Access to Health Care and Criminal Behavior: Short-Run Evidence from the ACA Medicaid Expansions*

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Abstract

I investigate the causal relationship between access to health care and criminal behavior following state decisions to expand Medicaid coverage after the Affordable Care Act. Many of the newly eligible individuals for Medicaid-provided health insurance are adults at high risk for crime. I leverage variation in insurance eligibility generated by state decisions to expand Medicaid and differential pre-treatment uninsured rates at the county-level. My findings indicate that the Medicaid expansions have resulted in significant decreases in annual crime by 3.3 percent. This estimate is driven by statistically significant decreases in both reported violent and property crime. A within-state heterogeneity analysis suggests that crime impacts are more pronounced in counties with higher pre-reform uninsured levels. The estimated decrease in reported crime amounts to an annual cost savings of approximately $13 billion.

Keywords: Crime; Medicaid; Health Insurance

JEL Classification: I13, K14, K42

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1 Introduction

Access to health care and criminal activity are two of the most important policy issues in the United States. High rates of uninsured individuals and crime have historically been prevalent in the U.S. In 2010 an estimated 50 million people did not have health insurance, a total representing over 16 percent of the total population (DeNavas-Walt et al., 2012). In the same year, the FBI reported over 13 million arrests nationwide. The socioeconomic costs associated with high rates of uninsurance have been well documented. Studies have shown being uninsured increases mortality (Currie and Gruber (1996); Wilper et al. (2009); Sommers et al. (2012)) and reduces productivity and financial stability (Hu et al. (2016); Brevoort et al. (2017); Dizioli and Pinheiro (2016)). Crime also generates large costs to society. Annually, criminal activity accounts for over $15 billion in economic losses for victims and $180 billion in police, judicial, and correctional costs (McCollister et al., 2010). When including less visible costs associated with fear and agony, as well as the opportunity cost of time lost, the annual implicit cost of crime has been estimated to be as high as $3.2 trillion (Anderson et al., 2012).

While high rates of uninsurance and criminal behavior separately impose heavy burdens on individuals and communities, statistics also indicate that the issues are closely related. Prior to 2014, it has been estimated 90 percent of the individuals entering local and county jails in the U.S. were uninsured (Hancock, 2016).[1] Despite this observed correlation, the causal relationship between health care and crime is not well understood. Few studies have examined on this relationship, most notably Bondurant et al. (2016) and Wen et al. (2017b). These studies, however, focus on the link between changes in health care utilization, specifically that of substance-use disorder treatment rates, and criminal behavior.[2] Both find

[1] In addition to being uninsured, a large number of individuals involved in the criminal justice system are poor and lack stable employment, circumstances that historically have created substantial barriers to health insurance. One study found that 60 percent of all individuals arrested earn monthly incomes that are less than 133 percent of the federal poverty limit (FPL) and 29 percent were unemployed at the time of their arrest (James, 2017).

[2] Somewhat related to the setting of this study, Wen et al. (2017b) instrument for changes in substance-use treatment utilization using state Medicaid expansions that took place between 2001 and 2008 through

increases in treatment rates significantly reduces criminal behavior at the local level.

In this paper, I fill an important gap in the literature by providing causal estimates pertaining to the impact of health insurance on criminal behavior. My empirical strategy combines plausibly exogenous variation in insurance eligibility with state and county-level panel data of reported crimes between the years 2010 and 2015.[3] I leverage this variation in two ways. First, I utilize changes in insurance eligibility generated by state decisions to expand Medicaid coverage beginning in 2014. The state option to expand coverage was one of the main components included in the Patient Protection and Affordable Care Act (ACA). Prior to the passage of the ACA, Medicaid eligibility was restricted to certain low income groups, including children, single mothers, pregnant women, the disabled, and the elderly. The categorical restrictions excluded millions of low-income adults from qualifying for the program. Many of these individuals, particularly single-adults without dependent children, are at especially high risk of committing crimes (Gates and Rudowitz, 2014). I harness the second source of variation by incorporating uninsured rate data at the county-level. This last source of variation allows me to explore heterogeneous treatment effects by estimating how rates of reported crime have changed across the distribution of predicted treatment intensity as measured by the uninsured rate in the year prior to the Medicaid expansions taking effect.

Using a difference-in-differences and difference-in-differences-in-differences approach, the analysis reveals economically meaningful and robust evidence that expanding health insurance eligibility reduces rates of reported crime. Specifically, I find that incidents of total reported crime decreased by 3.3 percent in states that chose to expand Medicaid coverage relative to states that did not expand. This effect is distributed among both reported violent and property crimes with estimated reductions of 6.0 and 3.1 percent, respectfully. I also find statistically significant impacts across many specific crime categories, including criminal homicide, aggravated assault, robbery, and vehicle theft. The results are robust across a

Health Insurance Flexibility and Accountability (HIFA) waivers.

[3] I incorporate known offenses in order to capture those crimes that come to the attention of law enforcement. This is opposed to other available data on arrests that are restricted to crimes that have been cleared by law enforcement.

number of alternative specifications and falsification tests. A back-of-the-envelope calculation suggests the annual social cost savings associated with the estimated crime reduction is approximately $13 billion.

In addition to the state-level findings, a treatment heterogeneity analysis suggests crime reductions were more pronounced in counties with higher pre-reform uninsured levels among the population eligible for Medicaid, i.e. in counties where coverage gains is expected to be largest. In particular, the results indicate the decrease in total reported crimes was 0.4 percent larger in counties where the uninsured rate was one percent higher in the year prior to the expansions taking effect. The analysis also reveals statistically significant reductions separately among violent and property crime. Taken together, these results indicate that the reductions in state-level crime rates were largest in counties where the fraction of the uninsured population was larger.

This paper contributes to the general economic literature as an investigation into the causal relationship between health insurance and crime. Without empirical evidence, it is unclear *ex ante* whether investments in health care via health insurance expansions can lead to significant crime reductions.[4] It should be noted that a recent working paper posted after my own by He (2017) also examines the effects of health insurance on crime following the ACA Medicaid expansions and finds similar results to those presented here. In addition, this study contributes to two distinct economic literatures. The first refers to a growing body of work in health economics that focuses on estimating the effects of expanding public health insurance. The second pertains to a rich area of research focusing on public policy as a means of criminal deterrence and improving public safety. Finally, this study contributes to the ongoing debate regarding the pros and cons of expanding health care coverage to near universal levels. The empirical approach used in this paper allows me to estimate treatment effects at both a broad (state) and local (county) geographic level. This is particularly useful from a policy perspective, especially in those states that are considering expanding Medicaid

[4]Section 2 discusses theses predictions in greater detail.

eligibility.

The remainder of the paper is structured as follows. Section 2 provides background information on the Medicaid program and the ACA, as well as discusses related studies that have considered the relationship between health and crime. Section 3 describes the data and section 4 presents the empirical strategy in detail. Section 5 presents the results of the main state-level analysis. Section 6 provides several alternative specifications and falsification tests. Section 7 presents results from the county-level heterogeneous treatment analysis. Section 8 describes the estimated social cost-savings associated with the reduction in crime. Section 9 discusses potential mechanisms and offers concluding remarks.

2 Background and Conceptual Framework

2.1 Medicaid and the ACA

Medicaid is a publicly-funded health insurance program for low-income families and individuals in the United States. Founded in 1965, Medicaid is the largest means-tested social insurance program in the country. Prior to 2014, the program covered nearly 60 million individuals with an estimated combined cost to state and federal governments totaling $390 billion (Buchmueller et al., 2015). States have historically had a great deal of autonomy when it comes to determining program generosity. As a consequence, there has always been considerable heterogeneity in Medicaid eligibility requirements across states. However, one relatively consistent policy across states has been categorical restrictions that limit coverage to only the disabled, the elderly, and members of families with dependent children. This requirement meant that millions of low-income individuals in the United States remained ineligible for Medicaid.

In the years prior to the ACA, several states attempted to provide Medicaid benefits to otherwise ineligible low-income adults, mainly through federal waivers and expanding in-state health care programs. Ultimately these expansions failed to result in widespread reductions in the uninsurance rate, largely due to restricted income eligibility and coverage that was limited to only a small subset of conditions (Maclean and Saloner, 2017). It was not until the ACA that eligibility for Medicaid benefits extended more broadly to low-income adults. In addition to establishing health insurance marketplaces and imposing an individual mandate that penalizes individuals that elect to remain uninsured, the ACA expands Medicaid benefits to all adults with incomes at or below 138 percent of the federal poverty level.[5][6] The Medicaid reform was originally formulated to occur nationwide, but was effectively made a state option by a 2012 Supreme Court ruling on the constitutionality of the ACA.

In states that have chosen to expand Medicaid eligibility, the categorical restrictions have been removed and eligibility is now based solely on income. Newly eligible individuals receive coverage for a wide range of treatments for mental illness, substance use disorders, and chronic diseases. Table 1 shows the states that chose to adopt the Medicaid expansions. As of 2017, 31 states and the District of Columbia have expanded their Medicaid programs. Coverage became effective on January 1, 2014 in most expansion states. A small number of states that chose to expand Medicaid coverage already had comparable eligibility requirements through their own state-run health insurance programs.

Figure 1 illustrates the trends in the average state uninsured rate before and after the Medicaid expansions became effective, both for total state populations in 1(a) and separately among the Medicaid eligible state populations in 1(b), using data from the Census Bureau (2017a) for 2010-2015. In both plots there is an obvious break in the probability of having no coverage after 2013 following the implementation of the key provisions of the ACA. However, states that expanded Medicaid eligibility experienced a more sizable decrease in their uninsured population. Using the same data, Table A1 shows results from a simple fixed effects differences-in-differences regression of state uninsured rates on indicators for whether the state expanded Medicaid. The results indicate that expansion states experienced greater

[5] This level corresponds to having an annual individual income of approximately $16,000 or less.

