In-Kind Welfare Benefits and Recidivism Risk: Evidence from Medicaid[1]

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ABSTRACT: Of the 600,000 persons returning to the community from state and federal prisons each year in the US, more than 44% are re-arrested within one year. Most adults who serve prison sentences carry substantial debt, have low income and relatively low education, and limited formal employment experience prior to entering prison. Reentry into the community is characterized by a high incidence of adverse outcomes for individuals and their communities - financial hardship, morbidity and mortality, and re-offense. Medicaid coverage, as a means-tested transfer program providing subsidized health insurance, may influence recidivism through both financial and health channels. In this paper, we provide a comprehensive look at the effects of public health insurance coverage on the post-release behavior of formerly incarcerated adults. We study a natural experiment in which two separate state policy changes resulted in a 60 percentage point increase in Medicaid enrollment at the time of release. Using a series of individual level linked administrative datasets, we estimate the effects of this huge change in Medicaid enrollment on recidivism. We find declines in recidivism at 6 and 12 months associated with the increase in Medicaid enrollment. We test for Medicaid enrollment effects on employment and treatment for substance use disorders as potential explanatory mechanisms by which Medicaid coverage may influence recidivism, and show that both employment and health care use increase among the formerly incarcerated as a result of Medicaid enrollment.

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1. INTRODUCTION

Over 600,000 persons return to the community from state and federal prisons each year in the US (Carson, 2020). More than 44% are re-arrested at least once within one year and more than 80% by 9 years later. Most adults who serve prison sentences carry substantial debt, have low income and relatively low education, and limited formal employment experience prior to entering prison. Circumstances do not typically improve during incarceration, and economic disadvantage is associated with increased risk of recidivism. A majority of state prison inmates have a history of substance use (Belenko and Peugh, 2005), an additional risk factor for recidivism (Winter et al., 2019). Reentry into the community is characterized by a high incidence of adverse outcomes for individuals and their communities - financial hardship (Harding et al., 2014), morbidity (Mallik-Kane and Visher, 2008), mortality (Binswanger et al., 2013), and re-offense (Alper and Markman, 2018).

Many strategies have been tested or proposed to support a successful transition from prison to community for formerly incarcerated adults (Berghuis, 2018; Moore et al., 2020). These include reentry interventions as well as public policy. Reentry interventions vary along several dimensions (Jonson and Cullen, 2015). Content and intensity vary from programs with a singular focus such as work and vocational training (Jacobs 2012) to multi-modal programs that include a mix of social, housing, and health care supports (Duwe 2012; Grommon et al., 2013), and deterrent programs such as DNA registration (Anker, Doleac, and Landersø, 2017). Participation eligibility may be determined by demographic, health or criminogenic factors, and interventions may be administered by correctional agencies or community organizations. Against this variation, budget and capacity constraints are relatively common features which limit the reach of reentry interventions. Public policy is an alternative strategy to influence reentry outcomes and potentially for a larger population, although some of the consequences may be unintended (Doleac and Hansen 2020). Prominent examples include state laws and local ordinances that prohibit employers from asking about prior convictions on initial job applications (Agan and Starr, 2018), and states' relaxation of a federal ban on the provision of Temporary Aid for Needy Families (TANF) and/or Supplemental Nutrition Assistance (SNAP) to individuals convicted of drug felonies (Yang 2017a, Tuttle 2019). Evidence also supports the hypothesis that strong labor market conditions post-release may independently ease the transition to the community and encourage desistance from crime (Yang 2017b).

Recent efforts have begun to recognize that access to health care could play an important role during the critical reentry period. While incarcerated prisoners have access to at least some medical care. But historically, upon release this declined, as 80% of former inmates were typically uninsured post-release (Mallik-Kane and Visher, 2008.) Unlike SNAP and TANF, Medicaid has not legally excluded the formerly incarcerated from coverage, but most were unlikely to be eligible prior to the Affordable Care Act because of family status eligibility requirements. Policies to increase Medicaid coverage for eligible individuals upon release from jails and prisons have since been proposed as part of Medicaid waivers and some states have moved toward helping inmates transition through enrollment assistance programs as part of the discharge planning process (Jannetta et al., 2018).

Medicaid coverage, as a means-tested transfer program providing subsidized health insurance, may influence recidivism through both a financial and a health channel. As a transfer program, Medicaid has the potential to increase the opportunity cost of recidivism and reduce the financial incentives to pursue criminal activity. This could decrease returns to prison and increase formal employment, although the potential for disincentives to work may limit the reach of any effects. Of course, the nature of the benefits means that Medicaid coverage is not a direct substitute for income, and its value to an

individual likely depends on their need for health care. Medicaid coverage may also influence recidivism directly by facilitating treatment of conditions associated with elevated risk of recidivism including substance use disorders (SUDs). Efficacious outpatient treatment exists for SUDs (Dolan et al., 2003; Lee et al., 2016); however, financial access to treatment is limited for low-income adults without health insurance (Ali et al., 2017). The symptoms that untreated SUDs can impose (e.g., impulsivity, impaired judgment, aggression) may directly increase the risk of committing crime. Additionally, the nature of addiction, combined with limited material resources, creates an incentive to commit crime to purchase or otherwise obtain the addictive substance (Chandler et al., 2009; Goldstein, 1985). By reducing the out-of-pocket price for treatment, acquiring Medicaid coverage may facilitate SUD treatment use and reduce symptoms, thereby reducing the risk and incentive to commit crime.

A growing collection of studies estimate the impact of Medicaid eligibility expansions and enrollment policy on recidivism defined alternately as aggregate crime rates (He and Barkowski, 2020; Vogler, 2020; Wen et al., 2017b), the likelihood of rearrest (Fry et al., 2020), and the likelihood of reincarceration (Aslim, 2019; Gollu and Zapryanova, 2020). The findings are generally consistent; the increased *availability* of Medicaid is associated with reduced recidivism. This research provides important insights into the social welfare benefits of the Medicaid program. However, in most cases these are intention-to-treat analyses which are not well-suited to estimating the effect of coverage per se or the pathways by which Medicaid coverage may impact recidivism. The mechanism matters because it affects how we think about targeted policies to reduce repeat offending. If for example, Medicaid operates primarily through a health channel, desistance from crime policies that further support access and adherence to the relevant treatment may be appropriate. Alternatively, if an income effect prevails, further attention to the appropriate income eligibility threshold may be important to secure the income benefits while minimizing work disincentives.

In this paper, we provide a comprehensive look at the effects of public health insurance coverage on the post-release behavior of formerly incarcerated adults. We study a natural experiment in which two separate policy changes resulted in a major shift in Medicaid enrollment for formerly incarcerated adults. Together, these policy changes resulted in a nearly 60 percentage point increase in the likelihood of having Medicaid coverage in the month of release for formerly incarcerated adults (Burns et al., 2021). Using a series of individual level linked administrative datasets, we use the individual timing of release relative to the Medicaid coverage policy in place as an instrument for Medicaid enrollment to estimate the effects of this huge change in Medicaid enrollment on recidivism. Additionally, we examine the Medicaid enrollment effects on labor market outcomes and consider whether effects differ for a subgroup of inmates with a history of substance use as potential explanatory mechanisms by which Medicaid coverage may influence recidivism.

The current study makes several contributions to understanding the role of in-kind welfare benefits on desistance from crime. First, the unique dataset and empirical context enables us to examine directly the channels through which Medicaid has been theoretically suggested to affect recidivism by observing Medicaid enrollment, employment, and substance use treatment for individuals released from state prison. Scholars have offered alternative theories about how substance use influences crime perpetration and recidivism that suggest different hypotheses about the types of crimes that are likely to decline when individuals receive treatment (Evans et al., 2019; Goldstein, 1985; Hakansson and Jesionowska, 2018). We are able to study these ideas empirically because we observe individual history of substance use, receipt of treatment, and type of crime among those reincarcerated. Second, each of these potential mediators, employment and treatment for substance use disorders, is of potential policy interest in and of themselves, independent of their potential role relative to recidivism. Yet, to date,

there has been no empirical research on the impact of Medicaid coverage upon release from a correctional setting on employment or substance use treatment. Third, observation of individual-level releases connected to employment and reentry outcomes is a substantial contribution relative to prior work on the effects of Medicaid that almost exclusively relies on aggregate analyses of cross state variation in Medicaid policy.

In a preview of our findings, we find declines in recidivism at 6 and 12 months associated with the increase in Medicaid enrollment. We test for Medicaid enrollment effects on employment and treatment for substance use disorders as potential explanatory mechanisms by which Medicaid coverage may influence recidivism. We show that both employment and access to treatment increase as a result of Medicaid enrollment in ways that suggest a role for both financial and treatment mechanisms.

The rest of paper proceeds as follows. We discuss the theoretical framework for the study in Section 2 and review the background literature in Section 3. The empirical plan for the paper is discussed in Section 4 followed by our results and conclusions in Sections 5 and 6 respectively.

2. THEORY / FRAMEWORK

Our expectations of the effects of Medicaid coverage on recidivism derive from an expansion of Becker's model (Becker, 1968) that incorporates the decision to reoffend as explicated by Doleac (2019). At its foundation, the model asserts that individuals will commit crime if the expected utility from doing so exceeds the expected utility from not doing so, and that this calculation of costs and benefit is likely to differ for first-time offenders relative to individuals who have already committed a crime. Here, we present a slightly streamlined version of the model to fix ideas.

(1) $(1\text{-}p)U_{c1} + pU_{c2} > U_{nc}$

In this framework, p is the perceived probability of punishment, U_{c1} is the benefit from committing crime if the person is not punished, and U_{c2} is the payoff if punished. The benefit to non-criminal activity, or the opportunity cost of crime, is represented by U_{nc}. The utility derived from criminal and non-criminal activity includes both financial and psychic costs and benefits. Multiple factors determine each of these payoffs.

(2) $U_{c1} = f_{c1}(b, m)$

The payoff from committing crime without punishment, U_{c1}, is a function of the financial and psychic benefit or enjoyment from the criminal activity, b; the material and psychic costs of committing the offense, m. The function $f_{c1}(\ )$ is decreasing in m, and increasing in b.

(3) $U_{c2} = f_{c2}(b, m, t)$

In addition to the same costs and benefits as U_{c2}, the payoff with punishment includes t, any perceived direct (e.g., incarceration) and indirect (e.g., stigma, ineligibility for public benefits) penalties from committing the offense, where $f_{c2}(\ )$ is decreasing in t and m, and increasing in b.

(4) $U_{nc} = f_{nc}(w)$

Finally, U_{nc} is a function of *w*, non-criminal wages, net of material costs to engage in non-criminal activity (e.g., transportation to get to work, job training, etc.). The function f_{nc}( ) is increasing in *w*.

Finally, the individual's future discount rate will influence how each of these factors affects the expected utility from criminal activity. Individuals with higher discount rates, for whom the immediate benefits of crime are more highly valued than the more distant costs, will be more likely to commit crime, all other things equal.

To reduce reoffending, a given policy or intervention must then increase *p or* U_{nc}, or reduce U_{c1} or U_{c2}. Medicaid coverage has the potential to influence recidivism through multiple channels within this model. As an income transfer, Medicaid coverage increases the opportunity cost of continued criminal activity, *w,* thereby increasing U_{nc}; it is a financial benefit that can only be enjoyed while living in the community and is suspended or lost when an individual is admitted to a jail or prison. Practically, individuals may experience the financial benefit as a reduction in financial risk or a freeing up of resources that they might otherwise spend on health care. As a health care payer, Medicaid coverage may influence U_{c1} *and* U_{c2} to the extent that coverage increases treatment for conditions that influence the individual's perceived financial or psychic benefits, *b*, from criminal activity, their capacity to identify and perceive direct or indirect penalties from getting caught, *t*, their earning capacity and thus non-criminal wages, *w,* and/or their future discount rate.

A complicating factor in understanding the influence of Medicaid coverage on recidivism is the role that such coverage may play in former inmates' employment status. Employment is an important component of a sustained return to the community (Raphael, 2010) because it generates income and reduces the time available for criminal activity (Becker, 1968; Ehrlich, 1973; Grogger, 1998). Whether Medicaid coverage itself is likely to increase or decrease employment among recently incarcerated adults is uncertain. Economic theory predicts, and empirical research in the general population demonstrates, that receipt of means-tested, in-kind welfare benefits can reduce employment (Dague et al., 2017; Garthwaite et al., 2014; Hoynes and Schanzenbach, 2012). This prediction may or may not hold for adults with recent criminal justice involvement who already face greater barriers to employment than their peers without such a history (Pager, 2007). Alternatively, if the benefits mitigate impediments to work that are especially salient for this population (e.g., untreated substance use disorders, mental health) employment outcomes may improve.

Individuals with a history of committing a crime may have a higher, U_{c1} than before the first offense because they have invested in activity, persons, or skills that support criminal behavior thereby increasing *b*, while decreasing *m*. U_{c2} may be higher or lower for individuals who have already committed an offense relative to first-time offenders depending on the direct and indirect penalties they faced after their prior offense and the consequences of those penalties. The perceived likelihood of getting caught, *p*, may be lower or higher than before the first offense. It may be lower if the individual has learned how to avoid apprehension. It may be higher, if individuals perceive that routine contact with law enforcement through parole or probation obligations increases the risk of being caught. The utility derived from non-criminal activity, $U_{nc,}$ may be relatively lower for individuals who have already committed an offense than those who have not done so because the opportunity and ability to obtain legal employment may be reduced (e.g., due to absence from the labor force, employment required background checks, etc.). In other words, the opportunity cost of continuing to commit crime is lower relative to individuals who have not yet committed an offense.

Certain types of crimes may be tightly aligned with addiction disorders in which case we might expect more pronounced effects for those individuals, if Medicaid operates through a health channel. However, we refrain from asserting an hypothesis about differential effects of Medicaid coverage by crime for two reasons. First, while self-reported drug-seeking as motivation for criminal activity varies by type of conviction, it is quite common. In the most recent available data, 21% of state prisoners reported that they committed their offense to obtain drugs or money to buy drugs (Bronson 2020). Second, we observe the conviction (e.g., property, violent, public order, etc.,) not the crime. The type of conviction is a function of the actual crime, any potential plea bargain, and it reflects only the most serious offense when an individual is convicted of multiple offenses (Sawyer 2020). Thus, there is some amount of unknown, likely non-random, measurement error in conviction type that obscures its potential usefulness as a signal for addiction.

On net, our expectation is that Medicaid coverage is likely to decrease recidivism, but that employment could increase or decrease, and that use of health care will increase. We also expect that any effects are likely to be larger for those with a history of substance use for whom a treatment mechanism is more likely to be salient. Although we do not posit specific hypotheses with respect to prior offenses or type of crime, we do present results separately by these groups in the empirical analyses.

3. BACKGROUND

As hypothesized above, Medicaid coverage may reduce the likelihood of recidivism to the degree that it improves either economic well-being or increases the use of treatment for substance use disorders. In this section, we discuss the availability of Medicaid for justice-involved adults and review the empirical literature addressing each component of these theoretical linkages.

3.1 Medicaid Availability and Justice Involved Adults

In the years preceding the implementation of the Affordable Care Act (ACA), 80% of adults who were recently incarcerated lacked health insurance in the 2-3 months following release (Mallik-Kane and Visher, 2008). However, the implementation of the ACA Medicaid expansions, now operating in 37 states, increased the proportion of recently incarcerated adults who are eligible for Medicaid largely by extending eligibility to adults without dependent children. Initial projections estimated that 21-34% of adults released from prison were likely to gain Medicaid coverage because of ACA Medicaid expansions (Cuellar and Cheema, 2014). In the year following implementation of the ACA, Medicaid coverage was 5 to 8 percentage points higher among individuals with a recent history of justice-involvement although the role of Medicaid expansions specifically was not identified (Saloner et al., 2016; Winkelman et al., 2016).

