The Effect of Child Health Insurance Access on Schooling

Evidence from Public Insurance Expansions

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ABSTRACT

Although a sizable literature analyzes the effects of public health insurance programs on short-run health outcomes, little prior work has examined their long-term effects. We examine the effects of public insurance expansions among children in the 1980s and 1990s on their future educational attainment. We find that expanding health insurance coverage for low-income children increases the rate of high school and college completion. These estimates are robust to only using federal Medicaid expansions and mostly are due to expansions that occur when the children are not newborns. Our results indicate that the long-run benefits of public health insurance are substantial.

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I. Introduction

Whether and how to provide access to affordable healthcare for low-income Americans has become a central policy issue in the United States. The importance of this issue is underscored by the intense debate surrounding the passage and implementation of the 2010 Affordable Care Act (ACA), one of the largest expansions of public health insurance in U.S. history. Most individuals from low-income households obtain medical insurance through Medicaid. Since its inception in 1965, Medicaid has gone through repeated expansions that have greatly increased the scope of the program as well as the public sector's role in health insurance provision. As a result, over 50 percent of children in the United States currently are eligible for publicly provided health insurance through this program,[1] and health insurance coverage is high among this population (DeNavas-Walt et al. 2013).

The expansions that generated this high level of coverage were expensive. In 2012, total state and federal spending on Medicaid was $415.2 billion (Henry J. Kaiser Family Foundation 2014), which makes it the largest government program that targets low-income Americans.[2] The substantial public funds devoted to providing health insurance to low-income children, as well as recent debates over the value of such insurance that surrounded the passage of the ACA, highlight the importance of understanding what benefits, if any, accrue to individuals due to health insurance access when they are young.

The effect of Medicaid expansions on access to healthcare and on subsequent child health has been studied extensively (Currie and Gruber 1996a, 1996b; Moss and Carver 1998; Baldwin et al. 1998; Cutler and Gruber 1996; LoSasso and Buchmueller 2004; Gruber and Simon 2008), typically showing that Medicaid expansions increase access to healthcare, decrease infant mortality, and improve childhood health. Furthermore, these expansions and Medicaid access more generally have been linked to a lower likelihood of bankruptcy and to less medical debt (Gross and Notowidigdo 2011, Finkelstein et al. 2012). Notably, this literature has focused almost exclusively on the short- or medium-run effects of Medicaid on health and financial outcomes. Such effects are of considerable policy importance, but without an understanding of how Medicaid eligibility when young impacts long-run outcomes, it is difficult to fully assess the impact of this large government program. Estimating the long-run effects of Medicaid has received very little attention in the literature and is the focus of this paper.

We provide the first evidence on how expanding health insurance for children throughout their youth influences their eventual educational attainment. Our analysis focuses on education for several reasons. First, there is a strong argument from human capital theory that the improvements in child health and increased financial stability associated with Medicaid could have large effects on educational attainment. Second,

1. Throughout this paper, we refer to "public health insurance" and Medicaid synonymously. Publicly provided health insurance also includes State Children's Health Insurance Plans (SCHIP). Medicare, however, is not included in our definition of public health insurance for purposes of this paper.

2. As a point of reference, total expenditure on food stamps (SNAP) in 2012 was $78.4 billion, and spending on Temporary Aid for Needy Families (TANF) was $31.4 billion. Total Medicare expenditure was $536 billion, which highlights that the Medicare and Medicaid/SCHIP programs are of roughly similar size.

cohorts affected by the Medicaid increases we study have been exposed to persistently high returns to human capital investment (Autor 2014; Autor, Katz, and Kearney 2008). Thus, examining the effects of Medicaid expansions on long-run educational attainment is of considerable policy interest.

Similar to prior work on Medicaid, we exploit the expansions of Medicaid and the State Children's Health Insurance Program (SCHIP) that took place in the 1980s and 1990s to examine how the educational attainment of these children was affected by access to these programs. We use data on 22–29-year-olds born between 1980 and 1990 from the 2005–12 American Community Survey (ACS) that allow us to match each respondent to his or her state of birth. We then use data from the March Current Population Survey (CPS) to calculate Medicaid eligibility by age, state, year, and race that we link to our ACS sample. With these data, we follow the method of simulated instrumental variables pioneered by Currie and Gruber (1996a, 1996b) and Cutler and Gruber (1996) to account for the fact that the demographic composition of a state may be endogenous to Medicaid eligibility rules. By using a fixed sample to calculate eligibility, the model is identified using eligibility rule changes only.

We make several contributions to the literature. First, we estimate the effect of health insurance access among both young and school-aged children on their long-run educational attainment. Second, we focus on Medicaid eligibility throughout one's childhood rather than just at birth. Virtually all of the prior work on Medicaid expansions focuses on point-in-time eligibility, particularly eligibility at birth (Levine and Schanzenbach 2009, Currie and Gruber 1996b).[3] From a policy perspective, focusing on eligibility at older ages is important because of the large amount spent on providing health insurance to non-newborn children. We present direct evidence that focusing just on point-in-time eligibility at birth provides an incomplete characterization of the effect of Medicaid on educational attainment. Our results show that it is repeated exposure throughout one's childhood that impacts these long-run outcomes, which has not been demonstrated previously.

Third, we are able to examine heterogeneous effects by the age at which a child is exposed to Medicaid expansions. There is a sizable body of research demonstrating a link between fetal health as well as the provision of fetal healthcare services on future educational outcomes (Figlio et al. 2014, Levine and Schanzenbach 2009, Currie and Gruber 1996b), but the effect of children's access to health insurance on their educational attainment has not been studied. Socioeconomic disparities in educational outcomes begin at young ages and largely persist throughout the lifecycle (Carneiro and Heckman 2002, Todd and Wolpin 2007). Our study provides insight into the ages at which Medicaid expansions have the largest long-run impacts on children in order to help close these educational gaps.

Fourth, we develop a new robustness test that allows us to assess the extent to which state-level Medicaid eligibility expansions are endogenously related to underlying trends in outcomes. Specifically, we isolate the variation in state Medicaid eligibility that

3. Currie, Decker, and Lin (2008) present suggestive evidence that exposure to Medicaid expansions when young leads to better health in adolescence, which suggests there could be an effect on educational attainment as well. In a related study, Brown, Kowalski, and Lurie (2015) use IRS tax data to show that the eligibility expansions in the 1980s led to higher earnings by the time individuals reached the age of 31. Their work does not examine educational attainment, but their results and ours strongly complement one another.

comes from changes in federal rules. These changes impact states differentially based on their preexisting welfare eligibility rules. Importantly, these expansions are unlikely to be related to outcome trends in any one state, which makes these estimates robust to state-specific educational attainment trends. This is a particularly important strategy in this paper because of our focus on average eligibility during one's youth. There are no sharp breaks across cohorts in childhood eligibility, but rather continuous increases the size of which are based on one's state and year of birth. This makes our estimates potentially more sensitive to state-specific trends than those of the prior literature that focuses on point-in-time eligibility. Our development and use of the federal eligibility instrument, in addition to our use in some specifications of state-specific time trends, provides evidence that our estimates are not being influenced by secular trends. That our estimates are similar when only using federal eligibility suggests as well that state Medicaid expansions are not endogenous, which helps validate the large body of work that uses them.

Finally, we contribute to the literature by showing that our results are insensitive to using current state versus state of birth. Because there are few data sets that include state of birth, most long-run analyses are forced to use current state as a proxy for childhood exposure. This is problematic if there is endogenous mobility related to Medicaid eligibility. Our estimates are inconsistent with such mobility, and thus our findings expand the possibilities for examining long-run Medicaid effects using other data sets that only contain current state of residence.

We find consistent evidence that Medicaid exposure when young increases later educational attainment. Our baseline estimates suggest a ten percentage point increase in average Medicaid eligibility between the ages of zero and 17 decreases the high school dropout rate by 0.4 of a percentage point, increases the likelihood of college enrollment by 0.3 of a percentage point, and increases the four-year college attainment rate (BA receipt) by 0.7 of a percentage point. These estimates translate into declines in high school noncompletion of about 4 percent, increases in college enrollment of 0.5 percent, and increases in BA attainment of about 2.5 percent relative to the sample means. In separate estimates by race, we find that the high school completion effects are larger among nonwhites, while the college enrollment and completion rate impacts are largest among white children. However, both groups experience substantial increases in educational attainment due to Medicaid expansions that occurred during their youth.

Our results on heterogeneity by age at the time of expansion, while imprecise, suggest that Medicaid expansions among children aged four to eight are the most important. We also find evidence that expansions among teens aged 14–17 increase educational attainment, though interestingly, there is little effect of expansions for children at birth or in their first few years of life. These findings highlight the importance of examining childhood eligibility rather than point-in-time eligibility and suggest that there are sizable returns to covering older children in Medicaid.

Overall, our results point to large effects of Medicaid expansions for children on their eventual educational attainment. These effects are particularly important because lower-income families are most affected by Medicaid and SCHIP expansions, and it is children from these families that have exhibited the most sluggish growth in educational attainment over the past 30 years (Bailey and Dynarski 2011). Our estimates suggest that the long-run returns to providing health insurance access to children are larger than just the short-run gains in health status.

The rest of this paper is organized as follows: Section II describes the public health expansions we use in our analysis, and Section III reviews the literature on the effects of health insurance on health and family finances as well as the literature examining the links between health, family resources, and educational outcomes. Section IV provides a description of the data. We outline our empirical strategy and detail our results in Sections V and VI, respectively, before concluding in Section VII.

