Investing in Health and Public Safety: Childhood Medicaid Eligibility and Later Life Criminal Behavior

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ABSTRACT

A growing body of research documents positive long-term impacts of public health insurance that go far beyond improving recipients' health. In this study, we expand the analysis to assess whether expanding Medicaid coverage generates reductions in crime. We find that increased Medicaid eligibility during childhood generates significant reductions in crime in early adulthood. Cohorts who experienced expanded Medicaid eligibility during childhood had significantly fewer arrests for property crime, drug-related crime, and driving under the influence in early adulthood. The effects are larger for males than females, for blacks than whites, and for eligibility later in childhood.

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I. INTRODUCTION

Since its inception in 1965, Medicaid has improved access to primary and preventative care, specialists, mental health professionals, dentists, and an array of other health-related services for generations of children. It is the largest health insurance program for children in the U.S., with more than half of the U.S. child population enrolled in 2020.[1] A large body of research has demonstrated that investments in Medicaid for children have positive impacts on an array of outcomes during childhood, including reduced infant and child mortality, enhanced children's health, and improved financial security for millions of families. Researchers have also documented that the health impacts of Medicaid access evolve over beneficiaries' lifetimes to include improved adult health, fewer hospitalization and emergency visits, and reduced disability and mortality in adulthood.[2] Childhood Medicaid eligibility also increases beneficiaries' long-term educational attainment, employment, earnings (and related tax revenues), and financial stability.

In this paper, we expand the analysis to examine the impact of expanded childhood Medicaid eligibility on criminal behavior in adulthood. Increased access to Medicaid could impact crime directly via improvements to health, particularly mental health, as mental illness and crime are frequently linked in the research literature. Reductions in crime could also arise indirectly via Medicaid's positive long-term impacts on education, employment, earnings, and household resources, all of which are associated with lower crime.

To identify the effect of childhood Medicaid eligibility on later life criminal outcomes, we exploit variation created by a series of Medicaid expansions in the 1980s and 1990s that dramatically increased eligibility. These expansions, which included both mandated changes for all states and optional policy changes for states that chose to adopt them, resulted in substantial variation in public health insurance eligibility both for children born in the same year residing in different states, and for children born in the same state in different years. To account for the endogeneity of economic and demographic characteristics that influence both Medicaid eligibility and criminal behavior, we utilize the simulated eligibility approach pioneered by Currie and Gruber (1996b) and now commonly used by researchers to isolate changes in state-level eligibility due to changes in Medicaid policy from changes in eligibility due to other economic or demographic factors.

We find that increased Medicaid eligibility during childhood generates significant reductions in crime. An additional year of eligibility during childhood leads to a 9% decrease in property crime over ages 19-24. Further, arrests during early adulthood for drug-related crime and driving under the influence (DUI) fall by 7% and 4%, respectively, for each additional year of childhood Medicaid eligibility. The effects for males are larger, indicating a 12% reduction in property crime and an 8% reduction in drug-related crime for each additional year of childhood Medicaid eligibility. The effects are also larger for blacks than for whites, for non-violent crimes, and for increased eligibility during later childhood relative to early childhood.

Our results reveal substantial long-term public safety benefits from providing public health insurance to low-income children. Medicaid not only improves short- and long-term health, education, and financial outcomes for beneficiaries, it also generates substantial positive externalities by reducing crime and improving public safety over the long term. Finally, because children whose parents engage in criminal behavior have a higher risk of becoming criminals themselves, the crime reductions generated by childhood Medicaid eligibility are likely to be more pronounced as beneficiaries grow to adulthood and have children of their own.[3]

II. CHANGES IN MEDICAID ELIGIBILITY

The Medicaid program is a partnership between the federal government and the states, primarily aimed at providing health insurance to low-income children, the disabled, and the elderly. Medicaid was first introduced in 1965 and was phased in by most states by 1970.[4] Until the 1980s, Medicaid eligibility was primarily limited to recipients of "cash welfare" through the Aid to Families with Dependent Children (AFDC) program, with the result that early Medicaid eligibility varied across states and was typically limited to children of very low-income single mothers.[5]

Medicaid expansions in the 1980s and 1990s increased access significantly. In short, the expansions first removed the family structure requirements for Medicaid eligibility to include children outside single-parent households, then raised the maximum income thresholds for Medicaid eligibility for pregnant women and infants, then raised these thresholds for young children, and eventually raised the thresholds for all children. The higher income eligibility thresholds were driven both by federal policies that mandated expanded coverage, and by state-level policies that expanded coverage beyond the federal minimums.

Figure 1 summarizes the increase in average years of childhood Medicaid eligibility across time for children born between 1976 and 1987 (the birth cohorts we study in this paper).[6] The solid line depicts national eligibility for the cohorts, while the grey dots show state levels of eligibility. The average child in the 1976 birth cohort received less than three years of eligibility during childhood, while the average child in the 1987 birth cohort was Medicaid-eligible for over seven years of childhood. The growth in eligibility also varied dramatically across states, as shown in Figure 2. In the states with the largest Medicaid expansions, average eligibility rose by 7-8 years for the 1976-1987 birth cohorts, while eligibility rose by 2-3 years for these birth cohorts in the states with the smallest expansions. Our analysis uses this variation in eligibility across states and time to evaluate the effects of eligibility for public health insurance during childhood on crime in early adulthood.

III. PREVIOUS RESEARCH AND POTENTIAL MECHANISMS

Although researchers have found that expanded Medicaid eligibility is associated with contemporaneous reductions in crime among adults, ours is the first study to examine the impact of childhood Medicaid eligibility on later-life crime.[7] There several mechanisms through which this impact could occur.

First, childhood Medicaid eligibility could impact later-life crime via improvements to health, particularly mental health, as mental illness and crime are frequently linked. There are high rates of mental illness among incarcerated populations, higher crime rates among those who suffer from mental illness, and adolescents who suffer from depression face a substantially increased probability of engaging in property crime later in life ((Teplin et al. 2002; Swanson et al. 2002; Anderson, Cesur, and Tekin 2014).[8]

The first onset of mental illness usually occurs in childhood or adolescence, and about half of all lifetime mental disorders start by the mid-teens (Kesser et al 2007). By incrementally expanding eligibility to include older children and teens, the Medicaid expansions increased the likelihood of screening, diagnosis, and treatment for mental disorders during these key stages of development. Indeed, Clemans-Cope et al. (2015) find that children who became eligible for public health insurance via the CHIP program were more likely than their uninsured counterparts to have specialty and mental health visits and to receive prescription drugs. Although they do not focus on children, Finkelstein et al. (2012) find improvements in mental health among adults who received Medicaid eligibility through the Oregon Health Experiment, and Austin et al. (2021) document that the Medicaid expansions stemming from the Affordable Care Act were associated with reductions in suicide among nonelderly adults.

Second, reductions in crime could arise indirectly via Medicaid's positive impact on education, employment, and earnings. Increases in primary and secondary education generate reductions in crime (Anderson 2014; Machin, Marie, and Vujic 2011; Lochner and Moretti 2004), as do better employment opportunities and higher earnings, particularly for low-income workers (Gould, Weinberg, and Mustard 2002; Machin and Meghir 2004).

Third, expansions in Medicaid eligibility could reduce criminal behavior by improving families' financial stability. The negative correlation between family resources and crime is well established (for example, see Calnitsky and Gonalons-Pons 2021), and Medicaid expansions improve family resources, increase household consumption, and reduce rates of bankruptcy (Gruber and Yellowitz 1999; Gross and Notowidigdo 2011).

Fourth, research has found that lower socioeconomic status is associated with higher stress for parents, which interferes with parenting and limits resources for investing in children, both of which are associated with increased criminal behavior (Johnson 2016; Duncan and Magnuson 2003). By increasing households' financial resources, Medicaid may in turn reduce financial stress, increase parent-child interaction, and improve parenting practices, all of which are key predictors of juvenile delinquency (Johnson 2016).

In sum, although our study design precludes us from examining the precise mechanisms through which expanded childhood Medicaid eligibility affects early-adult crime, existing research suggests several potential pathways, including Medicaid's impacts on physical and mental health, education, employment, earnings, increased household resources, and reduced parental stress. In the next sections, we describe the data and empirical strategy we use to estimate Medicaid's impacts on crime.

