



The long-term health impacts of Medicaid and CHIP[☆]



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ABSTRACT

This paper estimates the effect of US public health insurance programs for children on health. Previous work in this area has typically focused on the relationship between current program eligibility and current health. But because health is a stock variable which reflects the cumulative influence of health inputs, it would be preferable to estimate the impact of total program eligibility during childhood on longer-term health outcomes. I provide such estimates by using longitudinal data to construct Medicaid and CHIP eligibility measures that are observed from birth through age 18 and estimating the effect of cumulative program exposure on a variety of health outcomes observed in early adulthood. To account for the endogeneity of program eligibility, I exploit variation in Medicaid and CHIP generosity across states and over time for children of different ages. I find that an additional year of public health insurance eligibility during childhood improves a summary index of adult health by .079 standard deviations, and substantially reduces health limitations, chronic conditions and asthma prevalence while improving self-rated health.

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1. Introduction

Understanding how public health insurance programs for children impact health outcomes is a question of fundamental interest to both policy makers and researchers. From a policy evaluation perspective, the two main US health insurance programs for children – Medicaid and the Children's Health Insurance Program (CHIP) – constitute the single largest component of federal expenditures on child welfare, and improving child health outcomes is one of the central goals of these programs.¹ In addition to evaluating the efficacy of Medicaid and CHIP specifically, researchers have a strong and growing interest in the nature of health determination during childhood and its role in shaping human capital trajectories through the life-course (Heckman, 2007; Currie, 2009),

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¹ Specifically, the Urban Institute (2013) calculates that Medicaid and CHIP accounted for \$75.5 billion in federal expenditures in 2012, relative to \$52.2 billion spent on EITC payments, \$36.8 billion on SNAP nutrition benefits, \$12.1 billion on TANF payments, and \$7.9 billion on Head Start.

and health insurance coverage during childhood is a potentially important aspect of these developmental processes.

A well-established literature analyzes the effect of public health insurance programs on child health outcomes, with influential contributions coming from Currie and Gruber (1996b), Kaestner et al. (2001), Dafny and Gruber (2005), and Goodman-Bacon (2016), among others, and excellent reviews provided by Howell and Kenney (2012) and Bitler and Zavodny (2014). While insightful, an important limitation of this literature is that most studies estimate the relationship between *current* program eligibility and *current* health. This approach contradicts the widely accepted theoretical view, dating to the seminal work of Grossman (1972), that health is best modeled as a stock variable which is determined by the cumulative influences of health inputs over the life-course (in conjunction with a health endowment). Ideally, empirical estimates would explicitly account for the stock nature of health by estimating the effect of cumulative program exposure on long-term health outcomes.

The present paper attempts to produce such improved estimates by using longitudinal microdata to construct measures of public health insurance eligibility over the full course of childhood, from birth through age 18. I then estimate the effect of total program exposure during childhood on a wide variety of health outcomes that are observed in early adulthood. To account for the endogeneity of public health insurance eligibility, I use an adaptation of the now standard instrumental variables approach first developed by Currie and Gruber (1996a,b) and Cutler and Gruber (1996), which

relies on eligibility variation induced by differences in program generosity across states and over time for children of different ages.

My approach and findings contribute to an emerging literature that emphasizes the importance of evaluating the long-run effects of US child welfare policies. For instance several recent papers study the long-term effects of children's Medicaid programs on adult outcomes. These include [Brown et al. \(2015\)](#), who use individual tax return data and find that greater total Medicaid eligibility in childhood increases adult wages and tax receipts while reducing EITC participation; [Cohodes et al. \(2016\)](#), who find that Medicaid eligibility positively affects adult educational outcomes; [Meyer and Wherry \(2016\)](#), who exploit a Medicaid eligibility discontinuity and find that increased eligibility between ages 8 and 14 reduced mortality among black teenagers; and [Boudreaux et al. \(2016\)](#), who use Medicaid's initial rollout in the 1960s to document large reductions in adult chronic conditions resulting from program exposure in early childhood.²

In addition to these Medicaid-specific studies, other recent work has evaluated the effects of a broader set of US social policies and highlighted the importance of both accounting for cumulative program exposure and measuring outcomes into adulthood. For instance a recent re-evaluation of the Moving to Opportunity program ([Chetty et al., 2015](#)) found that when total childhood exposure to improved neighborhoods is accounted for and outcomes are observed into young adulthood, the program had large treatment effects, whereas earlier evaluations (e.g. [Kling et al., 2007](#)) had found little or no impact. Similarly, a common finding in the literature evaluating Head Start is that short-term test score impacts are modest in size and quickly fade, but that large sustained effects are observed for wages, final educational attainment, and other adult outcomes ([Carneiro and Ginja, 2014](#); [Duncan and Magnuson, 2013](#); [Ludwig and Miller, 2007](#)).

The present study contributes to this growing literature by being among the first to evaluate the effect of total Medicaid and CHIP exposure in childhood on adult health outcomes, especially in the context of modern implementations of these public insurance programs. The paper's main finding is that cumulative access to public health insurance has large long-term health impacts, with an additional year of Medicaid or CHIP eligibility occurring in childhood leading to a .079 standard deviation improvement in a summary index of health outcomes observed in early adulthood ($P < .05$). With respect to more specific health measures, I find that an additional year of public health insurance eligibility in childhood reduces the probability of a health limitation by 1.3 percentage points ($P < .10$) and reduces the probability of suffering from asthma by 1 percentage point ($P < .10$), effects that translate to reductions of approximately 10–30% from the mean prevalence rates. I also find practically large improvements in self-rated health and chronic condition prevalence, though in most specifications these effects are not statistically significant at conventional levels.

These findings are robust to the inclusion of geographically specific linear time-trends and to controlling for a variety of potentially confounding contemporaneous state policies and economic conditions, as well as to various alternative sample restrictions and modeling choices. My data and approach also allow me to examine heterogeneity in the effect of public health insurance eligibility occurring at different ages within childhood, and I find that in most cases program eligibility in early childhood has substantially larger

effects than eligibility in middle childhood or adolescence. Finally, I discuss and present suggestive findings on the potential mechanisms by which increased public health insurance eligibility may improve health outcomes, and find evidence of increased utilization of preventative health care and improved school performance.

The remainder of the paper proceeds as follows. Section 2 describes the data; Section 3 outlines the utilized empirical strategy; Section 4 presents the main findings and discusses their interpretation; Section 5 presents additional results including treatment effect estimates by child age, analyses of the sources of identifying eligibility variation, and a variety of robustness and specification checks; Section 6 discusses potential mechanisms underlying the main health findings, and Section 7 concludes.

2. Data

Data is drawn from the 1979 National Longitudinal Survey of Youth (NLSY79) and the corresponding NLSY Child/Young-Adult sample (NLSY-CYA). The main NLSY79 survey follows a sample of 12,686 individuals who were ages 14–21 as of 1979. Participants were eligible to be interviewed annually until 1994 and biannually thereafter, with the most recent wave available at the time of writing occurring in 2012. Starting in 1986, the NLSY-CYA was initiated as a separate biannual survey following the biological children of female NLSY79 respondents.

The main NLSY79 survey instrument is very extensive, and among other items collected detailed information on household income, as well as data on family structure and state of residence, which are the key variables needed to calculate public health insurance eligibility for any children living in the household.³ The NLSY-CYA then additionally collected detailed information on a variety of health measures as respondents progressed through childhood and into young adulthood, allowing me to link Medicaid and CHIP eligibility during childhood to health outcomes in early adulthood. I next describe the utilized eligibility and health measures in detail.

2.1. Total Medicaid and CHIP eligibility in childhood

To create public health insurance eligibility histories for NLSY-CYA respondents, I first calculate total family income for all the years in which each NLSY-CYA respondent was age 18 or under and was living with their mother (co-residence with mothers occurred in over 93% of annual childhood observations). I do so by taking the sum of income reported from the following sources for both the mother herself and her resident spouse, if present: Wages, salaries, business and farm operation profits, unemployment insurance and child support payments. These income sources correspond as closely as possible to those that would typically be considered in determining public health insurance eligibility.⁴ For each survey year, I then convert these total income measures to income-to-needs ratios using a measure of family size that corresponds to the one used in determining program eligibility and the annual federal poverty levels reported in [Social Security Administration \(2013\)](#). Once income-to-needs ratios are calculated annually for each child's household, I determine public health insurance eligibility by comparing these ratios to the applicable Medicaid and CHIP eligibility thresholds.

² Related work by [Wherry et al. \(2015\)](#) finds similar results with respect to adult hospital admission rates. In addition to the discussed studies, [Currie et al. \(2008\)](#) focus primarily on the contemporaneous effects of public insurance eligibility and their interaction with income-health gradients, but also present some estimates of the effect of eligibility at ages 0–7 on self-rated health observed at ages 9–17, finding non-negligible reductions in poor self-rated health.

³ State of residence is available in a restricted access NLSY-geocode supplement. See <http://www.bls.gov/nls/nlsgeo.htm> for application procedures.

⁴ Children with a resident parent receiving income from military service are excluded from eligibility calculations since they are typically enrolled in military sponsored health insurance programs.

Due to expansions of US public health insurance programs over time, the nature of the relevant thresholds vary by year, and can be divided into three broad periods.

In the first broad period, from 1978 to 1985, Medicaid was the only large public health insurance program available to children and eligibility was closely tied to participation in the AFDC cash welfare program. Given this, for 1978–1985 I consider a child to be eligible for public health insurance if their family's income-to-needs ratio was below the relevant state-year AFDC threshold (as compiled by Gruber and Yelowitz, 1999) and their mother was not currently residing with a spouse, since in this period most states restricted AFDC eligibility to single mothers. Eligibility for AFDC (and therefore Medicaid) was very restrictive in this period, typically requiring an income substantially below the federal poverty line.

