As such, this review includes evidence from crime outcomes during and after treatment.

[12] Treatment rates increased by 34% among state inmates and 90% among federal inmates from 1997 to 2004 (Mumola and Karberg, 2006). Core funding for these increases has come from the federal government through the Residential Substance Abuse Treatment (RSAT) initiative and funding for drug courts through the Bureau of Justice Administration (Taxman et al., 2007).

[13] See SAMHSA (2014) for a breakdown of admissions by treatment referral source.

[14] They also use as an instrumental variable state-level mandates requiring private group health plans to provide benefits for substance-use disorder treatment that are no more restrictive than the benefits for medical insurance parity mandates; however, it is always used in conjunction with the waiver expansion instrument, presumably due to a lack of independent power.

[15] The following six-digit NAICS codes identify treatment establishments: 621420—“Outpatient mental health and substance abuse centers” and 623220—“Residential mental health and substance abuse facilities.”

[16] NCHS homicides include deaths by another person with the intent to injure or kill. They do not include homicides due to legal intervention, operations of war, or homicides

from the National Cancer Institutes's Surveillance Epidemiology and End Results (Cancer-SEER) program to construct mortality *rates*. We also use these population data to create county-by-year controls for demographic characteristics.[17]

Our second source of crime data is based on the Uniform Crime Reports (UCR), which are a compilation of crime statistics reported by local law-enforcement agencies across the United States to the FBI. Specifically, we use the offenses known data from the Offenses Known and Cleared by Arrests UCR segment. These data, which we will refer to as UCR Offenses Known, include the most commonly reported violent and property crimes including criminal homicide, sexual assault, robbery, assault, burglary, larceny theft, and motor vehicle theft. We focus on known offenses in order to capture crimes that come to the attention of law enforcement, as opposed to alternative data sets that are available but are restricted to crimes that have been cleared by arrest. We use these data in conjunction with the UCR's estimates of the population covered by an agency in a given year to construct annual *agency*-level crime rates. We restrict our UCR sample to agencies that cover a single county and agency-years in which agencies are reporting the full 12 months of crime to the UCR program. We link the UCR agency-level data with county-level CBP data using the primary county in which each municipality resides and calculate crime rates using the annual reported population covered by each municipal agency.[18]

We restrict our analysis to U.S. counties with at least one treatment facility over the 1999–2012 time period and counties with available identifiers in the 48 contiguous states.[19] The resulting data include treatment facility, mortality, and crime data in 48 states, spanning 14 years. In Table 1 we present summary statistics for our sample, weighted by the relevant populations. CBP data indicate that counties have a population-weighted average of 49.5 SAT facilities. Importantly, there is substantial variation in the number of facilities with the average county experiencing 5.8 net facility openings and 3.7 net closings from 1999 to 2012, where a net opening is an observed increase in the number of facilities from one year to the next and a net closing is defined similarly. For reference, Table 1 also shows summary statistics for each mortality and crime outcome used in our analysis. Summary statistics for the control variables we use in our analysis are shown in the Online Appendix in Table A1.

Table 1
Summary statistics.

	Mean	Std Dev
Substance abuse treatment facilities (2,453 counties)		
Total	49.6	90.1
Net openings	5.8	10.1
Net closings	3.7	4.4
Facilities per 100,000	5.0	3.6
NCHS mortality files (2453 counties)		
Homicides per 100,000	5.8	5.1
UCR offenses known database (2184 counties, 9602 agencies)		
Homicides per 100,000	5.7	8.5
Sexual assaults per 100,000	32.0	26.8
Aggravated assaults per 100,000	309.2	288.0
Robbery per 100,000	164.8	180.0
Simple assaults per 100,000	1118.2	878.7
Burglary per 100,000	762.1	523.2
Larceny per 100,000	2551.8	1479.1
Motor vehicle theft per 100,000	424.9	458.7

Notes: These data span 1999–2012. The means and standard deviations for the substance-abuse treatment facilities are derived from the NCHS Mortality sample. The reported facility statistics are similar when using the UCR Known Offenses sample. The means and standard deviations from the NCHS Restricted Mortality Files represent rates per 100,000 residents in each county and are weighted by county population. The means and standard deviations for the UCR Offenses Known Database rates per 100,000 residents covered by the municipal law enforcement agency and are weighted by agency population coverage.