II. Medicaid and Public Health Care Expansions for Children

The Medicaid program was introduced in 1965 and phased in mostly over the late 1960s as a health insurance component for state-based cash welfare programs that targeted low-income, single-parent families. Beginning in the mid-1980s, the Medicaid program was slowly separated from cash welfare, first by extending benefits to low-income children in two-parent families and then by raising the income eligibility thresholds for two groups: children and pregnant women (Gruber 2003, Gruber and Simon 2008).[4] Since the 1980s, Medicaid has been expanded to many low-income families who did not previously qualify due to their income levels, family composition, and/or labor force participation. As a result of these expansions, by the mid-1990s, most children in America below the poverty line, and all young children below 133 percent of the poverty line, were eligible for Medicaid. In certain states, their parents were as well.

Importantly, for most of these expansions, states could choose to implement the expansion based on their own eligibility preferences. By the early 1990s, states were required to cover all children below 100 percent of the poverty line and children younger than age six below 133 percent of the poverty line. Many states opted to provide more generous coverage, however, for which the federal government would provide matching funds up to a certain threshold. In 1997, Congress passed the State Children's Health Insurance Plan (SCHIP), which was one of the largest expansions of public health insurance to date. SCHIP provided matching funds to states to expand coverage to children from households with incomes below 200 percent of the poverty line. Prior to SCHIP, states were permitted to cover children up to 200 percent of the poverty line, but without federal matching funds, few states did so.

In this paper, we exploit these expansions in Medicaid generosity in the 1980s and 1990s that were phased in at different times, and with different generosity levels across states, to identify the effect of Medicaid eligibility on long-run educational attainment. Thus, our identification strategy uses both state-level variation, which assumes the timing of state eligibility changes is exogenous with respect to underlying trends in educational attainment of residents, and federal variation to explicitly test the robustness of our estimates to the assumption that the state Medicaid variation is exogenous.

4. For more details on Medicaid expansions, see Currie and Gruber (1996a), Gruber (2003), and Gruber and Simon (2008).

III. Previous Literature

The effect of Medicaid eligibility on education flows through two main potential channels: better health due to Medicaid enrollment as well as higher household resources stemming from the insurance protection provided by Medicaid. There is a large literature that shows Medicaid expansions both increase medical care usage and improve health among children and adults (Currie and Gruber 1996a, 1996b; Currie 2000; Kaestner et al. 2000, 2001; Almeida, Dubay, and Ko 2001; Banthin and Selden 2003; Dafny and Gruber 2005; Buchmueller et al. 2005).[5] To the extent that health enters into the education production function, the health effects of Medicaid expansions could lead to higher educational attainment among affected children.

How are such changes in child health from Medicaid expansions predicted to affect educational attainment? Surprisingly little work has been done on this question. While much existing research has documented that better fetal health translates into better educational and adult outcomes (Miller and Wherry 2014; Figlio et al. 2014; Almond and Mazumder 2011; Almond, Edlund, and Palme 2009; Almond 2006; Black, Devereaux, and Salvanes 2007; Oreopoulos et al. 2008; Royer 2009), very little research estimates how childhood health after birth impacts such outcomes. Currie et al. (2010) find that children with health problems in early childhood have poorer long-run health, a higher likelihood of being on social assistance, and lower educational outcomes. Case, Fertig, and Paxson (2005) and Case, Lubotsky, and Paxson (2002) both show that worse health in childhood is negatively associated with long-run outcomes such as health, educational attainment, and labor market outcomes.[6]

Cox and Reback (2013) as well as Lovenheim, Reback, and Wedenoja (2016) examine the effect of access to healthcare *services* on educational attainment using the rollout of school-based health centers in the United States. The former study finds that center openings lead to higher attendance rates, while the latter shows they cause lower teen birth rates but do not affect high school dropout rates. The students treated by these centers are typically in high school, so the differences between these estimates and the large effects of health found by researchers examining younger children potentially may be due to heterogeneity in the effects of health at different times during childhood.

Another main channel through which Medicaid can influence educational attainment is through its effect on family resources. Recent work has suggested that public health insurance successfully shelters low-income families from financial risk associated with negative health shocks (Gross and Notowidigdo 2011, Dave et al. 2013, Finkelstein et al. 2012). Thus, Medicaid expansions better the financial position of households, which much prior work demonstrates can positively affect educational investments (Dahl and Lochner 2012, Lovenheim 2011, Michelmore 2013).

While we provide the first analysis in the literature of the long-run effects of Medicaid on educational attainment, there are two papers in the literature that are closely related to ours. The first is Levine and Schanzenbach (2009), which analyzes the effect of Medicaid and SCHIP expansions *at birth* on future educational achievement as measured by

5. Levy and Meltzer (2008) provide a recent review of this literature.

6. See Almond and Currie (2011) for a comprehensive overview of the fetal origins hypothesis and Eide and Showalter (2011) for evidence on the effect of health on human capital outcomes throughout the life cycle.

state-level National Assessment of Educational Progress (NAEP) scores. This paper is typical of the literature in its focus on point-in-time eligibility (at birth) rather than eligibility over a period of one's childhood. They examine differences in Medicaid expansions by state and the differences between age cohorts in a triple difference framework. Their results suggest that a 50 percentage point increase in Medicaid eligibility corresponds to a 0.09 standard deviation increase in reading test scores, but there are no effects on math scores.

Our analysis is distinguished from theirs along several dimensions. First, we focus on the effects of expanding health insurance throughout one's youth. This question is particularly important given the amount of money spent in the United States on providing healthcare to nonnewborn children through Medicaid.[7] Indeed, our results indicate that expanding eligibility to nonnewborns is an important driver of the long-run effects of Medicaid; estimates using point-in-time eligibility at birth show little effect of Medicaid on educational attainment. Second, we examine effects on long-run educational attainment rather than on test scores at younger ages. A growing body of evidence suggests that the effects of given educational interventions on test scores are poor predictors of their effects on the longer-run outcomes that are of greater interest, such as educational attainment and earnings (Ludwig and Miller 2007, Chetty et al. 2011, Deming et al. Forthcoming).[8]

The second related work by Brown, Kowalski, and Lurie (2015) uses IRS tax data to examine the effect of Medicaid expansions on earnings throughout a child's early life. They find results that are highly complementary to our own: Medicaid eligibility increases from 0–18 are associated with higher earnings, lower Earned Income Tax Credit (EITC) receipt, and higher labor force participation. That they obtain these estimates on a different data set using somewhat different cohorts is notable. Together, our results point to large effects of Medicaid expansions on the long-run outcomes of affected children.

IV. Data

We use three sources of data in our analysis of the effects of insurance expansions on educational attainment. Below, we describe these sources of data as well as the construction of the variables that we use in our investigation.

A. Medicaid Eligibility Data

Our Medicaid eligibility data are constructed using the March Current Population Survey (CPS) for the years during which the 1980–90 birth cohorts are between the ages

7. If health insurance among school-aged children did not positively affect these children, ostensibly the government could only offer Medicaid to pregnant women and households with very young children.
8. Much of this evidence suggests that it is particularly problematic to use effects on contemporaneous test scores to predict long-run outcomes. Levine and Schanzenbach (2009) examine effects on the NAEP scores of fourth and eighth graders, which themselves are longer-run test score outcomes. Furthermore, instructors are unlikely to manipulate NAEP scores endogenously with respect to Medicaid eligibility rates, which would not necessarily be the case for contemporaneous test scores used to evaluate a given educational intervention. Nevertheless, it is not at all clear that effects on NAEP scores would translate into higher educational attainment, which underscores the importance of our analysis.

of zero and 17. We construct two eligibility measures using state and year information on eligibility rules similar to those used in Gross and Notowidigdo (2011) and Gruber and Simon (2008).[9] Eligibility calculations are based on the household's income, the age and number of children in the household, and the gender and unemployment status of the head of household.

The first Medicaid eligibility measure that we construct is the proportion of households of a given race (white, nonwhite) with children of age i in state s and year t who are eligible for Medicaid, where $i \in [0, 1, \ldots, 17]$. Thus, for example, we calculate the proportion of households with five-year-olds in New York who are eligible for Medicaid in each year between 1985 (the 1980 birth cohort) and 1995 (the 1990 birth cohort). We calculate eligibility separately by child's race due to the strong correlation between race and Medicaid eligibility: A given change in eligibility rules is likely to impact nonwhites differently than whites even though the Medicaid rules themselves are race neutral.

These calculations allow us to measure the proportion of children of each age and race group that are Medicaid-eligible in each state and in each year between 1980 and 2007. As described below, our outcome data span the years 2005–12 and include the 1980–90 birth cohorts. These cohorts are between the ages of 22–29 in 2005–12, which is why our CPS sample ends in 2007 (when the 1990 birth cohort is 17).[10] We use three-year moving averages of calculated eligibility instead of yearly eligibility because the small sample sizes in the CPS within each age-race-state cell lead to measurement error in eligibility. While this measurement error is not problematic for our instrumental variables strategy, using one-year eligibility likely would attenuate the OLS estimates considerably. This makes comparisons between our OLS and IV estimates less informative.[11] Furthermore, the use of three-year moving averages is standard in the recent Medicaid literature that employs simulated instrument methods (Gruber and Simon 2008; Gross and Notowidigdo 2011; DeLeire, Lopoo, and Simon 2011), which facilitates comparisons between our estimates and those in prior work. Aside from making the estimates more precise, our use of these moving averages has little effect on the results. We refer to this measure of Medicaid eligibility as "actual eligibility."

Actual eligibility varies within states over time due to changes in eligibility rules, changes in demographic composition, and changes in the economic circumstances of households. In order to isolate the variation in Medicaid eligibility due only to eligibility rule changes, we follow the method first used in Currie and Gruber (1996a, 1996b) and Cutler and Gruber (1996) and calculate "simulated fixed eligibility," which is the proportion of the population in each state, age, race, year cell that would be eligible for Medicaid, calculated using a fixed national sample that does not vary across states or

9. We are extremely grateful to Tal Gross and Kosali Simon for providing us with the computer code that forms the basis for our eligibility calculations.

