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

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Abstract:

This paper exploits the original introduction of Medicaid (1966-1970) and the federal mandate that states cover all cash welfare recipients to estimate the effect of childhood Medicaid eligibility on adult health, labor supply, program participation, and income. Cohorts born closer to Medicaid implementation and in states with higher pre-existing welfare-based eligibility accumulated more Medicaid eligibility in childhood but did not differ on a range of other health, socioeconomic, and policy characteristics. Early childhood Medicaid eligibility reduces mortality and disability and, for whites, increases extensive margin labor supply, and reduces receipt of disability transfer programs and public health insurance up to 50 years later. Total income does not change because earnings replace disability benefits. The government earns a discounted annual return of between 2 and 7 percent on the original cost of childhood coverage for these cohorts, most of which comes from lower cash transfer payments.

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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 and cost $475 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 current 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 these effects may not justify the size of the program (Finkelstein, Hendren, and Luttmer 2015). For example, while Medicaid saves lives (Currie and Gruber 1996a, b, Goodman-Bacon forthcoming, 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. Because it primarily covers children during critical periods, Medicaid 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.

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 2014, 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 (Goodman-Bacon forthcoming). 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 provides new evidence on Medicaid's longer-run effects on both health and labor market outcomes, by exploiting the program's introduction between 1966 and 1970 and the federal mandate that Medicaid cover all cash welfare recipients ("categorical eligibility"). 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 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 and more likely to experience important health outcomes that may have been latent in early adulthood. Second, I provide a unified analysis of health and socioeconomic outcomes: cumulative mortality rates from 1980-2004 (using Vital Statistics data), self-reported disability rates, labor market status, transfer program participation, educational attainment, and the distribution of earnings and transfer income (from the 2000-2015 Census/American Community Survey). Third, Medicaid's introduction defines clear pre-treatment cohorts and facilitates direct tests of the research design that are not typically implemented in research using continuous eligibility instruments.

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 (Goodman-Bacon forthcoming) 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 support the validity of the design by showing the relationship between initial eligibility and adult outcomes for *each* cohort born up to 30 years before and five years after Medicaid implementation. 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 higher- versus lower-eligibility states for cohorts with increasing exposure; and flatten out for post-Medicaid cohorts with the same predicted childhood eligibility.

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The results show that cohorts with early-life Medicaid eligibility are healthier as adults, both in terms of mortality and disability, and that these health improvements reduce transfer program participation and increase employment. For whites, new earnings largely offset lower transfers, leaving individual income unchanged. Nonwhites have smaller reductions in transfer income, but similar increases in employment. Nonwhite high school graduation rates also increase, which leads to larger increases in earnings and total income and lower poverty rates.

The government saves on benefit payments *and* earns a small amount of new income tax revenue: $35.4 billion per year in total relative to a total cost of childhood coverage of about $132 billion in 2012 dollars. At a standard 3 percent discount rate, these changes imply about an 11 percent return *every year* on the initial investment. Using observed treasury rates to discount the costs and benefits implies about a 3 percent return and suggests that, between 2000 and 2015, the government recouped about 46 percent of the (true) original cost. Over half of this return comes from reductions in cash transfer payments, and the remainder comes from increased income tax revenue (35 percent) and lower public insurance spending (11 percent).

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 2015).[1] 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 relative 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.[2] In terms of

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[1] 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). 9 percent of respondents (with children) in the 1968 PSID reported that they could get "free care." Anecdotal evidence suggests that free care was 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).

[2] Parental reports of specific disease incidence appear not to provide reliable measures of disease burden. In the 1963-1965 National Health Examination Survey (USDHHS/NCHS 1991), for example, higher-income children are

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 2015, figure 1). Moreover, the categorical eligibility requirement created a strong link between changes in children's public insurance use and the states' prevailing AFDC participation rates. Figure 2 plots the change in the share of children who used public insurance between the year before Medicaid and 5 years after against the share of children on AFDC in the year Medicaid began. One percentage point in the initial AFDC rate is associated with a 1.34 percentage points of additional public insurance growth.

The large increase in coverage meant that poor children received substantially more medical care. Appendix table 1.1 presents cross-sectional differences in utilization by Medicaid eligibility across 10 surveys from before and after Medicaid implementation showing that children eligible

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more likely to report having mumps, bronchitis, scarlet fever, polio, allergies, or a heart murmur. However, poor children are more likely to have symptoms that are observable without a diagnosis such as a sore throat, colds, a "heart problem," or identifiable conditions such as whooping cough.

for Medicaid used much more medical care than ineligible poor children in the same state, or similar children in non-Medicaid states.[3] 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 negatively affects 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 forthcoming). 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.[4] 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).[5] In fact,

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[3] Appendices are available here: http://www-personal.umich.edu/~ajgb/medicaid_longrun_appendices_ajgb.pdf

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

[5] Stevens and Stevens (1974) emphasize that despite lags in the promulgation of EPSDT regulations, it was a major new proposal. EPSDT-screened children received 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 "1300 abnormalities were discerned in the first 1200 children screened" (quoting Howard Newman, pp. 257 note 51).

President Johnson stressed later-life effects when he advocated for EPSDT: "Ignorance, ill health, 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 uses both state-by-year variation and a birth date discontinuity in the 1980s eligibility expansions to estimate Medicaid's effects across the life course and finds striking improvements in health and economic outcomes. 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 2014), 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.[6] In fact, the only direct evidence on effects at older ages is mixed. Using the differential timing of Medicaid adoption across states, Boudreaux, Golberstein, and McAlpine (2016) 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.[7]

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

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[6] The age profile of chronic illness suggests that Medicaid's effects could change drastically after age 30. National Health Interview Survey (NHIS) data show that chronic conditions such as hypertension, diabetes, cancer, and arthritis strike adults aged 30-64 more than five times as often as adults aged 19-30, the typical age range used in existing long-run studies (Minnesota Population Center and State Health Access Data Assistance Center 2012).

[7] Furthermore, 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 as a consequence of expansions in cash welfare eligibility. Both of these programs have been shown to have longer-run effects (Aizer et al. 2014, Hoynes, Schanzenbach, and Almond 2012).

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.

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

I measure all outcomes as means at the race, state-of-birth, and year-of-birth level. Medicaid policy—when states implemented the program and the welfare participation rates that defined child eligibility—varies by state and year, and publicly available datasets on adult outcomes report state and year of birth. Moreover, cross-state patterns of AFDC participation differed strongly by race. 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 nonwhites, differences in initial eligibility reverse: 22 percent in Nevada versus 10 percent in New Mexico. I drop respondents born in Alaska, Hawaii, and Arizona, and keep cohorts born between 1936 and 1976. The main 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 eligibility varies with Medicaid's introduction, annual AFDC rates (categorical eligibility in a given state), and with cohort migration decisions (the states in which a cohort actually obtains eligibility).[8] The number of expected years of childhood Medicaid coverage 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:

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

The first component is a dummy, $1\{y \geq t_{\ell}^{*}\}$, that equals one if year y is after state ℓ's Medicaid implementation date ($1966 \leq t_{\ell}^{*} \leq 1970$). The second is the average monthly child AFDC participation rate by race (r), state (ℓ), and year (y): $AFDC_{ry\ell}$.[9] I construct each state's nonwhite

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[8] 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.

[9] Age-specific AFDC rates are not available at this time, but the 1970 Census shows that welfare participation rates are essentially constant during childhood. Details on the calculation of race-specific AFDC rates are in appendix 1.

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, I calculate the state of residence distribution every five years using the 1970-2000 Censuses and linearly interpolate to obtain the probability that members of a cohort born in state *s* in year *c* (race *r*) lives in state (ℓ) in calendar year (y): $\sigma_{rsc}^{y}(\ell)$.

Figure 2 plots m_{rsc} against event-cohorts. While annual eligibility jumps sharply after Medicaid, cumulative eligibility phases in across cohorts born closer to Medicaid's start date. Fully treated nonwhite cohorts accumulated about 5 years of eligibility and fully treated white cohorts accumulated about 1 year. Variation across states in cumulative eligibility is nearly as big as the average gains, and the dashed lines show that these gains are correlated with the categorical eligibility rates at Medicaid's inception.

B. *Cumulative Adult Mortality*

My first measure of adult health is the cumulative cohort-level mortality rate between 1980 and 2004 (conditional on living to 1980). The count of deaths from 1980 to 2004 for a given race-by-birth-state-by-birth-year cell come from the 1980-2004 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.[10]

My primary mortality outcome is the log of 25-year mortality rates from non-AIDS causes. 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 survived to benefit from highly active anti-retroviral therapies), and was concentrated among nonwhite men in New York and New Jersey, two relatively high-AFDC states. I use cause-elimination life table methods (Manton and Stallard 1984) to construct cumulative mortality rates that reflect the force of non-AIDS mortality

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[10] Summing deaths over 25 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).

(rather than the effect of AIDS on the size of the surviving population).[11] I calculate other cause-specific mortality rates in the same way. [12] The oldest cohort in the sample was born 1936, 30 years before the earliest Medicaid implementation date, and their mortality is measured between ages 45 and 69. 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 29.

C. Adult Self-Reported Disability

The disability variables in the Census and American Community Survey (ACS) (Ruggles et al. 2010) provide independent measures of health. These include: 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 2008-2015 data). The sample includes respondents between ages 25 and 64. 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.

D. Adult Socioeconomic Outcomes

I calculate other adult outcomes similarly using the 2000-2015 Census and ACS (and sometimes in the 1970, 1980, and 1990). Labor market outcomes include rates of labor force participation, employment (current and annual), and full-time/full-year employment. Safety-net outcomes

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[11] 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.

[12] 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 can be calculated similarly using annual deaths in the numerators and the surviving cohort population, $POP_{cs,1980} - \sum_{j=1980}^{y}(d_{cs,j}^{AIDS} + d_{cs,j}^{OTH})$, 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 $1 - \prod_{j=1980}^{1999}(1 - mr_{cs,j}^{k})$.

include cash transfer receipt (SSI, SSDI, TANF, or other programs) 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 12 or 16 years of completed schooling.

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

The research design adapts the difference-in-differences strategy in Goodman-Bacon (forthcoming) and compares cohorts born in different years and in states with different child AFDC rates in the year of Medicaid implementation. 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 identifies Medicaid's long-run effects if (1) $AFDC_{rs}^*$ is uncorrelated with other determinants of cross-cohort changes in adult outcomes (excludability) and (2) $AFDC_{rs}^*$ predicts cumulative eligibility (relevance).

A. Excludability: are initial AFDC rates correlated with pre-Medicaid state characteristics?

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 (Alston and Ferrie 1985, Bell 1965, Moehling 2007). The correlation between $AFDC_{rs}^*$ with past AFDC rates is the same in 1961, 1958, and 1948 (Goodman-Bacon forthcoming), which shows that my identifying variation did not emerge contemporaneously in the 1960s—it was in place at *least* two decades before. The variation itself derived from idiosyncratic state-level institutions such as constitutional language, industrial structure, or traditions of aid inherited from pre-AFDC programs, that were not correlated with levels or changes in children's circumstances.

I provide direct evidence on this point using two indices of potential confounders (Kling, Liebman, and Katz 2007). One combines Vital Statistics measures of child health from 1950-1965, and the other combines childhood socioeconomic (SES) measures from the 1950-1970 Censuses. I code the components to represent "good" outcomes, standardize them using the mean and standard deviation from 1950, and average them together with equal weight. I first regress each index on year dummies interacted with $AFDC_{rs}^*$, and then 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. I also pool all years and test for cross-sectional balance in the level of each index.

Figure 3 plots the results and shows little evidence that changing circumstances faced by infants and children in the 1950s and 1960s are correlated with initial AFDC rates. Neither trends nor levels in the health or SES indices differ significantly for whites. The white health index in states that were 5 percentage points apart in $AFDC_{rs}^*$ (more than two standard deviations) would only have diverged by 0.17 standard deviations over 15 years (0.0022*5*15 = 0.165). The differential pre-trends and level differences in the nonwhite health index are smaller by an order of magnitude. $AFDC_{rs}^*$ has a small correlation with levels and trends in nonwhite SES. Still, the magnitudes are small: the nonwhite SES index in states that were 8 percentage points apart in $AFDC_{rs}^*$ (the standard deviation of nonwhite AFDC rates) would have diverged by just 0.25 standard deviations over 15 years (0.0021*8*15 = 0.25). Note that this index includes child poverty which is individually orthogonal to $AFDC_{rs}^*$ (Goodman-Bacon forthcoming). This design does not compare richer to poorer states.

Appendix table A3.3 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. 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.

