THE LONG-RUN EFFECTS OF CHILDHOOD INSURANCE COVERAGE: MEDICAID IMPLEMENTATION, ADULT HEALTH, AND LABOR MARKET OUTCOMES

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Abstract:

This paper estimates the long-run effects of childhood Medicaid eligibility on adult health and economic outcomes using the program's original introduction (1966-1970) and its mandated coverage of welfare recipients. The design compares cohorts born in different years relative to Medicaid implementation, in states with different pre-existing welfare-based eligibility. Early childhood Medicaid eligibility reduces mortality and disability, increases employment, and reduces receipt of disability transfer programs up to 50 years later. Medicaid has saved the government more than its original cost and saved more than 10 million quality adjusted life-years.

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In 2015, the joint federal and state public health insurance programs, Medicaid and the Children's Health Insurance Program, covered 40 percent of children at a cost of $90 billion. Costs have been central to recent arguments about the size of these program. Six states have recently considered opting out of Medicaid entirely (Adamy and King Jr. 2010), and some proposals would limit Medicaid's federal financing and allow states to restrict eligibility and services in ways that have not been allowed since the 1950s (Goodman-Bacon and Nikpay 2017).

Short-run empirical evaluations show that Medicaid improves health, but for some groups these effects may not justify the size of the program (Finkelstein, Hendren, and Luttmer 2019). For example, while Medicaid saves lives (Currie and Gruber 1996a, b, Goodman-Bacon 2018c, Sommers, Baicker, and Epstein 2012), the health effects are small for middle-income groups, and costs per life saved can be high. The Oregon Health Insurance Experiment shows improvements in self-reported health measures but not in clinical measures, providing support for both Medicaid's advocates (Kishore 2014) and critics (Antos and Capretta 2014, Roy 2014).

Accounting for Medicaid's effects over the course of its recipients' lives, however, may change this cost-benefit calculation. However, many Medicaid expansions have focused on children during critical periods, which could lead to large effects later in life (Cunha, Heckman, and Schennach 2010). Improvements in adult health and economic outcomes can also *lower* net public costs by reducing spending on programs linked to poor health, or by increasing tax revenue (Hendren and Sprung-Keyser 2020).

New research revisits eligibility expansions from the 1980s and shows that Medicaid can have positive long-run effects on health, human capital, earnings, and tax payments (Brown, Kowalski, and Lurie 2014, Cohodes et al. 2014, Miller and Wherry forthcoming, Thompson 2017, Wherry and Meyer 2015, Wherry et al. 2015). Yet these studies observe cohorts in their 20s, so longer-run effects, especially those tied to health conditions that emerge at older ages, may be significantly larger or smaller than existing estimates. The 1980s expansions also provided a combination of medical, food, and cash benefits, making it difficult to infer which aspect of the reforms generate reduced-form treatment effects. Finally, the sequential eligibility expansions preclude common tests of the validity of the research design such as pre-trend comparisons, so we have little direct evidence on this point.

This paper builds on Goodman-Bacon (2018c) to provide new evidence on Medicaid's longer-run effects on health and labor market outcomes. I exploit the program's introduction between

1966 and 1970 and the federal mandate that Medicaid cover all cash welfare recipients ("categorical eligibility").[1] These two program features led to a sudden increase in public insurance eligibility that was larger in areas with higher welfare participation. From a long-run perspective, cohorts born closer to Medicaid spent more years potentially eligible for it, and those from higher-welfare states accumulated more eligibility per year. *Cumulative* childhood Medicaid eligibility thus phased in gradually across cohorts but more quickly for those from higher-welfare states.

This setting offers crucial advantages for understanding Medicaid's longer-run effects. First, the cohorts of young children covered by Medicaid at its inception were born in the mid-1950s through mid-1970s, so they are up to 30 years older than the cohorts covered in the 1980s. Second, I provide a unified analysis of health and socioeconomic outcomes: cumulative mortality rates from 1980-2016 (using Vital Statistics data), self-reported disability, labor market status, transfer program participation, educational attainment, and the distribution of earnings and transfer income (from the 2000-2017 Census/American Community Survey). Third, Medicaid's introduction defines clear pre-treatment cohorts and facilitates direct tests of the research design not typically implemented in research on the sequential expansions of the 1980s and 1990s.

I estimate reduced-form effects of childhood eligibility using a difference-in-differences design that compares cohorts born at different times relative to Medicaid implementation (first difference) in states with different categorical eligibility rates in the year *of* implementation (second difference). Variation in initial welfare rates came from long-standing institutional features of states, which matches direct evidence that they were uncorrelated with levels or trends in economic, demographic, health, and policy characteristics. Nevertheless, this variation strongly predicts contemporaneous Medicaid participation and cumulative Medicaid eligibility. Comparing adult outcomes across cohorts born in different years relative to Medicaid implementation and in states with different initial welfare rates is therefore unlikely to confound the program's effects with other health, socioeconomic, or policy changes.

Event-study specifications further support the validity of the design by showing that adult outcomes track patterns of eligibility closely. They are uncorrelated with initial Medicaid eligibility for respondents who are too old to have qualified as children; diverge gradually in

[1] Sohn (2017) and Boudreaux, Golberstein, and McAlpine (2016) also estimate longer-run effects of Medicaid's introduction, but primarily rely on differences in *when* states implemented Medicaid. Goodman-Bacon (2018c) discusses the validity of this timing variation.

higher- versus lower-eligibility states for cohorts with increasing exposure; and flatten out for post-Medicaid cohorts with the same predicted childhood eligibility. Cohorts with early-life Medicaid eligibility are healthier adults as measured by mortality and disability, and they work more and receive less in transfer income. New earnings largely offset lower transfers for white cohorts, leaving individual income unchanged. I find smaller reductions in transfer income, but similar increases in employment for nonwhite cohorts. Nonwhite high school graduation rates also increase, which leads to larger increases in earnings and total income and lower poverty rates.

The government, however, saves on benefit payments *and* receives new income tax revenue. Discounting these long-run fiscal externalities to 1965 yields a total savings of $200 billion (in 2017 dollars), three quarters of which comes from lower spending on cash and in-kind safety net programs. By comparison, the discounted costs of covering cohorts born before 1979 is just $92 billion. Childhood Medicaid coverage for these cohorts has therefore saved more than twice its cost. Combining changes in longevity and disability suggests that Medicaid's introduction also added 10 million quality-adjusted life-years.

I. Expected Effects of Medicaid Implementation on Later-Life Outcomes

Medicaid's original introduction provides an especially clean context in which to study the program's long-run effects. Before Medicaid, private insurance was rare among the poor, public medical programs were small, and free sources of medical care were uncommon and often of low quality (Goodman-Bacon 2018c).[2] As a result, poor children frequently went without medical care. Figure 1 shows that fewer than half of poor children in the early 1960s had seen a doctor in the previous year compared to three quarters of middle-income children.

Poor children were also strikingly unhealthy in ways that extended into adulthood. Their mortality rates were twice as high as those of non-poor children (National Center for Health Statistics 1965), and they suffered more often from a range of specific symptoms.[3] In terms of

[2] Only about eight percent of adults received any free care in 1960 (Morgan et al. 1962), and only 2.8 and 13.4 percent of low-income children in non-Medicaid states had doctor or clinic visits (respectively) without charges in 1969 (Loewenstein 1971, p. 2.11 table 2.31). Nine percent of parents in the 1968 PSID reported that they could get "free care." Moreover, free care was of low quality and hard to obtain. A 1964 Children's Bureau report describes a hospital outpatient department in Dallas, Texas as "deplorable." In Birmingham, Alabama "many [are] turned away from outpatient clinic (40 or more a day) due to lack of funds…a mother returned with her dead baby in a sack" (Lesser et al. 1964). One hospital administrator in New York City bemoaned the passage of Medicaid, asking "How do you expect [continuing medical research] to be carried out if patients come to the hospital only for medical care and are not interested in taking part in new and as yet unaccepted methods of treatment?" (Stevens and Stevens 1974, pp. 99).
[3] Parental reports provide unreliable measures of the burden of specific diseases. In the 1963-1965 National Health Examination Survey (USDHHS/NCHS 1991), for example, higher-income children report higher rates of mumps,

adult health, one highly publicized 1964 report showed that over one-quarter of Army inductees were rejected on medical grounds, most commonly for diseases and defects of the "bones and organs of movement" (President's Task Force on Manpower Conservation 1964). The report's "most significant finding" was that these differences were correlated with socioeconomic status and that "*75 percent of all persons rejected for failure to meet the medical and physical standards would probably benefit from treatment*" (italics in original, pp 25).

A. Medicaid Implementation, Children's Insurance Coverage, and Aggregate Utilization

Medicaid's passage as title XIX of the 1965 Social Security Act Amendments represented a major expansion in the availability and generosity of (publicly funded) medical care for poor children relative to the small existing federal/state medical financing system for welfare recipients. Medicaid removed federal reimbursement caps, increased federal matching rates, defined a set of required medical services (inpatient, outpatient, physician, lab/x-ray, and nursing home) and mandated coverage for recipients of cash transfer programs (the "categorical eligibility" requirement). Almost all categorically eligible children (89 percent) qualified through the Aid to Families with Dependent Children (AFDC) program (DHEW 1976). All states except Alaska (1972) and Arizona (1982) implemented Medicaid between 1966 and 1970.

Immediately following Medicaid implementation, public insurance coverage among children increased sharply while uninsurance rates fell. Less than one percent of children had public coverage in 1963, but about 15 percent did by the mid-1970s, and almost all of this increase reflected reductions in uninsurance (Goodman-Bacon 2018c). Moreover, the categorical eligibility requirement created a strong link between changes in children's public insurance use and the states' prevailing AFDC participation rates. Goodman-Bacon (2018c, Appendix Table 2.B4) finds that a one percentage point difference in the initial AFDC rate is associated with a 1.82 percentage points of additional public insurance growth after Medicaid.

The large increase in coverage meant that poor children received substantially more medical care. Appendix Table A3.1 presents cross-sectional differences in utilization by Medicaid eligibility across 10 surveys from before and after Medicaid implementation showing that children eligible for Medicaid used much more medical care than ineligible poor children in the same state,

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bronchitis, scarlet fever, polio, allergies, or a heart murmur. Poor children, however, report more symptoms that are observable without a diagnosis such as a sore throat, colds, a "heart problem," or identifiable conditions such as whooping cough.

or similar children in non-Medicaid states.[4] Figure 1 shows the net result of these utilization increases: the steep income gradient in children's doctor visits in the early 1960s almost completely disappeared by 1975.

B. Expected Longer-Run Effects

A large body of evidence suggests that this rapid growth in insurance coverage and medical care use should affect health and economic outcomes later in life. Infant and child health are strongly correlated with test scores, education, labor supply, earnings, and welfare receipt in adulthood (Currie, Decker, and Lin 2008, Smith 2009). Early life exposure to specific infectious diseases harms adult health, education, and earnings (influenza: Almond 2006, malaria: Barreca 2010, pneumonia: Bhalotra and Venkataramani 2015, hookworm: Bleakley 2007, gastrointestinal disease: Chay, Guryan, and Mazumder 2009, 2014, meningitis: Roed et al. 2013, typhoid fever: Beach et al. 2014). A number of mechanisms could link child and adult health, including an inflammatory immune response (Crimmins and Finch 2006), a diversion of nutritional resources (Fogel 1997), reduced energy and school performance (Adhvaryu et al. 2015, Bleakley 2007), direct organ damage, or changes in parental investments (Becker and Tomes 1976).

Evidence on Medicaid implementation, and the categorical eligibility requirement in particular, shows substantial short-run improvements in child health (Goodman-Bacon 2018c). Medicaid reduced infant deaths through improved hospital care with no discernible effect on health at birth, and reduced deaths, mainly from treatable infectious diseases, among young children.[5] Medicaid also included a mandate to identify and screen children for debilitating but treatable conditions. The Early and Periodic Screening, Diagnosis and Treatment (EPSDT) program required states to locate eligible children and "ascertain their physical or mental defects, and [provide] such health care, treatment, and other measures to correct or ameliorate defects and chronic conditions discovered thereby" (PL 90-248 quoted in Stevens and Stevens 1974).[6] In fact, President Johnson stressed later-life effects when he advocated for EPSDT: "Ignorance, ill health,

[4] Online appendices are available here: **https://my.vanderbilt.edu/ajgb/research-in-progress/**

[5] The lack of contemporaneous effects on health at birth likely rules fetal programming as an explanation for long run effects. However, acute care at birth can, itself, improve later-life outcomes (Bharadwaj, Løken, and Neilson 2013).

[6] Stevens and Stevens (1974) discuss lags in the promulgation of EPSDT regulations, but emphasize that it was a major new proposal. EPSDT provided a "full health history, an analysis of physical growth, developmental assessment, unclothed physical inspection, ear, nose, mouth, and throat inspection, vision testing, hearing testing, anemia testing, sickle cell, TB, urine and lead-poisoning testing, as well as nutritional and immunization status reports" (pp. 257, note 50). They also cite an early experience in Mississippi in which "1,300 abnormalities were discerned in the first 1,200 children screened" (quoting Howard Newman, pp. 257 note 51).

personality disorder—these are disabilities often contracted in childhood: afflictions which linger to cripple the man and damage the next generation" (Johnson 1967).

Recent work using both state-by-year variation and a birth date discontinuity in the 1980s Medicaid expansions finds later-life benefits. Childhood eligibility is associated with improvements in both teenage health (obesity, BMI: Cohodes et al. 2014, self-reported health: Currie, Decker, and Lin 2008, mortality: Wherry and Meyer 2013) and adult health (mortality: Brown, Kowalski, and Lurie 2014, obesity, BMI, chronic illness: Miller and Wherry forthcoming), and with reductions in adult hospitalizations for chronic disorders (Wherry and Meyer 2013, Wherry et al. 2015). Medicaid's long-run benefits extend beyond health to academic achievement (Levine and Schanzenbach 2009), educational attainment (Cohodes et al. 2014), and earnings (Brown, Kowalski, and Lurie 2014).

These results, however, may not provide a good guide to Medicaid's longer-run effects because the treated cohorts are still young. Longer-run effects could grow if Medicaid reduces the lifetime incidence of chronic conditions, or could fade if Medicaid simply delays the age of onset.[7] In fact, the only direct evidence on effects at older ages is mixed. Using the differential timing of Medicaid adoption across states, Sohn (2017) finds that cohorts covered at birth had lower mortality rates through adulthood. Boudreaux, Golberstein, and McAlpine (2016) use the Panel Study of Income Dynamics and find that, among adults who were poor in 1968, childhood Medicaid exposure leads to higher scores on an index of adult health outcomes but not on an index of economic outcomes.

In summary, the contemporaneous effects of Medicaid's introduction appear to have triggered life-course health processes that matter for adult outcomes in other contexts, including more recent Medicaid expansions. Medicaid's origin thus provides an opportunity to understand the program's longer-run effects in a way that has not previously been possible.

[7] The structure of the 1980s expansions often makes it difficult to know why Medicaid affects shorter- and longer-run health and economic outcomes. While medical care use increased for pregnant mothers and children who gained new coverage (Currie and Gruber 1996a, b), it may have fallen among those who switched from private insurance to Medicaid (Currie and Gruber 2001). Crowd-out families also gained disposable income (Leininger, Levy, and Schanzenbach 2012) but faced incentives to draw down savings (Gruber and Yelowitz 1999). New Medicaid recipients were also adjunctively eligible for food benefits (Bitler and Currie 2004) and, in some cases, gained Medicaid coverage because of expansions in cash welfare eligibility. Both programs have been shown to have longer-run effects (Aizer et al. 2014, Hoynes, Schanzenbach, and Almond 2012).

II. DATA: MEASURING ADULT OUTCOMES BY RACE, STATE-, AND YEAR-OF-BIRTH

To evaluate Medicaid's long-run effects, I compile data on average adult outcomes by race, state-of-birth, and year-of-birth. Medicaid policy (when states implemented the program and the welfare participation rates that defined eligibility) varies by state and year, and I stratify by race because cross-state patterns of AFDC participation differed strongly for white and nonwhite children.[8] I drop respondents born in Alaska, Hawaii, and Arizona, and keep cohorts born between 1936 and 1976. The estimation samples have 1,968 observations (48 states*41 cohorts) for each race group.

A. Cumulative Medicaid Eligibility

The endogenous variable is the expected number of years that members of each cohort would have been categorically eligible for Medicaid. Cumulative childhood Medicaid eligibility for a cohort born in state *s* in year *c* (of race *r*) is a weighted sum across the years of childhood (y) *and* the states of residence (ℓ) of that cohort:

$$m_{rsc} = \sum_{y=c}^{y=c+18} \sum_{\ell} \sigma_{rsc}^{y}(\ell) \cdot AFDC_{ry\ell} \cdot 1\{y \geq t_{\ell}^{*}\} \qquad (1)$$

The post-Medicaid dummy, $1\{y \geq t_{\ell}^{*}\}$, equals one if year *y* is after state ℓ's Medicaid implementation date ($1966 \leq t_{\ell}^{*} \leq 1970$). $AFDC_{ry\ell}$ denotes the average monthly child AFDC participation rate by race (*r*), state (ℓ), and year (y).[9] I construct each state's nonwhite share of AFDC children using printed tables for 1958 and 1961 (Mugge 1960, DHEW1963), microdata on AFDC recipients from 1967-1997 (DHEW 2000, 2011, United States Department of Health and Human Services 2013). I interpolate the race shares between missing years, multiply by average monthly counts of AFDC children (U.S. Department of Health and Human Services 2012), and divide by population (Haines and ICPSR 2010, Surveillance of Epidemiological End Results 2013) to calculate $AFDC_{ry\ell}$. Finally, $\sigma_{rsc}^{y}(\ell)$ is the probability that members of a cohort born in state *s*

[8] For example, 1.3 percent of white children in Nevada were eligible through AFDC when the state's program began in July 1967, but 5 percent were eligible when New Mexico's program started in December 1966. For nonwhite children, differences in initial eligibility reverse: 22 percent in Nevada versus 10 percent in New Mexico.

