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The effect of Medicaid expansion on crime reduction: Evidence from HIFA-waiver expansions☆

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ABSTRACT

Substance use figures prominently in criminal behavior. As such expanding public insurance and improving access to substance use disorder (SUD) treatment can potentially reduce substance use and reduce crime. We examine the crime-reduction effect of Medicaid expansions through the Health Insurance Flexibility and Accountability (HIFA) waivers. We find that HIFA-waiver expansion led to a sizeable reduction in the rates of robbery, aggravated assault and larceny theft. We also show that much of the crime-reduction effect likely occurred through increasing SUD treatment rate and reducing substance use prevalence. The implied benefit-cost ratio estimate of increased treatment on reducing crime ranges from 1.8 to 3.2.

1. Introduction

Substance use and crime are two of the most intractable social ills facing the United States, and they are inextricably linked. A positive correlation between substance use and crime has been observed in arrestee drug test results and inmate drug reports. Among arrestees who were booked on violent or property crimes, one in every four tested positive for illicit drug use at the time of arrest (ONDCP, 2012). Moreover, among prison inmates charged with violent crimes, 52% reported being under the influence of alcohol or drugs when committing the crime, or committing the crime to acquire money to purchase drugs; among those charged with property crimes, this number is 39% (Miller et al., 2006).

To the extent that this observed correlation involves causality running from substance use to crime, interventions to reduce substance use should also reduce crime. Nonetheless, empirical evidence suggests that punitive approaches to substance control such as prohibition and the "war on drugs" have not led to significant crime reduction (Miron, 1999; Kuziemko and Levitt, 2004; Markowitz, 2005).[1]

In this paper we explore an area that has garnered relatively little attention in the economic literature on crime reduction, namely public health insurance policy. Using county-level panels of crime data

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[1] Miron (1999) examined the U.S. national homicide rate from 1900 to 1995, and demonstrated that alcohol and drug prohibition was positively associated with homicide rate and accounted for half of the variation in the homicide rate. The proposed "violence-as-dispute-resolution" hypothesis stated that prohibition enforcement encouraged the substitution of violent for nonviolent dispute resolution in illegal markets. Kuziemko and Levitt (2004) used state-level crime data between 1980 and 2000, and demonstrated that a 15-fold increase in drug-offense incarceration during the study period reduced total crime rate by no >3%. A back-of-the envelope estimate suggested that locking up drug offenders crowded out criminals with higher marginal risks of recidivism, so drug-offense incarceration was not likely cost-effective. Markowitz (2005) used individual-level victimization surveys in the early 1990s, and showed that higher beer taxes and higher cocaine prices slightly lowered the probability of assault and robbery victimizations.

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between 2001 and 2008 across the United States, we examine the crime-reduction effect of state Medicaid expansions through Health Insurance Flexibility and Accountability (HIFA) waivers (CMS, 2001). The HIFA initiative provides states with federal matching funds to expand Medicaid to all low-income adults with family incomes up to 200% FPL in states. We also explore the extent that state HIFA-waiver expansions provide plausibly exogenous shocks for local SUD treatment rate, which serves as one of the potential pathways to substance use reduction and eventually leads to crime reduction. Our estimates reveal that state HIFA-waiver expansions are associated with an economically meaningful reduction in the rates of specific types of crimes for which theory suggests an increase in the SUD treatment rate should have an effect (i.e., robbery, aggravated assault and larceny theft). Our estimates also suggest that the effect of the HIFA-waiver expansions on increasing SUD treatment rate and reducing substance use prevalence is likely to be one of the driving forces behind the estimated crime-reduction effect.

This study has implications for both public health insurance policy and public safety policy. It provides previously undocumented evidence of significant reductions in crime rates arising from state Medicaid expansions. This has direct relevance to the current health care reform discussions surrounding insurance expansion and "mainstreaming" of SUD treatment. While the political sea change may lead to repeal of the Affordable Care Act (ACA), the effect of insurance expansion on social outcomes, such as crime reduction, may still be of interest for policy and research. We show that a set of state Medicaid expansions preceding the ACA benefitted people with SUDs by providing a cost-effective public health approach to crime reduction, partially through increasing their treatment use and reducing their substance use.

Previous studies of the economic benefits of SUD treatment have often emphasized the direct health returns on treatment through recovery from addiction and the related productivity gains (Belenko et al., 2005). We instead focus on the public finance aspects of SUD treatment and take a more comprehensive view of the cost of crime to the public sector, including direct, indirect and opportunity costs. Our instrumental variable (IV) estimates demonstrate a benefit-cost ratio of 1.8 to 3.2, that is, a 10 percent relative increase in the SUD treatment rate at an average cost of $1.6 billion yields a crime reduction benefit of $2.9 billion to $5.1 billion. This downstream benefit to public safety represents a sizable fraction of returns on SUD treatment. Specifically, as the U.S. criminal justice system scales back mandatory minimum sentences for low-level drug and other minor offenders who may also be substance users, replacing incarceration with better access to SUD treatment can be a cost-effective investment in public safety.

2. Background

2.1. Theories of substance use, SUD treatment and crime

Contemporary criminological theories suggest that substance use is one of the root causes of crime. The most cited criminological theory on this causal relationship is Goldstein's (2003) tripartite model, in which three hypotheses are provided to explain how substance use causes violent and property crimes. First, the pharmacological hypothesis states that violence may occur as a direct result of the intoxication. Intoxication of certain substances may trigger aggression and lead to violent offenses, or alternatively inhibit vigilance and result in victimization. Second, the economic motivation hypothesis states that substance users and addicts commit income-generating crimes to finance their substance use habits. Economic motivation is particularly pronounced among young people and those with low income from legal activities. The third hypothesis, the institutional hypothesis, states that being involved in an illegal drug market can expose one to an increased risk of criminal offense and victimization: crime may arise when a drug buyer robs a dealer of the drugs, when a drug dealer collects debts, and when rival drug gangs dispute over territories or compete for monopolistic power (Goldstein, 2003).

A systematic review of the three-decade long literature concludes that, for all three hypotheses Goldstein proposed, empirical support exists, yet causal interpretations are difficult to make (Bennett et al., 2008). Unobserved third factors, whether they be personal, situational, or environmental (e.g., low self-control, early-life trauma, social inequality, as well as poverty and other forms of social deprivation), may be the underlying causes of both substance use and crime. Nonetheless, to the extent that substance use is on the causal pathway to crime, health insurance and SUD treatment should have the potential not only to reduce substance use but also to reduce crime.

Though motivated by the intuition of Goldstein's tripartite model, our theoretical framework draws more directly upon Becker's rational choice model of crime (Becker, 1968). Based on Becker's model, we specify the following structural relationship between substance use and crime:

$$Crime_{i,j,t} = f\left(Substance\ Use_{i,j,t}, Substance\ Use_{i',j,t}, Law\ Enforcement_{j,t}, X_{1i,j,t}, X_{2i',j,t}, Z_{1j,t}\right) \tag{1}$$

In the structural equation, criminal offense is a function of the substance use by the potential perpetrator i, in the local j, during the time t ($Substance\ Use_{i,j,t}$), the substance use by the potential victim i', in the local j, during the time t ($Substance\ Use_{i',j,t}$), the law enforcement resources ($Law\ Enforcement_{j,t}$), the other observed and unobserved individual factors associated with the propensity for criminal offense ($X_{1i,j,t}$) and the propensity for criminal victimization ($X_{2i',j,t}$), as well as the observed and unobserved contextual factors ($Z_{1j,t}$) that help create or limit opportunities for crime.

Instead of estimating a structural relationship between substance use and crime, this paper estimates a reduced-form relationship between public health insurance policy and crime. We derive the reduced-form equation by first expressing the original terms of the substance use of the perpetrator and the victim as a function relating their substance use to SUD treatment:

$$Substance\ Use_{i,j,t} = f\left(SUD\ Treatment_{i,j,t}, Law\ Enforcement_{j,t}, X_{3i,j,t}, Z_{2j,t}\right) \tag{2}$$

$$Substance\ Use_{i',j,t} = f\left(SUD\ Treatment_{i',j,t}, Law\ Enforcement_{j,t}, X_{4i',j,t}, Z_{2j,t}\right) \tag{3}$$

where substance use by the potential perpetrator $Substance\ Use_{i,j,t}$ and by the potential victim $Substance\ Use_{i',j,t}$ is a function of SUD treatment use $SUD\ Treatment_{j,t}$, the law enforcement resources $Law\ Enforcement_{j,t}$, the other observed and unobserved individual factors of the perpetrator and the victim $X_{3i,j,t}$ and $X_{4i',j,t}$ that are associated with the propensity for substance use, as well as the observed and unobserved contextual factors $Z_{2j,t}$ that help create or limit the opportunities for substance use.

We then relate SUD treatment of the perpetrator and the victim to health insurance policy:

$$SUD\ Treatment_{i,j,t} = f\left(Health\ Insurance\ Policy_{j,t}, Law\ Enforcement_{j,t}, X_{5i,j,t}, Z_{3j,t}\right) \tag{4}$$

$$SUD\ Treatment_{i',j,t} = f\left(Health\ Insurance\ Policy_{j,t}, Law\ Enforcement_{j,t}, X_{6i',j,t}, Z_{3j,t}\right) \tag{5}$$

where SUD treatment by the potential perpetrator $SUD\ Treatment_{i,j,t}$ and by the potential victim $SUD\ Treatment_{i',j,t}$ is a function of health insurance policy $Health\ Insurance\ Policy_{j,t}$, the law enforcement resources $Law\ Enforcement_{j,t}$, the other observed and unobserved individual

factors of the perpetrator and the victim $X_{5i,j,t}$ and $X_{6i',j,t}$ that are associated with the likelihood of SUD treatment use, as well as the observed and unobserved contextual factors $Z_{3j,t}$ that help create or limit the opportunities for treatment use.

Substituting Eqs. (2), (3), (4) and (5) into the structural equation of crime Eq. (1), we obtain the following reduced-form equation:

$$Crime_{i,j,t} = f(Health\ Insurance\ Policy_{j,t}, Law\ Enforcement_{j,t}, X_{1i,j,t}, X_{2i',j,t}, X_{3i,j,t}, X_{4i',j,t}, X_{5i,j,t}, X_{6i',j,t}, Z_{1j,t}, Z_{2j,t}, Z_{3j,t}) \quad (6)$$

Because there is limited availability of individual person-level representative data that capture SUD treatment use and criminal behavior we estimate the effect of health insurance policy on aggregated levels of SUD treatment and crime:

$$Crime\ Rate_{j,t} = f\left(Health\ Insurance\ Policy_{j,t}, Law\ Enforcement_{j,t}, Z_{j,t}\right) \quad (7)$$

where the local aggregated rate of crimes $Crime\ Rate_{j,t}$ is a function of the health insurance policy shocks $Health\ Insurance\ Policy_{j,t}$, the local aggregated level of law enforcement resources $Law\ Enforcement_{j,t}$, and other aggregated factors that are correlated with both the public health insurance policy and crime rate.

A set of pathway analyses also allow us to explore the effect of health insurance policy shocks on increasing the SUD treatment rate, the effect of increased SUD treatment rate on reducing substance use prevalence, and the effect of increased SUD treatment rate on reducing crime rates. Assume the causal pathway from health insurance policy, through SUD treatment use, to substance use reduction, and eventually to crime reduction.

$$SUD\ Treatment\ Rate_{j,t} = f\left(Health\ Insurance\ Policy_{j,t}, Law\ Enforcement_{j,t}, Z_{j,t}\right) \quad (8)$$

$$\begin{aligned} Substance\ Use_{i,j,t} &= f\left(SUD\ Treatment\ Rate_{i,j,t}, Law\ Enforcement_{j,t}, X_{3i,j,t}, Z_{2j,t}\right) \\ &= f\left\{f\left(Health\ Insurance\ Policy_{j,t}, Law\ Enforcement_{j,t}, Z_{j,t}\right), Law\ Enforcement_{j,t}, Z_{j,t}\right\} \end{aligned} \quad (9)$$

$$\begin{aligned} Crime\ Rate_{j,t} &= f\left(SUD\ Use_{j,t}, Law\ Enforcement_{j,t}, Z_{j,t}\right) \\ &= f\left\{f\left(SUD\ Treatment\ Rate_{j,t}, Law\ Enforcement_{j,t}, Z_{j,t}\right), Law\ Enforcement_{j,t}, Z_{j,t}\right\} \\ &= f[f\{f(Health\ Insurance\ Policy_{j,t}, Law\ Enforcement_{j,t}, Z_{j,t}), Law\ Enforcement_{j,t}, Z_{j,t}\} Law\ Enforcement_{j,t}, Z_{j,t}] \end{aligned} \quad (10)$$

In these pathway analyses, we exploit the variation in the local SUD treatment rate induced by the health insurance policy shocks. Under the assumption that the effects of HIFA on crime were solely through increasing SUD treatment, the instrumental variable (IV) estimates provide a direct answer to the policy question of how much crime would be reduced by higher level of SUD treatment use. The estimated crime reduction effect of increasing SUD treatment use can, in turn, be used in comparison to other crime-reduction policies on a cost-benefit basis.

As shown in Eqs. (2) and (10), both treatment and enforcement can be potential strategies to reduce substance use and crime. With respect to the crime-reduction effect of enforcement, existing evidence has suggested that enforcement may neither be an effective nor a cost-effective strategy. Although some enforcement shocks may create temporary increases in the prices, their long-term equilibrium effect on price is at best modest (Caulkins and Reuter, 1998). Second, the effectiveness of enforcement can be further limited by the inelastic demand for substance use. A key insight from Becker and Murphy's (1988) model of rational addiction is that "adjacent complementarity" can make a rational substance user unresponsive to a temporary price increase, even a large spike (Becker and Murphy, 1988; Becker et al., 1991).[2] The degree of price elasticity may even be lower if time-inconsistent, present-bias preferences for substance use are taken into account (Gruber and Köszegi, 2001; O'Donoghue and Rabin, 1999).[3] Even if we assume enforcement can increase the equilibrium price of substances and reduces substance use, enforcement may still cost more at the margin than it saves. The direct criminal justice costs and the spillover public safety costs from increased violence associated with enforcement are unlikely to be offset by the savings in health care costs and the costs of productivity losses related to substance use (Donohue et al., 2011; Miron, 1999).[4] As such, the current level of enforcement may far exceed the socially optimal level from the perspective of social welfare maximization (Becker et al., 2006).

SUD treatment, on the other hand, can effectively reduce substance use. Contemporary neurobiology research recognizes addiction as a chronic disease of brain reward centers and ties clinical phenomena of the disease to specific neuronal mechanisms and pathological processes (Leshner, 1999; McLellan et al., 2000; Dackis and O'Brien, 2005; Everitt and Robbins, 2005; Kalivas and Volkow, 2005). This deeper understanding of the nature of substance use and addiction has led to the development of SUD treatment services based on scientific knowledge and empirical evidence. These evidence-based services combine pharmacotherapies (e.g., medications such as naltrexone for alcohol use, methadone and buprenorphine for opioid use, etc.) with cognitive behavioral interventions, integrate medical treatment with support services (e.g., ancillary mental health services, housing assistance, social skill development, mentoring and peer support, etc.), and are tailored to individual needs (Leshner, 1999). There is now clear evidence for the effectiveness of the SUD treatment: as longitudinal studies have shown, 40 to 60% of the clients who received recovery/rehabilitation-oriented SUD treatment are continuously abstinent from substance use at follow-up one-year after treatment (please see McLellan et al., 2000 for a comprehensive review of the literature). Furthermore, these services have a relatively low marginal cost and small negative externalities.[5]

In addition, SUD treatment may have a positive spillover onto enforcement in that expanded treatment may help increase the price elasticity of demand for substance use and improve the efficiency of enforcement. One the one hand, SUD treatment can alleviate the addictive effect of substance use, thereby reducing the degree of adjacent complementary between the marginal utility of current addictive consumption and future utility (McLellan et al., 1996). On the other hand, SUD treatment can also serve as a pre-commitment device to address the problem of self-control, thereby reducing the degree of time inconsistency in demand for substance use (Ainslie and Monterosso, 2003). Lower degrees of adjacent complementary and time inconsistency

[2] According to the B-M model, "adjacent complementary" or reinforcement means that the addictive goods/bads consumed in different time periods are complements. Because of the complementarity of addictive consumption across time, an increase in the addictive stock increases the marginal utility of current addictive consumption, which in turn, increases the future utility. Therefore, as Becker et al. (1991) point out, "[since temporary police crackdowns on drugs] raises current but not future prices … [and it] would even lower future prices if drug inventories are built up during a crackdown period, there is no complementary fall in current use from a fall in future use. Consequently, even if drug addicts are rational, a temporary war that greatly raised street prices of drugs may well have only a small effect on drug use." (Becker et al., 1991, pp. 241).