[6] Regulations that established the insurance marketplaces and the individual mandate were implemented in 2014 (KFF, 2013).

decreases in uninsured individuals across their entire populations (as shown in Panel A) with even larger estimated effects for the population that meets the new eligiblity requirements for Medicaid (as shown in Panel B).[7] [8]

2.2 Health Insurance and Crime

Predicting the net effects of increasing health insurance on crime is somewhat ambiguous, a feature that motivates this study. The relationship depends on how health insurance changes utilization of treatment and individual health behaviors. Negative effects are likely to result from increasing treatment utilization. This channel is particularly important among individuals that have previously committed crimes. Approximately 70 percent of individuals that have previously been arrested or incarcerated have a substance use disorder, mental health issue, and/or serious physical medical condition (Winkelman et al., 2016). When these individuals are unable to access stable sources of care for such conditions, untreated symptoms may lead to criminal behavior. Of the estimated 15 million individuals newly eligible for Medicaid coverage under the ACA, a third have had prior criminal justice involvement (NHCHC, 2013). Expanding health insurance through relaxed Medicaid eligibility requirements is a key mechanism that can facilitate utilization of needed care. In turn, improved management of health conditions may then contribute to reduced rates of crime.[9]

On the other hand, crime could *increase* as a result of expanding health insurance eligiblity. This is most likely to occur among crimes directly related to substance abuse. Among all crimes committed, substance-related crimes are quite common. Nearly 60 percent of all

[7] I include controls for median income and average unemployment rate.

[8] These esimates are comparable to findings by Courtemanche et al. (2016), who show that the proportion of residents with health insurance rose by 5.9 percentage points compared to a 3.0 percentage point increase in non-expansion states the first year after the ACA was implemented. The authors also find coverage gains were concentrated largely among a population newly eligible for Medicaid, namely young low income adults, minorities, and the unmarried. For other studies that assess the impacts of Medicaid expansions at the state and national level on coverage, see Baicker and Finkelstein (2011), Kaestner et al. (2016), and Wherry and Miller (2016).

[9] Another potential mechanism through which expanding health insurance might decrease crime is by decreasing the incentive for would-be criminals who commit crimes with the purpose of getting access to health care in jails or prisons. While there exists anecdotal evidence of this occurring, there are no studies that suggest this type of behavior is common among the population of individuals that commit crimes.

arrestees test positive for some illicit substance at the time of arrest (Bondurant et al., 2016). Expanding health insurance eligiblity makes it easier to access to common prescription medications that may facilitate criminal behavior (e.g. opioids, stimulants, and benzodiazepines). Moreover, substance abuse might increase as a result of an income-effect from lower out-of-pocket spending on health care. Many studies have found evidence that positive income shocks can increase consumption of complements to crime, including alcohol and illicit drugs (Dobkin and Puller (2007); Castellari et al. (2017); Carr and Koppa (2016)). Substance abuse might also increase through *ex ante* moral hazard behavior brought about by a reduction in health risks, though research has found no evidence of this behavior occuring following the ACA (Courtemanche et al. (2017); Simon et al. (2017)).

Finally, expanding insurance eligibility may not necessarily translate into any changes in crime if those newly enrolled continue to face substantial barriers to treatment. These individuals may lack adequate knowledge of the health care system needed to obtain regular access to a primary care physician. According to one study only 12 percent of adults in the US have proficient health literacy (Somers and Mahadevan, 2010). Newly enrolled individuals may also face barriers to treatment due to supply side capacity constraints. It has been argued the health care system in the United States, particularly substance abuse and mental health services, may not have the capacity to meet the increased demand arising from the Medicaid expansions.[10]

3 Data

I utilize two crime-related data sources in this study. Annual state-level crime data was obtained from the FBI's Uniform Crime Reports for the years 2010-2015 (UCR, 2017). This dataset includes state-wide counts of the most commonly reported violent and property crimes, namely criminal homicide, robbery, forcible rape, aggregated assault, burglary, larceny theft, and motor vehicle theft. The construction of this data is based on the number

[10]For more information see Mechanic (2014); Cummings et al. (2014); Bishop et al. (2014).

of crimes reported to the police by all law enforcement agencies operating within a given state during an entire calendar year. County-level crime data was gathered from the Inter-university Consortium for Political and Social Research UCR Program Data Series (ICPSR, 2017). Specifically, I use the *Offenses Known and Clearances by Arrest* files for the years 2010-2015. The categories of reported crimes are the same as the state-level dataset. The ICPSR data are available at the agency level, therefore I first aggregated the reported crime counts to the county-level. For agencies with jurisdictions that extend to multiply counties, the data is aggregated to the primary county that the agency operates in.

Summary statistics for state-level reported crime rates are provided in the top panel of Table 2. Property crimes (burglary, larceny, vehicle theft) make up the large majority of reported crimes in both expansion and non-expansion states. The average reported property crime rate is slightly larger in states that did not expand Medicaid, while the average reported violent crime rate is larger in states that chose to expand. Figure 2 presents aggregate trends in reported crime rates averaged across expansion and non-expansion states. There are two key observations to make regarding these figures. First, the pre-period trends in reported crime rates appear to be quite similar between expansion and non-expansion states. Second, the figures indicate the trends diverge in 2014, the first year that the majority of Medicaid expansions became effective. In particular, it appears the average reported crime rate decreased in expansion states while the trend for non-expansion states either remained constant or increased. The change in trends is most visible for reported violent crime, though the break is noticeable for property crimes as well.

4 Empirical Strategy

4.1 State-Level Analysis

I begin my main empirical analysis by estimating the impact of expanding health insurance eligibility on state-level reported crime rates. Using this quasi-experimental policy variation, I compare the change in reported crime rates in the expansion states to that in the non-expansion states before and after the Medicaid expansions became effective. Specifically, I estimate the following difference-in-differences regression model:

$$Y_{st} = \beta_0 + \beta_1 Expansion_{st} + \mathbf{X}_{st}\gamma_1 + \alpha_s * \mathbf{1}(State_s) + \delta_t * \mathbf{1}(Year_t) + \epsilon_{st}. \tag{1}$$

The outcome variable Y_{st} is defined as the natural log of reported crimes measured per 100,000 people in state s and year t. The main coefficient of interest is β_1 on the variable $Expansion_{st}$, which takes the value 1 if state s expanded Medicaid coverage in time t, and equals 0 otherwise.[11]

In my preferred specification, I include a full set of state and year fixed effects, denoted by $\mathbf{1}(State_s)$ and $\mathbf{1}(Year_t)$, in order to capture time-invariant state-specific unobservables and national time trends. I also include vectors of state-specific demographic and economic covariates indicated by $\mathbf{X}_{st}$. Demographic covariates include the fraction of the population that is white, black, and hispanic, the fraction of the population that is male, and the fraction of the population that is 10-19 years old, 20-29 years old, 30-39 years old, and 40-49 years old. To control for heterogenity in law enforcement across states, I also include the number of law enforcement officers (per 100,000 people) in each state. Economic controls include state unemployment rates, median per-capita income, the fraction of the population below the poverty line, and state government expenditures in several areas to account for the public investment that may help reduce crime.[12] These measures include contemporaneous and one-year lagged spending (per 100,000 people) on matters of police protection and correction, hospital and health, public welfare, and education. Finally, ϵ_{st} represents the error term. Regressions are weighted by the square root of state population to improve precision and

[11] I code treatment for New Hampshire to begin in 2015 as the reform did not become effective until the second-half of 2014. I do not include Alaska as a treatment state as the expansion did not become effective until late 2015.

[12] Population, racial composition, and age distribution data were gathered from the SEER database provided by the National Cancer Institute (2016). Law enforcement officer counts were gathered from ICPSR (2017). Unemployment data was gathered from the Bureau Of Labor Statistics (2016). Income and poverty data were obtained from the Bureau of Economic Analysis (2016). State government expenditures wer compiled by the Census Bureau (2017b) from the Annual Survey of State Government Finances.

standard errors are clustered at the state level to allow for serial correlation within states.

The difference-in-differences framework presented in equation 1 relies on the identifying assumption that trends in reported crime rates between expansion and non-expansion states were parallel in the period prior to treatment. In addition to this assumption, the empirical strategy assumes the decision to expand Medicaid was independent of trends in crime rates, i.e. the Medicaid expansion lead to changes in criminal outcomes, rather than trends in criminal outcomes leading to the decision to expand Medicaid. This concern is not likely to be an cofounding problem here, considering the decision to expand Medicaid benefits was based largely on the partisan composition of state governments and the generosity of the Medicaid program in a given state prior to 2010 (Barrilleaux and Rainey (2014); Lanford and Quadagno (2016)). Nevertheless, I present an event-study specification that both tests for confounding pre-trends as well as captures heterogeneous treatment effects over time. The event-study is defined using the following model:

$$Y_{st} = \sum_{t=-4, t\neq -1}^{2} \lambda_t Expansion_{st} + \mathbf{X}_{st}\gamma_1 + \alpha_s * \mathbf{1}(State_s) + \delta_t * \mathbf{1}(Year_t) + \epsilon_{st}. \quad (2)$$

The coefficients of interest are the λ_t's on the variable $Expansion_{st}$. The model includes four years of pre-expansion and two years of post-expansion coefficients with the year before the policy change place normalized to zero. This specification allows me to check for evidence of confounding pre-trends and possible endogeneity by testing whether the leads are statistically different from zero. The rest of the variables included are the same as Equation 1.

5 Results

5.1 Main Findings

In this section, I discuss results based on the empirical strategy presented in the previous section. Table 3 shows the estimated effects of the Medicaid expansions on state-level reported crime rates. Looking first at column (1), I find no impact on total reported crime rates when including only state and year fixed effects in the regression. After including controls, the estimate from my preferred specification in column (2) suggests states that expanded Medicaid eligibility experienced a 3.3 percent decrease in the annual rate of reported crimes relative to non-expansion states. This estimate is significant at the .05 level and corresponds to a reduction of 106 incidents of reported crimes per 100,000 people.[13]

Considering the total crime rate consists mainly of property crimes, I next dis-aggregate the data in order to investigate the effects of the Medicaid expansion separately for violent and property crime. Turning first to violent crime, results in Table 3 from specifications in columns (3) and (4) indicate between a 5.2 and 6.0 percent reduction in the annual reported violent crime rate in expansion states relative to non-expansion states. Both estimates are statistically significant at the .01 level. The estimate from my preferred specification in column (4) represents a decrease of 22.38 instances of violent crime per 100,000 people based on pre-expansion means. Turning to property crime in columns (5) and (6), the estimates are very similar to total crime, with the point estimate from my preferred specification indicating a marginally significant decrease in annual reported property crime rates by 3.1 percent, or 88.13 instances per 100,000 people. In sum, the results indicate that states that have expanded Medicaid eligibility have experienced statistically significant decreases in reported crime rates, both for violent and property crimes, relative to non-expansion states within the first two years of the reform.

