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MULTI-GENERATIONAL IMPACTS OF CHILDHOOD ACCESS TO THE SAFETY NET: EARLY LIFE EXPOSURE TO MEDICAID AND THE NEXT GENERATION'S HEALTH

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ABSTRACT

We examine multi-generational impacts of positive in utero and early life health interventions using state-year variation in public health insurance expansions that targeted low-income pregnant women and children. We use restricted use Vital Statistics Natality files to create a unique dataset linking individuals' childhood Medicaid exposure to the next generation's health outcomes at birth. We find robust evidence that the health benefits associated with treated generations' early life access to Medicaid extend to later offspring's birth outcomes. Our results imply that the return on investment is larger than suggested by evaluations of the program that focus only on treated cohorts.

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There is substantial evidence that health and socioeconomic inequalities persist across generations. A growing number of studies suggest that differences in early life health environments may causally contribute to these disparities. Negative shocks to the *in utero* environment, in particular, have been found to be harmful to individuals' later life health and earnings. A handful of studies also examine positive interventions and find that policies intended to improve early life experiences generate better adult outcomes.[1] By extension, literatures in economics, epidemiology, and child development predict that the causal impacts of these interventions should echo beyond the exposed generation. Little is known, however, about the extent to which the early life environment impacts future generations, or the potential for public policy to alter such linkages.

We consider whether positive public health interventions experienced *in utero* and during childhood subsequently affect the next generation's health. We focus on the impact of the largest source of health-related services for low-income children in the United States: the Medicaid Program. Changes in eligibility rules during the 1980s and 1990s, particularly for low-income pregnant women and children who were not otherwise tied to the welfare system, led to a dramatic increase in individuals' prenatal and early childhood Medicaid eligibility.[2] The additional coverage provided to pregnant women under the expansions represents the single largest effort by the federal government to improve birth outcomes. There was considerable variation in the timing and magnitude of these expansions across states, which prior empirical research has harnessed to examine the

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[1] See Almond and Currie (2011a, 2011b) and Almond, Currie and Duque (2017) for extensive summaries of this literature.

[2] Our empirical strategy exploits both changes in Medicaid and the creation and expansion of the State Children's Health Insurance Program. In what follows, we refer to both as "Medicaid."

program's effects on cohorts who gained access *in utero* and during childhood.[3] We build on research documenting effects on this "first generation" to investigate whether positive policy interventions in one generation transmit to the next generation.

Our analyses make several contributions to the literature relating the early life environment to later outcomes. First, the vast majority of studies establishing a causal relationship between early life health experiences and adult outcomes confine their analyses to treated cohorts. While an ever-expanding number of animal experiments provide substantive evidence that early life environmental effects can be transmitted to later generations,[4] human studies are nearly non-existent. We move the "early origins" literature forward by using a quasi-experimental design to document similar multi-generational effects in humans.

We are also the first to investigate whether the effects of a large-scale, positive, U.S. health intervention persist to later generations. Most of what we know about the long-run effects of early life conditions comes from studies of extreme, negative health experiences such as famine and disease outbreaks, which are difficult to extrapolate to the current policy environment. A much smaller literature is beginning to leverage variation in means tested programs to investigate whether positive interventions that generate more typical differences in early childhood experiences, affect exposed cohorts' long-term outcomes, but research investigating whether such interventions transmit beyond treated cohorts to subsequent generations is nearly non-existent. This is an important gap--particularly in light of current political debates about the cost of publicly-provided

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[3] See for example: Brown, Kowalski and Lurie (2017), Cohodes et. al (2016), Currie and Gruber (1996a,1996b), Currie, Decker, and Lin (2008), Dave et al. (2008), Howell et al. (2010), Levine and Schanzenbach (2009), Miller and Wherry (2018), Thompson (2017).

[4] Useful reviews of this literature include Daxinger and Whitelaw (2010; 2012), Heard and Martienssen (2014), Hochberg et. al. (2011), Nadeau (2009).

health insurance—as substantive multiplier effects would suggest that existing benefit-cost calculations underestimate the true value of government investments in children's health.

Our analyses are based on information that is available in the 1994-2015 Vital Statistics Natality files. We use restricted access versions of the Vital Statistics files that include information on mothers' state and exact date of birth to create a unique dataset that links information on individuals' *in utero* and childhood Medicaid eligibility to later offspring's health at birth. Following the pioneering work of Currie and Gruber (1996a,1996b), we use a simulated measure of maternal eligibility that isolates variation in health insurance access resulting from policy changes, rather than socioeconomic factors, and we employ a variant of a difference-in-differences model, where treatment varies by mothers' state of birth and year of birth. We include treatment variables for mothers' *in utero* and childhood eligibility, along with maternal state of birth and year of birth fixed effects and a number of state-year covariates that help control for other policies, health trends, economic, and demographic conditions that prevailed in the state and year the mothers were born.

We analyze health outcomes among infants whose mothers were born between 1979 and 1986, a period when there were dramatic increases in Medicaid coverage. We estimate Medicaid's impacts on the second generation's birth weight and length of gestation, which are known to be predictive of later life health and economic outcomes. We also estimate distributional effects of eligibility on these variables and document how eligibility affects the incidence of prematurity, low birth weight, and very low birth weight, as these outcomes are closely tied to other measures of infant health.

We find that mothers' early life Medicaid eligibility positively impacts their children's health at birth. For birth weight outcomes, the estimated effects of *in utero* eligibility are about ten times as large as the estimated effects associated with one additional year of eligibility later in childhood. The positive effects of mothers' eligibility on offspring's average birth weight are statistically significant and remarkably stable across a variety of specification checks, including the addition of region by cohort fixed effects, alternative state and year control variables, and different sample definitions, measures of eligibility, and weighting. Strong patterns in the estimates also point towards beneficial effects on the next generation's incidence of low birth weight, very low birth weight, and likelihood of being born prematurely.

Documenting the *presence* of multi-generational spillovers is an important contribution in its own right; however, back-of-the envelope calculations suggest that the magnitude of the spillovers may be economically important. For example, our point estimates of the effects of *in utero* Medicaid access on the next generation's probability of being low birth weight suggest medical cost savings in the first year of life that are about 30 percent of the costs of providing the first generation with *in utero* coverage. Importantly, this calculation does not include other types of benefits accrued to the second generation, such as savings related to medical costs and social supports accrued at older ages. Nor does it include benefits due to documented improvements in the first generation's health and economic outcomes. If these benefits were to be incorporated, the costs savings would likely be substantially larger.

We consider several possible mechanisms, including changes in fertility patterns among the treated generation. Medicaid-induced changes in later life fertility might affect the next generation's observed health by altering the timing of birth in a way that promotes health during pregnancy (such as avoiding a teenage birth) or by changing the composition of women who choose to give birth. We find no consistent evidence that Medicaid changes the number or timing of births. Our analyses do indicate a composition shift towards more white births, which tend to be healthier, but this cannot explain more than about a third of our estimated increase in the second generation's birth weight. Moreover, within racial subgroups the effects are similar. We also find a shift in births towards women who did not graduate from high school, which should negatively influence our estimated health effects. Considering changes on all observable characteristics together, we examine whether including controls for these (endogenous) demographics affects our main estimates and they do not. We conclude, therefore, that much of the observed change in the second generation's outcomes reflects direct improvements in their health, rather than a change in the composition of who selects in to childbearing.

Our results establish that public investments in prenatal health have persistent impacts beyond the treated generation. By quantifying these effects, we establish that benefit/cost ratios based only on cohorts immediately affected by Medicaid underestimate the program's overall efficacy. This finding has potential implications for a broad range of interventions that may also result in multigenerational effects.

The remainder of our paper proceeds as follows: Section I provides further information about the existing literature on "early life" health and multi-generational processes. In Section II, we describe the Medicaid program and the nature of the 1980s expansions. Sections III and IV describe our empirical strategy and data. We present our results in Section V and conclude with a discussion in Section VI.

I. Background

I.A. The Fetal Origins Hypothesis and Related First Generation Research in Economics

Twenty-five years ago, David Barker (1990) put forward a provocative hypothesis that the period of gestation has significant impacts on individuals' health that reach into adulthood. Since then, there has been growing scientific agreement that the time both before, and immediately after, birth are critical periods when the developing body takes cues from its surrounding environment, adapting to that environment in ways that may affect later life health. A key feature of the fetal origins hypothesis is that the health effects of the *in utero* environment can remain latent for many years. We have yet to achieve a full understanding of the processes underlying these phenomena, but a leading theory is that the fetus's surrounding environment alters genetic programming through the "switching on" of specific genes (Wadhwa et al., 2009).

Numerous economists and epidemiologists have used quasi-experimental designs to test the fetal origins hypothesis, and have found that *in utero* and early life health experiences can have important effects on later life outcomes. The vast majority of studies in economics have identified these effects using short-term events such as disease outbreaks and famines,[5] which are by nature both negative and extreme.

A handful of studies have recently emerged, however, demonstrating that widespread positive health interventions can also influence many measures of well-being in adulthood, including infant health, child cognition, and adult health and economic outcomes. These include studies of mother and child health centers (Butikofer, Loken and Salvanes 2017), water fluoridation (Glied and Neidell

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[5] Examples from the literature include Almond (2006), Almond and Mazumder (2005), Barreca (2010), Mazumder et al. (2009), Neelsen and Stratmann (2012) [disease], Chen and Zhou (2007), Painter, Roseboom and Bleker (2005), Ravelli, Stein, and Susser, (1976), Roseboom et al. (2001), Stein et al. (1975), Susser and Lin (1992), Scholte et al. (2015), Almond and Mazumder (2011), van Ewijk (2011), Almond et al. (2010) [nutrition]. Quasi-experimental studies of stress (Persson and Rossin-Slater 2016) and pollution (Sanders 2012) also find detrimental effects.

2010), antibiotic therapies (Bhalotra and Venkataramani 2015), surfactant and related treatments (Bharadwaj, Loken and Nielson 2013), and breast feeding encouragement programs (Fitzsimons and Vera-Hernandez 2014).[6] In addition, Hoynes, Schanzenbach and Almond (2016) find that *in utero* and early life access to the U.S. Food Stamp program leads to a large reduction in the incidence of conditions related to metabolic syndrome (such as obesity, high blood pressure, and diabetes) and, among women, an increase in economic self-sufficiency.

I.B. First Generation Effects of the Medicaid Program

In this vein, recent work has demonstrated that expansions in Medicaid eligibility for pregnant women and children during the 1980s and 1990s generated improvements in affected children's later life health. Focusing on variation generated by the 1980s expansions to pregnant women, Miller and Wherry (2018) find that *in utero* exposure to the program reduces the likelihood of having metabolic-syndrome and circulatory-system linked chronic illnesses in adulthood. Importantly for our study, when these later life diseases are experienced during pregnancy, women and their children are put at risk for a variety of health problems, including an increased risk of gestational diabetes, complications related to high blood pressure, and preterm birth (Catalano and Ehrenberg, 2006).

Other studies evaluate the long-run health effects of the 1980s and 1990s expansions to broader age groups, beyond the *in utero* period.[7] Currie, Decker,

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[6] Related literatures examine the long-term effects of education interventions such as Head Start (e.g. Carneiro and Ginja (2014), Deming (2009), Garces, Thomas and Currie (2002), Ludwig and Miller (2007)) and policies that reduce pollution exposure (Nilsson 2009; Isen, Rossin-Slater and Walker, 2017).

[7] Three other studies document how the introduction of Medicaid between 1966 and 1970 improved later life health. Using geographic variation in program roll-out to identify the effects of exposure to Medicaid under age 6, Boudreaux, Golberstein and McAlpine (2016) find that Medicaid reduced the likelihood of having a chronic health condition in adulthood. Using a similar strategy, Sohn (2017) finds that Medicaid's initial roll-out was associated with lower adult mortality. Goodman-Bacon uses variation in pre-existing welfare eligibility levels, since Medicaid

and Lin (2008) find evidence suggesting that early childhood eligibility is associated with better health status in adolescence. Wherry and Meyer (2015) find that childhood Medicaid expansions reduced mortality rates among black teens, while Wherry et al. (2018) find evidence of fewer hospitalizations in adulthood. Thompson (2017) finds that eligibility for Medicaid or the State Children's Health Insurance Program (CHIP) is associated with improvements in a summary index of adult health measures, with eligibility early in childhood (age 0 to 5) generating the largest effects. Brown, Kowalski, and Lurie (2017) find that childhood eligibility reduces adult mortality.