10. We have conducted extensive sensitivity analyses using different birth cohort ranges and ACS age ranges. Our results are not very sensitive to the age range or birth cohorts used. These sensitivity analyses are available from the authors upon request.

11. This method necessitates the use of CPS data through 2009 (which contains 2008 income information) to enable the construction of our three-year moving average. In Table A9, we show our estimates are robust to using one-year averages, although as expected the OLS estimates are attenuated. And in Table A7, we show they are robust to dropping all states that include cell sizes for zero-to-17 eligibility that come from under 100 observations (3.3 percent of the sample). Online appendix tables are available at http://uwpress.wisc.edu/journals/journals/jhr-supplementary.html.

over time. We use the 1986 CPS and calculate the share of this fixed population with a child of age *i* in year *t* and race *r* that would be eligible for Medicaid in each state using that state's Medicaid eligibility rules in that year, adjusting family income for inflation using the Consumer Price Index for All Urban Consumers. Critically, this sample does not vary by demographic characteristics across states or over time and thus is unaffected by state-specific trends in population or economic conditions that relate to both eligibility and coverage (such as a state-level recession). Finally, we collapse these estimates into unique state-year-age-race cells that yield the proportion of the fixed sample eligible for Medicaid in each cell.

Our baseline estimates include Medicaid eligibility variation coming from federal Medicaid expansions, state decisions about whether they will provide more generous benefits than required by federal law, and the timing of state expansions. Among these sources of variation, the one that is most worrisome is the timing of state expansions, since state expansion decisions may be endogenous with respect to underlying trends in educational attainment. Thus, we also construct measures of Medicaid eligibility that only are a function of federal rules. Federal Medicaid rules have different impacts on states due to preexisting state-level AFDC policies. Hence, we fix AFDC rules in each state as of 1980 and then calculate the three-year moving average of actual eligibility as well as yearly fixed simulated eligibility for each age, race, and state that would occur *only* due to changes in federal regulations governing Medicaid eligibility thresholds. Put differently, our federal eligibility measures yield state-year-age-race eligibility that would occur if no states provided more generous Medicaid access than required under federal law. The reason this is not simply a cohort-based analysis, then, is that the effect of federal rules varies by state according to (fixed) welfare policies. By design, this source of Medicaid eligibility variation is unlikely to be correlated with any decisions states can make regarding Medicaid eligibility.

Trends in our Medicaid eligibility measures, both overall and by race, are shown in Figure 1. For each birth cohort, we show the average eligibility between the ages of zero and 17 to which the cohort was exposed. The panels of the figure show, for the 1980–90 birth cohorts, actual eligibility that is a function of both state and federal rules as well as eligibility that uses only federal rules. As demonstrated in Figure 1, there was a dramatic rise in Medicaid eligibility that took place across the birth cohorts we study. Overall, average eligibility rates over the course of childhood increased 172 percent between the 1980 and 1990 birth cohorts. Much of this was the nonlinear increase in eligibility that came from the 1990 federal Medicaid expansion that extended eligibility to all children born after September 30, 1983, in families up to 100 percent of the poverty line. In Panels B and C of Figure 1, we show that the proportional increases experienced between whites and nonwhites were similar, but the higher baseline eligibility rates among nonwhites in 1980 led to much higher eligibility among the 1990 cohort than among the 1980 cohort. In our data, over 50 percent of nonwhites born in 1990 were eligible for Medicaid over the course of their childhood, while less than 30 percent of whites were eligible among this birth cohort.

Figure 1 also shows that the trends in overall eligibility track the trends in federal eligibility closely, especially after the 1984 birth cohort, which highlights the importance of federal Medicaid policies for identification. The simulated eligibility trends are very close to the actual trends as well. Thus, most of the aggregate pattern in Medicaid eligibility is due to policy changes rather than demographic shifts in the U.S. population.

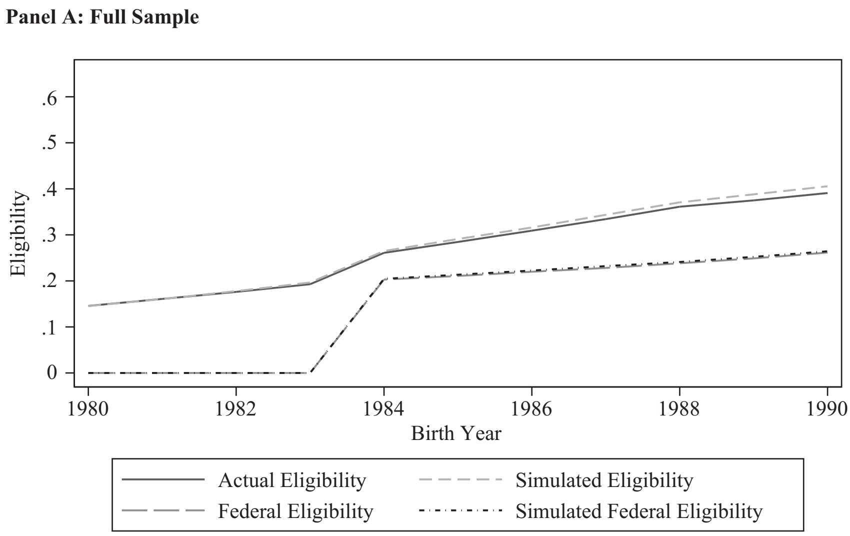

Figure 1

Medicaid Eligibility by Birth Cohort and Race

Notes: The figure shows average eligibility of 0–17-year-olds by birth cohort calculated using 1980–2007 CPS data combined with state by year Medicaid eligibility rules. Eligibility is calculated separately for whites and nonwhites. Simulated fixed eligibility is calculated by applying state-by-year rules to 1986 CPS data. Federal eligibility uses only federal Medicaid rules, applied to each state using fixed 1980 AFDC rules

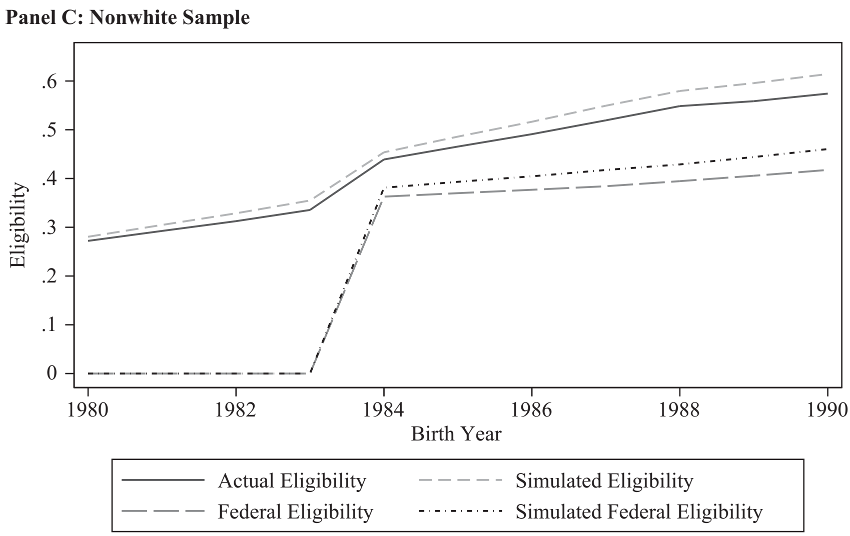

Figure 1 (*continued*)

B. Educational Attainment

The main outcome data we use come from the 2005–12 American Community Survey (ACS). The ACS was designed to replace the census, and thus the variables and design across the two surveys are almost identical. The sample for our analysis consists of birth cohorts from 1980–90 who are between 22 and 29 years old in 2005–12. For each individual in our sample, we observe eligibility in his or her birth state at each age between zero and 17. Table 1 shows the birth cohorts included in our analysis sample at each age and year. The top row shows the ACS (calendar) year, and the column shows the age of the respondent. For example, in the 2008 ACS, observations of 25-year-olds come from the 1983 cohort. This table illustrates that we do not observe each birth cohort in each ACS survey due to our constructed age cutoffs. For example, 29-year-olds are observed in 2009–12 and come from the 1980–83 birth cohorts only, whereas 25-year-olds come from the 1980–87 birth cohorts and are included in each of the ACS years in this analysis. Our use of 1980 as the earliest birth cohort is driven by our lack of information about state-specific Medicaid eligibility pre-1980, which makes it infeasible to use earlier birth cohorts.[12] Furthermore, we examine individuals only up to age 29, since by age 29 most education has been completed (Bound, Lovenheim, and Turner 2010). Including older individuals would reduce the number of calendar years in which we can identify eligibility for such respondents.

We calculate, for each respondent, indicators for whether she did not complete high school, whether she attended any college, and whether she obtained a bachelor's degree

12. We also note that Medicaid eligibility was very low pre-1980 and there were few expansions. Thus, our focus on birth cohorts between 1980 and 1990 captures most of the policy-driven variation in Medicaid exposure that has occurred since the program's inception.

