

## Early-Childhood Poverty and Adult Attainment, Behavior, and Health

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This article assesses the consequences of poverty between a child's prenatal year and 5th birthday for several adult achievement, health, and behavior outcomes, measured as late as age 37. Using data from the Panel Study of Income Dynamics (1,589) and controlling for economic conditions in middle childhood and adolescence, as well as demographic conditions at the time of the birth, findings indicate statistically significant and, in some cases, quantitatively large detrimental effects of early poverty on a number of attainment-related outcomes (adult earnings and work hours). Early-childhood poverty was not associated with such behavioral measures as out-of-wedlock childbearing and arrests. Most of the adult earnings effects appear to operate through early poverty's association with adult work hours.

Some 4.2 million infants, toddlers, and preschoolers lived in poverty in the United States in 2007. For a single mother with two children, this meant that total income was less than \$16,705; many poor families had income well below that amount (U.S. Census Bureau, 2008). Poverty and its attendant stressors have the potential to shape the neurobiology of the developing child in powerful ways, which may lead directly to poorer outcomes later in life. Poverty in early childhood can also affect adult attainment, behavior, and health indirectly through parents' material and emotional investments in children's learning and development.

The sensitivity of early childhood to environmental influences has been demonstrated in a wide range of infant, toddler, and preschooler intervention studies. Many descriptive studies show that, relative to nonpoor children, poor children will be

less successful in school and, as adults, in the labor market, have poorer health, and be more likely to commit crimes and engage in other forms of problem behavior (see Holzer, Schanzenbach, Duncan, & Ludwig, 2007, for a review of these studies). Despite these associations, it is far from clear to what extent poverty *itself* is the cause of these differences. Our primary goal in this study is to obtain relatively unbiased estimates of the total effects association between early-childhood poverty and adult attainment, behavior, and health. Extending the work of Duncan, Brooks-Gunn, Yeung, and Smith (1998), we use the most recent data from the Panel Study of Income Dynamics (PSID) to examine the long-run (i.e., as late as age 37) impacts of low income early in life, net income later in childhood, and other correlated family factors surrounding a child's birth.

### *Background*

Emerging evidence from human and animal studies highlights the critical importance of early childhood for brain development and for setting in place the structures that will shape future cognitive, social, emotional, and health outcomes (Sapolsky, 2004; Shonkoff & Phillips, 2000). How poverty early in childhood might affect these structures has been

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We thank William Dickens, Rob Dugger, Mimi Engel, Michael Foster, Rucker Johnson, Ann Siegal, Aaron Sojourner, Sergio Urzua, Sara Watson, and participants of the University of Michigan's National Poverty Center conference "Long-Run Impact of Early Life Events" and of seminars at the Brookings Institution, Brown University, Cornell University, and the University of Oxford for comments on related drafts. This project was funded in part by the Partnership for America's Economic Success. Ziol-Guest acknowledges the Robert Wood Johnson Foundation Health and Society Scholars program at Harvard University for its financial support.

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a matter of considerable research. Economic models of skill acquisition illustrate the potential importance of early environmental conditions on adult cognitive and noncognitive skills (Knudsen, Heckman, Cameron, & Shonkoff, 2006). Among other things, low income during early childhood may limit parents' ability to purchase adequate-quality health care or education during children's formative years.

Complementary studies in psychology and social epidemiology illustrate that both in utero environments and early-childhood experiences have long-run impacts on adult physical and mental health (Barker, Eriksson, Forsen, & Osmond, 2002; Danese, Pariante, Caspi, Taylor, & Poulton, 2007; Poulton & Caspi, 2005). Epidemiologists have suggested that early-childhood stressors related to low income could alter or dysregulate biological systems, with adverse implications for future health (Godfrey & Barker, 2000). Psychologists posit that poverty and economic insecurity undermine parents' mental health and parenting behavior. In animal models, optimal "mothering" behavior in critical periods of early development is associated with lifelong stress reactivity and cognitive strength (Sap, 2004).

Duncan and Brooks-Gunn (1997) were the first to take a broad look at the possible longer run consequences of early-childhood poverty. Twelve groups of researchers working with 10 different nonexperimental but longitudinal data sets estimate longitudinal models of early-childhood income effects on later attainment, behavior, and health. On the whole, the results suggest that family income has substantial, albeit selective associations with children's subsequent attainments.

First, family income had consistently larger associations with measures of children's cognitive ability and achievement than with measures of behavior, mental health, and physical health. Second, family economic conditions in early childhood appeared to be more important for shaping ability and achievement than did family economic conditions during adolescence. And third, the association between income and achievement appeared to be nonlinear, with the biggest impacts at the lowest levels of income.

#### *Why the Timing of Income May Matter*

The present study builds on the key conclusion from Duncan and Brooks-Gunn (1997) that family economic conditions in early childhood appear to matter more for shaping later development than economic conditions during adolescence. Develop-

mental theory suggests that given the nature of developmental tasks, sensitivity to change, and interactions with the environment, early childhood is a developmental period that may be especially sensitive to environmental conditions affected by family income (Shonkoff & Phillips, 2000). Moreover, early courses of development may reach well into adulthood. Waddington (1957) has described development as proceeding along the branches of a tree—although changes in developmental trajectories can occur at any point at which a new branch is formed, the ability of the individual to alter his or her developmental course substantially becomes increasingly difficult over time. These themes are reflected in economic, psychological, and neurobiological perspectives on the importance of early childhood.

Cunha, Heckman, Lochner, and Masterov (2005) proposed an economic model of development in which preschool cognitive and socioemotional capacities are key ingredients for human capital acquisition during the school years. In their model, "skill begets skill" as early capacities can affect the productivity of school-age human capital investments. Economic deprivation in early childhood could create disparities in school readiness and early academic success that persist or widen over the course of childhood. The theory proposed by Heckman and colleagues is novel in economics for its developmental perspective: Typically, the economics literature has ignored the notion that the effects on children's development of economic conditions may depend upon childhood stage and instead focuses on the role of "permanent" income, with the assumption that families anticipate bumps in their life-cycle paths and can save and borrow freely to smooth their consumption across these bumps (Blau, 1999).

Developmental psychology clearly stresses the importance of understanding children's distinct developmental stages (Bronfenbrenner & Morris, 1998). In the context of poverty studies, the greater malleability of children's development and the overwhelming importance of the family (as opposed to school or peer contexts) for preschoolers lead us to expect that family income in early childhood may be much more important for shaping children's ability and achievement than conditions later in childhood (Bronfenbrenner & Morris, 1998; Shonkoff & Phillips, 2000). For example, to the extent that poverty increases mothers' psychological stress or harsh parenting behaviors (Yeung, Linver, & Brooks-Gunn, 2002), this will be especially important during early childhood given the

primacy of sensitive mother–child interactions for the development of young children’s emotion regulation (Waters & Sroufe, 1983). Mastery of emotion regulation in early childhood can have long-run impacts on children’s achievement, behavior, and health (Fox, 1994). Similarly, to the extent that cognitively enriching early home environments lay the groundwork for success in preschool and beyond, parents’ ability to purchase books, toys, and enriching activities during this stage of development is paramount.

As its name suggests, the “fetal origins” hypothesis also points to a sensitive period in early childhood. In this case, the in utero environment is critical. This hypothesis posits an in utero programming process whereby stimulants and insults during this critical period of development have long-lasting implications for physiology and disease risk (Barker et al., 2002; Godfrey & Barker, 2000; Hertzman, 1999). Low income during the prenatal period may be associated with fetal undernutrition, low birth weight, or slow growth in the first 2 years of life. A pattern of small size at birth and low body mass index (BMI) at age 2, followed by rapid weight gain after age 2, is a risk factor for the development of insulin resistance and a disproportionately high fat mass in relation to muscle mass (Barker, Osmond, Forsén, Kajantie, & Eriksson, 2005; Barker et al., 2002). Low income is also associated with food insecurity, which has been associated with overweight and obesity in childhood, adolescence, and adulthood, especially among females (Frongillo, 2003). Overweight and obesity, in turn, can be physically debilitating, which could lead to a negative cycle of depression, overeating, or stress (Stunkard, Faith, & Allison, 2003).

#### *Methods for Assessing Causal Impacts of Poverty*

Researchers generally do not dispute simple correlations between income and child developmental outcomes. However, there is much controversy about whether these income effects are causal. The key estimation problems in assessing the causal impact of family income on child well-being are twofold: timing of measurement and omitted-variable bias. Theory suggests that the development of children’s cognitive and social skills is a time-consuming process. Attainments in, say, adolescence, may well be a product of economic conditions not only in adolescence but also in early and middle childhood and possibly during the prenatal period as well (Barker, 1998). Estimates of models of

income effects that measure income concurrently with the child outcomes risk bias if income is volatile across childhood. As there is abundant evidence that income is indeed volatile (Duncan, 1988), a longitudinal perspective on the role of income in shaping child well-being appears crucial.

