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The long term relationship between childhood Medicaid expansions and severe chronic conditions in adulthood

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[Corrections added on 18 July 2023, after first online publication: David Brady's email address has been updated in this version.]

Abstract

We test whether the expansions of children's Medicaid eligibility in the 1980s-1990s resulted in long-term health benefits in terms of severe chronic conditions. Still relatively rare in the field, we use prospective individual-level panel data from the Panel Study of Income Dynamics (PSID) along with the higher quality income measures from the Cross-National Equivalent File (adjusting for taxes, transfers and household size). We observe severe chronic conditions (high blood pressure/heart disease, cancer, diabetes, or lung disease) at ages 30-56 (average age 43.1) for 4670 respondents who were also prospectively observed during childhood (i.e., at ages 0-17). Our analysis exploits within-region temporal variation in childhood Medicaid eligibility and adjusts for state- and individual-level controls. We uniquely concentrate attention on adjusting for childhood income. A standard deviation greater childhood Medicaid eligibility significantly reduces the probability of severe chronic conditions in adulthood by 0.05 to 0.12 (16%-37.5% reduction from mean 0.32). Across the range of observed childhood Medicaid eligibility, the probability is approximately cut in half. Greater childhood Medicaid eligibility also substantially reduces childhood income disparities in severe chronic

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conditions. At higher levels of childhood Medicaid eligibility, we find no significant childhood income disparities in adult severe chronic conditions.

KEYWORDS

chronic conditions, health disparities, life course, Medicaid, social policy

In the late 1970s, children in the U.S. were very unlikely to be covered by Medicaid. By the mid-1990s, a much greater share of children could receive Medicaid for far more of their childhoods. These expansions substantially enlarged public health insurance and healthcare for low-income children compared to two decades earlier (Grogan & Andrews, 2015; Kamerman & Kahn, 2001; Katz, 2001; Kouser, 2002). Although U.S. healthcare remains much more privately-provided than in other rich democracies (Brady et al., 2016; Wendt et al., 2009), American health policy grew markedly more inclusive for children – and especially low-income children – over the 1980s and 1990s.

Figure 1 shows this based on Miller and Wherry (Miller & Wherry., 2019) measures standardised across the U.S. and applied to our sample (see below). In 1979, the "average" American child was eligible for Medicaid for about two-thirds of one year of their entire childhood. In the most generous state, the average American child would have been eligible for slightly more than 1.5 years. By the mid-1980s, the average American child would have been eligible for about 1.25 years and more than 2 years in some states. By the mid-1990s, the average American child was eligible for almost 2 years and about 2.75 years in some states. Whereas Medicaid eligibility was almost uniformly low across states in the 1970s, there was far more variation with a much higher average across states by 1995.



FIGURE 1 Childhood medicaid eligibility in years for typical child across states in the U.S., 1979–1995. [Colour figure can be viewed at wileyonlinelibrary.com]

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This study builds on the extensive and valuable literature on the effects of Medicaid expansions. We specifically investigate the relationship between childhood Medicaid eligibility (at ages 0–17) and adult severe chronic conditions (at ages 30–56). In a still relatively rare approach, we use prospective individual-level panel data from the Panel Study of Income Dynamics (PSID) merged with higher quality income data from the Cross-National Equivalent File (adjusting for taxes, transfers and household size). While our study builds on rich prior research, our analysis offers two advantages. First, we use more comprehensive measures of chronic health conditions and observe them at older ages. Second, our models more systematically adjust for prospectively measured individual characteristics across the duration of childhood. As a result, we can uniquely inform how Medicaid expansions reduced long-term income disparities in health. By innovating in these ways, we provide novel evidence by addressing limitations of past research. In the process, our study also illuminates affinities between the Medicaid and comparative social policy literatures. This includes both the measures of social policy and for understanding how social policy might explain cross-national differences in health.

1 | PAST RESEARCH

The research programme on the effects of Medicaid expansions has been very productive. The field provides many rigorous studies examining Medicaid's initial implementation since the 1960s (e.g., Boudreaux et al., 2016; Goodman-Bacon, 2018, 2021), varied expansions across states in the 1980s and 1990s (e.g., Miller & Wherry., 2019; O'Brien & Robertson, 2018), and the most recent expansion in the Affordable Care Act (e.g., Kaestner et al., 2017). By now, the literature already provides many excellent narratives of the programme's historical evolution and the precise policy changes in the 1980s-1990s (Grogan & Andrews, 2015; Katz, 2001). The literature also already offers meticulous accounts of potential mechanisms leading to various outcomes. Among other mechanisms, Medicaid access can influence stress, resource depletion, health, and healthy development, all of which could subsequently influence chronic conditions.

Many analyse how the 1980s-1990s expansions affected shorter term outcomes during childhood (e.g., Currie et al., 2008; Curie & Gruber, 1996a, Curie & Gruber, 1996b; Jackson et al., 2021; Levine & Schanzenbach, 2009). Others examine the longer term effects of childhood Medicaid eligibility on outcomes later in adulthood (e.g., Levere et al., 2019). Regarding the expansions in the 1980s and 1990s specifically, scholars have linked Medicaid to many beneficial outcomes (Brown et al., 2020; Cohodes et al., 2016; Lipton et al., 2016; Miller & Wherry., 2019; O'Brien & Robertson, 2018; Thompson, 2017).

The prevailing approach to the later life adult consequences of Medicaid expansions in the 1980s-1990s is as follows (e.g., Cohodes et al., 2016; Curie & Gruber, 1996a, Curie & Gruber, 1996b; Currie et al., 2008; East et al., 2023; Lipton et al., 2016). Scholars usually utilise cross-sectional data observed in adulthood that contains retrospective information on the date and location of childbirth. From this, individuals are assigned the Medicaid eligibility that existed in utero or in childbood. This approach is facilitated by the well-established measures of Medicaid eligibility discussed below. In a compelling study, Miller and Wherry (2019) analyse the cross-sectional National Health Interview Survey and Current Population Survey at ages 19–36 in years 1998–2015. They then retrospectively link that data to the Medicaid eligibility where and when those adults were in utero. This approach has indisputably been valuable. However, because childhood is not prospectively observed, one cannot adjust for many of the individual-level differences between respondents that occurred during childhood. Instead, these studies mainly rely on instrumental variables to offset unobserved confounding.

