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The Effect of Medicaid on Recidivism: Evidence From Medicaid Suspension and Termination Policies

Gultekin Gollu
Georgetown University

Mariyana Zapryanova
Smith College, mzapryanova@smith.edu

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Gultekin Gollu* Mariyana Zapryanova†

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Abstract

Although people who go through the prison and jail system in the United States have significant health care needs, many leave it with no health insurance and, as a result, they experience gaps in access to care. Exploiting variation in Medicaid eligibility policies for incarcerated individuals across states and using administrative prison release data, we find that suspending rather than terminating Medicaid upon incarceration decreases the probability of returning to prison within one year and three years of release by 2.91 and 4.58 percentage points, respectively. These effects are observed among different types of prisoners, but are greater for Black and repeat offenders. Our results suggest that faster and easier reinstatement of Medicaid benefits upon prison release decreases recidivism rate and are directly relevant to ongoing policy debates on the health care coverage of vulnerable populations.

JEL codes: I18, J18, K42.
Keywords: recidivism, prison, Medicaid, health insurance.

*Department of Health Systems Administration, Georgetown University, Washington, DC. Contact Email: gg746@georgetown.edu.

†Department of Economics, Smith College, Northampton, Massachusetts. Contact Email: mzapryanova@smith.edu (corresponding author). We would like to thank Charles J. Courtemanche (editor) and four anonymous referees for helpful comments. We thank Deborah Haas-Wilson, Simon Halliday, Susan Sayre, Vis Taraz, Jorge Vasquez, and conference participants at the 2019 Public Policy Analysis & Management Meetings and the 2019 Southern Economic Association Meetings for helpful suggestions and conversations. We also thank Crystal Yang for sharing her code for computing recidivism rates using the NCRP data. Taegan Mullane, Natalia Perkins, and Andie Rawson provided excellent research assistance.

1 Introduction

In 2018, over 600,000 people were released from state and federal prisons and almost 11 million people cycled through local jails in the United States (Carson, 2020; Zeng, 2020). Upon release, these individuals face serious collateral consequences of conviction that restrict their access to the labor market, by means such as licensing restrictions (Council of Economic Advisors, 2016), and restrict or deny eligibility for federal and state programs such as federal financial aid (Lovenheim and Owens, 2014), Supplemental Nutrition Assistance Program (SNAP) (Tuttle, 2019), and housing assistance (Council of Economic Advisors, 2016). Ex-offenders face significant and ongoing economic and societal challenges that often prevent them from becoming productive members of society. This can indirectly push them back to crime and prison. The significant challenges ex-prisoners face finding permanent legal employment might make securing private health insurance harder (Pager et al., 2009; Denver et al., 2017). In addition, people who go through the prison and jail system in the United States disproportionately have significant physical and behavioral health problems and are among the individuals in highest need of medical care (Maruschak et al., 2015).[1] Despite having significant health care needs, incarcerated individuals face decreased access to health care following release (Kulkarni et al., 2010; Mallik-Kane and Visher, 2008). The reasons for the lack of health care continuity for individuals moving from prison to the community are multifaceted, ranging from lack of employment to potential difficulty enrolling in social safety net programs providing health care coverage. In this paper, we explore whether faster and easier Medicaid enrollment after release affects recidivism rates.

Policies for individuals who are enrolled in Medicaid when admitted to custody vary across states.[2] These policies affect whether and when individuals can use Medicaid upon release. As of 2018, nineteen states terminated Medicaid upon incarceration, while the rest suspended coverage for the duration of the incarceration or for a specified period of time. When a state terminates Medicaid coverage upon incarceration, the individual is removed from its Medicaid rolls, and upon release, they must submit a new application for Medicaid

enrollment and again been deemed eligible.[3] When a state suspends Medicaid coverage, the individual is permitted to remain on the Medicaid rolls in a suspended status for the duration of the incarceration.[4] This ultimately means that the individual retains his or her eligibility for Medicaid coverage but his or her benefits are cut off during incarceration. Once the individual is released from prison, some of their time-variant eligibility requirements, such as family structure and address, are reassessed. However, this reassessment does not require a new application and thus can result in a faster and easier restoration of benefits, which might lead to faster access to access to mental health services, prescription medicines, and other needed care. Rosen et al. (2014) report that resumption of benefits in most suspension states, although not automatic, occurs within a month of release. This suggests that released prisoners whose Medicaid coverage was suspended might have their Medicaid benefits restored half a month to a month faster than those whose coverage was terminated. A natural question therefore is whether potentially easier and faster access to health care insurance and treatment in the community has any impact on subsequent criminal behavior. We use variation in states' policies of suspending or terminating Medicaid coverage upon incarceration to shed light on this important question.

Suspending Medicaid rather than terminating it could provide easier and faster access to medical services upon release, which could impact recidivism through several channels. First, it could help to eliminate any gaps in mental or substance-abuse care upon reentry into the community, and thus facilitate health care utilization.[5] As a result, ensuring continuity of care after release could reduce recidivism through improved management of health conditions.[6] Second, because having Medicaid coverage could decrease insurance and medical costs for released prisoners, it could also reduce their incentives to engage in income-generating criminal activities. Third, this income effect increases financial security which may in turn reduce financial stress and improve mental health. This eventually could lead to a reduction in recidivism though improvements to mental health. Finally, Medicaid eligibility upon release may create a feeling of being valued by the community, which eventually may reduce

incentives to commit crime. All these channels predict that suspending Medicaid coverage rather than terminating could decrease recidivism rates.

However, other theoretical factors might lead to observing increases or no change in recidivism rates. For example, anticipating to have their Medicaid coverage restored immediately after release might decrease convicts' opportunity cost of committing crime. Additionally, crime and recidivism could potentially increase as immediate access to Medicaid could make it easier to obtain prescription medication, such as opioids, that may facilitate both criminal behavior and substance abuse. Ensuring faster and easier reinstatement of Medicaid coverage upon release might not result in any changes in criminal behavior if those enrolled in Medicaid, especially those who have gone through the criminal justice system, face substantial barriers to treatment. These barriers may include supply-side capacity constraints to treat mental health and substance abuse problems, lack of assistance in helping inmates looking to restore coverage, former inmates' lack of information on how to obtain a primary care physician, and financial difficulties covering copays. Overall, the effect of suspending Medicaid upon incarceration on recidivism is ambiguous.[7]

Our main data source is the National Corrections Reporting Program (NCRP), and it contains detailed records on state prison releases from 2005 to 2013. Drawing on various primary sources, we assembled information on Medicaid policies for incarcerated individuals at the state level, including their effective implementation dates. Relying on plausibly exogenous variation in the implementation of Medicaid suspension policies across states, we estimate a series of difference-in-differences models that relate recidivism rates and Medicaid suspension upon incarceration. In our models, we include a wide variety of individual characteristics and time-varying state characteristics that could impact our outcomes of interest and we control flexibly for state and admission year effects.

We find that suspending Medicaid upon incarceration is associated with an approximately 4.58 percentage point reduction in the probability of returning to prison within three years of release. The Medicaid suspension policies decrease the probability of returning to prison

within one year by 2.91 percentage points. These findings are robust across a variety of specifications, including relaxing the difference-in-differences parallel pre-trend assumption. We also explore heterogeneous effects of the policy. We observe the policy effects in wide variety of prisoners in terms of gender, race, and recidivating crime type. We document that the policy effects are largest in magnitude among Black and repeat offenders.[8]

Because the Affordable Care Act (ACA) expanded health care coverage to low-income childless adults in 2014, more and more individuals might have Medicaid coverage upon incarceration over time. Without a process to suspend and reactivate benefits, many of these people risk losing coverage during their time in jail or prison. A loss of coverage could be both harmful at the individual level and a waste of the program resources used for re-enrollment after release. Our results are directly relevant to ongoing policy debates about providing social assistance, including health care coverage, to populations at high risk of reentering the criminal justice system. Legislators have recently introduced several bills aiming to fix the issue of lapse in care after prison release by maintaining Medicaid eligibility for pretrial inmates, requiring suspension rather than termination of Medicaid benefits for juvenile inmates, and reinstating Medicaid for inmates thirty days before their release.[9]

The remainder of the paper is organized as follows. In section 2, we provide an overview of Medicaid suspension and termination policies for released prisoners across states and put our study in the context of the broad literature exploring health and the criminal justice system. In section 3, we present our data sources and describe our estimation sample. In section 4, we describe our empirical approach, and in section 5 we report the main results, perform various robustness checks, and explore heterogeneous effects. Section 6 concludes.

2 Medicaid Suspension and Termination Policies for Incarcerated Individuals

Individuals who receive Medicaid prior to being incarcerated have their Medicaid coverage either terminated, suspended for the duration of incarceration, or suspended with a time-limit, depending on the state in which they are imprisoned. When a state terminates Medicaid coverage upon incarceration, the individuals' Medicaid case files are closed, and they must reapply for Medicaid after prison release. When a state suspends coverage, the inmates' Medicaid files are placed in a suspended status while they are incarcerated. The coverage may be suspended for a certain duration before the case is closed, or it may be suspended regardless of the length of their stay in prison.[10] In the case of suspension for the duration of incarceration, no new application is needed from released prisoner to have their Medicaid benefits restored after they are released from prison.

We collected the implementation dates of the specific state policies on Medicaid suspension or termination upon incarceration by reviewing multiple sources including state and federal Medicaid documents, research publications, state news, and contacting states' Centers for Medicare and Medicaid Services. We report these data in Table A1 and provide a visual representation in Figure A2.[11] Figure A1 shows the timing of the change from termination to suspension of Medicaid coverage for incarcerated individuals by state, which our identification strategy will exploit.

In 2001, Maine became the first state to allow an indefinite suspension of Medicaid for incarcerated individuals. By 2015, twenty-three states adopted Medicaid suspension for the duration of incarceration while six states did so for a specific period of time (e.g., 12 months). The enactment of the ACA not only broadened access to Medicaid for the incarcerated populations but also contributed to a sharp increase in the number of states that suspend rather than terminate Medicaid upon incarceration.[12] Changes in eligibility and enrollment policies implemented by the ACA have forced states to update their information technology

systems, which may have encouraged even more states to switch to Medicaid suspension for inmates.[13]

Policies expanding Medicaid coverage, such as the ACA, could be a way to cover more of the populations involved in the criminal justice system. However, even if access to health care is broadened for these populations, there might exists other barriers to the continuity of medical coverage and care after prison release. For released prisoners, termination of Medicaid upon incarceration could result in a much more complex and time-consuming application process than suspension. The literature has linked creating administrative barriers or increasing administrative burden to limiting access to Medicaid benefits (see, for example, Herd et al. (2013)). We build on findings of this literature and explore whether termination of benefits impacts recidivism rates.

As a means-tested program, Medicaid imposed categorical and income-eligibility requirements that could limit access to coverage for inmates. There is no national data on the number of individuals on Medicaid who are admitted to prison and as a result affected by Medicaid suspension policies upon incarceration. However, the nationally representative Survey of Inmates in State and Federal Correctional Facilities (SISFCF), conducted by the Bureau of Justice Statistics (BJS) in 2014, asks inmates in state and federal prisons whether or not anyone they were living with prior to incarceration was receiving public assistance such as Medicaid, SNAP, or housing assistance. Using SISFCF data, we find that about a quarter of the surveyed inmates lived in a household receiving public assistance, such as Medicaid, prior to prison admission.[14] Thus, a non-trivial fraction of prisoners might already be on Medicaid or likely meet Medicaid income and eligibility requirements when admitted to prison.

3 Related Literature

Previous studies have shown that providing treatment for substance abuse and mental health improves mental health outcomes (Prendergast et al., 2002). Few studies have examined the relationship between health care and crime. The evidence from these studies shows that increasing health care utilization leads to reductions in violent and property crime rates (Bondurant et al., 2018; Wen et al., 2017). More specifically, they find that increases in substance-abuse treatment rates significantly reduces criminal behavior. A couple of papers in the medical literature examine the effect of Medicaid enrollment on criminal recidivism (Morrissey et al., 2007, 2006). Morrissey et al. (2006) find suggestive evidence that those enrolled in Medicaid upon release face fewer detentions on average. In a subsequent paper, Morrissey et al. (2007) analyze the effect of expedited Medicaid enrollment upon release on mental health use and criminal recidivism. Using a sample of released individuals with severe mental illnesses, such as schizophrenia and bipolar disorder, the authors find that expedited Medicaid referrals result in higher Medicaid take up rates and use of mental health services a year after release, but have no effect on recidivism. The main shortcoming of these studies is that they use a sample of prisoners with severe mental illness, a population that might not be representative of the general prison population. Our work studies the effect of faster Medicaid re-enrollment for the general population of released prisoners on recidivism and we provide plausibly causal estimates of this effect.

We also build on the literature connecting health insurance to criminal activity. A couple of studies have explored the effect of Medicaid expansion under the ACA and through Health Insurance Flexibility and Accountability (HIFA) waivers on crime. Vogler (2017) finds that Medicaid expansion though the ACA has reduced violent crime by 5.8 percent and property crime by 3 percent, with the effects being larger in places that had higher pre-expansion uninsured rates among individuals subsequently eligible for Medicaid. He and Barkowski (2020) echo the findings of Vogler (2017) that Medicaid expansion led to decreases in violent and property crime rates. Wen et al. (2017) find large reductions in robbery, larceny theft,

and aggravated assault rated as a result of HIFA-waiver expansion of Medicaid. Even after expansion of Medicaid coverage, ex-prisoners could face significant barriers to access or tougher eligibility requirements. Accordingly, our study seeks to understand whether relaxed Medicaid eligibility rules that result in faster and easier access to Medicaid benefits upon prison release reduce recidivism. Taking advantage of the NCRP data, Aslim et al. (2019) uses variation in the implementation of the ACA Medicaid expansion to look at the effects of broadening access to public health insurance on recidivism. They find that the ACA Medicaid expansion decreases recidivism for both violent and public order offenders. However, they do not explore whether Medicaid administrative enrollment policies affect recidivism rates. Despite increasing coverage through the ACA, state Medicaid eligibility process could potentially impose administrative burdens that make it harder for Medicaid-eligible prison populations to actually gain access to health care even when eligibility is expanded. We complement Aslim et al. (2019) by exploring the effect of facilitating re-enrollment through potentially reducing the application burden and shortening the time of reinstatement of benefits after release. Our study furthers the literature and the policy debate by exploring whether Medicaid administrative policies of suspending rather than terminating Medicaid coverage upon incarceration, matter.

Using administrative data from two different states, two very recent papers examine how various Medicaid policies affect recidivism. Using data from South Carolina, Jácome (2020) finds that individuals who automatically disenroll from Medicaid at age 19 are more likely to be incarcerated by age 21 than a matched control group of low-income individuals. Using administrative data from Wisconsin, Badaracco et al. (2021) study the introduction of Medicaid enrollment assistance program and expanded Medicaid benefits to released prisoners in Wisconsin. Similarly to our results, both of these studies find that Medicaid eligibility affects recidivism rates and that effect is observed for all types of crime. We contribute to this body of work by providing evidence at the national level of how Medicaid policies for incarcerated individuals can explain recidivism rates.

