

LONG-TERM IMPACTS OF CHILDHOOD
MEDICAID EXPANSIONS ON CRIME

by

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TABLE OF CONTENTS

1. INTRODUCTION	1
2. MEDICAID PROGRAM BACKGROUND	5
3. LITERATURE REVIEW	9
Pathways Between Childhood Medicaid Eligibility and Adult Crime	9
Improvements to Health.....	9
Improvements to Educational and Labor Market Outcomes	12
Improvements to Family Resources	14
Childhood Social Policy and Adult Crime	15
Medicaid and Crime.....	16
4. ESTIMATION STRATEGY	19
Empirical Motivation	20
Data Sources and Variable Construction	24
Crime Measures and Data Sources: UCR.....	24
Sampling Restrictions.....	25
Linking Cohorts.....	26
Measurement Error Across Cohorts.....	26
Measurement Error Across States.....	27
Eligibility Measures and Data Sources: CPS	28
Estimating Equations.....	30
Econometric Issues	31
Conditions for Valid Instrument	31
Other Determinants of Crime.....	32
5. RESULTS	37
Main Estimates	37
Comparison of Estimates to Other Research.....	40
6. ROBUSTNESS CHECKS	44
Interstate Migration.....	44
Month of Birth	45
Simulated Eligibility National Sample Construction	45
Controls.....	46
Remaining Threats to Identification.....	46

TABLE OF CONTENTS – CONTINUED

7. CONCLUDING REMARKS.....	52
REFERENCES CITED.....	53

LIST OF TABLES

Table	Page
4.1 Summary statistics.....	34
5.1 Estimates for both sexes.....	42
5.2 Estimates by sex.....	43
6.1 Migration and Medicaid eligibility.....	48
6.2 IV estimates' sensitivity to inclusion of 1983 birth cohort	49
6.3 IV estimates' sensitivity to different national samples.....	50
6.4 IV estimates' sensitivity to the inclusion of controls.....	51

LIST OF FIGURES

Figure	Page
2.1 Eligibility across birth cohorts.....	8
4.1 Crime rates across cohorts by size of expansion.....	34
4.2 Age cohort observed at by UCR outcome year	35
4.3 Linking yearly by age crime counts to cohorts.....	35
4.4 Construction of Actual Eligibility measure.....	35
4.5 Construction of Simulated Eligibility measure.....	36
4.6 Actual and Simulated Eligibility across birth cohorts.....	36

ABSTRACT

This paper examines the effects of public health insurance expansions among children in the 1980s and 1990s on their criminal activity later in life. Using a panel of the states' 1980-1990 birth cohorts and a simulated eligibility instrumental variables strategy, I find that increases in the fraction of children eligible for public health insurance lead to substantial reductions in criminal activity. Considering the extraordinary costs of crime to victims, public budgets, and offenders, these findings suggest a previously unrecognized substantial benefit to the provision of public health insurance to children.

INTRODUCTION

Medicaid and the state Childrens Health Insurance Program (CHIP) represent the single largest component of federal expenditures on child welfare. These programs' substantial costs, and the current debate surrounding the reauthorization of CHIP, motivate a full accounting of these programs' benefits.¹ This paper seeks to explore the relationship between children's public health care eligibility and subsequent criminal activity, using a panel of the states' 1980-1990 birth cohorts. Beyond the policy implications of this study, Medicaid expansions may additionally serve as a plausibly exogenous shock to early-life determinants of crime (e.g., through Medicaid's impact on mental health), and so this study may offer suggestive information about the often endogenous relationship between crime and its early-life determinants.

Introduced in 1965, the Medicaid program represents a partnership between the federal government and the states to provide health care to low-income Americans. The program experienced a series of expansions² in the 1980s and 1990s that dramatically lowered eligibility requirements. I save the full discussion of these expansions for the following chapter, but in the broadest scope, these expansions first removed the family structure requirements for Medicaid eligibility, then granted states the option to cover pregnant women and young children with incomes up to a percent of the Federal Poverty Line (FPL), then made mandatory for states to cover pregnant women and young children with incomes up to a 133% of the FPL,

¹This paper does not distinguish between eligibility in the Medicaid program and eligibility in the CHIP program, and refers to the two programs jointly as "Medicaid" hereafter.

²These expansions include both legislation mandating the states to extend coverage to a given group, as well as legislation that provided federal matching funds to states that chose to extend coverage beyond the federal minimums.

and finally made it mandatory for states to cover all children (up to age 19) whose families' incomes were below 100% of the FPL. These expansions, with both mandated changes for all the 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.

I harness the variation created by these expansions for the 1980-1990 birth cohorts to identify the effect of health insurance eligibility in childhood on criminal outcomes over ages 18 to 24. To account for the endogeneity of economic and demographic characteristics that influence both Medicaid eligibility and criminal behavior, I utilize a simulated eligibility approach that constructs a measure of the generosity of state eligibility rules as an instrument for the fraction of children who were eligible for Medicaid among each state, year, and birth-cohort. As developed in the seminal works of Currie and Gruber (1996a,b) and Cutler and Gruber (1996), this approach isolates changes in state-level eligibility due to Medicaid eligibility policy from changes due to other economic or demographic factors.

Using arrest rates from the FBI's Uniform Crime Reporting System (UCR), I estimate the effect of childhood Medicaid eligibility for three criminal outcomes: the violent crime rate, the property crime rate, and the drug crime rate.³ I examine the effects of Medicaid eligibility on arrest rates jointly for both sexes as well as separately by sex. In the sample that includes both sexes, I find that Medicaid eligibility as a child generated large and statistically significant reductions in the violent crime rate and drug crime rate with an additional year of eligibility in childhood translating to a roughly 10 percent decrease in the violent and drug crime rates over ages 18-24.

³My criminal outcomes consist of the FBI's Part 1 offenses (minus arson), as well as the offenses of drug possession and drug manufacture/sale.

I find larger effects for males than for females, though the estimates for females are also fairly large in magnitude.

My study contributes to two literatures. The first is a growing literature on the long-term impacts of Medicaid eligibility in childhood. Discussed in detail in Chapter 3, researchers in this area have found that increased Medicaid eligibility in childhood is associated with long-term improvements to health, educational attainment, and labor market outcomes. This literature includes analyses at both the individual- and cohort-level, and has exploited for identification the staggered initial roll-out of the program, a discontinuity at the birth-month-level created by some of the expansions, and (as done in the present study) variations in eligibility produced by a series of expansions, after appropriately instrumenting for program generosity with a simulated eligibility instrument.

This paper also contributes to the strand of literature focused on the early determinants of criminal behavior. Although many studies have found correlations between crime and factors such as mental health, family environment, education, nutrition, social skills, substance use, and labor market opportunities, few have established causal links between criminal behavior and its early determinants. To the extent that becoming eligible for Medicaid due to policy changes serves as an exogenous shock to any of these factors, it can hopefully shed light on the early determinants of criminal behavior. Although this study does not fully explore the pathways through which increased Medicaid eligibility in childhood produces a long-term effect on criminal behavior, this could be a valuable area for future work.

The rest of the paper is organized as follows. In Chapter 2 I provide an overview of the Medicaid program and its expansions, and in Chapter 3 review the related literatures and consider the pathways through which childhood Medicaid eligibility may have an effect on criminal behavior later in life. Chapter 4 introduces my

empirical strategy and data sources, before presentation of my results in Chapter 5. I check the robustness of my findings in Chapter 6 and discuss their implications before giving my concluding remarks in Chapter 7.

MEDICAID PROGRAM BACKGROUND

The Medicaid program is a partnership between the federal government and the states aimed at providing health insurance to children in low income families. Medicaid was first introduced in 1965 and phased in by most states by 1970.¹ Until the 1980s, prior to the expansions used in this paper, Medicaid eligibility was primarily limited to recipients of “cash welfare” through the Aid to Families with Dependent Children (AFDC) program. The linkage of Medicaid eligibility to AFDC receipt restricted access in three ways (Currie and Gruber, 1996a). First, AFDC benefits were generally only available to single-parent households—even if a household were income and resource eligible for the program, their family structure may preclude them from AFDC (and therefore Medicaid) eligibility. Second, income thresholds for AFDC eligibility varied across states, and were often very low.² Third, the stigma of applying for cash welfare programs may have prevented eligible families from receiving Medicaid benefits. As a result, early Medicaid eligibility was typically limited to children of very low-income single mothers.

The Medicaid expansions of the 1980s and 1990s lowered eligibility requirements and increased access tremendously. These expansions were of two basic types: mandatory expansions, where it became mandatory for states to expand coverage to a given group;³ and optional expansions, which provided federal matching funds to states that chose to expand coverage beyond the federal minimums to a given group. These expansions first removed cash welfare’s family structure requirements and later cash welfare’s (often very low) income requirements for eligibility of pregnant women

¹Twenty-six states had adopted programs by the end of 1966 and by 1970 all states, save Alaska (1972) and Arizona (1982), had a program.

²For example, while Vermont had an income cutoff at 100% FPL in 1983, Kentucky in 1983 had an income cutoff of 28%.

³“Group” refers here to any combination of age, family income, and family structure.

and infants, with later expansions extending coverage to groups of older children and further lessening income requirements.

Medicaid eligibility's ties to cash welfare receipt were first weakened by the Deficit Reduction Act of 1984, which eliminated the single-parent household family structure requirements for Medicaid eligibility of young children by mandating states to cover children born after September 30, 1983 who lived in families that were income-eligible for AFDC (even if their family structure otherwise precluded eligibility for AFDC). These ties were further weakened by the Consolidated Omnibus Budget Reconciliation Act of 1985, which mandated states to cover pregnant women whose families were income-eligible for AFDC, regardless of family structure.

States were then given the option, and later the mandate, to extend Medicaid coverage to pregnant women and children up to age one in families with incomes below 100% FPL following the Omnibus Budget Reconciliation Act of 1986. The Omnibus Budget Reconciliation Act of 1987 (OBRA'87) gave states the option to extend coverage to pregnant women and children under age one in families with incomes up to 185% FPL, and well as granting states the option to extend coverage to children up to age eight born after September 30, 1983 in families with incomes below 100% FPL.