Even supposing that income is measured well across the entire period of childhood, it is difficult to isolate the causal impact of income, as many factors might jointly determine family income and child development and parental cognitive ability is a prime example (Rowe & Rodgers, 1997). Parents with higher cognitive ability are usually more successful in the labor market. At the same time, they are more likely to provide a higher quality learning environment for their children, regardless of how much money they may be spending on books or computers. Some studies find large reductions in the estimated impacts of income once adjustments for omitted-variable bias are implemented (Blau, 1999; Mayer, 1997).

State or national policies sometimes change in ways that provide researchers with opportunities to relate exogenous, policy-induced changes in family income to child well-being. Such is the case with the study by Dahl and Lochner (2005), who took advantage of the fact that the United States increased the generosity of its Earned Income Tax Credit (EITC) program during the 1990s. The EITC provides a refundable tax credit to low-income working families. The maximum size of the annual credit is now quite substantial—\$4,824—and it increased by about \$2,300 in the middle 1990s. Dahl and Lochner estimated that a \$3,000 increase in family income in early and middle childhood boosts reading achievement by about one tenth of a standard deviation and math achievement by about half that amount. These effects were two to three times as large, however, for children of non-White, unmarried, and less educated mothers, which corresponds to another key conclusion in Duncan and Brooks-Gunn (1997)—namely, that income effects are nonlinear such that increases in income matter more for lower income children than their higher income counterparts.

The different disciplines’ approaches to these questions each have their own merits and limitations. Developmental research articles on the impacts of poverty, although often paying close attention to the processes or pathways linking poverty with developmental outcomes, typically employ only a handful of family-based demographic covariates, opening the door for a host of omitted-variable biases. Research in economics,

which generally takes a “total effects” (reduced-form) approach, typically opts to exploit some kind of natural experiment—siblings differences within families or temporal or areal variation in income induced by policy (e.g., EITC expansion) or macroeconomic (e.g., unemployment differences) fluctuations. These approaches have stronger claims to internal validity (reduced bias) but, concentrating as they often do on narrow subpopulations, often sacrifice external validity. Such studies also often yield very imprecise estimates. And no prior study has been able to link income-based economic conditions in early childhood to outcomes measured well into adulthood.

We use data from a long-running national survey to surmount some of these limitations. As in many studies in economics, we aim to estimate the total effects (as opposed to a mediational model) of early-childhood poverty and adult attainment, behavior, and health. By taking a population-based approach, we minimize threats to external validity. And, in contrast to many developmental studies, we employ an unusually rich and powerful array of covariates to control for omitted-variable biases. In particular, our estimates of the impact of early-childhood poverty on adult attainment, behavior, and health include controls for income in middle childhood and adolescence. It is very difficult to think up omitted-variable bias stories involving early income that would not be controlled in large measure with the inclusion of income later in childhood.

This study draws on national data from the PSID, the longest-running longitudinal study of household income in the United States, to estimate linkages between income early in childhood and adult outcomes. Ours is the first study to link high-quality income data across the entire childhood period with adult outcomes measured as late as age 37. Our strategy is to measure income in every year of a child’s life from the prenatal period through age 15, distinguishing income early in life (prenatal through 5th year) from income in middle childhood and adolescence. Our analyses relate an array of adult achievement, social assistance, health and behavior measures to these childhood stage-specific measures of income, plus a host of relevant demographic control variables measured around point of the child’s birth. The wide range of adult outcomes we consider include educational attainment, earnings, work hours, receipt of food stamps and cash assistance, nonmarital childbearing, crime, and mental and physical health.

## Method

### *Sample*

We use 1968–2005 data from the PSID, which has followed a nationally representative sample of families and their children since 1968 (<http://psidonline.isr.umich.edu>). Our general strategy is to select children observed in the PSID between their prenatal year and at least age 25, although a number of our outcomes are drawn from the 2005 interview, in which individuals ranged from ages 30 to 37.

Our target study sample consists of the 2,599 individuals born to a sample member into PSID households between 1968 and 1975, who were thus between ages 30 and 37 in 2005. Sample losses to nonresponse are substantial, prompting us to use the PSID’s attrition-adjusted weights in all of our analyses. We required that these individuals have complete data on control variables measured around the time of their birth (defined next), leaving 93% of the birth sample ( $n = 2,414$ ). We further required that these individuals be in response families in at least 12 of the 17 years from prenatal year to age 15, a restriction that eliminated 479, or 18% of the original sample. Of the remaining 2,120, 1,589 participated in the PSID in adulthood and had nonmissing data on at least one outcome and a PSID attrition-adjusted weight. Sample sizes vary by outcome owing to when it was measured, and in some cases (distress and body mass) only information was collected from individuals who were “heads” or “wives” in PSID households.

### *Childhood Income*

We used the PSID’s high-quality edited measure of annual total family income, inflated to 2005 levels using the Consumer Price Index (CPI). We averaged these annual income measures across three periods: the prenatal year through the calendar year in which the child turned 5, ages 6–10, and ages 11–15. With income reported for calendar years and conceptions occurring continuously, there was some imprecision in matching income to the prenatal year and beyond. If a child was born prior to July 1, we took the prenatal year to be the prior calendar year. If the birth was after July 1, then the prenatal year was considered to be the year in which the birth occurred. Similarly, we defined “under age 6” as the last calendar year before the child’s sixth birthday. Thus defined, our “early-childhood” period consists of seven calendar years.

To account for a possible differential impact of increments to low as opposed to higher family income, we allowed the coefficients on average income within each childhood period to have distinct linear effects for average incomes up to \$25,000 and for incomes \$25,000 and higher. Considerable experimentation confirmed the utility of the \$25,000 “knot” in the case of the adult outcomes most sensitive to variations in early income; lower income knots reduced sample sizes for, and therefore the precision of, the low-income effect estimates; higher thresholds typically produced noteworthy reductions in the low-income segment coefficients.

#### *Adult Outcomes*

Dependent variables in our analyses spanned achievement, health, and behavioral domains. Years of completed schooling are based on the most recent report of schooling available in the data. In all cases, the report was taken when the individual was at least 22 years old and in most cases the individual was at least 25.

Adult work hours and natural logarithm of the child’s adult earnings were gleaned from all available annual reports of earned income and work hours reported by or for the child when the child was aged 25 or older. As with childhood income, we inflated the dollar values of earnings to 2005 price levels using the CPI. To adjust earnings for age and calendar year, we regressed all of our yearly earnings observations on sets of dummy variables measuring the age of the respondent in the given year, and the calendar year of measurement. We then generated residuals from this regression for each sample individual’s earnings observations and averaged these residuals across all of the yearly earnings observations that a given individual generated. We centered these average residuals around the sample mean by adding them to the overall sample mean earnings. We repeated this process to adjust work hours for age and year effects. As a final step in the case of earnings, we took the natural logarithm.

Food stamp and Aid to Families with Dependent Children/Temporary Assistance to Needy Families (AFDC/TANF) receipt are measured at the household level and are taken from all available surveys when the child was aged 25 or older. Although some adults under age 25 are program recipients, it was difficult to assign transfer income sources to individual household members prior to around age 25. We created calendar-year values of both

programs and inflated the values to 2005 price levels using the CPI. Like average annual earnings, we adjust for age and calendar-year effects by regressing all food stamp and AFDC/TANF values on age and calendar-year dummies, obtained the residuals, and calculated the average residuals and mean values across all available years for a given individual. We estimate food stamp models for the entire sample but AFDC/TANF models only for the females.

Our measure of poor overall health was based on the most recent response to the question “I have a few questions about your health, including any serious limitations you might have. Would you say your health in general is excellent, very good, good, fair, or poor?” Individuals are considered in poor health if they responded that their health was either fair or poor. Although the results are not reported in the tables, we also ran our models using a continuous measures of health-assigning integer values of 1–5 for these respective categories and with an alternative scaling of *excellent* = 100, *very good* = 85, *good* = 70, *fair* = 30, and *poor* = 0. Our regression results are robust to this specification. Because we used the most recently available report of self-rated health, our regression analyses of this outcome also include calendar-year dummy variables for when the individual’s report was taken.

Our measure of adult BMI was calculated based on reports in the 2005 survey of heads and wives of their weight in pounds and their height in feet and inches. We calculate BMI using the following formula:

$$\frac{\text{Weight} \times 703}{\text{Height}^2},$$

where weight is measured in pounds and height is measured in inches. We follow convention and define “overweight” as a BMI  $\geq 25$  (Centers for Disease Control and Prevention, 2003).

