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Carlos Díaz-Alejandro Lecture

What You Don't Know CAN Hurt You—Or at Least Mislead You:

Family Behaviors, Unobserved Heterogeneities, and the Determinants and Impacts of Human Resources over the Life Cycle

"What you don't know can't hurt you" is a common saying, variants of which date back at least to the sixteenth century.[1] However, a common theme in my research on Latin America and the Caribbean, for which I am honored to receive the Latin American and Caribbean Economic Association's (LACEA) 2008 Carlos Díaz-Alejandro Prize, is that what you don't know can, indeed, hurt you—or at least mislead you in interpreting empirical research. In this Carlos Díaz-Alejandro Prize Lecture, I first briefly recap a number of milestones in my journey as an empirical microeconomist involved in Latin America and the Caribbean, then I present a simple framework for analyzing household investments in human resources to illustrate why what you don't know can mislead you in the presence of unobserved heterogeneities, and finally I give an number of related empirical illustrations based on my research in Latin America and the Caribbean as well as elsewhere.

My involvement in analyzing Latin American and Caribbean economic issues has been a long journey, and it is still under way. Along the way, I have worked with many fellow travelers in many countries, institutions, and projects. Here I highlight a few of the larger projects before returning to the main theme of the paper.

From 1968 to 1970, with research continuing until 1976, I participated in a project coordinated and funded by the Chilean National Planning Office (ODEPLAN), Massachusetts Institute of Technology (MIT), and the Ford

Behrman is with the University of Pennsylvania.

1. The *Wordsworth Dictionary of Proverbs* gives an example written by G. Pettie in 1576: "So long as I know it not, it hurteth me not" (Apperson, 2003, p. 321).

Foundation, in which I was involved in empirical macroeconomic modeling with a focus on adjustments within a macroeconomic context.[2] Near the end of this period, I also undertook the Chilean country study in the National Bureau of Economic Research (NBER) multi-country comparative project on "Foreign Trade Regimes and Economic Development," headed by Jagdish Bhagwati and Anne O. Krueger.[3] My colleagues in the ODEPLAN project, whom I first met over forty years ago in Chile, included Carlos Díaz-Alejandro, in whose honor the LACEA prize was established, and Edmar Bacha, the first recipient of this prize in 1998.

From 1975 to 1987, I spearheaded the Nicaragua Socioeconomic Survey of Women, an interdisciplinary survey including data on a subsample of adult sisters. The data support analyses that control for unobserved components of common childhood family background in estimating the impacts of human resources.[4]

From 1980 to the present, I have participated in World Bank projects on a range of topics on the microeconomics of human resources in Latin America, some of which are reviewed below. Research topics included Brazilian school quality, the Brazilian governmental social welfare function underlying the distribution of public resources for schooling, Brazilian cohort effects and geographical effects, the impact of a Bolivian early childhood development program, attrition in longitudinal surveys in developing countries including Bolivia, the impact of human capital investments in Latin America and elsewhere, and the impact of birth weight over the life cycle in developing countries including Latin America.[5]

From 1982 to the present, I have been involved in a series of Inter-American Development Bank (IDB) and IDB Research Networks projects on macroeconomic modeling, human resource investments, early life nutrition, preschool child development, quality of life, intergenerational mobility, social protection, and distribution.[6]

2. See, for example, Behrman (1971, 1972a, 1972c, 1973a, 1973b, 1973c, 1977); Behrman and Garcia (1973).

3. Behrman (1976).

4. Bardhan, Behrman, and Fishlow (1987); Behrman and Wolfe (1984a, 1984b, 1984c, 1984d, 1984e, 1987a, 1987b, 1989); Blau, Behrman, and Wolfe (1988); Wolfe and Behrman (1982, 1983, 1984, 1986, 1987, 1992).

5. Behrman and Birdsall (1983, 1985, 1988a, 1988b); Birsdall and Behrman (1984); Behrman, Cheng, and Todd (2004); Alderman and others (2001); Knowles and Behrman (2004, 2005); Alderman and Behrman (2006).

6. See, for example, Behrman (1993, 1996, 1999); Behrman, Birdsall, and Pettersson (2009); Behrman, Birdsall, and Kaplan (1996); Behrman, Birdsall, and Székely (2000, 2007); Behrman, Duryea, and Maluccio (2008a, 2008b); Behrman, Duryea, and Székely (2002, 2003a, 2003b, 2004); Behrman, Gaviria, and Székely (2001, 2003); Behrman and Skoufias (2004); Graham and Behrman (2009); Parker, Behrman, and Rubalcava (2008).

From 1996 to the present, I have worked with a team evaluating Mexico's PROGRESA (now called *Oportunidades*), a program of human resource investment and anti-poverty conditional cash transfers. The evaluation was initially coordinated by the International Food Policy Research Institute (IFPRI) and subsequently continued by the Instituto Nacional de Salud Pública (INSP). Our research emphasized schooling and early life nutrition.[7]

From 2000 to the present, I have been involved with the Universidad de Chile and the government of Chile in implementing the Social Protection Survey. This collaborative data collection served as the basis for studies on pension systems, schooling, early life education, and evaluation.[8]

From 2000 to the present, I have worked with the Institute of Nutrition of Central America and Panama (INCAP) on the Guatemalan Longitudinal Nutritional Study. The study included data collection and the analysis of long-run effects of early life nutrition using longitudinal data for the period 1969–2007, together with the examination of various related structural relations.[9]

In an ongoing project that started in 2003, I currently am involved in longitudinal studies and evaluations under the auspices of the Universidad de los Andes and Fedesarrollo in Colombia.

In another ongoing project, dating to 2006, I am researching ways to align incentives for learning in upper secondary schools, in collaboration with the Mexican Ministry of Education.

The remainder of this paper summarizes some of the studies from the projects cited above in terms of their relevance for the theme in the title. The next section presents a simple model of intrahousehold allocations that illustrates the possible importance of unobserved factors such as abilities in determining human resources and their impacts. The paper then summarizes empirical estimates from the region and elsewhere on empirical topics related to five areas: preferences and intrahousehold allocations; schooling returns; school quality; early life nutrition; and program evaluation. The final section brings the journey thus far to a close with the conclusions.

7. Papers include Behrman (1998, 2000, 2007); Behrman, Fernald, and others (2009); Behrman, Gallardo-García, and others (2009); Behrman and Hoddinott (2005); Behrman and Parker (2007, 2009); Behrman, Parker, and Todd (2007, 2008, 2009); Behrman and Skoufias (2006, 2009); Behrman, Sengupta, and Todd (2005); Behrman and Todd (1999a, 1999b); Parker and Behrman (2008); Skoufias, Davis, and Behrman (1999).

8. Arenas de Mesa and others (2007); Bravo and others (2006).

9. Behrman (2008, 2009); Behrman, Gallardo-García, and others (2009); Behrman, Hoddinott, and others (2008); Behrman, Murphy, and others (2008); Engle and others (2007); Grajeda and others (2005); Hoddinott and others (2008); Hoddinott, Behrman, and Martorell (2005); Maluccio and others (2009); Martorell and others (2005); Quisumbing and others (2005); Stein and others (2005).

A Simple Model of Intrahousehold Allocations of Human Resource Investments

Households are the proximate determinants of most human resource investments, given household resources (and the distribution of those resources among household members with different preferences and allocation rules), relevant production technologies, market prices, and policies (including the local availability of public services related to human resource investment, such as schools and health clinics). Here I consider a simple static model of intrahousehold allocations of human resource investments between two children, developed by Behrman, Pollak, and Taubman, to illustrate the possible importance of unobserved factors for interpreting empirical estimates.[10]

In the Behrman-Pollak-Taubman (BPT) model, parents maximize a welfare function, W, that depends, among other things, on their children's expected adult full incomes Y_1 and Y_2, so that

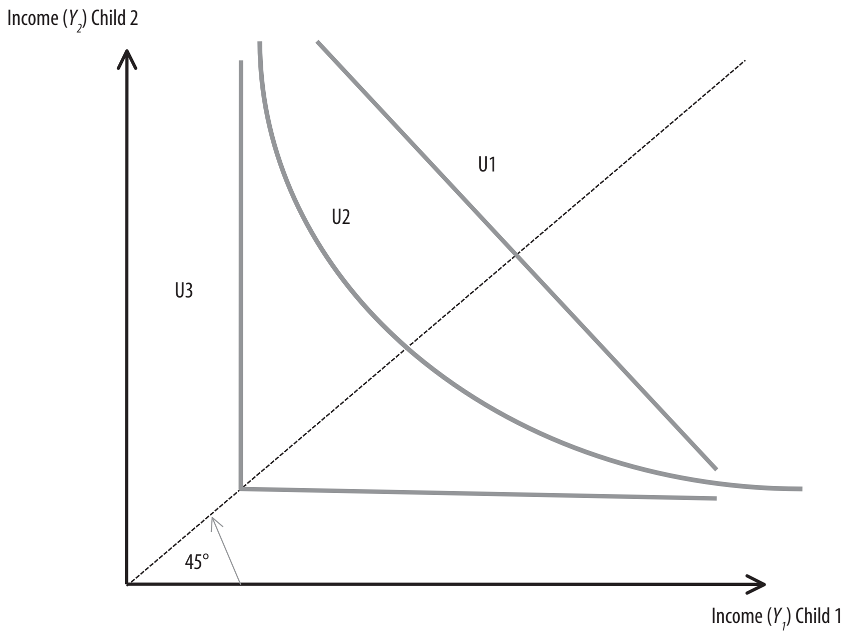
$$W = W\left(Y_1, Y_2 \ldots\right), \tag{1}$$

subject to production function constraints and resources allocated to children.[11] The subwelfare function, defined over Y_1 and Y_2, has two basic characteristics: inequality aversion and equal concern (see figure 1). Inequality aversion is related to the curvature and therefore the substitutability in parental welfare between outcomes for one child versus another. Figure 1 illustrates the two extreme cases and an intermediate case for a two-child family. In the figure, U_1 represents one extreme case in which all the parents

10. Behrman, Pollak, and Taubman (1982). The two-child case illustrates the basic points and permits a simple geometric presentation with no loss of generality.