[9] All eligibility measures refer to the expected number of *full* years of Medicaid eligibility. Treating 1967 as a full year of implementation and assuming that the monthly AFDC participation rate in Nevada of 1.3 percent is constant, then the expected number of *months* of eligibility in 1967 (the interval at which AFDC eligibility is actually determined) is 12*0.013 = 0.156, which is the same as 0.013 full years of eligibility. Because of churning in AFDC caseloads, the expected number of years with *any* Medicaid eligibility is higher.

in year c (race r) lives in state (ℓ) in calendar year (y), which I observe every five years in the 1970-2000 Censuses and linearly interpolate to get an annual measure.[10]

Figure 2 plots m_{rsc} by state and event-cohort. Cumulative eligibility phases in across cohorts born closer to Medicaid's start date and flattens out for cohorts born after Medicaid. Fully treated nonwhite cohorts accumulated about 5 years of eligibility and fully treated white cohorts accumulated about 1 year. Variation across states is nearly as big as the average gains.

B. Cumulative Adult Mortality

My first measure of adult health is the cumulative cohort-level mortality rate between 1980 and 2016 (conditional on living to 1980). The count of deaths for a given race-by-birth-state-by-birth-year cell come from the 1980-2016 Multiple Cause of Death Files (United States Department of Health and Human Services 2009), which contain information on decedents' state of birth. The corresponding population denominators come from the 1980 Census.[11]

My primary mortality outcome is the log mortality rate from non-AIDS causes between 1980 and 2016. During the 1980s and 1990s, AIDS killed more adults aged 25-54 than any other cause. AIDS mortality is also correlated to an extent with Medicaid exposure: it was highest for those born in the 1950s, fell strongly for those born in the 1960s (who benefitted from highly active anti-retroviral therapies), and was concentrated among nonwhite men in high-AFDC states like New York and New Jersey. I construct cumulative mortality rates that reflect the force of non-AIDS mortality (rather than the effect of AIDS on the size of the surviving population).[12] Results are

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[10] Using cohort-level migration patterns will introduce measurement error in m_{rsc} if the average mover goes to states with different AFDC rates than movers in the AFDC population. To test this, Appendix Figure A3.1 calculates m_{rsc} for PSID respondents with 1968 family income above or below \$6,000 (about twice the poverty threshold). After partialling out childhood state and cohort fixed effects I cannot detect differences in the relationship between income-specific cumulative eligibility measure and predicted cumulative eligibility instrument (defined below) by childhood income. This supports the use of average cohort migration patterns based on much larger samples.

[11] Summing deaths over 37 years provides an overview of mid-life mortality, and reduces noise relative to annual or decadal rates. Race reporting among Hispanic Census respondents also changed as the number of race categories grew, but this did not always occur on death certificates, which medical examiners or funeral directors fill out. Matching "white" and "nonwhite" deaths to similarly coarse denominators in 1980 avoids time-varying misclassification (Arias et al. 2008).

[12] Until 1998, US death certificates used the 9th Revision of the International Classification of Disease (ICD-9), which does not measure AIDS/HIV. The NCHS added special codes for HIV in 1987 (*042-*044). Measuring AIDS deaths before 1987 is difficult because HIV/AIDS patients typically die of opportunistic infections, and because stigma surrounding the diagnosis. (Georgia did not report AIDS death counties for counties with three or fewer deaths.) The technical appendix to the 1988 Multiple Cause of Death file describes the ICD codes that contained most HIV/AIDS deaths prior to the 1987 code change: "Deficiency of cell-mediated immunity (ICD No. 279.1), Pneumocystosis (ICD-9 No. 136.3), and Site unspecified (ICD-9 No. 173.9), under other malignant neoplasms of skin" (National Center for Health Statistics 1991). I use these codes to remove AIDS-related deaths before 1987.

similar, and slightly larger, without this adjustment. I calculate other cause-specific mortality rates in the same way, which includes a non-AIDS internal-cause measure that also removes deaths from accidents, suicide, and homicide which also spiked in the late 1980s (Evans, Garthwaite, and Moore 2018).[13] The oldest cohort in the sample was born in 1936, 30 years before the earliest Medicaid implementation date, and their mortality is measured between ages 45 and 82. The youngest cohort in this sample was born in 1976, six years after the latest Medicaid implementation date, and their mortality is measured between ages 4 and 41.

C. Adult Self-Reported Disability

The Census and American Community Survey (ACS) (Ruggles et al. 2010) include six self-reported disabilities: hearing or vision problems; difficulty with activities such as walking or carrying (ambulatory difficulty), going outside the home (mobility difficulty), getting around inside the home (self-care difficulty), learning, remembering, or concentrating (cognitive difficulty), or working at a job or business (work limitation). Because of a question text change I only use the 2000-2007 Censuses and ACS (the results hold in the 2008-2017 data). The 1943 cohort is the oldest one that I observe in every Census year. They were born 23 years before the earliest Medicaid implementation date, and I observe their disability rates between ages 57 and 64. I observe disability for the youngest cohort (1976) between ages 24 and 31. See Appendix 1 for summary statistics.

D. Adult Socioeconomic Outcomes

I calculate other adult outcomes similarly using the 2000-2017 Census and ACS. Labor market outcomes include rates of labor force participation, employment (current and annual), and full-time/full-year employment. Safety-net outcomes include cash transfer receipt, and public insurance receipt (Medicaid or Medicare). Income measures include averages and points in the distribution of income by source, wages, and poverty rates. Education outcomes include having a high school diploma or bachelor's degree.

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[13] Let the 1980 population of cohort c from state s be $POP_{cs,1980}$, and denote annual AIDS-related deaths by $d_{cs,y}^{AIDS}$, and non-AIDS-related deaths by $d_{cs,y}^{OTH}$. It is straightforward to calculate cause-specific mortality rates *in 1980* as $mr_{cs,1980}^{AIDS} = \frac{d_{cs,1980}^{AIDS}}{POP_{cs,1980}}$, and $mr_{cs,1980}^{OTH} = \frac{d_{cs,1980}^{OTH}}{POP_{cs,1980}}$. Subsequent mortality rates use annual deaths in the numerator and the surviving cohort population, $POP_{cs,1980} - \sum_{j=1980}^{y}\left(d_{cs,j}^{AIDS} + d_{cs,j}^{OTH}\right)$, in the denominator. If period mortality rates would have been the same in the absence of other causes (ie. independent risks), then an estimate of the cause-specific mortality rate from cause k is $1 - \prod_{j=1980}^{2016}\left(1 - mr_{cs,j}^{k}\right)$. See Manton and Stallard (1984).

III. RESEARCH DESIGN: MEDICAID IMPLEMENTATION, CATEGORICAL ELIGIBILITY, AND CUMULATIVE ELIGIBILITY ACROSS STATES AND COHORTS

Medicaid's introduction led to sharp increases in annual eligibility and coverage that were much larger in states with higher AFDC rates. The research design exploits these historical circumstances using a difference-in-differences strategy that compares cohorts born closer to or farther away from Medicaid's introduction (first difference) in states with higher or lower child AFDC rates in the year of Medicaid implementation (second difference).[14] Categorizing states by their initial categorical eligibility rate, denoted $AFDC_{rs}^{*}$, provides a fixed ranking by which to compare adult outcomes, and avoids using differences in Medicaid timing, year-to-year changes in AFDC rates, or cohort-level mobility as sources of identifying variation. This strategy eliminates bias from differential counterfactual trends if $AFDC_{rs}^{*}$ is uncorrelated with other cross-cohort changes in adult outcomes (excludability) and $AFDC_{rs}^{*}$ predicts cumulative eligibility (relevance).[15]

Initial AFDC rates are plausibly excludable instruments because cross-state variation in welfare participation came from predetermined institutional factors unrelated to the circumstances facing cohorts first treated by Medicaid. First, identifying variation did not emerge contemporaneously in the 1960s: the correlation of $AFDC_{rs}^{*}$ with past AFDC rates is the same in 1961, 1958, and 1948 (Goodman-Bacon 2018c). Second, the variation came from idiosyncratic state-level institutions such as constitutional language (Fox 2016), industrial structure (Alston and Ferrie 1985), discrimination (Bell 1965), or traditions of aid inherited from pre-AFDC programs (Moehling 2007, Skocpol 1992) not correlated with levels or changes in children's circumstances.

A. Event-Study Specification

As a test of the research design I estimate reduced-form event-study models that trace out the relationship between *adult* outcomes and $AFDC_{rs}^{*}$ for cohorts born in different years relative to Medicaid ("event-cohorts"). The estimating equation for outcome Y_{rsc} is:

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[14] Because states initiated Medicaid at different times, it is also possible to compare the same cohorts with the same AFDC rate but who were exposed to Medicaid for different amounts of time. In practice, Medicaid timing is correlated with a range of observables. I include a set of year dummies for each Medicaid introduction year, which limits comparisons to cohorts with the same number of years of Medicaid exposure, but different AFDC-based eligibility. Appendix Table A4.2 presents estimates without these fixed effects.

[15] I structure the analysis by cohort (holding survey years fixed) rather than calendar year (holding age fixed) because I observe more cohorts than years. One potential source of bias is different age profiles across states. I check this directly using untreated cohorts observed in the 1980 and 1990 Censuses.

$$Y_{rsc} = \boldsymbol{\mu_s} + \boldsymbol{\mu_c} + \boldsymbol{\mu_{R(s),c}} + \boldsymbol{\mu_{t_s^*,c}} + \boldsymbol{X'_{rsc}\beta} + AFDC^*_{rs}\left[\sum_{j=-(A+1)}^{-20} \pi_j 1\{c - t_s^* = j\} + \sum_{j=-18}^{B+1} \phi_j 1\{c - t_s^* = j\}\right] + \varepsilon_{rsc} \quad (2)$$

Fixed effects for state of birth ($\boldsymbol{\mu_s}$) and year of birth ($\boldsymbol{\mu_c}$) ensure that the estimator is a difference-in-differences model. Region-by-cohort fixed effects ($\boldsymbol{\mu_{R(s),c}}$) account for convergence in outcomes across U.S. regions unrelated to Medicaid (Chay, Guryan, and Mazumder 2009, Stephens and Yang 2013). Medicaid-year-by-cohort fixed effects ($\boldsymbol{\mu_{t_s^*,c}}$) eliminate comparisons between earlier and later Medicaid-adopting states, which had divergent socioeconomic and health outcomes before Medicaid.[16] $\boldsymbol{X'_{rsc}}$ includes the general fertility rate and per-capita income in each cohort's birth year. Identification in equation (1) comes from comparisons of respondents born in the same region in the same event-time across values of $AFDC^*_{rs}$. I cluster standard errors by birth state.

The coefficients of interest, π_j and ϕ_j, trace out changes in the relationship between $AFDC^*_{rs}$ and Y_{rsc} across event-cohorts relative to the omitted group, $j = -19$.[17] The π_j are falsification tests. Cohorts born more than 18 years before the introduction of Medicaid had no childhood coverage and can have no "treatment effects".[18] The ϕ_j are intention-to-treat (ITT) effects that measure the relationship between an additional percentage point of initial eligibility and changes in outcomes for cohorts first exposed $-j$ years after birth, i.e., at age $a^* = max\{0, t_s^* - c\}$. Because exposed cohorts are treated from age a^* to 18, each ϕ_j is analogous to a distinct experiment in which the Medicaid dose differs by $AFDC^*_{rs} \times (19 - a^*)$ *and* coverage begins at age a^*.[19] Note that the pattern of the ϕ_j for cohorts born after Medicaid (i.e., $a^* = 0$) provides an

[16] Between 1950 and 1970, for example, white child poverty fell by about 21 percent in states that implemented Medicaid before 1969, but by 33 percent in states that implemented in 1969 or 1970 (s.e. of the difference is 2.3).

[17] I report coefficients for event-cohorts born between a years prior to and b years after Medicaid implementation, chosen to be the window in which I observe all cohorts at each event-time and for all survey years. Cohorts born outside the event window [-A, B] are grouped into (unreported) terms for $-(A + 1)$ and $(B + 1)$.

[18] Members of these cohorts could still have qualified for Medicaid as public assistance recipients or through Medically Needy provisions, but survey data show a sharp drop in Medicaid use and eligibility after age 18.

[19] Appendix Figure A4.5 shows a fairly constant ratio of reduced-form to first-stage event-study coefficients. This is consistent with an underlying model where additional years of eligibility have similar effects conditional on the period of childhood where "coverage matters".

additional test of the design. These cohorts have the same "experiment" (19 years of eligibility starting at birth), so barring treatment effect heterogeneity over time, their ϕ_j should be similar.[20]

The event-study estimates also contain information about how and at which ages of exposure Medicaid affects later life outcomes. The ϕ_j will be zero if Medicaid has no effect when received at age $max\{0, a^*\}$ and older.[21] To determine when in childhood coverage matters, I conduct a series of trend-break tests on the event-study parameters to identify the break point (cf. Card, Mas, and Rothstein 2008). I fit a pre-trend that goes through zero at time -19, a phase-in trend that begins somewhere between time -19 and -1, and a post-trend that begins at zero, and report the coefficients for the break point that maximizes the F-statistics on the spline terms.

In the nonwhite sample, this exercise consistently identifies trend breaks but also pre-existing, opposite signed trends for untreated cohorts. To address this, I follow Bhuller et al. (2013) and Goodman-Bacon (2018a) estimate linear pre-trends for each Medicaid timing group on data through event-time -15, extrapolate through all cohorts, and subtract the fitted trend from all nonwhite data points.[22] Appendix Figure A4.4 shows that pre-trends are correlated with cohort migration rates, which is consistent with research showing negative consequences for Black migrants to the North in terms of incarceration rates (Derenoncourt 2019) and mortality rates (Black et al. 2015). This does not change the estimated trend breaks, it only alters the orientation of the event-study coefficients and the resulting IV estimates.[23]

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[20] Because my outcome data are state/cohort averages, the estimates may also capture any general equilibrium effects. For example, if hospitals use Medicaid revenue to expand capacity, then health may improve for cohort members who do not actually receive Medicaid. State budgets are another potential channel. Appendix Figure A3.4 uses Census data on state finances to show that states spent more on Medicaid but not on schools or hospitals.

[21] The ϕ_j do not separately identify heterogeneous effects by age at exposure versus amount of exposure, though, because cohorts who were young when Medicaid was passed also had more coverage. Conclusions about age versus amount of coverage require additional assumptions.

[22] Many difference-in-differences analyses include unit-specific linear time trends, which cannot distinguish between time-varying treatment effects and pre-existing trends (Lee and Solon 2011). The event-study figures clearly show such effects, and so I do not estimate state-specific cohort trends models.

[23] Partialling out a single pre-trend variable that equals $AFDC_s^* \times (c - t_s^*)$ yields algebraically identical trend breaks. Estimating separate pre-trends for each Medicaid timing groups is more flexible, but in practice has almost no effect on the estimated breaks. Appendix Figure A4.2 shows event-study estimates with and without this adjustment, Appendix Figure A4.3 shows robustness to the dates over which I estimate the trends. The estimates are fairly similar when I remove pre-trends estimated on longer or shorter windows. Estimates that remove a pre-trend through event-cohort -19, for example, are about two-thirds as large as my preferred estimates for mortality, disability transfer receipt, and employment, but about 20 percent larger for ambulatory difficult. Since the event-study coefficients and trend-break estimates do not show strong evidence of effects for people exposed as teenagers, and because this detrending strategy produces more precise results when it uses a longer pre-trend, I prefer the results detrended through event-cohort -15.

B. *Instrumental Variables Specification*

To express the effects in terms of years of childhood eligibility, I estimate instrumental variables models that use the predicted "dose" as an instrument for cumulative eligibility. The instrument equals the dose of Medicaid eligibility predicted by a cohort's year of birth and initial AFDC rate:

$$z_{rsc}^{L,U} = \sum_{y=c+L}^{c+U} 1\{y \geq t_s^*\} \cdot AFDC_{rs}^* \qquad (3)$$

$z_{rsc}^{L,U}$ equals predicted eligibility between ages L and U. The IV models contain the same covariates as in equation (2) and use the $z_{rsc}^{L,U}$ variables as instruments for similarly calculated versions of $m_{rsc}^{L,U}$. I measure eligibility between ages 0 and 5, 6 and 11, and 12 and 18, and test for differences in the IV estimates across ages. Using these tests as a guide, I also pool ages with similar effects, but always include endogenous variables and instruments that cover ages 0-18. For early eligibility coefficients, I present p-values from 250 draws of a wild-cluster percentile-t bootstrap

The resulting estimates give the ITT effect on adult outcomes per year of cohort-level childhood eligibility. Because AFDC participants cycle on and off the program, one year of expected eligibility most likely comes from a larger share of a cohort receiving Medicaid coverage for part of a year, rather than a fixed group receiving Medicaid for an additional year of their life (Berger and Black 1998).