There have been no national estimates of the effect of Medicaid expansions on insurance coverage for adults reentering the community from correctional facilities. Two single state studies evaluated the impact on Medicaid coverage of state-level policy changes related to eligibiliy and facilitated enrollment. In Indiana, each of three separate policies was associated with increased Medicaid enrollment within 120 days of release: a Medicaid eligibility expansion; submission of Medicaid applications on behalf of incarcerated individuals pre-release; and suspension rather than termination of Medicaid coverage upon incarceration (Blackburn et al., 2020.) In Wisconsin, Medicaid enrollment in the month of release from state prison grew from 8 percent of adults at baseline to 36 percent after a Medicaid eligibility expansion, and up to 61 percent after the introduction of pre-release enrollment assistance (Burns et al., 2021.) The latter paper describes the first stage for the current study.

3.2 Medicaid and Recidivism

As referenced earlier, a small but growing literature exploits variation over time and across states to examine the impact of Medicaid eligibility expansions and enrollment policy. Three studies evaluate the impact of Medicaid expansions on aggregate crime rates from the FBI's Uniform Crime Report program using difference-in-difference strategies with state- or contiguous border-county samples. Wen, Hockenberry and Cummings (2017) examined the impact of pre-ACA Medicaid expansions and found a decrease in total crime rates driven by reductions in three categories, robbery (-2%), aggravated assault (-1%) and larceny theft (-0.6%). Over a study period of 2010-2016, He and Barkowski (2020) assessed the impact of Medicaid expansions on the rates of specific types of crime. Across modeling strategies and specifications, they most consistently found reductions in the rate of motor vehicle theft and robbery. With a somewhat longer observation period, 2009-2018, Vogler (2020) found no impact of Medicaid expansions on overall rates of crime but a robust 5% decline in the rate of violent crime. This reduction was largely explained by a reduction in aggravated assaults.

Using individual level data for persons released from state prison between 2010-2016, Aslim and colleagues (2019) estimated the likelihood of reincarceration by type of crime. They observed negative, statistically insignificant effects for first-time reoffenders and a relative decrease in reincarceration for violent crime at 1- and 2-years for multi-time reoffenders. Fry, McGuire and Frank (2020) used a comparative interrupted time series (ITS) design to examine the change in the likelihood of rearrest among adults booked into county jails in three counties located in Medicaid expansion states relative to three matched counties in non-expansion states. Relative to their matched pair, rearrests decreased within 2 of the 3 expansion counties, and increased in the third, over the 2-year post-expansion period. Gollu and Zapryanova (2020) find that suspending rather than terminating Medicaid coverage is associated with a relative decline in the likelihood of recidivism within 3-years. Lastly, Jacome (2020) exploits age-based eligibility criteria for Medicaid to estimate the effect of losing Medicaid eligibility at age 19 on the likelihood of reincarceration among low-income young men in South Carolina. She finds that young men who lose access to Medicaid eligibility on their 19th birthday are 15% more likely to be incarcerated in the following two years than similar men who were not likely to be enrolled in Medicaid immediately before their 19th birthday. The findings were pronounced for men with a history of mental illness.

A subset of the above studies also examined the mediating effects of SUD treatment. Aslim and colleagues (2020) analyzed the impact of Medicaid eligibility expansions on the state count of admissions to SUD treatment facilities. Expanded Medicaid eligibility was associated with increased admissions for which Medicaid was the payer. Wen, Hockenberry and Cummings (2017) examined the explanatory role of SUD treatment in the reduction in crime rates that followed implementation of pre-ACA expansion expansions. Using 2SLS in which the Medicaid expansion and the presence of a state mandate for SUD treatment insurance parity served as instruments, they found that an increase in SUD treatment rates was associated with a reduction in the rate of select types of crime (e.g., robbery, aggravated assault, larceny theft). Vogler (2020) explored the potential role of SUD treatment in explaining an observed association between Medicaid expansion and a reduction in aggravated assaults by interacting state expansion status with the state Medicaid program's generosity of coverage for SUD services. There was no consistent difference in the association between Medicaid expansion and rates of crime according to generosity of coverage. Lacking data that directly links individual interactions with corrections, Medicaid, and SUD treatment, these studies offer suggestive but inconclusive evidence that SUD treatment mediates the relationship between Medicaid coverage and recidivism.

We conclude from this literature, that although findings are not uniform, there is consistent evidence of an association between expanded Medicaid eligibility and a reduction in recidivism. However, the intention-to-treat design of these studies makes it challenging to identify the mechanism(s) by which Medicaid may achieve reductions in criminal activity.

3.3 Economic Well-Being and Recidivism

Most adults who serve prison sentences carry substantial debt (Harper et al., 2020) have low income and relatively low levels of education (Harlow, 2003), and limited employment experience before they enter prison (Looney and Turner 2018). These circumstances do not typically improve during incarceration such that they face significant barriers to meeting basic needs and achieving economic security when they return to the community (Berk et al., 1980; Harding et al., 2014; Western 2002). In turn, economic disadvantage is associated with increased risk of recidivism (Link et al., 2019; Yukhnenko et al., 2020). This dynamic has motivated intervention and research to determine if reducing financial hardship during the post-incarceration period reduces the likelihood of recidivism, including income support, access to income transfer programs (e.g., SNAP) and opportunities for employment.

In an early experimental study, Berk and colleagues compared the likelihood of employment and arrests during the 12-months following release from prison among adults who were randomly assigned to eligibility for unemployment benefits or the control condition of job counseling (Berk et al, 1980). The treatment conditions varied in duration and the level of tax imposed on earnings, from 25% to 100%. Using an intention to treat analysis, they found no difference in arrests across the treatment and control conditions, and a relative decrease in employment among adults assigned to the unemployment benefits condition. In supplementary structural equation modeling, the employment effects were similar; however, unemployment benefits were associated with a relative reduction in arrests for both property and non-property crime. More recently, Yang (2017b) evaluated the impact of local labor market conditions at the time of release on risk of recidivism within 3 years of prison release by exploiting variation in the average low-skilled wages for men across counties and time within 43 states. Higher wages for low-skilled employment at the time of release is associated with a reduction in the risk of recidivism.

A separate literature considers the impact of access to income transfer programs on recidivism. Most recently three studies separately test whether improved access to public welfare benefits including Supplemental Nutrition Assistance Program and Temporary Assistance for Needy Families following release from prison reduces the risk of recidivism. They exploit changes over time in an eligibility exclusion for the programs that applies to individuals with felony drug convictions. Variation in state responses to this ban provide the basis for Yang's identification strategy, a triple difference-in-differences design comparing individuals released from prison for drug and non-drug related offenses across states and over time (Yang, 2017a). The risk of recidivism falls by roughly 10 percent following states' partial or full elimination of this exclusion. Tuttle (2019) uses a regression discontinuity design, and finds a robust though imprecisely estimated, increase in the likelihood of recidivism among adults convicted of drug trafficking following imposition of the exclusion that is driven by financially motivated crime. Luallen and colleagues (2018) used a regression discontinuity design that incorporates difference-in-differences estimation to compare the risk of recidivism for individuals who were admitted to prison before and after the initial adoption of the exclusion within six states. The treatment group included individuals with a conviction for a drug offense; the comparison group include individuals convicted of a non-drug offense. They found no evidence of a change in risk of recidivism for the affected group of individuals following the eligibility exclusion although there was suggestive evidence of heterogeneous effects according to time at risk.

3.4 *Medicaid and Economic Well-Being*

A growing literature demonstrates the positive effects of Medicaid on measures of financial security suggesting the plausibility of the income effect channel. Finkelstein et al., (2012), Baicker et al., (2013), Hu et al., (2016), and Gruber and Yelowitz (1999) study financial outcomes and find improvements in one or more of the following outcomes, medical debt, financial strain, credit scores, and out-of-pocket medical spending. Gross and Notowidigdo (2011) examine bankruptcies using the expansions of SCHIP and Medicaid from 1992 to 2004; they find that a 10-percentage point increase in insurance eligibility decreases bankruptcies by 8%. Two additional papers find decreases in out-of-pocket spending in the context of the early Medicaid expansions (Golberstein and Gonzales, 2015; McMorrow et al., 2016).

The literature generally presumes that financial outcomes are a proxy for the consumption-smoothing benefits of health insurance, although it is unclear to what degree the changes in finances reported result from decreased prices, reduced risk, increased effective income, or some combination. Medicaid coverage has the potential to affect financial security through each of these mechanisms. It changes the risk of large, unexpected expenses and the out-of-pocket price of health care services. It is also unclear to what degree the incidence of the financial benefits falls on the beneficiaries as opposed to providers of health care services (Finkelstein et al., 2018).

As a transfer program, Medicaid may also influence financial well-being more broadly through income effects in which resources previously allocated to health care are newly available for other purposes that may alter the incentives to recidivate. Most salient in this case is likely labor supply incentives, which have been studied in Medicaid but not previously in the justice-involved population. Pre-ACA work has found somewhat mixed effects. For example, there was no evidence of employment effects in the Oregon Health Insurance Experiment population (Baicker et al., 2014) but Wisconsin adults who were able to access an earlier limited Medicaid waiver program returned to the work force more slowly than those who were waitlisted for the program, with net employment declines of 3-5 percentage points (Dague et al., 2017). Some work finds evidence of large increases in employment in Tennessee following the TennCare disenrollment event, suggesting strong work disincentives, although other work on the change using panel data has not replicated this finding (Garthwaite et al., 2014). Recent cross-sectional work focused on the ACA Medicaid expansions does not typically find any declines in employment, which could be due in part to policy uncertainty, different affected income groups, labor market conditions, or to the availability of phased out Marketplace subsidies in non-expansion states (Gooptu et al., 2016; Kaestner et al., 2017; Leung and Mas, 2018). We do not rule out the possibility, which has theoretical and empirical support in the context of other transfer programs.

3.4 Treatment for substance use disorder and recidivism

Observational studies consistently find that treatment for substance use is associated with reduced criminal activity including recidivism (Bondurant et al., 2018, Bukten et al., 2012; Campbell et al., 2007; Deck et al., 2009; Durbeej et al., 2015; Gossop et al., 2005). Experimental studies of the effect of SUD treatment on recidivism commonly examine interventions that begin during the incarceration period with or without an additional post-release component (de Andreade et al., 2018; Perry et al., 2015; Glanville et al., 2021; Moore et al., 2020.) While the findings are not uniform (e.g., Lee at al 2016), two categories of interventions indicate promising effects of treatment on recidivism: residential therapeutic communities which provide comprehensive SUD treatment, support the development of vocational and independent living skills, and provide housing when implemented in the community (Sacks et al., 2012; Olson and Lurigio 2014); and pharmacologic treatment for opioid use disorder with or without concomitant behavioral therapy (Schwartz et al., 2009; Gordon et al., 2008.)We conclude that the

empirical literature suggests that receipt of SUD treatment is a plausible mechanism for reducing recidivism but may depend on the type and scope of treatment as well as the timing of treatment initiation (e.g., pre/post release) and continuity during the re-entry period. Whether Medicaid coverage can trigger this mechanism, depends on how coverage influences treatment use.

3.5 *Medicaid and the use of substance use disorder treatment*

While the effect of Medicaid *enrollment* on use of SUD treatment has not been well-studied, a growing evidence base examines the impact of Medicaid eligibility expansions on SUD service use. This research has generally evaluated three types of outcomes, the share of patients or services for which Medicaid is the payer, the population rate of SUD services provided overall and paid by Medicaid specifically, and the number of individuals or share of a given population that received any SUD services (M. Olfson et al., 2018). There is strong evidence that the share of services for which Medicaid is the payer increased in states that expanded Medicaid eligibility under the ACA relative to non-expansion states including admissions to specialty treatment facilities (Maclean and Saloner, 2019; Saloner and Maclean, 2020), outpatient SUD programs (Andrews et al., 2019), and emergency department visits and hospitalizations (Wen et al., 2020). The increasing role of Medicaid as payor is typically offset by a reduction in self-pay or uninsured as the payer source. Similarly consistent evidence shows that the rate of Medicaid-reimbursed prescription medications to treat opioid use disorder (OUD) and alcohol use disorder increased in expansion states compared to non-expansions states (Maclean and Saloner, 2019; Meinhofer and Witman, 2018; B. Saloner et al., 2018; Sharp et al., 2018; Wen et al., 2017a).

The impact of Medicaid expansions on population rates of SUD services provided is somewhat more mixed across outcomes and duration of follow-up. The rate of prescription medication fills for OUD increased in expansion counties relative to non-expansion counties within 5 states; there was no observed difference in the mean number of days of treatment supplied (Saloner et al., 2018). A small group of studies has evaluated the change in admissions to specialty treatment facilities following ACA Medicaid expansions using difference-in-differences and event study designs. In the short-term, the rate of opioid-related admissions to specialty treatment facilities in expansion states relative to non-expansion states increased (Meinhofer and Witman, 2018). The rate of all types of SUD admissions to specialty treatment facilities did not differ across expansion and non-expansion states initially but increased steadily in subsequent years resulting in a cumulative increase of 35.5% relative to non-expansion states (Maclean and Saloner, 2019; Saloner and Maclean, 2020). There was no notable change in the rate of opioid-related emergency department visits associated with either early Medicaid expansions (2005-2013) or post-2014 expansions (2014-2017). The post-2014 expansions were associated with a 10% reduction in the opioid-related hospitalization rate (Wen et al., 2020.)

Three survey-based studies have assessed the impact of Medicaid expansions on the size, or share, of a given population that receives SUD treatment. Among low-income adults with self-reported SUDs, Olfson and colleagues (2018) found no significant change in the percentage who reported receiving any treatment in the past year. Andrews and colleagues (2019) found no evidence of change in the total number of patients served by outpatient treatment programs. Finally, Saloner and colleagues (2016) assessed the impact of the ACA's as a whole on use of treatment for SUDs among adults with SUDs who also had recent justice-involvement (i.e., arrested, booked, on probation or parole.) There were no changes in the likelihood of receiving SUD treatment in the first year of the ACA's implementation.

We conclude from this literature review that Medicaid expansions are associated with a significant change in the payer composition for SUD services, and an increase in the population rate of some types of SUD treatment services across payers and paid by Medicaid specifically. Whether the expansions

increased the likelihood of receiving treatment within selected populations of policy interest remains less clear.

4. EMPIRICAL METHODS

4.1 Natural Experiment

We exploit two policy changes in the State of Wisconsin that greatly expanded the availability of Medicaid benefits to released prisoners. First, on April 1, 2014 Wisconsin expanded Medicaid eligibility to all adults with income below 100% of the federal poverty level (FPL). The state did not participate in the full Medicaid expansion authorized by the Affordable Care Act; this eligibility expansion was done under waiver authority and at a higher state funding share. Parents with income below 100% FPL were already eligible for Medicaid at that time and have remained so. However, before April 2014, Medicaid in Wisconsin (called BadgerCare) was generally unavailable to non-disabled adults without dependent children in the home ("childless adults", which would include non-custodial parents). The income and family composition of the vast majority of adults released from state correctional facilities would allow them to qualify as childless adults after the 2014 policy change, but not previously (Western and Smith, 2018).

Second, beginning in January 2015, the Wisconsin Department of Corrections (DOC) introduced pre-release Medicaid enrollment assistance. The pre-release enrollment assistance program is available to all adults under the supervision of the state's Division of Adult Institutions (DAI) incarcerated within state correctional facilities; these include state prisons, correctional centers, and DAI-contracted beds within county jails. Under the new enrollment assistance program, individuals may apply for Medicaid as early as the 20th day of the month prior to their month of release. In all facilities, DAI discharge planning staff provide guidance on how to apply for Medicaid, and individuals are given the opportunity to call an eligibility case worker from the correctional facility to do so. Additionally, five facilities share three paralegal staff who also assist inmates with the enrollment process. The DOC selected these five facilities for additional support based on the composition of their populations (e.g., relatively high prevalence of limited English proficiency, intellectual disabilities, mental illness, etc.) At all facilities, the eligibility decision is generally made at the conclusion of this single call. If deemed eligible, the Medicaid coverage is effective upon release from the correctional facility. Full implementation of the enrollment assistance program was complete at the end of March 2015.

