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. For whites, Medicaid eligibility before age 10 reduces mortality and disability, 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's annual discounted return of about 6 percent on the original expenditure for these cohorts' childhood Medicaid coverage, two thirds of which comes from lower cash transfer payments.

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In 2012, the joint federal and state public health insurance programs, Medicaid and the State Children's Health Insurance Program, covered 40 percent of children and cost $433 billion. Costs have been central to recent arguments about the size of the Medicaid program (Sommers and Epstein 2013). Current federal budget proposals would convert Medicaid into a block grant program, several states have implemented major cuts to eligibility and services, and six states have recently considered opting out of the program (Adamy and King Jr. 2010).

Short-run empirical evaluations show that Medicaid meaningfully improves health, but this has not resolved debates about the program's future. For example, while Medicaid saves lives (Currie and Gruber 1996a, b, Goodman-Bacon 2015, Sommers, Baicker, and Epstein 2012), the costs per life saved are generally high and health effects are small for middle-income groups. Experimental estimates from the Oregon Health Insurance Experiment (OHIE) show 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). Therefore, short-run health effects alone may not justify the size of the program (Finkelstein, Hendren, and Luttmer 2015).

Accounting for Medicaid's effects over the course of its recipients' lives, however, may drastically change this cost-benefit calculation. Because it primarily covers children during critical periods, Medicaid may have its largest effects later in life (Cunha, Heckman, and Schennach 2010). Medicaid-induced improvements in adult health and economic outcomes could also *lower* public costs by reducing transfers or health care spending in programs linked to poor health, or by increasing tax revenue (Brown, Kowalski, and Lurie 2014).

New research based on eligibility expansions from the 1980s 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, Currie, Decker, and Lin 2008, Levine and Schanzenbach 2009, Miller and Wherry 2014, Wherry and Meyer 2013, Wherry et al. 2015). Yet these studies observe cohorts in their 20s, meaning longer-run effects, especially those tied to health conditions that typically emerge at older ages, may be significantly larger or smaller than existing estimates. Furthermore, the 1980s expansions affected children in ways other than increased insurance coverage—including changes in consumption for families who dropped private insurance and increased eligibility for other transfer programs—leaving considerable uncertainty as to the mechanisms for both short- and long-run effects.

This paper provides new evidence on Medicaid's longer-run effects by exploiting the program's introduction between 1966 and 1970 and the federal mandate that Medicaid cover all cash welfare recipients ("categorical eligibility"). Treated cohorts are between their mid-30s and mid-50s today—much older than those treated in the 1980s. Medicaid's introduction was also not packaged with other policy changes and had little scope for crowd out because it targeted groups with extremely low health insurance rates. Finally, because Medicaid implementation affected all categorically eligible children, I can compare long-run effects for those exposed at different ages.

Medicaid's introduction and the legislative connection between eligibility and welfare receipt meant that contemporaneous public insurance eligibility increased suddenly after Medicaid and that the size of this increase was larger in areas with higher welfare participation. From a long-run perspective, cohorts born closer to Medicaid spent more years exposed to any Medicaid program and those from higher-welfare states were more likely to be eligible in each year. Thus, *cumulative* childhood Medicaid eligibility phased in gradually across cohorts, but more quickly for those from higher-welfare states.

To estimate the effect of cumulative Medicaid eligibility on adult outcomes, I use a difference-in-differences model that compares cohorts born at different times relative to Medicaid implementation in states with different categorical eligibility rates in the year *of* implementation. Medicaid programs were not fully operational in their first calendar year, so the initial welfare rate provides a fixed way to compare states with different levels of pre-Medicaid categorical eligibility. Variation in initial welfare rates resulted from long-standing institutional features of states, was uncorrelated with levels and trends in a range of economic, demographic, health, and policy characteristics, but strongly predicts cumulative Medicaid eligibility and contemporaneous Medicaid participation (Goodman-Bacon 2015). This suggests that comparing adult outcomes across cohorts born in different years relative to Medicaid implementation and in states with different levels of initial welfare rate is unlikely to confound the program's effects with trends in health or socioeconomic factors.

I merge measures of initial and cumulative Medicaid eligibility with state-by-year-of-birth data on adult outcomes from the 1980-1999 Vital Statistics Multiple Cause of Death files and the 2000-2014 Census and American Community Surveys (ACS). The analysis of adult health considers disability rates, an especially relevant health outcome with respect to labor market outcomes, and mortality, a separate health measure in itself and a potential source of selection. The analysis of labor market outcomes focuses on labor supply, program participation, and the distribution of income by source (earnings versus transfers). This leads to a unified analysis of Medicaid's effect on adult health and labor supply, as well as a fuller accounting of Medicaid's longer-run effects on public revenues and costs than has previously been possible.

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 5 years after Medicaid. Adult outcomes track patterns of eligibility closely: they are uncorrelated with initial Medicaid eligibility for respondents who are too old to have qualified as children; diverge gradually in higher- versus lower-eligibility states for cohorts with increasing years of exposure; and flatten out for post-Medicaid cohorts with the same amount of predicted childhood eligibility. To contextualize the semi-parametric estimates in terms of the effect of an additional year of childhood eligibility, I pool the cross-state and cross-cohort variation in instrumental variables (IV) models that use predicted cumulative eligibility based only on initial welfare rates in each cohort's birth state as an instrument for actual cumulative Medicaid eligibility.

The results show that cohorts with early life Medicaid eligibility experience lower adult mortality and disability rates. Among white adults, these health improvements increased and reduced the probability of receiving disability benefits and public insurance. New earnings largely offset lower transfers leaving individual income unchanged, but the government saves on benefit payments *and* earns a small amount of new income tax revenue: $18 billion per year in total. Child Medicaid spending on these cohorts was relatively low—about $132 billion in 2012 dollars—so these changes imply about a 6 percent discounted return *every year* on the initial investment in child Medicaid coverage. Just over two-thirds of this return comes from reductions in cash transfer payments and the remainder comes from increased income tax revenue (14 percent) and lower public insurance spending (16 percent).

I. EVIDENCE ON MEDICAID'S EFFECTS IN THE SHORTER- AND THE LONGER-RUN

Quasi-experimental evidence shows that Medicaid coverage increases the use of primary and acute care, reduces mortality, and improves health and financial outcomes (see Buchmueller, Ham, and Shore-Sheppard [2015] for a review). Medicaid has typically been valued based on these short-run effects, and the conclusions vary widely (Finkelstein, Hendren, and Luttmer 2015, Paradise and Garfield 2013, U.S. House Select Commitee on Children 1990). Yet there are good reasons to think that the largest benefits of child Medicaid coverage may come later in life. If Medicaid-funded care prevents future illness or limits harmful sequelae, for example, then a contemporaneous research design will fail to capture these effects.

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), adult health (mortality: Brown, Kowalski, and Lurie 2014, obesity, BMI, chronic illness: Miller and Wherry 2014), and reductions in adult hospitalizations for chronic disorders (Wherry and Meyer 2013, Wherry et al. 2015).[1] 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 cohorts affected by the 1980s expansions are mainly observed during their 20s. The 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.[2] 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 but not on an index of economic outcomes.[3] Yet direct estimates of longer-run effects are central to conclusions about the total return to Medicaid spending. When calculating Medicaid's return on investment, for example, Brown, Kowalski, and Lurie (2014) assume that effects at age 28 will persist for 32 years, and this assumption accounts for three quarters of the estimated return (42 of 56 cents per dollar, p. 21).

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

[1] De La Mata (2012) exploits discontinuities in income eligibility rules and finds no effect on contemporaneous or later-childhood self-reported health, school days lost or obesity. The effects on insurance coverage, however, are zero or negative, so the reduced form results should be zero even if insurance coverage improves health.
[2] 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 between 30 and 64 at more than five times the rate among adults aged 19-30, the typical age range used in existing long-run studies (MPC and SHADAC 2012).
[3] Boudreaux, Golberstein, and McAlpine (2016) use the Panel Study of Income Dynamics (PSID) which, when stratified by childhood income, leaves few observations and limited power to examine economic effects.

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).

Despite notable consistency across studies about the longer-run health and socioeconomic effects of the 1980s expansions, the evidence on mechanisms is mixed. The modest birth weight effects in Currie and Gruber (1996b) match well with reductions in endocrine-related chronic conditions and BMI in young adulthood (Miller and Wherry 2014, Wherry et al. 2015) through the fetal origins hypothesis.[4] The effect of infant coverage on test scores in Levine and Schanzenbach (2009) also fits with recent estimates of the effect of acute care at birth (particularly artificial lung surfactant) on subsequent academic achievement (Bharadwaj, Løken, and Neilson 2013). But the longer-run effects on educational attainment and earnings stem from child and *not* infant coverage. There is little evidence on which aspects of the child Medicaid expansions have these effects and whether they work through sustained health improvements or some other channel.

Medicaid implementation can help address both questions. Vital Statistics and Census/ACS data facilitate a unified analysis of Medicaid's effects on health (mortality and disability), human capital, labor supply and earnings. Since 50 years have passed since Medicaid began, I can estimate much longer-run effects than has previously been possible.

II. EXPECTED EFFECTS OF MEDICAID IMPLEMENTATION ON LATER-LIFE OUTCOMES

The original introduction Medicaid 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 care were uncommon and often of low quality

[4] Perinatal epidemiologists have expressed skepticism about the ability of (Medicaid-funded) prenatal care to increase birth weight—the primary motivation for the 1980s eligibility expansions. Medicaid's connection with WIC, however, provides a plausible channel for this effect. In fact, Bitler and Currie (2004) conclude that "it is likely to be difficult to disentangle the effects of Medicaid coverage at birth and WIC participation over the child's early life." If Medicaid's birth weight effects work through WIC referrals, then the same effects could have been attained at lower cost through WIC outreach (Rossin-Slater 2013). Maternal stress could also play a role (Aizer, Stroud, and Buka 2015), which is consistent with the findings of the Oregon Health Insurance Experiment (Finkelstein et al. 2012).

(Goodman-Bacon 2015).[5] As a result, poor children frequently went without medical care. Figure 1 shows that less than half of poor children in the early 1960s had seen a doctor in the last 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 for non-poor children (National Center for Health Statistics 1965), and they suffered more often from a range of specific symptoms.[6] In terms of 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

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[5] 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).

[6] 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 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.

Families with Dependent Children (AFDC) program (DHEW 1976). All states except Alaska (1972) and Arizona (1982) implemented Medicaid between 1966 and 1970.

During Medicaid's first decade states also expanded their efforts 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).[7] President Johnson stressed EPSDT's potential later-life effects when he advocated for the program: "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).

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 is reflected reductions in uninsurance (Goodman-Bacon 2015, figure 1).

The large increase in coverage meant that poor children received substantially more medical care. Appendix table 2.1 presents cross-sectional utilization rates by Medicaid eligibility across 10 surveys from before and after Medicaid showing that children eligible for Medicaid used much more medical care than ineligible poor children in the same state, or similar children in non-Medicaid states.[8] 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. State Differences in Contemporaneous and Cumulative AFDC-Based Medicaid Eligibility

Because Medicaid—through the categorical eligibility requirement—built on existing cash welfare system, it inherited the long-standing and large state differences in the size of these programs. Therefore, while all states experienced a sudden increase in public insurance eligibility

[7] Stevens and Stevens (1974) discuss lags in the promulgation of EPSDT regulations in the late 1960's, but emphasize that the program was a major new proposal, requiring screened children to receive 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).

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

when Medicaid began, this jump was much larger in higher-welfare states. Moreover, patterns of AFDC participation also differed strongly by race. For example, 1.3 percent of white children in Nevada were eligible through AFDC when its 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 are reversed: 22 percent in Nevada versus 10 percent in New Mexico. I therefore stratify the analysis by race to capture the distinct variation in white and nonwhite initial eligibility.

The sharp change in eligibility after Medicaid translates to a phasing in of cumulative eligibility across cohorts born in the years leading to implementation. A white child born (and raised) in Nevada in 1950 had two childhood years under Medicaid and a 1.3 percent eligibility rate at age 17, and a 1.6 percent eligibility rate at age 18. Her expected number of years of *full* eligibility is 0.029.[9] A similar child from New Mexico, however, had higher eligibility and three years of exposure, for 0.17 years of cumulative eligibility. For the 1960 cohort, born 10 years closer to Medicaid, cumulative childhood eligibility is 0.47 for Nevada and 0.85 for New Mexico.