4. Empirical approach

We identify the effects of SAT facilities using year-to-year variation within counties driven by facility openings and closings, controlling for state-by-year shocks common to areas within a state in addition to time-varying county characteristics. As we analyze both county and agency-level outcomes, we operationalize this strategy using a regression model that includes either county or agency fixed effects in addition to state-by-year fixed effects and county-year covariates:

$$y_{ast} = \theta Facilities_{cs,t-1} + \alpha_{as} + \alpha_{st} + \beta X_{cst} + \epsilon_{ast},$$

where y_{ast} represents outcomes in area a (either county or agency) in state s in year t. We use log rates to measure crime outcomes. We add one to all outcome counts before constructing log rates to avoid dropping area-year observations for which the outcome would otherwise be undefined, but we show that results of all of our analyses are similar if we instead simply focus on areas that always have a positive count, with the sample being defined separately for each outcome considered. We also show that results are similar using an inverse hyperbolic sine transformation instead of adding one before taking the log of counts. In support of using a log transformation, we have verified that Poisson models (where computationally feasible) yield very similar estimates. $Facilities_{cs,t-1}$ represents the number of SAT facilities in county c in state s in year t-1, α_{as} are area fixed effects, α_{st} are state-by-year fixed effects, and X_{cst} includes county unemployment rates, the number of firm births, number of law enforcement officers per 100,000, and the fraction of the county population that is: white, black, male, less than 10 years old, 10–19 years old, ... , 60–69 years old.[20,21] Finally, ϵ_{ast} is a random error term that we allow to be correlated within a county across years, and across all counties in any given year by estimating two-way standard errors following Cameron et al. (2011).[22] To be clear, our measure of facilities is a county-level measure even when we are considering crimes at the agency level. We also note that our main results are based on regressions that weight by the relevant population size in

(*footnote continued*)
from the Sept. 11, 2001 attacks.

[17] As reported by Stevens et al. (2015), the Cancer-SEER population data are more accurate than data interpolated from the Census because they "are based on an algorithm that incorporates information from Vital statistics, IRS migration files, and the Social Security database."

[18] The UCR Offenses Known data used in this study were collected and compiled by the Inter-University Consortium for Political and Social Research (ICPSR).

[19] Specifically, we drop all counties in HI and AK and combine counties that experience boundary changes over time. This involves combining Adams, Broomfield, Boulder, Jefferson, and Weld in Colorado; Prince George's and Montgomery in Maryland; Gallatin and Yellowstone National Park in Montana; Craven and Carteret in North Carolina; Alleghany and Clifton Forge in Virginia; Augusta and Waynesboro in Virginia; Bedford and Bedfort City in Virginia; Halifax and South Boston City in Virginia; Prince William and Manassas Park in Virginia; Southampton and Franklin in Virginia; and York and Newport News in Virginia.

[20] County unemployment rates are from the BLS Local Area Unemployment Statistics. Firm births are the number of firms reporting positive employment for the first time, as reported by the U.S. Census Statistics of U.S. Businesses. The number of law-enforcement officers per 100,000 residents are calculated using the UCR agency-specific employment reports available in the Law Enforcement Officers Killed and Assaulted (LEOKA) database. For our county level analysis, we use aggregated agency-level data.

[21] Our choice to use the number of facilities in the prior year as opposed to the number per capita is supported by an ancillary analysis of drug-induced mortality. Drug-induced mortality clearly responds to the number of facilities (as previously shown in Swensen, 2015), less so to facilities per capita. This is likely to be in part explained by larger areas tending to have larger facilities. In any case, this finding supports the idea that the number of facilities is a stronger predictor of utilization than a per-capita measure. We have also investigated models that include the number of facilities squared but never find its corresponding parameter estimate to be statistically significant, whether evaluating drug-induced mortality or crime outcomes. This suggests that any diminishing returns that may exist are not large enough to be detected using our data and identification strategy.

[22] That is, we estimate two-way standard errors clustered on counties and years. This approach yields more conservative estimates than estimates that solely cluster on counties, reflecting that there are unobserved shocks to outcomes that span counties.

Table 2
Estimated effects of SAT facilities on log homicide rates.