11. This welfare function may reflect bargaining between the parents if they have different preferences, but that possibility is not central to the emphasis here and is not made explicit in what follows for simplicity. I survey some of these issues in Behrman (1997) and present some related empirical estimates in Behrman and Rosenzweig (2006). The standard definition of full income includes the value of all of an individual's time (whether working or used in leisure). This definition does not include income generated by nonhuman assets. Behrman, Pollak, and Taubman (1982) argue that the parental subwelfare function might be defined over their children's full income without including nonhuman assets because the parents may hold the view that it is good to "earn your own way," so that study considers the value of an individual's human assets separately (and separably) from the value of nonhuman assets. The authors therefore refer to their model as a separable earnings-transfers (SET) model. Below I discuss briefly the implications of simply summing the returns from children's human and nonhuman assets in Becker and Tomes' (1976) and Becker's (1991) wealth model.

FIGURE 1. Alternative Parental Subwelfare Functions



care about is the total expected full income of their children, so the welfare curve is linear. This implies that the parents reinforce endowment differences by investing most in the most able child if the full-income production technology is such that innate endowments complement human resource investments. U_3 is the other extreme case in which all the parents care about is the expected full income of their worst-off child, so that the welfare curves are *L* shaped (Rawlsian). This implies that, starting at the corner, there is no parental welfare gain to investments that increase the expected full income of one child without increasing the expected full income of the other child; in this case, parents compensate for endowment differences by investing in the least able child if the full-income production technology is such that innate endowments complement human resource investments. U_2 is the intermediate case, in which parents are neutral in the sense that they neither reinforce nor compensate for endowment differences among their children.

Equal concern is related to the symmetry of the parental welfare curves around the forty-five degree ray from the origin, as in the three curves of figure 1. There would be unequal concern favoring child 1, for example, if the curves were shifted to the right. In such a case, U_3 would still be L shaped, but the corner would indicate higher expected full income for child 1 than for child 2, rather than equal expected full incomes for the two children as in figure 1.

Full income when the children become adults is produced by cognitive skills (CS) and noncognitive skills (NCS), which are produced by vectors of school-age, pre-school-age, and post-school-age experiences ($\mathbf{E}_S$, $\mathbf{E}_{S-}$, and $\mathbf{E}_{S+}$, respectively) and endowments ($\mathbf{E}_0$) such as genetics, with possible dynamic interactions:

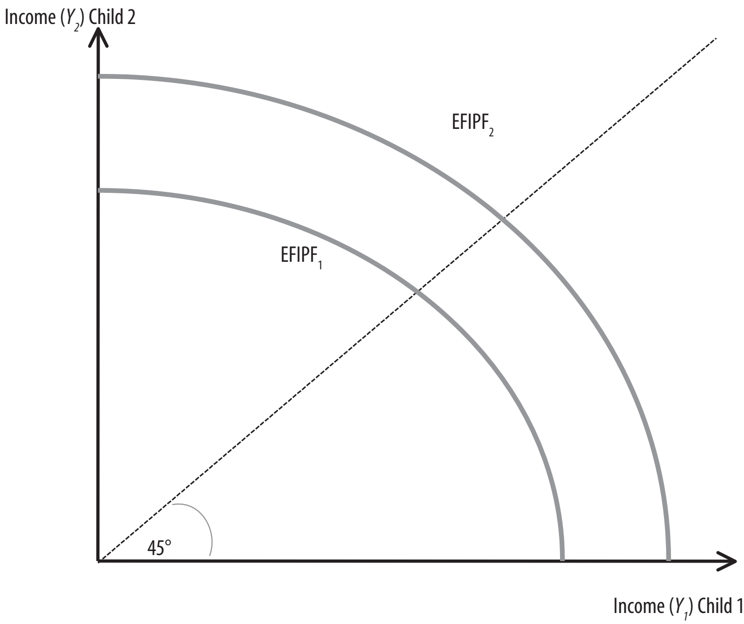
$$Y = Y\left(CS, NCS\right) = Y\left(\mathbf{E}_{S-}, \mathbf{E}_S, \mathbf{E}_{S+}, \mathbf{E}_0\right); \tag{2a}$$

$$CS = CS\left(\mathbf{E}_{S-}, \mathbf{E}_S, \mathbf{E}_{S+}, \mathbf{E}_0\right); \quad NCS = NCS\left(\mathbf{E}_{S-}, \mathbf{E}_S, \mathbf{E}_{S+}, \mathbf{E}_0\right). \tag{2b}$$

$\mathbf{E}_{S-}$ includes factors such as early life nutrition, stimulation, and infectious disease experience; $\mathbf{E}_S$ includes factors such as time in school, school quality, and home support for learning in the school ages; $\mathbf{E}_{S+}$ includes factors such as formal training and learning-by-doing through work and other time uses; and $\mathbf{E}_0$ includes genetic and other endowments.

Parents and children are assumed to be unable to borrow on capital markets to finance human resource investments in the children. The resources that parents devote to children, child endowments, human resource production functions, and inputs into those production functions (including publicly provided human-resource-investment services such as schools and clinics) and relevant prices yield the expected full income possibility frontier (EFIPF), which gives the maximum combinations of expected full incomes for the children. Figure 2 provides an illustration with two alternative frontiers. These frontiers are both concave because of the assumption of diminishing marginal returns to investments in child human resources, given fixed endowments. The frontier is further from the origin (for example, EFIPF_2 rather than EFIPF_1) if parents devote more resources to the children, public provision of resources for the children is better (in terms of either quantity or quality), prices for the relevant inputs are lower, the rates of return to the human resource investments are higher, or the children have greater endowments. The figure is drawn as if the expected full income possibilities for the two children are symmetrical around the forty-five degree ray from the origin, as would be the case if the child endowments were equal (for example, identical twins) and equal inputs and equal rates of return prevailed for the two children. The figure is

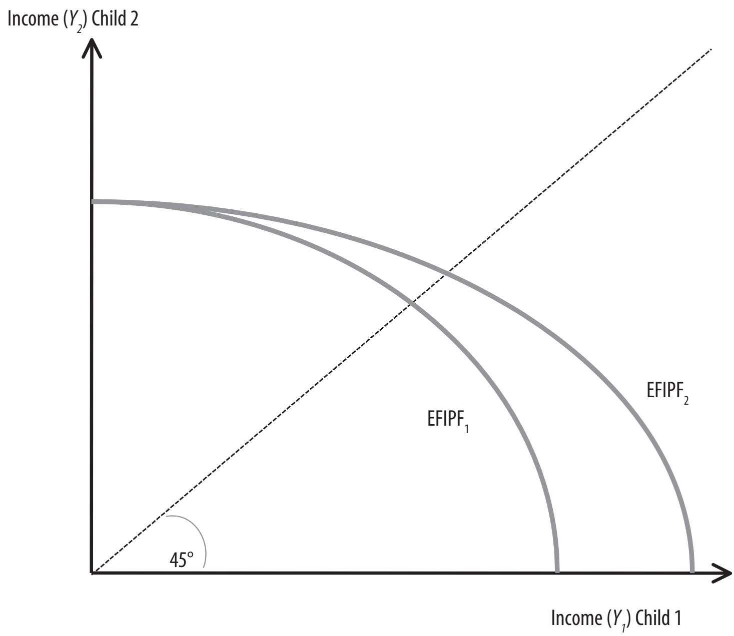
FIGURE 2. Expected Full Income Possibility Frontiers: Greater Options for Family 2 than for Family 1



also drawn as if whatever differences result in EFIPF$_2$ being further from the origin than EFIPF$_1$ affect the two children approximately equally.

In general, however, the frontiers are not symmetrical around the forty-five degree ray from the origin. Figure 3 illustrates the case in which the frontiers are elongated in the direction of child 1. This may occur because child 1 has greater endowments, better publicly provided human resource inputs, lower prices for the inputs, or higher rates of return to human resource investments than child 2. In general, with the possible exception of identical twins, genetic endowments vary among children in the same family even if there are strong average familial genetic effects. Other factors may differ among children within a family if, for example, there are gender differences in human resource services or in rates of return to human resource investments or if there are market or policy or household locational changes over time that affect children born at different times differentially. If any of the factors underlying the frontiers is better for child 1 with no changes for child 2, then the frontier is more elongated in the direction of child 1 (for example, EFIPF$_2$ rather than EFIPF$_1$).