4.2 Data

We use a person-level, longitudinal dataset for the years 2013-2017 which combines administrative data from the state of Wisconsin's Department of Corrections (DOC), the Medicaid program, and the Unemployment Insurance (UI) program as well as the Census Bureau's Quarterly Workforce Indicators (QWI) (U.S. Census Bureau, 2016). These records are matched within the Institute for Research on Poverty's Wisconsin Administrative Data Core (Brown and Thornton, 2020) using Social Security Numbers (last four digits), names, dates of birth, and other characteristics such as gender and race/ethnicity. Linkages are made using fuzzy matching methods to account for name variants, data entry errors, or other data quality issues.

We observe Medicaid enrollment status, which is defined at the month level, from the Medicaid program data. From the Department of Corrections' data, we observe individual demographic characteristics including age, sex, race (Black, White, American Indian/Alaskan Native, Asian or Pacific Islander, and unknown), level of education (>= high school degree), marital status, and whether the county of conviction is part of a metropolitan statistical area. County of conviction serves as a proxy for

county of release following prior research (Yang 2017b). Additionally, we observe the characteristics of each prison term including exact entry and release dates, the correctional facility, the Association of State Correctional Administrators' category of conviction offense(s) (i.e., violent, property, drug, and public order), and an indicator for having a history of substance use. The latter variable is derived from self-reported data collected through the DOC's risk and needs assessment tool, the Correctional Offender Management Profiling for Alternative Sanctions (COMPAS) (Northpoint, Inc. 2019; Northpointe Institute for Public Management, 2009). Additional information about this measure and the COMPAS tool is included in the Appendix. The QWI includes quarterly measures of average wages at the county-level stratified by age, sex, industry, and educational background which we use to adjust for local labor market demand.

The UI data includes quarterly wages for any individuals who are employed by an employer covered by Wisconsin UI law whose work could qualify them for UI benefits.[2] These measures may be missing some forms of employment (such as independent contracting) that the formerly incarcerated may be engaged in; so long as this is not differential across time periods this is not an issue for bias in our estimates, only for interpretation.

4.3 Sample

From these data, we create a release-level analytic sample that includes the available demographic and term characteristics in addition to the outcome measures described below. The study population includes all adults ages 18-64 incarcerated by the state and under supervision of the Division of Adult Institutions who were released to the community between January 2013 – June 2017. We required a minimum incarceration period of 30 days to increase the likelihood that individuals had adequate time to complete the discharge planning process and thus have the opportunity to enroll in Medicaid. The full sample include 32,846 individuals for whom we observe 41,001 releases. Table 1 includes the full set of summary characteristics on the sample by individual (defined at the first release) and for all releases.

4.4 Outcome Measures

Medicaid enrollment is our outcome for the first stage of the analysis. Because everyone is eventually exposed to the Medicaid expansion policy, we focus on outcomes that are defined relative to or at the time of release. Thus, even if someone released in December 2013 enrolls in Medicaid in April 2014, they were not able to do so at the time of their release. We create a binary variable for Medicaid enrollment that is equal to one if the individual is enrolled in Medicaid in the month of release and zero otherwise based on our observation of Medicaid enrollment following the data linking process.

We define recidivism as an admission to a state correctional facility, including prisons, detention centers, and state-contracted beds in county jails, within 6 and 12 months of release from state prison. Our measure of recidivism includes all types of admissions including for new sentences, revocations of parole and probation, and holds. In Wisconsin, holds may be imposed for any of the following reasons: for an investigation of an alleged violation of a rule or condition of supervision; after an alleged violation to determine whether to commence revocation proceedings; for disciplinary purposes; to prevent a possible violation by the offender; and pending placement in a program as an alternative to revocation.

[2] Precise information on covered employment is available from the Wisconsin Department of Workforce Development at https://dwd.wisconsin.gov/ui201/t1201.htm

Employment status and earnings are defined at the quarter-level using the QWI data. We define any employment as having non-zero earnings in a quarter. The quarter of release is set to be the one that contains the release date. We set earnings to zero if we do not observe any earnings in the data in that quarter for an individual. To adjust for differences in time available to have earnings and employment, in the empirical specifications we control for the fraction of the quarter of release remaining.

Using Medicaid claims data, we measured any substance use disorder-related and overall use of outpatient, emergency department, and inpatient care as well as medication treatment for opioid use disorder (MOUD). We constructed four binary measures of health care use within the immediate 30-days after release: any outpatient visit, any outpatient visit with a substance use disorder diagnosis (excluding tobacco dependence), any outpatient visit with an opioid use disorder diagnosis, and receipt of any medication for opioid use disorder. Extending the observation period to six-months after release, we assessed the following additional binary measures: any outpatient visit, any outpatient visit with a substance use disorder diagnosis, receipt of any medication for opioid use disorder, any emergency department visit, and any hospital admission. Additional details about the definition of these measures are included in the appendix.

5. EMPIRICAL MODEL & IDENTIFICATION

In general, recidivism and employment are likely to be endogenous to characteristics correlated with Medicaid enrollment including, in particular, income. The changes in Medicaid eligibility that occurred during this time period are a plausibly exogenous determinant of Medicaid enrollment. We thus use the different policy regimes as instruments for Medicaid enrollment.

We define the regimes as follows:

- Regime 0 (control): released January 2013-March 2014;
- Regime 1 (pure expansion): released April 2014-December 2014
- Regime 2 (expansion + EA phase-in): released January 2015-March 2015
- Regime 3 (expansion + full EA): released April 2015-December 2015

The IV model is given by the following two equations:

(1) $Y_i = \alpha Medicaid_i + \boldsymbol{X}_i\beta + \mu_i$
(2) $Medicaid_i = \sum_{n=1}^{3}\left(\lambda_n Regime_{n(i)}\right) + \boldsymbol{X}_i\delta + \upsilon_i$

In the model, Y_i is the outcome of interest for release *i*, *Medicaid* is an indicator for Medicaid enrollment, $Regime_{n(i)}$ is an indicator for whether release *i* was during regime n, and $\boldsymbol{X}_i$ is a vector of release characteristics that includes age, sex, race, level of education, marital status, months incarcerated, type of crime, and risk of substance use. The coefficient α is the parameter of interest. Finally, μ_i and υ_i are error terms.

Because prisoners do not have control over the timing of their release from a facility and are not able to delay their release until a more favorable policy regime is in place, we argue that release date is as good as randomly assigned. To support plausibility we include the results of a normalized difference as a balance test across regimes in Table 1. In large administrative samples such as ours, the t-statistic can be large in absolute value, and even small differences are statistically significant even if they are economically small (Imbens, 2015). The normalized difference is the difference in means over regimes

divided by the square root of half the sum of the group variances, so it is scale invariant. Large values for the normalized differences would suggest that the average covariate values in the two groups are substantially different and that we might be concerned for the plausibility of the as good as randomization assumption. A rule of thumb based on Imbens and Rubin (2015) is that differences of 1 or larger could be problematic and differences of .25 or smaller suggest good balance; all of the normalized differences in Table 1 are less than .2, with balance supporting the randomization assumption.

The exclusion restriction is that the timing of release (the regime) is unrelated to the outcome (recidivism, employment, and earnings) conditional on the other variables included in the model. The main threat to identification is changes over time in the outside environment (i.e., the economy), since there is no possible simultaneous control group. In order to address this, we include controls from the QWI for the employment to population ratio for men with low education levels at the time of release. In some models, we also include fixed effects for correctional facilities, as Medicaid take-up rates varied by facility.

Because we are overidentified, we estimate this model using GMM in Stata 16; specifically, we use the implementation in the *ivreghdfe* Stata package (Correia, 2018; StataCorp, 2019) which partials out controls and fixed effects. GMM is more efficient than alternatives like two stage least squares when there are more instruments than endogenous regressors and there is heteroskedasticity. We cluster standard errors at the individual level in models that include multiple releases per individual.

The two policies are not exactly the same, and the second was phased in over several months. Here, we combine the variation from these phases, but we also provide the reduced form estimates of the direct effects of the policies on the outcomes. The equation for the reduced form is:

(3) $Y_i = \sum_{n=1}^{3}\left(\gamma_n Regime_{n(i)}\right) + \boldsymbol{X_i}\theta + \epsilon_i$

The differences in the policies may attract different sets of enrollees, which is not only inherently interesting from the perspective of understanding Medicaid take-up but also might imply heterogeneity in the results, which could be useful for external validity considerations. We can describe the distribution of compliers' characteristics under the different regimes using the potential outcomes framework. Let $Medicaid_{ni}$ be the Medicaid enrollment indicator of release i under regime n. Because we do not observe $Medicaid_{ni}$ for each release i and every regime, it is not possible to identify directly the compliers at each regime and calculate the distribution of characteristics for these groups. However, we can describe the distribution of complier characteristics using the variation in the first stage across covariate groups (Angrist and Pischke, 2008).

Let X_i be a characteristic with two possible values–e.g., one if high school graduate and zero otherwise. Using Bayes rule, we can see if compliers in regimes 1-3 are more or less likely to be high school graduate than other releases with the following equation:

(4) $$\Pr(X_i = 1 | Medicaid_{ni} > Medicaid_{0i}) = \frac{\Pr(Medicaid_{ni} > Medicaid_{0i} | X_i = 1)}{\Pr(Medicaid_{ni} > Medicaid_{0i})}$$

$$= \frac{\mathrm{E}(Medicaid_i | Regime_{n(i)} = 1, X_i = 1) - \mathrm{E}(Medicaid_i | Regime_{0(i)} = 1, X_i = 1)}{\mathrm{E}(Medicaid_i | Regime_{n(i)} = 1) - \mathrm{E}(Medicaid_i | Regime_{0(i)} = 1)}$$

That is, the relative likelihood a complier is a high school graduate is given by the ratio of the first stage for high school graduates to the overall first stage. We provide the ratios as a descriptive exercise and more fully explore the potential for treatment effect bounds and heterogeneity in Section 7 below.

Some individuals have multiple releases in the data. In this version of the paper, our ability to observe releases prior to January 2013 among individuals in our sample is limited, although we are working to obtain additional information on prior justice involvement. This means there may be censoring related to the timing of release in what we consider to be the first release of an individual. The sample of all releases, however, includes multiple observations from repeat offenders who may be more or less likely to be affected by the policies. We include results from both a subsample of first releases and the full set of releases to help contextualize the results.

6. RESULTS

6.1 First Stage Results

Figure 1 illustrates the fraction of releases that were enrolled in Medicaid over time, binned by weeks relative to the policy changes. It provides clear evidence that the new Medicaid policies resulted in large increases in Medicaid enrollment at the time of release among former prisoners.

Table 2 summarizes the results of the first stage estimation (represented in the empirical model by equation (2) for our preferred specification, which includes facility fixed effects although the first stage is not specification sensitive. Among first releases, the availability of Medicaid to adults without dependent children with incomes below the poverty line (Regime 1) resulted in a 29 percentage point (p.p.) increase in Medicaid enrollment in the month of release. During the implementation of enrollment assistance (Regime 2), this increased to 46 p.p., and after full implementation of enrollment assistance it had increased to 58 p.p. (Regime 3). Among all releases, the increase was very similar, 1-2 p.p. larger across the board than among first releases. During the pre-period (Regime 0), average enrollment at the time of release was 10%. The policy-related increases thus represent a roughly 200% increase during Regime 1 and a 500% increase by Regime 3.

Table 2 includes the first stage results for specific subjects of interest: history of SUD and type of crime. Those with a history of SUD had a net increase that was 2-4 percentage points higher than first releases, and 1-2 p.p. larger than all releases. Among types of crime, the largest increase in Medicaid enrollment at Regime 1 was for those with a violent crime (32 percentage points). The largest net increase from the enrollment assistance relative to Medicaid expansion (Regime 3 – Regime 1) was for those with a drug crime (35 p.p.). The largest overall increase in enrollment across regimes was by those with public order crimes (63 p.p.). The major takeaway, however, is that the increases in enrollment were very similar across all groups. In terms of instrument relevance, there is not a weak instruments problem as indicated by the large first stage *F* statistics reported in the table.

Appendix Table 1 and Figure 2 report the ratios of the average complier characteristics in each regime with respect to the sample average. Note that the sample is that of first releases. (Appendix Table 2 presents the analogous results for the sample of all releases, but there are not substantial differences).

Compliers in regime 1 and 2 are not more or less likely to be female, white, or have a high school degree, with ratios not statistically different than 1 at a 5% confidence level. However, compliers in regime 3 are less likely to be female, more likely to be white, and more likely to have a high school degree than the average. In each of regimes 1-3, compliers are less likely to be married and more likely

to be older than 35 years relative to the sample average. There are not statistically significant differences in marital status of compliers across regimes.

The ratio for history of SUD is greater than one at every regime but not statistically difference than 1 at 5% confidence level. Meanwhile, compliers at regime 2 and 3 are more likely to be incarcerated for over a year than the average release in the sample, and the ratio in regime 1 is not statistically significant than 1.

Regarding the type of crime of conviction, compliers' releases are not more or less likely than average to be related to property, public order, or violent crimes. However, compliers at regime 1 are relatively less likely to be classified as drug related crimes. These results hold for the sample of all releases as well (Appendix Table 2).

6.2 Recidivism

We can see the results of OLS estimation of the effect of Medicaid enrollment on recidivism along with the IV estimated with GMM and the reduced form effects of the regimes on reincarceration within 6 months in Table 3. Panel A provides results for the sample of first releases and Panel B shows results for all releases. All models include controls for age, race, education, marital status, duration of incarceration, type of crime, calendar month of release, and some models include additionally controls for the employment to population ratio of low-educated men, or fixed effects for facility of incarceration. Here, we focus on our preferred estimates which include controls for both the employment to population ratio and facility fixed effects and discuss differences across specifications specifically when relevant.

The OLS results do not consistently show a relationship between Medicaid enrollment and reincarceration. However, the IV GMM results show that Medicaid enrollment is associated with a decline in the probability of recidivism for first releases of 4.4 p.p., statistically significant at the 1% level. For all releases, the effect size is 1.8 p.p. and statistically significant at the 5% level. Statistically significant effects are also evident in the reduced forms across all regimes. Average 6 months recidivism in the data is 17% for all spells and 16% for the first spell, so the implied effect size from the IV is a reduction in recidivism of 28% for first releases or 11% for all releases.

Table 4 shows the same results estimated for a longer post-release window, 12 months. Again the OLS results for first releases do not indicate a statistically significant correlation between Medicaid enrollment and recidivism, although for all releases there is a positive correlation. In the IV results, for first releases we see a statistically significant decrease in recidivism of approximately 5 p.p. depending on the specification. In the sample of all releases, the effects at 12 months are slightly attenuated and no longer statistically significant in all specifications, although they are consistently negatively signed and the reduced form indicates declines, particularly for Regime 1. Again, these effects are similarly evident in the reduced forms. Average 12 month recidivism is 31% of all spells and 30% of first spells, so the implied effect size is a reduction of 17% for first releases or 4% for all releases.

6.3 Employment and Earnings

Tables 5 and 6 show the results of estimation for the employment and earnings outcomes, respectively. Recall that these results represent changes in UI-reports and are therefore representative of a particular type of traditional employment, not including forms of self-employment or contract work. Similar to above, all models include controls for age, race, education, marital status, duration of incarceration, type of crime, calendar month of release, and some models include additionally controls for the

employment to population ratio of low-educated men and fixed effects for facility of incarceration. An important difference is that because the employment and earnings data are quarterly, here we control for the fraction of the quarter remaining in which the release occurred to adjust for differences in time available to obtain employment and gain earnings post-release.

In Panel A of Table 5, we show the results for the sample of first releases. There is generally a positive correlation between Medicaid enrollment and employment in the OLS estimates, particularly in the specification including facility fixed effects and controls for the employment to population ratio. The IV results suggest an increase in employment in the quarter of release of 5 p.p.. Panel B includes the results for the sample of all releases which shows an increase of 6 p.p. in our preferred specification. In the reduced form, these effects are most strongly evident in Regime 3. Relative to baseline Regime 0 employment of 21% for first releases and 20% for all releases, the implied effect sizes are 28% and 30% respectively.

