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Early Childhood Poverty and Adult Attainment, Behavior and Health[1]

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Abstract

Our paper assesses the consequences of poverty between a child's prenatal year and fifth birthday for a host of adult achievement, health, and behavior outcomes, measured as late as age 37. Using data from the Panel Study of Income Dynamics and controlling for economic conditions in middle childhood and adolescence, we find statistically significant and, in some cases, quantitatively large detrimental effects of early poverty on a number of attainment-related outcomes (adult earnings, work hours, receipt of transfer income), some health outcomes (adult body mass) but not on such behavioral outcomes as out of wedlock childbearing and arrests. Most of the adult earnings effects appear to operate through adult labor supply.

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Early Childhood Poverty and Adult Attainment, Behavior and Health

Some 4.1 million infants, toddlers and preschoolers lived in poverty in the United States in 2005. For a family of three, this meant that total income was less than $15,577; many poor families had income well below that amount.[2] 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 reduce material investments in children's learning and development, as well as interfere with the development of strong parent-child bonds and supportive parenting practices. Such a lack of material and emotional resources in the family environment can compound and amplify the neurobiological disadvantages that many poor children already face.

The sensitivity of early childhood to environmental influences has been demonstrated in a wide range of infant, toddler and preschooler intervention studies. With regard to economic deprivation, ample research shows 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 (Holzer, Schanzenbach, Duncan, and Ludwig, 2007). Despite these associations, it is far from clear to what extent poverty *itself* is the cause of these differences. Extending the work of Duncan, Brooks-Gunn, Yeung, & Smith (1998), we use the most recent data from the Panel Study of Income Dynamics to examine the long-run (i.e., as late as age 37) impacts of low income early in life, net of income later in childhood and other correlated family factors.

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 (Shonkoff & Phillips, 2000). How poverty early in childhood might affect these structures has been a matter of considerably research.

The potential role played by family socioeconomic status in determining child cognitive ability has been demonstrated in several quasi-experimental studies. Duyme et al. (1999) compared the pre- vs. post-adoption IQs of children adopted (with no evidence to suggest differential selection) into low-, middle- and high-SES families. All of the adopted children had low IQs (in the 60-86 range) prior to adoption and were adopted between ages 4 and 6. IQ prior to adoption was independent of placement-family SES. However, post-adoption IQ growth was strikingly different by SES (defined by father's occupation), with the gains associated with adoption into high- and middle-SES families much larger than the IQ gains for children adopted into low-SES families. Whereas Duyme et al. (1999) are unable to distinguish whether income or some correlated aspect of the family or community environments of higher SES families was responsible for the differential IQ gains, Plug and Vivjerberg (2005) show that genetically unrelated adoptees who have better access to financial resources show improved educational achievement and that this finding is robust to a range of sensitivity tests to tackle this bias.

The chapters in Duncan and Brooks-Gunn (1997) take a more general look at the possible consequences of childhood poverty. Twelve groups of researchers working with 10 different non-experimental but longitudinal data sets estimate longitudinal models of income effects on child well-being. On the whole, the results suggest that family income has substantial, albeit selective associations with children's attainments. First, family income had much larger associations with measures of children's 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 economic conditions during adolescence. And third, the association between income and achievement appeared to be non-linear, with the biggest impacts at the lowest levels of income.

Why the timing of income may matter

Cunha, Heckman, Lochman, and Masterov (2005) propose an economic model of development in which preschool cognitive and socio-emotional capacities are key ingredients for human capital acquisition during the school years. In their model, "skill begets skill" and early capacities can affect the productivity of school-age human capital investments. In this model, economic deprivation in early childhood could create disparities in school readiness and early academic success that persist or widen over the course of childhood. Much of the economics literature ignores 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 psychologists stress the importance of understanding children's distinct developmental stages, the transitions from one stage to the next, and the conditions prevailing during the various stages and transitions (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 economic conditions 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).

Finally, the "fetal origins" 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 and Barker, 2000; Hertzman, 1999).

Why poverty may matter

Economic models of child development (e.g., Becker, 1981) view families with higher economic resources as being better able to purchase or produce important "inputs" into their young children's development (e.g., nutritious meals; enriched home learning environments and childcare settings outside the home; safe and stimulating neighborhood environments), and, with older children, higher-quality schools and university education. The degree to which these inputs are purchased is presumed to vary with their cost, the family's household income, and parents' preferences for purchases that meet their own versus their children's needs.

Psychologists emphasize that higher incomes may improve family psychological processes such as parental emotional well-being and parenting (Chase-Lansdale & Pittman, 2002; McLoyd, 1990; McLoyd, Jayartne, Ceballo, & Borquez, 1994). A long line of research (reviewed in McLoyd, 1990) has found that low- as opposed to middle-income parents are more likely to use an authoritarian and punitive parenting style and less likely to provide them with stimulating learning experiences in the home. Poverty and economic insecurity take a toll on a parent's mental health, and this may be an important cause of low-income parents' non-supportive parenting. As described by Zahn-Waxler, Duggal, and Gruber (2002), depression and other forms of psychological distress can profoundly affect parents' interactions with their children.

Epidemiologists have suggested that the early years represent a sensitive period during which social processes become embedded in biology. As such, epigenetic modifications could be responsible for associations between early income and later life outcomes (Weaver et al. 2004). For example, low income is associated with exposure to some types of early life stressors, such as physical or emotional abuse; exposure to these types of trauma can result in physiological changes known to affect adult health characteristics such as body mass (Gunstad et al. 2006).

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 two-fold: 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, are 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 from a model of income effects that measures income concurrently with the child outcomes risks bias if income is volatile across childhood. Since there is abundant evidence that income is indeed volatile (Duncan et al., 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, since there is an abundance of factors that might simultaneously influence family income and child well-being. 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. Mayer (1997) tests for omitted-variable bias and finds large reductions in the estimated impact of parental income, leading her to conclude that much of the estimated effect of income in the literature is spurious. Blau (1999) estimates a number of models relating income and other aspects of parental family background to children's ability and achievement test scores as well as behavior problems. He finds small and insignificant effects of current income and larger (although still modest) effects of long-run income, although he does not investigate whether income effects differ by child age.

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 take 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 -- $4,400 – and it increased by about $2,100 in the middle 1990s. Dahl and Lochner estimate 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 the suggestion that correlations between income and child outcomes are non-linear, with bigger effects at lower income levels (Duncan et al., 1998; Shea, 2000; Votruba-Drzal, 2003).

A series of experimental welfare reform evaluation studies was undertaken in the United States and Canada during the 1990s (Morris, Huston, Duncan, Crosby, & Bos, 2001). An analysis of these data by Morris et al. (2001) revealed that welfare reforms that both increased work and provided financial supports for working families generally promoted children's achievement and positive behavior, although children's achievement appeared to improve more than their behavior. In contrast, welfare reforms that mandated work but did not support it financially had few impacts -- positive or negative -- on children. Thus, it appeared that merely increasing maternal employment had no impact on children's achievement, but increasing both work and income had a positive effect. Morris and Gennetian (2003) analysis of these data showed that a $1,000 increase in average annual income is associated with a .20 standard deviation increase in school achievement and a similarly-sized decrease in behavior problems, as well as .30 standard deviation increases in school engagement and positive behavior. Morris, Duncan, and Rodriguez (2005) estimate that a $3,000 increase in annual income sustained for between two and five years boosts child achievement by .18 of a standard deviation. As with the Duncan *et al.* (1998) longitudinal analysis, this pattern of impacts does not generalize to children in other stages of childhood. Pre-school children were helped by the reforms that increased family resources and, for the most part, unsupportive ones neither harmed nor helped.

