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THE EFFECT OF MEDICAID ON CRIME:
EVIDENCE FROM THE OREGON HEALTH INSURANCE EXPERIMENT

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ABSTRACT

Those involved with the criminal justice system have disproportionately high rates of mental illness and substance-use disorders, prompting speculation that health insurance, by improving treatment of these conditions, could reduce crime. Using the 2008 Oregon Health Insurance Experiment, which randomly made some low-income adults eligible to apply for Medicaid, we find no statistically significant impact of Medicaid coverage on criminal charges or convictions. These null effects persist for high-risk subgroups, such as those with prior criminal cases and convictions or mental health conditions. In the full sample, our confidence intervals can rule out most quasi-experimental estimates of Medicaid's crime-reducing impact.

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A randomized controlled trials registry entry is available at
<https://www.nber.org/programs-projects/projects-and-centers/oregon-health-insurance-experiment/oregon-health-insurance-experiment-documents>

1 Introduction

Crime is a major social problem, with annual economic costs in the United States as high as \$3.92 trillion per year (Anderson, 2021). As a result, crime-prevention policies have the potential to generate large social benefits. Prevention is difficult, however, because involvement with the criminal justice system is often rooted in complex problems such as poverty, substance use disorder, and mental illness. In particular, those incarcerated have disproportionately high rates of mental health disorders; over 40% of prisoners and jail inmates report a diagnosis for mental illness (Bronson and Berzofsky, 2017; Maruschak et al., 2021b), nearly half of all prison inmates meet the criteria for substance use disorder, and about two-fifths report using illegal drugs at the time of their offense (Maruschak et al., 2021a).

These high rates of drug use and mental disorders have led some to conclude that health-related interventions could reduce recidivism and prevent crime from occurring in the first place. Expanding eligibility for publicly-funded health insurance coverage through Medicaid, which provides health insurance to low-income individuals, is one such policy that has attracted increasing support from policymakers and advocates as a way to effectively reduce crime. For example, in 2023 the U.S. Department of Health and Human Services introduced an option for states to begin Medicaid coverage for incarcerated individuals prior to their release in order to enhance "...their ability to succeed and thrive during reentry, thereby lowering the risk of recidivism, helping make our communities healthier and safer." Other examples of policymakers and advocacy groups highlighting the potential for Medicaid to reduce crime include the President's Council of Economic Advisers (Council of Economic Advisers, 2021), the Prison Policy Initiative (Widra, 2022), and the American Hospital Association (American Hospital Association, 2019). Consistent with this view, multiple, recent quasi-experimental studies have found that Medicaid is associated with decreases in criminal justice-related outcomes, particularly among high risk individuals such as those re-entering society from incarceration and young men with mental health needs. Indeed, a recent review paper stated that, while most public programs have little impact on violent crime, Medicaid has been shown to be "one of the few in-kind transfers that reduces violence" (Ludwig and Schnepel, 2024). However, as we discuss in more detail below, not all studies have found effects of Medicaid on criminal justice-related outcomes, and challenges related to data quality and identification suggest the value of more work on this topic.

This study therefore uses the 2008 Oregon Health Insurance Experiment to provide new evidence

on the impact of Medicaid coverage and crime. A state-run lottery randomly gave some low-income, uninsured adults but not others the ability to apply for Medicaid. Prior work has found that adults selected by the lottery were 25 percentage points more likely to enroll in Medicaid than adults who signed up for the lottery but were not selected (Finkelstein et al., 2012) and, using the lottery as an instrument for Medicaid coverage, documented the impact of Medicaid coverage on health care use, health, financial outcomes and voter participation (e.g., Baicker et al., 2013; Finkelstein et al., 2012; Taubman et al., 2014; Baicker and Finkelstein, 2019).¹ We take advantage of this random assignment to examine the Medicaid-crime relationship. To do so, we link all of the study participants to their individual-level administrative records from the Oregon Judicial Information Network (OJIN) on criminal cases, charges, and convictions from 2007 to 2010, providing what is, to our knowledge, the first experimental evidence on the relationship between Medicaid and crime.

In contrast to many recent quasi-experimental findings (e.g., Jácome, 2022; Burns and Dague, 2023; Aslim et al., 2022, 2024; Deza et al., 2024a; Vogler, 2020), we do not detect any statistically significant effect of Medicaid coverage on interaction with the criminal justice system. In the two years after the lottery, our point estimates indicate that Medicaid reduces the probability of having a criminal case or charge by a statistically insignificant 0.006 percentage points (standard error = 1.08) – about 0.05 percent relative to the control mean of 11.6 percent – and increases the probability of having a criminal conviction by a statistically insignificant 0.10 percentage points (standard error = 0.961) – about 1 percent relative to the control mean of 9 percent. However, the two-sided confidence intervals do include some modest but meaningful effects: we are able to rule out that Medicaid decreases the probability of having any criminal case or charge by more than 2.1 percentage points (18.3%) or decreases the probability of any criminal conviction by more than 1.8 percentage points (19.8%).

While direct comparisons inevitably involve challenges, our confidence intervals appear to rule out most of the statistically significant Medicaid-induced declines in criminal justice involvement estimated in prior, quasi-experimental studies. However, some of these studies (e.g., Aslim et al., 2022, 2024; Burns and Dague, 2023; Jácome, 2022) examine high-risk sub-populations, such as those returning to the community from incarceration, where the effect of Medicaid may be different.

¹ Abdul Latif Jameel Poverty Action Lab (J-PAL) (2023) provides an overview of the main findings to date.

2 Medicaid and Crime: Hypotheses and Existing Evidence

2.1 Hypothesized mechanisms

Possible channels by which Medicaid, by reducing out-of-pocket costs of medical care for low-income individuals, might reduce crime include: increasing mental health treatments, increasing treatment for substance-use disorders and reducing financial strain. There is considerable quasi-experimental evidence that Medicaid does all three (e.g., [Ortega, 2023](#); [Wen et al., 2017](#); [Argys et al., 2020](#); [Brevoort et al., 2020](#); [Hu et al., 2018](#); [Miller et al., 2021](#)). Likewise, evidence from the Oregon Health Insurance Experiment indicates that Medicaid improves multiple measures of mental health – including a 9 percentage point (30 percent) decline in depression rates – increases the propensity to take prescription drugs that treat mental disorders, and reduces financial strain – including decreasing both average and so-called ‘catastrophic’ out-of-pocket medical expenditures ([Finkelstein et al., 2012](#); [Baicker et al., 2017, 2013](#)).²

These mechanisms through which Medicaid might reduce crime are plausible. Mental illness can distort the perception of threats or result in maladaptive responses to interpersonal interactions, which may increase the propensity to commit crimes; it can also worsen labor market outcomes which could lead to instrumental crimes (such as theft) arising from economic need. Indeed, individuals with mental illness are substantially over-represented among those arrested and incarcerated ([Maruschak et al., 2021b](#); [Bronson and Berzofsky, 2017](#)), although naturally this correlation may reflect omitted factors (such as exposure to violence at a young age) that affect both mental illness and crime ([Frank and McGuire, 2011](#)). Nonetheless, policymakers have called for expanded treatment of mental illnesses as a way to reduce criminal activity (e.g., [Health and Human Services, 2024](#)) and there is evidence that access to such services can reduce crime ([Deza et al., 2024b](#)). Closely intertwined with mental illness is substance use disorder, which is also over-represented among arrested individuals ([Maruschak et al., 2021a](#); [FBI, 2019](#)). Drug use may directly increase rates of arrest, since obtaining or having drugs on one’s person may itself be illegal. Finally, financial strain appears to increase property crime ([Ludwig and Schnepel, 2024](#)); for example, property crimes tend to fall after the receipt of monthly welfare benefits ([Foley, 2011](#)), and losing eligibility for SSI increases rates of income-generating crimes ([Deshpande and Mueller-Smith, 2022](#)).

²Previous analysis of the experiment did not examine care for substance use disorder directly except for drugs related to opioid abuse treatment, which showed no significant difference across treatment arms ([Baicker et al., 2017](#)). However, participants randomized to receive Medicaid used significantly more outpatient care in general – including primary care, preventive care, and emergency room visits ([Finkelstein et al., 2012](#); [Baicker et al., 2013](#); [Taubman et al., 2014](#)).

2.2 Existing Evidence

Several existing studies use quasi-experimental variation in Medicaid eligibility to assess the impact of Medicaid coverage on crime. Many of these studies have found decreases in criminal justice-related outcomes associated with gaining Medicaid eligibility, with effects particularly pronounced for young men with mental health needs (Jácome, 2022) and those re-entering society from incarceration (Burns and Dague, 2023; Aslim et al., 2022, 2024). These findings are notable since, while other public welfare programs have been shown to decrease property crime (e.g., Deshpande and Mueller-Smith, 2022), expanding Medicaid eligibility is one of the few interventions that has appeared to decrease both property and violent crime. However, not all studies have found effects of Medicaid on criminal justice-related outcomes, and there are potential concerns with some of the data and empirical strategies used to date.

One set of papers relies on geographically aggregated data on crime reported to law enforcement agencies from the FBI's Uniform Crime Reports (UCR), combined with variation in Medicaid eligibility policy, to use difference-in-differences designs to investigate the relationship between Medicaid and criminal behavior. The results have been mixed, both within and across studies. Vogler (2020) uses state-level information to examine the impact of the Affordable Care Act (ACA) Medicaid expansions, finding statistically significant decrease in violent crime but not other types of crime, while He and Barkowski (2020), using the same data and a similar approach, find somewhat different results, namely inconsistent evidence of reductions in violent crime but reductions in some types of property crime such as motor vehicle theft. Using the same data aggregated to the county level, Wen et al. (2017) finds that pre-ACA Medicaid expansions led to lower rates of crime. Finally, Deza et al. (2024a) analyze the large disenrollment of Medicaid enrollees in Tennessee in 2005; they use variation in the policy's impact across counties based on their pre-policy Medicaid coverage rates and find that losing Medicaid coverage increased crime, particularly non-violent crime.³ These papers provide valuable suggestive evidence on the relationship between Medicaid policy and crime, but grapple with well-known reporting issues in the UCR data. In particular, since the UCR rely on voluntary reports from law enforcement agencies, they suffer from substantial missing data problems - in any given year about one third of county agencies report no crime to the UCR - with the rate of missing data varying across agencies and years (see, e.g., Maltz and Targonski, 2002; Vogler, 2020). As a result, researchers

³Analysis of non-Medicaid insurance coverage, such as the dependent coverage mandate which increased coverage for 22 to 25 year olds but not 27-29 year-olds also find that coverage reduces rates of arrested

often rely on partially imputed data (e.g., [Vogler, 2020](#); [Wen et al., 2017](#)), or restrict the analysis to a geographic subset where the data are well reported (e.g., [Deza et al., 2024a](#)).

Another set of papers overcomes the limitations of the aggregate UCR data by using individual-level data on criminal justice outcomes. With the exception of [Jácome \(2022\)](#), these papers all focus on the impact of providing Medicaid coverage to incarcerated individuals as they are released from prison, a population where we might expect effects to be particularly pronounced. Due the difficulties in linking individual Medicaid enrollment data to criminal justice outcomes, most of these studies focus on a single state and use within-state variation over time or across subgroups to identify Medicaid effects. Several of these papers engage in pre-post analyses without a control group, which is potentially concerning given the declining trend in incarceration rates over the past decade ([Bureau of Justice Statistics, 2023](#)). They also find different results. For example, [Burns and Dague \(2023\)](#) analyze two policies in Wisconsin that together increased Medicaid enrollment among recently incarcerated residents and find that recidivism rates fell after the policy changes. However, when [Packham and Slusky \(2022\)](#) examine a policy change in South Carolina that likewise increased Medicaid enrollment among newly-released inmates, their similar design finds no impact on recidivism. Two other papers have been able to exploit state-year variation in Medicaid expansions in a difference-in-differences design by examining national data on recidivism among released inmates ([Aslim et al., 2022, 2024](#)); [Aslim et al. \(2022\)](#) find that ACA expansions reduced in the probability of re-incarceration only among a subset of offenders (e.g., those with public order offenses), while [Aslim et al. \(2024\)](#) find broader reductions in the number of re-imprisonments. Finally, [Jácome \(2022\)](#) conducts a difference-in-differences analysis of the impact on young men of losing Medicaid eligibility at 19 (relative to a matched sample of young men not enrolled in Medicaid), and finds that Medicaid reduces the likelihood of incarceration among young men overall, with especially large effects for those with prior mental health problems. These studies provide valuable evidence on the Medicaid-incarceration relationship using high-quality, linked administrative records. However, their tendency to focus on narrow, high-risk subsets of the population, the presence of mixed results across studies, and the potential vulnerability of some of the quasi-experimental designs to bias in the presence of time- or age-specific concurrent shocks, suggests that more research is needed on this topic.