C. *First Stage: are initial AFDC rates correlated with cumulative Medicaid exposure?*

Figure 3 plots event-study estimates from equation (2) of the first-stage relationship between initial AFDC rates and cumulative Medicaid eligibility measured throughout childhood, between ages 0 and 11, and between ages 0 and 5. The coefficients for untreated event-cohorts (-30 through -20) are small by construction, and the positive and increasing coefficients show that cohorts from states with higher initial eligibility accumulate more childhood eligibility per year of exposure to any Medicaid program. The coefficients for event times 1 through 5 flatten out (white) or erode (nonwhite), which underscores how the "dose" of childhood Medicaid exposure is the same for cohorts born after implementation. If Medicaid has long-run effects, then event-study estimates for other outcomes should have this pattern as well.

Each percentage point of initial eligibility corresponds to about 0.12 additional years of cumulative Medicaid eligibility for fully treated white cohorts, with 0.08 years accruing before age 11, and 0.05 years before age 5. The nonwhite estimates for fully treated cohorts are smaller for eligibility under ages 18 (0.07 years) and age 11 (0.06 years), but more similar for eligibility before age 5 (0.04 years).[24] Column 1 of Table 1 presents first-stage estimates that quantify these differences. Across all childhood years, $z_{rsc}^{0,18}$ strongly predicts white cumulative eligibility (0.61, s.e. = 0.16, *F*=15.1), but is only weakly related to nonwhite cumulative eligibility (0.38, s.e. = 0.17, *F*=5.2). (Estimates without the migration adjustment are in Appendix Table A3.2.) Columns 2 through 4 present stronger age-group-specific first-stage estimates, especially for nonwhite cohorts. Each year of predicted cumulative eligibility is associated with about 0.7 years of actual cumulative eligibility.[25]

IV. INTENTION-TO-TREAT EFFECTS OF MEDICAID ON ADULT HEALTH

A. Cumulative Mortality, 1980-2016

The event-study estimates in Figure 4 show that early childhood Medicaid eligibility is strongly associated with reductions in later-life non-AIDS mortality. The point estimates do not show a trend for cohorts with no childhood Medicaid exposure—a key test of the design—as well as for cohorts that were only eligible later in childhood.[26] Mortality begins to fall in higher-AFDC states for cohorts exposed under age 9 for nonwhite cohorts and under age 6 for white cohorts (see Appendix Figure A4.1 for F-statistics), and the estimated trend breaks are significant (white: -0.14, s.e.=0.04; nonwhite: -0.06, s.e. = 0.03). The white mortality estimates flatten out for post-Medicaid cohorts. These features match the “phase-in” shape of the first stage and support the AFDC-based research design.

For each percentage point of $AFDC_{rs}^{*}$, fully treated cohorts had about half of one percent lower mortality rates and, according to Figure 3, about 0.05 additional years of eligibility (from ages 0-5 for white cohorts and age 0-11 for nonwhite cohorts). This implies an ITT effect per year of about -10 percent for both groups (-0.5 percent/0.05 years of eligibility = -10 percent per year), a

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[24] The main reason why the first-stage estimates differ by race is that during the late 1970s and early 1980s, nonwhite AFDC rates converged across states to some extent. Nonwhite cohorts born in low (high) $AFDC_{rs}^{*}$ states had low (high) early childhood eligibility but not necessarily low (high) later childhood eligibility.

[25] Appendix 3 provides suggestive evidence on the relationship between initial eligibility and health care use. Appendix Figure A3.2 shows increases in nonprofit hospital admissions in higher-AFDC states. Appendix Figure A3.3 uses the 1963 and 1970 Surveys of Health Services Utilization and Expenditure to show that higher AFDC rates are correlated with higher measures of primary care use among poorer children relative to richer children. Boudreaux, Golberstein, and McAlpine (2016) also show that Medicaid’s introduction increased hospital admissions and reduced medical debt.

[26] The relative increase in mortality for older white cohorts comes from deaths at ages 65-80. Event-study estimates on mortality rates through 2010 or 2004 do not have this pattern, but do still show trend-breaks for cohorts exposed to Medicaid (see Appendix Figures A6.3 and A6.4).

magnitude confirmed in the IV estimates in Table 2. Consistent with research on life-course health and human capital development eligibility in the first five years of life is associated with large and precise mortality reductions (white: -14.5, s.e.=4.5; nonwhite: -8.7, s.e. = 5.05). I can reject that eligibility under age 6 has the same effects as later eligibility on white mortality, but the two earlier coefficients are indistinguishable for nonwhite mortality (although the age 0-5 coefficient is twice the 6-11 coefficient). Panels B and D group age-specific eligibility variables with indistinguishable coefficients and present effects for "early" eligibility. The point estimates are similar, but the *t*-ratios rise and bootstrap *p*-values are about 0.07.

Table 3 presents IV estimates for early eligibility by cause of death (see Appendix Figures A5.1 and A5.2 for event-study estimates). The results show that Medicaid's effects operate through a range of conditions. The main result is not sensitive to the adjustment for AIDS deaths (column 1), and the magnitudes are in fact larger than the non-AIDS estimates (reproduced in column 2). The non-AIDS estimates come largely from internal causes (column 3), which account for the majority of deaths. Cardiovascular diseases falls more for nonwhite than white cohorts, while chronic conditions fall more for white cohorts. The standard errors are too large to distinguish these estimates from each other, however.[27] I find no evidence of changes in 37-year cancer mortality, but cancer mortality does fall for both groups over a 25-year horizon (Goodman-Bacon 2018b), suggesting that Medicaid may delay cancer deaths. Column 8 shows large reductions in suicide (white: -23.4, s.e. = 10.3; nonwhite: -22.0, s.e. =7.3), consistent with reductions in the burden of chronic illness (Case and Deaton 2015) and with Medicaid's positive effects on contemporaneous and later-life mental health (Finkelstein et al. 2012, Miller and Wherry forthcoming).

Many analyses of insurance and mortality use deaths from external causes—homicides and accidents—as a falsification test. Medicaid eligibility, however, is slightly associated with reductions in these deaths. One interpretation is that an unmeasured factor generates these reductions. Effects on nonwhite SES (documented below) could change risky behaviors associated with accidents or change exposure to crime rates through moves to better neighborhoods. This

[27] This is comparable to the effects of college attendance for slightly older cohorts eligible for military service in Vietnam (Buckles et al. 2016). Behavioral changes could underlie this link, as could changes in the infectious causes of certain cancers such as the bacterium *h. pylori* (stomach cancer) or human papillomavirus (cervical cancer).

claim, however, requires data on behaviors and detailed residence, neither of which are available in public data.

What size effects among treated adults are required to rationalize these ITT effects? The proportional reduction per year of coverage among the treated, denoted δ_r, could be larger than the ITT since this group must account for *all* the averted deaths, but it could be smaller if treated adults have higher baseline mortality. The PSID's mortality supplement shows that white (nonwhite) respondents with AFDC income in 1968 have cumulative mortality rates that are about 1.57 (1.1) times higher than those without AFDC income (University of Michigan Survey Research Center 2016).[28] Multiplying observed mortality rates by these ratios, however, does not yield *counterfactual* mortality rates because Medicaid itself reduces observed mortality among the treated. The extent to which this matters is a function of the proportional average treatment effect on the treated and their amount of childhood eligibility. Treated white children spent about 1.2 full years on AFDC by age 6 and nonwhite children spent about 3.5 full years on AFDC by age 11 (Berger and Black 1998, Smith and Yeung 1998). Estimated mortality rates among the treated are therefore still too small by a factor of $(1 + 1.2 \cdot \delta_w)$ for white adults and $(1 + 3.5 \cdot \delta_{nw})$ for nonwhite adults. Combining these two adjustment factors implies that a year of childhood coverage reduces cumulative non-AIDS-related mortality rates by about 8.3 percent among treated white adults and 6.2 percent among treated nonwhite adults (see appendix 7 for details).

These reductions are comparable to estimates from the 1980s expansions. The results in Wherry and Meyer (2013) for black teens 15-18 translate to a proportional reduction in internal-cause mortality of about 6 percent. Brown, Kowalski, and Lurie's (2014) results imply about a 1 percent reduction in mortality between ages 18 and 27. These differences make sense. Both studies focus on eligibility later in childhood and for groups with higher incomes than categorically eligible children. Finally, Roy selection implies lower marginal treatment effects at higher eligibility levels like those that prevailed in the 1980s (Heckman and Vytlacil 2005).

B. Self-Reported Disability

Results using an independent measure of health, self-reported disability, affirm that childhood Medicaid eligibility improves adult health. Figure 5 plots event-study estimates for the most common self-reported disability in the Census, ambulatory difficulty. Linear pre-trends are again

[28] I thank Valentina Duque for sharing these calculations.

flat, and the best-fitting trend breaks come at age 10 for white disability rates (-0.019, s.e. = 0.009) and age 3 for nonwhite disability rates (-0.025, s.e. = 0.012). Crucially, the trends reverse for cohorts born after Medicaid implementation, and a joint test of the null of equal slopes for the oldest cohorts and post-Medicaid cohorts, a direct prediction of the design, yields p-values of 0.80 (white) and 0.71 (nonwhite). (Event-study estimates for 2008-2017 are in Appendix Figure A6.1.)

The corresponding IV estimates, presented in column 2 of Table 2, show that each year of cohort-level childhood eligibility reduces ambulatory difficulty by 4.26 percentages points for white cohorts (s.e. = 1.06, p-value = 0.004) and 5.73 percentage points for nonwhite cohorts (s.e. = 2.33, p-value = 0.004). I again use the PSID to calculate the counterfactual disability rate among adults with childhood Medicaid eligibility. In 2001, 29 percent of white respondents with 1968 AFDC income reported work limitations compared to 11 percent on average. Applying this ratio to the average rate of ambulatory difficulty for adults born between 1955 and 1975 (5.7 percent) implies an actual disability rate among treated adults of 14.5 percent. Treated white adults had about 2.4 years of eligibility by age 11. Subtracting the treatment effect of -4.26*2.4 percentage points yields a counterfactual disability rate for treated white adults of 25 percent, comparable to the rates for Army inductees in the early 1960s. This suggests that a year of cohort-level childhood Medicaid eligibility reduces the incidence of ambulatory difficulty among treated white adults by about 17 percent (-4.26/25). Similar calculations for nonwhite adults imply a proportional reduction per year of eligibility of 24 percent.[29]

Consistent with Medicaid's effects across causes of death, the white results show precise reductions in all self-reported disabilities (panel A of Table 4, event-study estimates in Appendix Figure A5.3). This points to broad improvements in functional capacity and is consistent with general improvements in health, but also underscores the difficulty in uncovering Medicaid's specific physiological channels.

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[29] The mortality and disability results may interact if Medicaid skews the composition of survivors towards those more likely to have a disability. The observed disability rate (dr_{rsc}) is the average of the rates among those who were saved by Medicaid (dr_{rsc}^1) and those who would always have survived (dr_{rsc}^0) weighted by the share of each cohort induced to survive because of Medicaid (p_{rsc}): $dr_{rsc} = (1 - p_{rsc}) \cdot dr_{rsc}^0 + p_{rsc} \cdot dr_{rsc}^1$. I use the contemporaneous infant and child mortality estimates in Goodman-Bacon (2018c) and the cumulative adult mortality estimates from Table 2 to construct true and counterfactual probabilities of surviving to the year 2000 and calculate p_{rsc} as their difference. Since $dr_{rsc}^1 \in [0,1]$, I bound the treatment effect on dr_{rsc}^0 using assumptions about dr_{rsc}^1 (see Bharadwaj, Løken, and Neilson 2013, table 4). Appendix Table t shows that the most extreme assumptions about the disability rates of those induced to survive by Medicaid only change the white disability estimates by 7 percent and nonwhite estimates by 34 percent.

I only find evidence for effects on nonwhite ambulatory difficulty. This contrasts with Medicaid's strong contemporaneous effects on nonwhite child health. The differences in short- and long-run effects may relate to racial differences in Medicaid utilization. In the late 1960s, white Medicaid-eligible children were 17 percentage points more likely to use medical care in a year than nonwhite children (65 versus 48 percent; Loewenstein 1971 table 2.1), and they saw private providers twice as often as nonwhite children (80 versus to 43 percent for most recent site of care; Loewenstein 1971 tables 2.45, 2.46, and 5.15).[30] In the short run, it may have been easy for simple medical care to save nonwhite children's lives, but differences in *types* of care received by nonwhite kids may limit their long-run health benefits. Alternatively, because nonwhite adults experience higher disability and mortality rates than white adults, even if Medicaid's effects are similar by race, competing risks may work against observing this effect in the population (Freedman and Spillman 2016).

V. INTENTION-TO-TREAT EFFECTS OF MEDICAID ON ADULT LABOR MARKET OUTCOMES

The results in section IV suggest that childhood Medicaid eligibility induces substantial improvements in adults' physical health. This section examines how these health improvements affect program participation, labor supply, education, and income.

A. Transfer Program Participation and Labor Supply

Medicaid's health effects translate into higher extensive margin labor supply and lower transfer program participation. The series with open markers in Figure 6 plots event-study estimates of Medicaid's effect on participation rates in Social Security Disability Insurance (SSDI) or Supplemental Security Income (SSI). The white results track changes in disability very closely. The pre-trend is small and insignificant (-0.007, s.e. = 0.011), there is a negative trend break for the same cohorts that experienced health improvements (-0.030, s.e. = 0.014) and a positive one for cohorts with full exposure (0.030, s.e. = 0.009). The IV estimate in Table 2 shows a reduction in disability transfer participation of 5.23 percentage points (s.e. = 1.40). Column 3 of Table 5 shows that other welfare receipt (mostly Temporary Assistance for Needy Families, TANF)

[30] This matches direct reports about provider availability/access. When asked "Do you think that people who are eligible to get free medical care through their local welfare departments must go to certain places or can they go anywhere?" Sixty one percent of white categorically eligible household heads reported that they could go "anywhere," compared to only 46 percent of nonwhite household heads. White categorically eligible families were also twice as likely as nonwhite families to have switched providers in the last two years (Loewenstein 1971). (There was no racial difference in provider switching for poor families in states with no Medicaid program in1968.)

actually rises slightly (0.49, s.e. = 0.13), suggesting that health improvements that disqualify households for disability benefits may simply lead some to take up TANF (Borghans, Gielen, and Luttmer 2014, Goodman-Bacon and Schmidt 2019, Schmidt and Sevak 2004). (Event-study estimates by type of public assistance are in Appendix Figure A5.4.)

Nonwhite event-study results have a similar pattern, and IV estimates show that each year of coverage under age 11 reduces disability transfer receipt by 3.90 percentage points (s.e. = 1.15).[31] Consistent with the ambulatory difficulty results, Table 2 shows that this effect is larger under age 5 (-4.51, s.e. = 1.57) than between ages 6 and 11 (-3.05, s.e. = 0.75) although the effects are not statistically distinguishable (*p*-value= 0.24). Participation in other welfare programs also rises for nonwhite cohorts (Table 5, 0.76, s.e. = 0.23).

The series in Figure 6 with closed markers plot event-study estimates for annual employment that are almost the mirror image of the transfer receipt results. The pre-trends are flat, and I find positive trend breaks in employment for the same cohorts whose transfer receipt fell (white: 0.042, s.e. = 0.016; nonwhite: 0.01, s.e. = 0.008). Table 6 shows that each year of childhood Medicaid eligibility affects extensive margin labor supply, measured as being out of the labor force, currently employed, or employed at all in the last year by 4 to 5 percentage points for both groups. Most new employment is full-time/full-year (white: 3.86, s.e. = 0.78; nonwhite: 3.72, s.e. = 1.77). (Event-study estimates for all employment outcome are in Appendix Figure A5.5.)

These results speak directly to empirical analyses of disability insurance and benefit programs. Research based on random assignment to evaluators show that, *holding health constant,* disability benefits reduce labor supply (French and Song 2014, Maestas, Mullen, and Strand 2013). Research that decomposes time-series changes in DI receipt *holds nothing constant*, and concludes that health improvements have had little impact on the rolls (Autor and Duggan 2006, Duggan and Imberman 2009). Reform proposals focus on ways to improve medical reviews, tighten eligibility criteria, smooth out the benefit structure (Autor and Duggan 2006), or increase administrative capacity (Liebman 2015). The results in Figure 6, on the other hand, show that *holding program*

[31] I also test the equality of the two "plateaus" (for the pre-trend cohorts and the post-Medicaid cohorts) and fail to reject equality for all event-study results in Figure 7 (*p*-values range from 0.21 to 0.99).

incentives constant (through the cross-state comparisons), health improvements reduce disability benefit receipt and increase labor supply.[32]

The changes in program participation and employment mean that Medicaid has important intertemporal effects for public and employer sponsored health insurance. Column 4 of Table 5 shows that the cohorts with the largest reductions in SSDI/SSI receipt also use public insurance *less* often as adults (white: -4.48, s.e. = 1.0; nonwhite: -3.68, s.e. = 2.6).[33] Increases in white private insurance rates from new full-time employment offset the reduction in public coverage so total insurance coverage does not change (-0.09, s.e. = 1.01). For nonwhite cohorts DI falls less while employment rises by a similar amount, so total insurance coverage increases (4.67, s.e. = 1.13).

B. Education

Long-run research based on the 1980s expansions finds that early life coverage for lower income families increases high school graduation (Miller and Wherry forthcoming), and later childhood coverage to slightly higher income families increase college attendance and completion (Brown, Kowalski, and Lurie 2014, Cohodes et al. 2014). The results in Table 7 show that early Medicaid eligibility increases nonwhite high school graduation rates by 5.61 percentage points (s.e. = 2.42, p-value = 0.004), but white graduation rates by just 1.81 percentage points (s.e.= 0.83, p-value = 0.228). (Event-study estimates are in Appendix Figure A5.6.) I find no evidence of an effect on college completion.