Our study draws on national data from the Panel Study of Income Dynamics (PSID), the longest-running longitudinal study of household income in the United States, to estimate linkages between income early in childhood and later life 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 control variables. 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.

Data and Methods

We use 1968-2005 data from the Panel Study of Income Dynamics, 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. We use the PSID's careful annual measurements of family income to compute average income during both the early (prior to age six) and later periods of childhood, and then relate childhood income to the children's completed schooling, adult earnings, annual hours worked, adult government program participation (Food Stamps and AFDC/TANF), physical and mental health, crime, and non-marital childbearing.

Sample. Drawing on PSID interviewing waves 1968-2005, we compile data on the eight cohorts of children born between 1968 and 1975. We required these individuals to be in response families in at least 12 of the 17 years from the prenatal period to age 15. Additional sample restrictions varied according to the dependent variable. In the case of completed schooling, we required that completed schooling be observed when the individual was 25 or older. Similarly, we required the individual to have been observed at least once after age 25 to measure adult earnings, work hours, and program assistance. In the case of self-reported physical health, information about the general health of the individual had to have been gathered at age 21 or later; and our measure of body mass index comes from the 2005 survey. Finally, mental health questions were only included in the 2003 survey.

Sample losses to nonresponse are substantial. Our target study sample consists of the 2,456 individuals born into PSID households between 1968 and 1975, who thus were between ages 30 and 37 in 2005. We required that these individuals not be missing on any control variables from childhood (defined below), leaving 77% of the birth sample (n =1,894). 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 30, or 1.2% of the original sample. Additional missing data reduced our completed school sample to 1,380 observations, while sample sizes for the earnings and transfer outcomes ranged from 1,084 to 1,256. In the case of distress and body mass, we required that that individuals be either "heads" or spouses of heads in 2003 or 2005.

Childhood income. We used the PSID's edited measure of annual total family income, inflated to 2005 levels using the Consumer Price Index. To adjust for the time value of money, we further discounted all childhood income amounts to the child's birth year, using a discount rate of 3%. We averaged these annual income measures across three periods: the prenatal year[3] through the calendar years in which the child turned five; ages six to ten; and ages 11 to 15. 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. Considerably experimentation confirmed the utility of the $25,000 thresholds; lower incomes reduced sample sizes and the precision of the low-income effect estimates; higher thresholds typically produced noteworthy reductions in the low-income segment coefficients.

Attainment-related 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 age 25 or older. As with childhood income, we inflated the dollar values of earnings to 2005 price levels using the Consumer Price Index (CPI) and discounted all earnings amounts to age 25, using a discount rate of 3%. To adjust earnings for age and calendar year effects, we regressed all of these earnings observations on age and calendar year dummies, obtained residuals, summed the residuals from these regressions and the overall sample mean earnings, averaged this summation across all available years for a given individual, and then took the natural log.

Food stamp and AFDC/TANF receipt are measured at the household level and are taken from all available surveys when the child was age 25 or older.[4] We created calendar-year values of both programs, inflated the values to 2005 price levels using the CPI, and discounted all values to age 25 using a discount rate of 3%. 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 sample mean values across all available years for a given individual. We estimate food stamp models for the entire sample, and AFDC/TANF models only for the females.

Health-related adult outcomes. 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?"[5] Individuals are considered in poor health if they responded that their health was either fair or poor. 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 body mass index 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 body mass index using the following formula: $\frac{(Weight \times 703)}{(Height^2)}$, where weight is measured in pounds and height is measured in inches. We follow convention and define "overweight" as a BMI greater than or equal to 25.

Our measure of *psychological distress* was based on responses to a 2003 administration of the K-6 Non-Specific Psychological Distress Scale, developed by Dr. Ronald Kessler of the Harvard Medical School. 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: i) so sad nothing could cheer you up?; ii) nervous?; iii) restless or fidgety?; iv) hopeless?; v) that everything was an effort?; vi) 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."[6]

Behavior-related adult outcomes. Our *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.

Our *non-marital 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. 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: i) dummy variables for seven of the eight birth years; ii) race (black, other, with white the reference category; iii) child sex (male=1); iv) whether the child's parents were married and living together at the time of the birth; v) the age of the mother at the time of the birth; vi) whether the child lived in the South at the time of the birth; vii) the total number of siblings born to the child's mother; viii) whether the child was the first born to his/her mother; ix) 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; x) the head's score on a sentence completion test administered in the 1972 interviewing wave; xi) the 1968-72 average response to an interviewer observation regarding the cleanliness of the respondent's dwelling,[7] and xii) the 1968-72 average of an index of parental expectations for children.[8]

Regression procedures. All of our regressions were run in STATA 9.0 SE, use the PSID's weights to adjust for differential sampling fractions and attrition,[9] and adjust for origin-family clustering on the mother using Huber-White methods. Continuous outcomes (*ln* earnings, work hours, and completed schooling) were analyzed with OLS; measures with substantial concentrations of zeroes (food stamp and AFDC/TANF receipt) were analyzed with Tobit regressions; dichotomous outcomes (poor health, high distress, ever arrested, ever jailed, and non-marital birth before age 21) were analyzed with logistic regression. The arrest and incarceration models are only run on males, whereas the AFDC/TANF and non-marital 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) on the probabilities of the given event occurring.

Results

Descriptive statistics. Case counts, means and standard deviations of each of our outcome variables are provided in Table 1 (see also Figures 1 and 2). These summary statistics are weighted using the weight for the year in which the outcome was measured (high distress, body mass index, overweight, arrest, and incarceration), or the most recent weight of the PSID for completed schooling, earnings, work hours, food stamp and AFCD/TANF receipt and nonmarital childbearing. Descriptive statistics are presented for both the overall sample and for children whose prenatal-to-age-5 incomes averaged: i) below the official poverty line; ii) between one and two times the poverty line; and iii) more than twice the poverty line. The final column of Tables 1 and 2 provides information on the statistical significance (at $p<.05$ or below, two-tailed test) of the mean differences across the three groups.

Table 1 and Figures 1 and 2 show striking differences in adult outcomes depending on whether childhood income prior to age six 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 two fewer years of schooling, work 424 fewer hours per year, earn about half as much, received $750 per year more in food stamps as adults and are about twice as likely to report poor overall health or high levels of psychological distress. Further, poor children have BMIs that are four points higher than those well above the poverty line, and are almost 50 percent more likely to be overweight as adults. Poor males are more than twice as likely to be arrested and have rates of incarceration that are two times as high as those of males in higher-income families. For females, poverty is associated with a nearly $200 annual increase in cash assistance, and a six-fold increase in the likelihood of bearing a child out of wedlock prior to age 21.

Table 2 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 and lower parental expectations compared with their higher income counterparts.