B. Event-Study Specification

A more direct test of the research design comes from 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 is:

$$Y_{rsc} = \mu_s + \mu_c + \mu_{R(s),c} + \mu_{t_s^*,c} + X'_{rsc}\beta$$

$$+ AFDC^*_{rs}\left[\sum_{j=-(a+1)}^{-20} \pi_j 1\{c - t_s^* = j\} + \sum_{j=-18}^{b+1} \phi_j 1\{c - t_s^* = j\}\right] + \varepsilon_{rsc} \quad (2)$$

Fixed effects for state of birth (μ_s) year of birth (μ_c) ensure that the estimator is a difference-in-differences model. Region-by-cohort fixed effects ($\mu_{R(s),c}$) 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 ($\mu_{t_s^*,c}$) eliminate comparisons between earlier and later Medicaid-adopting states, which had divergent socioeconomic and health outcomes before Medicaid.[13] 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 (48 clusters) and present p-values from 250 draws of a wild-cluster percentile-t bootstrap.

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, $j = -19$.[14] The π_j are falsification tests. Cohorts born more than 18 years before the introduction of Medicaid had no childhood coverage and should have no "treatment effects".[15] 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 $max\{0, t_s^* - c\}$. Because exposed cohorts are treated from age $max\{0, t_s^* - c\}$ to 18, each ϕ_j is analogous to a distinct experiment in which the Medicaid dose differs by $AFDC^*_{rs} \cdot (19 - max\{0, t_s^* - c\})$ *and* coverage begins at age $max\{0, t_s^* - c\}$. Note that the pattern of the ϕ_j around $t_s^* = c$ (i.e., $j = 0$) provides an additional test of the design. All cohorts born after Medicaid have the same "experiment" (19 years of eligibility starting at birth), and so barring treatment effect heterogeneity across calendar years, the ϕ_j should be similar for $j \geq 0$.

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[13] 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).

[14] I report coefficients for event-cohorts born between a years prior to and b years after Medicaid implementation. Cohorts born outside the event window [-a, b] are grouped into (unreported) terms for $-(a + 1)$ and $(b + 1)$.

[15] 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.

The pattern of the other ϕ_j estimates provides important information about when and how the Medicaid affects later life outcomes. The ϕ_j will be zero if Medicaid has no effect when received at age $max\{0, t_s^* - c\}$ and older. Recent work strongly suggests that "earlier is better" (Heckman 2007), but neither economics nor medicine offers sharp predictions about these features except in special cases such as in utero exposures.[16] 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.

This exercise consistently identifies trend breaks in the nonwhite reduced form estimates, but also pre-existing, opposite signed trends for untreated cohorts. This finding contrasts with the results in figure 3 showing that outcomes measured *before* Medicaid did not trend differently in higher or lower AFDC states. The fact that $AFDC_{rs}^*$ is associated with slightly worse outcomes later in life highlights how, in a long-run design, factors that emerge between treatment and follow-up can affect the validity of the design. To address this issue, I follow Bhuller et al. (2013) and estimate (and report) a linear pre-trend on data through event-time -15, and subtract the fitted trend from all nonwhite data points.[17] This does not change the estimated trend breaks, it only alters the orientation of the event-study coefficients and the resulting IV estimates.

C. Instrumental Variables Specification

To express the effects in terms of years of childhood eligibility and provide additional evidence on heterogeneity by age of exposure, I estimate instrumental variables models that use the predicted "dose" described above 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:

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

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[16] 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.

[17] 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.

$z_{rsc}^{\ell,u}$ equals predicted eligibility between ages ℓ and u. The IV models contain the same covariates as in equation (2) and use the $z_{rsc}^{\ell,u}$ variables as instruments for similarly calculated versions of $m_{rsc}^{\ell,u}$. 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.

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 part of a year's worth of Medicaid coverage in higher versus lower AFDC states, rather than a fixed group of recipients receiving Medicaid for an additional year of their life.

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

I use the event-study specification in (2) to estimate the first-stage relationship between initial AFDC rates and cumulative eligibility. Figure 4 plots first-stage event-study estimates that measure the relationship between $AFDC_{rs}^*$ and cumulative Medicaid eligibility measure 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 (whites) or erode (nonwhites), which underscores the earlier claim that 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). 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. Column 1 of table 1 presents first-stage estimates that quantify these differences. Across all childhood years, $z_{rsc}^{0,18}$ strongly predicts white

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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). Columns 2 through 4 present stronger age-group-specific first-stage estimates, especially for nonwhites.

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

A. Cumulative Mortality, 1980-2004

The event-study estimates in figure 6 show that early childhood Medicaid eligibility is strongly associated with relative reductions in later-life (non-AIDS) mortality. The point estimates are small for cohorts with no childhood Medicaid exposure—a key test of the the design—as well as for cohorts that were only eligible later in childhood. Consequently, the fitted trends through unaffected ages are small and not significant.[18] Mortality begins to fall in higher-AFDC states for cohorts exposed under age 9 for nonwhites and under age 6 for whites (see appendix figure 2.4 for F-statistics), and the estimated trend breaks are significant (white: -0.18, s.e.=0.07; nonwhite: -0.09, s.e. = 0.07). For whites, the trends 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 just under one percent lower mortality rates and, as appendix figure 2.1 shows, about 0.05 additional years of eligibility, which roughly implies an ITT effect per year of about -20 percent for both groups. Table 2 confirms this in IV models. Consistent with a wide range of work on life-course health and human capital development, eligibility in the first five years of life is associated with large and precise mortality reductions (whites: -18.0, s.e.=6.28; nonwhites: -26.8, s.e. = 10.4). For whites, I can distinguish these estimates from the coefficients at older ages. For nonwhites, the two earlier coefficients are indistinguishable even though the age 0-5 coefficient is twice as large. Panels B and D group age-specific eligibility variables with indistinguishable coefficients and present effects for "early" eligibility. As expected, the point estimates are similar, but the t-ratios rise.

The mortality data also include valuable information on reported underlying causes of death, and table 3 presents IV estimates for early eligibility by broad groups of causes. The results show that Medicaid's effects operate through a range of conditions. The basic 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). For whites. Medicaid's effects are strongest among the

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[18] The removed pre-trend for nonwhite mortality implied that mortality rates rose by 0.06 log points per cohort (s.e. = 0.02) for each percentage point in initial AFDC rates.

most plausibly affected conditions (internal causes; column 4), although they are slightly smaller for nonwhites. Cardiovascular conditions and infectious disease plays a more important role for nonwhite mortality than white mortality, while chronic conditions fall more for whites. Both groups have similar cancer effects—a rare finding in the literature on life-course health. Behavioral changes could underlie this link, as could changes in the incidence of infectious causes of certain cancers such as the bacterium *h. pylori* (stomach cancer) or human papillomavirus (cervical cancer). Column 7 shows large reductions in suicide (whites: -39.7, s.e. = 13.8; nonwhites: -36.0, s.e. =12.0), 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 2014). Note that because they are rare, suicides account for a small share of the aggregate effect.

Many analyses of insurance and mortality use deaths from external causes—homicides and accidents—as a falsification test. Consistent with this interpretation, column 8 shows no relationship between Medicaid eligibility and later life external-cause deaths for whites. Medicaid eligibility for nonwhites, however, is associated with large reductions in these deaths. One interpretation is that an unmeasured factor generates reductions in both internal and external nonwhite mortality and invalidates the nonwhite mortality results. Another interpretation is that data limitations contribute mechanically to this finding. External cause deaths occur most frequently in childhood and young adulthood, but the earliest cohorts have aged out of this phase by 1980. The estimated pre-trend in these deaths is large and positive because successive cohorts' mortality rates include more and more ages from high-risk years, and so netting out this trend mechanically contributes to the large effects in table 3.[19] Finally, effects on nonwhite SES (documented below) could change risky behaviors associated with accidents or change exposure to crime rates through moves to better neighborhood. This 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, will be larger than the ITT since this group must account for *all* the averted deaths, but it will be smaller if treated adults have higher baseline mortality. The PSID's mortality supplement shows that cumulative mortality

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[19] Note that this issue would tend to generate negative pre-trends for conditions that are more prevalent at older ages, and removing such a trend would attenuate my results.

rates of respondents on AFDC in 1968 are about 1.57 times higher than non-AFDC recipients for whites and 1.1 times higher for nonwhites (University of Michigan Survey Research Center 2016).[20] Estimated differences between treated and untreated adults do not reflect *counterfactual* mortality rates, though, because, as figure 5 shows, Medicaid itself reduces 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 1.3 full years on AFDC by age 6 and nonwhite children spent about 3.3 full years on AFDC by age 9 (Berger and Black 1998, Smith and Yeung 1998). Estimated mortality rates among the treated are therefore still too small by a factor of $(1 + 1.3 \cdot \delta_w)$ for white adults and $(1 + 3.3 \cdot \delta_{nw})$ for nonwhite adults. Combining these two adjustment factors implies that a year of childhood coverage reduces cumulative non-AIDS-related mortality rates by about 11.7 percent among treated white adults and 10 percent among treated nonwhite adults (see appendix 6 for details).

These reductions are larger than similar 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 7 plots event-study estimates for the most common self-reported disability in the Census: difficulties with activities like walking, climbing stairs, reaching, lifting, or carrying (ambulatory difficulty). Linear pre-trends are again flat (white: -0.007, s.e. = 0.008; nonwhite: -0.002, s.e. = 0.003) and best-fitting trend breaks come at age 10 for whites (-0.023, s.e. = 0.009) and age 4 for nonwhites (-0.020, s.e. = 0.010). Crucially, the trends reverse for cohorts with full childhood exposure (i.e., born after Medicaid implementation). A joint test of the null that these coefficients are equal, a direct prediction of the design, yields *p*-values

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

of 0.99 (whites) and 0.27 (nonwhites). The shape and precision of the event-study results provides strong evidence that childhood Medicaid improves adult health.

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 whites (s.e. = 1.06, p-value = 0.004) and 3.33 percentage points for nonwhites (s.e. = 1.13, p-value = 0.004). To gauge the magnitude of these results, I use the PSID to calculate the counterfactual disability rate among adults with childhood Medicaid eligibility. White respondents who were between 0 and 10 in 1968 and had AFDC income in that year, reported work limitations about 2.7 times as often as the average (0.29 versus 0.11). 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.[21] These adults had about 2 years of eligibility by age 11, which, when combined with a treatment effect of -4.26 percentage points, yields a counterfactual disability rate for treated white adults of 23 percent (comparable to the rates observed among 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 18.5 percent (-4.26/23). Similar calculations for nonwhite adults imply a proportional reduction per year of eligibility of 22 percent.

The mortality and disability results may interact if Medicaid skews the composition of survivors towards those more likely to be disabled. 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):

$$dr_{rsc} = (1 - p_{rsc}) \cdot dr_{rsc}^0 + p_{rsc} \cdot dr_{rsc}^1 \quad (4)$$

p_{rsct} is the share of each cohort that survived to appear in the Census/ACS data because of Medicaid. I use the contemporaneous infant and child mortality estimates in Goodman-Bacon (forthcoming) 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 lies between 0 and 1, I bound the treatment effect on dr_{rsc}^0 by assuming that dr_{rsc}^1 lies between 15 and 50 percent, similar to Bharadwaj, Løken, and Neilson (2013, table 4). Appendix 3 shows that selective survival plays almost no role in the treatment effects for white cohorts. For

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[21] The rate for currently poor adults in the Census is actually higher: 17.7 percent.

high values of dr_{rsc}^1, however, the nonwhite effects under age 4 are similar to the white effects. Selection adjustments do not change the racial differences in the ages at which coverage matters.

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). 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. Nonwhite results only appear to be reliable for ambulatory difficulty. Results for other disabilities are sensitive to choices about the de-trending procedure and to alternative specifications. I examine the robustness of the ambulatory difficulty finding below, but here note that the evidence does not point to reductions in other types of disability for nonwhites.

The mixed evidence on disability for nonwhites is puzzling because Medicaid's contemporaneous effects on child health were strongest and most precise for nonwhite children. The differences in short- and long-run effects may relate to racial differences in Medicaid utilization that counteract crude eligibility rates. 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).[22] 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 imply 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.

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[22] 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.)

