The Long-Term Effects of Early Life Medicaid Coverage*

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Abstract

Although the link between the fetal environment and later life health and achievement is well-established, few studies have evaluated the extent to which public policies aimed at improving fetal health can generate benefits that persist into adulthood. In this study, we evaluate how a rapid expansion of prenatal and child health insurance through the Medicaid program affected adult outcomes of individuals born between 1979 and 1993 who gained access to coverage in utero and as children. We find that those whose mothers gained eligibility for prenatal coverage under Medicaid have lower rates of obesity as adults and fewer hospitalizations related to endocrine, nutritional and metabolic diseases, and immunity disorders as adults, with particularly pronounced reductions in visits associated with diabetes and obesity. We also find that the prenatal expansions improved educational and economic outcomes for affected cohorts. Cohorts who gained Medicaid eligibility in utero have higher high school graduation rates and we find evidence suggesting that they have higher incomes in adulthood. We find effects of public eligibility in other periods of childhood on self-reported health, hospitalizations, and income later in life, but these effects are smaller in magnitude. Our results indicate that expanding Medicaid prenatal coverage had sizeable long-term benefits for the next generation.

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I. Introduction

A large and growing literature has shown that the intrauterine environment has dramatic effects on adult health and achievement. Given the significance of the fetal environment for long-term health and development, interventions that target the prenatal period are expected to bring higher returns than later interventions. However, there is little evidence as to whether policy interventions designed to improve fetal health can effectively generate long-lasting benefits.

Our project is the first to evaluate the long-term consequences of improved in utero health that resulted from a widespread, rapid expansion of Medicaid benefits to pregnant women from 1979 to 1993. This program represents the single largest effort on the part of United States government to improve birth outcomes. However, despite the historic magnitude of this expansion, little is known about whether this provision of care to pregnant women had any lasting effects on the health or economic trajectories of those born during this period. As suggested by the fetal origins literature, the effects of early life intervention may not be fully captured by measures of health at birth and may remain latent for long periods of time. This necessitates a long-run perspective when evaluating the benefits of early public intervention.

We exploit variation in the timing and generosity of Medicaid coverage expansions for pregnant women and children across states to identify how access to coverage in utero and at different points in childhood affects health and achievement in adulthood. We do this using a simulated eligibility approach that constructs a measure of generosity of state eligibility rules to instrument for the fraction of individuals eligible for Medicaid coverage for each birth cohort. As developed by Cutler and Gruber (1996) and Currie and Gruber (1996a,b), this approach isolates changes in state-level eligibility resulting from Medicaid eligibility policy rather than other socioeconomic factors. Using this technique, we evaluate the effect of public health insurance eligibility in utero and at other ages during childhood, documenting the marginal effect of an additional year of eligibility at ages 1-4, 5-9, 10-14 and 15-18.

We test for and document early differences in the health and economic trajectories of cohorts who gained early life exposure to Medicaid coverage. The cohorts we study are relatively young in our data (ages 19-35) and have not yet reached the period of adulthood when many chronic conditions begin to emerge. Yet, we find that the provision of Medicaid benefits had lasting and measurable effects on the health of individuals who were in utero during the expansions. A ten percentage point increase in eligibility for pregnant women decreased the probability that adults born during the expansion are obese by 1.2 percentage points, or approximately 5 percent, at ages 19-35.

Furthermore, we find that in utero Medicaid coverage lowers hospitalizations for conditions that have been previously shown to be sensitive to the in utero environment. Our results indicate that a ten percentage point increase in eligibility lowered hospitalizations related to endocrine, nutritional, metabolic diseases and immunity disorders by 8 percent; within this category, we find particularly pronounced reductions for hospitalizations associated with diabetes and obesity. These results imply that the expansion of the Medicaid program to pregnant women during the 1980s and early 1990s is responsible for a significant reduction in hospitalization costs today. The increase in prenatal eligibility over this period is associated with about $240 million in reduced hospital costs for a single cohort between the ages of 19 and 32, offsetting about 38 percent of the cost of the initial Medicaid eligibility expansion to pregnant women. We also find that Medicaid eligibility during other years of childhood improved self-reported health and decreased hospitalizations in adulthood, although these effects are smaller in magnitude than those associated with in utero eligibility.

We also find that the Medicaid expansions for pregnant women improved economic outcomes in adulthood. Cohorts whose mothers gained prenatal Medicaid eligibility are more likely to graduate high school; a ten percentage point increase in prenatal eligibility is associated with a 0.2 percentage point increase in high school graduation rates. Furthermore, we find some evidence of higher income and less reliance on food assistance in adulthood for cohorts whose mothers benefitted from these expansions.

Our findings suggest that the Medicaid expansions for pregnant women that occurred thirty years ago had persistent long-term health and economic benefits for the next generation. Strikingly, we find evidence of these effects among the affected cohorts at relatively young ages in adulthood. The observed reductions in obesity and hospitalizations for chronic illnesses like diabetes suggest that these cohorts are not only healthier today as a result of the Medicaid expansions, but that they are on a better lifetime health trajectory. In addition, the increases in high school completion for these cohorts and early adult income indicate meaningful changes in human capital and productivity. As these cohorts age and approach mid-life when income increases and chronic illness is more prevalent, the observable effects of this intervention on their health and economic well-being may become even more pronounced.

II. Background

The fetal origins hypothesis (Barker 1995) proposes that the fetal environment has a critical impact on the development of body structure and function in utero with lasting effects on health in adulthood. Lifelong changes in physiological and metabolic characteristics that occur during this critical developmental period may lead to obesity, diabetes, hypertension, and mental health conditions that do not materialize until much later in life. The evidence documenting the link between the fetal environment and later life outcomes is extensive in both the economics and epidemiology literature. In this section, we briefly summarize select findings from this literature; for a more detailed overview, see Almond and Currie (2011).

Many studies have investigated the fetal origins hypothesis by analyzing how insults to the fetal environment – such as poor nutrition or maternal infection - affect adult chronic disease.[1] For example, one of the earliest investigations into the hypothesis analyzed outcomes of cohorts that were in utero during the Dutch Famine of 1944 (Ravelli, Stein, and Susser, 1976). The authors found that cohorts that had been in utero during the famine were twice as likely to be obese at

[1] Also, a smaller literature has documented how negative shocks that occur after birth, but during childhood, have effects that persist into adulthood. For example, Bleakley (2010) finds that exposure to malaria in childhood results in lower incomes in adulthood. Reyes (2007) links childhood exposure to lead to criminal activity later in life. Case, Fertig, and Paxson (2005) show that poor health in childhood is associated with lower earnings and worse health in middle age.

age 18 as cohorts who were not exposed to the famine. Follow-up studies found that the exposed cohorts had a higher BMI and poor self-reported health (Roseboom et al. 2001), greater incidence of coronary heart disease and impaired glucose tolerance (Painter, Roseboom, and Bleker 2005), increased incidence of psychiatric disorders including schizophrenia and major affective disorder (Brown et al. 1995; Susser and Lin 1992; Susser et al. 1996; Susser, Hoek, and Brown 1998; Brown et al. 2000), and reduced life expectancy (Lindeboom, Portrait, and van den Berg 2010). Studies of other negative shocks to the fetal environment have also found evidence of poor health and disability (Almond and Mazumder 2010) in adulthood.

In addition to associations with poor health in adulthood, the later life consequences of negative shocks to the fetus include poor performance on a wide range of educational and economic outcomes. Studies have linked negative shocks that occur in utero to later life outcomes ranging from reduced educational achievement (Almond, Edlund, and Palme 2009; Barreca 2010; Almond, Mazumder, and van Ewijk 2011) to higher poverty later in life (Barreca 2010). These findings suggest that early life exposures and health have important consequences for a range of human capital outcomes in adulthood.

While a substantial body of research exists on the long-term effects of negative shocks that occur in utero, few studies have evaluated the positive impacts of public programs in the United States targeting the fetal environment.[2] Recent economic models predict a higher return on

[2] A separate strand of literature has investigated the effect of educational or economic interventions in early childhood on health later in life. For example, young children who received intensive early childhood education through the Abecedarian education experiment in North Carolina had lower levels of hypertension, obesity, and metabolic syndrome in their mid-30s relative to children in the control group (Campbell et al. 2014). Similarly, the pre-school program Head Start has been found to reduce childhood obesity (Frisvold and Lumeng 2011), and this reduction in obesity persists among teenagers who attended Head Start as children (Carneiro and Ginja 2013). Finally, recent work by Aizer, Eli, Ferrie, and Lleras-Muney (2014) analyzes the long-run impact of means-tested cash transfers via the Mother's Pension Program, a precursor to AFDC. The authors find that male children in families that received these transfers lived approximately one year longer than those in families who applied for, but did not receive, cash transfers.

interventions targeted to children earlier in their lifespan due to the self-productivity of human capital investments and dynamic complementarities gained from investing during earlier stages of the lifecycle (Heckman 2007, Cunha and Heckman 2007). Under these models, it is hypothesized that returns to investment in the prenatal period will be higher than postnatal investments, both initially and in the long-term (i.e. the "antenatal investment hypothesis," see Doyle et al. 2009).

The limited evidence available indicates that interventions targeted to the prenatal period improve health and economic outcomes in adulthood. Long-run improvements in maternal and child outcomes have been documented under two different randomized trials of the Nurse-Family Partnership (NFP) program, which provides home visitation by nurses to low-income women who are pregnant for the first time. During monthly visits in pregnancy and the first two years of infancy of the child, nurses provide guidance to mothers on health-related behaviors, child caregiving, and economic self-sufficiency. Benefits observed for children, particularly for those with high-risk mothers, include better school performance, less criminal activity, and reduced use of public benefits (see Olds et al. 2007, Eckenrode et al. 2010, Olds et al. 2010, Heckman et al. 2014).

In addition, Hoynes, Schanzenbach, and Almond (2012) evaluate how the U.S. Food Stamp Program has affected long-run outcomes. While this large-scale safety net program is not directly targeted to pregnant women and children, it may have lasting effects by influencing early life nutrition and resources. The authors study the implementation of the Food Stamps program and, using its staggered timing of adoption across counties, find that children gaining access to the program in utero and in early childhood (under age 5) experienced lowered incidence of metabolic diseases (such as diabetes, hypertension, and obesity) in adulthood. Although the authors do not have sufficient power to separately identify the impacts of in utero and early childhood exposure, exploratory analysis suggests that the impact on metabolic diseases is driven by exposure to the Food Stamp program during the pre- and early post-natal period.

Finally, recent work has demonstrated the potential for public policies that regulate environmental conditions to affect long-run outcomes. Isen, Walker, and Rossin-Slater (2015) examine changes in exposure to pollution early in life resulting from regulation under the 1970 Clean Air Act Amendment. The authors find that the policy increased labor force participation and earnings at age 30 for those benefiting from reduced exposure to pollution during their year of birth.

Our paper contributes to this understudied area, as well as to a small but growing literature examining the long-run effects of access to public health insurance in childhood. A handful of recent studies have linked exposure to public health insurance in childhood to improved teenage health (Cohodes et al. forthcoming, Currie, Decker and Lin 2009, Wherry and Meyer forthcoming), better educational or economic outcomes (Brown, Kowalski, and Lurie 2015, Cohodes et al. forthcoming, Levine and Schazenbach 2009), and reduced mortality (Brown, Kowalski, and Lurie 2015) and health care utilization as adults (Wherry et al. 2015). Each of these papers studies variation in public insurance under expansions in coverage over different periods of childhood from birth to age 18.