To construct a cohort-level measure of cumulative childhood Medicaid eligibility I use state-by-year-by-race data on AFDC rates, statutory Medicaid implementation dates, and state-of-residence information in the Census. 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 *and* the states of residence (ℓ) of that cohort:

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

$\sigma_{rsc}^{y}(\ell)$ is the share of cohort *c* (race *r*) born in state *s* living in state ℓ in year *y*.[10] The Medicaid implementation dummy, $1\{y \geq t_{\ell}^{*}\}$, equals one if year *y* is after state *r*'s Medicaid implementation date ($1966 \leq t_{\ell}^{*} \leq 1970$). $AFDC_{ry\ell}$ is the observed AFDC rate for children of race *r* in state ℓ in

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[9] All eligibility measures refer to the expected number of *full* years of Medicaid eligibility. Treating 1967 as a full year of implementation and assuming that the monthly AFDC participation rate in Nevada of 1.3 percent is constant, then the expected number of *months* of eligibility in 1967 (which is 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 (or with a given amount) is higher.

[10] Using the state of residence and 5-year migration variables in the 1970-2000 Censuses, I can calculate $\sigma_{rsc}^{y}(\ell)$ every 5 years starting in 1965. I linearly interpolate between these observations to obtain birth-state-by-birth-year-by-calendar-year estimates of the state of residence distribution. Goodman-Bacon (2015) shows that migration is uncorrelated with $AFDC_{rs}^{*}$, which would tend to depress cumulative eligibility for children from high-AFDC states and inflate it for children from low-AFDC states. The coefficient from a regression of migration-adjusted cumulative eligibility on state and cohort dummies and unadjusted cumulative eligibility is much smaller than one (0.88, s.e. = 0.004), consistent with this kind of measurement error in the unadjusted variable.

year y.[11] Since 89 percent of Medicaid children qualified through AFDC, $1\{y \geq t_\ell^*\} \cdot AFDC_{ry\ell}$ is a close proxy for total child Medicaid eligibility.

Differences in contemporaneous AFDC rates led to wide variation in cumulative childhood Medicaid eligibility. Figure 2 plots m_{rsc} for cohorts defined by year of birth relative to Medicaid implementation in their *birth* state. After Medicaid, white children gained about a year of eligibility on average and nonwhite children gained about five years (solid line), but differences across states in cumulative Medicaid eligibility are about as large as the average increases. These differences in initial AFDC-based eligibility strongly predict contemporaneous Medicaid *participation*: a one percentage point difference in initial AFDC rates led to a 1.9 point increase (s.e. = 0.4) in annual child Medicaid utilization (Goodman-Bacon 2015, appendix figure 2B.4).

C. 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 2015). Medicaid coverage reduced infant deaths through improved hospital care with no discernible effect on health at birth, and reduced deaths among young children mainly from treatable infectious diseases.[12] Since mortality is an extreme outcome, these changes likely reflect underlying improvements health that may influence later-life health and productivity.

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[11] 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.

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

Medicaid's effects during this period were strongest and most precise for nonwhite children (although white point estimates were similar), but this may not be the case for long-run outcomes. First, even among children eligible for Medicaid, white 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). Second, white Medicaid eligible children 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).[13] Many more nonwhite than white children were categorically eligible for Medicaid at the time of implementation, but, consistent with more recent research on Medicaid (Currie and Gruber 1995, Currie and Thomas 1995), the resulting increase in medical care appears to have been larger for eligible white kids.

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

Cumulative Medicaid eligibility may be correlated with adult outcomes if, for example, migration sorts healthier children to higher- or lower-eligibility states, or if changes in AFDC rates reflect demographic, economic, or policy conditions (Decker and Selck 2012). To address these potential biases, I adapt the difference-in-differences strategy in Goodman-Bacon (2015) and compare changes in outcomes across cohorts born in states with different child AFDC rates in the year of Medicaid implementation. This initial categorical eligibility rate, denoted $AFDC^*_{rs}$, provides a fixed ranking of states by which to compare adult outcomes and avoids comparisons between earlier and later Medicaid states, which differed on a range of characteristics.

A. Evidence on the Validity of the Initial-AFDC Research Design

For comparisons based on initial AFDC rates to generate consistent estimates of Medicaid's effect, $AFDC^*_{rs}$ must predict cumulative eligibility (relevance) and be uncorrelated with other determinants of cross-cohort changes in adult outcomes (excludability). Figure 2 provides crude evidence on the strength of $AFDC^*_{rs}$ for predicting cumulative eligibility. The dashed lines plot average cumulative eligibility in states with above- and below-median values of $AFDC^*_{rs}$. The

[13] 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?", 61 percent of white categorically eligible heads reported that they could go "anywhere", compared to only 46 percent of nonwhite heads. White categorically eligible families were also twice as likely as nonwhite families to have switched providers in the previous two years (Loewenstein 1971). (There was no racial difference in recent provider switching among poor families in states that had not implemented Medicaid by 1968.)

average difference across the state groups for fully treated cohorts is 0.47 years for white children (s.e. = 0.03) and 1.12 years for nonwhite children (s.e. = 0.13).

These comparisons suggest that nonwhite effects may be smaller in the long- than in the short-run because initial eligibility is a weaker predict of cumulative eligibility than of contemporaneous eligibility. The difference in *initial* eligibility across high- and low-eligibility states is almost 6 times greater for nonwhite than white children (0.127 versus 0.022), but cumulative eligibility is less than 2.5 times as large. This is the result of convergence in nonwhite AFDC rates in the 1970s (see figure 8 in Goodman-Bacon [2015]), which erodes the year-by-year differences in nonwhite categorical eligibility.

As to the second assumption, initial AFDC rates are plausibly excludable instruments because cross-state variation in welfare participation was stable for most of the 20th century (Moehling 2007) and arose largely because of historical institutional factors unrelated to the circumstances facing cohorts first treated by Medicaid (Alston and Ferrie 1985, Bell 1965, Moehling 2007). I present evidence supporting the excludability assumption in table 1. The first three rows of each panel show the annual relationship between $AFDC_{rs}^{*}$ and pre-Medicaid state characteristics (cross-sectional balance), and the fourth row shows the *p*-value from a test that these cross-sectional relationships did not *change* across years (differential trends). If the difference-in-difference design is valid then there should be no statistically significant change in the relationship between $AFDC_{rs}^{*}$ and state characteristics.

One concern is that stratifying states by participation in a means-tested program may simply separate rich and poor states. Panel A refutes this, showing that white child poverty rates bear no relationship to initial eligibility in 1950, 1960 or 1970 (they are slightly negatively correlated for nonwhite children), and that this relationship remained stable over these years despite historic reductions in average poverty rates. The opposite concern is that states with high AFDC rates had higher overall spending, particularly in ways that help poor children. Panel B shows that initial AFDC rates are uncorrelated with both the level of and trend in log state government expenditures in 1932, 1942 and 1962 (Sylla, Legler, and Wallis 2006). Both sets of results are consistent with the argument that longer-run idiosyncratic institutions drive AFDC variation, rather than contemporaneous deprivation or generosity.[14] The last two panels rule out demographic selection

[14] Importantly, the stability of AFDC rates in the mid-20th century means that while cohorts from higher- or lower-AFDC states have more childhood exposure to AFDC itself, this did not change in the years leading up to Medicaid.

through infant mortality (panel C) or fertility rates (panel D). Initial AFDC rates are not correlated with either infant mortality or fertility rates in 1947, 1957 and 1965, and this relationship is stable over the 18 years before Medicaid was passed. This suggests that infant health or birth selection cannot explain cross-cohort changes in adult outcomes between higher- and lower-AFDC states.

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 main analysis uses native-born respondents ages 25 to 64 born no later than 1976 (Alaska, Hawaii and Arizona are dropped) in the 2000-2014 Censuses and American Community Surveys (Ruggles et al. 2010) collapsed to averages by race (r; white and nonwhite), birth state (s), birth cohort (c), and survey year (t). The median cell has 322 observations for whites and 44 for nonwhites. I also construct cumulative mortality rates from 1980-1999 using information on state of birth from the Multiple Cause of Death Files (United States Department of Health and Human Services 2009) and denominators calculated from the 1980 Census.

The estimating equation for outcome Y is:

$$Y_{rsct} = \boldsymbol{X}'_{\boldsymbol{rsct}}\boldsymbol{\beta} + AFDC_{rs}^{*}\left[\sum_{j=-(a+1)}^{-20} \pi_j 1\{c - t_s^{*} = j\} + \sum_{j=-18}^{b+1} \phi_j 1\{c - t_s^{*} = j\}\right] + \varepsilon_{rsct} \quad (2)$$

My preferred specification of $\boldsymbol{X}'_{\boldsymbol{rsct}}$ includes fixed effects for state, cohort, age and survey year; region-by-cohort fixed effects, to account for convergence in outcomes across U.S. regions unrelated to Medicaid (Chay, Guryan, and Mazumder 2009, Stephens and Yang 2013); and Medicaid-year-by-cohort fixed effects, to eliminate comparisons between earlier and later Medicaid states, which were on different trajectories both in terms of socioeconomic and health outcomes before Medicaid.[15] I also include the general fertility and infant mortality rates, per-capita income in each cohort's birth year, and the average number of hospital beds per-capita in each cohort's first 12 years. Identification in equation (2), therefore comes from comparisons

Therefore, comparing cohorts with more/fewer years in which AFDC confers Medicaid eligibility should net out differences in childhood AFDC receipt itself.

[15] 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).

across values of $AFDC_{rs}^*$ between respondents born in the same region in the same event-time.[16] I cluster standard errors by birth state (48 clusters) throughout the analysis.

The coefficients of interest, π_j and ϕ_j, trace out changes in the within-event-cohort relationship between $AFDC_{rs}^*$ and y_{sct} for event-cohorts born between a years prior to and b years after Medicaid implementation (relative to the omitted group, $j = -19$).[17] The π_j are falsification tests, since cohorts born more than 18 years before Medicaid had no childhood coverage and changes in their outcomes should not be related to initial Medicaid eligibility.[18]

The ϕ_j are intention-to-treat (ITT) effects that measure the relationship between an additional percentage point of initial eligibility and changes in outcomes for cohorts first exposed $-j$ years after birth—i.e. at age $max\{0, t_s^* - c\}$. 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 $max\{0, t_s^* - c\}$.

These results can be used to test several features of both the treatment effects and the design. First, the ϕ_j will be zero if Medicaid has no effect when received at age $max\{0, t_s^* - c\}$ and older. Precisely *when* coverage matters is a salient issue in the literature on long-run effects, and the event-study specification yields flexible estimates of Medicaid coverage across childhood. In general, though, the ϕ_j do not separately identify heterogeneous effects by age at exposure versus amount of exposure because cohorts who were young when Medicaid was passed also had more coverage. Conclusions about age versus amount of coverage require additional assumptions. Second, the pattern of the ϕ_j around $t_s^* = c$ (i.e. $j = 0$) provides an additional test of the design because all cohorts born after Medicaid have the same age (0) and amount of exposure (19 years). Because the "experiment" is the same, the ϕ_j should not be appreciably different for $j \geq 0$.

C. *Instrumental Variables Specification*

To express the effects in terms of years of childhood eligibility, I estimate instrumental variables models that use the "dose" described above as an instrument for actual cumulative

[16] In some specifications the event-study results from (2) show a relatively smooth trend in the effects for older cohorts and a statistically distinguishable trend *break* in the coefficients. In these cases, I also show estimates from a procedure that directly estimates the coefficient on a linear event-time trend interacted with $AFDC_{rs}^*$before the break point, subtracts the estimated trend from the full data (ie. extrapolating it to the "post" period), and re-estimates equation (2) on the adjusted data.

[17] Cohorts born outside the event window [-a, b] are grouped into (unreported) terms for $-(a + 1)$ and $(b + 1)$.

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

eligibility m_{rsc}, defined in equation (1). The instrument, z_{rsc}, eliminates variation from annual changes in AFDC rates and from cohorts' migration decisions, and generates a predicted cumulative eligibility based only on initial AFDC rates in each cohort's birth state:

$$z_{rsc} = \sum_{y=c}^{c+18} 1\{y \geq t_s^*\} \cdot AFDC_{rs}^* = AFDC_{rs}^* \cdot (19 - max\{0, t_s^* - c\}) \quad (3)$$

IV estimates that use z_{rsc} as instruments for m_{rsc} can be interpreted as the average ITT effect of an additional year of cohort-level cumulative eligibility across ages of exposure. Splitting the eligibility variables by sub-periods of childhood allows a test of whether the average effect per year of eligibility differs by age at exposure.

