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THE EFFECT OF CHILD HEALTH INSURANCE ACCESS ON SCHOOLING: EVIDENCE FROM PUBLIC INSURANCE EXPANSIONS

Sarah Cohodes
Samuel Kleiner
Michael F. Lovenheim
Daniel Grossman

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The Effect of Child Health Insurance Access on Schooling: Evidence from Public Insurance Expansions
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ABSTRACT

Public health insurance programs comprise a large share of federal and state government expenditure, and these programs are due to be expanded as part of the 2010 Affordable Care Act. Despite a large literature on the effects of these programs on health care utilization and health outcomes, little prior work has examined the long-term effects of these programs and resultant health improvements on important outcomes, such as educational attainment. We contribute to filling this gap in the literature by examining the effects of the public insurance expansions among children in the 1980s and 1990s on their future educational attainment. Our findings indicate that expanding health insurance coverage for low-income children has large effects on high school completion, college attendance and college completion. These estimates are robust to only using federal Medicaid expansions, and they are mostly due to expansions that occur when the children are older (i.e., not newborns). We present suggestive evidence that better health is one of the mechanisms driving our results by showing that Medicaid eligibility when young translated into better teen health. Overall, our results indicate that the long-run benefits of public health insurance are substantial.

Sarah Cohodes
John F. Kennedy School of Government
Harvard University
79 JFK Street
Cambridge, MA 02138
cohodes@fas.harvard.edu

Samuel Kleiner
Cornell University
College of Human Ecology
108 Martha Van Rensselaer Hall
Ithaca, NY 14853
and NBER
skleiner@cornell.edu

Michael F. Lovenheim
Department of Policy Analysis and Management
Cornell University
102 Martha Van Rensselaer Hall
Ithaca, NY 14853
and NBER
mfl55@cornell.edu

Daniel Grossman
MVR Hall, Dept. of PAM
Cornell University
Ithaca NY 14853
dan.s.grossman@gmail.com

1. Introduction

Whether and how to provide access to affordable healthcare for low-income Americans has become a central policy issue in the US, driven in part by the large and persistent health disparities that exist across the socioeconomic spectrum. 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 US history. Medicaid is the primary method through which low-income families can access affordable health insurance. Since its inception in 1965, Medicaid has expanded greatly. Currently, over 50% of children in the United States are eligible for publicly-provided health insurance through this program,[2] and health insurance coverage is high amongst this population. The expansions that generated this high level of coverage were expensive, however. 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.[3] 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, highlights 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, (e.g., Currie and Gruber, 1996a, 1996b; Moss and Carver, 1998; Baldwin et al., 1998; Cutler and Gruber, 1996, LoSasso and Buchmueller, 2004; Gruber and

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[2] 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.

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

Simon, 2008), typically showing that Medicaid expansions increase healthcare access, 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). If Medicaid leads to better health outcomes among children and to more stable finances among low-income households, as suggested by prior research, Medicaid expansions could lead to long-run benefits for affected children. Given the persistently high returns to human capital investments (e.g., Autor, Katz and Kearney, 2008) as well as human capital models that suggest childhood health and family resources should both positively influence educational attainment, examining the effects of Medicaid expansions on long-run educational attainment is of considerable policy interest.

In this paper, we provide the first evidence in the literature on how expanding health insurance for young children influences their eventual educational attainment. 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 combine data on 22-29 year olds born between 1980 and 1990 from the 2005-2012 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), in which we use Medicaid eligibility of a fixed population in each age, state, year and race as an instrument for actual eligibility. This IV approach accounts for the fact that the 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. The underlying identification assumption for our purposes is that Medicaid rules are not changing due to unobserved cross-cohort trends in educational attainment. A large body of prior work has established the credibility of this assumption in terms of health, fertility and family bankruptcy (Currie and Gruber, 1996a; DeLeire et al., 2011; Gross and Notowidigdo, 2011). We extend this literature by implementing a series of robustness checks, including using only federal Medicaid variation that cannot be affected by state-level choices, to further support the validity of this methodology.

The main contribution of this paper to the literature is to demonstrate the effect of health insurance access among both young and school-age children on their long-run educational attainment. While 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 (e.g., Figlio et al., 2013; Levine and Schanzenbach, 2009; Currie and Gruber, 1996b), the effect of children's access to healthcare services on their educational attainment has not been studied previously. From a policy perspective, this is an important group to consider because of the large amount spent on providing health insurance to non-newborn children. Furthermore, socioeconomic disparities in educational outcomes begin at young ages and largely persist throughout the lifecycle (Carneiro and Heckman, 2002; Todd and Wolpin, 2007). It thus is critical to understand whether reducing health insurance disparities across the socioeconomic distribution among children can be useful as a means to close these persistent educational gaps that are present in later years.

We find consistent evidence that Medicaid exposure when young increases later educational attainment. A 10 percentage point increase in average Medicaid eligibility between the ages of 0-17 decreases the high school dropout rate by 0.5 of a percentage point, increases college enrollment by between 0.7 of a percentage point and 1.0 percentage point, and increases the four-year college attainment rate (i.e., BA receipt) by 0.9-1.0 percentage point. These estimates translate into declines in high school non-completion of about 5%, increases in college attendance of between 1.0% and 1.5% and increases in BA attainment of about 3.3%-3.7% relative to the sample means.

One of the main contributions of this analysis is to examine whether it is necessary for a child to be treated at birth or whether there are returns to expanding eligibility amongst older, largely school-age children. Prior work in this area has focused more on eligibility at birth than on the effects of eligibility at older ages (Levine and Schanzenbach, 2009; Currie and Gruber, 1996b).[4] We provide some of the first estimates in this literature on heterogeneity by age at the time of expansion. In particular, we show that Medicaid expansions have the largest effect on educational attainment when children are 2 years or older compared to when they are 0-1 years old. That is, Medicaid expansions to slightly older, mostly school-age children, increase educational attainment, while eligibility at birth has a smaller effect on this long-run outcome.[5]

As a means to understand a central mechanism through which the effects we find may operate, we examine the impact of Medicaid eligibility when young on teen health. Using data from the Youth Risk Behavior Surveillance System (YRBSS), we show that Medicaid eligibility between age 0 and one's age at the time of the survey has sizable positive effects on a range of health outcomes. For example, a 10 percentage point increase in Medicaid eligibility reduces

[4] Currie and Gruber (1996a) examine effects of Medicaid expansions on child mortality for children 1-14, but they do not break out the effects by child age at expansion.

[5] Part of the smaller effect of Medicaid eligibility at birth could be due to the fact that Medicaid reduces infant mortality (Currie and Gruber, 1996b). Any resulting compositional changes in birth cohorts likely would lead to a reduction in long-run outcomes, all else equal. To the extent Medicaid changes the composition of births and/or of older children through reduced mortality, this should attenuate our estimates. However, infant and child mortality rates are sufficiently low in the US that any such attenuation is probably very small.

risky sexual activity by 12%, reduces body mass index (BMI) by 1.3% and leads to a 4.3% decline in the likelihood of being obese. Furthermore, Medicaid eligibility decreases the amount of reported mental health issues, and students are less likely to report having an eating disorder. These results are consistent with better health being an important mechanism that drives at least part of the increased educational attainment we document.

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, and that part of the return to these expansions is a potential reduction in inequality and higher economic growth that stems from the creation of a more skilled workforce.

The rest of this paper is organized as follows: Section 2 describes the public health expansions we use in our analysis, and Section 3 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 4 provides a description of the data. We outline our empirical strategy and detail our results in Sections 5 and 6, respectively, before concluding in Section 7.