Our paper also relates to the broader literature examining the relationship between public assistance, such as SNAP and welfare, and crime or recidivism (Yang, 2017a; Foley, 2011; Hsu, 2017; Carr and Packham, 2019; Palmer et al., 2019; Tuttle, 2019; Luallen et al., 2018; Agan and Makowsky, 2018). Most of this literature finds that an increase in public assistance leads to a reduction in crime or recidivism mostly due to lowering the probability of engaging in the illegal sector as a source of income.

4 Data

We used data on prison admissions and releases from the NCRP compiled by the BJS. The NCRP is an offender-level data set to which participating states voluntarily submit data on prisoners entering and leaving their custody. For each prison spell, we observe the admission and release dates for each offender, which allows us to construct our recidivism measure and compute total time served.[15] Additionally, the NCRP contains rich information on offenders' demographic characteristics, such as age, race, highest grade completed, gender, and whether the offender has previously been incarcerated for a felony. We also observe up to three crimes for which the offender has been convicted, the sentence length for the most serious crime, the type of entry (for example, new conviction, parole or probation revocation), and the type of release (for example, parole or probation).

Because reporting to NCRP is voluntary, not all states provide data consistently over time. Appendix Table A1 lists the NCRP coverage by state and prison release year. We restrict our sample to individuals released from prison between 2005 and 2013 for two main reasons.[16] First, this insures that we can have a perfectly balanced estimation sample with the largest number of states that consistently report prison releases to NCRP. Our balanced sample allows us to observe prison releases over the same period of time for each state. This is important to ensure that the recidivism outcome is measured on the same set of states in each time period pre- and post-treatment. In addition, restricting the sample based on

release year instead of admission year results in roughly similar number of observations in each pre-treatment years, which increases the precision of our pre-trend estimation. Second, by excluding releases after 2013, we avoid confounding our estimates with the ACA implementation. Thus, our preferred estimation sample consists of states and release years highlighted in blue in Appendix Table A2.[17] Although most of the excluded from the sample states tare excluded because of NCRP coverage and/or because we do not observe enough post-treatment data, there are two states (California, Arizona) that are excluded for different reasons. We exclude California because it enacted the Public Safety Realignment Act (PSRA) as a solution to the state's prison-overcrowding problem, and as a result many convicts served their time in county jail rather than state prison after the enactment. Because we do not observe county jail admissions and releases, we are unable to accurately calculate our recidivism measure in California. Finally, we exclude Arizona from our analysis because per conversations with officials from there, suspension of Medicaid upon incarceration in Arizona was a staggered process. A pilot program was implemented that allowed just prisoners in a few state prisons had their Medicaid suspended upon incarceration, and later that program was expanded to the whole state. Unfortunately, because Arizona's Medicaid agency did not give us details about the implementation of the program, we do not know the exact timing of the policy implementation across the state.

We also drop individuals who have not yet been released, who died in custody, or who were sentenced to life with or without the possibility of parole. There are two drawbacks to the data for our purposes that could affect the calculation or the interpretation of our recidivism measure. First, recidivism is observed only within the same state. If an inmate reoffends in another state, it will appear as if he did not recidivate. Thus, our recidivism rate could be underestimated if criminals who are more likely to recidivate move across state borders.[18] Second, the NCRP data allows us to calculate only return to prison as a measure of recidivism. This is a proxy for serious reoffense and does not capture people, who have been arrested, have been arrested and sentenced to probation, or have received some other

form of noncustodial sanction.

Table 1 represents summary statistics of the demographic characteristics of our 1-year and 3-year recidivism samples, while Table 2 summarizes overall and crime-type-specific recidivism. It is not surprising that the summary statistics for the 1-year and 3-year recidivism sample are quite similar given that we restrict the sample to a balanced panel of nine years of prison releases. In our discussion of the summary statistics, we will focus on interpreting those for the 1-year sample. Male and Black prisoners comprise, respectively, 87.8 percent and about 43 percent of the sample. The average age at release is 35, and the majority of the offenders (about 39 percent) are high school drop outs. Of the offenders of the sample, 32 percent were previously incarcerated for a felony. The three most common crimes are violent, property, and drug offenses, representing 20 percent, 30 percent, and 30 percent of offenders, respectively. On average, offenders are sentenced to 57 months and 65.7 percent of them because of a new court commitment. Approximately 33 percent of offenders are released under discretionary parole, 16.8 percent are released under mandatory parole, and 32 percent serve their full sentence in prison. As seen in Table 2, 19.4 percent of offenders in our sample return to prison within one year of release, while almost double that (37.8 percent) do so within three years. We also note that property and drug crimes are the most common offenses for which a person returns back to prison.

5 Empirical Strategy

5.1 Difference-in-differences

To estimate the effect on recidivism of switching from termination to suspension of Medicaid upon incarceration, we exploit variation in the timing of implementation of the Medicaid suspension policy in each state. In a difference-in-differences framework, we use the effective dates of suspension policies as exogenous shocks that increase the relative ease of re-enrolling in Medicaid upon release.[19] Exploiting the panel nature of our data and the fact that states

switch from terminating to suspending Medicaid at different times, we estimate the following baseline regression:

$$Recidivate_{ist} = \alpha_0 + \beta Suspension_{st} + \alpha_1 X_{it} + \alpha_2 Z_{st} + \alpha_3 Expansion_{st} + \gamma_t + \delta_s + S_s \times t + \epsilon_{ist} \quad (1)$$

where i indexes the offender, s indexes the state, and t indexes year of prison admission. Our dependent variable of interest, $Recidivate_{ist}$ is an indicator variable that equals to 1 if an offender i, admitted to prison in state s in year-month t, returns to prison in the same state within one or three years of release.[20] Using one-year recidivism rates increases our sample size while using three-year recidivism rates may more fully capture recidivism probabilities because of the lengthy trial process in criminal cases. Thus, we consider recidivism rates both within one and within three years of release.

$Suspension_{st}$ is an indicator that equals to 1 if an individual is admitted to prison after the Medicaid suspension policy for incarcerated individuals was implemented in state s. In other words, individuals are treated if they are admitted to prison after the implementation of a policy that suspends rather than terminates Medicaid upon prison admission. Thus, treatment is defined based on prison admission date rather than the release date to better capture all prisoners whose Medicaid coverage potentially was suspended once they were incarcerated. Defining treatment status this way makes more sense in our context because treatment is triggered at prison admission.[21]

X_{it} is a vector of offender characteristics that are both time-invariant (race/ethnicity, gender, highest grade completed at entry) and specific to the particular prison spell (age at release, time served for this spell, offense committed for this spell, and prior felony incarceration indicator). The vector X_{it} also includes indicators for missing data on each of the time-invariant offender characteristics. Z_{st} captures time-varying state characteristics, such as unemployment rate and the number of sworn police officers per 1,000 people.[22] These variables are intended to capture time-varying state-level characteristics that impact recidi-

vism. $Expansion_{st}$ is an indicator that equals to one if a state s expanded Medicaid early in the year-month t in which the offender was admitted to prison. The terms γ_t and δ_s are admission year and state fixed effects, respectively. The $S_s \times t$ terms are state-specific linear time trends, which absorb possible pre-existing state trends in the outcome measure correlated with the treatment (Ghosh et al., 2020; Doleac and Hansen, 2020).[23] We cluster the standard errors at the state level because the treatment is defined at the state level.

Our identification of the impact of switching from termination to suspension of Medicaid upon incarceration compares observably similar offenders admitted to prison in the same state but who are admitted under a policy regime of suspending Medicaid coverage. The coefficient of principal interest, β, is identified by the random variation in the month of admission, whether prison entry occurred before or after the Medicaid suspension policy is adopted, and how an individual's probability of recidivism compares to that of other prisoners with similar characteristics.

The natural concern with using a difference-in-differences approach is endogeneity in the timing of the policy changes. There might be factors not controlled for in Equation (1) that are correlated with the decision of Medicaid state agencies to adopt Madeicaid suspension policy and individuals' propensity to recidivate. We take several approaches to mitigate this problem. We argue that endogeneity is unlikely to be a problem for two reasons. First, in most states, the suspension of Medicaid was not mandated by the passage of new laws; rather, the changes were made within Medicaid administrative agencies. Second, many states suspended Medicaid in response to the expansion of Medicaid under the ACA rather than in response to their recidvism rate or to other state efforts to address recidivism. We control for early Medicaid expansion and examine pre-existing trends to ensure as best as we can that the changes in recidivism are attributable to the causal effect of suspending Medicaid upon incarceration, as compared to other differences across states.

Note that our treatment, the switching from Medicaid termination to suspension upon incarceration, represents an *intent to treat*. We do not observe the actual treatment in the

data, nor can we identify whether the individuals in the NCRP are eligible for or already receiving Medicaid. Ideally, we would be able to identify everyone who is Medicaid-eligible and the Medicaid suspension policies would lead immediately to everyone having easy access to Medicaid when released. To the extent that this does not happen—that is, to the extent that the intent to treat does not indicate actual treatment—our estimates will be biased toward zero. The most likely threat to identification is that Medicaid policies for incarcerated individuals were adopted in states that were motivated to help ex-offenders reintegrate into society. If the timing of the Medicaid suspension policy coincides with new state interest in enrolling soon-to-be-released inmates in social safety nets, such as Medicaid, we expect our results to be biased downwards.

5.2 Event study

We extend our difference-in-differences framework to an event study by including treatment leads and lags as regressors. This allows us to test our identifying assumption and to estimate the average dynamic effects of switching from termination to suspension of Medicaid upon incarceration on recidivism.

We use the following event study specification (Jacobson et al., 1993):

$$Recidivate_{ist} = \alpha_0 + \sum_{L \in K} \beta_L Suspension_{st}^{L} + \alpha_1 X_{it} + \alpha_2 Z_{st} + \alpha_3 Expansion_{st} + \gamma_t + \delta_s + S_s \times t + \epsilon_{ist} \tag{2}$$

$K = \{-6, -5, -4, -3, -2, -1, 0, 1, 2, 3, 4\}$, with -6 capturing six or more years before and 4 capturing four or more years after the state switches from terminating to suspending Medicaid coverage upon incarceration. In other words, we set a baseline event window running from six years prior to a state's reform of Medicaid policies regarding incarcerated individuals to four years after.[24] The set of $Suspension_{st}^{L}$ dummies represents year relative to the enactment of the Medicaid suspension policy ($L = -1$ denotes the year prior to switching from Medicaid termination to suspension).[25] Each of the β_L coefficients is measured rela-

tive to the omitted category (the year prior to policy implementation). The validity of this research design relies on the assumption that outcomes in the treatment and control states would have behaved similarly in the post implementation years if the policy were not implemented. Finding β_L coefficients in the prior years that are indistinguishable from zero would indicate the outcome variables were on a similar trajectory before the Medicaid suspension policy was implemented, which is what we would expect to see if this assumption were true. As we will show throughout Section 6, the pre-trends we observe imply that the states that terminate Medicaid upon incarceration are likely to serve as a good counterfactual group.

6 Results

Table 3 presents our main difference-in-differences results, which estimate the effect of Medicaid suspension policies on an individual's probability of returning to prison within one year or three years of release. Table 3 Column 1 only controls for state and prison admission year fixed effects. It suggests that suspending as opposed to terminating Medicaid upon incarceration reduces the probability of returning to prison within one year and three years of release by 2.4 and 4 percentage points, or approximately 12 and 10.6 percent, respectively. The estimates are borderline statistically significant (with a *p-values* of 0.104 and 0.15, respectively). Controlling for prisoner demographic characteristics in Column 2 produces a slightly larger estimate of β that is statistically significant. In our preferred specification, Column 3, we additionally control for time-varying state characteristics (unemployment rate and police force size).[26] We find that Medicaid suspension reduces one-year and three-year recidivism rates by 2.91 and 4.58 percentage points.[27] Although we do not observe every initiative at the state or local level that may have affected recidivism rates, and therefore we cannot account for their influence, the stability of the estimates reported in Table 3 is reassuring.

The validity of the difference-in-differences results depends on the assumption that the

parallel pretreatment trends between the treatment and the control states would continue to be parallel in the absence of the treatment. While it is not possible to observe the counterfactual, we can test whether the pretreatment trends of the treatment and the control states are parallel using our event study specification described in Section 5.2. Our specification satisfies the assumption if we cannot reject the claim that all β_k coefficients with $k < -1$ in Model 2 are zero. The event study results are presented in Figure 1. We do not observe any noticeable pre-trends for the 1-year recidivism results. For 3-year recidivism results, the point estimates hint at a slight yet insignificant pre-trend.[28] This suggests that Medicaid suspension policies for incarcerated individuals are not likely to be proceeded by other initiatives in the treated states aimed at decreasing recidivism. The event study results in Figure 1 also provide suggestive evidence that the effect of the policy increases over time. This might not be surprising as Medicaid eligibility has been expanding over time.

6.1 Heterogeneous effects

When considering a policy change, it is vital to know whether the policy would have homogeneous influence across different types of prison populations. In general, this information is of interest to policy makers whose goal is to impact specific populations or geographic areas where the policy may be most effective. In this section, we explore heterogeneous effects of the Medicaid suspension policy by various prisoner characteristics.

Table 4 Columns (2)-(4) report difference-in-differences results by the crime type with which the offender returns to prison.[29] We observe a policy effect in similar magnitude for all return crime type recidivism. In terms of statistical precision, however, only the effect on 1-year property and drug-related recidivism and 3-year property recidivism are statistically significant. In contrast to Aslim et al. (2019), who finds that expanding Medicaid through the ACA led to no statistically significant effects on property and drug-related recidivism, we find that Medicaid suspension policies, which potentially could result in regaining Medicaid coverage after release easier and faster, have a statistically significant impact on these types

of recidivism.[30]

Especially prior to the enactment of the ACA, Medicaid eligibility rules have varied by state based on parental status and household income level. Pre-arrest income levels and parental status also vary by gender. So the suspension policies' impact on recidivism is likely to vary by gender. The BJS reports that 53% of men and women in prison were parents of minors before they were imprisoned and that 41.7% of women reported being the sole parent of their household (Glaze, 2008). Moreover, a greater proportion of men in jail had income from wages or salary before arrest compared to women (Bureau of Justice Statistics, 2002). These statistics of custodial responsibility and income imply that Medicaid suspension policies may have a stronger effect on female recidivism. Table 5 present our results by gender. These regressions include the full set of covariates and report the effect of switching from termination to suspension of Medicaid upon incarceration on recidivism within one year (Panel A) and 3 years (Panel B) of release. Our results indicate that the policy change reduces one-year recidivism of women by 2.06 and of men by 2.96 percentage points, respectively, and these findings are statistically significant at the 1% level. These policy's effects are percentage-wise comparable for male and female and are not statistically different. We observe similar patterns for 3-year recidivism (Table 5, Panel B). The corresponding event studies in Figures 2 and 3 provide evidence of a similar dynamic effect of the policy on 1-year and 3-year recidivism rates for female and male released prisoners, respectively.[31]

Table 5 also reports heterogeneous effects by race. Switching from termination to suspension of Medicaid upon incarceration is associated with a 3.27 percentage points (9%) statistically significant reduction in one-year recidivism rates among whites and a 5.74 percentage points (14%) statistically significant reduction for Blacks. However, we cannot reject the hypothesis that these estimates are statistically the same. The heterogeneous effects on one-year recidivism are smaller than those for three-year recidivism for both racial groups, greater for Blacks than whites, and not statistically significantly different from each other.