	(1)	(2)	(3)	(4)	(5)
Homicide data: NCHS restricted mortality files					
Facilities last year	−0.0022***	−0.0028***	−0.0025***	−0.0025***	−0.0025***
	(0.0007)	(0.0005)	(0.0004)	(0.0004)	(0.0004)
Homicide data: UCR offenses known database					
Facilities last year	−0.0019***	−0.0020***	−0.0016***	−0.0016***	−0.0016***
	(0.0005)	(0.0004)	(0.0003)	(0.0003)	(0.0003)
County/agency and year fixed effects	Yes	Yes	Yes	Yes	Yes
State-by-year fixed effects	No	Yes	Yes	Yes	Yes
Demographic controls	No	No	Yes	Yes	Yes
Economic controls	No	No	No	Yes	Yes
Officer rate per 1000	No	No	No	No	Yes

Notes: Estimates are based on 34,326 county-year observations for the NCHS Restricted Mortality Files and 106,965 agency-year observations for the UCR Offenses Known Database. Demographic control variables include the fraction of the population that are: white, black, male, ages 0–9, ages 10–19, ages 20–29, ages 30–39, ages 40–49, ages 50–59, and ages 60–69. Controls for economic conditions include the county unemployment rate and number of firm births. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions are weighted by county population when using the NCHS Mortality data and are weighted by agency population coverage when using the UCR Offenses Known data.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

order to improve efficiency though we subsequently explore unweighted estimates.

Our focus on within-area variation accounts for fixed characteristics of areas (both observable and unobservable) that may be correlated with the number of SAT facilities in a county and with our outcomes of interest. For example, this approach will address the fact that there are inherent differences between urban and rural counties. The inclusion of state-by-year fixed effects account for aggregate time-varying shocks, such as aggregate economic conditions or changes in the national drug-control strategy.[23] They also control for state-specific shocks such as changes in state funding for law enforcement services. The controls for unemployment rates and firm births account for the possibility that our outcomes of interest and treatment facilities may both be related to local economic conditions. The controls for demographics account for the possibility that compositional changes in a county's population may affect outcomes and investments in SAT facilities.

Our empirical approach closely follows Swensen (2015), who also conducts several ancillary analyses in support of the validity of the research design for estimating effects on drug-induced mortality. In particular, Swensen demonstrates that additional facilities lead to increases in treatment admissions and that the effects of additional facilities are greatest for causes of death that are most closely related to drug abuse.[24] To address concerns regarding reverse causality, Swensen plots drug-induced mortality rates leading up to and following changes in the number of facilities and finds no visual evidence of changes in drug-related mortality prior to changes in the number of facilities. Furthermore, his estimates from models that consider additional lags and leads of treatment facilities show that the previous- and current-year changes in the number of facilities is significantly related to drug-induced mortality, but that drug-induced mortality is not related to the number of facilities in future periods.[25] In a similar fashion, we estimate a version of Eq. (1) that also considers the effect of the number of facilities in the current, previous and subsequent years on the outcomes that are the focus of this paper. The results of this analysis, discussed in more detail below, indicate that changes in the number of treatment facilities are also not driven by recent changes in crime.

We note that a third of all treatment admissions are court-ordered, often as an alternative to incarceration. This is potentially important because links from increased crime to increased incarceration to increased SAT facilities could cause our empirical strategy to understate the reductions in crime generated by SAT facilities. Alternatively, links from increased incarceration to reduced crime (through incapacitation effects) and to SAT facilities could cause our empirical strategy to overstate the reductions in crime generated by increased SAT facilities. While we cannot rule out either of these possibilities, we note that any such changes would have to be happening differentially across counties within the same states to generate bias (because we control for state-by-year fixed effects). We also note that persistent shocks generating these sorts of relationships would be expected to generate significant links between current crime rates and future levels of SAT. Reassuringly, we do not find evidence of any such links.

5. Estimated effects on crime

5.1. Homicides

Before turning to estimates that are based on Uniform Crime Reports data, we begin with an analysis of homicide deaths recorded in NCHS mortality data. Though these also include justified homicides, 94% are unjustified criminal homicides and, as such, they can shed light on the degree to which treatment interventions affect the most serious and costly form of criminal activity.[26] The results of this analysis, shown in the first panel of Table 2, provide causal evidence that county-level homicide rates are reduced by SAT facilities. Specifically, the estimates indicate a 0.25% decline in intentional homicide death rates associated with an additional SAT facility. These estimates are similar across specifications with limited control variables and richer sets of control variables.