2. 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 programs, 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).[6] Thus, 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% of the poverty line, were eligible for Medicaid, and 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 1990's, states were required to cover all children underneath 100% of the poverty line, and children under age 6 underneath 133% of the poverty line. Many states opted in to 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 is one of the largest expansions of public health insurance to date. SCHIP provided matching funds to states to expand coverage to children underneath 200% of the poverty line. Prior to SCHIP, states were permitted to cover children up to 200% of the poverty line, but, without federal matching funds, very 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, we use both

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

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. Importantly, the federal variation had differential impacts on eligibility in different states based on pre-existing welfare rules. We use such variation explicitly below to test the robustness of our estimates to the assumption that the state Medicaid variation is exogenous.

3. Previous Literature

The effect of Medicaid eligibility on education will flow through two main potential channels: better health due to Medicaid take-up, as well as higher household resources stemming from the insurance protection provided by Medicaid. This paper thus relates to the large literature examining the effect of Medicaid on health care utilization, health outcomes and household finances, as well as the literature linking health and family resource changes to educational outcomes. Below, we discuss both sets of research in turn.

3.1 Effects of Medicaid Expansions on Utilization, Health Outcomes and Family Finances

Much prior research has documented the effects of Medicaid expansions on both the use of medical care and health status. In their examination of the effects of health insurance on utilization, Buchmueller et al. (2005) provide a detailed survey of this literature, noting that economic theory predicts that health insurance coverage induces greater medical care utilization by reducing the cost of care to patients (Phelps, 1997). Consistent with this prediction, studies of state-based Medicaid and federal expansions of coverage show that these programs lead to increases in health care use on both extensive and intensive margins, and that these effects are observed both among children and adults (e.g. Currie and Gruber, 1996a, 1996b; Currie, 2000; Kaestner et al., 2000; Kaestner et al., 2001; Almeida, Dubay, and Ko, 2001; Banthin and Selden, 2003; Dafny and Gruber, 2005). As noted by Levy and Meltzer (2008), while health insurance increases the quantity of care consumed, the effects of coverage will vary based on the availability of providers and efficacy of the medical care consumed as a result of increased coverage. While their review suggests that health insurance improves the health of infants and children (with little conclusive evidence shown for non-elderly adults), Finkelstein et al. (2012) present evidence that the expansion of public insurance improves both physical and mental health for adults as well.

Recent work also has suggested that public health insurance successfully shelters low-income families from financial risk associated with negative health shocks. Gross and Notowidigdo (2011) show that families exposed to Medicaid expansions are less likely to declare bankruptcy, while the estimates of Dave et al. (2013) indicate that Medicaid eligibility was associated with a decrease in the employment probability of women who recently gave birth. In their study of the randomized Medicaid experiment in Oregon, Finkelstein et al. (2012) also find that obtaining access to public health insurance reduces the amount of out-of-pocket medical expenditures as well as medical debt.

3.2 Effects of Health and Family Resources on Educational Attainment

How are such changes in child health and family finances from Medicaid expansions predicted to affect educational attainment? A sizable literature dating back to the seminal contribution of Grossman (1972) examines the effect of education on future health,[7] but much less work has been done estimating the effect of health in childhood on educational achievement and attainment. Existing research has documented that better fetal health translates into increased

[7] There currently is very mixed evidence on whether education affects long-run health outcomes (e.g., Adams et al., 2003; Cutler and Lleras Muney, 2006; Grossman, 2004; Clark and Royer, 2013), with much heterogeneity in terms of the credibility of the identification strategies used, the time periods and countries studied, and the education levels examined.

educational outcomes. These studies testing the “fetal origins” hypothesis overwhelmingly show that health interventions and shocks among pregnant women, as well as differences in measurable health at birth, have long-run consequences for cognitive abilities and educational outcomes of children (e.g., Figlio et al., 2013; Almond and Mazumder, 2011; Almond, Edlund and Palme, 2009; Almond, 2006; Black et al., 2007; Oreopoulos et al., 2008; Royer, 2009).

Despite the evidence linking fetal health to long-run outcomes, little research exists that examines how childhood health after birth impacts such outcomes. Currie et al. (2010) find that children with negative health experiences have worse 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. Historical evidence also suggests such a link exists: hookworm eradication led to more school attendance and literacy gains in the US south in the early 1900’s (Bleakley 2007), and malaria eradication efforts resulted in small gains in income for cohorts whose regions were treated before other cohorts (Bleakley 2010).[8]

Cox and Reback (2013) as well as Lovenheim, Reback and Wedenoja (2013) examine the effect of health care *access* on educational attainment using the rollout of school-based health centers in the US. The former study finds that center openings lead to high 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 might relate to different effects of health at different times during childhood.

[8] 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.

In work most closely associated with ours, Levine and Schanzenbach (2009) examine the effect of Medicaid and SCHIP expansions *at birth* on future educational achievement as measured by state-level NAEP scores. They examine differences in Medicaid expansion by state and the differences between age cohorts in a triple difference framework, and their results suggest that a 50 percentage point increase in Medicaid eligibility corresponds to a 0.09 standard deviation increase in reading test scores. They find no effect on math test scores, however.

Our analysis is distinguished from theirs along several dimensions. First, we focus on the effects of expanding health insurance to children of all ages. This question has been studied much less but is very important given the expected increase in the number of insured children due to the implementation of the ACA (Kenney et al., 2011) and the amount of money the US currently spends on providing health care to children through Medicaid.[9] Indeed, our results indicate that the long-run effects of Medicaid are driven by eligibility amongst non-newborn children, which further highlights the relative contribution of our analysis. Second, we examine effects on long-run educational attainment rather than on test scores at younger ages. A growing body of evidence points to the effect of given educational interventions on test scores being a poor predictor of the effects on the longer-run outcomes that are of higher interest, such as educational attainment and earnings (e.g., Ludwig and Miller, 2007; Chetty et al., 2011; Deming et al., 2013).[10]

[9] If health insurance among school-age children did not positively affect them, ostensibly the government could only offer Medicaid to pregnant women and households with very young children. Thus, it is important to understand what value there is to offering school-age children Medicaid.

[10] 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 4th and 8th 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.

The second main channel through which Medicaid can influence educational attainment is through its effect on family resources. Several recent studies attempt to isolate the causal impact of additional funds on educational achievement. Dahl and Lochner (2012) use the Earned Income Tax Credit (EITC) as an instrument for unexpected income changes and find that an additional $1000 of income for a family in poverty results in test score gains of about 0.06 standard deviations. Duncan, Morris, and Rodrigues (2011) find test score effects of a similar magnitude to those in Dahl and Lochner (2012) when they examine 11 random assignment experiments of welfare and anti-poverty programs from the 1990's. Michelmore (2013) shows that income changes from the EITC also lead to higher college enrollment and completion. Together, the changes in family finances and child health generated by Medicaid expansions could lead to impacts on long-run educational attainment. We provide the first analysis in the literature of these long-run effects.

4. 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.

4.1 Medicaid Eligibility Data

Our Medicaid eligibility data are constructed for the years during which our 1980-1990 birth cohort are between the ages of 0-17 using the March Current Population Survey (CPS). 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).[11] 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 we construct is the proportion of households of a given race (white, nonwhite) with children of age *a* in state *s* and year *t* who are eligible for Medicaid, where $a \in (0,1,...17)$. Thus, for example, we calculate the proportion of households with 5-year-olds in New York who are eligible for Medicaid in each year between 1980 and 2007. We calculate eligibility separately by child's race due to the strong correlation between race and Medicaid eligibility, such that a given change in eligibility rules is likely to impact nonwhites differently than whites. 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 2005-2012. We focus on the 1980-1990 birth cohorts who are between the ages of 22 and 29 in 2005-2012, which is why our CPS sample ends in 2007 (when the 1990 birth cohort is 17).[12] Due to small sample sizes in the CPS, particularly within each age-race-state cell, we use three-year moving averages of calculated eligibility instead of yearly eligibility.[13] 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 both changes in eligibility rules and changes in demographic composition. In order to isolate the variation in Medicaid eligibility

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[11] 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.

