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Intergenerational health effects of Medicaid

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A B S T R A C T

This paper investigates the effects of the introduction of Medicaid during the 1960s on next generations' birth outcomes. A federal mandate that all states must widen the coverage to all cash welfare recipients generated cross-state variations in Medicaid eligibility, specifically among nonwhites who largely overrepresented the target population. I implement a reduced-form difference-in-differences strategy that compares the birth outcomes of mothers born in states with higher cash welfare recipiency versus low welfare recipiency and different years relative to the Medicaid implementation year. Using Natality data (1970–2004), I find that Medicaid significantly improves birth outcomes. The effects are considerably larger among nonwhites, specifically blacks. The effects do not appear to be driven by preexisting trends in birth outcomes, preexisting trends in households' socioeconomic characteristics, changes in other welfare expenditures, and selective fertility. A back-of-an-envelope calculation points to a minimum of 3.9% social externality of Medicaid through income rises due to next generations' improvements in birth outcomes.

1. Introduction

Title XIX of the Social Security Act in 1965 initiated one of the largest federally-funded health insurance programs in US history, Medicaid. The primary purpose of Medicaid was to promote public health, specifically among low-income families. As of 2020, about 37 million children (51% of the child population in the country) are enrolled in Medicaid and Children's Health Insurance Program (Medicaid Snapshots, 2020). Medicaid expenditure surpasses other welfare programs, and as a consequence, it has brought controversies in the media and political environment regarding the size and quality of the program (Currie and Duque, 2019). In 2019, public spending on Medicaid added up to $604 billion, compared to $27 billion for unemployment insurance benefits, $68 billion for the Supplemental Nutrition Assistance Program, and $62 billion for Earned Income Tax Credit.[2] While the costs are explicit enough to be observed, the benefits have been difficult to quantify as they could reveal spillover effects on untargeted outcomes and appear in unintended areas.

The empirical evidence suggests that Medicaid increases the take-up rate of health insurance among low-income families, reduces the number of uninsured children, specifically among nonwhites, improves birth outcomes, and reduces infant mortality rates (Goodman-Bacon, 2018). Cohorts who were exposed to Medicaid during childhood have improved self-reported health outcomes in adulthood (Boudreaux et al., 2016) and improved labor market outcomes (Goodman-Bacon, 2021b). Most empirical evidence comes from the program's eligibility expansion during the 1980 s. These expansions were successful in improving infants' birth outcomes (Currie and Gruber, 1996a, 1996b), reducing infant mortality rates (Bhatt and Beck-Sagué, 2018), lowering adulthood hospitalization and chronic conditions (Miller and Wherry, 2019b), and declining later-life disease-related mortality (Currie and Gruber, 1996).

Imperfect take-up rates and crowd-out effects suggest that there are other mechanisms at work beyond simple increases in the coverage rates (Bronchetti, 2014; Currie and Gruber, 1996; Currie and Gruber, 1996; Shore-Sheppard, 2008). Indeed, studies show that Medicaid has spillovers in other areas that potentially influence health outcomes, including the consumption expenditure of households directly related to material well-being (Lindsey et al., 2010), private saving (Gruber and Yelowitz, 1999), hospital technology adoption (Freedman et al., 2015), and participation in other welfare programs (Bitler and Currie, 2018). In

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[2] See Internal revenue Services (2020), Center on Budget and Policy Priorities (2020), and Bureau of Labor Statistics (2020).

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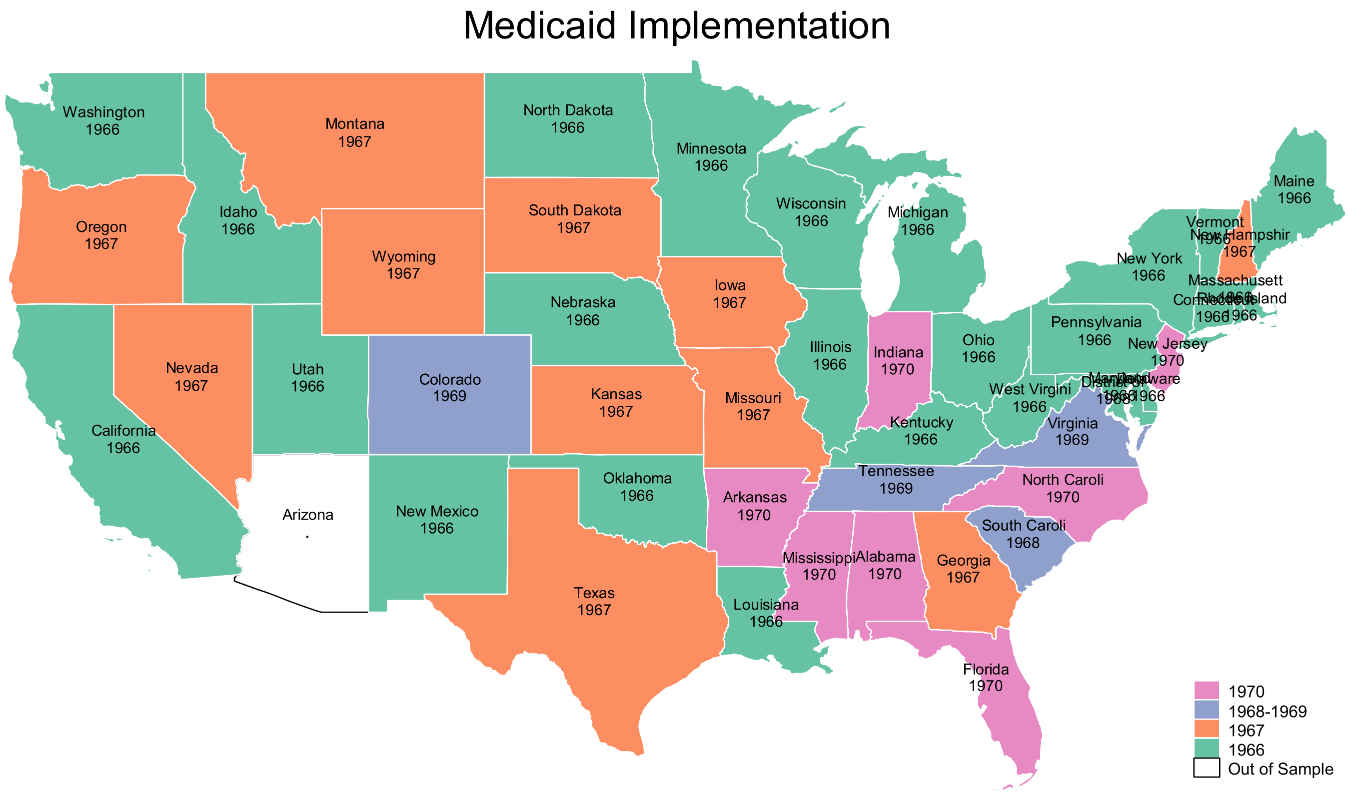

Fig. 1. Medicaid Implementation Timing across the US States.

addition, there is evidence that health improvements could have intergenerational spillover effects. A strand of literature documents the intergenerational transmission of socioeconomic status, human capital, and health (Ahlburg, 1998; Bevis and Villa, 2020; Bhalotra and Rawlings, 2013; Black et al., 2005; Caruso, 2017; Currie, 2009; Currie and Moretti, 2007; Lahti-Pulkkinen et al., 2018; Lundborg et al., 2018; Rossin-Slater and Wüst, 2020; Thompson, 2014).

Although several studies point to the long-run benefits of Medicaid, fewer studies explore the intergenerational effects of the program. An exception is the study of East et al. (2021) that explores the effect of expansions in Medicaid during the 1980s and 1990s on next generations' birth outcomes. They find significant effects of eligibility in utero on next generations' birth outcomes. The current study enters at this point and attempts to contribute to this literature by documenting the intergenerational health benefits of Medicaid introduction in the 1960s. This paper departs from East et al. (2021) in an important way. Before the Medicaid introduction a large share of low-income people did not have any health insurance (Goodman-Bacon, 2018).[3] The implementation of public health insurance could relieve the financial barriers faced by previously uninsured pregnant mothers for essential prenatal care. However, the Medicaid expansions during the 1980–90 s are mainly added benefits to already established insurance. The differential effects are also observed in the large differential intent-to-treat effects in the current study in comparison with theirs (section 6.3).

This paper builds on the methods developed by Goodman-Bacon (2018, 2021b) and exploits the variation in Medicaid implementation across states and over the years 1966–1970 in combination with the federal mandate requiring states to cover all individuals under cash welfare programs (so-called *categorical eligibility*) to provide new evidence of its intergenerational health effects for infants' birth outcomes. The categorical eligibility of cash recipients reflects long-established and institutional features of states' welfare programs and generates a wide cross-state variation in Medicaid implementation. This aspect of the program makes it orthogonal to levels and trends in socioeconomic characteristics or other policy implementations that could also influence the intergenerational links (Goodman-Bacon, 2021b).[4] Moreover, the categorical eligibility is significantly higher among nonwhites. This feature enables the research design to establish a larger identification for this subpopulation.

I apply a difference-in-differences framework that compares the birth outcomes of mothers who were born at different years relative to the state-of-birth-specific year of Medicaid implementation (first difference) in birth states with higher versus lower categorical eligibility (second difference). I provide evidence to rule out the concerns over preexisting trends in birth outcomes, endogenous fertility as a response to eligibility, and the association of Medicaid with other state-level welfare code changes that could potentially confound the estimates. An event-study analysis shows that the birth outcomes of mothers who were born in higher versus lower welfare states do not trend differently for years prior to Medicaid. However, the effects start to diverge from zero for cohorts who experienced Medicaid between ages 0–18 (partially exposed cohorts). The largest divergence occurs for cohorts who were born before the program implementation (fully-exposed cohorts). Moreover, the effects are more pronounced among nonwhites who were over-represented in the target population (welfare recipients). For a one-standard-deviation change in categorical eligibility among nonwhites (7.9% point change), mothers who were fully exposed to Medicaid (throughout the ages 0–18) reveal 15 g higher birth weight, 54 basis points lower probability of low birth weight, and 73 basis points lower likelihood of having a baby categorized as small for gestational age.

This empirical strategy has two advantages compared to other

[3] Kovar (1960a) reports that by 1960 about 90% of people have no doctor visit insurance.

[4] In Sections 6.1 and Appendix A, I explore the endogeneity concerns. The mentioned fact is specifically drawn from analyses in Appendix Table A-2 and further discussions in Goodman-Bacon (2021b).

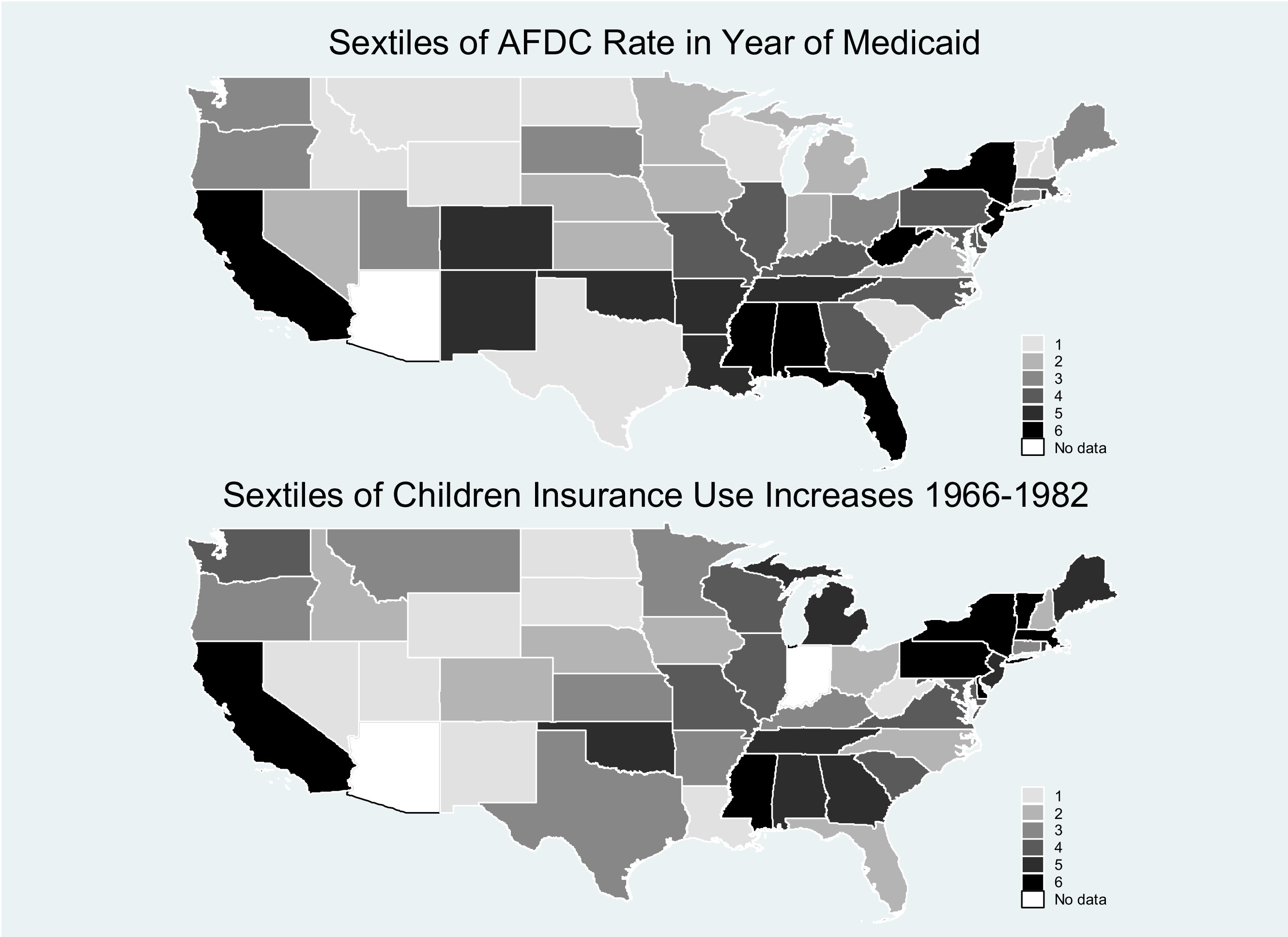

Fig. 2. AFDC Rates and Changes in Children's Insurance Use.

studies that explore the health effects of Medicaid implementation relying on the space-time variations in its timing (Boudreaux et al., 2016; Sohn, 2017) or the studies on sequential expansions during the 1980s (East et al., 2021; Miller and Wherry, 2019a). First, the early decision of states to adopt Medicaid could be correlated with the implementation of other welfare programs, states' budgetary constraints, and other state characteristics. These confounders cause a pre-trend in mothers' health status at birth, which might be transmitted intergenerationally. Second, the 1980 s expansion in Medicaid was accompanied by other benefits such as food and cash transfers, which makes it hard to isolate the effects of public insurance on health.

This paper contributes to the extant literature in two ways. First, this is the first study to shed light on the intergenerational effects of Medicaid introduction in the 1960s. Second, as Chetty (2006) suggested, an optimal level of social insurance is based on its costs and benefits. In the absence of intergenerational externalities, the cost-benefit calculations only reveal a sub-optimal level. The current study adds to the benefits side of this analysis by documenting the intergenerational health effects f the program. Therefore, the results of the paper call for policy reevaluations of public insurance as the intergenerational externality of the program brings considerable social returns. A back-of-an-envelope calculation suggests that the improvements in next generations' birth outcomes lead to a minimum of 3.9% return on the program's initial cost through their income rises in the future.

The rest of the paper is organized as follows. Section 2 reviews the background of Medicaid. Section 3 provides a brief literature review. Section 4 discusses the data sources and sample construction. Section 5 introduces the empirical strategy. Section 6 reviews the results. Section 7 discusses the implications of the findings. Section 8 provides some concluding remarks.

2. Background on medicaid

In 1965, President Johnson signed into law a series of legislations to promote public health insurance among low-income families. At the time, talks on such massive social insurance had advocates and critics who referred to the idea as a "socialized medicine" Cohen (1960). Prior to the reform, public financing of medical care was low, federal matching rates for medical care reimbursement were capped, and the states' public insurance was also limited. As a result, many people were left uninsured, specifically among low-income families. For instance, Kovar (1960b) uses US National Health Survey and reports that in 1960 and among families with less than $2000 income, roughly 67% did not have any hospital insurance, and 91% did not have any doctor visit insurance.[5]

Title XIX of the social security amendment of 1965 initiated the "medical assistant" program to increase insurance coverage of the poor, commonly known as Medicaid. States were required to widen the coverage of medical care insurance for all cash welfare recipients. In return, the federal government removed the reimbursement caps and raised the match rates. About 89% of these categorically eligible

[5] In 1960, the median family income was $5600 (US Census Bureau, 1960).

individuals were qualified through the Aid to Families with Dependent Children (AFDC) program (Goodman-Bacon, 2018, 2021b). Therefore, it is arguable that states with higher AFDC rates (equivalently, higher categorical eligibility) at the time of Medicaid implementation have received a higher share of Medicaid spending. By 1982, all states adopted Medicaid: 26 states in 1966, 11 states in 1967, and the rest until 1970.[6] Fig. 1 illustrates the state-wide variation in Medicaid implementation year. Although several southeast states were late adopters, there is no regional clustering with respect to the timing of the program implementation.

Despite imperfect take-up rates, Medicaid was successful in raising the insured rates, specifically among children (Bernard and Feingold, 1970; Davis, 1976; Okada and Thomas, 1978). For instance, Goodman-Bacon (2018b) shows that five years after Medicaid implementation public insurance rate increases by roughly 10% points among children and by 2% points among adults. In a visual depiction, Fig. 2 shows that states with higher AFDC rates (higher categorical eligibility) experience higher increases in children's insurance use between the years 1966–1982.