It has been less common to use prospective longitudinal data on respondents observed during both childhood and adulthood. One advantage of such prospective longitudinal studies (compared to the retrospective approach) is the capacity to adjust for individual-level differences during childhood. Using administrative tax data on ~10 million respondents, Brown et al. (2020) show childhood Medicaid eligibility lead to greater access to Medicaid at ages 12–15, and this resulted in greater college enrolment, wages and taxes paid and lower mortality and fertility at ages

19–28. A few use prospective individual-level panel surveys like us (e.g., Jackson et al., 2021). These studies observe a greater duration of childhood and more exhaustively adjust for differences in childhood characteristics. Using the National Longitudinal Survey of Youth 1979, Thompson (2017) finds childhood Medicaid eligibility predicts several outcomes at ages 18–20 (self-rated health, limited in ability to work/attend school, chronic conditions, and asthma attacks). Using the PSID like us, but analysing the initial rollout of Medicaid 1966–1982 (not the 1980s–1990s expansions), Boudreaux et al. (2016) show Medicaid exposure at ages 0–5 predicts health outcomes at ages 25–54.

2 | INNOVATIONS BEYOND PAST RESEARCH

Our analysis builds on this strong research programme by offering two critical advantages. First, we use more comprehensive measures of chronic health conditions and observe them at older ages (i.e., 30–56). Unlike prior research, we focus most on four more salient and consequential and "severe" chronic conditions – high blood pressure/heart disease, cancer, diabetes, and lung disease (Brady et al., 2022; Link et al., 2008). Further analyses precisely test each severe and three less severe conditions, and obesity (Appendix C). These chronic conditions are salient given they predict mortality, are a source of enormous private and public healthcare costs, constrain employment and productivity, and undermine well-being.

Despite clear contributions, prior studies use less comprehensive measures of chronic conditions. Miller and Wherry (2019) find significant effects for a "chronic disease index" of obesity, diabetes, heart disease, and high blood pressure. Thompson (2017) does not find robustly significant effects for "any condition that requires frequent medical attention, the regular use of medication, or the use of special equipment" or having had an asthma attack. Analysing the initial rollout of Medicaid 1966–1982, Boudreaux et al. (2016) analyse a chronic condition index of high blood pressure, heart disease/heart attack, adult onset diabetes, and obesity. Unlike Miller and Wherry (2019) and Boudreaux et al. (2016), we include cancer and lung disease. Unlike Miller and Wherry (2019) and Boudreaux et al. (2016), we analyse obesity separately from our main outcome (see Appendix C).¹

Previous research also shows it is essential to observe respondents at older ages as chronic conditions usually emerge later in life (Brady et al., 2022). Some studies analyse the mortality consequences of the initial rollout of Medicaid and therefore include older respondents (e.g., Goodman-Bacon, 2018, 2021). For example, Boudreaux et al. (2016) PSID sample includes 25–54 year olds. For all the studies of the 1980s–1990s expansions however, all samples are much younger than our sample of 30–56 year olds. (Miller & Wherry., 2019) sample is 19–36 years old, Thompson (Thompson, 2017) is 18–20 years old, Brown et al. (2020) is 19–28 years old, and Cohodes et al. (2016) is 22–29 years old. Thus, we examine the oldest –and arguably most appropriate – sample of any study of the 1980s–1990s expansions.²

Second, we adjust for prospectively measured individual characteristics – and especially income – across the duration of childhood.³ Doing so is justified because of the vast literature showing childhood circumstances shape long term health outcomes (e.g., Duncan et al., 2012; Link et al., 2017). Using the PSID, Johnson and Schoeni (2011) show family income and poverty at ages 13–16 influence self-rated health, asthma, hypertension, diabetes, stroke, heart attack, and heart disease at ages 39–56. Perhaps most relevant is the large interdisciplinary literature on the "long arm of childhood" (e.g., Hayward & Gorman, 2004; Turner et al., 2016). For instance, using the PSID, Brady et al. (2022) show that prospective high quality income measured at ages 0–17 predicts self-rated health, psychological distress, heart attack, stroke, and severe chronic conditions at ages 40–65. Given extensive research shows childhood income affects adult health; omitting childhood and adolescence are plausibly influential for subsequent health. Hence, our longer observed duration of childhood plausibly provides greater information about childhood circumstances.

To the best of our knowledge, the literature on Medicaid and adult outcomes mostly omits childhood income from models. Largely, this is a by-product of using contemporary cross-sectional data and retrospectively assigning

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childhood Medicaid eligibility. In the contemporary cross-sectional surveys used (e.g., NHIS, ACS), even retrospective measures of childhood circumstances are not usually available.⁴ Even when prospective childhood income measures are available however, some omit childhood income (e.g., Thompson, 2017). While not controlling for income during childhood, Boudreaux et al. (2016) adjust for head's education and official poverty during childhood and mother's education and marital status at birth.⁵ In the one study we could find that assesses robustness when controlling for income, Brown et al.s' (2020) results are quite sensitive to adjusting for childhood income.⁶

Prior research mostly uses instrumental variables to avoid omitted variable bias from unobserved characteristics like childhood income. Following Curie & Gruber, 1996a, Curie & Gruber, 1996b, many use simulated Medicaid eligibility to instrument for actual Medicaid eligibility. As well, when individual-level data are used, poverty is often used to calculate eligibility (but see en. 5). To the extent the instruments are exogenous; this approach can test the causal effect of Medicaid eligibility without adjusting for all relevant predictors. However, avoiding omitted variable bias with instrumental variables is only as credible as the exclusion restriction.

It seems fair to raise questions about whether this strong assumption is credible. There are actually several plausible backdoor causal pathways from simulated childhood Medicaid eligibility to childhood income. As one plausible pathway, childhood Medicaid eligibility is a form of welfare state generosity (Brady et al., 2016; Reynolds & Avendano, 2018), and welfare state generosity is endogenous to political institutions like power resources and racial regimes (e.g., Grogan & Andrews, 2015; Huber & Stephens, 2001; Kamerman & Kahn, 2001; Katz, 2001; Korpi, 1989; Quadagno, 1994; Wendt et al., 2009). In fact, interstate and temporal variation in Medicaid expansions in the 1980s and 1990s were influenced by such interstate and temporally varying factors (e.g., Grogan & Andrews, 2015; Grogan & Patashnik, 2003; Katz, 2001; Kouser, 2002; Lanford & Quadagno, 2022; Michener, 2018; Quadagno, 2006). Interstate and temporal variation in such factors also influences income and especially for the poor (e.g., Baker, 2022; VanHeuvelen & Brady, 2022). Therefore, such factors plausibly cause both Medicaid eligibility and income. In turn, the instrument for childhood Medicaid eligibility could be confounded with childhood income. As a complement to prior analyses, it would be productive to estimate models with a more complete set of controls.