Results for net earnings are in Table 6. Medicaid enrollment is generally negatively correlated with earnings in the OLS (likely since enrolling in Medicaid requires, mechanically, a lower income). The IV results, however, suggest an increase in net earnings consistent with the IV employment effects, of approximately $205 in the quarter of release, among both first releases and for all releases. Again in the reduced form, the effects are most strongly evident in Regime 3. Relative to baseline quarterly earnings of about $350 for first releases and similar for all releases, the effect sizes are roughly 59%.

6.4 Health Care Use Outcomes

Tables 7-8 report the results for the health care use outcomes. It should be noted that we only observe health care use with Medicaid as the payer. We therefore might think of these results as upper bounds on the total change in health care use from gaining Medicaid, since it may substitute for care paid by other sources (e.g., private insurance, charity care, etc.). However, we anticipate that such crowd-out is likely to be minimal because of the low rate of private insurance coverage within this population (Mallik-Kane et al., 2018; Mallik-Kane and Visher, 2008) and limited availability of services for uninsured individuals (Seo et al., 2019; Friedmann et al., 2003; Gryczynski et al., 2011). We also note that we do not observe whether the outpatient care obtained was therapeutic, diagnostic, or preventive in nature (with the exception of the OUD medication treatment outcome). Similarly, without further information about the content of acute care received in the emergency department and inpatient setting, we refrain from interpreting these increases as positive or negative indications of individuals' access to appropriate care. Because effects do not differ substantively across the sample of first releases vs. all releases, we discuss only first releases here (although the results for both are represented in the tables).

Within the first 30 days of release, 5 percent of first release former prisoners had a Medicaid-paid outpatient visit, .08 percent had an OUD related outpatient visit, and .3 percent had a SUD related visit. Just .02 percent had received medication treatment for OUD. Panel A of Table 7 shows that outpatient visits increased by 30 percentage points, roughly a five-fold increase (600%). Outpatient visits for OUD increased by 1.8 percentage points, nearly 22 times higher than baseline. Outpatient visits for SUD increased by 5 percentage points, 16 times as large as baseline, and medication treatment for OUD increased by .9 p.p., a 44-fold increase. These effects are evident in the reduced forms across all regimes.

We next consider the six months after release, keeping in mind that some of those released towards the end of regime 0 may have (and did) enroll in Medicaid as they later became eligible. In the first six months after release, 18% had an outpatient visit, 2.6 percent had an SUD related visit, .6 percent

received medication treatment for OUD, 8% had an ED visit, and 2% were hospitalized. Table 8 shows that Medicaid coverage increased outpatient visits among first releases in the first six months by 48 percentage points, an increase of 166%. Visits for SUD increased by 13.9 p.p. (187%) and OUD medication increased by 2.8 p.p (467%). Any ED visit increased by 7 percentage points or 88% and hospitalizations by 3 percentage points, an increase of 150%. Overall, these results are consistent with a large increase in access to health care services facilitated by Medicaid eligibility and enrollment assistance.

6.5 Summary of Subgroup Analyses

Table 9 includes the main IV-GMM coefficient from the specification including fixed effects for facilities for subsamples including those with a history of substance use disorder (SUD), and the indicators for types of crime. Recall that the first stage was largely similar across groups (Table 2).

For recidivism at 6 months, a reduction is most evident for those with a violent crime conviction (approximately 4 p.p.) and is least evident for those with SUD risk and/or public order crimes. For those with drug and property crimes, the effect size is similar to all releases (2 p.p.) but is not statistically different from zero at standard calculation levels. For recidivism at 12 months, the conclusion is very similar.

For employment, the largest employment changes are also among those with a history of SUD and public order and violent crimes, while the smallest employment shifts are among those with drug and property crimes. For net earnings, the largest changes are for those with drug crimes, followed by public order, and the smallest are for those with property crimes.

Looking at changes in health care use within 30 days, the largest increase in outpatient visits is among those with a history of substance use disorder, slightly larger than the average effect in the all releases sample. Across most of the health care use measures, changes for those with a history of substance use disorder are larger than average. Those with violent crime convictions consistently see the smallest change in health care use with the exception of any ED visit, when they have the largest increase.

Part of the purpose of the analysis of subgroups was to help us consider whether the hypothesis that the mechanism was increased treatment among those with a history of SUD. Recidivism results for those with identified history of SUD were not substantially different from (and in fact smaller than) average. Earnings and employment results were slightly larger than average. These results are consistent with both treatment and employment mechanisms.

7. OUTCOME BOUNDS AND MARGINAL TREATMENT EFFECT

7.1 Model of Selection into Medicaid under different policy regimes

The IV framework, as mentioned, provides an estimate of the local average treatment effect (LATE)—i.e., the average treatment effect on compliers. If the treatment effect is different for compliers, then the LATE may not be externally valid. In this context, we may be particularly interested in heterogeneity driven by the different policy regimes driving Medicaid takeup: voluntary enrollment, one one's own behalf, relative to an enrollment assistance process. In this section we explore this issue by following the framework of Kowalski (2016) to characterize the compliers, always takers, and never takers of each policy regime. Moreover, the exercise allows us to bound the outcomes of these groups as well as their average Medicaid coverage effects on recidivism and labor market outcomes. These bounds can be used

to forecast the recidivism and labor market outcomes of releases under alternative policies, such as auto-enrollment in Medicaid for eligible formerly incarcerated adults.

Let M be a binary variable indicating Medicaid coverage and Y an observed outcome, such as recidivism, wages, or employment. Let Y_M be the potential outcome of an individual in the state under Medicaid coverage ($M = 1$) and Y_U the potential outcome in the state without coverage ($M = 0$). The following equation relates the potential outcomes to the observed outcome:

(5) $Y = (1 - M) \times Y_U + M \times Y_M$

An individual enrolls into Medicaid if the overall net benefit, B_M, is greater than or equal to zero. The net benefit, B_M, consists of the difference between the observed net benefit p and the unobserved net cost U_M:

(6) $B_M = p - U_M$

For convenience, we follow the literature and normalize the distribution of the unobserved cost U_M as a uniform distribution between 0 and 1. The Medicaid policy change defined by the three policy regimes affect the observed net benefit p. We can define the probability of enrolling in Medicaid under each regime as $p_n \equiv P(M = 1|R_n = 1)$, where R_n is an indicator of regime $n = 0, \ldots, 3$.

Individuals with low unobserved net costs, $0 \leq U_M \leq p_0$, select into Medicaid even under regime 0 ($M = 1$ and $R_0 = 1$), so they are always takers. Meanwhile, individuals with high unobserved costs, $p_n < U_M \leq 1$ for n = 1, 2 or 3, do not select into Medicaid even under regimes 1 to 3 ($M = 0$ and $R_n = 1$), they are never takers. Finally, compliers are defined as the individuals with intermediate costs, $p_0 < U_M \leq p_n$. That is, compliers select into Medicaid under regime n, but not under regime 0.

The first stage identifies the share of compliers under each regime 1 to 3—i.e., by definition $p_n - p_0$ is equal to $\Pr(M = 1|R_n = 1) - \Pr(M = 1|R_0 = 1)$. Note that an individual who is an always taker or never taker in a given regime could be a complier in another regime (or in another alternative policy as well) if there is a different selection mechanism into Medicaid. Thus, the analysis of characteristics of always takers, compliers, and never takers is made by regime. We implement Kowalski's methodology to bound the treatment effect of always takers and never takers specific to each policy regime. In contrast to the main results of the paper, where we treat each regime as a different instrument.

7.2 Characteristics of always takers, compliers, and never takers

The average observable characteristics of always takers, compliers, and never takers can be identified following Katz et al. (2001) and Abadie (2003) which is also discussed in Kowalski (2016). Note that we require the distribution of the unobserved net cost U_M, conditional on $\boldsymbol{X}$, to be the same under every regime. The (conditional) shares of always takers, compliers, and never takers are labeled as p_0 for always takers, $(p_n - p_0)$ for compliers, and $(1 - p_n)$ for never takers under the policy defined by regime $n = 1, 2, 3$. Compliers cannot be observed directly in the data, but their shares allow us to identify their average characteristics.

Individuals who go untreated despite being under regime n identify the average characteristics of never takers: $E(X|M = 0, R_n = 1)$. The average characteristics of regime 0's individuals not covered by Medicaid, $E(X|M = 0, R_0 = 1)$, are a weighted average of the average characteristics of regime 0's

never takers and compliers. Using the shares of never takers and compliers we can identify the average characteristics of regime 0's compliers with:

(7) $\frac{1}{p_n - p_0}[(1 - p_0)E(X|M = 0, R_0 = 1) - (1 - p_n)E(X|M = 0, R_n = 1)]$

Similarly, individuals who enroll in Medicaid under regime 0 identify the average characteristics of always takers: $E(X|M = 1, R_0 = 1)$. The average characteristics of the Medicaid enrollees under regime n= 1, 2, 3, $E(X|M = 1, R_n = 1)$, are a weighted average of the average characteristics of regime n's always takers and compliers. Then, the average characteristics of regime n's compliers are identified with the equation:

(8) $\frac{1}{p_n - p_0}[p_n E(X|M = 1, R_n = 1) - p_0 E(X|M = 1, R_0 = 1)]$

We construct the sample analog of the previous expectation moments for each characteristic using the following equation, where X_i is the characteristic of the release i,

(9) $X_i = \pi_{00} + \sum_{n=1}^{3} \pi_{0n} R_{n(i)} + \pi_{10} M_i + \sum_{n=1}^{3} \pi_{1n} R_{n(i)} \times M_i + \widetilde{\boldsymbol{X}}_i' \phi + \xi_i$

where $R_{n(i)}$ is an indicator that release i happened during regime n, M_i is a Medicaid enrollment indicator, $\widetilde{\boldsymbol{X}}_i$ is the vector of characteristics included in our main IV framework without X_i, ξ_i is an error term, while ϕ amd π_{kn} for $k = 0,1$ and $n = 0, \dots, 3$ are parameters. Then, we can use these parameters to rewrite the expected characteristics of regime 0's compliers as (omitting the vector $\widetilde{\boldsymbol{X}}_i$ for exposition):

(10) $\frac{1}{p_n - p_0}[(1 - p_0)\pi_{00} - (1 - p_n)(\pi_{00} + \pi_{01})]$

and the expected characteristics of regime n's compliers as:

(11) $\frac{1}{p_n - p_0}[p_n(\pi_{00} + \pi_{0n} + \pi_{10} + \pi_{1n}) - p_0(\pi_{00} + \pi_{10})]$

We can obtain a weighted average of the characteristics of all compliers using the shares of releases under regimes 0 and n. Moreover, identification of the parameters in equation (9) provides identification of the average characteristics of always takers and never takers as well.

Table 10 and Figure 3 presents the average characteristics for the always takers, compliers, and never takers. Always takers are in a larger proportion female, white, less educated, younger and slightly fewer months in prison than compliers and never takers. Moreover, always takers have a lower SUD than compliers but similar to never takers. Meanwhile, all groups show similar shares of crime types.

Compliers in regimes 1 and 3 show similar gender composition, marriage status, education, and number of months in prison. Moreover, compliers at regime 3 are white in a larger proportion, younger, have a lower SUD, and have a higher employment to population ratio than compliers in regime 1. Meanwhile, never takers in regimes 1 and 3 show similar composition in terms of gender, race, marriage status, high school graduation, and age. But regime 3 compliers have a higher average of imprisonment months, higher SUD and employment to population shares.

7.3 Identification of Bounds of Outcomes and Treatment Effects

Here, we use the Kowalski (2016) exercise to identify bounds for the marginal treatment effect (MTE). Identification of the $MTE(p)$ follows from the same structure described in the previous section to identify the average observable characteristics of always takers, compliers, and never takers. We can follow the same steps but replacing the characteristics of the releases with the outcomes of interest.

The $MTE(p)$ is defined by the expected difference between the potential outcome of the marginal individual with and without Medicaid—i.e., the marginal individual is the one with an unobserved net cost U_M equal to the observed net benefit p of Medicaid:

(12) $MTE(p) = E(Y_M - Y_U | U_M = p)$

Following Kowalski (2016), the marginal treatment effect is the difference between the marginal treated outcome (MTO) and the marginal untreated outcome (MUO)—i.e., the expected outcome of the marginal individual with and without Medicaid, respectively:

(13) $MTO(p) = E(Y_M | U_M = p)$
(14) $MUO(p) = E(Y_U | U_M = p)$

Meanwhile, the LATE represents the average treatment effect for compliers $E(Y_T - Y_U | p_0 < U_D \leq p_n)$, which is equal to the difference between the compliers outcome with Medicaid coverage, $CO_n^{m=1} = E(Y_T | p_0 < U_D \leq p_n)$ and the compliers outcome without Medicaid coverage $CO_n^{m=0} = E(Y_U | p_0 < U_D \leq p_n)$ under each regime n=1, 2, 3. Define NTO_n^m as the never takers' outcome with Medicaid coverage if $m = 1$ and without coverage if $m = 0$. Similarly, always takers' outcome is ATO_n^m, the weighted average of always takers and compliers' outcomes is $ATCO_n^m$, and the weighted average of never takers and compliers' outcomes is $NTCO_n^m$. We can identify CO_n^0 and CO_n^1 by replacing X in the moments of equations (7) and (8) with an outcome Y:

(15) $CO_n^0 = \frac{1}{p_n - p_0}[(1 - p_0)NTCO_0^0 - (1 - p_n)NTO_n^0]$
(16) $CO_n^1 = \frac{1}{p_n - p_0}[p_n ATCO_n^1 - p_0 ATO_0^1]$

Where weighted average of never takers and compliers outcome in regime 0 without Medicaid is $NTCO_0^0 = E(Y | M = 0, R_0 = 1)$, never takers outcome in regime n without Medicaid is $NTO_n^0 = E(Y | M = 0, R_n = 1)$, always takers outcome in regime 0 with Medicaid is $ATO_0^1 = E(Y | M = 1, R_0 = 1)$, and the weighted average of always takers and compliers outcome in regime n with Medicaid is $ATCO_n^1 = E(Y | M = 1, R_n = 1)$. Table 11 presents the estimates of these expectations as well as the estimates of equations (15) and (16)

Kowalski points out that if we assume that $MTO(p)$ is weakly monotonic in p, then we can bound the average outcome of never takers with Medicaid coverage (NTO_n^1) under each regime n. Note that identification of a lower or upper bound depends on the empirical relationship observed between CO_n^1 and ATO_0^1. The average outcome of compliers under regime n provides an upper bound in the case of recidivism ($NTO_n^1 < CO_n^1 < ATO_0^1$), and a lower bound for the labor market outcomes ($NTO_n^1 \geq CO_n^1 \geq ATO_0^1$).

Similarly, under the assumption that $MUO(p)$ is weakly monotonic in p, then we can bound the average outcome of regime n's always takers without Medicaid coverage (ATO_n^0). The average outcome of compliers under each regime n without Medicaid provides an upper bound ($ATO_n^0 < CO_n^0 < NTO_n^0$) for labor market outcomes and a lower bound ($ATO_n^0 \geq CO_n^0 \geq NTO_n^0$) for recidivism.

The assumption of weak monotonicity implies a natural ordering of always takers, compliers, and never takers. This assumption is not testable but looking at the monotonicity of the average covariates' values from always takers to compliers to never takers can provide certain level of confidence in the weak monotonicity assumption. For regime 3, we see that the ordering for gender composition, white share, education, SUD, months in prison, and employment to population ratio is weekly monotonous. Meanwhile, monotonicity does not hold for marriage status and age, which probably is the result of the eligibility conditions before and after the policy.

Kowalski provides a test of global external validity using the bounds on outcomes. However, if $MUO(p)$ and $MTO(p)$ are both weakly decreasing or increasing, as in the case of all our outcomes, then the implied bounds on the treatment effects are not informative about global external validity. Nevertheless, these bounds can be used to forecast the recidivism and labor market outcomes associated with alternative policies, such as Medicaid auto-enrollment of eligible formerly incarcerated adults. This requires setting bounds on always takers (ATTE) and never takers (NTTE) treatment effects of Medicaid coverage.

