A Welfare Analysis of Medicaid and Recidivism

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Abstract

We present conservative estimates for the marginal value of public funds (MVPF) associated with providing Medicaid to inmates exiting prison. The MVPF measures the ratio between a policy's social benefits and its governmental costs. Our MVPF estimates suggest that every additional \$1 the government spends on providing inmates exiting prison with Medicaid coverage can result in social benefits ranging between \$3.45 and \$10.62. A large proportion of the benefits we consider stems from the reduced future criminal involvement among former inmates who receive Medicaid. Employing a difference-in-differences approach, we find that Medicaid expansions reduce the average number of times a released inmate is reimprisoned within one year by approximately 11.5%. By combining this estimate with key values reported elsewhere (e.g., victimization costs, data on victimization and incarceration), we quantify specific benefits arising from the policy. These encompass diminished criminal harm due to lower reoffense rates, direct benefits to former inmates through Medicaid coverage, increased employment opportunities, and reduced loss of liberty resulting from fewer future reimprisonments. Net-costs consist of the cost of providing Medicaid net of changes in the governmental cost of imprisonment, changes in the tax revenue due to increased employment, and changes in spending on other public assistance programs. We interpret our estimates as conservative since we deliberately err on the side of under-estimating benefits and over-estimating costs when data on specific items are imprecise or incomplete. Our findings align closely with others in the sparse literature investigating the crime-related welfare impacts of Medicaid access, underscoring the substantial indirect benefits public health insurance programs can offer through crime reduction, in addition to their direct health-related advantages.

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I. Introduction

The ineffectiveness of imprisonment in deterring crime combined with the high costs associated with incarceration has caused policy researchers to seek alternatives to law enforcement in combatting crime (Mungan, 2021). One of the surprising results obtained in recent scholarship investigating such alternatives is that increased access to public health insurance leads to a reduction in crime (Wen, Hockenberry, and Cummings, 2017; Vogler, 2020; He and Barkowski, 2020; Aslim et al., 2022). Although the precise mechanism that leads to such reductions needs to be further examined, there is some evidence suggesting that mental health and substance use disorder treatments can reduce self-control problems, and thereby reduce the impulsive commission of crimes (Aslim et al., 2022). Because these problems are particularly prevalent among exiting inmates who rejoin society (Chamorro et al., 2012; Bronson and Berzofsky, 2017), policies extending public health insurance access to these individuals can carry large social benefits. However, to date, there are few rigorous attempts at measuring these benefits against the cost of such policies where Jácome (2020) and Aslim et al. (2022) are the only exceptions of which we are aware.

Here, we add to this literature by estimating the marginal value of public funds (henceforth 'MVPF') (Hendren and Sprung-Keyser, 2020; Finkelstein and Hendren, 2020) devoted towards providing exiting inmates increased access to public health insurance. The MVPF measures the ratio between a policy's social benefits, measured in terms of the willingness to pay, and its governmental costs. We focus on the social benefits associated with such policies, which consist of the direct benefits from health insurance in addition to reduced criminal harm and reduced loss of liberty due to incarceration as well as increased employment. We then measure these against the costs of supplying health insurance net of additional fiscal externalities (e.g., changes in the governmental cost of imprisonment, tax revenue generated through employment, and spending on public assistance programs). Overall, our analysis suggests conservative estimates of the MVPF associated with these policies ranging between 3.45 and 10.62. Our MVPF ratios are comparable but slightly narrower than those reported by Jácome (2020), which range between 1.77 and 14.96. These ratios can be compared to those reported by Hendren and Sprung-Keyser (2020) for various social insurance programs.[1] For example, the MVPF for providing Medicaid coverage to low-income single adults in the Oregon Health Insurance Experiment is 1.16 (see, also, Finkelstein, Hendren, and Luttmer 2019), whereas expanding Medicaid coverage to young children pays for itself (see, e.g., Brown, Kowalski, and Lurie 2020).

To conduct this analysis, we first estimate the impact of providing an exiting inmate with public health insurance on the number of times they are reimprisoned after their release. We use administrative data on prison admissions and releases from the National Corrections Reporting Program (NCRP). Exploiting the variation in Medicaid expansions across states and over time in a difference-in-differences (DID) framework, we find that Medicaid expansions reduce the average number of times a released inmate is reimprisoned within a year by 11.5%. Combining this estimate with other key values obtained from the existing literature (e.g., average victimization costs for different types of crimes from Cohen and Piquero 2009 and Miller et al. 2021), we estimate the components described above to calculate the MVPF associated with providing exiting inmates increased access to public health insurance.

Our analysis adds to a small body of literature that similarly estimates the cost effectiveness of health insurance in delivering criminal-justice-related benefits. Among existing scholarship, Jácome (2020) is the most closely related analysis to ours. By exploiting the variation in public insurance eligibility, Jácome (2020) finds that losing access to Medicaid eligibility at age 19 increases the likelihood of incarceration among men by 15% in the following two years. Our analysis complements Jácome (2020) while differing from it in several ways. Most importantly, while Jácome (2020) estimates the impact of losing Medicaid eligibility, we analyze the impact of an expansion in this policy under the Affordable Care Act. Moreover, while Jácome's analysis focuses on male teenagers in South Carolina, we analyze data from 43 states available in the NCRP and for ex-offenders aged

[1]See the Policy Impacts Library to compare MVPF estimates across policies: https://www.policyimpacts.org.

25-54.

Our approach also diverges from the existing literature focusing on the relationship between Medicaid and crime generally (Wen, Hockenberry, and Cummings, 2017, Vogler, 2020, He and Barkowski, 2020, and Jácome, 2020). While these studies offer valuable insights into the relationship between Medicaid enrollment (or loss of coverage) and crime (or incarceration), their estimates likely encompass a combination of general and specific deterrence effects resulting from enrolling in or losing public coverage. This implies that the reductions in crime attributed to Medicaid are presumably driven by reductions in offenses committed by both first-time offenders and reoffenders. Here, we are able to identify the impacts of Medicaid on recidivism by analyzing the behavior of released inmates. This is necessary to inform cost effective reentry programs and policies.

A preceding article, Aslim et al. (2022), provides insights regarding this issue. Through a back-of-the-envelope welfare analysis of Medicaid expansions, it finds benefit-to-cost ratios ranging between 25% and 135%. However, the analysis in Aslim et al. (2022) covers a shorter period of time and focuses on whether an exiting inmate becomes a multi-time recidivist, rather than on the average number of times a released inmate is reimprisoned. This discrete approach is well-suited for investigating the possible mechanisms through which access to health care may influence the behavior of existing inmates. This is because it allows an investigation of whether Medicaid provision reduces repeated crime commission among people who have previously committed crimes which are more often committed impulsively, such as violent crimes and public order violations, which would be consistent with an impulsivity-reducing effect. However, because this approach focuses on the discrete outcome of whether or not a released inmate becomes a multi-time recidivist, it cannot be used to provide a specific estimate of the number of offenses that can be averted through the provision of Medicaid.

In this article, we adopt a complementary approach to obtain a more precise estimate of the criminal harms prevented by granting Medicaid access to exiting inmates. We achieve this by estimating the average number of crimes averted (categorized by type) through Medicaid access and converting these reductions into expected harms using estimates of the associated harms for each crime type based on previous research. Furthermore, our ability to analyze a longer period with a larger data set enhances the precision of the estimates and allows us to explore a previously unaddressed question: Do the dynamic effects of Medicaid persist in the long run? We answer this question affirmatively by employing methodologies very recently introduced in the DID literature (see, e.g., Sun and Abraham, 2021; Borusyak, Jaravel, and Spiess, 2021; Gardner, 2021).

Additionally, our analysis incorporates a wider range of considerations, such as increased employment, reduced loss of liberty, and fiscal externalities, enabling a more comprehensive and precise MVPF analysis. In contrast, Aslim et al. (2022) provides preliminary findings from a partial cost-benefit analysis. Consequently, our MVPF estimates, ranging between 3.45 and 10.62, suggest significantly larger gains from granting Medicaid access to exiting inmates compared to the previous, more conservative approach in Aslim et al. (2022), which reports benefit-to-cost ratios ranging between 25% and 135%.

This paper can also be situated within the growing body of literature that seeks to identify factors contributing to the reduction of recidivism. For example, Batistich, Evans, and Phillips (2021) focus on decreasing re-arrest rates among individuals with severe mental illness who have been incarcerated, by implementing a mental health outreach program that facilitates their connection to mental healthcare providers upon release. Their study reports significant reductions in recidivism rates within a 60-day period for participants compared to non-participants. Similarly, Arora and Bencsik (2021) explore a drug diversion program in Chicago that aims to redirect individuals caught with narcotics towards treatment rather than arrest, which leads to increased engagement in substance use treatment and a decrease in subsequent arrests. Furthermore, evidence suggests that expedited Medicaid enrollment for released inmates not only enhances access to mental health and substance use treatment (Morrissey, Domino, and Cuddeback, 2016; Gertner et al., 2019) but also may reduce recidivism (Gollu and Zapryanova, 2022). Taken together, these findings, alongside the broader literature, indicate the effectiveness of targeted interventions that address mental health and substance use disorders in significantly reducing recidivism among individuals with incarceration histories.[2]

II. Empirical Analysis

II.A. Data

We obtain administrative data on prison spells from the National Corrections Reporting Program (NCRP), which is collected by the U.S. Bureau of Justice Statistics. Similar to Aslim et al. (2022), we utilize the publicly available version of the NCRP.[3] The data provide a unique ID number for each inmate, allowing us to link prisoners across different prison spells within each state. We observe various inmate characteristics and obtain comprehensive information on judicial decisions and their administrative outcomes. This includes age at release, gender, race/ethnicity, offense of conviction, time served, sentence length, as well as prison admission type (e.g., new court commitment, parole violation/revocation, and other admissions including unsentenced, transfer, absent without leave, and escapee return) and release type (e.g., conditional release, unconditional release, and other releases including death, transfer, absent without leave, and escape).[4] Additionally, the data include the year of prison admission and mandatory release for each prison spell.

Exploiting the information on prison spells, we define reimprisonments as the number of times a released inmate is reimprisoned within 1-, 2-, and 3-years. To ensure that our policy variable does not interact with other social insurance programs, we focus on observations from inmates aged 25-54.[5] Following the methodology of Agan and

[2]See also Mueller-Smith and Schnepel (2021) for an investigation into the broader impacts of diversion programs.

[3]Aslim et al. (2022) demonstrate that exploiting the restricted version of the data, which includes more disaggregated information on inmate characteristics and admission date, yields similar estimates to those obtained from the publicly available version in the context of Medicaid expansions. The restricted version is not available for general dissemination due to the level of information it contains, such as last known addresses of each offender. For additional details on the data, refer to Aslim et al. (2022).

[4]Due to missing information on highest grade completed in the current version of NCRP employed in this study, we instead utilize data from the American Community Survey (2009-2019) to construct educational attainment at the state level.

[5]This approach specifically avoids potential interactions with the dependent coverage mandate and the Medicare program. Given that our age variable is categorical, we apply the most plausible restriction.

Makowsky (2018), we exclude the state of California due to the state's implementation of the Public Safety Realignment Act, aimed at reducing prison overcrowding. Overall, our data consist of an unbalanced panel of inmates released in 43 states between the years 2009 and 2019.[6]

Next, we collect information from the Kaiser Family Foundation (KFF) on states' decisions to expand Medicaid through the ACA, as presented in Table 1.[7] To assign states to the treatment and control groups, we follow the methodology employed by Courtemanche et al. (2017) and Aslim et al. (2022). We leverage the staggered adoption of Medicaid expansions, allowing us to identify early and late expansion states.[8]

Tables 2 and 3 report summary statistics for reimprisonments and inmate characteristics by expansion status, respectively. A few notable observations are the following. The number of reimprisonments in expansion states declines, on average, in the post-treatment period for the all-crime sample (including violent, property, drug, and public order crimes). The difference in the post- and pre-treatment mean is also statistically significant ($p < 0.01$). This decline appears to be driven by inmates who were initially imprisoned for violent and public order crimes.[9] More importantly, we do not observe similar trends in non-expansion states. As we show in Table 3, most of the inmates are admitted to prison through a new court commitment rather than returning from parole or probation. These findings are preserved when we employ a longer window for reimprisonments.

To provide further insights about our outcome variable and the type of inmates who are reimprisoned, we report the distribution of reimprisonments in Table 4. First, we find that most inmates have zero reimprisonments, and that the exiting inmates are not

[6] To construct our outcome variable, we exclude observations for the last year if a full prison spell is not observed (e.g., inmates released in June 2019). Additionally, observations where the release type is coded as death are also not considered.

[7] Information on eligibility rules and the expansion status can be obtained from the reports provided by KFF. See https://bit.ly/2JYkb0A for details.

[8] While Courtemanche et al. (2017) find qualitatively and quantitatively similar impacts on health insurance coverage when excluding early and late expansion states, Aslim et al. (2022) demonstrate statistically significant effects on crime-specific recidivism using alternative classifications of treatment and control groups. For further details, refer to Appendix Figure A2 in Aslim et al. (2022).

[9] Note that public order crimes include riots, driving under the influence (DUI) or driving while intoxicated (DWI), and vice offenses such as gambling and prostitution, among others.

likely to return to prison more than once in a year. We have similar findings when we extend the time-window, though, not surprisingly, inmates are slightly more likely to be reimprisoned more than once. Second, Table 4 suggests that most of the reduction in expansion states come from averting the first imprisonment upon release. Moreover, there is an increase in the number of reimprisonments, particularly the first reimprisonment, in non-expansion states, lending support to the idea that Medicaid expansions are effective in reducing reimprisonments in expansion states.

II.B. Empirical Methodology

To examine the causal impact of the ACA Medicaid expansions on the number of times a released inmate is, on average, reimprisoned, we estimate the following generalized difference-in-differences (DID) model:

$$Reimprisonment_{ist} = \beta_0 + \zeta_s + \eta_t + \beta_1 Expansion_{ist} + \mathbf{X}_{ist}\mathbf{\Gamma}_1 + \mathbf{\Omega}_{st}\mathbf{\Gamma}_2 + \epsilon_{ist}, \qquad (1)$$

where $Reimprisonment_{ist}$ measures the number of times inmate i, after being released from their first incarceration in state s and year t, has been reimprisoned.[10] We construct the number of reimprisonments within 1-, 2-, and 3-year windows by the category of crime for which an inmate was initially imprisoned: violent, property, drug, or public order. In other words, inmates are categorized by their initial offense type. $Expansion_{ist}$ denotes the treatment status of an inmate based on the first release year and conviction state. Our main coefficient of interest is β_1, which measures the causal effect of the ACA Medicaid expansions on the number of times previously incarcerated inmates are reimprisoned.[11] To identify the causal effects, we exploit the staggered adoption of Medicaid expansions and compare the outcomes of expansion states with those in the control group that have not been affected by the treatment. Following standard procedures in the literature (see,

[10]Note that a person's first imprisonment refers to their first imprisonment in our sample, and not necessarily the first imprisonment in their lifetime.

[11]Our findings remain robust when applying a Poisson regression model. However, for the sake of brevity, we present estimates from a linear regression model. Results obtained from the Poisson regression model are available upon request.

e.g., Sun and Abraham 2021), we examine the assumption of parallel trends between treated and never-treated (or last-treated) states by conducting a series of event studies.

A rich set of inmate-level covariates are included in the equation. Specifically, the vector $\mathbf{X}_{ist}$ contains an inmate's age at release, gender, and race/ethnicity. We also control for variables that describe the characteristics of the most recent crime(s) committed by an inmate, including sentence length, time served in prison, types of prison admission (court commitment, parole violation, other) and release (conditional release, unconditional release, other). Moreover, to alleviate concerns about state-level confounders, we control for a number of time-varying variables that gauge macroeconomic conditions for each state over time, including the minimum wage, housing price index, poverty rate, and the unemployment rate.[12] We also control for educational attainment at the state level.[13] We use $\mathbf{\Omega}_{st}$ to denote these state-level control variables. ζ_s and η_t capture the state fixed effects and release-year fixed effects, respectively. ϵ_{ist} is the error term. Robust standard errors are clustered at the state level.

We conduct a battery of robustness tests to examine the validity of our empirical results in terms of both estimation and inference. First, in all benchmark models, standard errors are clustered at the state level with 43 clusters. While the number of clusters is not small, we provide p-values obtained from wild cluster bootstrap iterations to check the sensitivity of our baseline inference. Second, we implement a randomization inference procedure where treatment status is randomly assigned to states based on the actual number of expansion and non-expansion states in each year from the working sample. We re-estimate Equation (1) using the newly constructed sample and calculate p-values from 1,000 replications of this process. We report these randomization inference p-values for all benchmark regressions.

In terms of estimation, we present point estimates from the static specification by

[12] The implicit assumption here is that these covariates are not caused by the treatment itself. Caetano et al. (2022) show that this assumption is implausible in various applications, leading to biased estimates of the average treatment effect on the treated (ATT), even in cases of canonical (two-period) DID models. We later test the validity of our assumption by (i) employing specifications without time-varying control variables and (ii) exploiting "imputation estimators" that address potential bias caused by controlling for time-varying covariates (see, e.g., Borusyak, Jaravel, and Spiess, 2021; Gardner, 2021). This approach is similar to the regression adjustment-type strategies proposed by Caetano et al. (2022).

[13] Excluding educational attainment in the regression analysis does not affect our results.

exploiting two alternative estimators other than the OLS estimator, given the staggered nature of the policy variable. The traditional two-way fixed model produces biased estimates of the average treatment effect on the treated (ATT) when there is treatment heterogeneity (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021). Intuitively, the bias stems from using already-treated units as a control, e.g., later-treated units versus earlier-treated units, in which the changes in treatment effects get netted out from the DID estimate. This may be less of a concern for Medicaid expansions due to (i) having a relatively large never-treated group and (ii) the timing for most treated units being closer to the middle of our sample period (Goodman-Bacon, 2021a). Nonetheless, we check the robustness of our estimates by using two different imputation strategies provided by Borusyak, Jaravel, and Spiess (2021) and Gardner (2021), respectively. These imputation estimators also allow for (i) the time-varying effects of our macroeconomic variables and (ii) the possibility of Medicaid expansions affecting these covariates. For the dynamic specification, we further provide estimates using the Sun and Abraham estimator (Sun and Abraham, 2021) to avoid contamination in leads and lags from other periods. We exclude (time-varying) macroeconomic variables in the dynamic specification to show that our approach does not necessitate *conditional* parallel trends.

II.C. Empirical Results

The validity of our identification strategy hinges on the assumption that the path of treated and untreated potential outcomes evolve in a parallel fashion (i.e., the parallel trends assumption). Although this assumption is not directly testable, in practice, it is critical to examine whether the treatment and control groups have parallel trends in the potential outcomes prior to the treatment. Therefore, we begin our empirical analysis by presenting the estimates from the dynamic specification, which depicts the trends in the number of reimprisonments between expansion and non-expansion states. We report the estimates in Figures 1, 2, and 3 for different time windows of our outcome variable and the type of crime. In each specification, we report OLS estimates as a benchmark. To obtain an appropriate weighted average of treatment effects for each state and each relative time pre- or post-expansion, we also exploit the interaction-weighted estimator developed by Sun and Abraham (2021), and report the corresponding estimates in each figure.

Figure 1 presents the event study results for the number of reimprisonments within a year. Our analysis reveals no statistically significant pre-trends between expansion and non-expansion states across specifications. Furthermore, the OLS and Sun and Abraham estimators yield consistent results.