[3] According to the G-K model, the self-control problem in impulsive consumption is characterized by a relatively high discounting rate over short horizons compared to the discounting rate over long horizons, which introduce a "time inconsistency" between the present and future preferences and a "present bias" to dynamic decision making. Under this time-inconsistency assumption, the demand for substance use with respect to a temporary price increase would be lower than under the B-M framework of rational, time-consistent addiction.

[4] In addition to the negative externalities on public safety, equity concerns have been raised, as racial profiling in arrests, prosecutions, and incarcerations may take a disproportionately heavy toll on racial minorities (Banks, 2003, Bobo and Thompson, 2006).

[5] There are "Not In My Back Yard" (i.e., NIMBY) concerns that the development of a SUD treatment facility in a community may reduce residential property value and bring an influx of non-locals that threaten community cohesion and place a strain on public resources. No empirical evidence exists for these claims.

result in a higher degree of price elasticity of demand for substance use, which in turn may improve the efficiency of the existing level of enforcement as discussed earlier (Becker et al., 2006).

2.2. Treatment gap & limited insurance coverage for SUD treatment

An estimated 23 million Americans suffered from SUDs in 2010, of which only 11% received specialty SUD treatment for their condition (SAMHSA, 2011). The lack of health insurance coverage and the lack of adequate insurance benefits for SUD treatment were cited as major financial barriers to SUD treatment among those who perceived a need for treatment (SAMHSA, 2011).

Individuals with SUDs are overrepresented among the uninsured, largely because they are more likely to be out of the workforce, unemployed or part-time working poor who can neither obtain insurance through an employer-sponsored plan nor afford insurance in the individual market (Wu et al., 2003). And among them, only a small proportion are eligible for Medicaid coverage. Left uninsured, those with SUDs may not be able to get access to the treatment they need. During the past decade, a set of state-level Medicaid expansions have significantly reduced the financial barriers to SUD treatment and consequently increased the SUD treatment rate.

2.3. Medicaid expansions through HIFA waivers

The Health Insurance Flexibility and Accountability (HIFA) initiative was introduced by the Bush administration in August 2001 to encourage innovative strategies by states to reduce the number of uninsured Americans. The HIFA initiative provided states with a streamlined waiver approval process as well as a significant amount of funding support and policy flexibility to reshape state Medicaid programs (CMS, 2001).

As a means-tested health insurance program for the most vulnerable populations in society, Medicaid traditionally covered only certain categories of families and individuals (e.g., low-income families with dependent children, low-income pregnant women, low-income individuals with disabilities, etc.) (KFF, 2013). Childless adults without disabilities did not fall into these traditional, welfare-based categories and were thus ineligible for Medicaid in most states regardless of their income level. The income eligibility threshold for adult members of poor families was much higher than the threshold for their dependent children. During the early 2000s, the national median income threshold for an adult from a low-income family was 60% of the federal poverty level (FPL); in over 20 states the threshold was lower than 50% of the FPL (KFF, 2013). Furthermore, a substance user who is disabled may still be deemed ineligible for Medicaid if his/her disability was solely caused by substance use (KFF, 2013). The expansions of Medicaid eligibility during the late 1980s and the 1990s were largely targeted at children from low-income families and pregnant women, thus having little impact on SUD treatment use among the adult population.

As a centerpiece of the Bush administration's health care policy agenda, the HIFA initiative provided federal matching funds for all low-income adults with family incomes up to 200% FPL in states that received approvals from the Center for Medicare and Medicaid Services (CMS) to expand Medicaid coverage. CMS also gave states broad authority and policy flexibility to implement the expansions using Section §1115 waivers. The Section §1115 waivers allows states to "waive" certain statutory Medicaid requirements for an expansion program. Not only is the actual income threshold of the expanded Medicaid eligibility left to state discretion, but states are also allowed to cap enrollment, impose a certain amount of premium and cost sharing, and limit the scope of the benefit package in an expansion program (Coughlin et al., 2006; Atherly et al., 2012; KFF, 2013).

Between 2001 and 2008, fifteen states received approval for HIFA waivers and eight of the fifteen waiver states implemented comprehensive Medicaid expansions to low-income adults. The other seven states either never implemented the expansions or only temporally enrolled a small proportion of the eligible individuals due to unexpected budget shortfalls. Coughlin et al. (2006) found that by the end of 2005, five fully or nearly fully implemented HIFA-waiver expansions (i.e., Arizona, Illinois, Maine, Michigan, and Oregon) had achieved projected or even higher-than-projected enrollment. Atherly et al. (2012) focused on the impact of those early HIFA-waiver expansions on the targeted low-income adult population, and found that the expansions increased the probability of being insured by a relative 13% among this targeted population (Atherly et al., 2012). In addition to the effect on health insurance coverage, prior research suggests that HIFA-waiver expansions also had positive effects on health care use and health outcomes. Sommers et al. (2012) examined three of those "early HIFA states" that adopted expansions between 2001 and 2002 and found that the HIFA-waiver reduced the mortality by 6% among the targeted low-income adults and reduced their probability of financial-related delays in care by 15 percent decrease (Sommers et al., 2012). Furthermore, single-state studies of the Massachusetts comprehensive health care reform and the Oregon Medicaid lottery experiment also show significant increases in self-reported health status and access to preventive care following the implementation of these expansions (Baicker and Finkelstein, 2011; Chen et al., 2011; Kolstad and Kowalski, 2012; Long et al., 2012; Baicker et al., 2013). If the HIFA-waiver expansions improved health insurance coverage, health care use and health outcomes among low-income adults, they should also have the potential for improving SUD treatment use and reducing crimes.

2.4. Concurrent changes related to SUD treatment use and crime

Concurrent with the implementation of HIFA-waiver expansions, two major changes during the 2000s were also shown to be associated with SUD treatment use or crime. First was the state enactment of SUD parity mandates that helped address the inadequate benefits for SUD treatment in private health insurance. Although benefits for SUD treatment are typically covered by private health insurance, SUD benefits in >80% of private health plans were subject to higher cost sharing or more treatment limitations than benefits for comparable medical/surgical treatment (BLS, 2009). To address the discriminatory restrictions in SUD benefits in private health insurance market, SUD parity was first introduced during the early 1980s in several states, primarily in the South, and has since been enacted by more than half of the states. As noted by Wen et al. (2013), ten states implemented SUD parity laws between 2000 and 2008 mandating insurance benefits for SUD treatment to be offered on par with, or no more restrictive than, those for comparable medical/surgical treatment. The implementation of these state parity mandates increased state-aggregate SUD treatment rate by a relative 9% in specialty SUD treatment facilities (Wen et al., 2013). Another study assessed a set of broadly defined behavioral health parity laws, and they found that state implementation of a parity mandate was associated with a reduction in uninsured admissions and out-of-pocket costs for people treated in specialty SUD treatment facilities (Dave and Mukerjee, 2011).

A second major change concurrent with the implementation of HIFA-waiver expansions was the proliferation of local drug courts across the United States. The total number of drug courts in operation increased three-fold in the 20th century from about 600 to over 2400. Such a rapid growth was seen in every state jurisdiction (Huddleston and Marlowe, 2008). The majority of these drug courts targeted adult non-violent offenders with substance use problems and combined treatment with the court's legal authority to increase SUD treatment use and reduce the recidivism related to substance use (National Association of Drug Court Professionals, 1997; GAO, 1997). In recent years, drug courts for juvenile offenders and offenders charged with driving while under the influence (DWI) have also emerged and grown (Huddleston and Marlowe, 2008). Mitchell et al. (2012) systematically reviewed 92 quasi-experimental and experimental evaluations of the effectiveness of adult non-DWI drug courts. They found that

reduced the recidivism rate among drug court participants from 50% to 38% relative to non-participants and the reduction in the recidivism rate lasts up to three years (Mitchell et al., 2012). The authors also reviewed juvenile and DWI drug court evaluations, but only found modest reductions in recidivism (Mitchell et al., 2012).

Our analyses directly control for the concurrent implementation of SUD parity mandates. However, due to the complexity of the information on local drug courts/diversion programs, our analyses were only able to use proxy measures of law enforcement resources and government investment in correction. Nonetheless, we would expect that these omitted variables to affect the HIFA-waiver expansions, SUD treatment rate and crime rates in the same direction, thus biasing our estimates downwards (i.e., towards the null hypothesis).

2.5. The significance of our study

Our study provides the first nationwide county-level and Core-Based Statistical Area (CBSA)-level estimates of the effect of state Medicaid expansions on reducing local crime rates. Our study also explores the effects of these expansions on the SUD treatment rate and substance use prevalence, which are likely to be on the key pathway leading from Medicaid expansions to crime reduction. Our aggregate-level analysis, which exploits changes in the state policy environment around SUD treatment financing, addresses the selection and self-reporting issues inherent in most previous individual-level studies.[6] Moreover, an aggregate-level analysis is more generalizable and more salient to the current health care reform and criminal justice reform debates, as it captures the population-level effect of health policy and SUD treatment on crime reduction.

3. Data

Our data consists of a panel of annual, county-level observations between 2001 and 2008. Data sources include the Uniform Crime Reports (UCR), the National Survey of Substance Abuse Treatment Services (N-SSATS), and other nationally representative datasets that provide supplementary information on important local-level socioeconomic and policy contextual measures (Table 1).

3.1. Dependent variables: crime rates

County-level crime rates (*Crime Rate*$_{c,s,t}$), were collected annually by the Federal Bureau of Investigation (FBI) in the UCR 2001–2008, and were calculated based on the number of crimes reported to the police of all law enforcement agencies within each given county *c* in state *s* over an entire calendar year *t*[7] (*Crime Rate*$_{c,s,t}$: number of crimes reported to all police agencies per 1000 residents).

UCR county-aggregate crime data are available for the eight Part I crime categories: criminal homicide, forcible rape, aggravated assault, robbery, burglary, larceny theft, motor vehicle theft, and arson. The first four crime categories are collectively referred to as violent crime, while the latter four as property crime.[8]

3.2. Primary independent variable: HIFA-waiver expansion implementation

We created a state-level dichotomous indicator ($HIFA_{s,t}$) to capture the implementation of state HIFA-waiver expansions between 2001 and 2008. $HIFA_{s,t}$ was assigned a value of 1 for each full year subsequent to the year in which the legislation was first implemented. Among the eight states with comprehensive HIFA-waiver expansions, four states were not classified as "HIFA States" in our analysis. Arizona implemented the expansion early in 2001, leaving a very limited pre-expansion window for analysis. In addition, the expansions in Oregon, Michigan, and Oklahoma provided limited or no insurance coverage for SUD treatment. Oregon's expansion program, the Oregon Health Plan Standard (OHP-S), initially covered specialty SUD treatment. In response to a growing fiscal crisis and special interest power, however, Oregon closed new enrollment to the OHP-S during the subsequent year and eliminated SUD benefits for the enrollees remaining in the program. In Michigan, the expansion program, the Adult Benefits Waiver (ABW), did not cover specialty SUD treatment. It only covered mental health services provided through Community Mental Health Centers. Oklahoma's expansion program mainly targeted working disabled adults that had higher incomes that the traditional income eligibility threshold for the state Medicaid program (Coughlin et al., 2006). Thus, we only classified the remaining four states that implemented HIFA waiver expansions during the study period (i.e., Illinois, Maine, New Mexico, and Massachusetts) as "HIFA states".[9]

3.3. Pathway variables: SUD treatment rate & substance use prevalence

The first variable on the pathway from HIFA-waiver expansions to crime reduction is the SUD treatment rate. We derived the county-level SUD treatment rate (*SUD Treatment Rate*$_{c,s,t}$) from facility-level information on annual SUD treatment counts in the N-SSATS 2000, 2002–2008.[10]

N-SSATS covers all known specialty SUD treatment facilities[11] across the United States and achieved 92–95% response rates during the study period, allowing for a nearly complete enumeration of specialty SUD

[6] Concerns have been raised over both internal validity and external validity of previous individual-level studies of SUD treatment use and crime involvement. First, selection bias may occur if those substance users who self-refer to treatment are also more self-motivated to change their behavior during and after the treatment process. Selection bias may also occur in coerced treatment regimes. Courts and other law enforcement agencies are likely to "cherry-pick" offenders with less severe addictions and less adverse life circumstances, and assign them to treatment programs in addition to, or in lieu of, incarceration (Chandler et al., 2009; Taxman et al., 2009). Second, the reliability of self-reported crime is questionable. This is particularly true in the tails of the distribution of criminal activity frequency: infrequent offenders tend to underreport criminal behavior and frequent offenders tend to overstate their criminal involvement (Levitt, 1996).

[7] The UCR 2001–2008 uses the following imputation procedures to deal with missing data: data for an agency reporting 3 to 11 months were augmented by a weight of 12 divided by the number of months reported; data for an agency reporting 1 to 2 months were imputed based on the other agencies located in the same geographic stratum within a state and reporting 12 months of complete data. No imputation was conducted for any agency missing data for all 12 months (Lynch and Jarvis, 2008).

[8] It has been well-recognized that the UCR data are the product of a set of social processes such that some crimes become "official" and "public facts" while others do not. Legal severity, victim-offender relationships, desires of the complainant, and the extent to which citizens and police see an incident as a public or private matter are all criteria related to reporting (Gove et al., 1985). Nonetheless, Gove et al. (1985) provide a strong argument that the UCR provides valid and reliable indicators of the Part I (index) crimes, which consist of relatively severe crimes likely to pass through the citizen and police filters and officially reported. Furthermore, if the measurement error in UCR data is simply random noise, our estimates would still be consistent (albeit with less precision).

[9] Federal matching funds were provided for all low-income adults with family incomes up to 200% FPL if states included them in the expansion. The actual income threshold of the expanded Medicaid eligibility was left to state discretion. Notably, the expanded income eligibility threshold varied across the four "HIFA states" (i.e., Illinois, 185% FPL; Maine, 125% FPL, only implemented up to 100% FPL; New Mexico, 200% FPL; and Massachusetts 150% FPL) (Atherly et al., 2012).

[10] Note that in 2002, the N-SSATS survey date was changed from September to March to enhance the response rate, leaving a gap period from September 2000 to March 2001 with no data collected. Accordingly, the annual treatment data (representing SUD treatment from April 2001 to March 2002) was matched with the same-year annual crime data (representing reported crimes from January 2002 to December 2002) for the year of 2002 and for each year afterward; while the 2000 treatment data (representing SUD treatment from October 1999 to September 2000) was paired with the 2001 crime data (representing crimes from January 2001 to December 2001).

[11] Specialty SUD treatment facility, according to N-SSATS, is defined as a hospital, a residential SUD facility, an outpatient SUD treatment facility, a mental health facility with an SUD treatment program, or other facility with an SUD treatment program providing the following treatment services: (a) Outpatient, inpatient, or residential/rehabilitation SUD treatment; (b) detoxification treatment; (c) opioid treatment programs (OPT) such as methadone and L-α-acetyl-methadol (LAAM) maintenance; or (d) halfway house services that include SUD treatment.

treatment services in the United States. All surveyed facilities were requested to report the total SUD treatment counts in the most recent 12 months prior to the survey. N-SSATS specified that the treatment count should only include the initial entry of a client into treatment; subsequent visits to the same service or transfer to a different service within a single continuous course of treatment were excluded. The facility-level treatment counts were then aggregated to each county c in each year t to determine the county-level annual SUD treatment rate (*SUD Treatment Rate*$_{c,s,t}$: number of SUD treatment entries into all specialty SUD treatment facilities per 1000 residents).