The first time a group became guaranteed coverage in their state as a function of their income relative to the FPL (rather than relative to their states' AFDC income thresholds) was infants up to age one in families with incomes below 75% FPL following the Medicare Catastrophic Coverage Act of 1988. The next "mandated to cover" group was children up to age six with family incomes up to 133% FPL following the Omnibus Budget Reconciliation Act of 1989 (OBRA'89). Finally, under the Omnibus Budget Reconciliation Act of 1990 (OBRA'90), states were required to cover all children under age nineteen who were born after September 30, 1983, and

whose family incomes were below 100% FPL.⁴

Although pregnant women and all children up to age 19 from families with incomes below 100% FPL were guaranteed coverage by the early 1990s, many children from poor families with incomes above 100% FPL were still without health insurance. In an aim to reduce the size of this group, the state Childrens Health Insurance Program (CHIP) was created in the Balanced Budget Act of 1997. The Act apportioned more than \$40 billion in federal matching funds over FY1998-FY2007 with the goal of extending eligibility for public insurance to children in families earning too much to qualify for Medicaid yet earning too little to afford private health insurance. States could expand their Medicaid programs by either (1) increasing income eligibility thresholds or extending coverage to age groups that were not previously eligible for Medicaid, (2) creating a new separate health insurance program for children, or both. Following CHIP, all children up to age 19 from families with incomes below 200% FPL were guaranteed health insurance coverage, with much higher eligibility thresholds in some states.

In summary, the expansions affecting the 1980-1990 birth cohorts first removed the family structure requirements for Medicaid eligibility to include those outside single-parent households, then lowered the income thresholds for Medicaid eligibility for pregnant women and infants, and then lowered these thresholds for young children and eventually all children. The lowered income requirements were driven by both federal expansions that mandated expanded coverage and by some states' choice to expand coverage beyond the federal minimums. The right panel of Figure 2.1 shows the increase in average total Medicaid eligibility during childhood across the 1980-

⁴While the lack of crime data that includes month-of-birth precludes a regression discontinuity design for my present study, the discontinuity at September 30, 1983 created by OBRA'87, OBRA'89, and OBRA'90 has been exploited by other researchers (Card and Shore-Sheppard, 2004; Wherry and Meyer, 2016).

1990 birth cohorts. The average child in the 1980 birth-cohort received slightly less than 3 years of eligibility during childhood, while the average child in the 1990 birth-cohort was Medicaid-eligible for almost 7 years of their childhood. The left panel of Figure 2.1 divides the states into three groups, based on the change in the total years of childhood eligibility between their 1980 and 1990 birth cohorts.⁵ Although the average child born in 1980 in each of the three groups had nearly the same level of eligibility, the states with the largest expansions gained nearly twice as much childhood eligibility across their cohorts as the states with the smallest expansions.

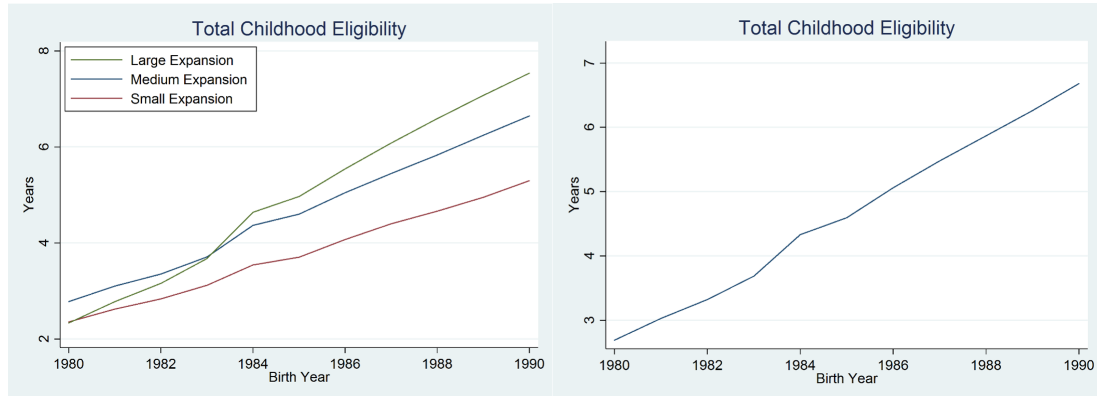


Figure 2.1: Average years of Medicaid eligibility during childhood by year of birth.

⁵Specifically, the “Small Expansion” group contains the ten states with the smallest change in eligibility (North Dakota, Alaska, Maine, Utah, Tennessee, Wisconsin, South Carolina, Wyoming, Kansas, Oregon); the “Large Expansion” group contains the ten states with the largest change in eligibility (New Mexico, Connecticut, West Virginia, New Hampshire, Arkansas, Pennsylvania, Maryland, Alabama, New Jersey, Missouri); and the “Medium Expansion” group contains the remaining 31 states.

LITERATURE REVIEW

In this chapter I explore the literature linked to potential pathways through which we may expect to see later impacts on criminal outcomes from Medicaid eligibility in childhood. These pathways include improved mental and general health, improved educational and labor market outcomes, and improvements to family resources. I next look at the research on later-life criminal outcomes impacted by childhood social policy, and conclude the chapter with discussion of the two previous studies that have estimated impacts of Medicaid on criminal behavior.

Pathways Between Childhood Medicaid Eligibility and Adult Crime

Improvements to Health

The first pathway through which childhood Medicaid eligibility could impact later-life crime is via improvements to health. Mental illness and crime are frequently linked in the research literature. Much of the work in this area has been correlational, noting high rates of mental illness among incarcerated populations (e.g., Teplin et al., 2002) and higher crime rates among those who suffer from mental illness (e.g., Swanson et al., 2002). While smaller than the correlational literature, several studies have attempted to identify a causal effect of mental illness on crime. Marcotte & Marcowitz (2010) examine crime and psychiatric drug prescription trends and find that sales of new generation antidepressants and stimulants are negatively associated with rates of violent offenses. Alternatively, they find no robust evidence of an effect of psychiatric treatment on property crimes, deaths from homicide, or arrests. Anderson, Cesur, & Tekin (2015) examine the effect of depression during youth on future crime. Using a longitudinal survey, the authors find that adolescents who suffer from depression face a substantially increased probability of engaging in property

crime¹ later in life, but they find little evidence that adolescent depression predicts later violent crime. My results differ in finding an effect on violent crime but less of an effect on property crime.

While I am not aware of any studies that have specifically looked at the effect of childhood Medicaid eligibility on mental health, there are studies that suggest a positive effect may exist. First, there is preliminary evidence of improvements to *adult* mental health following Medicaid eligibility as an *adult* from the Oregon Health Experiment (Finklestein et al., 2012), a program that randomly assigned Medicaid eligibility to childless adults. Further, childhood eligibility for public health insurance has been identified to increase mental health *treatment*.² While the effects of childhood public health insurance eligibility on mental health may be merely suggestive at this stage, there is both a large literature on short-term improvements to general health and a growing literature on long-term improvements to general health. I review the long-term general health papers below.

Exploiting the staggered timing of Medicaid’s adoption across the states, Boudreaux et al. (2016) use a longitudinal data set (Panel Study of Income Dynamics) for the 1955-1980 birth cohorts to estimate the effects of cumulative Medicaid exposure in early childhood on health and economic outcomes in adulthood. The authors find that increased Medicaid exposure in early childhood leads to meaningful improvements in adult health as measured by a chronic-conditions index. However, their imprecise estimates about the impact of Medicaid on later economic outcomes prevent them from reaching definitive conclusions.

¹The authors define the commission of a property crime as respondents who answered as having deliberately damaged property that did not belong to them or stealing at least once in the previous 12 months.

²For example, Clemens-Cope et al. (2015) find that compared to being uninsured, CHIP enrollees were more likely to have specialty and mental health visits and their parents were much more likely to feel confident in meeting the child’s health care needs; and compared to private insurance, CHIP enrollees had similar levels of health care use and unmet health care needs.

Using a different chronic-conditions index, longitudinal data set (National Longitudinal Survey of Youth, 1979), and study design, Thompson (2017) also finds that increased childhood Medicaid eligibility leads to improvements to health in adulthood.³ After instrumenting actual eligibility with simulated eligibility, Thompson (2017) finds that an additional year of public health insurance eligibility over the course of childhood results in a .079 SD improvement in health in young adulthood.⁴

Wherry and Meyer (2016) examine the immediate and longer-term mortality effects of public health insurance eligibility during childhood. Their identification exploits expansions in Medicaid eligibility (namely, OBRA'90) that applied only to children born after September 30, 1983. This feature resulted in a large discontinuity in the cumulative years of eligibility among children born around this birth date cutoff. The authors find “strong support for a sizable decline in the later-life internal-cause mortality of black children aged 15-18” but “find no evidence of a similar decline in the mortality of white children under the expansions.”

Beyond considering the impact of Medicaid eligibility in *childhood*, Miller and Wherry (2018) additionally considers the fetal environment: Medicaid eligibility for *pregnant mothers*. They use a simulated eligibility IV strategy similar to that of the present study and the others discussed in this chapter, instrumenting for childhood eligibility by simulated childhood eligibility, though they are the only study of which I

³While his treatment and outcome observations are drawn from the NLSY79, Thompson (2017) constructs a simulated eligibility instrument using the Current Population Survey (CPS) that mirrors mine in construction. Specifically, in that the national sample used for constructing the simulated eligibility instrument is drawn from all CPS respondents in all years, though I discuss this sample's construction further and test the sensitivity of my results to it in Chapter 6. Our simulated eligibility instruments also differ in construction by Thompson (2017)'s use of a subsample of the CPS whose mothers were members of the same cohorts as the mothers in the NLSY.

⁴Beyond the studied outcomes, Thompson (2017) additionally finds “clearly decreasing first stage estimates” across childhood ages, indicating that “the utilized instrument primarily identifies the marginal increase in total eligibility occurring at the lower end of the eligibility distribution” across years of eligibility.

am aware that additionally includes measures of prenatal eligibility (instrumented by simulated childhood eligibility). On health outcomes, the authors associate increased early life eligibility with decreases in hospitalizations and the presence of chronic conditions at ages 19-36.

Improvements to Educational and Labor Market Outcomes

Reductions in crime could also arise through improved educational and labor market outcomes. A number of papers have found increased education to decrease crime. Works such as Anderson (2014), Machin, Marie, & Vujić, (2011), and Lochner & Moretti (2004) use changes in compulsory schooling laws as exogenous shocks to education and find education to decrease crime in both the short- and long-run.

Studies of earlier-life educational interventions and later criminal outcomes are more limited, and have mixed findings. The largest-scale intervention, the Head Start program, aims to promote school-readiness for children under age 5 from low-income families. Garces et al. (2002) use the Panel Study of Income Dynamics and find that children who participate in Head Start are significantly less likely to report engaging in criminal activity (both minor and serious offenses) than siblings who attended another preschool. In contrast, with a similar study design comparing self-reported criminal activity between siblings (using data from the National Longitudinal Survey of Youth), Deming (2009) finds no effect on criminal activity from Head Start participation.

