HOW MUCH DOES CHILDHOOD POVERTY AFFECT THE LIFE CHANCES OF CHILDREN?*

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Why parental socioeconomic status correlates strongly with various measures of child and adult achievement is an important and controversial research question. After summarizing findings from recent contributions to this literature, we conduct two sets of analyses using data from the Panel Study of Income Dynamics. Completed schooling and nonmarital childbearing are related to parental income during early and middle childhood, as well as during adolescence. These analyses suggest that family economic conditions in early childhood have the greatest impact on achievement, especially among children in families with low incomes. Estimates from sibling models support the hypothesis that economic conditions in early childhood are important determinants of completed schooling.

Poverty rates among U.S. children are one-third higher than they were two decades ago and 1.5 to 4 times as high as the rates for children in Canada and Western Europe (Rainwater and Smeeding 1995). In 1995, some 15.3 million children lived in families in which total income failed to exceed even the Spartan thresholds (e.g., $12,158 for a family of three) used to define poverty (U.S. Bureau of the Census 1996).

The implications of these alarming poverty figures for America's children remain in dispute. There is little doubt that children raised in poverty have less enjoyable childhoods. But to what extent does poverty adversely affect cognitive and behavioral development and thereby reduce opportunities for success and happiness in adulthood? Securing answers to this important question is difficult for a variety of reasons (Brooks-Gunn and Duncan 1997; Mayer 1997).

First and foremost, past research linking economic disadvantage and child development has rarely incorporated the careful measurement of economic deprivation. Unless the data contain reliable measures of both family income and correlated aspects of parental socioeconomic status, it is impossible to estimate the separate contributions of each.

Income and social class are far from synonymous. Events like divorce and unemployment can alter permanently a family's economic and social position. Because family incomes are surprisingly volatile (Duncan 1988), the relatively modest correlations between economic deprivation and typical measures of socioeconomic background enable researchers to distinguish statistically between the effects on children's development of income poverty and those of its cor-

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related events and conditions (Hill and Duncan 1987; Sewell and Hauser 1975).[1]

The distinction is crucial, both conceptually and for public policy reasons. Programs that alter family income (e.g., time limits on welfare-program benefits, the Earned Income Tax Credit, the minimum wage) are often easier to design and administer than programs aimed at other family characteristics (e.g., promoting school completion of the mother or labor-force involvement of men; reducing out-of-wedlock childbearing).

Fortunately, several data sets containing reliable longitudinal measures of family income, socioeconomic status, and children's developmental outcomes have become available in the past decade. Much of the work to date using these data has estimated "reduced-form" models relating outcomes to income and other components of socioeconomic status and has left unanswered many important questions.

First, little is known about the importance of the *timing* of economic deprivation during childhood. Studies of children's early cognitive and physical development suggest that family income in the first five years of life is a powerful correlate of developmental outcomes in early and middle childhood (Duncan, Brooks-Gunn, and Klebanov 1994; Miller and Korenman 1994; Smith, Brooks-Gunn, and Klebanov 1997). Similar studies focusing on adolescent outcomes such as completed schooling and out-of-wedlock childbearing tend to find much weaker effects of income (Haveman and Wolfe 1995). Yet because the adolescent-based studies rarely have measures of parental-family income from early childhood, it is not known whether poverty early in childhood has noteworthy effects on later outcomes.

Second, little of this research has employed techniques to eliminate biases associated with the omission of typically unmeasured factors such as parental ability, mental health, or altruism in putting the needs of their children's development before their own needs.

Third, although this work has provided a rough guide to the magnitude of the income effect, it has not revealed the processes by which economic conditions affect children. If, for example, income is important because it enables families to provide richer learning environments for their children, then policies that enrich learning environments directly might be more efficient in meeting child-development goals than would a more general redistribution of income.

We use whole-childhood data from the Panel Study of Income Dynamics (PSID) to relate children's completed schooling and nonmarital fertility to parental income during middle childhood, adolescence, and, for the first time, very early childhood. Our analyses use both individual-based models and models based on sibling differences in schooling and parental income.

BACKGROUND

Several recent review articles (Corcoran 1995; Haveman and Wolfe 1995) and books (Mayer 1997) summarize the voluminous literature linking family income and developmental outcomes in adolescence and early adulthood. The consensus is that: (1) the effects of parental income vary from one outcome to another; (2) for achievement-related outcomes such as completed schooling and early-adult labor market success the estimated effects of parental income are usually statistically significant, but there is little consensus regarding the size of these effects; and (3) by not attending to the confounding effects of unmeasured parental and neighborhood characteristics, even the mostly modest estimates of the effects of parental income may be upwardly biased.

The comprehensive review by Haveman and Wolfe (1995) illustrates the first two of these points:

> With but one exception . . . , the family income variable is positively associated with the educational attainment of the child, and the variable is statistically significant in more than half of all cases where a positive relationship is estimated. Simulated changes in family economic

[1] For example, the modest correlations between income and other measures of parental socioeconomic status enabled Sewell and Hauser (1975) to conclude, "There can be little doubt that the association of socioeconomic background variables with son's earnings is due solely to the intergenerational effect of parents' income, while the latter cannot to any large extent be explained by the differing abilities, educational attainments, or occupational achievements of the sons of rich and poor families" (p. 84).

> resources, however, are associated with relatively small changes in educational attainments. The range of elasticities is wide—about .02 to .2. (P. 1856)

With respect to its relationship to out-of-wedlock childbearing,

> . . . parental income is negative and usually, but not always, significant . . . . The few reports of the quantitative effects of simulated changes in variables suggest that decreases in parental income . . . will lead to small increases in the probability that teen girls will experience a nonmarital birth. (Haveman and Wolfe 1995: 1863)

Recent Research

More recent contributions to this literature include a coordinated analysis by 12 groups of researchers working with 10 different developmental data sets, most of which offer longitudinal measurement of parental family income as well as measurements of the achievement, behavior, or health of individuals at various points in life (Duncan and Brooks-Gunn 1997). Some outcomes, such as IQ at age 2 and motor development between birth and age 3, were measured in the first years of a child's life. Others, such as career attainment and mortality, were measured as late as the sixth decade of life.