These long-term effects are consistent with studies documenting that the expansions led to contemporaneous gains in health insurance coverage, and health care utilization (Buchmueller, Ham, and Shore-Sheppard, 2016). Studies of the 1980s prenatal Medicaid expansions find that they increased use and improved the timing and adequacy of prenatal care (Currie and Gruber, 1996b; Currie and Gruber 2001; Dubay et al., 2001; Dave et al., 2008; Howell, 2001) and led to reductions in the incidence of infant mortality and low birth weight (Currie and Gruber 1996a, 1996b). Since maternal health at birth is predictive of future generations' birth weight (e.g., Currie and Moretti, 2007; Black, Devereux, and Salvanes, 2007, Royer, 2009), it is probable that the generation of children whose immediate and later life health improved because of the 1980s Medicaid expansions would also eventually give birth to healthier children. Beyond providing increased access to medical care, the Medicaid expansions may have also improved early and later life health by reducing family medical spending, and freeing up resources for other types of family investment. There is evidence that policies that increase family income lead to better child health (Kehrer and

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was originally linked to welfare receipt, and finds that the introduction of Medicaid reduced later life mortality and disability for white cohorts who were exposed to the program early in life (2016).

Wolin, 1979; Almond, Hoynes, and Schanzenbach, 2011; Hoynes, Miller and Simon, 2015), possibly due to income induced changes in parental behaviors, such as receipt of prenatal care or reduced smoking (Hoynes, Miller, and Simon, 2015). Along these lines, the expansions may have also affected children's health by reducing maternal stress: in an analysis of the Oregon Health Insurance Experiment, Finkelstein et al. (2012) find that those who gained health insurance through the experiment experienced substantive improvements in mental health, and there is evidence that stressful events experienced *in utero* affect later well-being (Aizer et. al. 2016; Black, Devereux, and Salvanes, 2016; Camacho, 2008; Mansour and Rees, 2012; Persson and Rossin-Slater, 2016; Valente, 2011). Psychologists also posit that children's experience of parental stress has important influences on their development (Conger et al. 1994).

Regardless of the exact processes by which *in utero* and early life Medicaid access improves health, it is easy to imagine how the documented health gains experienced by one generation might transfer to later generations. Indeed, a wealth of animal experiments described below confirm that biological mechanisms alone are likely to generate multi-generational spillovers. Beyond direct biological effects, however, several studies find that childhood exposure to public health insurance improves later life economic outcomes, which may have an independent effect on the next generation's well-being. Miller and Wherry (2018) find increased rates of high school graduation associated with *in utero* Medicaid eligibility, while Levine and Schanzenbach (2009) find evidence of improved test scores resulting from increased eligibility at the time of birth. Cohodes et al. (2016) examine childhood exposure to Medicaid and CHIP from birth to age 17 and find evidence of increased rates of high school and college completion. Brown, Kowalski, and Lurie (2017) find that childhood exposure to public insurance increased college enrollment, decreased receipt of the Earned

Income Tax Credit, and had a positive effect on females' adult earnings. These economic gains may have resulted in health improvements among treated cohorts' children.

I.C. Experimental Evidence on the Multigenerational Effects of Early Life Health Environments

Taken as a whole, the existing literature generates two broad conclusions. First, early life health shocks have long-term impacts on the health and economic outcomes of those who experience them. Second, many widespread public health interventions targeted at children have substantive positive benefits that last well into adulthood. A natural question is whether these effects endure to the next generation. Economists have previously documented that health and economic status persist across multiple generations (Solon, 2015; Clark, 2014), but quasi-experimental investigations are rare. We know little about what drives the correlations, or the potential for policy-based treatments to alter them. The dearth of work among social scientists likely results from multiple challenges of identifying exogenous variation in early life health environments *and* linking that variation to data that provides relevant information on *multiple* generations.

These challenges can be overcome in biological studies, where an accumulation of evidence based on animal experiments finds that prenatal health shocks have persistent generational effects. As an example, studies have documented that rats that are malnourished before or during pregnancy produce offspring with smaller brains and reduced cognition, even if the offspring receive sufficient nutrition after birth. Importantly, these effects are not only observed in the immediate offspring, but are present in the next generation as well.[8] Similar

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[8] A few examples include Zamenhof, Marthens and Grauel (1971), Cowley and Griesel (1966), Aerts and Van Assche (2006), Dunn and Bale (2009), Jimenez-Chillaron et al. (2009), Martinez et. al. (2014). Recent reviews of the literature on transgenerational epigenetic inheritance include

multi-generational patterns have been found with in-utero exposure to stress, and smoke.[9] One explanation for this pattern is that the biological predecessors of the ovaries and sperm cells, which will produce the next generation, are already present at the fetal stage, and are therefore exposed to any insult experienced by the fetus.

I.D. Natural Experiment Evidence on Multigenerational Effects

In spite of the methodological challenges, a few studies have been able to extend the use of historical "shocks" to look at how they affected the next generation, and have found evidence of persistent effects. Painter et al. (2008) investigate the multi-generational impacts of the Dutch Hunger Winter of 1944-1945, and find that the offspring of those exposed to extreme famine *in utero* experienced worse health in later life. Van den Berg and Pinger (2016) investigate the transgenerational effects of pre-pubertorial exposure to the German famine of 1916-1918 and find evidence of mental health effects on later generations, which they attribute to biological rather than social processes. Looking beyond the effects of extreme nutritional deprivation to the transgenerational impacts of disease exposure, Richter and Robling (2013) find that the children of those who were exposed to the 1918-1919 influenza pandemic *in utero* grew up to have lower levels of educational attainment. Similarly, Black et al. (2013) find that Norwegian cohorts exposed to radioactive fallout during the *in utero* period had children with lower cognitive ability. Focusing on later childhood disease exposure, Butikofer and Salvanes (2015) find that

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Daxinger and Whitelaw (2010), Daxinger and Whitelaw (2012), Grossniklaus et al. (2013), and Heard and Martienssen (2014).

[9] Examples include Iqbal et al. (2012), Grundwald and Brunton (2015), Morgan and Bale (2011), Rehan et al. (2012), and Maritz and Mutemwa (2014).

intergenerational persistence in educational attainment was mitigated by a 1940s Norwegian tuberculosis control program.

One study uses more recent data to examine persistent impacts of broader disease exposure. Almond, Currie and Herrmann (2012) use U.S. Vital Statistics data to examine how state level variation in infant mortality rates at the time of the mother's birth –which could be driven by many factors, including variation in access to medical care—relate to her offspring's health. They find that higher infant mortality in the year after the mother is born is associated with an increase in the probability that her baby will be born below the low birth weight threshold for whites, and the opposite effect for blacks. This is interpreted as the result of selective survival among blacks, with increased survival among women at lower risk of having low birth weight babies.

Very few studies have examined the multi-generational effects of "positive" shocks, or interventions. Of these, most investigate multi-generational effects of interventions targeting the post-natal period. Almond and Chay (2006) find that the racial gap in infant health decreased among the offspring of cohorts who benefited from Title VI of the Civil Rights Act, which expanded black infants' access to health care. Also, as part of their study on the long-term impacts of Norwegian mother and child health centers, Butikofer, Loken and Salvanes (2017) estimate that the centers reduced the intergenerational persistence of educational attainment by 10 percent. Two recent analyses of early life educational interventions in the U.S. and Denmark, also find evidence of generationally persistent economic and social effects (Barr and Gibbs 2017; Rossin-Slater and Wüst 2016). Importantly, the Head Start program, which is the focus of Barr and Gibbs' analyses, also provided a range of community development and health components, including nutrition, vaccinations, and dental services.

We build on this small number of studies by harnessing a policy-driven increase in access to a widespread public health program that is a critical component of the U.S. safety net. This allows us to establish multi-generational linkages associated with more common and contemporaneous variation in early life health experiences, while simultaneously quantifying long-term benefits of the Medicaid program that have not previously been measured.

II. Medicaid and the 1980s Expansions

Medicaid is the largest means-tested transfer program in the United States. It provides insurance coverage for four out of every ten children and nearly half of all births (Kaiser Family Foundation, 2018; Markus et al., 2013). Begun in 1965 as part of the Social Security Amendments, it is a joint federal-state program: the federal government sets important requirements, but states have flexibility in terms of eligibility rules, program benefits, reimbursement amounts and other aspects of their programs.

Until the 1980s, coverage for pregnant women and non-disabled children was primarily limited to families who received cash welfare under the Aid to Families with Dependent Children Program (AFDC). AFDC income eligibility thresholds varied by state, and were generally much lower than the federal poverty line. The average threshold was 61% of the federal poverty line (FPL) in 1979, and ranged from 24% to 99%.[10] Moreover, AFDC eligibility was largely restricted to single parent families.[11]

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[10] Authors' calculation based on payment standard for a family of 3 in 1979.

[11] Under the optional AFDC Unemployed Parent program, married parent families were able to receive benefits when the principal earner was unemployed, but the eligibility criteria were stringent, and in 1979 only 6% of families on AFDC included two parents (Duvall, Goudreau and March, 1982). In addition, states could choose to cover first-time pregnant women under their

Restrictions on AFDC participation meant that the vast majority of low-income pregnant women and children living in two parent families were not eligible for Medicaid, nor were most unmarried women who were pregnant for the first time. Starting in the 1980s, however, Medicaid coverage was greatly expanded to pregnant women and children not qualifying for AFDC benefits. Our identification strategy takes advantage of the tremendous variation in the timing and magnitude of the eligibility expansions across states. The first phase of expansions was targeted towards pregnant women and children with family incomes below AFDC eligibility thresholds, but who were not in single mother families. New options introduced in 1982 allowed states to extend Medicaid eligibility to first-time pregnant women who would later qualify for AFDC, as well as to pregnant women and "unborn children" who were income and resource eligible for AFDC but did not meet the family structure requirements for the program.[12] In addition, between 1984 and 1988, the federal government phased in a series of mandates for states to cover all pregnant women and children under age 7 meeting the income and resource requirements for AFDC.

These "targeted expansions" predated "broad expansions"[13] that later extended Medicaid eligibility to families with higher income levels (often well above the AFDC income thresholds). Beginning in 1987, new options allowed

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AFDC programs, but the Omnibus Reconciliation Act of 1981 (OBRA81) restricted participation for these women until the sixth month of pregnancy (Currie and Gruber, 1994).

[12] Following restrictions on AFDC participation by first-time pregnant women under OBRA81, states were given the option of providing Medicaid coverage to these women even if they were not yet able to receive AFDC benefits. Twenty-seven states exercised this option in 1982. In addition, states could also cover unborn children under their Ribicoff children programs and, therefore, cover pregnant women who did not meet the family structure requirements for AFDC. Seventeen states took up this option in 1982. During the period of study, states also had options to extend Medicaid eligibility to pregnant women and children in two parent families who did not qualify for AFDC but had incomes below AFDC levels, as well as to "medically needy" individuals who higher incomes but high medical expenses. Further details on each of these state options are outlined in the appendix of Currie and Gruber (1996b).

[13] This terminology was first used by Currie and Gruber (1996b).

states to cover pregnant women and infants in families with incomes up to 185% of the FPL, and children under age 8 with family incomes below 100% FPL. Then, new federal requirements mandated the coverage of pregnant women and children under age 6 with family incomes below 133% FPL, and children under age 19 with family incomes below 100% FPL. Finally, the 1997 Balanced Budget Act gave states the option to extend eligibility to even higher income levels for children. Importantly, these federally mandated expansions had heterogeneous effects on states because there was substantial variation across states in terms of the generosity of Medicaid eligibility rules *before* these expansions, due to state variation in AFDC eligibility criteria. Detailed information on these legislative changes may be found in the Appendix and Appendix Table 1.

As described in Section III, we examine the multi-generational effects of both *in utero* and later childhood Medicaid access. Our analyses focus on the offspring of first generation cohorts who were born between 1979 and 1986, for whom *in utero* access to Medicaid was affected by the targeted expansions, and later childhood eligibility was affected by both the targeted and the broad expansions. The *in utero* period is fundamentally different from other stages of child development. Moreover, pregnant women who enrolled in Medicaid received coverage for prenatal care and services, hospital and postpartum care, and one year of Medicaid eligibility for their newborns (Congressional Research Service 1988). Therefore, we include in our regressions two different measures of Medicaid access that separately capture each cohort's *in utero* eligibility and average years of eligibility between ages 1 and 18.