IV. Intention-to-Treat Effects of Medicaid on Adult Health

Figure 3 plots first-stage event-study estimates of equation (2) that measure the relationship between $AFDC_{rs}^*$ and cross-cohort changes in migration-adjusted cumulative Medicaid eligibility, m_{rsc}.[19] The coefficients for event-times -23 through -20 are small by construction: there is no relationship between $AFDC_{rs}^*$ and cumulative eligibility for cohorts with *no* childhood Medicaid exposure. The positive and increasing coefficients for event times -18 through 0 show that even after incorporating childhood migration, cohorts born in states with higher initial eligibility accumulate more childhood eligibility for each year they are exposed to any Medicaid program. The slope of this relationship is twice as steep for whites as for nonwhites (0.007 versus 0.003), and column 1 of table 2 presents first-stage coefficients on z_{rsc} that confirm this difference. On average, initial eligibility strongly predicts white cumulative eligibility (0.72, s.e. = 0.15), but is only weakly related to nonwhite cumulative eligibility (0.21, s.e. = 0.17). 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.

A. Cumulative Mortality, 1980-2000

I first examine cohort-level mortality—an extreme, but objective health outcome—in the years between their childhood Medicaid exposure and when I observe them in the Census/ACS. Using

[19] Because childhood eligibility, m_{rsc}, does not change across survey years in adulthood, these models use a state-by-cohort dataset (that is, with no t subscript).

information on state of *birth* (publicly available from 1979-2004) I construct 20-year mortality rates conditional on living to 1980 by dividing the count of deaths between 1980 and 1999 for a given cohort by population estimates derived from the 1980 Census. This measure has several attractive features. First, summing deaths over 20 years increases power relative to shorter time periods. Second, the 20-year mortality rate closely approximates the share of a cohort that survived until the Census/ACS data, and so can be used to evaluate the importance of selective survival. Third, Hispanic respondents reported their race very differently over time as the number of race categories grew, but this may not have been true on death certificates, which have fewer race codes and are usually filled out by funeral directors. Matching "white" and "nonwhite" death totals to similarly coarse denominators available in 1980 avoids the misclassification error introduced in later Censuses (Arias et al. 2008).[20]

Event-study estimates of Medicaid's effect on the log of 20-year mortality rates are plotted in panel A of figure 4, and strongly suggest that childhood Medicaid coverage reduces adult mortality.[21] The point estimates for white mortality (multiplied by 100 so that the y-axis is measured in log points) are small for cohorts with no childhood Medicaid exposure—a key test of the validity of the design. They are also small for cohorts who were only eligible in their teenage years, suggesting that later childhood eligibility has no effect on subsequent mortality. The coefficient on the interaction between $AFDC_{rs}^{*}$ and a linear event-time variable between -30 and -11 is small and insignificant (0.02, s.e. = 0.05), implying that mortality would only differ by about 0.4 percent between cohorts born 20 years apart in states with a one percentage point difference in initial AFDC rates (1*0.02*20). For cohorts with increasing exposure under age 10, the event-study estimates are negative and growing (the linear trend break is -0.20, s.e. = 0.14), and flatten out for cohorts with full childhood exposure (0.09, s.e. = 0.11). Both features match the shape of the first stage and support the AFDC-based research design.

The nonwhite trend-break estimates are very similar. Results from the same specification used for whites shows that these breaks are relative to an upward trend in mortality for the oldest cohorts (0.07, s.e. = 0.03). To address this, the nonwhite results plotted in figure 4 come a two-step

[20] The effects for black and non-black mortality rates (which have less misclassification) are plotted in appendix figure 3.7, and are nearly identical.

[21] The dataset includes cohorts born as early as 1936—aged 64 in 2000 and born 30 years before the earliest Medicaid implementation date—and as late as 1975—aged 25 in 2000 and born 5 years after the latest Medicaid implementation year. This fixes the event-times at which I observe cohorts from all states: [-30,5].

procedure in which I estimate the pre-trend on data through event-time -9 (the best-fitting trend break point), remove it from the full dataset, and estimate equation (2) on the adjusted data.[22]

The cohorts affected by Medicaid's introduction also endured one of the worst public health crises in the 20th century, the AIDS epidemic. During the 1980s and 1990s AIDS was the number one cause of death for adults aged 25-44, particularly nonwhite men. The approval of antiretroviral drugs (ARV) in 1996 dramatically reduced AIDS mortality, partly due to contemporaneous Medicaid drug coverage (Duggan and Evans 2008). The incidence of AIDS mortality across cohorts and states, unfortunately, means that it is correlated to some extent with childhood Medicaid exposure. It was highest for those born in the 1950s, fell strongly for those born in the 1960s (who survived to benefit from ARVs), and was especially concentrated in New York and New Jersey, two relatively high-AFDC states. Panel B shows that Medicaid has similar proportional effects on non-AIDS-related mortality.[23] I use cause-elimination life table methods (Manton and Stallard 1984) to construct a cumulative mortality rate that, under the assumption of independent risks, reflects the force of non-AIDS mortality only and is unaffected by AIDS-related changes in the population at risk of dying from any cause.[24]

To summarize these results, table 3 presents IV estimates that use predicted cumulative eligibility, z_{rsc} as an instrument for actual cumulative eligibility, m_{rsc}. Motivated by the event-study results, I measure eligibility separately for ages 0-10 and 11-18. (First-stage estimates for age-specific eligibility measures are in table 2, and age-group-specific first-stage event-study

[22] A common strategy to deal with trends in a difference-in-differences design is to include unit-specific linear time trends. The event-study figures clearly show time-varying treatment effects, however, in which case unit-specific trends cannot distinguish between treatment effects and pre-existing trends (Lee and Solon 2011). Note that this two-step procedure has no effect on the estimated trend breaks, which are identical in the unadjusted data. It only alters the orientation of the event-study coefficients and the resulting IV estimates.

[23] Empirically, New York and New Jersey are the primary reasons why AIDS affects the estimates. Dropping them yields overall mortality results similar to those in panel B and also yields null results on AIDS-related deaths.

[24] 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, $POP_{cs,1980} - \sum_{j=1980}^{y}\left(d_{cs,j}^{AIDS} + d_{cs,j}^{OTH}\right)$, in the denominator. Assuming that the period mortality rates would be the same in the absence of other causes (ie. independent risks), then the cause-elimination mortality rate from cause k can be calculated as $1 - \prod_{j=1980}^{1999}\left(1 - mr_{cs,j}^{k}\right)$. These rates are weakly larger than naïve cause-specific rates calculated as the sum of cause-specific deaths divided by 1980 population, because they implicitly apply the observed period-specific mortality rates to the population that does not die from the specified cause rather than the population that did not die at all.

figures are in appendix figure 2.1.)[25] The effects of early childhood eligibility are precise and similar by race for both overall mortality (white: -23.9 log points; nonwhite: -30.7 log points) and non-AIDS related mortality (white: -15.5 log points; nonwhite: -19.6 log points).

What does this imply for the proportional reduction in mortality among the treated subset of white adults? The proportional treatment effect on the treated is smaller than the ITT effect in table 3 both because it is driven by a negatively selected group—adults with childhood Medicaid eligibility—and because baseline adult mortality rates—the implicit denominator—*include* Medicaid's treatment effect. Appendix 8 shows how to use post-treatment data on mortality differences by poverty status (available in the National Longitudinal Mortality Study) and on the amount of childhood AFDC receipt for treated children to infer the proportional treatment effect on the treated that is consistent with the ITT in table 3.[26] Making these adjustments suggests that a year of childhood coverage reduces cumulative non-AIDS related mortality rates by 8 percent among treated white adults and 9 percent among treated nonwhite adults.

These reductions are larger than the other estimates of childhood Medicaid coverage on adult mortality. Wherry and Meyer (2013) find an ITT effect per year of eligibility of about -0.375 internal-cause deaths per 10,000 for black teens 15-18 which translates to a 6 percent decline in annual teenage internal-cause mortality per year of eligibility among the treated. The comparable ATET in Brown, Kowalski, and Lurie (2014) is about a 1 percent reduction in mortality between age 18 and 27. (See appendix 8 for details on the calculations.) These differences are to be expected given the nature of the 1980s expansions and the samples under study. First, both recent studies focus on eligibility at later ages when Medicaid has smaller effects (as in figure 4). Second, the treated subgroups, particularly in Brown, Kowalski, and Lurie's (2014) setting which includes eligibility for non-poor children, have higher income than categorically eligible children and may

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[25] The nonwhite age-specific first-stage estimates are stronger than the overall first stage, but still reflect the previously mentioned changes in nonwhite AFDC rates. For instance, predicted eligibility under age 10 (based only on birth states) is *negatively* associated with actual eligibility between ages 11-18. This is because born in the 1960s have high early eligibility, but their teenage years correspond to the period of nonwhite AFDC convergence. Therefore, being born in a *low* AFDC state in the 1960s means that nonwhites have low early childhood eligibility, but relatively higher later childhood eligibility, which induces the negative correlation.

[26] Two approaches give similar answers. The NLMS includes 11-year mortality rates for respondents from a range of CPS and Census samples from the 1980s and 1990s. Comparing 11-year mortality for white, native-born poor and non-poor respondents between the ages of 5 and 45 (the ages with early Medicaid exposure between 1980 and 1999) gives a ratio of 1.55 (2.22 among the poor versus 1.42 among the non-poor). The NLMS also records health insurance status for those 14 and older in about half of the data. The ratio of 11-year mortality rates for white, native-born Medicaid recipients and non-recipients aged 14-18 is 1.57 (1.38 percent among Medicaid children versus 0.87 percent overall).

benefit less. Third, the 1980s expansions led to higher levels of childhood eligibility than under implementation. If the adult mortality effects are concave, then the average treatment effects per year of coverage will be smaller for larger expansions.

The rest of table 3 uses information by cause of death to test the research design further and to examine the channels through which Medicaid may reduce aggregate mortality. Columns 3 through 7 show that Medicaid's effects are strongest among the most plausibly affected conditions—(non-AIDS) internal causes—but that it worked through a range of conditions within this group. The internal cause estimates (column 3) are noticeably larger than both the non-AIDS- and external-cause estimates (column 7), which are very small for whites, although not for nonwhites. The leading internal causes of death among adults other than AIDS were cardiovascular disease and cancer. Consistent with higher and earlier incidence, cardiovascular disease plays a more important role for nonwhite than white mortality (point estimates are -27.3 versus -15.5). The cancer effects are similar for both groups. Strikingly, column 6 shows large and precise reductions in suicide (-39.7, s.e. = 8.4 for whites, -39.3, s.e. =15.9 for nonwhites), although suicides account for a small share of the full effect. This result is consistent both with reductions in the burden of chronic illness as discussed in 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), both of which may reduce suicides.

Using the proportional effects on non-AIDS mortality to construct counterfactual mortality rates suggests that about 346,000 lives were saved between 1980 and 1999—54,000 among whites and 292,000 among nonwhites. Even assuming a relatively low value of statistical life of $840,000 (the lower end of the confidence in interval of Ashenfelter and Greenstone's (2004) estimates [table 3, converted to 2012 dollars]), this suggests that Medicaid's longer-run mortality reductions are worth at least *$290 billion*.

B. Self-Reported Disability

Mortality has important limits as a measure of Medicaid's long-run effects. Its implications for average well-being and public financial returns are ambiguous since those induced to survive may be in worse health and incur higher public costs than treated adults who were not on the margin of surviving. Moreover, 20-year mortality rates are low compared to the population treated by Medicaid, so even without offsetting composition effects, mortality misses Medicaid's potential to improve outcomes for the bulk of its target population. Fortunately, the Census/ACS data contain a series of disability variables that are less rare than mortality, commonly used to measure health (Bound et al. 2003), and extremely relevant to labor market outcomes.

Figure 5 plots event-study estimates for the most common disability: difficulties with activities like walking, climbing stairs, reaching, lifting or carrying (ambulatory difficulty). The results show that childhood Medicaid eligibility reduces adult disability.[27] The point estimates (multiplied by 100 so that the y-axis measures fractions of a percentage point) provide even stronger support the research design. They are small for cohorts with no childhood Medicaid exposure and, just like the mortality results, the estimates are also small for cohorts first exposed between after age 10 for whites (the linear trend from -23 to -11 is -0.006, s.e. = 0.007) and after age 4 for nonwhites (pre-trend = -0.002, s.e. = 0.003).[28] There are clear, statistically significant, negative trend breaks though after these ages (white: -0.022, s.e. = 0.009; nonwhite: -0.014, s.e. = 0.003). Crucially, the trends reverse for cohorts with full childhood exposure (ie. born after Medicaid). A test that these coefficients are equal, a direct prediction of the design, yields p-values of 0.99 for whites and 0.17 for nonwhites.

Table 4 presents IV estimates for ambulatory difficulty across specifications, using the trend breaks in figure 5 to break up eligibility ages at age 10 for whites and age 4 for nonwhites. The model in column 1 includes only event-time dummies and $AFDC^{*}_{rs}$, and the effects of early childhood eligibility are negative and imprecise. My preferred specification (column 2) shows that each full year of early life cumulative eligibility reduces white ambulatory difficulty rates by 3.87 percentage points (s.e. = 1.17) and nonwhite rates by 2.96 percentage points (s.e. -1.81). Column 3 shows that the white result is not sensitive to population weighting (Solon, Haider, and Wooldridge 2015). The magnitudes fall only slightly with the inclusion of state-specific linear cohort trends (column 4), but the flat trends and phase-in of the effects in figure 5 suggest that trends are not an appropriate control (Lee and Solon 2011).