Another literature examines the relationship between labor markets and crime. One strand of this literature examines recidivism and labor market conditions (e.g., wages, employment, proximity to work), generally finding that improved conditions lower the likelihood of reoffending (e.g., Wang et al., 2010; Schnepel, 2017; Yang, 2017). Another strand focuses on the impact of economic conditions on aggregate

crime (e.g., Gould, Weinberg, & Mustard 2002; Machin & Meghir, 2004; Corman & Mocan, 2005) and generally finds a negative relationship, while the experimental strand, evaluating programs that aim to improve the labor market opportunities of ex-offenders, has found mixed results on reoffending (see Cook et al., 2012 for a review of the experimental studies).

Several papers have found long-term positive impacts on educational and labor market outcomes from child Medicaid eligibility. Cohodes et al. (2016) follow a simulated eligibility strategy to estimate the effect of health insurance access among both young and school-aged children on their long-run educational attainment. The authors study the 1980-1990 birth cohorts, and their educational outcomes in 2005-2012. They estimate that an increase of 1.8 years of Medicaid eligibility (i.e., a 10% increase in fraction of the birth cohort eligible) reduces high school noncompletion by 0.39 percentage points and increases bachelor's degree attainment by 0.66 percentage points. Beyond their educational outcome findings, the authors offer two other contributions to the literature. Theirs is “the first paper to provide estimates using only federal variation, purging the models of the possibility for endogenous state decisions regarding Medicaid.” They find similar estimates from their federal-variation-only model, helping validate the widely employed assumption that state Medicaid expansions are exogenous. Additionally, the authors show that their results are insensitive to using current state versus state of birth, suggesting a lack of endogenous mobility related to Medicaid eligibility.⁵

Brown, Kowalski, & Lurie (2015) use longitudinal data from the IRS to examine the effects of childhood Medicaid eligibility on educational and labor market

⁵As I discuss in Chapter 4, my empirical strategy implicitly assumes that an individual arrested in young adulthood in a state spent their childhood in that same state. While I attempt to explicitly see if Medicaid eligibility has an effect on interstate migration in Chapter 6, this finding provides some reassurance.

outcomes at each age 19-28. The authors employ an empirical strategy similar to others simulated eligibility strategies, though their reported preferred estimates come from estimating the reduced form relationship (outcomes regressed directly on simulated eligibility) rather than from specifications that use simulated eligibility as an instrument. The authors find that children with more years of Medicaid eligibility in childhood enroll in college at higher rates and are less likely to have their first dependant child during their teenage years. Females were found to have a higher wage income starting at age 22, and their relative wage premiums get larger with age, while their estimates of the impact of Medicaid eligibility on male wage incomes were smaller and imprecise.

Miller and Wherry (2018) found prenatal Medicaid eligibility to be associated with a statistically significant increase of about 4 percent in high school graduation rates. In contrast with Cohodes et al. (2016) and Brown, Kowalski, & Lurie (2015), Miller and Wherry (2018) do *not* find significant effects of childhood Medicaid eligibility at ages 1-18 on educational attainment. However, the former two studies do not include a measures of prenatal eligibility (that often increased in concert with childhood eligibility), which may explain this inconsistency.

Improvements to Family Resources

A third pathway to consider is through family resources. While I am unaware of any studies that have explicitly assessed the relationship between Medicaid eligibility and household resources, from basic theory⁶ we can predict that for some proportion of families, at least those who were paying for health care without insurance or with

⁶As the “effective price” for a child’s health care falls, if a family (1) consumes the same quantity of child health care they will experience decreased expenditures, and if they consume a higher quantity of child health care their expenditures may (2) decrease if the family’s demand for child health care is inelastic or (3) increase if the family’s demand for child health care is elastic. For the discussion here, I only wish to claim that some proportion of families fall into scenario (1) or (2).

private insurance, Medicaid eligibility serves, in part, as an income transfer. This may free up money to be spent on other resources for their child, or may reduce the time a parent chooses to work, freeing up time to be spent with the child. The income transfer may further reduce parental stress, leading to reductions in drinking, drugs, or other abusive behaviors, and help create a more generally harmonious home environment for the child.

Childhood Social Policy and Adult Crime

The body of evidence on the effects of childhood policy changes on later criminal outcomes is somewhat limited. By far the most relevant to my present study is Barr & Smith (2017), who exploit the staggered rollout of the Food Stamps Program (FSP) in the 1960s to identify the program's effect on crime. In their main analysis, they examine county-level data in North Carolina for the 1964-1974 birth cohorts, and find that each additional year of FSP availability in childhood reduces the probability of a criminal conviction in adulthood by 3-4%. The authors additionally examine county-level arrest rates in the UCR, and find FSP availability age 0-5 reduced the violent arrest rate at ages 18-24 by about 15%.

There are two other large-scale policy changes affecting children that have been linked to later crime: the legalization of abortion and changes to permissible lead exposure. Donohue and Levitt (2001) suggest that a primary cause of the crime decline of the 1990s was due to the legalization of abortion in 1973. Their results on abortion and crime have been debated, with critiques by Lott & Whitley (2001), Joyce (2004), and Foote & Goetz (2005), with responses to Joyce (2004) in Donohue and Levitt (2004) and to Foote and Goetz (2005) in Donohue & Levitt (2006).

Childhood exposure to environmental lead has been linked to a variety of social behaviors, including crime. Reyes (2007) uses a panel of state-specific legislative

reductions in lead exposure for children born between 1976 and 1980, finding the reduction of lead exposures in the to be responsible for significant declines in violent crime in the 1990s. In a study across census-tracts in St. Louis, Boutwell et al. (2016) link increased aggregate lead exposure with higher violent and non-violent crime. Additionally, Feigenbaum & Muller (2016) suggest that cities' use of lead-water pipes in the early 20th century increased homicide rates.

Studies that have examined childhood interventions and later-life crime beyond those discussed are limited. The Nurse-Family Partnership Program sends nurses to visit homes of low income first time mothers, offering a small-scale randomized control trial. Follow ups at age 15 found substantially lower self-reported incidences of arrests, convictions, and violations of probation for the children of participants relative to the children of non-participants (Olds et al., 2007).

Mixed effects on crime have been found in studies of the Moving to Opportunity Project, a major randomized housing mobility experiment sponsored by the U.S. Department of Housing and Urban Development, which moved some treated families from public housing in very disadvantaged neighborhoods to private housing in less distressed communities. An interim follow up of the program by Kling, Ludwig, & Katz (2005) found reduced violent crime arrests for males and females 4 to 7 years after random assignment, but increased property crime arrests for male youths. A longer-term follow up by Sanbonmatsu et al. (2011) found no statistically significant impacts on either violent or property crimes.

Medicaid and Crime

I conclude this chapter by discussing the two prior studies that have estimated the effect of Medicaid expansions on criminal activity. The first, Cuellar & Markowitz (2007), examine the effects of increased Medicaid spending on psychotropic drugs on a

variety of outcomes that are strongly correlated with mood disorders, including crime. The authors estimate a *contemporaneous* relationship using a Fixed Effects Poisson estimator for a panel state-level outcomes for 1991-2001. Their crime measures are counts (rather than rates as in my present study) of violent and property offenses for both children and adults, and of juvenile violent and property arrests, taken from the UCR. The authors consider three Medicaid policies to be relevant: increases in eligibility thresholds⁷, greater access to psychotropic drugs, and expansions to managed care for mental health services. They regress contemporaneous crime counts on the contemporaneous Medicaid eligibility threshold, per enrollee spending on psychotropics, and the Medicaid managed care enrollment rate.

As the authors' main aim is evaluating the impact of psychotropic drug access on crime, they consider other Medicaid program characteristics (e.g., the state's overall Medicaid generosity) as a source of bias. To address this issue, they conduct a counterfactual analysis using cholesterol-lowering drugs. They find that per enrollee spending on older antidepressants and on stimulants lead to small declines (about 1%) in violent and property crimes, but find no effect for newer antidepressants, eligibility thresholds, or managed care rate. When examining only juvenile arrests, the authors actually find a very small increase from eligibility expansions.

My analysis differs from that of Cuellar & Markowitz (2007) in several ways. Most notably, they estimate a *contemporaneous* relationship between Medicaid expansions and crime, while my study estimates the relationship between cumulative exposure to Medicaid expansions in childhood and crime in adulthood. If increased Medicaid eligibility in childhood truly does reduce later-life criminal activity, through, say, improved educational and labor market outcomes, then this crime-reducing effect

⁷While in theory changes in eligibility thresholds should map quite well to changes in the fraction of a cohort eligible, I note their difference here.

may not be detectable in contemporaneous analyses. Another difference between the studies is in the considered changes to policy produced by Medicaid expansions: my study considers only changes to the eligibility threshold (as manifested through changes in the fraction of a cohort eligible for Medicaid), while Cuellar & Markowitz (2007) additionally consider changes in drug spending and changes to the Medicaid managed care rate.

The second prior study on Medicaid expansions and crime is Wen et al. (2017). The authors consider the impact of the Medicaid expansions undertaken in four states due Health Insurance Flexibility and Accountability waivers that provided states with federally matching funds to expand Medicaid to all low-income *adults* below 200% FPL. They use a panel of annual, county-level criminal observations from the UCR between 2001-2008, but their independent variable of interest is a state-level dichotomous indicator for “HIFA State”. The authors focus on the pathway of Substance Use Disorder (SUD) treatment, to the extent that they code three states that did actually receive HIFA-waivers as non-HIFA states, as these three states did not cover SUD treatment. The authors find that HIFA-waiver expansions led to a relative 2% reduction in the robbery rate, a 1% reduction in the aggravated assault rate, and a 0.6% reduction in the larceny theft rate. The authors additionally performed an IV analysis with SUD treatment rates, concluding that increased SUD treatment rates drive the majority of the observed crime reductions.

The above analysis differs from mine in both the treatment and population treated. Adults receiving SUD treatment and children receiving, say, cognitive behavioral therapy are likely two considerably different treatments on different populations. Additionally, while both analyses utilize difference-in-difference specifications, their “Medicaid treatment variable” is a dichotomous variable while my present paper uses a continuous measure of the fraction eligible.