Another – and perhaps even stronger – motivation for including and rigorously measuring childhood income is the capacity to test for income *disparities*. Largely, the literature understandably presumes Medicaid eligibility should have more powerful effects on low-income children. Indeed, many studies test for heterogeneous effects across income, poverty, parents' education, or Medicaid eligibility (e.g., Boudreaux et al., 2016; Brown et al., 2020; Curie & Gruber, 1996a; Currie et al., 2008; Jackson et al., 2021; Miller & Wherry., 2019). Therefore, heterogeneous effects by income are likely and worth testing. However, these prior tests of interactions have been forced to rely on less rigorous measures of family background than our measures of childhood income and family background.⁷ As a result, our analyses can uniquely interact with and adjust for a more reliable and valid measure of childhood income. Therefore, our analyses can better clarify how Medicaid expansions influence childhood income disparities in long term health.

3 | METHODS

We use the longitudinal, nationally representative Panel Study of Income Dynamics (PSID), which has been fielded annually 1968–1997 and biannually since. We also use the Cross-National Equivalent File (CNEF), which provides a supplementary set of higher quality income variables for the entire sample of PSID respondents (Frick et al., 2007).

The sample includes 4670 individuals who were children (0–17 years) in households interviewed 1979–1995 and followed until 30–56 years old. The 1979–1995 period is determined by the availability of Miller and Wherry's Medicaid measure.⁸ To observe adult chronic conditions, we select the last available observation for each respondent through the 2019 wave. This captures respondents at their oldest age possible. 81.3% of observations come from 2019 and the mean age when chronic conditions are observed is 43.1.⁹ Appendix A shows descriptive statistics and Appendix B shows bivariate correlations. We discuss all robustness checks at the end of the results section.

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3.1 | Adult chronic conditions

The PSID includes self-reports of whether a doctor has diagnosed the respondent as having any chronic conditions. We focus on a binary measure of any of four *severe chronic conditions*: high blood pressure/heart disease, cancer, diabetes, and lung disease (Brady et al., 2022; Link et al., 2008). 31.9% has at least one severe condition (see Appendix A). That means 1399 of 4670 cases of the unweighted sample (see Appendix C). Appendix C reports the prevalence of each chronic condition. The most common is high blood pressure/heart disease (23.9%), followed by diabetes (8.6%), cancer (4.4%), and lung disease (4.0%).

3.2 | Childhood Medicaid eligibility

Our key independent variable is Miler and Wherry's (2019) synthetic measure of the percentage of children eligible for Medicaid (which updates Curie & Gruber, 1996a, Curie & Gruber, 1996b). In each year 1979–1995, they draw a nationally representative sample from the Census Bureau's Current Population Survey of 1000 children in each of four age ranges (1–4, 5–9, 10–14, and 15–18). They then apply each state-year's eligibility criteria and estimate what share of that national age group would be eligible for Medicaid if it resided in a given state. This simulates a standardised percent of children eligible as if all states had the same national demographic characteristics. Miller and Wherry convert this information to the number of years covered by Medicaid for each age group in a state-year. We match each respondent to their state-year-age group. We then average this measure over all observed years of childhood to measure *childhood Medicaid eligibility* (henceforth childhood Medicaid).

This measure has several useful features. First, it reasonably captures intent to treat effects, which realistically gauge how state-level policy changes affect individuals when take-up and access are always incomplete. Policymakers have more control over legislated eligibility than the actual access to and enrolment in Medicaid (Heinrich et al., 2022; Herd & Moynihan, 2019). Second, because the measure is simulated based on a fixed nation-wide population, this prevents state-year eligibility conflating population needs with actual social policy generosity. Third, a vast comparative social policy literature provides a precedent for this approach. Since at least Korpi (1989), comparative social policy researchers have constructed measures of welfare generosity by indexing programmes to "typical worker" wages or "notional household types" (Beckfield & Bambra, 2016; Nelson et al., 2020; Scruggs & Tafoya, 2022). Similarly, these measures assess how Medicaid eligibility for the same nationally-standardised "typical child" varies across states and years.

Ideally, we would also measure receipt of Medicaid at the individual-level and/or instrument for it with the eligibility measure. Unfortunately, the PSID only began asking the necessary relevant questions in 1999. See Appendix D for a detailed summary of the PSID's Medicaid questions. Therefore, the PSID simply does not allow us to determine whether these children in the 1980s and 1990s were actually enrolled in Medicaid. Again, this means our estimates are intent-to-treat effects not treatment effects on the treated.¹⁰

Because the childhoods of respondents of different ages were observed for different periods of time 1979–1995, all models adjust for the *number of years childhood Medicaid* was observed (never statistically significant). For instance, a respondent who was 17 in 1980 could be observed for 2 years in our data (1979 and 1980) while a respondent who was 17 in 1995 could be observed for 16 years (1979–1995).

3.3 | Other independent variables

To optimise the measurement of *childhood income*, we incorporate leading standards in international income measurement (Brady et al., 2018; Duncan et al., 2002; Jäntti & Jenkins, 2015; Mazumder, 2016; Rainwater & Smeeding, 2003). We use the CNEF measure of "post-fisc" equivalized household income, defined as including market income, cash and near cash transfers (e.g., the Supplemental Nutritional Assistance Programme), and subtracting taxes, and adding tax credits (e.g., the Earned Income Tax Credit). This measure includes all household members, and we equivalize household size by dividing by the square root of HH members. To standardise income over time, we convert income to relative rank percentiles in each year. Fortunately, the PSID-CNEF has a large nationally representative sample each year. For each respondent, we then average their relative rank percentiles over childhood (i.e., ages 0–17).

This measure far outperforms cruder measures of income (or wealth, occupation or earnings) as a proxy for permanent income (Brady et al., 2018). Incorporating these standards also results in higher estimates of the intergenerational transmission of income (Jäntti & Jenkins, 2015; Mazumder, 2016), better predicts health and well-being (Brady, Curran, & Carpiano, 2023) and mortality (Brady, Kohler, & Zheng, 2023), and better explains Black-White inequalities and adult life chances (Brady et al., 2020). Using the PSID, Brady et al. (2022) show that this particular measure best predicts mature adult health, including chronic conditions. They also show it performs at least slightly better than several alternative prospective or retrospective measures of parents' occupation, social class, education, and absolute income.¹¹

We include four other family background measures averaged over all observations during childhood. Rather than the PSID identified "reference person", we define the household lead as the adult with the highest labour market earnings in a given year (Brady et al., 2018). Ties are broken by age (and randomly if age is tied). We use the lead to calculate the average *parent age* in years and average *parent education* in years of schooling. We also adjust for *childhood sibship size*, which is the average number of other children in the household during childhood. Finally, we include *childhood single motherhood* as the proportion of years in a single mother household during childhood. The average respondent grew up with a parent with 12.8 years of schooling and an average age of 36.8 years, and a sibship size of 1.5. On average, about 15% of years during childhood were in single mother households.

