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 public health insurance for pregnant women and infants under the Medicaid program affected the adult outcomes of individuals born between 1979 and 1993 who gained access to coverage *in utero* and during the first year of life. We conduct this analysis by exploiting state- and cohort-level variation in the timing and generosity of Medicaid expansions using a simulated eligibility instrumental variables model. We find that cohorts whose mothers gained eligibility for prenatal coverage under Medicaid have lower rates of chronic conditions as adults and experience fewer hospitalizations related to diabetes and obesity. We also find that the prenatal expansions increased high school graduation rates among affected cohorts. 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 early life exposure to public health insurance that resulted from a widespread, rapid expansion of Medicaid benefits to pregnant women and their infants from 1979 to 1993. This program represents the single largest effort on the part of United States government to improve birth and infant health outcomes. However, despite the historic magnitude of this expansion, little is known about whether this provision of care to pregnant women and their infants 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 motivates taking a long-run perspective when evaluating the benefits of this public intervention.

Other papers have evaluated the long-term effects of childhood health insurance coverage after birth through age 18.[1] However, given the unique role of the *in utero* environment in determining lifetime health and achievement, it is entirely plausible that the substantial investments in prenatal health made by the Medicaid program could have different, and possibly larger, effects than other expansions that affected children at later ages. This is particularly true for early expansions in Medicaid that targeted the most vulnerable pregnant women but did not necessarily expand eligibility for children.[2] This study is the first to examine the long-term

[1] E.g. Levine and Schanzenbach (2009); Brown, Kowalski, and Lurie (2017); Cohodes et. al., (2016); Thompson (2017); Wherry and Meyer (2016), Wherry et al. (forthcoming). See Section 2 for further discussion.

[2] To our knowledge these expansions were not incorporated into any previous analysis that evaluates the impact of Medicaid coverage at birth (e.g., Cohodes et. al. (2016)).

effects of public health insurance expansions that explicitly targeted the prenatal period, during which children are at their most receptive stage of development and interventions may yield the highest returns.

We exploit variation in the timing and generosity of Medicaid expansions for pregnant women across states to identify how access to coverage early 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 women who would be eligible for Medicaid coverage in the event of a pregnancy for each birth cohort.[3] As developed by Currie and Gruber (1996b), 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 early life and control for public health insurance eligibility during other ages of childhood.

We find 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 19-36) 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 early life eligibility is associated with approximately a 0.03 standard deviation decrease in the presence of chronic conditions at ages 19-36, a 2-3 percent decrease in hospitalizations, and an 8-10 percent decrease in hospitalizations related to 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, which offset about 26 percent of the cost of the initial Medicaid eligibility expansion.

Finally, the Medicaid expansions for pregnant women also improved educational outcomes in adulthood for their children. We find that cohorts whose mothers gained prenatal Medicaid

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[3]Under Medicaid eligibility rules, their baby would be deemed automatically eligible for coverage during the first year of life as well.

eligibility are more likely to graduate high school; a ten percentage point increase in prenatal eligibility is associated with a 0.11 percentage point increase in high school graduation rates.

Our findings suggest that the Medicaid expansions for pregnant women and their infants 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 chronic conditions 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 increase in high school completion for these cohorts indicates a meaningful change in human capital accumulation. 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) and Almond, Currie, and Duque (2017).

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.[4] 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 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, greater incidence of coronary heart disease and impaired glucose tolerance, increased incidence of psychiatric disorders including schizophrenia and major affective disorder, and reduced life expectancy.[5] Studies of other negative shocks to the fetal environment have also found evidence of poor health and disability in adulthood, including worse mental health (e.g. Almond and Mazumder 2011, Persson and Rossin-Slater forthcoming).

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 cognitive ability and educational achievement to higher poverty later in life.[6] 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

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[4]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.

[5] See Roseboom et al. (2001); Painter, Roseboom, and Bleker (2005); Brown et al. (1995); Susser and Lin (1992); Susser et al. (1996); Susser, Hoek, and Brown (1998); Brown et al. (2000); Lindeboom, Portrait, and van den Berg (2010).

[6] See Almond, Edlund, and Palme (2009); Barreca (2010); Almond, Mazumder, and van Ewijk (2011).

targeting the fetal environment.[7] Recent economic models predict a higher return on 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.[8]

In addition, interventions not directly targeted to pregnant women and young children have been shown to have important effects for these beneficiaries. Hoynes, Schanzenbach, and Almond (2012) find that children gaining access to the program Food Stamps Program *in utero* and in early childhood experience lowered incidence of metabolic diseases (such as diabetes,

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[7] 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 (2016) analyzes the long-run impact of means-tested cash transfers via the Mother's Pension Program, a precursor to AFDC, and find evidence of increased longevity for male children of recipients.

[8] See Olds et al. (2007), Eckenrode et al. (2010), Olds et al. (2010), Heckman et al. (2014)

hypertension, and obesity) in adulthood, with exploratory analyses suggesting this is driven by exposure during the pre- and early post-natal period. Meanwhile, 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 pollution during their year of birth.

Our paper contributes to this area, as well as to a small but growing literature examining the long-run effects of access to public health insurance in childhood. A few recent studies have linked exposure to public health insurance in childhood to improved later health and better educational or economic outcomes.[9] These papers study variation in public insurance under expansions in coverage over different periods of childhood from birth to age 18.

This is the first study, however, to examine the long-term effects of public health insurance expansions explicitly targeting pregnant women and 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 pregnancy-related coverage 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 health and economic effects of this policy intervention. Although previous work has provided estimates of the impact of Medicaid eligibility at birth or during early childhood, based on our reading of these articles, the authors have not incorporated expansions to pregnant women into their analyses.[10] This omission may be particularly important for the Medicaid expansions which occurred prior to 1987, many of which explicitly targeted pregnant women, and which have been shown to have particularly strong positive impacts on birth

[9] Health: Currie, Decker and Lin (2009); Brown, Kowalski, and Lurie (2017); Goodman-Bacon (2016); Thompson (2017); Wherry and Meyer (2016); Wherry et al. (forthcoming); Economic outcomes: Brown, Kowalski, and Lurie (2017); Cohodes et al. (2016); Goodman-Bacon (2016); Levine and Schanzenbach (2009).

[10] Currie, Decker, Lin (2009), Cohodes et al. (2016), Levine and Schanzenbach (2009) examine eligibility at age 0, Brown, Kowalski, and Lurie (2017) at ages 0-3, Thompson (2017) at ages 0-5.

outcomes (see Currie and Gruber's 1996b discussion of "targeted" expansions). As we demonstrate in our analysis, these early expansions were important drivers of the observed improvements in long-run outcomes we document in this paper.