A common element across these studies is a "replication" analysis in which the same measures—family income, maternal schooling, family structure—were included in a regression model predicting child and adult outcomes. Taken as a whole, the results suggest that family income at times had large but rather selective effects on children's attainments. Most noteworthy was the importance of the type of outcome being considered. Family income had its largest correlations with children's ability and achievement measures. In contrast, virtually none of the behavior, mental health, or physical health measures represented by the 12 developmental studies were predicted strongly by family income.

Second, the childhood stage at which income was measured was clearly significant. Family economic conditions in early and middle childhood appeared to be far more important for shaping ability and achievement than were economic conditions during adolescence. In fact, *none* of the achievement studies using exclusively adolescence-based income measures found large effects. In contrast, all of the studies of ability with income measured during early childhood found large effects.[2] Left unanswered in these and all other analyses is the importance for adolescent and early-adult outcomes of family economic conditions in the earliest stages of childhood.[3]

Smith et al. (1997) provide a useful set of benchmarks for the sizes of the effects of income on ability and achievement in early childhood. They draw data on parental socioeconomic status and ability and achievement measures from the National Longitudinal Survey of Youth and the Infant Health and Development Program. All of the tests were independently normed with means of 100 and standard deviations of around 15. To allow for a nonlinear relationship between income and achievement, Smith et al. use regressions in which family income between birth and the time of the test (adjusted for family size) is represented as a series of dummy variables and that also control for differences in the child's race, birth weight, age, and gender, as well as for the mother's education and family structure.

When compared with children in families with incomes between 1.5 and 2.0 times the poverty line, children in families with incomes less than one-half of the poverty line were found to score between 6 and 13 points lower on the various standardized tests. In all cases, these differences were statistically sig-

[2] Income effects were considered to be "large" if the regression-adjusted changes in the dependent variable associated with substantial income changes—(1) an additional $10,000 of income, (2) an increase in family income from below the poverty line to between the poverty line and twice the poverty line, and/or (3) a change from persistent poverty to no poverty—amounted to at least one-quarter of a standard deviation for most of the dependent variables used in a particular analysis.

[3] Haveman, Wolfe, and Spaulding (1991) estimated the effects of a combined poverty and welfare measure averaged over ages 4 through 7. Haveman and Wolfe (1994) estimated a stage-specific model of the effects of poverty alone, but the earliest measurement of it is at child's age 6.

nificant. Children in families with incomes closer to but still below the poverty line also did worse than children in the higher income reference group; the differences were smaller, although usually, but not always, statistically significant. The smallest differences appeared for the earliest (age 2) measure of cognitive ability, although there was no monotonic increase across the data in the estimated effect of poverty with the age of the child. Also noteworthy is the fact that the associations between family poverty and cognitive ability appear to be just as large for full-scale IQ measures as for the reading and math achievement tests. These findings are consistent with the hypothesis that increasing the incomes of children whose family incomes are below or near the poverty line will have a larger impact on early-childhood ability and achievement than would increasing the incomes of children in middle-class and affluent families.[4]

Some research has attempted to explain why economic conditions appear to affect achievement. Consistent with a number of other studies, Smith et al. (1997) find that the quality of the home environment—its opportunities for learning, the warmth of mother-child interactions, and the physical condition of the home—accounts for a substantial portion of the powerful effects of family income on cognitive outcomes. Specifically, differences in the home environments of high- and low-income children explained close to one-half of the effects of income on the cognitive development of preschool children and between one-quarter and one-third of the effects of income on the achievement scores of elementary school children. Thus, in the case of the cognitive development of preschoolers, income matters to a substantial degree because it is associated with a richer learning environment for the children.

Other studies have found evidence that low income produces economic pressures that lead to conflict between parents over financial matters (Conger, Conger, and Elder 1997; Conger et al. 1992, 1993). This, in turn, increases the harshness of the mother's parenting and undermines the adolescent's self-confidence and achievement. Specifically, a family's income level is a powerful predictor of the reported economic pressure felt by family members. Economic pressure has both direct and indirect effects on adolescent achievement. Parental financial conflicts were particularly detrimental to the self-confidence and achievement of boys.

Are the Income "Effects" Causal?

Much of the existing empirical literature consists of regressions relating developmental outcomes to parental income and a modest set of socioeconomic and demographic control variables. As such, they show the associations between parental income and various outcomes for children, after the regression techniques adjust statistically for measured socioeconomic and demographic differences between high- and low-income families.

A persistent concern with these kinds of analyses is that the estimated effect of income might be spurious, caused by the mutual association that parental income and the outcomes for children share with some unmeasured "true" causal factor. Suppose, for example, that the mental health of parents is the key ingredient for children's success and that measures of parental mental health were not included in the models. Because positive mental health in parents is likely to make parents more successful in the labor market as well as to lead to fewer problems with their children, the absence of adjustments for differences in parental mental health may produce a serious overstatement of the role income plays in causing children's success.

[4] Smith et al.(1997) show somewhat larger effects than those found in some of the other studies using the National Longitudinal Survey of Youth. Blau (1995) summarizes much of this literature with calculations of cognitive test score changes associated with a $10,000 increase in family income. Typical of the estimated effects are those of Korenman and Miller (1994), who report that a $10,000 increase in permanent income is associated with one-fifth of a standard deviation in outcomes when income is initially less than one-half the poverty line, but less than one-tenth of a standard deviation when initial income is well above the poverty line. Blau's analysis shows how much more responsive the test scores are to long-run income than to income measured in a single year near the time the test was administered.

Randomized experiments constitute one solution to this omitted-variables problem. The negative-income-tax experiments conducted in the 1970s provide inconclusive evidence on the effects of experimental increases in income on children's outcomes (Currie 1995). Substantial income effects were found on children's nutrition, early school achievement, and high school completion in some sites, but not in others. Because the site with the largest effects for younger children (North Carolina) was also the poorest, one interpretation of the results is that income effects are largest for the poorest families.

To illustrate nonexperimental solutions to the problem of omitted-variable bias, consider a simple model in which achievement at time t ($\textit{Ach}_t$) is a function of lifetime income up to point t ($\Sigma \textit{Income}_t$), a permanent and observed component of other aspects of parental background (*ParSchool*), an unobserved permanent family-specific component (*Fam*), an unobserved permanent individual component (*Ind*), and a random error term (ε_t):

$$\textit{Ach}_t = \alpha + \beta_1 \Sigma \textit{Income}_t + \beta_2 \textit{ParSchool} + \beta_3 \textit{Fam} + \beta_4 \textit{Ind} + \varepsilon_t. \tag{1}$$

Time-varying measures of family conditions (e.g., maternal employment) could be added to this model as well, although this raises issues of whether such conditions are jointly determined as part of the process by which families develop a strategy for having and raising their children (Blau 1995).