This is the first study to examine the long-term effects of public health insurance expansions explicitly targeting the prenatal period, when children are at their most receptive stage of development and interventions may yield the highest return. Despite the historic magnitude of the expansions in prenatal care under Medicaid in the 1980s and early 1990s and their documented impact on health at birth, there has been no study of the potential long-run effects of this policy intervention.

To our knowledge, only one other study has considered the long-term effects of access to public coverage during the prenatal period. Boudreax, Golberstein, and McAlpine (2015) examine early life exposure to Medicaid coverage under the rollout of the program in the 1960s. They find that cohorts who gained exposure to the program between conception and age 6 had better health as adults at ages 25-54, as measured by a 0.35 standard-deviation change in a composite index measure of chronic health conditions. One limitation of this study, however, is that it does not separately identify the effects of coverage during the prenatal period and early childhood. In addition, at the time of study, Medicaid coverage, including coverage for pregnant women, was primarily limited to very low-income single mother families receiving cash welfare. This precluded low-income women with first time pregnancies from receiving Medicaid, a population that has been clearly shown to benefit from early intervention under the NFP program.

In this paper, we study the impact of an intervention that was explicitly intended to establish broad access to prenatal care, including women with no previous children or receipt of public benefits. In addition, the policy variation we use allows us to separately identify the long-run effects of Medicaid prenatal coverage and coverage at other age periods in childhood by taking advantage of expansions in eligibility for both pregnant and children occurring over this time period. Prior work examining expanded Medicaid eligibility for children has not examined or accounted for the large changes in Medicaid prenatal eligibility that affected many of the same cohorts. This is an important omission given the large and growing literature on the importance of the early life environment in determining adult health and achievement. In the next section, we provide background on the Medicaid coverage expansions for pregnant women and children and review potential mechanisms that might impact long-run outcomes.

III. The Medicaid Expansions and Prenatal Coverage

a. Background on the Medicaid expansions

Established in 1965, the Medicaid program provides basic medical coverage to certain low-income individuals. Jointly financed by federal and state governments, states administer the program following federal guidelines, which include limitations on the categories of individuals who can be covered. Until the 1980s, coverage for pregnant women and children was primarily limited to recipients of cash welfare under the Aid to Families with Dependent Children (AFDC) program.[3] Income eligibility thresholds for the program varied by state but were typically well below the poverty line.[4]

Starting in 1984, eligibility for the Medicaid program was broadened to include coverage for low-income pregnant women and children not tied to the welfare system. Motivated by a comparatively high U.S. infant mortality rate in the early 1980s, a major national focus at the time was increasing access to timely and comprehensive prenatal care for low-income women (Howell 2001). Changes in Medicaid eligibility began with new requirements for state programs to cover all pregnant women meeting the financial standards for cash welfare, regardless of their family structure or participation in the AFDC program. In addition, the children born to women receiving Medicaid were deemed automatically eligible for coverage during their first year of life (Currie 1995).

Between 1986 and 1990, Congress took larger steps to expand Medicaid eligibility for pregnant women and their newborns with incomes exceeding AFDC thresholds. New options allowed states to expand coverage to pregnant women and their infants with incomes up to the poverty line and later to 185 percent of the poverty line. These options were followed by a mandatory requirement for all states to extend coverage to pregnant women and young children with family incomes under 133 percent of the poverty line. Additional information on these changes is available in Table A.1 in the Appendix.

First demonstrated in seminal work by Currie and Gruber (1996b), these changes led to dramatic growth in Medicaid eligibility for pregnant women at the national level, as well as considerable variation across states in both the timing and generosity of eligibility changes. Using data from

[3] There were a few other programs under which non-disabled pregnant women and children could qualify for Medicaid. However, these programs were optional for states, had narrow eligibility criteria, and limited eligibility to very poor women and children. Additional information on these eligibility pathways may be found in the Appendix.

[4] In 1989, state income limits ranged from 14 to 79 percent of the federal poverty line, with an average eligibility threshold of 48 percent of poverty (U.S. General Accounting Office 1989).

the March Current Population Survey (CPS) and detailed eligibility rules from this period, we estimate that the fraction of 15-44-year-old women who would be eligible for Medicaid coverage in the event of a pregnancy grew from 13 percent in 1979 to a staggering 44 percent in 1993. Figure 1 depicts national eligibility over this time period (in a solid black line), as well as state levels of eligibility for each year (in grey). Meanwhile, Figure 2 shows the change in the fraction of women eligible for prenatal coverage in each state during this period. While there was growth in eligibility across all states, there was tremendous variation in the timing and size of the expansions in each state.

These changes in Medicaid eligibility for pregnant women were accompanied by additional expansions in eligibility for children. A series of acts by Congress expanded eligibility to children who were not traditionally eligible for AFDC and with family income levels exceeding AFDC cutoffs. Mirroring the changes for pregnant women, these eligibility changes were first introduced as a state option and later by federal mandate. By 1992, almost one-third of children in the U.S. were eligible for public health insurance coverage (Cutler and Gruber 1996). Eligibility levels for children continued to grow through the 1990s as Medicaid eligibility changes continued to be phased in and later through the 2000s under optional state expansions to higher income children under the Children's Health Insurance Program (CHIP) (Lo Sasso and Buchmueller 2004).

This meant that, in addition to differences in in utero Medicaid coverage, cohorts born between 1979 and 1993 faced different eligibility criteria for public health insurance during childhood. To demonstrate this, we estimate eligibility during childhood for each birth cohort by calculating the fraction of children belonging to that cohort that were eligible for coverage at each age during childhood. We then sum the fraction eligible across ages 1-18 to construct a cumulative measure of public eligibility expressed as the average number of years of eligibility during childhood. Additional information on the methods used in this calculation follows in Section V.

Figures 3 and 4 depict changes in national and state-level childhood eligibility for public health insurance for cohorts born between 1979 and 1993. Children born in 1993 had almost 8 years of eligibility on average, more than twice the 3 years of eligibility for those born in 1979. Similar to the variation seen in prenatal eligibility, there is substantial variation across states in both the timing and size of the changes in childhood eligibility for these cohorts.

In our analysis, we use this variation in eligibility to separately evaluate exposure to public health insurance during the prenatal period and later exposure during childhood. We interpret our measure of prenatal eligibility as capturing the effects of both in utero coverage and coverage during the first year of life. As described above, the Medicaid expansions during this period were specified to apply to pregnant women and their newborns until their first birthday. Our specification also includes measures of eligibility for public coverage at later age ranges in childhood (ages 1-4, 5-9, 10-14, and 15-18).

b. Mechanisms for long-term effects

There are several ways in which early life Medicaid eligibility may have affected long-run outcomes for those whose mothers gained coverage. First, access to medical care during or following pregnancy may improve a child's health through the delivery of preventive care or the early detection and treatment of health conditions. Currie and Gruber (1996b) find evidence suggesting that pregnant women gaining eligibility were approximately half as likely to delay prenatal care. In addition, other studies (Dubay et al. 2001, Dave et al. 2008) find evidence of increased use or improved timing and adequacy of prenatal care among women of low-economic status who were most likely to be affected by the policy change. In a full review of the literature, Howell (2001) concludes that the weight of evidence points to a clear increase in Medicaid coverage and improvements in the use of prenatal care services among low-income women under the Medicaid expansions.

There is also strong evidence of increased utilization of medical technology and obstetric procedures during childbirth associated with the expansions. Currie and Gruber (2001) find increased use of a variety of obstetric procedures among pregnant women most likely to gain coverage under the Medicaid expansions. The authors estimate that eligibility had positive effects on the occurrence of cesarean section delivery, use of a fetal monitor, induction of labor, and receipt of an ultrasound among women who were unlikely to have had private insurance coverage before the Medicaid expansions. Dave et al. (2008) also find an increase in cesarean section delivery, although no change in the likelihood of delivery in a public hospital or in hospital length of stay. In general, there is evidence linking medical intervention at birth to better short- and long-run outcomes (Almond et al. 2010; Bharadwaj, Loken, and Neilson 2013; Almond, Guryan, and Mazumder 2014; Chay, Guryan, and Mazumder 2009).

Second, while there was a clear rise in the use of medical care under the expansions, other types of prenatal care may be important for the long-term health and development of the child. Prenatal interventions related to nutrition and breastfeeding, smoking cessation, and other healthy behaviors, as well as education regarding pregnancy and parenting, may have important consequences for healthy child development and later life outcomes. Table 1 provides information on the average experience of a pregnant woman receiving Medicaid-funded prenatal care during this time period.[5] In addition to the receipt of multiple medical services, these women were highly likely to receive guidance related to nutrition and weight gain during their pregnancies, as well as instructions to cut down or stop usage of alcohol, tobacco, and illegal drugs. Prenatal care may also provide important ties to social support services (Alexander and Kotelchuck 2001). For example, three-quarters of pregnant women receiving Medicaid-funded prenatal care in 1988 report that they received WIC during their pregnancy (Table 1). Forty percent report that they learned about the WIC program from a doctor, nurse or health provider.[6]

Third, the gain in insurance coverage may improve the mental health of the mother and, in this way, influence the well-being of the child. Recent findings from the Oregon Health Insurance Experiment show significant improvements in mental health and overall well-being among those

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[5] Calculated by the authors using the 1988 National Maternal and Infant Health Survey, a national followback survey of women experiencing live births and fetal and infant deaths in 1988 conducted by the National Center for Health Statistics.

[6] This is consistent with Joyce (1999), who finds increased enrollment in the Special Supplemental Food Program for Women, Infants, and Children (WIC) associated with participation in enhanced prenatal care initiatives adopted under Medicaid expansions in New York over this period.

gaining health insurance (Finkelstein et al. 2012). If mothers feel better off with insurance and this makes them less stressed or anxious, there could be important repercussions for the health and development of the child. Hypothesized to influence fetal neurodevelopment and growth, a significant body of research links prenatal maternal stress to adverse birth outcomes, as well as longer-run effects on physical and mental health (see discussion in Beydoun and Saftlas 2008; Schetter and Glynn 2011).

Finally, the expansions in Medicaid coverage may have influenced child outcomes by improving material well-being. For women who previously paid for private insurance coverage or out-of-pocket for health services, Medicaid coverage for pregnancy and infant-related care may free up household resources for other investments with long-term impacts for children. Gruber and Yelowitz (1999) document decreased household saving and increased consumption associated with expansions in Medicaid for children over the 1984-1993 period. Leininger, Levy, and Schazenbach (2010) find increased household expenditures among low-income families under later public insurance expansions for children, while Gross and Notowidigdo (2011) document reductions in personal bankruptcies. In addition, by providing access to health insurance that is not linked to employment, expansions in coverage may influence maternal labor supply decisions with potential consequences for both the pregnancy and home investment. Reductions in maternal employment among unmarried pregnant women have been linked to the Medicaid expansions in prenatal care (Dave et al. 2013).

