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The Long Arm of Prospective Childhood Income for Mature Adult Health in the United States

David Brady[1,2], **Christian Guerra**[1], **Ulrich Kohler**[3], **Bruce Link**[1]

[1]University of California, Riverside, CA, USA

[2]WZB Berlin Social Science Center

[3]University of Potsdam, Potsdam, Brandenburg, Germany

Abstract

Pioneering scholarship links *retrospective* childhood conditions to mature adult health. We distinctively provide critical evidence with *prospective* state-of-the-art measures of parent income observed multiple times during childhood in the 1970s to 1990s. Using the Panel Study of Income Dynamics, we analyze six health outcomes (self-rated health, heart attack, stroke, life-threatening chronic conditions, non-life-threatening chronic conditions, and psychological distress) among 40- to 65-year-olds. Parent relative income rank has statistically and substantively significant relationships with five of six outcomes. The relationships with heart attack, stroke, and life-threatening chronic conditions are particularly strong. Parent income rank performs slightly better than alternative prospective and retrospective measures. At the same time, we provide novel validation on which retrospective measures (i.e., father's education) perform almost as well as prospective measures. Furthermore, we inform several perennial debates about how relative versus absolute income and other measures of socioeconomic status and social class influence health.

Keywords

health disparities; income; life course; social class

Pioneering scholarship demonstrates a relationship between *retrospective* childhood conditions and mature adult health. Mounting evidence of a "long arm of childhood" shows that early life conditions have long-term consequences for health much later in life (e.g., Ferraro, Schafer, and Wilkinson 2015; Hayward and Gorman 2004; Lee and Ryff 2019; Montez and Hayward 2014; Turner, Thomas, and Brown 2016; Warner and Hayward 2006; Yang et al. 2020). This literature informs enduring theoretical debates on pervasive socioeconomic disparities in health (Case, Lubotsky, and Paxson 2002; Elo 2009; Marmot 2005). These studies also advance a core theme of life course research that childhood conditions place people on trajectories that shape life chances and health

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Corresponding Author: David Brady, School of Public Policy, University of California, INTS 4133, 900 University Ave., Riverside, CA 92521, USA. dbrady@ucr.edu.

The second through fourth authors are listed alphabetically to denote equal contributions.

SUPPLEMENTAL MATERIAL

Appendices A through J are available in the online version of the article.

(Melchior et al. 2007; Montez et al. 2016; Poulton et al. 2002; Wilson, Shuey, and Elder 2007). Moreover, these studies illustrate how childhood conditions are fundamental causes of health—operating as a distal cause that puts people at risk of risks for adverse health (Clouston and Link 2021; Link and Phelan 1995).

This literature has been innovative for at least two additional reasons. First, rather than linking childhood economic resources to proximate young adult health (e.g., Duncan et al. 2012; Duncan, ZiolGuest, and Kalil 2010; Lillard et al. 2015; Melchior et al. 2007; Poulton et al. 2002; Ziol-Guest, Duncan, and Kalil 2009), these studies show much longer-term consequences for older adults. Second, rather than the well-established connection between adult economic resources and adult health (e.g., Adler et al. 1994; Avendano and Glymour 2008; Chetty et al. 2016; Dupre et al. 2012), these studies show that some of the causal origins are from much earlier in life. In turn, childhood economic conditions can now serve as an early warning signal for health problems in mature adulthood. Moreover, public policy interventions can be more effectively targeted if we can gain an understanding of precisely how childhood economic resources matter.

Despite these contributions, the literature has been constrained by two key limitations. First, much of the literature relies on retrospective self-reports of childhood socioeconomic conditions from older adult respondents. Second, relatedly, the measurement of childhood economic resources could be improved.

The present study explicitly addresses these limitations while deepening understanding of the long arm of childhood. We analyze the Panel Study of Income Dynamics (PSID), which provides a large, nationally representative panel observed multiple years during both childhood and mature adulthood. Uniquely, we incorporate the Cross-National Equivalent File (CNEF), which is a supplement of the PSID that provides enhanced state-of-the-art measures of income. This enables us to combine better measures of income observed multiple times during a respondent's childhood with long-term follow-up for multiple health outcomes during mature adulthood. Compared to prior PSID research, we exploit more recent waves to observe health later in respondents' lives and to include a larger sample of children from the 1980s and 1990s who have now aged into mature adulthood. Moreover, we incorporate a wide variety of health outcomes. By doing so, we provide an even more robust test of the influence of childhood income. This approach provides critical and novel evidence for the long arm of childhood with prospective childhood income data. In the process, we inform several perennial debates about how relative versus absolute economic resources and social class influence health.

BACKGROUND

As previewed previously, many researchers link retrospective measures of childhood to mature adult health. For instance, Hayward and Gorman (2004) find that childhood economic disadvantage predicts mortality among 45- to 75-year-olds (see also, Montez and Hayward 2014; Warner and Hayward 2006). Those authors use retrospective questions from National Longitudinal Survey of Older Men when respondents were 45 to 59 years old about family circumstances when respondents were age 15. Specifically, they examine retrospective measures of parents' education, occupation, family structure, and nativity.

Several studies analyze retrospective self-reports of childhood among 25- to 74-year-olds in the National Survey of Midlife Development in the U.S. These studies integrate various self-reported indicators about childhood, such as parental unemployment, housing conditions, and subjective economic standing. For instance, Lee and Ryff (2019) use latent class analysis to scale adults' reports of multiple adverse childhood experiences. Ferraro and colleagues (2015) show childhood economic disadvantage significantly increases disease and undermines healthy aging among 25- to 74-year-olds (see also, Schafer and Ferraro 2011; Schafer, Ferraro, and Mustillo 2011). Morton, Mustillo, and Ferraro (2014) show that recollections of having received welfare or being financially worse off predicts acute myocardial infarction.

Others use retrospective self-reports of childhood in the Health and Retirement Study (Morton and Ferraro 2020). Again, composite scales have been constructed based on various indicators (e.g., whether father had a white- or blue-collar job; the family was "pretty well-off, about average, or poor"; financial difficulty caused the family to move; etc.; e.g., Hamil-Luker and O'Rand 2007). For instance, Luo and Waite (2005) find that retrospective adult self-reports of childhood conditions predict self-rated health, functional limitations, chronic conditions, and depression among 51- to 75-year-olds.