3. Literature review

There are several mechanism channels through which benefits of public health insurance during in utero and early childhood could transmit intergenerationally. This section reviews the empirical studies that have provided evidence for this long-term link.

Means-tested and asset-tested social insurance generates an incentive for households to reduce their wealth holding, decline their precautionary saving, and reduce personal bankruptcy (Chou et al., 2004; Chou et al., 2003; Clark and Mitchell, 2014; Gross and Notowidigdo, 2011; Gruber and Yelowitz, 1999). For instance, Gruber and Yelowitz (1999) show that expansions in Medicaid eligibility during the years 1984–1993 are associated with significant reductions in private saving and increases in consumption expenditure. Similarly, Lindsey et al. (2010) show that among low-income families in the Consumer Expenditure Survey, eligibility for the Children's Health Insurance Program (CHIP) is associated with an increase in overall consumption expenditure. They conclude that the program significantly improves the material well-being of low-income families.

This associated increase and compositional change in families' consumption behavior can influence the health of their infants in two ways. First, it could raise the consumption of materials that indirectly influence health, e.g., better nutrition and a healthier residential location, both of which are shown to improve birth outcomes (Almond et al., 2011; Almond and Mazumder, 2011b; Chay and Greenstone, 2003; Currie et al., 2009; Ga and Feng, 2012; Haeck and Lefebvre, 2016; Hill, 2018). Second, it could raise the demand of households for goods and services that directly affect infants' health. These goods and services are generally coupled with health insurance. For instance, households may change the quantity, quality, and timing of prenatal care, all of which has been documented to influence birth outcomes (Corman et al., 2019; Hoynes et al., 2015; Joyce, 1999; Kaestner and Lee, 2005; Leonard and Mas, 2008; Mocan et al., 2015; Shen, 2018; Sonchak, 2015).

A strand of literature assesses the degree to which health can be transmitted intergenerationally. The intergenerational correlations can be attributed to genetics, environment, and genetic-environment interaction. Thompson (2014) attempts to disentangle the genetic effect in the case of specific chronic health conditions. The intergenerational correlations suggest that children of parents with specific chronic health conditions are 100% more likely to have the same health problems in adulthood. However, comparing two sets of parent-biological-child and parent-adoptee-child, he finds that genetic transmission is responsible for only 20–30% of the observed correlation.

It should be noted that a health intervention during prenatal development could alter the programming of epigenomes.[7] This programming is the response of the mother's reproductive system for the sole purpose of survival of the fetus. For instance, a negative nutritional shock raises the risk of neonatal mortality by declining the nutritional intake available for the fetus. The programming change causes the epigenome to turn off some genes related to growth. Although it raises the survival likelihood of infants, it lowers their health endowment. The lower health endowment can be captured by adverse birth outcomes such as low birth weight (Almond and Currie, 2011b). In the absence of a health intervention, the rewritten epigenome of the new generation acts in the same way and turns off the growth-related genes for the next generation, too. Therefore, this theory offers a channel for intergenerational transmission of birth outcomes. Studies show that a wide range of health measures are indeed passed on from parents to children (Ahlburg, 1998; Alacevich and Tarozzi, 2017; Bevis and Villa, 2020; Black et al., 2009; Caruso, 2017; Carvalho, 2012; Classen, 2010; Classen and Thompson, 2016; Coneus and Spiess, 2012; Costa-Font and Jofre-Bonet, 2020; Currie, 2009; Dolton and Xiao, 2015, 2017; Gahlmann et al., 2010; Halliday et al., 2020; Lundborg et al., 2018; Schulkind, 2017). For instance, Currie and Moretti (2007) show that the incidence of low birth weight among children can be explained by the incidence of low birth weight among mothers. They document that the observed intergenerational link is stronger among women born in high poverty areas. Bhalotra and Rawlings (2013) examine the interaction of environment and intergenerational transmission of health across developing countries. They find that poor maternal health is associated with poor infant health outcomes and that this link is exacerbated if children are born during adverse economic conditions.

Insurance-induced improvements in health endowment at birth and the next generations' health also operate through intermediatory outcomes, including adults' health, education, and income. Behrman and Rosenzweig (2004) exploit a twin strategy and show that birth weight marginally improves adults' height, schooling, and earnings. Their results suggest that an increase of 17 oz. (about 481 g) in birth weight is associated with a 6% increase in lifetime earnings. Maruyama and Heinesen (2020) show that while birth weight is a determinant for short-term health outcomes such as infant mortality and cerebral palsy, it is not associated with improved test scores. Royer (2009) applies a twin strategy on two longitudinal datasets and finds that birth weight is related to education, later complications in pregnancy, and birth weight of the next generation. Black et al. (2007) show that infants with higher birth weight have improved outcomes during childhood and adulthood. They find that birth weight increases IQ scores, Body Mass Index, height, high school completion, earnings, and employment. Their results suggest that a 1% rise in birth weight is associated with a 0.12% rise in earnings. On the other hand, maternal education and income have been documented to influence birth outcomes which can be a mechanism channel between health at the birth of mothers to health at the birth of their children (Baird et al., 2011; Bharadwaj et al., 2019; Chou et al., 2010; Currie and Moretti, 2003; Elder et al., 2016; Figlio et al., 2009; Gage et al., 2013; Güneş, 2015; Hoynes et al., 2011, 2015; Kaplan et al., 2017; Kehrer and Wolin, 1979; McCrary and Royer, 2011; Mocan et al., 2015; Shen, 2018; Waldmann, 1992).

An early life health intervention -such as introduction or expansions in public health insurance- could have short-term effects as well as long-term impacts. Goodman-Bacon (2021b) explores the Medicaid

[6] There are two exceptions: Arizona adopted Medicaid in 1982 and Alaska in 1972.

[7] An Epigenome is a multitude of chemical compounds that are attached to the DNA and can set on or turn off some genes for specific reasons. The main key triggers are environmental factors including air quality, nutrition, stress, etc. once rewritten by these triggers, the epigenomes can transmit from mothers to children. See Almond and Currie (2011a), Almond and Currie (2011b), and Barker (1990).

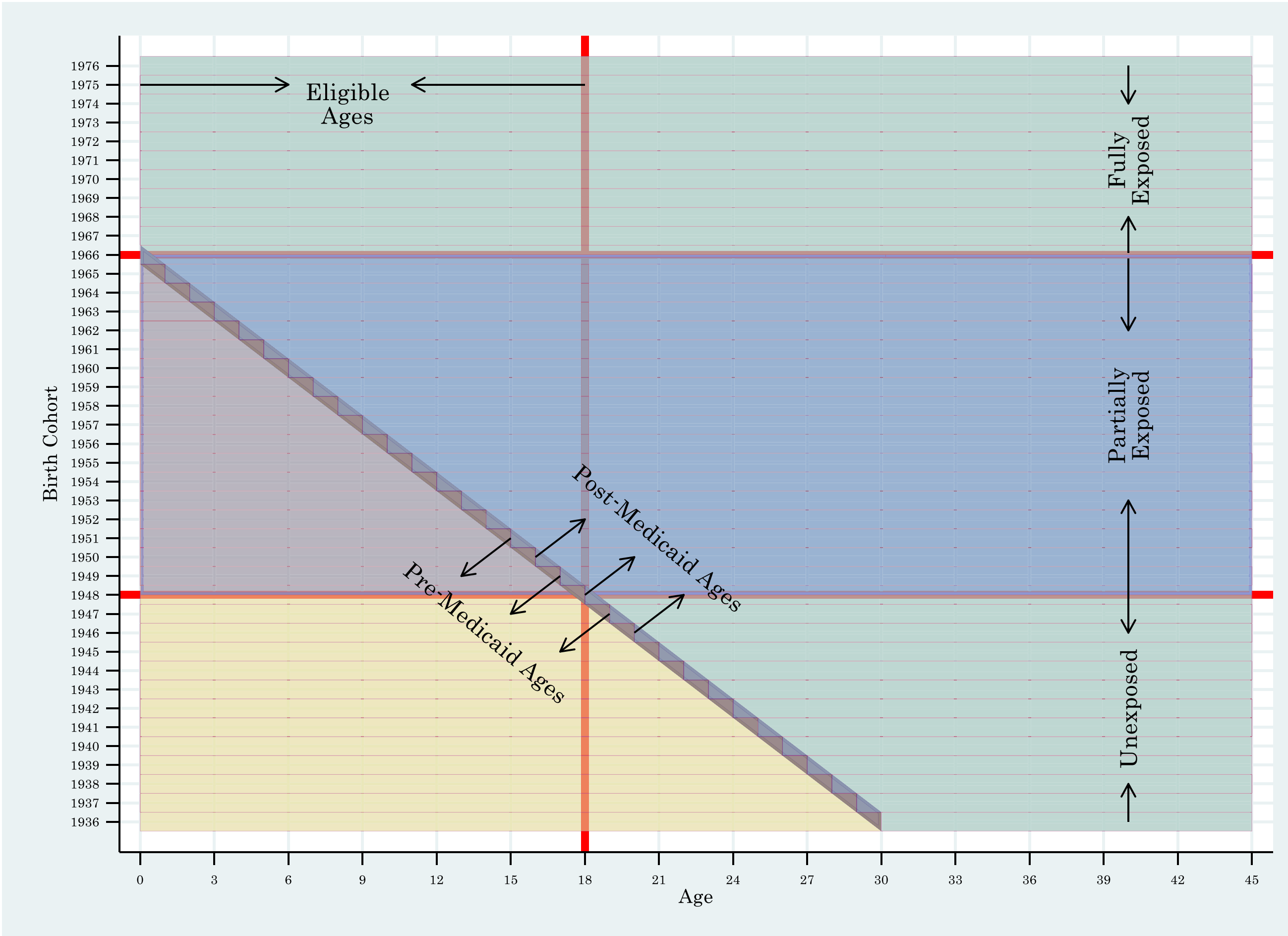

Fig. 3. Exposure to Medicaid Across Cohorts/Ages for States that Implemented Medicaid in 1966.

implementation during the 1960 s on adult health and labor market outcomes. He compares the outcomes of adults in high welfare states to low welfare states after Medicaid implementation to before. He finds that eligibility during childhood is associated with reductions in non-AIDS mortality, ambulatory difficulties, and disability transfer receipt. Moreover, exposed cohorts are more likely to graduate from high school and enjoy higher earnings during adulthood. Goodman-Bacon (2018a) shows that the introduction of Medicaid is associated with lower rates of infant mortality and significant improvements in birth outcomes. Wherry and Meyer (2016) use a regression discontinuity design taking advantage of sharp rises in Medicaid eligibility expansion for children born after September 1983. They find evidence that expansions in Medicaid reduce later-life disease-related mortality rates among blacks. Miller and Wherry (2019b) show that adults whose mothers were exposed to Medicaid expansions during the 1980 s reveal lower chronic conditions and hospitalization rates. Similar studies also show the long-term effects of early-life health environment (Almond et al., 2018; Almond and Currie, 2011a; Almond and Mazumder, 2005; Boudreaux et al., 2016b; Coneus and Spiess, 2012; den Berg et al., 2015; Karlsson et al., 2014; Lin and Liu, 2014; Myrskylä et al., 2013; Myrskylä, 2010a, 2010b; NoghaniBehambari et al., 2020; Rao, 2016; Rossin-Slater and Wüst, 2020; Smith, 2009).

4. Data sources and sample selection

The primary data source used in this study is Natality detailed files extracted from National Center for Health Statistics (2020). It contains the birth certificate of the universe of births in the US. The data reports limited parental characteristics, including information on the mother's age, race, state of birth, state of residence, education, and marital status. Moreover, it includes information on the father's age, race, and education.

The data also reports information on the child's health at birth which I use as the primary outcomes. These outcomes are based on two reported health measures: birth weight and gestational length. I follow the prevailing derivation rules in the literature to construct other health measures, which I discuss below (Hill, 2018; Hoynes et al., 2015).

Birth weight is the weight of the child at the time of birth and is measured in grams. Low birth weight is a dummy that equals one if birth weight is less than 2500 g. Very low birth weight is a dummy that equals one if birth weight is less than 1500 g. Extremely low birth weight is a dummy that equals one if birth weight is less than 1000 g. Full-term birth weight is the child's birth weight at maturity, which is having a gestational length of 37–42 weeks. Small for gestational age is a dummy that equals one if birth weight is at the bottom ten percentile of birth weight distribution within each gestational week. Fetal growth is the average weekly gain in weight, i.e., birth weight divided by gestational week. Preterm birth is a dummy that equals one if birth occurs before 37 weeks of gestation. Apgar score is a 10-minutes qualitative test consisting of five 2-minutes tests related to Appearance, Pulse, Grimace, Activity, and Respiration. It varies between 0 and 10.

I merge this data with information on Medicaid implementation data and AFDC rates (as a proxy for categorical eligibility) taken from

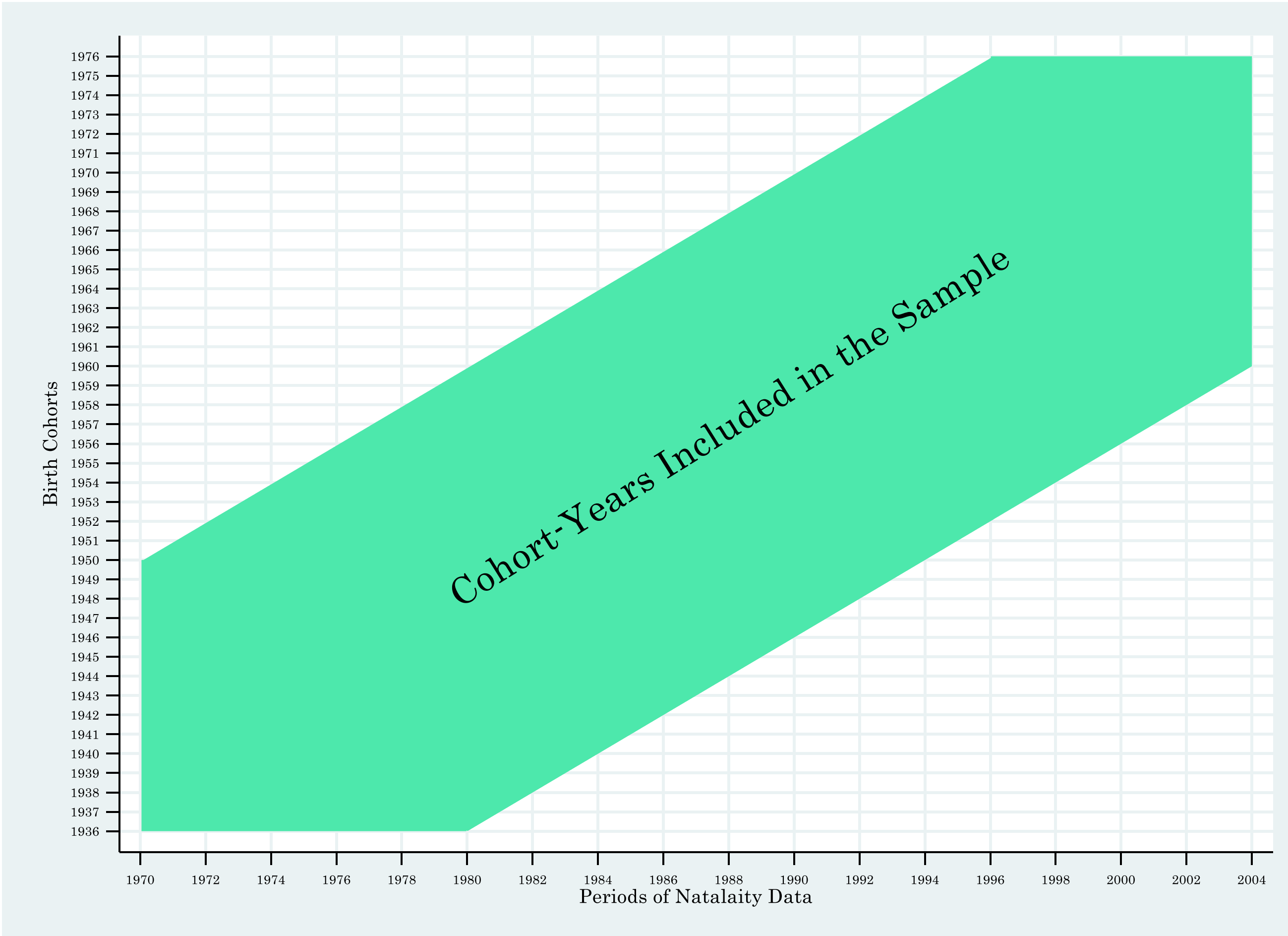

Fig. 4. Illustration of Cohort-Years in the Final Sample (Considering Age Restriction of 20–45).

Goodman-Bacon (2018a). The merging process is based on mothers' state of birth, year of birth,[8] and race. Following Goodman-Bacon (2018a) and Goodman-Bacon (2021b), I aggregate race data into two groups: whites and nonwhites. There are two reasons that I stratify race into two groups. First, there are large differences in categorical eligibility between whites and nonwhites. Second, the data source does not disaggregate nonwhites into smaller groups.