Figure 3 plots estimates of the event-study analysis defined by equation 2. This figures show how reported crime rates evolved over time in the period before and after the Medicaid expansions became effective as indicated by the dashed vertical line. In all three plots, I find the estimated changes in reported crime rates exhibit a sharp decrease after the Medicaid policies became effective in states that expanded Medicaid eligibility relative to those that did not. The obvious trend break supports my identifying assumption that trends

[13]This calculation is based on the pre-2014 total crime rate averaged across all states (approximately 3,216.15 instances of reported crime per 100,000 people) and assumes the effect of the policy is sustained annually.

in reported crime would have remained similar between the expansion and the non-expansion states in the absence of the policy change. The effects on total and reported property crime are relatively constant (negative) across each year after the policy change becomes effective while the effects on reported violent crime appears to be becoming more negative in each year following the reform. Table 4 presents results corresponding to the event-study plots in Figure 3. A full set of fixed effects and state-level controls are included in each regression. Across all specifications I find the coefficient for the year prior to the reform to be marginally different than zero. However, an F-test reveals the pre-period coefficients are not jointly different from zero. This provides further supportive evidence that the common-trends identification assumption is valid.[14]

Tables 5 and 6 present estimated effects for specific crime categories. I perform this exercise to not only to gain a more precise understanding of which types of crimes are most responsive to expanded insurance eligibility, but also given the social costs associated with crime vary greatly across crime types (McCollister et al., 2010).[15] Turning first to violent crimes in Table 5, the estimates from my preferred specification suggest highly significant reductions in reported criminal homicides, aggravated assaults, and robberies. Specifically, I estimate a 10.4 percent reduction in reported criminal homicides, or equivalently 0.46 homicides per 100,000 people. I find a 5.1 percent reduction in reported aggravated assaults, or approximately 12.13 incidents per 100,000 people. For reported robberies, my estimate suggests an annual reduction of 7.7 percent. This estimate corresponds to 7.54 robberies per 100,000 people. I find no evidence that expanding Medicaid eligibility impacted instances of forcible rape.[16] Figure 4 presents plots of the event-study analysis by each violent crime

[14]Moreover, note that the lead estimates are both of the opposite sign of the treatment effect and either constant or trending upward in the expansion states relative to the control states. Thus any confounding trends would bias the post-period estimates against finding a decreasing effect on crime rates.

[15]The variation in social costs is far greater across violent crimes compared to property crimes. In particular, the authors estimate (after adjusting for inflation to 2017 dollars) the total social costs associated with an incident of criminal homicide to be $10.2 million, $122,000 per incident of aggravated assault, $48,000 for robbery, and $274,000 per incident of forcible rape. Incidents of property crime range in costs between $4,000 (larceny) and $16,500 (burglary).

[16]One explanation for this result relates to rape and sexual assault be greatly under-reported relative to other types of violent crime. It has been estimated that only a third of actual rapes are reported (Taylor,

category. For each crime, I can reject the null hypothesis that the joint lead coefficients are statistically different from zero. The corresponding estimates are presented in Table A2.

Estimates for property crime categories are shown in Table 6. Results for reported burglaries are presented in columns (1) and (2). In my preferred specification, the estimate suggests expanding insurance eligibility decreases reported burglaries by approximately 5.7 percent, or 36.71 burglaries per 100,000 people. I find no significant change in reported incidents of larceny. In columns (5) and (6), both specifications indicate a statistically significant decrease in reported vehicle thefts, with my preferred specification suggesting a 12.9 percent reduction, or 26.97 vehicle thefts per 100,000 people. Figure 5 depicts plots of the event-study analysis by each property crime category with corresponding estimates presented in Table A3. Results from a F-test reveal the pre-policy coefficients are not jointly different from zero for larceny and motor vehicle theft. However, there is evidence of possible confounding pre-trends for reported burglaries (joint p-value=0.02). Therefore I refrain from drawing any causal conclusions on the effects of the Medicaid expansions on burglaries.

6 Robustness

6.1 Alternative Specifications

In this section I check the robustness of my state-level findings by providing estimates based on a set of alternative specifications. Table A4 presents results for reported total, violent, and property crimes. Column (1) shows the preferred difference-in-differences estimates. The specification in column (2) take into account the fact that crimes that generate the lowest cost to society (e.g. larcenies and burglaries) occur on average far more frequently that crimes that generate the highest costs, such as homicides. To adjust for this inverse relationship, I weight the incidence of each crime by it's cost relative to the cost of homicides prior to aggregating by total, violent, and property categories. As the results show, the magnitude

2006). Another explanation could be related to the classification of crime types, e.g. a large number of reported rapes are instead (or mistakenly) classified as incidents of assault.

of the estimates are all larger than the estimates from my preferred specification and remain statistically significant. Most notably, after weighting by social costs, the estimate on total crime rates is over twice as large than in column (1).

In column (3), I exclude the few states that chose to expand Medicaid but already had similar Medicaid eligibility requirements to those mandated by the ACA.[17] In addition, I exclude Wisconsin, a state that chose not to expand Medicaid but did receive federal approval to offer Medicaid to childless adults below 100 percent of the federal poverty level through it's state-run program (Gates and Rudowitz, 2014). All results from this restricted-sample specification are very similar to those using my preferred specification, although the estimate for property crime in Panel C is no longer significant. In column (4) I add pre-policy coefficients with the purpose of conditioning for potential endogeneity of state expansion decisions, thus minimizing concerns regarding reverse causality biasing the estimated average treatment effects. The magnifitude of the point estimates and standard errors decrease slightly using this specification, but overall are quite comparable to column (1).

In column (5) I add region-by-year fixed effects to capture annual, region-specific shocks that may impact criminal behavior (e.g. weather or pollution shocks).[18] However, including these controls may come at the high cost of reduced ability to accurately separate effects in expansion vs. non-expansion states. Indeed, the estimates on total and property crime are no longer signficant after including the controls. The estimate on violent crime remains large and highly signficant. In column (6) I explore adding treatment-specific linear time trends to the preferred model. This specification is intended to control for unobservable variables specific to Medicaid expansion and non-expansion states that evolve linearly over time (e.g. attidutes toward criminal activity.)[19] All estimates are highly significant when including the

[17] These states include Delaware, Massachusetts, New York, Vermont, and the District of Columbia (Ghosh et al., 2017).

[18] Census regions include Northeast, Midwest, South, and West.

[19] An alternative would be adding state-specific linear time trends. However, controlling for these trends will bias the point estimates if the treatment itself affects the trend in reported crime rates (Wolfers (2006); Meer and West (2015); Kaufman (2017)). By including treatment-specific trends, I capture unobserved trends with any bias likely to be much smaller.

linear trends. While the effect on violent crime is consistent with the estimates from the preferred specification, the evidence from column (6) for property crime suggests that the results from my preferred specification, which does not account for underlying trends, may be *underestimating* the true impact. Finally, in column (7) I accommodate the count-nature of the crime data by using Poisson regression. This specifications reveal highly significant and larger point estimates than my preferred specification using WLS and logged crime rates.

Tables A5 and A6 present alternative specifications across specific crime categories. For violent crimes, the estimates are generally robust to all alternative specifications. The one exception is the estimate for aggravated assaults become insignificant after including linear trends. The property crime estimates are also quite robust. Overall the robustness checks in both tables provide evidence that the state-level results are not driven by a variety of potentially confounding issues, with the large majority of estimates robust to all alternative specifications.

6.2 Placebo Test

In addition to the alternative specifications, I also conduct a series of placebo exercises. Specifically, I restrict the sample years to the pre-expansion period (2010-2013) and repeat the main analysis using three different pre- and post-period combinations. Since the Medicaid expansions did not begin until 2014, I do not expect to observe statistically significant estimates from these placebo pairs. Results are presented in Table A7, A8, A9. Indeed, I do not find any significant effects on reported crime rates.

7 Treatment Heterogeneity

7.1 Triple Difference Strategy

The state-level analysis allows me to estimate intent-to-treat effects of expanding health insurance eligibility on reported crime rates at the geographic level of the policy variation. I next expand on this analysis to explore whether the state-level estimates are most pronounced in areas with higher pre-expansion uninsured levels among individuals eligible for Medicaid. Specifically, I combine reported crime data with county-level uninsured rates between 2010 and 2015. I then check whether the effects on reported crime are larger in counties with higher pre-reform uninsured levels. It is in these counties that I expect larger gains in coverage. This approach is similar to that utilized by Courtemanche et al. (2016) and Ghosh et al. (2017) who explore the effects of the ACA Medicaid expansions on insurance status and prescription drug use. Figure 6 illustrates the variation in the baseline uninsured rate across counties in states that did and did not expand Medicaid.

To conduct the county-level heterogeneity analysis, I estimate the following difference-in-differences-in-differences equation:

$$\begin{aligned} Y_{cst} =& \beta_0 + \lambda_1 Expansion_{st} + \lambda_2 BaselineUninsured_{cs} \\ & + \lambda_3 Expansion_{st} * BaselineUninsured_{cs} + \mathbf{X}_{cst}\gamma_1 \\ & + \alpha_c * \mathbf{1}(County_c) + \delta_t * \mathbf{1}(Year_t) + \theta_{sy} + \epsilon_{cst}. \end{aligned} \tag{3}$$

The outcome variable is the natural log of the number of reported crimes in county c in state s in year t (plus 1) per 100,000 people. Defining the outcome variable in this way insures that counties that experienced zero reported crimes in a given year are not dropped from the sample. The variable $BaselineUninsured_{cs}$ indicates the uninsured rate in 2013, the year prior to the expansions becoming effective, for the "Medicaid eligible" population in county c in state s, i.e. those who are ages 18-65 with incomes at or below 138 percent of the federal poverty level.[20] The coefficient of interest is λ_3 on the interaction $Expansion_{st} * BaselineUninsured_{cs}$, which represents the differential change in reported crime rates in expansion-state counties with larger pre-reform uninsured rates relative to those with a lower uninsured population, with counties in states that did not expand Medicaid in the control group. This setup allows me to compare Medicaid-induced changes in the reported

[20] In the few states where I code the Medicaid expansions becoming effective in 2015, I use the uninsured rate in 2014.

crime rates between counties within the same states. I expect that there would be a more pronounced impact in counties where the treatment is predicted to be most intense, i.e. $\lambda_3 < 0$.

