Methodological Note:

Using Micro Data to Understand Better the Intergenerational Transmission of Poverty in Low Income Developing Countries

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Abstract

Good empirical analysis of the intergenerational transmission (IGT) of poverty is challenging. This note clarifies this challenge and possible contributions by considering: (1) what estimated relations would be informative for improving understanding within an intergenerational life-cycle behavioural framework with important unobserved variables (e.g. genetics); (2) possible resolutions to estimation problems; and (3) different types of data. The greatest progress can be made by focusing on:

- Links between parental background and adult child resource access for which effects are thought to be particularly large and relatively uncertain.

- High quality data regarding (a) representativeness, (b) power, (c) coverage of important concepts for such studies and (d) limited measurement error.

- Data that permit better estimates, including their robustness to different assumptions – e.g. with complete information on key variables for two or three generations, on intergenerational transfers, linked to time series records on contextual changes, sibling information, experiments, and/or longitudinal data.

Through careful examination of existing data, keeping in mind considerations in this note, much can be learned about the IGT of poverty. But it is also important to be alert to opportunities for improving data and for encouraging collection of new and better data.

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1. Background and objectives

This study has been commissioned by the Chronic Poverty Research Centre (CPRC), through the "Empirical Approaches to the Intergenerational Transmission of Poverty" theme. This theme is attempting to identify the extent to which the intergenerational transmission (IGT) of poverty takes place in developing countries and through what processes. It is examining the nature and reversibility of such processes in different contexts and occurring at different times; and the range of factors that increase the likelihood that poverty is passed from one generation to the next.

CPRC researchers have begun to disentangle the key factors and processes that, within the context of the broader economic and socio-political context, determine the poverty status of individuals and their households, the sources of this status, and the potential 'poverty trajectories' for those growing up in poor households. Important factors include differential access to, and control of, resources and the returns on those resources, unequal investments in the human capital formation of household members, differential access to markets and to publicly-provided services and unequal distributions of leisure and labour time. Factors important in determining these systematic inequalities were found to be due, in part, to: non-cooperative household decision-making processes, conflict and household disintegration; differentiation based on social status (e.g. gender, age, mental or physical impairment, relationship to household head, birth order etc.); alcohol and drug dependence; mental ill-health; and differential access to market and publicly-provided services. Many of these factors – lower investment in the education and nutrition of girls, for example – clearly have negative long-term poverty implications. Other factors may have far more complex long-term effects on their children's lives and livelihoods and need further investigation.

Although highly context-specific, individuals' asset bundles, their capabilities, and their power to exercise agency have been found to combine to mould the life-course of individuals and their households. Research undertaken by the Childhood Poverty Research and Policy Centre (CHIP) identified a range of factors that increase the likelihood of an individual's poverty status being (largely) irreversible. Systematic discrimination based on ethnicity or gender, for example, has been shown to limit the beneficial impact of pro-poor policy interventions on some groups of people. Individuals' aspirations, and how they are influenced by early life experiences, have also been found to play strong roles in the extent to which they are able to extract maximum benefit from any policy or programmatic interventions that create new opportunities over the individuals' life course.

Other work has shown that older people can be vitally important with regard to the IGT of poverty, especially through their role as carers and particularly in areas with high levels of morbidity and mortality from chronic disease. This research also indicates that poverty can be transmitted 'both ways' – i.e. that the poverty status of older people is affected by the status and behaviour of younger generations. The CPRC will use life course, life history and family history analysis in seeking to move beyond an instrumental viewpoint, towards one that recognises the hard choices that people often make in negotiating the trade-offs between present and future, personal and family well-being.

A number of sources of micro-empirical data have the potential to provide information about the drivers and maintainers of the IGT of poverty: panel data analysis, parent-child studies, life histories, family histories, cohort studies using time series of cross-sectional data, experimental and "quasi-experimental" data, etc. However, the data options are often very limited in any particular developing country context. It is therefore important to improve our understanding of what these alternative data sources and related estimation methods have to offer in the study of the IGT of poverty.

This methodological note focuses on what this range of data options and related analytical techniques have to offer. It suggests when different types of studies are likely to be most effective in delivering robust answers to core questions and it highlights the likely technical and methodological strengths and limitations of alternative studies, using existing data in low income developing countries, for research on the IGT of poverty. The thematic research questions to be addressed include:

- What are the strengths of alternative approaches?
    - What kind of IGT-related questions does each approach help to answer?
    - What are the relative advantages of each approach in answering these questions?
- What is each approach unable to deliver (and to what extent are other approaches more useful)?
- What kind of data does each approach need (quality, sample size, etc.)?

2. Analytical Framework for Guiding Empirical Analyses of the IGT of Poverty

To understand the strengths and the limitations of the various approaches to analyses with various types of data in Section 3 below, it is useful first to consider a generic model of the IGT of poverty. This helps to illuminate various problems in making empirical inferences about the impact of parental characteristics on child characteristics in the presence of unobserved factors such as intergenerationally-correlated genetic endowments, capital market constraints and purposive placement of different public-sector programmes (e.g. to respond to political pressures, which is likely to favour those better-off; to address poverty concerns, which is likely to favour those worse-off). For simplicity it is desirable to focus here on a few major pathways that might account for the IGT of poverty. But the same considerations hold for analyses of the much more detailed and richer specifications that might be used to investigate some aspects of the "key questions for the research of the IGT of poverty."

Within such a framework, human capital investments in children are made through intrahousehold (perhaps implicit) bargaining, given individual control over resources (partly bought into the marriage by each spouse) and preferences, market prices and options, and public-service provision. Important dimensions include:

- prevalence of distribution of both resources and outcomes by gender,
- differences by income level related to differential market access by income,
- critical windows of opportunity at certain life-course stages,
- dynamic decisions made with implications over the life course and across generations,
- changing extra-household options and therefore household roles with the process of development.

However, the relations of interest may be hard to estimate to obtain causal effects because of the roles of behavioural choices for right-side variables[1] in the presence of important unobserved and perhaps persistent (in some cases, intergenerationally persistent) unobserved factors.

[1] "Right-side" variables refers to the variables on the right side of relations such as those (e.g. relation 1) that are usually interpreted to determine the left-side variable in the same relation.

2.1 Generic Framework for Analysing Individual and Familial Decisions Related to the ITG of Poverty

To illustrate more concretely the general issues involved, consider the following more formal stylised model. When a child becomes an adult (indicated by a subscript a) s/he will have resources for her/his use (Y_a) that depend primarily on her/his income-generating capacities, the income-generation capacities of her/his spouse (if any) and other family members, and sharing rules for determining the distribution of resources within the household – all embedded within a specific market, kin, public services and social network context.[2] These resources will depend basically on that individual's capabilities (K_a) including intellectual and physical functioning, that individual's physical and financial assets (A_a), that individual's preferences regarding matters such as their use of time and desires to have children (P_a), that individual's endowments (E_0, given factors such as genetic abilities and innate health, gender, ethnicity, race, tribe – where the subscript 0 indicates that these are given factors) that may affect the nature of local labour income earnings and other resource options, that individual's bargaining power for intrahousehold allocations (B_a), and local community, market and other contextual factors (C_a), as well as on stochastic terms (U_a) for chance events:

$$Y_a = Y\,(K_a, A_a, P_a, E_0, B_a, C_a, U_a). \qquad (1)$$

Relation (1) is written as a general functional form, which includes the possibility of interactions among the arguments (e.g. differential returns to capabilities depending on gender and on markets) and other nonlinearities of the included variables (e.g. diminishing marginal returns to various capabilities). All of the variables in relation (1) in general are vectors with multiple components (e.g. as noted, capabilities are likely to include intellectual and physical dimensions as well as interpersonal skills).