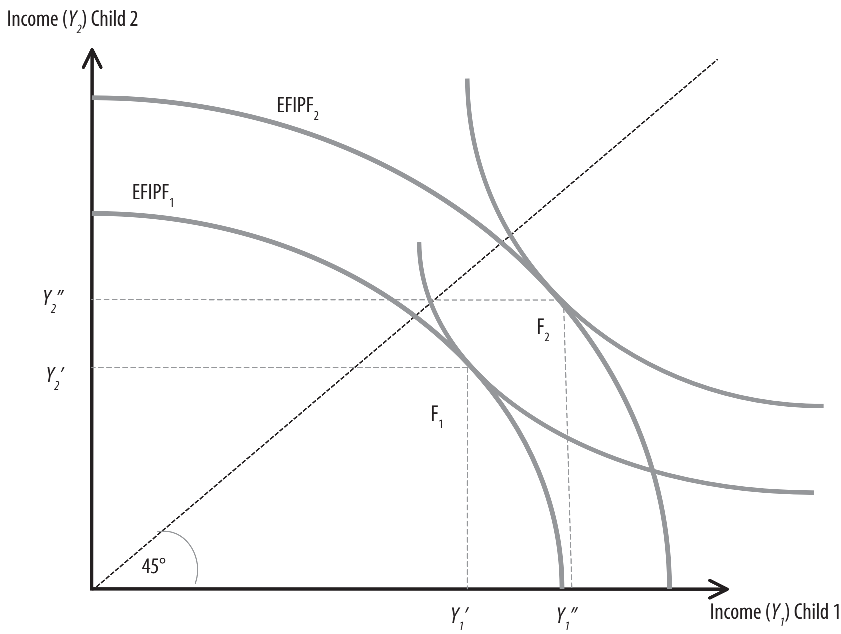
FIGURE 3. Expected Full Income Possibility Frontiers: Greater ($EFIPF_2$) vs. Fewer ($EFIPF_1$) Options for Child 1



Constrained maximization of the parental subwelfare function defined over the expected full income of their children subject to the relevant expected full income possibility frontier gives equilibrium investment in and expected full incomes for the children under the maintained assumption that the parents cannot impose binding commitments on their children to make transfers among the children when they become adults.[12] Figure 4 gives two examples for two families with different frontiers. As noted above, the frontier for one family may be outside of that for another family for a number of reasons, such as greater resources devoted to children, lower prices for human resource inputs,

12. The frontiers are defined here to be conditional on the total resources that the parents provide for human resource investments in their children and on the number of their children, both of which are choices that the parents make with trade-offs with their own consumption and possibly other arguments in their welfare functions. For simplicity and without important loss of generality regarding the points emphasized here, I am assuming a two-step maximization in which the parents first decide on their number of children and the total resources that are used to invest in the human resources of those children and then maximize their subwelfare function defined over the full incomes of their children.

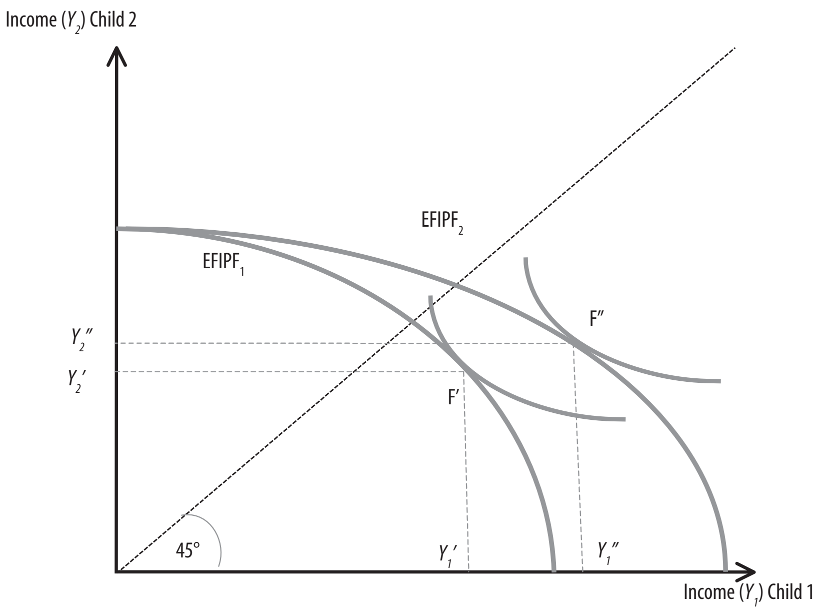
FIGURE 4. Constrained Maximization across Families with the EFIPF of Family 2 outside of Family 1



greater child endowments, higher rates of return to human resource investments, and better publicly provided human-resource related services. Whatever combination of such reasons explains the higher frontier for one or the other family, if the parental welfare functions are identical or sufficiently similar, the expected full incomes of both children are likely to be higher, as in figure 4 (that is, point F_2 rather than point F_1). This may reflect policies fairly directly (for example, differences in the public provision of human-resource-related services) or more indirectly (for example, differences in parental resources devoted to children as a result of differences in parental income stemming from different development policies).

In general, if there is some change in markets or policies that makes one child have higher expected full income, the effective increase in family resources is likely to make the other child better off, the more so the more the parental welfare function exhibits greater inequality aversion. Figure 5 provides an illustration in which there is a shift from EFIPF$_1$ to EFIPF$_2$, perhaps as a result of improved human-resource-related public services that affect

FIGURE 5. Constrained Maximization within Family with Alternative EFIPFs for Child 1

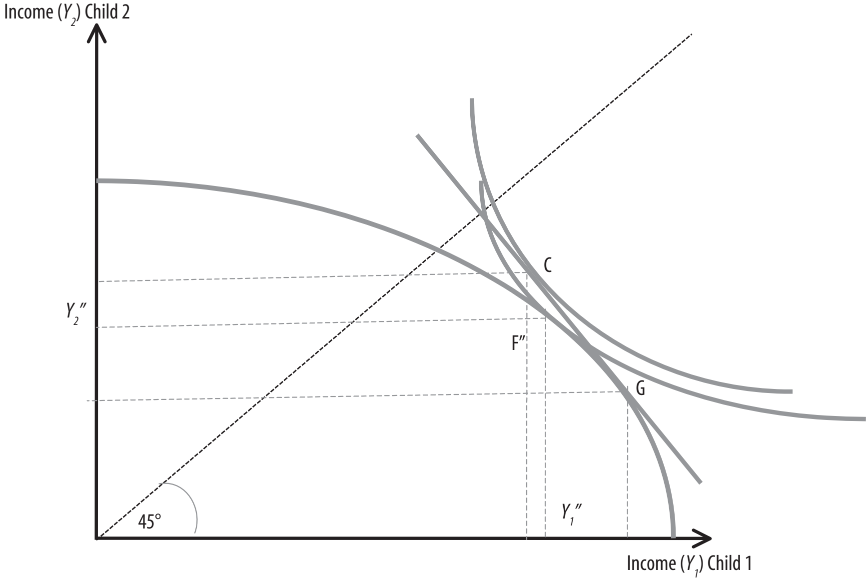


only child 1. In this case, the equilibrium moves from F' to F'', with both children gaining in terms of expected full incomes though with child 1 gaining more. The greater the parental inequality aversion (that is, the more sharply curved the parental welfare curves), the more the gain will be shared with child 2, all else equal. If the parental welfare function exhibits Rawlsian curvature and equal concern, then child 2 will gain the same as child 1 from a change that elongated the frontier only in the direction of expected full income for child 1. If the parental welfare function exhibits Rawlsian curvature and unequal concern favoring child 2, child 2 will have a greater increase in expected full income than child 1.

The constrained maximization implies reduced-form demand relations for outcomes (Z) such as Y, CS, and NCS and for endogenous inputs ($\mathbf{E}_{S-}$, $\mathbf{E}_{S}$, and $\mathbf{E}_{S+}$) into production functions with price (P), policy (POL), exogenous resource variables (R), and stochastic terms (U) on the right side. From a dynamic perspective, the right variables should be for all periods (actual for current and past periods and expected for future periods):

(3) $$Z = Z\left(P_i, POL_i, R_i, U_i\right),$$

FIGURE 6. Constrained Maximization with Transfers between Children at *C*, with no Transfers at *F″*



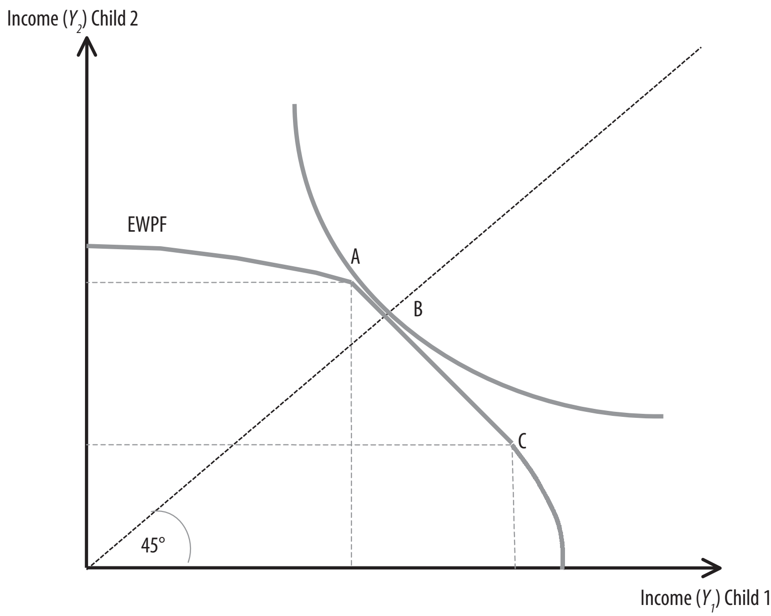
where i refers to all past, current, and future periods (with expected values for future periods). Under stronger assumptions, there may be conditional demand relations in which past stocks are sufficient statistics for past right-side determinants.