Figure 1 shows how the changes in Medicaid eligibility rules described above affected the fraction of children who were eligible while *in utero*, and at older ages.[14] Among those born in 1979, about 13% were eligible for *in utero* coverage. This cohort was also eligible for an average of 2.9 years between ages 1 and 18. Among children born in 1986, the fraction who were eligible for Medicaid was much higher: more than 19% of the 1986 cohort was eligible for *in utero* coverage (a 6.8 percentage point increase), and, between ages 1 and 18, the 1986 cohort was eligible for an average of 5.5 years. To demonstrate the resulting variation in the magnitude of the expansions across states, Figure 2 panel (a) shades states by the size of the change to *in utero* eligibility for our oldest and youngest cohorts. The bottom quartile states increased *in utero* eligibility by less than 4.2 percentage points. In contrast, states in the top quartile increased eligibility by more than 10.7 percentage points. Similarly, panel (b) shows large differences in expansions in childhood eligibility across states. The bottom quartile states expanded childhood eligibility by less than 1.8 years on average, and the top quartile expanded by more than 3 years. Interestingly, there are many states that fall into the top quartile for *in utero* expansions, but not for childhood expansions, and vice versa.

III. Empirical Strategy

We evaluate how state and federal policies that increased early life Medicaid eligibility affected later offspring's birth outcomes. Our main regression equation is:

$$(1)\quad y_{nb} = \alpha + \beta_1 InUteroMedicaid_{nb} + \beta_2 MedicaidAges1\_18_{nb} + \mu_n + \lambda_b + \gamma X_{nb} + \varepsilon_{nb}$$

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[14] Authors' calculations using the Current Population Survey, described in more detail below.

where y_{nb} is the average health outcome for infants whose mothers were born in state *n* and year *b.* We refer to the mothers as the “first” (exposed) generation, and to the infants as the “second” generation. The variable $InUteroMedicaid_{nb}$ measures the fraction of women in the first generation’s birth state, between the ages of 15 and 44, who would have been eligible for Medicaid in the event of a pregnancy during the first generation’s birth year. We generate this variable using the Current Population Survey (CPS), which contains information on the income, demographic characteristics, and the state and year of residence for a sample of 15-44 year old women. We use this information to calculate their likely eligibility if they were to become pregnant.[15] The coefficient β_1 is the effect of increasing the first generation’s *in utero* eligibility from 0% to 100%. Put differently, it is the effect of providing 100% of the second generation’s *grandmothers* with Medicaid coverage during their pregnancies. The average fraction of women who would have been eligible for Medicaid if they had become pregnant between 1979 and 1986 is 16%.

The coefficient β_2 is the effect on the second generation of providing the first generation with an additional year of childhood eligibility. $MedicaidAges1\_18_{nb}$ is the sum, across ages 1-18, of the fraction of the first generation’s cohort (based on state and year of birth) who were eligible for Medicaid at each age. The fraction eligible at each age is calculated using information in the CPS on each child’s family income, demographic characteristics, and state and year of residence, similar to the measure described above for the *in utero* period. We then sum these measures across ages 1-18. This variable could change by one unit if 100% of the maternal cohort gained an additional year of eligibility sometime between ages 1 and 18. Alternatively, a one-unit change would occur if 50% of the mother’s birth cohort became eligible

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[15] We cannot observe pregnancy status in the CPS.

for an additional 2 years, or if 25% of mothers became eligible for an additional 4 years. In theory, $MedicaidAges1\_18_{nb}$ can take on any value between 0 and 18, but in practice the mean of the variable is 4.1 years.

Equation (1) includes fixed effects for the mother's state of birth, μ_n, to account for fixed differences in the outcomes of mothers and their children that differ across states. We also include mother year of birth fixed effects, λ_b, to account for national shocks over time. With these controls, our identification relies on within state changes over time in the fraction of a cohort that is eligible for Medicaid. The identifying assumption is that state changes in Medicaid eligibility were not correlated with other state changes that also affected the first or second generation's outcomes. However, the fraction of a cohort that was Medicaid-eligible in a given state may vary due to factors besides changes in Medicaid policy. For example, if a state experienced a recession that reduced average income, more of the population may have become eligible for Medicaid, even if the rules surrounding Medicaid eligibility did not change. These changes in the economic environment may have also directly affected health outcomes.

We address this possibility in multiple ways. First, we employ an instrumental variables approach, pioneered by Currie and Gruber (1996a, 1996b), which isolates changes in eligibility that are driven *only* by variation in program eligibility rules and are independent of states' demographic composition. We do this by constructing a measure of "simulated eligibility" for each birth cohort's *in utero* period that is based on a fixed national random sample of women ages 15-44, in each CPS survey year. To this fixed national random sample, we apply the Medicaid eligibility rules in each state and year. This provides us with a state-year estimate of the fraction of women who would be eligible for Medicaid if they were to become pregnant that is unrelated to changes in state demographic characteristics, and varies only because of state specific changes in Medicaid policies. We use this “simulated eligibility” measure as an instrument for the actual fraction of women who would be Medicaid-eligible in each state and year upon pregnancy.

We also construct analogous instruments for childhood eligibility using fixed national random samples of children at each age between 1 and 18.[16] Specifically, we calculate, using these fixed national random samples, the fraction of children in each state, year, and age that would be eligible for Medicaid. We then aggregate these estimates across years, to create a measure of cumulative “simulated eligibility” throughout childhood for each cohort and state and use this as an instrument for actual childhood eligibility.

To further address the possibility that there were changes in state characteristics, or other state policies, that affected first or second generation outcomes *and* were correlated with the Medicaid policy changes, we also include a set of maternal state and year of birth control variables (X_{nb}). These include information on state demographic characteristics, economic characteristics, and policy variables and are described in Section IV. We also run a version of equation (1) that includes state-specific first generation cohort trends, region by first generation cohort fixed effects, and state-year controls at the time of the *second* generation’s birth. We discuss this analysis in Section V.

Our baseline regressions are weighted by the size of the maternal birth cohort, but we explore the robustness of our results to alternative weighting schemes, which we discuss in more detail below. We cluster our standard errors by mothers’ state of birth.

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[16] To create the *in utero* instrument, we use a random sample of 3,000 women from each CPS survey year. We construct measures of eligibility between ages 1 to 18 by taking a random sample of 1,000 children of each age in each CPS survey year.

IV. Data

Our main analyses are based on restricted-use versions of the 1994-2015 U.S. Vital Statistics Natality Data Files, which contain individual birth records for the full census of U.S. births. The Vital Statistics files include information on infants' health and year of birth, as well as detailed demographic information about each infant's mother (the first generation) including her year of birth and state of birth. The latter variables are critical, as they allow the measures of Medicaid eligibility to be matched to each mother. We exclude mothers who were born outside of the United States, and those born in Arizona, which did not adopt a state Medicaid program until 1982.

We examine health outcomes among infants whose mothers who were born between 1979 and 1986. These mothers are old enough to have been affected by the 1980s Medicaid expansions and have children, although they have not yet completed their childbearing years: while births can be observed for some cohorts through age 36, the youngest cohort is only observed through age 28. During our time frame, 72% of first births, and 59% of all births, were to women aged 28 or younger.[17] Our main specification restricts the sample to mothers between the ages of 15 and 28, which ensures that each maternal cohort contributes equally to the identifying variation, and that the analysis of second generation birth outcomes is based on births to women who are the same age. We also conduct additional analyses that include all mothers over the age of 15 (born between 1979 and 1986), and that explore heterogeneous effects across teen and non-teen mothers.

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[17] Authors' calculations from the Vital Statistics Natality Files.

We collapse the data into cells based on mother's state of birth and mother's year of birth. For each cell we calculate the second generation's average birth weight and average gestational weight (in weeks), as well as the fraction of births that are low birth weight (<2500 grams), the fraction that are very low birth weight (<1500 grams), the fraction of births that are preterm (< 37 weeks), and the fraction that are very preterm (<2 8 weeks). Finally, we look at the fraction that are small for gestational age (birth weight < 10th percentile for a given gestational age).[18] Some analyses examine the fertility of each maternal cohort, for which we calculate the cohort's birthrate, the rate of first births, average age at first birth, and average number of live births at the time of the infant's birth. In addition, we examine changes in the characteristics of mothers giving birth including their race, educational attainment, and marital status at the time of birth, as well as information on health conditions and behaviors during pregnancy. For these analyses, we calculate the average characteristic for each mother's state of birth and mother's year of birth cell.

We then merge each cell with corresponding measures of actual and simulated Medicaid eligibility, and with information on states' economic conditions (state unemployment rate and per capita income), demographic composition (average age, marital status, educational attainment and race), safety net generosity, and abortion policies. Additional details about these control variables and sources are provided in the Appendix.

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[18] The infant mortality rate would also be of interest as a measure of infant health; however, infant mortality data that include mother's year and state of birth during our time frame are currently unavailable.

V. Results

Table 1 presents our main results for the primary outcomes: gestational length and average birth weight.[19] Each column provides estimates from specifications that address possible confounders by including different sets of controls. All six columns include state-year controls measured at the time of the mother's birth. The last three columns also include variables that measure state conditions and policies in place in the year in which the child is born (described in more detail below). Across all six specifications, the estimates consistently suggest that the first generation's early life Medicaid eligibility is positively associated with the second generation's health.

Focusing on the specification in Column 1, the point estimates indicate that increasing the share of the first generation with *in utero* eligibility from zero to 1 increases the second generation's average birth weight by 30 grams ($p<0.10$) or about 1% of the mean (3271 grams). The estimated second generation effect of providing one more year of Medicaid eligibility between ages 1 and 18 is 3 grams ($p<0.05$). This suggests that the persistent effects of a point-in-time intervention are substantially larger if they are delivered during the *in utero* period, relative to later childhood. The results align with biological experiments documenting the *in utero* environment's influence on later generations, and they complement Miller and Wherry's (2018) finding that *in utero* Medicaid exposure is particularly predictive of later life health and socioeconomic outcomes. The estimated effects

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[19] Like previous studies, our first stage estimates indicate that there is a very strong relationship between the simulated eligibility measures and actual eligibility: the diagonal coefficient estimates are 0.950 (in utero eligibility, standard error estimate of 0.049) and 1.020 (eligibility at ages 1-18, standard error estimate of 0.071), indicating that much of the variation in eligibility over this period is driven by policy changes rather than demographic shifts, and confirming that changes in Medicaid eligibility policies over this period had a large impact on the fraction of pregnant women and children who were eligible. The Kleibergen-Paap (2006) statistic (8.763) also indicates that our first stage has sufficient explanatory power to identify the parameters of interest. Appendix Table 3 provides estimates produced by OLS and reduced form regressions.

of *in utero* and childhood eligibility on the second generation's gestational length are also suggestive of positive effects, but not statistically different from zero.

Identification in our model comes from state variation in the timing and magnitude of the Medicaid expansions. In the ideal differences-in differences research design, pre-existing trends across states will be identical, so a potential concern with our identification strategy is that inputs into the next generation's health (including the first generation's health) were evolving differently in states that adopted the most generous Medicaid expansions. We cannot include mother's state of birth by mother's cohort fixed effects in the analysis because this would absorb all of our identifying variation, but in column 2 we include mother's region of birth by mother's cohort fixed effects, and show that their inclusion has virtually no effect on the estimates.