Figure 1a illustrates that Medicaid expansions lead to a decrease in the number of reimprisonments for the all-crimes sample. Additionally, it reveals a dynamic effect over time, with the average causal effects gradually increasing in magnitude following the expansion of Medicaid coverage. Existing scholarship offers several potential explanations for this finding. First, the impact of medical treatment often exhibits a lagged and staggered pattern, as corroborated by previous studies (e.g., Park and Qiu, 2018). Importantly, some of these studies examine supply-side reasons for the lagged effect of Medicaid expansions. In their systematic review, Mazurenko et al. (2018) highlight the mixed evidence regarding the impact of Medicaid expansions on appointment availability and waiting times. On one hand, a few studies, such as Polsky et al. (2015) and Neprash et al. (2021), report increases in appointments following expansions.[14] Polsky et al. (2015) additionally explore the impact on waiting times, but find no significant effect. On the other hand, studies using administrative datasets document increases in appointment wait times for both primary and specialty care (Auty and Griffith, 2022), and research focusing exclusively on emergency departments also reports similar increases in wait times (Allen, Gian, and Simon, 2022; Wang, 2022).

Additionally, evidence suggests that Medicaid take-up increases over time due to state outreach and enrollment efforts, along with enhanced program awareness, which may create a "welcome mat" effect (McInerney, Mellor, and Sabik, 2021). Courtemanche et al. (2019) demonstrate that Medicaid expansions resulted in a 5.1-percentage-point increase

[14]Notably, Neprash et al. (2021) also found no evidence of an increase in the total labor supply of primary care clinicians. To accommodate the rise in appointments, these clinicians shifted away from commercially insured patients towards Medicaid patients, a change that did not necessarily result in major revenue losses.

in health insurance coverage between 2014 and 2017, effectively closing the coverage gap across income groups under the ACA. Furthermore, based on the most frequently observed offender demographics, Aslim et al. (2022) find a significant rise in Medicaid enrollment after 2014.[15] Additionally, there is evidence in the literature suggesting that expediting Medicaid enrollment for released inmates and offering enrollment assistance can increase Medicaid enrollment rates, as shown by Morrissey, Domino, and Cuddeback (2016) and Burns et al. (2021), respectively.[16] As Medicaid enrollment continues to grow and released inmates gain access to medical treatment, we can expect to observe greater reductions in reimprisonments over time.

We further analyze the outcome by categorizing the type of crime and present the corresponding findings in Figures 1b-1e. The results clearly indicate that the most significant reduction in reimprisonments is observed among released inmates who were initially incarcerated for committing violent crimes. Similar patterns emerge among inmates imprisoned for public order crimes, albeit with slightly lower statistical significance. While no immediate statistically significant effect is detected for property and drug crimes right after the treatment, there appears to be a modest reduction in reimprisonments towards the end of the treatment period in our analyzed sample.

Additionally, we provide estimates for 2- and 3-year windows of reimprisonment in Figures 2 and 3, respectively. The results obtained for these longer timeframes are both quantitatively and qualitatively similar to the analysis conducted for reimprisonments within a year. In summary, the event studies validate the robustness of our identification strategy and indicate that Medicaid expansions lead to a gradual reduction in the number of reimprisonments over time.

We proceed to estimate the static specification presented in Equation (1) and present

[15]The study narrows the American Community Survey sample based on inmate characteristics observed in the imprisonment data from the National Corrections Reporting Program (NCRP). This sample limitation relies on age, gender, race, ethnicity, and income. After applying these demographic filters, the authors observe a higher Medicaid enrollment rate within this subset compared to the general low-income Medicaid cohort. Furthermore, the study juxtaposes this initial estimate with findings from Saloner et al. (2016), who used data from the National Survey on Drug Use and Health to evaluate changes in Medicaid enrollment in 2014 among individuals with recent criminal justice involvement. The estimates of Medicaid uptake are remarkably consistent across these two studies.

[16]Furthermore, a qualitative study interviewing hospitalized male inmates conducted by Grodensky et al. (2018) identified enrollment assistance as a crucial factor in Medicaid uptake among eligible inmates.

the estimates in Table 5. Consistent with our previous approach, we report results for various time windows and categorize them by the type of crime. To account for potential bias resulting from the treatment's impact on time-varying covariates, we provide separate results from models with and without time-varying macroeconomic covariates. All specifications include state and release-year fixed effects, as well as inmate characteristics.[17]

Panel A of Table 5 displays the results for the number of reimprisonments within a year. In the absence of time-varying macroeconomic covariates, column (1) reveals that Medicaid expansions have a negative and statistically significant effect on the number of reimprisonments among the overall inmate population. Specifically, Medicaid expansions reduce the number of reimprisonments by 0.026 ($p < 0.05$), correponding to a 13.5% decline relative to the average number of reimprisonments prior to the treatment. Conversely, when including time-varying covariates, the specification suggests a reduction in reimprisonments of 11.5% (0.022/0.192, $p < 0.05$).

Columns (3)-(10) of Table 5 present the estimates disaggregated by the type of crime. We find a significant reduction in reimprisonments when focusing on violent and public order crimes. Specifically, the results reveal that Medicaid expansions lead to a decrease in reimprisonments by approximately 14.6% (0.026/0.178, $p < 0.05$) and 18.4% (0.035/0.190, $p < 0.01$) among inmates initially incarcerated for violent and public order crimes, respectively. In contrast, we observe negative but statistically insignificant effects for property and drug crimes.[18]

One possible concern is that these results may be driven by variations in the severity of offenses committed across states. For instance, if offenders in expansion states tend to commit more violent crimes compared to non-expansions states, they may receive longer imprisonment sentences and thus be incapacitated for a longer period, leading to

[17] Note that our estimates are not sensitive to the exclusion of inmate characteristics.

[18] Different potential effects of Medicaid expansions on drug-related crimes can be hypothesized. For instance, mental health care can reduce substance use disorder among some care recipients, and thereby reduce their illegal drug consumption. On the other hand, some may misuse their health benefits to acquire drugs for illegal sale, and others may complement their medication with illegal drugs. The fact that possession, production, consumption, and distribution offenses are not separately reported makes it difficult to identify which, if any, of these effects occur.

a mechanic reduction in their relative number of reimprisonments. These claims rely on the strong assumption of substantial variations in the commission of violent crimes across states. To assess the validity of this assumption, we test for it in Figure A7. However, our analysis yields no supporting evidence for this claim. Specifically, we explore whether individuals in expansion states serve a longer sentence relative to those in non-expansion states after the policy change. Perhaps not surprisingly, we do not find any statistically significant change in the likelihood of serving a longer sentence in the post-expansion period. Additionally, the pre-trends remain relatively flat around zero. Our summary statistics in Table 2 further demonstrate that the distribution of reimprisonments is fairly even across crime types during the pre-treatment period. Interestingly, even though individuals are more likely to be initially imprisoned for property and public order crimes, we observe a decline in the latter (which typically entails shorter sentences) following the policy change.

Our findings also indicate that Medicaid expansions result in similar reductions in the number of reimprisonments within 2- and 3-year windows, as illustrated in Panels B and C of Table 5, respectively. Comparing these results with those for the one-year window, we observe larger point estimates but smaller effect sizes when using longer windows across different samples. However, the statistical significance of these estimates is weaker in all samples presumably due to the reduced sample size and increased standard errors. Specifically, the point estimates remain statistically significant in all models, except for those employing the 3-year window for all crimes, as shown in columns (1) and (2) of Panel C. Furthermore, our analysis consistently reveals no statistically meaningful evidence of the effect of Medicaid expansions among inmates initially imprisoned for property or drug crimes, despite observing negative coefficients.

To explore potential heterogeneous effects, we present estimates from our static DID analysis for White and non-White samples, respectively. The heterogeneity analysis, detailed in Appendix Tables A1 and A2, reveals several noteworthy findings. First, we find that Medicaid expansions effectively reduce recidivism for both White and non-White inmates, though the effect is more pronounced among the latter group for certain types of crimes. Specifically, compared to the pre-treatment mean, the effect size for all crimes is larger for non-White inmates. In particular, we find a 13.6% decline in recidivism among non-White inmates for all crimes ($p < 0.05$), while this reduction is about 10% and statistically insignificant for the White population after accounting for state-specific time-varying controls. Nonetheless, the coefficient's magnitude closely aligns with our benchmark estimate.

Importantly, the negative coefficients for inmates convicted of drug crimes become statistically significant for certain sub-groups within the sample. When we further divide our sample by race, recidivism reductions among non-White inmates previously convicted of drug crimes become statistically significant.[19] This variation may be attributed to the varied impacts of Medicaid on drug recidivism, which likely affect racial groups differently due to distinct prevalence levels.

Additionally, it is important to highlight that effects are not solely concentrated among non-White inmates. Consistent with our benchmark analysis, we find an approximate 15% reduction in recidivism ($p < 0.05$) compared to the pre-treatment mean for violent crimes across both White and non-White inmates. We also detect notable decreases in recidivism among White inmates for public order crimes, with our estimates suggesting that Medicaid expansions reduce the number of public order crime reoffenses by around 22% for White inmates ($p < 0.01$) and 16% for non-White inmates ($p < 0.05$).

To assess the sensitivity of our inference, we report the p-values obtained from the randomization inference and wild bootstrap iterations for all regressions. It is evident that these p-values largely support our baseline inference. In addition, as presented in Table A3, the point estimates obtained from the two other imputation methods closely align with our baseline estimates reported in Table 5.

Taken together, our findings demonstrate that Medicaid expansions have a significant impact in reducing the number of reimprisonments among inmates in expansion states, particularly for those initially imprisoned for violent or public order crimes. The estimates show smaller effect sizes but are less precise when using longer windows to measure

[19]We provide the corresponding event study figures in the Appendix as Figures A1 through A6.

reimprisonments. Furthermore, alternative estimators yield consistent average causal effects, indicating that our approach effectively mitigates bias resulting from treatment heterogeneity.

Our empirical analysis extends and updates the findings of Aslim et al. (2022) by utilizing a larger data set and employing a different measure of recidivism. Specifically, we treat recidivism as a discrete variable, allowing us to quantify the costs associated with the reduction in reimprisonments, a crucial aspect often overlooked in previous studies treating recidivism as a binary outcome. Notably, our estimates indicate relatively smaller reductions in reimprisonments for violent and public crimes compared to similar specifications in Aslim et al. (2022) (14.6% reduction in violent reimprisonments versus a 38% reduction among violent multi-time reoffenders in Aslim et al., Panel B, column (2), Table 4). Our current estimates have implications for scaling the policy to the inmate population, providing a more cautious perspective for cost-benefit analyses compared to those presented in Aslim et al. (2022).

In order to effectively inform policy, it becomes crucial to quantify the effects specifically driven by reoffenders, as we accomplish in this paper. Additionally, we undertake a comprehensive analysis of the benefits and costs in Section II.D, considering a wide range of factors and incorporating fiscal externalities generated by this policy, encompassing, e.g., the reliance on other public assistance programs such as food stamps or welfare programs. Notably, previous cost-benefit analyses, except for those conducted by Jácome (2020), have not adequately accounted for these fiscal externalities. By addressing these aspects, our study offers a more nuanced understanding of the relationship between Medicaid expansions and recidivism, providing valuable insights for policymakers.

II.D. Marginal Value of Public Funds

Exploiting the causal estimates in Table 5, we next evaluate the welfare implications of providing Medicaid coverage to released inmates under the ACA. Following Finkelstein, Hendren, and Luttmer (2019) and Hendren and Sprung-Keyser (2020), we use the marginal value of public funds (MVPF) framework to quantify the welfare effects of

Medicaid expansions. The MVPF generally measures the ratio between the willingness to pay for a policy and the net cost of the policy to the government. As the MVPF gets larger, it generates more welfare per dollar spent. Since market distortions from raising government revenue are not internalized in the MVPF, this measure is commonly used to compare two policies to inform relative cost effectiveness. We obtain the MVPF for the expansion of Medicaid coverage using the following expression:

$$MVPF = \frac{WTP}{MC + FE}. \tag{2}$$

In Equation (2), WTP measures society's willingness to pay for expanding Medicaid coverage to released inmates. MC is the mechanical costs associated with this policy change, and FE covers fiscal externalities. Put differently, the denominator of the MVPF measures the net cost of the policy to the government. In the following sections, we discuss how we calculate each component of the MVPF. We present all values in 2020 dollars.

1. Willingness to Pay for the Policy

We begin our analysis by calculating WTP, which is society's willingness to pay for the policy. Specifically, the willingness to pay contains four parts in our analysis: (i) the willingness to pay for fewer criminal victimizations; (ii) the willingness to pay for improved labor market prospects; (iii) the willingness to pay for the value of public insurance transfer; and (iv) the willingness to pay for avoiding incarceration.[20]

To obtain a measure of willingness to pay for fewer criminal victimizations, we measure the reduction in social costs through averted victimizations per released inmate in expansion states. We present these averted social costs in Table 6.[21]

[20] It may appear that the society's WTP measure should only include recipients' WTP for Medicaid, since the social WTP is the sum of all individuals' WTP. However, this reasoning neglects the positive externalities generated by the policy, e.g., a reduction in criminal harms caused by the change in marginal re-offenders' behavioral responses. Moreover, the WTP for the value of public insurance transfer is calculated for the average person, who is unlikely to commit crimes. Thus, this measure excludes expected benefits of the policy from improved labor market prospects and avoiding incarceration, which are benefits enjoyed only by marginal re-offenders. Nevertheless, our conservative estimates exclude some of these benefits (e.g., WTP to avoid incarceration is considered as $0). Note also that most of the benefits accrue through fewer victimizations following Medicaid expansions.

[21] In the Appendix, we provide a more detailed derivation of the average social cost averted per crime as well as further explanations of victimization costs.

Our general approach towards calculating these averted costs is to estimate the reduction in the number of victimizations caused by Medicaid expansions and multiply them by the average victimization costs associated with each crime category (violent, property, drug, public order).

The reduction in the number of victimizations within each category is the product of three components, namely (i) the average reduction in the number of reimprisonments, (ii) the proportion of reimprisonment for each crime category, and (iii) the victimization-to-incarceration ratio for each category. For the first component, we use the reductions in the number of reimprisonments in a year, i.e., the estimate reported in Panel A, column (2) of Table 5, which is -0.022. For the second component, we calculate the share of reimprisonments for each category of crime in our working sample, which are reported in the first column of Table 6.[22]

We calculate the third component, when possible, as shown in Table A4 by drawing data from the National Crime Victimization Survey, National Prisoner Statistics, and the Supplementary Homicide Reports.[23] Following Heckman et al. (2010), we use average within-category victimization-to-imprisonment ratios.[24] As our data do not include information on victimization for drug and public order crimes, we assume a victimization-to-incarceration ratio of 1 for these categories to obtain conservative estimates of the upper bound of averted costs. However, we acknowledge the difficulty in conceptualizing victimization in the context of some drug offenses and public order crimes, as some people argue that at least a subset of these offenses are victimless. Therefore, we also consider an even more conservative approach by assigning a victimization ratio of 0 for drug crimes when calculating the lower bound of social costs, resulting in zero benefits from drug crime reduction.[25]

[22] Alternatively, one can use the share of reimprisonments in expansion states instead of the share of reimprisonments in the total sample. This approach, however, leaves our MVPF calculation quantitatively unchanged since the difference in these shares is negligible.

[23] See the notes in Table A4 for an explanation about sources.

[24] Using the same ratio for the marginal incarceration instances would be preferable. However, the data sets that can be used to estimate these ratios, such as the Criminal Justice Administrative Records System (CJARS), have important geographical limitations (see footnote 27).

[25] When calculating the lower bound of social cost per inmate, we assume the victimization-to-incarceration ratio to be 1 for public order crimes. Changing this ratio to 0 reduces the lower bound of total social cost by $26 (as shown in Table 6, column 2) without altering the lower bound of the MVPF

The average victimization costs for each crime category are calculated by multiplying two components, namely (i) the cost of each type of crime within a category (e.g., murder, rape, robbery, aggravated or simple assault, for violent crimes), and (ii) the within-category weights associated with each crime type. We calculate the within-category weights by using data presented in Jácome (2020), as explained in Table A5. We obtain lower and upper bound estimates for average victimization costs from Cohen and Piquero (2009) and Miller et al. (2021), respectively.[26]

Following this method, we obtain estimates of averted victimization costs for each category of crime. To calculate the total social cost per inmate, we sum the costs averted per inmate over the four crime categories. Using the lower and upper bound victimization costs, we estimate the total social cost per inmate as \$14,934 and \$21,562, respectively.

The second part of WTP is the willingness to pay for improved labor market prospects. We obtain this measure by estimating foregone income during incarceration. Specifically, we let the income loss during incarceration equal $q \times p^J \times$ the employment rate of low-income adults $\times$ the average annual income of low-income adults $\times$ the average sentence served, where q is our causal estimate of the reduction in the number of reimprisonments, and p^J is the share of reimprisonments for category $J \in$ {Violent, Property, Drug, and Public Order}. Moreover, we draw data from the American Community Survey (2009-2013) to measure the employment rate and the average annual income of low-income adults (below 138% of the federal poverty level, which is the means-tested threshold for Medicaid coverage under the ACA). We separately calculate income loss during incarceration for the specific J categories of crime since sentence length varies by J. Then, the total income loss is simply the summation of lost earnings over these categories. Our upper bound estimate of foregone income is \$307.[27] To be conservative, we define the

ratio at 3.45.

[26]We detail the average victimization costs for each crime and their sources in Appendix A3. Additionally, we report the victimization costs for public order crimes separately in Table A6, since these cost estimates are less commonly available and may be of particular interest to other researchers.

[27] Alternatively, we use the year-by-year estimates on total earnings and employment for recently released prisoners reported by Finlay and Mueller-Smith (2021). The authors calculate these measures using data from the Criminal Justice Administrative Records System (CJARS) linked with IRS W-2 returns. A potential caveat is that the data include information only from nine states and five justice-involved cohorts. Using these data, our upper bound estimate goes down from \$307 to \$166. Using these estimates, however, has a very small effect on our MVPF calculation: it reduces our higher estimate for

lower bound of foregone income as $0, assuming that the inmate is unemployed prior to incarceration.

The third part of WTP is the willingness to pay for the value of public insurance transfer. The value of public insurance transfer consists of two components: (i) the value of Medicaid to the recipient, and (ii) the cost of uncompensated care that would be received by released inmates were they were not insured.

This second component measures the actual cost of providing care, which would have to be paid by a source other than Medicaid when the care recipient is not covered by Medicaid. There are two possible ways to account for these costs in MVPF calculations, depending on the assumptions one makes about who bears these costs, namely the government, or the public.

First, if the government covers these costs, then the net cost of providing coverage to a person who is released would equal the mechanical cost of providing Medicaid to that individual minus the expected uncompensated cost of care that the government would have to pay if they had not provided Medicaid to that individual. This is because, the government would not be paying these (otherwise uncompensated) costs separately, but through Medicaid, when coverage is provided.

On the other hand, if the public covers these costs, then the government's cost of providing coverage would simply be the gross mechanical costs of providing care, without any further adjustments. But then, Medicaid coverage would reduce the public's burden of covering for the uncompensated costs that would otherwise be generated by the care received by uninsured individuals. In this case, these benefits would be captured in the numerator.

Our calculations reveal that the case where the public bears uncompensated costs results in a more conservative estimate, and thus we make this assumption when calculating the lower bound of MVPF. This may appear counter-intuitive, because this results in a greater WTP for the lower bound, but nevertheless results in a lower MVPF due to impacts on mechanical costs explained in the next subsection.

the MVPF from 10.62 to 10.54.

For the value of Medicaid to the recipient, we use Finkelstein, Hendren, and Luttmer (2019)'s estimate of 20% to 48% of the cost of Medicaid (denoted G).[28] We obtain the Medicaid average cost per beneficiary from the Centers for Medicare & Medicaid Services (CMS), which is \$5,873. We take $48\% \times G$ as the upper bound for the value of Medicaid per beneficiary. For the lower bound of our WTP measure for Medicaid we use $20\% \times G$ plus the average cost of uncompensated care per uninsured adults during the pre-ACA period (\$1,577).[29]

Finally, we consider the willingness to pay to avoid being incarcerated, which measures the value attached to liberties outside of prison. For the lower bound, we assume that ex-offenders are willing to pay \$0 to avoid incarceration. For the upper bound, we extract the value of willingness to pay to leave pretrial detention from Abrams and Rohlfs (2011), which is \$4,603 per year (measured in 2020 dollars). We then calculate the willingness to pay to leave prison based on our causal estimate q, the share of reimprisonments for category J crimes, p^J, and the average sentence served in prison for these different crime categories, which results in an upper bound estimate of \$242.[30]

Aggregating all components of WTP together, the total value of willingness to pay for the policy change ranges between \$17,686 to \$24,930.