These results help explain the relationship between health and labor market effects by race. White cohorts report lower disability rates, lower transfer receipt, and higher employment when covered under age 10. I find no evidence that education moderates these effects. The nonwhite disability results, on the other hand, are mixed while there are clear trend breaks in employment and transfer income for cohorts covered in early adolescence. High school graduation rates provide one explanation why.

[32] That employment rises more than disability assistance falls supports the claim that improved adult health is the main causal channel because even rejected disability applicants are quite unhealthy and work at low levels (Bound 1989). Underlying improvements in activity limitations would therefore tend to increase labor supply among *both* recipients and non-recipients of SSI/SSDI.

[33] The effects come from Medicaid, for which almost all SSI recipients are categorically eligible, and Medicare, which SSDI recipients can receive after two years. 94 percent of non-elderly SSI recipients have Medicaid and 33 percent have Medicare. 33 percent of SSDI recipients have Medicaid and 44 percent have Medicare.

C. Sources of Income

Increases in labor supply mechanically increase earnings, while reductions in transfer program receipt mechanically reduce transfer income. To what extent do these effects offset each other? Figure 7 plots a series of IV coefficients for early eligibility where the dependent variable equals the probability of reporting earnings, transfer income, or total income greater than or equal to x. When $x = 0$, for example, the earnings coefficient measures the probability of any earnings—i.e., annual employment—and the transfer coefficient measures the probability of any transfer income—i.e., public assistance participation. As x moves up, the results trace out Medicaid's effect on the distribution of income by source.

The distributional results provide two important pieces of information about Medicaid's effect. First, the figure shows that increased earnings largely offset reduced transfer income. Medicaid therefore has a larger effect on the composition income than on the amount. Second, the earnings effects are concentrated in the lower part of the distribution. Because income mobility for these cohorts is low (Chetty et al. 2014, Lee and Solon 2009), this lends further support to the claim that the effects are due to Medicaid's treatment of poor children. (Event-study estimates for specific values of x are in Appendix Figure A5.7.)

Table 8 quantifies the effects on average income by source. Column 1 show that, as expected, earned income increases (white: \$646, s.e. = 788; nonwhite: \$1,803, s.e. = 741).[34] Column 3 provides some suggestive evidence that wage changes account for the larger nonwhite earnings estimates. The extensive margin effects almost certainly bias average wages down, and I find that average wages *fall* for white workers (see Appendix Figure A5.8). Nonwhite workers, however, have positive wage effects, despite the likelihood of negative selection. Medicaid has a larger effect on education for nonwhite cohorts leading to higher employment and wages, while it has no detectable effect on education for white cohorts leading to higher employment but not higher wages.

Columns 4 and 5 show that transfer income falls by \$714 for white cohorts (s.e. = 235) and \$260 for nonwhite cohorts (s.e. =119). These magnitudes equal about 6 and 2.5 percent of the average annual DI income (about \$12,000 and \$10,000, respectively). This is close to the participation results in Table 5, suggesting that participation drives the transfer results, not benefit

[34] I follow Chetty et al. (2011) and trim earnings above \$100,000 to match the x-axis on Figure 7.

amounts. I only detect increases in nonwhite total income ($2,116, s.e. = 730).[35] As a result, each year of early Medicaid eligibility reduces nonwhite poverty by 2.68 percentage points (s.e. = 1.07), but has no detectable effect on white poverty (1.21, s.e. = 1.23).

VI. THREATS TO INTERNAL VALIDITY

The results so far have shown that looking across cohorts born between the mid-1950s and mid-1970 reveals a "phase-in" pattern of childhood Medicaid exposure, and nearly identical patterns in adult health improvements, increased labor supply, and lower public assistance receipt. The event-study results show that these changes do not come from pre-existing trends across cohorts. One concern may be that the affected cohorts experienced better childhood conditions before Medicaid began. Appendix Figure A2.1 plots the relationship between $AFDC^*_{rs}$ and cross-cohort trends in indices of health at birth (using Vital Statistics data) and childhood socioeconomic status (using 1940-1970 Census data).[36] Not only do I fail to find evidence of differential trends in pre-Medicaid circumstances, but the point estimates are small enough that even states with the most extreme AFDC rates are not predicted to diverge very much.[37]

Remaining sources of bias, therefore, include factors that changed for the same cohorts exposed to Medicaid sometime between birth and follow-up. Figures 8 and 9 plot IV estimates and 95-percent confidence intervals for early eligibility from alternative specifications that include direct tests for such confounders.

Row 1 reproduces the main estimates from Table 2. Rows 2-4 show results from models that exclude covariates and fixed effects, do not weight by population, and (for the white results in

[35] Because the effects in Figure 7 are essentially differences in (one minus) the CDFs of non-negative random variables, their integral approximates Medicaid's effect on the mean of each income source. Summing each point times $2,000 (the bin width) yields estimates very close to those in Table 8: a $1,723 increase in nonwhite earnings, a $304-dollar reduction in nonwhite transfers, a $2,362 increase in nonwhite income, a $441 increase in white earnings, a $760 reduction in white transfers, and a $164 reduction in white income.

[36] I first regress each index on year dummies interacted with $AFDC^*_{rs}$. Next I regress the index on a linear trend interacted with $AFDC^*_{rs}$ to increase the power of the test to reject the null of no differential (linear) trends. Then I pool all years and test for cross-sectional balance in the level of each index. Figure A2.2 uses data starting from 1950 in order to add percentiles of household income and poverty.

[37] Appendix Table A2.1 presents the results from three additional index-based balance tests. The first uses newly collected data on the incidence and eradication of polio (from the March of Dimes Archives) to show that one of the signature public health achievements of the 20th century, the dissemination of the Salk polio vaccine between 1954 and 1957, was uncorrelated with initial AFDC rates (also see Figure A2.3). The second test uses the 1963 Survey of Health Services Utilization and Expenditures to show that parents' self-reported willingness to seek care and views on the value of medical care are also uncorrelated with initial AFDC rates. Finally, I use data on the quality of dwellings and appliance ownership first available in the 1960 Census to show that home quality and durable goods consumption are also orthogonal to initial AFDC rates. These results all support the claim that variation in initial AFDC rates does not confound pre-existing cohort characteristics.

Figure 8) drop the highest white-AFDC state, West Virginia. The white results depend on the region and Medicaid timing fixed effects, but are nearly identical in the other specifications. The nonwhite labor market results are similar across specifications, but the unweighted results are larger for mortality and smaller for ambulatory difficulty.

The federal government's role in the hospital market grew tremendously in the mid-20th century, and one might expect that these interventions also affected the cohorts covered by Medicaid. The results in row 5 control for the expansion of hospital capacity under the 1946 Hill-Burton Act (Chung, Gaynor, and Richards-Shubik 2016) using per-capita hospital beds during childhood, and interactions between quintiles of pre-Hill Burton hospital capacity and cohort dummies.[38] The hospital controls are jointly significant in all models, but have negligible effects on the main estimates.

An obvious potential source of bias is the coincident roll-out of War on Poverty programs such as Food Stamps (Hoynes, Schanzenbach, and Almond 2012), Head Start (Johnson and Jackson 2017, Ludwig and Miller 2007), or Community Health Centers (Bailey and Goodman-Bacon 2015) that may also confer long-run benefits.[39] Figure 10 plots event-study estimates that use cumulative migration-weighted exposure to FSP, HS, or CHCs as outcome variables. For comparison, I overlay the first-stage relationship for Medicaid eligibility from age 0 to 11. The results provide no evidence that other War on Poverty programs confound the research design. Row 6 of Figures 8 and 9 show that while these variables highly significant as controls they do not appreciably change the main point estimates.

AFDC receipt formed the statutory basis for Medicaid eligibility, so cohorts with higher Medicaid eligibility received more welfare income, but the stability of AFDC rates over time ensures that this was also true of *pre*-Medicaid cohorts. If the distribution of AFDC rates spread out, though, younger cohorts from "high" initial AFDC states could have accumulated relatively more years of expected AFDC receipt. Figure 10 tests this by plotting the relationship between

[38] These measures come from the American Hospital Association's Guide Books. I thank Amy Finkelstein and Heidi Williams for sharing data from before 1975, and Jean Roth and NBER for providing the extracts from after 1975.

[39] I can observe the county-level roll-out of Head Start (HS; National Archives Community Action files), Community Health Centers (Bailey and Goodman-Bacon 2015), and the Food Stamp Program (Almond, Hoynes, and Schanzenbach 2011).[39] I calculate the share of children aged 0-9 (3-4 for HS) by state, race, and year who lived a county with each program, and form a migration-weighted sum this value from age 0 to 9 (ages 3-4 for HS) for each cohort. The resulting variables takes larger values than cumulative Medicaid eligibility because they do not incorporate (unavailable) data on eligibility or participation. To approximate utilization rates and make the exposure variables more similar to m_{rsc}, I multiply them by a 10 percent participation rate. This rate is higher than measured participation rates in this era, and does not affect the sign or significance of the results.

initial eligibility and cumulative migration-weighted AFDC receipt from ages 0 and 9 (available only back to 1948). AFDC exposure is relatively higher for cohorts with more Medicaid eligibility, but the rise began before Medicaid's estimated treatment effects start. Row 7 of Figures 8 and 9 show that controlling for cumulative AFDC exposure does not change the estimates.

Adult outcomes could vary if cohorts became more likely to make advantageous moves. In this case, we should not observe treatment effects within samples of respondents interviewed outside of their birth state (movers) or in it (stayers). In fact, rows 8 and 9 show that treatment effects of Medicaid are apparent for both groups. The effects are generally larger for stayers, especially in terms of employment and transfer receipt. Low-skill workers are less likely to move in response to labor demand (Bound and Holzer 2000), and recent evidence suggests that these flows may be correlated with health (Arthi, Beach, and Hanlon 2017). Therefore, larger treatment effects among stayers are consistent with the notion that Medicaid's effects come from the adults who were most likely to be eligible for Medicaid as children.

While the identification strategy hinges on respondents' birth state, circumstances in adult states could generate biases. Row 10 breaks out the state-of-birth/cohort means by state of residence and adds state-of-residence-by-cohort fixed effects. By limiting comparisons to residents of the same state who were born in the same year but in different states, this model controls non-parametrically for factors such as age-varying effects of state policies, trends in chronic pain and opioid abuse (Case and Deaton 2015), or AIDS incidence. The estimates are generally smaller, but remain statistically significant under this specification (nonwhite employment is an exception). Similarly, the results could also reflect differences in labor demand across states and years. The Great Recession, for example, may have eroded employment among older workers in certain areas, or respondents may report disabilities to justify an unemployment spells (Bound et al. 2003). Row 11 expands the data to the state-of-birth/year-of-birth/state-of-residence/survey-year level and adds interactions between cohort-by-year dummies and the aggregate unemployment rate. I find no evidence that contemporaneous economic conditions explain Medicaid's long-run effects.

The 1970 and 1980 Census asked comparable questions about public assistance receipt and employment, which allows a falsification test where I assign the Medicaid eligibility variables to untreated cohorts born 20 or 30 years before my main estimation sample, while measuring their outcomes during the same adult ages as in the main sample. Rows 12 and 13 show no effects of these false Medicaid eligibility variables. Age profiles of employment and transfer receipt were not correlated with AFDC for pre-Medicaid cohorts. (Corresponding event-study estimates are in Appendix Figure A2.4.)

Appendix 2 reports additional falsification tests based on adult income and country of birth. Motivated by the null results for the probability of having high earnings (Figure 7), Appendix Figure A2.5 and Table A2.2 cut the sample by adult income and show that the effects are almost entirely driven by low-income adults.[40] Appendix Figure A2.6 and Table A2.3 show no relationship between initial AFDC rates and cross-cohort outcome changes for a sample of foreign-born adults who arrived in the US after age 11 and therefore had no childhood Medicaid exposure (Almond and Chay 2006). Note that this addresses potential confounders that arise in later childhood or after and affect native and foreign-born people in the same way, not early childhood confounders (which foreign-born and native respondents do not share).

VII. Discussion: Medicaid's Benefits and Costs

The results above show that Medicaid's introduction had large effects on adult health, labor supply, and program participation. I quantify these benefits using two measures: recipients' willingness to pay for Medicaid and the government's return on its direct Medicaid outlays.[41] Appendix 8 uses a stylized two-period model to derive Medicaid's welfare effects and costs net of savings that come from behavioral changes (fiscal externalities). Medicaid affects social welfare by improving morbidity and longevity, represented by the present discounted value of the number of quality-adjusted life years (QALYs) saved.[42] Reductions in transfers linked to health subtract from welfare dollar-for-dollar, but earnings increase that result from higher wages (as opposed to labor supply)

[40] I cut the white samples into three groups: $0-$39,999; $40,000-$99,999; and $100,000+. The magnitude of the main results are always largest in the poorest group and smallest and never statistically significant in the richest group. Because of sample size concerns I cut the nonwhite sample at $40,000. The results are larger for the poorer group, but I do find some evidence that nonwhite adults with earnings over this threshold (many of whom likely did receive Medicaid as children) experience health improvements and increased employment.

[41] The ratio of the two is the marginal value of public funds (MVFP) (Hendren 2016, Kleven and Kreiner 2006, Mayshar 1990), but I focus on them separately because I find that Medicaid has already paid for itself which makes the MVPF infinite. Hendren and Sprung-Keyser (2019) find that later child Medicaid expansions also have an infinite MVPF. By contrast, Finkelstein, Hendren, and Shepard (2019) calculate short-run MVPFs between 0.28 and 1.29 for subsidized insurance plans in Massachusetts.

[42] Zeckhauser and Shepard (1976) first defined a QALY and argued for its use in evaluations of the (then) new Medicaid program, saying *"the real health benefits brought to the poor by Medicaid programs, for instance, are now being closely examined…massive wastage [will] almost certainly result from a failure to quantify the benefits expected from a program." (pp 18)*

increase it.[43] The government's return on Medicaid spending equals the ratio of fiscal externalities (increased tax revenue, lower benefit payments, and changes in net payments due to increased survival) to direct outlays.

A. Quality-Adjusted Life Years (QALYs): Value of Health Benefits

For each cohort, the number of QALYs discounted to 1965 equals:

$$QALY_{rsc} = P_{rsc} \sum_t \frac{p_{rsct} q_{rsct}}{(1+R)^{t-1965}} = P_{rsc} \sum_t \frac{q_{rsct}}{(1+R)^{t-1965}} \left[1 - \prod_{\tau=c}^{t} (1 - M_{rsc\tau}) \right]$$

I assume a subjective discount rate, R, of 3 percent. p_{rsct} is the annual survival probability which is a function of annual mortality rates, $M_{rsc\tau}$. For 1950-1979, before the mortality files record decedent's state of birth, I assign cohorts the mortality rate in their state of residence. For 1980-2016, I use the mortality data described above. I calculate counterfactual mortality rates (and therefore survival rates, p^0_{rsct}) using the treatment effects on infant and age 1-4 mortality from Goodman-Bacon (2018c) for 1966-1979, and on non-AIDS mortality from 1980-2016. I assume that the index value of a year of life, q_{rsct}, equals one minus the share that reports either a work disability (1980-1990 Census) or an ambulatory difficulty (2000 Census, 2001-2017 ACS).[44] By this measure, minors are in perfect health and the index value falls to about 0.86 between ages 51 and 60. I calculate counterfactual disability rates (and therefore quality indices, q^0_{rsct}) by subtracting the negative treatment effects reported in Table 2. Total QALYs per cohort equals cohort size at birth, P_{rsc}, times the average discounted stream of QALYs per member. I calculate counterfactual QALYs in the same way, but using p^0_{rsct} and q^0_{rsct} for the survival and quality of life parameters.

B. Direct Costs: Medicaid Spending on Sample Cohorts

Data on total expenditures between 1966 and 1975 come from Goodman-Bacon (2018c). I calculate the share of child Medicaid recipients in the CPS (Flood et al. 2015) born before 1976 for each calendar year between 1976 and 1993 (when the 1975 cohort was 18), and multiply this

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[43] The envelope theorem ensures that changes in labor supply do not change utility and therefore earnings that arise from labor supply changes do not affect willingness to pay. (Hendren and Sprung-Keyser 2019) discuss how changes in the return to work do increase utility, though, and are part of individual willingness to pay.

[44] Similar shares report each type of disability in the 2000 Census. The quality values are higher than those in Cutler and Richardson (1999), for example, but this has only a small effect on QALYs saved through increased longevity.

by annual child Medicaid spending (CMS 2013 table 13.10).[45] Undiscounted real costs for these cohorts are about $149 billion (in 2017 dollars), but $92 billion when discounted to 1965.[46]

C. Fiscal Externalities: Public Savings from Medicaid's Treatment Effects

Fiscal externalities include the changes in transfer spending, tax payments, and the net public cost of increased longevity. Each year of early childhood eligibility reduces cash transfer income by $714 for white cohorts and $260 for nonwhite cohorts, which for cohort sizes of 56 million (white) and 12 million (nonwhite) and average early eligibility of 0.42 years (white) and 2.38 years (nonwhite) implies an annual savings of $24.2 billion (56 million*0.42*-714 + 12 million*2.38*-260).[47] Reductions in public insurance participation also represent an important source of savings. Per-enrollee expenditures are very high for disabled recipients of Medicaid ($16,643; Kaiser Family Foundation 2012) and Medicare ($10,495; CMS 2013 table 3.6), but they are also strongly influenced by the right tail of spending. The *median* SSDI recipient on Medicare, for example, spends between $2,000 and $5,000 (and is probably on the lower end of this range since more than 47% of recipients spend under $2,000), and the average spending within that category is $3,326. This suggests that childhood Medicaid lowers current public medical costs by about $7 billion per year (56 million*0.42*$3,326*-0.045+12 million*2.38*$3,326*-0.037).