I now consider two variants related to the basic BPT model used thus far. First, parents can enforce transfers between their children, and, second, parents are interested not in the full income of the children, but their wealth. With regard to binding transfer commitments between the children, I have thus far assumed that parents cannot enforce binding transfers on their children. If they could, then they could determine an equilibrium outside of the frontier, all things equal. Figure 6 provides an illustration in which, rather than being limited to the zero-transfer full-income-consumption pair for the children at F'', the parents could guide investments toward the higher-endowed child 1, so the pretransfer full-income pair is at G (which itself is on a lower parental subwelfare curve than is F'') and enforce transfers from child 1 to child 2 (at a one-to-one rate, so the transfer line is at a forty-five degree slope). The parents then end up on a higher parental subwelfare function at point C. The possibility of parents being able to enforce binding transfers increases parental welfare and the expected full income (and presumably the welfare) of at least one child. The expected full income of the other child does not necessarily increase, however, and it may well be less than that child's expected full income without transfers. Indeed, figure 6 includes an example in which parents, with equal concern and less than extreme inequality aversion, enforce transfers so that post-transfer full incomes are equated on the forty-five degree ray from the origin, which is better than the nontransfer equilibrium at F'' for child 2, who is the recipient of the transfer, but worse for child 1, who is making the transfer. Such a possibility highlights the problems of enforcing the transfers, because child 1 would be worse off in this case than if there were no transfers. Of course, child 1 could possibly be better off with the transfers than without them; this would be the case, for example, if the frontier is so elongated to the right that where its slope is forty-five degrees (for the tangency with the transfer line), there is a tangency with the parental subwelfare function with equal concern at a higher level of expected full income for child 1 than at F''. In such a case, making and implementing a binding commitment would not be difficult if the only alternative were F'' because the parents and each of the children would be better off than at F''. If other options are allowed, such as at *G* on the elongated frontier, *G* would be better than *C* in terms of the expected full income for child 1, so there still is the problem of how to make commitments for such transfers binding. If the children innately are sufficiently altruistic about their siblings or if the parents can increase such altruism, then the parents may not need to be able to enforce such commitments because their children will provide for one another without binding commitments. If the parents can affect their children's altruism, it may be optimal for them to divert some of their resources from human resource investments in their children (thus having a frontier that is closer to the origin) to investment in developing sibling altruism of the child toward whom the frontier is elongated, so that higher welfare can be obtained by moving outside the frontier with transfers from that child to the other.

The second variant, in which parents are interested not in their children's wealth but rather their full income, is based on Becker and Tomes's wealth model.[13] The BPT model assumes that the arguments in the parental subwelfare function are children's full income (that is, based on human resources only). Becker and Tomes instead assume that the arguments in the parental

13. Becker and Tomes (1976).

FIGURE 7. Wealth Model with Sufficient Resources so Tangency on Linear Segment of Expected Wealth Possibility Frontier (EWPF)



subwelfare function are children's wealth, including income from both human and nonhuman assets. Figure 7 illustrates the Becker-Tomes wealth model for the case in which parents transfer (or invest) more than enough resources to both of their children to drive the marginal rate of return to human resource investments to the market rate (given diminishing returns owing to the fixed child endowments), beyond which the parents transfer nonhuman assets (that is, beyond points *A* and *C* in figure 7). In this case, there is a linear segment to the expected wealth possibility frontier (EWPF) because the market rate of interest earned by the nonhuman assets is constant no matter how those assets are distributed between the children. If the parents have equal concern and if the straight-line segment of the frontier crosses the forty-five degree ray from the origin (as in figure 7), the tangency of their welfare curves is on this straight line, and the parents allocate their investments in their children and their transfers of nonhuman assets to the children in order to equate the children's expected wealth. For each child, the parents invest in that child's human resources until the private rate of return to that investment equals the market rate of interest. Then the parents transfer sufficient nonhuman assets to that child to attain the desired expected wealth distribution between the children. This means that if the production technology is such that endowments and human resource investments are complements, the parents invest more in the human resources of the more able child (for example, child 1 in figure 7). If the parents have equal concern, however, they compensate by transferring more nonhuman assets to the less able child (for example, child 2). Thus, if the parents have equal concern and if the straight-line segment of the frontier crosses the forty-five degree ray from the origin, the equilibrium has unequal expected full incomes and unequal expected incomes from nonhuman assets, but equal expected wealth between the children, as at point *B* in figure 7. Investments in the children's human capital is efficient if the equilibrium is on the straight-line segment (including the endpoints) and if there are no distortions in any relevant prices, including the market interest rate and the prices for human-capital-related services such as those provided by schools and health clinics. However even if there are no distortions in prices, if parents do not invest "enough" in their children in the sense that they do not drive the marginal rates of return to human resource investments for both children to the market rate, then they provide no nonhuman assets to the child (children) for whom the marginal rate of return to human resource investments exceeds the market interest rates, and the parents' investments in human resources are not efficient for that child (children). If nonhuman assets are not transferred to either child, the wealth model reduces to the BPT model.[14]

The primary implication of these simple models for the purpose of this paper is that controlling for unobserved factors may be critical for understanding both the determinants of human resources and their impacts. In investigating the impact of parental schooling on child education, for example, the failure to control for child endowments or school quality is likely to bias the estimated impact because parental schooling is likely to be correlated with their own and therefore their children's endowments and with local school quality. Likewise, the failure to control for child endowments or school quality in investigating the impact of child schooling on subsequent outcomes such as earnings is likely to bias the estimated impact of schooling, because schooling attainment (grades completed) is likely to be correlated with their own endowments and with local school quality. The endowments, moreover, have both cross-family and within-family components, both of which may have important impacts. Finally, though the dynamics are not

14. Behrman, Pollak, and Taubman (1995).

developed in this section, experiences throughout the life cycle are likely to be important, as is suggested in the production functions in equation 2. If, for example, early-life investments are ignored, this may bias the estimated determinants and impacts of later experiences, such as schooling.

Empirical Estimates

This section uses the simple model discussed above as the starting point for reviewing selected empirical estimates related to household behaviors and exploring different substantive questions in the presence of unobserved variables from Latin America and the Caribbean and elsewhere. The studies reviewed here use alternative data collection and estimation strategies for controlling for the effects of unobserved variables that are of concern for the reasons suggested earlier. The strategies include within-sibling or twin estimates (fixed effects), instrumental variables, controlled experiments with random assignment, latent variables with multiple indicators, and marginal propensity score matching.

Preferences and Intrahousehold Allocations

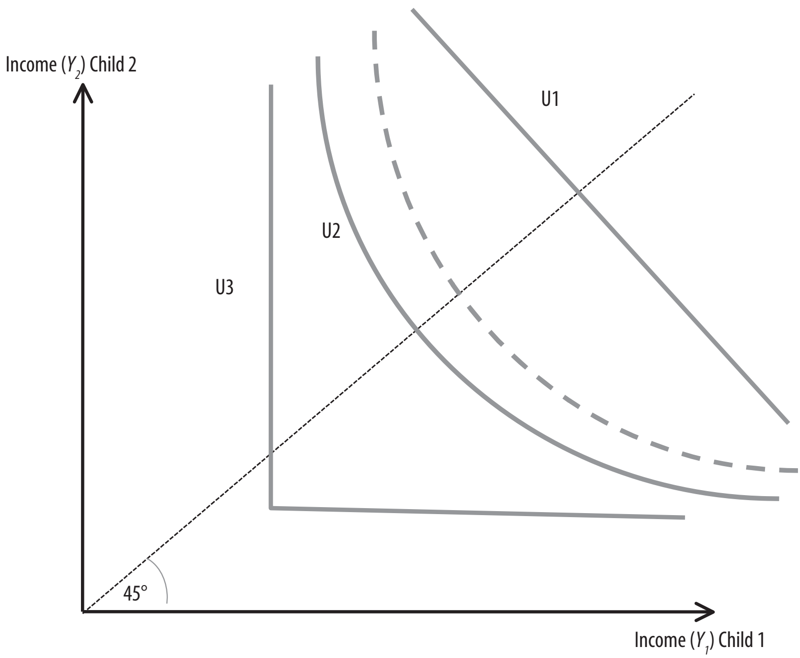
I start from the specific assumptions that the parental subwelfare function component of equation 1 is a constant elasticity of substitution (CES) function and the production functions in equation 2 for the outcomes that enter into parental welfare are log-linear in terms of human capital investments, endowments, and stochastic factors.[15] The critical parameters of the parental subwelfare function can then be estimated using within-sibling estimators to control for unobserved common family endowments.[16] Estimates for the United States and rural India reveal some interesting findings.[17] First, there

15. Parental subwelfare is assumed to be $\Sigma\alpha_i Y_i^c$, where α_i refers to equal concern and c to inequality aversion ($c = -\infty$ for Rawlsian, $c = 0$ for neutral, $c = 1$ for no inequality aversion).

16. The estimates in Behrman, Rosenzweig, and Taubman (1994) suggest that there could be individual-specific endowments beyond the family endowments. If so, within-sibling estimators may still have biases stemming from these individual-specific endowments even if they control for common family endowments. Identical-twin estimators arguably control for both common family and individual-specific endowments. The estimates reported below for the United States use identical twins. The estimates for India, however, use siblings, which may bias the estimated inequality aversion toward productivity concerns.

17. For the United States, see Behrman, Pollak, and Taubman (1982, 1986) and Behrman and Taubman (1986); for rural India see Behrman (1988a, 1988b).

FIGURE 8. Inequality Aversion between Neutral and Income Maximization Cases, Unequal Concern Favoring Child 1



is significant reinforcement of endowment differentials, but this reinforcement is tempered by some significant inequality aversion. Parental preference curves are like the dashed line in figure 8, between the pure investment strategy and the neutral case. As a result, genetic endowment effects are less than what they would be in the absence of inequality aversion. For the United States, the effects of a unit difference in genetic endowments would be about eleven times greater for schooling and 1.3 times greater for earnings if parents did not display significant inequality aversion. For rural India, the estimates indicate that inequality aversion is lower than in the United States but still significant, and it is lower in the lean season when food is scarce, when parents follow a strategy closer to pure investment.

Second, the studies report contrasting findings for unequal concern. For the United States, the estimates indicate that unequal concern favors girls in an extended model with both marriage outcomes and own-earnings outcomes, so in figure 8 child 1 would be a girl and child 2 would be a boy. Estimates for the United States also suggest that parental preferences favor higher birth orders, which tend to offset endowment differentials that favor lower birth orders. In India, in contrast, the estimated preferences favor males, with preference weights around 5 percent greater for boys than for girls for the same health outcomes in the lean season, implying that in figure 8 child 1 would be a boy and child 2 a girl. In India the promale preference weights are greater for lower caste and more-schooled household heads, and they favor lower birth-order children.