3 Empirical framework and data

3.1 The Oregon Health Insurance Experiment

In 2008, the state of Oregon used a random lottery to allocate 10,000 available enrollment spots in one of its Medicaid programs, Oregon Health Plan (OHP) Standard. This program covered non-elderly adults who were Oregon residents, U.S. citizens, uninsured for at least 6 months, had income below the Federal Poverty Level (FPL) and fewer than \$2000 in assets, and who were not already categorically eligible for Medicaid. Between January 28th and February 29th, 2008, the state allowed anyone to sign up for a list to be considered. About 75,000 individuals were placed on the list.

From this list, the state conducted eight random drawings between March and September of 2008. In total, the state randomly selected 35,169 individuals; they won the ability to apply for Medicaid within the next 45 days. However, not all those selected enrolled: about 40% did not complete the application that was mailed to them, and about half of those who completed the application were determined ineligible, primarily due to failure to meet the income requirement, which was based on the last quarter of income. Ultimately, about 30% of those selected by the lottery enrolled in Medicaid.

3.2 Data

The state provided researchers with data on all individuals on the lottery list and the drawing (if any) in which they were selected. These data included demographic characteristics at the time of sign up, as well as identifying information that allowed for linkages to external data sources. Using name, date of birth, and gender, we probabilistically matched individuals to data on criminal charges from the Oregon Judicial Information Network (OJIN). The OJIN contains information on judgment dockets and the official Register of Actions from all Oregon State Courts (trial and appellate) ([Oregon Judicial Department, 2020](#)) for cases filed between January 1, 2007 and December 31, 2010, and information on decisions rendered from January 1, 2007 through April 19, 2012,⁴ provided at the level of the criminal charge.

We use these data to analyze impacts of Medicaid on criminal charges, criminal cases, and convictions. The extensive margin of “any criminal case” and “any criminal charge” are always equal (an individual must have a criminal case to have a criminal charge), but the total number of criminal cases and the total number of criminal charges may differ because each case can have multiple charges. For

⁴Federal court cases, juvenile cases, adoption cases, mental health adjudications, and cases that fall under the Violence Against Women Act are not included.

example, an individual could be prosecuted simultaneously for a robbery, an assault, and a weapons offense (3 charges) that took place during the same criminal incident (1 case). We also categorize cases, charges and convictions based on the type of charge (felony, misdemeanor, parole violation, or unknown) and the type of crime (violent, controlled substance related, income-generating, or other). Crime types were determined by the research team prior to analyzing the data (see Appendix Tables A1-A4).

We also use information from administrative records on Medicaid enrollment and ED visits that had been previously linked to the individuals on the lottery list (Finkelstein et al., 2012; Taubman et al., 2014). We use the Medicaid data to study the first stage impact of lottery selection on Medicaid coverage, defined, as in prior work, as an indicator for enrollment in any Medicaid program after the lottery (specifically, between March 10, 2008 until July 15, 2010). Since existing research suggests that Medicaid may reduce crime by improving mental health care for those who need it (e.g., Jácome, 2022; Wen et al., 2017), we use the ED data to proxy for a pre-existing mental health condition with an indicator for having an ED visit prior to the lottery (between January 2007 and March 10, 2008) that included a diagnosis of a mental health disorder, including substance use disorders.⁵ These data are available for all 12 hospitals in the Portland area, so, following Taubman et al. (2014) we limit any heterogeneity analysis by pre-existing mental health condition to the approximately one-third of the sample in Portland-area zip codes.

Study period and summary statistics. We define our study period to cover all criminal cases in which the alleged incidents leading to a charge occurred between March 10, 2008 and July 15, 2010. The start date coincides with the date the first lottery applicant was notified of the lottery outcome; the end date precedes another major state health insurance offering (see Appendix A for more detail on the study time frame and weights). This 28-month observation period represents, on average, 25.1 months (standard deviation = 2.0 months) after individuals were notified of their selection and 23 months (standard deviation = 2.5 months) after insurance coverage was approved for those who are selected by and enroll in Medicaid. Given that several studies detect effects of Medicaid coverage on crime following one year or less (e.g. Burns and Dague, 2023; Jácome, 2022), we expect this follow-up period to be sufficient to detect similarly-sized effects. We define various pre-randomization measures

⁵We consider a visit related to mental health if the first three digits of any of the up to 10 listed ICD-9 diagnosis codes cover mental health conditions (i.e. fall between 290 and 319, inclusive), drug or alcohol poisoning (965, 967-970, 980), or contain an "E" code corresponding to accidental poisoning by drugs (E850-E859) or suicide or self-inflicted injury (E950-E959).

based on data from January 1 2007 to March 10, 2008; we use these as controls or for heterogeneity analysis.

Our study population has noticeably higher rates of engagement with the criminal justice system than the general adult population in Oregon (Appendix Table A5). For example, from January 1 2007 through July 15 2010 the number of cases per study participant in the control group is 0.38, more than three times the rate for all adults between the ages of 19 and 64 (0.138). However, the characteristics of cases—such as the number of charges per case, or their distribution among different types of charges (e.g., felony, misdemeanor) and crimes (e.g. violent, income-generating)—are similar in our control group and the Oregon adult population 19-64.

3.3 Empirical Approach

Our analyses were pre-specified in 2014.⁶ Subsequently, we made a small number of deviations from the pre-analysis plan to better speak to new contributions to the literature on Medicaid and crime that occurred after the pre-specification. We denote analyses that deviate from the pre-specified analysis plan with the symbol +. Our analytic approach follows prior analyses of the Oregon Health Insurance Experiment (e.g., [Finkelstein et al., 2012](#)).

Intent to Treat Impact of Lottery Selection. We compare outcomes for those randomly selected by the lottery (i.e., the “treatment” group) to those not selected (i.e., the “control” group) to estimate the intent-to-treat (ITT) effect of winning the lottery:

$$y_{ih} = \beta_0 + \beta_1 \text{LOTTERY}_h + X_{ih}\beta_2 + V_{ih}\beta_3 + \epsilon_{ih} \quad (1)$$

where i denotes an individual and h denotes a household; when individuals were signed up on the lottery list they could list interested household members. The state drew individual names from the lottery list but allowed all household members of a winning applicant to apply for Medicaid. Lottery_h is therefore an indicator for whether household h was selected by the lottery. Because the state’s selection procedure meant that individuals in a household in which more people signed up for the lottery had a higher chance of their household being selected, we include as controls X_{ih} indicators for the number of household members who signed up for the lottery. We also include controls V_{ih} that are not correlated with treatment probability but that may improve the statistical precision of our

⁶The pre-analysis plan is available at <https://www.nber.org/programs-projects/projects-and-centers/oregon-health-insurance-experiment/oregon-health-insurance-experiment-documents>.

results; for our baseline analysis we control for the number of charges a participant had for incidents between January 1, 2007 and March 9, 2008. We report robust standard errors clustered at the level of the household and, as in prior work on the Oregon experiment that uses data beyond the fall of 2009, we up-weight a portion of the study population to adjust for a new lottery for Medicaid that the state conducted beginning in the fall of 2009 (see, e.g., [Baicker et al. \(2013\)](#); [Finkelstein et al. \(2016\)](#) and [Appendix A](#) for more details).

Observable demographic characteristics of treatment and control participants are balanced once household size is accounted for (see [Appendix Table A6](#), which replicates prior balance results (e.g., [Finkelstein et al., 2012](#))). [Appendix Table A7](#) presents new balance tests for pre-randomization characteristics of about three dozen outcome measures. A few individual outcomes are unbalanced, as is expected given the large number of tests and the fact that we do not adjust inference to account for multiple hypothesis testing; when taken together, we cannot reject the null hypothesis that all treatment-control differences are equal to zero. The F-statistic associated with demographics is 1.659 (with associated p-value of 0.103) and with pre-treatment versions of the outcome variables is 0.650 (p-value 0.948). Considering both sets of variables together, the joint F-statistic is 0.787 (p-value 0.843).

Local Average Treatment Effect of Medicaid Coverage. In equation (1), the parameter β_1 gives the average difference in outcomes between those who were and were not selected for the lottery. However, as discussed above, not all those who were selected by the lottery ultimately enrolled in Medicaid. We therefore also estimate a local average treatment effect (LATE) parameter that shows the effect of Medicaid enrollment among those induced to apply for the lottery by scaling up the intent to treat estimates by the difference in Medicaid enrollment across those who were and were not selected by the lottery, i.e. the “first stage.” Specifically, the parameter of interest is the coefficient on $MEDICAID_{ih}$:

$$y_{ih} = \pi_0 + \pi_1 MEDICAID_{ih} + X_{ih}\pi_2 + V_{ih}\pi_3 + \nu_{ih}. \quad (2)$$

and we estimate π_1 using two-stage least squares, with the first stage given by:

$$MEDICAID_{ih} = \delta_0 + \delta_1 LOTTERY_h + X_{ih}\delta_2 + V_{ih}\delta_3 + \mu_{ih}. \quad (3)$$

The variable $LOTTERY_h$ is an instrument for Medicaid since it is correlated with Medicaid enrollment

and can be excluded from equation (2). Being selected by the lottery had a large and statistically significant effect on enrolling in Medicaid (e.g., [Finkelstein et al., 2012](#)); compared to those who were not selected, lottery winners were about 23.4 percentage points more likely to enroll in Medicaid over our study period. See Appendix Table [A8](#), which also shows that other measures of Medicaid enrollment over our study period were also significantly affected.

4 Results and Discussion

4.1 Results

Table [1](#) shows impacts on criminal cases and criminal charges. The top two rows show the effects overall, while subsequent rows show effects by type of charge (felony, misdemeanor, parole violation, or those of unknown penal code) and type of crime (violent, controlled substance related, income-generating, and other). The first three columns show effects on the probability that an individual had any criminal case or charge over the study period; the next three columns show effects on the number of cases or charges. For each outcome, we present the control group mean, the ITT estimate of the impact of winning the lottery, and the LATE estimate of the impact of Medicaid coverage.

We find no statistically significant effect on criminal cases or charges, and the point estimates are quite small. We estimate that Medicaid reduces the probability of having a criminal case or charge by 0.006 percentage points, or 0.05 percent of the control group mean of 11.63 percent; our 95 percent confidence interval rules out Medicaid decreasing the probability of having a charge or case by more than 2.1 percentage points (18.3% of the control group mean). Medicaid reduces the number of cases by about 0.005 cases per person, or 2.4% relative to the control group mean, and increases the number of charges by about 0.052, or 11.8% of the control group mean; our 95 percent confidence intervals rule out decreases as large as 0.058 fewer cases (27%) or 0.087 fewer charges (20%).

The next rows indicate no statistically significant impact of Medicaid on any specific type of charge. Our precision varies considerably across these categories. For example, we are able to rule out that Medicaid decreases the probability of having any felony by more than 11%, but can only rule out a decrease in the probability of any parole violation larger than 143%, in part because parole violations are very rare. Similarly, we find no significant effects on the number of charges by type and, while the estimates vary in precision, they are all close to zero.

The last four rows show no evidence of Medicaid reducing charges for any specific type of crime. We find that Medicaid *increased* the number of charges related to income-generating behavior by a

statistically significant 0.06 ($p=0.015$), but had no effect on the number of violent, controlled substance, or other charges. Given the large number of tests, and the fact that we do not specifically adjust for multiple hypothesis testing, this single significant result may be a false positive.

Table 2 examines the impact of Medicaid coverage on criminal convictions. Medicaid may affect convictions, even if it does not impact charges, if, for example, it frees up household resources that can be used to purchase access to legal expertise. However once again we do not detect any statistically significant effects. We can rule out an effect of Medicaid on any criminal convictions larger than about 1.8 percentage points (about 20% compared to the control group mean) and on the number of criminal convictions larger than 0.03 (about 14%). Mirroring our results for charges, we also do not find any statistically significant effect on convictions for particular charges or types of crime, except for an increase in the number of income-generating crimes that is significant at the 5% level.

Robustness. Our null findings persist under a range of alternative samples and specifications (see Appendix Tables A9 through A14). This includes limiting the analyses to alleged incidents that occurred by September 30, 2009, which is the end date used in some previous analyses of the experiment (e.g., [Finkelstein et al., 2012](#); [Taubman et al., 2014](#)), using fewer or more pre-period applicant characteristics as controls, and estimating non-linear specifications, specifically logistic regressions for binary outcome variables and negative binomial models for counts.