The impacts of parental background on the resources for use by this individual – and therefore whether this individual lives in poverty – are through (a) actual and potential transfers to the individual as an adult,[3] (b) affecting the returns to the human and physical assets that the individual has,[4] (c) intergenerationally-correlated endowments, and (d) investments in the individual or transfers in previous life-cycle stages. Given that the major asset of most of the poor is their time, the investments in their capabilities in previous life-cycle stages in group (d) are likely to be of particular importance for children from poor families. Identifying the causal effects of parental background on the capabilities of their adult children (as well as on other variables in relation 1), however, is likely to be difficult because such investments are made within a life-cycle framework in the presence of unobservables (such as ability and health endowments) in previous life-cycle stages and because of limitations in most available data (e.g. limited representation of capabilities, data generally not available from conception to adulthood).

[2] It may be desirable for some purposes to utilise a more-disaggregated representation of these resource sources, such as a labour income earnings function, a return to assets function and a sharing rule for household resources.

[3] These transfers may be in either direction (e.g., parental financial or time help for the individual in setting up a household or caring for children; financial or time help of the individual for sick or aging parents). While current transfers obviously may affect the individual's command over resources, potential transfers may also be important by, for example, affecting the individual's fallback position and therefore her/his threat point for intrahousehold bargaining if there are disagreements (e.g. Behrman and Rozenzweig, 2006).

[4] Parents, for example, may directly affect employment options for their adult children (e.g., on family farms or in other family enterprises).

To illustrate, consider adult intellectual functioning (K^i_a). The standard assumption is that adult intellectual functioning depends importantly on schooling, so consider three life-cycle stages:[5]

Life-Cycle Stage 1: pre-schooling (from conception through to about age five or six)

Life-Cycle Stage 2: schooling and adolescence (from age six or seven)

Life-Cycle Stage 3: adulthood to the time of the data

Adult intellectual functioning (K^i_a), then, can be considered to be determined by a production function in which the inputs are all previous experiences (E_i, i = 1, 2, 3 for the three life-cycle stages defined above; note that the subscript for life-cycle stage 3 is equivalent to the subscript "a" used in relation 1); genetic (and other) unobserved endowments (E_0) and stochastic terms (U_{3i} to reflect all other idiosyncratic, and assumed exogenous, learning experiences:

$$K^i_a = K^p\,(E_1, E_2, E_3, E_0, U_{3i}) \qquad (2)$$

where the first subscript for the right-side variables refers to the life-cycle stage, the second subscript if present refers to intellectual capabilities (i) and the superscript p refers to the function being a production function. There may be important interactions and nonlinearities in this production function (and in other relevant production functions). For example, individuals with better pre-school nutrition may learn more from their school-age experiences (so that the cross-derivative of relation (2) with respect to the first two variables is positive). This production function also may reflect that some processes are not likely to be reversible at reasonable costs. For example, nutrition early in the life cycle may establish basic patterns of neural development and of other aspects of development and it may be quite costly or impossible to offset these later in life (e.g. Barker, 1992, Engle *et al.*, 2006), which implies that E_2 and E_3 can only substitute imperfectly and to a limited extent for some components of E_1.

If one had good estimates of relation (2) and of parallel relations for the other right-side variables that enter into relation (1) and of relation (1) itself, then one could trace well the pathways from the effects of parental background during pre-adult life-cycle stages on the resources available for use by this individual and thus the extent of IGT of poverty for this individual. Estimation of relations such as (1) and (2), however, is challenging because at least in some cases the indicators of the right-side variables in relation (1) (the dependent variables in relations such as relation 2) are quite imperfect and because the experiences for the three life-cycle stages on the right side of these relations all reflect previous behavioural choices. For the latter reason, for example, ordinary least squares (OLS) estimates of relation (2) are likely to be inconsistent due to endogeneity of the life-cycle stage experiences.

To motivate the assumptions underlying the exploration of how parental family background affects the three life-cycle stage experiences and to elucidate the possible impact of the endowments on estimates that do not control for them, assume a very stylised model in which the "dynasty" (first the parents through intrahousehold bargaining between themselves

[5] The exact delineation of these life-cycle stages in terms of ages, of course, varies across contexts, with schooling for example tending to be of less duration in areas of greater poverty. The major transitions to adulthood also vary considerably in their timing (e.g. NRC/IOM, 2005). For any particular study, moreover, it may be desirable to consider other life-cycle stages, such as post-schooling youth or young adulthood. The use of these three life-cycle stages here, nevertheless, serves to make the basic points relevant for this note.

and perhaps other relatives, then the children themselves increasingly as they age into youth though with intrahousehold bargaining with their parents and other relatives and into adulthood usually with a spouse that involves further bargaining) make decisions so as to maximise a welfare function W that includes Y_a for each individual. This welfare function is maximised sequentially subject to the constraints at each life-cycle stage related to relevant current and expected production functions, resources allocated to this individual, community characteristics including community services and markets that affect household decisions, and stochastic factors.

<u>Life-Cycle Stage 1 (pre-schooling)</u>: The parents (perhaps implicitly) bargain between themselves (and possibly with others, such as the grandparents) to decide how to allocate resources to obtain the optimal E_1 for the child, given the child endowments, nutrients and other inputs into the E_1 production function that are allocated by the parents, the current community-determined options (e.g. availability of pre-school programmes), expected future community characteristics (e.g. expected schooling options in life-cycle stage 2, expected labour market options in life-cycle stage 3), the expected relation between E_1 and Y_a (via capabilities and the other right-side variables in relation 1), and the child endowments. The E_1 production function is:

$$E_1 = E_1^p\,(N_1, C_{1p}, E_0, E^f_0, U_{1E}), \tag{3}$$

where N is a vector of family-determined inputs into the production of E_1 (e.g. family-provided nutrients), C_{1p} is a vector of community inputs into the production of E_1 (e.g. community-provided pre-school programmes, community disease environment, community learning environment), E_0 is the child endowment that directly enters into the production of E_1 (e.g. innate robustness), E^f_0 is parental endowments that directly affect early childhood development (e.g. innate ability in raising children), and U_{1E} is a stochastic disturbance term that directly affects the production of E_1 (e.g. random fluctuations in the infectious disease environment). The parents choose the inputs into this production function N_1 and therefore E_1 in order to maximise the expected welfare W given: a vector of parental family characteristics such as parental schooling, parental preferences such as for child quality versus quantity or work versus leisure and parental assets in which the ownership of resources may matter because it may affect intrahousehold bargaining (F_1); all relevant community characteristics for this life-cycle stage C_1 (which includes the community characteristics that directly affect the production of E_1 through C_{1p}, but also other community characteristics that affect the household through other channels); all of the child endowments E_0; all the stochastic terms that affect outcomes in the first life-cycle stage of the child U_1 (which includes U_{1E} but also other stochastic factors that affect the family during the first life-cycle stage for this child since, for example, stochastic factors affecting the health of other siblings may affect the inputs devoted to this child) - plus the expected values of these variables in the next two life-cycle stages (F_{12}^e, F_{13}^e, C_{12}^e, C_{13}^e, U_{12}^e, U_{13}^e, where the first subscript refers to the life-cycle stage at which the expectations are held, the second subscript refers to the stage for which the expectations are held and the superscript e refers to expectations) because the optimal decision for investing in E_1 to maximize W depends in part on expectations regarding these variables over the next two life-cycle stages:

$$N_1 = N_1^d\,(F_1, C_1, E_0, E^f_0, U_1, F_{12}^e, F_{13}^e, C_{12}^e, C_{13}^e, U_{12}^e, U_{13}^e) \quad \text{and} \tag{4a}$$

$$E_1 = E_1^d\,(F_1, C_1, E_0, E^f_0, U_1, F_{12}^e, F_{13}^e, C_{12}^e, C_{13}^e, U_{12}^e, U_{13}^e), \tag{4b}$$

where the superscript d refers to reduced-form demand relations.