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[27] Because of changes in the text of the disability questions, these results use data from 2000-2007 only. This determines the event-time window because 64 year olds in 2007 were born 23 years before the earliest Medicaid year (1966) and 25 year olds in 2000 were born 5 years after the latest Medicaid year in the sample (1970). The results are not sensitive to including all survey years or to collapsing across survey years to the state-by-cohort level.

[28] The trend break estimates listed in the figure come from fitting linear trends with three sections. The pre-trend goes through zero at time -19 (like the event-study estimates), the phase-in trend begins somewhere between time -19 and -1, and the post-trend begins at zero, when all cohorts have full childhood exposure. I present estimates that maximize the F-statistic on the trend terms, thus remaining agnostic about the age when exposure begins to have longer-run effects. F-statistics are plotted in appendix figure 2.5. As in figure 3, the nonwhite results are from a two-step procedure that adjusts the data by removing a positive pre-trend in disability rates between event-cohorts -23 and -4. This does not affect the estimated trend breaks, just the orientation of the coefficients.

The final three columns expand the dataset by state of residence to address several ways in which characteristics of respondent's *current* state could confound comparisons based on state of birth. Column 5 tests whether self-reported disability is a response to labor demand conditions (Charles and Decicca 2008) by including interactions between cohort dummies and annual state-of-residence unemployment rates. Column 6 reports on a particularly stringent test: including cohort-by-state-of-residence fixed effects. This controls non-parametrically for factors that vary across cohorts and states of *residence* (strongly correlated with birth state) such as age-varying effects of state policies, trends in chronic pain and opioid abuse (Case and Deaton 2015), AIDS incidence, or adult migration. Column 7 relies only on cross-cohort comparisons for adults who made the same migration decision by including state-of-birth-by-state-of-residence fixed effects. None of these specifications alter the conclusion that early Medicaid exposure reduces disability.

For whites, the effects of early Medicaid eligibility extend to all disability measures. Panel A of table 5 shows negative and precise treatment effects for hearing or vision problems; difficulty going outside the home (mobility difficulty); getting around inside the home (self-care difficulty); learning, remembering or concentrating (cognitive difficulty); and working at a job or business (work limitation). Perhaps as a consequence of improvements in the more severe limitations, appendix figure 3.4 also shows a suggestive negative effect of early Medicaid coverage on the probability of living in group quarters (-0.38, s.e. = 0.22). The effects across types of disabilities are consistent with broad improvements in health due to Medicaid, but also underscore the difficulty in uncovering Medicaid's specific physiological channels.

How big are these effects relative to counterfactual disability rates among treated children? Unlike mortality, disability rates for adults with child welfare receipt are observable in the PSID (University of Michigan Survey Research Center 2016). Forty-one percent of white adults who received AFDC income as children in 1968 reported a work limitation in 2001, compared to 15 percent on average. This implies that the actual rate of ambulatory difficulty among the treated subset is (5.7*41/15) 15.6 percent (lower than the rate among poor adults in the Census, 17.7 percent).[29] This number *includes* Medicaid's treatment effect, however. Analyses of PSID data (Smith and Yeung 1998) and administrative data on welfare spells (Berger and Black 1998) suggest that the average white child with any childhood AFDC spent about 2 full years on the

[29] This calculation uses the average ambulatory difficulty rate not among all respondents (8.61, reported in table 3), but among those born from 1955 to 1975, which is 5.7 percent.

program by age 10. Therefore, the results in table 2 suggest that their adult disability rates are lower by (2*3.87) 7.7 percentage points. Adding this effect implies a counterfactual rate of ambulatory difficulty among the treated of 23 percent (5.7*41/15 + 3.87*2), and a proportional reduction in ambulatory difficulty per year of Medicaid eligibility among treated children of 16.5 percent. Similar calculations for nonwhite adults imply a reduction in adult disability 13 percent per year of childhood coverage among the treated.

These results suggest large improvements in quality of life in response to Medicaid. Activity limitations are fundamental to many concepts of well-being itself (Sen 1993), and are closely related to self-reported happiness and satisfaction.[30] Furthermore, the disability and mortality results reinforce each other, since deaths from potentially disabling conditions fall and because reductions in disability may feedback to a reduction in suicide (Giannini et al. 2010).

C. *Selective Survival*

The preceding evidence suggests that Medicaid's effect on disability could be biased by changes in the composition of survivors. If Medicaid saved the lives of those who ultimately became disabled, then the estimates in tables 4 and 5 will understate Medicaid's effect disability. Using direct estimates on mortality, however, allows me to bound the parameter that would be obtained in the absence of Medicaid's mortality effects. The observed disability rate (dr_{rsct}) is the average of the rates among those who were saved by Medicaid (dr^1_{rsct}) and those who would always have survived (dr^0_{rsct}):

$$dr_{rsct} = (1 - p_{rsct}) \cdot dr^0_{rsct} + p_{rsct} \cdot dr^1_{rsct} \qquad (4)$$

p_{rsct} is the share of each cohort that was induced by Medicaid to survive to year t. I use the contemporaneous infant and child mortality estimates in Goodman-Bacon (2015) and the cumulative adult mortality estimates from table 3 to construct true and counterfactual probabilities of surviving to 2000 and calculate p_{rsct} as their difference. Then, since dr^1_{rsct}, lies between 0 and 1, I bound the treatment effect on dr^0_{rsct} using different assumptions about dr^1_{rsct}, similar to Bharadwaj, Løken, and Neilson (2013, table 4).

[30] The 2001 National Health Interview survey (Minnesota Population Center and State Health Access Data Assistance Center 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.

Figure 7 plots effects for nonwhite ambulatory difficulty among always-survivors across the full range of possible counterfactual disability rates among those induced to survive (right axis). Unlike in table 4, which shows relatively similar effects by race for the ages when coverage matters most, the nonwhite estimates refer to eligibility under age 10 to facilitate comparisons with the white effects. The figure also includes a histogram of observed nonwhite disability rates across state/cohort cells to gauge the plausibility of the counterfactual assumption (left axis).[31] The average effect of childhood eligibility under age 10 becomes significant at counterfactual disability rates around 0.35—twice the observed level among all poor adults—and equals the white effect only under extreme selection. Compositional changes appear to affect the magnitude of the nonwhite disability effects, but not so much that they fully explain the differences.

Selective survival has a very small effect on white disability estimates because mortality in later childhood and early adulthood is not an important determinant of population dynamics. Medicaid's mortality effects lead to differences in survival of at most 0.4 percent—small relative to observed rates of ambulatory difficulty for treated cohorts (5.7 percent).

D. Infant versus Childhood Exposure

The evidence on disability and mortality focused on eligibility under age 10, but further disaggregation shows that Medicaid eligibility in infancy and in utero has even larger impacts on some health measures (again, for whites). Appendix table 3.3 presents IV estimates that use eligibility measures for ages 0-1 ("infant exposure"), 2-10 and 11-18. Infant exposure has a noticeably larger effect than early child exposure for ambulatory difficulty (-6.50 versus -3.08), although only the early childhood eligibility estimate is statistically significant (s.e. = 0.84) and I cannot reject the equality of the two effects (*p*-value = 0.33). For the mobility difficulty, self-care difficulty, and cognitive difficulty, *only* infant coverage significantly improves adult outcomes and in each case the effect is distinguishable from early childhood eligibility. (This is true to some extent for work limitations as well.) Hearing and vision difficulties, on the other hand, do not respond to infant coverage but do fall by 1.76 percentage points (s.e. = 0.38) for each year of early childhood coverage.[32]

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[31] Note that those induced to survive are a selected subset—on the mortality margin—of an already selected subset—those with childhood Medicaid eligibility. No data exist that allow a principled guess about their adult disability rates.

[32] Appendix table 3.6 examines effects in infancy for the cumulative mortality rates. There is no significant difference between infant and early childhood exposure for overall mortality rates (*p*-value= 0.87), and the coefficients are

This evidence suggests that both infant and childhood Medicaid eligibility play a role in influencing adult health, but that infant coverage has the biggest effects on serious disabilities and related causes of death. As noted above, this is not likely to come from Medicaid-induced improvements in health *at birth*, since there is no evidence of contemporaneous effects on measures such as birth weight and the sex ratio (Goodman-Bacon 2015). Most likely these results reflect the combined effect of improved care at and immediately following birth and the primary care available very early in life. These factors matter at slightly later stages of childhood as well, but not nearly as much as they do within the first year of life.

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 labor supply and household resources as well as public costs and revenues.

A. Labor Supply and Transfer Program Participation

Both event-study and IV estimates show that the positive health effects of childhood Medicaid eligibility translate into increased extensive margin labor supply and reduced program participation for whites. The (blue) series with closed triangles in figure 7 plots event-study estimates of Medicaid's effect on annual employment. The shape of these effects is strongly consistent with an effect of early childhood Medicaid eligibility. The trends for cohorts exposed to Medicaid after age 10 are flat (-0.003, s.e. = 0.01), and there is a clear trend break for the same cohorts that experienced health improvements (0.051, s.e. = 0.014) which is largely eliminated for groups with full childhood exposure (-0.038, s.e. = 0.014). IV estimates (table 6) show that each year of childhood Medicaid eligibility reduces the probability of being out the labor force by 6.78 percentage points (s.e. = 1.55) and increases employment (current: 6.02, s.e.=1.25; annual: 6.50, s.e. = 1.45), most of which is time/full-year (4.91, s.e. = 0.72). The 2001 PSID shows relatively small differences in employment between white adults with and without childhood AFDC receipt, which suggests that the implied 13-point increase in employment among treated children

relatively similar (-27 for infant coverage [s.e. = 25.5], -22.9 for early childhood coverage [s.e. = 8.6]). There are much larger, although generally imprecise effects of infant coverage for cardiovascular, other internal, and suicide mortality, which is consistent with the results for more serious disabilities, as these conditions are closely related to common causes of chronic pain and activity limitation. Cancer mortality is only significantly related to early childhood and not infant coverage.

represents an 18 percent increase over their counterfactual employment rate of 72 percent (based on the employment rate among whites born from 1955-1975, 85 percent).[33]

Medicaid's effects on disability benefit receipt (SSDI/SSI; the red series with open squares) are almost the mirror image of the labor supply effects, and track the disability results closely. The pre-trend is small and insignificant (-0.012, s.e. 0.011), there is a negative trend break for cohorts exposed at age 10 or younger (-0.023, s.e. = 0.013) and a positive one for cohorts with full exposure (0.039, s.e. = 0.006). The corresponding IV estimate in table 7 shows a reduction in disability transfer participation of -4.54 percentage points (s.e. = 1.14). Other welfare receipt (mostly Temporary Assistance for Needy Families, TANF) actually rises slightly (0.77, s.e. = 0.14). Disability benefits are higher than TANF benefits meaning that people who qualify for both tend to prefer SSI/SSDI. Therefore, health improvements that disqualify households from disability benefits may simply lead some of them to take up TANF.[34]

The connection between cash and in-kind benefits means that Medicaid also has important intertemporal effects within the public insurance system. Column 4 of table 7 shows that cohorts with higher Medicaid eligibility in early childhood grow up to use public insurance *less* often as adults (-4.16, s.e. = 1.06).[35] Because these adults work more, the reduction in public coverage is largely offset by private insurance, but because many appear not to work full time, there is a small but insignificant reduction in total insurance coverage (0.95, s.e. = 0.92).

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

[33] Panel B shows similarly signed effects for nonwhite labor supply, but the magnitudes are much smaller and none are statistically significant. The rest of this section presents and discusses results for white adults.

[34] Appendix 2 provides additional evidence on the validity of the design using employment and public assistance data from 1970, 1980 and 1990. In these survey years, I can observe much older cohorts during prime working years and estimate the relationship between $AFDC^*_{rs}$ and these outcomes for the same ages that I use in figure 5, but in calendar years when no cohorts are treated. I do this in two related ways. Figure 4.5 uses the 1980 and 1990 Census to extend figure 5'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.The main results would be biased not only by cohort patterns that vary across states, but also by *age* patterns that vary across states. 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.

[35] This effect comes both from Medicaid, for which almost all SSI recipients are categorically eligible, and Medicare, which SSDI 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.