To our knowledge, only one other study has considered the long-term health effects of access to public coverage during the prenatal period. Boudreax, Golberstein, and McAlpine (2016) 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.

Our study complements Boudreaux, Golberstein, and McAlpine (2016) by looking at expansions that occurred later in the twentieth century. The expansions we study targeted a broader population of women than those affected by the original rollout of Medicaid in the 1960s. At that time, 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 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 broader access to prenatal care, inclusive of women with no previous children or receipt of public benefits.

In the next section, we provide background on the Medicaid coverage expansions for pregnant women 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. Income eligibility thresholds for the program varied by state but were typically well below the poverty line.[11] Moreover, AFDC eligibility was largely restricted to single parent families.[12]

State options to provide Medicaid coverage to pregnant women not tied to the welfare system expanded greatly starting in the 1980s. 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). States were able to extend prenatal Medicaid eligibility to first-time pregnant women and those in two parent families with incomes below AFDC levels, as well as “medically needy” individuals with higher incomes but high medical expenses (see Currie and Gruber 1994 for detailed discussion). State options were followed by new federal requirements for states to cover all pregnant women meeting the financial standards for cash welfare, regardless of their family structure or participation in the AFDC program. Since these expansions were narrowly targeted to women and infants with family incomes below cash welfare levels, we follow prior work in referring to them as “targeted eligibility changes” (Currie and Gruber 1996b). In addition, under the expansions, the children born to women receiving Medicaid were deemed automatically eligible for coverage during their first year of life (Congressional Research Service 1988).

Between 1986 and 1990, Congress took larger steps to expand Medicaid eligibility for pregnant women and infants with family incomes exceeding AFDC thresholds. New options allowed states to expand coverage to pregnant women and their infants with incomes up to the poverty

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[11] 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).

[12] 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.

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. We refer to these later broader changes as "broad eligibility changes," following Currie and Gruber (1996b). Additional details on these legislative changes are available in Table A.1 in the Appendix.

First demonstrated in seminal work by Currie and Gruber (1996b), these expansions 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 the March Current Population Survey (CPS) and detailed eligibility rules from this period, we estimate that the fraction of 15-44-year-old women and their infants 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(a) 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 1(b) shows the change in the fraction of women and their infants eligible for Medicaid 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. In our analysis, we use this variation in eligibility to evaluate the long-term effects of exposure to public health insurance during the pre- and postnatal period.

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.[13]

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.[14] 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

[13] Almond et al. 2010; Bharadwaj, Loken, and Neilson 2013; Almond, Guryan, and Mazumder 2014; Chay, Guryan, and Mazumder 2009.

[14] 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.

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.[15]

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 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. 2015).

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[15] 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.

These last mechanisms are notable because they may lead to long-term effects for children even if many of the women who gained Medicaid coverage would have otherwise had private coverage. Existing analyses find a substantial reduction in private insurance coverage associated with the prenatal Medicaid expansions that affected the younger cohorts we study. Cutler and Gruber (1996) estimate crowd-out of private insurance as representing about one-half of the coverage increase for women of childbearing age between 1987 and 1992.[16] More recently, using administrative hospital data, Dave et al. (2010) estimate that 50 percent of the increase in Medicaid participation among pregnant women between 1985 and 1995 came from private insurance. As described earlier, this later period saw broader eligibility changes that affected higher income women than the earlier targeted changes in eligibility for existing low-income groups. It would be reasonable to expect smaller levels of crowdout under the targeted eligibility expansions since lower wage workers are less likely to have access to private health insurance through an employer (see, e.g., Hai 2015). However, we know of no existing work examining changes in private insurance coverage under these earlier, targeted expansions.[17]

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, under the targeted eligibility expansions, 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

[16] The authors' estimate for crowdout is much larger before they make adjustments for family-level decisions regarding health insurance coverage and conditional coverage in the event of illness or pregnancy.
[17] Prior research on crowdout under Medicaid expansions has focused mostly on non-pregnant individuals (see discussion in Dave et al. 2010).

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

We use data from both survey and administrative sources to estimate the effects of early life Medicaid coverage on adult health. In all of our analyses, we use the information available for individuals born between 1979 and 1993 at ages 19 and older. 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 VIII.d.).

National Health Interview Survey

To document changes in health, we first 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 2015; individuals in our sample are between the ages of 19 and 36.

With the NHIS, we examine health outcomes that have been linked to the fetal environment and early life exposures. We first construct a chronic disease index with comparable components (obesity, diabetes, heart disease or heart attack, high blood pressure) to those used by Hoynes, Schazenbach, Almond (2016) and Boudreaux, Golberstein, McAlpine (2016) in their studies of the long-run effects of Food Stamps and Medicaid, respectively.[18] We calculate obesity using the formula for body mass index and reported information on the respondent's height and weight. For the remaining conditions, the respondent reports if they were ever diagnosed with diabetes (excluding gestational diabetes), heart trouble (coronary heart disease, angina pectoris, heart attack, a heart condition or other disease), and high blood pressure.[19]

We next evaluate other health outcomes not linked to these conditions: very good or excellent self-reported health and the Kessler 6 score. The Kessler 6 (K6) score is a measure of psychological distress derived from six questions about the individual's recent experiences of depressive or anxiety symptoms.[20] Scores range between 0 and 24 with a higher score indicating higher severity of psychological distress.[21]

The bottom panels of Tables 2 and 3 display the weighted mean for each of these outcomes in the NHIS sample. We observe that 75.3 percent of respondents report that they are in very good

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[18] An earlier version of this paper examined the presence of a chronic health condition defined using a selection of conditions 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. Based on the advice of an anonymous referee, we have limited our study of chronic conditions to those with the strongest established linkages to the fetal environment. The estimates for our prior definition of chronic health conditions may be found in Appendix Table A.13.

[19] We previously examined continuous BMI and a measure of any health limitations. We chose to remove these two outcomes from our main analysis since the obesity indicator captures the most relevant range of BMI and because of the low prevalence rate of health limitations. The estimates for these outcomes may be found in Appendix Table A.13.

[20] 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.

[21] In the appendix, we report results for alternative dependent variables based on self-reported health status (fair or poor health, and continuous self-reported health on a 5-point scale) and the Kessler 6 scale (prevalence of severe mental illness using the clinically validated cutpoint of a K6 score of 13 or greater). See Appendix Table A.15.

or excellent health. Approximately 22.0 percent of respondents are obese, 8.0 percent have been diagnosed with high blood pressure, 3.7 percent have had a heart attack or other heart disease, and 1.2 percent have been diagnosed with diabetes. The average score on the Kessler 6 psychological distress measure is 2.7.