[28] Finkelstein, Hendren, and Luttmer (2019) further show that Medicaid resource costs are 0.4 $\times G$, and estimate the WTP for Medicaid to range between 0.5 and 1.2 $\times$ Medicaid resource costs, which is equivalent to 0.2 and 0.48 $\times$ G.

[29] We obtain the average cost of uncompensated care from the Kaiser Family Foundation (KFF). See Tables 1 and 2 in the following report: `https://bit.ly/3sFEnKb`. The main source for the number of uninsured is also KFF. See Figure 1 in the following report: `https://bit.ly/3qoqopy`. We do not find a substantial difference in the pre-ACA ratio (\$1,577) and the post-ACA ratio (\$1,628).

[30] Specifically, the calculation is as follows: The willingness to pay to avoid being incarcerated = $\sum_J (q \times p^J \times$ avg. time of sentence served $\times$ the willingness to pay to leave detention). As mentioned before, q is 0.022; p^J takes the values of 20.93%, 32.95%, 27.34%, and 18.78% for violent, property, drug, and public order, respectively; and the WTP for leaving detention is \$4,603 in 2020 dollars. Moreover, the average years of sentence served is 4.8 years for violent crimes. For property, drug, and public order crimes, the average years of sentence served is 1.75 years (Kaeble, 2021). An alternative approach is to directly calculate the change in the number of days incarcerated in prison. However, this is not possible using the publicly available version of NCRP since it only reports the year of prison admissions and releases. Therefore, we instead multiply out the average sentence length per crime type to account for time spent in prison in our calculations.

2. Mechanical Costs & Fiscal Externalities

Now, we turn our focus to the denominator of MVPF which measures the net cost to the government for expanding Medicaid coverage to one more recipient. As shown in Equation (2), the denominator consists of two components. The first component is the mechanical costs of the policy change.

As explained in subsection II.D.1., we consider two different ways of calculating this value, depending on the assumption one makes about whether the public or the government bears the cost of uncompensated care received by uninsured individuals. As a result, our lower bound estimate includes the total cost of Medicaid, G, assuming that individuals bear the cost of uncompensated care. For the upper bound, our estimated cost of providing Medicaid is equal to the total cost of Medicaid, G, minus the average cost of uncompensated care per uninsured adult. Our calculations yield a cost of providing Medicaid to an incarcerated individual that ranges between $4,296 and $5,873.

Next, we estimate the second component of the denominator by gauging three types of fiscal externalities to the government resulting from the Medicaid expansions. To start our analysis, we first look at the spending on public assistance programs that are usually provided to low-income individuals, including the Supplemental Nutrition Assistance Program (SNAP) and the Temporary Assistance for Needy Families (TANF). Consistent with our objective of providing conservative MVPF estimates, we note that released inmates may be more likely to rely on public assistance programs (Mueller-Smith, 2015). Therefore, reduced incarceration can increase the costs of these programs to the government, which need to be added as fiscal externalities in the denominator of Equation (2).[31] To take these possible fiscal externalities into account, we obtain participation and cost data on SNAP and TANF for the periods 2011-2013 from the US Department of Agriculture Food and Nutrition Service and the US Department of Health & Human Services Office of Family Assistance, respectively.[32] Exploiting these data, our larger estimate for

[31]Because we err on the side of providing conservative estimates, we do not account for the benefits receivable by released inmates from TANF and SNAP in calculating the numerator of the MVPF.

[32]Participation and cost data for SNAP can be obtained here: https://bit.ly/3iVtzlo, while similar data for TANF are available here: https://bit.ly/3qTdkcF.

the increased spending on public assistance is q × average total spending (on SNAP + TANF) per person, which is \$42.[33] Using the take-up rate of public assistance among felons reported by Sugie (2012) as a weight, we estimate a lower weighted average of spending on SNAP and TANF as \$22.[34]

Another type of fiscal externality to the government is the averted costs due to fewer incarcerations. To calculate the costs related to incarceration, we employ the following equation: $\sum_J(q \times p^J \times$ average time served × average daily cost per inmate). We replace the average daily cost per inmate with the marginal daily cost per inmate in our alternative set of calculations. Note that the average time served in prison varies by crime category. For the upper bound, we follow Aslim et al. (2022) and use the global average daily costs per inmate reported by the Vera Institute of Justice (Mai and Subramanian, 2017).[35] Specifically, the global average daily cost per inmate is \$99.56. For the lower bound, following Jácome (2020), we use the marginal cost of incarcerating an individual for one year reported by Owens (2009), and estimate the marginal daily cost per inmate to be around \$41.2. Overall, the averted costs for the government due to fewer incarcerations range from \$790 to \$1,909.

For the last type of fiscal externality, we estimate foregone tax revenue. To calculate the forgone tax revenue due to inmates' lost employment, we utilize an average tax rate of 20% on personal income (Hendren and Sprung-Keyser, 2020) for the upper bound. Consequently, the foregone tax revenue is equal to 0.2 × the willingness to pay for improved labor market prospects defined above, leading to an upper bound of \$61. For our more conservative estimate, we consider foregone tax revenue to be \$0.

Taken together, our analysis provides an estimate of the net costs to the government

[33] According to the Center on Budget and Policy Priorities, approximately 21% of the TANF funds have been allocated to providing basic cash assistance for families with children in 2020. For more details, see the following policy brief: https://bit.ly/3MrYEvZ. Consequently, our calculations only incorporate 21% of the total TANF spending.

[34] Specifically, we calculate the greater estimate by using the following formula: q × (0.19 × Average Total Spending on TANF (2011-2013)/Total Recipients + 0.55 × Average Total Spending on SNAP (2011-2013)/Total Recipients). Note that these cost estimates are in the denominator of Equation (2), and therefore the larger estimate yields a conservative estimate of MVPF.

[35] When a person avoids incarceration, the government's health care expenditures for that person within the prison system are averted. The prison spending data we utilize, as reported by Mai and Subramanian (2017), include these averted costs.

for expanding Medicaid coverage per eligible inmate, ranging from \$2,350 to \$5,139, in Table 7. Additionally, using our dynamic DID design, we illustrate in Figure A8 how the net costs associated with providing Medicaid to exiting inmates vary over time. Consistent with our baseline analysis, we observe a declining trend in net costs, accounting for fiscal externalities.

3. Estimates of MVPF

Our analysis provides an estimate of the net costs to the government for expanding Medicaid coverage per eligible inmate, ranging from \$2,348 to \$5,125. When combining these net costs with our previously estimated benefits for the policy change, we calculate the MVPF to be between 3.45 and 10.62. These findings align closely with Jácome (2020), who estimates the impact of Medicaid eligibility loss on incarceration among 19-year-old males in South Carolina, providing insights into the general deterrence effects of public coverage. However, our narrower estimates pertain to the specific deterrence effects of Medicaid expansions across multiple states among ex-offenders aged 25-54.

To further scrutinize the robustness of our findings, we recalculate the MVPF estimates by adjusting several inputs used in our calculations. These adjustments include the effect size of Medicaid expansions, victimization-to-incarceration ratios, employment rates and potential annual income of ex-offenders, average cost per Medicaid beneficiary, and average cost of incarceration. These results are presented in Appendix Figures B1 through B6. Overall, these MVPF estimates closely match those from our main analysis, with a few instances where the new MVPF estimates significantly exceed our benchmark results. This suggests that the benefits of providing Medicaid to released inmates outweigh the costs. For detailed derivations, please refer to Appendix B.

III. Discussion and Conclusion

A recent and expanding body of work finds that increased access to health insurance generates sizeable indirect benefits due to its crime-reducing effect. Here, we add to this scholarship by estimating the marginal value of public funds (MVPF) associated with providing Medicaid to exiting inmates, which measures the ratio between a policy's social benefits, measured in terms of willingness to pay, and its governmental costs.

Our conservative estimates suggest that providing Medicaid to exiting inmates generates benefits that are about three times as large as the net-costs of providing Medicaid, and are in all likelihood much greater. This places the provision of Medicaid to exiting inmates among the most welfare-enhancing policies whose marginal value of public funds have recently been estimated and reported (Hendren and Sprung-Keyser, 2020). To support this, we have gathered MVPF estimates in the domain of social insurance, with a focus on public health insurance, from the Policy Impacts Library.[36]

We present these estimates, alongside key details about policy implementation (including the year of implementation and the beneficiaries), in Table A7. A compelling narrative unfolds as we compare MVPF estimates: policies aimed at expanding public health insurance coverage yield significantly beneficial outcomes, especially for certain vulnerable groups. Notably, expansion efforts targeting children from low-income families and individuals involved with the justice system not only mitigate acute vulnerabilities but also manifest substantial societal value. For children in low-income families, the benefits are particularly striking, rendering the policy essentially self-financing. This highlights the unique effectiveness of targeted health insurance expansions in promoting long-term societal and economic benefits, setting these initiatives apart from those targeting the broader population of low-income adults or the elderly.

In our context, it is crucial to acknowledge the substantial challenges that justice-involved individuals face in accessing medical care, both during incarceration and after release. The government prohibits the use of Medicaid funds for healthcare services to individuals in carceral settings, including incarcerated individuals post-sentencing and those in custody.[37] This policy is known as the *inmate payment exclusion* and represents

[36]This library, as documented by Hendren and Sprung-Keyser (2020), converts estimates from various policy studies into their implied MVPFs and is accessible online.

[37]Notably, the Centers for Medicare and Medicaid Services (CMS) does not categorize individuals with "freedom of movement," such as those on probation or parole or those residing in halfway homes or under home confinement, as inmates. Consequently, these individuals may be covered by Medicaid upon meeting eligibility criteria and completing enrollment.

just one layer of multiple barriers to healthcare access. Furthermore, incarcerated individuals are often required to pay medical co-payments ("co-pays") for healthcare services (e.g., physician visits or medications) in most states, with co-pays ranging from \$2 to \$8.[38] For example, until 2019, Texas enforced a flat \$100 annual fee. While a \$2 co-pay might seem minimal, it is nonnegligible for inmates who are often unpaid for their work or, if paid, earn less than 50 cents per hour, typically making just slightly more than the co-pay amount per month. These co-pays aim to deter frivolous medical care and raise revenue at the expense of inmate health and increased risk of recidivism (Wiggins, 2021).

These healthcare access challenges continue upon reentry into society. Some states terminate Medicaid coverage during incarceration, creating a gap in coverage due to delays in eligibility determinations and other barriers to enrollment. Although evidence indicates that suspending, rather than terminating, Medicaid coverage can reduce recidivism, particularly among Black individuals and repeat offenders (Gollu and Zapryanova, 2022), the impact of Medicaid re-enrollment policies may vary by context (Packham and Slusky, 2023).[39]

In addition to re-enrollment policies, several states are implementing policy initiatives through Section 1115 waivers (also known as the Medicaid Reentry Section 1115 Demonstration Opportunity). These waivers are designed to provide Medicaid coverage for a range of pre-release services to incarcerated individuals up to 90 days before their release.[40] For example, California's waiver includes coverage for various pre-release services, such as physical and behavioral health assessments, clinical consultation services, and laboratory and radiology services. Eligible enrollees are also entitled to coverage for prescribed outpatient medications, including over-the-counter drugs, and durable medical equipment upon release. Although our analysis primarily focuses on providing coverage

[38]See the report by the Prison Policy Initiative, which lists medical co-pays across states, at: https://bit.ly/3FDu6Uf.

[39]Using linked administrative data from South Carolina, a state that has not expanded Medicaid, Packham and Slusky (2023) found no evidence of reduced recidivism resulting from a suspension policy, despite observing improvements in healthcare access and utilization among released inmates.

[40]As of March 2024, 17 states are awaiting approval for their Section 1115 reentry waivers. Meanwhile, California, Utah, and Washington have already received approval for their waivers. It is important to note that Utah's approved waiver specifically targets adult groups, including those who are homeless and justice-involved, up to 5% of the federal poverty level, and includes an enrollment cap. Additionally, Utah has a separate waiver under consideration that is specifically for incarcerated individuals.

upon release, the MVPF estimates could offer insights into the cost-effectiveness of reentry policies.

Our MVPF estimates suggest that the indirect benefits associated with expanding health insurance ought to be considered in contemporary health policy debates, and counsel in favor of policies that extend health coverage to individuals exiting incarceration. Providing this coverage prior to release and ensuring continuity of care could result in even greater societal benefits and reduce government costs through fiscal externalities.

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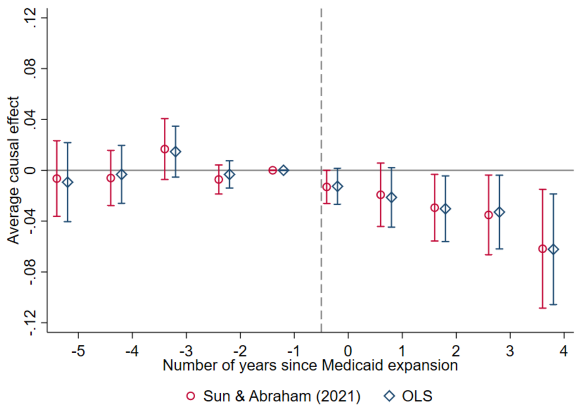
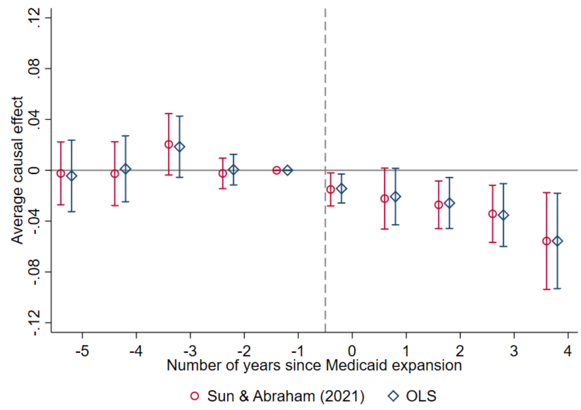
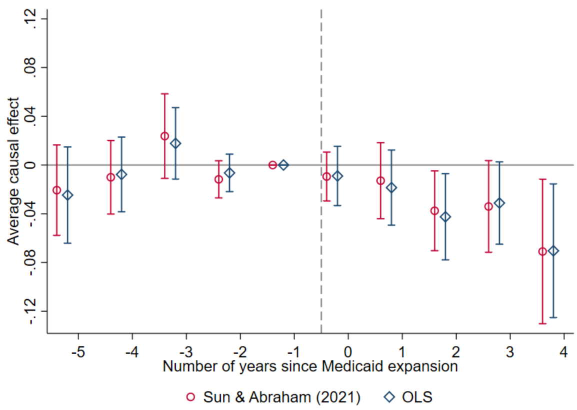


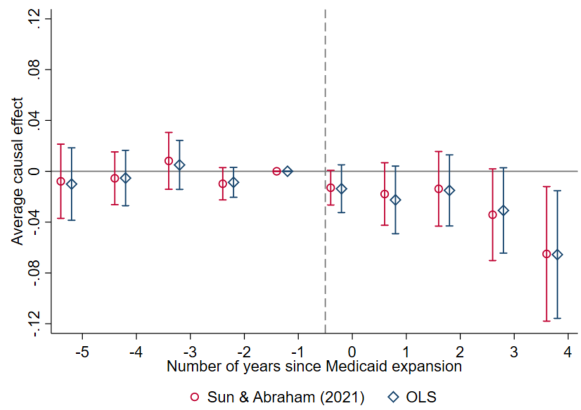
(a) **All Crimes**



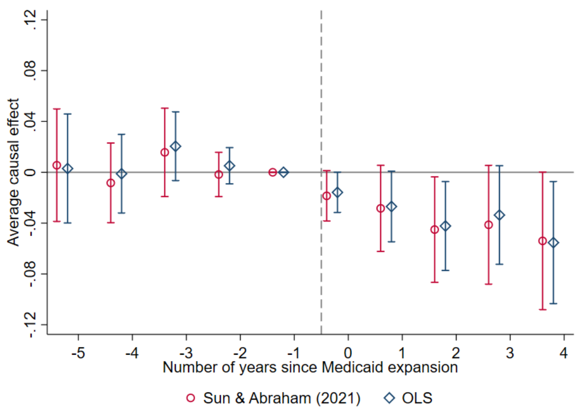
(b) **Violent Crimes**



(c) **Property Crimes**



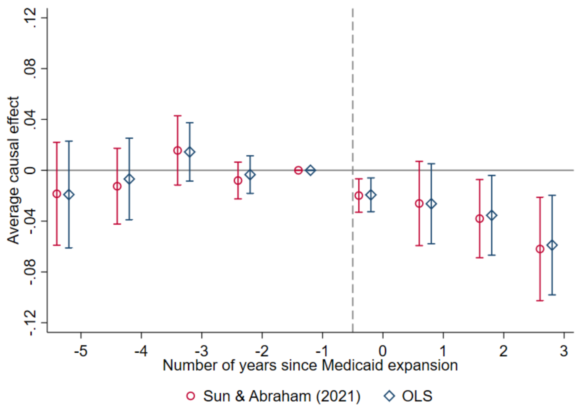
(d) **Drug Crimes**



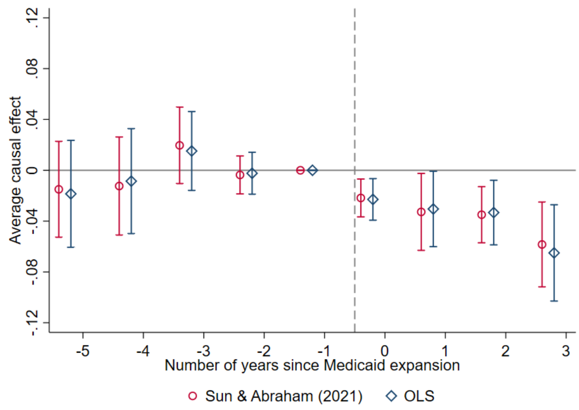
(e) **Public Order Crimes**

Figure 1. Dynamic Difference-in-Differences - Medicaid Expansions and the Number of Reimprisonments (1-Year)

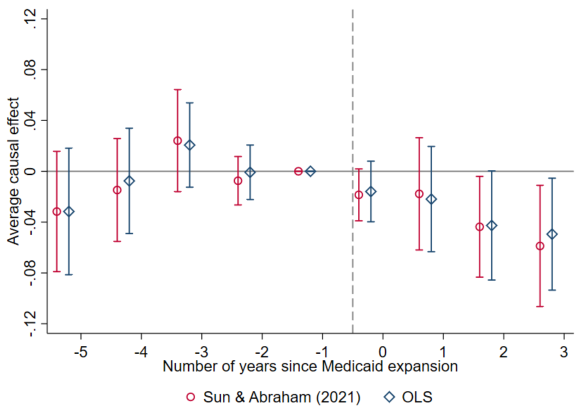
Notes: The figure reports event study estimates showing the effect of Medicaid expansions on the number of reimprisonments within a 1-year window. The horizontal axis shows relative event years. The vertical axis shows the intent-to-treat effects. We report estimates using both the OLS and the interaction-weighted estimator proposed by Sun and Abraham (2021). We also report the 95% confidence intervals in the figure.



