

“The Long-Run Effects of Childhood Insurance Coverage: Medicaid Implementation, Adult Health and Labor Market Outcomes”

Andrew Goodman-Bacon

Online Appendix

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APPENDIX 1. DATA

A. *Data on Recipients of Aid to Families with Dependent Children*

The data on the characteristics of AFDC recipients come from two sources. The race shares of adult and child recipients were entered from printed reports: “Aid to Dependent Children in a Postwar Year, Characteristics of Families Receiving ADC, June 1948” (Alling and Leisy 1950), “Characteristics of Families Receiving Aid to Dependent Children, November 1953” (Department of Health 1955), “Characteristics and Financial Circumstances of Families Receiving Aid to Dependent Children, Late 1958” (Mugge 1960) and “Characteristics of Families Receiving Aid to Families with Dependent Children, November-December 1961” (DHEW 1963). Biennial microdata on recipients comes from the National Archives Surveys of Recipients of Aid to Families with Dependent Children 1967-1979 (DHEW 2000, 2011). Except for the 1967 file, the data are at the AFDC unit level.

The race shares for children are the means of a white dummy, weighted by product of the sample weight and the number of recipient children in the household (under the assumption that the race of the children is the same as the race of the AFDC payee). The race code for Latina recipients is missing in some years and varies strongly between some years (from “other” to “white”). In these cases, I assign Latina recipients the average value of the binary race code observed among all other Latina recipients. I linearly interpolate the race shares for missing years between 1958 and 1979. To construct race-specific recipient counts, I multiply the estimated race shares by state-level counts of AFDC children transcribed from federal reports from 1948-1988. Data for the first half of 1948 and from 1971-1988 (except 1985) come from the “Current Operating Statistics” appendix of the Social Security Bulletin (Social Security Administration 1948, 1971, 1972, 1973, 1974, 1975, 1976, 1977, 1978, 1979, 1980, 1981, 1982, 1983, 1984f, c, a, e, g, d, b, 1985c, d, b, a, 1986a, b, 1988b, a, c, 1989g, c, a, e, h, d, b, f, 1990). Data for the second half of 1948 through the end of 1970 come from “Advanced Release of Statistics on Public Assistance” or “Public Assistance Statistics” (Department of Health 1953-1961, 1961-1962, 1962-1965, 1966b, 1966-1967, 1967-1968, 1968a, b, 1969-1970, 1966a, Federal Security Agency 1948-1950, 1951, 1951-1953). State-by-month data on AFDC recipients from July 1959 – December 1996 are from United States Department of Health and Human Services (2012). To calculate race-specific child AFDC rates, I divide by the state population ages 0-19 (Haines and ICPSR 2010, SEER 2013). The resulting measures are monthly AFDC participation rates observed once per year. Summing them over years of childhood as in equation (1) yields a cumulative eligibility measure that refers to full years of eligibility.

A. *Mortality by Birth State*

From 1979-2016, the Vital Statistics Multiple Cause of Death data contain information on decedents’ state of birth. I collapse the count of deaths by state of birth, year of birth, race (white/nonwhite), year of death, and cause of death listed in table 3 (based on the 34 or 39 cause recodes). The denominators are calculated by first calculating the joint distribution of state of birth and race by single age in the 1980 5% IPUMS Census extract (Ruggles et al. 2010) and multiplying this by population counts by age.¹

B. *Census and American Community Survey Data*

The main analyses use the 5% and 1% extracts from the 2000 Census and the 2001-2014 American Community Surveys (Ruggles et al. 2010). I keep respondents born in the US ages 25-64 and born no later than 1976 and collapse the data to the state-of-birth, year-of-birth, race, survey year level (and in some models also by state of residence). Table A1.1 lists underlying number of observations, and Figure A1.1 presents histograms of the cell sizes by race.

¹ Available here: <https://www.census.gov/popest/data/state/asrh/1980s/tables/stiag480.txt>

Figure A1.2 plots age profiles of disability measures before and after changes to the question text in 2008. These changes have significant effects on reported disability. For example, rates of ambulatory difficulty are 25-50 percent lower after “lifting or carrying” is removed as part of the prompt. The age pattern of cognitive difficulty, especially for children, is much different when “learning, remembering, or concentrating” is replaced by “concentrating, remembering, or making decisions.” Even though the work limitation question did not change appreciably between 2000 and 2001-2007, the age profile of responses is very different across years. Results for work limitation exclude 2000.

Figure A1.1. Cell Sizes, Disability Sample (2000-07), and Labor Market Sample (2000-17)

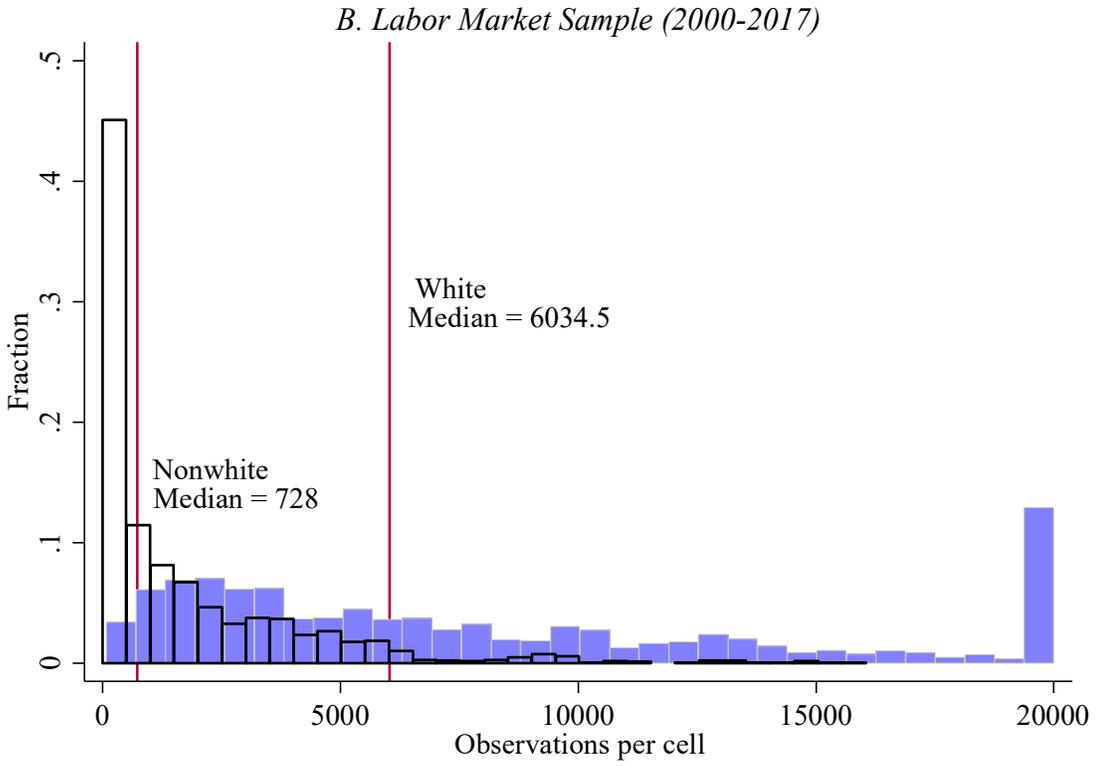
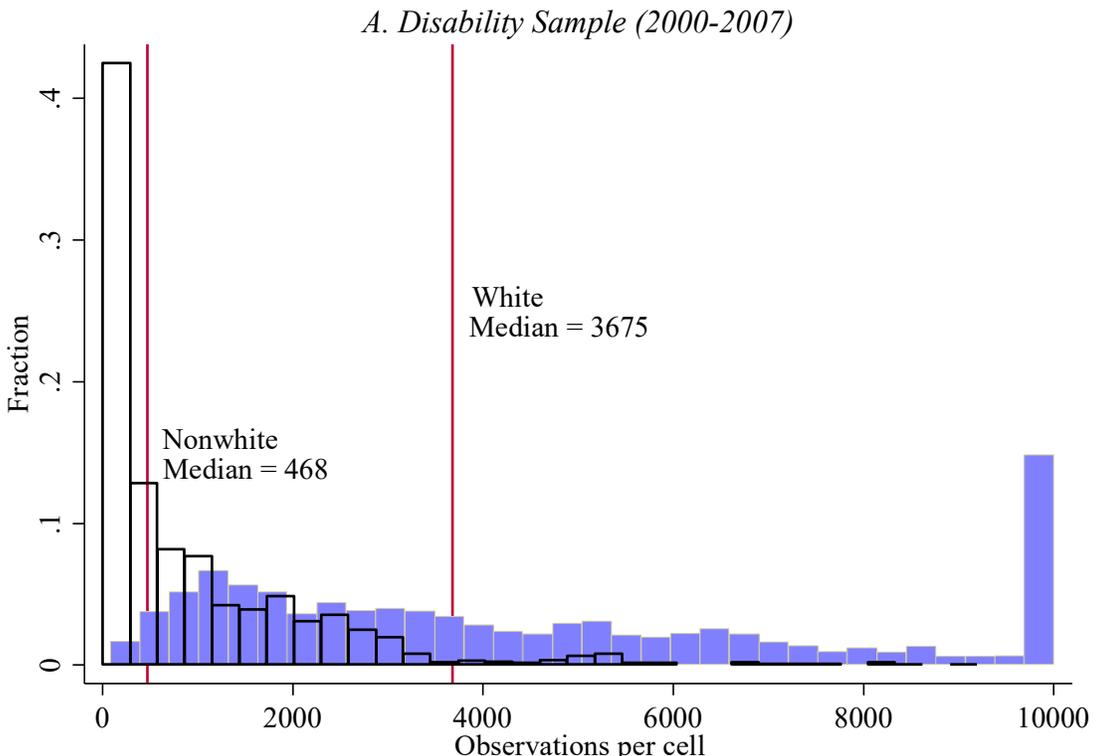
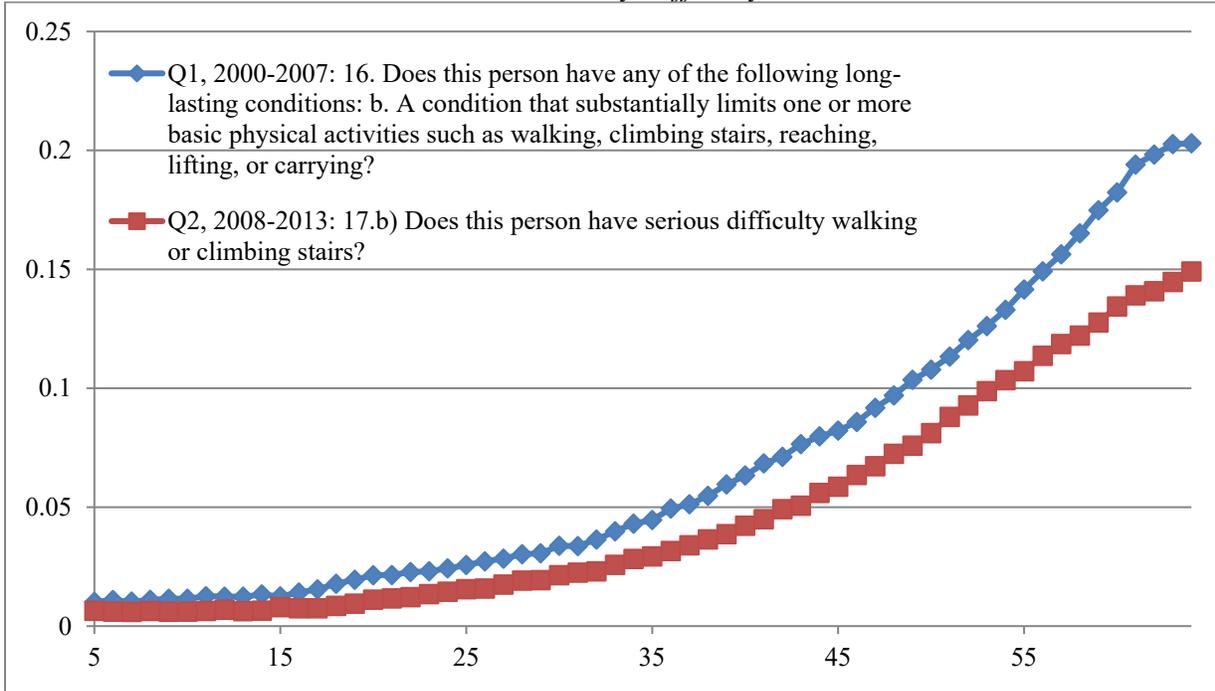
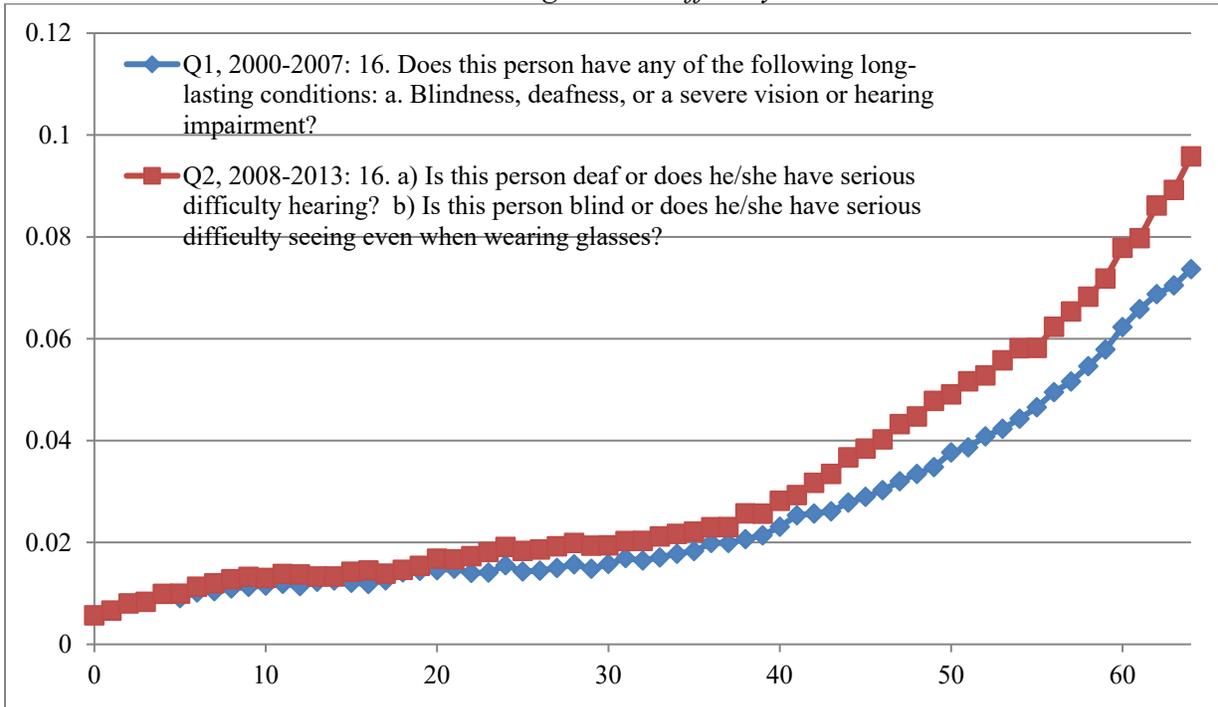