Our measure of psychological distress was based on responses to a 2003 administration to heads and wives of the K-6 NonSpecific Psychological Distress Scale, developed by Ronald Kessler. It includes six items, ranging from *all of the time* = 4, *most of the time* = 3, *some of the time* = 2, *a little of the time* = 1, and *none of the time* = 0. The questions are: “Now, I am going to ask you some questions about feelings you may have had over the past 30 days.” In the past 30 days, about how often did you feel: (a) so sad nothing could cheer you up? (b) nervous? (c) restless or fidgety? (d) hopeless? (e) that everything was an effort? and (f) worthless? The scores are summed; a score of 13 or higher is considered

to be the threshold for the clinically significant range of the distribution of nonspecific psychological distress, which we refer to here as “high distress.” As with the general health measure, we also analyzed a continuous measure of distress and found the same general patterns.

The crime outcomes consisted of responses to questions asked in the 1995 interviewing wave regarding past arrests and time in jail. A dichotomous “arrest” outcome was coded for an affirmative response to the question: “Not counting minor traffic offenses (has he/has she/have you) ever been booked or charged for breaking a law?” A dichotomous “jail” outcome was coded for an affirmative response to the question: “(Has he/Has she/Have you) ever spent time in a corrections institution like a jail, a prison or a youth training or reform school?” Owing to the infrequency of arrest and incarceration among females, this analysis was restricted to males.

Finally, our nonmarital birth outcome is based on a dichotomous indicator of whether the individual (females only) reported a nonmarital birth prior to her 21st birthday in the PSID’s fertility and marital histories.

#### *Control Variables and Regression Procedures*

To avoid attributing to income what should be attributed to correlated determinants of both childhood income and our outcomes of interest, we included the following control variables in all of our regressions: (a) dummy variables for seven of the eight birth years, (b) race (Black, other, with White the reference category), (c) child’s sex (male = 1), (d) whether the child’s parents were married and living together at the time of the birth, (e) the age of the mother at the time of the birth, (f) whether the child lived in the South at the time of the birth, (g) the total number of siblings born to the child’s mother, (h) whether the child was the firstborn to his or her mother, (i) years of completed schooling of the household head (usually the father in two-parent households, the mother in single-parent households) of the child’s household in the birth year, and (j) the head’s score on a sentence completion test administered in the 1972 interviewing wave.

Additionally, we include several variables taken in the earliest years of the PSID and use the survey response from the year closest to the child’s birth. All but the first of these measures are collected from the household head. First, we include the response to an interviewer observation regarding the cleanliness of the respondent’s dwelling.

Responses ranged from 1 (*very clean*) to 5 (*dirty*); therefore, higher values represent perceptions of less clean home environments. Dunifon, Duncan, and Brooks-Gunn (2001) found significant links between this measure and children’s completed schooling. Second, we include an index of parental expectations for children that reflects expectations for their own as well as their children’s futures. Components of this scale include responses to statements such as having explicit plans for children’s education and jobs. Third, we include two motivational measures: (a) an assessment of the importance of challenge versus affiliation and (b) a measure of an individual’s sense of personal control. Both of these measures are associated with long-run labor market success (Duncan & Dunifon, 1998). The challenge versus affiliation measure was created by averaging responses to two questions “Would you like to have more friends, or would you like to do better at what you try?” and “Would you prefer a job where you had to think for yourself, or one where you work with a nice group of people?” A score of 1 indicates preference for challenge, whereas those who preferred to have more friends or work with nice people received scores of 0. Sense of personal control was obtained through three questions: “Have you usually felt pretty sure that your life will work out the way you want it to, or have there been more times when you haven’t been sure about it?” “When you make plans ahead, do you usually get to carry out things the way you expected, or do things usually come up to make you change your plans?” and “Would you say that you nearly always finish things once you start them, or do you sometimes have to give up before they are finished?” Responses were scored 1 for a positive response, 0.5 for an equivocal response, and 0 for a negative response. Finally, we included a measure of risk avoidance, which ranges from 0 to 9 capturing avoiding risk on everyday things including having at least one car in good condition, all cars are insured, using seatbelts, health insurance or free medical care, family does not smoke, and family has some savings.

All of our regressions were run in STATA SE 9.0 standard error, use the PSID’s weights to adjust for differential sampling fractions and attrition, and adjust for origin–family clustering on the mother using Huber–White methods. Experimentation with both weighted and unweighted regression estimates revealed marked differences in coefficients on early-childhood income in several cases, which led to using the weights in the regression

analyses—a point discussed further in the Results section.

Continuous outcomes (ln earnings, work hours, and completed schooling) were analyzed with ordinary least squares (OLS), measures with substantial concentrations of zeroes (food stamp and AFDC/TANF receipt) were analyzed with Tobit regressions, and dichotomous outcomes (poor health, high distress, ever arrested, ever jailed, and nonmarital birth before age 21) were analyzed with logistic regression. The arrest and incarceration models are only run on males, whereas the AFDC/TANF and nonmarital childbearing models are only run on females. To facilitate their interpretation, logistic regression coefficients and standard errors are expressed in the tables in the form of marginal effects (and their associated standard errors), computed using the MFX command in STATA. These show the change in the probability (as opposed to the odds) of the outcome occurring associated with a unit change in the given independent variable.

## Results

### *Sample Description*

Case counts, means, and standard deviations of each of our outcome variables are provided in Appendix A. These summary statistics are weighted using the weight for the year in which the outcome was measured (high distress, BMI, overweight, arrest, and incarceration), or the most recent weight of the PSID (completed schooling, earnings, work hours, food stamp, and AFDC/TANF receipt and nonmarital childbearing). Descriptive statistics are presented for both the overall sample and for children whose prenatal-to-age-5 incomes averaged: (a) below the official poverty line, (b) between 1 and 2 times the poverty line, and (c) more than twice the poverty line. The final column of Appendices A and B provide information on the statistical significance (at  $p < .05$  or below, two-tailed test) of the mean differences across the three groups.

Appendix A shows striking differences in adult outcomes depending on whether childhood income prior to age 6 was below, close to, or well above the poverty line. Compared with children whose families had incomes of at least twice the poverty line during their early childhood, poor children complete 2 fewer years of schooling, work 451 fewer hours per year, earn less than half as much,

received \$826 per year more in food stamps as adults, and are more than twice as likely to report poor overall health or high levels of psychological distress. Further, poor children have BMIs that are 4 points higher than those well above the poverty line, and are almost 50% more likely to be overweight as adults. Poor males are twice as likely to be arrested and for females, poverty is associated with a \$200 annual increase in cash assistance, and a sixfold increase in the likelihood of bearing a child out of wedlock prior to age 21.

Appendix B reports the weighted descriptive statistics of the childhood period income measures and control variables for the total sample, as well as by poverty status in early childhood. Not surprisingly, children with average annual incomes below poverty in the earliest period have lower average income in all three periods compared with the other two groups. Additionally, the poorest children are less likely to be White and born into an intact family, and more likely to be born in the South, have younger mothers, more siblings, household heads with lower test scores and educational attainment, homes rated as dirtier by interviewers, lower parental expectations, and household heads who report less preference for challenge versus affiliation, less personal control, and less risk avoidance compared with their higher income counterparts.

### *Multivariate Results*

To motivate the estimates from our full regression models, we present in Table 1 standardized coefficient results from a series of descriptive regressions. Across the columns of the first row (Model 1), each outcome is regressed only on a measure of 17-year average childhood income. For ease of calculation of standardized coefficients for our health outcomes, we estimate OLS regressions using continuous measures of general health and distress. Further, to calculate the standardized logistic regression coefficients, we use the following equation from Menard (2004):  $b^* = (b)(s_x)(R) / s_{\text{logit}(\hat{y})}$ , where  $b$  is the logit coefficient,  $s_x$  is the standard deviation of  $x$ ,  $R$  is the square root of the  $R^2$  from the logit regression, and  $s_{\text{logit}(\hat{y})}$  is the standard deviation of the predicted value of  $y$ .