Another approach to estimating whether parents compensate for or reinforce endowment differentials is to use simplified forms of key relations in the model presented earlier and twins data.[18] In the semilog wage production function, the natural log wage of the *i*th individual from the *h*th family, Y_{ih}, depends on the *i*th individual's schooling, $\mathbf{E}_{S,ih}$, the *i*th individual's common family endowment, $\mathbf{E}_{0,h}$, the *i*th individual's individual-specific endowment, $\mathbf{E}_{0,ih}$, and a random disturbance term, v_{ih}:

$$Y_{ih} = \beta \mathbf{E}_{S,ih} + \mathbf{E}_{0,h} + \mathbf{E}_{0,ih} + v_{ih}. \tag{4}$$

The reduced-form relation for determining schooling for the *i*th individual (assuming a two-child family, *i* and *k*) indicates that schooling for the *i*th individual in the *h*th family, $\mathbf{E}_{S,ih}$, depends on the *h*th family common endowment, $\mathbf{E}_{0,h}$, the *i*th individual's specific endowment, $\mathbf{E}_{0,ih}$, the *k*th individual's specific endowment, $\mathbf{E}_{0,kh}$, and a random disturbance term, u_{ih}:

$$\mathbf{E}_{S,ih} = \gamma_1 \mathbf{E}_{0,h} + \gamma_2 \mathbf{E}_{0,ih} + \gamma_3 \mathbf{E}_{0,kh} + u_{ih}. \tag{5}$$

Cross-sectional estimates of the wage production function in relation 4 generally do not identify the wage impact of schooling, β, because, as indicated in relation 5, schooling, $\mathbf{E}_{S,ih}$, is correlated with the unobserved endowments, $\mathbf{E}_{0,h}$ and $\mathbf{E}_{0,ih}$. If the stochastic term in the schooling determination relation, u_{ih}, affects Y_{ih} only through $\mathbf{E}_{S,ih}$, however, β can be identified with identical (or monozygotic) twins:

$$\Delta^M Y_{ih} = \beta \Delta^M \mathbf{E}_{S,ih} + \Delta^M v_{ih}, \tag{4a}$$

where Δ^M is the within-identical-twins operator. The original and primary use of twin data in the economics literature has been to obtain consistent estimates

18. Behrman, Rosenzweig, and Taubman (1994).

of the schooling impact on earnings.[19] Sibling data generally do not permit the identification of β because the difference in sibling schooling, $\Delta\mathbf{E}_{S,ih}$, is correlated with the difference in unobserved sibling individual-specific endowments, $\Delta\mathbf{E}_{0,ih}$, based on relation 5. Thus while sibling estimators permit control for the common family component of endowments, they generally do not control for all endowments:

$$\Delta Y_{ih} = \beta\Delta\mathbf{E}_{S,ih} + \Delta\mathbf{E}_{0,ih} + \Delta v_{ih}, \tag{4b}$$

where Δ is the within-sibling operator.

Good estimates of relation 4a thus yield consistent estimates of the impact of schooling, β. Some estimation issues can arise, however, with this approach. First, as noted above, such estimates assume that the stochastic disturbance terms in the schooling determination relation (equation 5) and the wage production function (equation 4) are independent. This might happen, for example, if one twin in a pair were randomly assigned a more inspiring eighth grade biology teacher than the other, so that the former twin was inspired to obtain more schooling and only through that schooling obtained higher wages. If, instead, one twin obtained more schooling than the other because the latter had an illness or accident that not only reduced schooling but also had permanent effects that directly reduced wages, then the disturbance terms in the two relations are correlated and the within-identical-twins estimator overestimates the impact of schooling. Second, biases toward zero are increased with any fixed-effects estimator, including within-twins and within-sibling estimators, because the noise-to-signal ratio increases with the control for fixed effects. Many recent twins studies control for such random measurement error by using reports on schooling from other individuals as an instrument for own schooling in relation 4, under the assumption that the measurement errors in own reports and in other reports are uncorrelated.[20] Third, twins differ from other siblings in that they are born at the same time. In one respect, this is an advantage because they face exactly the same family circumstances. If parents are constrained by imperfect capital markets, however, then the fact that the spacing between twins is zero may imply different constraints on invest-

19. The estimation of such relations dates back at least to Behrman and Taubman (1976) and Behrman, Hrubec, and others (1980); see Behrman and Rosenzweig (1999) for new estimates and a survey of past estimates.

20. This procedure was introduced by Ashenfelter and Krueger (1994) using the other twin's report on the first twin's schooling and by Behrman, Rosenzweig, and Taubman (1994) using twin's children's reports on their twin parent's schooling.

ing in the two twins than would be the case for siblings born at different times. Nevertheless, investigations into whether spacing affects investment in children do not reveal significant effects.[21] Fourth, the distributions of various outcomes may be different for twins than for singletons. For example, the distribution of birth weights is significantly lower for twins than for singletons. In this case, the estimated impacts of birth weight on outcomes over the life cycle do not change substantially if observations are weighted to reflect the distribution of singleton birth weights rather than twins' birth weights.[22]

For these reasons, within-twins (and, for some of these reasons, within-sibling) estimates require assumptions or procedures to deal with such issues. The assumptions, however, may not be as strong as are required to obtain consistent estimates of β from ordinary least squares (OLS) cross-sectional relations for equation 4 (that is, despite relation 5, schooling is not correlated with unobserved endowments). Subject to such caveats, within-identical-twins and within-sibling estimates are informative. They also may have advantages over instrumental variable (IV) estimates based on school reforms, which are one fairly widespread alternative. Specifically, they refer to a fairly wide range of schooling experiences, whereas school reforms usually are local area treatment estimates (LATE) that refer to a fairly limited range of schooling (typically relatively low levels at which, say, reforms in required schooling levels have some impact).

To return to the question of whether households reinforce or complement endowments, in Behrman, Rosenzweig, and Taubman we use within-identical-twins estimates (with controls for random measurement error, as noted) to obtain consistent estimates of β.[23] Conditional on β, we show that we can identify $\gamma_2 - \gamma_3$, which is greater than zero if there is reinforcement and less than zero if there is compensation, with any other siblings that have different individual-specific endowments, including fraternal (or dizygotic) twins. Based on data on identical and fraternal twins in the United States, we find reinforcement of specific endowments for earnings and health; important individual-specific endowments, accounting for 27 percent of earnings variance and 42 percent of variance in the body mass index (BMI); and negative assortative mating on individual endowments in marriage, which is consistent with Beckerian specialization in marriage.

21. See, for example, Behrman, Hrubec, and others (1980); Behrman and Rosenzweig (2004).

22. Behrman and Rosenzweig (2004).

23. Behrman, Rosenzweig, and Taubman (1994).

Schooling Returns

The investigation of whether failure to control for endowments causes misleading understanding of the impacts of human resources has focused substantially on schooling impacts, originally on wages and more recently on other outcomes. The estimation issue that is addressed in these estimates is the same as discussed in the previous section with regard to relation 4—that is, what happens to estimates of β with the use of various methods to control for unobserved endowments.

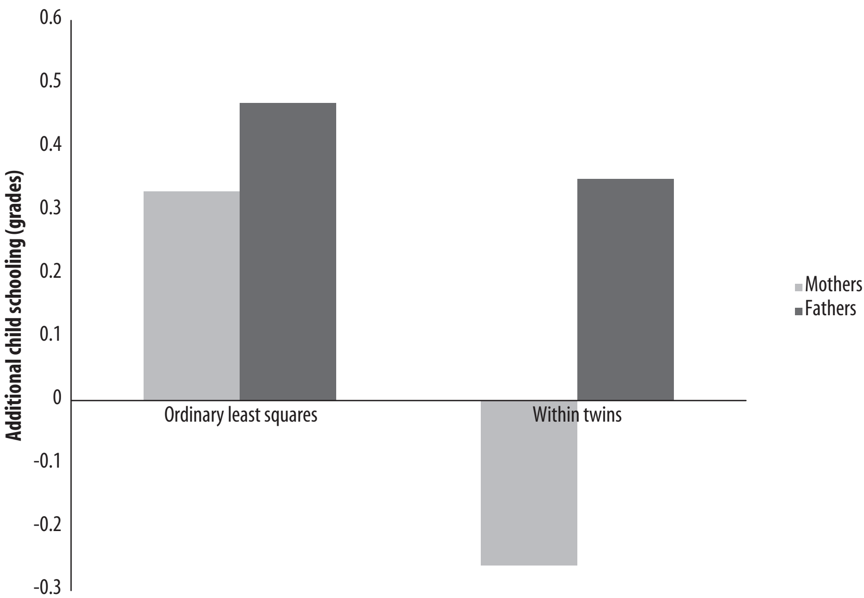
INCOME, EARNINGS, AND SOCIOECONOMIC STATUS. Estimates using data on adult Nicaraguan sisters and controlling for the common family component of endowments (but not individual-specific endowments) suggest that the failure to control for family endowments in OLS estimates of equation 4 results in considerable upward biases of schooling impacts: 33 percent for income and 67 percent for socioeconomic status.[24] Though these estimates may exaggerate the bias somewhat given the absence of controls for random measurement error, plausible degrees of measurement error would only account for part of the difference between the OLS and within-sibling estimates. Moreover, the inability to control for individual-specific endowment differences probably means that these estimates understate the impact of controlling for endowments.