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 table 8, on the other hand, show that *holding programmatic incentives constant* (through the cross-state comparisons), improvements in health greatly reduce disability benefit receipt and increase labor supply.[36] Multiplying the effect per year of eligibility (-4.54), times 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 940,688 fewer SSI/SSDI recipients because of Medicaid implementation—about 12 percent of the average number of white, non-elderly recipients between 2000 and 2014.

B. Sources of Income

Increases in labor supply and reductions in transfer program receipt offset each other in terms of income. Figure 8 plots a series of IV coefficients for eligibility between age 0 and 10 for dependent variables defined as 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 analysis provides two important pieces of information about Medicaid's effect. First, the earnings effect is concentrated in the lower part of the distribution. Because income mobility for these cohorts is so low (Chetty et al. 2014, Lee and Solon 2009), this lends further support to the claim that these effects are due to Medicaid's treatment of poor children. Second, the figure shows that increased earnings offset reduced transfer income. 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

[36] That the effect of early Medicaid coverage on employment (6.50) is larger than its effect on disability assistance (-4.54) 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.

transfer income in 2014) and $55,000 (close to the mean among employed whites in 2014) remain positive and marginally significant, suggesting that some respondents hold middle-class jobs.

Table 9 quantifies Medicaid's effect on average income by source.[37] Columns 1 and 2 show that average earned income increases, but the full-sample estimate is quite noisy because of high incomes. Trimming earnings above $200,000 (the 98th percentile of earned income and a level at which figure 7 shows Medicaid has no impact) leads to the expected significant increase in earnings: $2,690 (s.e. = 1,208). Transfer income falls by 593 dollars (s.e. = 163), and total income is higher ($1,197), but not by a significant amount (s.e. = 2,386). Because Medicaid largely affects the *composition* of income, it does not significantly reduce adult poverty rates (column 5), although comparing the 0-10 to the 11-18 estimate shows a reduction of 1.5 points (*p*-value = 0.17).

C. Human Capital

Long-run research based on the 1980s expansions finds increases in educational attainment (Brown, Kowalski, and Lurie 2014, Cohodes et al. 2014), but table 10 shows no evidence that Medicaid implementation affected high school completion, or college attendance and graduation probabilities. Multiplying the upper end of these confidence intervals by average early childhood eligibility (0.37) rules out cohort-level effects larger than 1.3 percent for high school graduation and about 5 percent for college attendance and graduation.

While the null result on education differs from related work, human capital is not the most likely mechanism for the extensive margin labor supply effects documented above. Long-run health studies that link earnings to improved education tend to work through higher wages rather than extensive margin labor supply. Bhalotra and Venkataramani (2015), for example, find that the introduction of sulfa drugs increased income and education among white men, but failed to affect employment rates (table 5, column3). Similarly black cohorts born in the South around the time of the 1964 Civil Rights Acts experienced large improvements in test scores, educational attainment and earnings, but little of this appears to derive from increases in employment (Chay, Guryan, and Mazumder 2009, 2014). Long-run evaluations of educational interventions come to similar conclusions. Chetty, Friedman, and Rockoff (2014), for example, find that extensive margin labor supply explains at *most* 23 percent of the long-run earnings effects of teacher quality.

[37] Because the effects in figure 7 are essentially differences in (one minus) the CDFs of non-negative random variables, their integral approximates Medicaid's effect on the mean of each income source. Summing each point times $2,000 (the bin width) yields estimates very close to those in table 9: $2,359 increase in earnings, $1,092 increase in total income and a $600 reduction in transfer income.

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). Thus, the reasons why exposure to Medicaid had no effect on educational attainment remain interesting, but the effects on health, employment, transfer participation and income are all much more consistent with improved health and reduced activity limitations than with higher human capital attainment.

VI. Discussion: Medicaid's Long-Run Return on Investment

The results above show large effects of early childhood Medicaid coverage on adult health, labor supply, and program participation, mainly for whites. With respect to total individual income these changes largely cancel out—Medicaid alters the composition but the not the amount of income. But the government gains from *both* increases in earnings and reductions in transfers. How big, then, are the aggregate changes in revenue and costs and how do they compare to the cost of childhood coverage for these cohorts?

In 2000 there were 56 million white adults in cohorts with any childhood Medicaid coverage, and they had an average of 0.37 years of eligibility. Each year of childhood eligibility increases average annual adult earnings by $2,690 (table 9, column 2), so at a 4.75 percent marginal tax rate, increased earnings bring in $2.6 billion per year (56 million*0.37*2,690*0.0475).[38]

Most of these newly employed adults appear to have left transfer programs, however, and the government saves *all* of the foregone benefits. Each year of early childhood eligibility reduces transfer income by $593 (table 9, column 3), which implies an annual savings of $12.5 billion (56 million*0.37*-593)—almost 5 times as much as the new tax revenue. Other research on Medicaid has neglected its impact on public assistance (although many long-run health studies do find such effects), and the large public return from reducing transfers shows that this is a crucial omission when estimating 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 4.16 percentage points (table 8, column 4), and the results for self-reported disability and disability transfer receipt

[38] I calculate the relevant marginal tax rate (MTR) as a weighted average of AGI-specific MTRs (SOI Table 3.5) with weights proportional to the effect sizes in figure 7. This is much smaller than average MTR using the actual distribution of AGI (8.4 percent) because the importance of extensive margin labor supply means that most of the gain in earnings happens at levels that are not taxed.

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 (ibid.). Using this as a benchmark for public insurance spending suggests that lower public medical costs save $2.9 billion per year (56 million*0.37*$3,326*-0.0416).

Relative to the cost of covering cohorts born between 1956 and 1975, the implied 18 billion in annual savings as a result of this coverage is 14 percent of the original cost of coverage. To calculate these costs, I first use data on total expenditures from 1966-1975 (Goodman-Bacon 2015) since all of this spending applies to the cohorts studied here. I use CPS data (Flood et al. 2015) to calculate the share of child Medicaid recipients born before 1976 for each calendar year between 1976 and 1993 (when the 1975 cohort was 18), and multiply this by total child Medicaid spending in each year (CMS 2013 table 13.10).[39] This suggests that the total cost of covering the cohorts that contributed to the effects documented above is $132 billion (in 2012 dollars).

Because the costs and long-run benefits are separated by several decades, discounting strongly affects the ultimate return calculations. Standard practice is to assume a 3 percent discount rate and express annual benefits (2000-2014) and annual costs (1966-1993) discounted to 2014. This yields an annual return of between 4.7 and 7.1 percent, and an average of 5.8 percent. If these effects closely approximate what similar policy changes would achieve today, then these returns reasonably reflect what the government can expect to earn back in a 3 percent interest rate environment. Just in the 15 years during which I account for benefits, this discounting assumption suggests that 87 percent of the original *discounted* cost has been saved.

This exercise is less well suited to examine the savings that the government has actually enjoyed as a consequence of Medicaid's introduction 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

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

method used by OMB 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 1.3 and 2.2 percent, and the government has saved 23 percent of the *true* cost of covering the original Medicaid cohorts.[40]

VII. CONCLUSION

This paper uses the original introduction of Medicaid combined with historical variation across states welfare-based Medicaid eligibility 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 2015). 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.

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 largely unchanged. This change in the composition of income, however, is a double benefit for the government. I calculate that the government saves between 1.5 and 6 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 transfer payments, which have not previously been studied in this context. A primary finding of this paper is that early life health programs can have large long-run returns if health improvements reduce reliance on cash and in-kind transfer programs.

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

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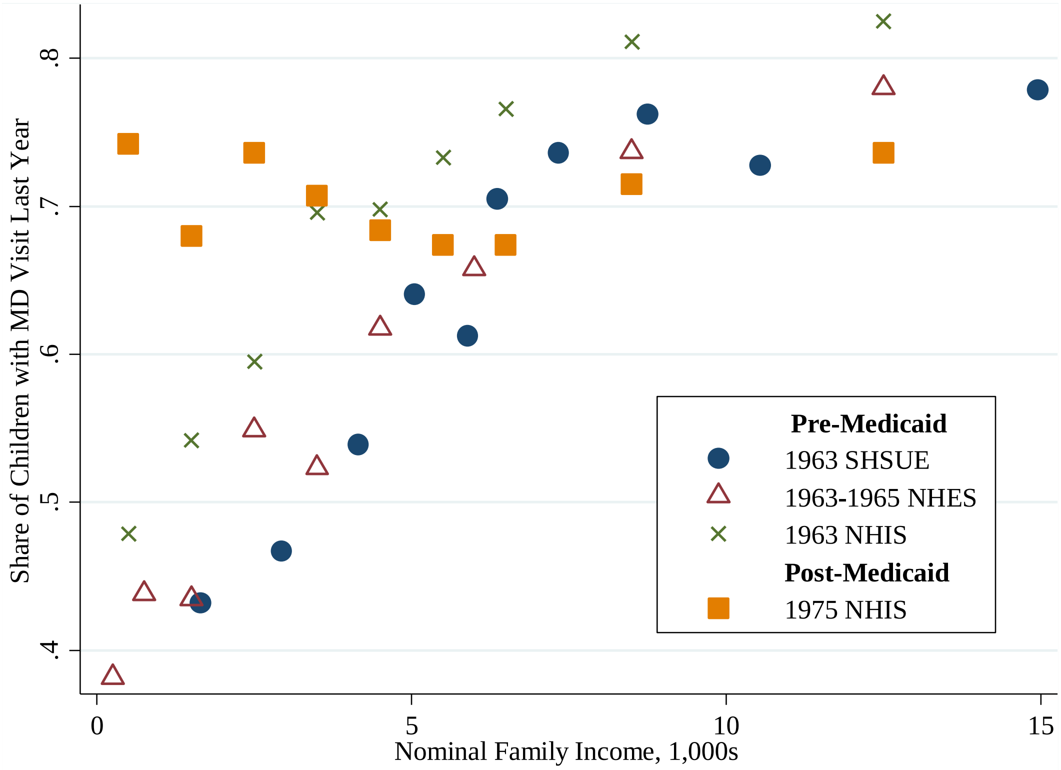
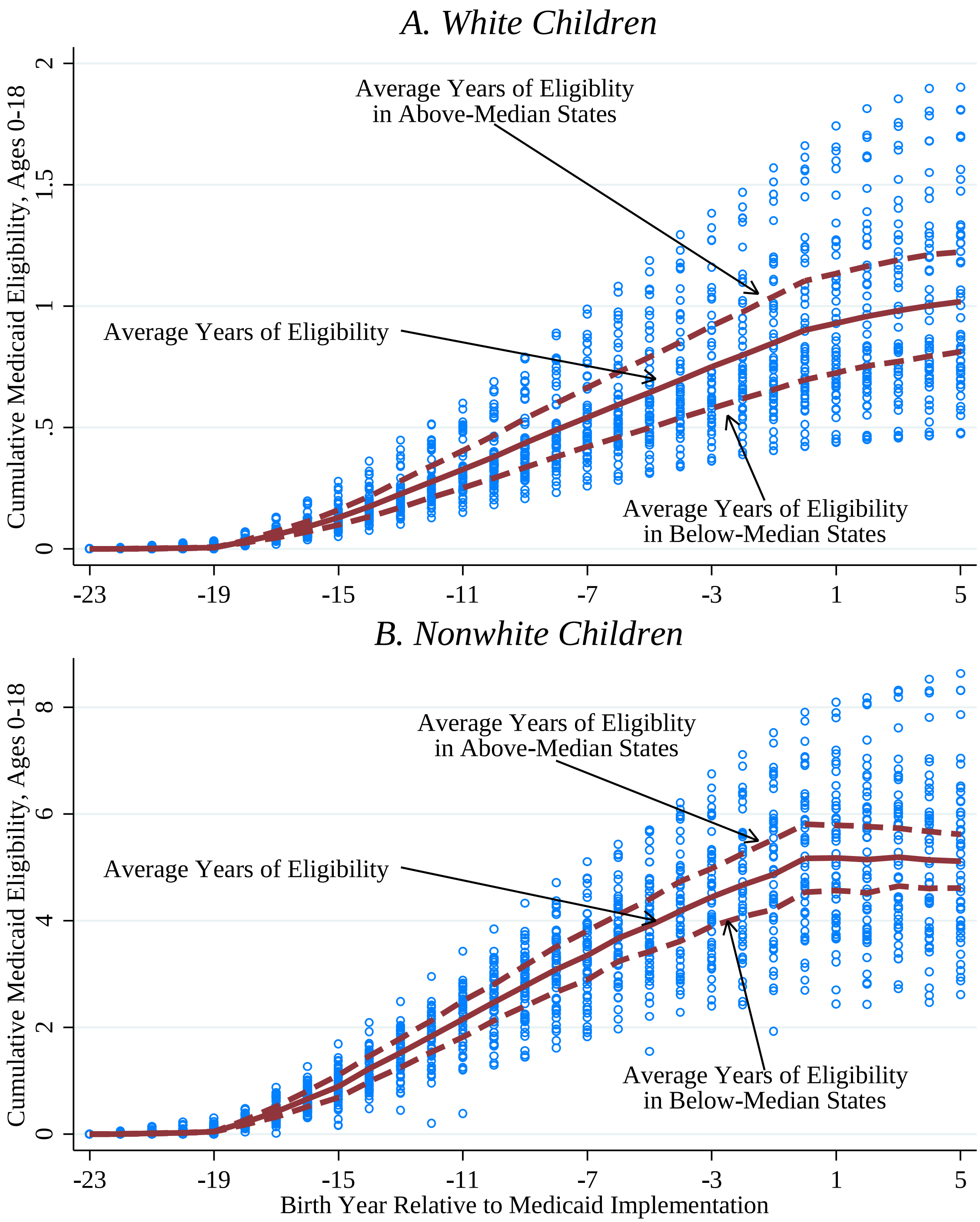
Figure 1. Family Income and the Probability that Children Saw a Doctor in the Last Year, Before and After Medicaid