A smaller literature uses prospective administrative or survey data when respondents were both children and mature adults (e.g., Gilman et al. 2002; Kauhanen et al. 2006; Link et al. 2017; Martikainen et al. 2020; Moorman, Greenfield, and Garcia 2019; Yang et al. 2020). For example, the 1958 British birth cohort National Child Development Study includes data on respondents ages 11 to 16 (e.g., mother's employment, father's occupation, subjectively perceived financial situation, self-reported household amenities; e.g., Jefferis, Power, and Hertzman 2002). The Medical Research Council National Survey of Health and Development has followed respondents from birth in 1946 in Great Britain. Using these data, Kuh and colleagues (2002) predict mortality ages 26 to 54 based on father's manual versus nonmanual social class measured at age 4 and subjective evaluations and interviewer scores of housing and living conditions. Using the California-based Child Health and Development Study, Link and colleagues (2017) show that a composite of prospective maternal education, paternal occupation, and family income in childhood is associated with self-rated health at age 50.

Others use the PSID to examine how children's economic resources in the 1960s to 1970s affect adult health (e.g., Duncan et al. 2012; Laditka and Laditka 2019; Vartanian and Houser 2010; Zheng, Dirlam, and Echave 2020; Ziol-Guest et al. 2009). For instance, Shuey and Wilson (2014) analyze how trajectories of official U.S. poverty beginning with ages 0 to 8 in 1968 influence trajectories of self-rated health at ages 42 to 49 in 1999 to 2009 (see also, Wilson et al. 2007). Using the PSID through 2003 and 2007, Johnson and Schoeni (2011) examine how family income and poverty at ages 13 to 16 in 1968 to 1975 influence self-rated health, asthma, hypertension, diabetes, stroke, heart attack, and heart disease at ages 39 to 52 or 39 to 56.

Improving the Measurement of Childhood Economic Resources

The prior literature has made substantial contributions. However, it has been constrained by two key limitations that our study distinctively addresses: Most have relied on (a) retrospective data and/or (b) imprecise measures of childhood economic resources. Even among those using prospective data, the measurement of economic resources could be improved, and there is a paucity of nationally representative samples of the United States with its well-known heterogeneities. Therefore, it would be valuable to have strong evidence from prospective nationally representative data (Elo 2009).

Previous research demonstrates that much can be learned from retrospective reports. However, retrospective reports of childhood conditions several decades later are prone to recall bias and are unlikely to be as strong as prospective measures (Song and Mare 2014). For instance, adults' retrospective reports are often inaccurate of whether their family was ever temporarily on welfare when they were young (Robins et al. 1985). Such error is common even with major adverse experiences in childhood (Hardt and Rutter 2004). Plausibly, sicker and distressed mature adults may reflect their poor health back into their memories of childhood. Poor health could undermine memories or even retrospectively construct negative narratives of childhood to account for present health problems (Schacter 2001). Such biases could inflate the relationship between childhood economic resources and mature adult health. Furthermore, such biases could be exacerbated when childhood conditions are measured subjectively (e.g., reports that one's family was "poor" or "well-off"). By contrast, prospective measures of childhood living conditions better predict adult mortality than retrospective questionnaires about childhood economic standing (Kauhanen et al. 2006). Given all this, our first contribution is to assess prospective measures and, in the process, compare prospective and retrospective measures.

Our second contribution is to improve the measurement of economic resources. In the long arm literature, many researchers construct composite scales of general childhood conditions or disadvantage. Even in the few prospective studies—largely because economic resources were often not as well measured in the 1940s to 1970s when the data were originally collected—childhood economic resources are often fairly crudely measured. Because composite scales incorporate many different indicators of varying quality, it remains uncertain which specific aspects of childhood economic resources are most salient. Moreover, if childhood disadvantage is a composite of many indicators, intervening on any one or a few indicators will not necessarily improve subsequent health if other indicators in a composite are what really matters. Also, it would be extremely difficult to intervene on some widely used indicators, such as father's occupation. By contrast, childhood income has the advantage of being more precise and concrete conceptually and provides a plausible basis for public health interventions.

For these reasons, we propose that the long arm literature can benefit from advances in international income research. Following the United Nations's (2011) *Canberra Group*, a consensus emerged on the criteria for optimal income measurement (Brady et al. 2018, 2020; Brady and Parolin 2020; Daly et al. 2002; Duncan and Petersen 2001; Rainwater and Smeeding 2003; Smeeding and Weinberg 2003). Measures of income should (1) include all sources from all household (HH) members, (2) incorporate taxes and transfers (i.e., be “post-fisc”), (3) be equivalized for HH size, and ideally, (4) include observations over multiple time points.

These criteria emphasize that individuals reside in HHs and have “disposable” income after taxes and transfers, and this measure best gauges economic resources. Living in HHs and accessing transfers are principal ways individuals smooth their incomes, manage volatility, and maximize well-being (Bartfeld et al. 2015; Brady et al. 2018; Gundersen and Ziliak 2003). For example, Hoynes, Schanzenbach, and Almond (2016) show that the Supplemental Nutritional Assistance Program (SNAP) improves children’s well-being and has lasting benefits into adulthood. As well, tax credits and transfers reduce poverty and inequality (Brady and Parolin 2020; Rainwater and Smeeding 2003). Given the volatility of income, the reliability of income measurement increases significantly with multiple observations (Brady et al. 2018; Fox, Torche, and Waldfogel 2016; Mazumder 2016). Multiple observations also reveal much greater intergenerational transmission of income and, hence, lower mobility (Brady et al. 2020; Fox et al. 2016; Mazumder 2016). Furthermore, multiple observations of childhood income better predict subsequent health, well-being, and life chances than single observations (Blau 1999; Brady et al. 2020; McLeod and Shanahan 1996).

Such measures of post-fisc HH equivalized income better predict adult life chances and better explain black–white inequalities in adult life chances (Brady et al. 2020). Also, Brady and colleagues (2018) demonstrate that this measure in one randomly chosen year explains about half of the variation in lifetime average “permanent” income. They also show that multiple time points of this measure can explain most of the variation in permanent income. Recall that several studies in the long arm literature rely on measures of parents’ occupation or subjective social class. Brady and colleagues (2018) demonstrate that post-fisc equivalized income predicts permanent income better than one-, two-, and three-digit occupation and the widely used Erikson-Goldthorpe measure of social class.

Prior studies in the long arm literature have not incorporated prospective post-fisc equivalized income. Even among prior PSID studies, studies have not used these optimal income measures (Daly et al. 2002). For instance, although Duncan and colleagues (2010:309) use the PSID’s “high quality edited measure of annual total family income,” their measure includes only earnings and investment income and cash transfers like TANF. By contrast, we incorporate taxes, tax credits, and near-cash transfers like SNAP. Innovating beyond such research with the PSID, we also equivalize for HH size.