[12] 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.

[13] This method necessitates the use of CPS data through 2009 (which contains 2008 income information), to enable the construction of our 3-year moving average.

due to eligibility rule changes, we follow the method first used in Currie and Gruber (1996a, 1996b) and Cutler and Gruber (1996) and calculate the proportion of each state, age and race in each year that would be eligible for Medicaid using a fixed national sample that does not vary across states or over time. We use a 20% national sample from the 1986 CPS and calculate the share of this fixed population with a child of age *a* 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. The 20% sample is comprised of 31,223 individuals, and these respondents are assigned to each state so that the total sample size is 1.6 million (31,223 × 51). Critically, this sample does not vary by demographic characteristics across states or over time. Thus, this measure is unaffected by state-specific trends in populations or economic conditions that relate to both eligibility and coverage, like a state recession. For each year, we use the same 1.6 million person sample and adjust family income for inflation using the Consumer Price Index for All Urban Consumers (CPI-U). We then calculate our measure of "simulated fixed eligibility" for individuals of a given state, age and race based on the federal and state specific Medicaid eligibility rules in effect in a given year.[14] 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. Since the fixed sample includes the same sample in every year, we calculate yearly eligibility rather than the three-year moving average that we use for actual eligibility.

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, as well as the timing of state expansions and their generosity levels. Because such state decisions may be endogenous with respect to underlying trends in educational

[14] Of the initial 31,223 individuals in the fixed sample, 23,870 are white and 7,353 are nonwhite.

attainment, 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 their pre-existing state AFDC policies. Hence, we fix AFDC rules in each state as of 1980, and then we calculate 3-year moving average actual eligibility as well as yearly fixed simulated eligibility for each age, race and state that would occur *only* due to federal regulations. Put differently, our federal eligibility measures yield state-race-age-year eligibility that would occur if no states provided more generous Medicaid access than required under federal law. By design, this source of Medicaid eligibility is uncorrelated with any decisions states can make regarding Medicaid policies.

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 0-17 to which the cohort was exposed. The panels of the figure show both actual eligibility that uses state and federal rules and well as eligibility that uses only federal rules for birth cohorts 1980-1990. 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% between the 1980 and 1990 birth cohorts. Much of this was the non-linear 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% of the poverty line. 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% of nonwhites born in 1990 were eligible for Medicaid over the course of their childhood, while less than 30% 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 US population.

4.2 Educational Attainment

The main outcome data we use come from the 2005-2012 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 1980-1990 who are between 22 and 29 in 2005-2012. Thus, for each individual in our sample, we observe eligibility in his or her birth state at each age between 0 and 17. Table 1 shows the birth cohorts included in our analysis sample at each age and year. 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 2009-2012 and come from the 1980-1983 birth cohorts only, whereas 25 year olds come from the 1980-1987 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, and thus it is not feasible to use earlier birth cohorts.[15] Furthermore, we examine individuals only up to age 29 as by age 29 most education has been completed (Bound, Lovenheim and Turner, 2010), and including older individuals would reduce the number of calendar years in which we can identify eligibility for such respondents.

[15] 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.

The central benefit of using the ACS data for this analysis is our ability to link each respondent to the state of his or her birth. Using the current state of residence is problematic because students may endogenously sort across states, especially if Medicaid indeed impacts education outcomes. One's state of birth is unlikely to be related to Medicaid rules, however, especially since prior work has found no link between Medicaid rules and fertility patterns (Zavodny and Bitler, 2010; DeLeire, Lopoo and Simon, 2011). We calculate, for each respondent, indicators for whether the person did not complete high school, whether she attended any college and whether she obtained a Bachelors Degree (BA). 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.[16] In 2008 and after, however, the ACS asks directly about GED completion. Using data from 2008-2012, we find little evidence that our main high school completion results are being driven by GEDs. Thus, our use of pre-2008 data is not biasing the main conclusions one might draw from our results about the relationship between Medicaid and high school completion.

We collapse the data to age, state of birth, survey year, race (white/nonwhite) means for all variables, using the individual census weights. We then link each age, state-of-birth, race, survey year cell to the Medicaid eligibility means discussed in Section 4.1. In particular, we calculate average eligibility for each birth cohort (*c*), state of birth (*s*), race (*r*) and survey year (*t*):

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

[16] Heckman and LaFontaine (2006) present evidence that the returns to a GED are lower than the returns to a high school diploma. Thus, examining patterns of substitution across these degrees is of interest.

where $\overline{elig}_{scri}$ is the average Medicaid eligibility in birth state *s* and birth cohort *c* of race *r* when the individual was age *i*.

We construct an identical measure using fixed simulated eligibility:

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

where *fs_eligibility* is fixed simulated Medicaid eligibility for each birth cohort (*c*), state of birth (*s*), race (*r*) and survey year (*t*) that is calculated using a constant sample from the 1986 CPS, as described above.

In addition to these treatment variables, we also calculate separately the percent of each state-of-birth, birth cohort and race cell who are male and who are married in each ACS survey year. 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% of the respondents are white. The gender and age composition of the sample varies little across race groups, but the proportion of whites who are married is much higher. Furthermore, the educational attainment of nonwhites is much lower than whites, while average Medicaid eligibility is much higher for nonwhites. Both of these patterns reflect the strong correlation between SES 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.

5. Empirical Methodology

To analyze the effect of Medicaid eligibility expansions on educational attainment, we use difference-in-difference methods, where our identification strategy makes use of differences across states over time in both the eligibility criteria (which varies by income, age, gender and family composition) and the timing of the expansions. Specifically we estimate models of the following form:

$$Y_{scrt} = \beta_0 + \beta_1 eligibility_{scrt} + \beta_2 X_{scrt} + \gamma_s + \delta_t + \varepsilon_{scrt}, \tag{3}$$

where Y_{scrt} is the educational outcome (high school non-completion rate, college attendance rate or college graduation rate) in state-of-birth s for birth cohort c, of race r in calendar year t. The variable $eligibility_{scrt}$ comes from equation (1) above and denotes the mean fraction of individuals from birth cohort c and of race r who are eligible for Medicaid in state s between the ages 0-17.

The vector X_{scrt} includes percent male, percent married, a full set of age fixed effects and an indicator for whether the observation is for the nonwhite sample or not. The age fixed effects in particular are important because they account for the fact that older individuals have more time to complete their education. In addition, equation (3) includes both state-of-birth fixed effects (γ_s) and ACS calendar year fixed effects (δ_t).[17] The 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. The year fixed effects account for any economy-wide shocks that could be correlated with prior Medicaid expansions.

The coefficient of interest in equation (3) is β_1; conditional on the set of controls and fixed effects in the model, the variation used to identify this coefficient comes from increases in eligibility within state across birth cohorts over time. This is basically a difference-in-difference specification, where the treatment dose varies across different cohorts depending on the state and year of birth as well as on one's race. As discussed in Section 4, this variation comes from two sources: the first is rule changes that expand Medicaid eligibility to different populations within

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

each state, and the second is demographic shifts that expand the proportion of individuals who meet pre-existing eligibility criteria.

For our analysis, the second source of variation is potentially problematic. If there are demographic changes that affect the proportion of people eligible for Medicaid, these demographic 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* 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 and unobserved trends in educational attainment. We do not believe such a situation is likely, however, since states likely would be more compelled to expand Medicaid eligibility due to increased, not decreased demand. This is a common identification assumption that has been invoked repeatedly in the Medicaid literature (e.g., 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 but 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. First, in some specifications we control for average state EITC amounts between the ages of 0-17 for each cohort.[18] Prior work linking EITC policies to educational outcomes suggests EITC generosity could be a confounding factor if it is correlated with Medicaid generosity. We also control for average school spending per pupil in the years in which each cohort was 5-17, 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 reform to higher long-run educational outcomes (Jackson, Johnson and Persico, 2014). If such spending changes are correlated with Medicaid expansions, it could generate a bias in our results. We view these alternative policies as the two that are most likely to produce confounding effects, and our estimates that control for these policies provide evidence on whether this is so. In some specifications, we also include state-of-birth by year fixed effects as well as age-by-year fixed effects. These fixed effects allow us to control flexibly for any contemporaneous age- or state-specific shocks that are correlated with prior Medicaid expansions.[19] The state-of-birth by year fixed effects also will remove policy variation at the state level that was instituted to affect particular birth cohorts.