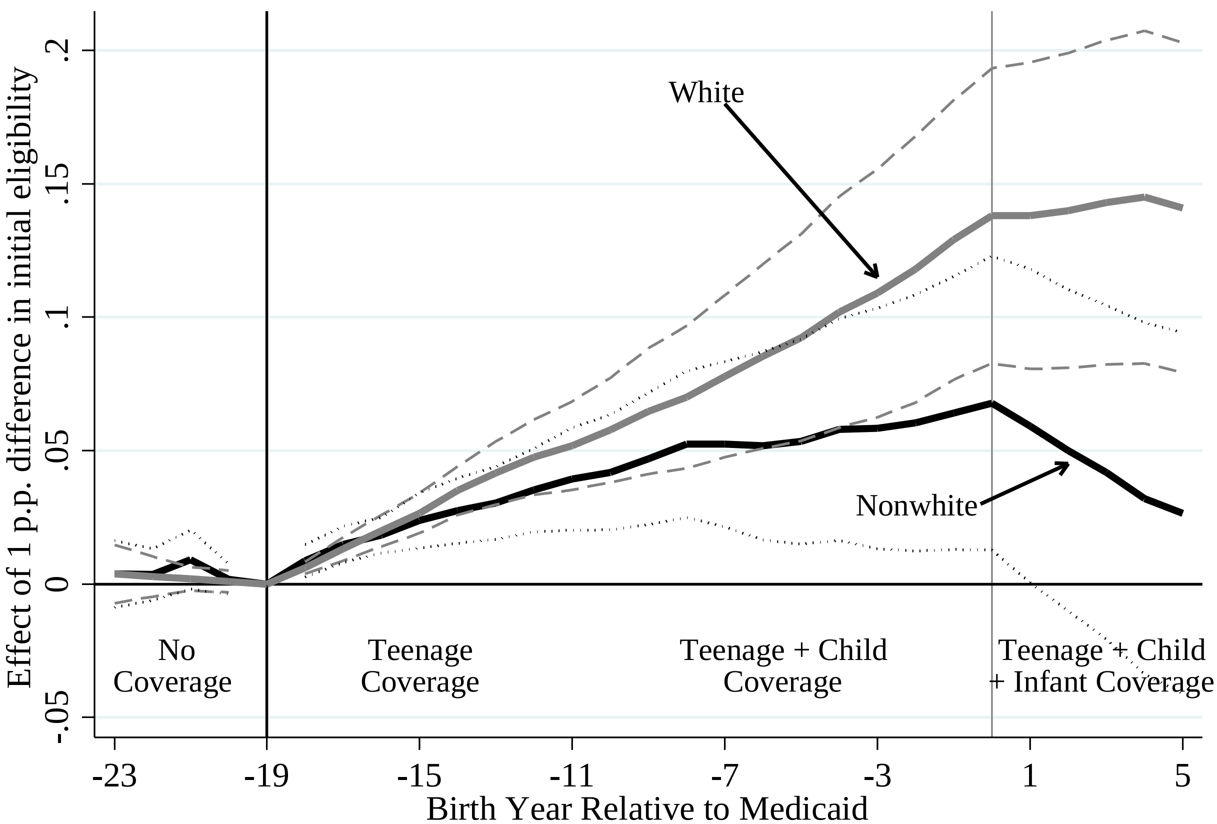
Notes: The figure plots the share of children who report having seen a doctor in the last year in four survey data sources: the 1963 Survey of Health Services Utilization and Expenditure (CHAS 1988), the 1963-1965 National Health Examination Survey (ICPSR); and the 1963 and 1975 National Health Interview Surveys. 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 same slopes on the observations under $10,000 of family income have the same pattern but are about twice as large (except for the 1975 slope: -0.004, s.e. = 0.004).

Figure 2. Cumulative Childhood Medicaid Eligibility by Race and Event Time



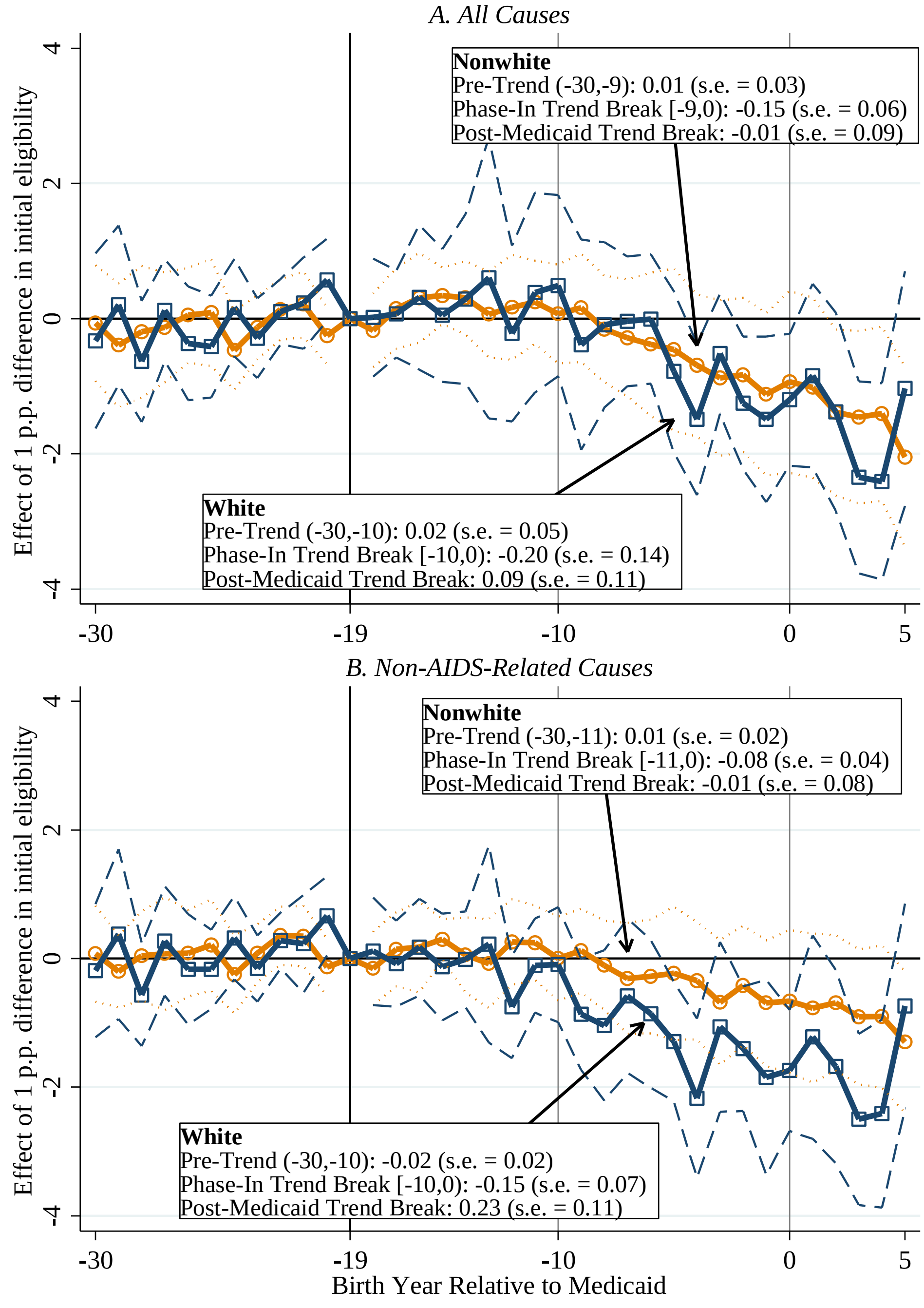
Notes: The figure plots cumulative childhood categorical Medicaid eligibility for birth cohorts from each state born from 23 years before Medicaid (1943-1947) and five years after (1971-1975). Note that the *y*-axes are different in the two panels. Cumulative eligibility is calculated according to equation (1) in the text. For each cohort, each year of childhood contributes a state-of-residence weighted mean of child AFDC rates interacted with a post-Medicaid dummy. These are summed over ages 0-18 to get an expected number of years of childhood eligibility. The solid line shows average eligibility across cohorts. The dashed lines show average eligibility in states with above- or below-median AFDC rates in the year of Medicaid implementation. For cohorts with full childhood coverage, eligibility differs across the two groups by 0.47 years for whites and by about 1.1 years for nonwhites.

Figure 3. First-Stage Relationship Between Cumulative and Initial Medicaid Eligibility Before and After Medicaid Implementation



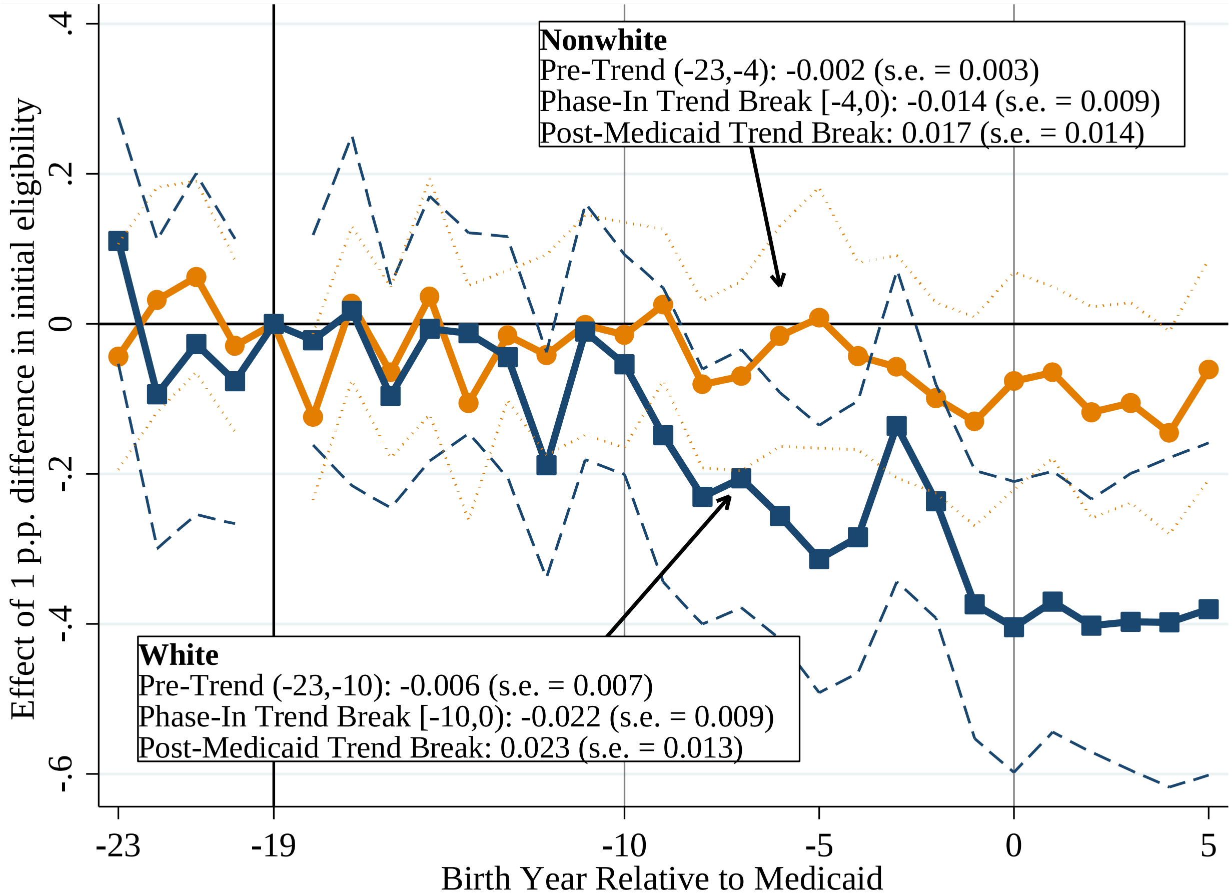
Notes: The dependent variable is each cohort's cumulative (migration adjusted) Medicaid eligibility for ages 0-18. The figure plots the estimated coefficients on interactions between initial AFDC rates and event-time dummies for each of 23 years before and 5 years after Medicaid. Time -19 is omitted. The dataset includes one observation per state/year cohort because cumulative eligibility is fixed over calendar time after age 18. The model includes birth-state, region-by-birth-year, and Medicaid-year-by-birth-year fixed effects, and state per-capita income and hospital beds averaged over childhood, and each cohort's the general fertility rate and infant mortality rate. The dashed lines are 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 is much larger for nonwhite than for white AFDC rates.