I restrict the sample to mothers aged 20–45. The age restriction is to avoid distorting effects from teenage pregnancy and the risks associated with pregnancy at older ages that might be correlated with birth outcomes (Arya et al., 2018; Ben-David et al., 2016; Carolan and Frankowska, 2011; Jacobsson et al., 2004; Kirdar et al., 2018; Liou et al., 2010; sun Lee et al., 1988; Swamy et al., 2012). In addition, I drop multiple births since their health at birth is consistently lower than singleton births for reasons other than the health environment during the intrauterine period (Almond and Mazumder, 2011a). Moreover, I restrict the sample to first-time mothers for two reasons.[9] First, mothers may respond to the health of their first child by reducing or increasing their fertility. This fertility response could potentially be correlated with other determinants of infants' health, such as mother's demographics and socioeconomic characteristics, which generate sample selection bias (Wolpin, 1997). Second, this sample restriction is a common choice in the literature.[10] I also drop respondents in Arizona, Hawaii, and Alaska.[11] Mothers are observed between the years 1970–2004.[12]

In addition, I restrict the sample to birth cohorts born between 1936 and 1976. The 1936-cohort had reached age 18, the maximum age of Medicaid coverage, 12 years before the first set of states implemented Medicaid. The 1976-cohort was born ten years after the program implementation date.[13] Therefore, the sample consists of several unexposed cohorts, some partially exposed (Medicaid was implemented when they were 1–17 years), and several fully exposed cohorts. Fig. 3 provides an illustrative example of how exposure to Medicaid varies across cohorts/ages for states that passed the law in 1966. For instance, those born in 1948 turn age 18 right before the law is passed and therefore have an assigned exposure of zero. The 1966-cohort (and those born afterward) are fully exposed to Medicaid (from birth to age 18). Those who were born in 1960 turn age 6 in Medicaid implementation year and are eligible only for the remaining years before they turn 18 (partially exposed).

Another dimension to consider is the age at which cohorts enter the analysis sample. For example, given the age restriction of 20–45-year-

[8] Mothers' year of birth is calculated using the year of observation and age.

[9] Appendix D shows the robustness of the main results without this sample section criteria and among all births.

[10] See, for instance, McCrary and Royer (2011) and Currie and Moretti (2003).

[11] Alaska implemented Medicaid in 1972 and Arizona in 1982.

[12] I use the public version of Natality files since the state of birth is omitted in 2005-onwards public Natality files. However, with the current sample selection and in the absence of data availability limitation, the sample size (before collapsing) would have increased by only 4%.

[13] I try to avoid including further cohorts as Medicaid expansions during the 1980 s could confound the eligibility and exposures in the analysis.

Table 1
Summary Statistics.

	Nonwhites			Whites		
	Observations	Mean	Std. Dev.	Observations	Mean	Std. Dev.
Children Characteristics:						
Birth Weight	1938	3218.39	133.2004	1946	3411.434	42.8423
Gestational Weeks	1932	38.8303	0.4812	1946	39.4343	0.2359
Fetal Growth	1932	83.0469	3.089	1946	86.5951	0.9317
Child Sex (Female=1)	1938	0.4934	0.0608	1946	0.4866	0.0098
Full-Term Birth Weight	1930	3356.0027	123.5782	1946	3496.2216	39.413
Low Birth Weight	1938	0.1027	0.0449	1946	0.0513	0.0101
Small for Gestational Age	1932	0.1436	0.0524	1946	0.0889	0.0157
Preterm Birth	1932	0.2182	0.0598	1946	0.1348	0.0236
Apgar Score	1912	8.8927	0.2221	1946	9.0201	0.1277
Birth Weight Rank in Gestational Age Distribution	1938	4.9808	0.5829	1946	5.6875	0.2118
Very Low Birth Weight	1938	0.0191	0.0151	1946	0.0074	0.0021
Extremely Low Birth Weight	1938	0.0095	0.008	1946	0.0034	0.0012
Extremely Preterm Birth	1932	0.0121	0.0105	1946	0.004	0.0017
Low Apgar Score	1912	0.0521	0.0464	1946	0.0338	0.0145
Birth Count	1938	6620.5826	9649.1746	1946	31218.995	39778.432
Mothers Characteristics:						
Birth Cohort	1938	1956.1821	11.8056	1946	1956.1074	11.8393
Age	1938	28.0732	4.8837	1946	28.8077	4.0332
Education	1938	12.1381	0.7598	1946	12.9907	0.5906
State Characteristics:						
Year of Medicaid Implementation	1938	1967.0759	1.4626	1946	1967.073	1.4606
AFDC Rate	1938	19.1851	7.9008	1946	2.593	1.5604
Hospital per Capita	1938	0.0391	0.0176	1946	0.0392	0.0179
Hospital Bed per Capita	1938	5.1284	0.9277	1946	5.0791	0.9306
Per Capita Income	1938	6.1805	4.4451	1946	6.6627	5.1442

Notes. The ***birth weight*** is the weight of the child at the time of birth and is measured in grams. ***Low birth weight*** is a dummy that equals one if birth weight is less than 2500 g. ***Very low birth weight*** is a dummy that equals one if birth weight is less than 1500 g. ***Extremely low birth weight*** is a dummy that equals one if birth weight is less than 1000 g. ***Full-term birth weight*** is the birth weight of the child at maturity, which is having a gestational length of 37–42 weeks. ***Small for gestational age*** is a dummy that equals one if birth weight is at the bottom ten percentile of birth weight within each gestational week. ***Fetal growth*** is the average weekly gain in weight, i. e., birth weight divided by gestational week. ***Preterm birth*** is a dummy that equals one if birth occurs before 37 weeks of gestation. ***Apgar score*** is a 10-minutes qualitative test consisting of five 2-minutes tests related to Appearance, Pulse, Grimace, Activity, and Respiration. It varies between 0 and 10. The birth count is the number of births in each cell.

old mothers and the fact that the sample period covers the years 1970–2004, the 1936-cohort (the earliest cohort) enters the sample at age 34 and is observed from 1970 until the year 1981 when they are 45 years old. Likewise, the 1976-cohort (the latest cohort) enters the sample in 1996 and is observed between ages 20–28. Fig. 4 illustrates the combination of cohort-years in the final sample considering the sample period and age restrictions.

The final sample is then collapsed at the mother's state-of-birth, year-of-birth, and race (white-nonwhite).[14] Table 1 shows summary statistics of the final sample. The incidences of adverse birth outcomes are considerably more prevalent among nonwhites compared to whites. For instance, the average low birth weight among whites and nonwhites is 10.3% and 5.1%, respectively. Similarly, 21.8% and 13.5% of infants are born immature among whites and nonwhites, respectively.

Moreover, the AFDC rates are strongly different among the two groups, which mirrors categorical eligibility differences across the two subpopulations. On average, the AFDC rate is 19.2% among nonwhites and 2.6% among whites. To illustrate the variations in AFDC rate, Fig. 5 depicts boxplots (with minimum, maximum, and interquartile range) and density distribution of the variable among nonwhites (top panels) and whites (bottom panels). Among nonwhites, the rate varies between 1 to roughly 42%, while half the observations lie in the range of 13 (first quartile) to 25 (third quartile) percent.[15]

In addition, to control for state-specific cohort changes in economic and demographic characteristics, I control for a series of mother's state-year-of-birth covariates. Mothers' birth year varies between the years 1936–1976. I use decennial censuses (1930–1980) to build several state-by-year aggregate measures and interpolate linearly for inter-decennial years. The census data is extracted from Ruggles et al. (2020). For further analysis in the appendices, I also use state spending on other welfare programs extracted from Almond et al. (2011). Furthermore, I

[14] One may argue that those (children) born in 1970 to mothers born in 1948 (who are themselves not eligible for Medicaid) are indeed eligible for Medicaid and are treated. However, once a mother is (or otherwise is not) treated, all later-life outcomes become endogenously determined. This also includes the choice of state of residence, health care utilization, and even the type of insurance use. For instance, if we assume that Medicaid exposure during childhood improves adulthood health and labor market outcomes (Brown et al., 2015; Goodman-Bacon, 2021b; Miller and Wherry, 2019a; Wherry and Meyer, 2016), then mothers may become ineligible for Medicaid once they enter maternity ward as they will have higher socioeconomic status. This difference is also evident in their later-life health care utilization (Wherry et al., 2018). Therefore, I ignore whether or not their children are eligible or exposed to Medicaid as it is determined endogenously. I only explore the reduced-form effect of mother's eligibility on child's outcome regardless of child's own eligibility status. Also, note that the sample of children are observed between 1970 and 2004, after the initial introduction and covering mostly Medicaid expansions of the 1980–90 s. The primary reason for collapsing the sample at mother's state and year of birth is to pool the outcomes regardless of their children's birth-place (and so Medicaid eligibility). This collapsing (and not including period-level controls) is a common practice in this literature, e.g., see (DeLeire et al., 2011; East et al., 2021; Goodman-Bacon, 2018, 2021b; Wherry et al., 2018; Wherry and Meyer, 2016).

[15] While I implement the AFDC rate as a continuous variable in the main analysis, I show that using a dummy to capture high versus low AFDC rate states reveal similar and robust results (panel B, Table 4). The idea is that AFDC rates do not vary over a wide range of values and are concentrated over a small interquartile range. Therefore, one may be concerned about imposing a linear assumption in the treatment effect. While the robustness practice rule out this concern similar papers also implement the same strategy and implemented a continuous measure of AFDC rates (Goodman-Bacon, 2018, 2021b).

use Census data (1930–2000) and American Community Survey (2001–2005) extracted from Ruggles et al. (2020).

5. Empirical strategy

5.1. Event study

I estimate a reduced-form effect of Medicaid that compares birth outcomes of mothers who were born in various years relative to Medicaid implementation in their state of birth and had higher versus lower categorical eligibility, peroxided by race-state-specific AFDC rates in the year Medicaid was implemented. Specifically, I apply regressions of the following forms:

$$y_{rcb} = \alpha + \eta_c \times \gamma_M + \zeta_b + \beta X_{cb} + \delta Z_{bM} \times T_c + AFDC_{rb} \left\{ \sum_{k=-\underline{T}}^{-20} \xi_k 1(c - t_b^* = k) + \sum_{k=-18}^{k=\overline{T}} \theta_k 1(c - t_b^* = k) \right\} + \varepsilon_{rcb} \quad (1)$$

The outcome y is the birth outcome of the child whose mother was born in state b, belongs to birth cohort c, and with race r. The index M in the right-hand side of the equation represents the year the Medicaid was implemented in state b. The parameter ζ represents the mother's birth state fixed effect. The mother's birth year fixed effect (represented by η) is interacted with Medicaid implementation year dummies (represented by γ) to account for different paths in cohorts' outcomes across early versus late adopters. In X, I include a series of controls measured at the mother's state-of-birth and year-of-birth level, including average married mothers, average number of prenatal doctor visits, average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. In Z, I use a series of economic and welfare measures at the state-of-birth-by-Medicaid-year level interacted with a linear trend in the birth cohort. These measures include per capita transfer receipt from Food Stamp, income per capita, and the number of hospitals per capita.[16]

The simple idea behind this event-study is to compare the outcomes across two dimensions: 1) eligibility based on each cohort's age at the time of Medicaid implementation (measured by event-time dummies); 2) the fact that states with a higher share of welfare recipients exhibit higher eligibility (measured by AFDC rate at the Medicaid implementation year). AFDC rate is normalized by its race-specific standard deviation to ease the interpretation of the effects. Each set of coefficients ξ and θ represent the (covariates-fixed-effects adjusted) association between initial AFDC rates and next generations' birth outcomes for cohorts born up to $\underline{T}$ periods before state-specific Medicaid implementation year (t_b^*) and $\overline{T}$ periods after the implementation, respectively. I group all cohorts who were born more than 30 years prior to Medicaid into one category. Therefore, $\underline{T}$ represents those born 31-and-more years before Medicaid (t <-30). Similarly, I group all cohorts born 10-and-more years after Medicaid into one group and so $\overline{T} = t > 10$. I eliminate the dummy for cohorts who turned 19 at the year of Medicaid implementation (k = -19) to normalize all the coefficients with respect to those cohorts. All regressions are weighted by birth counts in each cell. Standard errors are clustered at the state-of-birth of mothers. Finally, ε is a disturbance term.

The event-study analysis has two advantages for studying the later-life effects of Medicaid. First, it allows me to observe and detect the preexisting trends in birth outcomes for non-eligible cohorts. Second, it brings suggestive evidence for the ages for which health insurance becomes more effective.

[16] Finkelstein (2007) mentions that the timing of Medicaid implementation was restricted by the hospital capacity of states.

5.2. Difference-in-difference strategy

As a further analysis, I compare the outcomes of those cohorts that were fully eligible for Medicaid between ages 0–18 to those ineligible cohorts (first difference) in states with higher versus lower AFDC rates (second difference). Note that I eliminated those partially exposed cohorts to Medicaid to measure the maximum suggestive effect of Medicaid eligibility on next generations' health. Specifically, I use regressions of the following forms using ordinary-least-square estimation strategy:

$$y_{rcb} = \alpha + \eta_c \times \gamma_M + \zeta_b + \beta X_{cb} + \delta Z_{bM} \times T_c + \lambda AFDC_{rb} \times Exp_{cb} + \phi Exp_{cb} + \varepsilon_{rcb} \quad (2)$$

Where all parameters are as in Eq. (1). Also, I follow the same weighting and clustering rules as Eq. (1). The parameter *Exp* is a dummy that equals one if the respective birth cohort was eligible for Medicaid during ages 0–18 (born after Medicaid implementation) and zero otherwise. Finally, the parameter λ is the coefficient of interest that measures the intention-to-treat effect of the full exposure to Medicaid on birth outcomes of the next generation.

6. Results

6.1. Concerns over endogeneity

The primary assumption in establishing long-term links of Eqs. 1 and 2 -that the birth outcomes of mothers with higher exposure to Medicaid follows the same path and are influenced by the same determinants as birth outcomes of mothers with lower childhood exposure to Medicaid-can be violated due to three primary potential sources of endogeneity which I discuss below.

First, the combination of eligibility rules and implementation timing may generate incentives for women to select themselves into the maternity ward. This fact introduces bias in the long-run links if there are characteristics that affect fertility selection. To explore the concern over endogenous fertility, I regress a series of parental attributes on AFDC rate interacted with an exposure dummy, conditional on fixed effects and covariates. The results, reported in Table 2, do not provide any statistical evidence that eligibility is associated with changes in the composition of births, specifically across parental race, age, and education.

Second, Medicaid could have been accompanied by other state welfare programs, specifically the War on Poverty initiated during the same timeframe. Appendix Table A-1 shows that Medicaid implementation timing was not associated with a statistically significant change in other welfare programs.

The third source of endogeneity is the cross-cohort (cohorts in high-versus low-welfare states) preexisting trends in childhood circumstances and families' socioeconomic characteristics. I explore this confounding trend by regressing a series of family characteristics on the interaction between year dummies and AFDC rates using a state-year panel of census data between the years 1930–1970. The results, reported in Appendix Table A-2, provide no statistical evidence of pre-existing trends in households' socioeconomic characteristics, including wage, employment, education, home-ownership, house value, and house facilities.

6.2. Event-study results

The event-study result for birth weight is reported in Fig. 6 for nonwhites and whites in the top and bottom panels, respectively. The point estimate for cohorts who turned age 19 at the time of Medicaid implementation is set to zero so that other coefficients are normalized with respect to these cohorts. There are virtually no pre-trend movements among unexposed cohorts. Their point estimates are statistically

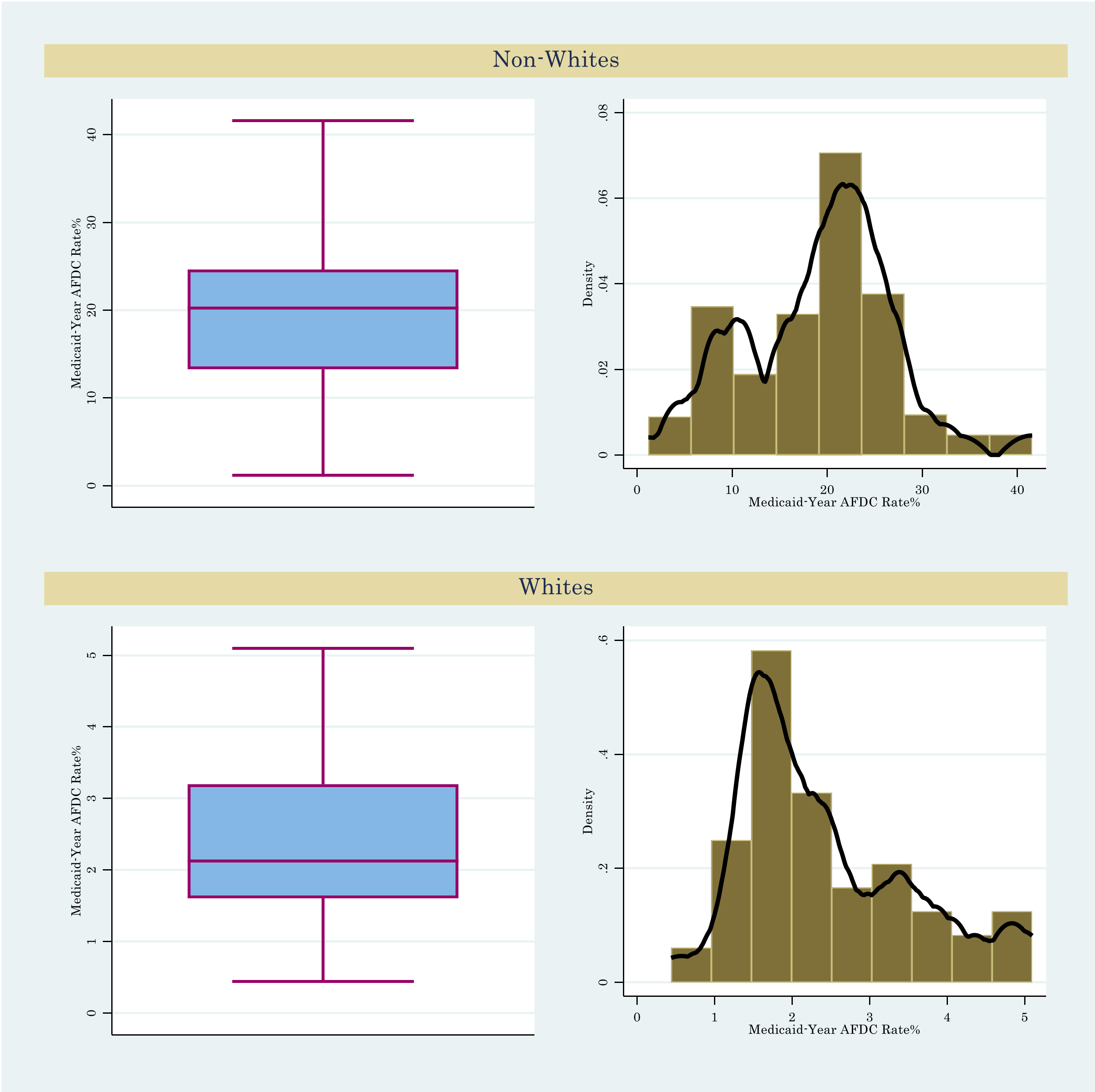

Fig. 5. Variation in AFDC Rate among Whites and Nonwhites.

insignificant at 90% level and quite small in magnitude (relative to exposed cohorts). The effects start to rise for both whites and nonwhites who experienced Medicaid eligibility between ages 1–5. Among nonwhites, the effects diverge significantly for those who turn age 10-and-below (i.e., $T \geq$-10) and become statistically significant. Fully exposed nonwhites exhibit point estimates between 10 and 13 g for a standard deviation change in AFDC rate. The same pattern is observed among whites while their point estimates are imprecisely estimated and much smaller in magnitude.