ESTIMATION STRATEGY

Based on the research discussed in the previous chapter, I hypothesize that increased eligibility for Medicaid in childhood will decrease criminal activity in adulthood. If this hypothesized relationship is correct, we would expect to see a more negative (or less positive) trend in crime over the birth-cohorts in states with larger expansions relative to states with smaller expansions. Figure 4.1 presents a rough summary of the relationship between increased childhood eligibility and later crime rates. As with Figure 2.1 above, Figure 4.1 separates the states into three groups based on their changes in total childhood eligibility for their 1980 and 1990 birth cohorts. For property crimes, there appears to be no clear differences between the states by size of expansion. For violent crime however, we see the states with the largest expansions (green line) experience declining average violent crime rates across the birth-cohorts, falling from about 60 violent crimes per 10,000 for their 1980 birth cohorts to 50 per 10,000 for their 1990 birth cohorts, while the states with the smallest expansions (red line) had a slight increase in the violent crime rate for their birth-cohorts. Similarly for the drug crime rate, the states with the largest expansions had a decrease from about 200 drug crimes per 10,000 to 180 per 10,000, while the states with the smallest expansions saw an increase of about 130 drug crimes per 10,000 to 160 per 10,000 (though much of this is driven by the 1980-1984 birth-cohorts, with drug crime rates remaining mostly flat for both groups of states' 1985-1990 birth cohorts). While these figures are suggestive, they do not account for other characteristics that impact crime and, importantly, they do not account for the endogenous relationship between eligibility and other factors plausibly related to crime (e.g., state income growth).

Towards that end, I begin by motivating an empirical strategy to estimate the

effect of childhood Medicaid eligibility on later-life crime. I next discuss my data sources and the construction of my variables, as well as my strategy for linking childhood observations with adulthood observations, discussing potential sources of bias. I then discuss my simulated eligibility instrumental variables (IV) strategy and present my preferred regression specifications, concluding with discussion of econometric issues.

Empirical Motivation

To identify the effect of childhood Medicaid eligibility on long-term outcomes, the ideal experiment would be to randomly assign eligibility for Medicaid to children of different ages. Then, so long as one could reliably follow the children into adulthood, one could simply compare the criminal outcomes of those who were assigned Medicaid eligibility relative to those who were not.

In practice, such an analysis is not currently feasible, as there have been no programs that randomly assigned eligibility to children. Even without random assignment, one could use the variation in Medicaid eligibility across states and birth-cohorts produced by the Medicaid expansions of the 1980s and 1990s to assess how the probability of individual i engaging in criminal behavior in adulthood, $\Pr(Crime_{i,t+T})$, is affected by their Medicaid eligibility in childhood, $Eligibility_{i,t}$, as in equation (1).

$$\Pr(Crime_{i,t+T}) = a + \beta_1 Eligibility_{i,t} + e_i \quad (1)$$

Linking an individual's Medicaid eligibility during their childhood to their criminal outcomes in adulthood requires longitudinal data that observes both. Such a study design would be subject to the common issues of longitudinal data sets, with

attrition issues likely amplified by the outcome of interest, criminal behavior. Rather than trying to observe and link eligibility and crime in individuals, we can instead compare the childhood eligibility of birth cohort c in state s with their criminal behavior in adulthood. To link a state-cohort’s criminal activity to their eligibility, one needs data on criminal activity disaggregated at the year, location, and age level. Because many criminal offenses go “unsolved”, the age of the offender remains unknown. Therefore, I measure the criminal activity of cohort c by their arrest rate (count of arrests per 10,000 population). Specifically, because criminal activity varies appreciably across the ages of young-adulthood, and I observe the different cohorts in different years for criminal activity at a given age, I measure criminal activity for cohort c as the *average yearly* arrest rate over ages 18-24.

$$CrimeRate_{sc} = a + \beta_1 Eligibility_{sc} + e_{sc} \quad (2)$$

In the specification of equation (2), I note the lack of a “year index”. This specification can be characterized as each cohort receiving a *single* treatment that is administered over their entire childhood (where $Eligibility_{sc}$ measures the treatment dose for birth-cohort c in state s), while the *single* response is observed over young adulthood (and $CrimeRate_{sc}$ measures the response to the dose for birth-cohort c in state s).

There are at least five potential sources of bias to β_1 , the estimated effect of childhood Medicaid eligibility on criminal behavior, in the specification of equation (2). I discuss two of these sources—the endogeneity of eligibility arising from factors such as family income that affect both eligibility and crime, and measurement error in linking a state-cohort’s criminal activity to their eligibility—in detail later in this chapter. A third potential source of bias to β_1 is time-invariant unobserved state characteristics that affect crime rates and are also correlated with Medicaid

eligibility. For example, low-income states would likely have both high rates of crime and high levels of Medicaid eligibility, regardless of any changes in eligibility rules. The inclusion of state fixed-effects would account for such time-invariant state characteristics that are common to all the birth cohorts of a given state. Additionally, β_1 could be biased if the nation experiences positively correlated trends in both crime and eligibility. The inclusion of time fixed-effects would mitigate this bias, by controlling for changes over time that are common to all the states. Because I measure Medicaid eligibility's effect on criminal outcomes by a cohort's *average yearly* crime rate, my time fixed-effects are at the birth cohort level.

$$CrimeRate_{sc} = a + \beta_1 Eligibility_{sc} + State_s + Cohort_c + e_{sc} \quad (3)$$

With the inclusion of state and time fixed effects, equation (3) represents a difference-in-difference specification that captures the effect of increased Medicaid eligibility (measured as a continuous rather than binary variable) for a given birth cohort on the cohort's early adulthood crime rate. While equation (3) controls for national time trends and *time-invariant* differences across the states, there remains the possibility of bias from *time-varying* differences across the states, which offers a fifth potential source of bias. Specifically, if changes in state-level unobserved characteristics affecting criminal behavior are correlated with unobserved state-level trends affecting $Eligibility_{sc}$, then they may introduce bias to estimates of β_1 . One method of mitigating this bias is to include state-specific time trends.

An additional method is to introduce a vector of time-varying state-level observable characteristics X_{sc} to control for changes over time within states that impact criminal behavior and Medicaid eligibility, as done in equation (4) below. The vector X_{sc} includes controls for other time-varying state-level policies (e.g., the

state's EITC and AFDC/TANF(cash welfare) benefit and eligibility levels) as well as the annual unemployment rate. More discussion of the controls in X_{sc} is presented near the end of this chapter.

$$CrimeRate_{sc} = a + \beta_1 Eligibility_{sc} + \beta_2 X_{sc} + State_s + Cohort_c + e_{sc} \quad (4)$$

Thus, the specification of equation (4) measures the $CrimeRate_{sc}$ of birth-cohort c in state s as a function of the their childhood Medicaid $Eligibility_{sc}$, while controlling for time-invariant characteristics across the states and national-level changes in eligibility and crime across the cohorts, as well as a set of controls for time-varying characteristics within states related to both crime and eligibility. The variable $CrimeRate_{sc}$ is the average arrest rate (number of arrests per 10,000 of the population) over ages 18-24, while $Eligibility_{sc}$ is a measure of total childhood eligibility experienced by birth-cohort c in state s , which is formed by summing the yearly fraction of the cohort eligible for Medicaid over that cohort's childhood (ages 0-18).

Note that a state-cohort's eligibility is function of both the generosity of the state's Medicaid policy *and* the cohort's socioeconomic characteristics (e.g., family income and family structure). The β_1 in equation (4) cannot separate the impact of Medicaid *policy* from other factors that impact eligibility. I discuss in more detail below the simulated instrumental variables method that I use to isolate the impact of Medicaid policy on crime.

Data Sources and Variable Construction

Crime Measures and Data Sources: UCR

Data on criminal outcomes (measured by arrest rates) come from the FBI's Uniform Crime Reporting system (UCR).¹ The UCR provides yearly arrest counts by offense and age at the police agency level. These counts are aggregated by offense and age to the state level for arrestees aged 18 to 24 (the oldest age at which the UCR records counts for a single age). These data span the years 1998 (youngest cohort aged 18) to 2014 (oldest cohort aged 24). Arrest rates are arrests per 10,000 of the specified age group population in the state.² I use three crime measures for my dependent variables: violent crime rate (composed of the offenses murder, rape, robbery, and aggravated assault), property crime rate (composed of the offenses burglary, larceny, and motor vehicle theft)³, and drug crime rate (composed of the offenses drug possession and drug sale/manufacture).

The UCR collects arrest data by the self-reporting of over 16,000 law enforcement agencies. One issue in working with UCR data is that agencies may drop in and out of reporting over time (i.e., not every agency reports in every year). This is concerning to my analysis if this drop-in-and-out is correlated with changes in Medicaid eligibility. For example, suppose Colorado has a Medicaid expansion that results in their 1984 birth cohort having more Medicaid eligibility than the 1983 cohort, but an agency dropped out of reporting in 2008. Then a decrease in the 1984 cohort's crime counts (observed in years 2002-2008) relative to the 1983 cohort's counts (observed in years 2001-2007) would bias my estimated impact of Medicaid eligibility away from zero.

¹The UCR Arrests data used in this study were collected and compiled by the Inter-University Consortium for Political and Social Research (ICPSR).

²Using county-year-age counts from the National Cancer Institute's Surveillance Epidemiology and End Results program, aggregated up to the state-year-age level.

³FBI Part 1 definitions, not including arson.

I address this issue by creating a “deflator” of the population covered by agencies that do report crime counts, for a given state and year. Thus, an agency dropping out of reporting will lower the state-level aggregate crime count (numerator), but will also lower the considered population (denominator). The deflator is constructed by first finding the percentage of the state population covered by reporting agencies in each year.⁴ I then create a yearly deflator for each state as the ratio of the percentage of the state population covered by reporting agencies *in that year* to the percentage of the state population covered by reporting agencies *in the year of their fullest reporting*.

Sampling Restrictions. The possible universe for my study would be 11 birth cohorts (1980-1990) and 51 states (561 observations). However, data limitations in the UCR prevent me from including all 51 states in my sample. I drop Florida and D.C. because they did not report crime counts to the FBI during the study period, and Illinois from regressions with drug crime because Illinois did not report drug crimes during most of the study period. I also drop Kentucky and New York due to extreme variance over the years in their reporting. This leaves 11 cohorts and 47 states (517 observations) in regressions with violent crimes and property crimes, and 11 cohorts and 46 states (506 observations) in regressions with drug crimes. I also drop some *years* of crime counts due to data quality. Because my final dependent variables are *average* rates, this does not affect my number of observations.⁵ Specifically, I drop Alabama’s crime counts for 2010-2014⁶, as they fall to less than 10% of their previous

⁴This assumes that if an agency reported counts for offense A but none for offense B, then the true count of offense B was zero.

⁵For example, dropping the yearly crime rates for the cohorts in 2011 means that the 1987 cohort’s average crime rates are averaged over ages 18-23 rather than 18-24, while the 1988 cohort’s are averaged over ages 18-22 and age 24 (and so on).