<u>Life-Cycle Stage 2 (school-age and youth)</u>: The dynasty (initially the parents but increasingly the child) decides on the schooling attainment component of E_2 of the child/youth conditional on (a) the outcome of Stage 1 E_1 that is assumed to summarize all the family and community

factors that determine pre-school investments,[6] (b) life-cycle stage 2 family, community and stochastic factors, and (c) the expected values of those factors for life-cycle stage 3:

$$E_2 = E_2^c\,(E_1, E_0, E^f_0, F_2, C_2, F_{23}^e, C_{23}^e, U_2, U_{23}^e), \qquad (5)$$

where the superscript c refers to the conditional demand function. Relation (4b) can be used to substitute for the life-cycle stage 1 experience E_1 in relation (5) to obtain the reduced-form demand relation for E_2:

$$E_2 = E_2^d\,(F_1, C_1, E_0, E^f_0, F_{12}^e, F_{13}^e, C_{12}^e, C_{13}^e, F_2, C_2, F_{23}^e, C_{23}^e, U_1, U_2, U_{12}^e, U_{13}^e, U_{23}^e). \qquad (6)$$

While the focus in this example is on completing schooling as a particularly important outcome determined in the second life-cycle stage, there are similar relations for a number of other important transitions during this life-cycle stage that also condition options in adulthood considerably. Leading examples include transitions into work, into sexual activity, into marriages or other forms of unions, into parenthood and away from the parental household and perhaps the parental community.

Life-Cycle Stage 3 (adulthood): The dynasty (primarily the post-school youth/young adult but perhaps with some input from the parents) decides on the post-schooling experience E_3 of the individual conditional on (a) the outcome of Stage 1 E_1 that is assumed to be a sufficient statistic for the family and community factors that determine pre-school investments, (b) the outcome of Stage 2 E_2 that is assumed to be a sufficient statistic for the family and community factors that determine schooling,[7] and (c) life-cycle stage 3 family, community and stochastic factors:

$$E_3 = E_3^c\,(E_1, E_2, E_0, E^f_0, F_3, C_3, U_3). \qquad (7)$$

Relation (4b) can be used to substitute for the life-cycle stage 1 experience E_1 and relation (6) can be used to substitute for the life-cycle stage 2 experience E_2 in relation (7) to obtain the reduced-form demand relation for E_3:

$$E_3 = E_3^d\,(F_1, C_1, E_0, E^f_0, F_{12}^e, F_{13}^e, C_{12}^e, C_{13}^e, F_2, C_2, F_{23}^e, C_{23}^e, F_3, C_3, U_1, U_2, U_3, U_{12}^e, U_{13}^e, U_{23}^e). \qquad (8)$$

Reduced-form relations for child's adult resource access (and other adult variables): Through the sequential life-cycle stage processes the adult capabilities in relation (2), and the other right-side variables in relations parallel to (2) for each of them, are determined as well. This implies, of course, that the critical (for this note) adult access to resources (or poverty indicator) can also be written as a reduced-form demand relation (by substituting relations 4b, 6, and 8 into relations such as 2 and then substituting those into relation 1) as:

$$Y_a = Y_a^d\,(F_1, C_1, E_0, E^f_0, F_{12}^e, F_{13}^e, C_{12}^e, C_{13}^e, F_2, C_2, F_{23}^e, C_{23}^e, F_3, C_3, U_1, U_2, U_3, U_{12}^e, U_{13}^e, U_{23}^e). \qquad (9)$$

[6] This is not a necessary assumption for estimating the adult capabilities production functions as in relation (2) but it is consistent with the exclusion of at least some of the first life-cycle stage determinants from directly appearing in relation (5) so that the impact of E_1 in that relation can be identified.

[7] Again, (a) and (b) are not necessary assumptions for estimating the adult cognitive achievement production functions in relation (2) but are consistent with the exclusion of at least some of the first and second life-cycle stage determinants from directly appearing in relation (7) so that the impacts of E_1 and E_2 in that relation can be identified.

Good estimates of the coefficients of the family background variables in relation (9) permit ascertaining the direct causal impacts of family background variables, including the command over resources by mothers versus fathers, if there is household bargaining, on the child's adult resource access and therefore poverty. As is indicated in this relation, the components of the vector of parental family background characteristics are likely to have effects at various stages of the life cycle (F_1, F_2, F_3), with an impact not only of the realised family background characteristics but also those that are expected in earlier life-cycle stages (F_{12}^e, F_{13}^e, F_{23}^e). The latter have an effect because if parents expect to be much better-off (or poorer-off) in the future (say, when the child is in the pre-school life cycle), they are likely to increase (decrease) their current investments in this child.

Good estimates of relation (9) permit answering a number of important questions about the IGT of poverty. How important is parental schooling? How much does the impact of parental schooling depend on the gender of the child or the nature of current or expected local markets? Are there differential effects of mother's versus father's schooling? How important is parental socioeconomic status (SES) or income? Are parental family characteristics more important for the pre-school or the school/youth years? To what extent do different community services, including in health and education, substitute for, or complement, parental background? Are there important differences in all of these relations by ethnicity or other demographic characteristics? By income/poverty level?

While good estimates of relation (9) are valuable in assessing the IGT of poverty, they are not the only estimates that would be illuminating regarding the IGT of poverty. Indeed, good estimates of any of the relations in this section (and of parallel relations for other pathways) would be illuminating for aspects of the IGT of poverty. For example: Just how important are various components of family background in determining schooling? Just how important are intellectual capabilities – or of schooling, one input into intellectual capabilities – in the determination of adult resource access? Are intellectual capabilities more or less important than physical capabilities? Do the importance of such factors depend on individual characteristics such as gender or on community characteristics such as the nature of labour or capital markets?

3. Estimation Issues and Some Possible Methodological Resolutions

Data limitations, no matter how good the data, lead to probable estimation problems. In all of the right-side relations in Section 2 there are vectors of variables, and a number of the components of those vectors are likely to be unobserved or poorly measured.[8] For the production function relations and the conditional demand relations, moreover, some of the right-side variables are determined endogenously within the life-cycle framework.[9] As a result

[8] Even if the life-cycle experiences are treated in the estimation as behaviourally-determined, if the true specification in relation (1) includes all the variables indicated above and (2) includes all three life-cycle experiences but a specification is used that excludes one or more of the relevant variables (e.g., only schooling is included), omitted variable bias is likely to result. This is likely to be the case because on the right side of each of the three reduced-form demand relations for the three life-cycle stage experiences (relations 4b, 6 and 8) are the endowments and the actual or expected values of the family, community and stochastic factors for all three life-cycle stages, which means that the three life-cycle experiences may be fairly correlated, and thus the right-side variables in relation (1) also fairly correlated. Of course, this is hardly surprising. *A priori*, a child with better parental family background or who lives in a better community in terms of health and educational services and job options is likely not only to have more schooling but also better pre- and post-schooling experiences.

[9] Direct estimates of relations such as (1) and (2) without controlling for the behavioural determinants of the three life-cycle experiences are likely to be biased because (as indicated in the reduced-form demand relations 4b, 6 and 8) each of the three life-cycle experiences depends on all the endowments. These biases could be in either direction. For instance, the "ability bias" on which the

of these estimation issues – unobserved variables, measurement errors and endogeneity – the disturbance terms in the relations to be estimated are likely to include not only the stochastic terms (U's) but also components that are correlated with the right-side variables in the relations. For example the disturbance term in relation (9) is likely to include unobserved parental abilities and parental innate health and parental preferences and family connections, unobserved individual abilities and innate health and unobserved community characteristics such as the disease environment that may be related to program placement. These unobserved characteristics are likely to be correlated with the observed ones; for instance, parental schooling is likely to be correlated with their innate abilities, preferences and with family connections. As a result the OLS estimation of relations such as (9) is likely to lead to biased estimates of the key parameters of interest because in the estimation, for example, parental schooling proxies in part for correlated unobserved parental abilities, preferences or family connections.