Estimates using identical twins for the United States with controls for random measurement error find upward biases in OLS cross-sectional estimates of β of 102 percent (0.101 versus 0.050) for the 1917–27 male birth cohort and of 12 percent for the 1936–55 female birth cohort.[25] These estimates indicate that the biases resulting from the failure to control for endowments may be considerable, but they may differ depending on differing household investment strategies and schooling policies (related to equation 5) and differing labor markets (as reflected in equation 4).

OWN HEALTH AND FAMILY NUTRITION. A comparison of OLS estimates for adult Nicaraguan sisters with a linear structural relations (LISREL) latent variable representation of endowments suggests that the standard estimates of significant positive effects of schooling on health and nutrition are probably substantially overstated.[26] The use of Danish within-identical-twins estimates indicates that the apparent significant reduction in hospitalization and

24. Behrman and Wolfe (1984d).
25. Behrman, Rosenzweig, and Taubman (1994); Behrman and Rosenzweig (1999).
26. Behrman and Wolfe (1987a, 1989).

FIGURE 9. Mothers' and Fathers' Additional Grade of Schooling and Additional Grades of Child Schooling



mortality with more schooling in standard estimates disappears with controls for endowments and with the use of registry data, so measurement error is not likely to be a problem.[27]

INTERGENERATION IMPACTS ON CHILD SCHOOLING. Standard cross-sectional OLS estimates for adult Nicaraguan sisters suggest a significant effect of 0.12 more grades of schooling for children age six to thirteen years per grade of mother's schooling, although within-adult-sister estimates are only a third as large and not significantly greater than zero.[28] With identical twins in the United States, the cross-sectional association suggests 0.33 more grades of child schooling per grade of mother's schooling, but within-identical-twins estimates suggest –0.26 fewer grades (figure 9).[29] For father's schooling, the significantly positive association with child schooling remains significantly positive in within-identical-twins estimates (figure 9). The result for fathers

27. Behrman, Kohler, and others (2009).
28. Behrman and Wolfe (1987b).
29. Behrman and Rosenzweig (2002, 2005).

reinforces the likelihood that the sharp drop and change in sign for mothers is not merely some artifact of using the within-identical-twins methodology. Additional within-identical-twins estimates for mothers indicate that, holding endowments constant, increasing female schooling tends to increase labor force participation, thereby reducing parenting time for mothers and affecting child schooling. (In the context studied, fathers' schooling has no such impact on their labor market time.) Thus, despite widely held conventional wisdom that greater female schooling increases investment in children and is more important than male schooling for determining investment in children, these estimates imply that at least in some contexts, increased female schooling changes the way women use their time, causing a reduction in schooling investments in children and substantially less positive impacts on child schooling than greater male schooling. These results are striking and surprising in light of conventional wisdom, but they also depend on context. Estimates for rural India at a time when more female schooling was not rewarded in local labor markets indicate that even with controls for endowments, more female schooling did increase child schooling.[30]

School Quality

Most studies of the impact of human resource investments on various outcomes represent human resource investment by grades (or levels) of schooling attained and do not control for the behavioral determination of schooling attainment. However, schooling attainment or quantity (roughly the time spent in school) would seem to be only one input into the production of cognitive and other skills that are rewarded in the labor market.[31] Home and community inputs and schooling quality would also seem to be important. If school quality (Q) and time in school (S) are complements in the production of earning capacities, then higher-quality public schools induce more time in school, and the expected full income possibility frontier (EFIPF) in figure 2 is further

30. Behrman, Foster, and others (1999).

31. Completed schooling, however, is not exactly equivalent to time in school because of grade repetition and dropping out and reentering. These factors are not usually incorporated into analyses, which is another example of how what is not observed or controlled for may result in misleading inferences. For example, the failure to consider repetition leads to overestimates of the rates of return to schooling (Behrman and Deolalikar, 1991). Similarly, gender gaps in enrollment favoring males do not necessarily imply schooling attainment that favors males: in most current contexts, average female schooling attainment conditional on any schooling is equal to or greater than average male schooling attainment because males have higher repetition rates (Behrman, Sengupta, and Todd, 2005; Grant and Behrman, 2009).

from the origin (for example, $EFIPF_2$ for higher public school quality versus $EFIPF_1$ for lower public school quality). Therefore, the usual natural log earnings functions with grades of school as the only right-hand-side schooling variable are likely to yield biased estimates of return to time in school if the true relation should also include *Q:*

$$\ln Y = \beta S + \gamma Q + \ldots + v. \tag{6}$$

Estimates based on a random subsample of Brazilian males from the 1970 census, with school quality measured by average schooling of local teachers, produce three main findings.[32] First, standard estimates significantly overstate the rate of return to the quantity of schooling (20.5 percent versus 11.7 percent). Second, internal social rates of return to quality are greater than the rates of return to quantity. This implies an equity-productivity trade-off because the concentration of resources to improve schooling quality for a given group of students has higher returns than dispersing resources to increase low-quality schooling for more students. Finally, geographic quality differentials substantially reduce the unexplained income differentials between migrants and nonmigrants.

Thus, controlling for factors that are unobserved in most studies (in this case, school quality) significantly changes our understanding of the impact of schooling, the most emphasized human resource investment.

Early Life Nutrition

The most critical periods for nutrition are thought to be in the womb, as reflected in birth weight, and the first two to three years of life. These are considered in turn.

BIRTH WEIGHT. While there are many positive associations between birth weight and outcomes over the life cycle, Behrman and Rosenzweig provide the first estimates of the causal effects of improving prenatal nutrient consumption (*C*), which would seem the most directly policy-relevant possibility related to birth weight.[33] Assume that outcomes over the life cycle, Y_{ih}, depend linearly on birth weight, various dimensions of household background, Z_h, common genetic endowments, $\mathbf{E}_{0,h}$, individual endowments, $\mathbf{E}_{0,ih}$, and a

32. Behrman and Birdsall (1983, 1985). Similar results are obtained for rural Pakistan, with a more extensive representation of school characteristics and control for endogenous choices of time in school (Alderman, Behrman, Ross, and Sabot, 1996; Behrman, Ross, and Sabot, 2008).

33. Behrman and Rosenzweig (2004).

stochastic term. Also assume that birth weight depends on C, Z_h, $\mathbf{E}_{0,h}$, $\mathbf{E}_{0,ih}$, and a stochastic term. Then,

$$Y_{ih} = \beta C_{ih} + \gamma Z_h + \mathbf{E}_{0,h} + \mathbf{E}_{0,ih} + v_{ih}. \tag{7}$$

OLS cross-sectional estimation of relation 7 results in biased estimates of the impact of improving prenatal nutrient consumption if such consumption is correlated with family background variables or endowments, which would seem likely given the apparent importance of maternal health, family background, and genetic endowments in determining birth weight and outcomes over the life cycle. Prenatal nutrition, C, for twins can be disaggregated into a common component, C_h (the average), and an individual-specific component, C_{jh} (the deviation from the average):

$$Y_{ih} = \beta\left(C_{jh} + C_h\right) + \gamma Z_h + \mathbf{E}_{0,h} + \mathbf{E}_{0,ih} + v_{ih}. \tag{7a}$$

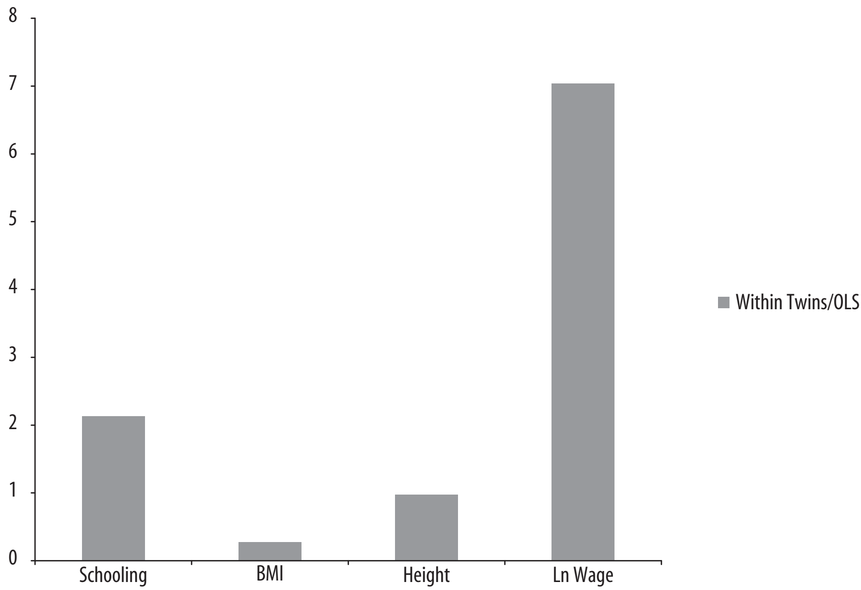
The within-identical-twins estimator is

$$\Delta^M Y_{ih} = \beta \Delta^M C_{jh} + \Delta^M v_{ih}. \tag{7b}$$

Estimation of relation 7b permits obtaining consistent estimates of β (that is, the impact of improved nutrition in the womb) on a range of outcomes over the life cycle. These estimates for identical female twins in the United States indicate that the causal effects of improved prenatal nutrition are twice as large for schooling and six times as large for wages as would be inferred from the associations obtained from OLS cross-sectional estimates of relation 7 (figure 10). The larger estimates from estimating relation 7b for schooling and earnings relative to estimating relation 7 may reflect, for example, a negative correlation between earnings and physical endowments that biases downward the estimate of β in equation 7 versus equation 7b. The magnitude of the estimates for earnings imply that if India had the United States' birth weight distribution and if the estimated earnings relation held for India, earnings in India would increase by 14.8 percent; this is a substantial impact, but it would reduce world inequality by very little (less than 1 percent).