Much of the recent work relating family income to developmental outcomes is based on estimating a version of this equation that omits and fails to adjust otherwise for the effects of the unmeasured family and individual variables. One way around this omitted-variables problem is to estimate change models. If the relationship in equation 1 holds, say, five years later, at $t + 5$, then we have:

$$\textit{Ach}_{t+5} = \alpha + \beta_1 \Sigma \textit{Income}_{t+5} + \beta_2 \textit{ParSchool} + \beta_3 \textit{Fam} + \beta_4 \textit{Ind} + \varepsilon_{t+5}. \tag{2}$$

Differencing these two equations eliminates the confounding effects of *Fam* and *Ind* (as well as *ParSchool*) and gives the following equation relating change in cognitive ability to the total income between t and $t + 5$:

$$\Delta \textit{Ach}_{t,t+5} = \beta_1 \Delta \Sigma \textit{Income}_{t,t+5} + \Delta \varepsilon_{t,t+5}, \tag{3}$$

where $\Delta X_{t,t+5}$ indicates the change in a variable from year t to year $t + 5$.

In their analysis of the effects of persistent poverty on IQ at age 5 and behavior problems, Duncan et al. (1994) estimate such an equation based on change data between ages 3 and 5 and find highly significant effects of parental income between children's ages 3 and 5 on changes in IQ between ages 3 and 5. Results for the estimated effects of income on changes in behavior problems were in the expected direction, but were not significant at conventional levels.

Change models estimated on nonexperimental data are not without their problems, as one still must worry about the source of the changes in the right-hand-side variables (Heckman and Robb 1985). In the context of developmental changes, one needs to make sure that the motivations, conditions, and events causing the income change either did not affect development directly or are somehow controlled for in the statistical analysis.

Another model-based approach is to estimate a level equation like equation 1, but to attempt to remove the spurious correlation between income and development through an instrumental-variables procedure. This procedure amounts to replacing the lifetime income variable ($\Sigma \textit{Income}$) with an instrumental variable that is purged of $\Sigma \textit{Income}$'s spurious correlation with unobserved factors such as family (*Fam*) and child's achievement (*Ach*). The trick is to find a variable that is highly correlated with $\Sigma \textit{Income}$ but is not highly correlated with the unobservable components of family (*Fam*) and individual (*Ind*). This task is difficult because almost all correlates of $\Sigma \textit{Income}$ are arguably correlates of unobserved determinants of children's development as well.

Mayer (1997) provides a set of tests for omitted-variable bias, including the addition of measures of parental income *after* the occurrence of the outcome as well as only those components of parental income that are fairly independent of the actions of the family. In the first case, she argues that future income cannot have caused the prior outcome, so that its inclusion adjusts for unmeasured characteristics of the parents. The addition of future income almost always produces a large reduction in the estimated effect of prior parental income; thus she concludes that much of the estimated effect of income from replication models is spurious.

In the second case, the argument is that the level of income components such as welfare and earnings (as well as the children's outcomes under study) may reflect the effects of important unmeasured parental characteristics. If components such as asset income are less affected by these unmeasured parental characteristics, their coefficients ought to provide a better gauge of "true" income effects. Following this procedure, Mayer finds small and often nonsignificant coefficients for these income components.

As Mayer points out, these procedures are not without their problems. If families anticipate future income changes and adjust their consumption accordingly, and the consumption changes benefit or hurt children, then future income does indeed play a causal role. The likely measurement error in income sources such as dividends and interest will impart a downward bias in their coefficients. Moreover, interest and dividends are almost universally absent from the income packages of families at or below the poverty line.

Another approach to eliminating bias is to use sibling differences. Suppose the relationship in equation 1 holds for two siblings, A and B, within the same family, and that the measured and unmeasured characteristics of the family do not change from one sibling to the next and:

$$\textit{Ach}_A = \alpha + \beta_1 \Sigma \textit{Income}_A + \beta_2 \textit{ParSchool} + \beta_3 \textit{Fam} + \beta_4 \textit{Ind}_A + \varepsilon_A; \quad (4)$$

$$\textit{Ach}_B = \alpha + \beta_1 \Sigma \textit{Income}_B + \beta_2 \textit{ParSchool} + \beta_3 \textit{Fam} + \beta_4 \textit{Ind}_b + \varepsilon_b; \quad (5)$$

Differencing across sibling pairs eliminates the Fam and ParSchool components and leaves:

$$\textit{Ach}_A - \textit{Ach}_B = \beta_1 (\Sigma \textit{Income}_A - \Sigma \textit{Income}_B) + \beta_4 (\textit{Ind}_A - \textit{Ind}_B) + (\varepsilon_A - \varepsilon_B). \quad (6)$$

Key to the estimation of this formulation is sufficient variability between siblings in their family income histories between birth and the point of measurement of *Ach*—a condition that is obviously not met in the case of twins, but is met in the case of nontwin siblings. If, as seems reasonable, sibling differences in the unobserved individual component (*Ind*) are largely independent of income differences, then estimating equation 6 with sibling data produces estimates of income effects that are largely free from the confounding effects of unobserved family characteristics.

DATA

Data for our analysis of these issues come from the Panel Study of Income Dynamics (PSID), a longitudinal survey of U.S. households. Since 1968 the PSID has followed, interviewed annually, processed, analyzed, and disseminated information from a representative sample of about 5,000 families (Hill 1992). Splitoff families are formed when children leave home, when couples divorce, and when more complicated changes break families apart. This procedure produces an unbiased sample of families each year as well as a continuously representative sample of children born into families each year.

The PSID's original design focused on poverty by oversampling low-income and minority households. Weights have been created and are used here to adjust for the original oversampling of the poor and for differential attrition.[5]

Our individual-based analyses use the sample of 1,323 children born between 1967 and 1973 and present in the PSID between birth and age 20. Our sibling analyses are based on the 328 sibling pairs drawn from the individual-based sample. Given the cohort range chosen, siblings cannot be more than six years apart in age. Barring nonresponse problems that are not corrected by

[5] For completed schooling, we use the individual weight associated with the interview year in which the schooling was reported. For nonmarital births, we use the individual weight associated with the interview year in which the most recent marital and fertility histories were reported.