C. Labor Supply and Transfer Program Participation

Event-study and IV estimates both show that Medicaid's health effects translate into higher extensive margin labor supply and lower transfer program participation. The series with open squares 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]. For whites, the results track changes in disability very closely. The pre-trend is small and insignificant (-0.011, s.e. = 0.013), there is a negative trend break for the same cohorts that experienced health improvements (-0.023, s.e. = 0.016) and a positive one for cohorts with full exposure (0.034, s.e. = 0.009). The corresponding IV estimate in table 5 shows a reduction in disability transfer participation of 4.90 percentage points (s.e. = 1.21). Other welfare receipt (mostly Temporary Assistance for Needy Families, TANF) actually rises slightly (0.57, s.e. = 0.11). Disability benefits are higher than TANF benefits, meaning that, abstracting from non-negligible application costs, people who qualify for both prefer SSI/SSDI, so health improvements that disqualify households for disability benefits may simply lead some to take up TANF (for a similar result see Borghans, Gielen, and Luttmer 2014).[23]

Nonwhite event-study results show a similar pattern to the white results, but with a shallower slope starting at age 14 (-0.01, s.e. = 0.006). The IV estimates show that each year of coverage under age 11 reduces disability transfer receipt by 3.43 percentage points (s.e. = 1.05). Consistent with the ambulatory difficulty results, table 2 shows that this effect is larger under age 5 (-3.94, s.e. = 1.42) than between ages 6 and 11 (-2.71, s.e. = 0.66) although the effects are not statistically distinguishable (*p*-value= 0.258).

The series in figure 7 with closed triangles plot event-study estimates for annual employment that are almost the mirror image of the transfer receipt effects. The pre-trends are flat, and positive trend breaks in employment occur for the same cohorts whose transfer receipt fell (white: 0.043, s.e. = 0.017; nonwhite: 0.011, s.e. = 0.007). IV estimates (table 6) show that each year of childhood

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[23] The appendices provide additional evidence on the validity of the design using employment and public assistance data from 1970, 1980, and 1990. These Censuses contain labor supply and program participation measures for much older cohorts during prime working years. I use these data to conduct two related falsification tests. Figure 4.8 uses the 1980 and 1990 Censuses to extend figure 7's pre-period to 45 years. There is no relationship between $AFDC_{rs}^*$ and employment or public assistance trends even for cohorts born in the 1920s. Figure 2.4 shifts event-time back for cohorts observed in the 1970 and 1980 Censuses and estimates "false" event-studies across the same *ages* used in the main analysis but in much earlier survey years. There is no evidence that age/employment or age/public-assistance patterns were correlated with $AFDC_{rs}^*$ for untreated cohorts.

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, but slightly more than 5 percentage points for both groups. For whites, most new employment is full-time/full-year (4.05, s.e. = 0.69), while only about half of it is for nonwhites (2.17, s.e. = 0.85).

The changes in program participation and employment means that Medicaid has important intertemporal effects within the public and employer sponsored insurance systems. Column 4 of table 7 shows that the white cohorts with the largest reductions in SSDI/SSI receipt also use public insurance *less* often as adults (-5.05, s.e. = 1.14), while there are negligible effects for nonwhites.[24] For whites, increases in private insurance, most likely from new full-time employment, offset the reduction in public coverage, and total insurance coverage does not change (-0.09, s.e. = 1.01). For nonwhites, who have smaller reductions in DI receipt, but similar increases in employment, total insurance coverage increases (8.61, s.e. = 2.08).

D. Human Capital

Long-run research based on the 1980s expansions finds that early life coverage for lower income families increases high school graduation (Miller and Wherry 2014), and later childhood coverage to slightly higher income families increase college attendance and completion (Brown, Kowalski, and Lurie 2014, Cohodes et al. 2014). Table 9 shows evidence consistent with this pattern. To get a better measure of completed education, I add data from the 1980 and 1990 Census, and calculate the share of each cohort that had at least 12 years of school or 4 years of college when they were between 35 and 55 years old. The results imply that early Medicaid eligibility increases high school graduation rates by 2.28 percentage points for whites (s.e. = 0.94, although the bootstrap *p*-value is 0.18) and 3.01 percentage points for nonwhites (s.e.=1.14, bootstrap *p*-value = 0.012). I find no evidence of an effect on college attainment.

These results help explain the relationship between health and labor market effects by race. Whites experience lower disability rates, lower transfer receipt, and higher employment when covered under age 10 (and mortality reductions associated with slightly earlier eligibility). I find no evidence that education moderates these effects. The nonwhite health results, on the other hand, suggest that disability only falls for some outcomes and for coverage under age 5 while the

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[24] The white effect comes both from Medicaid, for which almost all SSI recipients are categorically eligible, and Medicare, which SSDI recipients can receive after a two-year waiting period. ACS data show that among non-elderly SSI recipients, 94% have Medicaid and 33% have Medicare, while among SSDI recipients, 33% have Medicaid and 44% have Medicare.

employment and transfer results begin for cohorts covered in early adolescence. Higher graduation rates provide one explanation why.

E. Sources of Income

Increases in labor supply and reductions in transfer program receipt offset each other in terms of income, almost completely for whites and to a lesser extent for nonwhites. Figure 9 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 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. Second, the figure shows that increased earnings offset reduced transfer income. For whites, the positive earnings coefficients are larger than the negative transfer income coefficients, but the estimates for total income are insignificant. The effects for earnings between $20,000 (the 99th percentile of transfer income in 2014) and $40,000 (just under the mean for white workers in 2014) remain positive and marginally significant, suggesting some effect on middle-class jobs. The nonwhite results have a similar, although noisier, pattern. Earnings under about $50,000 increase, transfers fall, but total income increase more than for whites.

Table 8 quantifies the effects on average income by source.[25] Column 1 show that, as expected, earned income increases. The full-sample estimate for whites is imprecise, but column 2 shows that trimming earnings above $100,000 (as in Chetty et al. 2011) changes the point estimate by just 5 percent but cuts the standard error by a factor of almost 3, leading to a more precise increase

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[25] Because the effects in figure 9 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 $2,227 increase in nonwhite earnings, a $298 dollar reduction in nonwhite transfers, a $1,618 increase in nonwhite income, a $1,334 increase in white earnings, a $676 reduction in white transfers, and a $891 increase in white income.

of \$1,175 (s.e. = 633). Trimming has a larger effect on the nonwhite estimates, but even after doing so they are larger than the white effects (\$2,034, s.e. = 711s).

Column 3 provides some suggestive evidence that wage changes account for this difference. Note that by altering the share of low-skill workers who work, the extensive margin effects almost certainly negatively bias these estimates. Consistent with this, wages fall for white workers. Nonwhite workers, however, have positive wage effects, which makes sense given their increases in education. This fits well with simple employment and wage comparisons by disability status and by education. The wage gap by high school graduation status (\$6.50) is much larger than the wage gap by disability status (\$2.20), but the employment gap is much larger by disability (45 points) than by education (25 points). Medicaid-induced improvements in health and education for nonwhite cohorts, led to higher employment and wages. Medicaid had stronger effects on health for whites, but no detectable effect on education, and consequently white cohorts have large employment effects but no evidence of higher wages.

Columns 4 and 5 show that transfer income falls for both groups (whites: \$648, s.e. = 196; nonwhites: \$374, s.e. =153), and I can only detect increases in total income for nonwhites (white: \$634, s.e. = 711; nonwhite: \$2,548, s.e. = 913). As a result, each year of early Medicaid eligibility reduces nonwhite poverty by 3.08 percentage points (s.e. = 1.32), but has no detectable effect on white poverty (1.24, s.e. = 1.15).

VI.   THREATS TO INTERNAL VALIDITY AND ROBUSTNESS

The preceding evidence shows that initial categorical Medicaid eligibility is uncorrelated with pre-Medicaid cohort trends in child outcomes, and that its correlation with adult outcomes only appears for cohorts exposed relatively early in childhood (event-study results). This section presents additional evidence on the validity of my estimates based on alternative specifications and direct tests for confounders that also changed in the 1950s and 1960s.

A. Alternative Specifications

Figure 10 plots IV estimates and 95-percent confidence intervals for early eligibility that correspond to the outcomes in panels B and D of table 2. I overlay a solid line at zero and a dashed line at the estimates from my preferred model (highlighted in row 2). The model in row 1 includes only event-time dummies and $AFDC_{rs}^*$, row 3 presents unweighted estimates (Solon, Haider, and Wooldridge 2015), and row 4 drops West Virginia, an outlier in white AFDC rates (I do not reproduce this specification for the nonwhite results). Except for mortality, the white results

depend on the region and Medicaid timing fixed effects, but are nearly identical in the other specifications. The nonwhite employment and transfer results are also similar across specifications, but the unweighted results are much larger for mortality and smaller for ambulatory difficulty.

B. Changes in Hospital Capacity

Cohorts with differential Medicaid exposure may also have experienced differential exposure to growth in hospital capacity spurred by the 1946 Hill-Burton Act (Chung, Gaynor, and Richards-Shubik 2016). This could bias my estimates if hospital capacity affects later-life outcomes. I test this in row 5 by adding two control variables calculated from the American Hospital Association's guide books starting in 1932.[26] The first is the average value of per-capita hospital beds during each cohort's 9 years with the same migration adjustment used in equation (2). The second set of controls are interactions between cohort dummies and quintiles of the average per-capita beds variable from 1936-1940, which aims to capture Hill-Burton's role in equalizing geographic differences in hospital availability.[27] These controls are jointly significant for all outcomes and groups, but have negligible effects on the estimated effects of Medicaid.

C. Other War on Poverty Programs

Another potential source of bias is the concomitant roll-out of other War on Poverty programs that have also been show to confer long-run benefits 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). The event-study estimates and the pre-trend tests in figure 3 cannot rule out bias from these programs because they expanded around the same time as Medicaid. Using data on 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)[28] 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. I then sum this value from age 0 to 9 (ages 3-4 for HS) for each cohort, using the migration weights from equation (2). The resulting variables takes larger values than

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[26] I thank Amy Finkelstein and Heidi Williams for sharing the data from before 1975, and Jean Roth and NBER for providing the extracts from after 1975.
[27] If Medicaid causally affected hospital capacity, then the cohort-level measure of per-capita beds is not an admissible control, but the cohort interactions are. The results are similar if I enter the two controls separately.
[28] I thank Hilary Hoynes for sharing the FSP data.

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 has no effect on the sign or significance of correlations between Medicaid eligibility and program expansion.

Figure 11 plots event-study estimates that use cumulative exposure to HS, CHCs, or FSP as outcome variables. For comparison, I overlay the first-stage relationship for Medicaid eligibility from age 0 to 11. The figure also reports the coefficients on the early eligibility instrument, z_{rsc}^{0-11}. The results provide no evidence that FSP, HS, or CHCs, all of which could generate spurious effects, confound the research design. Both the event-study estimates and the single coefficient estimates are small and insignificant, especially compared to the true first-stage relationship. Row 6 of figure 9 includes the War on Poverty variables as controls in the main outcome models. The variables themselves are highly significant, particularly for whites, but none of the point estimates for my main outcomes change appreciably.

D. Exposure to AFDC per se

Because Medicaid eligibility was legally based on AFDC receipt, it is mechanically true that cohorts with higher Medicaid eligibility received more welfare income. The stability of AFDC rates over time, though, means that this was also true of *pre*-Medicaid cohorts. Still, if the distribution of AFDC rates spread out, then a given number of childhood years spent in a "high" initial AFDC state may translate to more expected years of welfare receipt after Medicaid than before. Figure 11 also plots cumulative AFDC receipt between ages 0 and 9 calculated in the same way as equation (2), but without the post-Medicaid dummy. AFDC rates by race are only available back to 1948, and so I do not observe a pre-period for this measure. AFDC exposure is relatively higher for cohorts with more Medicaid eligibility, but importantly this rise began before Medicaid's estimated treatment effects start, and so AFDC itself is unlikely to generate the treatment effects documented above. Adding cumulative AFDC rates as controls cuts the sample size, but appendix table X shows that this also does not change the estimated treatment effects.

E. Migration

Rows 7 and 8 re-estimate the treatment effects on samples of Census respondents interviewed outside their birth state (movers) or in it (stayers). The effects above, especially on labor market outcomes, could come from differences in cohort's underlying willingness to make advantageous

moves. If so, we should not observe treatment effects when looking within the samples of movers and stayers. The results of these analyses show that treatment effects of Medicaid are apparent for both movers and stayers, which helps rule out a migration explanation. The effects are larger for stayers, though, especially in terms of employment and transfer receipt. Low-skill workers are less likely to move in response to labor demand changes (Bound and Holzer 2000), and recent evidence suggests that these flows may also be correlated with health (Arthi, Beach, and Hanlon 2017). Therefore, larger treatment effects among stayers is consistent with the notion that Medicaid's effects come from adults who were more likely to be eligible for Medicaid as children.

F. Adult State Characteristics

The identification strategy hinges on respondents' birth state, but differential circumstances in their adult states could generate spurious correlations between long-run outcomes and Medicaid exposure in childhood. Row 9 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 respondents born in the same year but in different states, who lived in the same state at follow-up, 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).