In the second period, from 1986 to 1996, a series of federal policy changes decoupled Medicaid and AFDC eligibility and extended Medicaid coverage to many children from households with incomes above AFDC thresholds, and in many cases above 100% of the federal poverty line, with the specific thresholds varying across states and by child age. As such, for 1986–1996 I consider a child to be eligible for public health insurance if their family's income-to-needs ratio was below the relevant state-year-age Medicaid threshold.⁵

Finally, following the creation of the State Children's Health Insurance Program (CHIP) in 1997, eligibility was further expanded as many states created new health insurance programs for children, while others used CHIP funding to expand existent Medicaid programs. From 1997 onward I consider a child to be eligible for public health insurance if their family's income-to-needs ratio was below either the Medicaid or CHIP thresholds for the applicable state, year, and child age.⁶

To form my analysis sample I retain all children whose public insurance eligibility I observe at least 5 times total and for whom I also observe eligibility at least once in early childhood (ages 0–5), middle-childhood (ages 6–11) and adolescence (ages 12 through 18). Using this sample, I form a single public health insurance eligibility variable by calculating the proportion of all valid observations occurring from ages 0 through 18 in which each respondent was eligible for Medicaid or CHIP. To facilitate a clear interpretation of regression coefficients, I multiply this proportion by 19, so that its units are total years of eligibility during childhood. I refer to this measure as actual total eligibility.⁷

Restricting my sample to children with eligibility observed at least 5 times total and at varying ages is intended to ensure that the actual total eligibility variable substantively measures eligibility over the course of childhood. For instance, without such a restriction an individual who was observed in a single wave and was eligible at that point would be coded as having been eligible for public health insurance for their entire childhood, even though little is

⁵ Due to phase-in provisions in some of the Medicaid expansions over this period eligibility thresholds often additionally depend on whether a child was born after 1983, and where applicable I determine eligibility using separate thresholds for pre and post 1983 cohorts.

⁶ I assign year-based eligibility measures to children using the age that each child was for the majority of the relevant calendar year. As an example, consider a child who turned 10 during calendar year 2011. For such a child, I use income data from calendar year 2011 to assign age 10 eligibility if the child turned 10 in the first half of 2011, and was therefore age 10 for most of 2011. However, if the child turned 10 in the latter half of 2011, and was therefore age 9 for most of 2011, I use 2011 income data to assign their age 9 eligibility.

⁷ One potential issue with calculating total eligibility is that the NLSY79 went to a biannual survey design in 1994, so that eligibility is observed every year through 1993 but every other year from 1994 onward. Below I demonstrate that my main findings are robust to imputing eligibility in non-survey years after 1994 and to only using even-year eligibility observations throughout the NLSY79 sampling period.

known about their true eligibility. However, these restrictions may also affect the composition and representativeness of the working sample by disproportionately excluding children with the least stable living arrangements. To investigate this possibility, Appendix A provides a detailed comparison of the full NLSY-CYA sample and the subset of respondents used in the main analysis. While differences in the characteristics of these two samples do exist, the overall magnitude of these differences are very small, suggesting that using the subsample for which total childhood eligibility is reliably observed does not greatly compromise the representativeness of the results.

As discussed below, take-up for Medicaid and CHIP is typically incomplete, such that not all children who are eligible for a public health insurance program actually enroll. Information on actual Medicaid and CHIP enrollments, rather than program eligibility alone, would be of clear value, and the NLSY-CYA did include questions asking whether respondents were covered by "Medicaid or another public assistance health care program." However, this information was not collected until the launch of the NLSY-CYA in 1986, so that only eligibility, and not actual enrollment, is observed for the 1979–1985 survey waves. The accuracy of self-reported enrollment data is also questionable, since Medicaid and CHIP programs typically use state-specific names and often operate managed care programs through private contractors, leading some recipients to believe they are covered by private plans. For these reasons, I primarily focus on public health insurance eligibility rather than enrollment, but I do present and discuss results using the available enrollment measures as well.

2.2. Health outcomes

Since health has many important dimensions, I evaluate the effects of public health insurance eligibility on four separate health measures that are available in the NLSY-CYA. First are two global health measures: Whether each individual reports being limited in their ability to work or attend school for health reasons and whether they self-rated their health as "poor" or "fair" rather than "good" "very good" or "excellent."⁸ The limitation measure has strong economic relevance given that many of the economic implications of poor health derive from the fact that it limits human capital acquisition and labor market performance, while self-rated health is a well established predictor of morbidity and mortality (Idler and Benyamini, 1997). I also utilize two available measures of chronic conditions. First is whether each respondent reported currently suffering from "any condition that requires frequent medical attention, the regular use of medication, or the use of special equipment" while second is whether each respondent reported having had an asthma attack in the past year, asthma being the most common specific health condition in the NLSY-CYA sample.⁹

I measure each these four health outcomes using the first valid observation occurring after respondents had turned 18 and before they had turned 21. Approximately 90% of NLSY-CYA participants were age 18 or older when the most recent survey wave was fielded in 2012, but many respondents were still relatively young, so that using health observations at older ages is not yet possible without substantial sample size reductions. I exclude individuals with no valid health observations from ages 18 through 20 and control for the exact age at health observation in all specifications reported below.

While it is generally desirable to observe multiple health measures, the use of many dependent variables also presents some

⁸ Results are very similar if a continuous self-rated health measure is utilized instead of this binary recoding.

⁹ The NLSY-CYA collected data on dozens of specific chronic conditions, but only asthma had a sufficiently high prevalence rate to produce precise estimates.

estimation related issues. One such issue is multiple inference. Since I utilize four separate health outcomes and estimate models for various sub-samples and specifications, I often test dozens of hypothesis and this increases the risk of false rejection (Type-1 error). Additionally, many of the outcomes are closely related, with for instance asthma substantially reducing self-rated health and having a chronic condition increasing the likelihood of a health limitation. Such correlations across measures make it difficult to ascertain how much new information is contained in results for each individual outcome. A final issue is measurement error. All of the utilized outcomes can reasonably be viewed as components of a single underlying health state, but each specific outcome is likely measured with error, which can destabilize the corresponding estimates.

To address these issues I follow O'Brien (1984), Carneiro and Jinja (2014) and others and construct a composite index of the four described health measures. Specifically, I first standardize each measure to have a mean of zero and a standard deviation of one and equalize signs across outcomes so that positive values correspond to better health. I then take the weighted average of these standardized measures using weights that are equal to the inverse of the sample covariance matrix, which accounts for dependence across outcomes. Finally I restandardize this weighted mean so that corresponding regression coefficients can be interpreted in standard deviation units. This index has the desirable property that adding additional dimensions does not increase the risk of Type-1 error, and also accounts for correlations across the outcomes and reduces measurement error. For these reasons the index is my preferred health outcome measure, but for completeness I present results for each component measure as well.¹⁰

2.3. Descriptive statistics

After applying the eligibility related sample restrictions described above and excluding cases with missing information on health outcomes or basic demographic characteristics, I am left with a working sample of 5465. Table 1 and Fig. 1A and B report descriptive statistics for the primary variables used in the analysis.

The first two rows of Table 1 show that on average respondents were eligible for Medicaid or CHIP for approximately 6 years during childhood, and that their eligibility was observed a total of just over 11 times. Fig. 1A displays the histogram of the actual total eligibility variable. The figure indicates that approximately 36% of children in the analysis sample were not eligible for public health insurance at any point during childhood, while approximately 9% were eligible at every observation. The remaining 55% of respondents were Medicaid or CHIP eligible for some – but not all – of their childhood, with total years of eligibility distributed in a relatively uniform manner between the extremes of never-eligible and always-eligible. It is noteworthy that such a large portion of children in the sample moved in and out of eligibility during childhood, as studies using cross-sectional data would be forced to code such children as simply eligible or ineligible, depending on their status at the time of observation. Fig. 1A suggests that such binary eligibility measures partially misclassify the eligibility of a majority of children, and underscores the importance of constructing longitudinal eligibility measures.

Fig. 1B displays mean eligibility levels by age in the working sample. Eligibility rates are highest when children in the sample were older, increasing from approximately 27% among very young children to approximately 38% among adolescents. This increase

Table 1
Sample characteristics.

	Mean	Standard deviation	Minimum	Maximum
Years of eligibility	5.78	6.68	0.0	19.0
Number of times eligibility observed	11.12	1.89	5.0	15.0
Health limitation	0.112	0.316	0.0	1.0
Poor or fair self-rated health	0.087	0.281	0.0	1.0
Any chronic condition	0.058	0.233	0.0	1.0
Asthma attack in past year	0.035	0.185	0.0	1.0
Birth year	1986	4.82	1974	1994
White	0.71	0.45	0.0	1.0
Black	0.20	0.40	0.0	1.0
Hispanic	0.09	0.28	0.0	1.0
Female	0.50	0.50	0.0	1.0
Birth order	1.87	1.01	1.0	8.0
Mother's age at birth	25.2	4.7	14.0	36.0
Mother's highest grade completed	13.4	2.5	0.0	20.0
Income-to-needs ratio	2.95	3.43	0.0	65.1
Observations			5465	

Data from NLSY79 and NLSY-CYA samples. Custom child-level NLSY sampling weights are applied.

occurs despite the fact that most states use lower income eligibility thresholds for younger children, and is due to the large scale expansions in program generosity occurring over the sample period. One implication of this age pattern is that the identifying variation in eligibility will be driven by program expansions occurring in different periods depending on child age. As a result, when treatment effects are estimated for children of varying ages, the identifying policy variation and counterfactual environment also vary, which impacts the interpretation of age-specific estimates, an issue I discuss in detail in Section 5.

Means for the studied health variables are shown in rows 3–6 of Table 1 indicate that while these conditions are relatively rare they do have non-negligible prevalence rates: 11.2% of young adults in the sample report a health related limitation, 8.7% have poor or fair self-rated health, 5.8% had at least one chronic health condition and 3.5% had suffered an asthma attack in the past year. The remaining rows of Table 1 report descriptive statistics for demographic and SES related characteristics. The average year of birth for children in the sample is 1986, and just 6% of respondents were born before 1978, the first year that eligibility can be calculated. The sample is predominantly white (71%), with blacks and Hispanics making up approximately 20% and 9% of the working sample, respectively. On average, the mothers of responding children were 25.2 years old at the time of birth, completed 13.4 years of schooling, and had an average household income that was 2.95 times the federal poverty level. The large reported standard deviations and ranges of these characteristics indicate substantial socioeconomic diversity in the working sample.

