The Effect of Public Health Insurance on Criminal Recidivism*

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Abstract

Mental health and substance use disorders are highly prevalent among incarcerated individuals. Many prisoners reenter the community without receiving any specialized treatment and return to prison with existing behavioral health problems. We consider a Beckerian law enforcement theory to identify different channels through which access to health care may impact ex-offenders' propensities to recidivate, and empirically estimate the effect of access to public health insurance on criminal recidivism. By exploiting variation in state Medicaid expansion decisions, we find that increased access to health care through Medicaid coverage reduces recidivism among offenders convicted of violent and public order crimes. The decomposition of recidivism rates shows that this reduction is driven by marginal recidivists who, but for Medicaid expansions, would be reconvicted for the type of crime for which they were previously convicted. Analyses of potential mechanisms show an increase in criminal justice referrals to addiction treatment, which may reduce impulsive behavior. Back-of-the-envelope calculations also indicate that there are substantial cost reductions from providing Medicaid coverage to former inmates.

Keywords: Medicaid, Recidivism, Affordable Care Act, Substance Use Disorder
JEL: I13, K42

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I. Introduction

Over two-thirds of former prisoners recidivate within three years of release (Alper, Durose, and Markman, 2018). Most individuals cycling in and out of incarceration have high rates of chronic medical conditions, severe mental health disorders, and substance use issues (Bronson and Berzofsky, 2017). Despite the need for timely and continuous access to care, many ex-offenders do not receive the medical treatment they need while incarcerated or upon release, and return to prison with existing behavioral health issues (Mallik-Kane and Visher, 2008; Wilper et al., 2009). Evidence suggests that access to high quality in-prison health care and treatment programs during incarceration can improve health outcomes and reduce recidivism rates (Hjalmarsson and Lindquist, 2020). Especially, in the absence of such treatment programs or with low admission rates during incarceration, it may be critical to provide health insurance coverage to inmates upon release that includes services for mental health and substance use disorders (SUDs) to curb recidivism rates. In this paper, we investigate the effect of health insurance coverage on access to addiction treatment and the likelihood of returning to prison among former inmates.

In the crime literature, the phrase 'specific deterrence' is often used to describe the impact of punishment on the future behavior of convicts, whereas general deterrence effects refer to the impact of punishment on the general population's incentives to commit crime prior to experiencing punishment. As noted in the literature, there are many reasons to expect these effects to differ from each other, since a person's imprisonment experience,[1] as well as the presence of a criminal record,[2] can cause a person to view the prospect of punishment differently than he did prior to being convicted. Focusing on recidivism is especially important because it allows us to isolate the specific deterrence effects of access to health insurance from its potential general deterrence effects.

Studies focusing on crime rates are incapable of separating out specific deterrence effects, because changes in these rates are driven by a combination of both general and specific deterrence effects. Therefore, absent further analysis, one cannot infer whether a

[1] See, for example, Mueller-Smith (2015); Aizer and Doyle Jr (2015).

[2] See, for example, Rasmusen (1996); Funk (2004); Mungan (2017); Prescott and Starr (2019).

given reduction in crime is caused by recidivists committing fewer crimes or whether the policy is more effective on one-time offenders. This distinction matters for evaluating the strengths of different policies, e.g., one that targets individuals being released from prisons versus another geared towards reducing crime among the general population. Isolating the specific deterrence effects of increased access to health insurance allows us to identify a strong candidate for cost-effective crime reduction policies, namely prison-exit policies that states can adopt to combat recidivism.

In the present study, we provide the first evidence on the causal effect of public health insurance on crime-specific recidivism using individual-level administrative data from the National Corrections Reporting Program (NCRP). Specifically, we exploit a policy change in a majority of states that expanded public coverage to both include services for mental health and SUD and to cover low-income adults in 2014, which is known as the ACA Medicaid expansion.[3] In addition, we develop a simple Beckerian law enforcement model (Becker, 1968) and derive the potential impact of health insurance coverage on recidivism. We explore, both theoretically and empirically, possible channels through which health insurance coverage could affect recidivism. Our empirical analysis suggests that increased access to health insurance reduces recidivism, and our theory suggests that this reduction may be driven by the improved mental health conditions of ex-offenders.[4]

Following the economics of law enforcement literature (Polinsky and Shavell, 2007), we begin our theoretical investigation by assuming that a released ex-offender recidivates if they perceive benefits larger than costs associated with committing crime. We identify three distinct ways through which increased health insurance coverage can affect the way a potential offender compares these costs and benefits. First, increased health insurance coverage can increase the recipient's quality of life outside of prison, and hence increase the opportunity cost of committing crime, since this increased life quality is not enjoyed

[3] 42 U.S. Code § 18022. Essential health benefits requirements.

[4] We note that our theory supplies a rationale for our empirical findings. However, although we are unaware of any other theory that is consistent with our empirical results, it is, of course, impossible to rule out the existence of such a theory. Nevertheless, in section VI we consider an alternative and a priori plausible theory based on the idea that differences in imprisonment sentences between property and other crimes may be driving our results. We explain why our results are unlikely to be explained by this theory.

in prison.[5] Second, increased health insurance coverage can alter a person's monetary incentives to commit crime by reducing the recipients' expected medical costs and thus increase his disposable income for other things. This effect can reduce a person's need or tendency to commit property crimes for purposes of supplementing his (legal) income. Finally, access to more health care can impact the frequency with which one may act impulsively by losing self-control. This last effect can arguably have a negative or positive effect on a person's tendency to commit crime. This is because access to health care may reduce a person's self-control problems through the receipt of needed mental health treatment, and thus reduce his criminal tendencies. On the other hand, one may argue that access to prescription drugs which have the capacity to alter a person's mindset can increase a person's tendency to commit crimes. We call the former two effects, respectively, the relative well-being effect and the monetary incentive effect. We abbreviate the last effect as the 'perception effect', because we formalize it in our theoretical analysis through an inflation parameter which alters a person's perceived non-monetary benefits from crime.[6]

It is, of course, quite difficult to disentangle these three effects, because one does not directly observe what led a former inmate to reoffend, but only whether he reoffended. However, intuition supported by findings from both the psychiatry literature (de Barros and de Pádua Serafim, 2008; Barker et al., 2007; Cherek et al., 1997b,a; Walsh, 1987) as well as observed variations in detection rates of crimes suggests that some of these effects are more prevalent for some crimes than others.[7] In particular, because property crimes are more likely to be planned, and violent and public order crimes are more likely to be committed impulsively, we conjecture that the perception effect is more likely to play a role in affecting the behavior of individuals who have committed the latter types

[5]It is possible for ACA expansions to be accompanied by an increase in the quality of health care accessible by convicts. We allow for this possibility in our theoretical analysis.

[6]We explain, in footnote 29, how our analysis is robust to monetary benefits also potentially being incorrectly perceived by offenders. However, motivated by the existing literature, we focus on the case where only non-monetary acts are affected by self-control problems.

[7]An observation in the law enforcement literature is that violent crimes tend to have higher detection rates than property crimes (see, e.g. Shavell, 1993 n. 25 and accompanying text), and an explanation consistent with this pattern is that property crimes are often planned whereas many violent crimes are committed impulsively (Chamorro et al., 2012).

of crimes.[8] In fact, some studies in the psychiatry literature have specifically noted that SUD coupled with genetic dispositions can contribute to the impulsive commission of crimes (Tiihonen et al., 2015). These increased propensities to commit impulsive crimes are captured by perception effects, and it is plausible to think that they can be mitigated by effective medical treatment, including, most importantly, SUD treatments.

In contrast, the relative well-being effect is likely to have similar impacts across the board, and monetary incentive effects are likely to have greater effects on property crimes. Thus, if increased health coverage has no effect on property crimes, but causes reductions in violent and public order crimes, then increased health insurance coverage most likely mitigates self-control problems. Our empirical investigations using the NCRP data reveal evidence consistent with this theory.

Specifically, based on the general categorizations of crime provided by the NCRP, we investigate the potential effects on 1- and 2-year recidivism among offenders convicted of violent, property, drug, and public order crimes separately. Moreover, we distinguish between all, one-time, and multi-time *reoffenders* to test for heterogeneous effects as these groups could be different in observable and unobservable characteristics, including their underlying mental health and substance use conditions. While there is no direct measurement of inmates' mental illnesses or addiction problems in the NCRP data, we use the number of admissions to prison for recommitting a crime as a proxy. Perhaps more importantly, we are able to decompose the types of crime an offender was previously convicted for and when returning to prison.[9]

Employing a difference-in-differences approach, we find that the ACA Medicaid ex-

[8] Some scholarship in the psychiatry literature cited above suggest that this association is driven by identifiable characteristics, such as the offender's IQ, where low IQ offenders tend to commit impulsive and violent acts which deliver immediate gratification, whereas high IQ offenders tend to commit planned property crimes delivering delayed gratification. Another association noted in the literature is that impulsive offenders tend to have low brain serotonin turnover rates (Virkkunen et al., 1995), and some studies link this to genetic traits (Tiihonen et al., 2015).

[9] The psychiatry literature provides evidence that individuals with impulsivity are more likely to engage in violence with others and that impulsivity is correlated with, inter alia, dependent and schizotypal personality disorders, bipolar disorder, and ADHD (see, e.g., Chamorro et al., 2012). Moreover, axis I disorders assessed by the Diagnostic and Statistical Manual of Mental Disorders-IV (DSM-IV) are associated with low treatment use and can be mitigated by access to health insurance and care (see, e.g., Priester et al., 2016 for a comprehensive literature review on potential barriers to accessing these services).

pansion reduces 1- and 2-year recidivism significantly among multi-time reoffenders with prior violent crime convictions. Specifically, the ACA expansion reduces 1- and 2-year recidivism among multi-time reoffenders who were convicted of violent crimes by about 15 and 16 percent, respectively. We also find weak evidence that the ACA Medicaid expansion reduces 1- and 2-year recidivism among multi-time reoffenders who were previously convicted of public order crimes. However, no similar effects are present when we focus on all reoffenders or one-time reoffenders. Moreover, the estimated effects on recidivism among those with previous property and drug offense convictions are not statistically different from zero. These finding suggest that the policy is effective in reducing multi-time impulsive recidivism, which in turn can generate large economic and social benefits by averting the commission of multiple crimes.

To gain further insights about what might be driving reductions in recidivism, we decompose recidivism rates by first offense and reoffense types. We find negative effects on recidivism for those with the same type of reoffense as their first offense, but only among individuals convicted of violent and public order crimes. We do not find any effects on other combinations of offense types. These findings further suggest that the policy operates by mitigating the repeated commission of impulsive crimes.

Moreover, we note that the perception effects that we described can be realized only if the new recipient of access to health care actually makes use of these resources. Thus, we expect a greater reduction in recidivism among groups of individuals with higher increases in utilization rates of health care. To test this potential mechanism, we explore the impact of the ACA Medicaid expansion on access to SUD treatment.

Exploiting administrative records from the Treatment Episode Data Set (TEDS), we find that the number of admissions to SUD treatment increases for individuals covered by Medicaid in expansion states after 2014.[10] While confirming the findings of existing studies on the relationship between Medicaid expansions and SUD treatment, our paper's novel addition as it relates to TEDS is its findings regarding criminal referrals. We find

[10] In a different setting, Wen, Hockenberry, and Cummings (2017) find an increase in the access to SUD treatment and a decrease in substance use prevalence in (HIFA-waiver) expansion states, which are considered as potential mechanisms for crime reduction.

that the effect of the ACA Medicaid expansion is strongest for individuals referred to treatment from the criminal justice system, particularly for referrals from prison or while on parole or probation. By contrast, we find no significant effect on access to SUD treatment for individuals with private insurance or among self-paying individuals. Quite importantly, when we categorize ex-offenders by age, we find that age groups which experience large reductions in recidivism also experience high increases in utilization rates.[11]

These findings highlight the importance of categorizing the various sources through which welfare reforms might affect individuals' propensities to commit crime. Corman, Dave, and Reichman (2014), for instance, explain how welfare reforms targeting incentives to work may reduce property crimes. Here, we identify a policy, which produces effects that mostly concern violent and public order crimes. Our theoretical framework provides an explanation for how increased access to different kinds of resources may reduce people's tendencies to commit different types of crimes.

Finally, we conduct a partial cost-benefit analysis, which indicates that to reduce the number of 1- and 2-year multi-time recidivism among ex-violent offenders by one, 239 and 182 new enrollment in Medicaid among offenders are needed, respectively. In monetary terms, assuming a year of Medicaid coverage is needed to prevent inmates from reoffending, the total cost of averting one incident of multi-time recidivism within one and two years upon release among those convicted of violent crimes would be $1,329,318 and $1,012,284, respectively. These costs are more than offset by the criminal harm and incarceration cost reductions from lower recidivism among violent offenders, which we calculate as exceeding $1,370,882.

This paper joins a relatively new literature that attempts to understand how access to health insurance impacts criminal outcomes. Existing literature thus far focuses largely on the changes in aggregate crime rates as an outcome (Wen, Hockenberry, and Cummings, 2017; Vogler, 2020; He and Barkowski, 2020). Our study moves beyond these papers in several ways. Most importantly, as discussed earlier, employing recidivism as the outcome

[11] While the results are quite informative, it is worth noting that there are no unique individual identifiers to link criminal referrals in TEDS to NCRP.

allows us to isolate the specific deterrence effects from general deterrence effects. In addition, our ability to employ individual-level administrative data enables us to control for a rich set of individual-level characteristics that state-level or county-level models do not control for and that are likely to act as confounders, especially if the decline in the crime rate is driven by fewer crimes committed by recidivists.[12] We are also the first to provide a theoretical analysis which identifies possible mechanisms that may be driving the empirically observed differential effects of health insurance on different types of crimes.

The remainder of the paper is organized as follows. Section II provides background information on Medicaid eligibility requirements for former inmates and describes the related literature. Section III introduces a theoretical framework to study the relationship between access to health care and recidivism. In section IV, we describe the data and report summary statistics. Section V outlines our empirical strategy. Our main results as well as robustness checks are presented in section VI. SectionVII discusses our back-of-the-envelope calculations and the resulting policy implications, and section VIII concludes.

II. Background

II.A. Medicaid Eligibility Requirements for Ex-Offenders

With the aim of increasing access to health insurance and health care among low-income individuals, including ex-offenders, the ACA Medicaid expansions increased income eligibility limits and eliminated categorical eligibility requirements. This section provides background information on how these changes in Medicaid eligibility requirements affect former inmates.

Historically, Medicaid imposed categorical and income eligibility requirements that limited access to coverage for most ex-offenders after release, leaving this population

[12] These individual-level characteristics include most recent crimes committed, sentence lengths for the most recent crimes, time served in prison, prison admission type, and prison release type, among others.

largely uninsured.[13] Prior to the ACA, the populations eligible for Medicaid were low-income families, children, pregnant women, low-income elders, and low-income disabled individuals. Therefore, former inmates with incomes above the income eligibility threshold and/or without children were not covered through the Medicaid program.[14]

Based on income reported from the Federal Bureau of Prisoners to the Internal Revenue Service between 2009-2013, the mean annual earnings for ex-offenders is $13,889 in the first calendar year after release (Looney and Turner, 2018). This corresponds to around 70% of the federal poverty level (FPL) for a family size of three in 2013. Given the Medicaid income eligibility limits for a three person family in 2013, an inmate with average annual earnings was not eligible for Medicaid coverage in around half of the states in the US, among which more than one-third expanded eligibility limits to 138% FPL in 2014.[15] Thus, it is plausible that many former inmates became eligible for health insurance coverage after the increase in income eligibility limits under the ACA.[16]

Perhaps more importantly, childless adults constitute half of the prison population (Glaze, 2008), a group that tends to fall outside the traditional Medicaid coverage regardless of their income. With the policy reform, (non-disabled and non-elderly) former inmates without children gained access to public health insurance within the increased income eligibility limits in 2014. As a result of the elimination of the categorical eligibility requirements and the increase in income eligibility limits, existing studies find a significant increase in the take-up of Medicaid among justice-involved individuals in

[13]In addition to Medicaid eligibility requirements for former inmates, federal law prohibits the use of federal Medicaid funds for most health care services provided to current inmates, with the exception for care received as an inpatient in an outside medical institution, including a hospital, nursing facility, juvenile psychiatric facility or intermediate care facility (McKee et al., 2015). Despite the payment exclusion, there is no federal law that prohibits (eligible) current inmates from being enrolled in Medicaid during incarceration. If states are exploiting enhanced federal matching to increase state savings after 2014, this may potentially reduce the cost of committing a crime for former inmates in expansion states, and thus, increase recidivism and attenuate the effect towards zero. We also account for this in our theoretical model, as we incorporate well-being within prison.

[14]The income eligibility limits vary by state and time. The average income eligibility limit for families in the United States was 64% of the federal poverty limit in 2013. For a list of income eligibility limits for families, see `https://bit.ly/31236XG`.

[15]These are based on the authors' calculation using information from the Kaiser Family Foundation, Annual Updates on Eligibility Rules, Enrollment and Renewal Procedures, and Cost-Sharing Practices in Medicaid and CHIP (see `https://bit.ly/2JYkb0A`).

[16]In Section IV, we also discuss the potential implications of the ACA Medicaid expansion on previously-eligible inmates who were not enrolled in health insurance coverage before 2014.

the first year of expansion relative to 2009-2013 (Saloner et al., 2016). Our replications of Medicaid take-up using most frequently observed offender demographics in Appendix Figure A1 also confirm these findings.

With more former inmates being eligible for Medicaid under the ACA, facilitating enrollment prior to release or expediting Medicaid enrollment could improve former prisoners' prospects for successful reintegration into the community by reducing barriers to accessing appropriate medical services.[17] Studies that investigate state policies on expediting Medicaid enrollment for offenders find an increase in Medicaid enrollment and mental health service use within 90 days of release (Wenzlow et al., 2011; Cuddeback, Morrissey, and Domino, 2016). Continuity of care is particularly important for former inmates returning to the community, as they often have chronic medical conditions and behavioral health issues that increase the risk of mortality,[18] and poor health conditions increase the risk of recidivism among former inmates (Skeem and Louden, 2006; Mallik-Kane and Visher, 2008).[19]

II.B. Related Literature

One concern that policymakers have regarding ex-offenders is the constraint on labor market opportunities and its effects on recidivism. There is evidence that improving labor market conditions through higher wages and increased availability of jobs in certain sectors reduces the probability of reoffending (Galbiati, Ouss, and Philippe, 2015; Schnepel, 2017; Yang, 2017b; Agan and Makowsky, 2018). A set of papers analyzing how labor market conditions and policies affect the risk of recidivism are summarized in Panel A of Table A1.[20] Despite the intention of improving labor market outcomes among

[17]For example, the Ohio Department of Rehabilitation and Correction partnered with the Ohio Department of Medicaid to facilitate enrollment 90 days prior to release. In Indiana, the Department of Correction assists inmates to complete their Medicaid applications 60 days before release. As of 2016, more than 12,000 newly released inmates had been registered to the Medicaid program in Indiana (IDOC, 2016).

[18]Over 40% of prisoners and inmates in correctional facilities reported having a current chronic medical condition or a mental health disorder in 2011-2012 (Maruschak and Berzofsky, 2015). One leading cause of mortality after release is drug overdose (Binswanger et al., 2007).

[19]See Doleac (2018) for a discussion of the literature on how access to mental health or substance abuse treatment encourages desistance from crime.

[20]Based on the literature, we incorporate variables on labor market conditions that may differ across expansion and non-expansion states and drive the recidivism outcomes in the empirical analyses.

ex-offenders, some policies lead to higher statistical discrimination. A policy that did not yield the intended outcomes was the movement on "ban the box" (BTB) that limited employers' ability to ask questions about applicant's criminal history. Doleac and Hansen (2018) find, consistent with existing theory (see Mungan, 2018), that BTB policies have negative effects on employment for low-skilled black men aged 25-34.

There is a growing literature that focuses on the impact of welfare programs on criminal recidivism (Panel B of Table A1). In 1996, the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) banned ex-offenders with drug felony convictions from receiving welfare benefits and food stamps, where some states opted out of this federal reform. Yang (2017a) and Tuttle (2019) exploit the timing of the food stamp ban to explore its impact on the risk of returning to prison.[21] Yang (2017a) finds that welfare and food stamp eligibility reduces the probability of returning to prison. In support of this evidence, Tuttle (2019) shows that drug traffickers who are affected by the federal ban in Florida are more likely to return to prison. The author also finds that the decrease in financial support under the food stamp ban increases recidivism for financially motivated crimes.