We also explore whether the effect of Medicaid suspension policies on recidivism varies

by whether a prisoner is a repeat or first-time offender. We split the sample depending on whether or not the prisoner has had prior felony convictions and we report these result in Table 6. We find that the effect of Medicaid suspension on one-year and three-year recidivism is almost 60 percent larger in magnitude for repeat offenders. However, this corresponds to a relatively similar percent decrease (6% and 6.8% for 3-year-recidivism rates of first-time and repeat offenders, respectively). This is explained in part by the fact that recidivism rates are almost 50 percent greater among repeat offenders compared to first-time offenders (30% vs. 44%, respectively).

Some states have enacted legislation to implement their Medicaid suspension policy for incarcerated individuals, whereas others have implemented it by changing their administrative agency rules. We test whether the effect of the policy differs by implementation method. Table A3 presents results for our main specification, which includes one more interaction term than Equation (1). This additional term captures potential differential impact of switching from termination to suspension of Medicaid upon incarceration by whether this switch happens legislatively or administratively. The results provide no evidence that the effect differs by the implementation type.[32]

Finally, we check whether the effect of suspending Medicaid rather than terminating is greater among states that expanded Medicaid early.[33] We add an interaction term to our main model that captures the effect of both expanding Medicaid and switching from termination to suspension of Medicaid coverage upon incarceration. We report these results in Table A4. We find that the effect of switching from termination to suspension of Medicaid is greater in states that also expanded Medicaid. This is plausible because a greater portion of the inmates are likely to have Medicaid coverage prior to incarceration in states that have expanded eligibility. Thus, inmates in these states are likely to benefit more from the Medicaid suspension policies. The point estimate suggest that the suspension policy reduces the recidivism rate by 2.7 percentage points (roughly 7.1 %) more in states that have expanded Medicaid compared to states that have not. We find suggestive evidence that

the early Medicaid expansion, or in more general terms a higher Medicaid-coverage rate, might increase the magnitude of the effect of suspending rather than terminating Medicaid upon incarceration.

6.2 Robustness

Although our event study framework tests and statistically rejects existence of a differential linear pre-treatment period trends between the treatment and control states, this doesn't necessarily mean that there is no differential pre-treatment period trend. Indeed, our three-year recidivism results hint to a slight yet insignificant negative pre-trend. Rambachan and Roth (2020) provides a robust inference method that relaxes the parallel pre-treatment trends assumption in event study analysis. The method provides flexibility on the imposed assumptions on the trend between treatment and control states and gives opportunity to report results under various assumptions. We compare 95% confidence intervals obtained from our primary event study model against those obtained after allowing for per period deviations from a linear trend of up to an arbitrary amount, M. Figures 5 and Figure 6 display sensitivity plots under $0 \leq M \leq 0.3$ for post-treatment period $t = 3$ for our 1-year and 3-year results, respectively. Our 1-year recidivism results (Figure 5a) and 3-year (Figure 6a) are statistically different from zero when including a treatment group-specific linear trend (M = 0) and even when permitting deviations from a linear trend by as much as 0.22 and 0.14, respectively. While the point estimate of effect of Medicaid suspension policy decreases in magnitude and is sensitive to potential trend violations, its sign remains negative allowing us to draw qualitatively the same conclusion. Similar patterns are observed when we additionally impose a monotonically decreasing non-linear time trend difference in Figure 5b and Figure 6b.

A possible problem highlighted by the new literature on the difference-in-differences arises when the treatment is staggered and treatment effect varies over time (Goodman-Bacon, 2021). The issue is originated by the fact that the pre-treatment period for late adopters

includes the post-treatment period of the early adopters. Thus, using the early adopters as the control group of late adopters biases the results in the opposite direction of the true average treatment effect. In the context of our study, this possible problem is not likely to bias our results since the majority of the treated states in our balanced panel adopted Medicaid suspension policies roughly at the same time (three states adopted it in 2008, one in 2007 and one in 2005). Although we do not have many early treated states, we test the sensitivity of our results to only including the three treatment states, Florida, New York, North Carolina, which implemented the policy at the same time in 2008. In this way, we eliminate the possible issues due to staggered treatment. The results for 1-year and 3-year recidivism using this subsample are reported in Figure A4 and are both consistent with our main results.

We also test the sensitivity of our results to relaxing the state-specific time trends as suggested by Goodman-Bacon (2021). In Figure A5, we observe that not controlling for state-specific linear time trends does not change the pretrends for both 1-year and 3-year results in any substantial way. The estimated effects post-reform for 3-year recidivism rates are of smaller magnitude but are still statistically significant. However, our results for the 1-year recidivism disappear and become insignificant.

In addition to scrutinizing the identifying assumption for our difference-in-differences analysis, we perform various other robustness checks. First, we run several placebo regressions in which we set different policy implementation dates. We restrict our sample to only include data prior to the policy change in states that have implemented the Medicaid suspension policy. Each column in Table A7 presents results from a placebo test with a different treatment year. For example, Column (1) presents results from a model that sets the policy implementation date to be one year before the actual policy change date in each treatment state. The existence of significant effects in placebo regressions would undermine our results. For instance, a negative finding would indicate that there has been a preexisting negative trend in treatment states' recidivism even before the implementation of the Medicaid suspen-

sion policies. Reassuringly, all our placebo regression results are statistically insignificant, positive, and small in magnitude, which supports our research design's validity.

Second, to investigate whether our results might be sensitivie to the inclusion of a particular state, we employ a leave-one-state-out method by dropping one state out of the sample and re-estimating our main regression model. In these regressions, we expect to see a significant change in our main results if they are sensitive to excluding data from any of the states in our sample. One-year and three-year recidivism results for the leave-one-out method are presented in Figure A6 and Figure A7, respectively. We report the point estimate of our main result along with its 95% confidence interval on the y-axis and the dropped state from the sample on the x-axis. All coefficient estimates are negative and statistically significant at the 5% level. There are minor changes in the magnitudes of the point estimate across regressions, but qualitatively our results are robust.

Third, Table A5 presents various other robustness checks. Our main results are presented in Table A5 Panel A. The probit estimates are negative, of similar magnitude, and statistical significance compared to our main results. We interpret these results as suggestive that our estimates are not dependent on functional form assumptions. Several studies using the older versions of the NCRP data have identified issues with data reliability and have used a subset of states to ensure consistency (Neal and Rick, 2016; Pfaff, 2011). For instance, using 1983-2002 NCRP data, Pfaff (2011) compares counts of individuals entering and exiting state prisons to other official counts such as the National Prisoner Statistics. He concludes that only eleven states consistently reported prisoner-level data to the NCRP: California, Colorado, Illinois, Kentucky, Michigan, Minnesota, Nebraska, New Jersey, South Dakota, Virginia, and Washington. Further, Neal and Rick (2016) use 1983-2009 NCRP data to conduct several checks and confirm that these eleven states consistently reported prison admissions. Therefore, in Table A5 Panel C we restrict our sample to these eleven states to evaluate the robustness of our results. Our estimate for the policy effect on 1-year recidivism is larger in magnitude (-4.07 percentage points), but the effect on three-year recidivism

is still statistically significant and of similar magnitude. However, these coefficients lose significance when using wild bootstrap *p-values*, which account for the potential for too few clusters. We also exclude states that expanded Medicaid early, namely Minnesota and New Jersey, from our estimation sample (Kaestner et al., 2017).[34] The results are presented in Table A5 Panel D. We observe very similar results to those in main analysis (Panel A), but the coefficient estimates are slightly greater and we lose significant on the policy effect on three-year recidivism.

Fourth, we test the sensitivity of our results to the inclusion in various time trends. Table A6 presents results that explore the sensitivity of our estimates to general and state-specific time trends.[35] Our estimates in general are not sensitive to the exclusion of state-specific time trends. In four different specifications the policy effect on one-year recidivism is between -2.53 and -2.91 percentage points and all estimates are statistically significant at the 10% level. On the other hand, various specifications produce policy effect estimates that vary between -4.48 and -4.6 percentage points. All coefficient estimates for the policy effect on three-year recidivism are statistically significant at the 10% level. We want to note that our difference-in-differences results without state-specific linear time trends are reported in Column 3. The estimated effect of the Medicaid suspension policy on three-year recidivism rates are of smaller magnitude but are still statistically significant. However, our results for the one-year recidivism disappear and is imprecisely estimated

Finally, Table A8 demonstrates substantial cross-state variation in public assistance take up rate in the prison population. This provides suggestive evidence that such variation might be present for Medicaid take-up rates in the prison population. Therefore, we examine whether the effect Medicaid suspension policies is heterogeneous in that dimension. We define a state to have a high share of inmates receiving public assistance if the share of inmates that do so is above the median in the SISFCF sample. We report our results in Table A9. Indeed, the effect of Medicaid suspension for states with high share of prisoners on welfare is larger in magnitude and highly statistically significant. This suggests that our

main results might potentially be a lower bound of the actual treatment effect of Medicaid suspension on recidivism.

7 Conclusions

Medicaid provides health insurance coverage to millions of low-income Americans. However, in many states, Medicaid coverage of inmates, a particularly needy and at-risk population, is terminated rather than suspended upon incarceration. Suspension of coverage potentially results in faster and easier restoration of Medicaid benefits upon release. Using NCRP data on offenders admitted between 2005 and 2013 and employing a difference-in-differences models, we find that adopting policies that suspend rather than terminate Medicaid upon incarceration decreases the probability of returning to prison within three years by 2.88 percentage points. This result is statistically significant and robust to the inclusion of various control variables and time trends. We find heterogeneous effects of the Medicaid suspension policy across gender and race. Our analysis speaks to an important policy discussion about the effect of implementation of policies that suspend Medicaid coverage upon incarceration on recidivism.

Our analysis and results are, of course, subjected to some limitations. The number of states that have adopted suspension policies has more than doubled in the past five years, which can be attributed in part to the expansion of Medicaid under the ACA. Because our sample ends in 2016 and because of the need to create a balanced sample of states that consistently report data to the NCRP, relatively fewer states are treated in our estimation sample. Many more states adopted Medicaid suspension policies since 2015, and thus assessing how these policies will affect those states is a research question that should be addressed in future work. In addition, our estimates should be interpreted as *intent-to-treat* because we do not observe prisoners' Medicaid status at prison admission. Our inability to observe whether individuals are Medicaid beneficiaries before or after prison also means that we do

not observe any efforts that correctional and public-assistance institutions make at outreach or assistance before inmates' reentry into the community. If prison-reentry planning, including Medicaid application assistance, is prevalent in most of our treated states, then our results might be biased upwards. Lastly, our study is unable to speak to what the driving mechanisms are for the effects we observe. We believe that exploring these mechanisms is a fruitful future agenda that has big policy implications.

Ultimately, our analysis of Medicaid suspension speaks to prisoner-reentry policy in general. In a review of the literature, Doleac (2019) discusses how health care access could be critical for re-integrating inmates. Although it is important to learn how to enroll the justice-involved people in Medicaid, it is equally important to develop policies and systems to keep them from losing coverage and allow them to re-enroll faster upon release. Even more, our analysis contributes to an active policy discussion about the urge whether state Medicaid agencies should suspend rather than terminate Medicaid coverage upon incarceration.

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Notes

[1]Compared to the general US population, incarcerated individuals are much more likely to have chronic physical and mental health conditions, such as HIV/AIDS, a serious mental illness, or a substance abuse disorder (Binswanger et al., 2009).

[2]Even though inmates in correctional institutions can remain eligible for Medicaid in many states, the Social Security Act of 1935 prohibits states from billing Medicaid for any inmate care while in custody unless the covered individual requires an off-site hospital stay of twenty-four hours or more.

[3]Under federal guidelines, eligibility determination takes between 45 to 90 days. See, U.S. Code of Federal Regulations, Title 42, Public Health, §435.911.

[4]Some states limit suspension to a certain time frame after which they terminate Medicaid coverage. This time frame might or might not cover the whole incarceration duration. This is mostly done to avoid terminating coverage for those serving short sentences.

[5]It is well documented that Medicaid expansion and the ACA reduced the coverage gap across income and racial groups (Courtemanche et al., 2019).

[6]Individuals whose coverage is suspended upon incarceration could still face a gap in continuity of health care, although that gap is expected to be much shorter than if their coverage was terminated.

[7]Unfortunately, we are unable to examine the exact mechanisms through which Medicaid suspension policies affect recidivism because of lack of data at the national level. Ideally, we would need national data on Medicaid enrollment, healthcare utilization, especially mental health treatment, and employment of released prisoners.

[8]Note that the differences between these groups are not statistically significant.

[9]For more information refer to `https://www.congress.gov/bill/115th-congress/house-bill/165`, `https://www.govtrack.us/congress/bills/115/s874/details`, and/or `https://www.congress.gov/bill/115th-congress/house-bill/4005/actions`.

[10]Our main estimation sample include two states (Minnesota and North Carolina) with time-limited suspension.

[11]In some states, Medicaid suspension policies are not legislated by law, but rather are internal agency policies. However, both the legislative and administrative implementation of Medicaid suspension work in similar ways and are equally legally binding.

[12]The Centers for Medicare & Medicaid Services (CMS) has long encouraged states not to terminate coverage for enrolled inmates during their time in correctional facilities, but rather to temporarily suspend it until release (Centers for Medicare and Medicaid Services, 2014, 2016).

[13]States have the option to use federal funding to implement these changes in their systems, as mentioned in a 2013 CMS informational bulletin.

[14]While this estimate does not directly show how many individuals are on Medicaid prior to incarceration, it suggest that a non-trivial proportion of inmates might be receiving Medicaid or might likely be eligible for Medicaid due to being low-income. In addition, the proportion of inmates receiving public assistance prior to incarceration varies significantly across states, as shown in Table A8.

[15]Actual time served can differ from the sentence imposed because of early release via parole or time credited (Zapryanova, 2020).

[16]We also drop individuals admitted to prison prior to 2000 because well-documented reliability issued of the NCRP (Yang, 2017b).

[17]In earlier versions of our paper, we included more pre-treatment periods on the expense of making the estimation sample less balanced. Yet, our results remained relatively stable, but the pre-trends hinted to a slight trend in the outcome of interest.

[18]While we acknowledge that out-of-state relocation could be a problem, we believe it will not have large effects on our results for two main reasons. First, almost half of the released prisoners in our sample are released on parole, which often requires ex-prisoners to stay in the state they are released in. Second, (Durose et al., 2014) estimate that 3% of the released prisoners in one of the 30 states they have sampled were arrested out of state within one year and 7% were arrested out of state within 3 years. Moreover, the number of people imprisoned will be even fewer than the number of people arrested.

[19]We cannot test directly whether the implementation of these policies is truly exogenous. We acknowledge that our results could be conflated if increase in Medicaid enrollment led to excess demand for services or a supply shock.

[20]We also calculate recidivism rates by crime types to explore whether the treatment has any heterogeneous effects.

[21]Note, however, that in our main specification we include release year fixed effects to control for yearly shocks to the environment the prisoner is released into that might affect the likelihood of recidivism. In a robustness check in Table A6, we show that our results are not sensitive to excluding these fixed effects.

[22]State unemployment rates are obtained from the Bureau of Labor Statistics while the number of state sworn police officers per 1,000 comes from the FBI's Law Enforcement Officers Killed or Assaulted (LEOKA) program.

[23]In Section 6.2, we show that our results are robust to excluding state-specific linear time trends.

[24]We experimented with different leads and lags and our results are robust to the event window definition.