I use NBER's Taxsim 9.0 (Feenberg and Coutts 1993) to estimate changes in tax revenue. Following Agrawal and Hoyt (2016), I allocate family-level tax liability across people according to their share of income. Each year of early childhood eligibility increases average annual white tax liability by just $89 (s.e. = 281), but increases nonwhite tax liability by $279 (s.e. = 129). The distribution of tax liability, however, spreads out because of the increase in extensive margin labor supply (see Appendix Figure A5.9). Households with a counterfactual tax bill of zero either owe positive taxes or receive large EITC refunds. Recent work finds reductions in EITC payments (Brown, Kowalski, and Lurie 2014), which can occur if Medicaid moves earnings above the plateau range of about $18,000 or reduces fertility in a way that shifts down the EITC schedule. Changes in extensive margin labor supply, however, are consistent with higher EITC payments.

[45] I interpolate the share from 1 in 1975 to the observed 1980 value, the first year the CPS asks about Medicaid.

[46] Alternatively, the Office of Management and Budget compound past nominal outlays into 2017 present-value terms using nominal treasury rates. Because treasury rates were so high in the 1980s, this yields the present value of direct outlays of $445 billion.

[47] The populations used for fiscal externalities are smaller than for QALYs because I estimate fiscal externalities beginning in 2000, when about 10 percent of cohort members had already died.

Multiplying the revenue results by the total years of early childhood eligibility gives an annual increase in tax revenue of $10 billion.

I estimate that those induced to survive because of Medicaid would likely have received slightly more in transfers than they paid in taxes. Comparing tax bills to transfer income among Census respondents with income below 200 percent of the federal poverty line shows a discrepancy of -$2,271 and -$1,438 for white and nonwhite respondents. Multiplying by the difference in survival yields an annual cost of about $374 million.

The implied $40.8 billion in annual (undiscounted) savings represents a large share of the cost of covering cohorts born before 1976. The effects on transfer income, which have not previously been studied in the context of long-run Medicaid effects, account for about two-thirds of this amount. Discounting these savings to 1965 implies a total fiscal externality of $200 billion.

D. Willingness to Pay

Table 9 shows that Medicaid's introduction saved over 10 million discounted QALYs. These are about evenly divided between white and nonwhite cohorts. Without Medicaid the cohorts born between 1955 and 1975 would have experienced about 1.6 billion QALYs. Medicaid therefore increased the number of QALYs by about 0.6 percent. White and nonwhite cohorts get about the same benefit, though, which implies a large proportional increase in lifetime health for nonwhite people (1.7 percent) than white (0.3 percent). The top of Table 9 also shows quantiles of the empirical distribution of QALYs saved from 1,000 draws of a block bootstrap. I resample states, re-estimate the early eligibility IV coefficients for mortality, disability, public insurance, cash transfer income, and tax payments, and recalculate the number of QALYs saved. 95 percent of draws yield gains of more than 5 million QALYs.

Childhood Medicaid recipients value these health improvements by at least $64 billion. A standard value of a statistical life-year is $100,000 (Cutler 2004). Because Medicaid recipients consume less than the average family, they have a higher marginal utility of consumption and are in theory less willing to trade consumption for QALYs. Finkelstein, Hendren, and Luttmer (2019) assume a CRRA utility function in consumption (quasi-linear in health) with a coefficient of relative risk aversion of 3:

$$u(c, QALY) = \frac{c^{1-\sigma}}{1-\sigma} + \tilde{\phi}QALY$$

This implies a marginal rate of substitution between health and consumption of $\tilde{\phi}/c^{\sigma}$. They find that adult Medicaid recipients consume 40 percent as much as the general population, and so value health 6.4 percent (0.4^3) of the average valuation ($100,000). This implies $6,400 per QALY and a total valuation of the health benefits from Medicaid's introduction of $65 billion; over half of its gross cost. Under these assumptions, a risk aversion parameter of 2.6 equates the value of Medicaid's health benefits with its direct costs ($92 billion), and a risk aversion parameter of 1 (log utility) yields a valuation of $406 billion. If Medicaid population values a QALY half as much as the average ($50,000), the total value of health benefits is $507 billion; roughly the current annual cost of the entire Medicaid program. Note that these rough calculations do not account for the fact that parents in the 1960s were the ones making health decisions on behalf of their children.

In addition to health benefits, recipients also value the way that childhood Medicaid coverage changes their income. Table 9 shows that transfer income falls by $154 billion, which directly subtracts from well-being. Using the lowest QALY valuation ($64 billion) and subtracting all of the reduction in transfer income ($119 billion in cash and $34.6 billion in in-kind benefits) suggests a loss of $90 billion. A QALY valuation of about $15,000 equates the health benefits with transfer income reductions.

To the extent that increases in earnings come from wages as opposed to labor supply, however, recipients value this income as well (Hendren and Sprung-Keyser 2019). I do not find evidence of higher wages for white cohorts, but I do find higher educational attainment and wages for nonwhite cohorts, who gain $245 billion in discounted earnings. At the lowest QALY valuation just one-third of these earnings gains need to come from wages to zero out the willingness to pay. If three-quarters of nonwhite earnings growth (0.75*245 = 184) came from wages, then willingness to pay for Medicaid exceeds its gross cost (184-90 = $94 billion).

E. Return on Direct Outlays

I find that from the point of view of a 1965 policymaker, Medicaid has saved more than twice the cost of extending childhood coverage to the cohorts born before 1976. In addition to the $154 billion reduction in transfer spending, the government gains $48 billion in tax revenue and pays an additional $2.3 billion because of increased longevity. The ratio of savings to direct costs is 2.17. The empirical distribution of these savings shows that 91 percent of draws have a positive return, 85 percent have a return that exceeds 0.5, and 75 percent have a return that exceeds 1. The net "cost" of providing $1 of Medicaid in this era is therefore -$1.17.

Brown, Kowalski, and Lurie (2014) conclude that the 1980s expansions will save 56 percent of their costs after 60 years. I find larger returns for Medicaid's introduction mainly because I document large public savings on cash transfer programs, which are not available in administrative tax data. Another reason is that Medicaid's introduction covered much poorer children than the 1980s expansions did, and these groups may have higher returns.

VIII. CONCLUSION

This paper uses the original introduction of Medicaid combined with historical variation in welfare-based Medicaid eligibility across states to provide evidence on the effect of childhood insurance coverage on adult outcomes. I find that the cohorts covered by Medicaid as young children in the 1960s and 1970s, grow up to be healthier adults who work more and receive public assistance less often. These cohorts were not, however, differentially well off in childhood nor did they experience different underlying trends in early-life health or exposure to related public programs from the 1960s (Goodman-Bacon 2018c). Medicaid added 10 million quality adjusted life-years for cohorts born between 1955 and 1975 and saved the government more than twice its original cost. Childhood Medicaid coverage, especially for the program's poorest recipients, deliver large benefits later in life to both covered individuals and to the government.

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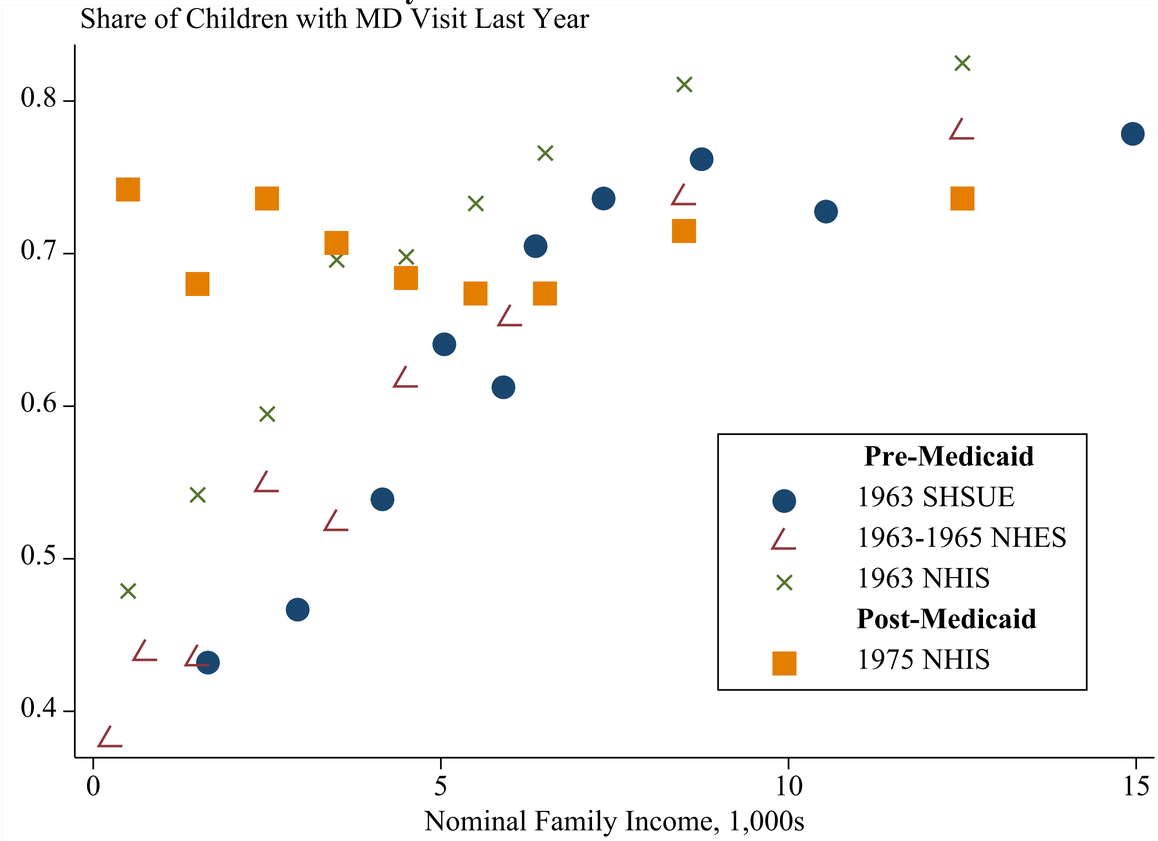
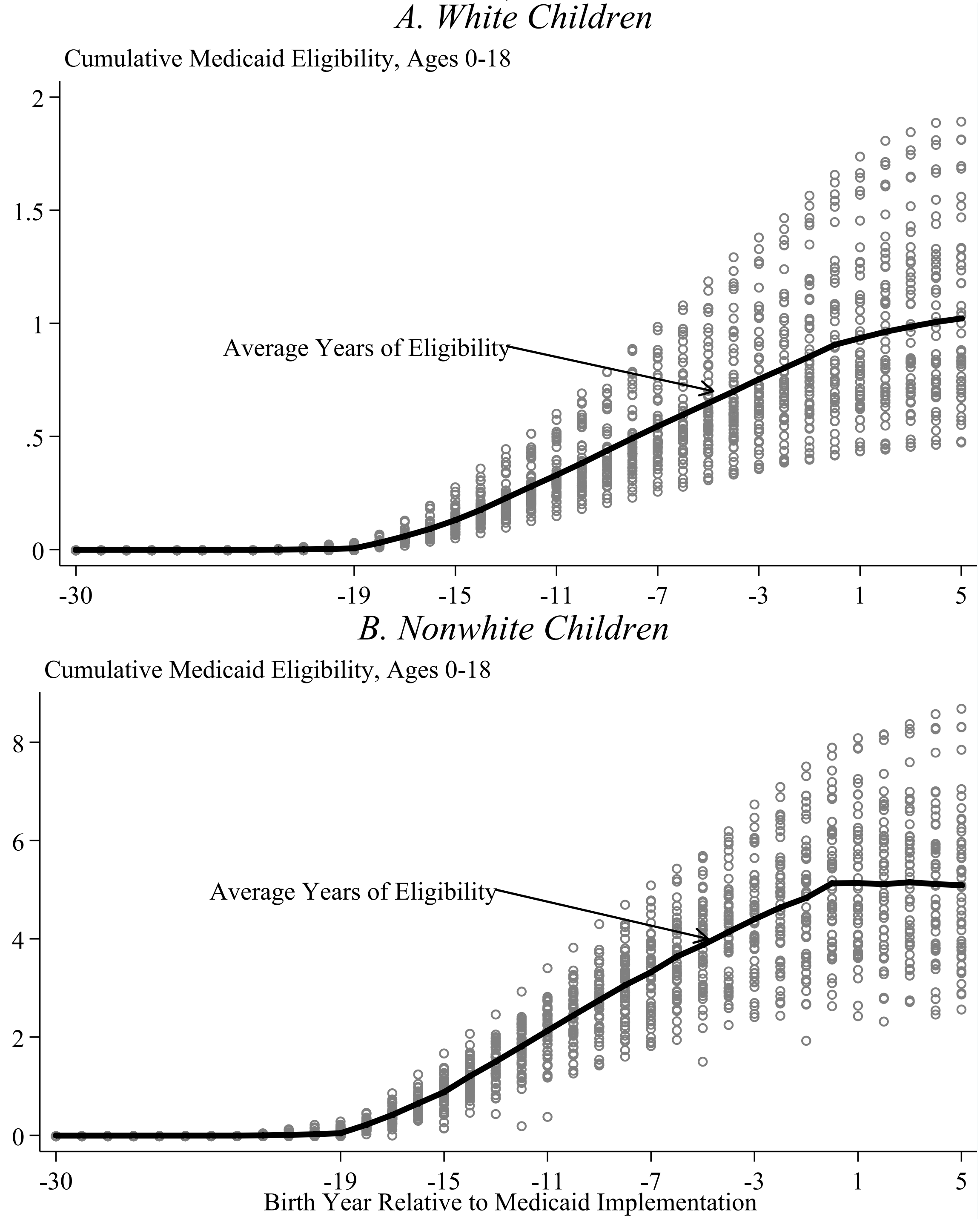
Figure 1. Health Care Use Increased for the Poorest Children After Medicaid: Family Income and the Probability that Children Saw a Doctor in the Previous Year



Notes: The figure plots the share of children who report having seen a doctor in the previous year in four survey data sources: the 1963 Survey of Health Services Utilization and Expenditure (CHAS 1988), the 1963-1965 National Health Examination Survey (ICPSR); and the 1963 and 1975 National Health Interview Surveys (NHIS). In all but the SHSUE, family income is reported as the median value of each bracket in which total family income is reported. In the SHSUE, it is the mean value within each decile. For scale, only bins less than or equal to $15,000 are plotted (income is measured in nominal dollars; the poverty line for a family of four is between $3,000 and $5,000). By this measure, income ceases to be a significant predictor of any annual doctor visit after Medicaid was implemented. The univariate regression slopes associated with these cell means are 0.027 (s.e. = 0.006) in the SHSUE, 0.027 (s.e. = 0.003) in the NHES, 0.029 (s.e. = 0.005) in the 1963 NHIS, and 0.0029 (s.e. = 0.002) in the 1975 NHIS. Given the clear nonlinearity in the pre-Medicaid years, the slopes on the observations of family income under $10,000 have the same pattern but are about twice as large (except for the 1975 slope: -0.004, s.e. = 0.004).

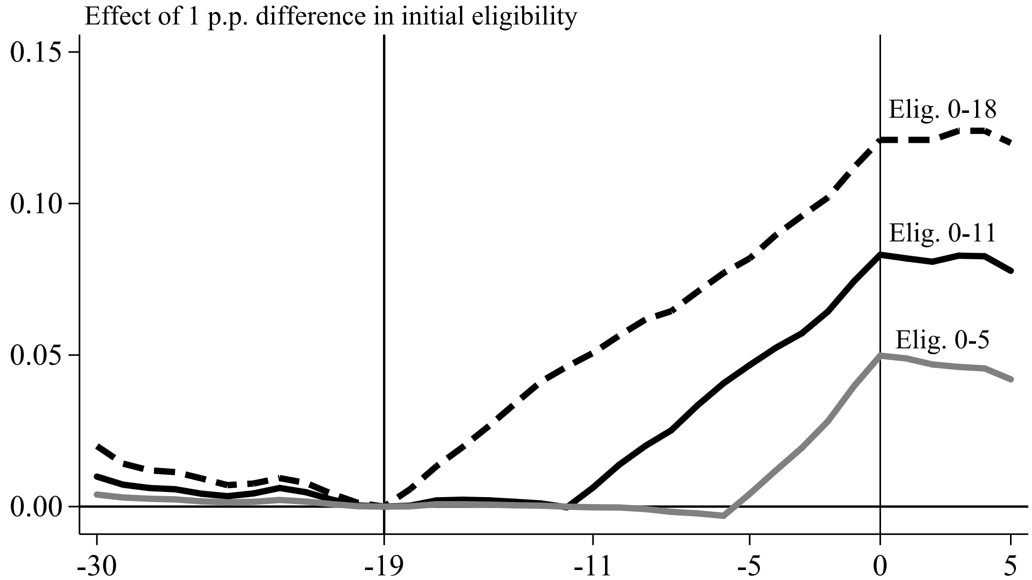
Figure 2. Cumulative Childhood Medicaid Eligibility by State of Birth, Event Cohort, and Race



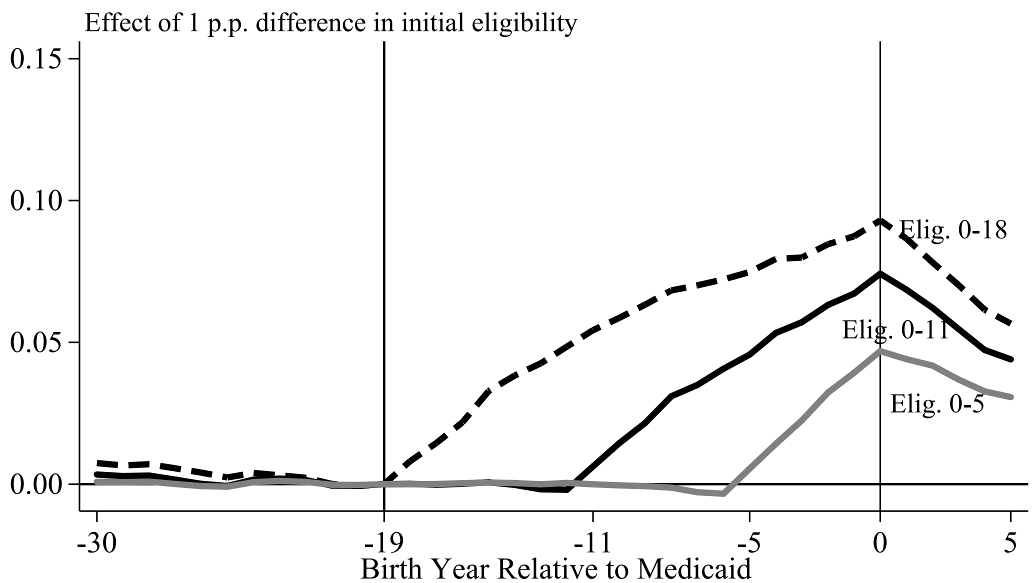
Notes: The figure plots cumulative childhood Medicaid eligibility for each state and event cohort (relative to Medicaid's introduction). Equation (2) defines cumulative eligibility, which comes from observed AFDC rates, the existence of Medicaid, and cohort migration patterns. The solid line equals average eligibility, and the dashed lines equal average eligibility in states with above- or below-median initial AFDC rates.