One may truly argue that the point estimates should be flattened for fully exposed cohorts as the eligibility rules are the same across those who were born one or ten years after Medicaid implementation. However, the point estimates of fully-exposed nonwhites show a slightly increasing trend, specifically when comparing cohorts born 1–3 years after the reform to those born 4–9 years after the reform. One reason could be low take-up rates that could vary by demographic characteristics (Currie and Gruber, 1996; Currie and Gruber, 1996). For instance, Aizer (2003) shows that minorities such as Asians and Hispanics reveal significantly low take-up rates for reasons such as application-cost difficulties, language barriers, and information shortage. In addition, she documents that the Application Assistant program initiated in California (1996–2000) could raise the enrolment rates. These demographic-specific enrolment barriers that may have been alleviated over time could partly explain the changes in point estimates of fully exposed nonwhites.

The same pattern of effects is observed for low birth weight (Fig. 7), small for gestational age (Fig. 8), fetal growth (Fig. 9), very low birth weight (Appendix Figure B-1), extremely low birth weight (Appendix Figure B-2), full-term birth weight (Appendix Figure B-3) gestational age (Appendix Fig. B-4), preterm birth (Appendix Figure B-

Table 2
Endogeneity in Fertility to Medicaid Eligibility.

	Outcomes:					
	Mother White (1)	Mother Age (2)	Mother Years of Schooling (3)	Father Years of Schooling (4)	Father Black (5)	Father White (6)
Born After Medicaid × AFDC Rate	-0.0047	-0.2157	-0.0388	0.0252	-0.0167	-0.0019
	(0.0092)	(0.1532)	(0.041)	(0.069)	(0.0108)	(0.0082)
Observations	3884	3884	3884	3845	3884	3884
R-squared	0.1625	0.8632	0.6215	0.8768	0.188	0.189
Mean DV	0.826	26.662	12.914	12.703	0.108	0.759
Percentage Effect	-0.568	-0.809	-0.301	0.199	-15.452	-0.255

Notes. Standard errors, reported in parentheses, are clustered at mother's state-of-birth level. Regressions include mother's birth cohort fixed effects and mother's state of birth fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. Regressions are weighted using the average of birth counts in each cell.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

5), and Apgar score (Appendix Figure B-6). There are two main takeaways from the series of event-study figures. First, the largest effects appear in adverse birth outcomes. For instance, among fully eligible nonwhites and for a one-standard-deviation rise in AFDC rates, the point estimates imply an increase in full-term birth weight and gestational weeks of 0.12–0.26% and 0.03–0.12% from the mean of the outcome, respectively.[17] However, the implied percent changes from the mean are larger for adverse birth outcomes such as low birth weight (3.7–6.3%) and small for gestational age (2.7–4.5%). This fact suggests that Medicaid was more effective for those (next generations') children at the bottom quantiles of health endowment. Second, the differences in percent changes from the mean suggest that the effects are more pronounced for outcomes related to birth weight rather than gestational age. This fact is better captured by looking at the fetal growth, which measures the intrauterine weekly growth in weight. The point estimates imply a 0.23–0.42% increase from the mean of fetal growth for a one-standard-deviation change in AFDC rates among nonwhites. These percent changes are almost identical to the percent changes for average birth weight (0.27–0.39%).

6.3. *Difference-in-difference results*

The difference-indifferences regression results are reported in Tables 3 and 4. Comparing eligible cohorts to ineligible cohorts among nonwhites, a one-standard-deviation rise in AFDC rates (equivalent to 7.9% points change) is associated with roughly 15 g higher birth weight. It is also associated with 54 basis points reduction in low birth weight, 73 basis points reduction in small for gestational age, and 0.36 g per week increase in fetal growth. The largest effects occur for adverse birth outcomes, as one can deduce by observing the percent change values (row 5 within each panel).

Moreover, two facts support the earlier findings that the effects appear mainly for weight-related outcomes rather than gestational age. First, the estimated coefficients of gestational weeks and preterm birth are insignificant. Second, the percent changes of fetal growth and birth weight are very similar (0.45% and 0.46%, respectively). Among whites (panel B), all the coefficients have the expected sign but are mostly imprecisely estimated. In addition, the marginal effects are quite small in magnitude. The only discernible effect is the coefficient of small for gestational age. It suggests a 17 basis points reduction among fully-eligible cohorts due to a one-standard-deviation change in AFDC rate (2.5% change).

The overall results are in line with the findings of East et al. (2021), who explore the multigenerational health effect of recent expansions in Medicaid and find significant effects on birth weight of next generations and no statistical links for gestational age. One difference between the findings of this paper and their study is the magnitude of the effects. They find that a 10% points increase in mothers' eligibility is associated with 7 g higher birth weight for their children. The implied coefficient of column 1 in Table 3 suggests that a 10% points increase in AFDC rate (a proxy for eligibility) is associated with 18.3 g higher birth weight among nonwhites and an insignificant increase of 10.4 g among whites.[18] Although both the current paper and the study of East et al. (2021) reveal intention-to-treat effects, this comparison suggests effect sizes due to Medicaid introduction that are considerably larger in magnitudes compared with later expansions during the 1980–90 s. One potential explanation for the observed difference is that Medicaid expansions in many cases provided added benefits and coverage to those already covered, and the effects are at the margin while the introduction of Medicaid during the 1960 s offered the very essentials of health care to those without any previous coverage.

To convert the intention-to-treat effects into treatment-on-treated effects, I use two values reported in (Goodman-Bacon, 2018). First, AFDC recipients constitute roughly 90% of welfare recipients. Second, I use the first stage effects on the impact of a 1% change in AFDC rates on children's insurance take-up (about 3.8% points rise). As a result, one can compute a treatment-on-treated effect of about 420 g. This effect is quite significant in comparison with other health intervention programs. For instance, Almond et al. (2011) explore the effect of the Food Stamp Program on birth outcomes. They calculate a treatment-on-treated impact of about 20 and 42 g additional birth weight among whites and blacks, respectively.

6.4. *Robustness checks*

I explore the robustness of the main results to alternative specifications in Table 4.[19] In panel A, I control for within-cohort confounders that vary by place of birth by adding the mother's census-region-of-birth-by-birth-cohort fixed effects. Among nonwhites and across outcomes, although the marginal effects become smaller in magnitude, they are statistically significant at conventional levels. In panel B, I replace the AFDC rate with a dummy indicating the state has an above-median (versus below-median) AFDC rate. Again, the effect sizes across both subsamples and all outcomes are quite similar and comparable to the main results. Finally, in panel C, I replicate the baseline analysis with

[17] These numbers are calculated based on the minimum and maximum coefficients of event-study results for each outcome in combination with the mean of the outcomes reported in Table 1.

[18] This is calculated by dividing the coefficients by their race-specific standard deviation from Table 1 and multiplying by 10.

[19] All models in this table include a full specification, containing all covariates and fixed effects of Table 3.

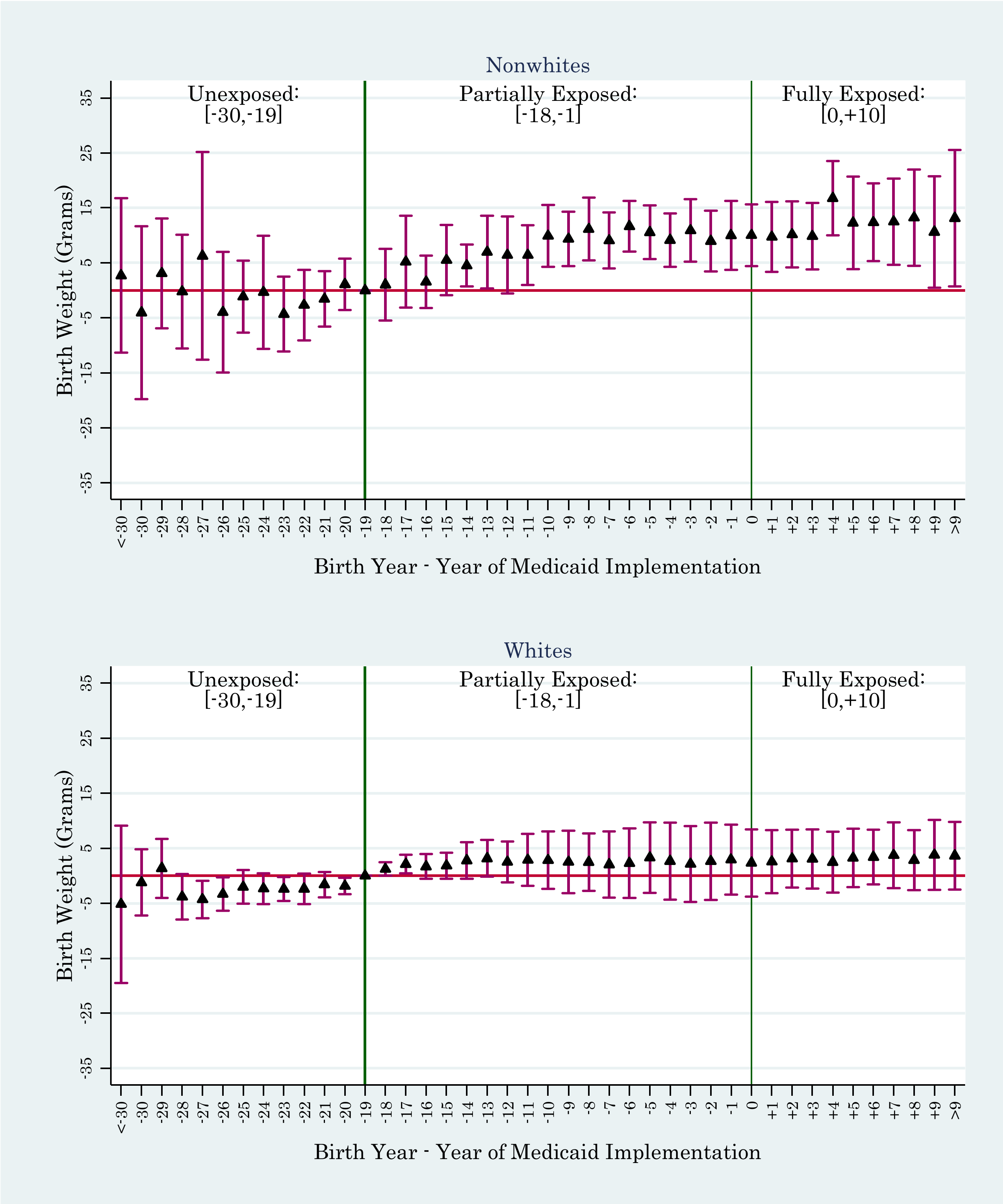

Fig. 6. Event-Study Results of Medicaid Eligibility on Next Generation Birth Weight. Notes. Each point represents the coefficient (and its 90% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is the birth weight of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.

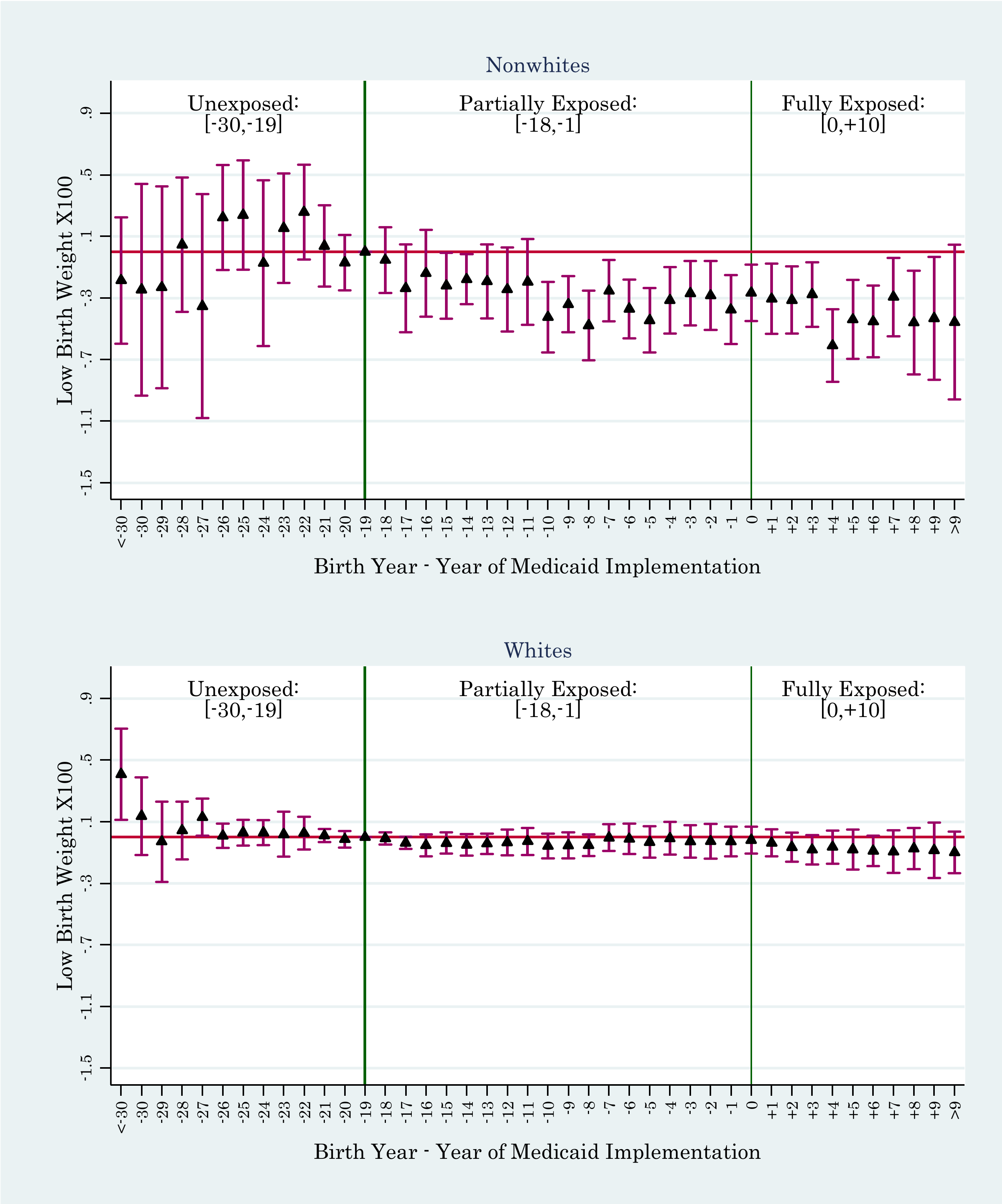

Fig. 7. Event-Study Results of Medicaid Eligibility on Next-Generation Low Birth Weight. Notes. Each point represents the coefficient (and its 90% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is low birth weight of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.

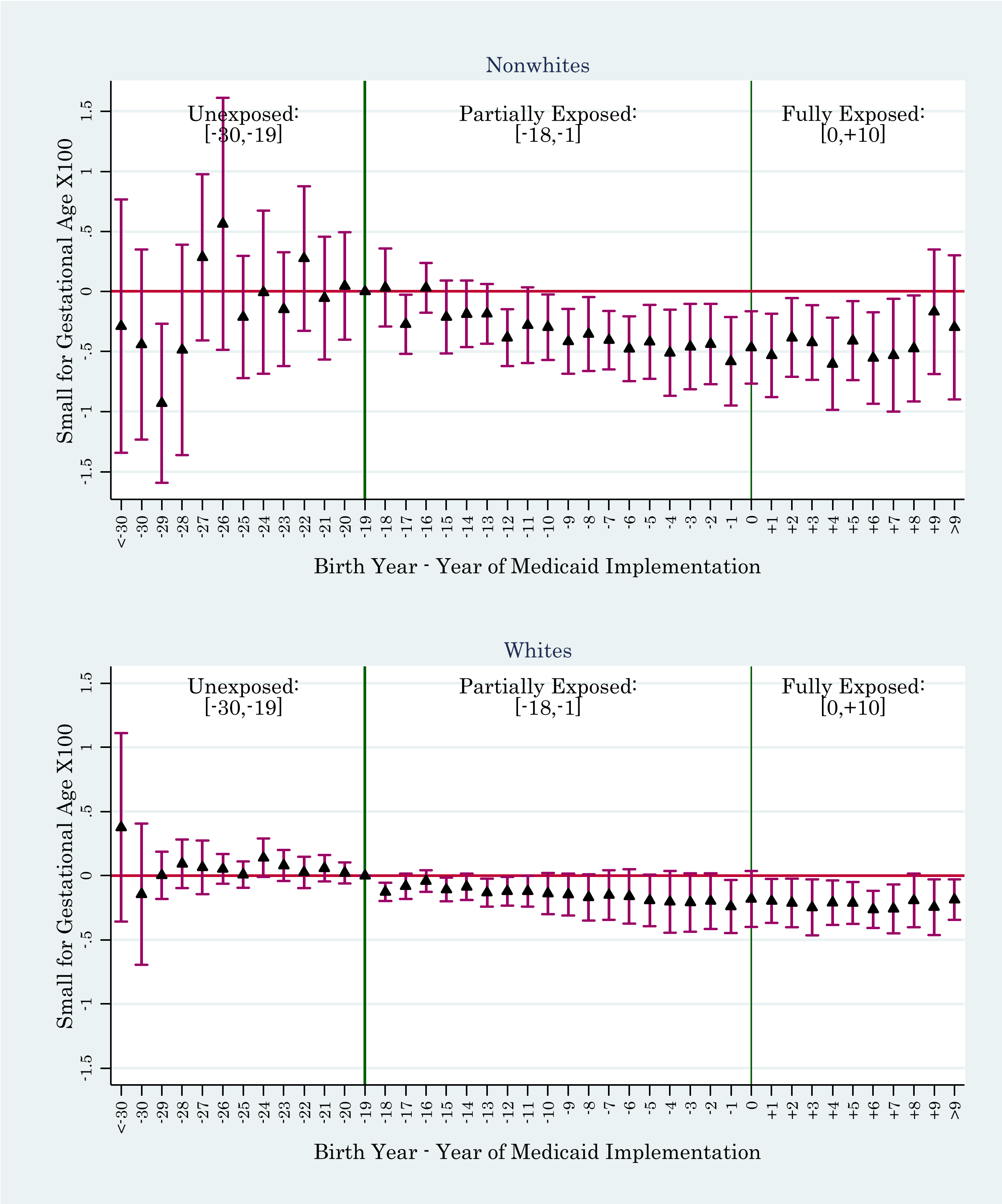

Fig. 8. Event-Study Results of Medicaid Eligibility on Next Generation Small for Gestational Age. Notes. Each point represents the coefficient (and its 90% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is small for gestational age of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.