G. Contemporaneous Economic Conditions

A similar concern is that the results reflect differences in contemporaneous labor demand. Age-varying effects of the Great Recession, for example, may have eroded employment among older workers in certain areas, generating cross-cohort differences not related to childhood exposure. Alternatively, self-reported disabilities may reflect a post-hoc justification for unemployment spells (Bound et al. 2003) or a true health effect that derives from economic conditions and not childhood exposure (Charles and Decicca 2008). Row 10 further 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. Again, there is no evidence that contemporaneous economic conditions can explain Medicaid's long-run treatment effects.

H. Falsification Tests Using Older Cohorts

The 1970 and 1980 Census asked comparable questions about public assistance receipt and employment, which allows me to 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. (Note that the transfer receipt variable in these models refers to any transfer, not disability related transfers.) These falsification tests help rule out the concern that age profiles of employment and transfer receipt differ by birth state. Reassuringly, the results show that adult outcomes for never-treated cohorts bear no relationship to Medicaid eligibility, even when I observe cohort outcomes at the same ages used in the main analysis.

VII. DISCUSSION: THE RETURN TO MEDICAID SPENDING

The results above show large effects of early childhood Medicaid coverage on adult health, labor supply, and program participation. Some of these benefits accrue to individuals. These results suggest that about 194,000 lives were saved between 1980 and 2004—21,000 among whites and 173,000 among nonwhites.[29] Even assuming a relatively low value of statistical life of $845,000 (the lower end of the confidence interval of Ashenfelter and Greenstone's (2004) estimates [table 2, converted to 2012 dollars]) suggests that Medicaid's longer-run mortality reductions are worth at least *$164 billion*. Moreover, the disability results suggest improvements in physical capacity that are fundamental to many concepts of well-being itself (Sen 1993) and closely related to self-reported happiness and satisfaction.[30] Incorporating the value of improved physical and cognitive functioning would add significantly to this number. This contrasts with recent welfare estimates from the Oregon Health Insurance Experiment showing that Medicaid coverage is worth *less* than its cost (Finkelstein, Hendren, and Luttmer 2015).

For whites, Medicaid alters the composition but the not the amount of income, and recipients are not materially better off. This trade-off suggests that that disability programs successfully insure against poor health and by doing so limit the extent to which early health investments can ameliorate adult poverty. Nonwhites experience this trade-off to a less extent. Increased education and wages mean that their earnings gains outstrip reductions in transfer income, ultimately reducing poverty.

These results speak directly to empirical analyses of disability insurance and benefit programs. Recent research uses random assignment of disability applications across evaluators with different award rates to show that, *holding health constant,* disability benefits reduce labor supply (French

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[29] This calculation uses the treatment effects on non-AIDS internal-cause mortality to calculate counterfactual mortality rates and the number of lives saved.

[30] The 2001 National Health Interview Survey (MPC and SHADAC 2012) shows that 21 percent of poor non-elderly adults with an activity limitation report being happy "a little" or "none" of the time. The figure for poor adults with no limitations is 8.6 percent and for non-poor adults with limitations is 12.7 percent.

and Song 2014, Maestas, Mullen, and Strand 2013). Another approach decomposes time-series changes in disability receipt into its components *holding nothing constant*, and concludes that health improvements have had little impact on the rolls (Autor and Duggan 2006, Duggan and Imberman 2009). Reform proposals therefore emphasize 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 figures 6 and 7, on the other hand, show that *holding program incentives constant* (through the cross-state comparisons), improvements in health greatly reduce disability benefit receipt and increase labor supply.[31] Multiplying the effect per year of eligibility (-5.88) by the population in affected cohorts (56 million whites with early childhood eligibility) and the average eligibility under age 10 (0.37 years) suggests that there are 1.2 million fewer SSI/SSDI recipients because of Medicaid implementation—about 15 percent of the average number of white, non-elderly recipients between 2000 and 2014.[32]

The government, on the other hand, gains tax revenue from the new earnings *and* saves on transfer payments. How big, then, are the aggregate changes in net revenue relative to the cost of childhood coverage for these cohorts? I estimate Medicaid's effect on measures of total income and payroll taxes, as well as specific tax items such as the EITC using NBER's Taxsim 9.0 (Feenberg and Coutts 1993). 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 federal tax liability by just $137 (s.e. = 257) for whites, but by $379 (s.e. = 134) for nonwhites. The distribution of tax liability, however, spreads out because of the increase in extensive margin labor supply. 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. My results on extensive margin labor supply and the low level of new earnings, however, are consistent with increases in EITC. Multiplying the revenue results by the total years of early

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[31] That the effect of early Medicaid coverage on employment is larger than its effect on disability assistance 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.

[32] These effects appear to get slightly stronger at older ages. Appendix figure 4.9 plots separate IV estimates for annual employment and disability benefit receipt for each of the 15 survey years. The design is unchanged, but the results describe effects for cohorts with early childhood coverage between their mid-30s and late-40s. For both outcomes, the estimate effects are larger when the relevant cohorts are older.

childhood eligibility (0.38 years*52 million whites with any eligibility; 2.3 years*11 million nonwhites with any eligibility) gives an annual increase in tax revenue of $12.2 billion.

The newly employed adults appear to have left transfer programs, however, and the government saves *all* of these foregone benefits. Each year of early childhood eligibility reduces transfer income by $648 for whites and $253 for nonwhites, which implies an annual savings of $19.2 billion (52 million*0.37*-648 + 11 million*2.3*-253)—1.5 times as much as the new tax revenue. Other research on Medicaid has not examined its impact on public assistance, and the large public return from reducing transfers shows that this omission matters greatly for the future savings of child Medicaid coverage.

Reductions in public insurance participation also represent an important source of savings. Each year of early Medicaid eligibility reduces public insurance coverage by 5.05 percentage points (table 6, column 4), and the results for self-reported disability and disability transfer receipt suggest that most of those who leave public insurance would have qualified through disability provisions. 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 $3.8 billion per year (52 million*0.37*$3,326*-0.05+11 million*2.3*$3,326*-0.0064).[33]

The implied 35.4 billion in annual savings equals 27 percent of the cost of covering cohorts born between 1956 and 1975. To calculate costs, I first use data on total expenditures from 1966-1975 (Goodman-Bacon forthcoming) since all of this spending applies to the cohorts studied here. 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 by annual child Medicaid spending (CMS 2013 table 13.10).[34] This implies that it cost $132 billion (in 2012 dollars) to cover the relevant cohorts that contribute to the effects documented above.

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[33] These public savings accrue mainly to the federal government, meaning that Medicaid's long-run effects represent a substantial intergovernmental transfer. States paid roughly half the cost of Medicaid in the 1960s and 1970s, but the federal government recoups most of the savings through SSDI and Medicare.

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

Because several decades separate the costs and long-run benefits, discounting strongly affects the ultimate return calculations. The standard approach discounts annual benefits (2000-2015) and annual costs (1966-1993) to 2015 dollars, which yields an annual return of between 9 and 14 percent (average of 11.4 percent). If these effects approximate what similar policy changes would achieve today, then the government can expect to earn this return over a similar time frame at interest rates of 3 percent. This discounting assumption suggests that we have saved 180 percent of the original *discounted* cost just in the 16 sample years.

This exercise is less suited to examining the savings that the government has actually realized because real interest rates that determined the cost of borrowing when these expenditures were made were often much higher than 3 percent. Nominal 10-year treasury bond rates, for example, were over 10 percent for the first half of the 1980s, when about one third of the nominal expenditures on these cohorts occurred. Following the method used by the Office of Management and Budget to conduct cost-effectiveness analysis, I also discount the (nominal) costs and benefits using observed (nominal) 10-year treasury bond rates. This yields similar benefits but much higher costs, suggesting that the annual return is between 2.5 and 4.3 percent (average of 3 percent), and the government has saved 46 percent of the *true* cost of covering the original Medicaid cohorts.[35]

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. Despite large contemporaneous effects and high participation, nonwhite children covered by Medicaid do not appear to experience significant changes in adult outcomes. White children, on the other hand, are healthier adults by a number of measures—cumulative mortality and self-reported disability—work more, and are less likely to receive public transfer benefits, particularly those tied to disability. 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 forthcoming). The results consistently show that coverage at younger ages, typically below age 10, matters the most. Since Medicaid coverage provided a broad range of medical services, the adult health effects, across causes of death and types of disability, are similarly widely distributed.

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[35] These calculations ignore distortions introduced by other methods of financing, such as increased taxes (Tax Foundation 1968).

The health improvements themselves are certainly quite valuable to individuals, but the labor supply and program participation effects offset each other so that material well-being—poverty and total income—are unaffected. These changes, however, doubly benefit the government. I calculate that the government saves between 3 and 11 percent of the original cost of covering these cohorts every year, depending on the method of discounting used. Two-thirds of these savings come from reductions in cash and in-kind transfers, which have not previously been studied in this context. Early-life health programs can improve later-life health directly, have little effect on poverty because they crowd out public benefits, and yet generate significant public savings.

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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
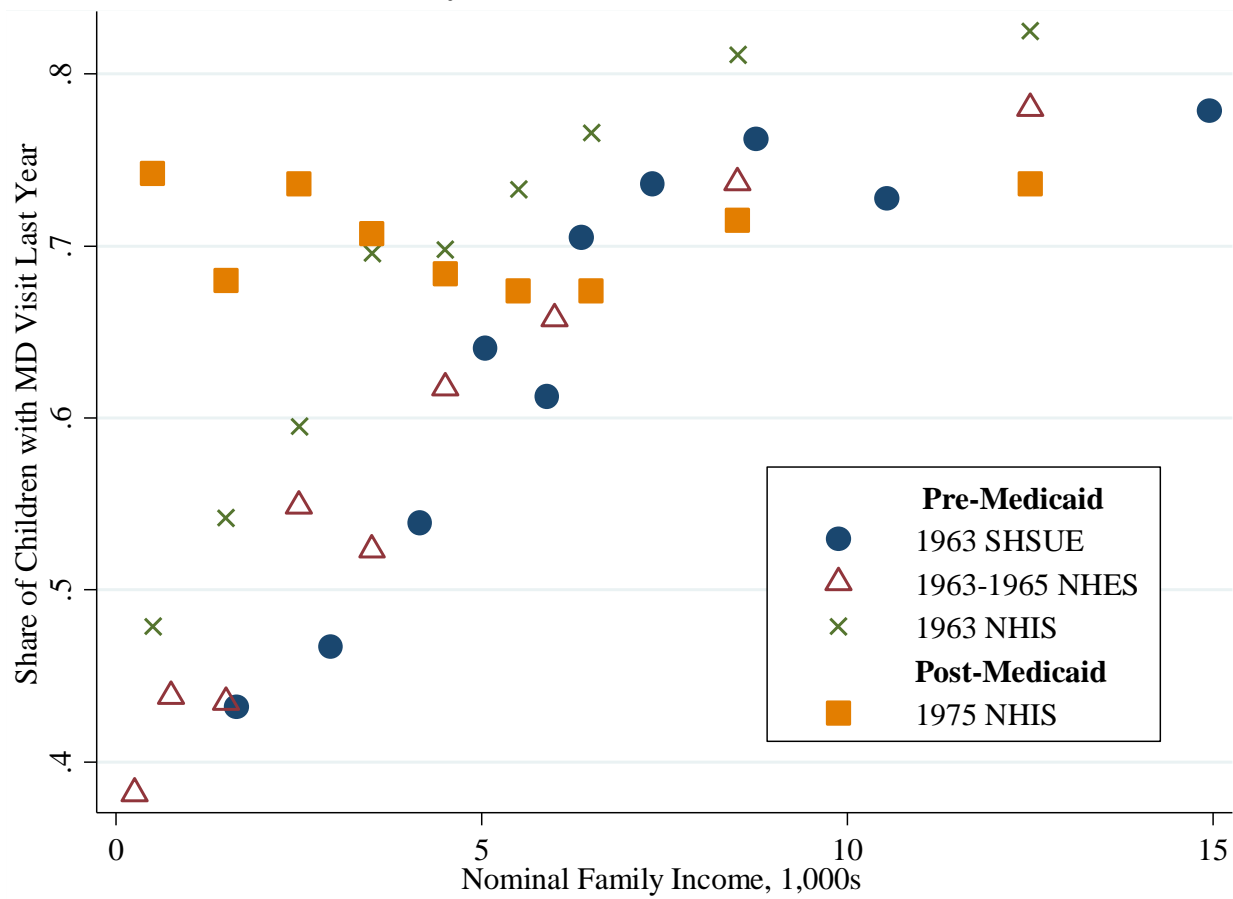
Notes: The figure plots the share of children who report having seen a doctor in the previous year in four survey data sources: the 1963 Survey of Health Services Utilization and Expenditure (CHAS 1988), the 1963-1965 National Health Examination Survey (ICPSR); and the 1963 and 1975 National Health Interview 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. Initial Categorical Eligibility Predicts Post-Medicaid Changes in Public Insurance Use
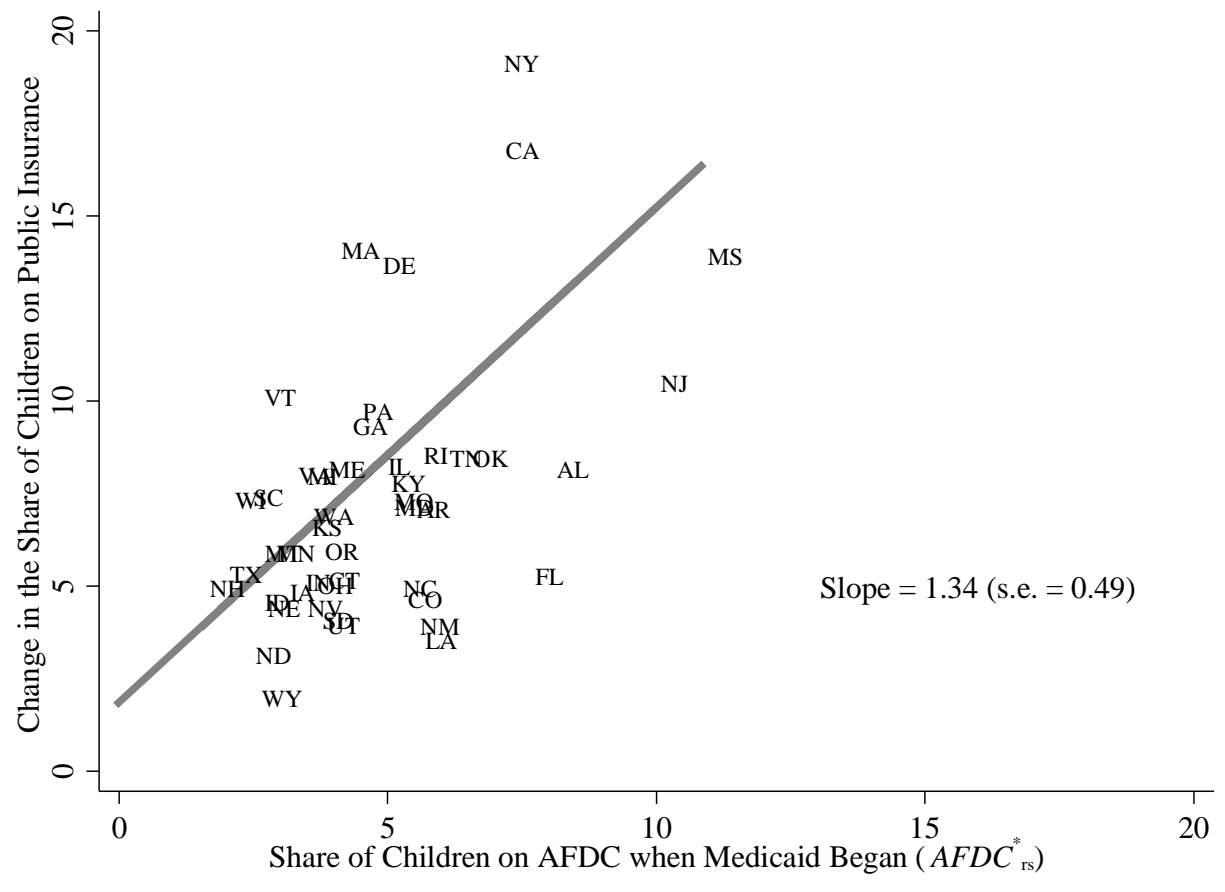