Figure 3. Initial Categorical Eligibility Predicts Cumulative Eligibility: First-Stage Relationship Between AFDC* and Expected Years of Medicaid Eligibility by Race

A. White



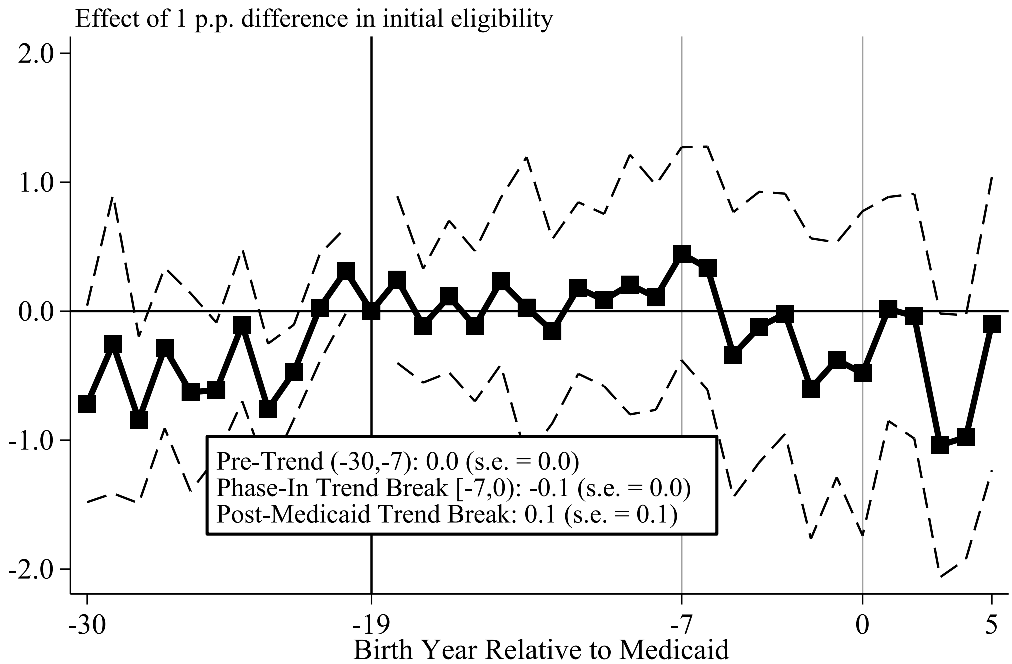
B. Nonwhite



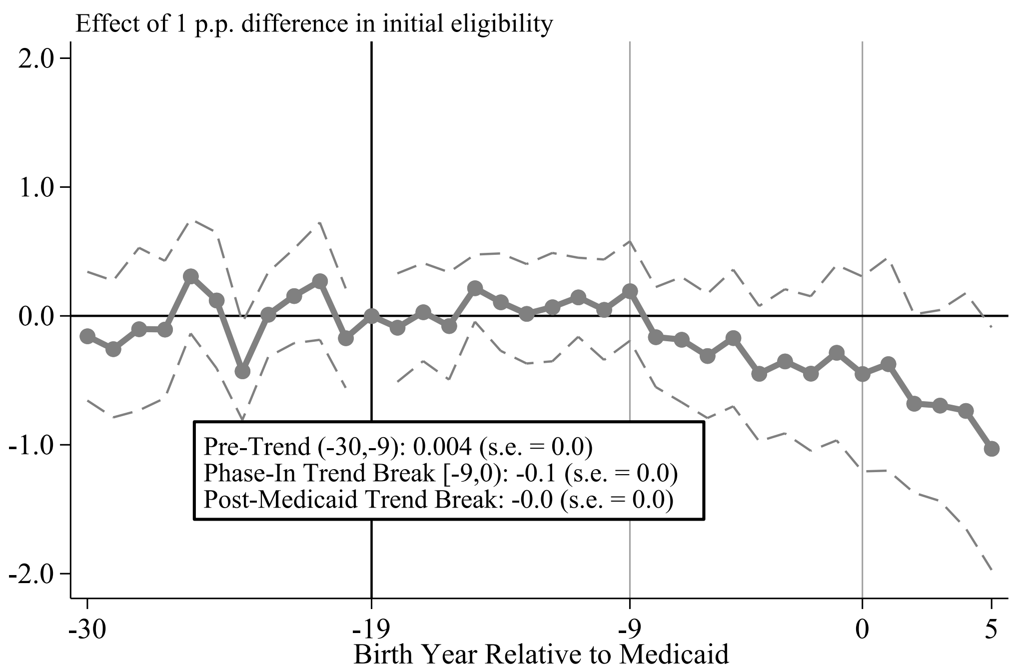
Notes: The dependent variable is each cohort's cumulative, migration-adjusted Medicaid eligibility for ages 0-18. The figure plots the estimated coefficients on interactions between $AFDC_{rs}^{*}$ and event-time dummies for each of 30 years before and five years after Medicaid. Time -19 is omitted. The dataset includes one observation per state/year cohort because childhood eligibility is determined by age 18. The model includes birth-state, region-by-birth-year, and Medicaid-year-by-birth-year fixed effects; birth year per-capita income and general fertility rate. The dashed lines are 95-percent confidence intervals based on standard errors clustered by birth state. While the above/below median differences in eligibility in Figure 2 are larger for nonwhite than white cohorts, the effect per point of the AFDC rate is smaller because the cross-state differences in nonwhite AFDC rates are much larger than for white AFDC rates.

Figure 4. Early Childhood Eligibility Lowers Adult Mortality: Event-Study Estimates of Medicaid's Effect on log 37-Year Non-AIDS Mortality Rates (coefficients × 100)

A. White



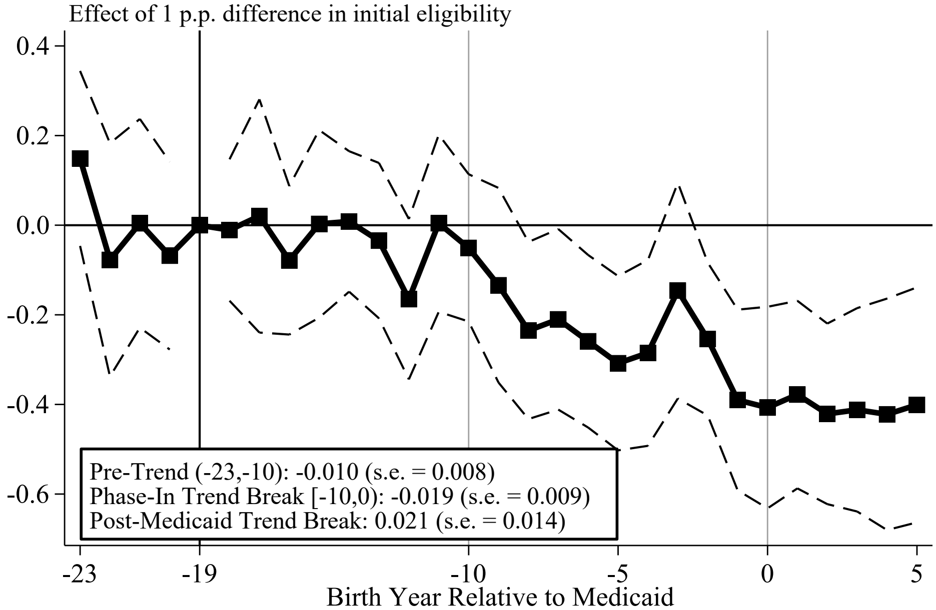
B. Nonwhite



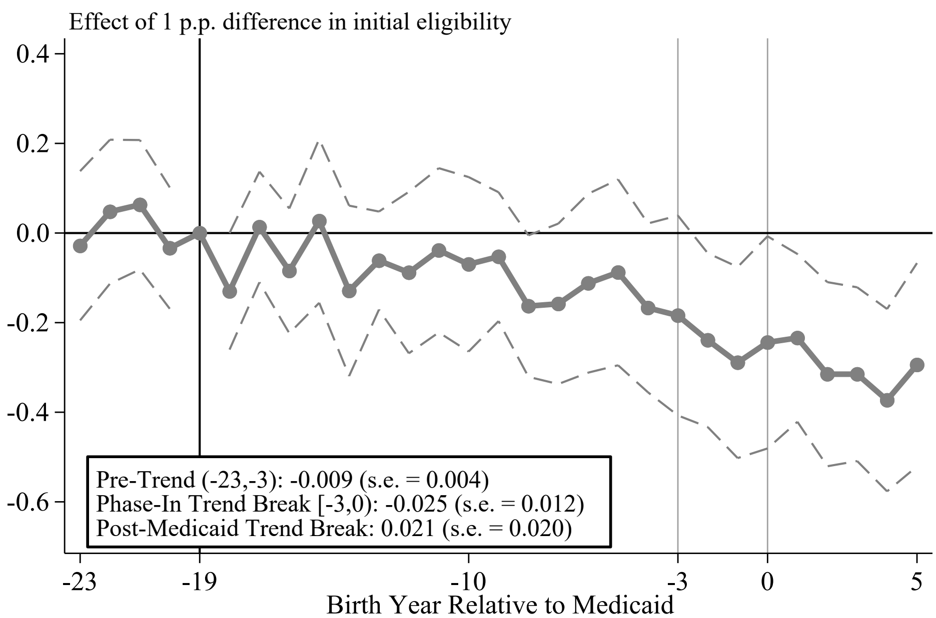
Notes: The figure plots the estimated coefficients on interactions between $AFDC^{*}_{rs}$ and event-time dummies for each of 30 years before and five years after Medicaid. Time -19 is omitted. The model includes birth-state, region-by-birth-year, and Medicaid-year-by-birth-year fixed effects; birth year per-capita income and general fertility rate. The nonwhite estimates also adjust for a linear trend interacted with $AFDC^{*}_{rs}$ for event-times prior to -15. Estimates are weighted by the 1980 population. The dashed lines are 95-percent confidence intervals based on standard errors clustered by birth state. Source: Ruggles et al. (2010), United States Department of Health and Human Services (2009).

Figure 5. Early Childhood Eligibility Lowers Adult Disability: Event-Study Estimates of Medicaid's Effect on Rates of Ambulatory Difficulty by Race (coefficients×100)

A. White

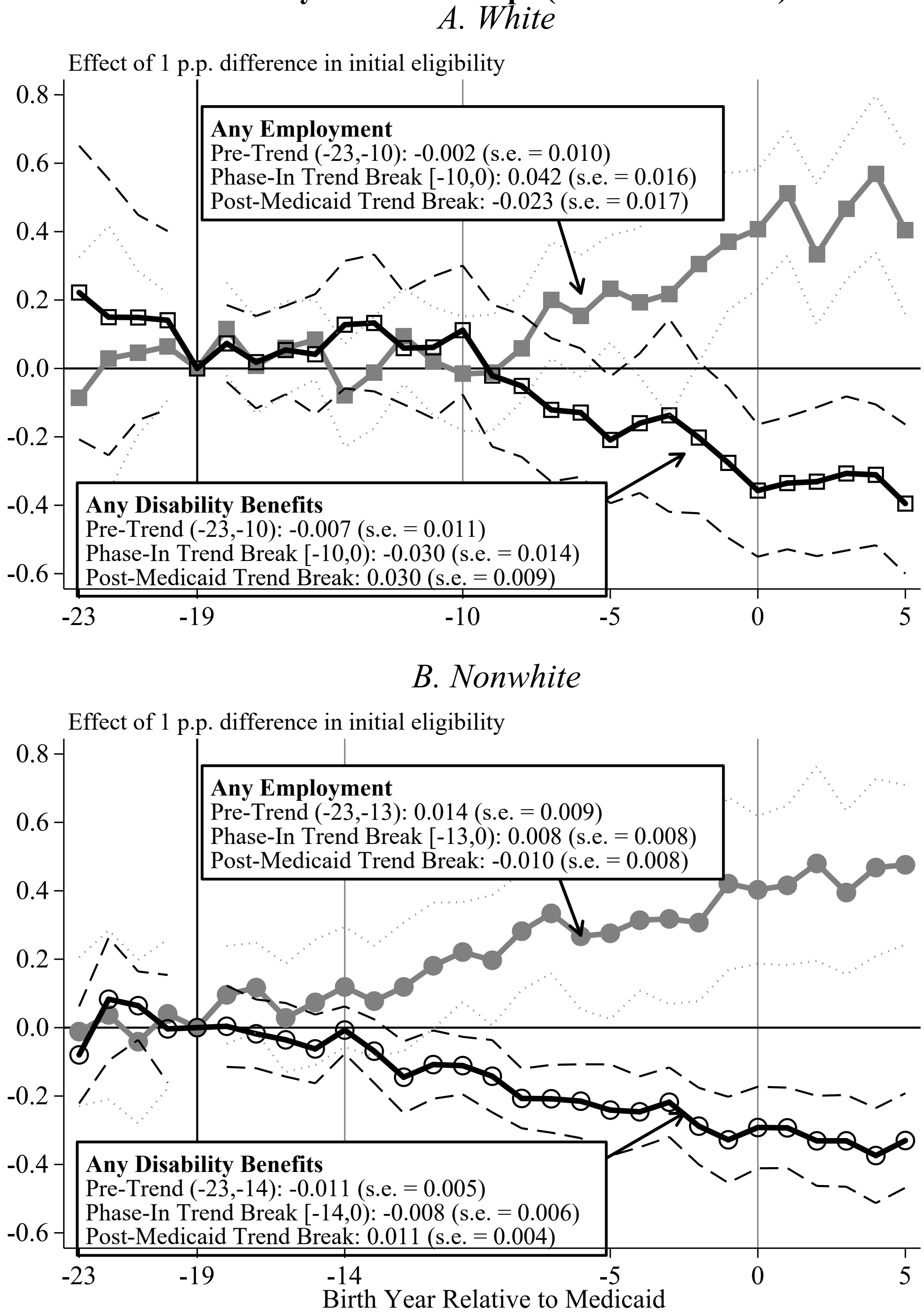


B. Nonwhite



Notes: The dependent variable is the share of respondents in each state-of-birth-by-cohort cell who report having a "long-lasting condition that substantially limits one or more basic physical activities such as walking, climbing stairs, reaching, lifting, or carrying" (ambulatory difficulty). The sample includes Census/ACS years 2000-2007, when the question text was comparable. The figure plots the estimated coefficients on interactions between $AFDC^{*}_{rs}$ and event-time dummies for 23 years before and five years after Medicaid. Time -19 is omitted. The model includes birth-state, region-by-birth-year, and Medicaid-year-by-birth-year fixed effects; birth year per-capita income and general fertility rate. The nonwhite estimates also adjust for a linear trend interacted with $AFDC^{*}_{rs}$ for each Medicaid year for event-times prior to -15. Estimates are weighted by the sum of the Census weights in each cell. The dashed lines are based on standard errors clustered by birth state. The trend break points come from maximizing the F-statistic on the three trend terms that use different break points from -22 through -2. A plot of these F-statistics is in Appendix Figure A4.1.