Heterogeneity analysis. Previous work has documented that Medicaid reduces criminal justice outcomes for particular groups, such as young men with a history of mental illness, and those re-entering society from prison (e.g., [Jácome, 2022](#); [Burns and Dague, 2023](#); [Aslim et al., 2024](#)). We might also expect the effects of Medicaid coverage to vary across other dimensions, such as age. Table 3 therefore examines the impact of Medicaid separately for men and women; those aged 50-64 and 19-49; those who did and did not request English language materials for the lottery; those with and without prior charges or convictions; and those in the ED sample who had at least one prior visit related to a mental health condition.⁷ We do not find any statistically significant effects for any sub-group for either criminal cases or convictions. Naturally our confidence intervals become substantially larger, especially when focusing on small, high-risk subpopulations such as those with pre-experiment charges or convictions.

⁷These subgroup analyses were pre-specified, with two exceptions: analyses by pre-experiment ED visit and convictions were added ex post in order to estimate effects on populations that were the focus of recent work on the impact of Medicaid on crime.

4.2 Comparison to prior estimates

A number of recent papers have examined the relationship between Medicaid and crime in quasi-experimental settings, and in many (but not all) cases, have found that Medicaid coverage reduces criminal justice involvement or crimes reported to the police. Here, we investigate how these estimates compare quantitatively to our experimental estimates from the Oregon Health Insurance Experiment. Overall, our impression from this exercise is that the impact of Medicaid on criminal justice outcomes is more modest in our setting than what has been found in high-risk sub-populations, suggesting that benefits in these populations may not extend to Medicaid beneficiaries more generally.

We face several challenges in making these comparisons. Differences in study population are perhaps the most important. On the one hand, our study covers a time period when rates of reported crime and incarceration were high relative to the late 2010s period analyzed in most other papers. On the other hand, our study population—low-income, non-elderly adults in Oregon—has less criminal justice involvement than some of the groups studied in the quasi-experimental literature, such as individuals re-entering society from prison ([Aslim et al., 2022, 2024](#); [Burns and Dague, 2023](#); [Packham and Slusky, 2022](#)).

The quasi-experimental literature examined somewhat different outcomes. [Burns and Dague \(2023\)](#), [Jácome \(2022\)](#), [Aslim et al. \(2022\)](#), [Aslim et al. \(2024\)](#) and [Packham and Slusky \(2022\)](#) examine incarceration, or re-incarceration, while [Vogler \(2020\)](#), [He and Barkowski \(2020\)](#), and [Deza et al. \(2024a\)](#) analyze the number of crimes reported to the police within a certain area or jurisdiction. In contrast, our analysis looks at individual-level charges, cases, and convictions. While a case and conviction are both necessary precedents to incarceration, not all cases become convictions and not all convictions result in incarceration. Likewise, not all reported crimes result in a corresponding charge.

Finally, two studies ([Jácome, 2022](#); [Deza et al., 2024a](#)) examine the impact of losing, rather than gaining, Medicaid coverage, which could generate different effects.

There are several other differences across studies, in addition to these innate differences in study population and measured outcomes, that we try to harmonize for purposes of comparison. First, the unit of time used to measure outcomes differs. We measure outcomes between March 2008 and July 2010, a period of, on average, 23 months following Medicaid enrollment. Other studies examine average incarceration or crime rates over different periods, e.g. over a 6-month period ([Burns and Dague, 2023](#)) or at quarterly or annual frequencies (e.g., [Jácome, 2022](#); [Vogler, 2020](#); [He and Barkowski, 2020](#)). To make estimates comparable across studies when examining level effects, we re-scale all

estimates so that they represent average annual impacts. Second, some of the existing papers only report the reduced form impact on crime from a change in Medicaid policy. For these papers, we either re-scale the reduced form estimates and its associated upper and lower ends of the 95 percent confidence interval by the provided first stage impact of the policy on Medicaid coverage, or, if such a first stage is not reported, we estimate our own first stage using public data.⁸ Appendix Section B provides further details on both of these adjustments, as well as the individual papers we examine.

Figure 1 plots the resulting comparisons. Estimates plotted in blue represent our estimates from the Oregon Health Insurance Experiment, while estimates from other settings are plotted in black. On the left side, panel (a) compares the level (percentage point or number of charges per capita) effects across studies, while on the right side, panel (b) expresses estimates as a percent of the baseline mean. Additional details and comparisons are provided in Appendix Tables A15 and A16.

Panel A compares our estimates to studies of the impact of Medicaid in the general adult population. The top two rows compare our estimated effect of Medicaid on the total number of charges (or total number of violent charges) to estimates of the impact of Medicaid on total reported crimes per capita (or total reported violent crimes per capita) from the Uniform Crime Reports. We estimate that Medicaid increases the number of charges by a statistically insignificant 0.027 charges per year (or 11.8%), with an associated 95 percent confidence interval ranging from -0.045 to 0.010 charges (or -19.6% or 43.2%), and decreases violent crime by a statistically insignificant -0.00039 charges per year (or -0.5%) with a confidence interval of -0.021 to 0.020 (or -28.7% to 27.6%). We found two quasi-experimental estimates of the impact of Medicaid on reported crime (one of which was statistically significant) and three (statistically significant) quasi-experimental estimates of the impact of Medicaid on violent crime. For total crime, we are able to reject the statistically significant estimate of the impact of Medicaid in both levels and percentage terms. For violent crime, we can only exclude the largest of the estimates (a decline of -0.15 charges per year) in level terms, but can exclude all of the estimated declines in percent terms.

Panel B compares our estimates to five quasi-experimental estimates of the effect of Medicaid in higher-risk subpopulations, specifically those re-entering society from prison (Aslim et al., 2022, 2024; Burns and Dague, 2023; Packham and Slusky, 2022) or young men from low-income areas (Jácome, 2022). Specifically, we compare our estimated effect of Medicaid on the annual probability of a conviction – a statistically insignificant 0.05 percentage points (95 percent confidence interval: -0.89pp to

⁸Note that in these cases the reported confidence intervals do not incorporate uncertainty about the size of the first stage, so they likely over-state the precision of the local average treatment effect of Medicaid.

0.99pp) – to four quasi-experimental estimates of the effect of Medicaid on the annual probability of re-incarceration for individuals re-entering society from prison. One, like us, also found a statistically insignificant positive impact of Medicaid. The three others estimated that Medicaid was associated with a statistically significant decline in annual incarceration rates of 1.7, 3.9, or 5.0 percentage points. Our 95 percent confidence intervals can rule out a reduction in conviction probabilities as large as all of these statistically significant incarceration declines.⁹ The right-hand panel shows that when we make these comparisons in percentage terms, our 95 percent confidence interval excludes two of the three papers that estimated significant declines. This reflects the lower baseline rate of criminal outcomes in our sample; our point estimate indicates a statistically insignificant 1.1% increase in the probability of having any conviction, but our 95 percent confidence interval includes an almost 20% decline. Finally, the last row compares our estimate of the impact of Medicaid on the annual *number* of convictions to [Aslim et al. \(2024\)](#)’s estimated impact on the annual number of re-incarcerations. We find a statistically insignificant Medicaid effect of 0.016 convictions (a 15.8% increase) with an associated confidence interval of -0.014 to 0.045 (or -14.1% to 45.7%). We are able to rule out the statistically significant estimate in [Aslim et al. \(2024\)](#) in both level and percent terms.

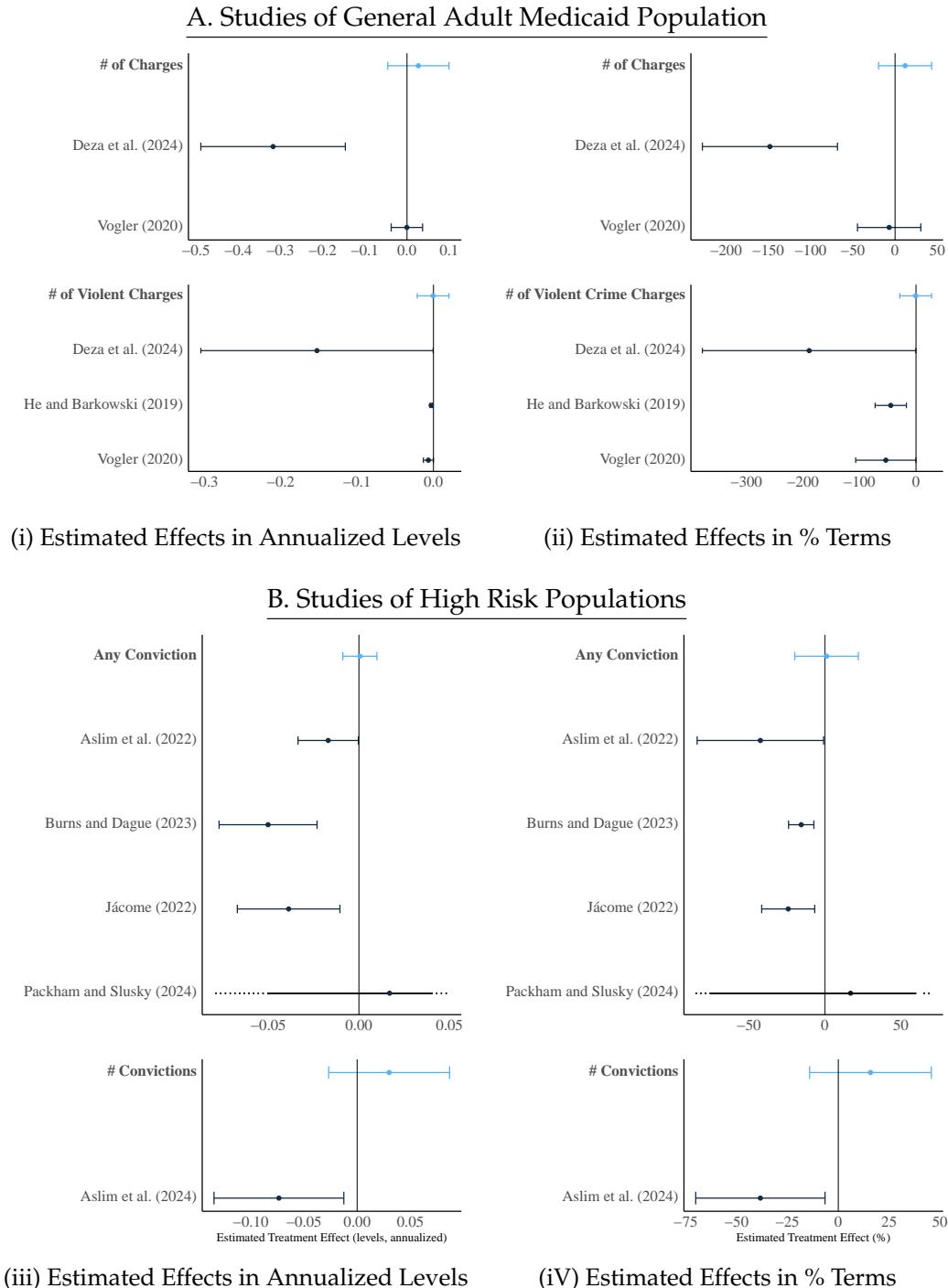
5 Conclusion

Medicaid eligibility and coverage have been linked to a number of positive outcomes, including better access to and use of medical care, improved mental and physical health, and increased economic security. In this paper, we investigate whether these beneficial effects of Medicaid may also help reduce or prevent criminal justice involvement. We take advantage of the unique setting of the Oregon Health Insurance Experiment in which some individuals were randomly selected for the opportunity to apply for Medicaid while others were not. We use this experimental variation as an instrument for Medicaid coverage and link to individual-level administrative records on criminal cases, charges, and convictions. We find no discernible effect of Medicaid coverage on criminal justice involvement, although our confidence intervals only rule out reductions in criminal justice involvement in the 10 to 20 percent range. We also find no impact of Medicaid for particular types of crimes and charges (such as violent crimes or misdemeanors) or particularly high risk subgroups (such as those with prior interactions with the criminal justice system or a history of mental health issues), although naturally

⁹When examining the most closely relevant subgroups in our population (i.e. the 6% of our sample with pre-treatment convictions or the 50% who are men), our confidence intervals increase substantially. Nevertheless, we are able to rule out effects for two of the three significant estimates in level terms (see last column of Appendix Table [A15](#)). However, we note that even these most relevant subgroups in our sample do not match very closely those studied in prior work.

our confidence intervals are wider for estimates derived from these smaller subsamples. Our findings suggest that the impact of Medicaid on criminal justice involvement may be concentrated among high-risk groups and not extend to the full population of Medicaid enrollees.

