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Long-term health effects of family income received at different stages in the life cycle

Nicardo Mcinnis

ABSTRACT

LONG-TERM HEALTH EFFECTS OF FAMILY INCOME RECEIVED AT DIFFERENT STAGES IN THE LIFE CYCLE

By

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When studying longer-term outcomes, there might be complicated dynamics which can only be understood by taking a holistic life cycle approach. I estimate the long-term health effects of family income received at different phases in the life cycle from conception up to the point at which health outcomes are observed in early adulthood. I construct a unique life-course data set from conception up to age 32, using the Panel Study of Income Dynamics. Employing a two-stage least squares estimator, I instrument for family income with two instruments: simulated income and simulated Earned Income Tax Credit benefits. For health status as of age 32, I find that beneficial health effects are primarily from income received after age 18, but income in the early childhood years also has beneficial long-term effects. I also find some suggestive evidence that income in the teenage years may have adverse long-term health effects. Similar patterns are observed for risky health behaviors such as drinking and smoking, with strong evidence that more income during the teenage years increases participation in these behaviors in early adulthood. These results indicate that the relative timing of income receipt matters for long-term health outcomes.

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NICARDO SEDAKA MCINNIS

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ACCEPTANCE

This dissertation was prepared under the direction of Nicardo S. McInnis's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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DEDICATION

For my family, the present generation and the generations to come. In a world of endless possibilities, don’t simply settle for what’s easily attainable. Aspire boldly and see goals attained with perseverance.

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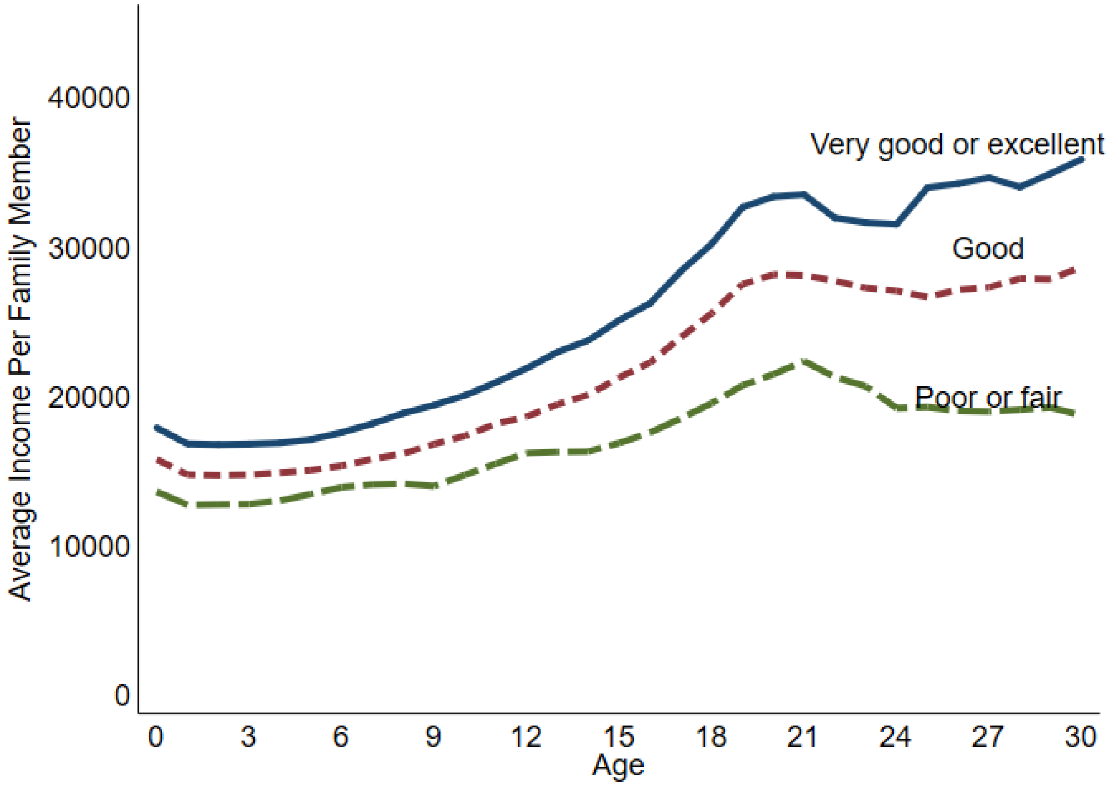
1. Introduction

Understanding the effect of one's prior economic circumstances on future outcomes is important. In Figure 1, I show the income path that led to the observed health status by age 32. This graph demonstrates that individuals observed with relatively poorer health in early adulthood, had lower income at all ages on average leading up to that point. Furthermore, the figure also shows that the income disparity by health status widens with age. However, this is purely descriptive. There are several states of the world that might explain this data generating process. For example, individuals could have gotten ill which led them to obtain less education which results in lower income in adulthood. Or due to low parental resources in childhood, individuals may end up with relatively lower income and poorer health in adulthood. This graph merely serves as motivation for why the relationship between income and health continues to be such an important one.

For several decades, economists and epidemiologists have documented a positive association between income and health and more broadly, socio-economic status and health (Cutler, Lleras-Muney & Vogl, 2008; Currie, 2009). Though research in this area spans several decades, our understanding of the relationship between socioeconomic status and health is still limited (Currie, 2009). In the absence of randomization, it is difficult to find reasonably good natural experiments to derive exogenous changes in income to conduct causal analyses. Many factors that are strong predictors of income also affect health directly, such as education, unemployment and work experience. The observed positive association could therefore arise because higher income leads to improvements in health, better health leads to increased income or because unobserved factors such as genetics, risk preferences and social background,

Figure 1: Income Path by Health Status at Age 32



Notes: While income is runs from conception into early adulthood, only one health status is only used from the age range of 28 to 32. Income is in real 2018 dollars.

might be correlated with both income and health. The direction of causality between income and health is widely debated (Meer et al., 2003; Smith, 1999). Another major factor limiting our understanding of the relationship between income and health is that only a few papers study this relationship within a dynamic framework. As discussed in Friedman (1957), there is a distinction between permanent and transitory income - individuals consume differently out of each type of income. Permanent income is best captured over the life course as opposed to just examining income at a single point in time. Among the studies conducted within a dynamic framework, there is very little evidence that there is a statistically significant correlation between health and past income or wealth. Smith (2007), for example, finds that neither past income nor wealth are correlated with the future onset of chronic health conditions.

In this paper, I estimate the long-term effects of family income received at different stages in the life cycle on health among individuals in the United States. I use the Panel Study of Income Dynamics (PSID) to construct a unique life-course data set for individuals born to an original PSID sample member. Because of the number of years that have elapsed since the survey started, there are several cohorts of children who were born in a PSID household and have been observed in the survey from birth until they become adults. Due to survey construction, most health outcomes for these individuals are observed between the ages of 28 and 32. As such, I focus on income from conception up to age 32. To detail a crude measure of permanent income, I take the average income over different phases in the life cycle. In my main analysis, I use average income from conception to age 18 and from age 19 to 32 and subsequently break out income in several other ways. Employing a 2-stage least squares (2SLS) estimator, I instrument for family income using two simulated instruments: simulated income and simulated EITC benefits.[1] These instruments exploit variation coming from idiosyncratic changes in income across demographic group over time at the national level, as well as from changes made to the EITC parameters over time at both the federal and state level, such as credit rate, phase-out credit rate, minimum income for maximum credit, maximum credit, beginning income for phase-out rate and ending income. This approach overcomes concerns that health might be a determinant of income or that unobserved factors might be correlated with both income and health.

[1] This approach builds on the idea in Currie & Gruber (1996a, 1996b) which simulates Medicaid eligibility across states for different groups.

The primary contribution of this paper is that I simultaneously account for income from conception up to the point at which health outcomes are measured in early adulthood. This builds on previous studies which examine the long-term effects of only early life environment (Almond, Currie & Duque, 2018; Almond & Currie, 2011; Currie & Almond, 2011). By simultaneously accounting for income throughout the life-course, I address an important criticism leveled at this growing literature. The criticism is that these studies do not account for what goes on in the years between early life and the point at which long-term outcomes are measured in adulthood. If what goes on in these "middle years" is also important, the policy implications of early life studies become more complicated. Another major contribution is that I construct instruments that are strong predictors of family income, allowing me to instrument for total family income as opposed to relying on one-off shocks to income. This builds on previous studies that consider only shocks encountered at a single point in time or over a short period of an individual's life. With this work, I also demonstrate the feasibility of conducting holistic life-course analysis using an existing data source. A secondary contribution is that I examine several measures of health and health behaviors, including general self-reported health, body mass index (BMI), smoking and alcohol consumption. This is important because health is multi-dimensional, and no single indicator of health sufficiently captures health status. This is supported by the findings from previous studies which have documented a mixture of null and beneficial effects of income across different indicators of health. For example, Apouey & Clark (2015) find no effect of lottery winnings on general health status or physical health but find improvements in mental health.

There are three major findings that emerge from this analysis. First, long-term beneficial health effects are primarily from income received in the later phase of the life cycle (between age

19 and 32). Second, the effects of income received before age 18 are not constant - income received in the earlier years of life appears to have beneficial long-term effects, while income in the teenage years show some evidence it might have adverse consequences on long-term health. These patterns are even more strongly reflected in risky health behaviors such as smoking and drinking. Given that these risky health behaviors are likely developed earlier in life, that might be the reason there is some suggestive evidence that income in the teenage years might have adverse long-term health effects. Third, using a simple approach like Ordinary Least Squares (OLS) often results in estimated effects that are biased. In most cases the bias appears to be toward zero, but there are also cases where the estimated effects are biased upward.

From a policy perspective, understanding the timing of receipt of income and transfers from safety-net programs (e.g., benefits received as a child or as an adolescent or both) is critical for designing efficient policies. whether transfers should be made in adulthood or childhood would crucially depend on three factors: the ages at which these transfers would be made; the length of time for which these transfers are being proposed; and whether more resources provided in early life affects the available resources later in life. Furthermore, if a policy maker only cares about health in early adulthood, the main policy implication is that transfers made in adulthood are most effective. This might also suggest that safety net programs that provide the largest benefits to families with children such as the Earned Income Tax Credit (EITC), could have unintended adverse consequences on health among adults if financing these EITC transfers hinges on reducing other program benefits for families without children and the effects are similar across the income distribution.

One of the main limitations of this study is that health outcomes are observed only in early adulthood. Even though data are available on multiple health outcomes in the data set, focus is placed only on a few. This is because for many of the health outcomes that are available such as heart attack, stroke, diabetes and hypertension, only a very small fraction of the sample would be diagnosed with any of these conditions by early adulthood. Another limitation is that the sample size is relatively small. This arises because of the need to use the panel nature of the data set and also the need to observe complete information for each individuals' family for several years.

The remainder of this paper is organized as follows. In section 2, I discuss the existing evidence on the effect of income on health and the literature on the long-term effects of early life conditions. In section 3, I discuss the data from the PSID and the Current Population Survey (CPS). Section 4 discusses the identification strategy used to estimate the causal effect of income received at different stages in the life cycle on long-term health. Section 5 presents the main findings and section 6 contains a discussion of those findings. Lastly, section 7 concludes.

2. Background and Prior Research

The Effect of Income on Health

Economic theory predicts that higher income relaxes the budget constraint which will allow individuals to obtain more of all normal goods. Insofar as health is a normal good (Grossman, 1972), we expect health to also improve. However, there might be other normal goods such as certain risky health behaviors which are negatively correlated with health, which would undermine the argument of income having a positive effect on health. Thus, the exact nature of this relationship is unclear *a priori*. In this section, I review the causal evidence on the effect of income on various measures of health.[2] While results vary across studies, overall, the existing evidence suggests that there is a weak causal link between income and health (Gunasekera et al., 2011).

One of the main challenges in disentangling the causal effect of income on health is finding instruments that are correlated with income but otherwise uncorrelated with health. Ettner (1996) was one of the first studies to examine the causal effect of income on health, by instrumenting for own and spousal income with the state unemployment rate, work experience, parental education and a number of spousal characteristics such as work experience, education and parental education. The identifying assumption is that these instruments are determinants of family income but are otherwise uncorrelated with an individual's own health. Under this assumption and using cross-sectional data from the 1980's, the study finds that increases in

[2] There are several studies examining the relationship between income, wealth and longevity for example Attanasio & Hoynes (2000) and Gerdtham & Johannesson (2004) which are not reviewed here. See Pickett & Wilkinson (2015) for a review of the causal evidence on the relationship between income inequality and health. Consistent with previous studies, in this review, I do not regard studies implementing cohort analyses as causal because of inherent limitations associated with the estimation strategy. Kim & Ruhm (2012) highlight that cohort level biases are not eliminated in these kinds of analyses and could potentially be a main source of disparity in observed results. For example, cohorts having better average quality health might lead to higher income but omitted factors such as medical technologies or lifestyle changes may be associated with both the average health and income level of cohorts.

income result in large improvements in mental and physical health, but also increased the prevalence of alcohol consumption. However, in more recent work, the validity of some of the instruments used have been questioned (Kawachi, Adler & Dow, 2010; Meer et al., 2003). For example, own experience is likely correlated with health other than through income, perhaps because of the type of job and how demanding it is both physically and mentally, or because individuals in poor health perform worse and run the risk of being unemployed more frequently or for longer periods. Using spousal characteristics might also be problematic if individual's health status is a determinant of the quality of the spousal match or whether an individual has a spouse to begin with.

More recently, the literature has moved toward the use of income shocks to estimate reduced form effects of various types of income shocks on health. Lindhal (2005) uses lottery winnings as an exogenous shock to income among individuals in three waves of the Swedish Level of Living Survey between 1968 and 1981 and finds that lottery income results in improvements in a number of self-assessed health variables. Additionally, the findings indicate that using lottery winnings as an instrument for net family income increases the point estimate relative to that obtained using the reduced form OLS regression. Cesarini et al., (2016), using Swedish administrative data, estimate an OLS regression with lottery winnings as an exogenous source of income and find no evidence that lottery winnings impact mortality or the utilization of health services among adults, except for a possibly small reduction in the consumption of mental health drugs. Gardner & Oswald (2007) and Apouey & Clark (2015) estimate a similar OLS regression to examine the effect of lottery winnings on health among individuals in the British Panel Survey. Gardner & Oswald (2007) find that individuals with medium sized wins had better psychological health. Apouey & Clark (2015) find no effect on overall self-assessed health or physical health but find improvements in mental health. Furthermore, Apouey & Clark (2015) find that lottery winnings increase the prevalence of smoking and drinking.

The use of lottery winnings as an instrument for income is problematic both in OLS regressions as well as in instrumental variables approach. The challenge is that the probability of winning the lottery varies with the number of tickets individuals purchase. One can reasonably expect that gambling habits are correlated with investments in health other than through income. In the case of Lindhal (2005) for example, he highlights that not only is the number of lottery tickets purchased unobserved but there is also no information on when individuals won the lottery. Apouey & Clark (2015) highlight a similar concern. Even though individual fixed effects are included in the model, it does not solve the endogeneity issue if, for example, individuals' risk preferences are time varying and these influence health investment decisions. Aside from the identification issues, lottery winnings as an instrument for income is also limited in its usefulness because it is a one-time shock to income that is more closely related to transitory income, not permanent income.

Inheritance is another source of income that has been used as an exogenous source of variation to examine the causal effect of income on health. Meer, Miller & Rosen (2003) estimate an instrumental variables probit model to obtain the effect of family wealth on self-assessed health among PSID sample members between 1984 and 1999, using 4 waves of the PSID data. They find that using the instrumental variables approach, the point estimate is unchanged as compared to the estimate obtained using the probit model, but the point estimate becomes statistically insignificant. They argue that the point estimates are miniscule and an inflation of the point estimates by 2 standard errors still render them economically small. Kim & Ruhm (2012) estimate the effect of inheritance on a number of health outcomes including self-assessed health and difficulty performing certain tasks, using 8 waves of the Health and Retirement Survey among adults age 51 and older. While inheritance increases out of pocket expenditure and the utilization of medical services, they find no meaningful effects on health nor any convincing evidence of offsetting lifestyle changes such as drinking, smoking or exercising. As highlighted by Kim & Ruhm (2012), the strategy to use inheritance suffers from the concern that inheritance may be correlated with unobserved factors that are determinants of health. Insofar as some components of health are genetic, individuals receiving inheritances earlier in life are more likely to be susceptible to adverse health outcomes. Furthermore, inheritances might not be unanticipated, and individuals may adjust their lifestyles in anticipation of expected inheritance.

Case (2004) examines the effect of old age pension on self-reported health status among South Africans and finds that pension reforms leading to unanticipated increases in wealth lead to improvements in the health status of blacks and colored individuals living in families with old age pensioners that pool income. Frijters, Haisken-DeNew & Shields (2005) estimate the effect of the fall of the Berlin Wall, which resulted in substantial income transfers to a large section of the East German population, on health satisfaction using data from the German socioeconomic panel between 1984 and 2002. They find that the fall of the Berlin Wall resulted in improvements in health satisfaction, but they argue the magnitude is very small.

The approach to use one-time shocks to income to estimate causal effects is useful. However, it is constrained because it only captures a one-time transitory income and is confined to very specific contexts. This approach requires the use of numerous types of income shocks to be able to narrate a convincing story about the causal effect of income on health. This is because different types of income shocks may generate different types of health effects if changes in individuals' consumption bundles depend on the source of the income shock.[3] It becomes even more demanding to make inferences about the effect of income on health using this strategy, given that the results are neither consistent within nor across the types of income shocks that are examined. For example, lottery winnings are shown to have a mix of null (Apouey & Clark, 2015) and beneficial (Gardner & Oswald, 2007) effects on health. Inheritances show no meaningful health effects (Kim & Ruhm, 2012; Meer, Miller & Rosen, 2003). Pension reform shows health improvements only among certain groups (Case, 2004) and a large economic shock had only very small health effects (Frijters, Haisken-DeNew & Shields, 2005). The usefulness of these studies might also be limited because they almost always consider the contemporaneous effects of income on health. With the growing evidence in support of the idea that the effect of early life circumstances persists into the future, it becomes even more important to examine the long-term effects of financial resources received at different stages in the life cycle and not just examine the contemporaneous effects using cross sectional data.

Effect of Early Life Conditions on Long-term Health

In recent years, there has been a growing number of studies examining the long-term effects of *in utero* and early childhood environment.[4] The outcomes which are commonly studied include education, labor market and health (Aizer et al., 2016; Black et al., 2019; Carneiro, Løken, & Salvanes, 2015; Hoynes, Schanzenbach, & Almond, 2016; Isen, Rossin-Slater, & Walker, 2017; Miller & Wherry, 2019). The overarching theme among these studies is that shocks in early childhood can have persistent effects many years later. For example, Aizer et al.,

[3] See Thaler (1999) for more information on how individuals may treat income from different sources differently (mental accounting) which violates the economic principle that money is fungible.
[4] For a comprehensive review of these studies, see Almond, Currie & Duque, 2018; Almond & Currie, 2011; Currie & Almond, 2011.

(2016), study the long run impact of mothers' application acceptances to the first government sponsored welfare program in the United States between 1911 and 1935 on their children's longevity, educational attainment, nutritional status and income in adulthood. Overall, they find that relative to the male children of mothers whose applications were rejected, male children of mothers with accepted applications obtained more schooling, were less likely to be underweight, earned higher income and also lived longer. Hoynes, Schanzenbach, & Almond (2016) conduct a similar kind of analysis where they estimate the effect of food stamp availability in the county of birth from conception to age 5 on several indicators of health, including health behaviors, education, earnings, income and program participation. They find that more access to food stamps during these childhood years led to significant improvements in health in adulthood and also increased economic self-sufficiency among women- which they argue likely come from improved nutrition which is consistent with the Barker hypothesis.

In the case of Hoynes, Schanzenbach, & Almond (2016), outcomes are measured roughly 25 years later while in Aizer et al., (2016), outcomes are measured much later. While the gap may vary, measuring long-term outcomes many years after a treatment is defined is not uncommon. Given the long period that elapses between early childhood and early adulthood, there could be either offsetting or compensatory investments based on childhood environment. For example, Liu, Mroz & Adair (2016) find strong evidence that birth outcomes and child development over the first two years of life influence parents' observable behaviors in ways that lead to large improvements in children's early physiological development. Furthermore, the pathways through which early life conditions affect longer term outcomes may not be limited to only early years in the life cycle. It is important to understand the dynamics of these long-term effects to design the most efficient set of policies.