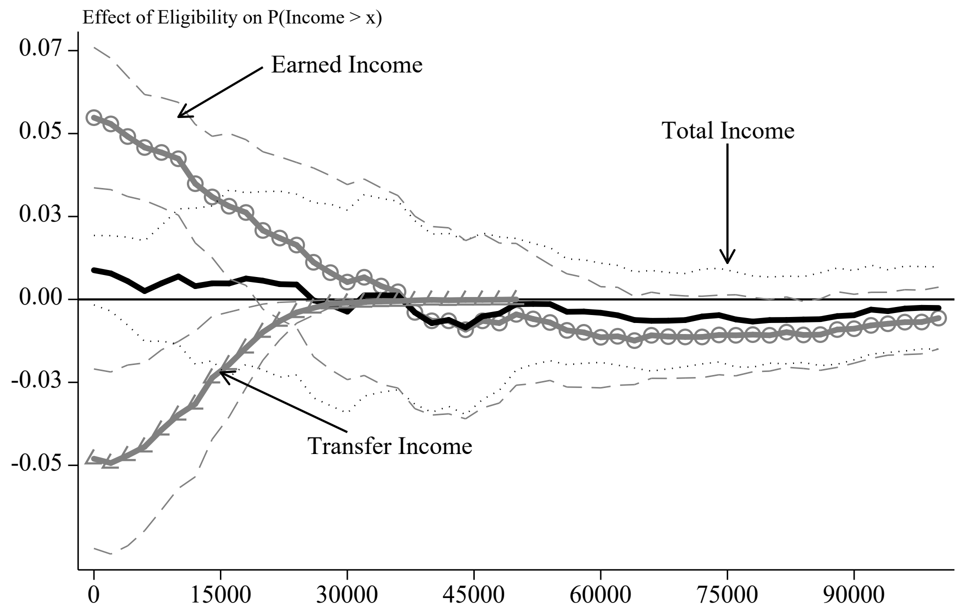
Figure 6. Early Childhood Eligibility Lowers Disability Transfer Receipt and Increases Employment: Event-Study Estimates of Medicaid's Effect on Rates of Employment and Disability Benefit Receipt (coefficients×100)



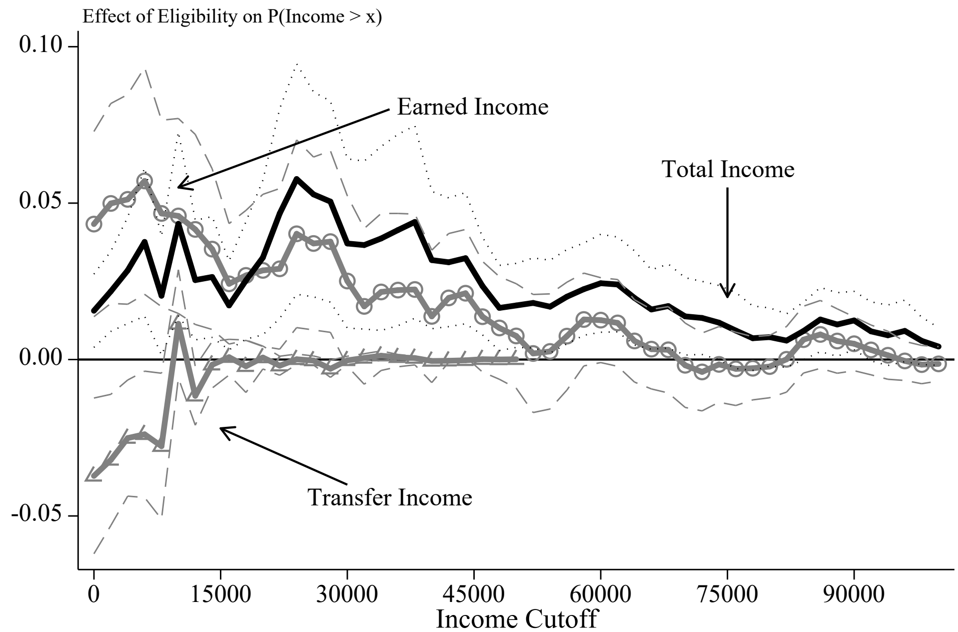
Notes: The dependent variable is the share of respondents in each state-of-birth-by-cohort cell who report having any annual employment (closed triangles) or receiving income from a disability-related transfer program such as SSI or SSDI (open squares). The estimation sample includes Census/ACS years 2000-2015. Because these questions are comparable over time, Appendix Figures A2.4 and A6.2 presents similar results using the 1980 and 1990 Census, which allows for a 45-year pre-trend (not all covariates are available for these cohorts). The estimates are nearly identical, and neither employment nor disability benefit receipt exhibit trends correlated with initial AFDC for cohorts born as early as 1920. For details on the specification, see text and notes to Figure 5. The nonwhite estimates also adjust for a linear trend interacted with $AFDC^{*}_{rs}$ for each Medicaid year for event-times prior to -15.

Figure 7. Early Childhood Medicaid Eligibility Shifts Income from Benefits to Earnings: Instrumental Variables Estimates on the Distribution of Income by Source

A. White

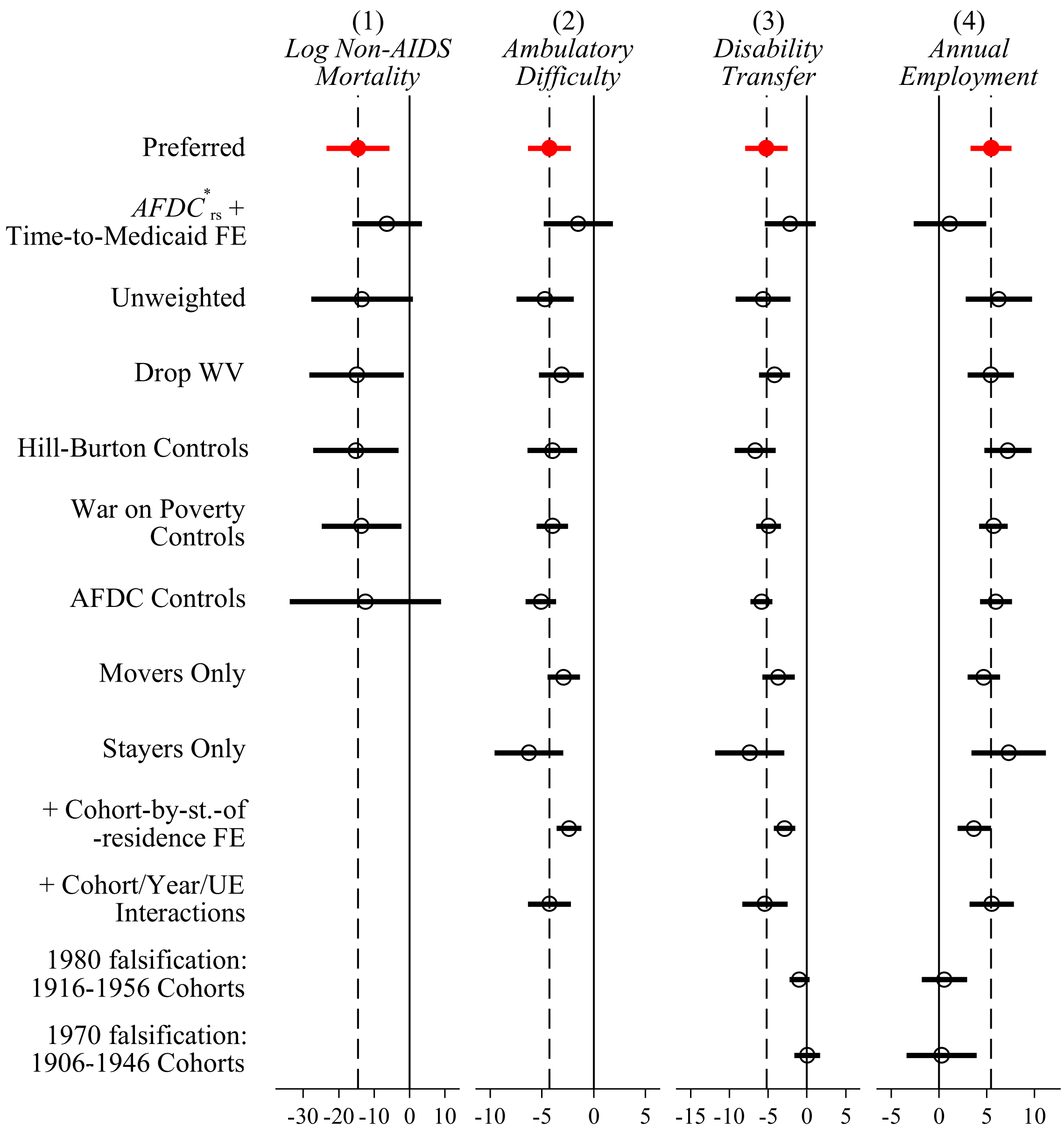


B. Nonwhite



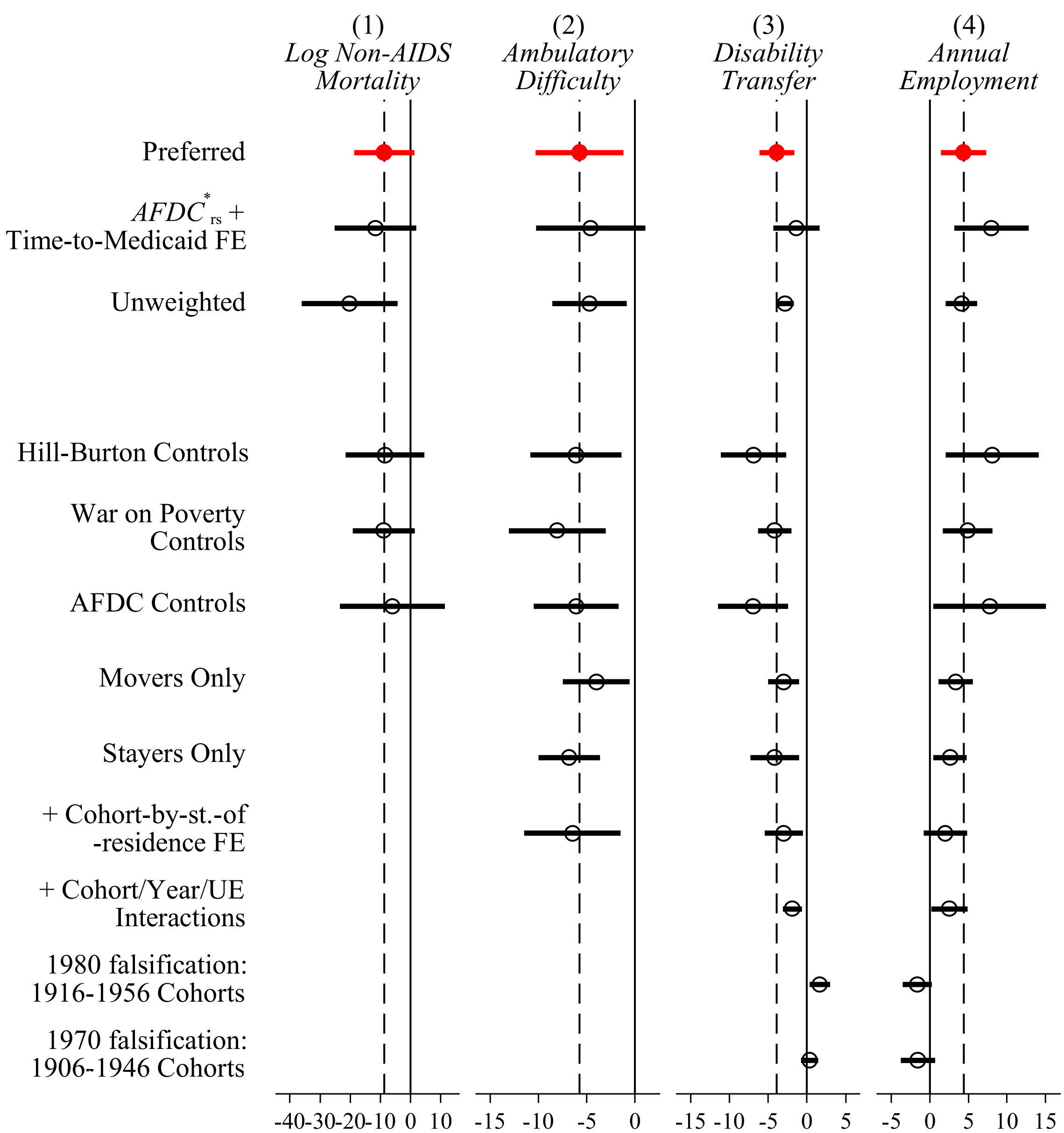
Notes: The figure plots instrumental variables estimates of the effect of cumulative Medicaid eligibility at ages 0-11 on the probability of earnings, transfer income, or total income greater than the amount on the *x*-axis (measured in \$2,000 bins in 2012 dollars). The sample includes Census/ACS years from 2000 to 2017. \$50,000 is the maximum of the transfer income variable.

Figure 8. Effects for White Cohorts on Health and Labor Market Outcomes are Robust to Alternative Specifications, Samples, and to Falsification Tests on Older Cohorts



Notes: The figure plots IV point estimates and confidence intervals (based on standard errors clustered by birth state) for early childhood Medicaid coverage. Row 1 includes only $AFDC^{*}_{rs}$ and event-cohort dummies as controls. Row 2 is the preferred specification. Row 3 is an unweighted version of row 2. Row 4 drops the highest white AFDC state, West Virginia. Row 5 controls for average per capita hospital beds from age 0-9, and interactions between quintiles of per capita hospital beds from 1936-1940 (prior to the 1946 Hill-Burton Hospital Construction Act) with cohort fixed effects. Row 6 includes cumulative exposure to Food Stamps, Head Start, and Community Health Centers. Row 7 controls for cumulative AFDC exposure (available starting in 1953). Rows 8 and 9 keep respondents living out of (movers) or in (stayers) their birth state. Row 10 expands the data to the state-of-residence/state-of-birth/cohort level and includes fixed effects for cohort by state of residence. Row 11 expands the data to the state-of-residence/state-of-birth/cohort/survey-year level and includes interactions between cohort, survey-year, and the unemployment rate. Rows 12 and 13 assign treatment variables for the 1936-1976 cohorts to the 1916-1956 cohorts (for 1980 data) and to 1906-1946 cohorts (for 1970 data). The these models use any cash transfer instead of disability related transfers.

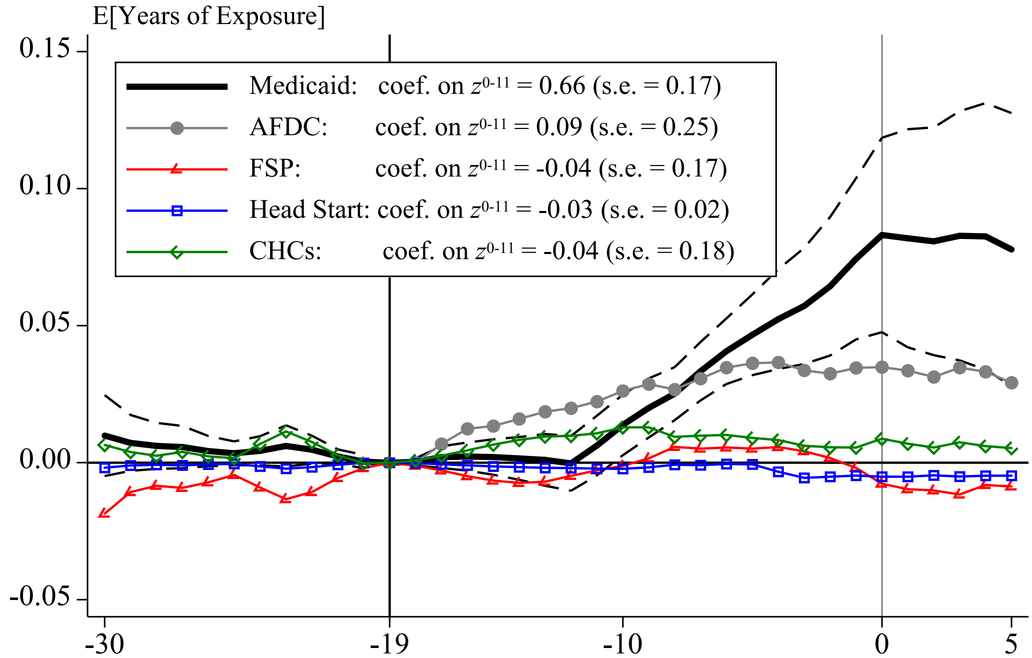
Figure 9. Effects for Nonwhite Cohorts on Health and Labor Market Outcomes are Robust to Alternative Specifications, Samples, and to Falsification Tests on Older Cohorts



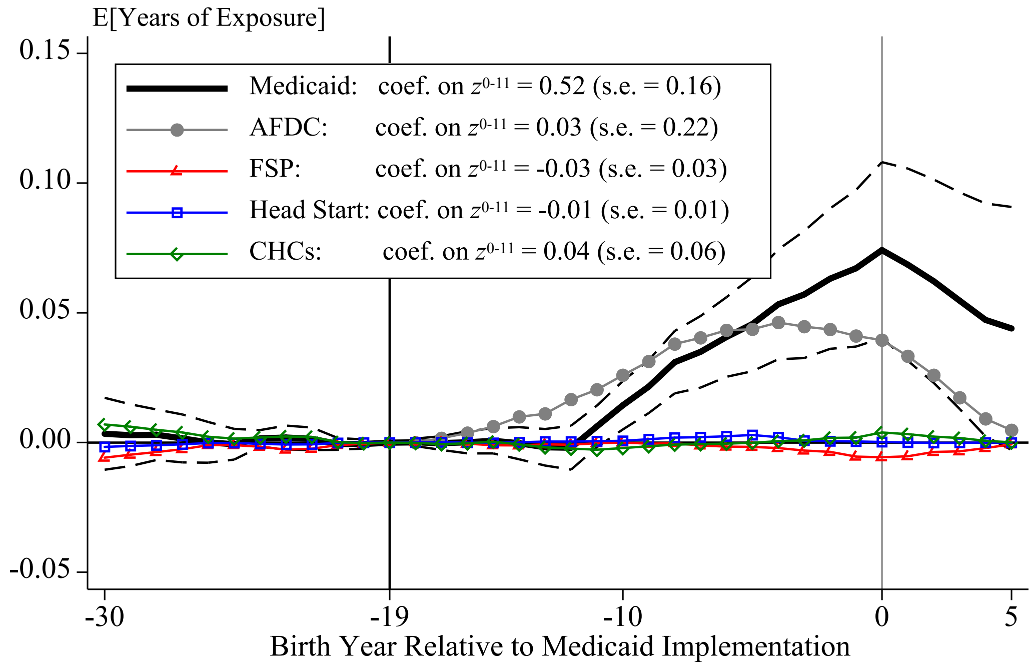
Notes: See notes to Figure 8. Row 4 is blank because West Virginia is not an outlier for nonwhite $AFDC_{rs}^{*}$.