Better data always helps deal with such problems. Section 4 addresses different types of data that may be used for the investigation of the IGT of poverty, and the better the data the less are likely to be such problems. But for given data, there exist standard methodologies for dealing at least in part with these problems. Some examples follow:

<u>Instrumental variable (IV) or two-stage-least squares (2SLS) estimates</u>: To break the correlation between the observed right-side variables and the compound disturbance terms that include unobserved determinants in addition to stochastic terms, one estimation strategy is to use instrumental variables (IV) or two-stage least squares 2SLS). In IV estimates the endogenous right-side variables are replaced by their predicted values that depend on "instruments" that do not appear directly in the relation of interest. Good instruments must (1) predict well the variable being instrumented and (2) not be correlated with the disturbance term in the second-stage relation of basic interest. The model should be suggestive of the set of potential instruments. The three reduced-form demand relations for the three life-cycle stage experiences in relations (4b), (6) and (8), for example, give the potential instruments to be used to identify the three life-cycle experiences in the adult intellectual capabilities production function in relation (2).[10] Note that these include experiments (e.g. receiving the Mexican PROGRESA treatment with random assignment by rural communities as examined in Behrman and Hoddinott (2005), Behrman, Sengupta and Todd (2005), Schultz (2004); the random assignment by communities of different nutritional supplements in the INCAP Guatemalan data as examined in Maluccio *et al.* (2006) and Martorell *et al.* (2005); the random assignment of treatment of worms among Kenyan school children as examined in Miguel and Kremer (2004)) and so-called "natural experiments" in the form of natural events

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schooling literature has focused is consistent with E_2 (schooling) being correlated positively with both U_{3r} and U_{3n} with the result that the coefficient of schooling is likely to be upward-biased in OLS estimates of relations (2) and (3). On the other hand if the summary measure of pre-school experience is some variable such as child stunting, and if ability and physical endowments are negatively correlated as suggested by Behrman and Rosenzweig (2002, 2004), then OLS estimates of relations (1) and/or (2) may lead to biases towards zero in the coefficient estimate for this variable.

[10] Note that on the right-side of each of these three reduced-form demand relations are the same endowments and the actual or expected values of the family, community and stochastic factors for all three life-cycle stages. That means that, though there may be instruments that seem *a priori* to have first-order effects on particular life-cycle experiences (e.g. pre-school programmes or nutrition on E_1, school characteristics on E_2, labour market characteristics on E_3), it would not be correct to assert *a priori* that a particular instrument identifies a particular life-cycle experience. Instead, there is a potential set of instruments that hopefully identifies the set of life-cycle experiences. This also means that it would not be a test of the plausibility of the instruments to see if subsequent life-cycle stage family or community variables are significant (e.g. if schooling characteristics or post-schooling labour market characteristics significantly determine pre-school experience E_1) because the expected value of those variables should be included. Instead it would be a test of to what extent expectations are rational in the sense that the expected values for subsequent stages are equal to the realised values.

(e.g. weather fluctuations that occurred when the individual was a child that are used to identify schooling and health impacts on access to resources in Indonesia by Maccini and Yang (2006)) and policy changes (e.g. the Indonesian school-building programme investigated by Duflo (2001)). Lagging such relations a generation suggests the potential set of instruments for the parental family background variables on the right-side of relation (9). The IV (or 2SLS) procedure basically consists of making first-stage estimates in which endogenous right-side variables in the relation of interest are regressed on the instrument set and then making second-stage estimates of the relation of interest that uses the predicted values of the endogenous right-side variables instead of the actual values.[11] If the instruments are good in the two senses defined above, the predicted values of the right-side endogenous variables represent well the variation in the right-side variable (the first characteristic of good instruments) but are not correlated with the disturbance term in the second stage (the second characteristics of good instruments). The second stage estimates then are good estimates of the local average treatment effects of the first-stage instruments. Good IV estimates, thus, can eliminate problems due to omitted (unobserved) variables, endogeneity and random measurement error.

Finding good instruments, however, is often not easy. Not all of the potential instruments that are suggested by the model structure, for example, are likely to be independent of the second-stage disturbance term. For the estimation of the adult intellectual capabilities production function in relation (2), for example, the reduced-form relations (4b), (6) and (8) suggest that family background characteristics are potential instruments. But if unobserved genetic ability endowments affect adult intellectual capabilities as posited in relation (2), if unobserved parental ability endowments affect their schooling attainment and income and if there are significant correlations between parental and child ability endowments, then parental schooling attainment and income may not satisfy the second condition for good instruments (and indeed do not in recent estimates of such a relation for Guatemala in Behrman *et al.*, 2006). It may also be difficult to find instruments that predict sufficiently well the second-stage right-side variables. The econometric literature has been evolving recently in the development of diagnostic tests for good instruments (e.g. Stock and Yugo (2002) on the use of the Cragg-Donald statistic for the extent of bias due to "weak instruments" that do not satisfy the first condition for good instruments as well as would be desired). Recent standard software packages (e.g. ivreg2 in Stata 9) provide fairly up-to-date diagnostics for IV estimates (e.g. Cragg-Donald statistics for weak instruments for the first condition for good instruments, Hansen J overidentification statistics for the second condition for good instruments).

<u>Propensity score matching (PSM) estimates</u>: Recently there has been increasing development and use by economists (e.g. Heckman, Ichimura and Todd, 1998) of propensity score matching methods that were developed originally in the statistical literature (e.g. Rosenbaum and Rubin, 1983). These methods have been developed primarily in the context of the programme evaluation literature. They try to find the best comparison for someone exposed to the programme ("treatment") among those not treated. The procedure is (1) to estimate a logit for whether one was exposed to treatment or not as a function of predetermined variables (i.e. variables not affected by the treatment), (2) to use the estimates to predict the latent propensity for treatment for everyone, and (3) to compare each individual treated with an individual or group of individuals not-treated but who are very similar in terms of the predicted latent propensity for being treated. This permits comparisons between very similar individuals who have received and who have not received treatment, where similarity is defined in terms of the weighted average of <u>observed</u> characteristics used

[11] Fixed effects estimates to control for fixed unobserved factors, such as are discussed below, are sometimes used together with IV estimates.

to predict the propensity to be treated.[12] An increasing number of studies have been undertaken to estimate in particular programme impacts in developing countries that are consistent with the general life-cycle framework presented in Section 2 (e.g. the impact of early childhood development programmes in Bolivia in Behrman, Cheng and Todd (2004), in Mexico in Behrman, Parker and Todd (2006, 2007) and in the Philippines in Armecin *et al.* (2006) and Ghuman *et al.* (2006a)). Recent standard statistical programs include matching estimators (nnmatch in Stata 9).

Perhaps the most natural potential use of PSM in the present context would be to define "treatment" to be whether or not the parental household lived in poverty as one of the particular right-side variables to represent family background in relations such as (9). Then, in principle, PSM estimates could be made to estimate the impact of the parental family household being in poverty on adult resource access. But to make such estimates, it would be necessary to have observed characteristics that were not affected by the "treatment" of the parents being in poverty, which is likely to be very demanding in terms of data – perhaps requiring information on the parents' own childhood.