3. Data

The data used in this study come from two main sources: the Panel Study of Income Dynamics (PSID) and the Current Population Survey (CPS). Given that family income in the PSID is arguably endogenous, I instrument for it with two instruments that I construct using information from the CPS data. In the next two sub-sections, I discuss the information contained in each survey that is pertinent for this project, as well as how the information is utilized.

Panel Study of Income Dynamics

The survey started in 1968 with 4,802 households. Of these households, 1,872 were from the low-income population which forms the Survey of Economic Opportunity (SEO) sample and 2,930 were taken from the Survey Research Center (SRC) national sampling frame, which forms a nationally representative sample.[5] Since the survey's inception, information has been collected on individuals living within a sample household and descendants of individuals who were originally sample members. The survey was conducted on an annual basis until 1997 when the survey became biennial. Data are collected in several domains including employment, income, state of residence, health, marriage, education and childbearing. However, information on some of these variables are collected only for heads of household and their spouse and are available only in particular years.

The PSID data set was chosen for two main reasons. First, it is the longest running panel survey that contains information on health. Second, it has family-level information on individuals in the primary sample for the years prior to when they became the head of a household or their spouse- when health information becomes available. As such, I can measure their family income over the course of their life.

[5] https://psidonline.isr.umich.edu/Guide/ug/psidguide.pdf

The PSID sample of interest includes individuals born between 1967 and 1989 who were the head of the household or spouse at least once, since 1999. This group is of interest for two reasons. First, information related to health is only collected for heads of household and their spouse. Second, with the exception of self-reported health, data on health outcomes are collected starting in 1999. Additionally, I restrict the sample to individuals who were born in a PSID family and whose family remained active in all years up to the point at which I observe the individual's health outcomes.[6] This condition is imposed because it is necessary to observe individuals from conception up to the point at which health outcomes are observed in early adulthood. Using these criteria, the final sample contains over 5,000 individuals. Table 1 contains summary statistics for the sample of interest and Table 2 contains summary statistics for the health outcomes and health behaviors that are studied. Figure 1 plots the number of individuals in the final sample by year of birth.[7]

The PSID does not collect information on EITC receipt so I use information on various types of income, assets, marital status, number of children and state of residence to estimate the annual amount of EITC benefits for which the family would be eligible, using the NBER TAXSIM model.[8] If the individual was the head or spouse at the time of the survey, their own information is applicable. But, if these individuals were not the head or spouse at that point in time, the information for the current head and spouse of the household is applicable.

[6] I test for selective attrition. The results are presented in appendix B.
[7] Table A1 in the appendix shows the number of individuals lost because of the selection criteria.
[8] https://www.nber.org/taxsim/

Table 1- Summary Statistics for Primary Sample

	Observations	Mean	SD
Total Sample	5,386	100.00	
Male=1	2,539	47.14	
Race			
White	3,564	66.17	
Non-white	1,822	33.83	
Married=1	3,543	65.78	
Age Health Outcomes Observed			
28	316	5.87	
29	375	6.96	
30	385	7.15	
31	2,191	40.68	
32	2,119	39.34	
Education			
Less than High School	359	6.67	
High School	1,580	36.00	
Some College	1,603	29.76	
At least a Bachelor's degree	1,844	34.24	
Average Annual Per Capita Family Income (2018 $'s)			
Conception to age 18	5,386	18,827.5	14,980.69
Age 19-32	5,386	30,886.96	29,117.53
Conception to age 32	5,386	22,561.68	17,415.82

Notes: Per capita family income is measured as total family income divided by the number of family members and are in real 2018 dollars.

Figure 2: Number of Individuals by Birth Year for Primary Sample

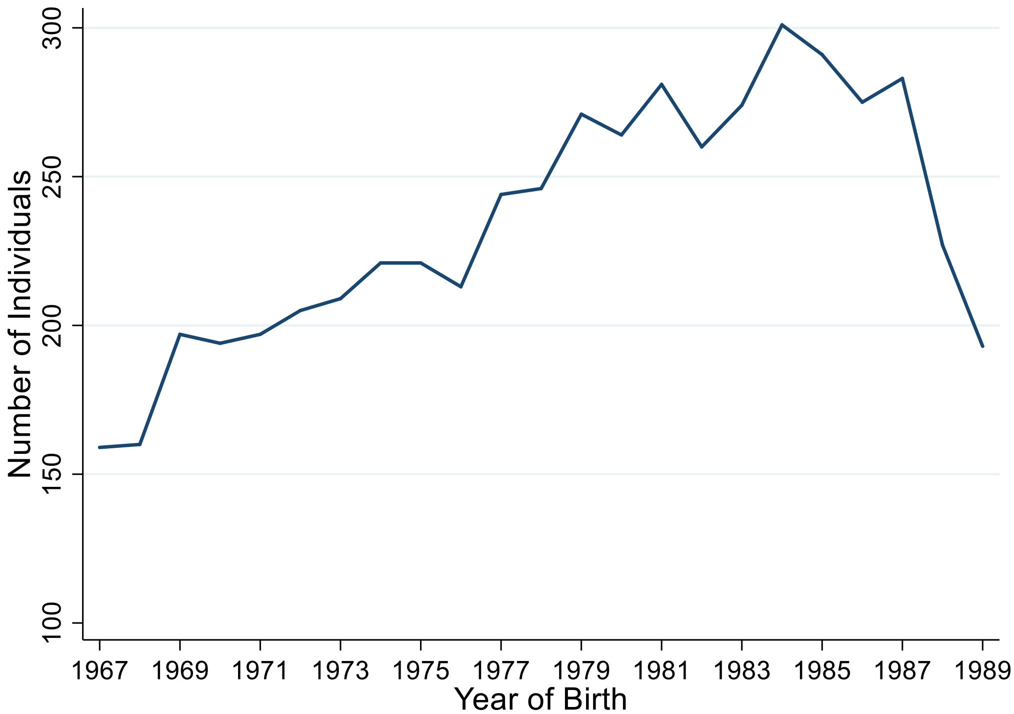


Table 2- Summary Statistics for Health Outcomes

	Observations	Mean	SD
Total Sample	5,386		
Health Outcomes			
General Health Status: Very Good or Excellent			
Very Good or Excellent	5,135	65.63	
Good	5,135	26.47	
Poor or Fair	5,135	7.90	
Body Mass Index	5,094	27.46	6.14
Obese=1	5,094	0.274	
Diabetes=1	5,336	0.033	
Hypertension=1	5,336	0.132	
Stroke=1	5,336	0.008	
Heart Attack=1	5,336	0.005	
Heart Disease=1 (e.g. Coronary heart disease,)	5,336	0.014	
Metabolic Syndrome=1[9]	5,093	0.011	
Cancer=1	5,336	0.020	
Health Behaviors			
Participate in any exercise at least 10 mins. per wk.	5,268	0.898	
Current Smoker=1	5,133	0.211	
Number of Cigarettes per Day (including non-smokers)	5,124	2.425	6.07
Quit Smoking=1	1,995	0.458	
Current Alcohol Drinker=1	5,131	0.705	
Number of Alcoholic Drinks Per Day (including non-drinkers)	5,095	1.603	1.82
Number of Days have 4 to 5 drinks per year (including non-drinkers)	4,323	6.205	27.59

Notes: The number of observations varies by health outcome because of missing data.

[9] Metabolic syndrome is a composite indicator of health which is based on a cluster of health conditions. Individuals having at least three of the following conditions; obesity, diabetes, hypertension, heart disease or heat attack are classified as having metabolic syndrome.

Current Population Survey

The CPS data has a very long history, dating back to early 1940s. It is the primary data source for obtaining information on labor force statistics in the U.S. It collects information on a monthly basis from roughly 50,000 households.[10] Each household is surveyed for 4 consecutive months, then exits the survey for an 8-month period. Once a household re-enters the sample, it is surveyed for an additional 4 months and then exits the sample permanently. Information is collected on a wide range of topics including employment, earnings and a detailed set of demographic characteristics. I use data on income and the demographic characteristics for the head and spouse to construct the two simulated instruments. Once these simulated instruments are formed, I then link them to individuals in the PSID data using the demographic information for head and spouse. This process is described in more detail in the methods section.

[10] https://www.census.gov/prod/2002pubs/tp63rv.pdf

4. Methodology and Econometric Specification

This paper examines the long-term impact of family income received at different stages in the life cycle on health and health behaviors in early adulthood. To overcome concerns that unobserved factors affecting health might be correlated with family income, I implement a 2-stage-least-squares approach. In doing so, I instrument for family income with two simulated instruments. Before providing details about how these instruments are constructed, I discuss the parameters of interest in the structural equation.

Structural Equation of Interest

The structural equation of interest is as follows:

$$Y_{ist}^{28-32} = \gamma_s + \gamma_{st} + \beta_1 X_{is} + \beta_2 \ln\left(Inc_{is}^{0-18}\right) + \beta_3 \ln\left(Inc_{is}^{19-32}\right) + \epsilon_{is} \qquad (1)$$

where i indexes individual, s indexes state and t indexes survey year. The dependent variable is a health or health-related outcome between age 28 and 32 (for simplicity, from here on the outcome is denoted as being observed at age 32), such as general health status. γ_s and γ_{st} are state and state by survey year fixed effects, respectively.[11] X_{is} is a set of controls which includes gender, race, a set of cohort dummies, birth cohort by survey year fixed effects and controls for the demographic variables used to match the instruments to the PSID data. Inc_{is} represents average "per capita" family income in each respective phase of life: conception to age 18 and age 19 to 32.[12] Family income includes labor earnings, transfer payments such as EITC, Temporary Assistance to Needy Families (TANF) and dividends and interests received for all members of the family whose income are reported in the PSID data. The PSID does not collect information

[11] Because health information is available for some individuals in multiple time period before and after reaching age 32, as a means of reducing potential measurement error in the dependent variables, I incorporate health information that is observed for these individuals between the ages of 28 and 32 for some of the health outcomes.
[12] For individuals whom health information is only observed prior to age 32, the average per capita income is measured up to the point that the health outcome is observed.

on EITC receipt. As such, I impute EITC benefits for each household in each year using the NBER TAXSIM Model and information for the head and spouse of the household and add that to all other sources of family income.

To get to "per capita" family income, I divide family income by family size. Defining average income in per capita terms accounts for differences in family size over time but it also implies that total family income is split equally among members of the family.[13] β_2 measures the average effect of a one log point increase in average per capita income from conception to age 18 on health at age 32, while holding constant the effect of income received between age 19 and 32. β_3 measures the average effect of a one log point increase in average per capita income from age 19 to age 32 on health at age 32, while holding constant the effect of average income from conception to age 18.

Using this specification, I can explicitly compare the health effects of a dollar received in phase 1 versus a dollar received in phase 2. This can be used to inform policy decisions about the stage of the life cycle in which a dollar of transferred income is relatively more effective as it relates to long-term health.[14]

Constructing Simulated Instruments

For an instrumental variables approach to yield causal estimates of income on health, one requires instruments that are correlated with income (relevance) but affect health only through income (exclusion restriction). I use simulated EITC benefits and simulated income as instruments for per capita family income. In the construction of these instruments, race, sex, age,

[13] I also estimate a model where family income is expressed relative to the poverty threshold instead of relative to the family size. The results are presented in the appendix (G).

[14] In the appendix, I also present results where I show the effects of "lifetime" average income (conception to age 32) and where I break out income from conception to age 32 into 3 phases and 5 phases. Coefficients estimated from separate regressions for income received in childhood and adulthood is also presented in the results section.

marital status, the number of children and the federal and state EITC parameters are considered exogenous. I discuss the construction of these simulated instruments separately in the next two sub-sections.

Simulating EITC Benefits

The EITC program is a federal program which was implemented in 1975 to assist low-income workers with qualifying children.[15] Since its inception, changes were made to the program at a number of margins over the years. These include the: credit rate, phase-out credit rate, minimum income for maximum credit, maximum credit, beginning income for phase-out rate and ending income. Starting in 1991, some of these parameters also varied based on the number of qualifying children. For example, in 1991, the maximum federal credit for one child was $2,128 and for two children was $2,205; but by 2018, the maximum federal credit for one child rose to $3,461 and rose to $5,716 for two children.[16] Some states also implemented their own EITC program which supplements federal EITC benefits for some recipients. Rhode Island, Vermont and Maryland were the first states to implement their own state-run EITC program in 1987; these state programs provide additional benefits to some residents who received federal EITC. As of March 2019, 29 states and the District of Columbia implemented their own version of the EITC program, with supplemental rates as high as 40% of the federal benefits.[17] In constructing simulated EITC benefits, I exploit variation coming from all these margins.

The steps for simulating EITC benefits are as follows. First, I use the NBER TAXSIM Software to estimate EITC benefits for every head and spouse in the CPS data for each year

[15] https://www.taxpolicycenter.org/statistics/eitc-parameters

[16] These benefits are in constant 2018 dollars. Additionally, starting in 1994 benefits were extended to childless individuals and in 2009 credit amounts differed for individuals who had three or more qualifying children.

[17] https://www.cbpp.org/research/state-budget-and-tax/states-can-adopt-or-expand-earned-income-tax-credits-to-build-a

between 1975 and 2017. For each of these individuals, I estimate their EITC benefits 51 times, each time assuming a different state of residence (including D.C.). Second, I form demographic groups for each state-year using only gender, race, marital status, number of children and age. Third, I take the average of the simulated EITC benefits by demographic group for each state-year which yields one observation per demographic group for each state-year. Fourth, I match each family in the PSID data in each year with their simulated EITC benefits based on only the demographic characteristics of the head, spouse and their state of residence.[18] For families where there is only a head of the household (no spouse present), that family is simply matched using the demographic characteristics of the head of the household. For families where there is both a head and a spouse present, that family is matched twice: first using the demographic characteristics of the head and the state of residence, then using the demographic characteristics of the spouse and the state of residence. I then take the average of the simulated EITC benefit for the head of the household and the average simulated EITC benefit for the spouse to form the simulated EITC benefit for that family. In essence, families with a spouse present are matched twice to capture the variation coming from changes in the income distribution of heads of households over time as well as the changes in the income distribution of spouses over time.

Intuitively, we can think of this instrument as the expected EITC benefits conditional on state, race, sex, age, marital status and the number of children. By using individuals from the CPS data who resemble a given head or spouse in the PSID on demographic characteristics to estimate EITC benefits, it breaks the link that would persist between an individual's family EITC

[18] Because of concerns that some groups will not be represented in all years in the CPS data due to the relatively small sample size, I create 5-year centered pooled cross sections to increase the number of observations for each year. For exposition, when deriving the simulated EITC benefits for each group in 1975, I use all CPS data from 1973 to 1977, when generating simulated EITC benefits for each demographic group in 1976, I use all CPS data from 1974 to 1978 and so forth. Taking centered pooled averages could also increase the predictive power of the instrument.

benefits and their unobserved characteristics. For example, the decision about how much time each member of the family will spend working will be correlated with both family income as well as the amount of time available for other activities that are expected to have long-term effects on children's health such as time spent on parenting activities. It does, however, treat the demographic characteristics as exogenous by controlling for these variables in the main equation of interest.[19] The identifying variation comes from the changes in the income distribution across demographic groups as well as the changes in the state and federal EITC parameters over time. Additionally, it treats the federal and state EITC parameters as exogenous by using these parameters to simulate EITC benefits. It also treats the state of residence at the time the health outcomes are observed as exogenous, by controlling for it in the equation of interest as fixed effects.

Simulating Income

I use data from the CPS to simulate pre-tax income. The steps are as follows. First, I form demographic groups for each year using sex, race, marital status, number of children and age. Second, I take the average household income by demographic group-year which yields one observation per demographic group for each year. Third, I match each family in the PSID data in each year with their simulated income based on the demographic characteristics of the head and the spouse.[20] Intuitively, this measures expected income conditional on the set of observable

[19] It might be argued that number of children for example might be endogenous. I examine how the results change when a different set of matching variables is used.

[20] I also use centered pooled cross sections because of concerns that some groups will not be represented in all years in the CPS data due to the relatively small sample size. By taking centered averages, this is also expected to be a better measure of expected income.

characteristics. Here, the set of demographic characteristics (sex, race, marital status, number of children and age) is treated as exogenous.[21]

Identifying Variation

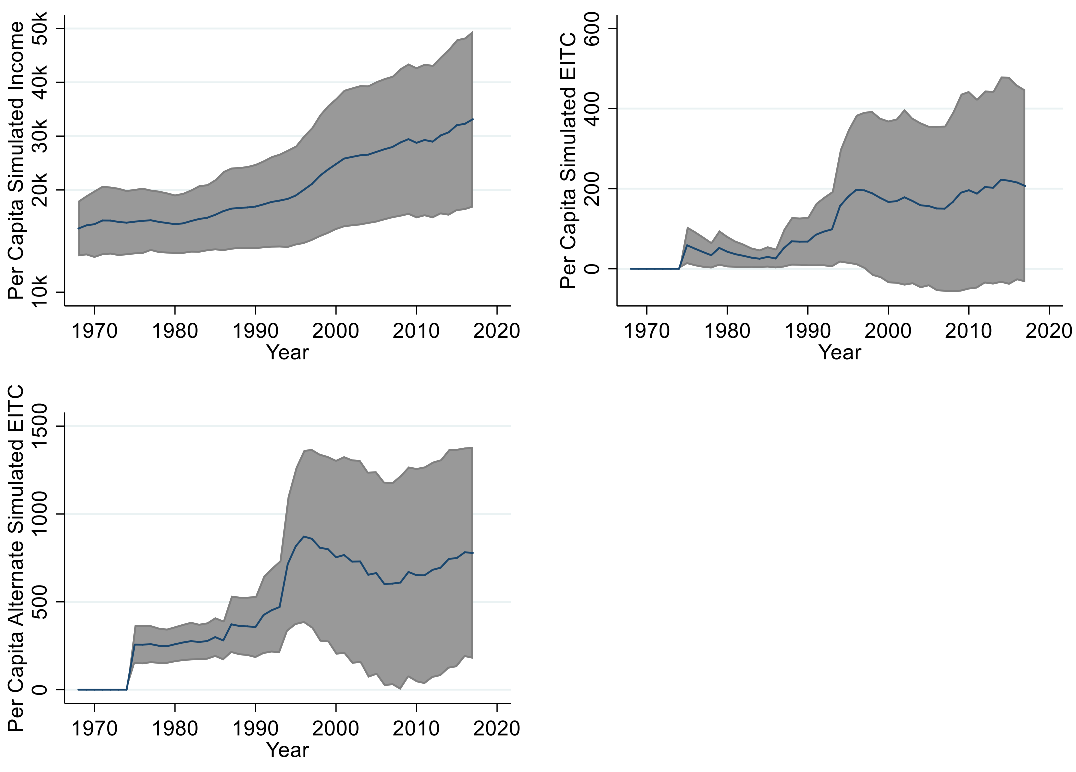
One of the primary sources of variation is changes made to the EITC program over time at both the state and federal levels. The other main source of variation that drives these instruments come from idiosyncratic changes in income across demographic groups over time. That is, how well individuals from one group are doing financially relative to individuals from other demographic groups. These sources of variation can also be described as being driven by changes in the income distribution across demographic groups over time. Identification also comes from individuals changing demographic groups over time based on changes in the demographic information of the head and or spouse of the household.

Figure 3 provides a graphical view of the average variation at the national level over time, for each of the simulated instruments. The blue line in each of the panels show the average year on year variation and the gray shaded are outlines the one standard deviation bandwidth. The upper left panel shows the variation for per capita simulated income which has a mean of $20,585 and a standard deviation of $12,954. The upper right panel shows the variation for per capita simulated EITC which has a mean of $158 and a standard deviation of $233. The lower left panel shows the variation for alternate approach to measuring per capita simulated EITC which has a mean of $585 and a standard deviation of $574. The difference between the average simulated EITC benefits across the first and the second approach to simulating EITC benefits comes from the fact that the first approach which uses actual income from the CPS data, will

[21] State of residence in each year of the survey is not used to simulate income because of concerns that the CPS data is not large enough to provide consistent estimates using state specific samples.

more closely reflect the actual amount of EITC benefits for which individuals would be eligible (across both recipients and non-recipients), while the second approach that uses the assigned income in constant 1975 dollars, will capture the variation in the generosity of EITC benefits over time and across states, but does not reflect the actual amounts of EITC for which individuals would be eligible and are more likely to receive. Additional graphs are provided in figure A1 and figure A2 in the appendix which shows the year on year variation in the two simulated instruments for EITC benefits for select states with their own version of the EITC program.