Figure 10. Early Childhood Eligibility is Not Correlated with Cumulative Exposure to Other Safety Net Programs

A. White



B. Nonwhite



Notes: Data on the race of AFDC recipients are available starting in 1948. Cumulative AFDC exposure follows equation (2) but without the post-Medicaid dummy. Data on Food Stamp implementation comes from Almond, Hoynes and Schanzenbach (2011). Data on Head Start and Community Health Centers come from the National Archives Community Action Program (NACAP) files and National Archives Federal Outlays (NAFO) file (see Bailey and Goodman-Bacon 2015). I calculate the share of children (ages 0-9) of each race in each state who live in a county with either FSP, CHC, or HS (ages 3 and 4). I weight these state-by-year variables together using the migration adjustment from equation (2) and sum over each cohort's first 9 years (ages 3 and 4 for HS).

Table 1. First-Stage Relationship between Predicted Eligibility and Migration-Adjusted Cumulative Medicaid Eligibility

	(1)	(2)	(3)	(4)
	Cumulative Eligibility, Ages 0 -18	*Cumulative Eligibility, Ages 0 -5*	*Cumulative Eligibility, Ages 6 - 11*	*Cumulative Eligibility, Ages 12 -18*
		A. White		
Predicted Eligibility at:				
Ages 0-18	0.61			
	[0.16]			
Ages 0-5		0.83	-0.14	0.07
		[0.20]	[0.14]	[0.14]
Ages 6-11		-0.07	0.72	-0.16
		[0.05]	[0.11]	[0.16]
Ages 12-18		0.00	-0.03	0.64
		[0.02]	[0.06]	[0.11]
Mean Eligibility|Any	0.66	0.22	0.26	0.35
F-statistic	15.1			
Kleibergen-Paap F-statistic		24.2	19.23	9.48
		B. Nonwhite		
Ages 0-18	0.38			
	[0.17]			
Ages 0-5		0.71	-0.42	-0.26
		[0.16]	[0.16]	[0.13]
Ages 6-11		-0.03	0.79	-0.34
		[0.05]	[0.11]	[0.18]
Ages 12-18		-0.01	-0.03	0.71
		[0.02]	[0.05]	[0.12]
Mean Eligibility|Any	3.59	1.29	1.40	1.70
F-statistic	5.2			
Kleibergen-Paap F-statistic		17.4	13.9	7.1

Notes: Column 1 presents first-stage estimates for the effect of predicted childhood Medicaid eligibility, z_{rsc}, on actual, migration-adjusted cumulative childhood Medicaid eligibility, m_{rsc}. Columns 2 through4 present similar first-stage estimates that split eligibility into sub-periods: ages 0-5, 6-11,and 12-18. F-statistics that measure the strength of the age-specific instruments for each eligibility variable are presented for these regressions (Angrist and Pischke 2009).

Table 2. Instrumental Variables Estimates of the Effect of Cumulative Medicaid Eligibility on Adult Health and Labor Market Outcomes

	(1) log Non-AIDS Adult Mortality	(2) Ambulatory Difficulty	(3) Disability Transfer Receipt	(4) Annual Employment
	A. White Estimates by Age of Eligibility			
Medicaid Eligibility:				
Ages 0-5	-13.50	-3.80	-4.96	6.68
	[4.53]	[1.68]	[1.83]	[1.92]
Ages 6-11	1.42	-4.93	-5.62	3.65
	[8.35]	[1.14]	[1.18]	[1.30]
Ages 12-18	9.17	-1.11	0.06	-0.70
	[6.35]	[1.56]	[1.96]	[1.76]
H_0: 0-5=6-11 (*p*-val)	*0.157*	*0.582*	*0.665*	*0.271*
H_0: 6-11=12-18 (*p*-val)	*0.442*	*0.0179*	*0.000112*	*0.0309*
	B. White Estimates for Early Eligibility			
Early Medicaid Eligibility	-14.50	-4.26	-5.23	5.46
	[4.53]	[1.06]	[1.40]	[1.10]
	(0.072)	(0.004)	(0.004)	(0.004)
Ages	0-5	0-11	0-11	0-11
	C. Nonwhite Estimates by Age of Eligibility			
Medicaid Eligibility at Age:				
Ages 0-5	-10.70	-5.65	-4.51	5.13
	[7.36]	[2.35]	[1.57]	[2.03]
Ages 6-11	-5.84	-1.62	-3.05	3.34
	[3.57]	[0.91]	[0.75]	[1.20]
Ages 12-18	4.49	-1.30	-1.73	1.41
	[3.08]	[1.05]	[0.68]	[0.99]
H_0: 0-5=6-11 (p-val)	*0.489*	*0.0315*	*0.240*	*0.324*
H_0: 6-11=12-18 (p-val)	*0.0174*	*0.767*	*0.0162*	*0.0112*
	D. Nonwhite Estimates for Early Eligibility			
Early Medicaid Eligibility	-8.73	-5.73	-3.90	4.38
	[5.08]	[2.33]	[1.15]	[1.50]
	(0.068)	(0.004)	(0.004)	(0.008)
Ages	0-11	0-5	0-11	0-11

Notes: The table presents instrumental variables estimates of the effect of a Medicaid's eligibility by age groups and for "early" eligibility. Early eligibility combines the age-specific variables that are significantly different from zero but not distinguishable from each other according to the *p*-values listed in the rows labeled H_0: 0-5=6-11, and H_0: 6-11=12-18. The specification includes the birth year general fertility rate, per capita income, and fixed effects for state of birth, cohort, region-by-cohort, and Medicaid-year-by-cohort. The nonwhite estimates also adjust for a linear trend interacted with $AFDC^{*}_{rs}$ for each Medicaid year for event-times prior to -15. The sample includes 1,968 observations on cohorts born between 1936 and 1976 in 48 states. Standard errors clustered by state of birth are in brackets, and *p*-values from 250 draws of a percentile-*t* wild cluster bootstrap are in parentheses.

Table 3. Instrumental Variables Estimates of Medicaid's Effect on log 37-Year Mortality Rates by Cause (coefficients×100)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					A. White, 1980-2016				
Cause of Death:	All Causes	Non-AIDS-Related Causes	Internal	Infectious	Chronic	Cardio-vascular	Cancer	Suicide	External (Homicide + Accidents)
Early Eligibility (0-5)	-17.50	-14.50	-8.56	-14.80	-18.00	-4.71	-6.86	-23.40	-10.10
	[5.29]	[4.53]	[5.05]	[13.70]	[7.35]	[6.23]	[7.88]	[10.30]	[6.63]
	(0.072)	*(0.024)*	*(0.236)*	*(0.316)*	*(0.092)*	*(0.564)*	*(0.572)*	*(0.080)*	*(0.180)*
Mean DV (deaths per 100,000)	13,900	13,700	11,700	557	3,860	3,770	4,560	623	1,690
					B. Nonwhite, 1980-2016				
Early Eligibility (0-11)	-12.00	-8.73	-7.73	-14.60	-7.52	-16.70	-7.39	-22.00	-17.10
	[6.14]	[5.08]	[4.76]	[8.39]	[6.61]	[5.31]	[4.79]	[7.13]	[8.79]
	(0.044)	*(0.096)*	*(0.124)*	*(0.120)*	*(0.396)*	*(0.004)*	*(0.192)*	*(0.008)*	*(0.048)*
Mean DV (deaths per 100,000)	19,700	18,800	16,200	1,050	5,550	6,440	5,350	297	2,830

Notes: The table presents instrumental variables estimates of Medicaid's effect on log cumulative mortality rates (1980-2016) by cause. The sample includes 1,968 observations on cohorts born between 1936 and 1976 in 48 states. Standard errors clustered by state of birth are in brackets, and p-values from 250 draws of a percentile-t wild cluster bootstrap are in parentheses. Mortality rates by cause do not add to the total because they are calculated using cause-elimination life table methods to account for the confounding influence of competing risks from the other causes.

Table 4. Instrumental Variables Estimates of Medicaid's Effect on Adult Disability Measures (coefficients×100)

	(1) Ambulatory Difficulty	(2) Hearing/Vision Difficulty	(3) Mobility Difficulty	(4) Self-Care Difficulty	(5) Cognitive Difficulty	(6) Work Limitation
			A. White			
Early Medicaid Eligibility (0-11)	-4.26	-1.36	-1.58	-1.17	-1.54	-3.74
	[1.06]	[0.33]	[0.37]	[0.22]	[0.38]	[0.87]
	(0.004)	*(0.004)*	*(0.004)*	*(0.004)*	*(0.008)*	*(0.004)*
Mean Dependent Variable	5.71	2.21	2.89	1.61	3.89	6.38
			B. Nonwhite			
Early Medicaid Eligibility (0-5)	-5.64	-1.85	0.00	1.04	-1.71	-0.270
	[1.88]	[0.98]	[0.48]	[0.6]	[0.8]	[0.44]
	(0.004)	*(0.036)*	*(0.992)*	*(0.188)*	*(0.080)*	*(0.548)*
Mean Dependent Variable	8.65	2.89	4.96	2.76	5.76	9.87
	Does this person have any of the following long-lasting conditions:		Because of a physical, mental, or emotional condition lasting ≥ 6 months does this person have any difficulty:			
Question Text	...substantially limits ≥1 basic physical activities such as walking, climbing stairs, reaching, lifting, or carrying?	Blindness, deafness, or a severe vision or hearing impairment?	Going outside the home alone to shop or visit a doctor's office?	Dressing, bathing, or getting around inside the home?	Learning, remembering, or concentrating?	Working at a job or business?

Notes: The table presents instrumental variables estimates of Medicaid's effect on all disability measures available in the Census. The specification is the same as in Figure 5 and Table 2. The sample includes 1,968 observations on cohorts born between 1936 and 1976 in 48 states. Nonwhite results use a procedure in which a linear pre-trend from event-time -23 to -15 is removed and IV estimates are based on these adjusted data. Standard errors clustered by state of birth are in brackets, and p-values from 250 draws of a percentile-t wild cluster bootstrap are in parentheses.

Table 5. Instrumental Variables Estimates of Medicaid's Effect on Transfer Program Participation and Insurance Coverage (coefficients×100)

	(1)	(2)	(3)	(4)	(5)
	Any Public Assistance	Disability Benefits (SSDI or SSI)	TANF or General Assistance	Public Insurance	Any Insurance
			A. White		
Early Medicaid Eligibility	-4.80	-5.23	0.49	-4.48	0.16
	[1.38]	[1.40]	[0.13]	[1.00]	[0.95]
	(0.012)	*(0.004)*	*(0.004)*	*(0.004)*	*(0.860)*
Mean Dependent Variable	6.3	5.4	1.3	12.5	88.4
			B. Nonwhite		
Early Medicaid Eligibility	-3.72	-3.90	0.76	-3.68	4.67
	[1.27]	[1.15]	[0.23]	[2.66]	[1.13]
	(0.012)	*(0.004)*	*(0.004)*	*(0.284)*	*(0.004)*
Mean Dependent Variable	12.2	9.5	3.4	24.3	81.3

Notes: The table presents instrumental variables estimates of Medicaid's effect on cash transfer receipt and insurance coverage in the 2000-2017 Census/ACS. The specification is the same as in Figure 6 and Table 2. The sample includes 1,968 observations on cohorts born between 1936 and 1976 in 48 states. Nonwhite results use a procedure in which a linear pre-trend from event-time -23 to -15 is removed and IV estimates are based on these adjusted data. Standard errors clustered by state of birth are in brackets, and *p*-values from 250 draws of a percentile-*t* wild cluster bootstrap are in parentheses.

Table 6. Instrumental Variables Estimates of Medicaid's Effect on Labor Supply (coefficients×100)

	(1)	(2)	(3)	(4)
	Out of the Labor Force	Currently Employed	Any Employment Last Year	Full-Time/Full-Year Employment
		A. White		
Early Medicaid Eligibility	-5.10	4.87	5.46	3.86
	[0.99]	[0.89]	[1.1]	[0.78]
	(0.004)	*(0.004)*	*(0.004)*	*(0.004)*
Mean Dependent Variable	17.9	78.1	84.3	55.5
		B. Nonwhite		
Early Medicaid Eligibility	-3.96	4.63	4.38	3.72
	[1.45]	[1.59]	[1.5]	[1.77]
	(0.044)	*(0.012)*	*(0.016)*	*(0.168)*
Mean Dependent Variable	23.9	68.8	77.2	48.3

Notes: The table presents instrumental variables estimates of Medicaid's effect on labor supply measures in the 2000-2017 Census/ACS. The specification is the same as in Figure 6 and Table 2. The sample includes 1,968 observations on cohorts born between 1936 and 1976 in 48 states. The nonwhite estimates also adjust for a linear trend interacted with $AFDC^{*}_{rs}$ for each Medicaid year for event-times prior to -15. Standard errors clustered by state of birth are in brackets, and *p*-values from 250 draws of a percentile-*t* wild cluster bootstrap are in parentheses.

Table 7. Instrumental Variables Estimates of Medicaid's Effect on Educational Attainment (coefficients×100)

	(1) High School Diploma	(2) Bachelor's Degree
	A. White	
Early Medicaid Eligibility (0-11)	1.81	1.23
	[0.83]	[1.64]
	(0.228)	*(0.580)*
Mean Dependent Variable	92.4	32.0
	B. Nonwhite	
Early Medicaid Eligibility (0-11)	5.61	0.98
	[2.42]	[0.70]
	(0.004)	*(0.248)*
Mean Dependent Variable	85.7	19.4

Notes: The table presents instrumental variables estimates of Medicaid's effect on the share of each cohort that has a high school diploma or bachelor's degree. These measures largely stabilize by the ages at which I observe responses, so they change little across survey years. I therefore use a 30-year pre-period even though the oldest cohorts are not observed in each year between 2000-2017. The specification is the same as in Figure 5 and Table 2. The means of the dependent variables refer to cohorts born between 1955 and 1975. Nonwhite results use a procedure in which a linear pre-trend from event-time -30 to -15 is removed and IV estimates are based on these adjusted data. Standard errors clustered by state of birth are in brackets, and p-values from 250 draws of a percentile-t wild cluster bootstrap are in parentheses.

Table 8. Instrumental Variables Estimates of Medicaid's Effect on Income by Source, Wages, and Poverty

	(1) Earned Income	(2) Log Wage	(3) Transfer Income	(4) Total Income	(5) Poverty Rate
			A. White		
Early Medicaid Eligibility (0-11)	646	-7.36	-714	-2	1.21
	[788]	[2.51]	[235]	[871]	[1.23]
	(0.500)	*(0.016)*	*(0.008)*	*(0.996)*	*(0.732)*
Mean Dependent Variable	33,780	301.7	623	35,796	7.9
			B. Nonwhite		
Early Medicaid Eligibility (0-11)	1,803	7.93	-260	2,116	-2.68
	[741]	[3.55]	[119]	[730]	[1.07]
	(0.052)	*(0.008)*	*(0.168)*	*(0.008)*	*(0.024)*
Mean Dependent Variable	26,832	281.60	1,012	29,122	18.50

Notes: The table presents instrumental variables estimates of Medicaid's effect on average income in the 2000-2015 Census. Columns 1 and 4 use average of income below $100,000. Column 2 uses the average of log wages calculated using usual weekly hours and total weeks work trimmed at $5,000 per hour. A continuous variable for weeks is only available in 2000-2007. The specification is the same as in Figure 6 and Table 2. The sample includes 1,968 observations on cohorts born between 1936 and 1976 in 48 states. The nonwhite estimates also adjust for a linear trend interacted with $AFDC_{rs}^{*}$ for each Medicaid year for event-times prior to -15. Standard errors clustered by state of birth are in brackets, and p-values from 250 draws of a percentile-t wild cluster bootstrap are in parentheses.

Table 9. Medicaid's Direct Costs, Fiscal Externalities, and Health Benefits

	All	White	Nonwhite
Health Benefits (millions of QALYs saved)	10.14	5.28	4.87
5th percentile	5.47	2.32	1.48
10th percentile	6.86	3.10	2.26
20th percentile	8.38	3.87	3.42
30th percentile	9.40	4.36	4.39
50th percentile	11.87	5.20	6.49
75th percentile	17.07	6.15	11.18
Direct Costs (billions)			
Medicaid Outlays, cohorts born before 1976	$92.07	$51.70	$40.37
Fiscal Externalities (billions)			
Change in Cash Transfers	-$119.66	-$84.31	-$35.35
Change in Cost of In-Kind Transfers	-$34.26	-$17.62	-$16.64
Change in Tax Receipts	$48.46	$10.52	$37.93
Change in Net Costs from Longevity	$2.28	$0.48	$1.80
Total Fiscal Externality	$200.10	$111.97	$88.12
Public Return (fiscal externality divided by direct costs)	217%	217%	218%
5th percentile	-86%	-78%	-310%
10th percentile	10%	1%	-135%
20th percentile	83%	84%	-14%
30th percentile	125%	140%	80%
50th percentile	194%	210%	184%
75th percentile	285%	288%	359%

Notes: All quantities are discounted back to 1965 using a 3 percent discount rate. Distributions for the public return and QALYs saved come from 1,000 draws of a block bootstrap that resamples states from within race and region strata and re-estimates the mortality, disability, tax, transfer income, and public insurance IV models.