⁶While not shown in the results of the present paper, estimates from models where Alabama is dropped entirely increases the precision of my estimates as well as their magnitudes. This is unsurprising, as Alabama was one of the states with the largest increases in Medicaid eligibility across the studied birth-cohorts, and so averaging yearly crime rates for their later cohorts (much

counts after 2010. I further am unable to use data on Kansas 1998-1999 or Wisconsin 1998-2001 due to these states not reporting to the UCR in those years. Finally, I drop Montana 2004 and Oregon 2013 due to being extreme outliers, and set Iowa's drug counts for males aged 23 in 2013 to the counts of the years before, as their drug arrest count went from about 100 in 2012 to over 4000 in 2013.

Linking Cohorts. This section describes how I link year-age-level arrest counts to cohort-level arrest counts. Figure 4.2 shows how, for example, we observe the 1980 birth cohort's crime in 1998 when they are 18, in 1999 when they are 19, and so on though 2004 when they are 24, and we observe the 1981 birth cohort's crimes when they are 18 in year 1999, when they are 19 in 2000, and so on. Figure 4.3 gives an example of linking year-age-level arrest counts in Alabama to Alabama's 1980-1983 cohorts. In 1998, I observe a crime count of 716 arrests of 18 year-olds (1980 cohort). In 1999, I observe a crime count of 721 arrests of 18 year-olds (1981 cohort) and a crime count of 543 arrests of 19 year-olds (1980 cohort). Once I have linked the crime counts from the year-age cells to the year-cohort cells, I divide the crime counts in each year-cohort cell by its' population,⁷ to produce yearly crime rates for each of the cohorts. The average yearly crime rate across ages 18-24 within a state-cohort then forms the dependent variable in my regressions.

Measurement Error Across Cohorts. This strategy for linking a state-cohort's eligibility in childhood to their criminal activity in young adulthood produces measurement error across cohorts and across states. Consider observing a 19 year-

higher eligibility than their earlier cohorts, relative to the average difference across cohorts in all the states) only over their observations when younger (e.g., over ages 18-20 for their 1990 cohort) biases the within-group estimator significantly for Alabama, and this bias shows up in the estimated $\hat{\beta}_1$ of equation (4).

⁷That is, I divide the crime counts of 18 year-olds in 1998 in Alabama (716 arrests) by the population of 18 year-olds in 1998.

old arrested in June of 2000. Such an individual could have been born at any time between June 1980 and June 1981. If they were truly born in, say, August 1980, then my specification would consider this individual as belonging to the 1981 birth cohort, inflating that cohort's crime rate and deflating the crime rate of the 1980 birth cohort. However, this pattern applies to all the cohorts: each cohort "gives" some of its crimes to the cohort before it, and "takes" some of the crime from the cohort after it. So long as this measurement error is independent and identically distributed, or most crucially that eligibility and the distribution of crimes or births across the months of a year are not correlated, it will simply attenuate my estimates towards zero. Because of the eligibility discontinuity at October 1983, this assumption may be invalid. I show in Chapter 6 that my results are not sensitive to the inclusion or exclusion of the 1983 birth cohort.

Measurement Error Across States. My linking strategy also assumes that an individual's state-of-birth and state-of-arrest are the same. For this measurement error to result in more than attenuation, interstate migration would have to be correlated with both Medicaid eligibility and criminal activity. Consider a high eligibility state (e.g., Connecticut) and a low eligibility state (e.g., Mississippi), and assume that 1,000 individuals born in each moves to the other at age 18. If the individuals who moved from Connecticut to Mississippi had a crime rate of .2, and the individuals who moved from Mississippi to Connecticut had a crime rate of .01, this would bias my estimated effect of eligibility on crime away from zero. In Chapter 6 I show that a birth cohort's childhood Medicaid eligibility does not appear to predict its members moving out of state by age 24.

Eligibility Measures and Data Sources: CPS

I construct two measures of total childhood Medicaid eligibility using the March supplement to the Current Population Survey (CPS).⁸ The first measure, which I refer to as “Actual Eligibility”, comes from applying individual level survey data on state of residence, year of birth, family structure, and family income to a “Medicaid Calculator” containing the relevant thresholds for eligibility, as shown in Figure 4.4. By comparing an individual child’s data to the relevant threshold, the calculator marks for each CPS respondent whether or not they were eligible for Medicaid in that year. For example, a 5 year-old CPS respondent in Alabama in 1985 would be compared against the Alabama eligibility criteria in that year to generate an indicator for whether or not they were eligible for Medicaid in 1985. The mean value within that cohort of the binary “eligible” variable then measures the fraction of the 1980 birth cohort that was eligible for Medicaid in 1985. This process would be repeated for respondents belonging to the 1980-1990 birth cohorts of each age in each year in each state, for all years 1980-2008 (the year in which the 1980 birth cohort was born through the year in which the 1990 birth cohort turned 18). Summing the yearly fraction eligible for each birth cohort in each state across that cohort’s childhood generates a cumulative measure of childhood eligibility for Medicaid for each state-cohort.

There are two sources of variation across the states and years in Actual Eligibility. The first source comes from changes in eligibility thresholds (middle column in Figure 4.4), the variation upon which this paper seeks to identify an effect. The second source of variation comes from changes in economic and demographic

⁸Sarah Flood, Miriam King, Steven Ruggles, and J. Robert Warren. Integrated Public Use Microdata Series, Current Population Survey: Version 5.0. [dataset]. Minneapolis: University of Minnesota, 2017. <https://doi.org/10.18128/D030.V5.0>.

characteristics (left column in Figure 4.4) across the states and years. That is, the fraction of a birth cohort eligible for Medicaid could increase (1) due to more generous Medicaid policy raising the eligibility thresholds, and (2) due to a fall in incomes (caused by, say, a recession in that state) that pushes more families with children below the existing eligibility threshold. This second source of variation is concerning for identifying the effect of Medicaid eligibility, as factors such as family income and family structure affect both Medicaid eligibility and criminal activity.⁹ Failure to control for this second source of variation would bias my estimates of the effect of childhood Medicaid eligibility on crime. To isolate the variation in eligibility that results only from policy changes, I follow Currie and Gruber (1996a) by instrumenting for changes in Actual Eligibility with an index of the state’s Medicaid program generosity.

This index, called “Simulated Eligibility,” is calculated by taking a national sample from the CPS, and finding the fraction of a given age-year cell in that sample that would have been eligible for Medicaid, if they had lived in a given state. That is, it simulates each state’s fraction eligible, based on the fraction eligible of a national (rather than state-level) sample.

By using a national sample that does vary across the states in demographic or economic characteristics, I remove the effect of state-specific population characteristics and isolate the generosity of the states’ Medicaid programs. As framed by Currie and Gruber (1996b), this simulated eligibility measure offers “a convenient parameterization of legislative differences affecting children in different state, year, and age groups,” as “a natural way to summarize the generosity of state Medicaid policy as it affects each group is in terms of the effect it would have on a given,

⁹That is, I hypothesize that the effect of an increase in Actual Eligibility on crime rates from these two sources of variation would be in opposite directions, as decreases in family income have some direct effect on criminal activity beyond an effect through Medicaid eligibility.

nationally representative, population.”

While Simulated Eligibility, *on average*, tracks Actual Eligibility quite closely across the cohorts (as shown in Figure 4.6), for some states they can be quite different. For example, applying Alaska’s eligibility rules to CPS respondents from Alaska’s 1980 birth cohort over their childhood (years 1980-1998) produces an estimated Actual Eligibility of 2.7 years, while applying Alaska’s eligibility rules to CPS respondents from the national 1980 birth cohort over their childhood (again, 1980-1998) produces an estimated Simulated Eligibility of 4.5 years. Or, in yearly terms, 6% of Alaska’s 1980 birth cohort was eligible for Medicaid in 1980, while 23% of the national 1980 birth cohort would have been eligible for Medicaid in 1980 if they had lived in Alaska.

Estimating Equations

My model’s first stage, shown in equation (5), regresses *ActualEligibility_{sc}* for cohort *c* in state *s* on the *SimulatedEligibility_{sc}* instrument for that state-cohort and the full set of controls to produce the predicted *ActualEligibility_{sc}*, $\widehat{ActualEligibility}_{sc}$, for a cohort *c* in state *s*, given their *SimulatedEligibility_{sc}*.

$$ActualEligibility_{sc} = \pi_0 + \pi_1 SimulatedEligibility_{sc} + \pi_2 X_{sc} + State_s + Cohort_c + v_{sc} \quad (5)$$

$$\widehat{ActualEligibility}_{sc} = \hat{\pi}_0 + \hat{\pi}_1 SimulatedEligibility_{sc} + \hat{\pi}_2 X_{sc} + State_s + Cohort_c$$

$$Eligibility_{sc} = \widehat{ActualEligibility}_{sc}$$

I refer to the predicted actual eligibility from the first stage regression as “Eligibility” throughout the rest of the paper. With *Eligibility_{sc}* purged of endogenous variation,

the second stage regression is then shown in equation (6),

$$CrimeRate_{sc} = \beta_0 + \beta_1 Eligibility_{sc} + \beta_2 X_{sc} + State_s + Cohort_c + u_{sc} \quad (6)$$

where $CrimeRate_{sc}$ is the average yearly arrest rate per 10,000 of cohort c in state s over ages 18-24, $Eligibility_{sc}$ is a measure of the fraction of cohort c in state s eligible for Medicaid (after instrumenting in equation (5) by $SimulatedEligibility_{sc}$, the fraction of the national cohort c who would have been eligible for Medicaid if they had lived in state s), X_{sc} is a vector of state-level time-varying controls, $State_s$ is a dummy variable for each state, and $Cohort_c$ is a dummy variable for each cohort. I estimate equation (5) for the full sample containing both sexes¹⁰ and estimate equation (6) for both the full sample and separately for males and females (results presented in Chapter 5), and check their sensitivity to different constructions of the simulated eligibility instrument, the inclusion or exclusion of controls, and the exclusion of the 1983 cohort (results presented in Chapter 6). Table 4.1 presents summary statistics of my data.

Econometric Issues

Conditions for Valid Instrument

The relevance condition is certainly satisfied for the simulated eligibility instrument, as shown by the very strong first-stage relationship in Panel C of Table 5.1. In other words, simulated eligibility strongly predicts actual eligibility.

The exclusion restriction for this instrument to identify the causal effect of Medicaid eligibility is that the only channel through which simulated eligibility affects

¹⁰Because eligibility for Medicaid does not depend on sex, and I assume that sex is evenly distributed across income levels within a birth-cohort, I do not estimate equation (5) separately by sex.

criminal outcomes is via its impact on eligibility for Medicaid. With the inclusion of state and time fixed effects, this amounts to the variation across states in the years of Medicaid expansions being independent of criminal outcomes, except through increased eligibility for Medicaid.