IV. DATA

We utilize two primary sources of data for the study, one to measure crime rates and another to measure Medicaid eligibility during childhood. We describe each of these, as well as the construction of our key variables, in more detail below.[9]

A. Crime Rates. Our data on criminal outcomes come from the FBI's Uniform Crime Reporting (UCR) system for the years 1995 to 2011.[10] The UCR collects self-reported arrest data from over 16,000 law enforcement agencies each year, including arrest counts by offense, sex, and individual age for those under 24, as well as a count of the total population covered by each agency. We aggregate crime counts to the state level by offense, age, and sex and compute arrest rates per 10,000 of the relevant age-sex group population in the state in each year. The Online Appendix provides more detail on how we computed the crime rates, as well as robustness checks for alternate methods of computation.

It is important to note that not all law enforcement agencies report to the UCR every year, and even when they do report, agencies may not report for all crimes. In our baseline estimates, we restrict the UCR data to include only agencies that report crime counts in all of our crime outcome years (1995-2011), assume that if an agency didn't report for a given crime in a given year (but did report on other crimes in that year) it was because no instances of that crime occurred in that agency in that year, and replace outlier crime rates (i.e., those that were more than 1.5 times their interquartile range above the third or below the first quartile values) with their inverse distance weight predicted values. Because 16 states had no law enforcement agencies that reported in every year, our sample includes 35 states.[11] For each of these states, we observe crime outcomes for 12 birth cohorts (1976-1987) over six years of age (one for each age 19-24), resulting in 2,520 state-cohort-age observations.

We link the crime data to each birth cohort by year, age, and sex. For example, the 1980 birth cohort is linked to crime rates for 19-year-olds in their respective states in 1999, to crime rates for 20-year-olds in 2000, and so on through 2004 (when they are 24).[12] Thus, each cohort is linked to six years of crime rates, assigned to correspond to the year that the cohort reached age 19, 20, and so on. This allows us to identify effects from crime rates that vary at the state-birth cohort-age level.

B. Medicaid Eligibility. We measure childhood Medicaid eligibility by comparing state of residence, year of birth, family structure, and family income for individual children in the March Current Population Survey (CPS) against the relevant state-year income thresholds for Medicaid eligibility.[13] In particular, for each state and year, we define a binary eligibility indicator as equal to one if an individual child's age, family structure, and family income imply that they are below the Medicaid eligibility threshold established by federal and state eligibility rules.[14] We then compare each child in the CPS against their relevant state-year eligibility threshold and assign the eligibility indicator accordingly. Thus, for each state and birth cohort, the mean value of the binary eligibility variable in each year of childhood measures the fraction of the birth cohort eligible for Medicaid in that state and year. We then sum the yearly fraction eligible for each birth cohort in each state across that cohort’s childhood to generate a cumulative measure of childhood eligibility for Medicaid for each birth cohort in each state.[15]

The fraction of a birth cohort eligible for Medicaid could increase due to more generous Medicaid eligibility thresholds, as well as due to a fall in incomes (caused by a recession, for example) that results in more families having incomes below the eligibility threshold. This second source of variation is concerning for identifying the effect of Medicaid eligibility since factors such as family income and family structure affect both Medicaid eligibility and criminal activity. To isolate the variation in eligibility that results only from policy changes, implementing the process described above using a randomly selected nationally representative 20 percent sample of the CPS, rather than using each state's individual-level CPS data.[16] By using a national sample that does not vary across states in demographic or economic characteristics, this instrument removes variation in eligibility that arises from state-specific demographic characteristics and thus isolates variation in the generosity of the states' Medicaid programs. Pioneered by Currie and Gruber (1996a), this *simulated eligibility* approach has been utilized in many recent studies examining the impacts of expanded public health insurance eligibility (Cohodes et al. 2016; Wherry et al. 2018; Miller and Wherry 2019; Brown, Kowalski, and Lurie 2020; Bacon-Goodman 2021).

V. ESTIMATION STRATEGY

We hypothesize that increased eligibility for Medicaid in childhood will decrease criminal activity in adulthood. If this is the case, we would expect to see lower crime rates during adulthood for birth cohorts in states with larger Medicaid expansions relative to those in states with smaller expansions. We compare the childhood eligibility of birth cohorts born in different states and times against the cohorts' criminal behavior in early adulthood, as in Equation 1:

$$(1) \quad CrimeRate_{s,c,a,y} = \beta_0 + \beta_1\, Eligibility_{s,c} + \beta_M\, Maternal_{s,c} + \beta_A\, Adult_{s,c,y} + \beta_C\, Childhood_{s,c} + \beta_{Age}\, Age_{c,a,y} + \beta_s\, S_s + \beta_y\, Year_y + \beta_{s,y}\, (S_s{*}Trend_c) + e_{s,c,y}$$

The variable $CrimeRate_{s,c,a,y}$ measures the number of arrests per 10,000 people for birth cohort c in state s at each age a of early adulthood (a = 19, 20, …, 24) while $Eligibility_{s,c}$ measures years of childhood Medicaid eligibility experienced by birth cohort c in state s during childhood (ages 0-18). The state fixed effects (S_s) control for time-invariant unobserved state characteristics common to all birth cohorts in a given state that affect crime rates and are also correlated with Medicaid eligibility. For example, cohorts in lower-income states would likely have both higher rates of crime and higher levels of Medicaid eligibility, regardless of any changes in eligibility rules. The inclusion of year fixed effects ($Year_y$) controls for changes over time that are common to all states and potentially correlated with both crime and eligibility. The inclusion of state-specific time trends for each cohort ($S_s{*}Trend_c$) controls for time-varying differences across states and cohorts that affect criminal behavior and eligibility.

Equation 1 also controls for a variety of observable characteristics that potentially impact crime and eligibility and change over time within states. The vector $Maternal_{s,c}$ includes controls for the average characteristics of mothers by state in the year when each of the cohorts was born (rates of single motherhood, teenage pregnancy, first trimester prenatal care, and the percent of births to nonwhites).[17] The vector $Adult_{s,c,y}$ includes controls for state-level differences in the economic, social, and legal environments during each year of the cohorts' early adulthood (unemployment rates, poverty rates, rates of alcohol consumption, police officers per capita, whether the state had a HIFA waiver program, and controls for firearm and marijuana policies).[18]

Finally, other policy and economic characteristics could be correlated with the environment faced by children (for example, AFDC/TANF and EITC programs also target low-income families and potentially affect crime (Barr and Smith 2017; Agan and Makowsky 2018)). Our identification strategy (simulated eligibility) abstracts from economic characteristics, but could pick up general generosity to the poor. The vector $Childhood_{s,c}$ includes state-level unemployment rates, state EITC credit amounts, AFDC/TANF benefit levels, and AFDC/TANF eligibility income thresholds, averaged over each birth cohort's childhood, to control for this issue.[19] Summary statistics for these control variables are reported in Table A2, and we show in the Online Appendix that our results are similar when we exclude the maternal, adult, and childhood controls from the regressions.

Because a birth cohort's Medicaid eligibility is a function of both the generosity of their respective state's Medicaid policy and the cohort's socioeconomic characteristics, estimates of β_1 in Equation 1 cannot separate the impact of changes in Medicaid policy from other factors that impact eligibility. As described above, we address this issue by utilizing a simulated instrumental variable to isolate the impact of Medicaid policy. Our instrumental variables model is shown in Equations 2 and 3:

$$(2)\ Eligibility_{s,c} = \alpha_0 + \alpha_1 Simulated\ Eligibility_{s,c} + \alpha_M\ Maternal_{s,c} + \alpha_A\ Adult_{s,c,y} + \alpha_C\ Childhood_{s,c} + \alpha_s\ S_s + \alpha_y\ Year_y + \alpha_{s,y}\ (S_s{*}Trend_c) + \varepsilon_{s,c,y}$$

$$(3)\ CrimeRate_{s,c,a,y} = \beta_0 + \beta_1\ \widehat{Eligibility}_{s,c} + \beta_M\ Maternal_{s,c} + \beta_A\ Adult_{s,c,y} + \beta_C\ Childhood_{s,c} + \beta_{Age}\ Age_{c,a,y} + \beta_s\ S_s + \beta_y\ Year_y + \beta_{s,y}\ (S_s{*}Trend_c) + e_{s,c,y}$$

Where $\widehat{Eligibility}_{s,c}$ is from Equation 2, a first-stage regression of actual childhood eligibility for birth cohort c in state s on the cohort's simulated eligibility, the controls for maternal, adulthood, and childhood characteristics, state and year fixed effects, and state-specific time trends.