The second set of pathway variables is the substance use prevalence from the restricted-access micro-level National Survey on Drug Use and Health (NSDUH) data. Due to confidentiality restrictions, we were required to aggregate the data to the level of "substate region" for analytic and publication purposes. A total of 383 substate regions across the United States were developed in a series of communications between SAMHSA staff and state officials to provide estimates for the geographic distribution of substance use prevalence that states would find useful for administrative, planning and reporting purposes. Most states drew the substate region boundaries based on county boundaries, while a few states drew the boundaries based on census tract. Using the criteria established in the Diagnostic and Statistical Manual of Mental Disorders, 4th Edition (DSM-IV), we created two measures of substance use prevalence based on individual respondent self-reported symptoms of substance use in NSDUH and aggregated to the substate region. The first measure assessed the percentage of residents in a substate region who were classified as having a past-year alcohol use disorder (*Alcohol Use Disorder Prevalence*$_{c,s,t}$: number of residents with past-year alcohol use disorder per 1000 residents). The second measure assessed the percentage of substate residents that had a past-year drug use disorder (*Drug Use Disorder Prevalence*$_{c,s,t}$: number of residents with past-year drug use disorder per 1000 residents).

3.4. Other controls

County- or substate region-level covariates $X_{1c,s,t}$ include demographic characteristics, economic conditions, and law enforcement resources. Demographic characteristics including age distribution and racial/ethnic composition of the population were measured as the percentage of residents who were (1) between the ages of 15 and 34,[12] (2) Black, (3) Hispanic/Latino, (4) Asian, and (5) members of other racial/ethnic groups. Economic conditions were measured as the county's (6) median household income, (7) poverty rate, and (8) unemployment rate. Law enforcement resources, another mechanism through which crime could potentially be deterred, were measured as (9) the number of sworn officers per 1000 residents.[13] We used both contemporaneous and one-year lagged values of law enforcement resources to account for the immediate and delayed effect of their deterrence on crime (Levitt, 1997). The demographic and economic measures were drawn from the Area Health Resource File; the law enforcement measure was taken from the UCR.

State-level covariates $X_{2s,t}$ include contemporaneous and one-year lagged values[14] of state government expenditures in several key domains to account for the public investment that may help reduce crime. Measures of state government expenditures include the dollar per capita spending on: (10) police protection and correction, (11) education, (12) hospital and health, and (13) welfare and other domains (e.g., government administration, highways, natural resources, etc.). The information on state government expenditures was compiled by the Census Bureau from the Annual Survey of State Government Finances. Three additional state-level measures were included to capture other relevant changes in the state policy environment during the study period: (14) state implementation of SUD parity mandates[15] (i.e., Montana, Rhode Island, Maine, New Hampshire, Oregon, Wisconsin, and West Virginia), (15) state excise tax rates on beer,[16] and (16) per capita amount of the Substance Abuse Prevention and Treatment Block Grant (SABG) allocated to states, which, as the largest sources of funding for safety-net SUD treatment, may affect state and local SUD treatment infrastructure and capacity. The information on SUD parity mandates was provided by the Substance Abuse and Mental Health Services Administration (Robinson et al., 2006), the National Conference of State Legislatures (NCSL), and other advocacy organizations. We also referred to the original state statutes to detect the subtlety in statutory language and to reconcile the inconsistencies among various sources (Wen et al., 2013). The information on state beer tax and SABG funding was compiled by the Alcohol Policy Information System (APIS) and the Treatment Improvement Exchange (TIE) database, respectively (Table 1).

4. Estimating the main effect of HIFA-waiver expansions on crime rates

We used a two-way (i.e., county and year) fixed effects model to estimate the effect of state HIFA-waiver expansions on county crime rates to isolate the within-local variations over time:

$$\textit{Crime Rate}_{c,s,t} = \beta_1 + \beta_2 \textit{HIFA}_{s,t} + \beta_3 X_{1c,s,t} + \beta_4 X_{2s,t} + \rho_c + \tau_t + \varepsilon_{c,s,t} \quad (11)$$

where c denotes county, s denotes state, t denotes year. ρ_c represents county fixed effects and τ_t represents year fixed effects. The two-way (i.e., county and year) fixed effects account for the time-invariant county heterogeneity and the national secular trend in crime rates. The two-way (i.e., county and year) fixed effects can be viewed as an extension of the difference-in-difference (DD) framework to fit a multiple-unit and multiple-time analysis that goes beyond the traditional two groups and two periods (Wooldridge, 2010). $X_{1c,s,t}$ is a time-varying, county-level vector of demographic, economic and law enforcement factors that may be correlated with both the state HIFA-waiver expansions and county crime rates. $X_{2c,s,t}$ is a time-varying state-level vector of government expenditures on crime-related functions, implementation of SUD parity mandates, beer tax rates, and the SAPTBG funding amount. Standard errors were clustered at the state level to correct for serial correlation. The clustered standard errors allow for arbitrary within-state correlation in the error terms but assume independence across the states (Bertrand et al., 2004).

Eq. (11) was first estimated using the sum total of the eight Part I crime categories, of the four violent crimes and of the four property

[12] Adolescents and young adults aged 15–34 are at high risk of participating in substance use (SAMHSA 2011) and in substance-related crimes (Brame and Piquero, 2003).

[13] Sworn officers, according to UCR, are defined as full-time, sworn personnel with full arrest powers including the chief, sheriff or other head of the agency as of October 31.

[14] We conducted extensive checks for the lag structure of state government expenditures. One might expect, for instance, that the expenditure on education or other prevention pathways may have a delayed effect on crime rates, so we assessed whether spending levels two and three years prior affected crime rates. Two- and three-year lagged values of state government expenditures were neither individually nor jointly significant in predicting crime rates, and thus excluded from our model specifications.

[15] "Parity states" included the states that first implemented SUD parity mandates during the study period and those that improved the comprehensiveness of their laws during the study period. Although the parity mandates differ in their comprehensiveness (i.e., full parity, partial parity, and parity-if-offered), we created a single generic indicator to capture the implementation of any SUD parity mandate during the study period regardless of its comprehensiveness and relative improvement in its comprehensiveness (Wen et al., 2013).

[16] State beer tax is defined as specific excise taxes levied per gallon at the wholesale or retail level.

crimes, as dependent variables, respectively. Eq. (11) was also estimated for eight additional models in which the dependent variable was each Part I crime category separately. In theory, the crime-reduction effects of Medicaid expansions should be concentrated among crimes related to substance use, and in which the substance users involved would be likely to seek SUD treatment if available and within their budget constraint. We would therefore expect the effect of Medicaid expansions to be concentrated in lower-level property and violent crimes such as theft, robbery and assault, but not in crimes typically committed by more 'hardcore' criminals such as homicide.

In addition to the contemporaneous policy indicator $HIFA_{s,t}$, we further estimated specifications with a series of lagged and leading policy indicators $HIFA_{s,t-2}$, $HIFA_{s,t-1}$, $HIFA_{s,t+1}$, $HIFA_{s,t+2}$ to track the longer-term effect of HIFA-wavier expansions and check for the policy endogeneity. In theory, it should be the policy shocks of HIFA-waiver expansions that drive the change in the SUD treatment rate and the subsequent reduction in crime rates, rather than some past shock to the SUD treatment rate and/or crime rates leading to the adoption of the expansion. As such, we would expect to see that only the contemporaneous and lagged policy indicators have a significant effect on the SUD treatment rate and crime rates.

We estimated the effect of HIFA waiver expansions on crime rates for a balanced panel of 2791 counties that had all data available over the 8-year period (i.e., 22,328 observations). To further test the robustness of our estimates, in sensitivity analyses, we estimated three alternative model specifications which are presented in the appendices. Appendix Tables A1–A3 display model results where we extended the analytic sample to an unbalanced panel consisting of all 23,537 non-missing observations (i.e., 3016 counties over an average of 7.8 years). Appendix Tables A4–A5 displays results where we included state-specific linear time trends $\rho_s t$ to account for the unobserved state-level factors that evolve over time at a constant rate (e.g., public sentiment towards crime and addiction). Appendix Tables A6–A11 and Appendix Figs. A1–A5 display results where we added three more years[17] to the pre-HIFA window to confirm the parallel-trend assumption in the DD framework.

Table 1
Summary statistics of the study variables.

Summary statistics	County-level
	Mean (S.D.)
Dependent variables (per 1000 residents)	
Total crime rate	40.11 (17.80)
Violent crime	4.93 (3.36)
Criminal homicide	0.06 (0.06)
Forcible rape	0.36 (0.37)
Robbery	1.51 (1.41)
Aggravated assault	3.01 (2.06)
Property crime	35.18 (15.45)
Burglary	7.44 (3.83)
Larceny theft	23.40 (10.19)
Motor vehicle theft	4.08 (3.29)
Arson	0.25 (0.21)
SUD treatment rate	12.81 (10.24)
Alcohol use disorder prevalence (substate region-level)	28.07 (12.10)
Drug use disorder prevalence (substate region-level)	75.25 (23.21)
Covariates	
County demographics, economics, & law enforcement:	
% age 15–34	27.71 (3.99)
% African/Black	12.78 (13.26)
% Hispanic/Latino	13.91 (15.76)
% Asian	4.32 (5.62)
% other racial/ethnic origins	2.74 (4.01)
$ median family income ($1000)	47.94 (12.88)
% poverty	12.56 (5.05)
% unemployment	5.14 (1.95)
% sworn officers	2.33 (2.54)
State government expenditures ($1000 per capita)	
$ police protection & correction	1.83 (0.44)
$ education	15.43 (3.20)
$ health & hospital	3.24 (1.22)
$ welfare & other domains	22.62 (6.44)
State beer excise tax rates ($ per gallon)	0.23 (0.16)
State SAPTBG funding ($ per capita)	5.52 (0.78)

The main estimates for the implementation of HIFA-waiver expansions suggest that a significant crime-reduction effect is present in three subcategories, namely robbery, aggravated assault, and larceny theft (Tables 2–4 upper panel: row 1). HIFA-waiver expansions reduced the robbery rate by 0.02 per 1000 residents, reduced the aggravated assault rate by 0.04 per 1000 residents (Table 3 upper panel: column 7 row 1), and reduced the larceny theft rate by 0.14 per 1000 residents annually (Table 4 upper panel: column 3 row 1). We also find a reduction in motor vehicle theft rate, albeit imprecisely estimated.

Translating the estimated marginal effects into percentage changes, HIFA-waiver expansions led to a relative 2 percent reduction in the robbery rate, a 1 percent reduction in the aggravated assault rate, and a 0.6 percent reduction in the larceny theft rate. The sizeable crime-reduction effect of HIFA-waiver expansions on robbery and aggravated assault suggests that Medicaid expansions, through increasing SUD treatment use and reducing substance use, may reduce the risk of personal violence that is likely to occur as a result of intoxication, which corresponds to Goldstein's (2003) pharmacological hypothesis. The sizeable effect on robbery and larceny theft suggests that Medicaid expansions may also reduce the motivation for financing substance use habits through illegal activities, which corresponds to Goldstein's (2003) economic motivation hypothesis.

Furthermore, we found that only the contemporaneous and lagged policy indicators of HIFA-waiver expansions have a significant effect on crime rates, while almost all the leads have insignificant effects with effect sizes close to zero (Tables 2–4 rows 2–6; Figs. 1–3). This indicates that it is the policy shock of HIFA-waiver expansions that drive the reduction in crime rates, rather than some past shock to crime rates leading to the adoption of HIFA-waiver expansions. In addition to the evidence for policy exogeneity, we also found that the significant crime-reduction effects of HIFA-waiver expansions on robbery, aggravated assault, and larceny theft generally emerged immediately or within one year after states began to implement the expansions and persisted at least two years subsequently.

The estimates without county-level controls $X_{c,s,t}$ and state-level controls $X_{s,t}$ (Tables 2–4 Bottom Panel) were consistent with our main estimates, albeit the effect size was generally larger, especially for the contemporaneous and lagged reduction in property crime rate. The estimated reductions in property crime rate were mainly driven by larceny theft rate. The estimates from the sensitivity analyses with the unbalanced panel, with the state-specific trends, and with the extended pre-HIFA window were also shown to be consistent with the main estimates (Appendix Tables A1–A11; Appendix Figs. A1–A5).

[17] We acknowledge that more previous data in 1990s would provide a more convincing check for the pre-HIFA parallel-trend assumption. However, the data for SUD treatment rate can only be traced back to 1997 and the 1990s' data (named "Uniform Facility Data Set (UFDS)" https://wwwdasis.samhsa.gov/dasis2/nssats.htm) have a different definition of "SUD treatment facility" and a different sampling methods, thus not consistent with the later N-SSATS data. Furthermore, the Bush administration introduced the HIFA waiver program in August 2001, while earlier expansions were targeted at women and children. Because of the measurement differences in the surveys, we chose to start our study period in 2001 and presented the estimates with the extended pre-policy window as robustness checks, which still confirmed parallel pre-trends despite the noted limitations of the earlier data.

Table 2
Estimated effect of HIFA-waiver expansions on crime rates per 1000 residents: Balanced-panel.

Total crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates					
	Total crime rate		Violent crime rate		Property crime rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Policy lags & leads						
HIFA (0/1)	−0.24* (0.10)		−0.06* (0.03)		−0.17† (0.09)	
2-year before T_{HIFA}[a]		0.04 (0.06)		0.003 (0.02)		0.04 (0.06)
1-year before T_{HIFA}		0.02 (0.02)		−0.001 (0.01)		0.02 (0.03)
Year of T_{HIFA}		−0.19† (0.10)		−0.05** (0.02)		−0.14 (0.08)
1-year after T_{HIFA}		−0.28* (0.13)		−0.07* (0.03)		−0.21† (0.12)
2-year after T_{HIFA}		−0.29* (0.15)		−0.08* (0.04)		−0.22† (0.13)
Total crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *without* county & state covariates					
	Total crime rate		Violent crime rate		Property crime rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Policy lags & leads						
HIFA (0/1)	−0.31*** (0.09)		−0.07** (0.02)		−0.24** (0.08)	
2-year before T_{HIFA}		0.06 (0.08)		−0.004 (0.01)		0.06 (0.08)
1-year before T_{HIFA}		0.05 (0.07)		−0.002 (0.01)		0.05 (0.07)
Year of T_{HIFA}		−0.19* (0.09)		−0.05* (0.02)		−0.14† (0.07)
1-year after T_{HIFA}		−0.27* (0.13)		−0.06* (0.03)		−0.21* (0.09)
2-year after T_{HIFA}		−0.29* (0.14)		−0.07† (0.04)		−0.22* (0.10)
# observations	22,328	22,328	22,328	22,328	22,328	22,328

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

5. Estimating the pathway effects of HIFA-waiver expansions on SUD treatment rate and of SUD treatment rate on crime rates

We first used a two-way fixed effects model to estimate the pathway effect of state HIFA-waiver expansions on county SUD treatment rate similar to the one used to estimate crime rates:

$$SUD\ Treatment\ Rate_{c,s,t} = \beta_1 + \beta_2 HIFA_{s,t} + \beta_3 X_{1c,s,t} + \beta_4 X_{2s,t} + \rho_c + \tau_t + \varepsilon_{c,s,t} \quad (12)$$

As in Eq. (11), we included in Eq. (12) ρ_c and τ_t to adjust for the time-invariant county heterogeneity and the national secular trend. We also included the full set of covariate vectors $X_{c,s,t}$ and $X_{s,t}$ to account for the time-varying county-level and state-level confounders. Standard errors were clustered at the state level to correct for the serial correlation. The analytic sample, again, was the balanced panel of 22,328 county-year observations. The estimates with county-level controls $X_{c,s,t}$ and state-level controls $X_{s,t}$ (Table 5 upper panel) suggest that HIFA-waiver expansions increased the SUD treatment rate by 2.57 per 1000 residents, or a relative 20.1 percent increase. The significant increase in the SUD treatment rate emerged immediately after the beginning of the HIFA-waiver expansion implementation and persisted at least two years subsequently. The estimates without controls (Table 5 lower panel), again, had a larger effect size for the contemporaneous and lagged increases in the SUD treatment rate.