Figure 1: Comparison of Estimated Impacts of Medicaid from the Oregon experiment to Quasi-Experimental Analyses



Note: Figure compares impacts of Medicaid from the Oregon experiment (in blue) to estimates from quasi-experimental analyses (in black). We report estimated impacts of Medicaid in annualized level terms (left panels) and in percentage terms (right panels) on different outcomes. The confidence intervals for [Packham and Slusky \(2022\)](#) are truncated in panel for legibility; we estimate the intervals for this estimate to be [-249%, 283%]. See text and Appendix Section B for details.

Table 1: Effect of Medicaid Coverage on Criminal Cases

	Percent with any			Number		
	Control Mean	ITT	LATE	Control Mean	ITT	LATE
<i>Overall</i>						
Criminal Cases	11.63	-0.001 (0.254)	-0.006 (1.083)	0.21	-0.001 (0.006)	-0.005 (0.027)
Criminal Charges	11.63	-0.001 (0.254)	-0.006 (1.083)	0.44	0.012 (0.017)	0.052 (0.071)
<i>Charges by type of charge</i>						
Felony	5.06	0.213 (0.175)	0.908 (0.746)	0.15	0.010 (0.009)	0.041 (0.039)
Misdemeanor	9.73	0.067 (0.236)	0.286 (1.009)	0.17	-0.002 (0.006)	-0.009 (0.024)
Parole violations	0.38	-0.032 (0.049)	-0.135 (0.208)	0.01	-0.001 (0.001)	-0.005 (0.004)
Unknown penal code	0.53	-0.006 (0.056)	-0.026 (0.240)	0.01	-0.000 (0.001)	-0.001 (0.003)
<i>Charges by type of crime</i>						
Violent	3.22	-0.026 (0.144)	-0.110 (0.616)	0.07	-0.000 (0.005)	-0.001 (0.020)
Controlled substance	5.13	0.143 (0.177)	0.612 (0.756)	0.09	-0.002 (0.004)	-0.007 (0.017)
Income-generating	3.84	0.042 (0.152)	0.177 (0.649)	0.08	0.014 (0.006)	0.060 (0.025)
Other	7.72	0.101 (0.212)	0.429 (0.906)	0.22	-0.002 (0.010)	-0.007 (0.043)

Notes: Variables are measured from 10 March 2008 - 15 July 2010 (inclusive). All regressions include controls for household size and the total number of criminal cases an individual had prior to the lottery (1 January 2007 - 9 March 2008). All regressions include weights that account for the probability of being sampled in the new lottery, and adjust standard errors for household clusters. Penal code classifications are given in administrative data. Crime classifications were defined (prior to analyzing treatment-control differences) by the study group. For all outcomes, N=74922. ITT estimates are based on estimating equation 1; LATE estimates are based on estimating equations 2 and 3.

Table 2: Effect of Medicaid Coverage on Criminal Convictions

	Percent with any			Number		
	Control Mean	ITT	LATE	Control Mean	ITT	LATE
<i>Overall</i>						
Convictions	9.00 (0.225)	0.023 (0.961)	0.099	0.19 (0.007)	0.007 (0.029)	0.030
<i>Convictions by type of charge</i>						
Felony	3.72 (0.149)	0.109 (0.635)	0.464	0.06 (0.004)	0.006 (0.015)	0.024
Misdemeanor	6.86 (0.203)	0.074 (0.865)	0.317	0.13 (0.005)	0.003 (0.022)	0.012
Parole violations	0.19 (0.033)	-0.029 (0.142)	-0.123	0.00 (0.000)	-0.001 (0.002)	-0.003
Unknown penal code	0.24 (0.037)	-0.038 (0.156)	-0.163	0.00 (0.000)	-0.001 (0.002)	-0.003
<i>Convictions by type of crime</i>						
Violent	1.82 (0.107)	-0.034 (0.456)	-0.145	0.03 (0.002)	-0.000 (0.008)	-0.001
Controlled substance	3.51 (0.146)	0.036 (0.625)	0.156	0.05 (0.002)	-0.001 (0.010)	-0.005
Income-generating	2.70 (0.129)	0.125 (0.552)	0.536	0.04 (0.003)	0.006 (0.012)	0.027
Other	5.25 (0.179)	0.035 (0.763)	0.150	0.09 (0.004)	0.001 (0.017)	0.003

Notes: Variables are measured from 10 March 2008 - 15 July 2010 (inclusive). All regressions include controls for household size and the total number of criminal cases an individual had prior to the lottery (1 January 2007 - 9 March 2008). All regressions include weights that account for the probability of being sampled in the new lottery, and adjust standard errors for household clusters. Penal code classifications are given in administrative data. Crime classifications were defined (prior to analyzing treatment-control differences) by the study group. For all outcomes, N=74922. ITT estimates are based on estimating equation 1; LATE estimates are based on estimating equations 2 and 3.

Table 3: Effect of Medicaid Coverage on Criminal Cases and Convictions - Heterogeneity

		Criminal Cases						Criminal Convictions		
		N	First Stage	Percent with any Control Mean	Number Control Mean	LATE	Percent with any Control Mean	Number Control Mean	LATE	Percent with any Control Mean
Full Sample		74922	0.294	11.63	-0.006 (1.083)	0.21	-0.005 (0.027)	9.00 (0.961)	0.099 (0.961)	0.19 (0.029)
<i>By Gender</i>										
Men		33674	0.253	17.64	-1.136 (1.729)	0.34	-0.026 (0.048)	14.17 (0.025)	-1.779 (1.569)	0.32 (1.072)
Women		41248	0.221	6.90	0.399 (1.246)	0.11	0.013 (0.025)	4.93 (0.025)	1.346 (1.072)	0.09 (0.027)
<i>By Age</i>										
Age 50-64		20108	0.250	6.71	-1.308 (1.520)	0.11	-0.030 (0.036)	5.01 (0.035)	-1.545 (1.307)	0.09 (1.307)
Age 19-49		54814	0.229	13.45	0.466 (1.377)	0.25	0.005 (0.035)	10.48 (0.035)	0.726 (1.230)	0.23 (0.038)
<i>Requested English language lottery materials</i>										
Yes		68482	0.241	12.37	-0.069 (1.134)	0.22	-0.002 (0.028)	9.58 (0.028)	0.180 (1.008)	0.21 (0.031)
No		6440	0.160	2.97	0.025 (3.043)	0.05	-0.055 (0.052)	2.20 (0.052)	-1.897 (2.512)	0.04 (0.051)
<i>Pre-period charge</i>										
Yes		6166	0.281	40.75	4.325 (4.906)	0.96	0.108 (0.194)	34.62 (0.021)	4.866 (4.735)	0.90 (0.184)
No		68756	0.230	8.96	-0.709 (1.069)	0.14	-0.014 (0.021)	6.65 (0.021)	-0.591 (0.928)	0.13 (0.026)
<i>Pre-period conviction (+)</i>										
Yes		4762	0.283	43.18	4.607 (5.574)	1.02	0.242 (0.215)	38.10 (0.023)	3.569 (5.435)	0.99 (0.219)
No		70160	0.231	9.44	-0.682 (1.073)	0.15	-0.023 (0.023)	6.98 (0.933)	-0.429 (0.933)	0.14 (0.026)
<i>Pre-period mental health ED visit (+)</i>										
Full ED sample		24646	0.225	12.29	1.753 (1.969)	0.26	-0.018 (0.060)	8.39 (0.060)	2.001 (1.652)	0.20 (0.056)
At least one visit		2588	0.233	24.46	4.905 (7.594)	0.64	-0.150 (0.263)	18.32 (0.263)	2.017 (6.726)	0.53 (0.263)
No visits		22058	0.224	10.79	1.311 (1.995)	0.21	0.001 (0.059)	7.17 (0.059)	1.941 (1.655)	0.16 (0.054)

Notes: Variables are measured from 10 March 2008 - 15 July 2010 (inclusive). All regressions include controls for household size and the total number of criminal cases an individual had prior to the lottery (1 January 2007 - 9 March 2008). All regressions include weights that account for the probability of being sampled in the new lottery and adjust standard errors for household clusters. Penal code classifications are given in administrative data. Crime classifications were defined (prior to analyzing treatment-control differences) by the study group. LATE estimates are based on estimating equations 2 and 3. The symbol (+) denotes analyses that were not included in the original pre-analysis plan.

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The Effect of Medicaid on Crime: Evidence from the Oregon Health Insurance Experiment

Appendix

Amy Finkelstein Sarah Miller Katherine Baicker

A Study Time Frame and Analytical Weights

There are three relevant dates for measuring criminal justice outcomes: when the alleged incident occurred, when the case was filed, when the decision was rendered. Our data include all case filings between January 1, 2007 and December 31st, 2010, and information on decisions rendered from January 1, 2007 through April 19, 2012. We define our study period to include all criminal cases in which the alleged incidents leading to a charge occurred between March 10, 2008 and July 15, 2010. The start date coincides with the date the first lottery applicant was notified of the lottery outcome; the end date precedes another major state health insurance offering.

The fixed time frame over which we observe case filings and decisions is a potential limitation for our analysis. For example, if an alleged incident occurred between March 10, 2008 and July 15, 2010 (i.e., our study period), but the case was not filed until after December 31, 2010 (i.e., 169 days later), we would not observe the case in our data. However, we are reassured that of alleged incidents in the data that occurred in 2007, 88.7 percent of them are filed within 169 days. Similarly, if a case is filed during our study period but the associated decision is not rendered until after April 19, 2012 (i.e., 626 days later), we will not observe the decision on that case. This again appears to be rare; of all incidents occurring in 2007, 88.8% had a disposition within 626 days. A small percentage of cases cover multiple alleged incidents; in this case, we consider the date of the first alleged incident to define whether it falls during or before the treatment period.

The decision to end our study period on July 15, 2010 is due to the timing of another major state health insurance offering. In the fall of 2009, the state of Oregon was able to further expand enrollment in OHP Standard, and therefore conducted a second lottery. To conduct this lottery, the state mailed postcards to the original lottery applicants who were not selected (i.e., the control group) asking if they would like to be included in the second lottery. The state then selected a first round of new lottery winners from those who returned the post card, after which they opened the new waiting list up to the general public and conducted drawings approximately every month. We refer to control group

participants who returned the post card as “opt ins”—that is, they opted in to the new lottery—and those who were drawn in the new lottery as “selected opt-ins.” To facilitate the interpretation of our treatment effects, we drop control group participants who were selected opt-ins and use weights to correct for this sample change, following [Baicker et al. \(2013\)](#).

The set of opt-ins is not a random sample of our study population because signing up was optional, and may therefore be selected in terms of their observable and unobservable characteristics. However, the set of *selected* opt-ins is a random sample of the opt-ins. Within any (even non-random) subset of the original study population, a randomly selected group can be weighted to stand in for the non-selected remainder based on the probability of that random selection (similar conceptually to, e.g., [Cole and Hernán \(2008\)](#) and [Kalton and Anderson \(1986\)](#)). We therefore weight each observation at the time of each of the second lottery drawings by the inverse probability of being in the sample, and we generate overall weights as the product of the weights across all time points. Weights are thus:

$$w_t = \begin{cases} \frac{1}{1-p_t} & \text{if in } O_t \text{ but not } S_t \\ 0 & \text{if in } S_t \\ 1 & \text{if not } O_t \end{cases}$$

where O_t denotes the opt in group that applied to and was eligible for the lottery on date t , S_t denotes the sub-set of that group that was selected in the lottery on date t , and p_t is the probability of selection on that date. The final analytic weight W is the product all the weights w_t introduced up to July 15, 2010, the end of our study period. July 15, 2010 was the last day before individuals were notified of selection in the first of a series of very large lottery draws that would have generated very large weights.

Appendix Table [A17](#) shows descriptive statistics about the average analytic weights applied to the analysis, both overall (top row), and among those with non-zero weights (next 3 rows). In general, weights are close to 1, with even the 95th percentile of weights only about 1.6.

B Comparison to other estimates

In this section we attempt, where feasible, to compare our estimates to other papers that have evaluated the impact of quasi-experimental changes in Medicaid eligibility—either due to policy changes or random variation embedded within existing rules—on various measures of criminal justice activity. In some cases, we have undertaken additional analyses in order to improve comparability, such as estimating the first stage impact on Medicaid coverage in papers that did not provide it, converting estimates from levels to percentage impacts, and converting our estimates and those in other papers to annualized estimates. We provide comparisons both as percent of the population’s baseline mean and in level terms (e.g., as percentage points or number of crimes per capita). This section describes these additional analyses and provides more details on our comparisons.