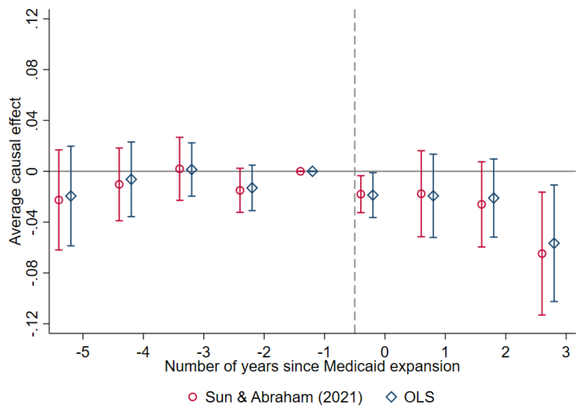
(a) All Crimes



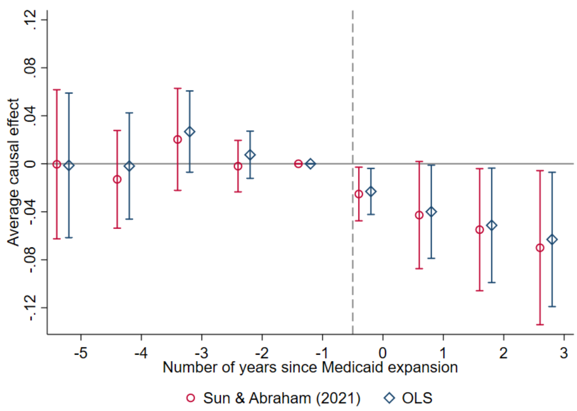
(b) Violent Crimes



(c) Property Crimes



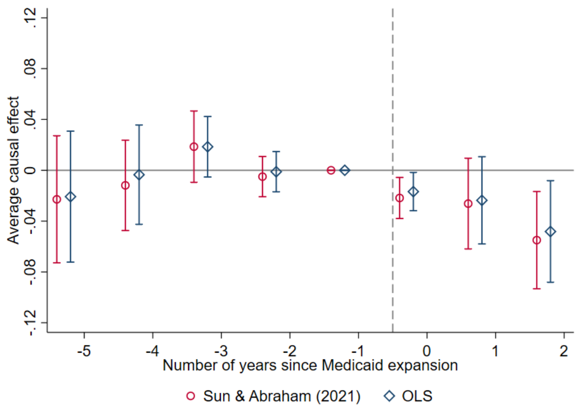
(d) Drug Crimes



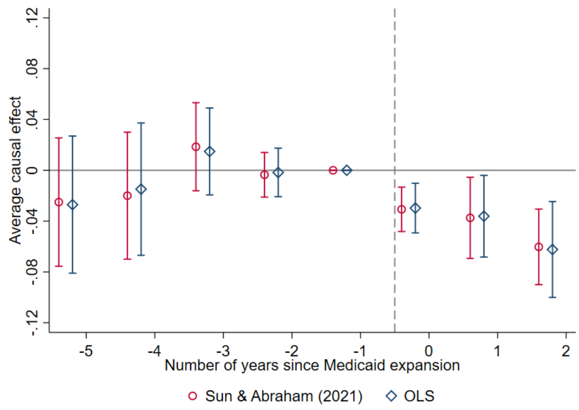
(e) Public Order Crimes

Figure 2. Dynamic Difference-in-Differences - Medicaid Expansions and the Number of Reimprisonments (2-Year)

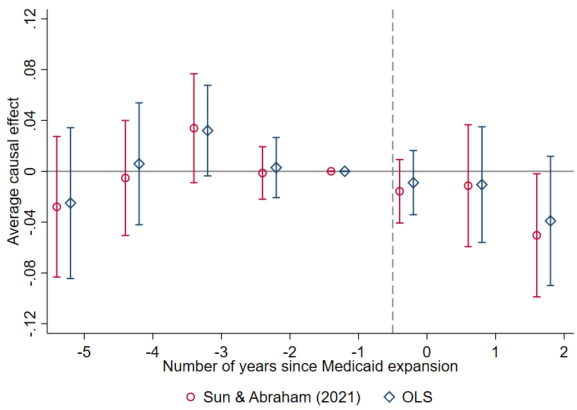
Notes: The figure reports event study estimates showing the effect of Medicaid expansions on the number of reimprisonments within a 2-year window. The horizontal axis shows relative event years. The vertical axis shows the intent-to-treat effects. We report estimates using both the OLS and the interaction-weighted estimator proposed by Sun and Abraham (2021). We also report the 95% confidence intervals in the figure.



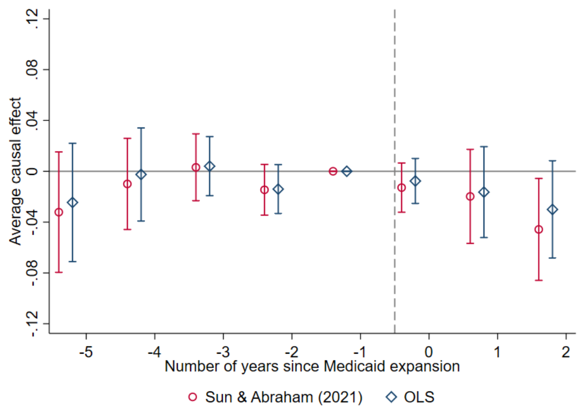
(a) All Crimes



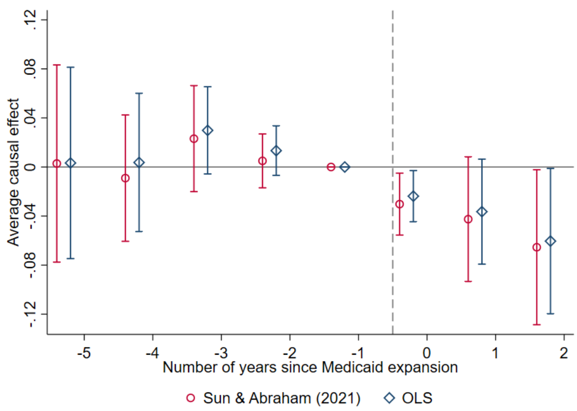
(b) Violent Crimes



(c) Property Crimes



(d) Drug Crimes



(e) Public Order Crimes

Figure 3. Dynamic Difference-in-Differences - Medicaid Expansions and the Number of Reimprisonments (3-Year)

Notes: The figure reports event study estimates showing the effect of Medicaid expansions on the number of reimprisonments within a 3-year window. The horizontal axis shows relative event years. The vertical axis shows the intent-to-treat effects. We report estimates using both the OLS and the interaction-weighted estimator proposed by Sun and Abraham (2021). We also report the 95% confidence intervals in the figure.

Table 1. Medicaid expansion profile by states

Control group		Treatment group							
Not expanded	Not in NCRP	Expanded		Early expansion/Prior comprehensive program		Expanded late		Not in NCRP	
Alabama	Idaho	Arizona	01/01/2014	Delaware	01/01/2014	Alaska	09/01/2015	Arkansas	01/01/2014
Florida	Virginia	California[†]	01/01/2014	District of Columbia	07/01/2010	Indiana	02/01/2015	Connecticut	04/01/2010
Georgia		Colorado	01/01/2014	Massachusetts	01/01/2014	Michigan	04/01/2014	Hawaii	01/01/2014
Kansas		Illinois	01/01/2014	Minnesota	03/01/2010	New Hampshire	08/15/2014	Vermont	01/01/2014
Maine		Iowa	01/01/2014	New York	01/01/2014	Pennsylvania	01/01/2015		
Mississippi		Kentucky	01/01/2014			Louisiana	07/01/2016		
Missouri		Maryland	01/01/2014			Montana	01/01/2016		
Nebraska		Nevada	01/01/2014						
North Carolina		New Jersey	01/01/2014						
Oklahoma		New Mexico	01/01/2014						
South Carolina		North Dakota	01/01/2014						
South Dakota		Ohio	01/01/2014						
Tennessee		Oregon	01/01/2014						
Texas		Rhode Island	01/01/2014						
Utah		Washington	01/01/2014						
Wyoming		West Virginia	01/01/2014						
		Wisconsin[*]	01/01/2014						
$N = 16$	$N = 2$	$N = 17$		$N = 5$		$N = 7$		$N = 4$	

Note: [†] We exclude California in our analysis given the state's enactment of the Public Safety Relignment Act (PSRA) in 2011 (see, e.g., Agan and Makowsky, 2018 for the effects of this policy). [*] We further include Wisconsin in the treatment group to account for the fact that childless adults with incomes up to 100 percent FPL are eligible for Medicaid in Wisconsin. See Aslim et al. (2022) for more details.

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

Table 2. Summary Statistics - Reimprisonments by Crime Type

Dependent Variables	All States		Expansion States					Non-Expansion States				
	Pre-treatment Mean	Post-treatment Mean	Mean	Std. Dev.	Pre-Treatment Mean	Post-Treatment Mean	Diff. p-values	Mean	Std. Dev.	Pre-Treatment Mean	Post-Treatment Mean	Diff. p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1-Year Reimprisonments by Crime Type:												
All	0.192	0.200	0.230	0.504	0.232	0.228	0.001	0.161	0.407	0.151	0.173	0.001
Violent	0.178	0.175	0.214	0.483	0.222	0.204	0.000	0.140	0.383	0.134	0.146	0.000
Property	0.237	0.246	0.285	0.553	0.284	0.287	0.180	0.200	0.448	0.193	0.209	0.000
Drug	0.167	0.192	0.203	0.468	0.197	0.210	0.000	0.155	0.399	0.139	0.175	0.000
Public Order	0.190	0.194	0.226	0.516	0.232	0.218	0.000	0.146	0.388	0.133	0.164	0.000
2-Year Reimprisonments by Crime Type:												
All	0.192	0.202	0.340	0.637	0.341	0.338	0.032	0.253	0.526	0.244	0.268	0.000
Violent	0.178	0.178	0.321	0.613	0.327	0.310	0.000	0.223	0.498	0.218	0.230	0.000
Property	0.237	0.247	0.414	0.691	0.413	0.417	0.220	0.313	0.575	0.307	0.322	0.000
Drug	0.167	0.193	0.301	0.592	0.294	0.312	0.000	0.245	0.517	0.230	0.269	0.000
Public Order	0.190	0.194	0.331	0.657	0.339	0.317	0.000	0.227	0.500	0.213	0.249	0.000
3-Year Reimprisonments by Crime Type:												
All	0.192	0.200	0.422	0.741	0.424	0.417	0.000	0.328	0.619	0.320	0.343	0.000
Violent	0.178	0.178	0.402	0.713	0.407	0.390	0.000	0.291	0.585	0.286	0.299	0.000
Property	0.237	0.245	0.510	0.801	0.511	0.508	0.379	0.406	0.682	0.402	0.413	0.001
Drug	0.167	0.190	0.374	0.686	0.369	0.387	0.000	0.316	0.607	0.304	0.341	0.000
Public Order	0.190	0.191	0.411	0.770	0.421	0.388	0.000	0.290	0.581	0.279	0.312	0.000

Notes: This table reports the summary statistics for the number of reimprisonments within 1-, 2-, and 3-year windows by crime type. Our sample corresponds to those reported in Table 5. Crime categories refer to the first offense type that end with an imprisonment. For non-expansion states, we consider years prior to 2014 as pre-treatment years for the purpose of constructing means for the outcome variable. Differences in p-values reported in columns (7) and (12) measure the statistical significance of the differences between post- and pre-treatment means of the outcome variables in expansion and non-expansion states, respectively.

Table 3. Summary Statistics - All Crime Sample

	1-Year Window		2-Year Window		3-Year Window	
Variables	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Age When Released						
25-34 years	0.510	0.500	0.509	0.500	0.508	0.500
35-44 years	0.298	0.458	0.297	0.457	0.296	0.457
45-54 years	0.186	0.389	0.188	0.391	0.190	0.392
Gender						
Female	0.170	0.376	0.168	0.374	0.166	0.372
Race/Ethnicity						
White	0.453	0.498	0.452	0.498	0.451	0.498
Black	0.269	0.443	0.272	0.445	0.275	0.446
Hispanic	0.148	0.355	0.150	0.357	0.151	0.358
Other Races	0.019	0.135	0.018	0.134	0.018	0.134
Time Served						
<1 year	0.457	0.498	0.457	0.498	0.458	0.498
1-1.9 years	0.206	0.404	0.206	0.405	0.206	0.404
2-4.9 years	0.198	0.399	0.199	0.399	0.200	0.400
5-9.9 years	0.089	0.284	0.088	0.284	0.088	0.283
>=10 years	0.050	0.218	0.050	0.217	0.049	0.216
Sentence Length						
<1 year	0.183	0.387	0.186	0.389	0.190	0.392
1-1.9 years	0.079	0.269	0.078	0.268	0.077	0.267
2-4.9 years	0.329	0.470	0.327	0.469	0.327	0.469
5-9.9 years	0.219	0.413	0.218	0.413	0.217	0.412
10-24.9 years	0.153	0.360	0.152	0.359	0.151	0.358
>=25 years	0.027	0.163	0.028	0.164	0.028	0.164
Life, LWOP	0.007	0.082	0.007	0.081	0.007	0.081
Admission Type						
New Court Commitment	0.904	0.295	0.904	0.295	0.905	0.293
Return from Parole / Revocation	0.072	0.259	0.073	0.260	0.073	0.260
Other	0.004	0.061	0.004	0.062	0.004	0.062
Release Type						
Conditional Release	0.659	0.474	0.651	0.477	0.642	0.479
Unconditional Release	0.283	0.451	0.288	0.453	0.293	0.455
Other Types of Release	0.008	0.088	0.008	0.090	0.008	0.092
Minimum Wage	7.463	0.884	7.398	0.778	7.337	0.696
Housing Price Index	258.921	73.183	254.238	71.207	250.201	70.184
Unemployment Rate	6.849	2.304	7.143	2.207	7.462	2.102
Poverty Rate	14.738	3.055	14.957	2.990	15.187	2.902
High School Diploma or Higher	0.862	0.030	0.860	0.030	0.858	0.030
Obs.	1,768,232		1,606,648		1,439,707	

Notes: This table reports the summary statistics for both individual- and state-level characteristics used in the analysis. Our sample corresponds to those reported in columns (1)-(2) in Table 5 for *all crimes*. Missing values are not reported in the table. However, we include an indicator variable for missing values in our analysis.

Table 4. The Distribution of Reimprisonments

Panel A: 1-Year Window

Number of Reimprisonments	Expansion			Non-Expansion			Expansion - Non-Expansion
	Pre-treatment	Post-treatment	Change	Pre-treatment	Post-treatment	Change	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
0	79.78%	80.45%	0.67%	86.07%	84.38%	-1.69%	2.36%
1	17.71%	16.93%	-0.78%	12.89%	14.07%	1.18%	-1.96%
2	2.15%	2.15%	-0.00%	0.97%	1.39%	-0.42%	-0.42%
3+	0.36%	0.47%	0.11%	0.07%	0.15%	0.08%	0.03%

Panel B: 2-Year Window

Number of Reimprisonments	Expansion			Non-Expansion			Expansion - Non-Expansion
	Pre-treatment	Post-treatment	Change	Pre-treatment	Post-treatment	Change	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
0	72.80%	73.35%	0.55%	78.99%	77.52%	-1.47%	2.02%
1	21.76%	21.19%	-0.57%	18.08%	18.87%	0.79%	-1.36%
2	4.36%	4.31%	-0.05%	2.54%	3.03%	0.49%	-0.54%
3+	1.08%	1.15%	0.07%	0.39%	0.58%	0.19%	-0.12%

Panel C: 3-Year Window

Number of Reimprisonments	Expansion			Non-Expansion			Expansion - Non-Expansion
	Pre-treatment	Post-treatment	Change	Pre-treatment	Post-treatment	Change	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
0	68.63%	69.32%	0.69%	74.16%	72.97%	-1.19%	1.88%
1	23.15%	22.68%	-0.47%	20.80%	21.29%	0.49%	-0.96%
2	6.21%	5.94%	-0.27%	4.08%	4.51%	0.43%	-0.70%
3+	2.01%	2.07%	0.06%	0.96%	1.22%	0.26%	-0.20%

Notes: This table reports the distribution of the outcome variable within 1-, 2-, and 3-year windows for all crime types. For non-expansion states, we consider years prior to 2014 as pre-treatment years for the purpose of obtaining the distribution in the counterfactual scenario. The last column reports the difference in columns (3) and (6).

Table 5. Static Difference-in-Differences - Medicaid Expansions and the Number of Reimprisonments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: 1-Year Window	**All**		**Violent**		**Property**		**Drug**		**Public Order**	
Expansion	-0.026**	-0.022**	-0.029***	-0.026**	-0.022	-0.019	-0.020	-0.015	-0.035**	-0.035***
	(0.011)	(0.011)	(0.010)	(0.010)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.011)
Randomization Inference p-value	0.016	0.042	0.001	0.011	0.060	0.095	0.075	0.190	0.009	0.004
Wild Bootstrap p-value	0.033	0.064	0.006	0.013	0.146	0.208	0.164	0.351	0.008	0.002
Pre-Treatment Mean Dep. Var.	0.192	0.192	0.178	0.178	0.237	0.237	0.167	0.167	0.190	0.190
N	1,768,232	1,768,232	513,608	513,608	438,782	438,782	502,062	502,062	313,780	313,780
Panel B: 2-Year Window										
Expansion	-0.026*	-0.027*	-0.030**	-0.034**	-0.022	-0.026	-0.016	-0.015	-0.044**	-0.045***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.018)	(0.017)	(0.015)	(0.016)	(0.018)	(0.016)
Randomization Inference p-value	0.039	0.045	0.015	0.015	0.119	0.094	0.160	0.231	0.010	0.005
Wild Bootstrap p-value	0.069	0.079	0.040	0.020	0.238	0.168	0.339	0.456	0.013	0.008
Pre-Treatment Mean Dep. Var.	0.192	0.192	0.178	0.178	0.237	0.237	0.167	0.167	0.190	0.190
N	1,606,648	1,606,648	463,207	463,207	399,959	399,959	457,084	457,084	286,398	286,398
Panel C: 3-Year Window										
Expansion	-0.024	-0.029	-0.034*	-0.042**	-0.018	-0.026	-0.008	-0.013	-0.045*	-0.043**
	(0.018)	(0.017)	(0.017)	(0.017)	(0.023)	(0.023)	(0.019)	(0.020)	(0.024)	(0.019)
Randomization Inference p-value	0.095	0.058	0.025	0.007	0.210	0.135	0.347	0.290	0.031	0.016
Wild Bootstrap p-value	0.193	0.111	0.067	0.017	0.476	0.280	0.724	0.587	0.047	0.027
Pre-Treatment Mean Dep. Var.	0.192	0.192	0.178	0.178	0.237	0.237	0.167	0.167	0.190	0.190
N	1,439,707	1,439,707	412,414	412,414	359,132	359,132	410,085	410,085	258,076	258,076
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Release-Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State-Specific Time-Varying Controls	×	✓	×	✓	×	✓	×	✓	×	✓

Notes: This table reports the static difference-in-differences estimates using the OLS estimator. Different panels correspond to various time windows (1-, 2-, and 3-year windows) of the outcome variable. Crime categories refer to the first offense type that ends with imprisonment. State-specific time-varying controls include the minimum wage, housing price index, the unemployment rate, poverty rate, and educational attainment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6. Social Costs per Released Inmate

Reoffense Type	Within-Category Weights (w_i^J, %)	Estimated Social Cost Based On	
		Cohen & Piquero (2009)	Miller et al. (2021)
	(1)	(2)	(3)
Violent Crimes (20.93% of all reoffenses)			
Murder	3.33	5,740,800	8,089,301
Rape	4.50	168,480	239,095
Robbery	48.83	14,976	28,592
Aggravated or simple assault	43.34	12,069	28,971
Exp. Cost Averted per Inmate (Violent Crimes)		14,100	20,464
Property Crimes (32.95% of all reoffenses)			
Larceny	17.47	562	2,730
Burglary	72.96	2,496	2,571
Motor vehicle theft	9.57	6,864	8,949
Exp. Cost Averted per Inmate (Property Crimes)		808	1,006
Drug Crimes (27.34% of all reoffenses)	100	0	5,387
Exp. Cost Averted per Inmate (Drug Crimes)		0	32
Public Order Crimes (18.78% of all reoffenses)	100	12,133	14,577
Exp. Cost Averted per Inmate (Public Order Crimes)		26	60
Total Social Cost per Inmate		14,934	21,562

Notes: Within-category weights (w_i^J) are calculated using the share of arrests for each sub-crime category and the average victimization-to-arrest ratios from Jácome (2020). See Table A5 for technical details. The estimated lower bound costs come from Cohen and Piquero (2009) (Table 5, inflated to 2020 dollars). The estimated upper bound costs come from Miller et al. (2021) (Table 5, inflated to 2020 dollars). See the Appendix for a detailed explanation on average victimization (social) costs by crime categories.