Table 1. Intercorrelations, Means, and Standard Deviations for the Family Income Variables: Panel Study of Income Dynamics

	Correlation Coefficients (Zero-Order)					
Income Variable	(1)	(2)	(3)	(4)	Mean	S.D.
(1) Family income at ages 0 to 5	1.00	—	—	—	3.70	2.10
(2) Family income at ages 6 to 10	.82	1.00	—	—	4.56	3.07
(3) Family income at ages 11 to 15	.72	.87	1.00	—	5.20	4.14
(4) Family income at ages 0 to 15	.87	.96	.95	1.00	4.49	2.91

Note: Family income is in $10,000s (1993 dollars).

our weighting adjustments, the experiences of this group of children ought to be nationally representative of the cohorts from which they were sampled. The sibling sample represents sibling pairs drawn from these cohorts, but not the more general set of children in these cohorts. These data enable us to test for the relative importance of family income in early and middle childhood as well as adolescence in explaining two important outcomes—years of completed schooling and the timing of a first nonmarital birth.

Most of our analyses use measures of schooling and fertility ascertained as recently as possible in the PSID. This is typically at age 25 or later—earlier only in the cases of individuals who were lost to attrition between the year they turned 20 and the 1995 interviewing wave. Our event-history analysis of nonmarital fertility begins at age 16 and is censored by attrition from the study, a marital birth, or the termination of a first marriage into which no children were born.

Our income measure is the total pretax income of all family members, inflated to 1993 price levels using the Consumer Price Index (CPI–UX1) and averaged over all the years of childhood or over all the years within the given childhood stage under consideration. A common practice in studies like ours is to use a size-adjusted measure of family income, typically the "income-to-needs" ratio, obtained by dividing total household income by the official U.S. poverty threshold corresponding to the size of the given household. A disadvantage of this formulation is that the ratio imposes restrictions on the size of the separate effects of income and family size. In our analyses, we include income and family size as separate variables.

Our analyses include control variables for race, gender, number of siblings, the completed schooling of the mother, the age of the mother at the time of the child's birth, whether the family ever lived in the South, family structure, maternal employment, and residential mobility. Our family structure measures are a series of dummy variables indicating whether the child was born into a nonintact family, and stage-specific measures of whether the child's parents experienced a divorce or remarriage. Maternal employment is captured by stage-specific measures of the number of years in which the mother worked 1,000 or more hours. Residential mobility is measured with stage-specific counts of the number of years in which the family reported a residential move. In the case of stage-specific analyses, the variables are measured over three age ranges: birth to age 5, ages 6 to 10, and ages 11 to 15.

RESULTS

Income Correlations across Childhood

We began our analysis with an investigation of the nature of family income across all childhood stages. Table 1 shows that the average family income increases substantially across childhood. Over the entire sample, income averaged across ages 11 to 15 is some 40 percent higher than income averaged across ages 0 to 5. Zero-order correlations of five-year average family incomes across the childhood stages are high—.82 and .87 for adjacent stages and .72 for average family incomes between child's birth and age 5 and between ages 11 to 15.

Table 2. Percentage Distribution of Family Income at Child's Ages 11 to 15 by Family Income at Child's Ages 0 to 5: Panel Study of Income Dynamics

	Family Income at Ages 0 to 5			
Family Income at Ages 11 to 15	Percent Less than \$15,000	Percent \$15,000 to \$24,999	Percent \$25,000 to \$34,999	Percent \$35,000 or more
Less than \$15,000	39.0	15.1	2.2	1.4
\$15,000 to \$24,999	33.2	31.4	10.7	3.2
\$25,000 to \$34,999	15.7	17.7	21.7	7.7
\$35,000 or more	12.1	35.9	65.4	87.8
Total	100.0	100.0	100.0	100.0

Note: Family income is in 1993 dollars.

Despite what appear to be high correlations, there was considerable movement of families across income classes. Table 2 cross-classifies family incomes averaged across child's ages 0 to 5 and 11 to 15. Only a minority (39.0 percent) of children with family incomes below \$15,000 in early childhood still had incomes that low in adolescence, and more than one-quarter (27.8 percent) of the initially low-income children had incomes in adolescence that were \$25,000 or more.

Year-to-year income changes also produce considerable differences in the income experiences of siblings (data not shown). Roughly one-fifth of the sibling pairs in our sibling sample had average family incomes between birth and age 5 that differed by more than \$5,000, while roughly one-quarter experienced income differences that large in the second and third childhood stages. When taking childhood as a whole, nearly one-half of the siblings had 15-year average incomes that differed by more than \$5,000.

Whole-Childhood Income Effects

Table 3 presents results from regressions fitting various functional forms for average total family income at child's ages 0 to 15: (1) OLS models predicting years of completed schooling; (2) logistic models predicting the successful completion of high school; and (3) Cox models of the timing of a first nonmarital birth. Control variables common to all the regressions are: the child's race and sex, total number of siblings, whether the family head was black, maternal years of schooling, age of the mother at the time of the child's birth, whether the child ever lived in the South, family structure, maternal employment, and residential mobility. Descriptive statistics and estimated coefficients for these control variables from a subset of the models are presented in Appendix A.

Model 1 for each dependent variable reports the coefficient and standard error on average annual family income in linear form and scaled in \$10,000s, 1993 dollars. As with past studies, income has a statistically significant but substantively modest impact on the outcome variables. An additional \$10,000 of family income is associated with a .14-year increase in years of schooling completed, a 26-percent (i.e., $e^{.23}$) increase in the odds of completing high school, and a 35-percent (i.e., $1 - e^{-.43}$) drop in the relative risk of a first nonmarital birth.