The exclusion restriction for this model to identify the causal effect of Medicaid eligibility is that the only channel through which simulated eligibility affects criminal outcomes is via its impact on actual eligibility. Given our inclusion of state and time fixed effects, this implies assuming that the state-to-state variation in the years and sizes of Medicaid expansions is independent of later criminal outcomes. This seems a reasonable assumption since it is unlikely that states enacted Medicaid expansion in response to subsequent crime rates. Nonetheless, we also include state-specific time trends to mitigate the concern that our results are driven by differences in trends in criminal behavior across states that expanded Medicaid at different rates across time.

Our baseline estimates measure the impact of Medicaid eligibility on rates of arrest for violent, property, and drug crimes, as well as on DUI rates, but we also report estimates for disaggregated crimes. We examine heterogeneous treatment effects by sex and age, and also estimate effects on adolescent crime. Finally, we test the sensitivity of our estimates to a wide array of alternatives and find them to be very robust.

VI. EMPIRICAL RESULTS

Table 1 presents our baseline estimates of the effect of Medicaid eligibility over childhood on arrest rates during young adulthood.[20] The first row of the table reports estimates from an OLS regression of crime rate on actual eligibility plus the control variables, while the second row reports estimates from a reduced form regression of crime on simulated eligibility plus the control variables. Both sets of estimates indicate that eligibility is associated with statistically significant reductions in crime rates. The instrumental variables (IV) estimates indicate strong, statistically significant negative impacts of childhood eligibility on three of four aggregate crime measures. An additional year of Medicaid eligibility during childhood does not appear to affect aggregate rates of violent crime, but it is estimated to reduce the annual rate of property, drug, and DUI crimes by roughly 14, 11, and 6 incidents per 10,000 of the relevant age group population, respectively. Interpreting the coefficient estimates relative to the mean crime rates (reported in the rows labeled "% change") implies that an additional year of childhood eligibility generates a 9% reduction in the property crime rate, a 7% reduction in the drug crime rate, and a 4% reduction in the DUI rate over ages 19-24.[21] The larger and more significant reduction in property crime relative to violent crime is consistent with empirical criminology research, which finds that property crimes tend to be more rational and require more planning (and would thus be more sensitive to economic and policy changes) than violent crimes, which tend to be more impulsive (see, e.g., Chalfin, Danagoulian, and Deza 2019; Clarke and Cornish 1985).

A. Heterogeneous Treatment Effects by Sex. Because crime is committed at markedly different rates across the sexes, and because the health-related pathways to criminality may differ by sex, in Table 2 we present estimates of the effects of childhood Medicaid eligibility separately by sex.[22] The estimates indicate that the impacts of Medicaid eligibility are larger and more precisely estimated for males than for females. For example, an additional year of eligibility reduces the rate of violent crime among males by 3%, but the estimated effect among females is small and statistically insignificant. Similarly, the estimates indicate that additional eligibility reduces male property, drug, and DUI crime rates by roughly 12%, 8%, and 4% respectively, but reduces property crime and DUI rates by about 4% for females while having no statistically significant impact on female drug crime rates.

B. Treatment Effects on Disaggregated Crime Rates. Estimates of the impacts of increased eligibility on disaggregated crime rates (Online Appendix Table A4) indicate that, among violent crimes, increased Medicaid eligibility has large and statistically significant impact only on rates of robbery among males, with an additional year of eligibility corresponding to a 5% reduction. For females, none of the estimated impacts on violent crimes is significant.

For property crimes among males, an additional year of Medicaid eligibility generates statistically significant reductions in burglary (6%), larceny (14%), and motor vehicle theft (15%), but has no discernable impact on rates of arson. The estimates indicate no impact of increased Medicaid eligibility on burglary, motor vehicle theft, or arson among females, but imply that an additional year of eligibility reduces rates of larceny among females by 4%.

The negative impact of expanded Medicaid eligibility on drug crimes among males comes from reducing rates of both drug sale and drug possession. An additional year of eligibility generates a 12% reduction in drug sale arrests and a 7% reduction in drug possession arrests.

C. Heterogeneous Treatment Effects by Age of Eligibility. Thompson (2017) finds that Medicaid eligibility received during different phases of childhood has heterogeneous impacts on later life health. In particular, he finds that increases in eligibility during early childhood (ages 0-5) has larger health impacts than increases during middle childhood (ages 6-11) or adolescence (ages 12-18). He hypothesizes that these differences arise because some health investments in early childhood (e.g., vaccinations) may have especially large long-term impacts. Additionally, Cohodes et al. (2016) find that the impact of increased eligibility on college attendance and completion is concentrated among those ages 14-17. Finally, in their application of an alternative weighting scheme to the Cohodes et al. (2016) estimates, Goldsmith-Pinkham, Sorkin, and Swift (2018) find that most of the identifying variation in eligibility occurs during schooling ages (5-16) rather than during early childhood.

In Table 3, we present estimates of the impact of expanded childhood Medicaid eligibility received at different ages. Each row of the table presents estimates of the impact of additional eligibility during each age range. The estimates indicate that the effects of increased eligibility are generally larger for eligibility received later in childhood. For males, being eligible for Medicaid at, say, age 15 has a greater effect on their propensity for crime as young adults than does being eligible at age three. We view this result as consistent with the hypothesis that increased Medicaid eligibility reduces crime at least in part by improving mental health outcomes, since rates of behavioral disorders, depression, and anxiety are more common during later childhood.[23] Additionally, it could simply be that the effect of increased eligibility on crime is greatest immediately following its receipt and declines the longer one is from being treated. The estimated effects at different ages of childhood eligibility for females (Online Appendix Table A5) are generally insignificant, with the exception that eligibility during ages 12-18 generates an 8% reduction in DUI arrests during early adulthood.

Figure 3 presents estimated coefficients on measures of eligibility received at each age of childhood. Consistent with the results in Table 3, the figure does not show a clear pattern of impacts of eligibility at different ages on violent crime for males or females. For property crime, the pattern of coefficients suggests consistent negative impacts of eligibility for both males and females beginning around age 10, with larger impacts for males than females. The pattern of coefficients is similar for drug and DUI crimes among males, with consistent negative impacts beginning around age 10, and larger impacts during the middle- and late-teen years. For females, the impacts are smaller and less significant for drug and DUI crimes.

D. Heterogeneous Treatment Effects Across Birth Cohorts. The Medicaid expansions of the 1980s and 1990s incrementally increased eligibility by first removing family structure requirements, then expanding income eligibility thresholds for pregnant women and infants, and later increasing income eligibility thresholds even further and extending coverage to older children. These changes imply that the marginal children gaining eligibility shifted from primarily children of low-income single mothers to poor children from families with higher incomes.[24] Since the marginal child gaining eligibility from the 1987 birth cohort would be expected to be from a higher income and differently structured family than their peers from the 1976 birth cohort, there may be heterogeneous impacts of expanded Medicaid eligibility across the birth cohorts. We examined this possibility by estimating Equation 3 while replacing $\widehat{Eligibility}_{s,c}$ with interactions of $\widehat{Eligibility}_{s,c}$ and indicators for each birth cohort. The resulting coefficient estimates are presented in Figure 4. The estimates indicate that although there do not appear to be abrupt changes in the impacts of expanded eligibility across cohorts, the impacts are slightly smaller for later, higher-income cohorts than for earlier cohorts, particularly for female property crime and male drug and DUI crime. Using drug crimes among males as an example, the estimated impact of an additional year of eligibility was to decrease drug crimes by about 12.5% for the 1976-1979 birth cohorts, but by 11% for the 1984-1987 birth cohorts.

E. Heterogeneous Treatment Effects by Race. Existing research suggests that Medicaid eligibility has heterogeneous impacts across racial groups. Estimates by race (Appendix Table A6) indicate that the negative impact of Medicaid eligibility on crime is much stronger among blacks than whites, with an additional year of childhood Medicaid eligibility for blacks generating statistically significant reductions in property crimes in adulthood by 9% and drug crimes in adulthood by 8%.[25] The estimates for whites are also negative, but are statistically insignificant.[26] The larger impact of Medicaid on black crime rates suggests that in addition to reducing crime, Medicaid also reduces racial gaps in crime in the US.

F. Effects on Crime During Adolescence. Although our primary focus is on crimes during early adulthood, the structure of the UCR allows us to examine the impact of Medicaid expansion on contemporaneous crimes during adolescence. In particular, the UCR reports crimes by age and sex for teens 15-18 years old, which allows us to link the birth cohorts to crime rates in their states in the years that they turn 15, 16, 17, and 18. Because two additional states (IA and SD) had no law enforcement agencies that reported on crime rates for these ages in every year, the sample falls to 1,584 (12 cohorts*4 years*33 states).