[25]For example, $Suspension_{1(st)}$ is an indicator that equals to 1 if prisoner i is admitted in a state between

one and two years after the state starts suspending Medicaid coverage and 0 otherwise.

[26]In Section 6.2, we provide evidence that our results are robust to various specifications, modeling assumptions, and sample restrictions.

[27]Using the 2004 SISFCF data, we calculate that about a quarter of state prisoners report to have received public assistance prior to being incarcerated. If we extrapolate from this estimate that Medicaid enrollment rate of prisoners is the same and assume recidivism rates are similar during the pre-treatment years between prisoners that receive public assistance and prisoners that dont, we can do a back-of-the-envelope calculation that the effect of the policy on the treated is about 18pp.

[28]We examine the pretrends in more detail in Section 6.2.

[29]In Figure 4 we present the associated event studies. We do not observe significant pre-trends in any return crime type which boost our confidence in our difference-in-differences results.

[30]We explore two additional types of recidivism (weapon-related and DUI) and we present our event-study results in Appendix Figure A3. We observe similar impacts of the policy although the point estimates are smaller and borderline significant. We also find suggestive evidence that that Medicaid suspension laws affect re-incarceration for "other" crimes, such as blackmailing and public order offense.

[31]The event studies also show that pretreatment period coefficients are not statistically different from zero, which indicates no differential pretreatment period trends between the control and treatment groups.

[32]Because only North Carolina is a treated state that suspends Medicaid administratively, these results should be interpreted with caution.

[33]We obtain the Medicaid expansion dates from the Kaiser Family Foundation. Minnesota and New Jersey expanded Medicaid in 2011 and are classified as early expansion states in our analysis. Note that only a small fraction of the prisoners in our sample (0.9%) were admitted in prison after these states expanded Medicaid so our results provide suggestive evidence of how the effect of Medicaid suspension policies varies by whether or not Medicaid is expanded.

[34]Although six states, California, Connecticut, Washington, D.C., Minnesota, New Jersey, and Washington expanded Medicaid early in 2011 (Kaestner et al., 2017), only two of them are in our balanced sample of the NCRP.

[35]Time is defined as prison admission year.

8 Tables

Table 1: Summary Statistics: Individual characteristics

	1-year Recidivism (1)		3-year Recidivism (2)	
	Mean	S.D.	Mean	S.D.
Black	0.430	0.495	0.432	0.495
White	0.464	0.499	0.464	0.499
Male	0.878	0.327	0.878	0.327
Female	0.122	0.327	0.121	0.327
Hispanic	0.127	0.333	0.128	0.334
Less than HS Degree	0.393	0.488	0.396	0.489
HS Degree	0.326	0.469	0.327	0.469
Some college	0.048	0.214	0.048	0.214
College Degree	0.007	0.086	0.008	0.086
Age at Release	35.174	10.431	35.177	10.423
Prior Felony Incarceration	0.320	0.466	0.323	0.468
Time served (days)	512.350	595.321	515.158	595.838
Sentence length (months)	56.946	73.463	57.286	73.626
New court commitment	0.657	0.475	0.661	0.473
Parole revocation	0.172	0.377	0.174	0.379
Probation revocation	0.080	0.271	0.080	0.272
Parole discretionary	0.331	0.471	0.335	0.472
Parole mandatory	0.168	0.374	0.169	0.375
Shock probation	0.081	0.273	0.080	0.271
Sentence expiration	0.324	0.468	0.325	0.468
Escape	0.003	0.052	0.003	0.052
Violent Offense	0.199	0.400	0.199	0.399
Property Offense	0.294	0.456	0.296	0.456
Drug Offense	0.305	0.460	0.306	0.461
Weapons offense	0.038	0.191	0.038	0.192
DUI offense	0.051	0.220	0.050	0.218
Other offense	0.109	0.312	0.108	0.310
Suspension	0.096	0.295	0.095	0.293
Expansion	0.009	0.096	0.009	0.097
N	3503372		3466574	

Notes: The samples in Column (1) and (2) consist of individuals who have at least 1 year and 3 year of post-release data, respectively. Listed offense types are indicators for the offense for which the offender initially went to prison. Expansion represent the average value of the early Medicaid expansion indicator for the state and month in which the offender was admitted to prison. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4.

Table 2: Summary Statistics: Recidivism

	1-year Recidivism (1)		3-year Recidivism (2)	
	Mean	S.D.	Mean	S.D.
Overall recidivism	0.194	0.396	0.378	0.485
Violent recidivism	0.039	0.193	0.079	0.269
Property recidivism	0.064	0.245	0.128	0.334
Drug recidivism	0.053	0.224	0.112	0.316
Other crime recidivism	0.038	0.192	0.078	0.268
N	3503372		3466574	

Notes: Column (1) has fewer observations to allow everyone to have 3 years of post-release data, where as Column (2) only requires 1 year of post-release data. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4.

Table 3: Effect of Medicaid Suspension Policies on Overall Recidivism

	Panel A: 1-year Recidivism		
	(1)	(2)	(3)
Suspension	-0.0240	-0.0296***	-0.0291***
	(0.0142)	(0.00795)	(0.00813)
Mean Dept. Var.	0.195	0.195	0.194
R-squared	0.0542	0.0891	0.0903
N	3606636	3605989	3503372
	Panel B: 3-year Recidivism		
	(1)	(2)	(3)
Suspension	-0.0401	-0.0482**	-0.0458**
	(0.0273)	(0.0221)	(0.0222)
Mean Dept. Var.	0.379	0.379	0.378
R-squared	0.0419	0.0993	0.100
N	3568402	3568088	3466574
State FE	Yes	Yes	Yes
Admission year FE	Yes	Yes	Yes
State-specific linear trends	Yes	Yes	Yes
Individual characteristics	No	Yes	Yes
State characteristics	No	No	Yes

Notes: The dependent variable is individual's probability to return to prison within 1 year or 3 years of release in Panel A and Panel B, respectively. Each column is estimated by a separate OLS. Suspension is an indicator for whether a state suspends Medicaid coverage upon incarceration. We control for demographic characteristics (race, ethnicity, gender, age, age squared, highest graded completed, prior felony incarceration indicator) and current crime and prison characteristics (main offense type, number of convicted counts, total sentence imposed, reason for release, time served, time served squared) as well as indicators for missing data on each of the time-invariant characteristics. In addition, we include time-varying state characteristics (unemployment rate and the number of sworn police officers per 1000 in the population) and an indicator whether the state expanded Medicaid under the ACA. We also include state, release year, and admission year fixed effects as well as state-specific admission year linear time trend. Robust standard errors clustered by state. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4. 4.
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Effect of Medicaid Suspension Policies on Overall Recidivism by Return Crime Type

	Any	Violent	Property	Drug
	Panel A: 1-year Recidivism			
	(1)	(2)	(3)	(4)
Suspension	-0.0291***	-0.00644	-0.00638***	-0.00675*
	(0.00813)	(0.00429)	(0.00172)	(0.00382)
Mean Dept. Var.	0.194	0.0389	0.0642	0.0528
R-squared	0.0903	0.121	0.133	0.104
N	3503372	3503372	3503372	3503372
	Panel B: 3-year Recidivism			
	(1)	(2)	(3)	(4)
Suspension	-0.0458**	-0.0129	-0.00984***	-0.0134
	(0.0222)	(0.00992)	(0.00339)	(0.00911)
Mean Dept. Var.	0.378	0.0787	0.128	0.112
R-squared	0.100	0.161	0.195	0.157
N	3466574	3466574	3466574	3466574

Notes: The dependent variable is individual's probability to return to prison within 1 year (Panel A) and 3 years (Panel B) of release with any crime (Columns (1)) or with the crime indicated in the column heading (Columns (2)-(4)). Each column is estimated by a separate OLS. Suspension is an indicator for whether a state suspends Medicaid coverage upon incarceration. We control for demographic characteristics (race, ethnicity, gender, age, age squared, highest graded completed, prior felony incarceration indicator) and current crime and prison characteristics (main offense type, number of convicted counts, total sentence imposed, reason for release, time served, time served squared) as well as indicators for missing data on each of the time-invariant characteristics. In addition, we include time-varying state characteristics (unemployment rate and the number of sworn police officers per 1000 in the population) and an indicator whether the state expanded Medicaid under the ACA. We also include state, release year, and admission year fixed effects as well as state-specific admission year linear time trend. Robust standard errors clustered by state. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4.

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Effect of Medicaid Suspension Policies on Overall Recidivism by Gender and Race

	Main	Male	Female	White	Black
	Panel A: 1-year Recidivism				
	(1)	(2)	(3)	(4)	(5)
Suspension	-0.0291***	-0.0296***	-0.0206***	-0.0259***	-0.0315***
	(0.00813)	(0.00804)	(0.00738)	(0.00680)	(0.00968)
Mean Dept. Var.	0.194	0.200	0.152	0.190	0.201
R-squared	0.0903	0.0903	0.0878	0.0944	0.0881
N	3503372	3075470	427682	1995954	1507418
	Panel B: 3-year Recidivism				
	(1)	(2)	(3)	(4)	(5)
Suspension	-0.0458**	-0.0465*	-0.0318**	-0.0327**	-0.0574*
	(0.0222)	(0.0231)	(0.0136)	(0.0134)	(0.0295)
Mean Dept. Var.	0.378	0.390	0.293	0.362	0.399
R-squared	0.100	0.0980	0.0966	0.103	0.0987
N	3466574	3045350	421007	1968365	1498209

Notes: The dependent variable is individual's probability to return to prison within 1 year (Panel A) and 3 years (Panel B) of release . Each column is estimated by a separate OLS. Suspension is an indicator for whether a state suspends Medicaid coverage upon incarceration. Coefficients show the effect of Medicaid suspension (as opposed to termination) on recidivism by gender (Columns (2) and (3)) and race (Columns (4)-(6)). Our main results are presented in Column (1) for comparison. We control for demographic characteristics (race, ethnicity, gender, age, age squared, highest graded completed, prior felony incarceration indicator) and current crime and prison characteristics (main offense type, number of convicted counts, total sentence imposed, reason for release, time served, time served squared) as well as indicators for missing data on each of the time-invariant characteristics. In addition, we include time-varying state characteristics (unemployment rate and the number of sworn police officers per 1000 in the population) and an indicator whether the state expanded Medicaid under the ACA. We also include state, release year, and admission year fixed effects as well as state-specific admission year linear time trend. Robust standard errors clustered by state. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4.

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Effect of Medicaid Suspension Policies on Overall Recidivism: First-time vs Repeat Offenders

	1-year Recidivism		3-year Recidivism	
	First-time	Repeat	First-time	Repeat
	(1)	(2)	(3)	(4)
Suspension	-0.0177**	-0.0291***	-0.0178**	-0.0299***
	(0.00636)	(0.00702)	(0.00602)	(0.00864)
Mean Dept. Var.	0.140	0.217	0.303	0.438
R-squared	0.0926	0.0877	0.111	0.0764
N	1229458	1119502	1229458	1119502

Notes: The dependent variable is individual's probability to return to prison within 1 year or 3 years of release. Suspension is an indicator for whether a state suspends Medicaid coverage upon incarceration. Each column is estimated by a separate OLS. Coefficients show the effect of Medicaid suspension (as opposed to termination) on recidivism within 1 year and 3 years of release, respectively, for first-time and repeat offenders. We control for demographic characteristics (race, ethnicity, gender, age, age squared, highest graded completed, prior felony incarceration indicator) and current crime and prison characteristics (main offense type, number of convicted counts, total sentence imposed, reason for release, time served, time served squared) as well as indicators for missing data on each of the time-invariant characteristics. In addition, we include time-varying state characteristics (unemployment rate and the number of sworn police officers per 1000 in the population) and an indicator whether the state expanded Medicaid under the ACA. We also include state, release year, and admission year fixed effects as well as state-specific admission year linear time trend. Robust standard errors clustered by state. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4 restricted to firt-time and repeat offenders..

*** p<0.01, ** p<0.05, * p<0.1

9 Figure Captions

Figure 1: Effect of Medicaid Suspension Policies on Overall Recidivism

Figure 2: Effect of Medicaid Suspension Policies on Overall Recidivism: Female

Figure 3: Effect of Medicaid Suspension Policies on Overall Recidivism: Male

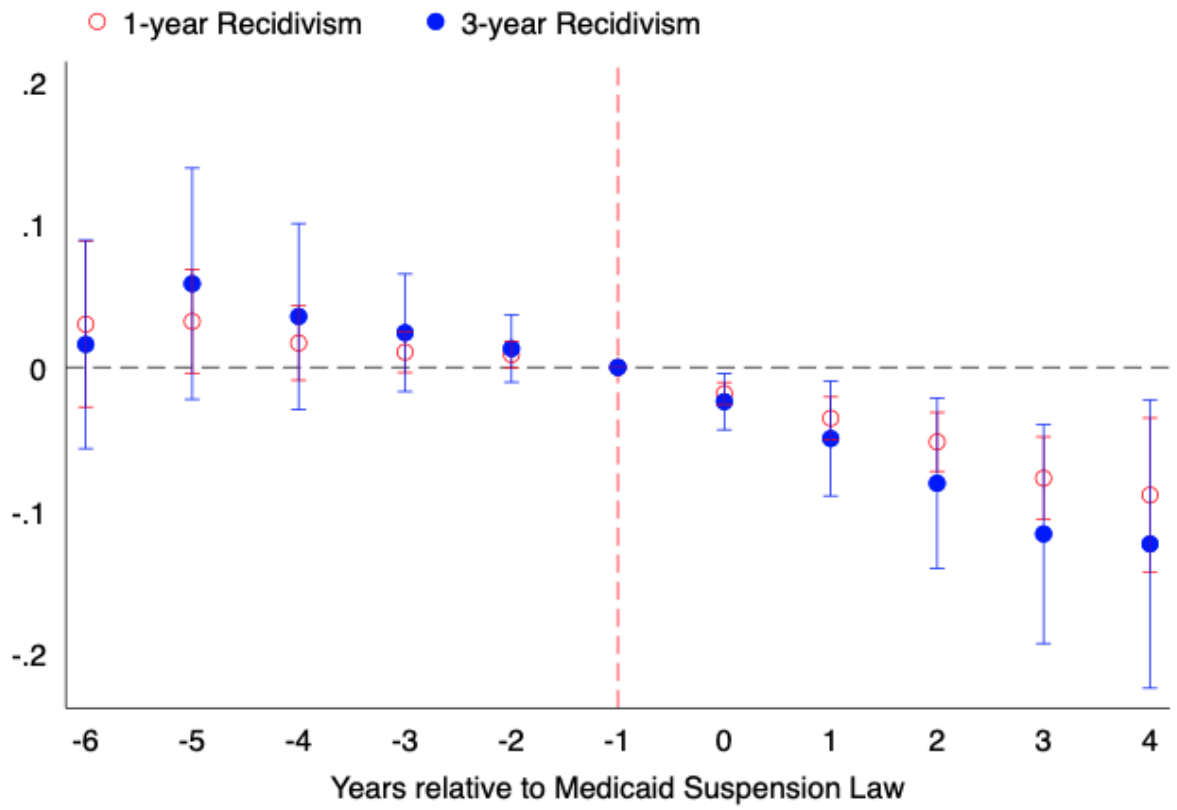
Figure 4: Effect of Medicaid Suspension Policies on Overall Recidivism by Reoffending Crime Type