The literature on recidivism has been thriving, while understanding how different welfare programs affect prisoner reentry needs further investigation. The present paper shows how the expansion of public health coverage affects criminal recidivism. We build on the literature that evaluates the causal impact of public policies on prisoner reentry, as well as the literature on health insurance and crime. The emerging literature on the ACA focuses particularly on health and labor market implications of access to fully or partially subsidized insurance (Barbaresco, Courtemanche, and Qi, 2015; Simon, Soni, and Cawley, 2017; Kofoed and Frasier, 2019; Aslim, 2019a). We add to that literature by ascertaining the changes in criminal behavior resulting from the expansion of public health coverage.

A handful of studies have addressed the link between public health insurance and crime (Panel C of Table A1) and have generally found beneficial effects. Exploiting the

[21] Yang (2017a) constructs an eligibility measure for food stamps that also takes into account the states that opt out of the ban.

Medicaid expansions through Health Insurance Flexibility and Accountability (HIFA) waivers, Wen, Hockenberry, and Cummings (2017) find a reduction in county-level crime rates, particularly in robbery, aggravated assault, and larceny theft.[22] Furthermore, they find an increase in access to SUD treatment and a decrease in substance use prevalence in expansion states, which are considered as potential mechanisms for crime reduction. Bondurant, Lindo, and Swensen (2018) also show that increasing access to substance abuse treatment reduces local crime. They find these effects to be strongest among relatively serious crimes, including homicides, aggravated assaults, robbery, and motor vehicle theft. In the context of the ACA's Medicaid expansion, Vogler (2020) and He and Barkowski (2020) both provide evidence of Medicaid-induced reduction in violent crimes. More importantly, both studies find limited effects of Medicaid expansions on property crimes. Using a state-level sample as well as a sample of contiguous-border counties, He and Barkowski (2020) find a negative but statistically insignificant effect on aggregated property crimes.[23]

III. Theoretical Framework

We consider a Beckerian law enforcement model wherein a former prisoner recidivates only if doing so increases his expected utility. We consider four components which affect the utility of an ex-offender, and to simplify the analysis we assume that these components are additive. Two of these components capture the health care related and health care independent effects of being convicted on a person's well-being, whereas the remaining two components capture the (perceived) non-monetary and monetary benefits from committing crime. Throughout our analysis we refer to the impact of access to health care (denoted a), which is a general term we use to capture the impact of Medicaid expansion policies on the availability of public health insurance for non-convicts as well as

[22] The HIFA initiative expanded coverage to low-income adults with incomes below 200% of the federal poverty level (FPL). The expansion states exploited in the analysis include Illinois, Maine, New Mexico, and Massachusetts.

[23] The event-study estimates at the state level, however, depict a slight decline in property crimes in the first year of expansion.

the impacts of related policy changes on convicts.[24]

To describe the first two components, we note that imprisonment naturally affects a person's well-being. Moreover, part of this impact may depend on the extent to which convicts as well as non-convicts have access to health care.[25] We denote the health care unrelated reductions in a person's well-being due to imprisonment as w. On the other hand, the positive impact of access to health care (denoted a) on a non-convict's utility is $h(a)$ whereas it is $\pi(a)h(a)$ for a convict. The term $\pi(a)$ can be interpreted as either the likelihood of getting similar access to health care as a non-convict, the relative quality of health care receivable by a convict, or a combination of these two considerations reflecting the expected health care receivable by a convict relative to a non-convict. We allow π to change in response to increased health care access to incorporate the possibility that expansion programs may alter when and how inmates receive health care. We note that the difference between the well-being of non-convicts and convicts equals $w+(1-\pi(a))h(a)$ and

$$\gamma(a) \equiv (1 - \pi(a))h(a) \tag{1}$$

is the health care dependent portion of this difference. Thus, γ' captures the impact of changes in health care policies on the relative well-being of non-convicts versus convicts.

Next, we note that a potential offender's monetary utility is given by $u(.)$ with $u' > 0 \geq u''$, and we normalize a person's initial wealth and the corresponding monetary utility associated with that wealth to 0. We assume that access to health care can increase a person's disposable income by an amount of $y(a)$ with $y, y' \geq 0$ when he is not convicted and an amount of $z(a)$ when he is convicted. To focus on the more realistic and intuitive case where access to health care has a lesser effect on convicts' versus non-convicts' monetary utilities, we restrict attention to cases where $y'u'(y(a) + b) \geq z'u'(z(a))$ where b denotes the benefit from crime as explained in further detail,

[24]As we noted, Hjalmarsson and Lindquist (2020) find that access to in-prison treatment programs improve health outcomes and reduce recidivism rates in Sweden. It is unclear whether a similar effect arises from the expansion of Medicaid in the United States, especially given that public health insurance does not cover health care services provided in prison. We discuss this in detail in footnote 13. Nonetheless, our model allows for this possibility but does not require it.

[25]We use the terms convict and non-convict, instead of health care receivable in prison versus out of prison, to reflect the fact that convicts are sometimes referred to out of prison treatment facilities.

below. This condition is trivially met when $z(a) \equiv 0$. Impacts on disposable income may occur due to possible reductions in health care and prescription drug expenditures as well as improved job prospects. Thus, due to the former consideration, increases in a person's disposable income caused by changes in a can potentially reduce the tendency of individuals with SUDs to commit property crimes to finance their drug habits. This possibility is formalized by noting that the successful commission of a property crime increases the wealth of a person by an amount of mb, where b denotes benefits and $m \in [0, 1]$ is a parameter that measures the degree to which the benefits from the crime are monetary versus non-monetary. Thus, a person's monetary utility is $u(y(a))$, $u(y(a)+mb)$ and $u(z(a))$, if he does not commit crime, commits crime but avoids conviction, and is convicted, respectively. Following the observations we make in the introduction, we also assume that potential offenders' expected monetary benefits from crime are unaffected by their mental state, but their non-monetary benefits may depend on the degree to which they exhibit impulsive behavior, as we explain next.[26]

The commission of a crime can also provide a person with non-monetary benefits, which would be evaluated as $(1 - m)b$, if the person were not acting impulsively. But, a person's perception of this benefit may be inflated to $\delta(a)(1 - m)b$,[27] which may be affected by the degree of access to health care. Our assumption is motivated by observations made in the literature that mental health problems and SUDs can contribute to impulsivity problems, which, in turn, can be mitigated through health care.[28] The case where a person's inflated perception of benefits are reduced as a result of health care would correspond to one where $\delta' < 0$. On the other hand, $\delta' > 0$ would be possible when, for instance, more access to prescription drugs through public health care increases

[26]We emphasize that this assumption is mainly simplifying. Our analysis extends to the case where potential offenders misperceive monetary benefits, but these misperceptions are impacted no more than their perceived non-monetary benefits are impacted by access to health care. We provide a more specific sufficient condition in footnote 29, below, after we introduce the necessary notation in the next paragraphs.

[27]We follow an approach similar to Cooter (1991), who formalizes the idea that one's lack of will power or lapse in judgement can be conceived of as unusual inflation of perceived benefits receivable in the present compared to costs receivable in the future. The literature on present bias is motivated by similar ideas and has been applied to study criminal behavior (e.g., McAdams, 2011).

[28]See, e.g., Kozak et al. (2019) for the association between impulsivity and SUDs and Chamorro et al. (2012) for the association between impulsivity and mental health problems.

a person's criminal tendencies.

To keep the analysis focused, we follow the law enforcement literature by assuming that a given individual faces an opportunity to commit a single crime. We later make cross-crime comparisons by focusing on the variable m, which relates to the nature of the crime being analyzed. Given these assumptions, a potential offender's expected utility from not committing crime is

$$w + h(a) + u(y(a)) \tag{2}$$

On the other hand, denoting by p the probability of detection upon committing crime, we can express the expected utility from crime as follows

$$(1-p)(w+h(a)+u(y(a)+mb)+(1-m)\delta(a)b)+p(\pi(a)h(a)+u(z(a))+(1-m)\delta(a)b) \tag{3}$$

where the term multiplied by $(1 - p)$ corresponds to the utility of the person when he commits crime and avoids punishment, and the term multiplied by p corresponds to the utility of the person when he is caught after committing crime. Thus, a person commits crime if,

$$(1-p)u(y(a)+mb) + pu(z(a)) - u(y(a)) + (1-m)\delta(a)b > p(w+\gamma(a)) \tag{4}$$

where $\gamma(a)$ is as defined in (1).

As in Becker (1968) and subsequent law enforcement models (see, e.g. Polinsky and Shavell, 2007), we assume that individuals differ from each other in their propensities to commit crime, and, thus, policy changes affect the crime rate by changing the incentives of marginal offenders. To capture these heterogeneities in the simplest way, we assume w differs from person to person, and $f(w)$ captures the density function of w with support $[0, \infty)$ and corresponding cumulative distribution function $F(w)$. To calculate the measure of individuals who commit crime it is useful to start by noting the critical value of w which makes a person indifferent between committing and not committing crime by re-writing (4) as

$$w^*(\gamma(a), y(a), \delta(a), m) \equiv \tag{5}$$
$$\frac{(1-p)u(y(a)+mb) + pu(z(a)) - u(y(a)) + (1-m)\delta(a)b}{p} - \gamma(a) > w$$

Thus, the measure of individuals who commit crime is given by $F(w^*)$.

We may now describe the various sources through which increased access to health care may have an impact on the crime rate by differentiating $F(w^*(\gamma(a), y(a), \delta(a), m))$ with respect to a, as follows:

$$\frac{dF(w^*)}{da} = f(w^*(a)) \left(\underbrace{\frac{\partial w^*}{\partial \gamma}\gamma'}_{\substack{\textit{relative} \\ \textit{well-being}}} + \underbrace{\frac{\partial w^*}{\partial y}y'}_{\substack{\textit{monetary} \\ \textit{incentives}}} + \underbrace{\frac{\partial w^*}{\partial \delta}\delta'}_{\substack{\textit{perceived} \\ \textit{non-monetary} \\ \textit{benefits}}} \right) \tag{6}$$

Effects due to changes in:

As (6) illustrates, impacts on crime due to changes in potential offenders' relative well-being, monetary incentives, and perceived non-monetary benefits, which we have described in the introduction, can be conveniently and discretely described in our theoretical framework. Next, we investigate each effect in further detail to note some of their properties which we have previously touched on. As noted in the introduction, we often refer to the third effect simply as the 'perception effect' to abbreviate descriptions. Evaluating these effects and writing them out explicitly we have that:

Relative well-being: $\frac{\partial w^*}{\partial \gamma}\gamma' = -\gamma'$

Monetary incentives: $\frac{\partial w^*}{\partial y}y' + \frac{\partial w^*}{\partial z}z' = -\left[\frac{u'(y)-u'(y+mb)}{p} + u'(y+mb)\right]y' + u'(z)z'$

Perceived non-monetary benefits: $\frac{\partial w^*}{\partial \delta}\delta' = (1-m)b\frac{\delta'}{p}$ (7)

A quick investigation of these effects reveals some important insights. First, diminishing utility from money contributes to monetary incentive effects through the first term in the squared brackets in (7) and this effect is proportional to $1/p$. However, monetary incentive effects may exist even when potential offenders have constant marginal utility from monetary outcomes. This is because non-convicts and convicts may experience different increases in their disposable incomes, and this difference may depend on access to health care. Second, the perception effect is similarly inversely related to the probability of detection whereas the relative well-being effect is not directly related to it. Therefore, the perception effect is magnified in comparison to the relative well-being effect due to the probabilistic nature of enforcement. Thus, even when access to health care increases the relative well-being of non-convicts and leads to monetary incentive effects, the overall impact of these increases can be small compared to the impact of access to health care through its perception effect. This result is more likely to be observed when marginal offenders possess close to linear utility from monetary outcomes. Third, the relative well-being effect is ambiguous, even when increased access to health care unambiguously increases the well-being of recipients, because the well-being of convicts may be increased by more than the well-being of non-convicts. This can be noted by observing that $\gamma' < 0$ if $h'(1-\pi) < \pi' h$, which is possible even when more access leads to an improvement in all individuals' well-being, if the well-being of convicts is more responsive to increased health-care access than the well-being of non-convicts.

As we noted earlier, it is quite difficult to disentangle these three effects from each other. However, as (7) illustrates, when the criminal benefit is exclusively monetary, it follows that the perception effect is negligible. Using this observation, we are able to formulate our prediction with respect to the effect of increased access to health care on crime rates, as follows.

Proposition 1. *(i) For $m = 1$, increased access to health care leads to a lower crime rate if either (a) it enhances the relative well-being of non-convicts (i.e. $\gamma' > 0$), or (b) it enhances the well-being of convicts no less than the well-being of non-convicts (i.e. $\gamma' \leq 0$), but affects monetary incentives enough to off-set the relative well-being effect. (ii) For $m = 0$, increased access to health care can lead to a lower crime rate if either* *(a) the combination of the relative well-being effect and the monetary incentive effect is negative (i.e. $\frac{\partial w^*}{\partial \gamma}\gamma' + \frac{\partial w^*}{\partial y}y' < 0$), or (b) it reduces the perceived nonmonetary benefits from crime (i.e. $\delta' < 0$). (iii) The ratio between the relative well-being effect and the perception effect converges to zero as the probability of detection approaches zero.*

Proof. Follows immediately from (7). ■

An implication of proposition 1, which is most relevant for our empirical findings, can be formulated as follows.

Corollary 1. *If increased access to health care has no impact on the crime rate when $m = 1$, but, leads to a reduction in crimes for which $m = 0$, this implies that $\delta' < 0$ for those crimes.*

Proof. No change in the crime rate when $m = 1$ implies via (7) that $-\gamma'(a) - \left[\frac{u'(y(a))-u'(y(a)+b)}{p} + u'(y(a)+b)\right]y'(a) + u'(z)z' = 0$. Thus, $\frac{dw^*(\gamma(a),y(a),\delta(a),0)}{da} = \frac{1-p}{p}(u'(y(a)) - u'(y(a)+b))y'(a) + b\frac{\delta'}{p}$. Therefore, $\frac{dw^*(\gamma(a),y(a),\delta(a),0)}{da} < 0$ implies that $\delta' < 0$. ■

Corollary 1 simply states that we can deduce from the lack of an impact of increased access to health care on criminal acts which confer only monetary benefits that the combination of relative well-being and monetary incentive effects for non-monetary crimes must be positive. This implies, via part (ii) of proposition 1 that any reductions in the commission of crimes for which the benefits are exclusively non-monetary must therefore be due to reductions in perceived non-monetary benefits.[29]

We conclude our brief theoretical investigation by noting a couple of important distinctions between the stylized model we have analyzed and the real life interactions that our empirical analysis focuses on. We do not suggest that property crimes represent $m = 1$ crimes and that violent and public order crimes represent $m = 0$ crimes. Nevertheless,

[29] We note that this result extends to the case where potential offenders perceive monetary benefits from crime as $k(a)b$ instead of b, and this misperception is, loosely speaking, no more responsive to health care access than similar misperceptions regarding non-monetary benefits. Specifically, a very conservative sufficient condition for corollary 1 to carry over to this case is that k' and δ' have the same sign with $|k'(a)| < |\delta'(a)|$ and $u' \leq 1$. A less restrictive, but also less intuitive condition that replaces the latter is that $|k'(a)|(1-p)u'(y(a)+k(a)b) < |\delta'(a)|$.

assuming m is larger for property crimes, evidence suggesting that increased access to health care lowers the commission of violent or public order offenses suggests that these effects are likely largely driven by perception effects. Moreover, intuition suggests that perception effects are likely greater when increased health care is not only present, but effective. Among young people, whose receipt of health care –as we show in our empirical analysis– is less responsive to health care expansion, reductions in crime rates are also less responsive than they are among older people. Similarly, one would expect perception effects due to increased health care to be larger among people who suffer more serious perception or self-control issues. Multiple reoffenders whose previous offenses are of an impulsive nature are more likely to fall into this category. As we discuss below, our empirical findings are consistent with these intuitions.

IV. Data

IV.A. Recidivism Data

Our empirical analyses are based on data from the National Corrections Reporting Program (NCRP). The NCRP data are constructed using nationally representative administrative data on prison admissions and releases provided by the Bureau of Justice Statistics (BJS). Because the NCRP only includes offender data sentenced to prisons, it does not include data on individuals in jails. Those in jails are typically serving shorter sentences than those serving prison sentences. In the present paper, we employ the selected version of the NCRP data (henceforth "selected NCRP"), which contain information on prisoners' age when they were released, gender, race, ethnicity, education, the year and type of admission and release, crime category, sentence length, and time served. The restricted version of the NCRP contains a slightly disaggregated version of a few categorical variables and the last known address of an inmate prior to incarceration. We prefer the selected NCRP mainly because it contains one more year of data, allowing us to analyze the effect on both 1- and 2-year recidivism with higher precision. Nonetheless, we employ the restricted data from the NCRP as a robustness check and provide complete details and background in the Appendix.

The NCRP data have some limitations despite being commonly used to explore recidivism rates. First, these data are reported voluntarily by each state and it is not available for a few states in the working sample of our paper (Table 1). Out of all 50 states and the District of Columbia, Arkansas, Connecticut, Hawaii, Idaho, Vermont, and Virginia did not release information on prison spells to the NCRP. These six states, however, only constitute 5.8% of the U.S. population and 5.5% of the national prisoner population based on our calculations using data from the 2019 American Community Survey and the Sentencing Project, respectively.[30] To mitigate potential reporting issues, Abt Associates, who serves as a data collection agent for the BJS, updates the NCRP retrospectively if a state fails to report data for one year but then provides it in the future. As with most voluntarily provided information coming from a variety of parties, this may not necessarily eliminate administrative or coding differences across states while reporting these individual-level data. However, described in detail later, we rule out the possibility of our results being driven by a specific state or a group of states.

Second, the selected NCRP provides information on the state of conviction but does not report the state of residence upon release. According to our calculation using the restricted NCRP, the state of last known residence prior to incarceration and the state of conviction matches in 93% of the present observations. Perhaps more importantly, most of these inmates are released into the state of their "most recent legal residence prior to incarceration" (Agan and Makowsky, 2018). As a result, we assume that the state of conviction is the state of former inmates' residence after incarceration. Finally, it is not possible to track offenders that cross state lines. The inmates would acquire a new inmate ID and appear for the first time in the destination state. This would underestimate the rate of recidivism in the data because a one-time offender serving a prison term in one state may actually come from a state where they had served a prison sentence (Rhodes et al., 2019).[31]

[30]The state-level criminal justice data from the Sentencing Project can be obtained from here: https://bit.ly/2MljYYq.

[31]However, this attenuation in the rate of recidivism should not affect our estimates because we do not find any evidence that this attenuation is more likely to happen in expansion states in the post period.

IV.B. Sample Construction

The working sample covers the time period between 2010 and 2016. In the main analyses, we make some restrictions on the data. First, we drop states whose data are missing for one or more years in the sample time period.[32] Second, states that implemented the ACA option or had a comprehensive program similar to the ACA prior to 2014, including Delaware, District of Columbia, Massachusetts, Minnesota, and New York, are dropped. About 30% of the ACA policy impact on Medicaid enrollment during 2014 and 2015 came from already-eligible adults, which is referred to as the "woodwork effect" (Frean, Gruber, and Sommers, 2017). This implies that already-eligible adults begin to take up Medicaid following the 2014 reform rather than the earlier coverage expansions in their states, mainly due to increased post-2014 outreach and navigation (Leung and Mas, 2018).[33] Given the evidence of large woodwork effects and the inability to observe prior coverage, we exclude early expansion states that may confound the interpretation of our recidivism estimates.[34]

We exclude late expansion states due to the lack of data for the "post" period in constructing recidivism rates.[35] Following the recidivism literature, we exclude California due to its enactment of the 2011 Public Safety Realignment Act (PSRA), which was a significant policy change in the criminal justice realm (Agan and Makowsky, 2018).[36]

[32] These states include Alaska, Kansas, Louisiana, Maine, Maryland, North Dakota, Oregon, and South Dakota.

[33] This can create two potential issues. First, when we calculate 1- and 2- year recidivism, we would be underestimating the effects in a state that expanded early (e.g., the 2010 expansion of Minnesota or the District of Columbia) if a large share of eligible individuals take up Medicaid after 2014. Second, in a staggered difference-in-differences setup, these early expansion states would be used as a control for the 2014 expansion states or later expanders. A potential jump or treatment heterogeneity in early expansion when the treatment status turns on for the 2014 expansion states or later expanders can bias the estimates.

[34] A potential implication of woodwork effect is that the recidivism rates are decreasing in all states due the ACA's Medicaid expansion, but at a larger rate in expansion states. An alternative approach is to employ simulated eligibility (for Medicaid) as the independent variable (Burns and Dague, 2017). Nonetheless, dropping early expansion states is always a preferred specification due to the potential issues discussed above, particularly in footnote 33.

[35] More than half of the late expansions happened in 2015, which limits our ability to construct 2-year recidivism as it would require data from 2017.

[36] The PSRA allows convicts to be redistributed between jails and prisons, aiming to reduce prison overcrowding. Those redistributed inmates are usually recorded as new admissions into the prisons. Consequently, it is difficult to construct an accurate measure of recidivism using data from California. In addition to the PSRA, California had limited prior expansion of Medicaid.