Figure 3: Variation in Simulated Instruments Over Time



For the instruments to be valid, two conditions must be satisfied. First, changes made to the state and federal EITC parameters must be uncorrelated with individuals' health. Second, a group's wellbeing with respect to income must be uncorrelated with other factors that affect an individual's health. In other words, all the information used to construct the instruments should be exogenous. While there is not a way to test these assumptions directly, I examine whether the results vary based on the set of demographic characteristics used in the construction of the instruments. If the results vary meaningfully when the set of demographic variables used to construct the instruments change, this might be suggestive evidence that this assumption does not hold.

In Appendix E, I show results for cases where I restrict the set of matching variables in different ways. Overall, the conclusions remain the same, with little variation in the magnitudes of estimated effects. I also show results for the case where I used a different approach to construct simulated EITC benefits. With the alternate approach, I assign a fixed real amount of income to each family in the CPS data in each year and simulate the amount of EITC benefits for which they would be eligible conditional on observables. This approach abstracts from observed income in the CPS data, which is potentially influenced by the structure of the EITC. The identifying variation for simulated EITC benefits in this case comes purely from changes made to the EITC parameters as well as from changes in the demographic composition of the household. These results are presented in Appendix D. In virtually all cases, the estimated coefficients are almost identical to those obtained using the first approach to simulate EITC benefits.

First Stage Regressions

The first stage regressions are as follows:[22]

$$\ln(Inc_{is}^{0-18}) = \alpha_s + \alpha_{st} + \alpha_1 X_{is} + \alpha_2 \ln(SimEITC_{is}^{0-18}) + \alpha_3 \ln(SimInc_{is}^{0-18}) + u_{is} \quad (2)$$

and

$$\ln(Inc_{is}^{19-32}) = \gamma_s + \gamma_{st} + \gamma_1 X_{is} + \gamma_2 \ln(SimEITC_{is}^{19-32}) + \gamma_3 \ln(SimInc_{is}^{19-32}) + v_{is} \quad (3)$$

where Inc_{is} is average per capita income, $SimEITC_{is}$ is average per capita simulated EITC benefits and $SimInc_{is}$ is average per capita simulated income.[23] X is the same set of controls described in the structural equation (equation 1) and the same set of survey-year, state and state by survey-year fixed effects are included.

The second stage of the IV regression is identical to the structural equation (equation 1), except that average income from conception to age 18 and average income from age 19 to 32 are replaced with their predicted values from equation (2) and (3), $\widehat{Inc}_{is}^{0-18}$ and $\widehat{Inc}_{is}^{19-32}$, which yields:

$$Y_{is}^{32} = \gamma_s + \gamma_{st} + \beta_1 X_{is} + \beta_2 \widehat{Inc}_{is}^{0to18} + \beta_3 \widehat{Inc}_{is}^{19to32} + \epsilon_{is} \quad (4)$$

I use the Stata command *ivregress 2sls* to estimate the regression coefficients, given that there is only one observation for each individual in this regression.

[22] Stata uses all excluded instruments for both endogenous variables. However, the regressions as outlined here do not reflect that. However, by excluding the instruments for income in adulthood when estimated the first stage regression for income in childhood (and vice versa), the F-stat(s) become smaller. Hence, these regressions provide a lower bound for the F-stat and gives a sense of the strength of the instruments in each phase for predicting income in each respective phase.

[23] Simulated EITC benefits contain zeros for individuals whose family are from demographic groups where no individual in the CPS data was eligible for EITC. As such, I add a small constant to simulated EITC benefits so all units contain strictly positive values. The added constant should have no impact on the results because this adjustment is only being made to the instrument. Supporting evidence can be provided upon request. In the appendix, I also show results for the case where I add simulated income and simulated EITC benefits which avoids having to add a normalizing constant to the instrument.

5. Results

In this section, I present first and second stage results. The second stage results are broken out into health indicators and health behaviors. Furthermore, I present two different sets of results for each; the first set of results include income from both phases simultaneously, while the second set of results include only income from one phase at a time. I also discuss the results from estimating separate regressions for income received in each phase of the life cycle and distinguishes the interpretations from the case where income in all phases are simultaneously included.

First Stage Results

Table 3 presents the results for the first stage regressions of log average per capita family income on log average per capita simulated EITC benefits and log average per capita simulated income in the respective phases. As mentioned earlier, average income in each phase includes predicted EITC benefits.

In the first case where the dependent variable is average income from conception to age 18, the F-stat is 71.55. The estimated coefficients are elasticities and should be interpreted as percentage changes. For example, the coefficient on simulated income during childhood is 0.443, which means that a 1 percent increase in average simulated income from conception to age 18 leads to a .443 percent increase in average per capita family income from conception to age 18. In the case where the dependent variable is average income between age 19 and age 32, the F-stat is 62.97. In both cases the F-stat is well above the conventional rule of thumb of 10, for the cases where there is a single endogenous variable.

The more relevant test for whether the instruments are weak come from using the Cragg-Donald Wald F-test for the case where there are multiple endogenous variables. Using the

Cragg-Donald Wald F statistic, the data rejects the null hypothesis that the instruments are weak. The minimum eigenvalue statistic is 103, which is much larger than the Stock-Yogo critical value of 16.87 for a 10% maximal IV size. Given that I have more instruments than endogenous variables, I also conduct tests of overidentification. The test statistics for both the Sargan and Basmann test of overidentification are statistically insignificant at the 10% level which means we cannot reject the null hypothesis that the instruments are valid.[24]

Table 3- First Stage Regression Results- Dependent Variables are Log Average Per Capita Family Income in Each Phase of the Life cycle

	(1) Age 0-18	(2) Age 19-32
Log Simulated Income (age 0-18)	0.443^{***}	0.227^{**}
	(0.0832)	(0.0907)
Log Simulated EITC Benefits (age 0-18)	0.0177	$(0.0930^{***}$
	(0.0118)	(0.1293)
Log Simulated Income (age 19-32)	-0.0482	0.584^{***}
	(0.0448)	(0.0489)
Log Simulated EITC Benefits (age 19-32)	0.0279^{***}	-0.0946^{***}
	(0.00647)	(0.0071)
Observations	5,386	5,386
F-stat	71.55	62.97

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.

[24] These are based on the second stage regressions. Additional results from the diagnostic tests can be provided upon request.

Simultaneously Estimating the Effect of Income from Both Phases on Long-term Health

In the next two sub-sections, I present the results for the long-term effect of income received during childhood and adulthood on health and health behaviors observed at age 32. All of these regressions simultaneously include income from conception to age 18 and income from age 19 to age 32. I first present the results for health outcomes followed by the results for health behaviors.

The regression results for all the health indicators are presented in Table 4. Each row contains results from two regressions (OLS and IV). Column 1 contains the name of the health outcome. Columns 2 and 4 contain the coefficients from the OLS regression for income in childhood and adulthood, respectively. Columns 3 and 5 contain the coefficients from the IV regression for income in childhood and adulthood, respectively. The coefficients should be interpreted as the effect of a one log point increase in income in the respective phase on the health indicator identified in column 1.

The data permits studying three health behaviors: exercising, smoking and drinking. Outcomes related to smoking and drinking are measured in several ways. The regression results are presented in Table 5.

Very Good or Excellent General Health

Consider first the dummy variable outcome measuring whether an individual is in very good or excellent health. The coefficient on income in childhood from the OLS regression in column 2 is positive but statistically insignificant. The corresponding coefficient on income in childhood from the IV regression in column 3 is also positive and statistically insignificant. Both the OLS and IV models suggest that income received during childhood does not have a long-term statistically significant effect on general health at age 32, while holding income in adulthood constant. However, the point estimate for income in childhood from the IV regression is economically meaningful (two and a half times larger). The coefficient on income in adulthood from the OLS regression in column 4 is positive and statistically significant and the coefficient on income in adulthood from the IV regression in column 5 is also positive and statistically significant. Both the OLS and IV models suggest that income received during adulthood increases the likelihood of being in very good or excellent health at age 32, while holding income in childhood constant.

The point estimate from the IV regression indicates that a 10% increase in average per capita income during adulthood increases the likelihood of being in very good or excellent health at age 32 by 1.2 percentage points.[25] For an individual living in a family of four at the poverty line, an additional per capita family income of $5,000 (in 2017 dollars) leads to an increase in the likelihood of being in very good or excellent health by 3.1 percentage points.[26] Relative to the baseline of 66%, this implies a 4.7% increase in the incidence of very good or excellent general health.[27] A crude comparison of this effect relative to the effect of having Food Stamp operating in the county from birth to age 5, suggests that the effect of Food Stamp is roughly 3.6 times larger.[28] The coefficient on income in childhood from the IV regression is approximately half the

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[25] The calculation is as follows: $100 * \{\beta * \log([100 + p]/100)\}$, where p is the percentage change in income (10%) and β is the point estimate (0.121)

[26] The calculation is as follows: $100 * \left\{\beta * \log\left[\frac{6{,}275+5{,}000}{6{,}275}\right] - \beta * \log(1)\right\} = 100 * \{0.121 * 0.254 - 0,\}$ since the natural log of 1 simplifies to zero. The poverty line for a family of four (two adults and two children) is $25,100 which in per capita terms is $6,275.

[27] To see whether these results depend on how the variable is measured, I ran two alternate regressions where general health is coded as: (1) a dummy variable for poor or fair health and (2) the untransformed 5-point scale general health variable. The relationship remains the same in both cases: statistically insignificant decline in general health due to income in childhood and statistically significant improvements in health due to income in adulthood.

[28] Hoynes et al., (2016) estimate that having Food Stamp operating in the county from birth up to age 5 increases the likelihood of being in good health by 11 percentage points. The 2017 average annual Food Stamp (Supplemental Nutrition Assistance Program) benefits for a family of four was $5,568 (in constant 2017 dollars) which works out to be approximately $1,392 per family member. Multiplying $1,392 by a factor of 3.6 would yield an equivalent change in Food Stamp Benefits per person as the change in per capita income per person of $5,000. Therefore, the estimated effect from Hoynes et al., (2016) is multiplied by a factor of 3.6. Please note that the estimate from Hoynes et al., (2016) was not precise ($\hat{\beta} = 0.110$, se=0.074).

size of the coefficient on income in adulthood, which suggests that a similar increase in income in childhood would increases the likelihood of being in very good or excellent health in early adulthood by approximately 1.7%. These effects sizes are not considerably large but provide evidence that income at different points in the life cycle has reasonably sized long-term effects on general health status, with evidence that income received after age 18 having a larger beneficial effect as compared to income received before age 18.

Obesity and Body Mass Index

The results for obesity from the IV regression show that an additional $5,000 in income during childhood increases obesity at age 32 by 2.8%, while an additional $5,000 of income during adulthood decreases obesity by 6.6%. However, only the coefficient on income received in adulthood is statistically significant at conventional levels. The corresponding point estimates from the OLS regression have the same signs as the coefficients from the IV regression, but they are smaller in magnitude.

More generally, the IV regression results for body mass index (BMI) show that a $5,000 increase in childhood income increases BMI in early adulthood by 0.3% while a similar increase income during adulthood decreases BMI by 1%. However, only income during adulthood is statistically significant.

The effect sizes for obesity are reasonably large. However, for BMI, the effects are smaller which might suggest that the effects of income vary with BMI. That is, at some points in the BMI distributions income received in either stage of the life cycle could have relatively larger effects, with the possibility of sign reversal as well.

Other Health Outcomes

As it relates to other health outcomes (metabolic syndrome, diabetes, hypertension, heart disease, heart attack, physical limitations, heart attack, arthritis, stroke and cancer) from the IV regressions, I find no statistically significant impact of either income received in childhood or income received in adulthood. These conditions are relatively rare among individuals between ages 28 and 32. As such, it is not surprising that we do not find any evidence of statistically significant effect of either income received during childhood or income received during adulthood. The estimates are very imprecise, with the standard errors in most cases being quite large relative to the point estimates. As it relates to the relative magnitudes, because these outcomes are rare among this age group of individuals, the change in the actual number of individuals being diagnosed with any of these conditions because of changes in income in either stage of the life cycle would be extremely small.

Additional Results for Health Indicators

Among all of the health outcomes, with the exception of general health status, obesity, BMI, Diabetes and Metabolic Syndrome, the point estimates for income in childhood were larger than the point estimates for income in adulthood in the IV regressions. Tests of differences between the coefficients for income in adulthood and income in childhood for each health indicator reveals statistically significant differences only for obesity, BMI, hypertension and physical limitations. These results are presented in Table A4 in the appendix. Larger coefficients on income in adulthood makes it more difficult to do policy comparisons of the relative efficiency of a dollar transferred in childhood versus a dollar transferred in adulthood. This is because income received in childhood has a longer time over which to have an impact as compared to income received in adulthood. Compared to the coefficients from IV regressions, the corresponding OLS estimates did not show systematic differences in terms of the relative magnitudes. This suggests that measurement error is not the main source of difference between the IV and OLS estimates. This is discussed in more detail later in the paper.

Table 4- The Effect of Log(Income) on Health

(1)	(2)	(3)	(4)	(5)
	Income in Childhood		Income in Adulthood	
Health Outcomes	OLS	IV	OLS	IV
Very good or excellent general health	0.0242	0.0619	0.112***	0.121***
	(0.0213)	(0.0464)	(0.0157)	(0.0266)
Obesity	0.00925	0.0426	-0.0766***	-0.0989***
	(0.0171)	(0.0384)	(0.0105)	(0.0273)
Body Mass Index	0.0171	0.484	-1.149***	-1.523***
	(0.230)	(0.682)	(0.191)	(0.452)
Diabetes	-0.00693	0.00354	-0.0167***	-0.0166
	(0.00460)	(0.0186)	(0.00466)	(0.00948)
Hypertension	0.0162	0.0572	-0.0351***	-0.0163
	(0.0116)	(0.0325)	(0.00948)	(0.0205)
Heart Disease	-0.000838	0.00265	-0.00225	0.00110
	(0.00501)	(0.0124)	(0.00427)	(0.00786)
Metabolic Syndrome	-0.00446	0.00831	-0.00672*	-0.0107
	(0.00272)	(0.0165)	(0.00325)	(0.00736)
Physical limitations	-0.00514	0.0667	-0.0495***	-0.0103
	(0.00900)	(0.0372)	(0.0103)	(0.0176)
Heart Attack	-0.00219	-0.0123	-0.00347	-0.00394
	(0.00200)	(0.00850)	(0.00298)	(0.00595)
Arthritis	-0.00858	0.0210	-0.0181*	-0.0171
	(0.00877)	(0.0210)	(0.00678)	(0.0130)
Stroke	-0.00238	-0.0121	-0.00494	-0.00119
	(0.00410)	(0.0129)	(0.00394)	(0.00740)
Cancer	0.00166	-0.00918	-0.00499	0.000921
	(0.00576)	(0.0161)	(0.00476)	(0.00805)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Exercise

The exercise variable measures whether an individual participates in any exercise for at least 10 minutes per week. The results from the IV regression (Table 5) show that childhood income has a small and statistically insignificant impact, while income during adulthood increases the likelihood of exercising at least 10 minutes per week. The point estimate on income in adulthood from the IV regression is more than twice the size of the coefficient on income received in childhood. The IV estimates were larger in absolute magnitude relative to the OLS estimates which would suggest that the OLS estimates are biased toward zero.

Smoking

I consider three smoking related outcomes: whether an individual currently smokes, the number of cigarettes currently smoked per day (including non-smokers) and whether an individual has quit smoking by age 32, conditional on ever being a smoker. The results are presented in Table 5. For whether an individual currently smokes, the IV results show statistically insignificant coefficient for income during childhood. While this coefficient is negative, it is an extremely small effect size (less than quarter of a percent). The coefficient on income during adulthood is negative and statistically significant, with a relatively large effect size of almost 9%.

Regarding number of cigarettes currently smoked per day, the IV results show a small (positive coefficient) and statistically insignificant effect of income during childhood while income in adulthood generates a large statistically significant reduction (12.7%) in the incidence of smoking. For quitting smoking, the coefficient on income in childhood is positive and statistically insignificant but the effect size is roughly 2.4% which might be considered a meaningful increase in the quit rate by age 32, among individuals who ever smoked. The IV results also show that income in adulthood generates a statistically significant increase in the probability of quitting, with an effect size of 5.2% which is reasonably large. However, it should be noted that quitting is a very selected outcome given that one needs to be a smoker before they can quit. There is no systematic difference between the magnitudes of the corresponding OLS and IV point estimates.

Drinking

As it relates to drinking, I consider three outcomes: being a current drinker, number of drinks consumed per day and the number of days that an individual consumed 4 or 5 drinks (binge drinking) per year. In all cases, both the OLS and IV point estimates for income in childhood and adulthood were positive. The IV regression for whether an individual currently drinks alcohol shows a statistically insignificant effect of income received during childhood (effect size of almost 2%) and a considerably larger sized statistically significant impact from income received in adulthood (effect size of 3.22%). For the number of drinks per day (including non-drinkers), neither income received in childhood nor income received in adulthood was statistically significant. The effect sizes are relatively small, but income received during childhood has a noticeably larger point estimate (almost double) than income received during adulthood. For binge drinking, income received in childhood is statistically significant (at the 10% level) and is more than twice the coefficient on income received during adulthood, while the point estimate on income received in adulthood is small and statistically insignificant.

Additional Results for Health Behaviors

Among all of the health behaviors, the IV point estimates for income in adulthood was larger than the point estimate for income in childhood in most cases. However, the differences in magnitudes were only statistically significant for whether individuals currently smoke and the number of cigarettes smoked per day. These results for the test of differences between the coefficient on income in adulthood and the coefficient on income in childhood from the IV regression for each health behavior is presented in Table A5 in the appendix. Nonetheless, the differences in magnitudes in most cases were economically meaningful, which will play an important role in determining the relative efficiency of a dollar transferred in earlier versus later life. Furthermore, for the number of cigarettes smoked, the sign on income in adulthood was opposite the sign on income in childhood, with income in adulthood reducing the number of cigarettes smoked per day. There was no systematic difference between the magnitude of the IV point estimates and the corresponding estimates from the OLS regressions, which suggests that if there is a bias when OLS is used, the direction of the bias could vary based on the behavior that is considered.

Table 5- The Effect of Log(Income) on Health Behaviors

(1)	(2)	(3)	(4)	(5)
	Income in Childhood		Income in Adulthood	
Health Behaviors	OLS	IV	OLS	IV
Exercise				
Any exercise at least 10 mins. per week	0.00405	0.0311	0.0485***	0.0721**
	(0.0119)	(0.0474)	(0.0105)	(0.0242)
Smoking				
Whether currently smokes	-0.0114	-0.00329	-0.125***	-0.101***
	(0.0158)	(0.0516)	(0.0129)	(0.0279)
Number of cigarettes currently smoked per day (including non-smokers)	-0.384	0.149	-1.657***	-1.705***
	(0.302)	(0.781)	(0.187)	(0.394)
Quit smoking	0.0397	0.0609	0.109***	0.130**
	(0.0268)	(0.0911)	(0.0254)	(0.0479)
Drinking				
Whether currently drinks alcohol	0.0514*	0.0760	0.100***	0.124***
	(0.0194)	(0.0628)	(0.0126)	(0.0287)
Number of drinks per day (including non-drinkers)	0.0351	0.151	0.0819	0.0759
	(0.0718)	(0.209)	(0.0548)	(0.106)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	1.125	4.151*	0.454	1.652
	(1.060)	(1.982)	(0.973)	(1.775)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Accounting for Testing Multiple Hypotheses

While several health related outcomes are available in the PSID data set, four outcomes were identified as being of primary interest. These include general health status, BMI and risky health behaviors- drinking and smoking. These outcomes were selected based on the existing literature and the fact that many of the health conditions that are available in the data rarely manifest by age 32. The challenge is that in a relatively small sample, it would be unlikely to detect statistical significance at conventional levels. Furthermore, by focusing on multiple outcomes, the risk of encountering type 1 error also increases.

Consistent with Veazie (2006), I conduct a joint F-test that all the coefficients on income in the regressions related to one of the above four health related outcomes are jointly zero.[29] In total, there were 8 outcomes. These include general health status, BMI, obesity, current smoking status, number of cigarettes smoked per day, current drinking status, number of drinks per day and binge drinking. This approach was taken because the outcomes that are considered are expected to be correlated. I use the stata *suest* command to account for the covariance when the joint F-test is conducted. This avoids relying on the assumption that the outcomes are independent. The null hypothesis that there is no effect of income at any stage in the life cycle (childhood nor adulthood) on any of the 8 outcomes is rejected at the 0.1 percent level (p-value<0.001). While this test does not allow us to say on which of the outcomes income has an effect, we can reasonably focus on the outcomes that were of primary interest based on the prior literature and the age of the sample members.