Figure 5: Effect of Medicaid Suspension Policies on 1-year Recidivism: pre-trend sensitivity analysis

Figure 6: Effect of Medicaid Suspension Policies on 3-year Recidivism: pre-trend sensitivity analysis

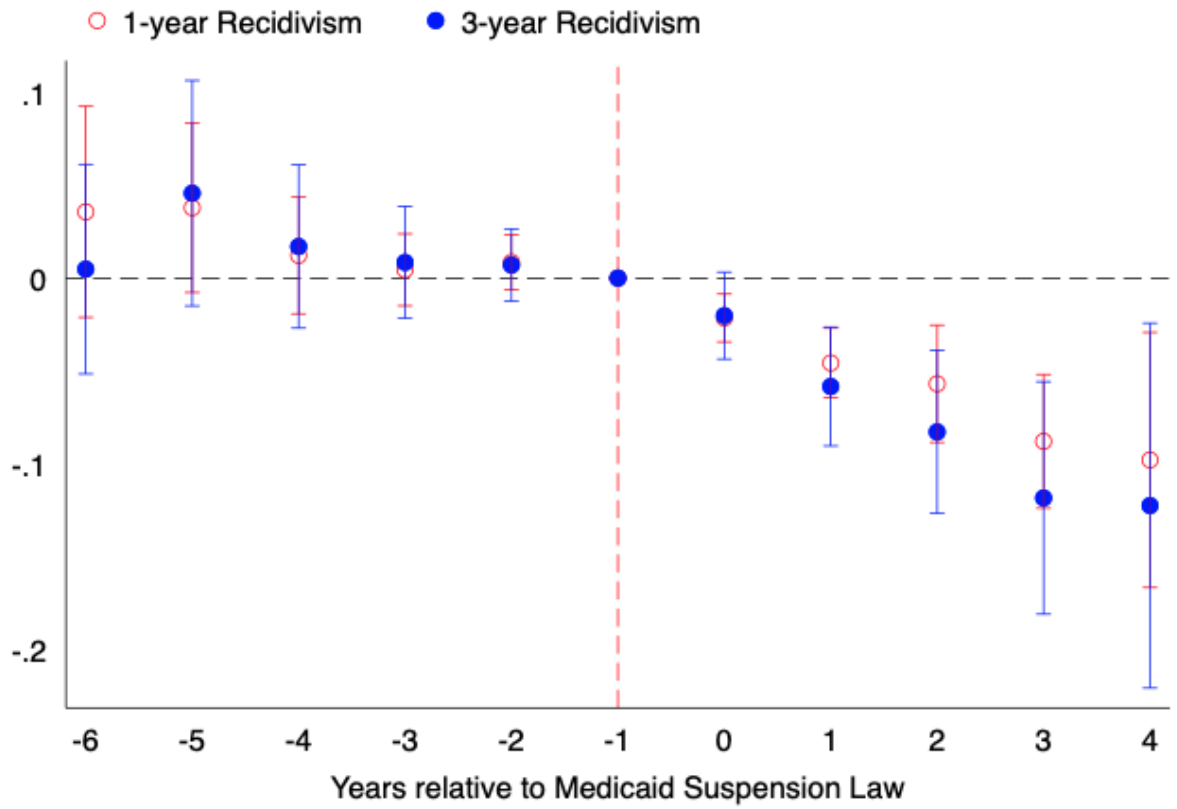
10 Figures

Figure 1



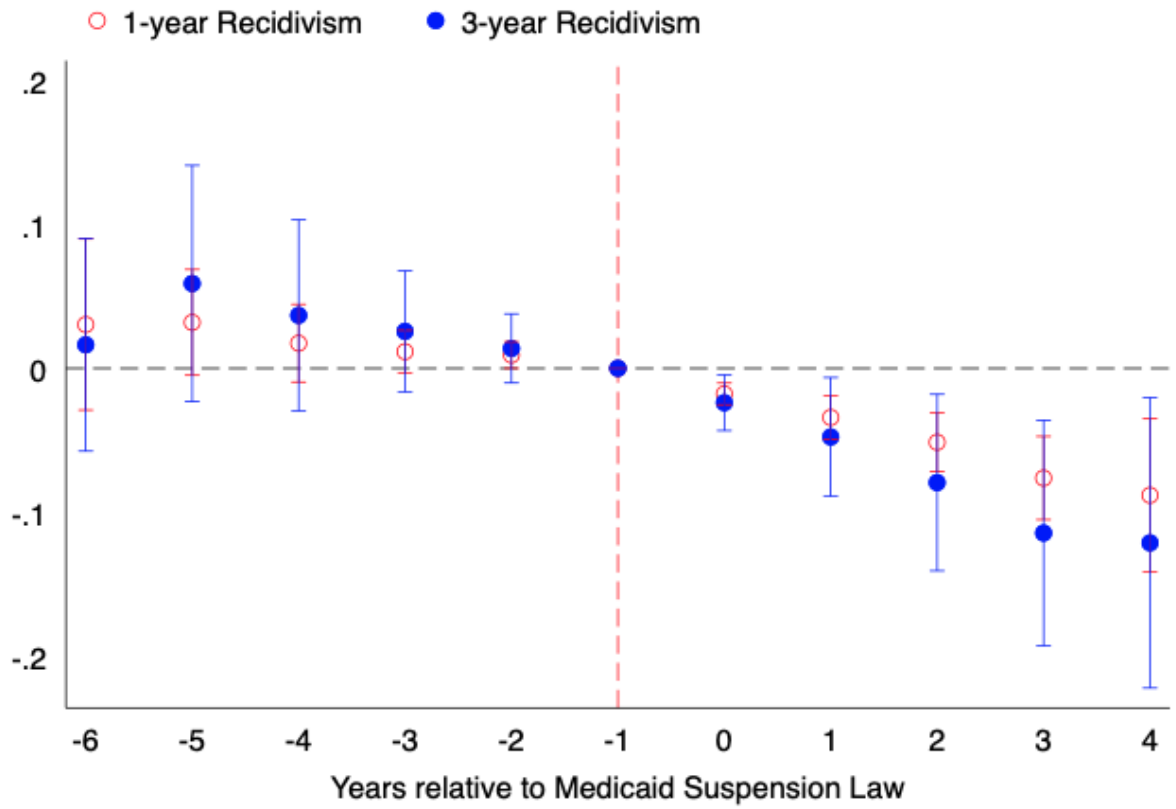
Notes: This figure plots the coefficient estimates (β_L) of Equation (2) and the corresponding 95 percent confidence bands. The outcome of interest is the probability of returning to prison within 1 year or 3 year of release. The omitted dummy is "year prior to implementation" of the policy that suspends Medicaid upon incarceration, so that coefficient β_{-1} has been set to zero. The first category $L = -6$ represents a prisoner admitted to prison 6 or more years prior to the implementation of Medicaid suspension policies for incarcerated individuals, and the final category $L = 4$ represents a prisoner admitted to prison 4 or more years after the implementation of the policy. We control for individual demographic characteristics (race, ethnicity, gender, age, age squared, highest graded completed, prior felony incarceration indicator) and reason for prison admission. We also include indicators for missing data on each of these control variables, as well as state fixed effects, admission year fixed effects, and state-specific time trends. Release year fixed effects are also included. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4.

Figure 2



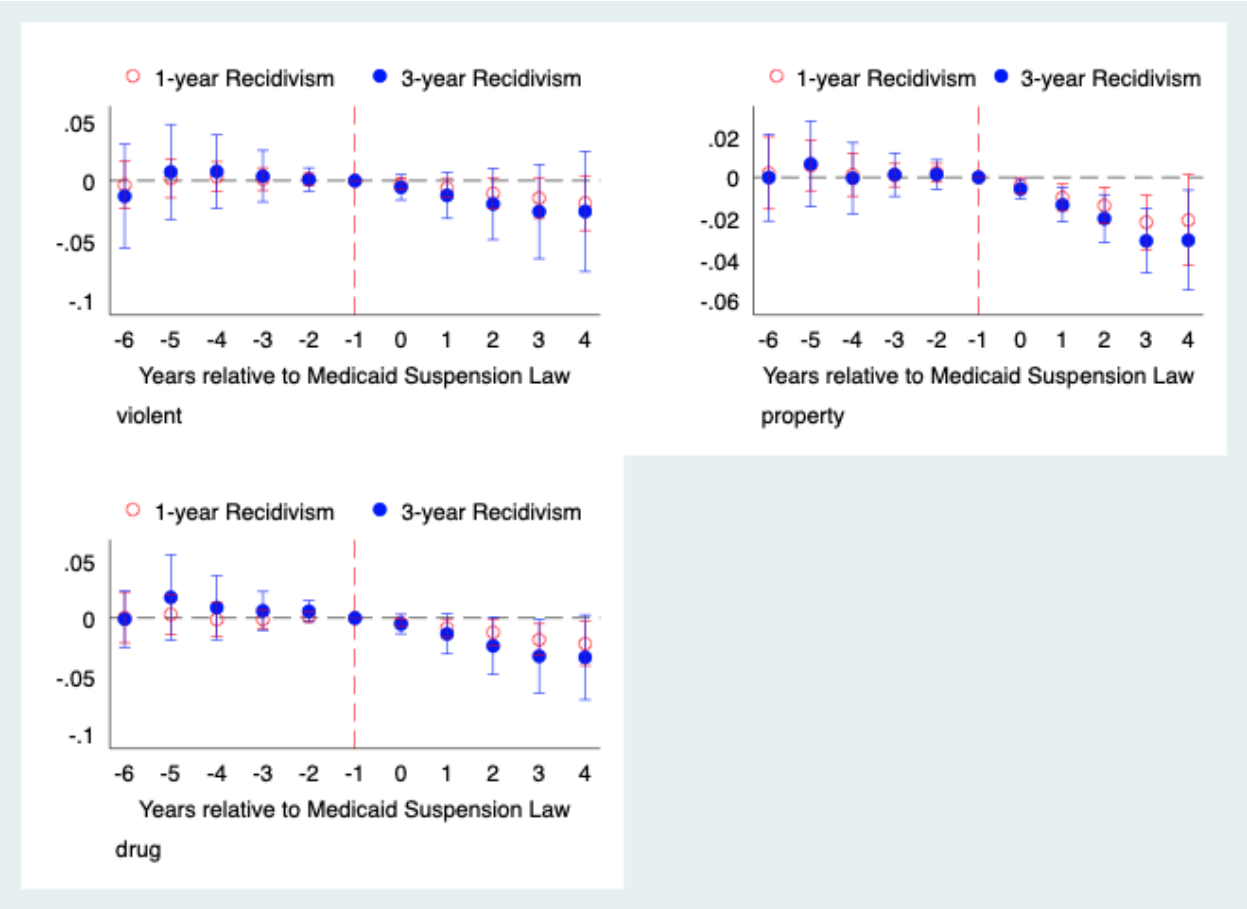
Notes: The outcome of interest is the probability of returning to prison within 1 year or 3 years of release. Sample is restricted to female offenders. See notes for Figure 1 for more detail.

Figure 3



Notes: The outcome of interest is the probability of returning to prison within 1 year or 3 years of release. Sample is restricted to male offenders. See notes for Figure 1 for more detail.

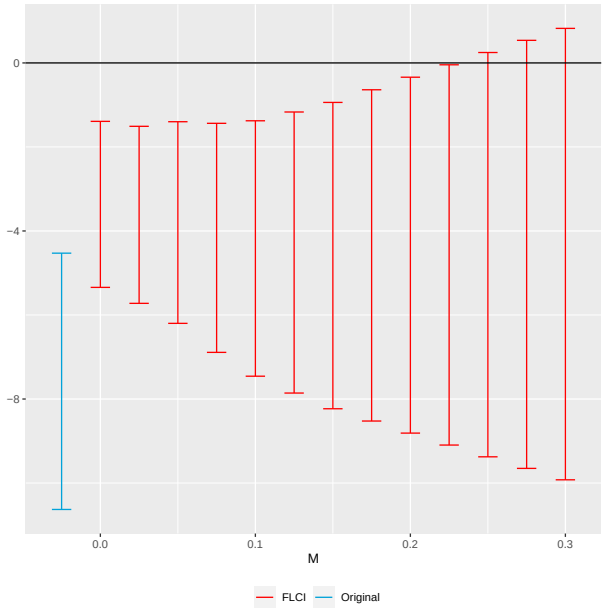
Figure 4



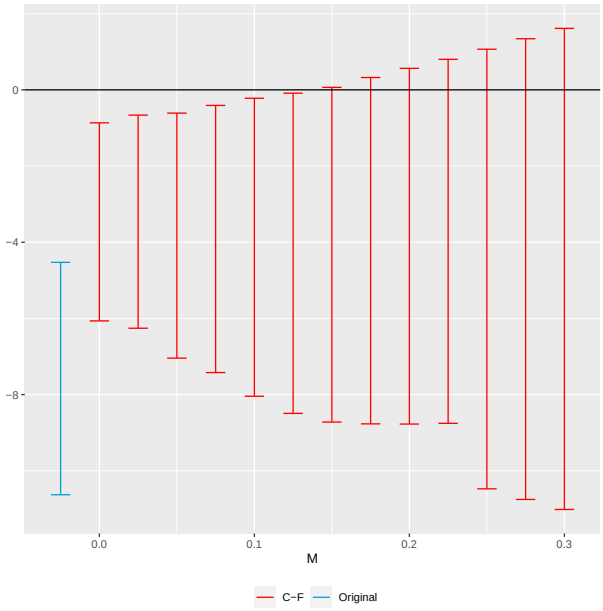
Notes: The outcome of interest is the probability of returning to prison within 1 year or 3 years of release with the crime specified in each panel. See notes for Figure 1 for more detail.