As we express estimates as standardized regression coefficients, entries in the first row amount to simple correlations between income and each of the outcomes. The directions of all of the correlations are as expected—positive for “good” outcomes and negative for “bad” ones—and significant for all

Table 1  
Standardized Regression Coefficients From Various Weighted Models of Childhood Income and Adult Outcomes

	Income measure	Schooling (years)	Annual earnings	Annual work hours	Food stamps	AFDC/TANF	Health status	Distress	BMI	Arrested	Nonmarital birth
Model 1: No controls; 17-year average childhood income	Prenatal to age 15	.34**	.31**	.15**	-.25**	-.13**	-.13**	-.11**	-.15**	-.17*	-.38**
Model 2: Background controls; 17-year average childhood income	Prenatal to age 15	.14**	.23**	.11**	-.08*	-.08†	-.02	-.06	-.02	-.09	-.26**
Model 3: Background controls; natural logarithm of 17-year average childhood income	Prenatal to age 15	.22**	.37**	.20**	-.24**	-.07	-.08*	-.09	-.09†	-.13†	-.18**
Model 4: Background controls; natural logarithm of average stage-specific childhood income	Prenatal to age 5 Ages 6-10 Ages 11-15	.10* -.04 .18**	.27** .06 .08	.20** -.03 .06	-.08† -.12* -.06	-.08 .07 -.07	.07 -.05 -.11†	.00 .14† -.25**	-.04 -.09 .04	-.11 .11 -.17†	.02 .07 -.27**

Note. Ordinary least squares analysis for schooling, earnings, work hours, food stamps, Aid to Families with Dependent Children/Temporary Assistance to Needy Families (AFDC/TANF), health status, distress, and body mass index (BMI); logit analysis for arrests and nonmarital births. Standardized regression coefficients for the logistic regression employ procedures from Menard (2004).  
†p < .10. \*p < .05. \*\*p < .01.

outcomes. The largest correlations, all in the .3-.4 range, are found for schooling, adult earnings, and nonmarital births.

In the second row (Model 2) results, our extensive set of background control variables, all measured either before or right around the time of birth, is added to each of the regressions. All of the coefficients become smaller (in absolute value); in the case of the three health measures and arrests, the coefficient on childhood income drops below the .05 threshold of statistical significance. Thus, a substantial portion of the simple correlation between childhood income and most adult outcomes can be accounted for by the disadvantageous conditions associated with birth into a low-income household.

To assess whether increments to low income may matter more than increments to the incomes of children growing up in middle-class or affluent families, we regressed the adult outcomes on the natural logarithm of the 17-year average childhood income plus background controls. Whereas our first two models assumed that, say, a \$5,000 increment to a poor family's income had the same beneficial effect on a child's adult outcomes as a \$5,000 increment to an affluent family's income, the logarithmic transformation assumes equal percentage effects. So, for example, the logarithmic model presumes that a 50% (and \$10,000) increase in average childhood income from \$20,000 to \$30,000 has the same effect as the 50% (but \$50,000) increase from \$100,000 to \$150,000. Higher standardized coefficients (in absolute value) in logarithmic as opposed to linear models would suggest that dollars matter more for the developmental outcomes of children reared in lower than higher income households.

As shown in the third (Model 3) row of Table 1, the standardized coefficients for the logarithmic models are uniformly higher than coefficients for the linear models (Model 2) in the case of achievement outcomes: completed schooling, earnings, work hours, and food stamps. Health outcomes and AFDC/TANF receipt are little affected by the change, whereas coefficients increase only slightly for arrests and actually fall for nonmarital births. The pattern of results for these adult outcomes is broadly consistent with the conclusions that Duncan and Brooks-Gunn (1997) reached for childhood outcomes: Income appears to be more predictive of achievement-related than either behavior or health outcomes.

To address the issue of the childhood stage-specificity of income effects, the final (Model 4) regressions in Table 1 replace the single 17-year average

log childhood income measure with three stage-specific measures of log income. All background controls are included in these models. With each childhood stage accounting for approximately one third of childhood, we would expect that the three coefficients should (approximately) sum to the all-childhood coefficient presented in Model 3. If childhood income mattered equally across all three stages, the three coefficients should be roughly the same size and about one third the magnitude of the Model 3 coefficients.

In the case of adult earnings and work hours, early-childhood income appears to matter much more than later income. For work hours, the standardized coefficient on prenatal-to-age 5 log average income (.20) is just as large as the Model 3 coefficient on all-childhood log average income, suggesting little role for income beyond age 5. Early income also has a statistically significant coefficient ( $p < .05$ ) in the case of completed schooling, but in this case adolescent income has a considerably larger standardized coefficient. Adult health outcomes and nonmarital births have strong associations with childhood income, but only with childhood income during adolescence.

Taken together, the descriptive regression results shown in Table 1 suggest that both childhood stage and outcome domain matter for understanding links between childhood income and adult success. Moreover, many of the adult outcomes appear to be more sensitive to increments to low as opposed to middle-class or high family incomes.

These lessons are incorporated into our main regression models, shown in Table 2 for achievement outcomes and program participation, Table 3 for health outcomes, and Table 4 for behavioral outcomes. Coefficients and standard errors for the childhood-stage-specific income variables are presented first. For each stage's income, two coefficients are presented, the first reflecting the estimated effect of an additional \$10,000 of annual income in the given stage for children whose income in that stage averaged  $< \$25,000$  and the second reflecting comparable effects for higher income children (all three sets of income variables, plus other controls, are included in all regressions). The column labeled "different slopes" reports results from a statistical test of the null hypothesis of equal within-period slopes. The final row shows results for a test of equality of all three  $< \$25,000$  segment slopes. In contrast to Table 1, coefficients in Tables 2–4 are unstandardized and therefore show changes in originally scaled dependent

variables associated with \$10,000 increments to average childhood stage-specific income.

Table 2 shows that additional income in the prenatal to age 5 period for the lowest-income children is associated with significantly greater adult earnings and work hours, and less food stamp receipt. To ensure that our spline models fit the earnings and work hours data reasonably well, we estimated models with early income represented with a set of dummy variables, and controls for later period incomes (ages 6 through 10 and ages 11 through 15). As shown in Figures 1 and 2, which include 95% confidence bands, the income range in which income responses flattened out was roughly between \$20,000 and \$30,000, thus supporting our use of a knot at \$25,000.

We can illustrate the nature of the income effects of early-childhood income using the natural log earnings regression. The "0.52" coefficient means that, adjusting for income later in childhood and the other control variables listed in Table 2, an additional \$10,000 per year of family income between the prenatal year and the child's fifth birthday is associated with an increase in the natural logarithm of adult earnings of 0.52—or 68.2% ( $e^{.52} = 1.682$ ). In contrast, increments to early-childhood income for higher-income children (i.e., annual average family incomes above \$25,000) are associated with an insignificant 0.05 increment in log earnings. The  $p$  value ( $p < .05$ ) reported in the "different slopes" column indicates that the slope for those with incomes less than \$25,000 per year in early childhood is significantly different from the slope for those with incomes greater than \$25,000 in the same period. Increments to incomes in middle childhood and adolescence are estimated to have nonsignificant impacts on log earnings, even among low-income children. The final row of Table 2 indicates that the three coefficients on the  $< \$25,000$  spline slopes across the childhood stages are significantly different from one another at the  $p = .08$  level.

Results for work hours are broadly similar to those for earnings—a highly significant estimated impact of early childhood but not later childhood income. In this case, a \$10,000 annual increase in the prenatal to age 5 income of low-income families is associated with more than 500 additional work hours per year after age 25. Tobit spline regressions for food stamps for the entire sample suggest that increases in income in all childhood periods for the lowest income children are associated with statistically significant reductions in food stamps. Both the coefficients on food stamps and welfare proved

Table 2  
*Ordinary Least Squares Spline Regression Models of Childhood Income and Completed Schooling, Adult Earnings and Annual Work Hours, and Tobit Spline Models of Childhood Income and Program Participation*