We then used a two-stage least squares (TSLS) instrumental variable (IV) model to estimate the pathway effect of SUD treatment rate on crime rates. We suspect that a naïve OLS regression may underestimate the crime-reduction effect of the SUD treatment rate due to the reverse causality between SUD treatment and crimes (i.e., higher crime rates may translate back to a higher SUD treatment rate through drug courts or diversion programs offered to a select group of non-violent offenders in need of treatment). Failing to address this inherent "structural endogeneity" and other inevitable biases such as omitted variables may result in a downward-biased OLS estimate.[18] To address these modeling concerns, we used HIFA-waiver expansions as our instrumental variables to exploit the variation in the SUD treatment rate (Eqs. (13) and (14): TSLS Stage I), and estimate its effect on crime rates (Eq. (15): TSLS Stage II) under the assumption that the effect of HIFA waivers on crime was only through increasing SUD treatment

$$SUD\ Treatment\ Rate_{c,s,t} = \alpha_1 + \alpha_2 HIFA_{s,t} + \alpha_3 X_{c,s,t} + \alpha_4 X_{s,t} + \rho_c + \tau_t + \varepsilon_{c,s,t} \quad (13)$$

$$SUD\ Treatment\ Rate_{c,s,t} = \alpha_1 + \alpha_2 HIFA_{s,t} + \alpha_3 Parity_{s,t} + \alpha_4 X_{c,s,t} + \alpha_5 X_{s,t} + \rho_c + \tau_t + \varepsilon_{c,s,t} \quad (14)$$

$$Crime\ Rate_{c,s,t} = \beta_1 + \beta_2 SUD\ Treatment\ Rate_{c,s,t} + \beta_3 X_{c,s,t} + \beta_4 X_{c,s,t} + \rho_c + \tau_t + \varepsilon_{c,s,t} \quad (15)$$

In addition to the sensitivity analyses with an unbalanced panel and with state-specific trends (Appendix Tables A16–A18 left panels) as we previously did when estimating the effects of HIFA-waiver expansions on crime rates and SUD treatment rate, we estimated an additional model specification in which we included the means of all independent variables (i.e., $SUD\ Treatment\ Rate_{c,s,t}$, $X_{1c,s,t}$ and $X_{2s,t}$ as well as the policy instruments $HIFA_{s,t}$ and $Parity_s$) from the adjacent counties (Appendix Table A19). The concern is that the county may be too small to capture the potential area where people engage in SUD treatment and crime. Therefore the crime-reduction effect of the increased SUD treatment rate in one county may spill over into the neighboring counties.

An alternative approach to the potential spill-over issue is to aggregate the data to a higher level. We aggregated the county-level data to

[18] The naïve solution of replacing or instrumenting the endogenous variable with its lagged form is problematic if the error terms are in effect serial-correlated.

Table 3
Estimated effect of HIFA-waiver expansions on specific violent crime rates per 1000 residents: Balanced-panel.

Violent crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates							
	Homicide rate		Forcible rape rate		Robbery rate		Aggravate assault rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	0.0001		−0.001		−0.02* (0.01)		−0.04**	
	(0.0003)		(0.01)				(0.02)	
2-year before T_{HIFA}[a]		0.001 (0.001)		−0.003		0.005 (0.01)		−0.002 (0.01)
				(0.01)				
1-year before T_{HIFA}		−0.0002 (0.001)		−0.01 (0.01)		0.0004 (0.005)		0.003 (0.02)
Year of T_{HIFA}		−0.0003		−0.003		−0.01† (0.005)		−0.04**
		(0.0004)		(0.01)				(0.02)
1-year after T_{HIFA}		0.0001 (0.001)		−0.004		−0.02* (0.01)		−0.06**
				(0.01)				(0.02)
2-year after T_{HIFA}		−0.0004		−0.003		−0.01† (0.005)		−0.07**
		(0.0004)		(0.01)				(0.02)
Violent crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *without* county & state covariates							
	Homicide rate		Forcible rape rate		Robbery rate		Aggravate assault rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	0.0001		−0.001		−0.01*		−0.06**	
	(0.0003)		(0.01)		(0.004)		(0.02)	
2-year before T_{HIFA}		0.001 (0.001)		−0.002		0.004 (0.004)		−0.003 (0.01)
				(0.01)				
1-year before T_{HIFA}		−0.0003 (0.001)		−0.004		−0.001		0.01 (0.02)
				(0.01)		(0.003)		
Year of T_{HIFA}		−0.0004 (0.001)		−0.001		−0.004		−0.04**
				(0.01)		(0.004)		(0.01)
1-year after T_{HIFA}		0.0005 (0.001)		−0.003		−0.01* (0.003)		−0.06**
				(0.01)				(0.02)
2-year after T_{HIFA}		0.0001 (0.0004)		−0.01 (0.01)		−0.01* (0.005)		−0.06* (0.03)
# observations	22,328	22,328	22,328	22,328	22,328	22,328	22,328	22,328

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

the level of Core-Based Statistical Area (CBSA). The term "CBSA"[19] refers collectively to both metropolitan statistical areas (MSAs) and micropolitan statistical areas (μSAs). We excluded the 1354 non-CBSA rural counties, which only account for 4% of the overall SUD treatment rate and 6% of the overall crime rate. We converted the remaining 1788 counties to 941 CBSAs (i.e., 335 MSAs and 526 μSAs), and subsequently separated those CBSAs across multiple states[20] to accommodate the state-level HIFA-waiver expansion indicator. The final CBSA-level samples thus include a balanced panel of 928 CBSA-like units over 8 years and an unbalanced panel of 981 CBSA-like units over an average of 7.9 years (Appendix Tables A16–A18 right panels).

The bottom panel of Table 6 presents the TSLS Stage-I estimates for the SUD treatment rate when instrumenting with HIFA-waiver expansions alone (Table 6 bottom panel column 1 and Fig. 4), and when instrumenting with both HIFA-waiver expansions and SUD parity mandates (Table 6 bottom panel column 2). The TSLS Stage-I estimates suggest that HIFA-waiver expansions increased the SUD treatment rate by 2.57 per 1000 residents, or a relative 20.1 percent increase, while SUD parity mandates (when the HIFA-waiver expansions also included) increased the SUD treatment rate by 0.86 per 1000 residents, or a 6.7 percent increase. The estimates without controls had a larger effect size for the increases in the SUD treatment rate. The F-statistics across all models exceed the critical values for Stock and Yogo (2002) weak instrument test.[21] In addition to the statistical evidence for the strength of HIFA waiver expansions and SUD parity mandates as instruments for the SUD treatment rate, the number of the instruments we identified allows for an overidentification test of the exclusion restrictions. The statistics from these tests (Appendix Table A21) lend support to the exogeneity of HIFA waiver expansions and SUD parity mandates with respect to crime rates of all subcategories.

The upper panel of Table 6 presents the TSLS Stage-II estimates for the effect of SUD treatment rate on crime rates (Table 6 upper panel columns 1 & 2).We found that, consistent with the estimated effect of HIFA-waiver expansions on crime rates, increased SUD treatment rate reduced the

[19] CBSA is a geographic area defined by the Office of Management and Budget (OMB) based around an urban center of at least 10,000 residents and adjacent areas that are socioeconomically tied to the urban center as determined by commuting patterns.

[20] For instance, Boston-Cambridge-Quincy is a CBSA that consists of 5 Massachusetts counties and 2 New Hampshire counties. Given that Massachusetts implemented an HIFA-waiver expansion between 2007 and 2008, while New Hampshire implemented an SUD parity mandate between 2004 and 2008, we aggregated the 5 Massachusetts counties to a CBSA-like group, and aggregated the 2 New Hampshire counties to another CBSA-like group.

[21] We also aggregated the data to the state level and the pre/post two-time period and re-estimated the effect of policy instruments on the SUD treatment rate. We used Donald and Lang (2007) method coupled with the two-step procedure described in Bertrand, Duflo and Mullainathan (2004, pp. 267) to accommodate the different effective times of the policies. Despite such an approach being quite restrictive, we found that the implementation of HIFA-waiver expansions alone increased the state-level SUD treatment rate by 2.47 per 1000 residents (S.E. = 0.89, $t = 2.80$), with an F-statistic of 7.8. When including both policies simultaneously, the implementation of HIFA-waiver expansions increased the treatment rate by 2.33 per 1000 residents (S.E. = 1.02, $t = 2.28$); the implementation of SUD parity mandates increased the SUD treatment rate by 1.73 per 1000 residents (S.E. = 0.58, $t = 3.01$), with an F-statistic of 6.6.

Table 4
Estimated effect of HIFA-waiver expansions on specific property crime rates per 1000 residents: Balanced-panel.

Property crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates							
	Burglary rate		Larceny theft rate		Motor vehicle theft rate		Arson rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	−0.01 (0.01)		−0.14† (0.07)		−0.03† (0.02)		−0.001 (0.001)	
2-year before T_{HIFA}[a]		0.01 (0.02)		0.04 (0.09)		−0.02 (0.03)		−0.001 (0.002)
1-year before T_{HIFA}		0.03* (0.01)		−0.02 (0.04)		0.01 (0.03)		−0.001 (0.002)
Year of T_{HIFA}		0.02 (0.03)		−0.15 (0.10)		−0.04† (0.02)		−0.002† (0.001)
1-year after T_{HIFA}		−0.05 (0.05)		−0.14* (0.06)		−0.01 (0.01)		0.001 (0.001)
2-year after T_{HIFA}		0.01 (0.01)		−0.19† (0.10)		−0.03† (0.02)		0.001 (0.001)
Property crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *without* county & state covariates							
	Burglary rate		Larceny theft rate		Motor vehicle theft rate		Arson rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	−0.01 (0.02)		−0.21* (0.09)		−0.03 (0.03)		0.001 (0.001)	
2-year before T_{HIFA}		0.02 (0.03)		0.02 (0.04)		0.02 (0.02)		−0.001 (0.001)
1-year before T_{HIFA}		0.01 (0.01)		0.03 (0.06)		0.01 (0.02)		−0.001 (0.001)
Year of T_{HIFA}		0.01 (0.01)		−0.13* (0.06)		−0.02 (0.03)		−0.001 (0.001)
1-year after T_{HIFA}		0.03 (0.04)		−0.22* (0.11)		−0.01 (0.03)		0.001 (0.001)
2-year after T_{HIFA}		−0.03 (0.02)		−0.17† (0.10)		−0.02 (0.01)		−0.001 (0.002)
# observations	22,328	22,328	22,328	22,328	22,328	22,328	22,328	22,328

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

rates of robbery, aggravated assault and larceny theft. Specifically, an increase in the SUD treatment rate of 1 per 1000 residents reduced the robbery rate by 0.03 to 0.04 per 1000 residents, reduced the aggravated assault rate by 0.16 to 0.18 per 1000 residents, and reduced the larceny theft rate by 0.52 to 0.54 per 1000 residents. Translating the estimated marginal effects into elasticity, the treatment-crime elasticity is −0.3 for robbery, −0.6 to −0.7 for aggravated assault, and −0.3 for larceny theft. The estimates without controls (Table 6 upper panel columns 3–4) were similar to those with controls. There is also additional evidence for a reduction in motor vehicle theft rate (Table 6 upper panel columns 3–4 row 9).

Appendix Tables A16–A19 present the estimates from the sensitivity analyses with the unbalanced panel, with state-specific trends, with adjacent-county means, and at the CBSA level. The TSLS Stage-I estimates from these sensitivity analyses indicate that the policy instruments remained strong. The TSLS Stage-II estimates for the crime-reduction effect of the increased SUD treatment rate was generally consistent with our main county-level estimates for the rates of robbery, aggravated assault, and larceny theft. An additional set of sensitivity analyses estimated the crime-reduction effect of the SUD treatment rate, using SUD parity mandates as the instrument (Eq. (16): TSLS Stage I) to check the robustness of our selection of instruments.

$$SUD\ Treatment\ Rate_{c,s,t} = \alpha_1 + \alpha_2 Parity_{s,t} + \alpha_3 X_{c,s,t} + \alpha_4 X_{s,t} + \rho_c + \tau_t + \varepsilon_{c,s,t} \quad (16)$$

Fig. 1. Estimated effect of HIFA-waiver expansions on total crime rates per 1000 residents: Balanced Panel with county & years fixed effects, with county & state covariates. Note: †$p < 0.10$, *$p < 0.05$, **$p < 0.01$. Standard errors used to calculate 95% CI are clustered at the state level. The reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

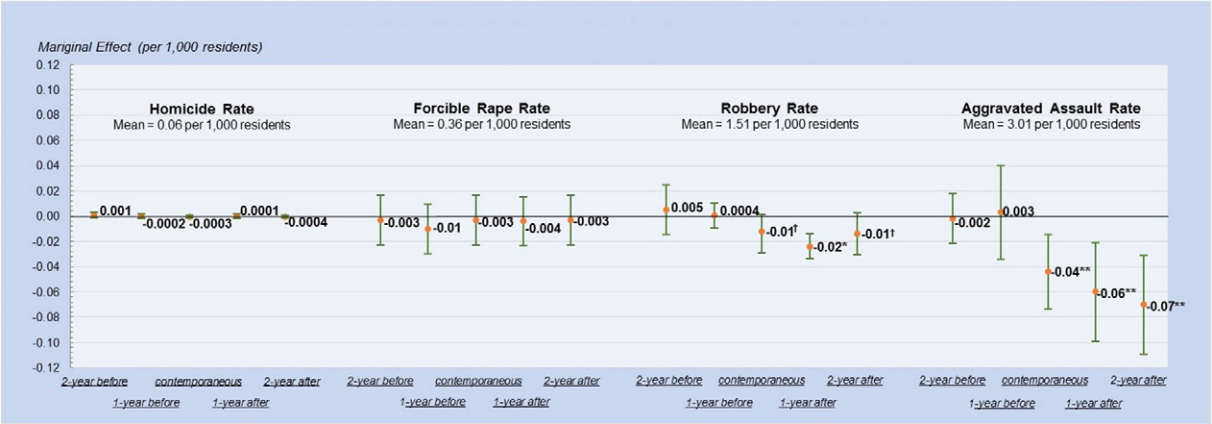


Fig. 2. Estimated effect of HIFA-waiver expansions on violent crime rates per 1000 residents: Balanced panel with county & years fixed effects, with county & state covariates. Note: $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$. Standard errors used to calculate 95% CI are clustered at the state level. The reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Note that SUD parity mandates were shown to be a weaker instrument than HIFA-waiver expansions (Appendix Table A20 bottom panel columns 1 & 4: TSLS Stage I). Thus the effects of the SUD treatment on some crime subcategories were less precisely estimated (Appendix Table A20 upper panel columns 1 & 4 TSLS Stage I). Furthermore, when instrumenting with the SUD parity mandates alone, the estimated reduction in larceny theft rate was larger than when instrumenting with HIFA-waiver expansions alone and with both policies, whereas the reductions in the rates of robbery and aggravated assault were smaller.

It is worth noting that as with any observational study, we cannot establish a definitive causal chain from state HIFA-waiver expansions, through SUD treatment use and substance use, to reducing crimes. A key assumption of the IV analysis is that a valid instrument should affect the outcome only through the independent variable of interest. Although our rigorous statistical tests and consistent results lend weight to the validity of our instrument, we acknowledge that the HIFA-waiver expansions may affect crimes through pathways other than SUD treatment use. Therefore, our pathway effect estimates are exploratory and provide suggestive evidence that SUD treatment use is one of the potential pathways through which the expansions leads to the crime reduction.