Nationwide Inpatient Sample

In addition to the survey data on health, we also analyze administrative data on hospitalizations from the Nationwide Inpatient Sample (NIS) provided by the Healthcare Cost and Utilization Project. The NIS samples hospitals within a state, and provides discharge-level data on all hospital visits to sampled hospitals in each year. The sample of states varies from year to year (see detailed description of the states in our sample in the Appendix).[22] We use the 1998-2011 years of the NIS. 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.

In our analysis of hospitalizations, we again focus on visits for conditions that have been closely linked to the fetal environment. Using the International Classifications of Diseases (ICD) system, we use the primary diagnosis code to classify visits as relating to any of the four chronic conditions analyzed in the NHIS: diabetes, obesity, heart disease and hypertension. Due to the low incidence of obesity- and hypertension-related visits, we group together visits related to metabolism (diabetes and obesity) and those related to the circulatory system (heart disease and

[22] We 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 similar effects of prenatal coverage on our NIS outcomes but most of these estimates are not statistically significant; in most specifications 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 under our main model.

hypertension).[23] Finally, we consider hospitalizations for mental health related diagnoses. We exclude all pregnancy or delivery-related care from these categories.

While there are many advantages to using administrative data on health care utilization,[24] there are also important limitations for our research design – mainly that the NIS does not have 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 the year and quarter during which the patient was admitted to the hospital,[25] and we assign birth state to be the state in which the hospitalization took place. In Section VIII.a. we conduct several sensitivity analyses examining these decisions and provide evidence that they are unlikely a concern for the analysis.

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[23] An earlier version of this paper also analyzed visits for chronic diseases 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 in Appendix Table A.13.

[24] Administrative records are likely to present a more accurate picture of health care use than survey data. 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 instance, in the NHIS we are unable to exclude pregnancy and delivery related hospitalizations (by far the most common hospitalization for this age group) from hospitalizations for diagnoses that have previously been linked to the fetal environment. For these reasons, we use administrative hospital records in our main analysis of health care utilization. We did examine self-reported utilization of medical care using the NHIS, but this analysis did not detect any significant effects of *in utero* 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 each measure of utilization associated with Medicaid prenatal eligibility but they are not statistically significant. These results are presented in Appendix Table A.12.

[25] We follow a method similar to that used in Rotz (2012) and this procedure is described in greater detail in Appendix Section C.

We aggregate the total number of hospital discharges and the number of discharges by diagnosis group (all chronic illness related, diabetes and obesity, heart disease and hypertension, 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.[26] 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 (see later discussion in Section VII.d.).

The bottom panel of Table 4 displays descriptive statistics from the NIS. Of the 2.6 million non-pregnancy-related hospital visits we observe, about 7 percent of all admissions are for any of the chronic conditions we evaluate, 4 percent are related to diabetes or obesity, 3 percent are related to heart disease or hypertension, and 21 percent are related to mental health diagnoses. There are about 374 non-pregnancy hospitalizations per 10,000 individuals at these ages.

American Community Survey

To examine the impact of these expansions on human capital outcomes, we use restricted data from the 2001 to 2015 years of the American Community Survey (ACS) with information on an individual's year of birth.[27] This nationwide survey collects information on population characteristics for individuals in the U.S., including information on income, educational attainment, and participation in certain public programs.

With the ACS data, we analyze high school graduation for all individuals in our sample,

[26] The excluded states are Alaska, Connecticut, Montana, Pennsylvania, Rhode Island, South Dakota, Vermont and Wyoming.
[27] The publicly available files impute birth year as survey year minus age in years. In addition to this difference, the restricted-use version of the survey has a sample size approximately 50% larger than the public-use version; this is done so as to reduce disclosure risk in the public-use version. See the appendix for more details on the ACS.

completion of some college education for respondents age 20 and older, and log personal income and household food stamp receipt for respondents ages 23 and older not currently enrolled in school.[28] We also restrict the analysis for log personal income to individuals with strictly positive income; a similar analysis for personal income in levels that does not exclude zeros and net negative income is included in Appendix Table A.11. Total personal income includes total pre-tax personal income or losses from all sources for the previous year.

The bottom panel of Table 5 presents weighted descriptive statistics from the American Community Survey for respondents born between 1979 and 1993. About 92 percent have graduated high school and 63 percent have some college attainment (age 20 and older). Among those age 23 and older who are not currently in school, average annual income is $32,469 in inflation-adjusted 2013 dollars and 17 percent live in a household that receives Food Stamps benefits.

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 were 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

[28] In an earlier version of this paper, we examined college attendance and degree completion separately, but we later combined these outcomes to facilitate the interpretation of the results and because the cohorts we examine are still relatively young and may have not yet completed their education. The estimates for completion of a college degree are reported in Appendix Table A.13.

determine eligibility).[29] We merge measures of prenatal Medicaid eligibility onto our three datasets using year of birth and state of birth.

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 Currie and Gruber (1996a, 1996b) 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 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.

States with more generous prenatal coverage may offer better coverage for children at other ages. For this reason, we also construct a measure of public health insurance eligibility at ages 1-18 for each birth year and state. For a given birth year, we calculate the fraction of children eligible for

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

coverage at each age during childhood in each state.[30] We sum the fraction eligible across ages to construct cumulative measures of public eligibility over ages 1-18 for each birth year and state. These measures represent the average number of years of public eligibility at these ages. We instrument with a measure of simulated childhood eligibility constructed in a manner similar to that for simulated prenatal eligibility. Additional details on the construction of these variables may be found in the Appendix.

The primary identifying assumption of our model is that there were no changes in birth cohort characteristics by state that would affect our outcome variables and were correlated with, but not caused by, each cohort's exposure to Medicaid eligibility *in utero* and in childhood. This assumption may be incorrect if, for example, states expanded other social program at the same time they opted to expand Medicaid eligibility. We enhance our model's robustness to this assumption by including a large number of time-varying covariates that measure other state-level policy choices as well as measures of economic and population characteristics measured at the cohort and state level. In certain specifications, we include region of birth by birth year fixed effects in certain specifications in order to allow for region-specific trends or shocks in the characteristics or outcomes of birth cohorts that might have occurred during this time period. We also take a more direct approach to attempt to control for changes over time in outcomes across states that are unrelated to the Medicaid expansions by including state-specific linear trends in outcomes by birth cohort in certain specifications.