The triple-difference specification also includes county-level demographic and economic covariates in $\mathbf{X}_{cst}$. The choice of control variables are the same as those included in the state-level analysis, only dis-aggregated to the county-level. The one exception is the state-level expenditures. Instead of including these controls, I add state-by-year fixed effects, θ_{sy}. These fixed effects capture unobserved state-specific shocks, including changes in annual state-expenditures. Also included are county and year fixed effects, indicated by $\mathbf{1}(County_c)$ and $\mathbf{1}(Year_t)$, respectfully. The regression is weighted by the square root of county population and standard errors are clustered at the county-level to allow for the error term to be correlated within a given county.

7.2 Results

Table 7 presents results from the treatment heterogeneity analysis. The estimate in column (1) indicates that in Medicaid expansion states the decrease in total reported crime is 0.4 percent larger in counties where the pre-expansion uninsured rate among individuals newly eligible for Medicaid is one percent larger, relative to similar counties in non-expansion states. Turning to column (2), I find that the decreases in reported violent crime is 0.3 percent greater in counties with a larger pre-reform uninsured population. The estimate for property crime shown in column (3) is identical to the total crime result. All three estimates are statistically significant.

In Table A10 I explore treatment heterogeneity by violent crime categories. Across all categories, the coefficients are estimated imprecisely. However, given that specific violent crimes occur relatively infrequently in a given county there is likely not enough power to identify any causal effects in this setting. Results for property crimes are reported in Table A11. Here I find highly significant decreases in reported burglaries, larcenies, and vehicle thefts. All estimates indicate a 0.4 percent decrease in counties that had a 1 percent larger pre-expansion uninsured rate. In sum, the results from the heterogeneity analysis provide evidence that the decreases in state-level crime rates presented in section 5.1 were greatest within counties where treatment is likely to be most intense.

8 Social-Cost Savings

In the final exercise of this study, I present a back-of the envelope calculation of the social cost savings associated with the estimated decreases in reported crime rates among Medicaid expansion states. To perform this exercise, I utilize inflation-adjusted cost estimates from McCollister et al. (2010).[21] I combine these cost estimates with the estimated number of reduced incidents of crime based on pre-expansion means. In my calculation, I only include crime categories that are estimated to be statistically different from zero using my preferred regression specification. These include incidents of criminal homicide, aggravated assault, robbery, and vehicle theft.[22]

Table 8 presents the estimated decrease in instances of crimes in states that expanded Medicaid eligibility and the corresponding social cost estimates associated with each crime category. I estimate that the Medicaid expansion accounts for an annual state-level reduction of 29.89 criminal homicides, 788.09 aggravated assaults, 489.87 robberies, and 1752.24 motor vehicle thefts.[23] This amounts to an average cost savings of $403 million per year in each state that expanded coverage. After multiplying this figure by the total number of expansion states in the sample, the total annual social cost savings is nearly $13 billion.

[21] McCollister et al. (2010) calculate cost estimates that incorporate tangible and intangible losses associated with more than a dozen major crime categories, including all specific crime categories included in this study. All estimates adjusted to 2017 dollars.

[22] I do not include incidents of burglary in this calculation as I find evidence that the estimate are potentially biased by confounding pre-trends.

[23] These estimates are calculated by combining the estimated annual change in reported crimes per-capita (100,000 people) with the average population covered in expansion states.

9 Discussion

9.1 Mechanisms

There are several potential mechanisms through which expanding health insurance eligibility may manifest into reductions in crime. The most likely channel is improvements in utilization of treatment. This channel is consistent with a dense literature that has shown treatment utilization has risen disproportionately in states that expanded Medicaid coverage relative to states that did not (Ghosh et al. (2017); Simon et al. (2017); Wherry and Miller (2016); Sommers et al. (2016)). The increase has been documented across a variety of treatments, including services targeting substance abuse (Maclean and Saloner (2017); Wen et al. (2017a)) and mental illness (Maclean et al. (2017). The findings presented in this paper are also consistent with recent work by Wen et al. (2017b) and Bondurant et al. (2016) who find evidence of a causal link between substance use disorder treatment utilization and reduced local-level criminal activity.

A second potential mechanism is an income effect, specifically improved financial well-being among the population newly eligible for Medicaid coverage. One hypothesis that is inline with a reduction in crime posits that having more wealth reduces stress on individuals and families, which then lead to a reduction in criminal behavior. Indeed, recent work has shown that the Medicaid expansions significantly reduced the number of unpaid bills and increased credit scores for the low-income and previously uninsured while also improving self-assessed mental health. (Hu et al. (2016); Brevoort et al. (2017); Courtemanche et al. (2017)).[24] Moreover, Courtemanche et al. (2017) find decreasing effects of the Medicaid expansions on alcohol consumption, evidence that the income effect is not leading to higher consumption of certain goods associated with criminal behavior.

[24] Similar findings of positive effects on consumer financial wellbeing following Medicaid eligiblity expansions are documented by Gross and Notowidigdo (2011); Finkelstein et al. (2012); and Baicker et al. (2013).

9.2 Conclusion

Low income individuals are both at high risk of committing crimes and are less likely to have health insurance coverage. The ACA Medicaid expansions have allowed many of these individuals to obtain insurance coverage, a necessity for most to utilize effective treatment for health conditions that may exacerbate criminal behavior. This paper investigated the causal effects of increasing health insurance coverage on reported crime rates. I find that in states that expanded Medicaid eligibility, reported crime has significantly decreased relative to non-expansion states. The analysis shows significant reductions for both property and violent crime, as well as across many specific crimes including criminal homicide, aggravated assault, robbery, and motor vehicle theft. The estimates are generally robust across a variety of sensitivity checks and placebo tests. In the second half of the analysis, I investigate treatment heterogeneity using county-level reported crime data. I find evidence that within states that expanded Medicaid eligibility, reductions in reported crime were largest in counties that had higher pre-expansion uninsured levels among individuals eligible for Medicaid. The estimated social cost savings attributed to the decrease in crime is over $12.9 billion annually.

There are several limitations in this study that inspire future research. First, more work is needed to better understand the key mechanisms. For example, it remains unclear how much of the reduction in reported crime is explained by health care utilization among certain populations, particularly those who have previously been arrested and incarcerated. Second, the empirical strategy utilized in this paper does not allow me to separate the broader intent-to-treat effects at the state and county levels from the effects on individuals who received coverage. Finally, this paper focuses on the contemporaneous relationship between health insurance eligibility and crime. However, a growing body of work suggests there are substantial long-term impacts of public health insurance on health and labor market outcomes.[25] The long-term impact of health insurance eligibility on criminal behavior remains unknown

[25] For example, see Brown et al. (2015); Boudreaux et al. (2016); Miller and Wherry (2016); Cohodes et al. (2016); Thompson (2017).

and may prove to be a promising area of research.

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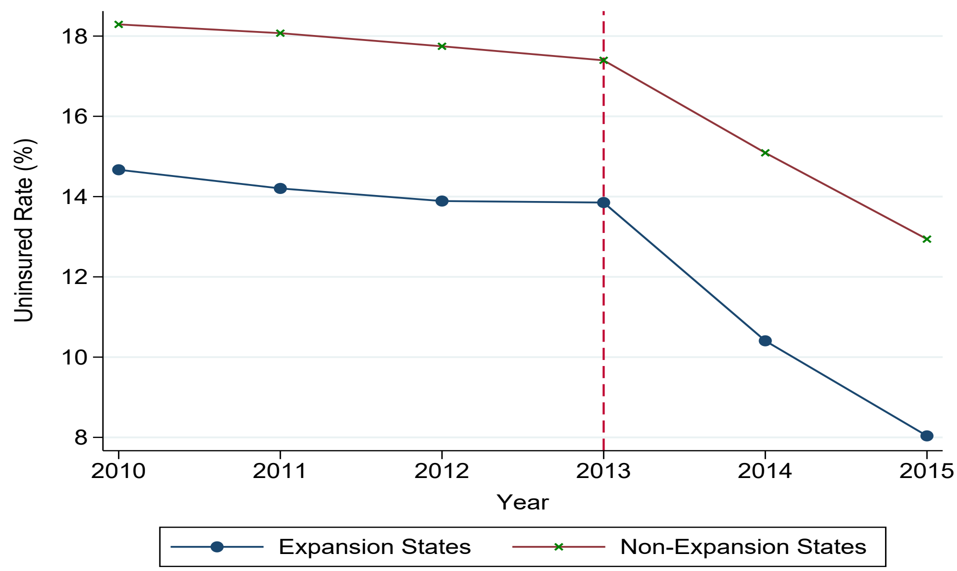
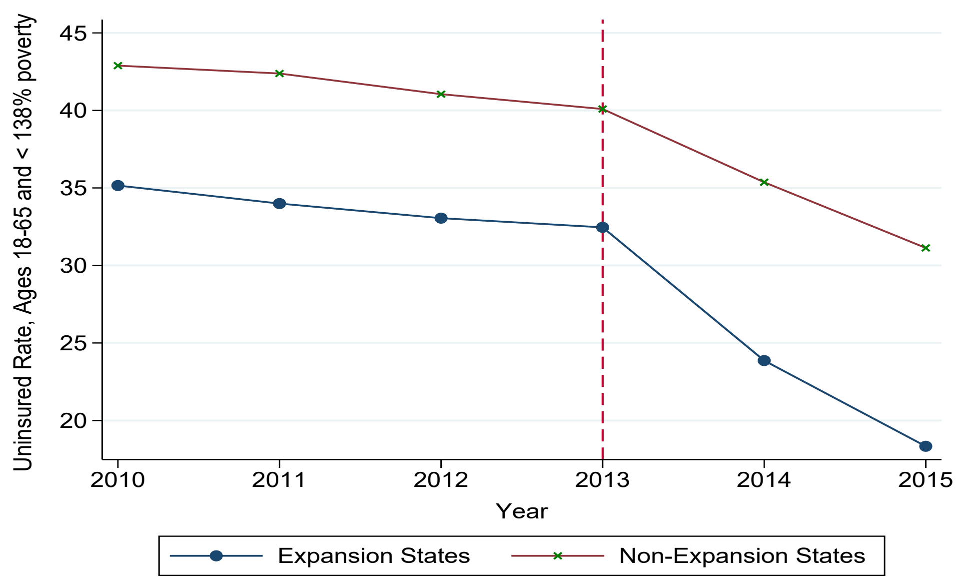
Figure 1: Trends in Uninsured Rates Among Medicaid Expansion and Non-Expansion States