[18] See Michelmore (2013) for an overview of state-level EITC laws. We thank Kathy Michelmore for providing us with these data.
[19] We do not estimate models with state-by-age fixed effects, since this is where much of the state policy variation in Medicaid expansion occurs.

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 4.1. The 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 pre-existing 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 as well. To the best of our knowledge, 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.

Finally, we conduct robustness tests by linking respondents when they are 18 to eligibility among 0-17 year olds in their state. These 18 year olds should not be affected by eligibility rates among younger children, but would be affected by unobserved state-level shocks correlated with educational attainment and Medicaid changes. We also present estimates that include linear birth cohort time trends by state-of-birth, which control for any differences in unobserved linear trends in educational attainment across states. Overall, our estimates are robust to using variation in Medicaid eligibility from different sources and to the series of robustness checks we conduct. 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.

6. Results

6.1 Main Results

Table 3 presents the main results from our estimation of equation (3). Each cell in the table comes from a separate regression, with the results in Panel A showing results that use all Medicaid eligibility and the results in 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 ("Red Form") on high school non-completion, 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, a 10 percentage point increase in fixed simulated eligibility reduces high school non-completion by 0.49 of a percentage point, increases college enrollment by 0.70 of a percentage point, and increases BA attainment by 0.85 of a percentage point. All estimates are statistically significantly different from zero at the 5% level. Relative to the mean attainment rates shown in Table 2, these estimates translate into a 5.2% decline in high school dropouts, a 1.1% increase in college attendance, and a 3.2% increase in BA receipt. As shown in Figure 1, there was a 24 percentage point increase in average eligibility during childhood between the 1980 and 1990 birth cohorts. Our estimates suggest this change would have reduced high school non-completion by 12.5%, increased college enrollment by 2.6%, and increased college completion by 7.7%.[20] Murnane (2013) shows that high school graduation rates increased by

[20] It is likely that these gains in educational attainment are even more pronounced among those that take up Medicaid. Child take-up rates for the earlier expansions were 24% (Cutler and Gruber, 1996), while Gruber and

about 6 percentage points between the 1980 and 1990 birth cohorts. Since a 24 percentage point increase in Medicaid would increase high school completion by 1.2 percentage points, our results indicate that 20% of this increase can be attributed to Medicaid expansions.

How do these effect sizes compare to effects from other education interventions? These comparisons are complicated by the fact that we are examining attainment at relatively older ages, which is rare in the literature examining early lifetime interventions. One point of comparison is the teacher quality literature. In a recent analysis, Chetty, Friedman and Rockoff (2013) show that having a teacher whose value-added is one standard deviation higher in one year leads to an increase in the likelihood of college attendance of 2.2%. This is very comparable to our college attendance result, which underscores that the effects we find are large. Another point of comparison is with the school quality literature. Deming et al. (2014) shows lottery-based results from an open enrollment system in the Charlotte-Mecklenburg schools that winning a lottery and thus attending a higher-quality school increases college attendance by 2.7%-7.5%. Finally, Garces, Thomas and Currie (2002) show that Head Start participants are 4 percentage points more likely to graduate from high school and 9 percentage points more likely to attend college in estimates that control for selection using mother fixed effects. Overall, our estimates indicate that a 10 percentage point Medicaid expansion produces educational attainment increases that are either of equal size or somewhat smaller than other notable educational interventions.

Rows (2)-(4) of Table 3 show our estimates are robust to adding additional controls for EITC and school spending as well as state-year and age-year fixed effects into the models. The estimates change little in terms of magnitude or statistical significance, and the some college

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Simon (2008) find that take-up for the SCHIP expansions of the late 1990's and early 2000's was approximately 7%.

estimates increase to an effect size of about 1.0 percentage point for each 10 percentage point increase in Medicaid eligibility.

Table 3 also demonstrates that the OLS and reduced form/IV results are quite different from each other. The OLS estimates in Panel A show Medicaid eligibility increases are associated with higher high school dropout and with lower college enrollment and completion, although in many cases the OLS estimates are not statistically significant at even the 10% level. These results are suggestive that the bias from failing to account for the correlation between demographics and Medicaid eligibility would cause one to find an adverse effect of Medicaid on educational attainment. Once this confounding factor is controlled for, however, Table 3 indicates Medicaid expansions have a positive and sizable impact on long-run educational attainment.

Panel B of Table 3 shows estimates that use only federal Medicaid eligibility. Focusing on the baseline estimates in Row (5), we show that federal eligibility expansions 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 estimate is very close to 1, suggesting that a 10 percentage point change in fixed simulated eligibility is associated with a 10 percentage point change in actual eligibility.[21] As expected, the link between federal Medicaid rules and actual eligibility is much weaker because we are ignoring state responses to the federal

[21] Our first-stage estimates are somewhat larger than have been found in prior work. Gross and Notowidigdo (2011) have an implied first-stage estimate of 0.61, while Cutler and Gruber (1996) report a first-stage of 0.84 for children and 0.95 for women. There are several potential explanations for this difference. We updated the code used to calculate Medicaid eligibility in both papers, we split our eligibility estimates by race, and we examine average eligibility over the years when respondents are 0-17 rather than contemporaneous eligibility. This averaging, plus our use of 3-year moving averages for actual eligibility, is likely to reduce measurement error-driven attenuation from the small sample sizes in the CPS.

regulation changes. However, the first stage for the federal variation still is sizable in magnitude and is statistically significant from zero at the 1% level.

Comparing the IV estimates from similar models across panels shows that using the federal variation only produces results that are quantitatively and qualitatively similar to the estimates that use state variation as well. For high school non-completion in the baseline specification (Row 1), the estimates indicate a 10 percentage point eligibility increase during childhood reduces dropout by 0.49 of a percentage point using all Medicaid variation, and it reduces dropout by 0.51 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), but they still are positive and statistically different from zero at the 10% level. 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 robust to the inclusion of EITC and school spending controls as well.[22] 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.

Another potential concern with the results in Table 3 is that they group GED and high school diploma recipients together. Starting in 2008, the ACS began asking separately about high school diploma and GED receipt separately, and in Table 4 we present estimates using 2008-2012 data. We now split up high school diploma non-receipt from diploma and GED non-receipt. Henceforth, we will only present IV and OLS estimates due to space considerations; reduced

[22] We do not present federal variation results that include state-year and 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.

form estimates are available upon request. As the table demonstrates, the effects are virtually identical across the two measures of high school completion, suggesting that our baseline estimates were not obscuring potential shifts between traditional diplomas and GEDs. In addition, the some college and college plus estimates are similar in the 2008-2012 sample, if somewhat larger for the college graduation outcome. These results suggest our estimates are not driven by the particular sample period we choose.[23]

6.2 Educational Attainment Results by Race and Age at Expansion

In Table 5, we present estimates that include an interaction between Medicaid eligibility and an indicator for the age-race-state-year cell being nonwhite. The interaction shows the difference in the effect of Medicaid eligibility on the educational attainment of nonwhites relative to whites. Although the estimates are not very precise, the baseline IV model provides suggestive evidence that the high school completion effect is isolated to the nonwhite sample, while the college enrollment and college completion effects are similar across race groups. Part of this difference in results across education level could be due to the much larger high school non-completion rate amongst nonwhites (see Table 2). Thus, nonwhite students are more likely to be marginal with respect to high school completion. The results that include state-year and age-year fixed effects in Panel B are qualitatively similar, but the some college coefficients are smaller and there is some evidence of a high school effect for whites as well. Despite the imprecision of these estimates, Table 5 suggests that the Medicaid expansions in the 1980s and 1990s helped to reduce the racial gap in high school completion somewhat, although they had no such effect for higher levels of education, where impact of Medicaid was more universal across racial groups.