3. Empirical strategy

The main regression specification used to estimate the effect of public health insurance eligibility during childhood on the discussed health outcomes in early adulthood is as follows:

$$Health_i = \alpha + \beta Actual\ Total\ Eligibility_i + \delta_s + \tau Y_i + \rho A_i + \gamma X_i + \varepsilon_i \quad (1)$$

¹⁰ For respondents who are missing one of the four utilized health outcomes, I calculate the composite index using the three remaining components. Results are very similar if instead the index measure is set to missing for these individuals.

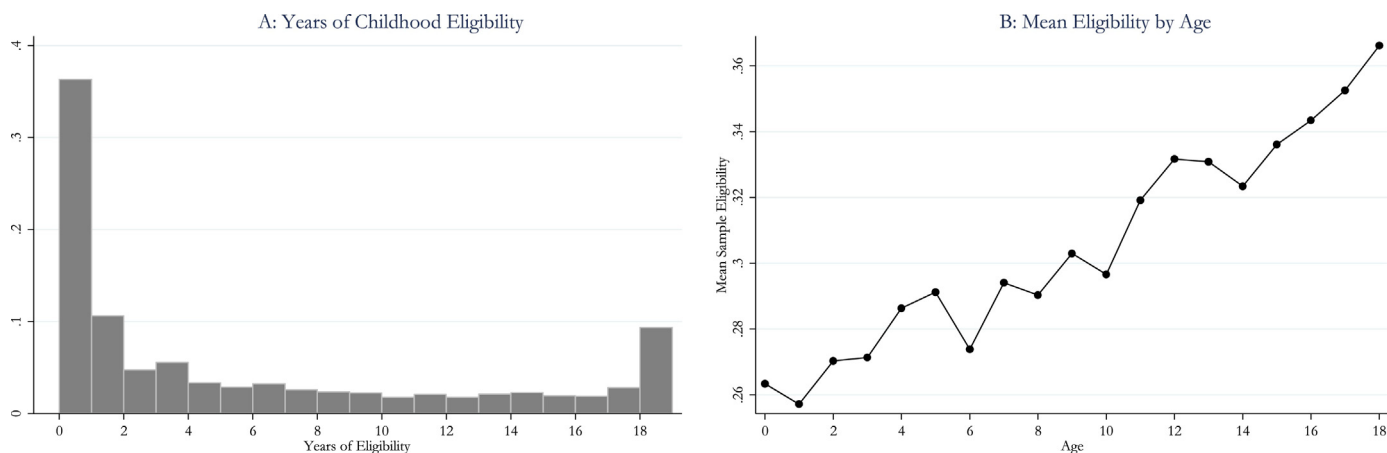


Fig. 1. Characteristics of childhood eligibility. (A) Years of childhood eligibility. (B) Mean eligibility by age. (A) The histogram of total childhood public health insurance eligibility using the NLSY sample described in Section 2 of the text. Fig. 2A displays mean eligibility rates in the same sample at each age from 0 through 18. Custom child-level NLSY sampling weights are applied.

where $Health_i$ denotes one of the health measures discussed above for individual i , $Actual\ Total\ Eligibility_i$ is the number of years individual i was eligible for public health insurance during childhood, δ_s is a state fixed-effect, and Y_i and A_i are sets of indicators for whether eligibility was observed at each possible age from 0 through 18 and in each possible calendar year between 1978 and 2011.¹¹ Regarding age the year effects, I note that in principle a birth cohort fixed-effect would account for both the ages and years in which eligibility was observed, but because not all respondents have valid data for each survey wave, two individuals from the same cohort are in practice often observed at different ages and in different calendar years, and using separate sets of indicators for each age and year of observation flexibly accounts for this. Finally, X_i is a vector of individual level controls, which in my baseline models includes each child's gender, race, and birth order, their mother's highest grade completed and age at the time of their birth, and indicators for the total number of eligibility observations and the exact age at which health outcomes were observed. Results using a more parsimonious set of individual level controls are reported in the robustness section below. The primary coefficient of interest in this specification is β , which estimates the change in a given health outcomes that is associated with one additional year of public health insurance eligibility during childhood.

To make the results of estimating Eq. (1) as representative as possible, throughout the analysis I apply custom NLSY sampling weights that help account for oversampling, clustering, and other features of the NLSY sampling design. In Section 5, I demonstrate that the main findings are similar if sampling weights are not applied. All standard errors are clustered at the state level, which allows for non-independence of the error terms for observations from the same state.

To serve as a basis for comparison, I begin by estimating Eq. (1) via OLS and the results are reported in Panel A of Table 2. These naive OLS estimates indicate that public health insurance eligibility is associated with a practically large and statistically significant deterioration in most of the health measures under study. For instance the OLS results from Column 1 indicate that an additional year of public health insurance eligibility during childhood is associated with a statistically significant 0.12 standard deviation decline in the composite measure of health described above, with

qualitatively similar associations for health limitations and poor self-rated health.

These simple OLS estimates are likely to be biased for at least three reasons. One issue is omitted variable bias: Even with the included fixed-effects and individual level controls, families with children who are eligible for public health insurance may differ from families with ineligible children in ways relevant to children's health. For instance eligible children may live in more disadvantaged neighborhoods with fewer primary care physicians or greater levels of pollution, or there may be state-specific economic shocks that increase both eligibility and health problems, among other possibilities. A second potential source of bias is reverse causality: An unhealthy child may reduce household income via reductions in parental labor supply or other channels, and these income reductions may cause the child to become eligible for public health insurance.¹² Finally, the utilized eligibility variable is likely subject to measurement error since it is constructed from self-reported income and does not account for the full array of eligibility requirements.¹³

To address these issues I use an adaptation of the instrumental variables strategy first developed in the seminal work of Currie and Gruber (1996a,b) and Cutler and Gruber (1996) and which has since been used in many influential studies on the effects of public health insurance programs. This approach instruments for children's actual public health insurance eligibility status using "simulated eligibility", which is an index of public insurance program generosity specific to state, calendar year, and child age.

Following previous studies, I construct a simulated eligibility instrument by first drawing a national sample of children from the CPS and calculating the fraction of children in this sample of each age that would have been eligible for public health insurance if they had lived in each individual state and during each year of the study period.¹⁴ Rather than use the full CPS sample, I calculate simulated eligibility for each state-year-age cell using a subsample of

¹² Reverse causality can also arise because individuals are typically allowed to retroactively enroll in public health insurance programs after a major medical event, leading to disproportionate enrollments among the sick, but this is less relevant in the present case because the independent variable of interest is eligibility not participation. I discuss issues related to take-up and crowd-out in Section 4.

¹³ For instance, in some state-years eligibility requirements included waiting periods or and face-to-face interviews. See Wolfe and Scrivner (2005).

¹⁴ Separate eligibility thresholds for children belonging to a post-1983 birth cohort are also used in this calculation. I additionally follow Ham and Shore-Sheppard (2005) by iteratively excluding CPS children from the state for which simulated eligibility is calculated.

¹¹ State fixed-effects are defined using each individual's modal state of residence during childhood.

Table 2
The effect of total childhood Medicaid/CHIP eligibility on adult health outcomes.

	(1) Composite index	(2) Health limitation	(3) Poor self-rated health	(4) Any chronic condition	(5) Asthma
Panel A: OLS	−0.012*** (0.004)	0.003*** (0.001)	0.004*** (0.001)	−0.000 (0.001)	0.001 (0.001)
Panel B: Reduced Form	0.073 [†] (0.037)	−0.012 (0.008)	−0.010 (0.008)	−0.009 (0.007)	−0.009* (0.005)
Panel C: First-stage	0.922*** (.126)	0.922*** (.126)	0.922*** (.126)	.936*** (.141)	0.922*** (.126)
[F-Statistic]	[52.99]	[52.99]	[52.99]	[43.05]	[52.99]
Panel D: IV	0.079* (0.035)	−0.013* (0.007)	−0.010 (0.008)	−0.010 (0.006)	−0.010* (0.005)
Observations	5465	5465	5465	5224	5465

Column headings indicate the dependent variable for each model. The reported coefficients are for a variable measuring total years of public health insurance eligibility occurring from ages 0 through 18, except for the First-Stage results in Panel C, which regress total eligibility onto simulated eligibility using the estimation sample for the listed outcome. Bracketed statistics show robust first-stage F-Statistics for the excluded simulated eligibility instrument. All models include state fixed-effects and complete sets of indicators for the ages and calendar years when each respondent's eligibility was observed, each child's gender, race, and birth order, their mother's highest grade completed and age at the time of their birth, and indicators for the total number of eligibility observations and the exact age at which health outcomes were observed. All standard errors are clustered at the state level using the modal state of residence during childhood. Custom child-level NLSY sampling weights are applied.

[†] Statistical significance at the 10%.

** Statistical significance at the 5%.

*** Statistical significance at the 1%.

CPS children whose mothers were members of the same cohorts as the mothers in the NLSY (specifically 1957–1964) and whose mothers were of similar ages as the mothers in the NLSY at the time of their births.¹⁵ Calculating simulated eligibility with a CPS sample that mirrors the NLSY leads to a substantially stronger first-stage than when using the full CPS sample, and improves the precision of the corresponding IV estimates, though the point-estimates of the treatment effects are very similar if the full CPS sample is used instead.

The use of a national CPS sample in calculating simulated eligibility (rather than state-level samples) is important because it isolates the effect of a state's program generosity from the characteristics of a state's residents. For instance Alabama has relatively restrictive public health insurance programs for children, but because it has a large low-income population, a relatively high proportion of children actually living in Alabama are still eligible for public insurance. Calculating simulated eligibility with a national sample removes the effect of state-specific population characteristics, and isolates the generosity of public health insurance programs in each state-year-age cell.

After calculating simulated eligibility in each state-year-age cell, I construct a simulated eligibility instrument that mirrors the actual total eligibility variable described above by taking the mean of the relevant state-year-age simulated eligibility values for each NLSY-CYA respondent over the course of their childhood then multiplying this value by 19. This variable, which I refer to as simulated total eligibility, can be viewed as an index of the public insurance program generosity that each NLSY-CYA respondent was subject to on average over the course of their childhood, given their state(s) of residence and birth cohort. I then use simulated total eligibility as an instrument for actual total eligibility in Eq. (1).