	Years of completed schooling		In Earnings (ages 25–37)		Annual hours worked (ages 25–37)		Annual food stamp (ages 25–37)		Annual AFDC/TANF Females only (ages 25–37)	
	Different slopes	Different slopes	Different slopes	Different slopes	Different slopes	Different slopes	Different slopes	Different slopes	Different slopes	Different slopes
Childhood income (in \$10,000)										
Average annual income (prenatal to age 5)	< \$25K 0.19 (0.33) <i>ns</i>	> \$25K 0.03 (0.04)	0.52* (0.21) $p < .05$	0.05** (0.02)	506.74** (135.39) $p < .001$	20.60* (9.78)	-279.5* (142.08) $p < .10$	-4.34 (14.17)	-157.28 (267.04) <i>ns</i>	12.02 (24.60)
Average annual income (ages 6–10)	< \$25K 0.65** (0.25) $p < .01$	> \$25K -0.06 (0.04)	0.14 (0.14) <i>ns</i>	0.01 (0.02)	-60.82 (109.42) <i>ns</i>	1.28 (8.36)	-380.30** (128.37) $p < .01$	3.91 (11.89)	419.06† (233.44) $p < .10$	-25.91 (27.44)
Average annual income (ages 11–15)	< \$25K -0.31 (0.20) $p < .10$	> \$25K 0.09** (0.03)	0.04 (0.12) <i>ns</i>	0.00 (0.01)	74.18 (83.33) <i>ns</i>	-0.92 (7.56)	-221.71* (101.53) $p < .05$	-6.07 (9.92)	172.35 (202.78) <i>ns</i>	-37.73 (23.59)
Other variables										
Black	0.76** (0.24)		0.10 (0.10)		-79.44 (82.40)		368.48** (78.44)		964.63* (155.28)	
Other minority	0.68 (0.57)		-0.12 (0.35)		-255.48 (219.46)		213.80 (134.61)		-312.59 (378.53)	
Child is male	-0.45** (0.13)		0.53** (0.07)		566.54** (48.12)		-191.19** (52.78)		—	
Child born into intact family	0.63** (0.22)		-0.02 (0.12)		47.66 (80.15)		76.63 (86.08)		-146.97 (172.44)	
Child born in South	-0.16 (0.18)		-0.09 (0.07)		-32.16 (57.65)		-35.53 (58.49)		-511.95** (116.70)	
Age of mother at time of birth	0.04* (0.02)		0.00 (0.01)		1.46 (6.17)		-8.98 (5.85)		-17.56 (12.87)	
Number of siblings	-0.09 (0.06)		0.02 (0.02)		-9.49 (21.89)		20.23 (16.95)		14.67 (35.90)	
Child is firstborn	0.32* (0.15)		0.19* (0.08)		102.51† (58.86)		-79.03 (61.51)		-246.83* (118.84)	
HH head test score (1972)	0.10* (0.04)		-0.01 (0.02)		-8.04 (17.10)		19.06 (15.02)		-33.86 (27.70)	
HH head schooling	0.10** (0.03)		0.02 (0.02)		-6.71 (10.84)		-24.49* (11.75)		17.37 (24.45)	
Observed "dirty" home	-0.09 (0.08)		-0.03 (0.04)		12.19 (26.51)		39.42 (29.48)		-12.67 (54.20)	
Parental expectations	0.14* (0.06)		0.00 (0.03)		15.16 (23.21)		-72.21** (24.41)		-187.05** (47.26)	
Challenge versus affiliation	-0.32 (0.23)		-0.04 (0.11)		-6.66 (81.76)		25.80 (80.99)		-9.63 (162.68)	
Personal control	0.26 (0.22)		0.04 (.12)		-78.53 (86.30)		98.89 (89.62)		227.59 (172.27)	
Risk avoidance	0.16** (0.05)		0.04 (0.03)		10.86 (19.70)		-37.43† (19.95)		24.36 (42.44)	
Birth year dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regression statistics										
R <sup>2</sup>	.29	.24	.24	.24	.24	.24	.03	.03	.04	.04
Number of observations	1,254	1,016	1,016	1,042	1,198	601				
p from test of equality of three < \$25K spline segments	.04	.08	.08	.01	.73	.38				

*Note.* Sample consists of Panel Study of Income Dynamics children born between 1968 and 1975. Incomes are in 2005 dollars and childhood incomes are scaled in \$10,000. Data in the "different slopes" column show  $p$  levels of test of equality of within-period < \$25K and > \$25K slopes. The coefficients and standard errors for the schooling, earnings, and hours are from ordinary least squares (OLS) analysis, and the coefficients and standard errors for food stamps and Aid to Families with Dependent Children/Temporary Assistance to Needy Families (AFDC/TANF) are from Tobit analysis. The AFDC/TANF analysis is only for females. Regressions are weighted and OLS analysis standard errors are corrected for multiple children born in the same household.

† $p < .10$ . \* $p < .05$ . \*\* $p < .01$ .

Table 3  
 Logistic Spline Regression Models of Childhood Income and Poor Health, High Distress, and Overweight; Ordinary Least Squares Spline Regression Model of Childhood Income and Body Mass Index

	Poor health		High distress (2003)		BMI (2005)		Overweight (BMI > 25 in 2005)	
	Different slopes	Different slopes	Different slopes	Different slopes	Different slopes	Different slopes	Different slopes	Different slopes
Childhood income (in \$10,000)								
Average annual income (prenatal to age 5)	< \$25K -0.009 (0.027) > \$25K 0.003 (0.003)	<i>ns</i>	0.011 (0.021) -0.002 (0.003)	<i>ns</i>	-3.51 (2.45) 0.04 (0.16)	<i>ns</i>	0.028 (0.113) 0.013 (0.014)	<i>ns</i>
Average annual income (ages 6-10)	< \$25K -0.008 (0.027) > \$25K 0.007† (0.004)	<i>ns</i>	0.013 (0.016) 0.006* (0.002)	<i>ns</i>	0.77 (1.51) -0.12 (0.09)	<i>ns</i>	-0.130 (0.107) -0.020* (0.010)	<i>ns</i>
Average annual income (ages 11-15)	< \$25K 0.001 (0.028) > \$25K -0.017** (0.003)	<i>ns</i>	-0.026 (0.021) -0.006* (0.002)	<i>ns</i>	0.34 (1.41) 0.06 (0.09)	<i>ns</i>	0.128 (0.097) 0.010 (0.008)	<i>ns</i>
Other variables								
Black	0.025 (0.025)		-0.024* (0.014)		0.36 (0.83)		-0.039 (0.089)	
Other minority	-0.009 (0.042)		—		-0.77 (0.87)		-0.014 (0.153)	
Child is male	0.014 (0.014)		-0.024† (0.014)		1.73** (0.47)		0.278** (0.043)	
Child born into intact family	0.014 (0.016)		-0.009 (0.021)		-0.09 (0.94)		0.059 (0.093)	
Child born in South	0.007 (0.015)		0.011 (0.013)		-0.20 (0.55)		-0.062 (0.053)	
Age of mother at time of birth	0.001 (0.001)		0.000 (0.001)		0.02 (0.06)		-0.009† (0.006)	
Number of siblings	-0.006 (0.004)		0.002 (0.003)		0.13 (0.18)		0.015 (0.018)	
Child is firstborn	-0.024† (0.014)		0.003 (0.010)		0.65 (0.49)		0.020 (0.052)	
Household head test score (1972)	0.000 (0.004)		-0.007* (0.003)		-0.17 (0.14)		-0.023† (0.013)	
Household head schooling (1972)	-0.004 (0.002)		-0.001 (0.002)		-0.16 (0.11)		-0.018† (0.010)	
Observed "dirty" home	0.000 (0.007)		-0.004 (0.005)		0.59* (0.29)		0.038 (0.028)	
Parental expectations	-0.007 (0.006)		-0.002 (0.005)		-0.16 (0.26)		0.004 (0.023)	
Challenge versus affiliation	0.023 (0.023)		0.027 (0.020)		-0.41 (0.71)		-0.012 (0.078)	
Personal control	0.002 (0.023)		-0.011 (0.015)		0.10 (0.86)		-0.014 (0.081)	
Risk avoidance	-0.004 (0.005)		0.003 (0.002)		-0.12 (0.19)		-0.017 (0.017)	
Birth year dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regression statistics								
R <sup>2</sup> (or pseudo)	.14	.22	.11	.10	.11	.10	.10	.10
Number of observations	1,137	529	875	875	875	875	875	875
p level of test of equality for the three < \$25K spline segments	.96	.42	.36	.39	.36	.39	.39	.39

Note. Sample consists of Panel Study of Income Dynamics children born between 1968 and 1975. Incomes are in 2005 dollars, and childhood incomes are scaled in \$10,000. Data in the "different slopes" column show p levels of test of equality of within-period < \$25K and > \$25K slopes. Marginal effects from logistic spline regressions presented for poor health, high distress, and overweight. Poor health regression includes year-of-report dummy variables. Regressions are weighted and standard errors are corrected for multiple children born in the same household. Logistic regression models present marginal effects and standard errors. Marginal effects are interpreted as probabilities and are computed using the MFX command in STATA. BMI = body mass index.

†p < .10. \*p < .05. \*\*p < .01.