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

[30] Since year of birth is not available in the CPS, we calculate the individual's birth year as the calendar year minus age.

$$y_{ibsy} = \beta_s + \beta_b + \beta_y + \beta_1 \, Prenatal_{bs} + \beta_2 \, Elig \; Age \; 1-18_{bs} + \beta_3 \, Z_{bs} + \beta_4 \, X_{ibsy} + \delta_s b + \varepsilon_{ibsy}$$

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*. The estimated coefficient on the prenatal eligibility measure ($\widehat{\beta_1}$) may be interpreted as the impact of an increase from 0 to 1 in the fraction of women and their infants who would be eligible for Medicaid in the event of a pregnancy. The estimated coefficient on the childhood eligibility measure ($\widehat{\beta_2}$) may be interpreted as the effect of an additional year of public health insurance eligibility during childhood. 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.[31] We also include a vector of additional variables Z_{bs} that 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 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.[32] In certain specifications, we include region of birth by birth year fixed effects to account for any trends or shocks by year that affect all states within a region. Alternatively, we include state-specific linear trends in birth year, $\delta_s b$, to account for any linear

[31] 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.

[32] 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.

trends across cohorts in the outcome variable that vary by state. An additional specification that excludes state by year controls, state linear trends, and region by birth year fixed effects is reported in the appendix (Appendix Table A.8). All analyses with the NHIS and ACS data employ survey weights. Standard errors are heteroskedasticity-robust and clustered by state of birth.[33]

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

$$\log(y_{bsy}) = \beta_b + \beta_s \times \beta_y + \beta_1 \, Prenatal_{bs} + \beta_2 \, Elig\ Age\ 1-18_{bs} + \beta_3 \, Z_{by} + \delta_s b + \varepsilon_{bsy}$$

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.[34,35] As in the previous specification, 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

[33] To account for multiple testing, we estimated family-wise error rate adjusted p-values for groups of outcomes in the NIS and NHIS data. However, due to long computation times, we were unable to compute these for outcomes in the ACS. Inference was similar when conducted with these p-values.

[34] Controlling for the size of the birth cohort may be problematic if prenatal Medicaid coverage affects fertility; however, existing evidence suggests it does not (Zavodny and Bitler 2010). Our results are very similar if this variable is excluded.

[35] 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.

abortion policies of the state in birth year *b*. We also present models that include state-specific linear trends in birth year, $\delta_s b$, as well as models without such trends that include region by birth year fixed effects. Additional specifications are reported in the Appendix (see Section VII). For all models, standard errors are heteroskedasticity-robust and clustered by state.

Each of these models contains two endogenous variables: prenatal Medicaid eligibility (which we interpret as capturing both *in utero* and infant exposure) and cumulative eligibility occurring between the ages of 1 and 18. We use simulated prenatal eligibility and simulated cumulative eligibility during childhood as instruments for these endogenous variables.

VI. Results

a. First stage

In the first stage, we regress each endogenous variable (actual eligibility *in utero* and in childhood) on the full set of instruments (simulated eligibility *in utero* and in childhood) and control variables. For all samples, each simulated eligibility measure is strongly predictive of its corresponding measure of actual eligibility, indicated by the coefficients reported on the diagonal of this table, which are very close to one and highly significant. The Kleibergen-Paap rank statistic indicates that the full set of instruments are able to identify the full set of structural parameters in the specified dataset (Kleibergen and Paap 2006). The first stage results are reported in Appendix Table A.4.

b. NHIS results

Tables 2 and 3 report the instrumental variables estimates for prenatal Medicaid eligibility.[36] For each outcome, we first report the model that includes only our policy variables of interest and

[36] The IV estimates for childhood eligibility are reported in the Appendix, along with the reduced form estimates for prenatal eligibility (see Table A.5). In a previous version of this paper, we included four different measures of childhood eligibility corresponding to eligibility at ages 1-4, 5-9, 10-14, and 15-18 in our model, rather than a single measure of eligibility at ages 1-18. We report estimates from this model in Table A.10. The estimates for prenatal eligibility are similar to those presented under our main specification.

individual and state by birth year characteristics as controls. Column two presents the same specification, but adds region of birth by birth year fixed effects. Column three presents results with state-specific linear trends in birth year, rather than region of birth by birth year fixed effects. Estimates from models that include only individual-level control variables and the policy variables of interest are presented in Appendix Table A.8.

Table 2 presents the estimates for the chronic condition index (columns 1-3) and its component measures (columns 4-15). The effect of prenatal Medicaid eligibility on chronic conditions ranges from -0.308 to -0.383 across specifications and is statistically significant at the 1 percent level. The magnitude of the coefficient estimates indicate that a 10-percentage-point increase in prenatal eligibility is associated with approximately a 0.03 standard deviation decrease in chronic conditions. The coefficient estimates for each of the individual components suggest decreases among all of the included conditions, although the estimates are not always statistically significant.

Table 3 presents estimates for other measures of health. We do not find consistently significant evidence of improvements in self-reported health or the Kessler 6 score.

The coefficients reported in the first row of Table 2 can be interpreted as the intent-to-treat effect of Medicaid coverage *in utero* and in infancy on later life health. 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 presence of chronic conditions in early adulthood by approximately 1.1 standard deviations.[37] This

[37] A potentially appealing exercise would be to scale our 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

estimate falls within the existing range of estimates in the literature examining the long-run effects of early life public policy interventions. Hoynes, Schanzenbach, and Almond (2016) estimate that participation in Food Stamps at ages 0-5 led to a decrease in chronic conditions of 0.68 standard deviations at ages 28-53. Meanwhile, Boudreaux, McAlpine, and Golberstein (2016) find a decrease in chronic conditions of 3.15 standard deviations at ages 25-54 among those individuals who were predicted to gain Medicaid coverage between conception and age 5.

c. NIS results

Table 4 presents the results from the analysis using administrative hospital records. Reported in columns 1-3, we find significant reductions in total hospitalizations of approximately 2 to 3 percent associated with a ten-percentage point increase in prenatal Medicaid eligibility, although the parameter estimate is not significant in the specification that includes region by year fixed effects.

Columns 4 through 6 display the effect of prenatal Medicaid eligibility on hospitalizations for chronic conditions that are sensitive to the early life environment. We find that a ten-percentage point increase in prenatal Medicaid eligibility is associated with a 6 to 8 percent reduction in visits for this category of diseases in adulthood, although the estimates are only marginally significant in the latter two specifications. We further narrow our analysis within this category of diseases by looking only at hospitalizations related to diabetes and obesity (columns 7-9). Here, we find that a ten-percentage point increase in prenatal Medicaid eligibility is associated with an 8 to 10 percent reduction in hospitalizations in this category. We find no statistically significant effect of early life Medicaid eligibility on hospitalizations for heart disease and hypertension (columns 10-12) or mental health related diagnoses (columns 13-15).