The estimated effects of Medicaid expansion on 15-18 year old crime rates are presented in Table 4. Because eligibility received at different ages may affect adolescent crime differently, we present estimates of the impact of expanded childhood Medicaid eligibility received at different ages, as we did in Table 3. Each row of the table presents estimates of the impact of additional eligibility during each age range. The estimates indicate that eligibility received at ages 0-5 generates reductions in property crime during adolescence for both males and females, with additional year of eligibility reducing property crimes at ages 15-18 by roughly 15%. Increased eligibility at ages 0-5 also reduced drug crimes among females, while increased eligibility at ages 6-11 generated reductions in adolescent drug crimes by 13% among males and 16% among females. Consistent with the expansions having a delayed rather than contemporaneous effect, an additional year of eligibility during ages 12-18 does not appear to reduce crime among 15-18 year olds. Not surprisingly, the expansions did not affect DUI crimes, which occur at much lower rates among adolescents than among young adults.

VII. ROBUSTNESS CHECKS

We examine the sensitivity of our estimates in several ways, including changing the construction of the crime and eligibility variables, including different sets of state-level trend variables, including additional controls, including fewer controls, and using alternative functional forms and levels of data aggregation. Overall, we find that our results are robust to a wide array of alternatives.[27]

A. *Childhood Environment and Alternative Policies*. Our baseline estimates include controls for the average economic conditions and welfare policies experienced by birth cohorts during their childhoods (i.e., state-level unemployment rates, AFDC/TANF eligibility thresholds and maximum benefit levels, and EITC credit amounts). There are three other large-scale policy changes affecting children that have also been linked to later crime: the legalization of abortion (Donohue and Levitt 2001), compulsory schooling laws (Anderson 2014; Gilpin and Pennig 2015), and changes in permissible lead exposure (Reyes 2007; Feigenbaum and Muller 2016). In addition, some the expansions in Medicaid we study included extensions of eligibility to pregnant women, which implies that part of the effect captured in our estimates could reflect impacts of the birth cohorts' eligibility in utero rather than eligibility during childhood. Finally, some of our outcome years coincide with the emergence of the crack cocaine boom of the 1980s and the more recent opioid crisis, both which have been linked to increases in crime (Evans, Garthwaite, and Moore, 2018; Dave, Deza, and Horn, 2020). We explore the robustness of our estimates to inclusion of these controls in Table 5.

Panel A of Table 5 reports our baseline estimates for comparison purposes. Panels B and C report estimates from regressions that also include controls for the rate of abortion in each state and year and for the minimum dropout age in birth cohorts' states when they were ages 14-18.[28] In Panel D, we include controls for the year of implementation and the rigor of states' prescription drug monitoring programs, which several researchers have linked to reductions in opioid-related in crime (Dave, Deza, and Horn 2018).[29] In Panel E, we include controls for the emergence of crack cocaine markets using data from Table A2 in Evans, Garthwaite, and Moore (2018).[30] The estimates are very similar across all five models.

Finally, in Panel F we include controls for prenatal Medicaid eligibility. Miller and Wherry (2019) find that cohorts whose gained access to Medicaid in utero had increased high school graduation rates and better health outcomes as adults. Because the Miller and Wherry prenatal eligibility calculator begins in 1979, we limit sample to nine birth cohorts (1,890 observations), repeat the baseline specification, and then add controls for prenatal eligibility. Although the results indicate smaller estimated effects for this subsample of cohorts, they are all consistently negative, and the estimated effects of childhood Medicaid eligibility are very similar when prenatal eligibility is included. Interestingly, the estimates also suggest that prenatal eligibility itself is associated with reductions in property and drug crime during early adulthood, although the estimates are significant at only the 10 percent level.[31]

Exposure to lead has also been linked to higher rates of crime, particularly violent crime and homicide (Reyes 2007; Feigenbaum and Muller 2016). The 1970 Clean Air Act generated dramatic reductions in the amount of lead in gasoline over 1975-1985, a period that overlaps with some of our birth cohorts' childhoods and implies that later birth cohorts in our sample were exposed to relatively lower rates of environmental lead. If states that were early or more aggressive adopters of lead-reducing policies were also states with greater expansions in childhood Medicaid eligibility, failure to control for lead exposure could bias our estimated impacts of Medicaid expansions away from zero. We explore whether policies aimed at reducing lead exposure impact our estimates in the Online Appendix Table A11 and conclude that reduced exposure to lead is unlikely to be a primary driver of our results, particularly for the estimated impacts among males and on non-violent crimes.

B. Falsification Tests. Table 6 presents the results from several falsification tests that examine age groups and outcomes that should not be affected by the changes in Medicaid eligibility that we exploit. Panels A and B report estimates using crime rates among 50-54 and 55-59 year-olds (who were too old to experience the Medicaid expansions we study) as dependent variables.[32,33] With the exception of the impact on drug crime among 50-54 year olds (significant at 0.10), the estimated impacts for older groups are all statistically insignificant.

In Panel C, we report estimates that result from linking the 1976-1987 birth cohorts' Medicaid eligibility measures to crime rates among cohorts born 20 years earlier and who thus should not be impacted by the Medicaid expansions. That is, we link our eligibility measures to crime rates from 1974-1991 instead of 1994-2011.[34] The estimates are again small and statistically insignificant.

Finally, Panel D presents estimates of the effect of expanded Medicaid eligibility on age, race, sex, and state-level minimum school dropout age policies, outcomes that should not be affected by Medicaid expansions. We generated these estimates by linking 1995-2011 CPS data for 19-65 year-olds in the states we study to the eligibility and control variables used in our baseline analysis. All of the estimates are close to zero, and none are statistically significant.

C. Interstate Migration. Because the UCR data do not include information on arrestees' state of birth or state of residence during childhood, our empirical strategy implicitly assumes that an individual arrested in young adulthood in a state spent their childhood in that same state. To the extent that interstate migration is uncorrelated with Medicaid eligibility, this measurement error will merely attenuate our estimated effects towards zero. However, if families with children who have a relatively higher propensity for crime are more likely to migrate into states with lower levels of Medicaid eligibility (for example, if poorer families, which have higher crime rates on average, move to states with lower levels of Medicaid eligibility), this could bias our estimates away from zero.

Note that this type of bias would require that poor families move *in the opposite direction* than implied by the "welfare magnet" hypothesis (under which, states with higher levels of public benefits disproportionately attract poor migrants). Further, Schwartz and Sommers (2014) find no evidence that Medicaid expansions that occurred in the early 2000s impacted interstate migration, and even if their estimated impacts were statistically significant they imply an upper bound on migration that is just 0.27 percent (0.0027) of the Medicaid population in expansion states, a figure they conclude is too small to have substantial impacts on Medicaid enrollment. Finally, Cohodes, et al (2016) find that their estimates of the impact of childhood Medicaid eligibility on later life educational outcomes are insensitive to using individuals' state of residence versus state of birth.

In sum, the research indicates that migration in response to expansions in Medicaid is unlikely and, if it does occur, is in the opposite direction from what would be needed to bias our estimates away from zero. Thus, we view interstate migration as an unlikely source of bias for our estimates.

IX. DISCUSSION and CONCLUSIONS

This paper provides the first estimates of the long-term impact of Medicaid eligibility in childhood on crime in adulthood. We find large and statistically significant reductions in crime rates resulting from increased childhood Medicaid eligibility, particularly for adolescent males. Our estimates are robust to a wide array of sensitivity and falsification tests, and indicate that an additional year of Medicaid eligibility during childhood reduces the rate of property crime by 9%, the rate of drug crime by 7%, and the rate of arrest for DUI by 4%. The estimated effects are larger for males than for females, for blacks than for whites, and indicate more significant impacts for Medicaid eligibility received during adolescence.

Our estimated effects compare favorably with those found in other research on the effects of social policy on crime. Barr and Smith's (2017) finding that an additional year of food stamp eligibility during early childhood reduces the rate of arrests at ages 18-24 by about 3% is similar to the reduction in male arrests for violent crime suggested by our estimates. Our estimates indicate that an additional year of Medicaid eligibility generates smaller and less significant reductions in murder and assault, similar reductions in motor vehicle theft and burglary, but larger reductions in larceny than those generated by an additional year of schooling (Lochner and Moretti 2004). Our findings by race are consistent with Garces, Thomas, and Currie (2002), who find that children who attended Head Start are significantly less likely to report having been booked for or charged with a crime as adults, with the results driven primarily by blacks. Thus, like other targeted social programs, Medicaid can generate important reductions in racial gaps in education, income, and crime.