Table 1
Birth Cohorts by Age in Each ACS Year

Age	2005	2006	2007	2008	2009	2010	2011	2012
22	1983	1984	1985	1986	1987	1988	1989	1990
23	1982	1983	1984	1985	1986	1987	1988	1989
24	1981	1982	1983	1984	1985	1986	1987	1988
25	1980	1981	1982	1983	1984	1985	1986	1987
26		1980	1981	1982	1983	1984	1985	1986
27			1980	1981	1982	1983	1984	1985
28				1980	1981	1982	1983	1984
29					1980	1981	1982	1983

(BA).[13] We collapse the data to birth cohort, state of birth, survey year, race (white/nonwhite) means for all variables, using the individual ACS weights. We then link each birth cohort, state of birth, race, and survey year cell to the Medicaid eligibility means discussed in Section IVA.[14] To do so, we calculate average eligibility for each birth cohort (c) in each survey year (t), state of birth (s), and race (r) over their childhood ages $(i \in [0, 17])$:

$$(1) \quad eligibility_{scrt} = \frac{1}{18}\sum_{i=0}^{17}\overline{elig}_{scirt},$$

where $\overline{elig}_{scirt}$ is the average Medicaid eligibility in birth state s, cohort c, survey year t, and of race r when the birth cohort was age i.

We construct an identical measure using fixed simulated eligibility:

$$(2) \quad fs\_eligibility_{scrt} = \frac{1}{18}\sum_{i=0}^{17}\overline{fs\_elig}_{scirt},$$

where $\overline{fs\_elig}_{scirt}$ is simulated Medicaid eligibility that is calculated using a constant sample from the 1986 CPS, as described above.

Descriptive tabulations of the analysis data for the full sample and by race group are shown in Table 2. In the full sample, the average respondent is 25, and about 68 percent of the respondents are white. The gender and age composition of the sample varies little across race groups. Furthermore, the educational attainment of nonwhites is much lower than that of whites, while average Medicaid eligibility is much higher for nonwhites. Both of these patterns reflect the strong correlation between socioeconomic status and race, which highlights the potential importance of any effect of Medicaid eligibility on educational attainment to help address gaps in educational outcomes between whites and nonwhites.

13. Our measure of high school completion includes GEDs, which is potentially problematic if Medicaid eligibility shifts students from obtaining a traditional high school diploma to a GED given the low returns to GED receipt found in the literature (Heckman and LaFontaine 2006). In 2008 and after, however, the ACS asks directly about GED completion. We show in Table 4 that our main high school completion results are not driven by GEDs.
14. Public insurance expansions can potentially alter the character of medical care for both individuals who experience a change in insurance coverage and also those who do not (Finkelstein 2007). Because we adopt an aggregated cohort-based empirical approach, we allow for the presence of these "spillovers" within cohorts.

Table 2
Summary Statistics for Analysis Samples

Variable Name	All	White	Nonwhite
No high school	0.094	0.071	0.143
	(0.048)	(0.029)	(0.045)
No high school or GED	0.126	0.102	0.176
	(0.054)	(0.038)	(0.050)
At least some college	0.656	0.694	0.572
	(0.086)	(0.062)	(0.071)
College graduate	0.265	0.309	0.172
	(0.108)	(0.096)	(0.065)
Age	25.001	25.031	24.936
	(2.156)	(2.155)	(2.157)
Male	0.504	0.508	0.497
	(0.039)	(0.032)	(0.049)
White	0.683	1.0	0.0
	(0.466)	(0.0)	(0.0)
Black	0.143	0.0	0.451
	(0.266)	(0.0)	(0.290)
Hispanic	0.123	0.0	0.386
	(0.230)	(0.0)	(0.255)
Other race	0.052	0.0	0.163
	(0.108)	(0.0)	(0.135)
Age zero to 17 three-year average Medicaid eligibility	0.236	0.156	0.410
	(0.152)	(0.077)	(0.127)
Age zero to 17 average fixed Simulated Medicaid eligibility	0.254	0.171	0.431
	(0.154)	(0.084)	(0.118)
Age zero to 17 three-year federal average Medicaid eligibility	0.113	0.068	0.208
	(0.140)	(0.070)	(0.194)
Age zero to 17 average federal fixed Simulated Medicaid eligibility	0.122	0.075	0.224
	(0.147)	(0.073)	(0.204)
Observations	5,494	2,754	2,740

Source: Author's tabulations from the 2005–12 ACS.
Notes: The samples consist of 1980–90 birth cohorts aged 22–29, for whom we observe Medicaid eligibility in every year in their birth state from age zero to 17. All tabulations were done using ACS sample weights. Standard deviations are shown in parentheses. Average eligibility is calculated using three-year moving averages. The GED tabulations only include ACS years 2008–12. Federal Medicaid eligibility is calculated using federal rules only, interacted with 1980 state AFDC rules as described in the text.

V. Empirical Methodology

In order to motivate our empirical models, it is helpful first to consider the ideal experiment one would use to identify the effect of Medicaid on long-run outcomes. Similar to the lottery for access to Oregon's Medicaid program (Finkelstein et al. 2012), the most credible way to estimate the program effects of interest would be to randomly assign eligibility for Medicaid to families with children of different ages. Such eligibility would last throughout the remainder of the child's schooling years, unless the household finances made them ineligible. With a long enough panel, we then could simply compare educational attainment among children who were randomly assigned Medicaid eligibility relative to those who were not. One also could calculate the effect of Medicaid coverage using randomized eligibility as an instrument (Finkelstein et al. 2012).

While such an experiment would identify the effect of Medicaid eligibility over one's childhood, in practice such an analysis is not currently feasible. The Oregon experiment did not target children,[15] and there is no other randomized Medicaid experiment of which we are aware. However, we can exploit the changes in both state and federal Medicaid eligibility rules that occurred over the 1980s and 1990s to approximate this experimental ideal. Because these policy changes never make Medicaid eligibility less generous, once a child's family is eligible for Medicaid in a state, he or she remains eligible for the duration of childhood unless the family's income or assets rise sufficiently. As we argue below, the variation in eligibility on which we focus is unrelated to demographic differences across individuals or to secular trends in educational attainment. Thus, these eligibility expansions mirror the assignment mechanism one would use in the ideal experiment.

We exploit the state and federal Medicaid eligibility expansions that occurred since 1980 using a difference-in-difference model that estimates how within-state changes in Medicaid eligibility across cohorts over their childhood impacted their educational attainment. Specifically, we estimate models of the following form:

$$(3) \quad Y_{scart} = \beta_0 + \beta_1 eligibility_{scrt} + \beta_2 X_{sart} + \gamma_{rs} + \delta_{rt} + \theta_{ra} + \varepsilon_{scart},$$

where Y_{scart} is the educational outcome (high school noncompletion rate, college attendance rate, or college graduation rate)[16] in state of birth s, birth cohort c, age a, of race r, in survey year t. The variable $eligibility_{scrt}$ comes from Equation 1 above and denotes the mean fraction of individuals of a given race and in a given birth cohort and birth state who were eligible for Medicaid.

Equation 3 also includes a vector of covariates, X_{sart}. In the baseline specification, X_{sart} consists only of an indicator for whether the observation is for the nonwhite sample or not. As we discuss below, we then include in X_{sart} some measures of potential confounding policies. In the baseline specification, the model includes as well a set of race-by-age fixed effects (θ_{ra}), race-by-state of birth fixed effects (γ_{rs}), and race-by-

15. Adult access to Medicaid through the Oregon lottery might indirectly influence children's outcomes through family financial stability or better parental health. However, the experiment occurred too recently to test its effects on children's long-run outcomes.

16. The "some college" outcome contains both college dropouts and those who receive an Associates Degree (AA). In Table A8, we show estimates that use "Associates Degree" rather than "Some College." The estimates are very similar in showing little effect of Medicaid eligibility on whether an individual obtains an AA.

calendar year fixed effects (δ_{rt}).[17] The race-by-age fixed effects in particular are important because they account for the fact that older individuals have more time to complete their education and that this age pattern might be different across whites and nonwhites. The race-by-state fixed effects control for fixed differences across states that are correlated with both Medicaid eligibility and educational attainment, such as the higher education structure and the industrial mix in the state, which we allow to vary by race as well. The race-by-year fixed effects account for any economy-wide shocks that could be correlated with prior Medicaid expansions and that might be different across racial groups.

The coefficient of interest in Equation 3 is β_1. It thus is important to clarify the sources of variation identifying this parameter, conditional on other controls in the model. As discussed above, we are exploiting variation from Medicaid eligibility expansions over the course of one's childhood. With the inclusion of state fixed effects, we are focusing on within-state changes in eligibility across cohorts and relating these to within-state changes in educational attainment. That is, within each state, we are using the fact that Medicaid eligibility for older cohorts is lower than that for younger cohorts, and thus we are essentially comparing across cohorts within states to identify β_1. When we pool all states, we are averaging these within-state effects together. Furthermore, the time-varying nature of the Medicaid expansions *across* states allows us to partial out age effects from calendar year effects.[18] As a result, our identifying variation comes from cross-cohort changes in childhood Medicaid eligibility within each state as well as cross-state variation in the timing of eligibility expansions.

Equation 3 incorporates a potentially restrictive set of assumptions about the cross-state variation we use, namely that the state and age fixed effects are constant across calendar years. We can relax this assumption by including race-state-year and race-age-year fixed effects in the model:[19]

$$(4)\quad Y_{scart} = \beta_0 + \beta_1 eligibility_{scrt} + \beta_2 X_{sart} + \gamma_{rst} + \theta_{rat} + \varepsilon_{scart}.$$

In Equation 4, including the age-by-year-by-race effects allows for any national birth cohort-specific shocks that could impact educational attainment or for more recent cohorts to obtain their degrees later. State-by-year-by-race fixed effects account for any state macroeconomic changes that could influence contemporaneous educational attainment. While Equation 4 is more flexible, it also is much more demanding of the data, which leaves us with less statistical power. As a result, these estimates tend to be imprecise.

Both Equations 3 and 4 are identified off of the fact that states expanded their Medicaid eligibility rules differentially across cohorts and the fact that the timing and size of

17. Henceforth, we will refer to "state fixed effects" and "state of birth fixed effects" synonymously.

18. If we estimated this model using one state, we could not estimate both age and year fixed effects. The reason is that, within a state, birth cohort fully describes the treatment intensity, and birth cohort and age-year interactions are perfectly collinear with each other.