[23] We omit results using federal variation from this table as they are similar to those in Table 3. They are available from the authors upon request.

As discussed in Section 3.2., one of the contributions of this paper is to identify whether there are effects of health insurance access after birth. Since prior work in this area has examined effects of Medicaid eligibility among pregnant women, our estimates are informative about any impacts of public health insurance among older children. In Table 6, we present IV estimates of equation (3) that control separately for Medicaid eligibility when a respondent was 0-1 versus 2-17. The results point to the importance of Medicaid eligibility amongst older children. Indeed, none of the 0-1 eligibility estimates are statistically significantly different from zero and of the expected direction, and they are universally smaller than the 2-17 estimates in absolute value. Thus, our results suggest expanding Medicaid eligibility to children after birth has a substantial effect on long-run educational attainment, particularly after infancy. This finding sheds some light on why our estimates are somewhat larger than those in Levine and Schanzenbach (2009), as they examine eventual test score effects of Medicaid eligibility at birth. Our results indicate that educational outcomes, at least in the longer-run, are more sensitive to Medicaid expansions that target older, mostly school-age, children.

6.3 Robustness Checks

In this section, we present two robustness checks that yield insight into the validity of our central identifying assumption that there are not differential underlying trends in educational attainment correlated with public health insurance eligibility expansions. In Table 7, we perform a falsification test that examines the educational attainment of individuals as a function of the 0-17 year old Medicaid eligibility rate when they were 18. Typically, Medicaid eligibility is lower for adults, and thus their eventual educational attainment should be less responsive to Medicaid expansions that occur when they are 18 and that affect younger children. However, if there are unobserved state-specific shocks that affect both education investment decisions and Medicaid expansions, then 18 year olds would plausibly be influenced by those shocks. In Table 7, we use the birth cohorts from 1962-1972, who turned 18 between 1980 and 1990, and we assign to each of them the 0-17 average eligibility in their state-year-race cell when they turned 18. Since this sample was born prior to the large Medicaid expansions we study, they had very low public health insurance eligibility rates when young.

The IV results in Table 7 show the estimates on the instrumented 0-17 eligibility measure when respondents were 18. If they are similar to the results in Table 3, it is evidence that our estimates are picking up underlying trends in educational attainment rather than causal effects of Medicaid on education.[24] There is little evidence of a relationship between eligibility of 0-17 year olds in a state when one is 18 and eventual educational attainment, however. For high school and college completion, the coefficients are not of the expected sign. And while the IV estimates for some college is positive, the standard errors also are large, which renders these estimates very imprecise.

In Table 8, we provide further evidence of the relevance of underlying confounding trends by presenting results from the models presented in Table 3 that include state-specific linear birth cohort trends. If there are differential trends in educational attainment correlated with Medicaid expansions, these results should yield substantively different results from our baseline model. However, the results are very similar to those in Table 3, suggesting that linear differences in trends across states are not biasing our baseline estimates.

6.4 The Effect of Childhood Medicaid Eligibility on Teen Health

As discussed in Section 4, one of the main mechanisms through which public health insurance can affect long-run educational attainment is through promoting better health amongst

[24] Alternatively, if Medicaid expansions have general equilibrium effects in terms of increasing the supply-side of the health care market, then there could be effects on 18-year-olds that are not evidence of confounding trends.

children. In order to examine the potential importance of this mechanism, we estimate the effect of Medicaid eligibility during childhood on health outcomes in the teenage years. To do this analysis, we use the Youth Risk Behavior Surveillance System (YRBSS), which is a nationally-representative survey of 9th to 12th grade students that is conducted by the Centers for Disease Control (CDC).[25] The data are available from 1991-2011[26] and contain state identifiers, so we can estimate equation (3) using these data with health outcomes as the dependent variable. A central drawback of these data, however, is that we do not observe state of birth, just state of current residence. These estimates therefore are potentially biased by endogenous mobility, and we consider them more suggestive than our main results due to this issue.

Table 9 contains health outcome estimates from the YRBSS data. Here, we calculate Medicaid eligibility from age 0 up until each respondent's age. Thus, for a 15-year-old, the eligibility measure is average eligibility in the respondent's state of residence he/she would have experienced between the ages of 0-14. These estimates also include state-by-year and age-by-year fixed effects. Across virtually all measures of health outcomes, Table 9 shows that Medicaid eligibility during childhood translates into better health and better health behaviors by the time one is a teenager. We construct a risky sex index based on several questions regarding sexual activity,[27] and our IV results indicate a 10 percentage point increase in Medicaid eligibility during youth reduces risky sexual behavior by 11.9% relative to the sample mean.[28] There also is

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[25] These data can be accessed at: http://www.cdc.gov/HealthyYouth/yrbs/index.htm?s_cid=tw_cdc16.
[26] We limit our sample to years 1995-2007 because in these years sample respondents between the ages of 14 and 18 are most similar to the 1980 to 1990 birth cohorts we analyze using ACS data.
[27] See Online Appendix Table A-1 for a list of the variables used to construct this index as well as individual estimates for each measure. The variables that constitute this index are indented directly below the Risky Sex Index in the table. We also list the variables and individual estimates for the variables that make up our measures of whether a respondent has a mental health issue or an eating disorder in Online Appendix Table A-1.
[28] The first stage estimate for these regressions is 0.882, with a standard error of 0.123.

a 7.0% reduction in the likelihood of being sexually active due to such an increase in public health insurance eligibility.

Our results suggest that those exposed to higher Medicaid eligibility at young ages have lower weight as well. There is a negative effect of Medicaid eligibility on BMI, on the order of a 1.3% reduction relative to the sample mean from a 10 percentage point increase in eligibility. We also find that the likelihood of being overweight declines by 11.1%. These estimates are significantly different from zero at the 10 percent level. Though not significant, obesity also declines by over 4% due to a 10 percentage point increase in eligibility during childhood. While there is a sizable increase in the likelihood of being a regular smoker, there are similarly large declines in marijuana use and alcohol consumption due to Medicaid eligibility. There also are large, although not statistically significant, declines in mental health problems and the likelihood of having an eating disorder.

On the whole, these estimates are consistent with the Medicaid expansions we examine producing healthier teens. To the extent that such increases in health enter into the education production function, they are likely to be one of the mechanisms driving the higher educational attainment that stems from the same Medicaid eligibility increases. While more work is necessary to clearly understand the role of health in producing educational outcomes, these results provide suggestive evidence that such a link is present and that health may be an important input into the education production function.

7. Conclusion

In this paper, we provide the first evidence on the effects of public health insurance expansions on long-run educational attainment. Overall, our results suggest large effects of childhood Medicaid expansions on eventual educational outcomes. Our baseline estimates indicate that a 10 percentage point increase in Medicaid eligibility between the ages of 0 and 17 decreases the likelihood of not completing high school by approximately 5%, increases college attendance by 1.1% to 1.5%, and increases the 4-year college completion rate by 3%-3.5%. We also present evidence that public health insurance expansions when children are between 2-17 are more closely linked with long-run educational attainment than are expansions that occur when a child is 0-1 year old. To the best of our knowledge, these are the first estimates to demonstrate the importance of health insurance eligibility amongst older children, particularly as it relates to educational outcomes. Our analysis concludes by showing that the health insurance expansions we examine also translated into better health amongst teenagers, which we posit is an important mechanism through which health insurance access for children impacts their educational attainment.