Matching respondents to their state-year, the models also adjust for several other temporally varying state-level variables averaged over childhood (VanHeuvelen & Brady, 2022). *Childhood state unemployment* (rate) and *childhood gross state product* per capita (GSP PC inflation-adjusted \$) capture the business cycle and level of economic development. *Childhood state government spending* (as % of GSP) and *childhood state unionisation* (% among nonagricultural workers) capture state size and labour market organisation as critical institutions.

We also identify the modal state in which each child resided across childhood. We then code that modal state into the 9 Census divisions, which we call *nine smaller regions*, and include fixed effects (FEs) for those regions. Unfortunately, there are too few respondents per state to include state FEs instead.¹² The advantage of including region FEs is we can adjust for stable unobserved differences between geographic areas across the U.S.

In the wave when health outcomes are measured, we adjust for the *age* of the respondent in years (mean 43.1). The models control for binary indicators of 1970s and 1980s birth cohorts (reference<1970 birth). We also include mutually exclusive binary measures of whether respondents are *Black* (14%), *Latino* or *Other Race* (reference = White).¹³ Finally, we control for a binary indicator for *Woman* (50%).

3.4 | Analytic approach

All models are linear probability models. We cluster standard errors based on the modal state for each respondent's childhood.¹⁴ We fit the following regression models with the individual as the unit of analysis:

$$Y_{ijk} = \beta_0 + \beta Medicaid_j + \beta X_i + \beta Z_j + \beta W_i + \beta R_k + \varepsilon_{ijk}$$

Severe chronic conditions (Y) are observed at age 30–56. Y varies between individuals (i), who are nested in states (j) and nine regions (k). Y is predicted by state-level childhood Medicaid (*Medicaid*_j) and state-level controls (Z_j), both averaged over childhood. We include individual characteristics (X_j) averaged over childhood or observed in

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adulthood. Because we use the oldest/most recent observation for each respondent and those observations come from various waves, we include wave FEs (W_i) when the outcome was observed. Again, the models include FEs for nine smaller regions (R_k).

We exploit that exposure to childhood Medicaid varied temporally within regions. Appendix G Figure A1 confirms and shows that children at various points 1979–1995 experienced substantially different Medicaid eligibility. While children in 1979 experienced more uniformly low levels, children in 1987 and especially in 1995 experienced far greater interstate heterogeneity with a much higher mean and maximum. Thus, our sample was exposed to very different Medicaid eligibility depending on where and when they were children.

The individual-level controls reduce the confounding with childhood and adulthood characteristics predicting adult chronic conditions. By including the individual-level controls, we also adjust for between-state differences in population composition in the individual characteristics (e.g., race and education). By adjusting for other state-level variables, we reduce the unobserved confounding of childhood Medicaid with other state-level variables. Including the wave and cohort FEs means any effects of childhood Medicaid are not simply due to nation-wide improvements in population health. Including nine smaller region FEs, alongside the individual- and state-level controls, we assess the effect of Medicaid from within-region temporal variation.

Our identifying assumption is that other unmeasured within-region changes in state-level characteristics did not co-occur with Medicaid policy changes. Of course, this assumption is probably just as strong as the exclusion restriction for instrumental variables. We emphasise that our models simply provide different and complementary evidence. We conjecture that the field benefits from evidence built on a variety of modelling assumptions.

4 | RESULTS

Table 1 shows various models of severe chronic conditions in adulthood on childhood Medicaid and other independent variables. Model 1 only includes childhood Medicaid, model 2 adds the individual-level controls, model 3 adds the region FEs, and model 4 only includes the state-level controls and region FEs. Model 5 includes all variables.

Across models, childhood Medicaid is negative and statistically significantly associated with severe chronic conditions. For childhood Medicaid, we report x-standardised coefficients (i.e., corresponding to a one standard deviation change in the independent variable). For a standard deviation increase in childhood Medicaid, the probability of a chronic condition declines by 0.05–0.12. In model 5, the standardised coefficient is –0.05. About 32% has a severe chronic condition. Therefore, a standard deviation higher childhood Medicaid is associated with about a 16%–37.5% reduction from the mean probability of having a chronic condition.

Using model 5, Figure 2 displays the declining probability of a severe chronic condition across the standardised range of childhood Medicaid. If the child experienced one standard deviation below average childhood Medicaid, the probability of a chronic condition exceeds 0.4. Near the middle of the distribution of childhood Medicaid, the probability of a chronic condition is about 0.3. At the high end, two standard deviations above the average, the probability of a chronic condition is only 0.2. Across the range of observed childhood Medicaid, the probability of severe chronic conditions in adulthood is approximately cut in half.

While the childhood Medicaid coefficient is robust, note the contrast between models 1 and 2.¹⁵ When individual-level controls – including especially childhood income – are added, the coefficient for childhood Medicaid declines by about 20%. Hence, omitting the individual-level variables may upwardly bias the childhood Medicaid coefficient. In addition, childhood income has a robustly significant negative association with severe chronic conditions before and after adjusting for region FEs and state-level controls. In model 5, a standard deviation increase in childhood income reduces the probability of a severe chronic condition by 0.04.

Beyond childhood Medicaid and childhood income, we note parent education and being Latino are robustly negatively associated with having a severe chronic condition.¹⁶ Among state-level variables, only GSP p.c. is

TABLE 1 OLS regression models of severe chronic conditions on childhood medicaid eligibility, childhood income and controls (N = 4670).

	(1)	(2)	(3)	(4)	(5)
Childhood medicaid eligibility	-0.117***	-0.093***	-0.053*	-0.066***	-0.050*
	(0.010)	(0.018)	(0.020)	(0.015)	(0.022)
Childhood income (average %)		-0.001*	-0.002**		-0.002*
		(0.001)	(0.001)		(0.001)
Parent education (years)		-0.012*	-0.010*		-0.011*
		(0.005)	(0.005)		(0.005)
Parent age (years)		0.002	0.002		0.002
		(0.002)	(0.002)		(0.002)
Childhood sibship size (#)		-0.015	-0.013		-0.013
		(0.011)	(0.011)		(0.011)
Childhood single motherhood (proportion)		0.039	0.032		0.031
		(0.045)	(0.045)		(0.045)
Age (years)		0.003	0.009*		0.004
		(0.004)	(0.004)		(0.004)
Black		0.026	0.012		0.024
		(0.030)	(0.028)		(0.030)
Latino		-0.366***	-0.395***		-0.375***
		(0.085)	(0.095)		(0.098)
Other race		0.023	0.023		0.030
		(0.043)	(0.042)		(0.042)
Woman		-0.027	-0.026		-0.026
		(0.018)	(0.018)		(0.018)
1970s Cohort		0.008	0.020		0.018
		(0.051)	(0.052)		(0.053)
1980s Cohort		0.001	0.020		0.025
		(0.071)	(0.070)		(0.072)
Childhood State unemployment rate				-0.007	-0.012
				(0.014)	(0.015)
Childhood State Government spending				-0.017	-0.017
				(0.010)	(0.010)
Childhood State GSP P.C.				-0.039**	-0.028*
				(0.013)	(0.013)
Childhood State unionisation				0.045	0.048
				(0.027)	(0.034)
# of Years childhood medicaid observed	0.001 (0.003)	0.002 (0.003)	0.001 (0.003)	-0.000 (0.002)	0.001 (0.003)
Constant	0.183	0.247	0.051	0.203	0.202
	(0.144)	(0.223)	(0.217)	(0.139)	(0.222)
Nine smaller region FEs	No	No	Yes	Yes	Yes
R ²	0.083	0.100	0.107	0.094	0.109