19. Note that we do not control for race-by-state-by-age fixed effects. Thus, some of the identifying variation could be coming from fixed differences across ages within a state. However, this would require the existence of shocks to specific ages (but not birth cohorts) in a state that happen to be correlated with Medicaid eligibility differences. We have estimated models using these fixed effects, and the results are qualitatively similar (if somewhat less precise). We do not include them in the analysis because there is little economic justification for these controls. Furthermore, note that the estimates that use only federal variation would be unaffected by any such shocks.

these changes varied across states. These models therefore are difference-in-difference specifications, where the treatment dose varies across different cohorts depending on the state and year of birth, as well as depending on one's race. As discussed in Section IV, this variation comes from two sources: The first is rule changes that expand Medicaid eligibility to different age groups within each state, and the second is demographic shifts that expand the proportion of individuals who meet preexisting eligibility criteria.

For our analysis, the second source of variation is potentially problematic even conditional on the fixed effects. If there are demographic changes that affect the proportion of people eligible for Medicaid, these changes are likely to be correlated with educational attainment. Our limited set of demographic controls cannot fully account for such changes, although demographic changes that expand Medicaid eligibility most likely generate a negative bias in estimating the effect of Medicaid on educational attainment. We therefore use an instrumental variables strategy that is robust to demographic shifts. This IV strategy amounts to using *fs_eligibility* from Equation 2 as an instrument for *eligibility*. Because *fs_eligibility* is based on eligibility rules in each year using a fixed sample of individuals from the 1986 CPS, it is only affected by eligibility rule changes over time within states.

Similar to any difference-in-difference analysis, there are two main assumptions we invoke. The first is that Medicaid expansions are not correlated with underlying trends in educational attainment across cohorts at the state level. A particular concern for our identification strategy would be if Medicaid expansions are occurring in states that are becoming more affluent. Then, even simulated fixed eligibility changes would be positively correlated with underlying trends in educational attainment. We do not believe such a situation is likely, however, since states probably would be more compelled to expand Medicaid eligibility due to increased, not decreased, demand for public insurance. This is a common identification assumption that has been invoked repeatedly in the Medicaid literature (Currie and Gruber 1996a, 1996b; Cutler and Gruber 1996; Gross and Notowidigdo 2011; Gruber and Simon 2008). The second assumption underlying our identification strategy is that there are no other state-level policies that are correlated with Medicaid expansions that themselves might affect educational attainment.

We provide an extensive set of robustness checks to provide additional confidence that our results are not being driven by endogenous state Medicaid eligibility expansions or by other policies. First, in some specifications, we control in X_{sart} for average state EITC amounts between the ages of zero and 17 for each cohort. Prior work linking EITC policies to educational outcomes suggests EITC generosity could be a confounding factor if it is correlated with Medicaid generosity.[20] We also control for average school spending per pupil in the years in which each cohort was five to 17, separately by urban, rural and suburban districts. Although there is a tenuous link between school expenditures and education outcomes (see Hanushek 2003 for an overview of this literature), recent work has linked school spending increases from school finance reforms to better long-run outcomes (Jackson, Johnson, and Persico 2014). We view these factors as the ones that are most likely to produce confounding effects, but our estimates that control for these policies provide evidence that this is not the case.

20. See Michelmore (2013) for an overview of state-level EITC laws. We thank Kathy Michelmore for providing us with these data.

We provide more direct evidence that endogenous state Medicaid expansions are not biasing our estimates by using only federal Medicaid eligibility rules as discussed in Section IVA. The race-by-state of birth fixed effects control for the fixed differences in AFDC rules across states, and the identifying variation in the federal model comes solely through the fact that federal rule changes have differential impacts on states due to preexisting AFDC policies. Thus, there is no scope in these models for endogenous state decisions regarding Medicaid, and to the extent we obtain similar results using this variation, it will provide confidence in the validity of the results that use state Medicaid variation. This is the first paper to provide estimates using only federal eligibility variation, so these results are of interest in their own right insofar as they help validate the widely employed assumption that state Medicaid expansions are exogenous.

We also conduct robustness tests that include race and state of birth specific linear trends across birth cohorts. These models are identified off of the nonlinear increases in Medicaid eligibility that followed from state and federal law changes, and they help guard against any upward bias from correlated secular trends in educational attainment and Medicaid eligibility. We further provide a robustness check in which we randomly assign observed eligibility levels across age-state-year cells. Overall, our estimates are robust to using variation in Medicaid eligibility from different sources and to the addition of controls for other policies affecting low-income populations. These findings support the validity of our identification strategy.

Because errors are unlikely to be independent within states of birth over time, we cluster all standard errors at the state of birth level. All estimates also are weighted using sample weights provided in the ACS.

VI. Results

A. Main Results

Table 3 presents the main results from our estimation of Equations 3 and 4. Each cell in the table comes from a separate regression, with Panel A showing results that use all Medicaid eligibility and Panel B showing results using only federal eligibility. The first column in the table presents the first stage, which shows how a change in fixed simulated eligibility translates into actual eligibility. The table also shows the effect of actual Medicaid eligibility ("OLS") and fixed simulated eligibility ("RF" for reduced form) on high school noncompletion, college enrollment, and four-year college completion, as well as the associated IV estimates.

Across outcomes and the specifications shown in different rows, we find consistent evidence that Medicaid eligibility when young increases educational attainment. Focusing on the baseline IV results in Row 1, a ten percentage point increase in Medicaid eligibility reduces high school noncompletion by 0.39 of a percentage point, increases college enrollment by 0.35 of a percentage point, and increases BA attainment by 0.66 of a percentage point. The high school and college completion estimates are statistically significantly different from zero at the 5 percent level. Relative to the mean attainment rates shown in Table 2, these estimates translate into a 4.1 percent decline in high school dropouts, a 0.5 percent increase in college enrollment, and a 2.5 percent increase in BA receipt. As shown in Figure 1, there was a 24 percentage point increase in

Table 3
The Effect of Average Medicaid Eligibility During Childhood on Educational Attainment

		No High School			Some College			BA Plus		
Specification	First Stage	OLS	RF	IV	OLS	RF	IV	OLS	RF	IV
Panel A: All eligibility										
1. Baseline	0.927***	–0.030**	–0.036**	–0.039**	0.023	0.032	0.035	0.019	0.061**	0.066**
	(0.111)	(0.014)	(0.014)	(0.015)	(0.018)	(0.022)	(0.025)	(0.017)	(0.029)	(0.033)
2. EITC and school spending	0.966***	–0.023	–0.036**	–0.038**	0.024	0.033	0.034	0.016	0.067**	0.069**
	(0.076)	(0.015)	(0.015)	(0.015)	(0.019)	(0.021)	(0.022)	(0.020)	(0.030)	(0.032)
3. EITC, school spending, R-S-Y and R-A-Y FE	0.943***	0.000	–0.019	–0.021	0.045	0.076*	0.081*	0.042	0.096	0.102
	(0.111)	(0.020)	(0.024)	(0.024)	(0.031)	(0.042)	(0.042)	(0.028)	(0.064)	(0.064)
4. Baseline + R-S-Y and R-A-Y FE	0.890***	–0.001	–0.022	–0.025	0.010	0.087	0.099	0.036	0.080	0.091
	(0.170)	(0.020)	(0.025)	(0.027)	(0.033)	(0.059)	(0.071)	(0.026)	(0.066)	(0.070)
Panel B: Federal eligibility										
5. Baseline	0.212***	–0.030**	–0.012***	–0.055***	0.023	0.002	0.011	0.019	0.017***	0.078***
	(0.030)	(0.014)	(0.004)	(0.021)	(0.018)	(0.007)	(0.032)	(0.017)	(0.006)	(0.028)
6. EITC and school spending	0.210***	–0.023	–0.011***	–0.054**	0.024	0.002	0.011	0.016	0.016***	0.077***
	(0.032)	(0.015)	(0.004)	(0.021)	(0.019)	(0.007)	(0.033)	(0.020)	(0.006)	(0.027)

Source: Authors' estimation of Equations 3 and 4 in the text using 22–29-year-old respondents from the 2005–12 ACS.
Notes: Each cell in the table comes from a separate regression ($N = 5480$). The "OLS" columns refer to models that use a three-year moving average of actual eligibility as the independent variable, and the "RF" columns refer to models that use fixed simulated eligibility as the independent variable. The "IV" columns refer to models that instrument for actual eligibility using fixed simulated eligibility. All estimates include an indicator for the cell being nonwhite or not as well as race-by-age, race-by-calendar year, and race-by-state of birth fixed effects. Rows 3 and 4 include race by state of birth by calendar year (R-S-Y) fixed effects and race by age by calendar year (R-A-Y) fixed effects as shown in Equation 4. Standard errors clustered at the state of birth level are in parentheses: *** indicates significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level.

average eligibility during childhood between the 1980 and 1990 birth cohorts. Our estimates suggest this change would have reduced high school noncompletion by 10.0 percent, increased college enrollment by 1.3 percent, and increased college completion by 6.0 percent.

To put these effects in perspective, it is helpful to compare them to educational attainment trends over this period. Murnane (2013) shows that high school graduation rates increased by about six percentage points between the 1980 and 1990 birth cohorts. Because our estimates show that a 24 percentage point increase in Medicaid would increase high school completion by 0.94 percentage points, this implies that 15.6 percent of this increase can be attributed to Medicaid expansions. Our tabulations from the Current Population Survey indicate that college completion rates among 23-year-olds between the 1980 and 1990 birth cohorts increased by 4.8 percentage points. A 24 percentage point Medicaid eligibility increase would increase BA attainment by 1.6 percentage points using the baseline results, which implies that Medicaid expansions can explain 33.3 percent of the overall BA attainment increases over this period.