Although the public health insurance expansions 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 (e.g., 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 felt 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, the ACA prohibits states from imposing eligibility and enrollment standards for Medicaid and SCHIP that were more restrictive than those in place in March 2010 (when the ACA was passed) until 2019. However, there have been attempts in Congress to repeal these provisions, which would essentially allow states to cut SCHIP benefits and eligibility. In addition, SCHIP funding is up for re-authorization in 2015, and its passage is far from assured. A back-of-the-envelope calculation using our data indicates that eliminating the SCHIP program would reduce eligibility for public health insurance by 15.4 percentage points. Our estimates suggest such a decline would increase the high school dropout rate by eight tenths of a percentage point and would decrease both the college enrollment rate by 1.1 percentage points and the college completion rate by about 1.3 percentage points. The results from this study highlight the need to account for the types of long-run effects of public health insurance provision when considering changes to the publicly provided health care system that is targeted at low-income children.

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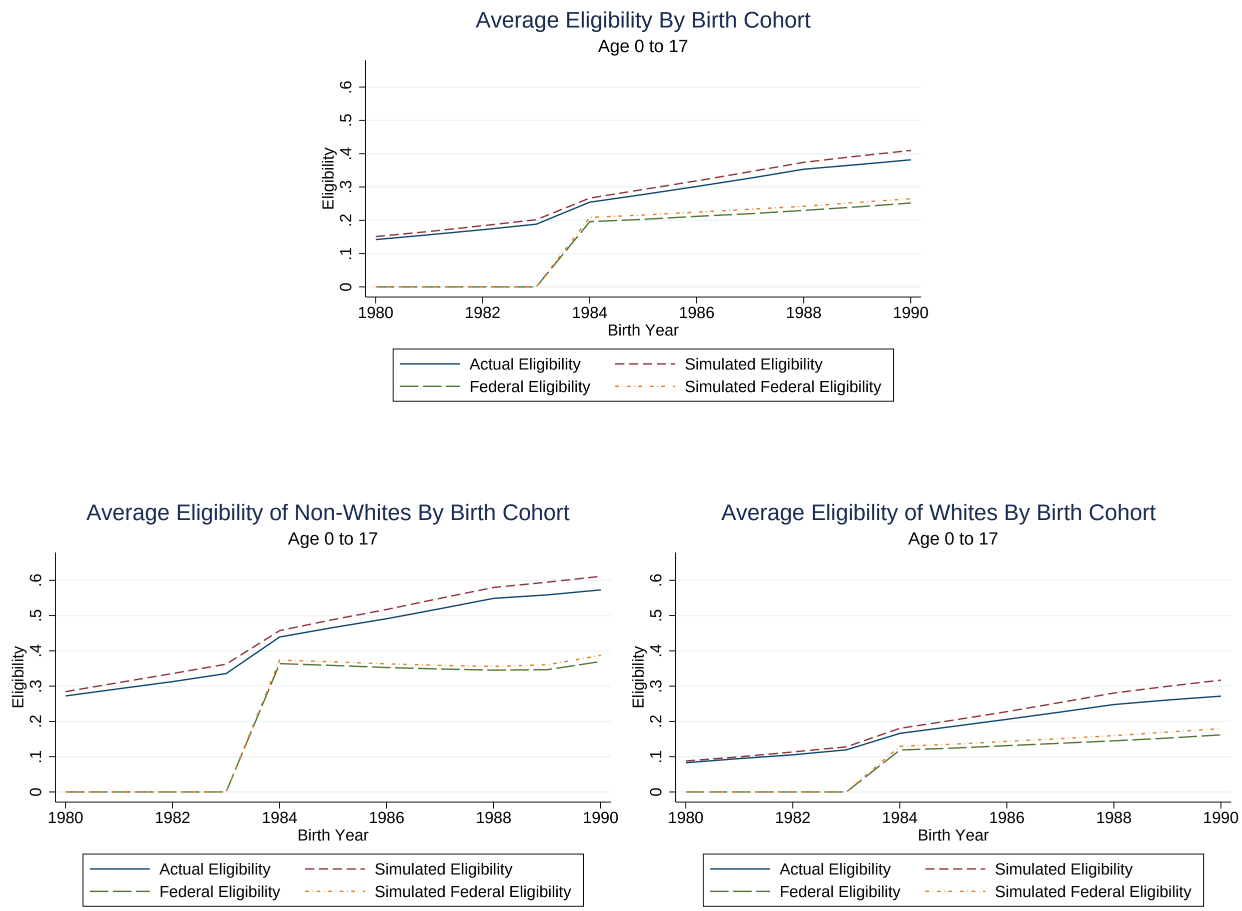
Figure 1: Medicaid Eligibility by Birth Cohort and Race/Ethnicity



Source: The figure shows average eligibility by birth cohort calculated using 1980-2004 CPS data combined with state by year Medicaid eligibility rules. Eligibility is calculated separately for whites and non-whites. 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.

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

Table 2: Summary Statistics for White and Nonwhite Analysis Sample, 2005-2012 CPS

Variable Name	All	White	Nonwhite
No High School Diploma	0.094	0.071	0.143
	(0.048)	(0.029)	(0.045)
No GED or High School Diploma	0.126	0.102	0.176
	(0.054)	(0.038)	(0.050)
Some College	0.656	0.694	0.572
	(0.086)	(0.062)	(0.071)
College Graduate (BA)	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	1.000	
	(0.466)	(0.000)	
Black	0.143		0.451
	(0.266)		(0.290)
Hispanic	0.123		0.386
	(0.230)		(0.255)
Other Race	0.052		0.163
	(0.108)		(0.135)
Married	0.273	0.306	0.202
	(0.136)	(0.138)	(0.099)
Fixed Simulated Eligibility Age 0-17	0.254	0.171	0.434
	(0.155)	(0.083)	(0.116)
Eligibility Age 0-17	0.237	0.156	0.410
	(0.152)	(0.077)	(0.127)
Federal Fixed Simulated Eligibility Age 0-17	0.115	0.074	0.201
	(0.133)	(0.073)	(0.183)
Federal Eligibility Age 0-17	0.108	0.068	0.195
	(0.130)	(0.070)	(0.178)
Observations	5494	2754	2740

Source: Author's tabulations from the 2005-2012 ACS. Means in the table are taken over state-year-age-race cell averages. The samples consist of 1980-1990 birth cohorts aged 22-29, for whom we observe Medicaid eligibility in every year in their birth state from age 0 through 17. All tabulations were done using ACS sample weights. Standard deviations are shown in parentheses. Average eligibility is calculated using 3-year moving averages.

Table 3: The Effect of Average Medicaid Eligibility During School Years on Educational Attainment

		No HS			Some College			College Plus		
Specification	1st Stage	OLS	Red Form	IV	OLS	Red Form	IV	OLS	Red Form	IV
Panel A: All Eligibility										
(1) Baseline	0.969***	0.007	-0.047**	-0.049***	-0.036	0.068**	0.070**	-0.116**	0.082**	0.085**
	(0.111)	(0.024)	(0.019)	(0.017)	(0.037)	(0.030)	(0.030)	(0.053)	(0.039)	(0.042)
(2) EITC & School	0.989***	0.012	-0.046**	-0.048***	-0.037	0.071**	0.072***	-0.125**	0.088**	0.090**
Spending	(0.092)	(0.026)	(0.020)	(0.018)	(0.039)	(0.029)	(0.028)	(0.057)	(0.042)	(0.043)
(3) EITC, School Spending,	0.989***	0.024	-0.041	-0.044*	-0.045	0.097***	0.100***	-0.149**	0.097*	0.099*
S-Y & A-Y FE	(0.119)	(0.032)	(0.026)	(0.022)	(0.048)	(0.036)	(0.032)	(0.067)	(0.059)	(0.058)
(4) Baseline +	0.974***	0.023	-0.041*	-0.045**	-0.053	0.097**	0.101**	-0.141**	0.091*	0.094*
S-Y & A-Y FE	(0.135)	(0.030)	(0.025)	(0.022)	(0.045)	(0.041)	(0.042)	(0.066)	(0.055)	(0.056)
Panel B: Federal Eligibility										
(5) Baseline	0.301***	0.007	-0.017***	-0.051***	-0.036	0.014**	0.043*	-0.116**	0.022***	0.072***
	(0.044)	(0.024)	(0.006)	(0.017)	(0.037)	(0.007)	(0.022)	(0.053)	(0.008)	(0.022)
(6) EITC & School	0.296***	0.012	-0.016***	-0.050***	-0.037	0.014*	0.043*	-0.125**	0.021***	0.071***
Spending	(0.043)	(0.026)	(0.006)	(0.018)	(0.039)	(0.007)	(0.023)	(0.057)	(0.008)	(0.024)