Figure 4. Reduced-Form Event-Study Estimates of Medicaid's Effect on log 20-year Mortality Rates (coefficients × 100), 1980-1999



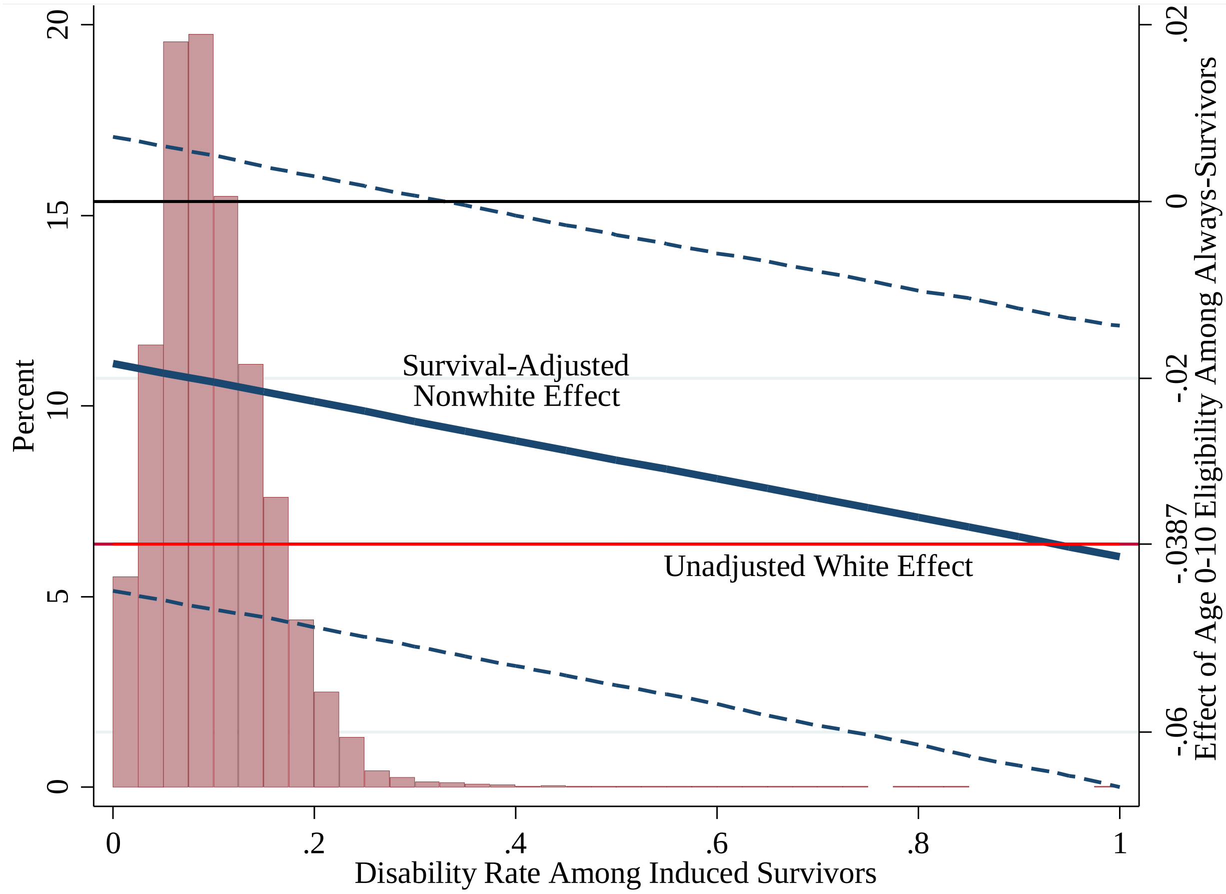
Notes: Outcome variables are the log of 20-year mortality rates, 1980-1999. Deaths are from the NBER mortality microdata (which contain decedents' state of birth from 1979-2004), and denominators are constructed using aggregate population data and the 1980 Census. The figure plots the estimated coefficients on interactions between initial AFDC rates and event-time dummies for each of 30 years before and 5 years after Medicaid. Time -19 is omitted. The model includes birth-state, region-by-birth-year, and Medicaid-year-by-birth-year fixed effects, and state per-capita income and hospital beds averaged over childhood, and each cohort's the general fertility rate. The nonwhite estimates also adjust for a linear trend interacted with $AFDC^{*}_{rs}$ for event-times prior to -10. Estimates are weighted by the 1980 population. The dashed lines are based on standard errors clustered by birth state. The break at zero is fixed because all subsequent cohorts are exposed to Medicaid for their entire childhoods.

Figure 5. Reduced-Form Event-Study Estimates of Medicaid's Effect on Adult Rates of Ambulatory Difficulty by Race (coefficients×100)



Notes: The dependent variable is the share of respondents in each state-of-birth-by-cohort-by-survey-year 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). This is the most prevalent disability, other than the potentially endogenous self-reported work limitation. 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 initial AFDC rates and event-time dummies for each of 23 years before and 5 years after Medicaid. Time -19 is omitted. The model includes birth-state, region-by-birth-year, and Medicaid-year-by-birth-year fixed effects, and state per-capita income and hospital beds averaged over childhood, and each cohort's the general fertility rate and infant mortality rate. 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 coefficients come from a model that fits straight lines between event times [-23, -11], [-10,-1], and [0,5]. The break at zero is fixed because all subsequent cohorts are exposed to Medicaid for their entire childhoods. The break at -10 comes 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 2.5.

Figure 6. Survival-Adjusted Bounds on Instrumental Variables Estimates of the Effect of Early Childhood Medicaid Eligibility on Nonwhite Ambulatory Difficulty

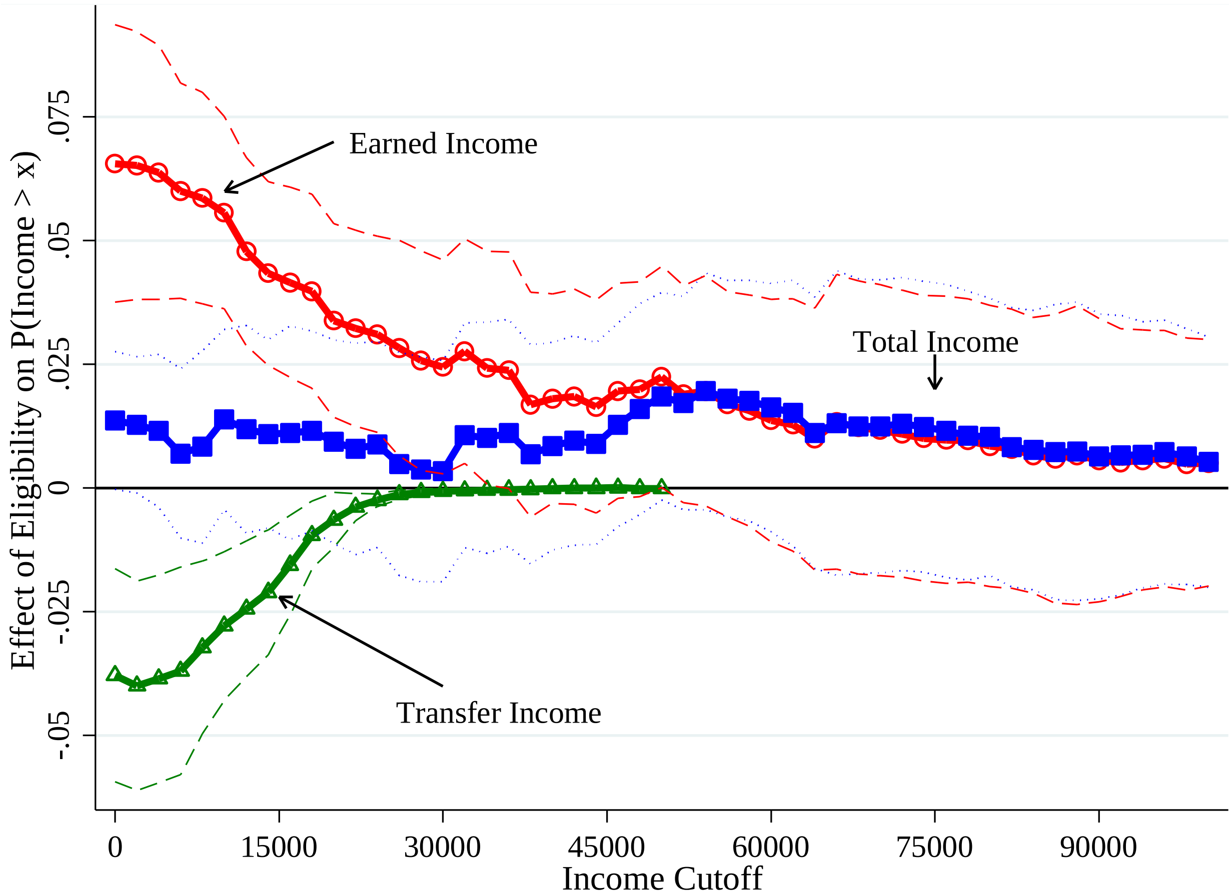


Notes: The solid line plots estimated IV effects of early childhood eligibility on nonwhite adult ambulatory difficulty rates among adults who would have survived to 2000 without Medicaid (right axis), under different assumptions about the disability rates among adults who were induced by Medicaid to survive to 2000. The share of the population induced to survive because of Medicaid is based on observed and counterfactual cumulative probabilities of surviving to 1980—constructed from the age-specific treatment effects in Goodman-Bacon (2015)—and, conditional on surviving to 1980, the probability of surviving to 1999—constructed from the results in column 2 of table 3. Even though the best-fitting trend break for nonwhite disability is at age -4, this figure estimates effects under age 10 to match the relevant range for whites and the ages when nonwhite mortality was affected. The histogram shows the distribution of observed nonwhite disability rates by state and year of birth for cohorts born between 1955 and 1970. These likely understate the counterfactual disability rates needed for this exercise, but are shown for reference.

Figure 7. Reduced-Form Event-Study Estimates of Medicaid's Effect on Employment Rates and Disability Benefit Receipt, White Respondents (coefficients×100)

Notes: The dependent variable is the share of white respondents in each state-of-birth-by-cohort-by-survey-year 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 2000-2014 (24,411 observations). 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 5.

Figure 8. Instrumental Variables Estimates of the Effect of Medicaid Eligibility Before age 10 on the Distribution of Income By Source, White Respondents



Notes: The figure plots instrumental variables estimates of the effect of cumulative Medicaid eligibility under age 10 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 2014. $50,000 is the maximum of the transfer income variable.

Table 1. Balancing Test: Initial AFDC Rates and Pre-Medicaid State Characteristics

	(1)	(2)	(3)	(4)
	White		Nonwhite	
Outcome year:	Mean	Coef. on Initial White AFDC	Mean	Coef. on Initial Nonwhite AFDC
A. Child Poverty Rate				
1950	0.38	-0.005	0.84	-0.008
		[0.016]		[0.002]
1960	0.22	-0.006	0.67	-0.011
		[0.013]		[0.003]
1970	0.10	0.005	0.44	-0.006
		[0.005]		[0.002]
H_0: Equal slopes (*p*-value)		**0.67**		**0.43**
B. Log Government Expenditure				
1932	13.32	0.11	13.61	0.03
		[0.13]		[0.02]
1942	13.92	0.14	14.29	0.04
		[0.13]		[0.02]
1962	15.06	0.17	15.53	0.02
		[0.15]		[0.02]
H_0: Equal slopes (*p*-value)		**0.95**		**0.80**
C. Infant Mortality Rate				
1947	29.72	0.09	47.89	-0.19
		[0.55]		[0.13]
1957	22.86	0.15	43.46	-0.04
		[0.25]		[0.09]
1965	21.65	-0.05	40.55	-0.02
		[0.21]		[0.1]
H_0: Equal slopes (*p*-value)		**0.81**		**0.53**
D. General Fertility Rate				
1947	112.40	-0.92	107.50	-0.51
		[1.55]		[0.4]
1957	120.10	-1.85	154.90	-0.75
		[1.12]		[0.43]
1965	91.46	-0.28	127.00	-0.21
		[0.41]		[0.27]
H_0: Equal slopes (*p*-value)		**0.41**		**0.54**

Notes: Regressions are weighted by sum of Census weights in panel A, total state population in panel B, births in panel C, and female population 15-44 in panel D. Heteroskedasticity robust standard errors are in brackets. Each panel presents cross-sectional relationships between $AFDC^*_{rs}$ and state characteristics in the years listed (a test of balance in levels), as well as the *p*-value from a test that these slopes are the same (a test of differential trends). Sources: child poverty rates are from IPUMS (Ruggles et al. 2010), government expenditures are from Sylla, Legler, and Wallis (2006), births and infant deaths are from printed volumes of Vital Statistics of the United States.