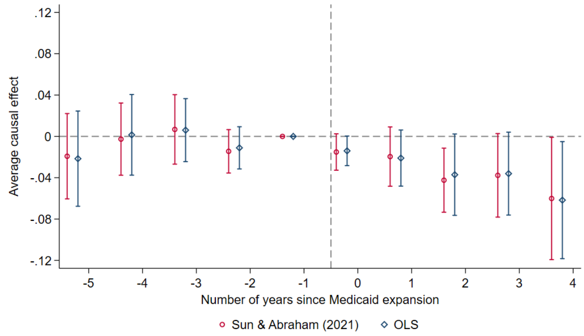
Table 7. Marginal Value of Public Funds (MVPF)

	Estimated Cost	
	(1)	(2)
Willingness to Pay:		
Fewer crime victimizations	\$14,934	\$21,562
Improved labor market prospects	\$0	\$307
Value of insurance transfer	\$2,752	\$2,819
Avoiding incarceration	\$0	\$242
Aggregate willingness to pay:	\$17,686	\$24,930
Costs to the Government:		
Cost of providing Medicaid	\$5,873	\$4,296
Public assistance	\$42	\$22
Fewer incarcerations	-\$790	-\$1,909
Foregone tax revenue	-\$0	-\$61
Net Cost:	\$5,125	\$2,348
Marginal Value of Public Funds:	3.45	10.62

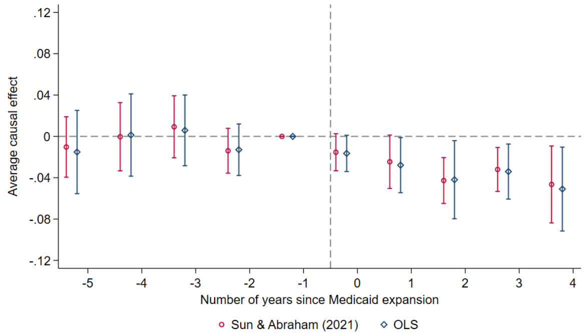
Notes: This table shows the welfare implications of providing Medicaid to a released inmate under the Affordable Care Act. Columns (1) and (2) report the lower and upper bound estimates for the marginal value of public funds (MVPF), respectively. The lower bound provides a conservative estimate for the MVPF ratio. We adjust the estimated values to 2020 dollars.

For Online Publication: Appendix A

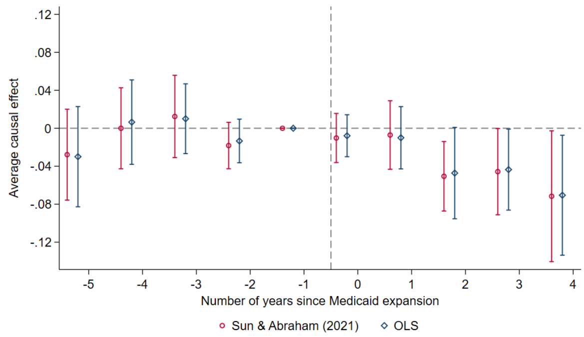
A1. Supplemental Figures & Tables



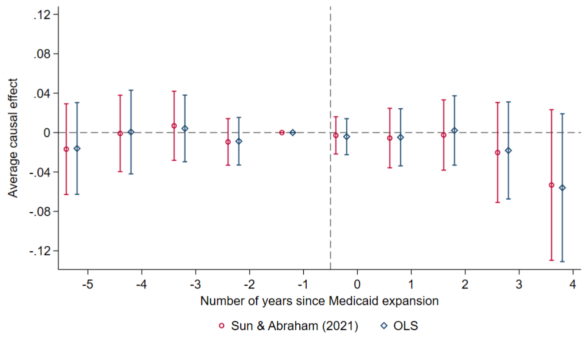
(a) All Crimes



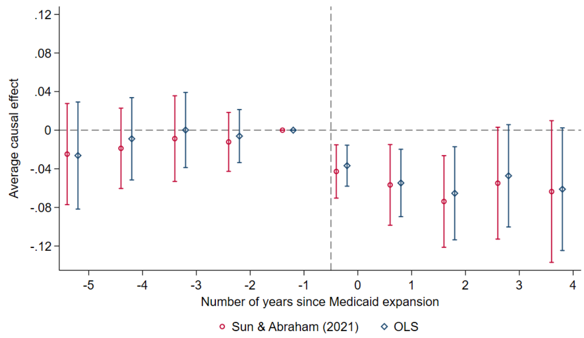
(b) Violent Crimes



(c) Property Crimes



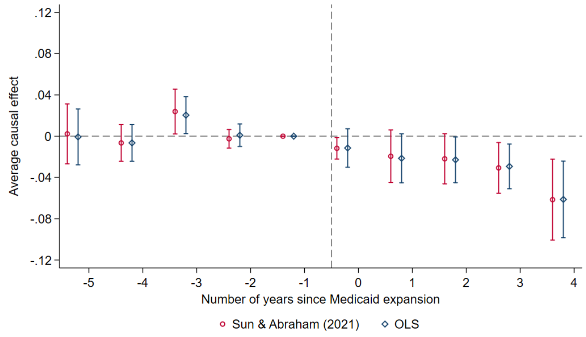
(d) Drug Crimes



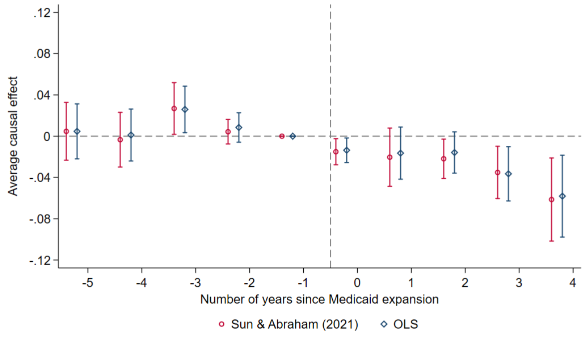
(e) Public Order Crimes

Figure A1. Dynamic Difference-in-Differences - Medicaid Expansions and the Number of Reimprisonments Among White Inmates (1-Year)

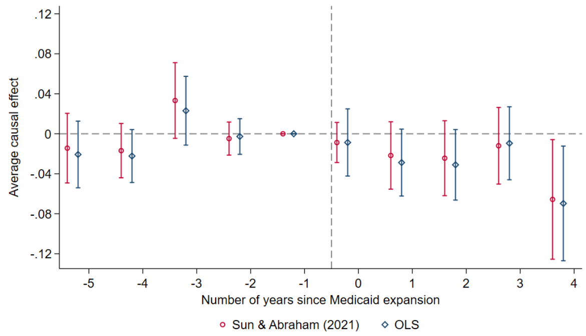
Notes: The figure reports event study estimates showing the effect of Medicaid expansions on the number of reimprisonments within a 1-year window among White inmates. The horizontal axis shows relative event years. The vertical axis shows the intent-to-treat effects. We report estimates using both the OLS and the interaction-weighted estimator proposed by Sun and Abraham (2021). We also report the 95% confidence intervals in the figure.



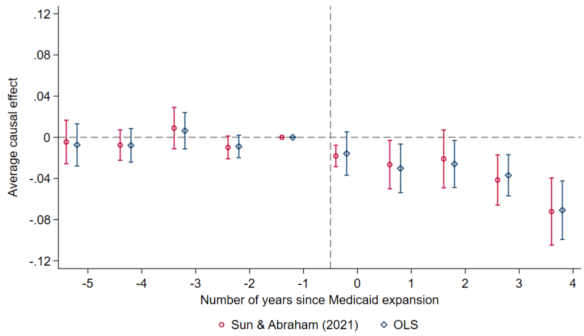
(a) All Crimes



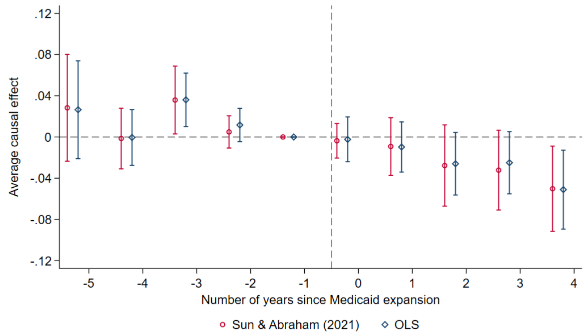
(b) Violent Crimes



(c) Property Crimes



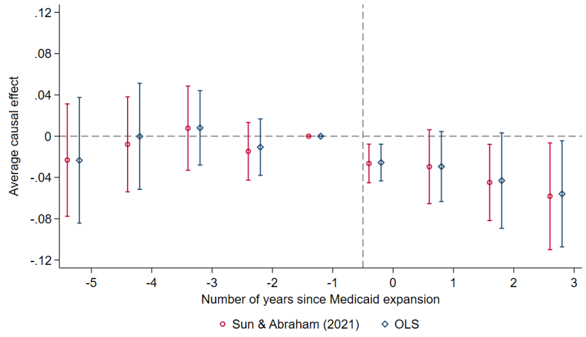
(d) Drug Crimes



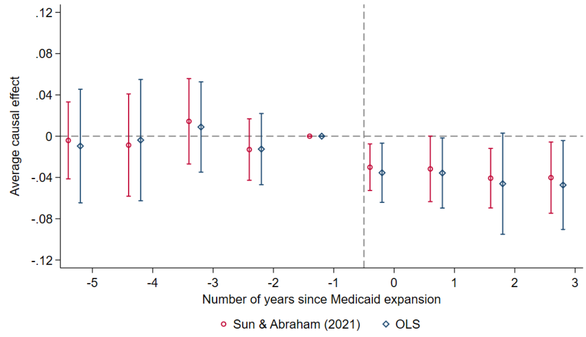
(e) Public Order Crimes

Figure A2. Dynamic Difference-in-Differences - Medicaid Expansions and the Number of Reimprisonments Among Non-White Inmates (1-Year)

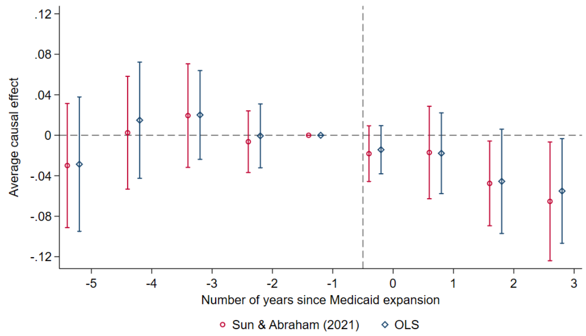
Notes: The figure reports event study estimates showing the effect of Medicaid expansions on the number of reimprisonments within a 1-year window among non-White inmates. The horizontal axis shows relative event years. The vertical axis shows the intent-to-treat effects. We report estimates using both the OLS and the interaction-weighted estimator proposed by Sun and Abraham (2021). We also report the 95% confidence intervals in the figure.



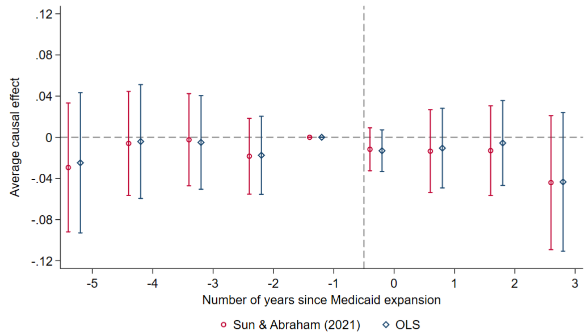
(a) All Crimes



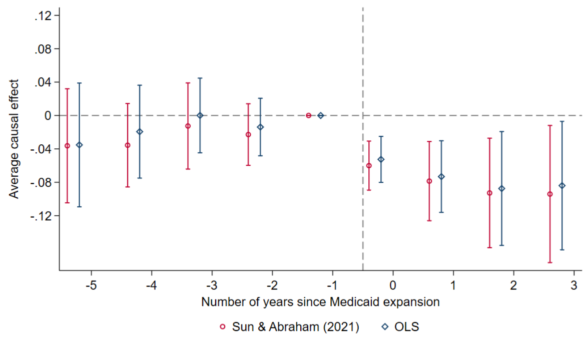
(b) Violent Crimes



(c) Property Crimes



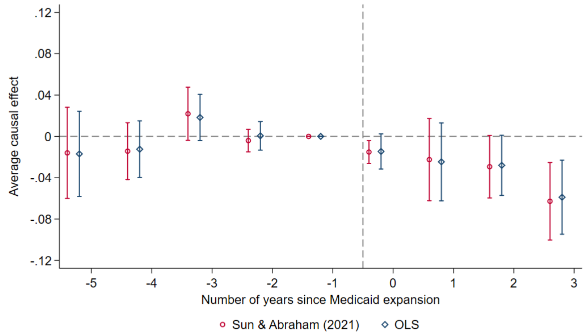
(d) Drug Crimes



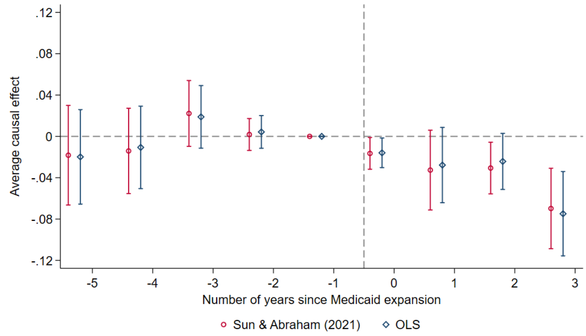
(e) Public Order Crimes

Figure A3. Dynamic Difference-in-Differences - Medicaid Expansions and the Number of Reimprisonments Among White Inmates (2-Year)

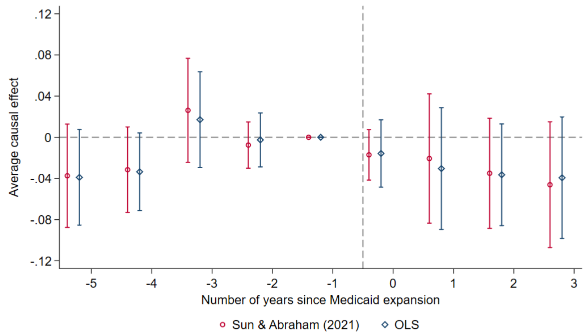
Notes: The figure reports event study estimates showing the effect of Medicaid expansions on the number of reimprisonments within a 2-year window among White inmates. The horizontal axis shows relative event years. The vertical axis shows the intent-to-treat effects. We report estimates using both the OLS and the interaction-weighted estimator proposed by Sun and Abraham (2021). We also report the 95% confidence intervals in the figure.



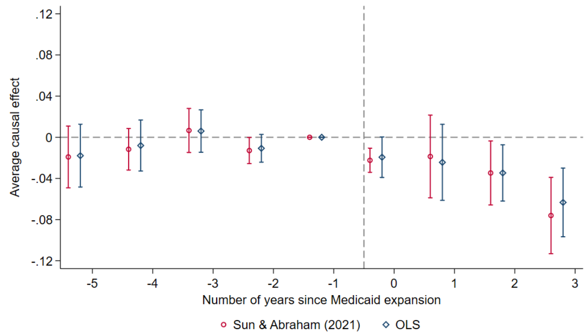
(a) **All Crimes**



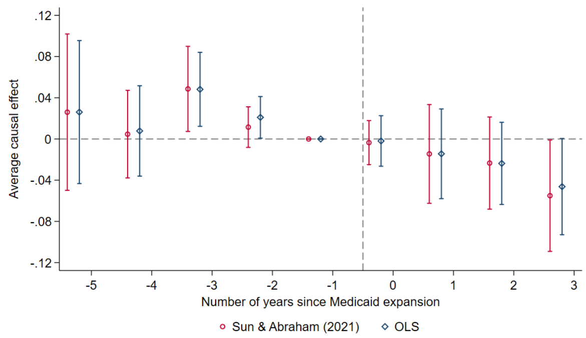
(b) **Violent Crimes**



(c) **Property Crimes**



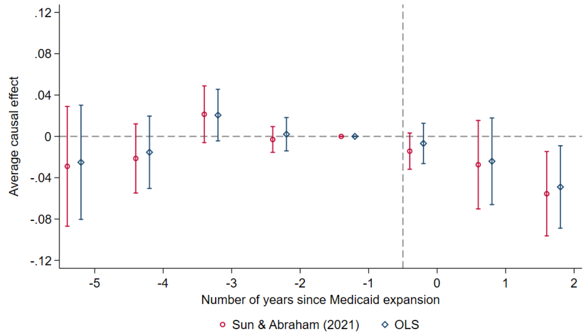
(d) **Drug Crimes**



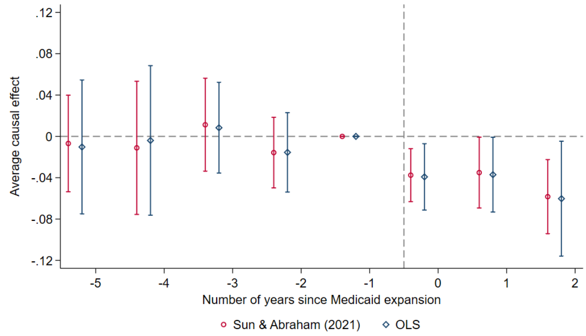
(e) **Public Order Crimes**

Figure A4. Dynamic Difference-in-Differences - Medicaid Expansions and the Number of Reimprisonments Among Non-White Inmates (2-Year)

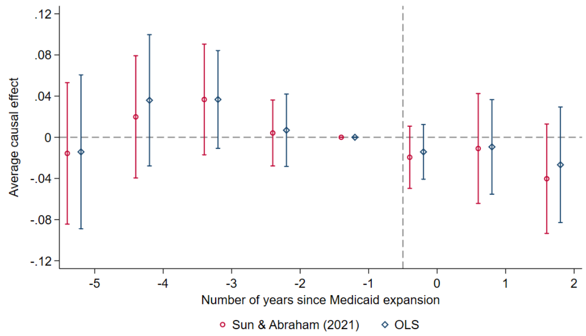
Notes: The figure reports event study estimates showing the effect of Medicaid expansions on the number of reimprisonments within a 2-year window among non-White inmates. The horizontal axis shows relative event years. The vertical axis shows the intent-to-treat effects. We report estimates using both the OLS and the interaction-weighted estimator proposed by Sun and Abraham (2021). We also report the 95% confidence intervals in the figure.