<u>Fixed effects (FE) estimates</u>: Some of the unobserved variables that are likely to cause problems if they are not controlled in the estimates are fixed across observations in the data. From a longitudinal perspective (i.e. fixed over time) these include variables such as individual and parental genetic ability and innate health endowments and some aspects of community culture and environment. From a cross-sectional perspective (i.e. fixed across observations in some group such as members of the same family or the same community) these include the family and community environments and endowments shared by siblings and other members of the same family, the school environment shared by students in the same school and the community environment shared by residents of the same community. Such factors that are fixed across observations can be controlled so that they do not bias estimates of observed variables through using dummy variables for each group of observations for which the control is desired (i.e. individuals or families over time, siblings or community members at a point of time). Such methods have been used extensively to investigate aspects of the framework in Section 2 (e.g. adult sister sibling estimates to control for shared childhood background in the estimation of the impact of mother's schooling attainment on child schooling in Nicaragua in Behrman and Wolfe (1987b); individual fixed effects to control for unobserved malnutrition that determined which children received nutritional supplements in the Mexican PROGRESA programme in Behrman and Hoddinott (2005) or which children were admitted to pre-school programs in the Bolivian pre-school PIDI programme in Behrman, Cheng and Todd (2004)). They have the advantage of controlling for unobserved fixed characteristics that otherwise might bias the estimates and numerous studies suggest that controlling for fixed effects changes the estimates substantially. For example, Behrman and Rosenzweig (2002, 2005) present a dramatic example regarding intergenerational schooling effects for the United States. Controlling for fixed characteristics including genetic endowments at conception between adult identical twins changes the estimated impact of maternal schooling on child schooling from significantly positive in OLS estimates to negative in FE estimates – apparently because, controlling for endowments such as innate abilities, women in that society who receive more schooling tend to spend more time in the labour market and less time caring for their children (there are not parallel changes in the estimated impact of paternal schooling – which is consistent with fathers not changing their time spent in child care much if they have more schooling).[13]

[12] Unobserved fixed factors, such as those discussed below, are also controlled in some matching estimates (e.g. the study of the impact of pre-school programs on early childhood development in Bolivia in Behrman, Cheng and Todd (2004) and in the Philippines in Ghuman *et al*. (2006a)).

[13] Other recent studies for European countries also report that OLS estimates of intergenerational schooling effects may be quite misleading. For instance, Plug (2004) uses data on adoptees to lessen

However, they have some limitations. First, they do not control for unobserved varying characteristics (e.g. time-varying prices in longitudinal estimates that may affect endogenous behaviours), for which reason in some studies they are combined with IV estimates (e.g. the investigation of the impact of nutrition on labour allocation in Bangladesh in Pitt, Rosenzweig and Hassan (1990) and in Pakistan in Behrman, Foster and Rosenzweig (1997)). Second, they tend to increase the importance of noise relative to the signal, which tends to cause a bias towards zero. For this reason, FE-IV estimates have been used in some studies (e.g. using other respondents' reports for schooling attainment in the United States in Ashenfelter and Krueger (1994) and Behrman, Rosenzweig and Taubman (1994)). Third, they do not permit estimates of the first-order impact of observed fixed variables, but only of variables that vary across the observations for which the fixed effects are used (though these may include interactions between fixed variables and variables that vary across the observations for which the former variables are fixed). Therefore, for example, they do not permit estimating the impact of parental schooling on child schooling unless parental schooling varies over time (as in the Rosenzweig and Wolpin (1995) study of the impact of young mothers' schooling on early childhood development in the United States)

<u>Construction of standard errors</u>: Most household sample surveys collect data from clusters (e.g. census tracts, villages, neighbourhoods) - or perhaps samples within clusters - because the fixed costs of data collection in a locale mean that a cluster design is much cheaper than would be, for example, a random sample of households in the overall population. The cluster design means that there are likely to be correlations across observations in the stochastic terms that, if not accounted for in the estimation of standard errors, might bias test statistics towards inferring greater significance to the results than is warranted. Estimation strategies that utilise within-family estimates may be further subject to this problem. Moulton (1990), for example, notes, "[i]t is reasonable to expect that units sharing an observable characteristic … also share unobservable characteristics that would lead the regression disturbances to be correlated." These correlations, if positive, may cause the estimated standard errors to be biased downwards. Therefore it is important to assess the sensitivity of the results to the construction of the standard errors. The starting point is to test for heteroscedasticity and correct, where appropriate, standard errors using established methods (e.g. Huber, 1967, White, 1980) that are readily available in standard estimation software. Most standard estimation software also has options to control for clustering among siblings or among members of the same sample cluster. Recent studies by Angrist and Lavy (2002) and Wooldridge (2003), however, suggest that these corrections for clustering are valid only when the number of units or groups or clusters of observations is large, say on the order of magnitude of 70 or greater. For many data sources this does not pose a problem, but for some it may because, for example, the data are from a relatively small number of samples. In such a case alternative standard error estimators can be constructed as indicated in Bertrand, Duflo, and Mullainathan (2004) by block bootstrapping the t statistics. Another approach is to aggregate all covariates up to their group means and carry out estimation on the average data (Wooldridge, 2003) at the cost of a considerable loss in degrees of freedom as the sample size drops from the number of households to the number of clusters. Explorations of such alternatives in a recent study using 16 birth-year cohorts from four villages in Guatemala suggest that at least in that case these methods do not change substantially the inferences from the estimates (Maluccio et al., 2006).

<u>Sample selection</u>: Selection may take many forms: only having data on wages for those who participate in the labour force, only having data on test scores for those attending school, only having information on health status or on health impacts of an intervention for those who attend health clinics, only having data on the impact of early childhood programmes for those who survived infancy and earlier childhood, only having data on those who do not attrit in

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problems of intergenerationally-correlated endowments and Black, Devereux and Salvanes (2005) use instruments based on changes in mandatory schooling.

longitudinal data. The general problem is that those who are selected are not likely to be a random subsample. A general solution is to model the selection rule and to use it to correct for selection in the estimates, such as in the well-known Heckman (1974, 1979) two-step procedure or other methods such as maximum likelihood estimates.
Because sample attrition is a major concern for one major type of data, some elaboration on this type of selection is provided here. Sample attrition has the potential to invalidate inferences that can be drawn from longitudinal data if the attrition is non-random with respect to the behaviour being studied. Consider the following canonical selection model:

$$L_t^* = b_2 + b_3 X_t + b_4 Z_t + U_t^* \quad \text{and} \tag{10}$$

$$Y_t = b_0 + b_1 X_t + U_t^{**} \; (Y_t \text{ observed only if } L_t^* < 0). \tag{11}$$

Relation (11) is the model of interest (e.g. a simplification of relation 9). The outcome variable, Y_t, is observed only for a subset of the entire sample, those for whom the latent index variable, L_t^*, is less than zero. Relation (10) is a selection function depending (possibly) on the same independent variables in (11) as well as on additional factors. In practice, it is known only whether an observation is observed or not, i.e. L_t=1 ($L_t^* < 0$) if observed and L_t=0 ($L_t^* \geq 0$), if not. If the error terms, U^{**}_t and U^*_t, are correlated, estimation of (11) on the observed sample, ignoring (10), may lead to inconsistent parameter estimates and thus incorrect inferences. Often attrition appears to be selective in the sense that mean values differ between those who attrite and those who do not (e.g. with respect to schooling attainment in the baseline). However, what is of concern is not the level of attrition nor such mean differences but whether, and to what extent, it invalidates the inferences that can be made using the data. It is desirable to attempt to address sample attrition, even if such efforts must be limited to considering attrition on observable variables. Some options include: (1) Testing with baseline data whether the coefficients in multivariate relations differ significantly for those who subsequently attrite and those who do not. Simple tests using data from both developing and developed countries often find no evidence of significant differences even if mean characteristics do differ significantly (e.g. Alderman *et al.* (2001b) for Bolivia, Kenya and South Africa, Moffitt (1998) for developing countries); (2) Include in the specification of relation (11) all the plausible covariates, some of which may be associated with attrition. Conditional on the maintained assumptions about the functional form, attrition selection on observed right-side variables does not lead to attrition bias (Fitzgerald, Gottschalk and Moffitt 1998a); (3) Implement correction procedures for attrition on observed variables that might relate to attrition even if they are not directly in the model, such as interviewer characteristics and whether other family members remain in the original sample unit (Fitzgerald, Gottschalk and Moffitt 1998a, b). Recent studies for developing countries find that most key results are not influenced by sample attrition on observed variables (Behrman *et al.* (2006), Maluccio *et al.* (2006)). Given the potential importance of attrition in confounding the results, nevertheless, it is desirable for studies of the IGT of poverty to test to the extent possible for attrition biases – and in new data collection, to try to limit the extent of attrition as much as possible (the Indonesian Family Life Survey, available on the web, provides an excellent model).