We generate a back-of-the-envelope measure of the crime-related rate of return of an additional year of Medicaid provision during childhood by combining our estimated effects with estimates the social costs of various crimes from Miller et al. (2020). Focusing on males, the estimated dollar value of crimes averted from an additional year of Medicaid eligibility are roughly \$108.[35] Given that each additional year of Medicaid eligibility during childhood for cohorts born between 1981 and 1984 cost \$593 (in \$2020) (Brown, Kowalski, and Lurie 2020), our calculations indicate that each \$1 invested in Medicaid provision during childhood for males generates a \$0.19 return by age 24 in terms of lower crime costs. The estimated impacts among females are more muted (\$0.04). However, because these calculations do not include spillover effects on recipients' peers, they likely underestimate the true crime-related rate of return (see, e.g., Carrell and Hoekstra 2010; Billings and Hoekstra 2019). Combining our estimates with those of Brown, Kowalski, and Lurie (2020) suggests that, at a minimum, by age 28 the government recoups \$0.75 for males and \$0.60 for females for each \$1 invested in Medicaid provision during childhood.[36]

Given the ongoing policy discussions over publicly provided health insurance, it is important to understand both the short- and long-term impacts of these programs, not only on health itself, but also on education, earnings, and other outcomes like crime. Our results contribute to a growing set of studies documenting the substantial long-term private and public benefits from investing in public health insurance for low-income children. Medicaid not only improves health outcomes and reduces health-related costs for beneficiaries themselves; it also generates substantial positive externalities by increasing educational attainment, earnings and tax payments, and reducing crime. The evidence increasingly indicates that childhood Medicaid coverage generates large benefits across the life cycle that more than offset the program's costs.

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XI. NOTES

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[1] We refer to Medicaid and Children's Health Insurance Program (CHIP) jointly as "Medicaid" throughout the paper. The Center for Medicaid and CHIP Services (2021) reported that there were 38.7 million enrollees in Child Medicaid and CHIP in March 2021. The U.S. Census Bureau (2017) estimates that there were 74 million children in the U.S. in 2021. https://www.census.gov/data/tables/2017/demo/popproj/2017-alternative-summary-tables.html.

[2] Bacon-Goodman (2021), Thompson (2017), Boudreaux, Golberstein, and McAlpine (2016), and Wherry and Meyer (2016) examine long term health outcomes, Cohodes et al. (2016) examine long run educational outcomes and Bacon-Goodman (2021) and Brown, Kowalski, and Lurie (2020) examine employment-related outcomes.

[3] For example, Besemer (2014) finds that children whose parents had been convicted of a crime have three times more convictions than children whose parents had not been convicted.

[4] Twenty-six states had Medicaid programs by the end of 1966. By 1970 all states, save Alaska (1972) and Arizona (1982), had a program.

[5] For example, in 1983 the income cutoff for AFDC eligibility was 100% of the federal poverty line (FPL) in Vermont and 28% of the FPL in Kentucky.

[6] We focus on these birth cohorts for two reasons. First, our identification strategy relies on cross-state variation and Current Population Survey (CPS) respondents' states were not separately identified prior to 1977. Second, the Medicaid eligibility calculator we use (described in more detail in Section IV) is only available through 2005 (the year in which those born in 1987 aged out of Medicaid eligibility).

[7] Wen, Hockenberry, and Cummings (2017) find that the 2001 Health Insurance Flexibility and Accountability (HIFA) expansions that provided Medicaid to low-income, childless adults led to reductions in robbery, aggravated assault, and larceny theft, changes they attribute to Medicaid-related increases in substance use disorder treatment. Cuellar and Markowitz (2007) find that increased Medicaid spending on antidepressants is correlated with small declines in violent and property crime among adults, but they find no significant contemporaneous relationship between adult crime and Medicaid eligibility thresholds.

[8] In related work, Marcotte and Markowitz (2010) find that sales of new generation antidepressants and stimulants are negatively associated with rates of violent crime.

[9] We describe the control variables we include in our regressions in Section V.

[10] Our identification strategy utilizes yearly variation in crime rates by age. The UCR reports arrest counts by *individual* age for those up to age 24, but only counts by *age groups* for those over 24. Thus, we utilize UCR data for 1995-2011 as it encompasses the year the 1976 birth cohort turned 19 through the year the 1987 birth cohort turned 24.

[11] The excluded states are AL, DE, DC, FL, GA, IL, KS, KY, LA, MS, MT, NH, NM, NY, VT, and WI.

[12] This linking strategy could potentially result in measurement error since we do not observe the month of birth for the cohorts (e.g., a 19-year-old arrested in June of 2000 could have been born at any time between June 1980 (putting them in the 1980 birth cohort) and June 1981 (putting them in the 1981 birth cohort)). So long as eligibility and the distribution of crimes or births across months of the year are not correlated, this will simply attenuate our estimates toward zero. Because of the eligibility discontinuity that occurred October 1983, we test that our results are not sensitive to the inclusion or exclusion of the 1983 birth cohort. The estimates (available from the authors) indicate that excluding the 1983 birth cohort from the analysis does not generate markedly different estimates.

[13] The eligibility thresholds are built from code provided by Tal Gross and Kosali Simon, to whom we are extremely grateful. The CPS data are from IPUMS (Flood et al. 2018).

[14] Variation in the eligibility rules across states and years arises due to differences in AFDC need thresholds across states and time, because states had different rules for phasing in eligibility for Ribicoff children, and because some states adopted an AFDC-Unemployed Parent (AFDC-UP) program before it was extended to all states by the Family Support Act of 1988.

[15] For example, if 100 percent of children were eligible for Medicaid in a given state in each year of a birth cohort's childhood, our cumulative measure would indicate 18 years of childhood eligibility for the average child in that state and birth cohort, whereas if 50 percent of children were eligible in each year of a cohort's childhood, our measure would indicate nine years of eligibility for the average child in that state and birth cohort.

[16] In our baseline we use a fixed-in-time national sample from the 2004 CPS for all years of simulated eligibility calculations. We show in Online Appendix Table A8 that our results are insensitive to using a time-varying national sample that links each year of the CPS to the year of the simulated eligibility calculation.

[17] These data are from the National Center for Health Statistics (NCHS) files at the National Bureau of Economic Research (NBER) (NCHS, 1977-1987).

[18] Unemployment rate data are from Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics Series. Poverty rates are from the US Census Bureau. Alcohol consumption data are from National Institutes of Health (Haughwout and Slater, 2018). HIFA waiver data is from Atherly et al. (2012). Police officers per capita are from the UCR. Firearm data are from the Rand State Firearm Law Database (Cherney, Morral, and Schell 2018). Marijuana policy data are from the National Alliance for Model State Drug Laws (2017).

[19] State EITC credit amounts are from Daniel Feenberg's listing at the NBER (2016). AFDC/TANF data are from the Urban Institute's Welfare Rules Database at www.urban.org. Unemployment rate data are from the BLS.

[20] The estimated coefficients on the maternal, adulthood, and childhood control variables are presented in the Online Appendix (Table A3). They are generally insignificant, but indicate that crime rates are positively related to the proportion of births to nonwhites and to per capita alcohol consumption.

[21] Because we estimate the effects of childhood eligibility for the Medicaid and CHIP programs, the estimates are intent-to-treat (ITT) rather than treatment-on-the-treated (ToT) estimates since not everyone who is Medicaid eligible enrolls in the program. Extrapolating our ITT estimates to ToT estimates would necessarily rely on assumptions between criminal propensity and Medicaid take-up. Neither the propensity for later-life crime nor the the likelihood of take-up is likely to be uniformly distributed across the eligible population. Additionally, spillover effects would also confound ToT estimates since one needs not actually receive treatment to have Medicaid eligibility affect their propensity for crime (e.g., several members of a peer group receiving treatment may influence the criminal propensity of the whole group). Because of all these uncertainties, we are wary of estimating the magnitude of ToT effects beyond saying that they are larger than the ITT effects. Furthermore, because policy makers can directly influence eligibility but only indirectly influence enrollment, we view the ITT estimates as the most policy relevant.

[22] For example, Fletcher and Wolfe (2009) find that childhood ADHD (which occurs at markedly higher rates among boys than girls) dramatically increases the probability of engaging in crime during adulthood. Anderson, Cesur, and Tekin (2014) find that youth depression (which manifests differently for boys and girls) is associated with increases in selling illicit drugs as an adult among girls but not among boys.