Similar comparisons of estimates of equations 7 and 7b for the body mass index (BMI) provide important insight into the significant association between birth weight and adult weight. If this association reflected causal impacts, it would suggest that improving birth weight would have negative effects by increasing adult obesity. However, it actually reflects genetic and other back-

FIGURE 10. Estimated Impact of Higher Birth Weight with Control for Endowments Relative to without Control for Endowments



ground factors rather than causal effects of improving prenatal nutrition on adult obesity.

There are no such direct within-identical-twins estimates of birth weight effects decades later based on developing country data. However, Alderman and Behrman compile the implications of the best estimates that we could locate for the impact of moving a baby in a low-income country from low birth weight status (below 2.5 kg) to normal birth weight status (table 1).[34] These estimates suggest fairly large resource gains in terms of saving resources that would have been used for morbidity and increasing productivity. Interestingly, the largest gains are not from reducing early-life morbidity and mortality and late-life chronic diseases (as emphasized in much of the literature), but from increasing adult productivity. The results also indicate the degree to which the estimates depend on the discount rate, which is not surprising given the long lag before some of the impacts are obtained.

34. Alderman and Behrman (2006).

TABLE 1. Present Discounted Values of the Benefits of Improving Birth Weight[a]
U.S dollars

Benefit	Annual discount rate 3 percent	5 percent	10 percent
Reduced infant mortality	95	99	89
Reduced neonatal care	42	42	42
Reduced costs of infant and child diseases	36	35	34
Productivity gain from reduced stunting	152	85	25
Productivity gain from increased cognitive ability	367	205	60
Reduced costs of chronic diseases	49	15	1
Intergenerational benefits	92	35	6
Total	832	510	257
Share of total at 5 percent discount rate (percent)	163	100	50

a. The table estimates the benefit, in dollars, of moving an infant from low birth weight status to normal birth weight status in a poor developing country. The 5 percent discount rate is the base case estimate.

Estimates of the impacts of birth weight alone do not reveal whether improving birth weights is a good investment. That analysis needs to be based on information on the costs of interventions to improve birth weight, as well as information on the impacts. Table 2 estimates the cost of reducing low birth weight. These estimates suggest that under most assumptions, the ratios of the present discounted value of the benefits to the present discounted value of costs exceed one for the interventions to reduce the prevalence of low birth weight.

EARLY LIFE NUTRITIONAL SUPPLEMENTS. The Institute of Nutrition of Central America and Panama (INCAP) undertook a nutritional supplementation trial in Guatemala in 1969–77 to explore the impacts of improved preschool nutrition. Three hundred rural communities with 500–1,000 inhabitants were screened to identify villages of appropriate compactness (so as to facilitate access to feeding centers; see below), ethnicity and language, diet, access to health care facilities, demographic characteristics, child nutritional status, and degree of physical isolation. Two sets of village pairs were selected (one with small villages with about 500 residents each and another with larger villages

TABLE 2. The Costs and Benefits of Selected Interventions to Reduce Low Birth Weight

Intervention	Benefits/costs
Treatment for women with asymptomatic bacterial infections	0.6–4.9
Treatment for women with presumptive sexually transmitted diseases	1.3–10.7
Drugs for pregnant women with poor obstetric history	4.1–35.2

Source: Behrman, Alderman, and Hoddinott (2004).

with about 900 residents each). Before the intervention, the village pairs were similar in terms of a variety of nutritional, social, and economic outcomes, although their educational outcomes varied somewhat. Child nutritional status before the intervention, as measured by length at three years of age, was similar across villages and indicated substantial undernutrition, with over 50 percent severely stunted (that is, height-for-age *z* scores of less than −3). Maternal height was also not statistically different across villages. Specially collected village census data showed similar patterns of the civil status of household heads, religious affiliation, agricultural employment, and housing characteristics across the four villages. One village, however, had somewhat higher literacy and schooling levels for adults.

Two of the villages, one from each pair matched on population size, were randomly assigned to receive a dietary supplement in the form of a high-protein energy drink, known as *atole* in the study after the Guatemalan name for porridge. *Atole* was composed of Incaparina (a vegetable protein mixture developed by INCAP and widely accepted for young children in Guatemala), dry skim milk, and sugar; it had 163 kcal and 11.5 grams of protein per 180 ml cup. It was served hot and was slightly gritty, but had a sweet taste. The designers of the intervention were concerned that the social stimulation of the children—resulting from their social interactions while attending feeding centers where the supplement was to be distributed, the observation and measurement of their nutritional status, and the monitoring of their intake of *atole*—might also affect child nutritional and cognitive outcomes, thus confounding efforts to isolate the nutritional effect of the *atole* supplement. To address this concern, an alternative supplement, *fresco,* was provided in the two remaining villages, under identical conditions. *Fresco* was a fruit-flavored drink, which was served cool. It contained no protein and only sufficient flavoring agents and sugar for palatability, and it had about one-third of the calories of *atole* per unit volume (59 kcal/180 ml). Several micronutrients (iron, thiamine, riboflavin, niacin, ascorbic acid, and vitamin A) were added to both *atole* and *fresco,* in amounts that yielded equal concentrations across the supplements per unit of volume.

The nutritional supplements (namely, *atole* and *fresco*) were distributed in each village in centrally located feeding centers and were available daily to all members of the village on a voluntary basis in 1969–77. The associated data collection focused on the 2,392 children from birth to seven years of age at any point during the intervention period. Data collected at the child level included precise measurement of actual daily supplement intake (from which caloric and protein intake can be calculated), periodic twenty-four-hour food recall, and periodic anthropometric measurements until the child reached seven years of age or until the survey data collection ended in 1977, whichever came first. Children in the sample, then, were all born between 1962 and 1977, and the type, timing, and length of exposure depended on their birth date and village. The total diets of young children from *atole* villages included more than 9 grams of protein, 100 kcal/day, and micronutrients. Their length increased by three centimeters, but only if they received *atole* in first three years of life.

In the follow-up Human Capital Study in 2002–04, we interviewed 1,581 of these individuals (84 percent of the 1,855 who were thought to be living in Guatemala), at which time they were from twenty-five to forty-two years of age.[35] In the 2006–07 Intergenerational Study, we interviewed the subset of these same individuals who lived in the original district in which the four villages are located or in the Guatemala City area and who had at least one living parent.[36] We used these additional rounds of data to explore the impacts of the original nutritional supplementation on outcomes later in the life cycle and in the next generation roughly three decades later. We have undertaken a number of difference-in-differences estimates of the effects of being exposed to *atole* rather than *fresco* in critical periods of early life (specifically, the first twenty-four to thirty-six months).[37] These results are robust to a number of different explorations, such as aggregating to sixty-four birth-year village cohorts and controlling for attrition with reweighted estimates. They can be broken down into three groups:

—Exposure to improved nutrition from birth to three years of age and education.[38] Female schooling attainment improved by 1.2 grades (0.36 standard deviations); both men's and women's inter-American reading scores improved by 0.28 standard deviations; and both men's and women's Raven's progressive matrices scores improved by 0.24 standard deviations.

—Exposure to improved nutrition in early childhood and economic activity.[39] Exposure to improved nutrition before, but not after, three years of age

35. Grajeda and others (2005).

36. Melgar and others (2008).

37. For example, [Average outcome for those exposed to *atole* for the entire first 0–36 months of life – Average outcome for those exposed to *fresco* for the entire first 0–36 months of life] – [Average outcome for those in *atole* villages but not exposed to *atole* for the entire 0–36 months of life – Average outcome for those in *fresco* villages but not exposed to *fresco* for the entire first 0–36 months of life].

38. Maluccio and others (2009).

39. Hoddinott and others (2008).

improved wage rates (income per hour) for men but not women. Exposure from birth to two years had the greatest impact: Wages increased by US$0.67 (95 percent confidence interval: 0.16, 1.17), or 0.45 standard deviations.

—Exposure to early childhood nutritional supplements and intergenerational effects.[40] Exposure, for females, but not for males, affected their children from birth to twelve years old with significant increases in height, head circumference, and height-for-age *z* scores.

The estimates of the own effects on adult educational and labor market outcomes are substantial for the first two or three years of life, but much less significant or even insignificant for exposure for older children. The exception is intergenerational effects, which are significant for exposure for female children older than thirty-six months.

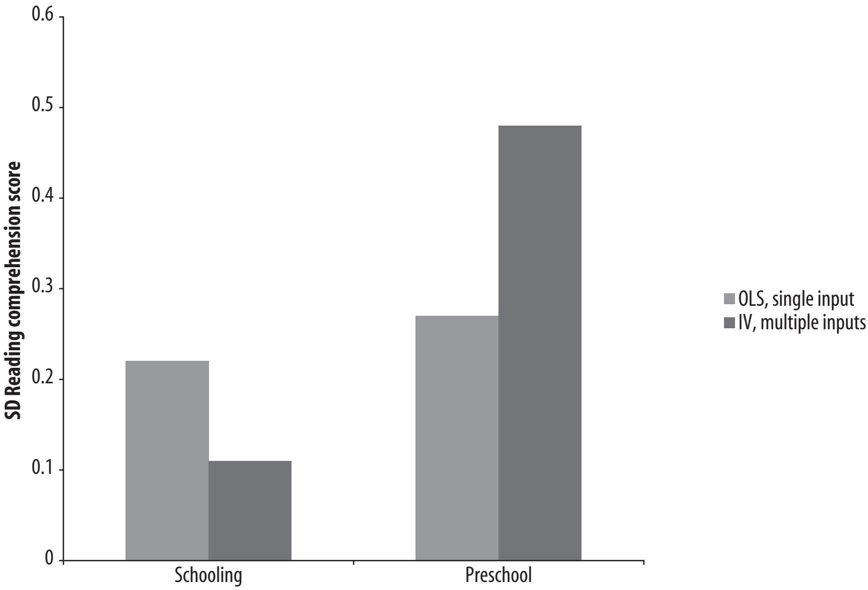
These very rich longitudinal data allow us to estimate not only the long-run impact of early-life nutritional supplementation, but also other interesting relations, such as adult cognitive production functions along the lines of equation 2b.[41] We posit, in contrast to much of the literature, that adult skills depend not only on school-age experiences, but also on pre-school-age experiences and post-school-age experiences and innate endowments and that these experiences are measured with error and are behaviorally determined (endogenous). We represent preschool experiences by the presence or absence of stunting at age seventy-two months (which reflects nutrition, stimulation, and disease history), school experiences by schooling attainment (completed grades of schooling) for comparison with the previous literature, and post-school experience by tenure in skilled occupation. We undertake alternative estimates, with different combinations of these three life-cycle experiences and with OLS versus IV estimates of adult reading comprehension and nonverbal skills.[42] These estimates suggest that our two concerns have a substantial impact on the estimated determinants of adult skills. Figure 11 illustrates, for example, that OLS estimates using only schooling overestimate the impact of schooling on the reading-comprehension skills production function by about 100 percent in comparison with IV estimates including all three life-cycle experiences. The impact of preschool experiences are substantially underestimated in OLS estimates, and the relative impact of schooling versus preschooling is substantially overstated in the usual OLS estimates. Indeed,