4. Strengths and Limitations of Analysis of Various Types of Data and Related Methods

The previous two sections point to considerable challenges in undertaking empirical estimates of causal relations pertaining to the IGT of poverty. Better data lessens such challenges. The ideal would be representative panel data with substantial detail updated frequently on every member of the family over several generations, substantial detail on the context (markets, public services, environment, kin and social networks) also updated frequently over the same time period, and a series of experimental and quasi-experimental shocks over the same time period that would permit identification of the short- and long-run causal effects. Such data are not available for any society, and the data that are available generally tend to be less satisfactorily (though not always) for developing than for developed economies. While it would always be desirable to obtain better data, it is also desirable to gain as much understanding as possible from existing data. Most data permit at least some examination of how robust the estimates of the IGT of poverty are to some major assumptions regarding possible data limitations. This section considers various types of data and related analytical techniques in turn and how they can be informative about particular causal mechanisms of IGT, with some examples of their use for developing countries.

4.1 Some Major Characteristics Pertaining to Data Quality

Before turning to different major types of data, it is useful to note four critical aspects of data quality that are common across different data options:

1) <u>Representativeness:</u> How representative are the data for the population of interest? Can inferences be made for some population of interest beyond the sample, perhaps through weighting the observations appropriately? Some potentially very interesting data, such as individual and family histories (e.g. Watkin's 2004 use of journals kept by four individuals on HIV/AIDS in Malawi), school- or clinic-based data (e.g. Miguel and Kremer's 2004 study of worms and education for those in school in Kenya) and much (though not all) qualitative data may raise interesting questions and conjectures for more systematic study, but be difficult to interpret with regard to their implications for broader populations.

2) <u>Power, sample size and sample design</u>: Power refers to whether the sample is large enough to identify the effect of interest at a given significance level. Power calculations indicate how large the sample size needs to be to identify such an effect with a specified degree of confidence (e.g. at the 5% level); standard software packages such as Stata can facilitate power calculations (e.g. Behrman and Todd, 1999b). For example, suppose that the question of interest is whether another grade of maternal schooling attainment increases adult children's access to resources by at least 3% at the 5% significance level. The sample size in terms of households necessary to have any particular level of statistical power, of course, varies depending on what question is being asked. For instance a larger number of households is required the more fine-tuned the question is with respect to demographic groups – so many more households will be needed to investigate the possibility of a given impact with given significance between parental schooling and stunting among three-year old girls than to investigate the possibility of the same percentage impact with the same significance between parental schooling and schooling attendance for all 6-12 year-old children (even with correction of the standard errors for clustering at the family level). If the sample design involves clustering, the number of clusters and the intracluster correlations are important in addition to the number of households (see discussion in Section 3). It is sensible for researchers to ask questions about power when they initiate analysis rather than bemoan that the sample size is too small after they have invested a lot of resources. Data that in other respects might appear very promising for the analysis of the IGT of poverty may not warrant analysis if the power is too low.

3) <u>Coverage of relevant variables</u>: To state the obvious, data are of value for the analysis of the IGT of poverty only if they include some information on variables for both the child and the parent that capture critical elements of the links across the life-cycle stages that are discussed in Section 3. Many data sets, for example, have information on individuals' income and schooling and their co-resident children's schooling to date (e.g. most labour force surveys designed to capture the current conditions in the labour market as used, for a specific illustration, in Behrman, Deolalikar and Tinakorn, 2006). Such data often can illuminate some part of the chain implicit in going from the right-side

of relation (9) to the adult child's resource access and poverty status (the dependent variable in relation 9) – such as the relation between adults' completed schooling and their income or the relation between parental household income and school progression of co-resident children. It might be in some cases possible to link together different components of the linkage as estimated from various data sets. But such data do not permit direct estimation of relation (9) nor of many of the links between adult children's income and their parental background.

4) Measurement errors: Data typically are imperfect representations of the underlying constructs of interest. Even for data such as self-reported completed schooling in developed countries, the noise-to-signal ratio[14] has been estimated to be on the order of magnitude of 10% (e.g. Behrman, Rosenzweig and Taubman, 1994). Random measurement error in right-side variables tends to cause biases in the estimated coefficients towards zero – intuitively the noise masks part of the effect of the signal so the absolute magnitude of the coefficient is underestimated. This effect tends to be exacerbated in fixed effects estimates because controlling for fixed effects tends to increase the noise-to-signal ratio. Random measurement error can be eliminated if there are multiple reports on a variable and the measurement error across the reports are not correlated (e.g. schooling attainment as reported not only by the individual but by others, as in Ashenfelter and Krueger (1994) or Behrman, Rosenzweig and Taubman (1994)). Instrumental variable (IV) estimates, as noted in Section 3, may also eliminate this bias towards zero due to random measurement error. However, measurement is not only random, it might also have systematic components, particularly on what might be perceived as sensitive topics such as the extent of pre-marital and extra-marital sexual relations (e.g. Mensch, Hewett and Erulkar (2003) on reported extent of such relations by Kenyan adolescents using different data collection methods). Such systematic errors may make inferences about such behaviours, even if they are very important in understanding the IGT of poverty, very difficult.

4.2 Some Major Types of Data for Investigating the IGT of Poverty

Cross-sectional data: Cross-sectional surveys and censuses are the most common type of available data. Cross-sectional household surveys tend to have some intergenerational (e.g. parent-child, grandparent-child) information for children of different birth cohorts (ages) such that they still are co-residing with their parents and possibly some of their grandparents. There are many cross-sectional surveys that are representative, often with a stratified cluster sample design, of populations of interests for this note. Censuses, of course, are by definition representative of the populations covered except for possible undercounting (particularly of more marginal groups). There are also many cross-sectional surveys that are not representative, but based instead on behaviours such as attending schools or health clinics. These non-representative data sources may have rich information - but interpretation of the implications of analysis for broader populations of interest may be difficult unless it is possible to control for the selection decision into the sample. In some cases it may be possible to control for such selectivity into the sample by using other representative or census data to estimate the selection rules on a set of variables common to the selected and the representative data sources.

[14] The "noise-to-signal" ratio refers to the fact that most concepts are not measured perfectly, particularly in self-reported data, but have some random measurement error (leaving aside for the moment systematic measurement error). This measurement error is referred to as "noise" (since it disguises or hides the systematic part or "signal" in the data). The variance in variable as measured therefore can be decomposed into the variance due to noise and the variance due to the signal, with higher "noise-to-signal" indicating more contamination due to random measurement error.

Cross-sectional data sources vary considerably in their sample sizes and statistical power – and, as noted above in Section 4.1, the required sample size for a given level of power and significance depends on the extent to which the question being asked is focused on a narrow or broader demographic group.