(a) Total Population



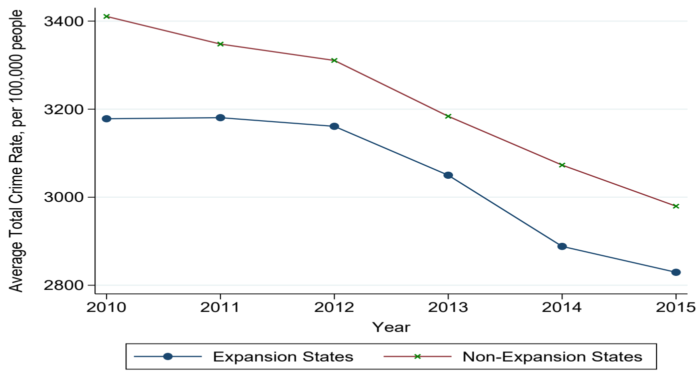
(b) Adults Age 18-65 and <138% Poverty Level



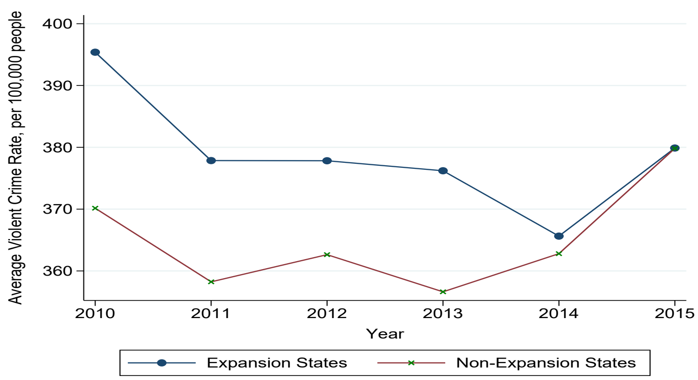
Notes: Figure 1(a) shows the average uninsured rate over time among states that did and did not expand Medicaid eligibility. Figure 2(b) shows uninsured rates among adults ages 18-65 with incomes at or below than 138% of the federal poverty limit. Data gathered from the U.S. Census Bureau Small Area Health Insurance Estimates.

Figure 2: Trends in Crime Rates Among Medicaid Expansion and Non-Expansion States

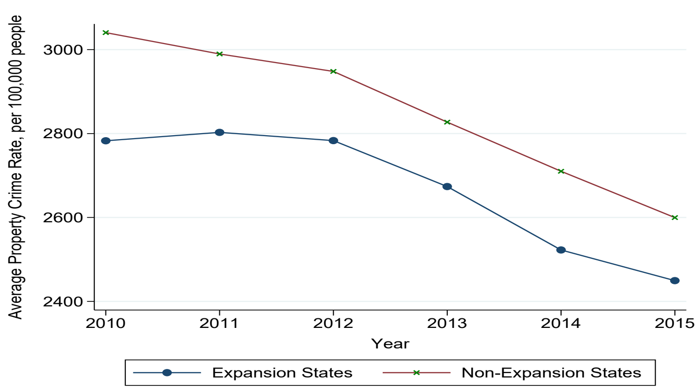
(a) Average Total Crime Rate



(b) Average Violent Crime Rate



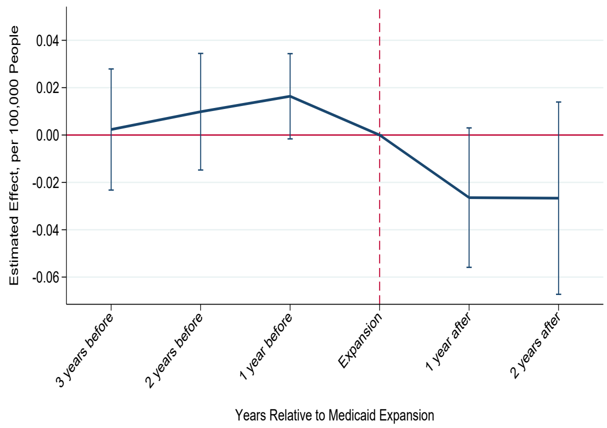
(c) Average Property Crime Rate



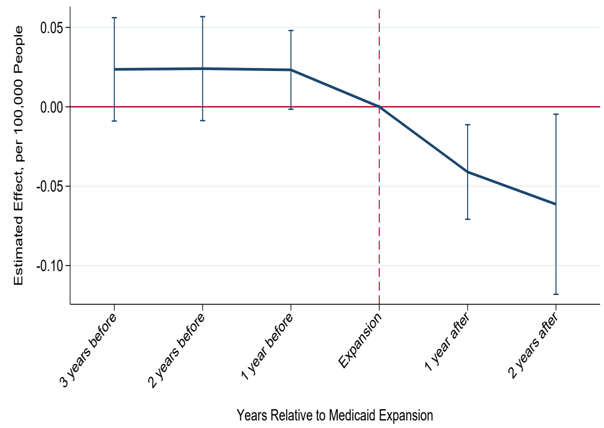
Notes: Figure 2(a) shows the average total crime rate per 100,000 people in states that did and did not expand Medicaid eligibility. Figure 2(b) shows the average violent crime rate per 100,000 people. Figure 2(c) shows the average property crime rate per 100,000 people. Each sample period is from 2010-2015.

Figure 3: Event-study plot of Medicaid expansion on aggregated crime rates

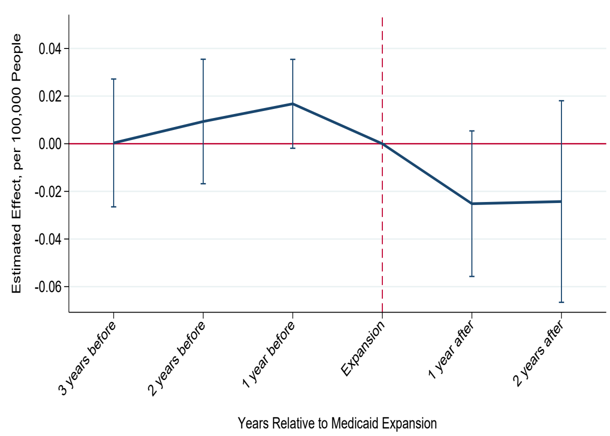
(a) Total Crime



(b) Violent Crime



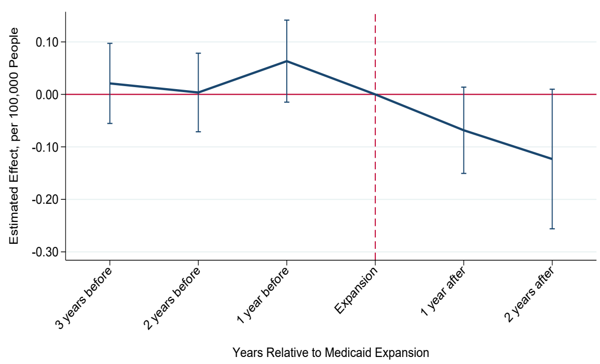
(c) Property Crime



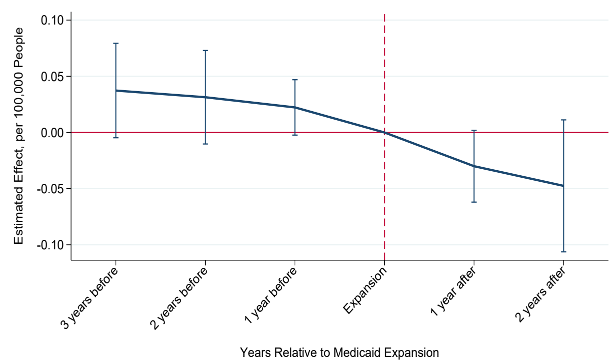
Notes: All models estimated using OLS where regressions are weighted by the square root of state population. The sample includes 50 states (plus District of Columbia) between 2010-2015. Vertical line indicates the end of the year before Medicaid expansions became effective. Bands indicate 95% confidence intervals. Standard errors are clustered by state.