While this seems very plausible, a potential threat in any panel study of a policy is legislative endogeneity. Instrumental variables estimates would be biased if states expanded Medicaid in response to state-level trends in crime. While this may be a valid concern when estimating the contemporaneous effect of Medicaid eligibility on a visibly related outcome (e.g., legislators responding to poor infant health in a state in the early 1980s by expanding Medicaid beyond the federal minimum in 1987), this is likely implausible for an unstudied outcome that’s not realized for over a decade.

Other Determinants of Crime

With measurement error produced by my linking strategy arguably only attenuating my results, the inclusion of state and time fixed effects to control for time-invariant state-level unobservables and national trends in eligibility and crime, and isolating changes in eligibility arising from changes to state Medicaid policy rather than from changes in family income and family structure by my IV strategy, I view the remaining threats to estimating the causal effect of Medicaid eligibility as “what belongs in X_{sc} ?” As discussed in this chapter’s first section, changes in the state-level unobservable characteristics affecting criminal behavior may introduce bias to estimates of β_1 in equation (6) if they are correlated with eligibility. This is the “parallel trends” assumption invoked in difference-in-difference specifications: in the absence of a Medicaid program, would changes in the rates of criminal behavior across the states over their birth-cohorts have been equal?

To address this question, I control for several state-level policies, as well as the

state-level unemployment rate, that affected the cohorts during their childhoods. For state-level policies, I currently control for exposure to the “cash welfare” programs AFDC/TANF (Temporary Assistance to Needy Families) by including the average maximum welfare benefit level and average minimum eligibility threshold that a state-cohort is exposed to over their childhood, as well as average state Earned Income Tax Credits (EITC) credit amounts.

Outside of these programs that I control for, did states that expanded Medicaid also implement other policies that affected criminal outcomes? For policies affecting the cohorts during adulthood, changes in policing practices could also result in changes to criminal behavior, especially as I measure criminal behavior by arrest rates. While my state fixed effects handle time-invariant differences across the states in policing practices, my estimates could be biased if changes in policing practices are correlated with changes in childhood Medicaid eligibility. My analysis does not currently control for changes in policing practices.

Policy changes affecting the cohorts during childhood could bias my estimated effects as well if they affected criminal outcomes and were correlated with Medicaid eligibility. Those identified in the literature that appear to have the greatest possibility of driving my results are the Food Stamps Program, the legalization of abortion, changes in compulsory schooling laws, and reduced environmental lead exposure following The Clean Air Act. I discuss the potential biases from the absence of controls for these policies at the end of Chapter 6.

Finally, in addition to changes in state-level policies, one may wish to control for changes in states’ population characteristics that may affect criminal behavior beyond an effect on Medicaid eligibility. While I currently only control for the unemployment rate, it may be desirable to control for other characteristics as well, such as income per capita, teen pregnancy rate in birth years, beer consumption, and firearms per

capita.

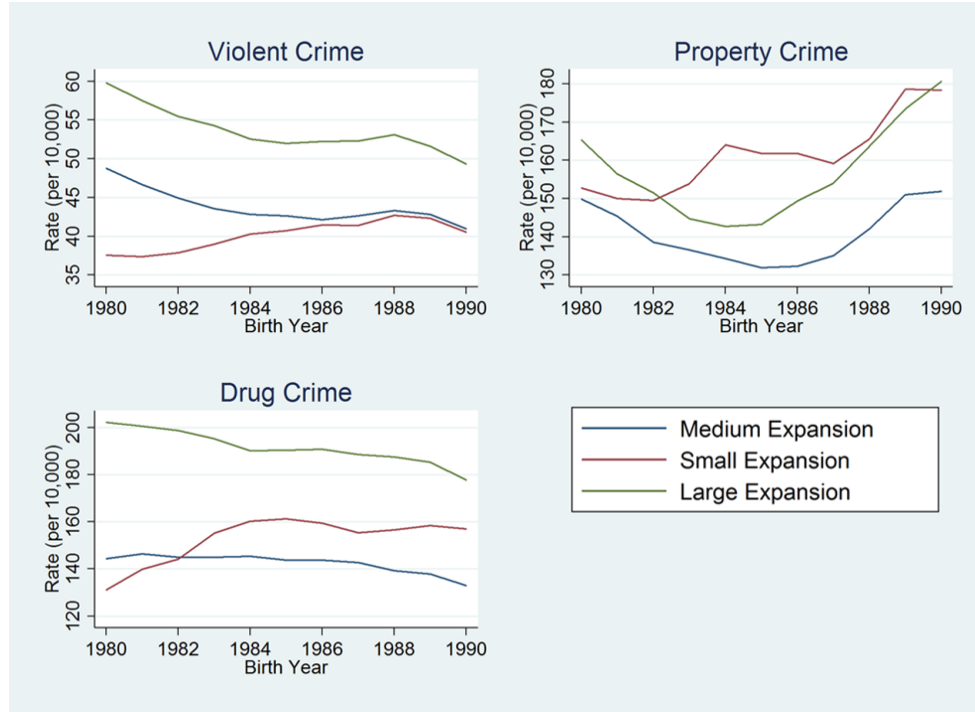


Figure 4.1: Crime rates across cohorts by size of expansion.

	Mean	Std. Dev	Min	Max
Total Eligibility	4.583	1.785	1.112	10.42
Violent Rate	44.72	20.43	9.337	101.1
Property Rate	148.5	44.51	40.12	290.1
Drug Rate	159.1	57.59	22.29	336.9
Observations	517			

Table 4.1: Summary statistics.

		Cohort observed at Age						
		18	19	20	21	22	23	24
Year	1998	1980						
	1999	1981	1980					
	2000	1982	1981	1980				
	2001	1983	1982	1981	1980			
	2002	1984	1983	1982	1981	1980		
	2003	1985	1984	1983	1982	1981	1980	
	2004	1986	1985	1984	1983	1982	1981	1980
	2005	1987	1986	1985	1984	1983	1982	1981
	2006	1988	1987	1986	1985	1984	1983	1982
	2007	1989	1988	1987	1986	1985	1984	1983
	2008	1990	1989	1988	1987	1986	1985	1984
	2009		1990	1989	1988	1987	1986	1985
	2010			1990	1989	1988	1987	1986
	2011				1990	1989	1988	1987
	2012					1990	1989	1988
	2013						1990	1989
	2014							1990

Figure 4.2: Age at which cohort's crimes are observed at by UCR outcome year.

State	Year	Counts by Age							Counts by Cohort			
		18	19	20	21	22	23	24	1980	1981	1982	1983
Alabama	1998	716	536	422	354	306	233	215	716			
Alabama	1999	721	543	538	409	363	330	303	543	721		
Alabama	2000	785	650	487	509	402	377	342	487	650	785	
Alabama	2001	763	667	563	503	482	408	355	503	563	667	763
Alabama	2002	713	670	525	467	434	399	287	434	467	525	670
Alabama	2003	590	484	416	426	329	332	261	332	329	426	416
Alabama	2004	542	462	425	414	314	300	286	286	300	314	414
Alabama	2005	731	579	542	479	426	384	343		343	384	426
Alabama	2006	942	767	672	618	475	459	381			381	459
Alabama	2007	873	771	658	623	501	437	402				402

Figure 4.3: Linking yearly by age crime counts to cohorts.

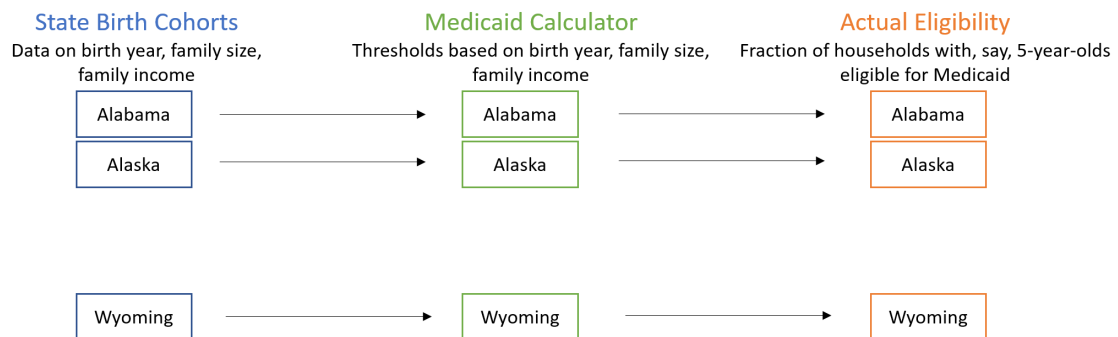


Figure 4.4: Construction of Actual Eligibility measure.

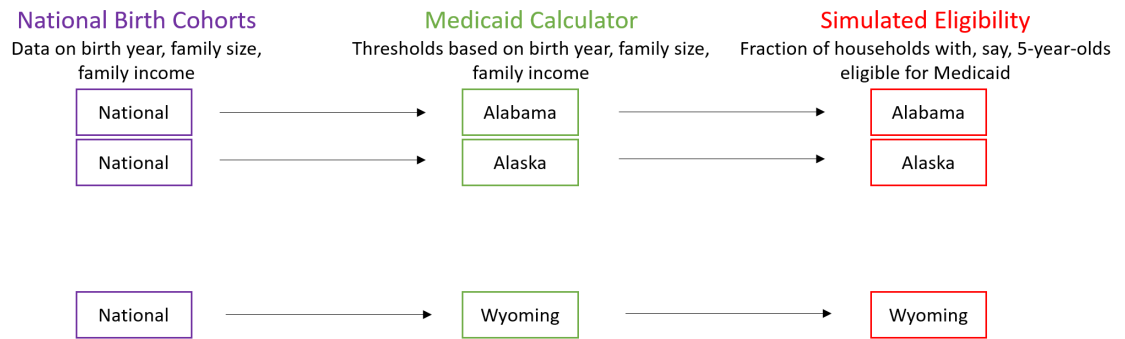


Figure 4.5: Construction of Simulated Eligibility measure (instrument).