Typically cross-sectional data do not include information on the variables necessary to estimate directly reduced-form relations between parental family background and adult resource access as in relation (9). That is because typically cross-sectional data do not collect much if any information on non-residents of households. Therefore, cross-sectional data can most commonly be used to estimate reduced-form relations in the spirit of relations (4b) and (6) – that is, what are the relations between parental family background and indicators of child development during preschool and the school years for children who are co-resident with their parents. There are many studies in the literature, for example, between parental characteristics and child enrolment in school, child progression rates through school (often represented by the gap between completed grades of schooling and the number of grades that would have been completed had the child started at the normal or legal age and progressed one grade each year), and child schooling attainment (though this variable is right-censored[15] for children still in school; see King and Lillard (1987) for estimates from the Philippines and Malaysia). If the information in such data is limited to co-resident children, however, the selection of which children have left the household may make such analysis difficult for older children. There are also many studies that relate parental characteristics to child health and nutritional status. The majority of the available estimates, though hardly all of them, indicate greater associations between maternal schooling and command over resources as reflected in income than between similar characteristics for fathers (e.g. Thomas (1990) for Brazil).[16]

The typical cross-sectional data permit some, but limited, control for the estimation problems that are discussed in Sections 2 and 3. For instance, the cluster structure of many cross-sectional data sets permits the control for unobserved cluster (e.g. community) effects that might be correlated with family background characteristics and cause biases in the estimated impact of family background characteristics if not controlled. That information is available on a number of children also permits the investigation of time-varying changes that affect siblings differentially; this is not likely to be useful often for the first-order effects of parental characteristics,[17] but may be for evaluating programme effects (e.g. Parker, Todd and Wolpin (2006) on the impact of the Mexican Oportunidades programme on schooling of children too old versus those of age to be affected by the programme) and possibly their interaction with family background characteristics. Also the inclusion of a number of assets in many such data sets permits the construction of more-permanent measures of parental household resources through using such assets as instruments in IV estimates rather than using current income (which often has large transitory components for poor families in developing countries - see Deolalikar and Gaiha (1993) for rural India), which tends to lead to much larger coefficients of parental income in child schooling relations and thus greater estimated

[15] "Right-censored" means, for example, that for children still in school their final schooling attainment (to be determined in the future) is not observed, just the schooling attainment to the time of the survey.

[16] The conventional wisdom held by some seems to be that this evidence is overwhelming. But a now-dated survey of all the estimates that could be located of associations between parental schooling and child schooling found that larger estimates were reported for mothers' schooling than for fathers' schooling in 52% of the cases (Behrman, 1997). Moreover, as noted above, estimates for mothers' schooling may be more upward biased due to omitted variable biases if women alter their time use more with schooling than do men, as seems to be the case in many societies (Behrman and Rosenzweig, 2002, 2005 given an example for the United States).

[17] An exception is the study by Rosenzweig and Wolpin (1995) on the impact of maternal schooling on preschool cognition in the United States for young mothers who went to school between births in which they controlled for all unobserved maternal fixed characteristics.

IGT of poverty than might appear with current income only (e.g. Behrman and Knowles (1999) for Vietnam).[18]

A subset of cross-sectional surveys have additional information that make them richer than most cross-sectional data sets for examining the questions considered in this note. Some examples include: schooling attainment for all of the parents of household members whether co-resident or not, which opens up the possibility of investigating the relation between income or expenditures of current adults and their parents' schooling attainment along the lines of relation (9) (e.g. for Brazil, see Lam and Schoeni, 1993, 1994); schooling attainment for all (not just co-resident) children of the adults in the household, which permits investigation of relation (6) without the selection problems due to older children having left the household prior to the survey (e.g. Parker, Todd and Wolpin (2006) for Mexico); information on income by individual adults including "nonlabour earnings" that arguably are not correlated with the unobserved endowments in relations (4b) or (6) so that the impact of mothers' versus fathers' control over resources can be investigated (e.g. Thomas (1990) for Brazil); information on assets bought into the marriage by the current adult parents that can be used to explore the impact of resources under control of mothers versus fathers on investments in children that arguably are independent of the endowments in relations (4b) and (6) (e.g. Quisumbing and Maluccio (2003) for Bangladesh, Ethiopia, Indonesia and South Africa).[19]

Many cross-sectional data sets can be enriched by linking them with time series administrative data on public services (particularly related to health and education), communication and transportation, and weather conditions. For example: (1) Even if the basic household data being used are cross-sectional, time series on available services may be informative for time periods earlier in their children's life if there is reason to believe that the impact of parental characteristics depends on the nature of such services (e.g. mothers' schooling enhances the positive impacts of health services when children are very young); (2) such data may make possible within-sibling estimates if different siblings faced different community services during critical periods such as early childhood, again, perhaps in interaction with parental characteristics; (3) such data may provide instruments that arguably are independent of the unobserved factors on the right-side of the relations in Section 2 but that predict sufficiently well the right-side parental characteristics that good IV estimates can be obtained (e.g. the nature of schooling options when parents were of school age might provide good instruments for parental schooling attainment, as for a different purpose for Indonesia in Duflo (2001); levels and variations in rainfall may provide good instruments for parental income in agricultural areas, as for a different purpose in India by Wolpin (1982) and in Thailand by Paxson (1992) and for purposes much more directly related to this note in the study of health, schooling and socioeconomic consequences in Indonesia by Maccini and Yang (2006)).

<u>Longitudinal or panel data</u>: These data follow individuals and/or households over time. They generally provide a more satisfactory means of identifying the IGT of poverty because: (1)

[18] Data on such assets may also permit the construction of wealth indices in the absence of income measures, such as the use of principle components of such assets for wealth as in the INCAP studies in Guatemala (e.g., Pollitt *et al.*, 1993, as was subsequently popularized by Filmer and Pritchett, 2001, using the DHS data for India). The economic interpretation of such indices, at least in comparison with weighting assets by their prices, is not clear.

[19] These last two examples are improvements over previous studies that used parental schooling or income to see if there were differential impacts between mothers' and fathers' resources and investments in children, but were difficult to interpret because these indicators are likely to be associated not only with control over resources but also time use and unobserved abilities and motivations. However, although these two examples probably are improvements, it is not clear that the representations that they use are independent of time uses and unobserved characteristics, so they may be subject to a weaker version of the same problem.

the prospective data gathered in earlier rounds is likely to be less contaminated with measurement error and more complete than recall data from cross-sectional data sources; (2) the multiple observations over time in some cases permit the control for unobserved individual fixed effects such as innate ability and health; (3) the multiple observations over time permit the exploration of dynamics of effects and whether they tend to diminish over time or are enhanced over time, perhaps in part in interaction with dimensions of the environment in which the individual is developing (e.g. do early-life nutritional shocks have only short-run or long-run effects, and to what extent does it depend on whether subsequently the school system or other institutions can in part or in whole compensate for them); and (4) the multiple observations over time permit exploring the impacts of possibly changing contextual factors, depending in part on how rich is the contextual information. On the other hand, longitudinal data are more expensive to collect than a time series of cross sections of equal size because of the costs and problems in following up with the same individuals, are subject to attrition because of factors such as mortality and migration, and are less likely to be representative of the current overall population (though not necessarily of particular birth cohorts) than a time series of cross sections even if there is not attrition.

There currently exist relatively few longitudinal household data sets from developing countries with panels over several decades as needed to see how parental characteristics measured prospectively early in the life cycle affect adult outcomes. But there are a few. Examples include: the INCAP Guatemalan data on children 0-7 years old in 1969-1977 with follow-up rounds in 1988-9 and 2002-4, at which time the children were 25-42 years of age (Martorell *et al.*, 2005); the Cebu (Philippines) Longitudinal Health and Nutrition data of births in 1983 with the last follow-up in 2005 when the children were up to 20-22 years old and their mothers were from 35 to 69 years old (Cebu Study Team, 1991, Daniels and Adair, 2004); the Pelotas Brazilian data on the birth cohort of 1982 with the last follow-up in 2004-5 when the children were up to 25 years of age (Victora, Victora and Barros, 1990, Victora and Barros, 2005); the NCAER rural Indian data starting in 1969-71 with follow-up until 2002; the Bangladeshi nutritional data with follow-up after over two decades (Pitt, Rosenzweig and Hassan, 1990, 2006).