Each of the mechanisms described above might affect health at birth, as well as latent health. Studies of the Medicaid expansions for pregnant women mainly examined two measures of infant health: birth weight and infant mortality. Currie and Gruber (1996b) find a significant 8.5 percent decline in the infant mortality rate associated with the expansions occurring between 1979 and 1992. They find a smaller and less significant reduction in the incidence of low birth weight of 1.9 percent. When restricting the analysis to eligibility changes that occurred for the lowest income women over this period– those with incomes below AFDC levels–the authors find much stronger effects for both measures of infant health. Evidence from other studies (Levine and Schanzenbach 2009, Dave et al. 2008, Dubay et al. 2001) confirm that any effects on birth weight or the incidence of low birth weight were relatively small and concentrated among more disadvantaged groups of women. Meanwhile, Currie and Gruber (1997) and Currie and Grogger (1997) also find evidence of sizeable declines in infant or fetal mortality associated with expanded Medicaid prenatal eligibility.

To our knowledge, there has been no study of any longer-run effects of the prenatal expansions on health or human capital outcomes. However, early life intervention may influence the baby's development and functional capacity in ways not captured by birthweight or other available measures of health at birth. In addition, through the mechanisms described above, it is reasonable to expect that prenatal care experiences may continue to influence the health and behavior of both the mother and infant well after delivery. In the next section, we describe our empirical strategy to examine the effects of Medicaid prenatal eligibility on adult health and economic outcomes.

IV. Data

National Health Interview Survey

We use data from both survey and administrative sources to estimate the effects of prenatal and childhood Medicaid coverage on adult health. To document changes in subjective health, we analyze a restricted-use version of the National Health Interview Survey (NHIS). This nationally-representative survey is conducted annually by the U.S. Census Bureau and contains self-reported information on an individual's health status and use of health services. The survey includes year of birth and, in the restricted-use version, state of birth. We use data from years 1998 to 2014 of the National Health Interview Survey, and include only individuals born between 1979 and 1993 who are over the age of 18. Individuals in our sample are between the ages of 19 and 35. We exclude individuals born in Arizona since the state did not adopt a Medicaid program until 1982. However, our results are not sensitive to this exclusion (see later discussion in Section VII.d.).

Within sampled households in the NHIS, all members are asked a set of questions on physical limitations, self-reported health, and health care utilization that occurred within the last year. These responses are recorded in the "person" file. Among adult household members, a randomly selected subset ("sample adults") is given more detailed interviews. We use both the person file and the sample adult file to conduct our analysis. The outcomes we consider from the person file are the probability of reporting health status to be "very good" or "excellent" and whether the individual reports having any health-related limitations. From the sample adult file, we examine the individual's body mass index (BMI, calculated as mass in kilograms divided by height in meters squared), whether or not the individual is obese (BMI > 30), and the presence of a chronic health condition, all of which have been linked to the fetal environment and early life exposures.[7] We also examine a measure of psychological distress, the Kessler 6 (K6) scale, derived from six questions about the individual's recent experiences of depressive or anxiety symptoms.[8] Scores range between 0 and 24 with a higher score indicating higher severity of psychological distress.

The first panel of Table 2 displays the weighted mean for each of these outcomes in the NHIS sample. We observe that 75.5 percent of respondents report that they are in very good or excellent health and 5.7 percent report having any health-related limitations. The average BMI is 26.3 and 21.3 percent of respondents are obese according to their reported BMI. In addition, 24.6

[7] We opted to examine the presence of any chronic condition rather than the presence of any specific conditions given the low prevalence of reported health conditions at these ages. We construct a measure indicating the presence of a chronic health condition if the individual reported ever being diagnosed with asthma or emphysema, cancer, diabetes (excluding gestational diabetes), ulcer, heart trouble (coronary heart disease, angina pectoris, heart attack, a heart condition or heart disease), or stroke; or if the individual was told they had chronic bronchitis, any kind of liver condition, or weak or failing kidneys during the past 12 months. The selection of these conditions is based on a list of self-reported conditions assembled by Chaudhry, Jin, and Meltzer (2005) to approximate the enumerated conditions of the Charlson Comorbidity Index. We were unable to include two conditions (arthritis and HIV) due to their inconsistent availability in the NHIS data.

[8] The K6 scale has been used by other studies to assess adult mental health, including Kling, Liebman, and Katz (2007). Alternative measures of adult mental health, such as depression, were not consistently available in the NHIS during our sample period.

percent of respondents report having at least one chronic health condition. The average score on the Kessler 6 psychological distress measure is 2.6.

Nationwide Inpatient Sample

In addition to the survey data, we also analyze administrative data on hospitalizations from the Nationwide Inpatient Sample (NIS) provided by the Healthcare Cost and Utilization Project. While contemporaneous Medicaid eligibility is usually associated with higher rates of health care utilization (e.g. Dafny and Gruber 2005, Finkelstein et al. 2012, Taubman et al. 2014), the effect of health insurance coverage in childhood on utilization later in life remains largely unexplored. Providing coverage in childhood may lower hospitalizations in adulthood by improving underlying health or helping in the management of chronic health conditions.

There are several advantages to using administrative data to examine health care utilization. First, administrative records are likely to present a more accurate picture of health care use. Self-reported healthcare utilization is subject to substantial recall bias (Bhandari and Wagner 2006). In addition, the accuracy of self-reported information varies by individual characteristics including health status with healthier individuals more likely to accurately report utilization (Short et al. 2009). Other advantages to using administrative hospital data are the large sample sizes and the ability to look more closely into the reason for hospitalization. For young adults, pregnancy and delivery care are overwhelmingly the most frequent reasons for hospitalization, representing over 60 percent of all visits. As described below, we are able to exclude hospitalizations related to delivery from the NIS analysis, as well as examine other types of hospitalization that may be influenced by both underlying health and access to medical care during the prenatal or childhood periods.

The NIS samples hospitals within a state, and provides discharge-level data on all hospital visits to sampled hospitals in each year. These data contain a sample of approximately 20 percent of all community hospitals among states that contribute to the project. In 1998, the first year of our sample, 22 states contributed to the NIS. By 2011, the last year of our sample, 46 states contributed.[9] Appendix Table 2 lists the states included in our sample in each year. As with the NHIS, we exclude Arizona from our sample. We observe about 2.6 million hospital visits, excluding hospitalizations related to pregnancy and delivery, from patients in the relevant birth cohorts who are over the age of 18. The data include information on diagnoses, procedures, patient demographics, and insurance status.

In our analysis of hospitalizations, we focus on visits for conditions that have been closely linked to the fetal environment. Using the International Classifications of Diseases (ICD) system, we classify visits as relating to endocrine, nutritional and metabolic diseases, and immunity disorders, as both the literature in epidemiology (e.g., Ravelli, Stein, and Susser 1976) and economics (e.g., Hoynes, Schanzenbach, and Almond 2012) suggests that early life health has a strong impact on these types of diseases later in life. We also specifically examine hospitalizations for diabetes and obesity, two common conditions within this category that have been linked to fetal health.[10] Finally, we analyze hospitalizations for mental health related diagnoses as previous studies (e.g., Ravelli, Stein and Susser 1976 and Kinsella and Monk 2009) have linked shocks during gestation to adult schizophrenia and major affective disorders.[11]

A limitation of the NIS is that it does not contain information on either birth year or birth state. We assign birth year probabilistically based on the age of the patient at the time of the visit and

[9] Beginning in 2012, NIS data are no longer available with state identifiers. Since we are restricted to data available before this year, we are only able to observe cohorts born between 1979 and 1991 in the NIS analysis.

[10] An earlier version of this paper also analyzed visits classified as "preventable" (i.e., ambulatory care sensitive). We found that in utero Medicaid coverage reduced ambulatory care sensitive visits, and particularly those related to chronic conditions. However, because diabetes is one of the most common chronic ambulatory-care sensitive visits, there is considerable overlap between these results and those presented in this version of the paper. These additional results on ambulatory care sensitive visits are available from the authors upon request.

[11] Both maternal stress (Kinsella and Monk 2009) and in utero exposure to famine (Hoek, Brown, and Susser 1998) have been linked to schizophrenia and affective disorders in adulthood. However, due to the low incidence of hospitalizations for these conditions, we are unable to analyze these specific types of mental illnesses. Instead, we analyze the effect of in utero Medicaid coverage on *all* hospitalizations related to mental illness.

the year and quarter during which the patient was admitted to the hospital.[12] We assign birth state to be the state in which the hospitalization took place. Given that a non-negligible share of each birth cohort moves from their state of birth, this treatment may be problematic for our analysis if it serves to attenuate our estimates of the relationship between Medicaid generosity at birth and later life outcomes, or if Medicaid eligibility or receipt influences sorting across states. To address this concern, we later run additional analyses to examine the mobility behavior of those affected by the Medicaid expansions using the American Community Survey, which has information on state of birth and state of residence. We find no significant association between the likelihood of an individual moving from his or her state of birth and expanded Medicaid eligibility (see later discussion in Section VII.a.).

We aggregate the total number of hospital discharges and the number of discharges by diagnosis group (all metabolic syndrome-related, diabetes and obesity, and mental health) over admission year periods by state and birth year cohort. In our analysis, we log each of these outcome measures. For some less populous states, there are some admission year, state, and birth year cohort cells with zero admissions for the diagnosis group measures. If a state has a cell with zero admissions for any year, we drop that state from the analysis.[13] As a result, although we use all states in our analysis of total hospitalizations, we have fewer state-year-birth cohort observations for less common diagnoses. As a sensitivity check, we report in the appendix results from models that use levels, rather than logs, as the dependent variable and use the full sample of states. These results are qualitatively similar to those discussed in the main text (see later discussion in Section VII.d.).

The second panel of Table 2 displays descriptive statistics from the NIS. Of the 2.6 million non-pregnancy-related hospital visits we observe, about 6 percent of all admissions are for endocrine, nutritional and metabolic diseases and immunity disorders, 4 percent are related to diabetes or

[12] We follow a method similar to that used in Rotz (2012). This procedure is described in greater detail in Appendix Section C. Because this method will result in some misclassification, it will bias our estimates towards zero.

[13] The excluded states are Alaska, Connecticut, Montana, Pennsylvania, Rhode Island, South Dakota, Vermont and Wyoming.

obesity, and 21 percent are related to mental health diagnoses. There are about 373 non-pregnancy hospitalizations per 10,000 individuals at these ages; about 22 of them are for hospitalizations related to endocrine, nutritional, and metabolic diseases and immunity disorders. Within this group, about 16 are related to diabetes and obesity. The hospitalization rate for mental health disorders is 77 per 10,000 individuals.

American Community Survey

To examine the impact of these expansions on human capital outcomes, we use data from the 2000 to 2013 years of the American Community Survey (ACS) downloaded using the integrated public use microdata series (Ruggles et al 2015). Similar to the other analyses, we exclude individuals born in Arizona and restrict the sample to individuals born between 1979 and 1993. Using these data, we analyze high school graduation rates and an indicator that the respondent has had some college (for respondents age 20 and older) and whether or not the respondent has a college degree (for respondents age 23 and older). For respondents ages 23 and older, we also consider total personal income in levels and total personal income in logs for those respondents who report strictly positive income. Total personal income includes total pre-tax personal income or losses from all sources for the previous year. We also examine whether anyone in the respondent's household was a recipient of Food Stamp benefits. For the analysis of income, we exclude those currently enrolled in school. The final panel of Table 2 presents weighted descriptive statistics from the American Community Survey for respondents born between 1979 and 1993. About 92 percent have graduated high school and 55 percent have some college attainment. Among those age 23 and older, about 38 percent have a college degree and 16 percent live in a household that receives Food Stamps benefits. Average annual income among those 23 and older (excluding those enrolled in school) is $25,343 in inflation-adjusted 2013 dollars.