Figure A1.2. Age Profiles of Disability Variables By Survey Years

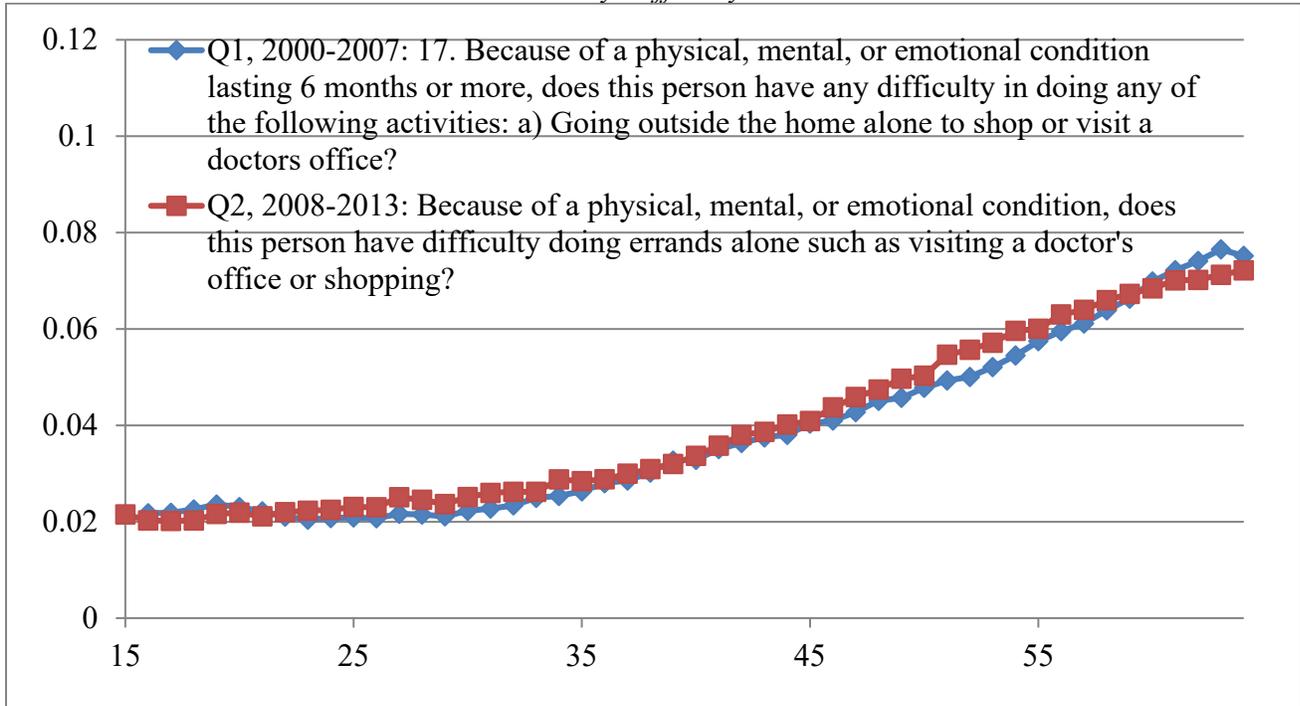
A. Ambulatory Difficulty



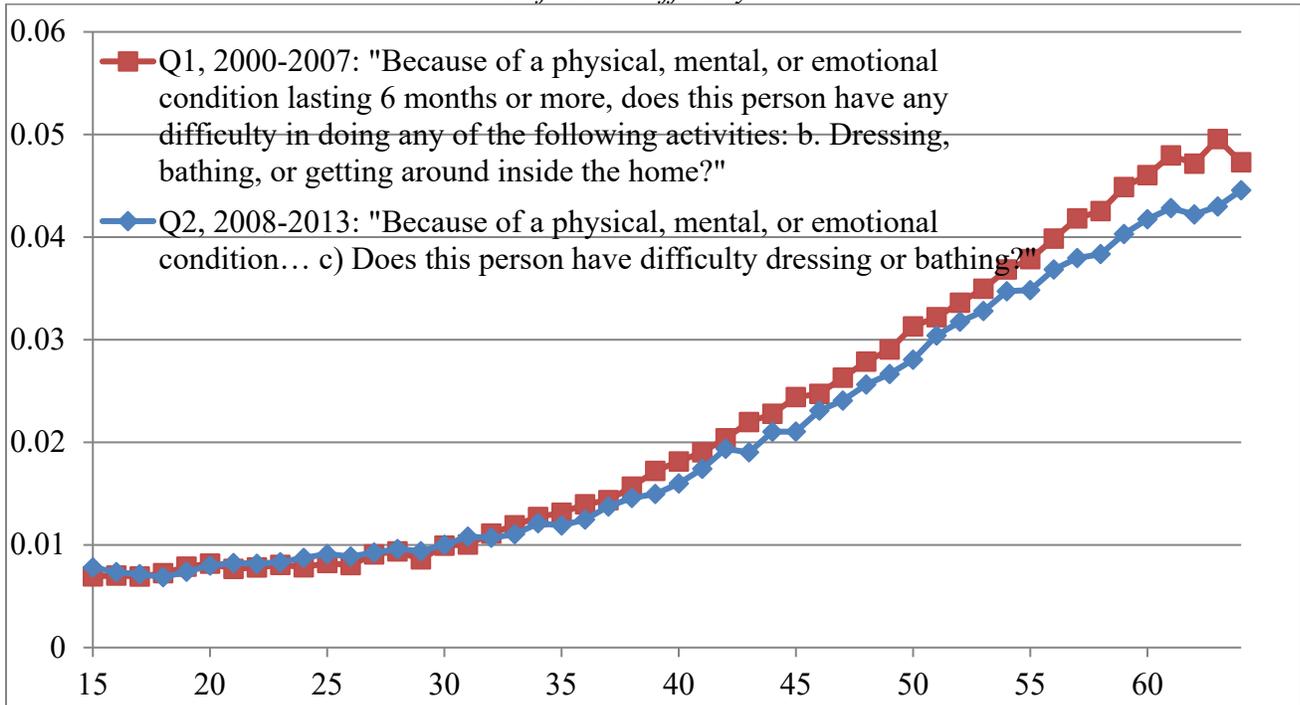
B. Hearing/Vision Difficulty



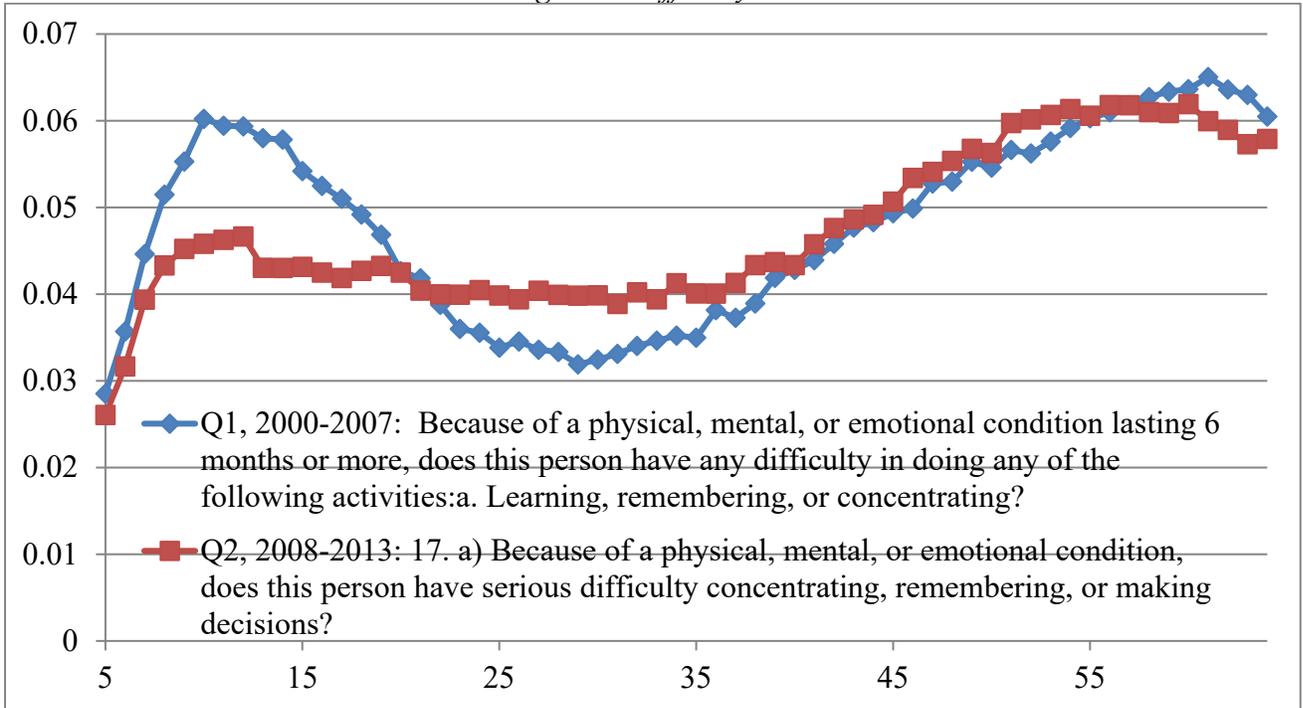
C. Mobility Difficulty



D. Self-Care Difficulty



E. Cognitive Difficulty



F. Work Limitation

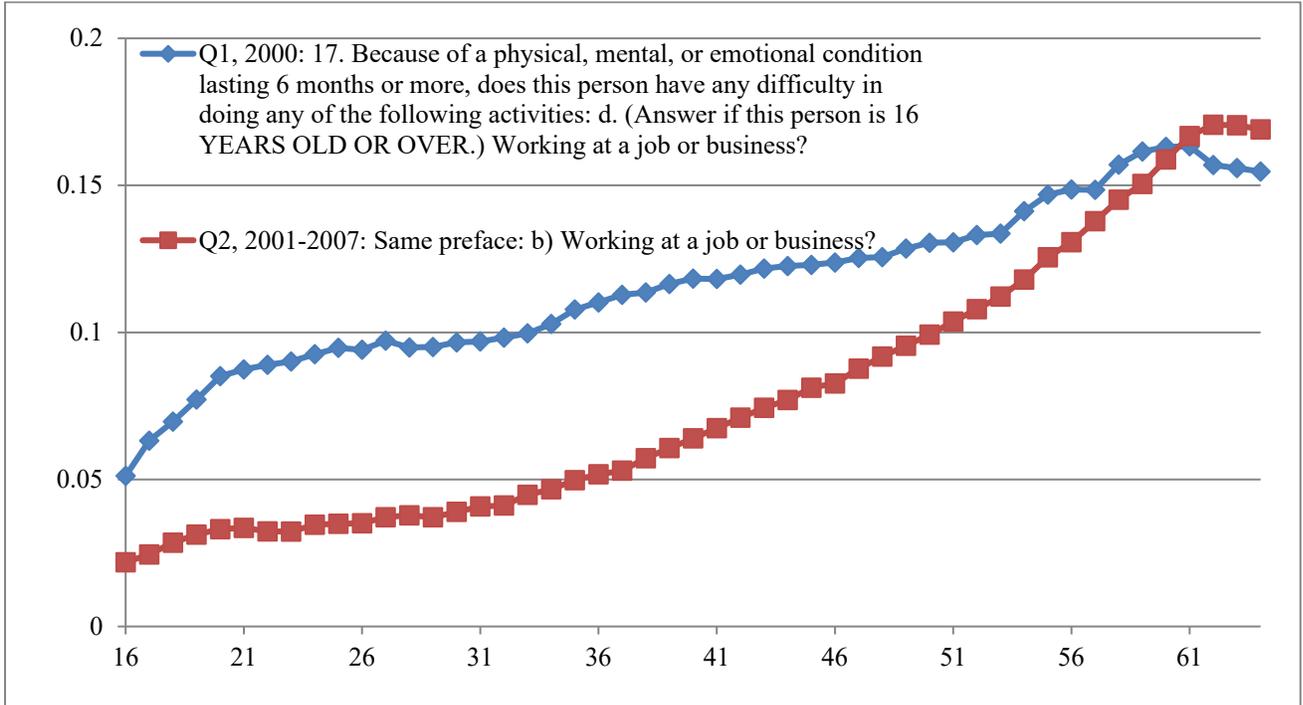
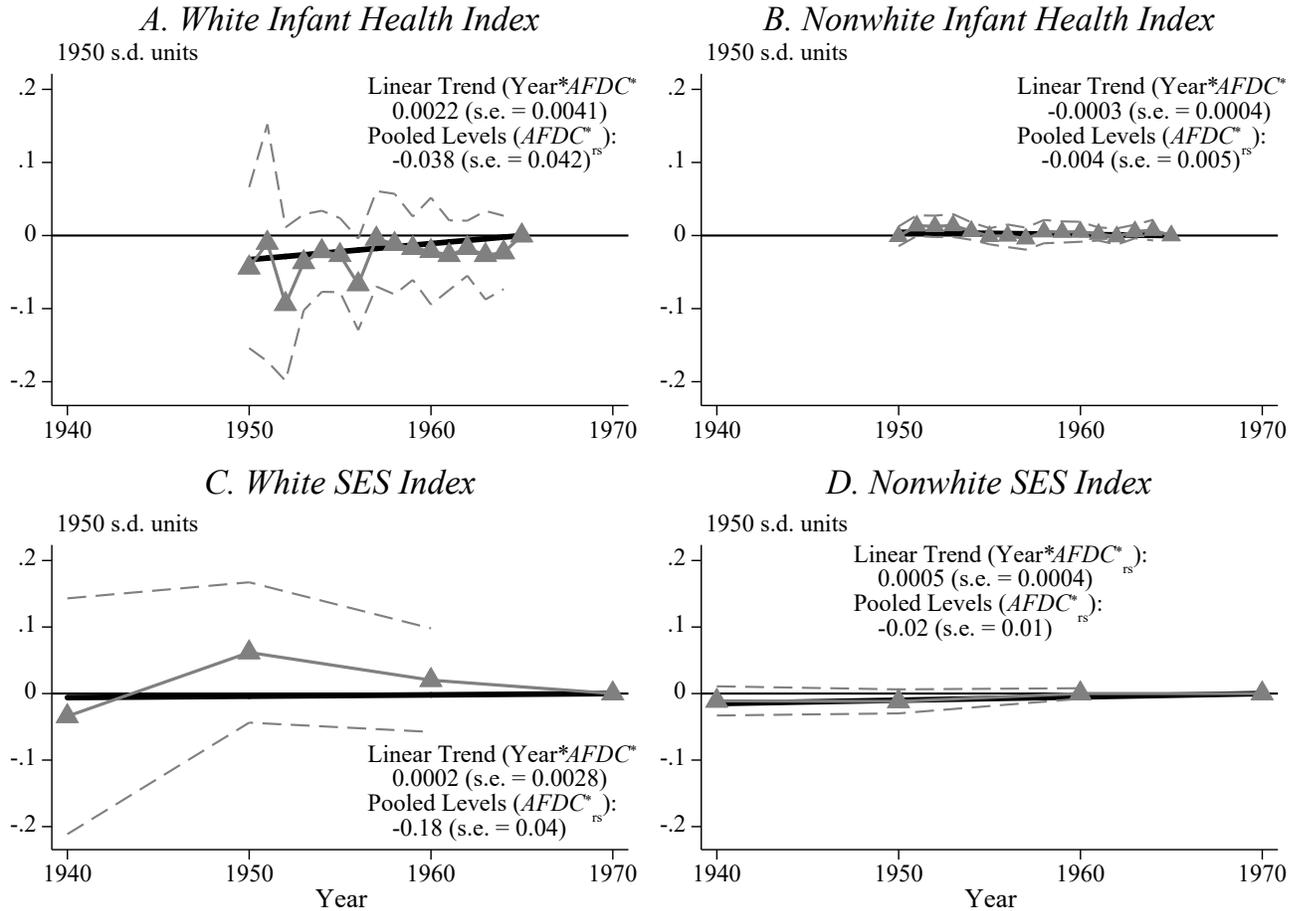


Table A1.1. Census/ACS Observations per Year

Year	Nonwhite	White
2000	1,191,162	6,343,627
2001	69,973	483,994
2002	62,475	433,843
2003	67,934	477,716
2004	66,050	476,566
2005	167,828	1,121,556
2006	180,277	1,126,515
2007	178,491	1,129,186
2008	174,472	1,130,917
2009	174,795	1,126,962
2010	176,781	1,120,076
2011	191,127	1,114,659
2012	186,869	1,114,797
2013	181,023	1,115,244
2014	180,399	1,108,779
2015	177,064	1,106,937
2016	175,773	1,098,609
2017	170,147	1,095,624

APPENDIX 2. ADDITIONAL EVIDENCE ON THE DESIGN

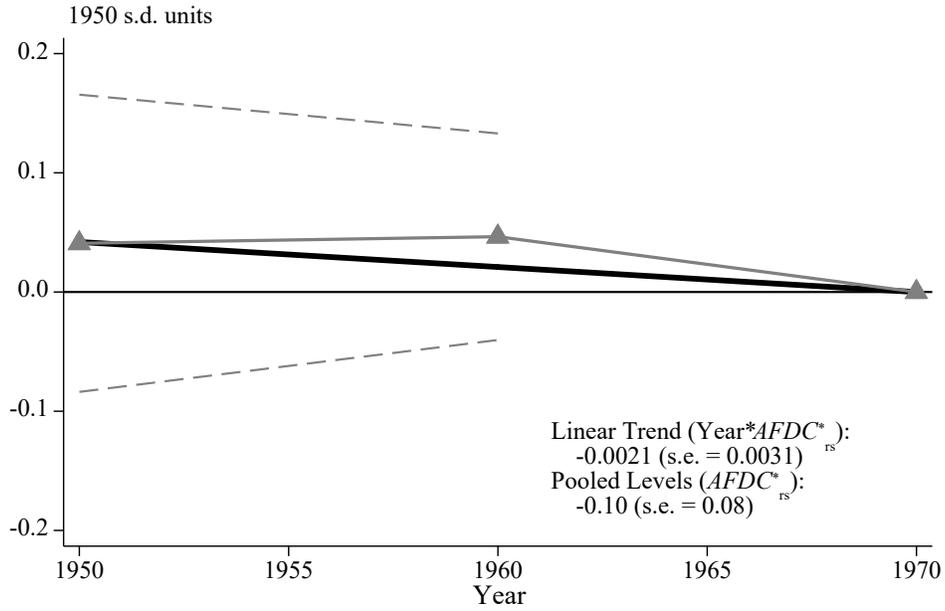
Figure A2.1. Initial Categorical Eligibility is Uncorrelated with Pre-Medicaid Trends in Health and Socioeconomic Measures



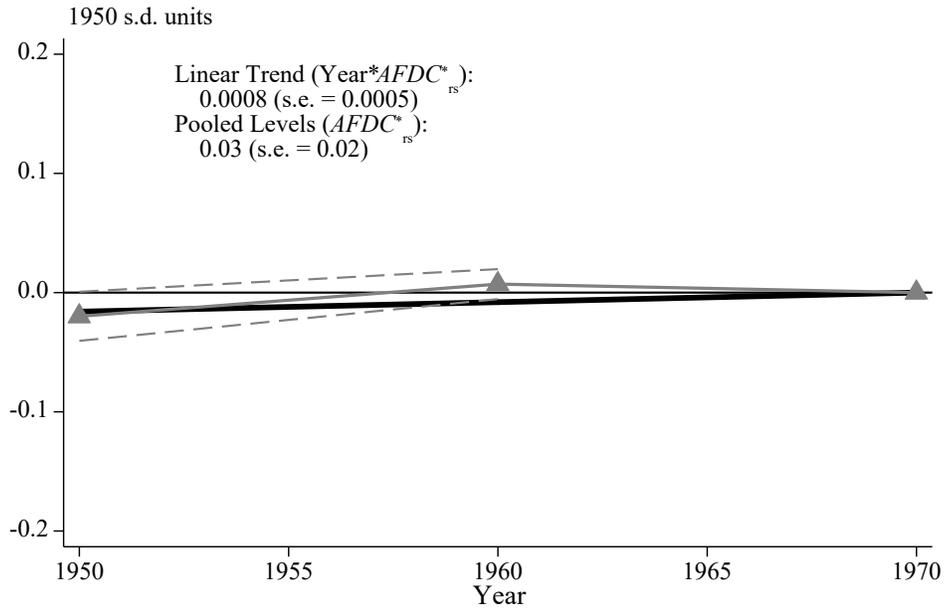
Notes: The infant health index is an equally weighted mean of the following variables standardized by their 1950 mean and standard deviation: low and very low birth weight rates, neonatal and postneonatal infant mortality rates, the sex ratio at birth, and the share of births in a hospital. The SES index is constructed similarly (for children under age 10) and includes the share of children in households whose head has a high school degree or more, is in the labor force, and is employed; the share of children who live with no parents or both parents; household size; and the share of children ages 4-6 enrolled in school. Appendix Figure A2.1 shows balance in family income and poverty using the 1950-1970 Censuses. The closed triangles are coefficients on the interaction between year dummies and $AFDC_{rs}^*$, and the straight lines are the estimated coefficient on an interaction between continuous year and $AFDC_{rs}^*$. The estimated slope and standard error are noted in the figure. The coefficient for “pooled levels” comes from a bivariate regression of the index on $AFDC_{rs}^*$. Regressions are weighted by births or the sum of Census weights, and standard errors (and the dashed 95-percent pointwise confidence intervals) are clustered by state.

Figure A2.2. Balance in Family Income and Poverty, 1950-1970

C. White SES Index



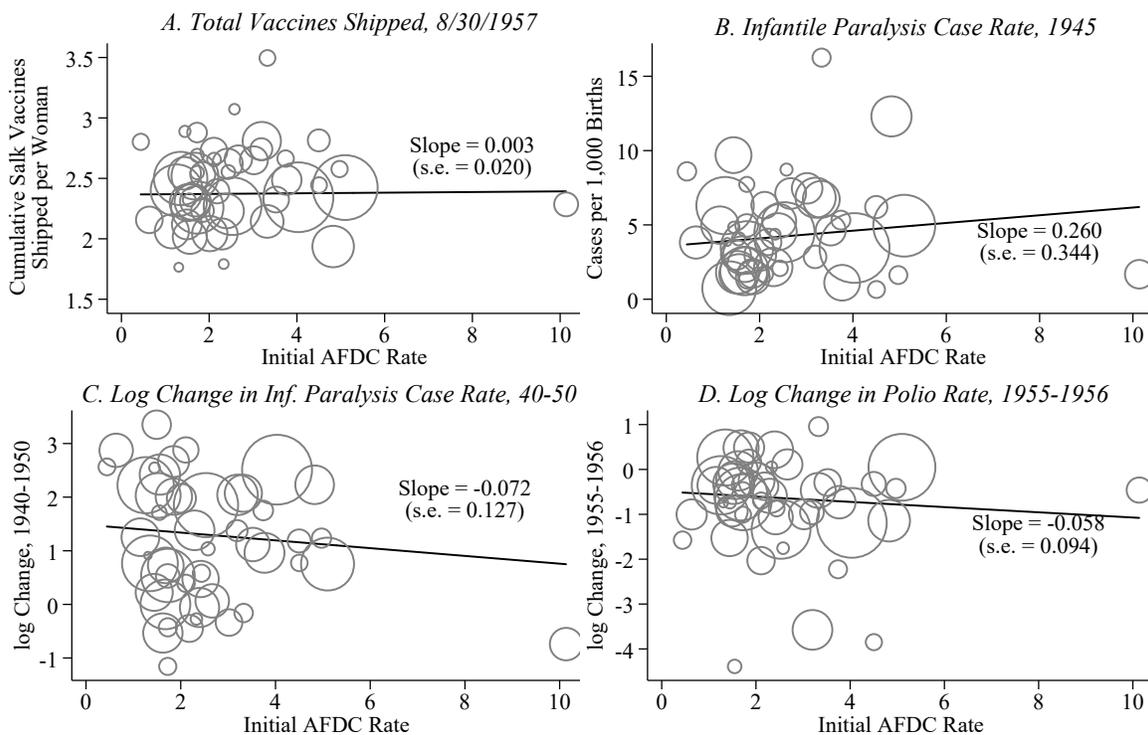
D. Nonwhite SES Index



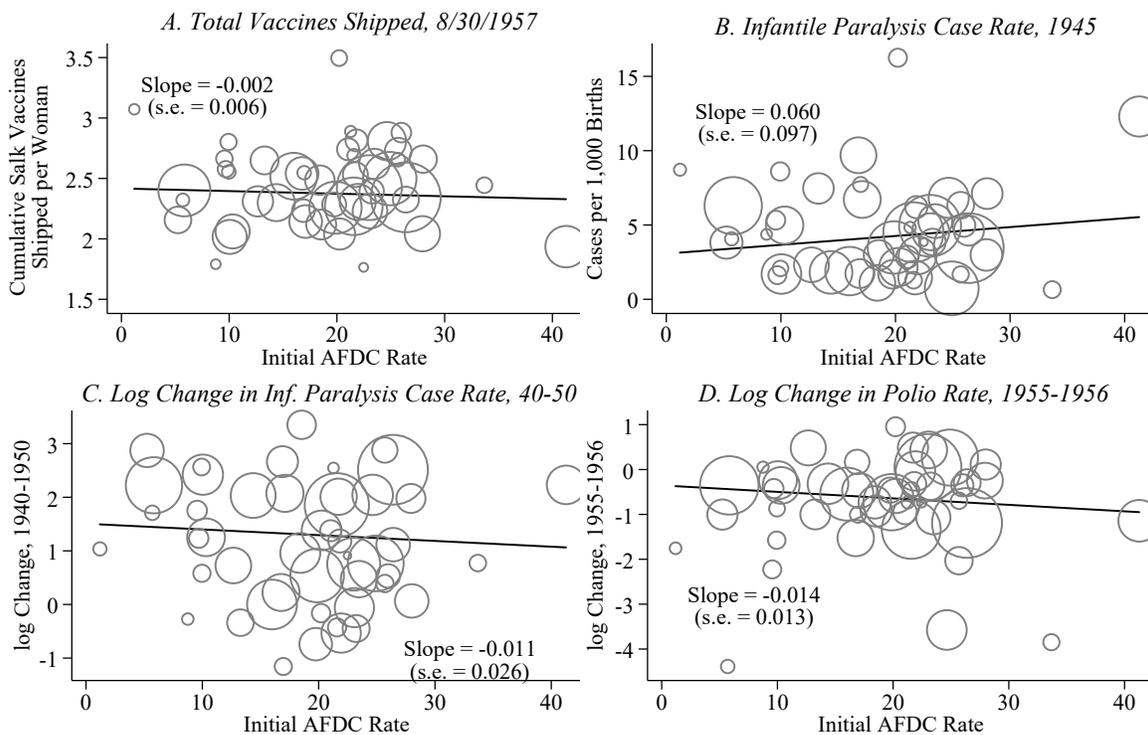
Notes: This figure is analogous to Figure A2.1, but adds the 25th, 50th, and 75th percentile of family income among children as well as the (negative of) the poverty rate to the indices. These are unavailable in the 1940 Census.

Figure A2.3. No Evidence of a Relationship between $AFDC_{rs}^*$ and Polio Incidence or the Dissemination of the Salk Polio Vaccine

A. White



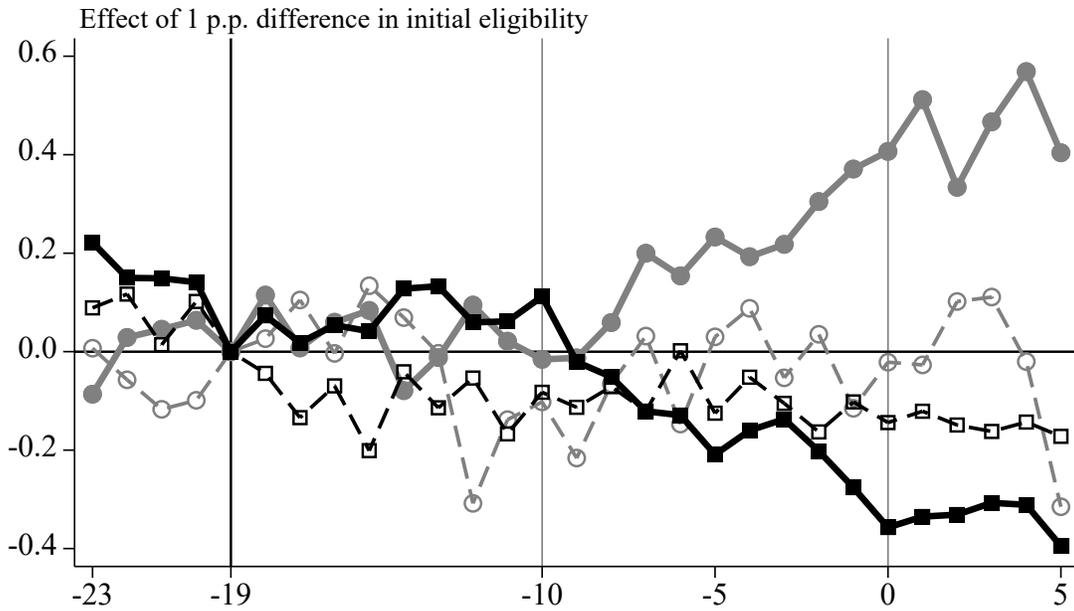
B. Nonwhite



Data on polio vaccines were collected from the March of Dimes Archives by Morgan Connolly with the help of (former) archivist David Rose.