Regression results. Results from our basic regression models of adult outcomes are summarized in Tables 3 (for achievement outcomes and program participation), 4 (for health outcomes) and 5 (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 less than $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.

Tables 3 shows that additional income in the prenatal to age five period for the lowest-income children is associated with significantly greater adult earnings and work hours, and less food stamp and (for females) AFDC/TANF receipt. The coefficient on early income in the schooling regression is .312 years, which is not statistically significant at conventional levels (p=.184).[10]

We can illustrate the nature of the income effects of early-childhood income using the ln earnings regression. The ".573" coefficient means that, adjusting for income later in childhood and the other control variables listed in Table 3, an additional $10,000 per year of family income per year between the prenatal year and the child's fifth birthday is associated with an increase in the natural logarithm of adult earnings of .573 – or 77.4%.[11] 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 .027 increment in log earnings. The p-value (p<.01) reported in the first two of 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 3 indicates that the three coefficients on the <$25,000 spline slopes across the childhood stages are significantly different from one another at the p=.035 level.

The pattern of effects implied by these six income coefficients in the log earning regression is graphed in Figure 3. Not surprisingly, the steepest slope is for low income in the prenatal to age-5 period and shows that an increase from $0 to $25,000 per year is associated with a more than 300 percent increase in adult earnings. Of course, $25,000 per year for the seven-year interval between the prenatal and fifth birthday years is a huge sum. A more policy-relevant income increase might be on the order of $3,000 per year, which is well within the maximum annual tax credit provided by the EITC. A $3,000 annual increase for the prenatal to fifth birthday year ($21,000 in all) is associated with a 19% adult earnings increase. We return to the potential policy relevance of this association in our concluding section. [12]

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 prenatal to age five income is associated with more than 450 additional work hours per year after age 25. Tobit spline regressions for food stamps for the entire sample and AFDC/TANF receipt for women suggest that increases in income in the early-childhood period are associated with statistically significant reductions in both food stamps and AFDC/TANF.[13]

Results for the controls variables in the completed schooling regression mirror past research, with blacks (adjusted for SES and other controls), females, children born first, into intact or smaller families, or born to older mothers, more educated parents or into cleaner homes, associated with more schooling. Few of these controls have persistently significant coefficients across all of the attainment-related outcomes in Table 3.

The marginal effects from logistic spline models for poor health, high distress and overweight, and OLS results for BMI are shown in Table 4 and show scattered income effects in middle childhood and adolescence, but not early childhood.

Table 5 presents marginal effects for the behavioral outcomes for men (arrests and jail) and women (nonmarital childbearing). The early childhood income segments are not statistically significant in any of these models.

Extensions. We explored the robustness of these results in various ways, first by testing for sex and race interactions. For only one outcome – food stamps – was gender differences found, with all of the apparent effect of early income shown in Table 3 concentrated among females.[14] For the race interactions, black incarceration appeared to be significantly less sensitive to increments in early-childhood income than white incarceration.

In averaging early childhood income over the seven-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 6, we present results from regressions that are identical to those in Table 3 through 5 except that the prenatal to age five period is further divided into two segments – the prenatal and birth years and ages one through five. In both cases (and with the age 6-10 and 11-15 income measures) we fit two-segment spline functions with $25,000 thresholds. A comparison of coefficients shows that point estimates of income effects for the five attainment-related outcomes are all larger when income is measured between ages one and five than earlier, although only in the case of AFDC/TANF is the difference statistically significant. Age one to five income also appears to matter more for reducing arrests and incarceration among sample males.

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, & Kalil (2007). This finding of the particular importance of income during the prenatal and birth years for adult BMI may support the “fetal origins” hypothesis. This hypothesis posits a fetal programming process whereby stimulants and insults during this critical period of development have long-lasting implications for physiology and disease risk

Table 7 explores the robustness of the prenatal-to-age-5 low income coefficients is a number of ways. The coefficients and standard errors in the first column are identical to those presented in Tables 3 through 5. In the second column we retain all of the control variables, but exclude income from the other childhood stages. This almost certainly overstates the unique contribution of early childhood income but give some idea of the role played by holding later childhood income constant. Not surprisingly, the coefficients are all larger than in the basic regression, and many become significantly associated with the study outcomes. Interestingly, the standard errors rarely drop by more than 25% when later income is removed, suggesting that the additional income controls are not introducing multicollinearity problems in our Table 3-5 models.

The third column of Table 7 presents the coefficients and standard errors from regressions that include all of the basic controls, but in this case maternal earnings have been subtracted from all three stage-specific income variables. This helps to address the problem that childhood income is a function of mothers' earnings, and 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 outcomes for which early income effects were statistically significant in Table 3-5, the new income coefficients are smaller but retain statistical significance for all but the AFDC/TANF outcome.

We next test models that repeat the Table 3-5 analyses except that childhood income is not discounted – a change that has very small effects on estimated coefficients. More consequential are the fifth column's results for unweighted models. Coefficients on the early income measures fall sharply for all of the attainment measures, although remain significant in the case of earnings and work hours. Given the lack of race-based interactions mentioned earlier, this is somewhat puzzling and is being explored.

Stage-specific income results presented in Heckman and Carneiro (2003, p 120) employ a somewhat different specification than ours in which 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. We find that permanent income plays a statistically significant role in schooling and food stamp receipt. Significantly more beneficial impacts of income in early childhood are observed for adult earnings, annual work hours, receipt of AFDC/TANF and psychological distress. The first three results are fully in keeping with the models presented in Table 3-5. We hesitate to attach much importance to the psychological distress result since it does not fit the general patterns observed in earlier tables.

Given the interesting results for the log earnings models, we sought to explore mediational pathways by sequentially introducing the predictors sets listed in the columns of Table 8. Controls for childhood maternal work hours and family composition had little impact on the key early-income coefficient.[15] Completed schooling accounts for very little of the income effect, which is hardly surprising given the weak links between early income and schooling (Table 3). 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 labor supply rather than wage rate.

As a final test of robustness, we estimated the earnings models using sibling fixed effects, a technique applied successfully to the PSID data in the early-conditions models of Johnson and Schoeni (2007). Roughly half of the observations in our full sample consist of individuals with brothers or sisters born into the 1968-75 birth cohorts and meeting our other selection criteria.

We first estimated our basic OLS model using the sibling sample. The coefficient on early income is considerably higher than it is for the full sample – .91 vs. .57 – and, despite the somewhat higher standard error, statistically significant. The sibling fixed effects version of the model produced an estimate of early-income effects (.48) that is similar that found in our full-sample OLS models, although the standard error for the estimate is very large. Considerable experimentation with simpler parameterization of income effects (e.g., with log income instead of two-segment splines) and with more divisions of childhood failed to produce sibling estimates with trustworthy precision.

Discussion

Our exploration of the role of economic deprivation early in childhood produced surprisingly strong "effects" in the case of such important adult attainments as earnings, work hours and transfer income. The coefficients imply that a $3,000 annual increase in income between a child's prenatal and birth year is associated with 19% higher earnings and a 135-hour increase in work hours. Moreover, most of the childhood-income effects on earnings appear to be accounted for by the labor supply differences, suggesting that the labor supply results are particularly important to understand.