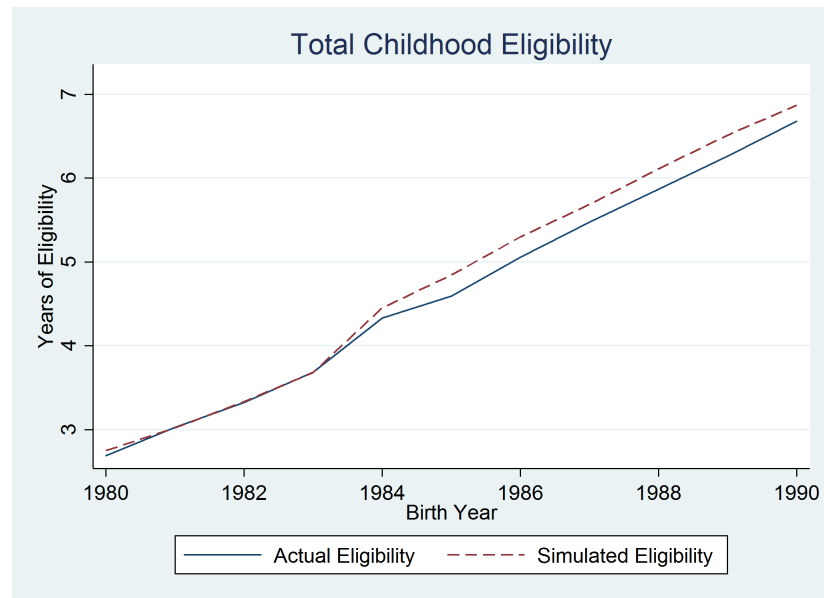


Figure 4.6: Actual and Simulated Eligibility across birth cohorts.

RESULTS

Main Estimates

In this chapter I present and discuss my model's estimated effects of childhood Medicaid eligibility on later-life criminal activity, for the full sample containing both sexes and separately by sex. The estimates in all of my tables come from specifications that include state and cohort fixed effects and the vector of controls for the other social policies mentioned above, with standard errors clustered at the state level.

Table 5.1 displays estimates for the full sample containing both sexes. My three dependent variables are the average yearly violent, property, and drug crime rate (per 10,000) for a state's birth-cohort during ages 18-24. The OLS estimates in Panel A are obtained by regressing crime directly on my Actual Eligibility measures. The OLS estimates are all negative, though only the estimated effect on the drug rate is statistically significant. The Reduced Form (RF) estimates in Panel B are obtained by regressing crime rates directly on the Simulated Eligibility instrument. The RF estimates all have more than triple the magnitude of the OLS estimates, indicating the bias towards zero in the OLS estimates from the endogeneity of household economic characteristics and highlighting the importance of the IV strategy. Recall that in the absence of instrumenting by Simulated Eligibility, Actual Eligibility can change due to both policy changes as well as socioeconomic changes, and thus β_1 is unable to distinguish between these two sources. That is, the results in Panel A of the estimated effect on crime following increased Medicaid eligibility captures not only the effect on crime from increased thresholds (policy change) but also the effect on crime from a greater fraction of family incomes falling below existing thresholds (socioeconomic change). Unsurprisingly, the estimates in Panel A (OLS) are closer to zero relative to the estimates of Panels B (RF) and D (IV).

Panel C gives the first-stage results from regressing Actual Eligibility on Simulated Eligibility in equation (5). The results show a strong first-stage relationship between Actual Eligibility and the Simulated Eligibility instrument, with F-statistics well over 100.

Panel D of Table 5.1 contains my preferred estimates from the IV specification, where I regress crime on the predicted Actual Eligibility (equation (6)) obtained by instrumenting with Simulated Eligibility (equation (5)). The estimated effect on property crime is negative but insignificant at traditional levels. For violent crime, the reported estimates suggest that a cohort gaining an additional year of eligibility for Medicaid during childhood results in 4.67 fewer violent crimes per 10,000 population on average—a 10% decrease relative to their average. And for drug crimes, an additional year of eligibility for Medicaid during childhood is associated with about 20 fewer drug crimes per 10,000 population on average—a 12.5% decrease relative to their average. In terms of standard deviations, a 1 standard deviation increase in childhood Medicaid eligibility leads to a .4 standard deviation decrease in the violent crime rate and .6 standard deviation decrease in the drug crime rate.

Because crime is committed at markedly different rates between the sexes, possibly resulting from different determinants, in Table 5.2 I present estimates of the effect of childhood Medicaid Eligibility by the sexes separately, by estimating equation (6) with only male crime rates and with only female crime rates.

Comparison of the IV estimates in the male panel of Table 5.2 to the full sample with both sexes in panel D of Table 5.1 shows that the point estimates for males are nearly twice as large as for the full sample. The estimates for violent and drug crime are significant at the .01 level, with an additional year of eligibility associated with 11% and 14% reductions relative to their means. For females, all estimates are negative, with significant effects at the .10 level for violent crime and at the .05 level

for drug crime. For violent and drug crime, the estimated crime reduction among females is less than 1/7th that among males in percentage points (absolute terms), and about half that among males in percentage.

An important consideration in interpreting these estimates is the role of incomplete take-up: that is, not all who are eligible for Medicaid are actually enrolled in the program. Because I estimate the effects of childhood *eligibility* for the Medicaid and CHIP programs, and not program *enrollment*, my reported IV estimates are intent-to-treat (ITT) estimates rather than treatment-on-the-treated (ToT) estimates.

I focus on eligibility and ITT effects rather than enrollment and ToT effects for several reasons. First, data availability and measurement error: while the CPS does ask about Medicaid coverage, there is considerable measurement error associated with this response as many states' Medicaid programs have state-specific names (e.g., Georgia's Medicaid program name is "Georgia Medical Assistance" and their CHIP name is "PeachCare for Kids") that families are enrolled in, but may respond "no" to survey questions about Medicaid coverage. Second, because policy makers can directly influence eligibility, but only indirectly influence enrollment, these estimates are arguably the most policy relevant. Third, and in my view the most important, take-up and enrollment can introduce their own endogeneity into the problem: suppose two children are both eligible for Medicaid, but only one child is enrolled by their family in the program—these two children may then differ in their criminal activity for reasons other than program enrollment (e.g., the level of parental investment in their children). Relatedly, while the endogeneity of eligibility from household characteristics can be addressed rather intuitively through a simulated eligibility instrument that indexes a state program's generosity by the fraction of a national sample who would be eligible for that state's program (as done in this paper),

I can not think of a comparable instrument for enrollment.¹

Comparison of Estimates to Other Research

In the absence of other studies of the effect of childhood Medicaid eligibility on later-life crime, I compare my estimated effects on crime to those found from other large-scale policies on crime, and to effects of childhood Medicaid eligibility on other later-life outcomes.

Comparing my results to similar work on crime, I first note the similarity with Barr & Smith (2017) who find an effect on convictions for violent crime but not on convictions for property crime. In their nationwide analysis using UCR data, Barr Smith (2017) estimate that an additional year of FS eligibility age 0-5 reduces the violent crime rate by about 15% relative to the average, which is quite close to my estimate of an additional year of Medicaid eligibility age 0-18 reducing the violent crime rate by about 10% relative to the average.

Wen, Hockenberry, & Cummings (2017) estimate that the HIFA-waivers targeted at expanding coverage to low-income adults reduced the rate of robberies by 2%, the rate of assaults by 1%, and the rate of larcenies by .6%. Direct comparisons with my results are difficult as they come from estimating the effect of different Medicaid eligibility variables (continuous measure of total childhood eligibility vs. a dichotomous variable indicating whether the state received a HIFA-waiver), and further we consider arguably very different treatments on different populations.

I now compare my estimated effect size of childhood Medicaid eligibility on

¹Though simulated eligibility could indeed be an instrument for actual enrollment, the first stage relationship will be weaker than with actual eligibility, reducing the precision of the IV estimates. For example, Thompson (2017) has a first-stage coefficient of .922 between actual eligibility and simulated eligibility, and a first-stage coefficient of .657 between actual enrollment and simulated eligibility.

later-life criminal outcomes with studies of childhood Medicaid eligibility on other later-life outcomes. Using the same cohorts with a similar empirical design, Cohodes et al. (2016) find that a 10% increase in the average yearly fraction eligible (which translates to $1.8 = (.10 \times 18)$ additional years for total eligibility) reduces high school noncompletion by 0.39 of a percentage point and increases bachelor's degree attainment by 0.66 of a percentage point. Relative to their means, and translating into total years of eligibility, their results suggest that an additional year of Medicaid eligibility in childhood reduces high-school noncompletion by 2.7% and increases bachelor's degree attainment by 1.4%. Brown, Kowalski, & Lurie (2015) find that an additional year of Medicaid eligibility in childhood increases the likelihood of enrolling in college by 1.3%.

Boudreaux et al. (2016) find that each additional year of Medicaid exposure during ages 0-5 leads to a decrease of .07 SD in a chronic conditions index, while Thompson (2017) finds an additional year of eligibility to result in 10-30 percent improvements in a variety of health measures.

In the absence of a baseline for comparison, I am unable to make quantitative judgments about my estimated effects, only qualitative comparisons. Relative to their means, my estimated effect of childhood Medicaid eligibility on crime is "larger" than the estimated effects on educational and labor market outcomes but similar to the estimated effects on health outcomes. I consider them qualitatively consistent with the estimated effects on crime from Food Stamp eligibility found in Barr & Smith (2017), from environmental lead exposure found in Reyes (2007), and from compulsory schooling interventions (for example, the findings of Lochner & Morretti (2004) translate to a ten percentage point increase in graduation rates reducing murder and assault arrest rates by about 20%).

	(1)	(2)	(3)
	Violent	Property	Drug
Panel A: OLS	-1.063 (0.680)	-1.141 (2.144)	-5.119** (2.268)
Panel B: RF	-3.746*** (1.372)	-6.422 (4.476)	-15.95*** (3.544)
Panel C: First Stage [F Stat]	.802*** [136]	.802*** [136]	.802*** [136]
Panel D: IV	-4.670*** (1.710)	-8.004 (5.579)	-19.88*** (4.418)
Means of dep vars (Std. Dev.)	44.72 (20.43)	148.46 (44.51)	159.09 (57.59)
Observations	517	517	506

Table 5.1: Estimates for both sexes. The reported coefficients are for a variable measuring total years of Medicaid eligibility occurring from ages 0 through 18, except for the results in Panel C, which regresses actual eligibility on the simulated eligibility instrument. All models include state and cohort fixed effects, as well as controls for average state AFDC/TANF maximum benefit levels and minimum eligibility thresholds, EITC credit levels, and unemployment rates over the course of each cohort's childhood. Standard errors are clustered at the state level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1)	(2)	(3)
	Violent	Property	Drug
Panel A: Male	-8.191*** (2.903)	-12.04 (7.420)	-35.65*** (7.786)
Means of dep vars	72.96	195.47	258.79
Panel B: Female	-1.253* (0.642)	-4.313 (4.465)	-4.546** (1.695)
Means of dep vars	15.03	99.10	54.83
Observations	517	517	506

Table 5.2: Estimates by sex. Column headings indicate the dependent variable for each model. The reported coefficients are for a variable measuring total years of Medicaid eligibility occurring from ages 0 through 18, instrumented by simulated eligibility over ages 0 through 18. All models include state and cohort fixed effects, as well as controls for average state AFDC/TANF maximum benefit levels and minimum eligibility thresholds, EITC credit levels, and unemployment rates over the course of each cohort's childhood. Standard errors are clustered at the state level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

ROBUSTNESS CHECKS

Interstate Migration

As discussed in Chapter 4, my estimation strategy assumes that individuals arrested in a given state spent their childhoods in that same state. This is obviously false as individuals do move across states, but to the extent that this interstate migration is uncorrelated with Medicaid eligibility it will merely attenuate my estimated effect of Medicaid eligibility on crime towards zero. I test this by using US Census data on respondents' state-of-birth, current state-of-residence, and age to estimate equation (7) to see if Medicaid eligibility is correlated with interstate migration.

$$P(Migrate_{isc} = 1) = G(\beta_0 + \beta_1 Eligibility_{sc} + \beta_2 Age_i + \beta_3 X_{sc} + State_s + Cohort_c) \quad (7)$$

To do so, I mark for each 18-24 year-old over the years 2000-2014¹ if their current state differs from their birth state. I then perform a probit analysis in equation (5) to estimate whether Medicaid eligibility for members of cohort c born in s , $Eligibility_{sc}$, predicts for members i of that state-cohort migrating to another to another state by age 24, ($Migrate_{isc} = 1$), controlling for individual's age, observable time-varying state-level controls X_{sc} , and state and birth cohort fixed effects.