The final working sample contains 14 non-expansion states and 13 expansion states for the benchmark analysis. Later, following Courtemanche et al. (2017), we provide a wide range of robustness checks in Figure A2 regarding our sample selection. We show that our estimates are not sensitive to different classifications of treatment and control groups.

To avoid interaction with the dependent coverage mandate, we restrict the sample to inmates aged 26-64.[37] The age of inmates is coded into categories in the selected NCRP data, and the most appropriate age restriction we can employ for inmates include those who were released between the ages of 25 and 54. We drop inmates who have not been released from prison after their first conviction. We also exclude the observation if an inmate had been convicted once and was released due to death.

IV.C. Descriptive Statistics

In Table 2, we report the summary statistics for 1- and 2-year recidivism rates by crime type for all and multi-time reoffenders, separately. All reoffenders are categorized as those who reoffend at least once (i.e., number of reoffenses ≥ 1), whereas multi-time reoffenders are those with at least two reoffenses (i.e., number of reoffenses ≥ 2). We note that the analyses of the two cases employ the same samples, and thus involve an equal sample size. Specifically, in the analysis of all reoffenses, the dependent variable is an indicator of recidivism that takes a value of 1 if an offender is reconvicted once or more times within the specified time interval (one or two years) and 0 otherwise. In the multi-time offender analysis, the dependent variable takes on a value of 1 if the offender is reconvicted multiple times and his first reconviction falls within the specified time interval, and 0 otherwise.

Moreover, we categorize recidivism by the type of crime for which an offender was initially convicted, i.e., his first offense. We later decompose recidivism rates by first

[37]Note that individuals below age 26 could stay on dependents' coverage, and those above age 64 are eligible for Medicare. The dependent coverage mandate is contingent on parents having private health insurance plans, a policy that is likely to affect individuals whose parents are of relatively high socioeconomic status. Former inmates are less likely to fall into this category. We find, however, that the benchmark findings are unchanged even if young adults are included in the sample. Despite being eligible for dependents' coverage, we also find later in the paper that the number of admissions to SUD treatment increases among individuals aged 18-24 who are referred by the criminal justice system and have Medicaid as the primary payment method.

offense and reoffense types for a detailed analysis of potential heterogeneities. Violent crimes include murder, manslaughter, forcible or statutory rape, armed robbery, and aggravated assault, among others. Property crimes range from burglary and auto theft to trespass against property or possession of burglary tools. Drug crimes include both drug trafficking and drug possession or use, whereas public order crimes include riots, driving under the influence or driving while intoxicated, vice offenses (gambling, prostitution, etc.), and others.

A few observations are notable from Table 2. First, for all four types of crime, the means of recidivism rates are at least thrice as large in the all reoffenders sample as those in the multi-reoffender sample. This implies that there is a larger share of one-time reoffenders in the sample. Second, in comparison to 1-year recidivism, the means of 2-year recidivism are considerably larger due to the longer period within which an ex-offender could reoffend. Third, the number of observations are fairly large for all samples, providing the foundation for precise estimations.

Table 3 summarizes the covariates for the subsample of both 1- and 2-year recidivism among violent offenders.[38] About 70% of the inmates are aged 25-44 at release. Among all the inmates, around 9% are female. In terms of racial and ethnic composition, about 38% of the inmates are white, 35% are black, and 18% are Hispanic. More than 60% of the inmates hold a high school or lower level of education. Although information on income is not available for the inmates, it is plausible that a great proportion of inmates may have limited sources of income when they are released, in part because of their relatively low educational attainment.

In terms of prison admission characteristics, approximately 58% of violent reoffenders receive a sentence more than 5 years and about 25% of them serve more than 5 years in prison. There is evidence that time served in prison is correlated with poor mental health status (see, e.g., James and Glaze, 2006). Additionally, we observe that inmates are more likely to be released conditionally and admitted by new court commitment

[38]We focus on recidivism among violent offenders as this is the category where we find the most salient effect. Therefore, the offender characteristics for this group are of particular interest. We report the summary statistics for other recidivism samples by offense type in the Appendix.

rather than a parole return or revocation. Finally, the macroeconomic and legislative conditions encountered in an inmates' state of conviction for 1- and 2-year recidivism, on average, are similar.

V. Empirical Strategy

V.A. Empirical Model

To investigate the impact of the ACA Medicaid expansion on recidivism, we implement a difference-in-differences approach estimating the following equation:

$$Recidivism_{ist} = \beta_0 + \zeta_s + \eta_t + \beta_1 Expansion * Post_{ist} + \mathbf{X}_{ist}\mathbf{\Gamma}_1 + \mathbf{\Omega}_{st}\mathbf{\Gamma}_2 + \epsilon_{ist}, \quad (8)$$

where the dependent variable is an indicator for recidivism. It takes a value of one if an individual inmate i returns to prison within a specific time span (1 year or 2 years) after being released from his first incarceration in state s in year t. We group recidivism rates using four main categories of first offense types: violent, property, drug, and public order crimes.[39] We estimate equation (8) for each of these categories for all reoffenders, one-time reoffenders, and multi-time reoffenders within each category. The former two groups of reoffenders include those with at least one reoffense and exactly one reoffense, respectively, and the latter includes those who reoffend at least twice. This allows us to detect possible heterogeneous effects across these groups of inmates since they can be different in terms of their criminal propensities as well as the types of crimes they commit. State fixed effects and release-year fixed effects are ζ_s and η_t, respectively. $Expansion * Post_{ist}$ signifies the treatment status of an individual inmate convicted in a specific state and released in a specific year.[40] Specifically, $Expansion * Post_{ist}$ is equal to one for inmates released in an expansion state during the post-expansion period and thus was exposed to the "treatment"; otherwise 0. Therefore, the main coefficient of interest

[39] See section IV for detailed definition of these crimes.

[40] As discussed in the data section, there is a large overlap between the conviction state and the last known residence of offenders. Using the NCRP, Agan and Makowsky (2018) also note that 95% of offenders lived in the state of conviction prior to incarceration.

is β_1, which measures the effect of the ACA expansion on recidivism.

$\mathbf{X}_{ist}$ is a vector of individual-level covariates, including the age when the inmate was released, gender, race/ethnicity, and the educational level of the inmate. $\mathbf{X}_{ist}$ also contains a set of variables that gauge the characteristics of the most recent crime(s) committed by the inmate, including the length of sentence for the most recent crime(s), time served, prison admission type (court commitment, parole violation, other), and prison release type (conditional release, unconditional release, other).[41] In addition, we control for a number of time-varying variables at the state level to mitigate the concern of macroeconomic confounders, notified as $\mathbf{\Omega}_{st}$. Specifically, $\mathbf{\Omega}_{st}$ includes the minimum wage, the housing price index, the poverty rate, and the unemployment rate.[42] In alternative specifications, we include more time-varying state characteristics related to the criminal justice system as well as state-specific time trends. In our analysis, we cluster standard errors at the state level. We also provide p-values obtained from the wild cluster bootstrap iterations to test for the sensitivity of our standard errors to the number of clusters, as suggested by Cameron, Gelbach, and Miller (2008).

V.B. Challenges to Identification

An important identifying assumption for the difference-in-differences approach is that the treatment and control groups share the same time trend with respect to the outcomes of interest should there be no treatment. Therefore, we implement a series of event studies to examine the pre-treatment trend in recidivism in the expansion states versus that in the non-expansion states. The results are presented in Figure 1. As shown in the

[41] If any of the covariates listed above contains missing values, we construct an indicator to signify the missing values, and we control for these indicators as well.

[42] Motivated by the existing literature discussed in Section II.B, we control for minimum wages in the empirical model, as it has been shown to be predictive of recidivism and health insurance enrollment. To account for economic conditions, we also control for the housing price index and the poverty rate. There are, however, arguments both in favor of and against the inclusion of the unemployment rate. Agan and Makowsky (2018), for example, find that the effect of minimum wage changes on recidivism is robust to the inclusion of the state unemployment rate. In our analysis, we also control for the state unemployment rate, though the estimates are not sensitive to the exclusion of the state unemployment rate. The unemployment data are collected from the Bureau of Labor Statistics. The state housing price indices are gathered from the Federal Housing Finance Agency. The minimum wage data are from the Washington Center for Equitable Growth (Vaghul and Zipperer, 2016) The poverty rates are obtained from the University of Kentucky Center for Poverty Research (UKCPR) National Welfare Data (available at https://bit.ly/2HeVav1).

figure, we find parallel trends before the ACA expansion in the expansion (treatment group) and non-expansion (control group) states for both 1- and 2-year recidivism among violent offenders. Hence, the results support the validity of our identification strategy. Moreover, the figures suggest that the ACA expansion has a statistically insignificant effect on recidivism among all reoffenders whose first offenses were violent within one and two years of release, whereas it leads to a substantial reduction in the same outcomes for multi-time reoffenders. Similar parallel pre-trends are found for other types of crimes as shown in Figures 2 and 3. These event studies further suggest that there is some evidence of a reduction in the likelihood of recidivism among multi-time reoffenders whose first offenses were public order violations.

In equation (8), we control for state fixed effects to account for potential unobserved differences across states and release-year fixed effects to capture changes over time that may confound the results. Moreover, in our preferred specification, we control for state-specific time trends to capture smooth changes in the outcomes for each state over time. After controlling for state-specific time trends, our model should capture the variation in recidivism caused by the sharp change in Medicaid coverage. Because our sample only covers a short time period before and after the ACA expansion, this procedure is potentially "over-controlling" for the unobserved time-varying effects. Yet, as shown later in the paper, we find substantial effects of the ACA Medicaid expansions on recidivism among violent offenders after employing this conservative approach.

We control for a number of variables to gauge the economic condition at the state level to further mitigate the concern of state-level confounders. One may still be concerned, however, that state-specific shocks, particularly those related to the legislative system and criminal behavior, may confound the recidivism estimates. Because we have already controlled for state and year fixed effects to capture the variation across states and years, such shocks are a threat to the estimation if observed for certain states at specific time periods. To address this concern, we control for a set of time-varying variables to account for the potential variation in the legislative and justice system in each state over time, as a robustness check. Specifically, we control for the share of Democrats in the U.S.

Congress, per capita total justice expenditure, and an indicator for states' legalization of recreational marijuana consumption. We present these alternative specifications after introducing our benchmark findings in the next section.

VI. Empirical Results

VI.A. Main Results

The baseline results obtained from estimating equation (8) are summarized in Tables 4 and 5 for 1- and 2-year recidivism, respectively. First, we present the estimates showing the effects of the ACA expansion on recidivism for all reoffenders and multi-time reoffenders, separately. Second, we show the estimates for one-time reoffenders, which are included in the Appendix. As discussed above in detail, for each specification, recidivism rates are categorized by the type of crime an offender was previously convicted for. Additionally, for each offense type, we report estimates from three different specifications. These specifications differ in whether they include state-specific time varying macroeconomic variables and state-specific time trends. Among these specifications, our preferred one includes both state-specific time varying controls and time trends, which is plausibly the most conservative specification.

Panel A in Table 4 presents the results on 1-year recidivism for all reoffenders by their first offense type. The dependent variable is an indicator of recidivism which takes a value of 1 if an ex-offender ever recidivated within 1 year after release. In Panel A, all of the coefficients are negative and statistically insignificant except for public order violations (column 5). Yet, that coefficient is also statistically insignificant if we consider the p-value obtained from wild cluster bootstrap iterations as more reliable. In short, the results suggest that we do not have any evidence to reject the null hypothesis that the ACA has no effect on 1-year recidivism for all reoffenders in general. We further observe that, for each first offense type, the coefficients obtained in most of the specifications are very similar.

Panel B reports the estimates for multi-time reoffenders. While the estimates for offenders whose first crimes were either property or drug crimes remain negligible, we find significant reductions on recidivism among violent offenders and weak negative effects on offenders with public order violations. Specifically, as shown in column 3 in Panel B, when our most conservative specification is considered, the results indicate that being exposed to the ACA expansion upon release reduces an inmate's probability of recommitting a crime and going back to prison by about 0.6 percentage points. This implies a 15 percent drop in the recidivism rate among multi-time reoffenders with violent first offenses. The effect is even larger when state-specific time trends are excluded from the regressions, as exhibited in column 2. Moreover, the evidence from the event study specifications in Figure 3 and the specification that includes control variables suggest that there is some reduction in recidivism rates when the first offense is a public order violation. In particular, the reduction is about 35 percent in the specification that includes control variables but not state-specific time trends (column 5).

It is clear that controlling for time-varying controls and/or state-specific trends substantially reduces the standard errors and leads to more precisely estimated coefficients. The magnitude of the estimates is fairly similar with or without time-varying macroeconomic control variables, although there is a slight difference in magnitude. A possible explanation for such a difference between the coefficients could be that the expansion of health insurance coverage among states was not a random assignment. Expansion states, however, are plausibly comparable to non-expansion states conditional on certain observable characteristics. This identifying assumption is common in the Medicaid literature, including the studies that estimate the effect of public health insurance on the propensity to commit crimes.[43] Therefore, we attach more importance to the specifications where at least the time-varying macroeconomic variables are controlled.

Employing the same strategy, we estimate the effect of the ACA expansion on 2-year recidivism. The estimates are reported in Table 5. We find results similar to those

[43]See, e.g., Jácome (2020) who matches on observable characteristics of men in groups with low and high Medicaid enrollment to assess the causal effect of losing Medicaid eligibility on the likelihood of incarceration. See, also, Vogler (2020) who conditions on state-level time-varying control variables and region-by-year fixed effects in dynamic and most of the static specifications that explore the difference in crime rates between expansion and non-expansion states.

presented in Table 4: the ACA expansion has no detectable effect on recidivism for all reoffenders within two years, except for some weak evidence of reductions in recidivism among offenders convicted of public order crimes. Contrary to the all reoffenders sample, there are significant and negative effects on recidivism among multi-time reoffenders convicted of violent crimes. Specifically, the ACA expansion reduces 2-year recidivism among multi-time reoffenders with violent offenses by about 16 percent. We again find some evidence suggesting reductions in recidivism within the 2-year window of release for those convicted of public order violations.

In the analyses, the standard errors are clustered at the state level. An important limitation of inference with cluster-robust standard errors is that asymptotic tests may over-reject with few clusters, which is often defined as less than 30 (Cameron, Gelbach, and Miller, 2008). In both Tables 4 and 5, we provide the *p*-values obtained from 1,000 wild cluster bootstrap iterations. Our statistical inference with regard to violent offender recidivism is robust to adjusting cluster-robust standard errors to correct for few clusters. On the other hand, the reason we frame the evidence for recidivism among public order violators as "weak" is because the estimates become marginally insignificant in some specifications that employ the wild cluster bootstrap procedure (see, e.g., Table 5, Panel B, columns 5-6).

Therefore, our main finding is that increasing access to public health insurance reduces the likelihood of reoffending for those previously convicted of violent crimes, which are strongly associated with mental health and substance abuse disorders (Hodgins et al., 1996; Silver, Felson, and Vaneseltine, 2008). On the other hand, as highlighted above, we find no statistically significant effects on the recidivism of individuals whose first offenses were property crimes, which tend to be financially motivated.[44] In Appendix Table A3, we further check whether the policy is effective on one-time reoffenders. These are offenders

[44] It is plausible that low clearance rates, defined as arrests for each reported crime or solved for reporting purposes, may introduce noise in recidivism rates, particularly for property crimes. According to data from the 2017 Uniform Crime Reports (UCR), clearance rates for violent crimes (0.62 for murder and nonnegligent manslaughter) were much higher than property crimes (0.14 for motor vehicle theft). However, previous studies that use the same data set suggest that the noise effect is not large enough to off-set the greater income effect, at least to an extent where (statistically significant) changes in recidivism rates become undetectable (see, e.g., Agan and Makowsky, 2018).

who return to prison only once. This allows us to gain more insights about whether the policy operates through reduced commission of crimes among one-time or multi-time reoffenders. The results indicate that there are no statistically significant effects of the ACA expansions on one-time reoffenders.

These findings altogether suggest that the policy is effective in reducing the offenses committed by multi-time recidivists, which could potentially generate large economic and social benefits in the form of criminal harm reduction.

One potential stage that may affect access to care is experiencing need for treatment. If the average policy effect is driven by certain types of offenders who are more likely to experience a need for treatment, we would expect the local policy effect to be larger for those groups. We estimate potential heterogeneity in treatment exposure among multi-time reoffenders by age categories with time-varying controls.[45] Figure 4 reports the result for both 1- and 2-year recidivism among violent offenders. We find a reduction in recidivism among violent offenders, whose statistical significance exhibits a U-shape in offenders' age at release. Specifically, reductions are most significant for inmates aged 35-44, and the statistical significance of reductions is decreased as one moves further away from this age group.

In Section VI.F, we also check whether access to SUD treatment through criminal justice referrals is higher for older individuals in expansion states after 2014 and confirm that this relationship is in fact present. This further supports the claim that reductions in recidivism due to increased access to health care are largely driven by perception effects, because these effects are present only if the person eligible for increased health care actually utilizes more health care.

[45] Given the structure of our age variable, we cannot strictly restrict our sample to inmates aged above 26 and below 65. The former group could be affected by the dependent coverage mandate and the latter have access to Medicare. When testing for the mechanism on access to SUD treatment, however, we are able use those above 65 as a falsification check. In addition, we are able show whether the null effects for inmates aged 18-24 are potentially due to the dependent coverage mandate or low rates of access to care. Note that inmates aged 18-24 might not necessarily benefit from parents' private coverage since they are likely to come from poor families or have no parents in the household.

VI.B. Decomposition of Recidivism

In our benchmark specifications, we categorize offenders based on their first offense. In this section, we further decompose changes in recidivism rates using both the first offense and reoffense types. The motivation here is to explore potential heterogeneous effects of the ACA expansions across offenders with the same type of first offense who differ in their reoffense types. This allows us to gain a better understanding about how the expansions reduce recidivism. It suggests that it operates by reducing the repeated commission of the types of impulsive crimes that led to the first conviction of some offenders.

Specifically, the decomposition of 1- and 2-year recidivism in Tables 6 and 7, respectively, reveals findings regarding the behavior of would-be multi-time reoffenders. In both cases, we find negative and statistically significant effects on the propensity of individuals to recommit the same type of offense as their first offense, but only among people with a violent crime or public order violation as their first offense. No other combinations of offense types yield effects that are statistically different from zero. This is an important finding that suggests that Medicaid coverage under the ACA reduces impulsive offense recidivism, which is consistent with our theoretical predictions that health insurance coverage operates by altering some individuals' perceived non-monetary benefits from crime.

We further use the data to evaluate an a priori plausible theory, which may be offered as an alternative to the one we have proposed in explaining the different effects of Medicaid coverage on different types of crimes. This theory asserts that Medicaid coverage effects on recidivism are likely to be greater for crimes associated with longer imprisonment terms, because being convicted for such crimes generates a longer Medicaid coverage loss. According to this theory, the impact of Medicaid on violent crime recidivism is likely to be larger, because violent crimes are typically associated with longer imprisonment sentences. We note that our results are not likely to be explained by this theory. This is because, as we report in Tables 6 and 7, we find no significant effect on violent crime recidivism among offenders whose first offense was not also a violent crime. Moreover, the distribution of time served in prison for property crimes and public order crimes is very similar (see Tables A2 (a) and A2 (c)). However, we find reduced recidivism rates among those with public order offenses in some specifications, while there is no statistically significant change in recidivism for those with property offenses in any of the specifications. Therefore, this alternative theory is unlikely to explain all observed differences.

VI.C. Alternative Specifications

In this section, we present results obtained from alternative specifications. In the following analysis, we focus on 1- and 2-year recidivism rates of multi-time reoffenders, which we find to be most substantially affected by the ACA Medicaid expansion.

In the benchmark analysis, we restrict the sample to include states that provide information on released inmates for each state and year in our working sample. To test if the results are sensitive to this restriction, we re-estimate equation (8) including states that have missing data in one or more years in the sample period. The results are presented in Panel B of Table 8, which suggest that including these states does not alter our findings.[46]

In our main specifications, we control for a rich set of covariates to mitigate concerns about individual- and state-level confounders, which could drive criminal behavior. Since the identification relies on the sharp change in the access to public coverage for a specific group of states, one concern could be that the effect we discover in the estimation captures the impact of other policy changes, especially those related to the justice system. To our knowledge, there is no such change that specifically affects the same group of states in the same time period. Nonetheless, we collect data on a number of variables that gauge variations in legislations and the justice system in states over time. Specifically, we collect data on per capita total expenditure in the justice system for each state and year. We also gather information from the UKCPR National Welfare Data on the partisan composition of the legislature by state and year.[47] In addition, we construct an indicator for marijuana

[46]In Panel A, we replicate the baseline results for the purpose of comparison.