One important policy issue is the persistence of these effects across adulthood. A supplemental 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 to 37), so we can safely assume that the childhood income impact persists for at least that 13-year period and perhaps beyond as well.

How does the 19% earnings impact of a $3,000 annual income transfer to low-income families compare with other human capital expenditures? The literature on earning impacts of completed schooling is the most voluminous (Card, 1999) and suggests that added years of schooling increase lifetime earnings by roughly 10%. Matching the 19% earnings impact of the $21,000 (i.e., $3,000 per year for the seven years between the prenatal and fifth birthday years) increase in early childhood income would take roughly two more years of schooling. With K-12 public schooling costs averaging around $8,700 per year, it would appear that funding extra years of schooling has more of a payoff than income transfers targeted to early childhood. The dearth of evidence on, for example, effective dropout prevention programs (Dynarski, 2001) suggests that inducing youth to acquire additional schooling is easier said than done.

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. On the other hand, the incomes we observe are determined, in part, by the actions of parents and other family members, which leaves them open to omitted-variable bias. Our list of variables controlling for conditions at the time of birth are substantial 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. It is difficult to think of omitted variables correlated strongly with our outcomes and with early childhood income that would not also correlate with income at other stages. The usual suspects, 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.

Table 1: Weighted Descriptive Statistics of Adult Outcomes by Prenatal to Age 5 Poverty

	Total Sample		Income below the official poverty line		Income between one and two times the poverty line		Income more than twice the poverty line		Significant Differences
			(A)		(B)		(C)		
	Mean or %	SD	Mean or %	SD	Mean or %	SD	Mean or %	SD	
Completed schooling	13.40	2.14	11.81	1.81	12.94	2.07	14.10	1.96	A<B,C; B<C
Unweighted n	*1,397*		*338*		*499*		*560*		
Earnings (2005$ not discounted)	34462.80	30815.19	17114.60	14440.33	29810.31	21762.53	40752.37	35467.50	A<B,C; B<C
Unweighted n	*1,111*		*234*		*388*		*489*		
Annual work hours	1887.33	700.72	1512.88	764.52	1942.70	737.35	1936.66	639.59	A<B,C
Unweighted n	*1114*		*234*		*389*		*491*		
Food stamps (2005$ not discounted)	202.92	702.62	810.77	1343.83	205.32	681.16	65.84	343.96	A>B,C; B>C
Unweighted n	*1,271*		*287*		*452*		*532*		
AFDC/TANF (2005$ not discounted; women only)	117.77	593.42	231.02	855.55	197.54	771.77	41.85	318.03	A>C
Unweighted n	*649*		*171*		*221*		*257*		
Poor health	7.64%	---	12.90%	---	9.01%	---	5.53%	---	A>C
Unweighted n	*1,292*		*285*		*465*		*542*		
High distress	3.74%	---	5.96%	---	3.03%	---	3.77%	---	ns
Unweighted n	*769*		*149*		*256*		*364*		
Body mass index	27.39	5.78	30.79	7.13	27.54	5.50	26.71	5.42	A>B,C
Unweighted n	*885*		*164*		*298*		*423*		
Overweight	60.71%	---	81.73%	---	62.32%	---	56.18%	---	A>B,C
Unweighted n	*885*		*164*		*298*		*423*		
Arrested (men only)	16.49%	---	28.09%	---	17.48%	---	13.38%	---	A>C
Unweighted n	*796*		*173*		*290*		*333*		

Incarcerated (men only)	8.78%	---	13.87%	---	10.70%	---	6.59%	---	ns
Unweighted n	*796*		*173*		*290*		*333*		
Nonmarital birth (women only)	18.58%	---	52.42%	---	20.51%	---	8.34%	---	A>B,C; B>C
Unweighted n	*836*		*217*		*297*		*322*		

Note: In “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-tail). 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-tail).

Table 2: Weighted Descriptive Statistics of Control Variables by Prenatal to Age 5 Poverty

	Total Sample		Income below the official poverty line		Income between one and two times the poverty line		Income more than twice the poverty line		Differences
			(A)		(B)		(C)		
	Mean or %	SD	Mean or %	SD	Mean or %	SD	Mean or %	SD	
Prenatal to age 5 average annual income (2005$)	49149.55	27774.31	22582.36	14558.32	37018.05	12208.31	63927.41	28445.47	A<B,C; B<C
Age 6 to age 10 average annual income (2005$)	58733.15	35850.14	26343.56	20024.47	47527.24	20750.76	74521.94	38021.07	A<B,C; B<C
Age 11 to age 15 average annual income (2005$)	69630.27	48154.03	30753.18	25557.08	56093.30	28824.19	88635.29	53041.92	A<B,C; B<C
White	77.16%	---	34.64%	---	75.11%	---	90.03%	---	A<B,C; B<C
Black	16.59%	---	54.17%	---	18.67%	---	5.06%	---	A>B,C; B>C
Other minority	5.21%	---	10.86%	---	5.77%	---	3.32%	---	A>C
Male	53.32%	---	53.16%	---	52.79%	---	53.69%	---	ns
Born into intact family	85.35%	---	49.33%	---	84.24%	---	95.85%	---	A<B,C; B<C
Born in the South	31.47%	---	50.65%	---	35.26%	---	23.88%	---	A>B,C; B>C
Age of mother at birth	25.41	5.88	24.56	7.07	24.27	6.31	26.34	5.04	A,B<C
Number of siblings	2.25	1.80	3.67	2.88	2.56	1.67	1.67	1.10	A>B,C; B>C
Child is first born	40.39%	---	33.26%	---	37.09%	---	44.38%	---	A, B>C
Household head test score (1972)	9.57	2.14	7.76	2.44	9.36	1.93	10.20	1.85	A<B,C; B<C
Household head schooling (1972)	11.77	3.38	8.39	3.89	11.07	2.99	13.14	2.57	A<B,C; B<C
Observed "dirty" home (average 1968 to 1972)	2.07	.89	2.90	.96	2.23	.86	1.74	.68	A>B,C; B>C
Parental expectations (average 1968 to 1972)	4.82	.80	4.25	.84	4.73	.78	5.04	.70	A<B,C; B<C
Unweighted n	*1,723*		*419*		*629*		*675*		

Note: In "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-tail). 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-tail).