Figure 4: Event-study plot of Medicaid expansion on violent crime categories

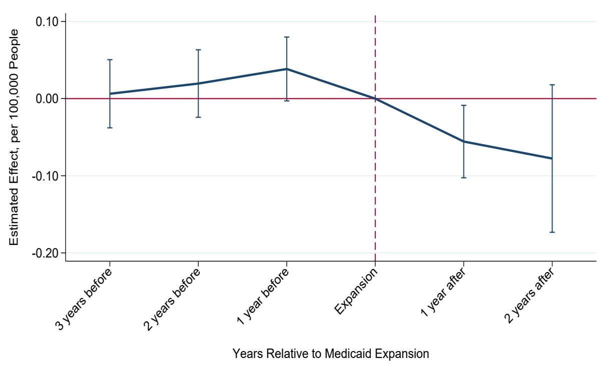
(a) Criminal Homicide



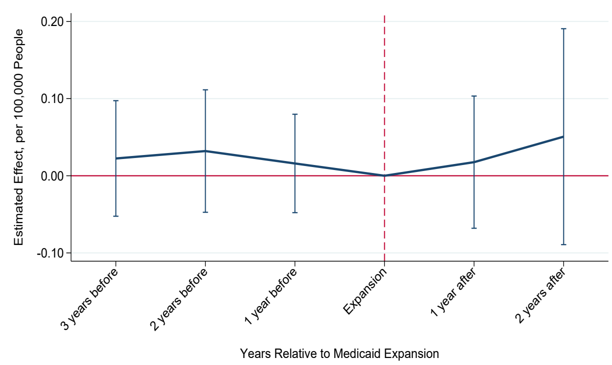
(b) Aggravated Assault



(c) Robbery



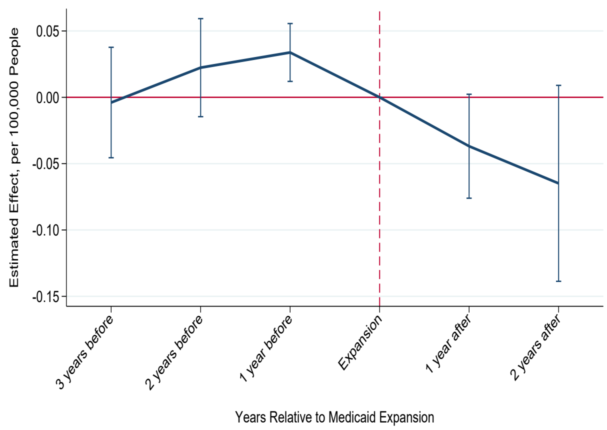
(d) Forcible Rape



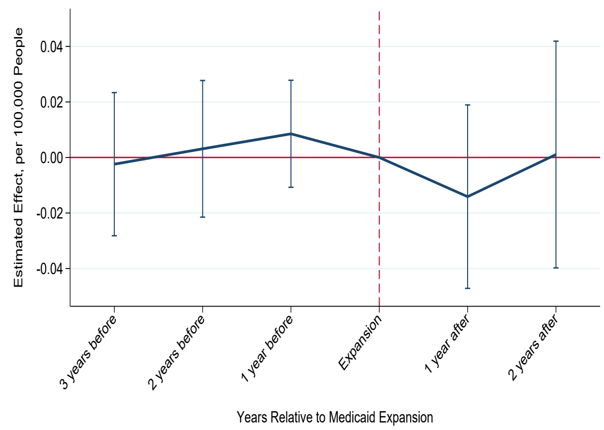
Notes: All models estimated using OLS where regressions are weighted by the square root of state population. The sample includes 50 states (plus District of Columbia) between 2010-2015. Vertical line indicates the end of the year before Medicaid expansions became effective. Bands indicate 95% confidence intervals. Standard errors are clustered by state.

Figure 5: Event-study plot of Medicaid expansion on property crime categories

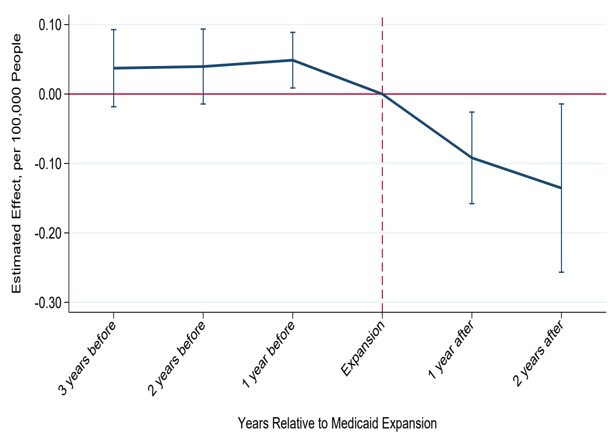
(a) Burglary



(b) Larceny



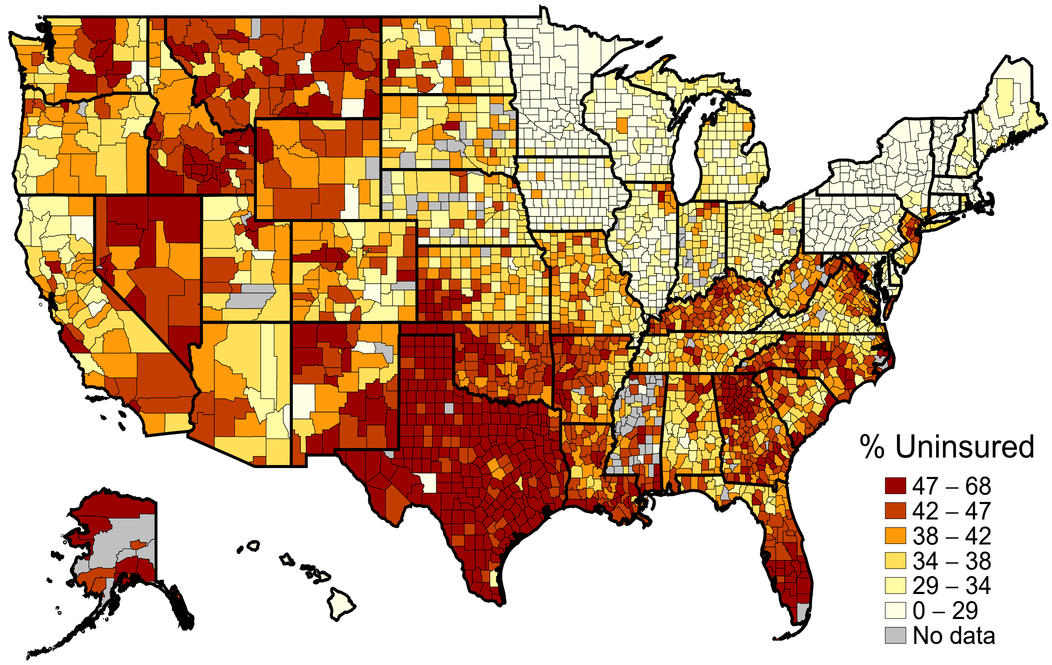
(c) Motor Vehicle Theft



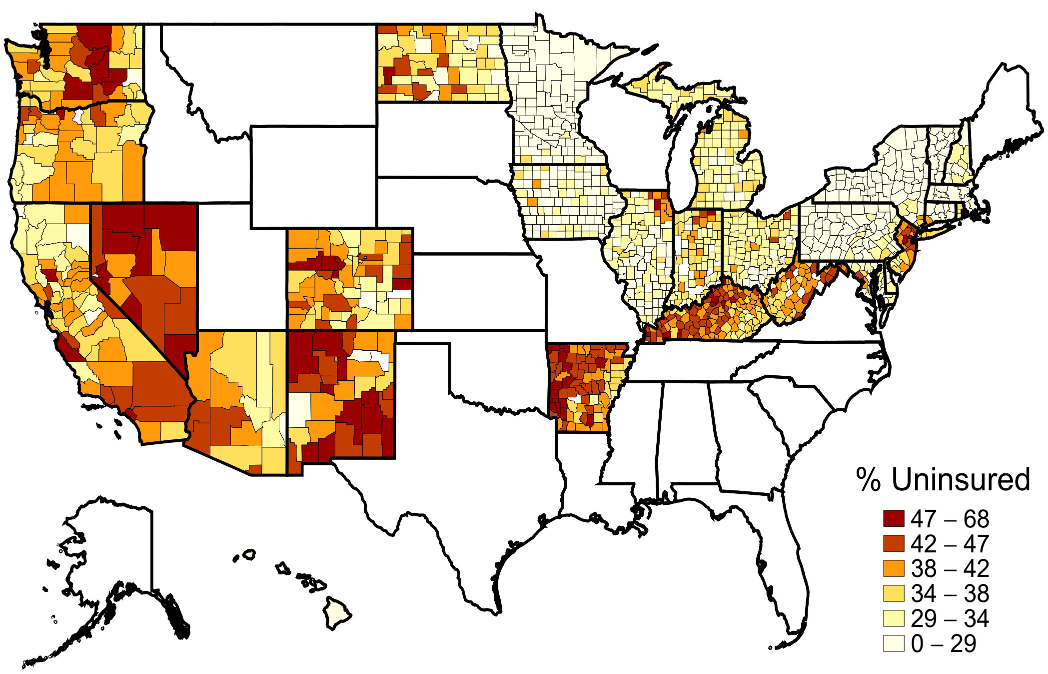
Notes: All models estimated using OLS where regressions are weighted by the square root of state population. The sample includes 50 states (plus District of Columbia) between 2010-2015. Vertical line indicates the end of the year before Medicaid expansions became effective. Bands indicate 95% confidence intervals. Standard errors are clustered by state.

Figure 6: Baseline Uninsured Rate Among Adults Age 18-65 and <138% Poverty Level

(a) All States



(b) Medicaid Expansion States



Notes: Figure 6(a) shows the county-level uninsured rate among adults ages 18-65 with incomes at or below than 138% of the federal poverty limits in 2013. For New Hampshire, Indiana, and Pennsylvania, the figure shows the the 2014 uninsured rate. Figure 6(b) shows only the baseline uninsured rate for states that expanded Medicaid during the sample period.

Table 1: State Medicaid Expansions: 2010-2017

State	**Expansion Date**
Adopted Medicaid Expansion	
Alaska	9/1/2015
Arizona	1/1/2014
Arkansas	1/1/2014
California	1/1/2014
Colorado	1/1/2014
Connecticut	1/1/2014
Delaware*	1/1/2014
District of Columbia*	1/1/2014
Hawaii	1/1/2014
Illinois	1/1/2014
Indiana	2/1/2015
Iowa	1/1/2014
Kentucky	1/1/2014
Louisiana	7/1/2016
Maryland	1/1/2014
Massachusetts*	1/1/2014
Michigan	4/1/2014
Minnesota	1/1/2014
Montana	1/1/2016
Nevada	1/1/2014
New Hampshire	8/15/2014
New Jersey	1/1/2014
New Mexico	1/1/2014
New York*	1/1/2014
North Dakota	1/1/2014
Ohio	1/1/2014
Oregon	1/1/2014
Pennsylvania	1/1/2015
Rhode Island	1/1/2014
Vermont*	1/1/2014
Washington	1/1/2014
West Virginia	1/1/2014

Notes: Expansion dates obtained from Maclean and Saloner (2017) and Kaiser Family Foundation. * indicates states that had comparative eligibility requirements to the ACA Medicaid coverage prior to adopting the expansions.