Notes: The figure plots the change in the share of children who received public insurance benefits between two years before Medicaid began and five years after. The x-axis equals the share of children who received AFDC and were therefore categorically eligible in the year that Medicaid began.

Figure 3. Cumulative Childhood Medicaid Eligibility by State of Birth, Event Cohort, and Race

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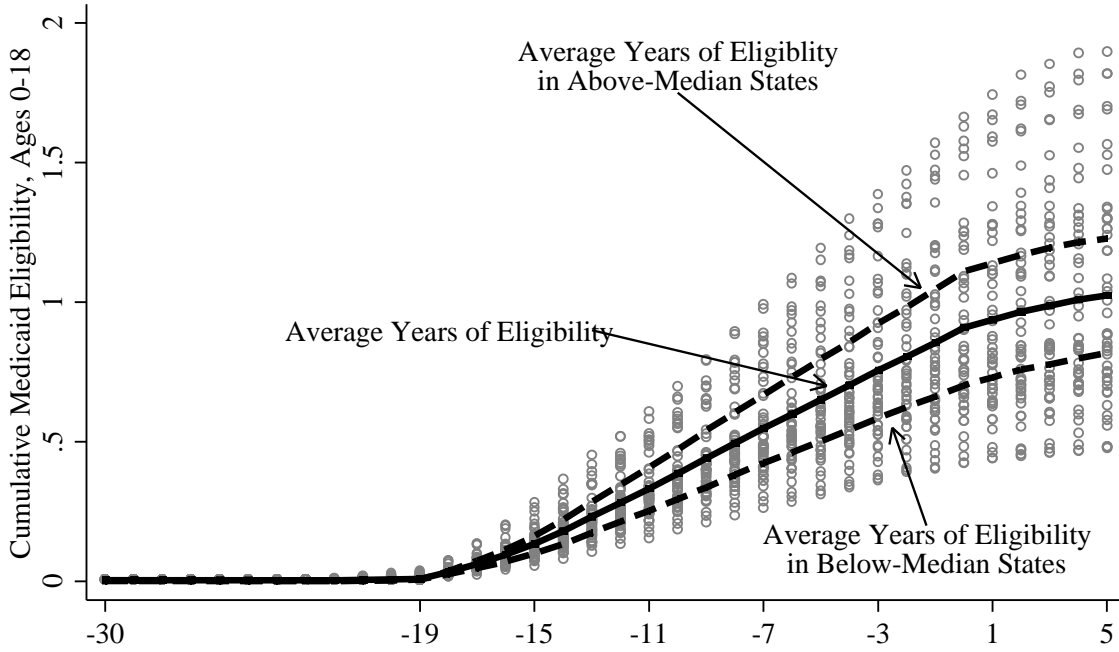
B. Nonwhite Children
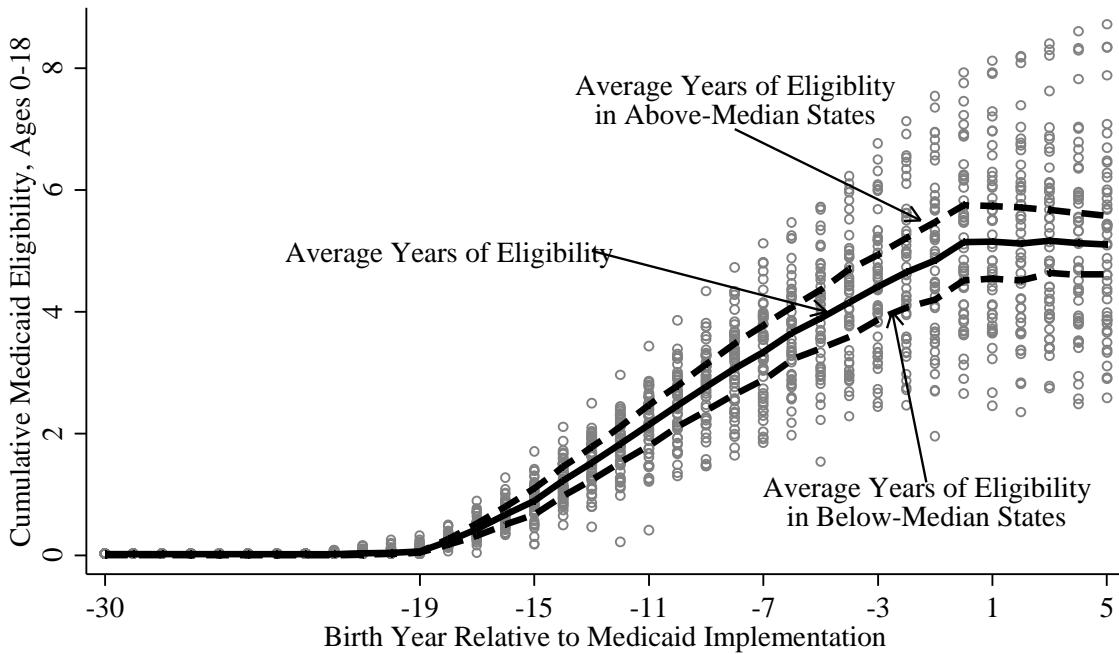
Notes: The figure plots cumulative childhood Medicaid eligibility for each state and event cohort (relative to Medicaid's introduction). Equation (2) defines cumulative eligibility, which comes from observed AFDC rates, the existence of Medicaid, and cohort migration patterns. The solid line equals average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

Figure 4. Initial Categorical Eligibility is Uncorrelated with Pre-Medicaid Trends in Health and Socioeconomic Measures

A. White Infant Health Index

Linear Trend (Year*$AFDC^*_{rs}$):
0.0022 (s.e. = 0.0041)
Pooled Levels ($AFDC^*_{rs}$):
-0.038 (s.e. = 0.042)

B. Nonwhite Infant Health Index

Linear Trend (Year*$AFDC^*_{rs}$):
-0.0003 (s.e. = 0.0004)
Pooled Levels ($AFDC^*_{rs}$):
-0.004 (s.e. = 0.005)

C. White SES Index

Linear Trend (Year*$AFDC^*_{rs}$):
-0.0024 (s.e. = 0.0039)
Pooled Levels ($AFDC^*_{rs}$):
-0.11 (s.e. = 0.11)

D. Nonwhite SES Index

Linear Trend (Year*$AFDC^*_{rs}$):
0.0021 (s.e. = 0.0010)
Pooled Levels ($AFDC^*_{rs}$):
0.05 (s.e. = 0.03)

Notes: The infant health index is an equally weighted mean of the following variables standardized by their 1950 mean and standard deviation: low and very low birth weight rates, neonatal and postneonatal infant mortality rates, the sex ratio at birth, and the share of births in a hospital. The SES index is constructed similarly (for children under age 10) and includes the 25th, 50th, and 75th percentiles of children's household incomes; the child poverty rate; the share of children in households whose head has a high school degree or more, is in the labor force, and is employed; the share of children who live with no parents or both parents; household size; and the share of children ages 4-6 enrolled in school. The closed triangles are coefficients on the interaction between year dummies and $AFDC^*_{rs}$, and the straight lines are the estimated coefficient on an interaction between continuous year and $AFDC^*_{rs}$. The estimated slope and standard error are noted in the figure. The coefficient for "pooled levels" comes from a bivariate regression of the index on $AFDC^*_{rs}$. Regressions are weighted by births or the sum of Census weights, and standard errors (and the dashed 95-percent pointwise confidence intervals) are clustered by state.