V. Empirical Strategy

a. IV strategy

To examine the effects of Medicaid prenatal eligibility on adult health and human capital, we regress individual-level outcomes from the NHIS and ACS and cohort-level outcomes from the NIS on measures of state-level eligibility for each birth cohort. Following Currie and Gruber (1996b), we estimate the fraction of women of reproductive age (15-44) who would have been eligible for coverage if they became pregnant in each state and year during the 1979-1993 period. Eligibility is calculated using detailed federal and state Medicaid eligibility rules and individual information on state of residence, family structure, and income from the Current Population Survey (CPS) March Supplement for each year (see Appendix for additional information on criteria used to determine eligibility).[14]

Changes in Medicaid eligibility may be driven by state-level changes in Medicaid policy (that are arguably exogenous to later life outcomes), or they may be driven by changes to sociodemographic characteristics, both of which may affect health and well-being over the life cycle. For example, the share of the state population eligible for Medicaid will increase if the Medicaid policy becomes more generous, but it will also increase if average income in a state falls (e.g., because of a recession) and more state residents earn an income below the Medicaid eligibility threshold. To isolate variation in eligibility that results only from changes to policy, we follow the innovation of Currie and Gruber (1996a, 1996b), as well as Cutler and Gruber (1996), by instrumenting for state-level changes in Medicaid eligibility with an index of the generosity of state Medicaid rules in order to identify changes in outcomes related to Medicaid policy. This index is constructed by applying state eligibility rules to a national sample of 3,000 women from each year. This nets out any changes in state demographic or economic characteristics that influence state-level eligibility, allowing us to isolate the variation that is due

[14] We use the 1980-1994 CPS survey years since income information is for the previous calendar year.

only to changes in Medicaid policy. This “simulated eligibility” approach has since been adopted by many studies to examine changes in outcomes resulting from expanded public health insurance eligibility.

As mentioned earlier, we interpret these measures as representing Medicaid eligibility during pregnancy but also during the first year after birth. Under the expansions, children born to mothers covered by Medicaid were automatically deemed eligible for coverage until their first birthday. For that reason, we do not separately estimate the effects of prenatal eligibility and eligibility before the age of one.

States with more generous prenatal coverage may offer better coverage for children at other ages. For this reason, we also construct measures of public health insurance eligibility at ages 1-4, 5-9, 10-14, and 15-18 for each birth year and state. For a given birth year, we calculate the fraction of children eligible for coverage at each age during childhood in each state.[15] We sum the fraction eligible across ages to construct cumulative measures of public eligibility over four different age ranges for each birth year and state. These measures represent the average number of years of public eligibility for a given age range.

Since state-level eligibility during childhood may also be influenced by changes in state demographics or economic conditions, we construct instruments that capture differences in eligibility resulting from state-specific eligibility criteria. These simulated childhood eligibility measures are constructed in a manner similar to that for simulated prenatal eligibility. First, we draw a national sample of 1,000 children at each age during childhood for a given birth year. We then estimate the fraction of this national sample that would have been eligible for coverage in each state in order to create state-age-birth year measures of eligibility. Again, we sum the fraction eligible across ages in order to construct cumulative measures of eligibility for each age range.

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[15] Since year of birth is not available in the CPS, we estimate the individual’s birth year as the calendar year minus age.

b. Regression specifications

We first examine the effects of Medicaid eligibility during pregnancy and childhood on adult outcomes using a range of self-reported measures from the NHIS and the ACS. Our regression model is given by

$$y_{ibsy} = \beta_s + \beta_b + \beta_y + \beta_1\, Prenatal_{bs} + \beta_2\, Elig\ Age\ 1-4_{bs} + \beta_3\, Elig\ Age\ 5-9_{bs} + \beta_4\, Elig\ Age\ 10-14_{bs} + \beta_5\, Elig\ Age\ 15-18_{bs} + \beta_6\, X_{ibsy} + \beta_7\, Z_{bs} + \delta_s b + \varepsilon_{ibsy}$$

where each outcome y_{ibsy} for individual *i* observed in survey year *y* is regressed on prenatal and childhood eligibility measures corresponding to their state of birth *s* and year of birth *b*. We also include individual-level control variables X_{ibsy} (race, ethnicity, sex, and age dummies), state of birth, year of birth, and survey year dummies.[16] A vector of additional variables Z_{bs} control for time-varying state-specific characteristics that may be related to birth outcomes. These variables include state demographic (population age distribution, marital status, educational attainment, race) and economic characteristics (inflation-adjusted per capita income, unemployment rate), as well as select policy variables (inflation-adjusted maximum welfare benefit for a family of 4, parental requirements for involvement in minor abortions, and Medicaid funding restrictions for abortion) in the state for each birth year cohort.[17] Finally, our preferred specification additionally includes state-specific linear trends in birth year, $\delta_s b$, to account for any linear trends across

[16] Due to the rolling interview design of both the NHIS and ACS surveys, we are able to separately control for survey year, year of birth, and age dummies in our model.

[17] For each state and year, we construct variables indicating the share of the population that is married, black, or other race; the share of adults that are high school dropouts, high school graduates, or have at least some college; and, the percent of the population that is age 0-4, 5-17, 18-24, 25-44, 45-64, and 65 and older using March Current Population Survey data. We use data on the unemployment rate by birth year from the Bureau of Labor Statistics and data on per capita income from the Bureau of Economic Analysis Regional Economic Information System. Information on state abortion policies were taken from Kearney and Levine (2012), while the maximum welfare benefit for each state and year was drawn from the University of Kentucky Center for Poverty Research (2014). Since we were unable to locate the maximum welfare benefit for 1979, we assume that the benefits in place in 1980 were also in effect the year prior.

cohorts in the outcome variable that vary by state. This allows for different trends in adult outcomes by state that are unrelated to the Medicaid expansions. All analyses with the NHIS and ACS data employ survey weights. Standard errors are heteroskedasticity-robust and clustered by state of birth.

To analyze the effect of early life Medicaid coverage on adult hospitalizations, we estimate

$$\log(y_{bsy}) = \beta_b + \beta_s \times \beta_y + \beta_1\, Prenatal_{bs} + \beta_2\, Elig\ Age\ 1-4_{bs} + \beta_3\, Elig\ Age\ 5-9_{bs} + \beta_4\, Elig\ Age\ 10-14_{bs} + \beta_5\, Elig\ Age\ 15-18_{bs} + \beta_6\, Z_{by} + \delta_s b + \varepsilon_{bsy}$$

where y_{bsy} is the total number of hospitalizations for a given birth year b and state s in admission year y. In addition to prenatal and childhood eligibility measures for each birth year and state, we include birth year dummies to control for fixed differences in hospitalizations across cohorts. Because different hospitals within a state are sampled each year in the NIS, we also include state by admission year fixed effects ($\beta_s \times \beta_y$). Combined with the birth year fixed effects, these state-by-admission year fixed effects account for any differences in hospitalization rates that vary by age. These models estimate the change in the number of admissions observed, which might be affected by the size of the birth year cohort in each state. To account for this, we include birth cohort size as a control variable along with other time-varying state characteristics by birth year and the population of the state by admission year and age.[18] As in the previous section, this vector of variables (Z_{by}) includes controls for the age composition, education composition, race composition, per capita income, unemployment rate, generosity of the welfare program, and abortion policies of the state in birth year b. In our preferred model, we also include state-specific linear trends in birth year, $\delta_s b$, although we also present models without such trends. For all models, standard errors are heteroskedasticity-robust and clustered by state.

[18] To the extent that expansions of the Medicaid program lowered mortality, our model will under-estimate the effect of Medicaid on hospitalizations later in life. In addition, any reductions in mortality earlier in life due to Medicaid may bias us against finding a positive effect on adult health or economic outcomes if they led to a longer lifespan for less healthy individuals.

Each of these models contains five endogenous variables: prenatal Medicaid eligibility (which we interpret as capturing both in utero and infant coverage) and cumulative eligibility occurring between the ages of 1 and 4, 5 and 9, 10 and 14, and 15 and 18. We use simulated prenatal eligibility and simulated cumulative eligibility over each age range during childhood as instruments for these endogenous variables.

VI. Results

a. First stage

Table 3 presents the first stage results in the NHIS, NIS, and ACS data. In the first five rows of each column, we report the coefficients on each of the simulated instruments. Below these columns, we report the Angrist-Pischke (AP) F-statistic (Angrist and Pischke 2009) for the specified sample and endogenous variable. This statistic is constructed for each endogenous variable by first regressing each instrument on the other four endogenous variables and collecting the residuals. The AP F-statistic is the standard F-statistic from a regression where these residuals are included as an explanatory variable and the remaining endogenous variable is the dependent outcome. In this way, the AP F-statistic asks whether there is enough variation in the instruments to explain the endogenous variable after accounting for the fact that the variation in the instruments will also be used to predict the other four endogenous variables. In the final row, we report the Kleibergen-Paap rank statistic (Kleibergen and Paap 2006). Whereas the AP F-statistic measures the first stage for each endogenous variable separately, the rejection of each null hypothesis separately does not require that there is enough variation to identify the structural parameters when taken together. The Kleibergen-Paap rank statistic tests whether the full set of instruments is able to identify the full set of structural parameters.

As seen in Table 3 for all samples, each simulated eligibility measure is strongly predictive of its corresponding measure of actual eligibility. In addition, both the AP F statistics and the Kleibergen-Paap rank statistic indicate that there is a meaningful amount of variation in state and birth-year cohort eligibility that results from changes in state Medicaid policy rather than changes in the sociodemographic composition of the state.

b. NHIS results

Table 4 reports the coefficient estimates for each period of Medicaid eligibility during childhood for adult health outcomes. The first two outcomes in the table are perceived health status and the presence of any health limitations (columns 1-4). We find no evidence of an impact of eligibility during the prenatal period, although we do find that Medicaid eligibility between the ages of 5 and 9 is associated with an increase in the probability of reporting that health is very good or excellent.

The next two outcomes are BMI and obesity (columns 5-8). While we do not find evidence of a significant impact of prenatal Medicaid eligibility on BMI, we do see a decrease in obesity. In our preferred model that includes state-specific linear trends as well as individual and cohort-level control variables, the coefficient estimates indicate that a 10-percentage-point increase in prenatal eligibility is associated with a 1.7 percentage-point reduction in the likelihood of adult obesity, which represents an 8 percent decrease over the sample mean.

Finally, we examine the presence of chronic health conditions and psychological distress as measured by the Kessler 6 scale (columns 9-12). We do not find any consistently significant evidence of an impact of prenatal Medicaid eligibility or eligibility during other points in childhood on these outcomes, although there is some evidence of a decrease in the presence of chronic health conditions associated with eligibility at ages 5-9. In addition, while not statistically significant, the coefficient estimates indicate that prenatal eligibility is associated with improvements on both of these measures.