Fig. 9. Event-Study Results of Medicaid Eligibility on Next Generation Fetal Growth. Notes. Each point represents the coefficient (and its 90% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is fetal growth of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.

Table 3
Intergenerational Health Effects of Medicaid Implementation for Birth Outcomes.

	Outcomes:									
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Full-Term Birth Weight	Small for Gestational Age	Fetal Growth	Gestational Weeks	Preterm Birth	Apgar Score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A. Nonwhites									
Exposed Ages 0–18 × AFDC Rate	14.5217 * **	-0.0054 * **	-0.0014 * *	-0.0009 * *	9.8045 * *	-0.0073 * **	0.3666 * **	0.013	-0.0003	0.0287 *
	(3.3152)	(0.0008)	(0.0005)	(0.0004)	(3.6562)	(0.0016)	(0.0765)	(0.0187)	(0.002)	(0.0159)
Observations	1058	1058	1058	1058	1054	1055	1055	1055	1055	1051
R-squared	0.9569	0.878	0.77	0.7366	0.9327	0.834	0.9403	0.9379	0.9236	0.8808
Mean DV	3157.581	0.108	0.022	0.012	3304.314	0.147	81.753	38.561	0.244	8.891
Pct. Effect	0.460	-4.986	-6.233	-7.200	0.297	-4.960	0.448	0.034	-0.143	0.323
	Panel B. Whites									
Exposed Ages 0–18 × AFDC Rate	2.7167	-0.0004	0.0001	0.0001	3.0063	-0.0017 *	0.0685	0.0006	-0.0003	-0.0064
	(2.8333)	(0.0004)	(0.0001)	(0.0001)	(2.7282)	(0.001)	(0.0712)	(0.0115)	(0.0009)	(0.0073)
Observations	1089	1089	1089	1089	1089	1089	1089	1089	1089	1089
R-squared	0.9473	0.904	0.7361	0.6071	0.9323	0.9187	0.9233	0.9783	0.975	0.9523
Mean DV	3402.865	0.050	0.008	0.004	3490.066	0.083	86.619	39.271	0.148	8.992
Pct. Effect	0.080	-0.857	-0.343	-0.216	0.086	-2.046	0.079	0.001	-0.224	-0.072
State of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State of Birth Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Standard errors, reported in parentheses, are clustered at mother's state-of-birth level. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

Table 4
Robustness Checks to Alternative Specifications.

	Subsamples and Outcomes:							
	Nonwhites				Whites			
	Birth Weight	Low Birth Weight	Small for Gestational Age	Fetal Growth	Birth Weight	Low Birth Weight	Small for Gestational Age	Fetal Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Adding Census Region by Birth-Cohort Fixed Effects								
Exposed Ages 0–18 × AFDC Rate	8.6806 * *	-0.0037 * **	-0.0052 * **	0.2758 * **	1.134	-0.0003	-0.0015	0.0723
	(3.7329)	(0.0012)	(0.0018)	(0.0952)	(2.8977)	(0.0004)	(0.001)	(0.0839)
Observations	1058	1058	1055	1055	1089	1089	1089	1089
R-squared	0.9631	0.8918	0.8536	0.9472	0.9588	0.9183	0.9326	0.9366
Mean DV	3157.581	0.108	0.147	81.753	3402.865	0.050	0.083	86.619
Pct. Effect	0.275	-3.435	-3.526	0.337	0.033	-0.560	-1.747	0.083
Panel B. Replacing AFDC Rate with an Indicator of High/Low AFDC Rate								
Exposed Ages 0–18 × AFDC Dummy	15.0741 * **	-0.0068 * **	-0.0075 * *	0.4019 * **	2.4091	-0.0002	-0.0018	0.0478
	(5.6079)	(0.0017)	(0.0029)	(0.1361)	(6.028)	(0.0008)	(0.0015)	(0.1382)
Observations	1058	1058	1055	1055	1089	1089	1089	1089
R-squared	0.9555	0.8755	0.8283	0.9384	0.947	0.9036	0.9173	0.9227
Mean DV	3157.581	0.108	0.147	81.753	3402.865	0.050	0.083	86.619
Pct. Effect	0.477	-6.278	-5.113	0.492	0.071	-0.396	-2.166	0.055
Exposed Ages 0–18 × AFDC Rate	14.5217 * **	-0.0054 * **	-0.0073 * **	0.3666 * **	2.7167 * **	-0.0004 * **	-0.0017 * **	0.0685 * **
	(1.8925)	(0.0006)	(0.0009)	(0.0514)	(0.4081)	(0.0001)	(0.0003)	(0.012)
Observations	1058	1058	1055	1055	1089	1089	1089	1089
R-squared	0.9569	0.878	0.834	0.9403	0.9473	0.904	0.9187	0.9233
Mean DV	3157.581	0.108	0.147	81.753	3402.865	0.050	0.083	86.619
Pct. Effect	0.460	-4.986	-4.960	0.448	0.080	-0.857	-2.046	0.079

Notes. Standard errors, reported in parentheses, are clustered at mother's state-of-birth level. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

clustering standard errors at the birth cohort level. It appears that state-of-birth is a more conservative level of clustering and produces larger estimated standard errors.

The difference-in-differences (DD) estimator –with a treatment-control dimension and a post-pre dimension- is a weighted average of all 2-by-2 DD comparisons (Goodman-Bacon, 2021a; Sun and Abraham, 2021). For instance, it compares groups that take the treatment with those that have yet to receive it and those who are treated later with those who received the treatment earlier. Sun and Abraham (2021) provide an event-study estimate robust to heterogeneous treatment across units and avoid contaminating the estimated coefficients by comparing later-treated to earlier-treated units. The results of Sun-Abraham estimates on birth weight as the outcome are reported in Fig. 10. I observe the same pattern of effect as the OLS-produced event-studies of Fig. 6. In addition, in Appendix Figure F-1, I show that the effects are robust to the dynamic treatment estimates introduced by de Chaisemartin and d'Haultfoeuille (2020). In Appendix Figure F-2, I report the results of Bacon-decomposition, where the overall OLS is disaggregated into its 2-by-2 DD estimates and their weights (Goodman-Bacon, 2021a). The results are robust across group comparisons.

6.5. Heterogeneity analysis

I explore the heterogeneity of the results by child's gender in Table 5 and Table 6 for nonwhites and whites, respectively. The effects are larger among girls for most of the outcomes. For instance, a one-standard-deviation change in AFDC rate implies a 6.8 (5.8) percent decline from the mean of low birth weight, 13 (2.3) percent reduction from the mean of very low birth weight, and 0.7 (0.5) percent increase from the mean of fetal growth among girls (boys). This marginally higher benefit for girls does not appear to hold for the whites' subsample (Table 6) though virtually all the coefficients are statistically insignificant.

One may truly argue that black people are more likely to be under welfare percipiency and so reveal higher exposure to Medicaid, which can, in turn, be reflected in larger marginal effects. The problem is that the Medicaid-year-specific AFDC rates are only available for nonwhites as a whole group and are not disentangled into detailed race groups. In Appendix E, I focus on the subsample of blacks in Natality data while using the AFDC rate of nonwhites as a proxy for the AFDC rate among blacks and replicate the main results. The estimated effects are larger than the sample of nonwhites reported in Table 3. For instance, the marginal effects on birth weight, low birth weight, and small for gestational age are 70, 57, and 64% higher among blacks than the average of non-whites. In the 1970 census, the share of welfare recipients among blacks is about three times that of nonwhite non-blacks. Looking at these statistics, one may a priori expect the estimated marginal effects of blacks to be much larger compared to the sample of non-white non-blacks.

Since adverse birth outcomes such as low birth weight have arbitrary (though standard) definition-thresholds, one may be concerned that the larger effects for these outcomes are sensitive to the associated departure points. To address this concern, I define various low birth weight dummies as the birth weight less than x grams where x varies between 1500 g to 3500 g. I then use Eq. (2) with each low birth weight dummy as the primary outcome. I illustrate the point estimates and their 90% confidence intervals in Fig. 11. Although with different magnitudes at different thresholds, the coefficients are statistically significant in all regressions of nonwhites. I repeat this exercise for size for gestational age. I define different dummies that equal one if the size for gestational age is at $x \in [1, 20]$ ventile of birth weight distribution within each gestational week. Fig. 12 depicts the marginal effects and 90% confidence intervals for these constructed dummies as the primary outcomes. Among nonwhites and for outcomes that refer to being "small for gestational age" ($x<10$), the coefficients are negative and statistically significant in most cases specifically for $x< 5$. For outcomes that imply "big for gestational age" ($x>10$), the effects become positive and mostly statistically significant. Although I observe the same pattern among whites, their marginal effects are insignificant in all cases.

6.6. Mechanisms of impact

A limited number of studies establish the long-term effects of Medicaid for employment, income, wealth, and other adults' health measures (Cohodes et al., 2016; Goodman-Bacon, 2021b; Miller and Wherry, 2019b). In addition, a relatively larger body of literature document such long-term links for other health interventions and health shock during childhood (Almond, 2006; Coneus and Spiess, 2012; Flores and Kalwij, 2014; Myrskylä et al., 2013). Moreover, since income and availability of resources could directly, e.g., through increases in health care utilization, or indirectly, e.g., through better nutrition and healthier environment, affect infants' and children's health outcomes, it could act as a channel of impact in intergenerational links (Currie, 2009).

While I mainly rely on the literature, I explore the mechanisms in two ways. First, in Appendix Table C-1, I show that higher eligibility is associated with a higher number of prenatal visits in the mothers' generation. The effect size is larger among nonwhites than whites. In Appendix Table C-2, I use census data (1970–2000) and American Community Survey data (2001–2005) and show that higher exposure to Medicaid is associated with higher personal income, increases in family income, and lower receipt of social security income. Again, the effects are considerably larger among nonwhites versus whites.

7. Discussion

The implications of the intergenerational impact of Medicaid for birth outcomes can be better understood by its short-term and long-term impacts on other outcomes during infancy and adulthood. I discuss the implication of the results by evaluating the importance of health at birth on two outcomes: infant mortality and adulthood income.

In the analysis of birth outcome and infant mortality, I use Matched Multiple Birth data from the National Center for Health Statistics (1995–1998). It reports birth outcomes to multiple births and tracks those infants up to their first year in life, and reports whether the infant is alive at age one or not. I stratify the sample by race (whites and nonwhites) and apply a twin-fixed-effect strategy to explore the effects of health at birth on infant mortality. The results are reported in Appendix Table G-1 and Appendix Table G-2 for nonwhites and whites, respectively. Focusing on the nonwhite sample, I find that an additional gram of birth weight is associated with a reduction in the probability of infant death by 0.16 basis points. Also, low birth weight increases the probability of infant death by 0.32% points, an increase of 7% from the mean of infant death among nonwhites. Using the Intention-To-Treat effects of Table 3 for low birth weight among nonwhites, a 20% higher AFDC rate (the average AFDC rate among nonwhites) leads to a 0.1% reduction (from the mean) in infant mortality rates. Although this effect seems small, it accrues over generations and years. For instance, in the absence of Medicaid, there would have been about 15,000 more deaths to infants between the years 1990–2004 as the indirect effect of Medicaid through the intergenerational transmission process.[20]

For the analysis of adulthood income, I rely on the estimations of Behrman and Rosenzweig (2004). They explore the long-run returns to birth weight and find that a 100 g additional birth weight leads to a 1.25% increase in lifetime earnings. Using the marginal effect on birth weight in Table 3 and 20% change in AFDC rates, the results suggest that next generations' lifetime earnings increase by roughly 0.45%. The American Community Survey of 2019 shows that the average personal income of nonwhites aged 25–55 was $45,504. For 25 years of

[20] Using Multiple-Cause-of-Death data from the National Center for Health Statistics, I calculate the total nonwhite death to infants as 161,985 counts.

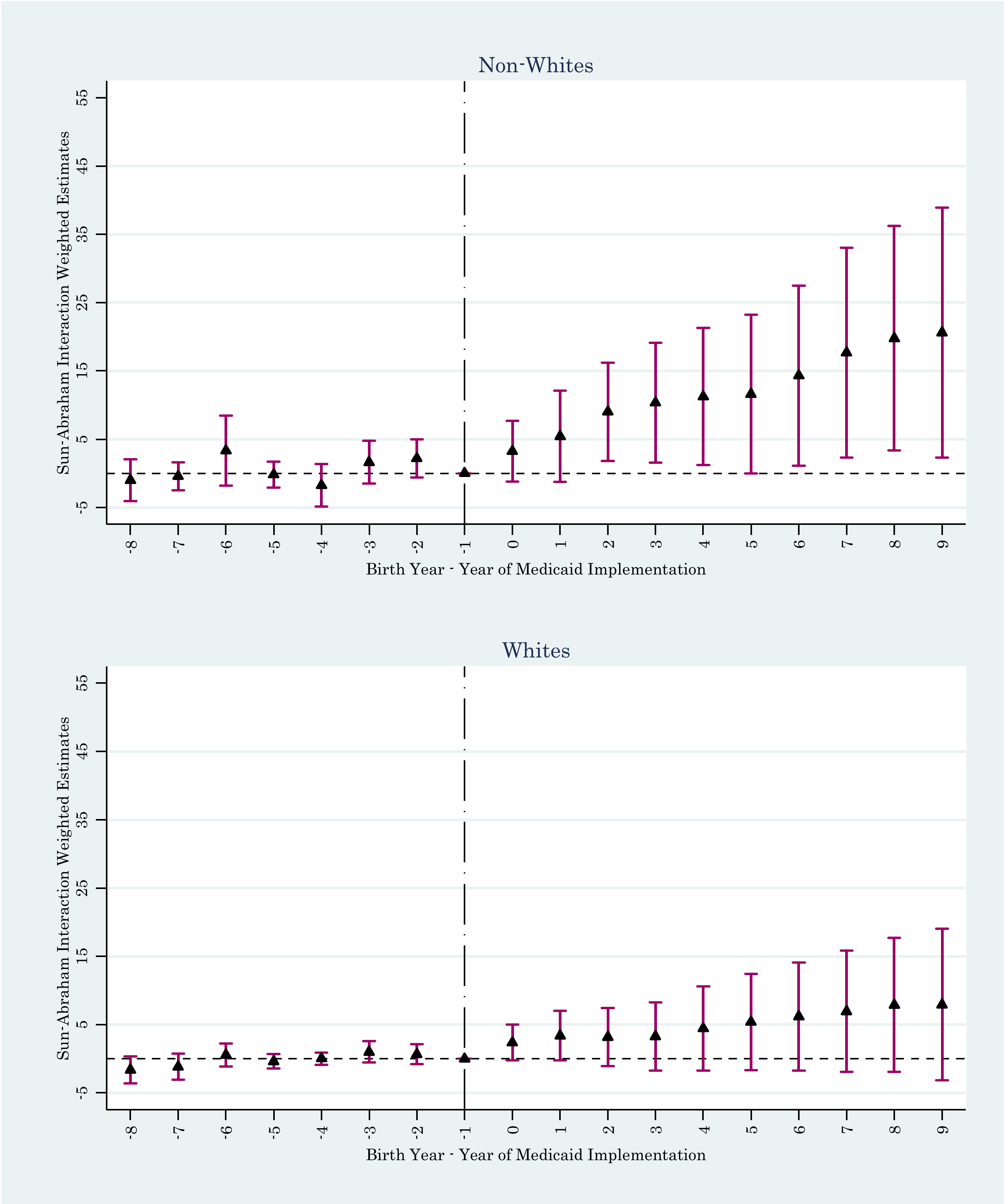

Fig. 10. Sun-Abraham Interaction Weighted Estimates with Dynamic Treatment. Notes. Each point represents the coefficient (and its 95% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is the birth weight of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. Regressions are weighted using the average of birth counts in each cell.

Table 5
Heterogeneity of the Main Results by Child's Gender among Nonwhites.