We 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

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.

average, there are about 15.5 visits for diabetes and obesity related conditions per 10,000 individuals in this age group each year. Our most conservative estimate of an 8.4 percent decrease in these types of visits associated with a 10 percentage point increase in eligibility implies 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. This resulted in about 4 fewer hospitalizations for these diagnoses per 10,000 individuals per year (i.e., 15.5 visits per 10,000 individuals x 24 percent). Put differently, for every ten thousand women who gained prenatal eligibility, there were about 13 fewer hospitalizations for diseases in this category in each year.[38] 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 43 fewer hospitalizations annually for conditions related to diabetes and obesity.

We can use our estimates to compare the cost of additional Medicaid coverage for pregnant women and their infants 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 pregnant women and their infants 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 1,520 fewer hospitalizations per year for this cohort,[39] or a total of 21,280 fewer hospitalizations between the ages of 19 and 32. The average amount charged for a hospitalization for this age

[38] With a mean incidence of diabetes or obesity related hospitalizations of 15.5 per 10,000, an 84% decrease from gaining eligibility, as indicated by our coefficient estimate, is equivalent to -0.840*15.5 = 13 fewer hospitalizations for diseases in this category for every 10,000 women who gained prenatal eligibility.

[39] As calculated in the previous paragraph, a thirty percentage point increase in eligibility is associated with 4 fewer hospitalizations per 10,000 individuals each year. With approximately 3.8 million individuals per birth cohort, this is 0.0004 x 3.8 million = 1520 fewer visits annually.

group is approximately $24,150. 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 $6,168. 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 $131.3 million in 2011 dollars.[40] The amount saved during these ages therefore represents approximately 26 percent of the total cost associated with the increase in early life Medicaid coverage for this birth year. [41] 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.

d. ACS results

We report the effects of early life Medicaid eligibility on education, earnings, and public assistance receipt in Table 5. 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. Our point estimates indicate that a 10 percentage point increase in prenatal eligibility increased the probability a respondent will have graduated high school by between 0.1 and 0.3 percentage points. Scaling the estimate from the specification with state trends by a 30 percent take up rate, it implies that gaining Medicaid coverage increased the probability that an individual graduated high school by 3.7 percentage points, an increase in the graduation rate of about 4 percent. We only find a statistically significant effect of

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[40] We have opted to conduct this analysis using only diabetes and obesity related visits, as these are consistently significant. However, performing a similar calculation using all hospitalizations would imply cost savings of approximately $63 million per year in 2011 dollars, or $882 million between the ages of 19 and 32.

[41] 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 even though 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.

prenatal eligibility on the probability that the respondent has attended college in the specification without state trends or region by birth year fixed effects.

These findings are broadly consistent with recent work showing improvements in educational attainment associated with childhood eligibility for Medicaid with a few notable differences. Cohodes et al. (2016) 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. Also different from these authors, we do not find consistent evidence of an increase in college attendance. While our point estimates suggest that a 10 percentage point increase in prenatal eligibility was associated with a 0.2 to 0.4 percentage point increase in college attendance, the estimates are not statistically significant in two of the three specifications. These estimates, however, are again of a similar magnitude to those estimated by Cohodes et al. (2016) for average childhood eligibility. The authors estimate a 0.3 percentage point increase in college enrollment associated with a 10-percentage point increase in average childhood eligibility.[42] Brown, Kowalski, and Lurie (2017) have a much larger estimate for a similar increase in childhood eligibility; they find a 1.8 percentage point increase in college enrollment at age 28 associated with a 10 percentage point increase in eligibility from birth to age 17 using administrative Internal Revenue Service data. Finally, in contrast with these authors, we do not find significant effects of childhood eligibility at ages 1-18 on educational attainment (estimates reported in Appendix Table A.5). However, given that our

[42] Our results may also differ from Cohodes et al. (2016) due to differences in our data and sample criteria. For example, we use a restricted version of the American Community Survey for the years 2000-2015 with exact year of birth to examine the 1979-1993 birth cohorts at ages 19-36. Cohodes et al (2016) use publicly-available ACS data for the years 2005-2012 to study the 1980-1990 birth cohorts at ages 22-29; their data does not contain exact year of birth, instead it is imputed using survey year minus age.

measures of prenatal and childhood eligibility are positively correlated (ρ=0.7191), these authors may be capturing information on the effect of prenatal eligibility in their estimates.

Columns 7 through 12 of Table 5 report the estimated effects of prenatal Medicaid eligibility on total annual personal income in logs and food stamps receipt. We find some evidence of an increase in income associated with Medicaid eligibility, although the estimate is not statistically significant in the specification with state trends. The estimates suggest between a 6 and 12 percent increase in personal income. We do not find any evidence of changes in food stamps receipt associated with Medicaid prenatal eligibility, although we do detect significant reductions in food stamps receipt associated with prenatal coverage in some alternative specifications (see the Appendix and Section VII).

VII. Heterogeneous Effects

a. Targeted vs. Broad Expansions in Eligibility

In their original study of the prenatal eligibility expansions, Currie and Gruber (1996b) separately estimated the effects of expansions that targeted particular groups of low-income women and the later broad expansions that extended coverage to more moderate-income women. The authors found stronger evidence of take up of coverage and effects on infant health under the earlier targeted expansions.

We also separately estimate the effects of the targeted and broad expansions.[43] Following Currie and Gruber, we estimate the impact of targeted eligibility changes for the full sample of cohorts, but focus on cohorts born in 1987 and later to estimate the impact of the broad eligibility

[43] Targeted expansions are defined as changes in Medicaid eligibility that resulted from changes in AFDC eligibility, changes in Medicaid eligibility for specific groups with incomes below AFDC levels that were not AFDC eligibility due to family structure requirements, and the medically needy. Broad expansions are defined as expansions that increased eligibility to women with incomes above AFDC thresholds.

changes. The estimates are presented in Table 6.[44] Consistent with their original analysis, we find stronger evidence of improvements in adult health and economic outcomes under the targeted eligibility changes.

b. Differential Effects by Race

In addition to examining the targeted and broad expansions separately, we also examine the overall effects of the expansions by child race. While all race groups saw an increase in eligibility over the period, blacks saw a greater absolute change in eligibility than whites (Lipton et al. 2016). Table 7 presents the estimates from two separate regressions that estimate the effects of the Medicaid eligibility expansions for black and white cohorts. For the NHIS outcomes, the coefficients suggest larger changes for blacks than whites associated with the expansions. Also, the coefficient for high school graduation in the ACS is larger for blacks than whites, although neither estimate is statistically significant. These patterns are consistent with a larger impact of the expansions for blacks than whites. In contrast, however, the NIS estimates suggest larger decreases in hospitalizations for whites than blacks. One caveat is that the NIS analysis for blacks is limited to a small number of states due to a sizeable number of states having zero cells for blacks.[45]

VIII. Alternative Specifications and Robustness Checks

a. Limitations of NIS analysis

[44] We are unable to estimate the models for broad eligibility for income and Food Stamps. Because the samples for these outcomes are restricted to individuals age 23 and older, there is not enough policy variation to estimate these models once we further restrict the sample to include only cohorts born in 1987 and later.