Table 2: Summary Statistics

Variables	*Expansion States* Mean (SD)	*Non-Expansion States* Mean (SD)
Dependent Variables		
Total Crime (per 100,000 people)	3047.92 (855.67)	3217.60 (673.55)
Violent Crime (per 100,000 people)	378.80 (206.84)	365.04 (141.84)
Criminal Homicide	4.34 (3.19)	4.67 (2.24)
Aggravated Assault	228.38 (109.14)	248.87 (108.56)
Robbery	111.31 (107.51)	73.74 (38.15)
Forcible Rape	34.78 (13.05)	37.76 (16.65)
Property Crime (per 100,000 people)	2669.12 (703.18)	2852.55 (570.47)
Burglary	537.31 (193.55)	631.80 (240.10)
Larceny	1878.28 (501.07)	2027.82 (330.51)
Vehicle Theft	217.53 (121.94)	192.93 (65.95)
Covariates		
Population (100,000)	64.97 (74.39)	57.59 (61.79)
% Age 10-19	12.99 (0.01)	13.49 (0.01)
% Age 20-29	14.04 (0.02)	14.03 (0.01)
% Age 30-39	12.80 (0.01)	12.77 (0.01)
% Age 40-49	13.34 (0.01)	12.87 (0.01)
% Male	49.23 (0.01)	49.51 (0.01)
% Hispanic	12.64 (11.01)	9.06 (7.95)
% White	69.57 (18.29)	72.51 (12.67)
% Black	10.17 (9.91)	13.36 (11.59)
% Unemployed	7.23 (2.14)	6.66 (1.94)
% Poverty	14.39 (3.11)	15.63 (3.10)
Police Officers (per 100,000 people)	245.32 (113.64)	238.72 (55.44)
Median Income ($1,000 per capita)	55.42 (9.17)	49.30 (7.26)
Policing Expenditures ($1,000 per 100,000 people)	0.25 (0.21)	0.21 (0.10)
Health Expenditures ($1,000 per 100,000 people)	0.45 (0.23)	0.40 (0.15)
Welfare Expenditures ($1,000 per 100,000 people)	1.89 (0.69)	1.45 (0.40)
Education Expenditures ($1,000 per 100,000 people)	2.20 (0.60)	1.95 (0.51)

Notes: Above table presents state by year summary statistics among states that did and did not expand Medicaid benefits between the years 2010-2015.

Table 3: Effect of ACA Medicaid Expansion on Crime Rates

	Total Crime		*Violent Crime*		*Property Crime*	
	(1)	(2)	(3)	(4)	(5)	(6)
Expansion	-0.008	-0.033**	-0.052***	-0.060***	-0.005	-0.031*
	(0.020)	(0.016)	(0.019)	(0.017)	(0.021)	(0.017)
Controls	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	51	51	51	51	51	51
Observations	306	306	306	306	306	306

Notes: Models estimated using OLS where regressions are weighted by the square root of state population. The sample includes 50 states (plus District of Columbia) between 2010-2015. The dependent variable is defined as the natural log of reported total, violent, and property crimes per 100,000 people, respectively. All specifications include state and year fixed effects. Columns (2), (4), and (6) add state-specific controls. Robust standard errors clustered at the state level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table 4: Event Study Crime Rate Estimates

	Total Crime (1)	*Violent Crime* (2)	*Property Crime* (3)
Expansion x 3 years before	0.002	0.024	0.000
	(0.013)	(0.016)	(0.013)
Expansion x 2 years before	0.010	0.024	0.009
	(0.012)	(0.016)	(0.013)
Expansion x 1 year before	0.016*	0.023*	0.017*
	(0.009)	(0.012)	(0.009)
Expansion x 1 year after	-0.026*	-0.041***	-0.025*
	(0.015)	(0.015)	(0.015)
Expansion x 2 years after	-0.027	-0.061**	-0.024
	(0.020)	(0.028)	(0.021)
P-Value: Joint Lead Test	0.34	0.23	0.33
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Clusters	51	51	51
Observations	306	306	306

Notes: Models estimated using OLS where regressions are weighted by the square root of state population. The sample includes 50 states (plus District of Columbia) between 2010-2015. The dependent variable is defined as the natural log of reported total, violent, and property crimes reported per 100,000 people, respectively. All specifications include state and year fixed effects, as well as state-specific controls. Robust standard errors clustered at the state level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table 5: Effect of ACA Medicaid Expansion on Violent Crime Categories

	Homicide		*Assault*		*Robbery*		*Rape*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Expansion	-0.081**	-0.104**	-0.042**	-0.051***	-0.063**	-0.077**	0.007	0.013
	(0.034)	(0.041)	(0.021)	(0.017)	(0.031)	(0.030)	(0.044)	(0.045)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	51	51	51	51	51	51	51	51
Observations	306	306	306	306	306	306	306	306

Notes: Models estimated using OLS where regressions are weighted by the square root of state population. The sample includes 50 states (plus District of Columbia) between 2010-2015. In each column, the dependent variable is defined as the natural log of the indicated reported violent crime per 100,000 people. All specifications include state and year fixed effects. Columns (2), (4), (6), and (8) add state-specific controls. Robust standard errors clustered at the state level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table 6: Effect of Medicaid Expansions on Property Crime Categories

	Burglary		*Larceny*		*Vehicle Theft*	
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Expansion	-0.025	-0.057**	0.007	-0.013	-0.095**	-0.129***
	(0.028)	(0.026)	(0.020)	(0.018)	(0.046)	(0.044)
Controls	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	51	51	51	51	51	51
Observations	306	306	306	306	306	306

Notes: Models estimated using OLS where regressions are weighted by the square root of state population. The sample includes 50 states (plus District of Columbia) between 2010-2015. In each column, the dependent variable is defined as the natural log of the indicated reported property crime per 100,000 people. All specifications include state and year fixed effects. Columns (2), (4), and (6) add state-specific controls. Robust standard errors clustered at the state level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table 7: Triple Difference Effect of Changes in County-Level Crime Rates

	Total Crime (1)	*Violent Crime* (2)	*Property Crime* (3)
Expansion x Baseline	-0.004***	-0.003*	-0.004***
	(0.001)	(0.002)	(0.001)
Controls	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes
Clusters	3060	3060	3060
Observations	18146	18146	18146

Notes: Models estimated using OLS where regressions are weighted by the square root of county population. In Columns (1)-(3) the dependent variable is defined as the natural log of reported total, violent, and property crimes (plus 1) per 100,000 people, respectively. Each specification includes includes county, year, and state-by-year fixed-effects as well as county-specific controls. Robust standard errors clustered at the county level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table 8: Estimated Annual Social Cost Savings

Crime	**Incidents**	**Cost Per Incident**	**Total Cost**
Criminal Homicide	29.89	$10,213,002	$305,227,820.37
Aggravated Assault	788.09	$121,675	$95,890,376.22
Robbery	489.87	$48,104	$23,564,889.28
Motor Vehicle Theft	1,752.24	$12,247	$21,459,694.30
Total (per state)			$446,142,780.17
Aggregated Total			**$12,938,140,625**

Notes: All dollar amounts in 2017 dollars. Per-state cost estimates are calculated by combining the estimated annual change in reported crimes per-capita with the average population in expansion states. Aggregated total calculated by muliplying the per-state total by number of states that expanded Medicaid in sample period (including District of Columbia). Data for estimated cost per incident obtained from McCollister et al. (2010).

Appendix Tables

Table A1: Effect of ACA Medicaid Expansion on State Uninsured Rates

	Total Population		*Medicaid Eligible*	
	(1)	(2)	(3)	(4)
Expansion	-0.012**	-0.011***	-0.044***	-0.042***
	(0.005)	(0.005)	(0.011)	(0.011)
Controls	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Clusters	51	51	51	51
Observations	306	306	306	306

Notes: Models estimated using OLS. In columns (1) and (2), the dependent variable is defined as the total state uninsured rate. In columns (3) and (4), the dependent variable is defined as the uninsured rate among adults age 18-65 with incomes at or below 138% of the federal poverty level. All specifications include state and year fixed effects. Columns (2) and (4) add control variables. Robust standard errors clustered at the state level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table A2: Event Study Violent Crime Rate Estimates

	Homicide (1)	*Assault* (2)	*Robbery* (3)	*Rape* (4)
Expansion x 3 years before	0.021	0.037*	0.006	0.022
	(0.038)	(0.021)	(0.022)	(0.037)
Expansion x 2 years before	0.004	0.031	0.020	0.032
	(0.037)	(0.021)	(0.022)	(0.040)
Expansion x 1 year before	0.063	0.022*	0.038*	0.016
	(0.039)	(0.012)	(0.021)	(0.032)
Expansion x 1 year after	-0.068*	-0.030*	-0.056**	0.018
	(0.041)	(0.016)	(0.023)	(0.043)
Expansion x 2 years after	-0.123*	-0.048	-0.078	0.051
	(0.066)	(0.029)	(0.048)	(0.070)
P-Value: Joint Lead Test	0.42	0.17	0.30	0.87
Controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Clusters	51	51	51	51
Observations	306	306	306	306

Notes: Models estimated using OLS where regressions are weighted by the square root of state population. The sample includes 50 states (plus District of Columbia) between 2010-2015. In each column, the dependent variable is defined as the natural log of the indicated reported violent crime per 100,000 people. All specifications include state and year fixed effects, as well as state-specific controls. Robust standard errors clustered at the state level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table A3: Event Study Property Crime Rate Estimates

	Burglary (1)	*Larceny* (2)	*Vehicle Theft* (3)
Expansion x 3 years before	-0.004	-0.002	0.037
	(0.021)	(0.013)	(0.028)
Expansion x 2 years before	0.022	0.003	0.040
	(0.018)	(0.012)	(0.027)
Expansion x 1 year before	0.034***	0.009	0.049**
	(0.011)	(0.010)	(0.020)
Expansion x 1 year after	-0.037*	-0.014	-0.092***
	(0.020)	(0.016)	(0.033)
Expansion x 2 years after	-0.065*	0.001	-0.135**
	(0.037)	(0.020)	(0.060)
P-Value: Joint Lead Test	0.02	0.77	0.11
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Clusters	51	51	51
Observations	306	306	306