	Outcomes:									
	Child Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Full-Term Birth Weight	Small for Gestational Age	Fetal Growth	Gestational Weeks	Preterm Birth	Apgar Score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A. Girls									
Exposed Ages 0–18 × AFDC Rate	23.3291 * **	-0.0081 * **	-0.003 * **	-0.0018 * **	13.8786 * **	-0.0105 * **	0.5297 * **	0.0393	-0.0035	0.0286
	(4.0574)	(0.0013)	(0.0007)	(0.0005)	(4.8308)	(0.0026)	(0.1108)	(0.0241)	(0.0028)	(0.023)
Observations	1028	1028	1028	1028	1023	1024	1024	1024	1024	1021
R-squared	0.9406	0.8283	0.6651	0.5983	0.9009	0.7707	0.9117	0.9011	0.8801	0.7917
Mean DV	3100.676	0.118	0.023	0.012	3241.552	0.176	80.203	38.604	0.238	8.899
Pct. Effect	0.752	-6.852	-13.017	-14.912	0.428	-5.938	0.660	0.102	-1.473	0.322
	Panel B. Boys									
Exposed Ages 0–18 × AFDC Rate	14.3027 * *	-0.0058 * **	-0.0005	-0.0004	12.8517 * *	-0.0084 * **	0.4134 * **	-0.0047	0.0024	0.0429 * *
	(5.341)	(0.0014)	(0.0007)	(0.0005)	(5.0124)	(0.0021)	(0.1066)	(0.0258)	(0.0029)	(0.0201)
Observations	1032	1032	1032	1032	1028	1031	1031	1031	1031	1025
R-squared	0.9361	0.7965	0.6845	0.6341	0.9063	0.7416	0.9119	0.896	0.8612	0.7972
Mean DV	3212.687	0.098	0.022	0.012	3365.828	0.119	83.255	38.519	0.249	8.883
Pct. Effect	0.445	-5.874	-2.319	-3.487	0.382	-7.037	0.497	-0.012	0.956	0.484
State of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State of Birth Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Standard errors, reported in parentheses, are clustered at mother's state-of-birth level. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

Table 6
Heterogeneity of the Main Results by Child's Gender among Whites.

	Outcomes:									
	Child Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Full-Term Birth Weight	Small for Gestational Age	Fetal Growth	Gestational Weeks	Preterm Birth	Apgar Score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A. Girls									
Exposed Ages 0–18 × AFDC Rate	12.0656	-0.0016	0.0001	0.0001	15.6315	-0.0083	0.2716	0.0149	0.0001	-0.0418
	(18.7203)	(0.0026)	(0.0008)	(0.0004)	(17.0385)	(0.0051)	(0.4524)	(0.0746)	(0.006)	(0.0484)
Observations	1088	1088	1088	1088	1088	1088	1088	1088	1088	1088
R-squared	0.9459	0.8505	0.6275	0.5006	0.9274	0.8764	0.916	0.974	0.9671	0.9322
Mean DV	3461.900	0.046	0.008	0.004	3558.048	0.065	88.254	39.204	0.157	8.979
Pct. Effect	0.349	-3.562	-0.015	3.567	0.439	-12.713	0.308	0.038	-0.026	-0.465
	Panel B. Boys									
Exposed Ages 0–18 × AFDC Rate	21.6866	-0.0039	-0.0004	-0.0003	21.618	-0.0131	0.5627	-0.0043	-0.0047	-0.0399
	(18.607)	(0.003)	(0.0007)	(0.0004)	(19.234)	(0.0082)	(0.4933)	(0.0749)	(0.0062)	(0.0469)
Observations	1088	1088	1088	1088	1088	1088	1088	1088	1088	1088
R-squared	0.932	0.8686	0.6073	0.4483	0.913	0.9001	0.9057	0.97	0.9607	0.9413
Mean DV	3340.599	0.053	0.008	0.004	3419.643	0.103	84.895	39.342	0.139	9.007
Pct. Effect	0.649	-7.349	-4.764	-7.748	0.632	-12.756	0.663	-0.011	-3.375	-0.443
State of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State of Birth Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Standard errors, reported in parentheses, are clustered at mother's state-of-birth level. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

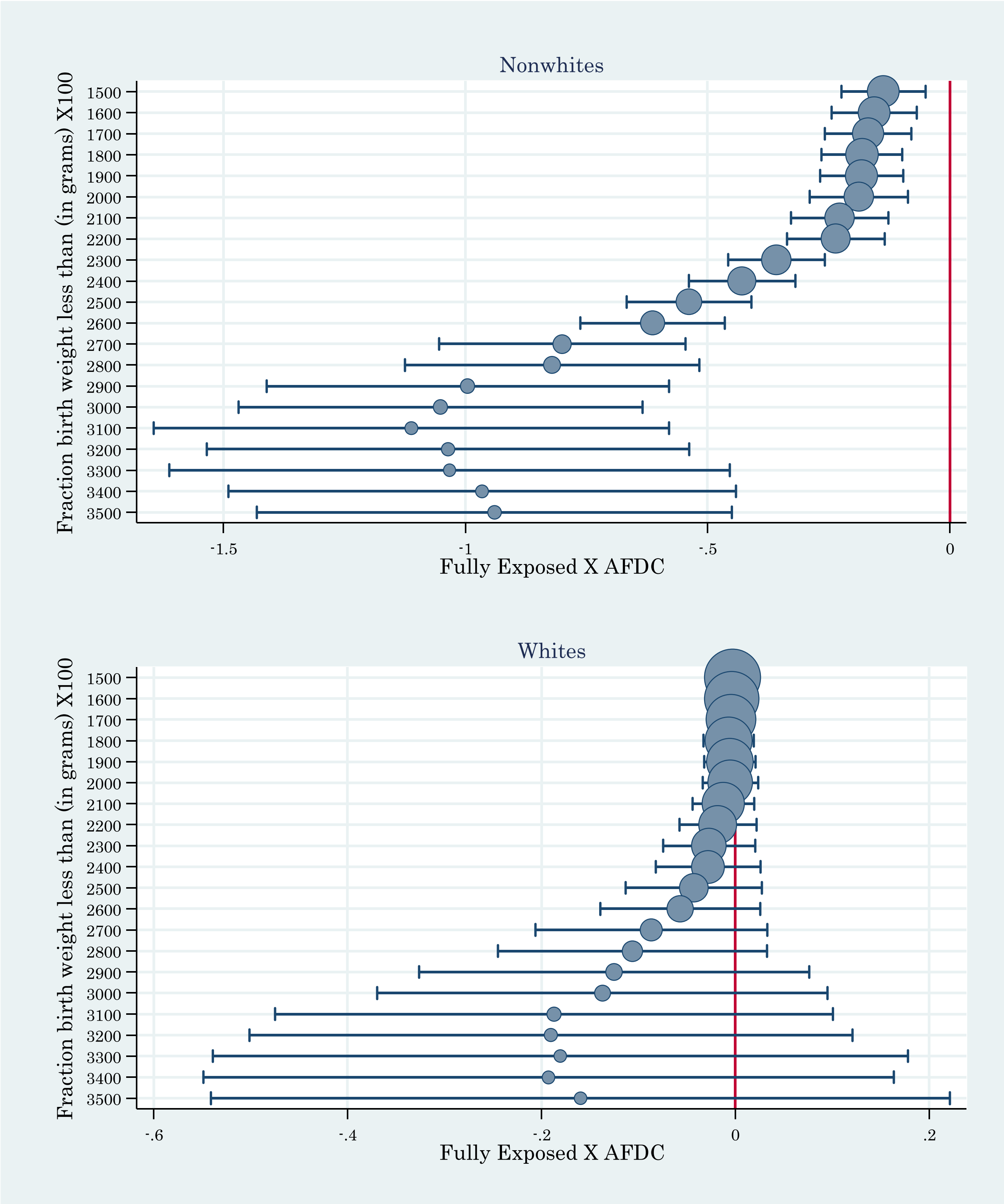

Fig. 11. Intergenerational Health Effects of Medicaid for Low Birth Weight across Different Thresholds of Low Birth Weight. Notes. Each point represents the coefficient (and its 90% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is the birth weight of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.

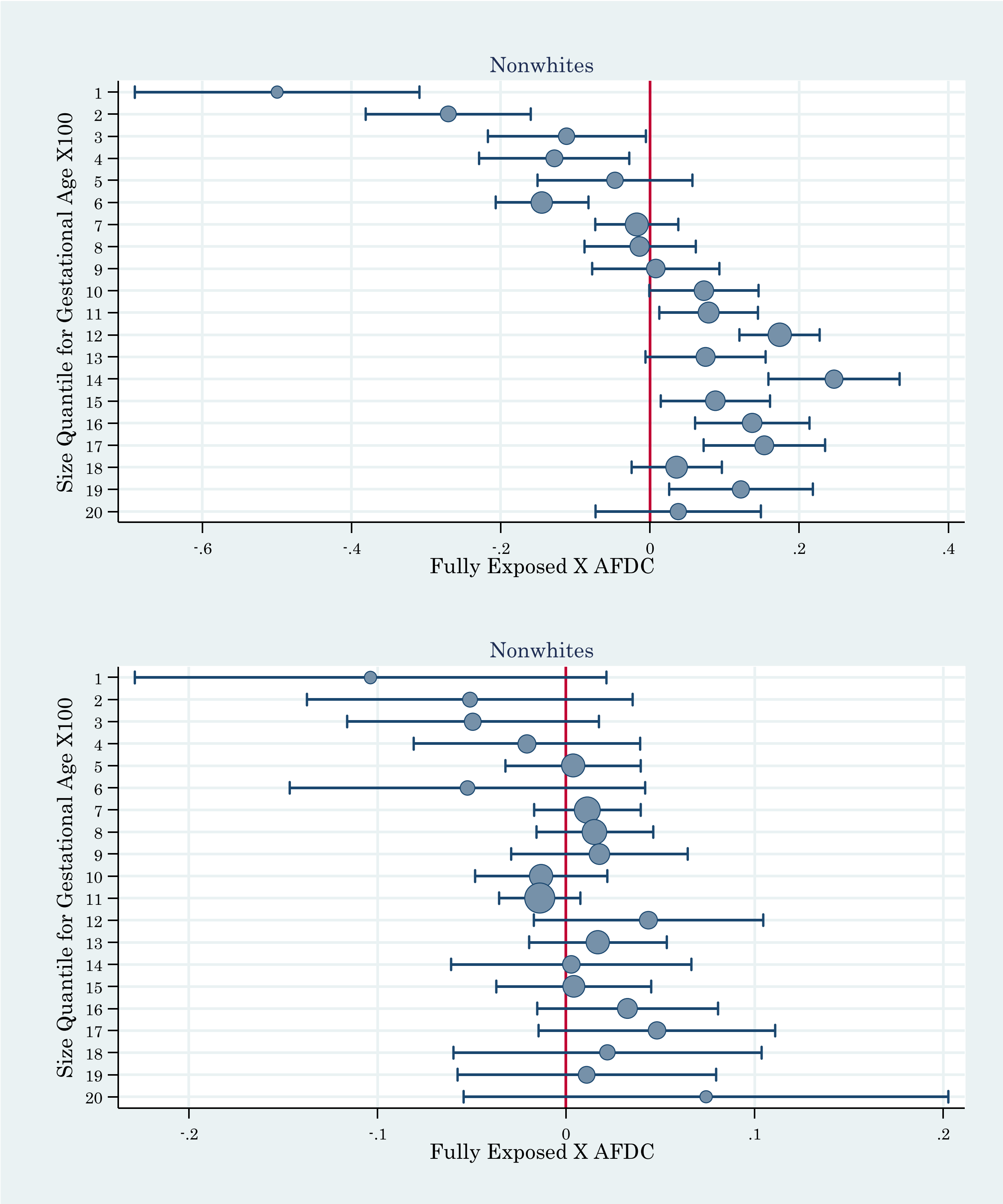

Fig. 12. Intergenerational Health Effects of Medicaid for Birth Size Quantile for Gestational Week across Different Quantiles. Notes. Each point represents the coefficient (and its 90% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is the birth weight of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.

employment, the intergenerational health effect of Medicaid can be translated into an increase of $5119 per nonwhite individual.[21]

From 1966–1976, the average annual cost of Medicaid was $6.37 billion in 2019 dollars. Unrealistically, I assume that all the Medicaid expenditure was spent on nonwhite children and that fully eligible mothers had only one child in their lifetime.[22] Following the national labor force statistics, I assume that 57% of females and 70% of males born to those mothers participate in the labor force in adulthood. I also assume a 3% discount rate. A simple back-of-an-envelope cost-benefit analysis suggests a minimum of 3.9% return through marginal increases in income of next generations as a result of improvements in birth weight.[23]

8. Conclusion

This paper explores the effect of Medicaid introduction during the 1960 s on birth outcomes of infants whose mothers were exposed to public health insurance during in utero and childhood. The results are the first to show that the introduction of Medicaid had intergenerational health benefits. Birth outcomes of mothers who were eligible for Medicaid in ages 0–18 reveal sizeable improvements. The effects are more pronounced for adverse birth outcomes such as low birth weight and small for gestational age. The program's intergenerational benefits are much larger among nonwhites who were overrepresented in the target population. The intergenerational links are also larger among girls compared with boys. The effects do not appear to be driven by other welfare spending changes, specifically the War on Poverty, changes in cohorts' socioeconomic status, and selective fertility.

Finally, I discuss the implications of birth outcomes in two aspects of later-life outcomes: infant mortality and adulthood lifetime earnings. A back-of-an-envelope calculation points to a minimum of 3.9% social externality of Medicaid through income rises as a result of next generations' improvements in birth outcomes.

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Conflict of interest

The author claims that he has no conflict of interest to report

Appendix A

This appendix explores additional endogeneity concerns. Medicaid could have been accompanied by other state welfare programs, specifically the War on Poverty initiated during the same timeframe. As these programs also have the potential and are shown to affect birth outcomes,[24] the results could be confounded by these welfare programs rather than the introduction of public health insurance. In Appendix Tables A-1, I examine this source of endogeneity by using a series of welfare spending (in logarithm and shown in columns) as the outcome of Eq. (2) for nonwhites (panel A) and whites (panel B) separately. There is no statistical evidence that Medicaid-eligible cohorts in high-welfare states were exposed to additional welfare spending for other programs such as AFDC, retirement benefits, military health benefits, public assistance, supplemental security income, unemployment insurance benefits, and spending on Food Stamp. The Medicaid exposure is significantly related to increases in Medicaid spending among blacks. Among nonwhites fully eligible for Medicaid to those ineligible, an 8%

Table A-1
Endogeneity of Medicaid Eligibility to other Welfare Programs.

	Outcomes in Logarithm:								
	AFDC Spending	Retirement Benefits	Military Health Spending	Public Assistance	Supplemental Security Income	General Assistance	Unemployment Insurance	Food Stamp Program	Medicaid Spending
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A. Nonwhites								
Born After Medicaid × AFDC Rate	-0.0027	0.0003	-0.0076	-0.0261	-0.0227	-0.0104	0.0018	0.0342	0.0552 * *
	(0.0029)	(0.0005)	(0.0111)	(0.0649)	(0.0297)	(0.0164)	(0.0026)	(0.1025)	(0.0255)
Observations	980	980	980	980	980	980	980	980	980
R-squared	0.9998	0.9864	0.7935	0.8477	0.7866	0.8120	0.9472	0.8493	0.8727
	Panel B. Whites								
Born After Medicaid × AFDC Rate	0.0115	0.0029	-0.0078	-0.0062	0.1704	0.0081	0.0306	0.1882	0.167
	(0.0133)	(0.0039)	(0.0398)	(0.4869)	(0.1766)	(0.0242)	(0.0287)	(0.4584)	(0.1394)
Observations	980	980	980	980	980	980	980	980	980
R-squared	0.9998	0.9857	0.8315	0.8330	0.7855	0.8520	0.9420	0.8481	0.8643

Notes. The state-by-year panel covers the years 1959–1978. Standard errors, reported in parentheses, are clustered at the state level. Regressions include year fixed effects, state fixed effects, and a state trend. Regressions are weighted using the average of birth counts in each cell. Lack of data availability prior to 1959 restricts the sample compared to the main sample.
$* ** p < 0.01$, $* * p < 0.05$, $* p < 0.1$

[21] I am assuming that over-age increases in income can be roughly captured by this across-age mean value. I also assume that future discount rate can be offset by wage raises.
[22] First-time children among nonwhites between the years 1990–2004 add up to 3,823,382 counts, 1,953,462 boys and 1,869,920 girls.
[23] I add up total benefits as the lifetime increase in earnings times the total first-time nonwhite children born between the years 1990–2004, a period that all mothers in the sample have been fully eligible. I compute the cost as the sum of Medicaid expenditure over the years 1966–1976.

[24] See, for instance, Almond et al. (2011), Amarante et al. (2016), and Hoynes et al. (2011).

Table A-2
Endogeneity of Categorical Eligibility to Pre-Medicaid Trends in Households' Socioeconomic Characteristics.