In the second panel of Table 2 we investigate the effects on

[23] For instance, state-by-year fixed effects control for nationwide effects of the substantial increases in federal funding for substance-abuse treatment services for inmates through the Residential Substance Abuse Treatment (RSAT) initiative and funding for drug courts through the Bureau of Justice Administration.

[24] Swensen uses data on admissions into facilities receiving public funding to offer "proof of concept" that increases in treatment facilities leads to a change in an underlying factor associated with treatment. Notably, other mechanisms—including perceptions toward treatment or factors influencing the quality and accessibility treatment—may also contribute to declines in substance abuse as treatment services expand.

[25] Swensen also estimates models using demand-side characteristics to predict treatment facility openings in order to offer insight into the degree to which treatment provision responds to changes in the demand for addictive substances. His results suggest that the number of treatment facilities varies directly with measures that proxy for the demand for addictive substances, he argues that not adequately accounting for these correlations would understate the effect of an additional treatment facility on drug-related mortality.

(*footnote continued*)

[26] For a breakdown of justified and unjustified homicides in 2013, see https://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2013/crime-in-the-u.s.-2013/offenses-known-to-law-enforcement/expanded-homicide.

Table 3
Estimated effects of SAT facilities on log violent crime rates .

	(1)	(2)	(3)	(4)	(5)
Homicides					
Facilities last year	−0.0019***	−0.0020***	−0.0016***	−0.0016***	−0.0016***
	(0.0005)	(0.0004)	(0.0003)	(0.0003)	(0.0003)
Sexual assaults					
Facilities last year	−0.0010**	−0.0004	−0.0004	−0.0004	−0.0005
	(0.0004)	(0.0003)	(0.0005)	(0.0005)	(0.0005)
Aggravated assaults					
Facilities last year	−0.0034***	−0.0021***	−0.0011*	−0.0012*	−0.0012*
	(0.0009)	(0.0007)	(0.0006)	(0.0006)	(0.0006)
Simple assaults					
Facilities last year	−0.0007	0.0004	0.0001	0.0001	0.0000
	(0.0006)	(0.0003)	(0.0004)	(0.0004)	(0.0004)
Agency and year fixed effects	Yes	Yes	Yes	Yes	Yes
State-by-year fixed effects	No	Yes	Yes	Yes	Yes
Demographic controls	No	No	Yes	Yes	Yes
Economic controls	No	No	No	Yes	Yes
Officer rate per 1000	No	No	No	No	Yes

Notes: Estimates are based on 106,965 agency-year observations. Demographic control variables include the fraction of the population that are: white, black, male, ages 0–9, ages 10–19, ages 20–29, ages 30–39, ages 40–49, ages 50–59, and ages 60–69. Controls for economic conditions include the county unemployment rate and number of firm births. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions are weighted by agency population coverage.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 4
Estimated effects of SAT facilities on log financially-motivated crime rates .

	(1)	(2)	(3)	(4)	(5)
Robbery total					
Facilities last year	−0.0016***	−0.0019***	−0.0011***	−0.0011***	−0.0011***
	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0002)
Motor vehicle theft					
Facilities last year	−0.0005	−0.0019***	−0.0012**	−0.0012**	−0.0012**
	(0.0011)	(0.0005)	(0.0004)	(0.0004)	(0.0004)
Burglary total					
Facilities last year	−0.0010***	−0.0010***	−0.0005	−0.0005	−0.0005*
	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Larceny theft					
Facilities last year	−0.0002	0.0001	−0.0006	−0.0006	−0.0006
	(0.0004)	(0.0004)	(0.0005)	(0.0005)	(0.0005)
Agency and year fixed effects	Yes	Yes	Yes	Yes	Yes
State-by-year fixed effects	No	Yes	Yes	Yes	Yes
Demographic controls	No	No	Yes	Yes	Yes
Economic controls	No	No	No	Yes	Yes
Officer rate per 1000	No	No	No	No	Yes