[45] Following the approach used in the main analysis, we exclude a state from the analysis sample for a given outcome if the state has any zero cells for admission years during the study period. This results in 46 states used to estimate the change in total hospitalizations for whites and 40 for blacks; 41 states used to estimate chronic illness related hospitalizations for whites and 20 for blacks; and 42 states used to estimate mental illness related hospitalizations for whites and 16 for blacks.

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 A.7. We find no statistically significant relationship between prenatal Medicaid eligibility and the probability of moving to a different state.

An additional limitation of the use of the NIS is that we do not have information on the patient's year of birth. Given that we do have information on the time of the year when an individual is admitted to the hospital, we assign birth year probabilistically using this information with the patient's age in years. We expect that the measurement error introduced through this imputation will bias our results toward zero.

In order to provide additional information regarding the performance of this imputation, we conducted additional analyses with the NHIS and ACS data, which have information on the respondent's year of birth, survey year and quarter of interview, and age in years. We imputed birth year for the respondent using the same approach used in the NIS. We then examined the correlation between imputed birth year and actual birth year, as well as the correlation between the Medicaid eligibility variables merged onto the data using this information. The results of this analysis may be found in Appendix Table A.6.[46] We find strong correlations between the

[46] All additional analyses and robustness checks presented in Section VIII use the model that includes individual controls (in the NHIS and ACS), birth state by birth year controls, and state-specific linear trends in birth year unless otherwise noted.

imputed and true values of the analysis variables. Actual and imputed birth year are correlated with a correlation coefficient of approximately 0.99, and all simulated eligibility measures derived from actual and imputed birth year have correlation coefficients that exceed 0.969.

We also conducted additional analysis by estimating our NHIS and ACS models using imputed birth year, rather than actual birth year, to assign our Medicaid eligibility variables (not reported here but available from the authors). We find that our estimates of the impact of prenatal Medicaid coverage on the chronic condition index and probability of high school graduation are the same sign, although attenuated, when using imputed birth year compared to when we rely on actual birth year information. Under the assumption that similar attenuation exists in our NIS results as a consequence of this measurement error, the estimates from these models can be considered lower bounds on the true effects of *in utero* Medicaid eligibility on later life hospitalizations.

b. Adult eligibility

In the previous sections, we demonstrated that expansions of Medicaid coverage for prenatal services resulted in lower rates of chronic conditions and fewer hospitalizations related to diabetes and obesity 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 Table A.8 report the results for models that include contemporaneous adult Medicaid eligibility and average cumulative adult eligibility for each of our datasets. The inclusion of adult eligibility does not appreciably change the results we reported in Section VI.

c. Placebo Tests

In this section, we evaluate the impact of prenatal Medicaid coverage on hospitalizations that are less likely to be affected by the *in utero* environment. First, we examine hospitalizations for appendicitis and injury. Second, we evaluate hospitalizations related to sickle cell anemia and kidney infection. These are acute conditions that are not obviously amenable to either improved fetal health and birth outcomes, although they may be affected by contemporaneous access to care.

The results are reported in Appendix Table A.9. We find no statistically significant effect of prenatal Medicaid eligibility on hospitalizations for either of these categories.

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. Similarly, we drop those with zero and negative income in the ACS analysis when evaluating log of income as the dependent variable. In the appendix, we report these results using the level, rather than the log, of hospitalizations and personal income. This does not require us to drop any observations from the sample. Estimating these models, we continue to find evidence of a reduction in hospitalizations for diabetes and obesity, although the estimate is no longer statistically significant. In the analysis of personal income, we find evidence suggestive of an increase in personal income although the estimates are not always statistically significant, consistent with our analysis using log of personal income as the dependent variable (Appendix Table A.11).

Second, we report results including Arizona in the analysis sample (Appendix Table A.8). 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.

Third, we estimate our IV models that exclude all state of birth by birth year control variables, including state trends and region by year fixed effects. This allows us to assess if our results are being driven by the inclusion of these controls. If our results are sensitive to the inclusion of these variables, it may indicate that there are additional omitted state of birth by birth year characteristics that are correlated with the decision of states to expand Medicaid and later life outcomes. The results are presented in Appendix Table A.8. The regression results are similar to those reported in Tables 2-5. Overall, these results do not indicate that the inclusion of state of birth by birth year demographic, economic, and policy variables are driving the main results.

Fourth, we examine the sensitivity of our results to how prenatal eligibility was assigned. Following the existing literature examining these prenatal expansions (Currie and Gruber 1996b, Dave et al. 2010, Dave et al. 2015), our main analysis merges on prenatal eligibility in an individual's year of birth. However, one might argue that prenatal eligibility at the time of conception is preferred. For this reason, we have added a new sensitivity analysis that uses available information on month of birth in the restricted versions of the ACS and NHIS data. We have re-assigned prenatal eligibility to individuals in our sample using average eligibility during a 9-month gestation period based on birth month.[47] The results from this analysis are similar to our main results and may be found in Appendix Table A.14.

Finally, we examine whether there are interaction effects between prenatal and childhood eligibility for public health insurance. A small literature has begun to examine whether investments in different periods of childhood serve as substitutes or complements (see discussion and review in Almond, Currie, and Duque 2017). In a similar spirit, we run models where we test for an interaction effect in addition to main effects of prenatal eligibility and eligibility at ages 1-18. Reported in Appendix Table A.16, our estimates do not present a definitive story but do suggest that childhood eligibility had a larger positive effect for cohorts who had less eligibility *in utero*. While we do not have data on the underlying mechanisms at work, it is reasonable to expect that medical care received in childhood may be able to mitigate some of the long-term consequences of health conditions not addressed earlier in life. This is also consistent with other recent work showing that later investments may compensate for earlier negative shocks (see Rossin-Slater and Wüst 2015; Gunnsteinsson et al. 2016; and Adhvaryu et al. 2015).