The exclusion restriction required for this instrument to accurately identify the causal effect of public health insurance eligibility is that simulated eligibility affects child health only via its impact on public health insurance eligibility. Given the included sets of state

fixed-effects and year and age indicators, this exclusion restriction is very similar to the identifying assumption of a difference-in-difference specification, specifically that state-to-state variation in the years of Medicaid and CHIP expansions for children in particular age groups are independent of children's health outcomes, except through increased public health insurance eligibility.¹⁶ This assumption seems plausible given that most changes in public health insurance program generosity were in response to new federal mandates and subsidies rather than reflecting policies initiated at the state level, and comparable assumptions have been invoked in the large existing literatures discussed above.

Fig. 2A and B illustrate the utilized variation in public health insurance generosity across time and geography. Fig. 2A displays the change in the Medicaid/CHIP eligibility threshold in each state over the study period (i.e. the difference between the 2011 threshold and the 1978 threshold), and indicates large increases in generosity across all states over the study period, with the average state increasing the income-to-needs eligibility threshold by 1.89, or 189% of the federal poverty level.¹⁷ Fig. 2A also indicates substantial heterogeneity in program generosity changes across states, with the increase in eligibility thresholds ranging from 1.05 (North Dakota) to 3.36 (Massachusetts), but no clear geographic pattern is apparent. Fig. 2B displays trends in eligibility levels over the study period disaggregated by Census region. The figure indicates that all regions substantially expanded eligibility over the study period, with the largest expansions occurring in the Northeastern region. Vertical lines in Fig. 2B mark the decoupling of Medicaid and AFDC after 1985 and the introduction of CHIP after 1997, policy changes that drove much of the utilized eligibility variation, and substantial discrete increases in eligibility as a result of these policy changes are apparent in all four regions.

While the simulated eligibility approach's main identifying assumption seems generally plausible, a potentially important violation is legislative endogeneity. For instance, IV estimates would

¹⁵ Specifically, in the full March CPS samples used to calculate simulated eligibility, the mean maternal age at birth was 24.60 years, while for children in the NLSY-CYA sample used in my main regression specifications the average maternal age at birth was 24.18 years. To account for these differences, I drew a subsample of CPS children by using propensity score matching on maternal age at birth and selecting the 10 nearest neighbors from the CPS for each NLSY-CYA observation. The average maternal age at birth in this CPS subsample was 24.15 years, very similar to the mean in the working NLSY-CYA sample.

¹⁶ It should be noted that the stated exclusion restriction does not invoke any specific mechanism by which Medicaid/CHIP eligibility may improve child health. For instance eligibility may improve child health through increased utilization of care, improvements in household finances, or changes in maternal stress, among other possibilities, but as long as these mechanisms are a direct result of public health insurance eligibility the exclusion restriction remains valid. Potential mechanisms are discussed in greater detail in Section 6.

¹⁷ Fig. 2A and B use the average eligibility threshold across all child age groups and birth cohorts for each state-year.

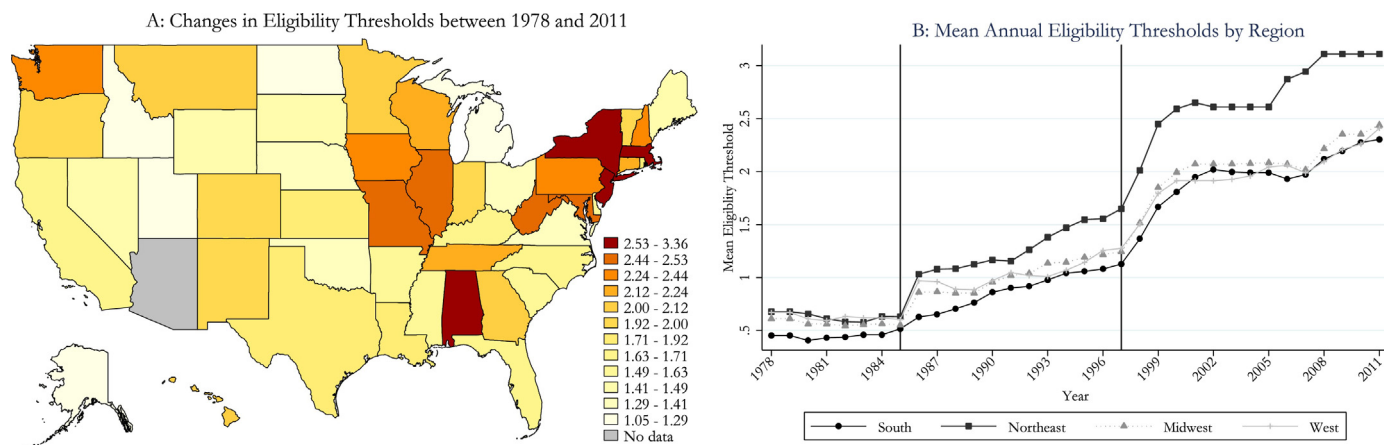


Fig. 2. Program generosity across geography and time. (A) Changes in eligibility thresholds between 1978 and 2011. (B) Mean annual eligibility thresholds by region. (A) The difference between the minimum Medicaid or CHIP eligibility threshold in 2011 and the minimum Medicaid threshold in 1978 for each state except Arizona, which did not have a Medicaid program in 1978. (B) The average minimum Medicaid or CHIP threshold in each region over the same period. Vertical lines in 2B indicate the decoupling of Medicaid and cash welfare programs after 1985 and the introduction of CHIP after 1997. Both figures use the average eligibility threshold across all child age groups and birth cohorts to calculate the threshold for each state-year or region-year.

be biased if states expand their Medicaid programs in response to state-level trends in child health outcomes. While legislative endogeneity is a valid concern, [Brown et al. \(2015\)](#) point out that its potential impact is mitigated when eligibility is measured throughout childhood rather than at a single point in time, since an endogenous policy would typically only affect eligibility for a portion of childhood, with eligibility variation from other periods remaining a valid source of identification, and [Baughman and Milyo \(2008\)](#) investigate the determinants of public health insurance program generosity directly and find little evidence of endogeneity. Additionally, I demonstrate below that my main findings are generally robust to adding geographically specific linear trends, suggesting that they are not driven by differential child health trends in states adopting different policies.

A related form of legislative endogeneity could occur if states simultaneously enacted both public health insurance expansions and other policies that affected child health, or if local economic conditions impacted both Medicaid/CHIP policy and child health. The most concerning policies that could confound the estimated effects of public health insurance programs are changes in AFDC/TANF program characteristics occurring during the welfare reforms of the mid-1990s and state earned income tax credit (EITC) policies, since these are targeted at low income families with children, varied substantially over the study period, and increase net household income, which has well established health effects for children ([Currie, 2009](#); [Case et al., 2002](#); [Ettner, 1996](#)). To address this concern, I assemble state level data on these policies, as well as local unemployment rates, and show in Section 5 that my main results are generally robust to controlling for exposure to these other policies and to local economic conditions.

4. Main findings

The paper's primary findings are presented in the remaining panels of [Table 2](#). Panel B reports reduced-form results that regress health outcomes directly onto simulated total eligibility and the control variables described above. In strong contrast to the OLS results, all five of the coefficients indicate that individuals exposed to more generous public health insurance policy environments over the course of their childhoods experience improved health in young adulthood. Panel C of [Table 2](#) reports first-stage results that regress actual eligibility onto simulated eligibility and the vector of controls, and show that the first-stage is generally strong, with

F-Statistics (for the excluded instrument) of over 50 and highly statistically significant coefficients of .922 on simulated eligibility.¹⁸

Panel D of [Table 2](#) reports the main IV results, which are simply the ratio of the reported reduced-form and first-stage estimates. The IV estimate for the composite health measure indicates that an additional year of public health insurance eligibility over the course of childhood results in a statistically significant .079 standard deviation improvement in health in young adulthood. The models in Columns 2–5 estimate that each additional year of childhood eligibility reduces the probability of a health limitation by 1.3 percentage points, and decreases the probability of poor self-rated health, of having any chronic health condition, and of having had an asthma attack in the past year by 1 percentage point. The estimates for health limitations and asthma are statistically significant at conventional levels, while the estimated effects for self-rated health and any chronic condition are not.

The effect sizes from the IV models in [Table 2](#) are fairly large relative to the sample means of the outcomes reported in [Table 1](#), typically translating to improvements of 10–30%. One consideration when evaluating these effect sizes is that below I demonstrate that the utilized instrument mostly impacts total eligibility at the lower end of the eligibility distribution, for instance by causing children to have two years of eligibility rather than one year. If marginal treatment effects are declining in total eligibility, then the estimated effects of an additional year of eligibility reported in [Table 2](#) will exceed the effects of an additional year of eligibility at a higher point in the eligibility distribution, and this issue is discussed in greater detail in Section 5. I also note that the magnitudes are broadly in line with the limited number of previous findings on the long-term effects of total childhood health insurance eligibility on other adult outcomes. For instance [Cohodes et al. \(2016\)](#) find that a 10% increase in insurance eligibility from birth through age 17 – equivalent to slightly under 2 additional years of coverage – reduces the probability of failing to complete high school by 4.9 percentage points (52%) and increases the probability of graduating from college by 8.5 percentage points (32%), and

¹⁸ The first-stage coefficient is less than one because at the margin not all changes in program generosity will impact the eligibility of a given child. For instance, if a state increases the income threshold of their Medicaid program from 100% FPL to 200% FPL, the simulated eligibility measure will increase for all children in the corresponding state-year cell, but actual eligibility will only change for children from families with incomes between 100% and 200% FPL.

Boudreaux et al. (2016) use Medicaid's introduction to show that having a Medicaid program in a child's state of residence each year from ages 0–5 decreases an index of chronic conditions in adulthood by .35 standard deviations (relative to never having a program present), an effect of .07 standard deviations per year of exposure. Given these considerations and prior findings, I believe it is most accurate to describe the magnitudes of the estimates in Table 2 as large but not implausible.