6. Estimating the pathway effect of SUD treatment rate on substance use prevalence

We took one step further to explore the pathway effect of SUD treatment rate on substance use prevalence using a TSLS model similar to the one used to estimate the pathway effect of treatment rate on crime rates. The previous Eqs. (13) and (14) capture the TSLS Stage I, and the following Eq. (16) captures the TSLS Stage II. Note that the level of aggregation here is substate region denoted by *r*. Our final sample includes a balanced panel of 347 substate regions over 8 years.

$$\begin{aligned} \textit{Substance Use Prevalence}_{r,s,t} = {} & \beta_1 + \beta_2 \textit{SUD Treatment Rate}_{r,s,t} \\ & + \beta_3 X_{r,s,t} + \beta_4 X_{r,s,t} + \rho_r + \tau_t + \varepsilon_{r,s,t} \end{aligned} \quad (17)$$

We examined the substate-level prevalence of alcohol use disorder and drug use disorder when using HIFA-waiver expansions as the lone instrument (Table 7 upper panel columns 1 and 3), and when using both HIFA-waiver expansions and SUD parity mandates as instruments (Table 7 upper panel columns 2 and 4). We found that an increase in the SUD treatment rate of 1 per 1000 residents reduced the prevalence rates drug use disorder by 0.21 to 0.25 per 1000 residents, equivalent to a relative 0.8 to 1.0 percent reduction. Or in terms of elasticity, the

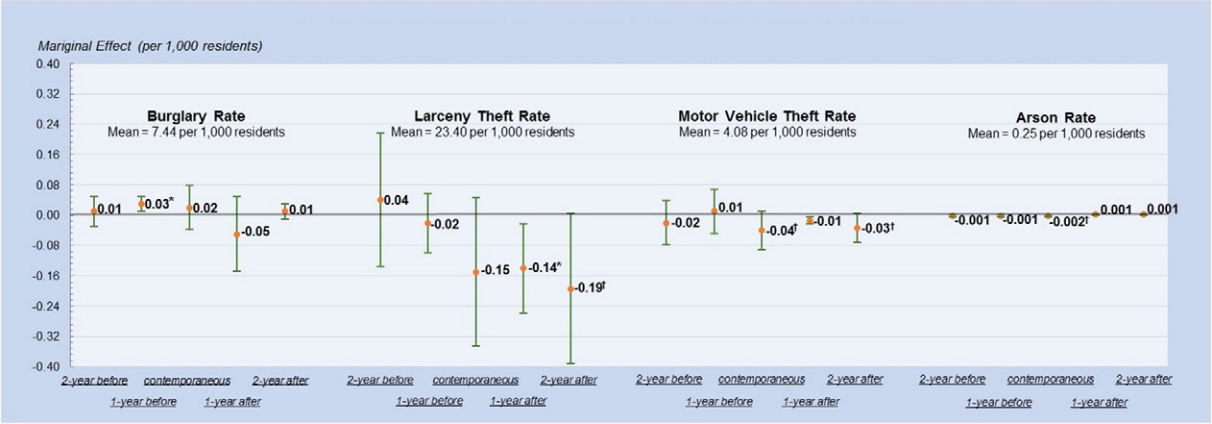


Fig. 3. Estimated effect of HIFA-waiver expansions on property crime rates per 1000 residents: Balanced panel with county & years fixed effects, with county & state covariates. Note: $^{\dagger}p < 0.10$, $^{*}p < 0.05$. Standard errors used to calculate 95% CI are clustered at the state level. The reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Table 5
Estimated effect of HIFA-waiver expansions on SUD treatment rate per 1000 residents: Balanced-panel.

SUD treatment rate per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates	
	SUD treatment rate	
	(1)	(2)
Policy lags & leads		
HIFA (0/1)	2.57*** (0.52)	
2-year before T_{HIFA}[a]		−0.33 (0.70)
1-year before T_{HIFA}		−0.54 (0.69)
Year of T_{HIFA}		1.85* (0.78)
1-year after T_{HIFA}		3.03*** (0.65)
2-year after T_{HIFA}		1.40† (0.75)
SUD treatment rate per 1000 residents	County-level balanced panel *with* county & years fixed effects, *without* county & state covariates	
	SUD treatment rate	
	(1)	(2)
Policy lags & leads		
HIFA (0/1)	3.34*** (0.64)	
2-year before T_{HIFA}		−0.29 (0.66)
1-year before T_{HIFA}		−0.45 (0.32)
Year of T_{HIFA}		3.40* (1.71)
1-year after T_{HIFA}		3.08** (0.60)
2-year after T_{HIFA}		2.17*** (0.55)
# observations	22,328	22,328

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

treatment-prevalence elasticity is −0.1 for drug use disorder. We also find a reduction in the prevalence of alcohol use disorder when instrumenting with both policies, albeit imprecisely estimated. The TSLS Stage-I estimates (Table 7 bottom panel) confirms that the policy instruments remain strong at the substate region level.

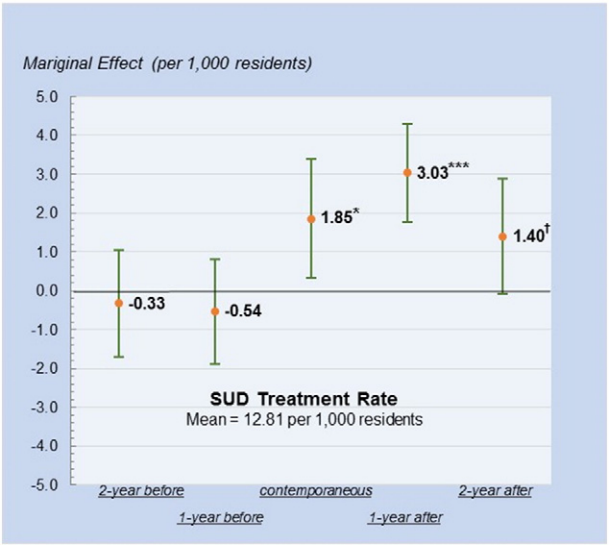


Fig. 4. Estimated effect of HIFA-waiver expansions on SUD treatment rates per 1000 residents. Note: †$p < 0.10$, *$p < 0.05$, ***$p < 0.001$. Standard errors used to calculate 95% CI are clustered at the state level. The reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

7. Discussion

Expanding public insurance coverage holds the potential to increase individual SUD treatment use, reduce substance use, and promote public safety through crime reduction. The main contribution of our study is that we uncovered a heretofore unrecognized relationship between the implementation of Medicaid expansions and the reduction in crime rates: we found that Medicaid expansions under HIFA-waiver reduced the rates of robbery, aggravated assault

Table 6
Estimated effect of SUD treatment rate on crime rates: Balanced-panel.

Crime rates per 1000 residents	County-level balanced panel			
	With county & years fixed effects, *with* county & state covariates		*With* county & years fixed effects, *without* county & state covariates	
	(1)	(2)	(3)	(4)
TSLS Stage II: Effect of SUD treatment on crimes				
Violent crime rates	−0.24* (0.11)	−0.21* (0.10)	−0.23*** (0.07)	−0.19* (0.08)
Criminal homicide	0.0006 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
Forcible rape	0.005 (0.05)	−0.02 (0.03)	−0.003 (0.04)	−0.02 (0.02)
Robbery	−0.04† (0.02)	−0.03† (0.02)	−0.04* (0.02)	−0.02* (0.01)
Aggravated assault	−0.18* (0.09)	−0.16† (0.09)	−0.18** (0.06)	−0.14* (0.06)
Property crime rates	−0.67* (0.31)	−0.71† (0.42)	−0.67* (0.27)	−0.70* (0.34)
Burglary	−0.05 (0.11)	−0.07 (0.10)	−0.04 (0.06)	−0.05 (0.06)
Larceny theft	−0.52 (0.33)	−0.54† (0.34)	−0.54* (0.26)	−0.55* (0.27)
Motor vehicle theft	−0.10 (0.06)	−0.11 (0.07)	−0.09† (0.05)	−0.10* (0.05)
Arson	0.004 (0.003)	−0.001 (0.01)	0.002 (0.002)	−0.005 (0.01)
TSLS Stage I: Effect of health insurance policy on SUD treatment				
HIFA (0/1)	2.57*** (0.52)	2.50*** (0.47)	3.34*** (0.64)	3.13*** (0.71)
Parity (0/1)		0.86* (0.45)		1.27*** (0.32)
# observations	22,328	22,328	22,328	22,328
F-statistic[a]	28.5	23.2	24.6	14.6

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.
[a] Stock and Yogo (2005) weak identification test critical values based on maximal TSLS size of a 5% Wald test of $\beta = \beta_0$ (size test): K1 = 1 & L1 = 1: 10%: 16.38; 15%: 8.96; 20%: 6.66; 25%: 5.53; K1 = 1 & L1 = 2: 10%: 19.93; 15%: 11.59; 20%: 8.75; 25%: 7.25.

Table 7
Estimated effect of SUD treatment rate on substance use prevalence.

Substance use prevalence per 1000 residents	Substate region-level panel	
	(1)	(2)
TSLS Stage II: Effect of SUD treatment on substance use		
Alcohol use disorder	−0.23 (0.15)	−0.25† (0.13)
Drug use disorder	−0.27* (0.13)	−0.21* (0.09)
TSLS Stage-I: Effect of health insurance policy on SUD treatment		
HIFA (0/1)	2.37* (1.15)	2.27** (0.77)
Parity (0/1)		1.29** (0.52)
# observations	2776	2776
F-statistic[a]	8.4	11.5

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
[a] Stock and Yogo (2005) weak identification test critical values based on maximal TSLS size of a 5% Wald test of $\beta = \beta_0$ (size test): K1 = 1 & L1 = 1: 10%: 16.38; 15%: 8.96; 20%: 6.66; 25%: 5.53; K1 = 1 & L1 = 2: 10%: 19.93; 15%: 11.59; 20%: 8.75; 25%: 7.25.

and larceny theft. Our study also contributes to the literature by revealing one of the important pathways between Medicaid expansions and crime reduction: the pathway from HIFA-waiver expansions to increasing SUD treatment rate, reducing substance use prevalence, and eventually to reducing crime rates. Among these pathway effects, the effect of increased treatment rate on reducing crime rates is particularly noteworthy. We found a relative 10 percent increase in the SUD treatment rate can reduce the rate of robbery by 3%, reduce the rate of aggravated assault by 6 to 7%, and reduce the rate of larceny theft by 3%.

Out study findings have important public policy implications. To better understand the policy implications, we further provide a back-of-the-envelope cost-benefit calculation. The best available estimates of the costs of crime come from Rajkumar and French (1997) and McCollister et al. (2010), which estimate the per-offense cost of crime across all major crime categories. These estimated costs of crime attempt to capture the direct tangible losses to crime victims and to the criminal justice system, the opportunity costs associated with the criminal's choice to engage in illegal rather than legal activities, as well as indirect and intangible losses suffered by crime victims, including pain and suffering, decreased quality of life, and psychological distress. Based on Rajkumar and French (1997) and McCollister et al. (2010), the annual costs are roughly $15 billion to $19 billion for robbery, $8 billion to $25 billion for aggravated assault, and $65 billion to $92 billion for larceny theft (2008 dollars). Given that the national spending on SUD treatment is approximately $16 billion annually (Mark et al., 2007), a 10 percent increase in treatment rate at an average cost of $1.6 billion can yield an average benefit of $2.9 billion to $5.1 billion from reducing crime rates. Thus the benefit-cost ratio of increased SUD treatment with respect to crime reduction ranges from 1.8 to 3.2. To put these numbers into context, incarceration, which has been attributed to one third of decline in crime during the 1990s, has a benefit-cost ratio centered around 1.5 (Levitt, 1996; Levitt, 2004). Therefore, SUD treatment not only appears to be a more effective but also a more cost-effective alternative to incarceration at reducing crime.

In conclusion, expanding public insurance coverage is an effective policy lever to encourage treatment use and reduce substance use, which in turn, can cost-effectively reduce crimes.

Appendix A

Appendix Table A1
Estimated effect of HIFA-waiver expansions on total crime rates: Unbalanced-panel.

Total crime rates per 1000 residents	County-level unbalanced panel *with* county & years fixed effects, *with* county & state covariates					
	Total crime rate		Violent crime rate		Property crime rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Policy lags & leads						
HIFA (0/1)	−0.21* (0.08)		−0.04† (0.02)		−0.17* (0.08)	
2-year before T_{HIFA}[a]		0.04 (0.05)		0.01 (0.02)		0.03 (0.04)
1-year before T_{HIFA}		−0.01 (0.02)		−0.02 (0.02)		0.02 (0.04)
Year of T_{HIFA}		−0.17† (0.09)		−0.02** (0.01)		−0.15† (0.09)
1-year after T_{HIFA}		−0.23* (0.12)		−0.04* (0.02)		−0.19† (0.10)
2-year after T_{HIFA}		−0.26* (0.12)		−0.05* (0.02)		−0.21† (0.11)
Total crime rates per 1000 residents	County-level unbalanced panel *with* county & years fixed effects, *without* county & state covariates					
	Total crime rate		Violent crime rate		Property crime rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Policy lags & leads						
HIFA (0/1)	−0.25*** (0.06)		−0.04† (0.02)		−0.21** (0.07)	
2-year before T_{HIFA}		0.01 (0.02)		−0.02 (0.02)		0.03 (0.04)
1-year before T_{HIFA}		0.01 (0.02)		0.01 (0.01)		0.01 (0.02)
Year of T_{HIFA}		−0.20† (0.11)		−0.04* (0.01)		−0.16† (0.10)
1-year after T_{HIFA}		−0.20** (0.07)		−0.04* (0.02)		−0.16* (0.07)
2-year after T_{HIFA}		−0.23* (0.10)		−0.04† (0.02)		−0.20* (0.09)
# observations	23,537	23,537	23,537	23,537	23,537	23,537

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A2
Estimated effect of HIFA-waiver expansions on violent crime rates: Unbalanced-panel.

Violent crime rates per 1000 residents	County-level unbalanced panel *with* county & years fixed effects, *without* county & state covariates							
	Homicide rate		Forcible rape rate		Robbery rate		Aggravate assault rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	0.00002 (0.0004)		0.001 (0.02)		−0.02* (0.01)		−0.03* (0.01)	
2-year before T_{HIFA}[a]		0.003 (0.002)		−0.01 (0.01)		0.005† (0.003)		0.001 (0.01)
1-year before T_{HIFA}		−0.0002 (0.001)		−0.01 (0.01)		−0.001 (0.01)		−0.02 (0.02)
Year of T_{HIFA}		−0.0004 (0.001)		−0.001 (0.01)		−0.01 (0.01)		−0.02* (0.01)
1-year after T_{HIFA}		0.0001 (0.001)		−0.004 (0.01)		−0.02* (0.01)		−0.03* (0.01)
2-year after T_{HIFA}		−0.0002 (0.001)		−0.002 (0.01)		−0.01* (0.005)		−0.04** (0.01)
Violent crime rates per 1000 residents	County-level unbalanced panel *with* county & years fixed effects, *with* county & state covariates							
	Homicide rate		Forcible rape rate		Robbery rate		Aggravate assault rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	0.00003 (0.0005)		0.004 (0.02)		−0.01* (0.004)		−0.03** (0.01)	
2-year before T_{HIFA}		−0.0005 (0.001)		−0.01 (0.01)		−0.003 (0.01)		−0.01 (0.01)
1-year before T_{HIFA}		0.0003 (0.002)		−0.004 (0.01)		0.006 (0.01)		0.01 (0.01)
Year of T_{HIFA}		−0.001 (0.001)		0.001 (0.01)		−0.01* (0.003)		−0.04** (0.01)
1-year after T_{HIFA}		0.0002 (0.001)		−0.002 (0.01)		−0.01* (0.003)		−0.03* (0.01)
2-year after T_{HIFA}		−0.00001 (0.0002)		−0.005 (0.01)		−0.01* (0.005)		−0.03** (0.01)
# observations	23,537	23,537	23,537	23,537	23,537	23,537	23,537	23,537

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A3

Estimated effect of HIFA-waiver expansions on property crime rates: Unbalanced-panel.