Another common specification check is to include state-specific trends, which in our case are trends in mother's birth year for each maternal birth state. A number of studies provide evidence that such models should be interpreted cautiously, however (e.g. Neumark et. al. 2014; Meer and West 2016; Wolfers 2006). One reason for caution is that the slope of the estimated state trend (and therefore the remaining identifying variation) will be sensitive to the choice of beginning and ending dates. This is a particularly salient issue in our case, where we have only eight cohorts with which to identify both the state trends and any shift in intercepts that are due to the expansions.[20] Nevertheless, as shown in column 3, our *in-utero* estimates are very robust to the inclusion of state trends, and the estimated effect of the first generation's *in utero* eligibility on the second generation's birth weight is similar to the estimate in Column 1. In contrast, the

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[20] Moreover, mother's year of birth is only available in natality records starting in 1989, so our ability to obtain accurate estimates of state trends in second generation outcomes for mothers who were born during the pre-expansion period, and in our sample age range, is limited.

estimate for older age Medicaid eligibility reverses sign and loses significance. Childhood eligibility may be more sensitive to the inclusion of state-specific trends due to the way that eligibility accumulates over childhood; when eligibility is expanded, newly eligible children born the year of the expansion will gain 18 years of coverage, those born one year prior will gain 17, etc. This type of accumulation may result in an exposure measure that is close to linear, making it difficult to separately identify state-specific linear trends.[21]

The last three columns show how the estimates change when we add controls specific to the first generation's state of birth and *second* generation's year of birth.[22] In column four we add measures of the state's demographic composition, economic conditions, welfare policies and access to family planning in the year the child was born.[23] This increases the magnitude of the coefficient estimates associated with both *in utero* and later childhood eligibility. The fifth column controls for the second generation child's own *in utero* Medicaid eligibility, which we calculate using a similar simulated instrument strategy as in our main analysis, and the sixth column includes a measure of the mother's cumulative adult Medicaid eligibility through the year of her child's birth.[24] Across these specifications, the point estimates are similar to the estimates in column one. Taken as a whole, these estimates provide strong evidence that

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[21] We note that, unlike the standard error estimates that are associated with the *in utero* coefficients, the inclusion of state trends causes the standard error estimates associated with older age eligibility to become larger, suggesting a loss in identifying variation. We also note that the model with state trends produces a first stage estimate of the correlation between actual and simulated older childhood eligibility that, relative to the model without state trends, is further away from one (1.020 without state trends vs. 0.875 with state trends). In contrast, the first stage estimates for in utero eligibility are very similar across the two models (0.950 without state trends vs. 0.926 with state trends).

[22] The control variables are determined by the mother's state of birth, instead of the child's state of birth, since mobility may be endogenous to Medicaid exposure.

[23] These controls variables are described in detail in the Appendix.

[24] These additional eligibility variables are described in detail in the Appendix.

Medicaid's positive effects persist beyond the first generation. Table 2 repeats our analyses, focusing on Medicaid's impact on the fraction of second generation infants who are at the low end of the birth weight and gestational length distributions. Changes in the prevalence of low birth weight and prematurity are important as they are closely linked to other newborn health measures, and predictive of longer-term cognitive outcomes (Figlio et al. 2014). Moreover, risk factors for these outcomes include maternal health characteristics such as chronic hypertension, pre-pregnancy diabetes, and obesity (Behrman and Butler, 2007)—three conditions that have been shown to improve as a result of positive policy interventions during the *in utero* period (Institute of Medicine, US 2007; Hoynes, Schanzenbach, and Almond 2016; Miller and Wherry 2018).

Again, the pattern of estimates makes a strong case that there are persistent effects of mother's Medicaid exposure on her offspring's risk of being in poor health. We find evidence of a decrease in preterm birth associated with mother's childhood eligibility for Medicaid. These estimates are consistent across all specifications except, similar to the case with our main estimates, the point estimate reverses its sign and is no longer statistically significant with the inclusion of state trends. Otherwise, the estimates indicate that an additional year of childhood eligibility decreases the second generation's incidence of preterm birth by 0.1 percentage points, or about 1% of the sample mean of 0.110. In addition, we find strong evidence of a decrease in very preterm birth associated with mother's *in utero* eligibility. The estimate from the baseline specification indicates that increasing the first generation's *in utero* eligibility from zero to one reduces the second generation's incidence of very preterm birth by 0.3 percentage points, or 43 percent of the sample mean of 0.007. The third and fourth rows of the table also reveal consistent evidence of decreases in the incidence of low birth weight and very low birth weight resulting from mother's *in utero* eligibility, although the estimates are not always statistically significant. The baseline estimate for low birth weight suggests that increasing the first generation's *in utero* eligibility from zero to one reduces the second generation's incidence of low birth weight by 0.5 percentage points, or 7 percent of the mean of 0.071.

In the final panel, we examine whether there are corresponding changes in our measure of birth weight conditional on gestational age. We consistently estimate negative effects on the second generation's likelihood of being small for gestational age, but the estimates never approach statistical significance. We find no evidence of an effect of mother's childhood eligibility at older ages for this outcome.

Appendix Table 5 shows that the pattern and significance of the estimates is consistent across a variety of additional specification checks that are common in the related literature, including different weighting schemes, alternative ways of measuring *in utero* eligibility, limiting the policy variation, including births to older women, and limiting the sample to first births. These specification checks are described in detail in the Appendix. Appendix Table 7 shows estimates from a version of equation (1) that breaks the older childhood Medicaid eligibility measure into three age groups measuring eligibility between ages 1-5, 6-14, and 15-18. Estimates based on this analysis suggest that birth weight and prematurity are most affected by Medicaid exposure during early childhood, but there are no consistent patterns linking age of exposure to the next generation's gestational length outcomes.

Magnitudes

Our analyses indicate that early life Medicaid coverage generates improvements in second generation birth outcomes. Adding estimated effects across the length of childhood suggests that a policy that expanded Medicaid to all children from the time of conception through age 18 would increase average birth weight in the next generation by 76 grams, or a little over 2%. The actual increase in *in utero* eligibility across our cohorts due to the targeted expansions was 6.8 percent, and the average gain in eligibility during later childhood was 2.6 years. The expansions' overall effect on the next generation's average birth weight may, therefore, seem small; but importantly, the increase in average birth weight is consistent with evidence of larger reductions in the incidence of low birth weight and very preterm births—outcomes that are indicative of other, often costly, health outcomes.

As a point of comparison, our estimate of the impact of expanding *in utero* Medicaid eligibility on the next generation's likelihood of being low birth weight is about 40% of Currie and Gruber's estimated effect of these expansions on the first generation's incidence of low birth weight.[25] This is roughly consistent with Currie and Moretti (2007), who find that the probability of being a low birth weight infant is nearly 50 percent higher among children whose mothers were themselves below the low birth weight threshold.[26]

Beyond demonstrating that the effects of early life health environments have spillover effects onto later generations, our findings make clear that previous calculations of Medicaid's return on investment—even those that have taken its long-run benefits into account—are too small. Noting that wide confidence intervals accompany many of our estimates, we can nevertheless give some sense of the potential magnitude of the additional returns (beyond documented benefits

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[25] Currie and Gruber (1996b) estimate a decrease in the incidence of low birth weight of 1.8 percentage points associated with a 100% increase in eligibility under the targeted eligibility changes, compared to our estimate of approximately a 0.7 percentage point decrease for the second generation.

[26] Other studies have estimated smaller intergenerational birth weight correlations (Black, Devereux, and Salvanes 2007; Royer 2009) but importantly, Currie and Moretti find that poverty increases the transmission of low birth weight from mother to child.

to the first generation) through the following exercise: first, we note that Currie and Gruber (1996b) estimate that the targeted expansions increased Medicaid spending per eligible woman by $450 (inflated to 2011 dollars). The estimated additional medical costs associated with a low birth weight birth in the first year of life were approximately $50,000 in 2011 dollars (March of Dimes, 2014). We find that eligibility is associated with a reduction in the probability of a subsequent low birth weight birth of about 0.005, resulting in approximately $250 ($50,000 x 0.005) in savings per woman made eligible. We assume these cost savings accrue equally across all birth years (1994-2015), and we discount these cost savings from each birth year back to the period of the initial Medicaid outlays (specifically, to the year 1981 when we have an estimate for the cost of Medicaid). Using the discount rate recommended by the Department of Commerce for life-cycle studies (3%), this suggests a cost savings of $127 (Lavappa and Kneifel, 2016).[27] Therefore, even when we restrict our benefit calculation to medical costs in the first year of life, the associated savings cover roughly 30% of the cost of the initial investment. Of course, this calculation ignores the additional medical cost savings that result from any health improvements beyond the first year of life that are associated with reductions in low birth weight. It also ignores the improvements in later life earnings (and tax revenues) that accompany higher birth weight (e.g. Black, Devereux, and Salvanes 2007; Bharadwaj, Lundborg, Rooth 2018).

This estimate is based on the return to the *in utero* Medicaid expansions. We note that consistent with prior work on first generation impacts, the second generation effects of *in utero* Medicaid access on birth weight are substantially larger than the effect of an additional year of Medicaid eligibility between the

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[27] Using instead the discount rate recommended by the Office of Management and Budget of 0.5%, the discounted value of the benefits is $223 (US Office of Management & Budget, 2016). Details of these calculations are in the Appendix.

ages of 1 and 18. The estimated costs of *in utero* and later childhood Medicaid are very similar, however,[28] indicating that providing *in utero* Medicaid coverage yields a bigger return on investment.

To get another perspective on magnitudes, we calculate treatment-on-the-treated estimates by dividing the estimates in the first column of Table 1 by estimated take-up rates among the first generation's mothers. Using the Current Population Survey, Currie and Gruber (1996b) estimate that 49 percent of pregnant mothers who gained eligibility through the targeted expansions enrolled in Medicaid.[29] However, Meyer, Mok and Sullivan (2009) document that program participation is underreported in surveys, so Currie and Gruber's estimate is likely an under estimate of *in utero* receipt; Klerman et al. (2009) find that Medicaid receipt in the CPS is under-reported by 30 percent. One way of converting the *in utero* estimates in Table 1 into treatment on treated effects would therefore be to divide by 0.7 (0.49/(1-0.3)). Using administrative data, Brown, Kowalski, and Lurie (2017) estimate a similar take up rate of about 70 percent for the full set of childhood Medicaid expansions.

This take-up rate implies that among the offspring of women whose pregnant mothers had enrolled in Medicaid, average birth weight increased by approximately 43 grams (30/0.7), which is an increase of about 1.3 percent relative to the sample mean. Similarly, the point estimate suggests that the incidence of low birth weight among later offspring fell by about 0.007 (-

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[28] In their study of cohorts born in 1981 to 1984, Brown, Kowalski, and Lurie (2017) estimate that each additional year of childhood Medicaid eligibility (ages 0-18) increased Medicaid spending by $447 in 2011 dollars.

[29] This takeup rate was calculated by dividing the estimate of the change in Medicaid coverage among women of reproductive age associated with the targeted eligibility expansions (5.6 percent increase as reported in Table 5 in Currie and Gruber 1996b) by the authors' estimate of the share of women who were pregnant at some point during the year (11.4 percent found on pg. 1282 in Currie and Gruber 1996b).

0.005/0.7), or 10 percent of the sample mean. Meanwhile, we find that an additional year of maternal coverage between ages 1 and 18 led to an increase in the second generation's average birth weight of approximately 4 grams (3/0.7). The point estimate for preterm birth suggests a decrease in its incidence of 0.1 percentage points (-0.001/.7) resulting from an additional year of maternal childhood coverage, or a decrease of 1.3 percent of the sample mean.

Few studies have employed natural experiment designs to investigate multi-generational effects of early life environments, and among the handful that do exist, differences in research settings, time frame, and outcomes examined make comparisons difficult. The closest study to ours is Almond and Chay (2006), who compare infant health outcomes among the offspring of black and white women who were born between the early and late 1960s, when the rapid adoption of Great Society programs (particularly Title VI of the Civil Rights Act) lead to dramatic improvements in black infants' health conditions. The treated generation's access to better quality care reduced the black-white gap in very low birth weight incidence among the second generation by 30%.[30]

We can also consider our estimates in light of two studies that have examined the effects of negative *in utero* health shocks on later generations' birth outcomes. Comparing the offspring of cohorts conceived before, during, and after the 1959-1961 Chinese famine, Almond et. al. (2010) find that first generation fetal exposure to malnutrition increases the incidence of low birth weight among the second generation by 8%. Using a similar approach, Painter et. al. (2008) find

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[30] Almond and Chay argue that access and quality of health care remained roughly constant for white infants during the 1960s, and that the black-white gap closed because of changes in health care available to blacks. Across the cohorts we study, blacks' and whites' *in utero* Medicaid eligibility increased by a very similar amount (about 7 percentage points), and the estimated second generation effects of first generation *in utero* Medicaid eligibility are not statistically different across racial groups. Therefore, while the targeted expansions improved second generation health for all groups, there is little evidence that they reduced racial disparities.

no evidence of birth weight differences between the children of cohorts who were exposed to the 1944-1945 Dutch famine *in utero*, versus those who were not exposed, but they do find that that the offspring of those who were exposed were almost twice as likely to experience poor health in later life.