In Model 2 we allowed the effect of income to vary with the level of income by fitting a two-segment spline function, with separate slopes for children in families with average total incomes under and over \$20,000. The first coefficient represents the estimated slope (with income scaled in \$10,000s) for the under-\$20,000 group, and the second coefficient represents the *difference* in slope between the over-\$20,000 and under-\$20,000 groups. This nonlinear form clearly fits the schooling data better, with much bigger estimated impacts for income increments for low-income than middle-income and high-income families. In the case of the nonmarital fertility model, the log-likelihoods for the linear and spline models are identical. For chil-

Table 3. Coefficients from the Regression of Child's Outcome Variables on Family Income at Ages 0 to 15: Panel Study of Income Dynamics

	Dependent Variables/Models											
	Years of Completed Schooling[a]				High School Completion[b]				Hazard of Nonmarital Birth[c]			
Independent Variable	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Family Income at Child's Ages 0 to 15												
Linear function	.14*	—	—	—	.23*	—	—	—	−.43*	—	—	—
	(.02)				(.07)				(.10)			
Spline function												
Income < $20,000	—	1.30*	—	—	—	1.97*	—	—	—	−.50	—	—
		(.29)				(.44)				(.41)		
Difference between income < $20,000 and > $20,000	—	−1.17*	—	—	—	−1.84*	—	—	—	−.08	—	—
		(.30)				(.46)				(.44)		
Natural logarithm	—	—	1.16*	—	—	—	1.35*	—	—	—	−1.18*	—
			(.11)				(.26)				(.26)	
Dummy Variables for Family Income												
$15,000 to $24,999	—	—	—	.82*	—	—	—	1.41*	—	—	—	−.54
				(.27)				(.38)				(.35)
$25,000 to $34,999	—	—	—	1.41*	—	—	—	1.83*	—	—	—	−.94
				(.28)				(.43)				(.41)
$35,000 to $49,999	—	—	—	1.69*	—	—	—	2.48*	—	—	—	−1.44*
				(.28)				(.45)				(.43)
$50,000 and over	—	—	—	2.35*	—	—	—	2.64*	—	—	—	−2.40*
				(.29)				(.49)				(.54)
Adjusted R^2	.192	.201	.219	.216	—	—	—	—	—	—	—	—
−2 Log likelihood	—	—	—	—	718.9	702.6	701.1	694.6	1,266.1	1,266.1	1,271.1	1,267.3

Note: Numbers in parentheses are standard errors. In Model 4, the omitted category for family income is "less than $15,000." The mean years of schooling completed was 13.5 (S.D. = 2.1); the mean rate of high school completion was .90 (S.D. = .30).

[a] OLS models; N = 1,323.

[b] Logistic models; N = 1,323.

[c] Cox models; N = 620.

*$p < .05$ (two-tailed tests)

dren in low-income families, a $10,000 increase in family-income is associated with 1.3 years of additional schooling, an effect that is nearly 10 times as large as the estimated impact from the linear form. Income increments for children in high-income families have a significantly smaller impact—only .13 (1.30 − 1.17) additional years of schooling per $10,000 income increment.[6]

The spline for high school completion also indicates a much larger incremental effect—a seven-fold increase in the odds of graduat-

[6] Although the .13 difference is small relative to the standard errors of the spline coefficients, its significance is better judged relative to the standard error (.02) of the linear income measure presented in Model 1. To investigate the bias in studies based on family income measured only in adolescence, we estimate completed-schooling models using the linear and spline functions and family income averaged between ages 11 and 15, the same demographic controls but no other income-related measures. We found that the linear effect was 64 percent (.09/.14) as large for the 11–15 age period versus the 0–15 age period. The coefficient on the first spline segment was 65 percent as large (.85/1.30).

ing per $10,000 increment—for low-income as compared to high-income children. In contrast to the school-related outcomes, the hypothesis of a linear effect of income cannot be rejected for nonmarital childbearing.

Model 3 indicates that the pattern of diminishing returns to increments in family income is approximated reasonably well with a logarithmic transformation of family income. In fact, the fit of the schooling models (but not the nonmartial fertility model) is better with the log form of income than with the spline function.

A disadvantage of the log form is that it does not isolate the portion of the income distribution producing the biggest impact on the dependent variable. For this reason, we also estimated a more flexible parameterization of the income-outcome relationships—a series of dummy variables, the results of which are presented in Model 4. Children in families with annual incomes that averaged less than $15,000 constitute the omitted group in these regressions. In contrast to these low-income children, children in families with incomes between $15,000 and $25,000 completed .82 years more schooling and enjoyed 4.1 times greater odds of completing high school, but had an insignificant lower risk of a nonmarital birth.[7] Schooling differences between the $15,000–$24,999 and $25,000–$34,999 groups were more than one-half of a year and were statistically significant at the $p < .01$ level. In the case of completing high school, there were much smaller improvements in the odds of graduating associated with income increases other than those at the very bottom of the income distribution.

Stage-Specific Income Effects

To allow for the differential impact of income by childhood stage, we estimated a second set of regressions that included measures of family income averaged over the first, second, and third five-year segments of the children's lives (Table 4). In all other respects, the regression models are identical to the whole-childhood regressions presented in Table 3. Because a given five-year average income level produces one-third the total childhood income of that same income level averaged over 15 years, a stage-specific model in which income was constant and timing did not matter will produce stage-specific income coefficients that are roughly one-third the size of a whole-childhood model.

Taken as a whole, the results show that timing matters a great deal for the schooling outcomes; income increments early in life for children in low-income families are associated with large increments to completed schooling. For example, the spline model suggests that, controlling for income in other stages, a $10,000 increment to income averaged over the first five years of life for children in low-income families is associated with an increment of .81 years in completed schooling and an increase of 2.9 times in the odds of finishing high school. These estimated effects are much larger than the corresponding estimated effects of income measured between child's ages 6 to 10 and 11 to 15.[8] The logarithmic version of the model shows that income during adolescence has an effect as powerful as income in early childhood for years of completed schooling. In the case of high school graduation, parental income during adolescence is much less important, suggesting that adolescent-based parental income is more important for college-related decisions (see below).

The more flexible dummy-variable version of the model (Model 4) confirms the greater importance of economic conditions during the first five years of life for completed

[7] The mean incomes of children in the < $15,000, $15,000–$24,999, $25,000–$34,999, $35,000–$49,999, and >$50,000 income groups were $11,403, $19,996, $30,553, $41,906, and $74,739, respectively. Thus, the increment in average income associated with membership in the first two income groups was about $8,600, while the increment associated with membership in the highest two income groups was much larger—about $32,800.