Another consideration in interpreting the IV models from Table 2 is the role of incomplete take-up. Because I estimate the effects of public health insurance eligibility, rather than actual program enrollment, the reported IV estimates are intent-to-treat (ITT) effects rather than treatment-effects-on-the-treated (ToT). Since take-up for public health insurance is typically incomplete, the magnitude of ToT effects are unambiguously larger than the ITT effects, but it would still be valuable to estimate ToT effects directly. As noted in Section 2, information on actual public health insurance enrollment was only collected after the NLSY-CYA was launched as a free-standing survey in 1986, when many of the children in the sample were relatively old, and substantial misreporting of actual enrollments is likely as well. While these data issues lead me to prefer eligibility-based estimates, Table 3 presents results from specifications similar to those reported in Table 2 but that use total self-reported public health insurance enrollment in place of the total eligibility measure. As with the total eligibility measure, total enrollment is measured by calculating the proportion of all valid observations occurring from ages 0 through 18 during which respondents were enrolled in a public health insurance program, then multiplying this proportion by 19.¹⁹

Panel A of Table 3 reports first-stage estimates that regress total childhood enrollment onto the simulated eligibility instrument.²⁰ The first-stage coefficients of .657 are weaker than the corresponding eligibility first-stage from Table 2 which is expected given that the simulated eligibility instrument impacts eligibility more directly than enrollment, but the effect is still substantively and statistically significant, and the first-stage *F*-Statistic falls to around 18, approximately one third as large as those for eligibility from Table 2 which will reduce the precision and reliability of the corresponding IV estimates.

Panel B of Table 3 reports IV estimates of the effect of public health insurance enrollment on health outcomes. The estimate from Column 1 of Panel B indicates that an additional year of Medicaid/CHIP enrollment during childhood improves health outcomes in young adulthood by .112 standard deviations, although the standard error of this estimate is relatively large and it only achieves statistical significance at the 10% level. This ToT estimate is 41% larger than its ITT counterpart from Table 2 which seems reasonable given what is known about Medicaid/CHIP take-up rates. Results for the other outcomes similarly estimate ToT effects that are substantially larger than the analogous ITT effects.

In addition to incomplete take-up, another issue when interpreting the results is the role of crowd-out. Even among the population of children who responded to Medicaid and CHIP expansions by enrolling in a program, it is highly plausible that some would have been covered by a private plan in the absence of the expansions, a phenomenon commonly referred to as crowd-out. An extensive literature on crowd-out has produced mixed findings, with estimates ranging from close to zero to over .5, which would indicate that more than half of new public health insurance enrollees from a given expansion were dropping or not taking-up

available private insurance (see Shore-Sheppard, 2008; Gruber and Simon, 2008 for careful discussions of the crowd-out literature).

In the presence of crowd-out, the effects reported in Tables 2 and 3 represent the weighted average of treatment effects among those who the expansions caused to transition from being uninsured to publicly insured, and those who transitioned from being privately insured to publicly insured. These two effects are unlikely to be equal, with the effect at the margin between being publicly insured and uninsured presumably greater than the effect at the margin between being publicly and privately insured. Indeed given the fact that not all providers accept public health insurance, the effect at the latter margin could very well be negative, though the effects of smaller provider networks for public insurance plans may be offset by reduced cost sharing.

Because policy makers can directly influence eligibility, but at best can only indirectly impact take-up and crowd-out rates, the results in Table 2 are arguably the most policy relevant. However, the above discussions of take-up and crowd-out highlight that eligibility based estimates likely mask substantial treatment effect heterogeneity, with the largest effects concentrated among children who actually enrolled in the program and would have otherwise been uninsured.

5. Additional results

5.1. Treatment effects by age

Because I observe public health insurance eligibility at an individual level throughout childhood, I am able to conduct some limited tests for heterogeneity in the effect of eligibility at different ages on health. Such heterogeneity is potentially important given that many researchers believe early childhood represents an especially sensitive period for health determination, and I am not aware of any previous work that assesses whether the effect of public health insurance on health varies across different periods of childhood.²¹ Table 4 reports results from IV models similar to those in Table 2 but that separately regress health outcomes onto total public health insurance eligibility occurring from ages 0 to 5, 6 to 11, and 12 through 18. The instruments in these models are simulated eligibility over the corresponding age ranges, and because eligibility occurring at different ages is strongly collinear I follow Currie et al. (2008) and estimate the effect of eligibility at each age range in a separate regression, though entering all three simultaneously produces similar but less precise estimates.

For all five health measures reported in Table 4 eligibility from 0 to 5 is the largest of the three coefficients and effects are statistically significant at conventional levels. Differences in effect sizes by age are particularly pronounced for the composite health measure, where an additional year of eligibility occurring from ages 0 to 5 is estimated to improve health by .441 standard deviations, while the estimated effects of eligibility occurring from ages 6 to 11 and 12 through 18 are much smaller. Large differences are also present for health limitations and asthma, with smaller but still non-negligible differences for poor self-rated health and chronic conditions.

The most intuitive interpretation of these differences in that features of the health production function or the nature of health services typically consumed early in life cause public health insurance eligibility occurring in early childhood to have larger effects on health than eligibility later in childhood. For instance health investments in early childhood may increase the efficacy of future

¹⁹ Controls for the ages and years that actual enrollment were observed replace their eligibility-based counterparts in Table 3.

²⁰ Reduced-form results are not impacted by using enrollment in place of eligibility and are therefore not reported.

²¹ A partial exception is Currie et al. (2008), but the authors do not directly observe eligibility at different ages, and therefore must assume that children have not moved and can only estimate reduced-form models for varying ages.

Table 3
The effect of childhood Medicaid/CHIP enrollment on adult health outcomes.

	(1) Composite index	(2) Health limitation	(3) Poor self-rated health	(4) Any chronic condition	(5) Asthma
Panel A: First-stage	.657*** (.152)	.657*** (.152)	.657*** (.152)	0.693*** (.150)	.657*** (.152)
[F-Statistic]	[18.26]	[18.26]	[18.26]	[20.83]	[18.26]
Panel B: IV	0.112* (0.062)	-0.015 (0.013)	-0.013 (0.010)	-0.018 (0.011)	-0.011* (0.006)
Observations	5449	5449	5449	5209	5449

Column headings indicate the dependent variable for each model. The reported First-Stage coefficients in Panel A are from regressing total enrollment onto simulated eligibility using the estimation sample for the listed outcome, while the bracketed statistics show robust first-stage F-Statistics for the excluded simulated eligibility instrument. The reported IV coefficients in Panel B are for a variable measuring total years of self-reported public health insurance enrollment occurring from ages 0 through 18. All models include state fixed-effects and complete sets of indicators for the ages and calendar years when each respondent's enrollment was observed, each child's gender, race, and birth order, their mother's highest grade completed and age at the time of their birth, and indicators for the total number of enrollment observations and the exact age at which health outcomes were observed. All standard errors are clustered at the state level using the modal state of residence during childhood. Custom child-level NLSY sampling weights are applied. ** Statistical significance at the 5%.

* Statistical significance at the 10%.

*** Statistical significance at the 1%.

Table 4
The effect of eligibility at different ages on adult health outcomes.

	(1) Composite index	(2) Health limitation	(3) Poor self-rated health	(4) Any chronic condition	(5) Asthma
Eligibility from 0 to 5	0.441*** (0.143)	-0.098*** (0.033)	-0.044* (0.024)	-0.049* (0.025)	-0.032** (0.015)
Eligibility from 6 to 11	0.125* (0.076)	-0.021 (0.017)	-0.001 (0.026)	-0.034** (0.016)	-0.000 (0.008)
Eligibility from 12 to 18	0.030 (0.103)	-0.000 (0.020)	-0.031** (0.015)	0.012 (0.017)	-0.015 (0.014)
Observations	5465	5465	5465	5224	5465

Each table entry is from a separate IV regression with the health outcome listed in the column heading as the dependent variable and total years of public health insurance eligibility occurring over the indicated age range as the independent variable of interest, which is instrumented for using simulated eligibility over the same age range. All models include state fixed-effects and complete sets of indicators for the ages and calendar years when each respondent's eligibility was observed, each child's gender, race, and birth order, their mother's highest grade completed and age at the time of their birth, and indicators for the total number of eligibility observations and the exact age at which health outcomes were observed. All standard errors are clustered at the state level using the modal state of residence during childhood. Custom child-level NLSY sampling weights are applied.

* Statistical significance at the 10%.

** Statistical significance at the 5%.

*** Statistical significance at the 1%.

health investments, or early vaccinations and disease prevention may have especially large long-term health impacts.

However, it should also be noted that due to the composition of cohorts most represented in the NLSY-CYA sample, the identifying variation from changes in eligibility rules also vary by age. For instance the eligibility of the NLSY-CYA sample at younger ages is affected primarily by the initial Medicaid expansions occurring after 1986, while eligibility at older ages is affected primarily by later CHIP-driven expansions. This is noteworthy because we would not generally expect all policy changes to have similar health impacts. For instance later public health insurance program expansions, especially the CHIP-driven expansions of the late 1990s, targeted higher income populations and had lower take-up rates than the earlier Medicaid-based expansions (Bitler and Zavodny, 2014; Sasso and Buchmueller, 2004). As a result, the age-based heterogeneity identified in Table 4 may reflect differences in the population affected by the simulated eligibility instrument at different ages, rather than being strictly due to the health production function or the nature of the health services provided in early childhood.

5.2. Distributional effects of simulated eligibility on actual eligibility

Because I define treatment status with a continuous variable, there are potentially many distinct treatment effects underlying the reported point estimates, in particular the effect of going from no eligibility to one year of eligibility, the effect of going from

one year of eligibility to two years, and so on. The baseline estimates from Table 2 represent a weighted average of each of these distinct treatment effects, with the weights determined by how individuals whose total childhood eligibility was impacted by the instrument are distributed over the range of total childhood eligibility. It is potentially insightful to directly estimate which portions of the treatment variable's distribution are influenced by the instrument, since this information indicates which of the many possible causal treatment effects are reflected in the single IV point estimates reported in Table 2.