Estimating first stages. We sought to compare LATE estimates of the impact of Medicaid coverage across papers. However, some papers reported only the reduced form change in the outcome and not the LATE. In these cases, we attempted to estimate the LATE as best as possible as follows:

- [Vogler \(2020\)](#) show the change in reported crime in states that did and did not expand Medicaid under the ACA, before and after the expansion; it also shows the change in insurance coverage due to the expansion in Appendix Table A1. However, the effect of the expansions on Medicaid coverage specifically may differ from those on insurance coverage more generally (e.g., if there is crowd-out of private insurance). Similarly, [He and Barkowski \(2020\)](#) do not report the effect of the expansions on coverage in the population. To generate an analogous estimate of the effect of the expansion on Medicaid coverage specifically, we use data from the 2010 to 2018 waves of the American Community Survey and estimate the following two-way fixed effects specification

$$Medicaid_{ist} = \beta_0 + \beta_1 Post_t \times Treated_s + \beta_t + \beta_s + \epsilon_{ist} \quad (4)$$

where our outcome *Medicaid* equals 1 if the respondent reports having Medicaid coverage and $Post_t \times Treated_s$ equals 1 for states who adopted the ACA Medicaid eligibility expansions in the years after the expansion was implemented, and 0 otherwise. We also include state and year fixed effects, β_t and β_s . We weight the regression by the survey weights and report cluster-robust the standard errors clustered at the state level. Our measure of the first stage is β_1 , which we report in Appendix Table [A18](#). We find a change in Medicaid coverage in the population

of about 3.2 percentage points. This is comparable to other estimates of the effect of the ACA Medicaid expansions on Medicaid enrollment in the full population (e.g., [Courtemanche et al., 2017](#), find an effect of 3.1 percentage points). We therefore scale the reduced form estimates reported in both [Vogler \(2020\)](#) and [He and Barkowski \(2020\)](#) by dividing the coefficient and upper and lower points of the 95 percent confidence intervals by 0.032 to provide a back-of-the-envelope measure of the implied local average treatment effect and its 95 percent confidence interval. Note that we likely over-state the precision of the local average treatment effect in this exercise because we are not incorporating uncertainty about the estimate of the first stage ([Inoue and Solon, 2010](#)).

- [Aslim et al. \(2022\)](#) and [Aslim et al. \(2024\)](#) report the reduced form change in re-incarceration or number of re-imprisonments for those leaving prison following an ACA Medicaid expansion. The authors do not report the “overall” first stage—that is, the change in Medicaid enrollment among those leaving incarceration resulting from the ACA Medicaid expansions—although [Aslim et al. \(2022\)](#) does report that the payor for substance use disorder treatments was more likely to be Medicaid after the expansions. For the purpose of comparison, we attempt to generate a more direct first stage estimate ourselves. We use publicly available data from the 2023 vintage of the Justice Outcomes Explorer (<https://joe.cjars.org/>) based on data from the Criminal Justice Administrative Records system, CJARS, for the years 2010 to 2018. These data have Medicaid enrollment rates for cohorts returning to society from incarceration for 17 states with differing years available. We again estimate model (4) using these CJARS data, weighting the regressions by the size of the cohort in each state/year and reporting cluster-robust standard errors clustered by state. We report the first stage for this population in the second column of Appendix Table [A18](#). We find that those returning from prison experienced a very large increase in Medicaid coverage as a result of the expansions of about 35.3 percentage points. We use this first stage to re-scale the reduced form estimate in [Aslim et al. \(2022\)](#) as well as the upper and lower ends of that estimate’s confidence intervals to generate a LATE.
- [Packham and Slusky \(2022\)](#) provide both the first stage (Table 2, Column 1) and reduced form effect of a Medicaid policy change in South Carolina on 1-year recidivism (Table 3, Column 1) estimates but do not themselves estimate a LATE. We provide a back of the envelope calculation of the LATE in their setting by dividing their reduced form estimate (and the upper and lower

points of its confidence interval) by the point estimate in their provided first stage.

Scaling LATE estimates to be in percent terms. The populations and settings differ between our estimated effects of Medicaid coverage and other analysis using quasi-experimental variation, and as a result, mean rates of involvement with the criminal justice system also vary. Therefore, although the papers tend to estimate impacts of Medicaid in levels, we also report LATE estimates as a percentage of the relevant baseline mean. Some papers analyze the effect of the ACA Medicaid expansions on total reported crime in an area per capita. Scaling these estimates by the area-level mean crime rate per capita seems undesirable, since those who gained Medicaid coverage through the expansions—i.e., the “compliers”—may have substantially higher rates of engaging in criminal activity than the population as a whole. Indeed, in the Oregon setting, we found that those who applied for the lottery had about 3.1 ($=0.77/0.248$) times more charges per capita as the general population of Oregon (Table A5). For studies that report only the area average mean (Deza et al., 2024a; He and Barkowski, 2020; Vogler, 2020), we therefore scale up average number of crimes per capita in the area population by multiplying by 3.1, and compare estimated effects to this higher mean. For studies examining criminal justice specific populations—such as residents returning from incarceration—we do not apply this adjustment.

Identifying similar populations. Papers estimating the effect of Medicaid on criminal justice outcomes have focused on a variety of different specific populations. Aslim et al. (2022), Burns and Dague (2023), and Packham and Slusky (2022), Aslim et al. (2024) examine the impact of Medicaid coverage on cohorts re-entering society from incarceration. While we focus on estimates in our full sample (primarily for reasons of precision and also generalizability), we try where feasible to report results for somewhat more comparable sub-populations. The most comparable sub-population available in our data are participants who have a pre-treatment record of a conviction, although of course not all convictions result in incarceration, so this subgroup is not fully analogous. Likewise, Jácome (2022) examines the effect on young men so we also compare her estimates to our estimated impact on men.

Determining comparable outcomes. Our study also differs from previous studies in terms of outcomes.

- Aslim et al. (2022), Burns and Dague (2023), Packham and Slusky (2022) examine rates of incarceration. We compare their estimates impacts on incarceration to our estimates impacts on convictions, although not all convictions lead to an incarceration. Likewise, Aslim et al. (2024)

analyze number of re-imprisonments; we compare this to our estimates on number of convictions.

- [Vogler \(2020\)](#) and [Deza et al. \(2024a\)](#) look at per capita rates of crimes reported in an area (e.g., county or state).¹⁰ Our most comparable outcome is number of charges, although not all crimes result in a criminal charge (for example, some remain unsolved), so once again these outcomes are not fully comparable.
- [He and Barkowski \(2020\)](#) and [Vogler \(2020\)](#) consider impacts of Medicaid on rates of violent crimes reported in an area. To compare to these outcomes, we examine our estimates for the impact of Medicaid on the number of charges for violent crimes.

Harmonizing frequencies. Papers examine variables measured at a variety of different frequency—for example, annual, quarterly, or half-year. Our main estimates look at the number or rate of outcomes over a period of about 2 years. In contrast, [Vogler \(2020\)](#); [Deza et al. \(2024a\)](#); [He and Barkowski \(2020\)](#); [Aslim et al. \(2022\)](#); [Packham and Slusky \(2022\)](#) all analyze annual rates of reported crime or incarceration, [Jácome \(2022\)](#) analyzes quarterly incarceration rates, and [Burns and Dague \(2023\)](#) analyze the probability of incarceration over a 6-month period. In order to facilitate comparisons across these estimates, we attempt to make all of these estimates annual when comparing level differences (e.g., differences in incarceration probabilities or number of crimes per capita) across studies, as in Figure 1 panel (a) and Appendix Table [A15](#). We do so in the following way:

- For outcomes that measure the probability an individual is incarcerated, we treat each measured event within the year as independent and use the estimate to construct the probability that the individual will be incarcerated at all during the year. For example, we take the 6-month probability of incarceration reported in [Burns and Dague \(2023\)](#) (p_{6mo} , reported to be 0.0254 in Table 2) and convert it to an annual probability by applying the following formula: $p_{ann} = 2 \times p_{6mo} - p_{6mo}^2$. We similarly convert the quarterly incarceration estimate reported in [Jácome \(2022\)](#). We apply this equation to the associated standard errors to generate confidence intervals.
- We use the same logic to convert the effect of Medicaid on the probability that an individual is convicted (Table 2) from a two-year to a one-year measure, by solving the equation $p_{2yrs} =$

¹⁰[Vogler \(2020\)](#) and [He and Barkowski \(2020\)](#) estimate the effect of Medicaid expansions on the log of the crime rate. Following their discussion of the magnitudes, we interpret the coefficient as an approximate percent effect, and return to level effects by multiplying this coefficient by the sample mean.

$2 \times p_{1yr} - p_{1yr}^2$ for p_{1yr} and reporting this one year estimate of the probability of conviction.

- For outcomes that are not probabilities, such as number of charges, crimes, or convictions, we simply divide the estimate by the appropriate frequency to arrive at an annual measure.

Table A1: Crimes classified as violent

Law Description	Statute Number
Aggravated Murder	163.095
Murder	163.115
Manslaughter – first degree	163.118
Manslaughter – second degree	163.125
Aggravated vehicular homicide	163.149
Rape – first degree	163.375
Sodomy – first degree	163.405
Unlawful sexual penetration – first degree	163.411
Robbery – first degree	164.415
Robbery – second degree	164.405
Robbery – third degree	164.395
Burglary – first degree	164.225
Assault – first degree	163.185
Assault – second degree	163.175
Assault – third degree	163.165
Assault – fourth degree	163.16
Kidnapping – first degree	163.235
Kidnapping – second degree	163.225
Arson – first degree	164.325
Sexual abuse – first degree	163.427
Sexual abuse – second degree	163.425
Sexual abuse – third degree	163.415
Subjecting another person to involuntary servitude – first degree	163.264
Subjecting another person to involuntary servitude – second degree	163.263
Trafficking in persons	163.266
Escape – first degree	162.165
Custodial sexual misconduct – first degree	163.452
Custodial sexual misconduct – second degree	163.454
Aggravated harassment	166.07
Intimidation – first degree	166.165
Criminal mistreatment – first degree	163.205
Criminal mistreatment – second degree	163.2
Assaulting a public safety officer	163.208
Unlawful use of an electrical stun gun, tear gas or mace – first degree	163.213
Criminally negligent homicide	163.145
Recklessly endangering another person	163.195
Riot	166.015
Strangulation	163.187
Vehicular assault of bicyclist or pedestrian	811.06
Menacing	163.19

Notes: Table shows list of offenses classified as "violent" for analysis purposes by the Oregon Health Study Group. Column 1 gives the description of the law, and Column 2 gives the statute number, or "orsno." Full descriptions of each offense are available at: <http://www.leg.state.or.us/ors/> or <http://www.oregonlaws.org/>.

Table A2: Crimes classified as related to controlled substances

Law Description	Statute Number
Unlawful manufacture of heroin within 1,000 feet of school	475.848
Unlawful manufacture of heroin	475.846
Unlawful delivery of heroin within 1,000 feet of school	475.852
Unlawful delivery of heroin	475.85
Unlawful possession of heroin	475.854
Unlawful manufacture of methamphetamine within 1,000 feet of school	475.888
Unlawful manufacture of methamphetamine	475.886
Unlawful delivery of methamphetamine within 1,000 feet of school	475.892
Unlawful delivery of methamphetamine	475.89
Unlawful possession of methamphetamine	475.894
Unlawful manufacture of 3,4-methylenedioxymethamphetamine within 1,000 feet of school	475.868
Unlawful manufacture of 3,4-methylenedioxymethamphetamine	475.866
Unlawful delivery of 3,4-methylenedioxymethamphetamine within 1,000 feet of school	475.872
Unlawful delivery of 3,4-methylenedioxymethamphetamine	475.87
Unlawful possession of 3,4-methylenedioxymethamphetamine	475.874
Unlawful manufacture of cocaine within 1,000 feet of school	475.878
Unlawful manufacture of cocaine	475.876
Unlawful delivery of cocaine within 1,000 feet of school	475.882
Unlawful delivery of cocaine	475.88
Unlawful possession of cocaine	475.884
Unlawful manufacture or delivery of controlled substance within 1,000 feet of school	475.904
Possessing or disposing of methamphetamine manufacturing waste	475.977
Unlawful manufacture of marijuana within 1,000 feet of school	475.858
Unlawful manufacture of marijuana	475.856
Unlawful delivery of marijuana within 1,000 feet of school	475.862
Unlawful delivery of marijuana	475.86
Unlawful possession of marijuana	475.864
Use of minor in controlled substance offense	167.262
Unlawful delivery to minors	475.906
Unlawful possession of inhalants	167.808
Unlawful possession of iodine in its elemental form	475.975
Unlawful possession of anhydrous ammonia	475.971
Unlawful possession of phosphorus	475.969
Unlawful possession of lithium metal or sodium metal	475.979
Driving under the influence of intoxicants	813.01
Operating boat while under influence of intoxicating liquor or controlled substance	830.325
Manufacture, fermentation or possession of mash, wort or wash	471.44
Prohibited sales, purchases, possession, transportation, importation or solicitation of alcoholic beverages	471.405
Purchase or possession of alcoholic beverages by person under 21	471.43
Violation of open container law	811.17
Alcohol on public property	Missing
Acquiring a controlled substance by fraud	Missing