Figure 5

(a) non-linear time trend difference



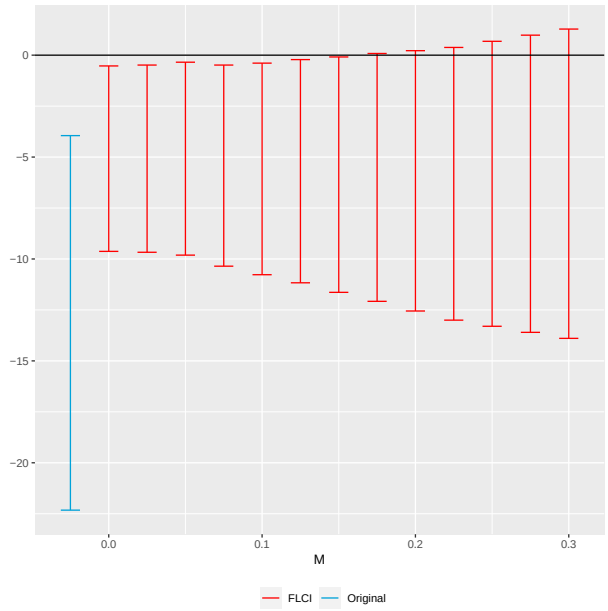
(b) monotonically decreasing time trend difference



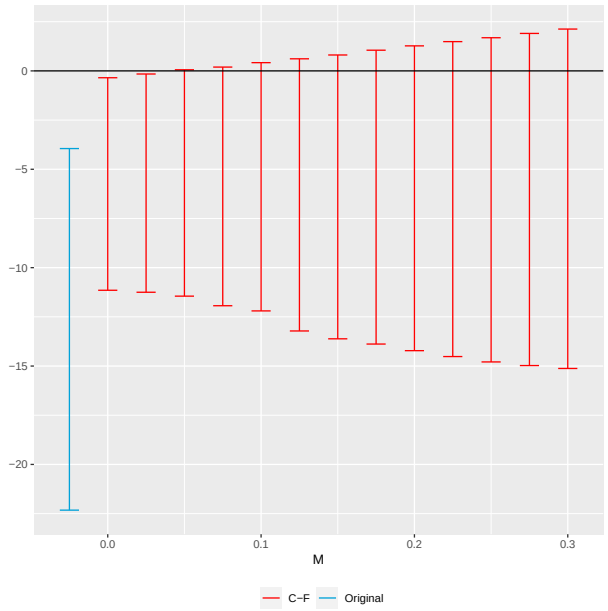
Notes: Figures show sensitivity analysis of estimated effects on 1-year recidivism rate to potential violations of the parallel trends assumptions per Rambachan and Roth (2020). We rescaled all point estimates and their corresponding confidence bands by 100 so that the results are in units of percentage points. The blue bar in each panel represents the 95% confidence interval of the DD estimate for relative time $t = 3$ from Equation (2). The red bars represent corresponding 95% confidence intervals when allowing for per-period violations of parallel trends of up to M. That is, M represents the largest allowable change in the slope of an underlying linear trend between two consecutive periods. Note that a treatment group-specific linear trend (M = 0) still allows for linear violations of the parallel trends assumption. Panel a) and b) show the sensitivity of our results under non-linear and monotonically decreasing non-linear time trend difference assumption, respectively.

Figure 6

(a) non-linear time trend difference



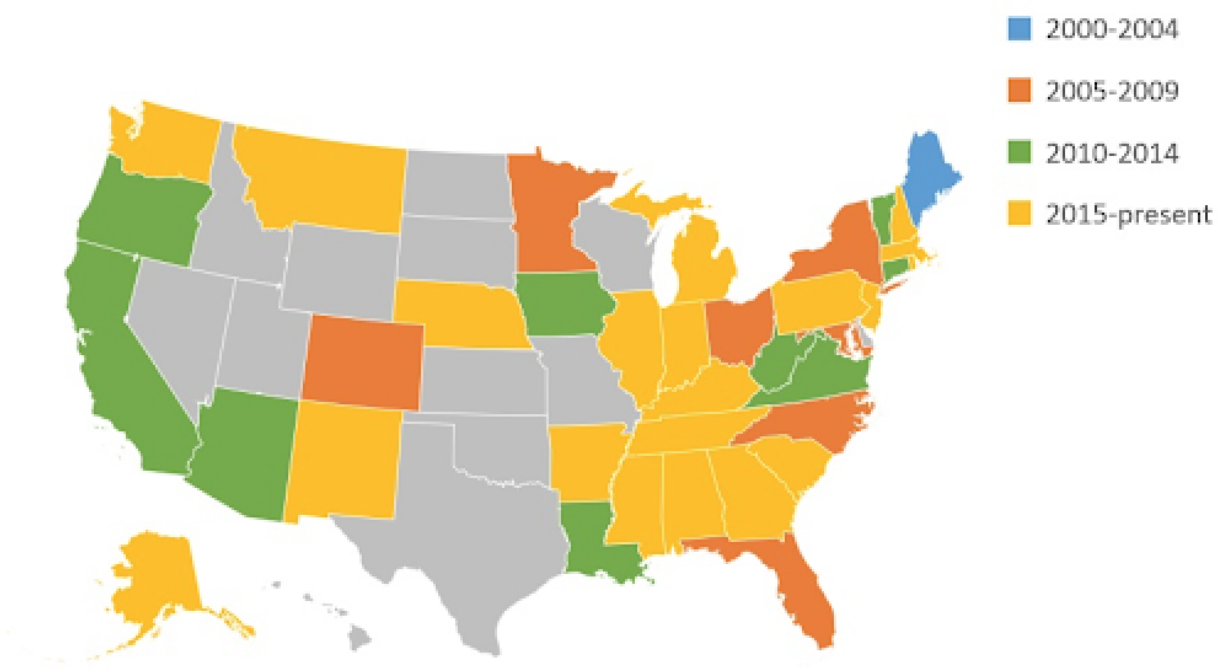
(b) monotonically decreasing time trend difference



Notes: Figures show sensitivity analysis of estimated effects on 3-year recidivism rate to potential violations of the parallel trends assumptions per Rambachan and Roth (2020). We rescaled all point estimates and their corresponding confidence bands by 100 so that the results are in units of percentage points. The blue bar in each panel represents the 95% confidence interval of the DD estimate for relative time $t = 3$ from Equation (2). The red bars represent corresponding 95% confidence intervals when allowing for per-period violations of parallel trends of up to M. That is, M represents the largest allowable change in the slope of an underlying linear trend between two consecutive periods. Note that a treatment group-specific linear trend (M = 0) still allows for linear violations of the parallel trends assumption. Panel a) and b) show the sensitivity of our results under non-linear and monotonically decreasing non-linear time trend difference assumption, respectively.

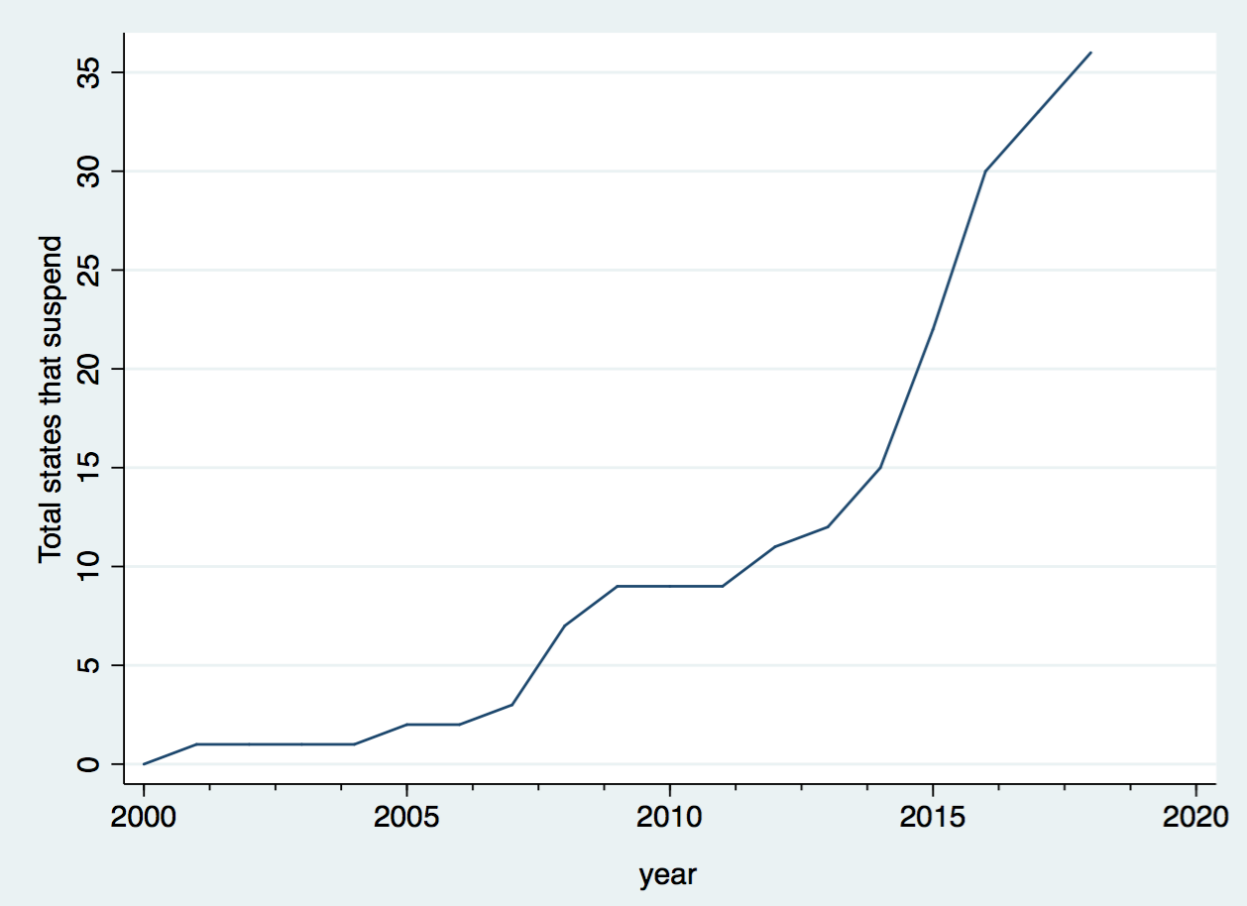
A Appendix Figures and Tables

Figure A1: Timing of Medicaid Suspension Policies



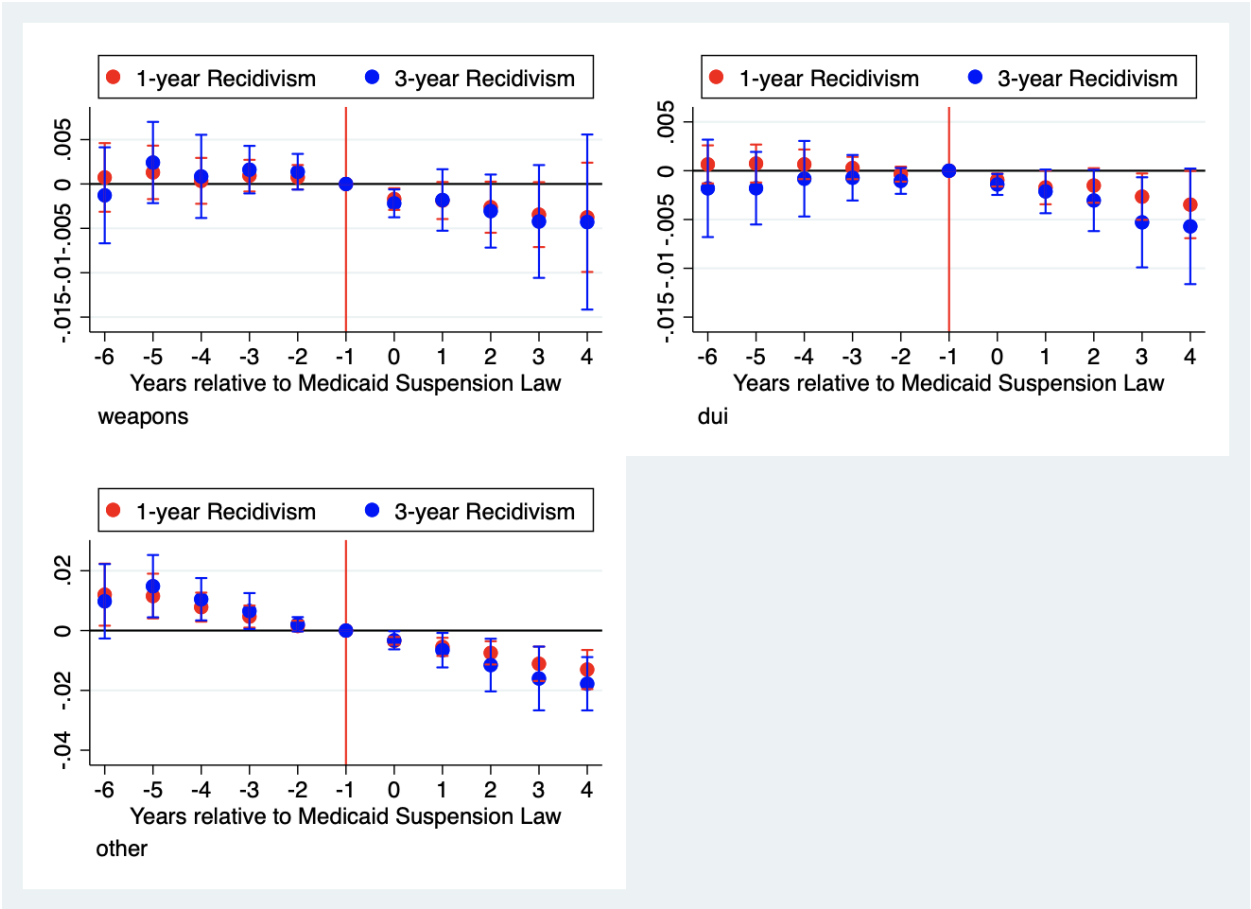
Notes: See notes in Table A1 for more detail on the source of our data. States in grey terminate Medicaid coverage.

Figure A2: Changes in States' Medicaid Suspension Policies for Prisoners over Time



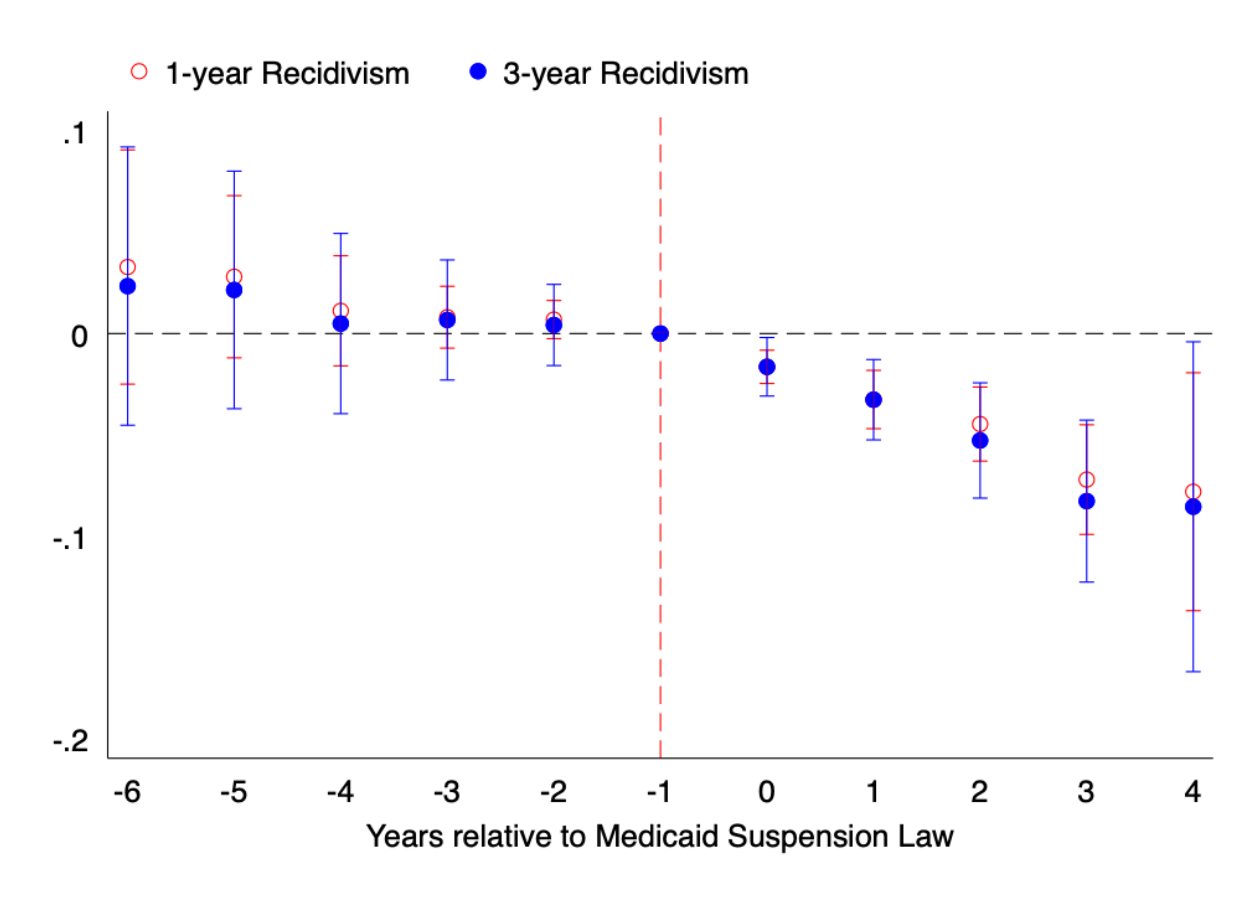
Notes: See notes in Table A1 for more detail on the source of our data.