To produce such estimates, I first transform the continuous total eligibility measure into a set of 18 dummy variables indicating n or more total years of eligibility, with $n = 1, 2, \dots, 18$. I then regress each of these indicators onto the simulated eligibility instrument and the same vector of controls used in the baseline models, and plot the coefficients in Fig. 3. These coefficients – which can be viewed as modified first-stage regressions – estimate the effect of a unit increase in the instrument on the probability of having n or more years of total childhood eligibility.²² If this effect is greater for lower levels of total eligibility than higher levels, then the instrument acts primarily on the lower end of the total eligibility distribution, and inversely if greater effects are observed for higher values of n .

²² These coefficients are also equal to $1 -$ the cumulative density function of the total eligibility variable evaluated at n . See discussion in Acemoglu and Angrist (1999).

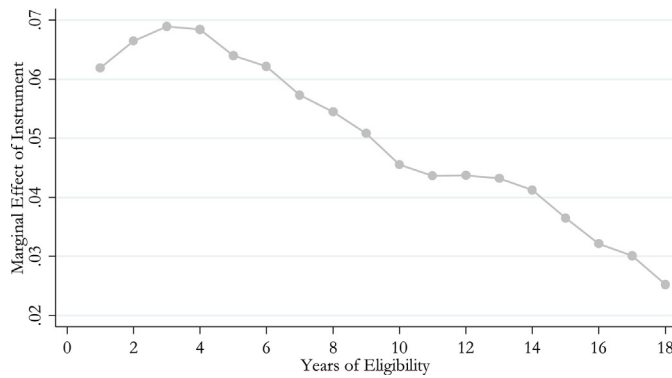


Fig. 3. Effect of simulated eligibility on actual eligibility. Each point in the figure comes from a separate regression in which the dependent variable is an indicator of whether individuals had n or more years of childhood eligibility and the independent variable of interest is the simulated eligibility instrument. Models also contain the vector of controls from the baseline models reported in Table 2. Each plotted coefficient can be interpreted as the effect of a unit increase in the instrument on the probability of having n or more years of total childhood eligibility.

Fig. 3 shows clearly decreasing “first-stage” coefficients as n moves from lower to higher levels of total eligibility. Specifically, a unit increase in the instrument increases the probability that an individual will have one or more years of actual eligibility by approximately six percentage points, but for total eligibility levels beyond four years, this effect declines and the effect of a unit increase in the instrument on the probability that an individual will have 18 or more years of eligibility is just over two percentage points. The declining magnitudes of these coefficients indicate that the utilized instrument primarily identifies the effect of marginal increases in total eligibility occurring at the lower end of the eligibility distribution.

This fact about the source of identifying variation is potentially important for policy design, since it indicates that the baseline IV estimates from Table 2 apply only to a context where eligibility expansions affect individuals who are beginning with relatively low levels of total childhood eligibility, and are less informative about the effect of expansions in contexts where total childhood eligibility levels are already relatively high. The findings from Fig. 3 are also potentially useful for explaining the relatively large effect size estimates for some outcomes in Table 2: If additional years of childhood eligibility produce diminishing marginal health benefits, then estimates from the total simulated eligibility instrument utilized in this paper will be relatively large, since the identifying variation comes from the lower end of the total eligibility distribution.

5.3. Robustness

A variety of sensitivity analyses are presented in Table 5. On balance, the results of these analyses help to rule out a number of potential competing explanations for the main findings and demonstrate the robustness of the baseline results to a variety of alternative modeling and sample construction choices. However, it should be noted that in many instances there are also substantial reductions in precision, indicating the need for some caution in interpreting the paper’s main findings. For brevity, the discussion focuses primarily on the composite health measure, but Table 5 reports results for each of the individual health outcomes as well.

One important issue, as noted above, is that the utilized identification strategy assumes that state-to-state variation in the timing of expansions for given age groups are independent of children’s health outcomes (except via increased eligibility). A partial test of this assumption is to check whether the results are sensitive to the inclusion of geographically specific linear trends, which allow any

underlying trends in unobservables to vary in a linear fashion for different geographic areas.

Panel A of Table 5 shows results that include a linear birth cohort variable interacted with indicators for each of the four census regions (South, Northeast, Midwest and West). These interactions would capture, for example, any health trends that were occurring across the studied cohorts in a particular region due to macroeconomic conditions or changing immigration patterns, among other possibilities. The estimated treatment effect for the composite health outcome reported in Column 1 of Panel A is .075, very similar to the baseline model, but is less precise and falls slightly below statistical significance at conventional levels ($P = .109$). Panel B of Table 5 reports results from models that include state-specific linear cohort trends. This specification is more flexible since it allows health trends to vary at the level of geography at which most Medicaid/CHIP policy is set, but because the state-trends absorb much of the identifying variation in eligibility, it does so at a cost of reduced precision, with the standard error for the composite health measure increasing from .047 to .060. The point estimate for the composite health outcome remains similar to the baseline point estimate at .095, and the estimated treatment effect remains slightly below conventional levels of statistical significance ($P = .113$).

While the imprecision of the estimates makes this exercise less than conclusive, the findings in Panels A and B of Table 5 can be interpreted as suggestive evidence that the main results from Table 2 are not an artifact of differential child health trends in states or regions that implemented larger Medicaid and CHIP expansions.

A related threat to identification is that states which expanded children’s public health insurance programs may have also implemented other policies which positively affected children’s health. Two policies of particular concern are cash welfare programs (AFDC and later TANF) and state EITC programs. Both of these policies are administered primarily at the state level, target low income families with children, varied substantially over the study period, and could plausibly affect child health via increases in household income. While not explicitly a policy variable, state level economic conditions pose a similar threat to identification, since they could plausibly impact both Medicaid/CHIP policy and child health outcomes.

I attempt to account for these possibilities by directly controlling for the potentially confounding state policies and economic conditions. To control for state EITC policies, I use data on state-level credit amounts (measured as a percentage of the federal earned income credit) and take the average of these values for the relevant state-years from birth through age 18 for each child in the NLSY-CYA.²³ To account for changes in cash welfare program generosity, I include controls for the average maximum AFDC/TANF benefit level and minimum AFDC/TANF eligibility threshold that each NLSY-CYA respondent was exposed to from birth through age 18, and also control for each NLSY-CYA respondent’s age when their state of residence was granted a major AFDC waiver or implemented TANF.²⁴ Finally, I include a control for the mean state unemployment rate occurring for each year from birth through age 18 for each respondent.

Models that add these controls to the baseline specification are reported in Panel C of Table 5. The estimated effect of public health insurance eligibility is generally similar to the estimate in

²³ State EITC data was transcribed from the documentation files of the TAXSIM program written by Daniel Feenberg of the National Bureau of Economic Research.

²⁴ Data on benefit levels and eligibility thresholds were collected from the Urban Institute’s Welfare Rules Database, and are with respect to a family of three with no income for benefit levels and for a family of three seeking initial eligibility for the eligibility level. Dates of AFDC waivers and TANF implementation are reported by the US Department of Health Human Services (1999).

Table 5
Robustness.

	(1) Composite index	(2) Health limitation	(3) Poor self-rated health	(4) Any chronic condition	(5) Asthma
<i>A: Region-specific cohort-trends</i>					
Years of childhood eligibility	0.075 (0.047)	−0.012 (0.009)	−0.009 (0.009)	−0.012* (0.007)	−0.011 (0.008)
Observations	5465	5465	5464	5224	5465
<i>B: State-specific cohort-trends</i>					
Years of childhood eligibility	0.095 (0.060)	−0.015 (0.011)	−0.005 (0.009)	−0.013 (0.009)	−0.015 (0.009)
Observations	5465	5465	5465	5224	5465
<i>C: EITC, AFDC/TANF and state unemployment controls</i>					
Years of childhood eligibility	0.060 (0.041)	−0.009 (0.010)	−0.012 (0.009)	−0.005 (0.008)	−0.010* (0.006)
Observations	5465	5465	5465	5224	5465
<i>D: Two child family-cohort interaction</i>					
Years of childhood eligibility	0.070* (0.040)	−0.010 (0.008)	−0.012 (0.008)	−0.008 (0.007)	−0.011* (0.006)
Observations	5465	5465	5465	5224	5465
<i>E: Weighted by eligibility observations</i>					
Years of childhood eligibility	0.071** (0.036)	−0.010 (0.008)	−0.011 (0.008)	−0.010 (0.006)	−0.009* (0.005)
Observations	5465	5465	5465	5224	5465
<i>F: Minimal controls</i>					
Years of childhood eligibility	0.096* (0.056)	−0.013 (0.012)	−0.017 (0.015)	−0.012 (0.012)	−0.013 (0.008)
Observations	5465	5465	5465	5224	5465
<i>G: Non-mobile subsample</i>					
Years of childhood eligibility	0.074** (0.030)	−0.006 (0.008)	−0.004 (0.010)	−0.007 (0.006)	−0.014** (0.006)
Observations	4564	4564	4564	4363	4564
<i>H: Unweighted</i>					
Years of childhood eligibility	0.082*** (0.028)	−0.015** (0.007)	−0.009 (0.007)	−0.010** (0.004)	−0.011** (0.005)
Observations	5465	5465	5465	5224	5465

Column headings indicate the dependent variable for each model. The reported coefficients are for a variable measuring total years of public health insurance eligibility occurring from ages 0 through 18, instrumented for using simulated eligibility over the same age range. Models in Panel A control for interactions between region of residence indicators and a linear birth cohort variable; Models in Panel B control for interactions between state of residence indicators and a linear birth-cohort variable; Models in Panel C include controls for state unemployment rates, EITC levels, AFDC/TANF benefit maximums and AFDC/TANF minimum eligibility thresholds over the course of each respondent's childhood, as well as each respondent's age when their state first received a major AFDC waiver or implemented TANF, in addition to the baseline controls used in the models from Table 2; Models in Panel D include an interaction between child birth cohort and an indicator of whether the family had two or more children, as well as main effects for cohort and family size and the baseline controls used in the models from Table 2; Models in Panel E are weighted by the product of the NLSY sampling weights and the number of eligibility observations, therefore giving greater weight to respondents with more eligibility observations; Models in Panel F control only for modal state of residence, the ages and years when eligibility was observed, and maternal age at birth; Models in Panel G restrict the sample to children whose state of birth was the same as their modal state of residence during childhood; Models in Panel H do not apply sampling weights. All standard errors are clustered at the state level using the modal state of residence during childhood.