Figure A2.4. Falsification Test: No Relationship Between Initial AFDC Rates and Employment or Public Assistance Receipt for Pre-Medicaid Cohorts in the 1970 and 1980 Censuses

A. White



B. Nonwhite

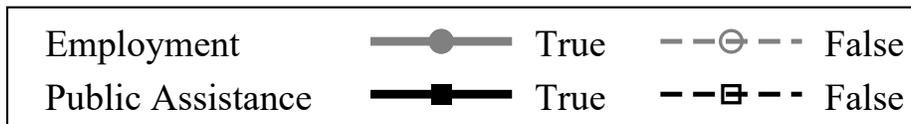
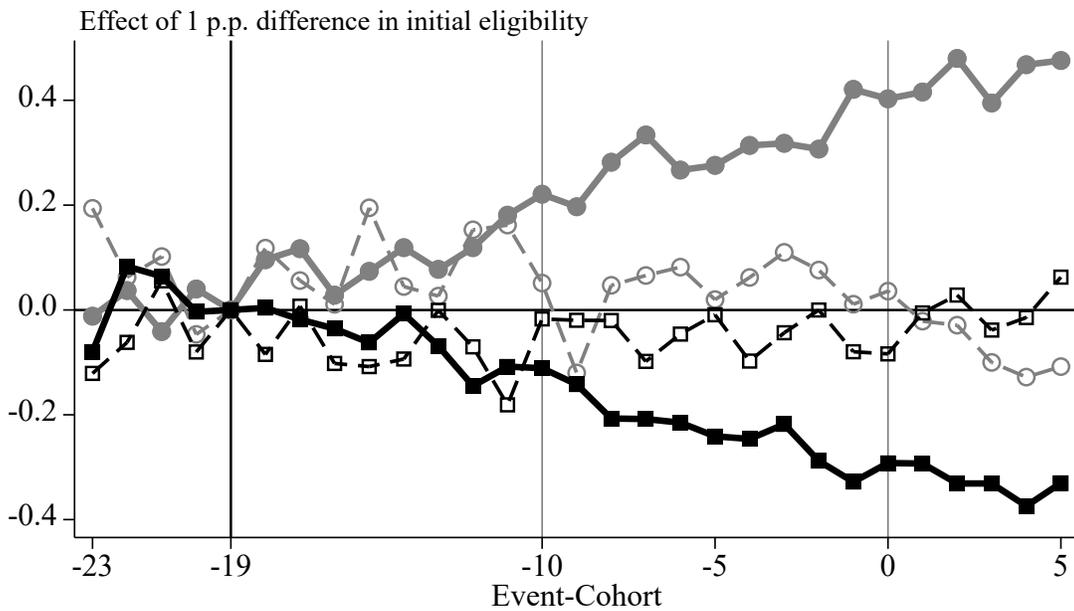
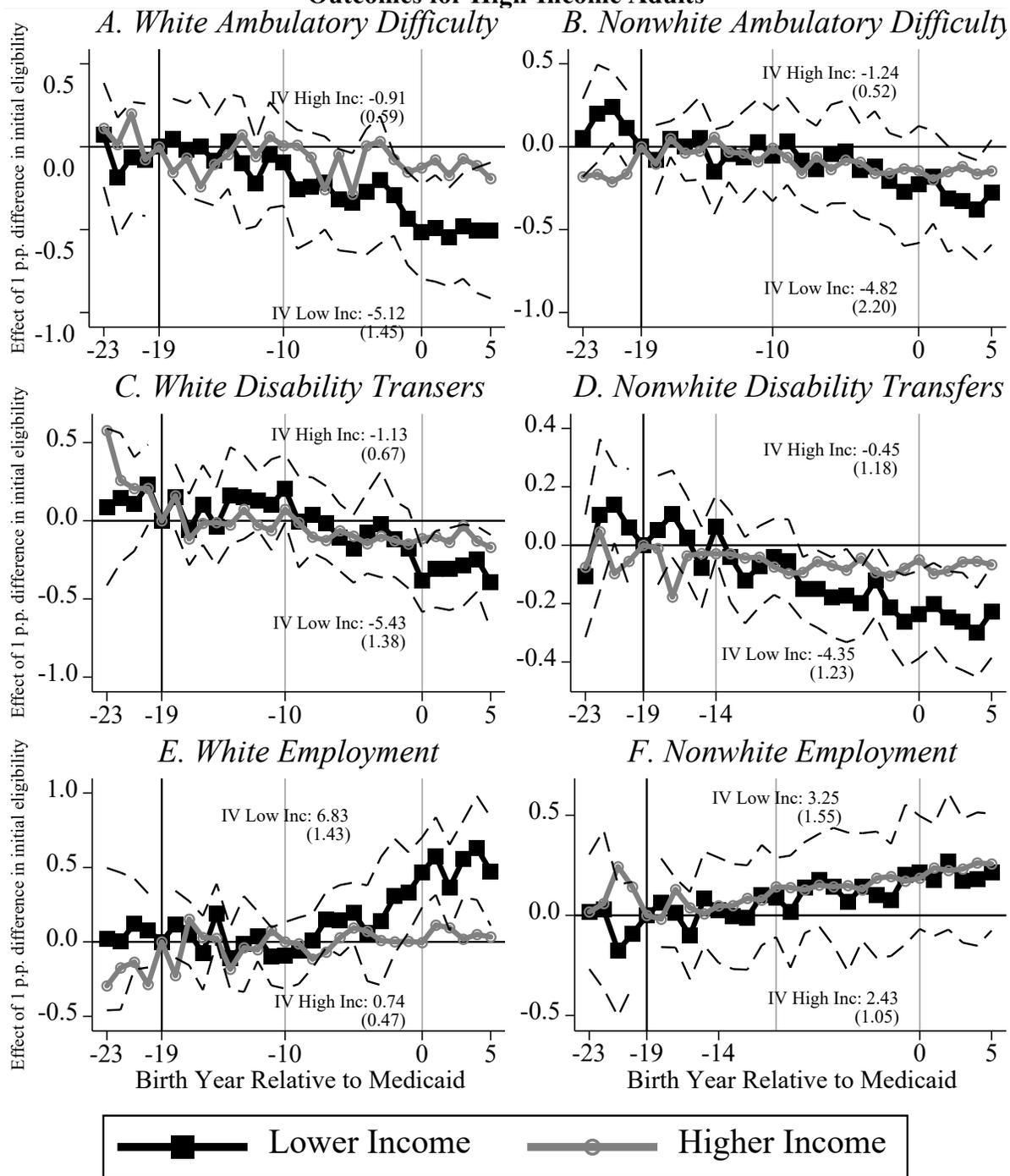
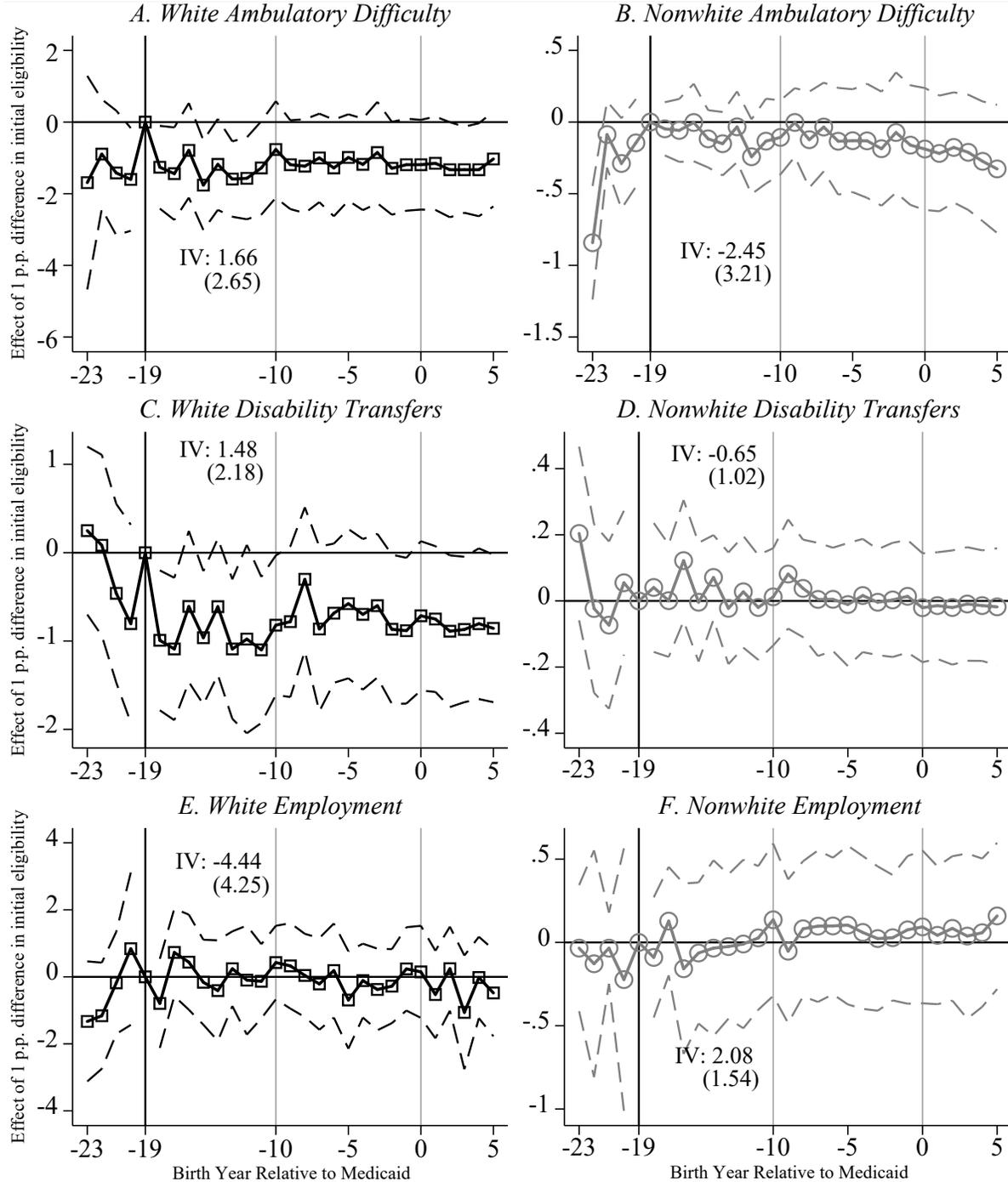


Figure A2.5. Falsification Test: No Relationship Between Initial AFDC Rates and Adult Outcomes for High-Income Adults



Notes: The figure plots event-study estimates for samples of lower- and higher-income adults. For white results lower-income means total personal income less than \$40,000, and higher-income means total personal income greater than \$100,000. Due to smaller sample sizes, nonwhite results only split the sample into those with incomes above or below \$40,000. Therefore, the differences in childhood Medicaid exposure are smaller for this group and so these reduced-form estimates by income differ less as well. Associated IV estimates are in Table A2.2.

Figure A2.6. Falsification Test: No Relationship Between Initial AFDC Rates and Adult Outcomes for Residents Born Outside the US



Notes: The figure presents event-study estimates using a sample of foreign-born respondents to the 2000-2017 Census/ACS who arrived in the use at age 12 or later. This ensures that these respondents had no childhood exposure to Medicaid. I assign them to false childhood states based on their state of residence in the Census. To ensure adequate sample sizes I do not split the foreign-born sample by reported race. I use the same outcome variables calculated for all foreign-born respondents in both the white and nonwhite regressions.

Table A2.1 Additional Balance Tests

	Polio Index	Opinion Index	Household Index
<i>A. White</i>			
AFDC _{rs} *	0.025	-0.054	-0.061
	[0.049]	[0.048]	[0.082]
	48	34	48
	0.007	0.035	0.025
<i>B. Nonwhite</i>			
AFDC _{rs} *	0.002	0.011	0.016
	[0.017]	[0.005]	[0.010]
	48	26	48
	0.001	0.07	0.043
Source:	March of Dimes Archives	SHSUE 1960	Census

Notes: The polio index includes total shipments of the Salk vaccine as of August 1957, the share of births with infantile paralysis in 1945, the change in the share of births with infantile paralysis from 1940 to 1950, and the change in the ratio of reported polio cases to total population from 1955 (mostly a pre-vaccine year) to 1956 (a fully post-vaccine year). The opinion index includes the share of below-median-income parents who agree or strongly agree (measured separately) with the following statements from the 1963 Survey of Health Services Utilization and Expenditure: medicine can cure any illness; even if a person feels good, he/she should get an annual physical exam; it is important to choose your doctor; if a doctor said I needed a major operation I would have it done immediately; the care I have received from doctors has been excellent; medicine is a man's highest calling. It also includes the negative of the share who agree or strongly agree with these statements: I'll avoid seeing a doctor whenever possible; home remedies are better; doctors are primarily interested in income; I wouldn't go to a hospital unless there was just no other way to take care of me; most people can recover without medical aid; health mainly depends on will power. The household quality index includes the following outcomes among children ages 10 and under from the 5% extract of the 1960 Census: dwelling has own kitchen, hot water, shower/bath, toilet, public sewer system, phone, washing machine, dryer, freezer, air conditioner, full plumbing; dwelling has more than one room; dwelling was built within the last 30 years; dwelling is in sound condition; dwelling is not in dilapidated condition; it also includes the number of cars owned.

Table A2.2. Instrumental Variables Estimates of Medicaid’s Effect by Adult Income

	(1) Ambulatory Difficulty	(2) Disability Transfer Receipt	(3) Annual Employment
<i>A. White Estimates for Early Medicaid Eligibility</i>			
Adult Income:			
0-\$39,999	-5.12 [1.45] (0.02)	-5.43 [1.38] (0.01)	6.83 [1.43] (0.00)
\$40,000-\$99,999	-1.98 [0.42] (0.00)	-2.80 [1.07] (0.01)	2.00 [0.70] (0.02)
\$100,000+	-0.91 [0.59] (0.18)	-1.13 [0.67] (0.17)	0.74 [0.47] (0.19)
Ages	0-11	0-11	0-11
<i>B. Nonwhite Estimates for Early Medicaid Eligibility</i>			
Adult Income:			
0-\$39,999	-6.94 [2.96] (0.00)	-4.35 [1.23] (0.01)	3.25 [1.55] (0.18)
\$40,000+	-1.96 [0.80] (0.03)	-0.45 [1.18] (0.81)	2.43 [1.05] (0.01)
Ages	0-5	0-11	0-11

Notes: The table presents IV estimates of the effect of early Medicaid eligibility on samples stratified by adult income. Figure 7 shows no effect of Medicaid on the probability of having high levels of family income. Low intergenerational mobility suggests that the probability of childhood Medicaid eligibility is much higher for lower-income adults than for higher-income adults. This suggests a dose-response exercise using adult income as a proxy for childhood Medicaid eligibility. For the white sample, I split respondents into three groups of family income: \$0-\$39,999; \$40,000-\$99,999; \$100,000+. The results are largest for lower-income respondents, smaller but statistically significant for middle-income respondents, and much smaller and less precise for high-income respondents. For nonwhite respondents I have fewer observations especially at the highest incomes. I only split this sample in to two groups at \$40,000 of family income. The results are generally larger for the lower-income adults but the differences are not as large because childhood Medicaid exposure is likely much more similar.

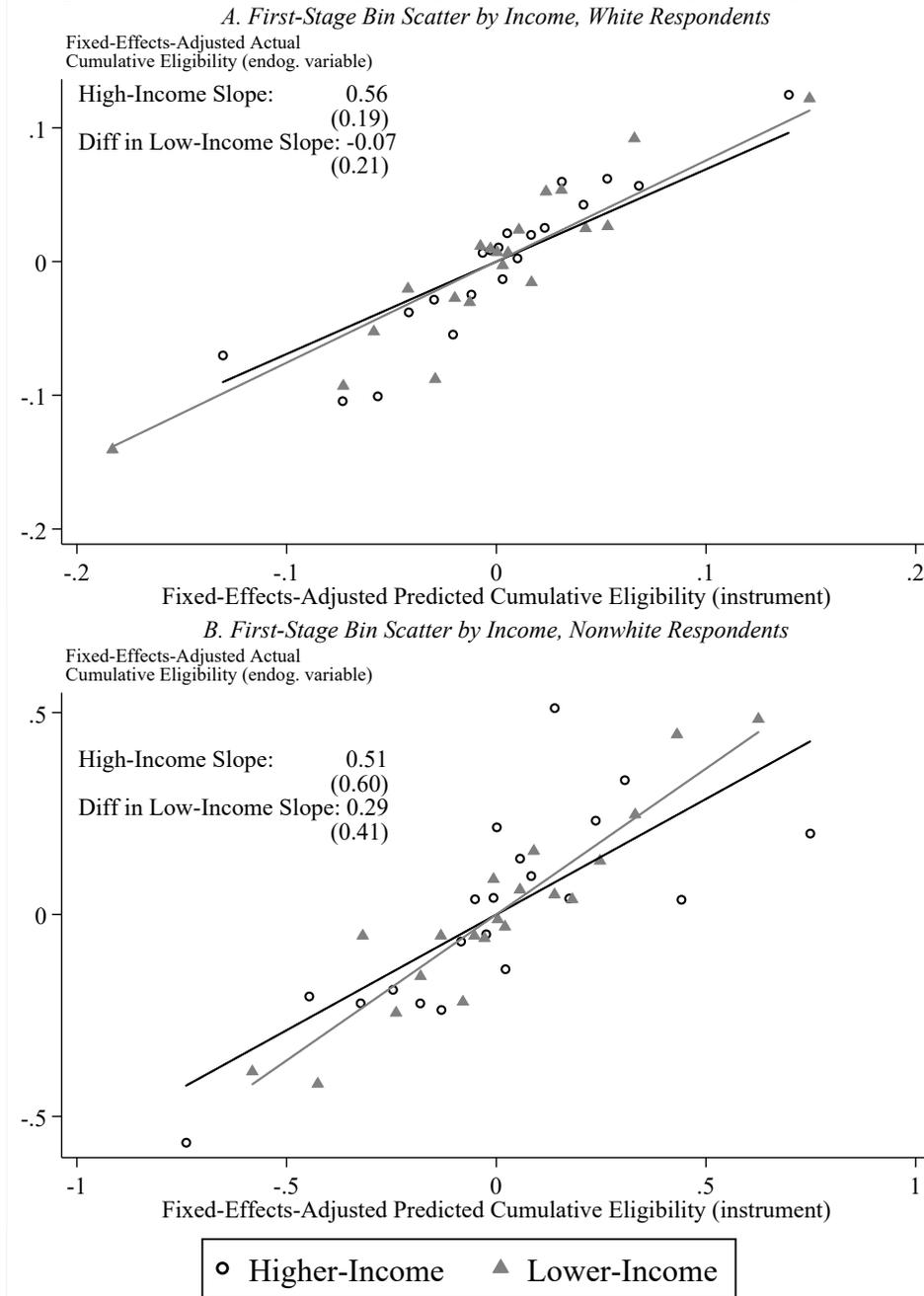
Table A2.3. Instrumental Variables Estimates of Medicaid’s Effect for Foreign-Born Adults

	(1) Ambulatory Difficulty	(2) Disability Transfers	(3) Employment
<i>A. White</i>			
False Early Medicaid Eligibility	1.66 [2.65]	1.48 [2.18]	-4.44 [4.25]
<i>B. Nonwhite</i>			
False Early Medicaid Eligibility	-1.1 [2.73]	-0.65 [1.02]	2.08 [1.54]

Notes: The table presents IV estimates of the effect of early Medicaid eligibility on samples of foreign-born adults in the 2000-2017 Census/ACS who arrived in the use at age 12 or later. This ensures that these respondents had no childhood exposure to Medicaid. I assign them to false childhood states based on their state of residence in the Census. To ensure adequate sample sizes I do not split the foreign-born sample by reported race. I use the same outcome variables calculated for all foreign-born respondents in both the white and nonwhite regressions.