The coefficients reported in the first row of Table 4 can be interpreted as the intent-to-treat effect of Medicaid coverage in utero on later life health. Similarly, the coefficients describing eligibility during childhood can be interpreted as the intent-to-treat effect of an additional year of coverage during each age period. Assuming that eligibility only affects the health of those who actually enroll in Medicaid, we can use these estimates to back out the implied treatment effect of enrollment in the Medicaid program during the prenatal period. Currie and Gruber (1996b) report that about 30 percent of women who gained eligibility over this period actually enrolled in Medicaid. We therefore scale our coefficients by 1/0.30 to provide a back-of-the-envelope calculation of the implied treatment effect of individual Medicaid enrollment. Our results imply that in utero Medicaid coverage decreased the probability of being obese in early adulthood by 56 percentage points.[19] Although this estimate is quite large, the confidence intervals on our coefficient estimates include smaller sized effects. Furthermore, if take-up of Medicaid prenatal coverage was greatest among the most at-risk mothers, this would explain an estimate that is potentially much larger than what the effect would be if a typical pregnant woman who became Medicaid-eligible were induced to enroll.

c. NIS results

Table 5 presents the results from the analysis using administrative hospital records. We find no significant effect of prenatal Medicaid coverage on adult hospitalizations overall (columns 1-2). However, we do find a statistically significant reduction in later life hospitalizations associated with access to coverage between the ages of 1 and 4. Our point estimates indicate that expanding eligibility by ten percentage points for one year in childhood at these ages would reduce hospitalizations as adults by between 1.1 and 1.5 percent.

Columns 3 and 4 display the effects of prenatal and childhood Medicaid eligibility on hospitalizations for endocrine, nutritional and metabolic diseases, and immunity disorders.[20] We

[19] A potentially appealing exercise would be to scale our reduced form coefficients by the change in overall insurance coverage rather than Medicaid coverage to arrive at a treatment effect of any insurance coverage on health. However, because Medicaid coverage could affect fetal, infant, and child health through mechanisms other than moving mothers from being uninsured to insured (e.g., by crowding out more expensive private insurance and thus increasing the available household resources), we believe this approach would ultimately be misleading.

[20] We group these diseases together following the ICD classification system.

find that a ten-percentage point increase in prenatal Medicaid eligibility is associated with an 8 percent reduction in visits for this category of diseases in adulthood. We further narrow our analysis within this category of diseases by looking only at hospitalizations related to diabetes and obesity (columns 5-6). Here, we find that a ten-percentage point increase in prenatal Medicaid eligibility is associated with a 9 to 11 percent reduction in hospitalizations in this category. We find no statistically significant effect of Medicaid eligibility during other periods of childhood on these types of hospitalizations later in life, although our confidence intervals cannot rule out small but meaningful changes. Finally, we find no statistically significant effect of in utero Medicaid coverage on hospitalizations for mental health related diagnoses (columns 7-8).[21]

We can perform a back-of-the-envelope calculation to translate these effects into the total cost of the hospitalizations that were avoided as a result of the Medicaid expansions to pregnant women. On average, there are about 21.5 visits for endocrine, nutritional and metabolic diseases, and immunity disorders per 10,000 individuals in this age group each year. Our estimates imply that a thirty-percentage point increase in prenatal Medicaid eligibility (approximately the size of the increase in eligibility that occurred between 1979 and 1991) reduced utilization for these types of visits by about 24 percent annually, resulting in about 5.2 fewer hospitalizations for these diagnoses per 10,000 individuals per year. Put differently, for every ten thousand pregnant women who gained eligibility, there were about 18 fewer hospitalizations for diseases in this category each year. If we scale these results by the 30 percent take-up rate, our estimates imply that for every ten thousand women who actually enrolled in Medicaid, there were about 60 fewer hospitalizations annually for endocrine, nutritional and metabolic diseases and immunity disorders.

[21] We also explored running the hospitalization models using only states that participated in the NIS for all 17 years. However, this exclusion greatly reduces the number of state-year observations by approximately half and drops the number of states from 46 to 20. We continue to observe negative, although not statistically significant, effects of prenatal coverage on diabetes and obesity hospitalizations and hospitalizations related to endocrine, nutritional and metabolic diseases in these models; however, the standard errors are over twice as large and we are no longer powered to detect effects that are of similar magnitude to those reported in Table 5.

We can also compare the cost of additional Medicaid prenatal coverage for a single birth cohort to the later savings in avoided hospitalizations. Currie and Gruber (1996b) estimate that each additional woman made eligible for Medicaid led to a $202 increase in Medicaid expenditures in 1986 dollars. When applying a 3 percent real discount rate, the total discounted cost of the thirty-percentage point increase in eligibility for Medicaid prenatal care for the last cohort in our sample was approximately $501.7 million in 2011 dollars.

In total, our results suggest that a 30 percentage point increase in eligibility would result in 2,014 fewer hospitalizations per year for this cohort,[22] or a total of 28,196 fewer hospitalizations between the ages of 19 and 32. The average amount charged for a hospitalization in this category for this age group is approximately $27,000. We deflate this amount charged with hospital-specific "cost-to-charge" ratios developed by HCUP to measure the resource costs of a hospital visit. We find that the cost of each visit to the hospital is about $8,500. This implies that a 30 percentage point increase in Medicaid prenatal eligibility is associated with a total discounted benefit of Medicaid eligibility for this cohort at ages 19 to 32 of $193.1 million in 2011 dollars. The amount saved during these ages therefore represents approximately 38 percent of the total cost associated with the increase in Medicaid prenatal care for this birth year.[23] In addition, it is reasonable to expect that the potential cost savings will increase over time if these effects continue or grow larger as these cohorts continue to age. The types of chronic conditions that have generally been linked to the early life environment tend to appear starting in the middle age years.

[22] As calculated in the previous paragraph, a thirty percentage point increase in eligibility is associated with 5.3 fewer hospitalizations per 10,000 individuals each year. With approximately 3.8 million individuals per birth cohort, this is 0.00053 x 3.8 million = 2,014 fewer visits annually.

[23] This calculation estimates the implied benefits and costs associated with the 30-percentage point increase in eligibility for the 1991 birth year cohort. We apply a 3 percent real discount rate to both the original cost of Medicaid and the future expected benefits in the form of avoided costs associated with reduced hospitalizations at ages 19-32 for this cohort. We opted not to further inflate hospitalization costs despite that the estimated decrease in hospitalizations at ages 19 and 32 will occur between 2010 and 2023 for this cohort, which should serve only to attenuate our estimate of total benefits.

d. ACS results

We report the effects of early life Medicaid eligibility on education, earnings, and public assistance receipt in Table 6. Columns 1 through 6 display results related to educational attainment. We find that prenatal Medicaid eligibility is associated with a statistically significant increase in high school graduation rates. In our preferred model, our point estimate indicates that a 10 percentage point increase in prenatal eligibility increased the probability a respondent will have graduated high school by about 0.2 percentage points, an increase in the graduation rate of about 0.2 percent. Scaling these effects by a 30 percent take up rate, our estimate implies that gaining Medicaid coverage increased the probability an individual graduated high school by 6.7 percentage points, an increase in the graduation rate of about 7.3 percent. We find no statistically significant effects of prenatal eligibility on the probability that the respondent has attended some college or received a college degree, although the coefficient estimates are positive in sign. We do not find consistently significant effects of eligibility at other ages on any of these outcomes.

These findings are broadly consistent with recent work showing improvements in educational attainment associated with childhood eligibility for Medicaid but diverge in some details. Cohodes et al. (forthcoming) examine average Medicaid eligibility at ages 0-17 for cohorts born between 1980 and 1990 and also find evidence of increased high school completion using ACS data. The authors estimate a 0.4 percentage point decrease in high school dropout associated with a 10-percentage point increase in average childhood Medicaid eligibility. These authors did not examine expansions in Medicaid prenatal eligibility that affected these cohorts. Of note, we estimate a change in high school completion of almost a similar magnitude associated with a 10-percentage point increase in prenatal eligibility alone. We do not find significant evidence of an impact of eligibility at other ages on high school completion and our estimates are close to zero. Also different from these authors, we do not find evidence of increases in college enrollment or attainment of a four-year degree. Brown, Kowalski, and Lurie (2015) also find evidence of increased college enrollment among females associated with childhood Medicaid eligibility using administrative Internal Revenue Service data.

Columns 7-10 of Table 6 report the estimated effects of childhood Medicaid eligibility on total annual personal income both in levels and logs. Models where the dependent variable is log of personal income exclude individuals reporting zero or negative values for total personal income. In the model of levels, we find a significant increase in personal income in the specification that includes state specific linear trends. This result implies that a 10-percentage point increase in prenatal eligibility is associated with an increase in personal income of about $285 in 2013 dollars for those in utero at the time of the expansion. In the model of log income, we find that a 10-percentage point increase in prenatal eligibility increased the average income of the affected cohorts by 1.3 to 1.5 percent, depending on the specification. The coefficient estimate under the model without state trends is only significant at the 10 percent level, however. We also find significant evidence that eligibility at ages 5-9 is associated with increased income in adulthood, although the coefficient estimates are smaller in magnitude. The estimates indicate that a 10-percentage point increase in eligibility at one of these ages lead to a 0.3-0.4 percent increase in income. While we do observe some significant effects for other ages on these two outcomes, none of them is consistently significant across the two models.

Finally, we evaluate how Medicaid expansions affected receipt of Food Stamps benefits (columns 11-12). The coefficient on prenatal eligibility is strongly significant and negative in the model including state-specific trends. It indicates that a 10-percentage point increase in Medicaid prenatal eligibility decreased the probability of receiving Food Stamps benefits by 0.6 percent (a 3.8% decrease over the mean). The estimate in the model without state trends, however, is smaller and not significant.

Although the findings for income and Food Stamp receipt are sensitive to specification choice, the point estimates in the models with state trends suggest that there are significant changes in both of these outcomes. Scaling these coefficients by a 30 percent takeup rate implies that gaining Medicaid coverage in utero increased an individual's average income by $9,483 in 2013 dollars and decreased their likelihood of receiving Food Stamp benefits by 18.3 percent.

VII. Alternative Specifications and Robustness Checks

a. State of residence vs. state of birth

As discussed earlier, a limitation of the use of the NIS is that information on the patient's state of birth is not available. In our analysis, we thus necessarily assign birth state to be the state in which the hospitalization took place. If mobility patterns are unaffected by exposure to Medicaid at birth, the lack of state of birth information in the NIS should serve only to attenuate our estimates of the relationship between early life Medicaid generosity and later life health. The main threat to the interpretation of our NIS estimates is if exposure to Medicaid in childhood influences mobility decisions and sorting across states in a manner that biases us toward finding positive effects on later life health. To investigate whether early life Medicaid exposure influences the decision to move out of state, we ran our regression specification using our ACS sample with an indicator for whether an individual moved from his or her state of birth as the dependent variable. These results are reported in Appendix Table 3. We find no statistically significant relationship between prenatal Medicaid eligibility and the probability of moving to a different state. Indeed, the small point estimate we uncover indicates that those who gained prenatal eligibility are less likely to move as young adults.

b. Adult eligibility

In the previous sections, we demonstrated that expansions of Medicaid coverage for prenatal services resulted in decreased obesity and fewer hospitalizations related to metabolic disorders for adults who were in utero during the expansions. One threat to our identification strategy may arise if birth cohorts who experienced more generous Medicaid coverage in early childhood also were more likely to benefit from public insurance expansions as adults if they reside in states with more generous Medicaid policies. If this is the case, the observed improvement in health associated with prenatal coverage may be instead capturing more generous contemporaneous coverage for these birth cohorts.