Table 2. First-Stage Relationship between Predicted Eligibility and Migration-Adjusted Cumulative Medicaid Eligibility

	(1)	(2)	(3)
	Cumulative Eligibility, Age 0 -18	*Cumulative Eligibility, Age 0 -10*	*Cumulative Eligibility, Age 11 -18*
		A. White Adults	
Predicted Eligibility at:			
Age 0-18	0.72		
	[0.15]		
Ages 0-10		0.77	0.09
		[0.23]	[0.07]
Ages 11-18		-0.04	0.57
		[0.1]	[0.13]
F-statistic	22.5		
Angrist/Pischke *F*-statistic		37.3	17.8
		B. Nonwhite Adults	
Ages 0-18	0.21		
	[0.17]		
Ages 0-10		0.41	-0.42
		[0.20]	[0.08]
Ages 11-18		-0.03	0.55
		[0.08]	[0.13]
F-statistic	1.5		
Angrist/Pischke *F*-statistic		18.5	14.5

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 and 3 present similar first-stage estimates that split eligibility into two sub-periods: age 0-10 and age 11-18. 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 3. Instrumental Variables Estimates of Medicaid's Effect on log 20-Year Mortality Rates by Race and Cause (coefficients×100)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cause of Death:	All-Cause	Non-AIDS-Related Causes	Internal (Non-AIDS, incl. suicide)	Cardio-vascular	Cancer	Suicide	External (Homicide + Accidents)
Childhood Medicaid Eligibility				*A. White Adults, 1980-1999*			
Ages 0-10	-23.9	-15.5	-31.7	-15.0	-19.0	-39.7	-1.9
	[9.8]	[5.4]	[6.9]	[9.6]	[9.0]	[8.4]	[6.7]
Ages 11-18	7.2	-11.0	-8.6	31.1	1.5	0.9	-10.4
	[14.3]	[7.2]	[7.2]	[10.8]	[7.8]	[11.2]	[14.3]
H_0: 0-10 = 11-18 (p-val)	0.19	0.70	0.08	0.01	0.13	0.03	0.65
Mean Dependent Variable (deaths per 100,000)	3,690	3,090	2,280	800	937	293	826
Childhood Medicaid Eligibility				*B. Nonwhite Adults, 1980-1999*			
Ages 0-10	-30.7	-19.6	-25.2	-27.3	-17.3	-39.3	-18.2
	[13.3]	[9.4]	[7.6]	[11.1]	[8.3]	[15.9]	[10.1]
Ages 11-18	10.0	4.8	7.8	6.7	2.2	-5.0	4.0
	[10.7]	[7.0]	[6.9]	[8.3]	[7.2]	[9.7]	[7.2]
H_0: 0-10 = 11-18 (p-val)	0.05	0.06	0.01	0.04	0.11	0.06	0.11
Mean Dependent Variable (deaths per 100,000)	7,980	5,600	3,910	1,880	1,360	203	1,830

Notes: The table presents instrumental variables estimates of Medicaid's effect on log cumulative mortality rates (1980-1999) by cause. Standard errors are clustered by state of birth. The average mortality rates by cause do not add to the total because they are calculated using cause-elimination life table methods to account for the confounding influence of competing risks from the other causes.

Table 4. Instrumental Variables Estimates of Medicaid's Effect on Adult Rates of Ambulatory Difficulty by Race and Across Specifications (coefficients×100)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Childhood Medicaid Eligibility				*A. White Adults, 2000-2007*			
Ages 0-10	-1.56	-3.87	-3.26	-2.33	-3.82	-2.73	-4.21
	[1.69]	[1.17]	[1.36]	[1.16]	[1.08]	[0.82]	[1.18]
Ages 11-18	1.63	-1.06	-1.34	-0.31	-0.81	0.70	-1.42
	[2.62]	[1.45]	[1.31]	[1.28]	[1.52]	[1.24]	[1.52]
H_0: 0-10 = 11-18 (*p*-val)	0.02	0.20	0.36	0.22	0.18	0.06	0.21
Observations		14,331				460,381	
Childhood Medicaid Eligibility				*B. Nonwhite Adults, 2000-2007*			
Ages 0-4	-1.33	-2.96	0.78	-2.75	-3.09	-5.24	-2.62
	[0.93]	[1.81]	[1.48]	[1.11]	[1.36]	[1.84]	[1.31]
Ages 5-18	0.04	-0.16	-0.15	-0.04	0.15	-0.49	0.29
	[0.44]	[0.71]	[1.08]	[0.64]	[0.54]	[0.92]	[0.6]
H_0: 0-4 = 5-18 (*p*-val)	0.11	0.04	0.62	0.01	0.01	0.00	0.01
Observations		14,105				132,334	
Covariates	Initial eligibility + Time-to-Medicaid FE	(1) + State FE+ Year FE + Medicaid-Year-by-Cohort Fe + Region-by-Cohort FE + Xst		(2) + State-specific cohort trends	(2) + Cohort-by-Year-by-Unemployment-Rate interactions	(2) + Cohort-by-state-of-residence FE	(2) + State-of-birth-by-state-of-residence FE
Weighted?	Y	Y	N	Y	Y	Y	Y
Level of Observation		State-of-birth/cohort/year				State-of-residence/state-of-birth/cohort/year	

Notes: The table presents instrumental variables estimates of Medicaid's effect on ambulatory difficulty across specifications. Columns 1-4 use a state-of-birth-by-cohort-by-year dataset (as in figure 5), and columns 5-7 use a state-of-birth-by-cohort-by-year-by-state-of-residence dataset. Standard errors are clustered by state of birth. The age ranges are determined by the best-fitting trend breaks from figure 5. Nonwhite results come from a two-step procedure in which a linear pre-trend from event-time -23 to -4 is removed and IV estimates are based on these adjusted data.

Table 5. Instrumental Variables Estimates of Medicaid's Effect on Adult Disability Measures by Race (coefficients×100)

	(1) Ambulatory Difficulty	(2) Hearing/Vision Difficulty	(3) Mobility Difficulty	(4) Self-Care Difficulty	(5) Cognitive Difficulty	(6) Work Limitation
Childhood Medicaid Eligibility			*A. White Adults, 2000-2007*			
Ages 0-10	-3.87	-1.18	-1.36	-1.26	-1.72	-3.30
	[1.17]	[0.29]	[0.36]	[0.29]	[0.4]	[0.81]
Ages 11-18	-1.06	0.31	-0.67	0.38	0.34	-2.54
	[1.45]	[0.71]	[0.57]	[0.5]	[0.64]	[1.23]
H_0: 0-10 = 11-18 (*p*-val)	0.20	0.06	0.33	0.02	0.03	0.60
Mean Dependent Variable	8.61	3.15	3.75	2.27	4.41	8.12
Childhood Medicaid Eligibility			*B. Nonwhite Adults, 2000-2007*			
Ages 0-4	-2.96	0.24	-0.48	0.001	-0.04	0.61
	[1.81]	[0.81]	[0.68]	[0.53]	[0.84]	[1.22]
Ages 5-18	-0.16	-0.21	0.25	0.01	-0.35	0.46
	[0.71]	[0.47]	[0.36]	[0.31]	[0.47]	[0.65]
H_0: 0-4 = 5-18 (*p*-val)	0.04	0.37	0.14	0.99	0.63	0.86
Mean Dependent Variable	12.70	4.03	6.53	3.93	6.87	12.20
	Does this person have any of the following long-lasting conditions:		Because of a physical, mental, or emotional condition lasting $\geq$ 6 months does this person have any difficulty:			
Question Text	...that substantially limits $\geq$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 4 and column 2 of table 4 (that coefficient is reproduced in column 1). Panel A has 14,331 observations, except column 6 (N=12,417), which omits the year 2000 because the work-limiting disability responses differ strongly from subsequent surveys (see appendix figure 1.3). Panel B has 14,105 observations (except column 6, which has 12,191). The age ranges are determined by the best-fitting trend breaks from figure 5. Nonwhite results come from a two-step procedure in which a linear pre-trend from event-time -23 to -4 is removed and IV estimates are based on these adjusted data.

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

	(1) Out of the Labor Force	(2) Currently Employed	(3) Any Employment Last Year	(4) Full-Time/Full-Year Employment
Childhood Medicaid Eligibility	*A. White Adults, 2000-2014*			
Ages 0-10	-6.78	6.02	6.50	4.91
	[1.55]	[1.25]	[1.45]	[0.72]
Ages 11-18	1.64	-2.30	-1.17	-2.56
	[2.]	[1.94]	[1.96]	[1.89]
H_0: 0-10 = 11-18 (*p*-val)	<0.01	<0.01	<0.01	<0.01
Mean Dependent Variable	22.0	74.2	80.9	51.4
Childhood Medicaid Eligibility	*B. Nonwhite Adults, 2000-2014*			
Ages 0-4	-2.23	2.18	2.07	-1.74
	[2.16]	[2.12]	[1.88]	[1.72]
Ages 5-18	-0.67	0.66	0.39	0.05
	[1.14]	[1.03]	[1.04]	[0.82]
H_0: 0-4 = 5-18 (*p*-val)	0.24	0.28	0.15	0.11
Mean Dependent Variable	27.4	65.6	74.3	45.0

Notes: The sample includes the 2000-2014 Census/ACS and has 24,411 observations. For details on the specification see notes to figure 5.

Table 7. Instrumental Variables Estimates of Medicaid's Effect on Public Assistance Receipt for Whites (coefficients×100)

	(1) Any Public Assistance	(2) Disability Benefits (SSDI or SSI)	(3) TANF or General Assistance	(4) Medicaid	(5) Any Insurance
Childhood Medicaid Eligibility					
Ages 0-10	-3.78	-4.54	0.77	-4.16	-0.95
	[1.1]	[1.14]	[0.14]	[1.06]	[0.92]
Ages 11-18	-2.34	-1.95	-0.25	-0.52	0.25
	[2.86]	[2.75]	[0.21]	[1.55]	[1.7]
H_0: 0-10 = 11-18 (*p*-val)	0.63	0.37	0.00	0.02	0.53
Mean Dependent Variable	9.6	8.7	1.1	12.6	87.7

Notes: The sample includes the 2000-2014 Census/ACS and has 24,411 observations. For details on the specification see notes to figure 5.

Table 8. Instrumental Variables Estimates of Medicaid's Effect on Average Income by Source for Whites (coefficients×100)

	(1) Earned Income	(2) Earned Income (Trimmed)	(3) Transfer Income	(4) Total Income	(5) Poverty Rate
Childhood Medicaid Eligibility					
Ages 0-10	2,212	2,690	-593	1,197	0.87
	[2,359]	[1,208]	[163]	[2,386]	[0.98]
Ages 11-18	-4,016	-4,185	-515	-7,115	2.38
	[3,323]	[1,798]	[425]	[3,152]	[0.75]
H_0: 0-10 = 11-18 (*p*-val)	0.25	0.01	0.85	0.11	0.17
Mean Dependent Variable	44,932	39,266	1,009	50,181	7.5

Notes: The sample includes the 2000-2014 Census/ACS and has 24,411 observations. For details on the specification see notes to figure 5.

Table 9. Instrumental Variables Estimates of Medicaid's Effect on Educational Attainment for Whites (coefficients×100)

	(1) High School Grad	(2) Any College	(3) Bachelor's Degree
Childhood Medicaid Eligibility		*A. White Adults, 2000-2014*	
Ages 0-10	1.15	1.59	0.62
	[1.11]	[3.08]	[1.97]
Ages 11-18	0.42	-1.32	-1.99
	[1.81]	[3.26]	[1.59]
H_0: 0-10 = 11-18 (*p*-val)	0.76	0.60	0.37
Mean Dependent Variable	91.8	62.7	31.5

Notes: The sample includes the 2000-2014 Census/ACS and has 24,411 observations. For details on the specification see notes to figure 5.