Table 4  
*Logistic Spline Regression Models of Childhood Income and Arrests, Jailed, and Nonmarital Births*

	Ever arrested (males)		Ever jailed (males)		Nonmarital birth (females)	
	Different slopes	Different slopes	Different slopes	Different slopes	Different slopes	Different slopes
Childhood income (in \$10,000)						
Average annual income (prenatal to age 5)	< \$25K 0.013 (0.055)	<i>ns</i>	0.017 (0.028)	<i>ns</i>	0.049 (0.046)	<i>ns</i>
	> \$25K -0.018 <sup>†</sup> (0.011)		0.001 (0.06)		0.002 (0.012)	
Average annual income (ages 6–10)	< \$25K -0.024 (0.061)	<i>ns</i>	-0.004 (0.027)	<i>ns</i>	0.013 (0.045)	<i>ns</i>
	> \$25K 0.016 <sup>†</sup> (0.009)		-0.001 (0.004)		0.000 (0.009)	
Average annual income (ages 11–15)	< \$25K -0.015 (0.050)	<i>ns</i>	0.000 (0.021)	<i>ns</i>	-0.035 (0.042)	<i>ns</i>
	> \$25K -0.013 <sup>†</sup> (0.007)		-0.009** (0.004)		-0.021** (0.008)	
Other variables						
Black	-0.052 <sup>†</sup> (0.032)		-0.013 (0.017)		0.135** (0.049)	
Other minority	-0.039 (.079)		-0.012 (0.044)		0.052 (0.110)	
Child born into intact family	-0.085 (0.054)		-0.041 (0.031)		-0.012 (0.034)	
Child born in South	-0.020 (0.033)		-0.015 (0.018)		-0.048* (0.024)	
Age of mother at time of birth	-0.005 (0.004)		-0.005* (0.002)		-0.007* (0.003)	
Number of siblings	0.020* (0.010)		0.004 (0.007)		0.020* (0.009)	
Child is firstborn	0.015 (0.036)		-0.014 (0.016)		-0.006 (0.029)	
Household head test score (1972)	-0.008 (0.008)		-0.002 (0.004)		-0.009 (0.006)	
Household head schooling	0.002 (0.07)		0.004 (0.004)		-0.009 <sup>†</sup> (0.005)	
Observed "dirty" home	-0.042* (0.021)		-0.015 (0.010)		-0.015 (0.013)	
Parental expectations	0.030* (0.015)		-0.003 (0.018)		-0.020 <sup>†</sup> (0.011)	
Challenge versus affiliation	0.076 (0.050)		0.020 (0.023)		0.044 (0.037)	
Personal control	0.015 (0.045)		-0.002 (0.023)		-0.093* (0.046)	
Risk avoidance	-0.036** (0.011)		-0.016 <sup>†</sup> (0.007)		0.004 (0.009)	
Birth year dummies?	Yes		Yes		Yes	
Regression statistics						
Pseudo R <sup>2</sup>	.13		.18		.29	
Number of observations	744		744		778	
p level of test of equality for the three < \$25K spline segments	.90		.87		.41	

*Note.* Sample consists of Panel Study of Income Dynamics children born between 1968 and 1975. Incomes are in 2005 dollars. Childhood incomes are scaled in \$10,000. Data in the "different slopes" column show *p* levels of test of equality of within-period < \$25K and > \$25K slopes. Regressions are weighted and standard errors are corrected for multiple children born in the same household. Logistic regression models present marginal effects and standard errors. Marginal effects are interpreted as probabilities and are computed using the MFX command in STATA.  
<sup>†</sup>*p* < .10. \**p* < .05. \*\**p* < .01.

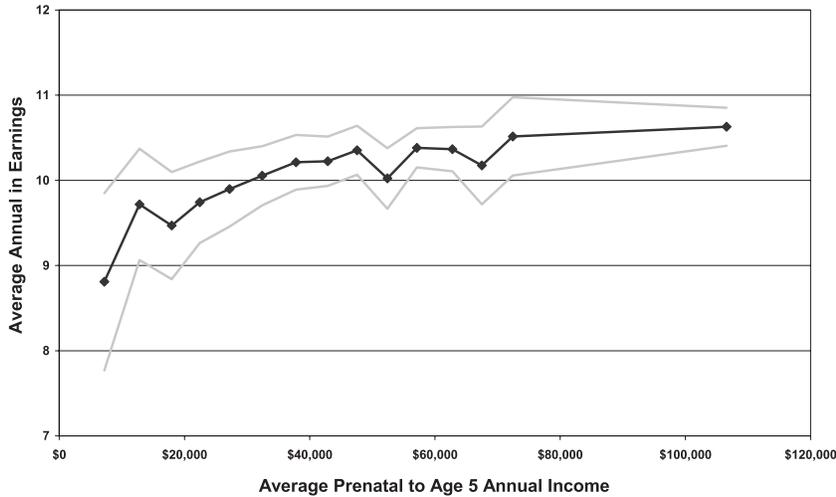


Figure 1. Adult In earnings by early-childhood income, with 95% confidence intervals.

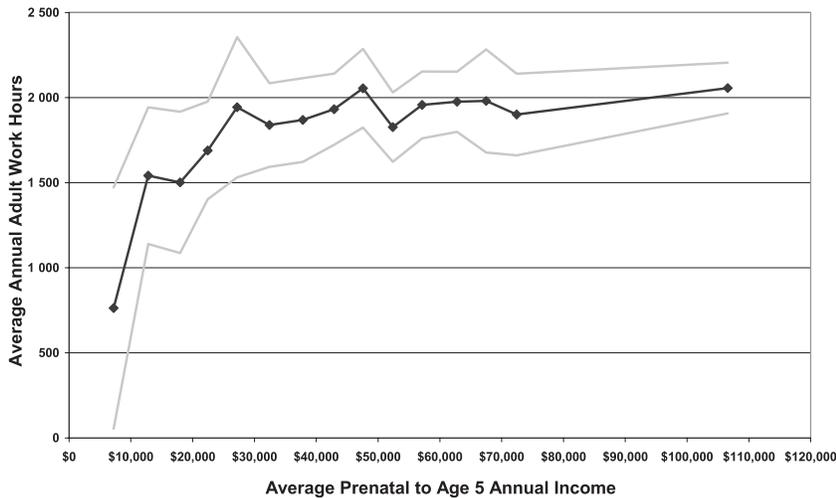


Figure 2. Adult work hours by early-childhood income, with 95% confidence intervals.

somewhat sensitive to outliers, resulting in a decision to truncate both dependent variables at the 99th percentiles of their respective distributions—\$4,913 in the case of annual food stamp receipt and \$2,954 in the case of annual AFDC/TANF receipt. Additionally, when separate food stamp models for males and females were conducted, we found a much larger and statistically different effect of early poverty for females than for males. The respective coefficients and standard errors for females were  $-\$483$  (268) and for males were  $-\$260$  (125). The female coefficient has a  $p = .06$ ; the male coefficient has a  $p = .04$ .

The coefficient on early income in the schooling regression is 0.19 years, which is not statistically

significant at conventional levels ( $p = .569$ ). The lack of a significant regression-adjusted association between early income and schooling is at odds with results presented in Duncan et al. (1998). The present analysis includes three different birth cohorts than Duncan et al. and the early income–schooling relation appears somewhat weaker for the new cohorts. A more interesting difference is that Duncan et al. assessed completed schooling by age 20—a kind of “on time” schooling measure. We tested several alternative schooling specifications, including completed schooling by age 21, on-time high school graduation (by age 18 or 19), had dropped out of high school as of age 21, and had attended some college as of age 21. In the case of

completed schooling by age 21, we found that the coefficient on early income was 0.338 and statistically significant ( $p < .05$ ). Further, when examining a specification that includes four time periods (prenatal through ages 2, ages 3 through 5, ages 6 through 10, and ages 11 through 15), the coefficient on age 3 through 5 was significant both for completed schooling by age 21 (0.399) and for on-time high school graduation (marginal probability effect from a logistic regression of 0.182). So it appears that early income may matter more for the on-time completion of schooling by the end of adolescent than for the sporadic increases in schooling that often occur later.

Results for the control variables in the completed schooling regression mirror past research, with Black people (adjusted for socioeconomic status and other controls), females, children born first, into intact or smaller families, or born to older mothers, more educated parents, greater household head expectations at birth or greater household head risk avoidance, obtaining more schooling. Few of these controls have persistently significant coefficients across all of the attainment-related outcomes in Table 2.

The marginal (probability change) effects from logistic spline models for poor health, high distress and overweight, and OLS results for BMI are shown in Table 3 and show scattered income effects in middle childhood and adolescence, but not early childhood. In no case are increments to low income in later childhood stages estimated to have a statistically significant impact on any of the health outcomes.

Table 4 presents marginal effects for the behavioral outcomes for men (arrests and jail) and women (nonmarital childbearing). Here again, the early-childhood income segments are not statistically significant in any of these models. As in the case of the health, increments to low income in middle childhood and adolescence are never estimated to have a statistically significant impact on any of the behavioral outcomes.

#### *Extensions to Main Analysis*

We explored the robustness of our results in various ways, first by testing for sex and race interactions. We found no statistically significant interactions by sex. For the race interactions, the incarceration of Black men appeared to be significantly less sensitive to increments in early-childhood income than the incarceration of White men. Specifically, the coefficient, expressed as a marginal

effect on the probability of occurrence, on White incarceration was 0.38 ( $SE = 0.16$ ) and the Black difference was  $-0.36$  ( $SE = 0.16$ ).

In averaging early-childhood income over the 7-year interval between the prenatal year and fifth birthday, there is a danger that we are missing a narrower sensitive period of income effects. In Table 5, we present results from regressions that are identical to those in Tables 2–4 except that the prenatal to age 5 period is further divided into two segments—the prenatal and birth years and ages 1 through 5. A comparison of coefficients shows that point estimates of income effects for four of the five attainment-related outcomes are all larger when income is measured between ages 1 and 5 than earlier, although only in the case of completed schooling is the difference statistically significant.