In order to control for this possibly confounding relationship between in utero coverage and coverage as an adult, we construct two measures of adult Medicaid eligibility by state and birth year cohort. First, we control for contemporaneous Medicaid eligibility (i.e., eligibility during the year we observe each birth year cohort in the NHIS or NIS data) in our models. Second, we control for average cumulative adult Medicaid eligibility (i.e., the average number of Medicaid eligible years in adulthood divided by the total number of years of adulthood for a given birth year cohort). These measures vary by state, birth year cohort, and survey or admission year. As with our measures of childhood eligibility, we instrument for actual adult eligibility with simulated adult eligibility measures. Additional details on the construction of these variables are found in Section B of the Appendix.

Appendix Tables 4-6 report the results for models that include contemporaneous adult Medicaid eligibility and average cumulative adult eligibility for each of our datasets.[24] The inclusion of adult eligibility does not appreciably change the results we reported in Section VI.

c. Placebo Test: Appendicitis and Injury

In this section, we evaluate the impact of prenatal Medicaid coverage on two types of hospitalizations that we would not expect to be affected by the in utero environment: hospitalizations for appendicitis and injury. These are acute conditions that are not obviously amenable to either Medicaid coverage in childhood or improved fetal health and birth outcomes.

The results are reported in Appendix Table 7. We find no statistically significant effect of prenatal Medicaid eligibility on hospitalizations for appendicitis and injury and the point estimate is small relative to the effects observed for other categories of illnesses that have been shown to be sensitive to fetal health. In addition, Medicaid eligibility during other periods of childhood does not appear to be associated with significant declines in hospitalizations in this category.

[24] For simplicity, we present robustness checks for our preferred specification with state linear trends only.

d. Additional Specifications and Analyses

In our main analysis, we model the log of the number of hospitalizations by state and cohort using the NIS. Because some states have zero visits in some year groups, these states are dropped from the dataset. In the appendix, we report the results using the level, rather than the log, of hospitalizations by state and cohort. This does not require us to drop states from the sample. Estimating this model, we continue to find a statistically significant reduction in hospitalizations for metabolic disorders, as well as a reduction in hospitalizations for diabetes and obesity although this estimate is only significant at the 10 percent level. We no longer find significant evidence of a decrease in non-pregnancy related hospitalizations associated with childhood Medicaid eligibility at ages 1-4 (Appendix Table 8).

Second, we report results including Arizona in the analysis sample (Appendix Tables 9-11). Arizona is excluded from our main analysis because it did not adopt a Medicaid program until 1982. In addition, it is difficult to find information regarding whether the state had comparable benefits prior to its implementation of Medicaid. However, we modeled eligibility in Arizona prior to the establishment of its Medicaid program as comparable to the state's eligibility criteria for AFDC based on reports that the state did provide government-supported health care for AFDC families (Freeman and Kirkman-Liff 1985). When we include Arizona in our analysis sample using this constructed eligibility measure, it does not, for the most part, meaningfully alter the results. The coefficient estimate for the impact of prenatal eligibility on obesity is similar to in the main analysis; however, the coefficient is only significant at the 10 percent level.

Third, we collapse our four childhood eligibility measures into one variable that measures total cumulative number of years eligible for Medicaid during childhood. We estimate our IV model using this measure and our prenatal eligibility measure, resulting in two, rather than five, endogenous variables. The results are presented in Appendix Tables 12-14. We continue to find that prenatal eligibility is associated with lower obesity rates, although this estimate is only significant at the 10 percent level. As in our main specification, in this model we find significant reductions in hospitalizations related to endocrine, nutritional and metabolic diseases, and immunity disorders and hospitalizations due to diabetes and obesity associated with prenatal coverage. Additionally, we continue to find significant reductions in high school graduation rates and increases in income associated with prenatal Medicaid eligibility. However, in none of these models do we find significant associations between total cumulative eligibility in childhood and these outcome variables.

Finally, we also estimate the effect of Medicaid eligibility on self-reported utilization of medical care using the NHIS data. This analysis did not detect any significant effects of in utero or childhood Medicaid coverage on the likelihood of an overnight hospital stay, ER visit or reporting 10 or more health visits during the last 12 months. The coefficient estimates suggest a decrease in overnight hospital stays and ER visits associated with Medicaid prenatal eligibility but they are not statistically significant. These results are presented in Appendix Table 15. As described earlier, there are several limitations associated with survey data on health care utilization relative to administrative records, including smaller samples, recall biases, and the inability to separate pregnancy and delivery related hospitalizations (by far the most common hospitalization for this age group) from hospitalizations for diagnoses that are more likely to be affected by early life Medicaid coverage. For that reason, we use administrative hospital records in our main analysis of health care utilization.

VIII. Conclusion

During the 1980s, the Medicaid program underwent ambitious coverage expansions aimed at improving the health of pregnant women and children. In this paper, we use variation in the timing and size of these expansions across states to show that adults who benefited from the expansions in utero exhibit better outcomes today along several dimensions. We find that expanding Medicaid coverage to pregnant women resulted in lower rates of obesity and fewer hospital visits for metabolic and endocrine diseases during adulthood among cohorts who were in utero during the expansions. We also find that those who were in utero during the prenatal expansions have higher high school graduation rates, indicating that the improvements associated with the prenatal eligibility expansions extend beyond health, improving human capital. We also find some evidence of improvements in income and less reliance on public assistance in adulthood. These results indicate that public health insurance expansions have benefits that materialize years after their implementation. As the cohorts born during this time period continue to age, it will be possible to investigate whether there are even longer-term effects of this early intervention.

While a well-established literature has shown that the fetal environment has large effects on adult health and achievement, relatively few papers have established how public interventions affect long-term outcomes. This paper provides a link between the research on the early life origins of adult well-being and the broader discussion about the role of the government in providing health insurance coverage to low-income populations. Establishing evidence on the effectiveness of expanded health insurance coverage, as well as other interventions that influence early and later life health and achievement, is crucial for public policy decisions that aim to improve population health and well-being.

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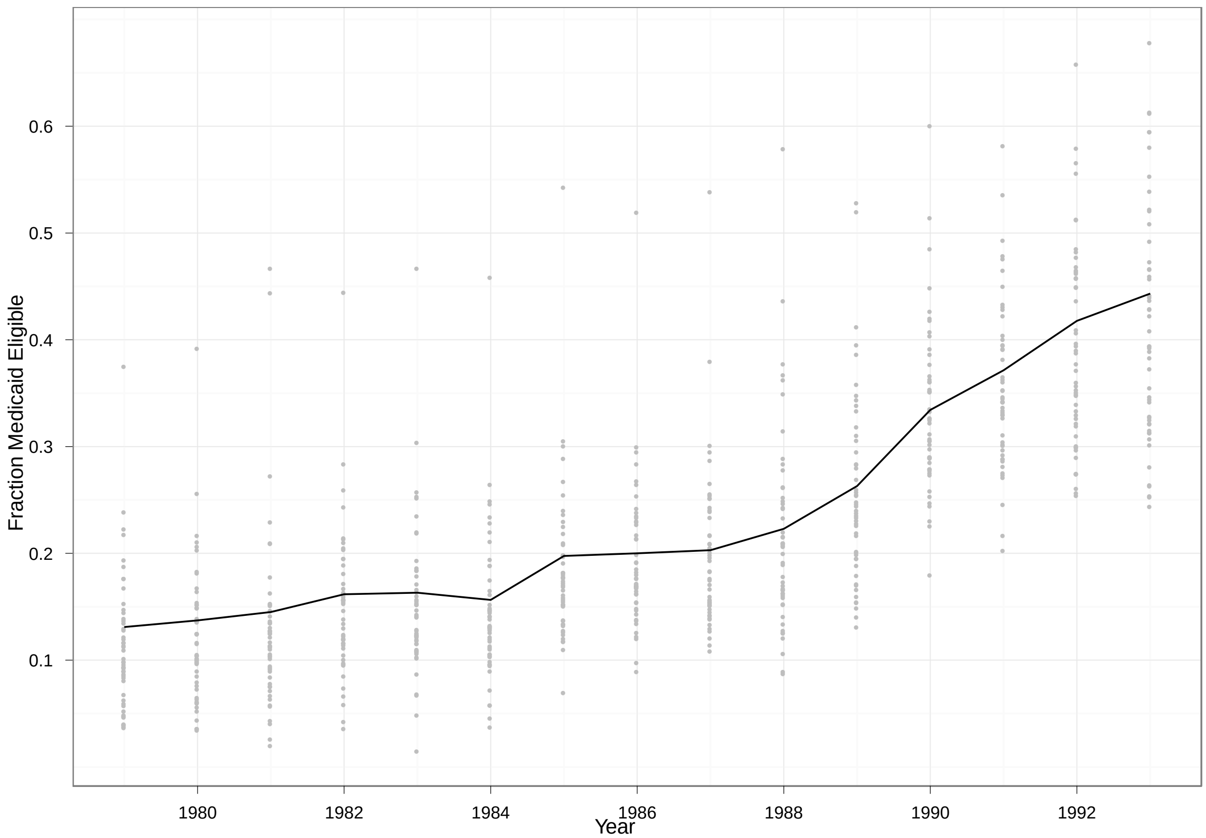
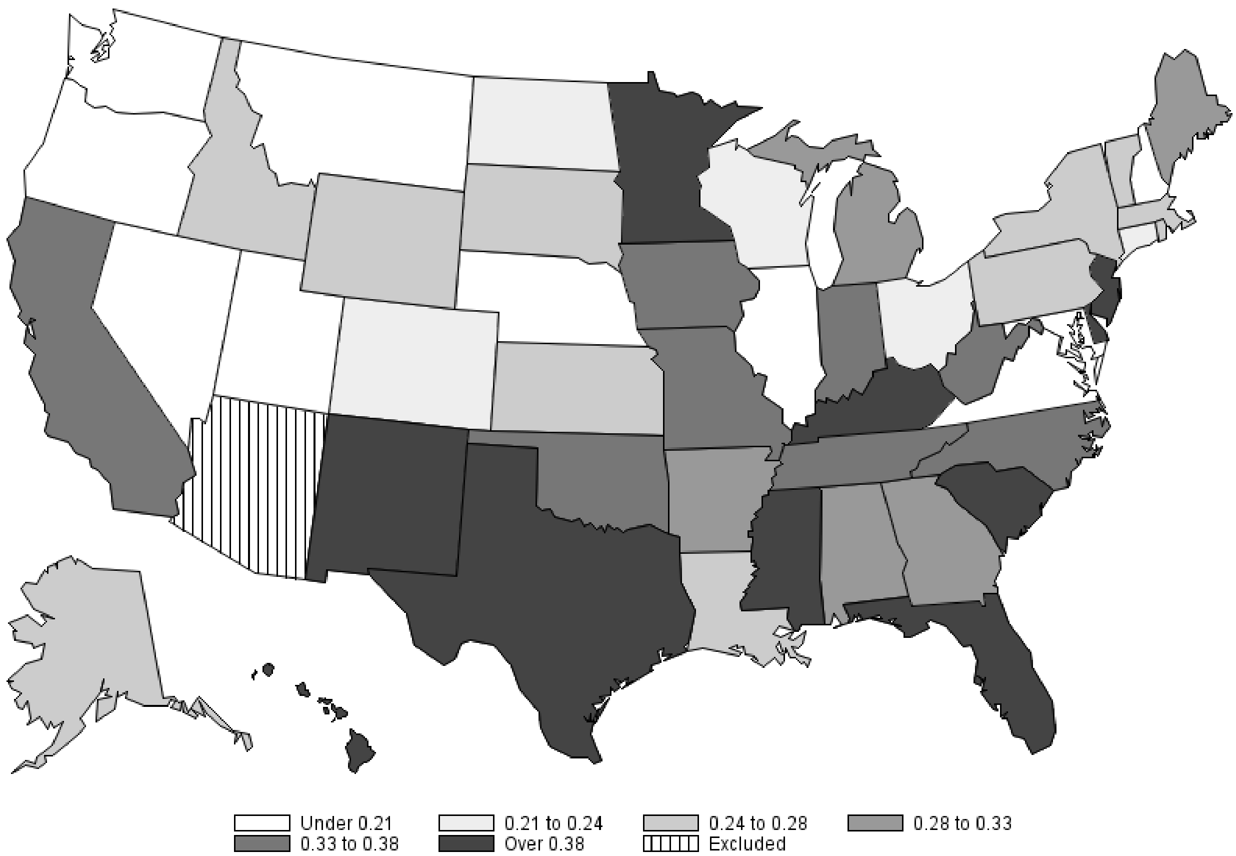
Figure 1. Fraction of women ages 15-44 eligible for Medicaid prenatal coverage in the event of a pregnancy, 1979 to 1993