IX. Conclusion

During the 1980s, the Medicaid program underwent ambitious coverage expansions aimed at improving the health of pregnant women and infants. In this paper, we use variation in the timing and size of these expansions across states to show that adults who benefited from the prenatal expansions exhibit better outcomes today along several dimensions. We find that expanding Medicaid coverage to pregnant women resulted in lower rates of chronic illnesses and fewer hospital visits for diabetes and obesity 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

[47] These averages were computed by weighting annual estimates of eligibility from the CPS by the share of the 9-month gestation period that occurred in each year. For instance, for a child born in April 1990, her 9-month average eligibility was calculated as 1989 eligibility * (5/9) + 1990 eligibility * (4/9). All individuals born during 1979 were assigned with the 1979 annual eligibility estimate since we did not have a measure of eligibility for 1978.

prenatal eligibility expansions extend beyond health, improving human capital. 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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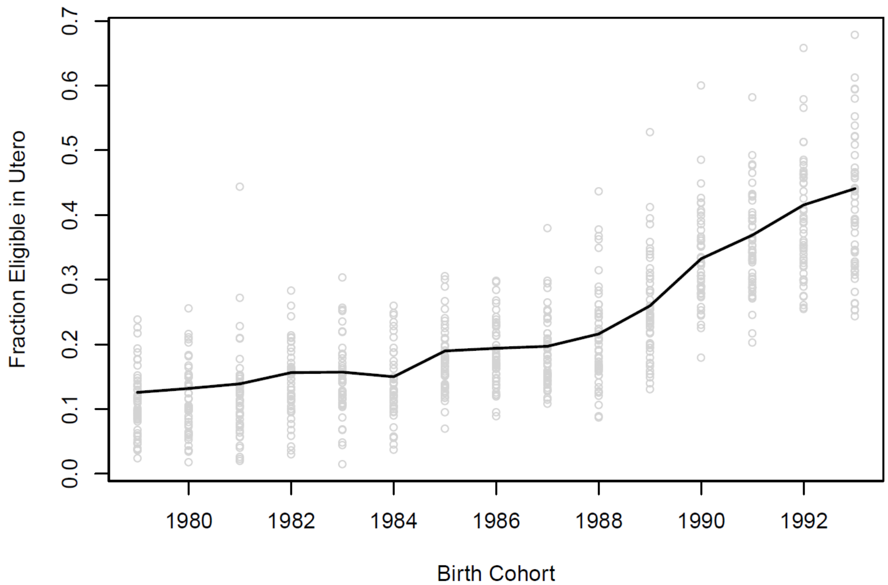
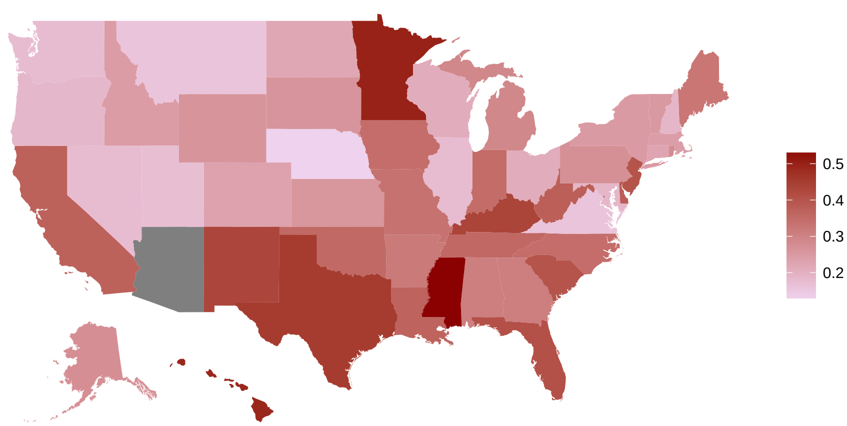
Figure 1: Fraction of women ages 15-44 eligible for Medicaid prenatal coverage in the event of a pregnancy, 1979 to 1993

(a) National and state estimates by year



Note: Solid line connects national estimates of prenatal eligibility over time while other data points represent state prenatal eligibility estimates for each year.

(b) Change from 1979 to 1993 by state



Note: Shading indicates change in state-level estimates of prenatal eligibility from 1993 to 1979.
Source: Eligibility estimates presented here based on 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. Instrumental Variables Estimates of the Effect of In Utero and Infant Coverage on Chronic Health Conditions, NHIS 1998-2015

				Components of index											
	Chronic condition index			Diabetes			High blood pressure			Obesity			Heart attack or other heart disease		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Prenatal eligibility	-0.383***	-0.308***	-0.329***	-0.033**	-0.020	-0.032**	-0.124***	-0.057	-0.116**	-0.145	-0.152	-0.129	-0.061*	-0.076**	-0.043
	(0.091)	(0.107)	(0.108)	(0.013)	(0.017)	(0.016)	(0.042)	(0.053)	(0.046)	(0.090)	(0.094)	(0.086)	(0.033)	(0.038)	(0.034)
Region x birth year fixed effects		X			X			X			X			X	
State-specific birth year trends			X			X			X			X			X
Mean of dependent variable					0.012			0.080			0.220			0.037	
N		58,900			60,318			60,295			58,948			60,303	

Notes: This table displays instrumental variable regression results using the 1998-2015 National Health Interview Survey. Robust standard errors clustered by state of birth are in parentheses. All models include individual characteristics (sex, race, ethnicity, age dummies), state-year of birth control variables (see text), survey year, state of birth, and year of birth fixed effects. Models include region by year fixed effects or state-specific linear trends in birth year when indicated. All regressions are weighted. First stage is reported in Table A.4. Significance levels: * = significant at the 10% level, ** = significant at the 5% level, *** = significant at the 1% level.

Table 3. Instrumental Variables Estimates of the Effect of In Utero and Infant Coverage on Other Health Measures, NHIS 1998-2015

	Very good or excellent health			Kessler 6 score		
	(1)	(2)	(3)	(4)	(5)	(6)
Prenatal eligibility	-0.007	0.016	-0.006	-1.247**	-1.247*	-0.840
	(0.032)	(0.038)	(0.041)	(0.607)	(0.692)	(0.840)
Region x birth year fixed effects		X			X	
State-specific birth year trends			X			X
Mean of dependent variable		0.753			2.67	
N		140,097			59,451	

Notes: This table displays instrumental variable regression results using the 1998-2015 National Health Interview Survey. Robust standard errors clustered by state of birth are in parentheses. All models include individual characteristics (sex, race, ethnicity, age dummies), state-year of birth control variables (see text), survey year, state of birth, and year of birth fixed effects. Models include region by year fixed effects or state-specific linear trends in birth year when indicated. All regressions are weighted. First stage is reported in Table A.4. Significance levels: * = significant at the 10% level, ** = significant at the 5% level, *** = significant at the 1% level.