[47]In Nebraska, the unicameral legislature body is elected in a non-partisan manner. Therefore, the data do not report partisan composition for Nebraska. We construct this variable for Nebraska using narrative evidence for each of the elected legislators throughout the years in our sample.

legalization, which takes the value of 1 if recreational use of marijuana is legal in a state in a specific year; otherwise 0.[48] As shown in Panel C in Table 8, the regression results remain intact after controlling these variables in equation (8).[49]

As discussed in section IV, we do not include early and late expansion states in the main analyses. As a robustness check, we add all these states back to the sample and re-estimate equation (8) for both 1- and 2-year recidivism.[50] The results are presented in Table A4 in the Appendix. The results echo our main findings that the ACA Medicaid expansions significantly reduce recidivism among offenders convicted of violent crimes. Moreover, we do not find any evidence to reject the null hypothesis that the effect of these health coverage expansions are statistically different from zero for other categories.

To further check the sensitivity of our results to the specific compositions of states that are included in the sample, we follow the classifications for treated and control groups used in Courtemanche et al. (2017) and define 2014 as the expansion year. Our main objective here is to test whether our initial sample cut matters for the analysis as opposed to the case where we use different treatment and control classifications. The sample period in Courtemanche et al. (2017) is between 2011 and 2014. Therefore, late expansion states are considered to be treated in 2014. In our classification of the treatment group, we only make adjustments to late expansion states. Since we have data after 2014, we are able to assign the "actual" treatment year for late expanders. For example, we classify Alaska, Indiana, and Pennsylvania as treated after 2014. Following Courtemanche et al. (2017), we also include any states with comprehensive or limited expansions prior to 2014 in the early expansion group. The early expansion states in the treatment group include Arizona, California, Colorado, Delaware, Illinois, Indiana, Iowa, Maryland, Massachusetts, Minnesota, New Jersey, New York, Oregon, Rhode Island,

[48]The data are collected from Maier, Mannes, and Koppenhofer (2017).

[49]While the total number of police officers per 10,000 in the population could be an important control variable, we do not include it in our estimations due to the potential endogeneity problem. Controlling for the total number of police officers, however, does not change the results. Data on police officers and justice expenditure can be retrieved from the Justice Expenditure and Employment Extracts Series published by the Bureau of Justice Statistics (see https://bit.ly/2Zb76Vo).

[50]In the selected version of NCRP, data from Louisiana are only available for offenders who were released after 2015. Therefore, there are no observations from Louisiana in the working samples for both 1- and 2-year recidivism.

Washington, and Washington, DC. The early expansion states in the control group include Maine, Tennessee, and Wisconsin.

Figure A2 reports estimates across specifications with different classifications of treatment and control groups. Specifically, the estimates come from the following classifications of the sample: our benchmark sample in this paper (13 states in treated group and 14 states in control group); including all of the states in the sample (26 states in treated group and 19 states in control group); dropping all of the states with comprehensive or limited early expansion in both treatment and control groups (9 states in treated group and 16 states in control group); keeping only 2014 expansion states in treated group (9 states in treated group and 19 states in control group); keeping only early expansion states with comprehensive or limited programs in treated group (17 states in treated group and 19 states in control group); and dropping late expanders (21 states in treated group and 19 states in control group). We further check the sensitivity of our estimates by dropping California. The aforementioned sensitivity checks suggest that our findings are not driven by our initial sample selection and our estimates are remarkably robust, especially for violent offenses.

Additionally, we utilize data from all states in our sample and implement a test by excluding data from one specific state at a time. For this analysis, we use the whole sample of states, including early and late expansion states. We display the results in Figure A3. According to the figure, the estimates do not change qualitatively when we leave any one state out from the analyses. The inference remains unaltered. The results obtained from this exercise suggest that the estimates are not likely to be driven by data from any specific state. These exercises interpreted jointly suggest that results are robust under various specifications.

VI.D. Permutation Test

Following Cantoni et al. (2017) and Yu and Mocan (2019), we further implement a permutation test (or randomization inference) that provides an alternative way to make inferences about causal effects. Specifically, we randomly assign treatment and nontreatment status to all states in the sample based on the real number of expansion and non-expansion states in our working sample. Then, we re-estimate equation (8) using the newly constructed sample and record the test statistic of the estimated effect. By replicating this process 1,000 times, we obtain a distribution of the test statistics and calculate the probability of observing an estimate as statistically significant as the one obtained in our benchmark results (reported in Tables 4 and 5). This probability can be simply interpreted as a p-value of the estimated effect of the ACA Medicaid expansion.

More specifically, we focus on 1- and 2-year recidivism rates among multi-reoffenders and depict the results in Figure A4. In addition, in Figure A4, we draw a vertical line to show the t-statistic obtained from our baseline estimations (Panel B in Tables 4 and 5) for comparison. The proportion of the t-statistics obtained from the replications, which is smaller than the benchmark t-statistics (which have negative values), is reported in the figure as well. Based on Figure A4, the t-statistics approximately follow a normal distribution centering at zero. For ex-offenders who committed violent crimes within a 1-year window, only in 3.8% of the replications, the t-statistics are equal or larger (in magnitude) than the one obtained from our benchmark estimation, suggesting that our baseline results are robust. Moreover, the randomization inference suggests that the p-value is 0.004 and 0.07 for recidivism among offenders with previous violent and public order offenses, respectively, within a 2-year window. The distributions of the t-statistics for the remaining categories are also consistent with the main results. Therefore, this permutation test indicates that our benchmark inference is robust.

VI.E. Evidence from the Restricted NCRP

In our main analyses, we employ the publicly available (selected) version rather than the restricted version of the NCRP data. The selected version is preferred because it contains data on inmates who were released in 2016, which are not available in the restricted NCRP data as of this study. In comparison with the restricted version, the selected NCRP provides a larger number of observations for the analyses of 1-year recidivism, and allows us to investigate the effect of the ACA expansion on 2-year recidivism as well.

Yet, it is still informative to explore whether employing the restricted NCRP data yields similar results. Therefore, we repeat the analyses in Table 4 using the restricted NCRP data. Due to the limitations of the data, we can only estimate the effect of the ACA expansion on 1-year recidivism.[51]

The results are reported in Appendix Table A5. The estimates are largely consistent with those in the main analyses. In fact, the effects on 1-year recidivism among multi-time reoffenders, notably for those with previous violent crime and public order violation convictions, are even larger using the restricted NCRP data. Therefore, the results strongly support the consistency of our findings in the benchmark case.

VI.F. Public Coverage and Access to Substance Use Disorder Treatment

Both the theoretical and main empirical findings suggest that the ACA Medicaid expansion could reduce recidivism, particularly by increasing access to health care among previous offenders. The salient effects of the expansion on recidivism among people with violent crime and public order violation convictions also suggest that the expansion might have had a more profound impact on individuals who are in need for mental illness and addiction treatment. Therefore, in this section, we explore whether the ACA Medicaid expansion has a positive effect on individuals' access to substance use disorder (SUD) treatment. We are particularly interested in individuals referred to treatment by the criminal justice system.

We employ state administrative records from the Treatment Episode Data Set (TEDS) by the Substance Abuse and Mental Health Services Administration (SAMHSA), from 2010 to 2016. TEDS is compiled by states with the goal of observing substance use treatment centers that receive state and federal public funding for the provision of alcohol and drug treatment services. While TEDS does not comprise the total national data for substance abuse treatment, the average number of admissions reported in the data was 1.77 million between 2010 and 2016.[52] The data contain, among other variables, demographic

[51] Although the selected and restricted NCRP data share much in common, they are different in a number of ways. We explain the details of sample restrictions and other sample selection procedures in the Appendix.

[52] Based on SAMSHA's key indicators for substance use and mental health in the United States, the

information, substance use characteristics, payment source, and the source of referral to treatment. Payment source describes if the clients' treatment is provided by a form of health insurance, self-payment, worker's compensation, or other government sources. Insurance payment sources include private insurance, Medicare, and Medicaid. The referral sources include self-referral, alcohol/drug use care provider, health care provider, school, employer, community referral, and court or criminal justice referral. For those referred from the criminal justice system, the reported sources are state or federal court, formal adjudication process, probation or parole, other legal entity, diversionary program, prison, and court referrals due to driving under the influence (DUI) or driving while intoxicated (DWI).

To make the working sample comparable, we impose the same restrictions applied to our benchmark specification. Motivated by the discrete nature of the dependent variable, as well as the ability to accommodate fixed effects without suffering from the incidental parameters problem, we estimate the following equation using a Poisson model:

$$Admissions_{st} = \kappa_{st} exp(\alpha_0 + \zeta_s + \eta_t + \alpha_1 Expansion * Post_{st} + \mathbf{\Omega}_{st}\mathbf{\Gamma}_1 + \epsilon_{st}). \quad (9)$$

The specification above defines the count of admissions to SUD treatment ($Admissions_{st}$) as a function of the ACA expansion in state s in year t. As in equation (8), this specification includes a full set of state fixed effects (ζ_s) and (admission)-year fixed effects (η_t). In addition, $\mathbf{\Omega}_{st}$ also includes a series of state time-varying covariates (the minimum wage, the housing price index, the poverty rate, and the unemployment rate). We proxy exposure for each unit with κ_{st} using state population.[53]

Table 9 presents the results obtained by estimating equation (9). Panel A shows the change in admissions by sources of payment. Among those using Medicaid as a primary

average number of individuals that received specialty treatment was 2.25 million between 2015 and 2016, and 3.8 million received any kind of substance use treatment in 2016. Given these numbers, 1.77 million admissions represent 79% of the average number of admissions to specialty substance use treatment between 2015 and 2016 or 47% of any substance use treatment admissions in 2016. These indicators can be obtained from: https://bit.ly/3tQayVD.

[53]Using state population (*population*) as a proxy for exposure in a Poisson model constrains the coefficient of ln(*population*) to 1. The estimates are also robust to the inclusion of ln(*population*) without imposing any restrictions. Population data come from the Bureau of Economic Analysis (https://bit.ly/2JKnWWc), and represent Census Bureau midyear population estimates.

payment method, we find an increase in the admissions to SUD treatment after the ACA expansion. When the payment source changes to private insurance or self-payment, the estimates are not statistically different from zero. Figure 5 also confirms that the difference in pre-existing trends across expansion and non-expansion states is about zero, supporting the validity of our estimates. These findings, including the effect sizes, are consistent with the findings in the literature (see, for example, Maclean and Saloner, 2019, Grooms and Ortega, 2019).[54]

This analysis differs from prior studies, as we are mainly interested in criminal justice referrals and how admissions to SUD treatment among the justice-involved population change with the ACA expansion. Panel B in Table 9 presents the estimates for both self-referrals and criminal justice referrals conditional on observing Medicaid as the payment source.[55] Note that the former group of referrals may also include ex-offenders, though we expect a larger effect among the latter group. Our findings confirm that conditional on Medicaid, there is an increase in admissions to SUD treatment for self-referrals and criminal justice referrals after 2014, where the effect is larger for the latter.[56] To further narrow down the effects on ex-offenders, we restrict the sample to referrals from prisons and while on probation or parole. We find an even larger effect on admissions to SUD treatment in expansion states after 2014. The trends in the number of admissions by types of referral in Figure 6 also show that the number of admissions is relatively flat in

[54]Moreover, Meinhofer and Witman (2018) find that aggregate opioid admissions to specialty treatment facilities from Medicaid beneficiaries increased 113% after Medicaid expansions. Their findings also suggest that Medicaid expansions not only increased utilization but also resulted in substantial availability gains such as greater acceptance of Medicaid and market entry among medication-assisted treatment providers.

[55]We also check whether conditioning on different payment methods, including other government sources, affect admissions to SUD treatment for self-referrals and criminal justice referrals (see Table A6 in the Appendix). Other government sources include commissions within the criminal justice system (e.g., the Sentencing Accountability Commission in Delaware), among other government agencies that pay for the treatment. As expected, we do not find any statistically significant change in admissions for self-referrals and criminal justice referrals conditional on other government payments.

[56]We note that the sample of Medicaid participants include both marginal participants (not eligible prior to the expansion) and inframarginal participants (eligible prior to the expansion). The average characteristics of these two groups, however, could be very different. When analyzing criminal justice referrals to SUD treatment conditional on Medicaid, the increases in expansion states relative to non-expansion states could be driven by both the differences between marginal and inframarginal participants' characteristics and potential changes in all participants' behavior. We are agnostic with respect to which of these potential mechanisms is driving the increase in admissions. Instead, our objective is to show the existence of such increases.

non-expansion states pre- and post-2014, whereas the number of admissions dramatically deviate from the common trend in expansion states after 2014.[57]

We further investigate whether the effects of the expansion on the number of SUD treatment admissions are heterogeneous by age groups. Specifically, we estimate the effect for the same age groups as those employed in Figure 4.[58] The estimates are reported in Figure A5. We find that, in general, age groups in the intermediate range (i.e., 25-34 to 55-64) are more affected by the Medicaid expansion in comparison with the youngest and oldest groups. The results echo our findings depicted in Figure 4, showing a U-shaped relationship between the statistical significance of recidivism reductions and age groups. In addition, the results show that all age groups between 18 and 64 years in expansion states are significantly affected by the expansion. Meanwhile, people aged 65 or older remain unaffected by the expansion. The reason is that people aged 65+ are eligible for Medicare in both expansion and non-expansion states.

Taken all together, the results provide strong evidence suggesting that the ACA Medicaid expansion sharply raises actual access to SUD treatment among the population covered by Medicaid. We do not find significant changes among people who are self-paying for treatment or those covered by private insurance. We find particularly strong effects among people who have Medicaid coverage and are referred by the criminal justice system to SUD treatment facilities. This indicates that the Medicaid expansion substantially affects access to SUD treatment for prisoners and potential criminals. As previously noted, the fact that age groups which experience the largest reductions in impulsive recidivism also experience increases in actual access to SUD treatment strengthens the claim that perception effects discussed in our theoretical analysis contribute to these reductions.

[57]We also estimate the effect of the ACA Medicaid Expansions on the SUD treatment admissions using linear regressions to check the robustness of the results presented in Table 9. Specifically, we re-estimate equation (9) with two changes. First, the natural logarithm of the number of SUD treatment admissions is used as the dependent variable. Second, now state population is added as a control variable in the regressions. Other covariates used in equation (9) are all included in the regressions. The results are reported in Table A7 in the Appendix. We show that our estimates are remarkably robust to these changes.

[58]Because of the more detailed age categories provided by TEDS, we are able to divide the population whose age is older than 55 into 55-64 and 65+.

VII. Policy Implications

To discuss the policy implications of our findings, we conduct a partial cost-benefit analysis. First, we calculate the number of newly enrolled offenders in Medicaid that is needed to reduce 1- and 2-year recidivism by 1 percent. Subsequently, we provide back-of-the-envelope calculations comparing the costs of reducing violent recidivism through increased Medicaid coverage against some of its more salient benefits. Because we find that the reduction in recidivism rates is mainly driven by the behavior of multi-time recidivists with previous violent offenses, our calculations in this section are based on this specific sample of reoffenders.

VII.A. Combining First-Stage and Reduced-Form Estimates

According to our regression estimates (see column 3 in Panel B of Tables 4 and 5), the reduction in 1- and 2-year recidivism rates is 15 and 16 percent relative to the mean, respectively. Moreover, we find a 37.8 percent increase in Medicaid take-up among the sample that approximates offender demographics in our first stage estimation using the ACS. Based on the same sample, there are 24,252 individuals enrolled in Medicaid between 2011 and 2013. Combining the first-stage estimate with the estimates from the reduced-form regressions, we find that Medicaid take-up would have to increase by 2.52 percent (= 37.8%/15%) and 2.36 percent (= 37.8%/16%) to reduce 1- and 2-year recidivism by 1 percent, respectively.

The average number of households in the United States between 2011 and 2013 is 121,156,667.[59] Similarly, the average number of households in the ACS between 2011 and 2013 is 3,121,887. Hence, roughly, the ACS represents about 2.58% of the households in the United States. If we assume that the share of Medicaid beneficiaries is close in the whole population of United States and in the ACS sample, $23,688$ $(= 24,252/2.58\% \times 2.52\%)$ and $22,184$ $(= 24,252/2.58\% \times 2.36\%)$ newly enrolled former offenders are needed, respectively, to decrease the probability of 1- and 2-year recidivism by 1 percent.

[59]The data on annual total numbers of households in the United States are from the Census Bureau, Table HH-1 (see https://bit.ly/3kt9Vgg).

We further calculate the number of newly enrolled offenders in Medicaid that is needed to avert 1- and 2-year recidivism by one incident. Specifically, in our sample of 1-year recidivism multi-time reoffenders, one percent of 1-year recidivism is equal to 248,410 (N) $\times$ 0.040 (mean of recidivism) $\times$ 1%=99 incidents. This means that a reduction of one incident in 1-year recidivism requires 23,688/99=239 newly enrolled offenders in Medicaid. Similarly, one percent of 2-year recidivism equals 209,961 (N) $\times$ 0.058 (mean of recidivism) $\times$ 1%=122 incidents. Therefore, every reduction in 2-year recidivism by one incident requires 22,184/122=182 newly enrolled offenders in Medicaid.

An immediate policy implication of our findings is that prison-exit programs that implement strategies to enroll Medicaid-eligible inmates and inform them about treatment options, especially for mental health and substance use disorders, may effectively curb recidivism rates. Recent studies that explore the effect of Medicaid eligibility on incarceration also show that access to Medicaid during childhood has long-term spillovers in terms of reduced incarceration in adulthood (Arenberg, Neller, and Stripling, 2020), whereas losing Medicaid eligibility has the opposite effect of increasing incarceration among men with prior mental health problems (Jácome, 2020). The upshot is that providing Medicaid coverage to former inmates has positive implications beyond improving health outcomes in the form of reduced incarceration. In the next section, we discuss the cost effectiveness of providing Medicaid coverage to former inmates.

VII.B. Costs and Benefits of Providing Medicaid Coverage

We conclude our discussion of policy implications by estimating the costs of expanding Medicaid for the number of newly enrolled offenders needed to avert one inmate from returning to prison. We compare the estimated costs with the benefits of expanding Medicaid in the form of reduced economic and social costs of victimization per crime as well as reduced economic and fiscal costs from fewer incarcerations. We use a dynamic approach and calculate costs and benefits for providing one to four years of Medicaid coverage. The main reason for adopting a dynamic approach is that an individual might experience social and economic improvements after being on coverage for a certain amount of time. These potential improvements imply that an individual might not necessarily be eligible for coverage every year. Nonetheless, we take the 4-year window as our benchmark since a former inmate who returns to prison would be incarcerated for an average of 4 years for committing a violent crime. The estimates for costs and benefits are reported in Table 10.

We begin our estimation by obtaining the average cost of providing Medicaid coverage per adults aged 20-64. We consider both individuals who were newly eligible for Medicaid under the ACA and those who were already eligible in both expansion and non-expansion states. Using administrative data from the Centers for Medicare & Medicaid Services (CMS) for fiscal year 2017, we find that the average cost of Medicaid per adult is \$5,562. We combine this cost estimate with the number of newly enrolled offenders in Medicaid that is needed to avert one inmate from returning to prison. Therefore, the 4-year total coverage cost is \$5,317,272 and \$4,049,136 to avert one incident of multi-recidivism within one year and two years upon release among violent offenders, respectively. If only one year is required to improve outcomes among inmates, then the total coverage cost can be as low as \$1,012,284 after two years of release.

Next, we calculate the benefits of providing Medicaid under three categories. The first category is the cost reduction through reduced economic and social costs of victimization per crime. We follow Miller et al. (2020) to measure the tangible and intangible costs of being a victim. Tangible costs include medical costs, lost productivity, property loss, and the use of public services, such as law enforcement, emergency services, or victim assistance, among others. Intangible costs are estimated monetary costs related to pain, suffering, and loss of life quality.

It is important to note that the reduction in the number of detected multi-time recidivists is less than the reduction in the number of crimes committed by recidivists. This is due to two important reasons. First, the probability of detecting each crime is less than one. Moreover, as indicated by our results in Tables 4, 5, and A3, the reduction in the number of multi-time recidivists is driven by a shift towards refraining from reoffending. Therefore, to calculate a conservative lower bound for cost reductions associated with fewer victimizations, we multiply the sum of tangible and intangible victimization costs, \$91,110, by twice the inverse of the probability of punishment. In the calculation, we only consider 85% of the economic and social costs (of \$91,110) because among the multi-time reoffenders in the working sample, about 85% of the reoffenses are violent crimes.[60] We use 0.131 as the probability of punishment for violent crimes, which we borrow from Shavell (1993).[61] The economic and social cost reduction through fewer victimizations obtained through this calculation is \$1,181,788.