The results in Table 3 represent the effect on educational attainment (the intent-to-treat) of exposure to Medicaid eligibility throughout one's childhood. From a policy perspective, this is a parameter of interest because the government cannot compel the takeup of Medicaid. It also is the parameter on which much of the Medicaid literature focuses.[21] However, it is of interest as well to understand how enrollment in Medicaid affects educational attainment (the treatment effect on the treated). This is a difficult calculation because we lack the ability to track how average eligibility in one's childhood relates to Medicaid takeup during childhood. The existing estimates on takeup in the literature are not the appropriate "first stages" in our context, as they provide the contemporaneous effects on enrollment, where we would need an estimate of the effect on takeup over one's entire childhood to scale our results.

In order to estimate the treatment on the treated effect, we use the marginal takeup rate of 0.156 calculated by Gruber and Simon (2008) for the period 1996–2002 and assume this rate represents a yearly "risk" of taking up Medicaid. That is, we assume that in each year of childhood, 15.6 percent of the eligible population that has not yet taken up Medicaid does so. For example, at age zero, 15.6 percent of eligibles will have taken up Medicaid and 84.4 percent will have not, and at age one, $[15.6 + (15.6*84.4)] = 28.8$ percent will have taken it up and 71.2 percent will have not (and so forth). This method implies that for a child continuously eligible beginning at birth, he or she has a 95 percent chance of being on Medicaid at some point before the age of 18. Because children are made eligible at different ages, we calculate the associated likelihood of taking up Medicaid conditional on first being eligible at each age between zero and 17. We then average over these takeup estimates by age and find that expanding eligibility increases the likelihood a child takes up Medicaid at some point during childhood by 71.4 percent. This average takeup estimate matches the average Medicaid takeup rate of 73 percent quite closely (Currie 2004). Thus, treating marginal takeup rates as a constant risk of Medicaid enrollment reconciles the evidence on low marginal but high average takeup rates, which provides some validation for the method we use to calculate treatment on the treated effects.

21. The other relevant papers that use simulated instruments to examine effects on child or family outcomes, namely Levine and Schanzenbach (2009), Currie and Gruber (1996b), and Gross and Notowidigdo (2011), only report these intent-to-treat estimates.

We calculate the treatment effect on the treated by dividing our IV parameter estimates for eligibility by the 71.4 percent takeup rate. These calculations allow us to interpret our results from the standpoint of an individual becoming eligible for Medicaid (eligibility changing from zero to one) rather than from the standpoint of a policymaker who can expand eligibility by a given percentage among the state population. Treatment on the treated estimates shows that enrolling in Medicaid decreases the likelihood of dropping out of high school by 5.5 percentage points and leads to a 9.2 percentage point increase in the likelihood of completing a BA. To put the magnitude of these results in perspective, they are similar to the estimated effects on educational attainment of attending a higher-quality high school (Deming et al. 2014) and of Head Start (Garces, Thomas, and Currie 2002).

Rows 2–4 of Table 3 show our conclusions are largely robust to adding additional controls for EITC and school spending (Row 2). In Rows 3 and 4, the addition of race-state-year and race-age-year fixed effects reduces precision considerably. For the high school noncompletion outcome, the magnitudes of the point estimates decline, while for the college graduation outcome, the point estimates increase. However, in both cases, they are qualitatively similar to the baseline estimates, and the confidence intervals include the baseline estimates. Overall, adding controls for other potentially confounding policies as well as a large array of fixed effects do not change the conclusion that Medicaid-eligibility expansions had sizable positive effects on long-run educational attainment.

Table 3 also demonstrates that the OLS and IV results are quite different from each other. The OLS estimates in Panel A show Medicaid eligibility increases are associated with smaller high school dropout declines (in absolute value) and with smaller college completion increases. These results are suggestive that the bias from failing to account for the correlation between demographics and Medicaid eligibility would cause one to find a smaller effect of Medicaid on educational attainment.

As discussed in Section V, an important identification concern with the estimates that use state-level policy variation is that this variation is potentially correlated with secular trends in educational outcomes. This is especially relevant in this study relative to the rest of the Medicaid literature since we are using average Medicaid eligibility over one's childhood. As a result, there are no sharp breaks in average eligibility that we can exploit. In Panel B of Table 3, we thus show estimates using only federal Medicaid eligibility that are unlikely to be correlated with the trends associated with any one state. Focusing on the baseline estimates in Row 5, we find that increases in Medicaid eligibility reduce high school dropout and increase college enrollment and completion. Comparing the estimates in Row 5 to the baseline results in Row 1, the point estimates for the reduced form are smaller in absolute value when only the federal variation is used. As the IV estimates show, this difference mostly reflects the smaller first stage. In Panel A, the first stage estimates are around 0.9, suggesting that a ten percentage point change in fixed simulated eligibility is associated with a nine percentage point change in actual eligibility.[22] As expected, the link between federal Medicaid rules and actual eligibility is much weaker because we are ignoring state responses to the federal regulation changes. However, the first stage for the federal variation still is sizable in magnitude and is statistically significantly different from zero at the 1 percent level.

22. Our first stage estimates are similar to what has been found in prior work. Cutler and Gruber (1996) report a first stage of 0.84 for children and 0.95 for women, while Gross and Notowidigdo (2011) have an implied first-stage estimate of 0.61.

Comparing the IV estimates from similar models across panels shows that using the federal-only variation produces results that are quantitatively and qualitatively similar to the estimates that use state variation as well. For high school noncompletion in the baseline specification (Row 1), the estimates indicate a ten percentage point eligibility increase during childhood reduces dropout by 0.39 of a percentage point using all Medicaid variation, and it reduces dropout by 0.55 of a percentage point using only federal variation (Row 5). For college enrollment, the estimates in Row 5 are smaller than those in Row 1, and they are inconsistent with all but a small increase in college attendance. Finally, for college completion, the IV coefficients across panels of Table 3 show very similar effects of Medicaid eligibility expansions. Comparisons of Rows 2 and 6 show that our estimates using federal variation are comparable when including the EITC and school spending controls as well.[23] That these two models yield similar estimates of the effect of changes in Medicaid eligibility among children on long-run educational attainment supports our use of all Medicaid variation, as it suggests state Medicaid eligibility variation is not endogenous with respect to long-run educational outcomes.

A final potential concern with the results in Table 3 is that the high school completion variable groups GED and high school diploma recipients together. Starting in 2008, the ACS began asking separately about high school diploma and GED receipt, and in Table 4 we present estimates using 2008–12 data where we separate high school diploma nonreceipt from diploma and GED nonreceipt. As the table demonstrates, the effects are extremely similar across the two measures of high school completion, suggesting that our baseline estimates do not obscure potential shifts between traditional diplomas and GEDs. In addition, the some college and college plus estimates are similar in the 2008–12 sample, if somewhat larger. These results suggest our estimates are not driven by the particular sample period we chose.

B. Educational Attainment Results by Age at Expansion

While these results indicate a beneficial overall effect of Medicaid expansions on educational attainment, from a policy perspective, it is important to discern whether it matters if one measures eligibility at a point-in-time (typically birth) relative to over one's childhood, as well as whether there are effects of health insurance access at different ages. In Table 5, we present IV estimates that control for eligibility at birth (similar to what was done in Levine and Schanzenbach 2009 and Currie and Gruber 1996b). Using both Equations 3 and 4, we find very little evidence that Medicaid eligibility at birth is associated with long-run educational attainment. With the exception of our college completion measure (and only when we include the full range of fixed effects), the rest of the estimates are small in magnitude and either are "wrong-signed" or are not statistically significant. This finding is suggestive that the test score gains found by Levine and Schanzenbach (2009) do not translate into higher educational attainment. However, when we add in eligibility at ages one to 17, we find that Medicaid eligibility does lead to more

23. We do not present federal variation results that include race-state-year and race-age-year fixed effects. Due to the limited amount of variation in federal Medicaid eligibility, including these fixed effects yields large standard errors that make the resulting estimates uninformative. Furthermore, the goal of using the federal variation is to find a source of variation that is unlikely to be related to state trends. As a result, there is little theoretical justification for including the race-state-year and race-age-year fixed effects in these models.

Table 4
The Effect of Average Medicaid Eligibility During Childhood on Educational Attainment, Separating GED and HS Diplomas, 2008–12

Specification	First Stage	No High School Diploma		No GED or High School		Some College		BA Plus	
		OLS	IV	OLS	IV	OLS	IV	OLS	IV
Panel A: All eligibility									
1. Baseline	0.927***	–0.020	–0.047**	–0.023	–0.043**	0.009	0.047	0.025	0.085**
	(0.115)	(0.018)	(0.019)	(0.020)	(0.020)	(0.020)	(0.037)	(0.019)	(0.042)
2. EITC and school spending	0.959***	–0.013	–0.046***	–0.015	–0.042**	0.021	0.039	0.021	0.087**
	(0.078)	(0.019)	(0.018)	(0.020)	(0.020)	(0.022)	(0.027)	(0.022)	(0.041)
3. EITC, school spending R-S-Y and R-A-Y FE	0.944***	0.010	–0.023	0.003	–0.016	0.032	0.079**	0.025	0.099
	(0.110)	(0.021)	(0.024)	(0.023)	(0.028)	(0.028)	(0.039)	(0.031)	(0.071)
4. Baseline + R-S-Y and R-A-Y FE	0.889***	0.009	–0.027	0.003	–0.022	–0.003	0.100	0.020	0.090
	(0.173)	(0.020)	(0.027)	(0.023)	(0.029)	(0.030)	(0.072)	(0.027)	(0.075)
Panel B: Federal eligibility									
5. Baseline	0.210***	–0.020	–0.075***	–0.023	–0.075***	0.009	–0.004	0.025	0.086***
	(0.030)	(0.018)	(0.025)	(0.020)	(0.026)	(0.020)	(0.038)	(0.019)	(0.030)
6. EITC and school spending	0.206***	–0.013	–0.073***	–0.015	–0.073***	0.021	0.000	0.021	0.087***
	(0.031)	(0.019)	(0.026)	(0.020)	(0.026)	(0.022)	(0.041)	(0.022)	(0.031)

Source: Authors' estimation of Equations 3 and 4 in the text using 22–29-year-old respondents from the 2008–12 ACS.
Notes: Each cell in the table comes from a separate regression ($N = 3957$). The "OLS" columns refer to models that use a three-year moving average of actual eligibility as the independent variable, and "IV" columns refer to models that instrument for actual eligibility using fixed simulated eligibility. All estimates include an indicator for the cell being nonwhite or not as well as race-by-age, race-by-calendar year, and race-by-state of birth fixed effects. Rows 3 and 4 include race by state of birth by calendar year (R-S-Y) fixed effects and race by age by calendar year (R-A-Y) fixed effects as shown in Equation 4. Standard errors clustered at the state of birth level are in parentheses: *** indicates significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level.