[23] See, for example, Centers for Disease Control and Prevention, Data and Statistics on Children's Mental Health available at: https://www.cdc.gov/childrensmentalhealth/data.html

[24] Note that while the Medicaid expansions shifted the marginal child gaining eligibility to be one from a relatively higher income family, the work requirements included in welfare reform of the mid-1990s moved many of the lowest-income families out of AFDC/TANF participation and into work. For some of these families, this also led to a loss of Medicaid (Kaiser Family Foundation 1998).

[25] A detailed description of the process for estimating impacts by race (and its limitations) is provided in the Online Appendix.

[26] Note that because the effects are estimated across all of adulthood (rather than for ages 19-24) and utilize less variation and a smaller sample than the baseline estimates, the insignificant estimates for whites do not necessarily imply that Medicaid has no effect on crime during young adulthood for whites.

[27] For brevity, we present the robustness checks using different methods to construct the crime rates, different sets of state-level trend variables, fewer controls, and using alternative functional forms and levels of data aggregation in the Online Appendix. The results of these robustness checks are all generally similar to our baseline results.

[28] The abortion rate data was obtained from the Guttmacher Institute (https://data.guttmacher.org/states). The minimum dropout age data for 2000-2010 was obtained from Table A1 of Gilpin and Pennig (2015). We are grateful to Greg Gilpin for providing data for earlier years.

[29] We compiled the PDMP data from the state profiles on the Prescription Drug Monitoring Program Training and Technical Assistance Center (https://www.pdmpassist.org/). We measure the rigor of the programs using an indicator for whether they extended beyond Schedule IV drugs.

[30] Because these measures are only available for a subset of states, we first repeat our baseline specification for the sample of 28 states for which we have data (the additional excluded states are AK HI, ID, ME, ND, SD, and WY), and then repeat the specification while including controls for crack cocaine markets.

[31] We view these estimates as suggestive and not conclusive in part because our data do not include month or state of birth, which limits our ability to perform a more careful assessment of the impacts of Medicaid expansions received in utero. We leave this to future research.

[32] Because the crime rates among older groups are not reported by individual age level in the UCR, we are limited to estimating the models at the more aggregated state-birth cohort level rather than at the state-birth cohort-age level.

[33] We choose these age groups because they are less likely to have children under 18, whose increased Medicaid eligibility could potentially impact their parents' crime rates.

[34] Because more states had no law enforcement agencies that reported in every year during the 1974-1991 period, this sample includes 30 states, resulting in 360 observations (30 states*12 years of crime outcomes).

[35] The estimates indicate dollar values of $6 for robbery, $2 for burglary, $31 for larceny, $4 for motor vehicle theft, $42 for drug crimes, and $24 for DUI for each additional year of childhood Medicaid eligibility.

[36] Bacon-Goodman (2021) finds that for cohorts born in the 1960s and 1970s, the cost-benefit ratio of Medicaid is $1.17.

Table 1 - Impact of Additional Years of Childhood Medicaid Eligibility on Early Adulthood Crime Rates

	(1)	(2)	(3)	(4)
	Violent	**Property**	**Drug**	**DUI**
Eligibility (OLS)	-0.93**	-8.51***	-6.72***	-5.58***
	(0.45)	(1.39)	(2.09)	(1.47)
Eligibility (RF)	-0.64	-10.37***	-7.92***	-4.11***
	(0.40)	(1.24)	(1.96)	(0.88)
Eligibility (IV)	-0.89	-14.43***	-11.02***	-5.72***
	(0.55)	(1.73)	(2.73)	(1.23)
% change	-1.7	-9.4	-6.7	-4.3
Dep. Var. Mean	53.3	153.2	165.5	133.3

Number of observations is 2,520. Columns report estimates of β_1 from Equation 3 in the text. The first stage estimate (standard error) [f-statistic] of α_1 from Equation 2 is 0.72 (0.01) [1,810] . Standard errors, reported in parentheses, are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include state, year, and age fixed effects, state-specific linear time trends, and the control variables listed in Table A2. Eligibility (OLS) estimates are from crime rate regressed on actual eligibility. Eligibility (RF) estimates are from crime rate regressed on simulated eligibility. Eligibility (IV) estimates are from crime rate regressed on actual years of Medicaid eligibility instrumented by simulated eligibility. The *% change* row reports the (coefficient/dep. var. mean)*100. *p < 0.10, **p < 0.05, ***p < 0.01.

Table 2 - Treatment Effects By Sex

	(1)	(2)	(3)	(4)
Males	**Violent**	**Property**	**Drug**	**DUI**
Eligibility	-2.27**	-25.47***	-21.38***	-7.97***
	(0.98)	(2.49)	(4.58)	(2.18)
% change	-2.6	-12.2	-7.9	-3.8
Females				
Eligibility	0.30	-3.69**	-0.95	-1.90***
	(0.29)	(1.60)	(1.09)	(0.69)
% change	1.8	-3.9	-1.8	-3.6

Number of observations is 2,520. Columns report estimates of β_1 from Equation 3 in the text. Standard errors, reported in parentheses, are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include state, year, and age fixed effects, state-specific linear time trends, and the control variables listed in Table A2. The % change rows report the (coefficient/dep. var. mean)*100. *p < 0.10, **p < 0.05, ***p < 0.01.

Table 3 -Treatment Effects By Age of Eligibility

	(1) First Stage	(2) Violent	(3) Property	(4) Drug	(5) DUI
Males					
Eligibility ages 0-5	0.86***	-1.77	-13.87	-18.90	-4.02
	(0.01)	(4.57)	(8.29)	(14.19)	(7.79)
% change		-1.9	-5.7	-6.6	-2.0
Eligibility ages 6-11	0.86***	-2.91	-21.22***	-7.95	1.75
	(0.01)	(3.13)	(6.54)	(9.73)	(4.95)
% change		-3.2	-8.8	-2.8	0.9
Eligibility ages 12-18	0.72***	-0.14	-25.24***	-40.66***	-23.85***
	(0.01)	(2.17)	(6.18)	(9.24)	(5.87)
% change		-0.2	-10.4	-1.4	-1.2

Number of observations is 2,520. Column (1) reports estimates of α_1 from Equation 2 in the text. Columns (2)-(5) report estimates of β_1 from Equation 3 in the text. Standard errors, reported in parentheses, are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include state, year, and age fixed effects, state-specific linear time trends, and the control variables listed in Table A2. *Eligibility ages 0-5* , *Eligibility ages 6-11* , and *Eligibility ages 12-18* measure cumulative eligibility for each of these sub-periods of childhood. The % change rows report the (coefficient/dep. var. mean)*100. Results for females are reported in Table A5. *p < 0.10, **p < 0.05, ***p < 0.01.

Table 4 - Treatment Effects on Adolescent Crime (ages 15-18)

	(1) First Stage	(2) Violent	(3) Property	(4) Drug	(5) DUI
Males					
Eligibility ages 0-18	0.73***	4.42	-32.08***	-17.55**	-3.45
	(0.02)	(2.99)	(7.77)	(8.19)	(3.39)
% change		4.0	-6.3	-6.4	-6.7
Eligibility ages 0-5	0.86***	-2.33	-81.00***	0.74	-8.17
	(0.02)	(7.00)	(26.38)	(24.34)	(10.36)
% change		-2.1	-15.9	0.3	-15.7
Eligibility ages 6-11	0.83***	8.85	-12.72	-34.63**	-6.93
	(0.02)	(5.43)	(18.99)	(16.54)	(9.02)
% change		8.1	-2.5	-12.7	-13.3
Eligibility ages 12-18	0.79***	2.80	-11.15	-13.21	1.46
	(0.01)	(3.52)	(15.33)	(15.80)	(7.66)
% change		2.6	-2.2	-4.9	2.8
Females					
Eligibility ages 0-18	0.73***	-2.28	-11.86**	-7.23***	-0.78
	(0.02)	(1.39)	(5.04)	(1.84)	(1.23)
% change		-12.0	-5.8	-16.3	-6.8
Eligibility ages 0-5	0.86***	-4.05*	-30.47**	-13.92**	-4.19
	(0.02)	(2.08)	(13.64)	(5.65)	(3.21)
% change		-21.3	-15.0	-31.3	-36.5
Eligibility ages 6-11	0.83***	-0.06	-9.86	-6.91**	-2.32
	(0.02)	(1.90)	(8.18)	(3.22)	(2.77)
% change		-0.3	-4.8	-15.5	-20.2
Eligibility ages 12-18	0.79***	-2.67	0.22	-1.66	2.04
	(0.01)	(2.43)	(8.50)	(3.10)	(2.37)
% change		-14.0	0.1	-3.7	17.8

Number of observations is 1,584. Column (1) reports the estimate of α_1 from Equation 2 in the text. Columns (2)-(5) report estimates of β_1 from Equation 3 in the text. Standard errors, reported in parentheses, are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include age, state and year fixed effects, state-specific linear time trends, and the control variables listed in Table A2. The % change rows report the (coefficient/dep. var. mean)*100. *p < 0.10, **p < 0.05, ***p < 0.01.