The bounds for the MTE of always takers and never takers for each regime are presented in Table 12 and Figure 4. For example, with respect to regime 3, the bounds on the ATTE imply that always takers' recidivism decreases at least 0.01 p.p. under Medicaid coverage—i.e., ATTE$\leq ATO_0^1 - CO_1^0$=0.16-0.17 since, under weakly monotonicity, $ATO_0^0 \geq CO_1^0$. The bounds on the NTTE imply that never takers have a Medicaid coverage effect on recidivism below 0.006 p.p.—i.e., NTTE<$CO_n^1 - NTO_n^0$= 0.121−0.115 since, under weakly monotonicity, $NTO_n^1 < CO_1^1$. Similarly, the bounds for 12 months recidivism imply a treatment effect below -0.02 and 0.03 p.p. for always takers and never takers, respectively. Meanwhile, for the labor market outcomes the bounds imply that the treatment effect on wages and employment for always takers are above $71 and -0.02 p.p., respectively. And the treatment effects on wages and employment for never takers are above -$230 and 0.04 p.p., respectively.

In the next section, under additional functional form assumptions, we provide more precise treatment effects on always takers and never takers.

7.4 Identification of Marginal Treatment Effect

In this section we present the identification framework for and estimates of the Marginal Treatment Effect (MTE). Identification of the MTE provides the Medicaid effect on always takers and never takers, and so, we can explore the effect on recidivism and labor market outcomes of alternative policies, such as the Medicaid auto-enrollment at release for eligible formerly incarcerated adults. Brinch et al. (2017) identify the MTE under the assumption of linearity. Similarly, Kowalski (2016) assumes that $MUO(p)$ and $MTO(p)$ are linear and so is MTE(p). Note that in our framework we observe a large change in Medicaid coverage due to the policy. This makes the linearity functional form assumption somewhat weaker than in contexts that implement this assumption with instruments that have small changes in participation.

Under the assumption of linearity, comparison of the observed outcomes of always takers (ATO_0^1) and compliers (CO_n^1) with Medicaid coverage identifies the slope of the marginal treated outcome function $MTO(p)$ from 0 to p_n. From Table 11 we see that the slope is negative for recidivism (both 6 and 12 months). This could be the result of individuals with higher recidivism rate selecting into Medicaid or a decreasing average treatment effect as Medicaid coverage increases. Meanwhile, the slope for wages and employment is positive, suggesting selection of individuals with worse labor market outcomes or decreasing average treatment effect of Medicaid on wages and employment as coverage is increased.

Note that the average slope of $MTO(p)$ from p_n to 1 is not identified since we do not observe the outcome of the never takers under Medicaid coverage. Moreover, the slope of $MUO(p)$ from 0 to p_0 is not identified because ATO_0^0 is not observed. Thus, extrapolation out of the support requires relying on the parametric functional form assumption.

Under linearity of these functions, $MUO(p)$ is identify with the points $([p_0 + p_n]/2, CO_n^0)$ and $([p_n + 1]/2, NTO_n^0)$:

$$(17)\ MUO(p) = \frac{(1+p_n)NTCO_0^0 - (1+p_0)NT_n^0}{p_n - p_0} + \frac{2(NT_n^0 - NTCO_0^0)}{p_n - p_0}p$$

while $MTO(p)$ is identified with the points $(p_0/2, ATO_0^1)$ and $([p_0 + p_n]/2, CO_n^1)$:

$$(18)\ MTO(p) = ATO_0^1 - \frac{p_0}{p_n - p_0}(ATCO_n^1 - ATO_0^1) + \frac{2(ATCO_n^1 - TO_0^1)}{p_n - p_0}p$$

Finally, the $MTE(p)$ is identified as the difference between $MUO(p)$ and $MTO(p)$:

$$(19)\ MTE(p) = \frac{1}{p_n - p_0}[p_n(ATO_0^1 - NTCO_0^0) + p_0(NT_n^0 - ATCO_n^1) + (NT_n^0 - NTCO_0^0)] + \frac{2}{p_n - p_0}[(ATCO_n^1 - NTO_n^1) - (ATO_0^1 - NTCO_0^0)]p$$

The results are presented in Table 13 and Figure 4. The slopes of the MUO and MTO are negative for recidivism and positive for the labor market outcomes. However, the slopes of the MTE are positive for recidivism, but in the case of labor market outcomes, the slope of the MTE is negative for wages and positive for employment.

The positive slope of the MTE for recidivism implies that the Medicaid effect on never takers is smaller in magnitude. Nevertheless, in the case of 6-month recidivism the slope is close to zero and the MTE is quite flat which implies that the estimated LATE is globally externally valid (under the linearity assumption). In the case of recidivism at 12 months the MTE decreases in magnitude and approaches zero with full coverage. This decreases of the treatment effect for recidivism at 12 months can be consequence of the data construction. We are using Medicaid enrollment at the month of release, but as time passes after the release, a large proportion of formerly incarcerated adults enroll in Medicaid in subsequent months. Thus, the lack of effect could be the result of these individuals receiving the treatment by 12 months after the release.

The slopes of the MTE on wages and employment suggest that the LATE on those outcomes is not globally externally valid. The MTE on wages suggest that the average effect on never takers—i.e., those without Medicaid coverage after the policy implementation—is close to zero. Meanwhile, the average

effect on employment for never takers is around 0.14 p.p. (-0.017+0.184(0.68+1)/2), substantially above the LATE.

The exercise suggests that if Medicaid auto-enrollment on eligible formerly incarcerated adults were implemented at the time of release, the additional enrolled individuals will face a decrease in 6-months recidivism and an increase in employment, but a smaller average effect on wages.

8. CONCLUSION

Approximately 1.2 million individuals are incarcerated in state prisons in the United States (Carson 2020). Their return to the community from prison is characterized by financial hardship, unstable housing, acute health care events, and limited social support (Mallik-Kane and Visher, 2008; Western et al., 2015; Harding et al., 2013). Faced with these challenges, it is perhaps unsurprising that 17.5% of individuals are reincarcerated within 1 year and 36% within 3-years (Durose et al., 2014). Interrupting this cycle and supporting a sustained return to the community is a widely held goal among policy-makers and advocates alike (National Reentry Resource Center, 2014; Subramanian et al., 2020). Identifying the policies and interventions that help individuals desist criminal activity and thrive in their communities is central to achieving this objective. In this study we focused on one such policy, the provision of public health insurance.

Using a natural experiment from one state, we showed large increases in Medicaid enrollment at the time of release resulting from both Medicaid eligibility expansion to the larger population and a specific enrollment assistance program targeted those leaving state prison facilities. We also examined take-up of Medicaid under the different policies by observed characteristics to examine whether the population of compliers was different under voluntary independent enrollment and enrollment assistance.

We examined the effects of Medicaid coverage upon release from state prison on reincarceration, and two channels by which Medicaid coverage may influence reincarceration, a financial and a health channel. While the magnitude and precision of point estimates varies across model specifications and subgroups, Medicaid coverage generally reduced the likelihood of reincarceration at 6- and 12-months. Among all releases, the implied effect sizes ranged from a 11% relative reduction at 6-months to a 44% relative reduction at 12-months. The magnitude of effects was larger when the sample was restricted to first releases with a relative reduction of 28% at 6-months and 17% at 12-months. To put these results into context, the estimated impact of expanded Medicaid eligibility on recidivism (including rearrests, convictions or reincarceration) varies from about a 0 to 5% relative reduction (Wen et al., 2017b; Aslim et al., 2019; Vogler 2020; He and Barkowski 2020). We would expect that if Medicaid has an impact on recidivism, the effect of enrollment would exceed that of eligibility as we see in our results. Our strong evidence that Medicaid enrollment decreases individual recidivism adds to the evidence that post-release support can make a difference in desistance from crime.

In terms of how Medicaid is operating on decisions, we found robust evidence that Medicaid coverage increases the likelihood of employment and earnings post-incarceration suggesting the effects on reincarceration operate at least in part through a financial channel. For all releases as well as first releases, Medicaid coverage increased the likelihood of employment during the 12-months post release by about 5 percentage points, with a corresponding increase in net earnings in the quarter of release of roughly $200.

We also found evidence that Medicaid enrollment within the month of release from prison increased the likelihood of Medicaid-paid health care use across each of the outcomes measured within 30-days of release and within 6-months of release. These increases included outpatient care for any cause as well as SUD- and OUD-specific outpatient care. Medication for OUD likewise increased, an established efficacious treatment to prevent relapse and overdose (Johnson et al., 1992; Petitjean et al., 2001). The implied effect sizes, relative to baseline, were in most cases quite large. The likelihood of any inpatient or emergency department within 6-months of release also increased. With the exception of the medication for OUD outcome, we cannot discern whether the outpatient care obtained was therapeutic, diagnostic or preventive in nature. Similarly, without further information about the content of acute care received in the emergency department and inpatient setting, we refrain from interpreting these increases as positive or negative indications of individuals' access to appropriate care. Nonetheless, our findings demonstrate that Medicaid coverage facilitated access to care during a time period of heightened vulnerability including evidence-based care indicated for OUD, the substance responsible for most drug overdose deaths in the country (Hedegaard, Minino, Warner 2020).

Together, these results could be interpreted in several ways. It could be that by facilitating formal employment, Medicaid makes desistance more attractive and time use shifts away from criminal activity; the increases in health care use may be purely incidental. Alternatively, the Medicaid-induced increase in use of treatment for SUDs and health care more generally, may enable desistance by affecting underlying symptoms that facilitated criminal behavior; the positive effects on employment may then be a function of both these treatment effects and time use. Next steps in our analysis will continue to work towards separately accounting for these channels.

Our findings should be considered in light of the study's limitations. The specific features of this state's Medicaid program and prerelease Medicaid enrollment program may limit the generalizability of our findings to other states' prison populations. For example, the Medicaid income eligibility threshold for most non-pregnant adults in Wisconsin is 100% of the federal poverty level (FPL) which is generally higher than other non-ACA expansion states and lower than the 138% FPL cut off in ACA expansion states. To the extent that the impact of Medicaid enrollment on the study's outcomes is non-constant across the income distribution, the marginal effect of coverage may vary by state. This study lacks a contemporaneous comparison group and is thus subject to potential confounding particularly due to changes in economic conditions. We mitigate this possibility by controlling for local labor market demand but recognize the possibility of residual confounding. With respect to our health care use outcomes we observe only health care use that is paid by Medicaid. Our results may overstate the effect of Medicaid on health care use if individuals who do not take up Medicaid obtain care paid by other sources (e.g., private insurance, charity care, etc.). We think this scenario is unlikely given persistent barriers to health care among uninsured adults even within publicly funded health centers (Seo et al., 2019; Friedmann et al., 2003; Gryczynski et al., 2011), and the relatively low rate of private insurance coverage among recently incarcerated adults (Mallik-Kane et al., 2018; Mallik-Kane and Visher, 2008).

Because of the state-based and ACA-related Medicaid expansions (Antonisse and Rudowitz, 2019), the large majority of adults leaving prison are income eligible for Medicaid (Western and Smith, 2018). This study's findings indicate that the value of Medicaid coverage for recently incarcerated adults may extend well beyond access to health care services. Further, it highlights the important role that facilitating enrollment, to ensure coverage before release, plays in distributing the benefits of Medicaid more widely the population of adults leaving prison. Prerelease enrollment assistance is unevenly available by state and correctional setting suggesting an opportunity for intervention to support a successful reentry to the community.

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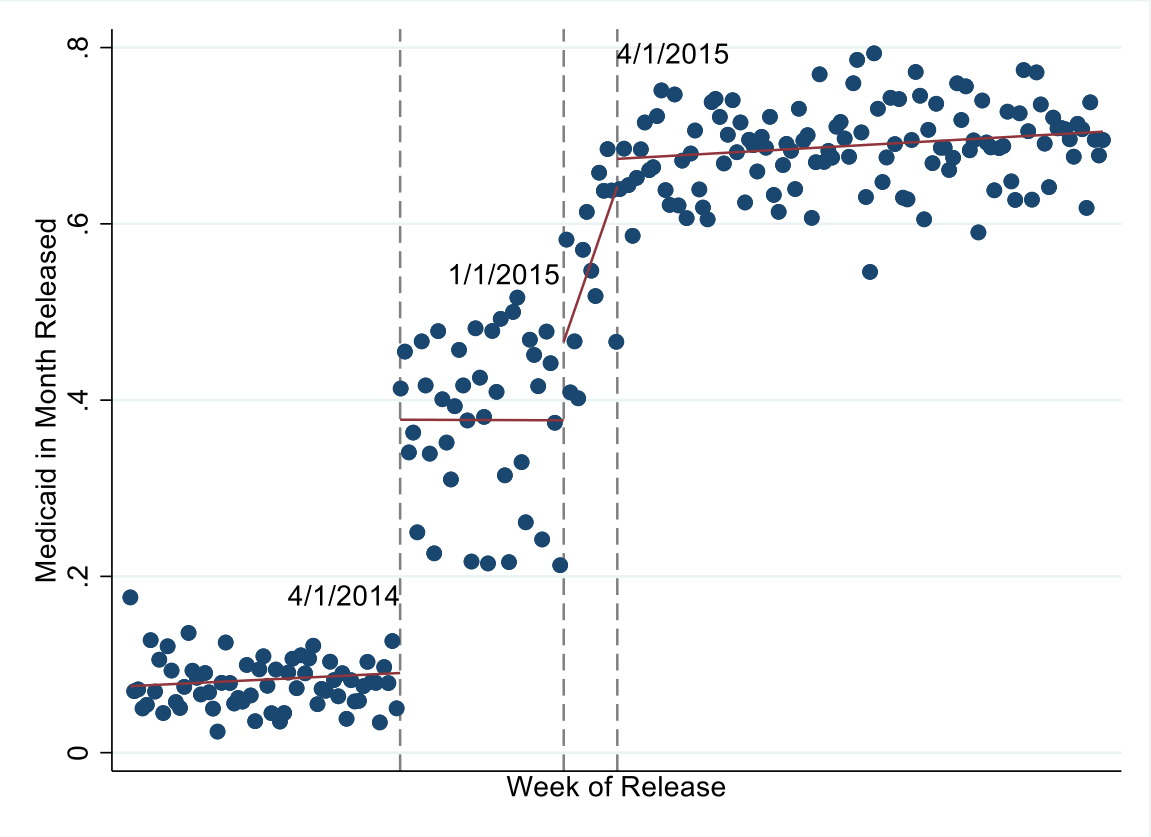
TABLES AND FIGURES

Figure 1. Probability of Medicaid Enrollment at Release



Source: Authors' calculations from Wisconsin administrative data. Notes: Figure shows fraction of releases with Medicaid enrollment in the month of their release, binned by week of release. Bins do not cross regimes. Linear trend superimposed.

Table 1. Summary of Release Characteristics and Omnibus Balance Test

	First Releases					All Releases				
		Regime Average			Normalized		Regime Average			Normalized
Characteristic	Regime 0	Regime1	Regime2	Regime 3	Balance	Regime 0	Regime1	Regime2	Regime 3	Balance
Female	8.1%	8.2%	9.8%	11.1%	0.0714	7.9%	7.9%	9.3%	9.8%	0.0454
Age 18-23	7.1%	5.9%	5.2%	6.4%	-0.0473	7.4%	6.6%	6.2%	7.6%	-0.0117
Age 24-35	36.1%	35.8%	33.7%	34.0%	-0.0340	36.1%	36.7%	35.1%	36.6%	0.0095
Age 36-55	46.8%	47.0%	49.4%	46.9%	0.0113	46.6%	46.0%	47.8%	44.8%	-0.0226
Age 55+	10.0%	11.3%	11.7%	12.8%	0.0727	9.9%	10.7%	10.8%	11.0%	0.0324
American Indian	3.7%	4.4%	4.5%	4.3%	0.0239	3.8%	4.3%	4.4%	4.5%	0.0274
Asian or Pacific Islander	0.8%	0.9%	1.1%	1.0%	0.0235	0.8%	0.8%	0.9%	0.9%	0.0128
Black	42.0%	38.4%	38.2%	36.6%	-0.0935	42.6%	40.0%	39.3%	38.8%	-0.0633
White	53.4%	56.2%	56.1%	58.1%	0.0772	52.9%	54.8%	55.3%	55.8%	0.0486
Education Level	1.159	1.034	1.007	0.972	-0.0890	1.173	1.072	1.071	1.020	-0.0676
Married	9.6%	9.6%	10.1%	10.7%	0.0225	9.6%	9.4%	9.7%	9.6%	-0.0019
Months Incarcerated	24.84	28.80	31.00	32.93	0.1763	24.15	25.57	26.45	26.39	0.0490
Substance Use Risk Flag	57.7%	55.5%	55.6%	60.6%	-0.0199	57.7%	56.7%	57.0%	61.8%	-0.0086
Violent Crime	42.3%	41.4%	43.9%	41.5%	-0.0351	42.2%	41.6%	43.8%	42.0%	-0.0110
Property Crime	32.5%	32.8%	29.9%	30.6%	0.0633	32.7%	32.8%	31.6%	32.4%	0.0459
Public Order Crime	42.7%	44.1%	44.2%	46.9%	0.0370	42.1%	42.9%	42.8%	45.5%	0.0253
Drug Crime	26.5%	26.8%	25.8%	28.6%	0.0229	26.2%	26.3%	24.9%	27.6%	0.0528

Source: Authors' calculations from Wisconsin administrative data. Notes: Table shows average characteristics by regime and the results of a normalized balance test comparing Regime 0 to the other regimes.