Table 5
IV Estimates of the Effect of Average Medicaid Eligibility at Birth and During Childhood on Educational Attainment

	Age Zero		Age Zero, One to 17	
Medicaid Age Eligibility	Baseline	FE	Baseline	FE
No high school				
Age zero eligibility	–0.006	–0.011	–0.004	–0.008
	(0.011)	(0.013)	(0.011)	(0.013)
Age one to 17 eligibility			–0.038***	–0.022
			(0.014)	(0.029)
Any college				
Age zero eligibility	–0.016	–0.001	–0.017	–0.011
	(0.012)	(0.014)	(0.012)	(0.017)
Age one to 17 eligibility			0.032	0.100
			(0.023)	(0.070)
BA plus				
Age zero eligibility	–0.024*	0.046*	–0.026*	0.039**
	(0.013)	(0.019)	(0.013)	(0.019)
Age one to 17 eligibility			0.062*	0.070
			(0.031)	(0.068)

Source: Authors' estimation of Equations 3 and 4 in the text using 22–29-year-old respondents from the 2005–12 ACS.
Notes: All estimates include an indicator for the cell being nonwhite or not, race-by-age fixed effects, race-by-calendar year fixed effects, and race-by-state of birth fixed effects. "FE" estimates are from Equation 4 and include race by state of birth by calendar year (R-S-Y) fixed effects and race by age by calendar year (R-A-Y) fixed effects. Standard errors clustered at the state of birth level are in parentheses: *** indicates significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level.

education among affected cohorts. It is the eligibility at older ages that is responsible for this relationship; eligibility at birth continues to be uncorrelated with long-run educational outcomes.[24] The age zero and age one to 17 estimates are statistically different from each other at the 10 percent level for no high school and at the 5 percent level for BA plus in the first column. But when we add in the fixed effects in the second column, the loss of precision makes these estimates not statistically different from each other (although they still remain qualitatively different from each other).

Table 6 expands upon the finding that the age at which one experiences Medicaid eligibility might matter for long-run outcomes. In this table, we estimate the effects of eligibility using ages zero to three, ages four to eight, ages nine to 13, and ages 14–17. These

24. The one exception is for the BA Plus outcome when estimating Equation 4. Here, we see a positive effect of eligibility at birth on college completion. But, the effect of eligibility at ages one to 17 still is larger (although also less precisely estimated).

Table 6
IV Estimates of the Effect of Average Medicaid Eligibility During Childhood on Educational Attainment, by Age at Eligibility

Age Range	No High School Diploma	Any College	BA Plus
Panel A: Baseline model			
0–3	–0.011	0.019	–0.003
	(0.014)	(0.015)	(0.017)
4–8	–0.030**	0.013	0.037**
	(0.010)	(0.014)	(0.014)
9–13	0.007	–0.025*	–0.026
	(0.010)	(0.015)	(0.017)
14–17	–0.012	0.061**	0.069**
	(0.008)	(0.011)	(0.016)
Panel B: Baseline + R-S-Y and R-A-Y FE			
0–3	0.003	–0.010	0.053
	(0.020)	(0.030)	(0.036)
4–8	–0.030	0.032	0.051
	(0.020)	(0.026)	(0.057)
9–13	0.003	0.012	–0.002
	(0.012)	(0.029)	(0.022)
14–17	–0.013	0.072**	0.025
	(0.015)	(0.028)	(0.025)

Source: Authors' estimation of Equations 3 and 4 in the text using 22–29-year-old respondents from the 2005–12 ACS.
Notes: Each column by panel in the table comes from a separate regression ($N = 5480$). All estimates include an indicator for the cell being nonwhite or not, race-by-age fixed effects, race-by-calendar year fixed effects, and race-by-state of birth fixed effects. Estimates in Panel B come from Equation 4 and also include race by state of birth by calendar year (R-S-Y) fixed effects and race by age by calendar year (R-A-Y) fixed effects. Standard errors clustered at the state of birth level are in parentheses: *** indicates significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level.

categories are selected to correspond to those Medicaid eligibility age categories delineated in Currie et al. (2008) as well as to correspond roughly to different schooling levels (preschool, elementary school, etc.). Panel A shows results from the baseline specification (Equation 3), while in Panel B we include our full set of fixed effects (Equation 4).

While the results are somewhat imprecise, they show evidence that eligibility at ages zero to three has little impact on educational attainment. For high school completion, it is eligibility at ages four to eight that is the most important.[25] For college

25. This is not to say that this insurance has no effect as they age. Indeed, one reason why Medicaid expansions among younger children might be more effective is because they are likely to be eligible for a longer proportion of their childhood. Of course, this does not explain why expansions among very young children do not affect educational attainment.

completion, the estimates are less consistent across panels. Focusing on Panel B, eligibility at all ages except nine to 13 are positively related to BA attainment. But in Panel A, only eligibility during teenage years impacts college completion. We also find evidence of a college enrollment effect due to eligibility expansions among teenagers. At least some of this effect may be due to contraceptive services that can be purchased with Medicaid (Lovenheim, Reback, and Wedenoja 2016; Kearney and Levine 2009). Taken together, the results from Tables 5 and 6 demonstrate that estimates of Medicaid eligibility at birth provide an incomplete characterization of how Medicaid affects educational attainment and that eligibility among older, school-aged children is particularly important for driving attainment outcomes.

C. Educational Attainment Results by Race

Thus far, we have estimated models that pool effects across racial groups. But, given persistent racial disparities in educational attainment, heterogeneous effects by race are of considerable interest. In Tables A1 and A2,[26] we estimate our models separately for whites and nonwhites, respectively. For whites, the effects on high school noncompletion are negative, but they are smaller in absolute value than in the pooled model and they are not statistically significant at conventional levels. There is a larger effect of Medicaid on college completion for whites, although these estimates also are not statistically significantly different from zero at conventional levels. The effect is on the order of 1.0 to 1.3 percentage points for each ten percentage point increase in Medicaid eligibility. There is a sizable, positive effect on college completion for whites using the federal variation as well. While these point estimates are large—suggesting a 2.5 percentage point increase from a ten percentage point Medicaid-eligibility increase—they are consistent with observed increases in white college completion across these cohorts.[27]

Among nonwhites, the effects on high school noncompletion are larger, particularly in the baseline model. High school noncompletion is reduced by 0.46 percentage points for each ten percentage point increase in Medicaid eligibility.[28] There also is evidence of a positive college completion effect on the order of 0.4 of a percentage point for each ten percentage point increase in eligibility. However, as we show in Table A6, the BA estimates for nonwhites are not robust to the inclusion of state-specific birth cohort trends. Overall, these results are consistent with a larger effect of Medicaid eligibility on higher education completion for whites and a larger effect on high school completion

26. All online appendices can be found at http://uwpress.wisc.edu/journals/journals/jhr-supplementary.html.
27. CPS tabulations indicate that college completion rates among white 23-year-olds increased by 6.4 percentage points between the 1980 and 1990 birth cohorts. White Medicaid eligibility expanded by 19 percentage points across cohorts, which would increase BA attainment rates by 4.75 (= 0.25*0.19*100) percentage points. This is 74 percent of the total BA attainment increase over this period.
28. It is notable that these estimates become much smaller in absolute value when we include the full set of fixed effects. However, they also become much less precise such that the baseline estimates are still within the 95 percent confidence intervals. Furthermore, the estimates using federal variation show a large effect of eligibility increases on high school completion.

for nonwhites.[29] Other than for the college completion estimates using federal eligibility variation, the estimates by race are not statistically different from each other, however.

D. Robustness Checks

In this section, we present several robustness checks that yield additional insight into the validity of our central identifying assumption—namely, that there are not differential underlying trends in educational attainment correlated with public health insurance eligibility expansions. First, in Table 7, we present results from the models presented in Table 3 that also include state-specific linear birth cohort trends, separately by race. If there are differential trends in educational attainment correlated with Medicaid expansions, these results should yield substantively different results from our baseline model. For the high school graduation rate estimates, the results are extremely similar to baseline. However, adding state-specific linear time trends reduces the college completion estimates that include state-level eligibility. As shown in Tables A5 (whites) and A6 (nonwhites), this average result is mostly due to the fact that there is a large effect of Medicaid eligibility on whites when including state-specific linear time trends, but no effect on nonwhites. These results also highlight that the federal variation estimates are robust to including state-specific linear time trends. Thus for whites, there continues to be a large effect of eligibility expansions on college completion, while for nonwhites the effects of eligibility expansions are localized to high school completion.