Taken together, the weight of the evidence is that early childhood environments generate substantive spillover effects onto later generations. While differences in research settings, time frame, and outcomes examined make it difficult to make exact comparisons, our estimates are consistent with meaningful, persistent benefits of health interventions suggested by the most related research.

Mechanisms

What are the mechanisms by which prenatal or childhood Medicaid access leads to improved health in the second generation? One possible channel is through changes in fertility.[31] The same (or related) biological processes that generated later life improvements in the first generation's health may have also affected the first generation's fecundity. Another channel is through the *composition* of women giving birth. For example, if children are a normal good then increases in the first generation's earnings might also lead to increases in the desired number of children. On the other hand, improvements in the first generation's economic opportunities may lead to delays in childbearing, some evidence of which was presented in Brown, Kowalski, and Lurie (2017).

We investigate these different possibilities in Tables 3 and 4. We estimate regressions similar to equation (1) replacing the dependent variable with measures of total fertility and maternal characteristics (age, educational attainment, marital

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[31] It is important to note that we are not able to measure total fertility, because we only observe women between the ages of 15 and 28 in our main sample.

status and race).[32] Across specifications, the results suggest that *in utero* and childhood Medicaid access had no effect on the overall number of births, the probability of delaying first birth, or the average number of births per mother (see Table 3).

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[32] Three of the outcomes analyzed in this section (mother's educational attainment, prenatal care utilization, and race) were affected by the introduction of the 2003 revision of the U.S. Standard Certificate of Live Birth, which replaced the 1989 revision that was in use during the remainder of the period covered by our analyses. State adoption of the revision is staggered over the period. By January 2011, 36 states and the District of Columbia had implemented the revised birth certificate. These states represent 83 percent of births to U.S. residents (Center for Disease Control and Prevention, 2011). Starting in 2011, the CDC no longer made available certain data items from the unrevised birth certificate, including maternal education and prenatal care utilization. As a result, information on these variables is incomplete, and only available for states that had fully implemented the 2003 revision. Fourteen states in 2011, 12 states in 2012, 9 states in 2013, 3 states in 2014, and 2 states in 2015 have incomplete information for these data fields. Our main analyses use data from all states and all years, but as a sensitivity check, we also run the main analyses excluding births to mothers who were themselves born in any of the states with incomplete data (Appendix Table 8). This does not meaningfully change our main results. In addition, even when the data fields are available, these two measures are not considered comparable before and after the 2003 revision. Prior to the revision, mother's education was classified into years of education: no formal education, 1-8 years of elementary school, 1-4 years of high school, 1-4 years of college, and 5 or more years of college. The 2003 revision classified mother's education into the following categories: 8th grade or less; 9th through 12th grade with no diploma; high school graduate or GED completed; some college credit, but not a degree; associate degree; Bachelor's degree; Master's degree; and, doctorate or professional degree. In our analyses, we code high school or less as having at least 4 years of high school under the 1989 revision, and being a high school graduate or having a GED completed under the 2003 revision. In addition, changes occurred in information collected on mother's race with the 2003 revision including more detailed race categories. Also, beginning in 2003, states had the option of allowing the report of multiple race categories. These multiple race combinations are bridged to a single race category for comparability to other reporting areas and years. We address the incomparability of these outcomes after the birth certificate revision by including in regressions for which maternal education, race, or prenatal care utilization are dependent variables or used to define subgroups, a measure of the fraction of birth records in that cell (mother's birth year x mother's state of birth) with a revised birth certificate. Also, to be sure that the timing of state implementation of the 2003 revision is not correlated with a cohort's exposure to Medicaid, we run our model with the share of revised birth certificate records for each mother's birth year and birth state cell on the left hand side. We find no evidence of a correlation (see Appendix Table 8). Finally, for regressions with maternal race as a dependent variable or for which race was used to define subgroups, we also include a control for the fraction of birth records in each cell that allowed for the reporting of multiple race categories.

Although we do not find changes in overall fertility, we do find some evidence that expanding the first generation's *in utero* and childhood Medicaid eligibility changed the characteristics of the mothers giving birth (reported in Table 4). In the baseline specification, we find that the *in utero* expansions led to a marginally significant decrease in the fraction of second generation births for which first generation mothers were high school graduates, and an increase in the fraction of births to white mothers. We also find that the expansions at older ages led to a slight increase in the fraction of births to white mothers and a decrease in the fraction of births to black mothers. While these estimates are only marginally significant ($p<.10$), we find additional evidence of compositional changes under the five remaining specifications. We explore the extent to which these changes in mothers' observable characteristics explain changes in the second generation's health by re-running the analyses presented in Tables 1 and 2 while adding controls for these maternal characteristics. The results are reported in Appendix Table 10, however, it is important to note that some of these characteristics may be endogenous to early life Medicaid. The estimates are similar to those presented in the main tables. The estimates for the effects of *in utero* eligibility on average birth weight and very preterm birth are particularly robust to this exercise, representing between 56 and 100 percent of the magnitude of the initial estimates. Moreover, since we expect the change in racial composition rather than the change in educational attainment to have a positive impact on infant health, we examine to what extent this specific change might explain our estimates. Back of the envelope calculations indicate that the change in the racial composition of births can explain no more than 37% of the overall estimated effect on average birth weight.[33] This suggests that, while we observe some change in the composition of women giving birth in response to the *in utero* expansions,

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[33] See the Appendix for details on this calculation.

selection on these maternal characteristics does not appear to be driving our findings.

To further explore the relationship between the expansions and maternal characteristics, we also examine how the expansions changed birth outcomes *within* groups (Table 5). Although many estimates are not statistically significant when we reduce our sample sizes, we continue to find patterns indicating that *in utero* and childhood Medicaid eligibility increase the next generation's birth weight for all subgroups. This lends further credence to the hypothesis that effects on the second generation's health are mostly due to changes in mothers' health, behaviors, or economic status, rather than selection into fertility. The estimates for the second generation's gestational length and birth weight are particularly large and statistically significant among high school dropouts – a group that we might expect to be particularly sensitive to Medicaid availability.

Next, we use information recorded on the birth certificate to examine how early life Medicaid affects maternal health and health behaviors in later life,[34] noting that health conditions reported on birth certificates are relatively limited, and are known to be underreported (Lain et al., 2012). As shown in Table 6, we examine prenatal care utilization, the presence of chronic health conditions, and reported use of alcohol or tobacco during pregnancy. We find no statistically significant effects of *in utero* Medicaid eligibility on these maternal risk factors or behaviors, although analyses of other health data have found that early life

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[34] Information on alcohol and tobacco use is not available on the birth certificate for all states and years. Alcohol use is only available through 2006. Therefore, we examine reported alcohol use only for cohorts between the ages of 15-19. Information on tobacco use is available through 2008. For this outcome, we examine reported use at ages 15-21. Finally, to confirm that the availability of any of these outcomes is not correlated with state Medicaid policy, we have also run our main regression model with the share of birth records that have alcohol or tobacco use information as the dependent variable. We find no evidence of a relationship with Medicaid eligibility (see Appendix Table 9).

Medicaid eligibility is associated with better health outcomes in adulthood (e.g., Thompson, 2017, Miller and Wherry, 2018). Surprisingly, we do see some evidence that childhood Medicaid eligibility is associated with reductions in the use of prenatal care during pregnancy. If anything, this would lead us to expect worse infant health outcomes, and suggests that the childhood effects would be larger in the absence of this association.

Finally, we investigate the extent to which the estimates may be attributed to Medicaid's effect on the first generation's adult income. To do this, we rely on point estimates from multiple studies, acknowledging that estimates in the related literature are often quite large and accompanied by large confidence intervals. We begin with Miller and Wherry's (2018) finding that *in utero* Medicaid eligibility under the targeted expansions generated an increase in annual personal income of 20 percent between ages 23 and 36, or approximately $5,974 (2009$s). Using our estimated take-up rate of 0.70, this translates into a TOT estimate of $8,534. Putting this together with Hoynes, Miller, and Simon's (2015) estimate that a $1000 increase in EITC income (2009$s) increases average birth weight by around 6.4 grams, suggests that Medicaid induced improvements in parental income should increase average birth weight by 55 grams. This estimate is close to our TOT estimate of 43 grams, and suggests that Medicaid's long run effect on the first generation's income may be an important mediator.

VI. Discussion

This paper advances the "early origins" literature by investigating multi-generational effects of early life health environments. We present new evidence that expanding health related services during childhood has persistent impacts on later generations' health. Specifically, we use variation induced by the 1980s targeted Medicaid expansions and find that greater *in utero* eligibility leads to significant increases in average birth weight among later offspring, with smaller but statistically significant increases in birth weight associated with Medicaid eligibility at later ages. These effects appear to be concentrated at lower points in the distribution as we find suggestive evidence that *in utero* Medicaid eligibility reduces the incidence of very premature births, low birthweight or very low birthweight in the second generation. We also find suggestive evidence that Medicaid eligibility between ages 1 to 18 increases gestation length and reduces the incidence of prematurity. While there is some evidence that expanded Medicaid eligibility altered the composition of women who were giving birth, changes in maternal characteristics do not appear to explain our findings. These results are robust to a number of specification tests, including controlling for second generation environmental conditions, alternative definitions of eligibility, and using different samples of mothers.

The exact mechanisms that lead to multi-generational linkages are not clear. Animal experiments provide biological evidence that the importance of early life health environments extends beyond treated generations, and there is growing evidence from these experiments that in at least some settings, epigenetics play a role. Such processes are obviously harder to document in humans, where corresponding experiments are nearly impossible to invoke, but some of our calculations suggest that Medicaid induced improvements in the first generation's economic outcomes may also be an important mechanism driving later generations' health gains.

Our analyses offer a new perspective on health inequalities and the potential role for government intervention. Generational persistence in the impacts of early life environments suggest that historical differences in fetal health conditions between advantaged and disadvantaged groups may undermine contemporaneous efforts to close health and economic gaps. At the same time, our results indicate that early life health investments have payoffs that extend well beyond those that social policymakers usually consider. It is notable that Medicaid's second generation effects are observed among cohorts who were born during roughly the same time frame for which recent studies by Aizer and Currie (2014) and Currie and Schwandt (2016a, 2016b) document large improvements and declining health inequality among children. Investigating a more complete range of program benefits to later generations is an important goal of future work, and is critical in light of increasing debates about the efficacy of the U.S. safety net.

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Table 1

Effects of Parental Medicaid Access on Second Generation Birth Outcomes

	First generation controls			Second generation state-year controls		
	Baseline Specification	Region *year fixed effects	State trends	State-child year of birth characteristics	Chlid's own in utero eligibility	Mother's cumulative adult eligibility
<u>Outcome: Length of gestation</u>						
In-utero eligibility	0.082	0.100	0.065	0.142*	0.088	0.086
	(0.089)	(0.092)	(0.071)	(0.077)	(0.092)	(0.089)
Eligibility at ages 1-18	0.005	0.005	-0.009	0.010***	0.007	0.005
	(0.005)	(0.005)	(0.010)	(0.004)	(0.006)	(0.005)
<u>Outcome: Average birth weight</u>						
In-utero eligibility	30.498*	36.961**	36.003**	44.514**	34.839*	30.851*
	(17.925)	(17.747)	(17.626)	(20.744)	(19.223)	(17.865)
Eligibility at ages 1-18	2.554**	2.184*	-0.722	3.568***	3.568***	2.512**
	(1.245)	(1.197)	(2.679)	(1.187)	(1.379)	(1.257)

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. Births to women born in Arizona are excluded from the sample. Coefficients are from IV regressions weighted by mother's birth cohort size that include mother's state of birth and mother's year of birth fixed effects, and additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). Second generation state-year controls are also included when indicated and are described in more detail in the text and appendix. Regressions also include mother's region of birth by mother's birth year fixed effects, or mother's state of birth linear trends in mother's birth year when indicated. Robust standard errors are clustered by mother's state of birth.