Table 3: Coefficients and Standard Errors from OLS Spline 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		ln Earnings (age 25-37)		Annual hours worked (age 25-37)		Annual Food Stamp (age 25-37)		Annual AFDC/TANF Females only (age 25-37)	
Childhood income (in $10,000)			Different slopes?		Different slopes?		Different slopes?		Different slopes?		Different slopes?
Average annual income Prenatal to age 5	<$25K	.312 (.235)	ns	.573** (.158)	$p < .01$	454.0** (106.7)	$p < .001$	-370.7** (116.8)	$p < .01$	-435.1** (154.2)	$p < .01$
	>$25K	.-.005 (.044)		.027 (.018)		16.4 (12.6)		14.6 (16.9)		16.9 (14.9)	
Average annual income Age 6 to 10	<$25K	.241 (.216)	ns	.082 (.120)	ns	-81.1 (97.2)	ns	-235.4* (104.9)	$p < .05$	-90.0 (129.9)	ns
	>$25K	-.002 (.056)		.019 (.021)		11.2 (12.6)		-10.5 (20.3)		-19.5 (26.2)	
Average annual income Age 11 to 15	<$25K	.139 (.190)	ns	.010 (.098)	ns	134.7 (71.3)	$p < .05$	-207.7* (85.1)	$p < .05$	166.2 (115.0)	ns
	>$25K	.096* (.044)		.001 (.021)		-14.9 (11.8)				-16.2 (18.6)	
Other variables											
Black		.804** (.256)		.205 (.109)		26.4 (80.0)		221.3* (86.1)		676.4** (115.9)	
Other minority		.072 (.374)		-.091 (.273)		-288.9* (140.5)		15.3 (126.1)		20.9 (183.9)	
Child is male		-.458** (.131)		.536** (.070)		543.8** (47.5)		-165.6** (53.0)		---	---
Child born into intact family		.662* (.217)		-.010 (.109)		15.8 (78.4)		-12.6 (91.3)		309.0* (127.6)	
Child born in South		-.199 (.184)		-.120 (.067)		-67.4 (53.7)		-36.0 (60.7)		-283.4** (77.9)	
Age of mother at time of birth		.043* (.015)		-.001 (.008)		-1.9 (5.8)		-13.5* (5.8)		-19.1* (7.5)	
Number of siblings		-.097*		-.018		-8.9		15.9		8.9	

		(.049)		(.026)		(18.2)		(17.4)		(22.4)	
Child is first born		.420** (.146)		.112 (.078)		83.2 (55.3)		-97.4 (61.9)		-195.6* (77.5)	
HH head test score (1972)		.053 (.041)		-.016 (.016)		-19.3 (15.6)		.8 (14.6)		20.3 (19.5)	
HH head schooling (1972)		.110** (.028)		.025 (.015)		9.4 (11.8)		-15.9 (10.7)		6.2 (14.0)	
Observed "dirty" home (average 1968 to 1972)		-.216* (.097)		-.061 (.044)		56.0 (34.6)		-38.8 (35.5)		-122.8* (44.0)	
Parental expectations (average 1968 to 1972)		.209 (.111)		.097* (.045)		59.5 (31.3)		15.5 (36.6)		-83.0 (47.1)	
Birth year dummies?		Yes		Yes		Yes		Yes		Yes	
Regression statistics											
R-squared		.264		.235		.239		.029		.030	
Number of observations		1,380		1,084		1,102		1,256		642	
p from test of equality of three <$25K spline segments		.865		.035		.007		.528		.014	

Notes: * indicates p<.05; ** p<.01

Sample consists of PSID children born between 1968 and 1975. Incomes are in 2005 dollars and are discounted back to the birth year using a 3% interest rate. Earnings, Food Stamps, and AFDC/TANF are in 2005 dollars and are discounted back to age 25 using a 3% interest rate. 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 OLS analysis, and the coefficients and standard errors for food stamps and AFDC/TANF are from Tobit analysis. The AFDC/TANF analysis is only for females.

Table 4: Coefficients and Standard Errors from Spline Models of Various Health Measures

		Poor Health		**High distress (2003)**		**Body Mass Index (2005)**		**Overweight (BMI>25 in 2005)**	
Childhood income (in $10,000)			Different slopes?		Different slopes?		Different slopes?		Different slopes?
Average annual income Prenatal to age 5	<$25K	-.021 (.019)	ns	-.009 (.011)	ns	-2.09 (1.53)	ns	.033 (.099)	ns
	>$25K	.004 (.004)		.000 (.001)		.06 (.19)		.009 (.014)	
Average annual income Age 6 to age 10	<$25K	-.034* (.017)	*p* < .05	.008 (.006)	ns	-1.37 (.92)	ns	-.247** (.096)	*p* < .05
	>$25K	.011* (.005)		.005* (.003)		-.09 (.14)		-.023 (.015)	
Average annual income Age 11 to age 15	<$25K	.014 (.019)	*p* < .10	-.018* (.009)	ns	1.12 (.76)	ns	.190* (.085)	*p* < .05
	>$25K	-.025** (.004)		-.005* (.002)				.012 (.012)	
Other variables									
Black		.040 (.028)		-.015* (.007)		.84 (.84)		.020 (.088)	
Other minority		-.005 (.040)		.000 (.006)		1.14 (1.40)		.058 (.142)	
Child is male		.025* (.012)		-.002 (.004)		1.76** (.45)		.278** (.042)	
Child born into intact family		.024* (.012)		-.003 (.010)		.59 (.77)		.076 (.089)	
Child born in South		.005 (.014)		.013 (.008)		.10 (.52)		-.096 (.053)	
Age of mother at time of birth		.000 (.001)		.000 (.000)		-.02 (.06)		-.009 (.006)	
Number of siblings		-.004 (.004)		-.001 (.001)		.23 (.17)		.019 (.018)	
Child is first born		-.015		.006		.68		.030	

		(.013)		(.006)		(.48)		(.050)	
Household head test score (1972)		.004 (.004)		-.003 (.002)		-.10 (.13)		-.019 (.014)	
Household head schooling (1972)		-.004 (.002)		.001 (.001)		-.23* (.09)		-.024* (.010)	
Observed “dirty” home (average 1968 to 1972)		.008 (.007)		.005 (.003)		.32 (.32)		.014 (.032)	
Parental expectations (average 1968 to 1972)		.011 (.008)		.001 (.003)		-.21 (.30)		-.003 (.031)	
Birth year dummies?		Yes		Yes		Yes		Yes	
Regression statistics									
R-squared (or pseudo)		.17		.290		.120		.104	
Number of observations		1,240		645		884		884	
p-level of test of equality for the three <$25K spline segments		.290		.100		.079		.031	

Notes: * indicates p<.05; ** p<.01
Sample consists of PSID children born between 1968 and 1975. Incomes are in 2005 dollar and are discounted back to the birth year using a 3% interest rate. Food stamps and AFDC/TANF are in 2005 dollars and are discounted back to age 25 using a 3% interest rate. 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.

Table 5: Marginal Effects and Standard Errors from Logistic Spline models of Childhood Income and Arrests, Jailed, and Nonmarital Births

		Ever Arrested (males)		**Ever Jailed (males)**		**Nonmarital Birth (females)**	
Childhood income (in $10,000)			Different slopes?		Different slopes?		Different slopes?
Average annual income Prenatal to age 5	<$25K	-.004 (.049)	ns	-.003 (.023)	ns	.009 (.039)	ns
	>$25K	-.012 (.015)		.007 (.008)		.012 (.011)	
Average annual income Age 6 to 10	<$25K	.000 (.057)	ns	.027 (.029)	ns	-.005 (.033)	ns
	>$25K	.023 (.012)		-.006 (.007)		-.009 (.011)	
Average annual income Age 11 to 15	<$25K	-.039 (.047)	ns	-.032 (.021)	ns	-.023 (.027)	ns
	>$25K	-.020 (.011)		-.007 (.006)		-.032 (.010)	
Other variables							
Black		-.045 (.039)		-.011 (.018)		.159** (.061)	
Other minority		-.081 (.051)		-.022 (.026)		.089 (.081)	
Child born into intact family		-.121 (.065)		-.039 (.036)		.018 (.029)	
Child born in South		-.001 (.040)		-.011 (.020)		-.043 (.026)	
Age of mother at time of birth		-.008* (.004)		-.005 (.002)		-.007* (.003)	
Number of siblings		.025* (.010)		.006 (.007)		.019* (.007)	
Child is first born		-.024 (.037)		-.014 (.018)		.004 (.028)	
Household head test score (1972)		-.002 (.008)		.004 (.004)		-.007 (.006)	
Household head schooling (1972)		-.010 (.005)		-.007* (.003)		-.001 (.004)	
Observed “dirty” home (average 1968 to 1972)		-.053* (.022)		-.015 (.011)		.013 (.013)	
Parental expectations (average 1968 to 1972)		.010 (.021)		.002 (.009)		-.015 (.016)	
Birth year dummies?		Yes		Yes		Yes	
Regression statistics							
Pseudo R-squared		.098		.201		.256	
Number of observations		773		773		825	
p level of test of equality for the three <$25K		.876		.411		.784	