[29] TheF-test could not be conducted jointly for the full list of outcomes in stata because the matrix size is too large for stata to handle.

Power Analysis

I also conduct a post-hoc power analysis for each outcome of interest in the context of a multiple linear regression model which confirms the prior belief that in a relatively small sample of individuals, there would not be enough power to detect statistical significance at conventional levels for the health conditions that rarely manifest by age 32. I use the Stata built in command *power pcorr*. I calculate the power given the sample size, the squared semi-partial correlation, the number of parameters to be jointly tested (1) and the number of additional covariates (including state and time fixed effects the total is 121). These results are presented in Table A2 and Table A3 in Appendix A.

For income in childhood, the results show relatively low levels of power for most of the outcomes. For income in adulthood, the results show relatively low power for those outcomes that rarely occur among individuals age 32 such as heart disease, heart attack, stroke and cancer. These results indicate that it will be less likely to detect a statistically significant relationship for the health conditions that rarely occur by age 32. Among the outcomes that are not as rare in early adulthood, these results indicate that it will be more likely to determine statistically significant effects for income received during adulthood, relative to income received during childhood.

The Effect of Income on Long-term Health: Separate Models for Income Received in Each Phase

In this section, I present the IV regression results from a model that includes only income from a single phase, along with results from the previous model that includes income in both phases. The previous results provide evidence that income received in both phases matter for long-term health. However, as discussed in Flavio & Heckman (2008) and Heckman (2007) there may be complementarity or substitutability of inputs to human capital development over time.[30] This could lead to different estimates based on whether income from both phases are included or just income from a single phase. Differences may also arise because of the correlation between income over the life cycle. Estimates from these two different sets of regressions would have different interpretations. Consider the simple model where there are no control variables:

$$y_i^{adult} = \beta_0 + \beta_1 inc_i^{child} + \beta_2 inc_i^{adult} + \varepsilon_i \qquad (5)$$

$$inc_i^{adult} = \alpha_0 + \alpha_1 inc_i^{child} + u_i \qquad (6)$$

That is, health in adulthood (y_i^{adult}) is determined by income in childhood and income in adulthood, while at the same time income in childhood is correlated with income in adulthood.

Table 6 below contains the results from two regressions of childhood income on income received during adulthood, while controlling for the full set of variables. Column 2 contains the coefficient on income during childhood from the OLS regression of childhood income on income during adulthood. Column 3 shows estimated effect of childhood income on income during adulthood using the 2-stage least squares estimator, where simulated income during childhood and simulated EITC benefits during childhood are used to instrument for income during childhood. The point estimate from the OLS regression shows that a 1% increase in income during childhood is associated with a 0.57% increase in income during adulthood. When using the instrumental variables approach, the effect of a 1% increase in income during childhood reduces to an estimated effect of a 0.36% increase in income during adulthood. This highlights that while accounting for the possible endogenous relationship between income in childhood and

[30] I also estimate a set of regression models where I include an interaction term for income received in childhood and income received in adulthood which are discussed toward the end of this section.

income in adulthood reduces the strength of the relationship, there still remains a strong link between childhood income and income received during adulthood. This suggest that income during adulthood might be one potential pathway through which income received during childhood affects health.

Table 6: Relationship Between ln(Childhood Income) and ln(Income in Adulthood)

	(1) OLS	(2) IV
ln(Childhood Income)	0.5663***	0.3558***
	(0.0218)	(0.0276)

Notes: Each estimate is from a separate regression.
Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Both regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Substituting for inc_i^{adult} in equation 5 yields:

$$y_i^{adult} = \gamma_0 + (\beta_1 + \beta_2\alpha_1)inc_i^{child} + v_i \qquad (7)$$

where $\gamma_0 = \beta_0 + \beta_2\alpha_0$ and $v_i = \varepsilon_i + \beta_2 u_i$. By substituting for income in adulthood, the regression model estimates the *total effect* of income received in childhood on health in adulthood which is given by $\beta_1 + \beta_2\alpha_1$. The *direct effect* of income received in childhood on health in adulthood is given by β_1 and the *indirect effect* of income received in childhood on health in adulthood (childhood income affects income in adulthood and in turn, income in adulthood affects long-term health) is given by $\beta_1\alpha_1$, where α_1 is the correlation between income received in childhood and income during adulthood. In this case, the relative efficiency of a dollar transferred in childhood versus a dollar transferred in adulthood requires comparing $(\beta_1 + \beta_2\alpha_1)$ to β_2. The relative efficiency crucially depends on the sign and absolute magnitude of β_1

and α_1. If, for example, income in childhood has no direct effect on health in adulthood then β_1 would be equal to zero and if the absolute value of α_1 is strictly between zero and one, then assigning income in adulthood would result in a relatively larger effect on health in adulthood as compared to assigning income in childhood. Whether this larger effect is beneficial to health depends on the outcome that is being considered and the sign of β_2.

In the context on the multiple regression model and using two different sets of instruments, the predictions are not as clear. To examine whether by accounting for income received during adulthood the effect of childhood income on health outcomes in early adulthood is changed in a systematic way, I estimate a regression model that excludes income received in adulthood and instrument for income received in childhood with simulated income in childhood and simulated EITC benefits in childhood. Because there are multiple covariates, it is not clear whether estimates from the model that simultaneously includes income received in both phases will be larger or smaller, as compared to the estimates from the model that includes only income from childhood. However, if we observe that by excluding income in adulthood, the long-term health effects of income during childhood vary in a systematic way, this suggests that higher income during adulthood is a mechanism through which more income during childhood affects long-term health.

I also estimate regression models that include only income received in adulthood (excludes income received in childhood). The coefficients from these regressions do not have a clear interpretation. This is because isolating the effect of income received in adulthood requires an instrument that is uncorrelated with income received during childhood. The instruments that I construct by definition do not satisfy this requirement. This is because the instruments exploit variation from changes in the income distribution across demographic groups over time and for any given family, the demographic group in which it falls in any given year will be correlated with both past and future demographic characteristics. Because of this, the variation in the instruments will also be correlated over time for any given family. As such, the coefficients from the model that excludes income during childhood are only examined on the premise of statistical significance. These results are not useful for distinguishing whether the effects of income are driven by income received during childhood or income received during adulthood. However, these results are useful for determining whether previous income affects health in early adulthood. Any observed effect will be driven by some combination of income received during adulthood and income received during childhood.

The estimates for health outcomes are presented in Table 6 and the results for health behaviors are presented in Table 7. Results in column 3 labelled “Long”, are from separate IV regressions of childhood income on the dependent variable of interest, while excluding income received in adulthood. Results in column 5 labelled “Short” are from separate IV regressions of income received in adulthood on the dependent variable of interest, while excluding income from childhood. The results in columns 2 and 4 of Table 6 and Table 7, which are labelled “Both”, are taken from Table 4 and Table 5 respectively. These “Both” results, are from the models that simultaneously include both income in childhood and income in adulthood. These “Both” results are included purely for ease of comparing the results across the two models.

The corresponding results from the regressions that include income from both phases look qualitatively similar to the results from the regressions that include only income from a single phase at a time in most cases. Regarding the coefficients on childhood income, for all except two outcomes (whether currently smokes and quit smoking), the signs of the coefficients were the same across the two models. There were no systematic differences as it relates to the relative magnitudes of the coefficients on childhood income across the two models. In some cases, the coefficient from the “Both” model was larger, while in other cases the coefficient from the “Long” model was larger. Statistical significance changed only for the obesity outcome. The total effect of income received in childhood results in a statistically significant increase in obesity in adulthood at the 10% level. Regarding the coefficients on income during adulthood across the two models: the signs were always the same and the magnitudes and statistical significance were almost identical.

These results suggest that the observed effects of income received in adulthood are not driven by income from conception to age 18. However, as it relates to the coefficients on childhood income, even though they are not statistically significant, for some of the outcomes, the effects become economically meaningful when income received during adulthood is simultaneously included in the regression.

To further investigate the dynamics of the relationship between income over the life cycle and health in early adulthood, I also estimate a model where I include an interaction term for childhood income and income received in adulthood. These regressions can be used to provide evidence of whether more resources being available in both stages of the life cycle for investments in health results in complementarities or substitutability. That is, will more resources being available during childhood make later life resources more productive or less productive in the health production process? In the case where the interaction term is positive, there is complementarity, whereas a negative interaction term implies that income in different phases are temporal substitutes. If the outcome is measured as a “good”, for example very good or excellent health and the interaction term is positive, then it means that income is each phase reinforce each other and make that measure of health better. These results are presented in Table A6 and A7 in

Appendix A. In most cases the interaction terms are negative, which implies that income over time are substitutes for most of these health outcomes. However, by including the interaction terms, the coefficients on income during adulthood and income during childhood reverses sign in most cases which likely occurs because of the strong correlation between income over time. As such, these results should be viewed with caution.

Given that, income received up to age 18 may have non-linear developmental effects, in the next sub-section, I discuss the results for the case where income is broken out into three phases instead of two phases.

Table 7- Comparing Health Effects of Log(Income) Across Two Models

(1)	(2)	(3)	(4)	(5)
	Income in Childhood		Income in Adulthood	
Health Outcomes	Both	Long	Both	Short
Very good or excellent general health	0.0619	0.00250	0.121***	0.117***
	(0.0464)	(0.0561)	(0.0266)	(0.0288)
Obesity	0.0426	0.0984*	-0.0989***	-0.103***
	(0.0384)	(0.0440)	(0.0273)	(0.0274)
Body Mass Index	0.484	1.364	-1.523***	-1.578***
	(0.682)	(0.847)	(0.452)	(0.466)
Diabetes	0.00354	0.0121	-0.0166	-0.0171
	(0.0186)	(0.0210)	(0.00948)	(0.00996)
Hypertension	0.0572	0.0639	-0.0163	-0.0199
	(0.0325)	(0.0345)	(0.0205)	(0.0211)
Heart Disease	0.00265	0.00228	0.00110	0.000856
	(0.0124)	(0.0119)	(0.00786)	(0.00779)
Metabolic Syndrome	0.00831	0.0154	-0.0107	-0.0117
	(0.0165)	(0.0179)	(0.00736)	(0.00752)
Physical limitations	0.0667	0.0725	-0.0103	-0.0151
	(0.0372)	(0.0379)	(0.0176)	(0.0174)
Heart Attack	-0.0123	-0.0101	-0.00394	-0.00330
	(0.00850)	(0.00946)	(0.00595)	(0.00620)
Arthritis	0.0210	0.0283	-0.0171	-0.0184
	(0.0210)	(0.0231)	(0.0130)	(0.0136)
Stroke	-0.0121	-0.0131	-0.00119	0.000371
	(0.0129)	(0.0128)	(0.00740)	(0.00713)
Cancer	-0.00918	-0.0118	0.000921	0.00198
	(0.0161)	(0.0183)	(0.00805)	(0.00876)

Notes: Each parameter in column 3 and column 5 is from a separate IV regression which includes only income from a single phase of the life cycle along with the control variables.
“Both” indicates that the estimates are from the model that simultaneously include both income in childhood and income in adulthood, “Short” indicates that only income in adulthood is included and “Long” indicates that only income during childhood is included.
Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table 8- Comparing the Effects of Log(Income) on Health Behaviors Across Two Models

(1)	(2)	(3)	(4)	(5)
	Income in Childhood		Income in Adulthood	
Health Behaviors	Both	Short	Both	Short
Exercise				
Any exercise at least 10 mins. per week	0.0311	0.00429	0.0721**	0.0689**
	(0.0474)	(0.0570)	(0.0242)	(0.0253)
Smoking				
Whether currently smokes	-0.00329	0.0454	-0.101***	-0.0998***
	(0.0516)	(0.0480)	(0.0279)	(0.0276)
Number of cigarettes currently smoked per day (including non-smokers)	0.149	0.846	-1.705***	-1.666***
	(0.781)	(0.839)	(0.394)	(0.398)
Quit smoking	0.0609	-0.0306	0.130**	0.122*
	(0.0911)	(0.0965)	(0.0479)	(0.0483)
Drinking				
Whether currently drinks alcohol	0.0760	0.0260	0.124***	0.117***
	(0.0628)	(0.0646)	(0.0287)	(0.0289)
Number of drinks per day (including non-drinkers)	0.151	0.153	0.0759	0.0600
	(0.209)	(0.216)	(0.106)	(0.105)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	4.151*	3.230	1.652	1.364
	(1.982)	(2.348)	(1.775)	(1.778)

Notes: Each parameter in column 3 and column 5 is from a separate IV regression which includes only income from a single phase of the life cycle along with the control variables. "Both" indicates that the estimates are from the model that simultaneously include both income in childhood and income in adulthood, "Short" indicates that only income in adulthood is included and "Long" indicates that only income during childhood is included.
Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Non-linear Developmental Effects During Childhood - Income Broken out into Three Phases

In this section, I discuss the results from the regressions where income is broken out into three phases instead of two. In addition to income received from age 19 to age 32, income during childhood is broken out into two groups: income from conception to age 6 and income from age 7 to age 18. The full set of results for these regressions are presented in Table A10 and Table A11 in the appendix. As it relates to the health outcomes, the magnitudes of the coefficient on income received from age 0 to 6 are very similar to the magnitudes of the coefficients estimated from the regressions where average income is take between conception and age 18 and age 19 to 32. However, for two of the health outcomes, very good or excellent general health and cancer, the coefficients on income between age 0 to 6 became statistically significant (at the 10% level). Income from conception to age 6 improves the likelihood of being in very good or excellent health and decreases the likelihood of being diagnosed with cancer in early adulthood. For very good or general health status, that the coefficient on income between age 0 to age 6 became statistically significant is not a surprise given that it was only marginally statistically insignificant in the regression for income between age 0 to 18. However, for cancer, the fact that the coefficient on income received between age 0 to age six results in a statistically significant reduction in the likelihood of being diagnosed with cancer in early adulthood is consistent with epidemiological literature which finds that adverse early life condition is an important pathway to ill health in adulthood, especially for cancer and metabolic diseases (Burdge, Lillycrop & Jackson, 2008; Kelly-Irving et al. 2013).

For physical limitation and arthritis, the coefficients on income received between age 7 and age 18 are positive and statistically significant at the 10% level. One potential explanation is that additional family income during these years may provide more resources for the children to engage in strenuous activities such as certain sporting activities, which could lead to later joint issues and physical limitations. However, Antony et al. (2016) in a meta-analysis highlights that existing research on the relationship between strenuous activities in childhood and knee structures is mixed.

The coefficients and statistical significance for income received in adulthood changes only very little as compared to the previous models. An interesting observation is that for many of the health indicators, the sign of the coefficients on income from age 0 to age 6 and age 19 to 32 are opposite the sign of the coefficients on income from age 7 to age 18. In many of these cases the results suggest long-term beneficial health effects of income received in the first seven years of life and income after age 18. This highlights that that the effects of income received at different stages in the life cycle on later life health might not only vary in terms of magnitudes but might also vary in terms of direction, which has important implications for policy analysis.

Considering the health behaviors, I find that higher income from conception to age 6 and income after age 18 leads to statistically significant declines in the incidence of smoking and the number of cigarettes currently smoked per day, while income received between age 7 and age 18 shows evidence of having the opposite effect. Additionally, income received between age 7 and 18 increases the number of days per year in which 4 or 5 alcoholic drinks are consumed. These results suggest that behaviorally something interesting occurs in the middle period. One possible explanation is that the period between age 7 and 18 years in which certain types of behaviors are developed, which results in either persistence of or encourages participation in certain behaviors in early adulthood. This is particularly interesting because these risky health behaviors are often thought of as being addictive, which makes sense for these behaviors to have been developed prior to early adulthood. I also estimated a model that breaks out childhood income from age 0 to age 12 and age 13 to age 18. The estimated effects for the coefficients on income received between ages 13 and 18 are almost identical to the coefficients for income received between age 7 and age 18 but are more precise and tells a stronger story that the increase participation in risky health behaviors in early adulthood might be coming from the teenage years. [31] The fact that income in the teenage years does not show strong evidence of having adverse effect on the health indicators could be due to the relatively young age of the sample that is considered. That is, by age 32, the adverse effects of participation in risky health behaviors would not have yet manifested.

Overall, the results suggest that higher income during those years between age 7 and 18 have no beneficial effect (limited evidence of harmful effects) on long-term health, but strong evidence that risky health behaviors such as smoking and binge drinking are induced by higher income between ages 7 and 18, which is primarily driven by income received in the teenage years. Potentially, individuals are more willing to indulge in activities in their “teenage years” that may have harmful health effects later in life.

Height as a Placebo Test

I use height in adulthood to examine whether an outcome that we do not expect to be affected *a prior* by income received in adulthood is in fact affected. I estimate the effect of income from conception to age 18 and income beyond age 18 on height in adulthood. Because, virtually no growth in height takes place after age 18, if the empirical strategy works, we should

[31] These results are available upon request. Additionally, I estimate a regression model where income is broken out into 5 phases which are roughly equally spaced. These results are presented in Tables A15 and A16 in Appendix A. These results also provide some supporting evidence for income around those teenage years having adverse effects, especially on risky health behaviors.

not see any effect of income from age 19 to age 32 on height in adulthood. The results in Table A8 in the appendix show no statistically significant effect of income on height in adulthood. The magnitude on income received after age 18 is extremely small. It implies that a $15,000 increase in per capita income each year between age 19 and age 32 (approximately a 50% increase in per capita income during adulthood) will increase height in adulthood by 0.172 inches. Even though there is no statistically significant difference between the coefficient on income received during childhood and the coefficient on income in adulthood, the point estimate on income received during adulthood is smaller. The fact that we do not find evidence of an effect of income received during childhood is consistent with the find across developed countries, that income does not affect height. Overall, these findings provide reassurance in the empirical strategy that was implemented.

Sensitivity Analyses

In the appendix, I present results from several sensitivity analyses. The set of sensitivity analyses include: adding state specific times trends; testing for and addressing potential endogenous migration; testing for and addressing potential attrition bias; using an alternate approach to simulate EITC benefits; summing simulated EITC benefits and simulated income to form a single instrument; varying the set of matching variables used to construct the instruments; and expressing family income relative to the poverty threshold instead of relative to the number of individuals in the family. In most cases the results vary only marginally across these analyses. In a few cases, the magnitudes of the estimated coefficients change somewhat, but the conclusions remain the same. The only exception is for the number of alcoholic drinks consumed per day. The exception is that when income is expressed relative to the poverty threshold, the coefficient on income during adulthood for the number of alcoholic drinks consumed per day switches sign to become negative and is statistically significant at the 5% level. Since this is an exception rather than a common theme, these results strengthen our confidence in the empirical strategy that is employed.

Given that the IV estimates in most instances are larger than the OLS estimates, one possible reason might be that the income variable is measured with error. As such, I examine the extent to which measurement error in the income variable alone might be able to explain the differences between OLS and IV estimates. This analysis regarding measurement error shows no evidence that the difference between the OLS and IV estimates can be fully explained by measurement error. These results are also presented in the Appendix F. Additionally, I estimate the effect of average life-time income instead of income in each phase of the life cycle. The analysis using average life-time income shows statistically significant effects of average lifetime income on long-term health, but mask important differences coming from income received at different points in the life cycle.

6. Discussion

The analysis shows that income has important effects on health and health behaviors that depend on the stage in the life cycle in which income was received. Income received during adulthood often has a larger beneficial effect than income received during childhood. I also find that treating income up to age 18 as a single measure of income in childhood, masks important differences coming from the age at which income was received during childhood. Income received in the first 7 years of life has beneficial long-term effects while income received between the ages of 7 and 18 tends to have some harmful effects. I also find that when income from either phase in the life cycle is omitted from the regression, the results vary and have more complicated interpretations. In most cases, omission affects the magnitudes of the coefficients while in a few cases it also leads to sign reversal. Additionally, I find that using a simple approach like OLS often results in estimates that appear biased. While in most cases the bias is towards zero, there are cases where this approach results in an upward bias. This suggest that measurement error might not be the main reason for the differences that arise between OLS and IV estimates. Additionally, this suggests that if endogeneity is the main source of these differences, unobserved factors may affect outcomes differentially.

While some of the previously discussed studies find very little to no causal effect of income on health, those estimates are limited in their relevance within this context. This is because their focus is often on one-off income shocks which are likely poor measures of permanent income. Furthermore, these studies typically focus on contemporaneous effects or effects in the very near future. However, most health outcomes often take years to materialize. Furthermore, the average amount of income families received from a one-time income shock is relatively small, which even if spread over a longer period of time might still not be big enough to generate large changes in health.