Table 2

Effects of Parental Medicaid Access on Second Generation Birth Outcomes

	First generation controls			Second generation state-year controls		
	Baseline Specification	Region *year fixed effects	State trends	State-child year of birth characteristics	Chlid's own in utero eligibility	Mother's cumulative adult eligibility
Outcome: Preterm birth						
In-utero eligibility	-0.000	0.000	-0.009	-0.002	-0.002	-0.000
	(0.006)	(0.006)	(0.010)	(0.007)	(0.006)	(0.006)
Eligibility at ages 1-18	-0.001**	-0.001**	0.002	-0.001	-0.001***	-0.001**
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Outcome: Very preterm birth						
In-utero eligibility	-0.003***	-0.004***	-0.007***	-0.005***	-0.004***	-0.004***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Eligibility at ages 1-18	0.000	-0.000**	0.000	-0.000**	-0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Outcome: Low birth weight						
In-utero eligibility	-0.005	-0.005	-0.007	-0.013**	-0.006	-0.004
	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
Eligibility at ages 1-18	-0.000	0.000	0.001	-0.001**	-0.001**	-0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Outcome: Very low birth weight						
In-utero eligibility	-0.002	-0.003	-0.007***	-0.005**	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Eligibility at ages 1-18	-0.000	0.000	-0.000	-0.000**	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Outcome: Small for gestation age						
In-utero eligibility	-0.008	-0.007	-0.003	-0.010	-0.008	-0.008
	(0.007)	(0.006)	(0.006)	(0.008)	(0.006)	(0.007)
Eligibility at ages 1-18	0.000	0.000	0.000	-0.001	0.000	0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. Births to women born in Arizona are excluded from the sample. Coefficients are from IV regressions weighted by mother's birth cohort size that include mother's state of birth and mother's year of birth fixed effects, and additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). Second generation state-year controls are also included when indicated and are described in more detail in the text and appendix. Regressions also include mother's region of birth by mother's birth year fixed effects, or mother's state of birth linear trends in mother's birth year when indicated. Robust standard errors are clustered by mother's state of birth.

Table 3

Effects of Parental Medicaid Access on Fertility Outcomes

	First generation controls			Second generation state-year controls		
	Baseline Specification	Region *year fixed effects	State trends	State-child year of birth characteristics	Chlid's own in utero eligibility	Mother's cumulative adult eligibility
Outcome: Total Birth Rate						
In-utero eligibility	-0.180	-0.151	0.066	-0.016	-0.157	-0.162
	(0.219)	(0.179)	(0.053)	(0.107)	(0.224)	(0.213)
Eligibility at ages 1-18	-0.005	-0.011	-0.008	0.003	0.000	-0.007
	(0.009)	(0.010)	(0.008)	(0.008)	(0.010)	(0.009)
Outcome: First Birth Rate						
In-utero eligibility	-0.093	-0.072	0.030	-0.022	-0.087	-0.086
	(0.111)	(0.094)	(0.023)	(0.057)	(0.116)	(0.108)
Eligibility at ages 1-18	-0.002	-0.004	-0.005	0.003	0.000	-0.003
	(0.004)	(0.005)	(0.003)	(0.004)	(0.005)	(0.004)
Outcome: Age at First Birth						
In-utero eligibility	-0.507	-0.382	-0.451	-0.250	-0.399	-0.508
	(0.770)	(0.683)	(0.299)	(0.424)	(0.811)	(0.752)
Eligibility at ages 1-18	-0.009	0.000	-0.033	-0.036	0.017	-0.009
	(0.031)	(0.036)	(0.047)	(0.030)	(0.031)	(0.033)
Outcome: Average Number of Births						
In-utero eligibility	0.005	0.021	-0.047	-0.007	0.024	0.011
	(0.059)	(0.059)	(0.038)	(0.045)	(0.051)	(0.059)
Eligibility at ages 1-18	-0.002	-0.007	0.004	-0.007**	0.002	-0.003
	(0.004)	(0.004)	(0.005)	(0.003)	(0.004)	(0.004)

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. Births to women born in Arizona are excluded from the sample. Coefficients are from IV regressions weighted by mother's birth cohort size that include mother's state of birth and mother's year of birth fixed effects, and additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). Second generation state-year controls are also included when indicated and are described in more detail in the text and appendix. Regressions also include mother's region of birth by mother's birth year fixed effects, or mother's state of birth linear trends in mother's birth year when indicated. Robust standard errors are clustered by mother's state of birth.

Table 4

Effects of Parental Medicaid Access: Selection

	First generation controls			Second generation state-year controls		
	Baseline Specification	Region *year fixed effects	State trends	State-child year of birth characteristics	Chlid's own in utero eligibility	Mother's cumulative adult eligibility
<u>Outcome: High School Graduate</u>						
In-utero eligibility	-0.060*	-0.02	-0.05	-0.058**	-0.060*	-0.054
	(0.034)	(0.034)	(0.033)	(0.026)	(0.034)	(0.034)
Eligibility at ages 1-18	0.000	-0.001	-0.007*	-0.005***	-0.001	-0.001
	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)
<u>Outcome: Married</u>						
In-utero eligibility	0.048	0.045	-0.008	0.085***	0.055	0.047
	(0.041)	(0.030)	(0.015)	(0.026)	(0.046)	(0.041)
Eligibility at ages 1-18	0.003	0.004*	-0.003	0.005***	0.004**	0.003
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
<u>Outcome: White</u>						
In-utero eligibility	0.051*	0.077***	0.057***	0.108***	0.045	0.053**
	(0.030)	(0.025)	(0.018)	(0.030)	(0.029)	(0.027)
Eligibility at ages 1-18	0.004*	0.004**	-0.001	0.006***	0.003	0.004*
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
<u>Outcome: Black</u>						
In-utero eligibility	-0.031	-0.057**	-0.031**	-0.075***	-0.025	-0.033
	(0.026)	(0.023)	(0.015)	(0.024)	(0.024)	(0.025)
Eligibility at ages 1-18	-0.003*	-0.003**	-0.001	-0.004***	-0.001	-0.003
	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)
<u>Outcome: Other</u>						
In-utero eligibility	-0.019	-0.021	-0.026**	-0.032*	-0.021	-0.02
	(0.019)	(0.015)	(0.012)	(0.018)	(0.019)	(0.018)
Eligibility at ages 1-18	-0.001	-0.001	0.002*	-0.002**	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. Births to women born in Arizona are excluded from the sample. Coefficients are from IV regressions weighted by mother's birth cohort size that include mother's state of birth and mother's year of birth fixed effects, and additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). An additional control for the share of births with revised birth certificate records is included for the outcomes related to high school graduation and mother's race. For regressions examining mother's race, we also include a control for the share of births with birth certificate records allowing for the report of multiple race categories. Second generation state-year controls are also included when indicated and are described in more detail in the text and appendix. Regressions also include mother's region of birth by mother's birth year fixed effects, or mother's state of birth linear trends in mother's birth year when indicated. Robust standard errors are clustered by mother's state of birth.

Table 5

Effects of Parental Medicaid Access on Second Generation Birth Outcomes by Subgroup

Baseline Specification

	Black	White	Other	Teen	Adults	High School Dropout	High School Graduate	Married	Unmarried
<u>Outcome: Length of gestation</u>									
In-utero eligibility	-0.02	0.061	-0.307	0.027	0.085	0.240**	0.030	0.011	0.024
	(0.153)	(0.090)	(0.288)	(0.155)	(0.080)	(0.112)	(0.078)	(0.084)	(0.084)
Eligibility at ages 1-18	0.009	0.001	0.007	0.001	0.008	0.008	0.000	0.008	0.007
	(0.012)	(0.005)	(0.017)	(0.010)	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)
<u>Outcome: Average birth weight</u>									
In-utero eligibility	8.659	31.272*	206.402***	58.523*	29.652	90.849***	13.752	13.592	17.436
	(28.084)	(18.508)	(69.339)	(32.724)	(18.542)	(31.825)	(17.479)	(18.975)	(18.446)
Eligibility at ages 1-18	1.510	1.463*	14.775***	0.793	2.989**	2.926	0.283	2.282*	2.908**
	(2.857)	(0.838)	(4.890)	(1.993)	(1.229)	(1.877)	(1.621)	(1.177)	(1.472)

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. Births to women born in Arizona are excluded from the sample. Coefficients are from IV regressions weighted by mother's birth cohort size that include mother's state of birth and mother's year of birth fixed effects, and additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). An additional control for the share of births with revised birth certificate records is included for the models for subgroups defined by high school graduation or mother's race. For subgroups defined by mother's race, we also include a control for the share of births with birth certificate records allowing for the report of multiple race categories. Robust standard errors are clustered by mother's state of birth.

Table 6
Effects of Parental Medicaid Access on Maternal Health and Behaviors

Baseline Specification

	Any Prenatal Care	Number of Prenatal Visits	Prenatal Care in First Trimester	Diabetes	Chronic Hypertension	Pregnancy-related Hypertension	Eclampsia	Alcohol use During Pregnancy	Tobacco use During Pregnancy
In-utero eligibility	0.002	0.641	-0.005	0.001	-0.003	-0.011	0.002	-0.012	-0.020
	(0.008)	(0.536)	(0.048)	(0.007)	(0.004)	(0.008)	(0.002)	(0.008)	(0.035)
Eligibility at ages 1-18	-0.001	-0.115***	-0.010**	-0.001	-0.000	0.001	0.000**	0.000	0.006
	(0.000)	(0.034)	(0.004)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.004)

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. The analysis for alcohol use is restricted to ages 15-19 and the analysis for tobacco use to ages 15-21. Births to women born in Arizona are excluded from the sample. Coefficients are from IV regressions weighted by mother's birth cohort size that include mother's state of birth and mother's year of birth fixed effects, and additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). An additional control for the share of births with revised birth certificate records is included for the outcomes related to prenatal care utilization. Robust standard errors are clustered by mother's state of birth.

Appendix Table 1

Federal Legislation Expanding Public Health Insurance Eligibility for Pregnant Women, Infants and Children

Year	**Legislation**	**Date Effective**	**Mandatory Expansion**	**State Option**
1984	Deficit Reduction Act, 1984 (DEFRA)	1-Oct-84	First-time pregnant women and those in two-parent families whose principal earner was unemployed, as well as children under age 5 born after September 30, 1983 whose families are income and resource eligible for AFDC	
1985	Consolidated Omnibus Budget Reconciliation Act, 1985 (COBRA)	1-Jul-86	Pregnant women whose families are income and resource eligible for AFDC	
1986	Omnibus Budget Reconciliation Act, 1986 (OBRA86)	1-Apr-87		Pregnant women and infants in families with incomes below 100% FPL
		1-Oct-87		Increase age level by 1 year each FY for all children under age 5 with incomes below 100% FPL
1987	Omnibus Budget Reconciliation Act, 1987 (OBRA87)	1-Jul-88		Pregnant women and infants in families with incomes below 185% FPL Children under age 2, 3, 4, or 5 and born after September 30, 1983 in families with incomes below 100% FPL
		1-Oct-88	Children under age 7 born after September 30, 1983 whose families are income and resource eligible for AFDC	Children under age 8 born after September 30, 1983 whose families are income and resource eligible for AFDC Children under age 8 born after September 30, 1983 with incomes below 100% FPL
1988	Medicare Catastrophic Coverage Act, 1988 (MCCA)	1-Jul-89	Pregnant women and infants in families with incomes below 75% FPL	
		1-Jul-90	Pregnant women and infants in families with incomes below 100% FPL	
1989	Omnibus Budget Reconciliation Act, 1989 (OBRA89)	1-Apr-90	Pregnant women and children under age 6 with family incomes below 133% FPL	
1990	Omnibus Budget Reconciliation Act, 1990 (OBRA90)	1-Jul-91	Children under age 19 born after September 30, 1983 with incomes below 100% FPL	
1996	Personal Responsibility and Work Opportunity Act of 1996 (PRWORA)	1-Jul-97	Established "Section 1931" family coverage category with minimum eligibility critera based on 1996 AFDC eligibility standards	Families with children at higher income levels
1997	Balanced Budget Act (BBA)	5-Aug-97		Children under age 19 in families with incomes below 200% FPL or higher

Notes: Reproduced from Miller and Wherry (2017). Legislative history is compiled from Congressional Research Service (1988, 1993), Kaiser Family Foundation (2002), Currie and Gruber (1994), Gruber (2003), and Broaddus et al. (2001).