Our third contribution is to inform several perennial debates about how best to measure and model how economic resources and social class shape health (Elo 2009). Importantly, prospective measures make it far more feasible to contribute to these debates than analyses based solely on composite indices and retrospective measures.

First, there has long been a debate between relative and absolute income (Elo 2009; Fox et al. 2016). Relative income locates one relationally and hierarchically within a distribution of a time and place. Income is a positional good and a basis for status compared to others in a cultural and historical context (Marmot 2005). The living standards in one’s context shape what gets defined as a "need," and therefore having sufficient resources to meet one's needs is always relative (Rainwater and Smeeding 2003). As incomes rise and with economic development, health will not necessarily improve if one's standing among others declines or is stagnant (Avendano 2012; Easterlin et al. 2010). By contrast, absolute income measures resources independent of time and place. Health and well-being should mechanically improve as absolute incomes increase (Stevenson and Wolfers 2013) and with economic development (Deaton 2013). If absolute income is more important than relative income, then health and well-being depend on one's fixed material resources regardless of others' material resources in a community or society (Ladin, Daniels, and Kawachi 2010; Veenhoven 1991).

Second, debate has long existed about the salience of income compared to other measures of socioeconomic status or social class, such as occupation and education (Brady et al. 2020), and how those different measures influence health (e.g., Daly et al. 2002; Elo 2009; Geyer et al. 2006; Herd, Goesling, and House 2007; Schnittker 2004). Third, if childhood income influences mature adult health, it would be useful to investigate the optimal functional form (Elo 2009). For example, using the PSID, Duncan and colleagues (2010) show there are differential impacts of increments of low versus high childhood income for outcomes in respondents' 30s. As a result, we also test whether the relationship between childhood income and mature adult health is nonlinear.

DATA AND METHODS

We used the Panel Study of Income Dynamics (PSID 2013). The PSID is a longitudinal, nationally representative study fielded annually from 1968 to 1997 and biannually since 1997. We also used the Cross-National Equivalent File, which is a supplement to the PSID (Frick et al. 2007). The CNEF distinctively provides the higher quality income measures incorporating taxes, tax credits, and transfers (Brady et al. 2020; VanHeuvelen and Brady 2021).

The sample was based on individuals who were children (0–17 years) in households interviewed 1970 to 1996 and subsequently followed until mature adulthood. To be included, we required that the respondent have valid income data at least twice during childhood and had valid health data at 40+ years old. The timing for our study was enhanced by the fact that a large sample of PSID children in the 1980s to 1990s had recently aged into mature adulthood. We selected the oldest/most recent observation for each respondent through the 2019 wave.[1] Thus, we used observations from the 1999 to 2019 waves even though most came from the 2019 wave.

Across outcomes, sample sizes ranged from 3,813 to 3,944. Appendix I in the online version of the article confirms that the results and conclusions were robust if we confined the analyses to a common N nonmissing on all outcomes. The descriptive statistics and a

1. This reduced sample attrition because we required only one observation at age 40+ regardless of when it was observed. If we focused only on respondents age 40+ in the 2019 wave instead of the last available wave for each respondent 40+, the sample size would be roughly 20% smaller. Also, if death occurred after age 40, we could use the last available observation. About 5% of the potential sample (i.e., children 0–17 in 1970–1996 waves with two income observations) died by 2019.

correlation matrix are displayed in Appendix A in the online version of the article. The mean age was 52 years old. About 10% of the sample was 62 to 65 years old, about 25% was 58 to 65, and only about 25% was 40 to 46 years old. In sensitivity analyses in the following, we report analyses based on samples of participants 30+ and 50+ years old.

Adult Health Outcomes

Recent PSID waves contained a variety of mature adult health outcomes. To the best of our knowledge, past studies have not used the most recent waves and/or included the full variety of health outcomes. One advantage of our approach is that we used both subjective assessments of well-being and objective reports of physician diagnosis. Whereas physician diagnosis requires access to health care and respondent recall, subjective assessments are also imperfect. We were most interested in robust relationships across both sets of outcomes.

We note that the health outcomes are self-reports for heads of households but proxy reports for spouses. Proxy reporting was a limitation, and Appendix F in the online version of the article shows results dropping proxy reports. Also, some readers may be concerned with including multiple adults per household, so Appendix F in the online version of the article includes only one adult per household (i.e., heads). Fortunately, dropping proxy reports and sampling only heads of household did not change our conclusions. So, we included both self-reports and proxy reports in the main analyses.

For general health status, we used the standard 5-point self-rated health item (Benyamini and Idler 1999; Brady et al. 2020; Johnson and Schoeni 2011; Schnittker and Bacak 2014): *self-rated health* (1 = poor, 2 = fair, 3 = good, 4 = very good, 5 = excellent). The mean self-rated health was 3.3.

We measured whether the respondent ever reported that a doctor had diagnosed that he or she had experienced each of two major acute health events: *heart attack* or *stroke* (Brady et al. 2020). Eleven percent of the sample had a heart attack, and 5% had experienced a stroke.[2]

The PSID provided a battery of self-reports of whether a doctor has diagnosed the respondent as having any chronic conditions. We divided seven chronic conditions into *life-threatening* and *non-life-threatening*.[3] Life-threatening included high blood pressure/heart disease, cancer, diabetes, and lung disease. Non-life-threatening included asthma, arthritis, and "other" chronic conditions. In the sample, 46% had at least one life-threatening chronic condition, and 41% had a non-life-threatening chronic condition.

Finally, *psychological distress* was scored 0 to 24 using Kessler and colleagues' (2002) K-6 non-specific scale (Brady et al. 2020; Duncan et al. 2010). The mean was 3.4.

2.In analyses available on request, we found similar results when combining these two into a general measure of acute health events. The Centers for Disease Control and Prevention (2019) shows our estimates are similar to national estimates in 2017 to 2018 for 45- to 64-year-olds for heart disease/attack (11.8%) and stroke (3.2%). Although we include cancer among life-threatening chronic conditions, 8.0% of our sample had cancer, which is similar to the Centers for Disease Control and Prevention estimate of 7.1%.

3.In analyses available on request, we combined all seven conditions into one measure, which yielded results similar to those for life-threatening chronic conditions. More than 60% of the sample has at least one of these conditions, and there is considerable comorbidity.

Childhood Economic Resources

To optimize measurement of parent income, we used the CNEF to measure post-fisc equivalized household income. Post-fisc includes cash and near cash transfers (e.g., SNAP), subtracts taxes (with the National Bureau of Economic Research's "TAXSIM" Model), and adds tax credits (e.g., the Earned Income Tax Credit). Our measure includes all HH members. We equivalized for HH size by dividing by the square root of HH members (Brady et al. 2018; Rainwater and Smeeding 2003).