[8] Although larger, the coefficients for the dummy variables for family income at child's age 0 to 5 were never *significantly* larger than the coefficients for the corresponding dummy variables for ages 6 to 10 and 11 to 15. However, a model that includes dummy variables for ages 0 to 5 family income categories and dummy variables for family income averaged over the 10-year period between ages 6 and 15 produces significant differences between all corresponding sets of coefficients.

Table 4. Coefficients from the Regression of Child's Outcome Variables on Childhood-Stage-Specific Family Income: Panel Study of Income Dynamics

	Dependent Variables/Models											
	Years of Completed Schooling[a]				High School Completion[b]				Hazard of Nonmarital Birth[c]			
Independent Variable	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Family Income at Child's Ages 0 to 5												
Linear function	.12* (.05)	—	—	—	.38* (.13)	—	—	—	−.16 (.14)	—	—	—
Spline function												
Under $20,000	—	.81* (.28)	—	—	—	1.05* (.43)	—	—	—	−.03 (.37)	—	—
Difference between income < $20,000 and > $20,000	—	−.72* (.29)	—	—	—	−.85 (.48)	—	—	—	−.16 (.42)	—	—
Natural logarithm	—	—	.54* (.18)	—	—	—	1.07* (.35)	—	—	—	−.37 (.31)	—
Dummy variables for family income												
$15,000 to $24,999	—	—	—	.66* (.25)	—	—	—	.56 (.36)	—	—	—	.10 (.32)
$25,000 to $34,999	—	—	—	.73* (.28)	—	—	—	1.15* (.44)	—	—	—	−.26 (.40)
$35,000 to $49,999	—	—	—	.78* (.30)	—	—	—	1.58* (.52)	—	—	—	−.97 (.47)
$50,000 and over	—	—	—	1.41* (.33)	—	—	—	1.53* (.67)	—	—	—	−1.13 (.67)
Family Income at Child's Ages 6 to 10												
Linear function	−.01 (.04)	—	—	—	−.07 (.10)	—	—	—	.06 (.13)	—	—	—
Spline function												
Under $20,000	—	.45 (.36)	—	—	—	.22 (.28)	—	—	—	−.21 (.45)	—	—
Difference between income < $20,000 and > $20,000	—	−.47 (.36)	—	—	—	−.30 (.30)	—	—	—	.30 (.48)	—	—
Natural logarithm	—	—	−.06 (.12)	—	—	—	−.18 (.40)	—	—	—	.20 (.36)	—
Dummy variables for family income												
$15,000 to $24,999	—	—	—	.16 (.30)	—	—	—	.80* (.44)	—	—	—	−.21 (.36)
$25,000 to $34,999	—	—	—	.24 (.35)	—	—	—	.32 (.53)	—	—	—	−.09 (.45)
$35,000 to $49,999	—	—	—	.44 (.38)	—	—	—	.36 (.62)	—	—	—	.22 (.35)
$50,000 and over	—	—	—	.33 (.40)	—	—	—	.32 (.72)	—	—	—	.89 (.62)

(Table 4 continued on next page)

(Table 4 continued from previous page)

	Dependent Variables/Models											
	Years of Completed Schooling[a]				High School Completion[b]				Hazard of Nonmarital Birth[c]			
Independent Variable	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Family Income at Child's Ages 11 to 15												
Linear function	.05* (.02)	—	—	—	.06 (.08)	—	—	—	−.29* (.09)	—	—	—
Spline function												
Under $20,000	—	.32 (.27)	—	—	—	.42 (.24)	—	—	—	−.05 (.38)	—	—
Difference between income < $20,000 and > $20,000	—	−.26 (.27)	—	—	—	−.40 (.26)	—	—	—	−.27 (.41)	—	—
Natural logarithm	—	—	.57* (.14)	—	—	—	.58 (.29)	—	—	—	−.89* (.26)	—
Dummy variables for family income												
$15,000 to $24,999	—	—	—	.34 (.27)	—	—	—	.38 (.41)	—	—	—	.22 (.54)
$25,000 to $34,999	—	—	—	.41 (.29)	—	—	—	.96 (.49)	—	—	—	−.16 (.40)
$35,000 to $49,999	—	—	—	.36 (.31)	—	—	—	.62 (.52)	—	—	—	−1.02 (.48)
$50,000 and over	—	—	—	1.08* (.32)	—	—	—	1.08 (.59)	—	—	—	−1.67* (.54)
Adjusted R^2	.192	.215	.220	.232	—	—	—	—	—	—	—	—
−2 Log likelihood	—	—	—	—	713.1	695.8	697.6	688.0	1,262.6	1,262.1	1,268.1	1,255.5

Note: Numbers in parentheses are standard errors. In Model 4, the omitted category for family income is "less than $15,000."

[a] OLS models; N = 1,323.

[b] Logistic models; N = 1,323.

[c] Cox models; N = 620.

*$p < .05$ (two-tailed tests)

schooling. Children with family incomes in early childhood in the $15,000–$24,999 range average .66 years more schooling than children in the lowest income group. In the case of high school graduation, income increments have similar effects across the three lowest income categories. With the exception of high-income adolescents, there was little consistent evidence of income effects on completed schooling in other stages of childhood. And with the exception of high parental income during early childhood and adolescence, stage-specific income failed to predict nonmarital childbearing.

To better understand the apparent effect on completed schooling of high parental income during adolescence, we estimated logistic regressions for college attendance and college completion (results not shown). The coefficient on the high-income-during-adolescence dummy variable was highly significant (and positive) in the college attendance model, but not in the college completion model. Thus, the primary way in which well-to-do parents of adolescents appear to affect completed schooling is by enabling their children to enter college.

We investigated whether the effects of family income varied across important demographic subgroups and found little evidence that this was the case for whole-childhood income. For example, the coefficient on the log of whole-childhood family income in the completed-schooling model was 1.16 (Table 3, Model 3). The corresponding coefficient for whites was 1.20; for blacks, .89; for females, 1.30; and for males, 1.04. In the stage-specific models of completed schooling, the coefficients on log income associated with the three childhood stages were .54, –.06 and .57, respectively (Table 4, Model 3). Corresponding coefficients for whites were .40, .00, and .77; for blacks, .96, .08, and .19; for females, .33, .20, and .71; and for males, .71, –.18, .61. Standard errors for these coefficients were in the .2 to .3 range, so one should not overinterpret these differences.