40. Behrman, Calderón, and others (2009).

41. Behrman, Hoddinott, and others (2008).

42. We use both OLS and IV estimates to control for random measurement error and endogeneity.

FIGURE 11. Standard Deviations in Reading Comprehension Score per Grade of Schooling or Preschool Child without Stunting



these estimates suggest that improving preschool experiences by eliminating a child's stunting has an impact equivalent to about four additional grades of schooling. Results are similar for adult nonverbal skills, with preschool experiences being even more important relative to schooling (with the latter becoming insignificant), whereas postschool experiences are more important than schooling when we control for endogeneity and include all three experiences. Thus, again, ignoring the possible importance of endowments or investigating the impact of one variable (such as schooling) without controlling for other variables (such as pre- and postschool experiences) can lead to considerable misunderstanding.

Program Impact Evaluation

Estimates of program impact evaluation may be misleading if the researcher does not control for selection into program participation, because the unobserved factors leading to such selection may well be correlated with the indicators of program impact. I here consider two examples of program evaluation in Latin America and the Caribbean in which I have been involved. Bolivia's Integrated Child Development Program (*Proyecto Integral de Desarrollo Infantil,* PIDI) provides an illustration of the potential selection bias and a possible way to control for it. Mexico's PROGRESA (now called *Oportunidades*) is a very important example of a large-scale social program that attempted from the start to create an evaluation strategy for producing impact estimates that are not contaminated by unobserved factors related to program selection.

PIDI is an early childhood development program in poor urban communities. The program includes nutrition, health monitoring, and child stimulation. Longitudinal data were collected on three groups: program participants, program nonparticipants in the same neighborhoods, and nonparticipants from areas without programs. Though the data are longitudinal, preprogram baseline data are not available for comparison. OLS estimates suggest a negative association between program participation and anthropometric indicators of child nutrition. However, decisions to enter the program apparently favored relatively malnourished children, although preprogram nutritional indicators are not available. Nonparametric marginal (or difference-in-differences) matching estimators control for the possibility that selection into the program was partly based on unobserved (by the analysts) preprogram nutritional characteristics. These estimates indicate that the program impact on cognitive and social tests depends on the duration of exposure to the program (positive for more than six months) and age at the time of the evaluation (forty-two to fifty-eight months for the largest effects). The estimated present discounted benefits of the program in terms of lifetime income relative to the governmental program costs are 1.7 to 3.7. Again, if the study does not control for the possibility that selection into the program is inversely associated with preprogram nutritional status, then the estimates are misleading and can even have the wrong sign.[43]

Mexico's PROGRESA is well-known partly because the designers of the initial evaluation adopted a strategy to minimize biases stemming from unobserved factors that might affect, for example, program participation. The strategy significantly enhanced the credibility of the initial evaluations and attracted many researchers to using the data.[44] The initial rural evaluation sample included 320 communities (with populations under 2,500) that were randomly assigned treatment in early 1998 and 186 communities (initial controls) that were assigned treatment twenty months later. A series of

43. Behrman, Cheng, and Todd (2004).
44. Behrman (2007).

follow-up surveys were conducted with the roughly 24,000 families in the initial evaluation sample during the first two years of the program, in 2003 (with additional matched controls), and in 2007. Short-term impact estimates are based on that differential exposure of twenty months (or, in many cases, the first year). Longer-term estimates include the effects of that differential exposure during the alleged critical window of the first three years of life for nutrition a number of years later and difference-in-differences (marginal) propensity score matching estimates. The results of the program evaluations in which I have been involved since 1997 include the following:

—Short-term impacts on schooling. Nonparametric estimates based on transition matrices reveal reduced drop-out rates, increased grade progression, and increased reentry, which together imply a long-run impact of about 0.7 grades completed, with evidence of forward-looking effects (that is, effects on the attendance of children in grades prior to the those eligible for the program). We find no evidence of spillovers to children in the same communities who were not in the program.[45]

—Short-term infant and toddler growth. OLS estimates suggest no effects. This association may reflect the nature of initial selection for the supplements, however, as there were initial supplement shortages that led to selective provision to more malnourished children.[46] In contrast, child fixed-effect estimates that control for the unobserved factors related to the initial program selection indicate about a one-sixth increase in the growth rate for children aged twelve to thirty-six months. This is simulated to increase the present discounted value of lifetime earnings by about 2.9 percent.[47]

—Medium-term schooling effects (by 2003). Estimates based on the original experimental design and propensity score matching suggest a slight reduction in the age of school entry for children who started the program at birth through two years and 8–9 percent increases in grades of schooling completed to date for children who were 6 to 8 years old at the start of the program start.[48]

—Impact of length of exposure. In 2007, we compared the groups that had a twenty-month difference in the start date of the program, based on children who were under three years old when they entered the program in 1998 or

45. Behrman, Sengupta, and Todd (2005).

46. Generally, however, statistical analysis confirms that the overall evaluation sample had random assignment of treatment and control (Behrman and Todd, 1999b).

47. Behrman and Hoddinott (2005).

48. Behrman, Parker, and Todd (2009).

1999. Estimates based on the original experimental design indicate that the group with longer exposure experienced a significant reduction in behavioral problems of about 0.15 standard deviations and improved language development of about 0.10 standard deviations. We find no significant effects on cognitive skills tests, anthropometrics, or health.[49]

These and other studies suggest that the program had significant impacts. These results led to the survival and expansion of the program in Mexico and the implementation of similar programs in many other countries, as well as the broader adoption of more systematic evaluation strategies for social programs.[50] The credibility of these studies was enhanced by the initial experimental design, which, together with the almost 100 percent enrollment in the original rural communities, controlled for possible selection into program participation based on unobserved factors and thus biases stemming from unobserved variables.[51] Most of the longer-run estimated effects, however, are conditional on stronger assumptions about attrition, matching estimators, and the stability of parameters that are based on the relatively short-duration experimental data.

Conclusions

Data limitations make empirical work very challenging. Unobserved factors and endogeneity may be critical, as indicated in the models and estimates presented in this paper. Estimates can be interpreted more confidently if they are based on explicit models of behavior that recognize the possible importance of unobserved factors and are linked to special data. The estimation methods should also control for factors such as selection on unobserved variables and endogeneity. A range of methods and data may, conditional on different assumptions, lead to better estimates. These include the use of better baseline and longitudinal data, experimental designs, and more careful econometric methods such as fixed effects, propensity score matching, regression

49. Behrman, Fernald, and others (2009).
50. Behrman (2007).
51. When the program was expanded to urban areas, only a little over half of the eligible families enrolled as a result of the different procedures for enrollment and different market and policy conditions. It is therefore necessary to control for selection into the program to obtain estimates of the effect of treatment on the treated. We have done so in a study of the short-run urban impact on schooling attainment and find significant effects of about the same magnitude as the short-term rural estimates (Behrman, Gallardo-García, and others, 2009).

discontinuity, and structural models. Estimates are often sensitive to the data and estimation method used, in some cases in terms of both magnitude and direction.

To date, the research in which I have been involved has contributed to substantial progress in approaches to data collection and empirical estimation for Latin America and the Caribbean. These advances have led to some important substantive results. In particular, my colleagues and I have shown that schooling (of given quality) is overemphasized in standard estimates; the importance of school (and other service) quality has been underemphasized; early life experiences are critical, including early-life nutrition and health; conditional cash transfers and early childhood development programs are very promising for some important aims; responses to incentives are important for human resource investments, as has been demonstrated for other behaviors; and there are substantial gains to careful systematic program evaluation.

At the same time, much work remains to be done on a number of important issues. For example, evaluations need to incorporate costs so as to obtain rates of return and cost-benefit estimates; distinguishing between private and social incentives would provide additional information on the efficiency motive for policies; increasing sensitivity to and estimates of critical dimensions of contexts would support greater generalization of the results (external validity); the impact of innovative incentive programs should be systematically investigated; and the capacity for evaluating counterfactual policies should be expanded.

I am proud to have been a participant in this journey toward improved empirical knowledge related to positive understanding and policies in Latin America and the Caribbean. And, again, I am very thankful to LACEA for recognizing my journey by awarding me the Carlos Díaz-Alejandro Prize. But this has been far from a solo trip. I thank my many traveling partners over the past forty years, a number of whom are co-authors, and I hope to welcome many more partners for new empirical challenges in understanding the determinants and impacts of human resources in Latin America and the Caribbean as I journey onward over the next forty years.

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