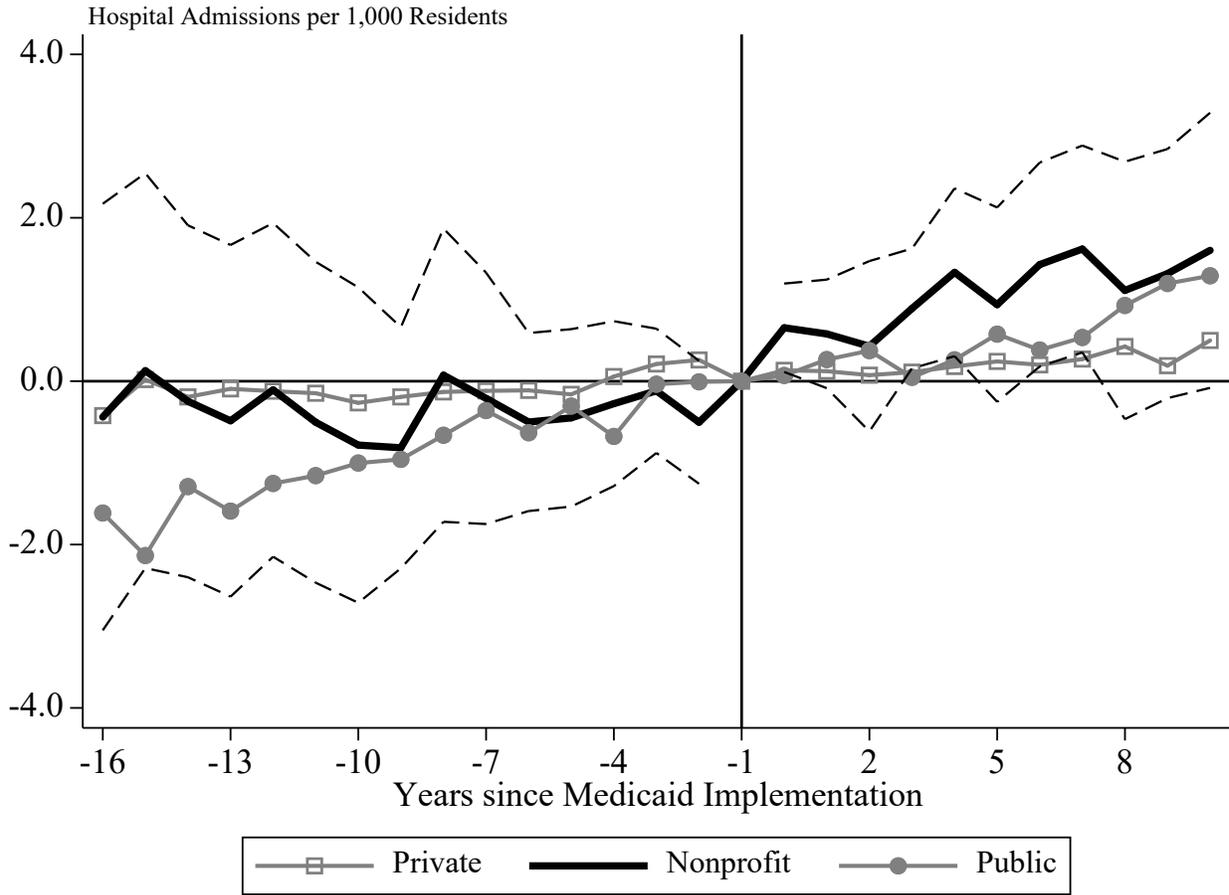
APPENDIX 3. ADDITIONAL FIRST-STAGE EVIDENCE

Figure A3.1. The First-Stage Relationship between Predicted and Actual Migration-Adjusted Cumulative Eligibility Does not Differ by Family Income



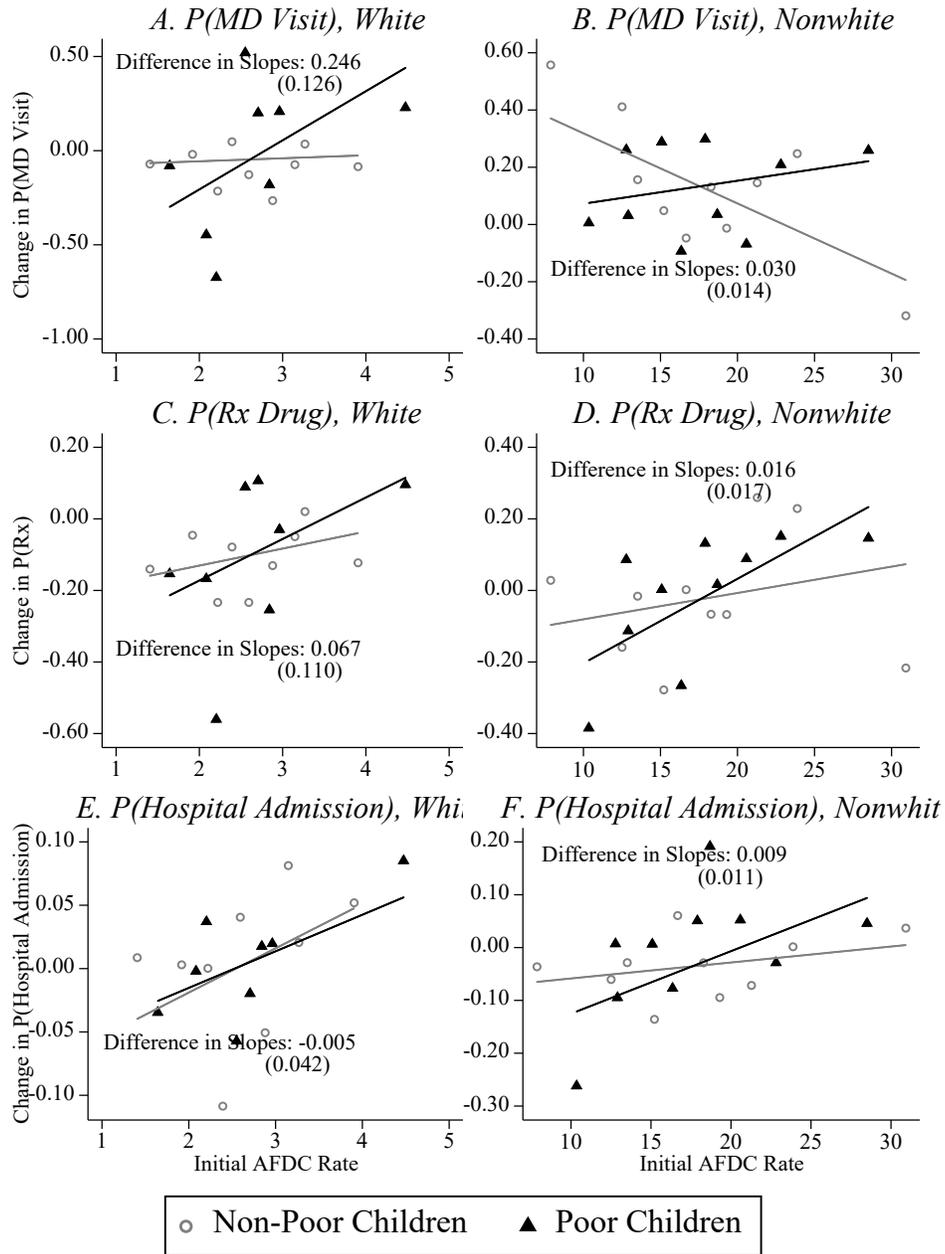
Notes: This figure uses 6,315 respondents born between 1953 and 1968 from the Panel Study of Income Dynamics (PSID) to calculate cumulative Medicaid eligibility. Lower-income children are those whose families made \$6,000 (about twice the poverty threshold for a family of 4) or less in 1968. This picks out the bottom quartile of weighted PSID families but half the unweighted sample. I average each respondent's cumulative eligibility (based on their actual moves) by income, race, birth year, and childhood state (from the question "where did you grow up?" [V311]), adjust for the fixed effects, and present bin-scatters of the residuals. The relationship is does not differ strongly by income which shows that lower- and higher-income children did not differentially move between higher- and lower-AFDC states. This suggests that using cohort-level migration to create my preferred cumulative eligibility measure does not mismeasure cumulative eligibility among lower-income children likely to have used Medicaid.

Figure A3.2. Relationship Between Initial AFDC Rates and Changes in Hospital Admissions by Ownership Status



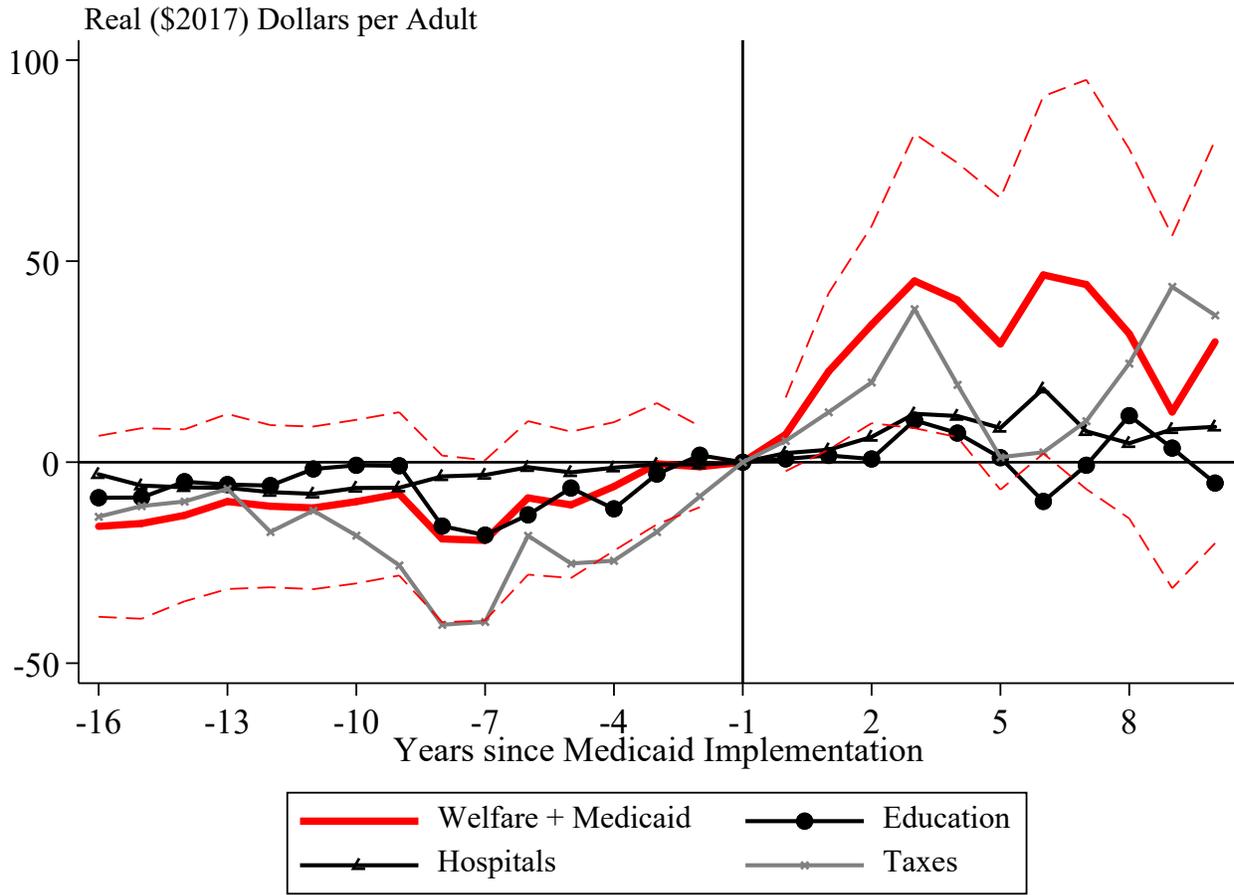
Notes: The figure plots event-study estimates from equation (2) using annual hospital admissions per 1,000 total residents by hospital ownership type as the outcome variable. These data were shared by Amy Finkelstein and used in (Finkelstein 2007). Because hospital admission are not recorded by race, I use the overall child AFDC. I find post-Medicaid increases in admissions for nonprofit hospitals, but not public hospitals (who already saw many poor patients) or for profit hospitals. The magnitude of the increase in nonprofit admissions is 1.5 admissions per 1,000 for each percentage point difference in initial AFDC rates.

Figure A3.3. Relationship Between Initial AFDC Rates and Changes in Health Care Use for Children, 1963-1970



Notes: This figure uses data on 4,873 children ages 0-11 from the 1963 and 1970 Surveys of Health Services Utilization and Expenditure. These data were used in Finkelstein and McKnight (2008). I obtained geographic codes for this survey directly from the National Opinion Research Council in 2010 and corrected an error in the ICPSR dictionary that read in the wrong survey weights. See Appendix A.3 in Bailey and Goodman-Bacon (2015). I calculate the 1963-1970 change in average outcomes by state and poverty status. I then estimate the relationship between changes in utilization and initial AFDC rates by race and poverty status controlling for average income, the presence of a CHC, and region fixed effects (all entered separately for poor and non-poor samples). The results show that these rough measures of utilization are more positively related to initial AFDC rates for poor than non-poor children.

Figure A3.4. Relationship Between Initial AFDC Rates and Budget Outcomes



Notes: The figure uses data on state budgets from the Census Bureau’s Annual Survey of State Governments (U.S. Census Bureau Various Years). I construct real (\$2017) budget items per adult by state and year from 1942-1980. This figure shows first-stage evidence that AFDC rates are correlated with spending on a broad welfare category that includes Medicaid (red solid line). There is not strong evidence of large increase in spending on education or hospitals (other items that could potentially explain the cross-cohort/cross-state adult outcomes). I also do not find systematic tax increases (gray line).

Table A3.1. Cross-Sectional Differences in Health Care Utilization by Medicaid Eligibility or Coverage

<i>1963-1965</i>	<i>1968-1969</i>	<i>1968-1969</i>	<i>1970-1976</i>	<i>1975</i>	<i>1976</i>	<i>1980</i>
Income < ~3k	Categorically Eligible			Medicaid Recipients		
~48%	52.50%	80%	70% (36% OPD)	81%	84%	85%
	Low-Income in Non-Medicaid State			Non-Medicaid Recipients		
	36%	68%		67%	72%	75%
NHES Cycle II, SHSUE, NHIS	Loewenstein (1971)	OEO 11 City Survey	DHEW Tables	Survey of Access to Medical Care	NHIS	NHIS

Notes: The table shows shares of children aged 0-18 with doctor visits in the previous year. This is merely suggestive of an effect of Medicaid on health care use because families with children who use a lot of medical care may be more likely to take up Medicaid (ie. medically needy).

Column 1 uses responses from three surveys conducted prior to Medicaid finding that just under half of low-income children saw doctors. This is reproduced from Figure 1.

Column 2 reports tabulations from a survey conducted in 1968 and 1969 by Regina Loewenstein entitled “Effect of Medicaid on Health Care of Low Income Persons.” The first row reports the share respondents in Medicaid states who are actually categorically eligible who saw a doctor. The second row reports the same share for low-income children living in states that had not yet implemented Medicaid.

Column 3 reports the share of Medicaid recipient and non-recipient children who saw a doctor in the last year in a survey of 11 Community Health Center catchment areas conducted by the Office of Economic Opportunity in 1968 and 1969 (see Bailey and Goodman-Bacon 2015).

Column 4 reports the share of child Medicaid recipients who had claims for physicians services or outpatient department visits from a series of annual tables based on aggregate data reported by states.

Column 5 reports the share of Medicaid recipient and non-recipient children who saw a doctor in the last year in the 1975 Survey of Access to Medical Care, a follow-up to the 1963 and 1970 Surveys of Health Services Utilization and Expenditure.

Columns 6 and 7 report the share of Medicaid recipient and non-recipient children who saw a doctor in the last year in two later waves of the National Health Interview Survey.

Sources: Andersen and Aday (2013), Center for Health Administration Studies and National Opinion Research Center (1984), United States Department of and Human Services. National Center for Health (1991), Loewenstein (1971), Minnesota Population Center and State Health Access Data Assistance Center (2012), U.S. Office of Economic Opportunity (2010).

Table A3.2 First-Stage Results without Migration Adjustment

	(1)	(2)	(3)	(4)
	<i>Cumulative Eligibility, Ages 0-18</i>	<i>Cumulative Eligibility, Ages 0-5</i>	<i>Cumulative Eligibility, Ages 6-11</i>	<i>Cumulative Eligibility, Ages 12-18</i>
<i>A. White</i>				
Predicted Eligibility at:				
Ages 0-18	0.79 [0.21]			
Ages 0-5		0.92 [0.22]	-0.18 [0.2]	0.11 [0.17]
Ages 6-11		-0.04 [0.06]	0.93 [0.13]	-0.23 [0.24]
Ages 12-18		-0.01 [0.03]	-0.02 [0.07]	0.90 [0.13]
Mean Eligibility Any	0.70	0.24	0.28	0.37
<i>F</i> -statistic	14.0			
Angrist/Pischke <i>F</i> -statistic		49.4	145.5	32.5
<i>B. Nonwhite</i>				
Ages 0-18	0.43 [0.19]			
Ages 0-5		0.75 [0.17]	-0.47 [0.18]	-0.36 [0.14]
Ages 6-11		-0.01 [0.05]	0.90 [0.12]	-0.38 [0.21]
Ages 12-18		-0.01 [0.02]	-0.01 [0.05]	0.85 [0.13]
Mean Eligibility Any	3.75	1.44	1.51	1.77
<i>F</i> -statistic	5.2			
Angrist/Pischke <i>F</i> -statistic		95.7	36.5	21.4

APPENDIX 4. EVIDENCE SUPPORTING THE REGRESSION SPECIFICATION

Because identification relies on comparing cross-cohort changes in high- versus low-AFDC states, non-linearity in age-specific mortality rates is not automatically a problem. Assume that for cohort c from state s the probability of dying at age a conditional on surviving to 1980 is:

$$f(a; s, c) = \exp\{\alpha_{sc}\} \cdot \exp\{g_s(y - c)\}$$

Where α_{sc} is a state-by-cohort component and $g_s(y - c)$ is an arbitrary state-specific mortality age profile (where age, $a \equiv y - c$).²

The probability of dying between 1980 and 2016 is:

$$MR_{sc} = \exp\{\alpha_{sc}\} \sum_{1980}^{2016} \exp\{g_s(y - c)\}$$

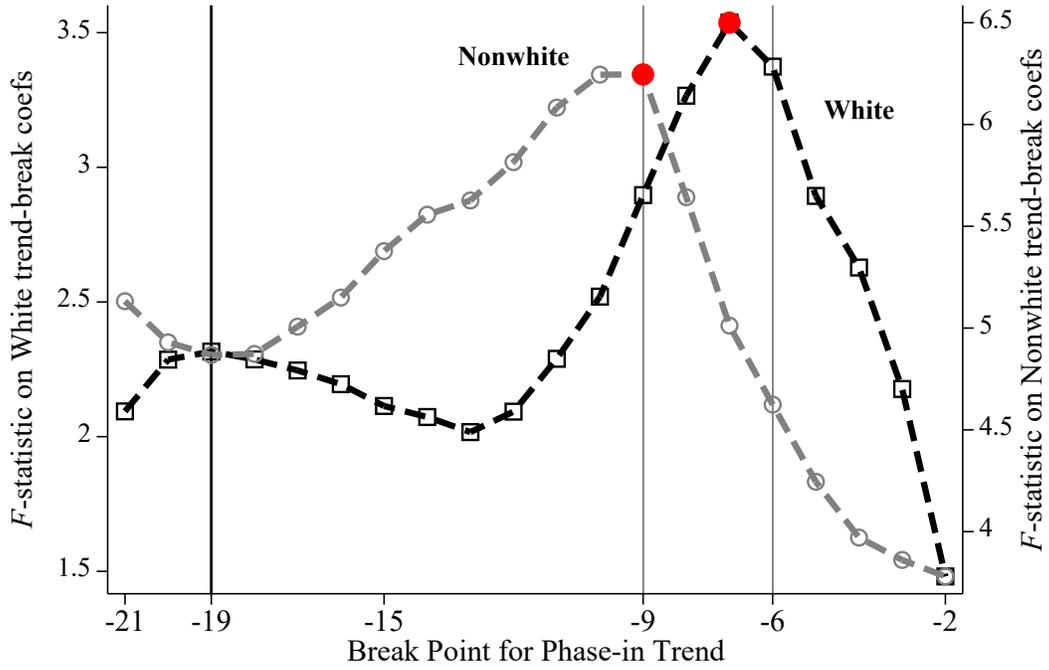
My mortality outcome is the log of MR_{sc} :

$$\ln(MR_{sc}) = \alpha_{sc} + \ln \left(\overbrace{\sum_{1980}^{2016} \exp\{g_s(y - c)\}}^{G_{sc} \equiv \text{age profile}} \right)$$

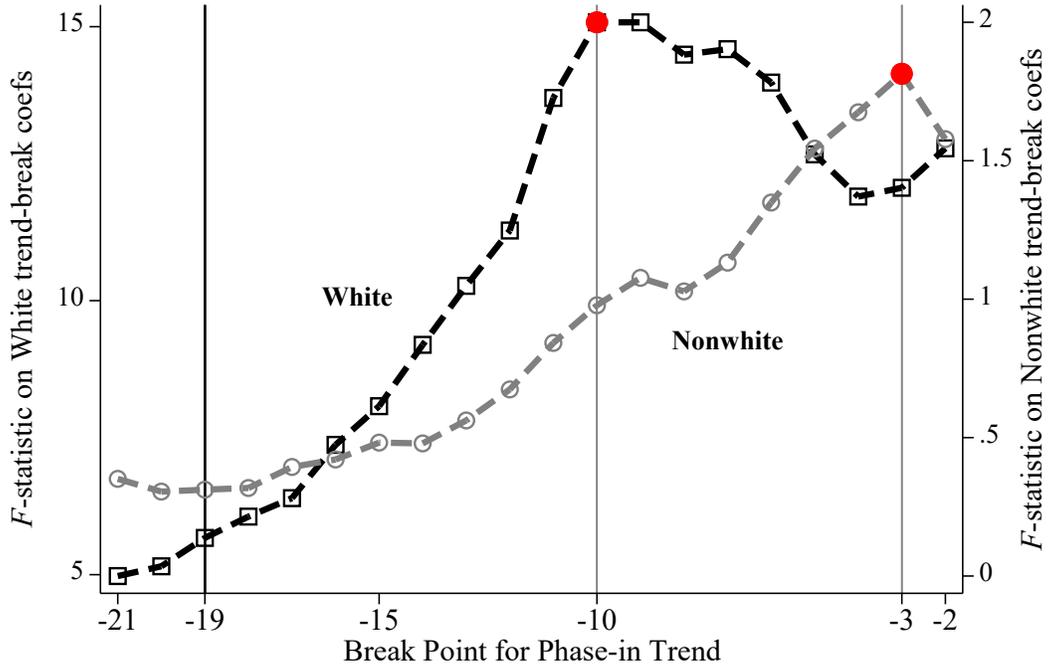
I assume that cohort and Medicaid exposure only affect the intercept, α_{sc} . This may be confounded by unobservables, but by definition it is not confounded by the age pattern of mortality rates across states, G_{sc} . I check for bias from G_{sc} in several ways using older Censuses and stratifying by adult circumstances.

² Chetty et al. (2016) show a log-linear relationship between period mortality rates and age. This does not yield a log-linear relationship between cumulative mortality and age ranges. The difference-in-differences analysis is valid if the state/cohort component multiplies an age (range) profile that is uncorrelated with AFDC rates. Appendix Figure A2.3. supports this claim using earlier Censuses. Appendix Figures A6.4 and A6.5 show that trend-breaks in mortality for cohorts with early Medicaid exposure are also clear for 25- and 30-year mortality rates. Changing the age range does not change the results, which is not consistent with bias from differential age profiles.