Figure 5. Initial Categorical Eligibility Predicts Cumulative Eligibility: First-Stage Relationship Between AFDC* and Expected Years of Medicaid Eligibility by Race


A. White

B. Nonwhite
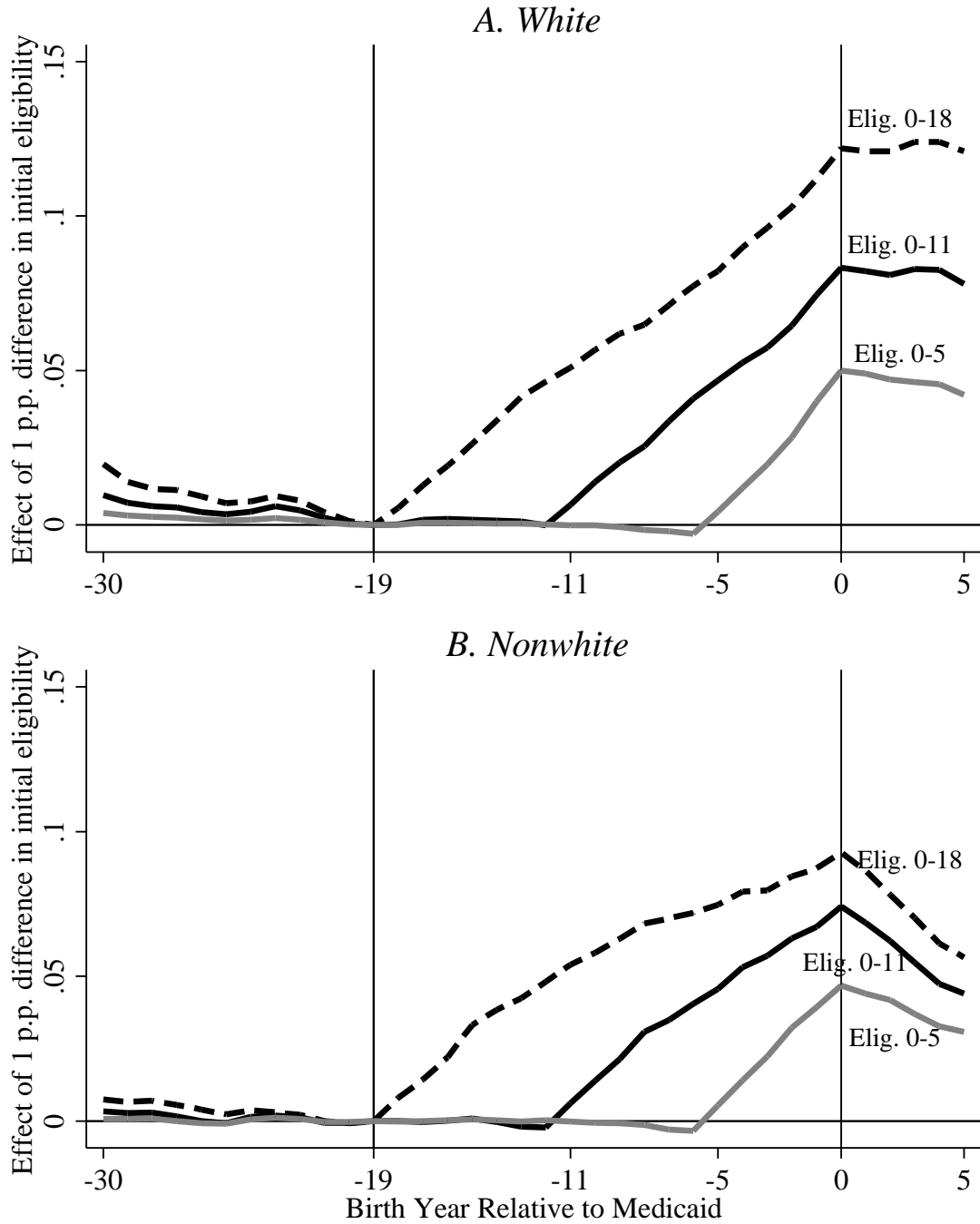
Notes: The dependent variable is each cohort's cumulative, migration-adjusted Medicaid eligibility for ages 0-18. The figure plots the estimated coefficients on interactions between $AFDC_{rs}^*$ and event-time dummies for each of 30 years before and five years after Medicaid. Time -19 is omitted. The 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 nonwhites than whites, the effect per point of the AFDC rate is smaller both because of the model's controls and because the underlying AFDC differences across high- and low-AFDC states are much larger for nonwhite than for white AFDC rates.

Figure 6. Early Childhood Eligibility Lowers Adult Mortality: Event-Study Estimates of Medicaid's Effect on log 25-Year Non-AIDS Mortality Rates (coefficients × 100)
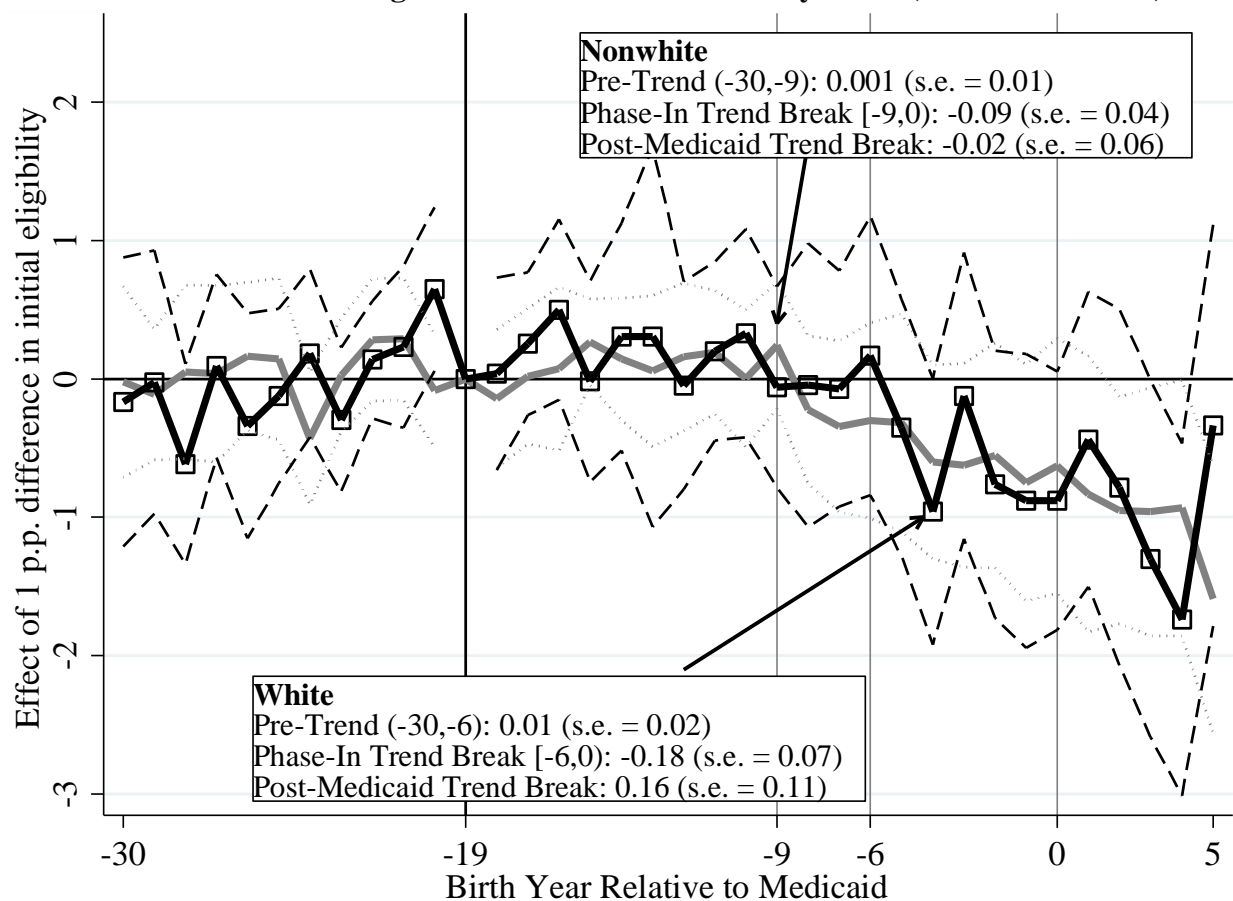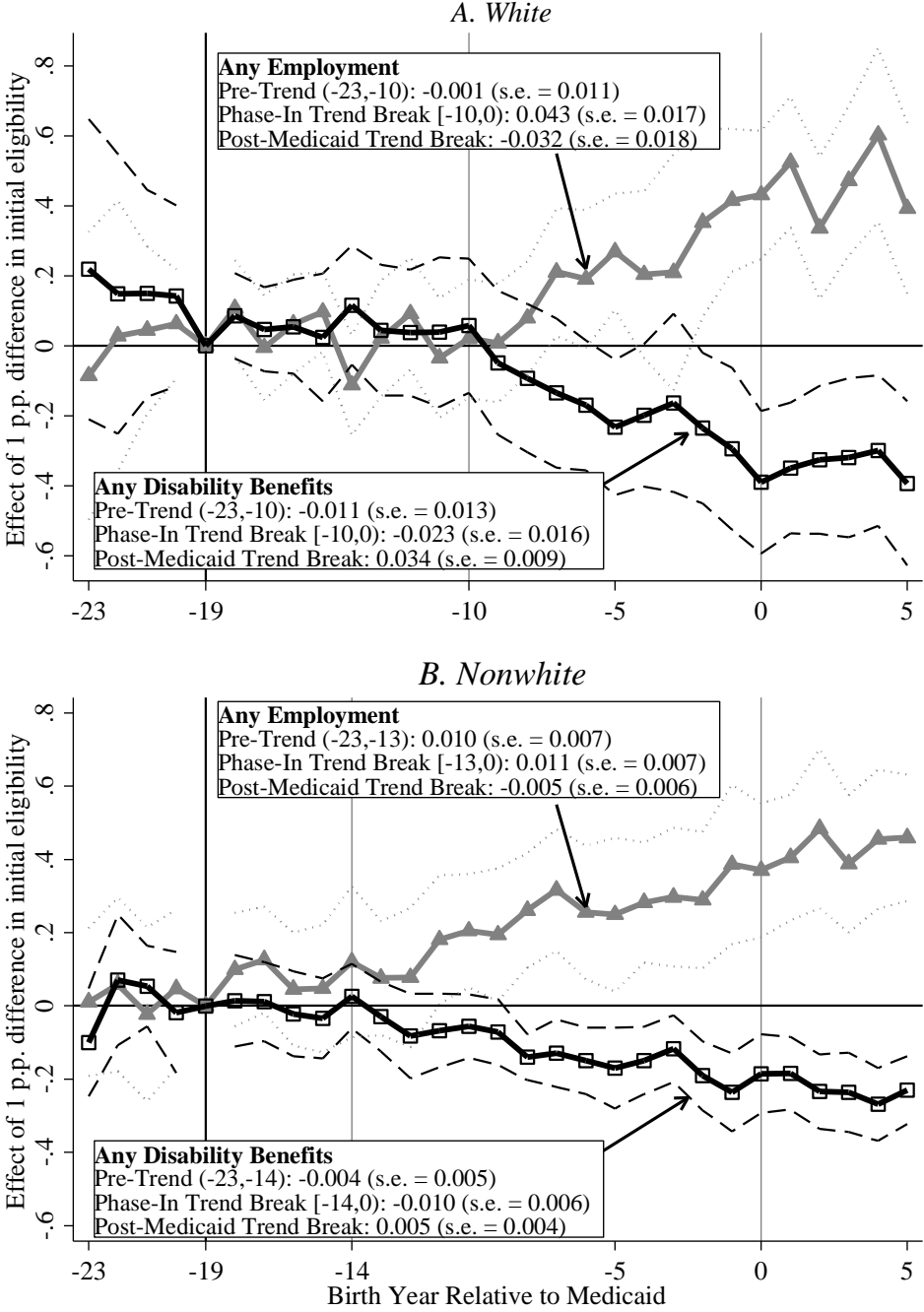

Notes: The figure plots the estimated coefficients on interactions between $AFDC^*_{rs}$ and event-time dummies for each of 30 years before and five years after Medicaid. Time -19 is omitted. The model includes birth-state, region-by-birth-year, and Medicaid-year-by-birth-year fixed effects; birth year 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 7. Early Childhood Eligibility Lowers Adult Disability: Event-Study Estimates of Medicaid's Effect on Rates of Ambulatory Difficulty by Race (coefficients×100)
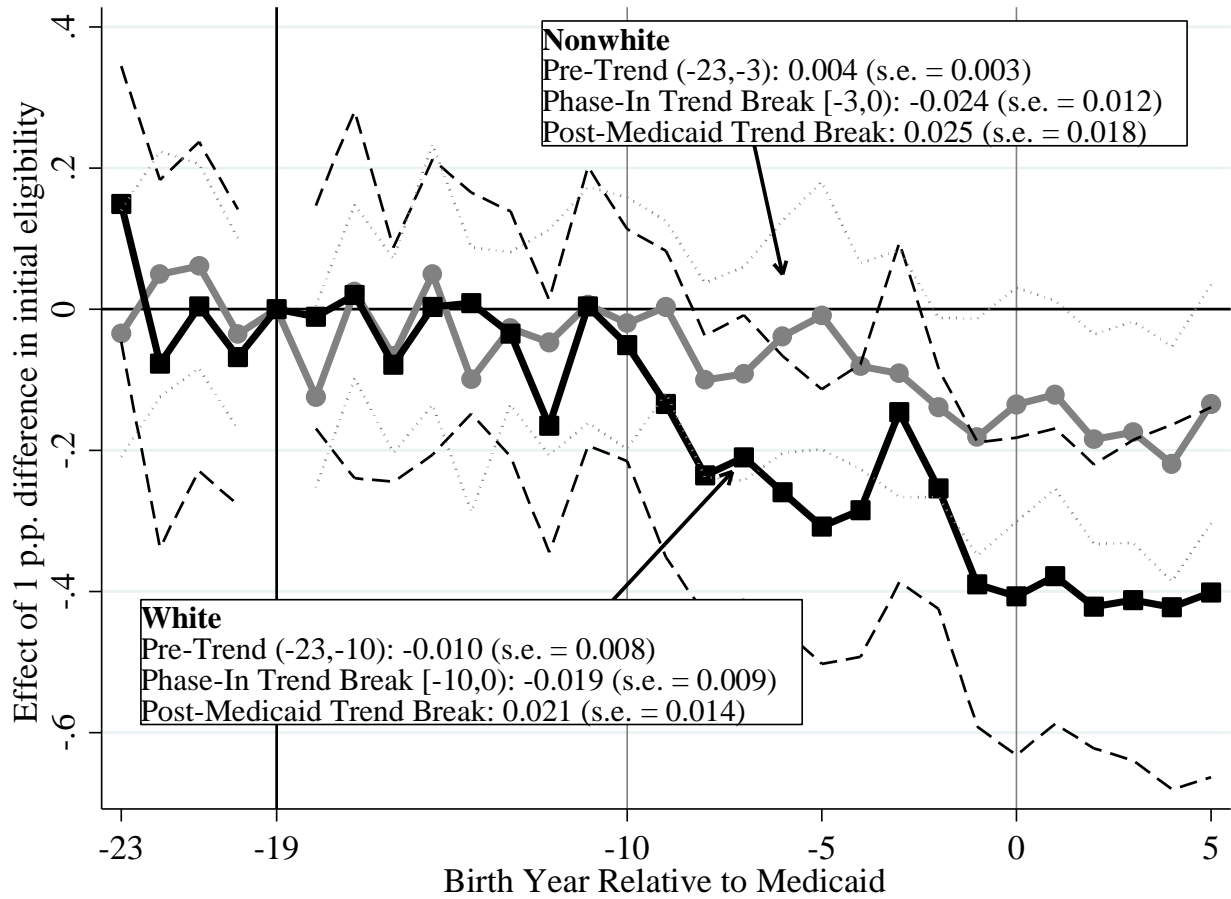