Notes: Estimates are based on 106,965 agency-year observations. Demographic control variables include the fraction of the population that are: white, black, male, ages 0–9, ages 10–19, ages 20–29, ages 30–39, ages 40–49, ages 50–59, and ages 60–69. Controls for economic conditions include the county unemployment rate and number of firm births. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions are weighted by agency population coverage.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

homicide rates using law-enforcement-agency-level data from the UCR's Offenses Known database. We estimate similar models when using these data, just modifying them to reflect that they are agency-year data by using agency fixed effects instead of county fixed effects and using agency covered population as the denominator to construct homicide rates. Analyses of these data continue to indicate that SAT facilities significantly reduce homicides in areas covered by municipal law-enforcement agencies, though the estimates are somewhat smaller, indicating a 0.16% decline in intentional homicide death rates associated with an additional SAT facility.[27]

5.2. Violent crimes more broadly

Having established that SAT facilities reduce the most costly of crimes (homicides), we next consider the degree to which treatment facilities affect other types of violent crime. In Table 3 we show a detailed breakdown of the effects of SAT facilities on violent crimes based on analyses of the UCR Offenses Known data.[28] While we focus our discussion below on the point estimates from models with the richest set of controls (Column 5), we note that the estimated effects are similar across specifications once state-by-year fixed effects and demographic controls are included as covariates. The estimates are not sensitive to the inclusion of other county-year control variables.

[27] In an earlier version of this study (Bondurant et al., 2016), we also investigated the impacts on homicides using the UCR's Supplementary Homicide Reports (SHR) database, an incident-level dataset that includes detailed information on each homicide as voluntarily reported by agencies participating in the UCR program. The results of this the analysis indicated that effects of SAT facilities on homicides are concentrated among homicide incidents in which the relationship to the offender was unknown or in which the offender was a friend.

[28] In most cases each outcome represents a distinct incident as 85% of UCR incidents are single-offense incidents, where an incident is a distinct time, place, victim (for crimes against the person), and offender. In cases of multiple-offense incidents, agencies are instructed to report the most severe offense according to the UCR hierarchy rule (CJIS, 2013).

Table 5
Expanding model to additionally consider contemporaneous and future facility counts .

	Homicide (NCHS Data)	Homicide (UCR Data)	Sexual assault	Aggravated assault	Simple assault	Robbery	Motor vehicle theft	Burglary	Larceny theft
Facilities last year	−0.0010*	−0.0016***	−0.0008	−0.0010	−0.0002	−0.0007**	−0.0006	−0.0003	−0.0003
	(0.0005)	(0.0005)	(0.0005)	(0.0008)	(0.0003)	(0.0003)	(0.0005)	(0.0003)	(0.0019)
Facilities this year	−0.0014*	−0.0002	0.0001	0.0003	0.0001	−0.0002	−0.0004	−0.0002	−0.0011
	(0.0008)	(0.0007)	(0.0004)	(0.0009)	(0.0002)	(0.0003)	(0.0004)	(0.0003)	(0.0022)
Facilities next year	−0.0005	0.0002	0.0004	−0.0008	0.0003	−0.0003	−0.0005	−0.0001	0.0009
	(0.0008)	(0.0007)	(0.0004)	(0.0005)	(0.0003)	(0.0003)	(0.0004)	(0.0002)	(0.0010)

Notes: Estimates are based on 34,326 county-year observations for the NCHS Restricted Mortality Files in the first column and 106,965 agency-year observations for the UCR Offenses Known Database for the remaining columns. Outcomes are in log rates. All estimates control for county fixed effects, year fixed effects, state-by-year fixed effects, demographic controls, economic controls, and the size of the police force in the area. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions are weighted by the population represented by each cell.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Across the first four panels of Table 3, we sequentially report the estimated effects on violent crimes of decreasing severity according to social cost estimates reported in McCollister et al. (2010): homicides ($9,881,198 per incident), sexual assault ($264,854), aggravated assault ($117,722), and simple assault.[29] We defer our consideration of robbery until the next section where we focus on financially motivated crimes. As mentioned above, the estimated effect on homicides indicates a significant reduction caused by SAT facilities. While the point estimate for the effect on sexual assault is also negative, suggesting that SAT facilities reduce sexual assault as well, it is not close to being statistically significant at conventional levels. The estimated effect on aggravated assaults also suggests a reduction in crime associated with SAT facilities, though this estimate is only marginally statistically significant. Finally, the estimates suggest no effect on simple assaults.