Source: Authors' estimation of equation (3) in the text using 22-29 year old respondents from the 2005-2012 ACS. Each cell in the table comes from a separate regression. The "OLS" columns refer to models that use a three-year moving average of actual eligibility as the independent variable, the "Red Form" columns refer to models that use fixed simulated eligibility as the independent variable, and the "IV" columns refer to models that instrument actual eligibility with fixed simulated eligibility. All estimates include controls for the percent of each state-of-birth, birth cohort and race cell who are male and married as well as an indicator for the cell being non-white or not, age fixed effects, calendar year fixed effects and state of birth fixed effects. S-Y refer to state of birth by year fixed effects and A-Y refer to age by calendar year fixed effects. Standard errors clustered at the state-of-birth level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 4: The Effect of Average Medicaid Eligibility During School Years on Educational Attainment, Separating GED and HS Diplomas, 2008-2012

		No HS Diploma		No GED or HS		Some College		College Plus	
Specification	1st Stage	OLS	IV	OLS	IV	OLS	IV	OLS	IV
(1) Baseline	0.960***	0.018	-0.056***	0.031	-0.061**	-0.047	0.076*	-0.109**	0.143***
	(0.117)	(0.025)	(0.020)	(0.027)	(0.024)	(0.038)	(0.041)	(0.054)	(0.053)
(2) EITC & School	0.979***	0.023	-0.055***	0.038	-0.060**	-0.044	0.074**	-0.121**	0.147***
Spending	(0.095)	(0.026)	(0.019)	(0.029)	(0.024)	(0.041)	(0.033)	(0.058)	(0.053)
(3) EITC, School Spending,	0.976***	0.034	-0.053**	0.053	-0.056**	-0.050	0.103***	-0.144**	0.164**
S-Y & A-Y FE	(0.120)	(0.030)	(0.022)	(0.032)	(0.028)	(0.046)	(0.036)	(0.065)	(0.067)
(4) Baseline +	0.956***	0.032	-0.053**	0.050	-0.058*	-0.059	0.106**	-0.135**	0.155**
S-Y & A-Y FE	(0.143)	(0.028)	(0.023)	(0.030)	(0.030)	(0.043)	(0.051)	(0.063)	(0.066)

Source: Authors' estimation of equation (3) in the text using 22-29 year old respondents from the 2008-2012 ACS. All models use both state and federal Medicaid eligibility variation. Each cell in the table comes from a separate regression. The "OLS" columns refer to models that use a three-year moving average of actual eligibility as the independent variable, and the "IV" columns refer to models that instrument actual eligibility with fixed simulated eligibility. All estimates include controls for the percent of each state-of-birth, birth cohort and race cell who are male and married as well as an indicator for the cell being non-white or not, age fixed effects, calendar year fixed effects and state of birth fixed effects. S-Y refer to state of birth by year fixed effects and A-Y refer to age by calendar year fixed effects. Standard errors clustered at the state-of-birth level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 5: The Effect of Average Medicaid Eligibility During School Years on Educational Attainment, Including Race Interactions

		No HS		Some College		College Plus	
Independent Variable	1st Stage	OLS	IV	OLS	IV	OLS	IV
Panel A:Baseline							
Medicaid	0.653***	0.086**	0.009	-0.216***	0.136	-0.334***	0.167
	(0.199)	(0.039)	(0.043)	(0.068)	(0.131)	(0.089)	(0.154)
Medicaid*Nonwhite	0.206**	-0.054**	-0.033	0.138***	-0.022	0.163***	-0.046
	(0.092)	(0.023)	(0.025)	(0.043)	(0.063)	(0.056)	(0.069)
Nonwhite	-0.009	0.079***	0.091***	-0.139***	-0.163***	-0.115***	-0.158***
	(0.033)	(0.010)	(0.007)	(0.015)	(0.013)	(0.019)	(0.016)
Panel B: Baseline + State-year & Age-year FE							
Medicaid	0.750***	0.044	-0.019	-0.155***	0.039	-0.247***	0.109
	(0.140)	(0.028)	(0.030)	(0.049)	(0.054)	(0.065)	(0.079)
Medicaid*Nonwhite	0.180**	-0.034*	-0.023	0.111***	0.025	0.122**	-0.018
	(0.074)	(0.018)	(0.019)	(0.034)	(0.031)	(0.046)	(0.039)
nonwhite	-0.024	0.082***	0.094***	-0.144***	-0.158***	-0.122***	-0.156***
	(0.029)	(0.009)	(0.006)	(0.014)	(0.010)	(0.017)	(0.012)

Source: Authors' estimation of equation (3) in the text using 22-29 year old respondents from the 2005-2012 ACS. Each cell in the table comes from a separate regression. The "OLS" columns refer to models that use a three-year moving average of actual eligibility as the independent variable, and the "IV" columns refer to models that instrument actual eligibility with fixed simulated eligibility. All estimates include controls for the percent of each state-of-birth, birth cohort and race cell who are male and married as well as an indicator for the cell being non-white or not, age fixed effects, calendar year fixed effects and state of birth fixed effects. Standard errors clustered at the state-of-birth level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 6: IV Estimates of The Effect of Average Medicaid Eligibility During School Years on Educational Attainment, by Age at Eligibility

No HS	Baseline	S-Y & A-Y FE
Eligibility Age 0-1	-0.006	-0.006
	(0.009)	(0.015)
Eligibility Age 2-17	-0.046**	-0.038
	(0.020)	(0.029)
Some College	**Baseline**	**S-Y & A-Y FE**
Eligibility Age 0-1	-0.017	-0.029
	(0.018)	(0.039)
Eligibility Age 2-17	0.080**	0.125**
	(0.033)	(0.063)
College Plus	**Baseline**	**S-Y & A-Y FE**
Eligibility Age 0-1	-0.049**	0.016
	(0.023)	(0.034)
Eligibility Age 2-17	0.111***	0.080
	(0.038)	(0.062)

Source: Authors' estimation of equation (3) in the text using 22-29 year old respondents from the 2005-2012 ACS. Each cell in the table comes from a separate regression, with actual eligibility in the given age range instrumented with fixed simulated eligibility in the given age range. All estimates include controls for the percent of each state-of-birth, birth cohort and race cell who are male and married as well as an indicator for the cell being non-white or not, age fixed effects, calendar year fixed effects and state of birth fixed effects. S-Y refer to state of birth by year fixed effects and A-Y refer to age by calendar year fixed effects. Standard errors clustered at the state-of-birth level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 7: Placebo Test of The Effect of Average Medicaid Eligibility During School Years on Educational Attainment: Sample born 1962-1972

		No HS		Some College		College Plus	
Specification	1st Stage	OLS	IV	OLS	IV	OLS	IV
Baseline	0.978***	-0.020	0.011	0.069	0.050	-0.094*	-0.111
	(0.165)	(0.044)	(0.065)	(0.061)	(0.090)	(0.054)	(0.078)
S-Y & A-Y FE	0.988***	-0.025	0.004	0.086	0.072	-0.080	-0.087
	(0.206)	(0.048)	(0.076)	(0.063)	(0.104)	(0.059)	(0.091)