We averaged this measure over a respondent's entire observed childhood (i.e., ages 0–17). Recall, we required respondents to have at least two income observations. In our sample, the mean respondent had about 11 observations. More than 95% of the sample had three or more observations.

Our principal measure was *parent income rank*. We used the PSID-CNEF to calculate relative rank as percentiles in each year. Fortunately, the PSID-CNEF had a large sample (typically >20,000) in each year, and the weights made the data set nationally representative. For each respondent, we then averaged their relative rank percentiles over childhood (i.e., ages 0–17).

In subsequent models, we included alternative measures of childhood economic resources. First, we measured *parent absolute income* (in inflation-adjusted 2021 dollars). Alternative analyses transformed both relative rank and absolute income into logs and polynomials. Second, we estimated the models while adding *two-digit occupations*. Third, we included two sets of retrospective childhood measures available in the PSID. Father's educational attainment was measured categorically as *father has college degree* and *father has high school degree* (reference = less than high school degree). Subjective retrospective family economic standing was measured with binary variables for *parents were poor* and *parents were well-off* (reference = neither poor nor well-off).

Control Variables

In addition to prospective parent income, we included four other prospective measures of family background observed during childhood.[4] These measures were also averaged over the entire set of observations during childhood (Brady et al. 2020). Rather than simply adopting the PSID identified head, we defined the HH lead earner as the adult with the highest labor market earnings in the HH in a given year (Brady et al. 2018). Ties were broken by age (and chosen randomly if age is tied). Across the respondent's observed childhood, we used the lead to calculate the average *lead age* in years and average *lead education* in years of schooling. We also adjusted for *sibship size*, which was the average number of other children in the HH during childhood. Finally, we included *parent single motherhood* as the proportion of years in a single-mother HH during childhood.

4. By including these other family background characteristics, we acknowledge that family background embodies a host of health benefits, many of which are not strictly economic (Elo 2009; Geyer et al. 2006; Herd, Goesling, and House 2007; Schnittker 2004). Still, we caution against comparing the coefficients of these characteristics against those for parent income because parent income is posttreatment to and mediates some of the effects of these characteristics (e.g., Elo 2009).

The average respondent grew up with a parent with 12 years of schooling and a lead average age of 39 years old during their childhood. The mean respondent had a sibship size of 1.8. On average, about 14% of years during childhood were in single-mother HHs.

In the wave when health outcomes were measured, we adjusted for the *age* of the respondent in years (*M* = 52). We also included mutually exclusive binary measures of whether respondents were *black, Latino*, or *other race* (reference = white). Approximately 14%, less than 1%, and 4% of the sample were black, Latino, and other race individuals. As is well documented for long-term PSID samples, the PSID only effectively samples black and white individuals (Brady et al. 2020). Finally, we controlled for a binary indicator for *female* (51%).

Analysis

The analyses were regression models, and the unit of analysis was the individual. The models predicted recent health outcomes (observed at 40+ years) as a function of income and other variables during childhood (observed 0 to 17 years) and a few contemporaneous controls (observed at 40+ years). All models were linear. This included the binary outcomes, which were linear probability models. In analyses available on request, we estimated logit models, and the results led to the same conclusions. Because we used the oldest/most recent observation for each respondent and those observations can come from the 1999 to 2019 waves, our analyses included fixed effects for the wave when the outcome was observed (not shown). The wave fixed effects applied to the observation of mature adult health (not childhood) and netted out any unusual qualities of any given year. Because HHs included multiple mature adult respondents and multiple adults who grew up in the same HHs, we used robust standard errors clustered at the original PSID HH level.

RESULTS

Table 1 shows the main models for the six outcomes. The models include all controls and wave fixed effects (not shown). The tables report standardized coefficients (stdBs) for continuous outcomes and x-standardized coefficients (xstdBs) for binary outcomes.[5]

Parent income rank has a statistically significant relationship with five of six outcomes: self-rated health, heart attack, stroke, life-threatening chronic condition, and psychological distress. Parent income rank does not significantly predict non-life-threatening chronic conditions. Overall, it appears that parent income rank is more strongly associated with the most serious health outcomes, including life-threatening chronic conditions and acute health events.

Parent income rank has similar coefficients for the continuous outcomes self-rated health and psychological distress. For a standard deviation increase in parent income rank (i.e., 24 percentage points; see Appendix A in the online version of the article), self-rated health is expected to be about .1 *SD* higher, and psychological distress is about .09 *SD* lower.

5. We report standardized coefficients to facilitate comparison of the substantive magnitude of coefficients across outcomes and across independent variables.

Compared to a sample mean of 46%, a standard deviation increase in parent income rank is expected to reduce the probability of a life-threatening chronic condition by about 4.8 percentage points. Thus, a standard deviation higher parent income rank translates to about a 10% reduction in the probability of a life-threatening chronic condition.

Based on a sample mean of about 11%, a standard deviation increase in parent income rank is expected to reduce the probability of a heart attack by about 2.2 percentage points. This translates to roughly a 20% reduction in the probability of a heart attack. Relative to a sample mean of about 5%, a standard deviation increase in parent income rank is expected to reduce the probability of a stroke by about 2.1 percentage points. Thus, a standard deviation higher parent income rank translates to about a 40% reduction in the probability of a stroke.[6]

Several controls are associated with mature adult health. For instance, age has a linear worsening relationship with all six health outcomes. Recall, the sample is age 40 to 65, and in analyses available on request, there is no evidence of a nonlinear relationship between these outcomes and age. Notably, the stdBs for parent income rank rival in magnitude those for age for several outcomes. For instance, a standard deviation increase in parent income rank (i.e., 24 percentage points) has a stdB larger similar to a standard deviation increase in age (i.e., 7.3 years; see Appendix A in the online version of the article) for self-rated health, stroke, and psychological distress. Parent's education also significantly predicts several outcomes. The other parent characteristics (age, sibship size, and single motherhood) are not significantly associated with mature adult health outcomes net of the other variables in these models. Again, we acknowledge parent's income is posttreatment control to these parent characteristics (i.e., their causal paths are likely blocked).[7]

After the main models in Table 1, we now compare alternative prospective and retrospective measures for childhood economic resources. Because we estimated a large quantity of models, we summarize the remaining models using figures. To be clear, these alternative models are still important, and we display the key coefficients in Appendices in the online version of the article. To summarize the results across these Appendices, Figure 1 summarizes the key coefficients for parent income, and Figure 2 summarizes model fit across models.