Note: Models include fixed effects for wave when adult health is observed. All state-level variables are standardised, so the coefficients are x-standardised. Robust clustered standard errors in parentheses.

 $p < 0.001, ^{**}p < 0.01, ^{*}p < 0.05.$



FIGURE 2 Probability of severe chronic conditions in adulthood across standard deviations of childhood medicaid eligibility. [Colour figure can be viewed at wileyonlinelibrary.com]

significantly associated with having a chronic condition. This should reassure readers that there are not many obvious unobserved state-level variables that vary within regions and that are confounded with childhood Medicaid.

Medicaid is targeted at low-income children. Obviously, childhood Medicaid should have a more powerful effect on severe chronic conditions for those with lower average childhood incomes. As a result, childhood Medicaid expansions should have narrowed childhood income disparities in adult chronic conditions. In turn, we interact childhood Medicaid with: (1) a binary indicator of whether income averaged in the bottom 50% of the distribution across childhood; and (2) childhood income rank.

Table 2 shows childhood Medicaid interacts significantly and (a) negatively with being in the bottom 50% of childhood income, and (b) positively with childhood income rank. Hence, childhood Medicaid reduces the harm of having a low childhood income and narrows disparities between poor and rich childhoods. As expected, childhood Medicaid does not have a statistically significant effect above the bottom 50% of the childhood income distribution (i.e., the "main effect" in model 1).

Figure 3 displays the interactions with continuous childhood income rank. For simplicity, we calculate the probability of a severe chronic condition for low and high childhood incomes (i.e., at \pm one standard deviations from the mean). Figure 3 shows there are large income disparities in the probability of a chronic condition where childhood Medicaid is low. At -1 standard deviations in childhood Medicaid, those with a low childhood income have a probability of a severe chronic condition exceeding 0.5. By contrast, those with a high childhood income only experience of a probability near 0.3. The gap between low and high childhood incomes fall precipitously as childhood Medicaid rises. At about 0.75 standard deviations above the mean childhood Medicaid, the confidence intervals overlap for low and high income childhoods. At higher levels of childhood Medicaid, the probabilities of having a severe chronic condition do not differ between those who had low and high childhood incomes.

	(1)	(2)
Childhood medicaid	-0.026	-0.117***
Eligibility	(0.025)	(0.028)
Childhood income in bottom 50%	0.048	
	(0.026)	
Childhood medicaid eligibility*	-0.056**	
Childhood income in bottom 50%	(0.016)	
Childhood income rank		-0.002**
		(0.001)
Childhood medicaid eligibility*		0.001***

TABLE 2 OLS regression models of severe chronic conditions on interactions between childhood medicaid eligibility and childhood income, and controls (N = 4670).

Note: Models include all variables and fixed effects from model 5 of Table 1. Childhood Medicaid eligibility is standardised,
so the coefficients are x-standardised. Robust clustered standard errors in parentheses.

0.112

***p < 0.001, **p < 0.01, *p < 0.05.

Childhood income rank

 \mathbb{R}^2



FIGURE 3 The probability severe chronic conditions in adulthood for Low- and High childhood income across standard deviations of childhood medicaid eligibility (*Source*: low income = mean-1 SD, high income = mean+1 SD). [Colour figure can be viewed at wileyonlinelibrary.com]

(0.000)

0.115

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4.1 | Robustness checks

We conducted a variety of additional analyses. Appendix C shows models of each chronic condition separately. We also model a binary measure of any "less severe" conditions, including arthritis, asthma, and "other" chronic conditions. The Medicaid coefficient is negatively signed for all. We combined all seven conditions into one binary measure, which yields similar results. The PSID also allows us to measure obesity, which Appendix C shows is negatively signed but not significant (see en. 1). Chronic conditions are measured with self-reports for household reference persons (i.e., "heads"), but proxy reports for spouses. Appendix D shows consistent results for reference persons when omitting proxies. Because we include multiple adults per household, that model also shows only one adult per household. Most reference persons are men, but Appendix D shows similar results for women and men. Chronic conditions are more common among older respondents, and Appendix D shows even stronger results among 40+ year olds. Appendix D also shows consistent results with logit.

Some readers may still be concerned that childhood income is endogenous to childhood Medicaid. We emphasise that models 1 and 4 in Table 1 actually show similar magnitude coefficients for Medicaid even when we omit childhood income. Regardless, Appendix D shows two measures of income from early childhood – prior to most of a child's exposure to Medicaid. These income measures are plausibly less endogenous to Medicaid. When childhood income is measured in the first observation or only at ages 0–5, childhood Medicaid has slightly larger coefficients. Thus, Appendix D suggests that, if anything, we may be underestimating the childhood Medicaid coefficient, which buttresses our conclusions.

Critiques have recently emerged of two-way fixed effects (TWFE) models (e.g., Callaway et al., 2021). We emphasise we do not have a traditional TWFE setup as we have panel data (i.e., we use childhood data to predict an adult outcome) but not a panel model (i.e., we only analyse one observation per respondent). Nevertheless, Appendix E addresses concerns regarding TWFE models. The easiest solution is to simply use only one wave of data for the outcome (i.e., the 2019 wave, which was 81.3% of cases). This removes any need for time/wave FEs. The key coefficient remains significant and is even stronger. Next, we employ Jakiela (Jakiela, 2021) tests for negative weights and effect heterogeneity. After getting the "residualized treatment" from a regression of childhood Medicaid on the region and wave FEs, we reestimate model 5 from Table 1 while dropping below mean or negative residualized treatments (i.e., those vulnerable to negative weights). The key coefficient remains significant and is even stronger. Next, we test the treatment effect homogeneity assumption. The significant interaction suggests we should not assume homogeneity. In turn, we conduct two robustness checks where we drop respondents with low residualized treatments (i.e., negative weights): (a) respondents below age 38, and (b) respondents from certain regions. Although the Medicaid coefficient is only significant at p < 0.10 level when dropping younger respondents, its magnitude (0.055) is slightly larger than in Table 1 model 5 (0.050). Further, the coefficient is larger and remains significant when we drop certain regions. Recall, Table 2 already embraces effect heterogeneity by childhood income. Ultimately, we acknowledge that the treatment effect homogeneity assumption is probably too strong for our models. Still, fortunately, all checks suggest our results and conclusions are robust.