Table 5 - Controls for Other Policy and Related Changes

	(1) Violent	(2) Property	(3) Drug	(4) DUI
Panel A: Baseline				
Eligibility	-0.89	-14.43***	-11.02***	-5.72***
	(0.55)	(1.73)	(2.73)	(1.23)
Panel B: Include abortion rates				
Eligibility	-0.95*	-14.50***	-10.94***	-5.64***
	(0.55)	(1.72)	(2.75)	(1.24)
Panel C: Include compulsory schooling laws				
Eligibility	-1.03*	-14.70***	-12.14***	-6.01***
	(0.56)	(1.78)	(2.70)	(1.24)
Panel D: Include controls for prescription drug monitoring programs				
Eligibility	-0.90	-14.55***	-11.60***	-6.11***
	(0.60)	(1.77)	(2.74)	(1.33)
Panel E: Include emergence of crack cocaine markets				
Crack markets sample, exclude controls for crack				
Eligibility	-1.02	-11.41***	-10.61***	-4.64***
	(0.70)	(1.97)	(2.98)	(1.38)
Crack markets sample, include controls for crack				
Eligibility	-1.02	-11.40***	-10.63***	-4.65***
	(0.70)	(1.98)	(2.98)	(1.39)
Number of observations	2,016	2,016	2,016	2,016
Panel F: Include Prenatal Medicaid Eligibility				
Prenatal eligibility sample, exclude prenatal eligibility				
Eligibility	-0.25	-7.48***	-4.45**	-1.42
	(0.69)	(1.34)	(1.89)	(1.42)
Prenatal eligibility sample, include prenatal eligibility				
Eligibility	-0.23	-8.03***	-5.06**	-1.15
	(0.70)	(1.39)	(1.97)	(1.44)
Prenatal eligibilty	1.63	-46.31*	-52.36*	23.48
	(12.53)	(24.79)	(28.11)	(20.36)
Number of observations	1,890	1,890	1,890	1,890

Number of observations in Panels A-D is 2,520. Columns report estimates of β_1 from Equation 3 in the text. Standard errors, reported in parentheses, are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include state, year, and age fixed effects, state-specific linear time trends, and the control variables listed in Table A2. *p < 0.10, **p < 0.05, ***p < 0.01.

Table 6 - Falsification Tests

	(1)	(2)	(3)	(4)	(5)
	First Stage	**Violent**	**Property**	**Drug**	**DUI**
Panel A: Crime Rates Among 50-54 year olds					
Eligibility	0.58***	-0.30	-0.72	-1.33*	-0.48
	(0.06)	(0.21)	(0.84)	(0.72)	(0.91)
% change		-4.1	-3.7	-8.3	-1.4
Panel B: Crime Rates Among 55-59 year olds					
Eligibility	0.58***	-0.13	-0.39	-0.21	-0.63
	(0.06)	(0.13)	(0.49)	(0.35)	(0.93)
% change		-3.0	-3.4	-2.9	-2.7
Panel C: Crime Rates Among 19-24 year olds born 20 years prior to study cohorts					
Eligibility	0.78***	-1.24	-9.60	5.05	-1.91
	(0.05)	(1.96)	(9.14)	(4.39)	(7.35)
% change		-2.0	-3.9	6.0	-1.3
Panel D: Other Outcome Variables	**First Stage**	**Age**	**White**	**Male**	**Minimum Dropout Age**
Eligibility	0.75***	-0.07	-0.00	0.00	-0.01
	(0.00)	(0.08)	(0.00)	(0.00)	(0.06)

Number of observations in Panels A, and B is 420, in Panel C is 360, and in Panel D is 1,385,792. Column (1) reports the estimate of α_1 from Equation 2 in the text. Columns (2)-(5) report estimates of β_1 from Equation 3 in the text. Standard errors, reported in parentheses, are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include state, year, and age fixed effects, state-specific linear time trends, and the control variables listed in Table A2. *p < 0.10, **p < 0.05, ***p < 0.01.

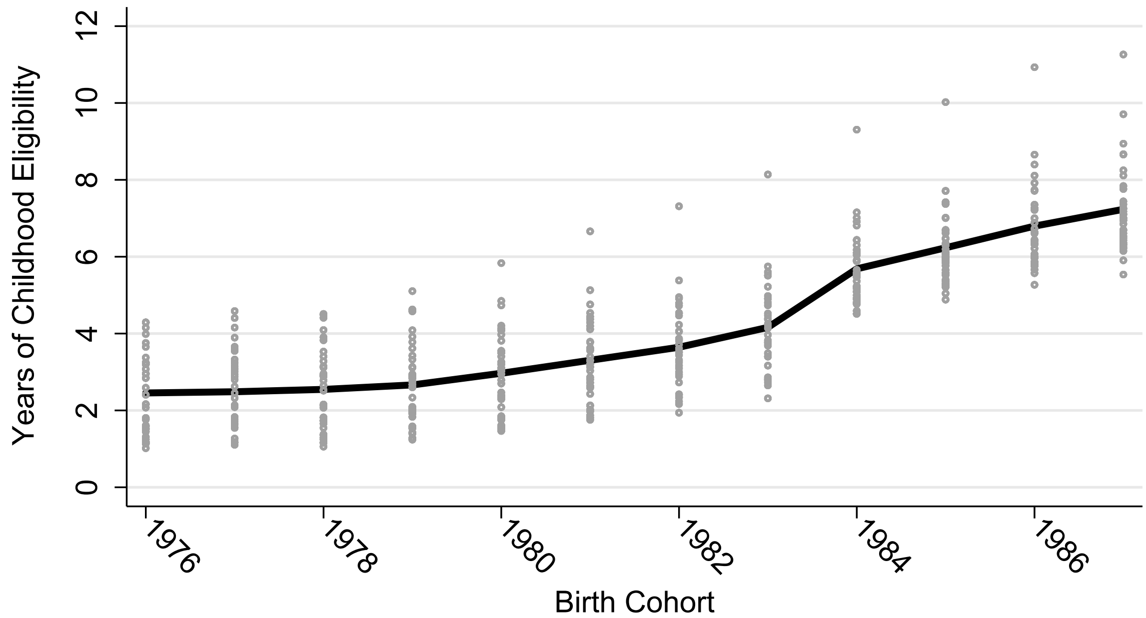

Figure 1
National and State Childhood Medicaid Eligibility by Birth Cohort

Notes: The solid line connects national estimates of eligibility for each cohort. The data points report state-level estimates of eligibility for each cohort.
Source: Authors' calculations based on CPS data. See text for description of the states included in the figure and the method used to calculate eligibility.