Appendix Table 2
Descriptive Statistics

Variable	Mean
Infant health	
Gestation length	38.779
Preterm birth	0.110
Very preterm birth	0.007
Birth weight	3270.524
Low birth weight	0.071
Very low birth weight	0.012
Small for gestational age	0.093
Fertility	
Birth rate	1.010
First birth rate	0.511
Number of births	1.766
Age at first birth	21.875
Mother's characteristics	
High school graduate	0.759
Married	0.453
White	0.762
Black	0.210
Other race	0.028
Mother's health and behaviors	
Diabetes	0.028
Chronic hypertension	0.008
Pregnancy-related hypertension	0.044
Eclampsia	0.003
Alcohol use during pregnancy	0.008
Tobacco use during pregnancy	0.197
Medicaid eligibility	
In utero eligibility	0.156
Simulated in utero eligibility	0.158
Eligibility at ages 1-18	4.071
Simulated eligibility at ages 1-18	4.147
State-year controls	
Age 0-4	0.235
Age 5-17	0.197
Age 18-24	0.123
Age 25-44	0.3
Age 45-64	0.191
Married	0.44
Black	0.12
Other race	0.029
High school dropout	0.267
High school degree	0.39
Some college	0.343
Unemployment rate	7.732
Personal income per capita	19.635
Maximum AFDC benefit for family of 4	586.642
Medicaid funding restriction for abortion	0.024
Parental consent and notification law for abortion	0.443

Appendix Table 3

Effects of Parental Medicaid Access on Second Generation Birth Outcomes

Baseline Specification

	OLS	Reduced Form
<u>Outcome: Length of gestation</u>		
In-utero eligibility	0.048	0.075
	(0.072)	(0.095)
Eligibility at ages 1-18	0	0.005
	(0.004)	(0.006)
<u>Outcome: Average birth weight</u>		
In-utero eligibility	14.946	27.559
	(15.399)	(17.548)
Eligibility at ages 1-18	0.51	2.567**
	(0.637)	(1.275)
<u>Outcome: Preterm birth</u>		
In-utero eligibility	0.002	0.000
	(0.005)	(0.006)
Eligibility at ages 1-18	-0.000	-0.001**
	(0.000)	(0.000)
<u>Outcome: Very preterm birth</u>		
In-utero eligibility	-0.001	-0.003**
	(0.001)	(0.001)
Eligibility at ages 1-18	0.000	0.000
	(0.000)	(0.000)
<u>Outcome: Low birth weight</u>		
In-utero eligibility	-0.003	-0.004
	(0.004)	(0.006)
Eligibility at ages 1-18	-0.000	-0.000
	(0.000)	(0.000)
<u>Outcome: Very low birth weight</u>		
In-utero eligibility	-0.000	-0.002
	(0.002)	(0.002)
Eligibility at ages 1-18	-0.000	-0.000
	(0.000)	(0.000)
<u>Outcome: Small for gestation age</u>		
In-utero eligibility	-0.003	-0.008
	(0.005)	(0.007)
Eligibility at ages 1-18	0.000	0.000
	(0.000)	(0.001)

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. Births to women born in Arizona are excluded from the sample. Regressions are weighted by mother's birth cohort size and include mother's state of birth and mother's year of birth fixed effects, and additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). Robust standard errors are clustered by mother's state of birth.

Appendix Table 4

Effects of Parental Medicaid Access on Fertility, Human Capital, and Marital Outcomes by Race

	White		Black		Other	
	Baseline Specification	State trends	Baseline Specification	State Trends	Baseline Specification	State Trends
Outcome: Total Birth Rate						
In-utero eligibility	-0.313	-0.054	-0.193	0.009	0.063	1.011***
	(0.232)	(0.068)	(0.251)	-0.135	(0.393)	(0.319)
Eligibility at ages 1-18	-0.004	-0.001	-0.028**	-0.024	0.006	-0.013
	(0.011)	(0.009)	(0.012)	-0.015	(0.038)	(0.048)
Outcome: Age at First Birth						
In-utero eligibility	-0.436	-0.323	-0.425	0.183	-1.729***	-1.241
	(0.876)	(0.310)	(0.754)	(0.455)	(0.481)	(0.781)
Eligibility at ages 1-18	0.01	-0.061	-0.023	-0.027	-0.071	-0.126
	(0.035)	(0.045)	(0.038)	(0.062)	(0.051)	(0.158)
Outcome: High School Graduate						
In-utero eligibility	-0.027	-0.052*	0.076**	0.028	-0.148***	-0.065
	(0.037)	(0.030)	(0.038)	(0.033)	(0.054)	(0.073)
Eligibility at ages 1-18	-0.002*	-0.008*	0.004	-0.010*	-0.008*	0.013
	(0.001)	(0.004)	(0.003)	(0.005)	(0.005)	(0.011)
Outcome: Married						
In-utero eligibility	0.049	-0.018	0.004	-0.013	0.067	0.029
	(0.047)	(0.016)	(0.028)	(0.021)	(0.071)	(0.112)
Eligibility at ages 1-18	0.003	-0.003	0.003**	-0.006	0	-0.006
	(0.002)	(0.002)	(0.002)	(0.004)	(0.007)	(0.017)

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. Births to women born in Arizona are excluded from the sample. Coefficients are from IV regressions weighted by mother's birth cohort size that include mother's state of birth and mother's year of birth fixed effects, and additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). Additional controls for the share of births with revised birth certificate records and the share of births with birth certificate records allowing for the report of multiple race categories are included. Regressions also include mother's state of birth linear trends in mother's birth year when indicated. Robust standard errors are clustered by mother's state of birth.

Appendix Table 5

Effects of Parental Medicaid Access, Alternative Specifications

	Baseline Model						
	Baseline Specification	Unweighted	Weighted by number of births	Alternative prenatal measure	Medicaid policy-only variation	Ages 15-36	First Births
Outcome: Length of gestation							
In-utero eligibility	0.082	0.102	0.096	0.092	0.115	0.105	0.081
	(0.089)	(0.085)	(0.095)	(0.094)	(0.108)	(0.064)	(0.105)
Eligibility at ages 1-18	0.005	0.001	0.004	0.005	0.005	0.000	0.001
	(0.005)	(0.007)	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)
Outcome: Average birth weight							
In-utero eligibility	30.498*	41.014**	30.188*	51.007*	32.266	31.793*	18.504
	(17.925)	(20.717)	(17.123)	(22.261)	(22.185)	(18.153)	(20.510)
Eligibility at ages 1-18	2.554**	1.425*	2.359*	2.191	2.218*	1.34	0.597
	(1.245)	(1.084)	(1.219)	(1.454)	(1.178)	(1.285)	(0.997)
Outcome: Preterm birth							
In-utero eligibility	-0.000	-0.008	0.001	-0.001	0.000	-0.004	0.007
	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)
Eligibility at ages 1-18	-0.001**	-0.001**	-0.001**	-0.001**	-0.001**	-0.000	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Outcome: Very preterm birth							
In-utero eligibility	-0.003***	-0.002	-0.003***	-0.004**	-0.002	-0.003***	-0.005**
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Eligibility at ages 1-18	0.000	-0.000*	-0.000*	0.000	-0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Outcome: Low birth weight							
In-utero eligibility	-0.005	-0.012	-0.004	-0.008	-0.002	-0.006	-0.004
	(0.006)	(0.009)	(0.005)	(0.006)	(0.008)	(0.005)	(0.007)
Eligibility at ages 1-18	-0.000	-0.001	-0.000	0.000	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Outcome: Very low birth weight							
In-utero eligibility	-0.002	-0.001	-0.002	-0.004*	-0.002	-0.003	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Eligibility at ages 1-18	-0.000	-0.000*	-0.000	0.000	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Outcome: Small for gestation age							
In-utero eligibility	-0.008	-0.011	-0.007	-0.010	-0.004	-0.011*	-0.007
	(0.007)	(0.012)	(0.006)	(0.008)	(0.008)	(0.006)	(0.009)
Eligibility at ages 1-18	0.000	0.001	0.000	0.000	0.000	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. Births to women born in Arizona are excluded from the sample. Coefficients are from IV regressions weighted by mother's birth cohort size that include mother's state of birth and mother's year of birth fixed effects, and additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). Robust standard errors are clustered by mother's state of birth.

Appendix Table 6

Effects of Parental Medicaid Access on Maternal Health and Behaviors

	First generation controls			Second generation state-year controls		
	Baseline Specification	Region *year fixed effects	State trends	State-child year of birth characteristics	Chlid's own in utero eligibility	Mother's cumulative adult eligibility
Outcome: Any Prenatal Care						
In-utero eligibility	0.002	0.000	0.003	0.007	0.002	0.002
	(0.008)	(0.008)	(0.003)	(0.005)	(0.008)	(0.008)
Eligibility at ages 1-18	-0.001	-0.001**	-0.001	0.000	-0.001	-0.001*
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Outcome: Number of Prenatal Visits						
In-utero eligibility	0.641	0.610	0.058	0.257	0.696	0.632
	(0.536)	(0.519)	(0.161)	(0.294)	(0.551)	(0.532)
Eligibility at ages 1-18	-0.115***	-0.112***	-0.004	-0.041*	-0.105***	-0.114***
	(0.034)	(0.033)	(0.026)	(0.023)	(0.033)	(0.034)
Outcome: Prenatal Care in First Trimester						
In-utero eligibility	-0.005	-0.016	0.002	-0.004	-0.005	-0.006
	(0.048)	(0.052)	(0.019)	(0.034)	(0.047)	(0.049)
Eligibility at ages 1-18	-0.010**	-0.008**	-0.006*	-0.003	-0.009**	-0.009**
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
Outcome: Diabetes						
In-utero eligibility	0.001	-0.003	0.003	0.005	0.001	0.000
	(0.007)	(0.006)	(0.005)	(0.006)	(0.007)	(0.007)
Eligibility at ages 1-18	-0.001	-0.001*	-0.001	0.000	-0.001	0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Outcome: Chronic Hypertension						
In-utero eligibility	-0.003	-0.001	0.003	0.006**	-0.002	-0.003
	(0.004)	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)
Eligibility at ages 1-18	-0.000	0.000	-0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Outcome: Pregnancy-related Hypertension						
In-utero eligibility	-0.011	-0.013*	0.009*	0.009	-0.011	-0.011
	(0.008)	(0.007)	(0.005)	(0.007)	(0.008)	(0.008)
Eligibility at ages 1-18	0.001	0.001	-0.000	0.001*	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Outcome: Eclampsia						
In-utero eligibility	0.002	0.002	0.001	0.003	0.002	0.002
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Eligibility at ages 1-18	0.000**	0.001***	0.000	0.000***	0.000**	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Outcome: Alcohol Use During Pregnancy						
In-utero eligibility	-0.012	-0.015*	-0.010	-0.005	-0.011	-0.012
	(0.008)	(0.009)	(0.011)	(0.010)	(0.008)	(0.008)
Eligibility at ages 1-18	0.000	0.001	0.002*	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Outcome: Tobacco Use During Pregnancy						
In-utero eligibility	-0.020	0.032	-0.012	-0.004	-0.022	-0.015
	(0.035)	(0.027)	(0.045)	(0.042)	(0.035)	(0.034)
Eligibility at ages 1-18	0.006	0.005**	0.022	0.007**	0.005	0.005
	(0.004)	(0.002)	(0.014)	(0.003)	(0.004)	(0.004)

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. The analysis for alcohol use is restricted to ages 15-19 and the analysis for tobacco use to ages 15-21. Births to women born in Arizona are excluded from the sample. Coefficients are from IV regressions weighted by mother's birth cohort size that include mother's state of birth and mother's year of birth fixed effects, and additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). An additional control for the share of births with revised birth certificate records is included for prenatal care utilization. Second generation state-year controls are also included when indicated and are described in more detail in the text and appendix. Regressions also include mother's region of birth by mother's birth year fixed effects, or mother's state of birth linear trends in mother's birth year when indicated. Robust standard errors are clustered by mother's state of birth.

Appendix Table 7

Effects of Mother's Childhood Medicaid Exposure on Infant Health

	Length of gestation	Preterm birth	Very preterm birth	Average birth weight	Low birth weight	Very low birth weight	Small for gestational age
Baseline model:							
In utero eligibility	0.093	0.002	-0.004***	31.404*	-0.004	-0.002	-0.009
	(0.085)	(0.006)	(0.001)	(18.854)	(0.006)	(0.002)	(0.007)
Eligibility at ages 1-5	-0.007	0.002*	-0.000**	6.241**	-0.000	-0.000	0.000
	(0.011)	(0.001)	(0.000)	(2.877)	(0.001)	(0.000)	(0.001)
Eligibility at ages 6-14	0.014**	-0.001	0.000	1.811	0.000	-0.000	0.000
	(0.006)	(0.001)	(0.000)	(1.770)	(0.000)	(0.000)	(0.001)
Eligibility at ages 15-18	-0.004	-0.002***	0.000	2.153	-0.001	0.000	0.001
	(0.008)	(0.001)	(0.000)	(1.829)	(0.001)	(0.000)	(0.001)
Mean	38.78	0.11	0.01	3270.52	0.07	0.01	0.09

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. Births to women born in Arizona are excluded from the sample. Coefficients are from IV regressions weighted by mother's birth cohort size that include mother's state of birth and mother's year of birth fixed effects, and additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). Robust standard errors are clustered by mother's state of birth.