The finding that beneficial effects of income on long-term health are driven by income received in the later phase of the life cycle is somewhat surprising, given that the vast number of studies which examine the long-term effects of only *in utero* or early life environment on later life outcomes find relatively large effects. Within the context of health, that *in utero* conditions and conditions during infancy have persistent effects on health, even when meaningful health effects are not observed in the short run, is widely documented (Gluckman, et al., 2008). The idea is that the physiology of the human body is such that certain latent attributes are developed at crucial stages in life, such as *in utero* and early childhood. Whether individuals experience harsh or friendly environments would make individuals more or less prone to certain kinds of diseases later in life, even if evidence of these effects do not manifest in early life according to this hypothesis.

Using the models that break out income into only two phases, there is little evidence that income during childhood has long-term health effects. However, in the models where income is broken out into three phases (age 0 to 6, age 7 to 18 and age 19 to age 32), reveal otherwise. This is because in many cases the sign on the coefficient on income in the later childhood years is the opposite of the sign on the coefficient on income in the early childhood years. Thus, by combing early and later childhood years as a single phase (childhood years), the estimated coefficients are smaller in absolute magnitudes. While in many cases the coefficient on income received between age 0 and age 6 are not statistically significant, the magnitudes are economically meaningful in many cases. That income received between ages 7 and 18 could have potentially harmful effects suggest there are complicated dynamics at work which requires a more detailed analysis.

Given that the results vary based on whether income received later in life is simultaneously included in the model along with early life income might be related to the capital formation model discussed in Flavio & Heckman (2008) and Heckman (2007). In the context of a multistage health production function, the idea is that health inputs at different periods may reinforce or offset each other. In the case of reinforcing effects, health investments in early childhood need to be followed up by investments later in life to be most effective. These findings also reflect the point made in Heckman (2007), that there might be several important periods in childhood. Furthermore, this also highlights that some of the health effects of early childhood income operates through increasing income in later periods of the life cycle.

As it relates to the health behaviors, these patterns are more pronounced. Given that many of the health conditions considered in this paper do not manifest frequently by age 32, could be the reason why the effects are not as large for some of the health indicators. However, as these cohorts get older, re-examining their health outcomes later in life may provide further insights.

That some of the IV estimates are larger in magnitude than OLS estimates is consistent with previous studies such as Ettner (1996) and Lindhal (2005). The conventional argument for why IV estimates might be larger than OLS estimates is that there is measurement error in the independent variable which results in attenuation bias in the OLS estimates. However, given that income is being averaged within a given phase in the life cycle, insofar as the measurement error is independent across the different waves of the data, potential concerns about measurement error is somewhat mitigated. Furthermore, with IV estimates as much as four times larger than OLS estimates, it is unlikely that such differences are driven purely by measurement error. This is supported by two sets of findings. The first set of findings which runs contrary to the reasoning that the differences between OLS and IV estimates can be fully explained by measurement error in the income variable is that in some cases the IV results are smaller than the OLS estimates. If it were a purely measurement error story, then we would expect the IV estimates to be larger than the OLS estimates. Given that is not the case, the differences between the OLS and IV estimates at least in part arises because of endogeneity.

The second set of results which runs contrary to the reasoning that the differences between OLS and IV estimates can be fully explained by measurement error in the income variable come from secondary analyses that I also conduct. In this analysis I estimate two sets of regressions. The first set of regression estimate the effect of average income at even ages on each health variable using OLS and the second set of regressions estimate the effect of average income at even ages on each health outcome using 2-SLS by instrumenting for income received at even ages with income received at odd ages. Under the assumption that any measurement error in income received at even ages would be independent of the measurement error at odd ages, the difference in the size of the OLS and IV estimates gives a sense of the degree of the measurement error. If the differences between these OLS and IV results are large enough, compared to the main regression results, this would provide evidence supporting the hypothesis that measurement error alone might account for the differences between the OLS and IV regressions from the main analyses. These results do not provided any evidence that the differences in magnitudes between the OLS and IV estimates in the main regressions can be fully explained by measurement error alone.[32] Also, the fact that the IV estimates are smaller in some cases is evidence against a measurement error story.

The issue of measurement error aside, larger IV estimates imply that unobserved factors which affect both income and health must be negatively correlated. By using the IV approach to

[32] These results are presented in the appendix.

solve this problem, the estimates become larger. The question is what unobserved factors might we expect to be positively correlated with income but affect health negatively? One potential candidate is risk preferences. Shaw (1996) finds that increases in wages is positively correlated with the preference for taking risk. Furthermore, Dohmen et al., (2011) find evidence of intergenerational transmission of risk preferences. Unfortunately, the PSID contain only very limited information related to risk avoidance from 1968 to 1972. When this information is included as a control variable there are only marginal changes in the coefficients on income.[33] However, along the line of risk preference, I find evidence that increased income results in more alcohol consumption, but less smoking. One potential explanation might be that certain risky health behaviors may be viewed as more socially acceptable and higher income results in participation in those kinds of risky health behaviors. There are other social activities such as the frequency with which higher income earners attend social events and consume more food or unhealthier food than they otherwise would have, which could potentially result in adverse effects of income on health. The PSID contains some information on "eating out" which I will examine to see if it can be used to test this hypothesis.

[33] Given that risk avoidance is measured for head of the household between 1968 and 1972, it should be noted that with the family structure changing over time, this might not be a good measure of risk preferences for the family after the relevant sample members were born.

7. Conclusion

In this paper, I study the effect of income received at different stages in the life cycle on long-term health. I implement an instrumental variables approach to obtain a causal interpretation. The instrumental variables approach exploits variation from two primary sources: variation from the changes made to the structure of the EITC program at the federal and state levels over time and changes in the income distribution across demographic groups. I find that income after age 18 and income before age 7 has beneficial health effects in the long-term. I also find some evidence that income received between ages 7 and 18 can have long-term adverse health effects which appears to be primarily driven by income in the teenage years.

These finding shed light on the discussion regarding the timing of allocating resources through transfers. While transfers received in the early childhood years hold the potential to have an effect over a longer period of time, because these effects are relatively smaller as compared to the effects of income received after age 18, transfers made in adulthood could potentially be more efficient. Furthermore, if transfers will be made over the entire childhood period up to age 18, the long-term effectiveness might be dampened because income in the teenage years shows some evidence of having adverse effects. However, parents might be able to smooth income over time. In such case, the timing of allocating transfers may not make much difference if parents behave differently in anticipation of the timing of income transfers from safety net programs.

Even though we observe differential long-term effects of income received at different stages in the life cycle, it will be difficult to design policies that isolate transfers to any single period in the life cycle, for example, to either childhood or adulthood. This is because most transfers are made to the family instead of to individuals. Given that the family is typically comprised of adults and children, providing benefits to the children indirectly affects of members of the family as well given that resources are shared among family members. Even in the case where non-transferable benefits are provided, for example, providing free school lunch or subsidized medical services to children, because the family's cost of caring for the child will reduce, this might free up some resources to be allocated in new ways.

Based on the findings of this study, whether transfers should be made in adulthood or childhood would crucially depend on three factors: the ages at which these transfers would be made; the length of time for which these transfers are being proposed; and whether more resources provided in early life affects the available resources later in life. Within the context of these results, if a policy maker only cared about health in early adulthood, the main take-way would be that transfers should be made in adulthood. This also suggest that safety net programs that provide the largest benefits to families with children such as the Earned Income Tax Credit (EITC), could have unintended adverse consequences on health among adults if financing these EITC transfers hinges on reducing other program benefits for families without children.

Another important take away is that it is informative to account for income received up to the point at which health outcomes are measured in adulthood. Models which include only income from early life have a different interpretation and could miss important dynamics regarding how these long-term effects evolve over the life cycle. Furthermore, the implications from a model that accounts for income over the life course can be argued to be more policy relevant.

One of the main limitations of this study is that health outcomes are observed only in early adulthood. Given that most health problems manifest later in life, it would be useful to conduct similar analyses for older individuals. Coupled with a relatively small sample size, the study also has limited power to detect statistical significance among the health outcomes that rarely occur in early adulthood. It might also be informative to observe information at earlier ages on certain health outcomes such as general health status and obesity as well as risky health behaviors. This could shed light on the health production process and the extent to which individuals change their behavior based on the stock of health that is transferred from the previous period.

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Appendix A: Additional Results

Table A 1: Sample Selection

Year of birth	**Total Number of Births**	**Individuals Lost Due to Sampling Restrictions**	**Final Sample Count**
1967	827	668	159
1968	754	594	160
1969	852	655	197
1970	859	665	194
1971	835	638	197
1972	859	654	205
1973	854	645	209
1974	867	646	221
1975	859	638	221
1976	831	618	213
1977	906	662	244
1978	893	647	246
1979	984	713	271
1980	999	735	264
1981	970	689	281
1982	969	709	260
1983	940	666	274
1984	945	644	301
1985	872	581	291
1986	840	565	275
1987	861	578	283
1988	820	593	227
1989	803	610	193
Total	20,199	14,813	5,386

Notes: Table A1 shows the number of births in an original PSID family from 1967 to 1989 along with the total number of individuals lost from each birth cohort because complete information was not observed for their family from conception to at least age 28.

Figure A 1: Variation in Simulated EITC Benefits Over Time by Select States

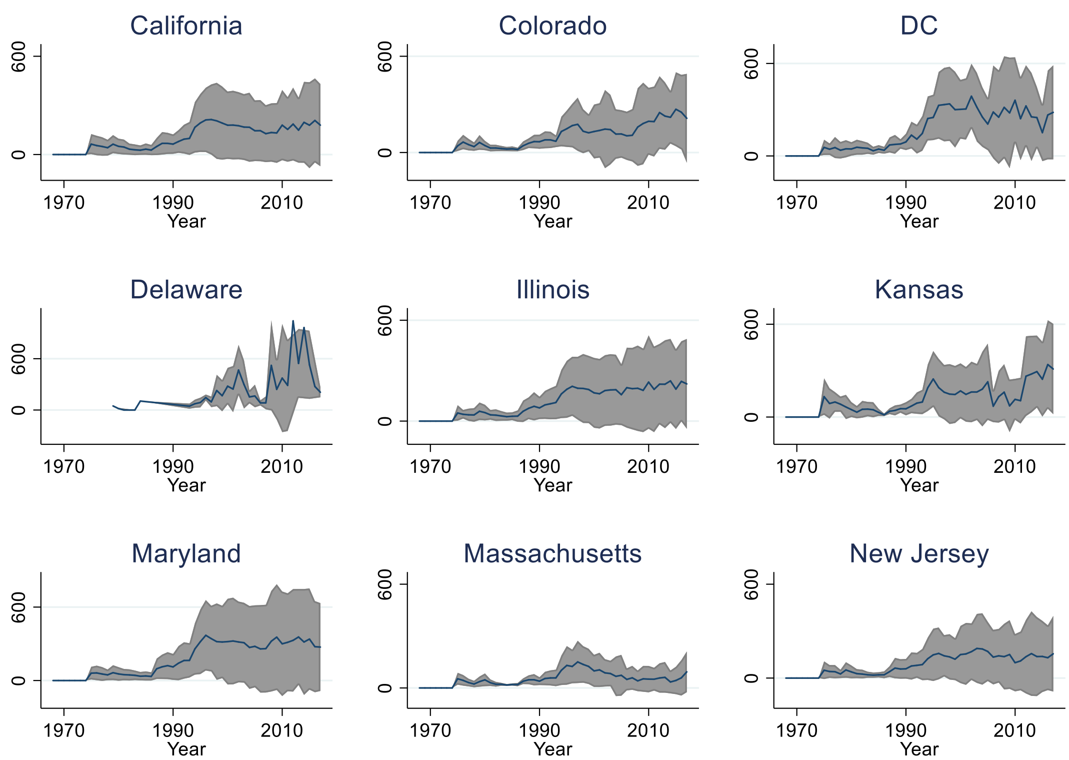

Figure A 2: Variation in Alternate Measure of Simulated EITC Benefits Over Time by Select States

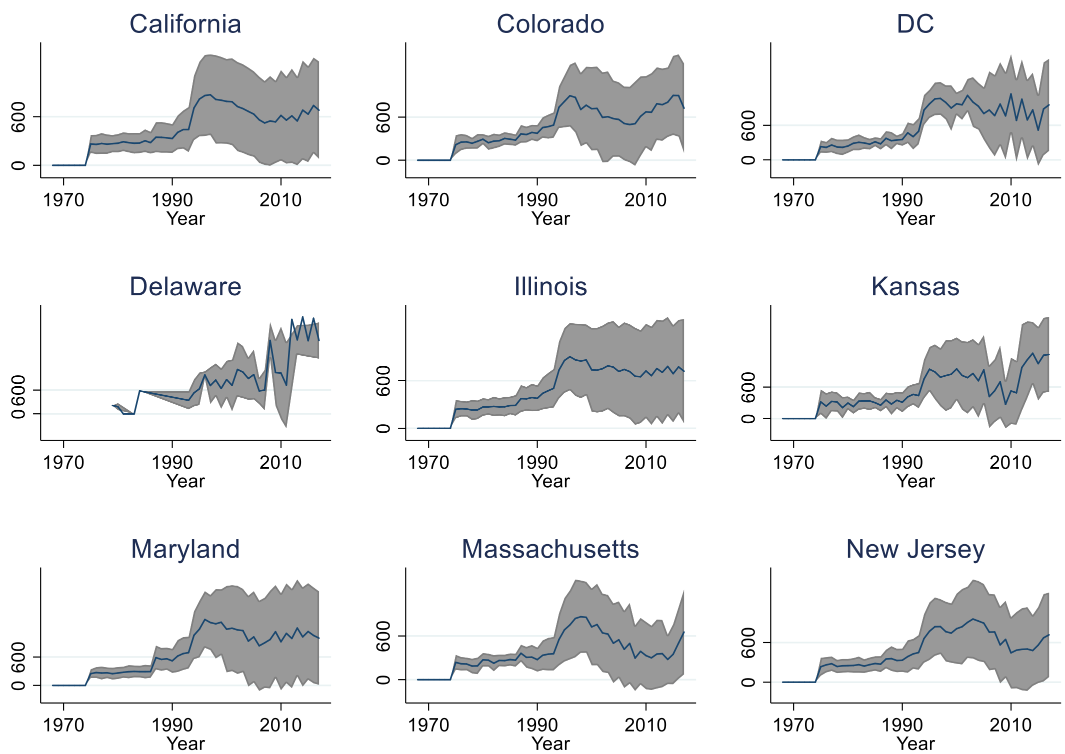

Table A 2: Power Calculation for Health Outcomes

(1) Health Outcomes	(2) Income in Childhood Squared Semi-Partial Correlation	(3) Sample Size	Power	(4) Income in Adulthood Squared Semi-Partial Correlation	(5) Sample Size	Power
Very good or excellent general health	0.0004	5,135	29.95	0.0103	5,135	100
Obesity	0.0001	5,094	11.01	0.0055	5,094	99.96
Body Mass Index	1.210e-06	5,094	5.07	0.0066	5,094	99.99
Diabetes	0.0002	5,336	17.83	0.0016	5,336	83.24
Hypertension	0.0003	5,336	24.42	0.0020	5,336	90.48
Heart Disease	7.290e-06	5,336	5.45	0.0001	5,336	11.30
Metabolic Syndrome	0.0003	5,093	23.52	0.0008	5,093	52.35
Physical limitations	0.0001	5,134	11.06	0.0062	5,134	99.99
Heart Attack	0.0001	5,336	11.30	0.0005	5,336	37.22
Arthritis	0.0002	5,335	17.83	0.0012	5,335	71.61
Stroke	0.0001	5,336	11.30	0.0006	5,336	43.24
Cancer	0.00002025	5,336	6.25	0.0002	5,336	17.83

Notes: The number or additional controls is 121 including the set of fixed effects included in the models. The sample size varies by outcome due to missing data and the number of parameters jointly tested is equal to 1 (either income received in childhood or income received in adulthood).

Table A 3: Power Calculation for Health Outcomes

(1)	(2)	(3)		(4)	(5)	
	Income in Childhood			Income in Adulthood		
Health Behaviors	Squared Semi-Partial Correlation	Sample Size	Power	Squared Semi-Partial Correlation	Sample Size	Power
Any exercise at least 10 mins. per week	0.000025	5,268	6.52	0.0048	5,268	99.9
Whether currently smokes	0.0001	5,133	11.05	0.0176	5,133	100
Number of cigarettes currently smoked per day (including non-smokers)	0.0006	5,124	41.48	0.0139	5,124	100
Quit smoking	0.0009	1,195	17.93	0.0090	1,195	90.85
Whether currently drinks alcohol	0.0018	5,131	86.02	0.0090	5,131	100
Number of drinks per day (including non-drinkers)	0.0001	5,095	11.01	0.0004	5,095	29.76
Number of days consume 4 to 5 drinks per year (including non-drinkers)	0.0002	4,323	15.34	0.0001	4,323	10.08

Notes: The number or additional controls is 121 including the set of fixed effects included in the models. The sample size varies by outcome due to missing data and the number of parameters jointly test is equal to 1 (either income received in childhood or income received in adulthood).

Table A 4: The Effect of Log(Income) on Health- Test of Difference Between Coefficients on Income Received in Childhood and Income Received in Adulthood from IV Regression

(1) Health Outcomes	(2) Income in Childhood IV	(3) Income in Adulthood IV	(4) Difference
Very good or excellent general health	0.0619 (0.0464)	0.121^{***} (0.0266)	0.0591 (0.329)
Obesity	0.0426 (0.0384)	-0.0989^{***} (0.0273)	-0.1415 (0.00225)
Body Mass Index	0.484 (0.682)	-1.523^{***} (0.452)	-2.007 (0.0241)
Diabetes	0.00354 (0.0186)	-0.0166 (0.00948)	-0.0201 (0.374)
Hypertension	0.0572 (0.0325)	-0.0163 (0.0205)	-0.0735 (0.0727)
Heart Disease	0.00265 (0.0124)	0.00110 (0.00786)	-0.0016 (0.905)
Metabolic Syndrome	0.00831 (0.0165)	-0.0107 (0.00736)	-0.0190 (0.291)
Physical limitations	0.0667 (0.0372)	-0.0103 (0.0176)	-0.077 (0.0662)
Heart Attack	-0.0123 (0.00850)	-0.00394 (0.00595)	0.0084 (0.455)
Arthritis	0.0210 (0.0210)	-0.0171 (0.0130)	-0.0381 (0.153)
Stroke	-0.0121 (0.0129)	-0.00119 (0.00740)	0.011 (0.366)
Cancer	-0.00918 (0.0161)	0.000921 (0.00805)	0.0101 (0.636)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table A 5: The Effect of Log(Income) on Health Behaviors-Test of Difference Between Coefficients on Income Received in Childhood and Income Received in Adulthood from IV Regression

(1) Health Behaviors	(2) Income in Childhood IV	(3) Income in Adulthood IV	(4) Difference
Exercise			
Any exercise at least 10 mins. per week	0.0311 (0.0474)	0.0721** (0.0242)	0.041 (0.505)
Smoking			
Whether currently smokes	-0.00329 (0.0516)	-0.101*** (0.0279)	-0.0977 (0.0673)
Number of cigarettes currently smoked per day (including non-smokers)	0.149 (0.781)	-1.705*** (0.394)	-1.854 (0.0374)
Quit smoking	0.0609 (0.0911)	0.130** (0.0479)	0.0691 (0.484)
Drinking			
Whether currently drinks alcohol	0.0760 (0.0628)	0.124*** (0.0287)	0.048 (0.482)
Number of drinks per day (including non-drinkers)	0.151 (0.209)	0.0759 (0.106)	-0.0751 (0.760)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	4.151* (1.982)	1.652 (1.775)	-2.489 (0.370)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table A 6- Including Interaction Term for Income in Childhood and Income in Adulthood in IV Regressions (Health Outcomes)

(1)	(2) Childhood	(3) Adulthood	(4) Interaction	(5) Joint Significance (P-value)
Very good or excellent general health	-0.448**	-0.351*	0.0541**	1.13e-09
	(0.202)	(0.213)	(0.0215)	
Obesity	0.0346	-0.0880	-0.00101	0.0167
	(0.195)	(0.193)	(0.0205)	
Body Mass Index	1.022	-0.564	-0.125	0.0125
	(4.509)	(4.079)	(0.449)	
Diabetes	0.316***	0.285***	-0.0316***	0.00165
	(0.0884)	(0.0891)	(0.00924)	
Hypertension	0.135	0.0691	-0.00905	0.443
	(0.179)	(0.169)	(0.0179)	
Heart Disease	0.0297	0.0359	-0.00320	0.844
	(0.0770)	(0.0696)	(0.00749)	
Metabolic Syndrome	0.0704*	0.0421	-0.00608	0.111
	(0.0378)	(0.0344)	(0.00377)	
Physical limitations	0.388***	0.333***	-0.0375***	0.0556
	(0.142)	(0.125)	(0.0137)	
Heart Attack	0.0130	0.0134	-0.00190	0.597
	(0.0386)	(0.0379)	(0.00375)	
Arthritis	0.238**	0.189**	-0.0233**	0.00328
	(0.0936)	(0.0957)	(0.00999)	
Stroke	0.0281	0.0298	-0.00309	0.973
	(0.0689)	(0.0710)	(0.00723)	
Cancer	-0.0437	-0.0367	0.00389	0.940
	(0.0825)	(0.0733)	(0.00789)	

Notes: Each parameter in column 3 and column 5 is from a separate IV regression which includes only income from a single phase of the life cycle along with the control variables.
Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table A 7: Including Interaction Term for Income in Childhood and Income in Adulthood in IV Regressions (Health Behaviors)

(1)	(2) Childhood	(3) Adulthood	(4) Interaction	(5) Joint Significance (P-value)
Exercise				
Any exercise at least 10 mins. per week	1.757	1.114	-0.159	0.409
	(1.732)	(1.547)	(0.164)	
Smoking				
Whether currently smokes	0.495*	0.335	-0.0514*	9.43e-08
	(0.287)	(0.282)	(0.0293)	
Number of cigarettes currently smoked per day (including non-smokers)	8.693**	6.184*	-0.905**	6.73e-08
	(3.722)	(3.468)	(0.374)	
Quit smoking	0.362	0.523	-0.0327	0.000075
	(0.406)	(0.419)	(0.0442)	
Drinking				
Whether currently drinks alcohol	-0.333	-0.359	0.0449	0.0134
	(0.325)	(0.312)	(0.0329)	
Number of drinks per day (including non-drinkers)	0.448	0.00518	-0.0234	0.238
	(0.848)	(0.762)	(0.0822)	
Number of days consume 4 to 5 drinks per year (including non-drinkers)	-37.60*	-41.48*	4.177*	0.215
	(21.64)	(21.84)	(2.226)	

Notes: Each parameter in column 3 and column 5 is from a separate IV regression which includes only income from a single phase of the life cycle along with the control variables.
Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Height as a Placebo Test

Table A 8: Placebo Test – The Effect of Income on Height

	IV	Test of Difference
Per capita family Income: Age 0 to 18	0.0115	0.0007
	(0.00911)	(0.939)
Per capita family Income: Age 22 to 18	0.0108	
	(0.0413)	
Mean Height in Meters	1.7213	
Mean per capita family Income: Age 0 to 18	18,827.5	
Mean per capita family Income: Age 19 to 32	30,886.96	
Observations	5,130	

Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Average per capita family income in each phase is measured in thousands of 2017 dollars.