Second, in Table 8, we show the mean and the 2.5th and 97.5th percentiles from 500 simulations that randomly assign Medicaid eligibility and fixed simulated eligibility across age-state-year cells. That is, we take combinations of actual and fixed simulated eligibility, and as a pair randomly assign them to different age-state-year cells, separately by race. This assignment is done with replacement. Both for the baseline model and for the model including race-state-year and race-age-year fixed effects, the average estimates are very close to zero. Furthermore, the nonparametric confidence intervals suggest these null estimates are precisely estimated. This robustness check suggests the results presented in Table 3 are due to the specific way the Medicaid eligibility expansions were rolled out over time within states. When we randomly assign eligibility levels, they are no longer meaningfully related to educational attainment.

In Tables A7–A11, we also present results that explore the sensitivity of our results to several modeling assumptions we have made throughout the analysis. In Table A7, we estimate our models excluding the small states that generate fewer than 100 observations for an underlying age-race-cohort-year eligibility calculation. The results are virtually identical to those in Table 3. Table A8 replaces the some college outcome with whether an individual earns an Associates Degree (AA). We fail to find an effect of Medicaid expansions on AA attainment, which supports our finding that the main impacts of Medicaid on educational attainment come through high school and BA completion. In Table A9, we use one-year instead of three-year Medicaid eligibility. Again, our results are very similar to baseline.

29. In Tables A3 and A4, we also show estimates by gender. Although the estimates are somewhat noisy, they suggest a high school completion effect exists for both males and females, while the positive college enrollment and completion results are isolated to males.

Table 7
The Effect of Average Medicaid Eligibility During Childhood on Educational Attainment, Including State of Birth Linear Time Trends

		No High School			Some College			BA Plus		
Specification	First Stage	OLS	RF	IV	OLS	RF	IV	OLS	RF	IV
Panel A: All eligibility										
1. Baseline	1.009***	−0.033**	−0.043***	−0.042***	0.011	−0.019	−0.019	0.028	0.026	0.026
	(0.059)	(0.016)	(0.015)	(0.015)	(0.022)	(0.029)	(0.028)	(0.022)	(0.025)	(0.024)
2. EITC and school spending	1.013***	−0.032**	−0.041***	−0.040***	0.007	−0.016	−0.016	0.022	0.035	0.034*
	(0.059)	(0.016)	(0.015)	(0.014)	(0.020)	(0.027)	(0.026)	(0.018)	(0.022)	(0.020)
3. EITC, school spending, R-S-Y and R-A-Y FE	1.019***	−0.009	−0.031	−0.029	0.046	0.000	−0.002	0.014	0.005	0.006
	(0.148)	(0.033)	(0.048)	(0.043)	(0.040)	(0.062)	(0.054)	(0.026)	(0.049)	(0.043)
4. Baseline + R-S-Y and R-A-Y FE	1.033***	−0.003	−0.023	−0.021	0.044	−0.000	−0.003	0.017	0.013	0.014
	(0.147)	(0.032)	(0.047)	(0.041)	(0.039)	(0.060)	(0.052)	(0.027)	(0.046)	(0.040)
Panel B: Federal eligibility										
5. Baseline	0.220***	−0.033**	−0.013***	−0.059***	0.011	0.004	0.019	0.028	0.018***	0.082***
	(0.032)	(0.016)	(0.004)	(0.021)	(0.022)	(0.007)	(0.030)	(0.022)	(0.006)	(0.026)
6. EITC and school spending	0.223***	−0.032**	−0.012***	−0.054**	0.007	−0.000	0.000	0.022	0.011*	0.049**
	(0.032)	(0.016)	(0.004)	(0.021)	(0.020)	(0.007)	(0.030)	(0.018)	(0.006)	(0.025)

Source: Authors' estimation of Equations 3 and 4 in the text using 22–29-year-old respondents from the 2005–12 ACS.
Notes: Each cell in the table comes from a separate regression ($N = 5480$). The "OLS" columns refer to models that use a three-year moving average of actual eligibility as the independent variable, and the "RF" columns refer to models that use fixed simulated eligibility as the independent variable. The "IV" columns refer to models that instrument for actual eligibility using fixed simulated eligibility. All estimates include an indicator for the cell being nonwhite or not as well as race-by-age, race-by-calendar year, and race-by-state of birth fixed effects. Rows 3 and 4 include race by state of birth by calendar year (R-S-Y) fixed effects and race by age by calendar year (R-A-Y) fixed effects as shown in Equation 4. Estimates also include race by state of birth linear time trends. Standard errors clustered at the state of birth level are in parentheses: *** indicates significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level.

Table 8
Placebo Tests with Randomly Assigned Medicaid Eligibility

	No High School Graduation		Some College		BA Plus	
	RF	IV	RF	IV	RF	IV
Baseline	0.0001	0.0001	–0.0004	–0.0005	2.87e–5	2.31e–5
	(–0.006, 0.007)	(–0.007, 0.007)	(–0.011, 0.010)	(–0.013, 0.012)	(–0.010, 0.011)	(–0.011, 0.012)
Baseline + R-S-Y & R-A-Y FE	0.0002	0.0002	–0.0004	–0.0005	0.0001	0.0001
	(–0.008, 0.007)	(–0.009, 0.008)	(–0.012, 0.011)	(–0.014, 0.013)	(–0.010, 0.011)	(–0.011, 0.012)

Source: Authors' estimation of Equations 3 and 4 in the text using 22–29-year-old respondents from the 2005–12 ACS.
Notes: We randomly assign age-state-year eligibility and fixed simulated eligibility, as a pair, across different age-state-year cells. This is done separately by race. We conduct 500 separate simulations for each outcome, both including and excluding state of birth by calendar year (R-S-Y) fixed effects and race by age by calendar year (R-A-Y) fixed effects. All estimates include an indicator for the cell being nonwhite or not as well as race-by-age fixed effects, race-by-calendar year fixed effects, and race-by-state of birth fixed effects. The table shows the mean estimate across all 500 runs, as well as the 2.5th and 97.5th percentiles in parentheses. The range in parentheses thus shows the nonparametric 95 percent confidence interval. IV estimates are constructed by dividing the reduced form (RF) by the first stage, which also is estimated using this method. First stage estimates are available upon request from the authors.

In Table A10, we assign Medicaid eligibility based on an individual's state of residence rather than state of birth. The results are very similar to the baseline analysis. This is especially notable since there are very few sources of data that include an individual's state of birth. Thus, any long-run analysis of Medicaid eligibility requires researchers to use an individual's current state of residence as a proxy for childhood exposure, which is problematic if there is endogenous mobility related to Medicaid eligibility. Our estimates are inconsistent with such mobility, and thus our findings expand the possibilities for examining long-run Medicaid effects using other data sets that only contain current state of residence. Finally, in order to assess whether the results are sensitive to cohort exposure to local labor market conditions, in Table A11 we control for average unemployment rates in each state of birth and for each birth cohort. The estimates change little from those in Table 3. Overall, these results show our conclusions are robust to different ways of constructing our analysis sample and to different modeling assumptions.[30]

VII. Conclusion

In this paper, we provide the first evidence on the effects of public health insurance expansions on long-run educational attainment in the United States. Overall, our results suggest large effects of childhood Medicaid expansions on eventual educational outcomes. Our baseline estimates indicate that a ten percentage point increase in Medicaid eligibility between the ages of zero and 17 decreases the likelihood of not completing high school by approximately 4 percent and increases the four-year college completion rate by 2.5 percent. The effects on high school completion are largest among nonwhites, while the effects on college completion are largest for whites. We also present evidence that public health insurance expansions when children are of school age are closely linked with long-run educational attainment; eligibility expansions beyond birth lead to higher educational attainment. To the best of our knowledge, these are the first estimates to demonstrate the importance of health insurance eligibility among older children, particularly as it relates to educational outcomes.

Although the public health insurance expansions that we study occurred in the past several decades, our results have several implications that are important for current public policy. First, they suggest that the long-run benefits of providing health insurance to low-income children may be much larger than the short-run gains. Evidence pointing to the large and growing returns to educational attainment (Autor, Katz, and Kearney 2008) as well as the importance of education in increasing intergenerational economic mobility (Black and Devereaux 2011, Chetty et al. 2014) suggests that the returns on the public investments in health insurance in the 1980s and 1990s will be realized for some time.

Second, our results relate to current policy discussions over the future of the SCHIP program, which have accompanied the larger debate over the ACA. More specifically,

30. In an earlier version of our paper (Cohodes et al. 2014), we present evidence using outcomes from the Youth Risk Behavior Surveillance System (YRBSS) that better health is one of the mechanisms driving our results by showing that Medicaid eligibility when young translates into better teen health. While our estimates from this analysis were typically not statistically significant at conventional levels, they provide support for the idea that better health is an important mechanism that drives at least part of the increased educational attainment we document.

the ACA prohibits states from imposing eligibility and enrollment standards for Medicaid and SCHIP until 2019 that were more restrictive than those in place in March 2010 (when the ACA was passed). However, there have been attempts in Congress to repeal these provisions, which would essentially allow states to cut SCHIP benefits and eligibility. A back-of-the-envelope calculation indicates that eliminating the SCHIP program would reduce eligibility for public health insurance by 15.4 percentage points. Our baseline estimates suggest such a decline would increase the high school dropout rate by six-tenths of a percentage point and would decrease the college enrollment rate by five-tenths of a percentage point and the college completion rate by one percentage point. The results from this study highlight the need to account for the long-run effects of public health insurance provision when considering changes to the publicly provided healthcare system that is targeted at low-income children.

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