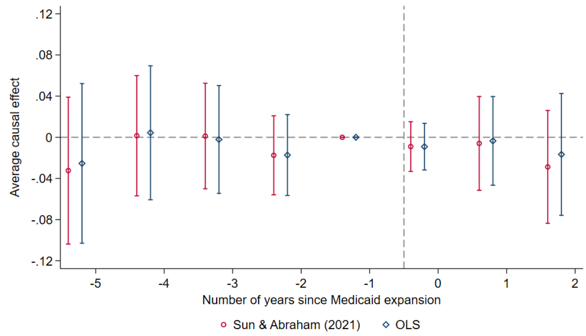
(a) All Crimes



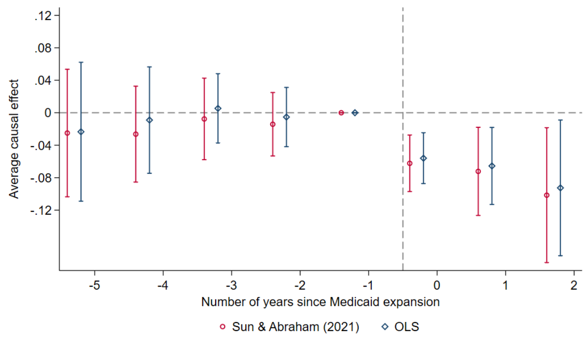
(b) Violent Crimes



(c) Property Crimes



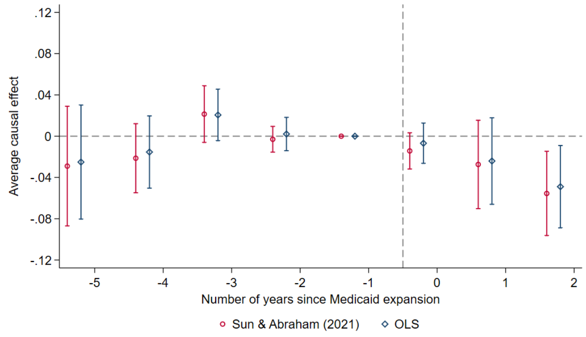
(d) Drug Crimes



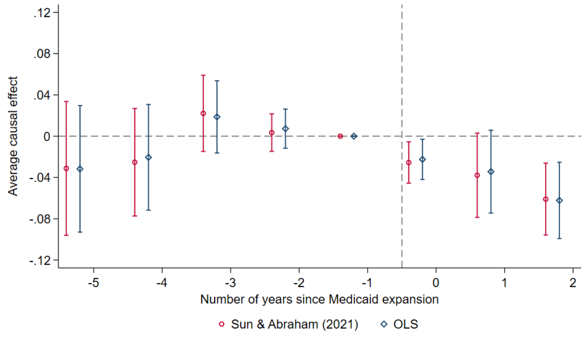
(e) Public Order Crimes

Figure A5. Dynamic Difference-in-Differences - Medicaid Expansions and the Number of Reimprisonments Among White Inmates (3-Year)

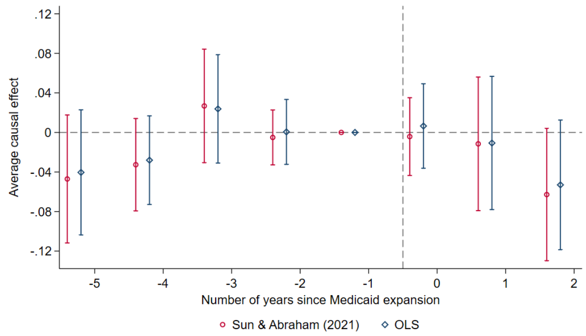
Notes: The figure reports event study estimates showing the effect of Medicaid expansions on the number of reimprisonments within a 3-year window among White inmates. The horizontal axis shows relative event years. The vertical axis shows the intent-to-treat effects. We report estimates using both the OLS and the interaction-weighted estimator proposed by Sun and Abraham (2021). We also report the 95% confidence intervals in the figure.



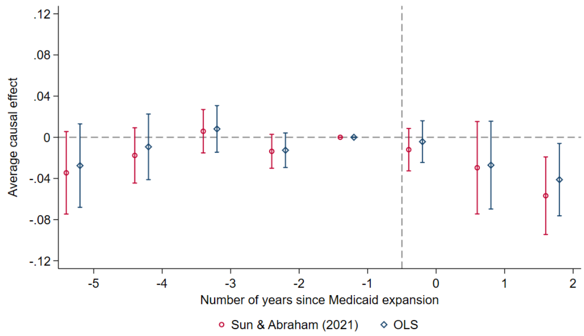
(a) All Crimes



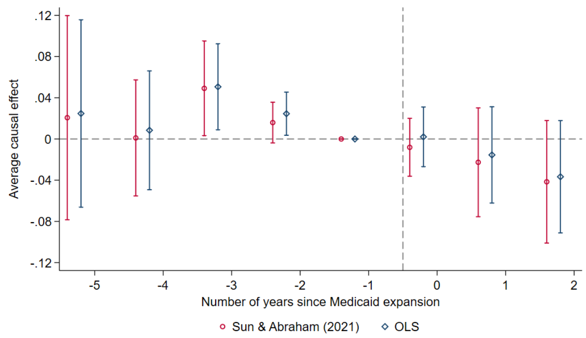
(b) Violent Crimes



(c) Property Crimes



(d) Drug Crimes



(e) Public Order Crimes

Figure A6. Dynamic Difference-in-Differences - Medicaid Expansions and the Number of Reimprisonments Among Non-White Inmates (3-Year)

Notes: The figure reports event study estimates showing the effect of Medicaid expansions on the number of reimprisonments within a 3-year window among non-White inmates. The horizontal axis shows relative event years. The vertical axis shows the intent-to-treat effects. We report estimates using both the OLS and the interaction-weighted estimator proposed by Sun and Abraham (2021). We also report the 95% confidence intervals in the figure.

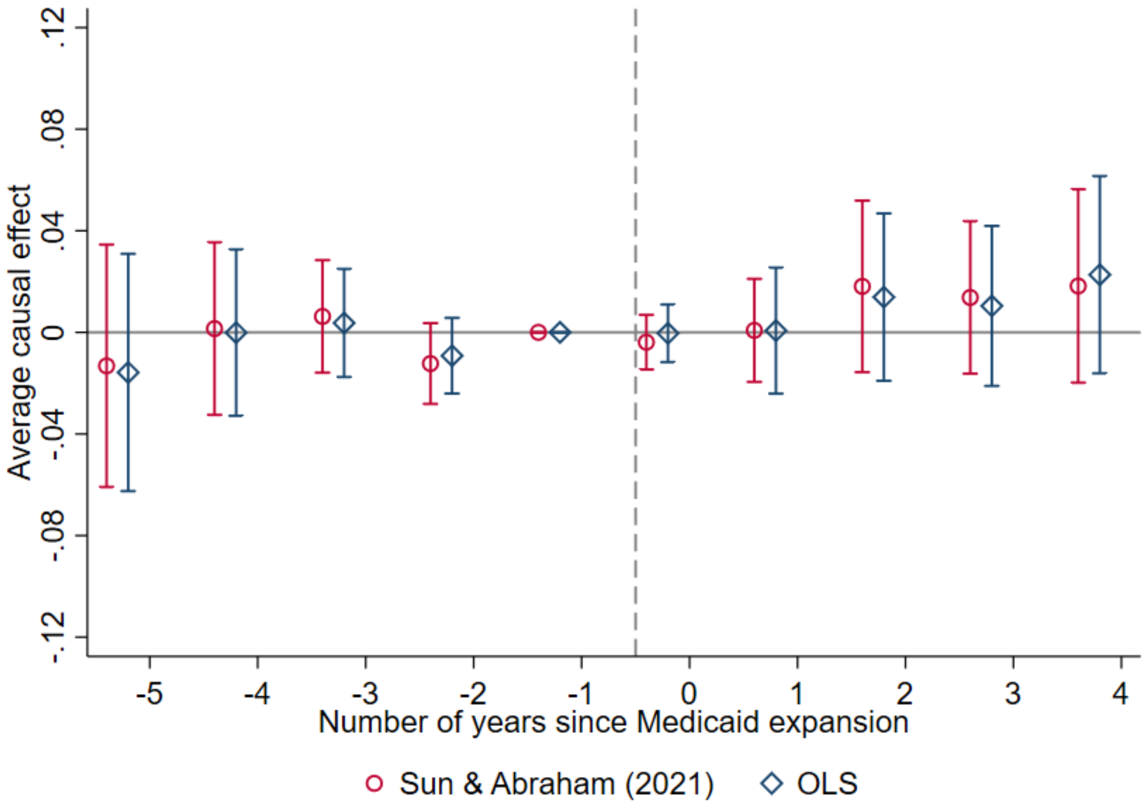


Figure A7. Dynamic Difference-in-Differences (DID) - Likelihood of Serving a Long Sentence - All Crimes

Notes: The figure contains dynamic DID estimates showing the effect of Medicaid expansions on the likelihood of a sentence being longer than 2 years for an individual's first offense. The vertical axis show the intent-to-treat effects. We report estimates using both the OLS and the interaction-weighted estimator proposed by Sun and Abraham (2021). We also report the 95% confidence intervals in the figure.

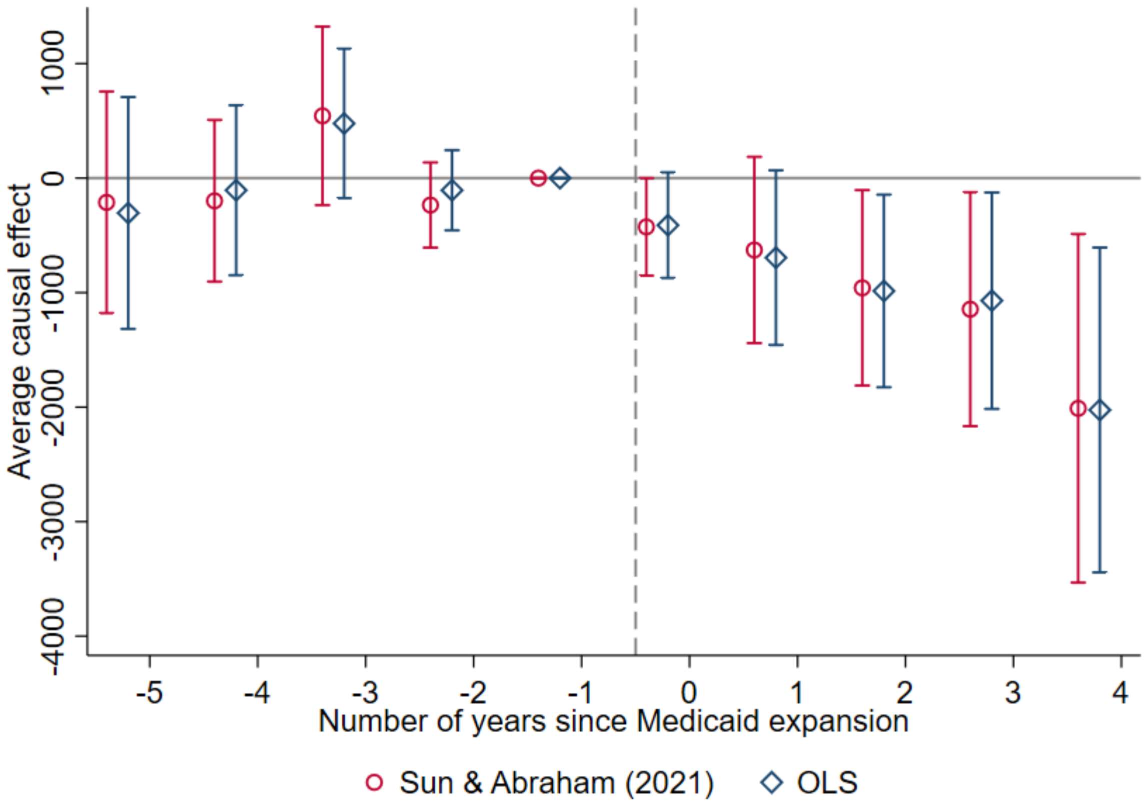


Figure A8. Dynamic Difference-in-Differences (DID) - Mechanical Costs Net of Fiscal Externalities (1-Year) - All Crimes

Notes: The figure contains dynamic DID estimates showing the effect of Medicaid expansions on net costs (mechanical costs net of fiscal externalities) associated with the reduction in the number of future reimprisonments within a 1-year window among all inmates. The vertical axis show the intent-to-treat effects. We report estimates using both the OLS and the interaction-weighted estimator proposed by Sun and Abraham (2021). We also report the 95% confidence intervals in the figure.

Table A1. Static Difference-in-Differences - Medicaid Expansions and the Number of Reimprisonments Among White Inmates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: 1-Year Window	**All**		**Violent**		**Property**		**Drug**		**Public Order**	
Expansion	-0.025*	-0.021	-0.027**	-0.028**	-0.024	-0.020	-0.010	-0.003	-0.041**	-0.045***
	(0.014)	(0.014)	(0.010)	(0.012)	(0.015)	(0.015)	(0.020)	(0.018)	(0.015)	(0.014)
Wild Bootstrap p-value	0.104	0.171	0.013	0.029	0.125	0.229	0.677	0.880	0.015	0.001
Pre-Treatment Mean Dep. Var.	0.213	0.213	0.186	0.186	0.256	0.256	0.190	0.190	0.209	0.209
N	800,912	800,912	195,611	195,611	242,852	242,852	216,633	216,633	145,816	145,816
Panel B: 2-Year Window										
Expansion	-0.030*	-0.028	-0.036**	-0.040***	-0.029	-0.029	-0.007	-0.001	-0.056***	-0.058***
	(0.017)	(0.018)	(0.013)	(0.015)	(0.018)	(0.019)	(0.024)	(0.022)	(0.020)	(0.021)
Wild Bootstrap p-value	0.091	0.169	0.016	0.009	0.136	0.167	0.798	0.975	0.005	0.015
Pre-Treatment Mean Dep. Var.	0.213	0.213	0.186	0.186	0.256	0.256	0.190	0.190	0.209	0.209
N	725,978	725,978	176,805	176,805	220,975	220,975	194,844	194,844	133,354	133,354
Panel C: 3-Year Window										
Expansion	-0.029	-0.030	-0.039**	-0.043**	-0.026	-0.032	-0.002	-0.001	-0.059**	-0.057**
	(0.020)	(0.021)	(0.016)	(0.017)	(0.023)	(0.023)	(0.027)	(0.026)	(0.024)	(0.023)
Wild Bootstrap p-value	0.165	0.225	0.026	0.011	0.266	0.220	0.955	0.979	0.017	0.020
Pre-Treatment Mean Dep. Var.	0.213	0.213	0.186	0.186	0.256	0.256	0.190	0.190	0.209	0.209
N	1,439,707	1,439,707	412,414	412,414	359,132	359,132	410,085	410,085	258,076	258,076
State Fixed Effects	√	√	√	√	√	√	√	√	√	√
Release-Year Fixed Effects	√	√	√	√	√	√	√	√	√	√
State-Specific Time-Varying Controls	×	√	×	√	×	√	×	√	×	√

Notes: This table reports the static difference-in-differences estimates using the OLS estimator. Different panels correspond to various time windows (1-, 2-, and 3-year windows) of the outcome variable. Crime categories refer to the first offense type that ends with imprisonment. State-specific time-varying controls include the minimum wage, housing price index, the unemployment rate, poverty rate, and educational attainment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2. Static Difference-in-Differences - Medicaid Expansions and the Number of Reimprisonments Among Non-White Inmates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All		**Violent**		**Property**		**Drug**		**Public Order**	
Panel A: 1-Year Window										
Expansion	-0.025**	-0.024**	-0.031***	-0.026**	-0.019	-0.020	-0.025***	-0.026***	-0.029**	-0.028**
	(0.010)	(0.009)	(0.011)	(0.011)	(0.016)	(0.015)	(0.007)	(0.007)	(0.012)	(0.011)
Wild Bootstrap p-value	0.013	0.020	0.003	0.020	0.279	0.277	0.002	0.010	0.014	0.024
Pre-Treatment Mean Dep. Var.	0.176	0.176	0.173	0.173	0.216	0.216	0.153	0.153	0.173	0.173
N	967,320	967,320	317,997	317,997	195,930	195,930	285,429	285,429	167,964	167,964
Panel B: 2-Year Window										
Expansion	-0.022	-0.029**	-0.028*	-0.030*	-0.014	-0.026	-0.021**	-0.031***	-0.033*	-0.036**
	(0.014)	(0.013)	(0.015)	(0.016)	(0.022)	(0.021)	(0.010)	(0.010)	(0.018)	(0.016)
Wild Bootstrap p-value	0.106	0.048	0.077	0.089	0.573	0.288	0.048	0.011	0.066	0.041
Pre-Treatment Mean Dep. Var.	0.176	0.176	0.173	0.173	0.216	0.216	0.153	0.153	0.173	0.173
N	880,670	880,670	286,402	286,402	178,984	178,984	262,240	262,240	153,044	153,044
Panel C: 3-Year Window										
Expansion	-0.018	-0.030*	-0.031	-0.041**	-0.003	-0.020	-0.011	-0.030**	-0.033	-0.034
	(0.019)	(0.018)	(0.019)	(0.020)	(0.031)	(0.032)	(0.016)	(0.014)	(0.026)	(0.022)
Wild Bootstrap p-value	0.385	0.111	0.123	0.037	0.915	0.577	0.512	0.046	0.230	0.123
Pre-Treatment Mean Dep. Var.	0.176	0.176	0.173	0.173	0.216	0.216	0.153	0.153	0.173	0.173
N	790,821	790,821	254,911	254,911	161,166	161,166	237,190	237,190	137,554	137,554
State Fixed Effects	√	√	√	√	√	√	√	√	√	√
Release-Year Fixed Effects	√	√	√	√	√	√	√	√	√	√
State-Specific Time-Varying Controls	×	√	×	√	×	√	×	√	×	√

Notes: This table reports the static difference-in-differences estimates using the OLS estimator. Different panels correspond to various time windows (1-, 2-, and 3-year windows) of the outcome variable. Crime categories refer to the first offense type that ends with imprisonment. State-specific time-varying controls include the minimum wage, housing price index, the unemployment rate, poverty rate, and educational attainment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3. Static Difference-in-Differences - Alternative Estimators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All		**Violent**		**Property**		**Drug**		**Public Order**	
Panel A: 1-Year Window										
Expansion	-0.026**	-0.026**	-0.029**	-0.031***	-0.023	-0.021*	-0.019	-0.019	-0.038***	-0.038***
	(0.013)	(0.010)	(0.012)	(0.010)	(0.015)	(0.013)	(0.014)	(0.013)	(0.015)	(0.012)
Pre-Treatment Mean Dep. Var.	0.192	0.192	0.178	0.178	0.237	0.237	0.167	0.167	0.190	0.190
N	1,768,232	1,743,512	513,608	504,197	438,782	432,593	502,062	496,751	313,780	309,971
Panel B: 2-Year Window										
Expansion	-0.024	-0.025**	-0.027*	-0.031**	-0.020	-0.019	-0.011	-0.013	-0.045**	-0.045***
	(0.015)	(0.013)	(0.015)	(0.014)	(0.019)	(0.015)	(0.017)	(0.014)	(0.019)	(0.017)
Pre-Treatment Mean Dep. Var.	0.192	0.192	0.178	0.178	0.237	0.237	0.167	0.167	0.190	0.190
N	1,606,648	1,590,320	463,207	457,039	399,959	396,084	457,084	453,396	286,398	283,801
Panel C: 3-Year Window										
Expansion	-0.019	-0.023	-0.027	-0.033*	-0.012	-0.015	-0.000	-0.004	-0.044*	-0.046**
	(0.018)	(0.016)	(0.018)	(0.017)	(0.024)	(0.019)	(0.020)	(0.017)	(0.024)	(0.021)
Pre-Treatment Mean Dep. Var.	0.192	0.192	0.178	0.178	0.237	0.237	0.167	0.167	0.190	0.190
N	1,439,707	1,433,053	412,414	409,915	359,132	357,832	410,085	408,323	258,076	256,983
Two-stage estimator à la Gardner (2021)	√	×	√	×	√	×	√	×	√	×
Imputation estimator à la Borusyak et al. (2021)	×	√	×	√	×	√	×	√	×	√

Notes: This table reports the static difference-in-differences estimates using the imputation estimators proposed by Gardner (2021) and Borusyak, Jaravel, and Spiess (2021), respectively. Different panels correspond to various time windows (1-, 2-, and 3-year windows) of the outcome variable. Crime categories refer to the first offense type that ends with imprisonment. All regressions include state fixed effects, release-year fixed effects, and inmate characteristics. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A2. Victimization-Related Costs and Benefits

Here, we provide a brief explanation of how we calculate expected criminal justice cost reductions associated with the receipt of Medicaid by a released inmate. We separate benefits from victimization cost reductions into four categories based on the expected reduction in the four categories of crimes we analyze. Specifically, b^J denotes the expected benefit generated by reductions in the number of victimizations of category $J \in$ {Violent, Property, Drug, Public Order} crimes. Thus, $b = \sum_J b^J$ denotes the total expected benefits from reduced victimization.