spline segments							

Notes: * indicates p<.05; ** p<.01

Sample consists of PSID children born between 1968 and 1975. Incomes are in 2005 dollar and are discounted back to the birth year using a 3% interest rate. 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.

Table 6: Coefficients and Standard Errors from Models in Which Prenatal to age 5 Income Is Divided Into (i) Prenatal and Birth Years and (ii) 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 age 1 to age 5 (<25K)	
Completed schooling	-.020 (.169)	.422 (.266)	ns
ln Earnings	.207 (.113)	.294* (.163)	ns
Annual hours worked	108.32 (68.71)	324.60** (99.58)	ns
Annual Food Stamp receipt	-187.89* (81.63)	-188.72 (108.46)	ns
Annual AFDC/TANF receipt (females)	-32.07 (99.46)	-356.67* (140.32)	$p < .10$
Poor health	.008 (.014)	-.030 (.018)	ns
High distress	.002 (.005)	-.003 (.006)	ns
Body mass index	-1.68** (.683)	-.454 (1.57)	ns
Overweight	-.122 (.069)	.148 (.112)	$p < .10$
Arrested (males)	.100* (.045)	-.131** (.056)	$p < .01$
Incarcerated (males)	.048 (.026	-.050 (.028)	$p < .05$
Non-marital childbearing(females)	-.020 (.029)	.056 (.036)	ns

Notes: * indicates p<.05; ** p<.01

Sample consists of PSID children born between 1968 and 1975. Incomes are in 2005 dollars and are discounted back to the birth year using a 3% interest rate. 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, age 1 through age 5, age 6 through age 10, and age 11 through age 15. Different slopes indicates whether the low-income segment between prenatal and birth is significantly different than between age 1 and age 5. OLS coefficients shown for completed schooling, earnings, annual hours worked, and body mass index, Tobit coefficients shown for food stamps and AFDC/TANF, and marginal effects shown for the dichotomous outcomes.

Table 7: Coefficients and Standard Errors on Average Annual Income Prenatal to Age 5 <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	No controls for age 6 to 15 income	Exclude maternal earnings from all childhood income measures	No discounting of prenatal to age 15 incomes	Basic regression, unweighted	Coefficient on low-income permanent income spline segment	Coefficient on low-income prenatal to age-5 spline segment
Completed schooling	.312 (.235)	.569** (.216)	-.066 (.179)	.259 (.248)	.136 (.175)	.829* (.398)	-.136 (.364)
ln Earnings	.573** (.158)	.640** (.130)	.334** (.091)	.558** (.179)	.194* (.100)	.287 (.165)	.456* (.202)
Annual hours worked	454.0** (106.7)	478.3** (84.9)	243.0** (61.5)	485.5** (116.9)	187.5* (81.0)	74.08 (140.9)	410.2** (152.3)
Annual Food Stamp receipt	-370.7** (116.8)	-702.2** (94.5)	-171.1* (78.1)	-342.8** (128.0)	-128.8 (119.3)	-648.0** (151.5)	-122.2 (167.4)
Annual AFDC/TANF receipt (females only)	-435.1** (154.2)	-388.1** (119.5)	-127.8 (98.1)	-495.4** (166.1)	-19.2 (127.6)	48.9 (189.8)	-446.3* (223.6)
Poor health	-.021 (.019)	-.057* (.027)	-.012 (.013)	-.028 (.021)	-.016 (.017)	-.024 (.030)	-.032 (.033)
High distress	-.009 (.011)	-.033* (.013)	-.004 (.006)	-.007 (.009)	-.035** (.013)	.005 (.016)	-.039* (.017)
Body mass index	-2.09 (1.53)	-2.60** (1.30)	-1.26 (.90)	-2.87 (1.75)	-.912 (.948)	-.113 (1.26)	-2.50 (1.77)
Overweight	.033 (.099)	-.055 (.080)	.014 (.062)	.028 (.108)	.007 (.064)	-.005 (.113)	-.059 (.108)
Arrested	-.004 (.049)	-.025 (.043)	.008 (.037)	.006 (.049)	-.060 (.038)	-.087 (.069)	.048 (.071)
Incarcerated	-.003 (.023)	.002 (.019)	.007 (.017)	.001 (.025)	-.012 (.024)	-.006 (.028)	.003 (.024)
Non-marital childbearing	.009 (.039)	-.028 (.039)	.014 (.029)	.012 (.043)	.052 (.043)	-.077 (.044)	.055 (.051)

Notes: * indicates p<.05; ** p<.01
Marginal effects reported for the dichotomous outcomes.

Table 8: 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	Sibling sample - OLS	Sibling Fixed Effects
Average annual income <$25K, prenatal to age 5	.573** (.158)	.565** (.156)	.527** (.156)	.529** (.157)	.535** (.160)	.111 (.109)	.908** (.235)	.480 (1.08)
Income age 6-10 and age 11-15	incl.	incl.	incl.	incl.	incl.	incl.	incl.	incl.
Other background controls	incl.	incl.	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-squared	.235	.235	.310	.319	.331	.637	.294	.643
Number of observations	1,084	1,084	1,084	1,084	1,084	1,084	510	510