On one hand, we do not find large differences across measures and models. Figure 1 shows the confidence intervals for the parent income coefficients overlap. Figure 2 also reveals the model fit is mostly similar across models. Hence, the main conclusion is that the results are robust. This should augment confidence in our prospective evidence for the long arm of childhood.

6. Because the standard deviation in parent income rank is 23.79 (see Appendix A in the online version of the article), one can multiply the standardized coefficients (stdBs) and x-standardized coefficients (xstdBs) by about 2.1 to predict the change in outcomes associated with a change from 25th to 75th percentile in parent income rank. Similarly, a change from 10th to 90th percentile can be estimated by multiplying the stdBs and xstdBs by about 3.36.

7. This also explains the lack of racial and sex disparities in mature adult health. Note that the bivariate correlations are as expected (see Appendix A in the online version of the article). However, the observed parent characteristics mediate the relationship between respondent race and health (Brady et al. 2020). If we omit only parent income rank and lead education, black respondents have worse mature adult health. Although female respondents are not significantly different, recall that most Panel Study of Income Dynamics heads are male and that spouses' health are proxy reports. Proxy reports likely attenuate estimates of women's adverse health (cf. Appendix F in the online version of the article). For instance, if we focus solely on self-reporting female heads, female respondents have worse mature adult health.

On the other hand, the models with parent relative income rank are equally as or slightly more preferred than alternative approaches. Hence, prospective relative income rank is slightly more robustly statistically and substantively significant and explains slightly more of the variance in mature adult health than alternative approaches.

Appendix B in the online version of the article shows that parent absolute income yields results consistent with the results for parent relative income rank. As Figure 1 shows, the confidence intervals for the coefficients overlap. The model fits are also similar. However, for every outcome, the coefficient for parent relative income rank is larger than the coefficient for parent absolute income. Compared to parent absolute income, parent relative income rank's coefficient is about 11% larger for self-rated health, 47% larger for heart attack, 50% larger for stroke, 55% larger for life-threatening chronic conditions, and 52% larger for psychological distress. Unlike parent relative income rank, parent absolute income is not statistically significant for heart attack. Thus, relative income rank appears to slightly better predict mature adult health than absolute income.

Appendix B in the online version of the article also shows that adding two-digit occupation categories in the models does not meaningfully improve understanding of how parent economic resources affect mature adult health (coefficients for occupations not shown). Figure 2 shows the adjusted R^2 does not improve meaningfully with the addition of a large quantity of occupation indicator variables. This is partly because the sample size drops considerably because parent occupation was missing throughout some respondents' childhoods. Still, Figure 1 shows the coefficients for parent income rank are similar even when adjusting for two-digit occupation.

Appendix C in the online version of the article compares models including two sets of retrospective measures. We first include all five prospective measures (parent income rank, lead education and age, sibship size, and parent single motherhood) and each set of retrospective measures in the same model. We then omit the prospective measures and feature each set of retrospective measures by themselves. Whenever we include retrospective measures, the Ns are lower than if we use prospective measures alone. However, Appendix C in the online version of the article also shows similar results for prospective measures when we confine the N to those nonmissing for retrospective measures.[8]

Figures 1 and 2 and Appendix C in the online version of the article reveal five general patterns. First, models combining prospective and retrospective measures do not really improve fit over models featuring prospective measures alone. Second, models featuring only prospective measures tend to fit as well as or slightly better than models featuring only retrospective measures. Third, it is plausible that prospective parent income is endogenous to these particular retrospective measures.[9] For instance, father's educational attainment likely causes parent income rank. Consistent with this, the retrospective measures have

8. Having a higher N is an additional benefit of prospective over retrospective measures because nonresponse is unlikely to be randomly assigned. Again, the bottom of Appendix C in the online version of the article shows very similar results for prospective parent income rank if the Ns are confined to cases nonmissing on the retrospective measures.

9. Indeed, parent income rank correlates moderately and positively with retrospective father high school degree and with father college degree. Also, parent income rank correlates moderately and negatively with retrospective subjective judgment that parents were poor and weakly and positively that parents were well-off.

larger and more significant coefficients when prospective parent income rank is omitted. Fourth, the coefficients for prospective parent income rank are similar even when adjusting for the two sets of retrospective measures. Even when adjusting for retrospective measures, prospective parent income rank continues to have similar statistically and substantively significant coefficients. Thus, although parent income rank may be partly endogenous to these retrospective measures, including prospective measures adds to the effort to capture a more comprehensive understanding of the long arm of childhood.

Fifth, although the results favor prospective over retrospective measures, one can still find evidence for the long arm of childhood with retrospective measures. Both categories of father's educational attainment are significantly associated with all six mature adult health outcomes when omitting prospective measures. Indeed, father's education even statistically significantly predicts non-life-threatening chronic conditions (even though parent income rank does not). The model fits—with father's education and omitting prospective measures—are near the model fits in Table 1 as well. Among retrospective measures, the evidence slightly favors father's educational attainment over subjective economic standing. Although both indicators of father's educational attainment significantly predict all outcomes and the perception that parents were poor significantly predicts five outcomes (including non-life-threatening chronic conditions but not stroke), the perception that parents were well-off is insignificant for all outcomes.

Ultimately, Appendix C in the online version of the article suggests that the evidence for the long arm of childhood is slightly stronger when using prospective measures. Most of the evidence supports the earlier theoretical and methodological arguments for prospective over retrospective measures. That said, researchers often have only retrospective measures. If prospective measures are unavailable, our evidence suggests father's educational attainment should be the preferred retrospective measure. Also, our results provide unique validation for studies based on retrospective measures by showing similar results with prospective measures. Retrospective father's education performs nearly as well as prospective parent income rank.

Robustness Checks

In addition to the alternative measures and models in Appendices B and C in the online version of the article, we conducted a variety of sensitivity tests. Again, the coefficients and model fit are summarized in Figures 1 and 2. Generally, these analyses confirm our results are robust.

Appendices D through E in the online version of the article alter the samples from the sample of 40- to 65-year-old heads and spouses. Appendix D in the online version of the article includes only respondents 50 to 65 years old given that older populations have worse health (House et al. 1994). This provides a more conservative test given that childhood conditions are more distal to 50- to 65-year-olds. This results in a loss of approximately 45% of the sample, so there is far less statistical power. Parent income rank still significantly predicts heart attack, stroke, and life-threatening chronic conditions. All signs are consistent with Table 1. However, the coefficients for self-rated health and psychological distress are not statistically significant. Appendix E in the online version of the article expands the sample to 30- to 65-year-olds. This increases the sample size substantially, so there is far more power. However, there is less heterogeneity in health among 30- to 39-year-olds than among 40+ (House et al. 1994). Still, the results are generally consistent.