5.3. Financially motivated crimes

Table 4 shows the estimated effects on financially motivated crimes. We again sequentially report the estimated effects on crimes of decreasing severity according to social cost estimates: robbery ($46,541), motor vehicle theft ($11,849), burglary ($7,108), and larceny ($3,885). As with the estimated effects on violent crimes, these estimates suggest more pronounced effects of SAT facilities on relatively serious crimes. The point estimates indicate that a SAT facility reduces robbery by 0.11%, motor vehicle theft by 0.12%, burglary by 0.05%, and larceny by 0.06%. The estimated effects larceny are not statistically significant at conventional levels.

5.4. Assessing endogeneity and lag structure

As discussed in Section 4, the main threat to the validity of our empirical strategy is the possibility that changes in the number of facilities in an area might be driven by trends in the outcomes we consider (or the correlates thereof) and/or recent shocks to the outcomes we consider (or the correlates thereof). To the degree to which such trends and/or shocks occur at the state level or relate to changing demographics, economic conditions, or the size of police forces, they should be captured by state-year fixed effects and the control variables included in our analysis. As this is fundamentally untestable, we propose a test of the validity of our identification strategy based on examining the lead and lag structure of the estimated effects. Specifically, we estimate versions of Eq. (1) that consider the link between our outcome variables and the number of SAT facilities in a county *in a future year*.

We also expand on Eq. (1) to consider contemporaneous versus lagged measures of SAT facilities. We do so in order to evaluate our choice to focus on the number of facilities in the prior year as our main variable of interest, a choice we made to avoid attenuation bias that would likely be caused by the fact that newly opened (or closed) facilities would only affect counties for some fraction of the year.

Table 5 shows estimates of this type for all of the outcomes considered across Tables 2 through 4. Specifically, it shows estimates based on our richest model while additionally considering the number of facilities in the current year and in the future year. Across the 9 outcomes we consider, the estimated effects of the number of facilities one year in the future are *never* statistically significant, even at the ten-percent level. We interpret these results as evidence that reverse causality, or the possibility that changes in the number of SAT facilities may be driven by recent changes in drug abuse and related outcomes, is not a major concern. As such, these results provide support for a causal interpretation of our main results.

These results also provide support for our focus on the lagged measure of facilities. In particular, where we see significant effects on outcomes, the number of treatment facilities in the prior year has a stronger effect than the number of treatment facilities in a given year in all but one instance. Moreover, the estimated effect of the number of treatment facilities in the current year is usually not statistically significant, suggesting that the effects are more likely to be driven by successful treatment as opposed to incapacitation effects.[30]

5.5. Alternative empirical approaches

As an additional test of the robustness of our estimates, in Panel A of Table 6 we show the estimated effects for each outcome based on the subset of areas for which the log outcome rate can be defined in each year without adding one.[31] For nearly all of the outcomes we consider, these estimates are virtually the same as our main results in both statistical and economic significance.

Panel B of Table 6 shows estimates that transform crime counts using the inverse hyperbolic sine function as an alternative to adding one before taking the log.[32] For all of the outcomes we consider, this approach yields estimates that are very similar or larger in magnitude to our main results.

Panel C of Table 6 shows estimates that do not use population weights. Notably, the estimated effects on homicide are smaller in

[29] Note that we have adjusted the cost estimates for inflation to put the amounts in 2016 dollars. McCollister et al. (2010) do not include estimates for simple assault.

[30] Further results along these lines are presented in the Online Appendix in Tables A2 through A4. In these tables, we explore models that consider alternative lag and lead specifications including an additional lead indicator. The results of these analyses lead to the same conclusions as in Table 5.

[31] As such, the set of areas contributing to the estimates varies across outcomes, with fewer areas contributing to the estimates focusing on rarer outcomes such as homicides.

[32] Specifically, the outcome variable we use here is $ln\left(\frac{count + \sqrt{count^2 + 1}}{population}\right)$.

Table 6
Estimates using alternative approaches .