The opposite is true of body mass—very early income appears to matter more than income after the birth year, a result explored in some depth in Ziol-Guest, Duncan, and Kalil (2009). This finding of the particular importance of income during the prenatal and birth years for adult BMI may support the “fetal origins” hypothesis.

A more technical concern involving the income measure is that it is pretax total family income. An after-tax measure might better capture a family’s spending opportunities and constraints. We re-estimated our models using childhood income measures from which PSID-calculated federal income taxes had been subtracted. The results proved somewhat sensitive to outliers but were generally quite similar to those shown earlier.

Table 6 explores the robustness of the prenatal-to-age-5 low-income coefficients in two additional models. The coefficients and standard errors in the first column are identical to those presented in Tables 2–4. In the second column, we retain all of the control variables, but in this case maternal earnings have been subtracted from all three stage-specific income variables. This helps to address the problem that mother’s earnings are a component of childhood income. Mothers’ labor supply decisions have implications for the amount of time mothers can spend with their children and may be affected by how successful the child’s development is viewed by the family. For earnings and annual work hours for which early income effects were statistically significant in Table 2, the new income coefficients are smaller but retain statistical significance.

Stage-specific income results presented in Carneiro and Heckman (2003, p. 120) employ a somewhat different specification than ours in which

Table 5

Coefficients and Standard Errors From Models in Which Prenatal to Age 5 Income Is Divided Into (a) Prenatal and Birth Years and (b) Ages 1–5

	Coefficients and standard errors on low-income < \$25K spline segment				Different slopes
	Average annual income, prenatal to birth (< 25K)		Average annual income, ages 1–5 (< 25K)		
Completed schooling	–0.46	(0.30)	0.63†	(0.38)	$p < .05$
ln Earnings	0.17	(0.18)	0.32	(0.21)	<i>ns</i>
Annual hours worked	188.70	(131.00)	317.60*	(129.23)	<i>ns</i>
Annual food stamp receipt	–276.59*	(137.75)	–160.59	(133.63)	<i>ns</i>
Annual AFDC/TANF receipt (females)	–100.68	(22.58)	–425.68*	(220.25)	<i>ns</i>
Poor health	0.01	(0.03)	–0.03	(0.03)	<i>ns</i>
High distress	0.01	(0.04)	0.00	(0.02)	<i>ns</i>
Body mass index	–2.16*	(1.08)	–1.45	(2.11)	<i>ns</i>
Overweight	–0.11	(0.10)	0.10	(0.11)	<i>ns</i>
Arrested (males)	0.02	(0.06)	–0.03	(0.06)	<i>ns</i>
Incarcerated (males)	0.02	(0.03)	–0.01	(0.03)	<i>ns</i>
Nonmarital childbearing (females)	0.00	(0.05)	0.08†	(0.04)	<i>ns</i>

Note. Sample consists of Panel Study of Income Dynamics children born between 1968 and 1975. Incomes are in 2005 dollars and childhood incomes are scaled in \$10,000. Coefficient and standard errors from regressions that contain all control variables and four spline segments: prenatal through birth year, ages 1 through 5, ages 6 through 10, and ages 11 through 15. Different slopes indicates whether the low-income segment between prenatal and birth is significantly different from between ages 1 and 5. Ordinary least squares coefficients shown for completed schooling, earnings, annual hours worked, and body mass index; Tobit coefficients shown for food stamps and Aid to Families with Dependent Children/Temporary Assistance to Needy Families (AFDC/TANF); and marginal effects shown for the dichotomous outcomes. Marginal effects are interpreted as probabilities and are computed using the MFX command in STATA.

† $p < .10$ . \* $p < .05$ .

Table 6

Coefficients and Standard Errors on Average Annual Income Prenatal to Age 5 &lt; 25K for Various Model Specifications

	Coefficients and standard error on low-income (< \$25K) spline segment				Controls for permanent (prenatal to age 15) income			
	Basic regression		Exclude maternal earnings from all childhood income measures		Coefficient on low-income permanent income spline segment		Coefficient on low-income prenatal-to-age-5 spline segment	
Completed schooling	0.19	(0.33)	–0.25	(0.22)	0.81*	(0.36)	–0.11	(0.37)
ln Earnings	0.52*	(0.21)	0.36**	(0.11)	0.33	(0.20)	0.39†	(0.22)
Annual hours worked	506.74**	(135.39)	220.65**	(78.04)	63.55	(124.30)	446.56**	(146.96)
Annual food stamp receipt	–303.14*	(152.09)	–102.12	(87.98)	–1152.47**	(187.07)	149.98	(195.37)
Annual AFDC/TANF receipt (females only)	–419.18	(264.28)	–90.34	(162.55)	943.17**	(340.79)	–570.57	(387.58)
Poor health	–0.01	(0.03)	0.012	(0.015)	0.014	(0.031)	–0.032	(0.0338)
High distress	0.01	(0.012)	0.001	(0.009)	–0.031	(0.030)	0.039	(0.043)
Body mass index	–3.51	(2.45)	–1.57	(1.03)	2.34	(1.47)	–4.62†	(2.62)
Overweight	0.03	(0.11)	0.038	(0.063)	0.058	(0.138)	–0.060	(0.140)
Arrested	0.01	(0.05)	–0.004	(0.041)	–0.008	(0.080)	–0.006	(0.071)
Incarcerated	0.02	(0.03)	0.014	(0.020)	0.012	(0.031)	0.006	(0.029)
Nonmarital childbearing	0.05	(0.05)	0.038	(0.029)	–0.082†	(0.044)	0.107†	(0.058)

Note. Marginal effects reported for the dichotomous outcomes. AFDC/TANF = Aid to Families with Dependent Children/Temporary Assistance to Needy Families.

† $p < .10$ . \* $p < .05$ . \*\* $p < .01$ .

childhood income is characterized by permanent (i.e., all-childhood-year average) income and just the early-childhood component. In this case, the coefficient on the early income component shows the coefficient *difference* from permanent income. As shown in the last pair of columns in Table 6, we find that early income is significantly more predictive of earnings and work hours than is permanent childhood income.

One concern in these types of analyses is omitted variable bias, and one approach to deal with this concern is to conduct sibling difference models that capitalize on within-family variation (Johnson & Schoeni, 2007). We tested these models for the earnings and annual work hours regressions. Because of small sample sizes, precision is a problem. For earnings, the coefficient (standard error) on the lower income spline segment was 0.30 (1.08) compared to 0.52 (0.21) in the full sample and the higher income spline was 0.07 (0.09) compared to 0.14 (0.14). For annual work hours, the coefficient and standard error on the lower income spline was 1047 (969) compared to 507 (135) in the full analysis; whereas the higher income early-childhood coefficient in the sibling fixed effects model was -8 (78) compared to 20 (10) in the analysis presented in Table 2.

Given the interesting results for the log earnings models, we sought to explore mediational pathways by sequentially introducing the sets of predictors listed in the columns of Table 7. Controls for childhood maternal work hours and family compo-

sition had little impact on the key early-income coefficient. Because controls for work hours and family structure are not so much mediators as additional childhood controls that might be biasing our income estimates, we have not included them in our basic models owing to their potential endogeneity. Completed schooling accounts for very little of the income effect, which is hardly surprising given the weak links between early income and schooling (see Appendix A). The same explanation probably underlies the lack of a mediational role for the behavior and health measures. On the other hand, the inclusion of adult work hours reduces the early-income coefficient by 80%, which suggests that much of the earnings impact of early-childhood income operates through annual work hours rather than wage rate.

The importance of work hours in the link between early-childhood income and annual adult earnings suggests that childhood income should have a relatively small effect on *hourly* earnings in adulthood. When we used average hourly earnings after age 25 as a dependent variable in our standard full-control regression model, the coefficient on early-childhood income was small and statistically insignificant. We investigated the work hours effects further by estimating whether early-childhood income appeared to operate by reducing unemployment or time out of the labor force altogether as opposed to full versus part-time work. For both men and women, the income effects were the strongest for full versus less-than-full time work.

Table 7  
Accounting for the Effects of Prenatal to Age 5 Average Annual Income on Log Earnings

	Coefficients and standard error on low-income < \$25K spline segment in ln earnings regression					
	Basic model	Add childhood conditions	Add schooling	Add adult behavior	Add adult health	Add adult work hours
Average annual income < \$25K (prenatal to age 5)	0.52* (0.21)	0.64** (0.22)	0.60** (0.21)	0.64** (0.21)	0.59** (0.20)	0.10 (0.14)
Average annual income (ages 6–10 and 11–15)	incl.	incl.	incl.	incl.	incl.	incl.
Other background controls	incl.	incl.	incl.	incl.	incl.	incl.
Prenatal to age 15 maternal work hours and family structure		incl.	incl.	incl.	incl.	incl.
Completed schooling			incl.	incl.	incl.	incl.
Arrests, jail, and out-of-wedlock childbearing				incl.	incl.	incl.
Adult health					incl.	incl.
Work hours						incl.
Regression statistics						
$R^2$	.24	.25	.33	.34	.35	.61
Number of observations	1,016	1,016	1,016	1,016	1,016	1,016

\* $p < .05$ . \*\* $p < .01$ .