Table 4. Instrumental Variables Estimates of the Effect of In Utero and Infant Coverage on Adult Hospitalizations, NIS 1998-2011

	All visits excluding pregnancy-related visits			Chronic condition related visits			Type of chronic condition visit						Mental health related visit		
							Diabetes/obesity related			High blood pressure/heart related					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Prenatal eligibility	-0.237**	-0.184	-0.260**	-0.766**	-0.560*	-0.555*	-1.034***	-0.958***	-0.840**	-0.323	-0.227	-0.0309	-0.0268	-0.0824	0.0337
	(0.109)	(0.119)	(0.114)	(0.321)	(0.328)	(0.310)	(0.394)	(0.353)	(0.392)	(0.324)	(0.338)	(0.464)	(0.190)	(0.231)	(0.144)
Region x birth year fixed effects		X			X			X			X			X	
State-specific birth year trends			X			X			X			X			X
Incidence (per 10,000 individuals)		374.0			25.3			15.5			9.8			77.7	
N		3,527			2,836			2,653			2,221			2,643	

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, and state by birth year control variables (see text). Additionally, region by birth year fixed effects and state-specific linear trends in birth year are included where indicated. First stage is reported in Table A.4. 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 Infant Coverage on Adult Human Capital, ACS 2000-2015

	High School Graduate			Some College or More			Personal Income (Logs)			Food Stamps		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Prenatal eligibility	0.028***	0.018***	0.011**	0.035***	0.016	0.014	0.116***	0.087**	0.061	0.013	0.026*	-0.013
	(0.006)	(0.007)	(0.005)	(0.010)	(0.012)	(0.011)	(0.033)	(0.036)	(0.039)	(0.014)	(0.014)	(0.011)
Region x birth year fixed effects		X			X			X			X	
State-specific birth year trends			X			X			X			X
Mean of dependent variable (std. dev.)		0.919			0.633			$32,468.54 ($33,161.37)			0.170	
N		6,870,000			6,310,000			3,360,000			3,760,000	

Notes: This table displays instrumental variable regression results using the 2000-2015 American Community Survey. The number of observations is rounded to the nearest 10,000 following Census disclosure rules. Robust standard errors clustered by state of birth are in parentheses. All models include individual characteristics (sex, race, ethnicity, age dummies), state-year of birth control variables (see text), survey year, state of birth, and year of birth fixed effects. Models include region by year fixed effects or state-specific linear trends in birth year when indicated. All regressions are weighted. First stage is reported in Table A.4. Significance levels: * = significant at the 10% level, ** = significant at the 5% level, *** = significant at the 1% level.

Table 6. Hetergeneous Effects: Targeted vs Broad Prenatal Expansions

NHIS outcomes

	Chronic condition index	Components of index				Very good or excellent health	Kessler 6 score
		Diabetes	High blood pressure	Obesity	Heart attack or other heart disease		
Targeted eligibility	-0.683***	-0.030	-0.278***	-0.294*	-0.084	0.028	-2.185*
	(0.234)	(0.038)	(0.095)	(0.154)	(0.068)	(0.100)	(1.175)
Broad eligibility	-0.019	-0.001	0.012	0.038	-0.022	0.012	-0.343
	(0.066)	(0.014)	(0.044)	(0.064)	(0.035)	(0.055)	(1.076)

NIS outcomes

	All visits (excl pregnancy-related)	Chronic condition related visits	Type of chronic condition visit		Mental health related visit
			Diabetes/obesity	High blood pressure/heart related	
Targeted eligibility	-0.281*	-0.803*	-1.384***	-0.0124	-0.0136
	(0.147)	(0.446)	(0.476)	(0.555)	(0.233)
Broad eligibility	-0.0363	0.142	0.139	-0.0270	-0.0994
	(0.0755)	(0.172)	(0.259)	(0.276)	(0.106)

ACS outcomes

	High school graduate	Some college or more	Personal income (Logs)	Food Stamps
Targeted eligibility	0.031**	0.040*	0.200***	-0.041**
	(0.013)	(0.022)	(0.062)	(0.016)
Broad eligibility	0.003	-0.021**	---	---
	(0.008)	(0.009)		

Notes: This table displays instrumental variable results from two separate regressions that estimate the effects of the targeted and broad Medicaid eligibility expansions. The sample for broad eligibility expansions is limited to cohorts born in 1987 and later. NHIS and ACS regressions include individual characteristics, state of birth, year of birth, survey year dummies, state of birth by birth year characteristics, and state of birth trends in birth year. NIS regressions include include state by year and birth year fixed effects, state by birth year control variables, and state trends in birth year. Significance levels: * = significant at the 10% level, **= significant at the 5% level, ***=significant at the 1% level.

Table 7. Heterogeneous Effects by Race Group

NHIS outcomes

	Chronic condition index	Components of index				Very good or excellent health	Kessler 6 score
		Diabetes	High blood pressure	Obesity	Heart attack or other heart disease		
Blacks	-0.736***	-0.098**	-0.151	-0.370	-0.099	0.049	0.738
	(0.245)	(0.049)	(0.124)	(0.228)	(0.074)	(0.120)	(1.317)
Whites	-0.204*	-0.016	-0.088*	-0.076	-0.018	-0.023	-0.824
	(0.116)	(0.019)	(0.050)	(0.085)	(0.040)	(0.050)	(1.039)

NIS outcomes

	All visits (excl pregnancy-related)	Chronic condition related visits	Type of chronic condition visit		Mental health related visit
			Diabetes/obesity	High blood pressure/heart related	
Blacks	-0.0931	0.926	-0.405	-0.166	-0.380
	(0.271)	(0.792)	(1.176)	(1.329)	(0.416)
Whites	-0.290**	-0.598	-0.979	0.512	-0.00614
	(0.140)	(0.392)	(0.673)	(0.634)	(0.228)

ACS outcomes

	High school graduate	Some college or more	Personal Income (Logs)	Food Stamps
Blacks	0.027	0.007	0.167	0.043
	(0.018)	(0.036)	(0.103)	(0.044)
Whites	0.007	0.011	0.013	-0.018
	(0.005)	(0.012)	(0.044)	(0.012)

Notes: This table displays instrumental variable results from two separate regressions that estimate the effects of the Medicaid eligibility expansions for black and white cohorts. NHIS and ACS regressions include individual characteristics, state of birth, year of birth, survey year dummies, state of birth by birth year characteristics, and state of birth trends in birth year. NIS regressions include include state by year and birth year fixed effects, state by birth year control variables, and state trends in birth year. Significance levels: * = significant at the 10% level, **= significant at the 5% level, ***=significant at the 1% level.