In light of the fact that some of our control variables could be viewed as endogenous, we estimated a version of our completed-schooling model that included a more limited set of predictors—the child's sex and total number of siblings, whether the family head was black, maternal years of schooling, age of the mother at the time of the child's birth, and whether the child ever lived in the South. The key coefficients on the stage-specific dummy variables differed only slightly from those presented in Table 4. For example, the new coefficients (compared with Table 4 coefficients in parentheses) for the child's age 0 to 5 family income categories were .70 (.66), .80 (.73), .86 (.78), and 1.52 (1.41). As with the estimates in Table 4, none of the coefficients for the child's ages 6 to 10 family income categories was statistically significant at a conventional level. For the child's ages 11 to 15 income categories, only the coefficient associated with the highest income dummy variable was significant, with a magnitude of 1.18 (versus 1.08 in Table 4).

Given the complications associated with the low-income portion of the PSID sample, we investigated the robustness of the findings by estimating (without weighting) the stage-specific completed-schooling models on the 681 observations from the cross-section portion of the sample. Not surprisingly, standard errors were considerably larger, but the pattern of coefficients, particularly for the first stage of childhood, was similar. The increments in schooling associated with income increases from less than \$15,000 to between \$15,000 and \$25,000 for the three stages were .88, –.35, and .99 years, respectively, with standard errors in the .4 to .6 range. The increments in schooling associated with income increases from less than \$15,000 to more than \$50,000 for the three stages were 1.39, .01, and 1.66, respectively.

Sibling Models

Last, we estimated a series of sibling models by drawing the 328 sibling pairs from the 1,323 children used in the individual-based models (Table 5). Model 1 includes only sibling differences in ages 0 to 15 average family income and sex (same-sex siblings were coded 0, female/male pairs were coded +1 and male/female pairs were coded –1). To adjust for important events that might have produced the income changes, Model 2 adds sibling differences in age of the mother at the time of the birth, and stage-specific differences in family structure, years of full-time maternal work, and the number of residential moves. Models 3 and 4 repeat these analyses but allow for differences in childhood-stage-specific average income.

Note the assumptions implicit in these sibling models. In particular, these models assume that the effects of nonconstant family variables are the same for each sibling, regardless of sex and position in the birth order. In addition, we restrict our analysis to linear effects of income. Our relatively small sample sizes precluded a more complete analysis; extensions along these lines are clearly warranted.[9]

In the whole-childhood income models (Models 1 and 2), the estimated effect of sibling differences in income (coefficient = .22) on differences in schooling completed was

[9] We attempted to fit a spline function to these sibling data to allow for different effects of positive and negative income differences. The coefficients and standard errors on the first segments were similar to those presented in Table 5. The standard errors on the second segments were too large (around .80) to provide any precision in the estimated coefficients. We also fitted a log model to these sibling data. The results were similar, although not as significant. In the case of the age 0 to 15 family income model with covariates, the coefficient and standard error on the income variable were .96 and .70, respectively. In the case of the stage-specific log model, coefficients and standard errors were .50 (.33), .16 (.36), and –.30 (.37).

Table 5. Coefficients from the Regression of Years of Schooling Completed on Selected Independent Variables: Siblings from the Panel Study of Income Dynamics

Difference between Siblings in:	Model 1	Model 2	Model 3	Model 4
Family income at ages 0 to 15	.22* (.08)	.20* (.09)	—	—
Family income at ages 0 to 5	—	—	.15* (.07)	.18* (.09)
Family income at ages 6 to 10	—	—	.01 (.06)	-.01 (.07)
Family income at ages 11 to 15	—	—	.06 (.06)	.04 (.08)
Sex	.34* (.11)	.36* (.11)	.33* (.11)	.33* (.11)
Age of mother at child's birth	—	.03 (.06)	—	.02 (.05)
Born into a nonintact family	—	.05 (.27)	—	.03 (.24)
Ever divorced, ages 0 to 5	—	–.25 (.44)	—	–.56 (.44)
Ever divorced, ages 6 to 10	—	–.35 (.41)	—	–.52 (.36)
Ever divorced, ages 11 to 15	—	–.48 (.34)	—	–.68* (.30)
Ever (re)married, ages 0 to 5	—	.57 (.48)	—	.64* (.32)
Ever (re)married, ages 6 to 10	—	.61 (.64)	—	–.07 (.53)
Ever (re)married, ages 11 to 15	—	.36 (.38)	—	–.53 (.33)
Years moved, ages 0 to 5	—	–.06 (.10)	—	–.07 (.10)
Years moved, ages 6 to 10	—	–.06 (.12)	—	.00 (.11)
Years moved, ages 11 to 15	—	–.15 (.12)	—	.23* (.12)
Years mother worked 1,000 hours or more, ages 0 to 5	—	.01 (.12)	—	–.02 (.09)
Years mother worked 1,000 hours or more, ages 6 to 10	—	–.08 (.08)	—	–.09 (.09)
Years mother worked 1,000 hours or more, ages 11 to 15	—	–.06 (.08)	—	–.13 (.09)
Constant	.37* (.37)	.41* (.14)	.37* (.08)	.26 (.13)
Adjusted R^2	.044	.038	.044	.044

Note: Numbers in parentheses are standard errors. All income measures are scaled in $10,000s (1993 dollars). N = 328.

*$p < .05$ (two-tailed tests)

somewhat larger than the corresponding coefficient (.14) in the individual-based model presented in Table 3. The standard error was also larger, although still less than one-half of the coefficient estimate. Adjustments for differences in family conditions had almost no effect on the income coefficient.

Estimates from the childhood-stage-specific income difference models were somewhat sensitive to the treatment of the few sibling pairs with large income differences. The results for Models 3 and 4 impose no truncation on the outliers and show, once again, that economic conditions are most important in early childhood. Controls for correlated family conditions increase slightly the coefficient estimate for early-childhood income.[10]

SUMMARY AND DISCUSSION

In our examination of links between family income and child development, we have summarized recent contributions to the literature and conducted new empirical work. Striking consistencies have emerged.