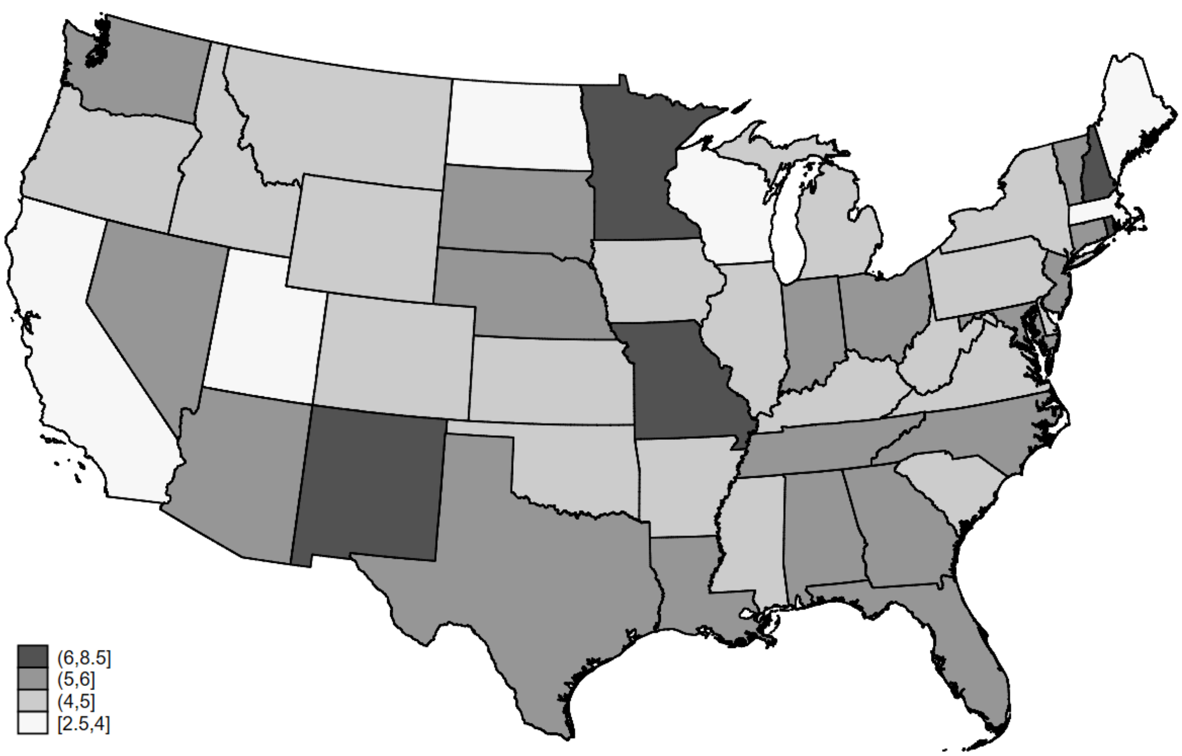

Figure 2
Increased Years of Childhood Medicaid Eligibility, 1976-1987 Birth Cohorts

Notes: Shading indicates change in state-level estimates of years of childhood Medicaid eligibility for the 1976-1987 birth cohorts.
Source: Authors' calculations based on CPS data. See text for description of the the method used to calculate eligibility.

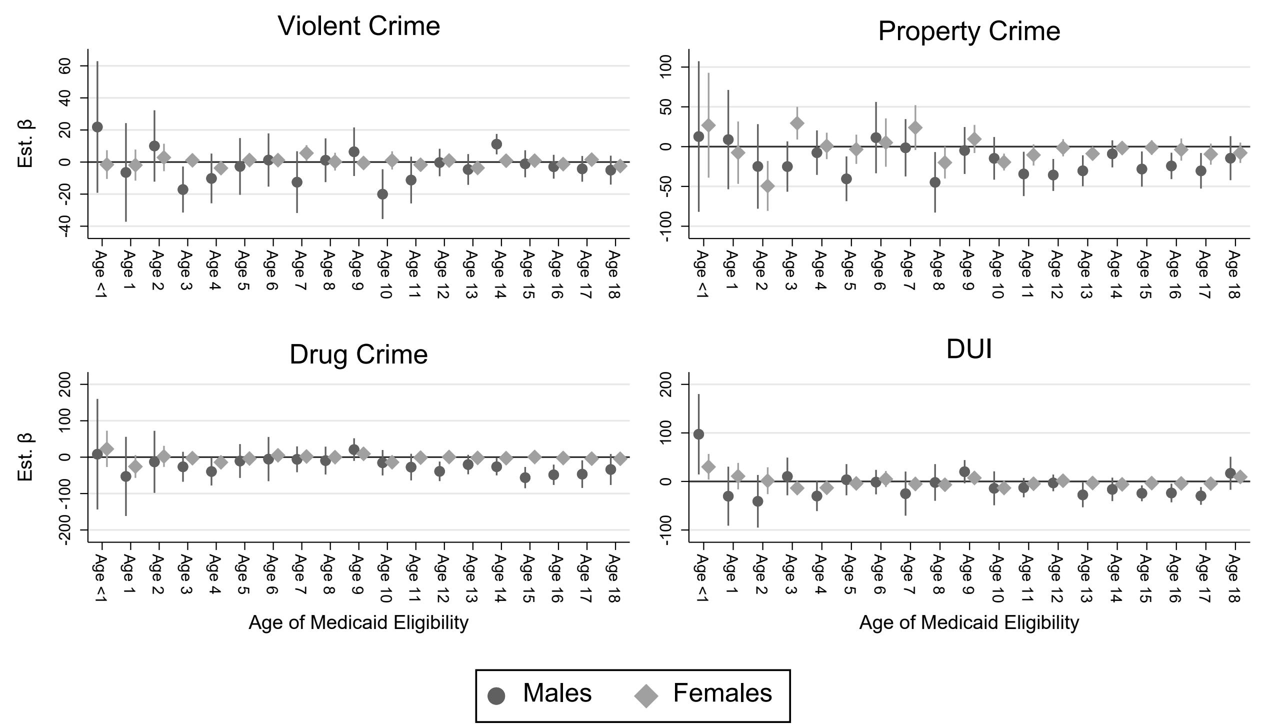

Figure 3
Estimated Effects of Eligibility Received at Different Ages

Graphs show estimated coefficients and 90% conficence intervals for β1 from Equation 3 in the text, with eligibility measured at each age of childhood.

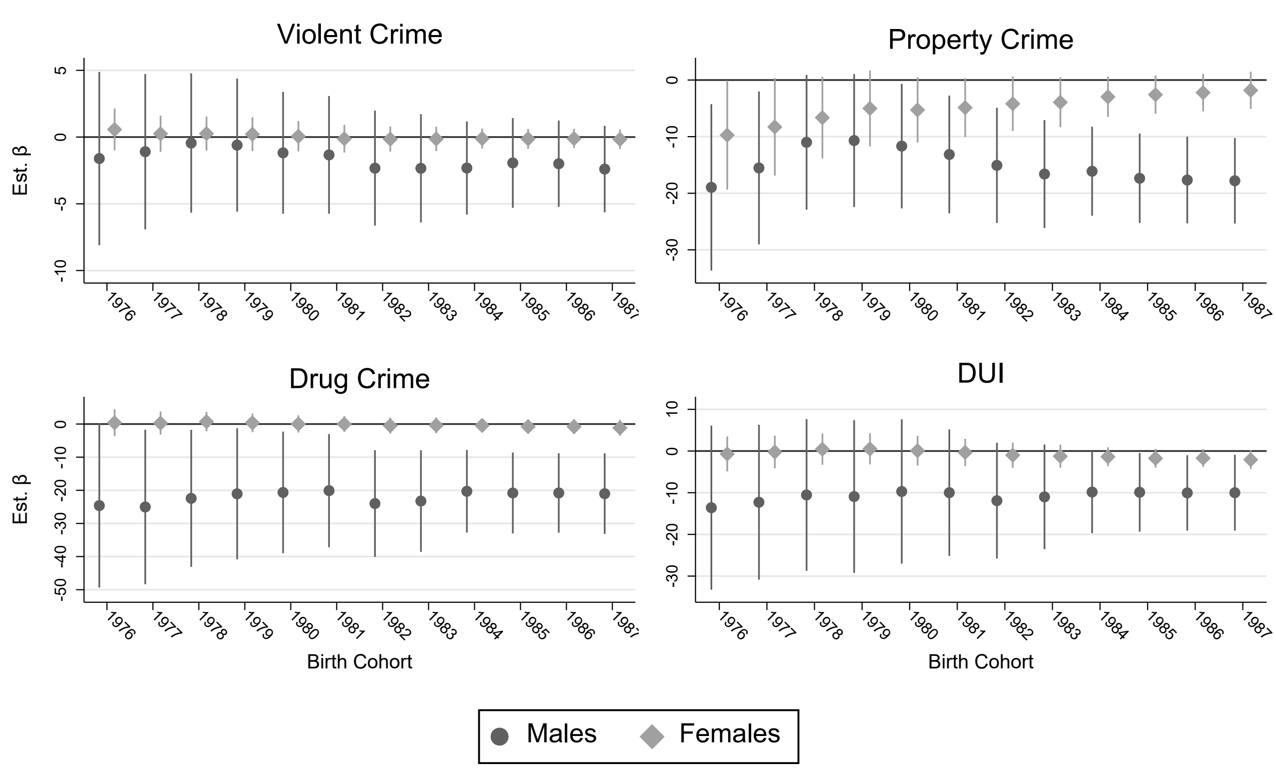

Figure 4
Estimated Effects of Eligibility For Different Birth Cohorts

Graphs show estimated coefficients and 90% conficence intervals for β(eligibility*birth cohort indicator) from Equation 3 in the text.