Notes: Table shows list of offenses classified as "involving controlled substances" for analysis purposes by the Oregon Health Study Group. Column 1 gives the description of the law, and Column 2 gives the statute number, or "orsno." Full descriptions of each offense are available at: <http://www.leg.state.or.us/ors/> or <http://www.oregonlaws.org/>

Table A3: Crimes classified as income-generating

Law Description	Statute Number
Burglary – first degree	164.225
Burglary – second degree	164.215
Robbery – first degree	164.415
Robbery – second degree	164.405
Robbery – third degree	164.395
Buying or selling a person under 18 years of age	163.537
Trafficking in persons	163.266
Aggravated theft – first degree	164.057
Theft – first degree	164.055
Theft – second degree	164.045
Theft – third degree	164.043
Theft by extortion	164.075
Theft by deception	164.085
Theft by receiving	164.095
Theft of services	164.125
Theft of lost, mislaid property	164.065
Organized retail theft	164.098
Laundering a monetary instrument	164.17
Trademark counterfeiting – first degree	647.15
Trademark counterfeiting – second degree	647.145
Trademark counterfeiting – third degree	647.14
Promoting prostitution	165.013
Prostitution	167.007
Loitering to solicit prostitution	142.405
Forgery – first degree	165.013
Forgery – second degree	165.007
Trafficking in stolen vehicles	819.31
Possession of a stolen vehicle	819.3
Trafficking in vehicles with destroyed or altered identification numbers	819.43
Criminal possession of a rented or leased motor vehicle	164.138
Forging, altering or unlawfully producing or using title or registration	803.23
Fraudulent use of a credit card	165.055
Sale of Unregistered Securities	Missing
Securities Fraud	Missing
Prohibited sales, purchases, possession, transportation, importation or solicitation of alcoholic beverages	471.405
Unlawful manufacture of heroin within 1,000 feet of school	475.848
Unlawful manufacture of heroin	475.846
Unlawful delivery of heroin within 1,000 feet of school	475.852
Unlawful delivery of heroin	475.85

Notes: Table shows list of offenses classified as "violent" for analysis purposes by the Oregon Health Study Group. Column 1 gives the description of the law, and Column 2 gives the statute number, or "orsno." Full descriptions of each offense are available at: <http://www.leg.state.or.us/ors/> or <http://www.oregonlaws.org/>

Table A4: Crimes classified as income-generating (cont.)

Law Description	Statute Number
Unlawful manufacture of methamphetamine within 1,000 feet of school	475.888
Unlawful manufacture of methamphetamine	475.886
Unlawful delivery of methamphetamine within 1,000 feet of school	475.892
Unlawful delivery of methamphetamine	475.89
Unlawful manufacture of 3,4-methylenedioxymethamphetamine within 1,000 feet of school	475.868
Unlawful manufacture of 3,4-methylenedioxymethamphetamine	475.866
Unlawful delivery of 3,4-methylenedioxymethamphetamine within 1,000 feet of school	475.872
Unlawful delivery of 3,4-methylenedioxymethamphetamine	475.87
Unlawful manufacture of cocaine within 1,000 feet of school	475.878
Unlawful manufacture of cocaine	475.876
Unlawful delivery of cocaine within 1,000 feet of school	475.882
Unlawful delivery of cocaine	475.88
Unlawful manufacture or delivery of controlled substance within 1,000 feet of school	475.904
Possessing or disposing of methamphetamine manufacturing waste	475.977
Unlawful manufacture of marijuana within 1,000 feet of school	475.858
Unlawful manufacture of marijuana	475.856
Unlawful delivery of marijuana within 1,000 feet of school	475.862
Unlawful delivery of marijuana	475.86
Use of minor in controlled substance offense	167.262
Unlawful delivery to minors	475.906
Manufacture, fermentation or possession of mash, wort or wash	471.44

Notes: Continued from previous table. Table shows list of offenses classified as "violent" for analysis purposes by the Oregon Health Study Group. Column 1 gives the description of the law, and Column 2 gives the statute number, or "orsno." Full descriptions of each offense are available at: <http://www.leg.state.or.us/ors/> or <http://www.oregonlaws.org/>

Table A5: Descriptive Statistics

	Adults age 19+			Adults Aged 19-64			Control Sample		
	N	Per Person	Per Case	N	Per Person	Per Case	N	Per Person	Per Case
Number of Cases	339837	0.117	.	325517	0.138	.	20673	0.380	.
Number of Charges	723164	0.248	2.128	691101	0.293	2.123	41862	0.770	2.025
Number of Convictions	295668	0.101	0.870	284256	0.120	0.873	19048	0.350	0.921
<i>Type of charge</i>									
Felony charges	239818	0.082	0.706	227710	0.096	0.700	14035	0.258	0.679
Felony convictions	92611	0.032	0.273	88750	0.038	0.273	6156	0.113	0.298
Misdemeanor charges	460622	0.158	1.355	441762	0.187	1.357	26611	0.490	1.287
Misdemeanor convictions	193633	0.066	0.570	186576	0.079	0.573	12415	0.228	0.600
Parole violations	12133	0.004	0.036	11417	0.005	0.035	480	0.009	0.023
Parole violation convictions	6346	0.002	0.019	5945	0.003	0.018	237	0.004	0.012
Charges of unknown penal code	10591	0.004	0.031	10212	0.004	0.031	736	0.014	0.036
Unknown penal code convictions	3078	0.001	0.009	2985	0.001	0.009	240	0.004	0.012
<i>Type of crime</i>									
Violent crimes charges	128294	0.044	0.377	119736	0.051	0.368	6416	0.118	0.310
Violent crime convictions	43213	0.015	0.127	40705	0.017	0.125	2265	0.042	0.110
Controlled substance crime charges	156090	0.054	0.459	151645	0.064	0.466	9423	0.173	0.456
Controlled substance crime convictions	72957	0.025	0.215	71230	0.030	0.219	5219	0.096	0.252
Income-generating crime charges	129919	0.045	0.382	123226	0.052	0.379	7975	0.147	0.386
Income-generating crime convictions	57502	0.020	0.169	54682	0.023	0.168	3916	0.072	0.189
Unclassified criminal charges	346590	0.119	1.020	331805	0.140	1.019	20080	0.369	0.971
Unclassified criminal charge convictions	137340	0.047	0.404	132118	0.056	0.406	8553	0.157	0.414

Notes: Table shows statistics on criminal charges from 1 January 2007 - 15 July 2010 (inclusive). An individual can have multiple criminal cases, and each criminal case can have multiple criminal charges. Criminal charges are categorized by penal code classification which was given in the criminal charges data (felonies, misdemeanors, violations, and "unknown") and also divided into three groups based on offense code (criminal charges that are violent, involve a controlled substance, or are income-generating - see text for additional details). Category of criminal charge is given in the left-most column. To measure outcomes per person in the general (or adult) Oregon population, we transform our data on counts of cases, charges, and convictions to population rates by dividing through by the Oregon population of adults, or adults ages 19 to 64 population using 2008 population statistics derived from the US Census Bureau's American Community Survey public use microdata sample file.

Table A6: Treatment-Control Balance: Lottery Variables

	Control Mean	Treatment - Control Difference	p-value
Birth Year	1968.01	0.14 (0.11)	0.203
Female	0.56	-0.01 (0.00)	0.008
English as preferred language	0.92	0.00 (0.00)	0.307
Signed up self	0.92	0.00 (0.00)	0.109
Signed up first day of lottery	0.09	0.00 (0.00)	0.924
Gave Phone Number	0.86	-0.00 (0.00)	0.596
Address is a PO Box	0.12	0.00 (0.00)	0.412
Zip code median household income	39300.96	-17.56 (81.72)	0.830
F statistic for lottery list variables			1.659
p-value			0.103

Notes: We report the control mean and the estimated difference (in the unit of the outcome or in percentage points) between treatments and controls for the outcome shown in the left-hand column (with standard errors in parentheses). All regressions include controls for household size and adjust standard errors for household clusters. Weights are used to account for the probability of being sampled in the new lottery. The final rows report the pooled F-statistics (and p-values) from testing treatment-control balance on sets of variables jointly. The sets of variables jointly tested are the variables recorded at the time of lottery sign-up, pre-lottery versions (measured 1 January 2007 - 10 March 2008) of the outcome variables in Tables 1 and 2, and the union of these two sets of variables.

Table A7: Treatment-Control Balance: Pre-Lottery Crime Variables

	Control Mean	Treatment - Control Difference	p-value
Any criminal case	0.084	0.004 (0.002)	0.052
Number of cases	0.132	0.003 (0.004)	0.521
Any criminal charge	0.084	0.004 (0.002)	0.052
Number of criminal charges	0.273	0.012 (0.011)	0.266
Any felony charge	0.039	0.002 (0.002)	0.320
Number of felony charges	0.097	0.005 (0.006)	0.439
Any misdemeanor charge	0.066	0.004 (0.002)	0.045
Number of misdemeanors	0.168	0.006 (0.007)	0.364
Any Parole violation	0.003	0.000 (0.000)	0.644
Number of Parole violations	0.004	0.000 (0.001)	0.461
Any charge of unknown penal code	0.003	0.000 (0.000)	0.528
Number of charges of unknown penal code	0.003	0.000 (0.001)	0.326
Any controlled substance charge	0.039	0.000 (0.002)	0.802
Number of controlled substance charges	0.063	0.001 (0.003)	0.847
Any violent crime charge	0.019	0.002 (0.001)	0.032
Number of violent crime charges	0.040	0.005 (0.003)	0.130
Any income-generating crime charge	0.026	0.002 (0.001)	0.183
Number of income-generating crime charges	0.053	0.006 (0.004)	0.114
Any unclassified crime charge	0.053	0.003 (0.002)	0.088
Number of unclassified crime charges	0.129	0.002 (0.006)	0.712
Any convictions	0.065	0.004 (0.002)	0.047
Number of convictions	0.123	0.008 (0.005)	0.136
Any felony conviction	0.028	0.001 (0.001)	0.542
Number of felony convictions	0.045	0.002 (0.003)	0.460
Any misdemeanor conviction	0.046	0.003 (0.002)	0.087
Number of misdemeanor convictions	0.075	0.005 (0.003)	0.170
Any violation conviction	0.002	0.000 (0.000)	0.594
Number of violation convictions	0.002	0.000 (0.001)	0.466
Any unknown penal code conviction	0.001	0.000 (0.000)	0.824
Number of unknown penal code convictions	0.001	0.000 (0.000)	0.573
Any violent crime convictions	0.010	0.002 (0.001)	0.025
Number of violent crime convictions	0.013	0.002 (0.001)	0.123
Any controlled substance conviction	0.027	0.001 (0.001)	0.538
Number of controlled substance convictions	0.035	0.001 (0.002)	0.597
Any income-generating crime conviction	0.018	0.002 (0.001)	0.121
Number of income-generating crime convictions	0.026	0.003 (0.002)	0.103
Any unclassified conviction	0.035	0.002 (0.001)	0.208
Number of unclassified convictions	0.055	0.002 (0.003)	0.415
F statistic for lottery list variables			0.650
p-value			0.948

Notes: We report the control mean and the estimated difference (in the unit of the outcome or in percentage points) between treatments and controls for the outcome shown in the left-hand column (with standard errors in parentheses). All regressions include controls for household size and adjust standard errors for household clusters. Weights are used to account for the probability of being sampled in the new lottery. The final rows report the pooled F-statistics (and p-values) from testing treatment-control balance on sets of variables jointly. The sets of variables jointly tested are the variables recorded at the time of lottery sign-up, pre-lottery versions (measured 1 January 2007 - 10 March 2008) of the outcome variables in Tables 1 and 2, and the union of these two sets of variables.