Panel A: Restricting sample to areas reporting positive counts in all years									
	Homicide (NCHS Data)	Homicide (UCR Data)	Sexual assault	Aggravated assault	Simple assault	Robbery	Motor vehicle theft	Burglary	Larceny theft
Facilities last year	−0.0024**	−0.0018***	−0.0005	−0.0012**	0.0001	−0.0009***	−0.0011**	−0.0005	−0.0001
	(0.0010)	(0.0005)	(0.0005)	(0.0006)	(0.0003)	(0.0002)	(0.0004)	(0.0003)	(0.0002)
N	9145	4878	26,772	67,357	89,722	39,382	66,040	93,240	100,647
Panel B: Inverse hyperbolic sine estimates using full sample									
	Homicide (NCHS Data)	Homicide (UCR Data)	Sexual assault	Aggravated assault	Simple assault	Robbery	Motor vehicle theft	Burglary	Larceny theft
Facilities last year	−0.0026***	−0.0019***	−0.0006	−0.0012*	0.0000	−0.0011***	−0.0012**	−0.0005*	−0.0007
	(0.0004)	(0.0003)	(0.0005)	(0.0006)	(0.0004)	(0.0003)	(0.0004)	(0.0003)	(0.0006)
N	34,326	106,965	106,965	106,965	106,965	106,965	106,965	106,965	106,965
Panel C: Unweighted									
	Homicide (NCHS Data)	Homicide (UCR Data)	Sexual assault	Aggravated assault	Simple assault	Robbery	Motor vehicle theft	Burglary	Larceny theft
Facilities last year	−0.0017*	−0.0005	0.0002	−0.0002	0.0002	−0.0014***	−0.0016***	0.0000	0.0002
	(0.0008)	(0.0003)	(0.0004)	(0.0005)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0003)
N	34,326	106,965	106,965	106,965	106,965	106,965	106,965	106,965	106,965

Notes: Outcomes are in log rates. Whereas the results in prior tables add one to counts to avoid dropping observations for which the outcome would otherwise be undefined, Panel A instead focuses on counties that never have a zero count for the specified outcome variable. Panel B transform counts using the inverse hyperbolic sine function as an alternative to adding one before taking the log such that the outcome variable is $ln\left(\frac{count+\sqrt{count^2+1}}{population}\right)$. Unweighted estimates from the prior tables are in Panel C. All estimates control for county fixed effects, year fixed effects, state-by-year fixed effects, demographic controls, economic controls, and the size of the police force in the area. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions in Panel A and B are weighted by the population represented by each cell. *, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

magnitude and no longer statistically significant at the five-percent level in these unweighted results. As discussed in Solon et al. (2015), differences between weighted and unweighted estimates can reflect heterogeneity in the effects across high-weight and low-weight observations (or high-population and low-population areas in our case). To gain greater insight into such heterogeneity, we examine how our (weighted) estimates vary as we exclude the largest 5 areas, the largest 10 areas, and so on.[33] The results of this analysis are shown in the Online Appendix in Table A5. Consistent with there being significant effects on homicides for populous areas but not for smaller areas, the estimates do eventually shrink to zero as we exclude more and more large areas. However, the estimates using agency-level homicide reports, which offer a lot more precision than the estimates based on county-level death records, continue to be statistically significant even when we omit the largest 200 agencies. Along similar lines, in Table A6 in the Online Appendix, we show the estimated effects on homicides if we *only* use the largest locations. Though the statistical significance varies depending on how many agencies are included (ranging from 20 to 500) and on whether the estimates are weighted, they routinely indicate that SAT facilities reduce homicides for populous areas.

6. Discussion and conclusion

In the preceding sections, we document statistically and economically significant effects of SAT facilities on several categories of crime. Our estimates of the effects on agency-level crime indicate that an additional facility in a county reduces municipal rates of homicide, aggravated assault, robbery, motor vehicle theft, and burglary.[34] Notably, the 0.16 annual reduction in homicide rates is driven by more populous areas where homicide rates are relatively high. In conjunction with social-cost-of-crime estimates from McCollister et al. (2010), our estimates indicate that an additional SAT facility in a county reduces county crime costs by approximately 2.9 million dollars if reductions in homicides are included, or 1.2 million dollars otherwise.[35] Estimates including homicides are most relevant for populous areas.