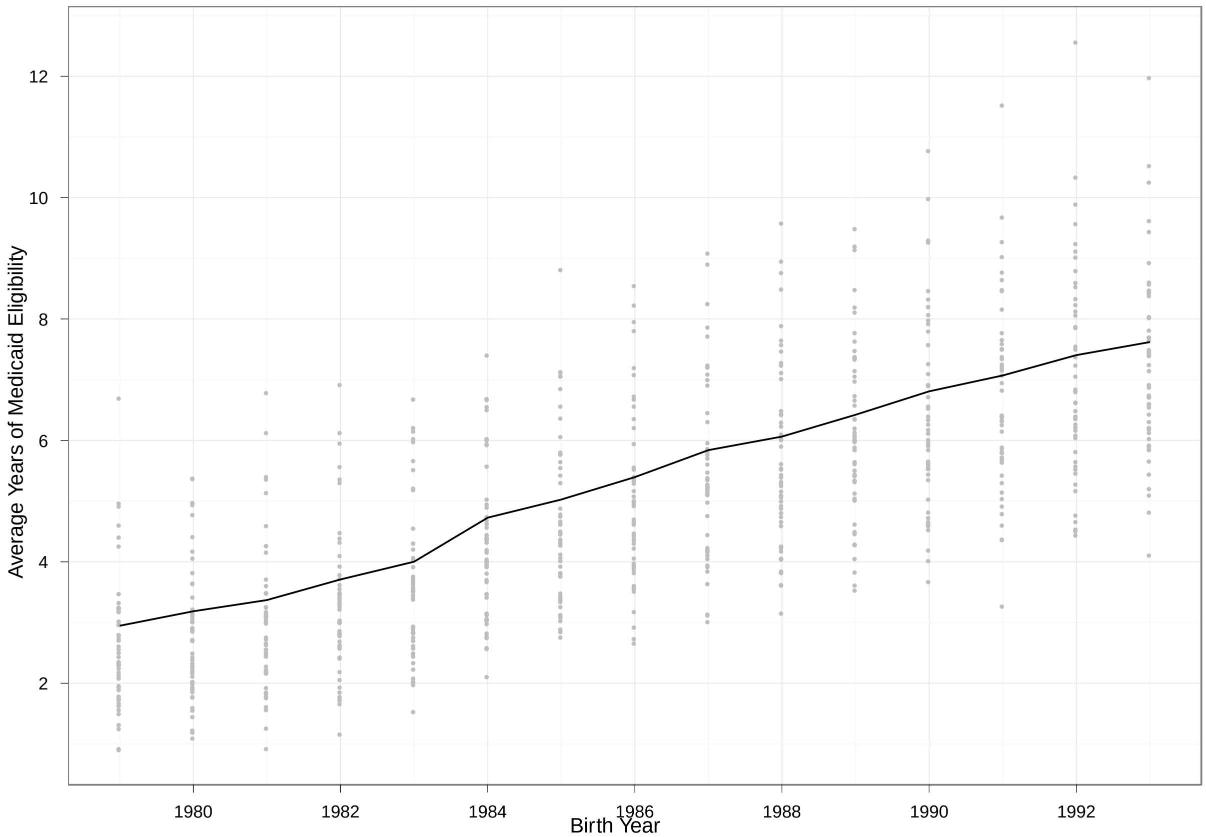
Source: Authors' calculations using Current Population Survey. See text and Appendix A for details.

Figure 2. Change in the fraction of women age 15-44 who would be eligible for Medicaid coverage in the event of a pregnancy from 1979 to 1993 by state.



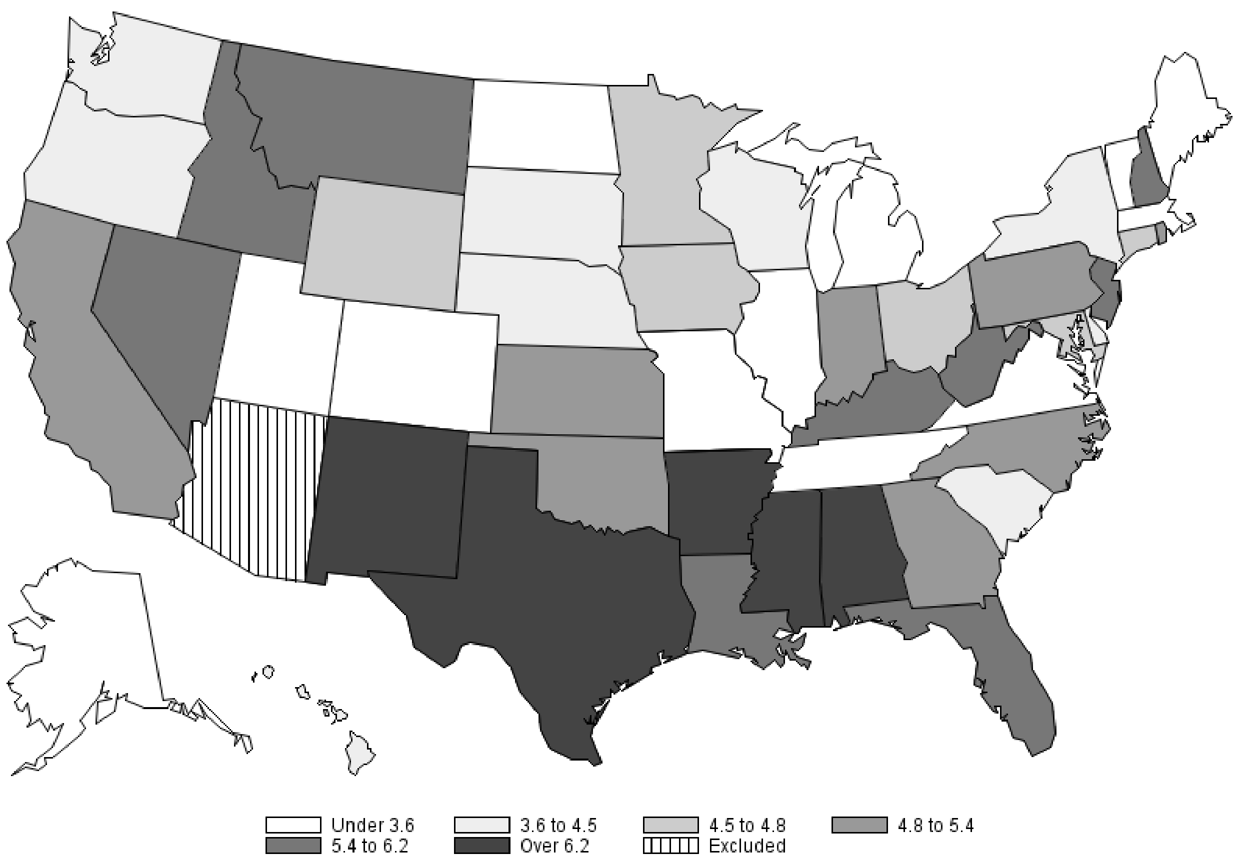
Source: Authors' calculations using Current Population Survey. See text and Appendix A for details.

Figure 3. Average number of Medicaid eligible years during childhood of cohorts born 1979 to 1993



Source: Authors' calculations using Current Population Survey. See text and Appendix A for details.

Figure 4. Change in the average number of Medicaid eligible years of cohorts born in 1979 to 1993 by state.



Source: Authors' calculations using Current Population Survey. See text and Appendix A for details.

Table 1. Average Experience of Pregnant Women Receiving Medicaid-Funded Prenatal Care, 1988

Characteristics of visits		**Services received from at least one prenatal care visit**	
# of weeks pregnant at first prenatal visit	11.02	Pregnancy test	0.70
Told by doctor, nurse or nutrition counselor how much weight to gain during pregnancy	0.70	Prenatal care checkups	0.96
		Lab tests	0.74
Told by doctor to stay in bed one or more weeks during pregnancy	0.24	Sonogram/ultrasound	0.60
		X-ray	0.07
Received WIC during pregnancy	0.76	Amniocentesis/CVS	0.06
Learned about the WIC program from a doctor, nurse or health provider	0.40	Emergency visits	0.17
Services received at first prenatal care visit		**Instructions received during at least one prenatal care visit**	
Pregnancy test	0.71	Take vitamin/mineral supplements	0.95
Blood pressure	0.90	Eat proper food during pregnancy	0.90
Pap smear	0.61	Try to breastfeed your baby	0.52
Urine test	0.89	Cut down/stop drinking alcohol	0.61
Blood test	0.74	Cut down/stop smoking	0.69
Weight/Measured	0.90	Not to use illegal drugs	0.68
Physical/Pelvic exam	0.73		
Health history	0.79		
Ultrasound/Sonogram	0.15		
Other	0.03		

Source: Weighted characteristics of women who reported Medicaid as paying for their prenatal care drawn from the 1988 National Maternal and Infant Health Survey. The dataset is a national file of women experiencing live births and fetal and infant deaths in 1988.

Table 2. Data Sources and Outcomes

National Health Interview Survey, 1998-2014 (1979-1993 Birth Cohorts)	
% unless otherwise stated	
Health is very good or excellent	75.5
Any health limitations	5.7
BMI, mean (SD)	26.3 (5.6)
Obesity	21.3
Presence of 1 or more chronic conditions	24.6
Kessler 6 psychological distress scale, mean (SD)	2.6 (3.4)
Nationwide Inpatient Sample, 1998-2011 (1979-1991 Birth Cohorts)	
Rates per 10,000 persons	
Any hospitalization (excluding those related to pregnancy and delivery)	373.0
Endocrine, nutritional and metabolic diseases, and immunity disorders diagnosis	21.5
Diabetes/Obesity	15.5
Mental Health	77.2
American Community Survey, 2000-2013 (1979-1993 Birth Cohorts)	
% unless otherwise stated	
High School Graduate	92.2
Some College	55.0
College Degree	38.2
Total Personal Income, mean (SD)	25,343 (27,063)
Food Stamp Receipt	16.0

Sources: All estimates from the NHIS and ACS are weighted. To calculate hospitalization rates, authors use population by single age from the 2010 Census and weighted national estimates from the NIS. Income values from the ACS are in inflation-adjusted 2013 dollars.