* Statistical significance at the 10%.

** Statistical significance at the 5%.

*** Statistical significance at the 1%.

the baseline specification from Table 2 with a coefficient of .060, and is not statistically significant at conventional levels ($P = .144$). This reduction in precision reflects the strong collinearity between Medicaid/CHIP policy and other state level social policies, but the robustness of positive and economically significant point estimates suggests that the main findings are not an artifact of associations between Medicaid/CHIP policy and other state policy changes or general economic conditions.

In addition to state level EITC initiatives, the federal EITC expanded greatly during the study period, and previous research (e.g. Hoynes et al., 2015) has found that these expansions positively impacted child health. Because Medicaid/CHIP policy is largely set at the state level, confounding with federal EITC policy is less of a concern, but it is still possible that children with greater public health insurance exposure also benefited from increased federal EITC payments during childhood in a manner not accounted for by the simulated eligibility instrument. One simple check on this possibility exploits the fact that most expansions of the federal EITC

provided larger benefit increases to families with two or more children than to families with only one child. Given this, an interaction between child birth cohort and an indicator of whether there were two or more children in the family will capture some of the differential exposure to the federal EITC, and results from models with such interactions included are reported in Panel D of Table 5. The estimated impact of public health insurance eligibility is approximately equal to the baseline estimate with a coefficient of .070, suggesting minimal confounding, and this effect is statistically significant at the 10% level ($P = .080$).

The working sample in the baseline models consisted of children whose eligibility was observed at least 5 times total and for whom eligibility was observed at least once at ages 0–5, ages 6–11, and ages 12 through 18. These restrictions ensured that eligibility was reliably measured over the course of childhood, and Appendix A demonstrates that children in the working sample have fixed characteristics very similar to those of children who were excluded by these restrictions. However, even within the utilized working

sample there is substantial variation in the number of eligibility observations that are available for different respondents, and it may be desirable to give greater weight to respondents with more eligibility observations. Panel E of Table 5 does so by weighting the baseline regressions by the total number of valid eligibility observations, and the treatment effect estimate for the composite outcome is very similar to the baseline results, with a coefficient of 0.071, and this estimate is statistically significant at the 5% level ($P = .046$).²⁵

The remaining panels of Table 5 address three additional robustness related issues.

First, the baseline specification controlled for a relatively extensive set of individual level controls, which improved the precision of the estimates, but it is useful to evaluate the sensitivity of the results to including a more parsimonious set of controls. To this end, Panel F of Table 5 reports results that control only for state of residence, the years and ages when eligibility was observed, and maternal age at birth, and finds a treatment effect of 0.096 for the composite outcome, similar to the baseline estimate, and this estimate is statistically significant at the 10% level ($P = .088$).

Second, an advantage of using longitudinal microdata is that it enables the accurate calculation of eligibility for respondents who moved across states during childhood, but this does not address the possibility that some families may intentionally move to states with more generous public health insurance programs, causing simulated eligibility to reflect the willingness and ability to move in order to improve child outcomes in addition to exogenous policy changes. To help address this concern Panel G of Table 5 reports models that use the sub-sample of children whose state of birth is the same as their modal state of residence over the full course of childhood, indicating a high degree of residential stability.²⁶ The results using this sample are very similar to the baseline findings, with a treatment effect estimate of 0.074 that is statistically significant at the 5% level ($P = .013$).

Finally, Panel H of Table 5 reports results from models that do not apply NLSY sampling weights. The estimated treatment effect for the composite outcome is 0.082 and is highly statistically significant.

As discussed in Section 2 above, the NLSY79 switched to biannual surveying after the 1994 wave, and as a result I was able to calculate Medicaid/CHIP eligibility every year from 1978 to 1993 but only every other year from 1995 through 2011 (the years for which eligibility is observed lag the NLSY79 survey years by one because income is reported with respect to the previous calendar year). Because the main eligibility measure used above is based on the fraction of annual observations in which each child was eligible, the switch to biannual surveying means that the period prior to 1994 is implicitly given more weight than the period after 1994 simply because children were observed more often in this period. This may be especially relevant for the models estimating the effect of eligibility in specific age ranges. I use two strategies to assess the extent to which biannual surveying impacts my findings, with the results presented in Table 6.

In Panel A of Table 6 I present estimates that use a total eligibility variable which imputes eligibility in the non-survey years from the later portion of the NLSY79. Specifically, I interpolate family incomes for non-survey years using the mean of the income

levels observed in the NLSY79 wave occurring immediately prior to the non-survey year and the wave occurring immediately after the non-survey year. To be as conservative as possible, I only perform this imputation if income in *both* the surrounding years is observed.²⁷ Eligibility calculations for non-survey years use similarly imputed family size and state of residence measures. Panel A of Table 6 reports results for both total childhood eligibility and eligibility at age 0–5, 6–11 and 12–18, and in all cases the results are very similar to those that only used waves where eligibility was directly observed.

In Panel B of Table 6 I present estimates that use a total eligibility variable which excludes even year observations in the earlier portion of the NLSY79. This approach measures total childhood eligibility in a consistent manner across the two sampling period, but does so at the cost of excluding useful information about eligibility in the earlier survey waves. Results for both total childhood eligibility and eligibility at age 0–5, 6–11 and 12–18 are again reported, and the findings are very similar to the baseline results that used all of the available eligibility observations.²⁸ Taken jointly, the results in Table 6 suggest that the paper's main findings are not an artifact of inconsistencies in the measurement of Medicaid/CHIP eligibility across earlier versus later phases of the NLSY79.

6. Mechanisms

The most intuitive underlying mechanism for the results presented above is access and utilization of health care services. Gaining insurance under Medicaid or CHIP reduces the marginal cost of consuming health care, especially if children are transitioning from being uninsured, and consumption of health services could in turn improve health. For instance a child with asthma symptoms who gains coverage under a public health insurance program may be prescribed a preventative inhaler or counseled to avoid common asthma triggers and experience an immediate and lasting health improvement. Receiving the full slate of recommended vaccinations may also be strongly affected by public health insurance coverage and could have significant lasting health effects. Utilization related mechanisms are also consistent with the larger treatment effects found among children actually enrolling in public health insurance programs found in Table 3.

The NLSY-CYA has only limited health care utilization information, but each wave did collect information on how recently children had last seen a doctor for a routine health checkup. I use this information to construct variables measuring whether each child had gone a year or more without a preventative doctor's visit at any point between birth and age five and the proportion of observations over the same age range in which parents reported that it had been over a year since the child's last routine checkup. I focus on the 0–5 age range because this is a period when most of the standard immunization schedule is being completed and when routine visits are universally recommended, and focus on routine preventative checkups because other types of health care utilization may confound access to care with morbidity.

Columns 1 and 2 of Table 7 report IV models that estimate the effect of total eligibility from ages 0 to 5 on the described health care utilization variables. The results in Column 1 indicate that an

²⁵ The weights applied in Panel E are the product of the custom NLSY sampling weights and the number of valid eligibility observations for each respondent.

²⁶ This restriction is used rather than simply excluding all children who ever moved between states because the latter restriction would eliminate 60% of the working sample. Focusing on children who largely remained in their state of birth is especially useful because, as noted by Cohodes et al. (2016), state of birth is unlikely to be related to public health insurance generosity given the difficulty of anticipating local Medicaid policy changes and the fact that prior analyses have found minimal relationships between Medicaid programs and fertility (DeLeire et al., 2011; Zavodny and Bitler, 2010).

²⁷ For instance, if income is observed in 2003 and in 2005, then income for 2004 is set equal to the mean of the observed years, but if income in either 2003 or 2005 is missing – for instance due to non-response or refusal – then income in 2004 is set to missing.

²⁸ For comparability with the baseline results, all of the models in Table 6 use the same estimation samples as the corresponding models in Tables 2 and 4. The results in Table 6 are very similar if the sample restrictions relating to total eligibility observations and having at least one observation at each phase of childhood are instead defined using the imputed/biannual eligibility measures.

Table 6
Alternative Medicaid/CHIP eligibility calculations.

	(1) Composite index	(2) Health limitation	(3) Poor self-rated health	(4) Any chronic condition	(5) Asthma
<i>A: Imputed eligibility from 1995 to 2011</i>					
Full eligibility (0–18)	0.079** (0.035)	−0.013* (0.007)	−0.010 (0.008)	−0.010 (0.006)	−0.010* (0.005)
Eligibility from 0 to 5	0.424*** (0.136)	−0.094** (0.032)	−0.042* (0.023)	−0.047* (0.025)	−0.031** (0.014)
Eligibility from 6 to 11	0.125* (0.075)	−0.021 (0.017)	−0.001 (0.026)	−0.033** (0.016)	−0.000 (0.008)
Eligibility from 12 to 18	0.033 (0.115)	−0.000 (0.022)	−0.034* (0.018)	0.013 (0.019)	−0.017 (0.016)
<i>B: Biannual surveying imposed throughout</i>					
Full eligibility (0–18)	0.082** (0.036)	−0.014* (0.008)	−0.011 (0.009)	−0.010 (0.007)	−0.010* (0.006)
Eligibility from 0 to 5	0.401*** (0.149)	−0.086** (0.034)	−0.046** (0.022)	−0.045* (0.026)	−0.029** (0.014)
Eligibility from 6 to 11	0.125* (0.075)	−0.019 (0.017)	−0.003 (0.024)	−0.033** (0.016)	−0.002 (0.008)
Eligibility from 12 to 18	0.034 (0.099)	−0.001 (0.019)	−0.031** (0.014)	0.011 (0.016)	−0.015 (0.013)
Observations	5465	5465	5464	5224	5465

Each table entry is from a separate IV regression with the health outcome listed in the column heading as the dependent variable and total years of public health insurance eligibility occurring over the indicated age range as the independent variable of interest, which is instrumented for using simulated eligibility over the same age range. In Panel A, eligibility data is imputed for non-survey years for the period after the NLSY79 switched to biannual surveying. In Panel B, eligibility is measured excluding even-year observations for the period prior to 1995, and therefore imposes biannual surveying on the full NLSY79. All models include state fixed-effects and complete sets of indicators for the ages and calendar years when each respondent's eligibility was observed, each child's gender, race, and birth order, their mother's highest grade completed and age at the time of their birth, and indicators for the total number of eligibility observations and the exact age at which health outcomes were observed. All standard errors are clustered at the state level using the modal state of residence during childhood. Custom child-level NLSY sampling weights are applied.