5 | CONCLUSION

This study builds on the productive and valuable research programme on Medicaid expansions. We analyse prospective individual-level panel data to investigate the relationship between childhood Medicaid and severe chronic conditions. Building on prior contributions, we use more comprehensive measures of chronic health conditions and observe those outcomes at older ages. Moreover, using longitudinal data for the duration of childhood, we more rigorously adjust for childhood income and family background. This also enables us to test the interaction between childhood Medicaid and childhood income. This uniquely informs how the effects of Medicaid expansion affect children at different income levels, which clarifies how Medicaid expansions influence childhood income disparities in long term health.

Our analysis confirms that Medicaid expansions in the 1980s and 1990s had enduring long-term health benefits. We find childhood Medicaid is robustly significant and negatively associated with severe chronic conditions. A standard deviation greater childhood Medicaid significantly reduces the probability of severe chronic conditions in adulthood by 0.05 to 0.12 (16%–37.5% reduction from mean of 0.32). Across the range of observed childhood Medicaid, the probability of having a severe chronic condition is approximately cut in half. Moreover, greater childhood Medicaid substantially reduces childhood income disparities in severe chronic conditions. At higher levels of childhood Medicaid, we find no significant adult disparities in severe chronic conditions arising from differences in household income during childhood.

In addition to confirming that childhood Medicaid is associated with long-term health benefits in adulthood, this study contributes to several fields. First, our use of prospective individual-level panel data complements and extends the existing Medicaid literature. We raise fair questions about omitted variable bias regarding childhood income and the credibility of the exclusion restriction in instrumental variable models. We acknowledge that our attempt to adjust for relevant confounders also requires strong assumptions. Still, we conjecture that the field is enhanced when multiple studies use different datasets with different strengths and weakness and different modelling approaches with different assumptions. We propose our study contributes novel insights and critical buttressing evidence that is complementary to and reinforcing of other studies on the long term effects of childhood Medicaid (e.g., Boudreaux et al., 2016; Cohodes et al., 2016; East et al., 2023; Lipton et al., 2016; Miller & Wherry., 2019).¹⁷

Second, to the extent our study fortifies that childhood Medicaid has long term benefits, we add to the literature's case that Medicaid's fiscal benefits are large relative to its fiscal costs (e.g., Brown et al., 2020; Goodman-Bacon, 2021). Severe chronic conditions result in enormous public and private healthcare costs, and undermine people's capacity to work, earn and pay taxes. Therefore, that childhood Medicaid substantially reduces severe chronic conditions in adulthood reinforces fiscal arguments for Medicaid.

Third, this study bridges the Medicaid literature and other research programmes. Extensive research shows that the "long arm of childhood" economic circumstances shape adult health. By reducing childhood income disparities, this study shows how that relationship can be moderated by generous social policies. Indeed, our analyses reveal no significant childhood income disparities – that is, no long arm – at higher levels of childhood Medicaid. This guides this literature to also investigate how policies and institutions interact with childhood (and adulthood) circumstances to blunt the long arm of childhood. Just as the long arm literature has studied neighbourhood disadvantage and other contextual factors, the long arm of childhood happens within policy and institutional contexts that exert influence as well.

As mentioned above, there is an implicit affinity between childhood Medicaid measures and prevailing and long established measures in the comparative social policy literature. Our impression is that the Medicaid literature might have underappreciated this connection. We highlight this to note that the broader scholarly enterprise benefits from stronger connections between social policy research concentrating exclusively on the U.S. – a highly unusual case (Brady et al., 2016; Wendt et al., 2009) – and the social policy developments and research occurring across rich democracies. Comparative social policy scholars can also emulate the research designs employed in this Medicaid literature. For instance, scholars could test the long term effects of variation in social policy generosity by linking well-established indices (e.g., Nelson et al., 2020; Scruggs & Tafoya, 2022) to longitudinal individual-level data.

To conclude, American health scholars are increasingly recognising that America's greater morbidity and mortality compared to other rich democracies may partly emanate from social policy differences (Brady, Kohler, & Zheng, 2023). Typically, scholars use cross-national research designs to analyse how generous social policies explain variation in health outcomes across rich democracies (e.g., Beckfield & Bambra, 2016; Reynolds & Avendano, 2018). The Medicaid expansions in the 1980s and 1990s and under the Affordable Care Act were concrete steps towards more generous social policies. This provides a different but useful perspective on how social policies shape crossnational differences. Even within the U.S. case, when the U.S. moves over time towards more generous social \perp Wiley-

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policies, there is convincing evidence of population health improvements. The Medicaid expansions show how U.S. population health could look more like other healthier rich democracies if more generous social policies were implemented.

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CONFLICT OF INTEREST STATEMENT

The authors declare no conflicts of interest.

DATA AVAILABILITY STATEMENT

Our data is the Panel Study of Income Dynamics (PSID) and is publicly available. The PSID however will not let us post the entire dataset as they require registration to access the data. However, we have already published code for building the dataset, and for merging our data with the Cross-National Equivalent File. We also provide a codebook for the data file. We will publish our entire code for data management and analyses as well.

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ENDNOTES

- ¹ Of course, we can include obesity into a chronic condition index. We view obesity as a risk factor or health behaviour (e.g., like smoking and drinking) more than a chronic condition. If we include obesity into an index of any chronic condition, fully 62% of the sample would be positive. Appendix C shows obesity as a separate outcome. If we include obesity in an index (not shown), childhood Medicaid is statistically significant and negative (b = -0.06, t = -2.6).
- ² East et al. (2023) innovatively analyse how prenatal and at birth exposure to the 1980s expansions influences the health and well-being of those respondents' subsequent infant children's health. Arguably, that is an even longer-term consequence and at similarly older ages. Still, we observe respondents' *own* health at older ages than prior studies.
- ³ For example, Brown et al. (2020) do not observe respondents during ages 0–12 and impute from the first observation at ages 12–15. Boudreaux et al. (2016) only analyse children at ages 0–5 and drop those only observed ages 13 +.
- ⁴ This is unlike studies examining how Medicaid expansions affect current child outcomes (e.g., Currie et al., 2008; Curie & Gruber, 1996a, Curie & Gruber, 1996b; Jackson et al., 2021). Even when such studies include income however, they use crude bracketed income measures (e.g., Curie & Gruber, 1996a, Curie & Gruber, 1996b). This is also unlike the long arm literature that normally uses surveys of mature adults with retrospective reports on childhood (e.g., Hayward & Gorman, 2004; Turner et al., 2016). On balance, the long arm literature rarely incorporates social policies.
- ⁵ The official poverty measure (OPM) occasionally used in this literature (often to predict Medicaid eligibility) is not a valid and reliable measure of income (Brady et al., 2018; Rainwater & Smeeding, 2003). The OPM thresholds are far too low and the family size adjustments are incoherent. The OPM's definition of income inconsistently includes some transfers (e.g., Temporary Assistance for Needy Families) but ignores taxes, tax credits (e.g., the Earned Income Tax Credit) and other transfers (e.g., Supplemental Nutritional Assistance Programme). Because the package of tax credits and transfers changed greatly over time, these omissions undermine both validity and reliability.