To calculate the fiscal costs of incarceration, we use data from the Vera Institute of Justice on state prison costs per inmate.[62] The average daily incarceration cost per inmate is \$91.16. An inmate, on average, serves 4 years in prison for committing a violent crime. Therefore, the reduction in total fiscal costs is \$133,094 (=\$91.16 $\times$ 4 years $\times$ 365 days). The last category of benefits relates to the economic costs of incarceration. We follow the approach provided by Arenberg, Neller, and Stripling (2020). The paper obtains the cost estimates from Mueller-Smith (2015), which considers a non-linear relationship between the costs of incarceration and time served in prison and reports estimates for 6 months, 1 year, and 2 years. We have already discussed above that time served in our sample exceeds 2 years. Therefore, as suggested by Arenberg, Neller, and Stripling (2020), fitting a linear line for the relationship between the length of time served in prison and economic costs would provide us the one-time prison penalty as well as the yearly cost of being incarcerated. In this case, the one-time prison penalty is the intercept of the fitted line (\$16,000), whereas the duration penalty per year is the slope (\$10,000). Multiplying these cost estimates with the average time served in prison gives us a total economic cost of \$56,000.

Our calculations suggest that there are substantial benefits associated with expanding Medicaid coverage. Specifically, the particular benefits from coverage we considered

[60]By doing so, we are implicitly assigning a cost of \$0 to the remaining crimes committed by this group. We do so to avoid over-stating the benefits of reducing recidivism within this group by assigning large values to the remaining crimes committed within this group.

[61]This probability has not changed much over time based on our comparison with recent data from the UCR and BJS on clearance rates and the probability of reporting.

[62]See the following report from the Vera Institute of Justice to obtain these cost estimates: https://bit.ly/3pFVbMd.

exceed its costs if a short duration (e.g., one year of Medicaid coverage) is sufficient for former inmates to receive the treatment they need. Moreover, we note that although this back-of-the-envelope analysis captures most of the costs associated with providing former prisoners with Medicaid, it only includes some of its benefits. This is because it excludes hard to measure benefits, such as the direct value of Medicaid coverage to all newly enrolled former prisoners and their families, as well as the value that would-be recidivists attach to the liberties they would lose upon being imprisoned. There is evidence that the uninsured rate in states that did not expand Medicaid coverage is double that of expansion states, 15.5% versus 8.3%.[63] In addition, we have only considered the benefits from reducing recidivism among reoffenders whose first offense was a violent crime. We also find some weak negative effects on recidivism among multi-time reoffenders whose first crime was a public order crime. Taking into account the potential benefits on this group, the total monetary benefit would be much larger than those reported in Table 10. These findings altogether provide a strong motivation for implementing an expansion policy in 12 states that do not provide Medicaid coverage to many low-income adults, in particular former inmates, as of 2021.

VIII. Conclusion

In this paper, we estimate the effect of increased access to public health insurance on criminal recidivism in the United States. Exploiting administrative data on prison spells, we show that the ACA Medicaid expansion significantly reduces the probability of returning to prison for multi-time reoffenders convicted of violent and public order crimes. Specifically, the effect for multi-time reoffenders with violent offenses is as large as a 16% reduction in recidivism rates between 2010 and 2016. We find no evidence, however, that Medicaid coverage affects prison reentry among one-time reoffenders or when considering all reoffenders together. Moreover, we decompose recidivism rates by first offense and reoffense types to investigate the potential drivers of the policy's effect on crime-specific

[63]See the following report on the coverage gap from the Kaiser Family Foundation: https://bit.ly/3bC4zeE.

recidivism. We find negative effects on recidivism rates for multi-offenders who were re-convicted for the same offense type as their first offense, but only among those convicted of violent and public order offenses. A plausible theoretical explanation for the heterogeneous effects found among these subgroups of ex-offenders is that people with greater self-control problems are more likely to become multi-time reoffenders. This difference may be further exacerbated by the impact of lengthier prison sentences on multi-time offenders' mental states. Therefore, perception effects are likely to be greater among this group, which would make it easier to detect reductions in their recidivism rates stemming from increased access to health insurance.

Increased access to health insurance can cause the type of perception effects that lead to reductions in recidivism only if potential reoffenders actually use their eligibility to receive treatment for mental health disorders and substance abuse. Thus, we also question whether the ACA Medicaid expansion raises the number of admissions to SUD treatment among people covered by Medicaid, and we find that it does. Particularly, we find the positive effect to be large among individuals who are referred by the criminal justice system to SUD treatment facilities, conditional on having Medicaid as the primary payment method. The extent to which former inmates experience a need for treatment could yield heterogeneous effects with regard to access to care and recidivism rates. To test for potential heterogeneity among former inmates, we stratify criminal justice referrals by age groups. The results show that the age groups who experience the most significant reductions in recidivism among violent offenders are also associated with significant increases in SUD treatment admissions. This finding lends further support to the idea that reductions in violent recidivism rates are driven by perception effects.

Our findings have clear policy implications. Specifically, our estimates suggest that providing health care to justice-involved individuals leads to substantial benefits beyond improving their health conditions in the form of reduced recidivism rates. Since these benefits materialize only if ex-offenders in fact take advantage of these opportunities, prison-exit programs wherein ex-offenders are informed and educated about the health care options that are available to them can lead to even greater reductions in crime.

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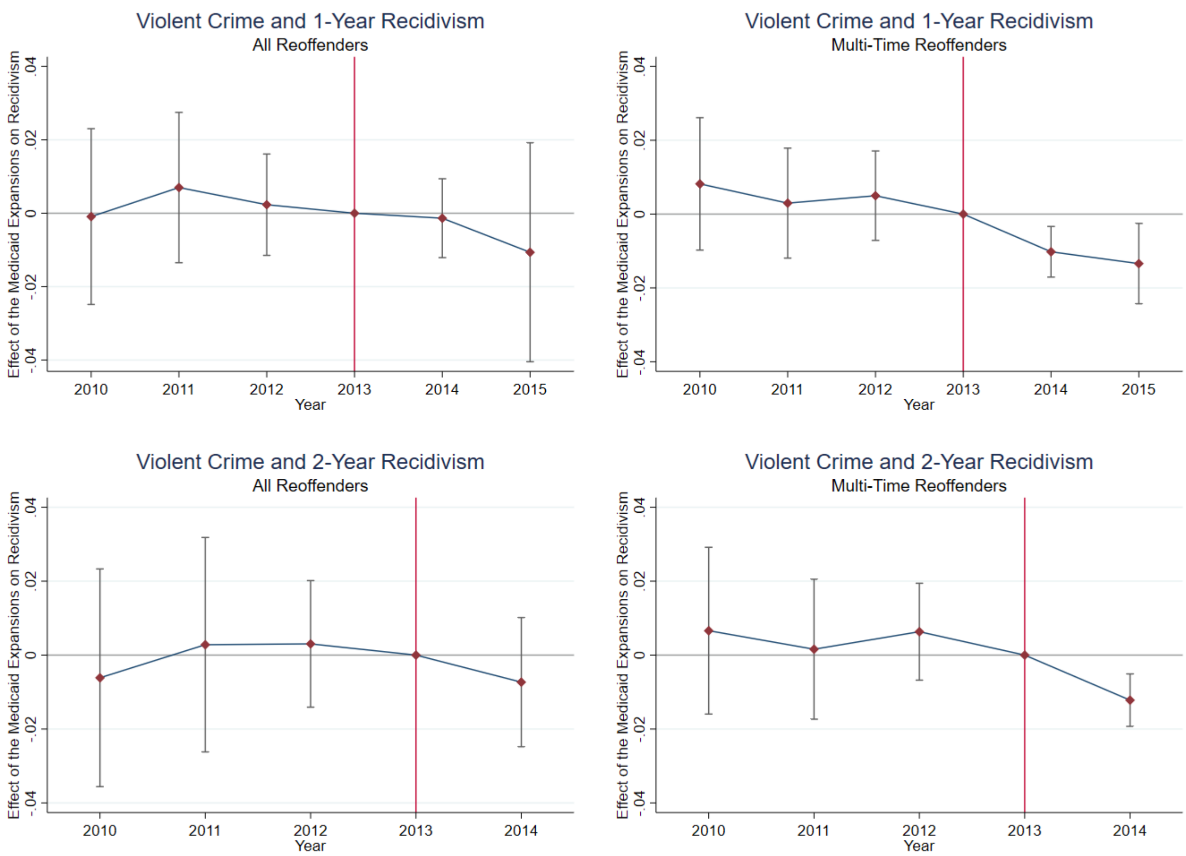

Figure 1. Event Study - Violent Crime

Note: The figure contains event study results for the effect of the ACA Medicaid expansion on 1- and 2-year recidivism among reoffenders with previous violent offenses. The X axis shows years, where 2013 (the year before the ACA expansion) is omitted from the analyses. The Y axis is the scale of the treatment effect. We report the 95% confidence intervals in the figure.

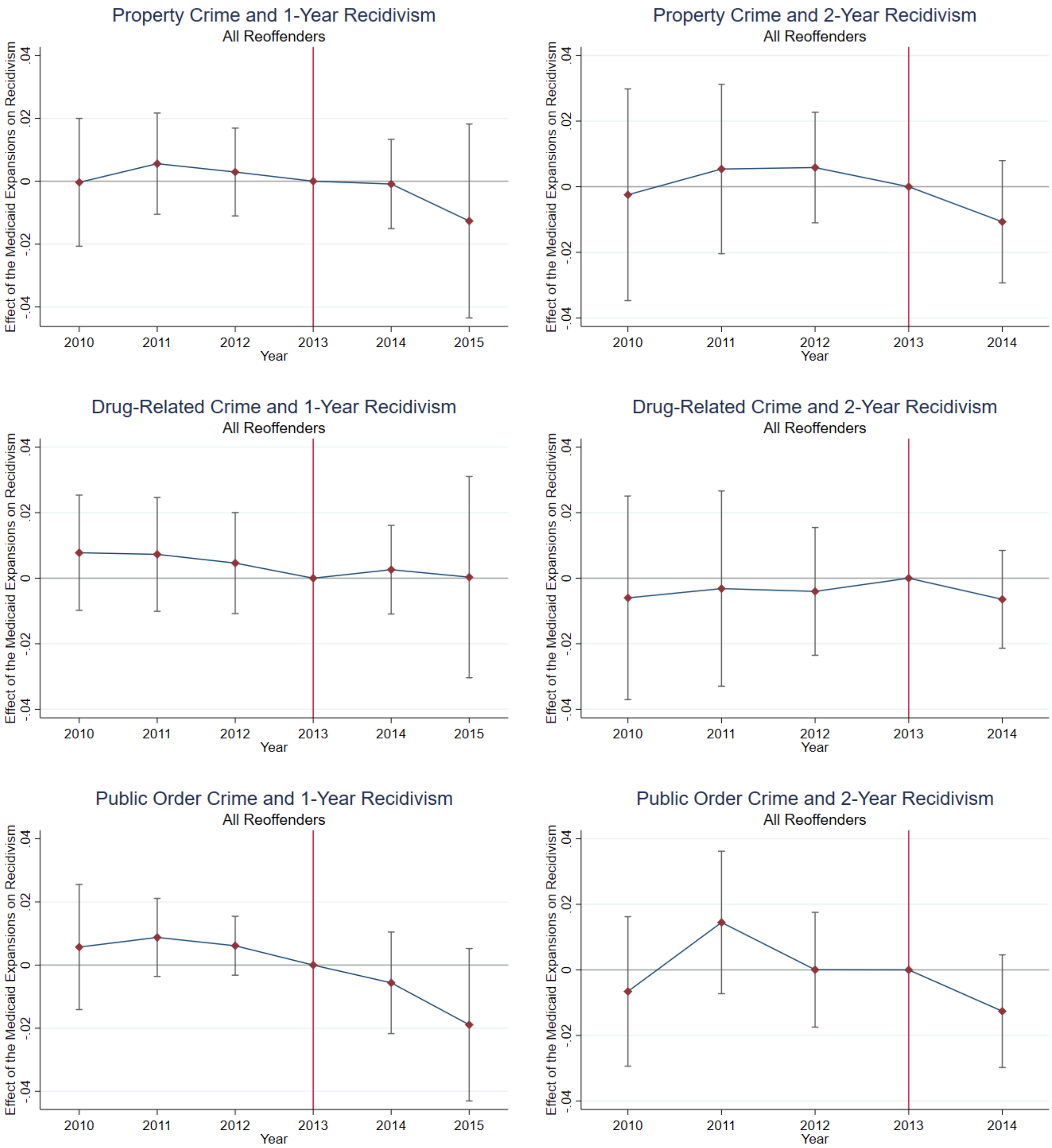

Figure 2. Event Study - Recidivism for All Reoffenders by Other Offense Types

Note: The figure contains event study results for the effect of the ACA Medicaid expansion on 1-year and 2-year recidivism among all reoffenders whose first offense was either a property crime, a drug-related crime, or a public order violation. The dependent variable is an indicator of recidivism that takes a value of 1 if an ex-offender ever committed a reoffense within a 1- or 2-year window after being released from prison; otherwise it takes a value of 0. The X axis shows the years, where 2013 (the year before the ACA expansion) is omitted from the analyses. The Y axis is the scale of the treatment effect. We report the 95% confidence intervals in the figure.

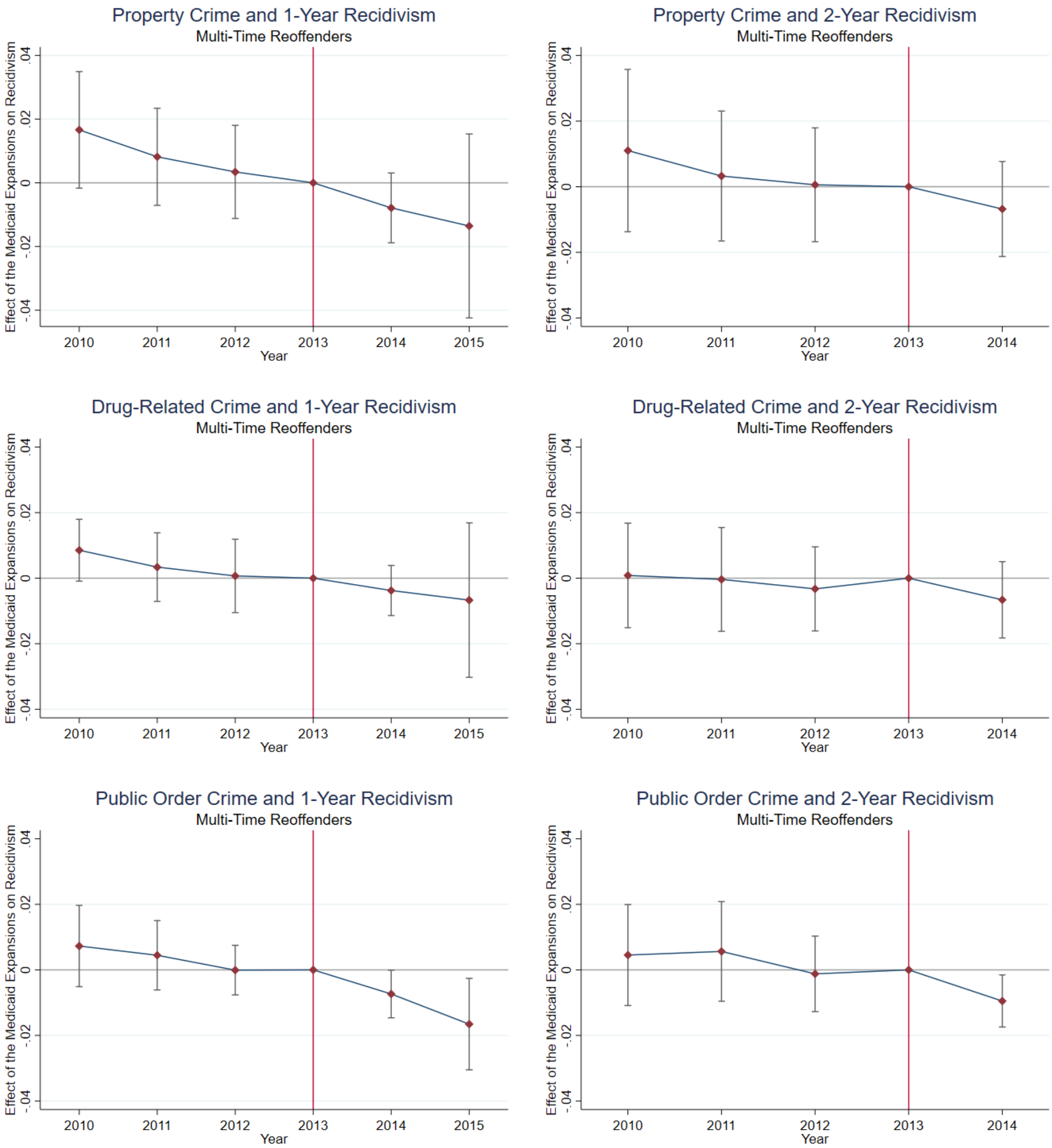

Figure 3. Event Study - Recidivism for Multi-Time Reoffenders by Other Offense Types

Note: The figure contains event study results for the effect of the ACA Medicaid expansion on 1-year and 2-year recidivism among multi-time reoffenders whose first offense was either a property crime, a drug-related crime, or a public order violation. The dependent variable is an indicator of recidivism that takes a value of 1 if an ex-offender reoffended within a 1- or 2-year window after being released from prison and if the ex-offender has multiple reoffenses in the sample period; otherwise it takes a value of 0. The X axis shows the years, where 2013 (the year before the ACA expansion) is omitted from the analyses. The Y axis is the scale of the treatment effect. We report the 95% confidence intervals in the figure.

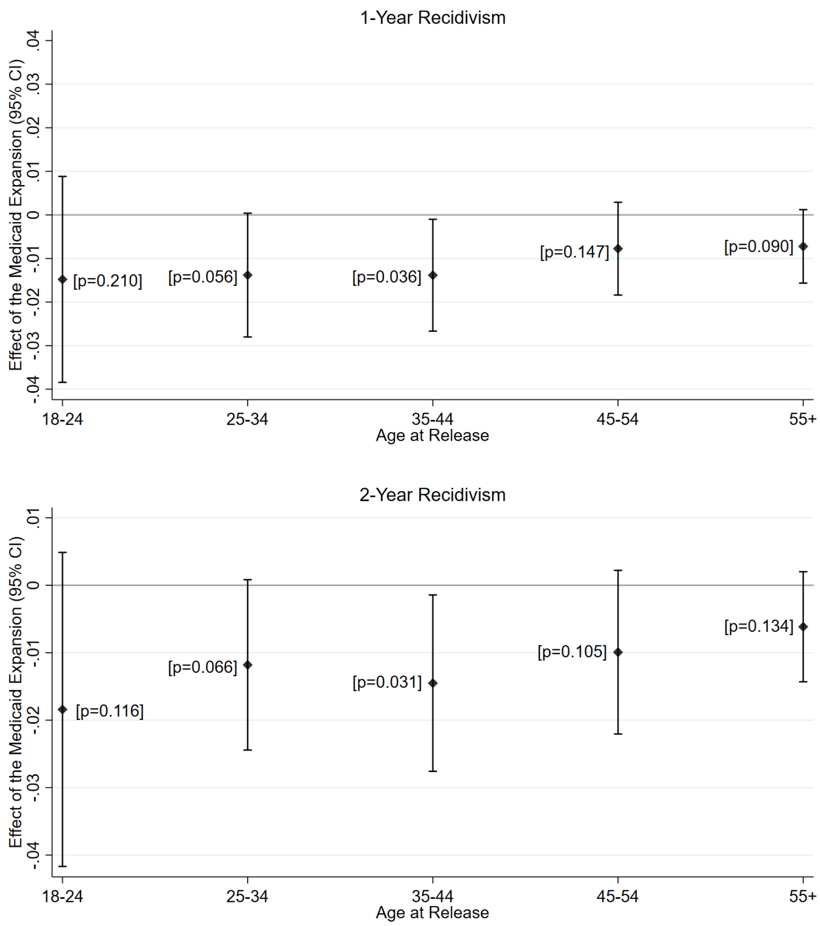

Figure 4. Effect of the ACA Medicaid Expansion on Recidivism by Age Group

Note: The figure reports the heterogeneous effects of the ACA Medicaid expansion on multi-time reoffenders by age categories, for both 1- and 2-year recidivism among reoffenders with previous violent offenses. We report the 95% confidence intervals in the figure. *p*-values of the estimates are reported in brackets.

Figure 5. Event Study - ACA Medicaid Expansion and Substance Use Disorder Treatment by Payment Methods

Note: The figure contains event study results for the effect of the ACA Medicaid expansion on the annual total number of SUD treatment admissions by payment method. The X axis shows the years, where 2013 (the year before the ACA expansion) is omitted from the analyses. The Y axis is the scale of the estimates obtained from Poisson regressions.