Figure A4.1. Trend-Break F -Statistics, Mortality and Ambulatory Difficulty
A. Mortality

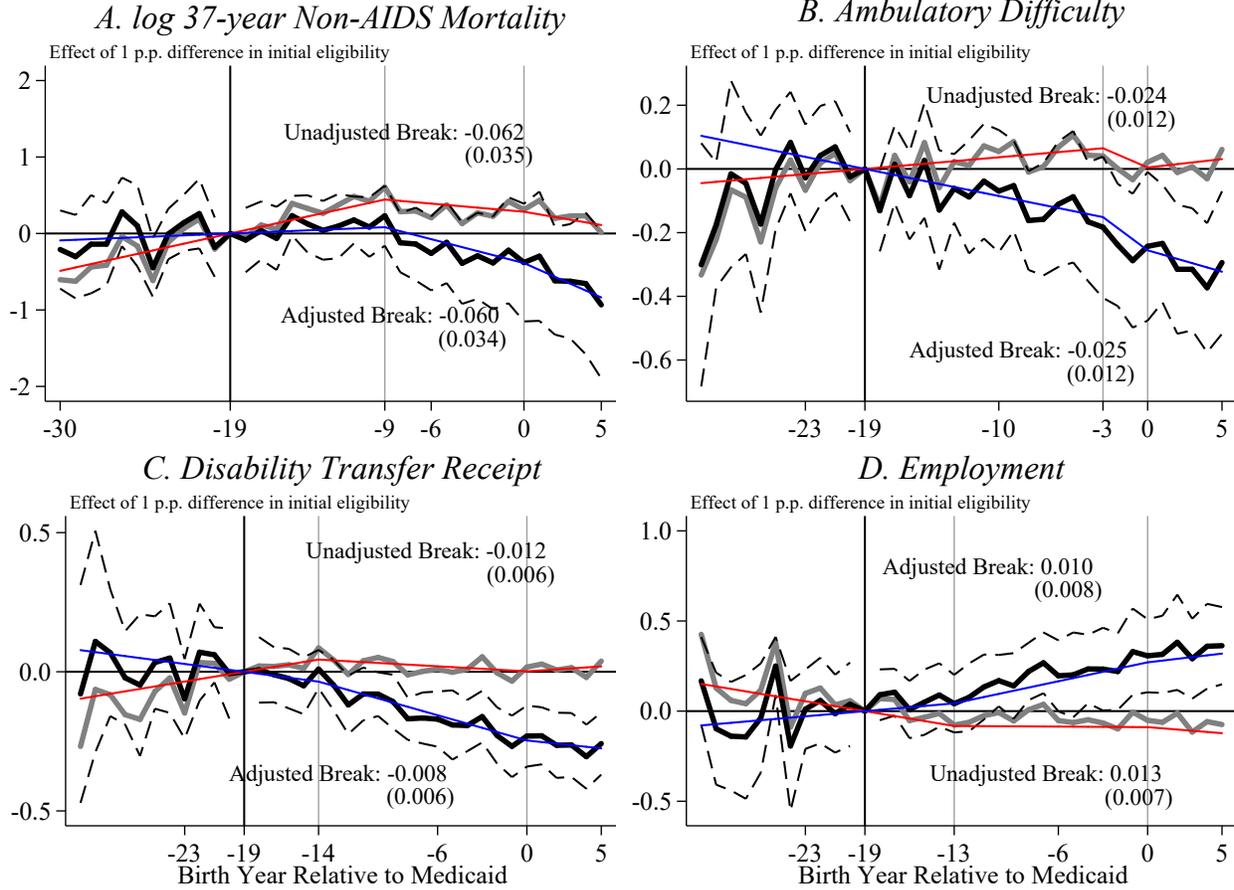


B. Disability



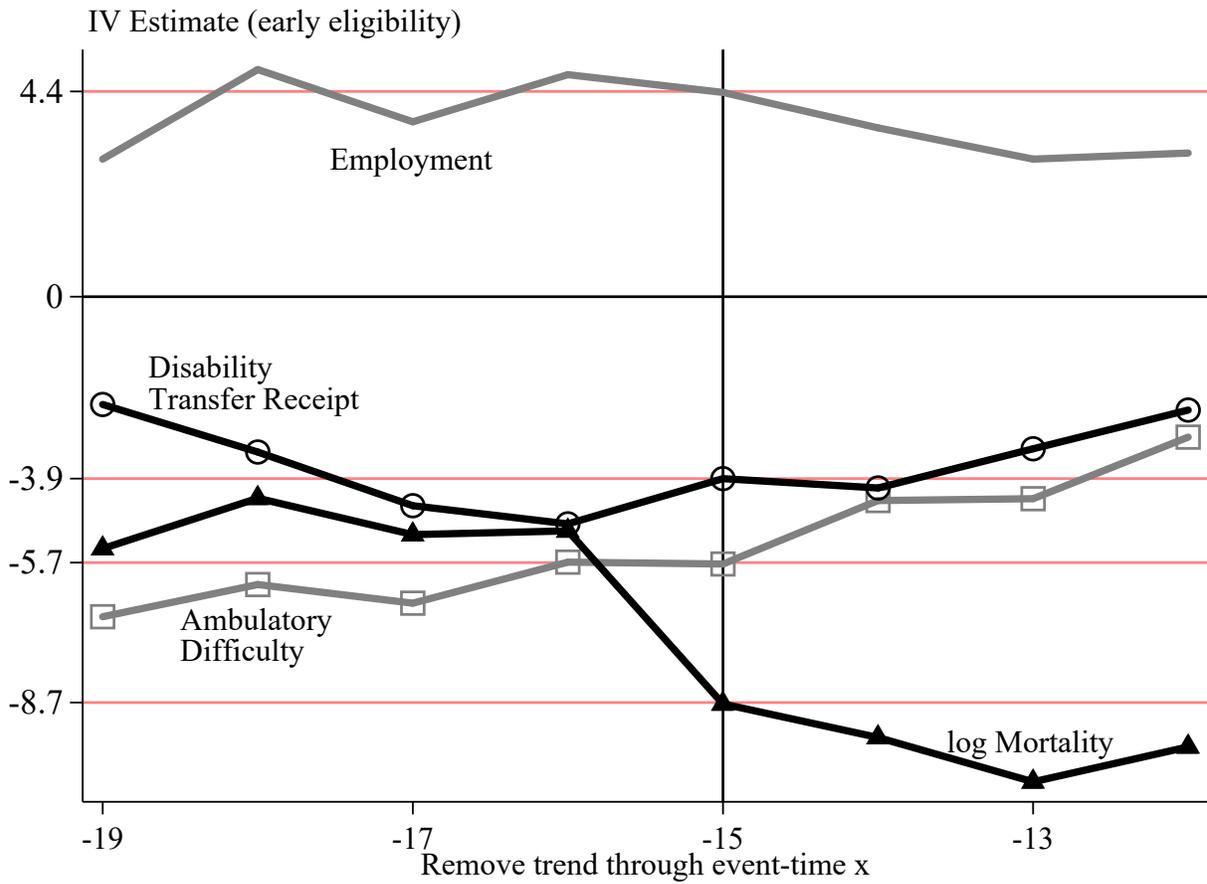
Notes: F -statistics are from the joint significance test of the event-cohort variable, its interaction with a dummy for event-cohorts greater than or equal to x (where x is given by the x -axis in the figure) and its interaction with a dummy for event-cohorts greater than or equal to zero.

Figure A4.2. Nonwhite Results with and without Detrending



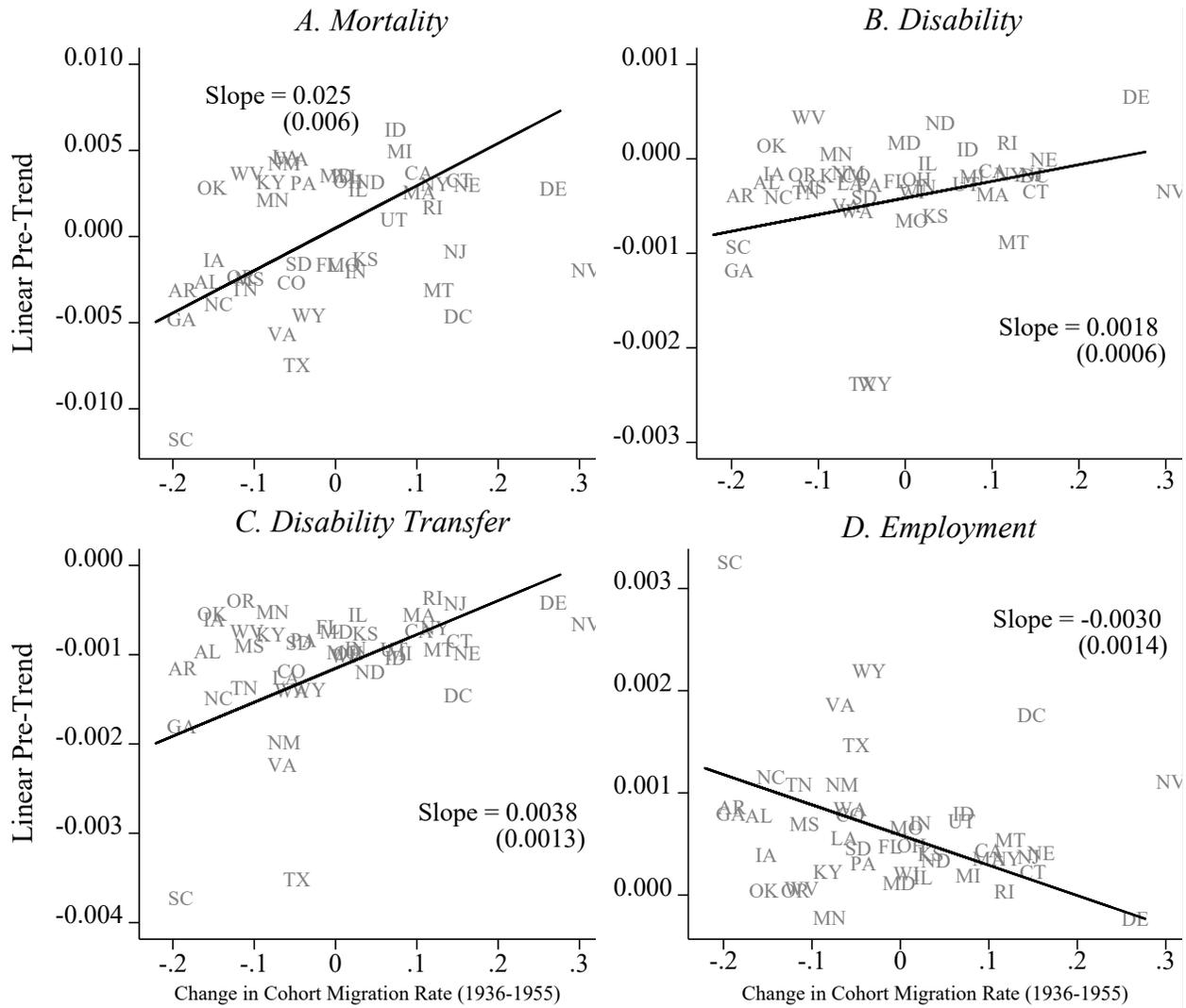
Notes: The figure shows nonwhite event-study estimates with (black) and without (gray) removing linear pre-trends. The detrending procedure is to keep data on all event-times -15 and earlier, regress the outcome on all fixed effects and covariates as well as the interaction of initial AFDC rates, linear event-time, and dummies for the Medicaid year. The detrended outcome variable equals the residuals from this regression. In this figure I show event-times back to -30 to highlight the trend that is being removed. The red lines reflect the linear spline specification on the unadjusted data, and I report the estimated “phase-in trend-break”. The blue lines reflect the linear spline specification on the adjusted data. Adjusting for *one* pretrend-- $AFDC_s^* \times (c - t_s^*)$ —leaves the unadjusted trend-breaks completely unchanged. This figure shows that the slightly more flexible specification that interacts these trends with Medicaid year dummies has very little effect on the trend breaks.

Figure A4.3. Nonwhite IV Estimates Using Different Detrending Windows



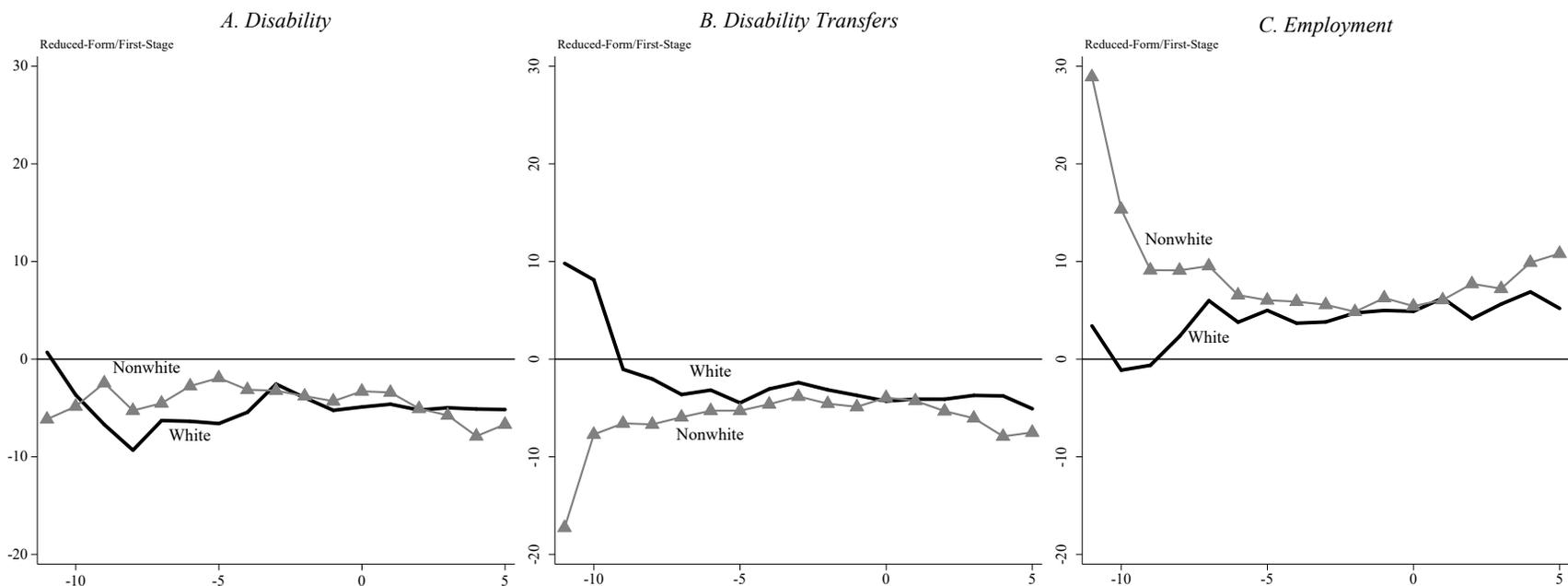
Notes: This figure shows that the nonwhite results are robust to many different ways to estimate and partial out pre-trends. The x-axis shows the upper limit of event-time over which I estimate the pre-trends. Each line represents the “early eligibility” IV estimate obtained from a specification that removes trends calculated over a different time frame. The results are not qualitatively different across a range of detrending choices, not does my preferred specification using event-time -15 pick out extreme estimates.

Figure A4.4 Correlation between Nonwhite Pre-Trends and Cohort Migration Rates



Notes: The figure plots the estimated state-specific pre-trends against the difference in the share of nonwhite cohort members who live outside their birth state for the 1955 versus the 1936 birth cohort. I report the weighted least squares slope and robust standard error. I drop ME, NH, and VT in the scatter plots because they have so few nonwhite cohort members. Including them does not change the conclusions.

Figure A4.5 Ratio of Reduced-Form to First-Stage Event-Study Coefficients



Notes: The figure plots the ratio of the reduced-form event-study coefficients from Figures 6 and 7 to the first-stage event-study coefficients from Figure 4. This does not exactly match the early eligibility IV estimates in the main text, which are generated by 2SLS with two instruments and two endogenous variables (early and late cumulative eligibility). Even so, the estimates are quite close. This also suggests that the long-run effect per year of eligibility is similar across early childhood. This could mean that heterogeneity in the effect at younger ages offset heterogeneity in the effect by total amount of coverage, or it could mean that the effect are roughly linear in cumulative exposure during early childhood.

Table A4.1. Sensitivity of Ambulatory Difficulty Estimates to Selective Survival

	(1) White	(2) Nonwhite
Assumed disability rate among survivors:		
0%	-4.24 [1.05]	-4.94 [2.10]
10%	-4.27 [1.05]	-5.11 [2.12]
20%	-4.30 [1.06]	-5.28 [2.14]
30%	-4.34 [1.06]	-5.44 [2.16]
40%	-4.37 [1.06]	-5.61 [2.18]
50%	-4.40 [1.07]	-5.78 [2.20]
60%	-4.44 [1.07]	-5.95 [2.22]
70%	-4.47 [1.08]	-6.11 [2.24]
80%	-4.50 [1.08]	-6.28 [2.26]
90%	-4.54 [1.08]	-6.45 [2.29]
100%	-4.57 [1.09]	-6.62 [2.31]

Notes: This table presents IV estimates for ambulatory difficulty for cohort members who would always have survived. Denote the disability rate among the share s of cohort members who would have survived without Medicaid by y_0 and the disability rate among those induced to survive by Medicaid by y_1 . Observed disability rates then equal $y = (1 - s)y_0 + sy_1$. I calculate s using the short-run mortality estimates from Goodman-Bacon (2018) and the longer-run mortality estimates from table 2. Then using assumptions about the disability rate among those induced to survive, shown in each row, I calculate an outcome that reflects disability rates among those who would always have survived: $y_0 = \frac{y - sy_1}{(1 - s)}$. This outcome is not affected by selective survival, which would bias the main estimates toward zero if survivors are less healthy. The results show that estimates are not very sensitive to even extreme assumptions about disability rates among those induced to survive by Medicaid.

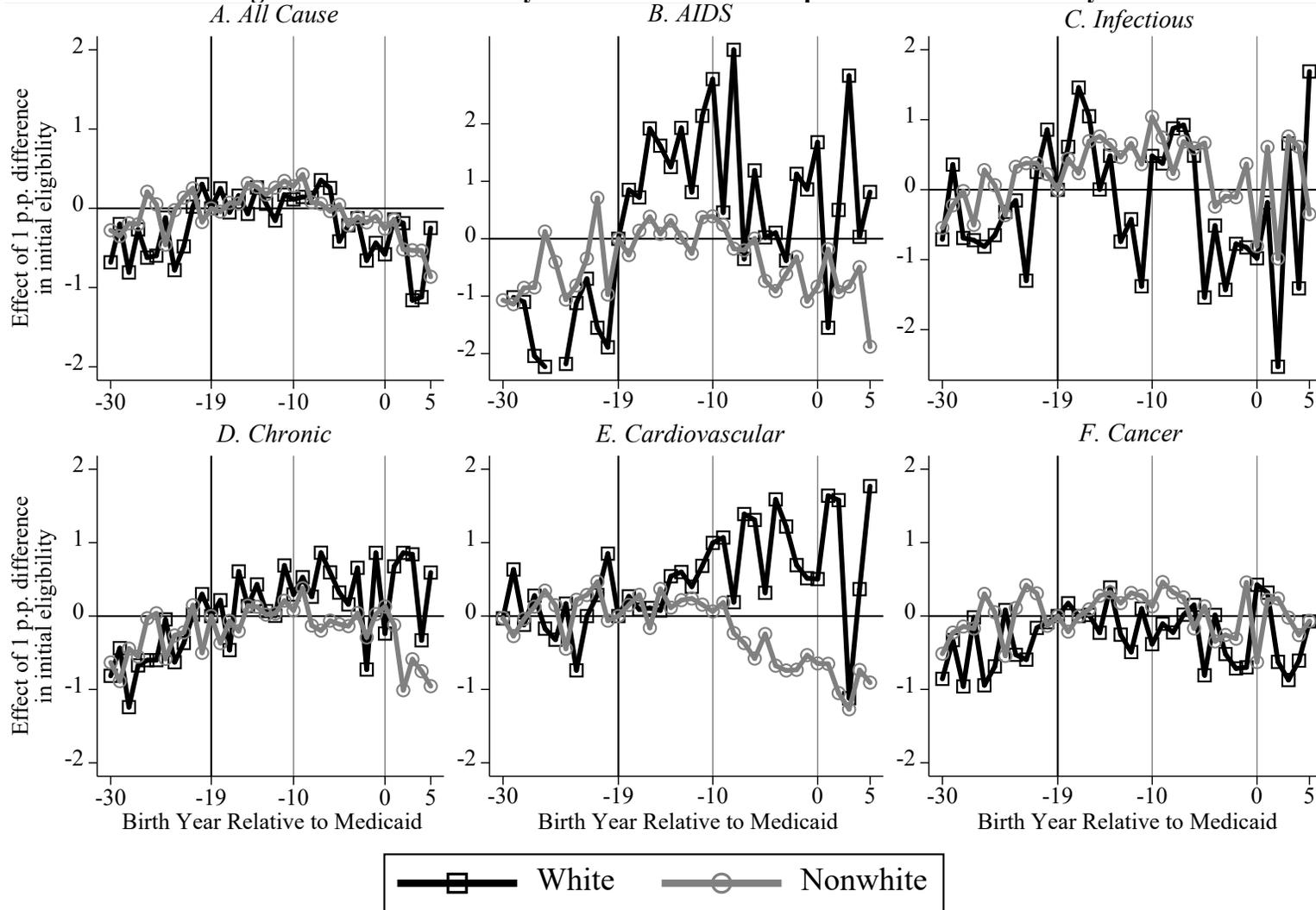
Table A4.2. IV Estimates Without Medicaid-Year-by-Cohort Fixed Effects

	(1)	(2)	(3)	(4)
	log Non- AIDS Adult Mortality	Ambulatory Difficulty	Disability Transfer Receipt	Annual Employment
<i>A. White Estimates for Early Eligibility</i>				
Early Medicaid Eligibility	-15.00	-4.28	-4.58	5.71
	[6.09]	[0.83]	[1.16]	[0.82]
Ages	0-5	0-11	0-11	0-11
<i>B. Nonwhite Estimates for Early Eligibility</i>				
Early Medicaid Eligibility	-9.01	-3.36	-2.44	4.93
	[4.95]	[1.56]	[1.65]	[2.16]
Ages	0-11	0-5	0-11	0-11

Notes: The table presents IV estimates for early Medicaid eligibility comparable to panels B and D of Table 2. This specification omits the Medicaid-Year-by-Cohort fixed effects.