Table 2. Medicaid Enrollment at Release (First Stage)

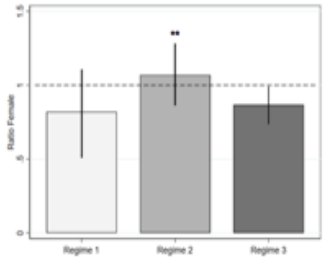
				Type of Crime			
	First Release	All Releases	SUD Risk	Drug	Public Order	Property	Violent
Regime 1	0.289***	0.300***	0.314***	0.247***	0.310***	0.317***	0.324***
	(0.00759)	(0.00660)	(0.00905)	(0.0131)	(0.0102)	(0.0117)	(0.0106)
Regime 2	0.457***	0.476***	0.491***	0.445***	0.478***	0.475***	0.527***
	(0.0130)	(0.0113)	(0.0150)	(0.0226)	(0.0174)	(0.0201)	(0.0170)
Regime 3	0.584***	0.611***	0.624***	0.602***	0.628***	0.634***	0.619***
	(0.00767)	(0.00428)	(0.00572)	(0.00867)	(0.00645)	(0.00765)	(0.00679)
Observations	31104	40721	23418	10585	17482	12901	16336
F-statistic	5021.2	7038.1	4076.1	1650.6	3235.4	2343.8	2901.1

Notes: Estimates from a model that includes controls for age, race, education, marital status, duration of incarceration, type of crime, calendar month of release, and fixed effects for facility

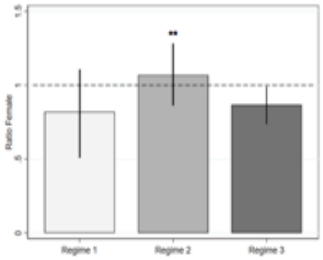
Figure 2: Average characteristics of compliers

Sample of first releases

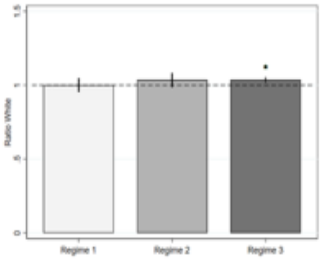
Female



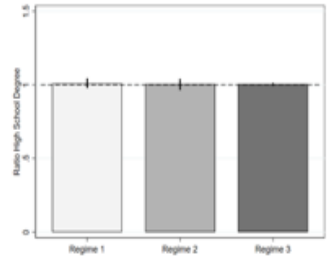
Married



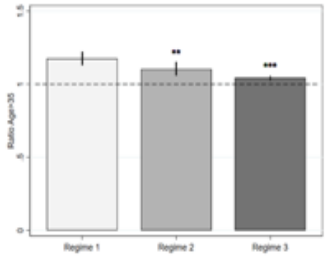
White



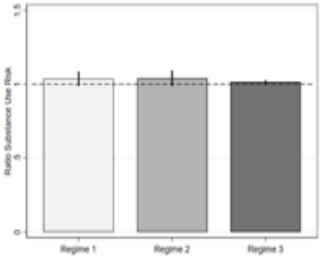
High school



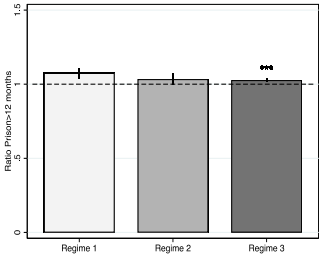
Age>35



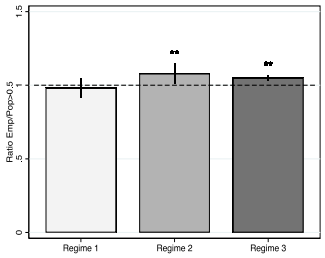
Substance Use Risk



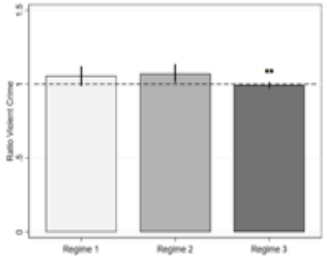
Months in prison



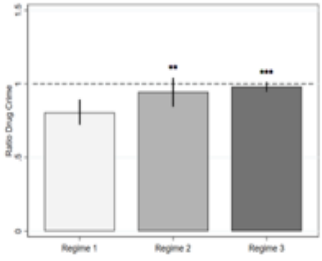
Emp/pop



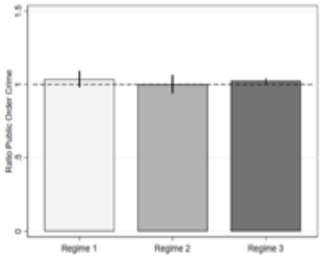
Violent crime



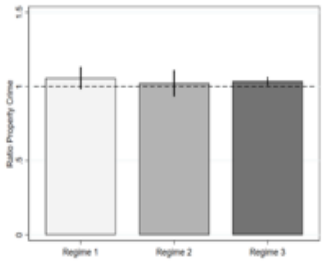
Drug crime



Public order crime



Property crime



Source: Authors' calculations from Wisconsin administrative data.
Notes: The table reports the analysis of compliers characteristics for each regime. The ratios in columns 3, 5, and 7 give the relative likelihood compliers have the characteristics indicator in each row. Bootstrap 95% confidence intervals at individual level in brackets.
Stars at the top on confidence intervals in regime 2 and 3 indicate that their difference with regime 1 is statistically significant. *, **, *** statistically significant at 10%, 5%, 1%, respectively.

Table 3. IV Results, Reincarcerated Within 6 Months

Panel A.	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form
Sample: First Releases									
Medicaid	-0.00573	-0.0545***		-0.00318	-0.0528**		-0.00389	-0.0440**	
	(0.00383)	(0.00741)		(0.00386)	(0.00756)		(0.00390)	(0.00755)	
Regime 1			-0.0290***			-0.0280***			-0.0252***
			(0.00590)			(0.00595)			(0.00592)
Regime 2			-0.0303**			-0.0310***			-0.0276**
			(0.00925)			(0.00930)			(0.00923)
Regime 3			-0.0342***			-0.0330***			-0.0276***
			(0.00455)			(0.00464)			(0.00462)
Facility Fixed Effects							X	X	X
Employment/Population				X	X	X	X	X	X
Observations		31408			31104			31097	
First Stage F-statistic		5268.9			5038.2			5015.5	
Panel B.									
Sample: All Releases									
Medicaid	0.00140	-0.0238***		0.00632	-0.0170*		0.00130	-0.0175*	
	(0.00366)	(0.00700)		(0.00372)	(0.00717)		(0.00371)	(0.00708)	
Regime 1			-0.0165**			-0.0141*			-0.0153**
			(0.00582)			(0.00588)			(0.00581)
Regime 2			-0.0176*			-0.0173			-0.0174*
			(0.00888)			(0.00895)			(0.00895)
Regime 3			-0.0158***			-0.0114*			-0.0118**
			(0.00442)			(0.00453)			(0.00446)
Facility Fixed Effects							X	X	X
Employment/Population				X	X	X	X	X	X
Observations		39504			39186			39185	
First Stage F-statistic		6906.3			6689.5			6593.4	

Notes: Authors' calculations from Wisconsin administrative data. Controls for age, race, education, marital status, duration of incarceration, type of crime, calendar month of release.

Table 4. IV Results, Reincarcerated Within 12 Months

Panel A.	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form
Sample: First Releases									
Medicaid	0.00565	-0.0659***		0.00920	-0.0640**		0.00913	-0.0500**	
	(0.00493)	(0.00943)		(0.00497)	(0.00962)		(0.00501)	(0.00958)	
Regime 1			-0.0372***			-0.0365***			-0.0330***
			(0.00750)			(0.00756)			(0.00751)
Regime 2			-0.0342**			-0.0359**			-0.0313**
			(0.0119)			(0.0120)			(0.0119)
Regime 3			-0.0414***			-0.0400***			-0.0315***
			(0.00577)			(0.00588)			(0.00585)
Facility Fixed Effects							X	X	X
Employment/Population				X	X	X	X	X	X
Observations		31408			31104			31097	
First Stage F-statistic		5268.9			5038.2			5015.5	
Panel B.									
Sample: All Releases									
Medicaid	0.0152***	-0.0217*		0.0210***	-0.0145		0.0157***	-0.0133	
	(0.00451)	(0.00859)		(0.00457)	(0.00878)		(0.00456)	(0.00867)	
Regime 1			-0.0210**			-0.0187**			-0.0200**
			(0.00712)			(0.00717)			(0.00711)
Regime 2			-0.0172			-0.0178			-0.0176
			(0.0111)			(0.0112)			(0.0110)
Regime 3			-0.0150**			-0.0103			-0.00968
			(0.00541)			(0.00553)			(0.00544)
Facility Fixed Effects							X	X	X
Employment/Population				X	X	X	X	X	X
Observations		39504			39186			39185	
First Stage F-statistic		6906.3			6692.4			6589.1	

Notes: Authors' calculations from Wisconsin administrative data. Controls for conviction county, age, race, education, marital status, duration of incarceration, type of crime, calendar month of release.

Table 5. IV Results, Employment

Panel A.	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form
Sample: First Releases									
Medicaid	0.00512	0.0567***		-0.00130	0.0583***		0.0237***	0.0513***	
	(0.00474)	(0.00887)		(0.00478)	(0.00902)		(0.00467)	(0.00872)	
Regime 1			0.0105			0.0133			0.0120
			(0.00715)			(0.00720)			(0.00694)
Regime 2			0.00941			0.0123			0.00857
			(0.0105)			(0.0105)			(0.0102)
Regime 3			0.0353***			0.0360***			0.0321***
			(0.00544)			(0.00553)			(0.00535)
Facility Fixed Effects							X	X	X
Employment/Population				X	X	X	X	X	X
Observations		31408			31104			31097	
First Stage F-statistic		5257.6			5060.7			5035.2	
Panel B.									
Sample: All Releases									
Medicaid	0.0154***	0.0558***		0.00799	0.0544***		0.0328***	0.0578***	
	(0.00413)	(0.00772)		(0.00416)	(0.00783)		(0.00406)	(0.00760)	
Regime 1			0.00554			0.00526			0.00907
			(0.00660)			(0.00663)			(0.00640)
Regime 2			0.0137			0.0164			0.0167
			(0.00936)			(0.00936)			(0.00910)
Regime 3			0.0339***			0.0325***			0.0354***
			(0.00487)			(0.00494)			(0.00478)
Facility Fixed Effects							X	X	X
Employment/Population				X	X	X	X	X	X
Observations		39504			39186			39185	
First Stage F-statistic		6908.2			6692.1			6588.5	

Notes: Authors' calculations from Wisconsin administrative data. Controls for conviction county, age, race, education, marital status, duration of incarceration, type of crime, calendar month of release, and fraction of quarter remaining.

Table 6. IV Results, Net Earnings

Panel A.	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form
Sample: First Releases									
Medicaid	-194.6***	208.3***		-211.8***	219.0***		-89.69***	205.5***	
	(14.83)	(28.23)		(15.12)	(28.95)		(13.18)	(26.98)	
Regime 1			9.698			14.35			17.72
			(21.92)			(22.00)			(20.70)
Regime 2			26.48			36.59			23.39
			(34.38)			(34.57)			(32.10)
Regime 3			133.4***			139.4***			131.6***
			(17.26)			(17.69)			(16.46)
Facility Fixed Effects							X	X	X
Employment/Population				X	X	X	X	X	X
Observations		31408			31104			31097	
First Stage F-statistic		5268.9			5102.6			5021.2	
Panel B.									
Sample: All Releases									
Medicaid	-157.5***	184.0***		-178.4***	178.9***		-60.99***	199.1***	
	(12.70)	(23.59)		(12.91)	(24.07)		(11.22)	(22.51)	
Regime 1			0.558			-0.406			12.38
			(19.66)			(19.73)			(18.56)
Regime 2			50.83			58.17			56.87*
			(30.09)			(30.22)			(28.50)
Regime 3			112.1***			108.3***			121.8***
			(14.71)			(15.02)			(14.00)
Facility Fixed Effects							X	X	X
Employment/Population				X	X	X	X	X	X
Observations		39504			39186			39185	
First Stage F-statistic		6906.3			6692.4			6589.1	

Notes: Authors' calculations from Wisconsin administrative data. Controls for conviction county, age, race, education, marital status, duration of incarceration, type of crime, calendar month of release, and month of quarter.

Table 7. IV Results, Health Care Use in First 30 Days

Outcome	Any Outpatient Visit			Outpatient, OUD			Outpatient, SUD			Any OUD Medication		
Panel A.	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form
Sample: First Releases												
Medicaid	0.246***	0.295***		0.0139***	0.0178***		0.0381***	0.0494***		0.00670***	0.00853***	
	(0.00431)	(0.00675)		(0.00107)	(0.00158)		(0.00185)	(0.00265)		(0.000741)	(0.00104)	
Regime 1			0.0993***			0.00546***			0.0194***			0.00245***
			(0.00540)			(0.00112)			(0.00215)			(0.000681)
Regime 2			0.128***			0.00352			0.0204***			0.000607
			(0.0102)			(0.00210)			(0.00434)			(0.00121)
Regime 3			0.177***			0.0119***			0.0291***			0.00678***
			(0.00434)			(0.00109)			(0.00174)			(0.000784)
Facility Fixed Effects	X	X	X	X	X	X	X	X	X	X	X	X
Employment/Population	X	X	X	X	X	X	X	X	X	X	X	X
Observations		31097			31097			31097			31097	
First Stage F-statistic		5015.5			5021.2			5015.5			5015.5	
Panel B.												
Sample: All Releases												
Medicaid	0.241***	0.292***		0.0140***	0.0165***		0.0367***	0.0471***		0.00718***	0.00839***	
	(0.00377)	(0.00590)		(0.000934)	(0.00130)		(0.00158)	(0.00223)		(0.000661)	(0.000853)	
Regime 1			0.100***			0.00517***			0.0187***			0.00248***
			(0.00506)			(0.00105)			(0.00197)			(0.000664)
Regime 2			0.135***			0.00166			0.0187***			-0.000195
			(0.00932)			(0.00179)			(0.00382)			(0.00115)
Regime 3			0.179***			0.0117***			0.0289***			0.00688***
			(0.00381)			(0.000907)			(0.00147)			(0.000654)
Facility Fixed Effects	X	X	X	X	X	X	X	X	X	X	X	X
Employment/Population	X	X	X	X	X	X	X	X	X	X	X	X
Observations		39185			39185			39185			39185	
First Stage F-statistic		6593.4			6593.4			6593.4			6593.4	

Notes: Authors' calculations from Wisconsin administrative data. Controls for conviction county, age, race, education, marital status, duration of incarceration, type of crime, and calendar month of release.