Property crime rates per 1000 residents	County-level unbalanced panel *with* county & years fixed effects, *with* county & state covariates							
	Burglary rate		Larceny theft rate		Motor vehicle theft rate		Arson rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	−0.01 (0.02)		−0.14* (0.06)		−0.03† (0.02)		−0.001 (0.001)	
2-year before T_{HIFA}[a]		0.03† (0.02)		0.03 (0.07)		−0.02 (0.03)		−0.001 (0.002)
1-year before T_{HIFA}		0.02* (0.01)		−0.01 (0.02)		0.01 (0.03)		−0.001 (0.002)
Year of T_{HIFA}		0.04 (0.05)		−0.14* (0.07)		−0.03† (0.01)		−0.001 (0.001)
1-year after T_{HIFA}		−0.06 (0.05)		−0.11 (0.06)		−0.02 (0.03)		0.001 (0.001)
2-year after T_{HIFA}		0.01 (0.03)		−0.17† (0.09)		−0.04 (0.03)		0.001 (0.001)
Property crime rates per 1000 residents	County-level unbalanced panel *with* county & years fixed effects, *without* county & state covariates							
	Burglary rate		Larceny theft rate		Motor vehicle theft rate		Arson rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	−0.01 (0.02)		−0.17* (0.05)		−0.03 (0.03)		0.001 (0.001)	
2-year before T_{HIFA}		0.05† (0.03)		0.02 (0.07)		0.01 (0.02)		−0.001 (0.001)
1-year before T_{HIFA}		0.01 (0.01)		0.01 (0.01)		0.01 (0.02)		−0.001 (0.001)
Year of T_{HIFA}		0.01 (0.02)		−0.14* (0.07)		−0.01 (0.02)		−0.002 (0.001)
1-year after T_{HIFA}		0.02 (0.02)		−0.16† (0.08)		−0.01 (0.03)		0.002 (0.001)
2-year after T_{HIFA}		−0.01 (0.01)		−0.16† (0.08)		−0.01 (0.01)		−0.001 (0.002)
# observations	23,537	23,537	23,537	23,537	23,537	23,537	23,537	23,537

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A4
Estimated effect of HIFA-waiver expansions on total crime rates: State-specific linear trends.

Total crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates, *with* state-specific trends					
	Total crime rate		Violent crime rate		Property crime rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Policy lags & leads						
HIFA (0/1)	−0.36** (0.12)		−0.06* (0.03)		−0.30** (0.10)	
2-year before T_{HIFA}[a]		0.05 (0.07)		0.005 (0.01)		0.05 (0.08)
1-year before T_{HIFA}		0.03 (0.03)		0.003 (0.01)		0.03 (0.03)
Year of T_{HIFA}		−0.23 (0.06)		−0.02* (0.01)		−0.21*** (0.06)
1-year after T_{HIFA}		−0.20* (0.08)		−0.04* (0.02)		−0.17* (0.08)
2-year after T_{HIFA}		−0.29* (0.13)		−0.04 (0.02)		−0.26* (0.12)
# observations	22,328	22,328	22,328	22,328	22,328	22,328

Standard errors in parentheses are clustered at the state level.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A5
Estimated effect of HIFA-waiver expansions on violent & property crime rates: State-specific linear trends.

Violent crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates, *with* state-specific trends							
	Homicide rate		Forcible rape rate		Robbery rate		Aggravate assault rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	−0.0002 (0.0002)		0.003 (0.01)		−0.01† (0.005)		−0.05** (0.02)	
2-year before T_{HIFA}[a]		0.001 (0.001)		0.004 (0.01)		0.002 (0.002)		−0.001 (0.01)
1-year before T_{HIFA}		−0.0005 (0.001)		0.002 (0.01)		−0.001 (0.003)		0.003 (0.01)
Year of T_{HIFA}		−0.001 (0.001)		0.005 (0.01)		−0.003 (0.004)		−0.02* (0.01)
1-year after T_{HIFA}		0.001 (0.001)		0.004 (0.01)		−0.01* (0.005)		−0.03* (0.02)
2-year after T_{HIFA}		−0.001 (0.001)		0.003 (0.01)		−0.01† (0.006)		−0.04* (0.02)
Property crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates, *with* state-specific trends							
	Burglary rate		Larceny theft rate		Motor vehicle theft rate		Arson rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	−0.04* (0.02)		−0.23* (0.10)		−0.03* (0.01)		−0.002 (0.002)	
2-year before T_{HIFA}		0.02 (0.02)		0.01 (0.02)		0.01 (0.01)		−0.001 (0.002)
1-year before T_{HIFA}		0.03 (0.01)		0.01 (0.02)		0.01 (0.01)		−0.001 (0.001)
Year of T_{HIFA}		−0.03* (0.01)		−0.24*** (0.06)		−0.01* (0.005)		−0.004 (0.003)
1-year after T_{HIFA}		−0.02† (0.01)		−0.16* (0.08)		−0.01 (0.005)		−0.002 (0.001)
2-year after T_{HIFA}		−0.03* (0.01)		−0.20† (0.11)		−0.03* (0.01)		−0.003 (0.002)
# observations	22,328	22,328	22,328	22,328	22,328	22,328	22,328	22,328

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A6
Estimated effect of HIFA-waiver expansions on total crime rates: Extended pre-HIFA window.

Total crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates					
	Total crime rate		Violent crime rate		Property crime rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Policy lags & leads						
HIFA (0/1)	−0.25* (0.10)		−0.05* (0.02)		−0.20* (0.08)	
5-year before T_{HIFA}[a]		0.04 (0.04)		−0.003 (0.01)		0.04 (0.05)
4-year before T_{HIFA}		−0.05 (0.08)		−0.005 (0.01)		−0.04 (0.08)
3-year before T_{HIFA}		−0.09 (0.10)		0.0002 (0.01)		−0.09 (0.09)
2-year before T_{HIFA}		0.05 (0.10)		−0.001 (0.01)		0.05 (0.10)
1-year before T_{HIFA}		0.02 (0.04)		0.005 (0.01)		0.01 (0.04)
Year of T_{HIFA}		−0.21* (0.08)		−0.05* (0.02)		−0.17** (0.06)
1-year after T_{HIFA}		−0.21* (0.08)		−0.05* (0.02)		−0.16* (0.07)
2-year after T_{HIFA}		−0.25* (0.12)		−0.07* (0.03)		−0.19* (0.09)

Standard errors in parentheses are clustered at the state level.
* $p < 0.05$.
** $p < 0.01$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A7
Estimated effect of HIFA-waiver expansions on total crime rates: Extended pre-HIFA window.

Total crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *without* county & state covariates					
	Total crime rate		Violent crime rate		Property crime rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Policy lags & leads						
HIFA (0/1)	−0.28* (0.12)		−0.07* (0.02)		−0.21* (0.09)	
5-year before T_{HIFA}[a]		−0.05 (0.07)		−0.004 (0.01)		−0.05 (0.05)
4-year before T_{HIFA}		0.07 (0.05)		−0.005 (0.01)		0.07 (0.04)
3-year before T_{HIFA}		−0.06 (0.07)		−0.002 (0.01)		−0.06 (0.07)
2-year before T_{HIFA}		0.02 (0.06)		−0.002 (0.01)		0.02 (0.06)
1-year before T_{HIFA}		0.02 (0.04)		0.001 (0.01)		0.02 (0.04)
Year of T_{HIFA}		−0.24* (0.12)		−0.06* (0.02)		−0.18* (0.07)
1-year after T_{HIFA}		−0.17* (0.08)		−0.08* (0.03)		−0.22† (0.12)
2-year after T_{HIFA}		−0.23* (0.12)		−0.08* (0.03)		−0.25† (0.14)

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A8
Estimated effect of HIFA-waiver expansions on violent crime rates: Extended pre-HIFA window.

Violent crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates							
	Homicide rate		Forcible rape rate		Robbery rate		Aggravate assault rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	0.0002 (0.0003)		−0.001 (0.01)		−0.01* (0.003)		−0.04* (0.02)	
5-year before T_{HIFA}[a]		−0.0005 (0.001)		−0.002 (0.01)		−0.001 (0.001)		−0.002 (0.01)
4-year before T_{HIFA}		0.0002 (0.001)		−0.004 (0.01)		−0.002 (0.001)		−0.003 (0.01)
3-year before T_{HIFA}		−0.0005 (0.001)		−0.004 (0.01)		−0.004 (0.003)		0.005 (0.01)
2-year before T_{HIFA}		0.0001 (0.001)		−0.004 (0.01)		0.004 (0.004)		−0.003 (0.01)
1-year before T_{HIFA}		−0.0002 (0.001)		−0.004 (0.01)		−0.001 (0.002)		0.01 (0.01)
Year of T_{HIFA}		−0.0004 (0.001)		−0.001 (0.01)		−0.004* (0.002)		−0.04** (0.01)

Appendix Table A8 (*continued*)

Violent crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates							
	Homicide rate		Forcible rape rate		Robbery rate		Aggravate assault rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1-year after T_{HIFA}		−0.0002 (0.001)		−0.003 (0.01)		−0.01* (0.005)		−0.06* (0.03)
2-year after T_{HIFA}		0.0003 (0.0004)		−0.003 (0.01)		−0.01† (0.005)		−0.06* (0.03)

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A9
Estimated effect of HIFA-waiver expansions on violent crime rates: Extended pre-HIFA window.

Violent crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *without* county & state covariates							
	Homicide rate		Forcible rape rate		Robbery rate		Aggravate assault rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	0.00004 (0.0003)		−0.001 (0.01)		−0.01* (0.004)		−0.06* (0.02)	
5-year before T_{HIFA}[a]		0.0003 (0.001)		0.003 (0.01)		0.001 (0.004)		−0.005 (0.01)
4-year before T_{HIFA}		0.0004 (0.001)		−0.002 (0.01)		−0.002 (0.003)		0.004 (0.01)
3-year before T_{HIFA}		−0.0003 (0.001)		−0.004 (0.01)		0.001 (0.004)		−0.01 (0.02)
2-year before T_{HIFA}		0.0001 (0.001)		0.002 (0.01)		0.001 (0.001)		0.01 (0.02)
1-year before T_{HIFA}		−0.0004 (0.001)		0.002 (0.01)		−0.001 (0.002)		0.01 (0.01)
Year of T_{HIFA}		−0.0004 (0.001)		−0.001 (0.01)		−0.005* (0.002)		−0.06** (0.02)
1-year after T_{HIFA}		−0.0003 (0.001)		−0.002 (0.01)		−0.01* (0.004)		−0.06** (0.02)
2-year after T_{HIFA}		−0.0001 (0.0003)		−0.005 (0.01)		−0.01† (0.005)		−0.07* (0.03)

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A10
Estimated effect of HIFA-waiver expansions on property crime rates: Extended pre-HIFA window.

Property crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates							
	Burglary rate		Larceny theft rate		Motor vehicle theft rate		Arson rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	−0.01 (0.01)		−0.17* (0.07)		−0.03 (0.02)		−0.001 (0.001)	
5-year before T_{HIFA}[a]		0.02 (0.01)		−0.02 (0.04)		0.01 (0.01)		−0.0003 (0.001)
4-year before T_{HIFA}		−0.01 (0.03)		0.02 (0.04)		−0.01 (0.01)		−0.0002 (0.001)
3-year before T_{HIFA}		−0.02 (0.03)		−0.05 (0.05)		−0.03 (0.02)		0.002 (0.001)
2-year before T_{HIFA}		0.03 (0.03)		0.01 (0.04)		0.02 (0.01)		−0.001 (0.001)

(*continued on next page*)

Appendix Table A10 (*continued*)

Property crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates							
	Burglary rate		Larceny theft rate		Motor vehicle theft rate		Arson rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1-year before T_{HIFA}		−0.01 (0.02)		0.002 (0.03)		0.01 (0.01)		−0.001 (0.001)
Year of T_{HIFA}		0.01 (0.01)		−0.14† (0.07)		−0.03 (0.02)		−0.001 (0.001)
1-year after T_{HIFA}		−0.02† (0.01)		−0.13* (0.06)		−0.02 (0.01)		−0.002 (0.001)
2-year after T_{HIFA}		−0.03* (0.01)		−0.16* (0.06)		−0.02 (0.01)		−0.001 (0.002)

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A11
Estimated effect of HIFA-waiver expansions on property crime rates: Extended pre-HIFA window.

Property crime rates per 1000 residents	County-level balanced panel *with* county & years fixed effects, *without* county & state covariates							
	Burglary rate		Larceny theft rate		Motor vehicle theft rate		Arson rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy lags & leads								
HIFA (0/1)	−0.01 (0.01)		−0.19* (0.08)		−0.02 (0.01)		−0.001 (0.001)	
5-year before T_{HIFA}[a]		−0.02 (0.02)		−0.02 (0.04)		−0.01 (0.02)		0.0004 (0.001)
4-year before T_{HIFA}		0.01† (0.01)		0.03 (0.03)		0.01 (0.01)		0.001 (0.001)
3-year before T_{HIFA}		−0.03 (0.02)		−0.02 (0.06)		0.01 (0.02)		0.001 (0.001)
2-year before T_{HIFA}		0.004 (0.02)		0.01 (0.01)		0.01 (0.01)		−0.0002 (0.001)
1-year before T_{HIFA}		0.002 (0.02)		−0.004 (0.02)		0.01 (0.005)		−0.002† (0.001)
Year of T_{HIFA}		0.01 (0.01)		−0.10† (0.06)		−0.01 (0.01)		0.001 (0.002)
1-year after T_{HIFA}		0.02 (0.02)		−0.24* (0.10)		−0.02 (0.01)		−0.002 (0.001)
2-year after T_{HIFA}		−0.02† (0.01)		−0.20† (0.11)		−0.02 (0.01)		−0.002 (0.002)

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A12
Estimated effect of HIFA-waiver expansions on SUD treatment rate: Unbalanced-panel.

SUD treatment rate per 1000 residents	County-level unbalanced panel *with* county & years fixed effects, *with* county & state covariates	
	SUD treatment rate	
	(1)	(2)
Policy lags & leads		
HIFA (0/1)	2.67*** (0.41)	
2-year before T_{HIFA}[a]		−0.36 (0.72)
1-year before T_{HIFA}		−0.47 (0.65)
Year of T_{HIFA}		2.17* (1.04)
1-year after T_{HIFA}		2.75** (0.60)
2-year after T_{HIFA}		1.66* (0.95)

Appendix Table A12 (*continued*)

SUD treatment rate per 1000 residents	County-level unbalanced panel *with* county & years fixed effects, *with* county & state covariates	
	SUD treatment rate	
	(1)	(2)
Policy lags & leads		
HIFA (0/1)	3.12*** (0.43)	
2-year before T_{HIFA}		0.12 (0.21)
1-year before T_{HIFA}		−0.22 (0.19)
Year of T_{HIFA}		2.98* (1.09)
1-year after T_{HIFA}		3.52* (1.72)
2-year after T_{HIFA}		3.15** (1.08)
# observations	23,537	23,537

Standard errors in parentheses are clustered at the state level.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A13
Estimated effect of HIFA-waiver expansions on SUD treatment rate: State-specific linear trends.

SUD treatment rate per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates, *with* state-specific trends	
	SUD treatment rate	
	(1)	(2)
Policy lags & leads		
HIFA (0/1)	3.52** (1.06)	
2-year before T_{HIFA}[a]		−0.12 (0.14)
1-year before T_{HIFA}		−0.28 (0.18)
Year of T_{HIFA}		3.79* (1.88)
1-year after T_{HIFA}		3.58** (1.23)
2-year after T_{HIFA}		2.72* (1.31)
# observations	22,328	22,328

Standard errors in parentheses are clustered at the state level.
* $p < 0.05$.
** $p < 0.01$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 3-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A14
Estimated effect of HIFA-waiver expansions on SUD treatment rate: Extended pre-HIFA window.