There are many more longitudinal data sets that cover shorter, but important segments of the life-cycle stages noted above. A few examples include: The Mexican PROGRESA data for 1997-2003; a number of the Demographic Health Survey (DHS) data sets; the Vietnam Living Standard Measurement (LSMS) Survey; the Chilean *Encuesta de Protección Social* survey from 2002-2006 (Bravo *et al.*, 2006); the Bolivian PIDI evaluation data (Behrman, Cheng and Todd, 2004); the Malawian Diffusion and Ideation Change Project Data for 1998-2006 (Watkins *et al.*, 2003); the Kenyan school-based sample (Miguel and Kremer, 2004); the Colombian *Familias en Acción* sample for 2002-6 (Attanasio *et al.*, 2004); the Philippines Early Childhood Development Survey for 2001-6 (Armecin *et al.*, 2006, Ghuman *et al.*, 2005, 2006a,b); the Mexican Family Life Survey (Rubalcalva and Teruel, 2004); the Indonesian Family Life Survey (Thomas *et al.*, 2003)

Longitudinal data can, and in some cases are, enriched in ways that are parallel to cross-sectional data: inclusion of questions for previous generations or other people not currently in the households, linkage to administrative data. In addition, some longitudinal data have built into their design controlled experiments with random assignment between treatment and controls groups.[20] Some prominent examples include: The Mexican rural PROGRESA programme with random assignment of initial treatment versus controls for 506 communities;

[20] Since such experiments almost always have baseline and post-intervention data rounds, they are longitudinal and not cross-sectional. In principle if the treatment and control groups are randomly selected then only looking at the cross-sectional post-treatment data should be informative. But it would not be possible in such a case to test whether or not the assignment really was random (as, for example, in Behrman and Todd (1999a) for the Mexican PROGRESA data).

the Kenyan random assignment of various treatments (including deworming, flip charts) among 75 schools; the Guatemalan INCAP data with random assignment of nutritional supplements among participant communities; experimental assignment of fees and distances to VCT clinics in the Malawian Ideation and Diffusion Change Project (MDICP, Watkins *et al.*, 2003); Thomas *et al.* (2003) with random assignment of iron supplements in Indonesia. Such experiments provide (a) capacity for identifying the causal effect of treatment and (b) the possibility of identifying the impact of one behavioural choice affected by the treatment on another by using the experimental assignment as an instrument for IV estimates. But there also are limitations of experiments: some experiments may be viewed as unethical or politically unwise; selective attrition between the treatment and control areas may introduce selectivity biases; and even very good experiments only provide "black box" estimates of the impact of the specific intervention used and not of alternative counterfactuals, including longer-run impacts.[21]

Time series of cross-sectional surveys: A time series of cross-sectional surveys provides a means of tracing cross-sectional associations over time as cohorts age and possibly permit controlling for cohort-specific unobserved factors. This has the advantage of using more readily available data than longitudinal data, as well as data that are representative for each cross section. Deaton and Paxson (1994) give an example in which they trace the persistence of earnings shocks experienced early in the adult life cycle as cohorts age in Taiwan and the United States. The possibilities for using such an approach to investigate the IGT of poverty seem limited, but perhaps underexplored.

Qualitative data sources: Most other possible data sources for investigating the IGT of poverty can be considered to fit within the categories of being either cross-sectional or longitudinal (particularly since cross-sectional and longitudinal data may be either quantitative or qualitative). The same general questions of data quality (Section 4.1) apply for such data sources. That is, the questions of representativeness, power, variable coverage and measurement errors hold for qualitative as well as for quantitative data. Extensive family or individual histories or focus groups may provide useful insights regarding hypotheses regarding IGT of poverty whether or not they are representative or have sufficient power or whatever the nature of the measurement errors. But if inferences are to be drawn from such data sources about aspects of IGT of poverty for some population larger than the sample itself, it is necessary to know how the sample relates to the larger population and to assure that power is sufficient and to understand measurement errors. Likewise, it is necessary to recognise that associations do not imply causality with qualitative data any more than with quantitative data. Indeed it may be equally important to attempt to control for unobserved factors in qualitative analyses as in qualitative analysis. With regard to representativeness of qualitative data, there is a strong attraction to drawing the sample in the same way that one would draw the sample for quantitative data. Indeed there are possibilities, that in a few cases have been exploited, of combining qualitative and quantitative data, not only with qualitative data collected first to inform the questionnaire design for quantitative data, but with the subsample for the qualitative data drawn randomly from that for the quantitative data so that not only the sample characteristics for the qualitative data are known but it is possible in the analysis to combine the quantitative and the qualitative data (e.g. such strategies have been followed for the MDICP project described in Watkins *et al.*, 2003).

[21] At the cost of the assumptions necessary to estimate structural models of the behaviours such as are outlined in Section 2, evaluations of counterfactual polices can be made (e.g. different treatments, impacts for longer time periods than observed in the data). Todd and Wolpin (2006) provide an example using the Mexican PROGRESA data. They estimate a structural model using baseline data, then test the model's predictions against the experimental results (and find that the model predicts fairly well), and the use the model to conduct counterfactual experiments (e.g. with different scholarship schedules for different grades, with the programme running many years).

5. Conclusions

Undertaking good empirical analyses of the IGT of poverty is important for improving the basis for both predicting what is likely to happen regarding poverty and for understanding what impact various changes, including policy changes, might have on the trajectory of poverty. Undertaking good empirical analysis of aspects of the IGT of poverty, however, is challenging given data limitations. These challenges are clarified in this note by: (1) considering the types of relations that might be estimated to be informative for understanding better aspects of the links that connect parental background and adult child resource access and therefore poverty within an intergenerational life-cycle framework in which there may be important unobserved variables such as genetic ability endowments; (2) considering possible resolutions to some of the estimation problems that such a framework implies; and (3) considering different types of data that are available that might permit advances in knowledge of the IGT of poverty in developing countries in light of such a framework of analysis and the related estimation alternatives. Throughout efforts are made to give illustrations of related studies, primarily on developing countries but also in some cases for developed economies.

Despite these challenges, a number of interesting options are available for estimating relations pertaining to aspects of the IGT of poverty and to explore the robustness of estimates to alternative strategies for dealing with the estimation problems. Of course, it is desirable to focus on where the expected gains are greatest. Relevant considerations in deciding where the gains are likely to be greatest would seem to include:

- Are there aspects of the links between parental background and adult child resource use that are described in Section 2 for which improved knowledge is particularly important because the effects are thought to be particularly large or because there is great uncertainty about the probable magnitudes of the effects?[22]

- What is the nature of data quality with regard to representativeness, power, coverage of important concepts in the linkage between parental background and adult child resources access and therefore the IGT of poverty, and measurement error?

- What special features of the data might permit better exploration of the IGT of poverty? Is there complete information on key variables for two or three generations? Is there information on intergenerational transfers? Can the data be linked to time series records on a range of contextual changes? Can the robustness of the estimates to at least some of the estimation problems be tested, for instance by exploiting information on siblings, members of the same sample cluster, experiments, and/or longitudinal data?

Through careful examination of existing data, keeping in mind the considerations that are discussed in this note, much can be learned about the IGT of poverty in developing countries. But at the same time, in order to create a better informational basis for such studies in the future, it is important to be alert to opportunities for improving data collection and to encourage the collection of new and better data.

[22] While such a question seems obvious to ask, it is not clear that it always is raised in determining the portfolio of social science research. Recent analysis suggests, for example, that the focus on particular health/disease conditions in the social science literature on health and development has emphasized HIV/AIDS and injuries relative to non-communicable diseases than current or project future distributions of these health/disease conditions would suggest was warranted (Behrman, Behrman and Perez, 2006).

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