Table 8. IV Results, Health Care Use in First 6 Months

Outcome	Any Outpatient Visit			Outpatient, SUD			Any OUD Medication			Any ED Visit			Any Hospitalization		
Panel A.	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form	OLS	IV (GMM)	Reduced Form
Sample: First Releases															
Medicaid	0.396***	0.486***		0.102***	0.139***		0.0219***	0.0281***		0.0643***	0.0705***		0.0241***	0.0300***	
	(0.00525)	(0.00912)		(0.00328)	(0.00515)		(0.00157)	(0.00247)		(0.00381)	(0.00641)		(0.00210)	(0.00354)	
Regime 1			0.226***			0.0542***			0.00966***			0.0873***			0.0155***
			(0.00757)			(0.00409)			(0.00177)			(0.00559)			(0.00281)
Regime 2			0.206***			0.0483***			0.00112			0.0358***			0.00857
			(0.0130)			(0.00748)			(0.00291)			(0.00934)			(0.00483)
Regime 3			0.293***			0.0836***			0.0193***			0.0421***			0.0182***
			(0.00578)			(0.00326)			(0.00161)			(0.00388)			(0.00217)
Facility Fixed Effects	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Employment/Population	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Observations		31097			31097			31097			31097				
First Stage F-statistic		5015.5			5021.2			5015.5			5015.5				
Panel B.															
Sample: All Releases															
Medicaid	0.383***	0.473***		0.0974***	0.133***		0.0240***	0.0317***		0.0585***	0.0715***		0.0278***	0.0371***	
	(0.00465)	(0.00802)		(0.00285)	(0.00442)		(0.00142)	(0.00217)		(0.00342)	(0.00575)		(0.00191)	(0.00321)	
Regime 1			0.222***			0.0513***			0.00909***			0.0858***			0.0163***
			(0.00709)			(0.00378)			(0.00168)			(0.00527)			(0.00269)
Regime 2			0.216***			0.0556***			0.00312			0.0380***			0.0103*
			(0.0117)			(0.00683)			(0.00284)			(0.00844)			(0.00450)
Regime 3			0.296***			0.0817***			0.0218***			0.0460***			0.0234***
			(0.00516)			(0.00280)			(0.00142)			(0.00356)			(0.00200)
Facility Fixed Effects	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Employment/Population	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Observations		39186			39185										
First Stage F-statistic		6692.4			6589.1										

Notes: Authors' calculations from Wisconsin administrative data. Controls for conviction county, age, race, education, marital status, duration of incarceration, type of crime, and calendar month of release.

Table 9. IV Results for Selected SubSamples

		Type of Crime			
	SUD History	Drug	Public Order	Property	Violent
Reincarcerated within 6 months	-0.00279	-0.0187	-0.00155	-0.0212	-0.0348**
	(0.00895)	(0.0128)	(0.00953)	(0.0125)	(0.0110)
Reincarcerated within 12 months	-0.00753	-0.0171	-0.00260	-0.0156	-0.0346*
	(0.0111)	(0.0166)	(0.0124)	(0.0153)	(0.0134)
Employed in quarter of release	0.0628***	0.0487**	0.0626***	0.0497***	0.0680***
	(0.00973)	(0.0151)	(0.0113)	(0.0130)	(0.0116)
Earnings in quarter of release	214.0***	253.1***	235.1***	135.8***	207.8***
	(28.53)	(64.59)	(35.49)	(35.68)	(33.95)
Within 30 Days					
Any Outpatient Visit	0.315***	0.306***	0.293***	0.303***	0.267***
	(0.00778)	(0.0116)	(0.00877)	(0.00999)	(0.00884)
Outpatient, OUD	0.0237***	0.0304***	0.0116***	0.0196***	0.00815***
	(0.00199)	(0.00342)	(0.00170)	(0.00252)	(0.00133)
Outpatient, SUD	0.0637***	0.0635***	0.0515***	0.0485***	0.0277***
	(0.00339)	(0.00487)	(0.00362)	(0.00392)	(0.00266)
Any OUD Medication	0.0119***	0.0140***	0.00591***	0.00935***	0.00527***
	(0.00131)	(0.00200)	(0.00100)	(0.00168)	(0.000992)
Within 6 Months					
Any Outpatient Visit	0.496***	0.487***	0.469***	0.485***	0.448***
	(0.0103)	(0.0158)	(0.0118)	(0.0135)	(0.0123)
Outpatient, SUD	0.178***	0.167***	0.144***	0.140***	0.0819***
	(0.00646)	(0.00958)	(0.00701)	(0.00773)	(0.00571)
Any OUD Medication	0.0416***	0.0517***	0.0210***	0.0493***	0.0137***
	(0.00328)	(0.00540)	(0.00303)	(0.00432)	(0.00233)
Any ED Visit	0.0797***	0.0579***	0.0712***	0.0768***	0.0853***
	(0.00742)	(0.0108)	(0.00827)	(0.0101)	(0.00887)
Any Hospitalization	0.0435***	0.0321***	0.0293***	0.0425***	0.0388***
	(0.00444)	(0.00562)	(0.00471)	(0.00580)	(0.00459)
Observations	23418	10585	17482	12901	16336
First stage F-statistic	4086.8	1648.5	3237.6	2355.1	2915.5

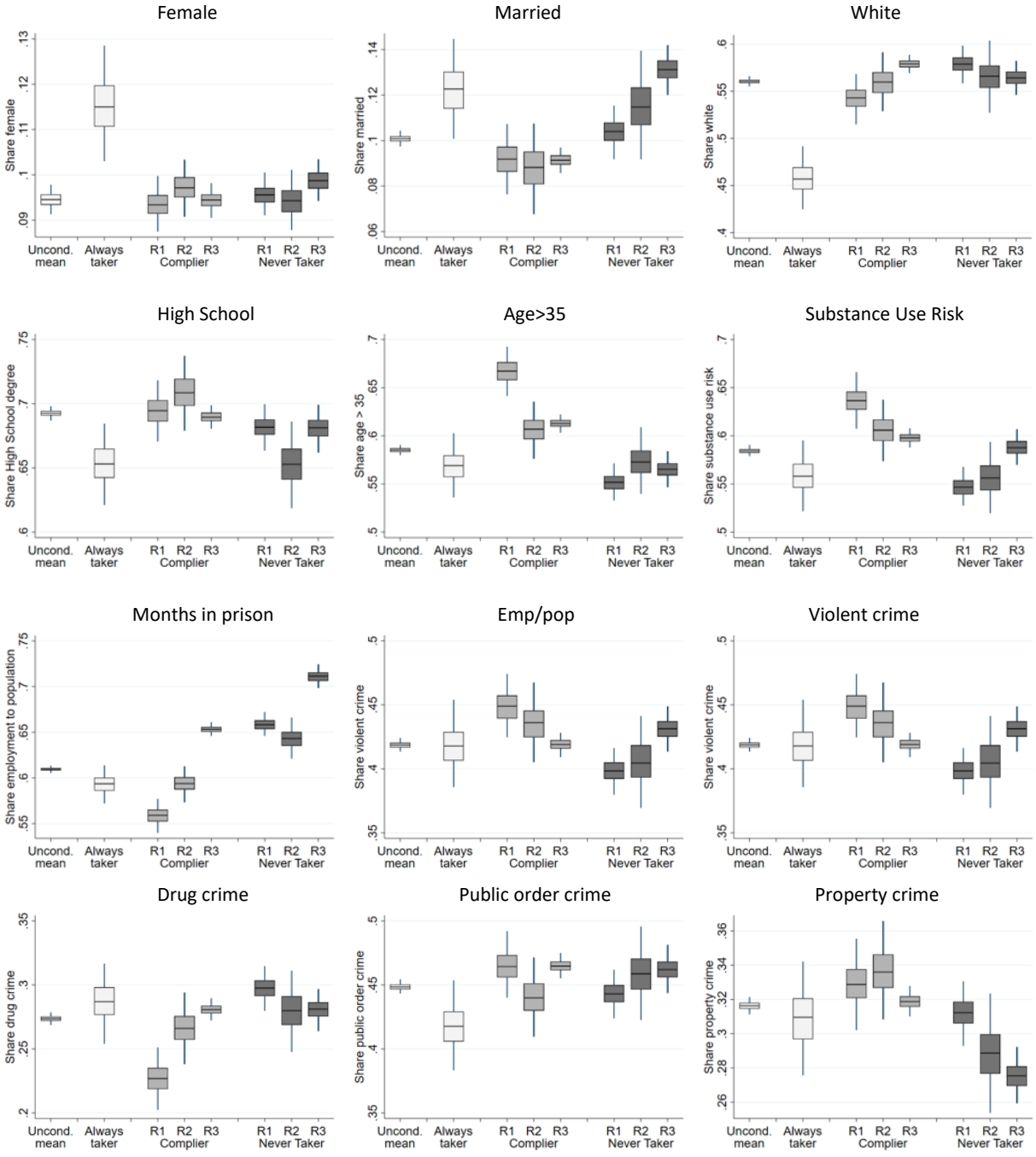
Source: Authors' estimates from Wisconsin administrative data. Notes: IV GMM estimates from a model that includes controls for age, race, education, marital status, duration of incarceration, type of crime, calendar month of release, and fixed effects for facility; employment and earnings models control for fraction of quarter relative to release.

Table 10: Characteristics of Always takers, Compliers, and Never takers
Sample of first releases

	Uncond. Mean	Always takers	Compliers			Never takers		
			Regime 1	Regime 2	Regime 3	Regime 1	Regime 2	Regime 3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	0.095	0.115	0.093	0.097	0.094	0.096	0.094	0.099
	[0.091;0.098]	[0.103;0.128]	[0.087;0.100]	[0.091;0.103]	[0.091;0.098]	[0.091;0.100]	[0.088;0.101]	[0.094;0.103]
Age>35	0.585	0.569	0.667	0.607	0.613	0.552	0.573	0.565
	[0.580;0.590]	[0.536;0.602]	[0.642;0.692]	[0.576.635]	[0.603;0.622]	[0.533;0.571]	[0.540;0.609]	[0.547;0.584]
White	0.560	0.457	0.543	0.560	0.579	0.579	0.566	0.564
	[0.555;0.566]	[0.425;0.491]	[0.515;0.568]	[0.529;0.591]	[0.570;0.588]	[0.559;0.598]	[0.528;0.604]	[0.546;0.582]
High school	0.693	0.653	0.695	0.709	0.689	0.682	0.653	0.681
	[0.687;0.698]	[0.621;0.684]	[0.671;0.718]	[0.679;0.737]	[0.681;0.699]	[0.664;0.700]	[0.619;0.686]	[0.662;0.699]
Married	0.101	0.123	0.092	0.088	0.091	0.104	0.115	0.131
	[0.098;0.104]	[0.101;0.145]	[0.076;0.107]	[0.068;0.107]	[0.086;0.097]	[0.092;0.115]	[0.092;0.139]	[0.120;0.142]
Months in prison	29.321	28.184	28.57	26.965	31.688	31.149	33.007	37.095
	[28.924;29.742]	[26.57;29.858]	[26.903;30.383]	[24.570;29.335]	[30.961;32.392]	[29.896;32.402]	[30.234;35.895]	[35.606;38.686]
Violent crime	0.419	0.418	0.449	0.436	0.419	0.398	0.404	0.431
	[0.414;0.424]	[0.386;0.454]	[0.425;0.474]	[0.405;0.467]	[0.409;0.428]	[0.380;0.416]	[0.37;0.441]	[0.414;0.449]
Property crime	0.316	0.31	0.329	0.336	0.319	0.312	0.289	0.275
	[0.311;0.321]	[0.276;0.342]	[0.302;0.355]	[0.308;0.366]	[0.31;0.328]	[0.293;0.330]	[0.254;0.323]	[0.259;0.292]
Public order crime	0.449	0.418	0.464	0.440	0.465	0.443	0.459	0.462
	[0.443;0.454]	[0.383;0.453]	[0.44;0.492]	[0.41;0.471]	[0.455;0.475]	[0.424;0.462]	[0.423;0.495]	[0.444;0.481]
Drug crime	0.274	0.287	0.227	0.266	0.281	0.297	0.28	0.281
	[0.269;0.279]	[0.254;0.316]	[0.202;0.251]	[0.238;0.294]	[0.272;0.29]	[0.280;0.314]	[0.248;0.311]	[0.264;0.297]
Substance Use Risk	0.584	0.558	0.636	0.606	0.598	0.547	0.556	0.588
	[0.579;0.590]	[0.522;0.595]	[0.608;0.666]	[0.574;0.637]	[0.588;0.608]	[0.528;0.567]	[0.52;0.593]	[0.570;0.607]
Emp/ Pop	0.609	0.594	0.559	0.594	0.653	0.658	0.643	0.711
	[0.605;0.613]	[0.573;0.613]	[0.540;0.577]	[0.574;0.612]	[0.646;0.661]	[0.646;0.672]	[0.622;0.666]	[0.699;0.724]

Source: Authors' calculations from Wisconsin administrative data.
Notes: The table reports the analysis of always takers, compliers, and never takers characteristics for each regime. Bootstrap 95% confidence intervals at individual level in brackets.

Figure 3: Average characteristics always takers, compliers, and never takers



Source: Authors' calculations from Wisconsin administrative data.

Note: The point in the box shows the estimate, the size the of the box represents the bootstrap inter-quartile range, and the whiskers show the bootstrapped 95% confidence interval.

Table 11: Average outcomes always takers, compliers, and never takers

	Uncond. Mean	Never takers			Compliers			Always takers
		R1	R2	R3	R1	R2	R3	E(Y|M=1,R0=1)
Recidivism								
6 months	0.134	0.118	0.119	0.115	0.131	0.124	0.121	0.159
	(0.002)	(0.006)	(0.012)	(0.005)	(0.010)	(0.013)	(0.004)	(0.013)
	[0.131;0.138]	[0.106;0.128]	[0.096;0.143]	[0.106;0.125]	[0.111;0.152]	[0.097;0.15]	[0.113;0.129]	[0.133;0.187]
12 months	0.260	0.235	0.233	0.228	0.266	0.267	0.258	0.291
	(0.002)	(0.007)	(0.016)	(0.006)	(0.013)	(0.018)	(0.006)	(0.016)
	[0.256;0.265]	[0.22;0.25]	[0.202;0.264]	[0.216;0.24]	[0.242;0.292]	[0.231;0.303]	[0.247;0.269]	[0.259;0.324]
Labor market								
Wages	450.918	483.685	543.122	707.371	321.367	337.487	477.841	327.783
	(7.478)	(23.824)	(59.040)	(28.746)	(28.566)	(33.503)	(13.905)	(23.111)
	[435;466]	[433;529]	[433;658]	[654;762]	[267;376]	[267;405]	[450;506]	[282;373]
Employment	0.230	0.230	0.227	0.229	0.246	0.238	0.273	0.204
	(0.002)	(0.007)	(0.014)	(0.006)	(0.012)	(0.016)	(0.005)	(0.013)
	[0.226;0.235]	[0.216;0.244]	[0.199;0.255]	[0.217;0.241]	[0.222;0.271]	[0.209;0.271]	[0.263;0.283]	[0.178;0.229]

	Uncond. Mean	E(Y|M=1,Rn=1)			Complier regime 0			NT & Complier
		R1	R2	R3	R1	R2	R3	E(Y|M=0,R0=1)
Recidivism								
6 months	0.134	0.137	0.129	0.125	0.220	0.180	0.169	0.150
	(0.002)	(0.008)	(0.011)	(0.003)	(0.018)	(0.014)	(0.006)	(0.004)
	[0.131;0.138]	[0.122;0.152]	[0.107;0.151]	[0.119;0.132]	[0.185;0.257]	[0.153;0.207]	[0.157;0.181]	[0.143;0.157]
12 months	0.260	0.271	0.270	0.262	0.382	0.330	0.310	0.282
	(0.002)	(0.010)	(0.015)	(0.005)	(0.022)	(0.018)	(0.008)	(0.005)
	[0.256;0.265]	[0.253;0.291]	[0.24;0.301]	[0.253;0.27]	[0.338;0.425]	[0.296;0.367]	[0.295;0.326]	[0.273;0.292]
Labor market								
Wages	450.918	322.702	336.103	460.832	255.896	281.495	256.758	410.759
	(7.478)	(22.317)	(28.737)	(12.222)	(62.909)	(61.857)	(23.562)	(11.841)
	[435;466]	[279;364]	[277;393]	[437;485]	[129;385]	[157;398]	[208;303]	[386;435]
Employment	0.230	0.238	0.233	0.265	0.208	0.219	0.220	0.223
	(0.002)	(0.009)	(0.013)	(0.004)	(0.021)	(0.016)	(0.007)	(0.004)
	[0.226;0.235]	[0.219;0.256]	[0.208;0.26]	[0.257;0.274]	[0.167;0.25]	[0.187;0.25]	[0.206;0.233]	[0.214;0.231]

Source: Authors' calculations from Wisconsin administrative data.