Figure A3: Effect of Medicaid Suspension Policy on Overall Recidivism by Reoffending Crime Type: Weapons, DUI, and Other



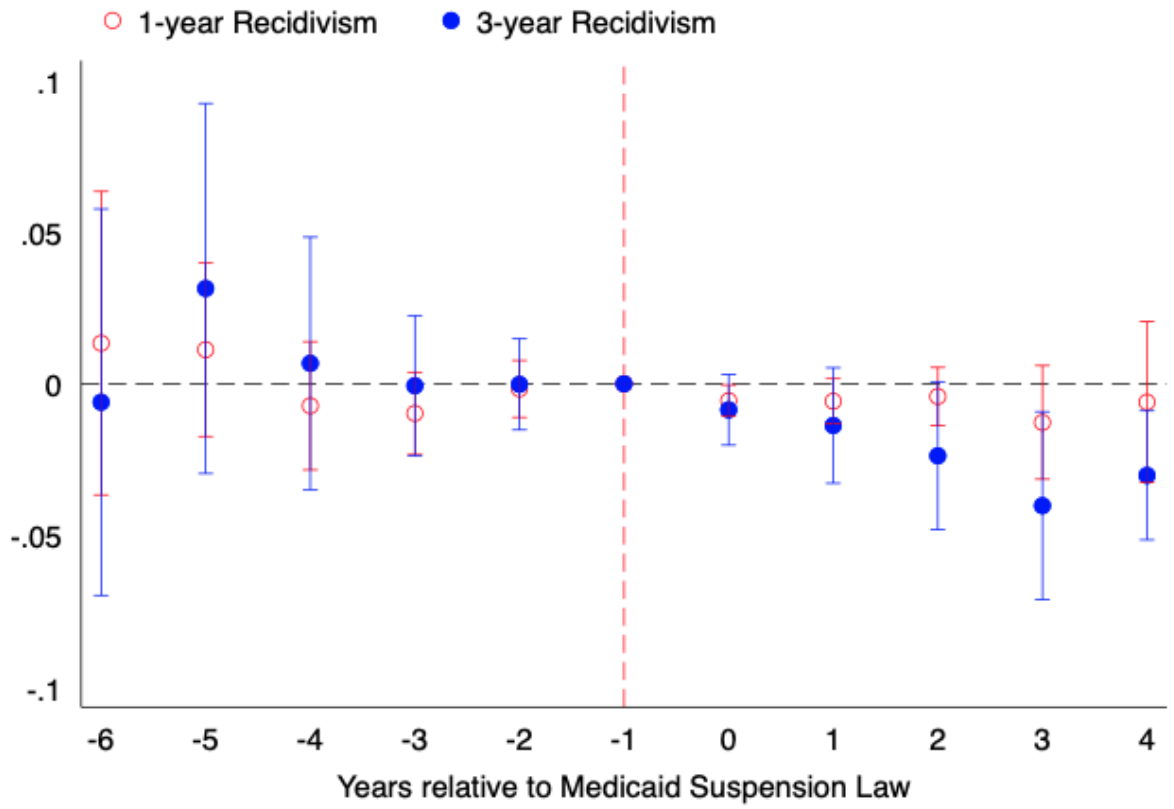
Notes: The outcome of interest is the probability of returning to prison within 1 year or 3 years of release with weapon-related crime, DUI crime, or any other type of crime (e.g., blackmail, public order offense, etc.). See notes of Figure 1 for more detail.

Figure A4: Effect of Medicaid Suspension Policies on Overall Recidivism: Treated states include only those that passed the Medicaid suspension policy in 2008



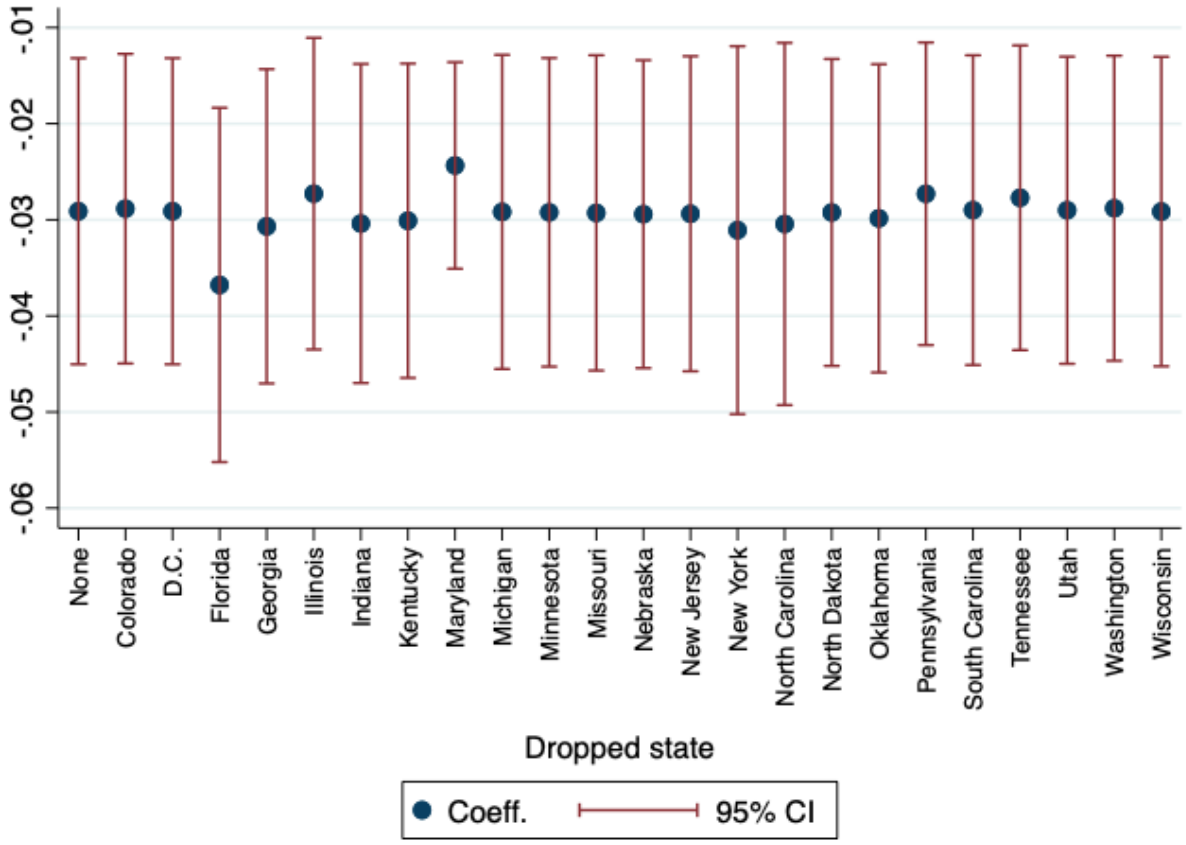
Notes: The outcome of interest is the probability of returning to prison within 1 year or 3 years of release with the crime specified in each panel. We include only three treated states (Florida, New York, North Carolina), all of which passed the law in 2008. See notes of Figure 1 for more detail. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4.

Figure A5: Effect of Medicaid Suspension Policies on Overall Recidivism: No state-specific linear time trends



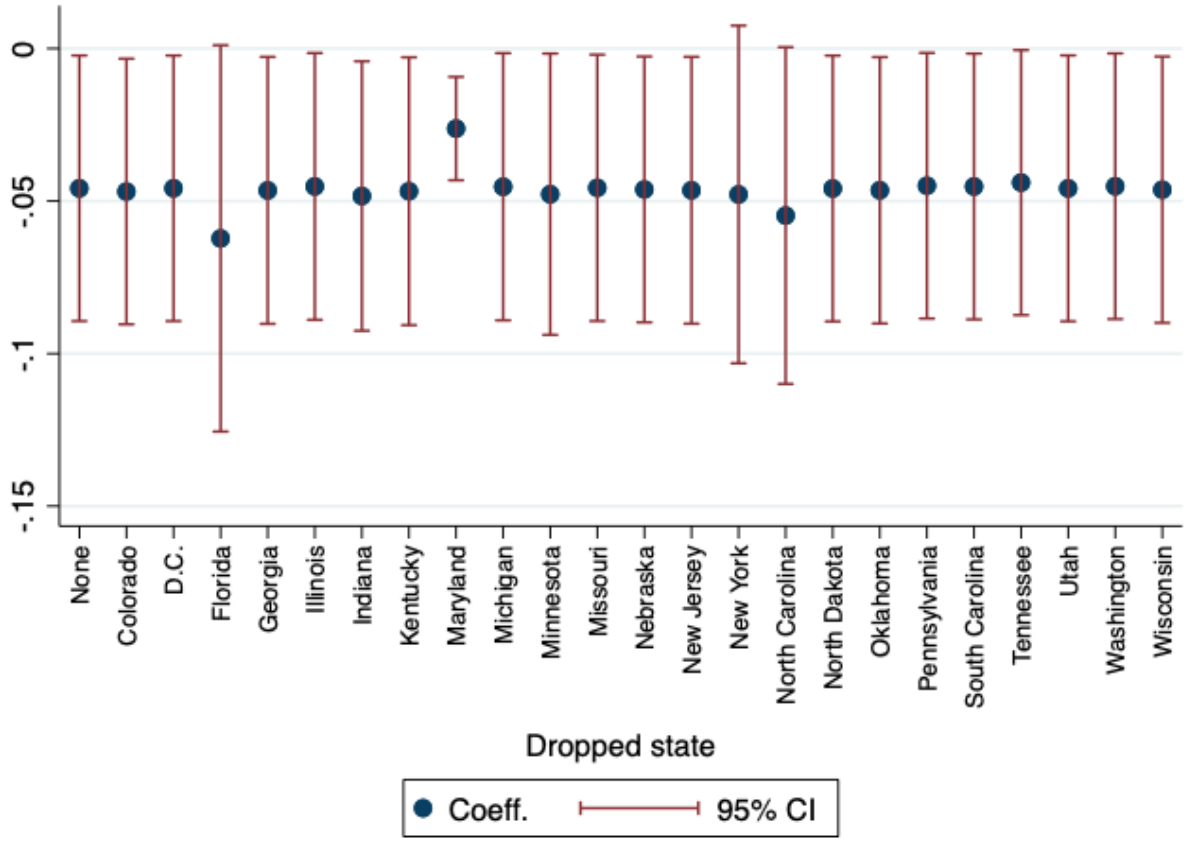
Notes: The outcome of interest is the probability of returning to prison within 1 year or 3 years of release. See notes for Figure 1 for more detail. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4.

Figure A6: Alternative Specifications for the Effect of Medicaid Suspension Policies on 1-year Overall Recidivism: Leave-One-Out Method



Notes: The figure reports the coefficient estimates of the full model in Table 3 Panel A along with their 95% confidence intervals resulting from dropping out data from one specific state at a time. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4.

Figure A7: Alternative Specifications for the Effect of Medicaid Suspension Policies on 3-year Overall Recidivism: Leave-One-Out Method



Notes: The figure reports the coefficient estimates of the full model in Table 3 Panel B along with their 95% confidence intervals resulting from dropping out data from one specific state at a time. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4.

Table A1: State Medicaid Policies for Incarcerated Individuals and NCRP Coverage

	(1)	(2)	(3)	(4)	(5) NCRP prison release year																				
State	Policy	Time Limit	Impl. Date	Type	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
Alabama	suspend		1/1/2018	legislative													X	X	X	X	X	X	X	X	X
Alaska	suspend		7/11/2016	legislative											X	X	X	X	X	X	X	X	X		
Arizona	suspend	12	8/1/2014	administrative						X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Arkansas	suspend	12	8/1/2015	administrative																					
California	suspend		1/1/2014	legislative	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Colorado	suspend		3/1/2017	legislative						X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Connecticut	suspend		6/1/2015	administrative																					
D.C.	suspend		6/9/2017	administrative								X	X	X	X	X	X	X	X	X	X	X	X	X	
Delaware	terminate			administrative															X	X	X	X	X	X	X
Florida	suspend		7/1/2008	legislative		X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Georgia	suspend	18	7/1/2018	administrative	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Hawaii	terminate			legislative																					
Idaho	terminate			administrative																			X	X	X
Illinois	suspend		8/20/2015	legislative	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	
Indiana	suspend	24	7/1/2017	administrative								X	X	X	X	X	X	X	X	X	X	X	X	X	X
Iowa	suspend		1/1/2012	legislative												X	X	X	X	X	X	X	X	X	X
Kansas	terminate			administrative																	X	X	X	X	X
Kentucky	suspend		8/19/2015	administrative	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Louisiana	suspend		10/1/2012	administrative																					
Maine	suspend		9/21/2001	legislative																		X	X	X	X
Maryland	suspend		7/1/2005	legislative						X	X	X	X	X	X	X	X	X	X	X	X	X			
Massachusetts	suspend		7/1/2015	legislative															X	X	X	X	X	X	X
Michigan	suspend		1/2/2015	legislative	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X		
Minnesota	suspend	12	7/1/2007	legislative	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Mississippi	suspend		11/30/2015	administrative										X	X	X	X	X	X	X	X	X	X	X	X
Missouri	terminate			administrative				X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Montana	suspend		11/30/2015	administrative																X	X	X	X	X	X
Nebraska	suspend		8/30/2015	legislative						X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Nevada	terminate			administrative														X	X	X	X	X	X	X	X
New Hampshire	suspend		8/29/2016	administrative																	X	X	X	X	X
New Jersey	suspend		12/31/2014	legislative									X	X	X	X	X	X	X	X	X	X	X	X	X
New Mexico	suspend		11/30/2015	legislative																X	X	X	X	X	X
New York	suspend		4/1/2008	legislative	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
North Carolina	suspend	12	9/1/2008	administrative					X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
North Dakota	terminate			administrative								X	X	X	X	X	X	X	X	X	X	X	X	X	
Ohio	suspend		4/7/2009	administrative															X	X	X	X	X	X	X
Oklahoma	terminate			administrative						X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Oregon	suspend	12	6/1/2011	legislative							X	X	X	X	X	X	X	X	X	X	X	X	X		
Pennsylvania	suspend	24	7/8/2016	legislative							X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Rhode Island	suspend	12	11/30/2015	administrative										X	X	X	X	X	X	X	X	X	X	X	X
South Carolina	suspend		7/1/2016	administrative						X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
South Dakota	terminate			administrative																			X	X	X
Tennessee	suspend		4/1/2015	legislative						X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Texas	terminate			administrative											X	X	X	X	X	X	X	X	X	X	X
Utah	terminate			administrative						X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Vermont	suspend		7/30/2014	administrative																					
Virginia	suspend		10/1/2014	legislative																					
Washington	suspend		7/1/2017	legislative	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
West Virginia	suspend		1/1/2014	administrative												X	X	X	X	X	X	X	X	X	
Wisconsin	terminate			administrative						X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Wyoming	terminate			administrative												X	X	X	X	X	X	X	X	X	X

Notes: Column (1) indicates whether a state Medicaid is suspended or terminated for enrollees who become incarcerated. For states that suspend Medicaid eligibility for a specific time period (rather than from the full duration of incarceration), we indicate this time limit, measured in months, in Column (2). Column (3) reports the exact implementation date for states that switch from termination to suspension of Medicaid upon incarceration. Column (4) indicates whether the Medicaid policy for incarcerated individuals is an administrative internal agency policy or legislated by law. Column (5) marks with an X the prison release years that are in NCRP by each state.