Figure 6. Event Study- ACA Medicaid Expansion and Substance Use Disorder Treatment by Referral Sources (Conditional on Paying through Medicaid)

Note: The figure contains event study results for the effect of the ACA Medicaid expansion on the annual total number of SUD treatment admissions, among patients who pay through Medicaid, by referral source. The X axis shows the years, where 2013 (the year before the ACA expansion) is omitted from the analyses. The Y axis is the scale of the estimates obtained from Poisson regressions.

Table 1. Medicaid expansion profile by states

Control group		Treatment group							
Not expanded	Not in NCRP	Expanded		Early expansion/Prior comprehensive program		Expanded late		Not in NCRP	
Alabama	Idaho	Arizona	01/01/2014	Delaware	01/01/2014	Alaska	09/01/2015	Arkansas	01/01/2014
Florida	Virginia	California†	01/01/2014	District of Columbia	07/01/2010	Indiana	02/01/2015	Connecticut	04/01/2010
Georgia		Colorado	01/01/2014	Massachusetts	01/01/2014	Michigan	04/01/2014	Hawaii	01/01/2014
Kansas		Illinois	01/01/2014	Minnesota	03/01/2010	New Hampshire	08/15/2014	Vermont	01/01/2014
Louisiana*		Iowa	01/01/2014	New York	01/01/2014	Pennsylvania	01/01/2015		
Maine		Kentucky	01/01/2014						
Mississippi		Maryland	01/01/2014						
Missouri		Nevada	01/01/2014						
Montana*		New Jersey	01/01/2014						
Nebraska		New Mexico	01/01/2014						
North Carolina		North Dakota	01/01/2014						
Oklahoma		Ohio	01/01/2014						
South Carolina		Oregon	01/01/2014						
South Dakota		Rhode Island	01/01/2014						
Tennessee		Washington	01/01/2014						
Texas		West Virginia	01/01/2014						
Utah		Wisconsin*	01/01/2014						
Wyoming									
$N = 18$	$N = 2$	$N = 17$		$N = 5$		$N = 5$		$N = 4$	

Note: Some of the expansion states had limited expansions before 2014 (see Aslim, 2019b for details). For states that had comprehensive programs throughout the sample period (Delaware, Massachusetts, and New York), we adjust the post period to be the year of the ACA Medicaid expansion. For the remaining early expansion states (District of Columbia and Minnesota), we use the initial expansion year under the ACA option. †California is dropped in the empirical analysis due to the enactment of Public Safety Relignment Act (PSRA) in 2011. Note also that California had limited prior expansion in 2011. *Although Wisconsin did not expand Medicaid under the ACA, childless adults up to 100 percent FPL are eligible for Medicaid. Thus, we include Wisconsin in the treatment group. Louisiana and Montana expanded Medicaid in 2016, but we exclude 2016 to construct 1- and 2-year recidivism, and hence these states are still in the control group during our sample period.

Source: Kaiser Family Foundation, Status of State Action on the Medicaid Expansion Decision (Accessed at https://bit.ly/2ApqilS); Kaiser Family Foundation, Annual Updates on Eligibility Rules, Enrollment and Renewal Procedures, and Cost-Sharing Practices in Medicaid and CHIP (Accessed at https://bit.ly/2JYkb0A).

Table 2. Summary Statistics - Recidivism Rates

	All Reoffenders			Multi-Time Reoffenders		
Dependent Variables	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
***1-Year Recidivism by Crime Type*:**						
Violent	0.157	0.364	248,410	0.040	0.196	248,410
Property	0.208	0.406	250,032	0.063	0.243	250,032
Drug	0.150	0.357	255,295	0.044	0.205	255,295
Public Order	0.142	0.349	161,262	0.039	0.194	161,262
***2-Year Recidivism by Crime Type*:**						
Violent	0.230	0.421	209,961	0.058	0.233	209,961
Property	0.292	0.455	213,726	0.089	0.284	213,726
Drug	0.216	0.412	218,634	0.063	0.242	218,634
Public Order	0.202	0.402	137,603	0.055	0.228	137,603

Note: The summary statistics of 1-year and 2-year recidivism rates by crime type are reported in this table. The samples of 1-year recidivism rates correspond to those in Table 4, and the samples of 2-year recidivism rates correspond to those in Table 5. The crime types listed in the table refer to reoffenders' first offense.

Table 3. Summary Statistics - Violent Crime Samples

Variables	1-Year Recidivism Mean	1-Year Recidivism Std. Dev.	2-Year Recidivism Mean	2-Year Recidivism Std. Dev.
Age When Released				
25-34 years	0.455	0.498	0.455	0.498
35-44 years	0.261	0.439	0.261	0.439
45-54 years	0.182	0.386	0.183	0.387
Gender				
Female	0.091	0.288	0.091	0.287
Race/Ethnicity				
White	0.384	0.486	0.382	0.486
Black	0.346	0.476	0.345	0.475
Hispanic	0.178	0.382	0.178	0.382
Other Races	0.022	0.146	0.021	0.145
Education				
<High School Diploma / GED	0.291	0.454	0.293	0.455
High School Diploma / GED	0.318	0.466	0.319	0.466
Any College	0.070	0.255	0.070	0.256
Time Served				
<1 year	0.286	0.452	0.293	0.455
1-1.9 years	0.158	0.365	0.161	0.368
2-4.9 years	0.209	0.407	0.210	0.407
5-9.9 years	0.142	0.349	0.136	0.342
>=10 years	0.104	0.305	0.099	0.299
Sentence Length				
<1 year	0.095	0.293	0.097	0.296
1-1.9 years	0.049	0.216	0.050	0.217
2-4.9 years	0.272	0.445	0.272	0.445
5-9.9 years	0.259	0.438	0.259	0.438
10-24.9 years	0.248	0.432	0.246	0.430
>=25 years	0.054	0.225	0.054	0.225
Life, LWOP	0.018	0.132	0.018	0.131
Admission Type				
Court Commitment	0.804	0.397	0.798	0.401
Return from Parole / Revocation	0.172	0.377	0.176	0.381
Other	0.007	0.081	0.007	0.083
Release Type				
Conditional Release	0.567	0.496	0.563	0.496
Unconditional Release	0.285	0.452	0.293	0.455
Other Types of Release	0.002	0.043	0.002	0.046
Minimum Wage	7.499	0.417	7.462	0.381
Housing Price Index	281.627	58.497	276.696	56.911
Unemployment Rate	7.549	1.926	7.976	1.762
Poverty Rate	15.637	2.705	15.863	2.669
Number of Police (per 10 thousand population)	21.352	2.870	21.384	2.884
Share of Democrats in the Congress	0.415	0.108	0.422	0.108
Marijuana Legalization	0.036	0.186	0.032	0.176
Justice System Expenditure (per capita)	607.036	92.813	605.049	94.386
Obs.	248,410		209,961	

Note: The samples used in this table correspond to those in columns 1 - 3 in Tables 4 and 5 for the Violent category. The categories of missing values for variables are not reported in the table. The summary statistics for the samples of 1- and 2-year recidivism on other categories are reported in the Appendix.

Table 4. The Impact of the ACA Medicaid Expansion on 1-Year Recidivism

	Violent			Property		
Panel A: All Reoffenders	(1)	(2)	(3)	(4)	(5)	(6)
Expansion*Post	-0.008	-0.008	-0.010	-0.008	-0.008	-0.008
	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
Wild Bootstrap p-value	0.495	0.411	0.263	0.482	0.395	0.533
Mean of Dependent Variable	0.157	0.157	0.157	0.208	0.208	0.208
Adjusted R^2	0.319	0.319	0.320	0.291	0.291	0.291
N	248,410	248,410	248,410	250,032	250,032	250,032
	Drug			**Public Order**		
Expansion*Post	-0.003	-0.003	0.001	-0.011	-0.016*	-0.007
	(0.011)	(0.009)	(0.008)	(0.008)	(0.009)	(0.008)
Wild Bootstrap p-value	0.804	0.786	0.890	0.192	0.156	0.417
Mean of Dependent Variable	0.150	0.150	0.150	0.142	0.142	0.142
Adjusted R^2	0.288	0.288	0.289	0.287	0.287	0.288
N	255,295	255,295	255,295	161,262	161,262	161,262
	Violent			**Property**		
Panel B: Multi-Time Reoffenders	(1)	(2)	(3)	(4)	(5)	(6)
Expansion*Post	-0.010	-0.015**	-0.006**	-0.007	-0.016	-0.000
	(0.009)	(0.007)	(0.003)	(0.015)	(0.012)	(0.007)
Wild Bootstrap p-value	0.359	0.078	0.034	0.725	0.288	0.989
Mean of Dependent Variable	0.040	0.040	0.040	0.063	0.063	0.063
Adjusted R^2	0.135	0.135	0.137	0.130	0.131	0.133
N	248,410	248,410	248,410	250,032	250,032	250,032
	Drug			**Public Order**		
Expansion*Post	-0.001	-0.007	0.001	-0.006	-0.014**	-0.004
	(0.010)	(0.009)	(0.006)	(0.008)	(0.007)	(0.003)
Wild Bootstrap p-value	0.936	0.497	0.895	0.567	0.091	0.238
Mean of Dependent Variable	0.044	0.044	0.044	0.039	0.039	0.039
Adjusted R^2	0.116	0.116	0.118	0.114	0.115	0.116
N	255,295	255,295	255,295	161,262	161,262	161,262
State Fixed Effects	√	√	√	√	√	√
Release-Year Fixed Effects	√	√	√	√	√	√
State-Specific Time Varying Controls	×	√	√	×	√	√
State-Specific Trends	×	×	√	×	×	√

Note: The dependent variables are 1-year recidivism indicators for different first offense types. In all regressions, we control for offender characteristics, release-year fixed effects, and state fixed effects. Standard errors in parentheses are clustered at the state level. p-values obtained from 1,000 wild cluster bootstrap iterations are also reported for the treatment indicator. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5. The Impact of the ACA Medicaid Expansion on 2-Year Recidivism

	Violent			Property		
Panel A: All Reoffenders	(1)	(2)	(3)	(4)	(5)	(6)
Expansion*Post	-0.010	-0.008	-0.016*	-0.014	-0.013	-0.018
	(0.011)	(0.010)	(0.009)	(0.009)	(0.010)	(0.012)
Wild Bootstrap p-value	0.465	0.455	0.156	0.160	0.185	0.254
Mean of Dependent Variable	0.230	0.230	0.230	0.292	0.292	0.29
Adjusted R^2	0.409	0.409	0.410	0.335	0.335	0.336
N	209,961	209,961	209,961	213,726	213,726	213,726
	Drug			**Public Order**		
Expansion*Post	-0.005	-0.004	-0.009	-0.013*	-0.016*	-0.018**
	(0.011)	(0.010)	(0.008)	(0.007)	(0.008)	(0.008)
Wild Bootstrap p-value	0.704	0.697	0.345	0.129	0.096	0.066
Mean of Dependent Variable	0.216	0.216	0.216	0.163	0.163	0.163
Adjusted R^2	0.350	0.350	0.351	0.350	0.350	0.350
N	218,634	218,634	218,634	137,603	137,603	137,603

	Violent			Property		
Panel B: Multi-Time Reoffenders	(1)	(2)	(3)	(4)	(5)	(6)
Expansion*Post	-0.009	-0.015**	-0.009***	0.002	-0.009	-0.001
	(0.010)	(0.006)	(0.003)	(0.016)	(0.011)	(0.006)
Wild Bootstrap p-value	0.473	0.060	0.002	0.933	0.545	0.812
Mean of Dependent Variable	0.058	0.058	0.058	0.089	0.089	0.089
Adjusted R^2	0.170	0.170	0.172	0.155	0.155	0.157
N	209,961	209,961	209,961	213,726	213,726	213,726
	Drug			**Public Order**		
Expansion*Post	0.003	-0.006	-0.004	-0.001	-0.011*	-0.007*
	(0.011)	(0.009)	(0.006)	(0.009)	(0.006)	(0.004)
Wild Bootstrap p-value	0.841	0.575	0.535	0.901	0.146	0.134
Mean of Dependent Variable	0.082	0.082	0.082	0.055	0.055	0.055
Adjusted R^2	0.144	0.144	0.145	0.139	0.140	0.140
N	218,634	218,634	218,634	137,603	137,603	137,603
State Fixed Effects	√	√	√	√	√	√
Release-Year Fixed Effects	√	√	√	√	√	√
State-Specific Time Varying Controls	×	√	√	×	√	√
State-Specific Trends	×	×	√	×	×	√

Note: The dependent variables are 2-year recidivism indicators for different first offense types. In all regressions, we control for offender characteristics, release-year fixed effects, and state fixed effects. Standard errors in parentheses are clustered at the state level. p-values obtained from 1,000 wild cluster bootstrap iterations are also reported for the treatment indicator. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6. Changes in 1-Year Recidivism Decomposed by First Offense and Reoffense

	First Offense Type			
	Violent	**Property**	**Drug**	**Public Order**
First Reoffense Type	(1)	(2)	(3)	(4)
Violent	-0.006*	0.000	-0.000	0.001
	(0.003)	(0.001)	(0.001)	(0.001)
Wild Bootstrap p-value	0.046	0.634	0.863	0.311
Mean of Dependent Variable	0.034	0.002	0.001	0.002
Adjusted R^2	0.137	0.068	0.063	0.081
Property	0.000	-0.002	0.001	0.001
	(0.001)	(0.007)	(0.001)	(0.001)
Wild Bootstrap p-value	0.964	0.759	0.184	0.229
Mean of Dependent Variable	0.002	0.055	0.003	0.003
Adjusted R^2	0.090	0.133	0.115	0.126
Drug	-0.000	0.001	0.000	0.000
	(0.000)	(0.001)	(0.006)	(0.001)
Wild Bootstrap p-value	0.484	0.676	0.955	0.798
Mean of Dependent Variable	0.001	0.003	0.038	0.002
Adjusted R^2	0.072	0.109	0.116	0.093
Public Order	0.000	0.001	0.000	-0.006*
	(0.001)	(0.001)	(0.000)	(0.003)
Wild Bootstrap p-value	0.637	0.439	0.939	0.059
Mean of Dependent Variable	0.002	0.002	0.001	0.032
Adjusted R^2	0.089	0.102	0.086	0.111
State Fixed Effects	✓	✓	✓	✓
Release-Year Fixed Effects	✓	✓	✓	✓
State-Specific Time Varying Controls	✓	✓	✓	✓
State-Specific Trends	✓	✓	✓	✓
N	248,410	250,032	255,295	161,262

Note: This table reports the estimated treatment effect of the ACA Medicaid Expansions on different groups of multi-time reoffenders. The dependent variables are 1-year recidivism indicators by first offense and reoffense types. In all regressions, we control for a full set of covariates, including offender characteristics and state time-varying variables (the minimum wage, the housing price index, the poverty rate, and the unemployment rate). Standard errors in parentheses are clustered at the state level. p-values obtained from 1,000 wild cluster bootstrap iterations are also reported for the treatment indicator. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7. Changes in 2-Year Recidivism Decomposed by First Offense and Reoffense

	First Offense Type			
	Violent	**Property**	**Drug**	**Public Order**
First Reoffense Type	(1)	(2)	(3)	(4)
Violent	-0.007***	0.000	0.000	0.001
	(0.002)	(0.001)	(0.001)	(0.001)
Wild Bootstrap p-value	0.004	0.742	0.897	0.224
Mean of Dependent Variable	0.047	0.003	0.002	0.003
Adjusted R^2	0.170	0.109	0.100	0.115
Property	0.000	-0.002	0.001	0.001
	(0.001)	(0.007)	(0.001)	(0.001)
Wild Bootstrap p-value	0.563	0.650	0.148	0.533
Mean of Dependent Variable	0.004	0.075	0.005	0.005
Adjusted R^2	0.149	0.156	0.180	0.194
Drug	-0.001	-0.000	-0.005	-0.001
	(0.001)	(0.001)	(0.006)	(0.001)
Wild Bootstrap p-value	0.335	0.962	0.485	0.545
Mean of Dependent Variable	0.002	0.006	0.053	0.004
Adjusted R^2	0.125	0.175	0.141	0.149
Public Order	0.000	0.001	-0.000	-0.008**
	(0.001)	(0.001)	(0.001)	(0.003)
Wild Bootstrap p-value	0.968	0.404	0.388	0.028
Mean of Dependent Variable	0.004	0.004	0.003	0.043
Adjusted R^2	0.134	0.145	0.132	0.128
State Fixed Effects	✓	✓	✓	✓
Release-Year Fixed Effects	✓	✓	✓	✓
State-Specific Time Varying Controls	✓	✓	✓	✓
State-Specific Trends	✓	✓	✓	✓
N	209,961	213,726	218,634	137,603

Note: This table reports the estimated treatment effect of the ACA Medicaid Expansions on different groups of multi-time reoffenders. The dependent variables are 2-year recidivism indicators by first offense and reoffense types. In all regressions, we control for a full set of covariates, including offender characteristics and state time-varying variables (the minimum wage, the housing price index, the poverty rate, and the unemployment rate). Standard errors in parentheses are clustered at the state level. p-values obtained from 1,000 wild cluster bootstrap iterations are also reported for the treatment indicator. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8. Robustness Checks: Alternative Specifications

	1-Year Recidivism				2-Year Recidivism			
	Violent	Property	Drug	Public Order	Violent	Property	Drug	Public Order
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Baseline Results (for comparison)								
Expansion*Post	-0.006**	0.000	0.001	-0.004	-0.009***	-0.001	-0.004	-0.007*
	(0.003)	(0.007)	(0.006)	(0.004)	(0.003)	(0.006)	(0.006)	(0.004)
Wild Bootstrap p	0.036	0.989	0.895	0.238	0.002	0.812	0.535	0.134
Mean of Dependent Variable	0.040	0.063	0.044	0.039	0.058	0.089	0.082	0.055
Adjusted R^2	0.137	0.133	0.118	0.116	0.172	0.157	0.145	0.140
N	248,410	250,032	255,295	161,262	209,961	213,726	218,634	137,603
Panel B: Including States with Missing Data								
Expansion*Post	-0.006**	-0.000	0.001	-0.003	-0.009***	-0.002	-0.004	-0.005
	(0.003)	(0.007)	(0.006)	(0.004)	(0.003)	(0.006)	(0.006)	(0.004)
Wild Bootstrap p	0.052	0.983	0.906	0.438	0.005	0.819	0.600	0.257
Mean of Dependent Variable	0.039	0.062	0.043	0.039	0.056	0.087	0.061	0.054
Adjusted R^2	0.138	0.134	0.119	0.118	0.173	0.158	0.147	0.141
N	267,110	263,201	270,483	168,427	227,169	225,582	232,407	144,246
Panel C: Controlling for Justice Measures								
Expansion*Post	-0.006*	0.000	0.005	-0.001	-0.008**	-0.004	-0.002	-0.004
	(0.003)	(0.007)	(0.006)	(0.004)	(0.003)	(0.006)	(0.006)	(0.004)
Wild Bootstrap p	0.070	0.966	0.477	0.800	0.083	0.564	0.746	0.341
Mean of Dependent Variable	0.040	0.063	0.044	0.039	0.058	0.089	0.082	0.055
Adjusted R^2	0.137	0.133	0.118	0.116	0.172	0.157	0.145	0.140
N	248,410	250,032	255,295	161,262	209,961	213,726	218,634	137,603

Note: The dependent variables are 1-year and 2-year recidivism indicators for different first offense types. In all regressions, we control for a full set of covariates, including offender characteristics and state time-varying variables (the minimum wage, the housing price index, the poverty rate, and the unemployment rate), as well as state fixed effects, release-year fixed effects, and state-specific time trends. Justice measures in Panel C include the share of Democrats in the Congress, total justice expenditure (per capita), and an indicator for marijuana legalization. Standard errors in parentheses are clustered at the state level. p-values obtained from 1,000 wild cluster bootstrap iterations are also reported for the treatment indicator. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 9. The Impact of the ACA Medicaid Expansion on Substance Use Disorder Treatment

	(1)	(2)	(3)
Panel A: By Payment Source:	Self-pay	Private Insurance	Medicaid
Expansion*Post	-0.078	-0.100	1.086**
	(0.179)	(0.154)	(0.510)
N	274	274	274
	(1)	(2)	(3)
Panel B: By Referral Source:	Self-Referral	Criminal Justice Referral	Criminal Justice Referral
(Conditional on Medicaid)		(All)	(Prison/Probation/Parole)
Expansion*Post	1.183**	1.224***	1.736***
	(0.576)	(0.382)	(0.475)
N	274	274	274

Note: The dependent variable is the count of annual admissions to SUD treatment at the state level. The reported sources for criminal justice referrals include state or federal courts, formal adjudication process, probation or parole, other legal entities, diversionary programs, prisons, and court referrals due to DUI or DWI. In all regressions, we control for state time-varying effects (the minimum wage, the housing price index, the poverty rate, and the unemployment rate), as well as state fixed effects and admission-year fixed effects. Standard errors in parentheses are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10. Cost-Benefit Analysis: Multi-Time Recidivists with Previous Violent Offenses

	(1) 1-Year Recidivism	(2) 2-Year Recidivism
Costs:		
Increase in Medicaid Expenditure:		
Average annual cost of Medicaid per adult	\$5,562	\$5,562
× Number of inmates needed to be covered	239	182
1-Year Total Costs:	\$1,329,318	\$1,012,284
2-Year Total Costs:	\$2,658,636	\$2,024,568
3-Year Total Costs:	\$3,987,954	\$3,036,852
4-Year Total Costs:	\$5,317,272	\$4,049,136
Benefits (Cost Reduction):		
Economic & Social Costs of Victimization per Crime:		
(Tangible costs per crime	\$14,055	\$14,055
+ Intangible costs per crime)	\$77,055	\$77,055
× Share of violent crimes	0.85	0.85
× Twice the inverse probability of punishment	15.26	15.26
Subtotal:	\$1,181,788	\$1,181,788
Fiscal Costs of Incarceration:		
Daily incarceration cost per inmate:	\$91.16	\$91.16
× Average time served in prison (years)	4	4
× Number of days incarcerated / Year	365	365
Subtotal:	\$133,094	\$133,094
Economic Costs of Incarceration:		
One-time prison penalty	\$16,000	\$16,000
Duration penalty per year	\$10,000	\$10,000
× Average time served in prison (years)	4	4
Subtotal:	\$56,000	\$56,000
Total Benefits:	\$1,370,882	\$1,370,882
Benefits / 1-Year Costs :	103.13%	135.42%
Benefits / 2-Year Costs :	51.56%	67.71%
Benefits / 3-Year Costs :	34.38%	45.14%
Benefits / 4-Year Costs :	25.78%	33.86%

Note: Medicaid spending data are from the Centers for Medicare & Medicaid Services (see https://go.cms.gov/3aJyqCH). The tangible and intangible costs of victimization are from Miller et al. (2020) measuring the costs per violent crime. Tangible costs include medical costs, lost productivity, property loss, and the use of public services, among others. Intangible costs are estimated monetary costs related to pain, suffering, and loss of life quality. We obtain the probability of punishment for violent crimes from Shavell (1993). This probability has not changed much over time based on our comparison with recent data from the UCR and BJS on clearance rates and the probability of reporting. The fiscal cost of incarceration per inmate is calculated based on the 2015 report on state prison cost per inmate by the Vera Institute of Justice (see https://bit.ly/3pFVbMd). The one-time and duration penalty of incarceration are from Arenberg, Neller, and Stripling (2020). Using results from Mueller-Smith (2015), they fit a linear line for the relationship between the length of time served in prison and economic costs. The one-time prison penalty is the intercept of the fitted line, whereas the duration penalty per year is the slope.