The results of this probit analysis are given in Table 6.1, with p-values reported for the three eligibility measures. All three eligibility measures zero effect of Medicaid eligibility on the probability an individual resides during ages 18-24 in a state different from their birth-state.

¹Prior to 2000, the Census was only available every decade, preventing using the years of 1998 or 1999.

Month of Birth

As discussed in Chapter 4, measurement error arising from month of birth should be uncorrelated with Eligibility, except for possibly the 1983 birth-cohort due to the discontinuity created at September 30, 1983. Table 6.2 presents IV estimates of equation (6) while excluding the 1983 cohort. The estimates indicate that the results are insensitive to the exclusion of the 1983 birth cohort, suggesting that this measurement error is not driving my results.

Simulated Eligibility National Sample Construction

In constructing my simulated eligibility instrument, there's a choice in which national sample to use. In constructing yearly simulated eligibility, one method is to draw a national sample of that year's CPS respondents. That is, for each state in 1995, find the fraction of 1995 CPS respondents in nation who would have been eligible under that state's eligibility rules.

Instead, my preferred sample uses respondents from all childhood years of the CPS (1980-2008). This seems preferable, as the simulated eligibility instrument is ultimately a way of indexing states' program generosity, and this guards against the possibility of economic trends in a handful of states driving trends in simulated eligibility (in addition to increasing the national sample size). Regardless, the outcomes are extremely similar between the two choices, as shown in Table 6.3, which displays estimates of equation (6) using different national sample constructions for the Simulated Eligibility instrument in the 1st stage equation (5).

Controls

In Table 6.4, I show the sensitivity of my results the sensitivity of my results to the inclusion of the controls for local economic conditions (average unemployment over childhood) cash welfare programs (average minimum AFDC/TANF eligibility threshold over childhood, average maximum AFDC/TANF benefit level over childhood) and the EITC program (average credit amount over childhood). The estimated impacts of Medicaid eligibility on violent and drug crime are very similar, regardless of whether these controls are excluded.

Remaining Threats to Identification

Remaining threats to the validity of the IV estimates presented in Panel D of Table 5.1 are changes in policing practices when the cohorts are in adulthood and changes from other large-scale policies affecting the cohorts in childhood. As discussed near the end of Chapter 4, changes in policing practices could also result in changes to criminal behavior, especially as I measure criminal behavior with arrest rates. While I am unaware of changes in policing practices when the cohorts are in young adulthood that may be correlated with their Medicaid eligibility in childhood, it would be valuable to test this directly in future work.

Changes in abortion access or the roll-out of the Food Stamps Program could also bias my estimates if they are correlated with childhood Medicaid eligibility. While the roll-out of the FSP occurred from 1964-1974 and the changes in abortion access took place before my studied cohorts, in theory they could still bias my estimates through “peer effects” from the cohorts of the 1970s. I do not consider an effect through this channel to be likely to drive my results, but I would like to test this directly in future work. Similarly, changes in compulsory education laws could

potentially drive my results if they are correlated with Medicaid eligibility.

The most concerning threat to the validity of my results comes from changes to environmental lead exposure following The Clean Air Act. While I argue that the three changes mentioned in the previous paragraph have too little year-to-year variation to substantially drive my results, reductions in environmental lead exposure *did* vary appreciably across the years within the states during my sample period. As mentioned previously, Reyes (2007) finds the reduction of lead exposure in the late 1970s and early 1980s to be responsible for significant declines in violent crime in the 1990s. While there is only one cohort of overlap between the cohorts of this paper and those in Reyes (2007), the environmental lead reductions across the states did continue into the early 1980s. If the states with low eligibility among their cohorts of the early 1980s were also the last to reduce childhood lead exposure (while the states with high eligibility among their early 1980s cohorts had already reduced lead prior to 1980), then my results may be capturing an effect from reduced environmental lead exposure. It is plausible that the states who were the last to implement environmental lead reductions could be the same states with the lowest eligibility for their birth-cohorts of the early 1980s. Future work should undoubtedly control for state-level changes in environmental lead exposure.

	(1)	(2)	(3)
	Migrate	Migrate	Migrate
Actual Eligibility	0.0111 (0.954)		
Simulated Eligibility		-0.000262 (0.999)	
Eligibility (IV)			-0.000346 (0.999)
Observations	2,195,512	2,195,512	2,195,512

Table 6.1: Migration and Medicaid eligibility. The dependent variable in each column is a binary variable corresponding to whether a Census individual lives in a state other than their state-of-birth by age 24. The reported coefficients are for a variable measuring total years of Medicaid eligibility occurring from ages 0 through 18, instrumented by simulated eligibility over ages 0 through 18. All models include state and cohort fixed effects, as well as controls for an individual's age, average state-of-birth AFDC/TANF maximum benefit levels and minimum eligibility thresholds, EITC credit levels, and unemployment rates over the course of childhood. Standard errors are clustered at the state of birth level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1)	(2)	(3)
	Violent	Property	Drug
Eligibility (With 1983)	-4.670*** (1.710)	-8.004 (5.579)	-19.88*** (4.418)
Means of dep vars	44.72	148.46	159.09
Observations	517	517	506
Eligibility (Without 1983)	-4.781*** (1.757)	-8.472 (5.936)	-20.76*** (4.683)
Means of dep vars	44.74	149.13	158.79
Observations	470	470	460

Table 6.2: IV estimates' sensitivity to inclusion of 1983 birth cohort. Column headings indicate the dependent variable for each model. The reported coefficients are for a variable measuring total years of Medicaid eligibility occurring from ages 0 through 18, instrumented by simulated eligibility over ages 0 through 18. The sample used in the top panel contains all of the 1980-1990 birth cohorts, while the bottom panel drops the 1983 cohort from the sample. All models include state and cohort fixed effects, as well as controls for average state AFDC/TANF maximum benefit levels and minimum eligibility thresholds, EITC credit levels, and unemployment rates over the course of each cohort's childhood. Standard errors are clustered at the state level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1)	(2)	(3)
	Violent	Property	Drug
Panel A: All Years			
First Stage	.8095***	.8095***	.8095***
[F Stat]	[136]	[136]	[136]
Eligibility	-4.670***	-8.004	-19.88***
	(1.710)	(5.579)	(4.418)
Panel B: Yearly			
First Stage	.8023***	.8023***	.8023***
[F Stat]	[136]	[136]	[136]
Eligibility	-4.419**	-7.987	-19.41***
	(1.721)	(5.594)	(4.474)
Observations	517	517	506

Table 6.3: IV estimates' sensitivity to different national samples. Table 6.3 shows the sensitivity of estimates to national sample selections in the construction of the simulated eligibility instrument. First stage estimates come from regressing actual eligibility on simulated eligibility. Eligibility is the independent variable for all models, obtained from each panel's first stage regression. All models include state and cohort fixed effects, as well as controls for average state AFDC/TANF maximum benefit levels and minimum eligibility thresholds, EITC credit levels, and unemployment rates over the course of each cohort's childhood. Standard errors are clustered at the state level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1)	(2)	(3)
	Violent	Property	Drug
Eligibility	-4.670*** (1.710)	-8.004 (5.579)	-19.88*** (4.418)
Unemployment	-2.937** (1.433)	-9.154* (4.918)	-12.04*** (3.774)
EITC	0.528** (0.243)	2.649*** (0.889)	1.834** (0.899)
Welfare Benefits	-0.00402 (0.0192)	0.166*** (0.0534)	0.135** (0.0506)
Welfare Eligibility	0.00918 (0.00572)	0.00755 (0.0233)	0.0313* (0.0176)
Eligibility (No Controls)	-4.407** (1.760)	-3.453 (5.762)	-18.56*** (4.500)
Observations	517	517	506

Table 6.4: IV estimates' sensitivity to the inclusion of controls. Column headings indicate the dependent variable in each model. Eligibility is a measure of total years of childhood Medicaid eligibility, obtained by the first stage regressions previously described. All models include state and cohort fixed effects. Standard errors are clustered at the state level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

CONCLUDING REMARKS

This paper estimates the long-term impact of Medicaid eligibility in childhood on criminal behavior in adulthood. I estimated large and statistically significant decreases to the violent crime rate and the drug crime rate resulting from increased childhood Medicaid eligibility, and demonstrated that my results are robust to interstate migration, the exclusion of the 1983 birth-cohort, different constructions of my simulated eligibility instrument, and controls for several social policies and the unemployment rate.

I consider three primary areas for continued study. The simplest is disaggregating from *total* childhood eligibility to consider eligibility at different stages of childhood. The effect of eligibility at early ages may be distinct from eligibility at later ages, and it would be valuable to explore these differential effects. The second area would be additionally including measures of prenatal eligibility—that is, the effect on an individual’s criminal behavior due to their mother’s eligibility for Medicaid *in utero*. Because many of the Medicaid expansions increased eligibility for an individual both *in utero* and in childhood, I may be partially capturing an effect of prenatal eligibility and attributing it to childhood eligibility. The final area for future research would be in assessing the sensitivity of my results to controls for additional policy changes. Based on my reading of the literature, I consider controlling for environmental lead exposure to be the most crucial, but would also like to control for changes in policing practices, compulsory education laws, and a richer set of population characteristics.

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