Notes: Models estimated using OLS where regressions are weighted by the square root of state population. The sample includes 50 states (plus District of Columbia) between 2010-2015. In each column, the dependent variable is defined as the natural log of the indicated reported property crime per 100,000 people. All specifications include state and year fixed effects, as well as state-specific controls. Robust standard errors clustered at the state level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table A4: Alternative Specifications: Effect of Medicaid Expansions on Crime

	Preferred (1)	*Weighted* (2)	*Restricted* (3)	*Policy Leads* (4)	*Reg-Yr FE* (5)	*Trends* (6)	*Poisson* (7)
Panel A: Total Crime							
Expansion	-0.033**	-0.072***	-0.030*	-0.026*	-0.015	-0.042**	-0.058***
	(0.016)	(0.021)	(0.018)	(0.014)	(0.013)	(0.017)	(0.016)
Panel B: Violent Crime							
Expansion	-0.060***	-0.076***	-0.054***	-0.046***	-0.062***	-0.051**	-0.069***
	(0.017)	(0.023)	(0.016)	(0.016)	(0.017)	(0.021)	(0.016)
Panel C: Property Crime							
Expansion	-0.031*	-0.047**	-0.029	-0.025*	-0.010	-0.042**	-0.057***
	(0.017)	(0.019)	(0.019)	(0.015)	(0.014)	(0.019)	(0.016)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	No	No	No	No	Yes	No	No
Linear Trends	No	No	No	No	No	Yes	No
Clusters	51	51	45	51	51	51	51
Observations	306	306	270	306	306	306	306

Notes: The sample includes 50 states (plus District of Columbia) between 2010-2015. Each panel-column is a separate regression. Models (1)-(5) estimated using OLS where regressions are weighted by the square root of state population. Column (1) shows the preferred difference-in-differences specification. Column (2) weights the incident of each crime by the social cost relative to homicides. Column (3) excludes states that had comparable Medicaid eligibility requirements prior to 2014 and Wisconsin. Column (4) includes pre-policy lead coefficients. Column (5) includes region-by-year fixed effects. Column (6) includes group-specific linear time trends to the preferred specification. Column (7) defines the outcome variable as the count of the reported crime and estimates the model using Poisson regression. Robust standard errors clustered at the state level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table A5: Alternative Specifications: Effect of Medicaid Expansions on Violent Crime

	Preferred (1)	*Restricted* (2)	*Policy Leads* (3)	*Reg-Yr FE* (4)	*Trends* (5)	*Poisson* (6)
Panel A: Criminal Homicide						
Expansion	-0.104**	-0.100**	-0.082*	-0.079*	-0.082*	-0.122***
	(0.041)	(0.041)	(0.045)	(0.042)	(0.044)	(0.031)
Panel B: Aggravated Assault						
Expansion	-0.051***	-0.046***	-0.035**	-0.054***	-0.030	-0.052***
	(0.017)	(0.017)	(0.016)	(0.017)	(0.023)	(0.016)
Panel C: Robbery						
Expansion	-0.077**	-0.063**	-0.061**	-0.072**	-0.092***	-0.110***
	(0.030)	(0.031)	(0.027)	(0.030)	(0.028)	(0.021)
Panel D: Forcible Rape						
Expansion	0.013	-0.030	0.026	0.008	0.029	-0.009
	(0.045)	(0.033)	(0.048)	(0.041)	(0.055)	(0.038)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	No	No	No	Yes	No	No
Linear Trends	No	No	No	No	Yes	No
Clusters	51	45	51	51	51	51
Observations	306	270	306	306	306	306

Notes: The sample includes 50 states (plus District of Columbia) between 2010-2015. Each panel-column is a separate regression. Models (1)-(5) estimated using OLS where regressions are weighted by the square root of state population. Column (1) shows the preferred difference-in-differences specification. Column (2) excludes states that had comparable Medicaid eligibility requirements prior to 2014 and Wisconsin. Column (3) includes pre-policy lead coefficients. Column (4) includes region-by-year fixed effects. Column (5) includes group-specific linear time trends to the preferred specification. Column (6) defines the outcome variable as the count of the reported crime and estimates the model using Poisson regression. Robust standard errors clustered at the state level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table A6: Alternative Specifications: Effect of Medicaid Expansions on Property Crime

	Preferred (1)	*Restricted* (2)	*Policy Leads* (3)	*Reg-Yr FE* (4)	*Trends* (5)	*Poisson* (6)
Panel A: Burglary						
Expansion	-0.057**	-0.050*	-0.044*	-0.044**	-0.066***	-0.079***
	(0.026)	(0.027)	(0.023)	(0.020)	(0.022)	(0.016)
Panel B: Larceny						
Expansion	-0.013	-0.013	-0.010	0.013	-0.027	-0.040**
	(0.018)	(0.019)	(0.015)	(0.016)	(0.019)	(0.018)
Panel C: Vehicle Theft						
Expansion	-0.129***	-0.101**	-0.103***	-0.108**	-0.102***	-0.142***
	(0.044)	(0.042)	(0.039)	(0.043)	(0.037)	(0.032)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	No	No	No	Yes	No	No
Linear Trends	No	No	No	No	Yes	No
Clusters	51	45	51	51	51	51
Observations	306	270	306	306	306	306

Notes: The sample includes 50 states (plus District of Columbia) between 2010-2015. Each panel-column is a separate regression. Models (1)-(5) estimated using OLS where regressions are weighted by the square root of state population. Column (1) shows the preferred difference-in-differences specification. Column (2) excludes states that had comparable Medicaid eligibility requirements prior to 2014 and Wisconsin. Column (3) includes pre-policy lead coefficients. Column (4) includes region-by-year fixed effects. Column (5) includes group-specific linear time trends to the preferred specification. Column (6) defines the outcome variable as the count of the reported crime and estimates the model using Poisson regression. Robust standard errors clustered at the state level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table A7: Placebo Test: Effect of Medicaid Expansions on Crime Rates 2010-2013

	Pre-period, Post-period		
	2010, 2011-2013	*2010-2011, 2012-2013*	*2010-2012, 2013*
	(1)	(2)	(3)
Panel A: Total Crime			
Expansion	0.015	0.014	0.003
	(0.015)	(0.015)	(0.013)
Panel B: Violent Crime			
Expansion	-0.009	-0.019	-0.017
	(0.020)	(0.020)	(0.019)
Panel C: Property Crime			
Expansion	0.019	0.017	0.004
	(0.015)	(0.016)	(0.014)
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Clusters	51	51	51
Observations	204	204	204

Notes: Models estimated using OLS where regressions are weighted by the square root of state population. The sample includes 50 states (plus District of Columbia). Alternative years are selected from 2010-2013 as the pre- and post-expansion periods. Each panel-column is a separate regression. Robust standard errors clustered at the state level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table A8: Placebo Test: Effect of Medicaid Expansions on Violent Crime Categories 2010-2013

	Pre-period, Post-period		
	2010, 2011-2013	*2010-2011, 2012-2013*	*2010-2012, 2013*
	(1)	(2)	(3)
Panel A: Criminal Homicide			
Expansion	-0.011	0.026	-0.028
	(0.044)	(0.042)	(0.047)
Panel B: Aggravated Assault			
Expansion	-0.025	-0.031	-0.027
	(0.023)	(0.024)	(0.020)
Panel C: Robbery			
Expansion	0.019	-0.000	-0.015
	(0.025)	(0.024)	(0.026)
Panel D: Forcible Rape			
Expansion	0.017	-0.004	-0.002
	(0.036)	(0.038)	(0.047)
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Clusters	51	51	51
Observations	204	204	204

Notes: Models estimated using OLS where regressions are weighted by the square root of state population. The sample includes 50 states (plus District of Columbia). Alternative years are selected from 2010-2013 as the pre- and post-expansion periods. Each panel-column is a separate regression. Robust standard errors clustered at the state level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table A9: Placebo Test: Effect of Medicaid Expansions on Property Crime Categories 2010-2013

	Pre-period, Post-period		
	2010, 2011-2013	*2010-2011, 2012-2013*	*2010-2012, 2013*
	(1)	(2)	(3)
Panel A: Burglary			
Expansion	0.036	0.019	-0.006
	(0.023)	(0.024)	(0.020)
Panel B: Larceny			
Expansion	0.016	0.018	0.009
	(0.014)	(0.014)	(0.013)
Panel C: Vehicle Theft			
Expansion	0.006	0.004	-0.025
	(0.030)	(0.030)	(0.031)
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Clusters	51	51	51
Observations	204	204	204

Notes: Models estimated using OLS where regressions are weighted by the square root of state population. The sample includes 50 states (plus District of Columbia). Alternative years are selected from 2010-2013 as the pre- and post-expansion periods. Each panel-column is a separate regression. Robust standard errors clustered at the state level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table A10: Triple Difference Effect of Changes in County-Level Violent Crime Rates

	Homicide (1)	*Assault* (2)	*Robbery* (3)	*Rape* (4)
Expansion x Baseline	-0.003	0.001	-0.002	0.003
	(0.003)	(0.002)	(0.002)	(0.004)
Controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes
Clusters	3060	3060	3060	3060
Observations	18146	18146	18146	18146

Notes: All models estimated using OLS where regressions are weighted by the square root of county population. The dependent variable is defined as the natural log of the indicated reported property crime (plus 1) per 100,000 people. Each specification includes includes county, year, and state-by-year fixed-effects as well as county-specific controls. Robust standard errors clustered at the county level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.

Table A11: Triple Difference Effect of Changes in County-Level Property Crime Rates

	Burglary (1)	*Larceny* (2)	*Vehicle Theft* (3)
Expansion x Baseline	-0.004**	-0.004***	-0.004**
	(0.002)	(0.001)	(0.002)
Controls	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes
Clusters	3060	3060	3060
Observations	18146	18146	18146

Notes: All models estimated using OLS where regressions are weighted by the square root of county population. The dependent variable is defined as the natural log of the indicated property crimes (plus 1) per 100,000 people. Each specification includes includes county, year, and state-by-year fixed-effects as well as county-specific controls. Robust standard errors clustered at the county level. ***, **, * represent significance at the .01, .05, and .10 level, respectively.