Appendix

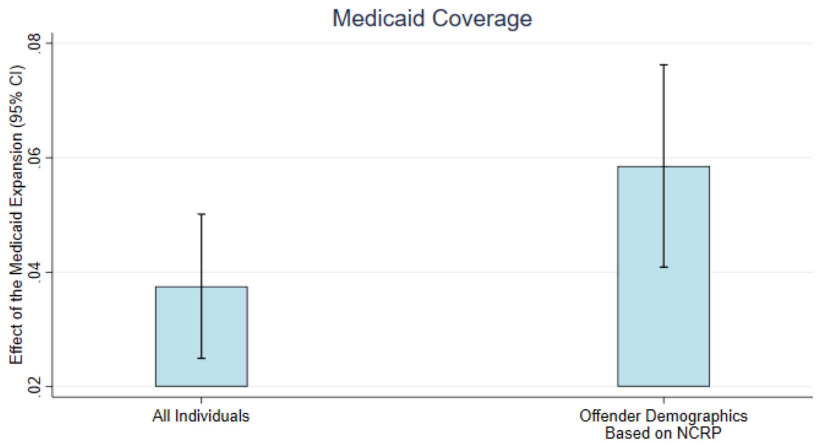

Figure A1. First-Stage Estimates

Note: The figure reports the effects of the ACA expansions on Medicaid take-up. To obtain the estimates for the first-stage, we use the classifications for treatment and control groups in our main analysis. The left bar includes all individuals aged 19 to 64. The right bar stratifies the sample by the most frequently observed demographics for offenders in our descriptive statistics from the NCRP data after adjusting by ethnicity-specific population. The sample in the right panel includes African-American or Hispanic males aged 24 to 55 with high school diploma or below. We report the 95% confidence intervals in the figure.

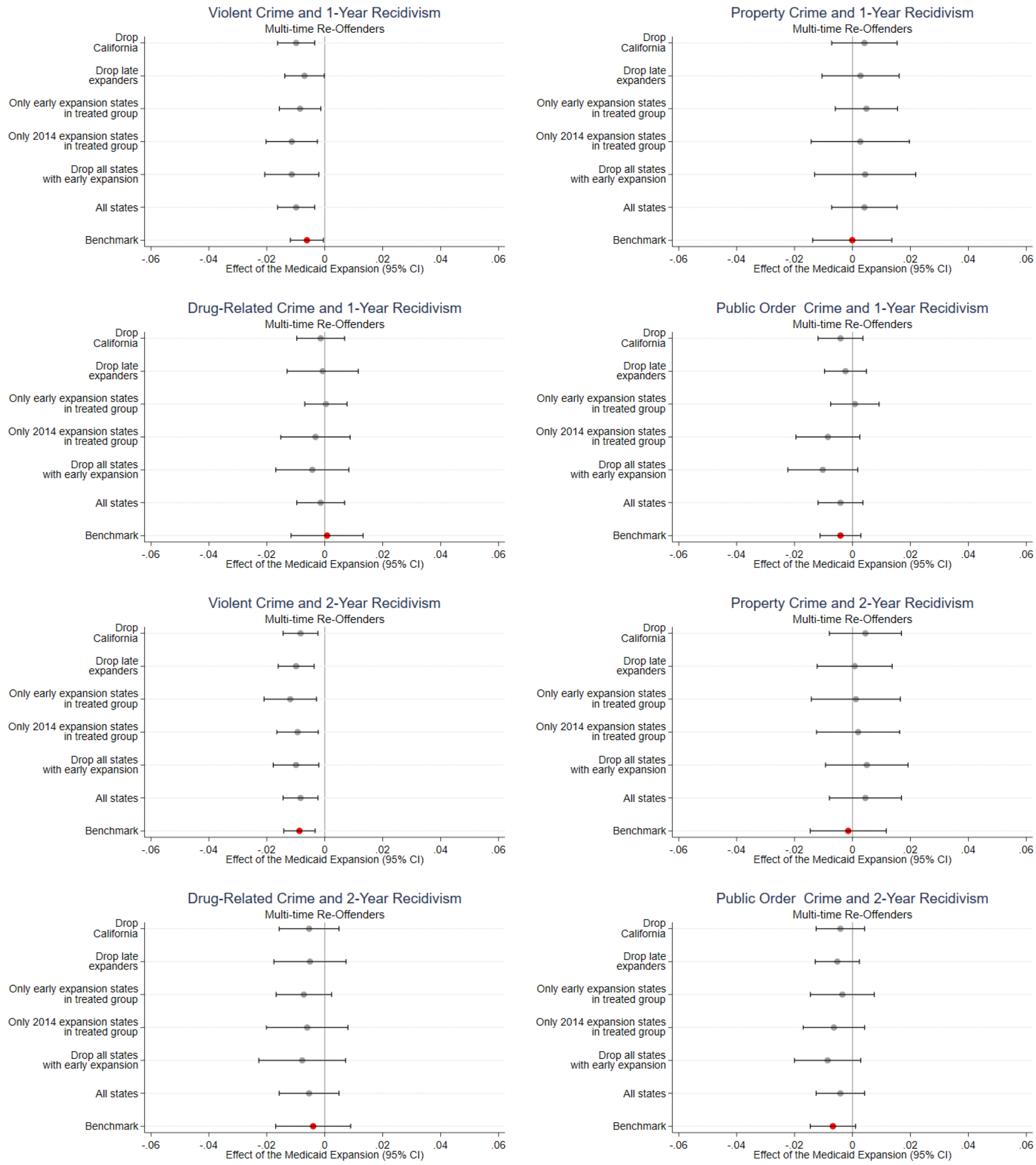

Figure A2. Different Classifications of Treatment and Control Groups

Note: The figure shows the sensitivity of the recidivism estimates to different classifications provided in Courtemanche et al. (2017). The benchmark estimate shown in red is obtained by using the classifications for treatment and control groups in our main analysis. The figure contains results for 1- and 2-year recidivism among multi-time reoffenders by first offense types. We report the 95% confidence intervals in the figure.

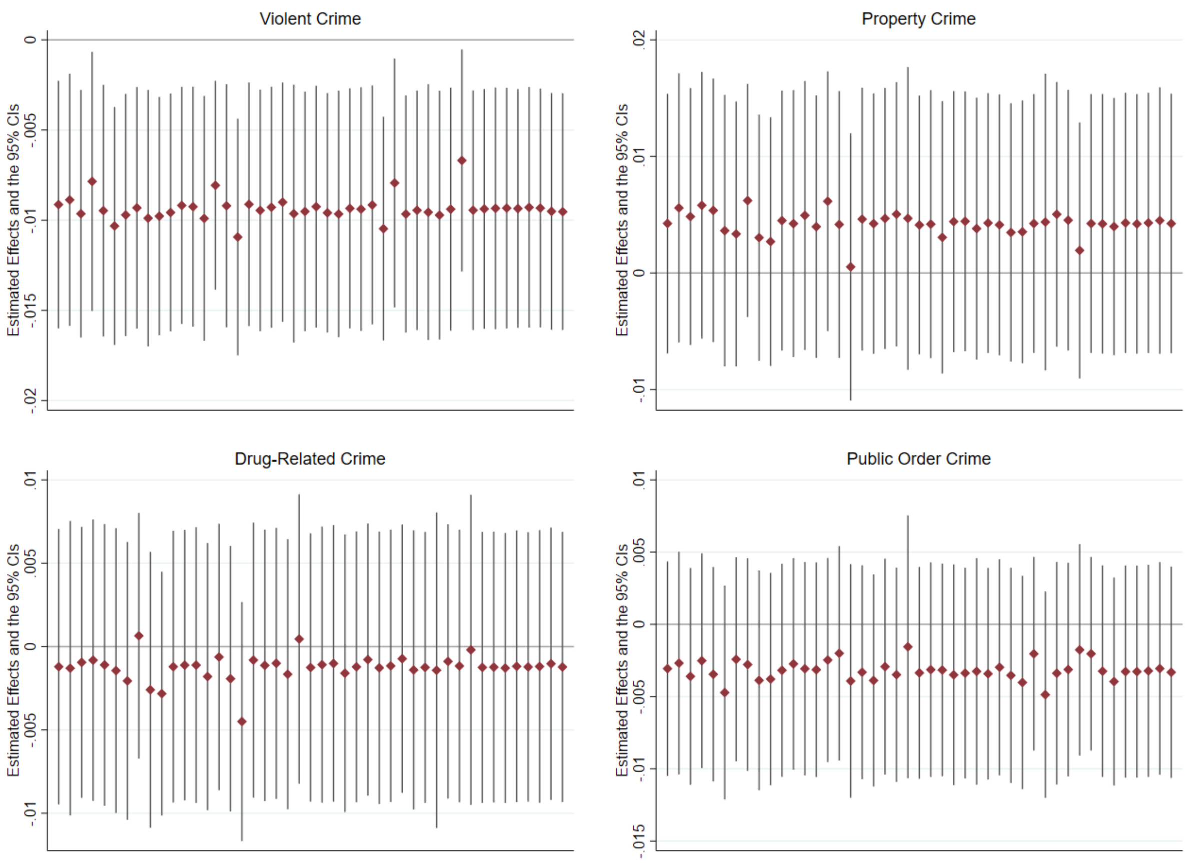

Figure A3. Alternative Specifications: Leave-One-Out Method

Note: The figure reports the coefficients and 95% confidence intervals resulting from dropping out data from one specific state at a time. The figures contain results for 1-year recidivism among multi-time reoffenders by first offense types.

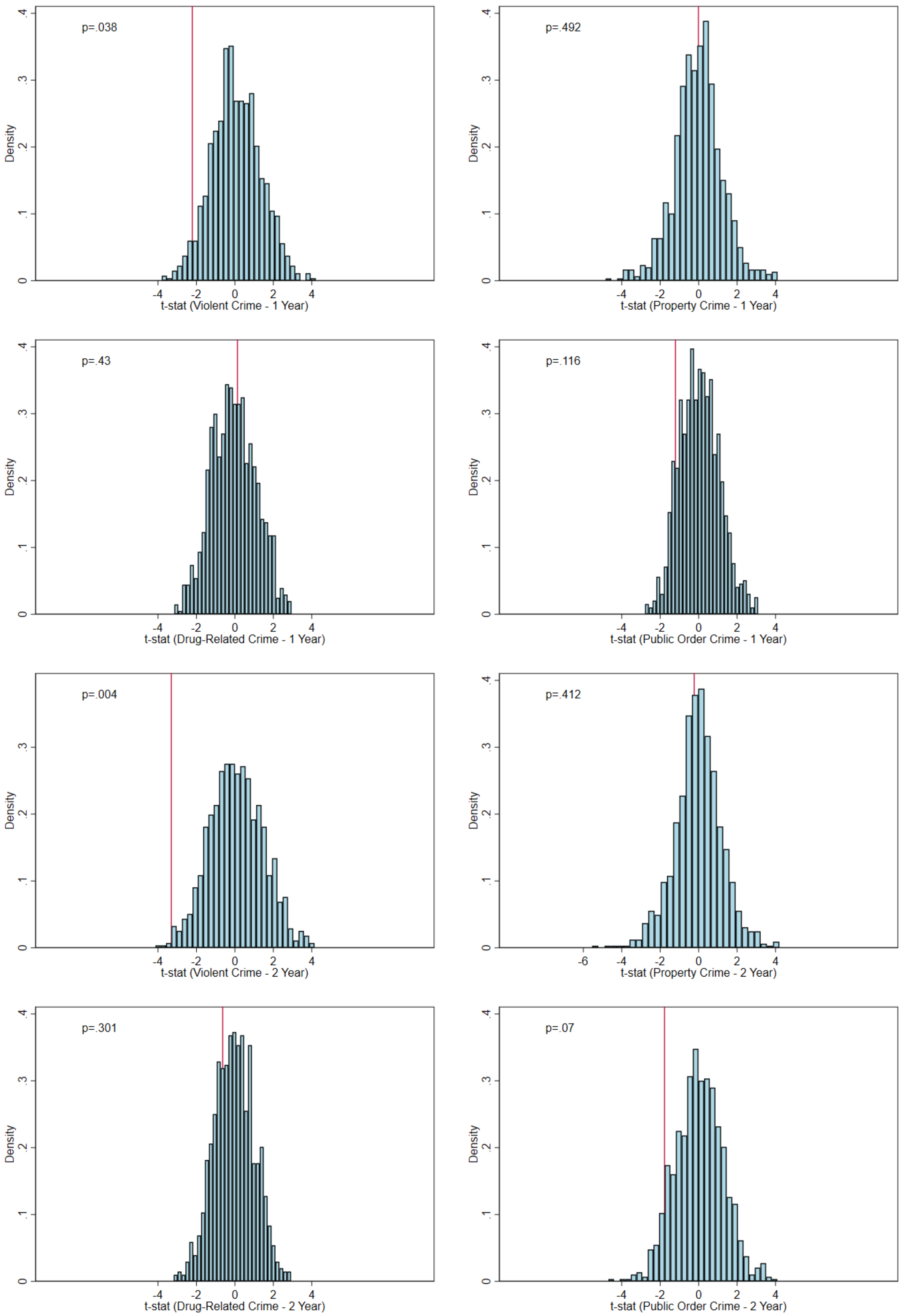

Figure A4. Permutation Tests: Randomly Assigned Expansion (Treatment) Status

Note: The figure reports the distribution of t-statistics resulting from 1,000 replications of randomly assigning treatment status among states in the working sample. The figure contains results for 1- and 2-year recidivism among multi-time reoffenders by first offense types. The vertical line depicts the t-statistic of the benchmark estimate reported in Panel B of Tables 4 and 5.

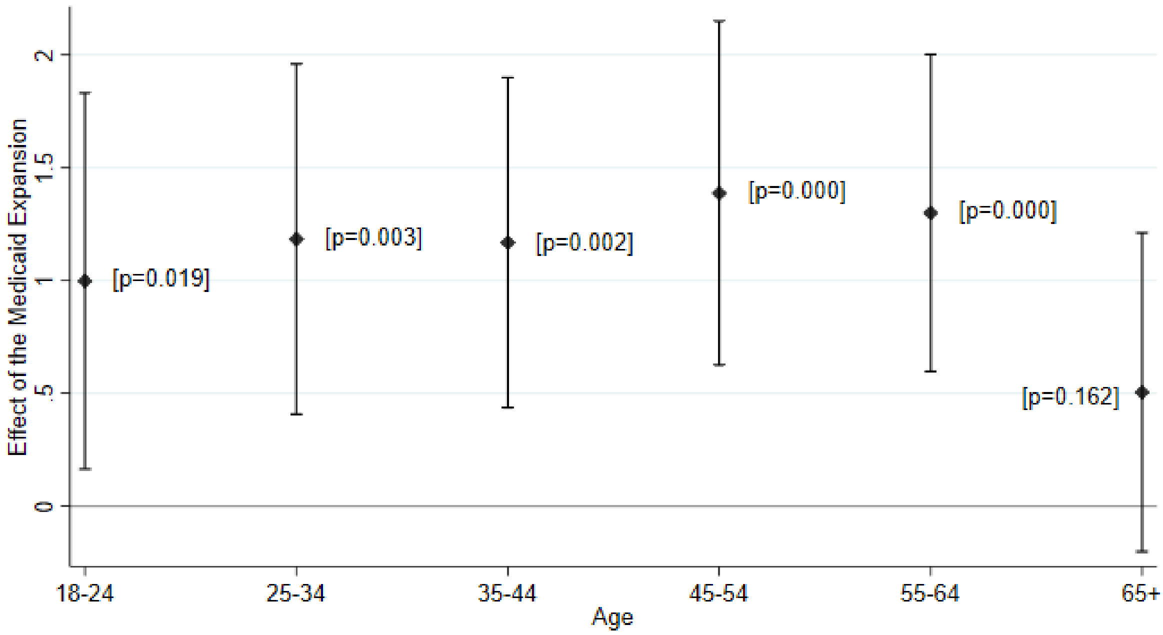

Figure A5. The Effect of the ACA Medicaid Expansion on Substance Use Disorder Treatment by Age Group (Conditional on Criminal Justice Referrals and Paying through Medicaid)

Note: The figure reports the estimated heterogeneous effects of the ACA Medicaid expansion on the annual total number of SUD treatment admissions for different age groups. The coefficients and 95% confidence intervals are depicted in the figure. We also report the *p*-values of the estimates in brackets.

Table A1. Summary of Related Literature

Study	Data, N	Identification strategy and specification	Effects of the Shock	Heterogeneity in mechanisms/effects
Panel A: Labor Market Conditions and Recidivism				
Galbiati, Ouss, and Philippe (2015)	Prison records data by French Department of Prison Administration, N=99,151, Feb-2009 through July-2010. Job vacancies data by French governmental agency for unemployment, 2009 and 2010. News and jobs posting data by Observatoire de l'Investissement, Jan-2009 through Dec-2010.	Effect of local labor market conditions and job information on recidivism using a linear regression model.	Job creation has no influence over recidivism. Media coverage of job creation reduce recidivism.	Job creation in manufacturing reduce risk of recidivism. Formal labor market opportunities reduce offending. Media coverage has salient effects on inmates with weak ties to legal labor market before incarceration.
Schnepel (2017)	NCRP 1993-2008, N=1,714,614. Working age males in California released to parole supervision. Quarterly Workforce Indicator for labor market data.	Log-linear model measuring the impact of labor demand on recidivism. The model includes fixed effect for year-by-quarter of release and county of sentencing, and a county-specific linear time trend.	Increases in employment opportunities affect recidivism negatively.	Significant effects on industries include construction and manufacturing.
Yang (2017b)	NCRP 2000-2013, N=4,029,781; Quarterly Workforce Indicators for labor market data.	Proportional hazard model; hazard rate for returning to prison in quarters with varying labor market conditions.	Ex-offenders are responsive to conditions in the labor market (as measured by low-skilled earnings). Offenders released in markets with higher wages are less likely to recidivate.	Black offenders are more likely to recidivate than similar white offenders. Hispanics and females are less likely to recidivate. Recidivism decreases with educational attainment. Older offenders with no prior felony incarceration and those who have served more time for the current offense are less likely to recidivate.