Notes: The dependent variable is the share of respondents in each state-of-birth-by-cohort cell who report having a "long-lasting condition that substantially limits one or more basic physical activities such as walking, climbing stairs, reaching, lifting, or carrying" (ambulatory difficulty). The estimation sample includes Census/ACS years 2000-2007, when the question text was comparable (see appendix figure 1.3). The figure plots the estimated coefficients on interactions between $AFDC^*_{rs}$ and event-time dummies for each of 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 event-times prior to -15. Estimates are weighted by the sum of the Census weights in each cell (unweighted estimates are similar and plotted in appendix figure 3.1). 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 2.4.

Figure 8. 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)

A. White

Any Employment
Pre-Trend (-23,-10): -0.001 (s.e. = 0.011)
Phase-In Trend Break [-10,0): 0.043 (s.e. = 0.017)
Post-Medicaid Trend Break: -0.032 (s.e. = 0.018)

Any Disability Benefits
Pre-Trend (-23,-10): -0.011 (s.e. = 0.013)
Phase-In Trend Break [-10,0): -0.023 (s.e. = 0.016)
Post-Medicaid Trend Break: 0.034 (s.e. = 0.009)

B. Nonwhite

Any Employment
Pre-Trend (-23,-13): 0.010 (s.e. = 0.007)
Phase-In Trend Break [-13,0): 0.011 (s.e. = 0.007)
Post-Medicaid Trend Break: -0.005 (s.e. = 0.006)

Any Disability Benefits
Pre-Trend (-23,-14): -0.004 (s.e. = 0.005)
Phase-In Trend Break [-14,0): -0.010 (s.e. = 0.006)
Post-Medicaid Trend Break: 0.005 (s.e. = 0.004)

Birth Year Relative to Medicaid

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 figure 4.8 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 7. The nonwhite estimates also adjust for a linear trend interacted with $AFDC_{rs}^*$ for event-times prior to -15.

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Figure 9. Early Childhood Medicaid Eligibility Shifts Income from Benefits to Earnings: Instrumental Variables Estimates on the Distribution of Income by Source
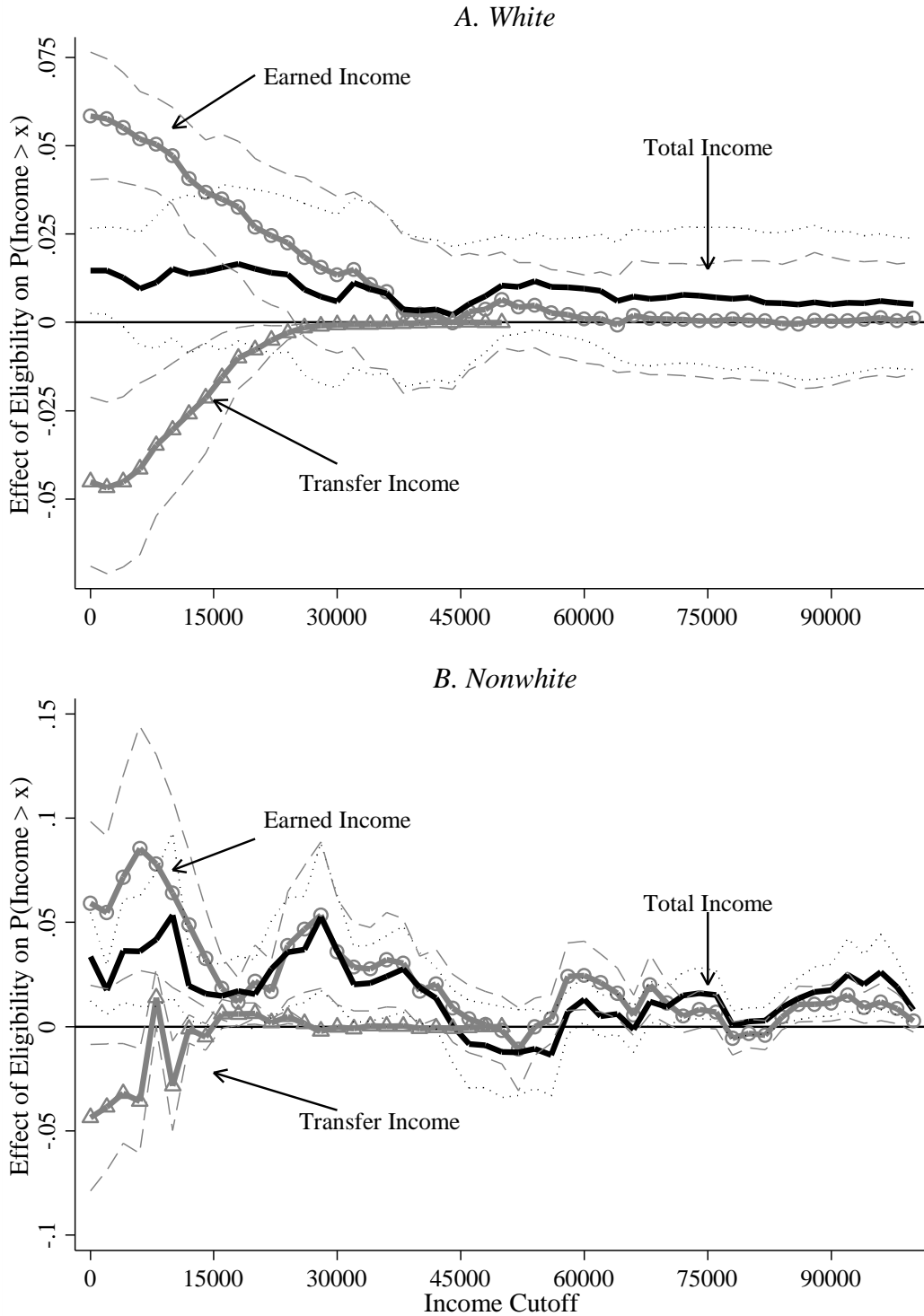
Notes: The figure plots instrumental variables estimates of the effect of cumulative Medicaid eligibility at ages 0-11 on the probability of earnings, transfer income, or total income greater than the amount on the *x*-axis (measured in $2,000 bins in 2012 dollars). The sample includes Census/ACS years from 2000 to 2015. $50,000 is the maximum of the transfer income variable.

Figure 10. Effects on Health and Labor Market Outcomes are Robust to Alternative Specifications, Samples, and to Falsification Tests on Older Cohorts

A. White

B. Nonwhite
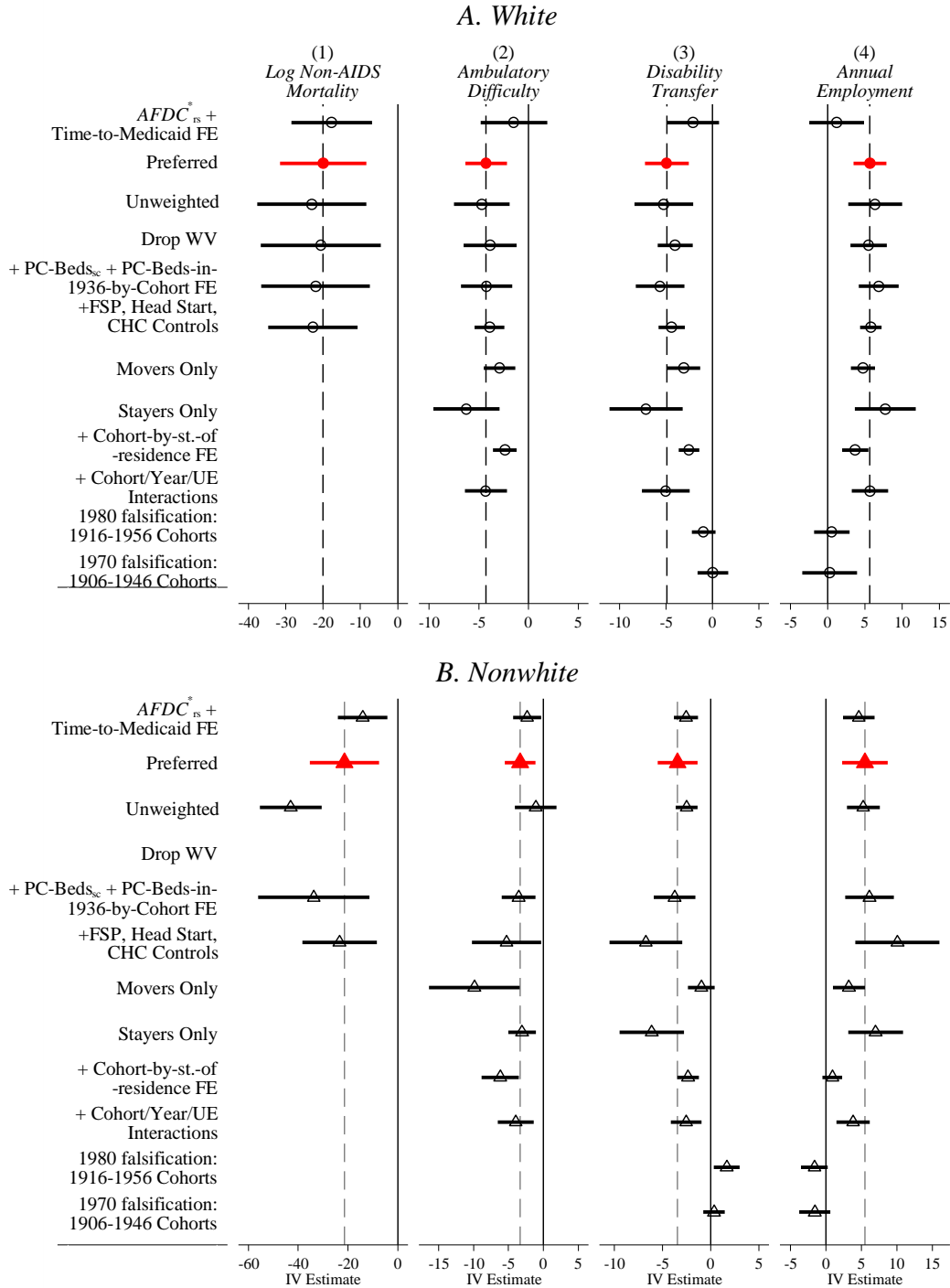
Notes: The figure plots IV point estimates and confidence intervals (based on standard errors clustered by birth state) for early childhood Medicaid coverage. Row 1 includes only $AFDC^*_{rs}$ and event-cohort dummies as controls. Row 2 is the preferred specification. Row 3 is an unweighted version of 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

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Health Centers. Rows 7 and 8 keep respondents living out of (movers) or in (stayers) their birth state. Row 9 expands the data to the state-of-residence/state-of-birth/cohort level and includes fixed effects for cohort by state of residence. Row 10 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 11 and 12 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 transfer variable in these models refers to any cash transfer instead of disability related transfers.