An important "stylized fact" in the recent literature is that family income has much stronger associations with achievement and ability-related outcomes for children than with measures of health and behavior. A second noteworthy result is that early childhood appears to be the stage in which family economic conditions matter the most. And third, the estimated impact of family income on completed schooling appears to be larger for children in low-income families than those in high-income families.

Our PSID-based analyses of the effects of family income during childhood on completed schooling and nonmartial fertility were consistent with each of these points. We found that family income had a stronger association with completed schooling than with nonmarital fertility. A second result was clear evidence that family income in early childhood had a bigger impact on completed schooling than did income during middle childhood. At the high end of the socioeconomic scale, our evidence suggests that entry into college is facilitated if parental income during adolescence is high. And third, the impact of family income on completed schooling was largest for children in low-income families.

Our attempt to use sibling differences to eliminate the influence of unmeasured persistent family characteristics from our estimated effects of income was only partially successful. Results from our sibling-based models were not inconsistent with results from our individual-based models; however, the imprecision of the estimates left the results far from definitive.

That high parental income during adolescence facilitates entry into college is not surprising. Why income early in childhood appears to matter more for achievement than for behavior may be due to the importance of school readiness in determining the course of schooling for children. Income poverty has a strong association with a low level of preschool ability, which is associated with low test scores later in childhood as well as grade failure, school disengagement, and dropping out of school, even when controls for family characteristics such as maternal schooling, household structure, and welfare receipt are included (Brooks-Gunn, Guo, and Furstenberg 1993; Guo, Brooks-Gunn, and Harris 1996).

Why might this be the case? Preschool ability sets the stage for children's transition into the formal school system. Children who have not learned skills such as color naming, sorting, counting, letters, and the names of everyday objects are at a disadvantage compared with children who have mastered these skills. Schools tend to classify children very early—language arts groups are often formed in kindergarten or first grade. Teachers also tend to identify children as having potential school problems in the first years, with these ratings being at least as predictive as reading- and math-readiness test scores (Entwistle and Alexander 1989).

The same is not as true for behavior problems. The correlations between preschool behavior problems and elementary school behavior problems are not as strong as those found for achievement (Guo et al. 1996). Moreover, behavior problems seem to be more strongly influenced than is school achievement by other family events (Campbell 1995; Links 1983; Sameroff et al. 1993). Other contextual factors gain in importance as children age—peers have a major impact on juvenile delinquency, for example. Thus, it may be possible for a child with moderate levels of behavior problems in the early years to have no such problems at the end of elementary school, while children with moderate readiness problems are less likely to be able to catch up in the academic sphere.

[10] Truncating income changes to be no more than $20,000 in absolute value produced coefficients and standard errors associated with income during the three childhood stages of .20 (.13), .16 (.13), and –.02 (.11), respectively, in models controlling for the full set of life events.

Taken as a whole, our data are consistent with the hypothesis that raising the incomes of poor families will enhance the abilities and attainments of their children. Most important appears to be the elimination of deep and persistent poverty during a child's early years. Income increments to nonpoor families or to families with older children may be desirable on other grounds, but do not appear particularly effective in enhancing children's achievement or changing their behavior.

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Appendix A. Means, Standard Deviations, and Unstandardized Coefficients for Control Variables Included in Model 4 of Table 4

		Coefficient		
Independent Variable	Mean (S.D.)	Years of Completed Schooling	High School Completion	Hazard of Nonmarital Birth
Child's Race/Gender[a]				
Nonblack male	.46 (.51)	–.33* (.11)	–.53* (.25)	—
Black male	.07 (.27)	.18 (.23)	.06 (.42)	—
Black female	.07 (.26)	.07 (.22)	–.23 (.40)	.51* (.28)
Total number of siblings	2.31 (1.89)	–.12* (.03)	–.05 (.06)	.28* (.05)
Mother's years of schooling	12.65 (2.06)	.18* (.02)	.16* (.06)	–.06 (.05)
Age of mother at child's birth	24.56 (7.87)	.00 (.01)	–.05 (.06)	–.04* (.02)
Nonmissing data on age of mother at child's birth	.96 (.20)	.68 (.38)	1.46* (.74)	–.09 (.66)
Ever lived in South	.37 (.49)	–.05 (.11)	–.47* (.22)	–.33 (.22)

(Appendix A continued on next page)

(Appendix A continued from previous page)

		Coefficient		
Independent Variable	Mean (S.D.)	Years of Completed Schooling	High School Completion	Hazard of Nonmarital Birth
Child's Family Structure				
Born into a nonintact family	.14 (.36)	−.24 (.18)	−.08 (.33)	.29 (.29)
Ever divorced, ages 0 to 5	.09 (.29)	.14 (.22)	.16 (.43)	.87* (.39)
Ever divorced, ages 6 to 10	.08 (.28)	.15 (.23)	.17 (.43)	.42 (.36)
Ever divorced, ages 11 to 15	.07 (.27)	.16 (.22)	.59 (.46)	.70 (.38)
Ever (re)married, ages 0 to 5	.06 (.24)	−.37 (.26)	−.26 (.47)	−.40 (.47)
Ever (re)married, ages 6 to 10	.06 (.24)	.10 (.26)	.18 (.51)	−1.14* (.47)
Ever (re)married, ages 11 to 15	.07 (.26)	.01 (.24)	−.74 (.45)	−.01 (.41)
Residential Mobility				
Years moved, ages 0 to 5	1.30 (1.28)	−.05 (.05)	−.24* (.09)	−.01 (.09)
Years moved, ages 6 to 10	.87 (1.14)	−.05 (.05)	−.12 (.10)	.02 (.09)
Years moved, ages 11 to 15	.69 (1.08)	−.13 (.06)	−.16 (.10)	.16 (.09)
Maternal Employment				
Years mother worked 1,000 hours or more, ages 0 to 5	1.04 (1.55)	.00 (.12)	.13 (.10)	−.09 (.09)
Years mother worked 1,000 hours or more, ages 6 to 10	1.59 (1.90)	−.29* (.12)	−.01 (.09)	.01 (.09)
Years mother worked 1,000 hours or more, ages 11 to 15	2.38 (2.08)	.20 (.13)	.07 (.07)	.08 (.07)

Note: For coefficients, numbers in parentheses are standard errors. N = 1,323.

[a] For child's race/gender, the omitted category is "nonblack female."

* $p < .05$ (two-tailed tests)

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