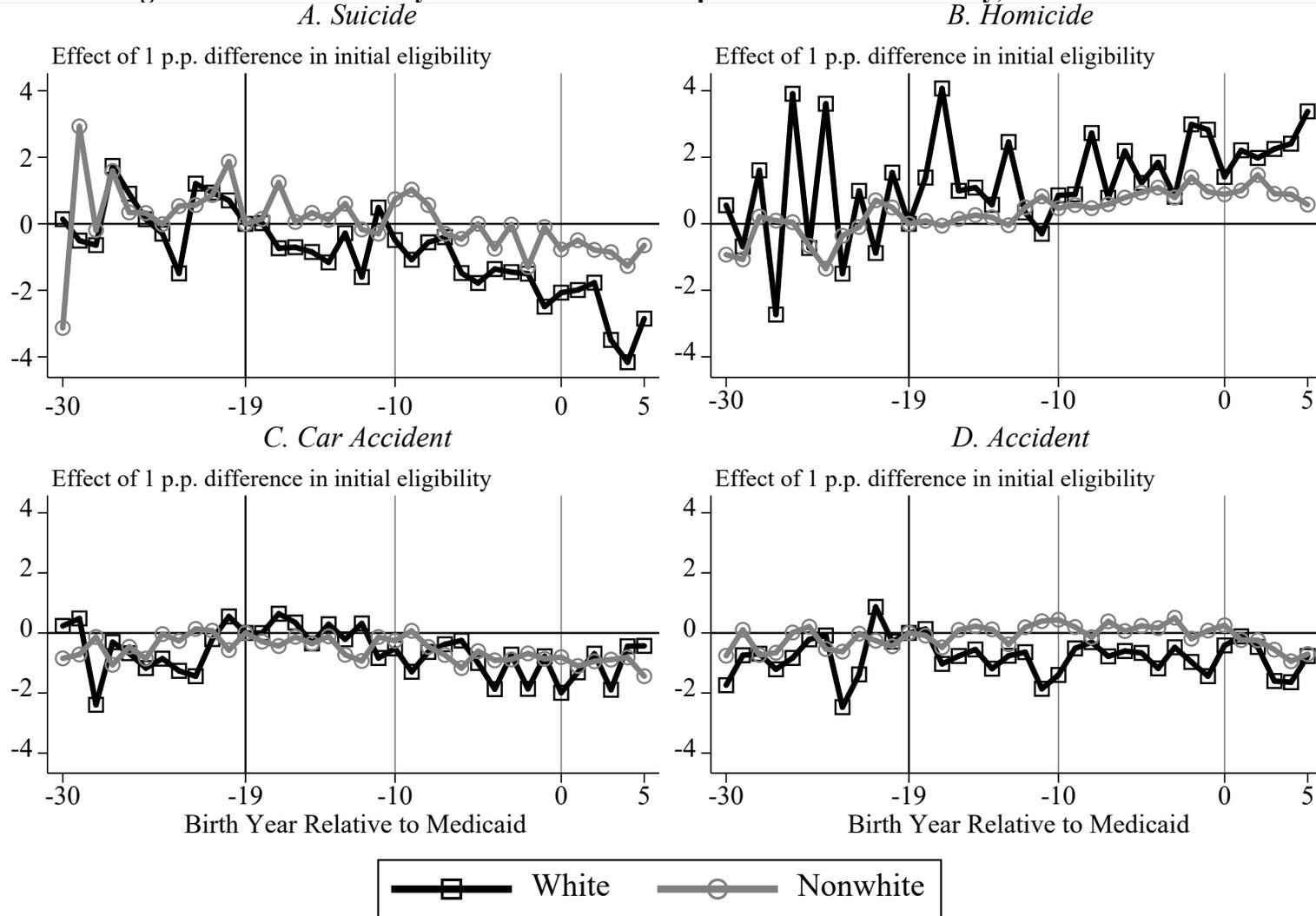
APPENDIX 5. ADDITIONAL EVIDENCE IN THE MAIN SAMPLES

Figure A5.1. Event-Study Estimates for Cause-Specific 37-Year Mortality



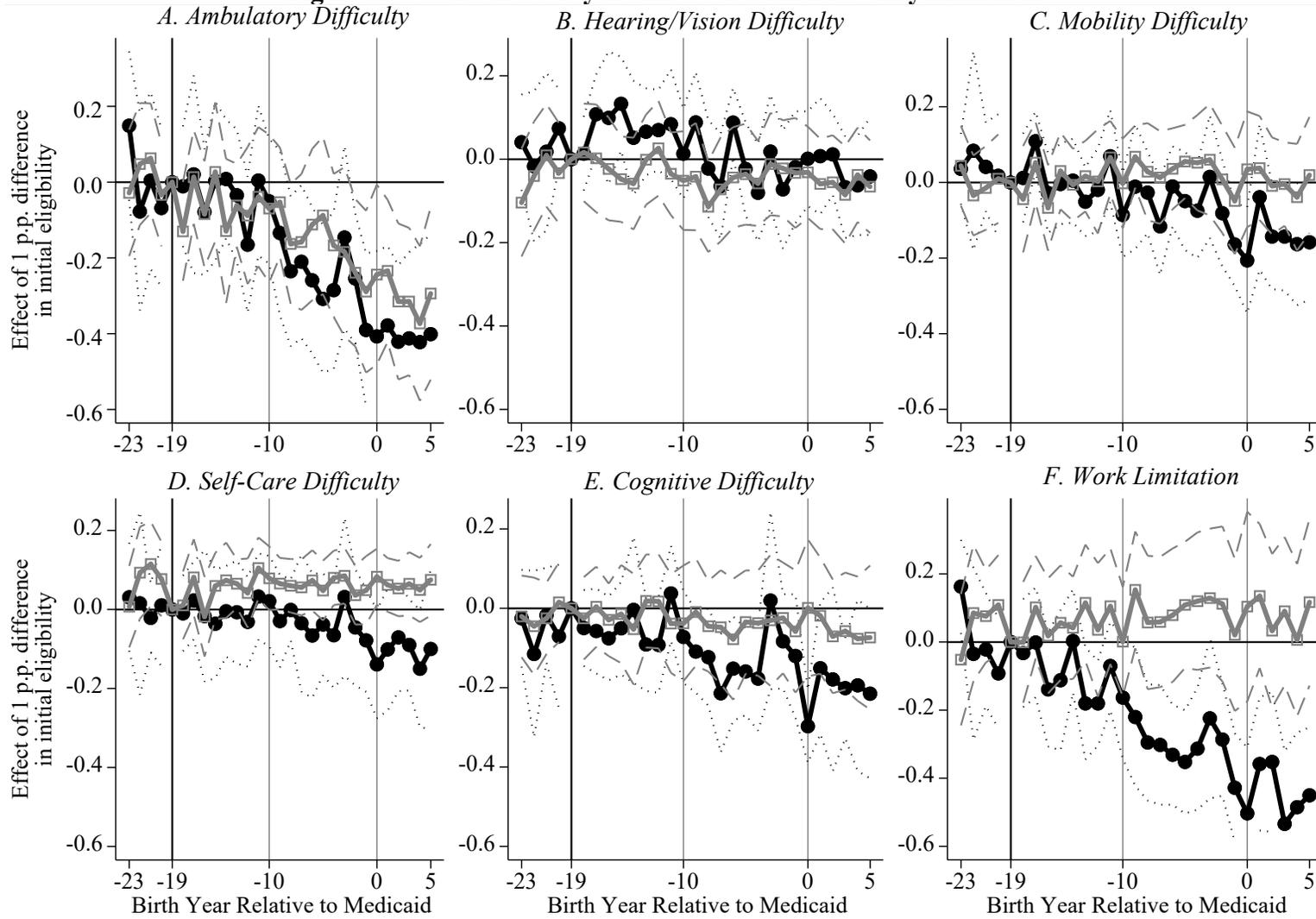
Notes: The specification corresponds to Figure 4 and Table 3.

Figure A5.2. Event-Study Estimates for Cause-Specific 37-Year Mortality, External Causes



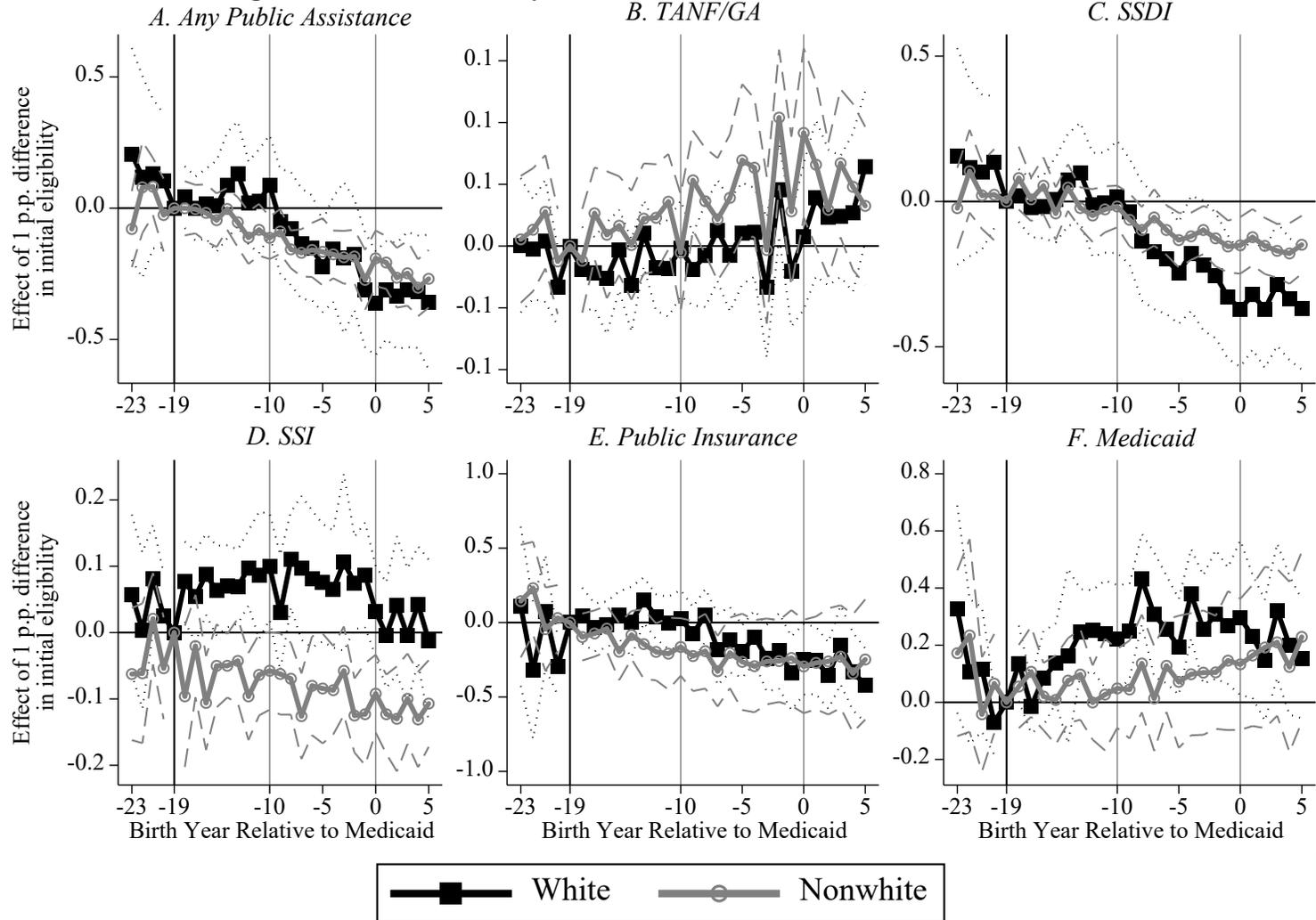
Notes: The specification corresponds to Figure 4 and Table 3.

Figure A5.3. Event-Study Estimates for All Disability Measures



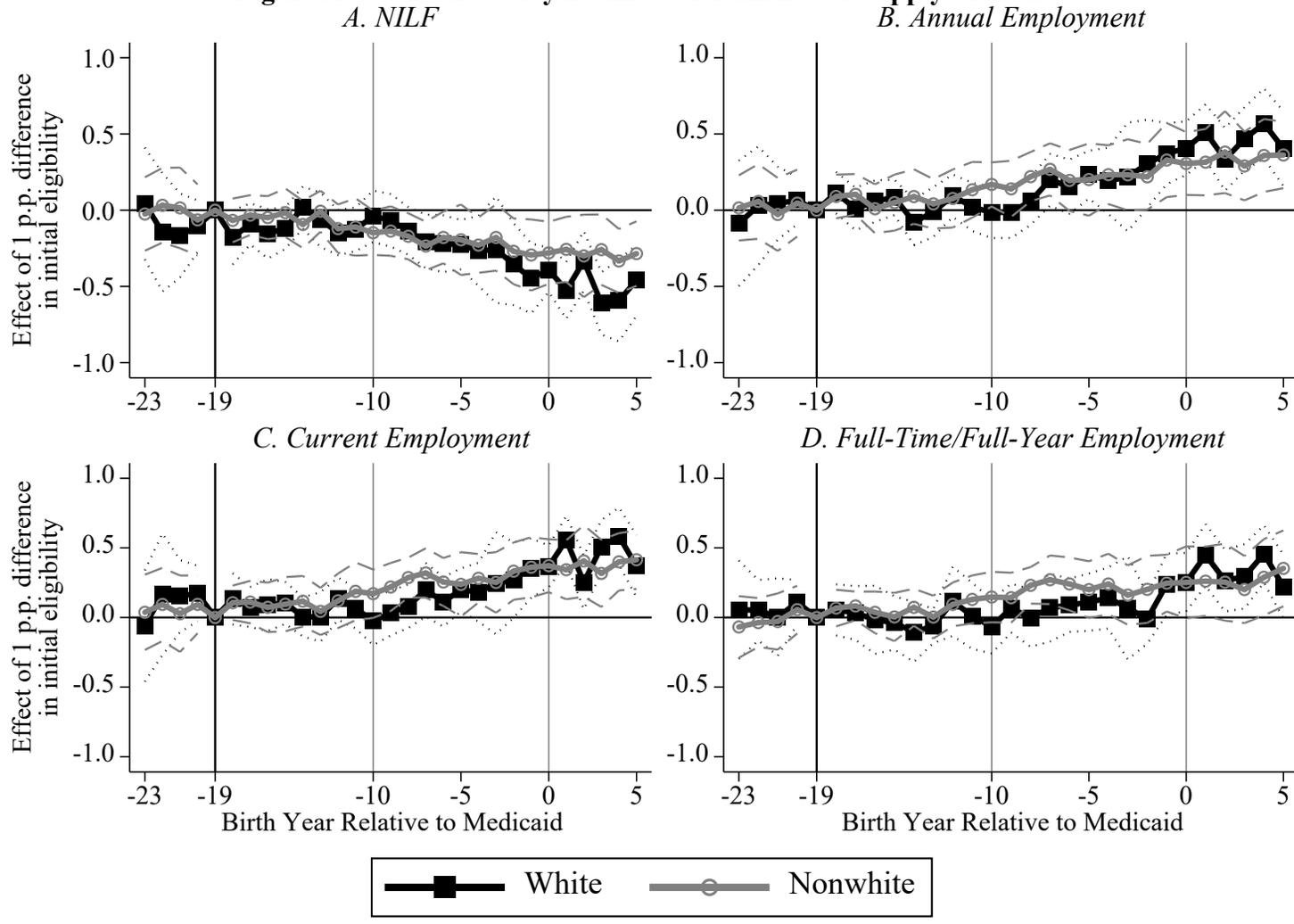
Notes: Estimates correspond to Figure 5 and Table 4.

Figure A5.4. Event-Study Estimates for All Public Assistance Measures



Notes: The estimates correspond to Figure 6 and Table 5. Effects on disability benefits are shown separately for Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI), and public insurance estimates are shown for both any public insurance and Medicaid.

Figure A5.5. Event-Study Estimates for All Labor Supply Measures

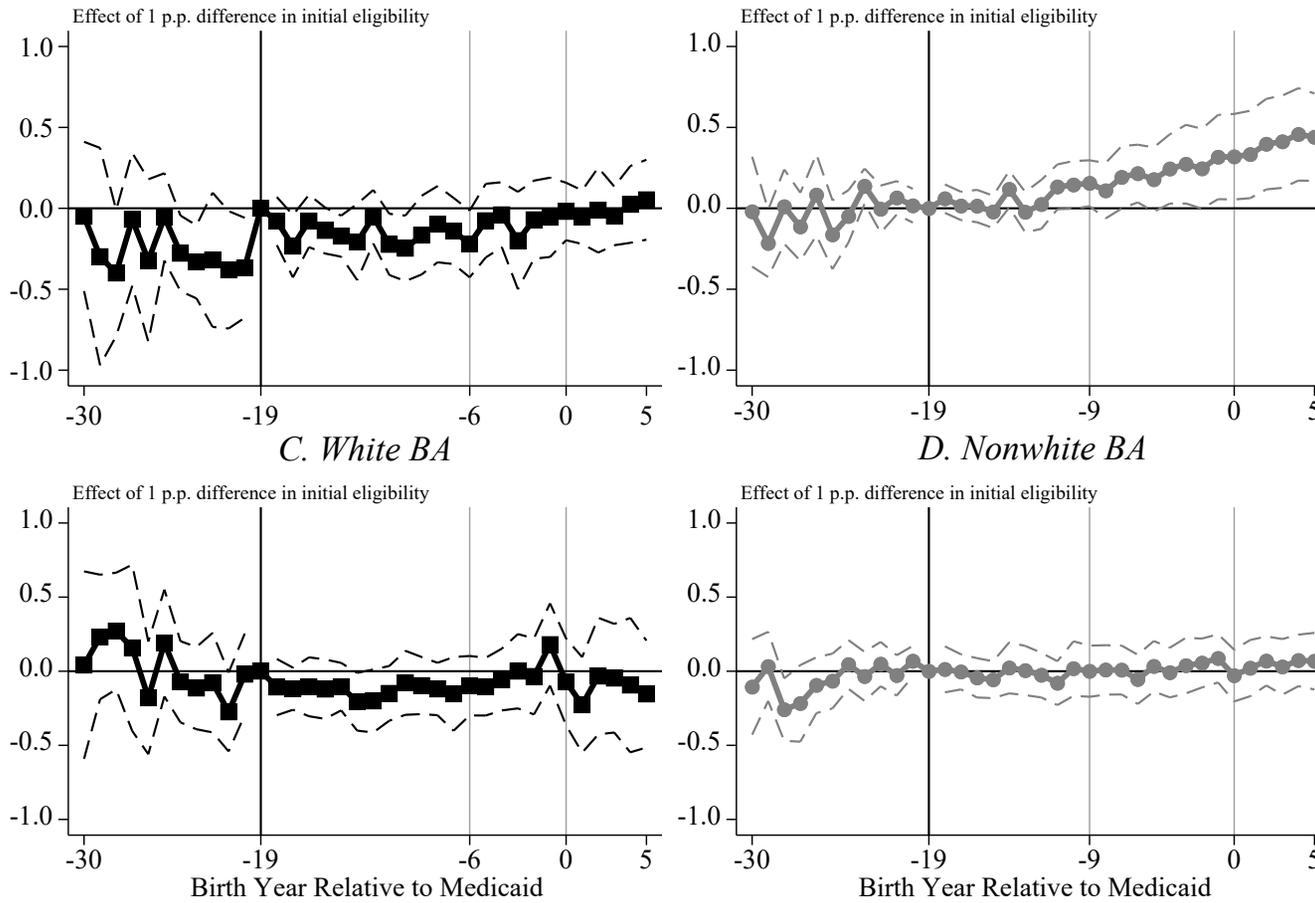


Notes: The estimates correspond to Figure 6 and Table 6.