SUD treatment rate per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates	
	SUD treatment rate	
	(1)	(2)
Policy lags & leads		
HIFA (0/1)	2.34*** (0.45)	
5-year before T_{HIFA}[a]		0.27 (0.25)
4-year before T_{HIFA}		0.57 (0.95)
3-year before T_{HIFA}		−0.20 (0.21)

(*continued on next page*)

Appendix Table A14 (*continued*)

SUD treatment rate per 1000 residents	County-level balanced panel *with* county & years fixed effects, *with* county & state covariates	
	SUD treatment rate	
	(1)	(2)
2-year before T_{HIFA}		−0.16 (0.26)
1-year before T_{HIFA}		−0.28 (0.37)
Year of T_{HIFA}		2.33*** (0.71)
1-year after T_{HIFA}		2.14** (0.82)
2-year after T_{HIFA}		2.16* (1.08)

Standard errors in parentheses are clustered at the state level.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A15
Estimated effect of HIFA-waiver expansions on SUD treatment rate: Extended pre-HIFA window.

SUD treatment rate per 1000 residents	County-level balanced panel *with* county & years fixed effects, *without* county & state covariates	
	SUD treatment rate	
	(1)	(2)
Policy lags & leads		
HIFA (0/1)	2.42** (0.72)	
5-year before T_{HIFA}[a]		−0.17 (0.21)
4-year before T_{HIFA}		0.76 (0.08)
3-year before T_{HIFA}		−0.27 (0.22)
2-year before T_{HIFA}		−0.13 (0.20)
1-year before T_{HIFA}		−0.45 (0.62)
Year of T_{HIFA}		2.35* (1.02)
1-year after T_{HIFA}		2.12** (0.62)
2-year after T_{HIFA}		2.28* (0.95)

Standard errors in parentheses are clustered at the state level.
* $p < 0.05$.
** $p < 0.01$.
[a] T_{HIFA} indicates the first full year after the effective time of HIFA-waiver expansion; the reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Table A16
Estimated effect of SUD treatment rate on crime rates: Unbalanced-panel & CBSA-level *with* county & years fixed effects, *with* county & state covariates.

Crime rates per 1000 residents	County-level				CBSA-level			
	Balanced panel		Unbalanced panel		Balanced panel		Unbalanced panel	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TSLS Stage II: Effect of SUD treatment on crimes								
Violent crime rates	−0.24* (0.11)	−0.21* (0.10)	−0.14† (0.07)	−0.13* (0.07)	−0.14* (0.05)	−0.12* (0.04)	−0.11 (0.09)	−0.09† (0.05)
Criminal homicide	0.0006 (0.001)	0.001 (0.001)	0.0002 (0.001)	0.001 (0.001)	−0.001 (0.001)	−0.001 (0.001)	−0.002 (0.001)	−0.002 (0.001)

Appendix Table A16 (*continued*)

Crime rates per 1000 residents	County-level				CBSA-level			
	Balanced panel		Unbalanced panel		Balanced panel		Unbalanced panel	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forcible rape	−0.005 (0.05)	−0.02 (0.03)	0.01 (0.06)	−0.01 (0.04)	−0.02† (0.01)	−0.02 (0.01)	0.01 (0.06)	0.006 (0.04)
Robbery	−0.04† (0.02)	−0.03† (0.02)	−0.03† (0.02)	−0.03* (0.02)	−0.02* (0.01)	−0.02* (0.01)	−0.03* (0.01)	−0.03* (0.01)
Aggravated assault	−0.18* (0.09)	−0.16† (0.09)	−0.12* (0.05)	−0.10† (0.05)	−0.08* (0.04)	−0.07* (0.04)	−0.08† (0.05)	−0.06 (0.04)
Property crime rates	−0.67* (0.31)	−0.71† (0.42)	−0.67* (0.32)	−0.72† (0.41)	−0.42 (0.36)	−0.43† (0.26)	−0.52 (0.34)	−0.58† (0.35)
Burglary	−0.05 (0.11)	−0.07 (0.10)	−0.05 (0.08)	−0.07 (0.09)	−0.01 (0.05)	−0.04 (0.06)	−0.03 (0.06)	−0.05 (0.07)
Larceny theft	−0.52 (0.33)	−0.54† (0.34)	−0.50† (0.28)	−0.55* (0.30)	−0.38 (0.24)	−0.36† (0.22)	−0.43† (0.26)	−0.46† (0.27)
Motor vehicle theft	−0.10 (0.06)	−0.11 (0.07)	−0.13 (0.09)	−0.11† (0.06)	−0.02 (0.03)	−0.04 (0.03)	−0.06 (0.06)	−0.07 (0.05)
Arson	0.004 (0.003)	−0.001 (0.01)	0.001 (0.005)	−0.0004 (0.01)	−0.001 (0.002)	−0.002 (0.004)	−0.004 (0.005)	−0.005 (0.006)
TSLS Stage I: Effect of health insurance policy on SUD treatment								
HIFA (0/1)	2.57*** (0.52)	2.50*** (0.47)	2.67*** (0.41)	2.60*** (0.35)	4.36** (1.41)	4.32** (1.38)	3.96** (1.26)	3.93** (1.21)
Parity (0/1)		0.86* (0.45)		0.91* (0.42)		1.39* (0.65)		1.47* (0.72)
# observations	22,328	22,328	23,537	23,537	7790	7790	7419	7419
F-statistic[a]	24.6	14.6	42.0	29.5	14.1	9.4	17.6	10.8

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.
[a] Stock and Yogo (2005) weak identification test critical values based on maximal TSLS size of a 5% Wald test of $\beta = \beta_0$ (size test): K1 = 1 & L1 = 1: 10%: 16.38; 15%: 8.96; 20%: 6.66; 25%: 5.53; K1 = 1 & L1 = 2: 10%: 19.93; 15%: 11.59; 20%: 8.75; 25%: 7.25.

Appendix Table A17
Estimated effect of SUD treatment rate on crime rates: Unbalanced-panel & CBSA-level *with* county & years fixed effects, *without* county & state covariates.

TSLS estimates	County-level				CBSA-level			
	Balanced panel		Unbalanced panel		Balanced panel		Unbalanced panel	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TSLS Stage II: Effect of SUD treatment on crimes								
Violent crime rates	−0.23*** (0.07)	−0.19* (0.08)	−0.15** (0.06)	−0.10* (0.04)	−0.12*** (0.03)	−0.10*** (0.03)	−0.10* (0.06)	−0.07† (0.04)
Criminal homicide	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	−0.0004 (0.001)	−0.0003 (0.0005)	−0.001 (0.001)	−0.001 (0.001)
Forcible rape	−0.003 (0.04)	−0.02 (0.02)	0.01 (0.05)	−0.01 (0.02)	−0.02† (0.01)	−0.02† (0.01)	0.01 (0.04)	0.01 (0.03)
Robbery	−0.04* (0.02)	−0.02* (0.01)	−0.03** (0.01)	−0.02* (0.01)	−0.02*** (0.004)	−0.02** (0.005)	−0.02* (0.01)	−0.03** (0.01)
Aggravated assault	−0.18** (0.06)	−0.14* (0.06)	−0.14** (0.05)	−0.07* (0.03)	−0.08*** (0.02)	−0.07** (0.02)	−0.09** (0.03)	−0.05† (0.03)
Property crime rates	−0.67* (0.27)	−0.70* (0.34)	−0.70* (0.31)	−0.74* (0.33)	−0.46† (0.26)	−0.48* (0.22)	−0.57† (0.31)	−0.58† (0.32)
Burglary	−0.04 (0.06)	−0.05 (0.06)	−0.03 (0.07)	−0.05 (0.07)	−0.01 (0.03)	−0.01 (0.04)	−0.02 (0.04)	−0.02 (0.05)
Larceny theft	−0.54* (0.26)	−0.55* (0.27)	−0.54* (0.26)	−0.60* (0.27)	−0.43† (0.23)	−0.45† (0.27)	−0.46* (0.22)	−0.48* (0.21)
Motor vehicle theft	−0.09† (0.05)	−0.10* (0.05)	−0.14† (0.08)	−0.08† (0.05)	−0.03 (0.02)	−0.02 (0.01)	−0.08 (0.07)	−0.08† (0.05)
Arson	0.002 (0.002)	−0.005 (0.01)	0.001 (0.004)	−0.004 (0.01)	−0.0003 (0.001)	−0.002 (0.002)	−0.003 (0.004)	−0.004 (0.004)
TSLS Stage I: Effect of health insurance policy on SUD treatment								
HIFA (0/1)	3.34*** (0.64)	3.13*** (0.71)	3.12*** (0.43)	3.01*** (0.46)	6.43*** (1.52)	6.32*** (1.57)	5.11*** (1.41)	5.01*** (1.44)
Parity (0/1)		1.27*** (0.32)		1.37*** (0.36)		1.43** (0.46)		1.50*** (0.67)
# observations	22,328	22,328	23,537	23,537	7419	7419	7790	7790
F-statistic[a]	28.5	23.2	50.9	33.6	27.8	18.8	22.1	14.9

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.
[a] Stock and Yogo (2005) weak identification test critical values based on maximal TSLS size of a 5% Wald test of $\beta = \beta_0$ (size test): K1 = 1 & L1 = 1: 10%: 16.38; 15%: 8.96; 20%: 6.66; 25%: 5.53; K1 = 1 & L1 = 2: 10%: 19.93; 15%: 11.59; 20%: 8.75; 25%: 7.25.

Appendix Table A18
Estimated effect of SUD treatment rate on crime rates: State-specific linear trends.

Crime rates per 1000 residents	County-level				CBSA-level			
	Balanced panel		Unbalanced panel		Balanced panel		Unbalanced panel	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TSLS Stage II: Effect of SUD Treatment on Crimes								
Violent crime rates	−0.16* (0.08)	−0.15† (0.08)	−0.15 (0.14)	−0.13 (0.11)	−0.22 (0.19)	−0.15 (0.14)	−0.18 (0.20)	−0.13 (0.21)
Criminal homicide	−0.0005 (0.001)	−0.0008 (0.001)	−0.002 (0.002)	−0.002 (0.002)	−0.007 (0.007)	−0.003 (0.003)	−0.008 (0.007)	−0.003 (0.002)
Forcible rape	0.008 (0.05)	0.005 (0.04)	0.01 (0.05)	0.01 (0.04)	−0.003 (0.02)	−0.004 (0.03)	0.02 (0.08)	−0.002 (0.04)
Robbery	−0.01 (0.01)	−0.02† (0.01)	−0.01 (0.01)	−0.01 (0.01)	−0.05 (0.03)	−0.02 (0.01)	−0.03 (0.02)	−0.01 (0.01)
Aggravated assault	−0.15* (0.07)	−0.14† (0.08)	−0.15 (0.14)	−0.17 (0.12)	−0.13 (0.10)	−0.09 (0.08)	−0.19 (0.12)	−0.10 (0.12)
Property crime rates	−0.76† (0.39)	−0.86* (0.39)	−0.67 (0.48)	−0.77 (0.52)	−0.83 (0.55)	−0.82† (0.47)	−0.78 (0.58)	−0.80 (0.56)
Burglary	−0.24* (0.11)	−0.26* (0.12)	−0.23† (0.13)	−0.24† (0.13)	−0.27† (0.16)	−0.23 (0.21)	−0.26 (0.18)	−0.20 (0.14)
Larceny theft	−0.44† (0.25)	−0.49† (0.26)	−0.36 (0.31)	−0.42 (0.34)	−0.41 (0.32)	−0.43† (0.23)	−0.39 (0.24)	−0.44† (0.24)
Motor vehicle theft	−0.07 (0.04)	−0.11* (0.06)	−0.07 (0.05)	−0.10 (0.06)	−0.16 (0.14)	−0.16* (0.08)	−0.14 (0.11)	−0.15† (0.07)
Arson	−0.007 (0.004)	−0.01 (0.008)	−0.003 (0.004)	−0.005 (0.006)	−0.009 (0.009)	−0.005 (0.006)	−0.01 (0.01)	−0.006 (0.006)
TSLS Stage I: Effect of health insurance policy on SUD treatment								
HIFA (0/1)	3.52** (1.06)	3.50** (1.07)	3.99** (1.50)	3.95* (1.62)	2.90* (1.29)	2.77* (1.14)	2.58** (1.26)	2.47* (1.17)
Parity (0/1)		0.48 (0.82)		0.50 (0.98)		2.00† (1.05)		2.03† (1.12)
# observations	22,328	22,328	23,537	23,537	7419	7419	7790	7790
F-statistic[a]	11.1	5.6	8.9	4.6	5.5	3.5	4.3	3.1

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
[a] Stock and Yogo (2005) weak identification test critical values based on maximal TSLS size of a 5% Wald test of $\beta = \beta_0$ (size test): K1 = 1 & L1 = 1: 10%: 16.38; 15%: 8.96; 20%: 6.66; 25%: 5.53; K1 = 1 & L1 = 2: 10%: 19.93; 15%: 11.59; 20%: 8.75; 25%: 7.25.

Appendix Table A19
Estimated effect of SUD treatment rate on crime rates: Adjacent-county means *with* county & years fixed effects, *with* county & state covariates, *without* state-specific trends.

TSLS Estimates	County SUD treatment rate				Mean treatment rate of adjacent counties			
	Balanced panel		Unbalanced panel		Balanced panel		Unbalanced panel	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TSLS Stage II: Effect of SUD treatment on crimes								
Violent crime rates	−0.27* (0.12)	−0.30*** (0.07)	−0.16† (0.08)	−0.17* (0.07)	0.02 (0.32)	0.03 (0.09)	0.02 (0.22)	0.03 (0.16)
Criminal homicide	0.0001 (0.002)	−0.0004 (0.002)	−0.0001 (0.002)	−0.0001 (0.002)	−0.0004 (0.005)	−0.001 (0.003)	−0.0001 (0.004)	−0.0002 (0.003)
Forcible rape	−0.004 (0.06)	−0.005 (0.06)	0.01 (0.08)	0.01 (0.09)	0.001 (0.06)	−0.004 (0.05)	0.003 (0.07)	−0.005 (0.07)
Robbery	−0.04* (0.02)	−0.04* (0.02)	−0.03* (0.01)	−0.04* (0.02)	0.01 (0.06)	0.02 (0.02)	0.01 (0.04)	0.01 (0.02)
Aggravated assault	−0.23* (0.11)	−0.26*** (0.04)	−0.13† (0.07)	−0.14* (0.05)	0.01 (0.06)	0.02 (0.05)	0.01 (0.19)	0.03 (0.08)
Property crime rates	−1.12** (0.43)	−0.92* (0.44)	−1.27* (0.42)	−0.99* (0.33)	0.24 (0.45)	−0.19 (0.50)	0.22 (0.39)	−0.04 (0.30)
Burglary	−0.20 (0.43)	−0.12 (0.20)	−0.24 (0.37)	−0.10 (0.15)	0.48 (0.71)	0.10 (0.17)	0.35 (0.67)	0.08 (0.18)
Larceny theft	−0.75** (0.29)	−0.68† (0.38)	−0.83* (0.36)	−0.73* (0.32)	−0.30 (0.83)	−0.34 (0.82)	−0.23 (0.51)	−0.20 (0.52)
Motor vehicle theft	−0.17 (0.16)	−0.11 (0.09)	−0.20 (0.15)	−0.16 (0.11)	0.05 (0.09)	0.04 (0.07)	0.10 (0.27)	0.09 (0.20)
Arson	−0.01 (0.07)	0.003 (0.01)	−0.003 (0.01)	−0.004 (0.02)	0.001 (0.02)	−0.0003 (0.02)	0.001 (0.01)	0.001 (0.03)
TSLS Stage I: Effect of health insurance policy on SUD treatment								
HIFA (0/1)	2.84* (1.25)	2.83* (1.15)	2.78* (1.26)	2.73* (1.28)	−0.66 (0.55)	−0.65 (0.53)	−0.56 (0.45)	−0.68 (0.59)
Adjacent HIFA (0–1)	0.71 (0.93)	0.59 (1.08)	0.76 (0.86)	0.67 (0.95)	2.88* (1.42)	2.85† (1.51)	2.75† (1.52)	2.69† (1.53)
Parity (0/1)		2.07 (1.47)		1.83 (1.39)		−0.72 (0.95)		−0.81 (0.92)
Adjacent parity (0–1)		−0.81 (1.17)		−0.50 (1.10)		2.16 (1.35)		2.24 (1.34)
# observations	22,328	22,328	23,537	23,537	22,328	22,328	23,537	23,537
F-statistic[a]	12.7	15.5	17.1	19.2	2.0	3.1	1.8	3.6

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.
[a] Stock and Yogo (2005) weak identification test critical values based on maximal TSLS size of a 5% Wald test of $\beta = \beta_0$ (size test): K1 = 1 & L1 = 1: 10%: 16.38; 15%: 8.96; 20%: 6.66; 25%: 5.53; K1 = 1 & L1 = 2: 10%: 19.93; 15%: 11.59; 20%: 8.75; 25%: 7.25.