Table A8: Effect of lottery on Medicaid coverage (First stage estimates)

	Control Mean	Estimated First Stage
Ever on Medicaid	0.188	0.234 (0.004)
Ever on OHP Standard	0.045	0.259 (0.003)
Number of Months on Medicaid	2.515	4.434 (0.071)
On Medicaid at the end of the study period	0.134	0.096 (0.003)

Notes: Column 1 reports the control mean for alternate definitions of *MEDICAID*. Column 2 reports the coefficient (with standard error in parentheses) on *LOTTERY* from equation (3). All regressions include indicators for the number of household members on the lottery list, adjust standard errors for household clusters, and include weights that account for the probability of being sampled in the new lottery. The study period starts on March 10, 2008 and ends on July 15, 2010. In all our analyses of the local-average-treatment effect of Medicaid in the paper, we use the definition in the first row: "Ever on Medicaid" over our study period. The subsequent rows shows impacts for whether the individual was ever enrolled in OHP Standard plan (the specific Medicaid plan that was lotteried), the number of months on Medicaid coverage, and whether the individual was enrolled in Medicaid at the end of the study period.

Table A9: Effect of Medicaid Coverage on Criminal Charges through September 30, 2009

	Percent with any			Number		
	Control Mean	ITT	LATE	Control Mean	ITT	LATE
<i>Overall</i>						
Criminal Cases	8.99	-0.017 (0.225)	-0.067 (0.878)	0.15	-0.001 (0.005)	-0.004 (0.019)
Criminal Charges	8.99	-0.017 (0.225)	-0.067 (0.878)	0.31	0.004 (0.013)	0.017 (0.050)
<i>Charges by type of charge</i>						
Felony	3.87	0.100 (0.154)	0.391 (0.602)	0.11	0.005 (0.007)	0.019 (0.029)
Misdemeanor	7.39	0.056 (0.207)	0.220 (0.811)	0.12	-0.001 (0.004)	-0.005 (0.017)
Parole violations	0.26	-0.002 (0.041)	-0.008 (0.159)	0.00	-0.001 (0.001)	-0.004 (0.003)
Unknown penal code	0.33	0.028 (0.046)	0.109 (0.178)	0.00	0.000 (0.001)	0.001 (0.002)
<i>Charges by type of crime</i>						
Violent	2.39	-0.099 (0.125)	-0.386 (0.490)	0.05	-0.001 (0.004)	-0.003 (0.016)
Controlled substance	3.71	0.172 (0.152)	0.672 (0.595)	0.06	-0.001 (0.003)	-0.002 (0.013)
Income-generating	2.86	0.062 (0.132)	0.242 (0.517)	0.06	0.007 (0.004)	0.026 (0.016)
Other	5.84	0.082 (0.185)	0.319 (0.725)	0.15	-0.002 (0.008)	-0.009 (0.031)

Notes: Variables are measured from 10 March 2008 - 30 September 2009 (inclusive). All regressions include controls for household size and the total number of criminal cases an individual had prior to the lottery (1 January 2007 - 9 March 2008). All regressions adjust standard errors for household clusters. Penal code classifications are given in administrative data. Crime classifications were defined (prior to analyzing treatment-control differences) by the study group. ITT estimates are based on estimating equation 1; LATE estimates are based on estimating equations 2 and 3.

Table A10: Effect of Medicaid Coverage on Criminal Convictions through September 30, 2009

	Percent with any			Number		
	Control Mean	ITT	LATE	Control Mean	ITT	LATE
<i>Overall</i>						
Convictions	6.84	0.110 (0.183)	0.431 (0.714)	0.14	0.005 (0.005)	0.018 (0.020)
<i>Convictions by type of charge</i>						
Felony	2.85	-0.008 (0.121)	-0.033 (0.474)	0.05	0.002 (0.003)	0.006 (0.011)
Misdemeanor	5.04	0.217 (0.161)	0.850 (0.630)	0.09	0.004 (0.004)	0.015 (0.014)
Parole violations	0.14	-0.010 (0.026)	-0.037 (0.102)	0.00	-0.000 (0.000)	-0.001 (0.001)
Unknown penal code	0.17	-0.031 (0.029)	-0.123 (0.113)	0.00	-0.000 (0.000)	-0.002 (0.001)
<i>Convictions by type of crime</i>						
Violent	1.30	0.007 (0.085)	0.028 (0.333)	0.02	-0.000 (0.001)	-0.001 (0.006)
Controlled substance	2.51	0.067 (0.115)	0.261 (0.449)	0.03	-0.001 (0.002)	-0.002 (0.007)
Income-generating	2.00	0.032 (0.103)	0.127 (0.402)	0.03	0.002 (0.002)	0.010 (0.008)
Other	3.90	0.127 (0.143)	0.495 (0.558)	0.06	0.001 (0.003)	0.005 (0.011)

Notes: Variables are measured from 10 March 2008 - 30 September 2009 (inclusive). All regressions include controls for household size and the total number of criminal cases an individual had prior to the lottery (1 January 2007 - 9 March 2008). All regressions adjust standard errors for household clusters. Penal code classifications are given in administrative data. Crime classifications were defined (prior to analyzing treatment-control differences) by the study group. ITT estimates are based on estimating equation 1; LATE estimates are based on estimating equations 2 and 3.

Table A11: Effect of Medicaid Coverage on Criminal Charges, Alternative Control Variables

	Baseline	Percent with any Without total number of cases in the pre-period	With lottery list variables	Baseline	Number Without total number of cases in the pre-period	With lottery list variables
<i>Overall</i>						
Criminal Cases	-0.006 (1.083) [0.996]	0.180 (1.125) [0.873]	-0.412 (1.128) [0.715]	-0.005 (0.027) [0.865]	0.002 (0.029) [0.953]	-0.009 (0.029) [0.753]
Criminal Charges	-0.006 (1.083) [0.996]	0.180 (1.125) [0.873]	-0.412 (1.128) [0.715]	0.052 (0.071) [0.460]	0.065 (0.073) [0.378]	0.027 (0.074) [0.715]
<i>Charges by type of charge</i>						
Felony	0.908 (0.746) [0.224]	1.016 (0.766) [0.185]	0.805 (0.772) [0.297]	0.041 (0.039) [0.289]	0.044 (0.039) [0.253]	0.035 (0.040) [0.379]
Misdemeanor	0.286 (1.009) [0.777]	0.447 (1.043) [0.668]	-0.083 (1.050) [0.937]	-0.009 (0.024) [0.707]	-0.004 (0.025) [0.874]	-0.015 (0.025) [0.546]
Parole violations	-0.135 (0.208) [0.516]	-0.129 (0.208) [0.535]	-0.190 (0.207) [0.359]	-0.005 (0.004) [0.186]	-0.005 (0.004) [0.193]	-0.006 (0.004) [0.132]
Unknown penal code	-0.026 (0.240) [0.913]	-0.017 (0.241) [0.944]	-0.050 (0.245) [0.838]	-0.001 (0.003) [0.689]	-0.001 (0.003) [0.718]	-0.001 (0.003) [0.645]
<i>Charges by type of crime</i>						
Violent	-0.110 (0.616) [0.858]	-0.057 (0.622) [0.926]	-0.068 (0.630) [0.914]	-0.001 (0.020) [0.971]	0.001 (0.020) [0.974]	0.000 (0.021) [0.995]
Controlled substance	0.612 (0.756) [0.418]	0.708 (0.771) [0.358]	0.578 (0.782) [0.460]	-0.007 (0.017) [0.667]	-0.005 (0.018) [0.774]	-0.008 (0.018) [0.643]
Income-generating	0.177 (0.649) [0.785]	0.265 (0.665) [0.690]	0.060 (0.674) [0.929]	0.060 (0.025) [0.015]	0.063 (0.025) [0.012]	0.051 (0.025) [0.041]
Other	0.429 (0.906) [0.636]	0.575 (0.935) [0.539]	0.051 (0.941) [0.957]	-0.007 (0.043) [0.872]	-0.001 (0.044) [0.987]	-0.024 (0.045) [0.594]

Notes: Table shows the local average treatment effect estimates of lottery selection on the outcome indicated. Column 1 shows baseline results from Tables 1 and 2 which control for the total number of criminal cases an individual had prior to the lottery (1 January 2007 - 9 March 2008). Column 2 shows results without controlling for total number of cases in the pre-period. Column 3 shows results controlling for both the total number of pre-period cases and characteristics recorded at lottery sign up: gender, year of birth, requested English-language sign-up materials, signed self up for the lottery, lives in a zip code in a metropolitan statistical area, signed up for the lottery on the first day, gave a phone number, gave an address that was PO Box, and median household income in zip code. All regressions control for household size and adjust standard errors for household clusters. LATE estimates are based on estimating equations 2 and 3.

Table A12: Effect of Medicaid Coverage on Criminal Convictions, Alternative Control Variables

	Baseline	Percent with any Without total number of cases in the pre-period	With lottery list variables	Baseline	Number Without total number of cases in the pre-period	With lottery list variables
<i>Overall</i>						
Convictions	0.099 (0.961) [0.918]	0.269 (1.000) [0.788]	-0.227 (1.005) [0.821]	0.030 (0.029) [0.305]	0.036 (0.031) [0.245]	0.024 (0.031) [0.437]
<i>Convictions by type of charge</i>						
Felony	0.464 (0.635) [0.465]	0.550 (0.650) [0.398]	0.470 (0.657) [0.475]	0.024 (0.015) [0.114]	0.026 (0.016) [0.097]	0.025 (0.016) [0.109]
Misdemeanor	0.317 (0.865) [0.714]	0.448 (0.891) [0.616]	0.075 (0.901) [0.934]	0.012 (0.022) [0.591]	0.016 (0.023) [0.489]	0.005 (0.023) [0.811]
Parole violations	-0.123 (0.142) [0.390]	-0.120 (0.142) [0.400]	-0.198 (0.140) [0.158]	-0.003 (0.002) [0.069]	-0.003 (0.002) [0.071]	-0.004 (0.002) [0.022]
Unknown penal code	-0.163 (0.156) [0.296]	-0.160 (0.156) [0.307]	-0.178 (0.160) [0.266]	-0.003 (0.002) [0.112]	-0.003 (0.002) [0.118]	-0.003 (0.002) [0.122]
<i>Convictions by type of crime</i>						
Violent	-0.145 (0.456) [0.751]	-0.112 (0.459) [0.807]	-0.091 (0.467) [0.846]	-0.001 (0.008) [0.848]	-0.001 (0.008) [0.895]	-0.000 (0.008) [0.972]
Controlled substance	0.156 (0.625) [0.803]	0.237 (0.639) [0.711]	0.152 (0.649) [0.815]	-0.005 (0.010) [0.633]	-0.003 (0.010) [0.745]	-0.005 (0.010) [0.633]
Income-generating	0.536 (0.552) [0.332]	0.605 (0.563) [0.283]	0.500 (0.572) [0.382]	0.027 (0.012) [0.022]	0.028 (0.012) [0.018]	0.026 (0.012) [0.031]
Other	0.150 (0.763) [0.844]	0.262 (0.783) [0.737]	-0.209 (0.791) [0.791]	0.003 (0.017) [0.853]	0.006 (0.018) [0.746]	-0.003 (0.018) [0.875]

Notes: Table shows the local average treatment effect estimates of lottery selection on the outcome indicated. Column 1 shows baseline results from Tables 1 and 2 which control for the total number of criminal cases an individual had prior to the lottery (1 January 2007 - 9 March 2008). Column 2 shows results without controlling for total number of cases in the pre-period. Column 3 shows results controlling for both the total number of pre-period cases and characteristics recorded at lottery sign up: gender, year of birth, requested English-language sign-up materials, signed self up for the lottery, lives in a zip code in a metropolitan statistical area, signed up for the lottery on the first day, gave a phone number, gave an address that was PO Box, and median household income in zip code. All regressions control for household size and adjust standard errors for household clusters. LATE estimates are based on estimating equations 2 and 3.