Income Broken out into 3 Phases

Table A 9: First Stage Regression Results- Dependent Variables are Log Average Per Capita Family Income in Each Phase of the Life cycle

	(1) Age 0-6	(2) Age 7-18	(3) Age 19-32
Log Simulated Income	0.921***	0.666***	0.920***
	(0.0227)	(0.0345)	(0.0263)
Log Simulated EITC Benefits	0.000457	0.0417***	0.0153
	(0.00637)	(0.0116)	(0.00865)
Observations	5,386	5,386	5,386
F-stat	80.58	69.95	80.42

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.

Table A 10: The Effect of Log(Income) on Health- IV Regression Results

	Income Between		
Health Outcomes	Age 0-6	Age 7-18	Age 19-32
Very good or excellent	0.0603*	-0.00928	0.125***
general health	(0.0302)	(0.0403)	(0.0268)
Obesity	0.0304	0.0203	-0.0951***
	(0.0210)	(0.0391)	(0.0259)
Body Mass Index	0.263	0.460	-1.516***
	(0.316)	(0.649)	(0.433)
Diabetes	-0.0162	0.0169	-0.0188
	(0.00939)	(0.0194)	(0.00984)
Hypertension	0.0203	0.0491	-0.0157
	(0.0165)	(0.0326)	(0.0205)
Heart Disease	-0.0107	0.0121	-0.00130
	(0.00590)	(0.0137)	(0.00774)
Metabolic Syndrome	-0.00494	0.0117	-0.0120
	(0.00545)	(0.0152)	(0.00732)
Physical limitations	-0.0142	0.0852*	-0.0168
	(0.0157)	(0.0336)	(0.0166)
Heart Attack	-0.00731	-0.00389	-0.00428
	(0.00400)	(0.00790)	(0.00593)
Arthritis	-0.0198	0.0505*	-0.0219
	(0.0123)	(0.0215)	(0.0131)
Stroke	-0.00781	0.000155	-0.00262
	(0.00614)	(0.0122)	(0.00723)
Cancer	-0.0164*	0.0110	-0.000983
	(0.00766)	(0.0136)	(0.00862)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table A 11: The Effect of Log(Income) on Health Behaviors- IV Regression Results

	Income Between		
Health Behaviors	Age 0-6	Age 7-18	Age 19-32
Any exercise at least 10 mins. per week	-0.00808	0.0332	0.0657**
	(0.0263)	(0.0414)	(0.0244)
Whether currently smokes	-0.0987**	0.113*	-0.115***
	(0.0310)	(0.0514)	(0.0243)
Number of cigarettes currently smoked per day (including non-smokers)	-1.844***	2.134**	-2.002***
	(0.495)	(0.736)	(0.352)
Quit smoking	0.0993	-0.0609	0.139**
	(0.0570)	(0.0889)	(0.0449)
Whether currently drinks alcohol	0.0141	0.0823	0.120***
	(0.0229)	(0.0565)	(0.0299)
Number of drinks per day (including non-drinkers)	0.0476	0.179	0.0787
	(0.137)	(0.181)	(0.106)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	0.237	5.991**	0.965
	(1.823)	(2.300)	(1.830)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Including Controls for State Specific Linear Time Trends

I construct controls for state specific linear time trends from conception to age 32. To do so, I first construct a variable for linear time trend and 51 dummy variables for state of residence in each year. Next, I generate 51 variables by interacting each state dummy variable with the linear time trend. I then take the average of these variables at the individual level from conception to age 32. For individuals whom health information is only observed prior to age 32, the average per capita income is measured up to the point at which the health outcome is observed. Using this approach imposes the restriction that the timing of the state of residence does not matter and that each additional year of living in a given state has the same constant effect.

The first stage results are presented in Table A10 and the second stage results are presented in Table A11 and Table A12. By including controls for state specific time trends, the F-stats declined, but are still well above the conventional rule of thumb (10). The second stage results are almost identical to the case of not including controls for state specific time trends.

Table A 12: First Stage Regression Results- Dependent Variables are Log Average Per Capita Family Income in Each Phase of the Life cycle- including state specific time trends

	(1) Age 0-18	(2) Age 19-32
Log Simulated Income	0.874***	0.920***
	(0.0397)	(0.0263)
Log Simulated EITC Benefits	0.0817***	0.0188*
	(0.0137)	(0.00874)
Observations	5,386	5,386
F-stat	60.39	58.89

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.

Table A 13: The Effect of Log(Income) on Health- IV Results including state specific time trends

(1)	(2) Income In Childhood	(3) Income In adulthood
Very good or excellent general health	0.0667	0.118***
	(0.0485)	(0.0261)
Obesity	0.0354	-0.107***
	(0.0375)	(0.0271)
Body Mass Index	0.462	-1.589***
	(0.684)	(0.432)
Diabetes	0.00663	-0.0139
	(0.0195)	(0.00993)
Hypertension	0.0470	-0.0142
	(0.0326)	(0.0199)
Heart Disease	0.00140	0.000584
	(0.0128)	(0.00789)
Metabolic Syndrome	0.00997	-0.0111
	(0.0172)	(0.00744)
Physical limitations	0.0600	-0.0118
	(0.0394)	(0.0183)
Heart Attack	-0.0126	-0.00406
	(0.00828)	(0.00597)
Arthritis	0.0157	-0.0173
	(0.0225)	(0.0131)
Stroke	-0.0100	-0.00145
	(0.0112)	(0.00691)
Cancer	-0.00867	0.00337
	(0.0162)	(0.00832)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table A 14: The Effect of Log(Income) on Health Behaviors- IV Results including state specific time trends

(1)	(2) Income In Childhood	(3) Income In Adulthood
Exercise		
Any exercise at least 10 mins. per week	0.0218 (0.0502)	0.0723^{**} (0.0254)
Smoking		
Whether currently smokes	-0.0242 (0.0494)	-0.0987^{***} (0.0277)
Number of cigarettes currently smoked per day (including non-smokers)	-0.0780 (0.710)	-1.676^{***} (0.392)
Quit smoking	0.111 (0.0885)	0.139^{**} (0.0485)
Drinking		
Whether currently drinks alcohol	0.0514 (0.0655)	0.119^{***} (0.0292)
Number of drinks per day (including non-drinkers)	0.0372 (0.212)	0.0599 (0.106)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	3.010 (1.876)	1.069 (1.786)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table A 15: The Effect of Log(Income Broken Into 5 Phases) on Health- IV Regression Results

Health Outcomes	Age 0-6	Age 7-12	Age 13-18	Age 19-23	Age 24-32	Joint Significance (P-value)
Very good or excellent general health	0.0554 (0.0394)	0.0372 (0.0591)	-0.0680 (0.0489)	0.0547* (0.0290)	0.105*** (0.0305)	3.51e-10
Obesity	0.0668* (0.0375)	-0.101* (0.0576)	0.107** (0.0521)	-0.0640* (0.0339)	-0.0404 (0.0262)	0.0202
Body Mass Index	0.705 (0.542)	-1.018 (0.785)	1.040 (0.804)	-0.358 (0.614)	-1.112** (0.481)	0.00636
Diabetes	-0.00935 (0.0142)	-0.0195 (0.0218)	0.0365 (0.0237)	-0.0141 (0.0132)	-0.00262 (0.0104)	0.167
Hypertension	0.0374 (0.0293)	-0.0212 (0.0487)	0.0558 (0.0415)	0.0265 (0.0200)	-0.0267 (0.0225)	0.0108
Heart Disease	-0.0165 (0.0108)	-0.000658 (0.0225)	0.0152 (0.0181)	-0.00226 (0.0108)	0.00318 (0.00628)	0.274
Metabolic Syndrome	-0.00535 (0.00717)	0.00437 (0.0195)	0.00494 (0.0184)	-0.00304 (0.00884)	-0.00551 (0.00605)	0.770
Physical limitations	-0.0389* (0.0236)	0.0192 (0.0382)	0.0953** (0.0413)	-0.0798** (0.0330)	0.0305 (0.0235)	0.0508
Heart Attack	-0.00645 (0.00545)	-0.00199 (0.0135)	-0.0000215 (0.00974)	-0.00723 (0.00463)	0.00218 (0.00439)	0.250
Arthritis	-0.0120 (0.0181)	-0.00526 (0.0340)	0.0518 (0.0348)	-0.0263 (0.0221)	-0.0167 (0.0138)	0.192
Stroke	-0.00347 (0.00894)	-0.00772 (0.0154)	0.0117 (0.0135)	-0.00502 (0.00562)	-0.000698 (0.00844)	0.869
Cancer	-0.0117 (0.00989)	0.00278 (0.0180)	-0.0000304 (0.0164)	0.00527 (0.0110)	-0.00552 (0.00938)	0.567

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table A 16: The Effect of Log(Income Broken Into 5 Phases) on Health Behaviors- IV Regression Results

Health Behaviors	Age 0-6	Age 7-12	Age 13-18	Age 19-23	Age 24-32	Joint Significance (P-value)
Any exercise at least 10 mins. per week	-0.260 (0.305)	0.773* (0.420)	-0.487 (0.531)	0.531* (0.321)	-0.583** (0.278)	0.0956
Whether currently smokes	-0.107*** (0.0407)	0.0777 (0.0641)	0.0786 (0.0607)	-0.0966*** (0.0352)	-0.0694*** (0.0263)	4.70e-10
Number of cigarettes currently smoked per day (including non-smokers)	-1.700*** (0.581)	1.044 (0.759)	1.289 (0.916)	-1.512*** (0.469)	-1.191*** (0.363)	7.34e-16
Quit smoking	0.0376 (0.0738)	0.0209 (0.113)	-0.117 (0.0819)	0.135** (0.0690)	0.0733 (0.0528)	0.00406
Whether currently drinks alcohol	0.00879 (0.0344)	0.0906 (0.0658)	0.0628 (0.0531)	-0.0373 (0.0355)	0.0950*** (0.0312)	0.000969
Number of drinks per day (including non-drinkers)	0.202 (0.185)	-0.214 (0.251)	0.613*** (0.218)	-0.266** (0.125)	0.0271 (0.0953)	0.0375
Number of days consume 4 to 5 drinks per year (including non-drinkers)	1.224 (2.310)	1.244 (3.445)	7.058* (4.086)	-2.363 (2.477)	-1.194 (2.308)	0.0984

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Appendix B: Endogenous Migration and Attrition

Testing for Endogenous Migration

In this section, using a simple approach, I test whether an individual's migration across state border is influenced by their health status or health behaviors. If this is the case, because the family's own state of residence is used to construct simulated EITC benefits this could introduce some bias. To test this hypothesis, I estimate a linear probability model. The dependent variable is a dummy variable equal to 1 if the individual lives in a different state than their birth state at the point at which health outcomes are observed. A separate regression is run with each health outcome and health behaviors as the right-hand side variable of interest. A statistically significant coefficient for a given health related outcome provides evidence of endogenous migration. Included as control variables in these regressions are individual demographic characteristics, state specific linear time trends and family income. The results are presented in Table B1 and Table B2 below, which show very little supporting evidence that individuals' migration decisions are influenced by their health outcomes and health behavior.

Table B 1: Test of Endogenous Migration- Health Outcomes

	Residing in a state other than birth state
Very good or excellent general health	-0.0157
	(0.0119)
Obesity	0.0399**
	(0.0134)
Body Mass Index	0.00278*
	(0.00108)
Diabetes	-0.00819
	(0.0301)
Hypertension	0.0167
	(0.0208)
Heart Disease	-0.0137
	(0.0483)
Metabolic Syndrome	0.0208
	(0.0499)
Physical limitations	-0.0280
	(0.0183)
Heart Attack	-0.0753
	(0.0772)
Arthritis	-0.0301
	(0.0234)
Stroke	0.0415
	(0.0465)
Cancer	-0.0309
	(0.0409)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table B 2: Test of Endogenous Migration- Health Behaviors

	Residing in a state other than birth state
Any exercise at least 10 mins. per week	0.000255 (0.0188)
Whether currently smokes	0.0276 (0.0165)
Number of cigarettes currently smoked per day (including non-smokers)	0.00197 (0.00130)
Quit smoking	-0.0265 (0.0214)
Whether currently drinks alcohol	-0.0206 (0.0175)
Number of drinks per day (including non-drinkers)	0.00492 (0.00466)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	0.000468* (0.000227)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Accounting for Endogenous Migration

I use a more exacting approach which accounts for possible selection bias similar to Dahl (2002). This entails the use of predicted migration probabilities to estimate control functions which are then included as controls in the outcome regression. Consistent with Dahl (2002), I assume that the index sufficiency condition holds. More specifically, I assume that controlling for the first best predicted probability (the predicted probability for an individual who moved to the current state of residence) and the retention probability (the predicted probability that an individual does not move from their birth state) exhausts all the information contained in the migration probabilities. With this assumption, I circumvent the infeasible challenge of having to estimate 51 correction functions for each of the 50 states and D.C., each of which in the absence of this assumption would be functions of 51 predicted probabilities. Instead, the 51 correction functions depend only on the predicted probability of moving to the observed state and the predicted probability of remaining in the origin state. That is, for two individuals who choose to move to the same state *k*, their selection bias can be described by the same distribution despite their origin state. This aids in identifying the coefficients in the health outcomes regression by ensuring that the 51 correction functions that enter the regression each depend on at most two predicted probabilities.

The migration probabilities are generated using a conditional logit model where the dependent variable is a dummy equal to 1 if the individual lives in a different state than their birth state. Included in this regression as controls are individual and family demographic characteristics, family composition, family income, state temperature, state unemployment rate and linear time trend. This probability is then included as a variable in the regression model to account for the probability of migrating from one state to another or remaining in the birth state.

The results are presented in Table B3 and Table B4 below. I show the coefficients that result when I account for migration along with the coefficients when I do not account for migration, which are simply the estimates presented in the results section. Overall, there are no meaningful differences between the estimates from the two models.

Table B 3: The Effect of Log(Income) on Health- Migration Adjusted and Unadjusted Coefficients

	Income in Childhood		Income in Adulthood	
Health Outcomes	Original	Adjusted	Original	Adjusted
Very good or excellent general	0.0619	0.0620	0.121***	0.121***
health	(0.0464)	(0.0464)	(0.0266)	(0.0266)
Obesity	0.0426	0.0425	-0.0989***	-0.0991***
	(0.0384)	(0.0385)	(0.0273)	(0.0274)
Body Mass Index	0.484	0.477	-1.523***	-1.522***
	(0.682)	(0.678)	(0.452)	(0.452)
Diabetes	0.00354	0.00367	-0.0166	-0.0167
	(0.0186)	(0.0187)	(0.00948)	(0.00950)
Hypertension	0.0572	0.0572	-0.0163	-0.0163
	(0.0325)	(0.0324)	(0.0205)	(0.0205)
Heart Disease	0.00265	0.00267	0.00110	0.00108
	(0.0124)	(0.0124)	(0.00786)	(0.00788)
Metabolic Syndrome	0.00831	0.00830	-0.0107	-0.0108
	(0.0165)	(0.0166)	(0.00736)	(0.00737)
Physical limitations	0.0667	0.0667	-0.0103	-0.0103
	(0.0372)	(0.0372)	(0.0176)	(0.0176)
Heart Attack	-0.0123	-0.0123	-0.00394	-0.00392
	(0.00850)	(0.00852)	(0.00595)	(0.00595)
Arthritis	0.0210	0.0208	-0.0171	-0.0170
	(0.0210)	(0.0209)	(0.0130)	(0.0130)
Stroke	-0.0121	-0.0120	-0.00119	-0.00124
	(0.0129)	(0.0129)	(0.00740)	(0.00742)
Cancer	-0.00918	-0.00911	0.000921	0.000946
	(0.0161)	(0.0161)	(0.00805)	(0.00806)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table B 4: The Effect of Log(Income) on Health Behaviors- Migration Adjusted and Unadjusted Coefficients

	Income in Childhood		Income in Adulthood	
Health Behaviors	Original	Adjusted	Original	Adjusted
Any exercise at least 10 mins. per week	0.0311	0.0294	0.0721^{**}	0.0719^{**}
	(0.0474)	(0.0477)	(0.0242)	(0.0242)
Whether currently smokes	-0.00329	-0.00373	-0.101^{***}	-0.101^{***}
	(0.0516)	(0.0516)	(0.0279)	(0.0279)
Number of cigarettes currently smoked per day (including non-smokers)	0.149	0.149	-1.705^{***}	-1.702^{***}
	(0.781)	(0.779)	(0.394)	(0.394)
Quit smoking	0.0609	0.0648	0.130^{**}	0.130^{**}
	(0.0911)	(0.0918)	(0.0479)	(0.0476)
Whether currently drinks alcohol	0.0760	0.0752	0.124^{***}	0.125^{***}
	(0.0628)	(0.0626)	(0.0287)	(0.0288)
Number of drinks per day (including non-drinkers)	0.151	0.148	0.0759	0.0762
	(0.209)	(0.209)	(0.106)	(0.106)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	4.151^{*}	4.084^{*}	1.652	1.645
	(1.982)	(1.987)	(1.775)	(1.777)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Testing for Attrition Bias

I examine whether individuals who later become non-responses are different in terms of health outcomes and health behaviors. If these individuals are different in terms of health outcomes and health behaviors, this could result in sample selection bias. I estimate a linear probability model. The dependent variable is a dummy equal to 1 if the individual dropped from the sample after the age at which their health outcome is observed. A separate regression is run with each health outcome and health behaviors as the right-hand side variable of interest. As control variables, I include individual demographic characteristics, state specific linear time trends and family income. A statistically significant point estimate for a given health related outcome is evidence of selective attrition. Table B5 and Table B6 contain the results from these regressions. The results show only very little evidence of selective attrition.