### Conclusion

Our exploration of the role of economic deprivation early in childhood produced surprisingly strong associations in the case of two important adult attainments—earnings and work hours. These results complement those reported in Duncan and Brooks-Gunn (1997), who found stronger correlations between early-childhood income and childhood achievement than between early income and either childhood behavior or health.

One way of assessing the policy significance of our coefficients is to express our results in income increments that have been associated with past policy changes. With the EITC transferring as much as \$4,800 to working families, we used \$3,000 as a possible increment. The coefficients imply that a \$3,000 annual increase in income between a child's prenatal year and fifth birthday is associated with 19% higher earnings and a 135-hr increase in work hours. Moreover, most of the childhood-income effects on earnings appear to be accounted for by differences in annual work hours, suggesting that the results for work hours are particularly important to understand. Apart from studies looking at adult "idleness" (Haveman & Wolfe, 1994), there is little work on the childhood determinants of full-time work.

One important policy issue is the persistence of these effects across adulthood. A supplemental earnings regression that included an interaction between early-childhood income and age at which adult earnings were measured (not shown in the tables) indicated that the 19% proportionate impact of early-childhood poverty did not change over the ages for which we are able to track the sample (ages 25–37), so we can safely assume that the childhood income impact persists for at least that 13-year period.

We end with the usual cautions regarding the causal interpretation of our income "effects." Among the many approaches to estimating the causal impacts of childhood income on adult outcomes, ours is more sophisticated than some but less sophisticated than others. We are the first study to link high-quality income data across the entire childhood period with adult outcomes measured as late as age 37.

The incomes we observe are determined, in part, by the actions of parents and other family members, which leave our analyses open to omitted-variable bias. Our list of variables controlling for conditions at the time of birth is extensive and ought to reduce a good deal of potential bias. More important, and unusual for studies such as ours, is that our estimates of the impacts of early-childhood

income control for income in middle childhood and adolescence. As such, factors such as genetic influences are as likely to affect later and early-childhood income, and thus are controlled, in some degree, by our inclusion of income in other childhood stages. Nevertheless, the possibility of lingering omitted-variable bias remains.

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## Appendix A

### Weighted Descriptive Statistics of Adult Outcomes by Prenatal to Age 5 Poverty

	Total sample		Income below the official poverty line (A)		Income between 1 and 2 times the poverty line (B)		Income more than twice the poverty line (C)		Significant difference
	M or %	SD	M or %	SD	M or %	SD	M or %	SD	
Completed schooling	13.39	2.14	11.83	1.83	12.70	2.02	13.96	2.01	A < B,C; B < C
Unweighted <i>n</i>	1,254								
Earnings (\$10,000)	34.56	30.93	17.86	16.00	26.79	21.17	39.67	33.96	A < B,C; B < C
Unweighted <i>n</i>	1,042								
Annual work hours	1,892.46	699.30	1,512.37	750.25	1,839.12	778.77	1,963.08	641.63	A < B,C; B < C
Unweighted <i>n</i>	1,042								
Food stamps	213.75	730.30	896.28	1,453.87	337.28	902.25	70.09	337.43	A > B,C; B > C

Appendix A  
Continued

	Total sample		Income below the official poverty line (A)		Income between 1 and 2 times the poverty line (B)		Income more than twice the poverty line (C)		Significant difference
	M or %	SD	M or %	SD	M or %	SD	M or %	SD	
	Unweighted <i>n</i>	1,157							
AFDC/TANF (women only)	158.72	750.60	285.63	804.07	380.61	1,231.28	46.14	344.06	A,B > C
Unweighted <i>n</i>	574								
Poor health	0.08	—	0.13	—	0.13	—	0.05	—	A,B > C
Unweighted <i>n</i>	1,167								
High distress	0.04	—	0.10	—	0.04	—	0.04	—	<i>ns</i>
Unweighted <i>n</i>	667								
Body mass index	27.46	5.87	30.63	7.27	28.05	6.70	26.86	5.22	A > B,C; B > C
Unweighted <i>n</i>	875								
Overweight	0.61	—	0.79	—	0.61	—	0.59	—	A > B,C
Unweighted <i>n</i>	875								
Arrested (men only)	0.16	—	0.26	—	0.21	—	0.13	—	A,B > C
Unweighted <i>n</i>	757								
Incarcerated (men only)	0.09	—	0.10	—	0.12	—	0.07	—	B > C
Unweighted <i>n</i>	757								
Nonmarital birth (women only)	0.20	—	0.50	—	0.28	—	0.09	—	A > B,C; B > C
Unweighted <i>n</i>	778								

*Note.* In the “significant difference” column, A < B signifies that the mean of those less than the poverty line is statistically significantly smaller than those between 1 and 2 times the poverty line at  $p < .05$  (two-tailed). A > B signifies that the mean of those less than the poverty line is statistically significantly larger than those between 1 and 2 times the poverty line at  $p < .05$  (two-tailed). Earnings, food stamp value, and Aid to Families with Dependent Children/Temporary Assistance to Needy Families (AFDC/TANF) value in 2005 dollars.

## Appendix B

## Weighted Descriptive Statistics of Control Variables by Prenatal to Age 5 Poverty

	Total sample		Income below the official poverty line (A)		Income between 1 and 2 times the poverty line (B)		Income more than twice the poverty line (C)		Significant difference
	M or %	SD	M or %	SD	M or %	SD	M or %	SD	
	Prenatal to age 5, average annual income (2005\$)	47,842.48	28,340.64	21,400.11	9,714.14	36,024.01	12,242.22	65,442.38	
Ages 6–10, average annual income (2005\$)	54,226.12	39,012.65	23,112.91	16,197.13	41,956.34	23,148.25	73,881.51	42,250.67	A < B,C; B < C
Ages 11–15, average annual income (2005\$)	59,067.89	45,369.20	24,743.16	20,140.42	44,137.52	27,222.75	81,649.59	49,337.59	A < B,C; B < C
White	0.77	—	0.38	—	0.70	—	0.88	—	A < B,C; B < C
Black	0.19	—	0.58	—	0.26	—	0.09	—	A > B,C; B > C
Other minority	0.04	—	0.04	—	0.04	—	0.04	—	<i>ns</i>
Male	0.53	—	0.52	—	0.52	—	0.54	—	<i>ns</i>
Born into intact family	0.84	—	0.44	—	0.78	—	0.94	—	A < B,C; B < C
Born in the South	0.32	—	0.51	—	0.39	—	0.26	—	A > B,C; B > C
Age of mother at birth	24.84	5.76	24.04	7.23	24.13	6.29	25.29	5.15	A,B < C
Number of siblings	2.21	1.79	4.00	3.07	2.56	1.73	1.74	1.16	A > B,C; B > C

Appendix B  
Continued

	Total sample		Income below the official poverty line (A)		Income between 1 and 2 times the poverty line (B)		Income more than twice the poverty line (C)		Significant difference
	<i>M</i> or %	<i>SD</i>	<i>M</i> or %	<i>SD</i>	<i>M</i> or %	<i>SD</i>	<i>M</i> or %	<i>SD</i>	
Child is firstborn	0.42	—	0.30	—	0.33	—	0.47	—	A, B < C
Household head test score	9.57	2.11	7.71	2.40	9.11	1.99	10.10	1.86	A < B,C; B < C
Household head schooling	12.09	2.94	9.20	3.21	10.78	2.61	13.16	2.39	A < B,C; B < C
Observed “dirty” home	2.18	1.09	2.85	1.09	2.38	1.09	1.80	0.91	A > B,C; B > C
Parental expectations	4.43	1.17	4.09	1.13	4.30	1.20	4.64	1.12	A < B,C; B < C
Challenge versus affiliation	0.66	0.34	0.49	0.33	0.64	0.33	0.74	0.32	A < B,C; B < C
Personal control	0.60	0.35	0.42	0.34	0.55	0.33	0.70	0.33	A < B,C; B < C
Risk avoidance	4.36	1.63	3.34	1.23	3.87	1.45	5.05	1.56	A < B,C; B < C
Unweighted <i>n</i>	1,589		297		506		786		

*Note.* In the “significant difference” column, A < B signifies that the mean of those less than the poverty line is statistically significantly smaller than those between 1 and 2 times the poverty line at  $p < .05$  (two-tailed). A > B signifies that the mean of those less than the poverty line is statistically significantly larger than those between 1 and 2 times the poverty line at  $p < .05$  (two-tailed).