Table 3. First-Stage Estimates for Each Analysis Sample

	Prenatal eligibility	Eligibility at ages 1-4	Eligibility at ages 5-9	Eligibility at ages 10-14	Eligibility at ages 15-18
National Health Interview Survey					
Simulated prenatal eligibility	0.998***	0.550***	-0.112***	0.244***	0.397***
	(0.002)	(0.016)	(0.023)	(0.024)	(0.020)
Simulated eligibility at ages 1-4	-0.051***	1.150***	-0.019	-0.129	0.022**
	(0.001)	(0.008)	(0.009)	(0.129)	(0.009)
Simulated eligibility at ages 5-9	-0.030***	0.104***	0.774***	0.033***	-0.023***
	(0.000)	(0.004)	(0.006)	(0.008)	(0.005
Simulated eligibility at ages 10-14	-0.010***	0.095***	-0.154***	0.799***	0.111***
	(0.001)	(0.004)	(0.005)	(0.007)	(0.005)
Simulated eligibility at ages 15-18	-0.015***	-0.018	-0.182***	-0.007	1.084***
	(0.001)	(0.006)	(0.007)	(0.008)	(0.007)
AP F statistic	1451.05	124.16	122.35	44.42	83.63
Kleibergen-Paap Rank Statistic (p-value)			15.08 (0.0001)		
Nationwide Inpatient Sample					
Simulated prenatal eligibility	1.003***	0.416	0.102	-0.0463	0.274
	(0.0450)	(0.365)	(0.372)	(0.406)	(0.455)
Simulated eligibility at ages 1-4	-0.033	1.203***	-0.0276	-0.219	-0.210
	(0.0231)	(0.152)	(0.128)	(0.196)	(0.185)
Simulated eligibility at ages 5-9	-0.0225*	0.103	0.802***	-0.0261	0.0296
	(0.0121)	(0.0960)	(0.0771)	(0.151)	(0.114)
Simulated eligibility at ages 10-14	-0.00462	0.0897	-0.185**	0.837***	0.216*
	(0.00839)	(0.105)	(0.0786)	(0.150)	(0.128)
Simulated eligibility at ages 15-18	0.00122	-0.000978	-0.193**	-0.0765	1.242***
	(0.00900)	(0.122)	(0.0775)	(0.166)	(0.147)
AP F statistic	539.58	54.63	122.71	37.67	52.59
Kleibergen-Paap Rank Statistic (p-value)			12.65 (0.0004)		
American Community Survey					
Simulated prenatal eligibility	0.994***	0.625***	-0.0913	0.135	0.381
	(0.0256)	(0.201)	(0.345)	(0.255)	(0.289)
Simulated eligibility at ages 1-4	-0.0488***	1.150***	0.000699	-0.108	0.00141
	(0.0117)	(0.101)	(0.137)	(0.191)	(0.140)
Simulated eligibility at ages 5-9	-0.0270***	0.129**	0.780***	0.0804	-0.00933
	(0.00828)	(0.0497)	(0.0746)	(0.132)	(0.0673)
Simulated eligibility at ages 10-14	-0.00639	0.107**	-0.142*	0.829***	0.117
	(0.00645)	(0.0476)	(0.0792)	(0.114)	(0.106)
Simulated eligibility at ages 15-18	-0.00904	-0.0170	-0.176**	-0.0145	1.092***
	(0.00944)	(0.0829)	(0.0713)	(0.125)	(0.113)
AP F statistic	1443.16	135.43	142.00	52.18	84.93
Kleibergen-Paap Rank Statistic (p-value)			16.28 (0.0001)		

Notes: This table displays statistics from the first stage regressions of each eligibility measure on the simulated eligibility measures. NHIS and ACS regressions include individual characteristics, state of birth, year of birth, survey year dummies, and state of birth trends in birth year. NIS regressions include state, birth year, birth cohort size in each state, state by survey year fixed effects, and state trends in birth year. Additional controls included in all regressions are unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state, birth year.

Table 4. Instrumental Variables Estimates of the Effect of In Utero and Childhood Coverage on Adult Health, NHIS 1998-2014

	Very good or excellent health		Any health limitations		BMI		Obesity		Presence of one or more chronic health conditions		Kessler 6 score	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Prenatal eligibility	0.016	0.008	0.007	0.026	-2.186*	-1.970	-0.165**	-0.167**	-0.112*	-0.069	-0.624	-0.345
	(0.045)	(0.051)	(0.020)	(0.023)	(1.319)	(1.387)	(0.073)	(0.082)	(0.064)	(0.082)	(0.726)	(0.805)
Eligibility at ages 1-4	-0.003	0.004	-0.002	-0.013	-0.004	-0.013	0.003	0.018	0.005	0.000	0.122	0.395
	(0.017)	(0.021)	(0.007)	(0.009)	(0.345)	(0.468)	(0.021)	(0.028)	(0.029)	(0.027)	(0.260)	(0.290)
Eligibility at ages 5-9	0.022**	0.034**	-0.001	-0.009	0.272	0.088	0.027	0.028	-0.036**	-0.050*	0.090	-0.022
	(0.010)	(0.016)	(0.004)	(0.008)	(0.327)	(0.529)	(0.018)	(0.025)	(0.016)	(0.030)	(0.152)	(0.237)
Eligibility at ages 10-14	-0.009	-0.006	0.002	-0.003	-0.255*	-0.643	-0.025**	-0.036	0.015	-0.002	0.233	0.060
	(0.007)	(0.016)	(0.003)	(0.007)	(0.152)	(0.460)	(0.010)	(0.023)	(0.012)	(0.020)	(0.190)	(0.213)
Eligibility at ages 15-18	0.016*	0.015	0.000	-0.004	-0.405*	-0.720	-0.009	-0.026	0.006	-0.020	-0.157	-0.172
	(0.009)	(0.021)	(0.005)	(0.007)	(0.232)	(0.547)	(0.019)	(0.036)	(0.017)	(0.023)	(0.134)	(0.183)
State Specific Trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	126,878		126,935		52,795		52,795		53,919		53,379	

Notes: This table displays instrumental variable regression results using the 1998 to 2014 National Health Interview Survey. Robust standard errors clustered by state of birth are in parenthesis. All models include individual characteristics (sex, race, ethnicity, age dummies), survey year, state of birth, and year of birth fixed effects, as well as state-specific linear trends in birth year when indicated. Additional control variables include demographic and economic characteristics by birth year and state of birth as described in the text. All regressions are weighted. First stage is reported in Table 3. Significance levels: * = significant at the 10% level, ** = significant at the 5% level, *** = significant at the 1% level.

Table 5. Instrumental Variables Estimates of the Effect of In Utero and Childhood Coverage on Adult Hospitalizations, NIS 1998-2011

	All visits excluding pregnancy-related visits		Endocrine, nutritional and metabolic diseases, and immunity disorders		Diabetes and Obesity		Mental Health	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Prenatal eligibility	-0.025	-0.041	-0.824***	-0.841**	-1.124***	-0.906**	0.043	0.146
	(0.124)	(0.130)	(0.281)	(0.346)	(0.397)	(0.454)	(0.212)	(0.197)
Eligibility at ages 1-4	-0.154***	-0.113***	0.070	-0.065	0.044	-0.098	-0.136*	-0.067
	(0.042)	(0.037)	(0.078)	(0.206)	(0.102)	(0.210)	(0.070)	(0.070)
Eligibility at ages 5-9	0.022	0.003	-0.027	-0.105	0.011	0.030	0.051	0.042
	(0.019)	(0.039)	(0.068)	(0.131)	(0.078)	(0.169)	(0.031)	(0.085)
Eligibility at ages 10-14	0.008	0.021	-0.046	-0.126	-0.056	-0.059	0.013	0.064
	(0.014)	(0.036)	(0.041)	(0.099)	(0.043)	(0.127)	(0.017)	(0.078)
Eligibility at ages 15-18	-0.036*	-0.003	-0.055	-0.121	-0.070	-0.076	-0.086	0.056
	(0.020)	(0.032)	(0.041)	(0.110)	(0.045)	(0.123)	(0.053)	(0.053)
State Specific Trends	No	Yes	No	Yes	No	Yes	No	Yes
N	3,527		2,972		2,689		2,712	

Notes: This table displays instrumental variable regression results using the 1998 to 2011 Nationwide Inpatient Sample discharges excluding cases where the primary diagnosis is related to pregnancy or delivery. Robust standard errors clustered by state are in parentheses. Dependent variable is the log of the number of visits by category for each state-year-birth cohort. States are excluded if there are zero discharges for any state-year-birth cohort observation. All models include state by year and birth year fixed effects, as well as state-specific linear trends in birth year. Additional control variables include demographic and economic characteristics by birth year and state of birth as described in the text. First stage is reported in Table 3. Significance levels: * = significant at the 10% level, **= significant at the 5% level, ***=significant at the 1% level.

Table 6. Instrumental Variables Estimates of the Effect of In Utero and Childhood Coverage on Adult Human Capital, ACS 2000-2013

	High School Graduate		Some College		College Degree		Personal Income (Levels)		Personal Income (Logs)		Food Stamps	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Prenatal eligibility	0.031***	0.021**	0.008	0.008	0.012	0.031	2,424	2,845**	0.126*	0.146**	-0.017	-0.055***
	(0.009)	(0.010)	(0.018)	(0.019)	(0.024)	(0.024)	(1,606)	(1,393)	(0.070)	(0.063)	(0.020)	(0.019)
Eligibility at ages 1-4	0.008**	0.003	0.000	-0.016***	0.004	-0.017**	1,264***	-786.9*	0.017	-0.002	0.012	-0.002
	(0.004)	(0.004)	(0.007)	(0.005)	(0.005)	(0.008)	(313.4)	(448.6)	(0.023)	(0.020)	(0.008)	(0.006)
Eligibility at ages 5-9	-0.003*	0.001	-0.003	0.005	0.010**	0.006	406.9	909.4*	0.033**	0.039**	-0.003	0.001
	(0.002)	(0.002)	(0.003)	(0.006)	(0.004)	(0.009)	(262.3)	(474.7)	(0.014)	(0.019)	(0.004)	(0.005)
Eligibility at ages 10-14	-0.002	-0.001	-0.001	0.001	0.000	-0.001	-412.6***	573.6	-0.007	0.018	-0.003	-0.002
	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.007)	(138.1)	(449.7)	(0.006)	(0.017)	(0.003)	(0.006)
Eligibility at ages 15-18	0.001	0.004	0.001	-0.000	-0.001	-0.007	-160.8	701.8	0.002	0.007	-0.005	0.003
	(0.002)	(0.003)	(0.004)	(0.005)	(0.004)	(0.007)	(205.0)	(431.1)	(0.010)	(0.018)	(0.003)	(0.005)
State specific trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	3,127,379		3,127,379		2,138,457		1,750,082		1,571,314		2,148,734	

Notes: This table displays instrumental variable regression results using the 2000 to 2013 American Community Survey public use microdata sample. Robust standard errors clustered by state of birth are in parentheses. Results for High School Graduation and Some College are conducted on a sample of those age 20 and older. Results for College Degree, Total Personal Income (levels and logs), and Food Stamps are conducted on a sample of individuals who are 23 years old and older and the sample for Total Personal Income also excludes individuals currently enrolled in school. All models include state by year and birth year fixed effects. State-specific linear trends are included as noted. Additional control variables include demographic and economic characteristics by birth year and state of birth as described in the text. All regressions are weighted. First stage is reported in Table 3. Significance levels: * = significant at the 10% level, **= significant at the 5% level, ***=significant at the 1% level.