These estimates suggest that reductions in crime account for a sizable share of the benefits of SAT facilities. Updated estimates of the effects on county-level drug-related mortality reported in

[33] Recall that we are focusing on counties in our analysis based on death records and police agencies in our analyses based on offenses known to police.

[34] In an interesting contrast, Dobkin and Nicosia (2009) evaluate a crackdown reducing methamphetamine consumption over an 18-month period and do not find statistically significant effects on property or violent crime. Noting that 62% of SAT is for alcohol—and alcohol has been causally linked to crime by a number of studies including Carpenter and Dobkin (2015), Lindo et al. (2018), and Anderson et al. (2017) (forthcoming), among many others—it could be that the effects that we find on crime are driven by reductions in alcohol abuse. Moreover, Dobkin and Nicosia find that the intervention they evaluate substantially increased alcohol abuse, which could attenuate the effects they find on crime. Another potential explanation for why we find statistically significant effects of expanding access to SAT on crime whereas they find no statistically significant effects of methamphetamine use on crime could relate to power. In particular, the effects of methamphetamine use on crime may be too small to be detected by their identification strategy. The 95% confidence interval for their estimated elasticity of homicides with respect to methamphetamine consumption (as measured by hospital admissions) includes 0.078. This is not directly comparable to our estimates, but does not seem at odds with our point estimate which indicates that an additional SAT facility in the county reduces municipal homicide rates by 0.16%.

[35] This is based on an average of six municipal governments in each county and municipal costs of $475,642 with homicides or $191,748 without homicides. Municipal cost calculations are based on a weighted average population of 315,030 and cost-of-crime estimates in 2016 dollars for homicides ($9,881,198 per incident), aggravated assault ($117,722), robbery ($46,541), motor vehicle theft ($11,849), and burglary ($7108).

(Bondurant et al., 2016) indicate that an additional SAT facility reduces drug-related mortality by 0.50% annually. Based on a value of 7–8 million dollars per expected life saved, the estimate implies a decline in a county's annual drug-related mortality costs by 4.2–4.8 million dollars.[36],[37] In total, these calculations suggest that the county-level benefits of an additional facility—in terms of drug-related mortality and criminal activity—are between 5.4 and 7.65 million dollars, depending on whether effects on homicides are included. Reductions in crime account for approximately 20-40% of these benefits.

To compare these benefits to the annual costs of treatment at each facility, we can consider the average number of annual treatment admissions (255) from the National Survey of Substance Abuse Treatment Services (N-SSATS), and treatment modality-specific cost estimates from French et al. (2008).[38] A back-of-the-envelope calculation indicates that the annual costs of treatment for a SAT facility are approximately 1.1 million dollars.[39] These calculations suggest that the benefits of expanding treatment facilities far outweigh the associated treatment costs.

While our data do not allow us to establish a direct link between substance-abuse treatment and incidents, the results of our analyses provide support for the idea that there are broad-based benefits of SAT facilities in terms of public safety. This evidence is in contrast to not-in-my-backyard arguments that have been used to hinder attempts to expand access to SAT through additional facilities. That said, an important limitation of our research design is that it identifies effects of having an additional SAT facility *in the county*, which could mask heterogeneous effects for areas in a county that are nearer versus farther from such a facility. Assessing whether such heterogeneity exists would seem to be an important avenue for future research.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.jue.2018.01.007

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[36] This is based on 10.9 drug-related deaths per 100,000 and an average weighted county population of 1.09 million.

[37] Kniesner et al. (2010) suggest a 7-8 million dollar value of a statistical life (VSL) for health and safety regulation cost-benefit analyses, which is consistent with median VSL estimates from meta analysis of existing VSL research (Viscusi and Aldy, 2003).

[38] Estimates from French et al. (2008) include all treatment delivery costs related to personnel, supplies and materials, contracted services, buildings and facilities, equipment, and miscellaneous items.

[39] We use the annual number of treatment admissions reported in Swensen (2015) based on the 2002–2008 N-SSATS data. More recent N-SSATS data do not include treatment admissions information. To calculate the total cost of treatment at a SAT facility, we use the median of the cost bands reported for each modality in French Popovici, and Tapsell weighted by the proportion of total admissions accounted for by each modality as reported in the 2013 N-SSATS reports.

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