	Outcomes for Household Heads:								
	Wage and Salary Income	Is Employed	Is Active in Labor Force	Education: High School Graduate	Education: Some College	Education: Bachelor-and-above	Home Ownership	House Value	Owning Kitchen
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A. Nonwhites								
Year 1940 × AFDC Rate	–	-0.0018	-0.0019	–	–	–	-0.0008	-62.781	-0.0005
		(0.0012)	(0.0010)				(0.0010)	(88.573)	(0.0005)
Year 1950 × AFDC Rate	9.703	-0.0002	-0.0011	-0.0016	-0.0005	-0.0002	0.0015	–	-0.0008
	(8.580)	(0.0012)	(0.0009)	(0.0010)	(0.0004)	(0.0002)	(0.0026)		(0.0006)
Year 1960 × AFDC Rate	32.642 * *	0.0020	0.0005	0.0026 *	0.0006	0.0001	-0.0018	54.419	-0.0006
	(15.070)	(0.0015)	(0.0011)	(0.0016)	(0.0006)	(0.0002)	(0.0016)	(98.285)	(0.0006)
Year 1970 × AFDC Rate	28.871	0.0015	-0.0001	0.0029	0.0009	0.0002	-0.0030	90.169	0.0059 **
	(18.243)	(0.0014)	(0.0010)	(0.0018)	(0.0007)	(0.0003)	(0.0019)	(163.125)	(0.0027)
R-Squared	0.969	0.766	0.835	0.957	0.907	0.896	0.928	0.932	0.987
Observations	192	244	244	194	194	194	244	198	244
	Panel B. Whites								
Year 1940 × AFDC Rate	–	-0.0042 *	-0.0007	–	–	–	-0.0009	72.380	0.0001
		(0.0024)	(0.0022)				(0.0050)	(62.741)	(0.0001)
Year 1950 × AFDC Rate	-25.337	-0.0014	-0.0014	-0.0019	-0.0016	-0.0014	0.0120	–	-0.0001
	(21.378)	(0.0047)	(0.0044)	(0.0074)	(0.0032)	(0.0017)	(0.0092)		(0.0001)
Year 1960 × AFDC Rate	17.945	0.0006	-0.0008	-0.0017	0.0028	0.0005	-0.0014	254.272	-0.0004**
	(82.832)	(0.0091)	(0.0073)	(0.0035)	(0.0045)	(0.0018)	(0.0042)	(424.988)	(0.0002)
Year 1970 × AFDC Rate	-25.605	-0.0019	-0.0033	-0.0030	0.0042	0.0006	-0.0049	637.060	-0.0011
	(133.740)	(0.0074)	(0.0064)	(0.0046)	(0.0070)	(0.0032)	(0.0067)	(714.957)	(0.0031)
R-Squared	0.985	0.772	0.749	0.769	0.970	0.966	0.986	0.972	0.999
Observations	192	244	244	194	194	194	244	198	244

Notes. Standard errors, reported in parentheses, are clustered at the state level. Regressions include state fixed effects and year fixed effects. Regressions are weighted using the sum of IPUMS-Census weights in each state-year. The reference year in columns 1, 4, 5, and 6 is 1930. The reference group of the rest of the columns is 1940 since the outcomes are not reported in the 1930 census. The house value is not reported for the 1950 census. The sample includes census years 1930, 1940, 1950, 1960, and 1970. The sample is restricted to household heads with at least one child and collapsed at the state-year level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

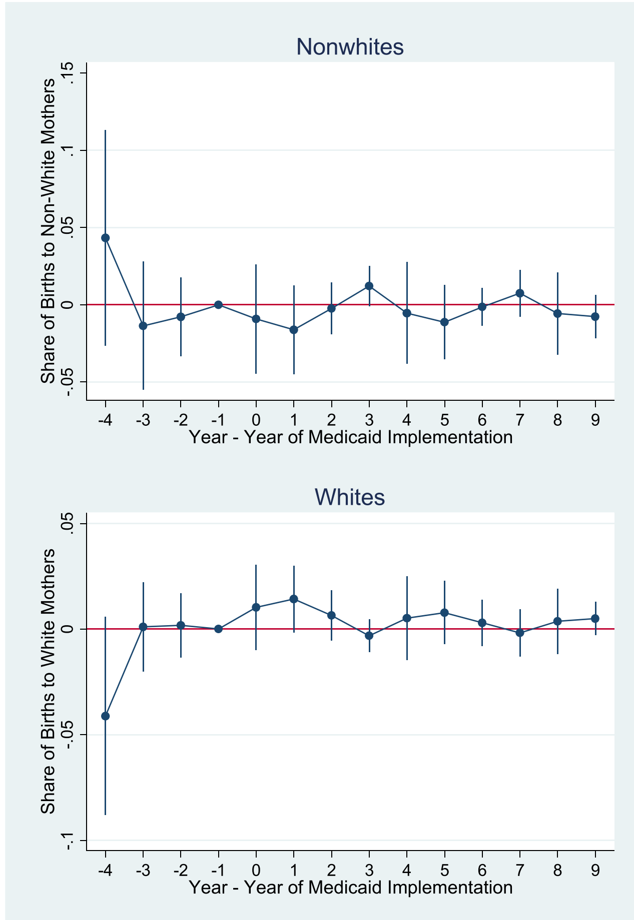

Fig. A-1. Medicaid Implementation and Endogenous Fertility. Notes. Each point represents the coefficient (and its 95% confidence interval) of the respective event-time dummy that measures the relative distance of each observation to Medicaid implementation. The outcomes (in the y-axis) are the share of births to white-nonwhite mothers. The observations are at the state-by-year level and cover the years 1963–1976. All regressions include state fixed effects, year fixed effects, as well as state of birth controls, including per capita income, per capita hospital, and per capita hospital beds. All Regressions are weighted using the average of birth counts in each cell. Standard errors are clustered at the state level.

rise in AFDC rates (the standard deviation of AFDC rates among nonwhites) is associated with a 55% rise in Medicaid spending. A one-standard-deviation change in AFDC rates (roughly 1.6%) among eligible compared to ineligible whites is associated with a 25.6% rise in Medicaid spending. However, the coefficient is imprecisely estimated.

Another source of endogeneity is the cross-cohort (cohorts in high- versus low-welfare states) trends in childhood circumstances specifically families' socioeconomic characteristics, that could influence birth outcomes of mothers and, in observable and unobservable ways, affect the health of their grandchildren. I explore this confounding trend by regressing a series of family characteristics on the interaction between year dummies and AFDC rates using a state-year panel of census data from 1930 to 1970. In so doing, I use household heads' characteristics that have at least one child in the household and are aged 25–65. I then collapse this sample at the state-year level and merge it with Medicaid data. The results are reported in Appendix Tables A-2. There is no statistical evidence of pre-existing trends in households' socioeconomic characteristics, including wage, employment, education, home-ownership, house value, and house facilities. Also, the point estimates are quite small, suggesting no divergence across cohorts living in states with high and low AFDC rates.

To complement selective fertility analysis in section 6.1, I report an event-study analysis to search for differential fertility timing among whites and nonwhites. In so doing, I monitor the state-year level share of births to nonwhites and whites in the years prior and following the Medicaid implementation. The results of this event-study estimation are reported in Appendix Figure A-1. All point estimates are economically small and statistically insignificant, which rule out the possibility of endogenous fertility.

Appendix B

This appendix shows the event study estimates for other health outcomes. The results are illustrated in Appendix Figs B-1 to B-6.

Fig. B-1. Event-Study Results of Medicaid Eligibility on Next Generation Very Low Birth Weight. Notes. Each point represents the coefficient (and its 90% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is very low birth weight of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.

Fig. B-2. Event-Study Results of Medicaid Eligibility on Next Generation Extremely Low Birth Weight. Notes. Each point represents the coefficient (and its 90% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is extremely low birth weight of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.

Fig. B-3. Event-Study Results of Medicaid Eligibility on Next Generation Full-Term Birth Weight. Notes. Each point represents the coefficient (and its 90% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is full-term birth weight of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.

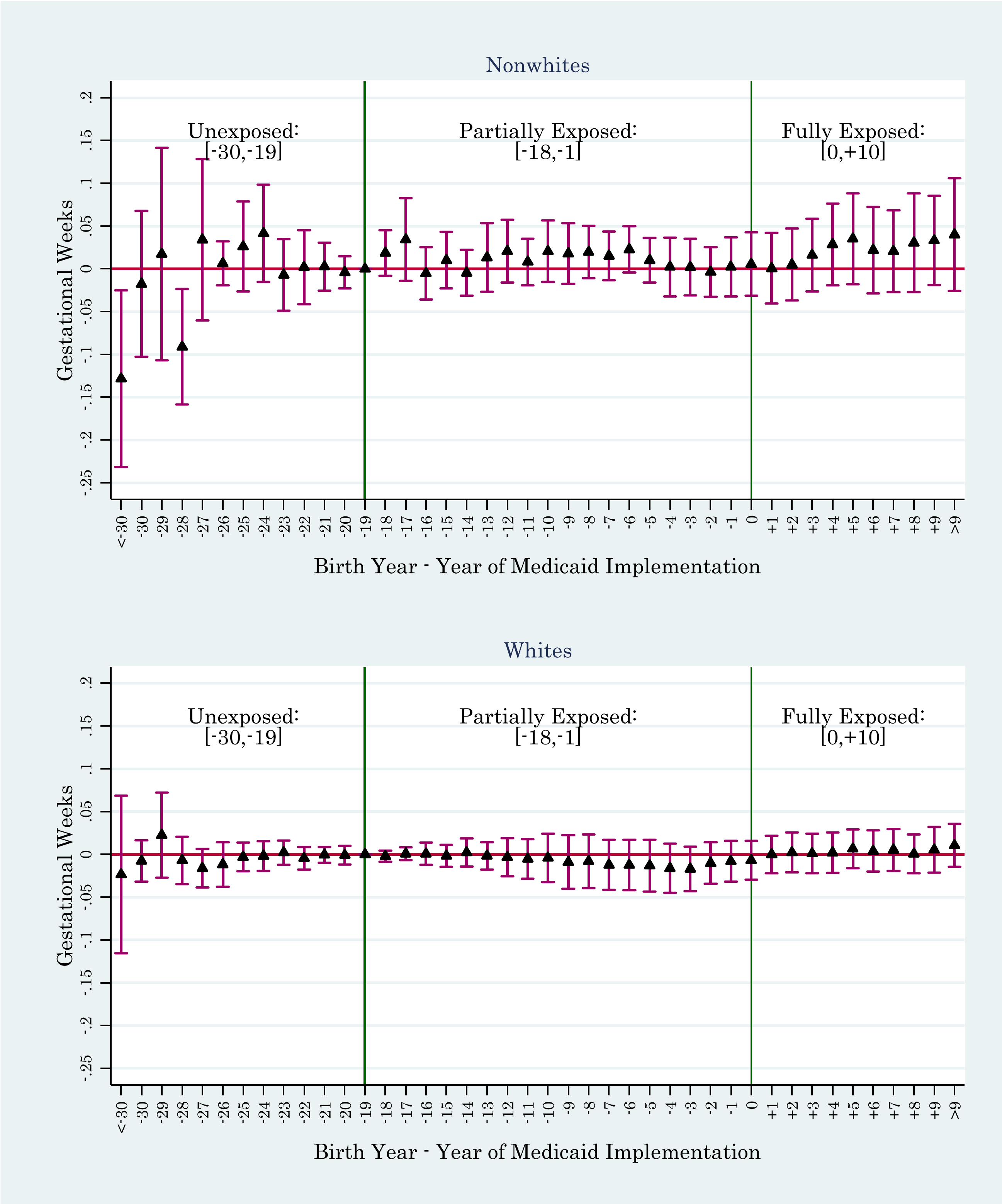

Fig. B-4. Event-Study Results of Medicaid Eligibility on Next Generation Gestational Age. Notes. Each point represents the coefficient (and its 90% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is gestational week of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.

Fig. B-5. Event-Study Results of Medicaid Eligibility on Next Generation Preterm Birth. Notes. Each point represents the coefficient (and its 90% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is preterm birth of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.

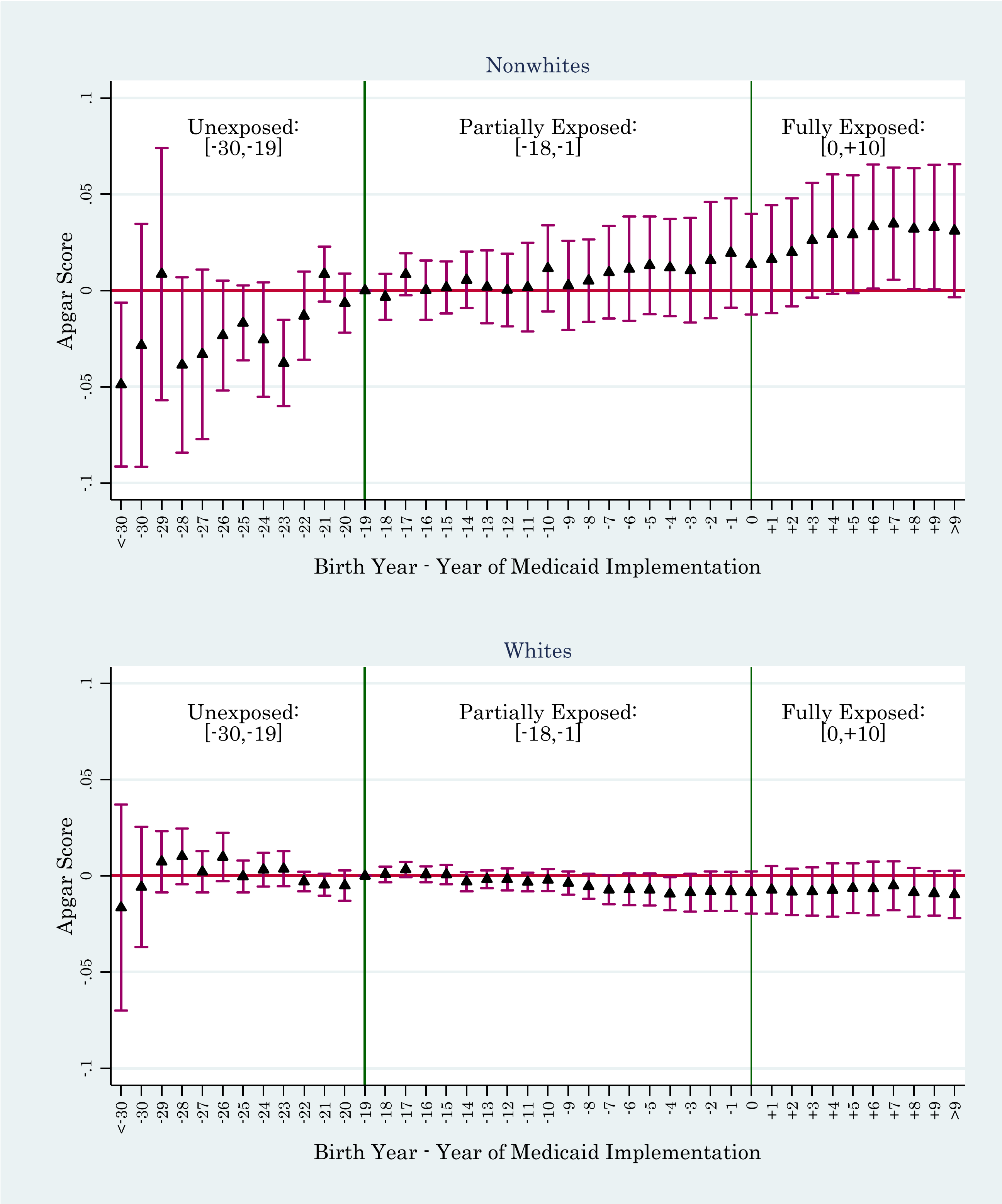

Fig. B-6. Event-Study Results of Medicaid Eligibility on Next Generation Apgar Score. Notes. Each point represents the coefficient (and its 90% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is Apgar score of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.

Appendix C

This appendix explores some mechanisms of impact. First, I investigate the role of Medicaid introduction on prenatal use at the time of mothers' birth. Since the Natality files start reporting prenatal care use from 1971 and mothers' birth cohorts in the main sample are limited to 1976, I am left with only six data years. The primary limitation of such analysis is that I need to only rely on cross-sectional analysis as every cohort in the data has received the treatment (Medicaid implementation year in the main sample varies between the years 1966–1970). Therefore, I cannot include the birth-place fixed effect. However, I try to control for confounders by including census-region-by-cohort fixed effects and additional covariates explained in the text. In the analysis, I regress two outcomes related to prenatal use: the number of prenatal visits and a dummy that equals one if the mother had any prenatal visits and zero otherwise. The primary independent variable is AFDC rate, as I cannot include exposure into the models (all cohorts are exposed). The results are reported in Appendix Table C-1. A 10% point rise in categorical eligibility is associated with 2.2 more visits among nonwhites and 1.2 more visits among whites, equivalent to a 26.5% and 11.3% rise from the mean of prenatal visits.

Table C-1
Medicaid Introduction and Prenatal Care.

	Subsamples and Outcomes:			
	Nonwhites		Whites	
	Prenatal Visits (1)	Any Prenatal Visit (2)	Prenatal Visits (3)	Any Prenatal Visit (4)
AFDC Rate	0.2208 * *	0.0034	0.1166 * *	0.0008
	(0.0943)	(0.0053)	(0.0504)	(0.0009)
Observations	235	235	235	235
R-squared	0.3419	0.1287	0.2857	0.1488
Mean DV	8.321	0.959	10.260	0.992
Pct. Effect	2.654	0.358	1.137	0.085

Notes. Standard errors, reported in parentheses, are clustered at mother's state-of-birth level. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

Second, I explore the effects on later-life labor market outcomes. Since the Natality data does not ask for information on income and sources of income, I use census data (1970–2000) and American Community Survey data (2001–2005) to explore the later-life earnings effect of Medicaid. I follow the same sample selection strategy as explained in section 4 and implement the difference-in-difference strategy of Eq. (2). I convert the dollar values to 2000 dollars. I explore the effects of three measures of income: personal income, family income, and social security income. As a robustness practice, I also show the effects on the log of these outcomes.

The results are reported in Appendix Table C-2 for nonwhites (panel A) and whites (panel B), separately. Among both groups, higher exposure to Medicaid was associated with higher personal income, improved family income, and lower receipt of social security income. The effects are considerably larger among nonwhites. Using the race-group-specific standard deviation of AFDC rates and comparing fully eligible to ineligible cohorts, 12.2% and 0.69% increase in personal income among nonwhites and whites, respectively. The same set of variations in AFDC rates also leads to a 0.47% and 0.29% reduction in social security income among nonwhites and whites, respectively, though the marginal effects are imprecisely estimated.