- ⁶ Brown et al. (2020: 813) write: "Controlling for income attenuates the OLS estimates, but they remain negative." Their pertinent Online Appendix 12 only shows the results graphically and only for taxes paid (but not other outcomes). Their appendix actually shows a dramatic decline in the size and significance of the Medicaid coefficient in both OLS and reduced form models. They suspect (2020: pp. 798-799) but provide no evidence that this is because income at age 15 is posttreatment control for Medicaid eligibility at younger ages.
- ⁷ For example, Brown et al. (2020) interact with the official poverty measure, which we explain is an invalid and unreliable income measure in en. 5 (recall also, they only observe childhood starting at ages 12–15 and impute ages 0–11). Jackson et al. (2021) interact with mother's education. Curie & Gruber, 1996a, Curie & Gruber, 1996b interact with a crude measure of non-equivalized pre-fisc income in bracketed categories (see en. 4). Boudreaux et al. (2016) interact with Medicaid eligibility based on the official poverty measure (see en. 5).
- ⁸ Another advantage of ending the observation of childhood in 1995 is that the PSID becomes biannual soon after in 1997. Hence, including years after 1997 would make the observation of childhood inconsistent.
- ⁹ This also reduces sample attrition as we only require one observation at age 30+ regardless of when. For instance, if death occurs by 2019 (~5% of sample), we can use the last observation.
- ¹⁰ Although the PSID does not allow us to measure and instrument for actual eligibility, there is precedent for using reduced form models (Jackson et al., 2021). Every study we have read that shows reduced form alongside instrumental variable models finds nearly identical estimates (e.g. Brown et al. 2020; Goodman-Bacon, 2018, 2021; Miller & Wherry., 2019; Thompson, 2017).
- ¹¹ Brady et al. (2022) also test nonlinear income effects with polynomials and logs. They find linear income fits as well or better than nonlinear income for all mature health outcomes.
- ¹² Twelve states have fewer than ten respondents, and six states have fewer than five respondents. By contrast, the nine smaller regions contain 154, 156, 455, 461, 468, 472, 527, 905, and 1072 respondents. We find larger and more significant childhood Medicaid coefficients when we use four larger region FEs rather than the nine smaller regions. For precedent, Miller and Wherry. (2019) also use region FEs.
- ¹³ As is well-documented, long-term PSID samples mainly include Black and White individuals (Brady et al., 2020). We found consistent results when collapsing Latinos into Other Race.
- ¹⁴ We find even more statistically significant effects for childhood Medicaid eligibility if we follow most PSID research (e.g., Duncan et al., 2012) and cluster standard errors at the original PSID household-level (Abadie et al., 2017).
- ¹⁵ It is unlikely that the individual-level controls are posttreatment control for childhood Medicaid. Race and sex are mostly immutable to policy. Parents' education and age are established mostly before a child's exposure to Medicaid. Childhood sibship size and single motherhood are averages across childhood and theoretically could change in response to Medicaid. However, these two are never significant predictors so are unlikely to be salient mediators. Appendix B also shows that childhood Medicaid is weakly associated with other variables except the dependent variable, parents' education, and age (which it should be correlated with as childhood Medicaid changed over time). Also, childhood income is more likely a confounder than a mediator as our income measure does not monetize Medicaid.
- ¹⁶ Of course, childhood income is likely posttreatment control for parent's education and other individual-level controls (Brady et al., 2022). Still, in model 4, the x-standardised coefficient for childhood income (-0.04) is larger than the x-standardised coefficient for parent education (-0.03), and the coefficient for being Latino (-0.01).
- ¹⁷ For comparison, we find 32% of our 30–56 year olds have a severe chronic condition, while Thompson (2017) find 5.8% of 18–20 year olds have any chronic condition, and Miller and Wherry. (2019) find 24.6% of 19–36 year olds have any chronic condition. It is difficult to compare effect magnitudes across studies. However, Thompson does not find robustly significant effects on any chronic condition. Medicaid eligibility is not significant in OLS, reduced form or instrumental variable models (Thompson, 2017; Table 2). Thompson finds a significant negative effect for eligibility at ages 0–5 and 6–11, but not 12–18 (Thompson, 2017;Table 4). His robustness checks show a significant negative effect in only two of eight models (Thompson, 2017;Table 5). Miller and Wherry find significant negative effects of prenatal and age 5–9 eligibility for any chronic conditions. They have more than 10 times as many cases with the National Health Interview Survey, which may account for why they find significant negative effects for diabetes and high blood pressure whereas we only find near significant negative effect for their chronic condition index among the low income but not the moderate income (Boudreaux et al., 2016; Table 2). Boudreaux et al., 2016; Table 4).

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APPENDIX A: DESCRIPTIVE STATISTICS (N = 4670)

	Mean	SD
(1) Severe chronic condition	0.32	0.47
(2) Childhood income (average %)	45.65	22.92
(3) Parents' education (years)	12.81	2.47
(4) Parents' age (years)	36.80	6.69
(5) Childhood sibship size (#)	1.48	0.99
(6) Childhood single motherhood (proportion)	0.15	0.26
(7) Age (years)	43.14	7.97
(8) Black	0.14	0.34
(9) Latino	0.00	0.03
(10) Other race	0.04	0.19
(11) Woman	0.50	0.50
(12) 1970s cohort	0.36	0.48
(13) 1980s cohort	0.29	0.45
(14) Childhood medicaid eligibility (years)	1.22	0.52
(15) Childhood State unemployment (average rate)	6.88	1.37
(16) Childhood State GSP PC (real \$)	42109.68	6924.11
(17) Childhood State Govt. spending (average %)	10.00	1.60
(18) Childhood State unionisation (%)	20.48	8.47