Table A13: Sensitivity of Results to Functional Form, Criminal Cases and Charges

	Percent with any		Number	
	Linear Model	Logistic Model	Linear Model	Negative Binomial Model
<i>Overall</i>				
Criminal Cases	-0.001 (0.254) [0.996]	0.000 (0.003) [0.973]	-0.001 (0.006) [0.865]	-0.002 (0.005) [0.657]
Criminal Charges	-0.001 (0.254) [0.996]	0.000 (0.003) [0.973]	0.011 (0.017) [0.508]	0.013 (0.016) [0.389]
<i>Charges by type of charge</i>				
Felony	0.213 (0.175) [0.224]	0.002 (0.002) [0.168]	0.009 (0.009) [0.329]	0.008 (0.008) [0.348]
Misdemeanor	0.067 (0.236) [0.777]	0.001 (0.002) [0.720]	-0.002 (0.006) [0.682]	-0.001 (0.004) [0.773]
Parole violations	-0.032 (0.049) [0.516]	-0.000 (0.000) [0.567]	-0.001 (0.001) [0.182]	-0.001 (0.001) [0.263]
Unknown penal code	-0.006 (0.056) [0.913]	-0.000 (0.000) [0.997]	-0.000 (0.001) [0.677]	-0.000 (0.000) [0.820]
<i>Charges by type of crime</i>				
Violent	-0.026 (0.144) [0.858]	-0.000 (0.001) [0.969]	-0.000 (0.005) [0.944]	-0.001 (0.005) [0.911]
Controlled substance	0.143 (0.177) [0.418]	0.002 (0.002) [0.338]	-0.002 (0.004) [0.677]	-0.001 (0.003) [0.765]
Income-generating	0.042 (0.152) [0.785]	0.001 (0.001) [0.645]	0.014 (0.006) [0.019]	0.012 (0.005) [0.019]
Other	0.101 (0.212) [0.636]	0.001 (0.002) [0.559]	-0.002 (0.010) [0.878]	0.002 (0.009) [0.810]

Notes: Table shows the estimated intent-to-treat (ITT) effect of lottery selection: the coefficient on lottery selection, the standard error (in parentheses), and the p-value [in brackets]. Column 2 shows, for binary variables, the marginal effects from an alternate logit specification. Column 4 shows, for continuous variables, the marginal effects from a negative binomial regression. Marginal effects are evaluated at the mean of the independent variables. Outcome variables cover the time period March 10, 2008 - 15 July 2010. All regressions control for household size, pre-period versions of the outcomes, and total number of cases in the pre-period and adjust standard errors for household clusters. ITT estimates are based on estimating equation 1 or variants as described in the notes.

Table A14: Sensitivity of Results to Functional Form, Convictions

	Percent with any		Number	
	Linear Model	Logistic Model	Linear Model	Negative Binomial Model
<i>Overall</i>				
Convictions	0.023 (0.225) [0.918]	0.000 (0.002) [0.849]	0.007 (0.007) [0.314]	0.004 (0.006) [0.505]
<i>Charges by type of charge</i>				
Felony	0.109 (0.149) [0.465]	0.001 (0.001) [0.357]	0.006 (0.004) [0.120]	0.005 (0.003) [0.142]
Misdemeanor	0.074 (0.203) [0.714]	0.001 (0.002) [0.626]	0.002 (0.005) [0.665]	0.001 (0.004) [0.778]
Parole violations	-0.029 (0.033) [0.390]	-0.000 (0.000) [0.422]	-0.001 (0.000) [0.069]	-0.001 (0.000) [0.090]
Unknown penal code	-0.038 (0.037) [0.296]	-0.000 (0.000) [0.336]	-0.001 (0.000) [0.110]	-0.000 (0.000) [0.145]
<i>Charges by type of crime</i>				
Violent	-0.034 (0.107) [0.751]	-0.000 (0.001) [0.871]	-0.000 (0.002) [0.813]	-0.001 (0.002) [0.656]
Controlled substance	0.036 (0.146) [0.803]	0.001 (0.001) [0.664]	-0.001 (0.002) [0.624]	-0.000 (0.002) [0.862]
Income-generating	0.125 (0.129) [0.332]	0.001 (0.001) [0.238]	0.006 (0.003) [0.032]	0.004 (0.002) [0.044]
Other	0.035 (0.179) [0.844]	0.001 (0.002) [0.729]	0.001 (0.004) [0.866]	0.000 (0.004) [0.910]

Notes: Table shows the estimated intent-to-treat (ITT) effect of lottery selection: the coefficient on lottery selection, the standard error (in parentheses), and the p-value [in brackets]. Column 2 shows, for binary variables, the marginal effects from an alternate logit specification. Column 4 shows, for continuous variables, the marginal effects from a negative binomial regression. Marginal effects are evaluated at the mean of the independent variables. Outcome variables cover the time period March 10, 2008 - 15 July 2010. All regressions control for household size, pre-period versions of the outcomes, and total number of cases in the pre-period and adjust standard errors for household clusters. ITT estimates are based on estimating equation 1 or variants as described in the notes.

Table A15: Comparison to Other Estimates (Annualized Levels)

Outcome	Population	Effect of Medicaid [Baseline Mean]	Annualized Effect of Medicaid [Annualized Mean]	Most comparable OHIE outcome	[Baseline Annualized Mean]	OHIE subpopulation Mean	OHIE Annualized Effect on Subpop. [Baseline Ann. Mean]
<i>Incarceration papers</i>							
Aslim et al. (2022)	1-year re-incarceration rate	Recently released multi-time re-offenders w/ violent 1st offense	-1.7pp (-3.4, -0.03) [4.0pp]	Already annual	Any conviction	0.05pp (-0.89, 0.99) [4.6pp]	Pre-treatment conviction [21.3pp]
Aslim et al. (2024)	Annual # of re-imprisonments	Recently released from prison	-0.07 (-0.13, -0.013) [0.192]	Already annual	# convictions	0.016 (-0.014, 0.045) [0.099]	Pre-treatment conviction [0.53]
Burns and Dague (2023)	6-month re-incarceration rate	Recently released from prison	-2.54pp (-3.9, -1.2) [16.3pp]	-5.02pp (-7.7, -2.3) [29.9pp]	Any conviction	0.05pp (-0.89, 0.99) [4.6pp]	Pre-treatment conviction [21.3pp]
Jácome (2022)	Quarterly incarceration rate	Young men from low-income high schools	-0.99pp (-1.70, -0.27) [4.08pp]	-3.99pp (-6.7, -1.1) [7.99pp]	Any conviction	0.05pp (-0.89, 0.99) [4.6pp]	Men -0.89pp (-2.4, 0.7) [1.84pp]
Packham and Slusky (2024)	1-year re-incarceration rate	Recently released from prison	1.7pp (-24.9, 28.3) [10pp]	Already annual	Any conviction	0.05pp (-0.89, 0.99) [4.6pp]	Pre-treatment conviction [21.3pp]
<i>Reported crime papers</i>							
Deza et. al (2024)	Annual reported crime per capita	Full population	-0.32 per person (-0.15, -0.49) [0.21 per person*]	Already annual	# charges	0.027 (-0.045, 0.010) [0.037 per person]	NA
Vogler (2020)	Annual reported crime per capita	Full population	-0.007 per person (-0.04, 0.03) [0.090 per person*]	Already annual	# charges	0.027 (-0.045, 0.010) [0.037 per person]	NA
Deza et. al (2024)	Annual reported violent crime per capita	Full population	-0.153 per person (-0.30, -0.0003) [0.08 per person*]	Already annual	# charges for violent crimes	-0.00039 (-0.021, 0.020) [0.011 per person]	NA
He and Barkowski (2019)	Annual reported violent crime per capita	Full population	-0.003 per person (-0.004, -0.001) [0.006 per person*]	Already annual	# charges for violent crimes	-0.00039 (-0.021, 0.020) [0.011 per person]	NA
Vogler (2020)	Annual reported violent crime per capita	Full population	-0.007 per person (-0.013, -0.00002) [0.012 per person*]	Already annual	# charges for violent crime	-0.00039 (-0.021, 0.020) [0.011 per person]	NA

Notes: Table compares estimated treatment effects in levels across other studies and those presented in this manuscript. Means marked with the symbol * have been re-scaled by the ratio of the control group's outcome to that in the adult population. Estimates are drawn from the following sources: [Burns and Dague \(2023\) Table 2](#); [Jácome \(2022\) Table 3](#); [Packham and Slusky \(2022\)](#) Authors' calculations from Tables 2 and 3; [Aslim et al. \(2022\) Table 5](#) and authors' calculation; [Vogler \(2020\) Table 3](#) and authors' calculation; [Deza et al. \(2024a\) Table 2](#); [He and Barkowski \(2020\) Table 4](#) and authors' calculation. See text and Appendix B for further details.

Table A16: Comparison to Other Estimates (Percent)

Outcome	Population	Effect of Medicaid [Baseline Mean]	Most comparable OHIE outcome	OHIE Estimated Effect [Control Mean]	Most comparable OHIE subpopulation	OHIE Estimated Effect on Subpopulation [Control Mean]
<i>Incarceration papers</i>						
Aslim et al. (2022)	1-year re-incarceration rate	Recently released multi-time re-offenders w/ violent 1st offense	-42.5% (-8.1%, -0.8%) [4.0pp]	Conviction rate	1.1% (-19.8%, -22.0%) [9pp]	Pre-treatment conviction 9.4% (-18.6%, 37.3%) [38.1pp]
Aslim et al. (2024)	Annual # of re-imprisonments	Recently released from prison	-38.3% (-70.1%, -6.5%) [0.192]	# convictions	15.8% (-14.1%, 45.7%) [0.19]	Pre-treatment conviction 24.6% (-18.7%, 68.0%) [0.99]
Burns and Dague (2023)	6-month re-incarceration rate	Recently released from prison	-15.6% (-23.9, -7.3) [16.3pp]	Conviction rate	1.1% (-19.8%, -22.0%) [9pp]	Pre-treatment conviction 9.4% (-18.6%, 37.3%) [38.1pp]
Jácome (2022)	Quarterly conviction rate	Young men from low-income high schools	-24.1% (-41.6%, -6.7%) [4.08pp]	Conviction rate	1.1% (-19.8%, -22.0%) [9pp]	Men -12.6% (-34.2%, 9.1%) [14.2pp]
Packham and Slusky (2024)	1-year re-incarceration rate	Recently released from prison	17% (-249%, 283%) [10pp]	Conviction rate	1.1% (-19.8%, -22.0%) [9pp]	Pre-treatment conviction 9.4% (-18.6%, 37.3%) [38.1pp]
<i>Reported crime papers</i>						
Deza et. al (2024)	Annual reported crime	Full population	.148.0% (-228%, -68.2%) [0.21 per person*]	# charges	11.8% (-19.6%, 43.2%) [0.21 per person]	NA NA
Vogler (2020)	Annual reported crime	Full population	-7.0% (-44.5%, -30.4%) [0.099 per person*]	# charges	11.8% (-19.6%, 43.2%) [0.21 per person]	NA NA
Deza et. al (2024)	Annual reported violent crime	Full population	-189% (-3.78%, -0.004%) [0.08 per person*]	# charges for violent crimes	-0.5% (-28.7%, 27.6%) [0.03 per person]	NA NA
He and Barkowski (2019)	Annual reported violent crime	Full population	-44.6% (-72.3%, -17.0%) [0.006 per person*]	# charges for violent crimes	-0.5% (-28.7%, 27.6%) [0.03 per person]	NA NA
Vogler (2020)	Annual reported violent crime	Full population	-53.4% (-106.7%, -0.2%) [0.012 per person*]	# charges for violent crime	-0.5% (-28.7%, 27.6%) [0.03 per person]	NA NA

Notes: Table compares estimated treatment effects in percent across other studies and those presented in this manuscript. Means marked with the symbol * have been re-scaled by the ratio of the control group's outcome to that in the adult population. Estimates are drawn from the following sources: [Burns and Dague \(2023\) Table 2; Jácome \(2022\) Table 3; Packham and Slusky \(2022\)](#) Authors' calculations from Tables 2 and 3; [Aslim et al. \(2022\) Table 5](#) and authors' calculation; [Vogler \(2020\) Table 3](#) and authors' calculation; [Deza et al. \(2024a\) Table 2](#); [He and Barkowski \(2020\) Table 4](#) and authors' calculation. See text and Appendix B for further details.

Table A17: Analytic Weights

	Mean	Standard Deviation	Minimum	Median	75th Percentile	95th Percentile	Max	N
Full Sample	1.000	0.479	0.000	1.000	1.000	1.598	4.966	74922
<i>Non-zero weights</i>								
Full Sample	1.149	0.303	1.000	1.000	1.131	1.598	4.966	65175
Control Participants	1.217	0.354	1.000	1.000	1.443	1.714	4.966	37015
Treatment Participants	1.060	0.185	1.000	1.000	1.000	1.443	2.958	28160

Notes: Table shows the distribution of weights used to account for the new health insurance lottery that started in the fall of 2009.

Table A18: First stage estimates for ACA Medicaid Expansions to apply to quasi-experimental studies

	Full Population (ACS)	Returning from Prison (CJARS-JOE)
$Post_t \times Treated_s$	0.032 (0.006)***	0.353 (0.065)***
Sample mean	0.190	0.273
N	31,290,943 (individuals)	212 (cohort x state x year)

Notes: This table presents estimates of the first stage effect of the ACA Medicaid expansions on Medicaid enrollment in the general population (first column) and on those re-entering society from prison (second column). Robust standard errors clustered at the state level are reported in parentheses. See text for more details.