Appendix D

In the main text, I restrict the sample to first-time mothers. Appendix Tables D-1 shows that the results are robust to this sample selection criteria.

Table C-2
Mechanism Channel: Adulthood Income Profile of Mothers Exposed to Medicaid during Childhood.

	Outcomes:					
	Personal Income (1)	Log Personal Income (2)	Family Income (3)	Log Family Income (4)	Social Security Income (5)	Log Social Security Income (6)
	Panel A. Nonwhites					
Exposed Ages 0–18 × AFDC Rate	998.6883 * *	0.0155 *	5520.9567 * *	0.0482 * **	-4.3194	-0.0006
	(380.0726)	(0.0085)	(2235.291)	(0.0171)	(3.5679)	(0.0048)
Observations	1801	1801	1801	1801	1801	1801
R-squared	0.9753	0.9849	0.9347	0.9471	0.7018	0.6961
Mean DV	17723.0729	9.1197	80671.5628	10.8628	144.9927	0.1614
Pct. Effect	5.6349	0.1694	6.8437	0.4436	-2.9790	-0.3824
	Panel B. Whites					
Exposed Ages 0–18 × AFDC Rate	139.6180 * *	0.0043	493.7555 * **	0.0082 * **	-0.9284	-0.0018
	(57.8566)	(0.00277)	(157.4406)	(0.0022)	(2.4369)	(0.0021)
Observations	1801	1801	1801	1801	1801	1801
R-squared	0.9284	0.9322	0.9293	0.9127	0.5201	0.5897
Mean DV	24638.3214	9.6927	52740.6621	10.4054	202.8639	0.2343
Pct. Effect	0.5666	0.0450	0.9361	0.0797	-0.4576	-0.7940
State of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
State of Birth Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Standard errors, reported in parentheses, are clustered at the state-of-birth level. State controls include hospitals per capita, hospital beds per capita, and per capita income. Regressions are weighted using the average of birth counts in each cell. Census 1990 and 2000 are pooled with American Community Survey 2005–2019 and collapsed at the state-of-birth-year-of-birth level for two race categories of whites and nonwhites.
** ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$*

Table D-1
Intergenerational Health Effects of Medicaid Implementation for Birth Outcomes: Sensitivity of Results to Including All Births (Rather than only first-time mothers).

	Outcomes:									
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Full-Term Birth Weight	Small for Gestational Age	Fetal Growth	Gestational Weeks	Preterm Birth	Apgar Score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A. Nonwhites									
Exposed Ages 0–18 × AFDC Rate	18.5937 * **	-0.0076 * **	-0.0014 * *	-0.0004	14.9199 * **	-0.0092 * **	0.4731 * **	0.0108	0.0009	0.0383 * *
	(5.3214)	(0.0014)	(0.0006)	(0.0004)	(5.4212)	(0.0023)	(0.1024)	(0.0297)	(0.0033)	(0.0181)
Observations	1121	1121	1121	1121	1117	1118	1118	1118	1118	1108
R-squared	0.9538	0.8693	0.7495	0.6983	0.9264	0.8222	0.9343	0.9302	0.9233	0.8335
Mean DV	3127.850	0.117	0.025	0.013	3283.711	0.156	81.012	38.513	0.247	8.876
Pct. Effect	0.594	-6.490	-5.472	-3.455	0.454	-5.887	0.584	0.028	0.349	0.432
	Panel B. Whites									
Exposed Ages 0–18 × AFDC Rate	11.9768	-0.0003	0.0001	0.0003	15.6172	-0.0109	0.2951	-0.0085	0.0008	-0.0378
	(23.0345)	(0.003)	(0.0008)	(0.0005)	(23.9569)	(0.0096)	(0.5898)	(0.0764)	(0.0059)	(0.0509)
Observations	1248	1248	1248	1248	1248	1248	1248	1248	1248	1245
R-squared	0.9568	0.9005	0.7894	0.6886	0.9479	0.9208	0.9406	0.9797	0.978	0.9246
Mean DV	3368.819	0.061	0.010	0.005	3471.778	0.091	85.832	39.203	0.159	8.975
Pct. Effect	0.356	-0.513	0.202	5.403	0.450	-11.962	0.344	-0.022	0.491	-0.421
State of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State of Birth Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Standard errors, reported in parentheses, are clustered at mother's state-of-birth level. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

Appendix E

In this appendix, I focus on the subsample of black mothers. As they constitute a higher share of AFDC recipients and welfare benefit recipients, I expect to observe higher effects for this subsample. The results of the difference-in-difference identification strategy, reported in Appendix Table E-1, confirm this fact.

Appendix F

This appendix shows that the results are robust to other recently developed difference-in-difference innovations. Appendix Fig. F-1 shows the event-study results using the dynamic estimates of de Chaisemartin and d'Haultfoeuille (2020). The results of bacon-decomposition, reported in Appendix Fig. F-2, are also robust across 2-by-2 DD comparisons.

Table E-1
Intergenerational Health Effects of Medicaid Implementation for Birth Outcomes: Heterogeneity By Race.

	Outcomes:									
	Birth Weight	Low Birth Weight	Very Low Birth Weight	Extremely Low Birth Weight	Full-Term Birth Weight	Small for Gestational Age	Fetal Growth	Gestational Weeks	Preterm Birth	Apgar Score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A. Blacks									
Exposed Ages 0–18 × AFDC Rate	24.8783 * **	-0.0085 * **	-0.0019 * **	-0.0011 * *	18.7087 * **	-0.012 * **	0.6234 * **	0.022	-0.0003	0.038 *
	(4.501)	(0.0011)	(0.0007)	(0.0005)	(5.1957)	(0.0023)	(0.1081)	(0.0245)	(0.003)	(0.0218)
Observations	976	976	976	976	971	974	974	974	974	969
R-squared	0.9242	0.7184	0.6365	0.6039	0.8858	0.622	0.8924	0.9284	0.9123	0.8929
Mean DV	3141.051	0.112	0.023	0.012	3291.390	0.151	81.404	38.520	0.249	8.887
Pct. Effect	0.792	-7.605	-8.323	-9.406	0.568	-7.954	0.766	0.057	-0.116	0.428
Panel B. Other Races										
Exposed Ages 0–18 × AFDC Rate	-15.9403	0.0051 *	0.0019	0.0004	-6.6704	0.0033	-0.3204	0.0198	0.0001	-0.0127
	(10.5)	(0.0029)	(0.0012)	(0.0008)	(13.3959)	(0.0052)	(0.3177)	(0.0322)	(0.0038)	(0.0256)
Observations	966	966	966	966	958	962	962	962	962	950
R-squared	0.787	0.3764	0.2485	0.2266	0.7692	0.4643	0.7448	0.5929	0.3743	0.386
Mean DV	3373.017	0.056	0.009	0.004	3464.793	0.091	86.298	39.089	0.178	8.941
Pct. Effect	-0.473	9.133	20.978	9.096	-0.193	3.578	-0.371	0.051	0.065	-0.142
State of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State of Birth Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Standard errors, reported in parentheses, are clustered at mother's state-of-birth level. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-year fixed effects. In addition, regressions include average state-Medicaid-year-level transfer receipt from food stamp, income per capita, and hospital per capita interacted with a linear trend in mother's birth year. Regressions also include mother's state-cohort controls, including average female, average male socioeconomic status, average number of homeowners, average literacy rate, and average female labor force participation. All models also control for the mother's marital status and an indicator for whether the mother had any prenatal visit. Regressions are weighted using the average of birth counts in each cell.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

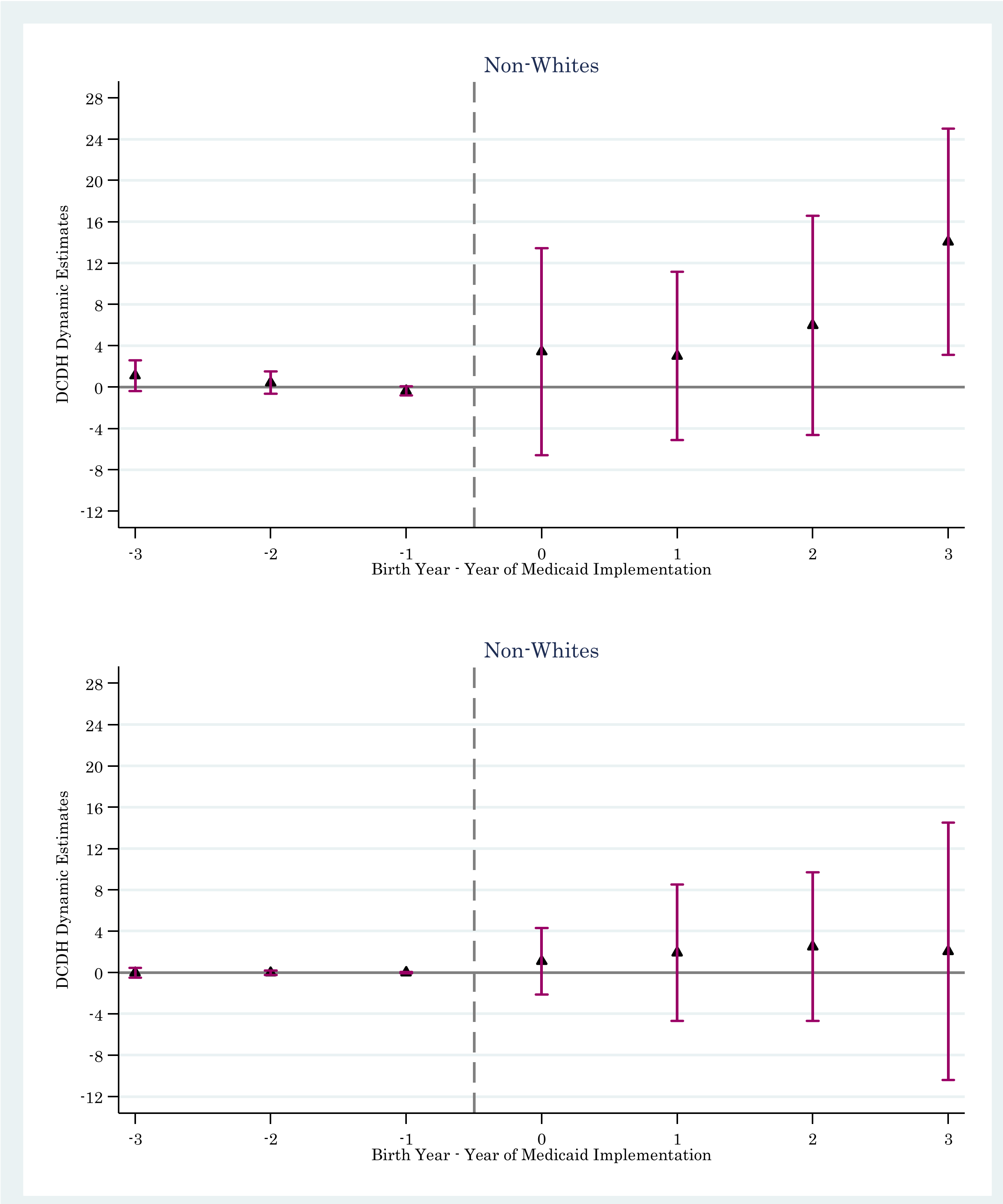

Fig. F-1. Event Study of de Chaisemartin and D'Haultfoeuille Dynamic Treatment DD Estimates. Notes. Each point represents the coefficient (and its 95% confidence interval) on the interaction term of AFDC rate (divided by its SD) and its respective event-time dummy. The event-time dummies capture the relative exposure of mothers to Medicaid implementation. The outcome is the birth weight of their children when they enter the maternity ward. Regressions include mother's birth cohort fixed effects, mother's state of birth fixed effects, and mother's cohort-by-Medicaid-Year fixed effects. Regressions are weighted using the average of birth counts in each cell.

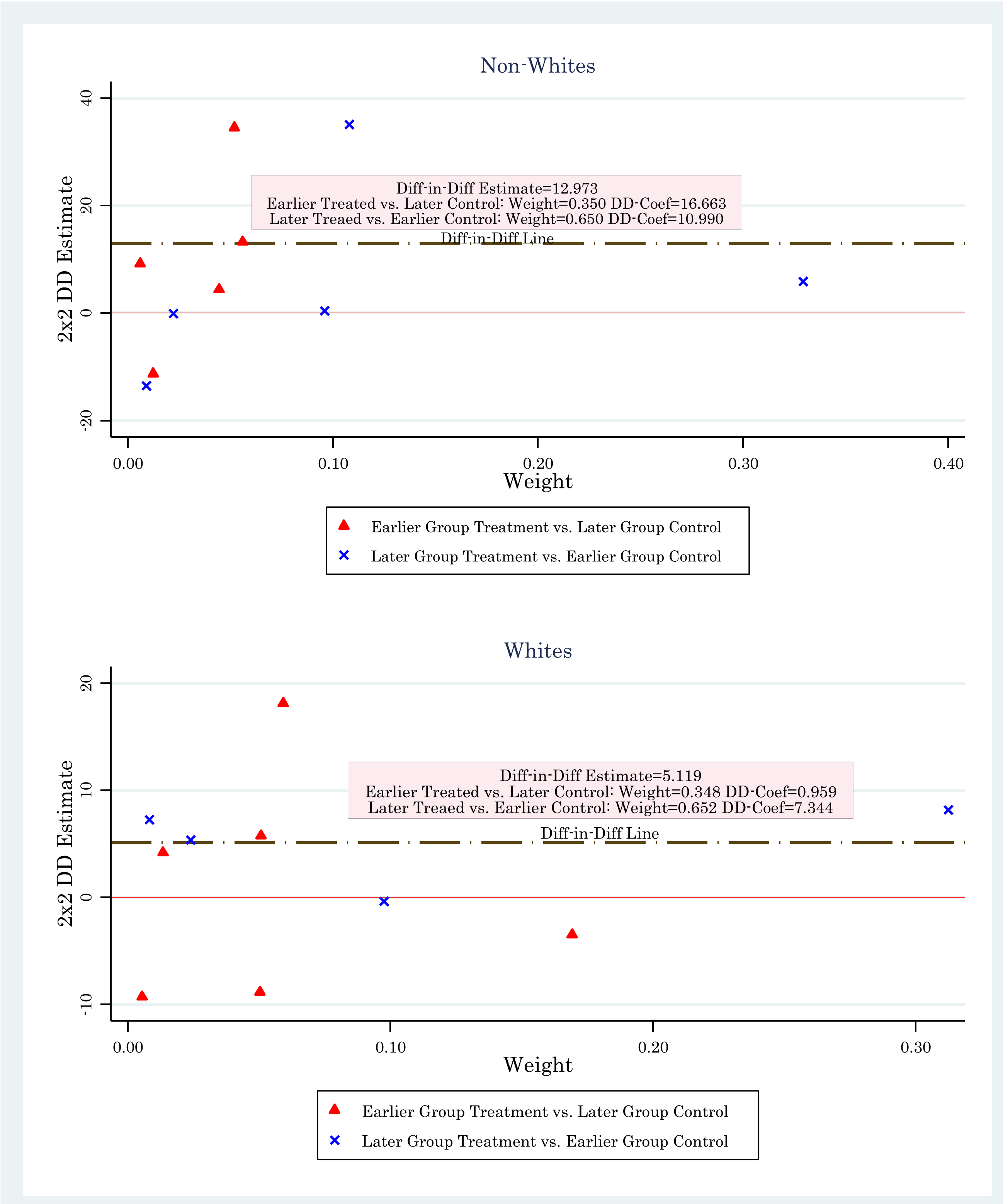

Fig. F-2. Bacon Decomposition for 2-by-2 Difference-in-Difference Estimations.

Appendix G

This appendix uses Matched Multiple Birth data (1995–1998) to explore the effect of birth outcomes on the probability of incidence of infant mortality. The regressions implement a twin fixed-effect strategy that includes a twin dummy in all regressions. I cluster the standard errors at the twin-pair level. The results are reported in Appendix Table G-1 and Appendix Table G-2 for nonwhites and whites, respectively.

Table G-1
The Twin-FE Estimates of Birth Outcomes and Infant Mortality (Death in less than One Year) among Nonwhites.

	Outcome: Infant Death (× 100)					
	(1)	(2)	(3)	(4)	(5)	(6)
Child Weight	-0.00164*** (0.00002)					
Low Birth Weight		0.31966** (0.15884)				
Very Low Birth Weight			2.413*** (0.561)			
Fetal growth				-0.0662*** (0.0099)		
Gestational Weeks					-0.1187 (0.17365)	
Preterm Birth						-1.1280 (0.9220)
R^2	0.77	0.77	0.77	0.78	0.78	0.77
Observations	87,042	87,042	87,042	87,042	87,042	87,042
Mean DV	4.55	4.55	4.55	4.55	4.55	4.55

Notes. All regressions include twin fixed effects and infant sex dummies. Standard errors, reported in parentheses, are clustered on twin pairs. The data include all twin births in the US between the years 1995–1998.

Table G-2
The Twin-FE Estimates of Birth Outcomes and Infant Mortality (Death in less than One Year) among Whites.

	Outcome: Infant Death (× 100)					
	(1)	(2)	(3)	(4)	(5)	(6)
Child Weight	-0.00164*** (0.00001)					
Low Birth Weight		0.5157** (0.0619)				
Very Low Birth Weight			3.0206*** (0.3151)			
Fetal growth				-0.0624*** (0.0041)		
Gestational Weeks					-0.1405 (0.9111)	
Preterm Birth						-0.3426 (0.4469)
R^2	0.75	0.75	0.75	0.75	0.75	0.75
Observations	324,978	324,978	324,978	324,978	324,978	324,978
Mean DV	2.52	2.52	2.52	2.52	2.52	2.52

Notes. All regressions include twin fixed effects and infant sex dummies. Standard errors, reported in parentheses, are clustered on twin pairs. The data include all twin births in the US between the years 1995–1998.

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