Source: Authors' estimation as described in the text using respondents from the 2005-2012 ACS born between 1962 and 1972 for whom we assign average Medicaid eligibility of individuals age 0 through 17 in the year a person is 18. Sample size is 8,951. Each cell in the table comes from a separate regression. The "OLS" columns refer to models that use a three-year moving average of actual eligibility as the independent variable, and the "IV" columns refer to models that instrument actual eligibility with fixed simulated eligibility. All estimates include controls for the percent of each state-of-birth, birth cohort and race cell who are male and married as well as an indicator for the cell being non-white or not, age fixed effects, calendar year fixed effects and state of birth fixed effects. S-Y refer to state of birth by year fixed effects and A-Y refer to age by calendar year fixed effects. Standard errors clustered at the state-of-birth level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 8: The Effect of Average Medicaid Eligibility During School Years on Educational Attainment, Including State-of-birth Specific Linear Cohort Trends

		No HS		Some College		College Plus	
Specification	1st Stage	OLS	IV	OLS	IV	OLS	IV
(1) Baseline	1.031***	0.021	-0.040**	-0.053	0.056*	-0.148**	0.076*
	(0.095)	(0.029)	(0.020)	(0.047)	(0.033)	(0.066)	(0.046)
(2) EITC & School	1.033***	0.021	-0.039**	-0.054	0.058*	-0.149**	0.081*
Spending	(0.095)	(0.029)	(0.020)	(0.047)	(0.033)	(0.068)	(0.048)
(3) EITC, School Spending,	1.032***	0.029	-0.042*	-0.059	0.086**	-0.177**	0.091
S-Y & A-Y FE	(0.130)	(0.035)	(0.026)	(0.056)	(0.041)	(0.078)	(0.062)
(4) Baseline +	1.035***	0.030	-0.041	-0.060	0.085**	-0.176**	0.091
S-Y & A-Y FE	(0.130)	(0.034)	(0.026)	(0.056)	(0.041)	(0.077)	(0.062)

Source: Authors' estimation of equation (3) in the text using 22-29 year old respondents from the 2008-2012 ACS. Each cell in the table comes from a separate regression. The "OLS" columns refer to models that use a three-year moving average of actual eligibility as the independent variable, and the "IV" columns refer to models that instrument actual eligibility with fixed simulated eligibility. All estimates include controls for the percent of each state-of-birth, birth cohort and race cell who are male and married as well as an indicator for the cell being non-white or not, age fixed effects, calendar year fixed effects and state of birth fixed effects. S-Y refer to state of birth by year fixed effects and A-Y refer to age by calendar year fixed effects. Standard errors clustered at the state-of-birth level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 9: The Effect of Average Medicaid Eligibility During School Years on Teen Health Outcomes and Behaviors, Observed Between Ages 14-18

Dependent Variable	Mean	OLS	IV
Risky Sex Index (n=1441)	0.962	-0.053	-1.147***
	(0.499)	(0.194)	(0.425)
Ever Had Sex (n=1400)	0.481	0.198	-0.335*
	(0.188)	(0.132)	(0.195)
Body Mass Index (n=1122)	23.21	-0.062	-3.064*
	(1.206)	(0.935)	(1.637)
Overweight (n=1122)	0.258	-0.070	-0.288*
	(0.106)	(0.087)	(0.168)
Obese (n=1122)	0.086	0.022	-0.037
	(0.061)	(0.069)	(0.113)
Ever Use Marijuana (n=1441)	0.432	0.019	-0.324*
	(0.150)	(0.125)	(0.182)
Number of Days Drank Past Month (n=1439)	2.690	-2.157*	-2.037
	(1.359)	(1.274)	(2.030)
Ever Smoke Regularly (n=1436)	0.197	0.087	0.519***
	(0.123)	(0.122)	(0.168)
Any Mental Health Issue (n=1441)	0.223	-0.096	-0.240
	(0.098)	(0.101)	(0.185)
Eating Disorder (n=1440)	0.101	-0.064	-0.104
	(0.065)	(0.048)	(0.093)

Source: Authors' estimation of equation (3) in the text using 14-18 year old respondents from the 1995-2007 YRBSS. Each cell in the table comes from a separate regression. Risky sex index is a count variable that includes ever had sex, no birth control last sexual encounter (a combination of no birth control and condom use variables), ever pregnant, and used alcohol or drugs last sexual encounter. See Appendix Table A-1 for estimates of individual components of the Risky Sex Index as well as the variables from which we calculate whether an individual has a mental health issue or an eating disorder. Medicaid eligibility is calculated as average from age 0 - current age, so for age 14 it is 0-14. The "OLS" column refers to models that use a three-year moving average of actual eligibility as the independent variable, and the "IV" column refers to models that instrument actual eligibility with fixed simulated eligibility. The first stage coefficient is 0.882 (se=0.123). The estimates include controls for the percent of each state-of-residence and birth cohort who are male and nonwhite as well as calendar year by age and state of residence fixed effects. Standard errors clustered at the state-of-residence level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Online Appendix

Not for Publication

Table A-1: The Effect of Average Medicaid Eligibility During School Years on Teen Health Behaviors and Outcomes, Observed Between Ages 14-18

Dependent Variable	Mean	OLS	IV
Risky Sex Index (n=1441)	0.962	-0.053	-1.147***
	(0.499)	(0.194)	(0.425)
Ever Had Sex (n=1400)	0.481	0.198	-0.335*
	(0.188)	(0.132)	(0.195)
No Birth Control (n=1338)	0.230	-0.180	0.302
	(0.140)	(0.129)	(0.193)
Used Condom Last Encounter (n=1338)	0.633	0.164	-0.423*
	(0.157)	(0.154)	(0.238)
Ever Pregnant or Impregnate Partner (n=1057)	0.051	0.061	-0.147
	(0.060)	(0.056)	(0.125)
Used Drugs or Alcohol Last Sexual Encounter (n=1339)	0.243	-0.063	0.189
	(0.137)	(0.126)	(0.255)
Any Mental Health Issue (n=1441)	0.223	-0.096	-0.240
	(0.098)	(0.101)	(0.185)
Considered Suicide (n=1441)	0.186	-0.096	-0.238*
	(0.084)	(0.091)	(0.133)
Planned Suicide (n=1441)	0.149	-0.091	-0.312
	(0.087)	(0.102)	(0.203)
Attempted Suicide (n=1438)	0.084	-0.030	-0.171
	(0.063)	(0.065)	(0.123)
Hurt Self Attempting Suicide (n=1438)	0.026	-0.036	-0.104
	(0.037)	(0.036)	(0.068)
Number of Suicide Attempts (n=1438)	0.180	-0.048	0.073
	(0.172)	(0.164)	(0.354)
Eating Disorder (n=1440)	0.101	-0.064	-0.104
	(0.065)	(0.048)	(0.093)
Bulimic (n=1440)	0.050	-0.021	-0.076
	(0.045)	(0.033)	(0.070)
Used Diet Pills (n=1440)	0.074	-0.063	-0.060
	(0.056)	(0.040)	(0.093)

Source: Authors' estimation of equation (3) in the text using 14-18 year old respondents from the 1995-2007 YRBSS. Any mental health issue is coded as 1 if an individual reports any of the suicide ideation variables included in the table. Any eating disorder includes both bulimia and diet pill use. Eligibility is calculated as average from age 0 - current age, so for age 14 it is 0-14. The first stage coefficient is 0.882 (se=0.123). The estimates include controls for the percent of each state-of-residence and birth cohort who are male and nonwhite as well as calendar year by age and state of residence by year fixed effects. Standard errors clustered at the state-of-residence level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.