Source: Several sources were consulted to construct this table: 1) section 1115 Waiver Demonstration documents from the Centers for Medicare and Medicaid Services; 2) state government documents and reports; 3) reports from national health policy organizations including The Henry J. Kaiser Family Foundation, the National Academy for State Health Policy, and the National Council of State Legislators; and 4) direct communications with officials at state's Centers for Medicare and Medicaid Services.

Table A2: Balanced Panel

State	Policy (1)	Limit (2)	Impl. Date (3)	Type (4)	Year (5)															# (6)
Georgia	terminate			adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	21
Kentucky	terminate			adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	21
New York	suspend		4/1/2008	leg	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	21
Washington	terminate			adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	21
Florida	suspend		7/1/2008	leg	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	20
Illinois	terminate			adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014		20
Minnesota	suspend	12	7/1/2007	leg	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	20
Michigan	terminate			adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013			19
Missouri	terminate			adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	18
North Carolina	suspend	12	9/1/2008	adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	17
Oklahoma	terminate			adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	17
Colorado	terminate			adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	16
Nebraska	terminate			adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	16
South Carolina	terminate			adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	16
Tennessee	terminate			adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	16
Utah	terminate			adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	16
Wisconsin	terminate			adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	16
Pennsylvania	terminate			adm		2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	15
Indiana	terminate			adm			2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	14
D.C.	terminate			adm			2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014		13
Maryland	suspend		7/1/2005	leg				2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	13
New Jersey	terminate			adm				2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	13
North Dakota	terminate			adm			2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014		13
Mississippi	terminate			adm					2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	12
Rhode Island	terminate			adm					2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	12
Texas	terminate			adm						2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	11
Alaska	terminate			adm						2005	2006	2007	2008	2009	2010	2011	2012	2013			9
Iowa	suspend		1/1/2012	leg				2006	2007	2008	2009	2010	2011	2012	2013	2014	2015				10
Oregon	suspend	12	6/1/2011	leg	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013						13
Wyoming	terminate			adm							2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	10
Alabama	suspend		1/1/2018	leg								2007	2008	2009	2010	2011	2012	2013	2014	2015	9
West Virginia	suspend		1/1/2014	adm							2006	2007	2008	2009	2010	2011	2012	2013	2014		9
Nevada	terminate			adm									2008	2009	2010	2011	2012	2013	2014	2015	8
Delaware	terminate			adm										2009	2010	2011	2012	2013	2014	2015	7
Massachusetts	suspend		7/1/2015	leg										2009	2010	2011	2012	2013	2014	2015	7
Montana	suspend		11/30/2015	adm											2010	2011	2012	2013	2014	2015	6
New Mexico	suspend		12/1/2015	leg											2010	2011	2012	2013	2014	2015	6
Kansas	terminate			adm												2011	2012	2013	2014	2015	5
New Hampshire	suspend		8/29/2016	adm												2011	2012	2013	2014	2015	5
Idaho	terminate	1		adm														2013	2014	2015	3
South Dakota	terminate			adm														2013	2014	2015	3
California	suspend		1/1/2014	leg	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	21
Arizona	suspend	12	7/31/2014	adm	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	16
Ohio	suspend		4/7/2009	adm										2009	2010	2011	2012	2013	2014	2015	7
Maine	suspend		9/21/2001	leg													2012	2013	2014	2015	4
Arkansas	suspend	12	8/1/2015	adm																	0
Connecticut	suspend		6/1/2015	adm																	0
Hawaii	terminate			leg																	0
Louisiana	suspend		9/30/2012	adm																	0
Vermont	suspend		7/30/2014	adm																	0
Virginia	suspend		10/1/2014	leg																	0

Notes: Column (1) indicates whether a state Medicaid is suspended or terminated for enrollees who become incarcerated. For states that suspend Medicaid eligibility for a specific time period (rather than from the full duration of incarceration), we indicate this time limit, measured in months, in Column (2). Column (3) reports the implementation of the Medicaid suspension policy if it occurs before 2013. Column (4) indicates whether the Medicaid policy for incarcerated individuals is an administrative internal agency policy or legislated by law. Column (5) indicated the prison release years sampled for each state. We also report the number of years of NCRP coverage for each state in Column (6). Our main estimation sample is a perfectly balanced sample that consists of release years shaded in blue. The states are treated states. For some of our robustness analysis, we extend this sample to include more years prior to 2005.

Table A3: Effect of Medicaid Suspension Policies on 1-year and 3-year Overall Recidivism by Policy Type

	1-year Recidivism	3-year Recidivism
	(1)	(2)
Suspension	-0.0304***	-0.0561*
	(0.00969)	(0.0287)
Suspension*Administrative	0.00576	0.0461
	(0.0104)	(0.0278)
Mean Sentence	0.194	0.378
R-squared	0.0903	0.100
N	3503372	3466574

Notes: The dependent variable is individual's probability to return to prison within 1 year of release (Column (1)) and 3 year (Column (2)) of release. Administrative is an indicator for whether the Medicaid suspension policy for incarcerated individual is administrative (See Appendix Table A2). See notes for Table 3 for more detail. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4. Column (2) has fewer observations to allow everyone to have 3 years of post-release data, whereas Column (1) only requires 1 year of post-release data.
*** p<0.01, ** p<0.05, * p<0.1

Table A4: Effect of Medicaid Suspension Policies on 1-year and 3-year Overall Recidivism

	1-year Recidivism	3-year Recidivism
	(1)	(2)
Suspension	-0.0292***	-0.0459**
	(0.00813)	(0.0222)
Suspension*Expansion	-0.0257***	-0.0263***
	(0.00756)	(0.00594)
Mean Sentence	0.194	0.378
R-squared	0.0903	0.100
N	3503372	3466574

Notes: The dependent variable is individual's probability to return to prison within 1 year of release (Column (1)) and 3 year (Column (2)) of release. Expansion is an indicator whether state expanded Medicaid early for the year-month into which the offender was admitted in prison. Medicaid expansion dates were obtained from the Kaiser Family Foundation. See notes for Table 3 for more detail. Column (2) has fewer observations to allow everyone to have 3 years of post-release data, whereas Column (1) only requires 1 year of post-release data. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4. *** p<0.01, ** p<0.05, * p<0.1

Table A5: Effect of Medicaid Suspension Policies on 1-year and 3-year Overall Recidivism: Robustness

	1-year Recidivism	3-year Recidivism
	Panel A: Main results	
	(1)	(2)
Suspension	-0.0291***	-0.0458**
	(0.00813)	(0.0222)
Mean Sentence	0.194	0.378
R-squared	0.0903	0.100
N	3503372	3466574
	Panel B: Probit	
	(1)	(2)
Suspension	-0.145***	-0.150*
	(0.0446)	(0.0788)
Mean Sentence	0.194	0.378
N	3503372	3466574
	Panel C: Consistent states	
	(1)	(2)
Suspension	-0.0407***	-0.0439***
	(0.0102)	(0.00875)
Wild bootstrap p	0.268	0.127
Mean Sentence	0.223	0.406
R-squared	0.0797	0.0990
N	1091380	1091380
	Panel D: Excluding early Medicaid expansion states	
	(1)	(2)
Suspension	-0.0311***	-0.0504
	(0.00993)	(0.0307)
Wild bootstrap p	0.0288	0.0583
Mean Sentence	0.189	0.372
R-squared	0.0933	0.103
N	3238281	3201483

Notes: The dependent variable is individual's probability to return to prison within 1 year of release (Column (1)) and 3 year (Column (2)) of release. See notes for Table 3 for more detail. Panel A presents our main results. Panel B estimates Equation (1) using a probit model. In Panel C, sample is limited to the following eleven states that consistently reported data as identified by Neal and Rick (2016): California, Colorado, Illinois, Kentucky, Michigan, Minnesota, Nebraska, New Jersey, South Dakota, Virginia, and Washington. Panel D excludes early Medicaid expansion states, namely Minnesota and New Jersey, that are in our balanced panel. As suggested by Cameron et al. (2008) in cases with a small number of clusters, we report p-values from 1000 wild-cluster bootstrap iterations for the results presented in Panels C and D. Column (2) has fewer observations to allow everyone to have 3 years of post-release data, whereas Column (1) only requires 1 year of post-release data. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4.
*** p<0.01, ** p<0.05, * p<0.1

Table A6: Effect of Medicaid Suspension Policies on 1-year and 3-year Overall Recidivism: Time Trend Sensitivity Analysis

	Panel A: 1-year Recidivism				
	(1)	(2)	(3)	(4)	(5)
Suspension	-0.0291***	-0.0287***	0.0006	-0.0265***	-0.0253***
	(0.00813)	(0.00819)	(0.00559)	(0.00699)	(0.00711)
Mean Sentence	0.194	0.194	0.194	0.194	0.194
R-squared	0.0903	0.0901	0.0889	0.0908	0.0910
N	3503372	3503372	3503372	3503372	3503372
	Panel B: 3-year Recidivism				
	(1)	(2)	(3)	(4)	(5)
Suspension	-0.0458**	-0.0450*	-0.0151*	-0.0460*	-0.0448*
	(0.0222)	(0.0219)	(0.00747)	(0.0223)	(0.0244)
Mean Sentence	0.378	0.378	0.378	0.378	0.378
R-squared	0.100	0.100	0.0990	0.101	0.101
N	3466574	3466574	3466574	3466574	3466574
State FE	Yes	Yes	Yes	Yes	Yes
State-specific linear time trend	Yes	Yes	No	Yes	Yes
Release year FE	Yes	No	Yes	Yes	Yes
State-specific quadratic time trend	Yes	No	No	Yes	No
State-specific cubic time trend	Yes	No	No	No	Yes

Notes: The dependent variable is individual's probability to return to prison within one year or three years of release (indicated in the panel heading). Each column includes different set of time fixed effects as specified in the lower panel of the table. See notes for Table 3 for more detail. Column (2) has fewer observations to allow everyone to have 3 years of post-release data, whereas Column (1) only requires 1 year of post-release data. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4.
*** p<0.01, ** p<0.05, * p<0.1

Table A7: Effect of Medicaid Suspension Policies on Overall Recidivism: Placebo Test

	t-1	t-2	t-3	t-4
	Panel A: 1-year Recidivism			
	(1)	(2)	(3)	(4)
Suspension	0.0220	0.0162	0.0181	0.0245
	(0.0131)	(0.0129)	(0.0152)	(0.0202)
Mean Dept. Var.	0.196	0.196	0.196	0.196
R-squared	0.0905	0.0905	0.0905	0.0905
N	3222203	3222203	3222203	3222203
	Panel B: 3-year Recidivism			
	(1)	(2)	(3)	(4)
Suspension	0.0312	0.0296	0.0281	0.0284
	(0.0240)	(0.0239)	(0.0223)	(0.0215)
Mean Dept. Var.	0.381	0.381	0.381	0.381
R-squared	0.0984	0.0984	0.0984	0.0984
N	3194415	3194415	3194415	3194415

Notes: The dependent variable is individual's probability to return to prison within 1 year of release (Panel A) and 3 years of release (Panel B). Each column defines treatment from 1 (t-1) to 4 (t-4) years earlier than actual implementation date. Suspension is an indicator for whether a state suspends Medicaid coverage upon incarceration. Robust standard errors clustered by state. Each column is estimated by a separate OLS. State and time FE include state and admission year fixed effects. Individual characteristics include demographic characteristics (race, ethnicity, gender, age, age squared, highest graded completed, prior felony incarceration indicator) and current crime and prison characteristics (main offense type, number of convicted counts, total sentence imposed, reason for release, time served, time served squared) as well as indicators for missing data on each of the time-invariant characteristics. State characteristics include time-varying state characteristics (unemployment rate and the number of sworn police officers per 1000 in the population) and an indicator whether the state expanded Medicaid under the ACA. Data are from the NCRP and the estimation sample is the balanced sample described in Section 4.
*** p<0.01, ** p<0.05, * p<0.1

Table A8: Proportion of State Inmates Likely Receiving Public Assistance at the Time of Prison Admission by State

state	prop	n	state	prop	n
Alaska	0.28	40	New Jersey	0.18	258
Colorado	0.16	162	New York	0.25	593
D.C.	0.14	21	North Carolina	0.21	347
Florida	0.19	629	North Dakota	0.14	7
Georgia	0.20	418	Oklahoma	0.22	203
Illinois	0.32	380	Pennsylvania	0.30	361
Indiana	0.17	239	Rhode Island	0.22	40
Kentucky	0.22	129	South Carolina	0.22	178
Maryland	0.20	202	Tennessee	0.27	197
Michigan	0.26	320	Texas	0.26	1596
Minnesota	0.26	43	Utah	0.19	62
Mississippi	0.34	148	Washington	0.24	155
Missouri	0.29	368	Wisconsin	0.26	175
Nebraska	0.24	41			

Notes: We report by state the proportion (prop) and number (n) of state prison inmates who answered affirmatively to the question "Were you or anyone living with you receiving public assistance or welfare, for example, Temporary Assistance for Needy Families (TANF), food stamps, Medicaid, Women, Infants, and Children Program (WIC), or housing assistance, before you were admitted to prison." The sample is restricted to states that are included in our main estimation sample (see Table A2).
Data comes from the 2004 Survey of Inmates in State and Federal Correctional Facilities.

Table A9: Effect of Medicaid Suspension Policies on Overall Recidivism: High vs low share of state prison inmates receiving public assistance

	1-year Recidivism		3-year Recidivism	
	High	Low	High	Low
	(1)	(2)	(3)	(4)
Suspension	-0.0425***	-0.0141	-0.0574***	-0.0325
	(0.00545)	(0.00806)	(0.00887)	(0.0259)
Mean Dept. Var.	0.230	0.146	0.421	0.318
R-squared	0.0766	0.0940	0.0856	0.103
N	2037389	1465983	2012446	1454128

Notes: We report heterogeneous effect of our main result (Table 3, Column 3) for states with high or low share of inmates receiving public assistance. See notes of Table 3 for a full list of controls included, definition of the variables, and details on the estimation sample. We define a state to have a high share of inmate receiving public assistance if the share of inmates that do is above the median in the sample of 2004 Survey of Inmates in State and Federal Correctional Facilities.
*** p<0.01, ** p<0.05, * p<0.1