Notes: Bootstrap standard errors and 95% confidence intervals at individual level in parentheses and brackets, respectivelly.

Table 12: Bounds' treatment effects always and never takers

	Always takers ATTE bound			Never takers NTTE bound			Compliers LATE		
	R1	R2	R3	R1	R2	R3	R1	R2	R3
Recidivism		ATTE<			NETE<				
6 months	-0.061	-0.021	-0.010	0.013	0.005	0.006	-0.089	-0.056	-0.048
12 months	-0.091	-0.039	-0.019	0.031	0.034	0.030	-0.116	-0.063	-0.052
Labor market		ATTE>			NTTE>				
Wages	71.9	46.3	71.0	-162.3	-205.6	-229.5	65.5	56.0	221.1
Employment	-0.004	-0.015	-0.016	0.016	0.011	0.044	0.038	0.019	0.053

Source: Authors' calculations from Wisconsin administrative data.

Table 13: Intercep and Slope of MUO, MTO, and MTE

Sample of first releases

	MUO		MTO		MTE	
	Intercept	Slope	Intercept	Slope	Intercept	Slope
Recidivism 6 months	0.213	-0.116	0.163	-0.111	-0.05	0.005
Recidivism 12 months	0.378	-0.177	0.295	-0.098	-0.083	0.080
Wages	-115.7	977.1	310.8	438.3	426.5	-538.8
Employment	0.212	0.019	0.196	0.204	-0.017	0.184

Source: Authors' calculations from Wisconsin administrative data.

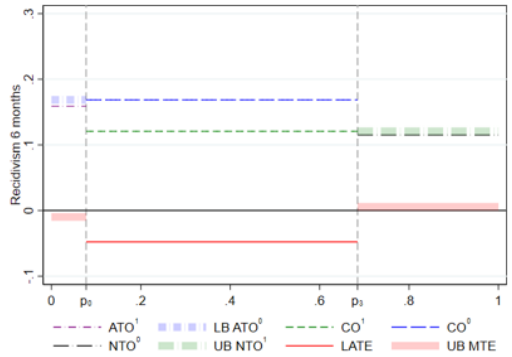
Notes: The table reports the intercept and slopes of the marginal untreated outcome, MUO(p), the marginal treated outcome, MTO(p), and the marginal treatment effect, MTE(p).

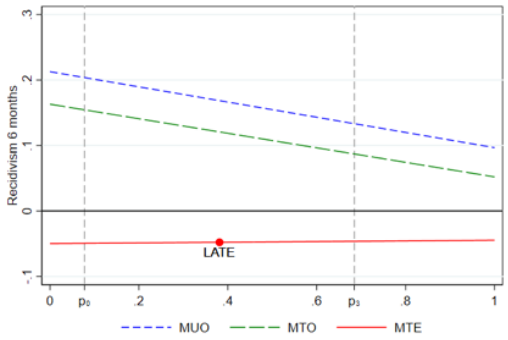
Figure 4: Bounds on outcomes and treatment effects

Monotonicity

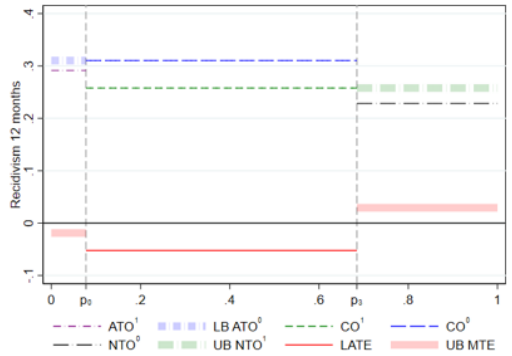
Linearity

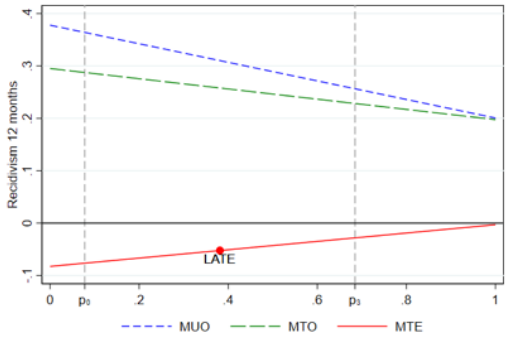
Recidivism 6 months



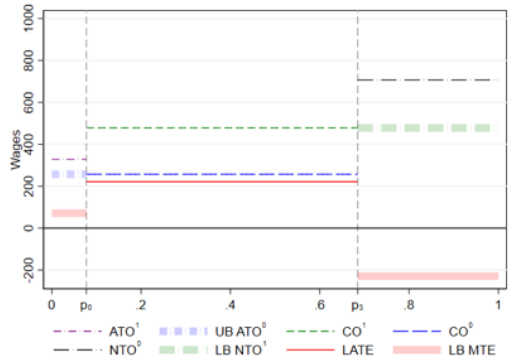


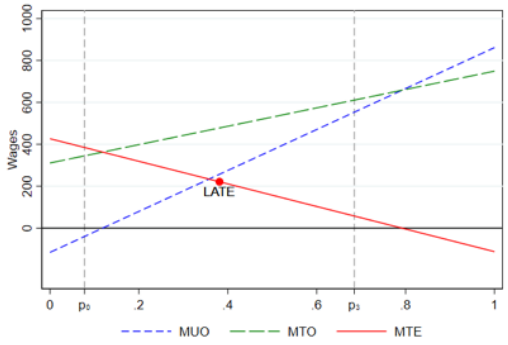
Recidivism 12 months



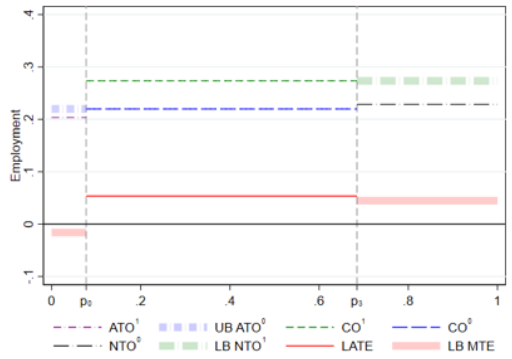


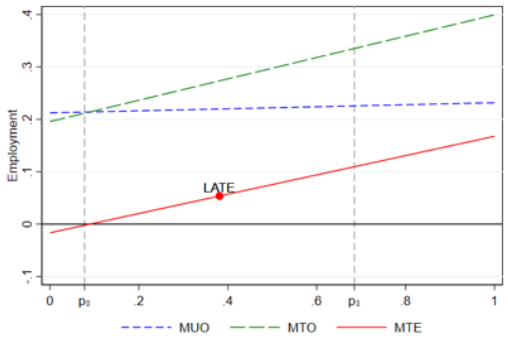
Wages





Employment





Source: Authors' calculations from Wisconsin administrative data.

APPENDIX

COMPAS Substance Use History Measures

The underlying function of the COMPAS instrument is to assess risk of recidivism including potentially modifiable correlates of recidivism including substance use.[1,2] Available assessments of the validity of the COMPAS substance use score concern the degree to which this score is associated with recidivism rather than a clinical diagnosis of substance use disorder.[1]

During our study period, the Wisconsin Department of Corrections (WI DOC) was adopting the COMPAS with the eventual goal of collecting two COMPAS assessments per person: one using the COMPAS Core instrument at intake; and one using the COMPAS Reentry instrument close to the time of release. During this implementation process, it was frequently the case that individuals completed just one assessment – either Core or Reentry – depending on the time of administration. Thus, for each subject we obtained from the WI DOC the most recently completed COMPAS assessment relative to the individual's release date, and no more than 120 days after their release. An assessment may have a date after the release if it was conducted through the community supervision program.

There are some differences in the Core and Reentry instruments with respect to the substance use history questions although the WI DOC generates the same 3-category score indicating a need for treatment from each instrument: highly probable, probable, and unlikely. The specific questions on which this score is based for each instrument are noted below. We do not have access to the proprietary algorithm used to generate the score. However, in our internal analysis the vast majority of individuals identified as "highly probable" using the Core instrument had three or more positive responses to the substance use history questions. Using the Reentry instrument, the vast majority of individuals identified as highly probable had five or more positive response to the substance use history questions. We use the determination of "highly probable" as our indicator for a history of substance use.

CORE Instrument Substance Use History Questions

1.Do you think your current/past legal problems are partly because of alcohol or drugs?
2.Were you using alcohol when arrested for your current offense?
3.Were you using drugs when arrested for your current offense?
4.Are you currently in formal treatment for alcohol or drugs such as counseling, outpatient, inpatient, residential?
5.Have you ever been in formal treatment for alcohol such as counseling, outpatient, inpatient, residential?
6.Have you ever been in formal treatment for drugs such as counseling, outpatient, inpatient, residential?
7.Do you think you would benefit from getting treatment for alcohol?
8.Do you think you would benefit from getting treatment for drugs?
9.Did you use heroin, cocaine, crack or methamphetamines as a juvenile?

COMPAS Reentry Instrument Substance Use History Questions

1.Committed Offenses while high/drunk?
2.Prior drug charges/convictions?
3.History of drug problems?
4.History of alcohol problems?
5.Prior treatments for drug/alcohol abuse?
6.Any history of failed drug/urine analysis test?
7. Is the inmate at risk for substance abuse problems?

Measures of Health Care Use

We adopt the diagnosis and procedure codes published by the Medicaid Outcomes Distributed Research Network to define visits for opioid use disorder (OUD) and substance use disorders (SUD), as well as medications for OUD. An outpatient visit is considered OUD- or SUD-related based on the presence of one of the relevant diagnoses shown below in any position on the claim.

Opioid Use Disorder

- ICD-9: 304.0x, 305.5x
- ICD-10: F11.xxx

Substance Use Disorders

- ICD-9: 303-305, exclude Tobacco 3051; exclude remission codes (5th digit = `3')
- ICD-10: F10-F19, exclude Tobacco F17, exclude remission codes; F55, O355, o9931, O9932

Medications for Opioid Use Disorder

- A prescription claim for: buprenorphine, Naltrexone (oral), Injectable Naltrexone, or buprenorphine/Naloxone; *or*
- A HCPCS code for buprenorphine or buprenorphine/Naloxone, oral: J0571, J0573, J0574, J0575; methadone administration, H0020; Naltrexone (extended-release injectable): J2315.

Measure of recidivism

Appendix Table 1: Complier-characteristics ratios
Sample of first releases

	Uncond.	Regime 1		Regime 2		Regime 3		Diff. statistically significant		
	Mean	Mean	Ratio	Mean	Ratio	Mean	Ratio			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(3)-(5)	(3)-(7)	(5)-(7)
Female	0.095	0.077	0.819	0.101	1.069	0.082	0.867	**		**
			[0.509;1.107]		[0.862;1.281]		[0.737;0.991]			
Age>35	0.585	0.687	1.175	0.645	1.103	0.610	1.042	**	***	***
			[1.131;1.220]		[1.061;1.150]		[1.026;1.058]			
White	0.561	0.560	0.999	0.580	1.034	0.580	1.035		*	
			[0.953;1.045]		[0.985;1.082]		[1.018;1.052]			
High school	0.693	0.699	1.009	0.696	1.005	0.695	1.004			
			[0.979;1.042]		[0.967;1.041]		[0.993;1.015]			
Married	0.101	0.082	0.817	0.091	0.907	0.086	0.851			
			[0.655;0.995]		[0.744;1.087]		[0.789;0.907]			
Prison>12 months	0.666	0.716	1.074	0.688	1.033	0.684	1.026		***	
			[1.040;1.109]		[0.995;1.07]		[1.015;1.037]			
Violent crime	0.419	0.442	1.056	0.448	1.071	0.416	0.993		**	**
			[0.990;1.120]		[1.013;1.134]		[0.972;1.016]			
Property crime	0.316	0.334	1.056	0.323	1.021	0.327	1.035			
			[0.983;1.13]		[0.935;1.108]		[1.008;1.062]			
Public order crime	0.448	0.464	1.036	0.448	0.999	0.460	1.025			
			[0.982;1.091]		[0.940;1.063]		[1.007;1.044]			
Drug crime	0.274	0.220	0.804	0.258	0.944	0.268	0.979	**	***	
			[0.722;0.892]		[0.846;1.041]		[0.948;1.011]			
Substance Use Risk	0.584	0.606	1.037	0.606	1.038	0.592	1.013			
			[0.988;1.085]		[0.988;1.092]		[0.997;1.029]			
Emp/ Pop>0.5	0.521	0.510	0.980	0.561	1.078	0.546	1.048	**	**	
			[0.916;1.047]		[1.012;1.148]		[1.029;1.069]			

Source: Authors' calculations from Wisconsin administrative data.
Notes: The table reports the analysis of compliers characteristics for each regime. The ratios in columns 3, 5, and 7 give the relative likelihood compliers have the characteristics indicator in each row. Bootstrap 95% confidence intervals at individual level in brackets.

Appendix Table 2: Complier-characteristics ratios
Sample of all releases

	Uncond.	Regime 1		Regime 2		Regime 3		Diff. statistically significant		
	Mean	Mean	Ratio	Mean	Ratio	Mean	Ratio			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(3)-(5)	(3)-(7)	(5)-(7)
Female	0.089	0.072	0.811	0.091	1.025	0.077	0.863	*		**
			[0.516;1.129]		[0.817;1.229]		[0.740;0.988]			
Age>35	0.563	0.660	1.173	0.617	1.097	0.586	1.041	***	***	***
			[1.134;1.218]		[1.056;1.136]		[1.026;1.055]			
White	0.548	0.554	1.011	0.561	1.024	0.570	1.040			
			[0.968;1.054]		[0.980;1.067]		[1.025;1.055]			
High school	0.683	0.688	1.008	0.689	1.009	0.683	1.001			
			[0.978;1.042]		[0.974;1.04]		[0.991;1.011]			
Married	0.096	0.085	0.884	0.086	0.896	0.082	0.860			
			[0.732;1.048]		[0.742;1.06]		[0.806;0.914]			
Prison>12 months	0.603	0.634	1.051	0.613	1.016	0.617	1.024			
			[1.013;1.089]		[0.977;1.053]		[1.014;1.035]			
Violent crime	0.421	0.448	1.064	0.457	1.087	0.422	1.003		**	***
			[1.009;1.122]		[1.032;1.144]		[0.983;1.023]			
Property crime	0.325	0.339	1.042	0.326	1.002	0.336	1.033			
			[0.977;1.114]		[0.935;1.065]		[1.010;1.057]			
Public order crime	0.440	0.455	1.035	0.437	0.993	0.450	1.024			
			[0.982;1.090]		[0.941;1.042]		[1.006;1.042]			
Drug crime	0.268	0.221	0.825	0.249	0.928	0.263	0.982	*	***	
			[0.744;0.902]		[0.849;1.009]		[0.955;1.009]			
Substance Use Risk	0.596	0.623	1.046	0.612	1.028	0.607	1.019			
			[1.000;1.087]		[0.988;1.068]		[1.005;1.034]			
Emp/ Pop>0.5	0.552	0.542	0.982	0.586	1.062	0.579	1.048	**	**	
			[0.929;1.042]		[1.003;1.124]		[1.031;1.065]			

Source: Authors' calculations from Wisconsin administrative data.
Notes: The table reports the analysis of compliers characteristics for each regime. The ratios in columns 3, 5, and 7 give the relative likelihood compliers have the characteristics indicator in each row. Bootstrap 95% confidence intervals at individual level in brackets.