Figure A5.6. Event-Study Estimates for Educational Attainment

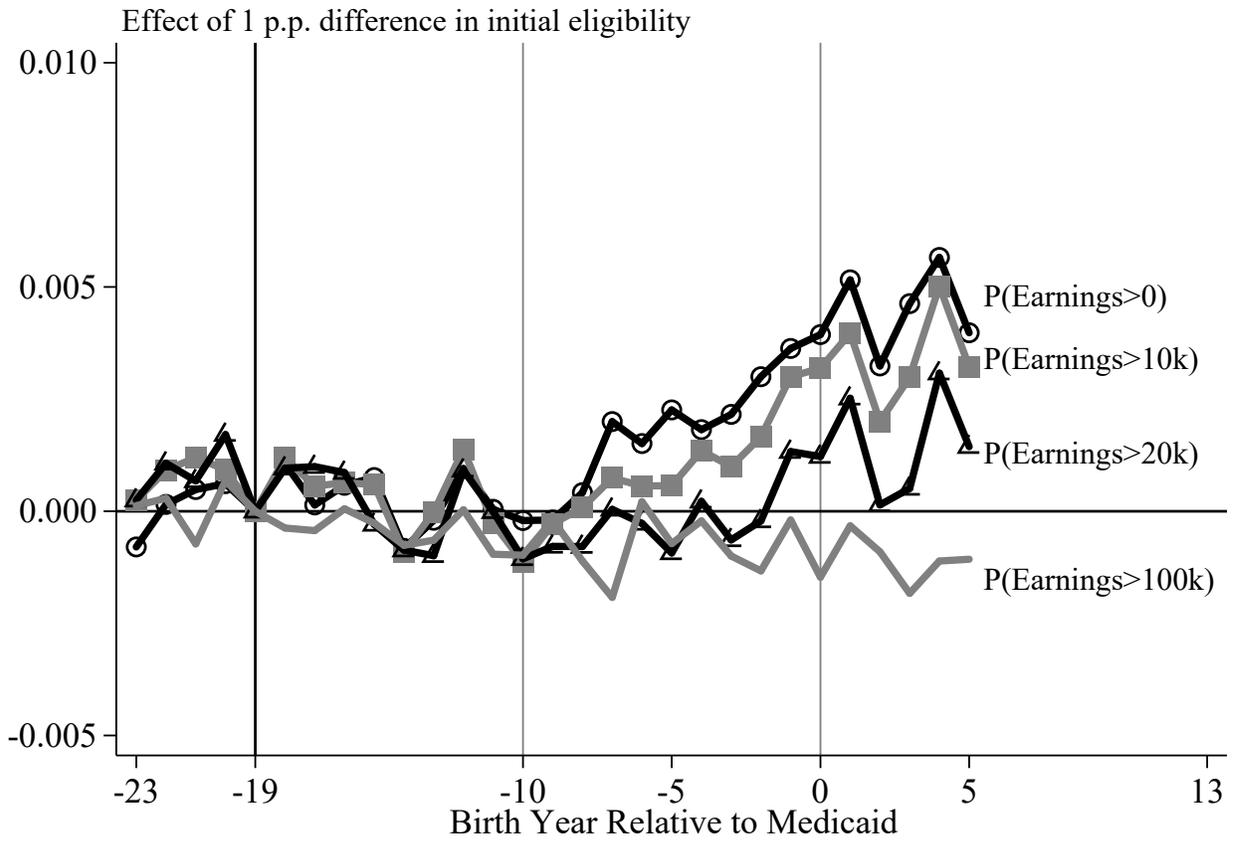
A. White HS Graduate

B. Nonwhite HS Graduate



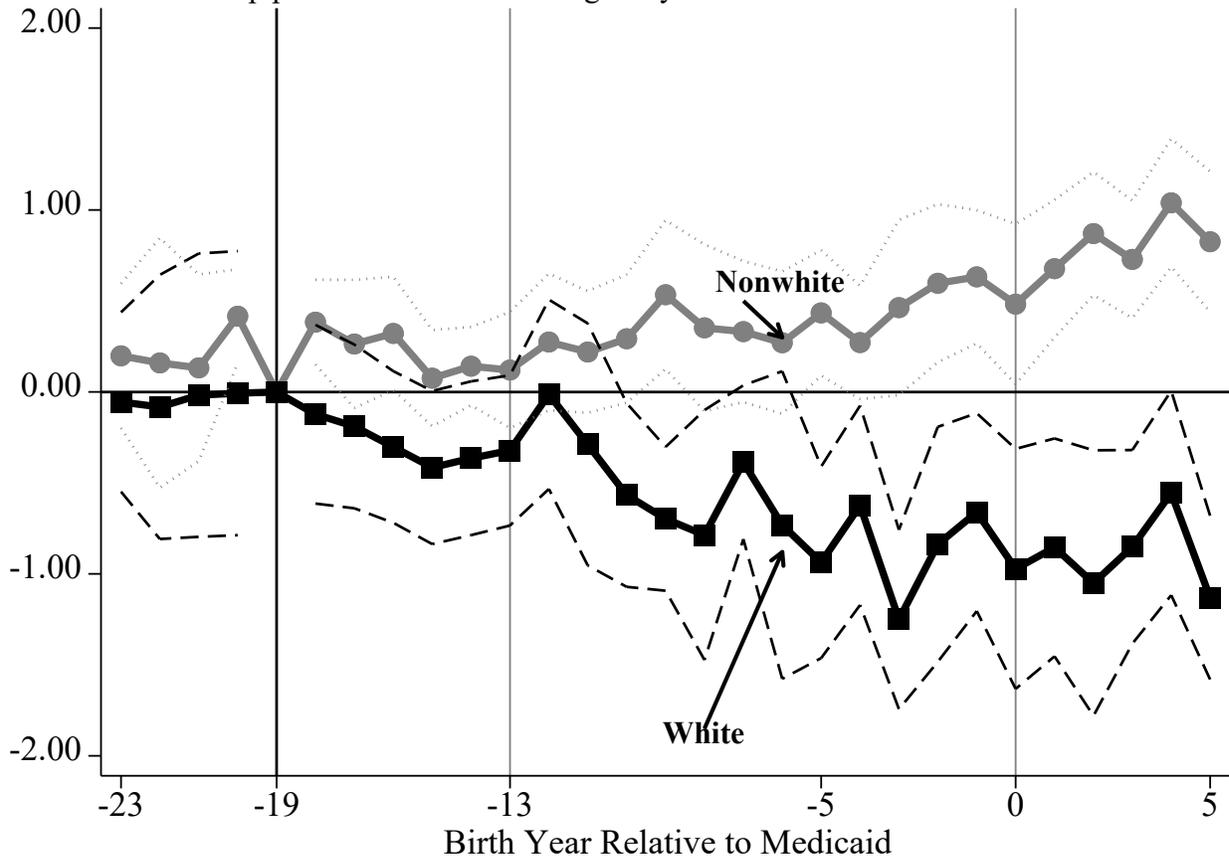
Notes: The estimates correspond to Table 7.

Figure A5.7. Event-Study Estimates for Selected Points in the Earnings Distribution, White Cohorts



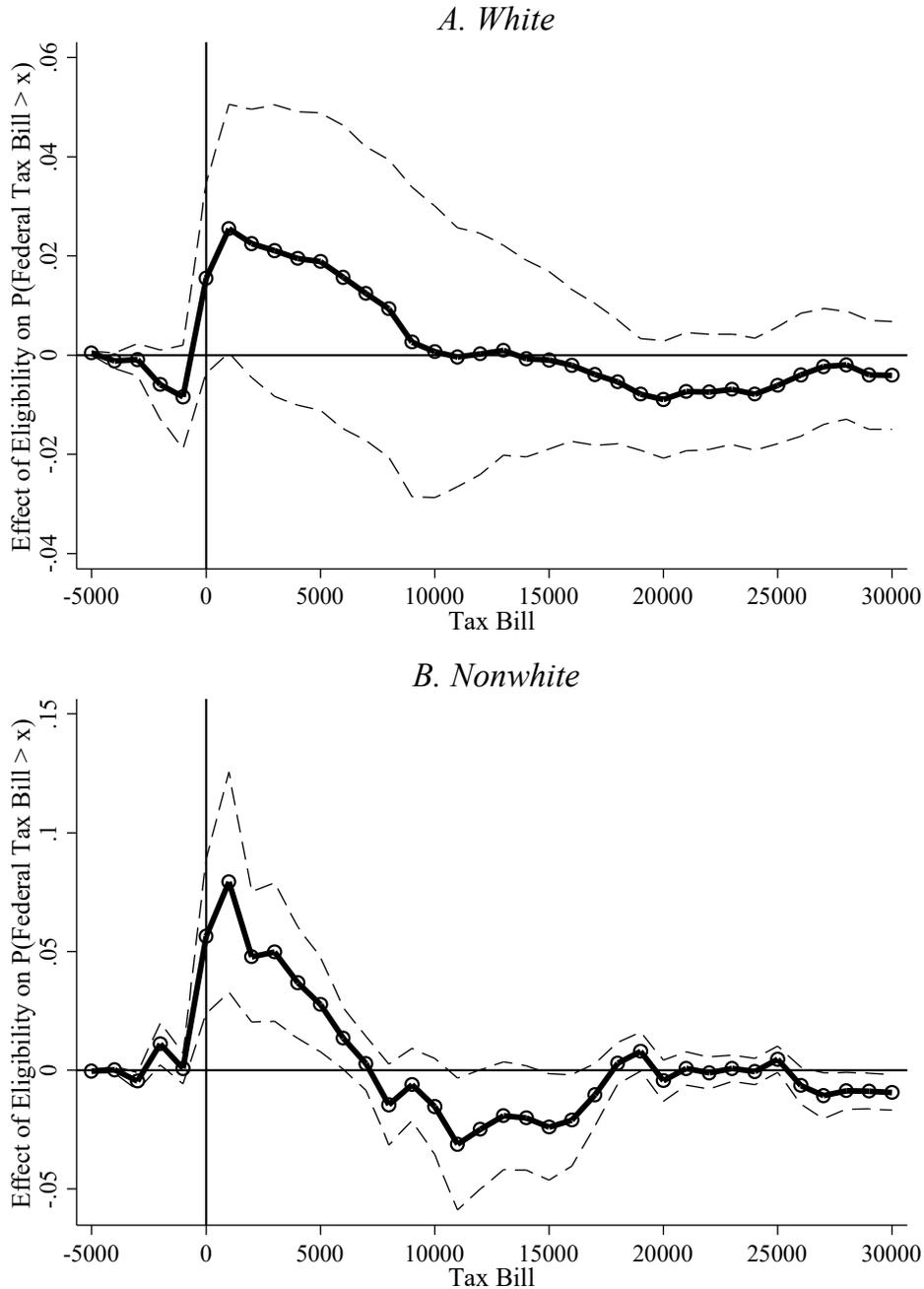
Notes: The figure plots event-study estimates corresponding to selected IV coefficients plotted in Figure 7.

Figure A5.8. Selection via Employment: Event-Study Estimates for Log Wages
 Effect of 1 p.p. difference in initial eligibility



Notes: The figure plots event-study estimates for average log wages and correspond to column 2 of table 8.

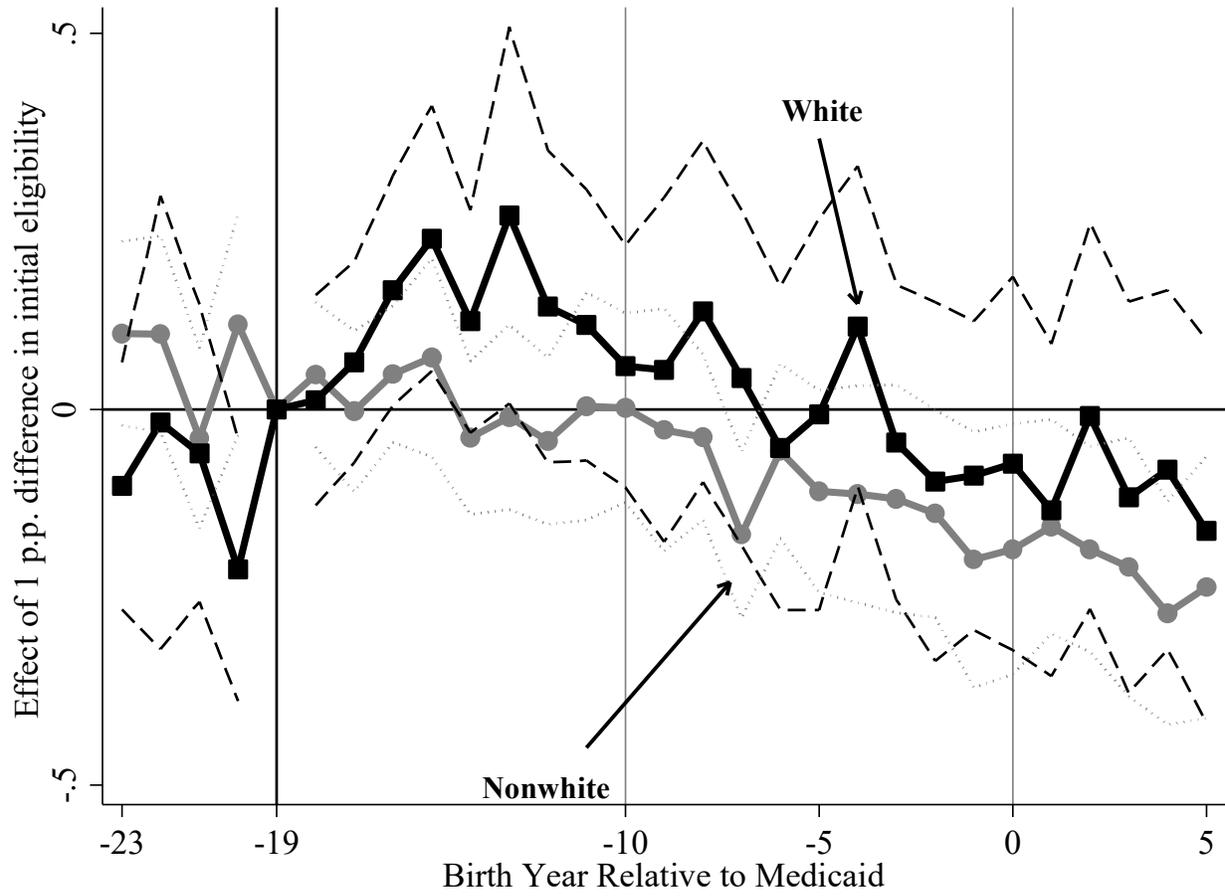
Figure A5.9. Instrumental Variables Estimates of the Effect of Medicaid Eligibility Before Age 11 on the Distribution of Tax Liability



The point estimates for positive tax liabilities have the same interpretation as the income results in Figure 7. They show that the probability of any tax liability (including payroll taxes) grew. The negative coefficients for negative tax liabilities show that Medicaid increased the amount of mass in the left tail of the tax liability distribution. The difference between estimates at a smaller minus larger cutoff equals the change in the probability of a tax bill in that bin. To see this, note that the estimate at a larger value is roughly $\Delta p(b > x_1)$, and the estimate at a smaller value is roughly $\Delta p(b > x_0) = \Delta p(b > x_1) + \Delta p(x_0 < b \leq x_1)$. Therefore, $\Delta p(b > x_0) - \Delta p(b > x_1)$ equals the change in the probability of a refund between x_0 and x_1 .

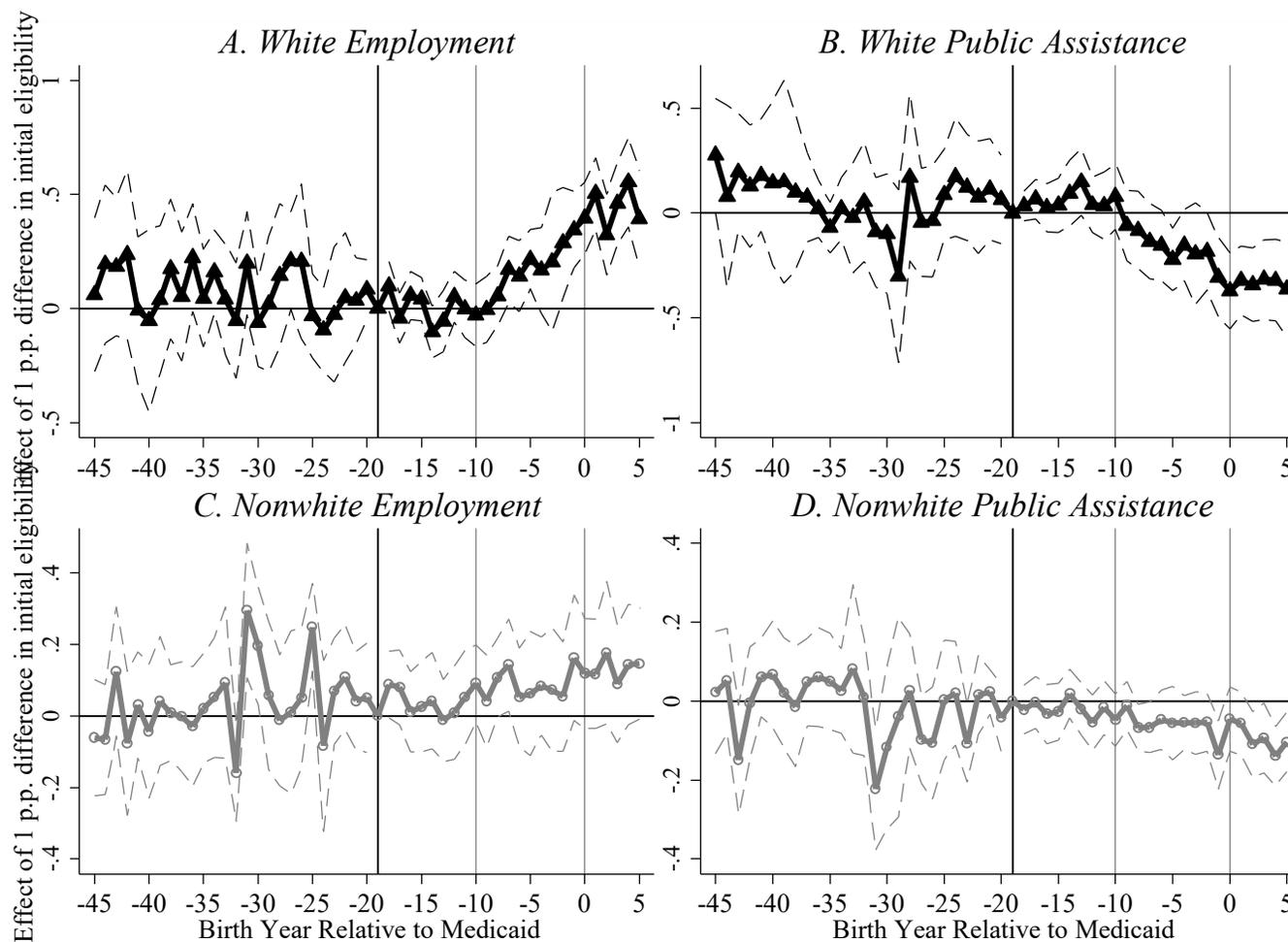
APPENDIX 6. ADDITIONAL EVIDENCE IN ALTERNATIVE SAMPLES

Figure A6.1. Event-Study Estimates for Ambulatory Difficulty, 2008-2017



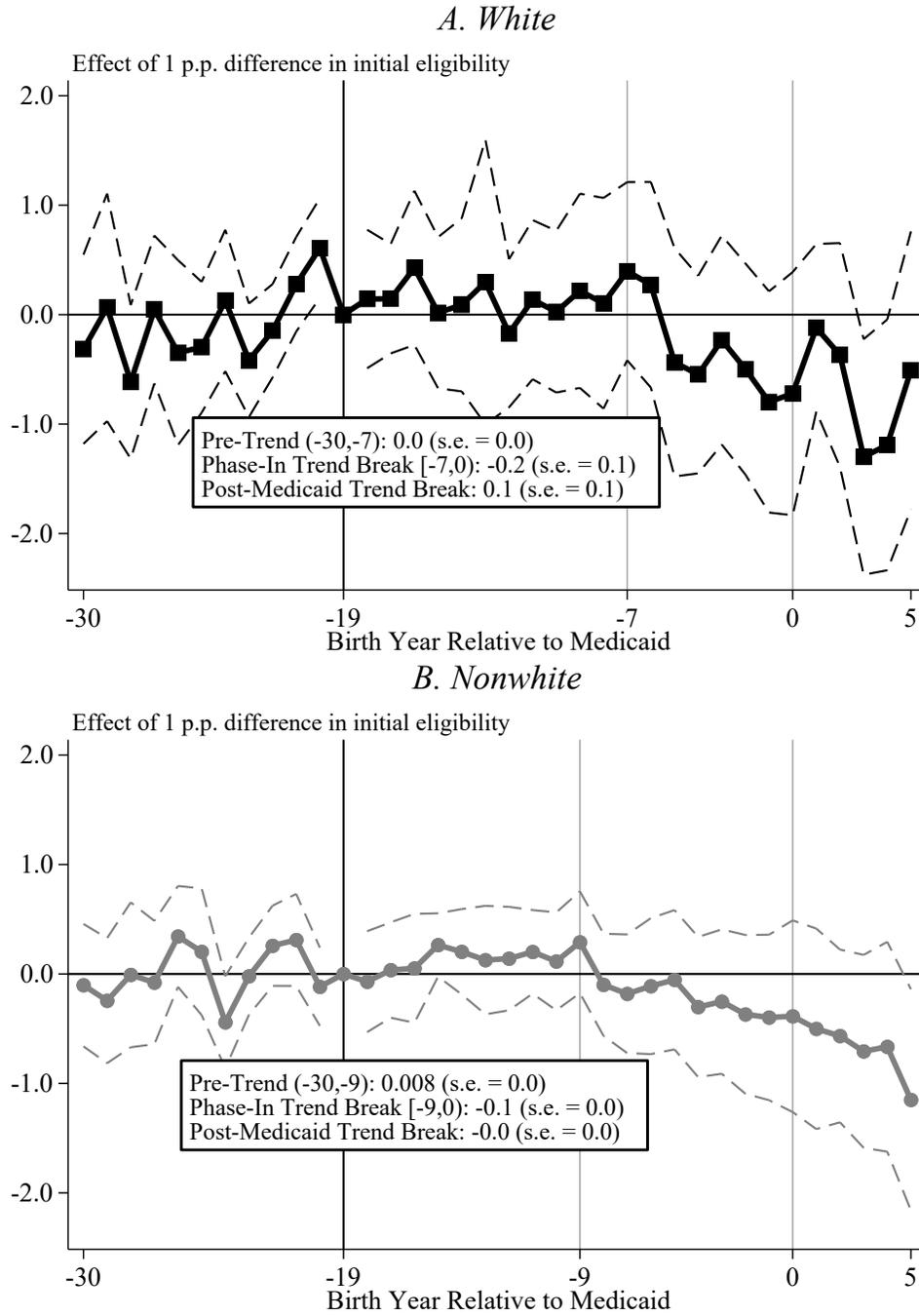
Notes: The specification is the same as in Figure 5.

Figure A6.2. Event-Study Estimates for Annual Employment and Public Assistance Receipt, Extended Sample



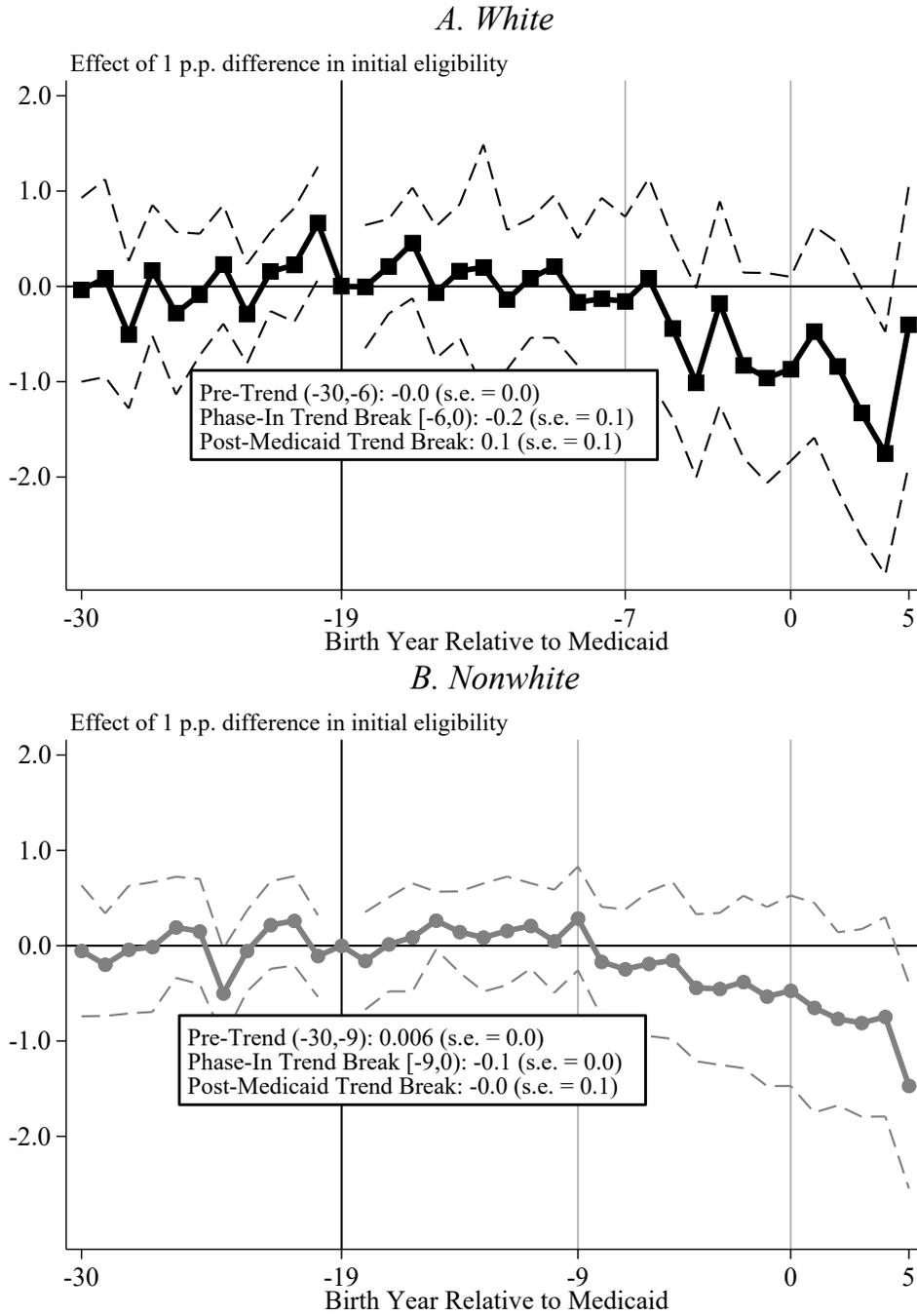
Notes: The sample adds to the main sample data on 25-64 year olds from the 1980 and 1990 Census. The specification corresponds to Figure 6, but also interacts the region-by-year and Medicaid-by-year fixed effects with dummies for 1980 and 1990 Census years. The continuous covariates are not available in all years, so I set them to zero when they are missing and include dummies for cells in which they are available.