Appendix F in the online version of the article uses only self-reports (of heads of household) and omits proxy reports (usually of spouses). There is a substantial loss of cases—usually about one-third of the sample. Partly for this reason, parent income rank has modestly smaller and only near significant coefficients for self-rated health, heart attack, and stroke. However, Figure 1 also reveals slightly larger statistically significant coefficients for life-threatening chronic conditions and psychological distress. Moreover, Figure 2 shows that the model fits improve when we omit proxy reports. Hence, the results are generally consistent if we use only self-reports.

Appendices G and H in the online version of the article consider nonlinear functional forms for both parent income rank and absolute income. Appendix G in the online version of the article features a log transformation, and online Appendix H displays a polynomial functional form. Both provide little evidence that a nonlinear functional form would be preferred to the linear functional forms. For example, Figure 2 shows model fit never meaningfully improves with a nonlinear functional form. Also, the squared terms are never statistically significant for parent relative income rank.[10]

DISCUSSION

Despite the contributions of the long arm literature, the field has previously been forced to rely on retrospective self-reports of childhood conditions and suboptimal measures of childhood economic resources. We distinctively use *prospective* post-fisc equivalized parent income rank based on multiple observations during childhood in the 1970s to 1980s. We link these measures to six health outcomes when respondents are age 40 to 65. We adjust for several other childhood and individual characteristics. Altogether, we provide novel evidence for the long arm of childhood with prospective state-of-the-art income measures and a broad variety of salient mature adult health outcomes.

Our first two contributions are to (a) assess prospective measures and, in the process, compare prospective and retrospective measures and (b) improve the measurement of economic resources. The analyses demonstrate that prospective parent income rank has statistically and substantively significant relationships with five of six mature adult health outcomes. The relationships with heart attack, stroke, and life-threatening chronic conditions are particularly strong. Therefore, we provide novel evidence of the long arm of childhood with a prospective state-of-the-art measure of childhood income.

The PSID does not allow for an exact replication and comparison with all the retrospective measures used in prior studies. Still, we assess two sets of retrospective measures: father's education and family subjective economic standing. Our evidence suggests that prospective

10. The squared terms are statistically significant for absolute income for four outcomes. However, the attenuation in the coefficients implied by the squared terms are not substantively significant. Although the increases in health for higher absolute incomes slow down at some point, the health benefits of higher absolute income continue at quite high incomes.

measures perform slightly better than retrospective measures. Yet one can still assess the long arm of childhood with retrospective measures if prospective measures are not available. In the process, we provide critical validation of studies based on retrospective measures. Moreover, this is a novel validation given that prior studies have heretofore not been able to compare optimal prospective and retrospective measures. Hence, this study buttresses past studies and guides ongoing studies using retrospective measures. Moreover, our distinctive comparison of prospective and retrospective measures clarifies which retrospective measures perform best and most similar to prospective measures. Among retrospective measures, father's education performs better than subjective economic standing.

On balance, the PSID does not have data on all aspects of childhood disadvantage. For example, there is considerable evidence for health effects of adverse life events during childhood (Lee and Ryff 2019). As well, it is plausible that unhealthy environments and poor health during childhood predict adult health. We focus more narrowly on childhood economic resources rather than all childhood conditions. We show that economic resources are one particularly salient aspect of childhood that is related to mature adult health. That said, economic resources likely predict and even cause many other unobserved childhood conditions and events. Parents with greater economic resources can "purchase" safer and healthier environments. In turn, their children should be less likely to experience accidents, toxicity, trauma, and injuries. Hence, childhood income likely serves as a fundamental cause of health by reducing the risks of adverse conditions (Clouston and Link 2021; Link and Phelan 1995).

Our third contribution is to inform several perennial debates about how best to measure and model how economic resources and social class shape health. First, we inform the debate between relative and absolute income. Whereas relative income locates one hierarchically within the distribution of a time and place, absolute income's value is invariant across time and space. Our results are generally consistent give that both relative and absolute childhood income robustly significantly predict mature adult health. However, the xstdBs and stdBs for parent relative income rank are always larger than those for parent absolute income. Compared to absolute income, the coefficients for relative income are about 11% larger for self-rated health, 47% larger for heart attack, 50% larger for stroke, 55% larger for life-threatening chronic conditions, and 52% larger for psychological distress. Furthermore, unlike parent relative income rank, parent absolute income is not statistically significant for heart attack. Overall, the evidence favors relative over absolute income—at least in terms of the predictive validity of childhood income for mature adult health.

Second, we inform debate about the salience of income compared to other measures of social class. Including two-digit occupation does not change inferences about childhood income or appreciably improve model fit. Also, the models do not meaningfully improve with nonlinear functional forms for income. There appear to be at least mostly monotonic returns of parent income for mature adult health. Our results certainly support the implicit emphasis in the long arm literature on childhood disadvantage and adversity. However, our finding of a mostly linear relationship also illustrates the health advantages of childhood affluence. Our evidence suggests that children in high-income families benefit even compared to children in middle-income families (Adler et al. 1994). There are meaningful health advantages to having a very high income, and this speaks to the health consequences of inequality alongside disadvantage (Elo 2009; Marmot 2005). An argument can be made for narrowing the gap between affluent and middle class to equalize mature adult health. This can complement raising the incomes of poor families to improve mature adult health decades later.

The results are also robust to several alternative samples and model specifications. We find similar results if we confine the sample to participants who are 50 years old and older, or expand the sample to include 30- to 39-year-olds, or focus solely on self-reports of heads. Appendix J in the online version of the article shows one final sensitivity analysis exploring different stages of childhood. As Appendix J in the online version of the article shows, income during any stage of childhood is quite highly correlated with income at other stages or income across all of childhood (see Panel A). On one hand, this illustrates that it is extremely difficult to tease apart the long-term health effects of income across various stages of childhood (at least in observational data; see Panel B). On the other hand, this confirms that income at any stage of childhood and across childhood in general is similarly consequential to mature adult health.

The present study has limitations that can guide future research. First, upcoming waves of surveys like the PSID could include the survey questions used to form prior composite indices of retrospective childhood disadvantage. This would allow an even closer comparison of prospective and retrospective measures. Second, other data sets besides the PSID are needed to study Asian American, Latino, and immigrant populations and ethno-racial minorities besides black people. Third, how race and sex interact with childhood income to drive racial and sex inequalities in mature adult health is beyond our scope but warrants attention. Fourth, the political-economic context of childhood varies in terms of social policies and institutions such as labor unions (VanHeuvelen and Brady 2021). Future research should analyze whether such contextual factors moderate the relationship between childhood income and mature adult health. Fifth, future PSID analyses can build on recent research that investigates the mechanisms at work in the long arm of childhood (e.g., Lee and Ryff 2019; Montez et al. 2016; Morton and Ferraro 2020). Because the PSID-CNEF has a large number of respondents followed over a long period of time— from childhood through mature adulthood—future analyses should investigate the potential mechanisms after childhood but before mature adulthood (e.g., ages 18–39), including health behaviors and economic attainment.