Table B 5: Testing for Selective Attrition by Health Outcomes

	Whether later attrite
Very good or excellent general health	-0.00815
	(0.0103)
Obesity	-0.0166
	(0.0108)
Body Mass Index	-0.00197*
	(0.000746)
Diabetes	-0.0135
	(0.0207)
Hypertension	-0.0227
	(0.0129)
Heart Disease	0.0546
	(0.0434)
Metabolic Syndrome	-0.0225
	(0.0465)
Physical limitations	0.0133
	(0.0241)
Heart Attack	0.0130
	(0.0719)
Arthritis	-0.0281
	(0.0169)
Stroke	0.104*
	(0.0477)
Cancer	0.0832*
	(0.0390)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table B 6: Testing for Selective Attrition by Health Behaviors

	Whether later attrite
Any exercise at least 10 mins. per week	-0.0679***
	(0.0187)
Whether currently smokes	0.0370**
	(0.0122)
Number of cigarettes currently smoked per day (including non-smokers)	0.00236**
	(0.000878)
Quit smoking	-0.0282*
	(0.0138)
Whether currently drinks alcohol	0.00733
	(0.0132)
Number of drinks per day (including non-drinkers)	0.000475
	(0.00201)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	-0.0000485
	(0.000159)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Adjusting for Attrition

I use a similar approach to adjust for potential attrition bias as discussed in the case of migration. The results are presented in Table B7 and Table B8. The coefficients from both models look very similar.

Table B 7: The Effect of Log(Income) on Health- Attrition Adjusted and Unadjusted Coefficients

	Income in Childhood		Income in Adulthood	
Health Outcomes	Original	Adjusted	Original	Adjusted
Very good or excellent general health	0.0619	0.0607	0.121***	0.121***
	(0.0464)	(0.0461)	(0.0266)	(0.0267)
Obesity	0.0426	0.0469	-0.0989***	-0.0972***
	(0.0384)	(0.0390)	(0.0273)	(0.0272)
Body Mass Index	0.484	0.529	-1.523***	-1.505***
	(0.682)	(0.701)	(0.452)	(0.451)
Diabetes	0.00354	0.00320	-0.0166	-0.0167
	(0.0186)	(0.0186)	(0.00948)	(0.00955)
Hypertension	0.0572	0.0592	-0.0163	-0.0156
	(0.0325)	(0.0327)	(0.0205)	(0.0207)
Heart Disease	0.00265	0.00220	0.00110	0.000924
	(0.0124)	(0.0125)	(0.00786)	(0.00798)
Metabolic Syndrome	0.00831	0.00857	-0.0107	-0.0106
	(0.0165)	(0.0168)	(0.00736)	(0.00738)
Physical limitations	0.0667	0.0652	-0.0103	-0.0110
	(0.0372)	(0.0374)	(0.0176)	(0.0176)
Heart Attack	-0.0123	-0.0125	-0.00394	-0.00403
	(0.00850)	(0.00863)	(0.00595)	(0.00593)
Arthritis	0.0210	0.0214	-0.0171	-0.0170
	(0.0210)	(0.0213)	(0.0130)	(0.0129)
Stroke	-0.0121	-0.0121	-0.00119	-0.00122
	(0.0129)	(0.0130)	(0.00740)	(0.00745)
Cancer	-0.00918	-0.00967	0.000921	0.000735
	(0.0161)	(0.0160)	(0.00805)	(0.00808)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table B 8: The Effect of Log(Income) on Health Behaviors- Attrition Adjusted and Unadjusted Coefficients

	Income in Childhood		Income in Adulthood	
Health Behaviors	Original	Adjusted	Original	Adjusted
Any exercise at least 10 mins. per week	0.0311	0.0328	0.0721**	0.0730**
	(0.0474)	(0.0479)	(0.0242)	(0.0242)
Whether currently smokes	-0.00329	-0.00155	-0.101***	-0.100***
	(0.0516)	(0.0523)	(0.0279)	(0.0282)
Number of cigarettes currently smoked per day (including non-smokers)	0.149	0.187	-1.705***	-1.689***
	(0.781)	(0.782)	(0.394)	(0.397)
Quit smoking	0.0609	0.0580	0.130**	0.129**
	(0.0911)	(0.0929)	(0.0479)	(0.0482)
Whether currently drinks alcohol	0.0760	0.0792	0.124***	0.126***
	(0.0628)	(0.0636)	(0.0287)	(0.0284)
Number of drinks per day (including non-drinkers)	0.151	0.170	0.0759	0.0837
	(0.209)	(0.211)	(0.106)	(0.105)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	4.151*	4.426*	1.652	1.799
	(1.982)	(1.977)	(1.775)	(1.769)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Appendix C: The Effect of Average Income from Conception to Age 32 (Lifetime)

Table C 1: First Stage Regression Results- Dependent Variable is Log Average Per Capita Family Income from Conception to Age 32

	(1) Lifetime Income
Log Simulated Income	0.956^{***}
	(0.0433)
Log Simulated EITC	0.0573^{***}
Benefits	(0.0138)
Observations	5,386
F-stat	91.25

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.

Table C 2: The Effect of Log(Lifetime Income) on Health

	Lifetime Income	
Health Outcomes	OLS	IV
Very good or excellent general health	0.130***	0.207***
	(0.0199)	(0.0497)
Obesity	-0.0666***	-0.156**
	(0.0173)	(0.0577)
Body Mass Index	-1.143***	-2.350**
	(0.240)	(0.721)
Diabetes	-0.0234***	-0.0191
	(0.00489)	(0.0172)
Hypertension	-0.0206*	0.0244
	(0.00981)	(0.0357)
Heart Disease	-0.00232	0.00557
	(0.00394)	(0.0158)
Metabolic Syndrome	-0.0110**	-0.0100
	(0.00410)	(0.0122)
Physical limitations	-0.0541***	0.0408
	(0.0116)	(0.0391)
Heart Attack	-0.00478	-0.00987
	(0.00282)	(0.00909)
Arthritis	-0.0255**	0.00223
	(0.00813)	(0.0246)
Stroke	-0.00701	-0.00903
	(0.00383)	(0.0192)
Cancer	-0.00303	-0.0104
	(0.00387)	(0.0112)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table C 3: The Effect of Log(Lifetime Income) on Health Behaviors

	Lifetime Income	
Health Behaviors	OLS	IV
Any exercise at least 10 mins. per week	0.0522^{***}	0.113^{*}
	(0.0121)	(0.0499)
Whether currently smokes	-0.135^{***}	-0.137^{*}
	(0.0177)	(0.0641)
Number of cigarettes currently smoked per day (including non-smokers)	-2.005^{***}	-2.257^{**}
	(0.318)	(0.793)
Quit smoking	0.146^{***}	0.214
	(0.0260)	(0.112)
Whether currently drinks alcohol	0.149^{***}	0.257^{***}
	(0.0227)	(0.0589)
Number of drinks per day (including non-drinkers)	0.117	0.224
	(0.0664)	(0.220)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	1.381	2.976
	(1.070)	(3.776)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Appendix D: Varying the Construction of the Instrumental Variables

Alternative Approach for Simulating EITC Benefits

The second simulated instrument for EITC benefits is defined as the amount of EITC for which the head and spouse would be eligible if each individual were given some fixed amount of income in every year, conditional on their individual attributes (number of qualifying children, marital status, state of residence, age). That is, I assign each head and spouse in the CPS data in each year the same real amount of income and use the NBER TAXSIM software to estimate the amount of EITC benefits for which each individual would be eligible in every year, given their individual attributes. Similar to the first approach for constructing simulated EITC benefits, the steps are the same, except that the assigned income is used instead of the income observed in the CPS data. Each year I assign each individual $3,276 in constant 1975 dollars. This level was assigned based on an individual working a total of 30 hours per week at the 1975 minimum wage rate ($2.10). In constant 2018 dollars, the 1975 minimum wage rate of $2.10 is $9.78 and the annual income in constant 2018 dollars at 30 hours per week is $15,257.

This approach explicitly fixes the number of hours worked. It exploits only limited variation arising from changes made to the federal and state EITC benefits structure. For example, this approach would pick up variation coming from extending EITC benefits to childless adults or increasing benefits amounts for families with qualifying children, but it would not capture any of the changes that would affect individuals who earn above $3,276 in real terms. In the case of the EITC, some changes it would ignore in later years include changes made to the minimum income for maximum benefit and changes made to the phase out rate. This is because $3,276 (real 1975 dollars) falls below the thresholds in most of the years subsequent to 1975. Relative to the first approach for constructing the simulated instruments, this approach is expected to yield larger standard errors because it is exploiting a limited amount of the variation in the changes made to the EITC program over time. This is because income is being held constant in real terms across individuals and so the only within state variation in any given year that is picked up is from the number of kids and the age of the head of household. Furthermore, there will only be across state variation for the EITC program starting in 1987, when states started implementing their own supplemental EITC program. While there is variation coming from changes made to the federal EITC program over time, these do not vary across states. If the estimated effects of income on health is meaningfully different based on the approach used to simulate EITC benefits, this might be a sign that income in the CPS data is endogenous.

The results from the first stage regression is presented in Table C1. The second stage results are presented in Table C2 and Table C3. The columns labelled Sim EITC2 contain the results using the second approach to simulating EITC benefits while the columns labelled Sim EITC1 contain the original results presented in the main paper for comparison. The results are almost identical.

Table D 1: First Stage Regression Results- Dependent Variables are Log Average Per Capita Family Income in Each Phase of the Life cycle

	(1) Age 0-18	(2) Age 19-32
Log Simulated Income	0.858***	0.927***
	(0.0400)	(0.0245)
Log Second Simulated EITC	0.0141	0.0387***
Benefits	(0.0114)	(0.00942)
Observations	5,386	5,386
F-stat	80.93	81.39

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.

Table D 2: The Effect of Log(Income) on Health Outcomes- IV

Health Outcomes	Income in Childhood		Income in Adulthood	
	Sim EITC1	Sim EITC 2	Sim EITC1	Sim EITC2
Very good or excellent general health	0.0619	0.0656	0.121^{***}	0.122^{***}
	(0.0464)	(0.0473)	(0.0266)	(0.0265)
Obesity	0.0426	0.0404	-0.0989^{***}	-0.100^{***}
	(0.0384)	(0.0382)	(0.0273)	(0.0275)
Body Mass Index	0.484	0.451	-1.523^{***}	-1.529^{***}
	(0.682)	(0.661)	(0.452)	(0.455)
Diabetes	0.00354	-0.000747	-0.0166	-0.0182
	(0.0186)	(0.0186)	(0.00948)	(0.00961)
Hypertension	0.0572	0.0527	-0.0163	-0.0174
	(0.0325)	(0.0339)	(0.0205)	(0.0206)
Heart Disease	0.00265	0.00138	0.00110	0.000872
	(0.0124)	(0.0124)	(0.00786)	(0.00791)
Metabolic Syndrome	0.00831	0.00831	-0.0107	-0.0107
	(0.0165)	(0.0165)	(0.00736)	(0.00736)
Physical limitations	0.0667	0.0630	-0.0103	-0.0114
	(0.0372)	(0.0376)	(0.0176)	(0.0179)
Heart Attack	-0.0123	-0.0131	-0.00394	-0.00406
	(0.00850)	(0.00840)	(0.00595)	(0.00600)
Arthritis	0.0210	0.0177	-0.0171	-0.0179
	(0.0210)	(0.0210)	(0.0130)	(0.0132)
Stroke	-0.0121	-0.0103	-0.00119	-0.00183
	(0.0129)	(0.0119)	(0.00740)	(0.00773)
Cancer	-0.00918	-0.00642	0.000921	0.00216
	(0.0161)	(0.0160)	(0.00805)	(0.00829)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table D 3: The Effect of Log(Income) on Health Behaviors- IV

	Income in Childhood		Income in Adulthood	
Health Behaviors	Sim1	Sim2	Sim1	Sim2
Any exercise at least 10 mins. per week	0.0311	0.0193	0.0721^{**}	0.0691^{**}
	(0.0474)	(0.0497)	(0.0242)	(0.0241)
Whether currently smokes	-0.00329	-0.0154	-0.101^{***}	-0.107^{***}
	(0.0516)	(0.0520)	(0.0279)	(0.0276)
Number of cigarettes currently smoked per day (including non-smokers)	0.149	0.126	-1.705^{***}	-1.745^{***}
	(0.781)	(0.771)	(0.394)	(0.386)
Quit smoking	0.0609	0.0655	0.130^{**}	0.134^{**}
	(0.0911)	(0.0937)	(0.0479)	(0.0480)
Whether currently drinks alcohol	0.0760	0.0614	0.124^{***}	0.118^{***}
	(0.0628)	(0.0629)	(0.0287)	(0.0292)
Number of drinks per day (including non-drinkers)	0.151	0.0753	0.0759	0.0384
	(0.209)	(0.218)	(0.106)	(0.102)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	4.151^{*}	3.793^{*}	1.652	1.351
	(1.982)	(1.858)	(1.775)	(1.823)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Using a Single Simulated Instrument (Sim Income Plus Sim EITC Benefits)

Table D 4: First Stage Regression Results- Dependent Variables are Log Average Per Capita Family Income in Each Phase of the Life cycle

	(1) Age 0-18	(2) Age 19-32
Log [Simulated Income Plus Simulated EITC]	0.877*** (0.0406)	0.929*** (0.0243)
Observations	5,386	5,386
F-stat	81.64	81.83

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.

Table D 5: The Effect of Log(Income) on Health- IV

Health Outcomes	Income in Childhood Original	Income in Childhood Sim1+Sim Inc	Income in Adulthood Original	Income in Adulthood Sim1+Sim Inc
Very good or excellent general health	0.0619	0.0638	0.121***	0.122***
	(0.0464)	(0.0457)	(0.0266)	(0.0267)
Obesity	0.0426	0.0455	-0.0989***	-0.0975***
	(0.0384)	(0.0388)	(0.0273)	(0.0275)
Body Mass Index	0.484	0.470	-1.523***	-1.522***
	(0.682)	(0.688)	(0.452)	(0.452)
Diabetes	0.00354	0.00197	-0.0166	-0.0174
	(0.0186)	(0.0188)	(0.00948)	(0.00943)
Hypertension	0.0572	0.0543	-0.0163	-0.0165
	(0.0325)	(0.0330)	(0.0205)	(0.0205)
Heart Disease	0.00265	0.00209	0.00110	0.00106
	(0.0124)	(0.0129)	(0.00786)	(0.00770)
Metabolic Syndrome	0.00831	0.00842	-0.0107	-0.0105
	(0.0165)	(0.0164)	(0.00736)	(0.00731)
Physical limitations	0.0667	0.0644	-0.0103	-0.0109
	(0.0372)	(0.0372)	(0.0176)	(0.0177)
Heart Attack	-0.0123	-0.0134	-0.00394	-0.00425
	(0.00850)	(0.00852)	(0.00595)	(0.00594)
Arthritis	0.0210	0.0194	-0.0171	-0.0177
	(0.0210)	(0.0218)	(0.0130)	(0.0129)
Stroke	-0.0121	-0.00558	-0.00119	0.000704
	(0.0129)	(0.0101)	(0.00740)	(0.00707)
Cancer	-0.00918	-0.0104	0.000921	0.00117
	(0.0161)	(0.0162)	(0.00805)	(0.00805)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table D 6: The Effect of Log(Income) on Health Behaviors- IV

Health Behaviors	Income in Childhood Original	Income in Childhood Sim1+Sim Inc	Income in Adulthood Original	Income in Adulthood Sim1+Sim Inc
Any exercise at least 10 mins. per week	0.0311	0.0258	0.0721^{**}	0.0713^{**}
	(0.0474)	(0.0498)	(0.0242)	(0.0243)
Whether currently smokes	-0.00329	0.00534	-0.101^{***}	-0.0986^{***}
	(0.0516)	(0.0504)	(0.0279)	(0.0279)
Number of cigarettes currently smoked per day (including non-smokers)	0.149	0.318	-1.705^{***}	-1.646^{***}
	(0.781)	(0.760)	(0.394)	(0.384)
Quit smoking	0.0609	0.0645	0.130^{**}	0.131^{**}
	(0.0911)	(0.0926)	(0.0479)	(0.0492)
Whether currently drinks alcohol	0.0760	0.0846	0.124^{***}	0.128^{***}
	(0.0628)	(0.0649)	(0.0287)	(0.0292)
Number of drinks per day (including non-drinkers)	0.151	0.205	0.0759	0.0935
	(0.209)	(0.209)	(0.106)	(0.107)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	4.151^{*}	4.838^{*}	1.652	1.783
	(1.982)	(2.070)	(1.775)	(1.772)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Appendix E: Varying the set of matching variables

In this section, I present regression results where I vary the set of matching variables used to form the simulated instruments. The full set of variables are state of residence, sex of head and spouse, race of head and spouse, age of head and spouse, marital status of head and the number of children in the household. Compared to the results in the main text, the first and second stage results are presented in tables E1A through E3B. Table E1A contains the first stage results for income received in childhood and Table E1B contains the first stage results for income received in adulthood. For the second stage results, all regressions simultaneously include income received in childhood and income received in adulthood. For ease of presentation, coefficients on income received in childhood and adulthood are reported in separate tables. Table E2A and Table E2B contain the regression coefficients for the effect of income received in childhood and income received in adulthood on health outcomes, respectively. Table E3A and Table E3B contain the regression coefficients for the effect of income received in childhood and income received in adulthood on health behaviors, respectively.

In each of these tables, column 1 shows the outcome of interest and columns 2-8 contain the associated regression coefficients. Regression results in column 2 are obtained using the full set of matching variables as listed in the third row while the results in columns 3-8 are obtained using a smaller set of matching variables. The excluded variables are listed in the third row for columns 3-8. Please note that the results in column 2 using the full set of matching variables are identical to those discussed in the paper.

As it relates to the first stage results, in all cases the F-stat is well above the conventional rule of thumb (10). For income received in childhood, the F-stat marginally increased in two cases as compared to using the full set of matching variables. These two cases are: excluding number of children; and excluding state of residence and number of children. These two cases also show a negative (statistically significant) sign on the coefficients for simulated EITC benefits, unlike the other cases. For income received in adulthood, the F-stat marginally increased in three cases as compared to using the full set of matching variables. These three cases are: excluding number of children; excluding state of residence; and excluding state of residence and number of children.

Considering the second stage results, using a smaller set of matching variables, the sign of the coefficients on income received in childhood remains the same as when using the full set of matching variables in most cases and the magnitudes are very similar. Furthermore, there is very little evidence that income received in childhood has a direct effect on health in early adulthood. For income received in adulthood, the sign of the coefficients remains the same as the when using the full set of matching variables in most cases, and the magnitudes are very similar with a few exceptions. There is strong evidence that income received as an adult affects individuals' health behaviors as well as general health status, obesity and BMI.

Table E1 A: First Stage Regression Results- Dependent Variable is Childhood Average Per Capita Family Income

(1)	(2) State, Sex, Race, Age, Marital Status and Number of Children	(3) Exclude: Number of Children	(4) Exclude: Marital Status	(5) Exclude: State	(6) Exclude: State and Number of Children	(7) Exclude: State and Marital Status
Simulated Income	0.864***	1.055***	0.730***	0.858***	1.090***	0.725***
	(0.0399)	(0.0498)	(0.0426)	(0.0399)	(0.0531)	(0.0426)
Simulated EITC Benefits	0.0516***	-0.0854**	0.0129	0.0743***	-0.139***	0.0609**
	(0.0118)	(0.0267)	(0.0116)	(0.0192)	(0.0378)	(0.0196)
Observations	5,386	5,386	5,386	5,386	5,386	5,386
F-stat	81.34	81.64	77.33	81.24	81.72	77.53

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects. Column (2) is from main text.

Table E1 B: First Stage Regression Results- Dependent Variable is Adulthood Average Per Capita Family Income

(1)	(2) State, Sex, Race, Age, Marital Status and Number of Children	(3) Exclude: Number of Children	(4) Exclude: Marital Status	(5) Exclude: State	(6) Exclude: State and Number of Children	(7) Exclude: State and Marital Status
Simulated Income	0.920***	1.014***	0.771***	0.930***	0.983***	0.770***
	(0.0263)	(0.0296)	(0.0244)	(0.0247)	(0.0319)	(0.0246)
Simulated EITC Benefits	0.0153	0.0748***	0.00601	0.0409***	0.131***	0.00294
	(0.00865)	(0.0177)	(0.00998)	(0.0102)	(0.0273)	(0.0108)
Observations	5,386	5,386	5,386	5,386	5,386	5,386
F-stat	81.07	81.87	73.36	81.37	82.00	73.35

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.