Figure A6.3. Event-Study Estimates for log 30-Year Mortality (1980-2009)



Notes: This figure is comparable to Figure 4 in the main text but uses the log of 30-year mortality rates (1980-2009) instead of 37-year mortality rates (1980-2016).

Figure A6.4. Event-Study Estimates for log 25-Year Mortality (1980-2004)



Notes: This figure is comparable to Figure 4 in the main text but uses the log of 25-year mortality rates (1980-2004) instead of 37-year mortality rates (1980-2016).

APPENDIX 7: RE-SCALING INTENTION-TO-TREAT EFFECTS TO AVERAGE TREATMENT EFFECTS ON THE TREATED

A. Average Treatment Effect on the Treated for log 37-year Mortality Rates

Consider a simple difference-in-differences IV estimate comparing log mortality between two cohorts in two states (0 and 1) with different levels of cumulative eligibility ($m_1^{POST} > m_0^{POST}$ and $m_1^{PRE} = m_0^{PRE} = 0$, defined in equation 1):

$$DD^{ITT} = \frac{[\ln(y_1^{POST}) - \ln(y_0^{POST})] - [\ln(y_1^{PRE}) - \ln(y_0^{PRE})]}{[m_1^{POST} - m_0^{POST}] - [m_1^{PRE} - m_0^{PRE}]}$$

Let the shares of adults with any childhood eligibility be d_1 and d_0 , which implies that cumulative eligibility among the treated is $\frac{m_0^{POST}}{d_0}$ and $\frac{m_1^{POST}}{d_1}$. (Note that the dose among the treated is not ordered even though cumulative eligibility is.) The effect on log mortality per year of eligibility is assumed to be constant: δ . Assume also that a fixed share p of every cohort is poor, that their mortality rates are greater than the non-poor by $(1 + \sigma)$, and that the treated are only drawn from the poor. For simplicity, assume that $p = d_1 > d_0$. This yields the following expressions for pre/post high/low log mortality rates:

$$\begin{aligned} \ln(y_j^{PRE}) &= \ln(p(1 + \sigma)y_j^{PRE} + (1 - p)y_j^{PRE}) \\ &= \ln(p(1 + \sigma) + (1 - p)) + \ln(y_j^{PRE}) \end{aligned} \quad (A1)$$

$$\begin{aligned} \ln(y_1^{POST}) &= \ln\left(p(1 + \sigma)\left(1 + \frac{m_1^{POST}}{d_1}\delta\right)y_1^{POST} + (1 - p)y_1^{POST}\right) \\ &= \ln\left((1 + \sigma)m_1^{POST}\delta + p\sigma + 1\right) + \ln(y_1^{POST}) \end{aligned} \quad (A2)$$

$$\begin{aligned} \ln(y_0^{POST}) &= \ln\left(d_0(1 + \sigma)\left(1 + \frac{m_0^{POST}}{d_0}\delta\right)y_0^{POST} + (p - d_0)(1 + \sigma)y_0^{POST} + (1 - p)y_0^{POST}\right) \\ &= \ln\left((1 + \sigma)m_0^{POST}\delta + p\sigma + 1\right) + \ln(y_0^{POST}) \end{aligned} \quad (A3)$$

The numerator of the IV estimate is:

$$\begin{aligned} &\ln\left((1 + \sigma)m_1^{POST}\delta + p\sigma + 1\right) - \ln\left((1 + \sigma)m_0^{POST}\delta + p\sigma + 1\right) + \\ &\quad [\ln(y_1^{POST}) - \ln(y_0^{POST})] - [\ln(y_1^{PRE}) - \ln(y_0^{PRE})] \end{aligned}$$

The second line is zero under the common trends assumption. Using $\ln(1 + x) \approx x$, the terms in the first line approximately equal:

$$(1 + \sigma)m_1^{POST}\delta - (1 + \sigma)m_0^{POST}\delta = (m_1^{POST} - m_0^{POST})(1 + \sigma)\delta$$

The denominator of the DD IV estimator is $(m_1^{POST} - m_0^{POST})$, so the DD ITT effect is approximately equal to the proportional treatment effect per year among the treated times a factor measuring underlying differences in mortality between treated and untreated groups:

$$DD^{ITT} \approx (1 + \sigma)\delta \quad (A4)$$

How can we estimate $(1 + \sigma)$? One way is to compute the ratio of poor to non-poor mortality rates for untreated periods or cohorts: $\frac{(1+\sigma)y_1^{PRE}}{y_1^{PRE}} = \frac{(1+\sigma)y_0^{PRE}}{y_0^{PRE}} = (1 + \sigma)$. That is the strategy used in (Goodman-Bacon 2015, appendix 4). When such data are not available, however, the only thing we can do is compare *observed* mortality between, say, the poor and non-poor in the post-period.

Mortality rates among the poor in treated cohorts will differ from mortality rates of the non-poor for two reasons. First, they will be larger by a proportion $(1 + \sigma)$ as assumed above. Second, they will be lower by virtue of having been treated. For a proportional ATET of δ and an average eligibility among the treated of $\frac{\bar{m}}{d}$, the extent to which treatment effects reduce the mortality gap by poverty status is $(1 + \frac{\bar{m}}{d} \delta)$. Therefore:

$$\frac{y^{POOR}}{y^{NON}} = \frac{(1 + \sigma) \left(1 + \frac{\bar{m}}{d} \delta\right) y^{POST}}{y^{POST}} = (1 + \sigma) \left(1 + \frac{\bar{m}}{d} \delta\right) \equiv (1 + \tilde{\sigma}) \quad (A5)$$

Substituting for $(1 + \sigma)$ in (A4) shows that, under these assumptions, the DD estimate is:

$$DD^{ITT} = \frac{(1 + \tilde{\sigma})}{\left(1 + \frac{\bar{m}}{d} \delta\right)} \delta \quad (A6)$$

Solving this expression for δ shows how to use a DD intention-to-treat (IV) estimate along with information on treatment dose $\left(\frac{\bar{m}}{d}\right)$ and post-treatment differences in the outcome $(1 + \tilde{\sigma})$ to calculate the ATET:

$$\frac{DD^{ITT}}{\left[1 + \tilde{\sigma} - DD^{ITT} \frac{\bar{m}}{d}\right]} = \delta \quad (A7)$$

The denominator shows that the counterfactual mortality rate among the treated is higher because of observed differences $(1 + \tilde{\sigma})$ and because of the effect of the program $(- DD^{ITT} \frac{\bar{m}}{d})$.

Table A7.1 lists the statistics necessary to calculate δ according to (A7). The DD^{ITT} estimates come from Table 2. The observed poor/non-poor mortality ratios $(1 + \tilde{\sigma})$ use cumulative death probabilities from the PSID mortality supplement. The average number of years on Medicaid among those with any Medicaid exposure $\left(\frac{\bar{m}}{d}\right)$ come from two sources. For cohorts born between 1969 and 1975, Smith and Yeung (1998) report the average number of childhood years with any AFDC income in Table 1 and the share of children with any AFDC in Table 3. These are reported separately by race. Berger and Black (1998, Figure 4) report monthly hazard rates for new AFDC-based Medicaid spells in Kentucky in 1986/1987. This suggests that among households with any AFDC in a given year, the average number of months on AFDC is about 8.2; 68 percent of the

year. I use this share to translate the number of years with any AFDC into the number of full years of AFDC receipt. The implied proportional ATETs are reported in row g of Table A7.1.

B. Average Treatment Effect on the Treated in Levels

Begin with the same diff-in-diff expression as above, but in levels not logs as in the disability specifications.

$$DD^{ITT} = \frac{[y_1^{POST} - y_0^{POST}] - [y_1^{PRE} - y_0^{PRE}]}{[m_1^{POST} - m_0^{POST}] - [m_1^{PRE} - m_0^{PRE}]}$$

Maintain the assumption that poor and non-poor outcomes differ proportionally by $(1 + \sigma)$, but now add an additive treatment effect, Δ . Post-treatment mortality in state 1, for example, is

$$p \left((1 + \sigma)y_1^{POST} + \frac{m_1^{POST}}{d_1} \Delta \right) + (1 - p)y_1^{POST}$$

This set-up simplifies immediately to:

$$DD^{ITT} = \frac{[(m_1^{POST} - m_0^{POST})\Delta + [(y_1^{POST} - y_0^{POST}) - (y_1^{PRE} - y_0^{PRE})]]}{[m_1^{POST} - m_0^{POST}]}$$

The second term is zero by common trends (in levels this time), meaning that the DD^{ITT} estimate in levels is the *same* effect per year of coverage as the ATET. The total effect of the policy is, of course, larger among the treated subset than among the full population, but this is because they have more years of coverage. The question when assessing magnitudes in this context is what baseline mortality rate to use as a denominator. For each year of coverage, mortality falls by Δ , and without the policy post-treatment mortality among the treated would have been:

$$y_{poor}^{POST} = (1 + \sigma)y^{POST} - \frac{\bar{m}}{d_1} \Delta$$

To assess the magnitude of ITT effects in levels, I use auxiliary data to obtain an estimate of the rate among the poor (or some other measure of the treated) and then subtract the total effect of treatment on the outcomes of the treated: $\frac{\bar{m}}{d_1} \Delta$. The implied proportional ATETs are reported in row l of Table A7.1.

Table A7.1 Calculating the Average Treatment Effect on the Treated from Reduced-Form Estimates

Statistic	Source	White	Nonwhite
1. Mortality			
a. ITT Estimate (logs)	Table 2	-14.5	-8.73
b. Mortality ratio by childhood AFDC ($1+\sigma$)	PSID	1.57	1.1
c. Average Years with Any AFDC (white 0-5, nonwhite 0-11)	Smith and Yeung (1998) Table 1	0.32	3.51
d. Share with Any Years	Smith and Yeung (1998) Table 3	0.18	0.70
e. Share of Year on AFDC Any AFDC	Berger and Black (1998) Figure 4	0.68	0.68
f. Total Medicaid Years Among Treated ($e*c/d$)		1.2	3.4
g. Proportional ATET ($a/100$)/($b-(a/100)*f$)		-0.083	-0.062
2. Ambulatory Difficulty			
h. ITT Estimate (levels)	Table 2	-4.26	-5.73
i. Average Years with Any AFDC (white 0-11, nonwhite 0-5)	Smith and Yeung (1998) Table 1	0.63	1.75
j. Share with Any Years	Smith and Yeung (1998) Table 3	0.18	0.70
k. Share of Year on AFDC Any AFDC	Berger and Black (1998) Figure 4	0.68	0.68
l. Total Medicaid Years Among Treated ($e*c/d$)		2.4	1.7
m. Average Disability Rate	Table 4	5.71	8.65
n. Disability ratio by childhood AFDC ($1+\sigma$)	PSID	2.66	1.65
o. Counterfactual Disability Any AFDC ($m*n-l*f$)		25.31	24.04
p. Proportional ATET (h/k)		-0.17	-0.24

APPENDIX 8: MEASURING MEDICAID'S COSTS AND BENEFITS

Assume agents live for at most two periods, consume goods (x_{it}), and supply labor (ℓ_{it}). They also value health, which is assumed to be a function of the medical care provided by Medicaid, $G_i(\theta)$.³ Following Hendren (2016), θ denotes the policy, which is simply an increase in $G_i(\theta)$ due to Medicaid. Health at the end of the first period also determines the probability that an agent survives to period 2. The utility function is:

$$u_{i1}(x_{i1}, \ell_{i1}, ; h_{i1}(G_i(\theta))) + \frac{p_i(h_{i1}(G_i(\theta)))}{1+r} u_{i2}(x_{i2}, \ell_{i2}; h_{i2}(\theta)) \quad (M1)$$

I use the shorthand $h_{i1}(\theta) \equiv h_{i1}(G_i(\theta))$, and $p_i(\theta) \equiv p_i(h_{i1}(G_i(\theta)))$ because the policy affects health and survival only through $G_i(\theta)$. The lifetime budget constraint equates the present discounted value of spending on goods with the PDV of income:⁴

$$\begin{aligned} x_{i1}(\theta) + \frac{1}{1+r} x_{i2}(\theta) \\ \leq T_{i1} + (1 - \tau_{i1}^\ell(\theta)) \ell_{i1}(\theta) \\ + \frac{1}{1+r} [T_{i2}(h_{i2}(\theta), \ell_{i2}) + (1 - \tau_{i2}^\ell) \ell_{i2}(\theta)] \quad (M2) \end{aligned}$$

Period 1 transfers, T_{i1} , are *not* directly affected by the policy. Period 2 transfers, $T_{i2}(h_{i2}(\theta), \ell_{i2})$, are determined by health (SSDI) and labor supply (means test). I assume wages equal 1.

Maximizing (M1) subject to (M2) yields a value function, V_i , expressed as the maximized Lagrangean:

$$\begin{aligned} V_i(G_i, \tau_{i1}^\ell, \tau_{i2}^\ell, T_{i1}, T_{i2}) \\ = u_{i1}(x_{i1}^*, \ell_{i1}^*, ; h_{i1}(\theta)) + \frac{p_i(\theta)}{1+r} u_{i2}(x_{i2}^*, \ell_{i2}^*; h_{i2}(G_i(\theta))) \\ + \lambda_i \left\{ T_{i1} + (1 - \tau_{i1}^\ell(\theta)) \ell_{i1}^*(\theta) + \frac{1}{1+r} [T_{i2}(h_{i2}(\theta), \ell_{i2}^*) + (1 - \tau_{i2}^\ell) \ell_{i2}^*(\theta)] \right. \\ \left. - x_{i1}^*(\theta) - \frac{1}{1+r} x_{i2}^*(\theta) \right\} \quad (M3) \end{aligned}$$

The only new term in (M3) is λ_i , the marginal utility of income for person i .

The effect of the policy on welfare is the integral across the population of the derivative of the value function (possibly weighted by social welfare weights):

$$\frac{dV_i}{d\theta} = \frac{\partial V_i}{\partial G_i(\theta)} \frac{\partial G_i(\theta)}{\partial \theta}$$

³ An obvious extension would be to add medical care as a choice variable into the health production functions.

⁴ This formulation assumes that annuity markets do *not* exist. Yaari (1965) and Barro and Friedman (1977) discuss the role of annuity markets in life-cycle consumption models. It is clear why agents discount future utility flows using survival probabilities. When they have access to actuarially fair annuities, though, the interest rate is higher by the amount of the mortality rate, and the optimal consumption path is not affected by mortality risk. Few poor people take out annuities, and so I formulate the model accordingly. Chakraborty and Das (2005) develop a similar model with health and mortality.

The first way that Medicaid affects welfare is by improving health:

$$\frac{\partial u_{i1}}{\partial h_{i1}} \frac{\partial h_{i1}}{\partial G_{i1}(\theta)} \frac{\partial G_i(\theta)}{\partial \theta} + \left[\frac{\partial p_i(\theta)}{\partial h_{i1}(\theta)} \frac{\partial h_{i1}(\theta)}{\partial G(\theta)} u_{i2} + p_i(\theta) \frac{\partial u_{i2}}{\partial h_{i2}(\theta)} \frac{\partial h_{i2}(\theta)}{\partial G(\theta)} \right] \frac{\partial G(\theta)}{\partial \theta} \frac{1}{1+r}$$

This expression exactly equals the change in quality-adjusted life-years: higher period utility and higher longevity. Write this as $\frac{\partial QALY_i}{\partial \theta}$.

The second way that Medicaid affects welfare is because health determines the amount of period 2 transfers (eg. SSDI):

$$\lambda_i \frac{p_i(\theta)}{1+r} \frac{\partial T_{i2}}{\partial h_{i2}(\theta)} \frac{\partial h_{i2}(\theta)}{\partial \theta}$$

The program could made people healthier but also move them off a program meant for disabled workers. Combining the pieces and diving by the marginal utility of income, we get Hendren's marginal willingness to pay:

$$\frac{dV_i}{d\theta} \frac{1}{\lambda_i} = \frac{\partial QALY_i}{\partial \theta} \frac{1}{\lambda_i} + \frac{1}{1+r} \frac{\partial T_{i2}}{\partial h_{i2}(\theta)} \frac{\partial h_{i2}(\theta)}{\partial \theta} \quad (M4)$$

To solve for Medicaid's net cost, differentiate the present value of expected net transfer to person i , which is defined as:

$$t_i(\theta) = c^G G_i(\theta) + \underbrace{\left[T_{i1} + \frac{p_i(\theta)}{1+r} T_{i2}(h_{i2}(\theta), \ell_{i2}(\theta)) \right]}_{E[PDV] \text{ of Transfers}} - \underbrace{\left[\tau_{i1}^\ell \ell_{i1}(\theta) + \frac{p_i(\theta)}{1+r} \tau_{i2}^\ell \ell_{i2}(\theta) \right]}_{E[PDV] \text{ of taxes } (\tau_2 \text{ fixed})}$$

c^G is the cost of a unit of medical care provided by Medicaid. Differentiate $t_i(\theta)$ with respect to θ (assume no change in period 1 labor supply):

$$\begin{aligned} \frac{\partial t_i(\theta)}{\partial \theta} = c^G \frac{\partial G_i(\theta)}{\partial \theta} + & \underbrace{\frac{\partial p_i(\theta)}{\partial \theta} \frac{1}{1+r} [T_{i2} - \tau_{i2}^\ell \ell_{i2}(\theta)]}_{\text{mech effect bc of survival}} + \underbrace{\frac{p_i(\theta)}{1+r} \frac{\partial T_{i2}}{\partial h_{i2}(\theta)} \frac{\partial h_{i2}(\theta)}{\partial \theta}}_{\text{mech eff on T bc of h}} \\ & \underbrace{\frac{p_i(\theta)}{1+r} \frac{\partial T_{i2}}{\partial \ell_{i2}(\theta)} + \tau_{i2}^\ell(\theta)}_{\text{beh eff on taxes}} \frac{\partial \ell_{i2}(\theta)}{\partial \theta} \end{aligned} \quad (M5)$$

Note that $\frac{\partial T_{i2}}{\partial \ell_{i2}(\theta)}$ is in the tax term because the rate at which transfers change with labor supply equals the transfer program's tax rate on benefits. Because I use data on all transfer income rather than changes that come from health alone or labor supply alone, I rewrite this expression as follows:

$$\frac{\partial t_i(\theta)}{\partial \theta} = c^G \frac{\partial G_i(\theta)}{\partial \theta} + \frac{\partial p_i(\theta)}{\partial \theta} \frac{1}{1+r} [T_{i2} - \tau_{i2}^\ell \ell_{i2}(\theta)] + \frac{p_i(\theta)}{1+r} \frac{\partial T_{i2}}{\partial \theta} - \frac{p_i(\theta)}{1+r} \tau_{i2}^\ell(\theta) \frac{\partial \ell_{i2}(\theta)}{\partial \theta}$$

The costs, then, include direct offsets from reductions in benefits via health, behavioral offsets via labor supply, increased tax revenue because of labor supply, and a mechanical effect that comes from cohort size and could be net positive or negative.

This model yields the following expression for the marginal value of public funds (MVPF):

$$\begin{aligned} MVPF &\equiv \frac{\int_i \frac{\eta_i}{\eta_i} \frac{dV_i}{d\theta} \frac{1}{\lambda_i} di}{\int_i \frac{\partial t_i(\theta)}{\partial \theta} di} \\ &= \frac{\int_i \frac{\eta_i}{\eta_i} \left[\frac{\partial QALY_i}{\partial \theta} \frac{1}{\lambda_i} + \frac{1}{1+r} \frac{\partial T_{i2}}{\partial h_{i2}(\theta)} \frac{\partial h_{i2}(\theta)}{\partial \theta} \right] di}{\int_i \left[c^G \frac{\partial G_i(\theta)}{\partial \theta} + \frac{\partial p_i(\theta)}{\partial \theta} \frac{1}{1+r} [T_{i2} - \tau_{i2}^\ell \ell_{i2}(\theta)] + \frac{p_i(\theta)}{1+r} \frac{\partial T_{i2}}{\partial \theta} - \frac{p_i(\theta)}{1+r} \tau_{i2}^\ell(\theta) \frac{\partial \ell_{i2}(\theta)}{\partial \theta} \right] di} \end{aligned}$$

The numerator equals the dollar value of the sum of QALYs gained minus the change in transfer income. The denominator equals the total direct cost of Medicaid, plus the additional costs (or savings) that come from survival, changes in transfer costs and changes in tax payments. I assume that the welfare weights, η_i , are constant. A preference for redistribution would put more weight on welfare changes for low-income agents and increase the MVPF.

Because the point estimates imply a negative net cost, the MVPF is infinite. This is also true for later child Medicaid expansions (Hendren and Sprung-Keyser 2019). The main text and Table 9 discuss the numerator (willingness to pay) and denominator (net cost) separately.

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