Appendix Table A20
Estimated effect of SUD treatment rate on crime rates: Additional SUD parity as the only IV.

Crime rates per 1000 residents	County-level balanced panel					
	With county & years fixed effects, *with* county & state covariates			*With* county & years fixed effects, *without* county & state covariates		
	(1)	(2)	(3)	(4)	(5)	(6)
TSLS Stage II: Effect of SUD treatment on crimes						
Violent crime rates	−0.14† (0.08)	−0.24* (0.11)	−0.21* (0.10)	−0.12† (0.08)	−0.23*** (0.07)	−0.19* (0.08)
Criminal homicide	0.003 (0.004)	0.0006 (0.001)	0.001 (0.001)	0.002 (0.003)	0.001 (0.001)	0.001 (0.001)
Forcible rape	−0.02 (0.02)	0.005 (0.05)	−0.02 (0.03)	−0.02 (0.02)	−0.003 (0.04)	−0.02 (0.02)
Robbery	−0.02 (0.01)	−0.04† (0.02)	−0.03† (0.02)	−0.01† (0.005)	−0.04* (0.02)	−0.02* (0.01)
Aggravated assault	−0.11† (0.06)	−0.18* (0.09)	−0.16† (0.09)	−0.10† (0.06)	−0.18** (0.06)	−0.14* (0.06)
Property crime rates	−0.95† (0.55)	−0.67* (0.31)	−0.71† (0.42)	−0.90 (0.56)	−0.67* (0.27)	−0.70* (0.34)
Burglary	−0.12 (0.11)	−0.05 (0.11)	−0.07 (0.10)	−0.06 (0.06)	−0.04 (0.06)	−0.05 (0.06)
Larceny theft	−0.74† (0.42)	−0.52 (0.33)	−0.54† (0.34)	−0.74† (0.42)	−0.54* (0.26)	−0.55* (0.27)
Motor vehicle theft	−0.09 (0.07)	−0.10 (0.06)	−0.11 (0.07)	−0.10 (0.06)	−0.09† (0.05)	−0.10* (0.05)
Arson	−0.005 (0.03)	0.004 (0.003)	−0.001 (0.01)	−0.01 (0.02)	0.002 (0.002)	−0.005 (0.01)
TSLS Stage I: Effect of health insurance policy on SUD treatment						
HIFA (0/1)		2.57** (0.52)	2.50*** (0.47)		3.34*** (0.64)	3.13*** (0.71)
Parity (0/1)	0.93* (0.46)		0.86* (0.45)	1.40** (0.41)		1.27*** (0.32)
# observations	22,328	22,328	22,328	22,328	22,328	22,328
F-statistic[a]	3.9	24.6	14.6	11.5	28.5	23.2

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.
[a] Stock and Yogo (2005) weak identification test critical values based on maximal TSLS size of a 5% Wald test of $\beta = \beta_0$ (size test): K1 = 1 & L1 = 1: 10%: 16.38; 15%: 8.96; 20%: 6.66; 25%: 5.53; K1 = 1 & L1 = 2: 10%: 19.93; 15%: 11.59; 20%: 8.75; 25%: 7.25.

Appendix Table A21
Sargan–Hansen J statistics from the overidentification test of instruments.

Sargan–Hansen J statistics	County-level		CBSA-level	
	Balanced panel	Unbalanced panel	Balanced panel	Unbalanced panel
	(1)	(2)	(3)	(4)
Violent crime	0.46 (0.50)	0.04 (0.84)	0.33 (0.56)	0.04 (0.84)
Criminal homicide	0.37 (0.54)	0.81 (0.37)	0.71 (0.40)	0.03 (0.86)
Forcible rape	1.40 (0.24)	1.42 (0.23)	0.61 (0.44)	0.20 (0.65)
Robbery	0.70 (0.40)	0.50 (0.48)	1.26 (0.26)	1.13 (0.29)
Aggravated assault	0.85 (0.36)	0.08 (0.77)	0.53 (0.47)	0.14 (0.71)
Property crime	0.09 (0.77)	0.09 (0.77)	0.63 (0.43)	0.10 (0.75)
Burglary	0.09 (0.77)	0.12 (0.73)	0.47 (0.49)	0.28 (0.60)
Larceny theft	0.10 (0.76)	0.13 (0.72)	0.47 (0.49)	0.05 (0.83)
Motor vehicle theft	0.04 (0.84)	0.19 (0.66)	0.81 (0.37)	0.02 (0.88)
Arson	0.34 (0.56)	0.06 (0.80)	0.40 (0.53)	0.08 (0.78)
# observations	22,328	23,537	7419	7790

Note: *p*-values in parentheses estimated from the overidentification test of the policy instruments HIFA (0/1) & Parity (0/1).

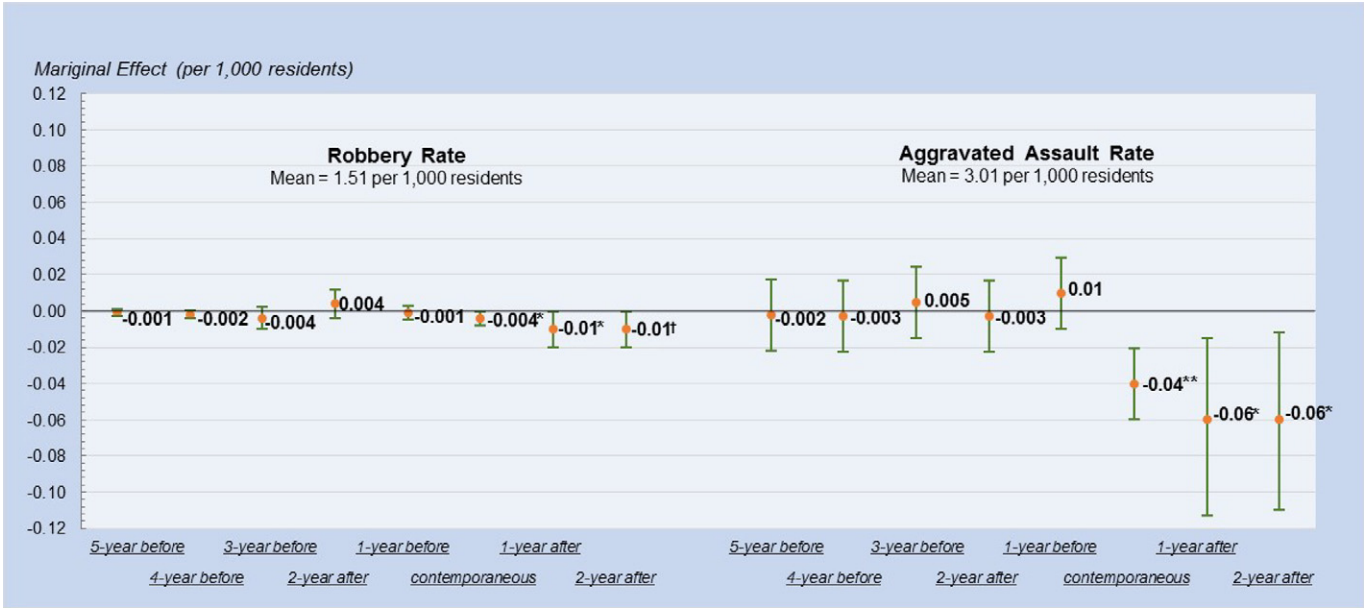
Appendix Table A22
Estimated effect of HIFA-waiver expansions on crime rates: *Without* vs. *with* SUD treatment rate.

Crime rates per 1000 residents	County-level balanced panel			
	With county & years fixed effects, *with* county & state covariates		*With* county & years fixed effects, *without* county & state covariates	
	(1)	(2)	(3)	(4)
Policy effect				
Total crime rates	−0.24** (0.10)	−0.11 (0.07)	−0.31*** (0.09)	−0.17 (0.12)
Violent crime rates	−0.06* (0.03)	−0.02 (0.03)	−0.07*** (0.02)	−0.02 (0.02)
Criminal homicide	0.0001 (0.0003)	0.0001 (0.0003)	0.0001 (0.0003)	0.0001 (0.0003)
Forcible rape	−0.001 (0.01)	−0.001 (0.01)	−0.001 (0.01)	−0.002 (0.01)
Robbery	−0.02* (0.01)	−0.005 (0.01)	−0.01* (0.004)	−0.002 (0.01)
Aggravated assault	−0.04* (0.02)	−0.01 (0.02)	−0.06** (0.02)	−0.02 (0.02)
Property crime rates	−0.17† (0.09)	−0.08 (0.05)	−0.24** (0.02)	−0.14† (0.08)
Burglary	−0.01 (0.01)	−0.005 (0.01)	−0.01 (0.01)	−0.004 (0.01)
Larceny theft	−0.14† (0.07)	−0.06 (0.04)	−0.22* (0.09)	−0.13† (0.07)
Motor vehicle theft	−0.03† (0.02)	−0.01 (0.02)	−0.03 (0.03)	−0.01 (0.02)
Arson	−0.001 (0.01)	−0.001 (0.01)	0.001 (0.01)	−0.001 (0.01)
SUD treatment rate included	No	Yes	No	Yes
# observations	22,328	22,328	22,328	22,328

Standard errors in parentheses are clustered at the state level.
† $p < 0.10$.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.

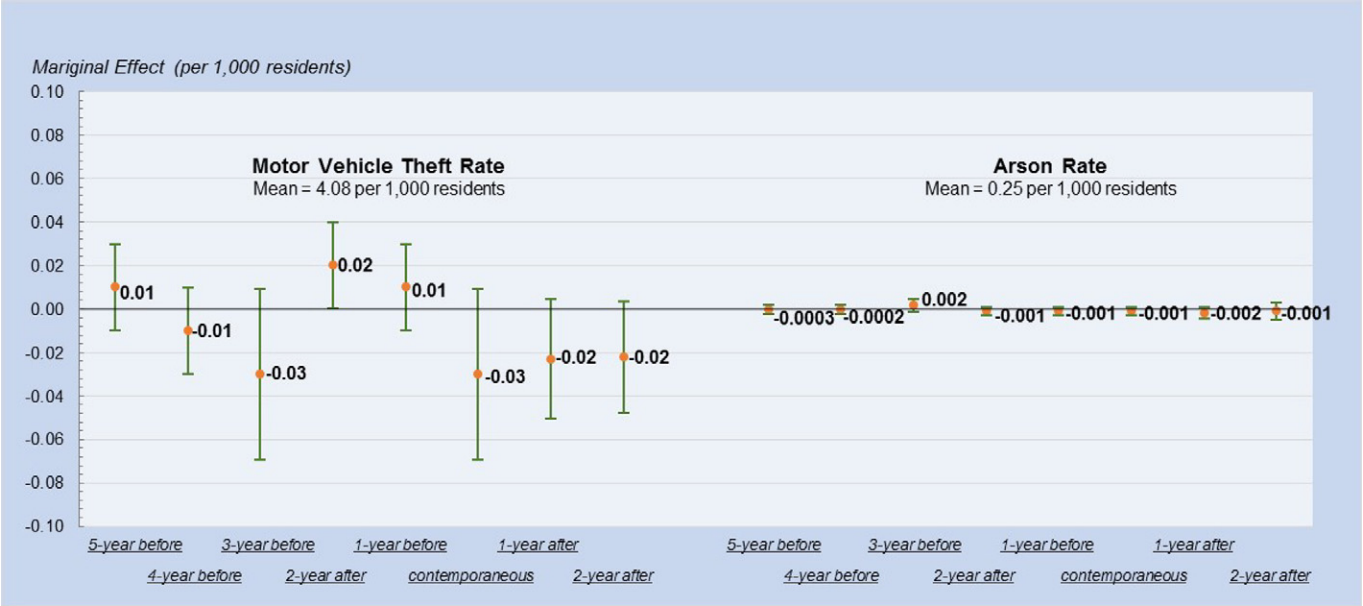
Appendix Fig. A1. Estimated effect of HIFA-waiver expansions on total crime rates per 1000 residents: Extended pre-HIFA window. Note: †$p < 0.10$, *$p < 0.05$, **$p < 0.01$, Standard errors used to calculate 95% CI are clustered at the state level. The reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Fig. A2. Estimated effect of HIFA-waiver expansions on violent crime rates per 1000 residents: Extended pre-HIFA window. Note: Standard errors used to calculate 95% CI are clustered at the state level. The reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

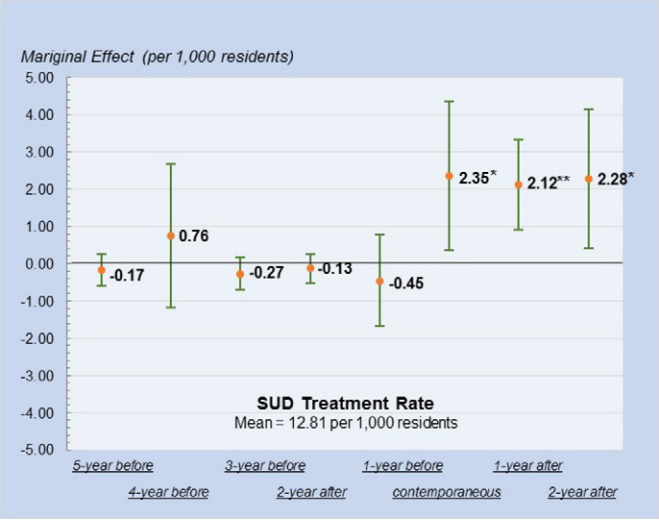


Appendix Fig. A3. Estimated effect of HIFA-waiver expansions on violent crime rates per 1000 residents (cont.): Extended pre-HIFA window. Note: $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$. Standard errors used to calculate 95% CI are clustered at the state level. The reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

Appendix Fig. A4. Estimated effect of HIFA-waiver expansions on property crime rates per 1000 residents: Extended pre-HIFA window. Note: $^{\dagger}p < 0.10$, $^{*}p < 0.05$. Standard errors used to calculate 95% CI are clustered at the state level. The reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.



Appendix Fig. A5. Estimated effect of HIFA-waiver expansions on property crime rates per 1000 residents (cont.): Extended Pre-HIFA window. Note: Standard errors used to calculate 95% CI are clustered at the state level. The reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.



Appendix Fig. A6. Estimated effect of HIFA-waiver expansions on SUD treatment rates per 1000 residents: Extended pre-HIFA window. Note:*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Standard errors used to calculate 95% CI are clustered at the state level. The reference category for the contemporaneous policy indicator $HIFA_{s,t}$ is the pre-HIFA period and the control states that did not implement HIFA-waiver expansions during the study period; the reference category for the lagged and leading policy indicators is the pre-HIFA period previous to 6-year before T_{HIFA} and the control states that did not implement HIFA-waiver expansions during the study period.

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