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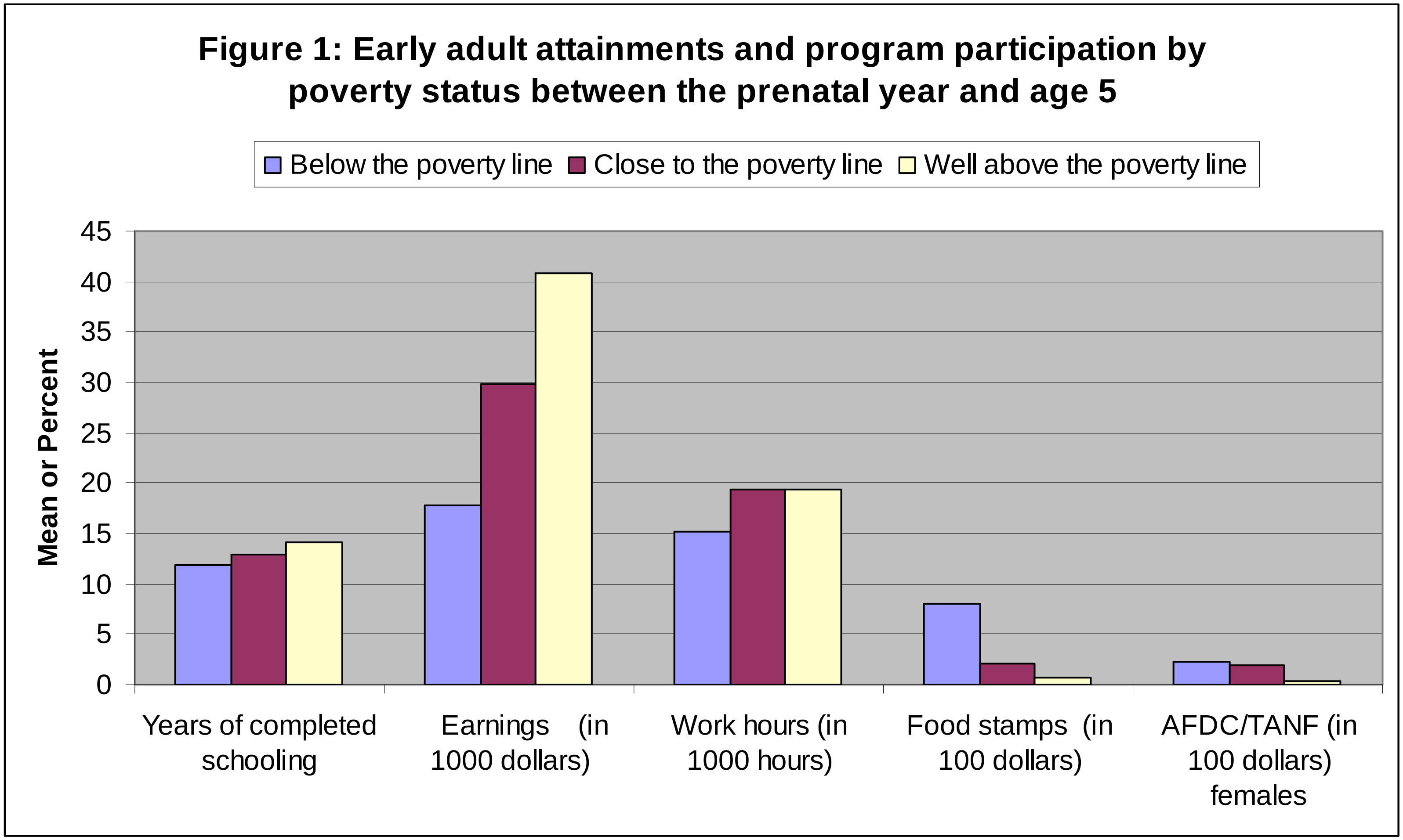


Figure 1: Early adult attainments and program participation by poverty status between the prenatal year and age 5
Below the poverty line
Close to the poverty line
Well above the poverty line
Mean or Percent
45
40
35
30
25
20
15
10
5
0
Years of completed schooling
Earnings (in 1000 dollars)
Work hours (in 1000 hours)
Food stamps (in 100 dollars)
AFDC/TANF (in 100 dollars) females

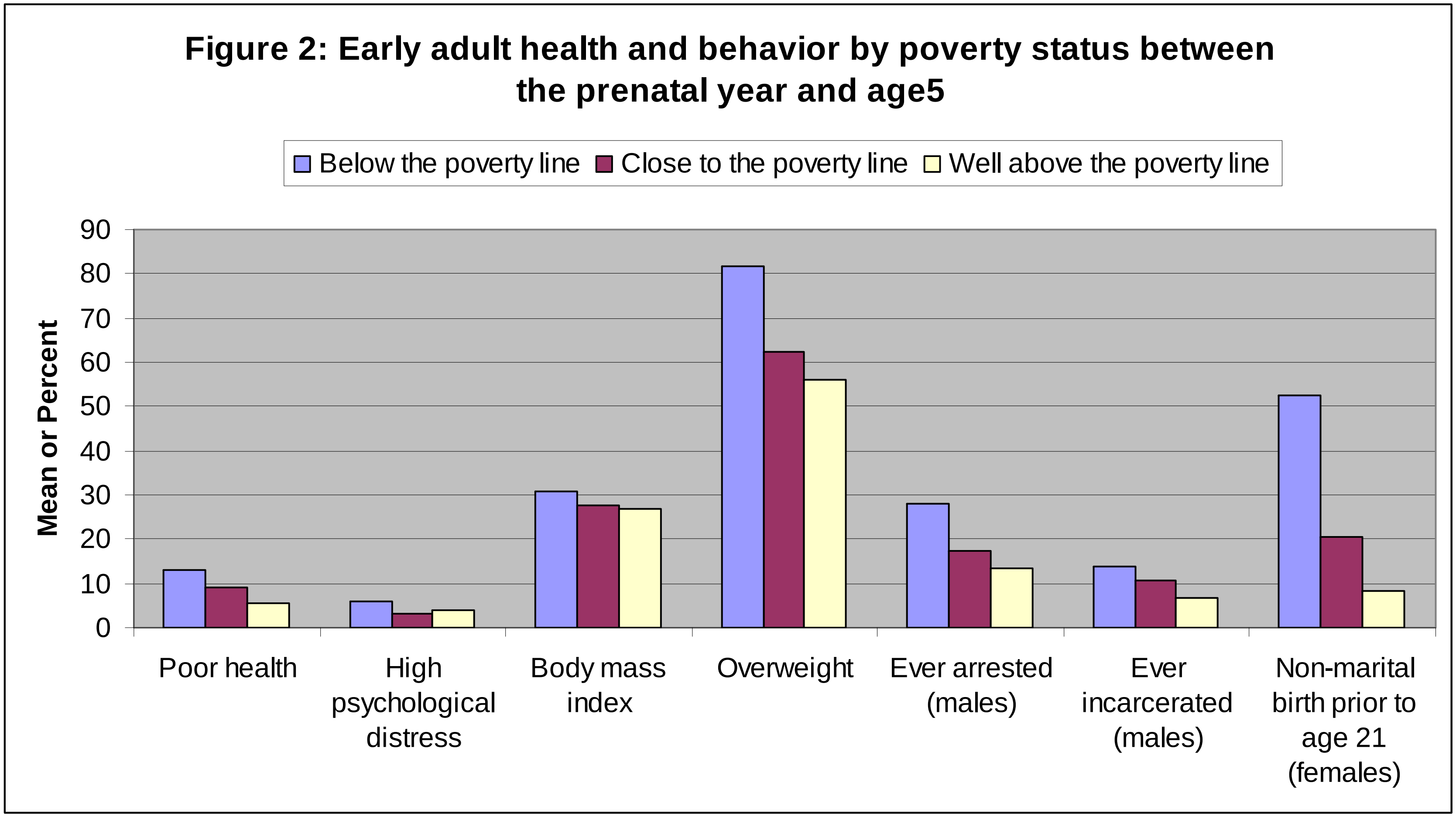
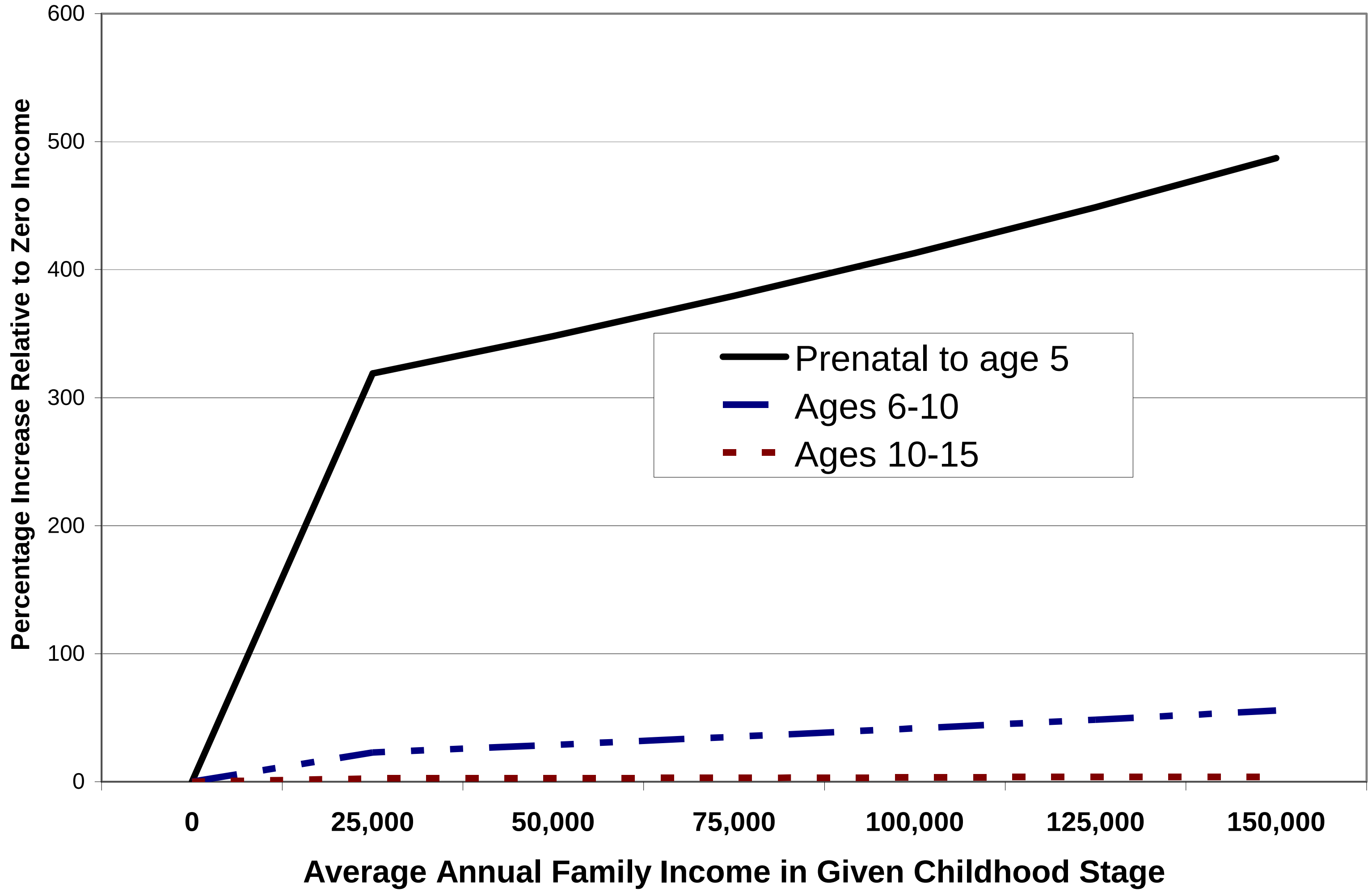