Appendix Table 8
Sensitivity analyses for 2003 birth certificate revision

	Revised birth certificate	Dropping 14 states without revised birth certificates in 2011 from sample			
		High school education	Any prenatal care	Number of prenatal	Prenatal care in first
Baseline model:					
In-utero eligibility	0.213	-0.04	-0.002	0.38	-0.047
	(0.205)	(0.031)	(0.008)	(0.530)	(0.045)
Eligibility at ages 1-18	-0.026	0.003	-0.001*	-0.147***	-0.014**
	(0.017)	(0.002)	(0.001)	(0.049)	(0.006)

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. Births to women born in Arizona are excluded from the sample. Coefficients are from IV regressions weighted by mother's birth cohort size that include mother's state of birth and mother's year of birth fixed effects, and additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). An additional control for the share of births with revised birth certificate records is included for the outcomes related to education and prenatal care utilization. Robust standard errors are clustered by mother's state of birth.

Appendix Table 9
Testing for association between variable availability and Medicaid eligibility

	Alcohol use	Tobacco use
Baseline model:		
In-utero eligibility	0.045	-0.687
	(0.039)	(0.530)
Eligibility at ages 1-18	0.002	-0.056
	(0.002)	(0.058)

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. Births to women born in Arizona are excluded from the sample. The analysis for alcohol use is restricted to ages 15-19 and the analysis for tobacco use to ages 15-21. Coefficients are from IV regressions weighted by mother's birth cohort size that include mother's state of birth and mother's year of birth fixed effects, and additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). Robust standard errors are clustered by mother's state of birth.

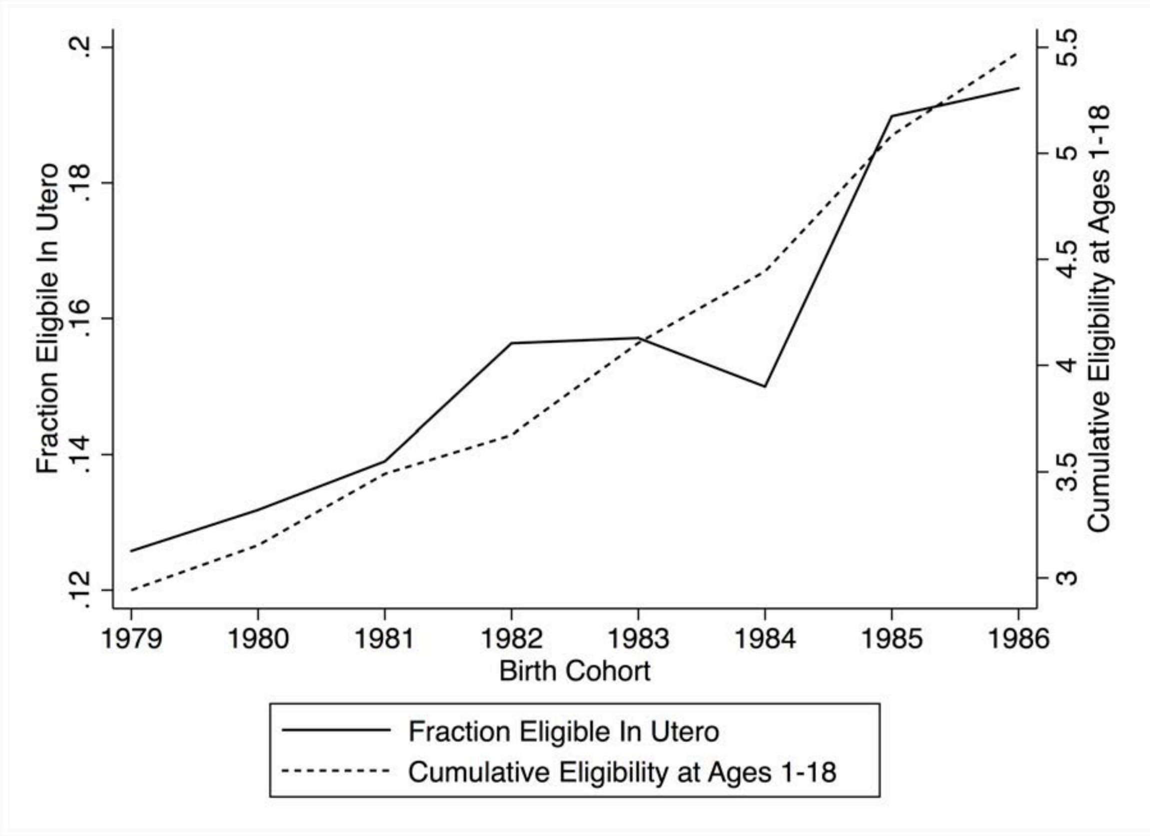
Appendix Table 10

Effects of Parental Medicaid Access, Controlling for Maternal Characteristics

	First generation controls			Second generation state-year controls		
	Baseline Specification	Region *year fixed effects	State trends	State-child year of birth characteristics	Chlid's own in utero eligibility	Mother's cumulative adult eligibility
Outcome: Length of gestation						
In-utero eligibility	0.047	0.077	0.065	0.097	0.049	0.053
	(0.075)	(0.074)	(0.066)	(0.075)	(0.074)	(0.073)
Eligibility at ages 1-18	0.003	0.003	-0.010	0.005	0.004	0.003
	(0.005)	(0.004)	(0.010)	(0.003)	(0.005)	(0.005)
Outcome: Average birth weight						
In-utero eligibility	23.390*	28.799**	30.953**	24.885	27.447**	23.402*
	(13.194)	(13.875)	(14.582)	(15.180)	(13.560)	(13.855)
Eligibility at ages 1-18	1.396	0.685	0.089	2.002**	2.236**	1.396
	(0.950)	(0.897)	(2.193)	(0.891)	(1.022)	(0.975)
Outcome: Preterm birth						
In-utero eligibility	0.000	0.002	-0.01	-0.001	-0.001	0.001
	(0.006)	(0.006)	(0.009)	(0.007)	(0.006)	(0.007)
Eligibility at ages 1-18	-0.001	-0.001	0.002	0.000	-0.001**	-0.001
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Outcome: Very preterm birth						
In-utero eligibility	-0.003**	-0.004***	-0.006***	-0.004***	-0.003***	-0.004**
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
Eligibility at ages 1-18	0.000	0.000	0.000	-0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Outcome: Low birth weight						
In-utero eligibility	-0.002	-0.003	-0.008	-0.008	-0.004	-0.001
	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)
Eligibility at ages 1-18	0.000	0.000	0.001	0.000	0.000	0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Outcome: Very low birth weight						
In-utero eligibility	-0.002	-0.003	-0.006**	-0.004**	-0.003	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Eligibility at ages 1-18	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Outcome: Small for gestation age						
In-utero eligibility	-0.009	-0.009	-0.001	-0.008	-0.009	-0.009
	(0.006)	(0.006)	(0.007)	(0.008)	(0.006)	(0.007)
Eligibility at ages 1-18	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)

Notes: Data are from the 1994-2015 detailed birth data files aggregated by mother's state of birth and mother's year of birth. Sample is composed of all non-multiple births to women born in 1979-1986 at ages 15-28. Births to women born in Arizona are excluded from the sample. Coefficients are from IV regressions weighted by mother's birth cohort size that include mother's state of birth and mother's year of birth fixed effects, controls for mother's race (black and other race), high school completion, and marital status, as well as additional control variables (unemployment rate, personal income per capita, maximum welfare benefit for a family of 4, indicators for state parental consent and notification laws and state Medicaid restrictions for abortion, and demographic controls for each state and year). Additionals controls for the share of births with revised birth certificate records and the share of births with birth certificate records allowing for the report of multiple race categories are included. Second generation state-year controls are also included when indicated and are described in more detail in the text and appendix. Regressions also include mother's region of birth by mother's birth year fixed effects, or mother's state of birth linear trends in mother's birth year when indicated. Robust standard errors are clustered by mother's state of birth.

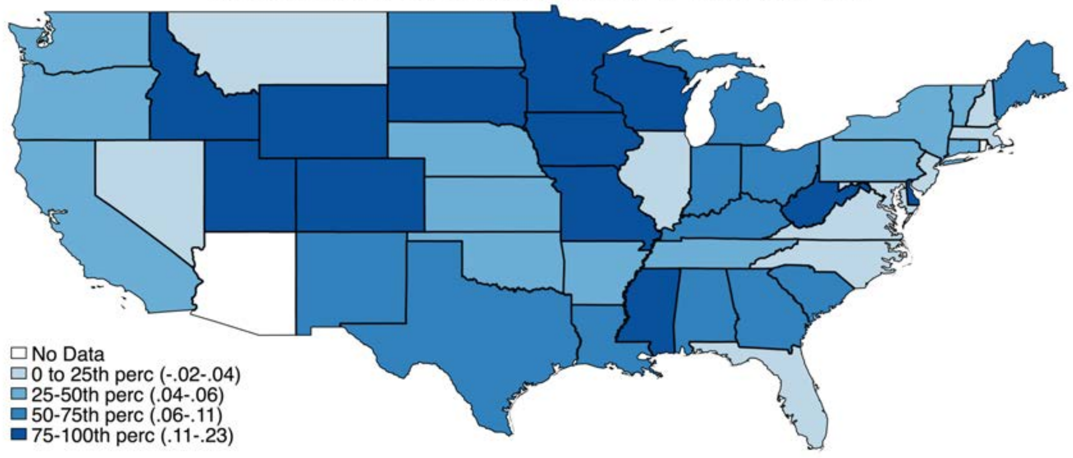
Figure 1: Trends in In Utero and Cumulative Childhood Eligibility by Cohort



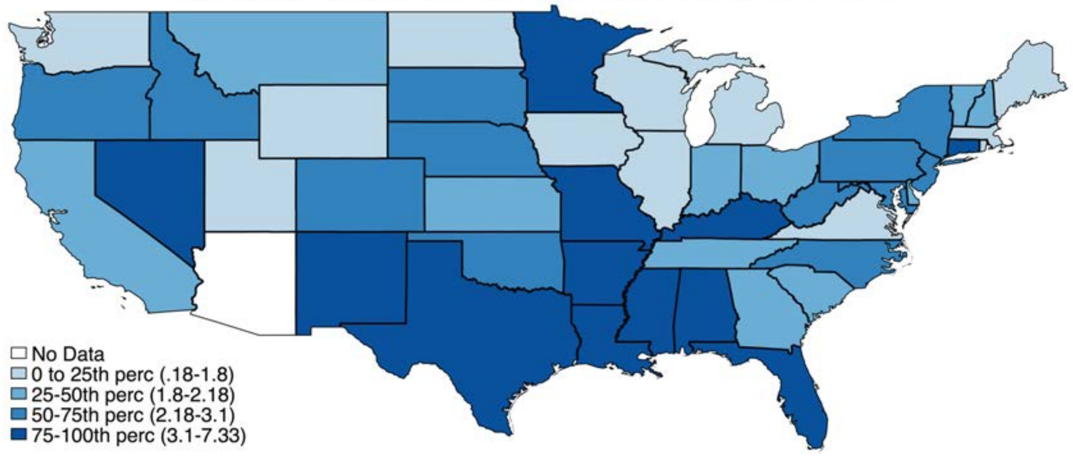
Notes: Authors' calculation from Current Population Survey and Medicaid eligibility rules. See text for further details.

Figure 2: Changes in Medicaid Eligibility by State, 1979 to 1986

(a) Changes in In Utero Medicaid Eligibility by State, 1979-1986



(b) Changes in Medicaid at Ages 1-18 Eligibility by State, 1979-1986



Notes: Authors' calculation from Current Population Survey and Medicaid eligibility rules. See text for further details.