Figure 11. Early Childhood Eligibility is Not Correlated with Cumulative Exposure to Other Safety Net Programs

A. White
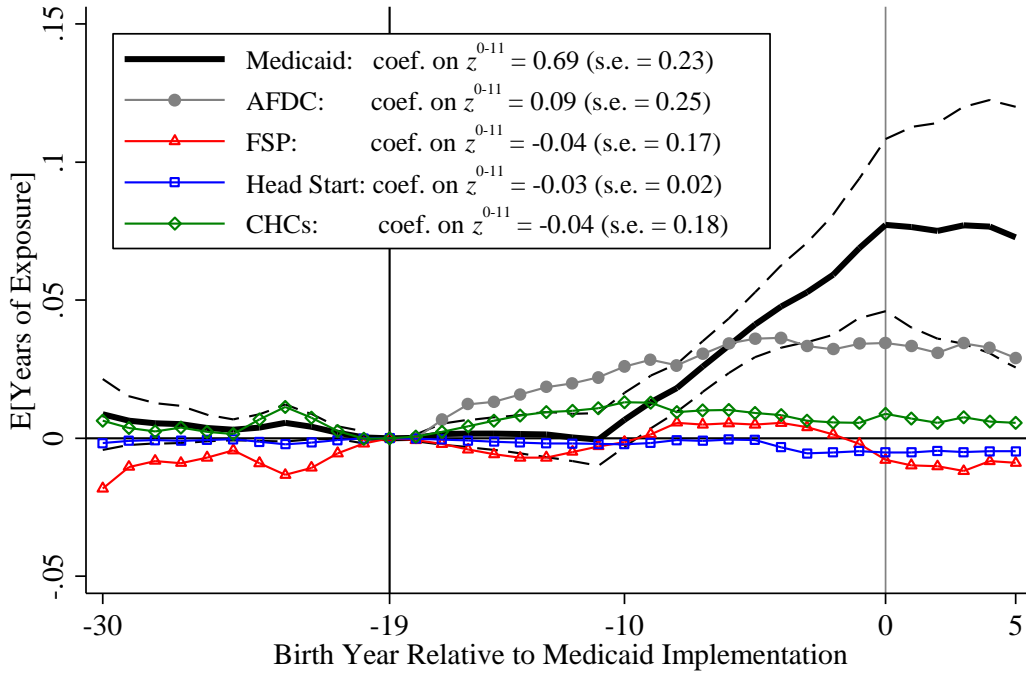
B. Nonwhite
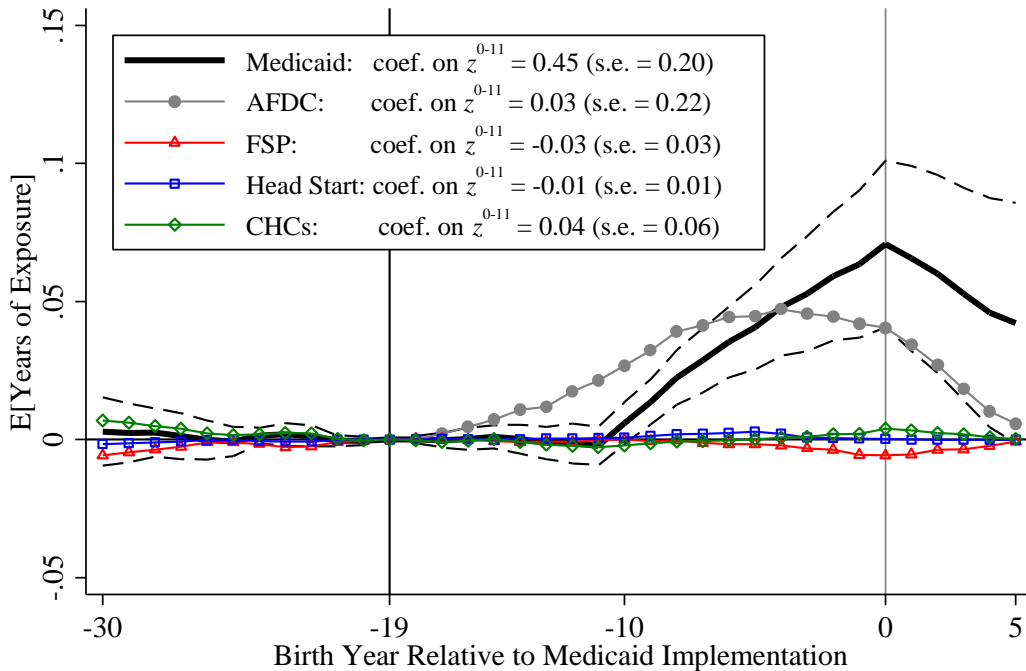
Notes: Data on the race of AFDC recipients are available starting in 1948. Cumulative AFDC exposure follows equation (2) but without the post-Medicaid dummy. Data on Food Stamp implementation comes from Almond, Hoynes and Schanzenbach (2011). Data on Head Start and Community Health Centers come from 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).

	(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			
Angrist/Pischke *F*-statistic		44.5	109.7	31.2
			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			
Angrist/Pischke *F*-statistic		77.3	33.7	18.4

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	-18.00	-3.80	-4.42	6.55
	[6.28]	[1.68]	[1.47]	[2.00]
Ages 6-11	-9.61	-4.93	-5.62	4.38
	[11.10]	[1.14]	[1.19]	[1.36]
Ages 12-18	5.23	-1.11	-1.05	-0.76
	[7.33]	[1.56]	[2.34]	[1.85]
H$_0$: 0-5=6-11 (*p*-val)	*0.576*	*0.582*	*0.286*	*0.452*
H$_0$: 6-11=12-18 (*p*-val)	*0.371*	*0.018*	*0.011*	*0.014*
B. White Estimates for Early Eligibility				
Early Medicaid Eligibility	-20.00	-4.26	-4.90	5.67
	[5.90]	[1.06]	[1.21]	[1.12]
	(0.018)	(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	-26.80	-3.30	-3.94	6.58
	[10.40]	[1.24]	[1.42]	[2.35]
Ages 6-11	-13.50	-0.56	-2.71	4.05
	[5.34]	[0.61]	[0.65]	[1.04]
Ages 12-18	-0.71	-0.43	-1.18	1.93
	[3.28]	[0.94]	[0.65]	[0.86]
H$_0$: 0-5=6-11 (p-val)	*0.199*	*0.040*	*0.258*	*0.227*
H$_0$: 6-11=12-18 (p-val)	*0.016*	*0.903*	*0.010*	*0.005*
D. Nonwhite Estimates for Early Eligibility				
Early Medicaid Eligibility	-21.40	-3.33	-3.43	5.53
	[7.11]	[1.13]	[1.05]	[1.63]
	(0.004)	(0.004)	(0.004)	(0.004)
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. Nonwhite estimates partial out a pre-trend estimated on data through event-cohort -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 25-Year Mortality Rates by Cause (coefficients×100)

	(1) All Causes	(2) Non-AIDS-Related Causes	(3) Internal	(4) Infectious	(5) Chronic	(6) Cardio-vascular	(7) Cancer	(8) Suicide	(9) External (Homicide + Accidents)
Cause of Death:									
				A. White, 1980-2004					
Early Eligibility (0-5)	-26.0	-20.0	-22.6	5.7	-30.8	-10.4	-18.4	-39.7	-2.8
	[8.9]	[5.9]	[7.1]	[26.6]	[10.6]	[10.8]	[9.4]	[13.8]	[8.6]
	(0.028)	(0.016)	(0.016)	(0.872)	(0.040)	(0.384)	(0.064)	(0.040)	(0.844)
Mean Dependent Variable (deaths per 100,000)	5,630	5,450	4,100	163	1,150	1,330	1,590	374	1,030
				B. Nonwhite, 1980-2004					
Early Eligibility (0-11)	-30.4	-21.4	-15.8	-35.6	-14.7	-24.1	-14.8	-36.0	-30.9
	[10.1]	[7.1]	[5.6]	[13.0]	[8.1]	[6.8]	[6.2]	[12.0]	[10.9]
	(0.004)	(0.008)	(0.016)	(0.012)	(0.128)	(0.004)	(0.072)	(0.004)	(0.008)
Mean Dependent Variable (deaths per 100,000)	12,700	10,800	8,490	684	2,790	3,400	2,300	247	2,400

Notes: The table presents instrumental variables estimates of Medicaid's effect on log cumulative mortality rates (1980-2004) 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.

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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)	-3.33	-3.45	1.82	1.93	-0.30	0.730
	[1.13]	[1.51]	[1.00]	[0.87]	[0.49]	[0.59]
	(0.004)	(0.012)	(0.084)	(0.108)	(0.600)	(0.176)
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 7 and table 3. 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) Any Public Assistance	(2) Disability Benefits (SSDI/SSI)	(3) TANF or General Assistance	(4) Public Insurance	(5) Any Insurance
			A. White		
Early Medicaid Eligibility (0-11)	-4.42	-4.90	0.57	-5.05	-0.09
	[1.19]	[1.21]	[0.11]	[1.14]	[1.01]
	(0.012)	(0.008)	(0.004)	(0.004)	(0.924)
Mean Dependent Variable	5.9	4.9	1.3	11.7	87.4
			B. Nonwhite		
Early Medicaid Eligibility (0-11)	-3.46	-3.43	0.26	-0.64	8.61
	[1.26]	[1.05]	[0.26]	[1.29]	[2.08]
	(0.004)	(0.004)	(0.332)	(0.648)	(0.004)
Mean Dependent Variable	11.6	8.8	3.5	23.0	79.6

Notes: The table presents instrumental variables estimates of Medicaid's effect on cash transfer receipt and insurance coverage in the 2000-2015 Census/ACS. The specification is the same as in figure 7 and table 3. 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 (0-11)	-5.36	5.14	5.67	4.05
	[1.02]	[0.93]	[1.12]	[0.69]
	(0.004)	(0.004)	(0.004)	(0.004)
Mean Dependent Variable	17.3	78.5	84.9	55.5
		B. Nonwhite		
Early Medicaid Eligibility (0-11)	-5.66	5.34	5.53	2.17
	[1.63]	[1.6]	[1.63]	[0.85]
	(0.004)	(0.004)	(0.004)	(0.004)
Mean Dependent Variable	23.2	69.2	78.0	48.3

Notes: The table presents instrumental variables estimates of Medicaid's effect on labor supply measures in the 2000-2015 Census. The specification is the same as in figure 7 and table 3. 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 7. Instrumental Variables Estimates of Medicaid's Effect on Educational Attainment (coefficients×100)

	(1) 12 Years of School	(2) 4 Years of College
	A. White	
Early Medicaid Eligibility (0-11)	2.28	1.28
	[0.94]	[1.78]
	(0.180)	(0.628)
Mean Dependent Variable	93.8	32.0
	B. Nonwhite	
Early Medicaid Eligibility (0-11)	3.01	-0.64
	[1.14]	[0.91]
	(0.012)	(0.540)
Mean Dependent Variable	93.8	32.0

Notes: The table presents instrumental variables estimates of Medicaid's effect on the share of each cohort that completed 12 or 16 years of education. The cohort means are based on respondents born between 1936 and 1976 and observed between ages 35 and 55 in the 1980 Census, 1990 Census, or 2000-2015 Census/ACS. The specification is the same as in figure 7 and table 3. 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 8. Instrumental Variables Estimates of Medicaid's Effect on Income by Source, Wages, and Poverty

	(1)	(2)	(3)	(4)	(5)	(6)
	Earned Income	Earned Income (Trimmed: $100k)	Log Wage (coef.×100)	Transfer Income	Total Income (Trimmed: $100k)	Poverty Rate (coef.×100)
A. White						
Early Medicaid Eligibility (0-11)	1,231	1,175	-5.28	-648	634	1.24
	[1,668]	[633]	[1.73]	[196]	[711]	[1.15]
	(0.604)	(0.108)	(0.012)	(0.004)	(0.472)	(0.680)
Mean Dependent Variable	$47,125	$34,024	$28.06	$542	$35,832	7.8
B. Nonwhite						
Early Medicaid Eligibility (0-11)	4,996	2,034	6.96	-253	1,128	-3.08
	[1,614]	[711]	[2.61]	[115]	[523]	[1.32]
	(0.004)	(0.004)	(0.004)	(0.004)	(0.020)	(0.016)
Mean Dependent Variable	$31,917	$27,123	$22.89	$905	$29,232	18.50

Notes: The table presents instrumental variables estimates of Medicaid's effect on average income in the 2000-2015 Census. Columns 2 and 5 use average of income below $100,000. Column 3 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 7 and table 3. 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.