Altogether, this study buttresses the case for the long arm of childhood. We uniquely demonstrate precisely how economic resources translate into health via prospective parent income rank. This should invite public policy debate. The broader concept implied by composite indices of childhood disadvantage is fairly diffuse, and intervening on such indices is therefore challenging. For composite childhood disadvantage, we do not know which specific indicators to prioritize, nor do we know that intervening on any particular indicator will necessarily improve adult health. By contrast, our evidence suggests policymakers can intervene with income transfers targeted at children to improve adult health. Policymakers can also equalize health by taxing high-income families. Altogether, this study suggests that raising and equalizing incomes during childhood can have long-term and equalizing health benefits much later in adulthood.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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AUTHOR BIOGRAPHIES

David Brady is a professor in the School of Public Policy at the University of California at Riverside. He is also a research professor on inequality and social policy at the WZB Berlin Social Science Center. He studies poverty, racial and health inequalities, social policy, and the political consequences of immigration and ethno-racial diversity. He is the author of *Rich Democracies, Poor People* and editor of *The Oxford Handbook of the Social Science of Poverty*.

Christian Guerra is a PhD candidate in the sociology department at the University of California, Riverside. His research focuses on assessing and predicting area-level mortality rates using Google search term data in the United States. He is currently focused on suicides, overdoses, and alcohol-related deaths.

Ulrich Kohler is a professor of methods of empirical social research in the Department for Economics and Social Science of the University of Potsdam. He is editor of *Survey Research Methods*. Current research interests include health consequences of economic positions in the childhood, the social roots of learning ancient languages, and the usage of nonprobability samples for sociological research. He recently published “Educational Expansion, Social Class, and Choosing Latin as a Strategy of Distinction” in *Zeitschrift für Soziologie*.

Bruce G. Link is distinguished professor of public policy and sociology at the University of California, Riverside. He is currently conducting research on the life course origins of health inequalities by race-ethnicity and socioeconomic status, the consequences of social stigma for the life chances of people who are subject to stigma, and intervention efforts aimed at reducing mental illness stigma in children attending middle school.

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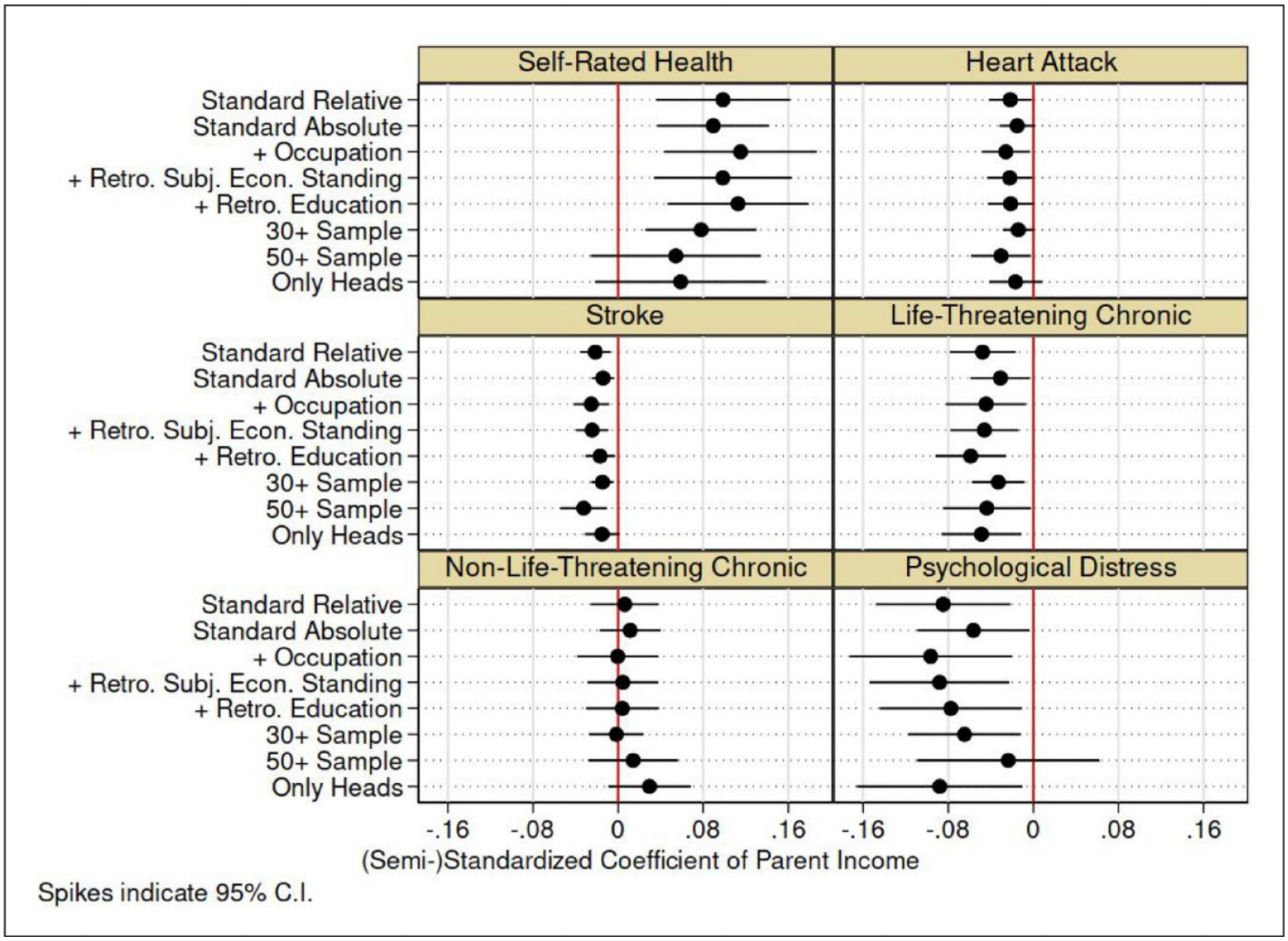

Figure 1.
Parent Income Coefficients (stdBs of xstdBs for Binary Outcomes) Coefficients across Table 1 and Appendices in the Online Version of the Article.
Note: Data from the Panel Study of Income Dynamics (2013), Waves 1970–2019. stdBs = standardized coefficients; xstdBs = x-standardized coefficients; econ = economic; retro = retrospective; subj = subjective.