Table E2 A: The Effect of Log(Income) in Childhood on Adult Health Outcomes- IV

(1)	(2) State, Sex, Race, Age, Marital Status and Number of Children	(3) Exclude: Number of Children	(4) Exclude: Marital Status	(5) Exclude: Marital Status and Number of Children	(6) Exclude: State	(7) Exclude: State and Number of Children	(8) Exclude: State and Marital Status
Very good or excellent general health	0.0619	0.0477	0.0829	0.0810	0.0536	0.0454	0.0699
	(0.0464)	(0.0467)	(0.0554)	(0.0508)	(0.0446)	(0.0467)	(0.0545)
Obesity	0.0426	0.0427	0.0162	0.0393	0.0374	0.0493	0.0172
	(0.0384)	(0.0383)	(0.0385)	(0.0387)	(0.0343)	(0.0385)	(0.0376)
Body Mass Index	0.484	0.521	0.406	0.755	0.492	0.603	0.439
	(0.682)	(0.569)	(0.753)	(0.715)	(0.642)	(0.570)	(0.753)
Diabetes	0.00354	-0.00219	-0.00962	-0.0119	-0.00304	-0.00422	-0.0103
	(0.0186)	(0.0174)	(0.0211)	(0.0205)	(0.0181)	(0.0174)	(0.0211)
Hypertension	0.0572	0.0403	0.0394	0.0510	0.0492	0.0430	0.0370
	(0.0325)	(0.0338)	(0.0345)	(0.0381)	(0.0333)	(0.0338)	(0.0331)
Heart Disease	0.00265	0.000152	-0.00584	-0.00468	0.00222	0.000151	-0.00454
	(0.0124)	(0.0136)	(0.0122)	(0.0135)	(0.0125)	(0.0134)	(0.0126)
Metabolic Syndrome	0.00831	0.00333	0.000929	0.00158	0.00582	0.00193	0.000436
	(0.0165)	(0.0136)	(0.0159)	(0.0140)	(0.0161)	(0.0131)	(0.0164)
Physical limitations	0.0667	0.0644^{*}	0.0453	0.0551	0.0622	0.0629^{*}	0.0433
	(0.0372)	(0.0323)	(0.0396)	(0.0362)	(0.0374)	(0.0316)	(0.0403)
Heart Attack	-0.0123	-0.0132	-0.00748	-0.0108	-0.0126	-0.0133	-0.00737
	(0.00850)	(0.00813)	(0.00771)	(0.00792)	(0.00847)	(0.00815)	(0.00773)
Arthritis	0.0210	0.0312	-0.000813	0.0245	0.0166	0.0329	-0.00246
	(0.0210)	(0.0202)	(0.0255)	(0.0254)	(0.0208)	(0.0197)	(0.0250)
Stroke	-0.0121	-0.00609	-0.00926	-0.00542	-0.00712	-0.00703	-0.00428
	(0.0129)	(0.00864)	(0.0113)	(0.00911)	(0.00996)	(0.00870)	(0.00791)
Cancer	-0.00918	-0.0118	-0.00635	-0.0115	-0.00451	-0.0123	-0.00432
	(0.0161)	(0.0153)	(0.0169)	(0.0183)	(0.0162)	(0.0149)	(0.0172)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects. Each regression simultaneously includes income in childhood and income in adulthood.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table E2 B: The Effect of Log(Income) in Adulthood on Adult Health Outcomes- IV

(1)	(2) State, Sex, Race, Age, Marital Status and Number of Children	(3) Exclude: Number of Children	(4) Exclude: Marital Status	(5) Exclude: Marital Status and Number of Children	(6) Exclude: State	(7) Exclude: State and Number of Children	(8) Exclude: State and Marital Status
Very good or excellent	0.121***	0.130***	0.0776*	0.0641	0.118***	0.128***	0.0748*
general health	(0.0266)	(0.0252)	(0.0343)	(0.0351)	(0.0271)	(0.0255)	(0.0344)
Obesity	-0.0989***	-0.0991***	-0.0921**	-0.0910**	-0.101***	-0.0967***	-0.0951**
	(0.0273)	(0.0269)	(0.0313)	(0.0316)	(0.0275)	(0.0267)	(0.0311)
Body Mass Index	-1.523***	-1.515***	-1.193**	-1.177**	-1.522***	-1.489***	-1.218**
	(0.452)	(0.448)	(0.430)	(0.413)	(0.456)	(0.448)	(0.433)
Diabetes	-0.0166	-0.0191*	-0.0328**	-0.0304**	-0.0189*	-0.0191*	-0.0327**
	(0.00948)	(0.00906)	(0.0118)	(0.0109)	(0.00959)	(0.00898)	(0.0116)
Hypertension	-0.0163	-0.0185	-0.0344	-0.0358	-0.0190	-0.0184	-0.0356
	(0.0205)	(0.0192)	(0.0236)	(0.0225)	(0.0201)	(0.0191)	(0.0235)
Heart Disease	0.00110	0.00263	-0.00334	-0.00296	0.00111	0.00258	-0.00263
	(0.00786)	(0.00749)	(0.00847)	(0.00922)	(0.00803)	(0.00741)	(0.00856)
Metabolic Syndrome	-0.0107	-0.0107	-0.00916	-0.00782	-0.0116	-0.0106	-0.00911
	(0.00736)	(0.00656)	(0.00785)	(0.00736)	(0.00733)	(0.00661)	(0.00758)
Physical limitations	-0.0103	-0.0134	-0.0118	-0.00906	-0.0118	-0.0137	-0.0115
	(0.0176)	(0.0174)	(0.0197)	(0.0198)	(0.0179)	(0.0172)	(0.0194)
Heart Attack	-0.00394	-0.00381	-0.00377	-0.00281	-0.00395	-0.00372	-0.00381
	(0.00595)	(0.00564)	(0.00557)	(0.00526)	(0.00588)	(0.00565)	(0.00536)
Arthritis	-0.0171	-0.0188	-0.0181	-0.0206	-0.0184	-0.0189	-0.0177
	(0.0130)	(0.0131)	(0.0188)	(0.0181)	(0.0132)	(0.0131)	(0.0185)
Stroke	-0.00119	-0.00204	-0.00183	0.00208	0.0000781	-0.000213	-0.00110
	(0.00740)	(0.00731)	(0.00587)	(0.00624)	(0.00713)	(0.00690)	(0.00550)
Cancer	0.000921	0.00365	0.00148	0.00520	0.00269	0.00361	0.00173
	(0.00805)	(0.00799)	(0.0116)	(0.0114)	(0.00820)	(0.00802)	(0.0114)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects. Each regression simultaneously includes income in childhood and income in adulthood. Standard errors are clustered at the state level to account for within state correlated error terms.

Table E3 A: The Effect of Log(Income) in Childhood on Adult Health Behaviors- IV

(1)	(2) State, Sex, Race, Age, Marital Status and Number of Children	(3) Exclude: Number of Children	(4) Exclude: Marital Status	(5) Exclude: Marital Status and Number of Children	(6) Exclude: State	(7) Exclude: State and Number of Children	(8) Exclude: State and Marital Status
Any exercise at least 10 mins. per week	0.0311	0.00441	0.0103	0.0265	0.0235	0.0209	0.0174
	(0.0474)	(0.0506)	(0.0474)	(0.0566)	(0.0445)	(0.0450)	(0.0436)
Whether currently smokes	-0.00329	-0.0304	-0.0342	-0.0350	-0.0114	-0.0308	-0.0268
	(0.0516)	(0.0491)	(0.0512)	(0.0534)	(0.0483)	(0.0477)	(0.0488)
Number of cigarettes currently smoked per day (including non-smokers)	0.149	-0.558	-0.643	-1.111	0.219	-0.615	-0.449
	(0.781)	(0.694)	(0.742)	(0.749)	(0.700)	(0.685)	(0.681)
Quit smoking	0.0609	0.108	0.105	0.0953	0.0618	0.108	0.102
	(0.0911)	(0.0881)	(0.101)	(0.0994)	(0.0901)	(0.0886)	(0.0987)
Whether currently drinks alcohol	0.0760	0.117	0.0592	0.0581	0.0569	0.125	0.0505
	(0.0628)	(0.0682)	(0.0640)	(0.0682)	(0.0603)	(0.0666)	(0.0617)
Number of drinks per day (including non-drinkers)	0.151	0.199	-0.0852	-0.0304	0.0250	0.213	-0.117
	(0.209)	(0.226)	(0.281)	(0.288)	(0.214)	(0.215)	(0.273)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	4.151^{*}	3.886	1.718	3.046	2.898	3.670	1.169
	(1.982)	(3.165)	(3.429)	(4.328)	(1.723)	(3.131)	(3.193)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table E3 B: The Effect of Log(Income) in Childhood on Adult Health Behaviors- IV

(1)	(2) State, Sex, Race, Age, Marital Status and Number of Children	(3) Exclude: Number of Children	(4) Exclude: Marital Status	(5) Exclude: Marital Status and Number of Children	(6) Exclude: State	(7) Exclude: State and Number of Children	(8) Exclude: State and Marital Status
Any exercise at least 10 mins. per week	0.0721**	0.0781**	0.0309	0.0272	0.0701**	0.0821***	0.0306
	(0.0242)	(0.0243)	(0.0238)	(0.0249)	(0.0252)	(0.0237)	(0.0245)
Whether currently smokes	-0.101***	-0.118***	-0.0564	-0.0417	-0.104***	-0.115***	-0.0557
	(0.0279)	(0.0290)	(0.0359)	(0.0372)	(0.0269)	(0.0287)	(0.0352)
Number of cigarettes currently smoked per day (including non-smokers)	-1.705***	-1.877***	-0.952	-0.564	-1.686***	-1.827***	-0.921
	(0.394)	(0.404)	(0.489)	(0.509)	(0.383)	(0.399)	(0.482)
Quit smoking	0.130**	0.156**	0.0787	0.0651	0.131**	0.158**	0.0751
	(0.0479)	(0.0495)	(0.0616)	(0.0661)	(0.0470)	(0.0504)	(0.0614)
Whether currently drinks alcohol	0.124***	0.109***	0.228***	0.209***	0.117***	0.113***	0.222***
	(0.0287)	(0.0292)	(0.0421)	(0.0395)	(0.0289)	(0.0295)	(0.0401)
Number of drinks per day (including non-drinkers)	0.0759	0.0613	0.524***	0.547***	0.0262	0.0741	0.497**
	(0.106)	(0.105)	(0.152)	(0.152)	(0.106)	(0.105)	(0.154)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	1.652	1.299	5.032*	5.406*	1.197	1.345	4.835*
	(1.775)	(1.687)	(2.230)	(2.440)	(1.878)	(1.688)	(2.230)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Appendix F: Assessing Potential Measurement Error in the Income Variable

In this section, I examine the degree to which there might be measurement error in the income variable. To do so, I divide income from conception to age 32 into income received at even ages and income received at odd ages. I then estimate the effect of average income at even ages on each health variable using OLS regression. Next, I rerun those same regressions using average income at odd ages as an instrument for average income at even ages. Because any measurement error in even years would be independent of measurement error in odd years, the difference in the size of the OLS and IV estimates gives a sense of the degree of the measurement error. If coefficients from both sets of regression are similar, that suggests the measurement error problem is not severe. If coefficients are very different, that suggests a high degree of measurement error. In all cases, both coefficients are very similar which suggests very little evidence of measurement error.

Table F 1: First Stage Regression Results- Dependent Variable is Average Per Capita Family Income at Even Ages

	(1) Average Income for Even Ages
Average Income for Odd Ages	0.930*** (0.00474)
Observations	5,386
F-stat	392.4

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.

Table F 2: The Effect of Log(Income) on Health

	OLS	IV
Very good or excellent general	0.108^{***}	0.106^{***}
health	(0.0159)	(0.0174)
Obesity	-0.0676^{***}	-0.0759^{***}
	(0.0148)	(0.0143)
Body Mass Index	-1.251^{***}	-1.319^{***}
	(0.192)	(0.197)
Diabetes	-0.0129^{***}	-0.0140^{***}
	(0.00350)	(0.00372)
Hypertension	-0.0403^{***}	-0.0451^{***}
	(0.00820)	(0.00900)
Heart Disease	-0.00386	-0.00189
	(0.00282)	(0.00306)
Metabolic Syndrome	-0.0343^{***}	-0.0410^{***}
	(0.00924)	(0.00987)
Physical limitations	-0.0469^{***}	-0.0497^{***}
	-0.0469^{***}	(0.00966)
Heart Attack	-0.00760^{***}	-0.00785^{**}
	(0.00208)	(0.00256)
Arthritis	-0.0272^{***}	-0.0265^{***}
	(0.00750)	(0.00661)
Stroke	-0.00790^{***}	-0.00896^{***}
	(0.00223)	(0.00255)
Cancer	-0.0119^{**}	-0.0133^{***}
	(0.00380)	(0.00391)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table F 3: The Effect of Log(Income) on Health Behaviors

	OLS	IV
Any exercise at least 10 mins. per week	0.0667***	0.0692***
	(0.00991)	(0.0106)
Whether currently smokes	-0.118***	-0.129***
	(0.0150)	(0.0167)
Number of cigarettes currently smoked per day (including non-smokers)	-1.814***	-1.945***
	(0.280)	(0.306)
Quit smoking	0.132***	0.140***
	(0.0206)	(0.0225)
Whether currently drinks alcohol	0.127***	0.142***
	(0.0131)	(0.0121)
Number of drinks per day (including non-drinkers)	0.132**	0.153***
	(0.0385)	(0.0397)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	0.684	0.959
	(0.703)	(0.676)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Appendix G: Family Income Expressed Relative to the Poverty Threshold

The results in this section are from specifications where family income is expressed relative to the poverty threshold as opposed to the family size. For ease of comparison, the results from the original specification which uses income expressed relative to the number of family members are presented in columns 2 and 3. The results for income expressed relative to the poverty threshold are included in columns 4 and 5. In all cases except for number of drinks per day the conclusions remain the same. Using income expressed relative to the poverty threshold, the coefficient on income during adulthood on the number of drinks switches signs to become negative and is also statistically significant at the 5% level.

Table G 1: First Stage Regression Results- Dependent Variable is Log Average Family Income Relative to the Poverty Threshold

	(1) Age 0-18	(2) Age 19-32
Log Simulated Income	0.905***	0.941***
	(0.0453)	(0.0335)
Log Simulated EITC	0.134***	-0.00973
Benefits	(0.0218)	(0.00923)
Observations	5,386	5,386
F-stat	76.32	67.08

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.

Table G 2: The Effect of Log(Income/Poverty) on Health

(1)	(2)	(3)	(4)	(5)
	Per Capita Income		Relative to Poverty Threshold	
Health Outcomes	Childhood	Adulthood	Childhood	Adulthood
Very good or excellent general health	0.0619	0.121^{***}	0.181^{***}	0.165^{***}
	(0.0464)	(0.0266)	(0.0540)	(0.0383)
Obesity	0.0426	-0.0989^{***}	0.0272	-0.109^{**}
	(0.0384)	(0.0273)	(0.0430)	(0.0344)
Body Mass Index	0.484	-1.523^{***}	0.110	-1.657^{**}
	(0.682)	(0.452)	(0.777)	(0.583)
Diabetes	0.00354	-0.0166	-0.0207	-0.0204
	(0.0186)	(0.00948)	(0.0182)	(0.0149)
Hypertension	0.0572	-0.0163	0.0648^{*}	-0.0154
	(0.0325)	(0.0205)	(0.0270)	(0.0258)
Heart Disease	0.00265	0.00110	-0.00282	0.00253
	(0.0124)	(0.00786)	(0.0104)	(0.0106)
Metabolic Syndrome	0.00831	-0.0107	0.00424	-0.0147
	(0.0165)	(0.00736)	(0.0135)	(0.00927)
Physical limitations	0.0667	-0.0103	0.000398	-0.0357
	(0.0372)	(0.0176)	(0.0332)	(0.0229)
Heart Attack	-0.0123	-0.00394	-0.0134	-0.00672
	(0.00850)	(0.00595)	(0.00860)	(0.00820)
Arthritis	0.0210	-0.0171	-0.00189	-0.0329
	(0.0210)	(0.0130)	(0.0249)	(0.0177)
Stroke	-0.0121	-0.00119	-0.0201	0.00308
	(0.0129)	(0.00740)	(0.0120)	(0.00816)
Cancer	-0.00918	0.000921	-0.0340	0.00503
	(0.0161)	(0.00805)	(0.0188)	(0.0111)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Table G 3: The Effect of Log(Income/Poverty) on Health Behaviors

(1)	(2)	(3)	(4)	(5)
	Per Capita Income		Relative to Poverty Threshold	
Health Behaviors	Childhood	Adulthood	Childhood	Adulthood
Exercise				
Any exercise at least 10 mins. per week	0.0311	0.0721**	0.0455	0.0915**
	(0.0474)	(0.0242)	(0.0444)	(0.0338)
Smoking				
Whether currently smokes	-0.00329	-0.101***	-0.156**	-0.177***
	(0.0516)	(0.0279)	(0.0584)	(0.0327)
Number of cigarettes currently smoked per day (including non-smokers)	0.149	-1.705***	0.241	-1.277***
	(0.781)	(0.394)	(0.228)	(0.226)
Quit smoking	0.0609	0.130**	-0.0370	0.106***
	(0.0911)	(0.0479)	(0.0228)	(0.0235)
Drinking				
Whether currently drinks alcohol	0.0760	0.124***	0.113	0.0735*
	(0.0628)	(0.0287)	(0.0578)	(0.0371)
Number of drinks per day (including non-drinkers)	0.151	0.0759	0.169**	-0.183**
	(0.209)	(0.106)	(0.0633)	(0.0582)
Number of days consume 4 to 5 drinks per year (including non-drinkers)	4.151*	1.652	1.105	-1.641
	(1.982)	(1.775)	(1.404)	(1.211)

Notes: Standard errors in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All regressions include individual demographic controls, controls for the matching variables, cohort fixed effects, cohort by survey year fixed effects and state fixed effects.
Standard errors are clustered at the state level to account for within state correlated error terms.

Vita

Nicardo Sedaka McInnis was born on the 26th day of September 1989 in a rural agrarian community in Flamstead, Jamaica. He is one of two sons of Marlene Perry and Ral McInnis. He received his primary education from the Riverside All Age School and then matriculated to Rusea's High School where he also completed an associate's degree. He then worked for a year as an accounting associate at the Affiliated Computer Services for before enrolling as an undergraduate student at the University of the West Indies, Mona campus. He began as an international relations major, but quickly developed an interest in economics. He graduated with a Bachelor of Science degree with the highest honors, double majoring in economics and international relations. Immediately following his undergraduate studies, he enrolled in a graduate program at the same university where he also worked as a graduate research assistant. He graduated with a Master of Science degree in economics, with the highest honors. He subsequently worked for a year as a Research Consulting Officer at Jamaica Promotions and as a Research Assistant in the Department of Sociology, Psychology and Social Work at the University of the West Indies, Mona campus. Nicardo joined the Georgia State University community in August of 2015 as a Ph.D. student, where he also worked as a graduate research assistant. He also served as instructor for Principles of Microeconomics and Principles of Macroeconomics. He holds a Master of Arts degree in Economics from Georgia State University and is expected to receive his Ph.D. in Economics in May of 2020.

Nicardo's upbringing inspired a passion for contributing to the betterment of vulnerable groups. His work centers on the low-income population. He studies the effects of income disparities and how safety net programs can be used to close those gaps. His current research mainly considers health outcomes, but his goal is to focus more broadly on child and human capital development and how they influence later life outcomes in the labor market, such as employment and earnings. In his dissertation, he studies the long-term health effects of family income received at different stages in the life cycle. This work sets the foundation for his future research agenda which is to develop a more detailed understanding of the intended and unintended consequences of income and safety net programs. His work also focuses on using cutting-edge techniques that were learned throughout his training in graduate school to get at cause and effect.

To date, Nicardo has had several internship appointments in Jamaica and also received several scholarships and awards. The institutions at which he interned include: PetroCaribe Development Fund, Caribbean Policy Research Institute, and the Bank of Jamaica. The major awards he has received include: a national diploma for outstanding performance in the Jamaican School Certificate Examinations; macroeconomic internship sponsorship from the Caribbean Regional Technical Assistance Centre, an agency of the International Monetary Fund; Association for Public Policy and Management Equity & Inclusion Student Fellowship; Association for Health Economists Diversity Scholarship; Carolyn McClain Young Scholarship; and the George Malanos Economics Scholarship.

Nicardo will officially join the economics department at California State University, Northridge campus in the upcoming fall semester as a tenure track Assistant Professor. However, he will be taking a year off to pursue a postdoctoral fellowship in the Population Studies Center at the University of Michigan, which is funded by the National Institute on Aging.