APPENDIX B: CORRE	LATION	MATRI)	K (N = 46	(02														
Variables	(1)	(2)	(3)	(4)	(5)	(9)	(2	(8)	(6)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
(1) Severe Chronic Condition	1.00																	
(2) Childhood Income	-0.09	1.00																
(3) Parents' Education	-0.16	0.60	1.00															
(4) Parents' Age	-0.01	0.46	0.16	1.00														
(5) Childhood Sibship Size	0.06	-0.35	-0.28	-0.12	1.00													
(6) Childhood Single Motherhood	0.03	-0.51	-0.24	-0.38	0.08	1.00												
(7) Age	0.26	0.01	-0.24	0.11	0.23	-0.03	1.00											
(8) Black	0.08	-0.51	-0.36	-0.27	0.26	0.45	-0.01	1.00										
(9) Latino	-0.01	0.01	0.02	0.00	0.00	-0.01	0.01	-0.02	1.00									
(10) Other Race	0.01	-0.03	-0.05	0.01	-0.01	-0.01	0.00	-0.15	0.00	1.00								
(11) Woman	0.01	-0.05	-0.06	-0.03	0.04	0.10	0.06	0.05	-0.02	0.00	1.00							
(12) 1970s Cohort	0.03	0.00	0.04	-0.12	-0.11	-0.05	0.12	-0.08	0.01	-0.02	-0.01	1.00						
(13) 1980s Cohort	-0.18	-0.03	0.17	-0.06	-0.11	0.07	-0.74	0.08	-0.02	0.01	-0.01	-0.61	1.00					
(14) Childhood Medicaid Eligibility	-0.24	0.05	0.28	-0.11	-0.20	0.03	-0.79	-0.01	-0.03	0.01	-0.04	-0.14	0.71	1.00				
(15) Childhood State Unemployment	0.11	-0.03	-0.12	-0.05	0.08	0.05	0.38	0.04	-0.01	0.03	0.03	0.21	-0.42	-0.40	1.00			
(16) Childhood State GSP PC	-0.18	0.17	0.25	-0.01	-0.14	0.00	-0.50	-0.07	-0.01	0.03	-0.04	-0.17	0.50	0.65	-0.33	1.00		
(17) Childhood State Govt. Spending	-0.08	-0.06	0.03	-0.03	-0.03	0.04	-0.32	0.06	-0.03	0.02	0.01	-0.12	0.32	0.36	0.13	0.04	1.00	
(18) Childhood State Unionisation	0.02	0.22	0.07	0.05	0.03	-0.08	0.35	-0.29	-0.01	0.01	0.00	0.05	-0.32	-0.16	0.46	0.12	0.00	1.00

Note: The correlations are based on standardised values of state-level variables.

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	Severe chronic conditions	High blood pressure/ Heart disease	Cancer	Diabetes	Lung disease	Less severe chronic conditions	Arthritis	Asthma	Other chronic condition	Obesity
Childhood	-0.050*	-0.022	-0.013	-0.012	-0.023**	-0.055*	-0.025+	-0.020	-0.007	-0.02
Medicaid eligibility	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
\mathbb{R}^2	0.109	0.110	0.030	0.066	0.039	0.061	0.077	0.025	0.026	0.068
z	4670	4670	4669	4670	4670	4602	4669	4669	4589	4587
% Positive for (Chronic Conditions									
Weighted %	31.86	23.91	4.43	8.57	4.01	33.42	13.09	13.21	16.16	33.49
Unweighted %	29.96	23.02	3.47	7.11	4.09	29.79	10.62	13.11	12.99	36.87
Vote: Models inc	lude all variables and	fixed effects from model 5 of	^c Table 1. Ro	obust cluster	ed standard e	rrors in parentheses. *** p	< 0.001, ** <i>p</i>	< 0.01, * <i>p</i> <	: 0.05.	

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APPENDIX D: ROBUSTNESS CHECKS: REGRESSION MODELS OF SEVERE CHRONIC CONDITIONS ON CHILDHOOD MEDICAID ELIGIBILITY AND CONTROLS

	Only reference persons	Woman	Man	Ages 40+	Logit	Childhood incomein 1st Obsv.	Childhood income Ages 0–5
Childhood	-0.051	-0.058*	-0.044	-0.066*	-0.312^{*}	-0.56*	-0.060*
Medicaid eligibility	(0.03)	(0.03)	(0.03)	(0.03)	(0.14)	(0.02)	(0.02)
R2	0.126	0.119	0.125	0.095		0.111	0.092
Ν	3306	2540	2130	2385	4668	4614	4111

Note: Models include all variables and fixed effects from model 5 of Table 1. Robust clustered standard errors in parentheses. ***p < 0.001, **p < 0.01, *p < 0.05.

APPENDIX E: ROBUSTNESS CHECKS INSPIRED BY RECENT ECONOMETRICS OF TWO-WAY FE MODELS

	Only 2019 wave	Omitting below mean residualized treatment cases	Omitting negative residualized treatment cases	Residualized outcome on residualized treatment* childhood Medicaid eligibility	Drop below age 38	Drop regions with low residualized treatment
Childhood medicaid eligibility	-0.074** (0.02)	-0.066* (0.03)	-0.057* (0.02)	0.013** (0.004)	-0.055 (0.03)	-0.064* (0.03)
Residualized treatment				-0.068*** (0.014)		
Childhood medicaid eligibility* residualized treatment				-0.032*** (0.008)		
R ²	0.111	0.079	0.111	0.149	0.094	0.100
Ν	3612	1595	3723	4670	2819	2842

Note: Models include all variables and fixed effects from model 5 of Table 1. Robust clustered standard errors in parentheses.

*** p < 0.001, ** p < 0.01, * p < 0.05.

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APPENDIX F: PSID QUESTIONS AND DATA AVAILABILITY ON MEDICAID

In 1999, the PSID began asking: (a) whether anyone in the household is covered by health insurance; (b) what kind of health insurance each member has; (c) with multiple options for each member; (d) with Medicaid as a potential answer; and for heads (e) whether the head applied for Medicaid; and (f) reasons for being denied Medicaid. From 1986 to 1997, respondents were asked if they or *any other* family members have "Medicaid/Medi-Cal/Medical Assistance/Welfare/Medical Services". In 1979–1981 and 1983–1984, respondents were asked whether *anyone* in the family received medical care paid for by Medicare *or* Medicaid. In 1977–1978, respondents were asked if *anyone* in the family received healthcare paid for by Medicaid. From 1969 to 1972, respondents were asked if they could get "free medical care as a veteran, through Medicaid, *or any other way*.



APPENDIX G

FIGURE G1 Distribution of Childhood Medicaid Eligibility in Years in analytical sample across periods.

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