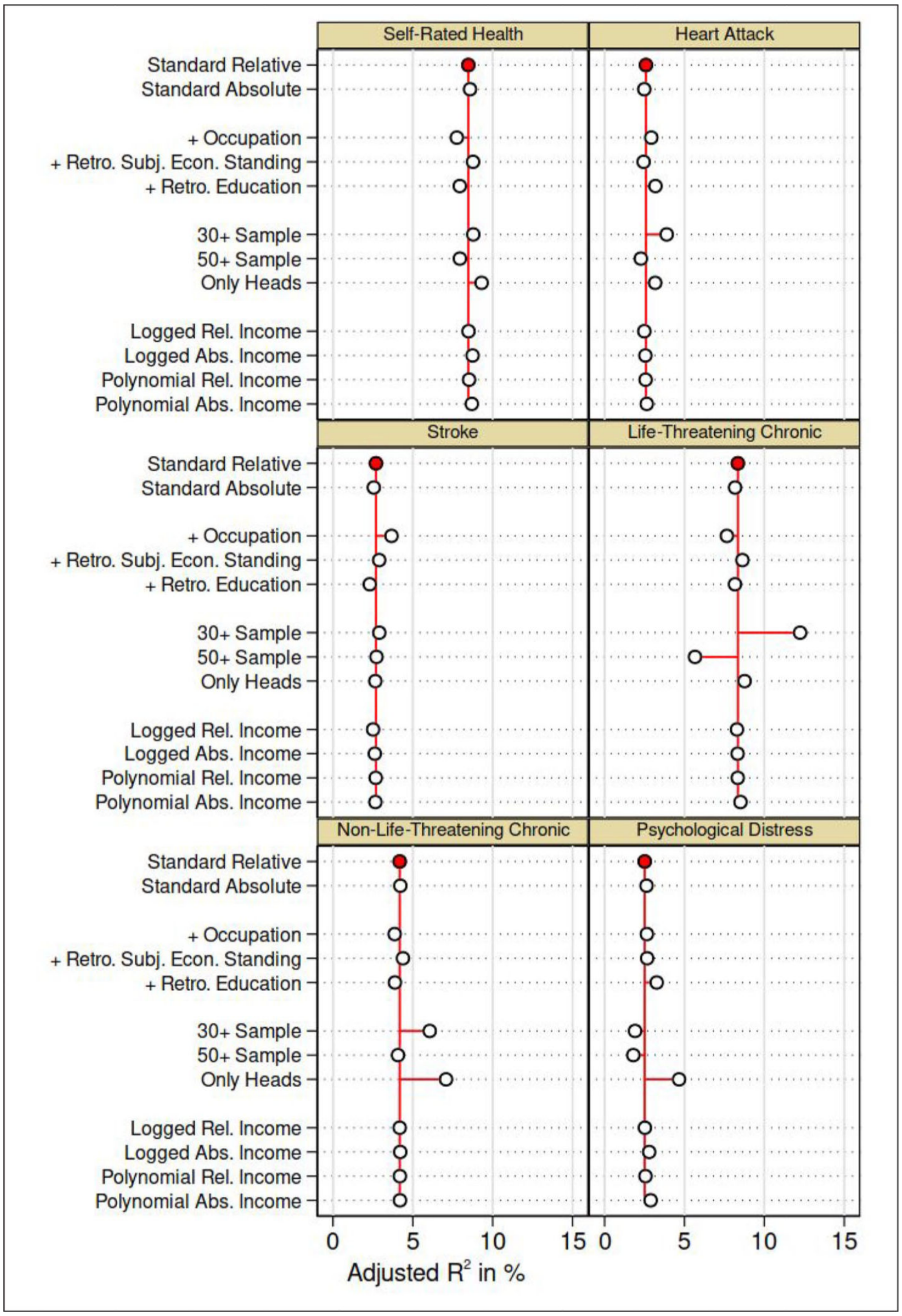

Figure 2.

Adjusted R^2s for Various Models Across Table 1 and Appendices in the Online Version of the Article.

Note: Data from the Panel Study of Income Dynamics (2013), Waves 1970–2019. Abs = absolute; econ = economic; rel = relative; retro = retrospective; subj = subjective.

Table 1.

Standardized and x-Standardized Coefficients from Linear Models for Health Outcomes at 40+ on Childhood Income Rank and Other Variables.

	Self-Rated Health	Heart Attack	Stroke	Life-Threatening Chronic	Non-Life-Threatening Chronic	Psychological Distress
Parent	.099**	−.022*	−.021**	−.048**	.006	−.085**
income rank	(.032)	(.010)	(.007)	(.016)	(.016)	(.032)
Lead	.184**	−.018*	−.010	−.035**	−.027*	−.082**
education	(.026)	(.009)	(.007)	(.013)	(.013)	(.028)
Lead age	.002	−.001	.008	−.009	.002	.030
	(.023)	(.007)	(.006)	(.011)	(.011)	(.024)
Sibship	.031	.001	−.005	−.020	−.015	−.028
size	(.024)	(.008)	(.005)	(.011)	(.012)	(.022)
Single	−.056*	.004	−.001	.005	.025*	.036
motherhood	(.025)	(.009)	(.006)	(.012)	(.012)	(.029)
Age	−.117**	.035**	.023**	.124**	.062**	−.080**
	(.024)	(.008)	(.006)	(.012)	(.012)	(.025)
Black	.049	−.015	.007	.040	−.007	−.253**
	(.066)	(.023)	(.017)	(.030)	(.032)	(.064)
Latino	.497	−.117	.019	−.227*	.158	.340
	(.329)	(.060)	(.030)	(.106)	(.363)	(.455)
Other race	.042	.026	.026	−.040	−.058	−.086
	(.095)	(.036)	(.030)	(.059)	(.051)	(.120)
Female	−.044	−.001	.013	−.027	.103**	.092*
	(.041)	(.013)	(.010)	(.020)	(.021)	(.044)
Constant	−.228	.039	.107	.543**	1.016**	−.305*
	(.388)	(.024)	(.085)	(.198)	(.085)	(.124)
N	3,884	3,944	3,944	3,862	3,813	3,894
Adjusted R^2	.085	.026	.027	.083	.042	.025

Note: Data from the Panel Study of Income Dynamics (2013), Waves 1970–2019. The numbers in parentheses are robust standard errors clustered by household. All models include wave indicators (not shown).

* $p < .05$,

** $p < .01$ (for two-tailed tests).