As indicated in Table 6, when possible, we break down each crime category in to subcategories, which we henceforth call 'types'. By letting i denote the type of crime within category J, e.g., for violent $i \in$ {Murder, Sex Offenses, Robbery, Assault }, we use the following notation:

M: number of released inmates receiving Medicaid[41];

q: reduction in the average number of reimprisonment per inmate (i.e., the coefficient reported in Table 5, Panel A, Column 2);

p^J: share of reimprisonment for category J crimes (i.e., the percentages reported in the first column of Table 6 for each reoffense type);

r^J: victimization-to-incarceration ratio for category J crimes[42];

$\bar{C}^J$: average victimization cost of a category J crime (i.e., the numbers reported in the lowest row of each crime category in Table 6, Columns 3 and 4);

C_i^J: victimization cost of a type i crime in category J (i.e., the numbers reported in Table 2, Columns 3 and 4); and

w_i^J: share of victimization reduction of type i crime within category J (i.e., the percentages reported in the second column of Table 6).[43]

It then follows that $qp^J M$ is an estimate of the number of category J *imprisonments*

[41]As will become clear from the derivations below, the specific value of M is irrelevant to the derivation of b^J and other values of interest, because it appears only in intermediate steps that are helpful in deriving expressions for these values.

[42] The numbers reported in Table A4 for $J \in$ {Violent, Property}. Because data on these ratios are not available for drug and public order crimes, we assume r^J=1 for these categories to obtain conservative estimates of b^J.

[43]The derivation of these weights are explained in Table A5 and the notes accompanying it.

averted by the policy change. Multiplying this number by r^J converts it to an estimate of the number category J *victimizations* reduced, which equals $qp^J r^J M$. Therefore, the total benefit from reduced category J victimizations is

$$B^J = qp^J r^J M \bar{C}^J \tag{3}$$

where

$$\bar{C}^J \equiv \sum_i w_i^J C_i^J \tag{4}$$

is the average victimization cost associated with a category J crime.

Therefore, the expected benefit from category J crimes obtained from receipt of Medicaid per released inmate is

$$b^J = B^J / M = qp^J r^J \bar{C}^J \tag{5}$$

which can be used to calculate the total expected victimization cost reductions associated with providing a released inmate Medicaid as

$$b = \sum_J b^J \tag{6}$$

A3. Average Victimization Costs by Crime Categories

In this section, we discuss how we obtain average victimization costs reported in Table 6. For consistency, we borrow some of the notation introduced in the previous section.

We use two sources for the costs associated with different types of crime within categories $J \in \{\text{Violent, Property, Drug, Public Order}\}$ crimes. Specifically, we extract victimization costs from Table 5 in Cohen and Piquero (2009) (henceforth 'C&P') and Table 5 in Miller et al. (2021) (henceforth 'MCSAH') for the lower and upper bound estimates, respectively. All victimization costs in Table 6 are reported in 2020 dollars. Because the estimated costs in C&P do not include costs associated with public services, e.g., the use of police and fire services, we also exclude these categories from MCSAH. Our discussion below follows the order of the category J crimes in Table 6.

Violent Crimes

There are four main types of violent crimes: murder, rape, robbery, and assault. We obtain the average victimization cost for murder directly from C&P and MCSAH. The lower and upper bound estimates, adjusted for inflation, are \$5,740,800 and \$8,089,301, respectively. Similarly, we extract victimization costs for rape and robbery from C&P and MCSAH and inflate the costs to 2020 dollars accordingly, which are \$168,480 and \$174,579, respectively. The lower bound for robbery is \$14,976 while the upper bound is \$28,592.

The average victimization cost for assault in MCSAH is \$28,971. C&P, however, report the cost estimates for aggravated and simple assaults separately. For consistency, we calculate a weighted average using the estimated costs for aggravated and simple assaults reported in C&P and the number of aggravated and simple assaults reported in MCSAH. Specifically, the weighted average for aggravated and simple assaults is $\frac{N_{aa}}{N_{aa}+N_{sa}} \times C_{aa} + \frac{N_{sa}}{N_{aa}+N_{sa}} \times C_{sa} = \frac{1{,}417{,}526}{1{,}417{,}526+7{,}492{,}068} \times \$37,000 + \frac{7{,}492{,}068}{1{,}417{,}526+7{,}492{,}068} \times \$4,500 = \$9,670.78$ (in 2007 dollars), where N_{aa} and N_{sa} are the number of aggravated and simple assaults extracted from Table 4 in MCSAH, and C_{aa} and C_{sa} denote victimization costs associated with aggravated and simple assaults, respectively. Adjusting to 2020 dollars, the estimated cost for assault is \$12,069.[44]

Using Equation (5), we estimate the expected benefit from averting violent crimes, which yields a lower bound estimate of $0.022 \times 20.93\% \times 14.51 \times (3.33\% \times \$5,740,800 + 4.5\% \times \$168,480 + 48.83\% \times \$14,976 + 43.34\% \times \$12,069) = \$14,100$. Employing the same approach, the upper bound estimate is $0.022 \times 20.93\% \times 14.51 \times (3.33\% \times \$8,089,301 + 4.5\% \times \$239,095 + 48.83\% \times \$28,592 + 43.34\% \times \$28,971) = \$20,464$.

Property Crimes

For property crimes, we obtain the lower bound cost estimates for larceny, burglary, and motor vehicle theft from C&P. After adjusting to 2020 dollars, the average victimization

[44]Information on the number of crimes is not available in C&P. For consistency, we employ the same number of crimes reported in MCSAH to calculate the weighted average for assault when using the estimated costs from C&P.

costs are \$562, \$2,496, and \$6,864, respectively. The upper bound cost estimates for the same type of crimes come from MCSAH. The average victimization costs for larceny, burglary, and motor vehicle theft are \$2,730, \$2,571, and \$8,949, respectively. Following the same approach discussed above using Equation (5), we estimate the expected benefit from averting property crimes, which yields a lower bound estimate of $0.022 \times 32.95\% \times 43.25 \times (17.47\% \times \$562 + 72.96\% \times \$2,496 + 9.57\% \times \$6,864) = \$808$. The upper bound estimate, on the other hand, is $0.022 \times 32.95\% \times 43.25 \times (17.47\% \times \$2,730 + 72.96\% \times \$2,571 + 9.57\% \times \$8,949) = \$1,006$.

Drug-related Crimes

Victimization costs for drug-related crimes are not reported in C&P. To be conservative, we consider the lower bound as \$0. Therefore, the expected benefit from averting drug-related crimes is also \$0. The intuition is that drug-related crimes such as drug trafficking and possessing are considered by some as "victimless" crimes (Meier and Geis, 1997).[45] For the upper bound, we directly obtain the estimated costs for drug possession/sales from MCSAH. The upper bound for drug-related victimization costs is equal to \$5,387 in 2020 dollars. Because there is only one type of crime under the category of drug-related crimes, the upper bound for the expected benefit is $0.022 \times 27.34\% \times 1 \times 100\% \times \$5,387 = \$32$.[46]

Public Order Crimes

The final category of crimes is public order. C&P report the victimization cost for DUI but not other type of public order crimes. However, C&P report the willingness-to-pay (WTP) for averting "other offenses", which include specific types of crimes under the category of public order crimes, which is \$1,000. There are ten different types of crimes that fall under this category (Table A6).[47] To obtain a lower bound, we use the

[45] In Jácome (2020), drug crimes include driving under intoxication (DUI) as well as drug trafficking and possessing. In the NCRP data, however, DUI is included under the category of public order crimes.

[46] As mentioned in footnote 42, we assume $r^J = 1$ for drug-related crimes to obtain a conservative estimate for the expected benefit.

[47] The type of public order crimes include DUI, other impaired driving, carrying weapons, prostitution/pandering, gambling, liquor laws, drunkenness, disorderly conduct, vagrancy, curfew/loitering violations. We also obtain the number of crimes for each type of crime from Table 4 in MCSAH.

WTP measure as a proxy for victimization costs for the types of crimes that fall under public order crimes. For our upper bound estimate, we directly obtain victimization costs for public order crimes from MCSAH. Using the number of crimes, and thus, the share of each type of crime (DUI, prostitution, gambling, loitering, etc.) reported in Table A6, we estimate a weighted average of victimization costs for public order crimes.[48] We find that the lower and upper bound estimates are $4,992 and $13,804, respectively. After converting these estimates to 2020 dollars, the average victimization costs range from $6,230 to $14,577. Employing Equation (5), we obtain the expected benefit from averting public order crimes. The lower bound for the expected benefit is $26, and the upper bound is $60.

Total expected victimization cost reductions

As our last step, we calculate the total expected victimization cost reductions using Equation (6). Specifically, the lower bound is $14,100 + $808 + $0 + $26 = $14,934, and the upper bound is $20,464 + $1,006 + $32 + $60 = $21,562.

[48] For each crime type, the *share* is simply equal to the number of the specific type of crime divided by the total number of public order crimes. We report the number of crimes and shares for each type of crime in Table A6.

A4. Calculating the Victimization-to-Incarceration Ratios

Table A4. Victimization-to-Incarceration Ratios (r^J)

	1996		1997		1998	
Reoffense Type	Total	Inc.	Total	Inc.	Total	Inc.
Violent Crimes						
Murder	19.645	16	18.208	14.724	16.974	14.22
Rape/sexual assault	98	32	115	34	110	35
Robbery	757	59	607	61	610	60
Aggravated or simple assault	1,910	45	1,883	49	1,674	52
All Violent	2,784.65	152.03	2,623.21	157.56	2,410.97	160.27
Property Crimes						
Larceny	4,216	38	3,955	41	3,693	44
Burglary	4,056	71	3,893	73	3,380	75
Motor vehicle theft	938	18	1,007	18	822	18
All Property	9,967	187.09	9,462	192.84	8,505	196.56
	1999		**2000**		**2001**	
	Total	Inc.	Total	Inc.	Total	Inc.
Violent Crimes						
Murder	15.522	12.673	15.586	12.907	16.037	13.264
Rape	141	34	92.4	35	83.6	34
Robbery	530	56	520.1	55	426.7	53
Aggravated or simple assault	1,503	53	1,292.50	57	1,222.20	57
All Violent	2,189.52	155.67	1,920.59	160.91	1,748.54	156.13
Property Crimes						
Larceny	3,394	46	3,177	50	3,176	49
Burglary	3,064	73	2,909	74	2,687	71
Motor vehicle theft	808	17	641.9	18	724.1	18
All Property	7,796	191.87	7,248	197.69	7,013.80	189.92
Average Violent Ratio (r^J)			14.51			
Average Property Ratio (r^J)			43.25			

Notes: Total denotes the total number of victimizations (in thousands) reported the National Crime Victimization Survey (NCVS), excluding murder. These victimization numbers are also consistent with those reported in Heckman et al. (2010). We obtain the total number of victimizations for murder from the Supplemental Homicidal Report (SHR). The number of victimizations in SHR can be easily obtained from the Office of Juvenile Justice and Delinquency Prevention: https://www.ojjdp.gov/ojstatbb/ezashr/. Inc. denotes the number of sentenced prisoners (in thousands). We calculate the number of sentenced prisoners based on the distribution of sub-crimes in the NCRP and the state prison totals obtained from the National Prisoners Statistics. Average victimization-to-incarcerations ratios (r^J) for each crime category is calculated using the following formula: $\sum_t$ (All Category J Crime Total $)_t/$(All Category J Incarceration$)_t$, where $J \in$ {Violent, Property} and t = 1996, …, 2001. Since data on these ratios are not available for Drug and Public Order Crimes, we assume r^J =1 for these categories to obtain conservative estimates of the expected benefit from category J crimes.

A5. Obtaining the Within-Category Weights

Table A5. Within-Category Weights (w_i^J)

Reoffense Type	Percent	Ratio	Weight	Within-Category Weights (w_i^J, %)
Violent Crimes				
Murder	9.91	1.52	15.06	3.33
Rape	4.48	4.55	20.38	4.50
Robbery	37.24	5.94	221.21	48.83
Aggravated or simple assault	48.36	4.06	196.34	43.34
All Violent			452.99	100
Property Crimes				
Larceny	14.43	17.28	249.35	17.47
Burglary	65.33	15.94	1041.36	72.96
Motor vehicle theft	20.24	6.75	136.62	9.57
All Property			1426.33	100
Drug Crimes	-	100	-	100
Public Order Crimes	-	100	-	100

Notes: Within-category weights are calculated using the share of arrests for each sub-crime and the average victimization-to-arrest ratios from Jácome (2020). Percent denotes the share of arrests for each sub-crime category that end with a custody. Ratio denotes the victimization-to-arrest ratio. Weight = Percent × Ratio. Within-Category Weights (w_i^J) = Weight/ $\sum_i$Weight, where i refers to the type of crime (e.g., murder, rape, larceny, etc.) within crime category $J \in$ {Violent, Property, Drug, Public Order}.

A6. Obtaining the Victimization Costs for Public Order Crimes

Table A6. Victimization Costs for Public Order Crimes

Type of Crime	Number of Crimes (1)	Share of Crimes (2)	Cohen & Piquero (2009) (2007 dollars) (3)	Miller et al. (2021) (2017 dollars) (4)
DUI	321,681	14.78%	$28,000	$83,665
Other impaired driving	668,997	30.75%	$1,000	$1,195
Weapons carrying	164,984	7.58%	$1,000	$3,646
Prostitution/pandering	36,247	1.67%	$1,000	$365
Gambling	3,237	0.15%	$1,000	$365
Liquor laws	207,332	9.53%	$1,000	$1,740
Drunkenness	366,824	16.86%	$1,000	$1,740
Disorderly conduct	353,151	16.23	$1,000	$1,740
Vagrancy	23,321	1.07%	$1,000	$1,740
Curfew/loitering violations	30,131	1.38%	$1,000	$1,740
Total	2,175,905	100%		
Weighted average			$4,992	$13,804

Notes: The number of crimes are obtained from Miller et al. (2021). The victimization cost for DUI is from Table 5 in Cohen and Piquero (2009). All other costs in column (3) are willingness-to-pay estimates for averting these crimes, which are obtained from Table 5 in Cohen and Piquero (2009). The victimization costs in column (4) are obtained from Table 5 in Miller et al. (2021). In the analysis, we adjust victimization costs to 2020 dollars.

Table A7. Selected MVPF Estimates in the Social Insurance Domain

Policy Implementation	Year	Beneficiaries	MVPF	Sources
Medicaid Introduction to AFDC-eligible Families	1968	Adults, Children, Children under 5, Parents	∞	Goodman-Bacon (2021b)
Medicaid Expansion to Children Born after September 30, 1983	1990	Children, Children under 5	∞	Card and Shore-Sheppard (2004); Wherry and Meyer (2016); Wherry et al. (2018)
Medicaid Expansions to Pregnant Women & Infants	1986	Adults, Children, Children under 5, Mothers, Parents	∞	Currie and Gruber (1996); Dave et al. (2015); Wherry et al. (2018)
Medicaid Expansions to Young Children	1986	Children, Children under 5	∞	Brown, Kowalski, and Lurie (2020)
Medicaid Expansions to Released Inmates (as part of low-income adults)	2014	Adults, Released Inmates	3.45	Current Paper
Introduction of Medicare Part D	2006	Seniors	1.98	Wettstein (2020)
Medicaid Eligibility for Teenagers in South Carolina	2013	Children	1.77	Jácome (2020)
Medicare Introduction in 1965	1965	Adults, Seniors	1.63	Finkelstein and McKnight (2008)
Oregon Health Insurance Experiment (provided to single adults)	2008	Adults	1.16	Finkelstein et al. (2012); Finkelstein, Hendren, and Luttmer (2019)
Health Insurance Subsidies in Massachusetts to Individuals at 250% of the FPL	2011	Adults	1.09	Finkelstein, Hendren, and Shepard (2019)
Government Payments to Medicare Advantage Plans	2000	Seniors	1	Cabral, Geruso, and Mahoney (2018)
Health Insurance Subsidies in Massachusetts to Individuals at 200% of the FPL	2011	Adults	0.85	Finkelstein, Hendren, and Shepard (2019)
Health Insurance Subsidies in Massachusetts to Individuals at 150% of the FPL	2011	Adults	0.80	Finkelstein, Hendren, and Shepard (2019)
Medicaid Expansions to Low-Income Adults	2014	Adults	0.7	Shupe (2023)
Taxation of Medigap Policies	2002	Adults, Seniors	0.40	Cabral and Mahoney (2019)

Notes: This table presents selected MVPF estimates derived from the Policy Impacts Library in the domain of social insurance. Some of the referenced papers do not directly provide MVPF figures. In such instances, estimates from these papers are converted into their implied MVPFs by Hendren and Sprung-Keyser (2020) and are documented in the library, accessible at `https://policyimpacts.org/`. Our selection specifically targets studies that examine the impact of public health insurance, with a summary of the corresponding policy implementations provided in the first column. The MVPF estimates relate to the effects of these policies across various outcomes, with sources for these estimates cited in the final column.

Appendix B

Sensitivity of MVPF Estimates

B1. Adjusting Effect Sizes of Medicaid Expansions

In our benchmark analysis, we use the point estimate from our regression to obtain the MVPF estimates. To examine the sensitivity of MVPF estimates to the potential values of the effect size, we extend our analysis to include not only the discrete lower and upper bounds of the 95% confidence interval (CI) but also to obtain effect sizes across a continuous domain within these bounds. This approach allows us to construct estimates for the effect of Medicaid expansions across a broad range within the 95% CI. Specifically, this range is determined by ($\beta \ + \ t \times \textit{std. err.}$), allowing t to vary between -2 to 2. Here, β represents the coefficient of the treatment effect (-0.022), and *std. err.* denotes the corresponding standard error (0.011). The analysis employs our comprehensive analytical sample, which aggregates all types of crimes without distinction. We calculate the MVPF estimates using both our conservative and liberal methods. The estimates obtained through these two methods are depicted in Figure B1 in red and blue, respectively.

The MVPF estimates depicted in the figure exhibit notable variations as the parameter t transitions from 2 to -2. As we move toward the left tail of the t distribution, the effect size, defined as $-0.022 + 0.011 \times t$, amplifies within the 95% CI. This implies a more pronounced impact of the Medicaid expansions in curbing recidivism, thereby yielding a higher MVPF. A comparison between the conservative and liberal approaches reveals distinct trends: the MVPF derived from the conservative approach appears to ascend linearly, whereas the liberal MVPF shows exponential growth. A dashed black line in the figure marks the MVPF threshold of 1, indicating that values above this point yield benefits exceeding the cost of one dollar spent. Remarkably, barring extreme scenarios where exceptionally large values of t are considered within the range of -2 to 2, both methods yield MVPF estimates surpassing the threshold of 1. This suggests that, across a range of potential effects, the benefits of Medicaid expansions are likely to outweigh the costs.