(Continued)

Study	Data, N	Identification strategy and specification	Effects of the Shock	Heterogeneity in mechanisms/effects
Panel B: Welfare Programs and Recidivism				
Yang (2017a)	NCRP 1971-2014, N=4,885,754. Federal and state level changes in law for the sample period covered by the NCRP.	Exploiting the timing of the federal public assistance ban under Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) in 1996, and the timing of state laws opted out of the federal ban. The ban applied exclusively to ex-offenders with drug felony convictions, allowing for a triple-differences approach.	Eligibility to public assistance (food stamps and welfare) reduce risk of recidivism.	Salient for newly released drug offenders.
Agan and Makowsky (2018)	NCRP 2000-2014. N= 5.8 million (5,786,062) prison releases from 4 million unique offenders in 43 states (1-year recidivism), and 4.8 million releases from 3 million individuals (3-year recidivism)	DD; the changes in the minimum wage and earned income tax credit (EITC) top-ups enactment that vary by state and year-month.	Higher minimum wages decrease recidivism.	EITC wage subsidies reduce recidivism for women.
Tuttle (2019)	Offender Based Information System - Florida Department of Corrections, October 1, 1995–October 1, 1997. SNAP Quality Control, 1996-2014. N=918.	RD; the effect of food stamp ban on recidivism using August 23, 1996 as the cutoff date.	The ban increases recidivism among drug traffickers.	Increase is driven by financially motivated crimes (lost transfer income).

(Continued)

Study	Data, N	Identification strategy and specification	Effects of the Shock	Heterogeneity in mechanisms/effects
Panel C: Health Insurance and Crime				
Wen, Hockenberry, and Cummings (2017)	UCR, county level, 2001-2008. National Survey of Substance Abuse Treatment Services. N=22,328.	DD; the effect of HIFA-waiver expansion on crime rates; exploring substance use disorder treatment as a mechanism.	Increases in SUD treatment through insurance coverage expansion reduce crime.	Significant effects for robberies, aggravated assaults, and larceny theft.
Vogler (2020)	UCR, state (N=306) and county (N=18,146) level, 2010-2015.	DD; the effect of medicaid expansions on crime rates.	Medicaid expansions have resulted in significant decreases in both reported violent and property crime.	Effects are strongest in counties with higher pre-expansion uninsured levels.
He and Barkowski (2020)	UCR, state (2010-2016, N=357) and county (2010-2014 & 2016, N=3,246) level.	DD; the effect of medicaid expansions on crime rates.	The ACA's Medicaid expansion has negative effect on crime.	Significant effects for burglary, motor vehicle theft, criminal homicide, robbery, and aggravated assault.

Table A2 (a). Summary Statistics - Property Crime Samples

	1-Year Recidivism		2-Year Recidivism	
Variables	Mean	Std. Dev.	Mean	Std. Dev.
Age When Released				
25-34 years	0.485	0.500	0.485	0.500
35-44 years	0.254	0.435	0.256	0.437
45-54 years	0.155	0.362	0.158	0.365
Gender				
Female	0.218	0.413	0.214	0.410
Race/Ethnicity				
White	0.545	0.498	0.536	0.499
Black	0.233	0.423	0.236	0.425
Hispanic	0.120	0.325	0.122	0.327
Other Races	0.015	0.121	0.015	0.121
Education				
<High School Diploma / GED	0.268	0.443	0.272	0.445
High School Diploma / GED	0.316	0.465	0.320	0.466
Any College	0.070	0.255	0.071	0.257
Time Served				
<1 year	0.539	0.498	0.542	0.498
1-1.9 years	0.194	0.395	0.196	0.397
2-4.9 years	0.126	0.332	0.127	0.333
5-9.9 years	0.026	0.159	0.025	0.156
>=10 years	0.009	0.096	0.009	0.095
Sentence Length				
<1 year	0.230	0.421	0.233	0.423
1-1.9 years	0.096	0.295	0.093	0.291
2-4.9 years	0.335	0.472	0.333	0.471
5-9.9 years	0.203	0.402	0.202	0.402
10-24.9 years	0.113	0.317	0.115	0.319
>=25 years	0.015	0.120	0.016	0.124
Life, LWOP	0.001	0.038	0.002	0.040
Admission Type				
Court Commitment	0.812	0.391	0.811	0.392
Return from Parole / Revocation	0.165	0.371	0.165	0.371
Other	0.008	0.090	0.008	0.092
Release Type				
Conditional Release	0.456	0.498	0.458	0.498
Unconditional Release	0.379	0.485	0.382	0.486
Other Types of Release	0.006	0.074	0.006	0.078
Minimum Wage	7.477	0.393	7.443	0.359
Housing Price Index	274.696	50.977	269.923	48.873
Unemployment Rate	7.626	1.938	8.032	1.778
Poverty Rate	15.945	2.613	16.177	2.541
Number of Police (per 10 thousand population)	21.190	2.703	21.225	2.713
Share of Democrats in the Congress	0.414	0.105	0.421	0.105
Marijuana Legalization	0.029	0.166	0.025	0.156
Justice System Expenditure (per capita)	594.272	94.493	592.126	95.546
N	250,032		213,726	

Note: The samples used in this table correspond to those in Tables 4 and 5 for the Property category. The categories of missing values for variables are not reported in the table.

Table A2 (b). Summary Statistics - Drug-Related Crime Samples

	1-Year Recidivism		2-Year Recidivism	
Variables	Mean	Std. Dev.	Mean	Std. Dev.
Age When Released				
25-34 years	0.486	0.500	0.488	0.500
35-44 years	0.286	0.452	0.286	0.452
45-54 years	0.161	0.368	0.163	0.369
Gender				
Female	0.208	0.406	0.202	0.402
Race/Ethnicity				
White	0.432	0.495	0.420	0.494
Black	0.302	0.459	0.310	0.462
Hispanic	0.164	0.371	0.165	0.371
Other Races	0.013	0.112	0.013	0.112
Education				
<High School Diploma / GED	0.281	0.450	0.286	0.452
High School Diploma / GED	0.322	0.467	0.324	0.468
Any College	0.062	0.242	0.063	0.243
Time Served				
<1 year	0.539	0.498	0.539	0.498
1-1.9 years	0.199	0.399	0.202	0.401
2-4.9 years	0.154	0.361	0.156	0.363
5-9.9 years	0.034	0.181	0.033	0.179
>=10 years	0.007	0.083	0.007	0.080
Sentence Length				
<1 year	0.225	0.417	0.224	0.417
1-1.9 years	0.075	0.264	0.074	0.261
2-4.9 years	0.312	0.463	0.310	0.462
5-9.9 years	0.231	0.421	0.232	0.422
10-24.9 years	0.134	0.341	0.137	0.344
>=25 years	0.016	0.125	0.017	0.128
Life, LWOP	0.001	0.036	0.001	0.036
Admission Type				
Court Commitment	0.819	0.385	0.816	0.387
Return from Parole / Revocation	0.156	0.363	0.156	0.363
Other	0.008	0.089	0.008	0.089
Release Type				
Conditional Release	0.541	0.498	0.544	0.498
Unconditional Release	0.332	0.471	0.333	0.471
Other Types of Release	0.005	0.072	0.006	0.075
Minimum Wage	7.449	0.372	7.419	0.341
Housing Price Index	274.022	53.076	270.094	52.298
Unemployment Rate	7.595	1.906	8.001	1.733
Poverty Rate	15.981	2.699	16.211	2.639
Number of Police (per 10 thousand population)	21.324	2.824	21.366	2.847
Share of Democrats in the Congress	0.415	0.104	0.422	0.103
Marijuana Legalization	0.020	0.140	0.018	0.134
Justice System Expenditure (per capita)	586.438	91.498	584.377	92.292
N	255,295		218,634	

Note: The samples used in this table correspond to those in Tables 4 and 5 for the Drug category. The categories of missing values for variables are not reported in the table.

Table A2 (c). Summary Statistics - Public Order Crime Samples

	1-Year Recidivism		2-Year Recidivism	
Variables	Mean	Std. Dev.	Mean	Std. Dev.
Age When Released				
25-34 years	0.410	0.492	0.408	0.491
35-44 years	0.291	0.454	0.293	0.455
45-54 years	0.225	0.417	0.228	0.419
Gender				
Female	0.124	0.329	0.121	0.326
Race/Ethnicity				
White	0.448	0.497	0.445	0.497
Black	0.251	0.434	0.249	0.432
Hispanic	0.206	0.405	0.208	0.406
Other Races	0.025	0.157	0.025	0.157
Education				
<High School Diploma / GED	0.280	0.449	0.284	0.451
High School Diploma / GED	0.346	0.476	0.348	0.476
Any College	0.081	0.272	0.081	0.273
Time Served				
<1 year	0.565	0.496	0.569	0.495
1-1.9 years	0.197	0.397	0.198	0.398
2-4.9 years	0.125	0.331	0.125	0.331
5-9.9 years	0.030	0.172	0.029	0.168
>=10 years	0.008	0.092	0.008	0.088
Sentence Length				
<1 year	0.264	0.441	0.267	0.442
1-1.9 years	0.081	0.273	0.081	0.273
2-4.9 years	0.391	0.488	0.390	0.488
5-9.9 years	0.177	0.381	0.176	0.381
10-24.9 years	0.068	0.252	0.068	0.252
>=25 years	0.008	0.087	0.008	0.089
Life, LWOP	0.001	0.033	0.001	0.033
Admission Type				
Court Commitment	0.837	0.369	0.837	0.369
Return from Parole / Revocation	0.144	0.351	0.143	0.350
Other	0.003	0.055	0.003	0.055
Release Type				
Conditional Release	0.530	0.499	0.533	0.499
Unconditional Release	0.340	0.474	0.344	0.475
Other Types of Release	0.002	0.042	0.002	0.044
Minimum Wage	7.483	0.400	7.449	0.366
Housing Price Index	275.004	54.759	270.504	53.417
Unemployment Rate	7.507	1.951	7.920	1.789
Poverty Rate	15.641	2.644	15.892	2.590
Number of Police (per 10 thousand population)	21.316	2.879	21.364	2.928
Share of Democrats in the Congress	0.421	0.109	0.427	0.108
Marijuana Legalization	0.025	0.156	0.022	0.147
Justice System Expenditure (per capita)	599.463	85.955	596.987	86.744
N	161,262		137,603	

Note: The samples used in this table correspond to those in Tables 4 and 5 for the Public Order category. The categories of missing values for variables are not reported in the table.

Table A3. The Impact of the ACA Medicaid Expansion on Recidivism – One-Time Reoffenders

	(1) Violent	(2) Property	(3) Drugs	(4) Public Order
Panel A: 1-Year Recidivism				
Expansion*Post	-0.004	-0.006	0.000	-0.003
	(0.008)	(0.013)	(0.011)	(0.009)
Wild Bootstrap p	0.629	0.781	0.963	0.699
Mean of Dependent Variable	0.117	0.145	0.106	0.103
Adjusted R^2	0.246	0.251	0.229	0.207
N	243,390	213,157	244,457	182,146
Panel B: 2-Year Recidivism				
Expansion*Post	-0.007	-0.017	-0.005	-0.011
	(0.009)	(0.014)	(0.012)	(0.009)
Wild Bootstrap p	0.438	0.494	0.787	0.281
Mean of Dependent Variable	0.172	0.204	0.154	0.147
Adjusted R^2	0.312	0.247	0.243	0.253
N	209,961	213,726	218,634	137,603

Note: The dependent variables are 1- and 2-year recidivism indicators by first offense type. In all regressions, we control for offender characteristics and state time-varying variables (the minimum wage, the housing price index, poverty rate, and the unemployment rate), as well as state fixed effects, release-year fixed effects, and state-specific time trends. The mean of the dependent variables and the adjusted R^2 are reported in the table. Standard errors in parentheses are clustered at the state level. p-values obtained from 1,000 wild cluster bootstrap iterations are also reported for the treatment indicator. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A4. The Impact of the ACA Medicaid Expansion on 1- and 2-Year Recidivism: Including Early and Late Expansion States

	1-Year Recidivism				2-Year Recidivism			
	Violent	Property	Drug	Public Order	Violent	Property	Drug	Public Order
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All Reoffenders								
Expansion*Post	-0.006	-0.003	0.006	-0.004	-0.010	-0.007	-0.005	-0.012
	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)	(0.011)	(0.008)	(0.008)
Wild Bootstrap p	0.396	0.713	0.514	0.581	0.301	0.601	0.533	0.182
Mean of Dependent Variable	0.173	0.221	0.160	0.162	0.247	0.305	0.227	0.224
Adjusted R^2	0.351	0.315	0.314	0.307	0.446	0.361	0.375	0.370
N	310,173	292,778	305,777	205,643	261,990	249,611	261,867	175,603
Panel B: Multi-Time Reoffenders								
Expansion*Post	-0.009***	0.005	-0.001	-0.003	-0.007**	0.005	-0.005	-0.002
	(0.003)	(0.006)	(0.004)	(0.003)	(0.003)	(0.006)	(0.005)	(0.004)
Wild Bootstrap p	0.016	0.442	0.828	0.454	0.080	0.475	0.396	0.597
Mean of Dependent Variable	0.051	0.072	0.050	0.052	0.071	0.099	0.070	0.070
Adjusted R^2	0.170	0.162	0.142	0.145	0.207	0.182	0.166	0.168
N	310,173	292,778	305,777	205,643	261,990	249,611	261,867	175,603
State Fixed Effects	√	√	√	√	√	√	√	√
Release-Year Fixed Effects	√	√	√	√	√	√	√	√
State-Specific Time Varying Controls	√	√	√	√	√	√	√	√
State-Specific Trends	√	√	√	√	√	√	√	√

Note: The dependent variables are 1- and 2-year recidivism indicators for different first offense types. The mean of the dependent variables and the adjusted R^2 are reported in the table. Standard errors in parentheses are clustered at the state level. p-values obtained from 1,000 wild cluster bootstrap iterations are also reported for the treatment indicator. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Evidence using the Restricted NCRP Data

In addition to the main analyses based on the selected version of the NCRP data, we provide supporting evidence employing the restricted NCRP data. To make the results obtained from using the restricted NCRP data comparable to those in the main analyses, we select the working sample following the restrictions discussed in the data section (Section IV). The working sample used in this section, however, is still different from that used in the main analyses due to the differences between the selected and restricted NCRP data. The major differences between the two working samples are as follows.

First, in the selected NCRP data, variables such as educational level and age are constructed into categories, while these variables contain continuous values in the restricted NCRP data. For instance, in the selected NCRP data, all inmates' age at release are grouped into 10-year age categories. In the restricted NCRP data, offenders' age at release can be precisely calculated.

Second, as of this study, the restricted NCRP data only span the time period up to 2015. In order to estimate the effect of the ACA expansion on 1-year recidivism, we have to employ a 1-year window to identify whether an inmate returned to prison or not. This is particularly important for the inmates in the control group: would recidivism rates have been different had they given one year? Therefore, we drop individuals who were released in 2015 so that a 1-year window is available for all inmates released in the post-ACA period.[64] Note that it is not possible to construct a 2-year window in the restricted NCRP because we do not observe inmates released in 2014 up to 2016. In the selected NCRP data, however, we only drop inmates who were released in 2016, allowing us to estimate the policy effect in a 1-year window as well as a 2-year window for those

[64] Also, because of the fact that we only have one treated year in the restricted NCRP data, controlling for state-specific time trends will capture almost all variations in the post-treatment period in the outcomes. As an alternative, we replace state-specific time trends with another time-variant variable at the state level. Specifically, we control for the rate of Medicaid beneficiaries (collected from the UKCPR National Welfare Data) that gauges state-level Medicaid take-up rates over time. This can be particularly important in the restricted NCRP sample because we only have the first year (2014) of the Medicaid expansions as the only post-treatment period in the sample. Consequently, because of potential lags in the increase in Medicaid take-up rates after the expansions, we expect to see stronger effects later than 2014 (which has been confirmed by the event studies in Figure 1). Therefore, it might be important to account for the actual rate of Medicaid beneficiaries in this analysis.

released in 2014.

Third, as a conservative approach, to identify the potential treatment status of the inmates, we restrict the sample in the restricted NCRP data to inmates whose state of conviction is the same as the state of incarceration. Ideally, information on inmates' last state of residence should be used to more precisely identify the treatment status because inmates are most likely go back to their last state of residence, which is the state to receive benefits from safety net programs such as Medicaid. Yet, there is a large number of missing values in the variable which records inmates' last known state of residence. Based on the restricted NCRP data, there is a significant overlap (over 93%) between the state of conviction and the state of last known state of residence. Therefore, it is plausible to employ the state of conviction as a proxy for inmates' last state of residence. As described earlier in the data section, the selected NCRP only provides information on inmates' state of conviction. We also use the state of conviction as a proxy for the last state of residence of the inmates.

Fourth, in the main analyses, we restrict the data to states who report information of inmates to NCRP in all the years within our sample period. We implement the same restriction using the restricted NCRP data. Such states in both datasets, however, do not perfectly match. In other words, the selected and restricted working samples contain data from different states, although the difference is minor.

As a result of more restrictions and the shorter time-span covered in the working sample, the number of observations is smaller when we repeat the analyses using the restricted NCRP. The estimates, as reported in Appendix Table A5, are largely consistent with the benchmark results. In fact, the effects on the 1-year recidivism among multi-time reoffenders in violent and public order crimes are even larger in terms of percentage changes when using the restricted NCRP data. Therefore, the results strongly support the consistency of our findings in the main analyses.

Table A5. The Impact of the ACA Medicaid Expansion on 1-Year Recidivism – Restricted NCRP (2010-2015)

	(1) Violent	(2) Property	(3) Drug	(4) Public Order
Panel A: All Reoffenders				
Expansion*Post	-0.008	-0.004	-0.009	-0.006
	(0.007)	(0.009)	(0.007)	(0.006)
Wild Bootstrap p	0.303	0.692	0.258	0.362
Mean of Dependent Variable	0.099	0.132	0.093	0.104
Adjusted R^2	0.246	0.251	0.229	0.207
N	243,390	213,157	244,457	182,146
Panel B: Multi-Time Reoffenders				
Expansion*Post	-0.011**	-0.011	-0.003	-0.011**
	(0.005)	(0.007)	(0.005)	(0.004)
Wild Bootstrap p	0.062	0.155	0.555	0.043
Mean of Dependent Variable	0.031	0.045	0.029	0.033
Adjusted R^2	0.099	0.106	0.083	0.083
N	243,390	213,157	244,457	182,146

Note: The dependent variables are 1-year recidivism indicators for different first offense types. In all regressions, we control for offender characteristics and state time-varying effects (the minimum wage, the housing price index, poverty rate, and the unemployment rate), as well as state fixed effects, release-year fixed effects, and release-month fixed effects. The mean of the dependent variables and the adjusted R^2 are reported in the table. Standard errors in parentheses are clustered at the state level. p-values obtained from 1,000 wild cluster bootstrap iterations are also reported for the treatment indicator. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A6. The Impact of the ACA Medicaid Expansion on Substance Use Treatment Admission by Payment Source - Other Government Payment

		Conditional on Other Gov. Payment		
	(1)	(2)	(3)	(4)
	Other Gov. Payment	Self-Referral	Criminal Justice Referral (All)	Criminal Justice Referral (Prison/Probation/Parole)
Expansion*Post	-0.163	-0.032	-0.141	0.016
	(0.153)	(0.195)	(0.139)	(0.116)
N	274	274	274	274

Note: The dependent variable is the count of annual admissions to SUD treatment at the state level. In all regressions, we control for state time-varying effects (the minimum wage, the housing price index, the poverty rate, and the unemployment rate), as well as state fixed effects and admission-year fixed effects. Standard errors in parentheses are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A7. The Impact of the ACA Medicaid Expansion on Substance Use Disorder Treatment - Linear Regressions

	(1)	(2)	(3)
By Payment Source:	Self-pay	Private Insurance	Medicaid
Expansion*Post	0.111	0.360	1.147**
	(0.459)	(0.345)	(0.524)
N	274	274	274
	(1)	(2)	(3)
By Referral Source: *(Conditional on Medicaid)*	Self-Referral	Criminal Justice Referral (All)	Criminal Justice Referral (Prison/Probation/Parole)
Expansion*Post	1.043*	1.276**	1.272**
	(0.523)	(0.497)	(0.500)
N	274	274	274

Note: The dependent variable is the natural log of the count of annual admissions to SUD treatment at the state level. The reported sources for criminal justice referrals include state or federal courts, formal adjudication process, probation or parole, other legal entities, diversionary programs, prisons, and court referrals due to DUI or DWI. In all regressions, we control for state time-varying effects (the minimum wage, the housing price index, the poverty rate, the unemployment rate, and population), as well as state fixed effects and admission-year fixed effects. Standard errors in parentheses are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$