* Statistical significance at the 10%.

** Statistical significance at the 5%.

*** Statistical significance at the 1%.

Table 7
The effect of Medicaid/CHIP on health care utilization and early educational outcomes.

	(1) No checkup in past year	(2) Proportion of observations without checkup	(3) Grade repetition	(4) On time high school completion
Years of eligibility	−0.061 (0.042)	−0.023 (0.028)	−0.020* (0.010)	0.017 (0.013)
Observations	4515	4515	5455	4047

The dependent variable for the model reported in Column 1 is an indicator of whether the respondent's parent ever reported that they had not had a routine doctor's visit in the past year from ages 0 through 5; The dependent variable in Column 2 is the proportion of observations occurring from ages 0 through 5 for which the respondent's parent reported that they had not had a routine doctor's visit in the past year; The dependent variable in Column 3 is an indicator of whether the respondent ever repeated a grade; The dependent variable in Column 4 is an indicator of whether the respondent had completed high school by age 19. The reported independent variable is total years of public health insurance eligibility occurring from ages 0 through 5 for the models in Columns 1 and 2, and total years of eligibility occurring from ages 0 through 18 for the models in Columns 3 and 4, in both cases instrumented for with simulated eligibility occurring over the same age range. All models include state fixed-effects and complete sets of indicators for the ages and calendar years when each respondent's eligibility was observed, each child's gender, race, and birth order, their mother's highest grade completed and age at the time of their birth, and indicators for the total number of eligibility observations. All standard errors are clustered at the state level using the modal state of residence during childhood. Custom child-level NLSY sampling weights are applied. ** and *** denote statistical significance at the 5% and 1% levels, respectively.

* Statistical significance at the 10%.

additional year of eligibility from ages 0 to 5 reduces the probability of ever going more than a year without a routine visit by 6.1 percentage points, a reduction of approximately 15% from the sample mean of .39. This estimate is not statistically significant at conventional levels, ($P = .150$). The estimate in Column 2 of Table 7 indicates that an additional year of eligibility reduces the proportion of observations for which parents reported that it had been over a year since the child's last routine checkup by 2.3 percentage points, a reduction of 11.5% from the sample mean of .20. While it has the expected sign, this estimate is quite imprecise and statistically insignificant, with a P -value of approximately .403.

While the lack of comprehensive utilization measures and the imprecision of the estimates from Table 7 warrant caution, these results do suggest that greater Medicaid and CHIP eligibility increased health care utilization among young children in the current sample. The credibility of this finding is supported by previous research demonstrating that public health insurance programs have positive health care utilization effects among children, especially routine preventative care (see for instance Boudreaux et al.,

2016; De La Mata, 2012; Currie et al., 2008; Joyce and Racine, 2005, among others).

Several studies have also found that vaccinations in particular are sensitive to public health insurance coverage and can have lasting health impacts. For instance Luca (2016) uses the introduction of state immunization mandates for identification and finds that vaccinations reduce the prevalence of various covered conditions, as well as improvements in longer term educational outcomes, while Greenberg et al. (2005) discuss the lasting negative health impacts of pertussis that are typically eliminated after the introduction of modern vaccines. Since most vaccinations occur before age 5, this explanation is consistent with the larger treatment effects for eligibility in early childhood found in Table 4.²⁹

²⁹ Also relevant is recent research by Bhalotra and Venkataramani (2011), which finds that effectively treating pneumonia in infancy results in long-term reductions in disability, as well as improvement in several economic outcomes.

In addition to health care utilization, another potential mechanism underlying the positive health effects reported above is improved school performance. Low income children eligible for public health insurance may have fewer missed school days or receive treatment for conditions that improve their ability to fully participate in school, for instance short-term hearing loss from chronic ear infections or difficulty seeing the instructor due to lack of optometry care. A positive relationship between educational attainment and health is well established (Currie, 2009), and if improvements in early educational success and childhood health are mutually-reinforcing, then education is a possible mechanism underlying the main health findings.

To investigate this possibility more directly, Columns 3 and 4 of Table 7 report IV models that estimate the effect of childhood Medicaid/CHIP eligibility on two early indicators of educational success, grade repetition and completing high school before age 19. The results indicate that an additional year of childhood public health insurance eligibility decreases the probability of repeating a grade by 2 percentage points and increases the probability of on-time high school completion by 1.7 percentage points. The estimate for grade repetition is statistically significant at the 10% level, while the estimate for high school completion is not statistically significant ($P = .217$). To the extent that these early indicators of educational success have positive health impacts, these results suggest that education may be another mechanism through which public health insurance eligibility improves longer-term health outcomes.

While data limitations prevent me from directly investigating additional potential mechanisms, previous research also suggests a number of plausible pathways by which public health insurance coverage could improve child health. One such pathway is household finances. Health insurance, like all forms of insurance, is fundamentally a risk management instrument which protects policy holders from catastrophic financial events. Medical events have been estimated to be a contributing factor to over half of all personal bankruptcies in the US (Himmelstein et al., 2009), and Medicaid eligibility specifically has been shown to reduce personal bankruptcies and medical debt (Boudreaux et al., 2016; Gross and Notowidigdo, 2011; Currie et al., 2008). Beyond preventing catastrophic financial events, the provision of free or very low cost health insurance transfers otherwise costly resources to eligible families, resulting in a general improvement in household finances. In turn, a large body of evidence indicates that both the overall level and the stability of household income have positive effects on children's health (Case et al., 2002; Ettner, 1996).

Other plausible mechanisms include changes in maternal labor force participation (Strumpf, 2011; Winkler, 1991); changes in the supply of health care providers or technology investments by health care providers (Freedman et al., 2015; Adams et al., 2003); and reductions in early childbearing due to contraceptive coverage, in either the child generation or among their mothers (Kearney and Levine, 2009; Lindrooth and McCullough, 2007). Additional research on the extent to which such responses to expanded public health insurance coverage affect health outcomes is warranted.

7. Conclusion

The primary contribution of this paper has been to estimate the relationship between public health insurance programs for children and health outcomes in a manner that accounts for the cumulative nature of health determination. When exposure to public health insurance programs is measured with full eligibility histories, these programs are found to have large effects on health limitations, self-rated health, chronic conditions and asthma observed in early adulthood. Eligibility occurring from ages 0 to 5

appear to have especially large long-run health effects. One caution in interpreting these results is that a limited sample size reduces their precision, and the baseline estimates for two of the five studied outcomes are not statistically significant at conventional levels.

The relationship between public health insurance programs for children and long-term health outcomes remains an understudied aspect of health policy. Future research analyzing a wider variety of health outcomes measured further into adulthood, using complimentary identification strategies, and directly investigating potential mechanisms and intermediate outcomes would allow for a more comprehensive accounting of the benefits of public health insurance programs for children.

Appendix A. Sample representativeness

As noted in Section 2, restricting my sample to children with eligibility observed at least 5 times total and during several different portions of childhood impacts the composition and possibly the representativeness of the NLSY-CYA sample used in the main analysis. For instance, if the poorest respondents with the least stable living situations are more likely to have missing data, then the main analysis is only applicable to a relatively privileged sub-population of Medicaid and CHIP eligible children.

Table A1 assesses the extent of such selective attrition by comparing the characteristics of children in the sample used above with a more expansive NLSY-CYA sample. Since the children excluded from my working sample are definitionally observed less frequently than the full sample, the table focuses on characteristics that can be reliably measured in a small number of observations occurring early in life, specifically maternal age at birth, maternal education, child race and ethnicity, gender, birth cohort, and birth order.

The first column of Table A1 reports means of these characteristics for the sample of 8135 children who have valid data on basic demographic and maternal socioeconomic characteristics, while the second column reports means for the 5465 children that remain in the working sample after the sample restrictions described in Section 2 are applied. Column 3 of Table A1 reports the differences in mean characteristics across these two groups, and Column 4 reports the P -values for these differences.

Table A1 does reveal some differences in the characteristics of the two samples, though few of these differences are of a practically large magnitude and only two are statistically significant at conventional levels. The largest differences in practical terms are

Table A1
Characteristics of full versus working samples.

	(1) Full sample	(2) Working sample	(3) Difference	(4) P -value of difference
Mother's age at birth	25.28	25.17	-0.11	0.19
Mother's highest grade completed	13.30	13.37	0.07	0.09
White	0.71	0.71	0.00	0.95
Black	0.20	0.20	0.01	0.46
Hispanic	0.09	0.09	0.00	0.35
Birth year	1986.04	1985.91	-0.12	0.17
Female	0.48	0.50	0.02	0.02
Birth order	1.87	1.87	0.00	0.88
Observations	8135	5465		

Column 1 reports means for all NLSY-CYA respondents with valid data on the listed characteristics, while Column 2 reports means for the NLSY-CYA respondents used in the baseline models from Table 2. Column 3 and 4 report the mean differences between the two samples and the P -values for a test of whether the characteristics of the two samples are equal. Custom child-level NLSY sampling weights are applied.

observed for maternal education and maternal age at birth, with the average child from the working sample having a mother who completed .07 more years of schooling and was .11 years younger at birth than the average child from the “full” sample. The two samples are virtually identical with respect to race, ethnicity and birth order, and a 2 percentage point gap is observed with respect to gender.

While the presence of even these modest differences in sample characteristics gives reason for some caution in applying the findings reported above to a broader population, the fact that the observed differences are relatively small suggests that the paper's main findings are useful for assessing the effects of public health insurance within a fairly expansive set of Medicaid and CHIP recipients.

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