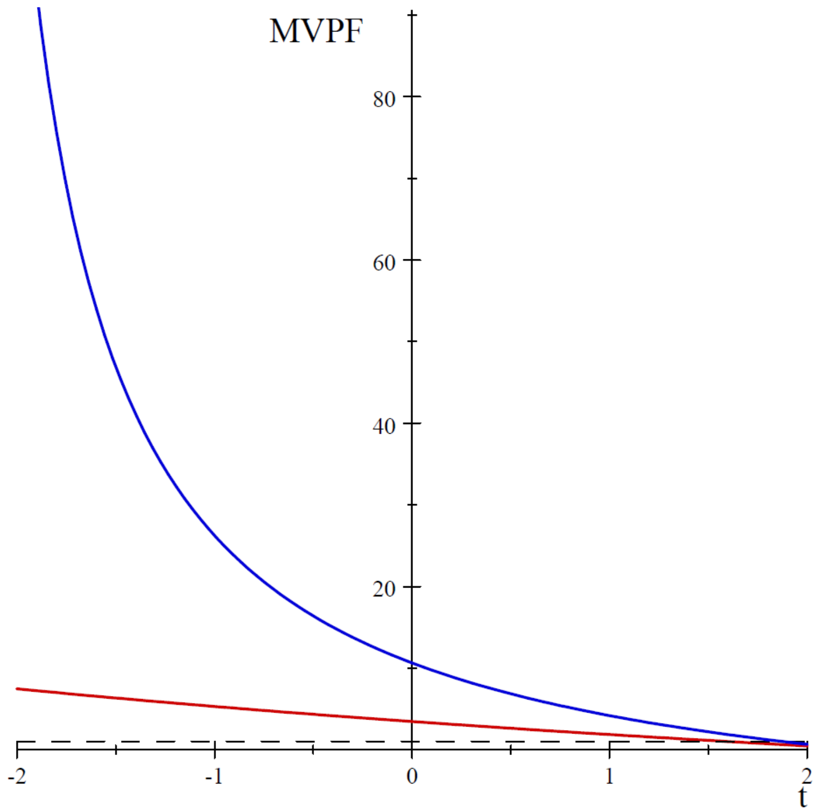


Figure B1. MVPF Estimates for Varying Effect Sizes of Medicaid Expansions

Notes: The red and blue curves represent the MVPF estimates calculated using the conservative and liberal approaches, respectively. On the horizontal axis, we continuously vary the t-statistics (t) between -2 and 2 to estimate varying effect sizes ($\beta + t \times \textit{std. err.}$) of Medicaid expansions within the 95% confidence interval. The black dashed line illustrates the threshold where MVPF equals 1, indicating that values above this threshold yield benefits exceeding the cost of one dollar spent.

B2. Adjusting Victimization-to-Incarceration Ratios for Drug and Public Order Crimes

Our initial sensitivity check focuses on *the willingness to pay for fewer crime victimization*. In our benchmark analysis, we assume victimization-to-incarceration ratios for drug and public order crimes to be 0 and 1, respectively. To test the sensitivity of our MVPF estimates to this assumption, we adjust the victimization-to-incarceration ratios for both drug and public order crimes to 0 and 1 for our conservative and liberal methods, keeping all other inputs constant. We present these four adjusted MVPF estimates in Figure B2, shown below. For reference, the benchmark MVPF estimates obtained using the conservative and liberal methods are also depicted in the figure as two vertical dashed lines, with the conservative estimate at 3.45 and the liberal at 10.62. Notably, the results indicate that modifications to the assumptions regarding the victimization-to-incarceration ratios for drug-related and public order crimes do not significantly affect the MVPF estimates.

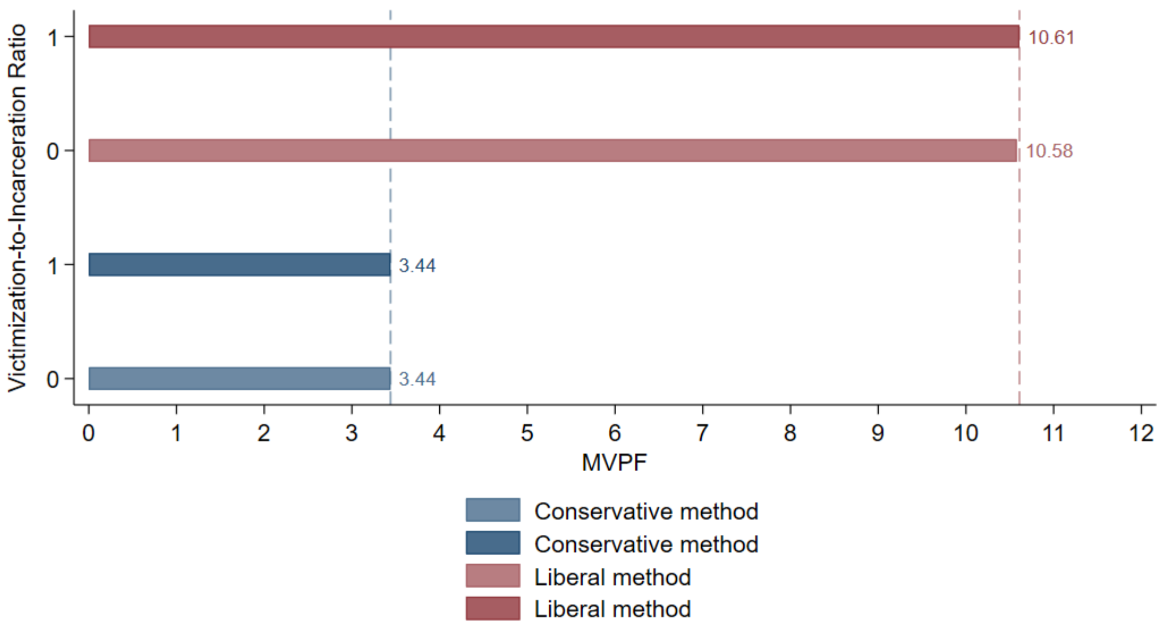


Figure B2. MVPF Estimates Using Various Victimization-to-Incarceration Ratios for Drug and Public Order Crimes

B3. Adjusting Employment Rates and Potential Income among Released Inmates

The second series of sensitivity checks focuses on *the willingness to pay for improved labor market prospects*. We adjust both employment rates and potential income among inmates, which are key inputs in estimating this measure. In our benchmark analysis, we assume an employment rate of 50.34% among released offenders, derived from analyzing American Community Survey (ACS) data between 2009 and 2013 for the low-income population. We now explore how MVPF estimates are affected by varying potential employment rates among released inmates. Specifically, we incrementally adjust the employment rate from 0% to 100% in 20 percentage point intervals, using these rates to calculate both conservative and liberal MVPF estimates. This results in six different MVPF estimates for each approach. We present these estimates in Figure B3, alongside the benchmark estimates depicted as dashed lines. Importantly, adjustments to the employment rates do not significantly impact the MVPF estimates.

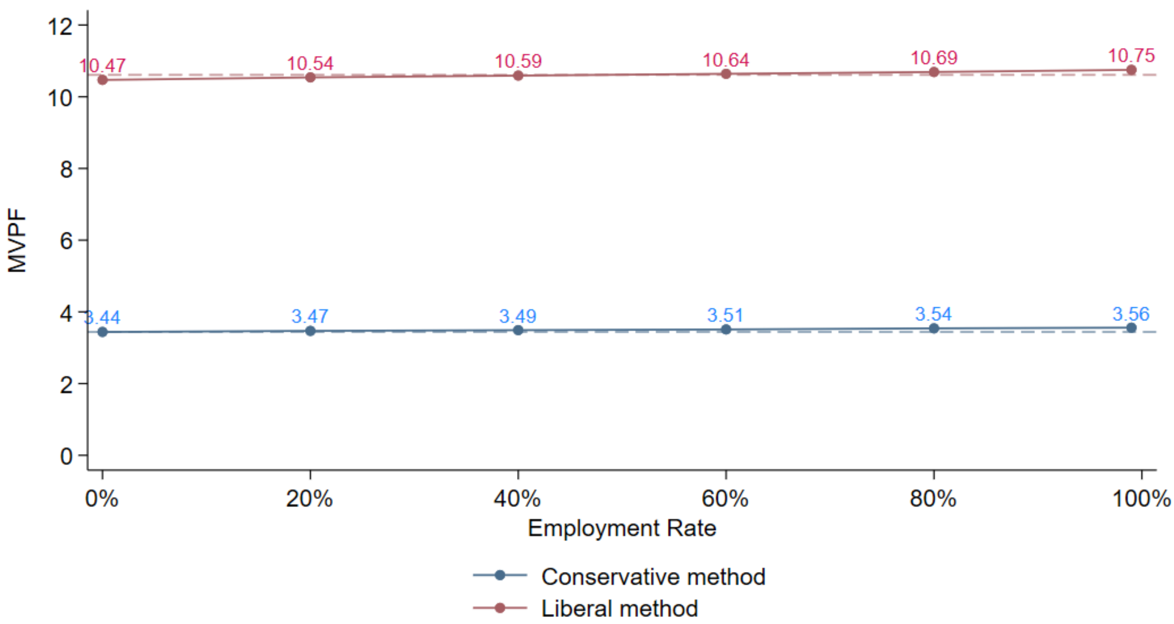


Figure B3. MVPF Estimates Using Various Employment Rates

Similarly, we adjust another crucial input for calculating the willingness to pay for improved labor market prospects: the potential income among released inmates. In our benchmark analysis, we use the average income among low-income individuals from ACS data between 2009 and 2013. For this analysis, we shift our focus to 2013 ACS data to examine the income distribution among low-income individuals. We narrow our data to childless individuals from households of one or two family members, with family income below 138% of the federal poverty line. Subsequently, we extract values at the 1%, 25%, 50%, 75%, and 99% percentiles of the income distribution as inputs for the potential income of released inmates. The MVPF estimates at different income levels, shown in Figure B4, pertain to our liberal approach.[49] The dashed line represents the benchmark estimate. Our findings highlight that adjustments to the income levels of released inmates have a negligible impact on MVPF estimates.

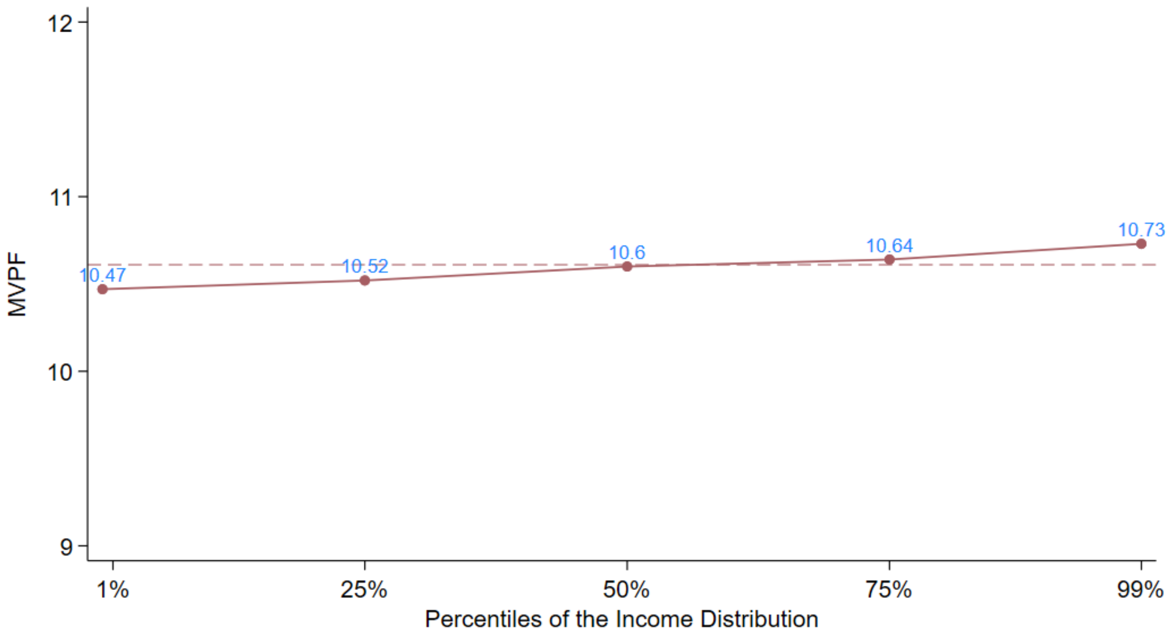


Figure B4. MVPF Estimates Using Income Levels at Different Percentiles

[49]Note that our benchmark estimates for the conservative case assume employment to be zero, eliminating the need to vary income levels for that scenario.

B4. Adjusting the Average Cost of Medicaid per Beneficiary

Next, we explore *the willingness to pay for the value of public insurance transfer* by adjusting the monetary costs associated with providing Medicaid to beneficiaries. In our benchmark analysis, we rely on the average cost of providing Medicaid per beneficiary, reported by the Centers for Medicare & Medicaid Services (CMS), which is $5,873. In our sensitivity analysis, we consider the average costs for Medicaid across five specific enrollee categories: newly eligible adults, children, individuals with disabilities, seniors, and all adults.[50] We then re-estimate the MVPF using these varied costs and present the results in Figure B5, with the benchmark estimates depicted as dashed lines. The red bars indicate the MVPF estimates obtained through the liberal method, which range from 1.82 to 76.29. These findings indicate that applying Medicaid costs for adult categories, including all adults and newly eligible adults, as well as for children, results in relatively high MVPF estimates, particularly for children. In contrast, the considerably higher costs associated with seniors and individuals with disabilities lead to lower MVPF estimates, which nonetheless exceed the threshold of 1.

Similar patterns emerge with the conservative method, where all MVPF estimates exceed the threshold of 1. Note that the cost of providing Medicaid strongly influences MVPF estimates. However, even under the most conservative estimates and the highest Medicaid costs, the benefits of providing Medicaid to released inmates outweigh the costs. It is imperative to highlight that since our study focuses on released inmates, extending these cost implications to other groups such as children or individuals with disabilities may not necessarily be appropriate. Nonetheless, to offer a comprehensive analysis, we have included MVPF estimates using Medicaid costs for all beneficiary categories.

[50] We source our data from CMS for the year 2016.

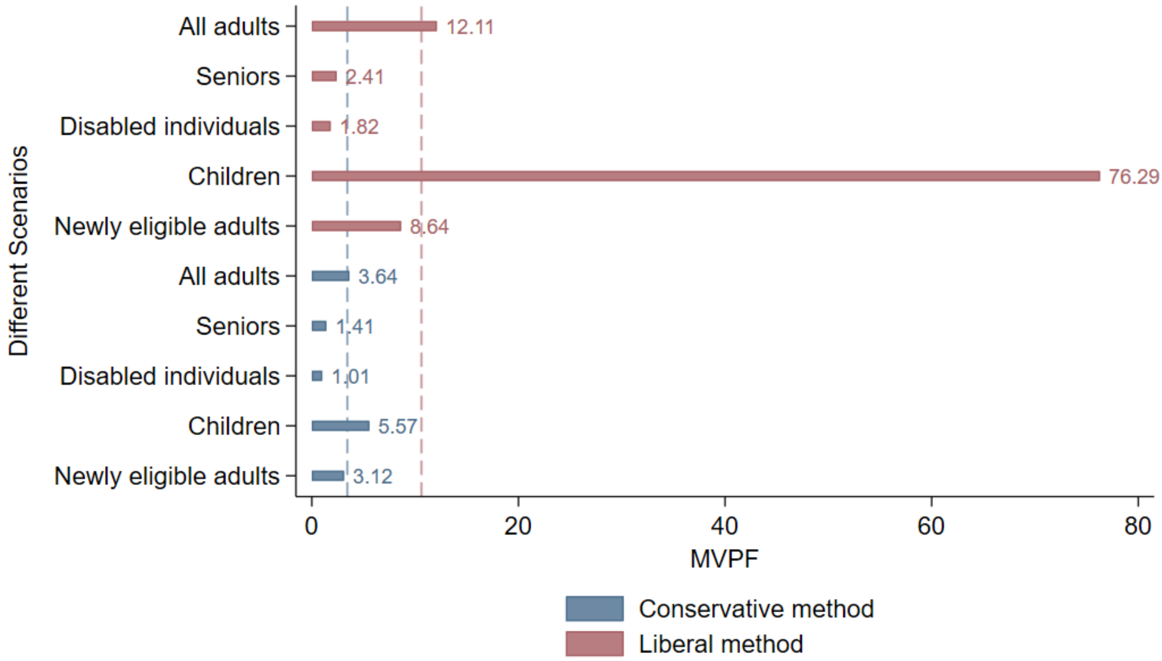


Figure B5. MVPF Estimates Using Various Medicaid Costs Across Enrollee Categories

B5. Adjusting the Incarceration Cost per Inmate

In our final set of sensitivity checks, we adjust the *cost per inmate for incarceration*. Initially, our benchmark analysis uses the national average cost per inmate. For the current analysis, we compile data on the average cost per inmate across 45 states.[51] We then analyze the distribution of these costs, extracting values at the 1%, 25%, 50%, 75%, and 99% percentiles of this distribution for MVPF calculations. The findings are depicted in Figure B6, with the benchmark estimates represented by dashed lines.[52] Figure B6 shows that MVPF estimates vary between 7.3 and 13.27 for cost values from the 1% and 75% percentiles of the distribution, closely aligning with the benchmark estimate of 10.61. However, adopting the highest incarceration cost nationally propels the MVPF estimate to 86.86. This demonstrates the substantial monetary benefit of

[51] We obtain the prison spending data from the Vera Institute of Justice's report titled "The Price of Prisons: Examining State Spending Trends, 2010-2015" (Mai and Subramanian, 2017).

[52] We apply this adjustment exclusively using the liberal approach because even the cost at the 1% percentile of the national distribution exceeds the conservative method's baseline costs. Thus, utilizing any of these values in the conservative approach would only increase the MVPF estimate, reinforcing our benchmark results.

providing Medicaid and reducing recidivism, even at the lowest incarceration costs. Nonetheless, we advocate using a cost figure from the 25th to 75th percentiles for more balanced estimation.

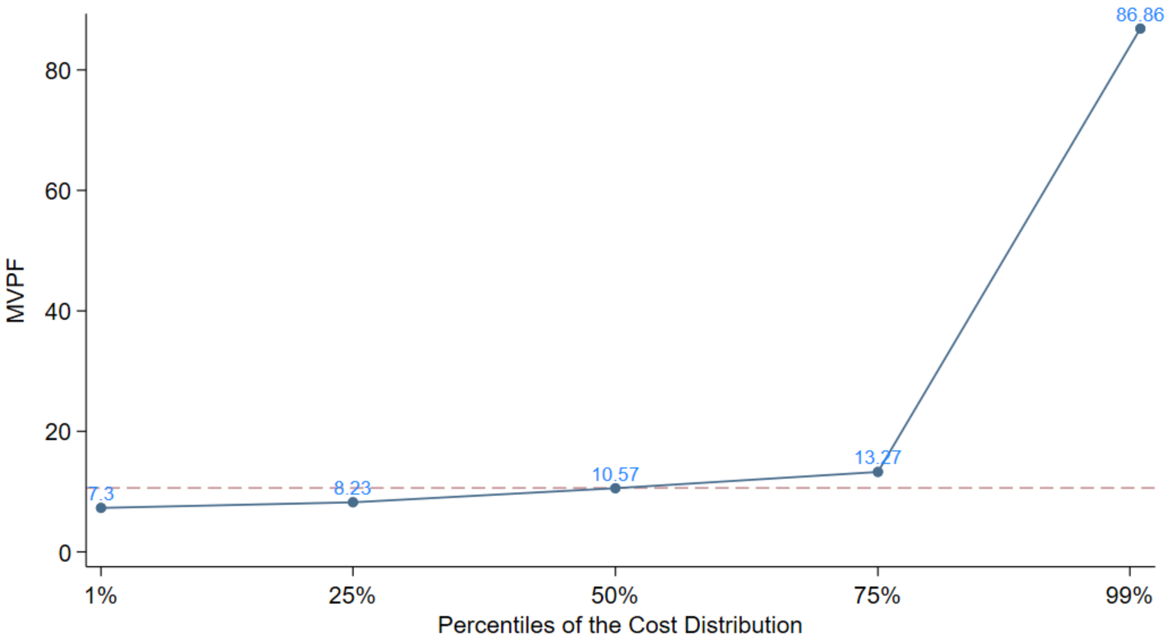


Figure B6. MVPF Estimates Using Incarceration Costs at Different Percentiles