Figure 2: Early adult health and behavior by poverty status between the prenatal year and age5
Below the poverty line
Close to the poverty line
Well above the poverty line
Mean or Percent
90
80
70
60
50
40
30
20
10
0
Poor health
High psychological distress
Body mass index
Overweight
Ever arrested (males)
Ever incarcerated (males)
Non-marital birth prior to age 21 (females)

Figure 3: Percentage Increase in Adult Earnings by Average Annual Income in Early, Middle and Late Childhood



ENDNOTES

[1] This project was funded in part by the Partnership for America's Economic Success. We thank William Dickens, Rob Dugger, Mimi Engel, Michael Foster, Rucker Johnson, Ann Siegal, Aaron Sojourner, Sergio Urzua, and Sara Watson for comments on a related draft.

[2] This is the 2005 OMB poverty threshold. See http://www.census.gov/hhes/www/poverty/threshld/thresh05.html

[3] With income reported for calendar years and conceptions occurring continuously, there was some imprecision in matching income to the prenatal year. 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.

[4] It is common for adults under age 25 to be recipients of income from these programs. We chose 25 as our starting point since it appears that early childhood income does not become a significant correlate of transfer income receipt until about age 25 and it was difficult to assign transfer income sources to individual household members prior to around age 25.

[5] 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. In neither case did significant impacts of early income emerge.

[6] As with our general health measure, we also analyzed a continuous measure of distress and failed to find significant impacts of early-childhood income.

[7] The question was "How clean was the interior of the DU [dwelling unit]?", with a response scale ranging from "very clean" (1) to "dirty" (5) in the 1968 through 1972 surveys. We averaged the perceived cleanliness of the home across all available years and use that average. Higher values represent less clean perceived home environments. Dunifon, Duncan and Brooks-Gunn (2001) find links between responses and children’s completed schooling.

[8] This measure is taken from the 1968 to 1972 surveys and captures parental expectations for their’s and their children’s future. We average the summary scale across all years. The summary scale consists of parents’ responses to statements such as having explicit plans for their children’s education and jobs.

[9] Experimentation with both weighted and unweighted regression estimates revealed marked differences in coefficients on early-childhood income in several cases, which led us to use the weights in our regression analyses. This is discussed further in our results section.

[10] The lack of a significant association between early income and schooling conflicts with results presented in Duncan et al. (1998). The present analysis includes three different birth cohorts than Duncan et al. (1998) and the early income-schooling relationship appears somewhat weaker for the new cohorts. A more interested difference is that Duncan et al. (1998) 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 .338 and statistically significant ($p < .05$). Further, when examining a specification that includes four time periods (prenatal through age two, age three through five, age six through ten, and age 11 through 15) the coefficient on age three through age five was significant both for completed schooling by age 21 (.399) and for on time high school graduation (marginal effect from a logistic regression of .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.

[11] $e^{.573} = 1.774$, which corresponds to a 77.4 percent increase.

[12] $e^{(.573*.3)} = 1.1876$, which corresponds to a 18.9 percent increase.

[13] Both sets of coefficients proved somewhat sensitive to outliers. We decided to truncate both dependent variables at the 99th percentiles of their respective distributions -- $3,917 in the case of annual food stamp receipt and $2,954 in the case of annual AFDC/TANF receipt. When we ran separate food stamp models for males and females, 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 -$551 (182) and for males were -$200 (86). The females coefficient has a p<.01; the male coefficient has a p=.02.

[14] These results are not shown in the tables. The coefficient on the low-income early spline segment for females was -694 with a standard error of 167. The make difference was +588 (standard error=228).

[15] 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.